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POLITICAL POWER AND MARKET POWER

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ABSTRACT

Brandeis (1914) hypothesized that firms with market power will also attempt to gain political power. To explore this hypothesis empirically, we combine data on mergers with data on lobbying expenditures and campaign contributions in the US from 1999 to 2017. We pursue two distinct empirical approaches: a panel event study and a differential exposure design. Both approaches indicate that mergers are followed by large and persistent increases in lobbying activity, both by individual firms and by industry trade associations. There is also weaker evidence for an association of mergers with campaign contributions (PACs). We also find that mergers impact the extensive margin of political activity, for example, by impacting companies' choice to establish their first in-house lobbying teams and/or first corporate PAC. We interpret these results within an oligopoly model augmented with endogenous regulation and lobbying.

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1 Introduction

Lobbying and campaign finance are essential elements of modern democracy (Ansolabehere et al., 2003; Cage, 2020; Grossman and Helpman, 1994). On the positive side, they can help elected officials gather information needed to make policy choices and can help voters become informed about candidates. However, they also raise legitimacy and fairness concerns, as agents with greater wealth can exercise greater influence over the political process.

In this paper, we study the link between political influence and industry concentration. This link is important for two reasons. First, businesses represent the largest source of lobbying spend. According to data from OpenSecrets, businesses accounted for 87 percent of total lobbying spending in the US in 2019 and 36 percent of contributions from Political Action Committees (PACs) in the 2017/18 political cycle (where labor and ideological contributions also contributed a big share).

Second, in recent years there has been rising concern that industrial concentration not only affects consumers directly through market power (potentially raising prices and reducing quantities), but also indirectly through politics (Wu, 2018; Zingales, 2017). Apprehension over the political influence of concentrated industries has appeared throughout the history of antitrust (e.g., Brandeis, 1914; Khan, 2017; Pitofsky, 1978).[1] Incumbent firms could lobby politicians to erect barriers to entry and protect their market power. This is another form of consumer harm, but one that flows through the channel of regulation. If lobbying exhibits economies of scale, a rise in market concentration should lead to an increase in lobbying activity. If this hypothesis is correct, market power begets political power.

To guide the empirical analysis (the core of this paper), we begin with a simple theoretical model capturing the relationship between market concentration and political influence. The model examines an oligopoly in which firms' profits may be affected by regulation. Firms engage in lobbying activity to influence their regulation using the menu auction model by Grossman and Helpman (1994).

We use our model to study how the political and product market equilibria change when two firms merge. A merger is a discrete event that leads to a change in concentration. We provide broad conditions for a merger to increase political influence activity. The intuition is that market competition within an industry partly dissipates the rents that accrue to firms from regulatory protection. By softening competitive pressure, a merger tends to increase the

[1] One example of this is Thomas Jefferson, who sought to add "freedom from monopolies" to the Bill of Rights in the U.S. Constitution (Jefferson, 1789).

incentive of firms to lobby for regulation. Our model generates predictions for the merging entities and for the industry as a whole. It also distinguishes between the impact of mergers both at the extensive margin (firms' choice to lobby at all) and the intensive margin (how much to lobby).

The core of the paper studies data spanning almost two decades, 1999-2017, and asks whether mergers are associated with an increase or a decrease in political influence activities. We examine SEC-registered companies, matching each company with data about both its federal lobbying and its campaign contributions in the US (both before and after mergers). Lobbying money is mostly spent to influence specific administrations and committees, whereas PACs are geared towards getting a party or a politician elected.

To investigate how political influence spending varies with a merger, we pursue two empirical approaches. In the first, we use a panel event study design (Athey and Imbens, 2022; De Chaisemartin and d'Haultfoeuille, 2020; Freyaldenhoven et al., 2021; Gentzkow et al., 2011; Goodman-Bacon, 2021). Qualitatively, identification in this approach relies on the idea that mergers are endogenous, but depend on fixed (or slow-moving) variables whose trends we control for. The identification assumption is that, after conditioning on all these other factors, mergers come from idiosyncratic shocks that are unrelated to the returns of political spending.

Our second research design is a differential exposure design (Borusyak and Hull, 2023; Breuer, 2022; Goldsmith-Pinkham et al., 2020) that uses a logic similar to the Bartik (1991) instrumental variable design. Like other Bartik-like designs, ours employs a combination of time-varying shocks and initial characteristics of companies that are exposed differentially to those shocks. For time-varying shocks, we use economy-wide pro-merger shocks, following the well-documented pattern of mergers arriving in waves (Gort, 1969; Nelson, 1959; Weston et al., 1990). These waves span multiple sectors and have several proposed causes ranging from macroeconomic shocks to technology shocks.

In both designs, our results suggest that mergers are positively associated with an increase in firms' spending on political influence activities. The average merger is associated with a \$70K to \$180K increase in the amount spent on lobbying per period (half year) after the merger, or approximately 15% to 35% of the average per-period spend of merging firms. The average merger is also associated with an approximately \$4K to \$10K increase in campaign contributions per period, but this association is not statistically significant in all specifications.

In particular, we link mergers to the extensive margin of influence – i.e., a firm's choice to establish political operations at all. At the beginning of our sample, only 8% of firms lobbied, and only 5% of firms had a corporate PAC (a vehicle for corporate campaign contributions).

During our sample period, the average merger is associated with a 1.5 to 2.1 percentage point increase in setting up an in-house lobbying operation for the first time in the company's history (at least since government lobbying records were kept). Merging is similarly associated with a 1.6 to 1.9 percentage point increase in initiating a corporate PAC. Once initiated, political operations are highly persistent. Following the establishment of an in-house lobbying operation, an average business lobbies in 87% of the remaining periods in our sample. Once a business sets up a PAC, the average PAC is active in 76% of remaining periods. Kerr et al. (2014) find similar results about persistence.

Across multiple specifications and outcomes, the association of mergers with influence activities is significantly stronger if the merging companies are larger, and if the merging companies belong to the same industry. Our results are consistent with the idea that lobbying scales with firm size. We find a similar positive association between mergers and political activity by the industry as a whole, and with the political spend of industry trade associations.

Finally, we pursue several robustness checks, highlighting two here. First, we consider a possible mis-specification problem. Merging firms may ramp up their influence activities *before* the merger, perhaps to increase the chance of the transaction being approved by regulatory authorities. However, we find little evidence in the data for such an anticipation effect. This null result may be a reflection of the fact that most mergers during our sample period were not scrutinized by US antitrust authorities (Wu, 2018).

Second, we measure whether firm-level *political risk* changes with mergers. Following a merger, firms may face more scrutiny from regulators if the merged entity becomes a politicized target of attack. The merged firm may increase lobbying, not because of rent dissipation and externalities (as in our theoretical framework), but because of a new adversarial environment. Hassan et al. (2019) develop methods for quantifying firm-level political risk based on the contents of quarterly earnings conference calls. Using this data, we find no evidence of higher political risk after a merger.

1.1 Related Research

The main contribution of our paper is to investigate the empirical association between mergers and political influence activities. It relates to three strands of literature.

Empirical Studies of Special Interest Politics. Our analysis is related to a small but growing set of empirical studies linking industry-level variables with lobbying activities.[2] The pioneering work in the area is Goldberg and Maggi (1999), which tests and estimates Grossman and Helpman's 1994 model with industry-level US data on lobbying and tariffs. Prior research suggests that politically well-connected firms enjoy higher stock returns (Cooper et al., 2010), are more likely to receive government assistance (Duchin and Sosyura, 2012; Faccio et al., 2006), and avoid enforcement against them (Correia, 2014; Lambert, 2019).

Some works have explored the connection between firm size and political influence. Bombardini (2008) shows theoretically that industries with a higher share of firms above a given size should lobby more. The prediction is borne out by the data. This in turn helps explain trade tariff patterns across industries. Bombardini and Trebbi (2012) show that industry concentration affects the mode of lobbying. In more oligopolistic sectors, firms are more likely to lobby individually, while in more competitive sectors they are more likely to use trade associations. McCarty and Shahshahani (2023) study firm lobbying to perform a "primarily descriptive" (p. 1174) analysis. They fail to find a strong relationship between economic concentration and the concentration of lobbying expenditure at the industry level. Our paper contributes to this literature by providing a different approach based on sudden, discrete changes in economic concentration due to mergers.

A set of recent related papers study how lobbying tries to influence trade agreements (e.g. Blanga-Gubbay et al., 2021). Bombardini et al. (2021) study lobbying in the US as a consequence of imports from China, showing differential responses between firms on the technological frontier and laggards. Bertrand et al. (2020) study the effect of the identity of a firm's shareholders on the patterns of campaign contribution of that firm. The probability that a firm's PAC donates to a politician supported by an investor's PAC doubles after the investor acquires a large stake. Like ours, their study uses changes within the same firm over time (in their case, changes to ownership).

A series of recent empirical papers documents increasing firm mark-ups, higher aggregate industry concentration, a decline in the labor share of output, larger firm and income inequality, and a reduction in business dynamism over the past few decades (Dube et al., 2020; De Loecker et al., 2020; Philippon, 2019). Showalter (2021) shows these trends were concurrent with increases in lobbying. Our paper aims to connect lobbying and concentration more directly, both using a theoretical model of lobbying and concentration, as well as through empirical evidence linking concentration and political influence. Our empirics are particularly

[2] For a survey of the empirical literature on lobbying see Bombardini and Trebbi (2020).

related to the political economy of antitrust. Mehta et al. (2020) and Fidrmuc et al. (2018) measure political interference in the antitrust review process from members of Congress and corporations. By contrast, we focus on the impact that merger policy can have on lobbying for regulation more generally.

Theories of Political Influence. We also contribute a novel political economy model of the relationship between political outcomes and marketplace dynamics. This topic has been the focus of many researchers outside of finance (e.g., Brandeis, 1914; Khan, 2017; Pitofsky, 1978; Wu, 2018, and others). Within economics, models by Hillman (1982); Stigler (1971); Tullock (1967) formalize early ideas of regulation as a function of industry influence. We follow that literature in using Grossman and Helpman's 1994 model as the basis for our theoretical approach. Huneeus and Kim (2018) study the relationship between firm size and lobbying, and the resulting misallocation of firm resources.

Callander et al. (2022) develop an integrated dynamic model of competition, innovation, and policy-making. They show the existence of a feedback loop between market power and political power. In equilibrium, the policy-maker "manages competition" to protect the incumbent, resulting in less competition and innovation.

Our main theoretical contribution consists in combining a lobbying model with a standard oligopolistic competition model, which allows us to make predictions on the effect of mergers on equilibrium lobbying activity. We derive results on the intensive margin (change in lobbying activity for firms that were already lobbying before the merger), on the extensive margin (probability that a non-lobbying firm starts lobbying after the merger), and on the whole industry (for example, via an industry trade association).

Mergers. Finally, we contribute to the study of mergers and acquisitions. From a firm's perspective, our results speak to a novel type of merger benefit: "non-market synergies" such as coordinated activity in government affairs (Baron, 1995; Feldman and Hernandez, 2021). Our theory model shows an example of a non-market strategy (lobbying to erect regulatory barriers to entry) complementing a marketplace strategy (merging and coordinating prices and quantities in product markets).

Since we study firms before and after a merger, our empirical approach follows the literature in financial economics by examining a bundle of firms as a single unit (including, e.g., *both* the target and the acquirer in an acquisition), and measuring the bundle's aggregate characteristics over time (e.g., Bradley et al., 1988; Cuñat et al., 2020). We build on this literature, and

extend it to the case of multi-firm mergers, which is relevant in our empirical context. Because of the abundance of multi-merger firms, defining these bundles (and measuring their internal structure over time) involves tracking multiple layers of nested acquisitions. As mentioned earlier, one of our research designs employs a differential exposure design, using a logic similar to the Bartik (1991) instrument. Similar Bartik-like designs have been deployed to study local labor effects of Chinese trade (David et al., 2013), native/immigrant substitution (Card, 2009), and credit shocks during the Great Recession (Greenstone et al., 2020). We propose and implement an adaptation of this strategy to examine merging firms.

While closely aligned with many of the works above, to our knowledge, ours is the first paper that tries to link, both theoretically and empirically, industrial concentration induced by mergers in the whole of the U.S. economy with lobbying activities and PAC spending.

The next section presents our theory, and Sections 3 and 4 provide an overview of our empirical approach and data. Sections 5 through 7 present our empirical strategies and results, and Section 8 concludes.

2 Theory

Our aim in this section is to provide a framework to think about how political influence activity is affected by mergers within an industry. We present a simple model of lobbying and competition. The model is composed of two building blocks: an industrial organization model of oligopoly with regulation, and a political economy model of lobbying for regulation. The model proceeds in two stages: firms first play the political economy game, making transfers to a regulator who chooses a policy that shapes the market. Second, firms compete against each other given the level of regulation decided in the first stage. We detail each stage below.

We first establish a baseline model of the simplest possible setting: an initial duopoly, to be assessed against a merger into a monopoly. Our aim is to study how political influence activity is affected by a merger. We then extend the baseline by requiring firms to incur fixed costs, to be paid before lobbying can begin, in order to understand which firms are more likely to lobby in the first place. This allows to shed light on the extensive margin of the lobbying decision. Finally we discuss how to generalize our model from two firms to n firms and examine lobbying by the industry as a whole (e.g., including trade associations). These results provide useful guidance on how to interpret and analyze data available to us. Proofs are in Appendix A.

2.1 Preliminaries

Competition. We begin with the industrial organization block consisting of a standard quantity competition model *augmented with regulatory variables*. We consider an industry with 2 firms. Each firm $i = 1, 2$ can set its own quantity q_i, as well as lobby for some regulation R. The resulting demand is assumed to be linear and equal to

$$P = A + R - Q,$$

where $Q = \sum_{i=1}^{2} q_i$ represents the total quantity produced by the firms and $A > 1$ is a parameter that proxies for market size. Assume a marginal cost, identical for each firm, normalized to 1.[3] The profit of a firm is thus

$$\pi_i = (A + R - Q)\, q_i - q_i.$$

In the absence of lobbying, this would be a standard Cournot model which we have augmented with regulation. R represents the effect of regulation on demand for the incumbents' products. We can think of $R \in \Re$ as government policy that favors the incumbents in the industry. For instance, R can be thought of as the result of an additional cost τ imposed on a competing product that *could* be sold in the industry. This applies, e.g., to at least two well-studied forms of regulation. First, the alternative product could come from the international competition and the cost τ is an import tax, as studied in the tariff lobbying of Grossman and Helpman (1994). Second, the alternative product could be a different set of domestic producers and τ would be a barrier to entry (either explicit or implicit).

By lobbying for R, the incumbents can fend off entry from these competitors by making τ sufficiently high. At first sight, R may appear to be similar to an investment in R&D or advertising that increases demand for a product. However, R is set by the regulator in the lobbying game (discussed next), rather than by each firm non-cooperatively (as in standard R&D or advertising games). In addition to the market activities above, each firm engages in non-market activities (lobbying) by making a transfer $\hat{t}_i$ to a regulator to influence R. We now turn to the lobbying block of our model.

Lobbying. The lobbying block follows the canonical lobbying model of Grossman and Helpman (1994), which in turn is based on the menu auctions studied by Bernheim and Whinston

[3] We use a Cournot setting as it results in the simplest analytical expressions one can obtain.

(1986). In Grossman and Helpman (1994), a regulator chooses the policy variable R.[4] The regulator has preferences over the choice of policy. We call $\overline{R}$ the policymaker's preferred policy in the absence of any lobbying. A common interpretation of this is that $\overline{R}$ is optimal for society more generally, and the regulator places some weight on social welfare.

The following direct preference function governs how the regulator assesses deviations from the optimal policy $\overline{R}$

$$w(R) = -w\frac{\left(R-\overline{R}\right)^2}{2}. \tag{1}$$

This is essentially a "quadratic loss" from the optimal policy $\overline{R}$. We assume the preferred policy (absent lobbying) is normalized to $\overline{R} = 0$ (no regulation).[5] The w coefficient in Equation (1) captures the cost of deviating from the optimal policy.[6]

The policymaker also cares about firms' lobbying efforts. These lobbying efforts $\hat{t}_i$ can be interpreted as bribes, campaign contributions, informational benefits, etc. given to the regulator by firm i. Following Grossman and Helpman (1994), the regulator chooses R to maximize

$$\sum_{i=1}^{2} \hat{t}_i + w\left(R\right),$$

where $w(R)$ is the policy maker's policy preference (1), and $\sum_i \hat{t}_i$ represents the total lobbying efforts. We can adapt to our setting the following central result from Bernheim and Whinston (1986) to study the main outcome in this paper: the equilibrium amount of lobbying (an observable outcome in our dataset).

Theorem 1 (Bernheim and Whinston)**.** *With $n = 2$, in any coalition-proof equilibrium of this lobbying game,*

(i) The policy maker selects

$$R^* \in \arg\max_R \sum_{i=1}^{2} \pi_i\left(R\right) + w\left(R\right)$$

[4]The model can be extended to a multi-dimensional policy vector.

[5]Assuming that $\overline{R} = 0$ is without loss of generality. If the optimal policy was, say $R^* \neq 0$, we could redefine $\tilde{R} = R - R^*$ and redefine as well the fixed component of demand $\tilde{A} = A + R^*$.

