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Abstract

Does higher office always lead to more favoritism? We argue that firms may lose their benefit from a connected politicians ascent to higher office, if it entails stricter scrutiny that may reduce favoritism. Around close Congress elections, we find RDD-based evidence of this adverse effect that a politicians win reduces his former classmates firms stock value by 3.2% after a week. Exploiting the entry of Craigslist across the U.S., we find that state-level scrutiny drives this effect. It further varies with politicians power, firm size and governance, and connection strength, and diminishes as a politicians career concern fades over time.

JEL-Codes: D720, D730, D850, G140, G320.

Keywords: favoritism, power, scrutiny, political connection, congressmen, close election, RDD.

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> *"Power tends to corrupt and absolute power corrupts absolutely."*
>
> —Lord Baron Acton (1887)

> *"Because power corrupts, society's demands for moral authority and character increase as the importance of the position increases."*
>
> —*Commonly attributed to* John Adams

1 Introduction

Discussions of politicians' favoritism usually evoke the widely shared view that politicians in higher office with more political power tend to give more favor to connected firms and interest groups. The age-old literature on distributive politics in the U.S. since Lasswell's (1936) *"Politics: Who Gets What, When, How"* has most often described more powerful U.S. congressmen, such as those holding more senior positions in powerful committees, as more likely to deliver funds and projects towards their constituencies and connected interests.[1] This view overlooks the possibility that, in response, existing institutions place stronger checks and scrutiny on more powerful positions, so that they cannot produce more favoritism. This aspect of institutional design has already figured among the chief concerns of the Founding Fathers of the United States, as highlighted in the epigraph. In this paper, we elaborate the role of scrutiny in its interplay with the power to give favor, and provide novel evidence from closed elections to the U.S. Congress that a politician's ascendance to Congress may even lead to lower, not higher favoritism towards his friends' firms.[2]

As Mayhew (1974) argued, scrutiny over Congress members matters most through their reelection concern. Therefore, it is important to consider their career dynamic, especially vis-à-vis their behaviors in office.[3] The politician faces the trade-off that giving more quid-pro-quo favor today may endanger his future career prospect. Rising to a position of higher power, but under tighter scrutiny, his decision to increase or decrease favoritism will thus depend on his concern for his future career and future capability to give out

[1] Examples abound in the literature of pork-barrel politics towards congressmen's constituencies, following Ferejohn's (1974) seminal work on the power of congressmen's membership and seniority in public works and appropriation committees, and also Ray (1981), Roberts (1990), Rundquist et al. (1996), Carsey and Rundquist (1999), Levitt and Poterba (1999), Rundquist and Carsey (2002), Cohen et al. (2011), DeBacker (2011), Fowler and Hall (2017), among others. In non-U.S. contexts, the literature on favoritism has demonstrated widespread evidence of favors from politicians promoted to more powerful positions across all forms of regimes, from Norway (Fiva and Halse, 2016) and Italy (Carozzi and Repetto, 2016) to China (Chu et al., 2021) and Vietnam (Do et al., 2017), among others.

[2] For convenience, as most Congress members are males, we address the politician by he/him/his.

[3] The literature on electoral control of politicians since Barro (1973) and Ferejohn (1986) has highlighted the key role of past behaviors as determinants of reelection. In particular, public media disclosure of politicians' malfeasance can weigh heavily on their chances of reelection, a fact that has not been neglected by those with strong career concerns (e.g., Ferraz and Finan, 2008, 2011, Larreguy et al., 2019).

favor, highlighted by Niehaus and Sukhtankar's (2013) as the "golden goose" effect on corruption.[4] Due to those dynamic concerns, the stream of favors can vary greatly along the politician's career by his positions' power and scrutiny.

We organize those intuitions into a minimal model of the politician's career dynamic that may oscillate between two levels of political offices, the higher of which enjoys more power to exert favoritism but faces stronger scrutiny. We focus on the difference in expected favoritism between the two offices, each understood as the present value of all future benefits for connected firms. This differential present value follows a simple, tractable recursive dynamic, from which we draw empirical implications on its sign and change in response to varying power, scrutiny, and career concerns. We highlight the case of the *adverse effect* of higher positions on favoritism for friends' firms, when a politician's promotion from low to high offices may *reduce* favoritism when scrutiny trumps power. This happens when the increase in scrutiny more than offsets the rise in power. Section 2 presents the model's setting and precise conditions, with further details in Appendix B.1.

In that case, a politician's career is composed of two stages: While in the later stage of his career a politician's higher position produces greater present value of favors for connected firms, in the earlier stage a higher position lowers the present value of favors. To put differently, the dampening effect of scrutiny on early-career favors more than compensates the positive effect of power on late-career favors, so that the net present value of the higher office is negative for connected firms.[5]

We examine those implications in the context of firms that are socially connected to candidates in U.S. Congress elections. Congress seats represent higher offices in the model, as opposed to positions in state-level politics.[6] We focus on classmate connections between politicians and corporate directors, following recent evidence of their importance in finance (Cohen et al., 2008), business (Lerner and Malmendier, 2013), and politics (Battaglini and Patacchini, 2018).[7] Data on corporate directors' educational backgrounds are

[4]In the context of India's largest rural welfare program, Niehaus and Sukhtankar (2013) exploits an exogenous increase in illicit rents that corrupt officials can appropriate, and estimates that the concern for future illicit rents, dubbed the "golden goose effect", reduces their theft by 64 percent.

[5]This is not inconsistent with the politician's willingness to win elections and ascend to more powerful offices (e.g., Groseclose and Stewart, 1998, Stewart and Groseclose, 1999). His net present value of higher office can still be positive, as he attributes an intrinsic value to the higher office.

[6]As studied in a long tradition in political science (Polsby and Schickler, 2002) and economics (Diermeier et al., 2005), U.S. Congressmen wield large political power and influence on economic activities, especially in their home state. Their power likely strengthens with their seniority and memberships in key committees (Groseclose and Stewart, 1998, Stewart and Groseclose, 1999). Notably, Roberts (1990) documents that, following the sudden death of Senator Henry Jackson, the ranking Democrat on the Armed Services Committee, the market value of defense contractors from his home state of Washington declined, while that of contractors from Georgia, home to the next-most-senior Senator on the same committee, increased. Section 6.1 will also show evidence that congressmen become more scrutinized in the media.

[7]Our link definition based on Cohen et al. (2008) restricts on the same university and within a year apart. Alumni networks are highly useful for favor exchange, as they reinforce mutual trust and guarantee reciprocal behaviors on network (Leider et al., 2009) thanks to their high network closure (Karlan et al., 2009). Unlike links based on political campaign contributions, alumni-based connections predate the studied period for decades, hence are not endogenous to a firm's immediate decisions. See Marsden (1990), Ioannides and Loury (2004), and Allen and Babus (2009) for reviews of social networks measurement.

gathered from BoardEx, and those regarding politicians are manually collected from archives of campaign websites and Lexis-Nexis biographies (section 4). The net value of a connected firm's present and future benefits from favoritism is reflected in its cumulative abnormal stock returns (CARs) around the election, which is used as the main outcome in our empirical analysis.

As abnormal daily returns may still reflect other sources of variation,[8] we seek to best identify the differential effect between the politicians' higher and lower offices by focusing on the Regression Discontinuity Design (RDD) of close elections, in which electoral victory and defeat are almost as random as a coin toss (Lee, 2008, Lee and Lemieux, 2010, de la Cuesta and Imai, 2016) (section 3).[9] That is, we compare the CARs of firms connected to elected candidates with those of firms connected to defeated ones in a cross-sectional identification that eliminates all potential differences along observable and unobservable characteristics between the two types of firms (Lee and Lemieux, 2010). The RDD estimates a Weighted Average Treatment Effect corresponding to the model's key differential favoritism effect between higher and lower offices.

We find robust evidence of the adverse effect of higher positions on favoritism towards friends' firms, as firms connected to elected congressmen lose in stock value in comparison with those connected to defeated candidates. The net effect ranges from 1.9% after one day (95% confidence interval of [0.7%,3.1%]) to 3.2% after one week (95% confidence interval of [1.6%,4.8%]). After one week, it is rather evenly distributed between winner-connected firms (a loss of 1.3%) and defeat-connected firms (a gain of 1.9%) (section 5).[10] For the median firm's market value in our sample ($656 million), the one-day and one-week net effects amount to $12 million and $21 million, respectively.[11]

In accordance with the model, the estimated effect is mostly due to challengers from state politics running for Congress, and not incumbents. It shows that on average firms benefit more when their connected politicians in state politics are defeated and remain entrenched in state politics, rather than get elected to Congress. This finding is reminiscent of the literature on rampant corruption across U.S. states (Glaeser and Saks, 2006, Campante and Do, 2014), with state officials wielding strong power and relatively weak checks and balances (Kousser and Phillips, 2012). In support of this paper's message, our companion study (Do et al., 2021) shows evidence that closely elected state governors add as much as 4.1% to the market value of

[8]Event studies of connections exploit identification strategies on the time dimension (e.g., Roberts, 1990, Fisman, 2001). Those daily events and daily measures of stock returns are still subject to (i) the prior probability that an event would happen, and (ii) potentially confounding news and reactions around election day. Unfortunately, prediction markets (Snowberg et al., 2007) were not available for the vast majority of the considered elections.

[9]In recent U.S. politics, many prominent politicians still face tough electoral battles. Our close-election sample includes powerful figures such as former Attorney General John Ashcroft and former Vice President Walter Mondale.

[10]Those effect sizes fall between Faccio's (2006) estimate of 1.4% in CARs following new political connections and Goldman et al.'s (2009) 9.0% in difference between Republican- and Democrat-connected firms after the 2000 presidential election.

[11]Subsection 6.3 shows the prevalence of the effect among smaller firms, as it vanishes around the sample-mean market value of $6,367 million, and becomes positive and significant for the largest firms. On average, firms in our sample are connected to 1.1 politicians in close elections on average. Section 3 discusses the generalizability of our RDD estimates to other politicians.

their former classmates' firms.

We find further evidence that supports the model's additional predictions, especially one on the role of scrutiny that the adverse effect of Congress connection is more pronounced in states with weaker local checks and balances (i.e., where there is a larger gap between state and federal scrutiny). First, we implement a Difference in Discontinuity specification using Craigslist's staggered entry across counties (Gao et al., 2020, Djourelova et al., 2023) as an exogenous event that weakens local newspapers, hence reduces local media scrutiny. Consistent with the prediction, we find that its entry implies a stronger adverse effect of Congress connection. Second, we consider isolated capital cities as a deep-rooted predictor of weak state governance, as shown by Campante and Do (2014), and find supportive evidence of the same prediction. Third, we find that this pattern also arises with other proxies for the scrutiny gap between state and federal politics, including voters' interest in politics, exposure to the media, and state's corruption (section 6.1).

Beyond the role of scrutiny, the model's prediction on politicians' career concerns is also supported in the data, in that the effect is mostly pronounced for the earlier part of their career, and subsequently fades away (section 6.2). We further find evidence that the adverse effect varies as predicted according to (i) proxies for politicians' power to give favor, (ii) firms' attributes, such as firm size and location, that may affect their benefits, and (iii) the strength and quality of connections (section 6.3).

In addition to those results on the mechanism, we also discuss evidence showing how the measurement of connections among classmates matters to the estimated adverse effect of Congress connection. We further show evidence of abnormally high trading activities on connected stocks around relevant elections, and address two alternative interpretations of the mechanism at work based on same-school homophily and on Shleifer and Vishny's (1994) and Bertrand et al.'s (2018) negative effect of political connections due to pressure to increase employment (section 7).

This paper's results can be best seen in comparison with the common monotonic finding that politicians' rise on the power ladder unfailingly increases favoritism, which has been a constant, long-standing feature in distributive politics, as recently reviewed in Golden and Min (2013). Related evidence in the U.S. comes from, e.g., surprising events regarding specific politicians in Roberts (1990), Jayachandran (2006), Fisman et al. (2012), and Acemoglu et al. (2016). Close presidential elections in the U.S. (Knight, 2007, Goldman et al., 2009, 2013, Mattozzi, 2008) also unveil the pattern of benefits to firms connected to the winning party. Relatedly, the literature has considered connections based on campaign contributions in corporate-sponsored Political Action Committees (PACs) in support of specific politicians (Cooper et al., 2010, Akey, 2015, Fowler et al., 2020),[12] and connections between top politicians and lobbying firms (Bertrand et al.,

[12]While earlier papers find a positive relationship between positions in Congress and contributors' stock values, the latest, most thorough exercise by Fowler et al. (2020) concludes that the average effect is very close to zero. It reaffirms Ansolabehere

2014, Blanes i Vidal et al., 2012). Beyond the U.S., from both cross-country and country-specific case studies, most evidence also points to the monotonic relationship between more powerful political positions and more favors targeted towards connected groups.[13]

Beyond such monotonic relationship, this paper introduces a novel, more nuanced pattern of favoritism's dependence on the interplay between political power and institutional scrutiny. Our empirical setting helps correctly identify the change of firm's value from favoritism associated with a politician's different positions. The evidence points to the key role of institutional checks and balances in curbing favoritism, and opens the natural question of how to design the optimal structure of the system of scrutiny and monitoring policies across different layers of government.

Besides this paper, we are aware of only two studies that have defied this common view of more favor from higher office. First, Bertrand et al. (2018) shows Shleifer and Vishny's (1994) mechanism in which connected politicians pressure French companies to hire more before their elections. Section 7 provides evidence that this important mechanism is not at work in our case. Second, Fisman et al. (2012) reports that stocks connected to Vice President Dick Cheney are not affected either by news related to his health and political future in two special events or by the probabilities of Bush's victory or the Iraq war. While such finding is explained as evidence of the strength of U.S. institutions, the paper stops short of showing how.

The importance of institutional checks and balances to reduce favoritism towards socially connected recipients may extend beyond the democratic setting studied in this paper, towards contexts of nondemocratic strong states that can impose disciplinary principles to curb favoritism. Fisman et al. (2020) demonstrates systematic evidence of disadvantage of high-ranked politicians that are more connected to top leaders in Chinese politics, consistent with the Chinese Communist Party's long-standing principle of anti-factionalism.

et al.'s (2003) prevalent view in political science that corporate campaign contribution is tightly restricted and could hardly promote firms' interests (at least before the U.S. Supreme Court's decision on *Citizens United* in 2010). The use of campaign contributions to measure connections between politicians and firms is the fundamental difference with our empirical exercise's reliance on alumni network links, which cannot be affected by firms' short-term decisions.

[13]Cross-country evidence includes Faccio's (2006) and Faccio et al.'s (2006) findings from connections between firms and politicians based on family ties, prior employment, or ownership, and Hodler and Raschky's (2014) results with country leaders' region of birth. While Burgess et al. (2015) found evidence of favoritism in Kenya towards the president's ethnic group only under autocracy, elsewhere similar evidence is established in both democracies such as Norway (Fiva and Halse, 2016), Sweden (Amore and Bennedsen, 2013), France (Coulomb and Sangnier, 2014), Germany (Baskaran and Lopes da Fonseca, 2017), Italy (Carozzi and Repetto, 2016), Spain (Curto-Grau et al., 2018) as well as countries with weaker institutions such as Indonesia (Fisman, 2001), Malaysia (Johnson and Mitton, 2003), Pakistan (Khwaja and Mian, 2005), Brazil (Claessens et al., 2008, Colonnelli et al., 2020), Ecuardor (Brassiolo et al., 2020), Thailand (Bunkanwanicha and Wiwattanakantang, 2009), Taiwan (Imai and Shelton, 2011), China (Fan et al., 2007, Chu et al., 2021, Kung and Zhou, 2021) and Vietnam (Do et al., 2017).

2 Theoretical intuitions and testable predictions

This section presents the intuitions that illustrate the trade-off between favoritism benefits and career concerns in a setting when both power to give favors and scrutiny over favoritism matter, and connect parameters that determine favoritism to testable implications in our RDD framework. Those intuitions are formally derived in Appendix B.1. While this is certainly not the only way to model such trade-off, we find it useful to provide this structure to our subsequent empirical analysis by different determinants of both power and scrutiny.

We consider the politician's career dynamic between two stylized types of political positions, such as Congress seats versus positions in state-level politics, which differ in both the power to favor connected firms and the extent of institutional scrutiny over favoritism.[14] In each position s and at time t, a connected firm expects a value $V_{s,t}$ from present and future favors, and the politician's expected present value is $W_{s,t}$. We define the firm's and politician's differences in values across those positions, and focus empirically on the former, as it naturally maps to observed changes in the firm's stock value:

Definition 1 *The firm's differential value $\Delta V_t \overset{def}{\equiv} V_{2,t} - V_{1,t}$ is the difference of its values from its connection to the politician's higher position ($s = 2$) versus the lower position ($s = 1$). Analogously, $\Delta W_t \overset{def}{\equiv} W_{2,t} - W_{1,t}$ is the politician's differential value.*

The politician's choice of favoritism towards the firm along his career faces a major trade-off between increasing his own immediate benefits from such action and further jeopardizing the next election to attain or keep a Congress seat, the importance of which depends on future benefits from the office. The problem can be reduced to a recursive dynamics of ΔV_t and ΔW_t, and admits a unique equilibrium (Appendix Proposition B.1).

We further parametrize each position's power to give favor (marginal benefit of favor) by $\beta_2 \geq \beta_1 > 0$, and the corresponding degree of scrutiny (marginal cost of favor on election success probability) by $\gamma_2 \geq \gamma_1 > 0$. The relative power is $\beta \overset{def}{\equiv} \frac{\beta_2}{\beta_1} \geq 1$ and the relative scrutiny $\gamma \overset{def}{\equiv} \frac{\gamma_2}{\gamma_1} \geq 1$. We obtain the following testable prediction on the firm's differential value of favoritism:

Proposition 1 *(i) If power trumps scrutiny, in that $\beta \geq \gamma$, then the connected firm enjoys higher present value when the politician attains higher office: $\Delta V_t^* \geq 0$ $\forall t$.*

(ii) If scrutiny trumps power, in that $\beta < \gamma$, over a long enough career, there exists a time $\bar{t}$ before which

[14] Our dynamic modeling of a politician's career concern under the risk of exit follows Barro's (1973) and Becker and Stigler's (1974) tradition, and more recently in Campante et al. (2009), Niehaus and Sukhtankar (2013), with a reduced-form negative relationship between favoritism and electoral success.

there is an adverse effect of higher position on the present value of favoritism: $\Delta V_t^* < 0 \ \forall t < \bar{t}$. *After* $\bar{t}$, ΔV_t^* *is positive and increasing in* t.

In the second case, over the politician's career ΔV_t^* follows a loosely upward longterm trend. It becomes positive and increasing in late career when electoral concerns subside, but at an early stage the strong electoral incentives induce the politician to reduce favoritism when he attains a higher position. This pattern is akin to Olson's (1993) famous "roving bandit" intuition, as a shorter horizon reduces electoral control on the politician. We will show robust evidence of the adverse effect of higher position in section 5, and illustrate this career-long trend in section 6.2.

We further derive comparative statics with respect to power and scrutiny, to be tested in corresponding comparative situations in sections 6.1 and 6.3:

Proposition 2 *When scrutiny trumps power, in presence of the adverse effect of higher position* ($\Delta V_t < 0$)*, its magnitude* $|\Delta V_t|$ *increases when:*

- β_2 *decreases and/or* β_1 *increases,*
- *both increase while their ratio* β *remains the same,*
- γ_2 *increases and/or* γ_1 *decreases,*
- *both decrease while their ratio* γ *remains the same.*

Appendix B.1 provides the proofs of Propositions 1 and 2.

3 Empirical methodology

3.1 Identification of the differential value of political connections

We bring section 2's predictions about the differential value of political connections, ΔV, to an empirical setting surrounding elections to the U.S. Congress. Those important events shape politicians' career prospects that can be broadly mapped to the high and low positions described in the theory. As the net present value V of a firm's connection to a politician is priced into its stock price, short-term changes in the stock price correspond to changes in V. It follows naturally that we can use event-study methods to associate electoral results with the changes in V over time.

Time-series identification and CARs. To implement this approach, we obtain daily stock data from the Center for Research in Security Prices (CRSP), and compute the Cumulated Abnormal Returns (CARs) on a firm's stock around the election day. We follow conventional event study methods (Campbell et al., 1997, c. 4) to calculate abnormal returns in a single-factor market model estimated from the pre-event window from day -315 to day -61, counting from the election day (always a trading day). CARs are summed from abnormal returns over the 7-day window from day -1 to day 5 (other pre- and post-election event windows are also considered in placebo and robustness checks).[15] They reflect the stock market's expectation of changes to a firm's value, which maps directly to changes in V, assuming no other event takes place at the same time.

Cross-sectional identification with RDD. The time-series identification still faces three key empirical challenges. First, a politician's electoral success can be endogenous, so that the estimated effect could reflect (i) a reverse causation channel from the firm's performance to the politician's victory or defeat, or (ii) an omitted variable bias when connected firms and politicians are affected by the same unobservable factor, such as a shift in public opinion. Second, as election days are determined and known in advance, there can be other concurrent events that confound the estimates of abnormal returns. Third, time variations in stock prices depend crucially on the market's prediction of event probability, which is not independently observable for lack of a prediction market on individual Congress elections (see discussions in Fisman, 2001, Snowberg et al., 2011). In particular, if the distribution of investors' beliefs of the probability of a politician's winning chance is biased, market reactions to electoral results will carry such biases, making it impossible to identify the true effect on changes in V.[16]

We thus combine the usage of CARs with a cross-sectional identification based on the Regression Discontinuity Design (RDD) of close elections (Hahn et al., 2001, Lee and Lemieux, 2010, de la Cuesta and Imai, 2016). As the vote shares between the top two candidates in each election tend to the threshold of 50%, the electoral outcome of a win or a loss approaches a random draw between the two. At this threshold, in expectation the distributions of any characteristics, observable or unobservable, are identical between winners and losers. Their comparison thus estimates the differential value of connection to a politician in high versus low positions, conditional on the vote shares being fixed at 50%. Thanks to the equivalence to a random draw, this RDD strategy is immune to the three aforementioned problems of event-study methods.[17]

Because of the almost-random properties of RDD, we expect that the inclusion of predetermined covari-

[15] Our results are not sensitive to the method of estimation of abnormal returns, such as using multiple factor models by Fama and French (1993) and Carhart (1997) (Appendix Table A5). Appendix B.2 summarizes the calculation of CARs, and argues that the quasi-random nature of RDD necessarily implies the estimate's robustness.

