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Out of Sight, Out of Mind:
The Value of Political Connections in Social Networks*

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ABSTRACT

Using networks of university classmates among corporate directors and U.S. congressmen and the regression discontinuity design of close elections from 2000 to 2008, we identify a significant but widely varying impact of political connections on firm value. Surrounding the election day, connections to powerful senators increase firm value by 8.59%, while connections to elected congressmen decrease firm value by 2.65% on average. Political connections are especially valuable at the state level, in highly regulated and corrupted states, and in small and financially dependent firms. Following elections, firms connected to the winner decrease state activities; meanwhile, their directors tend to serve shorter tenure.

Keywords: Social network, political connection, close election, regression discontinuity design, firm value, state-level politics.

JEL Classifications: G3, G10, G11, G14, G28, G30, G34, G38

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1. INTRODUCTION

The impact of political connections on firms has attracted a growing body of economic and finance literature. Political connections are reported to affect firm value, access to credit, business with government, corporate taxation, and regulatory oversight, in many parts of the world, especially where institutions are weak and politicians have much discretion and little accountability.[1] The existence of political influences on firms paves way to rent-seeking activities, with long-term detrimental effects on market efficiency, political accountability and, ultimately, economic growth.[2] In the United States, where institutions rank among the best in the world,[3] the evidence of the value of political connections is mixed, with positive estimates (Jayachandran 2006, Knight 2007, Goldman et al. 2009, Acemoglu et al. 2010), as well as estimates indistinguishable from zero (Fisman et al. 2006).

This paper enriches the body of empirical evidence on the impact of political connections in the U.S. by using novel methods to address major challenges raised by the extant literature. First, we extend beyond event studies of very specific cases by broadening the definition of political connections to social relations between politicians and corporate directors based on their educational backgrounds. Second, we address key identification problems in the empirics of social interactions by using the Regression Discontinuity Design (RDD) of close elections to Congress, subjected to thorough robustness checks with additional fixed effects and control variables.

Our first objective is to address the social relations between politicians and firms beyond direct family ties and share ownerships, which are very rare among American congressmen. While social connections could be carefully measured by coordination games in laboratory setups (e.g., Leider et al. 2009) or by extensive field surveys (e.g., Conley and Udry 2010), both methods are prohibitively costly to apply in our context.[4] Instead, we use the social networks defined by former classmates in tertiary education, an important type of social networks in the U.S.[5] This measurement can be clearly and unambiguously defined

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[1] The literature has covered Indonesia (Fisman 2001), Malaysia (Johnson and Mitton 2003), Pakistan (Khwaja and Mian 2005), Brazil (Claessens et al. 2008), France (Bertrand et al. 2008), Thailand (Bunkanwanicha and Wiwattanakantang 2009), Taiwan (Imai and Shelton 2010), and cross-country evidence (Faccio 2006, Faccio et al. 2006), among others.

[2] See for instance Shleifer and Vishny (2002), chapters 3-5 and 8-10, for discussions on political rent-seeking and its negative impacts on efficiency and growth.

[3] From 2000 to 2008, the U.S. rank consistently in the world's first decile in control of corruption, rule of law, regulatory quality and government effectiveness (by average scores of the World Bank's World Governance Indicators, Kaufman et al. 2011.)

[4] See Marsden (1990), Ioannides and Loury (2004), Jackson (2009), and Allen and Babus (2009) for network measurements.

[5] The social networks of former classmates have been explored, and their importance stressed, *inter alia*, by Cohen et al. (2008) and Fracassi (2009) in the context of American educational institutions, Bertrand et al. (2008), Kramarz and

based on publicly available information on educational backgrounds of all politicians and directors, and covers a sample much more representative than connections specific to a few politicians. We abstract from political connections based on campaign contributions (e.g. Cooper et al. 2011), as we find it difficult to establish clear links between specific firms and politicians based on these contributions in the U.S. During our period of study firms cannot contribute to political candidates, except in setting up political action committees to receive donations from its employees, often to both major parties. Even those committees often channel the contributed funds to larger-scale committees, and only a small fraction of those funds goes to specific candidates' campaigns.

Our second objective is to propose a convincing solution to the identification problem related to connections between politicians and firms. Many unobservable characteristics of politicians and firms can influence a political link (or the measure thereof) and the outcomes at the same time, thereby confounding the effect we want to attribute to social network connections.[6] In specific contexts, event studies using arguably exogenous news and event probabilities from prediction markets may provide time-series identification to this issue (see, for instance, Snowberg et al. 2007, or Fisman 2001). However, the reliance on specific events may compromise the generalizability of the empirical findings.

Our novel approach consists of a cross-sectional identification of the effect of social connections of politicians and directors, using close elections to Congress. Lee (2008) shows that close elections can be considered a Regression Discontinuity Design (RDD), a natural experiment that produces near-randomized-trial identification with strong internal validity. Accordingly, a connection to a politician elected to office by a small margin is almost identical to a connection to one defeated by a small margin, in both observable and unobservable characteristics, similarly to a randomized experiment around the threshold. The strength of RDD more than offsets a potential weakness of traditional event studies, in that we correctly estimate the value of connection even if the market misestimates the probability of event. Event-study techniques are still used in our approach only to improve estimation efficiency, and are not essential to the results. Moreover, Lee and Lemieux (2010) also show that the estimated effect is a Weighted Average Treatment Effect (WATE) representing all politicians with a nonzero chance of experiencing a close election.

Thesmar (2012), and Nguyen (2012) for France, and Lerner and Malmendier (2011) and Shue (2011) for Harvard Business School alumni. In the U.S., educational institutions received as much as $41.67 billion in 2010, or 14% of all charitable donations, second only to religious organizations (the Giving USA Foundation, 2011.)

[6] See Durlauf and Ioannides (2010) and Blume et al. (2011) on the identification challenge regarding social interactions.

The remaining identification challenge in social networks is the confoundedness of homophily. Coined by sociologists,[7] "homophily" refers to the phenomenon that people sharing the same characteristics are more likely to connect, thus confounding the effect of connections with the effect of shared characteristics. Earlier works using the social network of educational backgrounds (Cohen et al. 2008, Fracassi 2009) have distinguished between former classmate networks and alumni networks to highlight the effect of connections as opposed to that of shared characteristics. By including both politicians and directors, we are able to push this methodology further: we use school fixed effects to identify the effect of political connections by variations over time (school fixed effects are unusable in earlier works based solely on the connections of businessmen). We can thus ascertain that the discovered effects come from social connections, not homophily.

We obtain data on elections from 2000 to 2008 from the U.S. Federal Election Commission, from which we filter in only elections of a winning margin within 5% between the two frontrunners. We manually collect details of all politicians' educational backgrounds from the web archives of their campaigns, a process made difficult by the search for less prominent defeated candidates. We also obtain past education history for directors of public firms in the U.S. from BoardEx of Management Diagnostics Limited. We then form all pairs between close-election candidates (elected or defeated) and directors who graduated from the same educational institution (same campus) within one year of each other, and link each pair to the firm's stock performance around the date of the politician's close election. Each observation thus matches a firm's cumulative abnormal return on the event window to the win/loss status of the candidate who shares education background with a director of the firm.

We run a regression of cumulative abnormal returns of stock prices of connected firms on a Win/Loss dummy with semi-parametric controls as required in a RDD. This regression equation provides an estimate of the stock-market value of a new connection to a politician in Congress. As shown in Lee and Lemieux (2010), the RDD of close elections produces a consistent, unconfounded estimate of the effect of the treatment. In this context, a treated firm's connected politician gets elected to Congress, while a control firm's connected politician is defeated. This estimate is in fact as good as a randomized experiment around the vote share threshold of 50%, and can account for all confounding factors prior to the event, be they observable or unobservable. Therefore, the results are not affected when we control for any pre-event covariates, or vary the estimation method of abnormal returns. We can thus

[7] See McPherson, Smith-Lovin, and Cook's (2001) survey.

focus on a single regression, while varying the subsample used in the regression.

In the terminology coined by Lee and Lemieux (2010), we estimate the Weighted Average Treatment Effect (WATE), where the weight of each observation is the probability that a politician experiences a very close election. While some politicians are less likely to have that experience than others, the inclusion of highly visible politicians such as John Ashcroft or Walter Mondale in our sample implies that our estimate can cover a very large share of the population of politicians and is therefore generalizable, unlike previous interpretations of RDD which are considered only applicable to the threshold value. Taken together, our estimate identifies a treatment effect that can shed light on social connections between Congressmen and corporate directors.

