Research Article

The partisan ties of lobbying firms

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Abstract

This article examines lobbying firms as intermediaries between organized interests and legislators in the United States. It states a partisan theory of legislative subsidy in which lobbying firms are institutions with relatively stable partisan identities. Firms generate greater revenues when their clients believe that firms' partisan ties are valued highly by members of Congress. It hypothesizes that firms that have partisan ties to the majority party receive greater revenues than do firms that do not have such ties, as well as that partisan ties with the House majority party lead to greater financial returns than do partisan ties to the Senate majority party. These hypotheses are tested using data available under the Lobbying Disclosure Act from 2008 to 2016. Panel regression analysis indicates that firms receive financial benefits when they have partisan ties with the majority party in the House but not necessarily with the Senate majority party, while controlling for firm-level covariates (number of clients, diversity, and organizational characteristics). A difference-in-differences analysis establishes that Democratically aligned lobbying firms experienced financial losses when the Republican Party reclaimed the House in 2011, but there were no significant differences between Republican and Democratic firms when the Republicans reclaimed the Senate in 2015.

Keywords

Political parties, lobbying firms, Congress, House of Representatives, Senate, United States

Introduction

How interest groups attempt to influence Congress has been a significant topic of inquiry since the early days of the political science profession (Herring, 1929). The majority of research in this area has investigated either lobbyists as individuals or the interest group clients that they represent (Baumgartner et al., 2009). Despite this history, the field has only recently turned its focus to the business side of lobbying (Drutman, 2015; LaPira and Thomas, 2017). Research has begun to examine not only the substance of what lobbyists do – such as how they choose their issue positions – but also how they sustain an economic enterprise.

Developing scholarship on the business of lobbying calls attention to "the presence of the lobbying industry as an intermediary" in the political process (Bertrand et al., 2014: 3886). However, this progressing literature still gives only minimal attention to lobbying *firms* – the business organizations that are responsible for hiring and managing thousands of contract lobbyists. Some studies use data on lobbying firms (Bertrand et al., 2014; Blains i Vidal et al., 2012), but this work seeks to explain the behavior of lobbyists as *individuals* rather than the *firms* that employ them.

This article argues that lobbying firms themselves are institutions with relatively stable partisan identities that make them relevant intermediaries between organized interests and legislators. Firms may have partisan ties that affect their revenue generation by helping to resolve uncertainty on the part of potential clients about how the firm is likely to perform its work. To understand this intermediation, this article tests a model of lobbying firm revenue. It evaluates the extent to which partisan ties between lobbying firms and the partisan leadership of Congress explains the ability of firms to raise lobbying revenues from their clients. In extending beyond previous studies that analyzed lobbying revenue as an individual-level phenomenon, the model incorporates firm partisan identities, as well as firm clientele

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size, diversity, and other organizational characteristics. This analysis lays a foundation for appreciating how lobbying firms are political enterprises within the party system.

This article has three key findings. First, lobbying firms have identifiable and relatively stable partisan identities. Second, lobbying firm ties with the partisan leadership of the US House of Representatives are significantly associated with higher revenues for firms, approximately US$5000 to US$6000 per lobbyist per quarter. Third, change in the party controlling the House of Representatives is associated with revenue losses for firms tied to the party losing control, roughly US$40,000 per lobbyist for one year. Evidence regarding firms' ties with the partisan leadership of the Senate is mixed, with the presence of significant effects depending on model specification.

A partisan theory of legislative subsidy

To appreciate why lobbying firms may benefit financially from their connections with political parties, we consider why legislators pay attention to lobbyists at all. Hall and Deardorff (2006) explained that legislators face perpetual shortages of time and staff resources to work on issues they care about. If lobbyists bring policy information, political intelligence, and legislative labor to issues that legislators are concerned with, then legislators may be able to devote more time to those issues than they otherwise would. Meeting with lobbyists expands a legislator's time budget for working on an issue, thus subsidizing attention to that issue.

If legislators are to accept the information, intelligence, and labor provided by lobbyists, then they need to trust lobbyists. For this reason, legislators are most likely to meet with lobbyists who are among their closest allies (Hojnacki and Kimball, 1998). During a period of high partisan polarization – such as the contemporary era – allies are likely to be members of the same party (Poole and Rosenthal, 2000). Organizational identity is a key way that advocacy organizations become known with respect to their political loyalties (Heaney, 2004; Heaney and Leifeld, 2018). Thus, we argue that the partisan organizational identities of lobbying firms are important to them being understood as partisan allies to legislators.

Given the procedural advantages that accompany majority status in Congress, outside interests are willing to pay more to have access to the majority than to have access to the minority (Cox and Magar, 1999). If lobbyists gain access to legislators by being allies – which usually requires being co-partisans – then sharing partisan identification with the party in control of a chamber should translate not only into more access but also into greater payments from clients for the access that is granted.

Based on these arguments, we state:

Hypothesis 1: *Lobbying firms that have partisan ties to the majority party receive greater lobbying revenues than do lobbying firms that do not have such ties, other things equal*.

The willingness of legislators to accept subsidies provided by lobbyists likely depends not only on partisan alignment but also on the degree to which they are already supported by staff. A legislator with an extensive staff may place less reliance on lobbyists than would a legislator with fewer staff resources. Given notable differences between members of the House and Senate in access to staff resources – with the Senate allocating more resources per member than the House – lobbying clients may be willing to pay differently for lobbying the chambers (LaPira and Thomas, 2017: 13). Thus, we state:

Hypothesis 2: *Lobbying firms receive greater financial returns when they have partisan ties with the partisan majority of the House than when they have partisan ties with the partisan majority of the Senate, other things equal*.

In addition to the partisan theory of legislative subsidy, there may be other reasons to expect differential returns from lobbying the two chambers. Baker (2008: 144–151) interviewed 12 lobbyists about their perceptions of differences in lobbying the House and Senate. His respondents perceived Senators as being harder to lobby than House members because they viewed Senators as more cross-pressured by diverse constituencies, more concerned with broad national interests, and less attentive to the technical details of legislation. Thus, there may be reasons in addition to staffing disparities for differential lobbying returns between the chambers.

Organizational characteristics and lobbying firm revenue

Lobbying firms have organizational-level characteristics beyond their partisan identities that may affect their revenue stream. First, number of clients is important because larger firms are better known, more prestigious, and thus more capable of demanding higher payments for their services than are firms with fewer clients (Schiff et al., 2015). Second, client diversity with respect to issues and industries is important because of the long-standing expectation in economics that diversification spreads risk across investment portfolios (Markowitz, 1959). Third, firm organizational characteristics – such as whether a firm is a law firm, has international offices, the number of its domestic offices, and its age – may account for variations in prestige and economies of scale for client recruitment that may correspond with how firms earn revenue. For example, a lobbying firm that is also a law firm may assign its associates both legal and lobbying tasks, thus potentially reducing its lobbying revenue per lobbyist (but increasing its legal revenue).

Research design

We draw on lobbying reports that are available as a result of the Lobbying Disclosure Act (LDA) of 1995. We focus on reports filed from 2008 to 2016, after the passage of the Honest Leadership and Open Government Act of 2007, which changed the LDA reporting frequency from semi-annually to quarterly and modified rules for lobbyist registration. We combine publicly available lobbying data with original research on the lobbying firms appearing in the records.

We modeled our dependent variable, *Revenue per Lobbyist*, in real dollars, which reflects the aggregate income each lobbying firm earned from all clients in a period. We adjusted for inflation using the Consumer Price Index for All Urban Consumers: All Items (FRED, 2017).

Our focal independent variables are *Firm Partisan Alignment with House Leadership* and *Firm Partisan Alignment with Senate Leadership*. It is possible for a firm to be aligned with both the House and Senate, one or the other, or neither chamber. We did not consider bipartisan firms or those without clear partisan identifications to be aligned with either chamber.

We determined the partisan ties of a firm using campaign contributions of its lobbyists. If a firm's lobbyists gave a high percentage of their contributions to one party, then we considered the firm to be aligned with that party. In recognizing that the boundary between "partisan" and "bipartisan" firms may be fuzzy, we calculated partisanship using three different giving thresholds: 85%, 90%, and 95%. Further, we calculated two versions of each of these measures. For the first version, each firm's alignment was calculated once and fixed in time (based on lobbyists' campaign contributions dating to 1990), treating it as a relatively stable characteristic. The second version allowed the firm's identity to vary each year as a function of changes in its lobbying roster and campaign giving, treating it as a more fluid factor. Fleiss's κ, a reliability measure for categorial variables, was 0.86 for the time-fixed measure and 0.84 for the time-varying measure, indicating "substantial" to "nearly perfect" agreement (Landis and Koch, 1977). The time-fixed and time-varying measures of partisan ties exhibit highly correlated partisan classifications (ranging from 0.62 to 0.63, $p \leqslant 0.05$), yielding strong evidence of relatively stable firm partisan identities.

We examined the validity of our measures by using data from firms' web pages on the founders of lobbying firms. Firms were classified as Democratic if they had only Democratic founders, Republican if they had only Republican founders, and bipartisan if they had at least one founder from each party. We correlated these founder-based measures with the contribution-based measures discussed above. We found positive, statistically significant correlations between firms having only Democratic founders and those making homogeneously Democratic contributions, or between having only Republican founders and those making homogeneously Republican contributions (with correlations ranging from 0.31 to 0.55, $p \leqslant 0.05$), supporting the conclusion that our measures are relatively stable and valid indicators of firm identity. Indeed, the influence of the founders is not ephemeral; it persists over time.

We calculated the *Number of Clients* and *Client Diversity* using LDA data. Data on *Law Firms, International Offices, Number of Domestic Offices* and *Firm Age* were drawn from lobbying firm websites. These web-based variables contain substantial missing data because not all lobbying firms have websites. Smaller, newer, and disbanded firms were especially at risk of not having a site.

Statistical analysis

We analyzed quarterly data reported by an unbalanced panel of 1603 lobbying firms from the first quarter of 2008 through the third quarter of 2016. Registrants were taken from lobbying reports where the registrant and the client differed – indicating that the registrant was a contract lobbyist or firm hired by a client. To count as a firm, a registrant had to list two or more lobbyists as active in the same quarter at least once and have at least two quarters with nonzero dollars.

Descriptive statistics

Figure 1 shows the distribution of partisan giving, from which we calculated partisan ties, based on firms with nonzero campaign giving (84% of firms). It exhibits a bimodal pattern, with partisan firms clustered at the extremes of the distribution. The time-fixed and time-varying distributions are similar. One notable difference between them is that the percentage of Democratically tied firms is greater when using the time-varying measure than when using time-fixed measure. This pattern indicates that fewer firms reliably give 95% or more of their contributions to Democrats than there are erstwhile Democratic firms that meet this threshold periodically. Either way, the distribution reveals a drop-off in partisan ties after the 95% threshold. These findings indicate that roughly 42 to 60% of firms have a "partisan" bent and 40 to 58% have a "bipartisan" orientation, depending on the selected cutoffs.

