Online Appendix
Lobbyists into Government

Contents

A Data Description and Cleaning Process

As discussed in the manuscript, the majority of our data come from the data aggregator Legistorm. Legistorm acquires publicly available data, some of which we use in the Trump analysis described below, and performs numerous cleaning operations resulting in unique lobbyist, congressional staff, and bureaucrat IDs. These IDs are matched across databases. The most common form of cleaning is rectifying name mismatches, such as in the case "Thomas. H Jefferson" versus "Tom Jefferson". Legistorm uses a manual process of identifying individuals based on their professional and personal backgrounds to ensure these individuals are the same, and provides them with the same unique ID if so.

We also use Legistorm's version of publicly available House and Senate disbursements data, which detail expenditures by congressional offices and committees. From this data, we take congressional staff employment histories, on which Legistorm also performs a name rectifying cleaning process. These names are also matched to the database of lobbying data. From the disbursement data we also use staff salaries and information on the office in which they work for supplemental analyses described below.

Finally, for matching lobbyists to backgrounds in the federal government beyond congressional employment, we rely on two features of the data. First, Legistorm manually searches for individuals' backgrounds when they first register as lobbyists and adds them to their record in the data. These backgrounds include previous employment. They then track these registered lobbyists information especially when they show up in public employment information, such as publicly available government agency payrolls. We also rely on our own manual checks of individuals with listed backgrounds in federal government that comes after their lobbying disclosures. However, one limitation to these data is the lack of individuals who work in national security agency positions, whose names are restricted in public payroll information.

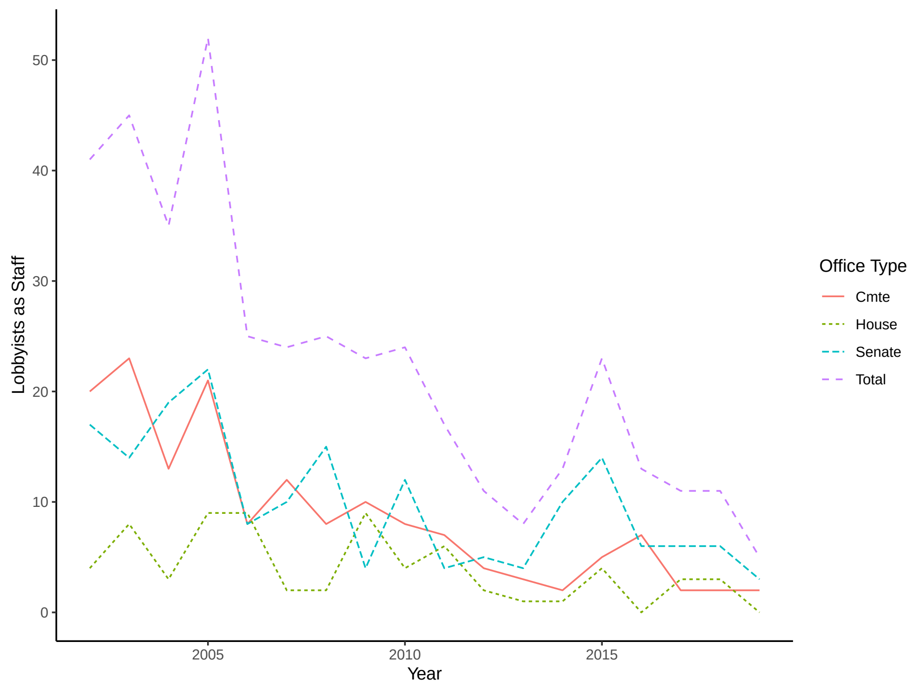

Figure A.1: Lobbyist Destinations among Congressional Staff.

A.1 Additional Descriptives

In Figures A.1 and A.2 we present complementary descriptives to those in the manuscript around the destinations of lobbyists who enter government over time. The former figure displays destination by congressional office type, either committees, a House member, or a Senate member. In general we see that the Senate and committees are more common destinations. In the latter figure, we show differences by party for those who enter into House or Senate options. There are not particular differences in trends over time, with some spikes by party depending on who is in the majority.

B Two-Way Fixed Effects Bias

In the main text, we show the intuition behind the bias arising in two-way fixed effects models with time-varing effects. Here, we briefly reproduce the counterfactuals in Goodman-Bacon (2018) and visualize the difference-in-differences structure of different treatment timings for

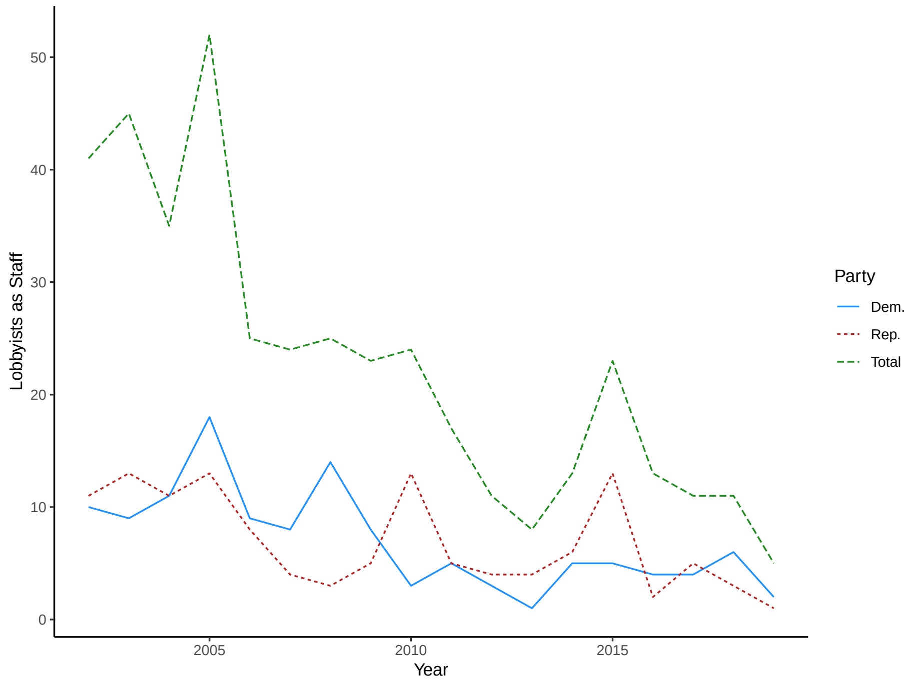

Figure A.2: Destinations by Party among Congressional Staff.

Table A.1: Committees with 5 or more firm lobbyists

Committee	Total Lobbyists
House Energy and Commerce Committee	14
House Ways and Means Committee	11
Senate Indian Affairs Committee	11
House Appropriations Committee	10
House Transportation and Infrastructure Committee	10
House Homeland Security Committee	8
Senate Commerce, Science and Transportation Committee	8
House Natural Resources Committee	7
House Science, Space and Technology Committee	7
Senate Finance Committee	7
Senate Judiciary Committee	7
House Education and Labor Committee	6
Senate Agriculture, Nutrition and Forestry Committee	6
Senate Homeland Security and Governmental Affairs Committee	6
House Financial Services Committee	5

Table A.2: Descriptives of personal offices lobbyists join (averages)

Year	Female	Vote Pct.	Ideol. Extremity	Power Cmte Member	Cmte Chair	Seniority	Democrat	Total
2001	0.19	64.43	0.39	0.44	0.07	5.39	0.49	120
2002	0.27	65.33	0.40	0.47	0.00	4.93	0.47	15
2003	0.26	63.89	0.37	0.37	0.07	4.41	0.33	27
2004	0.35	62.87	0.28	0.59	0.12	5.53	0.65	17
2005	0.25	61.74	0.34	0.47	0.12	5.00	0.50	32
2006	0.25	62.56	0.36	0.69	0.12	6.62	0.44	16
2007	0.26	60.78	0.31	0.43	0.09	4.00	0.74	23
2008	0.11	59.00	0.32	0.11	0.22	4.56	0.67	9
2009	0.22	59.56	0.31	0.44	0.11	4.89	0.78	18
2010	0.14	69.83	0.43	0.43	0.14	6.14	0.71	8
2011	0.16	55.95	0.43	0.26	0.05	2.37	0.16	19
2012	0.00	55.33	0.37	0.67	0.17	9.83	0.50	6
2013	0.33	54.62	0.40	0.22	0.00	2.44	0.44	9
2014	0.14	64.57	0.54	0.43	0.00	7.29	0.43	7
2015	0.11	63.33	0.46	0.50	0.11	4.89	0.22	18
2016	0.29	77.86	0.37	0.43	0.14	5.43	0.57	7
2017	0.10	59.12	0.36	0.50	0.40	4.60	0.40	10
2018	0.33	57.56	0.37	0.33	0.00	5.22	0.67	9
2019	0.14	67.00	0.39	0.29	0.00	5.71	0.71	7
Total	0.21	62.69	0.37	0.44	0.09	5.04	0.50	377

Table A.3: Comparison of revolving lobbyists to non-revolving lobbyists within firms

	Non-Revolvers	Revolvers
Revenue per Contract	64,811	59,140
Total Clients	8.3	6.3
Total Revenue	836,249	592,761
Total Contracts	15.1	9.7
Revenue per Client	92,687	77,983

three stylized groups in Figure B.3.

The bias is induced through this strategy because already-treated firms enter the control group for those that are contemporaneously treated. Following Goodman-Bacon (2018), we can see why these problems arise by considering a stylized data generating process. Assume that we have three groups of lobbying firms: One that is never treated[26], one that is treated early, and one that is treated late.

The difference-in-differences in Panels A and B compare early and late treated firms, respectively, to clean controls (i.e. those that are never treated). In Panel C, the difference-in-difference arises from a comparison of early treated firms to the late treated firms in the period *before* the latter are treated. This all yields unproblematic estimates of the ATTs. Finally, however, in Panel D, firms that are treated late are compared to the trends of early treated firms *while they are still treated*. This makes it clear that if there is any change in the ATT over time, the comparison in Panel D will yield a biased estimate—even if the assumption of parallel trends holds.