We obtain a variety of treatment effects, ranging from positive 8.59% for incumbent members of Senate Committees, to a negative 3.24% for challengers, to an overall effect of negative 2.65% during the event window from one day before to five days after the election. This result indicates that having a connected politician in Congress significantly decreases firm value by 2.65% on average and that the average effect is dominated by the effect of challengers. These effects' magnitude is sizeable: for an average market cap of $2.1 Billion in our sample, a loss of 3.24% means $68 Million, and a gain of 8.59% is equivalent to $180 Million. Our results are robust through many specifications, parametric and nonparametric, with different measures of outcomes, under different definitions of the social network (former classmates or alumni), and across many subsamples.

We interpret the seemingly counterintuitive negative impact of political connections results as follows. The connected politician is already providing benefits to the firm at state level, where he may have more time and focus for business deals, and faces less institutional and public checks and balances. If he is elected to federal office, the firm is expected to get less benefit, whereas if he is defeated, he will most likely remain as active in state politics, probably return to his previous position and strengthen his role in the state party apparatus. As a result, the estimated treatment effect is negative. We empirically test and confirm four implied predictions. First, the value loss effect is present for politicians coming from state, not for those from federal politics. Second, the effect is stronger for states with lower institutional quality and smaller firms. Third, firm activities, measured by local newspapers' citations of firm names, decrease in elected politicians' states, compared with defeated politicians' states. Fourth, directors connected to elected politicians tend to leave firm earlier than those connected to defeated politicians. Trading volume also increased significantly more for the stocks in our sample around election time, implying that the financial market pays particular attention to those events. Our result is thus interpreted as evidence of a higher value of connections for politicians at the state level than for politicians at the federal level.

This paper makes two main contributions to the literature on political connections. The first contribution is our solution to the identification problem. In extant literature, the study of political events, assumed as independent of political connections, has perhaps yielded the most convincing results. Knight (2007), Goldman et al. (2008, 2009), and Mattozzi (2008) exploit close elections in presidential races in the U.S.; Roberts (1990), Jayachandran (2006), Fisman et al. (2006), and Acemoglu et al. (2010) use news and events related to prominent American politicians; while Fisman (2001), Johnson and Mitton (2003), Bunkanwanicha and Wiwattanakantang (2009), Ferguson and Voth (2008), and Imai and Shelton (2010) treat politically important events in Indonesia, Malaysia, Thailand, Nazi Germany, and Taiwan. This strategy avoids the direct reverse causation channel, but, as discussed by Snowberg et al. (2008), many caveats persist, notably the unobserved prior probability of each event. The use of prediction markets as a helpful fix is unfortunately only limited to important events such as American presidential elections, and thus restrict the scope and undermine the generalizability of such analysis.

Other articles using non-political firm-related events such as appointments of directors (Faccio 2006, Goldman et al. 2009), bailouts (Faccio et al. 2006), IPOs (Fan et al. 2007, Francis et al. 2009) are subject to the endogeneity concern that these events are partly triggered by certain unobservable characteristics of the firms. Khwaja and Mian (2005), Dinç (2005), Leuz and Oberholzer-Gee (2006), Bertrand et al. (2008), Claessens et al. (2008), Li et al. (2008), and Boubakri et al. (2009) rely on fixed effects and/or difference-in-difference strategies, and are liable to confounding biases induced by time-varying characteristics of firms or politicians/political parties.

Despite extensive robustness checks of causality in prior literature, the endogeneity of political connections remains a thorny issue. Even in the best event-study setups with perfect measures of prior probabilities of events, it is hard to rule out the possibility of unobserved firm characteristics affecting both a firm's outcome and political connections (exceptions include randomized assignments to social networks as studied by Lerner and Malmendier 2011, and Shue 2011.) For instance, a defense technology firm can recruit a former secretary of defense because of his expertise in defense technologies, and will likely benefit from the political success of his pro-war former party fellow members, without this effect deriving from a "political connection," as previously defined. Our framework deals adequately with both the endogeneity of the connected politician and the selection bias in networks due to homophily, providing a powerful internal validity of the empirical results. Moreover, the estimated effect is a WATE across the sample of all politicians susceptible to experiencing a close election, and across sampled firms, which are comparable to Compustat's universe, therefore enforcing the external validity of the estimate.

In extant literature, RDD of close elections have been mostly used with politicians' behaviors and outcomes, such as election advantage, roll call votes or wealth accumulation (surveyed by Lee and Lemieux, 2010.) To the best of our knowledge, we are the first to apply close election RDD to outcomes of firms linked to politicians. This can pave way for further applications of RDD in corporate finance, a hitherto underexploited possibility.[8]

Our second contribution is the finding of a large variation in the value of political connections and the importance of state-level political connections. While the negative average estimated value of connection to congressmen appears at first glance counterintuitive, it does not contradict the existing literature on the positive value of political connections (e.g., Fisman, 2001, Faccio, 2006, Goldman et al. 2008). We argue that it results from the firm's lost benefits when the connected politician moves away from state politics. It is consistent with Fisman et al.'s (2006) finding that, on average, firms do not enjoy financial benefits from their connections to Vice President Dick Cheney. Contributing to the literature on corruption across U.S. states (Glaeser Saks, 2006), our results are, to our best knowledge, the first to provide strong empirical evidence of the remarkable variation in institutional quality between federal and state levels in the U.S. We uncover very different values of political connections, and highlight the importance of state-level political connections, which calls for further attention on state-level political research and institution design.

The remaining of the paper is organized as follows. Section 2 details the methodology. Section 3 provides data description. Section 4 reports the major empirical results and robustness checks. Section 5 discusses and explains the findings. Section 6 concludes.

2. EMPIRICAL METHODOLOGY

2.1 CONCEPTUAL FRAMEWORK OF THE IDENTIFICATION

Evidence of the impact of a political connection on firm value is subject to two types of endogeneity biases. The first bias comes from the endogeneity of the "political" part in "political connection." The estimated effect could reflect (i) a reverse causation channel when a well-performing firm may be able to help its connected politicians win elections, or (ii) an omitted variable bias when connected firms and politicians are affected by the same

[8] Exceptions include Chava and Roberts (2008), Cuñat et al. (2012), Kerr et al. (2011).

unobservable factor, such as a shift in public opinion. The second bias comes from the endogenous determination of the "connection," usually termed as the problem of homophily when individuals are connected because of similarity. While the first bias is best eliminated with a randomization of the assignment of a politician to office, the second bias can be solved with a randomization of connections between firms and politicians. In practice, both types of randomization are hard to find.

David Lee's (2008) pioneering work on Regression Discontinuity Designs points out that, under the key assumption that candidates are unable to precisely manipulate the result of the election, the event of winning close to the vote threshold of 50% is almost randomized between the top two runners. Intuitively, as candidates only have imprecise control over the assignment of win or loss, everyone has approximately the same probability of getting a vote share of just above or just below 50% – similar to a coin flip – independently of all pre-election observable and unobservable variables. In other words, conditional on the election being close, winners and losers are equal in all aspects. One can therefore estimate the average treatment effect of connections to elected politicians versus defeated politicians without any endogeneity bias, ensuring the internal validity of the results.

In addition to the cross-sectional identification by RDD, time-series identification from event-study market models is used to calculate stocks' Cumulated Abnormal Returns (CARs). However, while the use of CARs improves estimation efficiency by reducing market noises, it is not essential to our results, thanks to the near-random nature of RDD assignments. Traditional event studies rely on the event's exogeneity and assumptions of the market's prior beliefs, which only prediction markets may capture (see discussions in Fisman, 2001, and Snowberg et al., 2008). In contrast, our design is always valid, even if the market's prediction is largely incorrect. Indeed, suppose that the market predicts a winning probability of 65% instead of the correct probability of 50% (the RDD uses real vote share outcomes to address the realized 50% vote share threshold.) For \$100 of perceived value of winning, the pre-event connection will be priced by the market, incorrectly, at \$65. The post-event market reaction to a realized win is \$35, and that to a realized loss is negative \$65. RDD estimation still produces, correctly, the difference of \$35-(-\$65) = \$100, exactly the right value of having a connection to an elected politician. (See the appendix for more details.)

On their external validity, the results from the RDD are generalizable. Lee and Lemieux (2010) point out that the RDD estimate is not only informative for close elections but also for others. The estimate can be interpreted as a Weighted Average Treatment Effect (WATE) of being politically connected, where each politician's weight is her ex ante likelihood to be in a close election. This likelihood is nontrivial for most American politicians. Even very powerful politicians are not immune to close elections, as the Senate majority leader Harry Reid experienced in 2010. On the other hand, there is no particularity in our sample's firms, which are, as we show in Section 3, comparable to firms in the Compustat's dataset.

