Agency Breadth and Political Influence*

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Abstract

We study, theoretically and empirically, legislative influence over executive agencies, focusing on the breadth of agency responsibilities. We model interest groups, the legislature, and agencies. Politicians exert costly effort to influence agencies in exchange for interest groups' campaign contributions. Effort, however, can only be imperfectly targeted. When effort is spent on behalf of one group, some spills over to benefit other interest groups. This creates externalities of influence that are larger in broad agencies, deterring legislative influence. Empirically, we develop a novel lobbying-based measure of breadth and combine it with survey data on influence in 70 US federal agencies. Broad agencies report less influence, and we rule out several alternative explanations. These results are important for understanding how to insulate divisive tasks from political influence.

Keywords: Agency design; Money in politics; Regulatory politics; Regulatory capture; Special interest groups

JEL Classification Numbers: D72, D73, H11, K20

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1 Introduction

The division of responsibilities between electorally-accountable politicians and insulated bureaucrats is a classic issue in economics, and its tradeoffs have received substantial attention (Alesina and Tabellini, 2007; Besley and Coate, 2003; Maskin and Tirole, 2004; Stigler, 1971). Of course, bureaucrats are not fully insulated. Though they are not directly accountable to voters, they are accountable to the politicians (their political principals). This oversight can create a balance, allowing bureaucrats to draw on their technical expertise while preventing mission creep and over-regulation. However, it also creates the risk that bureaucratic agencies become politicized, especially in light of rising polarization in many legislatures.

Regardless of whether one believes political influence aligns bureaucrats' incentives with social welfare or distorts policy implementation, these normative concerns beg the following positive question: When are politicians most able to influence bureaucratic agencies? In this article, we attempt to understand how the design of an agency can make it more or less susceptible to political influence, focusing in particular on the range of tasks for which the agency is responsible. We show theoretically and empirically that the amount of political influence is greater for narrow agencies than for broader ones.

We model the interaction of politically-active interest groups, bureaucratic agencies, and a legislature. Agencies are responsible for regulating tasks, which affect a set of interest groups. Interest groups can lobby these agencies to adjust implementation and can use campaign contributions to contract with politicians to pressure the agency.[1] A key focus of our analysis is that agencies differ in how many tasks they are responsible for, which we call the *breadth* of the agency: A broad agency is one that performs tasks that affect more interest groups.

An interest group can "buy" Congressional influence over tasks that affect it, but our central assumption is that some of the pressure applied to that task spills over onto other tasks. For instance, suppose that Congress sought to reduce regulation of commercial foods. They might push for and approve an anti-regulation Commissioner for the Food and Drug Administration. This, however, will affect regulation of drugs in addition to food. If Congress seeks reduce television market concentration, they might increase funding for the Federal Communication Commission, who is responsibile for this form of anti-trust enforcement. However, the FCC does not have distinct line-item budget entries for television market activities, so some of the funding would be applied to regulating internet providers, which are a very different set of stakeholders.

[1]Since Grossman and Helpman (1994, 1996), the assumption that interest groups can enter into enforceable contracts with politicians using campaign contributions has been common but controversial. In Section 5.1 we discuss an alternative model without these contracts. There, agency breadth still causally affects legislative influence through a mechanism which is very different but (in our view) perfectly plausible. We then present evidence strongly rejecting that alternative mechanism as the explanation behind our main results.

Congress might hold oversight hearings to pressure the Federal Bureau of Investigations to reduce investigations of international banks (these investigations are costly in terms of banks' time and resources), but intensive Congressional oversight on this topic will surely redirect some of the agency's attention and resources away from civil rights investigations, counter-terrorism operations, and cyber-security, some of the Bureau's other responsibilities. Even detailed "limitation riders" in appropriations bills that narrowly circumscribe specific agency actions are often challenged in court, forcing them to be applied more broadly than originally intended (Zellmer, 1997). These forms of spillovers underpin our model.

Although all influence is costly for the legislature (including the unintended influence), we assume they cannot force other interest groups to "pay for" these spillovers, creating within-agency externalities from legislative influence. The more broad an agency is, the greater the externalities, and we show that the legislature will adjust the price of influence to compensate for these externalities. This price increase reduces the amount of influence interest groups buy and, in equilibrium, a particular task will experience less influence when embedded within a broader agency. This provides a novel channel by which politically sensitive tasks can be insulated from Congressional pressure.

We then use data from 70 US federal agencies to test our model's main prediction. We build a novel measure of breadth using lobbying disclosure reports. Guided by the model, we measure an agency's breadth as the number of interest groups observed lobbying the agency, with the core idea being that agencies lobbied by a broad set of interest groups must be responsible for a broad set of tasks.[2] We combine our measure of breadth with survey data in which high-level bureaucrats report how much influence Congress has over their agency. We show that broader agencies report significantly less Congressional influence. A one standard deviation increase in breadth is associated with a .43 standard deviation decrease in influence. We consider a range of alternative explanations and find no evidence that this correlation is explained by other agency characteristics, characteristics of the regulated groups, or reverse causality.[3]

In addition to testing for alternative explanations for our results, we also test for evidence supporting the underlying mechanisms of our model. To understand the mechanisms, we consider two variations of our basic model in which we allow the legislature to have intrinsic ideological preferences for different interest groups' objectives. We think this is realistic. For instance, depending on the party in control, Congress might support or oppose the agendas of

[2]We show our results are robust to changes in measuring lobbying, defining interest groups, addressing the timing or amount of lobbying, and using a non-lobbying based measure of breadth.

[3]To test whether the correlation is explained by other agency characteristics, we use every control that has been used in the (admittedly small) past literature, as well as several new ones. To test whether it is explained by characteristics of the regulated groups, we use instances where the same group is overseen by multiple agencies. To test whether it is explained by reverse causality, we use an instrumental variables strategy based on political circumstances at the time of agency creation.

labor unions or gun rights groups for reasons unrelated to campaign contributions.

We first discuss a variant in which agency breadth can reduce legislative influence *even without enforceable contracts*. As agencies take on a broader set of tasks, it is more and more likely that they oversee a blend of pro-left and pro-right groups. The legislature would like to support groups it likes and interfere with groups it dislikes, but spillovers make it impossible to perfectly target this influence. Broad agencies are more likely to be balanced (i.e., evenly divided between allied and opposing interest groups), which creates a disincentive for influence. We show evidence for a "law of large numbers" type result: As agencies become broad, they do indeed tend to become balanced. We also show that Congressional influence is lower for ideologically balanced agencies. These are necessary conditions for ideological balance to drive our results. We then directly test whether this explains the observed breadth/influence relationship. It does not. Even among extremely well-balanced agencies, broader ones report less influence.

In the second variant, we test whether interest groups' lobbying expenditures are as important as emphasized in our model. We show that when the Democrats (or Republicans, respectively) take control of Congress, interest groups aligned with the Democratic party (Republican party, respectively) spend more money lobbying agencies. In our framework, this is because each party is will provide more influence on behalf of the interest groups it is aligned with, and agency lobbying is a complement to Congressional influence.[4] More importantly, however, we show that this only happens for narrow agencies. When control of Congress shifts, there are large lobbying responses for narrow agencies, but lobbying of broad agencies does not respond to control of Congress. In our model, this is because the legislature exerts little influence over broad agencies, so the incentives to lobby the agency do not change when the legislature does.

In light of the battery of tests to which we subject our results and the support for the underlying mechanisms behind our model, we ascribe a causal interpretation to our results. Our core claim, then, is that agency breadth reduces legislative influence.

Our model is related to work on legislatures' decision to delegate authority to bureaucratic agencies (see Gailmard and Patty (2012) for a review). Most of this literature focuses on a single one-dimensional task, so there is no notion of breadth of agency responsibilities. Alesina and Tabellini (2008) and Ting (2002) are notable exceptions, though they focus on how features

[4]One interpretation for the complementarity comes from You (2017). In her model, Congress passes laws that create rents, then post-passage lobbying determines how those rents are divided among different actors. If a friendly legislature creates more rents, and lobbying of agencies affects the allocation of those rents, then a group's incentives to lobby agencies rise when the legislature is aligned. While it seems plausible that lobbying and influence would be substitutes rather than complements, our results suggest otherwise, as do others' (Tripathi, Ansolabehere, and Snyder, 2002).

of a task affect the decision to delegate it to an agency, rather than how features of an agency affect ex post influence after delegation. More generally, our model relates to the literature on linking incentive constraints, which typically finds that principals enjoy *more* (rather than less) influence when the agent is making more distinct decisions (Jackson and Sonnenschein, 2007; Frankel, 2016). The primary difference in the model here is that because of spillovers, the politician is not able to flexibly tie together the outcomes of each task. Our empirical results relate to recent work showing that Congressional influence increases as the number of oversight committees falls (Clinton, Lewis, and Selin (2014), whose survey-based measure of influence we use), as statutory features give them more mechanisms for influence (Selin, 2015), and as they are staffed with more political appointees (Berry and Gersen, 2017).[5]

The remainder of the paper is laid out as follows. In Section 2, we develop a simple model of the interactions between a political party, a series of government agencies, and a set of regulated interest groups. In Section 3 we discuss our data and empirical strategy. Section 4 presents our core results and identification tests. Section 5 explores the mechanisms of our model, and Section 6 concludes by discussing implications for future research and policy.

2 Theory

In this section, we present our core theoretical model. The basic model is kept simple to highlight the central strategic tension of interest: a legislature's influence over an agency. In the appendix, we enrich the model by broadening the set of political incentives facing the party-in-power in order to test the mechanisms behind the breadth/influence relationship.

2.1 The Political Party

We consider a political party currently in control of the legislature. We focus on the party in power (abstracting from strategic dynamics within the legislature) because it mirrors our empirical context where the available survey data we have asks about the influence of Democrats in Congress (who controlled the House and Senate at the time). In our baseline formulation, we assume that the party only cares about maximizing the campaign contributions that it receives

[5]Relative to these three papers, ours is the first in this literature to include an explicit formal model of the influence process and to empirically test the mechanisms behind our model. We also provide a more extensive exploration of causality and show that our feature of interest (breadth) has a substantively larger effect than those previously considered.

(in exchange for influence agencies), subject to effort costs it incurs by exerting influence.[6,7]

In our empirical analysis, we consider several hundred interest groups. Thus, we treat the party-in-power as a monopolist and allow them to set the price of influence. We let π_i denote the price that the party charges group i (to be paid in campaign contributions) in exchange for one unit of influence so the total contributions the party receives from interest group i is $\pi_i S_i(\boldsymbol{\pi})$, where S_i is influence exerted on behalf of i (i.e., the influence i purchased). However, the total amount of influence that affects the group, defined as $A_i(\boldsymbol{\pi})$, includes spillovers. Specifically, we assume that for every unit of targeted influence S_i on behalf of interest group i, each other interest group receives η units of influence, where $0 < \eta < 1$. Thus, total influence exerted on task i is:

$$A_i(\boldsymbol{\pi}) = S_i(\boldsymbol{\pi}) + \eta \sum_{j \neq i} S_j(\boldsymbol{\pi}).$$

This assumption – that $\eta > 0$ so that Congressional pressure exerted on one task affects the performance of other tasks the agency is responsible for – is the key ingredient of our model.[8] We view it as a realistic feature of many federal agencies. For instance, in June, 2018, top investigators from the Homeland Security Investigations (HSI) unit of the US Immigration and Customs Enforcement (ICE) agency requested that their unit be split off into a separate, independent agency (Texas Observer, 2018). They argued that investigations of transnational criminal organizations like drug cartels and human trafficking rings was "unnecessarily impacted by the political nature" of immigration enforcement. A former ICE deputy director went so far as to say that agents worried the unit was "just becoming a political pawn" and that "because of this whole immigration rhetoric – that immigrants are bad, they're criminals and rapists and all that – the focus is totally off mission." In other words, politically charged immigration enforcement pressures were affecting totally unrelated tasks and responsibilities housed within the same agency.

This is unsurprising. Many of the methods by which legislatures influence agencies – such as appointing directors (Wood and Waterman, 1991) or imposing oversight hearings (Kriner and Schwartz, 2008; Parker and Dull, 2009) – are necessarily blunt. Oversight hearings focused on one specific program or set of operations create chilling effects on other responsibilities and redirect priorities and resources, even if only senior managers' attention.

[6]The party's goal of maximizing campaign contributions in order to obtain reelection is consistent with the model used by Grossman and Helpman (1996). We consider this to be a simplification of the incentives present in a more complex contest which is "locally" valid when the probability of the party winning is near one half.

[7]In Section 5.2, we extend the model to allow for the legislature to have different ideological preferences for different groups.

[8]It is also plausible that spillovers might be *negative*, such that the party's influence partially redirects agency scrutiny towards other interest groups. We consider this model in Appendix D.

