Shadow Lobbyists

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Abstract

Special interest influence via lobbying is increasingly controversial and legislative efforts to deal with this issue have centered on the principle of transparency. In this paper we evaluate the effectiveness of the current regulatory framework provided by the US Lobbying Disclosure Act (LDA). Specifically, we study the role of ex-Congressional officials who join US lobbying firms in positions that could be related to lobbying activity but without officially registering as lobbyists themselves. We find that firm lobbying revenues increase significantly when these potential 'shadow lobbyists' join, with effects in the range of 10-20% that mainly operate via the extensive margin of new clients. This shadow lobbyist revenue effect is comparable to the effect of a registered lobbyist at the median of the industry skill distribution. As such, it is challenging to reconcile the measured shadow lobbyist effect with the 20% working time threshold for registering as a lobbyist.

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1 Introduction

The lobbying of government by special interest groups is one of the most controversial features of modern democratic politics. This is because of the perceived risk of special interest capture that organized, well-funded and well-connected lobbying efforts pose for the policy-making process. Indeed, in the case of the US, federal lobbying is a highly developed and financially significant industry with tight connections to government via a 'revolving door' of shared personnel. Lobbying boomed in the US starting from the late 1990s. Between 1998 and 2013, the total amount spent as part of registered federal lobbying activity increased by 68% in real terms from $1.9 billion to $3.2 billion dollars.[1] The public concern about the role of lobbying was symbolized during the 2016 Presidential election by Donald Trump's strident call to 'drain the swamp' of lobbyist influence in Washington.

Attempts to regulate lobbying and combat the risks of special interest influence have been dominated by a single major policy tool: transparency. Beginning with the Foreign Agents Registration Act (FARA) in 1938 the guiding principle of lobbying regulation in the US has been to balance the freedom to lobby against obligations to report on the nature of the lobbying activity being conducted. Since 1995, the Lobbying Disclosure Act (LDA) has provided the main legal framework for the reporting of lobbying activity at the federal level. The LDA mandated the registration of individuals and organizations involved in lobbying, stipulating specific financial and working time thresholds for this registration requirement to be triggered.

However, in recent years concerns have emerged that the LDA is not very effective in capturing the full range of lobbying activities at play in Washington. In particular, attention has been drawn to the practical robustness of the working time and financial thresholds for reporting laid out in the LDA. Specifically, a key point of concern has been the so-called '20% rule' for registration. This rule effectively states that individuals who spend less than 20% of their working time on lobbying are not required to register their activities. It is not clear however that this 20% threshold is systematically monitored and enforced.

In particular, the lobbying activities of a number of ex-Congressmen have been questioned in light of the 20% rule. The most famous case is that of Thomas Daschle, former Democratic leader in the Senate who joined the law firm Alston and Bird as a 'policy advisor'

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[1] Calculated via figures from *OpenSecrets.org* using NIPA (National Income Product Accounts) price deflators.

in 2004. Alston and Bird also runs a significant lobbying practice and revenues for this line of business increased from $2.6 million in 2004 to $6.6 million in 2005 after Daschle joined. Despite this boom in revenues Daschle did not register as a lobbyist, leading to questions about his level of contribution to Alston and Bird's lobbying practice, that is, whether he was credible as a 'less than 20%' lobbyist.[2]

Another prominent case is that of former House Speaker Newt Gingrich. During the 2012 Presidential election, controversy emerged about Gingrich's role as a consultant for financial institution Freddie Mac. Critics claimed that Gingrich (who it is estimated was paid more than $1.6 million by Freddie Mac) was utilising his contacts as a very active lobbyist for the company while Gingrich described his role as that of a general strategic advisor, drawing specifically on his expertise as a historian.[3]

Other investigative accounts (Auble 2012, 2014; Frates 2010) indicate that a range of ex-Congressional staffers and other government officials may also be employed in unregistered roles similar to those of Daschle and Gingrich. The extent of unregistered lobbying is thought to have increased in the late 2000s after the introduction of more regulation of revolving door movements by ex-staffers, creating extra 'career costs' of being officially registered as a lobbyist.

In this paper we therefore ask: is there a credible, economically significant pattern of potentially unregistered lobbying activity present at the federal level in the US? This is an important public issue because the presence of such unregistered activity would suggest that a critical fraction of paid-for special interest influence is not being measured. Transparency allows informed voters to make ex post evaluations of the interaction of lawmakers and special interests, providing an incentive for lawmakers to align their activities with the public interest. These incentives are naturally blunted when some of these interactions are not observable, as would be the case with a significant pattern of unregistered lobbying.

To answer this question we develop an empirical strategy in line with the recent literature on 'forensic economics' which traces out the consequences of (often illicit) hidden actions

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[2] See Scherer (2009) and Frates (2010) for a detailed account of Daschle's work with health industry clients in particular. It should be noted that Daschle eventually (in early 2016) registered as a lobbyist, nearly 12 years after exiting Congress and during which period he worked for three firms with major lobbying practices (Alston and Bird, DLA Piper, and Baker Donelson) (Arnsdorf 2016).

[3] Gingrich has a 1971 PhD in European History and his dissertation topic was 'Belgian Education Policy in the Congo 1945-1960' (Norman 2012). It was estimated that Gingrich was paid $1.65 million for 5.5 years of work for Freddie Mac. Contracts and agreements released about the work indicate that $25,000 per month was paid to his consulting firm, the Gingrich Group, and that the firm reported directly to Freddie Mac's public policy and lobbying office as part of the arrangement (Eggen 2012).

on observable data (Zitzewitz 2012). Specifically, we build up a database of all former Congressmen and major Congressional staffers who, after they exit their positions in the Congress, take up employment in firms that have significant lobbying practices but do not formally register as individual lobbyists themselves. Our detailed database includes the dates at which these ex-officials joined and (if applicable) left these lobbying firms. This information allows us to test for a systematic effect of unregistered ex-officials on lobbying firm revenues. Intuitively, while the full extent of the unregistered ex-officials' activities may not be declared, we can still back out their level of economic influence from the 'shadow' their presence casts on the firm-level revenue data.

Our empirical strategy therefore focuses on credibly parsing out this effect, dealing with two main challenges. The first challenge is distinguishing the posited shadow lobbyist effect from other correlated effects on firm revenue. The main underpinning of our 'forensic' strategy to deal with this concern is, of course, the pattern of variation evident in the entry and exit of shadow lobbyists across our sample of firms. In the first instance, this pattern of variation robustly indicates that the shadow lobbyist effect persists after controlling for common shocks and trends.

Other exercises we conduct are consistent with the shadow lobbying hypothesis. In particular, an event analysis of unregistered ex-officials joining lobbying firms shows that the associated effects are discontinuous and well-timed with the entry of the posited shadow lobbyists. The firm-client structure of our underlying data also allows us to test for the nature of the shadow lobbyist effect on revenue. Shadow lobbyists could add to the 'brand' of a firm, enhancing elite status and allowing firms to charge existing clients more. This would show up as an intensive margin effect. However, we find that the shadow lobbyist effect mainly operates along the extensive margin of new clients and contracts. This is in line with the effects associated with registered, revolving door lobbyists and positions shadow lobbyists as 'rainmakers' whose presence is associated with the acquisition of new clients for a firm.

Our event analysis also shows that there is no evidence of correlated hiring, that is, a systematic entry of new registered lobbyists at the same time as the hiring of the shadow lobbyists. Such correlated hiring would occur if the hiring of shadow lobbyists was part of a firm's general response to a demand shock. In this scenario, shadow lobbyists would be acting as support staff for the registered lobbyists. We further test for the general effect of non-registered support staff using a database of lobbying firm employees derived from the *Wayback Machine* internet archive. This is formulated as a placebo input at the equivalent

level of variation as the shadow lobbyists, but does not show strong, general revenue effects for unregistered workers. This adds to the 'rainmaker' evidence on client acquisition in establishing shadow lobbyists as being associated with the lobbying activities of the firm rather than acting as 'any other' unregistered employee.

The second main challenge for distinguishing the overall pattern of unregistered lobbying and efficacy of current regulation relates to the size of the revenue effects that we pick up. In principle, unregistered lobbying activity is still legitimate within the framework of the LDA as a result of the 20% rule. This therefore provides us with a benchmark for gauging the scope of unregistered activity. Specifically, to be in compliance with the LDA the measured shadow lobbyist effect should be substantially lower than the registered lobbyist effect after taking account of differences in the composition of the two groups along observable and unobservable dimensions.

Using the individual-level data on registered lobbyists we are able to quantify the determinants of lobbyist revenue and provide benchmarks for our measured shadow lobbyist effect. Our analysis indicates that the shadow lobbyist effect is comparable to that of a registered lobbyist at the median of the (unobservable) industry skill distribution. This sets a very high bar for reconciling the measured shadow lobbyist effect with strict compliance to the 20% rule. Simply put, this implies that shadow lobbyists are achieving a similar revenue effect to the median registered lobbyist but with one-fifth of the time input. In turn, this implies that the shadow lobbyists are either 'superstar' workers at the very top of the industry skill distribution or that there may be widespread shortfalls in adherence to the 20% working time threshold. Furthermore, it is also notable that, within the industry, shadow lobbyists are heavily clustered in the large lobbying firms where their contributions could be leveraged or hidden.

Finally, in terms of magnitudes, a decomposition of sectoral revenue trends finds that 6.5% of revenue changes amongst the relevant firms can be explained by the shadow lobbyist effect. Practically, this decomposition involves calculating the implied revenues generated by shadow lobbyist inputs over all periods in which they were present in firms and then comparing this to overall firm revenue growth. This can be thought of as an estimate of the value of the potential 'unaccounted for influence' associated with the growing number of unregistered ex-Congressional officials working in Washington lobbying firms. This estimate of 6.5% for shadow lobbying firms amounts to approximately 1.8% of revenues for all large lobbying firms or $149 million USD in direct financial terms. This contribution was mainly

generated in the last 5 years of the sample period as shadow lobbying started to grow quickly. While this arguably represents a small fraction of activity from the industry perspective it is not small from the perspective of the shadow lobbyists. We calculate that, under basic assumptions, shadow lobbyists are involved in raising $1.7 million in revenue per year for their firms which would likely translate into a healthy individual salary.

Furthermore, note that our estimates are likely to be a lower bound for the overall industry since feasible niches for potential shadow lobbying exist in both the in-house corporate lobbying sector and the 'deep underground' of completely unregistered entities such as think-tanks and political strategy consultancies who are not providing information as part of the LDA.

The policy implications of this paper are perhaps more pointed than usual. The evidence that we put together strongly suggests that, even if the 20% rule is being adhered to, the unregistered sub-sector that we measure seems to be a source of important and valuable inputs for Washington lobbying firms. Insofar as transparency policies such as the LDA are meant to capture economically important influence activities, our paper provides an impetus for revising the LDA to better cover this unregistered sub-sector. This could take the form of (for example) better monitoring or a lowering of the 20% threshold.

Related Literature. This paper contributes to and builds on a number of literatures. Firstly, there is the work on forensic economics as reviewed by Zitzewitz (2012). Our 'forensic' research design is most in line with a series of papers in this literature that map hidden behavior into observable information, such as: Hsieh and Moretti (2006) on Iraqi sanctions, Dube et al (2011) on CIA coup authorizations and insider trading, Della Vigna and La Ferrara (2010) on illegal arms trade, Downey (2022) on union officer prosecutions, Durante and Zhuravskaya (2015) on Israeli military strategy, Price and Wolfers (2007) on discriminatory behavior in basketball, and Zucman (2013) on the role of tax havens.

Secondly, we contribute to the lobbying literature. There has been an increasing number of papers that directly use the LDA data to address various political economy questions, for example: Bertrand et al. (2014), Bertrand et al. (2020), Blanes i Vidal et al. (2012), Ferguson et al. (2020), Igan et al. (2010), Kang (2016), Kerr et al. (2014) and Samphantharak et al. (2009). Reviews of the topic are given by Bombardini and Trebbi (2020) and de Figueiredo and Richter (2014), while a recent international perspective on the market for lobbying is provided by Della Vigna et al. (2016). Notably, a significant strand of work has emerged

in political science that focuses exactly on the value of different types of revolving door connections in Washington's lobbying market. McCrain (2018) shows that value also accrues according to the network of indirect connections lobbyists possess, while Egerod and McCrain (2023) look at how reverse transitions from lobbying back into government benefit lobbying firms. Ban et al. (2019) dissect how 'lobbyist value-added' is generated and identify top 'rainmaker' lobbyists as a result.

Thirdly, our work also contributes to the literatures on political selection (Besley 2005) and the related 'personnel economics of the state' (Finan et al. 2015). A main concern of these contributions has been the responsiveness of public officials to various types of financial and non-financial incentives (e.g. Ferraz and Finan 2011), which bears on our focus on how the pattern of post-Congressional careers may be changing. Again, the political science literature has contributed exactly to the topic of Congressional careers, with most of the focus falling on Members of Congress (MOCs). For example, Egerod (2023) shows how entry into lobbying by MOCs is influenced by the 'pull' of potential salaries while Palmer and Schneer (2019) study how cooling-off restrictions diverted MOCs away from lobbying careers and into corporate board positions. Cain and Drutman (2014) study how cooling off restrictions affected Congressional Staffer transitions into lobbying while Bolton and McCrain (2019) use Office of Personnel Management (OPM) data to look at the role of Executive Branch bureaucratic personnel in the lobby industry[4].

The paper is organized as follows. Section 2 provides the relevant institutional background while Section 3 explains the range of data we have assembled for the study. Section 4 documents important trends and descriptive statistics related to the key distinction between registered versus potential unregistered activity. Section 5 outlines the empirical modelling framework and Section 6 reports the results. The conclusion (Section 7) offers extra interpretation and policy implications.

2 Institutional Background

In this section, we provide some background on the LDA, as well as relevant regulations introduced in 2007 (HLOGA - the Honest Leadership and Open Government Act) and in 2009 (Executive Order 12490 - a major revolving door policy introduced by the Obama

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[4]Our empirical setting also speaks to some recent strands in the theoretical literature on lobbying that have featured an explicit role for lobbying firms as intermediaries (Groll and Thomas 2014, 2015; Groll and McKinley 2015) as well as contests for policy-maker attention (Cotton 2016, Cotton and Dellis 2016)

Administration).

The LDA was introduced in 1995 as a successor to the 1946 Regulation of Lobbying Act (RLA), which had long been considered unsatisfactory due to its narrow definitions of lobbying activity.[5] The LDA widened the definition of lobbyists to include those individuals retained financially by any client and also outlined specific activity thresholds for reporting. This threshold was that the lobbying services included 'more than one lobbying contact' and that the 'lobbying activities for that client must amount to 20% or more of the time that the individual expends on services to that client over a 6-month period' (Congressional Research Service 2020).

Note here that 'lobbying contact' refers to a specific form of lobbying, namely communication with covered executive and legislative branch officials. Furthermore, the need to report is only triggered when all three elements of the statutory definition are met, namely that (i) compensation is involved, (ii) more than one contact per 6-month reporting time period is made, and (iii) at least 20% of client-related working time is devoted to lobbying.

