Lobbyists as Government Employees: Evidence from the Bureaucracy

Benjamin C.K. Egerod Joshua McCrain

Preliminary draft, August 20, 2018

Abstract

A growing body of research examines the transition of government employees into the private sector, commonly known as the revolving door. A common conclusion in this work is that government employees cash in on their experience and, most importantly, their connections to current officials. We focus on two under-studied aspects of the revolving door: 1) the transition of lobbyists back *into* government and 2) the value of connections between lobbyists and government agencies. Exploiting the unexpected victory of Donald Trump in the 2016 presidential election and the staggered timing of appointments into his administration, we find that firms whose lobbyists went back through the revolving door saw 20-70% increase in revenue, and the increase was larger in Democrat-leaning firms. Our results shed light onto the political economy of the lobbying industry and suggest a need to further study the interaction between lobbying firms and the federal bureaucracy.

1 Introduction

A growing body of research empirically examines the phenomenon of government officials transitioning from government service into private sector jobs – often in the lobbying industry. The existing work on this process, known as the revolving door, has focused on Capitol Hill staffers who leave low-paying jobs and cash in on their connections (Bertrand, Bombardini, and Trebbi 2014; Blanes i Vidal, Draca, and Fons-Rosen 2012; McCrain N.d.).[1] As this research argues, quantifying the value of public service is an important task because it sheds light on the political economy of public employment and provides insight into what lobbyists do by relating their experience to lobbying revenue. In this short paper, we focus on two rarely studied aspects of lobbying and the revolving door: lobbying in the bureaucracy and the transition of lobbyists *into* government employment.

To provide empirical insight into these phenomena, we examine lobbying firms whose lobbyists were appointed to a position in the Trump administration during its first six months. To this end, we marshal a unique dataset of Trump appointees matched to lobbying disclosure reports and campaign finance data. Because of Trump's surprise victory and the staggered timing of appointments, this allows us to take advantage of empirical variation in order to test the effect of political connections. To do so, we first compare all firms in a fixed effects design. Second, we leverage the fact that some people were appointed a few months before others in difference-in-difference design. Political connections are highly endogenous, and these appointments provide a unique opportunity to study the value of connections as it relates to the revenue of lobbying firms. Employing this strategy, we find a substantial financial premium associated with gaining a connection – up to a 65% increase in revenue.

The focus of this research is timely. Despite candidate Donald Trump's proclamations about draining the swamp, President Trump promptly filled key positions across the federal bureaucracy with lobbyists (Schouten 2017). In order to gain insight into the normative implications of lobbyists filing back into government, and how this affects policy outcomes and the potential for capture, it is first necessary to understand the

[1]For a recent exception, see (Palmer and Schneer Forthcoming)

economic reaction of the lobbying industry to this unexpected increase in access. For instance, if we were to observe little economic benefit to a firm when an employee leaves for government service, one could argue that individuals revolving into government are doing so for personal reasons (e.g., a public service motivation) or simply to increase their own human capital (Parker 2009). Alternatively, and as our results suggest to be the case, a lobbyist transitioning into government may be a valuable asset to the firm that employed them – a marketable trait that is sold to clients. This latter story paints a more troublesome picture of lobbying, one where lobbying firms can control and sell differential access to key policymakers in positions to substantively affect policy outcomes (Brown and Huang 2017; Goldman, Rocholl, and So 2013).

1.1 Lobbying, the Revolving Door, and the Bureaucracy

We argue that lobbyists provide expertise that is of value to special interests, especially in an environment of high policy uncertainty – for instance, immediately after an unexpected election result.[2] Lobbyists possess a comparative advantage in resources and policy expertise and provide this information to resource-constrained policymakers with whom they have relationships (Hall and Deardorff 2006). The deeper the connections lobbyists have to policymakers serves to lower the transaction costs of establishing a relationship with a lobbying target because they facilitate building trust. Then, instead of capture or corruption, what clients most desire from lobbying efforts is the acquisition of information about a complex policy environment (LaPira and Thomas 2017). Lobbying firms value connections to government officials because it facilitates this process.

The majority of previous revolving door research focuses on Congress and connections to members and staff, yet the theories of what lobbyists do – and why connections are valuable – translates to bureaucratic lobbying. Moreover, the extant literature on lobbying the bureaucracy finds that lobbying is particularly effective when the target is a federal agency. Since policymaking in agencies is arguably less transparent and more

[2] The high level of uncertainty was particularly pronounced during the transition period as the new administration had difficulty finding qualified Republicans willing to work in government (Rein and Phillip 2017). There was little idea, in other words, what the policy priorities of the new administration would be – and who would be spearheading them.

complex than in Congress, special interests are able to inject themselves into the rulemaking and policy implementation stages to great effect (Haeder and Yackee 2015; You 2017). Premised on this logic, we hypothesize that firms will place particularly high value on access to key policymakers in the bureaucracy and, when they gain new (and unexpected) connections, there will be an increase in the firm's revenue. This increase reflects that clients paying for a firm's services will pay more for lobbyists connected to policymakers placed highly in federal agencies.

2 Data, Design and Identification

ProPublica has released data on the names and dates of appointment of the Trump administration's 1,066 appointees to federal agencies, who were hired by September 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). 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 quarterly revenue of the lobbying firms, and adjust it for inflation (base year is 2015). We use Q1-2013 through Q2-2017 as our sampling period. Again, we use the CRP's cleaned version of data released under the LDA. We describe the dataset in the appendix.

To identify the effect of gaining a connection to the new administration on lobby firm revenue, we use two strategies. First, we estimate twoway FE models comparing trends among connected firms to the non-connected ones before and after their lobbyist is hired into the Trump administration. However, it is not random which firms gain a connection, and there are large differences in revenue between connected and unconnected firms, which could cause different post-treatment trends. To deal with this, we run DiD models leveraging variation in the timing of appointments into the Trump administration. 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, in this strategy 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, which is plausibly exogenous to trends in revenue.