Even the most targeted methods for Congressional influence, so-called "limitation riders" in appropriations bills that pledge certain funding to specific tasks or prohibit certain uses of funds, can spill over. Often, appropriations bills give agencies a block of funding with an explicit requirement that it is used for a specific purpose. Money, of course, is fungible, and so in many cases agencies can redirect some of their resources that otherwise would have been applied to the task. Other times Congress explicitly bans the use of funds for certain tasks but these, too, can spill over. In a well-known example, the 1995 Emergency Timber Salvage Rider barred agencies from reviewing and blocking certain sales. But subsequent court rulings applied these bans to more than twice the sales originally targeted (Zellmer, 1997); arbitrarily specific statutory language is often not legally enforceable. These are exactly the sort of spillovers that our model captures.

Again let S_i be the influence the party exerts on behalf of i and A_i be the total influence produced that affects interest group i. We assume that the party in power pays linear costs on this total amount of influence. In other words, the legislature incurs effort costs on the spillovers, although it is not able to charge for them. With these elements and letting c denote the effort costs, the party's problem is to choose the vector of prices $\boldsymbol{\pi}$ to solve:

$$\max_{\boldsymbol{\pi}} \sum_{i=1}^{N} \pi_i S_i(\boldsymbol{\pi}) - cA_i(\boldsymbol{\pi}).$$

2.2 Interest Groups

Interest groups receive utility from two sources: "mission spending" and policy. We seek a formulation that allows a broad notion of interest groups, including collections of firms (in which case "mission spending" might be investments to improve future profits, wages to pay for current production, etc.) or not-for-profit entities or citizen groups (in which case "mission spending" might be public opinion campaigns, spending on conservation, provision of membership benefits, etc.). We denote the mission spending of interest group i as m_i.

With respect to policy, we assume that interest groups care about some task being performed by a government agency. To influence this task, they can either lobby the agency directly (ℓ_i) or contribute to the party in control of Congress in exchange for that party to pressure the agency on the group's behalf. In either case, the spending uses up some of the group's budget which could otherwise be devoted to mission spending.

As above, we denote the total support interest group i purchases with campaign contributions as S_i and the total amount of action that the party takes on i's behalf as A_i. With these

elements, we model the interest group's problem as:

$$\max_{m_i,\ell_i,S_i} m_i + \omega \ell_i^{\gamma_1} A_i^{\gamma_2}$$

subject to the budget constraint $m_i + \ell_i + \pi_i S_i = R_i$ where A_i is defined as before. To ensure that the solution to the interest group's problem is interior, we assume that the interest group's returns from spending on policy instruments are concave, so $\gamma_1 + \gamma_2 < 1$. We also assume that given the parameter ω which represents how important policy is to the interest group, the interest group's budget R is sufficiently large so that the interest group finds it worth spending money on their mission.

With this specification of interest group preferences, lobbying and influence are complementary and there are decreasing marginal returns to the total amount spent on them.[9] Consider, for instance, a firm that contracts with the political party to pressure an agency to change a policy. We assume this influence will be less effective without the firm also lobbying the agency to shape the implementation of that policy. The spending will first go towards removing the most onerous aspects of the regulations facing the firm, followed by those which are less costly. Eventually, the returns to spending on the firm's other (non-political) interests becomes more valuable than spending to influence policy.

2.3 Timing and Equilibrium

All players have complete information. The party first chooses what price to charge each interest group, after which the interest groups simultaneously choose how to allocate their budgets. Since this is a dynamic game of complete information, we solve for the subgame perfect equilibrium using backward induction.

Interest groups will exhaust their budgets on lobbying, influence, and spending on their mission, so their budget constraint can be substituted back into their objective function. Taking

[9]The assumption of complementarity is consistent with existing evidence (Tripathi et al., 2002; You, 2017), but not important for our results. Below, we show that lobbying increases when the allied party takes control of Congress, lending support to this formulation.

first order conditions and combining interest group best responses, we get

$$\ell_i^*(\boldsymbol{\pi}) = \left(\frac{\gamma_1 \pi_i}{\gamma_2}\right)^{-\frac{\gamma_2}{1-\gamma_1-\gamma_2}} (\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}} \tag{1}$$

$$\begin{aligned} S_i^*(\boldsymbol{\pi}) = &\frac{-(N-2)\eta - 1}{(N-1)\eta^2 - (N-2)\eta - 1} \left(\frac{\gamma_1 \pi_i}{\gamma_2}\right)^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}} (\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}} \\ &+ \sum_{j \neq i} \frac{\eta}{(N-1)\eta^2 - (N-2)\eta - 1} \left(\frac{\gamma_1 \pi_j}{\gamma_2}\right)^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}} (\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}} \end{aligned} \tag{2}$$

Thus, the party in power essentially faces the monopolist's problem, with the additional complication that it will only be paid for a proportion of what it produces. We can plug the interest groups' demand functions back into the political party's problem to find the optimal price level. The result is given in Lemma 1, which is proven in Appendix A.

Lemma 1 *In the unique pure strategy subgame perfect equilibrium, the political party charges price*

$$\pi_i^* = -\frac{c(\gamma_1 - 1)}{\gamma_2}((N-1)\eta + 1) \tag{3}$$

to interest group i for influence.

Equation (3) gives the price that the party in power charges for targeted influence. The price optimally trades off between bringing in more campaign contributions and paying higher effort costs. Even though the marginal cost is constant, this price is *increasing* in the number of regulated interest groups due to influence spillovers.

Proposition 1 *The political party's average influence on an agency is decreasing in agency breadth.*

Proposition 1 follows immediately from Lemma 1 and the fact that each interest group's demand is decreasing in the price they pay. Notice that an increase in N leads to a fall in *both* $S_i^*(\boldsymbol{\pi}^*)$, the amount of influence that the interest group pays for, and $A_i(\boldsymbol{\pi}^*)$, the total amount of influence (including spillovers) affecting interest group i. For each additional interest group, the party in power has to exert more effort to produce the same amount of targeted influence. Because of this, the party charges higher prices, and each interest group demands a lower quantity.

The theoretical result found in Proposition 1 implies that when controlling for other observable characteristics of a government agency, we should expect broad agencies (regulating many

interest groups) to report being less influenced than narrow agencies. This result is what will be explored empirically throughout the rest of the paper.

3 Empirical strategy

3.1 Measuring breadth

One contribution of this paper is developing a novel measure of the breadth of agencies. In our model, we defined an agency's breadth as the number of interest groups the agency regulates. While this number is not directly observable, the model implies that we can infer it by observing which interest groups lobby that agency. We do so using lobbying disclosure data from the Center for Responsive Politics (CRP; `www.opensecrets.org`), used extensively in past work (e.g., Blanes i Vidal, Draca, and Fons-Rosen (2012), Kang (2015), You (2017)).

The CRP data include lobbying of federal agencies and identifies the "category" of the organization hiring or employing the lobbyist. We refer to these categories as interest groups. They are organized hierarchically, ranging from 16 coarse 1-digit codes to 115 2-digit, 367 3-digit, and 424 4-digit codes.

It is helpful to consider an example. Among the 16 1-digit codes, the most lobbying of our sample agencies comes from interest group "H: Health, Education, and Welfare." Table B1 in the appendix shows how this single broad group breaks down into six narrower 2-digit groups, such as "H1: Doctors and health practitioners," "H2: Inpatient health facilities," and "H4: Medical supplies." These codes are further broken down into 3-digit codes. For instance, doctors includes "H11: Physicians" and "H14: Dentists," health facilities includes "H21: Hospitals" and "H22: Nursing homes," and medical supplies includes separate categories for manufacturers of medical equipment (H41) and pharmaceuticals (H43).

These 3-digit codes are far more precise, and it is easy to think of circumstances in which hospitals and nursing homes, for instance, might have different policy preferences or be affected by different agencies. These 3-digit codes are the level of precision that we use for our main specification, though we show our results are robust for all four levels of aggregation.

Finally, Table B1 shows that only one of the 19 3-digit codes breaks into separate 4-digit codes (physicians are divided into Optometrists, Other specialists, and Physicians not elsewhere classified).

For our primary measure of breadth, we use all lobbying disclosure forms available from 1998 to 2016. We define interest groups according to 3-digit CRP codes, and convert their lobbying expenditures of an agency into real 2017 dollars. Often a single lobbying contract involves multiple agencies being lobbied but with only one expenditure total. In these cases,

we divide those lobbying expenditures equally across lobbied agencies. We then aggregate total lobbying expenditure (across the full period) up to the interest group/agency dyad. We exclude any case in which an interest group spent less than $10,000 in 2017 dollars lobbying an agency (over the full 19 years) so as to isolate an appreciable amount of activity. Finally, for each agency we calculate the total fraction of all interest groups that are observed lobbying that agency. We refer to this share as the "breadth" of the agency. This describes our primary measure of breadth, but we show that our results are robust to the level of aggregation, the use of the $10,000 cutoff, and the choice to normalize multi-agency lobbying contracts.

3.2 Measuring influence

To measure the degree of Congressional influence, we turn to the existing political science literature and use the measure from Clinton, Lewis, and Selin (2014).[10] That measure is drawn from the authors' 2007 Survey on the Future of Government Service, which collected data from 2,368 federal agency administrators and program managers (what we refer to as "high-level bureaucrats"). Clinton et al. (2014) describe the data collection methods in detail.

Among other questions, respondents were asked to rate on a scale of 1 to 5 how much influence various groups have "over policy decisions in your agency." Options ranged from "a great deal" of influence to "none." The question was asked of Democrats in Congress (who controlled both the House and the Senate at the time), the White House (the president at the time was a Republican), and others. Clinton, Lewis, and Selin (2014) explicitly prime respondents to think about the *relative* influence of different groups.

For this reason, they intentionally use a simultaneous list to show respondents all nine groups at once. They do this to account for inherent differences across respondents in how to interpret "a great deal" of influence, "some" influence, etc. Because respondents answer about all groups at once, their measure of influence is calculated by taking the reported influence of Democrats in Congress and subtracting the reported influence of the president.[11] This is a natural normalization. These are executive agencies; therefore, they are explicitly designed to be underneath the President and it is intuitive that the degree of Congressional influence would be defined *relative to the statutory principal*.

[10]Berry and Gersen (2017) develop a creative alternative by focusing on how the geographic distribution of government spending responds to partisan control of Congress. Because of limited information in the spending data, the authors focus on only 22 cabinet-level agencies (mostly large departments). The only data on spending of detailed agencies comes from USA Spending, which is only available from 2005 onward. With such a short panel, one cannot reliably estimate agency-level spending responses to control of Congress. Data limitations aside, a conceptual advantage of the Clinton et al. (2014) measure is that it captures influence over the full range of an agency's responsibilities, rather than simply where it spends its money.

[11]Technically, Clinton, et al., use the negative: Presidential influence minus Congressional Democrats'.

3.3 Summary statistics

We create our final dataset by merging our lobbying-based measure of breadth and the survey-based measure of influence for every agency in the CRP data and the available Clinton, Lewis, and Selin (2014) data. The Data Appendix has further details. In total, we are left with 70 federal agencies. Table 1 presents summary statistics on these agencies, as well as the interest groups observed lobbying them.

Panel A considers interest groups. The average 3-digit interest group lobbied 27 of the 70 agencies in our main sample, though there is wide variation. Across the 367 groups, 10% lobbied 7 or fewer and 10% lobbying 46 or more. When we restrict to agencies the groups spent $10,000 or more lobbying (again, over a 19 year period), these numbers fall but only slightly because typical lobbying expenditures dramatically exceed $10,000. The average lobbying relationship (conditional on exceeding the $10,000 threshold) sees $158,000 spent lobbying an agency *each year* (median: $66,000). This amount is per agency, and is large relative to the amount spent on Congressional campaign contributions. Across our 367 groups, the median spends 9.3 times as much lobbying agencies in our sample as it spends on Congressional campaigns, closely resembling the 10:1 figure from Tripathi, Ansolabehere, and Snyder (2002).[12] As is seen in the far right column, groups that lobby more lobby more agencies, more per agency, and spend more on Congressional campaigns.

[12]That lobbying spending exceeds campaign spending is a point emphasized elsewhere (see, for instance, Drutman (2015) and You (2017)).