Importantly, the definition of lobbying activities counted as part of this 20% is broad. The term lobbying activities includes not only the time spent on a lobbying contact, but also *"efforts in support of such contacts, including the preparation and planning activities, research and other background work that is intended, at the time it is performed, for use in contacts, and coordination with the lobbying activities of others."* The LDA does give guidance and examples of reporting requirements covering points such as: prospective versus retrospective work around contact events, how supporting expenses are counted, and the precise timing of contacts amongst other issues.[6] However, in practice this calculation of time spent on lobbying is self-reported and not independently monitored or verified. The Government Audit Office (GAO) does publish an annual report on compliance with disclosure requirements but focuses its efforts on the technical accuracy of the submitted reports rather than potential cases of non-reporting.[7]

Indeed, the GAO also explicitly states that it is not obliged to identify cases of

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[5]This narrow interpretation was fostered by the *United States versus Harris* Supreme Court decision of 1954. In short, the decision exempted from reporting groups who spent their own money directly to lobby Congress, along with organizations whose primary mission was not lobbying (Straus 2015).

[6]See 'Guide to the Lobbying Disclosure Act' for detailed examples.

[7]For example, the GAO randomly selects a set of reports and requests documentation from registrants relating explicitly to those lobbying reports. The GAO investigations have typically found that registrants are able to provide evidence of the reported income and expenses in most cases but there are significant gaps in the reporting of the prior government experience or 'covered positions' of lobbyists (GAO 2015).

unregistered lobbying as part of its regular investigations. This is apparent in the GAO (2015) report which refers to its' official mandate, saying that this mandate:

> "...does not require us to identify lobbyist organizations that failed to register and report in accordance with LDA requirements. The mandate also does not require us to determine whether reported lobbying activity or political contributions represented the full extent of lobbying activities that took place".

In fact, over the entire history of the LDA the only referral of a case of illegal lobbying occurred in 2014.[8] As a result, in recent years there has been much criticism of the enforcement (or lack thereof) of the 20% rule for reporting, with even some professional bodies covering lobbyist issues warning that 'shadow lobbying cannot become the norm' (Miller 2016).

The most significant institutional changes to lobbying rules over the period that we consider arguably came with the 2007 Honest Leadership and Open Government Act (HLOGA). HLOGA changed some reporting rules (e.g. moving from reporting every 6 months to every 3 months) but was most noted for establishing 'cooling off' periods preventing ex-Congressional staffers and members from taking up lobbying positions within 12-24 months after their exit from the Congress. We provide more details on both HLOGA and the Obama Administration's 'reverse cooling off' rules (which restricted individuals from taking up positions in the Administration if they had recent lobbying experience) in Appendix A.1. These regulations are extensive enough in their detail to be worthy of separate study so we do not consider them here. However, we do note that they had the clear effect of increasing the career costs of registering as a lobbyist and this would have increased the personal incentive for acting as a potential shadow lobbyist from 2007 onwards.

3   Data

The main dataset used in this study is a firm-level panel constructed from two main sources: the database of lobbying reports released as part of the LDA and a database of political employment in the Congress, mainly sourced from the records kept by the political information company LegiStorm. As we discuss below, a key feature of this political employment database

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[8]In late 2015 it emerged that the lobbying firm the Carmen Group had agreed to pay a $125,000 settlement for failure to reporting lobbying activity and contributions. The case related to lobbying over emergency loans given to Carmen Group client Xavier University in the wake of Hurricane Katrina (Levinthal 2015). Note that the more recent case of Paul Manafort involved FARA and his unregistered work on behalf of pro-Russian Ukraine-based interests.

is that it lets us track the movement of ex-staffers into registered lobbying firms in cases where they do not register as individual lobbyists themselves.

3.1 Lobbying Reports Database

We use the lobbying reports database compiled from the original Congressional office reports by the Center for Responsive Politics (CRP). This database contains records that are effectively at the contract-level, giving information on the amount of expenditure involved, the registrant, and the lobbyists who participate in the work. The registrants can be divided into two categories: lobbying firms who take on multiple clients on a commercial basis and "self-filing organizations" who conduct in-house lobbying activities. This latter group of self-filing organizations includes corporations as well as peak industry groups and non-profit, single-issue advocates. Reporting of lobbying revenues (for lobbying firms) and lobbying expenditures (for self-filing organizations) was required every 6 months until HLOGA in 2007, which introduced quarterly reporting.

3.2 Political Employment

Our study also utilizes a database of political employment covering all individuals working in Congress across personal, committee, leadership, and administrative offices. Our source is the Congressional Staffer Salaries (CSS) database maintained by the political information company LegiStorm. This database is built up from the official published reports of staffer information by the House and Senate, covering the period from 2000 to the present day. The CSS is effectively a payroll database giving information on: the start and end dates of a given employment spell, the office of employment within the Congress, the job title held, and the total salary paid over the duration of the spell. The CSS database is comprehensively coded by LegiStorm with consistent identifiers for offices, members and staffers thereby allowing us to accurately track individual careers over the full period of the data.

Usefully, LegiStorm also maintains a list of former Congressional staffers who register as lobbyists during their careers - their 'revolving door' database. LegiStorm updates this list on a weekly basis, comparing lists of ex-staffers with the names of newly registered lobbyists. Importantly, LegiStorm verifies the accuracy of its basic name match with additional information drawn from publicly available biographical information posted on LinkedIn, Facebook, Twitter and company or lobbying firm websites.

Furthermore, in order to extend the coverage of the staffer-turned-lobbyist data backwards prior to the year 2000, we supplement the LegiStorm-based list with information from *lobbyist.info*, a professional directory of lobbyists published by Columbia Books over many decades. For example, using this source allows us to pick up cases where an individual who eventually registered as a lobbyist worked in the Congress during the late 1990s or earlier.

3.3   Shadow Lobbyist Information

3.3.1   Overall Approach

As part of its effort in tracking ex-staffers, LegiStorm keeps detailed biographical records of Congressional staffers across a range of career destinations both inside and outside government. This biographical database is very comprehensive, covering 10,200 individuals with government employment experience in Washington. Entries in the biographical database for these post-Congressional employment spells include: name of employer, dates with employer, and job title held.

Crucially, this information on career paths allows us to track cases of ex-staffers who join registered lobbying groups (that is, either lobbying firms or self-filing organizations) *but do not show up in the list of registered 'revolving door' lobbyists.* Furthermore, we exclude very junior staffers in order to apply a minimum threshold for intensive Congressional work experience.[9] The resulting group is the class of individuals that we define as potential unregistered or 'shadow' lobbyists. Also note that our estimating sample in this paper ends in 2012. This is both because data collection and verification is very labor intensive and because it gives us a long time horizon for ensuring that our identified class of shadow lobbyists are not simply slow to register, for example, as a result of the 'cooling off' period regulations mentioned above.

In addition to this information on ex-staffers, we also construct our own list of ex-members of Congress working on an unregistered basis in lobbying organizations by taking the list of all members who have exited since 1998 and manually looking up their career destinations in LegiStorm and other online sources.[10]

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[9]Specifically, we classify as 'Junior' those ex-staffers whose highest recorded job title is one of the following: Intern, Staff Assistant, State Scheduler, District Manager, Assistant Clerk, Assistant Tax Policy Advisor and Legislative Aide (Minor Position).

[10]Our master list of members of Congress comes from the database compiled by Stewart and Woon (2020) which also includes information on the characteristics and experience (e.g. committee service) of members.

3.3.2 Independent Fact-Checking Survey

As a further check, we also conduct our own fact-checking survey of the post-Congressional career destinations of former staffers. Here we select a random sample of approximately 300 staffers who left employment in the Congress between 2004-2007 but who do not have records in the LegiStorm biographical database. We then manually search for their post-Congressional career destinations via the usual online sources (LinkedIn, Wikipedia, company websites obtained through Google searches) and check their names against a historical database of lobbying firm employees that we develop using the *Wayback Machine* internet archive. This exercise lets us test the completeness of our shadow lobbyist measure over time. The findings confirm the accuracy of the LegiStorm database in regards to trends in shadow lobbying and we follow up with a further discussion in Section 4, giving precise details of the survey exercise in Appendix A.2.

3.3.3 Other Types of 'Shadow' Activity

Finally, it is useful to note the distinction between the type of potential unregistered 'shadow lobbyist' activity that we are examining in this paper versus other possible classes of shadow lobbying.

LaPira and Thomas (2013) investigate potential shadow lobbying in Washington following a broader definition. This definition covers the full set of professionals engaged in policy advocacy roles across both LDA-registered and completely unregistered organizations active in Washington. Examples of policy advocates working in unregistered organizations would include (say) employees in general political strategy consultancies or staff at policy think-tanks. Their definition also covers people working in policy advocacy roles who may or may not have prior government experience. Furthermore, 'policy advocacy' in this definition describes a very general set of activities that are likely to be wider than the LDA-based definition of lobbying as activities consciously directed at making contacts with members of the executive branch of government.[11]

In contrast, our definition is restricted to ex-government (specifically ex-Congressional) personnel *working in LDA-registered organizations*. We adopt this approach for two reasons. Firstly, it lets us test for an explicit economic link between lobbying revenues and the presence of posited shadow lobbyists. Secondly, by focusing on registered lobbying organizations we

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[11]For example, writing reports and developing public relations campaigns that do not involve direct communication with the executive branch.

are able to narrow down the focus to entities that are officially active in making contacts with the executive branch. The shadow lobbyists that we are parsing out in our definition are therefore located in organizations that are in the very center of the business of making contacts and lobbying as described in the LDA.

Briefly put, our definition is aimed squarely at picking up *economic evidence* of potential unregistered lobbying activity and this can only be done in cases where there is some information on revenues 'above ground' that can be related to what may be occurring 'underground'. This is not to discount that viable opportunities for unregistered shadow lobbying exist in other parts of Washington's policy-making economy. Indeed, LaPira and Thomas (2013) estimate that the size of the 'policy advocacy' sector they survey is comparable to that of the registered lobbying workforce. Repeated journalistic investigations (Lipton, Williams and Confessore 2013, Lipton and Williams 2016, Williams and Silverstein 2013) have questioned the role of a subset of think-tank activities that could be interpreted as unregistered lobbying efforts. The evidence we provide in this paper is therefore likely to be a lower bound for total amount of potential unregistered lobbying that could be taking place in Washington.

3.4   Unregistered 'Placebo' Workers

Along with the information above, we also collect data on lobbying firm employees who are neither registered as lobbyists nor are they ex-Congressional employees. We refer to these employees as the 'unregistered placebo' group in our later analysis.

Information on this class of unregistered employees is useful for us because they provide a counterpoint to the observed effects of unregistered ex-Congressional personnel. That is, including these 'unregistered placebo' employees in our regressions allows us to model the revenue effects of adding a generic unregistered employee to a firm's overall staff. It is plausible that our ex-Congressional shadow lobbyists could influence firm revenues mechanically through a simple 'added input' effect that is not directly related to lobbying activity and the data on unregistered placebo employees allows us to test for the size of this type of channel.

Collecting data on this class of unregistered employees is a significant challenge to say the least. It requires historical information on a wide range of lobbying firm employees (not just ex-Congressional staff) across a range of firms that simply do not report to a centralized database of any type. Furthermore, individual firms typically only keep lists of their *current* employees on their websites providing very little in the way of historical information.

We tackle this challenge by exploiting the *Wayback Machine*, a digital archive of the World Wide Web maintained since 2001 by the nonprofit Internet Archive organization. This service enables users to see archived versions of web pages across time. We exploit this facility to extract historical lists of lobbying firm employees at different points in time and retrieve information that has been effectively wiped from the present day versions of lobbying firm websites.

The full details of this exercise are outlined in Appendix A.3.2. In short, we focus on constructing a census of all Washington-based lobbying firm employees relevant to our main sample of firms. This census is centered on the year 2008 and, through name-matching against the lists of registered lobbyists and Congressional staffers, we are able to filter out a group of unregistered firm employees with no Congressional experience. We then randomly sample from this pool of possible 'unregistered placebo' employees and research their career biographies in order to pin down their entry and exit dates of employment at the firms. This allows us to construct a set of unregistered placebo employees similar in size to the set of shadow lobbyists who are the main group of interest.

4   Descriptive Statistics

In this section, we go over some information that helps establish the background and context of our main empirical strategy. We first discuss patterns in the data on registered lobbying activity, focusing in particular on the flow of ex-staffers into lobbying over time. Secondly, we break down the information we have assembled on shadow lobbyists along similar lines.

4.1   Trends in Registered and Shadow Lobbying

4.1.1   Industry Structure and the Growth of Shadow Lobbying

In Table 1 we provide some descriptives on the total population of lobbying firms reporting as part of the LDA. As discussed, our focus is on the lobbying firm sector instead of directly employed 'in-house' lobbyists because firms take multiple clients and will therefore see their revenues rise and fall with factors such as the composition of their staff and their connections with the Congress.

The most notable feature of the industry is its skewed structure in terms of firm sizes. We define 'firm size' here as the average number of registered lobbyists that a firm reports as active and engaged in lobbying contracts per 6-month reporting period. The median lobbying

firm has only 1 registered lobbyist, while a firm at the 75% percentile has 2 lobbyists, with a 90% percentile firm featuring 4 lobbyists on average.

Institutionally, this distribution is down to the fact that the federal lobbying industry is dominated by a combination of many single-person firms alongside a set of general professional services firms (e.g. law or strategy consulting firms) that only provide lobbying services as a part of their overall portfolio. Given this, we report descriptives in Table 1 according to heuristic firm size cut-offs - less than 5 lobbyists for 'small', between 5 and 10 lobbyists for 'medium' and 10 or more for 'large'.

The distribution of shadow lobbyists across firm size classes is striking. Shadow lobbyist activity is overwhelmingly concentrated in 'large' firms with 10 or more registered lobbying employees on average. Approximately 41.3% (43/104) of shadow lobbyists as we define them work in these 129 large firms (out of the approximate 4,600 firms in the industry). In Figure 1 we explore this in further detail by calculating the probability of employing a shadow lobbyist by discrete integers of firm size up to 10-plus lobbyists. This shows a major jump in the probability around the threshold of 9-10 lobbyists. However, there is no similar jump for employing registered, revolving door lobbyists (panel (b)). This pattern is compatible with the idea that shadow lobbyists are in a better position to leverage their contacts and experience - or hide their activity - when working within larger firms.

It is also clear from Table 1 that smaller firms have a different pattern of participation in the industry and have inherently more limited lobbying practices. Amongst the small firms with 5 or fewer lobbyists, the average number of periods in the sample is only 9.8 compared to 20.4 for large firms. This is also reflected in the measure of shadow lobbyist 'transitions' that we report in the final row of Table 1. 'Transitions' here are defined as the number of discrete shifts in the shadow lobbyist variable, for example, going from zero shadow lobbyists in period t to one lobbyist in $t+1$ (or vice versa for exit from a firm). This is the variation that underlies the within-groups and long difference models we use for modelling the revenue effects of shadow lobbying.

Table 1: Descriptive Statistics for Lobbying Firms.

