Policy Consequences of Revolving-Door Lobbying

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Abstract

This article presents the first direct analysis of the influence of revolving-door lobbyists over the content of adopted public laws. We use earmarks to evaluate both the effects of lobbying and the possible additional effects of lobbying by individuals who formerly worked as congressional members and staff. Employing a fixed-effects panel approach, we evaluate original data describing the lobbying efforts of the more than 5000 accredited U.S. colleges and universities over a 12-year timeframe. Our analysis indicates that schools that lobby in a given year can expect to receive 54 percent more earmarks and 24 percent more earmarked funds relative to other schools and other years. Further, there is an additional significant effect of revolving-door lobbying that is greatest at lower levels of lobbying expenditures. Our results contribute to the emerging literature on comparative lobbying and speak to concerns about the possible corrupting influence of revolving-door lobbying over public policy.

A rapidly developing literature has emerged investigating the nature of the so-called revolving door between government office holders and interest group lobbyists. Studies of revolving-door lobbying extend from the American States (Newmark 2017, Strickland 2020) and the U.S. Congress (Vidal et al. 2012, Makse 2016, LaPira and Thomas 2017, McCrain 2018, Liu 2020, Shepherd and You 2020, Weschle 2021) to Europe (Blach-Ørsten et al. 2017, Cerrillo-i-Martínez 2017, Dialer and Richter 2019, Silva 2019, Luechinger and Moser 2020, Belli and Bursens 2021), Argentina (Freille et al. 2019) and Australia (Robertson et al. 2019), among others. Yet despite the considerable attention the phenomenon has attracted, very few studies measure just how much influence revolving-door lobbyists wield. As a result, while many political actors and observers *assume* that revolving-door lobbyists have outsized influence on policy outcomes, there is very little direct evidence supporting that conclusion.

Moreover, the effects of lobbying itself are far less clear than is often assumed. Although a large number of studies examine the link between lobbying activity and public policy outcomes (see Bombardini and Trebbi 2020 for a review), threats to methodological inference make it difficult to draw definitive conclusions despite suggestive empirical relationships.

This article seeks to address these shortcomings in the literatures on lobbying and revolving-door lobbying by making two methodological improvements to previous work. First, our data circumscribe a complete universe of actors who are transparently interested in a specific legislative result, only some of which lobby. This sampling frame helps us avoid the problem of selecting on the dependent variable, as it is often difficult to identify entities that are interested in a policy outcome but do not lobby for or against it (Lowery 2013). Second, because our dataset covers the same organizations over a 12-year period, we use fixed effects

to control for the (mostly) time-invariant characteristics of each institution. This panel data approach builds on studies of the efficacy of lobbying across institutions (e.g., Apollonio 2005, Lowery 2007, Dür and Mateo 2012, Schlozman et al. 2012, Dür et al. 2015) by focusing in on variations in lobbying activity within the same institutions.

Our study casts needed light on the relative ability of interest organizations to secure desired policy outcomes as a function of their lobbying activity. It is also the first study we know of that directly examines whether revolving-door lobbyists have disproportionate influence over the substance of legislative outcomes relative to other lobbyists. Our findings speak to ongoing debates over the propriety of revolving-door lobbying and how legislatures should address the challenges presented by this type of advocacy, as well as the recent return of earmarks to the congressional appropriations process. Finally, since revolving-door lobbying occurs not just in the U.S. Congress but in all 50 states and in countries around the world (such as the UK, Austria, and The Netherlands, which are now considering laws to govern the revolving door), our findings are clearly generalizable across a range of organizations, policy domains, and institutional settings.

The Case of U.S. Congressional Earmarks

Members of Congress have considerable individual power to determine the geographic allocation of government money, and no type of "pork barrel" spending is more individualistic than the earmark.[1] Briefly, earmarks are congressional appropriations inserted into spending bills at the request of a single member. Typically, organizations in each member's constituency present the member with funding proposals; members then choose which projects to support in the funding process. Members have both wide discretion to back any projects they want to

and considerable influence over which projects ultimately get funded. Unlike some other congressional funding mechanisms, earmarks publicly identify the sponsoring legislator, thus creating good will the member among constituents (Lee 2003). In short, securing earmarked funding allows members of Congress to maximize all three of their reelection-motivated activities—position-taking, advertising, and credit-claiming (Mayhew 1974).

At the same time, earmark-seeking organizations compete against each other for earmarked dollars. The literature has established that members of Congress engage in zero-sum competition with one another to maximize their share of pork-barrel spending (e.g., Bickers and Stein 1996, Balla et al. 2002, Lazarus 2010). Less well appreciated, however, is that a similar situation exists for all of the interests within a Congress member's constituency that want this funding. If a member has only a fixed amount of money to distribute around the constituency (an oversimplification of the earmarking process, but not inaccurate), everyone in the state or district that seeks this funding is likewise engaged in a zero-sum competition with one another for those limited funds.

The combination of earmarks' individualized nature and competition among earmark-seeking organizations creates opportunities for lobbyists to significantly influence which projects ultimately get funded. Earmark seekers hire lobbyists to provide legislators with evidence that a given project would benefit the local community (as well as the member of Congress) and to urge the member to select their client's project for backing. In contrast to efforts to get a law passed or an amendment added on the floor, which require the actions of many members of Congress, an organization's lobbying activity aimed at a single legislator has a non-trivial chance of success in the form of earmarked funding.

Colleges and universities are commonly found among earmark-seeking organizations. Congressional earmarks have long been a common source of funding for U.S. universities' research, construction, and capital-intensive projects (Savage 1991, Balla et al. 2002, de Figueiredo and Silverman 2006, Kaiser 2010). During our time period (1999-2010), congressional earmarked funds amounted to 23 percent—and reached as high as 36 percent—of all U.S. federal dollars that went to higher-education institutions for research and development.[2] Moreover, American universities commonly engage lobbyists to advocate for congressional funding including earmarks. Although not every lobbyist hired or contracted by an institution of higher learning lobbies primarily for earmarks, a study of how the U.S. government funds academic science programs (Savage 1999) and an account of the rise of earmarking from the point of view of a lobbyist (Kaiser 2010) both describe academic lobbying for earmarks as a common element of university-government interactions. Colleges and universities enjoy relatively high esteem in their communities and are supported by a diverse set of groups, making them ideal for legislators to attach their names to.[3]

The Effectiveness of Lobbying

Despite the widespread perception that lobbying has an immense effect on public policymaking, surprisingly few studies have systematically demonstrated that lobbying influences public policy (Lowery 2013). In a review of the interest groups literature, Baumgartner and Leech (1998, 130) found that just six of 15 studies concluded that lobbying influenced congressional votes. Later, in the broadest study to date of the influence of lobbying over U.S. government policy, Baumgartner et al. (2009) found that lobbying intensity, as

measured by spending on lobbying and PAC contributions, was unrelated to a group's ability to secure its preferred policy outcomes.

