

The Cost of Political Turnover: Firm Lobbying after Legislator Departures*

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Abstract

How do firms rebuild political networks when a connected legislator departs? We combine lobbying disclosures with bill-sponsorship and cosponsorship records for 29 deaths and scandal-related resignations in the U.S. Congress during 2009–2018. Using weighted stacked difference-in-differences around the first public signal and formal exit, we find exposed firms broaden their networks and increase lobbying expenditures and participation. New connections span chambers and parties, while rebuilt portfolios diversify across legislative seniority rather than concentrating within the departing member's close network. Overall, rebuilding entails substantial costs, and evidence links rebuilt networks to legislatively active portfolios, consistent with costly replacement of relationship-specific capital.

Keywords: lobbying; political connections; networks; firm behavior; Congress

JEL codes: D22, D72, L14, H11

1 Introduction

Firms devote substantial resources to building and maintaining relationships with members of Congress. In 2024, organizations reported a record \$4.4 billion in federal lobbying expenditures.¹ Such investments can create relationship-specific political capital. Through repeated interactions, firms develop knowledge about individual legislators, secure access to their offices and staff, and learn how to communicate their interests effectively. The value of this political capital, however, depends on the extent to which it can be transferred across policymakers. What happens to a firm's accumulated political capital when a legislator with whom it has established a relationship unexpectedly leaves office?

This paper examines how firms adjust their lobbying activities and political networks when an established political relationship is unexpectedly disrupted, using these responses to evaluate the extent to which political capital can be transferred across policymakers. If a firm's relationship with one legislator can be easily substituted with a relationship with another, the legislator's departure should have little impact on the firm's lobbying behavior. By contrast, if political capital is tied to specific legislators, firms may need to devote resources to rebuilding their political connections. We therefore investigate both changes in firms' lobbying expenditures and the ways in which they restructure their portfolio of political relationships following a legislator's departure.

Identifying how firms respond poses two empirical challenges. First, firms may begin adapting before a legislator officially leaves office. For instance, an anticipated retirement can signal an impending departure well before the legislator's formal exit. To limit this concern, we focus on deaths in office and resignations prompted by scandals, events that are beyond firms' control and typically leave less opportunity for advance preparation. For each departure, we distinguish between two relevant dates: the first verified public signal and the legislator's formal departure from Congress. The public-signal date is defined as the first media report of a serious illness, public allegation, or investigation, capturing the point at which firms first receive observable information indicating a potential departure. The formal-exit date corresponds to the legislator's actual departure from Congress, after which the legislator can no longer serve. We separately analyze changes in exposed firms' lobbying expenditures, the breadth of their lobbying networks, and their likelihood of engaging in lobbying around each of these dates for every departure.

The second challenge is that firms connected to departing legislators may differ systematically from other firms in two ways. First, legislators who die while in office or resign following a scandal may differ systematically from those who continue serving in Congress. Second, firms connected to these departing legislators may differ from other firms in terms of lobbying expenditures, network breadth, and other characteristics. To address the first concern, we identify up to 40 placebo legislators for each departing legislator and event date. These placebo

¹Lobbying expenditures are reported under the Lobbying Disclosure Act (LDA) and compiled by OpenSecrets. The LDA requires registered lobbyists and lobbying organizations to file quarterly reports disclosing their lobbying activities and expenditures.

legislators serve in the same chamber, have a similar number of connected firms prior to the event, and remain in Congress throughout the event window. Firms connected to these placebo legislators constitute the pool of potential comparison firms. To address the second concern, we then match firms from this pool to treated firms based on their pre-event levels and changes in lobbying expenditures and network breadth. This procedure enables us to estimate the treatment effect on firms exposed to departing legislators by comparing their outcomes with those of a more comparable set of control firms.

Our analysis combines five data sources to examine how firms adjust their lobbying activity and political networks after a connected legislator leaves Congress. First, to identify congressional departures, we hand-collect an inventory of members of Congress who died in office or resigned following scandals between 1997 and 2020, along with the dates of the first public signal and the formal exit. Second, to identify connections between firms and legislators and construct our main outcomes, we use LobbyView, which organizes federal lobbying disclosures and links the bills cited in those disclosures to their congressional sponsors (Bacik et al. 2025; Kim 2018). We define a firm and a legislator as *linked* in a given year when the firm's lobbying disclosures cite a bill sponsored by that legislator. Third, we use the Bioguide register to obtain each legislator's chamber, state, and party. Fourth, we use the legislator-network data developed by Battaglini and Patacchini (2018) and Battaglini, Sciabolazza, et al. (2020) for the House in the 109th–113th Congresses to measure shared educational backgrounds and reciprocal cosponsorship. Fifth, we use official GovInfo BILLSTATUS records for both chambers in the 111th–115th Congresses to expand the coverage of reciprocal sponsor–cosponsor ties. Together, these sources allow us to examine how firms adjust their political connections and lobbying expenditures and whether they redirect their connections toward the departing legislator's alumni, reciprocal cosponsors, or same-state colleagues. Requiring three years of lobbying data before and after each departure produces a final sample of 29 events between 2009 and 2018.

We use the weighted stacked difference-in-differences framework of Wing et al. (2024) that compares treated firms with matched comparison firms defined separately for each event clock. We consider two event clocks: the date of the first public signal and the date of formal exit. For each departure and event clock, firms are classified as treated if they had a connection to the departing legislator during the previous three years. To form the comparison group, we select up to 40 placebo legislators from the same chamber who had comparable numbers of firm connections and remained in Congress for the entire event window. We then identify firms connected to these placebo legislators but not to the departing legislator. These comparison firms are matched to treated firms using their pre-event levels and changes in lobbying expenditures and network breadth. The stacked specification includes firm-by-event and event-by-year fixed effects.

The analysis produces two principal findings. First, firms exposed to the event expand their political networks under both event-time definitions. Excluding the departing legislator, the number of legislators connected to an exposed firm increases by 0.538 log points using the formal-exit clock and by 0.426 log points using the first-public-signal clock. Because these

outcomes enter as $\log(1 + x)$ and a large share of firm-years are zero, we read magnitudes from the same specifications estimated in levels, which yield 14.7 additional legislative connections under the formal-exit clock and 9.6 under the first-public-signal clock. Second, exposed firms increase both the intensity and incidence of their lobbying activity. Lobbying expenditures rise by 0.757 log points under the formal-exit clock and by 0.571 log points under the first-public-signal clock, or \$13,510 and \$9,400 per treated firm per year in levels. Aggregating these level estimates within departures yields a median of \$1.50 million in additional treated-firm spending per exit per year and \$6.02 million over the four post-event years. The likelihood of reporting positive lobbying expenditures also increases, by 6.6 percentage points under the formal-exit clock and 5.1 percentage points under the first-public-signal clock.

We further examine heterogeneity in firms' responses. First, we consider firms' pre-departure dependence on the departing legislator. Firms that were most dependent show somewhat smaller, but imprecisely estimated, responses than firms with lower dependence. Second, we examine heterogeneity by lobbying organization. Firms that primarily relied on in-house lobbyists before the event exhibit larger increases in network breadth than firms that primarily hired outside lobbying firms. This pattern is consistent with outside lobbyists providing relationships that clients can draw on when a connected legislator departs, although firms' choice of lobbying organization is not random.

We then examine where the newly formed connections are directed. First, the expansion of firms' political networks does not appear to be concentrated among legislators with particularly close ties to the departing member. The shares of connections involving alumni and same-state legislators remain largely unchanged. Although the share of reciprocal cosponsors declines under both event-time definitions, this outcome displays differential trends prior to the event. Taken together, these patterns indicate that firms respond to the loss of a political connection by diversifying and expanding their networks broadly, rather than by replacing the departing legislator through a single closely related network channel.

Following the departure of their linked legislators, treated firms establish approximately 586,000 firm-legislator links that did not exist prior to the events. We classify each new link by party affiliation and chamber and assess whether treated firms retain at least one connection within the departing legislator's reciprocal-cosponsor network. Roughly 54 percent of these new links cross party lines, and 38 percent cross chamber lines. The proportion of treated firms connected to at least one reciprocal peer falls from 26.9 percent in the first year to 22.5 percent by the third year. Results additionally show that rebuilt portfolios become more diversified across junior, midcareer, and veteran legislators. Together, these patterns suggest that firms' rebuilding efforts are not concentrated within a single partisan, institutional, seniority, or inherited-network pathway.

Finally, a GovInfo analysis restricted to departures with complete event windows shows that firms' non-exiter portfolios encompass more sponsored policy measures after an exit, while the shares of those measures receiving committee and floor action rise by 3.459 and 1.790 percentage

points. These outcomes indicate that firms rebuild their networks toward legislators with more active legislative portfolios.

The paper contributes to three distinct literatures. First, it contributes to research on the value of political connections. Stock-price event studies show that firms lose value when a connected politician dies, becomes ill, or leaves office (Akey 2015; Faccio 2006; Faccio and Parsley 2009; D. Fisman et al. 2012; R. Fisman 2001; Jayachandran 2006). Powerful legislators also influence firms' real decisions in their districts (Cohen et al. 2011). This literature establishes that political connections are valuable. We show how firms respond when such a connection disappears by tracing changes in their lobbying participation, expenditures, and connections to other legislators around the public signal and formal departure. Firms respond by spending more and expanding their political networks, consistent with the loss of relationship-specific capital that cannot be transferred immediately to another policymaker (Klein et al. 1978; Williamson 1979).

Second, the paper contributes to research on corporate lobbying. Existing studies show that lobbying relationships produce valuable access and expertise. Lobbyists with government experience lose revenue when officials they know leave office (Blanes i Vidal et al. 2012), and lobbyists follow politicians across committee assignments because those relationships command a premium (Bertrand et al. 2014). Campaign contributions can facilitate access to policymakers (Kalla and Broockman 2016), while lobbying may influence policy by providing information and support to allied legislators (Hall and Deardorff 2006). Lobbying also involves substantial fixed costs and is highly persistent (Bombardini and Trebbi 2012; Kerr et al. 2014). Its returns can be large (Igan and Mishra 2014; Kang 2016) and may affect the allocation of resources across firms (Huneus and Kim 2021). Although prior research has largely focused on the level and value of lobbying (Bombardini and Trebbi 2020; Figueiredo and Richter 2014), our findings highlight how rapidly firms modify their lobbying behavior following the loss of a political connection. Around the departure, firms increase both their lobbying expenditures and their propensity to engage in lobbying. We further estimate the additional private costs associated with rebuilding these connections.

Third, the paper contributes to the literature on political networks. Existing research shows that legislators' relationships with other members of Congress influence their legislative power and effectiveness (Battaglini and Patacchini 2018, 2019; Battaglini, Patacchini, and Rainone 2022; Battaglini, Sciabolazza, et al. 2020; Canen et al. 2023; Garro 2021). We extend this network perspective to firms' lobbying portfolios by examining how the departure of a legislator reshapes firms' ties to other members of Congress. In particular, we investigate whether firms replace lost connections by turning to legislators who are closely linked to the departing member, such as alumni, reciprocal cosponsors, and same-state colleagues. We further examine whether newly established relationships extend across party and chamber boundaries. Our findings indicate that firms broaden their congressional connections rather than simply relying on a single inherited network, showing that the consequences of political turnover reach beyond the loss and replacement of an individual connection.

2 Background

2.1 Firm-Legislator Connections

Since 2008, the Lobbying Disclosure Act has required registered lobbying organizations to submit quarterly reports detailing their federal lobbying activities. These filings provide information on the client, the lobbying registrant, lobbying expenditures, the broad policy areas targeted, and, in most cases, the specific legislation discussed. LobbyView compiles these disclosure reports into a relational database and connects each referenced bill to its congressional sponsor (Bacik et al. 2025; Kim 2017, 2018).

These data enable us to construct an annual network connecting firms and legislators. We consider a firm to be *linked* to a legislator in a given year when the firm’s lobbying disclosure references a bill sponsored by that legislator. For instance, if a pharmaceutical company reports lobbying on a drug-pricing bill sponsored by a specific member of Congress, we treat the company and that legislator as linked for that year. We then define a firm’s *network breadth* as the number of unique legislators with whom the firm is linked in a given year.

A link measures documented lobbying attention rather than direct interaction. It does not indicate that anyone at the firm communicated with the legislator or that the firm supported the cited bill, since firms may also lobby in opposition to legislation. Instead, it provides a dated and consistent measure of where a firm directs its attention within Congress. This is the dimension of political relationships studied throughout the paper, consistent with previous work using these data (Bertrand et al. 2014; Bombardini and Trebbi 2020; Huneeus and Kim 2021).

2.2 Congressional Exits

We focus on two types of congressional departures: deaths in office and resignations connected to public scandals. These events are not determined by firms linked to the departing legislators and generally provide less scope for advance planning. Related studies similarly use political deaths as shocks to existing political relationships. Faccio and Parsley (2009) examine how politicians’ sudden deaths affect firms headquartered in their hometowns, while Garro (2021) studies how legislators’ deaths affect the legislative effectiveness of their congressional peers. In contrast, ordinary retirements and electoral defeats are often anticipated and may be related to factors that also affect firms’ lobbying decisions, including a legislator’s seniority, committee position, and the political environment in the legislator’s district.

Deaths and scandal-related resignations offer more plausibly exogenous disruptions to political relationships, although such departures are not always entirely unexpected. A member’s serious illness may become publicly known well before death, just as an investigation or allegation may emerge before a resignation. To distinguish firms’ responses to the earliest public indication of a potential departure from their responses to the departure itself, we therefore employ two co-primary event clocks. The first defines event time relative to the first verified

public signal. For instance, Senator John McCain’s diagnosis of glioblastoma was publicly announced on July 19, 2017, providing firms with observable information about his potential departure more than a year before his death. The second event clock is centered on the member’s formal exit. In McCain’s case, this occurred with his death on August 25, 2018. From that point onward, he could no longer introduce or advance legislation, and firms could no longer sustain a connection with him in his capacity as a sitting senator.

We reconstruct the entire design separately around the two clocks. For each departure, Table 1 reports the first-signal and formal-exit dates and shows how the interval between them varies across events. Section 3 further explains how we identify these dates and construct the corresponding clock-specific samples. As Table 1 also shows, the first signal and formal exit occur in the same calendar year for some departures. Because the lobbying network is measured annually, the two clocks assign the same event year in these cases.

2.3 Outcome Predictions

We view each firm as holding a portfolio of political ties with members of Congress. These relationships can provide firms with information about upcoming legislation, access to lawmakers and their staff, greater credibility in communicating their policy interests, and support in navigating the legislative process. Because the death of a legislator or a resignation prompted by scandal is typically beyond the firm’s control and difficult to anticipate, such events unexpectedly reduce the future value associated with the affected relationship. In response, the firm must determine whether to modify its lobbying activities, establish a replacement for the lost connection, or diversify its network by developing relationships with a broader set of policymakers.

We consider three possible responses, each of which generates distinct empirical predictions. First, if political relationships are close substitutes and inexpensive to form, a firm can replace the lost relationship with a similar one. Under this *substitution* response, we expect little change in either total lobbying expenditures or the overall breadth of the firm’s network. Rather, replacement links should tend to remain within the same chamber or target legislators who belong to the departing legislator’s close circle. We define this close circle as legislators who attended the same educational institution, engaged in reciprocal cosponsorship, or represented the same state as the departing legislator. Accordingly, the proportion of the firm’s links directed toward members of the departing legislator’s close circle should increase.

Second, if political relationships contain person-specific capital and are costly to build, replacing the lost relationship requires firms to invest in new connections. Because the returns to prospective relationships are uncertain, firms may form links with multiple legislators rather than replace the departing member with someone from the member’s close circle. Under this *costly replacement* response, lobbying expenditures and network breadth should increase, while new links should be dispersed across a broader set of legislators rather than concentrated within the departing member’s close circle.

Third, if the lost relationship was sufficiently important to make lobbying worthwhile, its disruption may reduce the firm’s incentive to lobby. Under this *retrenchment* response, lobbying expenditures and network breadth should decline, and firms should become less likely to report any lobbying activity.

These three responses generate distinct empirical predictions. Substitution predicts relatively stable lobbying expenditures and network breadth, with new connections concentrated within the departing member’s close network. Costly replacement predicts higher expenditures, broader political networks, and more dispersed new connections. Retrenchment predicts lower expenditures, narrower political networks, and lower lobbying participation. The empirical analysis distinguishes among these patterns and uses legislative-activity measures to provide a preliminary characterization of the political portfolios that firms form after a departure.