	(1) Small (≤ 5)	(2) Medium (5-10)	(3) Large ($10\geq$)
Firms	4,281	207	129
Lobbyists	1.7	7.0	21.0
Revenue Per Lobbyist ($)	94,951	156,639	144,966
Shadow Lobbyists (Total)	47	14	43
(a) Ex-Staffers	40	10	35
Experience in the Congress (Years)	3.7	3.5	4.4
Number of Connections With a Politician	1.5	0.6	1.4
Number of Connections With a Committee	0.8	1.2	0.90
Republican Affiliated (% share)	0.48	0.40	0.35
Ideology Score (Index from -1 to +1)	0.06	0.07	-0.08
% Senior Position in the Congress	0.22	0.10	0.26
% Mid-Rank Position in the Congress	0.60	0.60	0.60
% Low-Rank Position in the Congress	0.18	0.30	0.14
Connection – Party Leadership (% share)	0.53	0.80	0.63
Connection – High Ranking Committee (% share)	0.40	0.30	0.43
Connection – Committee Chair (% share)	0.23	0.00	0.37
(b) Ex-Congressmen	7	4	8
Republican	0.43	0.25	0.25
Key Committee	0.43	0.25	0.50
Senator	0.29	0.25	0.25
Firm average number of periods	9.8	17.6	20.4
Total Shadow Lobbyist 'Transitions'	13	15	47

Notes: *Firms* is the number of firms present in each size category. *Lobbyists* is the average number of registered lobbyists that a firm reports across periods. The size bands for firms are then 5 or less registered lobbyists, strictly between 5-10, and 10 or more for column (3). *Shadow Lobbyists* is the total number of unregistered ex-Staffers and ex-Congressmen employed by firms in a given size group. This number is decomposed below into *(a) Ex-Staffers* and *(b) Ex-Congressmen*. The share of Democrat Affiliated is simply one minus the *Republican Affiliated*. *Firm average number of periods* is the average number of periods that firms report non-zero lobbying revenues. *Total Shadow Lobbyist 'Transitions'* is the number of single period, discrete 'switches' in the shadow lobbyist variable that occur once the firm revenue panel is constructed.

Figure 1: Probability of Employing a Shadow Lobbyist by Firm Size.

(a) Prob(Ever Shadow)

(b) Prob(Ever Revolving)
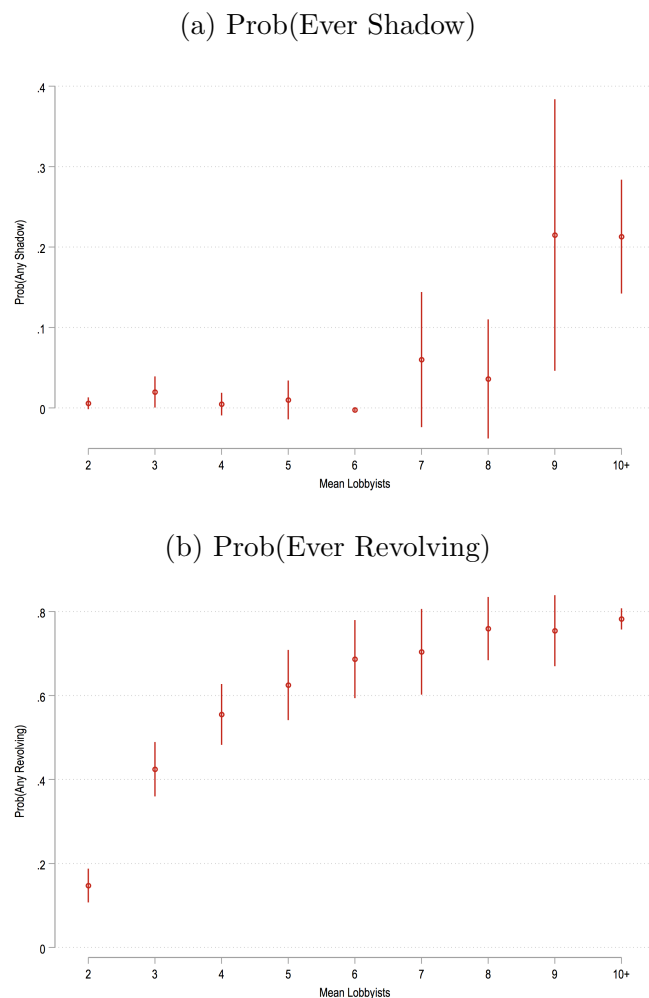
Notes: This figure reports results from using the flat cross-section of 4,618 lobbying firms (all sizes) to estimate the probability of ever employing a shadow lobbyist (panel (a)). This is compared to the probability of ever employing a (registered) revolving door lobbyist, which is reported in panel (b). The right-hand side in each regression is comprised of dummies for each integer of average firm size up to 10. All firms with 10 or more lobbyists on average are in a single bin.

Figure 2: Evolution of Shadow Lobbying, 1998-2012.

(a) Stock of Shadow Lobbyists

Stocks of Revolving Door & Shadow Lobbyists, By Period, 1998-2012.

(b) Average Firm Revenues
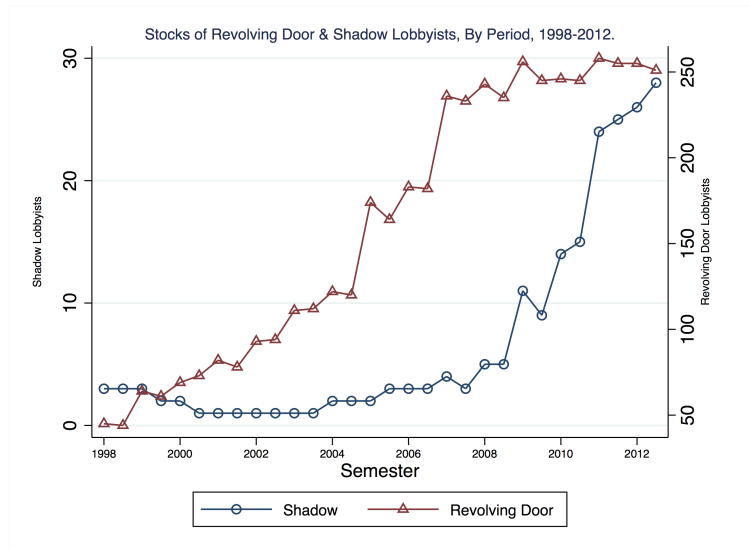
Average Revenues by Shadow & Non-Shadow Firm Groups
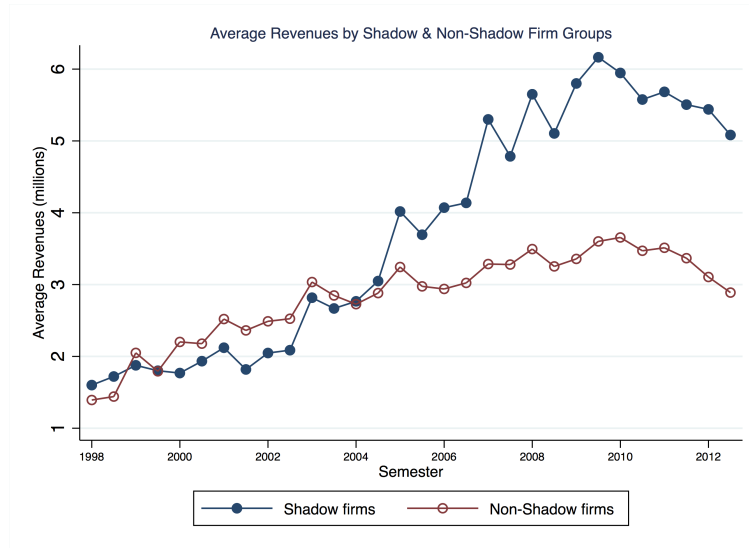
Notes: Figure (a) shows the stock of 'Shadow' and (registered) 'Revolving Door' lobbyists per period in the set of big firms which employ a shadow lobbyist at any point during the sample period (28 firms in total). 'Shadow' lobbyists are unregistered ex-Staffers or ex-Congressmen. Figure (b) shows the average revenues in the shadow (28 firms) and non-shadow sectors (101 firms). Average revenues are defined as the sum of sector revenues divided by the number of firms active in a given 6-month period.

The fact that the small firms have a lower average number of periods means that, in many cases, the transitions of our posited shadow lobbyists into and out of firms occur in periods where the firm is not recording revenues. Hence, while the small firms nominally have 47 shadow lobbyists attached to them there are only 13 transitions in practice. By comparison there are 43 shadow lobbyists in the large firms that we focus on but 47 transitions due to the fact that there are enough continuous periods to observe both moves in and out of firms. Our main analysis will therefore be conditioned on the set of these large firms although, as part of robustness exercises, we will also present estimates based on wider definitions.

In Figure 2(a) we then plot the number of shadow and registered revolving door lobbyists working in these large firms over time. This shows trivial numbers of shadow lobbyists prior to 2005 with very strong growth from 2007. The number of revolving door lobbyists also grew strongly but this growth was spread out more across all years. By the end of the period, the shadow lobbyist workforce in these firms was 11% as large as that of (registered) revolving door lobbyists. The revenue trends are then shown in Figure 2(b), differentiating between the 28 firms who employ a shadow lobbyist at some point during 1998-2012 and the remaining 101 firms. Average lobbying revenues across firms grew rapidly over this period and this growth was heavily concentrated in the shadow lobbyist firms.

By the end of the period, shadow lobbying firms were taking in $5-6 million in revenue per 6-month period, around double the amount for non-shadow firms. We discuss this divergence in revenues (and the potential role of shadow lobbying in driving it) again when considering magnitude calculations for our main results.

4.1.2 Validating our Shadow Lobbying Measure

A key issue in these descriptives is the striking nature of the rise in shadow lobbying from 2007 onwards. This finding caused us to consult the data more closely. In particular, one concern was that the level of shadow lobbying activity was understated before the mid-2000s since LegiStorm (our data source for the shadow lobbying series) was only founded as a company in 2006.

We therefore conducted our own survey of ex-staffer biographies to check how many transitions into potential unregistered lobbying may have been missed in the early-mid 2000s. Our sample frame was the set of people who permanently disappeared from the Congressional salaries database in the period 2004-2007 but had no recorded information in the LegiStorm biographies database (i.e. the database from which the shadow lobbyist information was

collected). Full details are provided in Appendix A.2. In short, this is a survey targeted at the senior Congressional staffers covering the period where the LegiStorm data is notionally at its weakest, namely the pre-2006 period. Practically, along with person-specific internet searches we also drew on the unique historical database of lobbying firm employment sourced from the *Wayback Machine* that was used to construct the 'placebo' workers discussed previously. This survey turned up very few ex-staffers working in registered lobbying organizations and, where this occurred, the ex-staffers were working in the class of very small firms that are not the main subject of our analysis. The major implication of this survey then is that the sharp rise in shadow lobbyist numbers from 2007 onwards does appear to be a genuine structural development in the lobbying industry.

4.1.3 Characteristics of Shadow Lobbyists

In Table 2, we provide a detailed comparison of the registered and unregistered 'shadow' lobbyists working in the large firm sample. The shadow lobbyists are broadly comparable to registered lobbyists in terms of characteristics like connections to senior Congressional figures and committees. The shadow lobbyist group does skew towards the Democrats and this is likely to be a function of the fact that the Democrats controlled the White House and the Congress in the post-2008 period when shadow lobbying increased sharply. As an additional check of comparability we also construct a set of propensity-score matched registered, revolving door lobbyists using all of the variables reported in this table. This subset of lobbyists is used in later robustness checks but we emphasise here that the shadow and registered lobbyist in the large firms sample of very similar on observables even without the matching.

5 Modelling Framework

5.1 Firm-level Revenue Models

Our approach to studying revenue effects is based on a firm-level panel. The information on different types of registered and potentially unregistered (shadow) lobbyists described in Section 4 is mapped into this panel. The most basic empirical model of revenues we can formulate is as follows:

$$ln(R_{jt}) = \alpha_j + \gamma_t + \beta Shadow_{jt} + \epsilon_{jt} \tag{1}$$

Table 2: Descriptive Statistics on Revolving Door Lobbyists: Registered vs. Shadow.

Rev. Door Lobbyists	(1) Shadow	(2) Registered	(3) Registered Matched Sample
(A) *Ex-Staffers*			
Tenure in Congress (years)	4.4	4.0	4.14
DW Nominate Ideology Score	-0.08	0.09**	-0.03
Republican	0.35	0.52*	0.43
Senior Job Titles	0.26	0.37	0.22
Connections - Party Leadership	0.63	0.45**	0.64
Connections - Committee Chair	0.37	0.38	0.51
Total Number	35	891	155
(B) *Ex-Congressmen*			
Tenure in Congress (years)	9.8	9.7	9
Tenure in Lobbying Sector (years)	3.3	10.5***	8.88**
DW Nominate Ideology Score	-0.08	0.18*	-0.07
Republican	0.25	0.59*	0.23
Senior Leadership Positions	0.13	0.05	0.1
Committee Chair	0	0.19	0
Senate	0.25	0.20	0.1
Total Number	8	74	40

Notes: This table shows descriptive statistics for the set of registered, revolving door lobbyists and shadow lobbyists included in the main estimating sample of 'large' firms. That is, all individuals considered have previous employment experience in Congress. We split between ex-staffers in panel (A) and former Members of Congress in panel (B). Tenure in Congress is years spent working in Congress (measured via LegiStorm for staffers and official dates for Congress members). Tenure in lobbying sector is calculated via lobbying reports (Registered) and biographical career data (Shadow). 'Senior Job Titles' is measured as a fractional share and includes: Chief of Staff, Legislative Director, Deputy Chief of Staff, Communications Director. 'Connections - Party Leadership' (share) includes connections to Senate or House leaders: Speaker, Majority or Minority Leader, Whips. Republican (share) and DW Nominate Ideology Score in panel (A) are measured with respect to the last Member served when working in the Congress. Column (3) reports the means for a propensity-score matched subset of revolving door lobbyists. We use all the variables reported in this table to estimate the propensity score and choose the nearest five neighbours (where available) for each shadow lobbyist to construct the total matched subset.

where $ln(R_{jt})$ represents the (log) revenues of lobbying firm j in period t; $Shadow_{jt}$ is a count of the unregistered ex-Congressional officials working at firm j in 6-month period t; α_j is a firm fixed effect; γ_t is a time effect defined at the period level; and ϵ_{jt} is the unobserved error term. The inclusion of the α_j fixed effects means that estimates of the β parameter are identified from variation associated with changes in the number of workers represented by $Shadow_{jt}$. Specifically, as a semi-log specification we can then read off the estimated coefficients as the (approximate) percentage change in revenue due to a one-person shift in the number of workers represented by $Shadow_{jt}$.

Our 'forensic' design focuses on whether $\beta > 0$ in practice. There are some clear challenges to the interpretation of the naive model of revenues as presented above. Firstly, the $Shadow_{jt}$ variable could be picking up the effects of firm-specific increases in demand for lobbying services. The $Shadow_{jt}$ workers could therefore be employed by firms as a response to this demand shock, plausibly as closely related 'support workers' to assist the officially registered lobbyist workforce at the firm. We can deal with the omitted variable bias implied by this hypothesis by including a range of controls:

$$ln(R_{jt}) = \alpha_j + \gamma_t + \beta Shadow_{jt} + \lambda Registered_{jt} + \delta Support_{jt} + \epsilon_{jt} \qquad (2)$$

where the new variable $Registered_{jt}$ is a count of the number of registered lobbyists. If the hiring of unregistered ex-Congress officials is related to a lobbying demand shock then this should be correlated with the actual number of registered lobbyists the firm employs from period to period.

