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Why don't firms lobby? How information shapes the market for lobbying services

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Abstract:

Why do some firms lobby consistently while others never lobby at all? We argue that most firms lack information about the market for lobbying services. This implies that even when there are large returns associated with a political presence, most firms will not lobby. We present comprehensive evidence from US publicly traded firms supporting this argument. First, using a natural experiment, we show that only firms that already have a political presence use lobbying to deal with political shocks. Second, firms start lobbying when they receive an influx of information about political strategy among other firms. Third, the information effect is present among firms that can rely on trade associations for lobbying needs. Our results suggest that lack of information keeps firms from lobbying that would benefit from it. This has large consequences for the input available to decision-makers.

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Why Don't Firms Lobby?*

How information shapes the market for lobbying services

Benjamin CK Egerod[†] Lasse Aaskoven[‡]

October 1, 2024

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Abstract

Why do some firms lobby consistently while others never lobby at all? We argue that most firms lack information about the market for lobbying services. This implies that even when there are large returns associated with a political presence, most firms will not lobby. We present comprehensive evidence from US publicly traded firms supporting this argument. First, using a natural experiment, we show that only firms that already have a political presence use lobbying to deal with political shocks. Second, firms start lobbying when they receive an influx of information about political strategy among other firms. Third, the information effect is present among firms that can rely on trade associations for lobbying needs. Our results suggest that lack of information keeps firms from lobbying that would benefit from it. This has large consequences for the input available to decision-makers.

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1 Introduction

A rich literature documents that individual corporations can wield significant influence over policy outcomes through their lobbying activities (e.g., Huneus and Kim, 2018; Richter, Samphantharak, and Timmons, 2009; Libgober, 2020). However, the overwhelming majority of US firms never lobby. This is clear from Figure 1, which shows the proportion of publicly traded US firms that have filed at least one report under the Lobbying Disclosure Act (LDA) in the period 2000-2019. This will surprise many, because a large amount of research on firm lobbying focuses on the dominant role of business and the potential benefits that can be reaped through lobbying. Why do so few firms lobby?

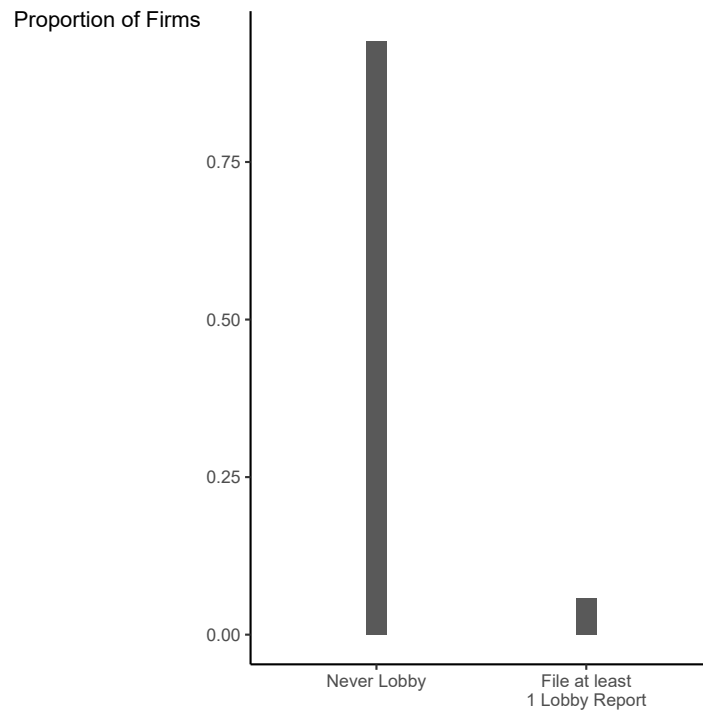


Figure 1: Most Firms Never Lobby. *Note:* The figure shows the proportion of firms that a) file at least one LDA report and b) that never file any report in the period 2000-2020. Data source: Kim (2018).

In this article, we argue that lack of information discourages entry onto the market for lobbying services. The market for lobbying services is poorly understood by outsiders, the returns to lobbying are shrouded with uncertainty, and eliminating them by gathering information is costly. This implies that even in situations where lobbying would produce large financial returns, most firms would not engage in it.

We derive three observable implications from this argument and test them comprehensively using a variety of data and research designs. First, we show that US firms rely heavily on default behavior when dealing with their political environment by exploiting a natural experiment, where a pilot program by the Securities and Exchange Commission (SEC), which randomly exposed firms to changes in their political environment.

Second, we show that firms do not lobby, because they lack information about lobbying. To get at this, we use a set of difference-in-differences designs showing that when the firm appoints a director, who has previously served on boards of firms that lobby, the firm becomes more likely to establish a lobbying presence. When firms receive information about the lobbying in other corporations, they start lobbying, too.

Third, since the information needed to assess a corporate lobbying campaign is highly firm-specific, the effects should be present among firms even if they can rely on trade associations for support in their lobbying needs. We use several measures of whether the firm should be able to rely on a trade association and show that the effect of ‘lobbying director’ arrival persists even among these types of firms.

Lack of information about lobbying has consequences for the individual firm, because lobbying provides competitive advantages (Huneus and Kim, 2018). Additionally, because lobbying is a key source of information for legislators (Ban, Park, and You, 2022; LaPira and Thomas, 2017), and bureaucrats (You, 2017; Lee and You, forthcoming), the frictions on the market for lobbying services constrain the set of actors that provides input for the political system. This biases the information available to decision-makers. Ultimately, this could skew policy outcomes towards the preferences of the select few firms that mobilize politically (McKay, 2022). We return to this point in the discussion, adding further data on the unequal distribution of lobbying expenditure.

By shedding light on this important concern, our results have implications for our understanding of the role of firms in politics, and the democratic consequences of corporate lobbying. By delving into the underlying reasons for the significant disparities in political engagement among firms, we address a critical gap in our understanding: why the majority of firms abstain from lobbying efforts. Additionally, in advancing one possible answer to

this question, our paper is closely related to the research examining why there is so little money in politics (Ansolabehere, De Figueiredo, and Snyder, 2003; Tullock, 1989; Gordon, Hafer, and Landa, 2007). According to our results, lack of information might play a vital role in explaining this puzzle.

2 Why Firms Lobby

Lobbyism is one of the most salient channels through which the interests of groups in society are transmitted into the political system (Grossman and Helpman, 2001). However, for the interest group system to be representative, free entry into the lobbying market is necessary. While we know that the representation of business groups is biased towards a select few (Hansen and Mitchell, 2000), we know little about why that is. This is because the literature on corporate lobbying has focused overwhelmingly on why firms choose to lobby on their own (for a review see Oliver and Holzinger, 2008), but does not provide an explanation for why most firms never lobby. Understanding what keeps firms from building a lobbying presence is key to assessing how and why the input decision-makers receive from the business world is biased.

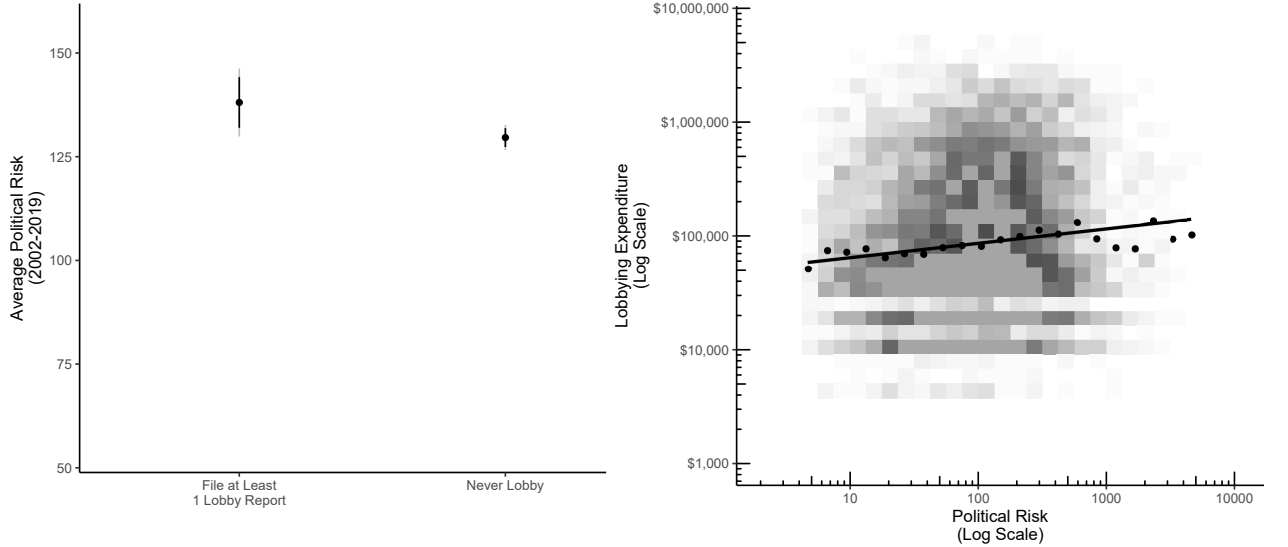
While the literature has established numerous reasons why firms lobby (Brasher and Lowery, 2006; Munger, 1988; Hansen, Mitchell, and Drope, 2005; Drope and Hansen, 2006; Kim, 2008), exposure to political risk *may* be the most well-established single cause (Hansen and Mitchell, 2000; LaPira and Thomas, 2017; Liu, 2020; Ban, Palmer, and Schneer, 2019). However, this crucial factor cannot be viewed in isolation from the issues with information we highlight. Therefore, in the following, we will first outline the canonical argument for why risk exposure should lead firms to start lobbying. We will then juxtapose this with our argument that lacking information about the market for lobbying will keep firms from engaging in it – even when faced with a costly shock to political risk. Our argument will have three focal points: We will outline *a)* how and why firm managers lack information about lobbying, *b)* how directors may transfuse information about how to estimate this into the firm, and *c)* how these estimates will be so firm-specific that trade associations will have a difficult time supplying them.

2.1 Political Risk Exposure and Corporate Lobbying

A highly persuasive account of lobbying proposes that firms engage in politics to resolve risks arising from their exposure to government policies (LaPira and Thomas, 2017; Ban et al., 2019; Liu, 2020). Through lobbying, firms extract information allowing them to resolve political uncertainties. Political risk exposure can come about due to changes in business regulation, enforcement of rules, and other types of government intervention. Importantly, the cost of political risk exposure often arises because the firm does not know the state of the future regulatory environment—not because politicians impose policies that are unfriendly to business (Canes-Wrone and Park, 2012). The uncertainty about policy causes significant frictions, forcing firms to postpone important decisions (Gulen and Ion, 2015; Canes-Wrone and Park, 2012), ultimately harming the firms’ financial returns (Iqbal, Gan, and Nadeem, 2020).

However, there is growing evidence that firms can use political connections to mitigate the negative effect of political risk exposure (Acemoglu, Johnson, Kermani, Kwak, and Mitton, 2016). It is typically hypothesized that firms can use lobbying to mitigate the negative effects of increased political uncertainty. Hence, the argument in the literature viewing lobbying as a form of insurance against political risk (LaPira and Thomas, 2017; Liu, 2020) is that firms mobilize politically to extract information from the political system, which allows them to reduce uncertainty (Christensen, Jin, Lee, Sridharan, and Wellman, 2023).

This argument is highly persuasive. Therefore, it will come as a surprise to most that firms do not tend to engage much more in lobbying when their managers worry more about political risks. In Figure 2, we use the Hassan, Hollander, Van Lent, and Tahoun (2019) measure of managers’ assessment of their firms’ political risk exposure. We will provide more detail on the measure in Section 4.2. For now, it is worth noting that while managerial assessment of political risk exposure *is* correlated with lobbying in a statistically significant way, the association is very weak. A tenfold increase in a firm’s political risk is only associated with an increase in lobbying expenditures of approximately five thousand dollars. While this measure is imperfect, it shows something



(a) Firm Lobbying and Political Risk Levels (b) Political Risk and Lobbying Expenditure

Figure 2: Political Risk Weakly Predicts Corporate Lobbying. *Note:* The figure shows the relation between political risk and lobbying activity among publicly held firms. Firms that lobby and firms that don’t are similarly exposed to political risk. Political risk measured with data from Hassan et al. (2019), lobbying measured with data from Kim (2018). Further data details in section 3. Firms that never lobby are excluded in panel b for aesthetic purposes.

highly important: firms whose managers tend to worry more about the effect of political risks on their earnings do not tend to lobby at far higher rates. In other words, 90% of firms never choose to engage with politics no matter their political environment—even when faced with costly political threats.

This presents us with an important and overlooked puzzle in the literature on corporate lobbying: Why do most corporations never lobby despite changes in their political environment that affect their bottom line? We argue that this is because of lacking information on the market for lobbying services.