3 Results

Figure 1 plots of the trends in quarterly revenue among treated and non-treated firms leading up to the time, when a firm gains a political connection. While the trend line is slightly less stable among firms that eventually gain a political connection, pre-treatment trends look approximately parallel. Additionally, after treatment we see a sizable divergence in quarterly revenue between the firms that do and do not receive a connection.

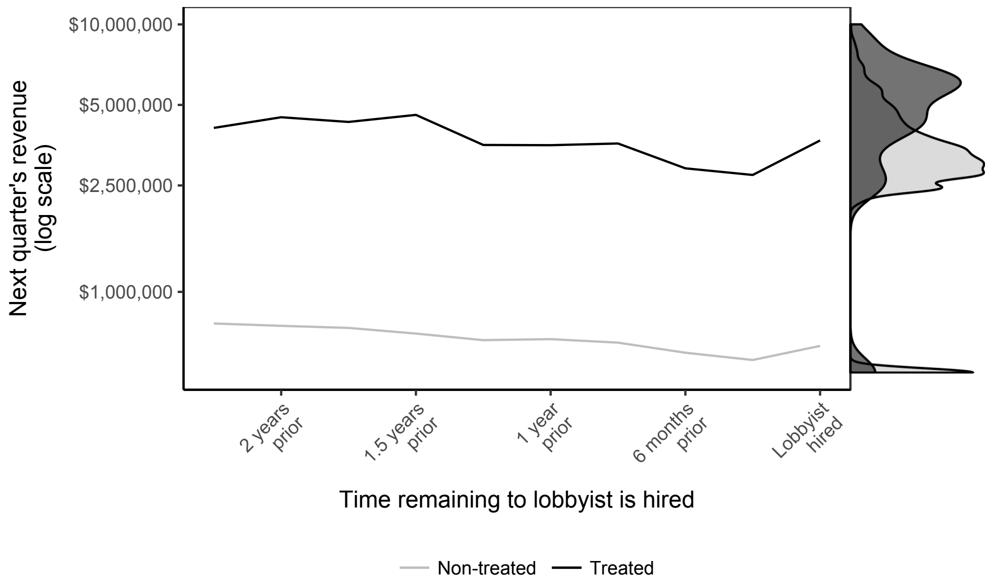

Figure 1: Firm revenue leading up to connection. *Note: solid lines are the quarterly means. Marginal distributions are from the full sampling period. Vertical axis is censored to range between $500,000 and $10,000,000 for presentational purposes. Full plot is in appendix.*

Figure 2 presents results from our two modeling approaches. The simple, uncontrolled FE estimate suggests a 21 percent increase in revenue from gaining a connection, while the DiD estimates a 65 percent increase.

Estimates from both these strategies are identified under the assumption of parallel

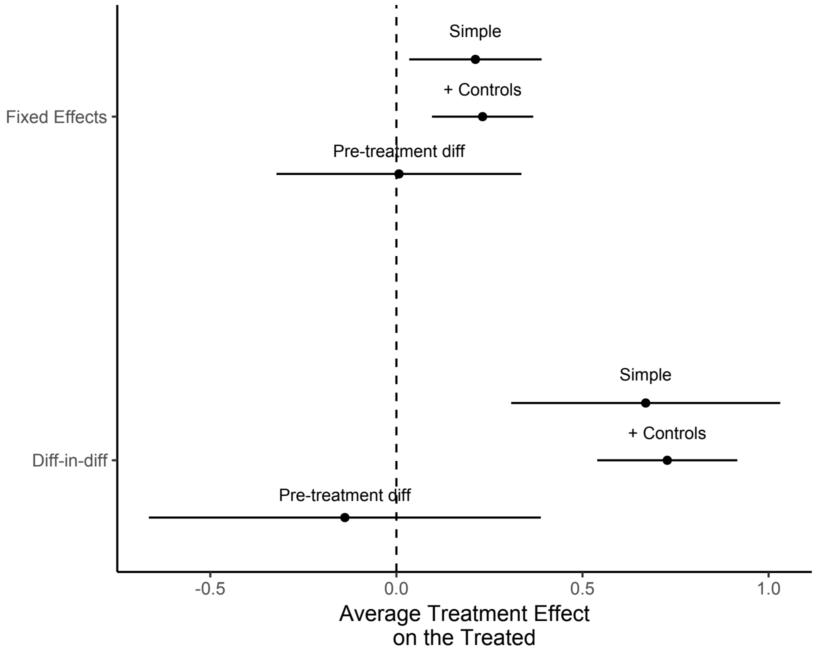

Figure 2: Fixed effects and Diff-in-diff results. *Note: DV is logged revenue with a one-quarter lead. Confidence intervals are 95 percent, calculated using SEs clustered on firm and time. Controls include: yearly numbers of lobbying contracts and active lobbyists. Additionally, total campaign contributions in 2016 by the firm's lobbyists and a lagged DV is included in the DiD. All firms compared in FE models. Only firms that at some point gain a connection are compared in the DiD models.*

trends in the absence of treatment. This is especially likely to hold in the DiD. Still, some factors could cause unobservable differences in counterfactual post-treatment trends. We apply two strategies to eliminate this possibility. First, we include a number of controls. To proxy firm size, we include a firm's yearly number of lobbying contracts and active lobbyists as covariates. To control for the level of a firm's political connectedness in the DiD model, we include the total amount of campaign contributions lobbyists hired in each firm donated in 2016. The results maintain. Importantly, this suggests that our results are not driven by the surprise election of Donald Trump having a disproportionate effect on the large and politically connected lobbying firms, a point we delve further into in the appendix.

