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Tilting the Scales: Investigating the nature of political lobbying using contract-level data in the US

Abstract

Evidence has demonstrated that firms can successfully lobby to influence policymaking and earn positive returns that far exceed the market. However, previous work quantifying these returns has generated mixed results, and the mechanisms driving the returns have remained largely unexplored. This dissertation contributes to the literature by evaluating the determinants and effects of lobbying for federal contract funding, providing evidence that a 1% increase in lobbying expenditure results in a 0.327% larger funding obligation for publicly-traded firms. Moreover, obtaining a contract enhances a firm's access to policymakers over time, supporting the theory that contracting complements firms' lobbying efforts.

(7498 words)

1 Introduction

Lobbying lies at the intersection of the political and economic arenas, providing a backdoor for firms to tilt the scales of policymaking in their favour. Using the variation in lobbying and contract funding in the US over 2020 and 2021, I find that lobbying plays a significant role in federal contract policymaking during times of crisis. An extra 1% of lobbying expenditure increases additional contract funding by 0.327% in the next quarter. Moreover, I find that this effect is 0.127% larger if the contract is long-standing, equivalent to gross return of 326%, rising to 1,147% for long-standing contracts. These findings offer the first empirical investigation into the effects of contract lobbying during times of crisis and provide the first evidence that contracts enhance firms' access to policymakers, enabling them to improve their returns over time (Kelleher & Yackee, 2008). Therefore, I argue that government contracts can be viewed as a political investment for a firm, complementary to political lobbying, and yielding considerable returns over time.

Political lobbying is the transfer of information between interest groups and politicians, their staff, and agents, within private meetings and venues (de Figueiredo & Richter, 2014). In order to gain access, interest groups can hire professional external lobbying firms or employ their own 'in-house' lobbyists, but they may not directly transfer funds to politicians. Establishing a political network in this way is an expensive investment, confining political access to the world's largest corporations – just 10% of public firms actively lobby (Kerr *et al*, 2014). Consequently, popular press declares that lobbying is a clandestine and rent-seeking activity, whereby only powerful and greedy corporations can buy their way into Congress to secure themselves a larger slice of the economic pie. However, controversy remains in evaluating how effectively lobbying can influence policymaking, and measuring the returns that a firm can achieve by investing in political access. Current estimates for the returns to lobbying range between 130% (Kang, 2015) and 22,000% (Alexander *et al*, 2009), far exceeding the market. The lack of consensus estimates has led leading theorists to label their identification as an "extraordinarily challenging question to tackle" (de Figueiredo & Richter, 2014, p.168), motivating this dissertation to turn to an advantageous new environment - the COVID-19 pandemic. I employ a multi-level fixed effects model and a two-step Heckman selection model to measure the returns to lobbying for federal contracts and understand the mechanisms underlying these returns whilst accounting for common endogeneity, selection, and reverse causality concerns prevalent in the literature.

Understanding the mechanisms driving lobbying returns is necessary to justify their magnitude. A related explanation is known as 'Tullock's puzzle', which asks why interest group expenditures are small relative to the value of government policy, thereby enabling successful firms to make attractive returns (Tullock, 1972). Barriers to entry are usually blamed for the failure of arbitrage in the political access market, but understanding how these constraints arise is key to justifying these market-beating returns. By measuring lobbying returns for federal contracts over the pandemic, this dissertation also investigates whether contract rights are complementary to a firm's lobbying efforts, in order to identify entry constraints.

The significance of the new environment in this study is crucial to the empirical design. The outbreak of the COVID-19 pandemic shook the global economy, and the US lobbying industry skyrocketed. Firms felt a surge in demand for political influence in the face of widespread uncertainty and vulnerability, with lobbying spending reaching a near-record of $903 million in Q1-2020[1]. As the US government rushed to implement a national stimulus programme, firms deployed lobbyists to Washington to state their claim for a share of the relief. In the end, six acts designed to contain the effects of the

[1] OpenSecrets, (2020). "COVID-19 Lobbying", July 2020.

pandemic were passed, with the government offering a menu of aid including loans, grants, direct payments, and contracts. The Coronavirus Aid, Relief, and Economic Security Act was signed into law on March 27th, 2020, and later the American Rescue Plan was initiated on March 11th, 2021, collectively obligating over $4 trillion to curb the consequences of the pandemic. Within these acts, over 55,000 government contracts[2] were administered at an unprecedented scale, with politicians and bureaucrats exercising considerable discretion over funding decisions and contract allocation (Rose-Ackerman, 2021).

This environment is advantageous for several reasons. Firstly, the uncertainty induced by the pandemic creates useful variation in both the demand for contracts, and the budgets of policymakers across time, to identify the returns to lobbying. Secondly, this is a rare occasion where the outcomes of lobbying are quantified in monetary amounts, which has been a key obstacle in explicitly measuring lobbying returns. Thirdly, since crises necessitate socially responsible policymaking, crisis lobbying is understood to be of primary importance, yet little is known about it (Jamieson & Louis-Charles, 2022).

I construct a novel panel dataset tailored to this environment by combining the lobbying reports of public firms in the US with the obligated funding for their COVID-19 contracts. The final dataset carries unique advantages over previous attempts. Firstly, I can partially control for additional funding opportunities, which create a reverse causality problem, since the dataset records a contract's 'potential' but not yet obligated value. Secondly, previous datasets commonly use a company's aggregate lobbying expenditure within a period, rather than on specific issues, creating a downwards bias in the returns. Since contracts are awarded by individual government agencies, I can more accurately quantify the returns to lobbying. Lastly, as

[2] UsaSpending, available at: https://www.usaspending.gov/search/.

aforementioned, the pandemic response creates sufficient identifying variation for a multi-level fixed effects model to be estimated.