[16] To illustrate this point, suppose that the market value of connection to a candidate is \$100 in case he wins, and zero otherwise. Prior to the election, if the market believes he already has a winning probability of 65%, pre-election connection is already priced by the market at \$65. An event study of election wins would report the post-event market reaction to a realized win of only \$100-\$65=\$35.

[17] The key RDD assumption in close elections is that of imprecise control, i.e., both sides of an election cannot manipulate with precision the result of the election (Lee, 2008, Lee and Lemieux, 2010). While its realistic nature has been debated (Caughey and Sekhon, 2011), de la Cuesta and Imai (2016) summarizes arguments and evidence in favor of its validity (e.g., support of balanced attributes at the threshold by Eggers et al., 2015).

ates does not matter to the main estimate (Lee and Lemieux, 2010). Since we could consider short-term CARs as functions of pre-event data on each stock and the short-term reactions to election results, RDD's property means that cross-sectional estimates using CARs would differ little from those using raw returns. Hence the backbone of our empirical strategy is a cross-sectional identification by RDD, while the usage of CARs just helps to reduce noise and improve precision.

Regarding external validity, Lee and Lemieux (2010) interprets the RDD estimand as a Weighted Average Treatment Effect (WATE) of being connected to a winner, in which each candidate is weighted by his ex ante likelihood to be in a close election. This likelihood is nontrivial for most candidates, as our sample includes prominent figures such as John Ashcroft, Walter Mondale, and Ted Stevens.[18]

3.2 Implementation of RDD

In practice, to estimate the discontinuity effect at exactly the threshold of 50%, RDD specifications use data points within a distance from this threshold, while accounting for separate functions of the vote shares on both sides of the threshold. We follow Lee and Lemieux's (2010) standard procedure for our main specification to estimate the differential value of Congress connection to firms:

$$CAR_{idt} = \beta Winner_{pt} + \delta_W VS_{pt}\mathbb{1}_{\{VS_{pt}\geq 50\%\}} + \delta_L VS_{pt}\mathbb{1}_{\{VS_{pt}<50\%\}} + \varepsilon_{idpt}. \quad (1)$$

Each observation is a combination of politician p, director d, firm i, and election year t such that (i) politician p is a top-two candidate in a close election in year t (i.e., within 5% of vote margin), (ii) director d is on the board of firm i in year t, and (iii) politician p and director d are connected as former classmates in the same university degree program (details in subsection 4.2). It thus represents a connection between a close-election top-two candidate and a connected firm's director (through a specific university program) for a given election year.[19]

CAR_{idt} is the firm's CAR, cumulated from day -1 to day 5 around the connected politician's election in our benchmark regression. This measure covers a week following the election day to fully capture reactions to uncertainties surrounding the result of a close election. The running variable VS_{pt} is politician p's vote share in election year t between the top two candidates. $Winner_{pt}$ is an indicator equal to one if politician p

[18] John Ashcroft was U.S. Attorney General (2001-2005) after he lost in Missouri's 2000 close Senate election. Walter Mondale was U.S. Vice President (1977-1981), the Democratic Presidential Candidate in 1984, and narrowly lost in Minnesota's 2002 Senate race. Ted Stevens was an influential Senator from Alaska (1968-2009), and the longest-serving Republican U.S. Senator when he left office. He faced one of the biggest political corruption cases in recent U.S. history, in which he was first convicted before the case was abandoned.

[19] Essentially, this baseline sample construction weighs politician-firm connections by the number of directors facilitating the respective connections. Using alternative sample construction at politician by firm level yields quantitatively similar results (Appendix Table A5).

wins in election year t, namely if $VS_{pt} > 50\%$, and zero otherwise. Controls include a first order polynomial of VS_{pt}, separately for winning and defeated candidates.

This strategy estimates the causal effect of having a connected politician in Congress versus out of Congress on the firm's value, which corresponds exactly to the differential value of Congress connection ΔV as discussed in the model.

Specification choices. The RDD specification in (1) employs a bandwidth of 5% of vote share, a rectangular kernel, and linear controls of the running variable. We make sure the results are robust to a broad range of bandwidth choices, from 1% to 10% vote shares (Appendix Figure A1). We also further perform Calonico et al.'s (2014) procedure of RDD bandwidth selection and adjustment,[20] control for various higher order polynomials of vote shares,[21] and examine alternative kernel functions.

Statistical inference. In our benchmark specifications, we estimate standard errors with correction for clustering by politician.This choice of clustering correction stems from the recent development of Abadie et al.'s (2020, 2023) design-based approach to statistical inference in causal empirical analysis of a finite but potentially large population. Abadie et al.'s novel framework takes into account both the traditional sampling-based uncertainty and the proposed design-based uncertainty that arises from the standard potential outcome framework in causal analysis. It focuses on finite population that could be substantially sampled in the data, instead of the traditional asymptotic approach of infinite superpopulations/data-generating processes. Based on Abadie et al.'s (2023) simulation results and recommendation, cluster correction is made at the level of the assignment's variation, which is by politician in our context. Abadie et al. (2023) shows that *"[...] the presence of cluster-level unobserved components of the outcome variable becomes irrelevant for the choice of clustering level"*, and coarser clustering likely results in statistical inference that is unnecessarily too conservative. Appendix B.4.1 provides a detailed discussion of this point.

Even based on the traditional framework with clustered standard errors, Appendix Table A5 shows that strong statistical significance remains under alternative clustering correction schemes, including clustering by firm or two-way clustering by politician and firm (Cameron et al., 2011). Appendix B.4.2 further discusses in detail the most difficult case of the coarsest level of clustering, that by the 5 elections in our sample. In this case, there remain two appropriate inference methods, namely the clustered wild bootstrap, as suggested by Cameron et al.'s (2008) simulation results and proven by Canay et al. (2021), and the more versatile

[20] Calonico et al.'s (2014) procedure may lead to drastically different split sample sizes across the many empirical exercises performed on split samples in the paper. Therefore, we keep the benchmark as Lee and Lemieux's (2010) standard procedure.

[21] Controlling for higher order (second to fifth) polynomials of vote shares yields qualitatively similar results, with higher order coefficients not statistically different from zero. We thus follow Gelman and Imbens's (2019) recommendation against using higher order polynomials of the running variable when higher order coefficients are not statistically significant.

Approximate Randomization Test by Canay et al. (2017) . They both show that our estimates attain the highest possible level of statistical significance for 5 clusters (*p-value* of $1/2^5 = 0.03125$).

In addition to those exercises to address clustering concerns, we further examine statistical inference with hypothetical placebo thresholds, with t-stats shown in Appendix Figures A6 and A7) and hypothetical placebo event days, with t-stats shown in Appendix Figures A8 and A9). Again, the results show particularly strong statistical significance of the paper's main results (detailed discussion in Appendices B.4.3 and B.4.4).

Test of RDD's internal validity. The RDD identification assumption implies that the distribution of any predetermined variable is smooth around the threshold. This implication can be tested on observables, using the same RDD specification as in equation (1) with each predetermined observable on the left hand side (Lee and Lemieux, 2010). Appendix Table A4 reports this test on a wide range of predetermined politician, director, firm, and state characteristics at the 50% vote share threshold. Among the 51 variables considered, only four discontinuities are statistically significant at 10%, no more frequent than what would occur by chance. We thus find no evidence against the RDD's internal validity in our setting.

Measure of connection. Following Cohen et al. (2008) and the subsequent literature, we focus on politician-director connections through their university alumni networks. A firm is defined as connected to a politician in an election year if at least one of its directors and the politician both graduated from the same university program within one year of each other.

It is commonly seen that networks among alumni from the same educational institution play an important role in fostering connections and cooperations. For example, in the U.S., gifts towards those institutions, largely through their alumni's links, amount to 15% of 390 billion of all charitable donations (Giving USA, 2017). Evidence abounds that this type of networks connects businessmen and firms, and influences their decisions (e.g., Cohen et al. 2008, Shue 2013, Fracassi 2017). Notably, in case of mergers and acquisitions, Ishii and Xuan (2014) shows that the stock market pays attention to directors' education connections between the acquirer and the target. On the other side, Battaglini and Patacchini (2018), Battaglini et al. (2020, 2023) show that the alumni networks of congressmen are crucial in shaping congressmen's cosponsorship, financial resources, legislative effectiveness, and abstention in Congress.

In the particular case of quid-pro-quo favoritism, alumni networks can be highly useful in enforcing cooperative behaviors and strengthening mutual trust under the threat of social punishment and ostracization from the network, when no legal recourse is possible. Karlan et al. (2009) predicts that favor exchange is facilitated by high *network closure*, which is likely the case of alumni networks.

Classmate links may be an imperfect measure of real friendship, which implies an attenuation bias that reduces the magnitude of the main estimate. Indeed, we do find that the magnitude of our key estimate decreases when we relax the link restriction on the same program or the graduation year (subsection 7.1). From a different angle, classmate links can be essential in the development of relationships after graduation by providing mutual trust, common ground in communication, and common access to the same social network. Such background may help former classmates later develop a strong connection, even if they were not close friends while at school.

Homophily and shared preferences. The RDD framework allows us to identify the links between firms and elected congressmen as an almost-random treatment. However, the full networks of classmates and alumni, including firms' links to both elected congressmen and defeated candidates, are still considered as exogenous. Hence, while our empirical design rules out direct reverse causality, the mechanism at work may still be due to homophily (McPherson et al., 2001), whereby unobserved shared characteristics influence same school attendance by politicians and businessmen, as well as their future outcomes. For example, a politician and a director may be both interested in military studies, and decided to join a university that specializes in military studies; years later, the election of the former has the potential to affect the latter's firm value through new defense policies, without passing through the social network. While the RDD still correctly identifies the effect of political connection defined by former classmate links, it is harder to claim that the effect comes directly from the social network links.

We propose to disentangle the homophily mechanism by using alumni links, as homophily should matter similarly between alumni links and classmate links. This approach also addresses the mechanism of shared preferences, whereby politicians and businessmen from the same university tend to align their preferences (Algan et al., 2023). The corresponding results in subsection 7.2 show that the mechanisms by homophily and by shared preferences cannot account for the estimated adverse effect of higher office.

4 Data description

4.1 Data sources and construction

Close elections. We obtain Congress election results from the Federal Election Commission (FEC) website. We calculate the margin of votes between the top two candidates in each election, and focus on the sample in which this margin is below 5% (i.e., when the vote shares between the top two candidates are between 47.5% and 52.5%.) The sample covers 126 out of 128 close elections during the period between 2000 and 2008.[22] Sensitivity checks using alternative sample restrictions ranging from 1% to 10% vote margin, as well as those suggested by Calonico et al.'s (2014) procedure, produce highly similar results.

Politicians. We construct a unique dataset of the education and career of top two candidates in the considered close elections through a long process of hand-collecting their biographical records from Lexis-Nexis, which contain active and inactive biographies in Who's Who publications. Our scope of search includes (i) Who's Who in American Politics, (ii) Member Biographical Profiles – Current Congress, (iii) World Almanac of U.S. Politics, and (iv) The Almanac of American Politics. Each candidate's biography includes the candidate's employment history, all undergraduate and graduate degrees attained, years of graduation, and the awarding institutions. For biographies unavailable in Who's Who, especially for defeated candidates, we search the Library of Congress Web Archives which cover multiple versions of Congress election candidates' websites archived at different moments during the electoral campaign. This comprehensive process allows us to collect sufficient data for 92% of the politicians on our search list.

Directors. We obtain biographical information and past education history for directors and senior company officers from BoardEx. The data include board directors and senior company officers in active and inactive firms from 2000 onwards, and comprehensive information on their employment history, educational background (including degrees attained, graduation years, and awarding institutions), remuneration, and participation in social and charity organizations. There are 55,353 board directors in 6,771 U.S. publicly listed firms covered in BoardEx between 2000 and 2008.

Firm and stock data. We match our data with stock data from the Center for Research in Security Prices (CRSP), and obtain information on firm characteristics and financial performance from Compustat. Section 3 describes the calculation of our main outcome of interest, the CAR around election events, which maps directly to changes in the firm's value of connection.

4.2 Baseline sample

Our final baseline sample includes 1,714 observations at the politician-by-director-by-firm-by-election year level, covering 123 close elections, 165 politicians, 1,136 directors, and 1,234 firms between 2000 and 2008 (Table 1). These 123 close elections cover a total of 40 U.S. states and have an average win/loss margin of 2.53%. Among them, there are 23 Senate elections, 100 House elections, and 63 elections for which both top two candidates are included in the baseline sample.

[22] We avoid the period after the Supreme Court's decision in *Citizens United vs. FEC*, which changed fundamentally the way firms could contribute to electoral campaigns.

Table 1: Baseline Sample's Descriptive Statistics

Election year	2000	2002	2004	2006	2008	2000-2008
No. of close elections	24	23	12	36	28	123
% of close elections	85.7%	88.5%	75.0%	92.3%	93.3%	88.5%
% of all congressional elections	5.1%	4.9%	2.6%	7.7%	6.0%	5.3%
No. of Senate elections	8	4	5	3	3	23
No. of House elections	16	19	7	33	25	100
No. of states covered	17	17	12	25	20	40
Avg. win/loss margin	2.48%	2.73%	3.74%	1.93%	2.79%	2.53%
No. of politicians	38	32	19	56	41	165
% of all election candidates	1.6%	1.5%	0.9%	2.6%	1.9%	2.1%
No. of winning candidates	18	17	11	33	21	100
No. of defeated candidates	20	15	8	23	20	86
Avg. no. of connected directors	7.58	5.94	7.11	7.59	7.27	7.18
Avg. no. of connected firms	9.26	6.94	9.26	10.09	9.07	9.07
No. of connected directors	235	190	135	415	294	1,136
Avg. no of connected politicians	1.23	1.00	1.00	1.02	1.01	1.05
Avg. firms per director	1.23	1.21	1.33	1.33	1.27	1.28
No. of connected firms	275	216	173	510	353	1,234
% of all listed firms	3.8%	3.4%	2.9%	8.6%	6.2%	12.5%
% of total market value	8.9%	4.7%	6.5%	18.2%	6.8%	9.0%
Avg. board size	14.8	14.8	12.3	12.3	11.8	12.7
Avg. no. of connected directors	1.05	1.06	1.03	1.08	1.05	1.06
Avg. no. of connected politicians	1.28	1.03	1.02	1.11	1.05	1.10
No. of academic institutions	39	32	20	57	43	121
No. of politician × director × firm × election year observations	357	229	179	572	377	1,714

Notes: This table reports the descriptive statistics of the baseline sample of 1,714 observations at the politician-by-director-by-firm-by-election year level. Close elections are those with a less-than-5% margin of votes between the top two candidates. Politicians and directors are considered connected if they were enrolled in the same university, campus, and degree program combination within one year of each other.

The 165 politicians record 100 wins and 86 defeats (19 of them experience multiple close elections). They are connected to 1,136 directors in 1,234 firms through 121 academic institutions. On average, each politician is connected to 7.2 directors and 9.1 firms in a close-election year. Undergraduate study is the most prevalent type of connection between directors and politicians: 72.3% of politicians and 87.4% of directors are connected through their undergraduate studies, having graduated from the same school in the same university within one year of each other (Appendix Table A2). The next most common types of connection are law and business school programs.

On average, each firm in our sample is connected to 1.1 close-election politicians through 1.1 directors in an election year. These firms cover a wide range of geographies and industries, with headquarters in 49 U.S. states and operations in 67 SIC 2-digit industries. They are on average larger than firms in the Compustat universe (Appendix Table A3).

5 The adverse effect of Congress-level connection on favoritism

To evaluate Proposition 1's theoretical prediction of a possible adverse effect of a politician's promotion on connected firms' value, we first estimate the key quantity $\Delta V = V_2 - V_1$, the average differential value to firms when their connected politicians win versus lose a seat in Congress. Table 2 relates stock price cumulated abnormal returns (CAR) of connected firms around the election day (from day -1 to day 5) to the connected politician's election result using the baseline RDD specification in equation (1) on the full sample of all firms connected to all top-2 politicians in close Congress elections from 2000 to 2008.

Table 2: Added Value of Congress-Level Connection to Firms Using RDD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable: CAR(-1, 5)							
Specification	Benchmark	Third-order	CCT	Additional controls			Winner/loser subsamples	
Winner	-0.032***	-0.034***	-0.028***	-0.030***	-0.036***	-0.030***		
	(0.008)	(0.012)	(0.011)	(0.009)	(0.012)	(0.011)		
Average CAR(-1,5)							-0.013**	0.019***
							(0.006)	(0.005)
Politician sample							Winners	Losers
3rd order polynomials		X						
Politician controls				X				
Director controls					X			
Firm controls						X		
Election year FEs				X				
University FEs					X			
Industry FEs						X		
Observations	1,714	1,714	559	1,714	1,714	1,468	943	771
Politicians	165	165	66	165	165	158	93	84
Directors	1,136	1,136	415	1,136	1,136	1,004	677	566
Firms	1,234	1,234	481	1,234	1,234	1,063	783	669

Notes: This panel reports the benchmark average differential value of Congress-level connection to firms ΔV using the baseline RDD specification in equation (1) (column 1). Column (2) additionally controls for a third order polynomial of vote shares (separately for winners and losers). Column (3) uses Calonico et al.'s (2014) procedure of bandwidth selection and adjustment with a triangular kernel. Column (4)'s politician controls include gender, age, age^2, party affiliation, incumbency dummy, Senate election dummy, ln(total campaign contribution), and ln(number of contributors). Column (5)'s director controls include gender, age, age^2, executive director dummy, and director tenure. Column (6)'s firm controls include age, age^2, ln(total assets), ln(total sales), ln(employment), capital expenditure/assets, return on assets, book leverage ratio, market-to-book ratio, and Tobin's Q. Columns (7) and (8) report average CAR(-1, 5) among firms connected to winners and firms connected to losers, after controlling for vote shares. All standard errors are clustered by politician.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Column 1 reports the baseline RDD specification (1), in which we control linearly for vote shares separately for winners and losers. The resulting estimate indicates that connections to the winners in close congressional elections generate stock price reactions that are on average 3.2 percentage points *below* those generated by connections to the losers, i.e., ΔV is -3.2% of firm value. In our sample, it is equivalent to 30% of the standard deviation of CARs, and \$21 million for the median firm's market value (\$656 million).[23] This discontinuity around the 50% vote share threshold is visualized in Figure 1's Panel A. The estimate

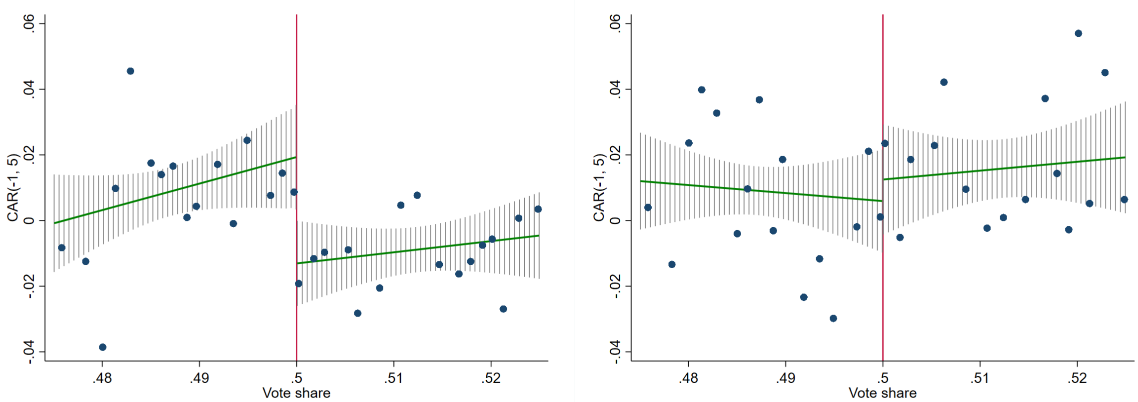
[23] In comparison to relevant event studies, Faccio (2006) reports an average effect of 1.4 percentage points among worldwide firms following an event of new political connection, while Goldman et al. (2009) show an effect of 9.0 percentage points in difference between Republican- and Democrat-connected firms around the 2000 presidential election.

is statistically significant at 1% and robust to controlling for cubic polynomials of vote shares (column 2) (further illustrated in Appendix Figure A2) and to applying Calonico et al.'s (2014) procedure (column 3).

The estimate is largely unaffected by the inclusion of predetermined covariates (Lee and Lemieux, 2010), such as politician characteristics and election year fixed effects in column 4, director characteristics and university fixed effects in column 5, and firm characteristics and industry fixed effects in column 6. The estimates reported in those columns, all of which statistically significant at 1%, are very close to the baseline effect in column 1. As the RDD identification guarantees that election outcome is as good as randomly assigned to treated and control groups around the 50% vote share threshold, the inclusion of any predetermined control variable should not significantly alter the estimate of the treatment effect. Put differently, in the baseline RDD specification, the estimated differential value of political connections is not confounded by any politician-, director-, firm-, year-, university-, or industry-specific unobservables.