2.2 EMPIRICAL SPECIFICATIONS

We follow Lee and Lemieux (2010) in designing two main econometric specifications to estimate the effect of political connection. Each observation 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. The dependent variable is the firm's stock price Cumulated Abnormal Return in a window around the election day. The treatment variable is an indicator whether the connected politician wins or loses that race.

The first specification consists of an OLS regression of the outcome variable on the treatment variable, controlling for vote shares of elected politicians and defeated politicians, where the sample is limited to all races with less than 5% vote margin. That is, we obtain the OLS estimate $\hat{\beta}$ in the following equation, where VS_i stands for vote share:

$$CAR_i = \beta WinLose_i + \delta_W VS_i \mathbf{1}_{\{VS_i \geq 50\%\}} + \delta_L VS_i \mathbf{1}_{\{VS_i < 50\%\}} + \varepsilon_i.$$

Standard errors are calculated from the OLS regression, and are clustered at the politician level for each election. In robustness checks, we also include polynomials of different orders of the vote shares, other levels of clustering, and two-way clustering.

The second specification uses nonparametric regressions of the outcome variable on the treatment variable on two separate subsamples of winners and losers. Predictions of the outcome variable are calculated at the threshold of 50% vote share for each sample, and their difference is reported. Technically, we use the nonparametric local cubic polynomial regression of the equation:

$$CAR_i = F(VoteShare_i) + \varepsilon_i$$

on the subsample where $VoteShare_i < 50\%$ to estimate the function $\hat{F}_-(.)$ and on the subsample where $VoteShare_i > 50\%$ to obtain $\hat{F}_+(.)$. The estimated effect is calculated as $\hat{F}_+(50\%) - \hat{F}_-(50\%)$.

2.3 OTHER IDENTIFICATION CONCERNS

Link definition: By defining connections as pairs of classmates, we may raise doubts about the realistic nature of those connections, as most people have only a small number of real friends even among classmates (Leider et al. 2009). Yet this should not be a concern to our finding. As real friendship is nuanced by classmate connections, the measurement errors will produce an attenuation bias that reduces the absolute size of the estimate and its statistical significance. The effect of real friendships can then be even larger than that found in this paper. Besides, classmate connections can be essential in the development of relationships after college or graduate school by providing mutual trust, common ground in communication, and common access to the same social network. Former classmates are therefore much more likely to later develop a strong connection, even if they were not close friends at school. In fact, several papers have shown the impact of this measurement of connections in many contexts (Cohen et al. 2008, Fracassi 2009, and Nguyen 2012).

Homophily: While the links between firms and elected congressmen are identified as an almost-random treatment in our context, the full social networks of classmates and alumni, including links to both elected and defeated congressmen, are taken as exogenously given. This definition of social network, while ruling out direct reverse causality, still tolerates the problem of 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 works through social network mechanisms.

We propose a simple, albeit partial, solution: common, time-invariant characteristics of school cohorts can be captured by school fixed effects. The estimated effect is then identified across years and by individuals who went to more than one school. As it turns out, the results are not much affected by the inclusion of school fixed effects, hence homophily is not a prevalent problem for our estimation.

Market pricing mechanism: A different concern arises regarding how political connections are translated into stock price reactions. Our framework, in fact, does not require that all potential investors know about the politician-director educational links and the election outcome. A few investors who follow related firms, including but not restricted to insiders, may be sufficient to create the stock price impact. Furthermore, at the state level, local investors might follow more closely the political connections to local firms. Indeed, we have checked trading volumes, and found evidence that there is particular market attention, in terms of abnormal trading volume, on the connected firms around the election day. As argued above, even if the market incorrectly predicts election probabilities, our estimate is still valid.

Control variables: As explained previously, since the RDD's assignment is almost random, the inclusion of any control variable calculated before the event and the use of various models to calculate CARs will not affect RDD's estimate. Controlling for poll results or campaign contributions, among others, do not affect any results. However, in certain cases additional control variables may improve estimation efficiency and reduce noises.

In summary, our research design consistently estimates the WATE of being connected to a politician in Congress, where the effect is averaged with weights over the sample of all politicians who stand a chance of experiencing a close election.

3. DATA DESCRIPTION

We build our sample using data from several sources. We collect the federal election results from the Federal Election Committee (FEC) website. Every two years, FEC publishes certified federal election results compiled from each state's election office and other official sources. The published data contain information on primary, runoff, and general election results for the U.S. Senate, the U.S. House of Representatives, and, when applicable, the U.S. President. For each election, we identify the candidate finishing first and second and calculate the margin of votes between the top two candidates. A close election is specified by a margin of votes of less than 5%.

As reported in Panel A of Table D1, we identify 128 close elections for U.S. Senate (23 elections) and Congress (105 elections) between 2000 and 2008. The average Win/Loss margin across all elections is 2.54% (2.42% with Senate elections and 2.57% with House of Representatives elections). Panel B shows summary statistics of elections and politicians per year. The average number of elections per election year is 26 (with a maximum of 36 and minimum of 15). Our sample elections involve on average 89 politicians per year; and the average number of connected firms per year is 362.

[Insert Table D1 Here]

Most importantly, we construct a unique dataset through a long process of hand-collecting biographical records of the candidates in these elections using Lexis-Nexis biographies, which contain active and inactive biographies from the *Who's Who* publications. Our scope of search includes 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*. For each candidate, *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 the biographies unavailable in *Who's Who*, we use Library of Congress Web Archives, Internet Archives, politicians' archived websites, and other sources on the World Wide Web. We retain entries for which we can positively identify the politician.

We obtain biographical information and past education history for directors and senior company officers from BoardEx of Management Diagnostics Limited. The dataset includes board directors, senior company officers for active and inactive firms, their employment history, educational backgrounds and their participation in social and charity organizations. We restrict our sample to board directors in U.S. publicly listed firms.

We construct our social network measure through educational institutions.[9] We define a political connection as a link between a firm's director and an election candidate who graduate from the same university's college or professional school within a year. We thereby match *Who's Who* and BoardEx biographies by institutions and degrees. Following Cohen, Frazzini, and Malloy (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. To identify a politician's alumni network, we relax the restriction on graduation year. Finally, we match our data to stock return data from the Center for Research in Security Prices (CRSP).

Panel C reports the distribution of common educational backgrounds of directors and politicians in our sample. Degrees for undergraduate studies seem to be the most prevalent type of connection between directors and politicians: 74.8% of politicians and 86.8% of directors are connected through their undergraduate studies, having graduated from the same school/university within one year. The figures are 9.6% and 3.6% for law school; 7.6% and 4.6% for business school; 6.8% and 4.2% for other graduate degrees. Medical school and doctoral degrees appear to be the least likely to connect politicians and corporate directors. Only 0.4% of politicians and 0.1% of directors are connected through medical school, while 0.8% of politicians and 0.7% of directors are connected through Ph.D. programs.

Panel D reports firm characteristics in our sample and compares them to those in the Compustat universe. The sample's firm market capitalization averages at $2.13 billion, with a maximum of $58.64 billion and a median of $0.40 billion; these figures are fairly comparable to those of the Compustat universe ($2.29 billion, $467.09 billion, and $0.24 billion,

[9] We did not construct links between people previously working in the same firm, as only a few in our sample of politicians have previously worked in a publicly listed firm.

respectively). Our average firm has a market-to-book ratio of 4.50 and age of 8.60 years, as compared to a ratio of 4.30 and age of 8.10 years for an average Compustat firm.

4. MAIN EMPIRICAL RESULTS AND ROBUSTNESS CHECKS

In subsections 4.1 and 4.2, we report results from the main empirical specifications, and propose an explanation of the findings. Subsections 4.3 and 4.4 present results from various subsamples and the network of alumni, respectively. Subsection 4.5 shows robustness checks using alternative and non-parametric estimations.