Figure 2 reports the average revenues over time for bipartisan, Democratic, Republican, and unclassified firms. We used the 90% partisan-giving threshold and the time-fixed measure of partisanship for this graph. Bipartisan firms consistently earned greater average revenues than did their partisan-leaning competitors. Republican and Democratic firms were roughly at parity over time. However, a marginal advantage traded back and forth corresponding with congressional control. Democratic firms earned higher average revenues when Democrats held congressional majorities from 2008 to 2010. Republican firms

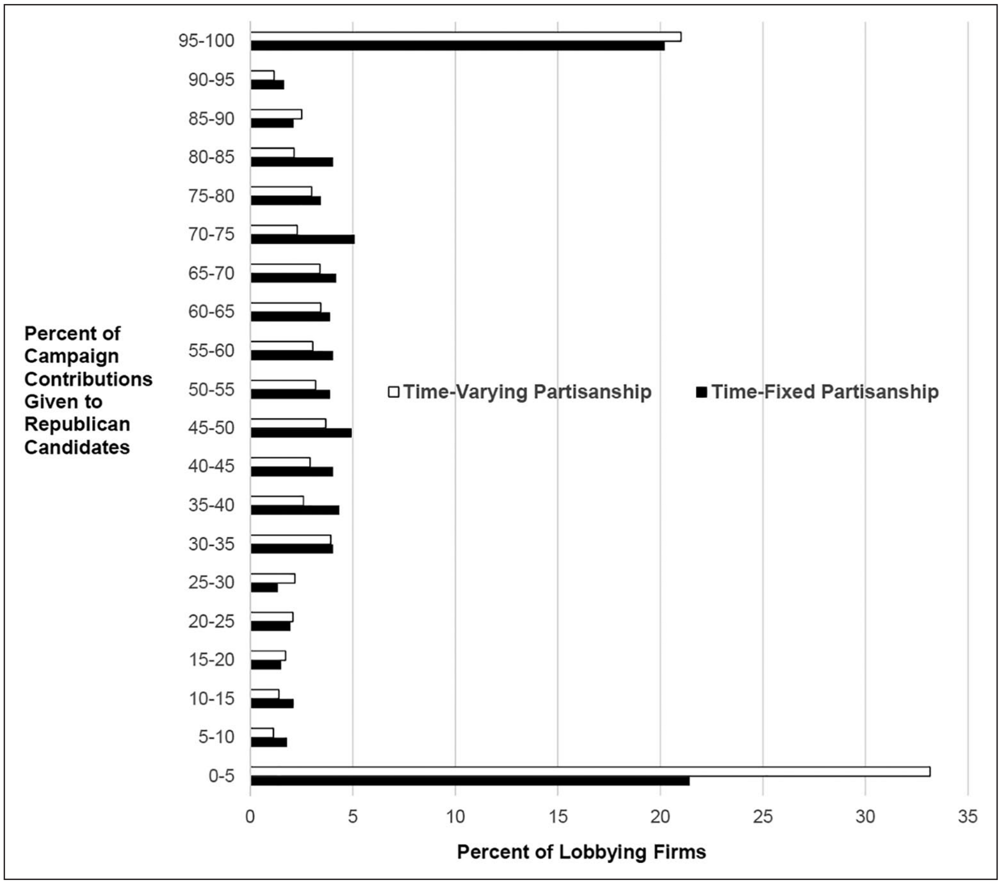

Figure 1. Distribution of partisan giving by lobbying firms.

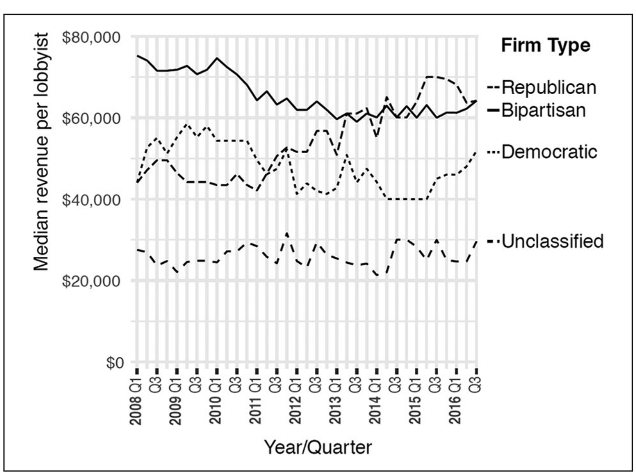

Figure 2. Revenue trends lobbying firms, 2008–2016.

earned more when Republicans reclaimed congressional control from 2011 through 2016. Unclassified firms earned consistently lower average revenues than did partisan and bipartisan firms.

We report variable definitions and descriptive statistics in Table 1.

Panel linear regression models

We estimated two sets of panel linear models. The first set was estimated on *Revenue per Lobbyist* as the dependent variable. Six versions were estimated to allow for time-fixed and time-varying measures of *Firm Aligned with House Leadership* and *Firm Aligned with Senate Leadership*, as well as 85%, 90%, and 95% cutoffs for each.

We included control variables for *Number of Clients* and *Client Diversity*, neither of which contained missing data. Our remaining, time-invariant control variables (*Law Firm, International Office, Number of Domestic Offices*, and *Firm Age*) contained missing data. We applied multiple imputation to address this issue (King et al., 2001). Imputation procedures and diagnostics are reported in Online Appendix A. Panel linear models 1.1 through 1.6 were estimated using random effects for firms and fixed effects for years (see Table 2). We employed HC3 Arellano standard errors (clustered by firm) that are robust to heteroskedasticity and serial autocorrelation (Arellano, 1987).

The results in Table 2 support Hypothesis 1 with respect to the House of Representatives. The coefficient on *Partisan Alignment with House Leadership* is significant in all six

Table 1. Variable definitions and ddescriptive statistics.

Variable	Minimum	Mean (SD)	Median	Maximum	Percent Missing
Revenue per Lobbyist per Quarter (dependent variable)	US$776	US$74,007 (US$71,481)	US$54,365	US$913,800	0.00
Firm Partisan Alignment with House Leadership – 1 if firm partisan identity matches House leadership; 0 otherwise					
Time-Fixed Partisan Identities – 85% threshold	0	0.23 (0.42)	0	1	0.00
Time-Fixed Partisan Identities – 90% threshold	0	0.19 (0.39)	0	1	0.00
Time-Fixed Partisan Identities – 95% threshold	0	0.14 (0.35)	0	1	0.00
Time-Varying Partisan Identities – 85% threshold	0	0.26 (0.44)	0	1	0.00
Time-Varying Partisan Identities – 90% threshold	0	0.23 (0.42)	0	1	0.00
Time-Varying Partisan Identities – 95% threshold	0	0.14 (0.35)	0	1	0.00
Firm Partisan Alignment with Senate Leadership – 1 if firm partisan identity matches Senate leadership; 0 otherwise					
Time-Fixed Partisan Identities – 85% threshold	0	0.26 (0.44)	0	1	0.00
Time-Fixed Partisan Identities – 90% threshold	0	0.22 (0.41)	0	1	0.00
Time-Fixed Partisan Identities – 95% threshold	0	0.17 (0.38)	0	1	0.00
Time-Varying Partisan Identities – 85% threshold	0	0.26 (0.44)	0	1	0.00
Time-Varying Partisan Identities – 90% threshold	0	0.23 (0.42)	0	1	0.00
Time-Varying Partisan Identities – 95% threshold	0	0.21 (0.40)	0	1	0.00
Number of Clients The unique number of entities a firm lobbied on behalf of	1	10.36 (17.94)	4	276	0.00
Client Diversity Firm issue diversity and firm industry diversity are calculated separately using Simpson's Reciprocal Index[a] and then added together	2	9.25 (9.28)	6	77	0.00
Law Firm 1 if firm was a law firm in 2015; 0 otherwise	0	0.26 (0.44)	0	1	34.42
International Office 1 if firm had an international office in 2015; 0 otherwise	0	0.16 (0.36)	0	1	35.74
Number of Domestic Offices The number of offices inside the United States	1	3.41 (7.20)	1	83	33.48
Firm Age Observed year minus founding year	0	18.31 (28.41)	8	180	9.33
N = 33,243					

Note: [a]Diversity = $[\Sigma[(n_i/N)^2]]^{-1}$, where n_i is the total dollars reported with that industry or issue for the firm in a given quarter and N is all dollars reported by the firm in a quarter (Simpson, 1949).

models. The results indicate that firm partisan ties are worth approximately US$5000 to US$6000 per lobbyist per quarter, other factors held constant. Hypothesis 1 is only supported with respect to the Senate when firm partisan alignment is based on a time-fixed measure (in models 1.1, 1.2, and 1.3). Hypothesis 2 is tested using a restricted hypothesis test that is supported only when firm partisan alignment is based on a time-varying measure (in models

Table 2. Firm revenue per lobbyist – panel linear models with firm random effects, temporal fixed-effects, and multiple imputation for missing data in firm attributes.

Model	1.1	1.2	1.3	1.4	1.5	1.6
	Time-Fixed Partisan Identities			*Time-Varying Partisan Identities*		
Partisan-giving threshold	85%	90%	95%	85%	90%	95%
	Coefficient (Standard error)					
Independent variable						
Firm Partisan Alignment with House Leadership	5536 *	6527 *	6096 *	5148 *	5346 *	4940 *
	(1734)	(1944)	(2132)	(1298)	(1343)	(1396)
Firm Partisan Alignment with Senate Leadership	3465 *	4624 *	4418 *	843	513	−267
	(1541)	(1712)	(1964)	(1091)	(1115)	(1155)
Number of Clients	1658 *	1656 *	1660 *	1651 *	1654 *	1653 *
	(245)	(245)	(245)	(246)	(246)	(246)
Client Diversity	1311 *	1306 *	1305 *	1317 *	1322 *	1323 *
	(320)	(319)	(320)	(322)	(321)	(322)
Law Firm	–5529 *	–5578 *	–5626 *	–6138 *	–6135 *	–6172 *
	(1993)	(1989)	(1990)	(2038)	(2039)	(2040)
International Office	–1005	–908	–912	–319	–343	–344
	(2265)	(2261)	(2270)	(2186)	(2183)	(2189)
Number of Domestic Offices	–304	–311	–296	–320	–321	–315
	(193)	(193)	(193)	(205)	(205)	(205)
Firm Age	–45	–42	–43	–29	–28	–29
	(25)	(25)	(25)	(23)	(23)	(23)
Constant	48,210 *	48,088 *	48,697 *	49,102 *	49,255 *	49,641 *
	(2860)	(2822)	(2813)	(2752)	(2753)	(2748)
Model Statistic						
N	33,243	33,243	33,243	33,243	33,243	33,243
Firms	1603	1603	1603	1603	1603	1603
T	2 to 35	2 to 35	2 to 35	2 to 35	2 to 35	2 to 35
F-statistic	4814 *	4846 *	4782 *	4815 *	4806 *	4760 *
F degrees of freedom	42, 33,200	42, 33,200	42, 33,200	42, 33,200	42, 33,200	42, 33,200
$\beta_1 = \beta_2$ Restricted hypothesis test *F*-statistic	4	3	1	20 *	23 *	24 *
$\beta_1 = \beta_2$ Test *F* degrees of freedom	1, 33,200	1, 33,200	1, 33,200	1, 33,200	1, 33,200	1, 33,200

Note: * $p \leq 0.05$.