Using potential outcomes notation, Goodman-Bacon (2018) shows how the four 2x2 difference-in-differences shown visually in the main text uncover three ATTs. Additionally, this allows us to see how the TWFE estimator produces a weighted average of them. Let the post(.), mid(.) and pre(.) operators denote which before-after comparison we are making. Below, we show how each of the differences-in-differences imply making three different counterfactuals, producing three different ATTs.

The first ATT arises from the before-after comparison of trends between never-treated firms and firms treated in the post-treatment window of group k. This yields the familiar result from a 2x2 differences-in-differences. The first term denotes the ATT for firms gaining a connection in the post-treatment period of timing group k. The second term is the bias arising from counterfactual trends that do not evolve in parallel (parallel trends, PT).

[26]Without changing the implications, we can also think of this group as always-treated. More generally, because the TWFE estimator demeans the data, firms whose treatment status does not change will be treated the same—no matter whether they are always or never under treatment. This highlights a problem present in many panel data applications TWFE models.

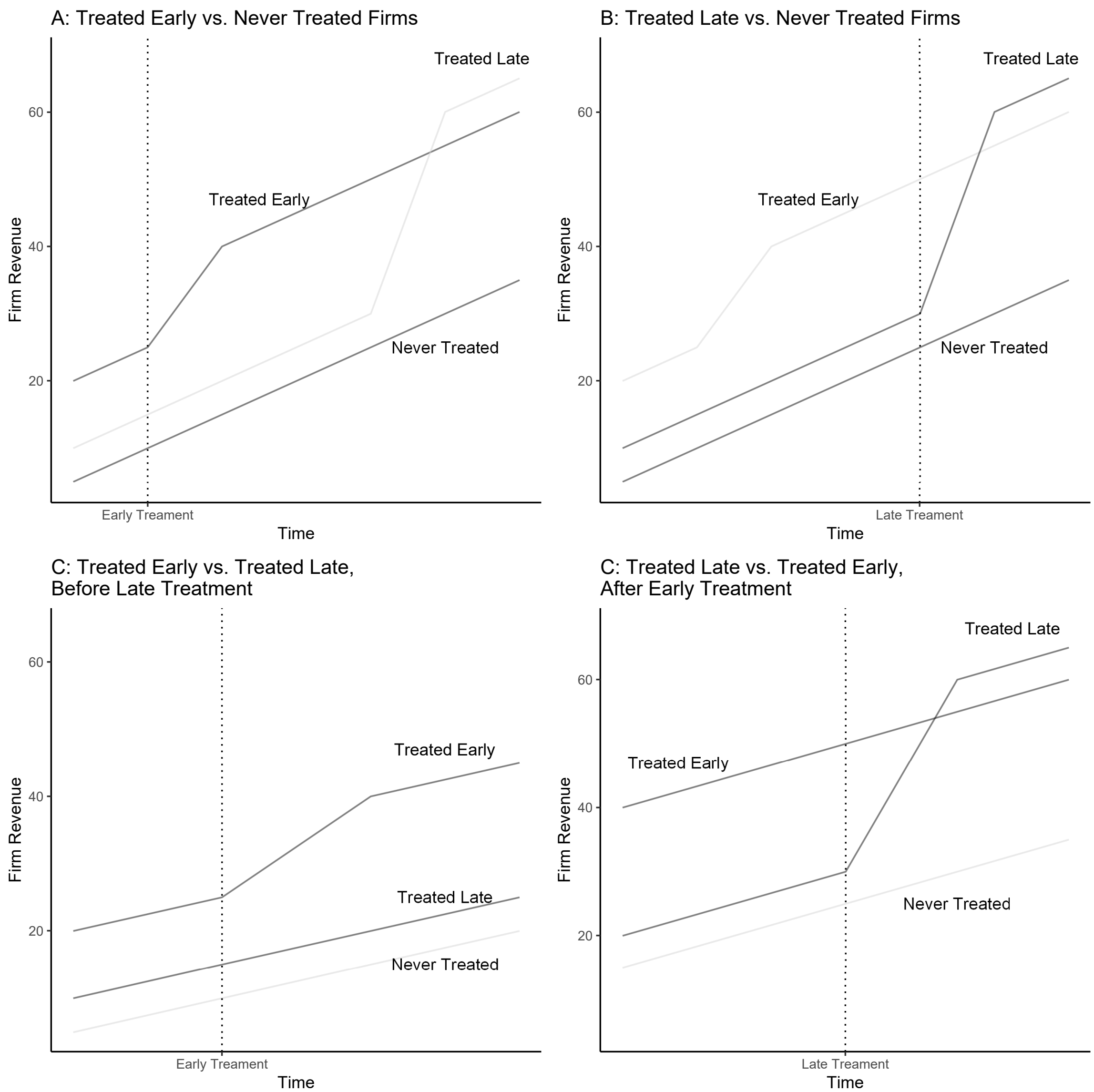

Figure B.3: Four Differences-in-Differences From Three Treatment Groups. *Note: Based on Figure 2 in Goodman-Bacon (2018). Group that is not part of comparison is lightest gray.*

$$\delta_{k,U}^{2x2} = \overbrace{[\Delta Y_k^1(post(k), pre(k)) - \Delta Y_U^0(post(k), pre(k))]}^{\text{ATT(post(k))}} +$$
$$\overbrace{[\Delta Y_k^0(post(k), pre(k)) - \Delta Y_U^0(post(k), pre(k))]}^{\text{PT(post(k,U))}}$$

Second, we have the ATT estimated by comparing the changes in revenue of early-treated firms to the not-yet-treated trends of the group of firms receiving treatment late. Again, this is an unproblematic comparison, as it yields the ATT plus any potential bias from a violation of the parallel trends assumption.

$$\delta_{k,l}^{2x2,k} = ATT(mid(k,l)) + PT(mid(k,l))$$

Finally, we have the group of firms receiving treatment *within the post-treatment window of the treated-early group*. Here, we obtain the familiar ATT and parallel trends terms. However, since we compare newly treated firms to the already treated group, the estimate will also contain the change in ATT between timing groups k and l.

$$\delta_{k,l}^{2x2,l} = ATT(post(l)) + PT(post(k,l)) -$$
$$[ATT_k(post(l)) - ATT_k(mid(k,l))]$$

Figure B.3 shows how this forms four simple differences-in-differences, each comparing two groups (2x2 differences-in-differences). We draw on Goodman-Bacon (2018) to show that the difference in treatment timing, even in this more general setting, results in biased TWFE estimates whenever already-treated firms are in the control group for newly treated firms. This will be the case in most real-world applications of TWFE where treatment timing is staggered over the period of study.

Goodman-Bacon (2018) shows that in the probability limit (with and increasing N and fixed T), the TWFE estimator can be decomposed into:

$$plim\ \delta^{TWFE} = \delta^{TWFE} = vwATT + vwPT - \Delta vwATT \tag{2}$$

Thus, the TWFE estimator yields a weighted average of all 2x2 difference-in-differences, where the weight is given by the size and variance of the treatment group. This has three implications.

First, the ATT identified by TWFE places higher weight on the ATTs of larger, high-variance groups—it uncovers a variance-weighted ATT (vwATT). Second, as always, the ATT will be biased in the presence of violations of the parallel trends assumption—denoted vwPT in the equation.[27] Most importantly, however, the change in variance weighted treatment effects ($\Delta vwATT$) is subtracted. This 'Goodman-Bacon' bias implies that *any estimator that compares newly treated to already-treated firms (like TWFE) will be biased in the presence of time-varying effects.*

Importantly, this stylized example with three timing groups generalizes to our case with 154 timing groups.

B.1 How the Bias Materializes

Next, we show that the TWFE estimator is very likely to yield biased – and even wrongly signed – estimates in this setting.

In Figure B.4, we show that treatment effects are highly variable over the time-period that we study. We do so by estimating the effects of becoming connected within in each semester in our dataset. It is clear, that effects are extremely variable, and change in a cyclical pattern. This extreme variability implies that a standard panel data approach would

[27] Again, it is weighted so violations in certain comparisons have more influence.

yield highly biased results.

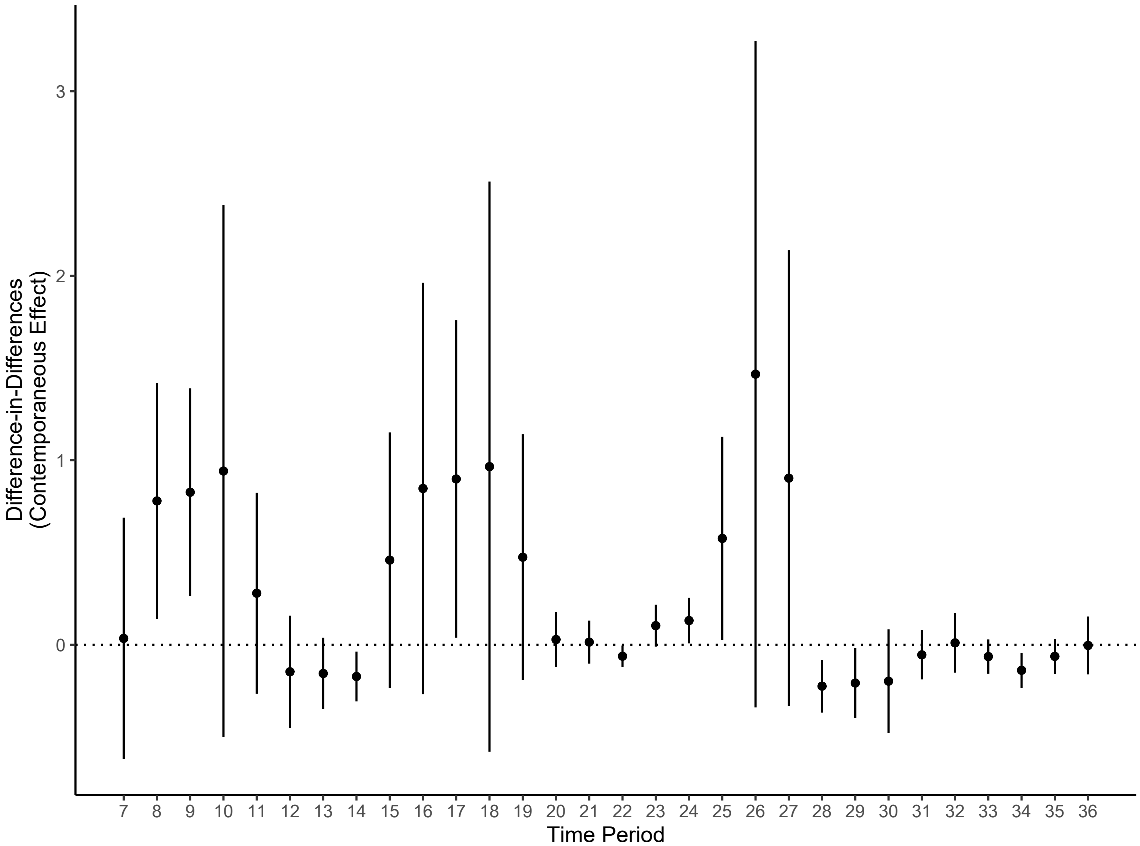

Figure B.4: How ATTs Vary Over Time. *Note: The graph shows a rolling window analysis, where the baseline matched difference-in-differences is estimated on a subset of the time-periods in the data. To allow for the baseline lag and lead lengths, six periods prior to treatment and five post-treatment periods are included.*