To the best of my knowledge, this is the first empirical attempt to quantify lobbying returns during the COVID-19 pandemic, one of the first to quantify the returns to lobbying for contract funding, and the first to find empirically test whether contract rights improve lobbying returns over time.

The remainder of this dissertation proceeds as follows. Section 2 provides a critical review of the existing literature on the effectiveness of lobbying. Section 3 outlines the sources and construction of my dataset. Section 4 describes the empirical strategy. Section 5 presents the results and Section 6 presents the robustness checks. Section 7 then concludes.

2 Literature Review

2.1 Econometric Considerations

The existing literature has investigated the effectiveness of lobbying across several different areas of policymaking, though the effects can only be quantified in monetary amounts in a handful of areas. Consequently, most papers focus on policy related to effective tax rates and tax repatriation holidays (Richter *et al*, 2009, Alexander *et al*, 2009), or budgeting, grants, and appropriations (de Figueiredo & Silverman, 2006, Cox, 2022). Other approaches choose to either estimate the average value of a successful lobbying effort (e.g., Kang, 2015, estimates each policy to be worth over $500 million), or to indirectly measure the financial outcomes of firms that lobby (Girard *et al*, 2023, Huneeus & Kim, 2018, Kim, 2008). Arguably, these indirect methods are susceptible to misspecification and measurement error, creating a motivation to focus on directly observable monetary returns through changes to federal contract funding.

There are five main econometric challenges that a successful study of lobbying returns should overcome. I first analyse the common endogeneity concerns prevalent in the lobbying field and outline my new strategies to overcome them, before exploring select successes and limitations in the literature to date in Section 2.2.

Firstly, cross-time variation in lobbying efforts can be limited due to 'stickiness', making causal identification difficult. De Figueiredo & Richter, 2014, provide the example of an interest group expending $100,000 to lobby each year, and being awarded funding of $1 million annually. This simple example suggests a gross return of 1000%. However, without a shock to the lobbyist's behaviour, or the size of the contract, the time-variation necessary for fixed effects is not present. This is a widespread problem which has resulted in a scarcity of studies and a huge variance in returns wherever a study is feasible. Whilst other authors have overcome this problem by lengthening their time period, I instead focus on crisis lobbying to capture sufficient variation.

Secondly, the optimal lobbying expenditure may be associated with the opportunity to receive a generous return, creating reverse causality. Consequently, the OLS estimates will only represent a partial correlation between lobbying expenditures and contract funding. This is the first empirical attempt able to directly control for opportunistic beliefs, since my dataset includes the federal estimate of a contract's expected future value. Other papers have utilised instrumental variables to overcome reverse causality, notably de Figueiredo & Silverman (2006) who consider higher-education institutions, though exogenous IVs are notoriously difficult to find and justify in the literature (for instance in, Kim, 2008, Goldstein & You, 2017).

The third and fourth econometric problems are related: the omitted-variable problem, and the self-selection problem. If an omitted-variable is correlated with the error term in a regression, then OLS estimates will be biased, and causal inference will be invalid (Wooldridge, 2018). Insufficient time-variation can compound this problem by making fixed effects inestimable, allowing time-invariant characteristics to bias the estimates. Since the decision to lobby is non-random, assignment into the treatment group could be correlated with outcomes, meaning that firms may only lobby if they expect greater returns. Ignoring this sample selection problem will introduce an upwards bias into the returns (de Figueiredo & Richter, 2014). My approach incorporates multi-level fixed effects to account for time-invariant endogeneity Heckman two-step selection model to tackle the selection problem.

Finally, whilst the Lobbying Disclosure Act of 1995 (LDA) improved data availability, measurement error remains a key concern as exact lobbying efforts on specific issues are not observed through lobbying disclosure reports. Inaccurate quantification of lobbying efforts will, at best, reduce the significance of our estimates, and at worst introduce an attenuation bias. Whilst impossible to fully overcome with the available data, I reduce measurement error by conditioning lobbying expenditures on being related to COVID-19 issues and being targeted at the awarding agency of a contract.

With these five econometric difficulties, we can review some of the literature's most prominent papers with a critical eye.

2.2 Empirical Approaches to Calculating Lobbying Returns

Richter *et al*, 2009, found that increasing lobbying expenditures by 1% translates into a reduction in effective tax rates by 0.5-1.6%, equivalent to a 600% average return. Using firm and time fixed effects, they estimate the following regression:

$$\mathrm{ETR}_{i,t} = \gamma \mathrm{Lobby}_{i,t-1} + \mathbf{X}_{i,t-1}^{\mathbf{T}} \boldsymbol{\beta} + \rho \mathrm{ETR}_{i,t-1} + \alpha_i + \alpha_t + \varepsilon_{it}$$

By exploring the long time period of 1998-2005, the authors identify sufficient variation in expenditure and effective tax rates. Moreover, they attempt to resolve omitted-variable bias through two-way fixed effects with time-variant controls known to affect tax rates, and a Heckman selection-model to test the self-selection problem. However, relying on a long time period allows time-variant omitted variables associated with lobbying, such as PAC donations, to potentially become significant (Tripathi *et al*, 2002). Further, the authors claim that applying a natural logarithm transformation necessitates dropping their control group of non-lobbying firms. This is a key limitation since the control group contains valuable counterfactual variation, and results can be obtained by transforming variables with $ln(1+X_{it})$. Finally, reverse causality remains an issue as the optimal level of lobbying expenditure may be associated with the opportunity to win lower effective tax rates, so the results only imply an equilibrium condition (Richter *et al*, 2009).