We add a further variable $Support_{jt}$ to deal with a second challenge. There could be a general 'non-registered' or 'non-lobbying' worker effect that is not directly linked to firm-specific increases in the demand for lobbying services. For example, as the firm adds more junior or administrative staff this could free up the time of the registered lobbyists working at the firm. The $Support_{jt}$ variable is therefore a count of unregistered workers at the firm who have no history of employment as Congressional personnel. Hence, this group of workers is a useful counterpoint or 'placebo' group relative to the unregistered, ex-Congressional workers measured by $Shadow_{jt}$. That is, the inclusion of the $Support_{jt}$ variable allows us to estimate the 'added input' effects that could be expected from employing a support worker with skills outside those represented by Congressional work experience.

We also do a 'benchmarking' analysis of our shadow lobbyist effect that aims to position the shadow lobbyist within the overall productivity distribution of registered lobbyists. This

allows us to gauge whether the estimated shadow lobbyist effect is consistent with a level of inputs provided in accordance with the 20% rule. We detail our approach in full in Appendix A.4. To summarize, we regress the revenues of registered ex-Congressional officials on individual fixed effects in order to extract the unobservable component, along with parameter estimates on observable measures. This shows that unobservable skill plays a large role in determining revenues - the lobbyist fixed effects explain around 55.5% of the variation in these models. We then sort these fixed effects from highest to lowest and put lobbyists into ranked deciles. Finally, we collapse dummy variables for these deciles to the firm-level and use them as covariates in the basic model described above. This allows us to estimate the effect of (say) a 'superstar' lobbyist in the 9th or 10th decile of individual effects on firm revenues and compare the estimates to those for our shadow lobbyists.

5.2 Event Analysis and Trends

A further general issue is trends in the pattern of revenues (as seen in Figure 2(b)). Total industry revenues grew rapidly over the period that we consider and some firms (or subsets of firms) picked up more of this growth than others. We deal with this in a number of ways. Firstly, we include linear firm-specific trends in our basic specification for all firms. This captures any systematic increase in revenues across a firm's observed life in our panel but has the potential cost of over-fitting the data. Secondly, we estimate first difference specifications across different time intervals. This has the advantage of differencing out the nonstationarity associated with any trends and also means that the β parameter is now effectively measuring the discrete 'jump' in revenues that occurs when a shadow lobbyist joins or leaves a firm. To be clear, we estimate the following equation:

$$\Delta_k ln(R_{jt}) = \alpha + \gamma_t + \beta \Delta_k Shadow_{jt} + \lambda \Delta_k Registered_{jt} + \varepsilon_{jt} \tag{3}$$

where α is a constant, and ε_{jt} is an error term. The subscript k denotes the order of differencing. For example, $k = 1$ denotes 1-period (which is 6 months), $k = 2$ denotes 2-periods (12 months) and so on. Hence in this case the β parameter is measuring the shift in revenues between a baseline period and a given period t. In contrast, the within-groups specification discussed above is comparing average revenues across multiple periods when shadow lobbyists are active versus the periods when they are non-active at the firm.

As an additional exercise, we also develop an event study specification in order to screen

for other unspecified shocks or revenue patterns that may be affecting firms at the time that shadow lobbyists enter. The important issue here is to understand the discreteness of the revenue shocks associated with the movement of shadow lobbyists into or out of a firm. The generic event model can be specified as follows:

$$ln(R_{jt}) = \alpha_j + \gamma_t + \sum_{L=l}^{L} \beta_l Shadow_{j(t_0+l)} + \lambda Registered_{jt} + \epsilon_{jt} \qquad (4)$$

where t_0 is the initial period in which a shadow lobbyist enters a firm and l indexes time periods before and after. Practically, we will take two approaches to testing this basic model. In the first approach we use all shadow lobbyist transition events (entries, exits and cases of multiple entry in the same firm) and follow the recent literature on 'staggered timing' in difference-in-difference (DID) models. While our empirical models are not formally framed as a causal analysis, in practice they face the same type of econometric issues as in the DID methodology. To address this we employ the DID_M estimator outlined in de Chaisemartin and D'Haultfœuille (2020). This estimator is suitable for non-binary treatments with discrete values such as our shadow lobbyist count and explicitly deals with variation due to 'switching in' and 'switching out' of treatment.[12]

The second approach will focus on isolating the subset of 'clean' shadow lobbyist first entries into firms where we condition on there being no prior shadow lobbyists already working in the firm. Note that this approach also excludes transitions associated with shadow lobbyist exits from a firm since entry events are the core part of the overall shadow lobbying hypothesis that we are testing.

6 Results

6.1 Baseline Results

Table 3 shows the results of estimating models equivalent to equations (1) and (2) on our firm-level panel of large lobbying firms who report revenues regularly. Our shadow lobbyist measure pools both unregistered ex-staffers and unregistered ex-Congressmen into the one count variable as part of the semi-log specificaton. Column (1), representing the naive model,

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[12] As de Chaisemartin and D'Haultfœuille (2023) describe eloquently, the DID_M estimator is 'a weighted average, across d and t, of DIDs comparing the $(t-1)$ to t outcome evolution of groups whose treatment goes from d to some other value from $(t-1)$ to t, and of groups with a treatment equal to d at both dates, normalized by the intensity of the treatment change experienced by the switchers.'

shows that an extra shadow lobbyist is associated with an 28.5 percent increase in revenue (although a null of zero cannot be rejected). The estimate becomes more precise and falls to approximately 22 percent when we include a control for the count of all the registered lobbyists working at the firm in column (2). Hence this indicates that shadow lobbyists have an association with revenue over and above any that might occur through a correlation with increases or decreases in the number of registered lobbyists (i.e. a traditional omitted bias argument).

The third and fourth columns disaggregate lobbyists by type. In column (3) it is evident that the shadow ex-staffers and shadow ex-Congressmen have similar point estimates for their associated revenue effects. The breakdown in column (4) then allows us to compare 'like-for-like' in terms of effects associated with unregistered shadow lobbyists versus the equivalent (registered) revolving door lobbyists. While as per the F-statistics a null of no difference in the coefficients cannot be rejected, the pattern of the effects we see here is of obvious interest for interpreting the potential scope of the activities of shadow lobbyists within these firms.[13] We return to this issue later as part of our benchmarking analysis.

6.2 Robustness

6.2.1 Correlated Shocks, Trends and Robustness

In Table 4 we present a number of robustness checks. As discussed, the main issue for parsing out the potential shadow lobbyist effect is distinguishing the variation in our $Shadow_{jt}$ variable from other potential time-varying influences on firm revenues.

Our baseline specification using a pooled measure of shadow ex-staffers and ex-Congressmen is repeated in column (1) of Table 4. As a simple step for controlling for unspecified time-varying influences we include a lagged dependent variable in column (2). This accounts for both period-to-period persistence in revenues and omitted time-varying factors that we cannot observe but that are plausibly correlated with the (lagged) dependent variable over time. This results in only a limited shift in the calculated long-run coefficient which indicates an effect of approximately 21.7 percent.[14]

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[13]This result of no statistical difference been registered and unregistered lobbyists also holds if a sub-group of propensity-score matched registered lobbyists is used for this exercise. See Appendix Table A4 for these results.

[14]That is, this is calculated as: 0.113 / (1- 0.429) = 0.198 then exponentiating with respect to column (2), Table 4.

Table 3: Baseline Firm-Level Revenue Models

	(1) ln(Revenue)	(2) ln(Revenue)	(3) ln(Revenue)	(4) ln(Revenue)
Shadow Lobbyists (All)	0.251 (0.155)	0.202*** (0.063)		
Unregistered Ex-Staffers			0.208** (0.075)	0.173** (0.073)
Unregistered Ex-Congressmen			0.183* (0.109)	0.173 (0.107)
Registered Ex-Staffers				0.091*** (0.016)
Registered Ex-Congressmen				0.164*** (0.043)
Registered Lobbyists		0.068*** (0.005)	0.068*** (0.005)	0.059*** (0.006)
F-stat (Staffers)				1.2 (0.28)
F-stat (MoCs)				0.01 (0.93)
Number of Firms	129	129	129	129
Number of Observations	2,631	2,631	2,631	2,631

Notes: In all regressions we control for firm and time fixed effects. All standard errors are clustered at the firm level. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. The F-statistics are for test of coefficient equality for shadow versus revolving door staffers and Members of Congress (MoCs). F-statistic is report with p-value in parentheses. All reported variables are counts of the number of employees by type at the firm j in period t. Registered employees are those who appear on the disclosed lobbying reports forms. *Registered Ex-Staffers* represents former Congressional staffers who are registered as lobbyists while *Registered Ex-Congressmen* measures Ex-Congressmen who are registered. The *Shadow Lobbyists (All)* variable is comprised of *Unregistered Ex-Staffers* and *Unregistered Ex-Congressmen* summed together. The variable *Registered Lobbyists* represents the number of registered lobbyists working at the firm who are not already counted as part of variables such as *Registered Ex-Staffers* and *Registered Ex-Congressmen*.

Table 4: Shadow Lobbying and Firm Revenues - Robustness

	(1) Baseline	(2) Lagged Depvar	(3) Firm trends	(4) Differences	(5) Winsorized	(6) More Balance	(7) Threshold = 8	(8) Drop Early
Shadow	0.202***	0.113***	0.156**	0.116***	0.198***	0.245***	0.179***	0.208***
	(0.063)	(0.035)	(0.065)	(0.042)	(0.071)	(0.068)	(0.053)	(0.056)
Registered	0.068***	0.043***	0.055***	0.048***	0.064***	0.067***	0.073***	0.068***
	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
$ln(Revenue)_{t-1}$		0.429***						
		(0.043)						
Number of Firms	129	124	129	129	129	95	177	124
Number of Obs	2,631	2,475	2,632	2,354	2,631	2,378	3,597	2,487

Notes: All standard errors are clustered at the firm level. $*p < 0.10, **p < 0.05, ***p < 0.01$. Column (2) adds a lagged dependent variable. Column (3) adds linear firm trends. Column (4) estimates a model in 2-period first differences. Column (5) estimates the baseline model but with a winsorized dependent variable (1st & 99th percentiles). Column (6) improves the 'balance' of the sample by dropping firms that appear less than 14 times in the panel. Columns (7) then changes the firm size threshold for inclusion in the sample to 8 lobbyists (i.e. instead of 10 in the baseline). Column(8) examines staggered timing issues by dropping firms that employed shadow lobbyists early in the sample (inclusive of the first 15 periods). All regressions include firm and time fixed effects.

Recall from Figure 2(b) that revenues for the shadow and non-shadow firms evolve differently for at least part of the sample period. We therefore take some steps to assess this. In column (3) we take the common step of including a separate linear trend for every firm and the shadow lobbyist effect is robust to this. As discussed, an alternative approach to dealing with trends is to first difference the data over different time intervals. The β parameter in such first difference models effectively measures the 'jump' in revenues when there is a discrete change in the number of shadow lobbyists at a firm. By differencing the data, this approach also deals with any nonstationarity associated with trends at the firm or industry sub-group level but avoids the potential overfitting that comes with including 129 linear trend terms.

Estimates for the 2-periods (1 year) model; are reported in columns (4) of Table 4. This indicates a revenue jump of approximately 12.3% when a shadow lobbyist enters a firm.[15] In Figure 3 we trace this across the full range of intervals ranging from 1-period (6 months) to 6-periods (3 years). A statistically significant jump in revenues is apparent from the 12-month model onwards and the effect increases slightly with the length of the interval. The fact that the longer interval models (i.e. up to 3 years) hold indicates the revenue effects of a shadow lobbyist are sustained well after initial entry into the firm. Furthermore, note that the range of these estimates is consistent with those in the linear trends model reported previously.

The rest of Table 4 examines general robustness. We winsorize the dependent variable at the 1st and 99th percentiles in column (5) and this has little effect. We then restrict the 'balance' of the panel in column (6), dropping any firm that does not appear at least 14 times in the data (that is, we only keep firms with a minimum of 7 years of information). The point estimates increase here as we would expect as the data on firms gets 'cleaner' with more information per firm to calculate within-group effects. In the seventh column we lower the firm size thresholds for inclusion in the sample to 8 lobbyists on average. This has limited effects and we return to this issue when discussing models using 'small' firms (i.e. all firms with less than 10 lobbyists on average).

The final column addresses a version of the 'staggered timing' issue that has been discussed in the recent difference-in-difference literature (Goodman-Bacon 2021). We note here that our approach is a forensic design focused on picking up particular revenue movements associated with unregistered lobbying rather than being a causal design aimed at estimating

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[15]Note that the variation used here involves both the entry and exit of shadow lobbyists from a firm. However, the majority (66.0%) of single period transitions of shadow lobbyists are cases of entry.

Figure 3: Long Difference Estimates - Shadow Lobbyist Effect.
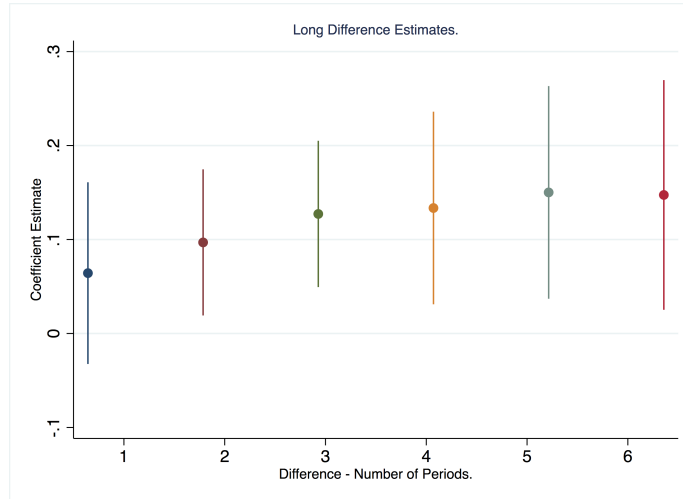
Notes: This figure shows the estimates (with 95% confidence intervals) of long difference models of different orders. For example, 1-period corresponds to $(X_t - X_{t-1})$ (6 months), 2-period to $(X_t - X_{t-2})$ (12 months) and so on. All regressions include period fixed effects and control for the total number of registered lobbyists (also in analogous differences), with clustering at the firm-level.

Average Treatment on the Treated (ATT) parameters. However, the econometric approach is analogous to a staggered difference-in-difference model, albeit one where the treatment can 'switch off' in cases where a shadow lobbyist exits a firm. In column (8) we therefore run an exercise where we entirely drop firms that employed shadow lobbyists early on in the sample (within the first 15 out of 30 periods) since they are primary source of problematic comparisons in staggered difference-in-difference settings. This is only a small group of firms since our sample is mainly comprised of either 'never shadow' firms or firms that began employing shadow lobbyists in the second half of the sample period. Hence, in column (8) we can see that that this cleaning of the (de facto) control group has only a minimal impact on our estimates.[16]

6.2.2 Event Analysis.

The next set of robustness checks uses event study methods to test for the discreteness of the shadow lobbyist revenue shocks that we have detected so far. Importantly, this allows us to assess the extent to which the revenue shocks associated with the shadow lobbyists

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[16]We also run a specification where we convert the shadow lobbying variable into a discrete 0-1 variable (that is, suppressing the count of lobbyists) and leave it switched on permanently after the first entry of a shadow lobbyist. This specification reflects the typical framing of the classical staggered timing problem. Again, dropping the 'early Shadow' firms has a minimal impact with an estimate of 0.337(0.085) (N=2,487) for the always on indicator versus a baseline of 0.374(0.085) (N=2,631).

are discontinuous and are indeed 'events' when it comes to the pattern of firm revenues. To do this we implement the DID_M estimator of de Chaisemartin and D'Haultfœuille(2020). As discussed, the modelling of shadow lobbyist effects in our setting has similarities with the 'staggered timing' issue in difference-in-difference analysis. The DID_M estimator has the advantage of utilising all of the variation - both the 'switching in' and 'switching out' - and deals comprehensively with the weighted comparison of movers and stayers across different treatment states.