This suggests that the Imai et al. (2021) method is much more appropriate than TWFE. What would be the consequence of using TWFE or another standard panel data approach? In Figure B.5, we delve into this by showing that TWFE yields very different results. Particularly, the TWFE estimates suggest that gaining a connection would lead to a very significant reduction in revenue in all periods after treatment. Importantly, as Goodman-Bacon (2018) points out, if the variability of the ATTs over time is large enough, the TWFE bias can be so large that we will experience sign-reversal. That seems to be the case here.

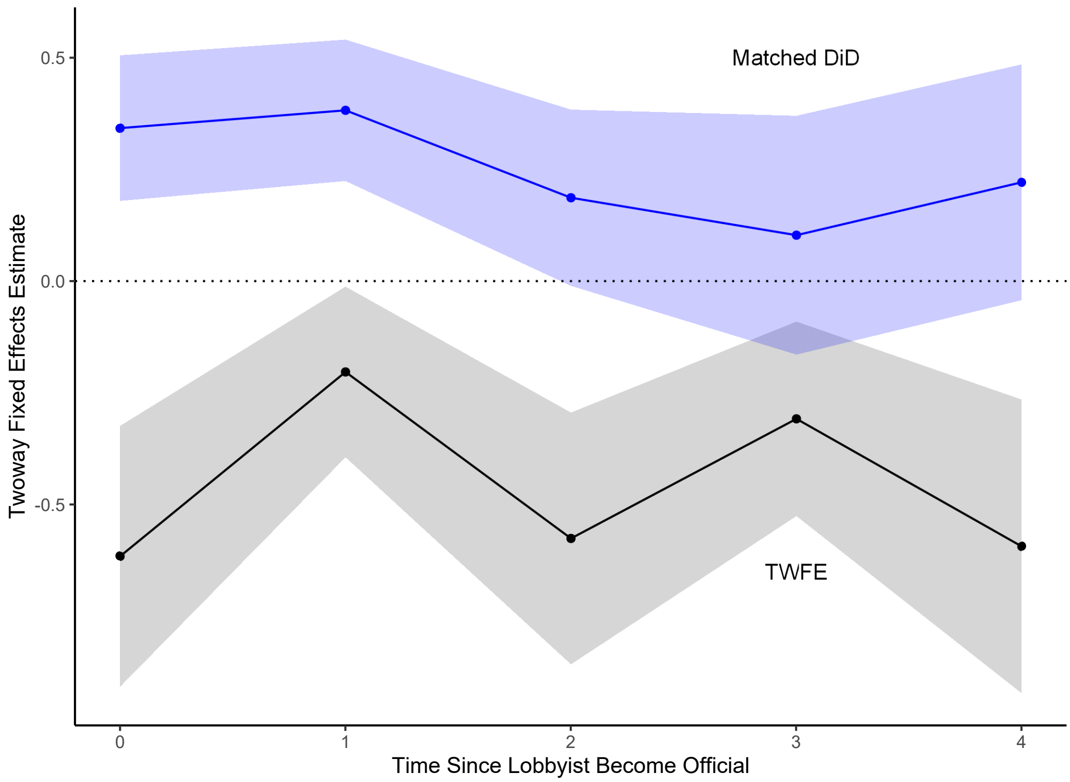

Figure B.5: Event-Study Twoway Fixed Effects Estimates. *Note: Gray-shaded estimates are from a regression with fixed effects for firm and time. Estimates are produced with dummy variables for relative event-time. The event is the first transition of a lobbyist into public service. Blue-shaded estimates are from the Imai-Kim-Wang differences-in-differences estimator. Shaded areas are 84% confidence intervals from firm-clustered robust standard errors. This corresponds to statistical significance at the 5% level when the two confidence intervals do not overlap.*

C Details on Matched Difference-in-Differences Estimation

C.1 How Many Firms are in the Control Groups?

We show this in Figure C.6, which illustrates that the lowest number of firms in a control is 1. Importantly, the median number of control firms is 762. This implies we have a high number of control firms to estimate our counterfactuals on.

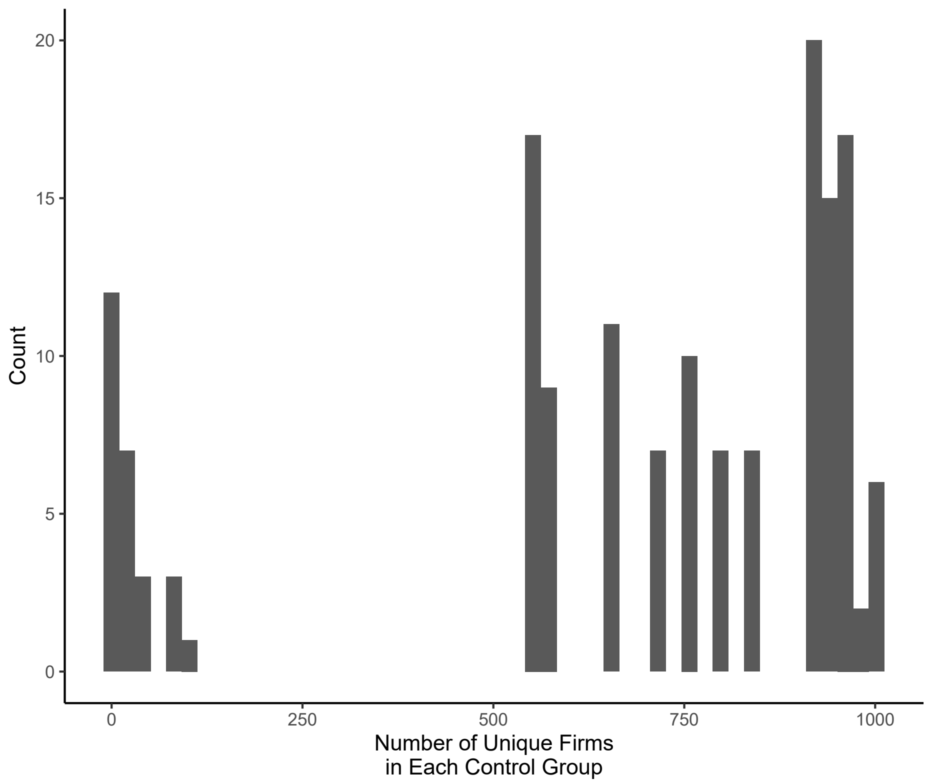

Figure C.6: Number of Control Firms.

C.2 No Pre-Trends in Covariates or Revenue

Next we investigate whether there are differential trends between treatment and control firms prior to treatment. Figure C.7 illustrates this by plotting the difference between treatment and control firms on log revenue, filings and lobbyists prior to treatment. We show both the pre-trends before and after adjusting for the covariates.

As we can see, there is no strong evidence of differential trends prior to treatment on either variable. However, matching the firms on the covariates in the pre-treatment windows has the effect of making the trends more stable and reducing the difference between treatment and control firms.

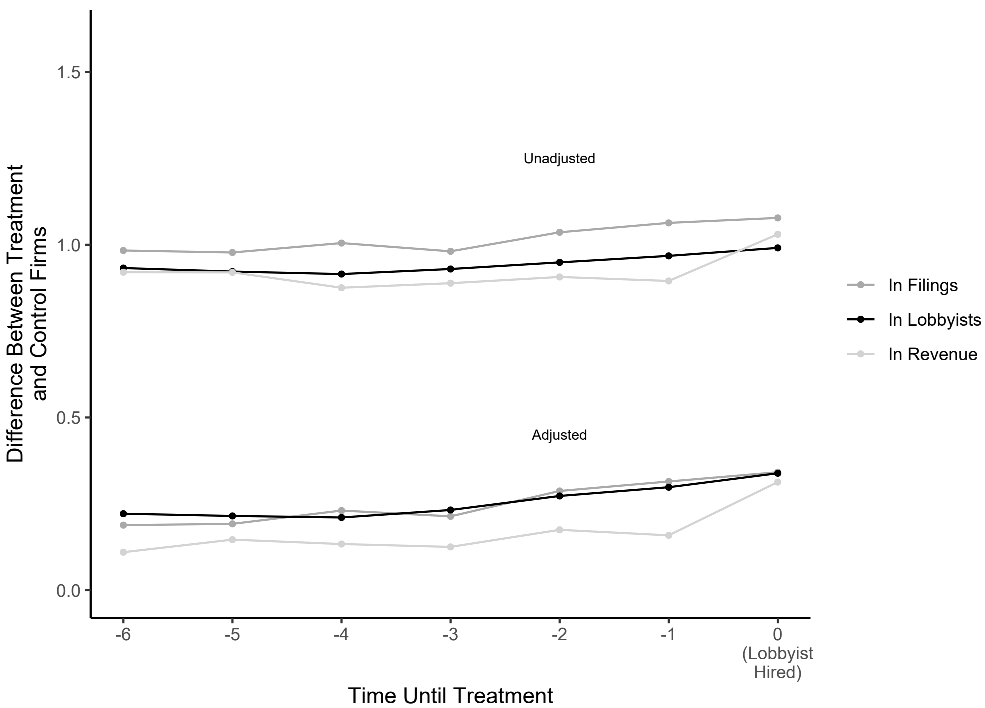

Figure C.7: No Differential Trends Prior to Treatment. *Note: The graph shows the trends in covariates and log revenue in the period leading up to treatment. Adjustments are conducted using the Mahalanobis distance between treatment and control firms.*

D Robustness

D.1 Choice of Pre-Treatment Window

As discussed in the manuscript, a feature of the Imai et al. (2021) estimator is that it requires creating a window of lagged periods for constructing appropriate control groups. Figure D.8 demonstrates the robustness of our results to different lag windows. As this figure shows, the results are highly robust in terms of substantive interpretation and statistical significance. The only exception is some noise in the lobbyist-turned-bureaucrats results, which aligns with the noise in the top-line estimates. However, many of the lag windows remain statistically significant.