Hill *et al*, 2013, present a second empirical paper influential to my design. The authors examine the relationship between lobbying activity and a firm's excess returns, finding evidence that shareholders do value lobbying activity. Firstly, probit and tobit models are employed to explore the determinants of the decision to lobby and lobbying intensity. Their results corroborate regular findings in the literature, such as a positive association between size, the decision to lobby, and lobbying intensity, often explained by the presence of scale economies within the lobbying industry. Secondly, the authors run a two-way fixed effects model to investigate the relation between lobbying and excess returns, finding a significant and positive relationship. To test the robustness of their findings, they utilise a Heckman two-step selection model, which I also estimate within my own robustness considerations in Section 6, as well as probit and tobit models to investigate the determinants of lobbying for contract funding.

2.3 Findings Specific to Contract Lobbying

Within federal contract policymaking, researchers have predominately focussed on the effects of electoral campaign contributions (Brogaard *et al*, 2016, Ferris *et al*, 2019), and attempts to evaluate the effects of lobbying empirically have only recently begun (Dusso *et al* 2019, Cox, 2022). Dusso *et al*, 2019, find that only directly lobbying the awarding agencies of federal contracts creates significant returns, rather than lobbying Congress itself. Further, Cox, 2022, finds that the extent of lobbying is decreasing in the degree of competition in the contract-allocation process. Given these recent results, I measure lobbying efforts related to the awarding agency of the contract only, rather than using a firm's overall lobbying expenditures. Additionally, I consider changes to contract funding once contracts have been administered, given the role of competition played in contract allocation, highlighted by Cox, 2022.

Finally, from a theoretical perspective, Kelleher & Yackee (2008) present important predictions that could help identify entry constraints in the lobbying industry underlying Tullock's puzzle. They hypothesise that obtaining a contract enhances a firm's access to politicians and find a 5-17% increase in overall interest group influence as contract rights limit competition and encourage coordination between contracted firms and government agencies. Relatedly, Dusso *et al*, 2019, suggest that "it is the long-term relationships that lobbyists build with bureaucrats that yield the most lucrative contracts" (Dusso *et al*, 2019, p.1), calling for empirical research to validate this claim. This is an important hypothesis for the politics of privatization, implying that a federal contract can be seen as a form of political investment distinct to lobbying, though complementary.

Therefore, a robust empirical design requires sufficient variation, ideally to enable fixed effects, minimal measurement error, and with a consideration of the sample selection bias and reverse causality problems. Section 4 explains how I have incorporated these lessons into my empirical method.

3 Data

3.1 Dataset Overview

To empirically investigate the effects of lobbying on federal contracting decisions, I have constructed a unique dataset from a variety of sources. Firstly, lobbying disclosure reports reported under the LDA are obtained from a new database published by *LobbyingData*[3]. These reports identify the lobbying company, which department(s) they lobbied, their expenditure, the individual lobbying firms they employed, their reasons for lobbying, and the Acts they lobbied on, on a quarterly basis. Secondly, US COVID-19 contract data was obtained from *USASpending*[4]. This dataset details the initial allocation and subsequent amendments to all contracts issued under the Acts passed to fight against the COVID-19 outbreak, including the unique contract ID, the recipient parent companies, and many other contract-specific attributes. The financial characteristics of contract recipients were acquired from *Bloomberg*, and regional COVID-19 data was obtained from *The New York Times COVID database*[5].

The completed dataset contains 27,000 observations across 360 public firms and 6,775 federal contracts at the firm-contract level from Q2-2020 to Q4-2021, with 38% of observations involving lobbying and 45% involving funding changes. Summary statistics for lobbying and non-lobbying firms are presented in Table I.

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[3] Acquired from: https://www.lobbyingdata.com/ [Accessed: 10/01/2023].
[4] Available at: https://www.usaspending.gov/search [Accessed: 16/02/2023].
[5] Available at: https://github.com/nytimes/covid-19-data/tree/master/rolling-averages [Accessed: 30/03/2023].

An important feature of these contracts is that some were issued many years prior to the onset of the pandemic. Policymakers could either award additional funding from COVID-19 resources to existing contracts already allocated to firms, effectively increasing their scope, or issue new contracts, with the obligation of COVID-19 funds beginning from April 1st, 2020. I utilise this structure to investigate the differential effect of lobbying on contracts assigned some years before the pandemic, compared to recently allocated contracts. Note that my dataset identifies the funding allocated specifically under the COVID-19 acts, even if the contract consists of previous non-pandemic funding too. I ensure that lobbying is also COVID-19-related to justify this approach.

3.2 Dataset Construction

The following methodology was employed to construct the dataset:

First, I identified lobbying reports associated with COVID-19 related issues by examining whether the report lobbied on any of the COVID-19 acts, or if the report mentioned any of the following keywords: 'COVID', 'COVID-19', or 'Coronavirus', within their reasons for lobbying, following Olson *et al*, 2020.