4.1 RDD ESTIMATIONS OF THE IMPACT OF POLITICAL CONNECTIONS

Similar to the prior literature (Fisman 2001, Faccio 2006, Goldman et al. 2009), we start by investigating connections to strong politicians, namely incumbent congressmen on powerful committees. Empirical results are reported in Table 1. We relate stock price cumulated abnormal returns (CAR) of connected firms around the election day to the connected politician's election result. Each observation pairs a firm's director to a candidate finishing first or second in a close election, both of whom graduate from the same university program within a year (Cohen et al. 2008). We follow conventional event study methods to calculate abnormal returns, cumulated from day -1 to day 5, in a single-factor model, with β estimated from the pre-event window from day -315 to day -61. The event day (day 0) is the election day reported by the Federal Election Commission, which is always a trading day. As we later show in Section 4.5, our results are not sensitive to the method of estimation of abnormal returns, unit of observation, or event window. We limit the sample to elections in which vote share between the top two candidates is between 48.5% and 52.5% (so the margin is at most 5%), and control separately for winners' and losers' vote shares, as suggested by Lee and Lemieux (2010), to obtain the effect at the exact threshold of 50%.

We focus first on firms connected to incumbents. While the average value of connection to an incumbent congressman is estimated to be insignificantly different from zero, as shown in column (1), certain congressmen may be particularly powerful, and garner above-average benefits for their connected firms. We explore this possibility by considering subsamples of members of important committees.

[Insert Table 1 Here]

Column (2) reports that connections to a member of one of the Senate committees who wins a close re-election generate a positive stock price reaction of 8.59% above that of the loser. This large impact is statistically significant at the 1% level. This effect is due mostly to senior members of the Senate. For senators with above-median seniority in committees, the effect is

10.40% and significant at the 1% level, as reported in column (3). Column (4) shows that the effect on firms connected to less senior senators is only 6.40%, significant at the 1% level. This finding confirms the role of seniority in Congress as previously stressed in political science (e.g., Roberts, 1990; Kellerman and Shepsle, 2009).

We also find consistent evidence of the value of connection to members of other committees in Congress. Columns (5) and (6) consider subsamples of Senate committees in charge of appropriations, natural resources, energy, and agriculture; and economic, financial, and budgetary matters, respectively, and show positive and significant impacts of comparable magnitude. Columns (7), (8), and (9) report results on firms connected to incumbents in the House. The estimates are not statistically significant at conventional levels.

Results from Table 1 show that connections to powerful incumbent politicians are beneficial to firms. We next investigate whether the impact is different with firms connected to challengers in close elections to the Congress. For this purpose, we collect information on the positions candidates have held up to election and classify three categories of politicians whose main occupation in the election year was either (1) in a public office at federal level; (2) in a public office at state level or below; or (3) in other environments, including NGOs, labor unions, and independent professions, such as doctors and professors. Table 2 reports the benchmark estimates by the corresponding subsamples.

[Insert Table 2 Here]

Column (1) shows the estimate for the subsample of challengers, including candidates in a race for an open seat from which the incumbent had retired. Contrary to the results on incumbents in Table 1, we find that, among challengers, the estimated effect of political connections is -3.24%, statistically significant at 1%. This result suggests that having a connected politician elected to Congress significantly reduces connected firms' value.

Columns (2) to (6) consider subsamples among challengers. Columns (2) and (3) distinguish between challengers coming from various positions at the federal level (for instance, in a senator's office) and the rest. The effect is -3.5% and significant for the latter, but insignificant and close to zero for the former. Column (4) reports the results on subsamples of challengers who had previously held state-level public offices, with an even stronger estimate of -4.36% (significant at 1%). Column (5) considers challengers with previous experience at top-level state positions, including governors and state legislators, and obtains a similar coefficient of -3.89%, significant at 5%. On the other hand, for the rest of the candidates, the estimated effect is -3.52%, but insignificant, as reported in column (6).

Taken together, Table 2 shows that a candidate's election to Congress appears to significantly destroy value of connected firms if the elected congressman has been sufficiently entrenched in his home state. In contrast, the impact of congressmen coming from positions in federal office is not significantly different from zero.

The value impact of political connections, as reported in Tables 1 and 2, depends on the position and status of the politicians. Table 3 investigates the general impact of political connections in the pooled sample.

[Insert Table 3 Here]

We find an overall negative and statistically significant value effect of connection to a winner in a close election. Column (1) shows our benchmark specification (vote share margin of 5% or less, controlling separately for winners' and losers' vote shares) with 1,819 observations across 1,268 firms and 170 politicians. We find an estimate of -2.65%, significant at 1%. Column (2) controls additionally for quartic polynomials of winners' and of losers' vote shares, and reports an even larger effect of -4.07%, significant at 1%. The results are similarly negative for polynomials of higher or lower degrees.

Columns (3) to (8) further show that the results are unaffected by "irrelevant covariates." As argued in section 2, the inclusion of any additional control variable calculated before the event should not significantly alter the estimate of the treatment effect. Column (3) controls for characteristics of the politician (dummy variables for the party, gender, incumbency, Senate/House race), column (4) for connected directors' characteristics (age, gender, nationality, executive/non-executive role), column (5) for firm characteristics (market capitalization, book value of equity, total assets, return on asset, capital expenditure, and leverage), producing estimates very close to the benchmark in column (1) and all significant at 1%. In a similar vein, unobservable characteristics of the election year or the industry also appear to be irrelevant covariates and thus do not alter much the main estimate, as shown in columns (6) and (7). As expected, the main results are not driven by any year-specific or industry-specific unobservables.

Including fixed effects for educational institutions, however, may substantially affect the main estimate, if a strong homophily factor pertains in the formation of the school networks that we consider, as discussed in the previous section. Controlling for school fixed effects, column (8) still produces a similar, slightly larger estimate of -2.75%, significant at 1%. It implies that network homophily is relatively unimportant to our treatment, and that shared school characteristics are not the factor behind the negative estimate of the value of connection reported in Table 3.[10]

While the cross-sectional distribution of CARs includes some very large observations, column (9) shows that even after taking out all CARs exceeding 50% in absolute value, the result still remains strong at -2.30% (significant at 1%).

In summary, Table 3 provides evidence that firms connected to the winner in a close election to the U.S. Congress between 2000 and 2008 experience, on average, significant loss in firm value, as compared with firms connected to the loser. The results remain consistent when we control for politicians' characteristics, firm size, election year-, industry- and school-fixed effects. The absolute size of the effect, namely -2.65% after 7 days, is 24% of the standard deviation of CARs in our sample. In comparison to other event studies, Faccio (2006) reports an average effect of 1.43% on CARs for worldwide firms experiencing an event of new political connection, while Goldman et al. (2009) show an effect on CARs of 8.97% in difference between Republican-connected and Democrat-connected firms in the event of the 2000 presidential election.

4.2 Explanation of the results

Our finding of a value-reducing effect of political connections appears, at first glance, counterintuitive and different from the extant literature. It is however consistent with the explanation that the value of political connection depends on the politician's position in a more complex way than previously studied: the value of connection to a congressman initially drops when the freshly elected congressman moves away from his previous position at state level, as proved in Table 2, and only increases once he becomes senior and powerful in Congress, as seen in Table 1. Before their elections to Congress, many politicians have held positions at the state level, which has probably already resulted in benefits for connected firms. If a politician wins his congressional election and moves to federal politics, connected firms' benefits may be much harder to maintain. On the one hand, an elected politician will have less time and focus for specific state matters that relate to their connected firms. He may need to accumulate experience and power over time, through a learning curve with much electoral uncertainties, until he is senior enough to support connected firms.

On the other hand, the strong checks and balances in federal politics in the United States may already block most channels by which firms connected with politicians through social networks could obtain significant financial benefits, as shown by Fisman et al. (2006) in the example of firms connected to former Vice President Dick Cheney. Consequently,

[10] We do not include company fixed effects, as there is very little variation within companies across years, with the majority of companies appearing only once, thus omitted from such a fixed-effect regression.

from a firm's perspective, it may be preferable that its socially connected politician remain at the state level, rather than get elected to federal office.

This line of argument reaffirms previous findings of positive values of connections to key politicians, by Goldman et al. (2009), Acemoglu et al. (2010), among others, as Table 1 consistently shows that connections to closely elected powerful incumbent congressmen significantly increase firm value, compared with connections to closely defeated powerful incumbents. Similarly, the value-reducing effect reported in Table 2 indicates that connections to state-level politicians are even more valuable than connections to junior Congress members. The overall results of Table 3 imply that the value-reducing effect caused by challengers outweighs the gain associated with incumbents in our sample.

There is further evidence that these effects are not coincidental. We use a market model from day -315 to day -61 before each election event to calculate the abnormal daily trading volume around the election day (Campbell and Wasley 1996) The results show that stocks in our sample are significantly more widely traded around the event, with 5.21% cumulative abnormal volume during the window (-5,-1), and 2.22% cumulative abnormal volume during the window (-1, 5), both statistics are significant at 1%. It implies that at least a part of the market does pay particular attention to those stocks during the relevant elections.