1.4, 1.5, and 1.6). Online Appendix B shows that these results are substantively the same as those obtained when using a fixed-effects specification. Online Appendix C indicates that support for Hypothesis 1 would be somewhat higher were we to consider the effects of multicollinearity on suppressing the significance of Senate effects. The control variables, *Number of Clients* and *Client Diversity*, are significant with positive coefficients, while our *Law Firm* control variable is significant with negative coefficients.

The second set of models was estimated on *Change in Revenue per Lobbyist* as the dependent variable. These first-differences specifications use *Change in* each of the independent variables. The advantage of estimating these models is that they remove residual unobserved heterogeneity (Greene, 2012: 356). They also remove time-invariant, firm-level independent variables from the models (i.e., *Law Firm, International Office, Number of Domestic Offices*), since these variables have $\Delta X = 0$ in all cases, as well as *Firm Age*, since $\Delta X = 1$, yielding a constant. Consequently, variables containing missing data drop from the equations, eliminating the need for imputation. We followed the same procedures for estimating standard errors as in the first set of models.

The results of the first-differences analysis are in Table 3. Hypothesis 1 is supported with respect to the House in five of the six models, with the coefficient falling just short of the conventional threshold of significance ($p \approx 0.07$) in Model 2.5. Substantively, these results indicate that a change in firm alignment with the House is worth approximately US$1800 to US$5000 per lobbyist per quarter. We find no support for Hypothesis 1 with regard to the Senate in the second set of models. Hypothesis 2 is supported with respect to differences between the House and Senate in two of six models. For the control variables, *Change in Number of*

Table 3. Change in revenue per lobbyist – panel linear models on first differences (i.e., ΔY on ΔX).

Model	2.1	2.2	2.3	2.4	2.5	2.6
	Time-Fixed Partisan Identities			*Time-Varying Partisan Identities*		
Partisan-giving threshold	85%	90%	95%	85%	90%	95%
	Coefficient (Standard error)					
Independent variable						
Change in Firm Partisan Alignment with House Leadership	3520 *	3638 *	5048 *	1819 *	1741	2370 *
	(1475)	(1641)	(1849)	(871)	(952)	(952)
Change in Firm Partisan Alignment with Senate Leadership	706	1915	2829	–787	–629	–585
	(1327)	(1493)	(1722)	(772)	(815)	(812)
Change in Number of Clients	3119 *	3119 *	3119 *	3119	3119 *	3120 *
	(316)	(316)	(316)	(315)	(316)	(316)
Change in Client Diversity	–657	–657	–658	–658	–658	–659
	(395)	(395)	(395)	(395)	(395)	(395)
Constant	–171 *	–170 *	–164 *	–178 *	–178 *	–177 *
	(70)	(70)	(70)	(70)	(70)	(70)
Model statistic						
N	33,243	33,243	33,243	33,243	33,243	33,243
Firms	1603	1603	1603	1603	1603	1603
T	2 to 35	2 to 35	2 to 35	2 to 35	2 to 35	2 to 35
F-statistic	381 *	381 *	382 *	381 *	380 *	381 *
F degrees of freedom	4, 31,635	4, 31,635	4, 31,635	4, 31,635	4, 31,635	4, 31,635
$\beta_1 = \beta_2$ Restricted hypothesis test *F*-statistic	2	1	1	4 *	3	4 *
$\beta_1 = \beta_2$ Test *F* degrees of freedom	1, 31,635	1, 31,635	1, 31,635	1, 31,635	1, 31,635	1, 31,635

Note: * $p \leqslant 0.05$.

Clients is significant in five of six models, while *Change in Client Diversity* is not significant in any model. Robustness analysis in Online Appendix D shows that these results are not an artifact of multicollinearity. Online Appendix E shows that our results are substantively similar if we use the partisan identities of firms' founders as our focal independent variable.

Difference-in-differences designs for party takeover "treatments"

In order to address questions of causation, we subjected the data to a harder test. If partisan efforts – or some other unobserved endogenous process – caused partisan firms' revenues to increase, then it is not likely that they occurred immediately after a new majority took over. Starting new House-majority-party-aligned firms immediately after a takeover occurs would be costly. We think it is more plausible that any year-over-year changes in party-aligned firms' revenues can be attributed to the immediately perceived value in an in-party-aligned firm. In Online Appendix F, we considered and ruled out the possibility that lobbyists switching to firms with different party loyalties in the aftermath of changing partisan control of a chamber is a significant factor.

We addressed causation by exploiting exogenous changes in House and Senate party leadership as temporal interventions. We used a kernel-weighted difference-in-differences estimator (Hazlett, 2019) to test temporal causality for changes in both institutions, which occurred as a result of separate electoral cycles. We exploited exogenous shocks created by changing control of the House in 2011 (from Democratic to Republican) and Senate in 2015 (from Democratic to Republican). Blanes i Vidal et al. (2012) and de Figueiredo and Richter (2014) recommended the difference-in-differences approach when dealing with panel datasets on lobbying because it effectively addresses persistence issues – the continuation of previous trends despite a changing state of the world, as may be induced when firms keep the same clients that they had the year before – that commonly affect this type of data (Buraimo et al., 2016).

We estimated the causal effect of a lobbying firm being tied to the party gaining control over a chamber of Congress. A firm was "treated" when its aligned party gained control of a chamber. These firms were compared to newly ousted-party firms. We uncovered a positive treatment effect on *Revenue per Lobbyist* when the Tea Party movement helped Republicans regain of the House majority in 2011. Figure 3 plots the values for the 2010–2011 takeover treatment effects. Lobbyists at Republican-aligned firms are estimated to have gained just over US$10,000 for the year.

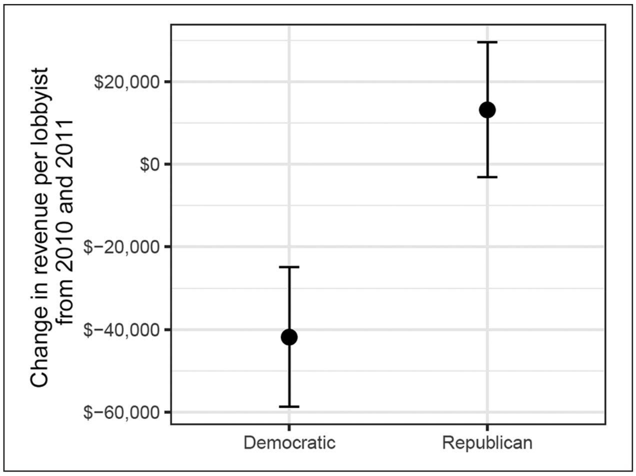

Figure 3. Revenue per lobbyist difference-in-differences for 2010 to 2011.

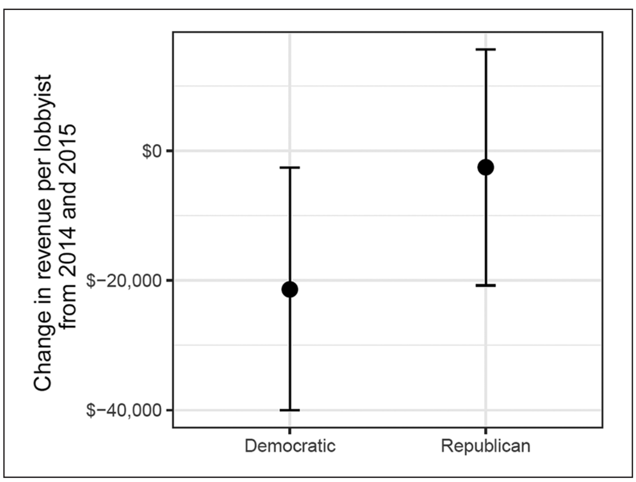

Figure 4. Revenue per lobbyist difference-in-differences for 2014 to 2015.

Ousted-Democratic lobbying firms are estimated to have lost more than US$40,000 in annual revenues, despite still holding the Senate majority. We observed no significant effects after the Republican Party's takeover of the Senate in 2015, as we report in Figure 4. Additional information on the difference-in-differences estimation is in Online Appendix G.

Conclusion

Lobbying firms have relatively stable partisan identities that matter in their ability to attract revenue from interest group clients. Our measures of firm partisan identities are reliable, their validity is supported by their correlation with the partisan identities of firms' founders, and scarcely few lobbyists leave a partisan firm from one party to work for a partisan firm of the opposite party. Partisan ties help to define lobbying firms as institutions.

Partisan ties between lobbying firms and the partisan leadership of the House of Representatives help to boost lobbying firm revenues, with 12 of 13 tests supporting this conclusion, including a difference-in-differences analysis. However, evidence of financial benefits from partisan ties with the partisan leadership of the Senate is more limited, with only 3 of 13 tests favoring this view (though this support would be somewhat higher if we considered the effects of multicollinearity in models 1.4, 1.5, and 1.6). There is some indication that ties to the House are more revenue-enhancing than ties to the Senate, with 5 of 12 tests backing this expectation. Since the outcomes of the 2018 midterm congressional elections led to another switch in partisan control of the House (from Republican to Democrat), these events will present another opportunity to test these hypotheses once the 2019 lobbying data become available.

Our analysis favors the view that partisan ties with the House are a more reliable source of revenue than are ties with the Senate. This difference may be attributed to how differences in congressional staffing by chamber affect the perceived value of lobbying by firms. Nonetheless, there may be viable alternative explanations rooted in the longer electoral cycle, supermajoritian rules, and higher member prominence in the Senate. Moosbrugger's (2012) theory of institutional vulnerability may provide a fruitful starting point for parsing these explanations in future research.

Our findings extend beyond Bertrand et al. (2014: 3915) by demonstrating that partisan identities (and other organizational characteristics) of *firms* matter, as opposed to only the partisan identities of individual lobbyists. Our finding that ties to the *House* leadership have greater reliability for revenues than do ties to the Senate leadership contrasts with Blanes i Vidal et al. (2012), which found a greater value of being connected to the leadership of the Senate than to the leadership of the House (see Cox and Magar, 1999 for a related finding). Future research could explore interactions among lobbying firms, legislative subsidies, chamber leadership, campaign contributions, and fundraising to further extend our understanding of partisan lobbying firms as political institutions.