Table 1: Summary statistics

	Panel A: Interest group characteristics ($n = 367$)							
	Mean	Standard deviation	Percentiles 10^{th}	25^{th}	50^{th}	75^{th}	90^{th}	Corr. with log(Lobbying)
Num. of agencies lobbied	27.0	14.1	7	18	27	36	46	.819
Num. of agencies lobbied 10K+	25.0	13.5	6	16	25	34	43	.834
Lobbying per agency per year (*th.*)	158	280	10.4	24.4	66.0	158	351	.653
Congressional contrib. per year (*th.*)	865	1,882	.736	23.2	141	809	2,591	.294
Democratic share	.447	.235	.204	.299	.421	.513	.883	-.052
Ratio: Agency lobbying to contributions	342	3,297	.742	3.13	9.34	24.5	105	.032
	Panel B: Agency characteristics ($n = 70$)							
	Mean	Standard deviation	Percentiles 10^{th}	25^{th}	50^{th}	75^{th}	90^{th}	Corr. with Breadth
Breadth (1-digit)	.837	.206	.531	.813	.938	.938	.938	.599
Breadth (2-digit)	.527	.274	.117	.365	517	.739	.895	.965
Breadth (3-digit)	.355	.256	.042	.165	.293	.534	.756	1
Breadth (4-digit)	.347	.255	.036	.150	.283	.519	.748	.999
Congressional influence	-.582	.634	-1.23	-1	-.549	-.320	.138	-.432
Year founded	1933	53.1	1856	1913	1947	1970	1979	-.244
Employees (*th.*)	48.4	120	.364	1.29	5.30	36.0	131	.220
Number of oversight committees	3.06	.580	2.37	2.63	3.00	3.42	3.81	.498
Political appointee share	.137	.172	0	0	.102	.182	.300	-.159

Panel A is based on 3-digit interest groups that ever lobby an agency in the main sample. "Corr. with log(Lobbying)" refers to the correlation with the log of the inflation-adjusted amount of total lobbying of all agencies during the full period (1998-2016). "Number of agencies lobbied" (resp., "10K+") refers to the number of agencies in our main sample ($n = 70$) that the interest group lobbied (resp., spent $10,000 or more (in 2017 dollars) lobbying during the full period). "Lobbying per agency per year" is measured in thousands of 2017 dollars (as is Congressional contributions) among agencies which the group spent $10,000 or more on. "Democratic share" refers to the share of the group's Congressional contributions spent on Democratic candidates. "Ratio of agency lobbying to Cong. contribs" is the ratio of the group's average annual lobbying expenditures (on *all* agencies) to its average annual Congressional campaign spending. Panel B is based on 70 agencies that appear in the lobbying data and the Clinton et al. (2014) data. "Breadth (n-digit)" is the share of all n-digit interest groups that are observed lobbying the agency by $10,000 (in 2017 dollars) or more during the period. To obtain "Congressional influence" (which we take from Clinton et al. (2014)), each respondent's Likert scale response is normalized by his/her own response about presidential influence, and a simple average is taken across respondents within the same agency. "Employees" refers to 2007 employment (when the survey was conducted). "Number of oversight committees" and "Political appointee share" are also averaged over survey responses.

Panel B presents statistics on the 70 agencies used in our analysis. The first four rows summarize our measure of breadth for different levels of aggregation, always interpretable as the *fraction* of groups that lobby the agency. Using the coarsest measure of breadth (with only 16 interest groups), we are left with the impression that nearly all groups lobby nearly all agencies (the average agency is lobbied by 84% of groups, and the median is 94%). When adopting a more precise set of interest group codes, however, this figure falls rapidly, stabilizing at 3-digits, where the average agency is lobbied by 36% of all groups (median: 29%). The variation is large: The most narrow 10% of agencies are lobbied by less than 4% of groups, while the most broad 10% are lobbied by 75% of groups.

Most agencies report more presidential influence than Congressional influence (as expected, given that these are *executive* agencies). Our average agency has 48,000 employees, though this varies dramatically. The 10^{th} percentile is only 364 employees and the 90^{th} is 131,000. Importantly, the correlation between employment and agency breadth is only .22, which in practice means that our regressions should be able to separate the two and we can avoid conflating broad agencies with large ones.

The final two rows show the number of Congressional oversight committees responsible for the agency (the substantive focus of Clinton, Lewis, and Selin (2014)) and the prevalence of political appointees (the substantive focus of Berry and Gersen (2017)). The correlation between breadth and political appointees is small (-.16), and so our regressions should be able to easily separate them. The correlation with oversight committees is larger (.50), as would be expected: Broad agencies are overseen by more committees. Nonetheless, below we show that controlling for this has little effect on our estimates.

3.4 Examples of agencies

Although summary statistics illustrate aggregate patterns in the data, it is helpful to consider some specific agencies to better understand exactly what our lobbying-based measure of breadth captures. We divide the agencies in our main sample into quartiles based on their breadth. We then identify the agency, within each quartile, that minimizes the squared residual from some of the regressions that we estimate later on. Thus, these agencies are literally the representative cases from our regressions. The representative agencies are shown in Table 2.

Table 2: Examples of more and less broad agencies

Agency group	Representative agency	Key groups lobbying agency
Most narrow (breadth range: .008 - .165)	Bureau of Labor Statistics (breadth: .087)	Most lobbying from (main group): Pharm. manufacturing Furthest right group: Pro-business associations Furthest left group: Labor unions
Second quartile (range: .168 - .293)	Federal Deposit Insurance Commission (breadth: .252)	Main group: Commercial banks Right: International trade associations Left: Human rights associations
Third quartile (range: .304 - .523)	Department of the Air Force (breadth: .322)	Main: Defense aerospace contractors Right: Pro-business associations Left: For-profit educational institutions
Most broad (range: .534 - .829)	Federal Communications Commission (breadth: .534)	Main: Communications services Right: International trade associations Left: Courts and justice system actors

"Agency group" is based on quartiles of the agency breadth distribution. Within each quartile, we choose the agency with the smallest squared residual from the regression of influence on breadth without the OMB outlier (i.e., the regression in Table C3 column 2). Thus, these agencies are those for which our regressions are most representative. The agencies that minimize the squared residual from the regressions *with* the outlier are very similar (in order: BLS, FDIC, Dept. of the Navy, Dept. of the Interior). "Main group" refers to the interest group (3-digit CRP category) that spent the most on lobbying the agency. "Furthest left" (and "furthest right," respectively) group is that with the highest share of Congressional contributions going to Democrats (lowest share, respectively), among groups spending more than $10,000 lobbying the agency.

The most representative agency from the highest breadth quartile is the Federal Communications Commission (FCC), which "regulates interstate and international communications by radio, television, wire, satellite, and cable" and "implement[s] and enforc[es] America's communications law and regulations." This is clearly a broad mandate, and with the changing role of the internet in society, the FCC has become increasingly relevant for retail, entertainment, and banking. Unsurprisingly, then, the agency is lobbied by over 50% of all interest groups in the data, but communications services companies (including internet and wireless telephone companies) do the most lobbying.

The most representative agency from the lowest breadth quartile, on the other hand, is the Bureau of Labor Statistics (BLS), which is responsible for "measuring labor market activity, working conditions, and price changes." It might seem surprising that interest groups lobby this organization, and even more surprising that it is manufacturers (mostly pharmaceutical manufacturers). When we explore the disclosure reports, most state that the lobbying is in relation to the Occupational Safety and Health Act (OSH Act). While they are not more specific, we can speculate.

The OSH Act was passed in 1970 and is primarily enforced by the Occupational Safety and Health Administration (OSHA). Under the OSH Act, employers must adopt potentially expensive workplace protections to guard against certain hazards. Current OSHA regulatory standards require employers to *proactively* guard against "recognized hazards," including those recognized as common, recurring dangers within the industry. Determining which hazards are common and recurring within the industry depends on the Survey of Occupational Injuries and Illnesses (SOII), designed and conducted by the BLS.

In other words, a single BLS data collection program provides the foundation that OSHA uses to determine recognized hazards, which determines which risks employers must actively prevent. As a result, manufacturers (and especially chemically-intensive pharmaceutical manufactures) are obviously greatly affected by relatively mundane details like how the BLS classifies certain injuries or incidents and where the BLS draws the line between different industries. For our purposes, this is a perfect example of an agency with only a single narrowly-defined task that directly affects interest groups.[13] It matches well with the notion of breadth that we model: It is unlikely that any spillovers caused by Congressional influence over the SOII would meaningfully affect other agency operations in any way that matters to donating interest groups.

Table 2 shows that among agencies that typify our sample and drive our main results, our lobbying-based measure is reflective of an agency's breadth, and that the lobbying groups seen

[13]The BLS, of course, has many more responsibilities, but these are largely irrelevant for interest groups. We do observe some lobbying on behalf of trade groups and employee membership organizations to adjust industry and occupation codes, but the lion's share of lobbying relates to the OSH Act.

in the data have a clear connection to agency policy decisions in very much the same conceptual setting as our model.

3.5 Econometric strategy

Our primary estimates correspond to a simple OLS specification in which we regress agency-level reported influence on agency-level breadth of tasks:

$$Influence_a = \alpha + \beta Breadth_a + \varepsilon_a \tag{4}$$

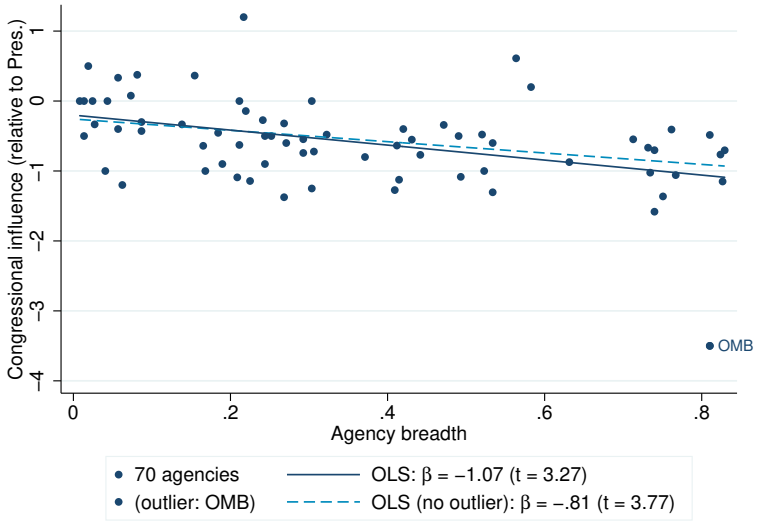
We present a rich set of robustness tests to address various measurement-related concerns regarding the lobbying data, the reliability of the underlying survey measure of influence, and the role of outliers in our small sample.

We do not have an explicit exogenous or quasi-random source of variation in $Breadth_a$. Instead, our strategy is to take seriously several identification threats which we think are plausible and to provide realistic, fair tests for them.

Specifically, we consider the possibility that agency breadth is correlated with other structural features of the agency, including every control from the past literature, as well as several others. Next, we consider the possibility that the negative correlation we observe is caused by characteristics of the interest groups rather than the agency. We exploit the fact that the same interest group is often regulated by (and observed lobbying) multiple agencies, allowing us to include interest group fixed effects so that identification is driven solely by different agencies regulating the same interest groups. Finally, we consider the possibility of reverse causality by instrumenting for the breadth of an agency using the timing of its creation.

In none of these tests do we find evidence for an alternative, non-causal explanation of the breadth-influence relationship. We find these tests reassuring. However, our core result is cross-sectional and there are naturally many potential concerns about causality. It is difficult to imagine a different approach. Our primary measure of influence is survey data which we have for only one time period. Even if we could measure influence over time, there is likely little over-time variation in the breadth of an agency's responsibilities, and the variation there is may well be just as endogenous as the cross-sectional variation. Sometimes tasks are shifted from one agency to another, which affects breadth. However, these shifts are part of broader agency restructuring, meaning that one must still account for changes in staffing, funding, and policies (the exact identification threats we face cross-sectionally). Moreover, without a *task-level* measure of influence (which we think is unrealistic to expect), one would still need to prove that changes in agency-level influence resulted from changing *the breadth of* responsibilities as opposed to changing *which* responsibilities agencies' held. In other words, a time-varying

Figure 1: Agency breadth and political influence



Each observation is an agency. Corresponding regression results found in column 3 of Table 3 (all agencies) and column 2 of Table C3 (without outlier).

measure of influence would not change the identification threats we consider.

4 Results

4.1 Main results

Our core result is that more broad agencies report less Congressional influence. Figure 1 shows this flexibly using the raw data on the 70 agencies in our main sample. On the x-axis is our preferred measure of breadth: The share of 3-digit interest groups that lobby the agency. On the y-axis is reported relative Congressional influence. The obvious negative relationship is reflected in the OLS slope, which is highly statistically significant ($p = .002$). While there is a clear outlier (the Office of Management and Budget [OMB]),[14] excluding this agency has a modest effect on the estimated slope (which falls by 25%) but actually makes it more precise and more significant ($p < .001$).

Table 3 presents the formal econometric results behind Figure 1. Interpreting these coefficients in light of the summary statistics in Table 1, a one standard deviation increase in breadth is associated with a .43 standard deviation decrease in reported influence.[15] Table 3 shows that

[14]The function of OMB is to "assist the President in meeting his policy, budget, management, and regulatory objectives." This and the fact that the Congressional Budget Office performs a similar role for the legislature likely explain why OMB experiences so little Congressional influence.

[15]The coefficient in our primary specification is -1.07. When we exclude OMB, the coefficient shrinks (towards

the estimated negative relationship between agency breadth and Congressional influence is robust across all four definitions of interest groups. Because breadth is always measured as the *fraction* of groups observed lobbying the agency, the coefficients are comparable across columns; they are notably stable.

Table 3: Broad agencies report less influence

DV: Influence	(1)	(2)	(3)	(4)
Breadth (1-digit)	-0.927***			
	(0.267)			
Breadth (2-digit)		-1.002***		
		(0.262)		
Breadth (3-digit)			-1.072***	
			(0.328)	
Breadth (4-digit)				-1.069***
				(0.332)
R^2	0.091	0.188	0.187	0.184
N	70	70	70	70

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an agency. Breadth is measured as the fraction of interest groups that lobby the agency (for 1-, 2-, 3-, and 4-digit levels of aggregation; see Table B1 for an example). Because they measure the fraction of all possible interest groups, coefficient magnitudes are comparable across columns.