[6]We assume this coefficient is large enough to produce an interior solution for the policy choice. As it will become apparent below, in our setting a sufficient condition is $w > 1/2$.

(ii) The lobbying efforts must satisfy

$$\begin{aligned}\hat{t}_1 &\geq \pi_2(R^*_{\{2\}}) + w(R^*_{\{2\}}) - (\pi_2(R^*) + w(R^*))\\ \hat{t}_2 &\geq \pi_1(R^*_{\{1\}}) + w(R^*_{\{1\}}) - (\pi_1(R^*) + w(R^*))\\ \hat{t}_1 + \hat{t}_2 &\geq \max_R w(R) - w(R^*)\end{aligned}$$

where $R^*_{-I} \in \arg\max_R \sum_{j\notin I} \pi_j(R) + w(R)$.

Theorem 1 states that the regulator chooses the policy R that maximizes a weighted average of industry profits and policy utility (1) (we assumed equal weights). Moreover, each firm's *transfers* are dictated by what the regulator could do in the alternative coalitions without them.

Having established the setup of the model, we can now proceed to our baseline model.

2.2 Baseline: Pre-Merger Equilibrium

The baseline model features two (unmerged) firms playing the lobbying and market games sequentially. At time $t = 1$, firms play the lobbying game, where both the policy and the transfers are determined. At time $t = 2$, firms play the competition game, when quantities are set. To solve the game, we proceed backwards.

In the second stage, standard calculations show that equilibrium firm profit and total industry profit are respectively

$$\pi_i = \frac{(A+R-1)^2}{9}, \qquad \text{and} \qquad \Pi = \frac{2(A+R-1)^2}{9}. \tag{2}$$

In the first stage, the policy maker selects R to maximize

$$\Pi - w\frac{R^2}{2},$$

with an interior solution resulting from

$$R^* = \frac{1}{w}\frac{d}{dR}\Pi.$$

The explicit solution in our case is

$$R^* = \frac{4(A-1)}{9w-4}.$$ [7]

Turning to lobbying spending, simple computations show that the constraint on the grand coalition made by both firms is binding

$$T = \sum_{i=1}^{2} \hat{t}_i = w\frac{R^{*2}}{2} = \frac{8w(A-1)^2}{(9w-4)^2}.$$

Total transfers therefore reflect the policy that maximizes the sum of firm profits and the regulator's policy preferences. The comparative statics around total lobbying are sensible: regulation and transfers are higher the larger the affected market (high A), and the cheaper the social cost (low w). Notice that transfers are convex in market size A. In a symmetric equilibrium, it is

$$\hat{t}_1 = \hat{t}_2 = \frac{4w(A-1)^2}{(9w-4)^2}.$$

2.3 The Consequences of a Merger

Imagine now the two firms merge to a monopoly. What is the effect on lobbying activity?

The profit of the merged firm, denoted as 12, is $\pi_{12} = \pi_1 + \pi_2 = P(q_1+q_2) - (q_1+q_2) = (P-1)Q$ with resulting equilibrium profits at $t=2$ given by

$$\pi_{12} = \frac{(A+R-1)^2}{4}.$$

Turning to the lobbying game at $t=1$, the policy maker selects the policy to maximize

$$\frac{(A+R-1)^2}{4} - w\frac{R^2}{2},$$

with an interior solution

$$R_{12}^* = \frac{A-1}{2w-1}.$$

[7] Footnote 6 stated our assumption that the w coefficient is large enough to produce an interior solution.

The lobbying spending needs to compensate the regulator for the social loss

$$\hat{t}_{12} = w\frac{{R_{12}^*}^2}{2} = \frac{w(A-1)^2}{2(2w-1)^2}.$$

Comparing with the results from the previous section (no merger), the effect of a merger on the policy is positive because

$$R_{12}^* - R^* = \frac{w(A-1)}{(9w-4)(2w-1)} > 0.$$

The effect of a merger on total lobbying transfers is positive because if $R_{12}^* > R^*$, then also

$$w\frac{{R_{12}^*}^2}{2} > w\frac{R^{*2}}{2}.$$

This leads to:

Proposition 1 (Mergers Increase Lobbying)**.** *A merger between two firms increases equilibrium lobbying effort and equilibrium regulation.*

The result comes from the fact that a merger increases the *marginal value of lobbying*. In a duopoly, the rents generated by an increase in regulation R are partly dissipated by competition between the firms. A merger reduces the number of competitors and leads them to take into account the price externality they impose on each other. This in turn makes regulation R more beneficial to firms and induces them to invest more in lobbying effort.

We highlight that Proposition 1 is *not* driven by a change in firm size. When two firms merge, the resulting firm is likely to be of similar size as the combined firms. Instead, what drives our theoretical results is the impact on the marginal returns from lobbying. These increase sharply after the merger. They increase because rents from lobbying are less dissipated after the merger, which happens because product market competition lowers after the merger. Although size could be very sticky, the merger still generates higher returns not by changing sizes, but through diminished rent dissipation.

There could be additional reasons why mergers lead to more lobbying activity, which are not captured by Proposition 1. In Grossman and Helpman's equilibrium, the level of regulation is set at the efficient level from the perspective of firms (as the regulator considers

the total profits of the industry), and there is no mis-coordination, as it is provided by the policy-maker. However, one could imagine other models where some mis-coordination occurs around funding (perhaps because of asymmetric information). In this case, Proposition 1 would hold for an additional reason: the merger eliminates mis-coordination.

Proposition 1 relies on our assumption that regulation is beneficial to the incumbents. All firms have an interest in higher levels of regulation. However, the model could be extended to other types of industry regulation, which could generate results in the opposite direction. In some settings, regulation could imply negative externalities for some firms. For example, regulation could divide competitors by helping some at the expense of others. This would apply, e.g., when a market leader lobbies for regulations to protect its position, while a challenger opposes the regulations (and/or prefers others). Should the incumbent merge with the challenger, this form of rivalrous lobbying would diminish (this case is covered in our earlier working paper, Cowgill et al. 2022). This possible ambiguity calls for an empirical approach.

2.4 Fixed Cost of Lobbying

A majority of US firms spend zero dollars on lobbying activities. A merger could potentially affect this choice to lobby at all —i.e., the *extensive margin*. This is distinct from the *intensive margin* of lobbying, or the intensity of lobbying among those who have chosen to participate. Extensive margin changes are visible in our data when a firm establishes in-house lobbying or a corporate PAC for the first time.

We now introduce extensive margin choices into our model, and show how they are affected by merging. To model these choices, we require firms to incur a fixed set-up cost to begin lobbying. Prior work by Bombardini's 2008 and Kerr et al. (2014) suggests that up-front costs are an important component of business lobbying.

Formalization. To incorporate set-up costs, we add an initial stage. At $t = 0$, each firm independently decides whether to pay a set-up cost F. To begin lobbying, the firm must incur this fixed cost F. Then, at stage $t = 1$, only the firms that have paid set-up costs can engage in the lobbying game, and market competition proceeds afterwards. The level of F is exogenous,

and we will derive thresholds of F under which paying the cost is profitable. We model the decision at $t = 0$ as a choice each firm makes individually, in contrast with the transfers and policy that are decided jointly with the regulator.

The fixed cost F can be thought of as setting up a public policy department, and hiring staff with the necessary relationships and skills. In our data, we observe lobbying by "in house" lobbyists separately, in addition to "outsourced" lobbying through third-party agencies (so-called "K-Street" agencies for-hire). Insofar as outsourcing involves lower startup costs, our model results should be particularly relevant for in-house lobbying. In our empirical section, we will study both in-house and outsourced lobbying.

Results. We now show the effect of a merger on firms' choice to lobby. We first characterize the equilibrium in the absence of a merger. Which firm(s) lobby at all in the presence of fixed lobbying costs?

Proposition 2 (Extensive Margins without Merging)**.** *Imagine each firm needs to spend F in order to lobby. There are thresholds $k_2 < k_1$ such that*

- *If $F/(A-1)^2 \leq k_2$ there is lobbying, with both firms lobbying;*
- *If $k_2 \leq F/(A-1)^2 < k_1$ there is lobbying, with only one firm lobbying;*
- *If $F/(A-1)^2 > k_1$ there is no lobbying.*

The result shows that, when lobbying involves a fixed cost, lobbying should be observed in those industries that are *large enough* (high A) compared to the set-up cost F. By contrast, firms in more niche industries will find it too costly to spend the fixed costs.

How does a merger change these results?

Proposition 3 (Extensive Margin with Merging)**.** *For a given market size, merged firms can justify paying higher set-up costs for lobbying than without the merger.*

Intuitively, if firms merge, the profitability of lobbying can justify set-up costs, even if they are relatively high. Without a merger, set-up costs can be justified only if they are inexpensive.

To conclude, our results summarized by Proposition 1 and 3 show that a merger increases lobbying both at the intensive margin *and* at the extensive margin.

2.5 Discussion with multiple firms and industry analysis

Proposition 1 shows that a merger to monopoly increases overall lobbying activity. However, industries are typically made up of a much higher number of firms. With multiple firms, outsiders to a merger may react differently from insiders. Political influence activity could come both from merging and non-merging firms, or from industry-level trade associations. Conceivably, a merger could affect the balance between individual and collective lobbying. Our data allow us to consider all these types of lobbying. Hence, it is of interest to consider how a whole industry reacts to the change induced by a merger.

In Appendix A.3, we discuss how to adapt our mechanisms to a n-firm industry. The main result that regulation increases after a merger carries forward. For intuition, consider a very large number of firms: all rents generated by R would be dissipated because competition would lower profits to zero. Hence there would be no lobbying. But as the industry becomes more oligopolistic, the marginal value of lobbying increases, and hence firms invest more in lobbying efforts. When it comes to the whole industry (considering merging parties, outsiders to the merger, and industry trade associations), we also show that collective lobbying effort is expected to increase after a merger. These effects are more sizable when mergers happen in concentrated industries, and more muted when industries are more fragmented.

We also study mergers asymmetrically, distinguishing between mergers involving "large" or "small" firms (Appendix A.3). We show how lobbying effects are diluted in the latter type of mergers. Effects are instead more substantial for mergers that involve larger firms, compared to mergers among firms that are smaller to begin with, because mergers between larger firms create a larger increase in market power.

3 Empirical Overview

We now turn to measuring these ideas in a large sample of real companies. We examine publicly-listed firms in the period 1999-2017 and their influence activity on the U.S. federal government. Part of our contribution is to document the increase in political activity following mergers. Mergers are highly strategic and non-random, and M&A endogeneity is a

longstanding challenge in prior empirical literature. We hope to attenuate these endogeneity concerns as much as possible through a variety of carefully-executed strategies. In this section we lay out the broad specifications and preview our empirical strategies.

3.1 Data Structure: Composite Firms

Our approach uses a unit of analysis called a *composite firm*. Composite firms are clusters of one or more firms that eventually merge together. For each *component* firm (original, underlying firms), we can identify its *composite* firm at the beginning of the sample (before the merger takes place). We can link each firm to a composite firm (and sibling firms) for all periods in the sample, and leverage *within-composite firm variation* over time. Composite firms do not exist in standard merger databases, but can be assembled from datasets about mergers and their timing.

Appendix B presents a visualization of a simple multi-merger composite firm as a graph, and how we represent this firm in regression-friendly panel data. Using the composite firm graph, we can observe the evolution of each composite at every point in our sample — including when the underlying component firms are independent, while they merge, and after they are completely unified.

The composite firm representation is particularly helpful in analyzing multi-merger firms. Mergers are relatively rare. However, among companies that *do* merge with others in our sample, 42% are involved in multiple mergers or acquisitions.[8] Multi-merger firms are especially common among larger companies that may be the source of important political and/or economic influence. Composite firms with more than two components comprise 58% of all lobbying spending.[9] Such firms are often both targets and acquirers in the same sample. Appendix C describes why multi-merger firms present research design challenges, and how the composite firm representation addresses those challenges.[10]

Our sample includes around 12K composites. These 12K composites are made from over

[8] This number rises to 68% if unlisted companies are included.
[9] This number rises to 83% if unlisted companies are included.
[10] Our framework also accommodates spin-offs, breakups, or other events that increase the number of separate component firms within the sample.

15K *component* firms in our original Compustat sample. Each of the 15K component firms has exactly one composite parent into which it is eventually merged. Many component firms never merge with any others; their composite parent is (essentially) itself. Using this panel of composite firms, we execute multiple research designs, all focused on the timing of mergers.

3.2 Regression Equations

Our results come from estimating two panel specifications. The first looks at the impact of a merger on the merging parties, while the second considers the whole industry. The first examines a panel of composite firms. Our regression equation is

$$\sum_{f \in \mathcal{F}_i} y_{ft} = \beta_0 + \beta_1 \text{MergerIndex}_{it} + \beta_2 X_{it} + \delta_i + \gamma_t + \epsilon_{it}. \tag{3}$$

The left-hand side $\sum_{f \in \mathcal{F}_i} y_{ft}$ represents the sum of political activity y_{ft} over all component firms within composite firm i at time t.[11] The coefficient of interest is β_1, the coefficient on the MergerIndex_{it}. In our main specification, we examine a simple count of the number of component firms within each composite firm i at time t. This decreases each time a merger occurs, and allows β_1 to be interpretable as the effect of a merger. Because a merger corresponds to a *decrease* in the number of firms within the bundle, a negative coefficient means that political spending *increased* after the merger.[12]

We include a set of X_{it} controls in our specification. The first of these controls that appears in all specifications in the paper is size. Because lobbying can scale with size, all regressions control for the total size of the composite firm. We use revenue as a proxy for size. For each composite firm i in time t, we sum the total revenue across all component firms (including those yet to merge), that is, $\sum_{f \in \mathcal{F}_i} \text{Size}_{ft}$. This measure of the aggregate size of each composite firm could be sticky in practice; when two firms merge, the resulting firm is likely to be of similar size as the combined firms. Yet, in line with our theory, the market rents will change after the merger, possibly leading to a change in the incentives to lobby.

[11] y_{ft} represents political influence spending of component firm f at time t. $\mathcal{F}_i$ denotes the composite firm i to which component firm f belongs.

[12] Equation 3 also admits other measures of concentration for MergerIndex_{it} besides the number of component firms. In the Appendices, we use as an alternative measures of concentration the Herfindahl-Hirschman Index of the composite firm (HHI)). Empirical results are qualitatively similar (Appendix M).

We also include fixed effects for composite firms (δ_i) and time periods (γ_t) in all results. We choose the other control variables in coordination with our research designs in our later sections. These include trends by industry and other firm characteristics found in Section 5. In general, we use controls to address possible threats to exogeneity, and sometimes to increase the precision of our main estimates. We also show results both with and without controls as checks on the robustness of our findings (Altonji et al., 2005; Oster, 2019). Standard errors are clustered by composite firm.

Outcome Variables. Under the assumptions of our research designs stated in Sections 5 and 6 below, we use this specification to measure the effects of merging on a variety of outcomes. We examine two measures of political influence: Federal lobbying spending and donations from PACs. These measures are analyzed separately, as different outcomes, and are described in detail in our data section (Section 4). For each outcome variable, we study both total dollar amounts as well as the first instance of each type of political spend.

To study amounts, we present results in levels, so they represent absolute dollar increases. The levels specification aims to express the economic significance of the results in units that are transparent and accessible to readers. Alternatively, one could employ a specification in logs, capturing the idea that merger effects could be proportional to the pre-merger level of lobbying. A logarithmic transformation is arguably not particularly indicated for our data (where we know from the descriptive statistics that many firms do not lobby or merge at all). Still, we do consider logs in Section 7.1. Reassuringly, we find qualitatively similar results across both logs and levels specifications.

Extensive Margins. In light of our theory model, we are also interested in the extensive margin of political activity (i.e., the first occurrence of political activity in the history of the composite firm). We create binary variables that begin as zero, and become one the first time that any component firm lobbies (through in-house lobbyists or at all) or contributes through a corporate PAC, and use these as outcomes variables in Equation 3.

Our specification uses the non-merging firms (and pre-periods) as controls. However, non-merging firms may also change their spend in reaction to the merger. This is part of our

motivation for our next specification.

Industry Panel Regressions. Our industry-level regressions are identical to Equation (3), but the cross-sectional unit is different. Rather than studying a panel of composite firms, we study a panel of industries. $\sum_{f \in \mathcal{F}_i} y_{ft}$ describes the sum of all lobbying of all component firms in industry i at time t.[13] We include fixed effects for industries (δ_i) and time periods (γ_t). Standard errors are clustered by industry. For this portion of the analysis, each composite firm is assigned to a single industry classification for the entire sample.[14] As we describe in the data section, we use hand-coded OpenSecrets industry classifications. This allows us to study the effects of mergers on every firm in the industry (both merging and non-merging parties). We also develop a method to identify trade associations and other collective lobbying organizations at the industry level, and we measure the effects of lobbying on their political spend.

Because of the level of aggregation, the sample size decreases dramatically. However, industry-level regressions help measure potentially important effects. We can measure whether total spend increases (including non-merging firms) along with mergers, and whether industry association groups increase their federal lobbying and campaign finance spend.