The results using the DID_M estimator are shown for a wide window in Figure 4 and clearly show a jump at the common switch point. The trend in revenue is relatively flat prior to the transitions which helps to rule out the possibility that the revenue shocks associated with shadow lobbyists are tied up with some type of broader general firm adjustment. Such an adjustment would be plausibly spread out over several periods before the transitions and show up as an anticipation effect. In contrast, the evidence in Figure 4 indicates that the revenue shocks are precisely timed with the observed transitions. We therefore take the results here as an indicator that our main results in Tables 3 and 4 are clearly underpinned by a set of discretely timed, revenue-increasing events that are centered around the entry points of shadow lobbyists into a firm.

In Appendix Figure 7 we also implement a more traditional event study that focuses only on shadow lobbyist entry events, specifically the first entry of a shadow lobbyist into a firm. In this case, we standardize each entry point as t_0 and track revenues for a total of nine periods (four before, four after and a period 0 switch point) in line with the specification outlined by equation (3). We do this for the subset of transitions that can be considered 'clean' entry events such that there is no shadow lobbyist working in the firm immediately beforehand.[17] Note that this necessarily reduces the number of shadow lobbyist cases that drive the variation. This version of the event study strongly corroborates the results from the DID_M estimator and shows the central role of entry effects in driving the overall shadow lobbyist effect.[18]

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[17]Tightening this further by conditioning on 4 periods of continuing presence in the firm after entry does not substantively alter the event study. Arguably, such conditioning would keep only the 'successful' shadow lobbyists, biasing our estimates. But we do not find that such effects are at play.

[18]Specifically, 26 shadow lobbyist entry events are involved in this exercise out of a total of 47 shadow lobbyists discrete 'switching' events in total. Amongst the remaining 21 events are 13 that relate to exits from the firm while 8 events are entries take place when in situations where there is already a shadow lobbyist working at the firm.

Figure 4: Staggered DID Estimation of Shadow Lobbyist Effects.
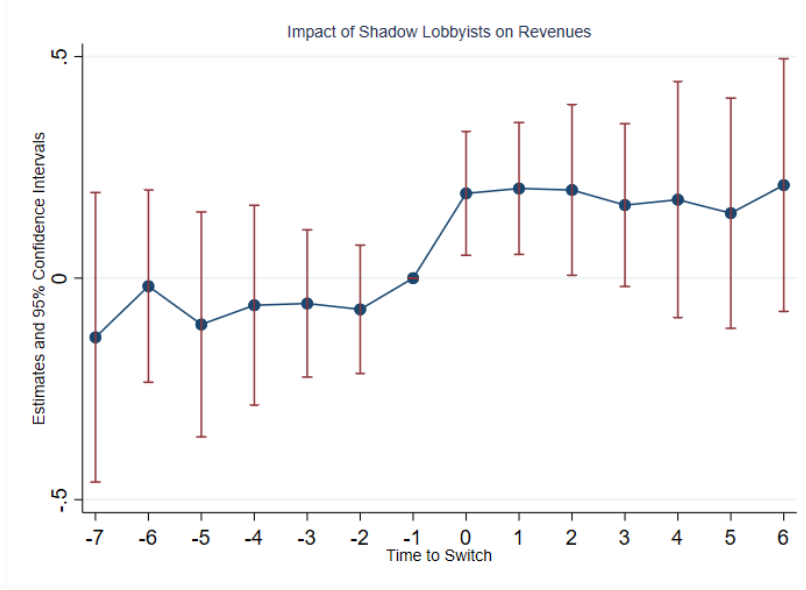
Notes: This figure uses the DID-M estimator of de Chaisemartin and D'Haultfœuille(2020) to estimate the period-by-period effects of all types of shadow lobbyist transitions (entry and exit inclusive of multiple cases per firm. Standard errors clustered at the firm level with 95% confidence intervals reported.

6.2.3 Testing for Correlated Input Changes.

Our main revenue models include controls for other types of registered lobbyists alongside time and firm fixed effects. These controls for registered lobbyists help deal with potential firm-specific increases in the demand for lobbying services. Firms could be hiring shadow lobbyists as 'support workers' to assist officially registered lobbyists, or indeed as substitutes for official lobbyists so that they can be reallocated within the firm. To assess this further we conduct some event studies that use registered lobbyists as the dependent variable in regressions on our shadow lobbyist variable plus firm and time fixed effects. These are reported for the DID_M estimator in Figure 9 and the 'entry only' model in appendix Figure 10. Consistent with our main regression models these event studies do not show any discrete shifts in registered lobbyists (including specifications focusing only on revolving door lobbyists) when shadow lobbyists move in or out of a firm. This confirms that the shadow lobbyist transitions are not occurring as part of exceptional firm-specific demand shocks and seem to be distinct revenue-generating events for firms.

6.3 Rainmakers? Intensive Versus Extensive Margin Effects.

Our firm-level revenue measures are built up from contract-level information that is defined in terms of different clients. Usefully, this allows us to distinguish between the intensive and extensive margin effects of shadow lobbyists. Specifically, the revenue contribution of shadow lobbyists could be derived from the securing of new clients, through the re-pricing of services to existing clients, or a via a mixture of both effects.

Client acquisition is a driving force in high-level professional services industries and 'rainmakers' who can bring multiple new clients to a firm are highly valued. In contrast, intensive margin effects are more subtle. The re-pricing of contracts could occur either because explicit new services are being added (i.e., a shadow lobbyist directly contributes services) or because of 'brand' effects. That is, the elite status of shadow lobbyists could increase the prestige of a firm and allow it to charge more across all of its client base.

In Table 5 we investigate this first by using the (log) number of clients as the dependent variable in our main model. This is reported in column (2) with our baseline log revenue model repeated in column (1). There is a strong relationship such that one additional lobbyist is associated with approximately 14.7% (i.e., exp(0.137)) more clients.[19] Next, in column (3), we include the log number of clients as a regressor in our main revenue model. This effectively controls for shifts in log revenue that are due to changes in the client base. The estimated coefficient on the shadow lobbyists variable can now be interpreted as a pure intensive margin effect. The estimated effect (0.058 (0.034)) falls by nearly three-quarters relative to column (1), indicating that the extensive margin is driving the overall shadow lobbyist effect.

However, this dominant role of the extensive margin in determining shifts in total revenue is not unique to shadow lobbyists. In the final two columns of Table 5 we present our disaggregated model which separates out lobbyists by type. The inclusion of log clients as a control leads to comparable changes in coefficients, indicating the pervasive role of the extensive margin in the lobbying industry. That said, the evidence here means that we can rule out 'brand' or prestige effects - which would have raised the value of existing contracts - as a main mechanism underpinning our shadow lobbyist effect.

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[19]More tangibly, there are an average 42 clients per firm-period in our data, implying 6.3 new clients based on this model estimate.

Table 5: Intensive versus Extensive Margin - Client Models.

|	(1) ln(Revenue)	(2) ln(Clients)	(3) ln(Revenue)	(4) ln(Revenue)	(5) ln(Revenue)
Shadow (All)	0.202***	0.137***	0.058*		
	(0.063)	(0.056)	(0.034)		
Unregistered Ex-Staffers				0.173***	0.046
				(0.073)	(0.036)
Unregistered Ex-Congressmen				0.173	0.071
				(0.107)	(0.068)
Registered Ex-Staffers				0.091***	0.008
				(0.016)	(0.006)
Registered Ex-Congressmen				0.164***	0.045**
				(0.043)	(0.021)
Registered Lobbyists	0.068***	0.055***	0.010***	0.059***	0.009***
	(0.005)	(0.004)	(0.002)	(0.006)	(0.002)
log(Clients)			1.056***		1.052***
			(0.037)		(0.037)
Number of Firms	129	129	129	129	129
Number of Obs	2,631	2,631	2,631	2,631	2,631

Notes: All standard errors are clustered at the firm level. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. All regressions include firm and time fixed effects. Column (1) repeats our baseline specification from Table 3. In column (2) log(Clients) is the dependent variable. Column (3) then returns to the log(Revenue) specification but includes log(Clients) as a RHS control variable. In the final two columns the dependent variable is log(Revenue).

Figure 5: Non-registered 'Placebo' Support Workers

(a) Within Groups
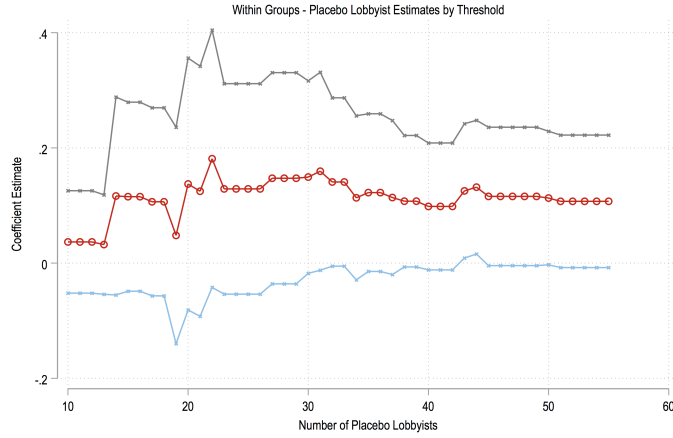
(b) First Differences (2-periods)
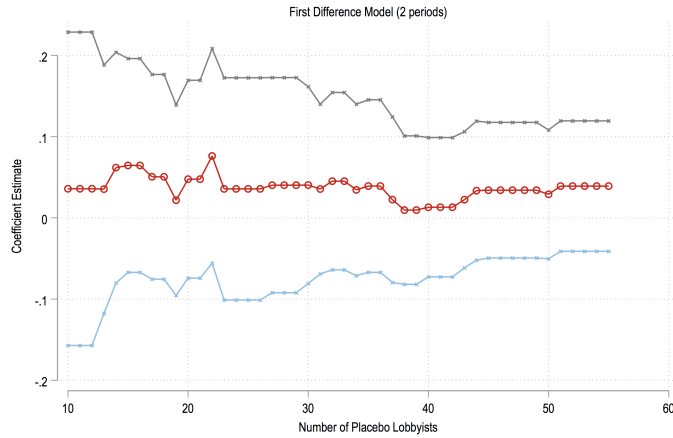
Notes: These figures show estimates for different numbers of non-registered 'placebo' lobbying firm employees drawn from our Wayback Machine historical lobbying firm sample. In each regression we randomly select k employees from the database and construct a count variable based on their actual employment histories. We vary k by integer step between 1 and 55 along the x-axis. The estimates are reported with 95% confidence intervals.

6.4 Non-registered 'Placebo' Support Workers

Our next exercise looks into the general effects of non-registered employees working in support roles on firm revenue. As discussed, it is plausible that there could be a general indirect effect on lobbying revenues associated with the addition of staff at a firm, even if they are unregistered. Our approach tests for this by looking at the effects of a set of 'placebo' unregistered employees. This is the group of employees with no known experience as Congressional staffers or office holders. The information on this set of employees was assembled from historical *Wayback Machine* data according to the procedure described in section 3.4.

Our overall sample of unregistered employees with no Congressional experience comprises of 4,969 workers. This is very large compared to our set of 43 actual unregistered shadow lobbyists. Hence, to construct a realistic comparison we randomly sample from the population frame of 4,969 workers according to different thresholds in the range of 10-55 workers. This gives us a set of historical employment episodes at lobbying firms that we collapse down into firm-level count variables. These placebo worker counts can then be included as an additional variable in our main within-groups and first-difference specifications. Overall, this exercise is similar in spirit to a randomization inference-style procedure except that we are using a set of real-world employment histories (rather than arbitrary permutations), as well as testing for the effects of different numbers of potential unregistered lobbyists.

We report estimates for the non-registered, placebo worker variable for different thresholds in Figure 5. On average, these non-registered, non-Congressional workers contained in the 4,969 worker sample have longer employment spells than our shadow lobbyists. That means a comparable level of variation across the variables is reached at the level of 15 non-registered workers.[20] It is apparent from Figure 5 that the placebo variable is not significant across any of our functional forms (within groups, first difference) at the 15 worker threshold. The placebo variable does drift towards significance as we approach more than 40 workers in the within-groups model but this is not repeated for the first-difference models which deal with trends or nonstationarity. Indeed, if we include firm trends in a within-groups model featuring a placebo variable derived from 43 workers the estimate falls to a negligible -0.002(0.061). This pattern is repeated even when we condition on only using the most senior non-registered

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[20]To be specific: 6.1% of observations in our main $N = 2,631$ firm sample have non-zero values for the $Shadow_{jt}$ variable, which is underpinned by 43 unique shadow lobbyists overall. Only 15 non-registered workers need to be used to generate an equivalent level of 6.1% non-zero observations for the placebo variable.

lobbying firm employees as part of our initial pool of workers. This is shown in Table 11 in the Appendix. Interestingly, the placebo effect is stronger when using very senior employees but it is still formally weak when considered at the comparable threshold for variation (i.e. 15 workers).

In Appendix Table A1 we also test some alternative placebos based on ex-Congressional staffers who only had junior roles during their time in the Congress. These include both registered and unregistered ex-staffers. The pattern of results here is similar to that of our *Wayback Machine* placebo. While there is weak evidence that very junior registered ex-staffers may drive a revenue contribution in the within-groups model, this is attenuated once the first differencing is implemented. There is no trace of an effect related to unregistered junior staffers.

Two takeaway results useful for our overall study arise from Figure 5 and Table A1. Firstly, we do not pick up any systematic effect of placebo workers on firm lobbying revenues across specifications. As a result, the data do not support the hypothesis that general 'support workers' have a strong influence on firm revenues through channels such as freeing up the time of registered lobbyists. This is important for the interpretation of our shadow lobbyist effect because it implies that the window for our ex-Congressional shadow lobbyists to have a passive, indirect effect on firm revenues is very limited. Secondly, the estimated coefficients on our main shadow lobbyist variables are unaffected by adding the control for unregistered placebo workers, again showing the robustness of the main results to the inclusion of additional time-varying influences on firm revenues.

6.5   Shadow Lobbying in Small and Medium-sized Firms

As discussed, our estimates are premised on the sample of 'large' firms with 10 or more lobbyists where, following Table 1, shadow lobbying activity is heavily concentrated. We explore the association between revenues and potential shadow lobbyist activity across different firm size bands in Table 6. As section 4.1 outlined, there are 61 posited shadow lobbyists working in the small and mid-sized firms over our sample period. However, there are gaps in the revenue data, especially for firms with 5 or fewer registered lobbyists active per period. For example, while there are 47 shadow lobbyists affiliated with this group of firms, there are only 13 usable transitions in the firm-level data. In short, many of the small firms have a low attachment to the federal lobbying industry and do not necessarily report lobbying-derived revenues across many continuous periods.

Table 6: Shadow Lobbying and Firm Revenues in Small and Medium-Sized Firms.