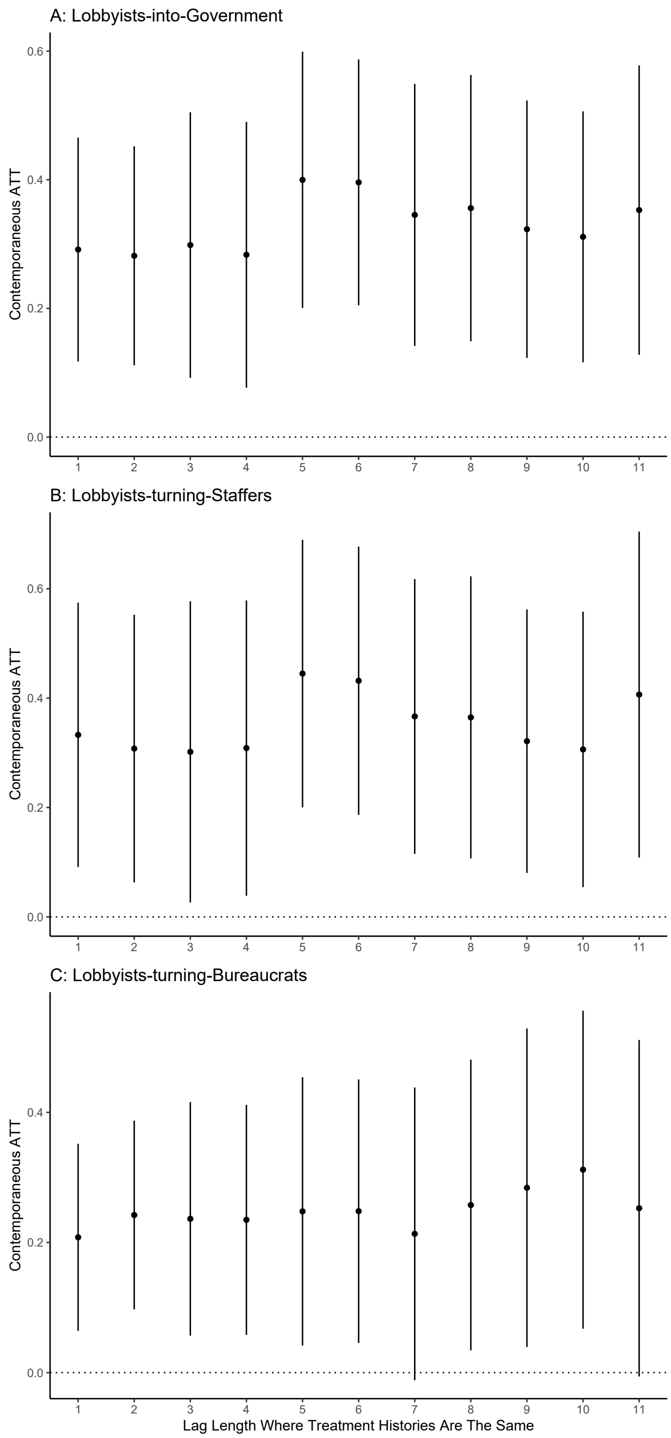

Figure D.8: Robustness to Varying Lag Lengths. *Note: Each point is the estimated contemporaneous ATT with a different lag window where treatment histories are constrained to be the same in the treatment and control groups.*

D.2 Committee vs. Personal Staff

In Table D.4 we re-run the congressional staff connections models presented in the manuscript. However, we run separate models for the type of staff connection gained by the lobbying firm: committee staff or personal office staff. There are reasons to suspect heterogeneity in the results. In previous research, committee staff turned lobbyists have been linked to different types of lobbying activity associated with deeper specialization (Bertrand et al. 2014; McCrain 2018). However, they have also been associated with lower revenues when they revolve. Here, despite splitting the treatment, we find similar results as the primary finding: a substantial initial increase in revenue to the firms that gain these connections despite staff type. The results are stronger and more persistent among firms that gaining committee staff connections.

Table D.4: Lobbyists as Committee Staff

	Dependent variable:				
	ln Revenue				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: Lobbyist as Committee Staff					
Lobbyist Becomes Committee Staffer	0.385**	0.364**	0.289	0.256	0.422*
	(0.177)	(0.167)	(0.182)	(0.232)	(0.250)
Panel B: Lobbyist as Personal Office Staff					
Lobbyist Becomes Personal Office Staffer	0.088	0.191**	−0.010	−0.050	0.137
	(0.099)	(0.096)	(0.156)	(0.195)	(0.167)
Treatment Events	58	58	58	58	58
Control Firms	48,613	48,613	48,613	48,613	48,613
Unique Controls	2,154	2,154	2,154	2,154	2,154

Notes: Estimates are the averages of all possible 2x2 differences-in-differences where control groups include firms with identical treatment histories prior to each respective treatment event. Future-treated firms and treatment-switchers are never included. Weighted firm-blocked bootstrapped standard errors in parentheses. 1,000 trials used. *p<0.1; **p<0.05; ***p<0.01

D.3 Changes in the Number of Lobbyists and Filings

Next, we analyze the relationship between a lobbying firm gaining connections through losing employees to government service and change in the number of lobbyists that firm employs. The idea behind this test is that lobbying firms may compensate for losing employees to government by additional hiring, which as a result increases firm-wide revenue. In the main text we show that the number of clients and number of individual filings stays constant. In Table D.5, we show that there is no evidence for an increase in number of lobbyists hired. Though there is a positive and statistically significant coefficient in the third semester ($t+2$) after losing a lobbyist, that washes out in the following semester. These results are additional evidence that the revenue increase to lobbying firms is driven by the gain in connections and not changes to hiring patterns.

Table D.5: Lobbyists Turning Government Employee and Change in Number of Lobbyists

	Dependent variable:				
	Change in Number of Lobbyists				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: Unadjusted Estimates					
Lobbyist Becomes Government Employee	−0.074	−0.087	0.492**	−0.453**	0.115
	(0.176)	(0.143)	(0.203)	(0.182)	(0.160)
Panel B: Adjusted Estimates					
Lobbyist Becomes Government Employee	−0.144	−0.061	0.397*	−0.434**	0.003
	(0.187)	(0.146)	(0.209)	(0.187)	(0.166)
Treatment Events	123	123	123	123	123
Control Firms	85,125	85,125	85,125	85,125	85,125
Unique Controls	2190	2190	2190	2190	2190

Notes: Estimates are the averages of all possible 2x2 differences-in-differences where control groups include firms with identical treatment histories prior to each respective treatment event. Future-treated firms and treatment-switchers are never included. In Panel B, adjustments to the control group are made by matching firms using the Mahalanobis distance calculated on logged firm revenue, logged number of contracts and logged number of lobbyists, all calculated in the pre-treatment windows. Weighted firm-blocked bootstrapped standard errors in parentheses. 1,000 trials used. *p<0.1; **p<0.05; ***p<0.01

Table D.6 reports similar results to Table 3 in the manuscript. Instead of number of clients, however, we use individual filings. Due to the Lobbying Disclosure Act, these figures are identical in the pre-2007 period, where filings were reported semesterly. However, since we aggregate quarterly filings to semesterly filings in the post-2007 period, there can be differences since a firm may only report lobbying activity in one quarter and not both quarters of a semester. Regardless, results are almost identical to what we find when using number of clients and revenue per client.

Table D.6: Lobbyists Turning Government Employee and ln Number of Filings

	Dependent variable:				
	ln Clients				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: ln Number of Filings					
Lobbyist Becomes Government Employee	0.019	0.006	0.019	−0.004	0.030
	(0.026)	(0.030)	(0.034)	(0.041)	(0.047)
Panel B: ln(Revenue / Filing					
Lobbyist Becomes Government Employee	0.323***	0.375***	0.168	0.106	0.191
	(0.111)	(0.108)	(0.125)	(0.165)	(0.164)
Treatment Events	142	142	142	142	142
Control Firms	103,373	103,373	103,373	103,373	103,373
Unique Controls	2334	2334	2334	2334	2334

Notes: Estimates are the averages of all possible 2x2 differences-in-differences where control groups include firms with identical treatment histories prior to each respective treatment event. Future-treated firms and treatment-switchers are never included. Weighted firm-blocked bootstrapped standard errors in parentheses. 1,000 trials used. *p<0.1; **p<0.05; ***p<0.01

D.4 Before and After HLOGA

The Honest Leadership and Open Government Act (HLOGA) changed the regulatory environment facing people considering to move from government to private sector employment. Most notably, the HLOGA imposed some ethics concerns regulating relations between public officials and lobbyists, introduced a cooling off period for non-elected public servants before they can register as lobbyists, and extended the cooling off period for senators. While the HLOGA did not regulate the movement into government, we cannot in advance preclude the possibility that it imposed general equilibrium changes affecting lobbyists entering public service (Cain and Drutman 2014).

In Table D.7, we estimate separate models before and after the HLOGA. While the effects do seem larger before the act was introduced, the estimates are sizable afterwards, too.