As in our first specification, the coefficient of interest is on the MergerIndex_{it}. Following the above, we implement a simple count of the number of component firms within each industry firm i at time t.

Research Designs. Because mergers are endogenous, we examine several different empirical approaches. Although causal identification about mergers is difficult, we pursue two approaches. The first is a panel event study (Athey and Imbens, 2022; De Chaisemartin and d'Haultfoeuille, 2020; Freyaldenhoven et al., 2021; Gentzkow et al., 2011; Goodman-Bacon, 2021). The second is an exposure design (Bartik, 1991; Borusyak and Hull, 2023; Breuer, 2022; Goldsmith-Pinkham et al., 2020). In this second approach, we develop an instrument for

[13] y_{ft} still represents political influence spending of component firm f at time t. $\mathcal{F}_i$ now represents the industry partition for an industry i.

[14] In any period where a composite firm contains unmerged entities from more than one industry, we select the industry where revenues are higher.

MergerIndex_{it}. In both cases, we present our results not as one-size-fits-all lobbying effects, but rather as an average of heterogeneous effects that likely vary across firms. Both designs are based on the timing of mergers. In order to explain our designs, we first describe the structure and sources of our data in the next section.

4 Data and Descriptive Statistics

Our study of public firms from 1999-2017 combines data from four sources. This section describes these sources and summarizes the key properties of our data. In Appendix D, we describe the data in more detail, including how they are merged.

4.1 Sample

Our underlying sample consists of all firms present in the Compustat database from 1999 to 2017. This includes publicly traded companies as well as private companies that are large enough to publicly disclose financial statements. We study M&As among these firms. As discussed above, our empirical strategy requires pre-merger size data for all component firms. We use Compustat to obtain a sample of firms and key firm financial data, including size (revenue) and industry (NAICS). Some of these Compustat firms merged with non-Compustat firms over our sample, but we are not using these mergers to make inferences. This sample is similar to those used in other studies of mergers between public firms.[15]

The sample dates are affected by the availability of political influence data. Detailed data on federal lobbying began only in 1999 following the Lobbying Disclosure Act ("LDA") of 1995. LDA reports are required only once every half-year. As a result, half-years are the temporal unit of our panel, and we summarize all variables at the half-year level.[16] We include all firms that are available in Compustat for each half-year.

[15] See, for example, Gaspar et al. (2005), Harford et al. (2011), Bena and Li (2014).

[16] In 2007, a new disclosure law was passed ("The Honest Leadership and Open Government Act") requiring that lobbying disclosures take place twice as often (quarterly). Nonetheless, we continue our analysis on a half-year basis for consistency.

4.2 Merger Data and the Composite Firm Graph

Our composite firm database uses Thompson Reuters' SDC Platinum database of acquisitions and mergers. SDC Platinum contains the universe of global M&A transactions and is used in academic papers about M&As (Matvos and Ostrovsky, 2008; Rossi and Volpin, 2004).[17] For each acquisition, SDC Platinum identifies the acquirer, target and dates associated with the merger. The date variables are particularly important in the analysis as they allow us to use pre-/post- variation in merger status. We use the completion date of each merger as the basis for when a merger happens, as the completion of the merger is when the merging parties are unambiguously able and incentivized to begin cooperating.

Using the methods described in Appendix D, we produce the composite firm graph. Once calculated, we then track the evolution of composite firms over time. The key output from this procedure is a panel dataset with a MergerIndex_{it} variable for each composite firm i at time t. Our simplest measure of concentration is a count of the number of intermediate firms that still remain un-merged within each composite i at each time t. This variable consists of integers that decrease by 1 with each successive merger. For this variable, negative coefficients mean that the outcome variable increased as concentration increased with the merger.

4.3 Political Influence Data

Federal lobbying data comes from *LobbyView*,[18] an NSF-funded project compiling federal lobbying data (Kim, 2017, 2018). *LobbyView* contains disclosures for in-house lobbyists as well as lobbying performed by external firms hired by each company. Lobbying firms are required to identify their clients in these disclosures, so we can sum each company's in-house and outsourced lobbying. We also use data from the Center for Responsive Politics' *OpenSecrets* project about campaign contributions from corporate PACs.[19]

[17] Barnes et al. (2014) audit the accuracy and completeness of the SDC Platinum database and find positive results, particularly for the sample dates and for large companies that we analyze in this paper. Bollaert and Delanghe (2015) evaluate other sources of merger data, including Zephyr (`https://zephyr.bvdinfo.com/`) and also find positive results for SDC.

[18] `https://www.lobbyview.org/`

[19] `https://www.opensecrets.org/bulk-data/`; other papers using this data include Bertrand et al. (2014); Blanes i Vidal et al. (2012).

Both datasets include each company's own lobbying and campaign funds, but do not include funds sent from a company through an intermediary organization such as an industry association.[20] We describe our data about industry associations in the next section, but we cannot link this back to individual companies (except through broad categories).

4.4 Industry Trade Association Data

Our final dataset includes lobbying expenditures and campaign contributions by identifiable trade associations. To our knowledge, no such dataset exists in prior literature. To assemble it, we match the names of each lobbying and donation group in the OpenSecrets data against a set of known trade associations. By matching against a known list, we can separate trade associations from other organizations within an industry (such as smaller, non-public companies). Appendix D contains more detail about our procedure for gathering this data.

Our approach delivers a set of industry and trade associations, each with an industry identifier that uses the hand-coded OpenSecrets industry classification system. Our sample includes 60 industries in the OpenSecrets classification system.[21] When necessary, we map our trade association data to other industry classifiers using a crosswalk file developed by users of the OpenSecrets database.[22]

4.5 Summary Statistics

Table 1 and 2 display summary statistics about our composite firms. Five broad patterns emerge from the data. Although some of these patterns have been documented elsewhere in the literature (e.g., Ansolabehere et al., 2003), we mention these to set the context of our empirical application.

1. **Mergers among public companies are not uncommon**. 45% of composite firms have been involved in a merger, although most of these mergers are acquisitions of small, unlisted

[20] In our data, the intermediary's lobbying would be attributed to the intermediary. It cannot be traced back to the originating company/donor. This issue affects all research that uses lobbying data from the disclosure laws.

[21] The industry categorizations are visible at this URL. In total there are approximately 100 industries, but some industries have no constituency in our Compustat sample of (mostly) public firms.

[22] https://groups.google.com/g/opensecrets-open-data/c/nXYSeFrtwxk/m/NXRoVQhoBwAJ

Table 1: **Descriptive Statistics: All Composite Firms**

	Mean	Std.Dev	Min	P25	P50	P75	Max
Years in Sample	8.78	6.44	0.50	3.00	6.50	14.50	19
Avg Revenue ($10M, per Half Year)	62.95	392.77	0	0.01	1.87	16.15	18,359
Lobby Spend ($1K, per Half Year)	54.09	558.41	0	0	0	0	40,365
Lobbied at all (per Half Year)	0.08	0.23	0	0	0	0	1
In-House Lobby Spend ($1K, per Half Year)	36.33	458.92	0	0	0	0	37,828
Lobbying Intermediary Spend ($1K, per Half Year)	17.76	139.79	0	0	0	0	7,182
Lobbied at all (ever)	0.16	0.37	0	0	0	0	1
PAC Donations ($1K, per Half Year)	2.26	25.03	-0.12	0	0	0	1,903
PAC Donations$>$ 0 (per Half Year)	0.05	0.19	0	0	0	0	1
PAC Donations$>$ 0 (Ever)	0.08	0.27	0	0	0	0	1
Individual Donations ($1K, per Half Year)	0.59	3.92	-1.75	0	0	0.01	157
Individual Donations$>$ 0 (per Half Year)	0.06	0.14	0	0	0	0.05	1
Individual Donations$>$ 0 (Ever)	0.29	0.45	0	0	0	1	1
Ever M&A	0.10	0.31	0	0	0	0	1
# of Component Firms	1.24	1.23	1	1	1	1	39

Notes: This table displays simple summary statistics for all composite firms and all periods in our sample.

companies. 10% of our composite companies feature a merger between Compustat-listed companies.

2. **Political influence is rare (per firm) but increasing over time**. 84% of composite firms in our data have no lobbying at any time during our sample, in any component firm. Similarly, 92% of composite firms have no corporate PAC for any component firm, at any time during our sample. On the individual donor side, only 29% of composites have at least one individual donor reported who listed one of the component firms as an employer. Spending on lobbying, however, has grown over time in aggregate.

3. **Firms spend more on lobbying than on campaign contributions**. This is true in aggregate, but also at the individual composite firm level. Of composite firms that spend both on donations and lobbying, 90% spend more on lobbying.

4. **Merging, revenue and political influence activity are correlated**. Large composite firms are more likely to lobby and have PACs. They are also more likely to merge with another Compustat-listed firm and to have a longer lifespan.

5. **Influence activity is persistent over time.** Once firms become politically active, they remain active over time. In our data, firms are active in lobbying in about two-thirds of all

half-year periods following their first lobbying spend. Following the establishment of an in-house lobbying operation, they are active in 87% of the remaining half years. On the campaign contributions side, their PAC is active in about 76% of periods after the PAC's first spend.

Table 2: **Merged vs Non-Merging Composite Firms: Differences in Means**

	Never Merged	Merged	Difference
Years in Sample	7.98	15.67	-7.69***
Avg Revenue ($10M, per Half Year)	35.18	300.14	-264.97***
Lobby Spend ($1K, per Half Year)	21.29	334.31	-313.02***
Lobbied at all (per Half Year)	0.06	0.30	-0.24***
Lobbied at all (ever)	0.12	0.50	-0.38***
In-House Lobby Spend ($1K, per Half Year)	12.46	240.20	-227.73***
Lobbying Intermediary Spend ($1K, per Half Year)	8.82	94.11	-85.29***
PAC Donations ($1K, per Half Year)	0.82	14.58	-13.76***
PAC Donations> 0 (per Half Year)	0.03	0.22	-0.19***
PAC Donations> 0 (Ever)	0.05	0.34	-0.30***
Individual Donations ($1K, per Half Year)	0.30	3.08	-2.77***
Individual Donations> 0 (per Half Year)	0.04	0.20	-0.16***
Individual Donations> 0 (Ever)	0.24	0.78	-0.54***
# of Component Firms	1	3.33	-2.33***

Notes: This table displays average differences between composite firms that merge and composite firms that do not.

The descriptive tables present these patterns at the composite level, but we find the same patterns in our disaggregated dataset of individual component firms.

The averages in Tables 1 and 2 also highlight some important dimensions of heterogeneity. While most firms do not lobby, there is a sizable minority of firms that lobby a lot. Conditional on lobbying, the average composite firm spends over half of a million dollars on lobbying per year ($670K) in our sample (median of $56K/year). At the top of the distribution, there are firms that spend tens of millions of dollars per year. As the raw correlations in Table F2 show, these firms tend to be the largest firms and are also more likely to engage in merger activity (the core question of our paper).

Other time trends emerge. In the two decades of our sample, total lobbying spend steadily increased by $67.2M per year on average. Among firms lobbying at all, the median lobby spend increased by 2.5 times, from $80K in 1999 to $200K in 2017 inflation-adjusted, a large

increase. Also during this period, the number of firms at any cross-section of our sample decreased by less than 1% per year. The reduction in publicly traded companies has been documented in other studies (Doidge et al., 2017; Grullon et al., 2015). The proportion of these firms in our sample that were lobbying at any time increased very slightly over time.

5 Panel Event Study

Panel event studies are a type of econometric model studied by De Chaisemartin and d'Haultfoeuille (2020); Freyaldenhoven et al. (2021); Goodman-Bacon (2021). In this approach, the estimation of Equation (3) is straightforward (i.e., no instrument or first stage). Mergers in this setup are endogenous, but we assume they depend on fixed (or slow-moving) variables whose trends we control for. The consummation of the merger creates a sharp discontinuity in the firms' ability to coordinate price externalities.

The threat to identification in this strategy comes from a potential unobserved confound C_{it}. The confound C_{it} can include potentially unobserved time-specific factors for each composite firm, as well as an idiosyncratic component i.e., $C_{it} = \lambda_i' F_t + \xi \eta_{it}$. Freyaldenhoven et al. (2021) notes that Equation (3) is identified with a two-way fixed effects model, as long as C_{it} is low-dimensional and $F_t = 0$. In our setting, a confound would violate this criterion if it affects political influence activities through a non-merger mechanism, and would coincide with the merger event.

To complement this approach, we add unit-specific, time-varying controls that may capture such confounds. In particular, we include a measure of firm size (revenue) and allow for industry-specific trends within a narrow category (NAICS5). We also include firm-specific political cycle effects,[23] as well as controls for differential revenue effects depending on the number of mergers during the sample. In our regressions about industry- and trade-association spend, we use the equivalent variables at the industry level. The identification assumption is that, after conditioning on all these other factors, mergers come from idiosyncratic shocks

[23] Our firm-specific political cycle controls would capture the possibility that "Walmart tends to spend a lot in the midterms," or "Boeing spends a lot during the presidential election years," and so forth. To implement this, we codify each half-year in our sample based on its timing within a four year (eight half-year) political cycle between presidential elections. The main effect of political cycles is absorbed by our half-year fixed effects. We then interact these cycle indicators with firm identifiers to produce firm-specific political cycle effects.

that are unrelated to the returns of political spending.

A challenge that is unaddressed by this specification is the possibility of pre-merger increases in lobbying activity. Firms could initiate this form of pre-merger lobbying to influence the merger's review by regulators. Alternatively, firms may anticipate a positive review, and begin coordinating and integrating lobbying activity before the official merger date. Note that such pre-merger activity would bias the "control" period upwards, resulting in a smaller difference coming from the merger. The resulting bias is likely to work against finding a positive effect by inflating the pre-merger levels. We address this with an additional specification controlling for anticipation effects (the results are summarized in the next section and reported in Appendix H).

5.1 Results: Composite Firm Panel

Table 3 shows results on the amount of lobbying spend and PAC donations using our main specification in Equation (3). The first four columns study the amount of lobbying spend and the latter four study the amount of PAC contributions. We study both sparse controls and a richer set.

In all our specifications, coefficients have the same sign: Greater concentration (that is, a reduction in the number of component firms within a certain composite firm) increases the amount of composite firms' spend on political influence activities (both lobbying spend and PAC spend). Results about lobbying are more statistically and economically significant than those about campaign contributions. We find the average merger increases lobbying spend by about $140,000 per year. PAC results are weaker (roughly $8,000 per year) and not always significant. Results are robust to an alternative index of concentration of the composite firm (see Appendix M).

To visualize these effects, Figures 1 and 2 display event study plots. Each point bar represents the cumulative effect of the merger on per-period spend at each period of time.[24] Although some data points are estimated noisily, the broad pre/post effects are visible.

[24]These plots include a window of 8 periods on either side of the merger. In some approaches to event study plots, coefficients are estimated to place additional bars on the plot that aggregates for all pre- and post- window observations. We have not estimated these coefficients as they significantly decrease our sample size.

Table 3: **All Firms, Panel Event Study**

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
# Component Firms	-74,286**	-68,934**	-4,470*	-3,898
	(33,691)	(28,188)	(2,382)	(2,514)
Additional Controls		Y		Y
Observations	223,043	223,022	223,043	223,022
R^2	.79	.83	.32	.47

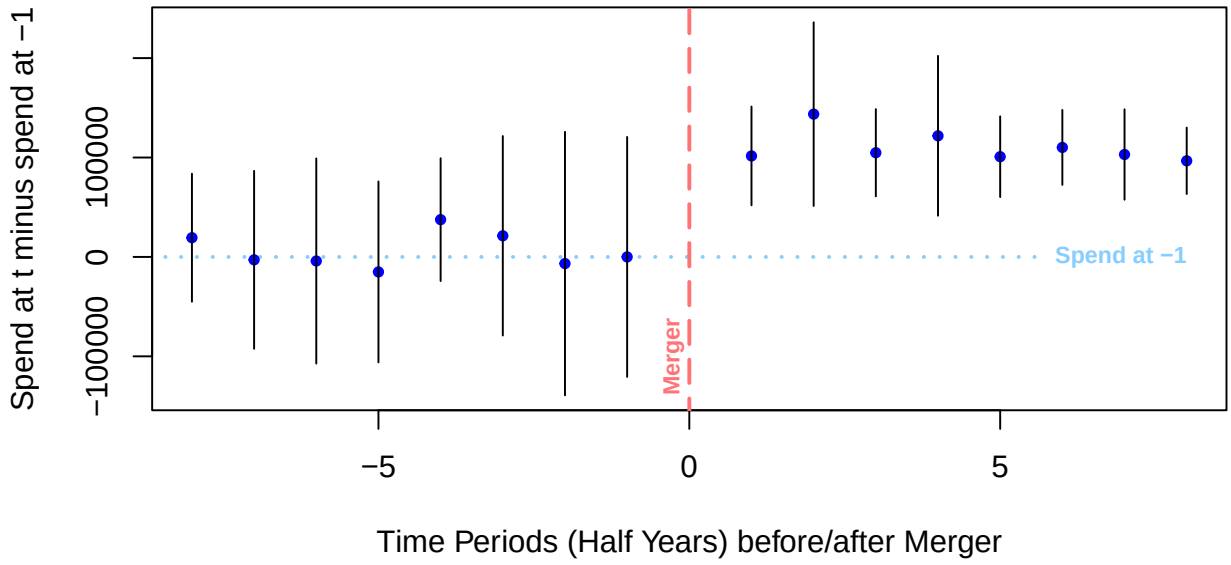
Notes: This table shows results from our panel event study specification (Section 5) on lobbying and PAC donations. Lobbying and PAC donation outcomes are regressed against composite-firm fixed effects, composite firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. Standard errors are clustered by composite firms.