Recent research has been more unified in concluding that lobbying does have an influence on policy outcomes, and in a variety of settings. Businesses that spend more on lobbying pay lower effective tax rates (Richter et al. 2009) and benefit disproportionately from tax holidays (Alexander et al. 2009). Greater lobbying intensity is a strong predictor of whether a policy proposal is adopted in Congress (McKay 2012) and in U.S. states (Shipan and Volden 2006, 2008, Grasse and Heidbreder 2011). Cities that lobby the U.S. government are more likely to receive congressional earmarks or recovery grants than cities that do not lobby (Goldstein and You 2017). Lobbyists who hold fundraisers for legislators can successfully induce U.S. senators to insert favourable language into legislation (McKay 2018). As well, a growing consensus in studies of lobbying outside the U.S. finds evidence of lobbying effects (although the effects may be conditional; see Klüver 2011, Dür et al. 2015, Giger and Klüver 2016, Rasmussen et al. 2018, Hopkins et al. 2019, Junk 2019).[4]

We posit that hiring a lobbyist gives an entity that is competing for congressional earmark dollars an advantage in that competition. Our first hypothesis is that *for U.S. colleges and universities, success in obtaining congressional earmarked funds is positively related to lobbying activity.*

Revolving-Door Lobbyists

Normative concerns about the revolving door are motivated by the possibility that those lobbyists who formerly worked in government (which we term 'revolvers') may provide unfair advantages to certain interest organizations (Alexander et al. 2009, Cerrillo-i-Martínez 2017,

Robertson et al. 2019, Silva 2019, Belli and Bursens 2021). Researchers have found an abundance of evidence that revolvers are perceived to be especially effective at policy influence. Relative to other lobbyists, revolving-door lobbyists make higher salaries, are retained by a larger number of clients, and generate more revenue for their employing firms (Vidal et al. 2012, LaPira and Thomas 2017, McCrain 2018, Strickland 2020), and companies are more likely to prefer revolvers over conventional lobbyists when policy uncertainty is greater (Liu 2020). Despite this perception of influence, only a few studies measure the effects of revolving-door lobbying on policy outcomes, however, and those measures tend to be indirect. LaPira and Thomas (2017) and Makse (2016) show that revolving-door lobbyists are more likely to work on U.S. congressional legislation that has a stronger chance of passing. And Baumgartner et al. (2009) find that lobbyists who have previous U.S. government experience are significantly more likely than other lobbyists to realize their preferred legislative outcomes (2009, 203).

Revolvers have two advantages over conventional lobbyists. The first advantage is informational: revolving-door lobbyists know more about the policymaking process than conventional lobbyists. LaPira and Thomas (2017) argue that revolving-door lobbyists' most important advantage is the specialized knowledge about the policy process that they can bring to bear when advocating for their clients (also see Parker et al. 2012, Makse 2016). In addition to this process knowledge, former congressional staff have a better sense of the personalities involved: which legislators are most necessary to win over in order to move a bill in a desired direction and which legislators might be more susceptible to influence on particular measures. This gives revolving-door lobbyists a leg up in knowing which legislators or staff to approach,

when to approach them, and what to say during a meeting to best make their clients' case. This package of insider process knowledge, LaPira and Thomas (2017) argue, makes revolving-door lobbyists more effective at policy advocacy than conventional lobbyists.

Second, revolvers start the job with a more extensive network of contacts in the legislature relative to conventional lobbyists. Revolving-door lobbyists' experience in the legislature puts each of them in contact with elected representatives and staff members in dozens if not hundreds of legislative offices. This network gives them, at least initially, access to a greater number and variety of legislative offices than conventional lobbyists have (Vidal et al. 2012, McCrain 2018). Revolvers may be able to secure meetings in more legislative offices—and perhaps find better reception in those meetings—than conventional lobbyists. When former legislative staff become lobbyists, they contribute their legislative contacts to the pool of their new employer's resources to further increase access for the organizations they now represent. From this perspective, revolvers' true value as lobbyists lies not in what they know, but in who they know.

We are agnostic as to the source of revolvers' comparative advantage. Indeed, we see no reason why expertise and connection networks cannot both be valuable to the lobbyist, and by extension to the client (e.g., Bertrand et al. 2014). Whether revolvers' advantages are a function of more detailed knowledge of the policy process, their extensive lists of congressional contacts, or a combination, the predicted substantive outcome is the same: revolving-door lobbyists should be able to ensure that their clients' preferred outcomes are included in bills—and help navigate these bills through the legislative process—more successfully than conventional lobbyists.

There is one additional factor to consider in framing our second hypothesis. Lobbying organizations report their combined expenditures on lobbying each reporting period. These lobbying expenditures represent the combined salaries of the lobbyists working for the client in a period (and not any decisions by lobbyists about how that money is spent). Given that revolvers, on average, charge their clients more than nonrevolvers charge, lobbying expenditures should grow higher as the number of revolvers increases. Therefore, school-years that spend more on lobbying—and which are more likely to have revolvers as lobbyists—do not benefit as much from having a revolver compared to school-years that spend less. To capture this intuition, we create an interaction term between revolving-door lobbying and the amount of money a school spends on lobbying. Our second hypothesis, then, is that *for U.S. colleges and universities, success in obtaining congressional earmarked funds is positively related to revolving-door lobbying activity, at least at lower levels of lobbying expenditures.*[5]

Data

We assume that all colleges and universities would rather receive congressional funding via earmarks than not to receive these funds. Further, we assume that all higher-education institutions are free to lobby, and that those that do lobby are doing so at least in part toward the same end: to obtain government funding to support their research and teaching activities.

Universe: Carnegie Foundation's list of American colleges and universities

We use as our universe the comprehensive list of all accredited American colleges and universities as published in 2000, 2005, and 2010 by the Carnegie Foundation for the Advancement of Teaching.[6] Of these, 19 percent lobbied during our time period. We employed regular expressions to match our list of earmarks to schools on the Carnegie list. We followed

with extensive manual coding using the unique ID numbers generated by the Carnegie Foundation. Schools that were not on the Carnegie list were dropped, because either they did not exist or were not accredited at that time.[7]

Dependent variables: Higher-education earmarked funding, 1999-2010

We again used regular-expressions text-matching followed by manual matching to link our list of educational institutions to a comprehensive list of congressional earmark funds that go to colleges and universities in a given year. Originally, this list was collected and published by private American organizations, but in 2008 President Obama directed the Office of Management and Budget to publish the full list of congressional earmarks. We rely on Citizens Against Government Waste for 1999-2006, and the government list for 2008-2010. There were no earmarks in 2007 due to a one-year congressional ban. Each earmark is listed with its amount, recipient, and the bureaucratic division from whose budget the money was taken.