2.2 Why Firms Don’t Lobby

Based on the literature on political risk, it is puzzling that more firms do not lobby – particularly, because other research documents important financial returns to lobbying (Huneus and Kim, 2018). In the following, we will argue that firms are not aware of the exact returns to lobbying. The deterrent effect of this is compounded by large fixed costs associated with building a political presence. The combination keeps firms out of

lobbying.

2.2.1 Lacking Information and Fixed Costs of Lobbying

Our main argument is that most firms do not know much about lobbying, and that this lack of information keeps them from developing a political presence. We can distinguish between *habits* and *conscious investment decisions* as reasons why firms do not lobby when they lack information.

The conscious decision not to lobby happens, because policy is unpredictable, and returns to lobbying are highly uncertain (Drutman, 2015). On the other hand, building a lobbying presence is costly (Bombardini, 2008; Kerr, Lincoln, and Mishra, 2014; Drutman, 2015). Based on standard corporate finance, we know that firms will require higher returns to make investments in assets whose benefits are associated with uncertainty (Dixit and Pindyck, 1994). The same goes for lobbying – managers may believe that there are benefits, but if they do not know the exact magnitude, they will hold off investing until more information can be obtained. While the uncertainty inherent in lobbying returns can be eliminated, doing so requires effort. As Drutman (2015) argues, corporations have to invest in political skills before lobbying makes sense. They have to build the ability to define their political goals in more concrete terms than, e.g., a lower regulatory burden. Otherwise, estimating potential returns is not possible. Therefore, while information about potential returns to lobbying is obtainable, doing so requires a serious investment. It is the combination of limited knowledge of what the returns might be, difficulty in estimating them, and noticeable costs of building a lobbying presence that keep firms from investing in lobbying.

On the other hand, in many firms strategic decisions are based on ‘habits’ – firm behavior is largely explainable by how they have done business previously, and changes in strategy are commonly incremental extensions of the firm’s previous strategy (Quinn, 1981). Even when excessive amounts of data are available, managers often do not use it (Pittenger, Glassman, Mumbower, Merritt, and Bollenback, 2023). When it comes to lobbying, firm managers tend to focus on their business and do not think of it as a

possibility (Drutman, 2015). In such a situation, information about lobbying and the potential returns might move firm managers away from their previous habits. However, given that they will not be searching for it, they are unlikely to obtain it on their own, and they will stick to their habit of not lobbying – even when faced with costly political exposure.

In sum, the lack of information on the market either implies that firm managers do not *think* about lobbying as a possibility, but rely on habits. Alternatively, lacking information makes it difficult for firm managers to trade off costs and benefits of developing a political strategy, and they consciously choose not to lobby due to this uncertainty. In the former case, they do not think about lobbying at all but concentrate on their core business. In the latter case, they have thought about lobbying, but have chosen not to lobby as a conscious investment decision. Our argument is agnostic about which of these mechanisms drive the effect (in reality, it is likely to be a mix of them), and both imply that even if external factors like political (risk) exposure harm the company, they do not know whether lobbying to reduce such costs would be worthwhile. Based on this, we can moderate the prediction about the impact of political risk on firm lobbying and revenue: Firms should only react to political risk by increasing lobbying if they already have built a political presence. For that type of firm, lobbying becomes a default solution to deal with political risk factors. For those with no pre-existing political engagement, lobbying does not enter as a solution to political problems, because they do not know the returns.

2.2.2 Collecting Information on the Market for Lobbying Services

How do firms gather the information about the market for lobbying services? One way is to learn from established practices of other corporations. Management research suggests that one important way firms learn from the arrival of a new member of the board of directors, which transfuses information from other firms into the organization (McDonald, Westphal, and Graebner, 2008; Strang and Soule, 1998). This allows the corporation to learn about successful and unsuccessful strategies elsewhere (Zhu and Chen, 2015). This is particularly useful for firms that have little knowledge about the context they are

supposed to make decisions about (Xia, Ma, Tong, and Li, 2018).

In particular, the board member—who has served as a director of a firm that lobbies—provides information about the standard practices in other firms, how the market for lobbying services functions, and how other companies have evaluated the effectiveness of lobbying. In this way, the arrival of a new lobbying-experienced director transfuses information about lobbying into the corporation. If the decision not to lobby is based on habits, and the firm managers simply do not think of lobbying as a possibility, the new director might make the company aware of the lobbying market, and which opportunities it might present. If the decision is based on firm managers consciously trading off costs and expected benefits of lobbying, while discounting potential returns based on uncertainty (Dixit and Pindyck, 1994), the new director might help the company reduce the uncertainty surrounding the returns. Insofar as this new information suggests that lobbying would be useful for the company, it will make it more likely that the firm will establish a political presence, where they previously had none.

2.2.3 Trade Associations as Information and Lobbying Providers

A key insight from extant research on lobbying is that meticulous attention to very particular policy details characterize lobbying efforts – particularly for corporations (Godwin, Ainsworth, and Godwin, 2012; You, 2017; Kim, 2017).

Therefore, firms’ lobbying endeavors often focus on shaping specific provisions that directly benefit individual firms, while excluding competitors from said benefits, making policy influence a private good (Gordon and Hafer, 2007). For instance, lobbying efforts may target the allocation of subsidies, eligibility criteria for tax breaks, or the precise metrics used to measure regulatory compliance (Godwin et al., 2012; Richter et al., 2009; Egerod and Justesen, 2021). Such granular details hold immense significance for firms, influencing their operational strategies, financial performance, and competitive standing within the market. Therefore, firms often hold vastly different policy preferences, even within the same industry (Kim, 2017).

This has implications for the extent to which trade associations can lobby on behalf

of firms, and advise them in their use of lobbying by themselves. While trade associations are able to do many things better than the firm, they may not know better than the firm which returns a lobbying presence would generate, because they lack the necessary insight into the firm's operations, which are needed to provide such estimates. Additionally, trade associations are often not even able to advise or represent a firm on a policy issue, because its competitors have diverging preferences on the topic. This implies that firms will often be left to fend for themselves, when there are policy disagreements within the trade association.

Therefore, we expect that the effect of obtaining information about lobbying returns through the arrival of a new director will still be present among firms who may rely on trade associations.

3 Empirical Implications and Strategy

Our argument implies at least three observable implications. First, when faced with immediately increased political risk, a firm with current lobbying activity will increase its lobbying activity, whereas a non-lobbying firm facing the same increase in political risk will not immediately start lobbying due to lack of information about the lobbying market. This happens, because there is uncertainty about whether a lobbying presence would help decrease the risk from the political shock.

Second, when a firm acquires information about the returns associated with lobbying, this will push the previously non-lobbying firm into lobbying activity. This second implication is the most important one for our argument, as it most directly tests the impact of new information.

Third, even among firms that can receive lobbying advice from their trade association there will be an effect of information about lobbying returns.

Testing each implication requires drawing on a variety data sources and using a number of different designs. Because we use multiple data sources and research designs, we provide comprehensive details before we present each set of results. This is to ensure that the reader has the data and design in fresh memory. In this section, for clarity, we will

briefly summarize them. Table 1 presents an overview of how we measure firm lobbying (the outcome variables) as well as how we will capture the three observable implications of lacking information.

Table 1: Research Designs and Data Sources

Concept	Measure & Design	Source	Sample
<i>Measuring Outcome variables</i>			
Lobbying expenditure	Intensity of political activity	LDA filings through LobbyView	Compustat firms 1998-2019
Any lobbying?	Commencement of political activity	LDA filings through LobbyView	Compustat firms 1998-2019
<i>Treatment variation</i>			
Implication 1:			
Shock to political risk has no effect on lobbying among firms that never lobbied, but increases lobbying among firms with previous lobbying activity	Difference in lobbying among firms randomly chosen for the SEC experiment compared to firms not chosen. Subset analyses among firms that have and have not lobbied.	Litvak, Black, and Yoo (2016)	Russell 3000 firms in 2004.
Implication 2:			
Firms start lobbying, when director arrives from firm that lobbies	Staggered difference-in-differences design comparing changes in lobbying between firms that experience the arrival of a ‘lobbying director’ and firms that do not.	BoardEx and LobbyView	Compustat firms that have never previously lobbied 2008-2019.
Implication 3:			
The effect may be smaller but still present among firms that can draw on trade associations for advice.	Difference-in-differences analysis from implication 2 subset to firms connected to trade associations	BoardEx, LobbyView, Encyclopedia of Associations	Compustat firms that have never previously lobbied 2008-2019.

Note: More variables are used throughout the analysis. They will be introduced, along with more design detail, in the relevant sections.

We construct our main sample based on the US firms in **Compustat**, which tracks

all publicly traded corporations and supplies financial data on them. We use firm lobbying as our outcome variable in all models. We collect data on corporate lobbying from the LobbyView database maintained by Kim (2018). The database collects the original lobbying filings made available under the Lobbying Disclosure Act (LDA) and contains the unique **Compustat** firm identifier (the **gvkey**) that allows us to easily match lobbying behavior to firm-level risk and financial characteristics.¹ We test the first implication – that firms should only increase lobbying in the face of political risk if they already lobby – using lobbying expenditure, as this captures the intensive margin. Following Kroeger and Silfa (2023), our identification strategy relies on a pilot project by the SEC that randomly removed the so-called uptick rule for a subset of public corporations among the Russell 3000, which instills random variation in firm-level political risk exposure.

The second implication is that firms should start lobbying once they obtain information about the returns to lobbying. We hypothesize that one way this can happen is when they hire a director from a firm that does lobby. We rely on BoardEx to capture the arrival of new board members in the firm, and use LobbyView to measure whether the other firm in question lobbies. Because our expectation is that the arrival of this new director will make firms build a lobbying presence for the first time, we examine only the subset of firms that have never previously filed an LDA report. This also implies that firms will drop out of the sample once they start lobbying and are observed only up to and including the year where they filed their first LDA report. Our outcome variable will be a binary indicator of the filing of the firm’s first LDA report. The third implication is closely related to this, as it suggests that there should be an effect even when the firm can rely on the association for representation and advise in relation to lobbying. To investigate this, we measure the firm’s relation to the trade association using data from BoardEx and from the Encyclopedia of Associations.

¹Because expenditure is calculated differently depending on whether the lobbyist works on contract or is in an in-house capacity, we exclude in-house lobbyists when examining lobbying expenditure. Our results hold when using a binary indicator for lobbying instead of expenditure, where we can include both in-house and contract lobbying.

3.1 Stylized Facts on the Stickiness of Lobbying

An interesting byproduct of our argument is that once a corporation has paid the fixed costs necessary to enter the market for lobbying services, they should use lobbying as a default strategy for dealing with the political realm. This suggests that lobbying activities should be highly path dependent or ‘sticky’. In this section, we present a number of descriptives that document this pattern.

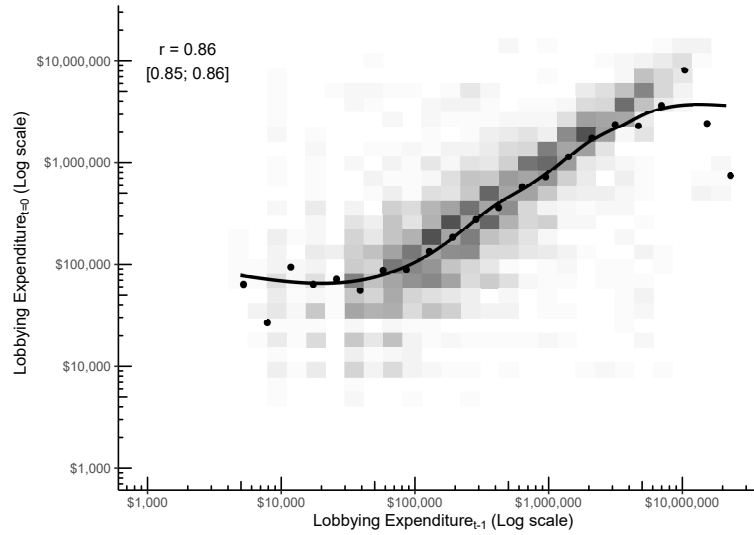
In Figure 3, we present some stylized facts on the stickiness of lobbying (see also Huneus and Kim (2018) for additional facts). Panel A visualizes the correlation between contemporaneous lobbying expenditure and expenditure in the previous year. This reproduces a finding in Huneus and Kim (2018).