3 Data

Our analysis combines five sources of data: federal lobbying disclosures from LobbyView, a hand-collected inventory of congressional departures and public signals, the Congressional Biographical Directory (Bioguide), House legislator-network data, and official GovInfo BILL-STATUS records. We describe each source before explaining how we construct the outcomes, events, and treated and comparison groups.

3.1 LobbyView

Our primary source is the archived LobbyView release v2024.10.01, which compiles federal lobbying disclosures filed under the Lobbying Disclosure Act (Bacik et al. 2025; Kim 2017, 2018). The database contains information on lobbying clients and registrants, report-level lobbying expenditures, the issues and bills cited in each filing, and the members of Congress who sponsored those bills. We use the client register, report-level filings, and firm–legislator bill network to construct annual measures of firms’ lobbying activity and political connections.

The LobbyView data enable us to observe both firms’ lobbying expenditures and the legislators connected to the legislation targeted by their lobbying activities. We aggregate expenditures reported at the individual report level to the firm–year level to construct a measure of each firm’s annual lobbying spending. We then use the bill network to identify connections between firms and legislators. Specifically, a firm is considered linked to a legislator in a given year when its lobbying disclosures reference a bill sponsored by that legislator. We use these firm–legislator connections to construct our measures of firms’ political networks.

The bill–sponsor network has limited coverage before 2006 and effectively terminates after 2021.² Accordingly, we limit our analysis to 2006–2021. Since our empirical design requires

²Although the LobbyView release includes lobbying reports after 2021, usable coverage of the bill–sponsor network is limited to the 2006–2021 period. The network records only approximately 2,100 firm–legislator links in 2022 and none in subsequent years, so we restrict the analysis to 2006–2021.

three years of observations on both sides of each congressional departure, the departure events included in the analysis span 2009 to 2018.

3.2 Congressional Departures

Our second dataset consists of a hand-collected inventory of members of Congress who either died while in office or resigned amid a public scandal between 1997 and 2020. We compile this inventory using records of congressional deaths and resignations and manually match each member to a unique Bioguide identifier. For every departure, we document the member, the type of departure, the official date of exit, and, to capture possible anticipation effects, the earliest dated public indication that the member might leave office.

Depending on the nature of the event, the first public signal may take the form of an announcement of a serious illness, an indictment, an ethics investigation, or the first substantiated allegation that is later followed by resignation. We verify each signal using dated public reports or official records and retain the relevant source URL for every case. The dates of the initial signal and formal exit establish the two co-primary event clocks used throughout the paper.

Combining the departure inventory with the LobbyView bill–sponsor network and imposing a requirement of three years of data both before and after formal exit produces 29 usable events between 2009 and 2018. These include 11 deaths and 18 scandal-related resignations, involving 22 House members and 7 senators. Reconstructing the design using the first public signal as the event date yields 26 usable events, three fewer than under the formal-exit design. Ted Kennedy is excluded because his first signal occurred in 2008, which would require network data beginning in 2005, prior to the start of usable coverage. Trey Radel and Mark Takai are also excluded because neither has connected firms during the three years preceding their respective first-signal years, making it impossible to define a treated group. Table 1 lists all 29 departures together with their formal-exit dates and verified first public signals.

3.3 Congressional Biographical Directory

Our third source is the Congressional Biographical Directory (Bioguide), which provides information on members of the U.S. Congress and assigns each member a unique Bioguide identifier. We use these identifiers to link the congressional departure inventory to the legislators appearing in the LobbyView bill network. For each member, we obtain the chamber in which the member served, the member’s home state, and party affiliation.

These characteristics are used at several stages of the analysis. Chamber is used to construct the pool of potential placebo legislators and to distinguish firms’ connections within and across chambers. State identifies the departing member’s home-state congressional delegation, while party affiliation allows us to examine whether firms form new links with members of the same or opposing party following a departure.

Table 1: Congressional departures and their first public signals

Member	Chamber	Party-state	Exit type	Formal exit	First public signal
Ted Kennedy	Senate	D-MA	Death	2009-08-25	2008-05-20
John Murtha	House	D-PA	Death	2010-02-08	2010-01-28
Eric Massa	House	D-NY	Scandal	2010-03-08	2010-03-03
Mark Souder	House	R-IN	Scandal	2010-05-21	2010-05-18
Robert Byrd	Senate	D-WV	Death	2010-06-28	2009-05-18
Chris Lee	House	R-NY	Scandal	2011-02-09	2011-02-09
John Ensign	Senate	R-NV	Scandal	2011-05-03	2009-06-16
Anthony Weiner	House	D-NY	Scandal	2011-06-21	2011-05-27
David Wu	House	D-OR	Scandal	2011-08-03	2011-07-22
Donald M. Payne	House	D-NJ	Death	2012-03-06	2012-02-10
Jesse Jackson Jr.	House	D-IL	Scandal	2012-11-21	2012-06-25
Daniel Inouye	Senate	D-HI	Death	2012-12-17	2012-12-06
Frank Lautenberg	Senate	D-NJ	Death	2013-06-03	2010-02-19
Bill Young	House	R-FL	Death	2013-10-18	2013-10-09
Trey Radel	House	R-FL	Scandal	2014-01-27	2013-11-19
Michael Grimm	House	R-NY	Scandal	2015-01-05	2014-04-28
Alan Nunnelee	House	R-MS	Death	2015-02-06	2014-05-12
Aaron Schock	House	R-IL	Scandal	2015-03-31	2015-02-02
Chaka Fattah	House	D-PA	Scandal	2016-06-23	2015-07-29
Mark Takai	House	D-HI	Death	2016-07-20	2015-10-29
Ed Whitfield	House	R-KY	Scandal	2016-09-07	2015-03-25
Tim Murphy	House	R-PA	Scandal	2017-10-21	2017-10-03
John Conyers	House	D-MI	Scandal	2017-12-05	2017-11-20
Trent Franks	House	R-AZ	Scandal	2017-12-08	2017-12-07
Al Franken	Senate	D-MN	Scandal	2018-01-02	2017-11-16
Louise Slaughter	House	D-NY	Death	2018-03-16	2018-03-14
Blake Farenthold	House	R-TX	Scandal	2018-04-06	2017-12-01
Pat Meehan	House	R-PA	Scandal	2018-04-27	2018-01-20
John McCain	Senate	R-AZ	Death	2018-08-25	2017-07-19

Notes: The formal-exit sample contains 29 departures: 22 from the House and seven from the Senate. The first public signal is the earliest verified public disclosure of the forthcoming departure. Formal-exit dates come from the Bioguide crosswalk, except Michael Grimm’s January 5, 2015 resignation, which is dated using the U.S. House Historian. Signal descriptions and source links are recorded in the accompanying event-date audit.

3.4 Legislator Networks

Beyond the main analysis of lobbying expenditures and network breadth, we use additional legislator-network data to examine whether firms redirect their connections toward legislators in the departing member’s close network. Our fourth source is the House network archive assembled by Battaglini and Patacchini (2018) and Battaglini, Sciabolazza, et al. (2020). The archive covers the 109th–113th Congresses, corresponding to 2005–2014, and contains member characteristics, common-school ties, and directed bill-cosponsorship matrices. We map its member identifiers to Bioguide IDs for 2,218 member–Congress observations.

We construct two close networks from these data. First, the *alumni network* links the departing member to legislators who attended at least one common educational institution. Because these ties are available only for House members and for a shorter period, the alumni analysis is limited to ten formal-exit departures in the 111th–113th Congresses, corresponding to 2009–2014. Nine of these departures also yield a usable first-signal stack. Our main definition does not restrict alumni ties by graduation year because requiring graduation within four years leaves six of the ten departing members without an alumni peer.

Second, the *reciprocal-cosponsorship network* connects a departing member to another legislator when both lawmakers cosponsored at least one measure sponsored by the other during the departing member’s full exit Congress, following Garro (2021). More precisely, legislator j is considered a peer of departing member i when j cosponsored a measure sponsored by i and, conversely, i cosponsored a measure sponsored by j . Thus, a reciprocal tie exists only when the cosponsorship relationship operates in both directions. We extend the chamber and temporal coverage of this measure using our fifth data source, the U.S. Government Publishing Office’s GovInfo BILLSTATUS records. From these records, we construct directed sponsor–cosponsor ties for both chambers over the 111th–115th Congresses, covering 2009–2018. Within each chamber and Congress, we consider bills, joint resolutions, simple resolutions, and concurrent resolutions. By combining the Battaglini and GovInfo data, we are able to apply the reciprocal-cosponsorship analysis to the same event stacks used in the main analysis, comprising all 29 formal-exit stacks and 26 first-signal stacks.

3.5 Construction of the Analysis Sample

We combine the departure, LobbyView, and Bioguide data to construct a stacked firm–event–year panel centered on each of the 29 congressional departures. We supplement this panel with measures of legislators’ close networks where data coverage permits. We then describe the construction of the main outcomes, event windows, and treated and comparison groups.

3.5.1 Outcomes

Our primary outcomes capture the breadth and intensity of a firm’s lobbying activity. For firm i in year t , we define the firm as *linked* to a legislator if its lobbying disclosures cite at least one bill sponsored by that legislator. *Network breadth* is the number of distinct legislators to whom the firm is linked in year t , while *lobbying expenditure* is the total amount reported across all of the firm’s lobbying disclosures in that year. We also define *any lobbying expenditure* as an indicator equal to one if the firm reports positive lobbying expenditure in a given year. In every departure-specific event stack, whether centered on the formal exit or the first public signal, we exclude the departing member e from the network-breadth measure throughout the event window. This prevents the measure from declining mechanically when the member leaves Congress.

We construct several additional outcomes to examine how firms reorganize their political connections. *Links to the departing member* counts the number of bills sponsored by departing

member e that the firm’s lobbying disclosures cite in a given year. This outcome serves as a design check by showing whether the departure disrupts the relationship captured by our treatment definition. To measure firms’ connections to close networks, we define alumni, reciprocal-cosponsor, and same-state peer sets for each departing member and hold these sets fixed within each event stack. In each event year, the close-network outcome is the share of the firm’s non-exiter links that fall within the indicated peer set. We set the share to zero when the denominator is zero.

We transform the primary count and dollar outcomes using $\log(1 + x)$, which allows firm-year observations with zero lobbying activity to remain in the sample. Because coefficients on $\log(1 + x)$ outcomes with a substantial mass at zero do not generally have a percentage-change interpretation (Chen and Roth 2024), we interpret them as changes in the transformed outcomes. To illustrate economic magnitudes and assess sensitivity to functional form, we re-estimate the same specifications in levels, as reported in Table A2 and Figure A2. Close-network shares enter the regressions in percentage points. Lobbying participation enters as a zero-one indicator, with its effects expressed in percentage points in the text.

3.5.2 Events

Let $d \in \{\text{exit}, \text{signal}\}$ index the two event clocks. For each departing member e and clock d , we construct a seven-year event window comprising the three years before the corresponding event, the event year, and the three subsequent years. Under the formal-exit clock, $d = \text{exit}$, event time zero is the year in which the member leaves office, marking the point at which the member can no longer sponsor or shepherd legislation. Under the first-signal clock, $d = \text{signal}$, event time zero is the year of the earliest verified public signal, marking the point at which firms can first respond to dated public information. The formal-exit design contains 29 events between 2009 and 2018, while the first-public-signal design contains 26 events over the same period.

3.5.3 Treatment and Comparison Groups

For departure e and clock d , we define a firm as treated if its lobbying disclosures cite at least one bill sponsored by the departing member in any of the three years preceding that clock’s event year. We later examine whether firms’ responses vary with treatment intensity, measured by the departing member’s share of the firm’s pre-event bill-linked lobbying activity. This treatment definition identifies firms with a recent observed connection, but it also creates an important selection problem. Because firms linked to a member’s legislation tend to be relatively active lobbyists, comparing them with arbitrary firms would confound exposure to the departure with pre-existing lobbying activity.

We therefore construct the comparison group using the same underlying measure of political connections that defines treatment. For each departure-clock, we first calculate the departing member’s average number of connected firms over the three pre-event years. We then identify up to 40 *placebo legislators* from the same chamber whose average numbers of connected firms

are closest to that of the departing member on a $\log(1 + x)$ scale and who remain in Congress throughout the event window. Firms linked to these placebo legislators form the pool of potential comparison firms. A firm linked to the departing member is excluded from that event's comparison pool, so treated and comparison firms are mutually exclusive within an event. We also exclude candidate firms that are exposed to another treated departure during an overlapping event window.

Within each event and clock, we match treated and potential comparison firms using four standardized pre-event characteristics: the levels and changes in lobbying expenditure and network breadth, expressed on log scales. Each treated firm is matched to up to three nearby comparison firms within a caliper of 0.30. Treated firms for which no comparison firm falls within the caliper are excluded from the estimation sample. Appendix C provides the eligibility rules, covariate definitions, and matching-distance standard. Figure A3 re-estimates the main specifications using alternative placebo-legislator and firm-matching choices, including a placebo distance that incorporates legislator tenure and firm matching based on industry. The estimates remain robust across all alternative specifications.

For the panel centered on formal exits, the sample contains 95,781 firm–event–year observations and 13,683 firm–event units, consisting of 7,748 treated and 5,935 comparison units across 29 formal exits. For the panel centered on first public signals, the sample contains 91,798 observations and 13,114 firm–event units, consisting of 7,377 treated and 5,737 comparison units across 26 first public signals. Because the same firm may be treated or serve as a comparison firm for more than one event, these counts refer to firm–event units rather than unique organizations. As an additional robustness check, we restrict the sample to firms treated in no more than one event stack, with results presented in Figure A2. Table A1 compares the construction of the formal-exit and first-signal samples from the initial departure inventory through the final estimation samples.

Table 2 reports summary statistics and balance between treated and comparison firms over the three pre-exit years in the formal-exit panel. The matching procedure produces groups with very similar pre-period changes in the primary outcomes. The normalized difference, defined as the treated–comparison mean difference divided by the pooled standard deviation, is 0.03 for growth in lobbying expenditure and 0.04 for growth in network breadth. Thus, the corresponding gaps between the two groups amount to only 3 and 4 percent of a pooled standard deviation. Differences are also relatively modest for average expenditure, lobbying participation, and the log-expenditure matching covariate, with normalized differences of 0.19, 0.20, and 0.23, respectively.

A larger difference remains in the pre-departure level of network breadth. Treated firms are linked to approximately 33 legislators per year on average, compared with 20 among comparison firms, corresponding to a normalized difference of 0.50. Thus, although the matching procedure substantially aligns the groups on pre-departure changes and several measures of lobbying activity, it does not eliminate the difference in baseline network breadth. Our empirical specifications absorb persistent firm–event differences through fixed effects, and Section 5

Table 2: Treated and comparison firms before the exit

Average over $k = -3$ to $k = -1$	Treated	Comparison	Difference	Norm. diff.
<i>Panel A: Outcome variables</i>				
Distinct legislators connected, excluding the exiter	33.02 (29.84)	20.46 (19.89)	12.55	0.495
Lobbying expenditure (thousands of dollars)	137.64 (185.86)	104.04 (158.59)	33.60	0.194
Any lobbying expenditure (0/1)	0.79 (0.33)	0.73 (0.34)	0.07	0.197
Bill links to the departing member	1.32 (1.52)	0.00 (0.00)	1.32	1.235
<i>Panel B: Other pre-period characteristics</i>				
Lobbying reports filed	3.84 (3.07)	3.07 (2.54)	0.77	0.274
Bills lobbied	108.68 (153.02)	66.35 (120.22)	42.33	0.308
Log(1 + lobbying expenditure), matching covariate	9.24 (3.96)	8.33 (4.09)	0.91	0.226
Log(1 + legislators connected), matching covariate	2.13 (1.09)	1.76 (0.88)	0.37	0.378
<i>Panel C: Pre-period growth used in matching</i>				
Growth in log expenditure, $k = -3$ to $k = -1$	0.29 (5.52)	0.14 (6.41)	0.16	0.026
Growth in log breadth, $k = -3$ to $k = -1$	0.12 (3.00)	0.02 (2.93)	0.10	0.035
Treated firm–event units		7,748		
Comparison firm–event units		5,935		
Exit events		29		
Years covered		2006–2021		

Notes: Each firm–event unit contributes its average over the three years before formal exit. Treated firms cited at least one bill sponsored by the departing member during that period. Comparison firms are drawn from clients of same-chamber placebo legislators with similar numbers of connected firms who remained in Congress throughout the event window, then matched within each departure on pre-event levels and changes in expenditure and network breadth. A link is a firm–legislator pair connected through a disclosed bill citation. Columns report means, with standard deviations in parentheses. The difference is the treated mean minus the comparison mean; the normalized difference divides that gap by the pooled standard deviation. Comparison firms have no pre-event link to the departing member by construction. Counts refer to firm–event units, so a firm appearing in multiple stacks is counted once in each.

additionally reports a propensity-score matching design based on a richer set of pre-treatment characteristics. That procedure reduces the largest standardized imbalance to 0.05 and preserves the positive, statistically significant effects on all three outcomes.