We use a series of dependent variables that are related but distinct. First is whether or not the school achieved any earmarks in a given year. Second is the logged total dollars in earmarks the school receives in a year. Third is the number of earmarks received, and fourth is the dollar amount of earmarks received. This approach of analysing multiple dependent variables developed in literature on pork barrel politics because several studies generated hypotheses specific to either earmark counts or dollar totals of earmarks (e.g., Bickers and Stein 1996, Balla et al. 2002). As such, it has become standard in that literature to run identical analyses on multiple dependent variables (e.g., Bertelli and Grose 2009, Lazarus 2010).

Explanatory variables: Lobbying by U.S. colleges and universities, 1998-2009

To briefly reiterate, we propose first, that lobbying matters, and second, that lobbying by those who have been through the revolving door matters even more. We test our first hypothesis with an indicator variable that designates whether the school does *any lobbying* in the year before earmarks are distributed. We obtain this information from the Center for Responsive Politics (CRP), which provides a database of information, derived from lobbyist-filed reports, on its website OpenSecrets.org. We first identified all lobbyists coded by CRP under the industry category Schools and Colleges. We then linked them to the colleges and universities for whom they lobbied using each institution's six-digit Carnegie identification code. In addition to *any lobbying*, we also include the school's total *lobbying expenditures* to control for the intensity of lobbying. We divide the raw dollar amounts by 1,000,000 to make the coefficients easier to interpret.

We test our second hypothesis – that revolving-door lobbyists are better able to secure earmarks than other lobbyists – with a dummy variable indicating whether the school uses *any revolvers*. We employ a dichotomous measure of revolving-door lobbying because we suspect that the difference between having any revolvers and having none is of greater consequence than the difference between a smaller number of revolvers and a larger number. One revolver can share with the rest of her organization considerable information about the inner workings of Congress, or at least of a particular member's office, and having additional revolvers as lobbyists is likely to have diminishing returns. In addition, since we control for lobbying expenditures as well as whether any lobbyists work on education issues for the client, we do not need our revolving door variable to exhibit the magnitude of an organization's lobbying

effort.[8] Finally, we created an interaction term to assess whether revolvers might influence the efficiency of lobbying. This variable, *any revolvers X expenditures*, is the product of whether any revolvers lobbied for the school and the expenditures incurred. This variable can be interpreted as the marginal effect of additional lobbying expenditures for those schools that use revolvers.

To determine whether each lobbyist in the dataset is a revolver or not, we initially examined the open field in which lobbyists list their self-declared covered official status as a previous congressional employee (as also done by Baumgartner et al 2009 and others). We searched this field using regular expressions in Stata 16 to identify words that clearly indicated congressional experience (e.g., leg(is)?(lative)?ass(istan)?t?|Sen(.|ator)?|Rep(.|resentative)?). Our theory does not extend to former employees of U.S. government agencies, and so we do not search for these in the lobbying data. As the Center for Responsive Politics cleaned and consolidated its lobbying data, we too enhanced our set of revolvers using these data. Since we know there is a bias toward underreporting of lobbyists' previous U.S. government experience (Thomas and LaPira 2017), we generally converted lobbyists we had designated as nonrevolvers to revolvers when there was evidence of this, and not the other way around. If a lobbyist has any covered official status in one time period, we assume that she is a covered official throughout her career, since it is rare that staff lobby first, then move to U.S. government staff, then move back to lobbying, and we found no instances of it. We supplemented our revolving-door data with the dataset of LaPira and Thomas (2017), which the authors generously provided to us.[9]

All models include fixed effects for both the institution over time and for each year in the time period. Our use of fixed effects means the model drops any school that does not vary

in receiving earmarks over time (thus dropping schools that always or never receive earmarks). This reduces the N from about 44,000 school-years to about 16,000. Lastly, because congressional budgets are passed a year prior to when the spending actually takes place, we lag lobbying by one year. For instance, the earmarks that distributed money during Fiscal Year 2010 were part of a budget that was passed by Congress in calendar year 2009, and we use as our independent variable the lobbying activity that took place in 2009. Given that no earmarks were awarded in 2007, we drop lobbying that occurred in 2006.[10] The over-time patterns of lobbying and earmarks are illustrated in Figure 1, and summary statistics appear in Table 1.

Results

Figure 2 presents the bivariate relationship between the earmarked dollars the school receives and the numbers of its lobbyists and revolving-door lobbyists, respectively. We see that hiring more lobbyists, and hiring more revolvers, are both associated with receiving more earmarked dollars for the school. Further, we see that the slope on the number of revolvers (represented by the thicker line) is greater than the slope on the number of lobbyists (the thinner line), suggesting that revolvers bring an additional marginal benefit to the schools that hire them. It is worth noting, however, that because there are always at least as many lobbyists as there are revolvers, the range of the x-axis is greater for lobbyists than for revolvers, and this fact contributes to the difference in slopes.