Second, as a check on the parallel trends assumption, we estimate pre-treatment differences by using the lag of revenue. In neither case can we reject that there are no differences in pre-treatment revenue trends.

In the appendix, we show that pruning the dataset using Coarsened Exact Matching

(Iacus, King, and Porro 2012) and Generalized Synthetic Controls (Xu 2017) provides essentially the same results. The findings are also robust to different assumptions about uncertainty, different ways of modeling time and exclusion of influential observations.

3.1 Firms connected to Democrats drive the effect

One important observable implication of our theory of connections to a presidential administration and its bureaucracy is that there should be heterogeneous effects depending on the firm's preceding stock of connections. Specifically, we would expect firms that already are highly connected to the Republican Party to experience a smaller return to new connections – they have some ability to provide access to the new Republican administration before their employee is appointed. Firms connected to the Democrats, however, should experience above average returns to a connection because this new access becomes especially valuable.

To test this, we subtract the proportion of Democratic donors from the proportion of Republican ones among a firm's lobbyists, giving us the net proportion of donations to the Republican Party. This gives us three classes of firms on a continuum from least to most connected to the Republican Party a) those, whose employees exclusively donated to Democrats (-1), b) those that did not employ donors (0), and c) those with only Republican donors employed (1). We use 2016 data on this, collected from the CRP. This proxies the strength of the ties between a lobbying firm and both of the major parties, before the firm gains a direct connection to the new presidential administration.

In Figure 3, the FE model includes an interaction between our treatment variable and net proportion of donations to Republicans. The DiD is a threeway interaction between treatment group, treated period, and net proportion of donations to Republicans, including all constitutive terms as well. This makes it a difference-in-difference-in-differences (DiDiD) specification.

In the FE model, we estimate that firms employing 50 percent Republican donors experience a 45 percent smaller return to an additional connection than firms with no donors employed. Firms with no donors experience a 36 percent higher revenue, than

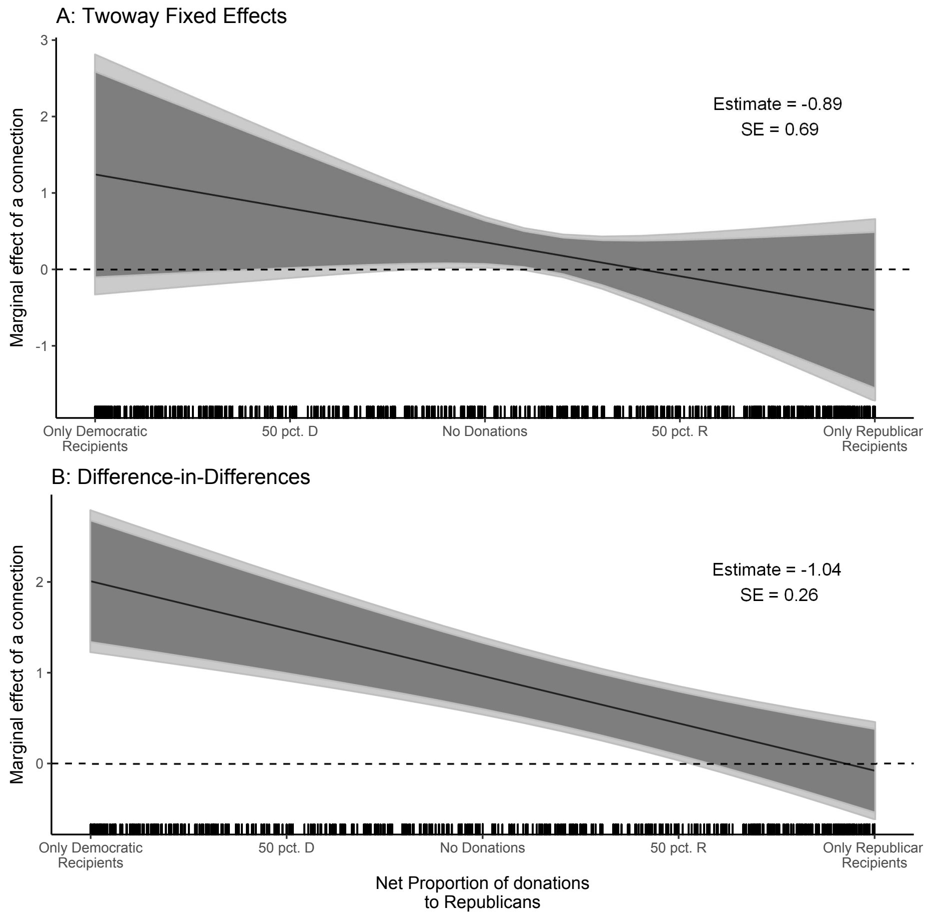

Figure 3: Firms connected to Democrats drive the effect. *Note: DV is logged revenue with a one-quarter lead. Gray-shaded areas are 95 percent (light) and 90 percent (dark) confidence intervals, calculated using SEs clustered on firm and time in FE models and time only in DiDiD models.*

they otherwise would have. In the DiDiD model, we estimate this difference to be 52 percent. While these estimated differences in effects are very large, only the ones from the DiDiD are statistically significant. Overall, however, this supports the proposition that the effects are driven by firms connected to the Democratic Party – even though the FE estimates only do so tentatively.

4 Discussion and Conclusion

The results presented in this short paper elucidate an under-studied feature of the lobbying industry: when firms send their employees into government service, the firm sees a significant increase to their revenue. This finding aligns with previous work on the value of connections in lobbying and suggests that the clients who hire firms prefer those with the strongest connections – those to a previous employee. Though this paper focused on a short time period because of the empirical leverage it provides, it is likely this mechanism is prevalent in other settings, especially when uncertainty about the policy process is highest. Moreover, the finding that Democrat-connected firms gain the most from a connection suggests a previously uncovered empirical relationship: firms primarily associated with the minority party are incentivized to hire bipartisan employees when power switches hands.