4 Empirical Strategy

4.1 Main Specification

We estimate the effect of a departure using the weighted stacked difference-in-differences estimator of Wing et al. (2024) (WFH). Appendix C specifies the observation weights and the

aggregation across departures and event years. Let $d \in \{\text{exit}, \text{signal}\}$ index the two event clocks. For every departure and each clock, we construct a separate seven-year panel with its own treated and matched comparison firms, then stack the event-specific panels within d . Treated firms are therefore compared only with comparison firms selected for the same departure, event clock, and calendar window.

We adopt this design because congressional departures and their public signals occur in different years, creating a staggered-treatment setting. A conventional two-way fixed-effects specification can compare newly treated units with already-treated units and may be difficult to interpret when effects vary across cohorts or time (Baker et al. 2022; Callaway and Sant’Anna 2021; Goodman-Bacon 2021; Sun and Abraham 2021). Building a separate comparison group within each event window removes such comparisons by construction.

For firm i , departure e , clock d , and calendar year t , the pooled specification is

$$Y_{iedt} = \alpha_{ied} + \lambda_{edt} + \beta^d (\text{Treated}_{ied} \times \text{Post}_{edt}) + \varepsilon_{iedt}, \quad (1)$$

where Y_{iedt} is a firm-level lobbying outcome. Treated_{ied} equals one if firm i ’s lobbying disclosures cite at least one bill sponsored by departing member e in any of the three years preceding clock d ’s event year, and zero for the comparison firms matched to it within that stack. Post_{edt} equals one in that clock’s event year and three subsequent years. Firm-by-event fixed effects α_{ied} absorb time-invariant differences within a stack; event-by-year fixed effects λ_{edt} absorb shocks common to its treated and comparison firms. We estimate the equation separately by d , so β^{exit} measures adjustment from formal departure and β^{signal} measures adjustment from the first verified public signal.

Because firms can be exposed to more than one legislator departure, they may appear in multiple event stacks. As a robustness check, Table A2 and Figure A2 restrict the sample to firms treated in at most one event and yield consistent results.

We complement the pooled difference-in-differences specification with an event-study specification that traces the dynamics before and after each departure:

$$Y_{iedt} = \alpha_{ied} + \lambda_{edt} + \sum_{k \in \{-3, -2, 0, 1, 2, 3\}} \beta_k^d \left[\text{Treated}_{ied} \times \mathbf{1}\{t - T_e^d = k\} \right] + \varepsilon_{iedt}, \quad (2)$$

where T_e^d is the formal-exit or first-signal year and $k = -1$ is omitted. Using the same WFH weights, each β_k^d measures the treated–comparison difference in event year k relative to the year immediately before the corresponding event. Plotting β_k^{exit} and β_k^{signal} together shows whether adjustment begins with public information or waits until the relationship formally ends.

Standard errors are clustered at the firm level, allowing observations for the same firm to be correlated across years and stacks. Table A2 also clusters by exit, estimates Equation (1) separately for each event and tests the mean, and reports an exact sign test, addressing the limited number of shocks without relying on large-cluster asymptotics (Abadie et al. 2023; Cameron et al. 2008).

4.2 Heterogeneity by Pre-Event Connection Intensity

We next ask whether firms respond differently depending on how important the departing member was within their pre-event lobbying portfolio. For every treated firm, departure, and event clock, we define connection intensity as

$$S_{ied} = \frac{\sum_{k=-3}^{-1} \text{BillsExiter}_{iedk}}{\sum_{k=-3}^{-1} \text{AllLinkedBills}_{iedk}}, \quad (3)$$

where the numerator counts bill-linked lobbying activity involving bills sponsored by the focal legislator and the denominator counts all of the firm's bill-linked lobbying activity.

The denominator of S_{ied} is the firm's own pre-event portfolio, so a firm scores high either because the departing member genuinely accounted for much of its political activity or simply because it lobbied on very few bills. We therefore rank S_{ied} within departure and within four equal-sized bins of pre-event network breadth, and form three mutually exclusive groups from those within-bin ranks: the top 10 percent, the 50th–90th percentiles, and the bottom 50 percent. For each group, outcome, and clock we retain the original comparison firms from the baseline stack rather than rematching, and estimate Equations (1) and (2) to see whether firms that depended most on the departing member adjust more than firms for which the member was a smaller part of the portfolio.

4.3 Heterogeneity by Lobbying Mode: In-House versus Outside Lobbyists

We examine whether firms respond differently depending on whether they primarily rely on in-house or outside lobbyists. For each firm and event, we classify lobbying mode using active, non-amended reports from the three pre-event years: firms with more in-house reports are classified as primarily in-house, while those with more outside reports are classified as primarily outside. Firms with tied or unclassifiable reports are excluded. Because lobbying mode is defined before the event, subsequent changes in lobbying organization do not affect classification.

We estimate heterogeneity separately around the formal exit and first public signal, comparing treated firms with matched firms using the same pre-event lobbying mode. Firms that primarily use outside lobbyists serve as the reference group, so the interaction term shows whether firms relying mainly on in-house lobbyists respond differently after the event. Table 4 reports the outside-group effect β , the in-house-group effect $\beta + \delta$, and their difference δ , as defined in the table notes.

4.4 Identification Concerns and Solutions

The identifying assumption is that, in the absence of the anticipated departure, treated firms and their matched comparison firms within each clock-specific stack would have followed similar changes in lobbying activity. Deaths and scandal-induced resignations are not determined by treated firms and, compared with routine retirements or electoral losses, are less likely to

be associated with gradual career developments that may independently influence lobbying behavior. Nevertheless, two potential concerns remain: anticipation and selection based on recent political activity.

First, firms may still anticipate a legislator’s potential departure, although we address this concern directly by using the earliest verified public signal as a co-primary event. Some anticipation may nevertheless persist if firms learn about an impending departure through private communication before any information becomes publicly available, but such private information is inherently beyond our ability to observe. The main event studies in Section 5 show limited pre-event movement in expenditure and participation, but network breadth exhibits a modest differential pre-trend. We therefore assess sensitivity to differential linear trends and placebo timing rather than treat the event studies as ruling out anticipation in Section 5.4.

Second, treatment status is determined by whether a firm had a recent lobbying connection to the departing legislator. Because treated firms are identified partly on the basis of recent lobbying activity, subsequent changes may capture mean reversion rather than the effect of the legislator’s departure. Our baseline matching procedure mitigates this concern by matching firms on both pre-treatment levels and changes in lobbying expenditures and network breadth. We also implement a propensity-score matching approach using a broader set of pre-treatment characteristics, including both levels and changes, to achieve greater balance between treated and comparison firms; the resulting estimates are reported in Table A3. As a further diagnostic for mean reversion, we assign a placebo departure date two years prior to the actual departure and re-estimate the same specification using this artificial treatment timing. If the observed post-departure changes in lobbying activity are driven primarily by mechanical mean reversion associated with the recent links used to define treatment, a comparable pattern should emerge around the placebo date. The results of this exercise are reported in Table A2.

5 Results

5.1 Main Results under the Two Event Clocks

Figure 1 reports the event-study estimates from Equation (2) using the two co-primary definitions of event time. The light blue-grey series defines event time relative to the member’s formal exit, whereas the darker mustard series defines it relative to the first verified public signal.

Under both definitions of event time, firms respond to the departure of a connected legislator by increasing, rather than scaling back, their lobbying activities. Panel (a) serves as a design check, showing a sharp decline in bill links to the departing legislator under both event-time measures.³ Panel (b) indicates that treated firms expand their connections with other legislators after the departure. Panels (c) and (d) similarly document increases in lobbying expenditures

³By construction, treated firms have at least one such link during the three-year pre-event window, whereas comparison firms have none. Selection on this outcome can therefore generate pre-event differences relative to the

and in the likelihood of reporting positive lobbying spending. Pre-event expenditure and participation trajectories remain largely flat, while network breadth exhibits modest differential pre-event movement, which we address in Section 5.4. Moreover, the estimates based on formal exit and first signal display qualitatively similar patterns.

Table 3 reports the pooled estimates using Equation (1). Centering on formal exit, treated firms increase network breadth by 0.538 log points, lobbying expenditure by 0.757 log points, and participation by 6.6 percentage points. Centering on the first public signal yields smaller but still precisely estimated increases of 0.426 log points, 0.571 log points, and 5.1 percentage points. Because breadth and expenditure enter as $\log(1 + x)$ and a large share of firm-years are zero, these coefficients are not proportional changes in the underlying outcomes, and we do not translate them into percentages or dollars (Chen and Roth 2024).

We instead read magnitudes from the same specifications estimated in levels, reported in Table A2 and Figure A2. Treated firms establish connections with an additional 14.7 legislators when departures are defined using the formal-exit clock and an additional 9.6 legislators under the first-signal clock. Annual lobbying expenditures also increase by \$13,510 and \$9,400 per treated firm, respectively, which is roughly a 10 percent increase relative to pre-event spending levels. The size of these effects suggests that congressional turnover imposes economically meaningful adjustment costs on firms that had previously invested in relationships with departing legislators. Multiplying each departure-specific level estimate by its number of treated firms yields a median increase in treated-firm lobbying expenditures of \$1.50 million per departure per year, equivalent to \$6.02 million over the four-year post-event period.

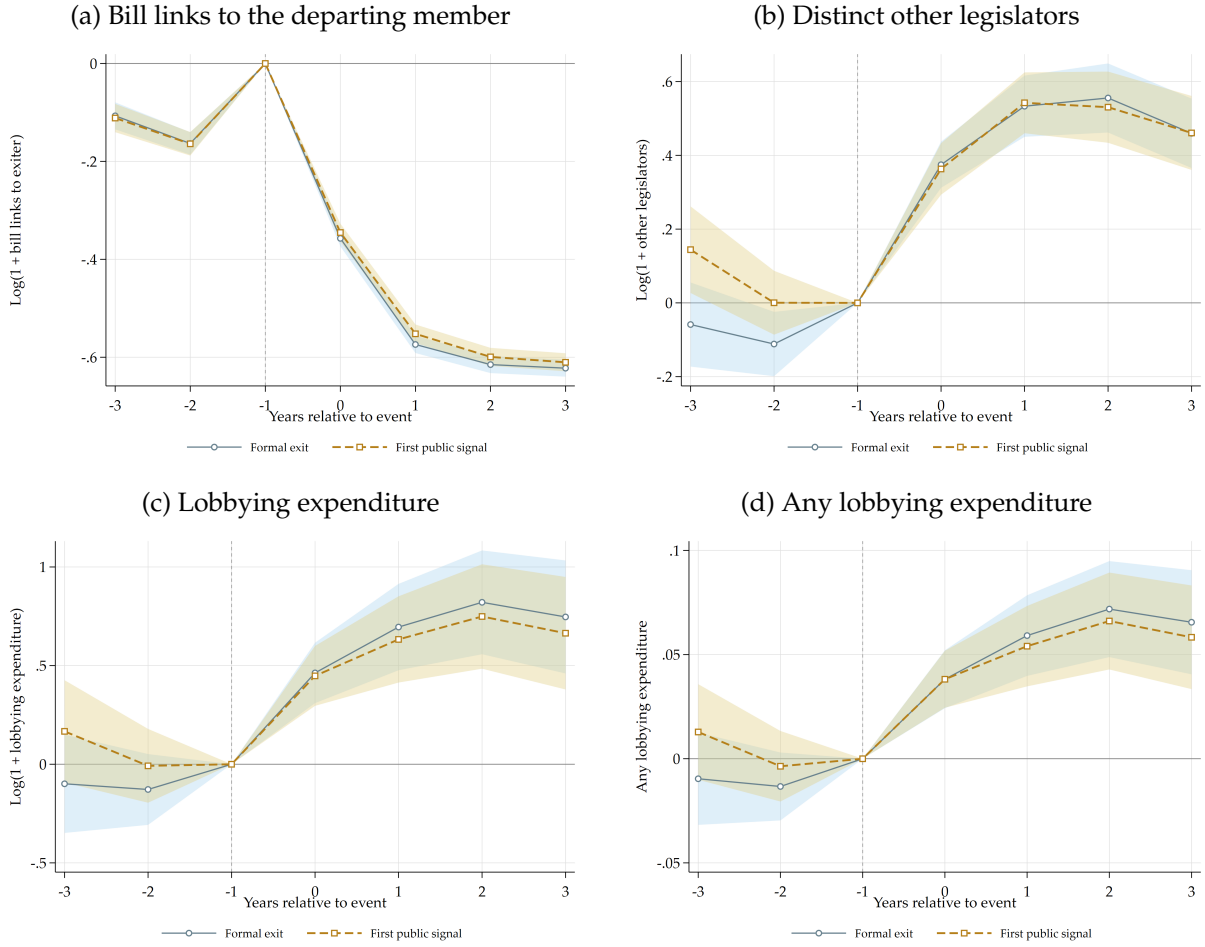
5.2 Heterogeneity by Pre-Event Connection Intensity

We next divide treated firms by the pre-event connection-intensity measure in Equation (3), ranked within departure and pre-event size bin as described in Section 4.2. The top 10 percent are the treated firms most dependent on the departing member relative to firms of similar size, the 50th–90th percentile group has intermediate dependence, and the bottom 50 percent the least.

Figure 2 compares each intensity group with the original control firms under both event clocks. For network breadth, the point estimates generally suggest smaller responses among firms in the top-intensity group and larger responses among firms in the bottom half, with the 50th–90th percentile group typically falling in between. Lobbying expenditure and participation often exhibit a similar descriptive ordering. However, the confidence intervals overlap substantially across the three groups, and several subgroup event-study paths exhibit differential pre-event movement. We therefore treat the ordering of the point estimates as descriptive and do not interpret it as robust evidence of statistically distinct responses across levels of dependence. Panels A and B of Table A4 report the corresponding pooled estimates, while Panel C presents an auxiliary continuous within-treated specification. The continuous rank

omitted year ($k = -1$), even in the absence of anticipation. The statistically significant pre-event coefficients in this panel should thus not, on their own, be interpreted as evidence of anticipatory behavior.