Column 1's main estimate is further decomposed into the market reactions among firms connected to winners (column 7) and losers (column 8) (both controlling for vote share, as in equation (1)). The market reaction to loser-connected firms is slightly stronger,[24] hinting that the corresponding stock-market-based predicted probabilities of elections may be slightly biased towards eventual winners, which cannot invalidate our cross-sectional RDD identification (subsection 3.1), but which would have biased event-study strategies.

Figure 1: Discontinuity of Market Reaction at 50% Vote Share Threshold



A. CAR(-1, 5)

B. CAR(-7, -1)

Notes: This RDD figure plots connected firms' cumulative abnormal returns (CARs) against the connected politician's vote share around the 50% threshold, including separately fitted linear functions of vote share on either side of the threshold (Equation (1)) and their 95% confidence intervals. **Subfigure A** shows the estimated discontinuity of -3.2% on CARs between days -1 and 5 around the election. **Subfigure B** shows balanced CARs before the election between days -7 and -1. 16 dots on each side of the threshold represent approximately equal-sized bins of close elections.

[24]However, the difference between those columns is not statistically significant.

Robustness. To examine if this discontinuity is sensitive to our baseline bandwidth choice, we run a series of sensitivity tests using alternative sample restrictions ranging from 1% to 10% election vote margin. Appendix Figure A1 shows that throughout this wide range of bandwidths, the resulting coefficients remain quantitatively similar to our benchmark estimate. Appendix Table A5 further exhibits our results' robustness to using alternative observation units (which affects weighting schemes), clustering schemes, kernel functions, Calonico et al.'s (2014) sample selection, and methods to compute abnormal returns.

The RDD implications of smooth distributions of predetermined observable variables are further tested in Appendix Table A4 (as explained in section 3.2). In particular, Figure 1's Panel B shows no discontinuity in the CARs from day -7 to day -1.

Statistical inference. As explained in section 3.2, our choice of clustering adjustment by politician is based on the recent design-based framework by Abadie et al.'s (2020, 2023). We further verify its strong robustness in the traditional framework of clustered standard errors with different levels of clustering, as shown in Appendix Table A5. Even in the most difficult case of the coarsest level of clustering, that by the 5 elections in our sample, Appendix B.4.2 provides strong significant results based on both the clustered wild bootstrap (Cameron et al., 2008, Canay et al., 2021) and the versatile Approximate Randomization Test (Canay et al., 2017, Cai et al., 2023).

Further strong statistical significance is shown in exercises with hypothetical placebo thresholds (t-stats shown in Appendix Figures A6 and A7) and hypothetical placebo event days (t-stats shown in Appendix Figures A8 and A9).

Alternative event windows. Table 3 investigates the impact of election outcome on CARs calculated in various windows before and after the election event. As expected from the close election design, in columns 1 and 2, we find no differences in pre-election CARs between firms connected to eventual winners and those connected to eventual losers, using either a window from the pre-election Friday to Monday (event days -2 and -1) or one that includes one more week (from day -7 to day -1).

Column 3 shows that the main effect already attains -1.9% as soon as day 1 (significant at 1%), and extends to -3.2% after day 5 (the benchmark result, replicated in column 4), implying that market reaction from day 1 to day 5 shows a significant post-election effect (column 5). This may reflect either unresolved uncertainties surrounding very close elections,[25] or sluggish market reactions. In the latter case, one can create a portfolio based on election results on day 1 that shorts on firms connected to closely elected politicians

[25] In very tight elections, e.g., the Minnesota 2008 Senate race deadlock between Al Franken and Norm Coleman, the results could still be uncertain after election day, and news on precise vote counts continue to be meaningful in the following week.

and longs on those connected to closely defeated ones (equal weights by connections). Over (1, 5), this portfolio yields a return of 2.2% (column 5). Finally, column 6 reports an insignificant, largely noisy estimate for the following 4 weeks, suggesting that the market has fully priced in election outcome news after day 5.

Table 3: Effect in Different Event Windows

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: CAR					
	Pre-election		Around-election		Post-election	
Event window	(-7, -1)	(-2, -1)	(-1, 1)	(-1, 5)	(1, 5)	(6, 25)
Winner	0.007	-0.002	-0.019***	-0.032***	-0.022**	0.013
	(0.011)	(0.007)	(0.006)	(0.008)	(0.008)	(0.022)
Observations	1,714	1,714	1,714	1,714	1,714	1,714
Politicians	165	165	165	165	165	165
Directors	1,136	1,136	1,136	1,136	1,136	1,136
Firms	1,234	1,234	1,234	1,234	1,234	1,234

Notes: This table reports the effect of Congress-level connection on firm's cumulative abnormal returns (see subsection 4.1) in different event windows using the baseline RDD specification in equation (1). These include pre-election event windows in columns (1) and (2), around-election event windows in columns (3) to (5), and post-election event windows in columns (6) and (7). All standard errors are clustered by politician.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

In sum, we find robust evidence of Proposition 1's predicted adverse effect of higher offices on favoritism, as friends in higher positions bring *less* value to connected firms ($V_2 < V_1$).

6 The mechanism of Congress connection's adverse effect

6.1 Scrutiny and the adverse effect of Congress connection

Based on section 2's theoretical intuitions, scrutiny is the key element in both explaining (Proposition 1) and shaping (Proposition 2) the mechanism of the adverse effect of Congress connection. We focus on scrutiny via the media, as the political economy literature since Besley and Burgess (2002) has provided ample evidence on how media coverage of politics influences voters' knowledge and behaviors, hence politicians' accountability (Snyder and Strömberg, Gentzkow et al., 2010, 2011, and surveys in Anderson et al., 2016).

Change in media scrutiny among elected versus defeated candidates Proposition 1's prediction of the adverse effect of higher office on favoritism relies on the key condition that, comparing elected congressmen with their defeated opinions, scrutiny tightens even more than the gain in power, namely $\frac{\gamma_2}{\gamma_1} > \frac{\beta_2}{\beta_1}$. Appendix Table A6 provides evidence that media scrutiny is markedly higher for winners than losers ($\frac{\gamma_2}{\gamma_1} \gg 1$). We measure media attention by the number of search hits for the politician's name on his state's newspapers based on Newslibrary.com, normalized by the number of search hits for the neutral keyword "September." On average, elected congressmen experience an increase in media attention, while defeated candidates experience a reduction of similar magnitude. The difference between these opposite changes, estimated using the baseline RDD specification, is large and statistically significant. There is practically no pre-election difference in media presence between winners and losers in the considered close elections, while the post-election media presence difference comes immediately in the first two years, for challengers and incumbents alike. Furthermore, winners' increased media attention is driven solely by challengers, while incumbent winners only maintain the level of pre-election newspaper attention. Symmetrically, losers' reduction in media attention is mostly driven by incumbents losing their Congress seats.

Longitudinal variation of media scrutiny due to Craigslist's entry. To identify the role of scrutiny in shaping the adverse effect of Congress connection, we first exploit the natural experiment of the entry of Craigslist (henceforth CL), the world's largest online platform for classified ads, across U.S. states. Based on Djourelova et al. (2023), CL's entry crowds out local newspapers, lowers their coverage of politics, decreases their readership, and ultimately undermines voters' electoral participation.[26] We thus consider CL's entry as an exogenous proxy for γ_1, namely local media scrutiny by state and year. As CL's entry lowers γ_1, Proposition 2 predicts an increased differential value $|\Delta V|$, i.e., a strengthened adverse effect of Congress connection.

We consider CL's presence in a state since at least two years before the election, and CL's penetration, measured as the share of counties where CL has entered since at least two years before the election. The lag period of two years reflects the time needed for CL's entry to fully affect local newspapers (Djourelova et al., 2023). We enhance specification (1) with the interactions of a measure of CL's entry with all right-hand side variables, including the winner indicator $Winner_{pt}$ and the running variables of vote shares $VS_{pt}\mathbb{1}_{\{VS_{pt}\geq 50\%\}}$ and $VS_{pt}\mathbb{1}_{\{VS_{pt}<50\%\}}$. This specification effectively amounts to a Difference in Discontinuity approach.

This approach brings several advantages. First, CL's entry has been argued as largely exogenous to local political conditions (Djourelova et al., 2023, Gao et al., 2020), hence it avoids the apparent issue of more direct measures of media scrutiny, such as newspapers' coverage of politics or voters' interest in politics. Second, CL's entry brings a longitudinal dimension to measure meaningful changes in media scrutiny across states, which is a strong advantage over other popular measures discussed in the literature that provide mostly meaningful cross-sectional variations (e.g., based on Snyder and Strömberg's (2010) newspaper's congruence measure or Campante and Do's (2014) measure of capital isolation.) Most of CL's entry happened over the period 2000-2010, which coincides with our sample. Its longitudinal nature allows us to also control for an

[26] Djourelova et al. (2023) also finds that CL's entry enhances extreme candidates' chances and reduces split-ticket votes, an indicator that voters increasingly use national cues instead of local ones. Those findings all point to less media scrutiny due to local newspaper closure. Gao et al. (2020) also uses this natural experiment to account for newspaper closure.

interaction between the winner indicator $Winner_{pt}$ and a set of state fixed effects, which can take out any state-specific time-invariant characteristics that may influence the adverse effect of Congress connection.

Table 4's columns 1 to 6 report results using this strategy. Data on CL's entry come from Djourelova et al. (2023), either based on links scraped from the Internet Archive at https://www.archive.org that fully cover all counties (columns 1 to 3), or from CL's official sources on a number of key counties (columns 4 to 6). Columns 1 and 4 show that higher CL's penetration in a state leads to a stronger adverse effect of Congress connection. It means that, as local media scrutiny decreases, the adverse effect of Congress connection on firms increases in magnitude $|\Delta V|$. Columns 2 and 5 further show that this effect remains equally strong even in presence of the interactions between the winner indicator $Winner_{pt}$ and a set of state fixed effects, which controls for any time-invariant state characteristics' influence on the outcome. Finally, in columns 3 and 6 we replace CL's penetration with CL's presence, a coarser variable. The results become noisier, but remain sizable and statistically significant at 10%.

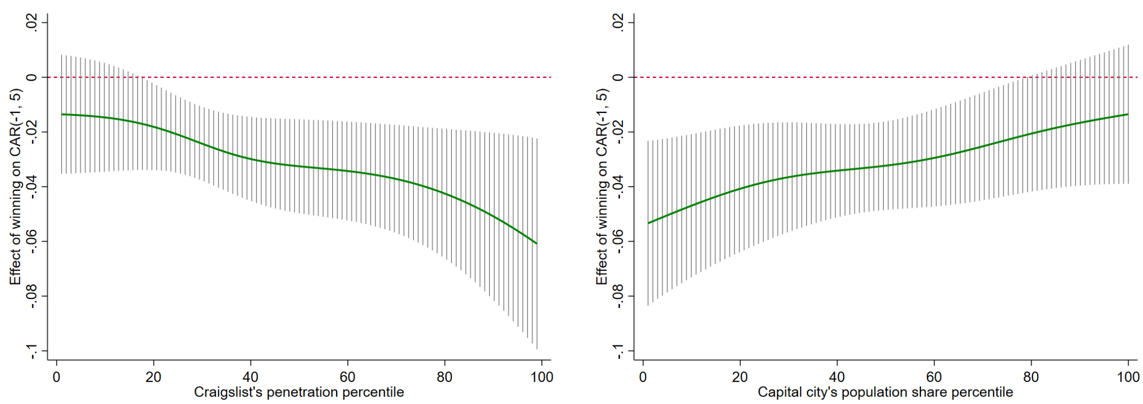
Table 4: Effect by Exogenous Shift to Local Media Presence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable: CAR(-1, 5)							
Local media shifter	Craigslist's presence in state						Capital city's population	
Winner	-0.014			-0.017*			-0.039***	-0.053***
	(0.009)			(0.009)			(0.008)	(0.013)
W × Craigslist's penetration	-0.179**	-0.184**		-0.136**	-0.130**			
	(0.078)	(0.072)		(0.065)	(0.062)			
W × I(Craigslist)			-0.055*			-0.048*		
			(0.029)			(0.028)		
W × I(Capital is largest city)							0.030*	
							(0.018)	
W × Capital primacy								0.558**
								(0.273)
Craigslist data source	Scraped	Scraped	Scraped	Official	Official	Official		
Election year FEs	X	X	X	X	X	X		
Winner × State FEs		X	X		X	X		
Observations	1,714	1,714	1,714	1,714	1,714	1,714	1,714	1,714
Politicians	165	165	165	165	165	165	165	165
Directors	1,136	1,136	1,136	1,136	1,136	1,136	1,136	1,136
Firms	1,234	1,234	1,234	1,234	1,234	1,234	1,234	1,234

Notes: This table reports how the adverse effect of Congress-level connection on firms ΔV depends on determinants of local media scrutiny. Each column's specification builds on the RDD specification in equation (1), and further includes interactions between a determinant of local media scrutiny and the right hand side variables in (1). Craigslist data are from Djourelova et al. (2023). *Craigslist's penetration* measures the share of counties where Craigslist has entered at least two years before the election month. *I(Craigslist)* is an indicator that Craigslist has been present in the state at least two years before the election month. *I(Capital is largest city)* is an indicator that the state's capital is also its largest city in 1980. *Capital primacy* is the state capital's 1980 population share of the state population. Columns (1) to (3) calculate Craigslist's entry from Craigslist links scraped from the Internet Archive (https://archive.org), and have full coverage. Columns (4) to (6) use Craigslist's official entry data, and have partial coverage. All standard errors are clustered by politician.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Subfigure A of Figure 2 further plots the adverse effect of Congress connection as a nonlinear function of CL's penetration, by applying a semi-parametric version of column 2's specification (methodological detail in Appendix B.3). Their strong relationship is consistent throughout the sample and does not arise from any particular range of CL's penetration.

Figure 2: Effect by Determinants of Local Media Scrutiny



A. Craigslist's penetration

B. Capital city's population share

Notes: This figure plots semi-parametric estimates of the differential value of Congress-level connection to firms ΔV as a function of the percentiles of the X-axis variable, together with their 95% confidence intervals. In **Subfigure A**, the X-axis variable is the share of counties per state where Craigslist has entered for at least 2 years. In **Subfigure B**, the X-axis variable is the population share per state of the state capital city. The point estimate at each value of the X-axis variable is obtained from the baseline RDD regression in equation (1), weighted by a Gaussian kernel function of the percentile on the X-axis with a bandwidth equal to 20% (details in Appendix B.3). Standard errors are clustered by politician.

Deep-rooted variation of media scrutiny by capital city isolation. Next, we consider a source of cross-state variation in media scrutiny that is deep-rooted in each state's history and geography. We build on Campante and Do's (2014) result that states with an isolated capital city, i.e., one that is not surrounded by a large population, have less local media scrutiny of state politics, hence weaker checks and balances.[27] That is, states with an isolated capital have weaker state-level scrutiny γ_1, leading to a higher differential value $|\Delta V|$, namely a stronger adverse effect of Congress connection as per Proposition 2 .

We take two simple measures of state capital primacy from the 1980 census as exogenous determinants for local media scrutiny, namely the indicator whether the state capital city is its largest city, and the share of capital city population of the state population. Such deep-rooted measures are generally highly persistent, and not affected by contemporaneous factors. Columns 7 and 8 of Table 4 show results from specification (1) enhanced with the right-hand side's interactions with those proxies. Again, the interaction terms' coefficients provide support to Proposition 2's prediction on state-level scrutiny.

Using the same method as Figure 2's subfigure A, its subfigure B further plots the adverse effect of Congress connection as a nonlinear function of a state's capital city primacy (methodological detail in

[27]The finance literature has used this result to build exogenous proxies for state-level governance, such as in Smith's (2016) analysis of corruption and corporate financial policies.

Appendix B.3), which supports Proposition 2's prediction throughout the full range of capital city primacy.

Additional checks with other drivers of scrutiny. We have so far established how the adverse effect of Congress connection on firms varies by two exogenous determinants of scrutiny. Appendix Figure A3 shows similar patterns for two direct proxies of voters' scrutiny, including the share of voters with strong interest in election and the share of voters following media coverage of election, constructed from the American National Election Studies (ANES) over 2000-2008, and a measure of corruption by state from internet search hits for "corruption" near the state's main city (following Saiz and Simonsohn's (2013) approach of "downloading wisdom from online crowds"). Overall, the magnitude of the adverse effect of Congress connection increases in states with more scrutiny and less corruption.

Appendix Table A7 further provides additional evidence of the mains specification (1) in different subsamples split by proxies of media scrutiny, including the previous measures of voters' interest in politics and voters' attention to media, Campante and Do's (2014) Average Log Distance to capital city, plus alternative measures of corruption based on conviction cases (Glaeser and Saks, 2006) and search hits by city name or state name. The empirical patterns of those estimates of ΔV largely follow Proposition 2's prediction on the role of scrutiny.[28]

Those results do not rule out the role of the variations of power, which we will examine more directly in subsection 6.3. One may also ask whether variations in power can fully explain Proposition 1's main adverse effect. For example, one may posit that, first-term Congress members may have much less power to give favor, compared with seasoned state-level politicians (i.e., $\frac{\beta_2}{\beta_1} \ll 1$). Appendix Table A8 offers some insight into this possibility by replicating Table A7 in the subsamples of challengers versus incumbents. While the estimates and precision are weaker for incumbents than for challengers, the main pattern in Table A7 remains similar for both groups in Table A8, suggesting that the role of scrutiny remains important in determining the adverse effect of higher office.

6.2 Politician's career concern

As scrutiny affects politicians' career prospects, it likely matters more in the early stage of their career. Proposition 1 highlights this intuition in a form of weak monotonicity of ΔV over the course of a political career, in that it likely starts out below zero and may eventually moves above zero late in the career. This subsection verifies this prediction in the sample of challengers to avoid the potentially confounding effect of tenure and accumulated power in Congress.

[28] On the other hand, we do not find ΔV to vary with firm's distance to DC, suggesting that greater geographical distance between firms and connected congressmen is not a key channel behind the effect.

Column 1 of Table 5 shows that the adverse effect of connection to a congressman fades out with the politician's age, as the coefficient of the interaction between $Winner_{pt}$ and politician's age (normalized at the median age of 56) is positive and statistically significant at 5%. The coefficients imply an effect of -3.4% at age 56, which would fade to zero around age 67. Columns 2 and 3 show that the effect's magnitude is much larger among younger-than-median politicians (4.8%) and smaller among older ones (2.6%), although the difference is not statistically significant. Columns 4 to 7 further report the estimated benchmark effects across the four quartiles of politician age that follow a gradually increasing pattern. Especially in the top quartile, the estimate becomes positive, although not statistically significant.

A very similar pattern of estimates is also found in the full sample of all politicians, as shown in Appendix Table A9. Appendix Figure A4 further illustrates semi-parametric estimates of ΔV as a function of politician's age that goes towards zero as age increases, for both the full sample of politicians and that of challengers only (methodological details in Appendix B.3).

Table 5: Effect by Politician's Age among Challengers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: CAR(-1, 5)						
Politician sample	All	Below med.	Above med.	Age Q1	Age Q2	Age Q3	Age Q4
Winner	-0.034***	-0.048***	-0.026	-0.052**	-0.041***	-0.030	0.003
	(0.009)	(0.012)	(0.018)	(0.026)	(0.013)	(0.028)	(0.034)
W × (Pol. Age - 56)	0.003**						
	(0.001)						
Difference		*-0.022*					
		(0.021)					
Observations	1,121	698	423	373	325	236	187
Politicians	110	83	27	52	31	14	14
Directors	801	532	291	294	240	159	136
Firms	922	628	371	344	305	216	169

Notes: This table reports how the differential value of Congress-level connection to firms ΔV varies by the politician's age, using the baseline RDD specification in equation (1), for the subsample of firms connected to challenger candidates. Column (1) interacts the treatment (i.e., winning the election) with the politician's age (relative to the median of 56). Columns (2) and (3) compare subsamples of younger (at most 56) and older (above 56) politicians. Columns (4) to (7) consider the subsamples of politicians in age quartile 1 to 4 as determined with respect to the full baseline sample. All standard errors are clustered by politician.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

6.3 Determinants of a firm's benefits and the adverse effect of its connection

In this section, we turn to study firm, director, politician, and relationship characteristics that influence firms' potential benefits from political connections (β's) and their implications on ΔV. As distinguished in the model, we consider factors that affect β_1 and β_2 separately and those that affect both of them in the same direction.

Politician's experience. Table 6 reports how ΔV varies with the politician's type and level of experience. Columns 1 and 2 first compare the differential values of connections to challengers versus incumbents in Congress elections. One would expect β_2 to be quite small for challengers (power to give favor from a newly elected Congress member), but considerably larger for incumbents thanks to their empowerment and entrenchment in Congress. As expected from the theory, the magnitude of the differential value among challengers is larger than that among incumbents (the difference is statistically significant at 10%).

