Lobbyists into Government

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Abstract

Lobbying firms seek access to policymaking by hiring individuals with connections to government officials and with previous experience as government employees. This paper examines a different avenue for access: the transition of a firm's lobbyists into government roles. We find firms frequently forge connections to government in this manner and their business benefits as a result. Using panel data from 2001-2020 of U.S. federal bureaucrats and congressional staff matched to lobbying records, we quantify the value to lobbying firms when their employees enter government service. We find lobbying firms that gain government connections through the departure of one of their lobbyists experience a 36% revenue increase, or roughly $320,000 per year. In examining what drives this increase in revenue, we find firms present connections as a premium service to existing clients and that connections to congressional offices are more valuable than those gained to executive branch agencies. These results shed light onto the business model of lobbying firms and the political economy of the lobbying industry.

Keywords: lobbying the bureaucracy, revolving door, political connections, federal agencies, congressional staff, political appointees.

How do special interests gain access to policymakers? The lobbying industry frequently relies on hiring former government employees, a phenomenon known as revolving door lobbying, as a key element of a firm's strategy for gaining access to the policy process. Existing research finds a consistent positive relationship between the earnings of individual lobbyists and gaining such connections to policymakers, suggesting firms or clients view them as particularly valuable personnel (Bertrand et al. 2014; Blanes i Vidal et al. 2012) Additionally, recent research indicates that interest groups indeed can gain access by hiring revolvers (Egerod et al. 2021). In this paper, we provide further evidence of the value of connections by focusing on the transition of lobbyists *into* government positions. We find that this understudied method of generating access is common practice in the lobbying industry and results in significant boons to a firm's business: on average, we find firms experience a 36% increase in year-over-year revenue, roughly $320,000, when one of their lobbyists enters government. This builds on the existing literature (Bertrand et al. 2014; Blanes i Vidal et al. 2012; McCrain 2018) by showing that political connections shape lobby firm revenue – not just the fortunes of individual lobbyists.

We then assess how gaining connections shapes a lobbying firm's business model. We find that newly connected firms primarily monetize connections through extracting more revenue from existing clients; however, they neither gain additional clients nor hire additional lobbyists. There is some evidence that the firm's revenue becomes more highly concentrated among existing clients, but this remains quite suggestive. We then show heterogeneity in value based on the types of connections: specifically, congressional-based connections are associated with a larger and more persistent increase in revenue compared to executive branch connections (approximately 40% and 20% respectively).

We also find that the value of connections varies substantially over time because the political economy of lobbying firms unsurprisingly depends on idiosyncrasies of the political environment. As a result, we demonstrate that a methodological approach that does not take into account this time variability will produce biased results. Because of the dynamic

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nature of connections, where firms can gain and lose them over time, we employ recent methodological innovations (Imai et al. 2021) that account for staggered treatment timing (in this case, as is common in the literature, connections are the treatment). This method constructs appropriate counterfactuals to treated firms by comparing them to similar firms based on observed firm traits and their individual treatment histories. As a more general result, we suggest many settings that use classic approaches to analyzing panel data, such as two-way fixed effects, will produce biased or even wrong-signed estimates of the quantity of interest.

This collection of results is consistent with prior expectations in a connections-based theory of lobbying (Bertrand et al. 2014).[1] If lobbyists benefit from connections, as previous work finds, then when firms gain new connections (or strengthen existing ones) their revenue should increase. We build on the body of evidence of previous research by finding a) a substantial value in *direct* connections to government, measured by a previous employee entering public service, and b) quantifying a similar value to connections in executive branch lobbying. This research also directly relates to a large economics and management literature on corporate political connections as a key tenet of a firm's non-market strategy (e.g., De Figueiredo and Richter 2013; Hillman 2005; Hillman et al. 1999; Palmer and Schneer 2019; Richter et al. 2009). We demonstrate empirical evidence for an understudied method of firms gaining political connections: seeing their employees transition into government service (rather than, for instance, boards hiring ex-politicians). We document some suggestive evidence that this is a strategy for individual lobbyists: over 35% of lobbyists that go into government eventually re-register as lobbyists, with over 50% of those lobbyists returning to their previous firm.

Taken together, this paper makes two substantive contributions to the literature on lobbying and policy access by special interests. First, there is little research and few stylized facts available about the transition of lobbyists *into* government – a potentially more worry-

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[1]Or, alternatively, in an insurance-based theory of lobbying where access to knowledge of policymaking is valuable in reducing risks to special interests (Ban et al. 2019; LaPira and Thomas 2017).

ing revolving door than the reverse from a normative stand point (a point we return to in the conclusion). If former lobbyists are directly involved in policymaking (as well as indirectly, e.g. Hall and Deardorff 2006), *and* their previous firms see substantial revenue increases, then policymakers have stronger standing in their creation of revolving door restrictions. A story that aligns with this evidence is one where lobbying firms can control and sell differential access to key policymakers in positions to substantively affect policy outcomes (Brown and Huang 2017; Furnas et al. 2017; Goldman et al. 2013). Additionally, our comprehensive dataset allows us to present the first comparison of the value of connections to congressional staff and federal bureaucrats. We find evidence that the political economics of lobbying are similar for the two types of connections: the lobbying industry rewards connections in both institutional settings. However, there is a smaller premium associated with connections to federal agencies, indicating that lobby clients view them as less essential to their non-market strategy.

Lobbying, Access and Political Connections

A rich literature in political science examines the relationship between lobbyists and legislators towards understanding how special interests influence public policy. This literature proposes that lobbyists primarily provide information to policymakers after establishing relationships that allow the policymakers to trust the information they acquire (Austen-Smith and Wright 1992, 1994; Schnakenberg 2016). Since policymakers are resource constrained, lobbyists likely subsidize the efforts of the legislators with whom they work, suggesting that influence comes through aiding legislators already aligned with the lobbyists' policy priorities (Cotton 2015; Hall and Deardorff 2006). The value of political connections, then, comes through the relationships lobbyists have with *like-minded* legislators.

Empirical research finds evidence for a connections-oriented lobbying and influence industry. From a labor market perspective, lobbyists with prior congressional staff experience

are associated with higher lobbying revenues than their peers with no record of public service (Cain and Drutman 2014; LaPira and Thomas 2014). The evidence suggests that these lobbyists are most rewarded based on connections to lawmakers and their staff (Blanes i Vidal et al. 2012; McCrain 2018), though there is an added premium for individuals with specialized knowledge as well as connections (Ban et al. 2019; Bertrand et al. 2014; LaPira and Thomas 2017).[2] Thus, working in government provides both valuable expertise and connections that is in high demand to lobbying firms and the clients that hire them. The fact that staffers-turned-lobbyists earn more based on their connections is not evidence of policy influence per se, but it is suggestive of the mechanism that lobbyists use their relationships as an avenue to influence policy, as the subsidy theory suggests (Hall and Deardorff 2006).[3]

We argue that lobbyists provide expertise that is of value to special interests. 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 through lowering transaction costs of gaining access to public officials in the first place.

Prior knowledge surrounding these questions has two features: first, existing studies overwhelmingly focus on congressional lobbying and the revolving door for congressional staff, with little stylized facts known about bureaucratic lobbying generally. However, what we do know suggests the importance of studying executive branch lobbying: previous research finds

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[2]LaPira and Thomas (2017) and Brown and Huang (2017) find evidence that knowledge and connections benefit in hedging against political uncertainty and risk.

[3]Moreover, subsidy lobbying can result in shifts in policy outcomes away from a policymaker's ideal point through information asymmetries (Hirsch and Shotts 2012). Even if lobbyists primarily work with allies to whom they are connected, policy outcomes can begin to more closely align with ideal points of special interests (and not necessarily voters).

that lobbying is particularly effective when the target is a federal agency or the rulemaking process. Since policymaking in agencies is arguably less transparent and more technical than in Congress (see Godwin et al. 2012, ch. 4), special interests are able to inject themselves into the rulemaking, policy implementation and enforcement stages to great effect (Gordon and Hafer 2005; Haeder and Yackee 2015; You 2017)

Second, the empirical research does not examine another common feature of revolving door lobbying (and the feature that makes it a true *revolving* door) – the transition of lobbyists *into* government. Extending the logic of the previous lobbying research, we argue that connections gained through a former employee gaining a job in government is just as – if not more – valuable than a former public servant becoming a lobbyist. From the perspective of the lobbying firm who sees their employee enter government service, and the clients who choose to hire the lobbying firm, there is a strong case the firm can make that they have valuable insight and access into the business of government. From a theoretical point of view, this is no different than a lobbying firm advertising to potential clients that they have government connections based on hiring (for instance) a legislator's chief of staff. In a model of lobbying access, both types of connections would serve to reduce transaction costs around subsidy or informational lobbying. Additionally, the research that does look at political connections in lobbying examines them in the context of individual lobbyist career concerns – i.e., whether gaining or losing a connection affects that lobbyist's portfolio.[4] It is entirely possible that individual lobbyists, who lose (gain) a connection then also loses (gains) revenue, but that the firm as a whole is not affected due to other changes in its business model. Here, we explicitly look at how the firm itself is affected by connections.

What does the decision to enter into government look like from an individual employee's perspective? Put differently: why might a lobbyist give up their well-paying private sector employment for a return to relatively low-paid government work (which is especially true if they become a congressional staffer; Congressional Management Foundation 2012; Furnas

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[4]For an exception, see Furnas et al. (2017) which examines firm revenue based on the firm's partisan status.

and LaPira 2020)? From a perspective of individual career concerns, one reason is the value of maintaining political connections for lobbyists. Blanes i Vidal et al. (2012) document a 24% decrease in lobbyists' revenue when they lose a political connection, thus refreshing these connections through returning to government should be of clear benefit. Another reason may be a particular demand for lobbyists' expertise given a particular legislative focus (i.e., healthcare, tax reform) which allows the lobbyist to transition into relatively more senior, higher paying government positions.[5] Finally, lobbyists may be particularly public service motivated (Perry and Wise 1990), driving them to re-enter public service given an appealing opportunity.[6] Importantly, either explanation leads to the same empirical expectation vis-a-vis informational and subsidy theories of lobbying: a boon to the lobbying firm through gaining political connections.

The value of political connections to firms, broadly, is well-established in the political science and management literatures. In management in particular it is well known that firms use political connections as part of their broader non-market strategy (Brown and Huang 2017; Hillman et al. 1999; Yan and Chang 2018). Hillman (2005), for instance, finds that boards with more politicians have better than expected market performance. We believe the setting of the lobbying industry is of *per se* importance and represents a beneficial environment to further study this question. Additionally, this context provides needed empirical leverage to bypass selection effects that make inferences difficult in this literature, which we discuss in the next session. Though we examine this relationship in the context of the lobbying industry, we note that the theoretical premise is potentially more general and applies to other firms that interface with government.[7]

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[5]Alternately, the pool of potential hires for senior agency officials may be sparse leading to an increase in demand for lobbyists, which as we discuss below was the case during the beginning of the Trump Administration.

[6]A similar motivation might be for a more stable and less demanding job in government. Below we document some stylized facts about the likelihood of these individuals returning to lobbying after working in government which suggests some heterogeneity in these motivations.

[7]Though the specific mechanism of connections facilitating lobbying subsidy is theoretically uniq ue to the lobbying industry, the broader mechanisms behind the value of political connections generalize to a variety of firms (Hillman et al. 1999).

Specifically, we hypothesize a positive relationship between firm revenue and gaining a political connection through previous employees becoming public servants. We expect to see a positive increase in revenue for firms that gain connections in this manner, relative to *similar firms that did not have employees become government employees.* This nuance is important theoretically and empirically. Theoretically, not all firms operate in the same business model or might have a differential ability to see their employees enter into government. This might be due to firm size, with few lobbyists to spare, or some feature of the firm's specialization or portfolio. Alternately, small firms may see larger benefits from connections through a model of subsidy lobbying, where an additional connection is of greater marginal value than it would be to a larger firm. Thus, creating an appropriate counterfactual, and not pooling firms together, is important beyond the typical research design concerns. We formalize this idea in our econometric approach below, and conduct a robustness analysis that uses staggered timing of lobbyist entry into government following a shock election result.

Data Overview and Stylized Facts

We use a comprehensive dataset of lobbying disclosure reports, which are publicly available reports at the semesterly (prior to 2007) and then quarterly (after 2007) level, as mandated by the 1995 Lobbying Disclosure Act (LDA).[8] These reports are separately filed with the House and Senate and comprise all lobbyists and lobbying activity, including revenue, lobbying registrant, and lobbying clients.[9] We acquired a cleaned version of the raw data from the private firm Legistorm for the years 2001-2020. Legistorm parses the disclosures and assigns unique IDs to lobbyists, firms, and clients through a process of rectifying and manually

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[8]The 1995 LDA and the 2007 Honest Leadership and Open Government Act (HLOGA) are the two primary sources of revolving door regulations in this time period of study. The HLOGA, however, largely does not apply to the entry of lobbyists into government; it only limits the behavior of previous government employees who enter into lobbying by imposing cooling off periods. While we cannot exploit variation produced by the former due to data constraints, in the appendix we examine if there is variation in revolving door dynamics around the HLOGA due to potential shifts in labor markets.