It is important to note that the types of political connections we study do not need to be salient market-wide, in order for the relevant prices to fully react to news from the elections. Instead, a few traders or investors with privileged information on political connections can be sufficient to move market prices of connected stocks.

4.3 EFFECTS BY DIFFERENT SUBSAMPLES

The previous sub-section shows the robust, consistent, and strong impact of political connections on firm value. We now explore whether that impact is present in different subsamples, and report results in Table 4.

[Insert Table 4 Here]

Our identification strategy is based on close Senate and House elections from 2000 to 2008. As the Senate and the House serve different missions, one might expect that the value of a firm's connection to a member of the House or to a member of the Senate might be different. We thus rerun the benchmark regression in column (2) of Table 1 for subsamples of members of the Senate and the House, and report the respective results in columns (1) and (2) of Table 4. For both subsamples, the results are consistent with our pooled regression results from Table 1, and significant at 1% and 10% respectively. Firms connected to the winner experience significant loss in firm value, with stronger effects for close Senate elections, as compared with close House elections (-4.24% against -2.14%).

We also explore whether a candidate belongs to the chamber majority or chamber minority affect our results, by partitioning the sample accordingly. Regression results from columns (3) and (4) show a loss of value effect in both subsamples. In columns (5) and (6), we further explore the sample of Democrats and of Republicans. In both cases, the effects are statistically significant at 5%, and very close to each other. This result echoes Snowberg et al.'s (2007) finding that, when it comes to holding majority in Congress, partisan differences matter little to the market.

We further investigate the variation of the estimate by the nature of social links. We sort educational institutions by the number of observations in the sample, considered as proxy for the prominence of each school-based social network. When a network is better represented in the sample, its links are arguably stronger in Granovetter's (1974) sense, in that each pair shares more common connections among high-profile politicians and businessmen. Such a network has a higher measure of Karlan et al.'s (2009) network closure, therefore is more conducive to agreements that require commitments between pairs in the network, such as a business, tit-for-tat deal. In contrast, Karlan et al. (2009) also show that a low level of network closure leads to better information sharing.

Through this exercise, Harvard comes out as the most represented university (large state universities would have dominated if class size were used.) Column (7) reports a strong estimate of -5.45% for the subsamples of connections based on the Harvard network, including all undergraduates and graduates. Columns (8) and (9) show the results for the subsamples of universities that rank below and above median by number of observations, both significant at 5%. The effect is markedly stronger for Harvard, yet little difference exists between the subsamples above and below median. Network strength and network closure thus appear to matter only at the very top schools and not elsewhere. The evidence suggests that connections are valuable more as commitment devices for deals than as information sharing channel, at least for most prevalent universities. Results for Harvard and Yale, the two most represented universities combined, are similar and available upon request.

In summary, Table 4 shows that our finding – that connections to a politician in a close election incur a significant loss in firm value – is consistent and robust across several subsamples and subgroups.

4.4 ALUMNI NETWORKS

We have so far defined social links between a board director of a firm and a politician who graduate within one year from the same university, campus, college, or professional school. In this subsection, we consider alumni networks by relaxing the restriction on graduation year. Columns (1) to (8) in Table 5 report tests replicated from Table 3.

[Insert Table 5 Here]

The benchmark regression in column (2) shows that an additional connection to an elected politician in alumni networks reduces a firm's CARs by 0.58%. This estimate, statistically significant at 5%, is much smaller than the estimate of -2.65% for classmate networks in column (1) of Table 3. Across the columns of Table 5, the negative and significant estimates of the value of alumni-network political connection on the CARs remain consistent, but with coefficient sizes much smaller than in Table 3.

The smaller estimates in Table 5, as compared with Table 3, can be explained in two different ways. First, the links between alumni who are not classmates should be less important than those between classmates. Therefore the average effect over all pairs of connected individuals should be smaller in size in alumni networks than in classmate networks. Second, as our connection variable is only a proxy for friendships or acquaintances in reality, measurement errors will likely produce an attenuation bias on our estimates. As the alumni networks are more prone to measurement errors, the attenuation bias will be larger for the alumni networks, leading to smaller estimates, as found in Table 5.

Overall, Table 5 shows that our main results on political connections remain consistent among alumni, a sample constructed with relaxed definition of social networks.

4.5 ALTERNATIVE SPECIFICATIONS AND ROBUSTNESS CHECKS

In this subsection, we explore alternative specifications with different event windows and calculations of the CARs. Table 6 summarizes this exercise.

[Insert Table 6 Here]

In Panel A, we check the consistency of our results by varying the event windows used in Table 3. Column (1) of Panel A reports the results of regressions using CARs from a pre-event window from day -7 to day -1. The coefficient of interest is very small and not statistically significant. This shows that the treatment has not been predicted by the market prior to the event, as expected from the close elections design.

While column (3) reproduces Table 3's benchmark result for the (-1,5) window, columns (2), (4), and (5) consider different event windows, (-1,1), (0,5) and (1,5) respectively, and report consistently negative and significant coefficients. Interestingly, we find negative and significant coefficients on the Win/Lose dummy, of about 70% the size of the benchmark estimate in column (5) for the (1, 5) window. This result implies that market reaction after one day accounts to only about 30% of the full effect, and substantial further reaction occurs even after day 1 up to day 5. We can consequently create a portfolio on day 1 after the event, having known all election results, shorting on firms connected to closely elected politicians and longing on those connected to closely defeated ones, with equal weights on firm connections. Over (1, 5), this portfolio yields a risk-free return of 1.85%.

Beyond our benchmark window, for the window (6, 20) after the elections, column (6) reports an insignificant estimate of the value of connection. While this finding is consistent with the market having fully priced in the news after day 5, it could also be due to the presence of other noises, which hinders statistical significance.

In all regressions throughout the paper, we calculate the heteroskedasticity-corrected standard errors clustered at the level of politician-election year level to avoid the potential downward bias of standard error estimates when the error terms are autocorrelated among observations sharing the same politician and election year (Bertrand, Duflo, and Mullainathan 2004). The qualitative results are strongly robust to other levels of clustering, including by director, firm, year, state, and to two-way clustering, and are available upon request.

Given the high cross-sectional variance of CARs, one may worry that our results are affected by stocks with aberrantly high volatility. Simply censoring aberrant values, as shown in column (9) of Table 3, may not entirely solve the issue, because of a potential censoring bias. A different approach consists of normalizing each stock's CAR by its standard deviation derived from the market model within the event window. Panel B of Table 6 repeats Table 3's regressions with this new outcome variable, with the same qualitative results as in Panel A. Being connected to an elected politician has a statistically significant impact of about negative 32.2% on a firm's standardized CAR, or about one third of a standard deviation of the firm's CARs during the event window.

In further robustness checks, Table 7 reports nonparametric specifications as detailed in Section 2. Column (1) shows a 1%-statistically significant estimated effect of negative 3.40%, even stronger than in Table 3. Columns (2) to (5) indicate that the effect is robust in size and statistically significant across a wide range of bandwidths used in the local polynomial regressions.

[Insert Table 7 Here]

In columns (6) to (9), we further test the robustness of our result by applying the same method to "placebo" thresholds of vote share, instead of the actual cut-off at 50%. For example, in the sample used for column (6), a politician is marked as elected if his vote share is 48% or above, and marked as defeated otherwise. We then apply the nonparametric regression around the placebo cutoff of 48% and report the corresponding estimate. Given the counterfactual placebo threshold, we do not expect to find results similar to column (1). Columns (6) to (9) confirm our prediction: for the placebo thresholds of 48%, 49%, 51% and 52%, the estimate is always positive and mostly not statistically significant at 10%.

Figures 1.A and 1.B visualize the numerical results presented in Table 7 by plotting the outcome variable, namely firms' CARs over the window (-1,5), against vote shares. Each graph represents the fitted local polynomial of degree 3 for vote shares, separately for elected or defeated politicians. Figure 1.A also includes bins of actual observations, represented as dots, while they are removed in figure 1.B to clarify the gap at the discontinuity point of 50% vote share. There is a sizeable gap at discontinuity, whereas the gradient of the graph is relatively small elsewhere, as already tested with placebo thresholds in Table 7. Furthermore, there is visual evidence of a "Z" shape: CAR is first increasing in vote share, then drops sharply at the threshold of 50%, and then increases again. This Z shape is predicted in a pricing model (Cuñat et al.'s, 2009), where the market internalizes available information before election and partially anticipates the discontinuity effect at 50%, to an extent proportionate to the difference between prior probabilities of winning or losing and the threshold of 50%. For example, for an election resulting in vote shares of 52% versus 48%, the market should have rationally expected the first candidate's winning probability around 52%, hence a part of the discontinuity effect has already been incorporated in market prices even before the election. That explains why we do not see a large difference between the CARs at 48% and at 52% vote shares on the graphs in Figure 1.