Table 6: Effect by Politician's Prior Experience

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: CAR(-1, 5)					
Politician sample	Challengers	Incumbents	State	House	Senate	All
Winner	-0.043***	-0.013	-0.048***	-0.010	0.086***	-0.048***
	(0.009)	(0.014)	(0.013)	(0.016)	(0.017)	(0.013)
W $\times$ Politician's experience						0.036**
						(0.015)
Difference	*-0.030**			*-0.038**	*-0.134****	
	(0.017)			*(0.020)*	*(0.021)*	
Observations	1,121	593	574	508	129	1,211
Politicians	110	64	58	58	12	124
Directors	801	440	436	372	103	838
Firms	922	517	506	438	127	934

Notes: This table reports how the differential value of Congress-level connection to firms ΔV varies by the politician's prior experience, using the baseline RDD specification in equation (1). Column (1) considers the subsample of all challengers and column (2) – incumbents. Column (3) considers the subsample of politicians with immediate prior position in state politics; column (4) – politicians with prior experience as House members (but not in state politics or the Senate); and column (5) – politicians with prior experience as Senators. Column (6) interacts the treatment with the politician's level of experience, which ranges from 0 to 2 and corresponds to the subsamples in columns (3) (level of experience = 0) to (5) (level of experience = 2). Row *Difference* reports the difference in ΔV between columns (1) and (2), and between column (3) and each of the columns from (4) to (5). All standard errors are clustered by politician.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

We also categorize politicians based on their career prior to the election: those in a position in state-level politics and those with previous positions in the House or in the Senate. Among those categories, we expect that the ratio β_2/β_1 is increasing in this order. Indeed, coming from state politics, one should expect β_1 to be relatively large and β_2 to be small. In contrast, those who have already been in Congress should naturally enjoy a very large β_2 (likely larger in the Senate than the House), but a small β_1. Based on this order, the pattern of the estimated differential effect matches with the theoretical predictions, as shown in columns 3 to 6. From columns 3 to 5, the estimate increases from strongly negative to less negative, to even a positive estimate among senators.[29] When we combine those estimates in a specification with an interaction term with the order among those cases in column 6, the coefficient of the interaction term is positive and statistically significant at 5%.[30]

[29]This finding of a positive differential value among connections to senators partly vindicates Prediction 1's first point in case power trumps scrutiny. Our companion paper Do et al. (2021) also shows the positive net value of firms' connections to elected state governors.

[30]Unlike those variations by political power, we did not find much difference of the adverse effect between Democrats and

Firm size. We further exploit firm size as a key determinant of β_1 and β_2. While Table 2's main results show that on average firms benefit less from connections to politicians in higher positions, this pattern may reverse for very large firms which stand to benefit more from federal-level connections (as a larger β_2 would increase ΔV). In contrast, smaller firms operating mostly within the politician's state likely experience a larger β_1, implying a smaller (more negative) ΔV. Thus, as β_2/β_1 is likely increasing in firm size, so is ΔV. This pattern is confirmed in Figure 3's Subfigure A, which plots the semi-parametric estimate of ΔV as a function of firm size (methodological details in Appendix B.3).

Appendix Table A11 provides more details in this relationship, with a positive estimate of ΔV at 1.1% (column 2, not statistically significant) among the largest firms (the larger half of S&P 500 firms) but at -3.8% among the rest (column 3). The effect is even stronger at -4.7% (column 4) for local firms, i.e., those headquartered in the politician's state or within 500km of its capital.[31]

Column 1 further helps quantify the adverse effect's variability by firm size. Since the market value used in the interaction term is centered at its median, the coefficient of $Winner$ represents the effect of -0.031 at the median market value of around $656 million, equivalent to -$20.3 million. At the mean market value of $6,367 million, the effect is close to zero at $-0.031 + \ln(6367/656) \times 0.012 \sim 0.000$. At the low end, for a firm valued at $100 million, the effect is $-0.031 + \ln(100/656) \times 0.012 \sim -0.054$, equivalent to -$5.4 million.

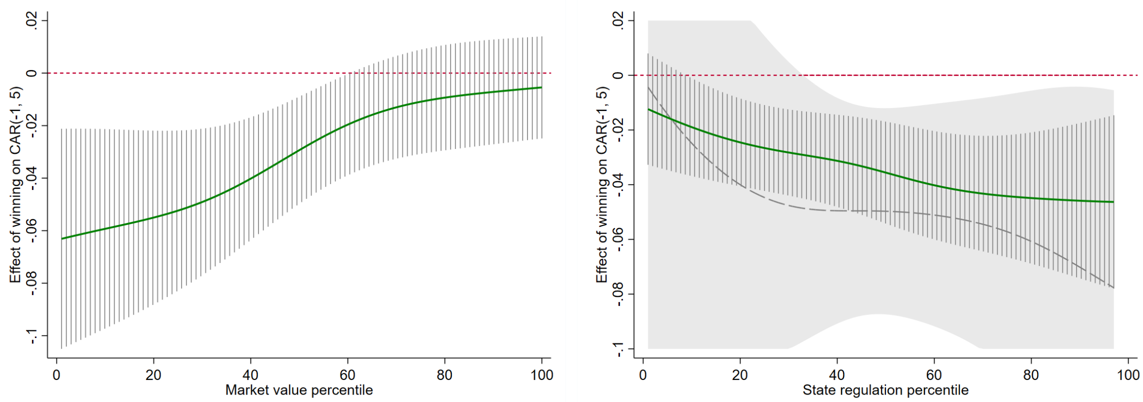
State regulations. State-level connections are likely more beneficial to firms (larger β_1) in states with more regulations, where there is greater potential to grant benefits to connected firms on a discretionary basis. This implies a smaller (more negative) differential value of higher-office connections ΔV. Figure 3's Subfigure B confirms this pattern with a plot of the semi-parametric estimate of ΔV as a function of state-level regulation, using the 1999 state-level regulation index from Clemson University's Report on Economic Freedom (variable description in Appendix Table A1, methodological details in Appendix B.3).

Appendix Table A11 shows further supporting results, including the negative, statistically significant estimated coefficient on the interaction between the treatment and state regulation index (column 5) and the estimates of ΔV among high-regulation states (-4.4% in column 6, significant at 1% level) and among low-regulation states (small and not significant). Furthermore, the gradient of this difference is more pronounced among local firms, to which state level regulations and thus related benefits from local political connections are more relevant (interaction term of -8.3% in column 8, compared to that of -4.4% in column 5), as also shown by the dashed line of the corresponding semi-parametric estimate in Figure 3's Subfigure B.

Republicans or between the President's party or the opposition (Appendix Table A10).

[31] Varying this 500 kilometer cutoff does not qualitatively affect the findings.

Figure 3: Effect by Firm Size and State-Level Regulations



A. Firm's market value

B. State's regulation index

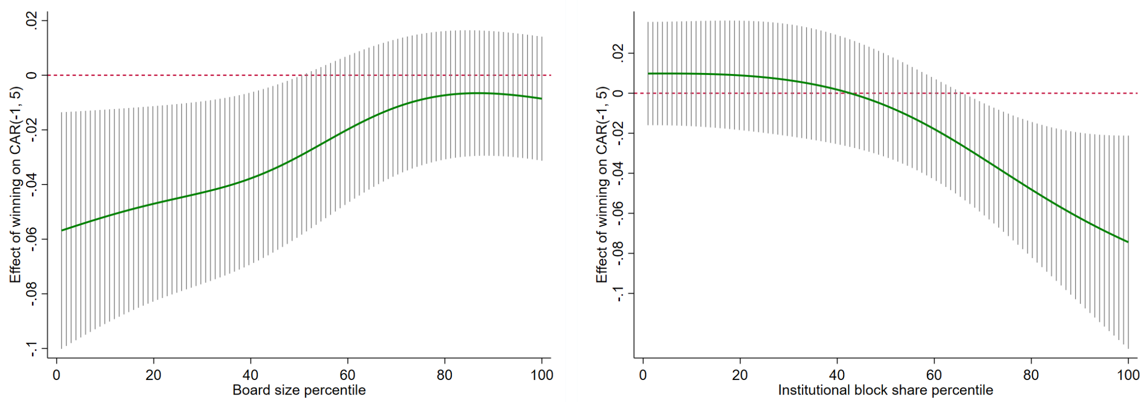
Notes: This figure plots semi-parametric estimates of the differential value of Congress-level connection to firms ΔV as a function of the percentiles of the X-axis variable, together with their 95% confidence intervals. The X-axis variable is firm's market value in **Subfigure A**, and state's regulation index in **Subfigure B**. In Subfigure B, the dashed line represents the estimate among local firms only. The point estimate at each value of the X-axis variable is obtained from the baseline RDD regression in equation (1), weighted by a Gaussian kernel function of the percentile on the X-axis with a bandwidth equal to 20% (details in Appendix B.3). Standard errors are clustered by politician.

Firm's corporate governance. Next, we investigate how ΔV depends on a firm's corporate governance, which predicts its ability to extract value from favors from both high and low offices (variations of both β_1 and β_2). As commonly used in the corporate finance literature, small board size and large institutional block share are associated with better corporate governance (Ferreira and Matos, 2008, Yermack, 1996).[32] Figure 4 plots the adverse effect of Congress connection as a function of those two variables, with a pattern that confirms Proposition 2's prediction that as both β_1 and β_2 grow proportionally, so does the magnitude of the differential value $|\Delta V|$.

Appendix Table A12 further provides supplementary evidence with split-sample regressions based on those two measures of corporate governance. It further considers subsamples split by state-level generalized trust from the ANES (2000-2008), as higher trust in non-contractual transactions likely implies higher β_1 and β_2; and also by alumni reunion years (Shue, 2013), as strengthened alumni relationships should increase β_1 and β_2. The patterns of the estimates of ΔV broadly follow Proposition 2's predictions.

[32]See also the survey by Shleifer and Vishny (1997). In addition, using alternative measures of corporate governance quality, such as number of institutional block owners or total institutional shares, also yields similar results.

Figure 4: Effect by Corporate Governance



A. Firm's board size

B. Firm's institutional block shares

Notes: This figure plots semi-parametric estimates of the differential value of Congress-level connection to firms ΔV as a function of the percentiles of the X-axis variable, together with their 95% confidence intervals. The X-axis variable is the firm's board size in **Subfigure A**, and the firm's institutional block share in **Subfigure B**. The point estimate at each value of the X-axis variable is obtained from the baseline RDD regression in equation (1), weighted by a Gaussian kernel function of the percentile on the X-axis with a bandwidth equal to 20% (details in Appendix B.3). Standard errors are clustered by politician.

7 Discussions on measurement and interpretation

7.1 Precision of connection measured by educational institutions

As discussed in subsection 3.2, while two individuals' going to the same university at the same time is a relevant and appropriate proxy for their being connected later in life (Cohen et al., 2008, Nguyen, 2012, Fracassi, 2017), it may still contain measurement errors, leading to a potential attenuation bias of the estimate of ΔV. This bias should decrease with the quality of our connection measure.

Table 7 confirms this pattern that the magnitude of the estimated differential value ΔV decreases steadily as we increasingly relax the definition of politician-director connection, from requiring each pair to have graduated from the same university, campus, school, and program combination (column 1) to only same university and program combination (column 3), and from at most one year apart (columns 1-3) to two to four years apart (columns 4-6), and all the way to the full alumni network (column 7, in which the estimate is close to zero). Similarly, the estimate is not statistically different from zero among the networks of the 15-most enrolled universities (column 8), where the chance that they actually know one another is slim.

Our defined connection may also reflect cases of politicians and directors who only connect later in their careers, especially when they have already reached important positions, when their shared alma maters may act as a catalyst.[33] Hence, their connection is likely stronger in networks that are more likely to provide

[33] Results regarding alumni reunion year in columns 7 and 8 of Table A12 also hint at this possibility.

Table 7: Effect by Quality of Politician-Director Connection Measure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent variable: CAR(-1, 5)								
	Same institution definition			Graduation year difference				Total enrollment	
Network sample	Strict	Baseline	Loose	2 year	3 year	4 year	Alumni	Top 15	Others
Winner	-0.039***	-0.032***	-0.029***	-0.020**	-0.016**	-0.015*	-0.003	-0.011	-0.036***
	(0.010)	(0.008)	(0.007)	(0.008)	(0.007)	(0.008)	(0.006)	(0.024)	(0.009)
Observations	1,785	1,714	1,847	2,939	4,079	5,237	26,084	267	1,447
Politicians	155	165	173	181	189	193	213	28	145
Directors	1,131	1,136	1,237	1,811	2,410	2,962	8,974	181	958
Firms	1,233	1,234	1,309	1,812	2,212	2,533	4,264	214	1,067

Notes: This table reports how the *estimated* value of Congress-level connection to firms ΔV varies with the quality of the politician-director connection measure, using the baseline RDD specification in equation (1). In the baseline definition, a politician-director pair is considered connected if they graduated from (i) the same university, campus, and degree program combination (ii) at most one year apart (column 2). Columns (1) and (3) vary the same-institution restriction, from requiring the same university, campus, school, and degree program combination (column 1) to only same university and degree program (column 3). Columns (3) to (8) vary the restriction on graduation years, from difference of at most one year (columns 1 to 3) to up to four years (column 6) to including all alumni (column 7). Columns (8) and (9) compare subsamples of universities in versus outside the top 15 in total enrollment. All standard errors are clustered by politician.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

better benefits, e.g., by containing more important businesspeople. We use a network's size in BoardEx to proxy for its benefits. Appendix Table A13 indeed shows that the adverse effect of Congress connection is largest among brand-name universities that are the most represented in our sample, namely Harvard University in column 1, the top 3 of Harvard, Stanford University, and University of Pennsylvania in column 3, and Ivy League schools in column 5.

7.2 Homophily and shared preferences as alternative mechanisms

As discussed in subsection 3.2, our empirical design takes the classmate connections between politicians and directors as exogenously given. So the estimated effect could still be due to the homophily mechanism, whereby both same school attendance and linked future outcomes of politicians and businessmen are driven by certain shared characteristics (McPherson et al., 2001). Another possible mechanism is that based on shared preferences earned from their attendance at the same university (Algan et al., 2023). Those mechanisms are different from our suggested mechanism of direct influences among classmates.

In case of those alternative mechanisms, we would expect a politician's win to have similar effect on his classmates' firms as well as other alumni's firms.[34] The following specification formalizes this intuition in an enlarged sample that gathers all pairs of firms and politicians with an alumni connection, i.e., a director on the firm's board and the politician have attended the same university at some point, not necessarily in the same class. It extracts the estimated effect on firms connected to the running candidates through *classmate links* (the baseline sample, for which $Class_{dp} = 1$) from the effect on firms connected through *alumni links*

[34] Hence, the lack of significant result among firms connected to politicians through the alumni network (Table 7, column 7) already suggests that homophily is not a first order concern.

(for which $Class_{dp} = 0$), controlling for a full set of university-by-election year fixed effects θ_{st}:

$$CAR_{idt} = \gamma Winner_{pt} \times Class_{dp} + \beta Winner_{pt} + \rho Class_{dp} + f(VS_{pt}, Class_{dp}) + \theta_{st} + \varepsilon_{idpt}.^{35} \quad (2)$$

Table 8: CONTROLLING FOR HOMOPHILY

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: CAR(-1, 5)						
	Same institution definition			Year difference		Network sample	
Network sample	Baseline	Loose	Strict	10 years	5 years	Harvard	Big network
Winner × Classmate	-0.040***	-0.036***	-0.044***	-0.039***	-0.038***	-0.031*	-0.032*
	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.016)	(0.016)
Winner	0.002***	0.002***	0.002***	0.008	0.003	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.005)	(0.007)	(0.001)	(0.001)
University × Election year FEs	X	X	X	X	X	X	X
Observations	25,988	27,438	27,971	11,113	6,107	5,523	7,088
Politicians	213	215	213	219	193	23	28
Directors	8,934	9,285	8,635	5,217	3,343	795	1,518
Firms	4,245	4,306	4,231	3,486	2,724	1,013	1,653

Notes: This table compares the effect of close election outcome on firms connected to the running candidates through the classmate network and those connected only through the alumni network, using equation (2) which controls for a full set of university-by-election year fixed effects. Columns (1) and (3) vary the same institution definition (see notes to Table 7 for details). Columns (4) and (5) restrict the samples to only politician-director pairs that are at most 10 years (column 4) or 5 years (column 5) apart in school. Columns (6) and (7) consider the alumni network of Harvard University (column 6) and top three most represented universities in our director sample (Harvard University, Stanford University, and the University of Pennsylvania) (column 7). All standard errors are clustered by politician.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

The coefficient of interest γ captures the differential value ΔV associated with classmate-connected firms after eliminating the common effects of all contemporaneous elections linked to the corresponding alma mater, which includes the homophily effect and the effect of shared preferences. Table 8 presents different estimates of this coefficient $\widehat{\gamma}$ corresponding to different restrictions of the networks, on the scope of the same university (columns 1 to 3), the scope of the politician-director time gap (columns 4 and 5), and among the most represented universities in our director sample. Across those different samples, the estimate remains particularly stable between -3.6% and -4.4%, and statistically significant. They are close to, and slightly stronger than the benchmark of -3.2% (Table 2), indicating that homophily does not contribute to explaining the adverse effect of higher positions found in this paper.

7.3 Medium-term effects on firms and directors

We further find that the main results (Table 2) that firms benefit less from their connections to elected congressmen carry over to firms' medium-term performances. Columns 1 and 2 of Table 9 report that firms connected to elected congressmen reduce their activities in the corresponding state in the year following the

[35] $f(VS_{pt}, Class_{ipt})$ includes the full interaction between VS_{pt} and $Class_{dp}$, separately for each side of the winning threshold. That is, $f(VS_{pt}, Class_{dp}) = \delta_W VS_i \mathbb{1}_{\{VS_i \geq 50\%\}} + \delta_L VS_i \mathbb{1}_{\{VS_i < 50\%\}} + \psi_W VS_i \mathbb{1}_{\{VS_i \geq 50\%\}} Class_{dp} + \psi_L VS_i \mathbb{1}_{\{VS_i < 50\%\}} Class_{dp}$.

election, as measured by firm's presence on local media,[36] relative to those connected to defeated candidates. Furthermore, directors connected to elected congressmen, whose connections are now less valuable to their firms, are also more likely to leave the firms after the election, based on results from both a Cox proportional hazard model (in which the hazard event is the director's leaving the firm after the election) (column 5) and an RDD specification (in which the outcome variable is whether the director leaves the firm within three years of the election) (column 6).

Table 9: Effects of Congress-Level Connection on Firm's Real Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	**Local media mention**		**ln(employment)**		**Director leaving firm**	
	Year 0	Year 1	Year 0	Year 1	Hazard	Within 3yrs
Model	RDD with LDV		RDD with LDV		Cox	RDD
Winner	-0.004	-0.015*	-0.011	-0.016	0.335**	0.168**
	(0.006)	(0.008)	(0.040)	(0.033)	(0.147)	(0.065)
Observations	1,700	1,704	1,611	1,591	1,431	1,138
Politicians	164	164	165	165	148	121
Directors	1,130	1,131	1,087	1,072	940	731
Firms	1,229	1,229	1,160	1,143	1,047	842

Notes: This table reports the effect of close election outcome on connected firms' and directors' real outcomes. Columns (1) to (4) use the baseline RDD specification in equation (1) with additional lagged dependent variable (LDV) control. The dependent variable in columns (1) and (2) is media coverage of firm, as measured by the normalized hit rate from a search for the firm in local newspapers, in the year of the election (year 0) and the year following the election (year 1) respectively. The dependent variable in columns (3) and (4) is firm's ln(employment) in years 0 and 1 respectively. Column (5) employs a Cox proportional hazard model with the hazard event being the director's leaving the firm after the election, with controls for vote shares (separately for each side of the winning threshold) and the director's age and tenure at the firm at year 0. Column (6) uses the baseline RDD specification in equation (1) with (i) the dependent variable being an indicator the director's leaving the firm within three years of the election and (ii) additional controls for the director's age and tenure at the firm at year 0. Columns (5) and (6) restrict the sample to directors under 62 in year 0 to exclude natural retirement within three years. Column (6) further restricts the sample to election years 2000, 2002, 2004, and 2006, so that at least three years after each election are fully observed. All standard errors are clustered by politician.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

On the other hand, there is no difference in employment between winner-connected and loser-connected firms, both before and after the election (columns 3 and 4). This result is inconsistent with the potential mechanism according to Shleifer and Vishny's (1994) theory that politicians pressure connected firms to increase hiring to support their electoral candidacies.

7.4 Market's attention and trading volume

Are classmate connections salient enough for investors to be priced into connected firms' stocks? Let us remark that arbitrage based on such information of connections does not require the information to be widely held by all potential investors. Instead, a few analysts and investors "in the know" who follow

[36]Unfortunately, data on firm's economic activities by state are not readily available. Similar to a politician's media presence (Table A6), a firm's media presence is calculated as the number of search hits for the firm's name on the corresponding state's newspapers based on Newslibrary.com, normalized by the number of search hits for the neutral keyword "September." The resulting hit rate proxies for the firm's activities within the state in the search period. At the national level, this variable is remarkably correlated with changes in firm's sales, investments, R&D, employment, and cash flows.

those firms, including but not restricted to insiders, may be sufficient to create the stock price impact. If they receive other investors' attention because of the election, information cascades (Bikhchandani et al., 1992, 1998) can lead to abnormal increases in the trading volume of related stocks around the election day (especially since close elections' results are unpredictable *ex ante*).

Indeed, we find evidence of abnormal trading volume (Campbell and Wasley, 1996) of stocks of firms connected to close-election candidates around the corresponding election day. Using a market model from day -315 to day -61 before each event to calculate the abnormal daily trading volume around the election day, we find that stocks in our sample are traded significantly more around the event, with 16.4% cumulative abnormal volume during the (-5,-1) window, and 16.2% cumulative abnormal volume during the (-1, 5) window, both statistics significant at 1%.

8 Concluding remarks

This paper challenges the commonly evoked view that higher positions always lead politicians to distribute more favors to their socially connected firms. Our intuitions emphasize the balance between a position's power to give favors and how much scrutiny it faces. If this balance tilts towards scrutiny, the attainment of a higher position may result in an adverse effect on connected firms' value.

We empirically assess this claim using the Regression Discontinuity Design of close elections in order to estimate the differential value of connection to a politician elected to the U.S. Congress versus a defeated candidate. We find robust, statistically significant, and economically important effects ranging from -1.9% (after a day) to -3.2% (after a week) of firm's market value. This adverse effect is most prominent among younger candidates, when career concerns are arguably the strongest. It also varies with predictors of the balance of power and scrutiny according to the theoretical intuitions.