Panel B shows how expenditure evolves over time after the initial filing, and Panel C shows the proportion of firms that keep filing each year after their initial filing.²

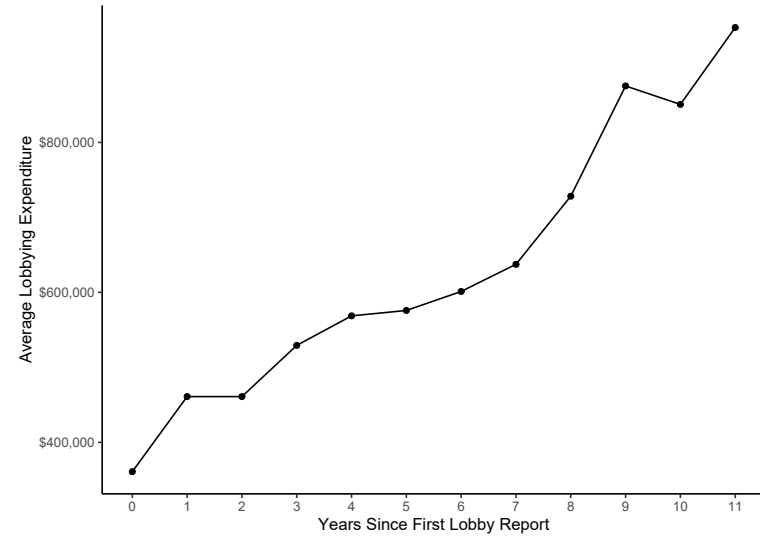
The patterns all testify to the stickiness of corporate lobbying. Once a firm files a lobbying report, it tends not only to keep doing so—it also tends to strongly intensify its lobbying activity.

Table 2 describes the firms in terms of financial characteristics. We split the sample into firm-years where there were no prior history of lobbying (Panel A), and where there were some (Panel B). Two important patterns emerge. First, firms that engage politically are on average many times larger (in terms of assets), more liquid, more profitable, and have a higher revenue. Second, for firms with no history of lobbying, filing the first lobbying report is a very rare occurrence with a probability of only 0.3%.

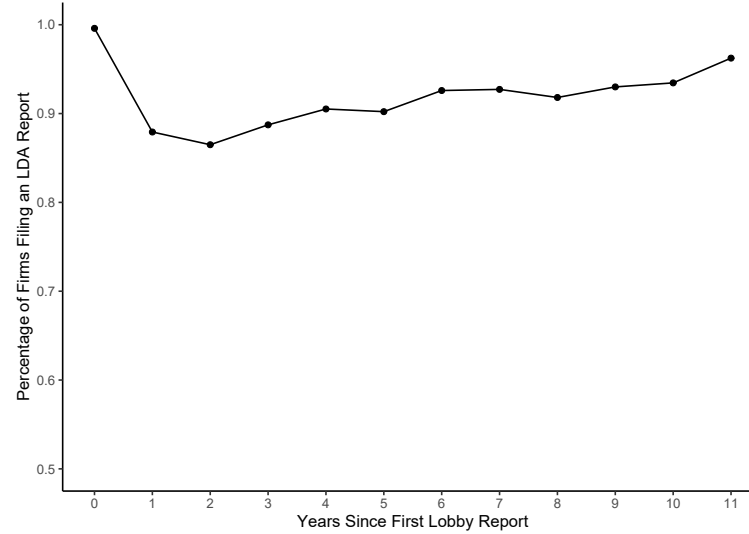
²Note that since some firms start lobbying closer to the end of the panel, the proportion reverts to 100% as we approach year 12 after lobbying commences. This is because only the firms that lobby for the entire period remain at that point.



(a) Lobby Expenditure is Path Dependent



(b) Lobby Expenditure Trends Upward After Initial Filing



(c) Proportion of Firms Filing after Initial Year with Lobbying

Figure 3: Once You Start Lobbying, You Never Go Back. *Note:* Panel A shows the correlation between lagged and contemporaneous lobbying expenditure, reproducing a finding in Huneus and Kim (2018). Pearson's r and a 95% confidence interval printed in top left. Panel B shows the yearly average lobbying expenditure over time after the firm files its first LDA report. Panel C shows the proportion of firms that keep filing lobbying reports over time since their initial filing.

Table 2: Descriptive Statistics by Prior Lobbying Behavior

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Panel A: Firms with no Previous Lobby Activity							
Total Assets	203,939	9,093.679	83,493.520	0.000	28.514	1,468.778	3,771,200.000
Cash Holdings	200,329	391.835	4,942.862	−32.000	1.547	68.000	574,044.400
Gross Profits	199,336	809.688	4,130.041	−45,026.000	2.327	245.648	137,808.000
Revenue	199,373	2,470.449	12,801.460	−15,009.330	10.226	751.476	521,426.000
Proportion w/ LDA Report	247,400	0.003	0.055	0	0	0	1
Lobbying Expenditure	247,400	1,085.402	58,525.420	0	0	0	10,323,120
Panel B: Firms with Some Previous Lobby Activity							
Total Assets	4,794	24,497.890	135,569.700	0.002	317.813	9,337.264	3,083,139.000
Cash Holdings	4,718	1,454.707	10,555.440	0.000	26.210	600.583	309,314.900
Gross Profits	4,703	2,245.641	6,456.154	−4,141.334	67.761	1,659.604	101,612.000
Revenue	4,703	6,481.634	18,015.610	−272.867	197.935	4,875.574	285,873.800
Proportion w/ LDA Report	4,808	0.661	0.473	0	0	1	1
Lobbying Expenditure	4,808	608,367.900	1,561,282.000	0	0	404,834	21,292,500

Note: Data is a firm-year panel. Firm financial characteristics for firm-years where the firm had filed no prior LDA reports are reported in Pane A. Panel B shows firm-years with some prior lobbying activity.

4 Implication 1: Political Exposure and Corporate Lobbying

In this section, we will present evidence on the first observable implication. We expect a highly conditional effect of political exposure on the corporation’s lobbying behavior—those with a history of lobbying should react strongly, while those without one should not react at all.

4.1 Randomized Rule-Change as a Source of Political Risk Exposure

To examine the first observable implication, we leverage a federal-level randomized natural experiment to confirm the causal effect of federal government intervention on corporate lobbying.

On July 28, 2004, the Securities and Exchange Commission (SEC) announced the only randomized trial in the agency’s history. Since the 1930s, short selling³ had been regulated through price restrictions, which constrained short sales to always be conducted at a higher price than what the security was previously traded for. This was known as the ‘uptick rule’. From 2005 through 2007, the SEC selected one-third of the Russell 3000 and suspended specified securities sold by those firms from the short selling restrictions (see Litvak et al. (2016); Kroeger and Silfa (2023) for further information). Importantly, this pilot experiment allows us to capture firms that were randomly exposed to a political intervention—a risk inducing change in their political environment. This is because policymakers announced a change in the regulatory environment of the firms, and delivered little prior information about what exactly the new regulatory context would entail. Suspending short selling for specified securities is no simple task. It requires, among many other things, selecting securities that are exempt from the uptick rule, how brokers should go about marking sales as long, short or short exempt, and how to treat sales that are

³Investors short sell when they borrow a security and sell it. This often happens with an expectation that they can buy back the security later at a lower price.

registered before the pilot commences, but remain open during the pilot period. These, and other questions, were initially left unanswered (Litvak et al., 2016), and were sources of uncertainty that the firm would have to address, potentially through lobbying. By examining the period *after* the announcement and *before* the actual rule-suspension, we ensure that we are isolating the effect of an announced change in the political environment, and not any effect the actual regulation might have on the firm. In this period, there was no actual policy change, but because policymakers announced that they would intervene upon the treated firms, this should induce them to lobby to extract information about the regulatory change in order to mitigate any short-term uncertainty induced by the experiment. The SEC’s experiment was previously used in Kroeger and Silfa (2023) to show that a change in the content of regulation increases firm lobbying on average. While their results bolster our argument that this captures an exogenous increase in the regulatory environment, our errand is different, as we are interested in the heterogeneous effect of the shock depending on prior lobbying experience. A final reason why the SEC pilot is extremely helpful is that the randomization was conducted in a transparent way that allows us to ensure that treatment was not manipulated by the firms: Securities were ranked by average dollar volume that was traded, and the agency then selected every third for treatment. We support this with by testing pre-treatment balance on covariates in Appendix B.1.

We obtain data on the experiment from Litvak et al. (2016). Importantly, the SEC unintentionally ruined their own randomization scheme by partially lifting the uptick rule from the control group among firms in the Russell 1000, while the control group among smaller firms remained as clean controls (Black, Desai, Litvak, Yoo, and Yu, 2020).⁴ To deal with this, we zoom in on the Russell 1000, where the randomization still holds (as recommended by Black et al. (2020)). An additional reason for this choice is that the removal of the uptick rule was highly unlikely to affect substantive short-selling among the largest firms, since they were typically also traded on regional stock exchanges and

⁴While Kroeger and Silfa (2023) do not account for this problem in the randomization scheme, our results are quite similar, although of a smaller magnitude.

electronic exchanges that did not limit short-selling. The combination of looking at firms that were most unlikely to be affected by the rule-change and in the period before the rule-change went into affect bolsters the claim that any increased lobbying activity was driven by the political intervention itself—but unrelated to any substantive effects the policy-change might have.⁵

Lobbying reports filed under the LDA before the Honest Leadership and Open Government Act (HLOGA) summarize lobbying expenditures at the semester-level. Therefore, we construct a panel dataset of firm-semester observations and track whether the firm was included in the SEC pilot as well as whether it lobbied. We examine the period 1998 through 2004 and consider the final semester of 2004 as the treated period. This is after the experiment was announced, but before it went into effect. We use the period from 1998 and until the treatment to establish a baseline of lobbying behavior. To examine whether the effect of the experiment differs depending on prior lobbying behavior, we estimate a regression of the following form:

$$Lobby_{is} = \delta_1 RegSHO_{is} + \delta_2 LobbyHistory_{is} + f(RegSHO_{is} \cdot LobbyHistory_{is}) + \epsilon_{is} \quad (1)$$

Here, $Lobby_{is}$ denotes the logged semesterly level lobbying expenditure firm i . $RegSHO$ as a binary indicator denoting the group of pilot firms treated in the SEC’s experiment during the period after the announcement, and $LobbyHistory$ captures the proportion of semesters in the pre-treatment period where the firm filed at least one lobby report. Importantly, since the interaction between $LobbyHistory$ and $RegSHO$ is non-linear, we use the Hainmueller, Mummolo, and Xu (2019) binning estimator to estimate $f(\cdot)$, the functional form of the interaction between the two variables. This allows us to zoom in on the firms that have no history of lobbying and compare them to firms with varying degrees of lobbying experience. Due to missingness, we track 996 firms at the semesterly level from 1998 through 2004, producing 13,974 firm-semester observations.

⁵Litvak et al. (2016), indeed, find no increase in share price nor short-sale trading volume, indicating little to no substantive effect on the firm’s market conditions.

In Appendix B.2, we run a number of robustness checks. First, we analyze the data with a difference-in-differences estimator. Second, we estimate the effect among small firms. Both analyses yield results that are very similar to the ones presented in the main text. This shows that our results are not driven by focusing on large firms or our model specification.

4.1.1 Randomized Rule-Change Conditionally Increases Lobbying

We now present estimates of how firm-level political risk exposure – in the form of the SEC experiment – affects corporate lobbying. In Appendix B.1 we study covariate balance, and show that being selected for the treatment group in the SEC’s experiment is uncorrelated with previous lobbying behavior as well as a number of measures of firm size and financial performance. This suggests that the randomization scheme was successful in eliminating average differences between firms.

Next, we estimate the effect of the rule change, comparing changes in lobbying behavior in the final semester of 2004 to pre-treatment semesters among treatment and control firms. We allow effects to vary depending on prior tendency to engage in lobbying, using the Hainmueller et al. (2019) binning estimator.

We estimate a very precise null effect for firms that have never lobbied before. The point estimate is almost exactly zero, and the confidence interval is extremely narrow, ruling out any substantial effect. The average effect on lobbying, documented by Kroeger and Silfa (2023), are thus fully concentrated with firms that have tended to lobby before the pilot was announced. That is, firms with lobbying activity used that to react to the shock, while firms without such a presence used different means.

We cannot investigate the persistence of these effects, because looking into the more distant future, the rule-change would come into force. However, this suggests that there is a causal effect of risk-inducing changes in the firm’s political environment on corporate lobbying, and that this causal effect is concentrated among firms that have a history of lobbying.