[9]See `https://lobbyingdisclosure.house.gov/amended_lda_guide.html` for precise definitions of "lobbying activity" under the Act.

checking names.

In the raw form, the data consist of over 6 million lobbyist-lobbying filing observations with 70,000 unique lobbyists and 1.2 million filings. To clean the data, we first subset the full data to firm lobbyists (otherwise known as multi-client lobbyists).[10] We then match these lobbyists to a comprehensive dataset of congressional staff employment records, also provided by Legistorm.[11] To determine lobbyists that transition into agencies, we use Legistorm's extensive coding of lobbyist backgrounds, involving a manual process of examining and tracking the careers of registered lobbyists.[12]

Next, we aggregate the lobbying data to a lobbying firm-semester level to include all activity during that period. For our purposes, we aggregate the total number of contracts and total revenue per firm-semester. Firm lobbying reports by law must state how much clients spend for the lobbying activity covered by that report (for more detail, see Blanes i Vidal et al. 2012; McCrain 2018). The final panel consists of over 76,000 firm-semester level observations. Before we turn to the empirical approach, it is worth noting the structure of this firm-level data. In line with previous theories and empirical work on connections in lobbying, we conceptualize a firm gaining a connection to a government by seeing one its employees enter into government as a 'treatment.' By construction, firms can either remain entirely untreated, treated, or have treatment turn on and off any number of times, as Figure 1 makes clear. Indeed, 85 firms experience multiple treatment periods. This feature of the data holds important implications for potential empirical strategies, threatening inferences from standard procedures frequently used in this literature.

Overall, we identify 815 lobbyists from 412 distinct lobbying firms that revolve into gov-

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[10]These lobbyists are distinct from in-house lobbyists, who are employed by an organization and lobby exclusively for that organizing (e.g., Amazon employing a chief lobbyist).

[11]This dataset consists of all individuals listed in congressional disbursements from 2000-2020. The data include staffers' names and office of employment. Legistorm's version of this publicly available data includes an ID that matches staffers to the lobbying disclosure data.

[12]We manually checked a large sample of Legistorm's matches and found them to be accurate. Additionally, we were able to verify among the set of lobbyists who transition into government then back into lobbying the accuracy of this matching process since these lobbyists disclose their prior experience in government as required by the Lobbying Disclosure Act.
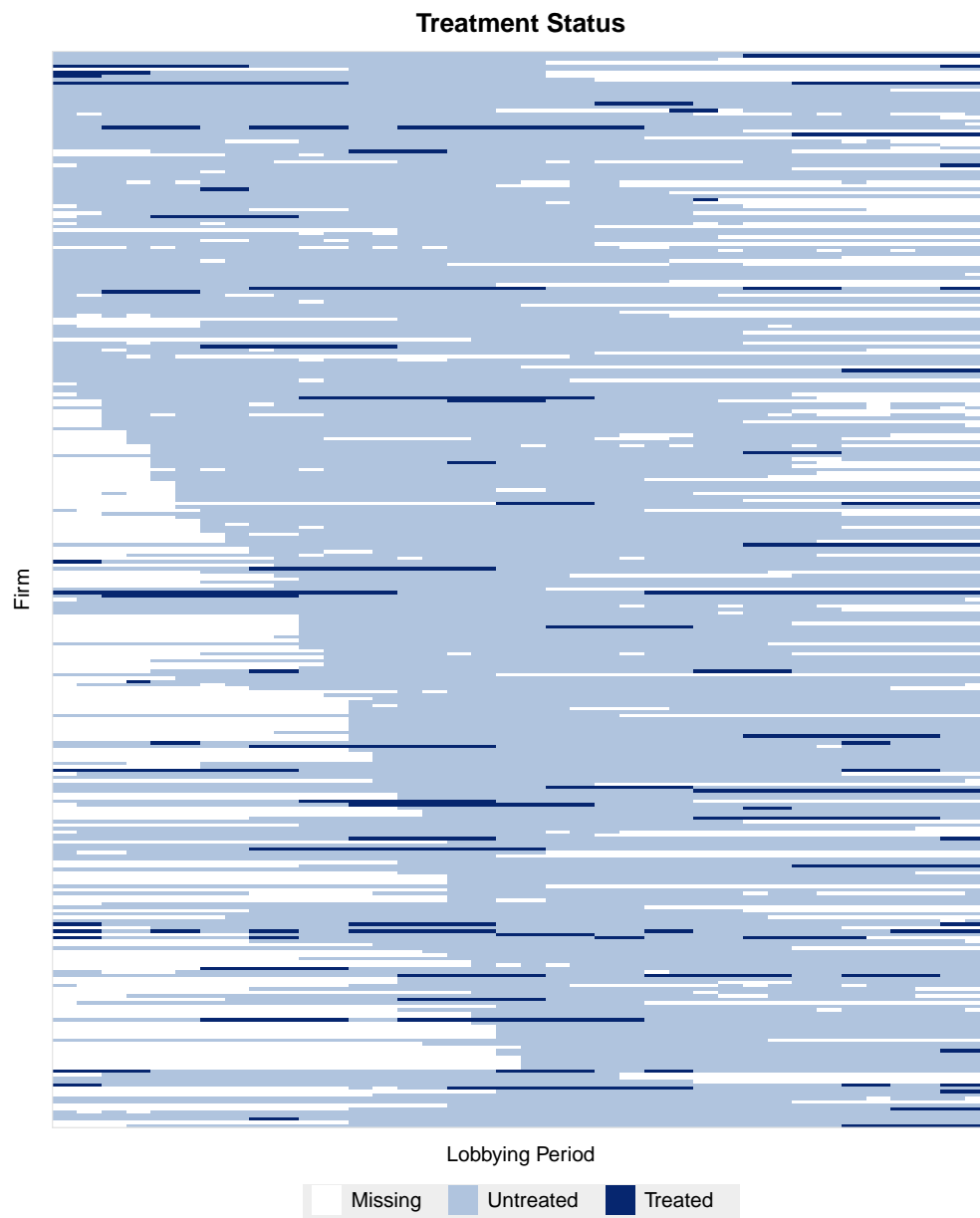
Figure 1: Firms and Treatment Timing. *Note: This figure displays a random sample of 315 firms in our data, indicating when firms are 'treated' (they have a former lobbyist in government) versus 'untreated'. White areas indicate the firm did not register any lobbying activity.*

ernment – 610 of these lobbyists become congressional staff and 205 become agency officials. These lobbyists represent sizable percentages of the overall populations of revolving door lobbyists; the 610 lobbyists as staffers is roughly 14% of all lobbyists with any congressional staff backgrounds, while the 205 lobbyists with bureaucrats are 24% of the lobbyists with identified executive branch experience. Figure 2 displays the number of lobbyists that take government positions by semester and the cumulative number of lobbyists working in government. Additional heterogeneity in the data, which we explore analytically below, is by the type of congressional office lobbyists revolve into or the specific agency. Table 1 shows that the most common destination for those revolving into Congress is the House at 287 lobbyists, or 47.1% of all lobbyists. While the White House is the most common destination for those entering the executive branch, with 50 lobbyists, there are nearly 50 other agencies in which lobbyists tend to enter.

In the Appendix, we report additional descriptive statistics on the individual lobbyists. In general, the lobbyists that revolve into government are slightly lower-performing lobbyists than those who do not revolve within the same firm; they are associated with 5 fewer contracts per year and just under 9% less revenue per contract (Appendix Table A.3 displays full descriptives on these differences). Focusing on the destinations of these revolving lobbyists, Table A.1 demonstrates that there are a wide array of committees that lobbyists join, with House Energy and Commerce as the most common, followed by Senate Indian Affairs, House Transportation and Infrastructure, and House Homeland Security. Table A.2 shows that staff who enter into personal member offices join fairly typical offices on average; for instance, the average previous vote share of an office hiring a lobbyist is 63% (relative to the congressional average of 66%); the average seniority of the member is 5 terms, aligning with the congressional average. The most common job titles are Legislative Assistant, Chief of Staff, and Counsel, indicating lobbyists move into higher level positions in Congress – this is especially notable since only 27 of the lobbyists-as-staff have prior Capitol Hill experience.

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Lobbyists are also slightly more likely to join offices of members on powerful committees.[13] We return to the implications of these stylized facts in our discussion and conclusion.
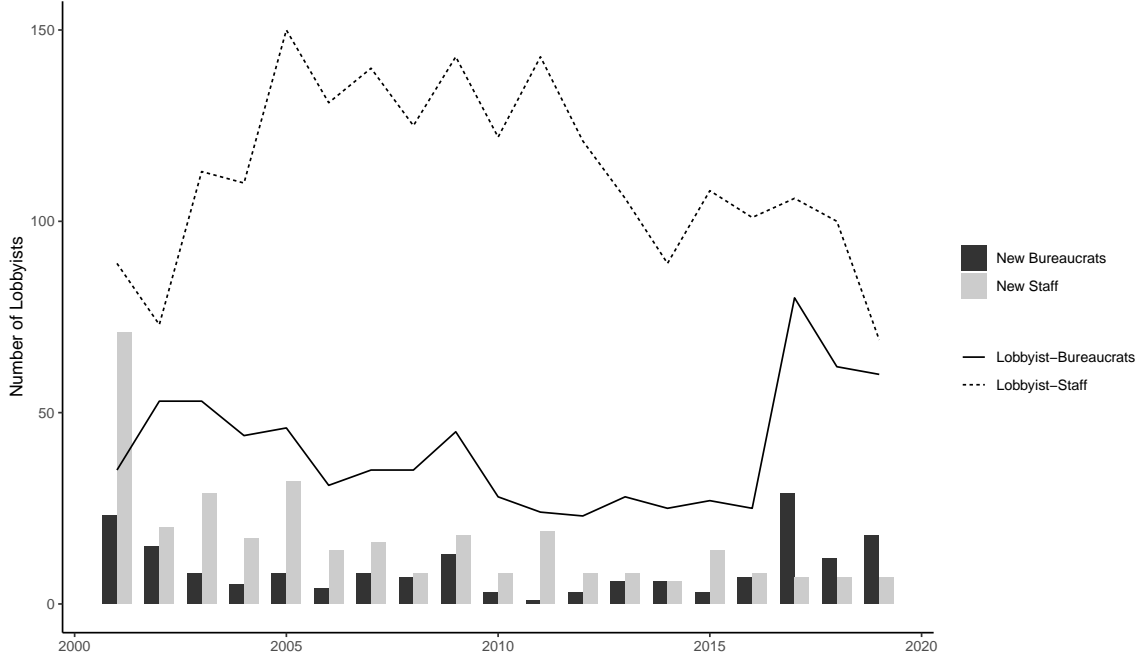

Figure 2: Lobbyists in Government by Year. *Note: The bars display the number of firm lobbyists entering government by year, separated by whether they are bureaucrats or congressional staff. The lines indicate cumulative lobbyists as government employees.*

From the firm side, Table 1 shows that 412 unique firms have lobbyists become government employees, with most firms (340) having lobbyists exit into Congress. Additionally, 11% of firms have lobbyists enter into both executive branch and congressional roles. The mean number of lobbyists entering into government per firm (conditional on any lobbyist entering government) is 1.8. We also calculate average longevity in the market, and compare this between firms that are never treated and those that are treated at some point during the period we study. Firms that are eventually treated have a much larger longevity, which is consistent with the finding in Appendix D.1 that treated firms are much larger (we adjust for firm size in the empirical approach detailed in the next section).

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[13]Interestingly, lobbyists also join offices that are ideologically moderate, they are evenly likely to join Democrat versus Republican offices, but they are more likely to join the office of a woman member of Congress, relative to the congressional average. More descriptives by party and time variation in the data is shown in Appendix Figures A.1 and A.2.

Table 1: Descriptive Statistics: Lobbyists and Firms

By Destination:		
Congressional Offices	610	74.8%
Senate	89	14.6%
House	287	47.1%
Committees	234	38.4%
Federal Government	205	25.2%
(Top 5 Destinations)		
White House	50	24.4%
Dept. of Education	16	7.8%
Dept. of Labor	11	5.3%
U.S. Trade Representative	9	4.9%
Homeland Security	9	4.9%
Total	**815**	
By Lobbying Firm:		
Firms	412	
Congress Departure	340	
House	204	
Senate	75	
Committee	163	
Senate & House	30	
Agency Departure	113	
White House	22	
Other	96	
Agency and Congress	46	
Firm Longevity	5.9 Years	
Treated	13.9	
Control	5.4	

Note: This table displays descriptive statistics of lobbyists who revolve into government employment based on their destination in government. Figures depicting variation in these destinations over time are displayed in the appendix. For firms, note that firms can have lobbyists enter into multiple destinations, so numbers will not add up to the overall totals.