[Insert Figures 1.A and 1.B Here]

The Z shape is imperfect, however, because it could be distorted by confounding unobserved factors affecting the whole range of vote shares between 48.5% and 52.5%. The RDD only guarantees the consistency of our estimate at the 50% threshold.

Additional checks in the Appendices demonstrate that the results are strongly robust to different methods of calculation of CARs (Fama-French three- and Fama-French-Carhart four-factor models, and raw returns), different units of observations (collapse by firm or director), and two-way clustering of standard errors. We then verify RDD's near-randomness by regressions showing that none of the observed characteristics related to politicians, directors, firms, industries and states experience the discontinuity jump at 50% vote share. Finally, quantile regressions show that the negative effect comes mostly from lower quantiles, i.e. from firms with negative CARs.

In summary, Tables 6 and 7, and Appendices Tables show that our results are robust to different methodological specifications. Furthermore, they are found only in specifications where the treatment matters, and not in placebo tests. Consequently, political connection must be the causal factor behind these results.

5. TESTS OF FURTHER PREDICTIONS

The previous sections have shown an overall negative impact of political connections on firm value. In this section, we investigate potential channels of this effect. Based on prior literature, we advance four predictions:

Prediction 1: In states with stronger institutional checks and balances, firms receive fewer benefits from their state-level political connections through social networks.

Prediction 2: Firm characteristics, such as firm size and dependence on external finance, determine the value of political connections.

Prediction 3: Firm activities in the connected politician's state will likely decline following the politician's successful election, as compared with an unsuccessful one.

Prediction 4: Directors are more likely to leave the firm following the connected politician's successful election, as compared with an unsuccessful one.

We will test these predictions on sub-samples based on institution quality measures, firm characteristics, firm activities, and directors. We run the benchmark regression in each subsample and compare the estimates. The following subsections will detail the corresponding results.

5.1 STATE CHARACTERISTICS AND THE VALUE OF POLITICAL CONNECTIONS

Moving beyond politicians' backgrounds, Prediction 1 concerns a different dimension of our explanation: under better checks and balances at the state level, the estimated effect of connection should be weaker. Table 8 shows ample supports for this prediction.[11]

[Insert Table 8 Here]

Columns (1) and (2) distinguish between states having more or less than median regulations. The index of regulation by state is measured for 1999 in Clemson University's Report on Economic Freedom, http://freedom.clemson.edu. This report combines information on labor and environmental regulations and regulations in specific industries such as insurance. As expected, we find a large negative and significant effect in states with more regulations, where there is greater potential for politicians to grant benefits to connected firms on a discretionary basis.

[11]These results are also confirmed by regressions that include an interaction between our main explanatory variable, Win/Lose, and a dummy variable for good/bad institutions as in Table 8. These regressions implicitly impose the same coefficients of vote shares in the subsamples of winners and losers, unlike suggested by Lee and Lemieux (2010). The corresponding results are available upon request.

Instead of regulations, columns (3) to (8) divide states by actual level of corruption. The most commonly used measure of state-level corruption comes from Glaeser and Saks (2006), who extract actual conviction data from the Department of Justice's "Report to Congress on the Activities and Operations of the Public Integrity Section" to form a measure of the ratio of convicted corruption cases by population size, averaged from 1976 to 2002 to remove periodical noises. Using that measure, columns (3) and (4) show a more sizable and significant effect in more corrupt states.

As one may expect that actual conviction cases only amount to a small fraction of real corrupt deals, the measure of actual conviction may not truly depict the extent of corruption in a state. We overcome this concern by using Saiz and Simonsohn's (2008) approach of "downloading wisdom from online crowds." More specifically, columns (5) and (6) use a measure of web-based 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, to divide the sample of all states into those with higher or lower than median corruption, as reported in the news. For columns (7) and (8), we use the dataset of all newspapers gathered in Newslibrary.com to search for the word "corruption" close to the state name, then normalize the resulting number of search hits by that for the state name alone. Both measures, covering corruption cases reported on the internet and in newspapers, unambiguously support our prediction: the negative impact of connections to elected congressmen is stronger and more statistically significant in more corrupt states.

While the RDD correctly identifies the value of political connections, it is harder to ascertain that its variation across states is caused by the differences in institutional quality. While we avoid direct reverse causation by using some measures calculated before 2000, the results are still exposed to endogenous selection by unobservables, such as historical or cultural factors, that may cause both institution quality and the value of political connections across states. In columns (9) and (10) we control for this problem by using GCISC, a measure of population concentration around the state capital city in 1970. As shown by Campante and Do (2012), this measure is strongly predictive of state-level corruption across American states (higher concentration around state capital implies better media coverage of state politics, therefore less corruption.) This measure is highly persistent over time, and arguably not directly affected by unobservable determinants of corruption. In support of our prediction, the estimated effect is stronger and more statistically significant among states of lower-than-median population concentration shown in column (9), than those in column (10).

In sum, Table 8 provides evidence that the estimated effect of connections to elected congressmen is all the more important in states that are more corrupt, have more regulations, and have worse institutions. It is entirely in accordance with our explanation for the differential value of political connections between state-level politics and federal politics.

5.2 FIRM CHARACTERISTICS AND THE VALUE OF POLITICAL CONNECTIONS

We now study firm characteristics as potential determinants of the relationship between political connections and firm value, and detail the results in Table 9.

[Insert Table 9 Here]

Columns (1) and (2) report regression results on two subsamples of firms whose market capitalization is above or below the median. The contrasting results indicate that smaller politically connected firms experience greater loss of value when the connected politician wins an election to Congress (loss of 6.56% for smaller firms, significant at 1%, as compared with no effect among larger firms.) To put differently, political connections are more important to smaller firms. Larger firms may be connected to many politicians, and the financial benefit of connection to one more politician may represent only a small fraction of the firm's value; hence, for larger firms, we expect a smaller effect.

One may conjecture that firms benefit from political connections thanks to easier access to finance, as shown by Khwaja and Mian (2005). Accordingly, we investigate whether the value of political connection is associated with the firm's dependence on external finance. We construct Rajan and Zingales's (1998) measure of dependence on external finance by 3-digit SIC industries as the industry average of *(CapEx – Cash flow from Operations)/CapEx*, then divide our sample into industries with above and below median scores. Columns (3) and (4) of Table 9 report benchmark regression results on these two sub-samples. For industries relying more on external finance, the estimated effect is -2.99% and significant at 5%; in contrast, for the subsample of industries less dependent on external financial sources, the estimated effect is insignificant at conventional levels. Firms that are financially independent seem not to be affected after election results.

The effect appears to be particularly strong in some subsamples selected based on two determinants. Column (5) shows that small firms that rely heavily on external finance incur a very high loss of value: the average loss is 5.64% (significant at 1%) as a result of a connected politician's election success. Column (6) considers the subsample of states with higher than median corruption (using the Newslibrary.com measure) and to which the distance from the firm's headquarter is in the smallest quartile (less than 650km). Such distance is a proxy for the presence and interests of the firm in the politician's state, as we expect the effect to be stronger for firms that do more business in that state. The estimated effect in column (6) of Table 9 is even stronger than that in column (7) of Table 8.[12] In column (7) of Table 9, the sample is limited to states with higher than median corruption and industries with higher than median reliance on external finance. The effect is strongly significant, and much larger than those in Table 9's column (3) and Table 8's column (7).

5.3 POLITICAL CONNECTIONS AND FIRMS ACTIVITIES IN CORRESPONDING STATES

Table 9's estimation results indirectly corroborate the storyline that firms benefit from politicians before their election to federal office, relying on market reaction to news to infer firm's values. A more direct test of Prediction 3 can be based on the change in firm activities after the event of the election. Unfortunately, systematic data on firm activities by state and year, measured either by sales or investment, are not publicly available.

We overcome this difficulty by providing a new measure of firm activities by state and year. Again, we follow Saiz and Simonsohn's (2008) idea of "downloading wisdom" by searching each company's name through local newspapers in the connected politician's state within each year, using Newslibrary.com. We also normalize the number of search hits by that for the neutral keyword "September" across the same set of newspapers. The resulting hit rate proxies for a firm's activities within a state in a certain year. At the national level, this variable is remarkably correlated with changes in firm sales, investments, R&D, employment, and cash flows (results are available upon request.) We further remove any firm-state unobservable characteristics by taking the difference in the hit rate after each year, and use this measure across various windows and subsamples as the dependent variable in our benchmark regression. Table 10 reports the results.