Figure 1. Lobbying activity and education earmarks over time

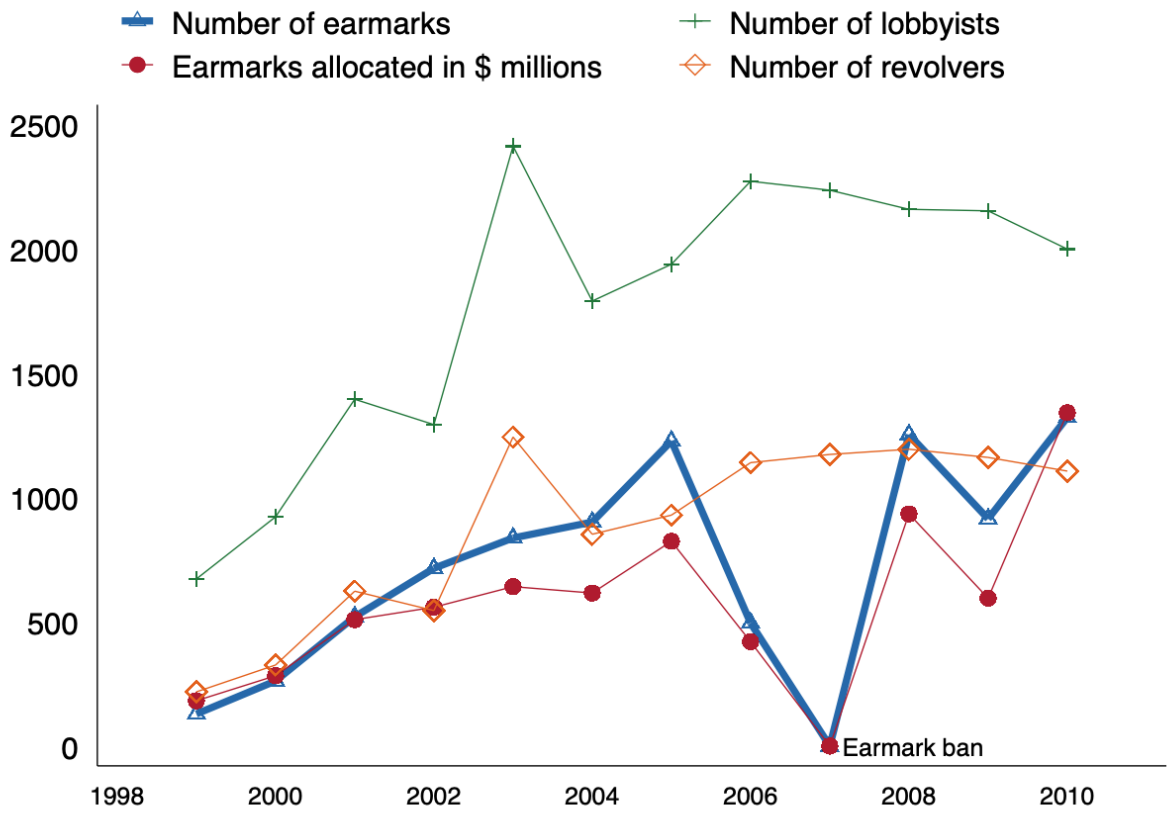
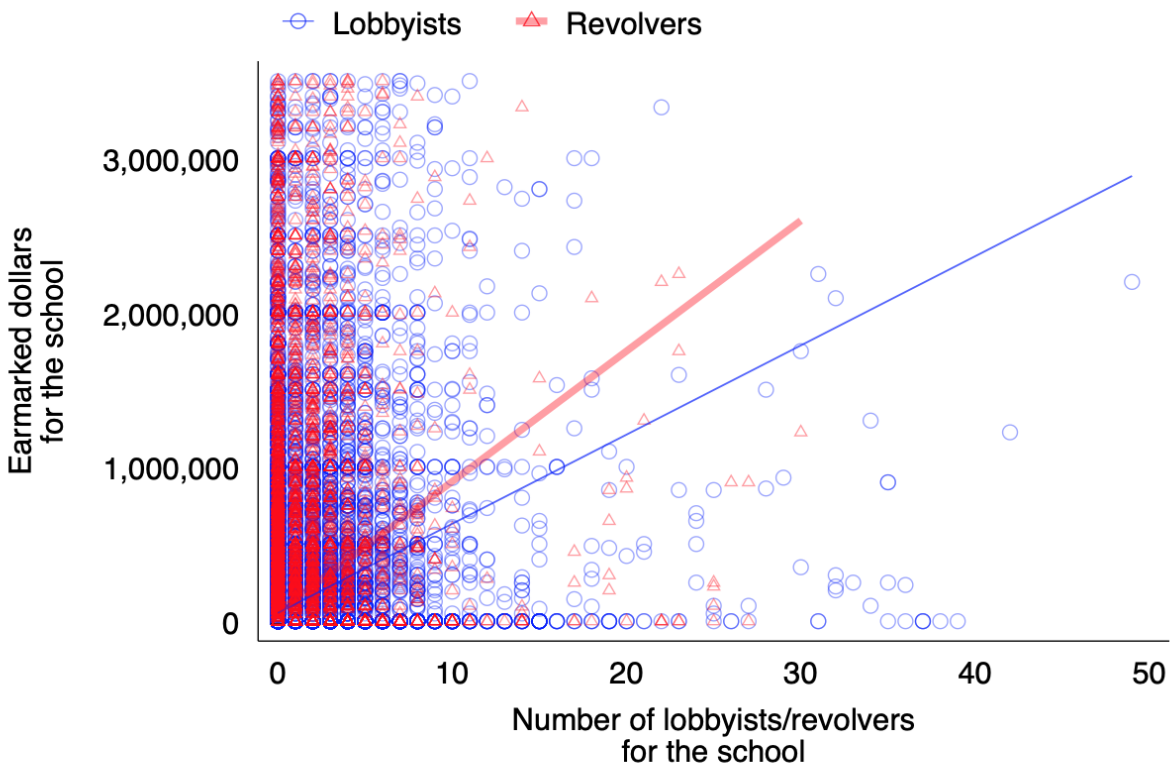


Table 1. Summary statistics

Variable	Min.	Max.	Mean	S.D.	N
Number of earmarks	0	20	.19	.75	44,213
Earmarked dollars	0	100,500,000	156,092	1,151,382	44,213
Logged earmarked dollars	0	18	1.5	4.2	44,213
Number of lobbyists	0	49	.43	1.8	44,213
Number of revolvers	0	30	.21	1.1	44,213
Whether any lobbyists	0	1	.11	.31	44,213
Whether any revolvers	0	1	.076	.26	44,213
Lobbying expenditures ($ millions)	0	2.6	.012	.058	44,213

Notes: The unit of analysis is the school-year dyad, where every school is paired with every earmark year unless the school is not on the Carnegie Foundation's list in that year or in the preceding four years. About 89 percent of dyads are empty of both lobbying and earmarks. Lobbying in 2006 is dropped since no earmarks were given in 2007.

Figure 2. Bivariate relationship between the number of lobbyists or revolvers and the dollars received in earmarks, 1998-2010



Note: The 1% highest earmark amounts are omitted for legibility.

To better understand this relationship, we estimate several different models, including the variables described above, which we present in Table 2. Models 1 and 2 are our most basic models: First is a logistical estimation of whether the school receives any earmarks in the year (we present the results using odds ratios); second is an ordinary least-squares regression that estimates the logged number of earmark dollars received.

Next, to estimate the effect of lobbying and revolving-door lobbying on the number and size of earmarks, we employ fixed-effects Poisson regression for panel data using robust standard errors.[11] The Poisson models estimate the number of earmarks each school receives and the aggregate dollar value of those earmarks, respectively. We present incidence rate ratios (IRRs), which describe the rate of the dependent variable as a function of the explanatory variable relative to the base category. (For example, if the IRR is 2, it implies that the rate of earmarks is twice as great when lobbying = 1 as when lobbying = 0.) Poisson models 5 and 6 restrict the sample to just those institutions that lobby, to estimate whether such schools receive an additional marginal benefit from using revolving-door lobbyists.