Finally, we note that the passage of HLOGA of 2007 closely coincided with the Democrats gaining unified control of government in late 2008, and the Obama administration introducing a number of ethics rules aimed at curtailing private influence in the executive branch (Crabtree 2010). Therefore, it is difficult to ascribe changes in estimates to the passage of HLOGA.

Table D.7: Effects Before and After the Passage of HLOGA

	Dependent variable:				
	ln Revenue				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: Before HLOGA					
Lobbyist Becomes Government Employee	0.541**	0.478**	0.422*	−0.157	−0.309
	(0.245)	(0.217)	(0.237)	(0.378)	(0.468)
Treatment Events	45	45	45	45	45
Control Firms	22,203	22,203	22,203	22,203	22,203
Unique Controls	973	973	973	973	973
Panel B: After HLOGA					
Lobbyist Becomes Government Employee	0.124	0.191**	−0.109	−0.025	0.302***
	(0.097)	(0.097)	(0.155)	(0.158)	(0.109)
Treatment Events	64	64	64	64	64
Control Firms	55,025	55,025	55,025	55,025	55,025
Unique Controls	1,659	1,659	1,659	1,659	1,659

Notes: Estimates are the averages of all possible 2x2 differences-in-differences where control groups include firms with identical treatment histories prior to each respective treatment event. Future-treated firms and treatment-switchers are never included. In Panel B, adjustments to the control group are made by matching firms using the Mahalanobis distance calculated on logged firm revenue, logged number of contracts and logged number of lobbyists, all calculated in the pre-treatment windows. Weighted bootstrapped standard errors in parentheses. 1,000 trials used. *p<0.1; **p<0.05; ***p<0.01

D.5 Lobbyist Leaving Government Service

Next, we investigate whether firms lose revenue, when their connection leaves office. In Table D.8 we run models similar to the baseline specifications, but count firms as treated when they *lose* a connection. That is, we estimate the difference-in-differences around the time when their former lobbyist leaves government service. As we can see, there is no effect of such events on lobby firm revenue.

There are a number of reasons why this result is not surprising. First, we find in the main models that the effect of gaining a connection dissipates over time. Hence, if the effect of gaining a connection has disappeared, there may not be a strong reason to expect a negative effect of losing a connection. Second, if the lobbying firm used their lobbyist as a bridgehead to build relationships with other people in government, then the new networks will remain even after the former lobbyist leaves government. Third, we have shown that many lobbyists return to their former firm after their stint in government. The ones who do this will bring their connections to government officials with them as an asset. If that is the case, we would not expect a decrease in revenue.

Table D.8: Former Lobbyist Leaving Government Job and Lobby Firm Revenue

	Dependent variable:				
	ln Revenue				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: Unadjusted Estimates					
Lobby Firm Loses Connection	−0.056	−0.033	−0.020	−0.016	0.093
	(0.081)	(0.114)	(0.111)	(0.116)	(0.117)
Panel B: Adjusted Estimates					
Lobby Firm Loses Connection	−0.006	−0.025	0.016	−0.024	0.043
	(0.080)	(0.114)	(0.110)	(0.116)	(0.116)
Treatment Events	215	215	215	215	215
Control Firms	190,038	190,038	190,038	190,038	190,038
Unique Controls	2444	2444	2444	2444	2444

Notes: Estimates are the averages of all possible 2x2 differences-in-differences where control groups include firms with identical treatment histories prior to each respective treatment event. Future-treated firms and treatment-switchers are never included. In Panel B, adjustments to the control group are made by matching firms using the Mahalanobis distance calculated on logged firm revenue, logged number of contracts and logged number of lobbyists, all calculated in the pre-treatment windows. Weighted bootstrapped standard errors in parentheses. 1,000 trials used. *p<0.1; **p<0.05; ***p<0.01

D.6 Effects Depending on Partisanship

An important alternative explanation is that firms may gain in revenue, when their partisanship matches that of a new incoming administration or a majority in Congress. If so, our results would be driven by ideology-based connections rather than the entry of a lobbyist into government. Table D.9 shows the effects split out on parties, and suggest there there is no large difference depending on partisanship.

Table D.9: Effects Conditional on Partisanship of Connection

	Dependent variable:				
	ln Revenue				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: Democrats					
Lobby Firm Gains Connctions	0.290	0.347	0.396*	0.565***	0.609**
	(0.253)	(0.234)	(0.239)	(0.218)	(0.240)
Treatment Events	35	35	35	35	35
Control Firms	26,317	26,317	26,317	26,317	26,317
Unique Controls	2,192	2,192	2,192	2,192	2,192
Panel B: Republicans and Cmte Staff					
Lobby Firm gains Connection	0.368**	0.347**	0.115	0.070	0.131
	(0.148)	(0.146)	(0.206)	(0.276)	(0.266)
Treatment Events	75	75	75	75	75
Control Firms	54,857	54,857	54,857	54,857	54,857
Unique Controls	2,234	2,234	2,234	2,234	2,234

Notes: Estimates are the averages of all possible 2x2 differences-in-differences where control groups include firms with identical treatment histories prior to each respective treatment event. Future-treated firms and treatment-switchers are never included. Weighted bootstrapped standard errors in parentheses. 1,000 trials used. *p<0.1; **p<0.05; ***p<0.01

More importantly, Figure D.9 investigates whether the effect of a Democrat or Republican connection varies over time. In particular, we should be concerned if the effect of gaining a connection to a party is fully driven by periods after they enter the administration or gain a congressional majority. As we can see, there is no strong evidence to suggest that this is the case. Actually, it turns out that estimates of the return to Democrat and Republic connections are correlated: In periods where the return to a Democratic connection is high, so is the return to a Republican connection.

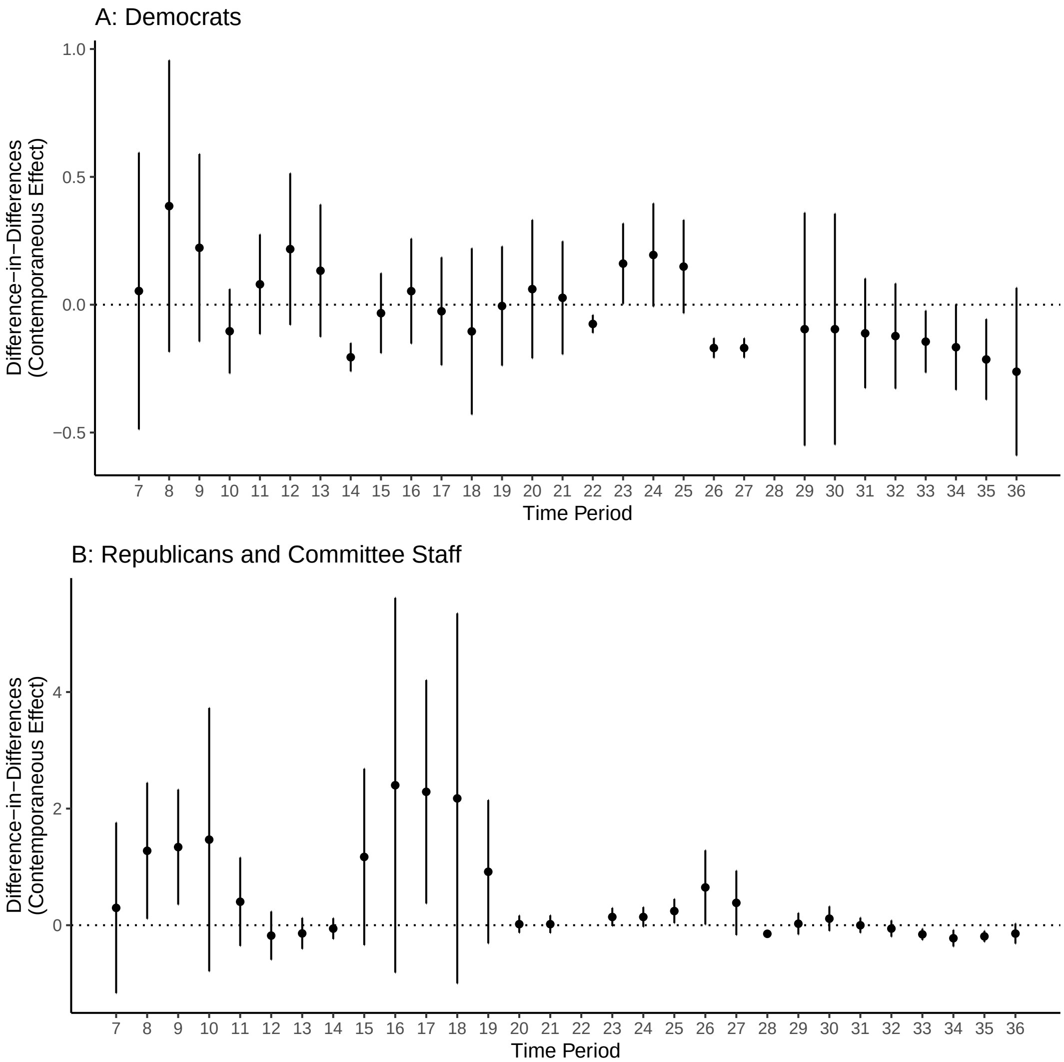

Figure D.9: Time-Varying Effects Depending on Party. *Note: The graph shows a rolling window analysis, where the baseline matched difference-in-differences is estimated on a subset of the time-periods in the data, separately for Democrats and Republicans and committee staff. To allow for the baseline lag and lead lengths, six periods prior to treatment and five post-treatment periods are included.*

D.7 Effects Depending on Unified and Divided Government

In this appendix, we examine whether returns to certain types of connections differ depending on whether control of government is unified or divided, and whether.

In Table D.10, we split our treatment indicator into two separate variables. One indicating movements of lobbyists into government during unified control, the other capturing movements during divided control.

Interestingly, the results suggest that returns to a bureaucratic connection may be higher during periods of divided control. On the other hand, returns to a staff connections ae larger during periods of unified government. This makes sense, because Congress will be gridlocked during those periods, increasing the amount of policy-making being done in federal agencies.

Next, Table D.11 shows the results from treatment events split on connections made to the majority and minority party. While the estimates are noisy for connections made to the majority party, they suggest that there are returns to both types of connections. While the estimated returns to connections to the minority party are more precise (and statistically significant), they do seem to be smaller than connections to the majority party. This conclusion is somewhat speculative, because the estimated returns to majority party connections are imprecise.