Research Design: Identification and Estimation

The transition of lobbyists into new roles as bureaucrats and congressional staffers represents an appropriate application of a differences-in-differences design. The lobbying firm that recently employed the lobbyist-turned-government-official gains a valuable political connection, allowing us to measure changes in a firm's revenue around the time of the event. Essentially, each of these hiring events can be viewed as a shock where all lobbying firms that do not gain such a connection can be used as a control group. This implies that we conceptualize firms gaining political connections through experiencing one of their employees enter government as 'treated'. This is similar to other empirical papers on the political economy of lobbying that leverage external events that change the connectedness of firms (e.g. Blanes i Vidal et al. 2012; Furnas et al. 2017). The difference in our setting comes through the *method* in which firms gain these connections—the identifying assumptions are the same as in the prior work.

Staggered Differences-in-Differences

Traditionally, the individual differences-in-differences (DiD) would be pooled together by estimating a regression with fixed effects for lobbying firm and time period – the two-way fixed effects estimator (TWFE)[14]:

$$ln\ revenue_{it} = \delta C_{it} + \gamma_i + \omega_t + \epsilon_{it} \tag{1}$$

Here, γ and ω denote a full set of dummies for firm and time (lobbying reporting) period, respectively. C is an indicator of the firm i having a former lobbyist working in federal government job at time t. *revenue* is the natural log of firm's revenue for the given reporting period. In this case, δ is the differences-in-differences estimate, summarizing the effect of

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[14]Alternatively, variations of the first-difference estimator or other classic panel data techniques could be used. They would all be plagued by the same form of bias as TWFE.

gaining a political connection on firm revenue. ϵ is an idiosyncratic error term.

Recent methodological research on DiD designs demonstrate that this is equivalent to estimating a weighted average of all possible differences-in-differences, where the weights are equivalent to the size and variance of the treatment groups (Goodman-Bacon 2018). Importantly, some of the weights attached to treatment groups can be negative, meaning that TWFE often yields biased estimates of the average treatment effect on the treated (ATT) (Callaway and Sant'Anna 2019; De Chaisemartin and d'Haultfoeuille 2020; Imai et al. 2021). In particular, this bias in δ occurs if already-treated firms enter the control group for newly-treated firms, and the ATT varies over time. This implies that the bias in many real-world settings will be so large that "summarizing time-varying effects using [the TWFE estimator] yields estimates that are too small or even wrong-signed, and should not be used to judge the meaning or plausibility of effect sizes" (Goodman-Bacon 2018, p. 17).

In Appendix B, we discuss this bias in a simplified setting with three groups of firms treated at different times. Substantively, for this bias *not* to exist, we must assume that the effect of gaining a connection remains constant over time. There are, however, many reasons to expect violations of this assumption. Not only will changes in the political environment (e.g. because of partisan power shifts) produce differences in the returns to connections, the lobbying industry itself has changed significantly since the beginning of the period we investigate. This holds from a political economic perspective as well as through new regulations. Additionally, the value of a connection varies for firms depending on their business models. If a firm that relies strongly on its connections is treated at one point, while other firms that depend less on connections are treated during other periods, this will be enough to induce bias. In Figure B.4 in the appendix, we show that estimates produced by difference-in-differences models within each time-period produces extremely variable results, suggesting that the ATT varies significantly over time. In Appendix Figure B.5, we show that TWFE yields wrongly signed estimates in this setting. Together, this suggests that the TWFE estimate of the ATT is highly biased in this context.

Estimation

To solve this problem, Imai et al. (2021) proposes a three-step procedure[15]. First, each lobbying firm that gains a connection is matched to a control group of firms that do not gain a connection at the same time, but has the exact same treatment history within a pre-specified lag period. For each of these individual treatment events, we can estimate a single difference-in-differences, where a newly connected firm is compared to its own control group. Finally, the overall ATT is obtained by averaging over all of these individual difference-in-differences. This ensures that some groups of firms do not receive disproportionate weight in the estimate of the ATT. Importantly, since firms are matched to un-treated control groups, the method deals with Goodman-Bacon bias arising from heterogeneous effects over time. The difference-in-differences can be estimated with all leads, in which case the control group should be further manipulated to exclude firms whose treatment status changes within the lead window. It is straightforward to extend the framework to include controls by matching the treated and control firms prior to estimation of the difference-in-differences. Uncertainty is estimated through a firm-blocked bootstrap procedure where each observation is weighted by its weight in the matching procedure. Importantly, the Imai et al. (2021) procedure allows for firms losing a connection, which makes it highly suited for our setting compared to other new difference-in-differences estimators which assume that treated firms never lose their connection (e.g. Callaway and Sant'Anna 2019).

Figure 3 illustrates the functioning of the Imai et al. (2021) procedure. The single, fully colored red cell represents the first period where the firm in question is treated with a connection. The fully colored blue area represents the pre-treatment period and the firms that are not treated within the period, but have an identical treatment history before the treated firm gains a connection. The latter functions as the control group for this particular treatment event. Together, this makes up the data on which the difference-in-differences for this single firm is estimated. Notably, we see that another firm (top row, shaded red)

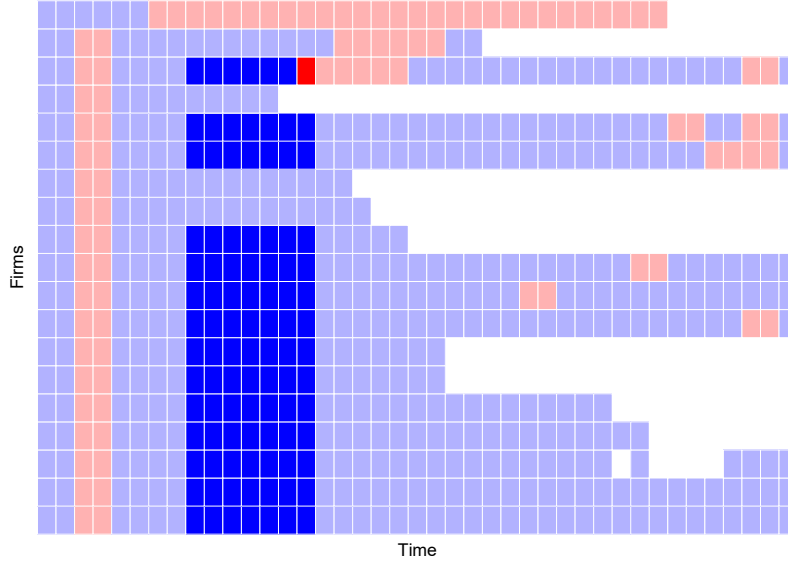[15]Implemented in the PanelMatch package for R (Kim et al. 2021)

Figure 3: Creating the Control Group for a Single Treatment Event. *Note: This figure is an example of a control group constructed through the Imai et al. (2021) method. Light shaded areas are are firm-periods not included in this specific difference-in-differences. Dark shaded areas include the control group and pre-treatment period. The darkest individual cell, colored red, is the treated firm-period. Light red cells are separate treatment periods. Missing observations (periods when firms do not report lobbying activity) are in white.*

is also treated within the period. That firm, however, is excluded from the control group, as are firms with missing observations within the period. Additionally, note that all firms included in the control group have the same treatment history, and remain untreated in the post-treatment period, where they also have non-missing observations. This process of identifying treatment events, constructing an untreated control group for that event, and estimating a single difference-in-differences is repeated for all firms that gain a connection. The estimates are averaged to produce a single ATT.

Our main specification constructs a control group of firms that have the exact same treatment history within a lag of six semesters. This has the consequence of dropping some treatment events from our estimations. However, as we show in Appendix Figure D.8, our results are robust to decreasing the lag length, thereby including more treatment events.

For each specification, we report two pieces of information on the control group. First, because the stability of the estimates will depend on the total number of (non-unique) control

firms, we report that. Second, because firms that are used in the control group of multiple events will be counted each time, we report the number of unique firms in the control group. Finally, we also report the total number of treatment events.

Appendix Figure C.6 shows the distribution of control firms across events. Additionally, it is likely that there are trends in which firms gain connections and when they gain them. For instance, wave elections causing significant partisan turnover would provide connections to certain types of firms at specific points in time, which could bias our results. Matching exactly on treatment histories—thereby conditioning on past connections—is an effective way of dealing with this source of confounding. Additionally, in Appendix D.6 we further probe whether there are partisan cycles in the effects—e.g. because of wave elections—that could bias our estimates. The results indicate that the matching on treatment history is an effective solution. Finally, Appendix Figure C.7 compares treatment and control firms in terms of size, and shows how matching effectively deals with the differences.

Results

We now present the results of several matched difference-in-differences estimations. Figure 4 compares revenue trends between treated and control firms leading up to the semester where a firm sees a lobbyist enter government service. Two important points arise. First, differences in revenue remain notably steady in the period prior to the time when some firms receive a connection. This shows that treatment and control firms follow parallel trends before treatment. In Appendix Figure C.7, we show that this is the case for all covariates for which we have data. Second, the revenue of connected firms increases in the first treated semester. The change compared to the pre-treatment period is sizable, indicating a large effect of gaining connections on firm revenue.

Next, we present formal estimates of the size of the difference-in-differences. Panel A in Table 2[16] shows the unadjusted contemporaneous (t=0) effect of having a lobbyist going

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[16]As mentioned previously, note that there are fewer treatment events in the specifications presented in
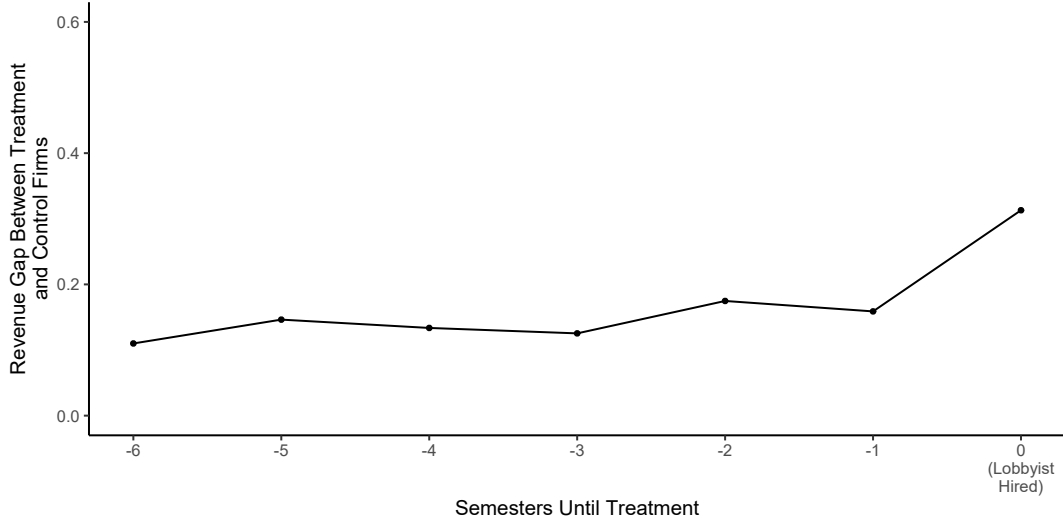
Figure 4: Firm Revenue Leading Up To Connection. *Note: Points are differences in mean revenue between treatment and control firms. Means are adjusted for pre-treatment numbers of clients and lobbyists (both logged).*

into government on firm revenue, and how this effect changes over time from one semester into the future (time t+1) through four semesters into the future (time t+4). It is clear that there is a strong effect of gaining a connection on revenue in the short term. We estimate an increase of approximately 36% in the first two semesters after the appointment. This increase is larger but of a similar magnitude to previous research on the value of connections in lobbying, including Blanes i Vidal et al. (2012) who find a 24% decrease in the average lobbyist own earnings when they *lose* a political connection.

In Panel B, we adjust the estimates for various pre-treatment measures of firm size, including firm revenue, the number of lobbyists employed in the firm, and its number of clients. We match on these three variables in a window of four semesters prior to treatment as described above. This is an important adjustment as it ensures that newly connected firms are matched with highly similar firms in terms of size generating realistic counterfactual firms. This adjustment increases the magnitude of the coefficient as well as the statistical precision up to t+3.