We also probe the robustness of our results to pre-merger anticipation effects (see Appendix H). One could imagine that merging firms may engage in lobbying activities to get the merger approved. However, we find little evidence of a pre-merger increase in lobbying in the six months that precede the merger. We find the same result when we restrict the sample to the subset of firms strictly above the Hart-Scott-Rodino merger threshold, that is, those mergers that had to be reported to the antitrust authorities and that could, in principle, be further investigated (Wollmann, 2019).

The low level of anticipatory spending is consistent with the observation that, in the period under consideration, the U.S. antitrust authorities scrutinized a small proportion of mergers (Wu, 2018). Between 2010 and 2019, the Federal Trade Commission and the Department of Justice issued "Second Requests" for between 2.2% and 3.9% of transactions, depending on the year (Simons and Delrahim, 2020). This means that in each of those years, over 95% of proposed mergers that were notified, were approved within 30 days with no additional information requests.

Heterogeneity: Size and Similarity. Our specification allows us to examine heterogeneity across different types of firms. Our theory features two aspects in particular. First, it is a theory of horizontal mergers of similar firms, since a merger would reduce the competitive pressure between these two firms and increase the marginal benefits from lobbying. Second,

Figure 1: **Lobby Spending: Event Study Plots**



Notes: This figure shows an event study plot displaying lobbying spending differences before and after the merger (window length = eight half years before/after), using our event study design. Each point bar represents the cumulative effect of the merger on per- half year spend. Standard errors are clustered by composite firms.

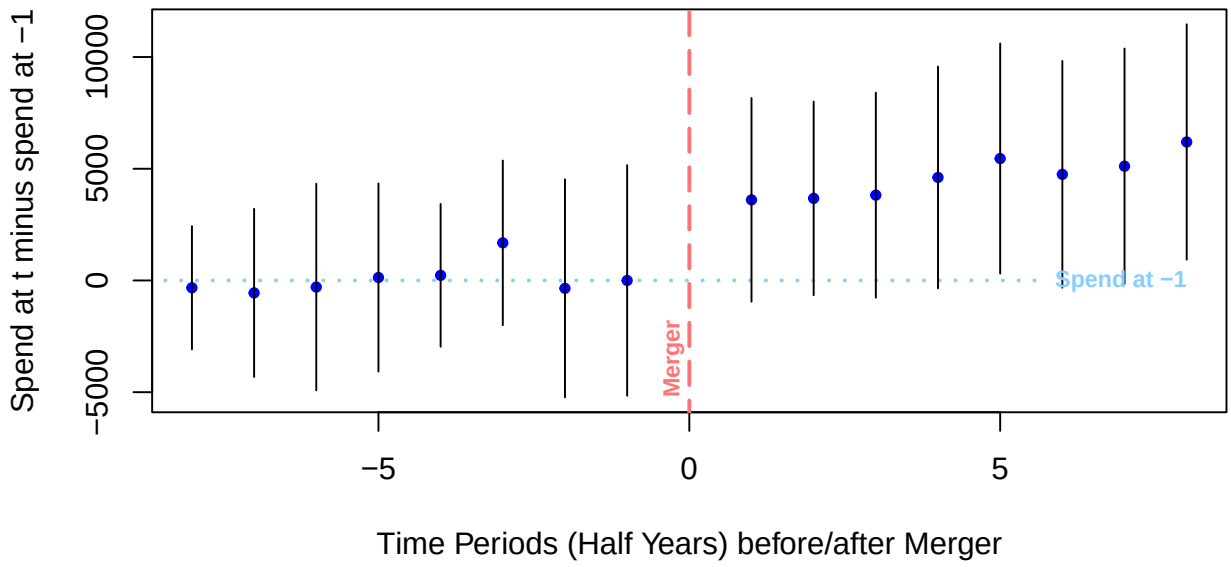
our theory intuitively applies particularly to "large" firms, especially if there are fixed costs associated with lobbying, as these are the firms that are more likely to incur such costs in order to lobby.

We can operationalize these concepts using our data. For size, we use revenue. We sum all revenue across the entire sample for each composite firm, and examine companies above and below the median.[25] In Table 4, we find that although mergers broadly increase lobbying spend across both sets of firms, the effects on large firms are both bigger and statistically more significant. Again, PAC results are fuzzier. Table 4 examines above/below the median firm size, and Appendix G presents robustness to other splits.

Our theory also suggests that a merger of more closely-related firms would have a bigger effect. Such firms are more likely to have common, overlapping interests. To measure close vs. distant mergers, we use data about the industry categorizations of component firms (mea-

[25] Although this splits our composite firms in half, it does not split the entire panel in half because the large firms have more observations, possibly because of survivorship bias.

Figure 2: **PAC Donations: Event Study Plots**



Notes: This figure shows an event study plot displaying PAC spending differences before and after the merger (window length = eight half years before/after), using our event study design. Each point bar represents the cumulative effect of the merger on per- half year spend. Standard errors are clustered by composite firms.

sured by NAICS codes). For each composite firm, we measure the number of unique NAICS codes at the beginning of the sample. Composite firms with a high number of unique NAICS codes represent firms that merge across industries (distant), while those with few unique NAICS codes represent within industry mergers (close).

Tables 5 shows the close-vs-distant results. We interact the MergerIndex_{it} variable with our measure of industry distance. By looking at this interaction coefficient, the effect on lobbying is higher when the merging firms interact within the same industry. We find similar (but insignificant) results on PAC contributions.

Extensive Margins. The raw data show that many firms do not lobby at all. In our theory model, we rationalized this by adding a fixed cost to setup lobbying. The model predicts that a merger increases the probability that a firm starts lobbying. That is, mergers increase lobbying at the extensive margin. Also, we argued that the theory captures particularly in-house rather than outsourced lobbying. Table 6 shows extensive margin results for all firms.

Table 4: **Heterogeneity by Firm Size (Panel Event Study)**

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
# Component Firms	-15,835	-66,208**	-823	-3,788
	(17,269)	(28,513)	(1,107)	(2,513)
Sample	Below Median Revenue	Above Median Revenue	Below Median Revenue	Above Median Revenue
Observations	76,773	146,249	76,773	146,249
R^2	.55	.84	.72	.47

Notes: This table shows results from our panel event study specification (Section 5) on lobbying and PAC donations. Results are separated by firm size (measured by revenue), above and below the median. Lobbying and PAC donation outcomes are regressed against composite firm fixed effects, composite firm size (measured in revenue), and time-period fixed effects. All regressions in this table contain the additional controls described in Section 5. For additional discussion of this specification, see "Heterogeneity: Size and Similarity" in Section 5. Standard errors are clustered by composite firms.

The outcome variables are binary variables that change from zero to one the first time that any component firm lobbies or contributes to political campaigns through a corporate PAC.

Results suggest that mergers increase both lobbying and PACs at the extensive margin. Following a merger, firms without an in-house lobbying team or corporate PAC were more likely to create them by around 1.5 to 1.7 percentage points. For comparison, 8% of firms lobby in any form in our first period, and only 5% of firms had a corporate PAC. Effects are higher and more statistically significant for in-house lobbying than for outsourced.

Appendix Tables I2 and I1 study extensive margin effects heterogeneously. We show larger effects for mergers involving larger firms and for horizontal mergers. This is again in line with the simple theoretical predictions that showed that lobbying is more likely to be started in large rather than niche industries.

5.2 Results: Industry and Trade Association Panel

We now turn to the results at the industry level. Table 7 looks at the impact of a merger on total spend on lobbying and PACs made by trade associations in a given industry. Table 8 reports the effect of a merger on spending by all firms in that industry.

Table 5: **Heterogeneity: Close vs Distant Mergers (Panel Event Study)**

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
# Component Firms	-91,572**	-91,351**	-3,028	-2,481
	(41,214)	(35,909)	(2,555)	(2,160)
# Component Firms × Unique NAICS	8,204**	8,360**	105	70
	(3,816)	(3,866)	(207)	(146)
Additional Controls		Y		Y
Observations	223,043	223,022	223,043	223,022
R^2	.79	.83	.32	.48

Notes: This table shows results from our panel event study specification (Section 5) on lobbying and PAC donations. We include interactions with how many industries are included among the merging firms using NAICS codes. Lobbying and PAC donation outcomes are regressed against composite firm fixed effects, composite firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. For additional discussion of this specification, see "Heterogeneity: Size and Similarity" in Section 5. Standard errors are clustered by composite firms.

Table 6: **All Firms, Extensive Margin Effects**

	(1) Started In-House Lobbying	(2) Started In-House Lobbying	(3) Started Outsourced Lobbying	(4) Started Outsourced Lobbying	(5) Started PAC	(6) Started PAC
# Component Firms	-.015***	-.015***	-.0039	-.0068*	-.016***	-.017***
	(.0036)	(.0037)	(.0035)	(.0038)	(.004)	(.004)
Additional Controls		Y		Y		Y
Observations	223,043	223,022	223,043	223,022	223,043	223,022
R^2	.86	.88	.83	.86	.88	.9

Notes: This table shows results on extensive margins (first lobbying and PAC donations in the firm's history) using our panel event study specification (Section 5). Each outcome is regressed against composite firm fixed effects, composite firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. For additional discussion of this specification, see "Extensive Margins" in Section 5. Standard errors are clustered by composite firms.

As expected, the sample size is greatly reduced by the aggregation. The number of observations goes down from 220K for the composite level analysis to just over 2K for the industry level analysis. Despite this drastic sample reduction, we do have several pieces of evidence

suggesting that mergers increase the political activity of trade associations. The effect of mergers on lobbying spend is large and statistically significant, and the effect on PAC contributions is significant in some specifications. The sign for all of our coefficients indicates an average increase from mergers.

Results about the entire industry (including non-merging firms) are in a similar direction (Table 8), but less precise for PACs.

Table 7: **Trade Associations, Panel Event Study**

	(1) Lobby Spend, Industry Associations	(2) Lobby Spend, Industry Associations	(3) PAC Spend Industry Associations	(4) PAC Spend Industry Associations
# Unmerged Firms	-154,264**	-208,735**	-49,180	-104,891**
	(72,699)	(84,784)	(34,224)	(50,041)
Additional Controls		Y		Y
Observations	2,206	2,206	2,206	2,206
R^2	.54	.63	.62	.69

Notes: This table shows results on lobbying and PAC donations by trade associations using our panel event study specification (Section 5). Outcomes are regressed against industry fixed effects, industry firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. Standard errors are clustered by industry.

Table 8: **Industry Analysis, Panel Event Study**

	(1) Lobby Spend, Full Industry	(2) Lobby Spend, Full Industry	(3) PAC Spend, Full Industry	(4) PAC Spend, Full Industry
# Unmerged Firms	-100,208	-222,571***	3,665	-6,556
	(79,742)	(77,868)	(5,027)	(9,823)
Additional Controls		Y		Y
Observations	2,206	2,206	2,206	2,206
R^2	.94	.95	.63	.74

Notes: This table shows results on lobbying and PAC donations at the industry level using our panel event study specification (Section 5). Outcomes are regressed against industry fixed effects, industry firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. Standard errors are clustered by industry.

6 Differential Exposure Design

Our panel event study approach suffers from the possibility that unobserved confounds could impact lobbying through channels different from mergers, but happening at the same time as mergers. One way to address this concern is to control for more potential confounds. We do this in Section 7.1. In this section, we introduce a second approach to identification. The approach in this section relies on an instrument in the spirit of Bartik (1991) or other shift-share designs (Borusyak and Hull, 2023; Breuer, 2022; Goldsmith-Pinkham et al., 2020).

The idea behind these designs is that units are affected by shocks, but they have differential exposure to these shocks. In an influential paper developing this strategy, Bartik (1991) examined how employment growth affects wage growth. Because employment growth is endogenous, the author developed an instrument, exploiting the idea that economy-wide demand shocks in different industries have idiosyncratic effects in local markets. These shocks varied systematically according to the pre-shock characteristics of the local market.

We pursue a similar strategy to study mergers. To construct the instrument, we use a long-noticed fact about mergers: they arrive in waves (Gort, 1969; Nelson, 1959). These waves span multiple sectors (Maksimovic et al., 2013), and have several underlying causes including macroeconomic shocks (Maksimovic and Phillips, 2001; Rhodes-Kropf and Viswanathan, 2004), regulatory and technology shocks (Mitchell and Mulherin, 1996), uncertainty (Bonaime et al., 2018; Toxvaerd, 2008), and connections between industries (Ahern and Harford, 2014).

We utilize economy-wide pro-merger shocks at different times to construct a time-varying instrument similar to the Bartik (1991) approach. At various times and industries during our sample, mergers have been particularly popular (or unpopular) compared to overall trends. We quantify these shocks, and interact them with measurements of a firm exposure to these shocks. As we show later, this instrument has a strong first stage.

6.1 Implementation

To implement this design, we again use Equation (3), including the same set of additional controls. This time, however, we develop an instrument for the key measure of concentration.

The instrumented variable is MergerIndex_{it}, which measures how concentrated composite firm i is at time t. As is common for Bartik-like designs, our instrument is an inner product of two terms: a term that captures the initial exposure ("shares" $\vec{Z}_{i0}$), and a time-varying term (merger waves, or "shifts" $\vec{G}_t$). Using the notation of Goldsmith-Pinkham et al. (2020), the instrument we construct is

$$B_{it} = \vec{Z}_{i0} \cdot \vec{G}_t = \sum_{k=1}^{K} z_{ik0} g_{kt}, \tag{4}$$

where $\vec{Z}_{i0}$ is a $1 \times K$ vector containing what "share" of composite firm i is in each of $k = 1, ..., K$ industries. To operationalize these "shares," we use the percentage of revenue from each industry. We use the top-level NAICS to define industries. For the growth vector $\vec{G}_t$, we use a $K \times 1$ vector representing how many mergers occurred in each of K industries until time t.

Qualitatively, our instrument measures the predicted number of mergers for composite firm i at time t. It calculates a weighted sum of the merger activity across industries, weighted by the share of composite i's exposure across industries. As such, a "high" instrument corresponds to a high number of mergers. In our first stage, this corresponds to a lower number of component firms (culminating with one, when all have merged together).

Because Bartik-like instruments are products, researchers typically argue that one (or both) elements are exogenous (Borusyak et al., 2022; Goldsmith-Pinkham et al., 2020). In our application, we portray the time-varying shocks as exogenous (Borusyak et al., 2022), and the initial "shares" as endogenous.[26] However, this does not mean that the timing (or other aspects) of mergers are entirely exogenous. Like other IV designs, our instrument simply acts as one source of exogenous encouragement.

Some of our empirical results in Section 5 examined outcomes at the aggregated industry level (such as trade associations and industry-wide spend). Because Bartik-like designs use cross-industry variation within each unit, we cannot aggregate to the industry level without eliminating part of the instrument. As such, our specifications in this section are about composite firm outcomes (comparable to Tables 3 and 6), and not about industries as a whole.

[26] The shares term is related to the identity of merging partners, and thus likely to be endogenous.

First Stage. We use the B_{it} term defined in Equation (4) to instrument the MergerIndex$_{it}$ term in Equation (3) by using the following first stage regression

$$\text{MergerIndex}_{it} = \lambda_0 + \underbrace{\lambda_1 B_{it}}_{\text{IV, Eq. 4}} + \lambda_2 X_{it} + \zeta_i + \tau_t + \eta_{it}. \tag{5}$$

This is the same regression as Equation (3), but the dependent variable is now MergerIndex$_{it}$, and the main independent variable is now the instrument B_{it}. The other terms are the same but given separate names; the coefficients are now λs, the error term is η_{it}, composite firm fixed effects are ζ_i, and time-period fixed effects are τ_t. We include industry-specific trends of each composite firm's main NAICS1 industry. Diagnostics on the instruments (compliers, instrument strengths and first-stage coefficients) are performed in Appendix K and below.

Table 9 presents our the first stage. For ease of interpretation, we standardize the instrument and the MergerIndex$_{it}$. We find that a one-standard deviation increase in the instrument (expected mergers) lowers number of firms inside the bundle by about 0.3 standard deviations on average. Our instrument has a strong first stage, featuring strong F statistics (as measured using the metrics proposed by Montiel-Olea and Pflueger 2013 and Kleibergen and Paap 2006; Stock and Yogo 2005). The economic interpretation of the first stage is that merger waves in industries where composite firm i has a high share of revenue tend to produce mergers in firm i (lowering the number of components in composite firm i).