	(1) Large	(2) Mid-Sized	(3) Small	(4) Mid & Small	(5) All Firms
A: Within-Groups					
Shadow	0.202***	-0.035	-0.460*	-0.144	0.156*
	(0.063)	(0.134)	(0.249)	(0.162)	(0.082)
Registered (All)	0.068***	0.146***	0.270***	0.192***	0.102***
	(0.005)	(0.011)	(0.010)	(0.010)	(0.006)
Number of Firms	129	205	3,669	3,874	4,003
Number of Obs	2,631	3,610	36,099	39,709	42,340
B: First Differences					
Δ Shadow	0.116***	-0.005	-0.095	-0.037	0.080**
	(0.042)	(0.054)	(0.114)	(0.059)	(0.040)
Δ Registered	0.048***	0.102***	0.200***	0.144***	0.073***
	(0.006)	(0.008)	(0.009)	(0.007)	(0.006)
Number of Firms	124	194	3,122	3,316	3,440
Number of Obs	2,354	3,140	27,271	30,11	32,765

Notes: All standard errors are clustered at the firm level. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. Panel (A) reports regressions within-groups ('levels') regressions for the four firm groups of 'Large' (10 or more registered lobbyists on average per period); 'Mid-Sized' (between 5-10 lobbyists); Small (less than 5 lobbyists); combined 'Mid & Small' (less than 10 lobbyists), and finally All Firms with no restrictions. Panel (B) reports first difference regressions for the same firm groups using 2-period differencing (effectively a 12-month difference). All regressions include firm and time fixed effects.

This is borne out in the estimates reported in Table 6, where we present results for our preferred within-groups and 2-period first difference models. Columns (2) and (3) isolate the mid-sized firms (more than 5 but strictly less than 10 lobbyists on average) and small firms (strictly less than 5 lobbyists) respectively. The results here are generally negative and imprecise. A weakly significant within-groups estimate for small firms is not corroborated by the first difference specification in panel (B). We are not able to execute a 'before-and-after' comparison of revenues for the mid-sized and small firms (i.e. an event study similar to Figure 7) because of the lack of continuously observed firms in this part of the sample.

The final column pools the data across all firms regardless of size. A shadow lobbyist effect is still evident and we can infer from columns (1)-(4) that this is driven by activity in the large firms. But the qualitative point here is that unregistered shadow lobbyist activity is heavily concentrated in the larger firms where abilities and contacts can be better leveraged or activity can be hidden.

Figure 6: Benchmarking Shadow Lobbyists

(a) Within Groups

(b) First Differences
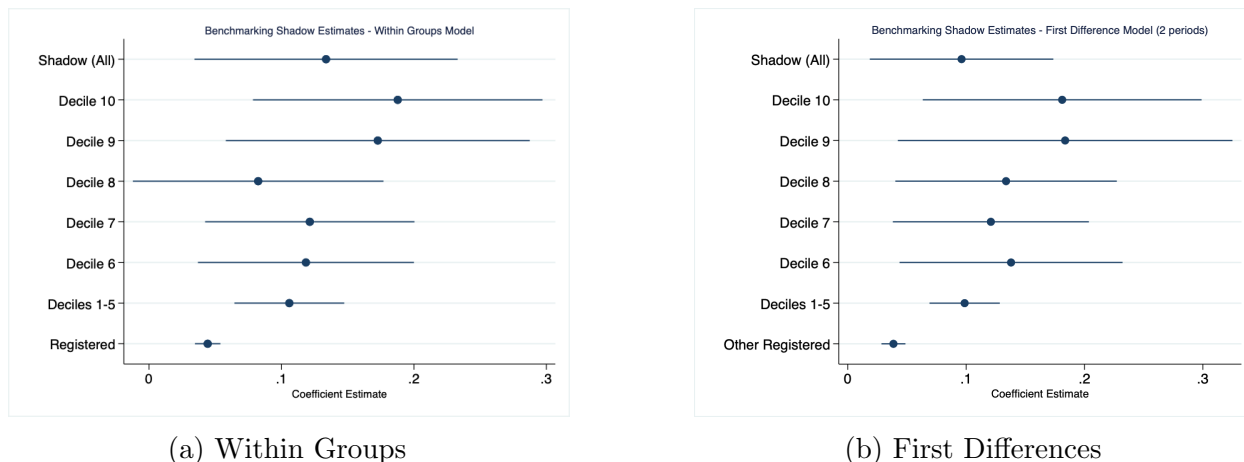
Notes: These figures show the results for specifications that include counts of registered lobbyists by fixed effects-based productivity decile alongside the shadow lobbyist variable. Deciles 1-5 are pooled together as the base case. Standard errors clustered by firm in these regressions with firm and time period fixed effects. The estimates are reported with 95% confidence intervals.

6.6 Benchmarking

We now benchmark the magnitude of the shadow lobbyist revenue effects found as part of the previous results. As discussed in section 5.1 (and detailed further in Appendix A.4) our approach here is to generate indicators of lobbyist productivity at the individual level and then plug these into firm-level regression models as variables analogous to our shadow lobbyist count variables. This allows us (for example) to compare the revenue effects of a 'superstar' registered lobbyist in the 9th or 10th decile of the lobbyist fixed effects distribution to the effects of a shadow lobbyist.

The results of this analysis are reported in Figure 6. We show the estimates for shadow lobbyists alongside estimates for the 10th to 6th deciles of revolving door lobbyists with deciles 1-5 pooled as the default category. This is repeated for the within-groups and the 2-period first difference models. The confidence intervals overlap in most cases so we can only make statements about differences in point estimates. The within-groups panel (a) puts the shadow lobbyists at the upper end of the registered revolving door lobbyist distribution (i.e. with point estimates above those for the 9th and 10th deciles), while the first difference model (panel (b)) positions them at around the median (i.e. between the estimates for deciles 1-5 and for the 6th decile).

The key insight from this benchmarking exercise is that the shadow lobbyist effect

that we uncover over the course of the paper is a realistic one, most likely putting shadow lobbyist around the middle of the registered lobbyist skill distribution. That said, an extra dimension of the analysis here is that shadow lobbyists are, following the LDA, presumed to spend no more than 20% of their time on lobbying activities. Simply put, this implies that shadow lobbyists are achieving a similar revenue effect to median registered lobbyists but with one-fifth of the time input of a full-time lobbyist.

6.7 Magnitudes

A natural remaining question for the results as presented is: how big are the detected shadow lobbyist effects in aggregate terms? The benchmarking exercise focused on comparisons of different estimated parameters but there is the more practical question of how much money can be attributed to shadow lobbyist activity in the overall industry context.

In order to study this we first calculate the predicted contribution of the growth of shadow lobbyists to total firm revenues. As a comparison, we also calculate the contribution of the registered, ex-Congressional 'revolving door' lobbyists to revenues. Recall from Figure 2 that there was also a major influx of registered, revolving door lobbyists into the industry during the mid-2000s. The basic form of the predicted revenue effect is as follows:

$$R_j^{Shadow} = \hat{\beta} * \sum_{t=1}^{T=t} Shadow_{jt} * \bar{R}_j \tag{5}$$

where $\hat{\beta}$ is the estimated shadow lobbyist coefficient and $\sum_{t=1}^{T=t} Shadow_{jt}$ is the sum of shadow lobbyist 'shocks' for firm j. We posit this expression as a measure of total shadow lobbyist inputs: in most cases it is a count of the number of periods that a firm had a non-zero value for the discrete shadow lobbyist indicator. This is in turn multiplied by firm-specific average revenues, $\bar{R}_j$. The overall expression therefore measures the total revenue that can be attributed to shadow lobbyists across all the periods that they were present in the firm. We then divide this by the total revenue reported by firm j over all periods when they appear in the data. The resulting number is the share of revenue that can be attributed to the sum of shadow lobbying inputs across time. In the case of revolving door lobbyists we perform a similar calculation.[21]

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[21]More precisely, we calculate $R_j^{Revolving} = \hat{\beta} * (\sum_{t=1}^{T=t} Revolving_{jt} - \sum_{t=1}^{T=t} Revolving_{j0}) * \bar{R}_j$, where $\hat{\beta}$ is the estimated coefficient for large firms. In short, this is designed so that the overall expression represents the growth of revolving door lobbyists over the sample period relative to an initial baseline stock. We do not adjust in this way for shadow lobbyists because, as the descriptive analysis shows, shadow lobbying is so close to zero at baseline.

The results of these calculations are presented in Table 7. We weight firms by their mean revenue so that the estimates represent the industry in aggregate.[22] Row (A) show the results for the shadow lobbying firms and indicates that 6.5% of total revenues can be explained by their shadow lobbying activity. This is about one-eighth as much as the amount due to the growth of registered revolving door lobbyists (the 50.5% figure reported in the second column). Interestingly, revolving door lobbyists account for a much lower share of total revenues for non-Shadow firms (row (B) - 33.6% versus 50.5%).

Hence, these decomposition results imply that the shadow lobbying firms grew faster both because of their hiring of shadow lobbyists but also because they added more connected, revolving door lobbyists. This is in line with the trends seen in Figure 2. Revolving door lobbyists began increasing around 2005 (see Figure 2(a)) with a consequent uplift in revenues (Figure 2(b)). This uplift was then sustained by the accelerated entry of shadow lobbyists from 2007 onwards. We can conclude that shadow lobbying was a significant driver of the divergence of the shadow sub-group of firms but that the rise in revolving door lobbying was more fundamental to this relative growth.

In closing, we highlight two sets of statistics that encapsulate the magnitudes well. Firstly, the 1.8% of total sectoral revenues explained by shadow lobbying can be thought of as a measure of 'unaccounted for influence'. That is, it is the fraction of industry revenues that can be explained by unregistered ex-Congressional personnel working at lobbying firms in our sample. Converting this into a tangible number we calculate that this 1.8% amounts to approximately $149 million which was mostly generated in the last 5 years of the period. Even more tangibly, we calculate that this translates to approximately $1.7 million per annum in revenue at the level of individual shadow lobbyists.[23] Hence, while the total amount of shadow-associated revenues could be considered modest from an industry perspective, the potential benefits for individual lobbyists is high.

Secondly, shadow lobbyists are an important component of the overall workforce at the shadow group of firms. By the end of our sample, we estimate that the shadow lobbying workforce is 3.8% as large as the total workforce of LDA registered lobbyists at these firms and 10.7% as large as the workforce of registered, revolving door lobbyists. We think that,

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[22]The unweighted estimates - that is, treating each firm equally when calculating the mean - are not qualitatively different. As an example, the unweighted figures for row (A) of Table 7 are 6.0% (shadow firms) and 31.0% (non-shadow)

[23]Specifically, this is the $149 million figure divided by the 85.5 years of total shadow lobbyist tenure in our main sample of large firms.

along with our estimates of revenue generation, these workforce numbers indicate that shadow lobbyists are clearly non-trivial inputs into the business activities of these firms.

Table 7: Aggregate Magnitudes.

Group	%Shadow	%Revolving Door	Total
(A) Shadow Lobbying Firms (N=29)	6.5	50.5	57.0
(B) Non-Shadow Firms (N= 100)	na	33.6	33.6
(C) Sector (N=129)	1.8	38.3	40.1

Notes: This table shows the results of the calculations described in section 6.7. Row (A) reports the revenue contributions of shadow versus revolving door lobbyists for the shadow lobbying firms and row (B) contains results for the remaining firms, Row (C) presents the total. We weight by average firm revenues when making these calculations.

7 Conclusion

In this paper we have tested for economic evidence of unregistered, 'shadow' lobbying activity amongst Washington lobbying firms. Our research design has focused on mapping the movements of ex-Congressional politicians and staffers into the lobbying revenue data in order to infer the potential contributions of these personnel. The shadow lobbyist database that we build shows two interesting patterns - a steady increase in shadow lobbying activity from the late 2000s onwards and a very notable clustering of this activity in larger firms where shadow lobbying may be easier to conduct. This could plausibly be because the unregistered ex-Congressional personnel joining these large firms could have more leverage there (i.e. they have more co-workers to share tasks with) or because such an environment is more suitable for working close to (or above) the 20% rule for officially registering as a lobbyist.

Our empirical models then indicate that lobbying firm revenues do move significantly with the movements of these unregistered ex-Congressional personnel into and out of firms. In particular, the shifts in revenue we see cannot be conflated with general time shocks and occur discretely with the entry of shadow lobbyists into a firm. Further to this, we are able to rule out the hypothesis that these revenue effects are the result of a general 'non-lobbying worker' effect by looking at a sub-group of unregistered workers with no track record of

working in the Congress. We find that no revenue effects are associated with the movements of these unregistered 'placebo' workers into and and out of lobbying firms.

The shadow economy of potential unregistered lobbying that we uncover is modest in size but strategically positioned. By the end of the period that we consider, shadow lobbyists represent around 10% of the ex-Congressional workforce in Washington's largest and most influential lobbying firms. While it is not possible to calculate individual rewards precisely, based on the average $1.7 million of revenues that is generated by shadow lobbyists such rewards are likely to be high. Our estimates also only capture the activity that is visible via registered, multi-client lobbying firms. It necessarily misses individuals who argued to be working for in-house lobbying groups (such as former Speaker Newt Gingrich) and the 'deep underground' of unregistered organisations that partially overlap with the lobbying industry (such as think-tanks).

Finally, we stress that our results cannot be interpreted as evidence of illegal or corrupt activity. The activities of the unregistered shadow lobbyists within the relevant firms are simply not observable using any dataset that we are aware of. However, our benchmarking of the observed shadow lobbyist effects against those for registered 'revolving door' lobbyists indicates that they (the shadow lobbyists) have effects that are equivalent to the median 'revolving door' lobbyists but with (notionally) one-fifth of the effort. Hence, even if the 20% rule is being adhered to it is clear that unregistered ex-Congressional personnel provide some highly valuable inputs to Washington lobbying firms. Given that transparency policies such as the LDA are meant to capture economically important influence activities, our paper provides an impetus for either a program of better monitoring with respect to LDA compliance or a revision of the legislation to make the reporting requirements on activities counted as part of the 20% rule more rigorous.

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A Data and Additional Background Appendix

A.1 Additional Background: Regulation of Revolving Door Movements

Ongoing controversy over the role of lobbyists contributed to the introduction in September 2007 to a set of reforms as part of the Honest Leadership and Open Government Act (HLOGA). This legislation contained some measures relating to the reporting of activity and campaign contributions but, for the first time, also imposed a set of restrictions on revolving door movements out of Congress.[24]

This was done mainly via the introduction of 'cooling off' periods on lobbying activity for staffers and Members who leave employment with the Congress. Senators became subject to a two-year cooling off period during which they were prohibited from making lobbying contact with any current employee or elected office holder in either the House or the Senate. Senior staff from the Senate (defined as those staffers earning at least 75% of a Member's salary) also faced a 12-month ban on making lobbying contacts with any former Senate colleagues. The remaining, less senior Senate staff faced a narrower ban with a 12-month prohibition on lobbying contacts with their former office of employment within the Congress.[25] Senior staffers in the House were also subject to this type of narrow ban on lobbying their specific former office of employment for 12 months.

One important point to note here is that these cooling off restrictions do not prevent an ex-staffer or Member from registering as a lobbyist during the defined periods, they simply limit their ability to make certain types of contacts. Ex-staffers or Members are therefore still able to register and make lobbying contacts with offices outside of the proscribed areas.