Finally, this paper suggests a number of avenues for future research. Scant political science research, for example, has focused on the transition from the private sector to public employment – in other words, walking back through the revolving door. Our findings suggest firms stand to gain considerable revenue from sending an employee into government. Future work looking into the career paths of these revolvers would provide more insights into this phenomenon. Further, to date we have little understanding of the political economy of lobbying the bureaucracy, though empirical work suggests lobbyists possess substantial ability to affect the policy process when targeting the bureaucracy. It is important to understand which bureaucrats become lobbyists, what predicts their value as lobbyists, and how they behave once in the lobbying industry. This article begins to shed light onto these questions by giving insight into the political economy of lobbying firms and the value of connections to key agency officials.

References

Bertrand, Marianne, Matilde Bombardini, and Francesco Trebbi (2014). "Is it whom you know or what you know? An empirical assessment of the lobbying process". In: *The American Economic Review* 104.12, pp. 3885–3920.

Blanes i Vidal, Jordi, Mirko Draca, and Christian Fons-Rosen (2012). "Revolving door lobbyists". In: *The American Economic Review* 102.7, pp. 3731–3748.

Brown, Jeffrey R and Jiekun Huang (2017). *All the President's Friends: Political Access and Firm Value*. Tech. rep. National Bureau of Economic Research.

Goldman, Eitan, Jörg Rocholl, and Jongil So (2013). "Politically connected boards of directors and the allocation of procurement contracts". In: *Review of Finance* 17.5, pp. 1617–1648.

Haeder, Simon F and Susan Webb Yackee (2015). "Influence and the administrative process: Lobbying the US President's Office of Management and Budget". In: *American Political Science Review* 109.3, pp. 507–522.

Hall, Richard L. and Alan V Deardorff (2006). "Lobbying as legislative subsidy". In: *American Political Science Review* 100.01, pp. 69–84.

Iacus, Stefano M, Gary King, and Giuseppe Porro (2012). "Causal inference without balance checking: Coarsened exact matching". In: *Political analysis* 20.1, pp. 1–24.

LaPira, Timothy M. and Herschel F. Thomas (2017). *Revolving Door Lobbying: Public Service, Private Influence, and the Unequal Representation of Interests*. University Press of Kansas.

McCrain, Joshua M. (N.d.). "Revolving Door Lobbyists and the Value of Congressional Staff Connections". In: *Journal of Politics*. Forthcoming.

Palmer, Maxwell and Benjamin Schneer (Forthcoming). "Post-Political Careers: How Politicians Capitalize on Public Office". In: *Journal of Politics*.

Parker, Glenn (2009). *Capitol investments: the marketability of political skills*. University of Michigan Press.

Rein, Lisa and Abby Phillip (2017). *Help wanted: Why Republicans won't work for the Trump administration*. https://www.washingtonpost.com/politics/help-wanted-why-republicans-wont-work-for-the-trump-administration/2017/06/17/61e3d33e-506a-11e7-b064-828ba60fbb98_story.html?noredirect=on&utm_term=.4b382894adc3. (June 17, 2017).

Schouten, Fredreka (2017). *Ex-lobbyists swarm Trump administration, despite 'drain the swamp' pledge.* https://www.usatoday.com/story/news/politics/2017/06/21/president-donald-trump-lobbyists-hired/416749001/. (June 21, 2017).

Xu, Yiqing (2017). "Generalized synthetic control method: Causal inference with interactive fixed effects models". In: *Political Analysis* 25.1, pp. 57–76.

You, Hye Young (2017). "Ex post lobbying". In: *The Journal of Politics* 79.4, pp. 1162–1176.

Online appendix for: Lobbying and Valuable Connections to the Bureaucracy

August 20, 2018

Contents

A Data overview

A.1 Descriptive statistics

Table A.1 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 the full dataset, which the fixed effects models are based on.

Table A.1: Descriptive Statistics

Statistic	N	Mean	St. Dev.	Min	Max
Panel A: Connected firms only					
Revenue (logged)	317	13.395	3.464	−0.693	18.045
Total Donations	325	164,034.500	310,226.100	0	1,492,215
Prop. Donations to R	355	0.151	0.570	−0.768	1.000
Active Lobbyists	355	14.772	19.159	1	92
Number of Contracts	355	39.228	50.925	1	219
Panel B: Full dataset					
Revenue (logged)	15,954	10.154	4.222	−0.693	18.045
Total Donations	15,873	25,024.740	86,218.760	0	1,492,215
Prop. Donations to R	18,668	0.035	0.581	−1.000	1.000
Active Lobbyists	18,668	3.025	5.639	1	92
Number of Contracts	18,668	7.832	15.675	1	219

A.2 Pairwise Correlations

Table A.1 shows the pairwise correlations between revenue and the controls included in our difference-in-differences models. Therefore, it is the reduced sample only including firms that gain a connection. Revenue is strongly correlated with our measures of size (number of active lobbyists and number of contracts) and total donations. It is negatively correlated with the net proportion of donations to Republican candidates.

The measures of size are very strongly correlated, reflecting the fact that they capture the same underlying phenomenon. Firm size is also strongly correlated with the total donations of a firms employees, which is likely driven by the fact that they simply have more lobbyists employed. Finally, firms that mainly donate to Republican candidates have fewer lobbyists employed and are hired on fewer contracts. They also donate less overall.