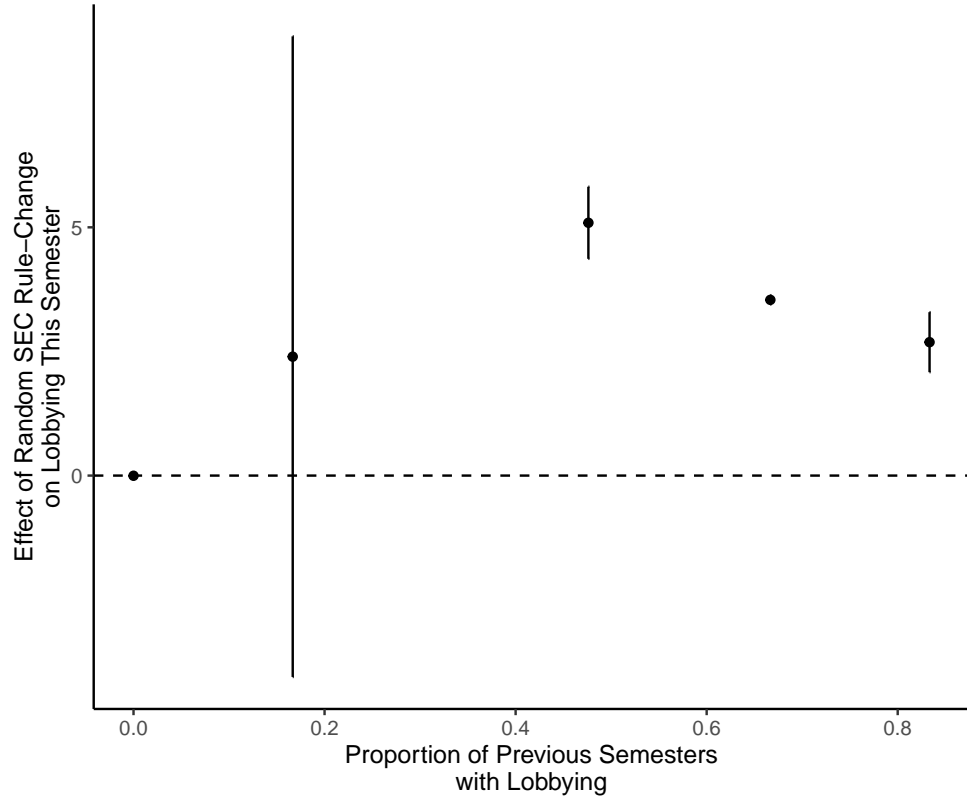


Figure 4: How the Effect of the SEC Experiment Varies with Prior Lobbying.

Note: The figure shows the impact of the SEC's announcement for different levels of prior lobbying activity. Estimates produced using the Hainmueller et al. (2019) binning estimator. Robust confidence intervals are 95% with firm-level clustering. Bins are chosen to reflect the 25th, 50th and 75th percentile among firms that do lobby. A fifth bin is added for firms with no prior lobbying activity.

4.2 External Validity

The SEC experiment provides causal estimates. However, it represents only a single instance of political risk exposure, and we might worry that the results do not generalize beyond it. Additionally, since it takes time to build a lobbying presence, it could be the case that the shock does not last for a period that is long enough for it to make sense for firms to start lobbying.

Consequently, in Appendix C, we draw on Hassan et al. (2019) who construct and validate a text-based measure of firm managers' assessment of generalized political risk. They leverage the fact that most publicly listed corporations hold quarterly earnings conference calls, where management shares its view on firm performance. Because this is an imperfect measure, we use this as a supplemental analysis. However, the weaknesses are different from those in the SEC experiment, which is randomized, and where the target of measurement is very clear. Hence, if both measures produce similar results, this would provide strong support for our prediction.

We find that the results from the SEC experiment generalize in a number of important dimensions. We find extremely precise null effects of shocks to managers' risk perception for firms that never lobbied. This holds for shocks that persist for one, two and three years. In all cases, increases in managements' risk perception increase the firm's spending on lobbying among firms that already have a lobbying presence. Overall, these results suggest that our SEC results generalize across types of political risk and to more persistent exposure.

5 Implication 2 & 3: Information Moves Firms Into Lobbying

Our final two predictions are that firms will start lobbying once they obtain information about the returns to engaging politically, and that this should hold even when trade associations are present to assist in the lobbying endeavor. In particular, we expect that firms can obtain such information when they appoint a director who has served on the

board of a firm that does lobby.

5.1 Measuring Arrival of Lobbying Information

To test this, we draw on the BoardEx database, which contains comprehensive information about the population of directors serving on the boards of publicly traded corporations from 2000 through 2019. This allows us to identify which directors serve on the boards of specific firms, and when new directors arrive. We merge the database with LobbyView to construct a measure of which firms that lobby. We use this to capture new appointments of directors with a background of board service in firms that do lobby.

This measurement strategy captures two types of information that are important for our theoretical reasoning. First, the new director could arrive with information about the average returns to lobbying. Second, the new director has knowledge of what is standard non-market strategy in other firms. Thus, even if the new director does not know the exact returns associated with lobbying, she is likely to have the tools to estimate them, because lobbying was standard in the board she previously served on. It is important to note, of course, that this will capture a compound of the director's information and the strength of their preferences for lobbying. Future research should attempt to disaggregate the pure information effect from how hard the arriving director pushes to get the firm to engage in lobbying.

We construct a firm-year panel, which we subset to firms that have never previously lobbied, and estimate a set of difference-in-differences model. We only examine the period after HLOGA. To estimate the average treatment effect on the treated (ATT) of appointing a director with knowledge of lobbying on the firm's probability of lobbying, we use a staggered difference-in-differences approach. A regression with fixed effects for firm and year would be biased (Goodman-Bacon, 2021). Therefore, we use the estimator recently proposed by Callaway and Sant'Anna (2020). While we do not estimate two-way fixed effects models, examining the well-known regression equation may provide some clarity regarding our approach:

$$Lobby_{iy} = \beta DirectorArrival_{iy} + \omega_i + \sigma_y + \epsilon_{iy} \quad (2)$$

Lobby captures whether firm i files its first LDA report in year y . *DirectorArrival* is a binary indicator of the arrival of a new director with a background in firms that lobby. ω and σ are firm and year fixed effects, respectively. The technique we use extracts each firm that is treated by the arrival of a director with lobbying knowledge, and constructs a never-treated control group of firms (a 'clean' control group). For each of the treatment cohorts, the difference-in-differences is then estimated. This is similar to estimating eq. 2 separately for each treatment cohort with a clean control group. All the separate estimates are averaged to produce an overall ATT.

Overall, 3,903 firms are eventually treated with the arrival of a director from a firm that does not lobby, while 13,816 firms are never treated. Figure 5 shows how the arrival of these lobbying directors is timed for a random sample of 500 firms.

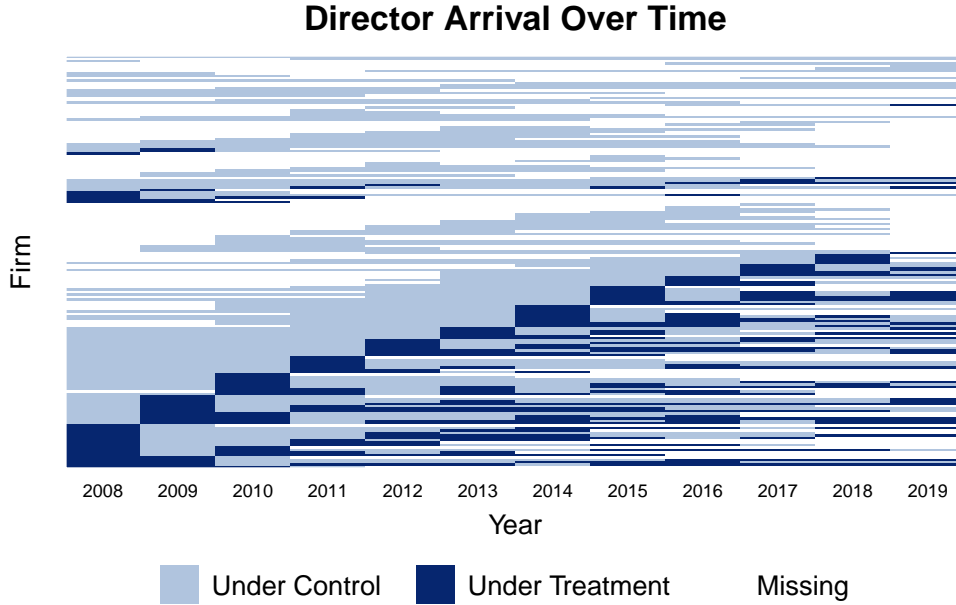


Figure 5: Director Arrival in Firms over the Sampling Period. *Note:* The figure shows the timing of the arrival of a director with experience from a firm that has lobbied for a random sample of 500 firms.

5.2 Implication 2: Appointment of Directors with Lobby Experience Increases Lobbying

In Figure 6, we examine whether firms start lobbying once they appoint a director with knowledge about lobbying. We then study whether the probability that the firm files its first LDA report increases, when a director arrives from a firm that does lobby. In the figure, we present event study estimates from the Callaway and Sant’Anna (2020) estimator for difference-in-differences with staggered treatment timing. We show how the probability of filing the firm’s first LDA report changes in the years leading up to and after the arrival of the new director. As we can see, the probability that the firm starts lobbying for the first time in its history increases dramatically and discontinuously when the director arrives, and it remains elevated for several years after the arrival. Before the new director’s arrival, the largest differences in probability are very small, approximately 0.001. Thus, the average probability that a firm files its first LDA report is thirty times the pre-treatment differential, which provides strong evidence against a pre-trend. Additionally, in Appendix D, we use the Rambachan and Roth (2019) estimator to show that the pre-treatment differences cannot explain our results. This analysis also shows that our results are highly robust to potential violations of the parallel trends assumption.

In Table 3, we present a number of robustness checks on this result, where we average the ATT over the full period after the new director arrives. In column 1, we present the model with no controls, corresponding to averaging the estimates in the post-treatment period from Figure 6. When firms that hire a director that has served on boards of firms that lobby, the probability of beginning the firm’s first lobbying campaign increases by two percentage points over the full post-treatment period. Importantly, since the baseline probability of filing the first LDA report is 0.3%, this is a very large impact. To account for the potential that large firms may experience differential trends compared to smaller firms, we control for the firm’s pre-treatment total assets (logged) in column 2. In column 3, we allow for differential trends by industry by matching on the firm’s NAICS industry

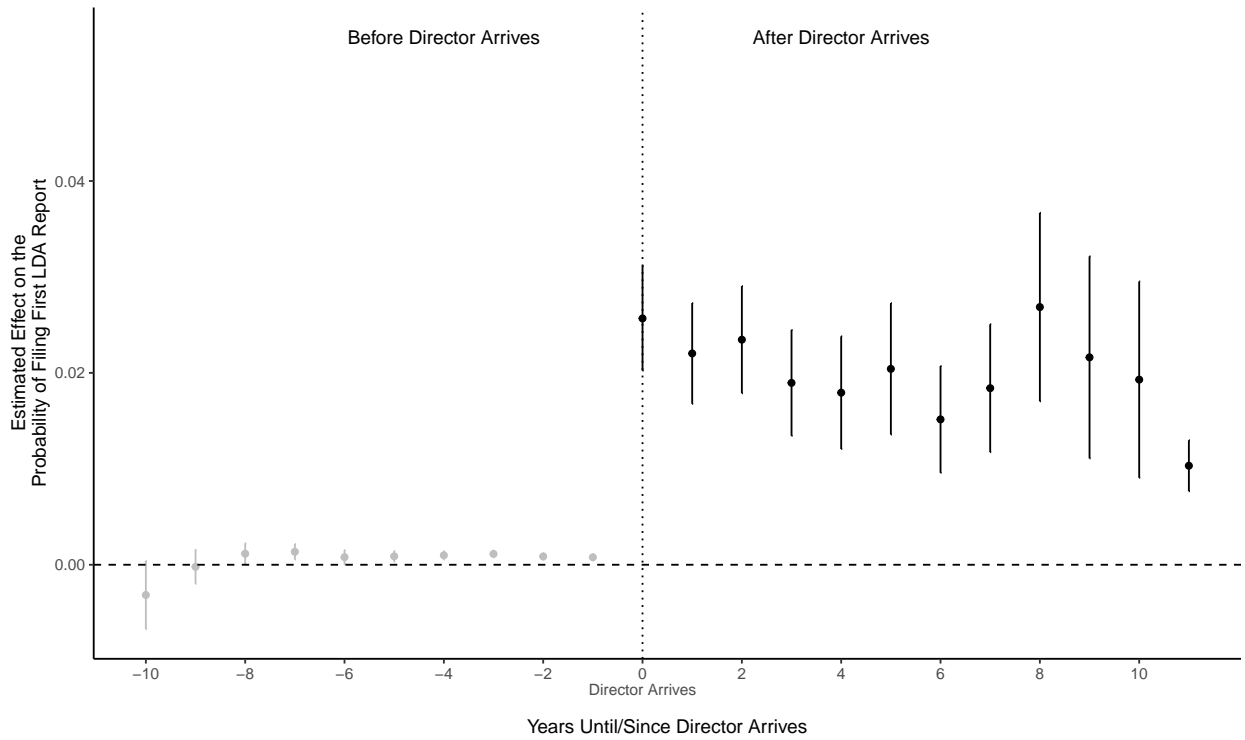


Figure 6: Directors with Information on Lobbying Move Firms Into Lobbying.