[Insert Table 10 Here]

Columns (1) to (3) focus on the subsample of challengers with state experience used in Table 2. Column (1) shows that being connected to a newly elected congressman significantly reduces a firm's activities in the corresponding state from the election year (in which elections are held in November) to the following year. The estimate of -0.0096 is about half a standard deviation (0.0187) of changes in hit rates. Column (2) presents a pre-event placebo test, from one year before until the election year. The resulting small estimate – insignificantly different from zero – indicates that the treated and control samples are very similar before the event, thus confirming the RDD. We notice from column (3) that any adjustment following the event is accomplished within 1 year after the election, as the estimated effect is close to zero for the window from year 1 to year 2. Column (4) reports an

[12] Similar results on partitions based on the distance between firm headquarters and politician's state are available upon request.

even larger effect for the subsample of challengers with top state experience.

Focusing on the main event window from the election year to the year after, columns (5) and (6) of Table 10 follow Table 2 in treating subsamples of candidates from federal offices and from other backgrounds. We do not see any significant results in those subsamples, confirming the prediction that the effect on firm activities passes uniquely through the movement of politicians from state to federal offices.

The examination of firm characteristics and activities by state, as shown in Tables 9 and 10, thereby provides evidence that certain firms benefit from political connections at state level more than others, and that such firms are more likely to move out of the state when the favor is over.

5.4 POLITICAL CONNECTIONS AND DIRECTORS' TENURE AFTER ELECTION

Based on our explanation, as firms benefit more from connections to defeated Congress candidates, compared with elected ones, the directors bridging those connections are also more valuable. We therefore predict that directors connected to defeated politicians are more likely to remain longer at the firm. Similar to prediction 3, this prediction does not depend on market reactions to news. Table 11 reports tests of this prediction.

[Insert Table 11 Here]

Columns (1) to (7) report RDD regressions similar to Table 10, using the connected director's remaining time length at the firm as dependent variable, and controlling for directors' elapsed tenure. Column (1) considers the subsample of all incumbent candidates, and reports that directors connected to winners or losers do not experience significant differences in their post-event tenure, confirming results in column 1 of Table 1. In contrast, the result is strongly negative and statistically significant at 5% for the subsample of challengers shown in column (2): directors connected to elected challengers remain in their positions on average 2 years less than those connected to defeated challengers. Repeating the subsamples used in Table 10, we find a negative effect of -2.6 years in column (3) for politicians holding a position in state politics, and of -3.7 years in column (4) for politicians with prior experience in top state positions. As expected, the estimate is not significant for the samples of challengers from federal offices in column (5) and from other backgrounds in column (6). On the other hand, column (8) shows that a connection to winner or loser does not predict the director's elapsed tenure before the election, thus confirms the RDD's near-randomness of treatment. Finally, column (7) shows that the effect is strongly driven by the middle quintile of director's age (from 52 to 56 years old), and not by too young or too old directors. (Results for all age quintiles are available upon request.)

The evidence in Table 11 is thus consistent with prediction 4, that after the politician leaves state politics, the connected director is less likely to remain with the firm. This may result from either his reduced value to the firm, or his better options elsewhere, thanks to his connections. Taken together with the verifications of predictions 1 to 3 across Tables 8 to 10, this section provides a wide array of support for the explanation that politicians bring more benefits to connected firms before than they do after elections to Congress.

6. CONCLUDING REMARKS

This paper investigates corporate benefits of political connections from the social network of directors and politicians. We use the Regression Discontinuity Design (RDD) to identify the value of connection to a politician elected to the U.S. Congress in a closely contested race. The estimate during the period 2000 to 2008 shows an economically and statistically significant impact of connection on cumulative abnormal return of negative 2.65% surrounding the election date. The results are robust to various specifications, throughout different measures of outcome variables, with different definitions of social network, and across many subsamples.

Our contribution to the existing literature is twofold. First, we propose an internally valid identification strategy using the RDD of close elections that deals with the endogeneity of political connections more effectively than traditional event study methods. The results' external validity builds on the fact that the estimated Weighted Average Treatment Effect (WATE) is averaged over the sample of all politicians susceptible to experience a close election, and that firms in our sample are comparable to Compustat's universe.

Second, we uncover complex variations in the value of connection to U.S. congressmen. Strong connections, e.g. connections to powerful incumbent members of Senate committees, are associated with positive stock price reaction. We also find an average negative value of connection to newly elected congressmen, especially with congressmen coming from state politics in more corrupt states. The facts are consistent with an explanation that firms benefit more from political connections when the connected politician remains in state politics than when he moves to the federal level. We empirically test several resulting predictions and find a wide range of evidence supporting our hypothesis. Do et al. (2012) provides further support in showing that firms' connections to governors elected in close elections create value and enjoy better (lagged) accounting performances.

A note of caution might arise in generalizing the empirical results for several reasons. First, while our estimate is a 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 on those politicians than some others who expectedly win (or lose) by large margins. Second, our analysis is limited to elections from 2000 to 2008, given excessive data collection costs, and focuses uniquely on classmates' social links. Extrapolations before and after this period, or towards other types of political connections, require careful consideration. Third, we measure market valuations from investors' trading behaviors according to their beliefs and expectations, and investors may be surprised by actual behaviors of firms and politicians after the elections. (Our results on firm's activities and director's tenure are not affected by this concern.) We also stop short of inferring potential effects on general welfare. These topics are natural targets for future research in this line of work.

Overall, our study identifies the value of political connections through social networks in the United States and uncovers its variation across various state and federal political environments. This remarkable gap in the value of connections calls for further attention and research on the theory and empirics of political connections, and on state-level institutional design.

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APPENDICES (FOR ONLINE PUBLICATION)

METHODOLOGY

From Lee and Lemieux (2010), the Average Treatment Effect we estimate is defined and expressed as:

$$\beta_{RDD} \stackrel{\text{def}}{=} \lim_{VoteShare \downarrow 50\%} E(CAR_i|Win) - \lim_{VoteShare \uparrow 50\%} E(CAR_i|Lose)$$
$$= E(CAR_i(Win) - CAR_i(Lose)|VoteShare = 50\%). \quad (1)$$

β_{RDD} identifies the impact on *CAR* when equation (1) holds, under the assumption (A1) that the density of *VoteShare*, conditional on all characteristics of an observation, is continuous. Indeed, this condition implies that for any pre-election characteristics U, observable or unobservable, the expected difference at 50% vote share is:

$$\lim_{VoteShare \downarrow 50\%} E(U_i|Win) - \lim_{VoteShare \uparrow 50\%} E(U_i|Lose)$$
$$= E(U_i(Win) - U_i(Lose)|VoteShare = 50\%) = 0. \quad (2)$$

So winning and losing are randomized at the limit of 50%, and the difference at 50% must be the impact of winning versus losing. Assumption (A1) is guaranteed when the incidence of winning cannot be perfectly manipulated by candidates (Lee and Lemieux, 2010).

The variables $CAR_i(Win)$ and $CAR_i(Lose)$ represent market reactions (Cumulated Abnormal Returns) to election results, expressed as $P_i - PreP_i$. P_i is the stock price contingent on election outcome, and the pre-event price $PreP_i$ takes into account the pre-event market's predicted probability of winning $MPPW_i$: $PreP_i = MPPW_i \times \big(P_i(Win) - P_i(Lose)\big) + \pi_i$, where $P_i(Win) - P_i(Lose)$ is the value of being connected to an elected candidate, and π_i is the remaining component independent of the event. From equation (2): $E(MPPW_i(Win)|VoteShare = 50\%) = E(MPPW_i(Lose)|VoteShare = 50\%)$, so:

$$\beta_{RDD} = \lim_{VoteShare \downarrow 50\%} E(P_i - PreP_i|Win) - \lim_{VoteShare \uparrow 50\%} E(P_i - PreP_i|Lose)$$
$$= E(P_i(Win) - P_i(Lose)|VoteShare = 50\%).$$

Therefore, β_{RDD} correctly identifies the value of connection, even when the market's predicted probability of winning is wrong (when even *CAR* is incorrectly estimated).