4.2 Robustness

Here, we discuss robustness tests based on various measurement- and sample-related issues. In the next subsection, we discuss identification tests. Robustness checks are in the online appendix. In Table C1, we show that our results are robust to the key decisions made in measuring breadth. Whether we use a more strict cutoff for the minimum amount of lobbying, eliminate the $10,000 cutoff altogether, or leave multi-agency contracts un-normalized, the results are nearly identical to those in Table 3.

Our baseline specification uses all available lobbying data (1998-2016), though influence is only measured in 2007. We do this to smooth over idiosyncratic year-to-year variations in the

zero) by 25% to -0.81. However, OMB is an outlier in terms of Congressional influence so excluding it also shrinks the standard deviation of influence by around 25%. Thus, both specifications deliver the same conclusion that a one standard deviation increase in breadth implies a .4 standard deviation decrease in influence.

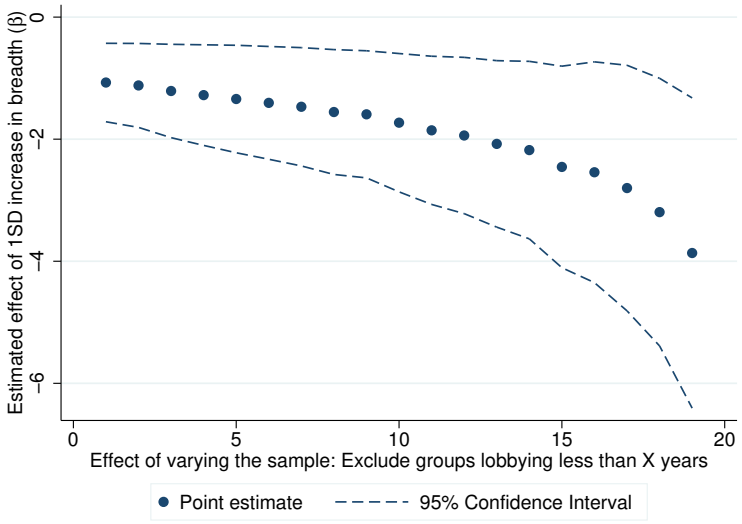
decision to lobby. In Table C2, we progressively restrict the lobbying data we use to a smaller and smaller range of years around 2007 (in column 4, to only 2007). As we do, our estimates are quite stable. Estimates become more negative as we eliminate years further from 2007, perhaps suggesting that our main approach (using all years) introduces measurement error.

Of course, the decision to lobby is endogenous. For instance, we show in Section 5.2 that it responds to the control of Congress. Our primary measure of breadth counts an interest group as being regulated by an agency whenever that interest group is observed lobbying the agency *at all* (even once throughout the sample period). Does this bias our results? In Figure 2 we show that it does not. Specifically, we iteratively exclude interest groups from the calculation of breadth and plot the resulting coefficients and confidence intervals estimated from our primary specification. The far left dot corresponds to our main measure; we include any interest group that lobbied the agency even once. Figure 2 shows that as we adopt a more strict definition of lobbying, our estimates become more negative. For instance, excluding interest groups that lobbied an agency during fewer than 10 years yields an estimate that is roughly double our primary estimate. Excluding groups lobbying during fewer than 15 years yields a similar conclusion, but using the most strict definition (including only groups that lobbied during all 19 years for which we have data) yields an estimate that is almost four times our primary estimate (and more statistically significant). For these groups ("core" regulated groups observed lobbying in every year), the endogeneity of the decision to lobby is likely less relevant, and indeed we find that the relationship between breadth and influence is even stronger. This suggests that endogenous lobbying decisions (like those we study in Section 5.2) likely bias our estimates towards zero if anything.

In Table C3 we show that our results are not an artifact of a subset of agencies. As discussed above, excluding OMB as an outlier makes the coefficient smaller (less negative, closer to zero) but more statistically significant. Our primary specification treats all agencies equally, even small ones, but weighting by 2007 employment yields similar results. Dropping either military agencies (which Congress may hesitate to influence) or cabinet-level departments (which may be best thought of as conglomerates of agencies) yields larger (more negative) effects. As noted by Clinton, Lewis, and Selin (2014), not all bureaucrats the authors attempted to survey actually responded, and we arrive at the same conclusion whether weighting by the number of actual respondents (statistically appropriate but implicitly over-weighting larger agencies) or the response rate. In all of these specifications, the estimated coefficient fluctuates between -.8 and -1.7, and is always statistically significant at 5% or better.

Our model may be better suited for non-cabinet agencies than for large cabinet-level departments (like the Department of Defense, which is in our sample). In Table C7, we replicate our causality exercises excluding cabinet-level departments. Finally, in Table C8 we replicate our

Figure 2: Effect of excluding weakly attached interest groups from breadth calculation



Each dot is the coefficient from a separate regression corresponding to our main estimating equation (equation (4)). For each regression, we re-calculate agency-level breadth excluding interest groups that lobby the agency during fewer than X years. For instance, the far left dot corresponds to our primary specification: Breadth is calculated using every interest group that lobbied the agency during even just one year of the sample. The far right dot corresponds to the strictest subset: Breadth is calculated using only interest groups that lobbied the agency during each of the 19 years of the sample.

results using the number of policy areas the agency covers – from 1-17 – a measure of breadth suggested by Clinton, Lewis, and Selin (2014).

4.3 Identification

Thus far, we have been careful to describe our empirical results as correlations. However, our model lays out a clear channel by which an agency's breadth might causally affect the amount of Congressional influence. Is there a clear *alternative* explanation for the correlation that we document? In this section, we consider three potential alternatives.

4.3.1 Other agency characteristics

First, it is possible that breadth is simply correlated with some other agency characteristic that affects influence. As noted in Table 1, breadth is correlated with both Congressional oversight and the prevalence of political appointees, which Clinton et al. (2014) and Berry and Gersen (2017), respectively, have suggested affect Congressional influence.

Our core approach to this concern is including controls. Given the paucity of previous research on this topic, we are able to essentially tie our hands and use the full set of controls

used in the entire past literature. The results are in Table 4.

In column 2 we include the full set of controls from Clinton, Lewis, and Selin (2014), including the number of oversight committees (their focus) and the share of political appointees (Berry and Gersen's focus).[16] In addition to controlling for key explanations from the existing literature, these controls also account for core alternative hypotheses, such as large agencies being more difficult to influence (since we control for log employment), narrow agencies being intrinsically more political or salient (since we control for whether or not the agency was an important part of the Bush Administration's agenda), and the possibility that average ideology systematically differs for broad agencies (since we include for a survey-based measure of the liberal/conservative ideology of bureaucrats).

Given our sample is only 70 agencies, including eight additional controls (many of which are correlated with breadth) increases our standard error by over 70%, but the coefficient on breadth is virtually unchanged (falls by less than 10%) and still statistically significant ($p = .084$).[17]

For brevity, coefficients on the control variables are relegated to Table C9 of the Appendix. However, the coefficient on the number of committees is negative (as in Clinton, Lewis, and Selin (2014)) and significant ($p < .10$), while the coefficient on appointees is positive (as in Berry and Gersen (2017)) and not quite significant ($p = .109$).[18] In terms of magnitudes, however, the coefficient on breadth is larger: A one standard deviation change in breadth implies a .40 standard deviation change in influence, while a one standard deviation change in committees and appointees imply a .21 and .27 standard deviation change in influence, respectively.

The controls used in column 2 are important features of the agency's political context. However, broad agencies may also be designed differently. Selin (2015) studies 50 statutory features of agency design and uses a Bayesian latent factor model to identify two factors of independence-by-design (one describing the independence of key decision makers, and one describing the independence from political review). In column 3, we control for these factors, and again our point estimate is virtually unchanged (shrinks by 10%) and still statistically significant ($p < .05$).

[16] We do not control for the number of policy areas in which an agency is listed because it is meant to capture the same conceptual feature as breadth.

[17] Figure C1 depicts the relationship visually in a Frisch-Waugh plot. The downward slope is clear and strong, and if we exclude the two outliers (OMB and the Joint Chiefs of Staff, which has far less breadth than its observable characteristics would predict) the relationship becomes far more precisely estimated. Without these outliers, the coefficient shrinks from -.99 to -.79, but becomes highly significant ($p < .01$).

[18] Berry and Gersen (2017) focus on a small number of very large agencies. For a relatively large number of often-small agencies (as is in our sample), we are aware of no source for the prevalence of appointees. Thus, we follow Clinton, Lewis, and Selin (2014) and use the share of survey respondents who are appointees. This may introduce measurement error which makes the coefficient on appointees smaller and less significant.

Table 4: Evidence that breadth causally affects influence

DV: Influence	(1)	(2)	(3)	(4)	(5)	(6)
Breadth	-1.072***	-0.987*	-0.941**	-0.950*	-1.053**	-1.492**
	(0.328)	(0.562)	(0.456)	(0.564)	(0.473)	(0.735)
R^2	0.187	0.394	0.292	0.188	0.136	0.158
N	70	69	66	65	9871	70
First stage F						13.9
Controls		CLS-14	Selin-15	Other		
Agency-IG panel					Yes	
IG FE					Yes	
IV						Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an agency. Column 2: 8 controls from Clinton, Lewis, and Selin (2014) are log employment, number of Congressional oversight committees, whether it is a commission, whether agency is cabinet-level, whether it has field offices, the share who are political appointees, whether it was part of the Bush administration's agenda, and the Clinton and Lewis (2008) ideology (missing for one agency). Column 3: 2 controls are independence estimates from Selin (2015). Column 4: 2 controls are average lobbying spending per group (logged) and agency expertise (see text). See Table C9 for estimated coefficients on the controls. The instrument is an indicator for whether the agency was established during the Franklin D. Roosevelt, John F. Kennedy, or Lyndon B. Johnson administrations, which tended to be less broad agencies (see Table C4 for evidence that these periods produced more agencies and Table C5 for the first stage). "IG" stands for "Interest group".

Columns 2 and 3 control for, essentially, the exhaustive set of controls identified in the past literature. There are other potential concerns, however. We consider two.

First, it is possible that not every agency regulating the same group is equally important for that group. It is possible that some agencies regulate many groups, but in a relatively marginal way, while the agencies whose actions are most important tend to be more specialized. If Congress only seeks to influence the important actions, then we would observe more influence in narrow agencies. To account for this, we control for the log of average lobbying spending per lobbying group. If narrow agencies were doing much more important activities, then we would expect a group to spend more on one of those agencies than it would on a broad one.

Second, it is possible that there are fixed costs of establishing expertise, and that broad agencies regulating many groups have paid those fixed costs. For this to bias our results, it would have to be the case that Congress systematically avoided influencing expert agencies. While we are ex ante skeptical that politicians exercise this type of restraint, this explanation is plausible

to some. To control for agency expertise, we use data from Lewis (2008) on the occupational composition of each agency. We define expertise as the share of workers in "professional" occupations, defined as "white collar occupations that require knowledge in a field of science or learning characteristically acquired through education or training equivalent to a bachelor's or higher degree with major study in or pertinent to the specialized field... [This work] requires the exercise of discretion, judgment, and personal responsibility for the application of an organized body of knowledge."[19] Within our sample, the professional share of employees varies from 4.4% (Federal Motor Carrier Safety Administration) to 70% (Food and Drug Administration).

Column 4 controls for these potential explanations. Again, these characteristics are correlated with breadth and so the standard error increases by 70%, but the coefficient is roughly the same (89% as large) as in our baseline specification, and remains significantly different from zero ($p = .097$).[20]

When we control for all twelve alternative controls at once, and therefore restrict ourselves to a sample size of 65 with 13 independent variables, breadth is not quite statistically significant ($p = .109$), but the coefficient is actually 11% larger (more negative) than in our baseline specification. When we apply variable selection methods (e.g., Lasso), breadth is always selected as an important predictor. Thus, despite the rich set of potential agency-level confounds that we consider, we conclude that the influence/breadth relationship is not spuriously driven by other agency characteristics.

4.3.2 Interest group characteristics

As an alternative interpretation, our evidence that broader agencies experience less influence might have nothing to do with features of the agency, but with features of the regulated interest groups instead. For instance, it may be that any agency that regulates a highly divisive, politicized group (e.g., labor unions) will experience large amounts of influence, but such groups might be concentrated in relatively narrow agencies. To investigate this hypothesis, we estimate a modified form of our main specification based on a dyadic data structure. That is, we create a dataset of agency-group dyads in which each agency appears alongside each group that lobbied it (equivalently, each group alongside each agency it lobbied). We then estimate our main specification including interest group fixed effects:[21]

$$Influence_{ia} = \alpha_i + \beta Breadth_a + \varepsilon_{ia}$$

[19]The other 5 occupation categories in the Lewis (2008) data include administrative, technician, clerical, other white collar, and blue collar.