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Supplemental materials

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Appendix A. Imputation Procedures and Diagnostics

Appendix B. Fixed-Effects Models

Appendix C. Variation in Model Specification to Evaluate the Possible Effects of Multicollinearity on the First Set of Models (1.2 and 1.5)

Appendix D. Variation in Model Specification to Evaluate the Possible Effects of Multicollinearity on the Second Set of Models (2.2 and 2.5)

Appendix E. Models using Partisanship of Founders to Measure Firm Identity

Appendix F. Analysis of Firm Switching by Lobbyists

Appendix G. Difference-in-Differences Estimation

Appendix A. Imputation Procedures and Diagnostics

To address the substantial missing data in our hand-coded variables, we adopted a multiple-imputation approach using Amelia in R (King, Honaker, Joseph, and Scheve 2001; Honaker, King, and Blackwell 2011). Multiple imputation with Amelia relies on the assumption that data are not missing at random. That is, it assumes that the missingness is a function of observed variables, rather than of other missing variables. Because of this assumption, it is standard practice to include more variables in the matrix for imputation than what one will ultimately include in models for estimation. We used *Revenue per Lobbyist* (in 2015 dollars), *Firm Partisan Alignment with House Leadership* at the 85 percent, 90 percent and 95 percent thresholds, *Firm Partisan Alignment with Senate Leadership* at 85 percent, 90 percent and 95 percent thresholds*,* *Law Firm*, *International Office*, *Number of Domestic Offices*, *Firm Age*, *Democratic Campaign Contributions*, *Republican Campaign Contributions,* and *Democratic Share of Total Campaign Contributions*. We ran this imputation process for both time-fixed and time-varying models.

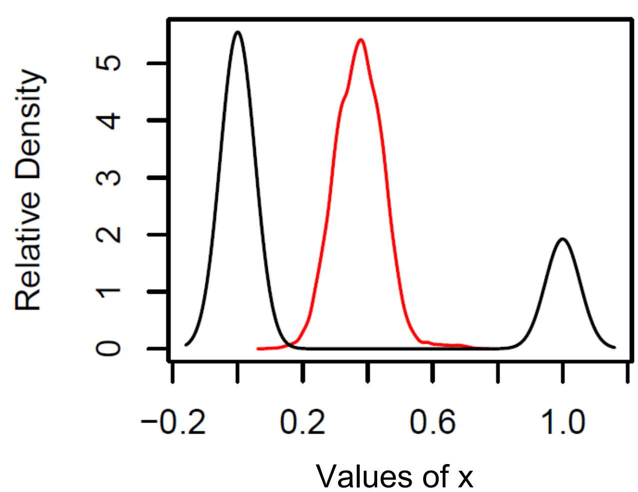
Because imputation assumes multivariate normality, we logged *Number of Lobbyists*, *Number of Clients*, *Number of Domestic Offices*, *Firm Age*, *Democratic Campaign Contributions, and Republican Campaign Contributions*, which were all substantially skewed right. Simple multivariate normal imputation models tend to perform as well as more complicated models, even though multivariate normal distributions may poorly approximate the distributions of mixed data (King, Honaker, Joseph, and Scheve 2001; Schafer 1997; Schafer and Olsen 1998). For the imputation, logical bounds of [0,1] were imposed on all dichotomous variables, while other variables were bounded at their minimums and maximums. As Honaker, King, and Blackwell

(2011) suggest, we allow missing ordinal variables to take continuous values from the imputation, as these estimates more accurately convey the uncertainty of the imputation than would forcing integer values.

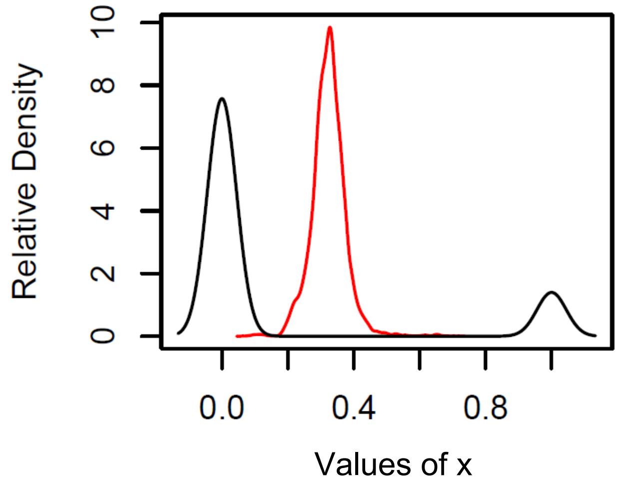
Figures A.1 and A.2 show the distributions of imputed data (red lines) plotted in comparison to the kernel density estimates of distributions for observed variables (black lines) for the year and quarterly panels. We should not expect these densities to match exactly, unless data were missing completely at random. Indeed, the fact that they may not match is the reason to impute values in the first place. For dichotomous variables (e.g., *Law Firm* and *International Office*), the imputed distributions appear between the modes of the kernel density estimates, proportionately closer to the larger modes. For the continuous variables, the imputed distributions appear coterminous to, or slightly to the right of, the kernel density estimates.

Figure A.1. Observed versus Imputed Densities for Model 1.2 with Time-Fixed Identities

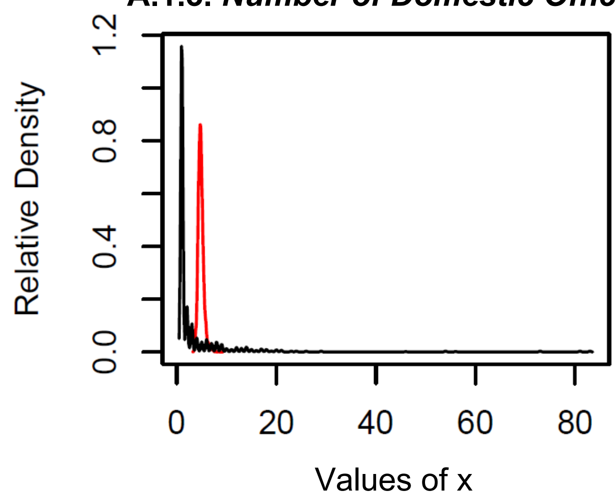
A.1.1. *Law Firm*



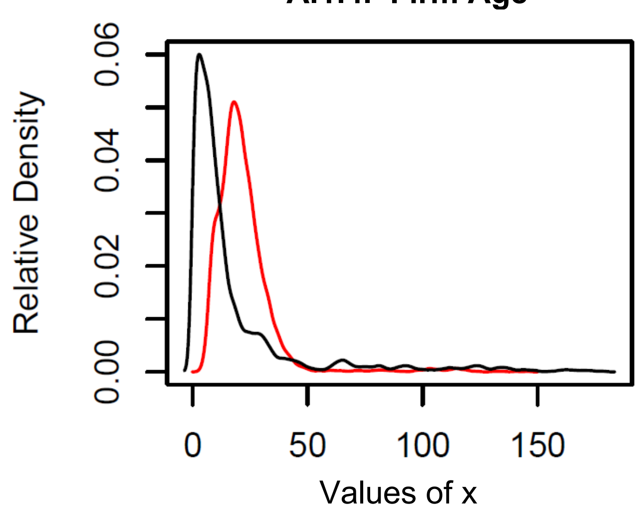
A.1.2. ***International Office***



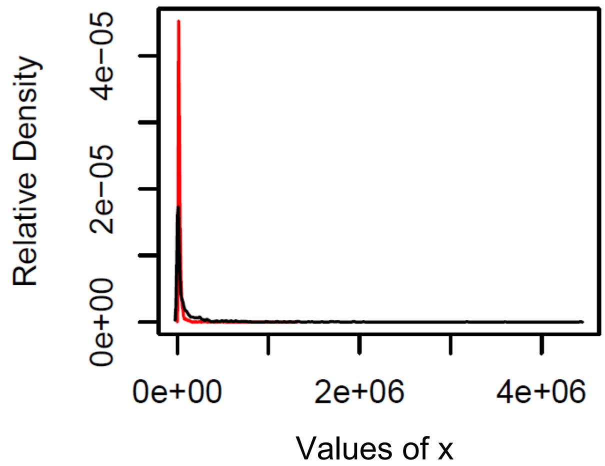
A.1.3. ***Number of Domestic Offices***



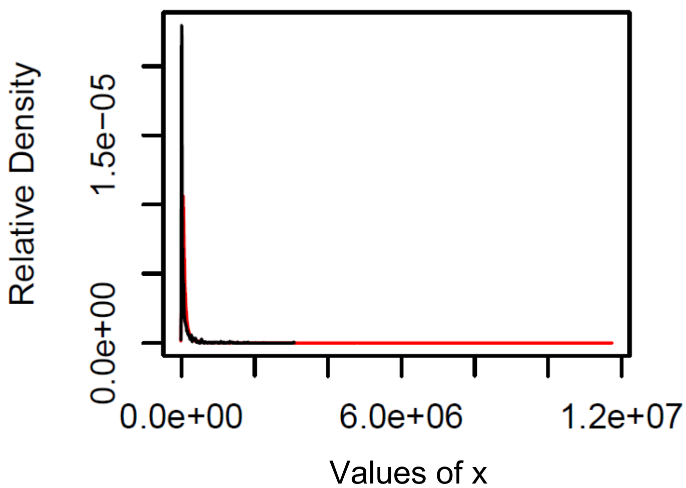
A.1.4. Firm Age



A.1.5. ***Democratic Campaign Contributions***



A.1.6. ***Republican Campaign Contributions***



A.1.7. ***Democratic Share of Total Campaign Contributions***

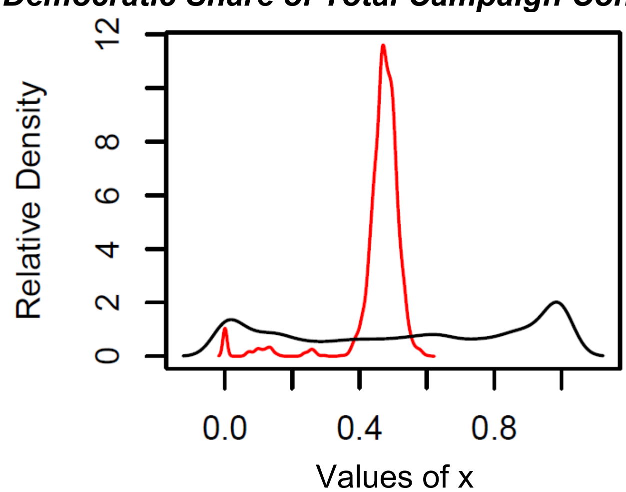
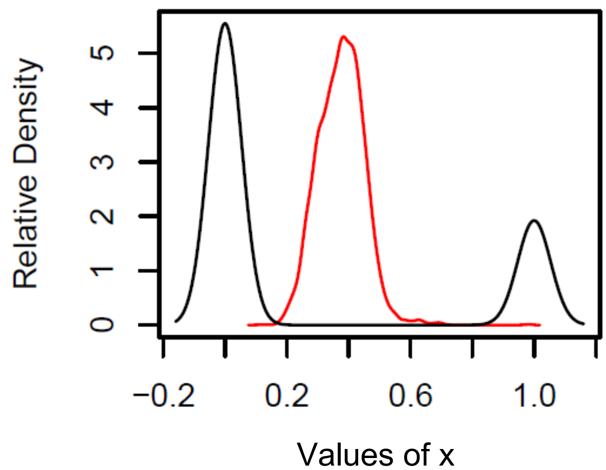
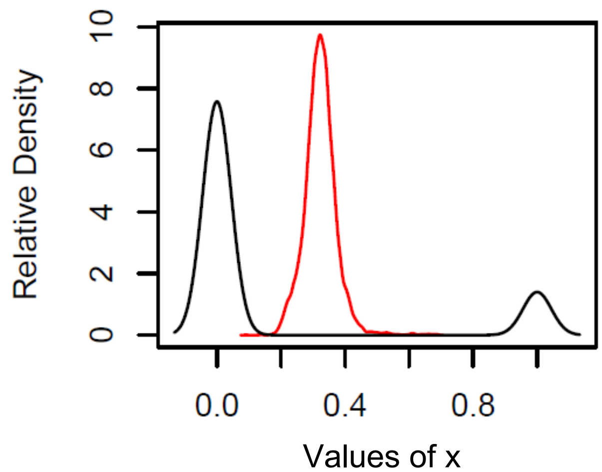