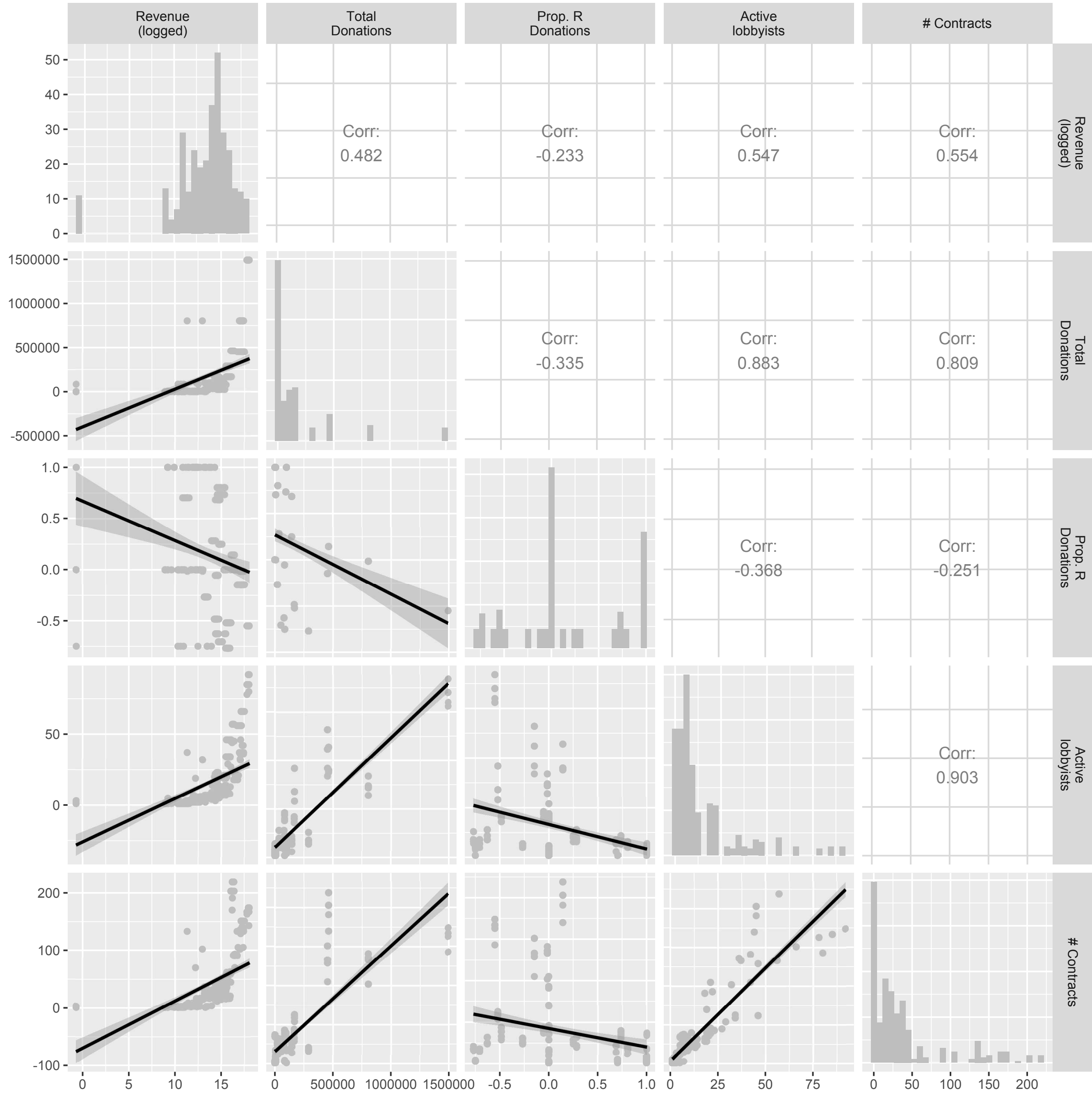

Figure A.1: Distributions and correlations among connected firms.

A.3 The Full Pre-Trend Plot

In the main text, we censored the vertical axis and left out the individual data points to make the graph of pre-treatment trends more readable. In Figure A.2, we show revenue trends along with all of our data.