Table 9: **First Stage, Exposure Design**

	(1) # Component Firms (std)	(2) # Component Firms (std)
Mergers IV (B_{it}, std)	-.3***	-.28***
	(.025)	(.027)
Controls		Y
F-Statistic	136	107
Observations	216,563	216,563
R^2	.64	.69

Notes: This table shows the first stage regression results for the IV design. Standard errors are clustered by composite firms.

6.2 Results

Table 10 contains our results about lobbying spend and lobbying amounts. Our results suggest that greater concentration increases composite firms' spend on political influence activities (both lobbying spend and PAC spend). In Table 10, the average merger identified by this design increases lobbying by about $300K per year (columns 1 & 2). We estimate that the impact on PAC donations is around $20K per year (columns 3 & 4).

Table 10: **Exposure Design Results: Lobbying and PAC Spend**

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
# Component Firms	-142,059***	-179,470***	-11,085*	-9,426*
	(53,186)	(55,196)	(6,271)	(5,025)
Controls		Y		Y
F-Statistic	136	107	136	107
Observations	216,563	216,563	216,563	216,563

Notes: This table shows results on lobbying and PAC donations using our instrumental variables specification described in Section 6 (first implementation). We instrument for the Merger Index$_{it}$ using a Bartik-like instrument that combines merger waves and exposure. All regressions control for composite firm fixed effects, composite firm size (measured in revenue), and time period fixed effects. For columns with additional controls, these are described in Section 5. IV diagnostics appear in Appendix K. Standard errors are clustered by composite firms.

Table 11 examines results on our extensive margin outcomes. A merger is associated with a increase in around 7% increase in the probability of starting an in-house lobbying group, and about a 4.5% increase in the probability of starting a PAC.

Across all of our outcomes, results from the exposure design, compared respectively to Tables 3 and 6, are in the same order of magnitude as the panel event study, but larger (and also with larger standard deviations). Our results characterizing the "compliers" to our instruments show that the compliers are more likely to be large firms (Appendix K). Our theory intuitively applies particularly to "large" firms (particularly with fixed costs), and our panel event study results also found larger effects for big firms.

Table 11: **Exposure Design Results: Extensive Margin**

	(1) Started In-House Lobbying	(2) Started In-House Lobbying	(3) Started Outsourced Lobbying	(4) Started Outsourced Lobbying	(5) Started PAC	(6) Started PAC
# Component Firms	-.067***	-.073***	-.025	-.066	-.045*	-.046
	(.021)	(.023)	(.037)	(.04)	(.025)	(.029)
Controls		Y		Y		Y
F-Statistic	136	107	136	107	136	107
Observations	216,563	216,563	216,563	216,563	216,563	216,563

Notes: This table shows results on lobbying and PAC donations using our instrumental variables specification described in Section 6 (first implementation). We instrument for the Merger Index$_{it}$ using a Bartik-like instrument that combines merger waves and exposure. All regressions control for composite firm fixed effects, composite firm size (measured in revenue), and time period fixed effects. For columns with additional controls, these are described in Section 5. IV diagnostics appear in Appendix K. Standard errors are clustered by composite firms.

7 Firm-Level Political Risk (and Other Robustness Measures)

Our theory section proposed that a merger helps firms avoid rent-dissipation of lobbying for a common cause. However, another mechanism could also produce an increase: After a merger, regulators could increase scrutiny as a result of negative attention from third parties. Because of this attention, the merged entity could increase political spending — not because of rent dissipation and externalities, but in response to a more adversarial environment.

To investigate this possibility, we examine measures of *firm-level political risk*. If the political environment becomes more negative after a merger, then we may expect exposure to political risk to increase after the merger. A highly-cited paper by Hassan et al. (2019) develops an empirical strategy for measuring firm-level political risk over time. The approach uses text-mining methods to quantify "[T]he share of [a firm's] quarterly earnings conference calls that they devote to political risks." We use the measures from this paper as the outcome variables in our panel specifications above.

The Hassan et al. (2019) metrics not only contain an overall measure of firm-level risk, but also additional detailed data about the *type and direction* of political risk. Higher sentiment indicates more positive discussion. In addition, the data contains detailed breakdowns about

the level of political risk across eight topics: economic policy & budget, environment, trade, institutions & political process, health, security & defense, tax policy, and technology & infrastructure. Our main results focus on the "economic policy & budget" variable and the overall level of political risk, but we include the full set of categories for completeness.

Sample. Political risk measures are available only for the subset of firms that have regular investor calls. Appendix L contains descriptive statistics for firms that are in our investor call sample (compared to those that are not), and other details of how we integrated this data into our composite firm panel. Our panel of composite firms that use investor calls is about one third of the size of the sample as a whole. Firms with regular investor calls are generally larger and more politically active.

Results. Table L2 contains the results using our panel event study. For ease of interpretation, we normalize all measures of political risk. In Columns 1 and 2, we replicate our main results on lobbying and PAC spending on the subsample. Our results on this subsample have the same direction and size as our main results, although less precise, partly as a result of the smaller sample size (31% of the main sample).

The remaining columns show the effect of mergers on political risk, particularly risks around economic policy. We find no evidence of higher political risk after a merger (in any specification). Even if mergers could in principle lead to increased scrutiny of larger firms (including scrutiny from the media), this is not reflected in a higher share of attention devoted to economic or political risk when firms relay information to their investors. Estimates generally fail to reject zero, with standard errors small enough to rule out large effects. In one case, we obtain statistically significant results in the opposite direction: political sentiment becomes more positive after the merger (although the size of this effect is small). Table L3 contains all measures of political risk,[27] and Appendix M contains results with an alternative index of concentration of the composite firm.

[27] In total we study ten measures of political risk. Trade policy is one area where we do find a small statistically significant difference in risks after mergers.

7.1 Other Robustness Exercises

Finally, Appendix J contains some additional robustness exercises. First, we re-consider all our main results with a specification in logs instead of levels. Second, for the panel event study, we discuss two possible threats to identification.

Logs. We conducted our main analysis in levels. Alternatively, one could transform the data into logs, to capture the idea that merger effects could be proportional to the pre-merger level of lobbying, and interpret results as percentage increases. There are two potential drawbacks with a logarithmic transformation. This can only be applied to positive values. This limitation requires special handling in cases where the original dataset contains zeroes. Second, since logarithmic transformations compress data distribution, extreme values may have less influence on the analysis, which could lead to potential misinterpretation of the results if those values carry important information. Both aspects are present in our data. We know from descriptive statistics that many firms do not lobby or merge at all. These are typically small firms in size, as also suggested by our theoretical findings. Notwithstanding these limitations, we do implement a log transformation, and also transform the measure of total size.[28] As shown is Appendix N we find qualitatively similar results across both logs and levels specifications.

Time-Varying Misvaluation. A threat to identification comes from relative misvaluation: Time-varying misvaluation could drive both M&A and political spending. For example, undervalued firms are likely to be bought by relatively overvalued firms, so there is more M&A activity in times of greater misvaluation (Dong et al., 2006; Rhodes-Kropf et al., 2005). To address this question, we created a market-to-book ratio following Golubov and Konstantinidi (2019). Results in Appendix J show that our findings are the same qualitatively (and are also similar in the point estimate) when controlling for this term.

Industry×Time Fixed Effects. Some potential confounds could affect an entire industry. For example, a merger may be timed in such a way to get a favorable treatment by the anti-trust authority. Alternatively, low-cost foreign competition in a sector could compel both mergers

[28] To address zeros variable, we add one to $\sum_{f \in \mathcal{F}_i} y_{ft}$ before taking the log.

and lobbying in that sector. One useful way to address this possibility is to include a full set of half-year×industry fixed effects (instead of industry-specific trends, as done in our main specification). This would control for industry-specific, time-varying factors (such as the level of anti-trust investigations or foreign competition) at the half-year×industry level. Appendix J includes these controls, and finds similar results as in our overall analysis.

8 Conclusion

Our paper hopes to contribute to the lively debate on the increase in industry concentration and changes in business dynamics (Dube et al., 2020; De Loecker et al., 2020; Philippon, 2019), as well as its causes and policy implications (Autor et al., 2020; Azar et al., 2020; Berry et al., 2019; Dube et al., 2020; Grullon et al., 2015).

We contribute to this discussion by introducing an additional element, political influence, and by studying how firms vie to gain political power both in theory and in the data. Our theoretical model takes a standard model of competition and extends it to include regulatory variables set by strategic lobbying. While our data come from a developed economy within a democratic state, our model is agnostic about the form of government (or the level of development). In countries with less democratic accountability, some of the forces in our model could be stronger or weaker. State capture by business interests also appears as an issue in development economics (Canen and Wantchekon, 2022).

Our findings suggest that firms increase lobbying after mergers. This pattern survives a number of robustness checks and alternative explanations. The association is stronger for mergers involving large firms, and for mergers involving firms in the same industry.

Our results from the U.S. are indicative of a robust pattern, but they are far from conclusive. We hope this is a starting point for richer and deeper analyses of the political and other non-market effects of mergers. We see at least three avenues for more research. First, future research could explore the link between lobbying activity and government regulation. When a merger occurs, which policies is the additional influence activity directed toward? This type of research will probably focus on one specific industry, as regulation is highly industry-

specific (see, e.g., Kang 2016, for an application to federal legislation and lobbying activities in the U.S. energy sector). A recent example in this direction is Moshary and Slattery (2024) who look at mergers and lobbying in the auto retail industry.

A second avenue of research relates to the effect of market power on the organization of lobbying. When an industry becomes more concentrated, does its approach to policy influence change? For instance, do firms tend to rely more or less on trade associations? Do they tend to do more or less in-house lobbying? Do they attempt to establish a direct relationship with people close to policy making, for instance by hiring former office holders?

Finally, industry-supported government regulation is very likely to benefit industry, but it can benefit consumers (as perhaps in the case of safety and environmental standards) or hurt them (as perhaps in the case of barriers to entry). These elements would each be important to understand whether the political channel we analyze constitutes another form of consumer harm of market power — one delivered through the channel of regulation rather than directly through markets.

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Supplemental Appendix: For Online Publication

A Theoretical Appendix

A.1 Proof of Proposition 2

Proof for Proposition 2. Without a merger, let us first re-consider what happens at $t = 1$. In a duopoly, we obtain

$$\pi_{LL} \equiv \frac{(A+R-1)^2}{9} - \hat{t} = \frac{(A-1)^2 w}{9w-4},$$

where the subscript LL indicates that both firms lobby. If none of the firms lobby, then it is immediate that profits are

$$\pi_{NN} \equiv \frac{(A-1)^2}{9},$$

where the subscript NN indicates no firm lobbies.

If only one firm lobbies, then we need to analyze the asymmetric case. There is now only one possible coalition. The policy maker selects R to maximize $\frac{(A+R-1)^2}{9} - w\frac{R^2}{2}$ with an interior solution $R^* = \frac{2(A-1)}{9w-2}$ and one positive effort only $\hat{t} = \frac{2(A-1)^2 w}{(9w-2)^2}$. The (asymmetric) profits of the firms are

$$\pi_{LN} \equiv \frac{(A+R-1)^2}{9} - \hat{t} = \frac{(A-1)^2 w}{9w-2},$$
$$\pi_{NL} \equiv \frac{(A+R-1)^2}{9} = \frac{9(A-1)^2 w^2}{(9w-2)^2},$$

where the first expression refers to the firm that lobbies and the second one to the firm that does not (but free rides on the first firm).

We can now turn to the first stage at $t = 0$ which is summarized in Figure A1.

Figure A1: Fixed Lobbying Cost

Firm 1/ Firm 2	Lobby	Don't lobby
Lobby	$\pi_{LL} - F, \pi_{LL} - F$	$\pi_{LN} - F, \pi_{NL}$
Don't lobby	$\pi_{NL}, \pi_{LN} - F$	π_{NN}, π_{NN}

The analysis of this initial first stage is helped by the fact that there is a clear and intuitive ranking of the gross payoffs

$$\pi_{LL} > \pi_{NL} > \pi_{LN} > \pi_{NN}.$$

Define $k_2 \equiv \frac{\pi_{LL}-\pi_{NL}}{(A-1)^2} = \frac{4w}{(9w-2)^2(9w-4)}$ and $k_1 \equiv \frac{\pi_{LN}-\pi_{NN}}{(A-1)^2} = \frac{2}{9(9w-2)}$. We concentrate on the case when w is high enough, namely $w > 2(2+\sqrt{2})/9 \simeq 0.76$ so that it is $k_1 > k_2$. Proposition 2 follows immediately.[29] □

A.2 Proof of Proposition 3

Proof of Proposition 3. Whether the merged firm will spend the set-up cost or not at $t = 0$ is immediate to analyze. Without spending its set-up cost, it will achieve the standard monopoly profits $\frac{(A-1)^2}{4}$. With the lobbying facilities it will instead earn

$$\frac{(A+R_{12}-1)^2}{4} - \hat{t}_{12} - F = \frac{(A-1)^2 w}{2(2w-1)} - F.$$

Thus the lobbying set-up costs F will be paid iff net profits exceed $\frac{(A-1)^2}{4}$, which happens when

$$\frac{F}{(A-1)^2} < k_{12} \equiv \frac{1}{4(2w-1)}.$$

Comparing the lobbying threshold in the merger case with those derived without mergers, it is immediate to show that $k_{12} > \max[k_1, k_2]$. If one imagines that fixed set up costs (relative to the size of the market) are independent random draws for each firm, we can conclude Proposition 3. □

A.3 Case with $n > 2$ Firms

We sketch here the analysis with $n > 2$ firms. To do this in a Cournot setting requires a small modification of the setup. As is well-known, with 3+ firms mergers may be unprofitable (the so-called Cournot "merger paradox"). We thus add a cost-saving element to the merger, so that the marginal cost goes down by s for the merged entity.

We also consider potential asymmetries among firms, in order to study the possible difference between mergers involving larger or smaller firms. The simplest way to do so is to imagine that a subset $m \leq n$ of firms have a marginal cost of 1 like in the baseline, and the remaining $n - m$ firms have a marginal cost of $1 + d > 1$. In equilibrium, the former group of firms will be larger in size than the second group of firms. For this reason, we call the first group "large" firms and the second group "small" firms.

A.3.1 Lobbying

We generalize lobbying to n firms. The policy maker selects

[29] If the case that instead $1/2 < w < (2+\sqrt{2})/9 \simeq 0.76$, in the region between k_1 and k_2, there are no asymmetric equilibria with only one firm lobbying, while there are multiple equilibria with both lobbying or none lobbying.

$$R^* \in \arg\max_R \sum_{i=1}^{n} \pi_i(R) + w(R).$$

To determine the lobbying effort $\hat{t}_i$, let

$$g_i(R) = \pi_i(R) - \hat{t}_i$$
$$R^*_{-I} \in \arg\max_R \sum_{j \notin I} \pi_j(R) + w(R).$$

In equilibrium, $g_i(P^*)$ lies on the upper contour of the set defined by

$$\text{for every } I \subset \mathcal{I}, \sum_{i \in I} g_i(R^*) \leq \sum_j \pi_j(R^*) + w(R^*) - (\sum_{j \notin I} \pi_j(R^*_{-I}) + w(R^*_{-I})). \tag{6}$$

We can apply Bernheim and Whinston (1986) to our setup. If we subtract $\sum_{i \in I} \pi_j(R^*)$ from both sides of (6) and reverse the signs, we get

$$\text{for every } I \subset \mathcal{I}, \quad \sum_{i \in I} \hat{t}_i \geq \left(\sum_{j \notin I} \pi_j(R^*_{-I}) + w\left(R^*_{-I}\right) \right) - \left(\sum_{j \notin I} \pi_j\left(R^*\right) + w\left(R^*\right) \right).$$

This constitutes a system of inequalities putting a lower bound on the value of the vector of lobbying effort $\hat{t}$.

In the original theorem, inequalities were defined in terms of lobby payoffs and payoffs had to belong to the upper contour of the set of payoff vectors that satisfied the system of inequalities. That requirement now means that the vector $\hat{t}$ must belong to the Pareto-efficient frontier of this set (from the perspective of lobbies). Namely, $\hat{t}$ is on the efficient frontier if it satisfies the system of inequalities and there does not exist another $\bar{t}$ that also satisfies the system of inequalities whose elements are all weakly smaller than $\hat{t}$ with at least one strict inequality.