Secondly, to identify publicly-listed companies, I developed an algorithm that utilises *fuzzywuzzy*, a Python 'string matching' package. The *fuzzywuzzy* package identifies similar strings by calculating the minimum number of operations need to transform one string to another, and then assigns a '*fuzzy ratio'* score to indicate the degree of comparability. This algorithm was employed twice, firstly to match parent companies to a master list of all publicly-traded firms on the NYSE/NASDAQ and obtain their tickers, and secondly to match reported company names in the lobbying database to the public contract-receiving companies. All matches were manually checked to ensure a high degree of accuracy.

Lobbying reports are also riddled with ‘amendments’, where a lobbyist previously filed an inaccurate report, these reports made up around 5% of the sample. I identified pairs of reports with matching lobbying firms and quarters and removed the original, inaccurate, disclosure to reduce measurement error.

Lobbying companies often employ multiple external lobbying firms simultaneously, and therefore have multiple reports for the same quarter. I collapsed the lobbying reports by company name and quarter, creating a single report for each company in each quarter than contained all the information present across multiple reports. Similarly, I collapsed the relief data into quarters.

A panel dataset was then constructed containing entries for each company and for each contract they receive, for each quarter. Initially, the lobbying reports are identical across all contracts for the same company. However, a firm’s lobbying expenditure on COVID-19 issues may be heterogeneously split across their contracts. To account for this, I counted each time a firm lobbied the awarding agency of a contract and divided this by the total number of times that company lobbied a government agency in the quarter. For instance, if Pfizer lobbied the Department of Defence 3 times in Q3-2020, and they lobbied 10 departments in the period, then 30% of their COVID-19 lobbying expenditure is assumed to be related to their contract from the Department of Defence. The advantage of this approach is that ‘targeted’ lobbying expenditures are more relevant than overall expenditures[6], though it assumes that a company will not lobby a specific government department on COVID-19 issues for any reason other than their contract with that department. If this assumption is falsified, then this approach will downward-bias the returns to lobbying. With this approximation, the unit of

[6] Dusso *et al*, 2019, finds that contract lobbying generates returns when directed at awarding agencies, rather than the Senate or the President’s Office.

observation is firm-contract since a firms' lobbying activity may be heterogenous across their contracts.

Once the panel was created, entries were adjusted to account for the start and end quarters of contracts. Lobbying prior to contract rights being issued, and the initial allocation of funding upon awarding contract rights are ignored since my design only considers changes to contract funding once rights are issued.

The panel was then supplemented with financial and COVID-19 data. Financial characteristics including firms' size (measured by total assets), their employee count, free cash flow, and leverage were merged into the panel as controls associated with their lobbying decisions. Although public companies operate across multiple US counties and states, the contracts they are assigned relate to a specific geographical region. A relevant time-variant control may be the number of COVID-19 cases within a contract's primary county and state (Junk *et al*, 2021), so the panel was supplemented with this data.

Table I: Summary Statistics

Explanatory Variables	(1) Lobbied		(2) Not Lobbied		Difference in Means	
	N	Mean	N	Mean	Difference	*p*-value
Size	10,137	10.80	16,702	9.80	1.00	0.000
Employees	10,086	11.44	16,463	10.46	0.98	0.000
Leverage	9,119	1.44	16,498	1.16	0.28	0.000
Free Cash Flow	10,137	0.02	16,664	-0.16	0.18	0.084
County COVID Cases	10,112	2.52	16,663	2.64	-0.12	0.000
State COVID Cases	10,137	2.56	16,776	2.66	-0.10	0.000
Potential Obligations	10,137	6.71	16,776	5.77	0.94	0.000
Long-Standing Contract	10,137	0.66	16,776	0.04	0.62	0.000

Table I presents summary statistics for important characteristics. (1) is the group of contracts with reported lobbying and (2) is the group of contracts with no reported lobbying at time t. The sample consists of quarterly firm-contract observations for Q2-2020:Q4-2021. *Lobbied* is a binary variable equal to 1 if a firm, i, lobbied on a contract, c, in the quarter, t, and 0 otherwise. *Size* is the natural logarithm of a firm's total assets. *Employees* is the natural logarithm of 1 + a firm's total employees. *Leverage* is the natural logarithm of a firm's long- and short-term debt scaled by total assets. *Free cash flow* is as a fraction of total assets. *County COVID Cases* and *State COVID Cases* record the quarterly average per 100,000 of the population + 1 and transformed with the natural logarithm. *Potential obligations* is the natural logarithm of 1 + the difference between the potential and current value of the contract. *Long-standing Contract* is a dummy variable indicating whether a contract was issued over 5 years prior to end of the period.

4 Empirical Approach

4.1 Lobbying Characteristics using Probit and Tobit Models

With the hypothesis that lobbying firms receive more generous federal contracts, I design an approach to evaluate the evidence of this claim.

Firstly, I estimate a probit regression to identify the attributes associated with a firm's decision to lobby a government agency, and then a tobit regression to identify the attributes associated with lobbying intensity, given that observed expenditures are truncated at zero, following Hill *et al*, 2013. Whilst the tobit results are interesting, they are of secondary importance. The probit results are later used in a Heckman two-stage selection model to evaluate the robustness of my primary regression. I estimate the following probit regression.

$$\mathbb{P}(\mathrm{Lobby}_{\mathrm{i,c,t}} = 1 | \mathbf{X_{i,c,t}}, \mathbf{D_t}) = \Phi(\beta_0 + \mathbf{X_{i,c,t}^T}\boldsymbol{\beta} + \mathbf{D_t^T}\boldsymbol{\delta})$$

X is a vector of firm-contract specific characteristics, and D is a vector of time dummies. The explanatory variables are described with the results in Table I. $\Phi()$ represents the cumulative density function of the standard normal distribution.