Figure 1: Firm responses around formal exit and the first public signal



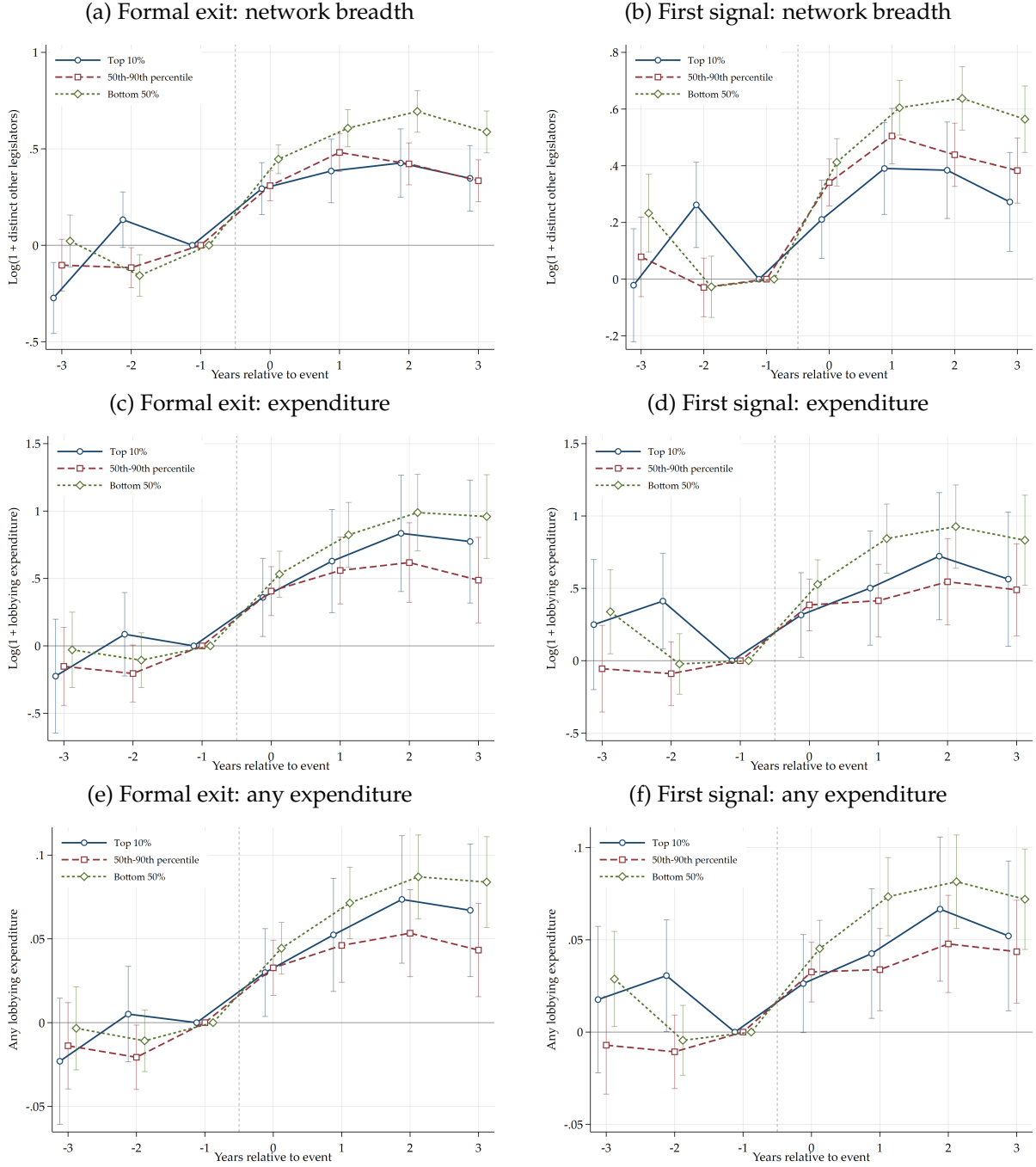
Notes: Event-study coefficients from Equation (2) are relative to $k = -1$. Solid blue-grey lines with circles denote formal exit; dashed mustard lines with squares denote the first public signal. Shaded bands are 95 percent confidence intervals based on standard errors clustered by firm. Regressions use WFH corrective weights and absorb firm–event and event–year fixed effects. Each clock has its own treated and matched comparison firms. Formal-exit estimates use 95,781 observations, 13,683 firm–event units, and 29 departures; first-signal estimates use 91,798 observations, 13,114 units, and 26 departures. Counts and dollar expenditure use $\log(1 + x)$. Network breadth excludes the departing legislator; any expenditure is an indicator for positive expenditure.

gradients point in the same direction, with the negative gradient clearest for network breadth and weaker evidence for expenditure and participation.

5.3 Heterogeneity by Lobbying Mode: In-House versus Outside Lobbyists

Table 4 compares firms that relied mainly on in-house lobbyists with those that relied mainly on outside lobbying firms during the three years before the event. The larger increase in connections among in-house firms is concentrated in the event year and the following year, as shown in Figure A1. This pattern also holds when we classify firms based on where they spent most of their lobbying dollars before the event and when we restrict the sample to firms that used only one type of lobbying, as reported in Table A5. We find no clear difference between the in-house and outside groups in how their lobbying spending or any lobbying expenditure changes.

Figure 2: Firm adjustment by pre-event connection intensity



Notes: Each panel compares the indicated treated-firm intensity group with the original comparison firms matched to the same departure and event clock. Coefficients are relative to $k = -1$; vertical bars are firm-clustered 95 percent confidence intervals. Solid navy circles denote the top 10 percent, dashed maroon squares the 50th–90th percentile, and short-dashed green diamonds the bottom 50 percent. Intensity is the departing member’s share of the firm’s bill-linked activity in the three pre-event years. Firms are ranked by that share within departure and four pre-event network-breadth bins, using average ranks for ties. Regressions recompute WFH corrective weights for each group and absorb firm–event and event–year fixed effects. Breadth and dollar expenditure use $\log(1 + x)$; any expenditure is an indicator for positive expenditure. Table A4 reports pooled estimates and sample sizes.

Table 3: Firm responses under the two event definitions

	(1) Bill links to the departing member (<i>design check</i>)	(2) Distinct legislators connected log(1 + count)	(3) Lobbying expenditure log(1 + USD)	(4) Any lobbying expenditure (0/1)
<i>Panel A: Formal exit</i>				
Treated × Post	-0.452*** (0.005)	0.538*** (0.034)	0.757*** (0.118)	0.066*** (0.010)
Observations	95,781	95,781	95,781	95,781
R^2	0.395	0.456	0.638	0.604
Firm–event units	13,683	13,683	13,683	13,683
Exit events	29	29	29	29
Years covered	2006–2021	2006–2021	2006–2021	2006–2021
<i>Panel B: First public signal</i>				
Treated × Post	-0.435*** (0.005)	0.426*** (0.037)	0.571*** (0.118)	0.051*** (0.010)
Observations	91,798	91,798	91,798	91,798
R^2	0.386	0.457	0.634	0.602
Firm–event units	13,114	13,114	13,114	13,114
Exit events	26	26	26	26
Years covered	2006–2021	2006–2021	2006–2021	2006–2021

Notes: Each column within a panel estimates Equation (1) separately. Panel A uses formal exit; Panel B uses the first public signal. Post includes the event year and the three following years. Count outcomes and lobbying expenditure use $\log(1 + x)$, with expenditure measured in dollars. Network breadth excludes the departing legislator throughout the event window. Any lobbying expenditure is an indicator for positive expenditure, so its coefficient is a change in probability. All regressions use WFH corrective weights and absorb firm–event and event–year fixed effects. Standard errors clustered by firm are reported in parentheses. R^2 reports the overall fit. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The larger increase in network breadth among firms that rely mainly on in-house lobbyists suggests that outside lobbying firms may already have broader relationships they can draw on when a connected legislator leaves. By contrast, firms that rely on their own lobbying staff may need to build more new connections after losing an existing one. This interpretation is suggestive rather than causal, however, because firms choose how they organize their lobbying and our data do not show which lobbyist created each firm–legislator connection.

5.4 Robustness and Inference

5.4.1 Parallel Trends and Placebo Timing

The event-study estimates from Figure 1 show a modest pre-event trend in network breadth. We therefore allow treated firms to follow their own linear event-time trend by performing a sensitivity check below:

$$Y_{iet} = \alpha_{ie} + \lambda_{et} + \beta^{\text{trend}} (\text{Treated}_{ie} \times \text{Post}_{et}) + \rho (\text{Treated}_{ie} \times k_{et}) + u_{iet}, \quad (4)$$

Table 4: Firm adjustment by pre-event lobbying organization

	Network breadth log(1 + count)	Lobbying expenditure log(1 + USD)	Any expenditure 0/1
<i>Panel A: Formal exit</i>			
Primarily outside × Post	0.492*** (0.035)	0.687*** (0.123)	0.061*** (0.011)
Primarily in-house × Post	0.880*** (0.153)	0.798* (0.449)	0.064 (0.040)
In-house minus outside	0.388** (0.157)	0.111 (0.466)	0.004 (0.041)
Observations	94,325	94,325	94,325
R^2	0.458	0.640	0.607
Firm–event units	13,475	13,475	13,475
Exit events	29	29	29
Years covered	2006–2021	2006–2021	2006–2021
<i>Panel B: First public signal</i>			
Primarily outside × Post	0.387*** (0.038)	0.521*** (0.123)	0.047*** (0.011)
Primarily in-house × Post	0.671*** (0.160)	0.516 (0.453)	0.047 (0.040)
In-house minus outside	0.284* (0.164)	-0.005 (0.469)	0.000 (0.041)
Observations	90,363	90,363	90,363
R^2	0.459	0.638	0.606
Firm–event units	12,909	12,909	12,909
Exit events	26	26	26
Years covered	2006–2021	2006–2021	2006–2021

Notes: Firms are classified by the majority of active, non-amended reports in the three pre-event years. Self-filed reports indicate in-house lobbying; reports filed by other registrants indicate outside lobbying. Ties and unclassified firms are excluded. For each event clock, suppressing clock subscripts, the model is $Y_{iet} = \alpha_{ie} + \lambda_{etm} + \beta(T_{ie}P_{et}) + \delta(T_{ie}P_{et}H_{ie}) + \varepsilon_{iet}$, where T indicates treatment, P the event year and three following years, and H primarily in-house lobbying. Firm–event and event–year–mode fixed effects absorb the remaining lower-order terms. Outside firms are the reference group: the outside row reports β , the in-house row $\beta + \delta$, and the difference row δ . Each effect compares treated firms with their original matched comparison firms of the same mode. WFH corrective weights are recomputed within departure–mode groups on each balanced estimation sample. Standard errors are clustered by firm; those for $\beta + \delta$ include the covariance between estimates. Breadth and expenditure use $\log(1 + x)$; expenditure is measured in dollars. Breadth excludes the departing legislator, and expenditure combines in-house expenses and outside registrant income. Any expenditure is an indicator for positive expenditure. Sample statistics describe the pooled regression; R^2 reports the overall fit. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

so that β^{trend} measures the post-departure break from that path rather than from a common trend. Table A2 shows that the results remain positive and statistically significant across all three main outcomes, indicating that the baseline findings are robust to allowing for differential linear trends.

To assess whether the baseline increase in network breadth reflects mechanical mean reversion induced by defining treatment using recent links, we conduct a separate placebo check. Specifically, we assign a placebo departure two years before the actual event and restrict the

analysis to pre-departure data. Table A2 shows no positive placebo analogue of the baseline pattern: under the formal-exit clock, all three estimates are close to zero, while under the first-signal clock the breadth estimate is modestly negative and the expenditure and participation estimates are statistically indistinguishable from zero. This pattern provides little evidence that mechanical mean reversion drives the positive post-event effects.

5.4.2 Inference and Influential Events

Because identification comes from only 29 formal exits, conventional inference may understate uncertainty if outcomes are correlated within departure events, and the pooled estimates could be disproportionately influenced by a small number of exits. We address these concerns using exit-clustered standard errors, inference based on separately estimated event effects, exact sign tests, and leave-one-exit-out estimates. Table A2 shows a consistent pattern across these approaches: the estimated effects remain positive and statistically significant, while omitting any single exit produces only modest changes in magnitude. Thus, the main conclusions do not hinge on clustering at the firm level or on any particular departure.

5.4.3 Construction of the Comparison Group

Because the comparison group is constructed in two stages—first selecting placebo legislators and then matching their connected firms to treated firms—the results could be sensitive to the particular choices made at either stage. We therefore examine alternative approaches to comparison-group construction. Table A3 uses propensity-score matching based on a broader set of pre-treatment characteristics, while Figure A3 systematically varies the selection of placebo legislators and the firm-matching procedure. Across these specifications, the estimated effects on network breadth, lobbying expenditure, and lobbying participation remain positive and statistically significant. The estimates are more sensitive in magnitude to choices made at the firm-matching stage than to the selection of placebo legislators, but the baseline estimates are not unusually large relative to the alternatives. We also require comparison firms to belong to the same broad NAICS sector as each treated firm, retaining the baseline matching covariates and caliper. The resulting estimates in Figure A3 remain positive and statistically significant for all three outcomes. This check uses the smaller sample for which industry classifications and suitable within-sector matches are available. Overall, the main conclusions are robust to alternative methods of constructing the comparison group.

5.4.4 Alternative Samples and Specifications

Because the same firm may be exposed to multiple departures and therefore appear as treated in several event stacks, firms with repeated exposure could exert disproportionate influence on the pooled estimates. To address this concern, Panel B of Table A2 and Figure A2 restrict the sample to firms treated no more than once under each event clock and recompute the WFH weights. All three estimates remain positive. Network breadth remains precisely estimated under both clocks, and expenditure and participation remain significant under the

formal-exit clock. The first-signal expenditure and participation estimates are smaller and significant at the 10 percent level, but not at 5 percent. The network-expansion finding therefore persists without repeated treated exposure, while the corresponding first-signal spending and participation evidence is less precise.

To distinguish adjustments among established lobbyists from changes along the extensive margin, we restrict the sample to firms that report positive lobbying expenditure in every year of the event window. Table A2 shows that network breadth continues to increase substantially among these firms, while lobbying expenditure changes little and is statistically indistinguishable from zero. Because lobbying participation does not vary within this restricted sample, its effect cannot be estimated. This pattern suggests that established lobbyists respond primarily by expanding and reallocating their political connections rather than by increasing total spending. It is also consistent with the full-sample expenditure and participation effects arising primarily through extensive-margin adjustments.

Finally, because departures differ in their cause, timing, and predictability, the pooled estimates could be driven by a particular type of event or by signals of uncertain quality. Figure A4 and Table A6 therefore re-estimate the specification across alternative departure types and signal definitions. The effect on network breadth remains positive and statistically significant across all definitions. The expenditure and participation estimates are also generally positive, although they are less precisely estimated for the small sample of sudden deaths. Overall, the central finding that firms broaden their political networks is not driven by any single departure type or definition of event timing.

6 Formation of New Political Connections

The previous section establishes firms' initial response to the departure of a connected legislator: they broaden their political networks, increase their lobbying expenditures, and devote economically meaningful private resources to adjustment. These results show the scale of firms' responses but not where the expansion in political connections is directed.

The next question is therefore which legislators firms approach as they rebuild their networks. We first examine whether firms form connections with the departing member's close network. We then consider whether firms expand across chambers, which legislators receive the new connections, whether the reconstructed portfolios become more diversified by party affiliation and legislative seniority, and what types of legislative activity these portfolios encompass.

6.1 Do Firms Rebuild through the Departing Member's Close Network?

We use the same WFH-weighted pooled and event-study specifications under both clocks to ask whether firms direct their broader post-event networks toward the departing member's close legislative network. For each departure e and clock d , we partition all other legislators into a fixed close set C_{ed} and its complement. Let L_{iedt} be the number of legislators other than the exiter linked to firm i , and let L_{iedt}^C be the number in C_{ed} . The close-network outcome is

$100 \times L_{iedt}^C / L_{iedt}$, set to zero when $L_{iedt} = 0$. This share measures the composition of the firm's network and accounts for the large post-event expansion in overall breadth. Holding the peer set fixed within a clock-specific stack ensures that changes reflect the firm's connections rather than changes in the composition of the network itself.

Because close legislative networks can be defined in several ways, we use three definitions drawn from the literature. First, alumni ties capture shared educational and social backgrounds (Battaglini and Patacchini 2018; Battaglini, Sciabolazza, et al. 2020). Second, reciprocal cosponsorship captures demonstrated legislative cooperation (Garro 2021). Third, same-state service captures geographic proximity and membership in a common congressional delegation (Cohen et al. 2011; Faccio and Parsley 2009).

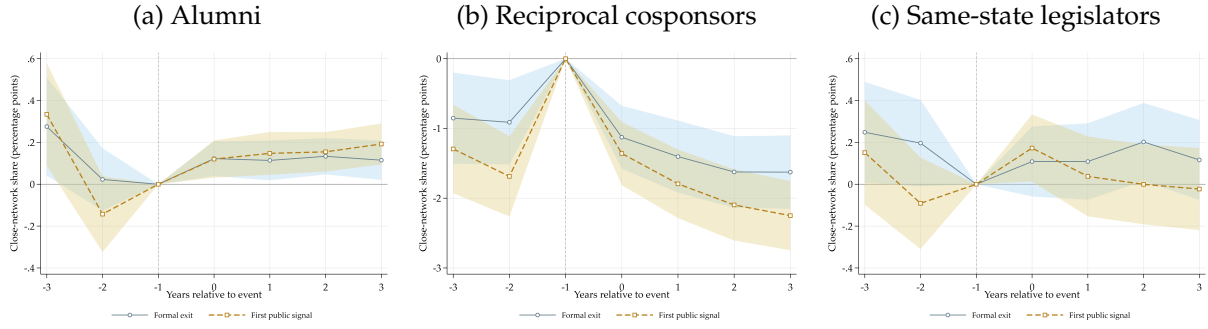
For each definition, we use the broadest available sample. The alumni analysis contains 37,023 observations and ten events under the formal-exit clock and 36,498 observations and nine events under the first-signal clock. The reciprocal-cosponsorship and same-state analyses cover all baseline events, comprising 95,781 observations and 29 events under the formal-exit clock and 91,798 observations and 26 events under the first-signal clock. Events without a reciprocal peer are retained and assigned a share of zero. We preserve the original clock-specific treatment and comparison groups, introduce no additional matching, and do not remove events to improve the pre-event trajectories.