We can directly specialize this general result to our case. A key feature of our setting is that all lobbies have policy preferences that go in the same direction: the profit of every firm is increasing in regulation R. This implies that in the system of inequalities above the only binding constraint is that of the grand coalition:

$$\sum_{i=1}^{n} \hat{t}_i = \max_R w(R) - w(R^*) = w\frac{R^{*2}}{2}.$$

To see why the grand coalition constraint must be binding, consider the case with two lob-

bies (the proof for $n > 2$ lobbies follows a similar structure and is omitted). Let

$$\begin{aligned} R^* &= \arg\max_R \pi_1(R) + \pi_2(R) + w(R) \\ R_1^* &= \arg\max_R \pi_1(R) + w(R) \\ R_2^* &= \arg\max_R \pi_2(R) + w(R) \\ R_0^* &= \arg\max_R w(R). \end{aligned}$$

Note that π_1 and π_2 are increasing for $R > 0$, while w is decreasing for $R > 0$. Therefore, we have

$$R^* \geq \max(R_1^*, R_2^*) \geq \min(R_1^*, R_2^*) \geq R_0^*.$$

Assume without loss of generality that $R_1^* \geq R_2^*$, so that

$$R^* \geq R_1^* \geq R_2^* \geq R_0^*.$$

The system of inequalities above is

$$\begin{aligned} \hat{t}_1 &\geq A_1 \equiv \pi_2(R_2^*) + w(R_2^*) - (\pi_2(R^*) + w(R^*)) \\ \hat{t}_2 &\geq A_2 \equiv \pi_1(R_1^*) + w(R_1^*) - (\pi_1(R^*) + w(R^*)) \\ \hat{t}_1 + \hat{t}_2 &\geq B \equiv w(R_0^*) - w(R^*). \end{aligned}$$

The Pareto efficient frontier of the set of transfers that satisfies the three inequalities above requires that at least one of these two conditions be satisfied: (i) both the first two inequalities bind; (ii) the third inequality binds. Note that (ii) is what we wish to prove.

To show that (ii) must hold, we will show that $A_1 + A_2 \leq B$. Then it cannot be that (i) holds and (ii) does not.[30] To prove that $A_1 + A_2 \leq B$, note that:

$$\begin{aligned} A_1 + A_2 &= \pi_1(R_1^*) + \pi_2(R_2^*) + w(R_1^*) + w(R_2^*) - (\pi_1(R^*) + \pi_2(R^*) + 2w(R^*)) \\ &\leq \pi_1(R_1^*) + \pi_2(R_1^*) + w(R_1^*) + w(R_2^*) - (\pi_1(R^*) + \pi_2(R^*) + 2w(R^*)) \\ &\leq w(R_2^*) - w(R^*) \\ &\leq w(R_0^*) - w(R^*) = B, \end{aligned}$$

where the first inequality is because π_2 is increasing in R, the second inequality is because R^* maximizes $\pi_1(R) + \pi_2(R) + w(R)$, and the third inequality is because w is decreasing in R. Hence total lobbying spending is uniquely determined in the equilibrium characterized in Theorem 1. However, the allocation of that spending to firms can potentially be distributed in multiple ways across firms.

To prove that the merger also increases the lobbying activity of individual firms, one needs additional assumptions. For instance, if one restricts attention to equilibria where the relative distribution of lobbying effort across firms remains constant after the merger, then Proposition 1 immediately implies that lobbying effort strictly increases also for all individual firms.

[30] In the general case with $n > 2$, one must consider any collection of subsets of lobbies such that all lobbies belong to exactly one subset. The vector $\hat{t}$ is on the efficient frontier if for at least one of these collections of subsets the inequalities are binding for all subsets in the collection. The rest of the proof follows a similar argument.

A.3.2 Competition

Pre-merger equilibrium. The m "large" firms with marginal cost 1 maximize $\pi_i = (A + R - Q)\, q_i - q_i$, while the $n - m$ "small" firms with marginal cost $1 + d$ maximize $\pi_j = (A + R - Q)\, q_j - (1 + d) q_j$.

The Cournot equilibrium yields[31]

$$q_i = \frac{A + R - 1 + d(n - m)}{n + 1}, q_j = \frac{A + R - 1 - d(m + 1)}{n + 1},$$

with corresponding profits $\pi_i = (q_i)^2, \pi_j = (q_j)^2$. Total industry profits are $\Pi = m\pi_i + (n - m)\pi_j$.

We know from Grossman-Helpman that the equilibrium lobbying level will solve

$$\max_R \Pi - w\frac{R^2}{2}.$$

Thus, the equilibrium level of regulation is

$$R^* = \frac{1}{w}\frac{d}{dR}\Pi.$$

Hence regulation depends on the marginal impact of R on total industry profits

$$\begin{aligned} &\frac{d}{dR}\Pi = m\frac{d\pi_i}{dR} + (n - m)\frac{d\pi_j}{dR} = 2(m\frac{dq_i}{dR} + (n - m)\frac{dq_j}{dR}) \\ &= 2\frac{n\,(A + R - 1) - d(n - m)}{(n + 1)^2}. \end{aligned} \tag{7}$$

To determine the lobbying activity of the whole industry, recall that the grand coalition transfer is binding in the Grossman-Helpman characterization, and we have

$$\sum_{i=1}^{n} \hat{t}_i = w\frac{R^{*2}}{2}.$$

Therefore, total lobbying activity of the whole industry is also determined by R^*.

Post-merger equilibrium. Let us consider first a "large" merger between two of the firms with marginal costs of 1. The profit function of the merged firm is

$$\pi_{12} = (A + R - Q)\, q_{12} - (1 - s)\, q_{12}.$$

[31] We assume that d is small enough to ensure an interior solution for every firm.

Standard computations obtain the following Cournot equilibrium

$$q_{12} = \frac{A+R-1+d(n-m)+s(n-1)}{n}, q_i = \frac{A+R-1+d(n-m)-s}{n}, q_j = \frac{A+R-1-dm-s}{n},$$

with corresponding profits $\pi_{12} = (q_{12})^2, \pi_i = (q_i)^2, \pi_j = (q_j)^2$ respectively for the merging firm 12, for a large non-merging firm i, and for a small non-merging firm j. Total industry profits are $\Pi = \pi_{12} + (m-2)\pi_i + (n-m)\pi_j$.

We are now in a position to discuss the effect of the merger on lobbying incentives. Regulation will go up after the merger if the industry incentive to regulate, $\frac{d}{dR}\Pi$, increases with the merger.

The marginal effect of regulation on industry profit before the merger is given by (7). The marginal effect after the merger is

$$\begin{aligned} \frac{d}{dR}\Pi &= \frac{d\pi_{12}}{dR} + (m-2)\frac{d\pi_i}{dR} + (n-m)\frac{d\pi_j}{dR} \\ &= \frac{2(n-1)(A+R-1)}{n^2} + \frac{2s}{n^2} - \frac{2d(n-m)}{n^2}. \end{aligned} \tag{8}$$

By taking the difference between the two marginal effects (8) and (7), we have

$$\begin{aligned} &\frac{2(n-1)(A+R-1)}{n^2} + \frac{2s}{n^2} - \frac{2d(n-m)}{n^2} - 2\frac{n\,(A+R-1)-d(n-m)}{(n+1)^2} \\ &= 2\frac{n^2-n-1}{n^2\,(n+1)^2}\,(A+R-1) + \frac{2s}{n^2} - \frac{2d(1+2n)(n-m)}{n^2(1+n)^2}. \end{aligned}$$

We can derive several insights from the previous expression. First, and as expected, the whole expression goes down to zero when n is very large, as there is no lobbying with or without a merger in an industry that approximates perfect competition. Instead, lobbying effects get larger as the industry becomes more concentrated. Second, in case of a merger in an industry that starts in a symmetric state and with no synergies ($s = d = 0$), only the first term matters, which is positive since $n \geq 2$. Thus, the merger always increases lobbying. Third, when the merger involves synergies ($s > 0$) lobbying incentives increase even further; see the second term. Last, the presence of asymmetric firms ("large" vs "small", $d > 0$) is a countervailing force; see the last term. This can just dilute the effect, not reverse it, as we assumed that d has to be small enough for the existence of an interior solution.

Thus, the merger increases R^* as well as $\sum_{i=1}^{\tilde{n}} \hat{t}_i$, where $\tilde{n} = n$ pre-merger and $\tilde{n} = n-1$ post-merger. This is because, if R^* increases, then $\sum_{i=1}^{\tilde{n}} \hat{t}_i = w\frac{R^{*2}}{2}$ also increases (the grand coalition transfer is binding also after the merger). A merger increases total lobbying activity by the whole industry. This leads to a generalization of Proposition 1.

We conclude by briefly considering the case of a merger that involves "small firms". The

profit function of the merged firm is

$$\pi_{12} = (A + R - Q)\, q_{12} - (1 + d - s)\, q_{12}.$$

The analysis mirrors the previous one, only some expressions change. The Cournot equilibrium is

$$q_{12} = \frac{A + R - 1 - d(1 + m) + s(n - 1)}{n},$$

$$q_i = \frac{A + R - 1 + d(n - m - 1) - s}{n},$$

$$q_j = \frac{A + R - 1 - d(m + 1) - s}{n}.$$

Total industry profits are $\Pi = \pi_{12} + m\pi_i + (n - m - 2)\pi_j$. The marginal effect of regulation on industry profit before the merger is unchanged and given by (7). After the merger, the marginal effect is

$$\begin{aligned} \frac{d}{dR}\Pi &= \frac{d\pi_{12}}{dR} + m\frac{d\pi_i}{dR} + (n - m - 2)\frac{d\pi_j}{dR} \\ &= \frac{2(n-1)(A + R - 1)}{n^2} + \frac{2s}{n^2} - \frac{2d(n - m - 1)}{n^2}. \end{aligned}$$

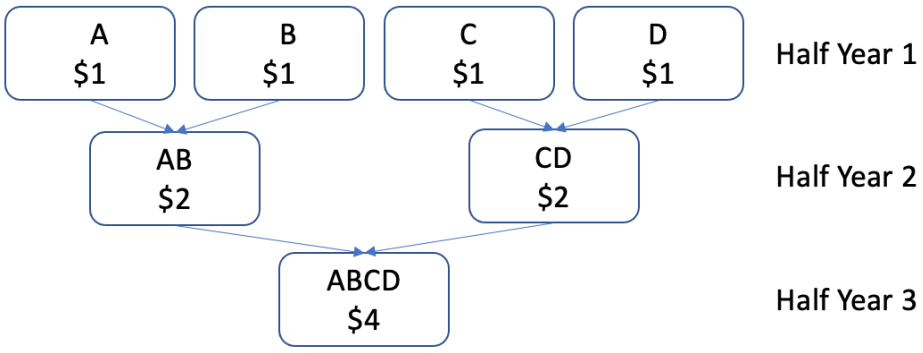
Compared to (8), the first two terms of this expression are identical (as they do not reflect differences in costs), while the only difference comes from the last term involving cost asymmetries d. This term is smaller in size now. Hence asymmetries produce more muted effects when mergers involve "small" firms than when they involve "large" firms.

B Example of a Composite Firm

We show a visual example of a composite firm that starts off as four distinct component firms (A-D) and merges into one over three periods (half years in our sample). Figure B1 shows the evolution of this composite firm from period 1 (top) to period 3 (bottom).

In this example, all component firms' revenue was normalized to \$1 for all periods, and there was no organic growth over the three periods. At the end when all four firms are merged, the final firm is worth \$4. This example keeps size/revenue constant for clarity; our actual data include organic growth. In the example, the MergerIndex_{it} varies across the three periods, which we can measure either as a reduction in the number of independent, as-yet-unmerged firms within the composite ("# of component firms"), or as an increase in the HHI index (this is the HHI of the composite firm, as formally described in Appendix M).

Figure B1: **Graphical Representation of Composite Firm "ABCD"**



Notes: This figure shows a four companies eventually merging into one over two periods after a total of three mergers.

Table B1: **Tabular Representation of Figure B1, Composite Firm "ABCD"**

HalfYearID	CompositeFirmID	Total Revenue (Size)	MergerIndex_{it}		
			# of Component Firms	HHI Index	
1	"ABCD"	\$4	4	2,500	$=(1/4)^2 \times 4 \times 10K$
2	"ABCD"	\$4	2	5,000	$=(1/2)^2 \times 2 \times 10K$
3	"ABCD"	\$4	1	10,000	$=(1/1)^2 \times 1 \times 10K$

Notes: This table represents the composite firm graph and merger sequence in Figure B1 in panel format.

C Codifying Multi-Merger Firms

As described in Section 4, our composite firm representation is particularly helpful for analyzing multi-merger firms. Mergers are relatively rare. However, among companies that *do* merge with others in our sample, 42% are involved in multiple mergers or acquisitions. This number rises to 68% if unlisted companies are included. Multi-merger firms are particularly common among larger companies that may be the source of important political and/or economic influence. Composite firms with more than two components comprise 58% of all lobbying spend (83% if unlisted companies are included). Such firms are often both targets and acquirers in the same sample.

Multi-merger firms present a data representation challenge. More generally, analysis of networks featuring merging nodes is rare in any network setting. Hernandez and Menon (2018) examine "node collapse" through simulations. Our approach of building a "composite node" (in our case, a composite firm) for handling this problem may have applications in other empirical settings featuring merging nodes.

In standard datasets of corporate mergers, target firms disappear after an acquisition. How-

ever, the target firm has not disappeared, it has been joined into a larger entity. Some researchers drop the target firm from analysis entirely, and focus only on the outcomes of the acquiring firm (both before and after the merger). This is problematic in settings like our model, where researchers want to study changes in the combined output outcomes of both firms (compared to pre-trends before the merger).

In addition, if one drops a target firm entirely then the target's own prior acquisitions (as an acquirer) would also be dropped. As described above, this would remove a large volume of potentially important activity. One could also keep the targets, and represent them as targets in some acquisitions and acquirers in others. However, the double-appearance of these firms would need to be accounted for in standard error clustering.

Our composite firm representation addresses these issues. Rather than dropping firms or double-counting them, we create a unit of analysis (the composite firm) that can represent multi-merger firms, single-merging firms, and non-merging firms. We can track internal changes to the composition of composite firms over time, and cluster standard errors around these composites.

D Additional Information: Data Sources

As described in Section 4, our dataset brought together four separate datasets: 1) financial data from Compustat, 2) a dataset about mergers from SDC Platinum, 3) a lobbying dataset from *LobbyView*[32] (Kim, 2018), and 4) corporate PAC contribution data from the Center for Responsive Politics' *OpenSecrets* project.[33] Below we list additional details about each dataset, and in Section D.5 we show how these datasets were merged together.

D.1 Merger Data and the Composite Firm Graph

Our composite firm database uses Thomson Reuters' SDC Platinum database of acquisitions and mergers. SDC Platinum contains the universe of global M&A transactions and is used in many academic papers about M&As (Blonigen and Pierce, 2016; Matvos and Ostrovsky, 2008; Rossi and Volpin, 2004).[34] For each acquisition, SDC Platinum identifies the acquirer, target and dates associated with the merger.[35] The date variables are particularly important in our analysis as they allow us to use pre-/post- variation in merger status.

This section includes an overview, and readers interested in even more detail can see the procedures in Appendix E. The procedure takes the above merger dataset and a date. For each underlying component firm, we identify a set of sibling firms who are connected through a

[32] https://www.lobbyview.org/

[33] https://www.opensecrets.org/bulk-data/

[34] Barnes et al. (2014) audit the accuracy and completeness of the SDC Platinum database and find positive results, particularly for the sample dates and for large companies that we analyze in this paper. Bollaert and Delanghe (2015) evaluate other sources of merger data, including Zephyr (https://zephyr.bvdinfo.com/) and also find positive results for SDC.

[35] The SDC dataset also includes other variables (such as the date of the merger announcement) as well as non-merger events such as rumored mergers. We do not use these in our analysis.

merger or acquisition happening *before the specified date*. This procedure is "transitive" in the sense that if Firm A is bought by Firm B, which is then purchased by Firm C, then A is not only siblings with B, but also with C. Together, they form a composite firm which we can call "ABC."

We run the procedure using the final date of the sample. This assembles composites using all connections between firms at any point during our sample. We use this set of 12K composite firms as the i variable in our $i \times t$ panel.

We then measure the evolution of each composite firm over time. To measure this, we run the procedure in Appendix E for each half-year (the t dimension of our panel) in our sample. This produces a dataset that connects each component firm j to its eventual parent i, as well as to its intermediate parent k at time t.

The intermediate parent k is a potentially smaller composite firm that eventually merges into the main composite firm. In cases towards the end of our sample, the intermediate parent k is the final composite firm. Using these intermediate steps, we calculate the change in concentration over time. Our simplest measure of concentration is a count of the number of intermediate firms that still remain un-merged with each composite i at each time t. This variable consists of integers that decrease by 1 with each successive merger.

D.2 Lobbyview

LobbyView data have been used in several other papers (Bombardini and Trebbi, 2020; Ellis and Groll, 2018; Huneeus and Kim, 2018). Lobbying disclosures are required on a half-year basis (quarterly after 2008). The disclosures are made on forms that *LobbyView* converts into structured, machine-readable data.[36] Importantly, *LobbyView* matches companies not only on its name, but also to a structured identifier that we can merge with our other data.

D.3 OpenSecrets

Like *LobbyView*, the *OpenSecrets* project takes government disclosures and standardizes them into machine readable format. The *OpenSecrets* process of standardization includes a greater level of manual review than *LobbyView*. Coverage spans the 1998 to 2018 electoral cycles. Campaign contributions include contributions from companies' PAC, as well as contributions by employees or owners of the organizations, or these individuals' family members. Before the Citizens United decision in 2010, companies could not directly donate to political campaigns. Afterwards, companies could donate directly to "Super PACs" (PACs with greater spending discretion), and these contributions are included in our dataset.

[36] An example of a lobbying disclosure report can be viewed here.