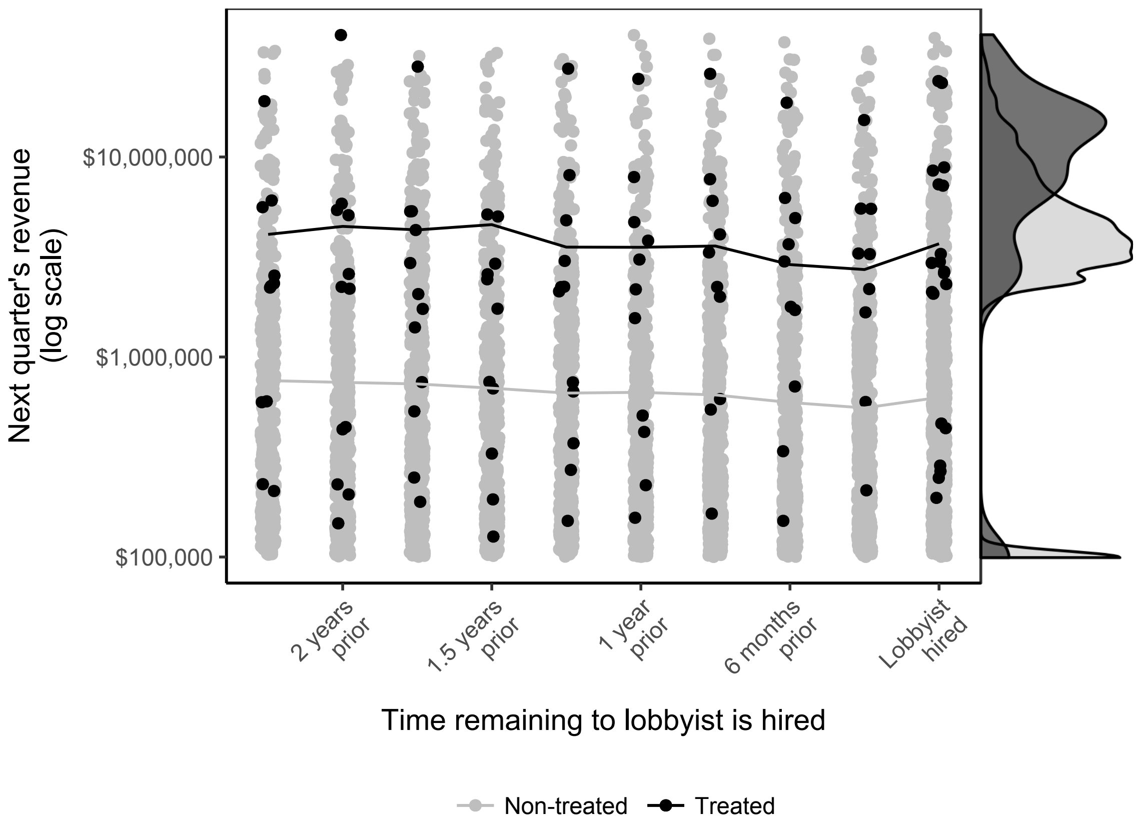

Figure A.2: Firm revenue leading up to connection. *Note: solid lines are the quarterly means. Marginal distributions are from the full sampling period. All firms included.*

B Additional Issues of Causal Identification

A highly salient threat to identification is if the largest and most politically connected lobbying firms are the ones, who gained the most from the election of Donald Trump. This would be consistent with the 2016 election inducing high levels of uncertainty, which created new business for lobbyists that disproportionately flowed toward large and connected firms. In the fixed effects sample, this could manifest itself by the large and connected firms seeing their lobbyists appointed to the new administration. In the difference-in-differences sample, those firms could have their lobbyists appointed *sooner* than the others.

In this section, we apply coarsened exact matching (Iacus et al. 2012) and generalized synthetic controls (Xu 2017) to test the robustness of our main findings. Both of these techniques imply making different identifying assumptions to recover the average treatment effect on the treated (ATT). Importantly, they relax the assumption of homogeneous shocks to the system, which are made in both the fixed effects and the difference-in-differences models. If the results are robust across these different techniques, it will substantially improve our confidence in our estimates. Additionally, we conduct some further tests of this in Appendix C.

B.1 Generalized Synthetic Control Estimates

First, we present the results from two different specifications using generalized synthetic controls. This technique uses an interactive fixed effects model on control group revenue to simulate counterfactual trends among our connected firms. We use the difference-in-differences sample, which implies that only connected companies are included. Because we have relatively few treated firms, we use the expectation maximization algorithm, which provides more precise estimates at the cost of more computational intensity. We use cross validation to choose the number of factors in the underlying interacted twoway fixed effects model. Figure B.3 presents the results. Panel A shows estimates without controls, while they are included in Panel B.

As we can see, both models are highly successful in predicting pre-treatment trends among the connected firms. There is close to no error in the model without controls. When including controls, the error grows slightly, but remains extremely small. The ATT estimated in the simple model is very large (approximately 200 percent), which is largely driven by the fact that synthetic firms are predicted to experience a marked decrease in revenue. Including controls decreases the estimated ATT to approximately the same size as our baseline results. This change seems to be mostly driven by the fact that synthetic firms also experience an increased revenue, which remains considerably smaller than the improvement that connected firms see.

B.2 CEM, G-synth, FE and DiD

Figure B.4 allows comparison of our main results and two additional causal techniques. First, we use coarsened exact matching to match observations on 2016 campaign contri-

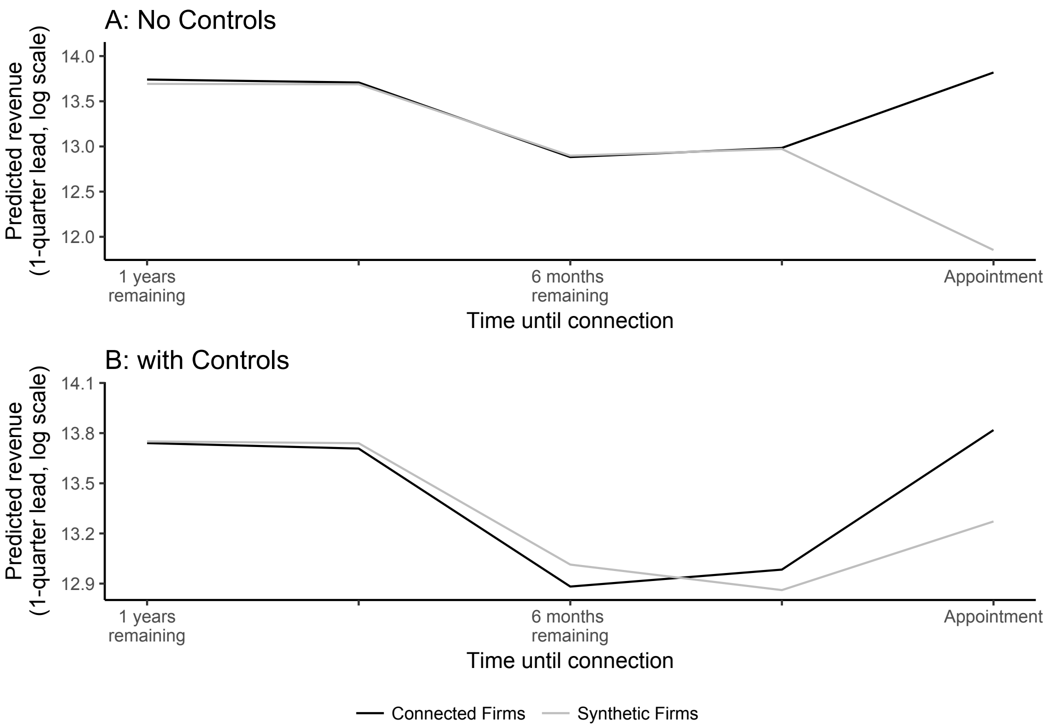

Figure B.3: Generalized Synthetic Control Estimates *Note: Expectation maximization used due to small number of firms. Five fold cross validation used to find optimal number of factors – three are used. Dynamics in revenue modeled using an AR(1) process.*