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the table than in the full data set. This is due to the exact matching on treatment histories. In Appendix D.8 we show that the results are robust to a shorter (consequently including more observation), and a longer lag length (excluding more observations but obtaining more plausible identification).

We also inspect heterogeneity depending on firm size. In Appendix D.9 we do this using a quantile difference-in-differences estimator (Callaway and Li 2019), allowing us to estimate how the ATT varies across the entire distribution of revenue. The estimator is very demanding in terms of data, and the results are noisy. However, they do suggest that low and high revenue firms see low returns to connections – the effects are driven by firms between the 25th and 75th percentile.

Table 2: Lobbyists Turning Government Employee 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					
Lobbyist Becomes Government Employee	0.342***	0.382***	0.186	0.103	0.221
	(0.119)	(0.118)	(0.139)	(0.184)	(0.187)
Panel B: Adjusted Estimates					
Lobbyist Becomes Government Employee	0.396***	0.411***	0.251*	0.105	0.145
	(0.120)	(0.120)	(0.140)	(0.184)	(0.189)
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. 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

Mechanisms and Heterogeneity

Existing Lobbying Clients Pay More

Next, we interrogate from where the firm's revenue increase arises. In broad terms, there are two possibilities. First, firms might advertise their newly gained connections, attracting new clients. Depending on the general equilibrium with respect to other lobbying firms, the

increase in the number of clients could allow firms to charge more for their services. Second, instead of benefiting through gaining new clients, firms may feature their new connections as a premium service to their most important clients. This could come about, either because existing clients pay more per hour, or because they increase their use of the lobbying firms, thereby billing more hours. Since we cannot observe the hourly rates charged by the lobbying firm, we cannot distinguish between the two different mechanisms in the 'premium service' account.

In Table 3, we delve into this by examining the effect on a) number of clients and b) revenue per client. As we can see, the effects are very clearly driven by revenue per client, which increases by 32%. The lower limit of the 90% confidence interval would imply an increase of approximately $5,000 per client, while the upper end would imply an increase of $15,000 per client. Since the average lobbying firm has approximately 12 clients, this amounts to a large increase in revenue, corresponding to the results presented in Table 2. On the other hand, we estimate a very precise null effect of connections on the number of clients. The upper limit of the 90% confidence interval implies an increase of less than 7% in the number of clients, corresponding to less than one additional new client.[17]

Substantively, then, we suggest that this is evidence that firms generally do not change their business model when acquiring new connections through a former employer entering government service. They continue with similar portfolios of clients and numbers of filings, but are able to either charge existing clients more for their services, or to bill more hours to those same clients. In the appendix we investigate another possibility for this increase: do firms hire more lobbyists, resulting in more net revenue but potentially less revenue per lobbyist? The results, presented in Appendix Table D.5, are imprecise, small, and centered around zero, suggesting no change in the number of lobbyists a firm employs. While this suggests that firms do not change their business model, it is still possible that firms with

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[17] In Appendix Table D.6 we run the same models using number of lobbying filings and revenue per filing. These values are closely correlated by not exactly the same, since firms report one filing per client. However, since we aggregate from quarterly filings to semesterly filings in the post-2007 period, there are some differences. Results are nearly identical to Table 3.

certain strategies benefit more than others. In particular, depending on why clients value political connections, firms that are either generalist or highly specialized could benefit more. In Appendix D.8 we investigate this by constructing a measure of firm specialization, drawing on the issue areas reported on their lobbying reports. Using this to split the data into specialist and generalist firms suggests that the effect of gaining connections might be larger among firms relying on specialist knowledge. However, there are few treatment events and the estimates are noisy. Importantly, the results are consistently positive, large and statistically significant among generalist firms, indicating that our results are not driven by demand shocks, impacting firms with different levels of specialization differently.

Table 3: Lobbyists Turning Government Employee and ln Total Clients

	Dependent variable:				
	ln Revenue per Client				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: ln Number of Clients					
Lobbyist Becomes Government Employee	0.015	−0.012	0.009	−0.016	0.009
	(0.023)	(0.026)	(0.027)	(0.034)	(0.039)
Panel B: ln(Revenue / Client)					
Lobbyist Becomes Government Employee	0.327***	0.394***	0.177	0.119	0.211
	(0.114)	(0.109)	(0.128)	(0.166)	(0.167)
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

Connected Firms Rely More on Fewer Clients

An open question is how much the political connection shapes the business model of lobbying firms. One way of examining this is to estimate how acquiring a connection shapes the reliance on groups of clients. If the connection is offered as a premium service to certain

important clients, we could expect that the firm's reliance on that group in generating revenue increased. To get at this, we construct a Herfindahl-Hirschman Index (HHI) of client concentration for each firm.[18] In particular, we use each client's share of the firm's revenue to compute the index, which then captures how reliant each firm is on fewer clients. To deal with skew, we take the log of the index.

The results are shown in Table 4. We estimate that the HHI index increases by 4-7% on average when firms gain political connections. The standard deviation of the index is approximately 0.8, indicating that the connection increases concentration by just short of 10% of a standard deviation. While not precisely estimated, this is a substantial increase, and it is significant at the 10% level. However, it is not so large that it suggests that firms *fundamentally* change their business model – they are able to extract more revenue from particular existing clients. Adjusting for covariates does not appreciably change the point estimates, but it does cause the estimates to become more noisy. Overall, we take this provides suggestive evidence that connections might make firms slightly more reliant on smaller groups of clients.

Heterogeneity by Connection Type: Bureaucracy and Congress

Next, we investigate heterogeneity in the results by the type of connection lobbying firms gain. Specifically, we separate connections as either being to a congressional office or a federal agency. The vast majority of work examining the value of revolving door connections focuses on Congress—indeed, we are not aware of similar work on the bureaucracy; however, as described above, a significant number of individuals enter into agencies directly from lobbying.[19]

There are reasons to think these connections may have differential value to lobbying firms. Prior theoretical work suggests connections to members of Congress are valuable due

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[18]Specifically, for a given firm i with n specific clients j we calculate the following quantity: $HHI_i = \sum_{j=1}^{n}(client\_revenue_j / firm\_revenue_i)^2$

[19]Though beyond the scope of this paper, a significant number of individuals also revolve into lobbying from agencies (e.g., Lupkin 2018).

Table 4: Lobbyists Turning Government Employee and Lobby Firm Revenue

	Dependent variable:				
	Client Concentration (ln HHI)				
	t=0	t+1	t+2	t+3	t+4
	(1)	(2)	(3)	(4)	(5)
Panel A: Unadjusted Estimates					
Lobbyist Becomes Government Employee	0.049	0.072*	0.067*	0.006	0.048
	(0.036)	(0.040)	(0.039)	(0.039)	(0.040)
Panel B: Adjusted Estimates					
Lobbyist Becomes Government Employee	0.046	0.062	0.056	−0.011	0.033
	(0.038)	(0.039)	(0.041)	(0.040)	(0.043)
Treatment Events	142	142	142	142	142
Control Firms	103,373	103,373	103,373	103,373	103,373
Unique Controls	36,144	36,144	36,144	36,144	36,144

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

to subsidy lobbying (Hall and Deardorff 2006). These connections then are particularly valuable for both lobbying firms and members of Congress, as it enables both parties to better create policy aligned with their ideal points. On the other hand, connections to federal agencies are slightly more complicated. Rulemaking is a diffuse, complicated process and it is less clear of the specific subsidy provided by lobbyists (You 2017). However, the complications of rulemaking may also mean firms and their clients benefit through gaining an understanding of the behind-the-scenes process. Thus, connections to the bureaucracy still may enable subsidy and access to important information, resulting in value to the firm. However, since agencies are much larger than congressional offices, an individual connection to an agency employee is potentially less valuable than that of one to a member of Congress.

We analyze the differential value in these connections through the same model specifications as above but with separate treatments for gaining congressional or executive branch connections. Tables 5 and 6 show the results for staffers and bureaucrats, respectively. Panels A and B show results with and without adjustments for firm size. The estimated revenue produced by a connection to a congressional staffer is larger than a connection to a bureaucrat. Additionally, we can easily reject the null for a connection to staff, while this is mostly not the case for bureaucrats.

As we can see from Table 6, we also estimate sizable effects of connections to bureaucrats. However, the results presented here are generally not statistically significant, without matching on covariates. To make sure that the apparent effect is not an artifact of model specification, below we describe a further investigation of the value of agency connections, focusing on the shock of Trump's election.

A pattern worth mentioning across both types of connections is that the value attenuates gradually overtime, with significant declines for bureaucratic connections. We suggest that there is not an ex ante expectation as to why – that is, we have no theoretical prior for a diminishing value of an established connection. The heterogeneity hints at some explanations, however. For instance, in Appendix Table D.10 we show that the political environment

predicts more or less volatility in over-time value. When government is divided, the connections seem more valuable. One explanation consistent with previous research for this finding is that divided government leads to higher policy uncertainty, which connections are valuable insurance against. We discuss more heterogeneity below that sheds light onto variation in the over-time value of political connections.

Table 5: Lobbyists Turning Staffer 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					
Lobbyist turns Staffer	0.418***	0.417***	0.254	0.276	0.330
	(0.150)	(0.148)	(0.186)	(0.229)	(0.226)
Panel B: Adjusted Estimates					
Lobbyist turns Staffer	0.432***	0.398***	0.264	0.207	0.189
	(0.152)	(0.151)	(0.189)	(0.230)	(0.230)
Treatment Events	106	106	106	106	106
Control Firms	78,384	78,384	78,384	78,384	78,384
Unique Controls	2,294	2,294	2,294	2,294	2,294

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 boot-strapped standard errors in parentheses. 1,000 trials used. *p<0.1; **p<0.05; ***p<0.01

Committee and Personal Staff

In the appendix, we use additional features available in the congressional staff data to analyze further heterogeneity in the results. One potential source of variation in the value of congressional staff connections for lobbying firms is the type of employment possessed by

Table 6: Lobbyists Turning Bureaucrat 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					
Lobbyist turns Bureaucrat	0.137	0.195*	−0.019	−0.263	0.002
	(0.121)	(0.116)	(0.073)	(0.223)	(0.243)
Panel B: Adjusted Estimates					
Lobbyist turns Bureaucrat	0.248*	0.250**	0.108	−0.105	0.040
	(0.128)	(0.121)	(0.085)	(0.229)	(0.248)
Treatment Events	48	48	48	48	48
Control Firms	44,473	44,473	44,473	44,473	44,473
Unique Controls	2,302	2,302	2,302	2,302	2,302

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

the staffer. Appendix Tables A.1 and A.2 display descriptive statistics on lobbyists as committee staffers and personal staffers, respectively. Previous research has found heterogeneity in the value of different staff backgrounds for revolving door lobbyists, with committee staff generally associated with less revenue upon revolving into lobbying (Bertrand et al. 2014; McCrain 2018).[20] An implication from this result is that there may be differential value in lobbying firms acquiring connections to committee staff versus staff in personal offices of members of Congress. In Appendix Tables D.4 we present results separating out these varying types of connections as separate treatments. We find similar results to Table 5 overall, however connections gained to committee staff are more valuable over time to lobbying firms. This suggests an interesting implication for future research, focusing on the value of different types of congressional connections from the lobbying firm's perspective.

An Out-of-Sample Test of Bureaucratic Connections

Our investigation of gaining a connection to the executive branch yields noisy results. In Appendix E, we further probe this finding by providing an out-of-sample replication of the results on connections to the bureaucracy. This test provides confidence that our positive finding is not purely an artifact of our model specification. To do so, we collect an additional dataset consisting of the lobbyists appointed during the beginning of the Trump Administration. Studying this period is helpful, because it allows us to hold constant the partisan environment. We are also able to use precise timing of the appointments, thanks to a data collection effort by the Center for Responsive Politics, to compare similar firms to each other based on appointment timing in a traditional difference-in-differences framework.[21] We find 35 contract lobbyists appointed in the first two quarters of 2017, our period of analysis, with 12 appointed in the first quarter. The full results, presented in Appendix Table E.17,

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[20]An explanation for the differential value is that committee staff tend to specialize in a given policy area, whereas personal office staff are more generalists. Personal office staff are also more likely to be broadly connected across Capitol Hill, a valuable asset for lobbyists. This variation, however, has not been explored in the reverse direction: when lobbying firms gain connections through seeing their lobbyists enter Congress.