Those findings highlight the crucial role of scrutiny in restraining favoritism at all political levels, and lead to the question of institutional and policy design of scrutiny across different layers of institutions. If resources to monitor politicians are limited, and favoritism is broadly considered undesirable, but all the more so at higher positions, then there is clearly an argument to focus more monitoring on politicians at higher level. American institutions that place congressmen under a lot more scrutiny than, say, state-level officials, may already reflect this trade-off.

Finally, a note of caution on generalizing the empirical results for several reasons. First, while our estimate is a Weighted Average Treatment Effect (WATE) across all politicians, we acknowledge that some politicians may naturally have higher chances of competing in a close election, and correspond to larger weights in the WATE. Our interpretation is therefore more informative about those politicians than some others who expectedly win (or lose) by large margins. Second, extrapolations before and after this period, or towards other types of political connections, require careful consideration. Third, we also stop short of inferring the effect of connections on general welfare. These topics are natural targets for future research in this line of work.

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A Appendix Tables and Figures

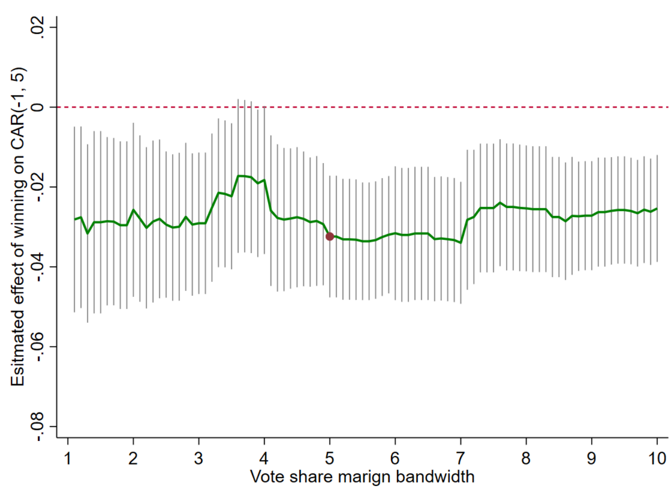
Table A1: Description of Variables

Variable	Description and construction
	Social network variables
Alumni	A firm's director and a Congress election candidate are counted as coming from the same alumni network if both graduate from the same university degree program. Following Cohen et al. (2008), we group the degrees into six categories: (i) business school (Master of Business Administration), (ii) medical school, (iii) general graduate (Master of Arts or Master of Science), (iv) Doctor of Philosophy, (v) law school, and (vi) general undergraduate. They are counted as classmates if they come from the same alumni network and they graduate within one year of each other. *Source: BoardEx, Lexis-Nexis biographies, and authors' manually collected data.*
Classmates	Two alumni are further counted as classmates if they come from the same alumni network and they graduate within one year of each other. *Source: As above.*
Top 15 universities	Indicator of the top 15 largest universities (among those represented in our baseline sample) in terms of total enrollment: (1) Arizona State University, (2) University of Florida, (3) Texas A&M University, (4) University of Texas at Austin, (5) Ohio State University, (6) University of Minnesota, (7) Pennsylvania State University, (8) Michigan State University, (9) University of Illinois, (10) New York University, (11) University of Wisconsin, (12) University of Michigan, (13) Brigham Young University, (14) University of Southern California, and (15) University od Arizona. *Source: http://www.matchcollege.com/top-colleges.*
Big-network universities	Indicator of the top three most represented universities in our director sample: Harvard University, Stanford University, and the University of Pennsylvania. *Source: BoardEx.*
Reunion year	Indicator of whether the election year coincides with the most recent alumni reunion. *Source: Authors' manually collected data.*
	Politician variables
Educational background	Biographies in (i) Who's Who in American Politics, (ii) Member Biographical Profiles – Current Congress, (iii) World Almanac of U.S. Politics, and (iv) The Almanac of American Politics. Who's Who biographies provide a brief vita, including the candidate's employment history, all undergraduate and graduate degrees attained, the year in which those degrees were awarded, and the awarding institution. For biographies unavailable in Who's Who (especially for defeated candidates), we search the Library of Congress Web Archives which cover multiple versions of Congress election candidates' websites archived at different moments during the electoral campaign. *Source: Lexis-Nexis biographies, Library of Congress Web Archives, authors' manually collected data.*
Gender	The politician's gender. *Source: As above.*
Age	The politician's age. *Source: As above.*
Level of experience	The politician's prior political experience, which takes value of 0 when the politician has immediate prior position (State politics experience = 1), 1 – the politician has prior experience only in the House (but not state politics or the Senate) (House experience = 1), and 2 – the politician has prior experience in the Senate (Senate experience = 1). *Source: As above.*
Vote shares	The vote share between the top two candidates (ignoring all other candidates' votes). *Source: Federal Election Commission (FEC).*
House/Senate	Indicator of whether the race is for House of Representatives or Senate. *Source: FEC.*
Incumbency	Indicator of whether the politician is the incumbent candidate. *Source: FEC.*
Party affiliation	The politician's party affiliation. *Source: FEC.*
Campaign contribution	Total campaign contribution (in dollar value) that the politician receives. *Source: FEC*
Number of contributors	Total number of contributors towards the politician's campaign. *Source: FEC.*
Media mention	The number of search hits for the politician's name on his state's newspapers based on Newslibrary.com, normalized by the number of search hits for the neutral keyword "September". To avoid misclassification, we pay particular attention to politicians having common first and last names to avoid false positive search hits, as done in Campante and Do (2014). *Source: http://www.newslibrary.com.*
	Director variables
Educational background	BoardEx provides information on directors' attained undergraduate and graduate degrees, the years in which those degrees were awarded, and the awarding institutions. *Source: BoardEx.*
Gender	The director's gender. *Source: BoardEx.*
Age	The director's age. *Source: BoardEx.*

Executive director	Indicator of whether director has an executive role. *Source: BoardEx.*
Tenure	The director's tenure in the firm. *Source: BoardEx.*
	State variables
Craigslist's penetration	The share of counties in each state where Craigslist has entered by November of two years before the election year. Two measures are calculated: one based on the first day where the website became available in scraped data, and another based on Craigslist's official records. *Source: Djourelova et al. (2023).*
Craigslist's presence	The indicator whether Craigslist has entered into any county in a state by November of two years before the election year. Two measures are calculated: one based on the first day where the website became available in scraped data, and another based on Craigslist's official records. *Source: As above.*
Capital primacy	The ratio of the state capital's population over the state's population, based on the 1980 census. *Source: U.S. Census 1980.*
Average logarithm of distance (ALD)	ALD is calculated as the average of the natural logarithm of the distance from a state's inhabitants to its capital city in 1980. *Source: Campante and Do (2014).*
State election turnout	The average voter turnout rate in state elections over 2000-2008 minus average turnout rate in presidential elections in 2000, 2004, and 2008 (each rate is normalized by the state's voting-age population based on the U.S. census). *Source: David Leip's Atlas of U.S. Presidential Elections, http://www.uselectionatlas.org, U.S. Census.*
Political interest	The share of answers to the question *"How much would you say that you personally care(d) about the way the election to the Congress came out?"* as "very much" or "pretty much", as opposed to "not very much" or "not at all", averaged for each state over 2000-2008. *Source: American National Election Studies (ANES).*
Media exposure	The share of respondents following election news via television, newspaper, or radio, averaged for each state over 2000-2008. *Source: ANES.*
Corrupt main city	The number of search hits for the term "corruption" near the name of the main city in each state gathered in on Exalead.com, normalized by the number of search hits for the name of that main city in 2009 (Saiz and Simonsohn, 2013). *Source: http://www.exalead.com/search.*
Corrupt state	The number of search hits for the word "corruption" close to the state name based on all newspapers based on Newslibrary.com, normalized the resulting number of search hits by that for the state name alone in 2009 (Campante and Do, 2014). *Source: http://www.newslibrary.com.*
Conviction cases	The number of federal convictions for public corruption between 1976 and 2002, normalized by average population in the corresponding state during the same period, as used in Glaeser and Saks (2006). *Source: Department of Justice.*
Regulation	State-level regulation index as used in Glaeser and Saks (2006). It combines information on labor and environmental regulations and regulations in specific industries such as insurance, measured in 1999. *Source: Clemson University's Report on Economic Freedom, http://www.freedom.clemson.edu.*
Generalized trust	The share of answers to the standard trust question *"Generally speaking, would you say that most people can be trusted, or that you can't be too careful in dealing with people?"* as "most people can be trusted", as opposed to "can't be too careful" or "other, depends"), averaged for each state over 2000-2008. *Source: ANES.*
	Firm and stock variables
Cumulative Abnormal Return (CAR)	CARs are calculated as cumulation of Abnormal Returns (ARs) in specific windows, with the benchmark window (-1,5) counts from 1 day before to 5 days after the election day (day 0). ARs are estimated from a market model of return prediction using daily data from day -315 to day -61. CAR-FF uses the Fama-French (Fama and French, 1993) three-factor model instead. CAR-FFM uses the Fama-French plus momentum four-factor model instead (Carhart, 1997). *Source: CRSP, Fama and French (1993), Carhart (1997).*
Standardized CAR (SCAR)	SCARs are CARs normalized by volatility during the event period. *Source: CRSP.*
Abnormal trading volume	Abnormal trading volumes are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61 (Campbell and Wasley, 1996). *Source: CRSP.*
Market value of equity	Market value of total equity (CSHO $\times$ PRCC_F). *Source: CRSP.*
Common equity	Book value of common equity (CEQ). *Source: Compustat.*
Market to book ratio	Market value of total equity (CSHO $\times$ PRCC_F)/book value of common equity (CEQ). *Source: Compustat.*
Firm age	The number of years from IPO or the start of Compustat coverage. *Source: Compustat.*
Total assets	The firm's total assets (AT). *Source: Compustat.*
Total sales	The firm's total sales (SALE). *Source: Compustat.*

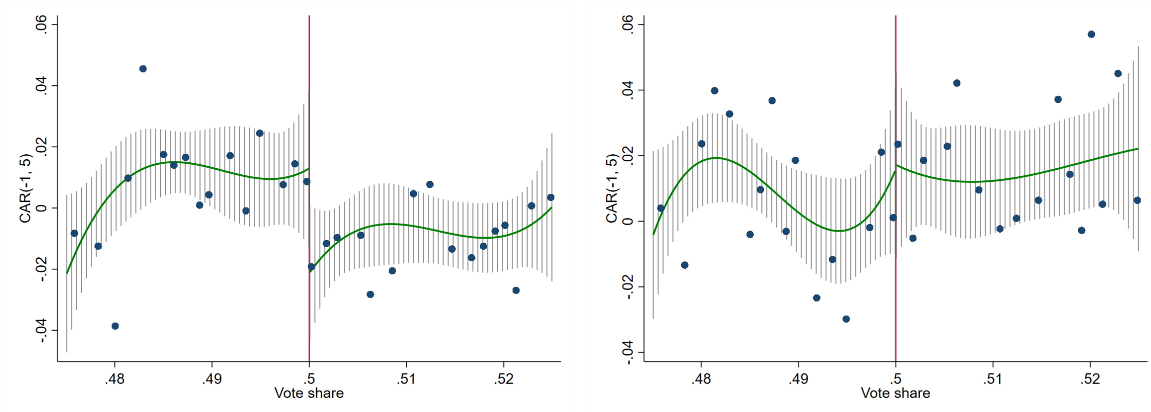
Total employment	The firm's total employment (EMP). *Source: Compustat.*
Capital expenditure	Capital expenditure (CAPX)/total assets (AT). *Source: Compustat.*
Return on asset (ROA)	Income before extraordinary items (IB)/total assets (AT) at $t-1$. *Source: Compustat.*
Book leverage ratio	Book value of debts (DLC + DLTT)/book value of total assets (DLC + DLTT + CEQ). *Source: Compustat.*
Tobin's Q	Total assets (AT) - total shareholder's equity (SEQ) + market value of total equity (CSHO $\times$ PRCC_F)/total assets. *Source: Compustat.*
Board size	The number of directors on the firm's board. *Source: BoardEx.*
Institutional block shares	The fraction of institutional shareholding. *Source: Thomson Reuters.*
Local firm	Indicates whether a firm's headquarter is in the politician's state or within 500 kilometers of the state's capital. *Source: BoardEx.*
Distance to state capital	Geodesic distance between the firm's headquarter ZIP code and election state's capital. *Source: BoardEx.*
Distance to Washington D.C.	Geodesic distance between the firm's headquarter ZIP code and Washington D.C. *Source: BoardEx.*
Local media presence	The number of search hits for the firm's name in the state's local newspaper based on Newslibrary.com, normalized by the number of search hits for the neutral keyword "September." *Source: http://www.newslibrary.com.*

Figure A1: Sensitivity Tests Using Alternative Sample Restrictions



Notes: This figure plots RDD estimates of firms' differential value of Congress connection, as well as their 95% confidence intervals, for different values of the bandwidth used in the RDD specification in equation (1).

Figure A2: Discontinuity of Market Reaction with Cubic Function Controls

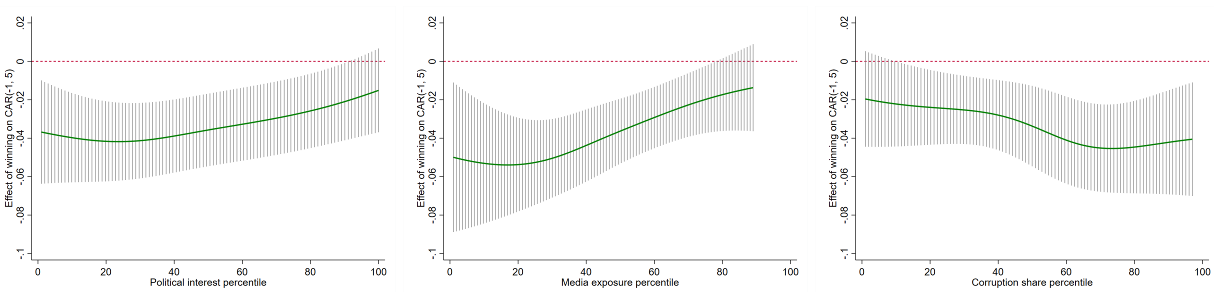


A. CAR(-1, 5)

B. CAR(-7, -1)

Notes: This RDD figure plots connected firms' cumulative abnormal returns (CARs) against the connected politician's vote share around the 50% threshold, including separately fitted cubic functions of vote share on either side of the threshold (Equation (1)) and their 95% confidence intervals. **Subfigure A** shows the estimated discontinuity of -3.4% on CARs between days -1 and 5 around the election. **Subfigure B** shows balanced CARs before the election between days -7 and -1. 16 dots on each side of the threshold represent approximately equal-sized bins of close elections.

Figure A3: Effect by Voters' Political Interest and State's Corruption Level



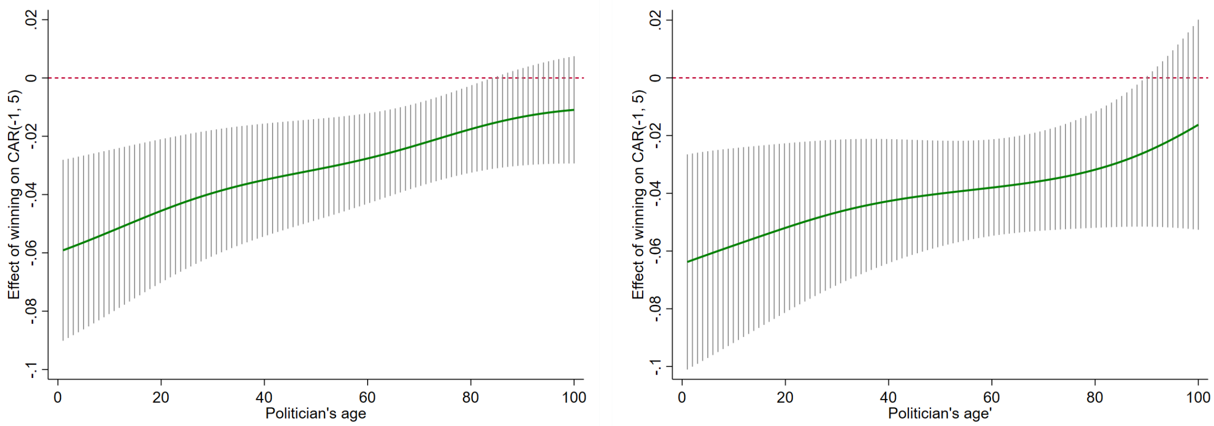
A. Voters' political interest

B. Voters' election media exposure

C. State's corruption level

Notes: This figure plots semi-parametric estimates of the differential value of Congress-level connection to firms ΔV as a function of percentiles of X-axis variables together with their 95% confidence intervals. In **Subfigure A**, the X-axis variable is the share of respondents by state with strong interest in election outcomes. In **Subfigure B**, the X-axis variable is the share of respondents following election news on television, newspaper, or radio. Both of those measures are from the American National Election Studies over 2000-2008. In **Subfigure C**, the X-axis variable is the number of search hits on Exalead.com for the term "corruption" near the name of the main city in each state, normalized by the number of search hits for the name of that main city. The point estimate at each value of the X-axis variable is obtained from the baseline RDD regression in equation (1), weighted by a Gaussian kernel function of the percentile on the X-axis with a bandwidth equal to 20% (details in Appendix B.3). Standard errors are clustered by politician.

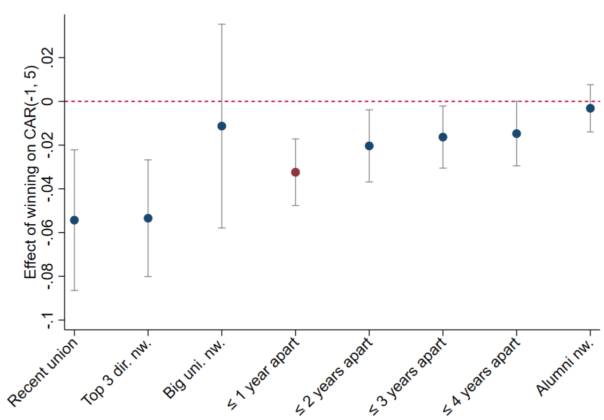
Figure A4: Effect by Politician's Age



A. All politicians B. Challengers

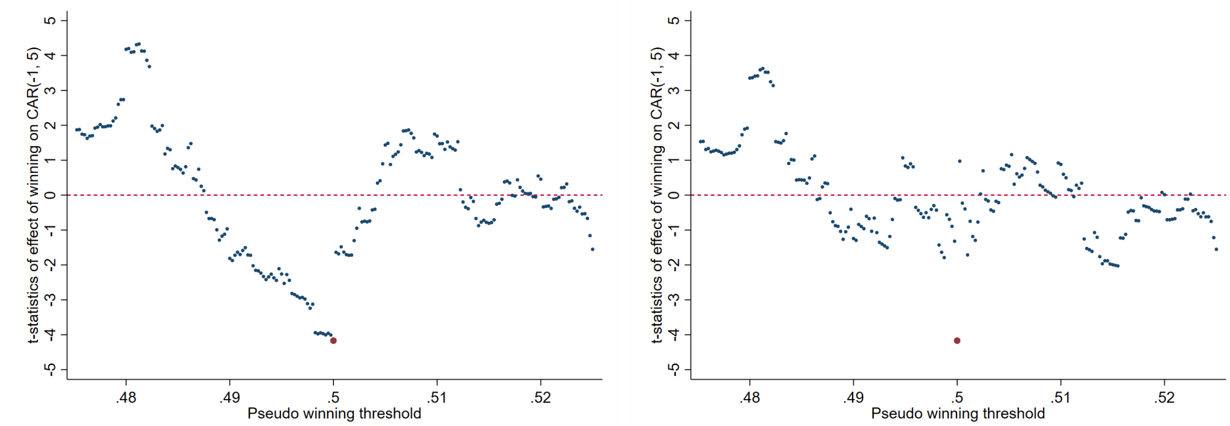
Notes: This figure plots semi-parametric estimates of the differential value of Congress-level connection to firms ΔV as a function of the connected politician's age percentile on the X-axis, together with their 95% confidence intervals. **Subfigure A** includes all politicians in the baseline sample and **Subfigure B** includes challenger candidates. The point estimate at each value of politician's age is obtained from the baseline RDD regression in equation (1), weighted by a Gaussian kernel function of politician's age percentile with a bandwidth of 20% (details in Appendix B.3), among the subsample of challengers. The X-axis shows ages corresponding to each age quintiles. Standard errors are clustered by politician.

Figure A5: Effect by Strength and Precision of Connection



Notes: This figure plots estimates of the adverse effect of Congress connections based on various definitions of connections. The three left-most estimates come from restricted samples in years of recent alumni reunions, among top-3 universities in terms of directors network size, and among big universities. The five right-most estimates consider gradually relaxing the network definition in terms of years apart, with the middle estimate being the benchmark result from Table 2. Standard errors are clustered by politician.