Table 2. Models predicting earmarks and earmarked dollars as a function of lobbying in the previous year, 1998-2010

	Logit odds ratios	OLS coef-ficients	Fixed-effects Poisson incidence-rate ratios			
	Whether the school gets any earmarks (1)	Earmark dollars (ln) (2)	Number of earmarks (3)	Earmark dollars (4)	Number of earmarks (5)	Earmark dollars (6)
Whether the school does any lobbying	**2.08** (.25)	**1.70** (.29)	**1.54** (.13)	**1.24** (.12)		
Whether the school uses any revolvers	**1.69** (.22)	**1.42** (.33)	**1.25** (.09)	1.14 (.11)	**1.29** (.10)	**1.25** (.14)
Lobbying expenditures (in $ millions)	**45.02** (56.64)	**10.83** (3.15)	3.00 (1.69)	**6.49** (4.53)	**6.53** (3.32)	**10.56** (8.05)
Any revolvers X lobbying expenditures	**.08** (.10)	**-6.91** (3.15)	**.36** (.19)	**.20** (.13)	**.21** (.10)	**.13** (.09)
Constant		**.60** (.10)				
N	15,717	15,955	15,955	15,955	4,223	4,223
Number of schools	1,454	1,478	1,478	1,478	627	627
Pseudo-R2	.17					
Overall R2		.18				
Rho	.54	.24				

Notes: Estimates in bold are significant at p<.05. The unit of analysis is the school-year dyad. Robust standard errors appear in parentheses below each estimate. All models use fixed effects for the year and for the school. All models drop schools for which there is no over-time variation in the dependent variable. Lobbying activity is lagged by one year, except that lobbying in 2006 is dropped since no earmarks were granted in 2007. Earmarks awarded to a university system and lobbying by university systems are excluded. School-years are dropped when they are absent from the Carnegie Foundation's list.

First hypothesis: Lobbying is more effective than not lobbying

Because our dataset contains all colleges and universities that could have lobbied, whether they did so or not, we can first observe how often schools that do not lobby nonetheless do receive earmarks. As shown in Table 3, of the 39,398 school-year dyads that contain no lobbying activity, 6 percent still receive an earmark the following year. Yet while

lobbying is not essential to securing earmarks, it certainly seems to help. Of the 4,815 school-years that contain lobbying activity, 53 percent (2,538) receive one or more earmarks (Table 3). Further, lobbying may also have a cumulative effect on earmark allocation: over the full 12-year period, of the 865 institutions that ever lobby, 85 percent are successful at least once.

Table 3. Distribution of school-years by lobbying and earmarks, 1998-2010

			Schools that lobby	
	School does not lobby	School lobbies	Lobbies with revolvers	Lobbies without revolvers
No earmark	93.7%	47.3%	44.0%	54.0%
Earmark	6.3%	52.7%	56.0%	46.0%
N	39,398	4815	3341	1494
Note: Cells constructed from the number of school-year observations in each category.				

Turning now to the models shown in Table 2, the effect of lobbying is positive and significant across all four dependent variables and in each of the logit, OLS, and Poisson models. The logit model (model 1) indicates that the log-odds that a school receives any quantity of earmarks are 2.08 times greater when the school lobbies compared to when it does not. The average marginal effect (Hanmer and Kalkan 2013) of lobbying in model 1 is to increase the predicted probability of getting an earmark from .08 to .13.

The OLS model (model 2) predicts that schools that lobby increase the predicted effect on logged earmark dollars in the following year by 70 percent relative to school-years that do not contain lobbying. Controlling for the other variables in the model as well as the school and year fixed effects, lobbying in a given year increases the predicted logged earmark dollars received the following year from 2.80 to 7.79. Exponentiating these predictions leads to biased estimates (Wooldridge 2010), so we do not do so here. Instead, we next employ Poisson models predicting the number and raw dollar value of earmarks.

Poisson models 3 and 4 suggest respectively that lobbying results in an increase of 54 percent in the rate of earmarks, and an increase of 24 percent in the rate of earmarked dollars, relative to not lobbying, while also controlling for revolving-door lobbying, lobbying expenditures, and an interaction between revolvers and expenditures.

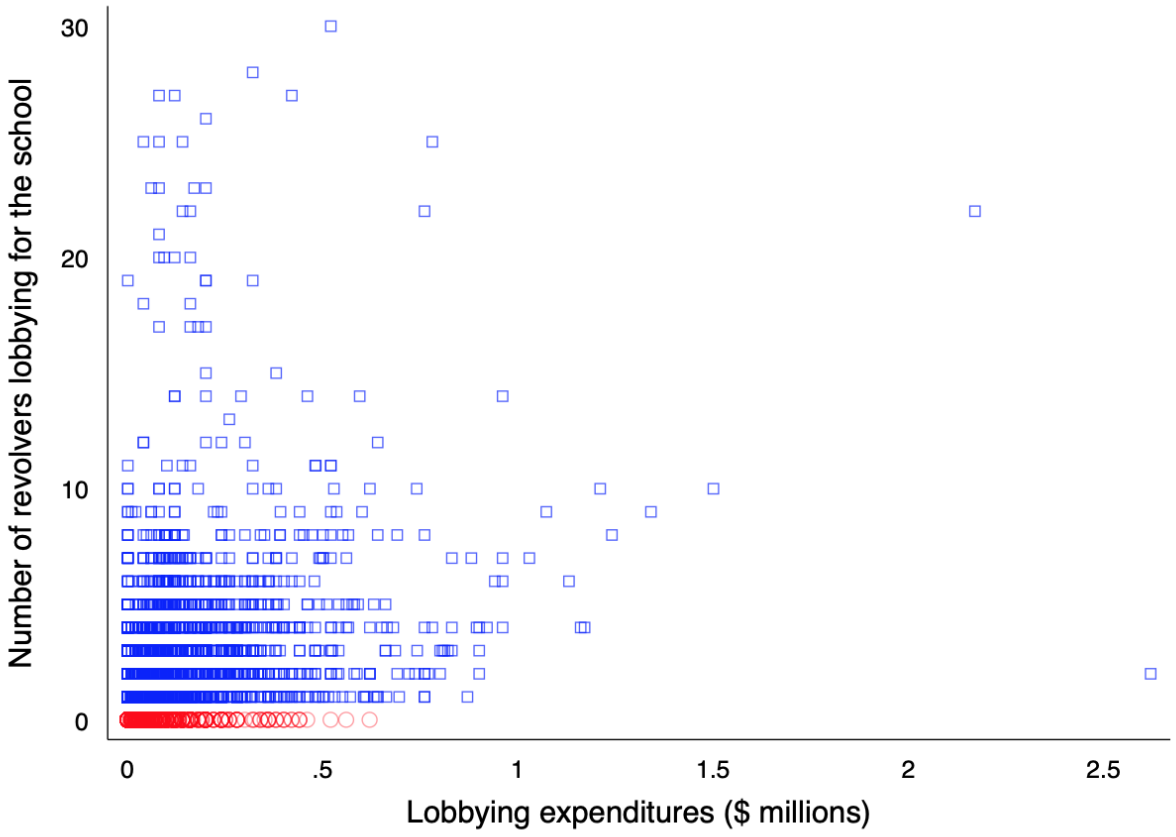
Regarding control variables, lobbying expenditures have a positive effect in all models that is significant except in model 3. And, as we might expect given the changing nature of congressional politics and in the broader economy over time, the effect of the year (the year fixed-effect) is significant in all cases, suggesting that there were better and worse years for congressional earmark seekers.