[21]The data aggregation and empirical strategy are discussed in greater detail in Appendix E

show a precisely estimated effect similar in magnitude to that of the main staff results –
an approximate 36% increase in revenue (significant at $p < .01$).[22] One possibility in the
pooled analysis above, then, is that the noise in these estimates is a product of the staggered
DiD approach being less efficient (Imai et al. 2021). Alternately, the shock of the election
victory and unprecedented (in terms of magnitude) hiring of lobbyists was truly a boon to
the lobbying firms able to take advantage. In any case, this exercise suggests that connec-
tions to the bureaucracy do, indeed, matter. Additionally, we use the fact that the partisan
environment is held fixed in this setting to rule out spurious relationships created by partisan
demand shocks. Specifically, we collect data on the campaign donations of the lobbyists in
our dataset on Trump appointments to measure the partisanship of the lobbying firms. We
then investigate whether there are heterogeneous effects of connections depending on firm
partisanship. We estimate almost exactly the same effects for Democratically aligned firms
and bipartisan firms as we do for Republican firms. This suggests that our findings are not
driven by partisan demand shocks.

Returning to Lobbying?

The primary focus of the above analyses has been on the benefit to lobbying firms specifically
once they gain connections. However, an outstanding and somewhat puzzling feature of this
phenomenon is the willingness of highly paid multi-client lobbyists to enter into lower paid
and frequently less prestigious government jobs. At the individual lobbyist level, there are
multiple possibilities driving this choice. For instance, these individuals may desire a more
stable and less demanding job as a federal employee, or they may be particularly public
service motivated and wish to use their expertise gained in lobbying for the benefit of public
policymaking (Perry and Wise 1990). Additionally, if lobbying is in fact a connections-based
business as the evidence suggests, individuals likely benefit from refreshing their existing
connections through renewed time in government. This new public service experience would

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[22]We also show in the appendix these results are robust to a number of alternative specifications.

also provide new substantive expertise. In short, there are many individual-level human capital traits that lobbyists can bolster through giving up cushy lobbying jobs for government service.

To better understand these motivations, we examine the career trajectories of the individuals we identify as revolving into government from the lobbying industry. We note that this is a descriptive exercise given truncation of our sample (some might not have *yet* returned to lobbying) and the fact that much of what we can observe is post-treatment. Nonetheless, the quantities are illustrative of features of this labor market. Of the 815 firm lobbyists comprising our revolving door sample, 289 (35%) ultimately return to firm lobbying after their stint in government. 156 of these lobbyists return to the firm from which they initially departed. The average tenure in government is just over 3.5 years.

Additionally, their lobbying activity upon return, relative to their activity before entering government, is suggestive of individual benefits to human capital. These lobbyists experience an average revenue gain of over $16,000 in their first year back in the lobbying industry and an increase in 2 additional contracts. Substantial heterogeneity exists based on the type of government service: individuals who re-enter lobbying after congressional experience see a much larger gain of $52,000 in revenue compared to a substantial decrease of $110,000 in revenue for those who return from agency experience. Again, we note these quantities are suggestive evidence of individual-level career concerns of these lobbyists. A fruitful area for future research would be to fully document the career trajectories of top bureaucrats and congressional staff and further examine heterogeneities that drive some individuals to seek private sector jobs and others to remain in government.[23] We believe a more systematic analysis of these individuals might also highlight why some connections remain

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[23]In Appendix Table D.8, in the same staggered DiD framework, we explicitly analyze the shock to firms who *lose* connections once their prior employee leaves government (in other words, the lobbyist who revolved *into* government again revolves *out* of government.) We again caution interpretation of these results due to sample truncation and post-treatment bias. We find a negative shock to firms' revenue, consistent with results from Blanes i Vidal et al. (2012), however the coefficient is smaller and imprecisely estimated. Substantively, the initial revolving event, when lobbyists enter government, may produce the desired effect from the firm's perspective of refreshing connections, attenuating the future loss of the direct connection on government exit (so, still a net positive event in the eyes of the firm).

valuable to firms and others do not. Are firms that start to lose value in connections also those most likely to see their lobbyists return?

Robustness and Alternative Explanations

In this section we consider alternative explanations for the results presented previously. We find the results are robust to sensitivity analyses for unobserved confounders, alternative specification choices, accounting for partisanship of connections gained, and splitting the sample based on pre- and post-regulatory change.

Sensitivity and Modeling Choices

The primary threat to inference in the research design above (and similar research designs in political economy research) is an unobserved confounder that produces large increases in revenue at the same time that a lobbying firm sees one of its lobbyists enter into a government job. In other words, the confounder should at the same time make selection into a connection much more likely and increase revenue considerably. We have outlined an array of steps taken to guard against such confounders. However, since we by definition never can be certain that we have dealt with all sources of bias, we present a sensitivity analysis of how strong a confounder would have to be to invalidate our we results. We draw on Keele et al. (2019), who show that in a 2x2 difference-in-differences framework the sensitivity of the estimated ATT can be computed by using the methods for matched pairs proposed by Rosenbaum (2002). The method proposed by Imai et al. (2021) essentially identifies all treatment events and computes a 2x2 difference-in-differences for each. Therefore, it is straightforward to extend this approach by simply extracting each individual difference-in-differences estimate and testing the sensitivity of the average of them all.

The results are presented in Figure 5. The horizontal axis shows simulated scenarios where we vary the strength of self-selection among lobbying firms. In the lower end of the

axis, treatment and control firms are equally likely to gain a connection. This corresponds to our baseline scenario. We then use the Rosenbaum (2002) permutation test to investigate the effect of treated firms being able to self-select into government connections. We plot both the lower and upper bound of the ATT, and consider all scenarios up to and including the case when the confounder makes treatment five times more likely in the treatment group.

Richardson et al. (2014) suggest using the point of sign-change as the point where the results do not hold any longer. As we can see, the lower-bound of the ATT does not cross zero until the confounder makes a connection more than four times more likely in the treatment group.

To put these results into context, in our dataset, the strongest predictor of gaining a connection is firm size as measured by the logged number of contracts. For reference, firms at the 75th percentile are approximately 3.5 times more likely to be connected compared to firms at the 25th percentile. This suggests that any unobserved confounder would have to increase the likelihood of a connection by more than the difference between large and small firms. Importantly, this change would have to set in suddenly and shape revenue within a single semester to change our conclusion. This suggests that our results are highly robust to the presence of potentially unobserved confounders.

To further assess the robustness of our estimation strategy, we analyze one choice that must be made when using estimators such as the Imai et al. (2021) estimator: how long of a window to choose for lagged periods to construct appropriate control groups based on treatment histories. In Appendix Figure D.8 we show that our results are not sensitive to this choice; across a variety of lag lengths the results remain unchanged and, with only a few exceptions, statistically significant.[24]

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[24]We additionally show in Appendix Figure C.7 that there is no evidence of differences in pre-trends among observable covariates.
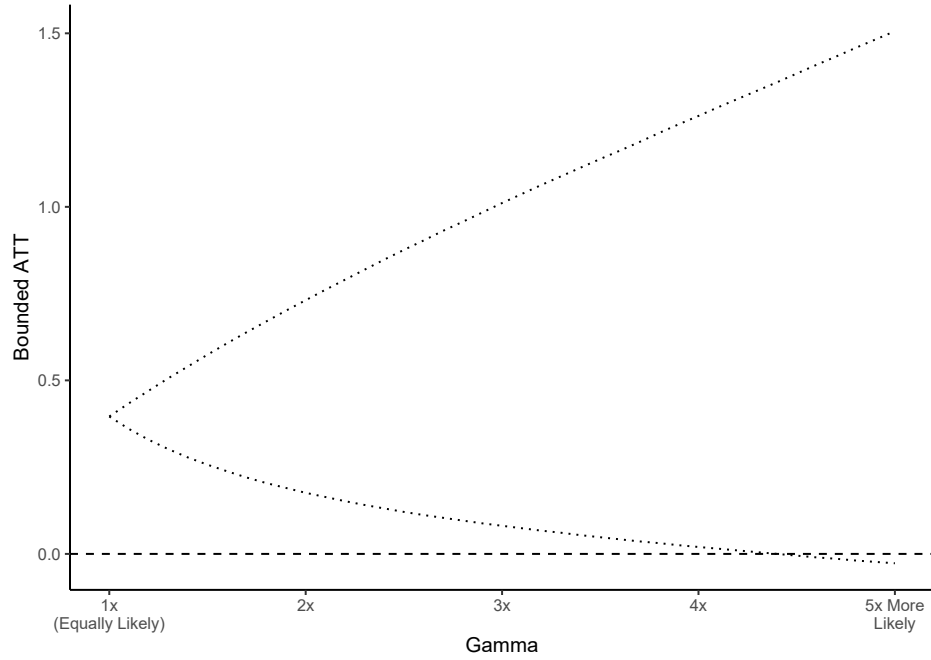
Figure 5: Bounding the ATT. *Note: The figure plots the bounded ATT of gaining a connection on revenue, using the Rosenbaum (2002) permutation test. Gamma denotes how much more likely the treated firms are to gain a connection, because of an unobserved confounder. Estimates are based on the model that adjusts for pre-treatment revenue, number of lobbyists and number of contracts.*

Alternative Explanations

Next we consider alternative explanations that may drive the primary results presented above. First, it is possible that the results on gaining congressional staff connections hide interesting heterogeneity and the value is primarily driven by gaining *certain* staff connections. Notably, existing research has found different revolving door markets for staff with committee experience versus those in member personal offices (Cain and Drutman 2014; McCrain 2018). In Appendix Table D.4 we show positive increases associated with gaining both types of staff connections. However, gaining connections through staff who enter into committee offices is more persistently valuable and more precisely estimated. A fruitful area for future research would be to delve further into the heterogeneity of lobbyist connections by type of office in which staff work.

Similarly, we analyze partisan differences in the type of staff connections gained in Appendix D.6. An important alternative explanation to the one we advance is that firms may gain in revenue when their partisanship matches that of an incoming administration or a majority in one of the chambers in Congress. If so, our results would be driven by firms having an easier time gaining access to likeminded politicians rather than the entry of one of their lobbyists into a government job. Table D.9 shows the effects split out on parties, and suggest there are relatively minor partisan differences and little difference based on the partisanship of the office staff enter into. 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. It turns out that during periods where the return to a Democratic connection is high, so is the return to a Republican connection.[25]

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[25]Appendix Table D.10 also investigates heterogeneity by whether lobbyists enter into government during unified or divided government. In general, both cases see positive returns to the firm, with some heterogeneity based on whether the lobbyist becomes a staffer (unified government results in higher returns) versus a government (divided government results in higher returns).

A different, but related, concern could be that changes to the partisan environment could increase client's demand for firms with certain partisan leanings, while the labor of staffers with certain backgrounds also increase. These differential shocks to partisan demand could drive the association between gaining connections and revenue. In Appendix D.10, we investigate this by zooming in on the final two years of presidential administrations, where Congress tends to be unproductive. In those periods, therefore, little sudden demand should arise for firms and staffers with certain partisan leanings. Thereby, this holds constant differential partisan demand shocks. Importantly, the results during lame duck periods are almost identical to the more general estimates.

Another explanation for our results is that firms who lose lobbyists to government compensate by hiring multiple additional lobbyists immediately afterwards, increasing the revenue through increasing their staff. In Appendix Table D.5, we analyze this possibility using the same modeling strategy as above, with the outcome variable the change in number of lobbyists in a firm. We find no evidence that firms hire additional lobbyists. While there is an increase of less than one half of a lobbyist hired in the 3rd post-treatment period, it washes out in the subsequent periods.

Finally, we assess the possibility that changes to regulations around revolving door lobbying produced by the Honest Leadership and Open Government Act of 2007 (HLOGA) produced differential value in the connections firms gained to lobbyists. While these regulations only affected individuals revolving *out* of government, by introducing "cooling off periods", it is possible that it produced changes to the macro revolving door political economy. We split the sample to analyze pre- and post-HLOGA value of connections using the same difference-in-differences strategy with results presented in Appendix Table D.7. We find larger effects in the pre- period, but find substantively similar effects in the post-period, though slightly smaller and somewhat less precise. We return to a discussion of regulatory changes in the conclusion.

Discussion and Conclusion

The revolving door is a well-documented source of access for special interests into the policymaking process. A large amount of scholarly evidence aligns with journalistic accounts of the phenomenon, demonstrating individuals revolving from government into lobbying are financially rewarded based on their experience and, specifically, based on their connections to individuals in government. Scholarly evidence of revolving door lobbying, however, primarily focuses on one direction of revolving: from government into lobbying. In this paper we analyzed the reverse direction, when lobbyists become public employees, and find this method of forging connections produces benefits to lobbying firms.