Figure A6: RDD ESTIMATES WITH HYPOTHETICAL THRESHOLDS

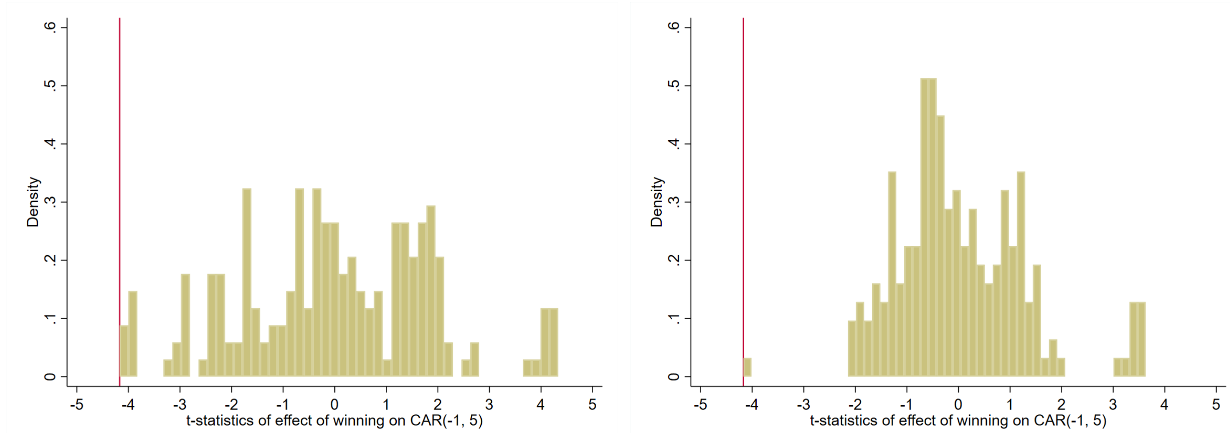


A. Rolling sample without truncation

B. Truncated rolling sample

Notes: This figure reports t-statistics of the main specification applied to alternative (placebo) thresholds of $VoteShare$ from 47.5% to 52.5%, using an enlarged sample of $VoteShare$ from 45% to 55%. In Panel A, for each estimation, we further truncate the sample so that it does not extend past the true 50% threshold. Panel B relaxes this restriction of non-intersection with the 50% threshold.

Figure A7: DISTRIBUTIONS OF RDD ESTIMATES WITH HYPOTHETICAL THRESHOLDS

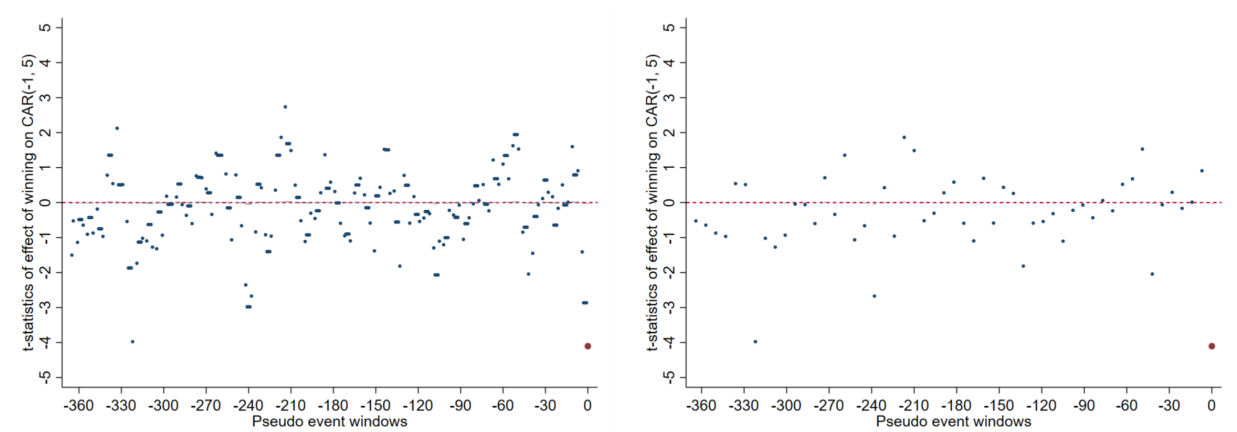


A. Rolling sample without truncation

B. Truncated rolling sample

Notes: This figure reports distributions of t-statistics of the main specification applied to alternative (placebo) thresholds of $VoteShare$ from 47.5% to 52.5%, using an enlarged sample of $VoteShare$ from 45% to 55%. In Panel A, for each estimation, we further truncate the sample so that it does not extend past the true 50% threshold. Panel B relaxes this restriction of non-intersection with the 50% threshold.

Figure A8: RDD estimates with hypothetical event windows

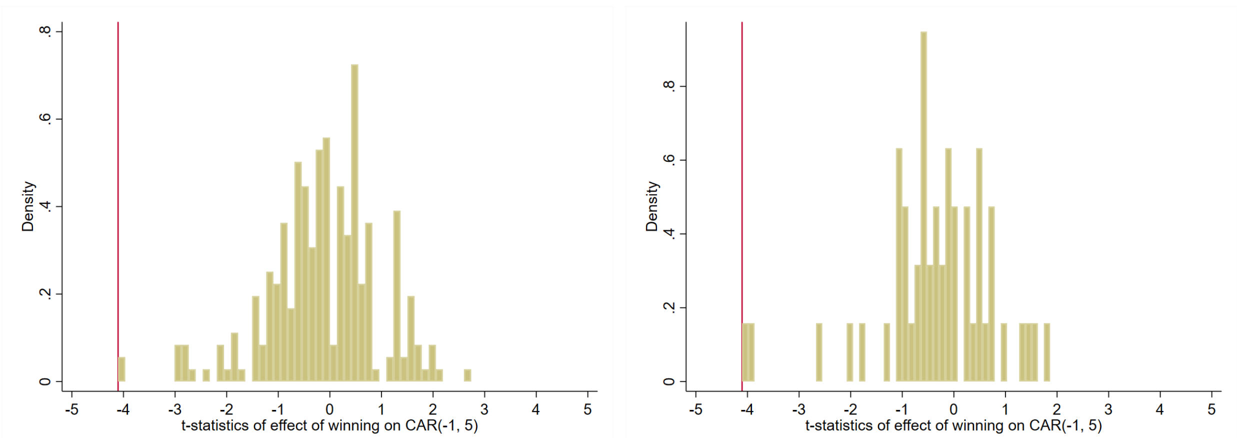


A. Hypothetical event days

B. Hypothetical event Tuesdays

Notes: This figure reports t-statistics of the main specification applied to alternative (placebo) event days over the period of a year prior to the true event day. Panel A considers all alternative days, while Panel B shows all Tuesdays (as the election day).

Figure A9: Distribution of RDD estimates with hypothetical event windows



A. Hypothetical event days

B. Hypothetical event Tuesdays

Notes: This figure reports the distributions of t-statistics of the main specification applied to alternative (placebo) event days over the period of a year prior to the true event day. Panel A considers all alternative days, while Panel B shows all Tuesdays (as the election day).

Table A2: Distribution of Degree Program and Graduation Year

Degree program	Politicians	Directors	Conn. pairs	Graduation year	Politicians	Directors	Conn. pairs
Business school	5.6%	4.8%	4.4%	< 1950	0.5%	0.4%	0.5%
Medical school	0.5%	0.1%	0.1%	1950-1959	5.7%	3.7%	3.5%
General graduate	8.0%	3.2%	3.0%	1960-1969	24.6%	37.2%	37.7%
Ph.D.	1.4%	0.5%	0.5%	1970-1979	42.2%	41.5%	40.2%
Law school	12.2%	3.9%	3.7%	1980-1989	20.4%	15.0%	14.8%
Undergraduate	72.3%	87.4%	88.3%	≥ 1990	6.6%	2.2%	3.4%

Notes: This table reports the distribution of degree program and graduation year among connected politician-director pairs in our baseline sample. A politician and a director are considered connected if they graduated from the same university, campus, and degree program combination within one year of each other. All academic degrees are classified into one of the above six program categories, following Cohen et al. (2008).

Table A3: Baseline Firms' Characteristics Compared to Compustat Firms

Sample	Baseline sample			Compustat universe		
	Mean	Median	Std. dev.	Mean	Median	Std.dev.
Firm's age (year)	18.88	13.00	15.58	15.30	11.00	13.16
Market value ($ million)	5,810	589.1	25,336	3,548	290.1	16,661
Common equity ($ million)	2,062	247.8	8,434	1,347	127.2	6,301
Market-to-book ratio	3.381	2.023	14.72	4.684	1.950	92.31
Total assets ($ million)	12,689	764.5	91,372	8,141	379.9	70,219
Sales ($ million)	4,033	446.0	14,420	2,627	188.5	11,976
Employment (thousand)	13.90	1.546	53.41	9.080	0.775	38.09
Capital expenditure/assets	236.0	14.79	983.9	187.9	7.743	1,040
Return on assets (%)	-6.052	2.494	41.54	-4.976	1.612	49.54
Book leverage ratio	0.307	0.343	2.391	0.344	0.301	10.80
Tobin's Q	2.007	1.401	1.909	2.422	1.394	4.623

Notes: This table reports the characteristics of the 1,234 firms in our baseline sample (weighted by observation count) and compares them to firms in the Compustat universe (which include all firms within Compustat in 2000, 2002, 2004, 2006, and 2008).

Table A4: RDD Randomness Checks

Panel A. Politician characteristics

Sample		Politician × Election year				Baseline			
Dependent variable		Winner	S.E.	*Mean*	Obs.	Winner	S.E.	*Mean*	Obs.
1	Indicator (I): Gender = Male	0.084	(0.116)	*0.785*	186	0.135	(0.126)	*0.840*	1,714
2	Age at election year (year)	-1.253	(2.293)	*52.89*	186	3.278	(2.206)	*54.66*	1,714
3	I: Attended brand-name university	-0.012	(0.120)	*0.237*	186	-0.139	(0.234)	*0.478*	1,714
4	I: Senate election candidate	0.065	(0.116)	*0.210*	186	0.077	(0.237)	*0.318*	1,714
5	I: Incumbent candidate	-0.080	(0.137)	*0.387*	186	-0.233	(0.195)	*0.346*	1,714
6	I: Party affiliation = Democrat	0.016	(0.138)	*0.516*	186	0.278	(0.191)	*0.585*	1,714
7	I: Same party as chamber majority	0.189	(0.143)	*0.489*	186	-0.069	(0.226)	*0.488*	1,714
8	I: Same party as presidency	0.077	(0.142)	*0.483*	186	-0.079	(0.195)	*0.396*	1,714
9	I: Experience in state politics	-0.173	(0.137)	*0.323*	186	-0.226	(0.198)	*0.335*	1,714
10	I: Experience in Congress	-0.056	(0.141)	*0.430*	186	-0.212	(0.197)	*0.372*	1,714
11	Local media presence in election year	-0.005	(0.076)	*0.144*	186	-0.052	(0.057)	*0.149*	1,714
12	Total campaign contribution ($ million)	-0.494	(0.822)	*2.246*	186	-0.200	(1.614)	*2.667*	1,714
13	Number of contributors	-110.2	(130.4)	*586.6*	186	-383.6*	(201.9)	*581.6*	1,714
14	Number of connected directors	2.567	(2.255)	*7.183*	186	3.169	(5.420)	*16.01*	1,714
15	Number of connected firms	4.162	(2.950)	*9.070*	186	6.871	(7.449)	*21.42*	1,714

Panel B. Director characteristics

Sample		Director × Politician × Year				Baseline			
Dependent variable		Winner	S.E.	*Mean*	Obs.	Winner	S.E.	*Mean*	Obs.
16	I: Gender = Male	-0.009	(0.039)	*0.912*	1,336	-0.019	(0.043)	*0.899*	1,714
17	Age at election year (year)	3.446	(2.176)	*54.28*	1,336	3.307	(2.079)	*54.50*	1,714
18	Number of years since graduation	3.536	(2.221)	*31.69*	1,336	3.548	(2.227)	*31.88*	1,714
19	I: Link via big-name university	-0.082	(0.221)	*0.418*	1,336	-0.101	(0.229)	*0.436*	1,714
20	I: Link via big-size university	0.084	(0.098)	*0.161*	1,336	0.054	(0.101)	*0.156*	1,714
21	I: Link via undergraduate program	0.014	(0.063)	*0.873*	1,336	0.040	(0.072)	*0.872*	1,714
22	Number of related firms	0.144	(0.081)	*1.283*	1,336	0.643*	(0.330)	*1.680*	1,714
23	I: Executive director (avg.)	-0.062	(0.053)	*0.204*	1,336	-0.077	(0.049)	*0.176*	1,714
24	Tenure in firm at election year (avg.)	-0.826	(0.744)	*4.632*	1,336	-0.716	(0.710)	*4.519*	1,714

Panel C. State characteristics

Sample		State × Politician × Year				Baseline sample			
Dependent variable		Winner	S.E.	*Mean*	Obs.	Winner	S.E.	*Mean*	Obs.
25	I: Craigslist's presence 2 years prior	0.047	(0.139)	*0.382*	186	0.212	(0.200)	*0.394*	1,714
26	Craigslist's penetration 2 years prior	0.035	(0.049)	*0.143*	186	0.056	(0.058)	*0.149*	1,714
27	Average log distance to capital city	-0.029	(0.026)	*0.298*	183	0.027	(0.041)	*0.301*	1,675
28	Difference in voter turnouts	-0.007	(0.010)	*0.179*	163	-0.017	(0.014)	*0.182*	1,540
29	Voters' political interest	0.010	(0.023)	*1.674*	183	0.048	(0.033)	*1.676*	1,675
30	Voters' election media exposure	0.002	(0.004)	*0.974*	183	0.003	(0.004)	*0.974*	1,675
31	State's corruption level	0.184*	(0.104)	*0.262*	186	0.158	(0.171)	*0.231*	1,714
32	State's regulation index in 1999	0.050	(0.135)	*6.148*	186	-0.102	(0.194)	*6.157*	1,714
33	State's generalized trust level	0.006	(0.036)	*0.481*	183	-0.031	(0.051)	*0.476*	1,675

Panel D. Firm characteristics

Sample		Firm × Politician × Year				Baseline			
Dependent variable		Winner	S.E.	*Mean*	Obs.	Winner	S.E.	*Mean*	Obs.
34	Age at election year (year)	2.404	(1.745)	*18.89*	1,681	2.540	(1.744)	*18.88*	1,708
35	Lagged market value ($ billion)	4.162	(2.937)	*6.008*	1,687	4.105	(2.870)	*5.924*	1,714
36	Lagged common equity ($billion)	1.240	(0.887)	*1.957*	1,687	1.228	(0.864)	*1.931*	1,714
37	Lagged market-to-book ratio	0.879	(2.009)	*2.727*	1,576	1.039	(1.949)	*2.753*	1,603
38	Lagged total assets ($ billion)	3.139	(8.212)	*11.01*	1,687	3.192	(8.019)	*10.86*	1,714
39	Lagged total sales ($ billion)	2.937	(1.898)	*3.690*	1,687	2.955	(1.844)	*3.652*	1,714
40	Lagged total employment (thousand)	0.565	(7.080)	*13.91*	1,610	0.810	(6.910)	*13.77*	1,637
41	Lagged capital expenditure/assets	0.003	(0.006)	*0.044*	1,564	0.002	(0.007)	*0.044*	1,589
42	Lagged return on assets	-0.030	(0.038)	*-0.042*	1,636	-0.037	(0.038)	*-0.043*	1,663
43	Lagged book leverage ratio	0.018	(0.111)	*0.367*	1,630	-0.019	(0.108)	*0.367*	1,657
44	Lagged Tobin's Q	0.238	(0.361)	*2.371*	1,576	0.292	(0.361)	*2.379*	1,603
45	Lagged board size	0.065	(0.577)	*9.467*	1,148	0.027	(0.579)	*9.450*	1,165
46	Lagged institutional block shares	0.005	(0.022)	*0.227*	1,005	0.006	(0.022)	*0.227*	1,018
47	Local media presence in election year	0.015	(0.042)	*0.056*	1,677	0.014	(0.042)	*0.056*	1,704
48	I: Local firm	-0.127	(0.089)	*0.255*	1,687	-0.133	(0.091)	*0.258*	1,714
49	Distance to state capital (km)	117.1	(182.5)	*1,511*	1,687	142.1	(182.8)	*1,502*	1,714
50	Distance to Washington D.C. (km)	523.6	(407.7)	*1,251*	1,648	488.3	(410.2)	*1,250*	1,675
51	Number of connected directors	-0.306*	(0.183)	*1.132*	1,687	-0.299*	(0.180)	*1.130*	1,714

Notes: This table reports the differences between closely elected and defeated candidates and between their connected directors, firms, and states, using the baseline RDD specification in equation (1) with different dependent variables.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table A5: Robustness Checks for Main Effect

Panel A. Alternative specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable: CAR(-1, 5)							
Specification	Alternative clusterings			Alt. obs. unit	Alternative kernels & samples			
Winner	-0.032***	-0.032***	-0.032***	-0.031***	-0.026***	-0.026***	-0.028***	-0.026***
	(0.008)	(0.010)	(0.008)	(0.008)	(0.009)	(0.009)	(0.011)	(0.009)
Clustering scheme	State × Yr.	Firm	Two-way					
Observation unit				Pol. × Firm				
Kernel function					Tri	Epa	Tri	Epa
Sample selection							CCT	CCT
Observations	1,714	1,714	1,714	1,687	1,714	1,714	559	1,714
Politicians	165	165	165	165	165	165	66	165
Directors	1,136	1,136	1,136	1,122	1,136	1,136	415	1,136
Firms	1,234	1,234	1,234	1,234	1,234	1,234	481	1,234

Panel B. Alternative CAR models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	**SCAR(-1, 5)**		**CAR(-1, 5)**					
Model	Baseline MM		Raw		Fama-French 3 factors		4 factors	
Winner	-0.398***	-0.512***	-0.026	-0.052**	-0.028***	-0.031***	-0.032***	-0.037***
	(0.116)	(0.150)	(0.019)	(0.023)	(0.007)	(0.011)	(0.008)	(0.011)
University FEs		X		X		X		X
Observations	1,714	1,714	1,714	1,714	1,714	1,714	1,714	1,714
Politicians	165	165	165	165	165	165	165	165
Directors	1,136	1,136	1,136	1,136	1,136	1,136	1,136	1,136
Firms	1,234	1,234	1,234	1,234	1,234	1,234	1,234	1,234

Notes: This table reports the robustness checks for the benchmark average differential value of Congress-level connection to firms ΔV, which is estimated using the baseline RDD specification in equation (1) and reported in column (1) of Table 2. **Panel A:** Columns (1) to (3) cluster standard errors (i) by state -by-election year, (ii) by firm, and (iii) two-way by politician and firm respectively. Each observation in column (4) is a combination of politician p, connected firm f, and election year t. Columns (5) and (6) use triangle and Epanechnikov kernel weights, and columns (7) and (8) use samples selected by Calonico et al.'s (2014) method with triangle and Epanechnikov kernel weights respectively. **Panel B:** Columns (1) and (2)'s use standardized CARs (CARs normalized by volatility during the event period) computed using the baseline market model as the dependent variable. Columns (3) and (4) use raw returns. Columns (5) and (6) use CARs computed based on the Fama and French's (1993) three-factor model. Columns (7) and (8) use CARs based on Fama and French's (1993) plus Carhart's (1997) momentum four-factor models. Columns (2), (4), (6), and (8) additionally include university fixed effects. Standard errors are clustered by politician unless noted otherwise.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table A6: Greater Scrutiny of Winners After Election

Panel A.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: Change in media mention (-1, 1)						
Politician sample	All winners	Challenger winners	Incumbent winners	All losers	Challenger losers	Incumbent losers	All candidates
Mean	0.036***	0.056***	0.002	-0.037***	-0.014**	-0.071***	
	(0.009)	(0.014)	(0.006)	(0.011)	(0.006)	(0.026)	
Winner							0.111***
							(0.029)
Difference		*0.055****			*0.057***		
		(0.015)			*(0.026)*		
Observations	100	63	37	86	51	35	186
Politicians	93	63	32	84	50	35	165

Panel B.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable: Media mention in local newspapers							
Time period	Year -1	Year 0	Year 1	Year 2	$\Delta(-1, 1)$		Δ(pre, post)	
Politician sample	All politicians				Challengers	Incumbents	Challengers	Incumbents
Winner	-0.014	-0.005	0.098*	0.078*	0.093**	0.122***	0.074***	0.112**
	(0.050)	(0.076)	(0.053)	(0.044)	(0.033)	(0.044)	(0.027)	(0.050)
Observations	186	186	186	186	114	72	114	72
Politicians	165	165	165	165	110	64	110	64

Notes: This table reports changes in media attention on a candidate after his election, and compares those changes between winners and losers. Media attention is measured by the normalized hit rate from a search for the politician in local newspapers based on Newslibrary.com. Each observation is a politician p in election year t (politician p is a close-election top-two candidate in election year t). All standard errors are clustered by politician. **Panel A** reports the average change in media mention of the politician between year 1 and year -1, separately for winner and losers. Columns (1) to (3) consider all winners, challenger winners, and incumbent winners, respectively. Columns (4) to (6) consider all losers, challenger losers, and incumbent losers, respectively. Column (7) applies equation (1)'s RDD specification on the full sample of all politician-by-election year's, using the same change in media mention of politician as the dependent variable. **Panel B** reports the difference in media mention of elected and defeated politicians before and after the election, using an RDD specification similar to that in equation (1) with media mention of the politician as the dependent variable. Columns (1) to (4) consider media mentions from year -1 to year 2. Columns (5) and (6) consider changes in media mention between year 1 and year -1. Columns (7) and (8) consider changes in media mention between pre-election (years -1 and 0) and post-election (years 1 and 2). Columns (5) and (7) consider challenger politicians and columns (6) and (8) incumbent politicians.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table A7: Effect by Degree of Scrutiny at Different Levels

Panel A. Effect by voters' political interest

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable: CAR(-1, 5)							
Measure of scrutiny	ALD to capital		Voter turnout		Political interest		Media exposure	
State sample	High	Low	Low	High	Low	High	Limited	Strong
Winner	-0.038***	-0.029***	-0.044***	-0.023*	-0.042***	-0.027**	-0.057***	-0.022**
	(0.013)	(0.010)	(0.012)	(0.014)	(0.013)	(0.011)	(0.015)	(0.009)
Difference	*-0.010*		*-0.021*		*-0.015*		*-0.035***	
	(0.016)		*(0.018)*		*(0.016)*		*(0.017)*	
Observations	872	803	737	803	820	855	820	855
Politicians	94	68	60	85	80	82	86	76
Directors	621	540	511	560	574	598	565	602
Firms	722	635	605	659	673	703	659	702