[20]Neither measure is statistically significant in the regression. Moreover, log average spending per group (or "importance") is positively, not negatively correlated with breadth.

[21]We use two-way clustered standard errors, clustered at the interest group and agency levels.

Ultimately, this modified data structure uses the exact same variation as our primary specification: We are interested in agency-level influence and agency-level breadth, neither of which varies across interest groups (i.e., neither of which varies within agency). To understand why this addresses concerns about the set of regulated groups, note that this regression could not be estimated if there were never multiple agencies regulating the same interest group (β would not be identified). If each interest group were only regulated by one agency, there would be no "within interest group" variation in breadth (no variation would be left after the interest group fixed effects absorbed the between group variation) and we could not estimate β. Thus, this regression is identified only from the fact that different agencies (with different breadth) sometimes regulate the same interest group(s). The group fixed effects, however, account for any feature of the group itself, ensuring that our results do not simply reflect agencies experiencing more influence simply due to the groups they regulate.

Interest group fixed effects are included in column 5. The coefficient barely changes and is still statistically significant ($p < .05$). Thus, the relationship between breadth and influence is not explained by features of the regulated groups.

4.3.3 Reverse causality

Finally, one might worry that the relationship is not driven by omitted agency-level or group-level variables, but is actually a causal relationship in the opposite direction. For instance, some agencies may be exogenously subject to more Congressional influence, and this might induce regulated entities to influence agency activity through Congress, rather than lobbying the agency. In this case, agencies experiencing a lot of influence might be lobbied by relatively few groups, which would be reflected as a narrow agency in our lobbying-based measure, but it rather reflects the effects of influence on the decision to lobby.[22]

To address the possibility of reverse causality, we develop an instrument for the breadth of an agency so as to isolate variation in breadth that is certainly *not* caused by current Congressional influence. In doing so, we draw on evidence that an agency's design is affected by the national political circumstances in place at the time of its creation (Lewis, 2004). Specifically, we note that periods of rapid agency creation tended to produce more narrow agencies. This is unsurprising: When *many* new agencies are being created, new responsibilities are more likely to be split between them rather than condensed into one agency out of convenience.

The logic for this is shown in appendix Table C4. During the Franklin D. Roosevelt administration (the Great Depression and the New Deal), agencies were created at a rate unmatched

[22]This explanation is suspect to begin with because it assumes lobbying and Congressional influence are substitutes. Existing models (You, 2017) and empirical evidence (Tripathi, Ansolabehere, and Snyder, 2002) imply they would be complements. Below, we show lobbying increases when the allied party takes control of Congress, further suggesting they are complements.

before or immediately after (the Truman and Eisenhower administrations). These agencies were significantly more narrow than other agencies. Similarly, the next period of rapid agency creation was under the John F. Kennedy/Lyndon B. Johnson administrations, when the Great Society programs were launched. This period, too, saw the rapid creation of new agencies, which tended to be relatively narrow. Table C5 shows the formal first stage: Agencies created during the FDR administration are one standard deviation (and those during the JFK and LBJ administrations two-thirds of a standard deviation) more narrow than other agencies in our sample. Pooling the three periods, we see that agencies born during administrations of rapid agency creation tend to be .8 standard deviations narrower than others ($p < .01$; $F = 13.9$). We use this dummy variable as an instrument for agency breadth.

Importantly, our instrument is non-monotonic in agency age. Our sample includes agencies created before FDR, after LBJ, and between FDR and JFK. Thus, the instrument neither isolates the oldest nor the youngest agencies (nor those of intermediate age). We see no obvious reason why this particular feature of the timing of agency creation would *directly* affect Congressional influence 50 years later, relative to agencies that are a decade older or younger.[23]

This instrument does not solve all identification concerns. While these agencies tend to be more narrow than others, they may differ in other design characteristics as well. Thus, in interpreting these IV results, it is important to keep in mind that column 2 showed that the breadth-influence relationship is *not* explained by other agency traits.[24]

Column 6 shows our IV results. We estimate a slightly larger, negative, and statistically significant ($p < .05$) relationship between breadth and influence. Thus, we conclude that the breadth-influence relationship is not driven by reverse causality.

In sum, we document that broader agencies report less Congressional influence. Our model suggests this is because breadth reduces legislators' willingness to exert influence. We rule out three alternative explanations: We find no evidence that our results are explained by other observable agency characteristics or features of the regulated groups, and our results are not explained by reverse causality. While we cannot possibly rule out every potential identification threat, and the breadth of an agency's design will never be randomly decided and unrelated to other characteristics, these findings help support a causal interpretation.

[23]It is worth mentioning that these administrations were all Democratic, and these agencies tend to be younger and more likely to be commissions. In Table C6, we consider each of these three controls. They weaken the first stage substantially, so the standard error on breadth rises by 15-60%. As a result, the coefficient is not always statistically significant. Nonetheless, the point estimate is quite similar to our primary IV specification.

[24]Put differently, if our IV results *did not* find a significantly negative effect of breadth, then we would have good cause to worry about reverse causality. Finding a negative effect, however, does not address all possible identification concerns. It only rules out reverse causality.

5 Mechanism

5.1 An alternative mechanism: Balance

The core mechanism in our model is compensation for spillovers: the idea that the legislature cannot perfectly target its influence to only affect a single specific task, which matters because of the legislature's ability to contract on influence. The broader the agency, the greater the incidental spillovers than the legislature cannot be compensated for.

An alternative mechanism by which breadth might matter is balance.[25] If the legislature can apply pressure to improve or worsen an agency's treatment of certain groups, then the ideological balance of an agency is important. If an agency exclusively regulates groups the legislature opposes, it can pressure the agency to worsen groups' treatment without concern for the negative spillovers on other groups (since they, too, are enemies of the legislature). Likewise, if the agency only regulates supportive groups, the legislature can exert pressure to benefit them, and any spillovers will only help other supportive groups. If an agency is balanced, though, the some pressure to help supportive groups spills over and inadvertently helps enemies, and vice versa for effort to undermine opposition groups.

Thus, when effort cannot be perfectly targeted and spillovers are important, agencies' ideological balance (a mix of ally and opponent groups) creates a disincentive for legislative influence. If broad agencies are more likely to have a balanced mix of groups (through a law of large numbers type argument), while narrowly defined agencies sometimes only regulate left-leaning or only right-leaning groups, then we would expect average influence to fall for broad agencies.

This is not an implausible mechanism. One way to define the ideology of an interest groups is to calculate the share of Congressional campaign contributions going to Democratic candidates. We do so for each interest group in our data, and then subtract the mean (roughly .44) to get a measure of Democratic preferences that is mean zero across groups. To get an agency-level measure of balance, we take the average of Democratic preferences across all groups we observe lobbying the agency. If this measure is especially high then pro-Democratic groups vastly outnumber pro-Republican groups, and vice versa if it is low. If it is near zero, though, the agency has an even balance of left-leaning and right-leaning groups.

Panel a of Figure 3 shows the distribution of average Democratic preferences, within each agency, by agency breadth. There is clear support for a law of large numbers type result: Irrespective of breadth, the *average* agency tends to be balanced. But among narrow agencies, it is common to have highly imbalanced ones (regulating only far-left or far-right groups) while virtually all broad agencies are well-balanced.

[25] This mechanism was the focus of a previous version of this paper. Details and a model are available upon request.

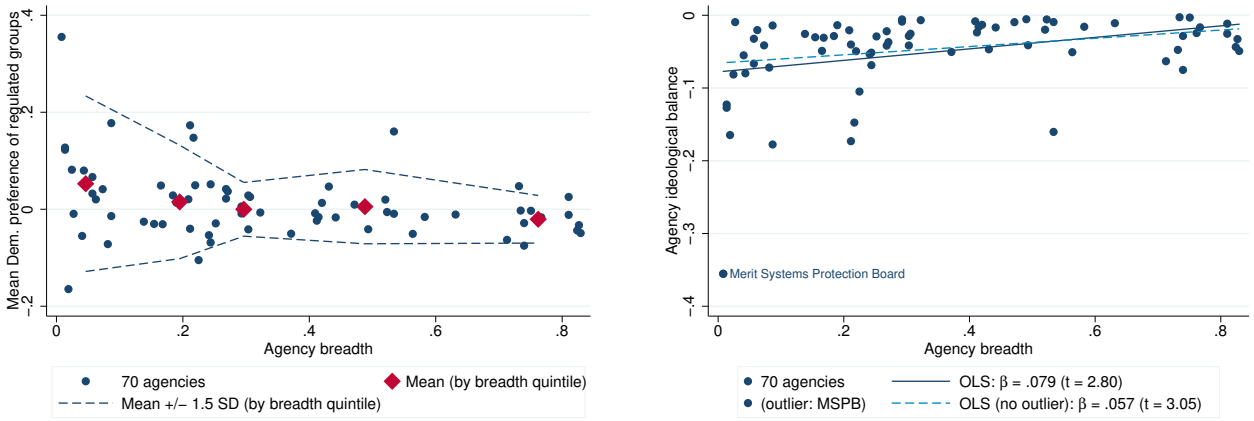
To create an empirical measure of balance, we first calculate the absolute value of average Democratic preferences (which will be negative for right-leaning groups); we refer to this as a measure of imbalance. To calculate balance, we simply take the negative of imbalance:

$$Balance_a = -\left|\frac{1}{n_a}\sum_{i}^{n_a} D_i\right|$$

where D_i is the Democratic preference of group i: $D_i = DemShare_i - E\big(DemShare\big)$.

Panel b of Figure 3 shows that *average* balance is increasing in breadth (although plenty of well-balanced narrow agencies exist, which we exploit later). This relationship is statistically significant with or without the outlier (the Merit Systems Protection Board, an agency which protects the employment rights of federal employees, is overwhelmingly lobbied by Democratic-leaning labor unions).

Figure 3: Broad agencies become ideologically balanced



(a) Average Democratic Preferences

(b) Average Balance

Each observation is an agency. To calculate the Average Democratic Preferences (Panel a), we first calculate the share of Congressional campaign contributions from each interest group that go to Democrats. We subtract the average value (roughly .4) so this variable is mean zero, and refer to this as the Democratic preference of groups. The agency-level Average Democratic Preferences is simply the average Democratic preference among all groups observed lobbying the agency. To calculate Balance (Panel b), we calculate the absolute value of Average Democratic Preferences, which measures the partisan bias of regulated groups, or the imbalance of regulated groups. Balance is the negative of imbalance. The Merit Systems Protection Board is a quasi-judicial agency to protect the employment rights of federal employees. It is overwhelmingly lobbied by Democratically-aligned labor unions.

In Table 5, we investigate whether agencies' ideological balance explains our results. In column 1, we report our baseline estimate of the effect of breadth. In column 2, we show that balanced agencies, on average, experience significantly less influence ($p < .05$). This is, of course, a necessary condition for balance to drive our main findings. In column 3, we control

for both breadth and balance. The coefficient on breadth barely changes, but the coefficient on balance falls by over 50% and is no longer significant. This horserace suggests that breadth is important above and beyond (and not due to) balance.

Table 5: Ideological balance does not explain breadth-influence relationship

DV: Influence	(1)	(2)	(3)	(4)	(5)	(6)
Breadth	-1.072***		-0.957***	-1.213***	-1.049***	-1.650***
	(0.328)		(0.351)	(0.418)	(0.291)	(0.522)
Balance		-3.003**	-1.452			
		(1.227)	(1.129)			
R^2	0.187	0.071	0.201	0.208	0.209	0.299
N	70	70	70	52	35	18
Drop what share of least balanced agencies?				25%	50%	75%

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an agency. To measure "Balance," we first calculate the share of Congressional campaign contributions from each interest group that go to Democrats. We subtract the average value (roughly .4) so this variable is mean zero, and refer to this as the Democratic preference of groups. For each agency, we calculate the average Democratic preference among all groups observed lobbying the agency. This is the average Democratic preference of regulated groups. We calculate the absolute value of this, which measures the partisan bias of regulated groups, or the imbalance of regulated groups. Balance is simply the negative of imbalance (across our 70 agencies, min: -.355, max: -.003, mean: -.050, sd: .056).

Of course, balance might matter non-linearly. In columns 4-6, we progressively drop more and more imbalanced agencies. The idea is to focus only on the most balanced agencies, and see whether influence still decreases in breadth. We take this approach because, as shown in Figure 3, there are no imbalanced agencies among the broad ones; if we want to study how influence varies by breadth, we must consider only the most balanced agencies.

As we focus on a smaller and smaller subset of balanced agencies, our core relationship between influence and breadth is unchanged, becoming slightly stronger. In column 6, including only the 25% of agencies that are the most balanced, our estimates imply breadth matters 50% more than in our baseline specification, and that this relationship is highly significant ($p < .01$).

In summary, while we find evidence that broader agencies are more balanced, and that more balanced agencies report less influence, these relationships do not explain our core results. This suggests a model in which breadth matters for other reasons. Our model based on spillovers and the contractability of campaign contributions and influence is one such possibility.