To summarize the tests of our first hypothesis that lobbying matters, the evidence is clear that the act of lobbying significantly increases the likelihood that a U.S. college or university secures more and higher-dollar earmarks.

Second hypothesis: Revolving-door lobbyists are more effective than conventional lobbyists

As seen in Table 3, of the 4,815 school-year dyads that contain lobbying, 70 percent (3,341) use at least one congressional revolver. Figure 3 illustrates the pervasiveness of revolving-door lobbying in the U.S. Congress, showing that as lobbying expenditures increase, the number of school-years that do not use revolvers (in red circles) grows vanishingly small.

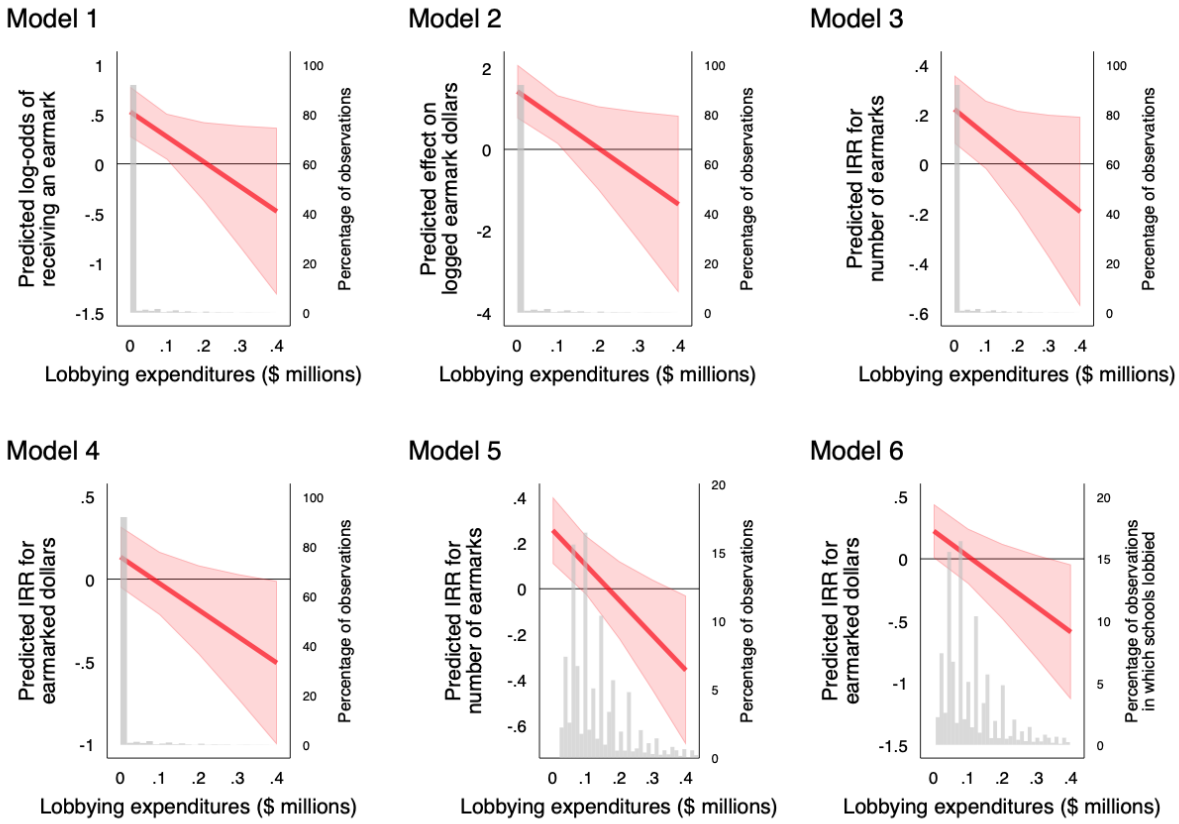
Figure 3. Number of revolvers by levels of lobbying expenditures (N=44,213 school-years)



Returning to the analysis, the results suggests that revolving-door lobbying is even more effective than lobbying alone. First, in the logit model (model 1) shown in Table 2, *any revolvers* is a positive and significant predictor of the probability of receiving an earmark. The joint hypothesis that *any revolvers* and the interaction term are equal to zero (Brambor et al. 2006) can be rejected at p=.09. A one-unit increase in *any revolvers* significantly increases the log-odds of receiving an earmark up until about the 57th percentile of positive lobbying expenditures, or $110,000. This threshold is seen in panel 1 of Figure 4 as the point where the lower confidence bound of the predictive margin crosses zero. (Figure 4 also provides the distribution of lobbying expenditures in gray; see Berry et al. 2012.) For school-years that spend

more than $110,000 on lobbying in a year, the use of revolvers does not significantly affect the predicted probability of getting an earmark. Thus, as expected, there are diminishing returns to using revolvers for school-years that spend more on lobbying.

Figure 4. The effect of lobbying expenditures on the predictive margins for models 1-6



Notes: The panels in Figure 4 each present the contrast in predicted margins for school-years that use revolvers compared to school-years that do not use revolvers, across levels of lobbying expenditures, surrounded by 95% confidence intervals. The distribution of lobbying expenditures is shown in light gray. The highest-spending 5% of schools are included in the models but omitted from the graphs for legibility.