Panel B. Effect by state's corruption level

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: CAR(-1, 5)					
Proxy for corruption	Search hits w. city name		Search hits w. state name		Conviction cases	
State sample	High	Low	High	Low	High	Low
Winner	-0.053***	-0.017*	-0.049***	-0.021**	-0.043***	-0.025**
	(0.014)	(0.010)	(0.013)	(0.010)	(0.013)	(0.010)
Difference	*-0.036***		*-0.028**		*-0.018*	
	(0.017)		*(0.016)*		*(0.016)*	
Observations	855	859	829	885	818	896
Politicians	98	67	91	74	88	77
Directors	602	589	592	618	584	598
Firms	681	709	679	730	674	708

Notes: **Panel A** reports how firm's differential value of Congress-level connection ΔV varies by the degree of scrutiny in state politics (γ_1) and federal politics (γ_2) measured in each politician's home state, using the baseline RDD specification in equation (1). Columns (1) and (2) compare subsamples of states with above and below median Average Log Distance (ALD) to state capital city in 1980 (Campante and Do, 2014). High ALD implies low γ_1. Columns (3) and (4) compare subsamples of states with above and below median average voter turnout in state elections (minus turnout in presidential elections). Low state-election turnout implies low γ_1. Columns (5) and (6) compare subsamples of states with below and above median level of political interest (share of responses of strong interest in election outcome, from ANES). Low level of political interest implies small γ_1 and γ_2. Columns (7) and (8) compare subsamples of states with below and above median in media exposure around election time (share of respondents following election news via television, newspaper, or radio, from ANES). Limited media exposure implies small γ_1 and γ_2. **Panel B** reports how the differential value of Congress-level connection to firms ΔV varies by the degree of state corruption level, using the baseline RDD specification in equation (1). High corruption level implies small γ_1 and γ_2. Columns (1) and (2) measure corruption based on the number of search hits on Exalead.com for the term "corruption" near the name of the main city in each state, normalized by the number of search hits for the name of that main city. Columns (3) and (4) measure corruption based on the number of search hits on Exalead.com for the term "corruption" near the name of the state, normalized by the number of search hits for the name of that state. Columns (5) and (6) measure corruption based on the number of federal convictions for public corruption between 1976 and 2002, normalized by average population in the corresponding state during the same period (Glaeser and Saks, 2006). All standard errors are clustered by politician.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table A8: Effect by Degree of Scrutiny among Challengers and Incumbents

Panel A. Subsample of challengers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dependent variable: CAR(-1, 5)									
Measure of scrutiny	ALD to capital		Voter turnout		Political interest		Media exposure		Corruption	
State sample	High	Low	Low	High	Low	High	Limited	Strong	High	Low
Winner	-0.050***	-0.039***	-0.047***	-0.036**	-0.043***	-0.042***	-0.064***	-0.035***	-0.060***	-0.028**
	(0.017)	(0.012)	(0.013)	(0.016)	(0.013)	(0.013)	(0.019)	(0.010)	(0.017)	(0.012)
Difference	*-0.011*		*-0.011*		*-0.001*		*-0.029*		*-0.032*	
	(0.021)		*(0.021)*		*(0.018)*		*(0.022)*		*(0.021)*	
Observations	514	578	509	535	498	594	509	583	573	548
Politicians	59	50	40	56	49	60	56	53	61	49
Directors	391	435	362	400	372	453	375	452	413	407
Firms	475	524	445	484	459	540	468	533	490	504

Panel B. Subsample of incumbents

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dependent variable: CAR(-1, 5)									
Measure of scrutiny	ALD to capital		Voter turnout		Political interest		Media exposure		Corruption	
State sample	High	Low	Low	High	Low	High	Limited	Strong	High	Low
Winner	-0.036	-0.008	-0.016	0.007	-0.043	-0.001	-0.040*	0.002	-0.044**	0.010
	(0.028)	(0.016)	(0.035)	(0.023)	(0.029)	(0.013)	(0.023)	(0.018)	(0.023)	(0.020)
Difference	*-0.028*		*-0.023*		*-0.042*		*-0.042*		*-0.054**	
	(0.032)		*(0.042)*		*(0.032)*		*(0.029)*		*(0.031)*	
Observations	358	225	228	268	322	261	311	272	282	311
Politicians	36	26	22	34	34	28	34	28	39	25
Directors	259	173	168	199	241	191	238	195	209	233
Firms	317	205	205	241	292	237	286	238	243	289

Notes: This table replicates the columns in Table A7 for two partitioned subsamples, that of challengers in **Panel A** and that of incumbents in **Panel B**. The coefficients show how firm's differential value of Congress-level connection ΔV varies by the degree of scrutiny in state politics (γ_1) and federal politics (γ_2) measured in each politician's home state, using the baseline RDD specification in equation (1). Columns (1) and (2) compare subsamples of states with above and below median Average Log Distance (ALD) to state capital city (Campante and Do, 2014). High ALD implies low γ_1. Columns (3) and (4) compare subsamples of states with above and below median average voter turnout in state elections (minus turnout in presidential elections). Low state-election turnout implies low γ_1. Columns (5) and (6) compare subsamples of states with below and above median level of political interest (share of responses of strong interest in election outcome, from ANES). Low level of political interest implies small γ_1 and γ_2. Columns (7) and (8) compare subsamples of states with below and above median in media exposure around election time (share of respondents following election news via television, newspaper, or radio, from ANES). Limited media exposure implies small γ_1 and γ_2. Columns (9) and (10) compare subsamples of states with above and below corruption level, measured as the number of search hits on Exalead.com for the term "corruption" near the name of the main city in each state, normalized by the number of search hits for the name of that main city. High corruption level implies small γ_1 and γ_2. All standard errors are clustered by politician.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table A9: Effect by Politician's Age among All Politicians

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: CAR(-1, 5)						
Politician sample	All	Below med.	Above med.	Age Q1	Age Q2	Age Q3	Age Q4
Winner	-0.031***	-0.044***	-0.014*	-0.059**	-0.037**	-0.017	-0.010
	(0.008)	(0.011)	(0.008)	(0.022)	(0.015)	(0.013)	(0.012)
W × (Pol. Age - 56)	0.002**						
	(0.001)						
Difference		*-0.030***					
		(0.014)					
Observations	1,714	961	753	432	529	343	410
Politicians	165	115	55	66	52	20	37
Directors	1,136	691	497	331	377	215	290
Firms	1,234	780	601	382	445	280	354

Notes: This table reports how the differential value of Congress-level connection to firms ΔV varies by the politician's age, using the baseline RDD specification in equation (1), for the full baseline sample. Column (1) interacts the treatment (i.e., winning the election) with the politician's age (relative to the median of 56). Columns (2) and (3) compare subsamples of younger (at most 56) and older (above 56) politicians. Columns (4) to (7) consider the subsamples of politicians in age quartile 1 to 4 as determined with respect to the full baseline sample. All standard errors are clustered by politician.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table A10: Effect in Different Politician Subsamples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable: CAR(-1, 5)							
	Election type		Politician type		Party affiliation		President's party	
Politician sample	House	Senate	Chal-lengers	Incum-bents	Democrat	Republican	Different	Same
Winner	-0.028***	-0.046***	-0.043***	-0.013	-0.026**	-0.037***	-0.029***	-0.034**
	(0.011)	(0.011)	(0.009)	(0.014)	(0.012)	(0.012)	(0.010)	(0.014)
Observations	1,169	545	1,121	593	1,003	711	1,036	678
Politicians	129	36	110	64	88	77	89	80
Directors	802	376	801	440	701	500	717	502
Firms	906	456	922	517	805	609	834	598

Notes: This table reports the differential value of Congress-level connection to firms ΔV using the baseline RDD specification in equation (1). Columns (1) and (2) consider subsamples of House and Senate elections. Columns (3) and (4) compare challengers and incumbents. Columns (5) and (6) show results with Democrat and Republican politicians. Columns (7) and (8) compare politicians belonging and not belonging to the same party as the contemporaneous President.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table A11: Effect by Firm Size and State-Level Regulations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable: CAR(-1, 5)							
Firm/state sample	All firms	Very large firms	Smaller firms	Local firms	All states	High reg. states	Low reg. states	Local firms
Winner	-0.031***	0.011	-0.038***	-0.047**	-0.034***	-0.044***	-0.023**	-0.045**
	(0.008)	(0.014)	(0.009)	(0.022)	(0.008)	(0.011)	(0.009)	(0.022)
W × ln(Market value)	0.012**							
	(0.005)							
W × State regulation index					-0.044**			-0.083*
					(0.017)			(0.050)
Difference		*0.049****				*-0.021*		
		(0.016)				*(0.014)*		
Observations	1,714	194	1,520	443	1,714	861	853	443
Politicians	165	73	165	114	165	86	79	114
Directors	1,136	142	1,059	352	1,136	617	597	352
Firms	1,234	131	1,116	368	1,234	711	712	368

Notes: This table reports how the differential value of Congress-level connection to firms ΔV varies by the benefits of state- (β_1) and federal-level (β_2) connection to the firm, using the baseline RDD specification in equation (1). Column (1) interacts the treatment (i.e., being connected to a winning candidate) with ln(median-centered market value), so that the coefficient of Winner reflects the effect on the median firm size. Columns (2) and (3) compare subsamples of very large firms and smaller ones, distinguished at the threshold of market value above the median of S&P 500 firms; very large firms likely have large β_2. Column (4) considers the subsample of local firms. A firm is classified as local if its headquarter is in the politician's state or within 500 kilometers of the state's capital; local firms likely have large β_1. Column (5) interacts the treatment with the state regulation index in 1999; more state regulations imply large β_1. Columns (6) and (7) compare subsamples of states with above-median and below-median state regulation index. Column (8) interacts the treatment with state regulation index among the subsample of local firms. All standard errors are clustered by politician.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table A12: Effect by Corporate Governance and Relationship Strength

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Board size		Institutional block shares		State's trust level		Reunion year	
Sample	< 10	≥ 10	Large	Small	High	Low	On	Off
Winner	-0.054***	-0.004	-0.047***	0.008	-0.042***	-0.020**	-0.054***	-0.027**
	(0.018)	(0.012)	(0.018)	(0.016)	(0.011)	(0.010)	(0.016)	(0.011)
Difference	*-0.050***		*-0.056***		*-0.022*		*-0.027*	
	(0.024)		*(0.026)*		*(0.015)*		*(0.019)*	
Observations	679	486	502	516	835	840	515	864
Politicians	116	111	120	124	80	82	57	92
Directors	548	368	402	417	611	536	372	589
Firms	574	365	408	406	703	625	457	689

Notes: This table reports how the differential value of Congress-level connection to firms ΔV varies by the firm's ability to extract value from its political connection, using the baseline RDD specification in equation (1). Columns (1) and (2) compare subsamples of firms with board size of below and at least median (10) number of directors; small board size implies large β_1 and β_2. Columns (3) and (4) compare subsamples of firms with at least and below median (20%) institutional block shares; large institutional block shares implies large β_1 and β_2. Columns (5) and (6) compare subsamples of politicians from states with at least and below median generalized trust, calculated as the share of ANES respondents in the state responding positively to the standard trust question during the 2000-2008 period; higher generalized trust implies large β_1 and β_2. Columns (7) and (8) compare subsamples in which the election year coincides or not with the alumni reunion year (if not missing); election in reunion year implies large β_1 and β_2. All standard errors are clustered by politician.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table A13: Effect by School Network Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent variable: CAR(-1, 5)								
Network definition	At most one year apart						Alumni		
Network sample	Harvard	Others	Big network	Others	Ivy League	Others	Harvard	Big network	Ivy League
Winner	-0.065***	-0.029***	-0.053***	-0.029***	-0.039***	-0.029***	-0.021**	-0.020**	-0.011
	(0.021)	(0.008)	(0.014)	(0.008)	(0.012)	(0.011)	(0.010)	(0.008)	(0.008)
Difference	*-0.036*		*-0.025*		*-0.011*				
	(0.022)		*(0.015)*		*(0.016)*				
Observations	205	1,509	336	1,378	658	1,056	5,523	7,088	11,497
Politicians	21	156	25	152	38	145	23	28	44
Directors	141	997	243	895	387	751	795	1,518	2,625
Firms	173	1,099	295	1,000	489	829	1,013	1,653	2,368

Notes: This table reports how the value of Congress-level connection to firms ΔV varies with the university network characteristics, using the baseline RDD specification in equation (1). Columns (1) and (2) compare Harvard and non-Harvard networks. Columns (3) and (4) compare three most represented networks in our director sample (Harvard University, Stanford University, and the University of Pennsylvania) and the remaining networks. Columns (5) and (6) compare Ivy League and non-Ivy League networks. Columns (7) to (9) consider the full alumni network of Harvard University (column 7), column (3)'s top three universities (column 8), and Ivy League schools (column 9). All standard errors are clustered by politician.

*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

B Internet Appendices to be made available online

B.1 Theoretical framework on favoritism and career concerns

In this section we illustrate the trade-off between favoritism benefits and career concerns in a setting when both power to give favors and scrutiny over favoritism matter. We clarify the intuitions and connect the parameters that determine favoritism to testable implications in our empirical RDD framework of close Congress elections. We highlight that the relative balance of power versus scrutiny between high and low positions is the key determinant of the differential value of favoritism between elected and defeated, which is the key estimate in the empirics.

We consider the politician's career dynamic between two stylized types of political positions, namely high versus low, that differ in both the power to favor connected firms and the level of institutional checks and balances over favoritism. Empirically, the high office corresponds to seats in Congress, and the low office to positions outside Congress, with focus on state-level politics.[37]

The politician's career consists of a sequence of positions s in consecutive terms $(s_t)_{t=1,\ldots,T}$: in each term t, $s_t = 2$ (1) designates the high (low) position. The transition matrix $\mathbf{P}_t = [P_{ijt}]_{i,j\in\{1,2\}}$ indicates the probabilities of transition P_{ijt} from state $s_t = i$ in term t to state $s_{t+1} = j$ in term $t+1$. For simplicity, we assume the following functional form, with $\gamma_2 \geq \gamma_1 > 0$ as the marginal costs of favoritism on the politician's future (thus the relative marginal cost $\gamma \overset{def}{\equiv} \frac{\gamma_2}{\gamma_1} \geq 1$).[38]

$$P_{11}(x_1) = \gamma_1 x_1 + P_{11}(0), \quad P_{12}(x_1) = -\gamma_1 x_1 + P_{12}(0) \ \ (= 1 - P_{11}(x_1)),$$
$$P_{21}(x_2) = \gamma_2 x_2 + P_{21}(0), \quad P_{22}(x_2) = -\gamma_2 x_2 + P_{22}(0) \ \ (= 1 - P_{21}(x_2)).$$

The politician chooses career-long sequences of the level of favoritism targeted towards its connected firm $x_{st} \in [0, \bar{x}]$, which produces $v_s(x_{s,t})$ for the firm per term t in state s. The firm's expected present value from the stream of $v_s(x_{s,t})$ is denoted $V_{s,t}$. We further assume a simple proportional sharing rule for the politician's kickback gain of $w_s(x_{st}) = \frac{1}{\rho} v_s(x_{st})$ each term, with the functional forms $w_1(x_1) = \sqrt{\beta_1 x_1}$ and $w_2(x_2) = \sqrt{\beta_2 x_2}$, with $\beta_2 \geq \beta_1 > 0$ as measures of power (thus the relative power $\beta \overset{def}{\equiv} \frac{\beta_2}{\beta_1} \geq 1$).[39] Besides $w_s(x_{st})$, the politician's other benefits from holding position s is denoted r_s, with $r_2 > r_1 > 0$. Those benefits accumulate to the expected present value $W_{s,t}$, which is his maximand.

We now define the firm's and politician's differences in values across positions as in Definition 1, recopied below:

Definition 1 $\Delta V_t \overset{def}{\equiv} V_{2,t} - V_{1,t}$ *is the firm's differential value from its connection to the politician's higher position versus the lower position (in short, the differential value of connection). Analogously,* $\Delta W_t \overset{def}{\equiv} W_{2,t} - W_{1,t}$ *is the politician's differential value.*

ΔV_t is the main focus of our empirical analysis, as changes in V_s naturally maps to observed changes in firm's stock value.

[37]Our dynamic modeling of a politician's career concern with finite horizon follows Barro's (1973) and Becker and Stigler's (1974) tradition. We incorporate the voter's decision problem into a reduced-form negative relationship between favoritism and electoral success.

[38]The transition can be thought of mainly, but not only, as electoral contests, and the transition probabilities as electoral success chances. By definition, $P_{11} + P_{12} = P_{21} + P_{22} = 1$. We further assume $P_{22}(0) > P_{12}(0)$, expressing the incumbency advantage in Congress elections (Erikson, 1971, Lee, 2008).

[39]The functions $w(\cdot)$ and $v(\cdot)$ may represent different forms of benefits, such as the firm's new or better contracts, support for the firm when under financial distress, and illicit private payment or political contribution to the politician. In many cases, favoritism involves favor trading with other political and government actors, which is by nature hard to observe. On this topic, see Karlan et al. (2009) for a model of favor trading on networks, and Do et al. (2017) on favoritism by officials without direct authority through favor trading.

To assure the existence and uniqueness of the equilibrium, we further make the following standard functional form assumptions:

Assumption B.1 *Assume that $w(\cdot)$ and $v(\cdot)$ are increasing, concave, and differentiable, and P_{22} and P_{12} (P_{21} and P_{11}) are decreasing (increasing) convex functions of x.*

The politician's dynamic problem can be written in the following Bellman equations, such that the politician chooses the optimal amounts $x^*_{s,t}$, $s \in \{1,2\}$, to maximize $W_{s,t}$, given the future expected values $W_{s',t+1}$, $s' \in \{1,2\}$, discount factor $\delta \in (0,1)$ and transition probabilities $P_{ss't}(x_{s,t})$.

$$\begin{aligned} W_{1,t} = & \max_{x_{1,t}}[r_1 + w_1(x_{1,t}) + \delta P_{11,t}(x_{1,t})W_{1,t+1} + \delta P_{12,t}(x_{1,t})W_{2,t+1}], \\ W_{2,t} = & \max_{x_{2,t}}[r_2 + w_2(x_{2,t}) + \delta P_{21,t}(x_{2,t})W_{1,t+1} + \delta P_{22,t}(x_{2,t})W_{2,t+1}]. \end{aligned} \tag{B.1}$$

$$\begin{aligned} V_{1,t} = & \; v_1(x^*_{1,t}) + \delta P_{11,t}(x^*_{1,t})V_{1,t+1} + \delta P_{12,t}(x^*_{1,t})V_{2,t+1}, \\ V_{2,t} = & \; v_2(x^*_{2,t}) + \delta P_{21,t}(x^*_{2,t})V_{1,t+1} + \delta P_{22,t}(x^*_{2,t})V_{2,t+1}, \end{aligned} \tag{B.2}$$

with $t \in \{1,2,\ldots,T\}$ and $W_{s,T+1} = V_{s,T+1} = 0$, $s \in \{1,2\}$. We consider a finite-horizon (nonstationary) problem to illustrate the evolution of the values of connections. The infinite-horizon, stationary problem, in which T is replaced by ∞ yields similar predictions on the comparative statics of ΔV with respect to the parameters of interest.

The state-differences among equations B.1 and B.2 yield the following recursive dynamic:

$$\Delta W_t = \Delta r + \Delta w_t + \delta \Delta \tilde{P}_t \Delta W_{t+1}, \tag{B.3}$$

$$\Delta V_t = \Delta v_t + \delta \Delta \tilde{P}_t \Delta V_{t+1}, \tag{B.4}$$

with $t \in \{1,\ldots,T-1\}$, and $\Delta\tilde{P}_t \overset{def}{\equiv} P_{11,t} - P_{21,t} = P_{22,t} - P_{12,t} \geq 0$.

We first establish the model's unique equilibrium and the related first order conditions:

Proposition B.1 *The model admits a unique equilibrium $(x^*_{s,t}, W_{s,t})_{t=1,\ldots,T,s\in\{1,2\}}$. In the last period $x^*_{s,T} = \bar{x}$, and for all $t < T$ the following first order conditions hold:*

$$\begin{aligned} w_1'(x^*_{1,t}) - \delta P'_{11,t}(x^*_{1,t})\Delta W_{t+1} = & \; 0, \\ w_2'(x^*_{2,t}) - \delta P'_{21,t}(x^*_{2,t})\Delta W_{t+1} = & \; 0. \end{aligned} \tag{B.5}$$

Proof. Those first order conditions are derived directly from the optimization problem in equations (B.1). Existence and unicity of $x^*_{s,t}$, given $W_{s,t+1}$ are obtained from the assumptions on $w_s(\cdot)$ and $P_{ss'}(\cdot)$. At the terminal point, future career no longer matters as $\Delta W_{T+1} = 0$, so $x^*_{1,T} = x^*_{2,T} = \bar{x}$. Backward induction then yields the unique solution $(x^*_{s,t}, W_{s,t})_{t=1,\ldots,T}$. ■

We focus on the case the politician always prefers higher office, so $\Delta W_t > 0 \; \forall t \leq T$ (e.g., when Δr is sufficiently large). The FOCs yield the following solution for $t \in \{1,\ldots,T-1\}$, which allows the calculation of the full path of favoritism together with equations (B.3) and (B.4):

$$\begin{aligned} x^*_{1,t} = \frac{\beta_1}{(2\delta\gamma_1)^2}{\Delta W^*_{t+1}}^{-2}, \qquad & x^*_{2,t} = \frac{\beta_2}{(2\delta\gamma_2)^2}{\Delta W^*_{t+1}}^{-2}, \\ \Delta v^*_t = \rho \Delta w^*_t = \frac{\rho B}{2\delta}{\Delta W^*_{t+1}}^{-1} \;\; \forall t < T, \qquad & \textit{with } B \overset{def}{\equiv} \frac{\beta_2}{\gamma_2} - \frac{\beta_1}{\gamma_1} = (\beta - \gamma)\frac{\beta_1}{\gamma_2}, \\ x^*_{1,T} = x^*_{2,T} = \bar{x}, \qquad & \Delta V^*_T = \Delta v^*_T = \sqrt{\bar{x}}(\sqrt{\beta_2} - \sqrt{\beta_1}). \end{aligned} \tag{B.6}$$

Per-period favoritism $x^*_{s,t}$ is decreasing in the politician's relative value of high office in the next period ΔW^*_{t+1}, and given ΔW^*_{t+1}, $x^*_{s,t}$ is increasing in power β_s, but decreasing in scrutiny γ_s. The net present value of favoritism from a higher position, ΔV^*_t, follows a more nuanced pattern, as previously stated in Proposition 1, recopied below:

Proposition 1 *(i) If power trumps scrutiny, in that $\beta \geq \gamma$, then the connected firm draws higher net present benefit when the politician attains higher office, namely $\Delta V^*_t \geq 0$ $\forall t$.*

(ii) If scrutiny trumps power, in that $\beta < \gamma$, and T is big enough, then there exists a time $\bar{t}$ before which there is an adverse effect of higher position on the net present value of favoritism: $\Delta V^*_t < 0$ $\forall t < \bar{t}$. After $\bar{t}$, ΔV^*_t is positive and increasing in t.