Figure 3 plots the event-study estimates for changes in the percentage of each firm's non-exiter connections that fall within the departing member's alumni, reciprocal-cosponsor, or same-state network. The estimated alumni share changes little under either event clock. The reciprocal-cosponsor share declines after the event and continues to fall through the third post-event year, while the estimated change in the same-state share remains close to zero. Because the reciprocal-cosponsor share also exhibits differential pre-event movement, we interpret its decline as a descriptive pattern rather than a clean causal effect of the departure. Overall, the expansion in firms' political networks is not disproportionately directed toward alumni, reciprocal cosponsors, or same-state legislators.

Table 5 quantifies these patterns using the WFH-weighted pooled specification. The reciprocal-cosponsor share declines by approximately 0.9 percentage points under both event clocks. The alumni and same-state estimates are much smaller and statistically indistinguishable from zero at the 10 percent level. The pooled results therefore reinforce the conclusion that firms do not disproportionately redirect their expanding networks toward the departing member's close connections.

6.2 Chamber, Party, and Close-Network Composition of New Connections

We next examine where firms redirect their expanding networks, beginning with the chamber in which new connections are formed. Figure A5 and Table A7 decompose network breadth according to whether connected legislators serve in the departing member's chamber or in the other chamber. Connections increase both within and across chambers following a departure. The table also shows that network breadth, lobbying expenditure, and lobbying participation

Figure 3: Close-network responses around formal exit and the first public signal

Notes: Coefficients are relative to $k = -1$ and measure changes in close-network shares in percentage points. Shares exclude the departing legislator and equal zero for empty portfolios. Solid blue-grey lines with circles denote formal exit; dashed mustard lines with squares denote the first public signal. Shaded bands are 95 percent confidence intervals based on standard errors clustered by firm. WFH-weighted regressions absorb firm–event and event–year fixed effects. Alumni samples contain 37,023 observations across ten formal exits and 36,498 across nine first-signal events. Reciprocal-cosponsor and same-state samples retain all baseline events: 95,781 observations across 29 exits and 91,798 across 26 first-signal events. Network definitions and data sources follow Table 5; treatment and comparison groups are unchanged.

Table 5: Close-network shares under the two event definitions

	(1) Alumni share	(2) Reciprocal-cosponsor share	(3) Same-state share
<i>Panel A: Formal exit</i>			
Treated $\times$ Post	0.022 (0.051)	-0.857*** (0.166)	-0.014 (0.063)
Firm $\times$ event fixed effects	Yes	Yes	Yes
Event $\times$ year fixed effects	Yes	Yes	Yes
Observations	37,023	95,781	95,781
Firm–event units	5,289	13,683	13,683
Exit events	10	29	29
<i>Panel B: First public signal</i>			
Treated $\times$ Post	0.090 (0.055)	-0.881*** (0.167)	0.027 (0.064)
Firm $\times$ event fixed effects	Yes	Yes	Yes
Event $\times$ year fixed effects	Yes	Yes	Yes
Observations	36,498	91,798	91,798
Firm–event units	5,214	13,114	13,114
Exit events	9	26	26

Notes: Each outcome is the percentage of a firm’s non-exiter links in the indicated close network, set to zero when the firm has no non-exiter links. Alumni ties use the Battaglini-coverage sample. Reciprocal-cosponsor ties combine Battaglini and GovInfo records for both chambers and all four legislation types in the 111th–115th Congresses. Same-state ties use Bioguide. The latter two definitions retain every baseline event, including events with no reciprocal peer. The original clock-specific treatment and comparison groups are unchanged. All regressions use WFH corrective weights and absorb firm–event and event–year fixed effects. Standard errors clustered by firm are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

increase after both House and Senate departures. Overall, firms expand their connections on both sides of the Capitol rather than concentrating their adjustment in the chamber of the lost connection.

To examine this cross-chamber expansion at the level of individual connections, we identify the new links formed by treated firms after each departure. A link is defined as *new* if it appears in the departure year or the following three years but was absent during the three preceding years; the departing member is excluded. This procedure identifies 585,506 new firm–legislator links formed by 4,702 treated firm–event units. Panel (b) of Figure 4 visualizes the chamber destinations of these links, while Panel B of Table A8 reports the corresponding shares. Consistent with the treated–comparison estimates, 61.9 percent of new links remain within the departing member’s chamber, while a substantial 38.1 percent cross chambers.

Panel (a) of Figure 4 shows that these new connections span both major parties. Panel A of Table A8 quantifies this pattern: 45.8 percent are with legislators from the departing member’s party and 53.6 percent are with the other major party. These shares describe treated firms’ new links rather than treated–comparison differences. Because the departing member’s party held roughly half of the seats in the relevant chamber during this period, the nearly even split does not indicate that firms actively target the opposing party. It nevertheless shows that new connections are not concentrated within the departing member’s party.

Finally, Panel (c) of Figure 4 tracks close-network composition by classifying firms in each of the first three post-exit years according to whether they retain at least one connection inside the exiter’s reciprocal-cosponsor circle. The share inside the circle falls from 26.9 percent in year one to 25.5 percent in year two and 22.5 percent in year three; most firms remain outside throughout, and exits from the circle become more common than entries. Like the party and chamber shares, these flows are descriptive, but together they reinforce the main results: firms rebuild broadly across chambers and parties rather than replacing the departing legislator primarily through a politically similar or closely connected counterpart.

6.3 Partisan and Seniority Diversification

We next examine whether firms differentially alter the composition of their annual non-exiter portfolios following the shock. We focus on two dimensions of portfolio composition: party affiliation and legislative seniority.

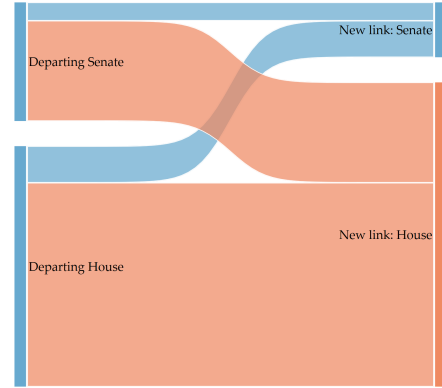
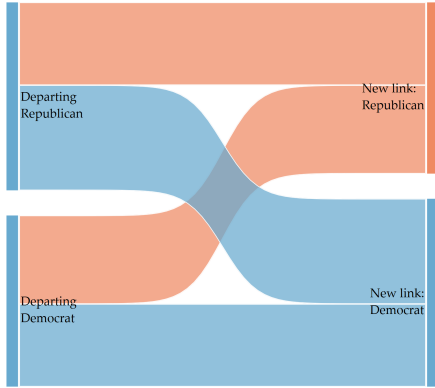
For party affiliation, we use two complementary measures. The opposite-party share captures the direction of adjustment and is defined as the percentage of a firm’s Democratic and Republican non-exiter links belonging to the major party opposite the departing member. The two-party balance index captures overall partisan diversification. Let s_D and s_R denote the Democratic and Republican shares of the firm’s major-party links. The index, $4s_Ds_R$, ranges from zero when all such links belong to one party to one when they are evenly divided between the two parties.

For legislative seniority, we examine whether firms respond by approaching relatively new legislators or by distributing their connections more evenly across lawmakers at different career stages. The freshman share is defined as the percentage of classifiable non-exiter links serving their first Congress and captures adjustment toward newly elected members. The seniority diversity index captures the overall distribution of connections across junior legislators serving

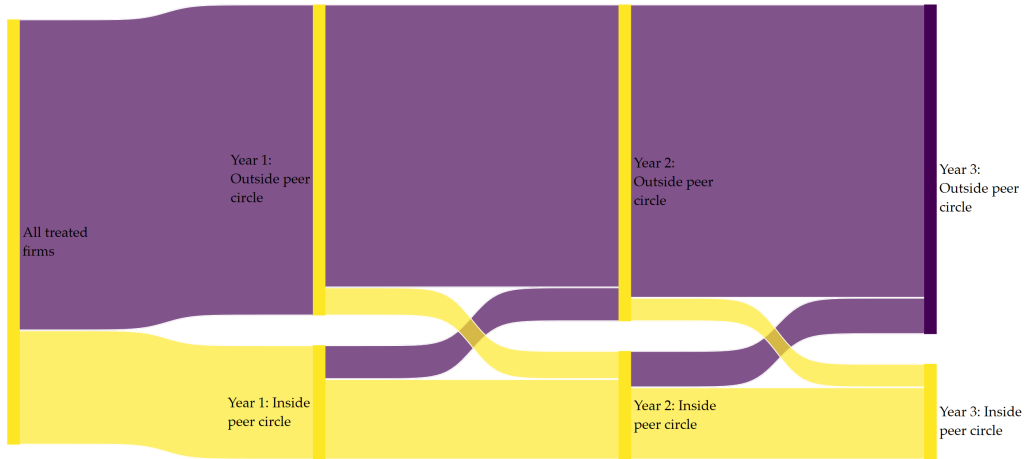
Figure 4: Where firms form and maintain political connections after an exit

(a) Party of exiter and new connection

(b) Chamber of exiter and new connection



(c) Treated firms inside and outside the reciprocal-cosponsor circle



Notes: Ribbon widths represent links in Panels (a)–(b) and firm–event units in Panel (c). A new link appears in the exit year or one of the next three years, but not in the three pre-exit years; the departing legislator is excluded. In Panel (b), destination chambers are blue for Senate and orange for House. Panels (a)–(b) contain 585,506 new links formed by 4,702 treated firm–event units. Panel (c) includes all 7,748 treated units. A firm is inside the reciprocal-cosponsor network if it has at least one link to a reciprocal cosponsor of the departing member, using combined Battaglini–GovInfo coverage; otherwise it is outside. All panels show descriptive flows, not treated–comparison differences.

their first or second Congress, midcareer legislators serving their third through fifth Congresses, and veteran legislators serving their sixth or a later Congress. Let s_J , s_M , and s_V denote the respective portfolio shares. The index, $(3/2)(1 - s_J^2 - s_M^2 - s_V^2)$, ranges from zero when all links fall within one seniority group to one when they are evenly distributed across the three groups.

Table 6 reports two sets of estimates. Panel A captures total changes, assigning zero when a firm has no eligible non-exiter links—that is, no Democratic or Republican links for the party measures or no links with classifiable seniority for the seniority measures. It therefore combines network formation with changes in composition. Panel B restricts the sample to portfolios with

eligible links in every event-window year and controls for current network breadth. Because breadth responds to the shock, Panel B is a post-treatment composition decomposition rather than a total causal effect. Its balanced active samples contain 1,502 firm–event units across 28 formal exits and 1,565 units across 26 first-signal events.

Panels (a) and (b) of Figure 5 and the first two columns of Table 6 show that the clearest partisan change among complete-window active portfolios is an increase in the share of links to the departing member’s opposing party under both event clocks. By contrast, the size-adjusted two-party balance estimates are statistically indistinguishable from zero under both clocks. The results therefore provide stronger evidence of expansion toward the opposite party than of a more evenly balanced partisan portfolio among continuously active networks.

Panels (c) and (d) of Figure 5 and the final two columns of Table 6 suggest changes in seniority composition. The size-adjusted freshman-share estimate is positive under the first-signal clock but significant only at the 10 percent level; the formal-exit estimate is not statistically distinguishable from zero. Seniority diversity increases significantly at formal exit but is less precisely estimated under the first-signal clock. These patterns are consistent with some diversification across career stages, but do not establish a precise sequence of adjustment because the clocks use different samples.

We also examine whether rebuilt portfolios shift toward women, Black, or Latino legislators. Panel A of Table A9 reports positive total changes under both event clocks, combining the formation and expansion of classified networks with changes in their composition. Panel B restricts attention to portfolios active in every event-window year and adjusts for current breadth. Women’s share, gender balance, and Latino share show no statistically clear conditional change under either clock. The Black-legislator share increases under the formal-exit clock, while the first-signal estimate is less precise. These conditional results describe continuously active portfolios and do not establish a general demographic reorientation across both clocks.

6.4 Legislative Activity Represented in Rebuilt Portfolios

The previous results show where firms redirect their lobbying attention, but not whether the connected legislators sponsor measures that receive congressional consideration. A connection to a legislator whose measures receive committee consideration or reach the chamber floor may represent a more legislatively active and potentially more effective political connection than one whose measures remain at the introduction stage. Using GovInfo BILLSTATUS records for the 111th–115th Congresses and restricting to departures in 2012–2015 with complete seven-year windows within the 2009–2018 coverage, we count each legislator’s sponsored bills and joint resolutions and identify measures receiving committee or floor action. We then measure the number of sponsored measures represented in each firm’s non-exiter portfolio and the shares receiving these actions.

Table A10 shows that, following an exit, firms’ non-exiter portfolios encompass more sponsored policy measures and larger shares of measures receiving committee and floor action. The estimated increase is 1.192 log points for sponsored measures, 3.459 percentage points for

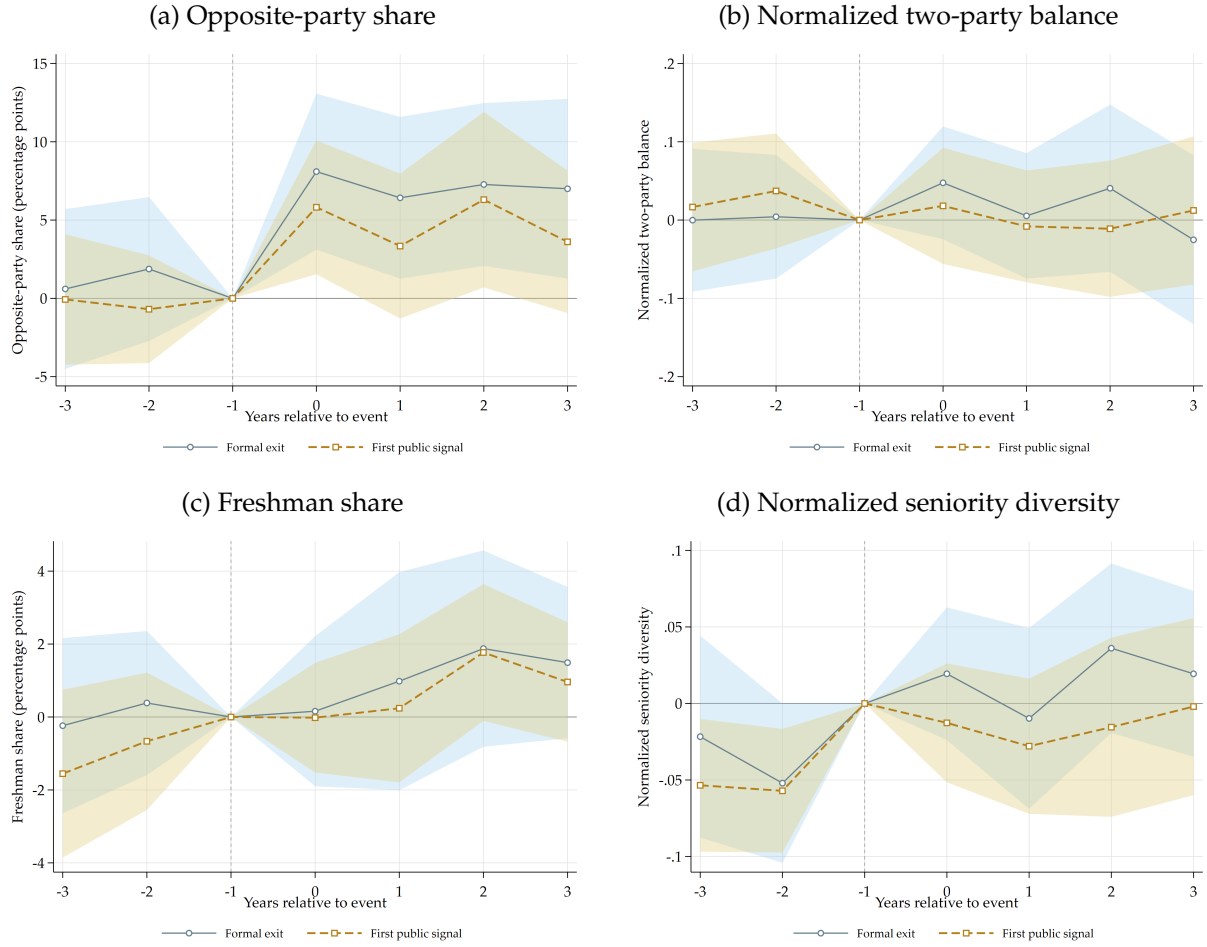
Table 6: Partisan and seniority composition of rebuilt networks