5.2 Interest group heterogeneity and lobbying

In the model from Section 2, interest groups lobby an agency more when they are able to buy more political influence with that agency. This arises due to the assumption of complementarity between lobbying and influence. Within the context of the model, an interest group lobbying an agency implies that they're also paying politicians to influence that agency. The relationship between lobbying and influence demand is what allows us to identify which agencies an interest group wants politicians to influence.

While the model described in Section 2 doesn't include any heterogeneity among interest groups, minor modifications allow for lobbying and influence to vary between the political party's supporters and opponents. Without making any changes to the interest group preferences which generate Equations (1) and (2), we modify the political party's preferences to include support of or opposition to interest groups' policy goals. More specifically, in the party's problem we add the utility that each interest group receives from policy multiplied by the scalar θ_i, which represents the party's value for the interest group's policy goals. Each interest group is either a supporter with $\theta_i = \theta_S > 0$ or an opponent with $\theta_i = \theta_O < 0$. Thus, the party's problem can be written as

$$\max_{\boldsymbol{\pi}} \left(\sum_{i=1}^{N} \pi_i S_i(\boldsymbol{\pi}) - cA_i(\boldsymbol{\pi}) \right) + \left(\sum_{i=1}^{N} \theta_i \ell_i(\boldsymbol{\pi})^{\gamma_1} A_i(\boldsymbol{\pi})^{\gamma_2} \right)$$

In this modified model, the party's basic problem is the same, but each price it chooses now has an additional effect: Raising prices is less beneficial when the interest group is a supporter, but more beneficial when they are an opponent. While the slightly more complex model no longer gives a simple solution for the price as in Lemma 1, we can show that lobbying and political influence vary in an intuitive way with control of the legislature.

Proposition 2 *In the modified model with heterogeneity, both influence and lobbying are higher for the political party's supporters.*

Thus, our model implies that lobbying activity responds to changes in the partisan control of Congress. Specifically, Congressional influence pressures agencies to implement policies in a way that is more favorable to allies of the party in power. Since agency activities and lobbying are complements, this implies a group's lobbying increases when its allied party is in control. However, since Congressional influence is lower in broad agencies, the swing in policy implementation (and therefore the response of complementary lobbying) should be smaller for broad agencies.

We test this using our 19 years of lobbying data. Specifically, we estimate the following

triple-difference specification:

$$Y_{iat} = \alpha_{ia} + \delta_{at} + \beta_1 1\{i\text{'s supported party controls Congress}\}_{it} + \beta_2 1\{i\text{'s supported party controls Congress}\}_{it} \times Breadth_a + \varepsilon_{iat}$$

where i denotes interest groups, a denotes agency, and t denotes year. We use various outcome measures for Y_{iat}, discussed below.

In this specification, we have already accounted for i's time-invariant tendency to contribute to a (with the group-agency fixed effect α_{ia}) and the universal (i.e., cross-interest-group) tendency to contribute to agency a in a particular year (with the agency-year fixed effect δ_{at}), which might be driven by the changing importance of policy issues that a works on. Identification comes from changes in partisan control of Congress. Specifically, the results reflect the change in a Democratic-leaning interest group's lobbying expenditure when the Democrats controlled Congress from 2007-2010, for instance. Because we include an interaction between agency breadth and the change in partisan control, β_1 reflects the lobbying change for an extremely narrow agency ($Breadth_a = 0$) and $\beta_1 + \beta_2$ is the change for the broadest possible agency (lobbied by all groups, $Breadth_a = 1$).

To measure whether interest group i's supported party controls Congress, we divide all interest groups in the lobbying data into three equally sized terciles based on the share of Congressional campaign contributions going to Democrats. This identifies Republican-aligned groups (the lowest tercile), Democratic-aligned groups (the highest), and centrist groups. We code a group's supported party as controlling Congress only when they control both houses. This setup implies that some groups are not connected to any party (centrist groups) and that in some years neither party controls Congress (the chambers are split), though in the appendix we show that neither issue matters.

There are frequently years in which we see that group i does not lobby agency a, despite doing so in other years. In other words, we often have zeros in our dependent variable. There are many ways to handle this issue, and we choose to be exhaustive. In column 1 we use the inverse hyperbolic sine which, unlike logartihms, is well-defined at zero. In column 2, we isolate the extensive margin of lobbying using an indicator for whether there was any spending (ignoring how much). In column 3, we use logarithms to focus on the intensive margin, implicitly conditioning on there being positive lobbying (which, as we show, is an endogenous control).[26]

Finally, because we include agency-group fixed effects α_{ia}, none of our identification comes from variation in lobbying across agency-group pairs. Thus, in column 4 we normalize each

[26]We show that alignment increasees lobbying on both the extensive (column 2) and intensive (column 3) margins. If the marginal contributions crowded in by partisan alignment tend to be small, then our intensive margin estimates in column 3 will be biased towards zero.

year's lobbying by dividing by the time-invariant agency-group average lobbying: $L_{iat}/\bar{L}_{ia}$.[27] This variable, by construction, has mean 1 within each agency-group pair. Its advantage is that it captures intensive margin changes (unlike column 2) and extensive margin changes (unlike column 3), without relying on extreme non-linearities around zero (unlike the inverse hyperbolic sine in column 1). Coefficients can be interpreted as the change in lobbying, as a percentage of the mean (i.e., $\beta_1 = .2$ would mean a 20% increase over the average level).

The results are shown in Table 6. We find that an interest group increases lobbying expenditures when its aligned party is in power. The main effects (i.e., ignoring the interaction) imply a 30% increase in lobbying, a 2.5 percentage point increase in the decision to lobby, an 11% increase in the amount of lobbying (conditional on doing any), and a 13% increase in lobbying over the mean. All four coefficients are statistically significant ($p < .05$). This is consistent with a model in which lobbying and friendly legislation are complements.

Most important for our purposes, however, is that the interaction term is consistently negative, always large, and statistically significant in three of the four columns (all except for the log lobbying expenditures which have known bias, as discussed above).

To facilitate interpretation, at the bottom of the table, we calculate the implied change in lobbying depending on the breadth of the agency. The results show that the increased lobbying in narrow agencies (those with median or lower breadth) are always large, positive, and statistically significant. For agencies at the 90^{th} percentile, however, the estimated response to changes in control are small, not consistently signed, and never statistically different from zero. In other words, our results suggest that lobbying only responds to partisan control for narrow agencies. In our model, this is because there is little Congressional influence over broad agencies, so the control of Congress is irrelevant.

In Table C10 in the appendix, we show that this conclusion is robust to a number of alternatives, including group-specific linear time trends,[28] restricting to only agencies in our main sample (Table 6 is based on all agencies in the lobbying data, many of which do not appear in our survey-based influence data), excluding years where the chambers of Congress are split (and thus "control of Congress" is ambiguous), and excluding centrist groups (which our main specification does not link to either party). None of these affect our results.

[27] $\bar{L}_{ia}$ is a time-invariant agency-group variable, but it is not literally absorbed by the agency-group fixed effects because it enters the dependent variable non-linearly (as a denominator). If we logged this dependent variable, the denominator would be colinear with the fixed effect, but of course we cannot do this because the variable has zeros (which is what motivates us to use it in the first place).

[28] We do not present specifications with group-by-year fixed effects because they do not allow us to estimate the main effect. When we estimate these regressions, however, the interaction coefficient is unchanged (results available upon request).

Table 6: Lobbying responses to changes in partisan control

	(1)	(2)	(3)	(4)
DV: L_{iat} = Interest group i lobbying expenditures on agency a in year t	$sinh^{-1}(L_{iat})$	$1\{L_{iat} > 0\}$	$\ln(L_{iat})$	$L_{iat}/\bar{L}_{ia}$
Supported Party in Power	0.299***	0.025***	0.106**	0.133**
	(0.102)	(0.009)	(0.050)	(0.055)
Supp. Party Power × Breadth$_a$	-0.702***	-0.062***	-0.128	-0.248***
	(0.222)	(0.018)	(0.078)	(0.092)
R^2	0.541	0.477	0.710	0.060
N	285399	285399	103031	285399
Fixed effects	ia, at	ia, at	ia, at	ia, at
Implied lobbying effects of change in partisan control, by percentiles of agency breadth				
10^{th} percentile of Breadth$_a$	0.292***	0.024***	0.105**	0.131**
25^{th}	0.280***	0.023***	0.103**	0.127**
50^{th}	0.240***	0.019**	0.095**	0.112**
75^{th}	0.126*	0.009	0.075**	0.072*
90^{th}	-0.040	-0.005	0.044	0.013

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an interest group-agency-year triad. Standard errors (two way clustered at the agency and interest group levels) are in parentheses. All columns include agency-by-group fixed effects and agency-by-year fixed effects. Column 4 ($L_{iat}/\bar{L}_{ia}$) is observed lobbying divided by the time-invariant agency-group mean.

6 Conclusion

We have studied the relationship between regulatory agency breadth and congressional influence. We first show theoretically that the party in power in the legislature will exert more effort influencing very narrow agencies, because their incentives for influence declines when the agency becomes broad. We then show empirically that breadth is negatively correlated with congressional influence. This relationship is robust to a number of identification strategies, and the empirical mechanism is consistent with the implications of the model.

Our model has taken the allocation of interest groups across agencies as given. A clear pathway for future work is to study the strategy behind agency formation and interest group allocation. Those forming the agency must consider both how they will be able to influence it,

as well as how the opposition will do so in the event that they come into power. At the same time, technical and practical concerns create constraints preventing agencies from being too broad or narrow. Empirically, it would be useful to generate data that studies how congressional influence varies over time, as well as cross-country analysis comparing how differing institutions and political environments affect agency formation.

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Agency Breadth and Political Influence
Online Appendix

A Theory

Lemma 1 *In the unique pure strategy subgame perfect equilibrium, the political party charges price*

$$\pi_i^* = -\frac{c(\gamma_1 - 1)}{\gamma_2}((N-1)\eta + 1)$$

to interest group i for influence.

Proof. Given interest group i's problem, we take first order conditions over ℓ_i and S_i to obtain

$$\begin{aligned}
\ell_i\text{:}\ & \omega\gamma_1\ell_i^{\gamma_1-1}\left(S_i + \eta\sum_{j\neq i} S_j\right)^{\gamma_2} = 1 \\
S_i\text{:}\ & \omega\gamma_2\ell_i^{\gamma_1}\left(S_i + \eta\sum_{j\neq i} S_j\right)^{\gamma_2-1} = \pi_i.
\end{aligned}$$

Combining these equations and assuming that spillovers won't be too large (which they aren't in equilibrium) gives

$$\begin{aligned}
\ell_i^*(\boldsymbol{\pi}) &= \left(\frac{\gamma_1\pi_i}{\gamma_2}\right)^{-\frac{\gamma_2}{1-\gamma_1-\gamma_2}}(\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}} \\
S_i^*(\boldsymbol{\pi}) &= \left(\frac{\gamma_1\pi_i}{\gamma_2}\right)^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}(\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}} - \eta\sum_{j\neq i} S_j.
\end{aligned}$$

We can then rewrite the best response functions for each interest group as a system of linear equations where

$$\begin{bmatrix} 1 & \eta & \cdots & \eta \\ \eta & 1 & \cdots & \eta \\ \vdots & \vdots & \ddots & \vdots \\ \eta & \eta & \cdots & 1 \end{bmatrix}\begin{bmatrix} S_1^*(\boldsymbol{\pi}) \\ S_2^*(\boldsymbol{\pi}) \\ \vdots \\ S_N^*(\boldsymbol{\pi}) \end{bmatrix} = \begin{bmatrix} \left(\frac{\gamma_1\pi_1}{\gamma_2}\right)^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}(\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}} \\ \left(\frac{\gamma_1\pi_2}{\gamma_2}\right)^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}(\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}} \\ \vdots \\ \left(\frac{\gamma_1\pi_N}{\gamma_2}\right)^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}(\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}} \end{bmatrix},$$

which has the solution given in Equation (2). Substituting this demand back into the party's

problem, we get

$$\max_{\boldsymbol{\pi}} \sum_{i=1}^{N} S_i\pi_i - cA_i = \sum_{i=1}^{N} \pi_i \Bigg[\frac{-(N-2)\eta - 1}{(N-1)\eta^2 - (N-2)\eta - 1} \left(\frac{\gamma_1\pi_i}{\gamma_2}\right)^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}} (\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}} + \sum_{j\neq i} \frac{\eta}{(N-1)\eta^2 - (N-2)\eta - 1} \left(\frac{\gamma_1\pi_j}{\gamma_2}\right)^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}} (\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}} \Bigg] - c\sum_i \left(\frac{\gamma_1\pi_i}{\gamma_2}\right)^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}} (\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}}$$

First order conditions with respect to $\boldsymbol{\pi}$ then eventually lead to Equation (3). ■

Proposition 2 *In the modified model with heterogeneity, both influence and lobbying are higher for the political party's supporters.*