Note: The graph shows the impact of the appointment of a director that served on a board of a firm that does lobby on the likelihood that the new firm starts lobbying. The sample consists of firms with no previous lobbying activity, the dependent variable is the filing of the firm's first LDA report. Estimates for years relative to the arrival of the new director. Points are difference-in-differences estimates and lines are 95% confidence intervals from 1,000 bootstrap iterations with firm clustering. The Callaway and Sant'Anna (2020) estimator is used.

code (six digits) before the difference-in-differences is estimated.⁶ The results maintain.

One concern would be that this could be driven by organizational changes in general, and not the acquisition of new information. To deal with this, in column 4, we use firms that appoint a new director from a firm that *does not lobby* as a placebo control group. We use the Imai, Kim, and Wang (2018) technique⁷ as it allows us to easily construct an estimator comparing the two effect sizes. We do this by estimating the difference-in-differences of appointing directors with and without lobby experience within each bootstrap iteration. We then compute the difference between the two estimates within each iteration, and finally average this. The resulting estimate shows how much larger the effect of appointing a director from a firm that *does* lobby is compared to appointing a director from a firm that *does not* lobby. The estimate is smaller than the baseline estimate, but still significantly larger than the unconditional probability of lobbying. This suggests that firms generally increase their lobbying activity when they appoint any director, but that the effect is considerably larger when that director arrives from a firm that lobbies.

The major threat to identification is that firms appoint board members with lobbying experience, because they know that they are going to start lobbying. We follow two analytical strategies to make sure this does not drive our results. First, in column 5, we use data from BoardEx containing announcements made by the firm about individual directors. Among other situations, these announcement are made when directors pass away while they serve on the firm’s board. We use this to capture when new board members arrive to replace directors who pass away. We create a new treatment variable that takes the value 1, when a director with lobbying experience is appointed during the year a director passes away. This captures board seats that are filled, because a board

⁶We use the Sant’Anna and Zhao (2020) doubly robust estimator to control for logged assets, because it is highly robust to functional form misspecification. We use inverse probability weighting (IPW) to control for industry, because NAICS fixed effects were perfectly collinear.

⁷The Imai et al. (2018) estimator is very similar to Callaway and Sant’Anna (2020), and produces a similar estimated ATT of approximately 0.02.

member dies. These seats are unlikely to be filled, because the firm wants to start a lobbying presence, but happen for reasons unrelated to the firm’s strategy. We still find a highly significant and very large increase in the probability that a firm will file its first lobbying report. Importantly, even if the identifying assumption holds, it might not be random, which type of firm that chooses to appoint a director with lobbying experience. Faced with the shock of, say, a director passing away, a non-random set of firms may select into filling the opening on their board by appointing a director with lobby experience. In a recent paper, Borusyak and Hull (2020) show how the random nature of the original shock can be used to remove bias from the confounding effect of non-random exposure. In Appendix E we use their technique and show that our results cannot be explained by firms endogenously choosing lobbying directors when faced with an exogenous opening on their board.

The second way we reassure ourselves that our results are not driven by anticipation is by examining the durability of the effects in Figure 6. It is important to note a subtle feature of the way we design the sample: When a firm starts lobbying, it drops out. This ensures that we only analyze data from firms that have never previously lobbied. However, it also aids us in rejecting that our results can be driven by anticipation, because the effects are quite stable over time. If a firm hires a director in anticipation of commencing a lobbying campaign, we would expect that it would start engaging in lobbying shortly after the directors arrival. However, the effect remains stable many years into the future, which indicates that a significant subset of firms wait for a long period of time after hiring the new director.

Table 3: Appointing a Director from a Firm that Lobbies Increases Lobbying

	<i>Dependent variable:</i>				
	File First LDA Report				Director Death
	Staggered DD	Matched DD	Matched DD	Triple DD	
	(1)	(2)	(3)	(4)	(5)
Director w. Lobby Experience	0.021*** (0.001)	0.026*** (0.002)	0.023*** (0.002)		
Director w. Lobby vs. No Lobby Experience				0.004*** (0.001)	
Director Pass Away + Lobby Director					0.030*** (0.011)
Estimator	CSA	CSA	CSA	Imai et al	CSA
Treatment Events	3903	3754	3754	12020	61
Never Treated Firms	13816	9234	9234	12598	17658
Placebo Events				23826	
Assets Control?	No	Yes	No	No	No
NAICS 6-Digit Industry?	No	No	Yes	No	No

Note: Data is a firm-year panel consisting only of firms that have never filed an LDA report. The dependent variable is an indicator of whether the firm files its first LDA report. Standard errors in parentheses are bootstrapped with 1,000 replications using firm-level clustering. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively. CSA = Callaway and Sant'Anna (2020).

5.3 Implication 3: The Role of Trade Associations

Our argument implies that directors arriving with information on lobbying should change firm behavior, even if they can rely on trade associations for some of their lobbying needs. This is because the association *a)* does not have the necessary information about the firm's operations to assess returns to lobbying, and *b)* will often not be able to advise or represent the firm, because competitor firms have diverging interests.

Examining this implication empirically is difficult, because there exists no data on firm membership of associations. However, we use two strategies to examine the threat. First, we use BoardEx, which contains data on membership of association boards. Since association boards will be comprised of representatives of firms who are members of the association, this allows us to construct a sample of firms who we know are association members. The drawback is that we will not be able to know whether the firms in our sample are association members if they are not on any boards. Thus, this will represent a conservative sample that excludes many firms who are association members, but do not have board seats in the association. Second, we use the Encyclopedia of Associations (EA) to identify trade associations and which industries they serve. This is possible, because the EA maps trade associations to NAICS-6 codes. We then match trade associations to LDA disclosures and identify which of them are active in lobbying. We use this to create a measure of which NAICS-6 industries are served by trade associations that lobby. It is of note that only approximately 700 of the 2,200 trade associations are active in federal lobbying.

We use these two measures to conduct a number of different robustness checks, which are presented in Figure 7. Panel A shows the results from controlling for our two binary measures. Our results are robust to this. However, particularly for the measure of association board membership, controlling is likely to be a sub-optimal approach. Hence, in Panel B we subset to include only firms that are members of association boards, and in Panel C we subset to include only firms in industries where an association lobbies. The first row of each of these two panels presents a simple difference-in-differences estimate

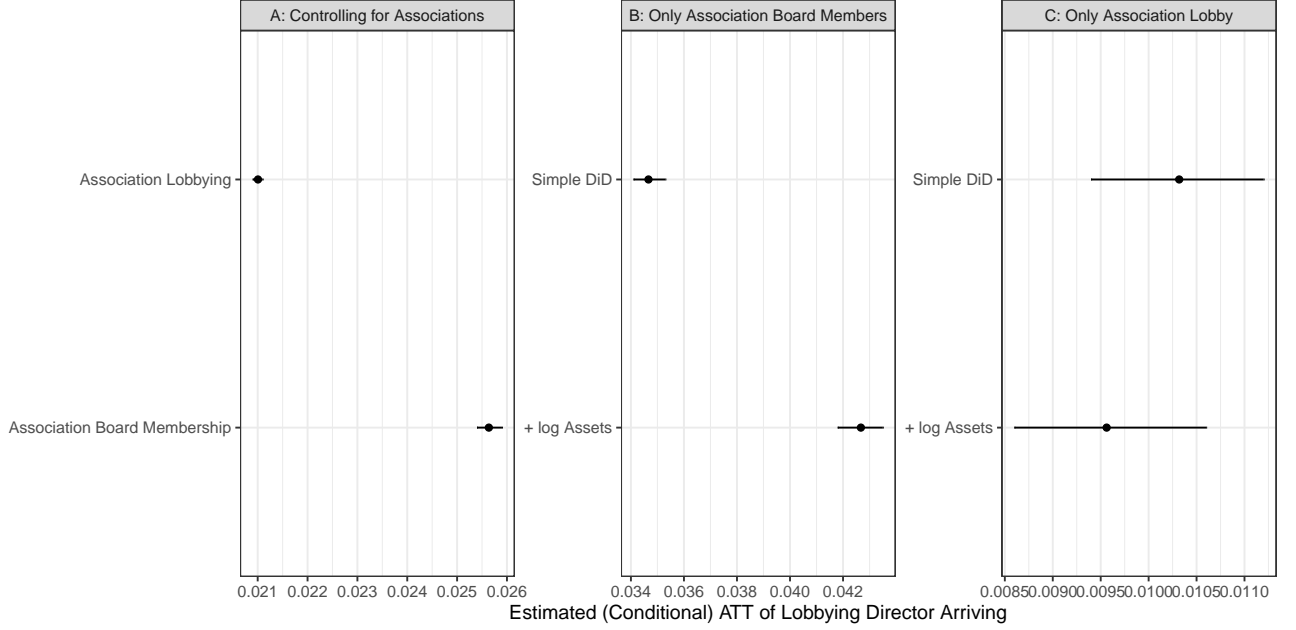


Figure 7: Associations Do Not Drive the Results. *Note:* Panel A shows the results after controlling for whether the firm is *a)* operating in a NAICS-6 industry where an association is active in lobbying, or *b)* member of an association board. The models in Panel B includes only firms that are members of an association’s board, while the models in Panel C includes only firms from NAICS-6 industries where an association lobbies. The second row in Panels B and C controls for log total assets. Solid lines are 90% bootstrapped confidence intervals with firm-level clustering.

without controls, whereas the second row controls for firm size.⁸ Across all these tests, our estimates remain large and statistically significant in line with our expectations.

5.4 Further Mechanisms: Habits or Choice?

We have previously discussed habits and conscious investment choice as two potential mechanisms for staying out of lobbying. Since our core argument is about information and how it shapes corporate lobbying behavior, our theory is agnostic to which of these mechanisms dominate. While, in reality, both are likely to be in play, it would still be valuable to present some evidence on these mechanisms.

We know that firms are acutely aware of the strategic decisions made by their competitors (Yang and Hyland, 2006), and lobbying is no exception to this (Godwin et al.,

⁸As we exclude a significant portion of our data, we cannot include industry fixed effects due to perfect collinearity.

2012; Egerod and Junk, 2022). Because firms tend to know the strategies pursued by their competitors, the decision not to imitate them is a conscious one (Yang and Hyland, 2006). This suggests that when a firm’s competitors have chosen to lobby, yet the firm continues to avoid lobbying as a strategy, its managers have likely determined that the risks of lobbying outweigh the potential benefits. This would imply that in industries where some firms are active in lobbying, it would be a conscious decision by the others not to lobby. Therefore, if lobbying is partly a strategic financial decision – not just a habit – we would expect larger effects of director arrival in industries where some firms lobby.

In Appendix F, we examine this and find that while there is an effect of director arrival in both types of industries, the effect is statistically significantly larger in industries, where some firms lobby.

It is important to note that this analysis is not aimed at disproving habits as a driver of lobbying – it has previously been shown that they play a key role in corporate non-market decisions (Drutman, 2015). However, our analysis here shows that habits are unlikely to explain the entire phenomenon, and that conscious investment decisions also play a role in driving firms decision about whether or not to lobby.

6 Discussion and Conclusion

In this paper, we have shed new light on an overlooked puzzle in the study of the political behavior of corporations: Why do some firms lobby consistently, while other firms never engage in political activity at all? We theorized that due to lack of information about the returns to lobbying, most firms simply do not think of it as an option. Instead, they rely on default behavior when exposed to risk-inducing changes in their political environment. However, when firms receive information about lobbying in other firms, they start building a political presence.

Analyses of lobby activities of US firms support these predictions. First, we document that firms that lobby and firms that never do so react in fundamentally different ways to marginal changes in their political environments. Firms that have a history of lobbying

increase their lobbying expenditure when faced with heightened political risk. However, firms with no such history *never* use lobbying as a response to changes in their political risk exposure. This holds both for periods of generalized political risk, and when they are exposed to randomized government intervention.

Second, we show that when a new director is appointed, and she has board experience from a firm that does engage in lobbying, the firm starts establishing a political presence. This shows that firms react to new information about lobbying. In particular, when they learn about the practices and experiences with lobbying elsewhere, they are more likely to start lobbying, too. This effect persists even when trade associations are present to assist which testifies to the firm-specific nature of the information, as predicted by our theory.

It is worth considering what this might imply for democratic government. In particular, since lobbying is the way firms deliver information to policymakers (Grossman and Helpman, 2001), the information available to ensure the quality of political decisions may be poor if few firms lobby. In Appendix G we delve into this by showing that firm lobbying is extremely concentrated both within and between industries: Few industries lobby, and even within the industries where lobbying happens, it is concentrated among an exceedingly small number of firms. Our findings imply that this inequality in corporate lobbying is, in part, due to firm managers having little knowledge about lobbying.