	Opposite-party share (pp)	Two-party balance	Freshman share (pp)	Seniority diversity
<i>Panel A: Total composition effect</i>				
Formal exit: Treated $\times$ Post	11.107*** (0.526)	0.105*** (0.008)	1.724*** (0.156)	0.132*** (0.008)
Observations	95,781	95,781	95,781	95,781
R^2	0.415	0.408	0.325	0.428
Firm–event units	13,683	13,683	13,683	13,683
Exit events	29	29	29	29
Years covered	2006–2021	2006–2021	2006–2021	2006–2021
First public signal: Treated $\times$ Post	9.562*** (0.552)	0.074*** (0.008)	1.658*** (0.167)	0.110*** (0.008)
Observations	91,798	91,798	91,798	91,798
R^2	0.422	0.407	0.326	0.431
Firm–event units	13,114	13,114	13,114	13,114
Exit events	26	26	26	26
Years covered	2006–2021	2006–2021	2006–2021	2006–2021
<i>Panel B: Size-adjusted composition among active portfolios</i>				
Formal exit: Treated $\times$ Post	6.376*** (2.137)	0.016 (0.030)	1.076 (1.077)	0.041** (0.020)
Event-clustered p -value	0.006	0.445	0.197	0.062
Observations	10,514	10,514	10,514	10,514
R^2	0.588	0.442	0.516	0.825
Firm–event units	1,502	1,502	1,502	1,502
Exit events	28	28	28	28
Years covered	2006–2021	2006–2021	2006–2021	2006–2021
First public signal: Treated $\times$ Post	5.022*** (1.928)	-0.015 (0.027)	1.476* (0.851)	0.022 (0.019)
Event-clustered p -value	0.009	0.431	0.007	0.185
Observations	10,955	10,955	10,955	10,955
R^2	0.573	0.433	0.527	0.832
Firm–event units	1,565	1,565	1,565	1,565
Exit events	26	26	26	26
Years covered	2006–2021	2006–2021	2006–2021	2006–2021

Notes: Opposite-party share is the percentage of Democratic and Republican non-exiter links belonging to the party opposite the departing member. Two-party balance is $4s_D s_R$, where s_D and s_R are the two parties' shares. Freshman share is the percentage of links with known seniority belonging to first-Congress legislators. Seniority diversity is $(3/2)(1 - s_J^2 - s_M^2 - s_V^2)$ for junior (first or second Congress), midcareer (third through fifth), and veteran (sixth or later) legislators. Both indices range from zero to one. Party and first-service dates come from the archived Bioguide last-party and term-start fields. Panel A assigns zero to empty relevant portfolios. Panel B requires an active, classifiable portfolio throughout the event window and controls quadratically for current $\log(1 + \text{network breadth})$; it is a conditional composition decomposition because breadth is post-treatment. All regressions use WFH corrective weights and absorb firm–event and event–year fixed effects. Standard errors clustered by firm are reported in parentheses. Stars use firm-clustered inference; Panel B also reports departure-clustered p -values. Statistics describe each estimation sample; R^2 reports overall fit. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the committee-action share, and 1.790 percentage points for the floor-action share. Overall, the

Figure 5: Partisan and seniority composition of rebuilt networks



Notes: Coefficients are relative to $k = -1$. Solid blue-grey lines with circles denote formal exit; dashed mustard lines with squares denote the first public signal. Shaded bands are 95 percent confidence intervals based on standard errors clustered by firm. Samples require an active, classifiable portfolio throughout the event window. Regressions use WFH corrective weights, absorb firm-event and event-year fixed effects, and control quadratically for current $\log(1 + \text{network breadth})$. Because breadth is post-treatment, these estimates describe conditional composition changes. Shares are in percentage points; diversity indices range from zero to one. Outcome definitions follow Table 6.

results suggest that firms rebuild their networks toward legislators with more active legislative portfolios.

Nevertheless, these results serve only as proxies for the potential effectiveness of firms' rebuilt networks and do not establish that lobbying causes legislative action. They describe connected legislators' full policy portfolios rather than only the bills cited by firms, and the observed links indicate disclosed lobbying attention rather than direct contact or support. Whether these portfolios generate policy benefits for firms therefore remains an open question.

6.5 Interpretation

Section 2.3 distinguishes among three possible responses to the loss of a political connection: substitution toward a similar legislator, costly network rebuilding, or retrenchment from lobbying. Section 5 shows that firms expand their political networks, increase their lobbying activity, and incur substantial adjustment costs following a departure. Section 6 further shows that the

new connections are broadly dispersed rather than concentrated within the departing member's close network. These patterns are most consistent with costly replacement and suggest that political relationships contain person-specific capital that firms rebuild by diversifying across multiple legislators.

The findings identify how firms rebuild political capital and the costs associated with doing so. They do not establish that the new connections restore the value of the lost relationship. Moreover, the legislative-activity measures provide only proxies for the potential effectiveness of rebuilt networks and do not reveal whether those networks generate policy benefits for firms. The ultimate returns to rebuilding therefore remain an open question.

7 Conclusion

Because the value of political connections may be tied to individual lawmakers, this paper asks how firms restructure their political engagement when an established congressional connection disappears. Studying deaths in office and scandal-related resignations around both the first public signal and the formal exit, we find that exposed firms expand rather than withdraw. They connect with more legislators, increase their lobbying expenditures, become more likely to engage in lobbying, and devote economically meaningful private resources to rebuilding their networks.

The new connections extend across chambers and parties and span legislators at different stages of their congressional careers, rather than concentrating within the departing member's close network. The rebuilt networks also encompass more legislatively active portfolios. Firms primarily using in-house lobbyists expand their networks more than firms primarily hiring outside lobbyists, consistent with differences in access to portable political relationships. Taken together, these findings are most consistent with costly replacement rather than simple substitution or retrenchment. Political relationships appear to contain person-specific capital that cannot be transferred through a one-for-one replacement, leading firms to invest in a broader and more diversified set of connections.

Future research can build on this analysis by examining whether new connections restore the value of lost relationships and whether rebuilt networks generate policy benefits for firms. More broadly, it can investigate whether political turnover shifts access toward firms best able to bear the costs of rebuilding and whether this redistribution affects which interests are represented in policymaking.

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A Appendix Tables

Table A1: Sample construction

Step	Formal exit	First public signal
Congressional exits in the source workbook, 1997–2020	152	152
deaths in office / resignations	38 / 114	38 / 114
Exits matched to a Bioguide identifier	152	152
Deaths and scandal resignations	67	67
with a full seven-year window inside the network years	29	28
final analysis events: deaths / scandal resignations	11 / 18	9 / 17
Treated firm–event units	7,748	7,377
Matched comparison firm–event units	5,935	5,737
All units lobbying in all seven years	5,632	5,404
Firm–event–year observations	95,781	91,798
Legislators in the peer panel: treated / comparison	257 / 886	–
events behind the peer panel	21	–
Event years covered	2009–2018	2009–2018
Calendar years covered	2006–2021	2006–2021

Notes: Network coverage spans 2006–2021. Requiring three years before and three years after each event therefore admits event years 2009–2018. Under the first-public-signal clock, 28 departures have complete windows; two have no connected firms in the three pre-signal years, leaving 26 estimable events. Firm counts refer to firm–event units: a firm appearing in multiple stacks is counted once in each. The surviving-legislator panel uses formal exit only.

Table A2: Robustness and inference under both event clocks

Specification	Event clock	Network breadth	Expenditure	Any expenditure	Observations
<i>Panel A: Inference and influential events</i>					
Baseline, firm-clustered	Formal exit	0.538*** (0.034)	0.757*** (0.118)	0.066*** (0.010)	95,781
	First signal	0.426*** (0.037)	0.571*** (0.118)	0.051*** (0.010)	91,798
Exit-clustered inference	Formal exit	0.538*** (0.047)	0.757*** (0.175)	0.066*** (0.015)	95,781
	First signal	0.426*** (0.064)	0.571*** (0.178)	0.051*** (0.015)	91,798
Differential linear trend	Formal exit	0.439*** (0.039)	0.464*** (0.096)	0.038*** (0.009)	95,781
	First signal	0.428*** (0.044)	0.463*** (0.096)	0.039*** (0.009)	91,798
Mean event-specific estimate	Formal exit	0.596*** (0.062)	0.776*** (0.155)	0.069*** (0.013)	95,781
	First signal	0.473*** (0.084)	0.683*** (0.184)	0.061*** (0.016)	91,798
Leave-one-exit-out range	Formal exit	[0.516, 0.559]	[0.691, 0.864]	[0.061, 0.076]	Varies
	First signal	[0.407, 0.450]	[0.507, 0.680]	[0.046, 0.061]	Varies
<i>Panel B: Alternative samples, exposure, and functional form</i>					
Always-lobbying firms	Formal exit	0.773*** (0.066)	0.040 (0.027)	–	39,424
	First signal	0.623*** (0.072)	0.004 (0.025)	–	37,828
Continuous treatment intensity	Formal exit	1.673*** (0.074)	-0.082 (0.373)	-0.020 (0.034)	95,781
	First signal	1.707*** (0.076)	-0.209 (0.378)	-0.026 (0.034)	91,798
Placebo event two years early	Formal exit	0.003 (0.047)	0.035 (0.100)	0.003 (0.009)	41,049
	First signal	-0.144*** (0.051)	-0.171 (0.105)	-0.015 (0.009)	39,342
Outcomes in levels	Formal exit	14.653*** (1.328)	13.514*** (3.091)	0.066*** (0.010)	95,781
	First signal	9.553*** (1.468)	9.403*** (3.192)	0.051*** (0.010)	91,798
Firms treated at most once	Formal exit	0.521*** (0.039)	0.477*** (0.132)	0.039*** (0.011)	61,152
	First signal	0.382*** (0.040)	0.234* (0.130)	0.019* (0.011)	58,884

Notes: Regressions use WFH corrective weights and firm–event and event–year fixed effects. Parentheses report firm-clustered standard errors unless departure clustering is indicated. Mean event-specific rows report standard errors of the mean; stars use exact two-sided sign tests across 29 formal-exit or 26 first-signal estimates. Leave-one-exit-out rows report coefficient ranges with varying sample sizes. Differential-trend models add a treated-group linear event-time trend. Continuous-intensity models replace treatment with the departing member’s pre-event share of bill-linked activity. Placebos shift events two years earlier and use only pre-event observations. Any expenditure is constant in the always-lobbying sample. Levels models use legislator counts and expenditure in thousands of dollars; expenditure is capped at the pooled formal-exit 99th percentile (USD 1,000,000). At-most-once models exclude firms treated in multiple stacks under each clock. Other breadth and expenditure outcomes use $\log(1 + x)$; any-expenditure effects are probability changes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Propensity-score matching

	Network breadth	Lobbying expenditure	Any expenditure
Baseline: Treated $\times$ Post	0.538*** (0.034)	0.757*** (0.118)	0.066*** (0.010)
Propensity-score matched: Treated $\times$ Post	0.774*** (0.052)	0.688*** (0.161)	0.058*** (0.014)
Baseline observations	95,781	95,781	95,781
Matched observations	87,115	87,115	87,115
Baseline treated / comparison units	7,748 / 5,935	7,748 / 5,935	7,748 / 5,935
Matched treated / comparison units	7,360 / 5,085	7,360 / 5,085	7,360 / 5,085
Exit events	29	29	29
Years covered	2006–2021	2006–2021	2006–2021
Largest absolute standardised difference, before / after	0.417 / 0.050	0.417 / 0.050	0.417 / 0.050

Notes: Both specifications use the formal-exit clock. Propensity scores are estimated within departure using pre-event levels and changes in expenditure, network breadth, reports filed, and bills lobbied. Each treated firm is matched to three comparison firms, with replacement, within a 0.05 caliper and on common support. WFH corrective weights are combined with comparison-firm reuse weights. Regressions absorb firm–event and event–year fixed effects. Breadth and dollar expenditure use $\log(1 + x)$; any expenditure is an indicator for positive expenditure. Standard errors clustered by firm are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Firm adjustment by pre-event connection intensity

	Network breadth	Expenditure	Any expenditure
<i>Panel A: Formal exit</i>			
Top 10% $\times$ Post	0.410*** (0.067)	0.695*** (0.182)	0.062*** (0.016)
Observations	46,872	46,872	46,872
50th–90th percentile $\times$ Post	0.460*** (0.040)	0.637*** (0.131)	0.055*** (0.011)
Observations	63,889	63,889	63,889
Bottom 50% $\times$ Post	0.629*** (0.039)	0.871*** (0.127)	0.076*** (0.011)
Observations	68,110	68,110	68,110
<i>Panel B: First public signal</i>			
Top 10% $\times$ Post	0.234*** (0.067)	0.305 (0.188)	0.031* (0.016)
Observations	45,199	45,199	45,199
50th–90th percentile $\times$ Post	0.400*** (0.042)	0.507*** (0.130)	0.045*** (0.011)
Observations	61,530	61,530	61,530
Bottom 50% $\times$ Post	0.486*** (0.042)	0.677*** (0.127)	0.060*** (0.011)
Observations	65,387	65,387	65,387
<i>Panel C: Continuous rank among treated firms, formal exit</i>			
Intensity rank $\times$ Post	-0.312** (0.125)	-0.288* (0.160)	-0.026* (0.014)
Observations	54,236	54,236	54,236

Notes: Panels A–B compare each treated-firm intensity group with the original matched comparison firms under the indicated clock. Intensity is the departing member’s share of pre-event bill-linked activity, ranked within departure and four pre-event network-breadth bins. WFH corrective weights are recomputed for each group. Panel C is a continuous-rank gradient among treated firms under the formal-exit clock; it is not a binary difference-in-differences estimate, and treated-unit WFH weights equal one. All models absorb firm–event and event–year fixed effects. Breadth and dollar expenditure use $\log(1 + x)$; any expenditure is an indicator for positive expenditure. Standard errors in parentheses are clustered by firm in Panels A–B and by both firm and departure in Panel C. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Alternative definitions of pre-event lobbying organization

Clock	Definition	Outcome	Outside effect	In-house effect	Difference
Formal exit	Majority of amount	Breadth	0.501*** (0.037)	1.039*** (0.149)	0.538*** (0.153)
		Expenditure	0.731*** (0.130)	1.204*** (0.388)	0.473 (0.408)
		Any Expenditure	0.064*** (0.011)	0.094*** (0.035)	0.030 (0.036)
	Exclusive use	Breadth	0.446*** (0.036)	1.107*** (0.167)	0.661*** (0.170)
		Expenditure	0.590*** (0.126)	1.047* (0.534)	0.457 (0.551)
		Any Expenditure	0.053*** (0.011)	0.087* (0.047)	0.034 (0.049)
	Majority of amount	Breadth	0.370*** (0.041)	0.912*** (0.157)	0.542*** (0.161)
		Expenditure	0.554*** (0.130)	0.706 (0.436)	0.153 (0.453)
		Any Expenditure	0.050*** (0.011)	0.058 (0.037)	0.008 (0.038)
First public signal	Exclusive use	Breadth	0.334*** (0.039)	0.911*** (0.187)	0.577*** (0.191)
		Expenditure	0.456*** (0.125)	0.565 (0.542)	0.109 (0.557)
		Any Expenditure	0.042*** (0.011)	0.052 (0.048)	0.010 (0.049)

Notes: Both classifications use the three pre-event years. Majority of amount assigns firms by whether reported in-house expenses or outside registrant income is larger; zero totals and ties are excluded. Exclusive use requires at least one classified report of one mode and none of the other; unresolved reports do not count toward either mode. Regressions retain the original clock-specific matched comparison firms, recompute WFH corrective weights within departure-mode groups, and absorb firm-event and event-year-mode fixed effects. Outside, in-house, and difference columns report β , $\beta + \delta$, and δ from Table 4. Breadth and dollar expenditure use $\log(1 + x)$; any expenditure is an indicator for positive expenditure. Standard errors clustered by firm are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Main outcomes under alternative definitions of the event

Definition	Breadth	Expenditure	Any spend	Events	Observations
All deaths + scandals	0.538*** (0.034)	0.757*** (0.118)	0.066*** (0.010)	29	95,781
Deaths only	0.536*** (0.035)	0.603*** (0.127)	0.053*** (0.011)	11	48,524
Scandals only	0.539*** (0.049)	0.908*** (0.156)	0.080*** (0.013)	18	47,257
Sudden/short-notice deaths	0.528*** (0.043)	0.248 (0.167)	0.023 (0.015)	5	23,919
All at anticipation year	0.426*** (0.037)	0.571*** (0.118)	0.051*** (0.010)	26	91,798
Earlier-anticipated deaths	0.314*** (0.058)	0.589*** (0.173)	0.053*** (0.015)	4	17,773
High-confidence anticipation	0.383*** (0.042)	0.460*** (0.128)	0.041*** (0.011)	20	70,329
Years covered	2006–2021				