4.2 Lobbying Returns using a Multi-Level Fixed Effects Model

I develop a multi-level fixed effects regression to test my primary hypothesis. In my preferred specification, the unit of observation is firm-contract, since firms may be allocated numerous contracts and strategically exert their lobbying efforts across contracts conditional on unobserved characteristics. Intuitively, contracts and firms may have inherent characteristics making them more prone to favourable budget re-allocations, for instance a contract may receive greater funding because it has larger ‘scope’ or ‘emergency status’. Therefore, I include firm and contract fixed effects, to control for time-invariant unobserved heterogeneity. Time fixed effects are included to absorb common shocks and trends across firms, such as the national progression of the pandemic.

I control for various financial characteristics that may be associated with a firm’s optimal lobbying expenditure and the government’s budgeting decision. These characteristics include the size of a firm (measured by total assets), their employee count, their free cash flow as a portion of total assets, and their leverage. For example, a firm with many employees may be seen as a suitable target for a generous contract since this assignment could be expected to create more work opportunities. Alternatively, a highly levered firm could be seen as a risky contract recipient, though they themselves may hope to obtain a contract to alleviate their leverage concerns.

To address the reverse causality problem, where the opportunity to receive greater funding drives lobbying efforts, I include a variable named ‘potential obligations’, representing the additional funds the government may obligate to this contract. This variable is useful to capture a firm’s opportunistic beliefs and is a unique advantage of my dataset.

Finally, as discussed in Section 3, I have limited measurement error. Whilst lobbying expenditure on specific contracts is not observed, I approximate the amount lobbied conditional on being related to COVID-19 and directed at the contract’s awarding agency to ensure that the lobbying expenditure is being used to desired ends.

I estimate the following multi-level fixed effects model:

$$\ln(1+\Delta\mathrm{Contract}_{i,c,t}) = \alpha_1 \ln(1+\mathrm{LobbyExp}_{i,c,t-1}) + \mathbf{X}^{\mathbf{T}}_{\mathbf{i,c,t-1}}\boldsymbol{\beta}$$

$$+ \alpha_2 \ln(1+\mathrm{PotentialObligations}_{i,c,t-1}) + \alpha_i + \alpha_c + \alpha_t + \varepsilon_{i,c,t}$$

Where I add a constant of 1 to the lobbying expenditure to retain the control group. X is a vector of financial and COVID-19 related controls, potential obligations represent the federal estimate of potential additional funding. The model includes company, contract, and time fixed effects.

To test the theoretical predictions of Kelleher & Yackee (2008) and Dusso *et al* (2019), I also include a specification where the natural logarithm of lobbying expenditure is interacted with a dummy indicating whether a contract is 'long-standing'. Results are re-estimated after varying the cut-off, without affecting the significance or magnitude of the findings.

$$'\text{Long} - \text{Standing}' \;=\; \begin{cases} 1 & \text{if contract issued } > \text{ 5 years ago} \\ 0 & \text{if contract issued } \leq \text{ 5 years ago} \end{cases}$$

5 Results

5.1 Characteristics of Lobbying and Lobbying Intensity

The results for the probit and tobit regressions are reported in Table II. Column 1 describe the characteristics associated with the decision to lobby, whilst Columns 2 and 3 describe the characteristics associated with lobbying intensity. These results provide descriptive insight into the determinants of the lobbying process during the pandemic.

We can observe that a firm's size is negatively associated with the decision to lobby, though the literature usually finds that size increases the likelihood of lobbying because larger firms benefit from scale economies. In this context, the negative association could be explained by the period of crisis – contracting firms most economically affected may choose to lobby for additional funding. Moreover, since the sample consists of large, US public firms, the positive size effect is less relevant and generally applies to very small firms relative to large corporations.

Firms with more employees are more likely to lobby, and lobby with greater intensity, supporting the initial belief that policymakers may be more likely to award funding to firms with a larger workforce. Highly leveraged firms also lobby more intensively, which is supported by Faccio (2010), who claims that politically-connected firms have greater access to external financing. The literature finds mixed results regarding effect of free cash flow, theoretically, Jensen, 1986, identifies agency conflicts as increasing the likelihood of lobbying as managers derive utility from political engagement. However, this argument has not often been supported empirically, such as in Hill *et al*, 2013, nor here.

Finally, an increase in regional COVID-19 cases where the contract is primarily assigned reduces the likelihood of lobbying and the intensity. It may be that pandemic disruptions strain the resources of a firm, curtailing their lobbying efforts.

Table II: Determinants of the Decision to Lobby and Lobbying Intensity

Explanatory Variables	(1)	(2)	(3)
Size	-0.863*** (0.000)	-0.383*** (0.000)	-0.191*** (0.000)
Employees	2.757*** (0.000)	1.488*** (0.000)	0.188*** (0.000)
Leverage	0.039 (0.891)	1.141*** (0.000)	1.156*** (0.000)
Free Cash Flow	-0.071 (0.960)	-1.730*** (0.009)	-2.945*** (0.000)
County COVID Cases	-0.102* (0.090)	-0.013 (0.812)	0.094* (0.052)
State COVID Cases	-0.016 (0.802)	-0.110* (0.054)	-0.156*** (0.002)
Potential Obligations	0.034* (0.063)	0.035*** (0.000)	0.027*** (0.000)
Long-Standing Contract	0.630 (0.886)	0.550** (0.039)	0.022 (0.882)
Q3 2020	-0.177*** (0.001)	-0.199*** (0.003)	-0.032 (0.591)
Q4 2020	-0.759*** (0.000)	-0.780*** (0.000)	0.112 (0.177)
Q1 2021	-0.951*** (0.000)	-0.784*** (0.000)	0.670*** (0.000)
Q2 2021	-1.376*** (0.000)	-1.586*** (0.000)	-0.291*** (0.000)
Q3 2021	-1.005*** (0.000)	-1.212*** (0.000)	-0.175** (0.022)
Q4 2021	-0.459*** (0.000)	-0.685*** (0.000)	0.254*** (0.002)
Constant	-22.905*** (0.000)	-9.084*** (0.000)	-0.357 (0.176)
Method	Probit	Tobit	Tobit
Dependent Variable	Lobby	Lobbying Exp	Lobbying Exp Ratio
Observations	25,344	25,344	25,344