Second, like the logit model, the OLS model shows a positive and significant effect for both *any revolvers* and the interactive term *any revolvers X lobbying expenditure*, and the hypothesis that *any revolvers* and the interaction term are jointly insignificant is rejected at

p=.06. Model 2 shows that the effect of using revolvers on expected earmarked dollars is positive and significant up until about the 60th percentile of positive lobbying expenditures ($120,000), above which the difference is not significant, as illustrated in the second panel of Figure 4. Note that 92 percent of school-years whose lobbying expenditures exceed $120,000 use one or more revolvers, and this fact may explain the lack of significance of revolving-door lobbying here.

In the first Poisson model (model 3), the revolving-door variable is positive and significant. But the joint hypothesis that any revolvers and the interaction term have no effect is significant only at p=.11. As lobbying expenditures increase, the marginal effect of revolving-door lobbying on the rate of earmarks declines, and at about the 51st percentile of positive lobbying expenditures ($90,000), it becomes insignificant, as shown in the third panel of Figure 4. In model 4, the joint hypothesis that *any revolvers* and the interaction term are both zero can be rejected at p=.02. The contrast in margins in Figure 4 indicates that the benefit of revolvers starts above zero but quickly drops as lobbying expenditures increase, and revolvers are not a significant predictor of the total dollars awarded in earmarks.

While models 3 and 4 evaluate the whole population of school-years for which the number of earmarks varies over our 12-year timeframe, Models 5 and 6 evaluate only those school-year dyads that contain lobbying. In model 5, the joint hypothesis that the revolving-door variable and the interaction term are both zero is rejected (p<.00), and revolving-door lobbyists significantly increase the predicted rate of earmarks up until about the 51st percentile of lobbying expenditures ($90,000).

Lastly, in model 6 *any revolvers* is significant at p<.05, and the joint hypothesis that *any revolvers* and the interaction term are zero is rejected at p<.00. By testing across the full range of *lobbying expenditures* for those schools that spend any money on lobbying, we see that the effect of revolvers is significant only for the bottom decile, i.e., school-year dyads in which spending is less than $30,000 (see Figure 4).

To summarize our tests of the revolving-door hypothesis, the analyses indicate that, relative to other school-year dyads whose earmark success varies over time, schools that spend up to $90,000 in a year on lobbying significantly increase the probability of receiving earmarks, and the number and dollar value of earmarks received, if they use revolvers (models 1-3). Further, compared to other school-years that lobby, schools that spend up to $90,000 on lobbying receive more earmarks if they use revolvers (model 5), and schools that spend up to $30,000 on lobbying receive significantly more earmark dollars in a year if they use revolvers (model 6). Above the just-mentioned levels of lobbying expenditures, *any revolvers* becomes insignificant—though lobbying expenditures on their own are positively associated with earmark success in all but one of the models.

The results suggest that revolving-door lobbying better predicts *whether* a school receives an earmark, and if so, the *number of* earmarks won, than it predicts the combined *dollar value* of earmarks won. We think it is plausible that legislators and their staff are inclined to direct earmark money toward their former colleagues, either because of interpersonal relationships (Vidal et al. 2012, McCrain 2018) or because revolving-door lobbyists possess specialized knowledge of policy processes (LaPira and Thomas 2017) that give the revolvers a better understanding of when, how, and whom to lobby for earmarks. However, this enhanced

ability to request earmarks does not equate to a greater dollar value for these earmarks. As the literature on U.S. congressional earmarks demonstrates (e.g., Lee 2003, Engstrom and Vanberg 2010, Lazarus 2010), earmark allocation decisions are influenced by numerous factors specific both to the legislator (committee assignments, leadership status, seniority) and to the constituency (population density, public lands, industries supported), and we do not think these factors are wholly outweighed by the effects of lobbying.

Another implication of these results is that for groups that wish to lobby, revolving-door lobbyists may substitute for greater lobbying dollars: Universities can increase their lobbying success by spending more money or by hiring a revolving-door lobbyist, but doing both generally does not appear to produce more or more valuable earmarks. This pattern of results suggests that the marginal benefit of hiring a revolving-door lobbyist is higher for resource-poor universities that cannot afford lavish lobbying budgets. These universities may be able to compensate for their limited budgets by hiring a revolving-door lobbyist.

We conclude from our analyses that revolving-door lobbyists are likely to be more effective than lobbyists who have not formerly worked in the Congress, at least when lobbying expenditures are lower. Our conclusion is broadly consistent with the findings of Baumgartner et al. (2009) and LaPira and Thomas (2017) showing that the use of revolving-door lobbyists is associated with higher levels of policy success, as well as studies that show that firms associated with revolving-door lobbyists are more financially successful than other firms (Luechinger and Moser 2014, 2020).

Conclusion

This study compiles a three-dimensional dataset that connects lobbying activity, legislative funding, and a comprehensive set of fund-seeking organizations across a 12-year period. By examining a finite universe—accredited American colleges and universities—over time as they vary their lobbying activity, we can say with confidence that an institution's lobbying is associated with more earmarks, and more earmark money, versus not lobbying. The odds that a school in our data receives an earmark are more than twice as great when it lobbies, relative to other schools and other years. The predicted number of earmarks is 54 percent greater, and predicted earmark dollars is 24 percent greater, for school-years that lobby relative to other schools and other years.

In addition to the demonstrated effect of lobbying on earmark outcomes, revolving-door lobbyists seem to bring a further marginal benefit to entities that spend less on lobbying expenditures. Among other results, schools that spend up to $110,000 on lobbying in a given year significantly increase the probability of receiving an earmark by using revolving-door lobbyists (model 1). Restricting the comparison to just those school-year dyads that contain lobbying, schools that spend up to $90,000 in a year significantly increase their rate of earmarks by using a revolver (model 5). Above these thresholds, while further lobbying expenditures are associated with greater earmark success, there is no apparent additional benefit to using revolvers. This lack of significance is not surprising given that few school-years lobby at high levels of expenditures without using revolving-door lobbyists.

By selecting a domain in which every member of a universe is (at least in principle) interested in a policy outcome but only some institutions exert lobbying effort to get it, we

demonstrate compelling evidence of the influence of lobbying. By examining the same institutions over a 12-year period, we can isolate links between lobbying activity and legislative outcomes that do not depend on organizational traits or political circumstances. By showing that schools that lobby more intensely—including by using revolvers—are more likely than other schools to secure more earmarks and more earmarked dollars, we contribute to a growing body of evidence of the significant, measurable effects of lobbying both in the U.S. (e.g., Alexander et al. 2009, Baumgartner et al. 2009, Richter et al. 2009, Mian et al. 2010, Vidal et al. 2012, LaPira and Thomas 2017) and in other countries (e.g., Klüver 2011, Rasmussen et al. 2018, Dür et al. 2019, Hopkins et al. 2019, Junk 2019).