Notes: Each row re-estimates Equation (1) for the indicated event definition. First-public-signal specifications reconstruct treatment, placebo legislators, matched comparison firms, and the seven-year window around that signal. Signals and exits in the same calendar year share a treatment year because outcomes are annual. Breadth and dollar expenditure use $\log(1 + x)$; any expenditure is an indicator for positive expenditure. All regressions use WFH corrective weights and absorb firm–event and event–year fixed effects. Standard errors clustered by firm are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Where the rebuilt network goes: reallocation across chambers

<i>Panel A: Which legislators the firm connects to, all exits</i>					
	All other	Own chamber	Other chamber	House	Senate
Treated $\times$ Post	0.538*** (0.034)	0.354*** (0.030)	0.372*** (0.025)	0.490*** (0.033)	0.236*** (0.023)
Observations	95,781	95,781	95,781	95,781	95,781
Exit events	29	29	29	29	29

<i>Panel B: Main outcomes by the departing member's chamber</i>			
	Network breadth	Expenditure	Any expenditure
House exits: Treated $\times$ Post	0.551*** (0.040)	0.698*** (0.138)	0.061*** (0.012)
Observations	61,894	61,894	61,894
Exit events	22	22	22
Senate exits: Treated $\times$ Post	0.509*** (0.043)	0.883*** (0.140)	0.077*** (0.012)
Observations	33,887	33,887	33,887
Exit events	7	7	7

Notes: All specifications use the formal-exit clock. Panel A separates non-exiter connections by chamber, defined relative to the departing member. Panel B estimates the main outcomes separately for House and Senate departures. Counts and dollar expenditure use $\log(1 + x)$; any expenditure is an indicator for positive expenditure. Chamber is assigned using the archived Bioguide last-chamber field. All regressions use WFH corrective weights and absorb firm–event and event–year fixed effects. Standard errors clustered by firm are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Where the replacement connections go

Destination of the new link	New links	Share (%)	Links per firm
<i>Panel A: Party of the new connection</i>			
Same party as the departing member	267,884	45.8	56.97
The other major party	313,691	53.6	66.71
An independent or minor-party member	3,931	0.7	0.84
All new links, major-party exits	585,506	100.0	124.52
<i>Panel B: Chamber of the new connection</i>			
Same chamber as the departing member	362,289	61.9	77.05
The other chamber	223,217	38.1	47.47
All new links	585,506	100.0	124.52
Treated firm–event units with a new link		4,702	
Exit events		29	
Years covered		2006–2021	

Notes: A new link is a firm–legislator pair observed in the exit year or one of the next three years but not in the three pre-exit years. The departing legislator is excluded. Party and chamber are assigned using the archived Bioguide register. Panel A restricts to departures by major-party members; all departures in this sample meet that condition. Both panels therefore cover 585,506 new links formed by 4,702 treated firm–event units. Shares use each panel’s total as the denominator; links per firm use firm–event units with at least one new link. These are descriptive flows among treated firms, not treated–comparison differences.

Table A9: Race and gender composition of rebuilt networks

	Women's share (pp)	Gender balance	Black share (pp)	Latino share (pp)
<i>Panel A: Total composition effect</i>				
Formal exit: Treated $\times$ Post	3.748*** (0.285)	0.098*** (0.006)	1.791*** (0.149)	0.938*** (0.107)
Observations	95,781	95,781	95,781	95,781
R^2	0.369	0.417	0.340	0.307
Firm–event units	13,683	13,683	13,683	13,683
Exit events	29	29	29	29
Years covered	2006–2021	2006–2021	2006–2021	2006–2021
First public signal: Treated $\times$ Post	3.045*** (0.297)	0.087*** (0.007)	1.711*** (0.158)	0.879*** (0.112)
Observations	91,798	91,798	91,798	91,798
R^2	0.366	0.416	0.340	0.311
Firm–event units	13,114	13,114	13,114	13,114
Exit events	26	26	26	26
Years covered	2006–2021	2006–2021	2006–2021	2006–2021
<i>Panel B: Size-adjusted composition among active portfolios</i>				
Formal exit: Treated $\times$ Post	0.825 (1.692)	0.007 (0.028)	2.426*** (0.855)	-0.710 (0.687)
Event-clustered p -value	0.537	0.808	0.007	0.306
Observations	10,514	10,514	10,514	10,514
R^2	0.393	0.513	0.451	0.298
Firm–event units	1,502	1,502	1,502	1,502
Exit events	28	28	28	28
Years covered	2006–2021	2006–2021	2006–2021	2006–2021
First public signal: Treated $\times$ Post	-0.309 (1.748)	0.030 (0.023)	1.174 (0.816)	0.312 (0.424)
Event-clustered p -value	0.774	0.251	0.022	0.522
Observations	10,955	10,955	10,955	10,955
R^2	0.391	0.514	0.455	0.305
Firm–event units	1,565	1,565	1,565	1,565
Exit events	26	26	26	26
Years covered	2006–2021	2006–2021	2006–2021	2006–2021

Notes: Women's share is the percentage of non-exiter links with known gender coded female. Gender balance is $4s_F s_M$, where s_F and s_M are female and male shares; it ranges from zero to one. Black and Latino shares use the corresponding Center for Effective Lawmaking indicators among links with classified traits. These indicators do not form an exhaustive, mutually exclusive classification of race and ethnicity. Panel A assigns zero to empty relevant portfolios. Panel B requires an active, classifiable portfolio throughout the event window and controls quadratically for current $\log(1 + \text{network breadth})$; it is a conditional composition decomposition because breadth is post-treatment. All regressions use WFH corrective weights and absorb firm–event and event–year fixed effects. Standard errors clustered by firm are reported in parentheses. Stars use firm-clustered inference; Panel B also reports departure-clustered p -values. Statistics describe each estimation sample; R^2 reports overall fit. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

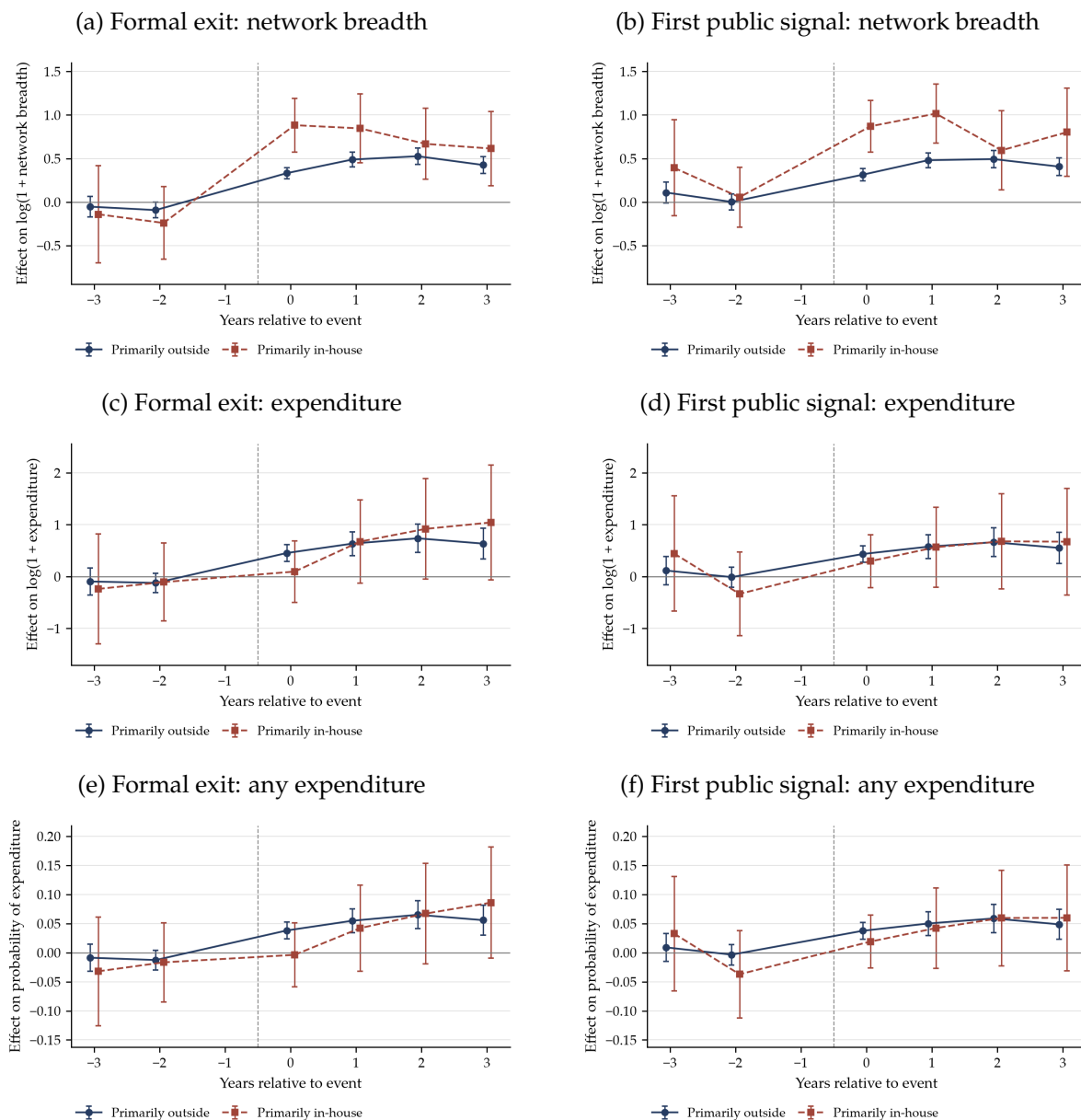
Table A10: Legislative activity represented in rebuilt portfolios

	Sponsored measures log(1+count)	Committee action Percentage points	Floor action Percentage points
Treated $\times$ Post	1.192*** (0.094)	3.459*** (0.379)	1.790*** (0.299)
Observations	28,826	28,826	28,826
Firm–event units	4,118	4,118	4,118
Exit events	9	9	9
Firm–event fixed effects	Yes	Yes	Yes
Event–year fixed effects	Yes	Yes	Yes

Notes: The formal-exit sample retains departures in 2012–2015 with complete seven-year windows within GovInfo’s 2009–2018 coverage. Sponsored measures are bills and joint resolutions represented in firms’ non-exiter portfolios, measured as $\log(1 + \text{count})$. Committee- and floor-action shares are measured in percentage points and set to zero for empty portfolios. These outcomes describe connected legislators’ full policy portfolios, not lobbying success or the causal effect of lobbying on legislative action. All regressions use WFH corrective weights and absorb firm–event and event–year fixed effects. Standard errors clustered by firm are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

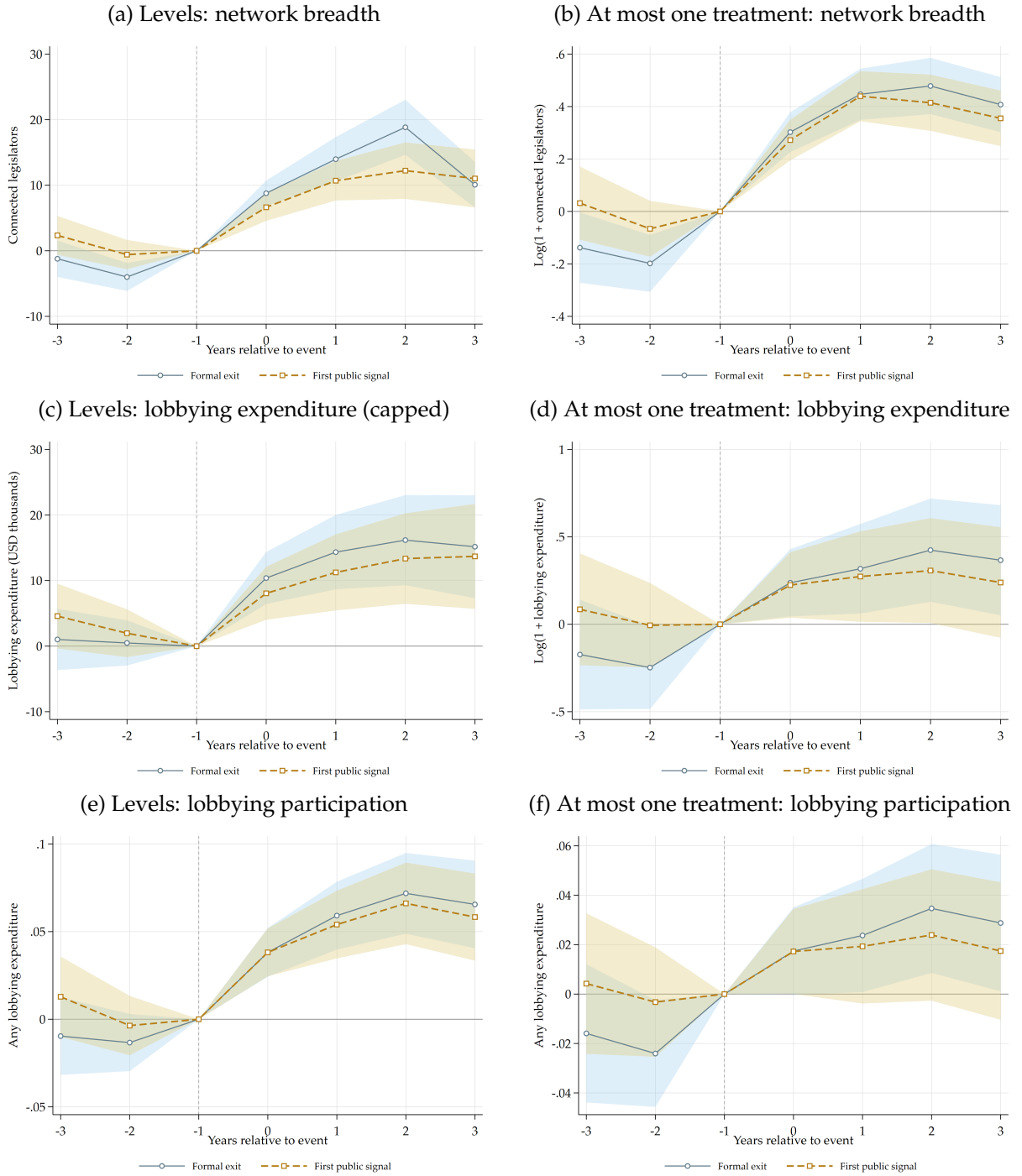
B Appendix Figures

Figure A1: Firm adjustment by pre-event lobbying organization



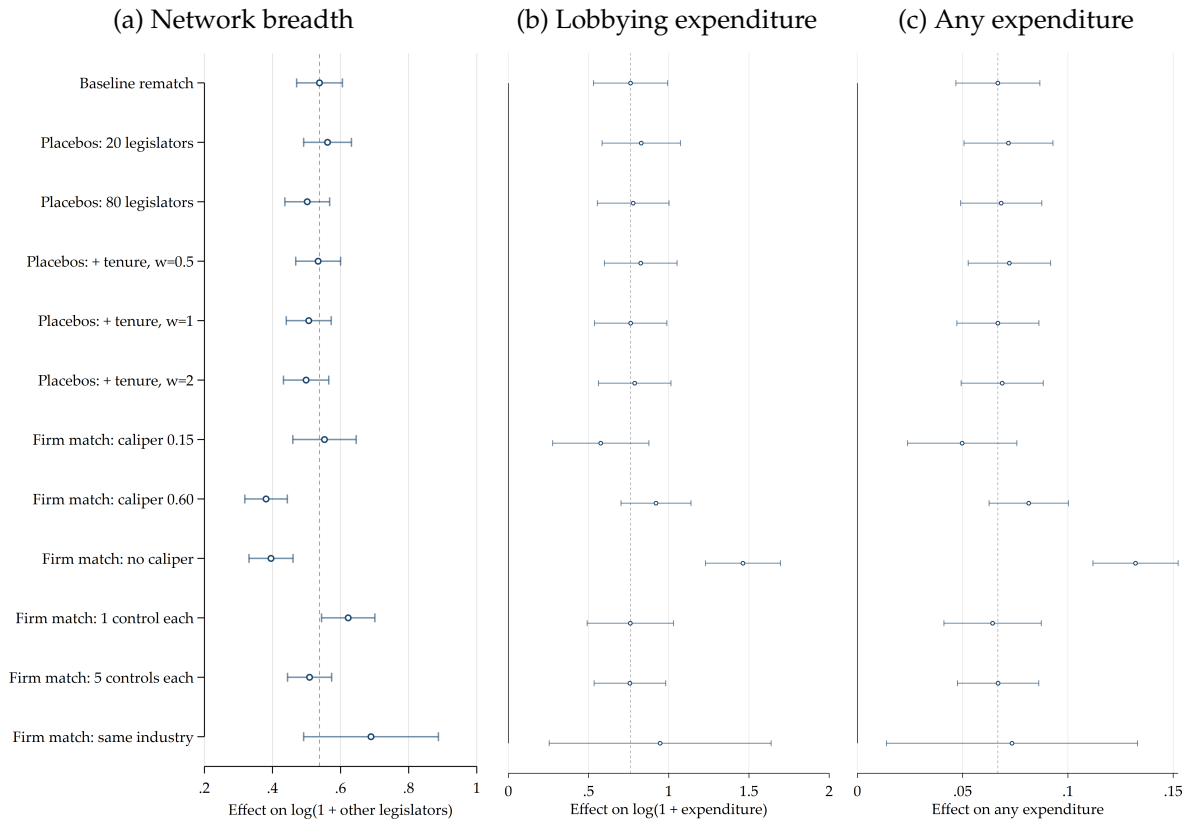
Notes: The left column uses formal exit; the right uses the first public signal. Rows show network breadth, lobbying expenditure, and any lobbying expenditure. Firms are classified by the majority of active, non-amended reports in the three pre-event years; ties and unclassified firms are excluded. Solid navy circles denote primarily outside firms; dashed maroon squares denote primarily in-house firms. Each series compares treated firms with their original matched comparison firms of the same mode, relative to $k = -1$. Vertical bars are 95 percent confidence intervals based on standard errors clustered by firm. Regressions recompute WFH corrective weights within departure-mode groups and absorb firm-event and event-year-mode fixed effects. Breadth excludes the departing legislator; breadth and dollar expenditure use $\log(1 + x)$. Any expenditure is an indicator for positive expenditure, so effects are probability changes. Table 4 reports pooled estimates and sample sizes.