Our findings suggest firms experience substantial shocks to their revenue when they gain connections to policymakers through a previous employee entering government service. We find that firms see a roughly 36% increase in lobbying revenue over the first year after which they gain a connection—an increase of $320,000. We further document evidence for the mechanism of this increase: they are able to generate additional revenue from their existing clients but do not necessarily gain new clients. Given the large estimated increase in revenue we uncover, one could imagine that firms are strategic about encouraging their employees to seek government employment for promises of future benefits once they complete their government service (e.g., Rappeport 2018). Some suggestive evidence lends credence to this explanation, with nearly 20% of all identified lobbyists-as-government-employees eventually returning to their previous lobbying firm. Additionally, we uncover that connections do not necessarily remain persistently valuable, something that has not yet been clarified in the empirical literature. We highlight heterogeneity to this effect: connections to congressional committees, for instance, retain high value after the initial shock whereas connections to agencies quickly diminish in value.

An additional contribution of our analysis is exploring heterogeneity in the type of connection gained based on political institution. The primary focus of existing research is on congressional staff or members of Congress who then become lobbyists. We employ

two unique datasets of federal agency officials who were first lobbyists and find differences based on this background. On average, firms see larger revenue increases when they gain connections to congressional offices (roughly 40%) than to government agencies (20%). The congressional connection result is about twice the magnitude of the value of connections found in previous research (e.g., Blanes i Vidal et al. 2012). We further investigate the value of executive branch connections using the Trump administration's well-documented hiring of lobbyists to fill agency spots. In this setting, firms saw revenue increases similar in magnitude to when they gain congressional connections in the full results. Along with the importance of establishing this heterogeneity, we also describe the types of congressional offices and committees that hire lobbyists, and how these lobbyists differ from lobbyists from similar firms that do not see their employees become staffers. These descriptive patterns deserve further detailed research, combining in-depth qualitative studies of the inner workings of firms and the hiring practices of congressional offices with the available data we describe.

The research design used in this paper is an important methodological contribution to the study of connections in lobbying and the non-market strategy of firms' political connections more generally (Hillman 2005; Hillman et al. 1999). A concern in previous work is that appropriate counterfactuals do not exist for un-treated firms (i.e., those that do not gain connections). One approach in this literature to address this has been through the use of two-way fixed effects research designs, which holds fixed unobserved time-invariant features of the firm which produces identification through gaining or losing a connection. However, building from recent advances on this research design approach (Goodman-Bacon 2018; Imai et al. 2021) and as we formalize and describe in detail in this paper, this approach can produce biased estimates of the true treatment effect. This bias is also theoretically relevant to the study of political connections. As Goodman-Bacon (2018) documents, if the treatment effect is likely to vary over time—which is almost certainly the case in the study of lobbying and access—then the bias will be worse. We suggest that future work on the political economy of lobbying adopts recent techniques on difference-in-differences with staggered

treatment timing when appropriate.

The implications of these results, combined with other work on the value of political connections, depict a complicated environment for regulators who wish to reduce private access in politics through the revolving door. Existing regulations only affect individuals who seek lobbying jobs after government employment. However, some presidential administrations announced they would not hire registered lobbyists (Crabtree 2010). Regulators who wish to limit the ex post careers of government employees must confront the equilibrium selection effects this has on who chooses to enter government in the first place. Similarly, despite normative concerns of former lobbyists working directly in government roles, these individuals are often leading experts in their policy area and may increase the effectiveness of certain agencies or policies on the margin. An optimal regulatory scheme from a social welfare perspective is not straightforward, with some obvious-sounding restrictions to revolving door lobbying being potentially net worse than the status quo.

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 a recent exception, Hübert and Rezaee (2019) formalizes a setting in which this may actually *decrease* the influence of special interests in policymaking. Future work looking into the career paths of these revolvers would provide more insights into this phenomenon and would establish important stylized facts to better understand this normatively important process. 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. Are bureaucrats who work in highly-regulated industry-areas differentially valuable as lobbyists? Filling in these details also serves to progress our knowledge of how lobbying and special interests impact policy outcomes. 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.

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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.
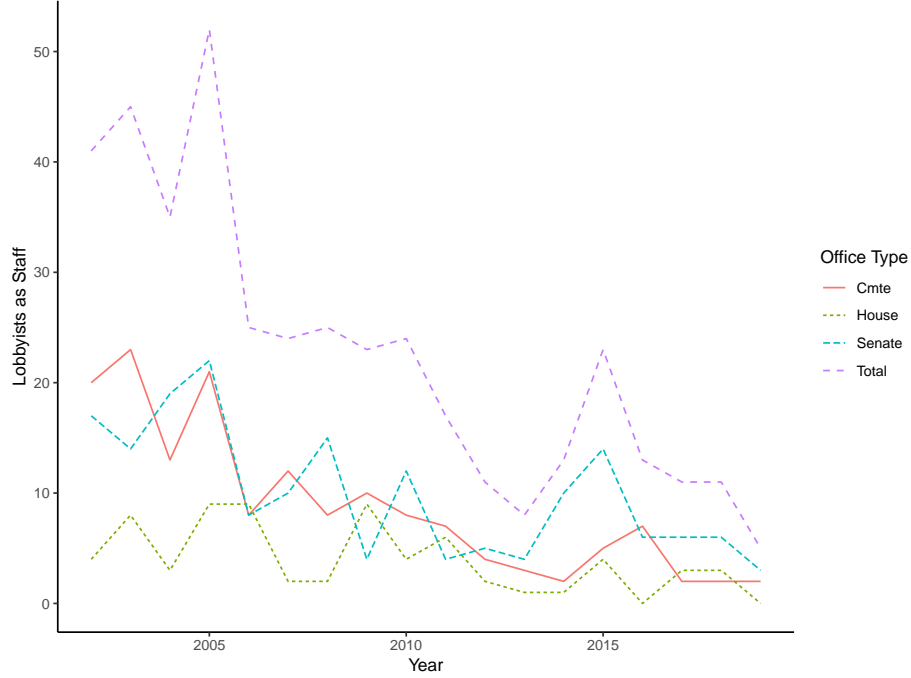
Figure A.1: Lobbyist Destinations among Congressional Staff.

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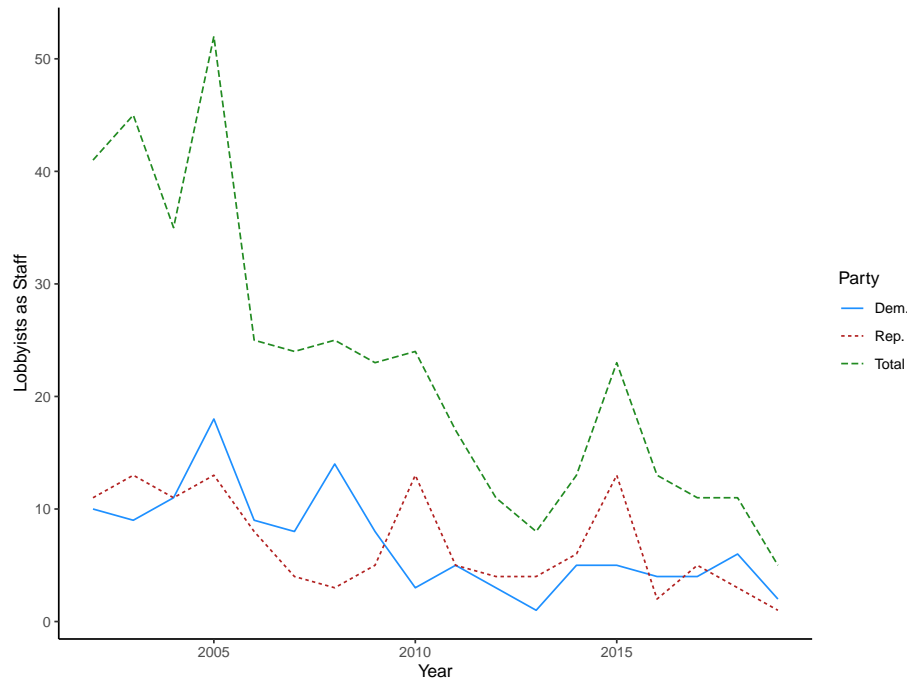
Figure A.2: Destinations by Party among Congressional Staff.

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

Committee	Total Lobbyists
House Energy and Commerce Committee	12
Senate Indian Affairs Committee	10
House Transportation and Infrastructure Committee	9
House Homeland Security Committee	8
House Science, Space and Technology Committee	8
House Appropriations Committee	7
House Ways and Means Committee	7
Senate Commerce, Science and Transportation Committee	7
Senate Finance Committee	7
House Financial Services Committee	6
House Natural Resources Committee	6
Senate Judiciary Committee	6
House Judiciary Committee	5
Senate Agriculture, Nutrition and Forestry Committee	5
Senate Homeland Security and Governmental Affairs 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).

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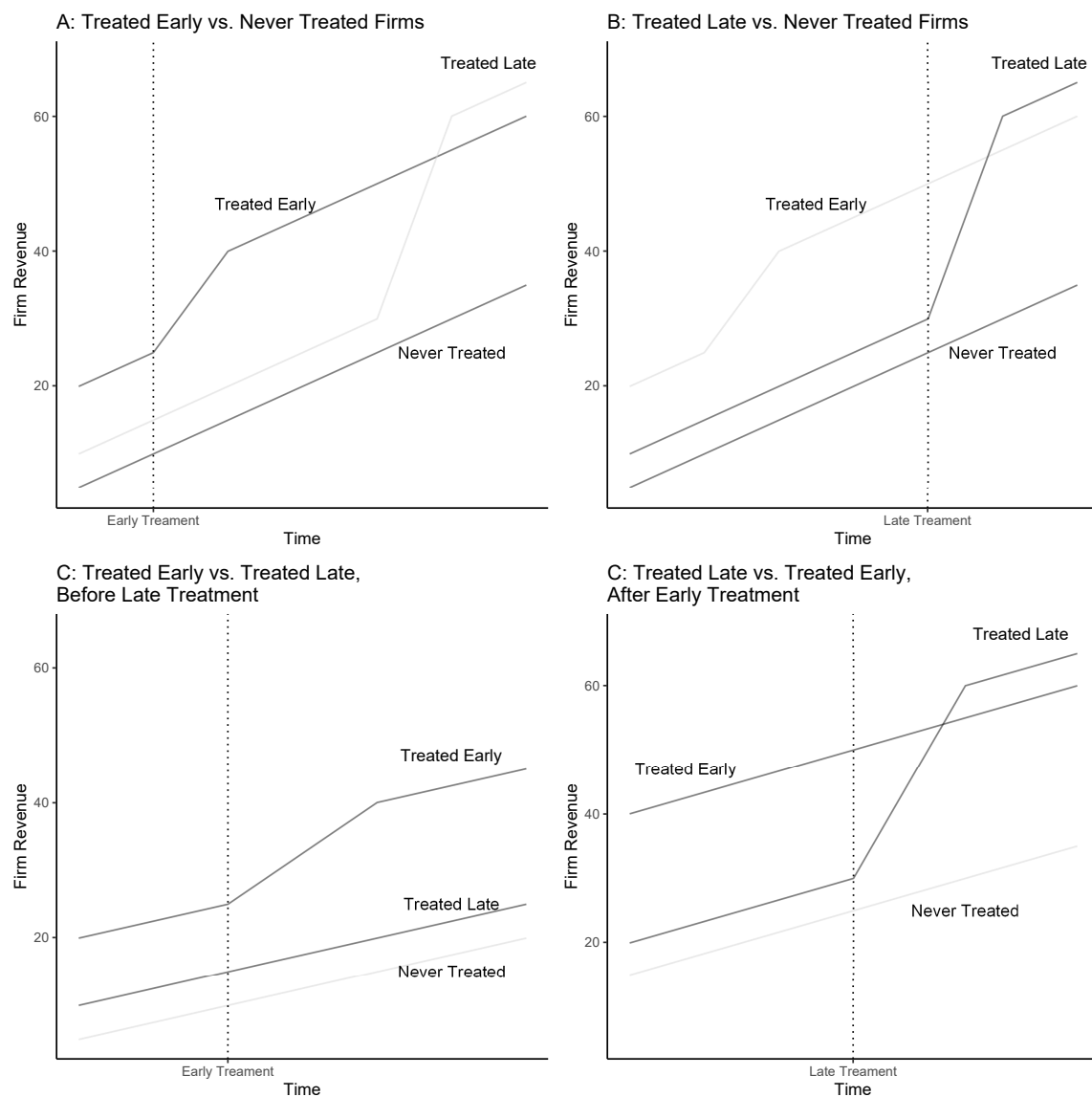
Figure B.3: Four Differences-in-Differences From Three Treatment Groups. *Note: Based on Figure 2 in Goodman-Bacon (2018). Group that is not part of comparison is lightest gray.*

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

$$\overbrace{[\Delta Y_k^0(post(k), pre(k)) - \Delta Y_U^0(post(k), pre(k))]}^{\text{PT(post(k,U))}}$$

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:

$$plim \ \delta^{TWFE} = \delta^{TWFE} = vwATT + vwPT - \Delta vwATT \qquad (2)$$