D.4 Industry Associations

To be classified as a trade association, we require that a lobbying group a) not be *uniquely* linked to a specific company, and b) not be classified by OpenSecrets as an "Ideology/Single-Issue" group. In addition, the group would have to meet at least one of the following three criteria:

1) Appears in FEC Committee Data categorized as a trade association,[37] or

2) Appears in the Directory of Associations dataset,[38] or

3) Appears in IRS database of non-profits, with activity codes relating to industry, business or professional associations.[39]

We used text matching to match the names exactly (after removing common, non-identifying words and standardizing abbreviations). While the data sources above are not necessarily comprehensive, they give us broad coverage of industry associations.

The procedure above delivers a set of industry and trade associations, each with an industry identifier that uses the hand-coded OpenSecrets industry classification system. Our sample includes ≈ 60 industries in the OpenSecrets classification system.[40] When necessary, we map our trade association data to other industry classifiers using a crosswalk file developed by users of the OpenSecrets data.[41]

D.5 Merging the Datasets Together

Our merging procedure mostly used standardized identifiers (GVKEY and CUSIP) with the exception of the text-matching used to incorporate the *OpenSecrets* data.

1) *Compustat* identifies companies sing both CUSIP and GVKEY identifiers, thus allowing linkages with other data below using either key.

2) The SDC platinum data identify both target and acquiring companies using CUSIP identifiers. Before integrating this data, we added the composite firm identifiers using the procedure described in Appendix E.

[37] https://www.fec.gov/data/browse-data/?tab=bulk-data, documentation at https://www.fec.gov/campaign-finance-data/committee-master-file-description/. Each committee has an "interest group category" containing one of six categories, one of which (T) represents "Trade association."

[38] https://directoryofassociations.com/, this is a database of about 38K associations.

[39] The IRS nonprofit database can be found at https://www.irs.gov/charities-non-profits/tax-exempt-organization-search-bulk-data-downloads. Each nonprofit can list up to three activity codes as its main objective. Non-profits that listed activity codes 200-249 in their three codes were classified as trade associations. Activity codes 200-229 corresponds to "Business and Professional Organizations." Codes 230-249 correspond to "Farming and Related Activities" which contains industry groups for agriculture. No other set of IRS industry codes corresponded to trade organizations. The full list of activity codes can be seen https://www.irs.gov/pub/irs-tege/p4838.pdf.

[40] The industry categorizations are visible at this URL. In total there are approximately 100 industries, but some industries have no constituency in our Compustat sample of (mostly) public firms.

[41] https://groups.google.com/g/opensecrets-open-data/c/nXYSeFrtwxk/m/NXRoVQhoBwAJ

3) *LobbyView* indexes companies using GVKEY identifiers. We link *LobbyView*'s data with other datasets using the GVKEY/CUSIP crosswalk from Compustat.

4) Unlike *LobbyView*, *OpenSecrets* data does not index companies by a standardized numeric identifier, but instead by standardizing company names. We merged this data into the other datasets by using a text matching procedure we validated by manual inspection.

E Procedure for Creating the Composite Firm Graph

The procedure below takes the SDC Platinum merger dataset described in the main paper (and in Appendix D above) and a date.

We begin by removing all M&A observations after the specified date. Then we use the SDC data to create a graph that connects all merged firms before that date. Although this graph's edges have a direction (i.e., target $\rightarrow$ acquirer), for our purposes in this section an undirected graph connecting targets and acquirers will suffice.

We then find the connected components of this graph. A connected component is a maximal connected subgraph. All nodes within the subgraph are reachable from every other node in the subgraph, either directly or through paths. However, all nodes in the component subgraph cannot necessarily reach all nodes in the overall graph. In short, a connected component is an "island" of nodes that are connected with each other, but not the rest of the graph.

In our setting, a composite firm is a collection of firms (nodes) that are interconnected to each other by mergers (edges). These connections can either be direct (two firms merging) or through paths (A merging with B, which previously merged with C). The members of these clusters of course typically are not necessarily connected to all other firms (directly or through paths), and thus each cluster of inter-merged firms is an isolated, connected subgraph of the larger merger graph.

Connected components of a graph can be calculated using efficient, well-known algorithms such as the Hopcroft and Tarjan (1973) algorithm. We used the implementation provided by the igraph scientific computing package (Csardi and Nepusz 2006, `http://igraph.org`), Version 1.2.6 (published October 6, 2020).

F Additional Descriptive Statistics

Table F1 reports descriptive statics for firms who lobby, to complement the descriptive statics of Table 1 and 2. Table F2 shows some raw correlations in the data.

Table F1: **Descriptive Statistics: Firms Who Lobby**

	Mean	Std.Dev	Min	P25	P50	P75	Max
Years in Sample	14.36	5.78	0.50	9.50	18.50	19.00	19.00
Avg Revenue ($10M, per Half Year)	274.98	902.06	0.00	2.84	38.04	164.74	18359.17
Lobby Spend ($1K, per Half Year)	335.09	1355.80	0.16	7.11	28.96	138.00	40365.12
Lobbied at all (per Half Year)	0.50	0.35	0.03	0.16	0.45	0.86	1.00
In-House Lobby Spend ($1K, per Half Year)	225.06	1123.69	0.00	0.00	0.00	40.92	37828.85
Lobbying Intermediary Spend ($1K, per Half Year)	110.03	333.07	0.00	6.05	23.34	78.42	7182.46
Lobbied at all (ever)	1.00	0.00	1.00	1.00	1.00	1.00	1.00
PAC Donations ($1K, per Half Year)	13.05	60.22	0.00	0.00	0.00	4.57	1903.46
PAC Donations$>$ 0 (per Half Year)	0.25	0.37	0.00	0.00	0.00	0.50	1.00
PAC Donations$>$ 0 (Ever)	0.38	0.48	0.00	0.00	0.00	1.00	1.00
Individual Donations ($1K, per Half Year)	2.74	8.89	-1.75	0.00	0.13	1.61	157.41
Individual Donations$>$ 0 (per Half Year)	0.19	0.20	0.00	0.00	0.13	0.32	1.00
Individual Donations$>$ 0 (Ever)	0.71	0.45	0.00	0.00	1.00	1.00	1.00
Ever M&A	0.32	0.47	0.00	0.00	0.00	1.00	1.00
# of Component Firms	1.99	2.69	1.00	1.00	1.00	2.00	39.00

Notes: This table displays simple summary statistics for all composite firms in our sample that lobby in at least one period.

Table F2: **Descriptive Statistics: Correlations**

	Years	Revenue	Lobby	PAC	Individual	Ever Merged
Years	1					
Revenue	0.16***	1				
Lobby	0.13***	0.49***	1			
PAC	0.12***	0.49***	0.84***	1		
Individual	0.18***	0.42***	0.49***	0.49***	1	
Ever Merged	0.37***	0.21***	0.17***	0.17***	0.22***	1

Notes: This table displays raw correlations between some of the key variables in our analysis. Our panel dataset is described in Section 4, and composite firms are defined at the beginning of Section 3.

G Merger Effects by Size

In Table G1, we introduce an interaction term for "above a Nth Percentile Threshold." We examine percentiles from 45th and above (increments of 5). The results show consistently that, for all thresholds, the merger effect is larger for firms above the threshold. The coefficient of the interaction term also increases with size, as expected given our previous discussions. These differences are all statistically significant, except for one, and that is the 50th percentile (the p-value for this interaction was 0.11). We also examined a cutoff of the 51st percentile, and this was also statistically significant ($p = 0.07$).

Table G1: **Size, Mergers and Lobbying**

	(1) Lobby Amount	(2) Lobby Amount	(3) Lobby Amount	(4) Lobby Amount	(5) Lobby Amount	(6) Lobby Amount	(7) Lobby Amount	(8) Lobby Amount	(9) Lobby Amount	(10) Lobby Amount
# Component Firms	-6,688	-11,773	-5,457	-3,595	1,805	-856	5,629	4,593	10,509	5,378
	(20,292)	(17,894)	(12,555)	(10,572)	(8,027)	(7,487)	(7,390)	(7,644)	(8,438)	(8,819)
# Component Firms × Above Size Threshold	-62,720*	-57,684	-64,433**	-66,529**	-72,715**	-70,534**	-78,109**	-78,500**	-88,741**	-91,074**
	(37,896)	(36,252)	(32,728)	(32,296)	(32,634)	(32,504)	(33,767)	(35,141)	(38,475)	(42,874)
Threshold	45th Percentile	50th Percentile	55th Percentile	60th Percentile	65th Percentile	70th Percentile	75th Percentile	80th Percentile	85th Percentile	90th Percentile
Additional Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Sample	Full	Full	Full	Full	Full	Full	Full	Full	Full	Full
Observations	223,022	223,022	223,022	223,022	223,022	223,022	223,022	223,022	223,022	223,022
R^2	.83	.83	.83	.83	.83	.83	.83	.83	.83	.83

Notes: This table shows results from our panel event study specification (Section 5) on lobbying and PAC donations. Results are separated by firm size (measured by revenue), above and below various percentiles. Lobbying and PAC donation outcomes are regressed against composite-firm fixed effects, composite firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. For additional discussion of this specification, see "Heterogeneity: Size and Similarity" in Section 5. Standard errors are clustered by composite firms.

H Anticipation

As mentioned in Section 5, a key identification challenge is the possibility of pre-merger increases in lobbying activity. Firms could initiate this form of pre-merger lobbying to influence the merger's review by regulators. Alternatively, firms may anticipate a positive review, and begin coordinating and integrating lobbying activity before the official merger date. To address this, we add terms to Equation (3) to capture the change in each composite firm's MergerIndex_{it} between the current period and one period in the future. We denote these as $\Delta\ \text{MergerIndex}_{it}$, t+1. Our additional term measures lobbying one period *ahead* of a merger. Table H1 presents these results. Compared to our results without this term in Table 3, we see approximately the same magnitudes.

In addition, we study mergers that fall above a key policymaking threshold in the United States. The Hart-Scott-Rodino (HSR) Act exempts mergers below certain size thresholds from prenotification requirements (Wollmann, 2019). The threshold was $200M from 2001-2005,

Table H1: **Merger Anticipation Effects**

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
# Component Firms	-77,995**	-72,040***	-4,780*	-4,016
	(33,742)	(27,897)	(2,452)	(2,533)
Δ # Component Firms, $t+1$	-9,265	-10,381	-457	775
	(21,880)	(27,683)	(1,486)	(2,049)
Additional Controls		Y		Y
Observations	210,344	210,325	210,344	210,325
R^2	.79	.83	.32	.47

Notes: This table shows results from our panel event study specification (Section 5) on lobbying and PAC donations. We add an anticipation as described in Appendix H. The additional term, Δ # Component Firms$_{it}$, t+1, measures lobbying one period *ahead* of a merger. Lobbying and PAC donation outcomes are regressed against composite-firm fixed effects, composite firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. Standard errors are clustered by composite firms.

and then was indexed to GDP growth.[42] We have obtained the deal sizes and adjusted thresholds for the years in question.[43]

In Table H2, we study only deals *above* the HSR threshold. These are deals that, in principle, could be subject to some antitrust scrutiny. To do this, we label every deal in our data as above or below the (contemporaneous, GDP-adjusted) HSR threshold for the date of the deal's closing. We then draw a window around each HSR deal of width six years (three years before and after the HSR event window). As Table H2 shows, we do not find anticipatory results, which is consistent with the relatively low level of merger scrutiny during this time. Our results are robust to including window-lengths of four or eight years as well.

[42] As Wollmann (2019) wrote, "all deals are reportable whose transaction sizes exceed $200 million."
[43] We thank Thomas Wollmann for providing us with the HSR thresholds over time.

Table H2: **HSR Merger Anticipation Effects**

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
# Component Firms	-328,554	-330,862	-23,240	-24,611
	(233,367)	(246,684)	(19,799)	(23,483)
Δ # Component Firms, $t+1$	50,000	139,762	12,485	45,129
	(157,680)	(238,403)	(15,321)	(44,869)
Additional Controls		Y		Y
Observations	169,786	169,771	169,786	169,771
R^2	.79	.83	.29	.51

Notes: This table shows results from our panel event study specification (Section 5) on lobbying and PAC donations. We study only deals above the HSR threshold described in Wollmann (2019) and in the surrounding text. We add an anticipation as described in Appendix H. The additional term, Δ # Component Firms$_{it}$, t+1, measures lobbying one period *ahead* of a merger. Lobbying and PAC donation outcomes are regressed against composite-firm fixed effects, composite firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. Standard errors are clustered by composite firms.

I Heterogeneity of Extensive Margin Effect

In the tables below, we examine the heterogeneity of our extensive margin effects by firm size (Table I1) and by close/distant mergers (Table I2).

Table I1: **Heterogeneity by Firm Size (Panel Event Study)**

Panel A: Lobbying and PAC

	(1) Started Lobbying (Any)	(2) Started Lobbying (Any)	(3) Started PAC	(4) Started PAC
# Component Firms	.0018	-.003	.00023	-.015***
	(.015)	(.0041)	(.0063)	(.004)
Additional Controls	Y	Y	Y	Y
Sample	Below Median Revenue	Above Median Revenue	Below Median Revenue	Above Median Revenue
Observations	76,773	146,249	76,773	146,249
R^2	.84	.86	.88	.9

Panel B: In-House vs Outsourced

	(1) Started In-House Lobbying	(2) Started In-House Lobbying	(3) Started Outsourced Lobbying	(4) Started Outsourced Lobbying
# Component Firms	-.0071	-.013***	.0023	-.0039
	(.011)	(.0038)	(.016)	(.0039)
Additional Controls	Y	Y	Y	Y
Sample	Below Median Revenue	Above Median Revenue	Below Median Revenue	Above Median Revenue
Observations	76,773	146,249	76,773	146,249
R^2	.86	.88	.84	.86

Notes: This table shows results from our panel event study specification (Section 5) on lobbying and PAC donations. Results are separated by firm size (measured by revenue), above and below the median. Lobbying and PAC donation outcomes are regressed against composite-firm fixed effects, composite firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. For additional discussion of this specification, see "Heterogeneity: Size and Similarity" in Section 5. Standard errors are clustered by composite firms.

Table I2: **Extensive Margin: Close vs Distant Mergers**

Panel A: Lobbying and PAC

	(1) Started In-House Lobbying	(2) Started In-House Lobbying	(3) Started Outsourced Lobbying	(4) Started Outsourced Lobbying
# Component Firms	-.017***	-.019***	-.013**	-.015**
	(.0058)	(.0059)	(.0064)	(.0066)
# Component Firms × Unique NAICS	.0022***	.0022***	.0023***	.0017*
	(.0006)	(.00063)	(.00085)	(.00097)
Additional Controls		Y		Y
Observations	223,043	223,022	223,043	223,022
R^2	.86	.88	.83	.86

Panel B: In-House vs Outsourced

	(1) Started In-House Lobbying	(2) Started In-House Lobbying	(3) Started Outsourced Lobbying	(4) Started Outsourced Lobbying
# Component Firms	-.017***	-.019***	-.013**	-.015**
	(.0058)	(.0059)	(.0064)	(.0066)
# Component Firms × Unique NAICS	.0022***	.0022***	.0023***	.0017*
	(.0006)	(.00063)	(.00085)	(.00097)
Additional Controls		Y		Y
Observations	223,043	223,022	223,043	223,022
R^2	.86	.88	.83	.86

Notes: This table shows results from our panel event study specification (Section 5) on lobbying and PAC donations. We include interactions with how many industries are included among the merging firms using NAICS codes. Lobbying and PAC donation outcomes are regressed against composite-firm fixed effects, composite firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. For additional discussion of this specification, see "Heterogeneity: Size and Similarity" in Section 5. Standard errors are clustered by composite firms.

J Additional Robustness Checks

J.1 Market-to-Book Ratios

As described in the text, we address the possible problem of misvaluation: Time-varying misvaluation could drive both M&A and political spending. We created a market-to-book ratio. Results in Tables J1 show that our findings are the same qualitatively (and are also similar in the point estimate) when controlling for this term.

Table J1: **Market-to-Book Controls**

Panel A: Panel Event Study Design

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
# Component Firms	-75,431**	-70,559**	-4,553*	-3,821
	(35,714)	(29,254)	(2,351)	(2,517)
Additional Controls		Y		Y
Observations	219,454	219,433	219,454	219,433
R^2	.79	.83	.32	.47

Panel B: Exposure Design

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
# Component Firms	-183,653***	-246,106***	-14,848*	-12,667**
	(71,141)	(80,321)	(8,064)	(6,358)
Controls		Y		Y
F-Statistic	78	64	78	64
Observations	213,186	213,186	213,186	213,186

Notes: This table shows results on lobbying and PAC donations using two main specifications (panel event study and exposure design). All regressions include composite firm size, composite firm fixed effects and time-period fixed effects. Market-to-Book controls are included in all specifications. Additional controls are described in Section 5. Standard errors are clustered by composite firms.

J.2 Industry $\times$ Time Fixed Effects

One possible threat to exogeneity might be the question of antitrust enforcement. A merger may be timed in such a way to get a favorable treatment by the anti-trust authority. Lobbying helps reduce the risk of anti-trust investigation when such a risk is high. One suggested way to address this possibility is to include fully flexible year$\times$industry fixed effects, which would control for the level of anti-trust investigations at year$\times$ industry level.