Table D.10: Lobbying Revenue and Unified versus Divided Government

	Dependent variable:				
	ln Revenue				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: Full Sample (Unified)					
Lobbyist Revolves (Unified Govt.)	0.362**	0.338**	0.099	−0.206	−0.350
	(0.148)	(0.132)	(0.191)	(0.289)	(0.284)
Panel B: Full Sample (Divided)					
Lobbyist Revolves (Divided Govt.)	0.219	0.415**	0.295*	0.358**	0.539***
	(0.196)	(0.177)	(0.165)	(0.171)	(0.182)
Panel C: Bureaucrats (Unified)					
Lobbyist becomes Bureaucrat (Unified Govt.)	0.052	0.234**	0.062	−0.610	−0.620
	(0.079)	(0.113)	(0.118)	(0.492)	(0.500)
Panel D: Bureaucrats (Divided)					
Lobbyist becomes Bureaucrat (Divided Govt.)	1.032*	1.047*	0.830	1.019*	1.428***
	(0.537)	(0.589)	(0.536)	(0.555)	(0.553)
Panel E: Staff (Unified)					
Lobbyist becomes Staffer (Unified Govt.)	0.489**	0.350*	0.078	−0.095	−0.253
	(0.207)	(0.183)	(0.271)	(0.359)	(0.350)
Panel F: Staff (Divided)					
Lobbyist becomes Staffer (Divided Govt.)	−0.071	0.168**	0.085	0.125	0.204*
	(0.174)	(0.081)	(0.084)	(0.091)	(0.117)
Treatment Events - Unified (Full Sample)	78	78	78	78	78
Treatment Events - Divided (Full Sample)	60	60	60	60	60
Treatment Events - Unified (Bureaucrats)	23	23	23	23	23
Treatment Events - Divided (Bureaucrats)	17	17	17	17	17
Treatment Events - Unified (Staff)	54	54	54	54	54
Treatment Events - Divided (Staff)	45	45	45	45	45

Notes: Estimates are the averages of all possible 2x2 differences-in-differences where control groups include firms with identical treatment histories prior to each respective treatment event. Future-treated firms and treatment-switchers are never included. In all models, adjustments to the control group are made by matching firms using the Mahalanobis distance calculated on logged firm revenue, logged number of contracts and logged number of lobbyists, all calculated in the pre-treatment windows. Weighted firm-blocked bootstrapped standard errors in parentheses. 1,000 trials used. *p<0.1; **p<0.05; ***p<0.01

Table D.11: Lobbying Firm Revenue and Majority versus Minority Connections

	Dependent variable:				
	ln Revenue				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: Majority Staffer Connection					
Lobbyist as Staffer (in Majority)	0.367	0.402	0.420	0.468	0.317
	(0.347)	(0.311)	(0.337)	(0.295)	(0.318)
Panel B: Minority Staffer Connection					
Lobbyist as Staffer (in Minority)	0.193**	0.222*	−0.405*	−0.018	0.295
	(0.089)	(0.114)	(0.213)	(0.133)	(0.246)
Treatment Events (Majority)	24	24	24	24	24
Treatment Events (Minority)	36	36	36	36	36

Notes: Estimates are the averages of all possible 2x2 differences-in-differences where control groups include firms with identical treatment histories prior to each respective treatment event. Future-treated firms and treatment-switchers are never included. Panel A includes lobbyists as staffers in personal offices in the majority party; Panel B includes lobbyists as staffers in personal offices of the minority party. All models' control groups are made by matching firms using the Mahalanobis distance calculated on logged firm revenue, logged number of contracts and logged number of lobbyists, all calculated in the pre-treatment windows. Weighted firm-blocked bootstrapped standard errors in parentheses. 1,000 trials used. *p<0.1; **p<0.05; ***p<0.01

D.8 Heterogeneity by Firm Specialization

As we discuss in the manuscript, one possibility is that our results are driven by variation in policy demand that coincides with lobbyists entering government. We investigate this by splitting the sample into specialist and generalist firms and re-estimating our main models within each split. We create these categories by creating a measure of issue area specialization, using issue areas reported in lobbying disclosure reports. Firms that report above the median amount of average issues areas are considered generalist, and those below the median specialist. This is similar to the specialist classification used in Bertrand et al. (2014). The idea is that, if results were driven by unmeasured, coincidental policy demand, we might see a difference based on how specialized firms on in what they work. For instance, firms working on only a few issues might be very subject to whims of policy demand. In Tables **??** , **??**, and D.14, we run similar specifications to the primary results. In each table, the first two models split the sample by whether firms are specialist firms or generalist firms. In the third model, we use the average number of issue areas as a matching covariate.

The results suggest that the effect of gaining a connection might be larger for more specialized firms. Across all specifications, the point estimates are larger. However, due to few treatment events, the estimates are also more noisy. Therefore, the results remain suggestive of this pattern, and future research would benefit from a careful matching of firm policy area expertise to client policy interest. This is complicated and would require a complex manual classification system, but these results are suggestive of a difference.

It is important to note that the estimated effects on revenue and revenue per client are large and precise among generalist firms, where there are more treatment events. This indicates that the effect is present there too. Therefore, when we match on firm specialization (instead of splitting the sample), the overall findings maintain. This is important to reassure ourselves that our findings are not driven by differential demand shocks based on firm specialization. In particular, one worry could be that policy shocks increase the demand for certain types of firms, and certain skills among legislative staffers. This could bias our results. However, since matching on firm specialization produces results that are very similar to the baseline estimates, we do not believe that our findings are driven by these shocks.

Table D.12: Lobbyists Turning Government Employee and Firm Revenue: Firm Specialization

	Dependent variable:				
	ln Revenue				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: Specialist Firms (issues per client)					
Lobbyist Becomes Government Employee	0.513	0.691	0.621	−0.406	−0.200
	(0.573)	(0.447)	(0.527)	(0.969)	(1.031)
Panel B: Generalist Firms (issues per client)					
Lobbyist Becomes Government Employee	0.328***	0.337***	0.114	0.117	0.212
	(0.114)	(0.115)	(0.139)	(0.176)	(0.171)
Panel C: Combined Adjusted Results					
Lobbyist Becomes Government Employee	0.381***	0.401***	0.251*	0.115	0.187
	(0.120)	(0.120)	(0.141)	(0.184)	(0.190)
Treatment Events (specialist)	16	16	16	16	16
Treatment Events (generalist)	126	126	126	126	126
Treatment Events (full sample)	142	142	142	142	142

Notes: Estimates are the averages of all possible 2x2 differences-in-differences where control groups include firms with identical treatment histories prior to each respective treatment event. Future-treated firms and treatment-switchers are never included. Panel A includes firms that are below the 50th percentile of issues per client; Panel B includes firms above the 50th percentile of issues per client. Panel C includes the full sample with full matching covariates, including issues per client. Weighted firm-blocked bootstrapped standard errors in parentheses. 1,000 trials used. *p<0.1; **p<0.05; ***p<0.01

Table D.13: Lobbyists Turning Government Employee and ln Clients: Firm Specialization

	Dependent variable:				
	ln Revenue				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: Specialist Firms (issues per client)					
Lobbyist Becomes Government Employee	0.026	0.007	0.056	−0.167	−0.135
	(0.050)	(0.046)	(0.089)	(0.103)	(0.119)
Panel B: Generalist Firms (issues per client)					
Lobbyist Becomes Government Employee	0.012	−0.022	−0.013	−0.014	0.006
	(0.025)	(0.030)	(0.028)	(0.035)	(0.040)
Panel C: Combined Adjusted Results					
Lobbyist Becomes Government Employee	0.019	−0.004	0.018	−0.007	0.002
	(0.024)	(0.028)	(0.028)	(0.035)	(0.041)
Treatment Events (specialist)	16	16	16	16	16
Treatment Events (generalist)	126	126	126	126	126
Treatment Events (full sample)	142	142	142	142	142

Notes: Estimates are the averages of all possible 2x2 differences-in-differences where control groups include firms with identical treatment histories prior to each respective treatment event. Future-treated firms and treatment-switchers are never included. Panel A includes firms that are below the 50th percentile of issues per client; Panel B includes firms above the 50th percentile of issues per client. Panel C includes the full sample with full matching covariates, including issues per client. Weighted firm-blocked bootstrapped standard errors in parentheses. 1,000 trials used. *p<0.1; **p<0.05; ***p<0.01

Table D.14: Lobbyists Turning Government Employee and ln Revenue per Client: Firm Specialization

	Dependent variable:				
	ln Revenue				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: Specialist Firms (issues per client)					
Lobbyist Becomes Government Employee	0.488	0.684	0.565	−0.239	−0.066
	(0.564)	(0.421)	(0.556)	(0.969)	(1.017)
Panel B: Generalist Firms (issues per client)					
Lobbyist Becomes Government Employee	0.316***	0.359***	0.127	0.131	0.205
	(0.109)	(0.105)	(0.125)	(0.155)	(0.148)
Panel C: Combined Adjusted Results					
Lobbyist Becomes Government Employee	0.361***	0.405***	0.233*	0.123	0.185
	(0.115)	(0.111)	(0.129)	(0.166)	(0.169)
Treatment Events (specialist)	16	16	16	16	16
Treatment Events (generalist)	126	126	126	126	126
Treatment Events (full sample)	142	142	142	142	142

Notes: Estimates are the averages of all possible 2x2 differences-in-differences where control groups include firms with identical treatment histories prior to each respective treatment event. Future-treated firms and treatment-switchers are never included. Panel A includes firms that are below the 50th percentile of issues per client; Panel B includes firms above the 50th percentile of issues per client. Panel C includes the full sample with full matching covariates, including issues per client. Weighted firm-blocked bootstrapped standard errors in parentheses. 1,000 trials used. *p<0.1; **p<0.05; ***p<0.01

D.9 Heterogeneity by Firm Size

Understanding which firm that drive the effect would get us closer to the mechanism producing the results. For small firms, losing a difficult-to-replace employee could cause a blow the the firm, decreasing revenue. On the other hand, for large firms – that are often highly connected already – gaining a single political connection might not add that much. Among medium sized lobbying shops, other lobbyists can offset the loss of the employee, and the connection might help them more.