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

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

yield highly biased results.
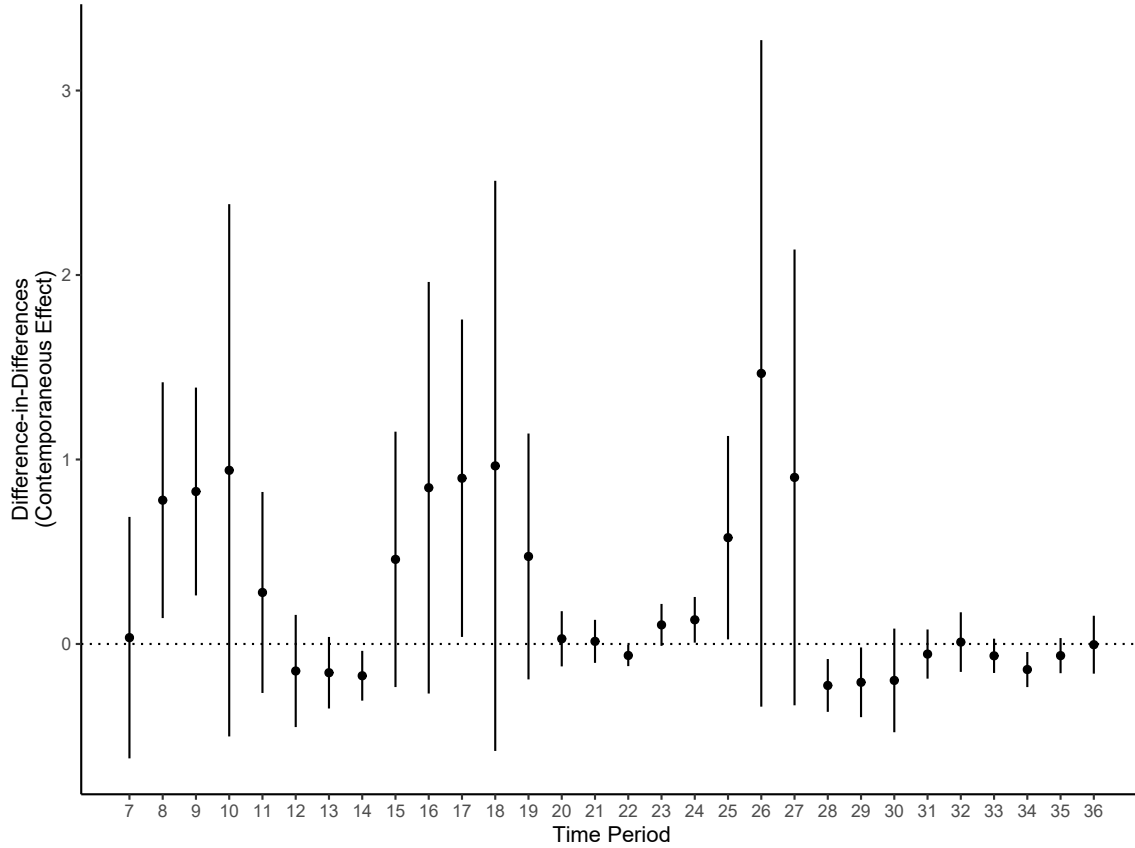
Figure B.4: How ATTs Vary Over Time. *Note: The graph shows a rolling window analysis, where the baseline matched difference-in-differences is estimated on a subset of the time-periods in the data. To allow for the baseline lag and lead lengths, six periods prior to treatment and five post-treatment 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.
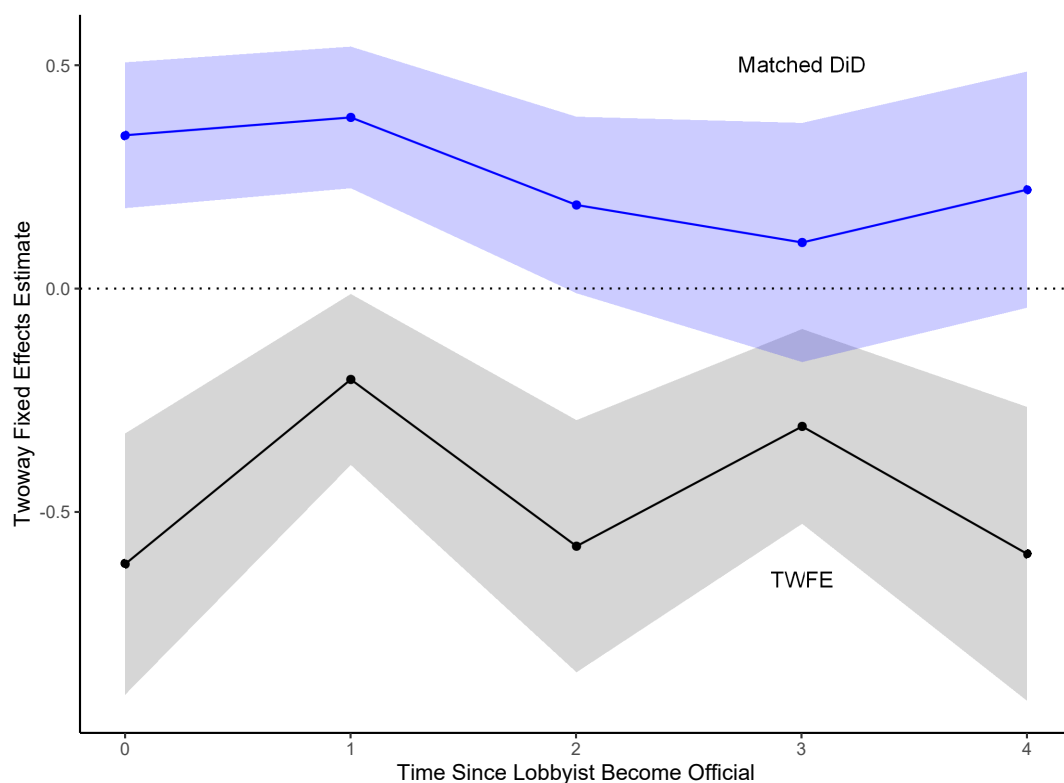
Figure B.5: Event-Study Twoway Fixed Effects Estimates. *Note: Gray-shaded esti-mates are from a regression with fixed effects for firm and time. Estimates are produced with dummy variables for relative event-time. The event is the first transition of a lobbyist into public service. Blue-shaded estimates are from the Imai-Kim-Wang differences-in-differences estimator. Shaded areas are 84% confidence intervals from firm-clustered robust standard er-rors. 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
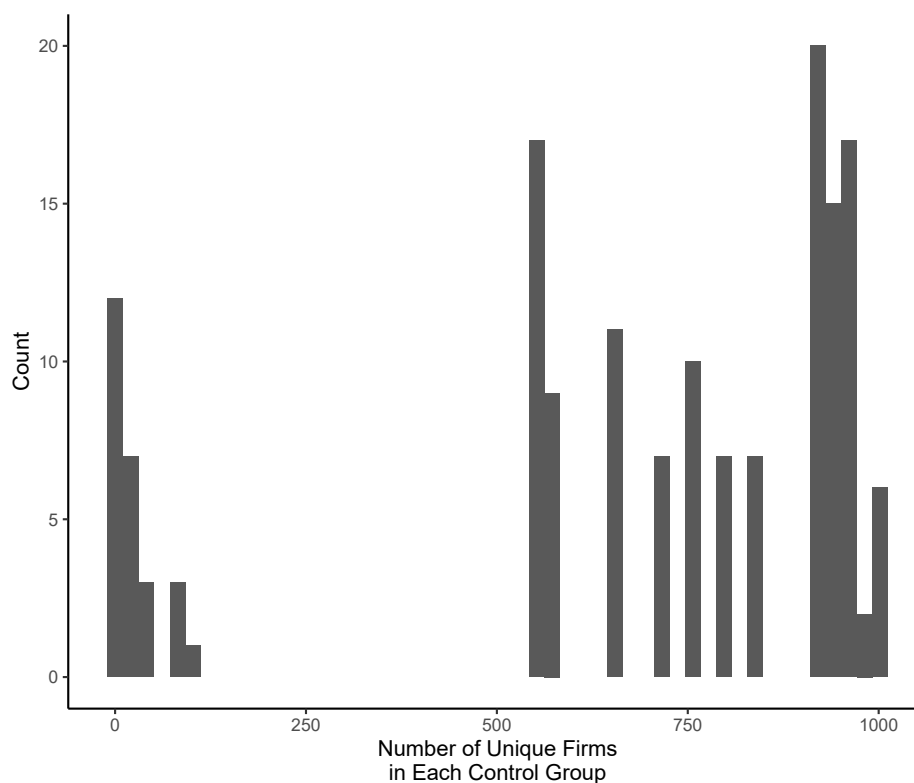
pre-trends before and after adjusting for the covariates.

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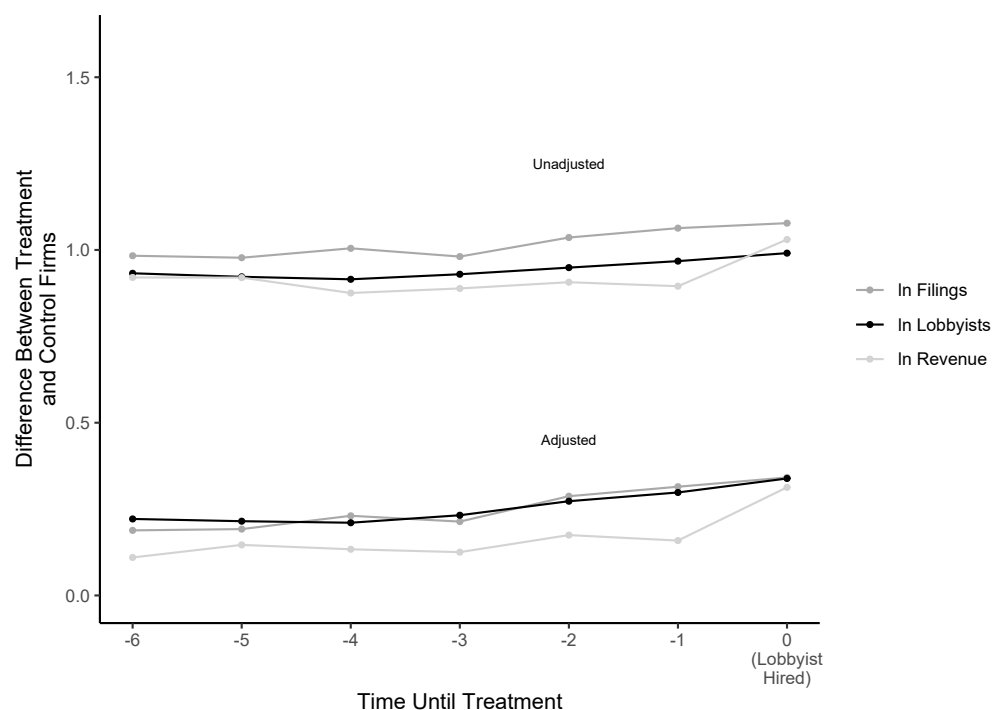
Figure C.7: No Differential Trends Prior to Treatment. *Note: The graph shows the trends in covariates and log revenue in the period leading up to treatment. Adjustments are conducted using the Mahalanobis distance between treatment and control firms.*