Table II displays the probit and tobit results. *Lobbied* is a binary variable equal to 1 if firm, i, lobbied on a contract, c, in the quarter, t, and 0 otherwise. *Lobbying Exp* is the natural logarithm of a firm's expenditure plus $1. *Lobbying Exp Ratio* is a firm-contract lobbying expenditure divided by the firm's total assets. Explanatory variables are defined as in Table I. Standard-errors are clustered at the firm-contract level, reported in parentheses. ***p<0.01, **p<0.05, *p<0.10

5.2 Returns to Lobbying

Results for the multi-level fixed effects regression are presented in Table III, Column 6 is the preferred specification. Column 1 presents the basic regression with firm, contract, and time fixed effects, whilst Column 2 includes interaction variables found to be significant. Columns 3–6 contain all independent variables, controlling for potential new funding, and the effect of a contract's age, whilst varying the fixed effect specification for robustness.

We can draw a several important observations from these regressions. Firstly, lobbying expenditure is a positive and significant determinant of the size of additional contract funding. In the preferred specification, a 1% increase in lobbying expenditure increases the change in contract funding by 0.327% on average. Secondly, this result corroborates the theoretical predictions of Kelleher & Yackee (2008), since this average effect is 0.127% larger if the contract is over 5 years old. Theoretically, contractors develop a relationship with policymakers once they have obtained a contract over time, so the possession of contract rights increases a firm's influence over budgeting decisions (Dusso *et al*, 2019). Therefore, government contracts can be viewed as a firm of political investment for a firm, distinct but complementary to lobbying.

We can also observe that the potential value of a contract is a significant driver of additional funding. Including this effect causes the elasticity of lobbying expenditure to drop by 0.167%, demonstrating the upwards bias that exists when the reverse causality problem is ignored, as in Richter *et al*, 2009.

Column 3 is the only specification where lobbying does not create a significant return for firms, it is also the specification which does not include a contract fixed effect. By ignoring contract-specific characteristics, this approach is susceptible to an omitted variable bias. Therefore, the multi-level fixed effects model considering both innate firm and contract characteristics is vital to measure lobbying returns.

Applying mean firm-contract characteristics and lobbying expenditure, this 0.327% increase in the value of contract amendments translates into an average return of 326%, conditional on lobbying and a contract duration under 5 years. Compared with the literature, this return is less than Richter *et al* (2009) finding of 600%, and much less than the 22,000% found by Alexander *et al* (2009). Given the size of these estimates, and their econometric drawbacks, this result seems sensible and provides another example of the significant positive returns to lobbying. Moreover, if the contract is more than 5 years old, this return increase to 1,147%. This substantial differential provides strong support for the theoretical prediction put forward by Kelleher & Yackee (2008) and demonstrates that contract rights are a channel of political influence overlooked in the literature. Finally, conditioning on lobbying alone (and using average contract age) gives an average return of 372%, since most COVID-19 contracts are new.

Table III: The Returns to Lobbying

Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)
Lobbying Exp$_{i,c,t-1}$	0.197*** (0.000)	0.494*** (0.002)	0.0723 (0.610)	0.361** (0.011)	0.327*** (0.010)	0.327** (0.022)
Size$_{i,t-1}$	0.996** (0.035)	0.986** (0.037)	0.519*** (0.006)	0.177 (0.500)	0.821** (0.040)	0.821* (0.069)
Employees$_{i,t-1}$	0.531** (0.034)	0.629** (0.012)	0.142 (0.425)	0.305 (0.124)	0.450** (0.016)	0.450** (0.034)
Leverage$_{i,t-1}$	-0.033 (0.922)	-0.230 (0.485)	-0.0478** (0.040)	-0.517 (0.104)	-0.416 (0.128)	-0.416 (0.179)
Free Cash Flow$_{i,t-1}$	-2.963** (0.046)	-2.855* (0.052)	-2.268* (0.090)	-2.454 (0.111)	-2.467* (0.068)	-2.468 (0.107)
State COVID Cases$_{c,t-1}$	0.153 (0.220)	0.163 (0.192)	0.316*** (0.001)	0.213* (0.068)	0.215** (0.037)	0.215* (0.066)
County COVID Cases$_{c,t-1}$	0.182 (0.126)	0.169 (0.156)	0.066 (0.481)	0.166 (0.132)	0.169* (0.083)	0.169 (0.125)
*Size * Lobbying Exp*		-0.037** (0.011)	-0.001 (0.934)	-0.025** (0.049)	-0.023* (0.051)	-0.023* (0.084)
*Leverage * Lobbying Exp*		0.075*** (0.000)	0.071*** (0.000)	0.053*** (0.001)	0.056*** (0.000)	0.056*** (0.000)
Potential Obligations$_{c,t-1}$			0.396*** (0.000)	0.402*** (0.000)	0.402*** (0.000)	0.402*** (0.000)
*Potential Obligations * Lobbying Exp*			-0.002 (0.232)	-0.007*** (0.000)	-0.007*** (0.000)	-0.007*** (0.000)
*Long-Standing Contract * Lobbying Exp*			0.161*** (0.000)	0.126*** (0.004)	0.127*** (0.001)	0.127*** (0.004)
Firm FE	✓	✓	✓	✗	✗	✓
Contract FE	✓	✓	✗	✓	✗	✓
Firm-Contract FE	✗	✗	✗	✗	✓	✗
Time Fixed Effects	✓	✓	✓	✓	✓	✓
Observations	23,414	23,414	24,473	23,415	23,414	23,414
Adjusted R^2	0.433	0.434	0.363	0.485	0.486	0.486