Estimating this is difficult, because it requires conditioning on the dependent variable itself. However, Callaway and Li (2019) have developed an estimator of quantile treatment effects on the treated specifically for difference-in-differences designs. Under the assumption that the distribution of revenue would have changed in parallel absent treatment, the Callaway and Li (2019) estimator allows us to estimate effects across the entire distribution of revenue. While this is a stronger assumption than the classical parallel trends assumption, this is necessary for identifying effects in this setting.

To do this in a way that is comparable to our baseline results, we construct the treatment and control matches using our baseline Imai et al. (2021) specification. We then create a stacked dataset on relative event time, and proceed to compute the Callaway and Li (2019) estimates. Results are presented in Figure D.10. Panel A shows the entire distribution, while Panel B excludes the estimates for the lower end. We do this for presentational purposes: the confidence interval for the lowest estimate is very wide, and it is difficult to gauge the general trend in estimates among the other estimates. It should be noted that – because it estimates effects across the entire distribution – the Callaway and Li (2019) technique is extremely data hungry, and we are unlikely to be powered to conduct this exercise. The estimates should be interpreted with this in mind.

While there is a considerable degree of uncertainty in the estimates, the trend in ATTs across the distribution suggests that effects are concentrated in the center of the distribution. We observe small point estimates in the upper and lower tails, respectively.

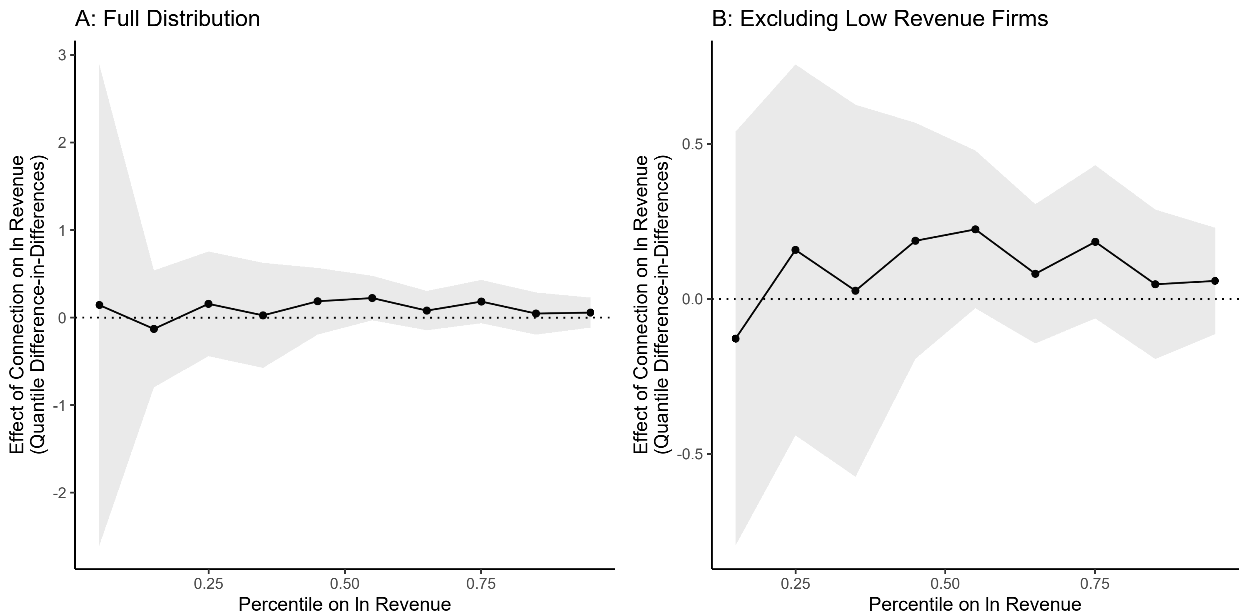

Figure D.10: Heterogeneous Effects by Revenue using Quantile Difference-in-Differences. *Note: The figure presents estimates from the Callaway and Li (2019) quantile difference-in-differences estimator. Treatment and control matches are generated using the baseline Imai et al. (2021) technique. The dataset is then stacked. Shaded area is the 90% bootstrapped confidence interval produced with firm-blocked resampling.*

D.10 Effect of Connections During Lame Duck Periods

Partisan demand shocks could increase the likelihood that partisan lobbyists enter government service, while also increase the demand for the services of partisan firms, thereby increasing their revenue. As an additional way of guarding against the possibility that our results are driven by these shocks, we zoom in on lame duck periods in our sample. These are interesting, because Congress is relatively unproductive in those periods. This implies that it is unlikely that a sudden need for the services of partisan firms will arise.

To do so, we define the last two years in all administrations as lame duck periods (i.e. 2000, 2001, 2007, 2008, 2015, 2016 and 2019, 2020). We only count lobbyists that enter government during those periods in our treatment events. We use this lame duck treatment and re-estimate our baseline specification. The results are presented in D.15. The estimates are very similar to our baseline results. This provides additional evidence that our results are not driven by partisan demand shocks.

Table D.15: Effects of Connections Made During Lame Duck Periods

	Dependent variable:				
	ln Revenue				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: Democrats					
Lobby Firm Gains Connctions	0.357	0.476*	0.419	0.590**	0.680**
	(0.231)	(0.244)	(0.263)	(0.285)	(0.292)
Panel B: Republicans and Cmte Staff					
Lobby Firm Gains Connctions	0.403*	0.470*	0.441*	0.585**	0.656**
	(0.232)	(0.245)	(0.261)	(0.283)	(0.294)
Treatment Events	49	49	49	49	49
Control Firms	40,129	40,129	40,129	40,129	40,129
Unique Controls	2138	2138	2138	2138	2138

Notes: Estimates are the averages of all possible 2x2 differences-in-differences where control groups include firms with identical treatment histories prior to each respective treatment event. Future-treated firms and treatment-switchers are never included. Weighted bootstrapped standard errors in parentheses. 1,000 trials used. *p<0.1; **p<0.05; ***p<0.01

E Connections and a "Shock" Election

Despite candidate Donald Trump's proclamations about draining the swamp, President Trump promptly filled key positions across the federal bureaucracy with lobbyists (Schouten 2017). The Trump Administration's hiring of lobbyists is not mechanistically unique from other instances of lobbyists filling government positions. The difference in this time period is the *demand* for lobbyists to fill these roles, in part because the new administration was seemingly less worried about the public perception of this strategy and in part because they were simply struggling to fill many jobs and had to turn to lobbyists (Pramuk 2017). Nonetheless, individuals still chose to leave highly paid lobbying jobs for government service, and it is likely many did so not due to the returns it generated for their firm but for other individual motivations. The result for the lobbying firms is the same: new, direct connections to top-level government officials.

We use the context of the surprising result of the 2016 election as a shock to the lobbying industry both in terms of partisan power shift (e.g., Furnas et al. 2017) and for the labor market for revolving door lobbyists (Blanes i Vidal et al. 2012). One drawback of our aggregated data used in the previous analyses is lack of granular timing information about the appointment/transition of individuals into government service, so we rely instead on when the individual first appears in government employment records and disappears from lobbying reports. However, it is possible that the firms benefit prior to employment beginning through the ability to recruit clients and increase rates based on advertising the immediate appointment. The data we use here resolves these concerns. Finally, this empirical setting permits a more straightforward difference-in-differences application and allows us to assess the magnitude of the above results under less stringent assumptions.

Data, Design and Results

ProPublica released data on the names and dates of the Trump administration's 1,066 appointees to federal agencies who were hired by the summer of 2017. We match these names to those of contract lobbyists registered under the Lobbying Disclosure Act (LDA), which is cleaned and made available by the Center for Responsive Politics (CRP).[28] In this dataset, 35 contract lobbying firms had one or more employees appointed to the executive branch during the first two quarters of the Trump administration. These are the firms that are 'treated' with a connection to the new administration and bureaucracy. As our dependent variable, we use the change in quarterly revenue (logged) of the lobbying firms, and adjust it for inflation (base year is 2015). We use Q1-2015 through Q1-2017 as our sampling period.

E.1 Matching Trump Appointees to the Lobbying Data

To identify the contract lobbyists appointed to work in the Trump Administration, we use data released by ProPublica on names and employment histories of the appointees. These data were initially acquired through requests filed under the Freedom of Information Act (FOIA). The data we use were downloaded in September 2017 and contains 1,066 appointees. While the list of appointees have been expanded to include later on, this smaller one contains the data for the first two quarters of 2017 – the period relevant to our investigation.

We matched these data to the names of contract lobbyists released under the LDA, which is cleaned and made available by the Center for Responsive Politics. Before matching, we removed all records of in house lobbyists – retaining only contract lobbyists.

We identified former contract lobbyists among the political appointees by first using fuzzy string matching. We then manually combed through the matches, validating all matches against ProPublica's own record of the employment history of the appointee as

[28]The matching procedure is outlined in the appendix. This dataset is based on the same raw data as the Legistorm dataset. However, we rely on the CRP data because it facilitates more specific timing of lobbyist departure and entry in government, and the matching procedure was aided by CRP's identification of lobbyists-turned-bureaucrats.

well as the appointee's LinkedIn profile. In this way, we corrected all false positives, and identified revolving door appointees, who were not included in the first broad matching procedure. This allowed us to leverage ProPublica's investigative work – where the procedure was somewhat intransparent – to guide our name matching.

In total, we identified 35 contract lobbyists appointed to the Trump administration during the first two quarters of 2017, twelve of whom were hired in the first quarter. This number is slightly smaller from the early reports on the ProPublica data (e.g. Mathis-Lilley 2018), which is because we only focus on contract lobbyists, while these early reports also include in house lobbyists for special interests.

E.2 Descriptive Statistics

Table E.16 shows descriptive statistics for the variables included in our models. Panel A shows the sample of firms included in the difference-in-differences models, while Panel B shows data for the full sample of lobbying firms. It is clear that there are very large differences between the two samples of firms – the firms that gain a connection to the Trump administration are much larger (as measured by revenue, lobbying contracts and active lobbyists). This is a prime reason for our identification strategy in the Trump case study.