A second wave of revolving door restrictions were introduced in January 2009 as part the Obama Administration's Executive Order 12490, which was framed as an 'Ethics Pledge' for new appointees in all executive agencies. This applied similar post-employment bans to HLOGA for executive agency staff but also introduced qualitatively new 'reverse cooling off' restrictions.[26] The restriction applied to new appointees and stipulated that they were not

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[24]In terms of reporting, the frequency of reports was increased to quarterly and new requirements were introduced for the reporting of campaign contribution 'bundling' by lobbyists and member travel financed by outside sources.

[25]For example, Senate personal office staff may not lobby their specific former personal office or any committee where they may have had 'substantive committee responsibilities'. Dedicated committee and leadership office staff face analogous limits. See SOPR (2012) *Guidance on the Post-Employment Contact Ban* for details.

[26]Specifically, a two-year prohibition on communicating with employees of one's former executive agency

allowed to have lobbied the executive agency that they aspired to join for two years *before* the proposed date of appointment (White House 2009).

In September 2009, the Obama Administration then extended the coverage of this reverse cooling off restriction on former registered lobbyists to all executive agency advisory boards and commissions. This move created palpable angst in the lobbying industry and provoked some legal challenges by lobbyists affected by the ban (Smith 2012). The Obama Administration has also granted a number of waivers to Executive Order 12490 since 2009 but the majority of these have been at very senior levels (US Office of Government Ethics 2017). Hence the 'reverse cooling off' restriction still represents a major obstacle to ex-lobbyists aspiring to move into executive agency positions over their career.

A.2 Ex-Staffer Employment Destinations - Independent Survey

We conducted a targeted independent survey (using web-based resources) of staffer employment destinations after leaving the Congress. This was designed to evaluate the completeness of the LegiStorm database and, in particular, check the extent to which LegiStorm's biographical data is successful in picking up the majority of shadow lobbyist activity in Washington.

The focus of this survey was the cohort of staffers who left the Congress between 2004-2007. We chose this period because it occurs just as LegiStorm was established (circa 2006), creating the concern that the rise in shadow lobbying we see in the data after 2007 might be a result of incomplete coverage earlier on. Targeting this group further, we focus on a subset of 2,700 'Top Aides' (Chief of Staff, Deputy Chief of Staff, Legislative Director, Legislative Assistant, Legislative Counsel, Legislative Correspondent, and Communications Director) most likely to be attracted by unregistered lobbying opportunities.

Out of these 2,700 individuals, we remove three sets of individuals: (i) 500 individuals who are officially registered lobbyists but did not have their employment history tracked by LegiStorm; (ii) 267 individuals who are officially registered lobbyists (as they appear in lobbying contracts tracked down by Opensecrets) with LegiStorm also following their employment history; (iii) 100 individuals who are tracked by LegiStorm while not being officially registered lobbyists. We can remove these individuals as the information we have already allows us to track them in our data.

After subtracting these individuals with biographical data in LegiStorm we are left with

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after leaving a position and an even stronger ban on contact for the remainder of the Administration if an appointee left to be an official registered lobbyist.

1,849 people whose post-Congress employment destination is unknown. We randomly select around 20% of this group for our survey. Their post-congressional destinations have been obtained via a manual search using information from LinkedIn, Wikipedia, and Google. Table A3 shows the job-destinations of a random sample of 381 ex-congressional staffers occupying the following positions during their work spells in Congress: Chief of Staff (32%), Deputy Chief of Staff (3%), Legislative Director (4%), Legislative Assistant (25%), Communications Director (7%), Legislative Correspondent (28%), and Legislative Counsel (2%).

In almost 50% of the cases these workers move to occupy positions in government, politics, and the private and legal sector. In only two percent of the cases did we find ex-Congressional staffers being employed in lobbying firms within Washington DC, even if not officially registered as lobbyists. For our purposes, it is worth noticing that the lobbying firms in Washington DC hiring these ex-Congressional staffers are quite limited in their activity: Reed Smith (6.8 registered lobbyist per period, on average); Powell Tate (5.1); Jennings Policy Strategy (1.6); DCI group (8.2); Elmendorf Strategy (4.7); Financial Dynamics (1 registered lobbyist). Crucially, these firms are small and do not meet the threshold for inclusion in our main sample (i.e. a minimum average of 10 lobbyists per period). This suggests that LegiStorm's biographical database is very effective at tracking the movement of unregistered ex-Congressional personnel into large Washington lobbying firms, the main focus of our estimates.

A.3 Digging Deeper: The Wayback Machine

A.3.1 Supplementing the Independent Survey.

The exercise described above did not provide any result for 109 ex-Congressional staffers, for whom we were unable to track their career (26% of the 412 positions occupied by our 381 individuals in Table A3). To further investigate the presence of potential shadow lobbyists within this pool of ex-congressional staffers we took a different strategy. In this exercise we have attempted to reconstruct the historical list of all workers in lobbying firms contained in our estimating sample. We then checked for the presence of these 109 'missing-in-action' ex-Congressional staffers within these lobbying firms.

To perform this analysis, we used the *Wayback Machine* digital archive of the world wide web created by the Internet Archive, a non-profit organization, based in San Francisco. The Internet Archive launched the *Wayback Machine* in October 2001. The service enables

users to see archived versions of web pages across time. In turn, we are able to do a census of Washington lobbying firm employees using historical information that has disappeared from current versions of company websites.

For each firm in our estimating sample we: 1) searched the most recent company website; 2) copied and pasted the URL into the *Wayback Machine*; 3) searched for the version of the website as close as possible to the year 2008; and 4) downloaded the list of all workers in the firm who were based in Washington DC. We then standardized the names of these workers (eg: removing suffixes, prefixes, titles and nicknames).

The final step consisted of a manual search of the names contained in the list of 109 ex-Congressional staffers with unknown destinations amongst the list of workers downloaded via the *Wayback Machine*. The manual search did not reveal any single instance in which the given name and surname of ex-Congressional staffers matched that of a worker employed in a lobbying firm. Both lists of workers are available upon request.

Overall, these two exercises reduce concerns about the presence of significant omissions in the tracking of ex-Congressional staffers working on an unregistered basis in our sample of large lobbying firms.

A.3.2 'Placebo' Non-Registered Workers

To construct the group of unregistered 'placebo' workers with no experience of working in the Congress we started by using the master list of 6,516 workers obtained via the exercise described above that used the *Wayback Machine*. Recall that this group represents the population of *all* type of employees working in these firms, not just registered lobbyists.

Using a name matching procedure, we removed the registered lobbyists and unregistered shadow lobbyists from this overall list. This left 4,969 workers employed in our lobbying firms in a variety of roles during the period of analysis. We then drew a random sample of 55 workers since this amount allowed us to mimic a similar level of variation to that generated by the set of shadow lobbyists.

We then reconstructed their career paths using *LinkedIn* and similar sources. Around 75% of these workers were employed in the role of Associate or Partner, whilst the remainder took up various roles such as Analyst, Counsel, and Senior Associate. We were able to precisely track the semester of entry and exit (if applicable) of these individuals for the lobbying firms in our sample.

We were therefore able to construct a measure of 'placebo' workers within firms over

time. This analysis reduces the concern that the effects we identify could simply reflect a mechanical increase in lobbying revenue associated with a generic expansion in the labor force within the firm.

A.4 Benchmarking Analysis

A.4.1 Overall Approach

The second part of our research design asks: is the observed, statistical pattern of revenue shocks we find consistent with a level of worker inputs that plausibly fits with the 20% rule? In principle, the unregistered ex-Congressional officials we study are able to contribute to firm lobbying activities up to this (unmonitored) 20% threshold of time.

The actual revenue that results from this notional 20% of full-time inputs will be shaped by the observable and unobservable determinants of productivity that prevail at the individual level. Having estimates of the parameters on these determinants would therefore let us develop some bounds on the work contributions of shadow lobbyists. The practical question here is: based on their productive characteristics, what revenue effects would we expect from a set of ex-Congressional lobbyists working 20% of the time?

Our window into these determinants is the data on registered lobbyists. Specifically, we develop an analysis of the individual revenues of registered ex-Congressional officials as they come from the same notional population as the unregistered shadow lobbyists. We formulate the following empirical model of individual lobbyist revenues:

$$ln(R_{it}) = \mu_i + \mu_j + \tau_t + X_{it}'\theta + \varepsilon_{it} \tag{6}$$

where $ln(R_{it})$ is (log) revenues at the individual lobbyist level while X_{it} is a vector of lobbyist characteristics (such job titles held in the Congress or tenure in the lobbying industry). The terms μ_i and μ_j are lobbyist and lobbying firm fixed effects, respectively. Finally, τ_t is a common time effect and ε_{it} is the unobserved error term.

Our main interest in this model is the breakdown between the observable and unobservable components, along with estimates of parameters on the observables. To preview the findings, we will see that observables explain a small fraction of lobbyist-level revenues - most of the action lies in the individual fixed effects. This leads us to a strategy for quantifying the role of unobserved skills in the firm-level revenue equations.

Specifically, based on this individual-level model, we can construct benchmarks for the

contribution of unobservable lobbyist 'talent' to firm revenues. We do this by extracting the estimates of the individual lobbyist fixed effects, $\hat{\mu}_i$ and ranking lobbyists by deciles. Lobbyists in the top decile according to their $\hat{\mu}_i$ can be characterised as the 'superstars' with the highest level of unobservable talent for attracting revenue to firms. Indicators for different types of lobbyists can then be aggregated up to quantify the effects of such workers in the type of firm-level models that we use for studying the revenue effects of shadow lobbyists.

Based on this approach, we can denote lobbyists according to their fixed effect by an indicator variable $STAR_i^d$ where d indexes the decile. This indicator is then aggregated up by firm-period to get a new count variable $STAR_{jt}^d = \sum_{i=1}^{D^j} Star_{ijt}^d$ (where D_j denotes the number lobbyists of decile type d in firm j). This allows us to characterize the contribution of 'superstars' (or indeed, other lower ranked lobbyists) in the firm-level revenue equations as follows:

$$ln(R_{jt}) = \alpha_j + \gamma_t + \beta Shadow_{jt} + \lambda_1 STAR_{jt}^{10} + \lambda_2 NONSTAR_{jt} + \epsilon_{jt} \qquad (7)$$

where $NONSTAR_{jt}$ is a count of all of the registered lobbyists in firm j at time t, excluding those in the top $d = 10$ decile of lobbyists by their individual-level fixed effect. We can break the $NONSTAR_{jt}$ variable to parse out other types of lobbyists, for example $STAR_{jt}^9$ for those in the ninth decile, $STAR_{jt}^8$ for those in the eighth decile and so on. With this information, we can then make an assessment of where (based on the estimated $\hat{\beta}$) the shadow lobbyists may fit in terms of the distribution of lobbyist productivity. In particular, we run a specification that encompasses the top 5 deciles of the lobbyist fixed effect distribution since this allows us to observe the gradient of lobbyist productivity and assess how distinctive the top deciles may be.

A.4.2 Determinants of Lobbyist Revenue.

Appendix Table A2 reports information on the adjusted R^2 from implementations of the lobbyist-level equation eq. (6). The aim of this exercise is to gauge the overall contribution of observables versus unobservables to revenues in this setting. The available observables are: indicators for whether a lobbyist is an ex-staffer; a set of common job titles (defined in terms of highest post held); and periods of experience working in the lobbying industry.

The first four columns of Appendix Table A2 run lobbyist or 'worker'-level regressions of log revenue on these available characteristics. Column (1) with a simple dummy ('Revolving Door (All)') for whether a lobbyist has any Congressional work experience. Column (2) then

adds in various job titles dummies for jobs held in the Congress but this barely boots the baseline adjusted R^2 of 0.091. The addition of tenure-in-lobbying variable in column (3) does however boost the adjusted R^2 measure (to 0.130). That said, column (3) indicates that it is worker unobservables (captured by lobbyist fixed effects) that have the most explanatory power, raising the adjusted R^2 to 0.69.[27]

In the last three columns of Table A2 we then use the observables to try and explain the lobbyist fixed effects themselves. In this case around 20% of the variation in the fixed effects are explained but only a small portion of this can be accounted for by job titles. It seems clear then that, once the basic fact of having a prior employment connection to the Congress is taken into account, there is limited scope for other observables (e.g. job titles, tenure) to explain what drives differences in lobbyist revenues.

Appendix Figure 8 then plots the distribution of fixed effects for the set of lobbyists working in our sample firms, where we have conditioned on at least three periods of activity in the lobbying industry for inclusion in this analysis. The figure clearly shows how ex-Congressional staffers dominate the industry in terms of average revenues. Our regression exercise then involves dividing this distribution of fixed effects estimates into deciles and constructing firm-level counts of lobbyists by their levels of unobservable contribution.

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[27]We include both worker and firm fixed effects in column (4) for the sake of parsimony in presentation. If we include only firm effects the adjusted R^2 is 0.32 and if we include only worker effects then the adjusted R^2 is 0.68.

Table A1: Alternative Placebos based on Junior Staffers.

	(1)	(2)	(3)	(4) 2-period difference Δ_k	(5) 2-period difference Δ_k	(6) 4-period difference Δ_k	(7) 4-period difference Δ_k
	log(Rev)	log(Rev)	log(Rev)	log(Rev)	log(Rev)	log(Rev)	log(Rev)
Shadow Lobbyists (All)	0.202*** (0.063)		0.204*** (0.062)				
Junior Unregistered Ex-Staffers		0.041 (0.076)	0.063 (0.078)				
Junior Registered Ex-Staffers		0.120* (0.063)	0.116* (0.062)				
Δ_k Shadow Lobbyists (All)					0.117*** (0.042)		0.142*** (0.053)
Δ_k Junior Unregistered Ex-Staffers				-0.008 (0.033)	0.010 (0.032)	-0.053 (0.033)	-0.033 (0.034)
Δ_k Junior Registered Ex-Staffers				0.029 (0.043)	0.028 (0.043)	0.021 (0.042)	0.027 (0.042)
Registered Lobbyists	0.068*** (0.005)	0.068*** (0.005)	0.067*** (0.005)				
Δ_k Registered Lobbyists				0.048*** (0.006)	0.048*** (0.006)	0.054*** (0.006)	0.054*** (0.006)
Number of Firms	129	129	129	124	124	119	119
Number of Observations	2,631	2,631	2,631	2,354	2,354	2,110	2,110

Notes: We control for firm and time fixed effects in columns (1)-(3). The difference specifications in column (4)-(7) include time fixed effects only (firm effects are differenced out). All standard errors are clustered at the firm level. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. The variables Shadow Lobbyists (All) and Registered Lobbyists are defined as per the notes in Table 2. The variables Junior Unregistered Ex-Staffers and Junior Registered Ex-Staffers represent counts of junior ex-staffers (Intern, Page, Staff Assistant, Scheduler) employed at the firm. As per main tables, Δ_k denotes where a variable has been differenced across k-periods. Columns (4)-(5) implment 2-period differencing, columns (6)-(7) implement 4-period differencing.