Proof of Proposition 1. First, note that $\Delta v_t \geq 0$ iff power trumps scrutiny. Proposition B.1 also implies that in the last period $\Delta V_T = \rho \Delta w_T(\bar{x}) > 0$. When power dominates in the first case, δv^*_t is positive in all periods following equation (B.6), hence the conclusion obtains immediately for ΔV_t.

In the second case, we apply backward induction using equation (B.4) from $t = T$ down to $t = 1$. Since $\Delta v^*_t \leq 0$ when scrutiny dominates, and because $\delta \Delta \tilde{P}_t \in (0, 1)$, $\Delta V_t < \Delta V_{t+1}$ whenever $\Delta V_{t+1} > 0$. When the sequence ΔV_t eventually reachers below zero as t decreases to a value $\bar{t} - 1$ (which is inevitable when T is large enough), the monotonicity of ΔV_t no longer holds necessarily. However, for all $t < \bar{t}$, equation (B.4) guarantees that $\Delta V_t < 0$. ■

Intuitively, the relative balance between power and scrutiny B (equation (B.6)) is key to the adverse effect of higher position. When it tilts towards scrutiny, in each period the firm would benefit *less* when the politician attains a higher position ($\Delta v^*_t < 0$) and chooses to reduce favoritism to preserve his career. However, by the end of his career, as electoral concerns ease, the net present value of higher position ΔV^*_t increases towards its terminal value Δv^*_T, which is positive. Over the politician's career, ΔV^*_t follows a loosely upward longterm trend,[40] as it is negative at an early stage, but becomes positive and increasing in late career. This career pattern follows from Olson's (1993) famous "roving bandit vs. stationary bandit" intuition, as a shorter horizon implies less electoral control on the politician, who would be more willing to engage in favoritism.[41]

Next are the comparative statics with respect to the key parameters of power and scrutiny, which will be tested in corresponding comparative situations in sections 6.1 and 6.3 (previously stated in proposition 2, recopied below):

Proposition 2 *When scrutiny trumps power, in presence of the adverse effect of higher position ($\Delta V_t < 0$), its magnitude $|\Delta V_t|$ increases with B's magnitude ($B < 0$), e.g., when:*

- *β_2 decreases and/or β_1 increases,*
- *both increase while their ratio β remains the same,*
- *γ_2 increases and/or γ_1 decreases,*
- *both decrease while their ratio γ remains the same.*

Proof of Proposition 2. We focus on the case when scrutiny trumps power and an increase in $B < 0$ (i.e., a dencrease in its magnitude) in the four cases described in Proposition 2.[42] First, we expand the recursive

[40] The upward trend is only 'loosely' so, as one cannot establish the monotonicity of ΔV_t when it is negative, although the monotonicity is more pronounced when $\Delta \tilde{P}_t$ is closer to 1 (i.e., strong incumbency advantage). As the career becomes very long (large T), going backward towards $t = 0$, ΔV_t converges to a fixed negative value.

[41] In this model, we simply incorporate a politician's deeper entrenchment into his power parameters. See Campante et al. (2009) for an analysis of the combination of both this entrenchment effect and the politician's horizon effect.

[42] Because ΔW_T and ΔV_T depend directly on β_2 and β_1, a change in B does not guarantee a monotonic change in ΔW_T and ΔV_T. The comparative statics still hold separately with respect to changes in the β_s's and γ_s, but only approximately with respect to a change in B.

solution formula of ΔW_t as follows:

$$\Delta W_t = \Delta r + \frac{B}{2\delta \Delta W_{t+1}} + \delta \left[-\frac{B}{4(\delta \Delta W_{t+1})^2} + P_{22}(0) - P_{12}(0) \right] \Delta W_{t+1}$$
$$= \Delta r + \frac{B}{4\delta \Delta W_{t+1}} + \delta \Delta \tilde{P}_0 \Delta W_{t+1} \quad with \quad \Delta \tilde{P}_0 \overset{def}{\equiv} P_{22}(0) - P_{12}(0).$$

As $B < 0$, the right hand side expression is increasing in both B and ΔW_{t+1}. Therefore, when B increases towards 0, the whole path $(\Delta W_t)_{t=1,\dots,T}$ increases.

It gets more complicated to show the monotonicity of the path of $(\Delta V_t)_{t=1,\dots,T}$ when B changes, since this sequence also depends directly on the sequence $(\Delta W_t)_{t=1,\dots,T}$. To do so, we first write the solution formula of ΔV_t in a more tractable way:

$$\Delta V_t = \frac{\rho B}{2\delta \Delta W_{t+1}} + \delta \left[-\frac{B}{4(\delta \Delta W_{t+1})^2} + \Delta \tilde{P}_0 \right] \Delta V_{t+1}$$
$$= \frac{B}{2\delta \Delta W_{t+1}} \left[\rho - \frac{\Delta V_{t+1}}{2\Delta W_{t+1}} \right] + \delta \Delta \tilde{P}_0 \Delta V_{t+1} \quad . \tag{B.7}$$

Next, note that the difference between ΔV_t and $\rho \Delta W_t$ is the discounted sum of the stream of Δr, with the discount factors being the products of the by-period discount factor $\delta \Delta \tilde{P}_t$. This statement is best proved by induction from $t = T$ down to $t = 0$. Indeed, denote recursively this difference as R_{t+1} in $\Delta V_{t+1} + R_{t+1} = \rho \Delta W_{t+1}$, we obtain $\Delta V_t + R_t = \rho \Delta W_{t+1}$ with $R_t = \Delta r_t + \delta \Delta \tilde{P}_t$. This recursive formula implies that R_t is a discounted sum of the stream of Δr.

Each discount factor $\delta \Delta \tilde{P}_t = \delta \left[-\frac{B}{4(\delta \Delta W_{t+1})^2} + P_{22}(0) - P_{12}(0) \right]$ decreases as B increases towards 0, since ΔW_{t+1} increases while $|B|$ decreases. Hence the compound products of those discount factors over $t \in \{k+1, \dots, T\}$ decrease as well. Therefore, R decreases when B increases. Since $\Delta V_t = \rho \Delta W_t - R_t$, it follows that when B increases, ΔV_t increases even more than ΔW_t, therefore ΔV_t is increasing in B. ■

Remark that, as the whole path of $(\Delta V_t)_{t=1,\dots,T}$ increases following an increase in B towards 0, it follows that the moment $\bar{t}$ through which ΔV_t switches sign (from negative before $\bar{t}$ to positive after $\bar{t}$) decreases. That is, ΔV_t switches sign earlier, thus the adverse effect of promotion on connected firm's value becomes less prevalent.

B.2 Estimation of cumulative abnormal returns

For each company's stock i, its daily return on day t is defined from daily stock price $P_{i,t}$ as $R_{i,t} = \frac{P_{i,t}}{P_{i,t-1}} - 1$. Related to an event (an election in our case) on day 0, stock i's market model $R_{i,t} = \alpha_i + \beta_i R_{m,t} + \epsilon_{it}$ is estimated from the time series of the market daily returns $R_{m,t}$ over the window (-351,-61) counting from the event day (including both starting and end days), where $R_{m,t}$ is the market's return on day t. Abnormal returns on day t is then calculated as $AR_{i,t} = R_{i,t} - (\widehat{\alpha}_i + \widehat{\beta}_i R_{m,t})$. Cumulative abnormal returns over the benchmark window (-1,5) are calculated as

$$CAR_i^{(-1,5)} = \sum_{t=-1}^{5} AR_{i,t} = \sum_{t=-1}^{5} \left[R_{i,t} - (\widehat{\alpha}_i + \widehat{\beta}_i R_{m,t}) \right]. \tag{B.8}$$

In robustness checks, we also calculate CARs that take into account other moments in the estimation of $AR_{i,t}$, following Fama and French's (1993) three-factor model or Carhart's (1997) four-factor model.

CARs sum up changes in a firm's stock price over the benchmark window, filtering out a function of the stock's pre-event data (as encompassed in the estimators $\widehat{\alpha}_i$ and $\widehat{\beta}_i$ and market-wide data that vary only by the time dimension. Its cross-sectional variation maps directly to the variation in the changes of the value of connection V, assuming no other event takes place at the same time.

Given that close elections' results can be considered as almost-random draws, they must be independent of the aforementioned part that is filtered out from the sum of raw returns in CARs. Therefore, we should expect that estimates using CARs calculated from different market models (with either one, three, or four factors) as the outcome variable do not differ from estimates that use the sum of raw returns instead. This prediction is confirmed in Appendix Table A5's Panel B. While the choice of the market model should not affect the magnitude of the estimates, the appropriate model choice may help reduce the noises inherent in stock returns, which may help improve the estimates' precision.

B.3 Semi-parametric estimation of heterogeneous effects

Following Do et al. (2017), we modify equation (1)'s baseline RDD specification to examine the heterogeneous effects of having Congress-level connection on firm value as a non-parametric function $\beta(\cdot)$ of a variable of interest x:

$$CAR_{idt} = \beta(x)Winner_{pt} + \delta_W(x)VS_{pt}\mathbb{1}_{\{VS_{pt}\geq 50\%\}} + \delta_L(x)VS_{pt}\mathbb{1}_{\{VS_{pt}<50\%\}} + \varepsilon_{idpt}. \quad \text{(B.9)}$$

We first define the percentiles of x as $p_x \in [0, 1\%, \ldots, 100\%]$. The function $\beta(\cdot)$ is estimated from semi-parametric local linear regressions based on equation (1) at each value over a grid of 101 points of p_x (the focal point). In each local regression around x, each observation at a percentile q is weighted by a Gaussian kernel function $\frac{1}{\sqrt{2\pi}}\exp\left[-\frac{1}{2}\left(\frac{q-p_x}{b}\right)^2\right]$, with a bandwidth equal to 20%. The shape of the estimated function $\beta(\cdot)$ remains robust to a broad range of cross-validated bandwidths.

B.4 Additional robustness consideration of statistical inference

In this appendix subsection, we discuss the question of statistical inference regarding the main estimates in the paper from different angles, and provide more details on the inference results shown in the paper. First, we justify the main paper's choice of clustering adjustment by politician based on Abadie et al.'s (2020, 2023) design-based statistical inference framework. Second, we also implement robust statistical inference in the traditional framework of clustered standard errors (with infinite sample asymptotic), and even at the coarsest level of clustering by election year.

B.4.1 Design-based statistical inference.

Abadie et al. (2020, 2023) propose a novel framework of statistical inference (with potential clustering) that takes into account both the traditionally focused sampling-based uncertainty and their newly suggested design-based uncertainty that arises from the standard potential outcome framework in causal analysis, and argue that this framework fits better many empirical designs of causal analysis. While the traditional asymptotic approach is based on the consideration of data-generating processes that grow the sample to infinite superpopulations, this novel framework focuses on repeated substantial samples of finite populations, with uncertainties of both the sampling process and the causal nature of the empirical design.

This new framework is strongly relevant to our context for a couple of reasons. First, our setting is fundamentally a causal analysis, in which the RDD framework proves that the assignment of the treatment variable (win vs. lose) is almost random a setting that fits very well with Abadie et al.'s (2020). Second, our setting concerns a well-defined, finite population of politicians and firms, within a specific time frame, so the inference should be best considered within those limits, instead of the traditional infinite superpopulation framework.

In this framework, Abadie et al. (2023) shows that "[...] the sampling process and the treatment assignment mechanism solely determine the correct level of clustering; the presence of cluster-level unobserved components of the outcome variable becomes irrelevant for the choice of clustering level." This is a strong result that frees empiricists of the concern of potential within-cluster correlations of the residuals, which is the source of clustering adjustments in the traditional inference framework. The following quote from Abadie et al. (2023) highlights this drastic change:

> We use our framework to highlight three common misconceptions surrounding clustering adjustments. The first misconception is that the need for clustering hinges on the presence of a nonzero correlation between residuals for units belonging to the same cluster. We show that the presence of such correlation does not imply the need to use cluster adjustments. The second misconception is that there is no harm in using clustering adjustments when they are not required, with the implication that if clustering the standard errors makes a difference, one should cluster.

Abadie et al. (2023) goes on to provide an intuitive example why clustering is unnecessary if we are interested in inference based on repeated sampling of a finite population, instead of a data-generating process of an infinite superpopulation:

> To see that both claims are incorrect, consider the following simple example. Suppose that, based on a random sample from the population of interest, we use the sample average of a variable to estimate its population mean. Suppose that the population can be partitioned into clusters, such as geographical units. If outcomes are positively correlated in clusters, the cluster variance will be larger than the robust variance. However, standard sampling theory directly implies that if the units are sampled randomly from the population, there is no need to cluster. The harm in clustering in this case is that confidence intervals will be unnecessarily conservative, possibly by a wide margin.

Abadie et al. (2023) still suggests the use of clustering adjustment, but recommends it at the level of the variation of the main assignment of the treatment variable. In our context, the key variation is the cross-sectional variation of election results. Hence the treatments assignment varies at the level of politician per election, which is very similar to the clustering level of politician used in our paper (ours is just slightly coarser). Coarser clustering will likely result in statistical inference that is unnecessarily too conservative.

Taking those points into account, we believe it is best to adjust for clustering at the level of politician, as it is still relatively conservative, without running the risk of excessively conservative confidence intervals.

B.4.2 Clustering adjustment with traditional statistical inference.

We also examine our results' statistical inference in the traditional framework (i.e., with infinite sample asymptotic) when standard errors may be correlated within clusters. First, we show in Appendix Table A5 the strong statistical significance under alternative clustering correction schemes, including clustering by election (i.e., state by year), firm, or two-way clustering by politician and firm. In our context, the coarsest clustering adjustment is that by time, in which case clustering-adjusted estimated standard errors and related tests have poor properties because of the small number of only 5 different election dates in our sample (Cameron et al., 2008).

We take two approaches to deal with the issue of few clusters. First, Cameron et al.'s (2008) results from simulations show that the **clustered wild bootstrap procedure with the t-statistic** performs best, and sufficiently close to the correct test size. This result has been revisited in various contexts in the econometric literature, and was recently proven theoretically by Canay et al. (2021). We thus carry out inference by the clustered wild bootstrap to verify the potential issue of clustering by election.

The clustered wild bootstrap procedure (using t-statistic) consists of resampling all clusters with Rademacher weights. That is, in each cluster all of its observations are either kept the same, or switched sign (i.e., multiplied by -1), each option with probability 0.5. The same regression is then executed on each wild-bootstrapped sample, and inference is performed based on the distribution of t-statistics obtained from all such samples.

In our particular case of only 5 clusters from 5 elections, the clustered wild bootstrap only produces a superpopulation of size 25=32, so the distribution of wild-bootstrapped t-statistics could only contain 32 values, and the lowest p-value possible based on this distribution is 1/32=0.03125. Incidentally, Cai et al. (2023), also warns that 5 clusters is the lowest possible number of clusters on which one could still perform the clustered wild bootstrap.

As we construct those 32 wild-bootstrapped samples, it turns out that the true t-statistic from our main specification is the lowest (most negative) among all of the 32 wild-bootstrapped t-statistic. That is, we still obtain the lowest possible p-value of 0.03125 in this context.

Second, Canay et al. (2017) shows a powerful result on the **Approximate Randomization Test (ART)** that could be used to deal with a small number of clusters. We follow Cai et al.'s (2023) implementation of the ART in case of a linear regression with a small number of clusters (down to a minimum of 5 clusters), with heterogeneous distributions across clusters but not too strong dependence within each cluster. This exercise is a useful robustness check beyond the wild bootstrap, as Canay et al. (2021) shows that theoretically the clustered wild bootstrap requires similar distributions across clusters, while the ART does not.

In our case, the ART procedure consists of the five following steps:

1. estimating the main coefficient $_j$ from the main specification within each cluster j,
2. compute the statistic $S_j = \sqrt{n_j}{}_j$ from each cluster,
3. compute the actual samples test statistic $T = \frac{1}{5}\left|\Sigma_{j=1}^{5} S_j\right|$ over all 5 clusters,
4. construct the population of comparison statistics by applying Rademacher weights to each cluster that is, construct the set of $2^5 = 32$ values of $T_k = 1/5\frac{1}{5}\left|\Sigma_{j=1}^{5} g_j S_j\right|$, with each g_j taking value of either +1 or 1, and
5. compute the test and p-value by comparing the actual test statistic T with the distribution of T_k's.

Using our data and main specification, ART yields test statistic T that has the largest magnitude among all 32 comparison values T_k's that is, we obtain the lowest possible p-value of 0.03125. Indeed, the estimate β_j is negative for all 5 clusters $j \in \{1, \dots, 5\}$, implying that T takes the maximal magnitude among the T_k's.

Taken together, even under the most unfavorable setting, the most conservative approach still highlights the strong statistical significance of our result. We are thus confident that our result is clearly a statistically important feature in the data, and not the result of arbitrariness.

B.4.3 Inference from alternative thresholds

In Appendix Figure A6, we further check the statistical inference at the $VoteShare$ threshold of 50% by estimating the main specification in (1) using alternative hypothetical $VoteShare$ thresholds from 47.5% to 52.5%, and compare the distribution of t-statistics from those tests with the benchmark test, as shown in Table 2. In Panel A, as the bandwidth is 5% $VoteShare$, the estimation sample is a rolling window of $VoteShare$ between -2.5% and +2.5% from each hypothetical threshold (this exercise requires an enlarged sample of $VoteShare$ from 45% to 55%). In Panel B, we further restrict the sample to not intersect with the 50% threshold. That is, if the hypothetical threshold is below (above) 50%, then the estimation sample is restricted below (above) 50%.[43]

Both panels show consistently the statistical importance of the estimated t-statistic for the true threshold of 50% $VoteShare$. In Panel A, the rolling window naturally produces the continuity of the t-statistic with respect to the hypothetical threshold, including when it is close to the 50% threshold. Hence there are a few of those estimates whose values are close to the true t-statistic. However, the true value (the red dot at the 50% threshold) still stands out as the lowest (most negative) estimate. In Panel B, the true t-statistic (the red dot) stands out particularly strongly in comparison with all other potential thresholds, with a magnitude of at least twice as large as all other estimates. Even if we consider the absolute value of the estimates, i.e., a two-sided test instead of a one-sided test, the true t-statistic is still the largest in magnitude.

To better assess the particularity of the estimated t-statistic at the true threshold of 50% $VoteShare$, we plot the corresponding distributions of t-statistics for alternative thresholds in Appendix Figure A6 in the respective panels in Appendix Figure A7. In each panel, the true-threshold t-statistic, marked by the red vertical line, stands out as the strongest negative value, indicating statistical significance at 0.1%.

One may further remark that there is a cluster of alternative thresholds around 48% of $VoteShare$ with large t-statistics. The occurrence of a small number of high t-statistics *at some placebo threshold* in this kind of exercise with placebo thresholds is indeed statistically common. Indeed, when we run Monte Carlo simulations of the estimated model's data generating process and then estimate the t-statistics as in Panel B, the largest t-statistic turns out to be larger than the tru-threshold statistic for 40% of the simulations. Regarding this particular range around 48% of $VoteShare$, when we exclude this range from the sample, all results remain very similar.

B.4.4 Inference from alternative events prior to true event

In Appendix Figure A8, we further check the statistical inference for the event date by estimating the main specification in (1) using alternative hypothetical election dates over a year before the true event, and compare the distribution of t-statistics from those tests with the benchmark test, as shown in Table 2. To be cautious, we do not consider alternative dates *after* the true event, as there could be long-run effects on the treated versus control observations at those dates, which would contaminate the distribution of placebo t-statistics.

Panel A reports all days, while Panel B focuses on the sample of all Tuesdays (as the election day is always a Tuesday). The choice to space out the dates in Panel B helps create non-overlapping windows of CAR calculation, so as to limit the autocorrelation of the estimates between close hypothetical election dates. Figure A6 further reports the corresponding distributions of t-statistics, where the true event's value is marked by the red vertical line.

Appendix Figures A8 and A9 show consistently the statistical importance of the estimated t-statistic for the outcome of the true election day. In both panels of both figures, the true t-statistic stands out strongly in terms of both its negative value and its absolute value, indicating statistical significance at 0.1%. In both panels, it is reassuring that the distributions of the hypothetical t-statistic estimates have rather standard shapes, which underlines the randomization nature of this inference exercise.

[43] This restriction naturally reduces the estimates for hypothetical thresholds close to 50%, because for those thresholds one side of the RDD sample includes only a small range of $VoteShare$. This concern is not important for hypothetical thresholds farther from 50%.