Table III presents the results fixed effects results. Control variables are defined as in Table I. Fixed effect specifications vary as described. Intercepts are omitted. Standard errors are clustered at the firm-contract level. Time dummies are not tabulated. ***p<0.01, **p<0.05, *p<0.10

6 Robustness

6.1 Heckman Two-Step Selection Model

The robustness results are presented in Table IV. Column 1–4 present the multi-level fixed effects models with different sample sizes to check whether the results are driven by outliers. Columns 5–6 are the results of a Heckman two-step selection model, where Column 6 runs the same model adjusting for outliers in lobbying expenditure. In all the specifications lobbying remains a positive and significant determinant of contract funding decisions. Moreover, a contract's age remains a significant and positive secondary channel that is complementary to lobbying efforts. Therefore, the findings are not determined by outliers or a sample selection bias, under the modelling assumptions.

A Heckman selection model is employed to test the sample-selection problem, which arises since the decision to lobby is the result of an unobserved decision function, meaning that the treatment is not randomly-assigned. The Heckman two-step procedure uses results from the probit regression in Table II and is instrumented with state and county COVID cases.

1st Stage: $\mathbb{P}(\text{Lobby}_{i,c,t} = 1 | \mathbf{X_{i,c,t}}, \mathbf{D_t}) = \Phi(\beta_0 + \mathbf{X^T_{i,c,t}}\boldsymbol{\beta} + \mathbf{D_t}\boldsymbol{\delta})$

2nd Stage: $\ln(1 + \Delta\text{Contract}_{i,c,t}) = \alpha_1 \ln(1 + \text{LobbyExp}_{i,c,t-1}) + \mathbf{X^T_{i,c,t-1}}\boldsymbol{\beta} + \rho\lambda$

$$+ \alpha_2 \ln(1 + \text{PotentialObligations}_{i,c,t-1}) + \alpha_i + \alpha_c + \alpha_t + \varepsilon_{i,c,t}$$

In the second-stage λ is the estimated inverse Mill's ratio to account for the selection effect, defined as:

$$\lambda = \frac{\phi(\mathbf{X^T_{i,c,t-1}}\boldsymbol{\beta})}{\Phi(\mathbf{X^T_{i,c,t-1}}\boldsymbol{\beta})}$$

A common difficulty in the literature is finding reliable instruments. Any instrument must satisfy the relevance and exclusion restriction conditions (Wooldridge, 2018), meaning that they should be associated with the decision to lobby, though they should not directly affect the funding decision. In our context, there is no evidence of weak instruments; local COVID-19 cases are strongly associated with the lobbying decision. The exclusion restriction relies on a theoretical argument. I argue that the local case rate does not significantly affect a contract's funding because contracts are not administered as a form of relief. Whilst a firm in a high-COVID area may seek additional government aid through loans, contracts are a separate form of pandemic response, primarily distributed through efficiency considerations. Therefore, a policymaker should be impartial to the distribution of COVID-19 cases when making contract funding decisions. Admittedly, these instruments are not perfect, areas with high COVID-19 cases face greater supply chain issues, which has efficiency implications and, therefore, affect the policymaker's decision. However, since Wolfords & Siegels' (2019) meta-review of Heckman models finds that only a third of papers attempt to satisfy the exclusion restriction, validity of exogeneity is not critical, and this new instrument helps to assure robustness.

Importantly, the insignificant Mill's ratio suggests that there is no evidence of a sample selection bias taking place, which is a common finding when the sample consists of US public firms (Hill *et al*, 2013, Richter *et al*, 2009).

6.2 Further Robustness Checks

Another potential time-variant omitted variable is the total funding of the contract itself - firms may be more likely to lobby on larger contracts, and larger contracts also receive greater funding. Estimating the model both through controlling for previous total obligated funding and then again after redefining the dependent variable to measure the proportional change in the value of the contract did not alter the significance of findings. After controlling for total previous funding, lobbying still significantly increases the value of additional funding by 0.269%.

The findings are also robust to varying the cut-off point of the contract age dummy. For instance, contracts over 3 years old increase the lobbying return by 0.111%, and those over 10 years old increase the returns by 0.191%. I also re-estimate the model measuring the age effect as an interaction between lobbying expenditure and the number of years since the contract was issued. This specification still finds that lobbying returns increase with the age of a contract, with diminishing returns to age.

Assuming validity of the exclusion restriction, these robustness tests imply that the self-selection problem does not have a significant effect on the estimated effect of lobbying. Moreover, the significance of findings is robust to the choice of dependent variable, outliers, and alternative specifications of independent variables.