Figure A.2. Observed versus Imputed Densities for Model 1.5 with Time-Varying Identities

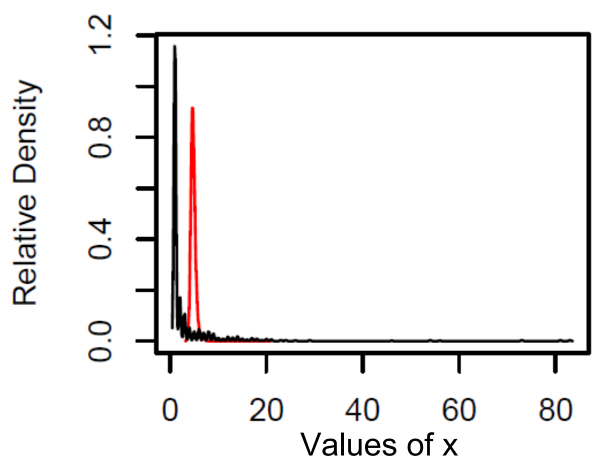
A.2.1. ***Law Firm***



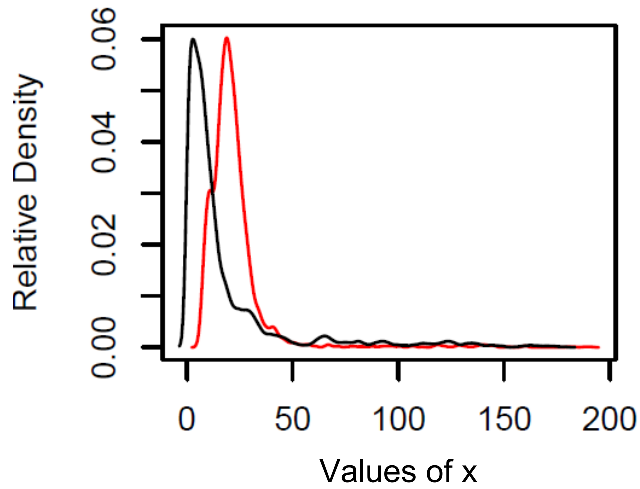
A.2.2. ***International Office***



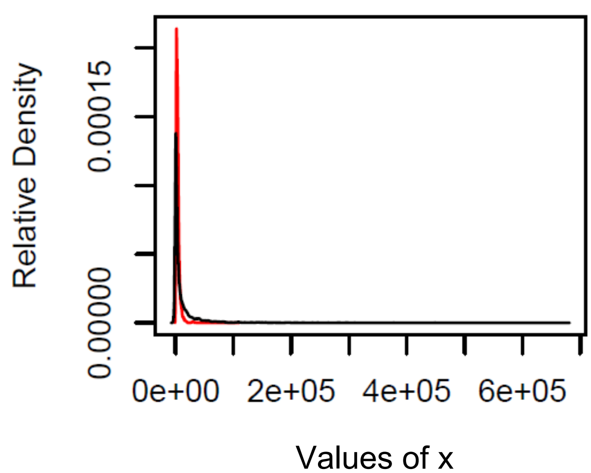
A.2.3. ***Number of Domestic Offices***



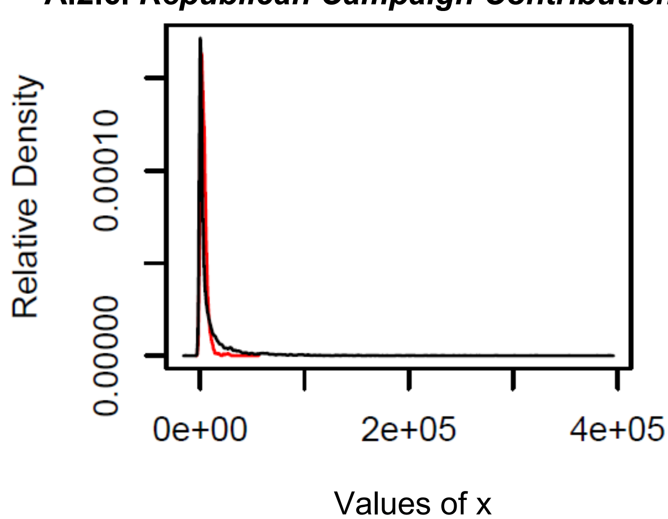
A.2.4. Firm Age



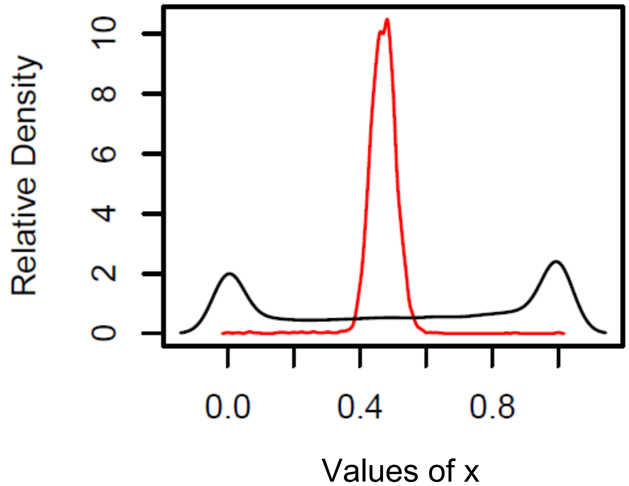
A.2.5. *Democratic Campaign Contributions*



A.2.6. *Republican Campaign Contributions*



A.2.7. *Democratic Share of Total Campaign Contributions*



For both yearly and quarterly panels, we imputed 100 datasets. For each of these, the models were applied to all 100 imputed datasets the estimates and standard errors were combined using Rubin's (1987) rules for combining results.

The paths of the expectation-maximization chains in Figures A.3 and A.4 demonstrate convergence towards the same principal component from dispersed starting values (represented by different color lines). This convergence indicates a well-behaved likelihood function by showing that variations in the starting values do not yield considerable differences in results.

Figure A.3. Paths of the Expectation-Maximization Chains for Model with Time-Fixed Identities

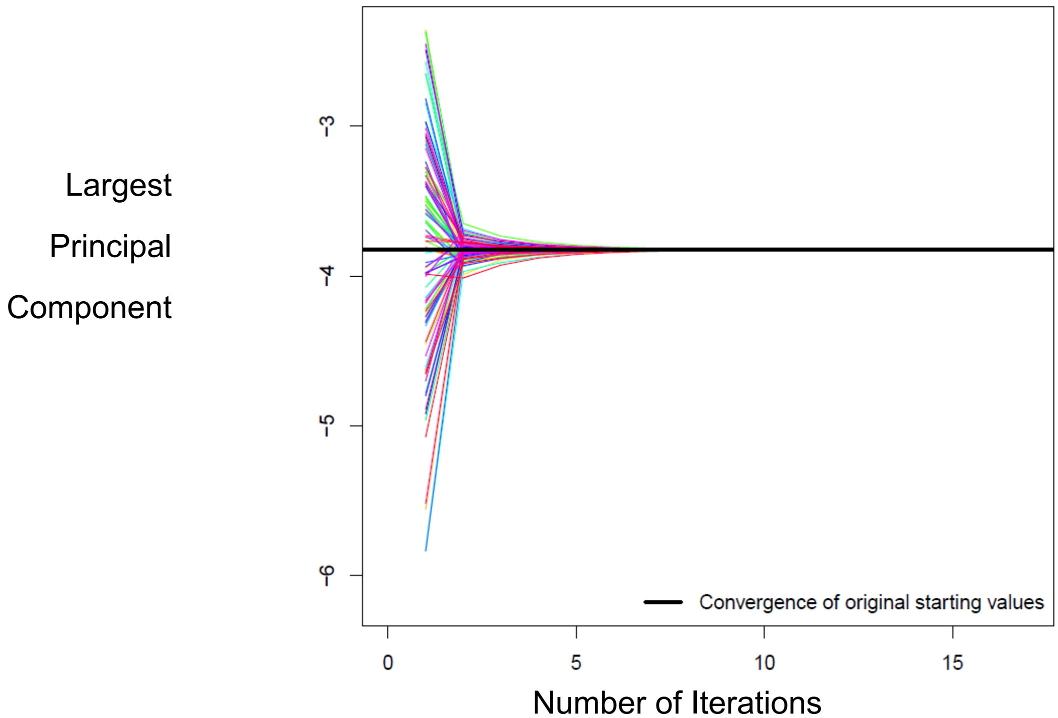
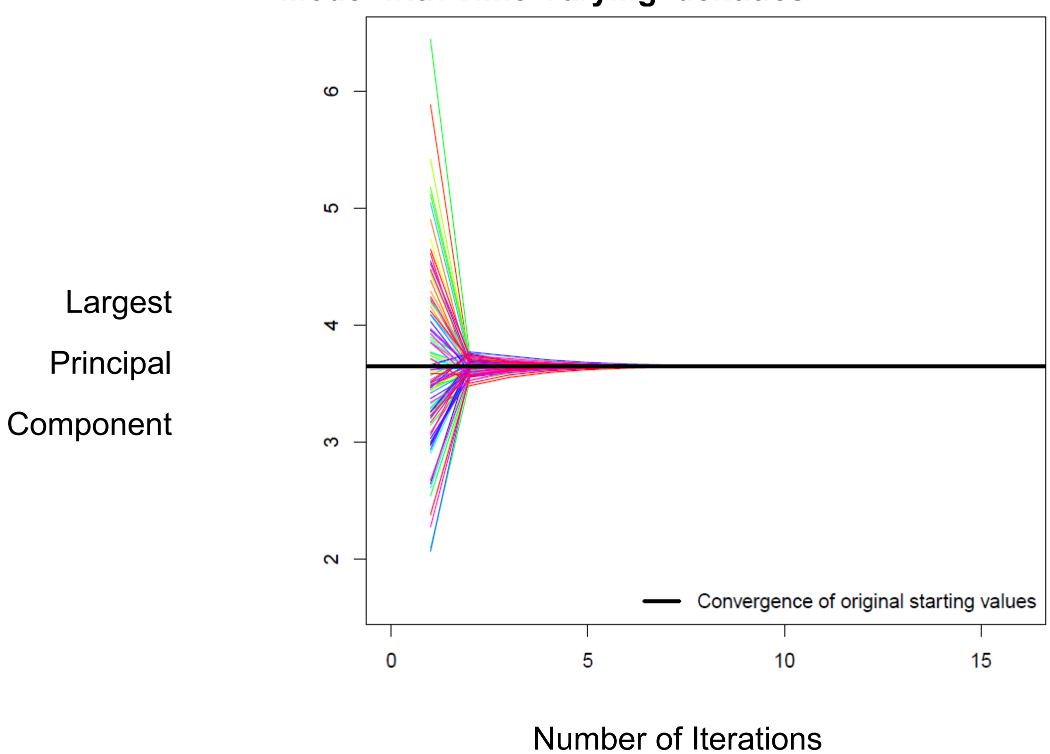