Do these results help us understand whether lobbying in general shapes policy? Only to an extent. The directors with experience in lobbying arrive from companies that have consistently lobbied. This implies that their experiences with lobbying are likely to be positive. They will give a positive assessment of the lobbying market, and might even overestimate the returns to lobbying or underestimate the uncertainty associated with them. Of course, they might be wrong, and lobbying might not be beneficial for the company. What this paper shows is that positive information about the lobbying market is highly effective in pushing firms into building their own political presence, indicating that information is plays a big role in keeping firms out of lobbying.

There are a number of possible extensions for future research. First, it would be

valuable for future research to examine state-level lobbying. This would also help delineate how far the results presented here can be generalized. Second, while lobbying is the most utilized political strategy of firms, it would be very fruitful to examine other strategies, too. For example, does lack of information also help us understand why most firms choose not to hire politically connected personnel (Shepherd and You, 2020), not to provide input in the regulatory rule-making process, (Yackee and Yackee, 2006; Libgober, 2020; You, 2017), and not to donate to political campaigns (Stuckatz, 2022)?

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Online appendix for: Why Don't Firms Lobby?

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A Lobbying Produces Financial Returns

In this appendix, we analyze the firm-level financial returns to lobbying, replicating parts of the analysis in Huneus and Kim (2018). Using annual firm-level data, we estimate OLS regressions with fixed effects for firm and year. We use revenue from sales and gross profits as our dependent variables and lobbying expenditure as our independent variables. All variables are logged.

Table A.1 show the results. Column 1 and 3 show the association between, respectively, sales and gross profits and lobbying expenditure. As we can see, there are large financial returns associated with lobbying for the average firm. To ensure that we are not simply capturing differences between small and large firms operating in the same industries, columns 2 and 4 control for logged total assets. The results maintain.

Table A.1: Returns to Lobbying

	<i>Dependent variable:</i>			
	ln Sales		ln Gross Profit	
	(1)	(2)	(3)	(4)
ln Lobby Expenditure + 1	0.019*** (0.002)	0.008*** (0.001)	0.007*** (0.001)	0.005*** (0.001)
ln Total Assets		0.582*** (0.010)		0.163*** (0.005)
Firm Fixed Effects?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes
Observations	100,867	100,855	100,104	100,032

Note: Data is a firm-year panel. Standard errors in parentheses are robust to firm-level clustering. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

B Robustness of the Findings from the SEC Experiment

B.1 Covariate Balance in the SEC Experiment

Table B.1 shows that an important set of pre-treatment covariates are balanced. We examine covariates in the pre-treatment period 2000-2003, and show that firms that were treated with the removal of the uptick rule were neither more likely to lobby, to spend more on lobbying, to be larger (in terms of assets), to be more high-performing (in terms of revenue and profits), nor to be more liquid. This suggests that the SEC was successful in its randomization scheme.

B.2 Alternative Model Specifications

In the main paper, we make two important choices regarding the model specification when examining the impact of the SEC experiment. First, we do not include fixed effects for firm and time, but rather estimate a simple difference in means that is allowed to vary depending on prior lobbying behavior. Second, we zoom in on the 1,000 largest firms in the Russel 3000, because the SEC ruined its own randomization scheme by partially treating the control group among the 1,000 largest firms.

In Figure B.1, we examine the robustness of these choices. In Panel A, we include firm and time (semester-year) fixed effects and estimate a difference-in-differences model. In Panel B, we examine the effect among the 2,000 smallest firms. Importantly, both alternative specifications yields similar results.

Table B.1: Covariate Balance in the SECs Short-Selling Experiment

	<i>Dependent variable:</i>	
	Regulation SHO Pilot Firm?	
	(1)	(2)
Lobby?	−0.033 (0.055)	
ln Lobbying Expenditure + 1		−0.003 (0.005)
ln Total Assets + 1	−0.004 (0.009)	−0.004 (0.009)
ln Revenue + 1	−0.008 (0.013)	−0.008 (0.013)
ln Gross Profits + 1	0.014 (0.014)	0.014 (0.014)
ln Cash Holdings + 1	−0.005 (0.006)	−0.005 (0.006)
Observations	21,427	21,427
R ²	0.001	0.001
Adjusted R ²	0.0003	0.0003
Residual Std. Error (df = 21418)	0.471	0.471
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01	

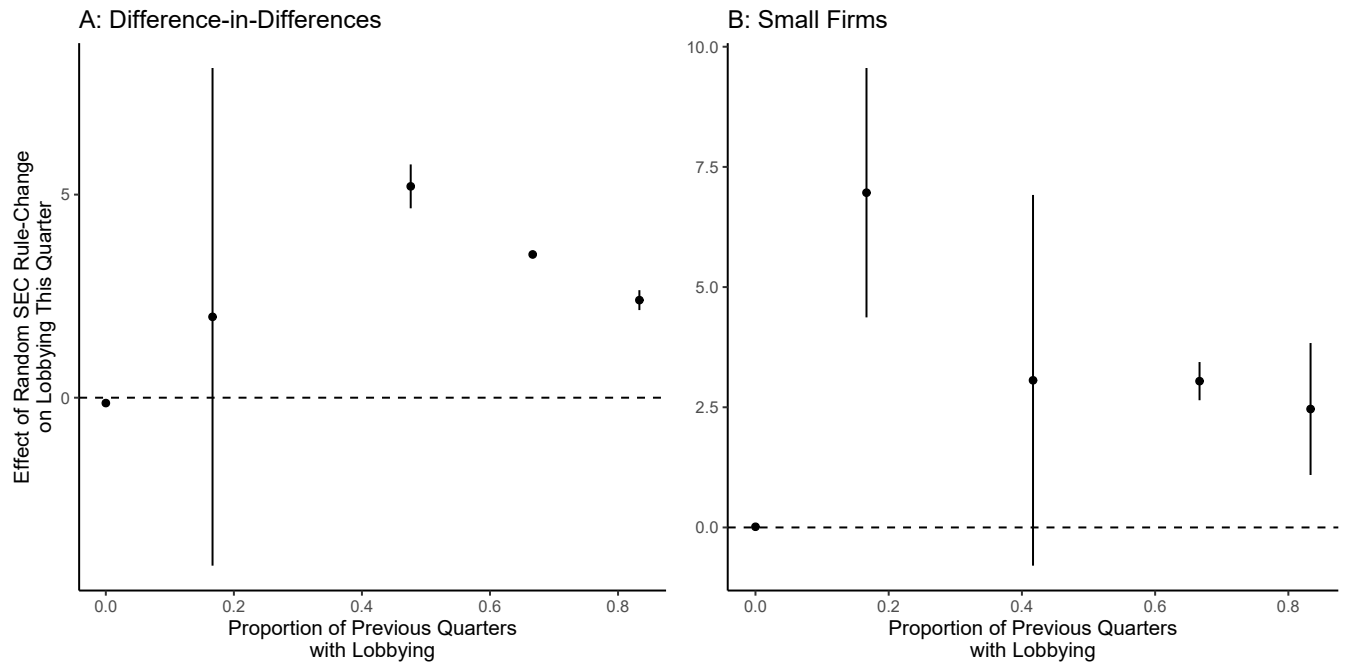


Figure B.1: Robustness of the Findings from the SEC Experiment to Alternative Specifications. *Note: The figure shows the robustness of the main findings to alternative specifications. Panel A shows results from adding firm and semester-year fixed effects to estimate a difference-in-differences specification similar to that in Kroeger and Silfa (2023). Panel B shows the results of the experiment among small firms. Confidence intervals are 95% with firm-level clustering. The Hainmueller et al. (2019) binning estimator is used.*

C External Validity: Generalized Political Risk Conditionally Increases Lobbying

To the extent that political factors are deemed to play a part in shaping earnings, these calls can be used to quantify management’s assessment of the firm’s political exposure. Hassan et al. (2019)’s measure uses two dictionaries, capturing words related to risk and politics, respectively. They use them to extract all bigrams from the earnings calls that deal with both risk and politics. They count the political risk bigrams and normalize this by the length of the transcript. Thus, this captures how large a part of the earnings call senior management and investors devoted to talk about exposure to politics that they deemed would be risk factors for the firm.

There are several reasons why this is a good measure of the type of firm-level political exposure we are interested in. First, we are interested in the risk induced by political exposure, because theory predicts that firms will seek to use lobbying to extract information from the political realm to eliminate risk. Second, this measure captures the management’s assessment of the firm’s exposure to political risk. This is important, because it is their subjective appraisal of the political environment that should lead them to lobby. On the other hand, this risk measure is not perfect: management’s choice of topics are endogenous, and the concept that is being measured is imprecisely defined.

In this analysis, we use the log of lobbying expenditure as the dependent variable, as this best captures the intensity with which firms lobby. This allows us to examine a key part of the first observable implication, which is that firms with a history of lobbying should lobby more intensively as a reaction to a political shock. We subset the data to the period after the HLOGA went into effect, i.e. after 2007. The HLOGA changed some reporting requirements in the LDA, among other things that reports should be filed quarterly rather than bi-annually. Since there is plenty of data after the HLOGA, focusing on this period allows us to avoid imperfect aggregation, and considering whether the passage of the law itself changes confounds our variables of interest. Combined with the data on firm-level risk, this provides an unbalanced panel consisting of 10,372 firms

observed quarterly combining to a total of 260,746 firm-quarter observations.

When we examine the link between political risk and lobbying behavior, we estimate the following regression model at the firm-quarter level:

$$\ln Expenditure_{iq} = \delta \cdot \ln PRisk_{iq} + \delta_2 LobbyHistory_{is} + f(\ln PRisk_{iq} \cdot LobbyHistory_{is}) + \gamma_i + \sigma_q + \epsilon_{iq} \quad (3)$$

Here, *Expenditure* is the amount firm *i* spends on lobbying, and *PRisk* is the Hassan et al. (2019) estimate of political risk. We log both these measures and add one to deal with zeros. *LobbyHistory* is the proportion of previous quarters, where the firm filed a lobby report. We are interested in both the correlation between *PRisk* and *Lobby*, and the interaction between *LobbyHistory* and *PRisk*. Again, we use Hainmueller et al. (2019) to estimate the non-linear interaction between randomized political exposure and a prior history of lobbying. γ is a firm fixed effect, and σ capturing homogeneous shocks in time. ϵ is an idiosyncratic error term.

C.1 Results

C.2 Average Impact of Political Risk on Lobbying

In Table C.2, we present fixed effects regressions of the overall correlation between lobbying activity on political risk. Column one shows the relation between political risk and the probability of lobbying. Column three shows the relation between risk and lobbying expenditure. Columns two and four adds controls for firm size. While we do estimate very precise correlations, the coefficients are very small.⁹

⁹The results are similar to the ones reported in Hassan et al. (2019), besides the fact that we include firm fixed effects. It is important to note, however, that if we include firm fixed effects but use the replication data from Hassan et al. (2019), we find no statistically significant correlation. This may be because the LobbyView (Kim, 2018) data is of higher quality and contains less noise.

Table C.2: Political Risk and Corporate Lobbying

	<i>Dependent variable:</i>			
	Lobby?		ln Lobby Expenditure	
	(1)	(2)	(3)	(4)
ln Political Risk	0.001*** (0.0002)	0.004** (0.002)	0.007*** (0.002)	0.044** (0.020)
ln Total Assets		0.011 (0.020)		0.423** (0.207)
ln Cash Holdings		0.009 (0.010)		0.089 (0.107)
Firm Fixed Effects?	Yes	Yes	Yes	Yes
Year-Quarter Fixed Effects?	Yes	Yes	Yes	Yes
Observations	262,488	11,112	262,488	11,112
Residual Std. Error	0.121	0.241	1.334	2.587

*Note: Data is a firm-quarter panel. Robust standard errors with firm-level clustering in parentheses.. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.*

C.3 Effects among Ever and Never-Lobbying Firms

As we have seen in the previous appendix, the average impact of political risk on lobbying is very small. Importantly, the reason for this is that the large majority firms *never* engage in lobbying, and political risk plays no role mobilizing those firms politically. Figure C.2, which plots the results from non-linear interaction model, shows this by allowing different effects of political risk conditional on the firm's average pre-treatment tendency to lobby. Panel A shows how a one-quarter increase in political risk correlates strongly with lobbying expenditure among firms with a history of lobbying. The association varies in a non-linear fashion, and is strongest among firms with intermediate lobbying experience. Importantly, we uncover an extremely precisely estimated null correlation among the firms that never have lobbied before. Because it takes time to build a lobbying presence, it might not make sense to start lobbying in the face of a short term political risk shock.

Therefore, we investigate whether our conclusion changes, when a firm is exposed to a prolonged period of political risk. To do so, we calculate three moving averages of political risk. Panels B through D examines the correlation between lobbying expenditure and a 1% increase in political risk averaged over one, two and three years, respectively. The correlation increases dramatically among firms that have lobbied before, and even approaches an elasticity of 1 among firms with intermediate prior lobbying levels in Panel D. This suggests that a sustained increase in political risk over a longer period of time is associated more strongly with lobbying expenditure among firms that already lobby. However, even a prolonged period of political risk does nothing to push firms that have not lobbied before into corporate political activity—in all three panels, we uncover very precisely estimated null correlations among this subset of firms.