Table A2: Observable and Unobservable Determinants of Lobbyist-level Revenues

|	(1) log(Revenue)	(2) log(Revenue)	(3) log(Revenue)	(4) log(Revenue)	(5) Worker FE	(6) Worker FE	(7) Worker FE
Revolving Door (All)	0.660***	0.651***	0.713***	na	0.800***	0.749***	0.555***
	(0.038)	(0.044)	(0.042)		(0.034)	(0.040)	(0.037)
Staff Director		0.163	0.250*			0.356***	0.235**
		(0.149)	(0.156)			(0.163)	(0.148)
Chief of Staff		0.172***	0.282***			0.248***	0.162**
		(0.085)	(0.082)			(0.080)	(0.070)
Legislative Director		0.101	0.192**			0.211**	0.155**
		(0.078)	(0.079)			(0.076)	(0.069)
Legislative Assistant		-0.125**	0.024			0.082**	0.028
		(0.062)	(0.064)			(0.059)	(0.060)
Ex-Congressman		-0.057	-0.122			-0.198	-0.0126
		(0.107)	(0.100)			(0.090)	(0.086)
Tenure			0.046***		0.049***	0.050***	0.049***
			(0.003)		(0.002)	(0.002)	(0.002)
Adjusted R2	0.091	0.092	0.130	0.68	0.214	0.220	0.465
Lobbyist fixed effects	No	No	No	Yes	na	na	na
Firm fixed effects	No	No	No	Yes	No	No	Yes
Number of Firms	129	129	129	129	129	129	129
Number of Lobbyists	3,110	3,110	3,110	3,110	3,110	3,110	3,110
Number of Observations	43,167	43,167	43,167	43,167	3,110	3,110	3,110

Notes: Columns (1)-(4) estimate lobbyist-level revenue equations for the lobbyists working in our main sample firms. Revolving Door (All) is a dummy for any lobbyist with experience as a Congressional staffer or Congressman. The variables Staff Director, Chief of Staff Legislative Assistant are then dummy variable for whether a lobbyist ever served in that type of position. No coefficients for the positions are defined in column (4) because they are absorbed by the lobbyist fixed effects. Columns (5)-(6) then use estimates of the lobbyist fixed effects as dependent variables. The variable Tenure measures the number of periods a lobbyist has been active in lobbying. This is defined as the total number of periods in Columns (5)-(6).

Table A3: Destinations of a Random Sample of 381 Ex-Congressional Staffers.

Employment Sector	Number Ex-Staffers	% Share
Government	63	0.153
Private	49	0.119
Politics	27	0.066
Communications	24	0.058
Other Type of Employment	15	0.036
University	14	0.034
Lobbying firm (outside Washington DC)	11	0.027
Non-Profit	9	0.022
Health	9	0.022
Education	9	0.022
Lobbying firm (Washington DC)	9	0.022
Retired	8	0.019
In-House Lobbying (registred org)	7	0.017
Deceased	5	0.012
Non-Corporate Advocacy	2	0.005
Not Found	109	0.265
Total Positions	412	1.000

Notes: this table shows the job-destinations of a random sample of 381 workers across 412 positions. Percentages of job-destinations after Congress do not sum-up to 100%, because some cases the worker switches job and type of industry, or because the position can be defined by two labels (e.g. Government Position in the Health Department). This corresponds to 20% of our sample, previously employed in Congress occupying positions such as: Chief of Staff (32%), Deputy Chief of Staff (3%), Legislative Director (4%), Legislative Assistant (25%), Communications Director (7%), Legislative Correspondent (28%), and Legislative Counsel (2%). Files containing the manual search, notes, and related links for this random sample of ex-congressional staffers are available upon request. Government positions include security, defense, diplomacy, administration. Politics positions include positions in the White House or non-congressional offices.

Figure 7: Entry Effects of Shadow Lobbyists.
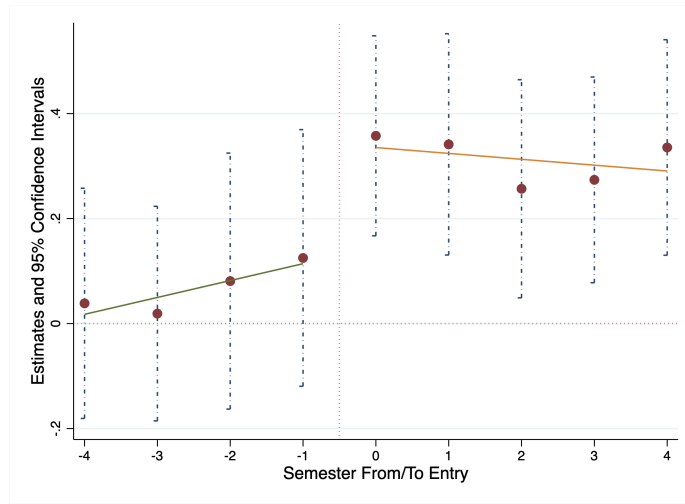
Notes: This figure shows period-by-period estimates of firm revenues for the subset of shadow lobbyist transitions associated with the first entry of any shadow lobbyist into a firm. This is shown for 4 periods before and 4 periods after with period 0 denoting the switch point. Specification includes firm fixed effects, period fixed effects and a control for the total number of registered lobbyists. Standard errors are clustered at the firm-level.

Figure 8: Distribution of Lobbyist Fixed Effect Estimates.
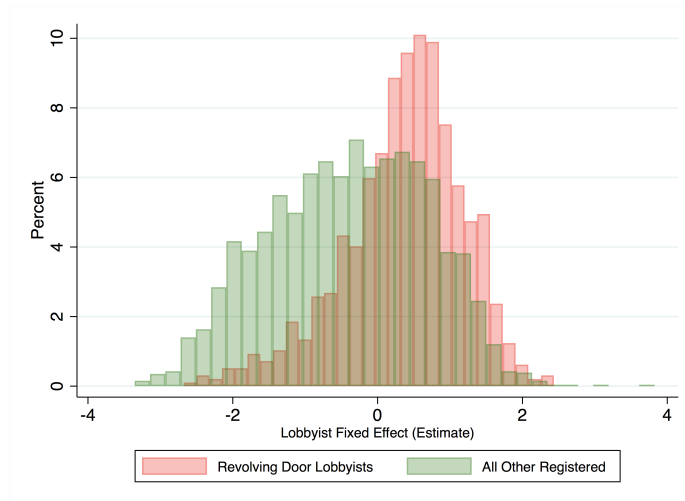

Notes: The figure shows the distribution of lobbyist fixed effect estimates for the sample of 129 large lobbying firms studied in Tables 3-6. This represents 5,631 unique registered lobbyists where 970 are revolving door lobbyists with previous Congressional work experience. We condition on the lobbyist appearing for at least 5 semesters (2.5 years) in the lobbying reports data before estimating the fixed effects.

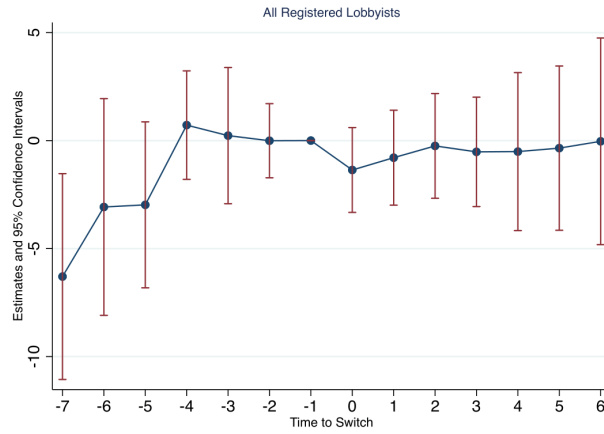Figure 9: Testing for Correlated Input Shocks - Entry Only

(a) Any Registered Lobbyist
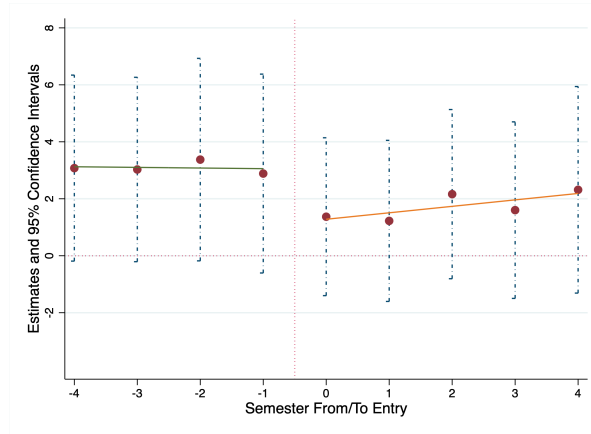
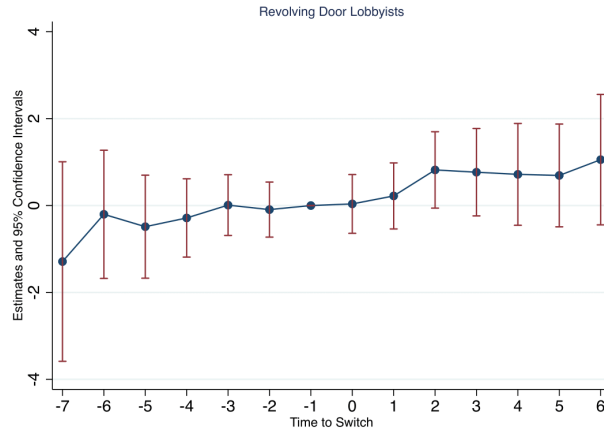

(b) Revolving Door Lobbyists
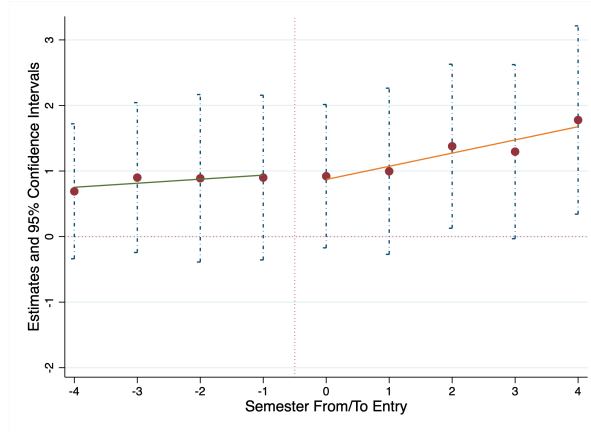
Notes: These figures show the evolution evolution of registered lobbyists with respect to first entry of a Shadow Lobbyist into a firm. Panel (a) shows results for Any Registered Lobbyist while panel (b) uses Revolving Door Lobbyists (that is, lobbyists with prior Congressional work experience) as the dependent variable. Firm and time fixed effects are included. Standard errors are clustered at the firm level, 95% confidence interval shown.

Figure 10: Testing for Correlated Input Shocks - All Transitions (DID-M)

### (a) Any Registered Lobbyist


(b) Revolving Door Lobbyists

Notes: These figures show estimates from a DID-M estimation (following de Chaisemartin and D'Haultfœuille (2020)) of the evolution of registered lobbyists with respect to transitions of Shadow Lobbyists. Panel (a) shows results for Any Registered Lobbyist while panel (b) uses Revolving Door Lobbyists (that is, lobbyists with prior Congressional work experience) as the dependent variable. Firm and time fixed effects are included. Standard errors are clustered at the firm level, 95% confidence interval shown.

Figure 11: Non-registered 'Placebo' Support Workers (Senior Employees)

(a) Within Groups
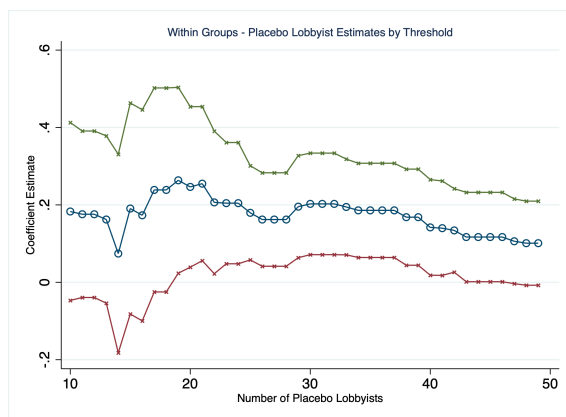
(b) First Differences (2-periods)
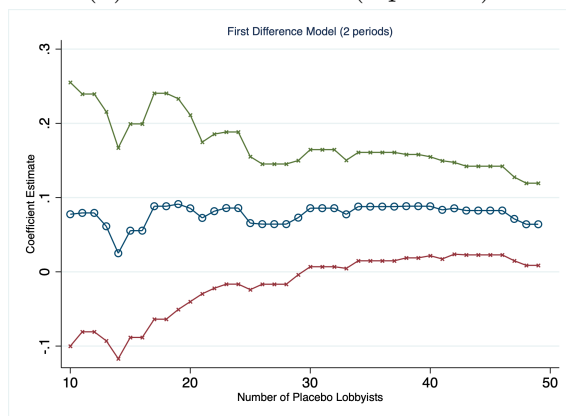
Notes: These figures show estimates for different numbers of non-registered 'placebo' lobbying firm employees drawn from our Wayback Machine historical lobbying firm sample. It is the same as the version presented in Figure 5 except that we condition only on including senior firm employees when defining out initial sample. These senior employees are defined as either: Associates, Senior Associates, Partners, and Partner or Cheif Human Resource Office. In each regression we randomly select k employees from the database and construct a count variable based on their actual employment histories. We vary k by integer step between 1 and 50 along the x-axis. The estimates are reported with 95% confidence intervals.

Table A4: Firm-Level Revenue Models with Matched Controls

	(1) log(Revenue)	(2) log(Revenue)	(3) Δ log(Revenue)	(4) Δlog(Revenue)
Unregistered Ex-Staffers	0.173**	0.202***	0.112***	0.129***
	(0.072)	(0.071)	(0.038)	(0.038)
Unregistered Ex-Congressmen	0.173*	0.161	0.067	0.052
	(0.107)	(0.107)	(0.108)	(0.101)
Registered Ex-Staffers	0.091***	0.108***	0.095***	0.088***
	(0.016)	(0.025)	(0.012)	(0.016)
Registered Ex-Congressmen	0.164***	0.130	0.175***	0.114**
	(0.043)	(0.079)	(0.035)	(0.054)
Registered Lobbyists	0.059***	0.068***	0.037***	0.047***
	(0.006)	(0.005)	(0.005)	(0.006)
F-stat (Staffers)		1.49(0.23)		1.03(0.31)
F-stat (MoCs)		0.06(0.81)		0.31(0.58)
Number of Firms	129	129	129	129
Number of Observations	2,631	2,631	2,354	2,354

Notes: In all regressions we control for firm and time fixed effects. All standard errors are clustered at the firm level. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. Columns (2) and (4) use the propensity score matched subset of registered staffers and ex-Members of Congress (MoCs). Specifically, we use the variables outlined in Table 2 as covariates for estimating the propensity score and select the five nearest neigbours to each shadow staffer or MoC. This set of nearest neighbor observations is then collapsed into new count variables. The final two columns are 2-period first difference specifications (the independent variables in this case are defined as differences but we omit the Δ subscript for the sake of simmpler presentation). All reported variables are counts of the number of employees by type at the firm j in period t. Registered employees are those who appear on the disclosed lobbying reports forms. *Registered Ex-Staffers* represents former Congressional staffers who are registered as lobbyists while *Registered Ex-Congressmen* measures Ex-Congressmen who are registered. The *Shadow Lobbyists (All)* variable is comprised of *Unregistered Ex-Staffers* and *Unregistered Ex-Congressmen* summed together. The variable *Registered Lobbyists* represents the number of registered lobbyists working at the firm who are not already counted as part of variables such as *Registered Ex-Staffers* and *Registered Ex-Congressmen*.