Proof. In the problem with modified party preferences, the party is solving

$$\max_{\boldsymbol{\pi}} \sum_{i=1}^{N} S_i\pi_i = \sum_{i=1}^{N} \pi_i \Bigg[\frac{-(N-2)\eta - 1}{(N-1)\eta^2 - (N-2)\eta - 1} \left(\frac{\gamma_1\pi_i}{\gamma_2}\right)^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}} (\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}} + \sum_{j\neq i} \frac{\eta}{(N-1)\eta^2 - (N-2)\eta - 1} \left(\frac{\gamma_1\pi_j}{\gamma_2}\right)^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}} (\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}} \Bigg] + \sum_{i=1}^{N} \theta_i\omega \left(\frac{\gamma_1\pi_i}{\gamma_2}\right)^{-\frac{\gamma_2}{1-\gamma_1-\gamma_2}} (\omega\gamma_1)^{\frac{\gamma_1+\gamma_2}{1-\gamma_1-\gamma_2}} - c\sum_{i=1}^{N} \left(\frac{\gamma_1\pi_i}{\gamma_2}\right)^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}} (\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}}.$$

Defining N_S as the number of supporters among the interest groups, the first order conditions for the supporters simplify to

$$\left[\left(\frac{-\gamma_2}{1-\gamma_1-\gamma_2}\right)\frac{-(N-N_S-1)\eta}{(N-1)\eta^2-(N-2)\eta-1} + \frac{-\theta_S}{1-\gamma_1-\gamma_2}\right]\pi_S + \frac{(N-N_S)\eta}{(N-1)\eta^2-(N-2)\eta-1}\pi_O^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}\pi_S^{1-\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}} + \frac{\gamma_1-1}{1-\gamma_1-\gamma_2}\frac{(N-N_S)\eta}{(N-1)\eta^2-(N-2)\eta-1}\pi_O = \frac{c(\gamma_1-1)}{1-\gamma_1-\gamma_2},$$

while those for opponents simplify to

$$\left[\left(\frac{-\gamma_2}{1-\gamma_1-\gamma_2}\right)\frac{-(N_S-1)\eta}{(N-1)\eta^2-(N-2)\eta-1}+\frac{-\theta_O}{1-\gamma_1-\gamma_2}\right]\pi_O$$
$$+\frac{N_S\eta}{(N-1)\eta^2-(N-2)\eta-1}\pi_S^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}\pi_O^{1-\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}$$
$$+\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}\frac{N_S\eta}{(N-1)\eta^2-(N-2)\eta-1}\pi_S=\frac{c(\gamma_1-1)}{1-\gamma_1-\gamma_2}.$$

We can then combine these, divide by π_O, relabel $\frac{\pi_S}{\pi_O}$ as R, and divide by $R^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}$. Then we get

$$\left[\left(\frac{-\gamma_2}{1-\gamma_1-\gamma_2}\right)\frac{-(N-N_S-1)\eta}{(N-1)\eta^2-(N-2)\eta-1}+\frac{-\theta_S}{1-\gamma_1-\gamma_2}\right]R^{1-\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}$$
$$+\frac{(N-N_S)\eta}{(N-1)\eta^2-(N-2)\eta-1}R^{1-2\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}+\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}\frac{(N-N_S)\eta}{(N-1)\eta^2-(N-2)\eta-1}R^{-\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}$$
$$-\left[\left(\frac{-\gamma_2}{1-\gamma_1-\gamma_2}\right)\frac{-(N_S-1)\eta}{(N-1)\eta^2-(N-2)\eta-1}+\frac{-\theta_O}{1-\gamma_1-\gamma_2}\right]R^{-\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}$$
$$-\frac{N_S\eta}{(N-1)\eta^2-(N-2)\eta-1}-\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}\frac{N_S\eta}{(N-1)\eta^2-(N-2)\eta-1}R^{1-\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}=0$$

Notice that $\frac{(N-N_S)\eta}{(N-1)\eta^2-(N-2)\eta-1}$ is negative for $\eta<1$, so as $R\to\infty$, the left-hand side of the equation is negative. Furthermore, $\frac{N_S\eta}{(N-1)\eta^2-(N-2)\eta-1}$ is negative, so as $R\to 0$, the left-hand side is positive. When $R=1$, the left-hand side has the same sign as $\theta_O-\theta_S$. This implies that when $\theta_O<\theta_S$, the ratio which solves this is less than 1, so $\pi_O>\pi_S$ and the party charges higher prices to the opposition. Combining this fact with the demand functions for lobbying and influence gives the result. ■

B Data

B.1 Data construction

The CRP lobbying disclosure data sometimes does not include "catcode" (which we refer to as "category" or "interest group"). We exclude those contracts, rather than attempting to manually code them. Data on the amount of lobbying is merged with the agencies lobbied using the unique contract ID provided. Sometimes a contract involves lobbying an agency and a non-agency (e.g., a member of Congress). In these cases, we made no effort to account for non-agencies in our normalization (i.e., we divided the lobbying amount by the number of *agencies* only). However, as we show in Table C3, our results are not sensitive to our normalization.

For campaign contribution data, we use the DIME data from Bonica (2013), though the variables we use are entirely drawn from the CRP version of the data that underlies the DIME. We keep only contributions made by committees (contributor type: C) and to federal House or Senate candidates. We include only contributions with an identified contributor category (our formalization of interest group and again created by the CRP), and exclude transactions of type 24A (independent expenditures *against* the candidate) and 24N (communication costs *against* the candidate) and negative transactions (which most often reflect repayment of a loan that appeared earlier in the data). We also exclude contributions to non-Democrat, non-Republican candidates. We include contributions to candidates from either of these parties, regardless of whether those candidates made it to the general election.

Direct, publicly available replication data from Clinton, Lewis, and Selin (2014) does not include agency names (making it impossible to merge with data from other sources, such as the CRP lobbying disclosure reports). Agency names were added using three different sources: a supplemental file provided by the authors (which was our main source, but excluded agencies with few respondents), response rates included in the replication data and published in the appendix of Clinton et al. (2012), and Clinton and Lewis (2008) scores included in the replication data. Sometimes combinations of these variables were used to match names to agencies. Our sample is smaller than that of Clinton, Lewis, and Selin (2014) partly because some agencies were not included in the lobbying data and partly because some agencies could not be identified. In all regressions, follow Clinton, Lewis, and Selin (2014) to average over all respondents within the agency.

We merged the CRP lobbying data with the Clinton, Lewis, and Selin (2014) and Selin (2015) data using a crosswalk of agency names that we created and are happy to provide to interested researchers. We coded agency birth years ourselves using publicly available sources (primarily Wikipedia).

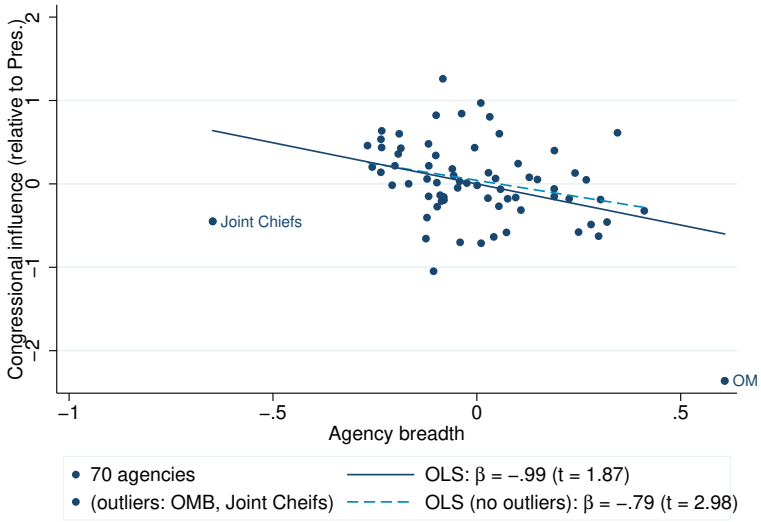
B.2 Data examples

Table B1: Multi-digit interest group codes (Example: H: Health, Education, and Welfare)

2-digit code	3-digit codes (if any sub-codes)	4-digit codes (if any sub-codes)
H0: Health, education, and welfare, NEC		
H1: Doctors and health practitioners	H11: Physicians	H110: Physicians, NEC H112: Optometrists & Ophthalmologists H113: Other physician specialists
	H14: Dentists H15: Chiropractors H17: Other Health Practitioners	
H2: Inpatient health care facilities	H21: Hospitals H22: Nursing homes	
H3: Health services	H30: Health services, NEC H31: Home care H32: Outpatient facilities H33: Optical/vision services H34: Medical laboratories	
H4: Medical supplies	H41: Medical supplies manufacturing H43: Pharmaceutical manufacturing H44: Pharmaceutical sales	
H5: Education	H50: Education, NEC H51: Schools and colleges H52: Technical and vocational schools	
H6: Welfare and social work		

Interest group codes are based on a scheme developed by the Center for Responsive Politiccs (`www.opensecrets.org`), and provided in both campaign contribution data and lobbying disclosure data. We thank them for making this resource available. "NEC" stands for "Not elsewhere classified".

Figure C1: Frisch-Waugh plot of agency breadth and political influence



Each observation is an agency. Corresponding regression results can be found in Table 4 (column 2).

C Additional Results

C.1 Measurement-related robustness

C.2 Sample-related robustness

C.3 Identification

C.3.1 IV strategy

C.3.2 Other identification-related results

C.4 Mechanisms

D Negative Spillovers

This appendix considers a variant of the model of Section 2 which allows for negative rather than positive spillovers. Section D.1 summarizes the changes in the model and gives intuition for how and why results change. Section D.2 contains the formal theory.

D.1 Summary

Allowing spillovers to be negative requires a few changes to the model. First, we assume that the spillover parameter η is negative rather than positive. Thus, additional interest groups being regulated *decrease* the net influence that is exerted in favor of any individual interest group. Making η negative then requires the assumption that rather than effort costs being proportional to the net influence an interest group receives, the party pays for total *absolute* influence.

In the equilibrium of the model in Section 2, the sum of absolute demand for influence depends only on the price the party charges. Because spillovers lead the party to charge higher prices with more interest groups, total demand decreases with N. The model with negative spillovers has two countervailing forces: spillovers still lead to the party charging higher prices with more interest groups, decreasing demand for *net* influence. However, as more interest groups are added each individual interest group demands more positive influence to counteract the negative influence coming from the spillovers, increasing total influence. The sum of these effects can be either positive or negative, depending on the other parameters of the model.

D.2 Theory

Here, we allow for negative rather than positive spillovers in the model. The interest group still receives

$$A_i = S_i + \eta \sum_{j \neq i} S_j$$

total influence, but η will be negative rather than positive. To account for the possibility that A_i is negative, we adjust the interest group's utility function to be

$$\max_{m_i, \ell_i, S_i} m_i + \omega \ell_i^{\gamma_1} \left(\max\{A_i, 0\}\right)^{\gamma_2}$$

Obviously, with this specification the interest group's utility function is not concave in the amount they spend on influence, so we need an additional assumption that η and N are small enough for an interior solution to be optimal. With these assumptions, equilibrium demand for influence will still satisfy Equation (2).

Next, we change the sign of η in the Party's utility function to ensure that it "pays" for the

influence spillovers. The party solves

$$\max_{\boldsymbol{\pi}} \sum_{i=1}^{N} S_i(\boldsymbol{\pi})\pi_i - cS_i(\boldsymbol{\pi}) + c\eta \sum_{j\neq i} S_j(\boldsymbol{\pi}),$$

which can be rewritten as

$$\max_{\boldsymbol{\pi}} \sum_{i=1}^{N} (\pi_i - c + (N-1)\eta c) S_i(\boldsymbol{\pi}).$$

This modified setup leads us to a slightly different pricing function for the party.

Lemma 2 *In the unique pure strategy subgame perfect equilibrium of the game with negative spillovers, the political party charges price*

$$\pi_i^* = \frac{c(1-\gamma_1)}{\gamma_2}[1-(N-1)\eta] \tag{5}$$

to interest group i for influence.

Proof. The proof is algebraic and follows the steps of the proof of Lemma 1. ■

Thus, even with spillovers going in the opposite direction, the relationship between price and N holds: the party charges higher prices when the agency regulates more interest groups.

In the main model, average influence is defined as $\frac{1}{N}\sum_{i=1}^{N} A_i$. This is not the appropriate variable of interest in the model with negative spillovers, because it is not the total amount of influence that the party produces. A_i is the sum of the positive and negative influences on a particular agency, but influence is the sum of the absolute values of these influences. Thus, with negative spillovers we define average influence as

$$\frac{1}{N}\sum_{i=1}^{N}\left[S_i - \eta\sum_{j\neq i} S_j\right].$$

Proposition 3 *When spillovers are negative, increasing agency breadth has an ambiguous effect on the political party's average influence.*

Proof.