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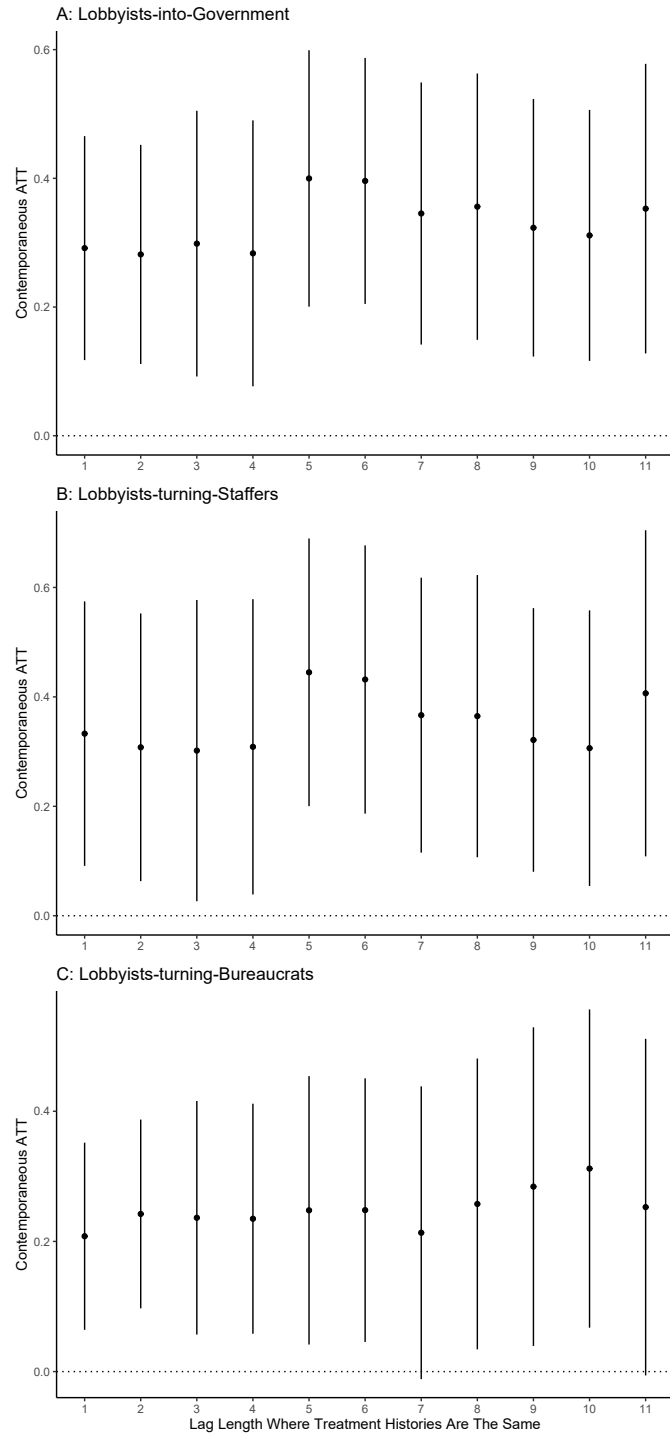
Figure D.8: Robustness to Varying Lag Lengths. *Note: Each point is the estimated contemporaneous ATT with a different lag window where treatment histories are constrained to be the same in the treatment and control groups.*

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.

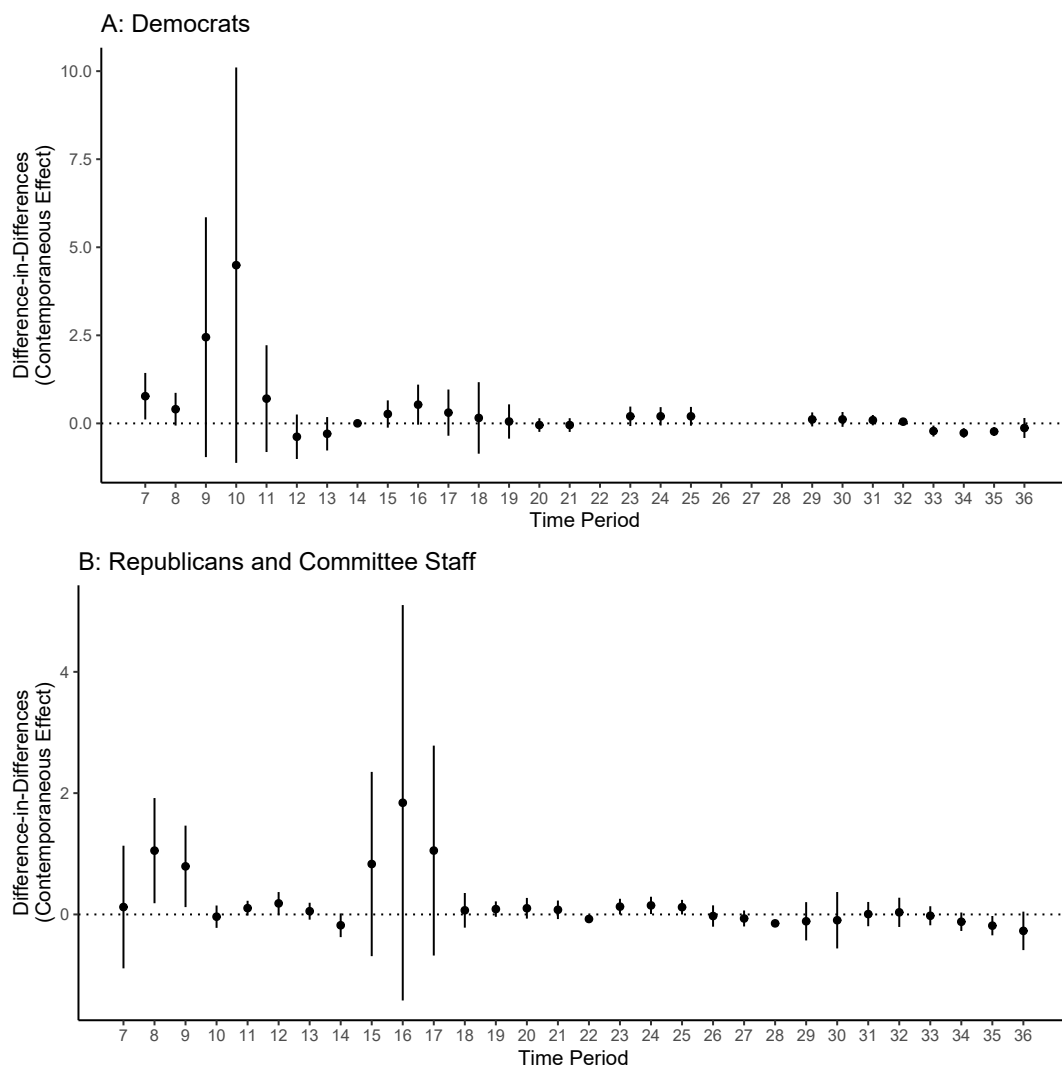**Figure D.9: Time-Varying Effects Depending on Party.** *Note: The graph shows a rolling window analysis, where the baseline matched difference-in-differences is estimated on a subset of the time-periods in the data, separately for Democrats and Republicans and committee staff. To allow for the baseline 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
	(0.349)	(0.314)	(0.339)	(0.296)	(0.320)
Panel B: Minority Staffer Connection					
Lobbyist as Staffer (in Minority)	0.193**	0.222**	−0.405*	−0.018	0.295
	(0.088)	(0.110)	(0.215)	(0.132)	(0.252)
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 D.12 , D.13, 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
	(0.569)	(0.441)	(0.519)	(0.973)	(1.036)
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)	(0.140)	(0.175)	(0.174)
Panel C: Combined Adjusted Results					
Lobbyist Becomes Government Employee	0.381***	0.401***	0.251*	0.115	0.187
	(0.113)	(0.111)	(0.133)	(0.177)	(0.177)
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 / Client				
	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
	(0.053)	(0.045)	(0.090)	(0.107)	(0.119)
Panel B: Generalist Firms (issues per client)					
Lobbyist Becomes Government Employee	0.012	−0.022	−0.013	−0.014	0.006
	(0.026)	(0.030)	(0.029)	(0.037)	(0.044)
Panel C: Combined Adjusted Results					
Lobbyist Becomes Government Employee	0.019	−0.004	0.018	−0.007	0.002
	(0.025)	(0.028)	(0.027)	(0.033)	(0.040)
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 Clients				
	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
	(0.576)	(0.428)	(0.562)	(0.965)	(1.015)
Panel B: Generalist Firms (issues per client)					
Lobbyist Becomes Government Employee	0.316***	0.359***	0.127	0.131	0.205
	(0.107)	(0.105)	(0.126)	(0.155)	(0.150)
Panel C: Combined Adjusted Results					
Lobbyist Becomes Government Employee	0.361***	0.405***	0.233*	0.123	0.185
	(0.109)	(0.102)	(0.121)	(0.161)	(0.164)
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.
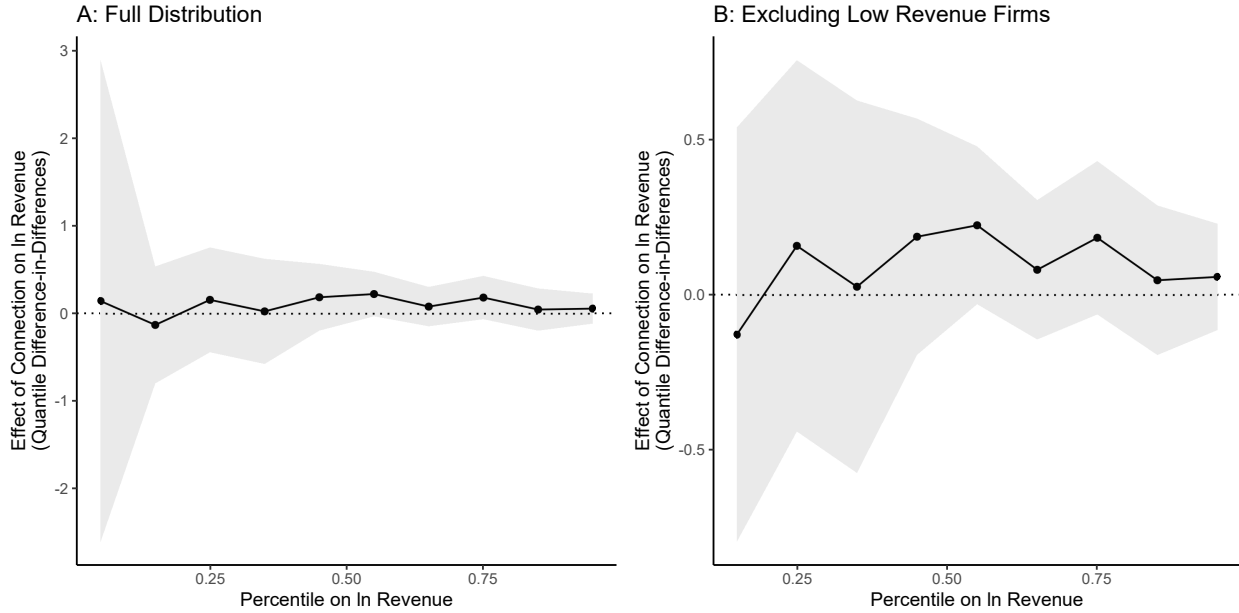
Figure D.10: Heterogeneous Effects by Revenue using Quantile Difference-in-Differences. *Note: The figure presents estimates from the Callaway and Li (2019) quantile difference-in-differences estimator. Treatment and control matches are generated using the baseline Imai et al. (2021) technique. The dataset 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	0.327***	0.410***	0.145	0.042	0.203
	(0.126)	(0.124)	(0.164)	(0.223)	(0.201)
Panel B: Republicans and Cmte Staff					
Lobby Firm Gains Connctions	0.362***	0.426***	0.172	0.024	0.087
	(0.128)	(0.126)	(0.167)	(0.227)	(0.205)
Treatment Events	109	109	109	109	109
Control Firms	80,457	80,457	80,457	80,457	80,457
Unique Controls	2332	2332	2332	2332	2332

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.

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[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.

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 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 dif-

ferences 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.

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

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.

We estimate the DiDs using variations of the following model:

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

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.

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

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

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

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.
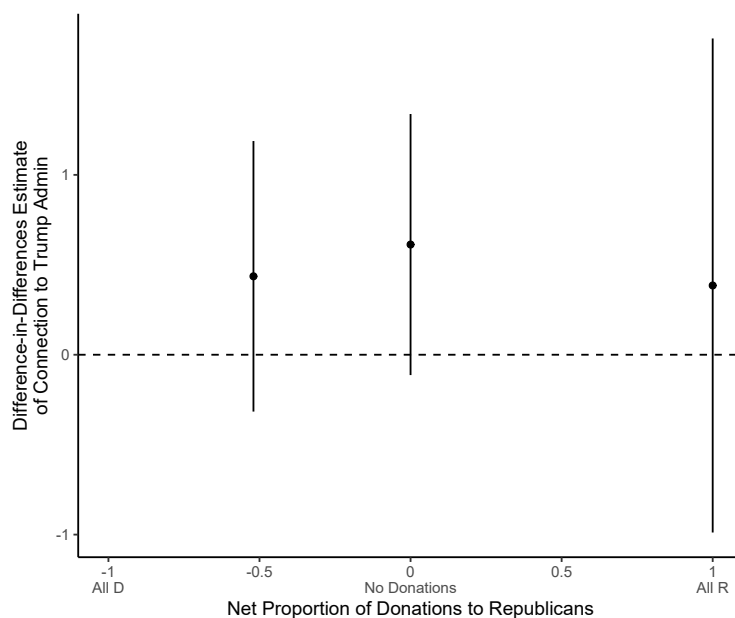

Figure E.11: Firm Partisanship Does Not Moderate the Effect. *Note: The figure shows estimates of how the effect of gaining a connection to the Trump administration varies depending on the lobbying firm's campaign donations. We measure donations using the net proportion of donations to Republicans. This 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.*