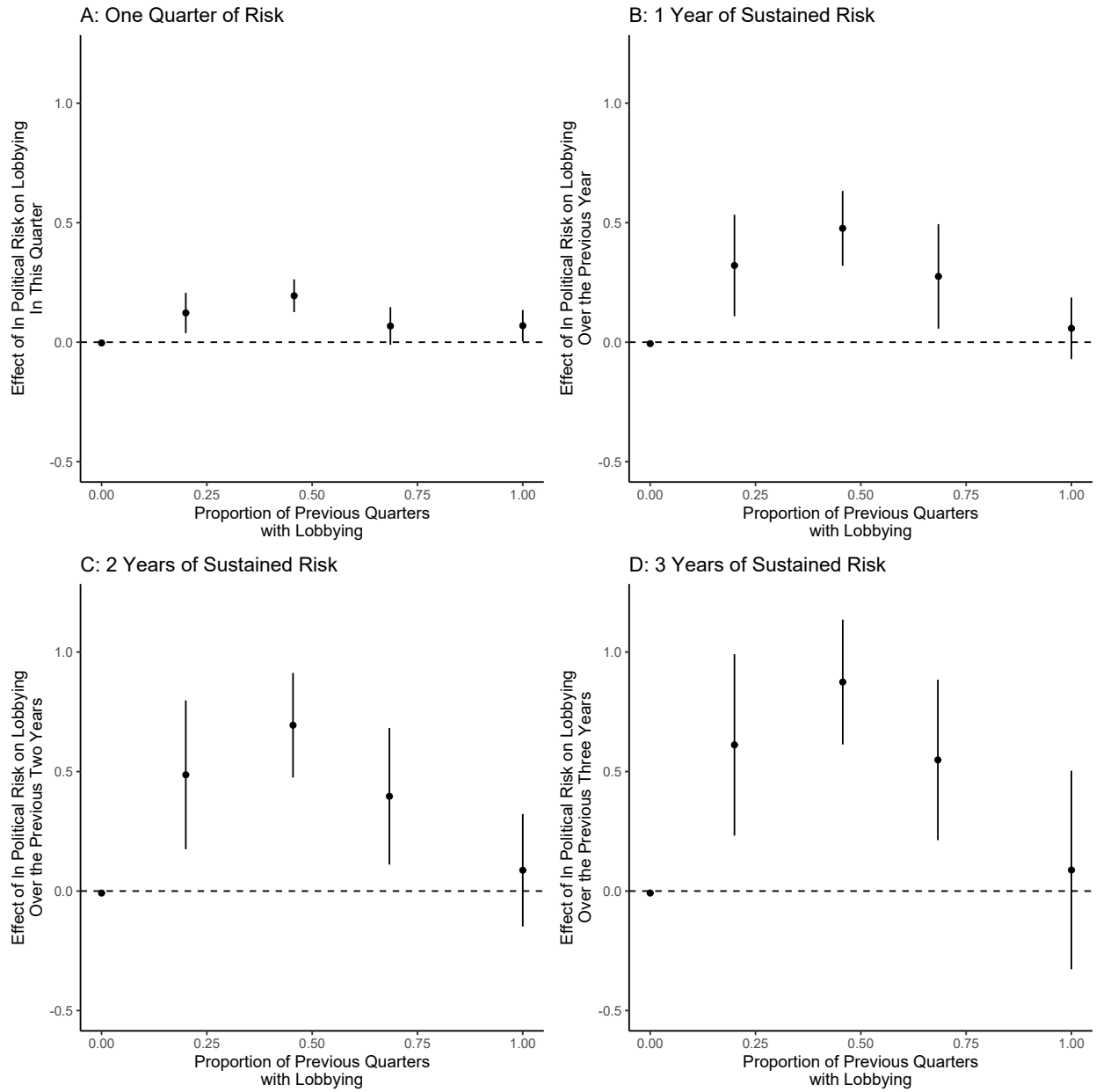


Figure C.2: History of Lobbying Moderates the Effect of Political Risk. *Note:* The figure shows the relationship between political risk exposure and lobbying for different levels of previous lobbying history. Estimates produced using the Hainmueller et al. (2019) binning estimator. Robust confidence intervals are 95% with firm-level clustering. Bins are chosen to reflect the 25th, 50th and 75th percentile among firms that do lobby. A fifth bin is added for firms with no prior lobbying activity. Table with estimate can be found in Appendix C.2.

D Sensitivity to the Parallel Trends Assumption

The identifying assumption of this difference-in-differences design is that if companies had not appointed a lobbying director, they would have followed a parallel trend in filing their first LDA report compared to those that did appoint one – the parallel trends absent treatment assumption. This assumption is fundamentally untestable, as it requires assumptions about a counterfactual world where the firm never hired the lobbying director. Recently, however, Rambachan and Roth (2019) has proposed a sensitivity estimator that corrects for parallel trend violations of varying levels. Specifically, the estimator uses observed violations in the pre-treatment period and provides bounds on the ATT by imposing violations in some post-treatment period of comparable sizes. In Figure 3c we present results from two ways of doing this. In Panel A, we first impose a linear trend through the pre-treatment period (0 on the horizontal axis). All points after that show the sensitivity to deviations from the linear pre-trend compared to the largest observed deviation. That is, 1 on the horizontal axis shows bounds on the Callaway and Sant’Anna (2020) estimate after correcting for a deviation from the linear trend that is as large as the largest deviation observed in the pre-treatment period. The last point, 11, is eleven times as large. In Panel B, we do not impose a functional form on the pre-trends, but simply observe the largest single deviation between the treated and control group in the pre-period. 1 on the horizontal axis shows the bounds on the ATT after correcting for a parallel trends violation that is at least as large as the largest deviation observed in the pre-treatment period. 11 is, again, eleven times as large.

We can see from Panel A, that the ATT remains distinguishable from zero until we impose a deviation from the linear trend that is eight times larger than what was observed in the pre-treatment window. Panel B shows that the ATT remains distinguishable from zero until we impose a deviation from the parallel pre-trend that is ten times larger than what was observed in the pre-treatment window. Importantly, as we can see, the midpoint between the bounds change very little, and most changes are driven by the increasing bounds. This suggests that the changes are driven by the increasing uncertainty that arises through the estimation procedure.

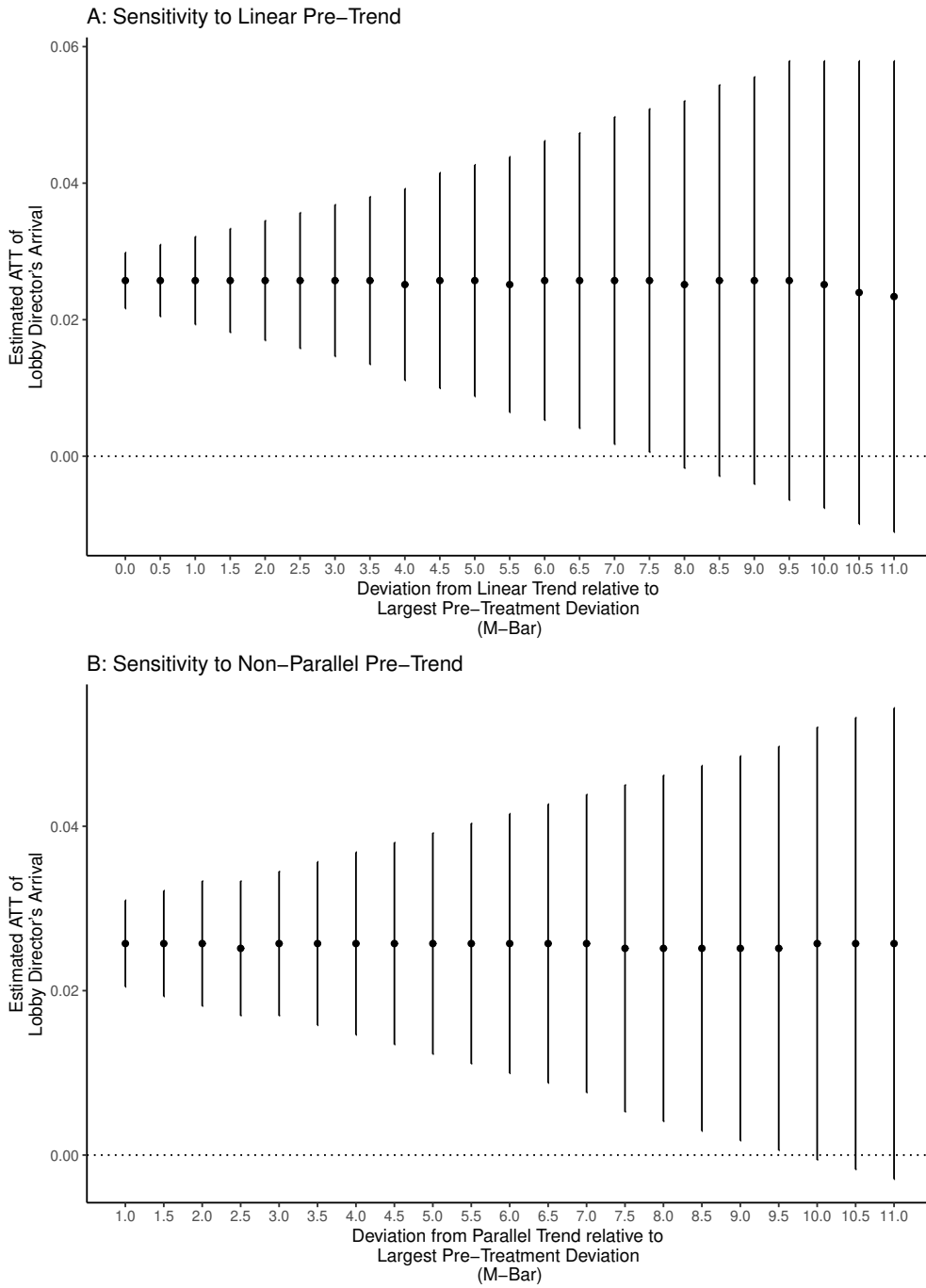


Figure D.1: Sensitivity to Parallel Trends Violations. *Note:* The figure shows estimates from the Rambachan and Roth (2019) estimator. In Panel A, each point shows the Callaway and Sant’Anna (2020) estimate after correcting for deviations from a linear trend. 0 is corrected for a linear pre-trend, while each following estimate is corrected for a deviation from that trend. In Panel B, the estimates are corrected for deviations from parallel pre-trends. 1 denotes a correction as large as the largest deviation observed in the pre-treatment period. Solid lines represent bounds on the estimated ATT, and the points are midway between the bounds. Uncertainty estimates are corrected for firm-level clustering.

Overall, this suggests two things. First, it shows that our findings cannot be explained by differential pre-trends, i.e. that treated firms either were trending differently or often received shocks comparable to the estimated effect. Second, it shows that a parallel trend violation would have to be extremely large compared to what has been observed historically before the results would become statistically insignificant – and even larger than anything we impose here to markedly change the estimated ATT. This reassures us that our findings are not driven by parallel trend violations.

E Accounting for Non-Random Exposure to Lobby Directors

An important threat to our difference-in-differences design is that a certain type of firm may choose to replace their current director with one that has experience with lobbying. Importantly, while our approach using the demise of a director plausibly makes it impossible for the firm to anticipate when it will need to appoint a new director, it is still a particular firm that chooses directors with lobbying experience.

However, in a recent paper Borusyak and Hull (2020) show how to deal with such ‘non-random exposure to random shocks’. In our case, we rely on the assumption that while it may be possible to predict *that* a particular director will pass away, it might not be possible to predict *when* it will happen. This provides us with random variation in when a new director will be needed. Borusyak and Hull (2020) show that this assumption about the structure of the randomness in treatment exposure provides with the necessary information to control away the fact that the exposure might not be random. In particular, by randomly permuting the timing of the death within the firm – the source of randomness – while keeping constant the source of non-random exposure – that some firms are more likely to hire lobbying directors – we can simulate the probability of a director passing away. By including this as a control in the model, it is possible to remove the potential bias arising from the fact that some firms are more likely to hire directors with knowledge about lobbying.

We permute randomly the timing of the arrival of a new director as well as the timing of a director’s passing. In Table E.1 we present the results from including this as a control in the relevant model. We use the Callaway and Sant’Anna (2020) estimator as we do in the main models. Column one shows the estimated impact of a lobbying director arriving, adjusting for the permuted arrival of any director. In column two, we adjust for the simulated probability of a director passing – this is the most important specification. As we can see, the results are robust to this. This suggests that our findings are not driven by non-random exposure of a certain type of firm to directors with lobbying experience.