butions, pre-treatment averages of lobbying contracts and number of lobbyists. Importantly, this allows us to incorporate pre-treatment political connectedness in the fixed effects model. By estimating our models including only observations that are matched non-parametrically on pre-treatment averages, we are likely to soak up the potentially confounding effect of shocks with differential effects. There is close to no change in the fixed effects model, while the estimate in the difference-in-differences is slightly larger than the simple model and when including controls. The results, however, are substantively the same.

Second, we show the point estimate from the the generalized synthetic control estimates presented above. While the estimated ATT without controls is substantially larger than other estimates, including covariates brings it to the same magnitude as the baseline estimates.

Overall, the results are robust to these different estimators, both with and without the inclusion of time-varying controls. This reassures us that we have recovered a good estimated of the ATT.

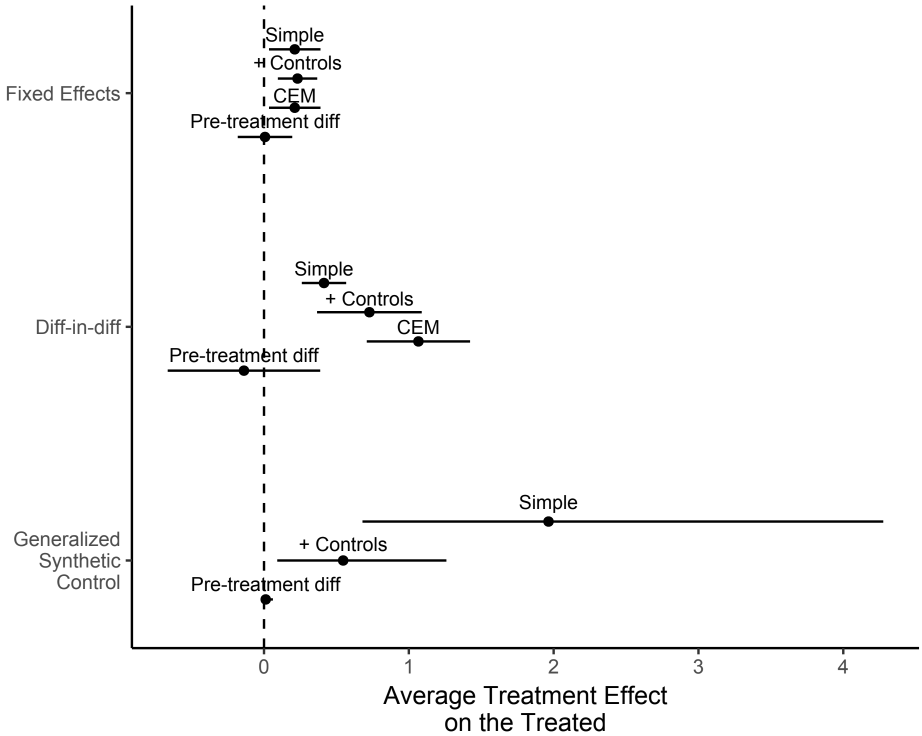

Figure B.4: Comparing fixed effects, Diff-in-diff, CEM and g-synth estimates. *Note: Confidence intervals are 95 percent calculated using SEs clustered on firm as well as time in the fixed effects and diff-in-diff models. CIs are the relevant quantiles of the distribution of 500 parametric bootstrap resamples in the g-synth models.*

C Robustness: Outliers, Autocorrelation and Differential Shocks

Since there are relatively few treated firms in both the fixed effects and the difference-in-differences models (35 and 12, respectively), the results are potentially vulnerable to firms with extreme revenue trends. To test how sensitive our results are to this, we use jackknife resampling, where we iterate over the entire sample, leaving one observation out each time and reestimating the baseline models. Figure C.5 plots the coefficients from all of these iterations. For the difference-in-differences model (Panel B), the jackknifed distribution is highly concentrated around the baseline estimate, and the lowest estimated effect is approximately 0.37. This indicates that the main estimate is robust to the presence of extreme observations. Thus, the large and positive effect of connections on revenue seems to be something that is common to the treated firms in the difference-in-difference model.