Figure A.4. Paths of the Expectation-Management Chains for Model with Time-Varying Identities



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Appendix B. Fixed-Effects Models

Some readers may be interested in how fixed effects would affect the results reported in Table 2. We used random effects in those models because inclusion of the time-invariant control variables (*Law Firm*, *International Office*, *Number of Domestic Offices*, and *Firm Age*) preclude the use of a fixed-effects estimator. Thus, to respond to result to these concerns, we reported fixed effects versions of models 1.1 through 1.6 in Table B1. The results indicate that there are no substantive differences from the random effects models reported in Table 2. They yield the same conclusions with respect to the focus variables.

Table B1. Firm Revenue Per Lobbyist – Panel Linear Models with Two-Way Fixed Effects

Model	**3.1**	**3.2**	**3.3**	**3.4**	**3.5**	**3.6**
	Time-Fixed Partisan Identities			*Time-Varying Partisan Identities*		
Partisan-Giving Threshold	85%	90%	95%	85%	90%	95%
	Coefficient (Standard Error)					
Independent Variable						
Firm Partisan Alignment with House Leadership	5275 *	6241 *	5837 *	4844 *	5041 *	4632 *
	(1770)	(1988)	(2182)	(1311)	(1357)	(1407)
Firm Partisan Alignment with Senate Leadership	3239 *	4401 *	4193 *	710	353	-394
	(1575)	(1753)	(2026)	(1103)	(1128)	(1170)
Number of Clients	1838 *	1835 *	1839 *	1839 *	1841 *	1839 *
	(249)	(249)	(250)	(250)	(250)	(250)
Client Diversity	1109 *	1102 *	1101 *	1112	1117 *	1118 *
	(327)	(327)	(327)	(329)	(329)	(329)
Model Statistic						
N	33243	33243	33243	33243	33243	33243
Firms	1603	1603	1603	1603	1603	1603
T	2 to 35	2 to 35	2 to 35	2 to 35	2 to 35	2 to 35
F-statistic	719 *	726 *	712 *	718 *	716 *	707 *
F degrees of freedom	4, 31602	4, 31602	4, 31602	4, 31602	4, 31602	4, 31602
$\beta_1 = \beta_2$ Restricted Hypothesis Test F-statistic	3	2	1	18 *	21 *	22 *
$\beta_1 = \beta_2$ Test F degrees of freedom	1, 31602	1, 31602	1, 31602	1, 31602	1, 31602	1, 31602

Note: * $p \leq 0.05$.

Appendix C. Variation in Model Specification to Evaluate the Possible Effects of Multicollinearity on the First Set of Models (1.2 and 1.5)

Some readers may be concerned that conclusions drawn from our first-difference models (reported in Table 2) may be partly an artifact of multicollinearity. When considering the focus variables, for example, it is conceivable that *Firm Partisan Alignment with House Leadership* and *Firm Partisan Alignment with Senate Leadership* are correlated in a way that affects their significance or insignificance. We checked to see if this problem might exist. We did find some of multicollinearity on our results.

The results reported in Table C1 demonstrate the absence of significant effects from multicollinearity on the coefficients for our focus variables in Model 1.2 (with time-fixed partisanship). However, the results show that multicollinearlity suppresses the significance of the coefficient on *Firm Partisan Alignment in Firm Partisan Alignment with Senate Leadership* in Model 1.5 (with time-varying partisanship). Variations on Model 1.4 and Model 1.6 (not reported here) also reveal significance on the Senate variable when the House variable is omitted from the equation. Thus, the conclusions drawn from these models differ from what we reported in the main text of the article, offering somewhat more support for Hypothesis 1 with respect to the Senate.

Table C1. Firm Revenue Per Lobbyist – Panel Linear Models with Firm Random Effects, Temporal Fixed Effects, and Multiple Imputation for Missing Data in Firm Attributes

Model	**4.1**	**4.2**	**4.3**	**4.4**
	Time-Fixed Partisan Identities		*Time-Varying Partisan Identities*	
Partisan-Giving Threshold	90%	90%	90%	90%
	Coefficient (Standard Error)			
Independent Variable				
Firm Partisan Alignment with House Leadership	7644 *		5567 *	
	(2016)		(1315)	
Firm Partisan Alignment with Senate Leadership		6759 *		3122 *
		(1870)		(1150)
Number of Clients	1655 *	1654 *	1653 *	1655 *
	(245)	(245)	(245)	(246)
Client Diversity	1313 *	1310 *	1322 *	1320 *
	(319)	(320)	(321)	(320)
Law Firm	-5621 *	-5580 *	-6137 *	-6133 *
	(1989)	(2000)	(2038)	(2047)
International Office	-1057	-990	-346	-437
	(2264)	(2263)	(2184)	(2186)
Number of Domestic Offices	-298	-289	-320	-307
	(194)	(193)	(205)	(205)
Firm Age	-42	-44	-28	-30
	(25)	(25)	(23)	(23)
Constant	48855 *	49230 *	49322 *	49931 *
	(2791)	(2806)	(2744)	(2755)
Model Statistic				
N	33243	33243	33243	33243
Firms	1603	1603	1603	1603
T	2 to 35	2 to 35	2 to 35	2 to 35
F-statistic	4810 *	4748 *	4805 *	4704 *
F degrees of freedom	41, 33201	41, 33201	41, 33201	41, 33201

Note: * $p \leq 0.05$.

Appendix D. Variation in Model Specification to Evaluate the Possible Effects of Multicollinearity on the Second Set of Models (2.2 and 2.5)

Some readers may be concerned that conclusions drawn from our first-difference models (reported in Table 3) may be partly an artifact of multicollinearity. When considering the focus variables, for example, it is conceivable that *Change in Firm Partisan Alignment with House Leadership* and *Change in Firm Partisan Alignment with Senate Leadership* are correlated in a way that affects their significance or insignificance. We checked to see if this problem might exist. We found that it did not.

The results reported in Table D1 demonstrate the absence of significant effects from multicollinearity on the coefficients for our focus variables in Model 2.2 and Model 2.5. We estimated variations in the models with *Change in Firm Partisan Alignment with House Leadership* and without *Change in Firm Partisan Alignment in Firm Partisan Alignment with Senate Leadership*, and vice versa. The conclusions drawn from these models do not differ from what we reported in the main text of the article.

Table D1. Change in Revenue Per Lobbyist – Panel Linear Models on First Differences (i.e., ΔY on ΔX)

Model	**5.1**	**5.2**	**5.3**	**5.4**
	Time-Fixed Partisan Identities		*Time-Varying Partisan Identities*	
Partisan-Giving Threshold	90%	90%	90%	90%
	Coefficient (Standard Error)			
Independent Variable				
Change in Firm Partisan Alignment with House Leadership	3671 *		1482	
	(1636)		(890)	
Change in Firm Partisan Alignment with Senate Leadership		1983		112
		(1486)		(772)
Change in Number of Clients	3119 *	3121 *	3119 *	3121 *
	(316)	(316)	(316)	(316)
Change in Client Diversity	-657	-657	-658	-658
	(395)	(395)	(395)	(395)
Constant	-173 *	-177 *	-178 *	-180 *
	(70)	(70)	(70)	(70)
Model Statistic				
N	33243	33243	33243	33243
Firms	1603	1603	1603	1603
T	2 to 35	2 to 35	2 to 35	2 to 35
F-statistic	508 *	507 *	507 *	505 *
F degrees of freedom	3, 31636	3, 31636	3, 31636	3, 31636

Note: * $p \leq 0.05$.

Appendix E. Models using Partisanship of Founders to Measure Firm Identity

Table E1. Firm Revenue Per Lobbyist – Panel Linear Models using the Partisanship of Firms' Founders to Measure Partisan Alignment

Model	**6.1**	**6.2**
	Firm Random Effects, Temporal Fixed Effects	*Two-Way Random Effects*
Independent Variable		
Firm Partisan Alignment with House Leadership	4150 *	4057 *
	(1320)	(1323)
Firm Partisan Alignment with Senate Leadership	310	432
	(1093)	(1096)
Number of Clients	1643 *	1825 *
	(247)	(252)
Client Diversity	1330 *	1129 *
	(322)	(330)
Law Firm	-6087 *	
	(2069)	
International Office	-890	
	(2338)	
Number of Domestic Offices	-318	
	(206)	
Firm Age	-46	
	(25)	
Constant	48792 *	
	(2785)	
Model Statistic		
N	33243	33243
Firms	1603	1603
T	2 to 35	2 to 35
F-statistic	4752 *	704 *
F degrees of freedom	46, 33196	46, 33196
$\beta_1 = \beta_2$ Restricted Hypothesis Test F-statistic	18 *	11 *
$\beta_1 = \beta_2$ Test F degrees of freedom	1, 33200	1, 31602

Note: * $p \leq 0.05$.

Appendix F. Analysis of Firm Switching by Lobbyists

Some readers may be concerned that our results may be driven, in part, by strategic lobbyists who move from firm to firm in the aftermath of changing partisan control. If these lobbyists frequently moved from Democratic-leaning firms to Republican-leaning firms (and vice versa) in response to changing chamber control, it could raise questions about the causes behind the results. We examined our data for the possibility of such effects and found their presence to be implausible.

Of the 42,201 lobbyist-firm-years in our dataset from 2008 to 2016 (across 1,593 total firms), we observed that 4,679 lobbyists worked for a different firm in year t and t-1. Slightly more than 10 percent of lobbyists in a given year had recently transitioned from a prior firm. These figures likely *overestimate* the degree of movement between firms since a firm with a name change (or merger) would be counted here as a new firm and, thus, all lobbyists in that firm would appear to have moved. Of these, 2,509 lobbyists have partisan identities using a 90 percent contribution threshold.