The use of symmetry in combination with the above definition gives that average influence is equal to

$$[1-(N-1)\eta]S_i.$$

When we plug in equilibrium prices and demand, we get

$$[1-(N-1)\eta]S_i = \frac{[1-(N-1)\eta]^{\frac{-\gamma_2}{1-\gamma_1-\gamma_2}}}{1+(N-1)\eta}\left(\frac{\gamma_1}{\gamma_2}\right)^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}(\omega\gamma_1)^{\frac{1}{1-\gamma_1-\gamma_2}}\left[\frac{(1-\gamma_1)c}{\gamma_2}\right]^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}.$$

The first part of this equation is the only part which depends on N, so that is what we will focus on. The derivative is

$$\left[\frac{\gamma_2\eta}{1-\gamma_1-\gamma_2}\right][1+(N-1)\eta]^{-1}[1-(N-1)\eta]^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}} - \eta[1+(N-1)\eta]^{-2}[1-(N-1)\eta]^{\frac{-\gamma_2}{1-\gamma_2-\gamma_2}}$$

which further simplifies to

$$\left[\frac{\gamma_2}{1-\gamma_1-\gamma_2}\eta - \eta + \left[\frac{-\gamma_2}{1-\gamma_1-\gamma_2}-1\right](N-1)\eta^2\right]\frac{[1-(N-1)\eta]^{\frac{\gamma_1-1}{1-\gamma_1-\gamma_2}}}{[1+(N-1)\eta]^2}.$$

The sign of this value is ambiguous. For instance, when γ_2 is low, total influence tends to increase with N. On the other hand, when γ_2 is large total influence can decrease with N. ■

Table C1: Robustness to changes in measuring breadth

DV: Influence	(1)	(2)	(3)	(4)
Interest group digits:	1	2	3	4
	Panel A: \$10k cutoff, normalized (main spec.)			
Breadth	-0.927***	-1.002***	-1.072***	-1.069***
	(0.267)	(0.262)	(0.328)	(0.332)
R^2	0.091	0.188	0.187	0.184
N	70	70	70	70
	Panel B: \$100k, normalized			
Breadth	-0.821***	-0.997***	-1.171***	-1.179***
	(0.222)	(0.281)	(0.383)	(0.388)
R^2	0.116	0.186	0.175	0.174
N	70	70	70	70
	Panel C: No cutoff cutoff, normalized			
Breadth	-1.153***	-1.011***	-1.053***	-1.048***
	(0.309)	(0.260)	(0.313)	(0.318)
R^2	0.087	0.185	0.187	0.184
N	70	70	70	70
	Panel D: \$10k cutoff, non-normalized			
Breadth	-1.124***	-1.013***	-1.054***	-1.050***
	(0.313)	(0.261)	(0.314)	(0.319)
R^2	0.081	0.185	0.188	0.185
N	70	70	70	70

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an agency. Breadth is measured as the fraction of interest groups that lobby the agency (for 1-, 2-, 3-, and 4-digit levels of aggregation). The "cutoff" refers to the total amount of lobbying expenditure that must be exceeded (total over a 19-year period) for us to count the interest group as being regulated by the agency. The "normalized" indicates that we divide the amount of a lobbying contract across multiple agencies if it included multiple agencies.

Table C2: Robustness to changes in lobbying data

DV: Influence	(1)	(2)	(3)	(4)
Lobbying data years	1998 - 2016	2001 - 2013	2004 - 2010	2007 - 2007
Breadth	-1.072***	-1.127***	-1.171***	-1.455***
	(0.328)	(0.345)	(0.372)	(0.489)
R^2	0.187	0.193	0.182	0.149
N	70	70	70	70
SD of breadth	0.256	0.247	0.231	0.169
Effect of 1SD change	-0.274	-0.279	-0.271	-0.245

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an agency. Breadth is measured as the fraction of 3-digit interest groups that lobby the agency.

Table C3: Assorted robustness

DV: Influence	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Breadth	-1.072***	-0.811***	-0.859***	-1.218***	-1.731**	-0.796***	-0.858***
	(0.328)	(0.215)	(0.280)	(0.339)	(0.725)	(0.226)	(0.232)
R^2	0.187	0.148	0.141	0.228	0.227	0.195	0.151
N	70	69	70	64	55	70	70
Excluded agencies		OMB (outlier)		Military agencies	Cabinet-level departments		
Weights			Log of emp.			Num. of respondents	Response rate

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an agency. That OMB is an outlier can be seen in Figure 1. Military agencies are the Joint Chiefs of Staff, the Marine Corps, and the Departments of the Army, Navy, Air Force, and Defense. Cabinet-level departments are Agriculture, Commerce, Defense, Education, Energy, Health & Human Services, Homeland Security, Housing & Urban Development, Justice, Labor, State, Interior, Treasury, Transportation, and Veterans Affairs. "Actual respondents" and "response rate" are based on responses to the Clinton, Lewis, and Selin (2014) survey from which our dependent variable is drawn.

Table C4: Periods of rapid agency creation produce narrow agencies

	(1)	(2)	(3)	(4)
Period	Years	Agencies created	Agencies per year	Average breadth
Pre-FDR	158	22	.14	.46
FDR	12	9	.75	.17
Between FDR & JFK	16	11	.68	.42
JFK & LBJ	8	9	1.13	.24
Post-LBJ	40	19	.48	.33

FDR, JFK, LBJ short for Franklin D. Roosevelt, John F. Kennedy, and Lyndon B. Johnson, respectively. All calculations are based on the 70 agencies in our main sample.

Table C5: First stage for IV strategy

DV: Breadth	(1)	(2)	(3)	(4)	(5)
FDR	-0.209***			-0.234***	
	(0.061)			(0.063)	
JFK		-0.136		-0.184	
		(0.144)		(0.148)	
LBJ			-0.127*	-0.167**	
			(0.075)	(0.077)	
FDR+JFK+LBJ					-0.202***
					(0.054)
R^2	0.076	0.008	0.023	0.125	0.121
N	70	70	70	70	70
F	11.81	0.88	2.91	5.13	13.88

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an agency. FDR: Agency was created between 1933 and 1944. JFK: Agency was created between 1961 and 1963. LBJ: Agency was created between 1964 and 1968.

Table C6: Including controls for IV strategy

DV: Influence	(1)	(2)	(3)	(4)	(5)
	OLS	IV	IV	IV	IV
Breadth	-1.072***	-1.492**	-1.775**	-1.351	-1.440
	(0.323)	(0.735)	(0.854)	(1.172)	(0.962)
R^2	0.187	0.158	0.151	0.175	0.167
N	70	70	70	70	70
First stage F		13.9	10.8	5.4	9.0
Controls					
Agency birth year			Yes		
Created under Dem. Pres.				Yes	
Agency is commission					Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an agency. The instrument is an indicator for whether the agency was established during the Franklin D. Roosevelt, John F. Kennedy, or Lyndon B. Johnson administrations, which tended to be less broad agencies.

Table C7: Causal evidence without cabinet-level departments

DV: Influence	(1)	(2)	(3)	(4)	(5)	(6)
Breadth	-1.731**	-1.283	-1.599*	-1.746*	-2.396*	-2.414
	(0.725)	(0.819)	(0.801)	(0.985)	(1.207)	(1.663)
R^2	0.227	0.395	0.346	0.246	0.294	0.191
N	55	54	51	50	5764	55
First stage F						4.4
Controls		CLS-14	Selin-15	Other		
Agency-IG panel					Yes	
IG FE					Yes	
IV						Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an agency. Table excludes 15 cabinet agencies from main sample: Agriculture, Commerce, Defense, Education, Energy, Health & Human Services, Homeland Security, Housing & Urban Development, Justice, Labor, State, Interior, Treasury, Transportation, and Veterans Affairs. Controls in column 2 are drawn from Clinton, Lewis, and Selin (2014): log employment, number of Congressional oversight committees, whether it is a commission, whether agency is cabinet-level, whether it has field offices, the share who are political appointees, whether it was part of the Bush administration's agenda, and the Clinton and Lewis (2008) ideology (missing for one agency). Controls for column 3 are the two independence estimates from Selin (2015). Controls for column 4 are average lobbying spending per group (logged) and agency expertise (see text). The instrument is an indicator for whether the agency was established during the Franklin D. Roosevelt, John F. Kennedy, or Lyndon B. Johnson administrations, which tended to be less broad agencies (see Table C4 for evidence that these periods produced more agencies and Table C5 for the first stage).

Table C8: Robustness to measuring breadth as "Policy areas"

DV: Influence	(1)	(2)	(3)	(4)	(5)	(6)
Policy areas	-0.055***	-0.040**	-0.032**	-0.045***	-0.042**	-0.166*
	(0.013)	(0.019)	(0.015)	(0.016)	(0.016)	(0.095)
R^2	0.171	0.461	0.321	0.183	0.145	
N	60	59	56	57	9160	60
First stage F						3.4
Controls		CLS-14	Selin-15	Other		
Agency-IG panel					Yes	
IG FE					Yes	
IV						Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an agency. "Policy areas" is taken from Clinton, Lewis, and Selin (2014) and measures how many issue areas the agency deals with (1-17). Controls in column 2 are drawn from Clinton, Lewis, and Selin (2014): log employment, number of Congressional oversight committees, whether it is a commission, whether agency is cabinet-level, whether it has field offices, the share who are political appointees, whether it was part of the Bush administration's agenda, and the Clinton and Lewis (2008) ideology (missing for one agency). Controls for column 3 are the two independence estimates from Selin (2015). Controls for column 4 are average lobbying spending per group (logged) and agency expertise (see text). The instrument is an indicator for whether the agency was established during the Franklin D. Roosevelt, John F. Kennedy, or Lyndon B. Johnson administrations. The instrument does significantly decrease the number of policy areas ($p = .072$), though the F-statistic falls short of conventional standards. "IG" stands for "Interest group".

Table C9: Estimated coefficients on controls

DV: Influence	(1)	(2)	(3)	(4)
Breadth	-1.072***	-0.987*	-0.941**	-0.950*
	(0.328)	(0.562)	(0.456)	(0.564)
Log employment		0.112		
		(0.076)		
Commission		-0.232*		
		(0.125)		
Num. of oversight committees		0.743**		
		(0.295)		
Cabinet-level agency		0.185		
		(0.175)		
Has field offices		0.153		
		(0.320)		
Political appointee share		0.983		
		(0.604)		
Explicit part of Pres. Bush's policy agenda		-0.048		
		(0.124)		
Clinton-Lewis agency ideology measure		0.017		
		(0.080)		
Statutory decision-maker independence			0.097	
			(0.183)	
Statutory independence from political review			0.126	
			(0.129)	
Log average lobbying expenditures per group				-0.023
				(0.090)
Agency expertise				0.447
				(0.409)
R^2	0.187	0.394	0.292	0.188
N	70	69	66	65
Controls		CLS-14	Selin-15	Other

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an agency. Column 2: 8 controls from Clinton, Lewis, and Selin (2014) are log employment, number of Congressional oversight committees, whether it is a commission, whether agency is cabinet-level, whether it has field offices, the share who are political appointees, whether it was part of the Bush administration's agenda, and the Clinton and Lewis (2008) ideology (missing for one agency). Column 3: 2 controls are independence estimates from Selin (2015). Column 4: 2 controls are average lobbying spending per group (logged) and agency expertise (see text).

Table C10: Robustness of estimated lobbying responses

	(1)	(2)	(3)	(4)
DV:	$sinh^{-1}(L_{iat})$	$1\{L_{iat} > 0\}$	$\ln(L_{iat})$	Norm. L_{iat}
	Panel A: Including group-specific linear trends			
Supported Party in Power	0.264***	0.021**	0.126**	0.135**
	(0.098)	(0.008)	(0.050)	(0.055)
Supp. Party Power × Breadth$_a$	-0.651***	-0.055***	-0.138*	-0.230**
	(0.210)	(0.017)	(0.072)	(0.089)
R^2	0.716	0.680	0.994	0.183
N	285399	285399	103031	285399
	Panel B: Main sample agencies only			
Supported Party in Power	0.342**	0.026*	0.116*	0.140*
	(0.161)	(0.014)	(0.065)	(0.078)
Supp. Party Power × Breadth$_a$	-0.739**	-0.063**	-0.134	-0.233**
	(0.290)	(0.024)	(0.097)	(0.115)
R^2	0.555	0.485	0.705	0.034
N	167542	167542	70627	167542
	Panel C: Excluding years where chambers are split			
Supported Party in Power	0.314***	0.026***	0.124**	0.153**
	(0.104)	(0.009)	(0.049)	(0.060)
Supp. Party Power × Breadth$_a$	-0.622***	-0.057***	-0.129*	-0.228**
	(0.215)	(0.018)	(0.075)	(0.097)
R^2	0.554	0.492	0.712	0.080
N	225315	225315	76836	225315
	Panel D: Excluding centrist groups			
Supported Party in Power	0.371***	0.032***	0.095*	0.158***
	(0.110)	(0.010)	(0.050)	(0.059)
Supp. Party Power × Breadth$_a$	-0.655***	-0.061***	-0.102	-0.238**
	(0.219)	(0.018)	(0.076)	(0.093)
R^2	0.526	0.469	0.696	0.067
N	170392	170392	59335	170392

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an interest group-agency-year triad. Standard errors (two way clustered at the agency and interest group levels) are in parentheses. All columns include agency-by-group fixed effects and agency-by-year fixed effects. Normalized lobbying (column 4) is given by observed lobbying divided by the time-invariant agency-group mean: $L_{iat}/\bar{L}_{ia}$.