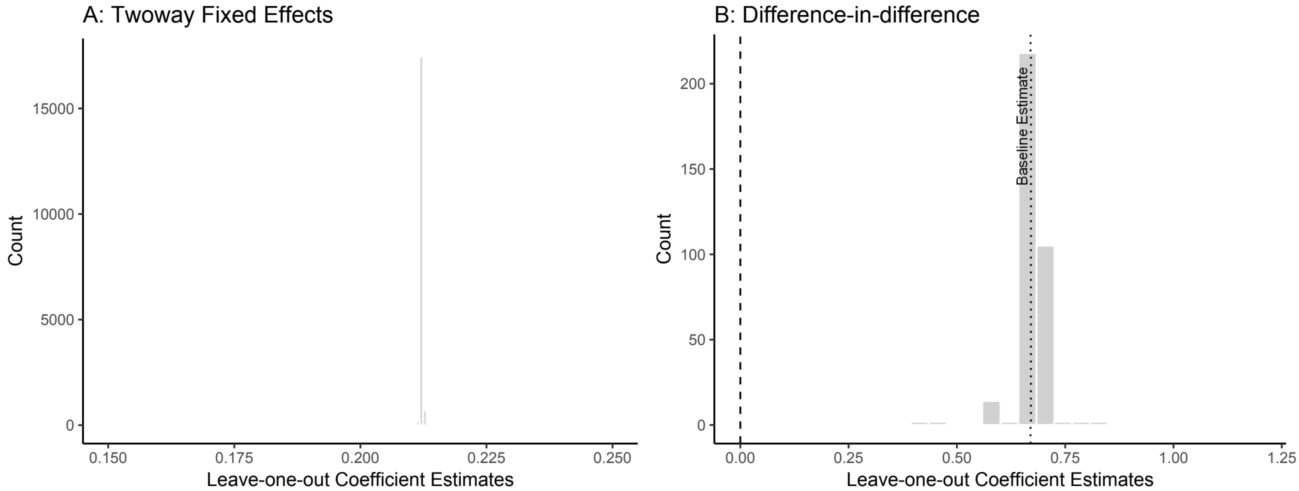

Figure C.5: Sensitivity to Excluding Observations. *Note: The plot shows the histograms of estimated coefficients, when leaving one observation out and reestimating the main specification.*

The fixed effects distribution is highly concentrated around the baseline estimate as well. Excluding one observation, however, does yield a large change, reducing the coefficient estimate to as little as .03. Therefore, as an additional test, we exclude all firms with a dfbeta above the standard threshold of $2/\sqrt{N}$. The changes from the baseline estimate are very negligible (the coefficient is about .2), and it remains significant statistically speaking ($p<0.02$). This indicates that the extreme observations in the fixed effects model pull in opposing directions, and excluding all of them at once yields a result that is very similar to the baseline estimate.

Figure C.6 shows a number of additional robustness checks. In the main specifications, we cluster standard errors on both firm and time. This deals with the dual problems of temporal and contemporaneous autocorrelation as well as cyclicality in revenue trends. Especially for the difference-in-differences model, however, there might be too few observations required for standard asymptotic results to hold. One symptom of this is negative eigenvalues in the variance matrix. To make sure this does not drive our results, we first include a lagged dependent variable and then use the first difference of revenue as the dependent variable. In both cases, the results maintain.

Finally, we deal with the potential for differential shocks in two additional ways. First, if there is some recurrent shock to revenue in the second quarter that is specific to the largest firms, this could bias our results. To deal with this, we allow the second quarter of each year to have a differential impact across firms. The results maintain.

Second, we include an interaction between all controls and the time fixed effects. This allows shocks to have differential effects depending on firm size and campaign donations. Thus, if there are unobserved shocks that disproportionately impact the large and politically connected lobbying firms, this would correct for the potential bias induced in this way. The difference-difference results are largely unchanged. The fixed effects coefficient is similar to the baseline estimate (approximately .19), but it is only significant at the 10 percent level.

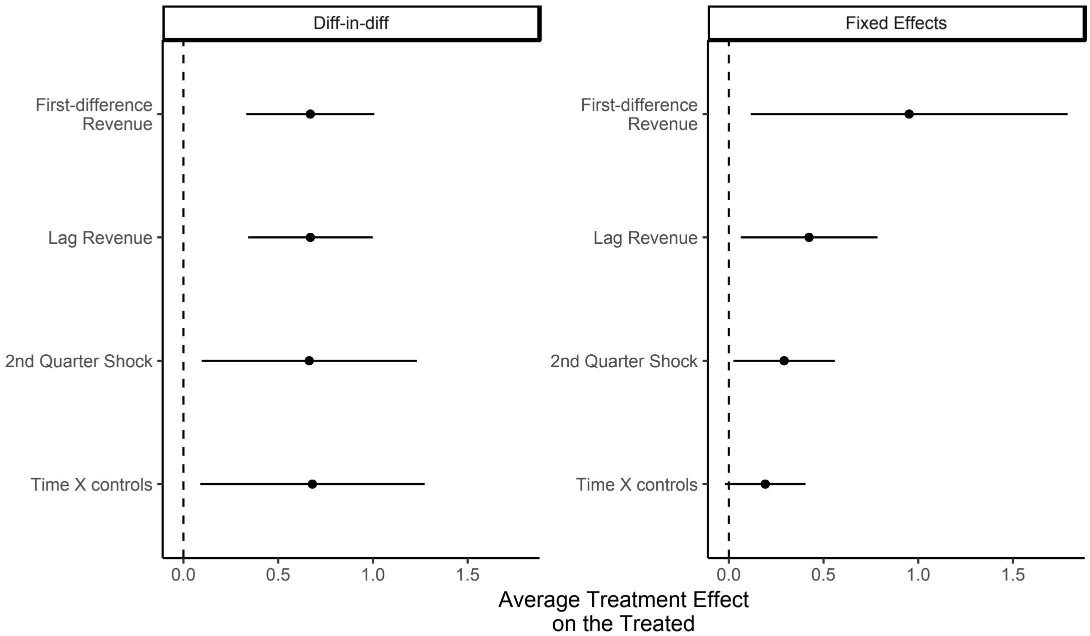

Figure C.6: Sensitivity Analysis. *Note: Each model shows the robustness check specified in the row label. If nothing else is specified, standard errors are clustered on firm and time.*

References

Iacus, Stefano M, Gary King, and Giuseppe Porro (2012). "Causal inference without balance checking: Coarsened exact matching". In: *Political analysis* 20.1, pp. 1–24.

Xu, Yiqing (2017). "Generalized synthetic control method: Causal inference with interactive fixed effects models". In: *Political Analysis* 25.1, pp. 57–76.