Of the 2,519 partisan lobbyists who moved, only 58 lobbyists (2 percent) were partisans leaving a firm with the opposite party identity, and only 57 lobbyists (2 percent) were partisan lobbyists with a destination firm that was of the opposite party identity as their own. Of those 58 leaving firms affiliated the opposite party of their own, 17 moved to new firms that with the same partisan identity as those they left. Only 6 out of 58 moved to firms that aligned with their own identity; the rest moved to bipartisan firms.

When we compare the partisan identity of the firm that these 2,509 partisan lobbyists left with the partisan identity of their new firm, we can see that the majority of lobbyists (65 percent) moved from their firm to another of the same type, as is reported in Table F1. There is a strong and statistically significant tendency in these data for lobbyists to move to the same type of firm ($\chi^2_{(4)}$=651, $p \leq 0.05$). Only 11 lobbyists (less than one half of one percent) in the entire dataset moved from Republican to Democratic firms or vice-versa. The rest of the moves consisted of lobbyists cycling between partisan and bipartisan firms, occurring at an average rate of less than 5 percent of all partisan lobbyists per year.

Table F1. Number of Lobbyists Switching Firms by Party Ties

		New Firm		
		Bipartisan Firm	*Democratic Firm*	*Republican Firm*
	Bipartisan Firm	1228	246	103
Old Firm	*Democratic Firm*	310	258	5
	Republican Firm	192	6	130

Appendix G. Difference-in-Differences Estimation

Identification with a difference-in-differences estimator relies on a parallel-trends assumption. That is, the identification assumes that the average change in the potential outcomes between the treatment and control units between two time periods would be the same, and the difference in the change across the two groups can be attributable to the intervention on the treatment units. Mathematically, this assumption is as follows:

$$E[Y_{i1}(0) - Y_{i0}(0)|D_i = 1] = E[Y_{i1}(0) - Y_{i0}(0)|D_i = 0]$$

where Y_{it} is the observed outcome Y for unit i in time t and D_i is the treatment indicator.

In this article, we estimated the causal effect of a party gaining control over a chamber of Congress for lobbying firms that are aligned with that party. In this case, a firm was "treated" when the party it aligns with gains control of a chamber. These firms were compared to the newly out-party firms. Lobbying firms did not appear to exhibit parallel trends clearly, however, as their fortunes were tied to numerous other factors, such as issue and industry portfolios, and how these factors interact with the legislative agenda.

To address this situation, we used a weighting procedure to allow for the control group to more closely compare to the treatment group. This procedure is similar to a matching or synthetic-control approach. However, the usual calculations of standard errors that account for sampling variation when used on matching estimators do not incorporate the uncertainty from the matching process. Researchers have often used bootstrapped standard errors but, as Abadie and Imbens (2008) note, the extreme lumpiness of the matching process violates the smoothness condition necessary for bootstrapping. As a result, bootstrapped variance and actual variance diverge, making bootstrapped standard errors inappropriate for matching estimators.

Instead, we used a weighting procedure developed by Hazlett (2019), in which none of the unit weights are set to 0. As a result, the estimator satisfies the conditions for the bootstrap to work. Rather than trying to achieve balance on all covariates – such that the treatment and control groups appear identical to each other – Hazlett's approach targets the need for the non-treatment potential outcomes for treated and control groups to be equal to each other. It achieves this goal directly by using a kernel-balancing procedure to derive weights which yield similar multivariate distributions for the covariates of the treated and control groups when applied (Hazlett 2019). This procedure allows for unbiased estimation of the Average Treatment Effect on the Treated with bootstrapped standard errors that appropriately account for the uncertainty introduced by the weighting process. We estimated a kernel-balanced difference-in-differences estimator using Hazlett's procedure.

We estimated a difference-in-differences estimator between Republican and Democratic firms for four year-pairs: 2009-2010, 2010-2011, 2013-2014, and 2014-2015. In each case the Republican firms were considered the "treated" units and the Democratic firms were weighted using the *Revenue per Lobbyist* (in 2015 dollars), *Number of Clients, Client Diversity*, *Law Firm*, *International Office*, *Number of Domestic Offices*, Firm Age, *Number of Lobbyists*, *Total Campaign Finance Contributions*, and a dichotomous variable for whether we found a website for the firm as covariates (*Has a Website*). For each pair of years for which the difference-in-differences estimator was calculated (noted as t=0 and t=1), kernel balancing was used on the units from t=0, and then applied to both t=0 and t=1. Units that were present in t=0 but not t=1 were assigned a *Revenue per Lobbyist* value of 0, as a firm that closes earns no revenue.

The weights from the balancing were then used to calculate a weighted difference-in-means test on the difference in *Revenue per Lobbyist* for each firm between t=0 and t=1 across the treatment and control groups. This test was performed with the R package "weights" (Pasek 2016). Because the kernel balancing used variables that were imputed in the yearly panel, the balancing and weighted difference-of-means test was performed on 100 imputed datasets. The results were combined using Rubin's (1987) rules for combining estimates from multiple imputed datasets.

Hazlett (2019, p. 21) introduces a measure of imbalance based on the L1 norm of the imbalance terms, which he shows can be interpreted as the average gap in the estimated density between the treated and control groups for each observation. In Table G1, we reported this measure of imbalance before and after the weighting for each estimate. In every case, the post-weighting norm was roughly an order of magnitude smaller than the pre-weighting norm.

Table G1. Pre- and Post-Weight Imbalance L1 Norm

Year	2009	2010	2013	2014
Pre-Weight L1	0.02723	0.02913	0.01982	0.02069
Post-Weight L1	0.00076	0.00099	0.00119	0.00131

For every difference-in-differences estimate, we produced a weighted parallel-trend plot shown below. We applied the weights derived from year t=0 (i.e., 2010 for the 2010-2011 difference-in-differences, 2014 for the 2014-2015 difference-in-differences) and reported the weighted-mean *Revenue per Lobbyist* in Table G2.

Table G2. Weighted Mean *Revenue Per Lobbyist*

Year	2009-10	2010-11	2013-14	2014-15
Difference in Mean	14399	55018 *	-3369	18773
Standard Error	(12418)	(16900)	(16492)	(18605)

Note: * $p \leq 0.05$.

Figures G1, G2, G3, and G4 reflect this analysis. In each case, we saw some deviations from the parallel trends in years where we noted institutional change: 2010-2011 when Republicans gained control of the House and, to a lesser extent, 2014-2015 when Republicans gained control of the Senate. However, by and large, the trends tracked each other, excepting these substantively meaningful deviations. This finding lends credence to the validity of the parallel-trends assumption necessary for identification in difference-in-differences. In particular, the parallel trends plot for the 2010-2011 difference-in-differences estimate – identification of which is most critical to testing our hypotheses – is quite good.

Figure G1. Parallel-Trends Analysis for 2009.