Figure A2: Functional-form and repeated-treatment robustness



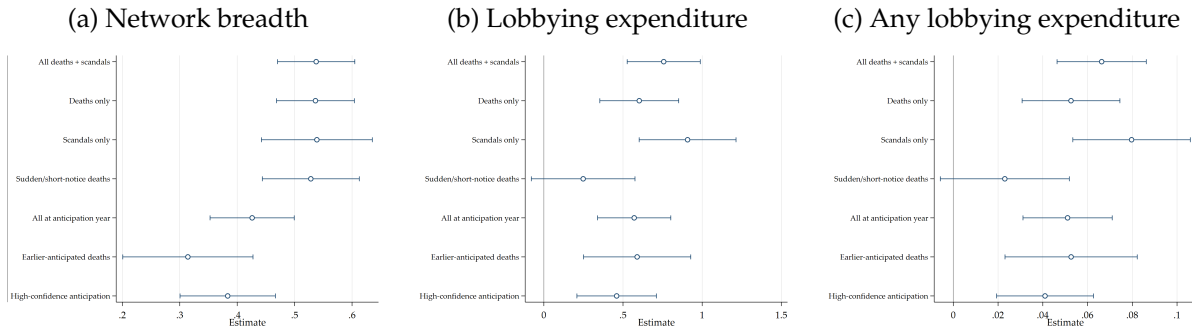
Notes: Coefficients are relative to $k = -1$ under the indicated robustness check. Solid blue-grey lines with circles denote formal exit; dashed mustard lines with squares denote the first public signal. Shaded bands are 95 percent confidence intervals based on standard errors clustered by firm. Regressions use WFH corrective weights and absorb firm–event and event–year fixed effects. Weights are recomputed for each estimation sample. Levels panels use legislator counts, dollar expenditure in thousands capped at the pooled formal-exit 99th percentile, and the any-expenditure indicator. At-most-once panels exclude firms treated in multiple stacks under the relevant clock and retain $\log(1 + x)$ for breadth and dollar expenditure. Breadth excludes the departing legislator; any-expenditure effects are probability changes. Table A2 reports pooled estimates.

Figure A3: Sensitivity of the main estimates to how the comparison group is built



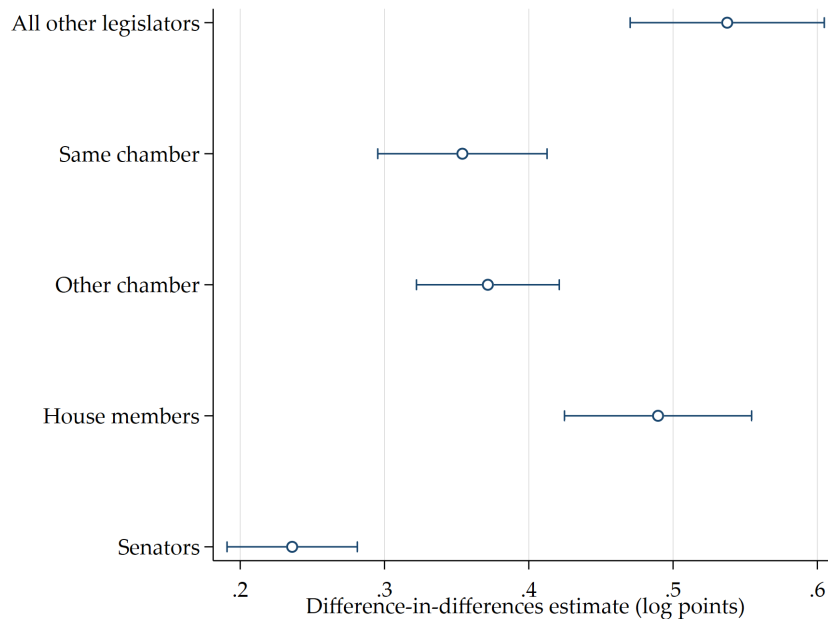
Notes: Dots report formal-exit estimates of β^{exit} from Equation (1); horizontal bars are firm-clustered 95 percent confidence intervals. Regressions use WFH corrective weights and firm–event and event–year fixed effects. The dashed line is the baseline rematch, which can differ slightly from the prepared sample because of tied matching distances. Row labels apply to all panels. The first block varies the number of placebo legislators and adds standardized log tenure to the placebo-matching distance at the indicated weight. The second varies the firm-matching caliper, uses Euclidean distance in the four pre-event covariates, or changes the number of comparison firms. Same-industry matching retains the baseline covariates, 0.30 caliper, and up to three comparison firms within broad NAICS sectors, combining 31–33, 44–45, and 48–49. Industry assignments are not year-specific and combine current and archived LobbyView records with documented web evidence. That sample contains 421 treated and 397 comparison firm–event units (5,726 observations). Specifications cover 29 departures over 2006–2021; breadth and dollar expenditure use $\log(1 + x)$, and any expenditure is an indicator.

Figure A4: Results under narrower event definitions



Notes: Dots report pooled estimates from Equation (1) for the indicated event definition; horizontal bars are firm-clustered 95 percent confidence intervals. Regressions use WFH corrective weights and absorb firm–event and event–year fixed effects. Weights are recomputed for each estimation sample. First-public-signal specifications reconstruct treatment, placebo legislators, matched comparison firms, and the seven-year window around the signal. Signals and exits in the same calendar year share a treatment year because outcomes are annual. Breadth and dollar expenditure use $\log(1 + x)$; any expenditure is an indicator for positive expenditure. Table A6 reports the estimates and sample sizes.

Figure A5: Changes in political connections across chambers



Notes: Dots report formal-exit estimates from Equation (1) for the indicated count of connected legislators, measured as $\log(1 + x)$. The departing legislator is excluded. Horizontal bars are firm-clustered 95 percent confidence intervals. Regressions use WFH corrective weights and absorb firm–event and event–year fixed effects. Same chamber and other chamber are defined relative to the departing member using the archived Bioguide last-chamber field. All specifications use 95,781 observations across 29 departures over 2006–2021. Table A7 reports the corresponding estimates.

C Control-Group Construction and WFH Estimation

This appendix details the matching procedure and the corrective weights used in the main estimates. We construct and estimate the formal-exit and first-public-signal designs separately. Throughout, T_e^d is the year of departure e under clock d , and $k = t - T_e^d$ indexes the seven-year window from -3 to 3 .

C.1 Constructing the Comparison Group

Treatment and the placebo-legislator pool. A firm is treated in a departure-clock stack if it cites at least one bill sponsored by the departing member during $k = -3, -2, -1$. To select comparison firms with a similar history of political connections, we first identify placebo legislators in the same chamber. Candidates must have pre-event connected-client data, have a recorded final term-end year at least as late as $T_e^d + 3$, and be absent from the project's inventory of deaths and resignations. The departing member is excluded. Thus, the operational service screen uses the Bioguide chamber and final term-end fields together with the departure inventory.

For each eligible legislator j , let $\bar{n}_j$ denote the mean annual number of connected firms in the available legislator-year records during the three pre-event years. We rank candidates by

$$D_{je} = |\log(1 + \bar{n}_j) - \log(1 + \bar{n}_e)|, \quad (C1)$$

and retain up to 40 with the smallest distances. This step matches legislators on the scale of their pre-event client networks; the baseline does not additionally match their party, state, or tenure.

Potential comparison firms are those linked to at least one selected placebo legislator during the same three pre-event years. We remove firms linked to the departing member and firms classified as treated for another eligible event under the same clock whose seven-year window overlaps the focal window. The latter exclusion applies when $|T_{e'}^d - T_e^d| \leq 6$. These restrictions make treatment and comparison status mutually exclusive within a stack. Before firm matching, the construction requires at least five treated firms and ten candidate comparison firms.

Firm-level matching standard. Let S_{ik} be firm i 's annual lobbying expenditure and B_{ik} its total number of distinct connected legislators in event year k . Within each departure-clock pool, we construct four pre-event covariates:

$$\begin{aligned} x_{i1} &= \log\left(1 + \frac{S_{i,-3} + S_{i,-2} + S_{i,-1}}{3}\right), \\ x_{i2} &= \log\left(1 + \frac{B_{i,-3} + B_{i,-2} + B_{i,-1}}{3}\right), \\ x_{i3} &= \log(1 + S_{i,-1}) - \log(1 + S_{i,-3}), \\ x_{i4} &= \log(1 + B_{i,-1}) - \log(1 + B_{i,-3}). \end{aligned} \quad (C2)$$

The level covariates take the log of one plus the three-year average, rather than averaging annual logs. The change covariates measure the difference between the last and first pre-event years. Matching uses total network breadth, including the departing member where linked; the main breadth outcome subsequently excludes that member throughout the event window.

We standardize each covariate using its mean and sample standard deviation across all treated and candidate comparison firms in the departure-clock pool before matching. Write the resulting covariates as $z_{ir} = (x_{ir} - \bar{x}_r)/s_r$; a zero or undefined standard deviation is replaced by one. For treated firm i and candidate comparison firm j , the matching distance is

$$\delta_{ij} = \left[\sum_{r=1}^4 (z_{ir} - z_{jr})^2 \right]^{1/2}. \quad (\text{C3})$$

Each treated firm retains up to its three nearest comparison firms satisfying $\delta_{ij} \leq 0.30$. The caliper applies to the joint Euclidean distance, not separately to each covariate or to a propensity score. A treated firm with no qualifying match is dropped. Comparison firms may be selected by multiple treated firms, but each selected comparison firm enters the final event stack only once. Consequently, baseline matching determines sample membership and does not assign reuse or inverse-distance weights. The caliper does not guarantee equality of the groups' aggregate means; Table 2 reports the remaining imbalance.

Balanced panels and the industry check. Each retained firm contributes all seven event years. In constructing the panel, years without a corresponding lobbying or network record receive zero expenditure and activity counts. We reconstruct treatment, placebo legislators, covariates, and matching separately under the first-signal clock rather than merely shifting the formal-exit panel. The resulting samples contain 7,748 treated and 5,935 comparison firm–event units across 29 formal exits, and 7,377 treated and 5,737 comparison units across 26 first-signal events.

Industry matching is a separate formal-exit robustness check, not a restriction on the main sample. It rematches baseline treated firms to candidates from the original pre-match comparison pool, requiring the same broad NAICS sector and retaining the four covariates, their original pool standardization, the 0.30 caliper, and up to three comparison firms. Sectors 31–33, 44–45, and 48–49 are grouped. Selected controls again enter once per event. This check retains 421 treated and 397 comparison units across 29 departures. Industry classifications are incomplete and not year-specific, so this smaller sample also reflects classification availability and within-sector common support. Its estimates and classification sources are reported in Figure A3.

C.2 Corrective Weights and the Main Estimand

Observation weights. We apply the corrective weighting of Wing et al. (2024) to the matched panels. For a given outcome and clock, let N_{1e}^d and N_{0e}^d be the numbers of retained treated and comparison firm–event units in stack e , and let $N_g^d = \sum_e N_{ge}^d$ for $g \in \{0, 1\}$. Each unit is counted once, not seven times. The weight on every annual observation of firm i in stack e is

$$q_{ied} = \begin{cases} 1, & \text{Treated}_{ied} = 1, \\ \frac{N_{1e}^d/N_1^d}{N_{0e}^d/N_0^d}, & \text{Treated}_{ied} = 0. \end{cases} \quad (\text{C4})$$

Thus, all treated observations receive unit weight, while comparison observations receive a constant weight within each stack. A stack containing a larger share of all treated units than of all comparison units has its comparison observations upweighted. The correction makes each stack’s share of total comparison weight equal its share of treated units: $q_{0ed}N_{0e}^d/N_0^d = N_{1e}^d/N_1^d$. It does not force the treated and comparison totals within each stack to be equal.

For the full formal-exit sample, the comparison weight is $(N_{1e}^{\text{exit}}/7,748)/(N_{0e}^{\text{exit}}/5,935)$; for the full first-signal sample it is $(N_{1e}^{\text{signal}}/7,377)/(N_{0e}^{\text{signal}}/5,737)$. We estimate Equations (1) and (2) by weighted least squares using these weights, firm-by-event fixed effects, and event-by-year fixed effects. Standard errors are clustered on the original firm identifier across years and event stacks.

Aggregation across departures and time. The equal-length, balanced main panels and the correction in Equation (C4) give departures weights proportional to their numbers of matched treated firms. To make this explicit, define $G_{ek}^d = \bar{Y}_{1ek}^d - \bar{Y}_{0ek}^d$ as the within-stack treated–comparison mean outcome gap in event year k , and $\pi_e^d = N_{1e}^d/N_1^d$. The main event-study coefficient can be written as

$$\hat{\beta}_k^d = \sum_e \pi_e^d (G_{ek}^d - G_{e,-1}^d), \quad k \in \{-3, -2, 0, 1, 2, 3\}. \quad (\text{C5})$$

The pooled coefficient instead compares the average of the four post-event years with the average of the three pre-event years:

$$\hat{\beta}^d = \sum_e \pi_e^d \left[\frac{1}{4} \sum_{k=0}^3 G_{ek}^d - \frac{1}{3} \sum_{k=-3}^{-1} G_{ek}^d \right]. \quad (\text{C6})$$

It therefore need not equal the average of the four post-event event-study coefficients, which use only $k = -1$ as their reference. Under the identifying assumptions in Section 4, the pooled specification targets an average post-event effect for matched treated firm–event units. Larger treated stacks contribute more; departures are not weighted equally. A firm exposed to several departures can contribute several treated units, so the target is not an equal-weight average over distinct organizations.

Recomputing weights for other specifications. Before calculating the weights, each regression identifies its nonmissing estimation sample after fixed-effect singleton removal, retains only units observed in every year of the requested event window, and excludes stacks without both treatment groups. We then recalculate the group counts and weights on that sample. In particular, subgroup, industry-matched, and leave-one-event-out estimates do not reuse the full-sample weights.

Where a specification supplies matching weights a_{ied} , as in the propensity-score check, the same calculation uses group masses $M_{ge}^d = \sum_{i:\text{Treated}_{ied}=g} a_{ied}$ in place of counts, and the final observation weight is $a_{ied}q_{ied}$. The baseline and industry-matched samples have $a_{ied} = 1$. A regression on a single event has corrective weight one for both groups. The equal-weight mean of separately estimated event effects in Table A2 is therefore a distinct event-level diagnostic. Corrective weighting aligns aggregation across stacks; it does not itself resolve differential trends, spillovers, or confounding from other departures.