To identify the effect of gaining a connection to the new administration on lobby firm revenue, we leverage variation in the timing of appointments into the Trump administration in a difference-in-differences specification. While some firms in our sample gain a connection as early as the first quarter of 2017, when Trump took office, others do not gain one until the second quarter. Thus, we only compare trends among firms that at some point receive a positive shock to their political connections, but use the fact that some firms gain their connection a few months earlier than others. This provides us with variation in connections to the new presidential administration.

Table E.16: Descriptive Statistics (Trump Case Study)

Statistic	N	Mean	St. Dev.	Min	Max
			Panel A: Connected Firms		
Revenue	277	6,176,056.000	12,777,035.000	0	68,660,000
Total Donations	258	163,839.500	309,909.200	0.000	1,492,215.000
Prop. Donations to R	277	0.426	0.392	0.000	1.000
Active Lobbyists	277	15.372	19.852	1	92
Number of Contracts	277	40.650	52.444	1	219
			Panel B: All Firms		
Revenue	17,449	716,888.500	3,014,285.000	0.000	68,660,000.000
Total Donations	15,145	25,937.550	88,030.650	0.000	1,492,215.000
Prop. Donations to R	17,450	0.270	0.392	0	1
Active Lobbyists	17,450	3.134	5.789	1	92
Number of Contracts	17,450	8.229	16.095	1	219

We estimate the DiDs using variations of the following model:

$$\Delta R_{fq} = \beta_1 C_f + \beta_2 H_q + \beta_{DiD} \cdot C_f H_q + \delta X_{fq} + \theta_f + \gamma_q + \epsilon_{fq} \tag{3}$$

Where ΔR is the change in revenue of lobby firm (logged) f in quarter q. C is an indicator of whether the firm is in the 'treatment group', i.e. whether it gains a connection early (first quarter of 2017 as opposed to the second), and H is an indicator of the first quarter of 2017—the 'treated period', when the lobbyist becomes a bureaucrat. β_{DiD} is the coefficient of interest, capturing the differences-in-differences as the interaction between C and H. Besides this simple differences-in-differences, we run a series of more restrictive models. We add θ_f, which is a set of firm fixed effects that capture time-invariant features of the firm—e.g. the firm's prior level of political connectedness. γ is a set of time fixed effects which adjusts for common shocks to the industry. X is a vector of pre-treatment controls. To proxy the firm's size, these include the total number of lobbyists employed in the firm and the number of contracts the firm works on (both logged). Importantly, we allow for some forms of differential trends by interacting the time fixed effects with the controls. Some firms might historically see larger quarter-by-quarter changes in revenue. To control for this, we also add an interaction between the lagged change in revenue and time. In these specifications, the parallel trend assumption is not enforced between small and large firms. Finally, ϵ is the idiosyncratic error term. Depending on the exact model specification, we rely on between 283 and 249 firm-quarter observations. We describe the dataset in the appendix, where we also show that treated firms are very different from the ones that never gain a connection, illustrating why using the full sample provides a potentially misleading control group for our study.

It is worth discussing, in substantive terms, what would constitute a threat to causal identification. Importantly, since we draw on a differences-in-differences design the identifying assumption is that trends in revenue would have evolved in parallel, had the lobbyist been appointed into the Trump administration one quarter later. In plain terms, only factors that happen simultaneously with the appointment of the lobbyist and affects revenue differentially across the treatment and control groups will bias our estimates—being a large firm, e.g., does not in itself threaten identification. The most important threat to identification arises from the environment itself—the election of Trump caused economic and political tumult. If large firms are more likely to profit from this and, simultaneously, more likely to have their lobbyists transition into the bureaucracy, this could bias our results. This is the main motivation for interacting time fixed effects with our controls—this explicitly allows for a differential impact of the environment itself depending on firm size and a history of large changes in revenue.

Results: Connections to the Trump Administration

Table E.17 presents the results of various differences-in-differences specifications. In the most simple specification in column 1, we estimate that quarterly revenue increases by 62%, when a lobbying firm has its lobbyists employed into the Trump bureaucracy.

In column 2, we add controls to the model, and in column three we interact the controls with the time fixed effects. In column four we add firm and time fixed effects.[29] While the estimate drops, the results maintain across all specifications. The 95% confidence interval around the estimate in column four, the most precisely estimated specification, implies that a connection to the bureaucracy increases firm revenue by between 30% and 43%. For the median firm, this translates into an increase in quarterly revenue amounting to between $470,000 and $660,000.

Table E.17: Lobbyist Appointment to the Trump Administration and Firm Revenue

	Dependent variable:					
	Change in ln Revenue					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.372^{***}	-0.278^{***}	-0.244^{***}		-0.043	-0.301^{***}
	(0.070)	(0.061)	(0.061)		(0.059)	(0.099)
Treated Period	-0.376^{**}	-0.422^{**}	0.400		-0.384^{**}	
	(0.163)	(0.172)	(2.562)		(0.163)	
Treated X Treated Period	0.618^{*}	0.604^{*}	0.579^{***}	0.364^{***}	0.489^{**}	
	(0.324)	(0.330)	(0.209)	(0.040)	(0.229)	
Treated X Placebo Period						-0.089
						(0.229)
Controls?	No	Yes	Yes	Yes	No	No
Time FE X Controls?	No	No	Yes	Yes	No	No
Time FE X Revenue t-1?	No	No	Yes	Yes	No	No
Firm FE?	No	No	No	Yes	No	No
Time FE?	No	No	No	Yes	No	No
Observations	275	275	246	246	249	275

Note: Panel-corrected standard errors in parentheses. Controls include: total number of lobbyists employed in the firm, and the total number of contracts the firm works on (both logged). *p<0.1; **p<0.05; ***p<0.01

[29] Note that adding the two-way fixed effects differences out the indicators of treatment group and period

We run two additional robustness checks. First, there are a number of very low-revenue firms in the treatment group, which could be a less than ideal comparison with firms in the control group. In column 5, we exclude the lowest 10% in the revenue distribution, and the results maintain. Second, if there is cyclicality in revenue that affects the treatment and control group differentially, this could drive the results. We test this by using a placebo indicator for the same quarter in the previous year. We find no effect on revenue. Taken together, the statistical significance and magnitude of these results aligns with what we present above. However, these results add additional context the noisy results on gaining bureaucratic connections, suggesting two possibilities: first, the staggered DiD approach reduced precision as Imai et al. (2021) discuss as a possibility. Second, this specific context—the surprise election of Trump and unprecedented hiring of lobbyists—was a substantively more important shock to lobbying firms that prior hiring events. Either way we believe the evidence here bolsters confidence in the previous results.

Results: Effect is not Moderated by Partisanship

An important threat to identification is that the political environment might cause lobbyists in some firms to be in higher demand. The same environment might cause those firms to see higher revenue. In particular, lobby clients may be more interested in the services of firms with ties to the party in control of government. At the same time, the lobbyists in those firms could be a coveted type of employee in the bureaucracy and as legislative staff, as an incoming administration seeks to staff positions with knowledgeable and loyal individuals.

The election of Trump provides a nice testing ground for ruling this out as it holds constant the partisan environment. However, it also allows us to test an additional observable implication: If the effects were driven by partisan demand for lobbyists, we should see differentially large effects among firms connected to the Republican Party. To test this, we collect data on campaign donations of individual lobbyists from the Center for Responsive Politics, and use this to construct a measure of the balance of campaign donations. Specifically, we compute the proportion of donations from a firm's employees that go to either party, and subtract the two. This gives us the net proportion of donations to Republican candidates as one end of the scale (1), the net proportion of donations to Democrats in the other end of the scale (-1), and firms with no donations in the middle (0).

To estimate effects flexibly, we use the Hainmueller et al. (2019) binning estimator for interactions to estimate the difference-in-differences at different points. Figure E.11 shows the results. As we can see, the estimates are positive at all points in the distribution, and there is no evidence to suggest that the effect differs depending on partisanship.

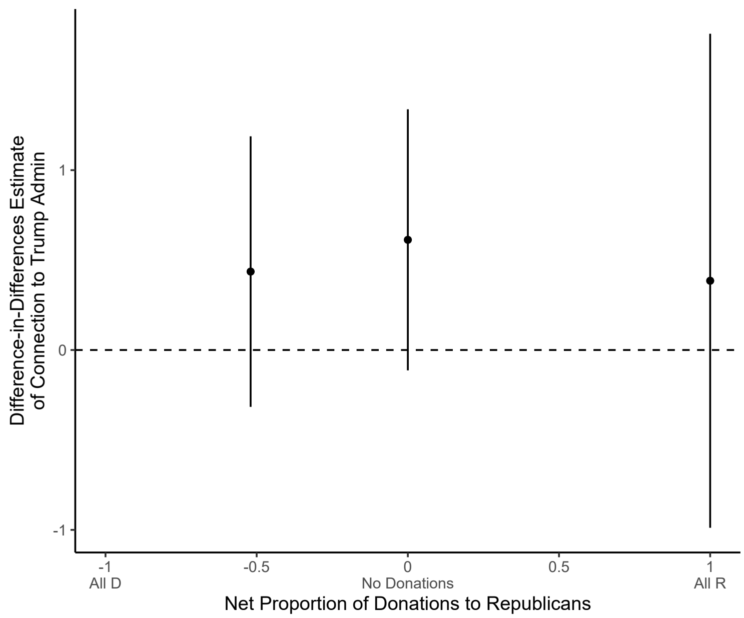

Figure E.11: Firm Partisanship Does Not Moderate the Effect. *Note: The figure shows estimates of how the effect of gaining a connection to the Trump administration varies depending on the lobbying firm's campaign donations. We measure donations using the net proportion of donations to Republicans. This measure ranges from only donations to Democrats (-1), over no donations at all (0) to only donations to Republicans (1). We estimate marginal effects of a connection within quartiles of the distribution of this net proportion measure using the Hainmueller et al. (2019) binning estimator. Lines are 95% robust confidence intervals with firm-level clustering.*