Table IV: Robustness Checks of the Returns to Lobbying

Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)
Lobbying Exp$_{i,c,t-1}$	0.327** (0.022)	0.322* (0.081)	0.344** (0.019)	0.522** (0.012)	0.321** (0.031)	0.300** (0.043)
Size$_{i,t-1}$	0.821* (0.069)	3.222** (0.022)	0.814* (0.071)	0.597 (0.148)	3.119*** (0.008)	3.842*** (0.001)
Employees$_{i,t-1}$	0.450** (0.034)	-2.049 (0.168)	0.444** (0.037)	0.389* (0.072)	-1.532 (0.248)	-1.383 (0.269)
Leverage$_{i,t-1}$	-0.416 (0.179)	-0.798 (0.219)	-0.560* (0.076)	-0.761** (0.029)	-0.696 (0.127)	-1.116** (0.013)
Free Cash Flow$_{i,t-1}$	-2.468 (0.107)	-3.097 (0.499)	-2.388 (0.118)	-2.684* (0.094)	-2.770 (0.411)	-0.766 (0.824)
State COVID Cases$_{c,t-1}$	0.215* (0.066)	-0.431 (0.821)	0.213* (0.091)	0.253* (0.065)		
County COVID Cases$_{c,t-1}$	0.169 (0.125)	0.169 (0.372)	0.212* (0.077)	0.150 (0.229)		
*Size * Lobbying Exp*	-0.023* (0.084)	-0.010 (0.563)	-0.024* (0.068)	-0.039** (0.047)	-0.010 (0.471)	-0.011 (0.439)
*Leverage * Lobbying Exp*	0.056*** (0.000)	0.0370* (0.078)	0.057*** (0.001)	0.026 (0.402)	0.039** (0.021)	0.056*** (0.002)
Potential Obligations$_{c,t-1}$	0.402*** (0.000)	0.448*** (0.000)	0.402*** (0.000)	0.397*** (0.000)	0.460*** (0.000)	0.449*** (0.000)
*Potential Obligations * Lobbying Exp*	-0.007*** (0.000)	-0.012*** (0.003)	-0.007*** (0.000)	-0.006*** (0.006)	-0.012*** (0.000)	-0.010*** (0.000)
*Long-Standing Contract * Lobbying Exp*	0.127*** (0.004)	0.103** (0.040)	0.156*** (0.000)	0.127* (0.052)	0.103*** (0.008)	0.128*** (0.001)
λ					1.275 (0.492)	1.610 (0.270)
Methodology	FE	FE	FE	FE	Heckman	Heckman
Sample	Full	Only Lobbyists	P90 – P10	P75 – P25	Full	P90 – P10
Observations	23,414	8,240	22,422	19,215	24,936	24,001
Adjusted R^2	0.486	0.482	0.483	0.477		

Table IV presents the results of robustness methodologies. Variables are defined as in Table I. All models include firm, contract and time fixed effects. Intercepts are omitted. Standard errors are clustered at the firm-contract level. The dependent variable is the natural logarithm of $1 plus the change in contract value in period t. Time dummies are not tabulated. P90–P10 omits the top and bottom 10% of non-zero lobbying expenditures. P75–P25 omits the top and bottom 25% of non-zero lobbying expenditures. λ is the estimated inverse Mill's ratio. Only the second-stage results are reported for the Heckman model, but first-stage results indicate strong instruments. ***p<0.01, **p< 0.05, *p<0.10

7 Conclusion

In conclusion, this paper presents new results explaining lobbying efforts and contract funding decisions, from which I draw three conclusions. Firstly, I find strong empirical support for a positive *causal* effect of lobbying the awarding agency of a contract on additional funding decisions, under the modelling assumptions. Secondly, contracts can be viewed as a political investment for the firm, which enhance their influence over policymakers and create additional barriers to entry that help to explain the mechanisms underlying Tullock's puzzle. Thirdly, I present determinants of lobbying and lobbying intensity that influence the optimal lobbying expenditure, such as a firm's total employees, assets, and leverage.

However, my design is not without limitations. Foremost, the data on lobbying remains imperfect, since we cannot directly observe specifically why firms lobby. Although lobbying expenditures are linked to the contract's awarding department, this does not mean that these expenditures relate to the contracts themselves. Consequently, the design inevitably contains a measurement error despite efforts to ensure that recorded lobbying expenditures are accurately recorded. Relatedly, lobby expenditures are recorded on a quarterly basis, and as such I have modified the contract funding data to fit the same frequency. However, there may be spill over effects where some lobbying expenditure occurs in the same quarter that the additional funding is received. Finally, a more complete approach would incorporate potentially important time-variant variables such as campaign donations. Whilst lobbying expenditures far outweigh campaign spending (Milyo et al, 2000), implying that the endogeneity arising from campaign donations should be comparatively small, and although my time period is short so political connections are approximately time-invariant, a more complete investigation of a firm's political influence would consider all channels.

Considering these results, the policy implications are not straightforward. Whilst one could use these results to contend that firms lobby to exploit government funding for their own benefit, and to the detriment of society, this has not been proven. Whether lobbying is an efficient transfer of information between knowledgeable firms and nescient policymakers, or a tool for first-world corruption is an open debate. In times of crisis, the implications of either argument being correct only increases, policy must be informed and socially responsible. Moreover, whilst the influence of firms appears to grow over time, this is also not necessarily inefficient. Although contracts become more fruitful to firms over time, they may also become more valuable to policymakers.

8 Appendix

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