Table E.1: Accounting for Non-Random Exposure to Lobbying Directors

	<i>Dependent variable:</i>	
	File First LDA Report	
	Staggered DD	Director Death
	(1)	(2)
Director w. Lobby Experience	0.068*** (0.008)	
Director Pass Away + Lobby Director		0.375*** (0.091)
Estimator	CSA	CSA
Control	Prob. new director	Prob. director passing

Note: Data is a firm-year panel consisting only of firms that have never filed an LDA report. The dependent variable is an indicator of whether the firm files its first LDA report. Standard errors in parentheses are bootstrapped with 1,000 replications using firm-level clustering. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively. Following Borusyak and Hull (2020), to control for non-random exposure, column 1 includes a control for the simulated probability of any new director arriving. Column 2 controls for the simulated probability of a director passing away. CSA = Callaway and Sant’Anna (2020).

F Larger Effects in Industries with Lobbying

To examine whether the arrival of directors with knowledge about lobbying has a different effect when competitors lobby, we split our sample into firms in *a)* NAICS 6-digit industries where *no firms lobby*, and *b)* *some firms lobby*. As we show in Appendix G, there are many industries where no firms lobby, which makes this subset analysis possible.

Figure F shows the results. As we can see, the effect is statistically significantly larger in industries where some firms lobby.

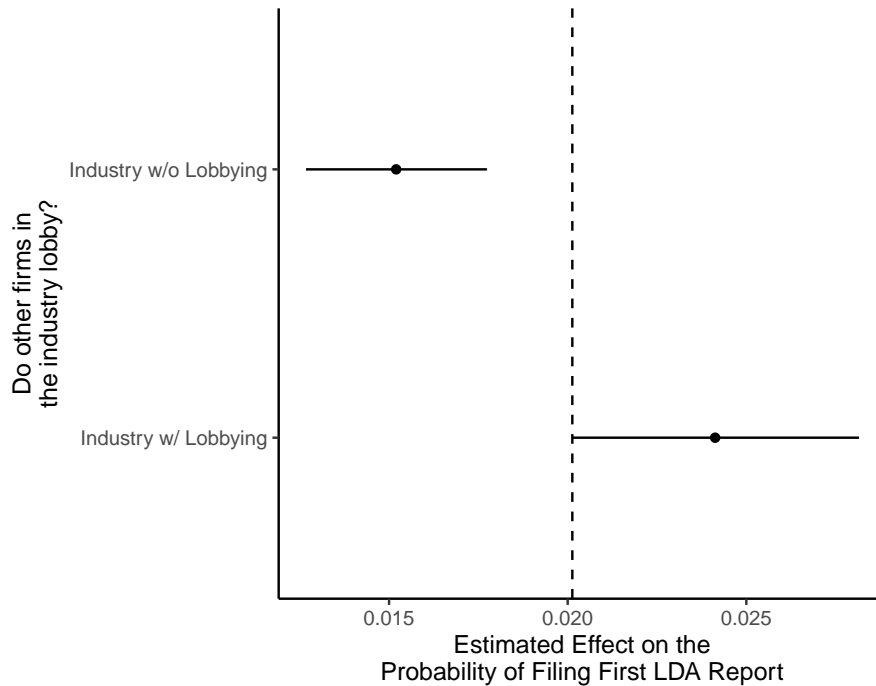


Figure F.1: The Effect of Director Arrival is Largest in Industries Where Some Firms Lobby. The figure shows results from estimating the Callaway and Sant’Anna (2020) difference-in-differences model within subsets of industries where 1) no firms lobby and 2) where some firms lobby. Solid lines are 95% robust confidence intervals with firm-level clustering. The vertical dashed line shows the lower limit of the confidence interval in industries where firms lobby.

G Lobbying is Strongly Concentrated within and between Industries

Lobbying is surprisingly concentrated both within and between industries: It is only in some industries that *any* firm lobbies, but even within the industries where firms lobby,

it tends to be concentrated among very few firms. This implies that decision-makers only hear from a biased subset of firms on any particular issue.

To illustrate the severity of this issue, we present two stylized facts showing how unequally lobbying is distributed. Panel A of Figure G.1 shows how much each industry (NAICS 6-digit) has spent in the average year throughout the period of study. As we can see, lobbying expenditures are very unequally distributed, with few industries spending vastly more than the rest. In 30% of industries no firms have lobbied at all during the period. From a decision-maker perspective, this will give rise to biased information. For example, firms in the insurance carrier industry are among the very top spenders with an average total lobbying expenditure of \$80 million per year. On the other hand, firms in the Insurance and Employee Benefit Funds industry (i.e. corporations providing insurance and benefits for employees in sponsor firms) have not lobbied at all during this period. Similarly, firms that construct residential buildings spent \$400,000 in the average year, while firms constructing non-residential buildings spent nothing at all. Importantly, both types of insurance providers, and both types of construction companies, are subject to similar regulation, and input from firms in both industries would be valuable.

In Panel B, we plot the relation between the standard deviation of lobbying expenditure and total expenditure within industries. As we can see, the largest differences in expenditure exist within the highest spending industries. This implies that even in the highest spending industries, few firms are likely to be behind most spending.

To show this directly, Figure G.2 zooms in on the 10 industries that spend most on lobbying and plots the within-industry distributions of lobbying expenditure. As we can see, even in these high-spending industries, very few firms lobby. Without exception, the spending distributions are strongly concentrated at zero, with only few firms in each industry spending anything.

These stylized facts show that both between and within industries, lobbying is highly unevenly distributed. Policymakers only receive information from a biased subset of industries. Even within industries, they only hear from a small group of high-spending firms. This implies that on any given regulatory issue, policymakers will only receive input

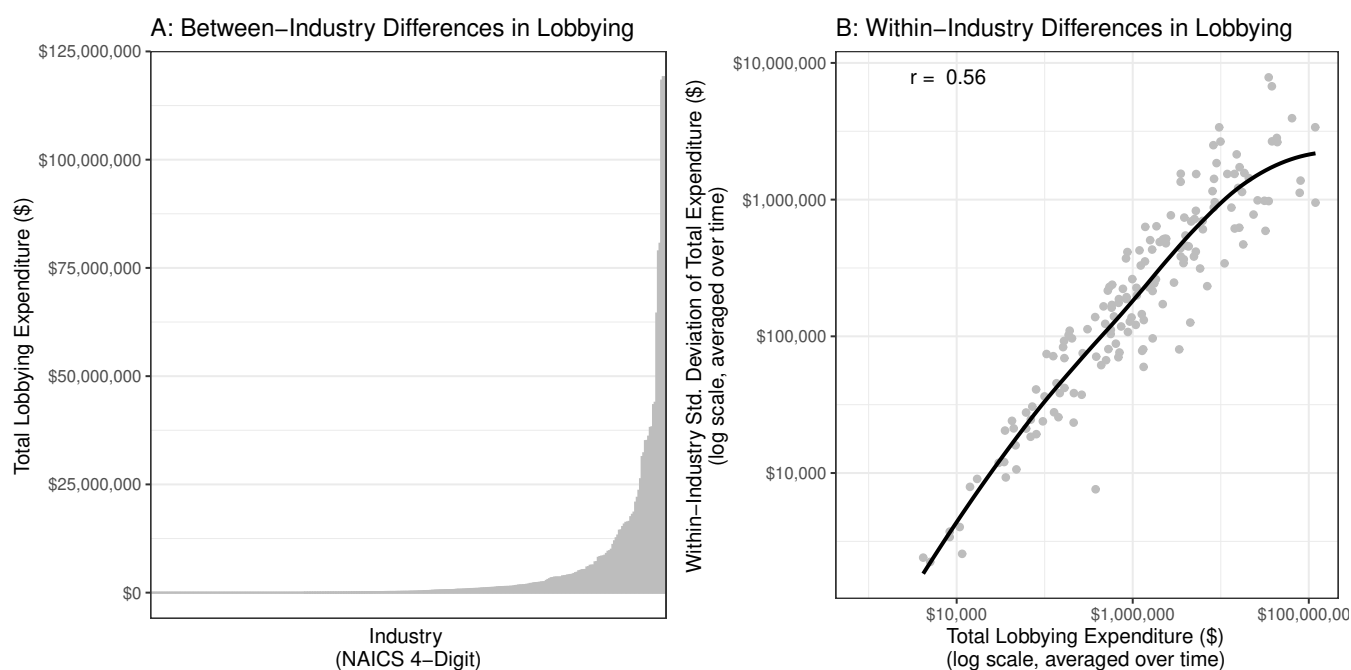


Figure G.1: Between- and Within-Industry Differences in Lobbying Expenditure. *Note: Panel A computes the yearly total expenditure in each NAICS-4 industry and then averages within industry over the period 2008-2019. Panel B computes the yearly standard deviation within each NAICS-4 industry and then calculates the average standard deviation over the period. It then shows the relation between this variance measure and the same total expenditure we use in Panel A.*

from a small minority of firms that are subject to the specific regulation. The estimated returns to lobbying (in Appendix A) suggest that this bias in the representation of firms has large consequences.

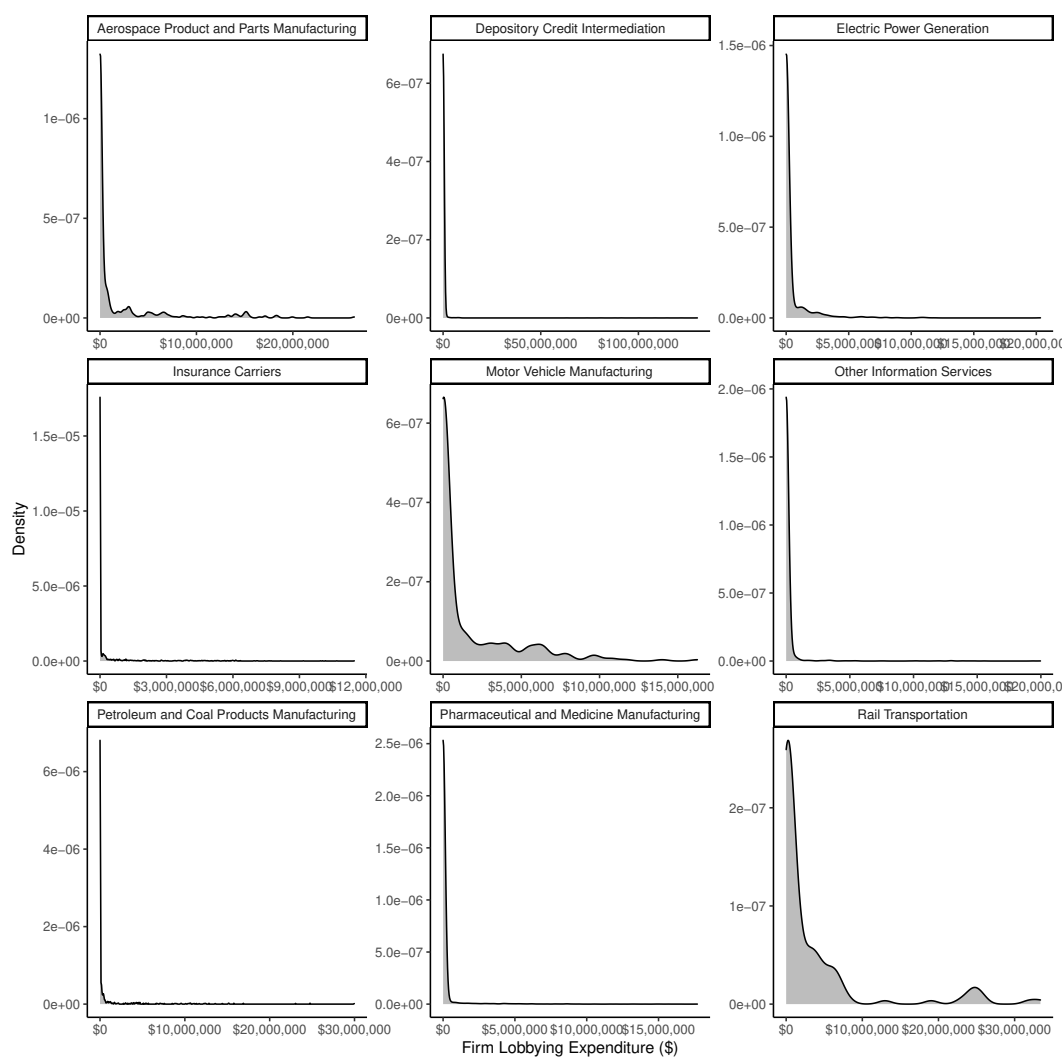


Figure G.2: The Distribution of Lobbying Expenditure Within the Top 10 Industries. *Note: The figure shows the within industry distribution of lobby spending in the 10 most highly spending NAICS-4 industries.*