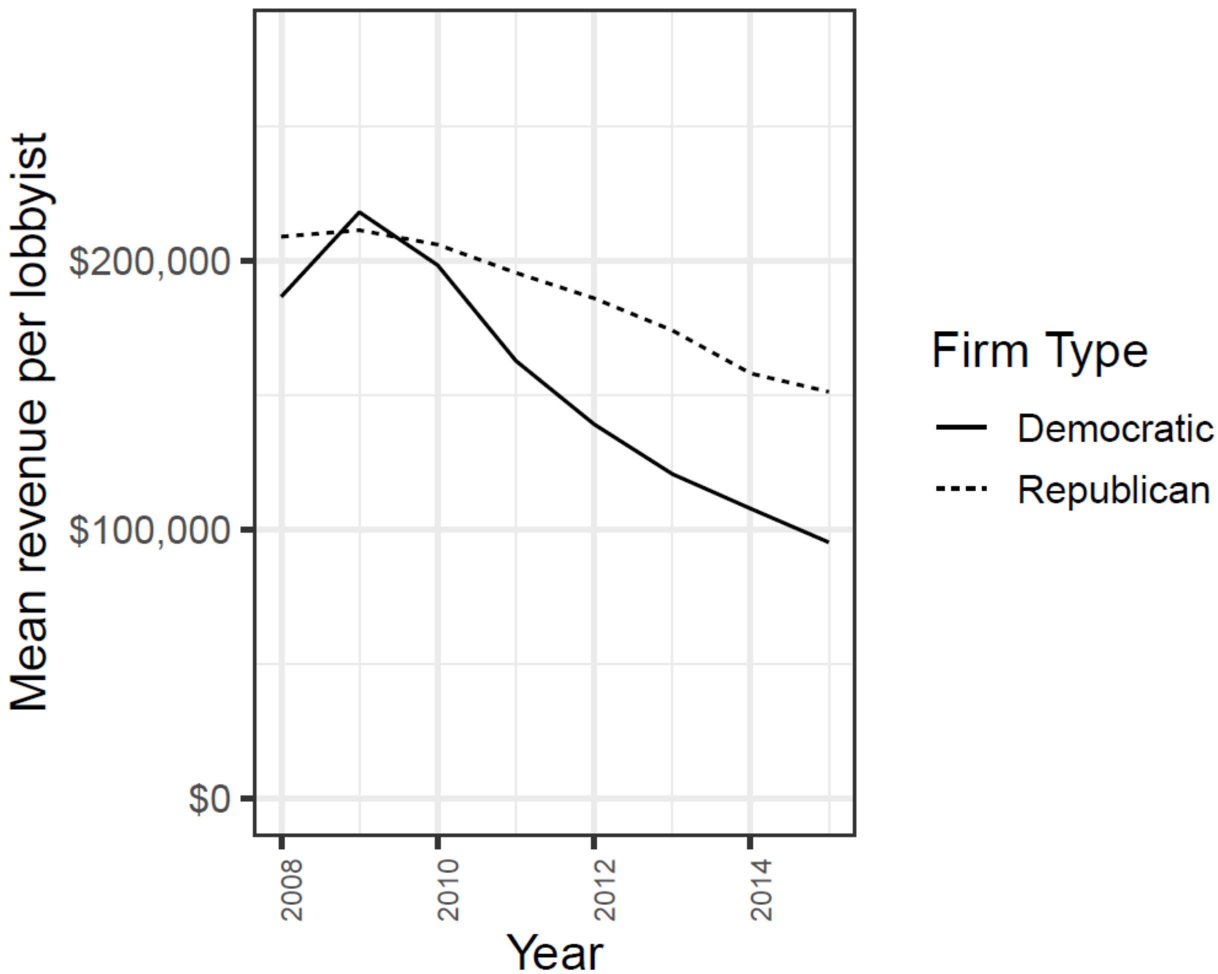


Figure G2. Parallel-Trends Analysis for 2010

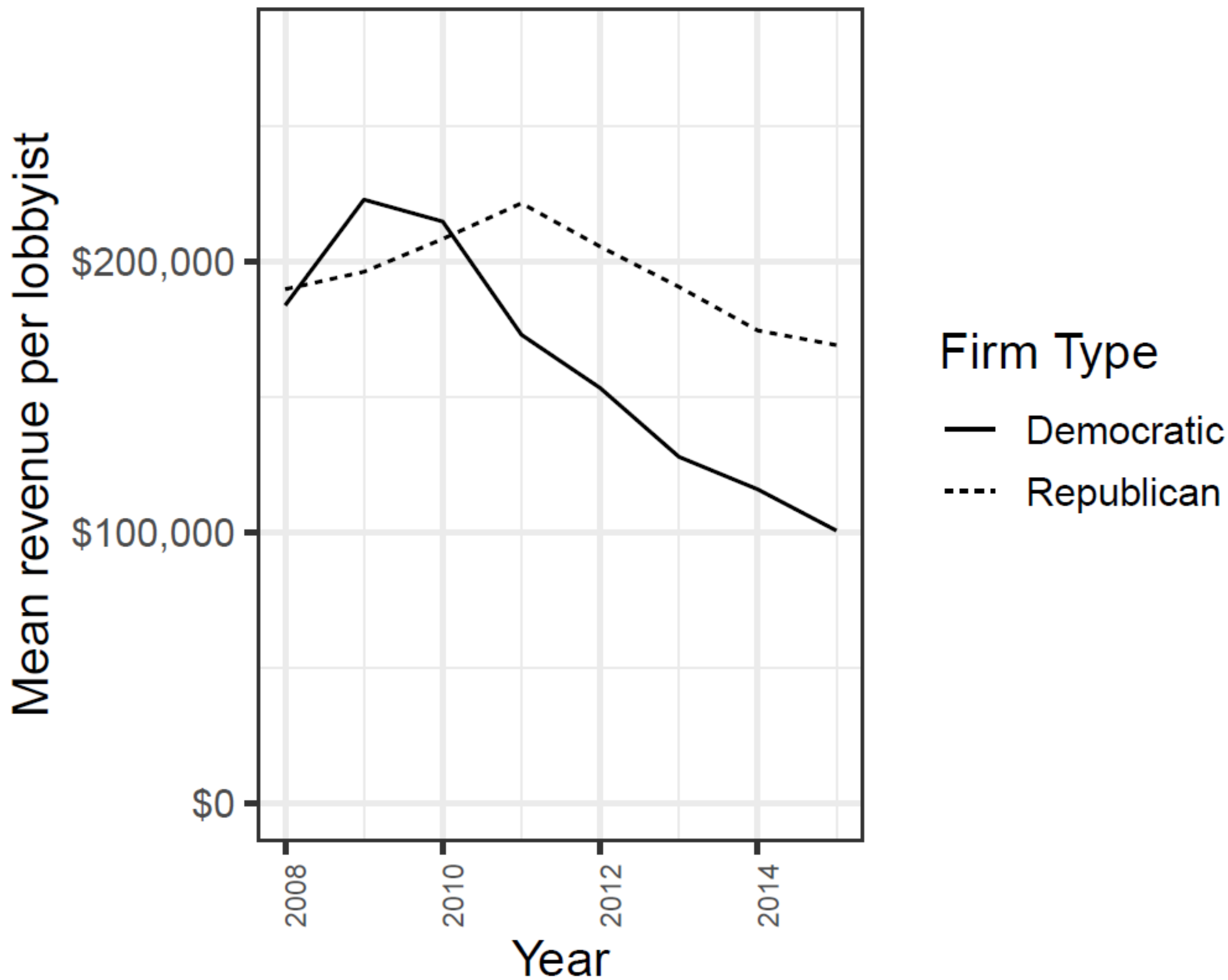


Figure G3. Parallel-Trends Analysis for 2013

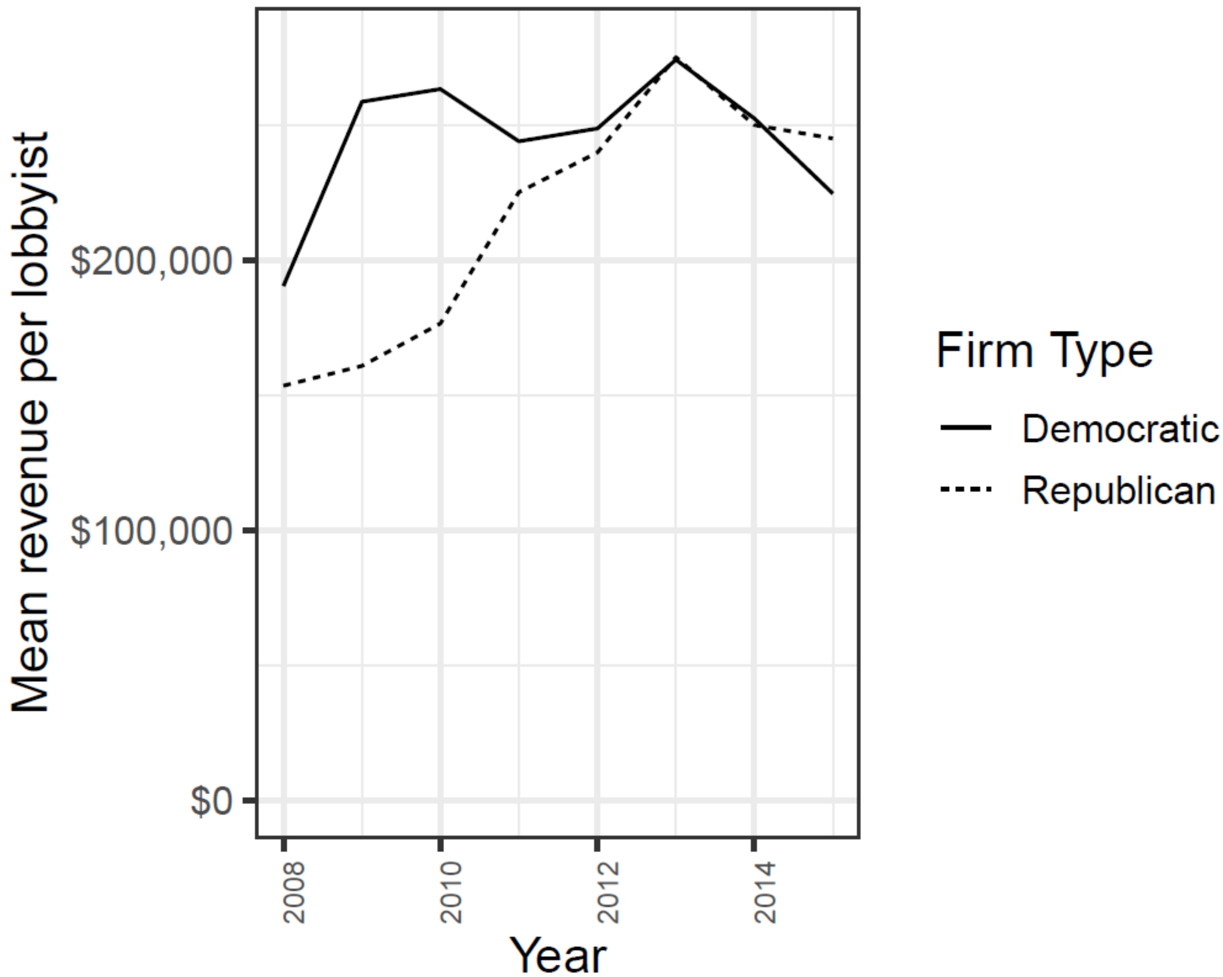
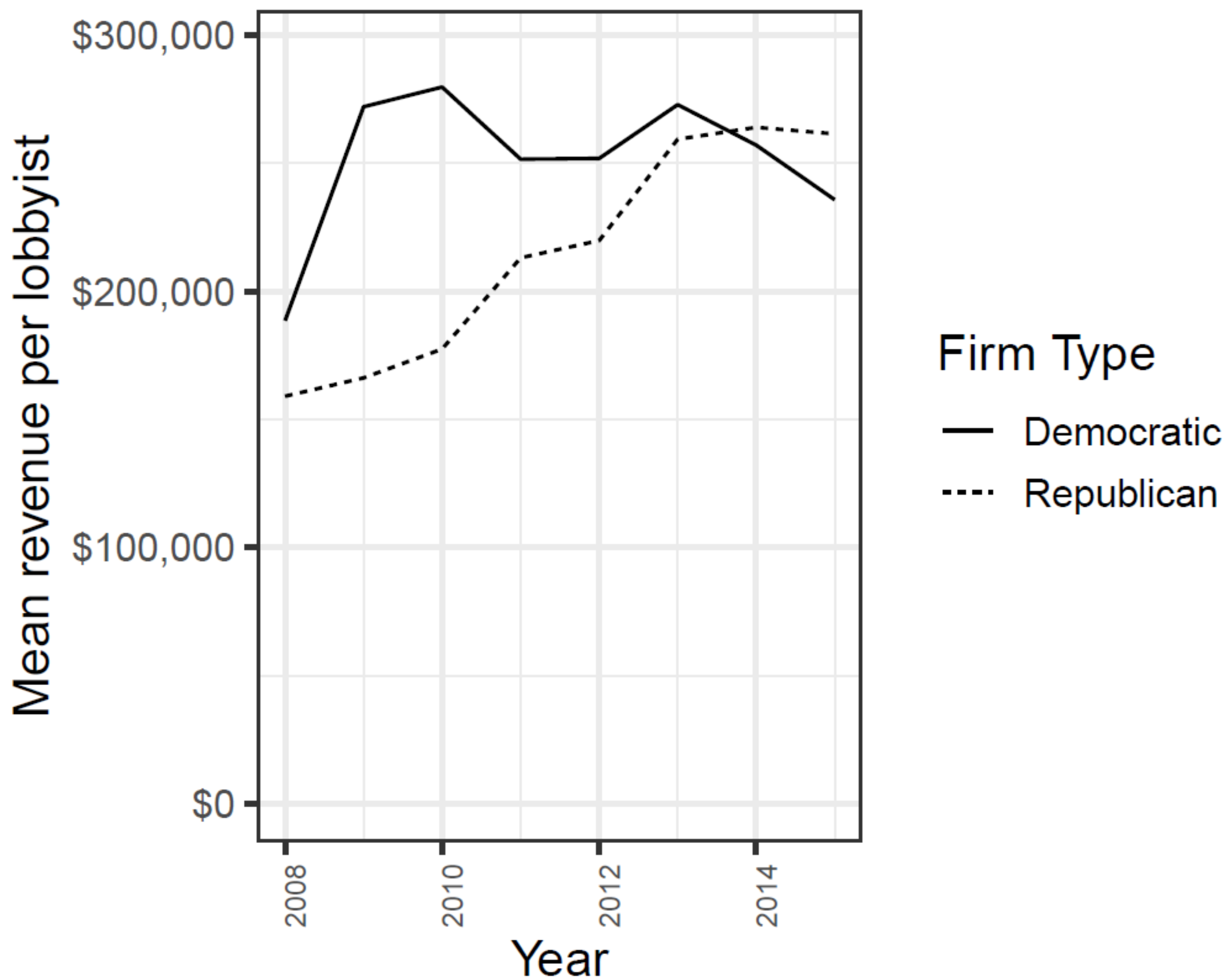


Figure G4. Parallel-Trends Analysis for 2014



References for Appendix G

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