We have sought to minimize sources of potential bias by estimating fixed-effects models on a universe of data over time. Still, there are possible caveats to note. First, some factors may vary within a school over time that our model does not capture, and which may influence the school's ability to procure earmarks. However, we have no reason to think that these factors, such as change in an institution's leadership or rankings, would vary systematically in a way that would affect our conclusions. Similarly, there may be exogenous factors, such as changes in the economy, that structure how many lobbyists a school deploys as well as how many earmarked dollars it receives: We know that the numbers of lobbyists and revolving-door lobbyists have increased over time, as have the value of earmarks granted, and our use of fixed effects for the year absorbs this positive association. As well, changes in the membership of the two congressional appropriations committees—the committees that ultimately have authority over earmark funding—might change the intensity of lobbying by schools as well as the probability that schools receive earmarks. To evaluate this possibility, in robustness analyses, we identified

whether each school was represented by one or more members of the Senate Appropriations committee each year and whether that senator was in the majority party; we also tested the effect of party control. We find that membership on the Senate Appropriations committee is significantly associated with greater earmarked dollars for higher-education institutions in that senator's state. However, in these alternative models the effects of lobbying and revolving-door lobbying retain the significance they have in models 3 and 4.

This article contributes to the considerable literature on lobbying by providing clear evidence that in the context of U.S. colleges and universities seeking congressional earmark funding, school-years that lobby are more likely to be successful than other school-years. We also contribute to the growing literature on revolving-door lobbying by performing a robust test of the commonly held, but seldom investigated, belief that revolving-door lobbyists provide their employers with measurable marginal benefits above and beyond the benefits of lobbying alone. While future research should continue to investigate the effects of lobbying on distributive policies, it may be even more important for scholars to continue studying the potential of revolving-door lobbying to bias government decisionmaking.

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[1] Although Congress ended its process of earmarked appropriations in 2011, in 2021 the Congress brought earmarks back. (Our data collection ended before 2021.)

[2] These numbers are our calculations based on data about earmarks as a portion of the total federal funds devoted to research and development as reported by colleges and universities to the National Science Foundation. (Data available at https://www.nsf.gov/statistics/herd/pub_data.cfm.)

[3] Additionally, the United States has arguably a more transparent system of registering lobbying activity than any other country (Chari et al. 2019). Organizations report the money spent on lobbying and the specific issues they work on, as well as the names of the lobbyists they use and those individuals' previous employment in Congress (if any). These data allow us to test the effects of the revolving door and all lobbying on policy outcomes.

[4] As we do, de Figueiredo and Silverman (2006) examine lobbying by investigating earmarks going to American colleges and universities. Although we use data similar to those of de Figueiredo and Silverman (2006), we offer both data and methodological improvements. They examine 533 earmarks granted in one year (2001); we examine 9,445 earmarks granted over a 12-year period. Our longer time span allows us to apply university-level fixed effects; this design enables us to estimate more directly the effects of lobbying on universities' success in receiving this funding than de Figueiredo and Silverman's two-stage least-squares regression models.

[5] More technically, our second hypothesis is that the marginal effect of revolving-door lobbying on earmarking is positive, but above some value of lobbying expenditures, the use of revolving-door lobbyists has no effect or a decreasing effect on earmark outcomes (see Berry et al. 2012).

[6] This list is now being updated by the Center for Postsecondary Research at Indiana University Bloomington.

[7]We also drop school-years before 2000 or after 2010 if they are not listed in Carnegie's 2000 or 2010 list, respectively. For the years 2001-2009, we assume that if a school is listed in 2005 it existed from 2001-2004, and if it is listed in 2010, we assume it existed from 2006-2009. (These assumptions affect a maximum of 11 percent of the 44,213 observations). As well, several colleges and universities lobby as systems of universities, such as the California State University System, rather than as individual schools. Likewise, several earmarks are awarded to systems rather than to individual institutions within those systems. We considered aggregating up to the system level, but this would discard information unnecessarily and would introduce unit heterogeneity, as the majority of schools lobby independently of systems. We therefore dropped the 31 system-years that lobbied and the 45 system-years that received earmarks, as

well as empty dyads for those systems in other years, for a total of 0.32 percent of the data. This choice does not substantially influence our results.

[8] We did test the effect of the percentage of revolvers on earmarks while controlling for the control variables used in the reported models; the percentage of revolvers was not significant. This result is consistent with our theory that the use of *any* revolvers matters more than the number of revolvers.

[9] LaPira and Thomas sampled 1600 lobbyists active in 2009 and determined whether or not they had previously worked for the U.S. government by using human searching of online resources such as LinkedIn.com. We used the unique ID numbers for individual lobbyists created by CRP to match 40 percent of CRP's list of education lobbyists active in our time period to those in LaPira and Thomas's dataset. We updated our data to indicate that 49 lobbyists we had coded as nonrevolvers were in fact revolvers, and 8 of our coded revolvers had not in fact worked in Congress. (Ten lobbyists we had coded as revolvers were considered nonrevolvers by LaPira and Thomas; on further checking we determined that 8 of these were nonrevolvers and 2 were revolvers.) In our resulting sample, 36 percent of education lobbyists are congressional revolvers, which is comparable to the 39 percent of the sample that LaPira and Thomas (2017) found were congressional revolvers. (It is also plausible that education lobbyists would be slightly less likely to be revolvers relative to the overall population of lobbyists.)

[10] We considered using the lobbying that took place in 2006, along with lobbying in 2007, to predict earmarks in 2008. We combined 2006 and 2007 lobbying using the mean, sum, and maximum amounts of expenditures and revolvers. These did not predict 2008 earmarks any better or worse than the 2007 lobbying alone.

[11] Poisson regression is increasingly considered the ideal estimation technique for nonnegative, skewed dependent variables, even if they are not count variables, because of Poisson's robustness to distributional assumptions (Wooldridge 1999, Santos Silva and Tenreyro 2010, Correia et al. 2020) and the way that it handles large numbers of meaningful zeroes (Gourieroux et al. 1984). Fixed-effects Poisson regression using panel data is considered robust to distributional assumptions so long as robust standard errors are utilized (Wooldridge 2010). Because the literature regarding how to best model panel data with skewed nonnegative dependent variables is evolving, we also considered and estimated alternative models, particularly negative binomial and generalized linear models. The results of some of these are stronger than the results we have presented, and none lead us to different conclusions.