We have added year×industry fixed effects to the firm-level analysis and present the results of our main specifications in Table J2 (both the panel event study and the exposure design specification). These are equivalent to Tables 3 and 10 in the main paper. The Industry × Year controls appear in all columns. As the coefficients show, our results are qualitatively unchanged from the addition of this control, and the coefficients are also of comparable magnitude.

Table J2: **Including Industry × Half Year FEs**

Panel A: Panel Event Study Design

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
# Component Firms	-67,894**	-69,257**	-4,000*	-4,019
	(34,102)	(29,763)	(2,299)	(2,735)
Additional Controls		Y		Y
Observations	223,022	223,022	223,022	223,022
R^2	.8	.84	.33	.48

Panel B: Exposure Design

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
# Component Firms	-144,460***	-174,858***	-10,980*	-9,871*
	(55,714)	(58,693)	(6,300)	(5,755)
Controls		Y		Y
F-Statistic	133	95	133	95
Observations	216,563	216,563	216,563	216,563

Notes: This table shows results on lobbying and PAC donations using two main specifications (panel event study and exposure design). All regressions include composite firm size, composite firm fixed effects and time-period fixed effects as well as fully-flexible industry× time period fixed effects. Additional controls are described in Section 5. Standard errors are clustered by composite firms.

K Diagnostics of the Exposure Design Instruments

Compliers & Instrument Strength. Compliers to the instrument are composite firms that contain mergers, but whose timing of mergers are sensitive to waves. Many other mergers happen on a timeline unaffected by these waves, or never happen at all; these are not identified by our instrument. In Table K1, we assess whether instrument compliance differs by size (measured in revenue). We find that large companies are more likely to be compliers to our instrument; as a result, our IV estimand will capture effects on companies that are larger than the average company in our sample.

This property of the instrument also limits our ability to do heterogeneity analysis on the

main effects of mergers, because our instrument is weaker for smaller companies. Overall, our instrument has a strong first stage in both implementations, featuring strong F statistics (as measured using the metrics proposed by Montiel-Olea and Pflueger 2013 and Kleibergen and Paap 2006; Stock and Yogo 2005); see Table 9 for first-stage coefficients.

Table K1: **IV Compliance Heterogeneity: Firm Size in Revenue**

	(1) # Component Firms (std)	(2) # Component Firms (std)
Mergers IV (B_{it}, std)	-.0043	.019
	(.019)	(.025)
Instrument × Large Firm	-.32***	-.34***
	(.025)	(.029)
Controls		Y
Observations	216,563	216,563
R^2	.65	.69

Notes: All variables have been standardized, and regressions include half-year fixed effects and controls for revenue. Standard errors are clustered by composite firms.

L Political Risk Analysis

L.1 Details of Investor Call Sample

Our investor call data comes from the method developed by Hassan et al. (2019). Data from this measure are distributed at `https://firmlevelrisk.com`. Political risk measures are available only for the subset of firms that have regular investor calls. The original format of this data is indexed in CUSIP identifiers. We merged these into our composite firms format using the following rules. Mergers in our sample fell into three categories:

1) Mergers where all merging firms were in the investor calls. In this case, the composite firm was included in the investor call sample. We measured the overall political risk for the composite firm i at time t as the revenue-weighted average of all the component firms.

2) Mergers where some (but not all) of the merging firms held regular investor calls. This occurred when a large firm with regular calls acquired a smaller firm that did not have regular calls. We measured the overall political risk for the composite firm i at time t as the revenue-weighted average of all the component firms that held calls. We included these instances in the investor call sample.

3) Finally, mergers where none of the merging firms were in the investor call subsample. We excluded these firms from our investor call sample.

Table L1 contains descriptive statistics for firms that are in our investor call sample, compared to ones that are not.

Table L1: **Descriptive Statistics: Investor Call Sample**

	Not in Sample	In Sample	Difference
Years in Sample	6.48	13.59	-7.11***
Avg Revenue ($10M, per Half Year)	19.17	154.38	-135.21***
Lobby Spend ($1K, per Half Year)	8.69	148.90	-140.21***
Lobbied at all (per Half Year)	0.03	0.19	-0.16***
Lobbied at all (ever)	0.07	0.36	-0.30***
In-House Lobby Spend ($1K, per Half Year)	3.85	104.17	-100.32***
Lobbying Intermediary Spend ($1K, per Half Year)	4.85	44.73	-39.89***
PAC Donations ($1K, per Half Year)	0.29	6.39	-6.10***
PAC Donations>0 (per Half Year)	0.01	0.12	-0.11***
PAC Donations>0 (Ever)	0.02	0.20	-0.17***
Individual Donations ($1K, per Half Year)	0.11	1.61	-1.50***
Individual Donations>0 (per Half Year)	0.02	0.14	-0.12***
Individual Donations>0 (Ever)	0.13	0.63	-0.50***
# of Component Firms in Compustat	1.05	1.65	-0.60***

Notes: This table displays simple summary statistics for all composite firms and all periods in our sample for which we have measures of political risk (Hassan et al., 2019). This is about 1/3 of our full panel sample. Our panel dataset is described in Section 4, and composite firms are defined at the beginning of Section 3. Section 7 discusses our use of political risk scores.

L.2 Political Risk Results

In this section we present the results for the main outcomes (Table L2) and additional/supplementary outcomes (Table L3). Because our Bartik-like instrument loses strength in this subsample, we present only the panel event study specifications.

Table L2: **Firm-Level Political Risk**

	(1) Lobby Amount	(2) PAC Contribs	(3) Political Risk	(4) Econ. Policy Political Risk	(5) Political Sentiment
# Component Firms	-40,267	-1,539	-.0043	-.0069	-.0099
	(27,186)	(1,643)	(.0082)	(.0077)	(.01)
Additional Controls	Y	Y	Y	Y	Y
Observations	69,789	69,789	69,789	69,789	69,789
R^2	.88	.51	.59	.58	.6

Notes: This table examines firm-level political risk. We have firm-level political risk scores for approximately 1/3 of our sample using the method in Hassan et al. (2019). We use these values as outcomes. For additional discussion, see Section 7. Standard errors are clustered by composite firms.

Table L3: **Firm-Level Political Risk from Earnings Calls (Additional Measures)**

	(1) Environment	(2) Trade	(3) Institutions	(4) Health	(5) Security & Defense	(6) Taxes	(7) Technology
# Component Firms	-.00059	-.013**	.00092	.0016	-.011	-.0033	-.005
	(.0068)	(.0065)	(.0076)	(.0055)	(.0071)	(.006)	(.0094)
Additional Controls	Y	Y	Y	Y	Y	Y	Y
Observations	69,789	69,789	69,789	69,789	69,789	69,789	69,789
R^2	.51	.46	.55	.52	.54	.51	.54

Notes: This table examines firm-level political risk. We have firm-level political risk scores for approximately 1/3 of our sample using the method in Hassan et al. (2019). We use these values as outcomes. For additional discussion, see Section 7. Standard errors are clustered by composite firms.

M Empirical Results using HHI as Merger Index

In this appendix we employ the Herfindahl-Hirschman Index (HHI) of the composite firm as an alternative measure for MergerIndex_{it}, instead of the simple count of the number of independent firms within each composite firm that we used in the main text.

The HHI is defined as the sum of the squared relative revenue share of each independent firm within the composite firm, or $\text{HHI}_{it} = 10K \sum_{f \in \mathcal{F}_i} [x_{ft}^2]$, where $x_{ft} = r_{ft} / \sum_{f \in \mathcal{F}_i} r_{ft}$ and r_{ft} is revenue. It is a term that can take values between 0 and 10,000. An example is provided in Table B1. When a merger is completed, the number of intermediate parents shrinks, and the revenue share is larger inside the intermediate parent that absorbed one of the firms, resulting in a higher HHI. The HHI takes into account that merging entities can be of very different sizes.

Results are shown in the tables below and are qualitatively similar to those in the main body of the paper. Note that an increase in concentration *reduces* the Merger Index_{it} in the main text, while HHI would *increase* it.

Table M1: **Results, Panel Event Study**

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
HHI	6*	7.3*	.37	.39
	(3.5)	(4.1)	(.28)	(.35)
Additional Controls		Y		Y
Observations	223,043	223,022	223,043	223,022
R^2	.79	.83	.32	.47

Notes: This table shows results on lobbying and PAC donations using our panel event study specification as described in Section 5. We use HHI as the merger index. Standard errors are clustered by composite firms.

Table M2: **Heterogeneity (Firm Size in Revenue): Panel Event Study**

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
HHI	.65**	13	.014**	.73
	(.26)	(8)	(.0069)	(.67)
Additional Controls	Y	Y	Y	Y
Sample	Below Median Revenue	Above Median Revenue	Below Median Revenue	Above Median Revenue
Observations	76,773	146,249	76,773	146,249
R^2	.55	.84	.72	.47

Notes: This table shows results on lobbying and PAC donations using our panel event study specification in described in Section 5. We use HHI as the merger index. We include interactions with firm size. To measure size, we use revenue. In particular, we sum all revenue across the entire sample for each composite firm, and examine companies above and below the median on this dimension. For additional discussion of this specification, see Section 5. Standard errors are clustered by composite firms.

Table M3: **Close vs Distant Mergers (HHI)**

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
HHI	5.9	6.2	-.23	-.3
	(10)	(9.6)	(.47)	(.62)
HHI × Unique NAICS	-2.6	-1.8	.28	.33
	(9.5)	(8.7)	(.5)	(.62)
Additional Controls		Y		Y
Observations	223,043	223,022	223,043	223,022
R^2	.79	.83	.32	.47

Notes: This table shows results on lobbying and PAC donations using our panel event study specification in described in Section 5. We use *HHI* as the merger index. We include interactions with how many industries are included among the merging firms using NAICS codes. For additional discussion of this specification, see Section 5. Standard errors are clustered by composite firms.

Table M4: **Results: Exposure Design**

	(1) Lobby Amount	(2) Lobby Amount	(3) PAC Contribs	(4) PAC Contribs
Composite firm HHI	125***	168***	9.8*	8.8*
	(46)	(52)	(5.6)	(4.7)
Controls		Y		Y
F-Statistic	119	78	119	78
Observations	216,563	216,563	216,563	216,563

Notes: This table shows results on lobbying and PAC donations using our exposure specification in described in Section 6. We use *HHI* as the merger index. Standard errors are clustered by composite firms.

Table M5: **Firm-Level Political Risk**

	(1) Lobby Amount	(2) PAC Contribs	(3) Political Risk	(4) Econ. Policy Political Risk	(5) Political Sentiment
HHI	24	.49	3.4e-06	6.6e-06	-4.0e-06
	(21)	(1.2)	(6.6e-06)	(6.2e-06)	(6.8e-06)
Additional Controls	Y	Y	Y	Y	Y
Observations	69,789	69,789	69,789	69,789	69,789
R^2	.88	.51	.59	.58	.6

Notes: This table examines firm-level political risk. We use *HHI* as the merger index. We have firm-level political risk scores for approximately 1/3 of our sample using the method in Hassan et al. (2019). We use these values as outcomes. For additional discussion, see Section 7. Standard errors are clustered by composite firms.

Table M6: **Firm-Level Political Risk from Earnings Calls (Additional Measures, HHI)**

	(1) Environment	(2) Trade	(3) Institutions	(4) Health	(5) Security & Defense	(6) Taxes	(7) Technology
HHI	-9.3e-07	.00001*	-1.9e-06	6.6e-06	3.3e-06	-1.8e-06	4.0e-06
	(5.2e-06)	(5.5e-06)	(5.1e-06)	(8.4e-06)	(5.9e-06)	(5.6e-06)	(6.1e-06)
Additional Controls	Y	Y	Y	Y	Y	Y	Y
Observations	69,789	69,789	69,789	69,789	69,789	69,789	69,789
R^2	.51	.46	.55	.52	.54	.51	.54

Notes: This table examines additional measures of firm-level political risk. We use *HHI* as the merger index. We have firm-level political risk scores for approximately 1/3 of our sample using the method in Hassan et al. (2019). We use these values as outcomes. For additional discussion, see Section 7. Standard errors are clustered by composite firms.

N Logs Specifications

Table N1: **All Firms, Panel Event Study**

	(1) Log(Lobby Amount+1)	(2) Log(Lobby Amount+1)	(3) Log(PAC Contribs+1)	(4) Log(PAC Contribs+1)
# Component Firms	-.11**	-.1**	-.032	-.016
	(.042)	(.045)	(.041)	(.042)
Additional Controls		Y		Y
Observations	223,043	223,022	222,936	222,915
R^2	.77	.81	.81	.84

Notes: This table shows results from our panel event study specification (Section 5) on lobbying and PAC donations. Lobbying and PAC donation outcomes are regressed against composite-firm fixed effects, composite firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. Standard errors are clustered by composite firms.

Table N2: **Heterogeneity by Firm Size (Panel Event Study)**

	(1) Log(Lobby Amount+1)	(2) Log(Lobby Amount+1)	(3) Log(PAC Contribs+1)	(4) Log(PAC Contribs+1)
# Component Firms	.38	-.095**	.0013	-.0066
	(.38)	(.046)	(.14)	(.043)
Additional Controls	Y	Y	Y	Y
Sample	Below Median Revenue	Above Median Revenue	Below Median Revenue	Above Median Revenue
Observations	76,773	146,249	76,767	146,148
R^2	.73	.81	.79	.84

Notes: This table shows results from our panel event study specification (Section 5) on lobbying and PAC donations. Results are separated by firm size (measured by revenue), above and below the median. Lobbying and PAC donation outcomes are regressed against composite firm fixed effects, composite firm size (measured in revenue), and time-period fixed effects. All regressions in this table contain the additional controls described in Section 5. For additional discussion of this specification, see "Heterogeneity: Size and Similarity" in Section 5. Standard errors are clustered by composite firms.

Table N3: **Heterogeneity: Close vs Distant Mergers (Panel Event Study)**

	(1) Log(Lobby Amount+1)	(2) Log(Lobby Amount+1)	(3) Log(PAC Contribs+1)	(4) Log(PAC Contribs+1)
# Component Firms	-.21***	-.21***	-.051	-.03
	(.064)	(.066)	(.079)	(.08)
# Component Firms × Unique NAICS	.02*	.019*	.0087	.0089
	(.011)	(.01)	(.0069)	(.0074)
Additional Controls		Y		Y
Observations	223,043	223,022	222,936	222,915
R^2	.77	.81	.81	.84

Notes: This table shows results from our panel event study specification (Section 5) on lobbying and PAC donations. We include interactions with how many industries are included among the merging firms using NAICS codes. Lobbying and PAC donation outcomes are regressed against composite firm fixed effects, composite firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. For additional discussion of this specification, see "Heterogeneity: Size and Similarity" in Section 5. Standard errors are clustered by composite firms.

Table N4: **Trade Associations, Panel Event Study**

	Log(Lobby Amount+1)	Log(Lobby Amount+1)	Log(PAC Contribs+1)	Log(PAC Contribs+1)
# Unmerged Firms	-.0039	-.0041	-.008*	-.014
	(.006)	(.013)	(.0045)	(.011)
Additional Controls		Y		Y
Observations	2,206	2,206	2,206	2,206
R^2	.69	.71	.78	.8

Notes: This table shows results on lobbying and PAC donations by trade associations using our panel event study specification described in Section 5. Outcomes are regressed against industry fixed effects, industry firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. Standard errors are clustered by industry.

Table N5: **Industry Analysis, Panel Event Study**

	Log(Lobby Amount+1)	Log(Lobby Amount+1)	Log(PAC Contribs+1)	Log(PAC Contribs+1)
# Unmerged Firms	-.0026	-.014	-.0086*	-.007
	(.0061)	(.0099)	(.005)	(.0079)
Additional Controls		Y		Y
Observations	2,206	2,206	2,203	2,203
R^2	.85	.86	.86	.88

Notes: This table shows results on lobbying and PAC donations at the industry level using our panel event study specification described in Section 5. Outcomes are regressed against industry fixed effects, industry firm size (measured in revenue), and time-period fixed effects. For columns with additional controls, these controls are described in Section 5. Standard errors are clustered by industry.

Table N6: **Exposure Design Results**

	(1) Log(Lobby Amount+1)	(2) Log(Lobby Amount+1)	(3) Log(PAC Contribs+1)	(4) Log(PAC Contribs+1)
# Component Firms	-.28	-.48	-.0066	-.092
	(.34)	(.38)	(.2)	(.22)
Controls		Y		Y
F-Statistic	136	107	135	107
Observations	216,563	216,563	216,461	216,461

Notes: This table shows results on lobbying and PAC donations using our instrumental variables specification described in Section 6 (first implementation). We instrument for the Merger Index$_{it}$ using a Bartik-like instrument that combines merger waves and exposure. All regressions control for composite firm fixed effects, composite firm size (measured in revenue), and time period fixed effects. For columns with additional controls, these are described in Section 5. IV diagnostics appear in Appendix K. Standard errors are clustered by composite firms.

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