Moreover, if we let the effect be heterogeneous across observations, i.e., $\beta(W_i)$ with W_i representing all observable and unobservable characteristics of each observation i, then the estimate can be rewritten as follows:

$$\beta_{RDD} = \int \beta(W) \frac{f(50\%|W)}{f(50\%)} dG(W),$$

where $G(W)$ is the cumulative distribution of W, $f(x)$ is the density of *VoteShare*, and the weight $\frac{f(50\%|W)}{f(50\%)}$ represents the ex-ante likelihood of an observation with characteristics W to produce a close election. β_{RDD} is thus a Weighted Average Treatment Effect across all possible observations.

In the nonparametric specification, the standard error is calculated as a standard error of the difference of two independent variables, as the two subsamples are completely separate from one another. Cluster-adjusted standard errors are not shown. In each local polynomial regression, the clusters near the threshold are very similar to single observations, therefore cluster-adjusted standard errors will not differ much from unclustered ones.

ADDITIONAL ROBUSTNESS CHECKS

In other tests of robustness reported in Appendix Table A1, we calculate the CARs using different methods, including the cumulative daily stock (raw) returns in columns (1) and (2), Fama-French's three-factor model (Fama and French 1993) in columns (3) and (4), and the four-factor model (Carhart 1997) in column (5) and (6). We find estimates mostly similar to those reported in Table 3, either including or excluding school fixed effects.

Appendix Table A1 also reports results for alternative specifications of a unit of observation. In the benchmark model, we choose an observation as a classmate connection between a politician and a director for a given election year, where the treatment variable is binary. That empirical design implies the interpretation of the estimate as the WATE of an additional connection to a politician in office. In alternative specifications, we can choose a unit of observation as a director or a firm (each for a given election day), where the treatment variable is the count of connections to elected politicians. The difference is in the weights: while each connection has the same weight in the benchmark setup, in alternative specifications, the same-weight unit could be director, or firm, or politician. Columns (7) to (9) of Appendix Table A1 show very similar results. Finally, column (10) reruns the benchmark regression in Table 3 with standard errors subject to two-way clustering of both Politician-Year and Company-Year (Cameron et al., 2011), yielding similar results.

We check the near-randomness of winning or losing a close election as highlighted by Lee (2008) and report supporting results in Appendix Table A2. Each column serves to show that a dependent variable's distribution is continuous at the cutoff point of 50% vote share. These dependent variables are those used as control variable in Tables (1) to (10). Panel A shows results for politician's gender, age, chamber, logarithm of campaign contribution, logarithm of number of contributors, and incumbency. Panel B considers challenger's party and different backgrounds, director's age, gender and executive/non-executive role, and social network size. Panel C displays results with different firm characteristics. Panel D reports regressions with industry's financial dependence, state's institution quality and corruption measured in different ways. Across regressions we do not find any significant relationship between different dependent variables and our main independent variable (Win/Lose dummy). This confirms the near-randomness of the win/lose treatment induced by close elections for US Senate and Congress between 2000 and 2008.

We also investigate the heterogeneous effects of political connections from quantile regressions for challengers from state politics, and report supporting results in Appendix Table A3. Column (1) shows results from median regression (50% quantile). Columns (2) to (5) report the estimation using quantile regressions at the 1st, 2nd, 3rd and 4th quintiles. The estimate's negativity is shown to come mostly from the lower quintiles, i.e. among firms with negative CARs.

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DESCRIPTION OF VARIABLES

Variable Name	Variable Definitions and Constructions	Source
	Social Connection	
Alumni network	A firm's director is connected to a Congress election candidate if both graduate from the same university degree program. Following Cohen, Frazzini, and Malloy (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.	BoardEx, Lexis-Nexis biographies, and authors' manually collected data

Classmate network	A firm's director is connected to a Congress election candidate if both graduate from the same university degree program within a year. Following Cohen, Frazzini, and Malloy (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.	BoardEx, Lexis-Nexis biographies, and authors' manually collected data
	Politician Variables	
Campaign Contribution	Total campaign contribution (in dollar values) that a candidate receives in an election.	FEC
House/Senate	Indicates the race for House of representatives or Senate.	FEC
Incumbency	Indicates whether the candidate is the incumbent.	FEC
Number of Contributors	Total number of contributors in an election.	FEC
Party affiliation	The politician's party affiliation.	FEC
Politician's current position	Marked as "from state politics" if coming from a position in a legislative, executive or judiciary body in a state. Marked as "from federal office" if coming from a political position at federal level.	Lexis-Nexis biographies and authors' manually collected data
Senate "Resource" Committees	Includes appropriations, energy & natural resources, and agriculture committees.	Congressional Record
Senate "Economy, Budget & Finance" Committees	Includes budget, commerce, banking, finance, small business, taxation, economic, and public works committees.	Congressional Record
Seniority	Average number of years across all committee memberships by each congressman.	Congressional Record
Top state experience	Politician having previously held position(s) in state legislative bodies or as governors.	Lexis-Nexis biographies and authors' manually collected data
Vote shares	The vote share between the top two candidates.	FEC
	State Variables	
Corrupt Conviction	The average ratio of convicted corruption cases by population size for each state from 1976 to 2002.	Department of Justice, Glaeser and Saks 2006
Corrupt Main City	Web-based 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 in 2009 (Saiz and Simonsohn 2008).	Exalead.com
Corrupt State	Number of hits for the word "corruption" close to the state name based on all newspapers gathered in Newslibrary.com, normalized the resulting number of search hits by that for the state name alone in 2009 (Campante and Do 2012).	Newslibrary.com
GCISC 1970	Population concentration around the state capital in 1970 (Campante and Do 2012).	U.S. Census Data, Campante and Do 2012
Regulation	State-level regulation index that combines information on labor and environmental regulations and regulations in specific industries, measured in 1999.	Report on Economic Freedom
	Firm and Director Variables	
Age	The number of years from IPO date.	Compustat
Capital expenditure	Capital expenditure (CAPX)/book value of common equity (CEQ).	Compustat
CAR	Cumulative Abnormal Returns (CAR) are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. CAR-FF are CAR calculated based on the	CRSP/Fama-French

	Fama-French (1993) three-factor model. CAR - FFM are CAR calculated based on Fama-French (1993) plus momentum four-factor model. CAR – raw returns are calculated based on raw returns.	
Cash reserve ratio	Cash and short-term investments (CHE)/total assets (AT).	Compustat
Common equity	Book value of common equity (CEQ).	Compustat
Distance	Distance between the firm's headquarter and election state capital is calculated based on the coordinates of the two ZIP codes.	Boardex/ZIP
External finance dependence	Industry median of the sum of firms' use of external finance over the 1990s divided by the sum of capital expenditure over the 1990s. External finance is defined as capital expenditure (CAPX) - net cash flow from operations (OANCFC), divided by CAPX (Rajan and Zingales 1998).	Compustat
Firms activities	Firm activities in a given state in a given year are measured as the number of search hits for the firm's name in all local newspapers, normalized by the number of search hits for the neutral keyword "September".	Newslibrary.com
Leverage	Book value of debts (DLC + DLTT) over book value of total assets (DLC + DLTT + CEQ).	Compustat
Market value of equity	Market value of total equity (CSHO*PRCC_F).	CRSP
Market to book ratio	Market value of total equity (CSHO*PRCC_F)/book value of common equity (CEQ).	Compustat
Payout	Total dividends (DVT) + purchase of common and preferred stock (PRSTKC).	Compustat
Q	Total assets - total shareholder's equity + market value of total equity (CSHO*PRCC_F)/total assets.	Compustat
RND	Research and development expenditure (XRD)/total assets (AT) at (t-1).	Compustat
ROA	Income before extraordinary items (IB)/total assets (AT) at (t-1).	Compustat
SCAR	Standardized CARs are CARs normalized by volatility during the event period.	CRSP
Tangibility	Net value of property, plants, equipment (PPENT)/total assets (AT).	Compustat
Director Characteristics	Include the director's age, gender, nationality, and a dummy variable indicating whether the director has an executive role.	BoardEx
Director's Remaining Time at Firm	Director's End Date – Election Date	BoardEx

FIGURE 1.A: DISCONTINUITY EFFECT WITH BINS OF OBSERVATIONS

This figure shows the RDD with Cumulative Abnormal Returns (CAR) of connected firms around close elections to US Congress between 2000 and 2008. The x-axis represents vote share between the top two candidates, and the y-axis represents CARs of firms with directors having graduated from the same university program within a year of the politician. CARs are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. The lines are predicted CARs from local cubic polynomial regressions on samples of vote shares above or below 50%, with 95%-confidence intervals in shaded areas. The dots represent bins of observations.

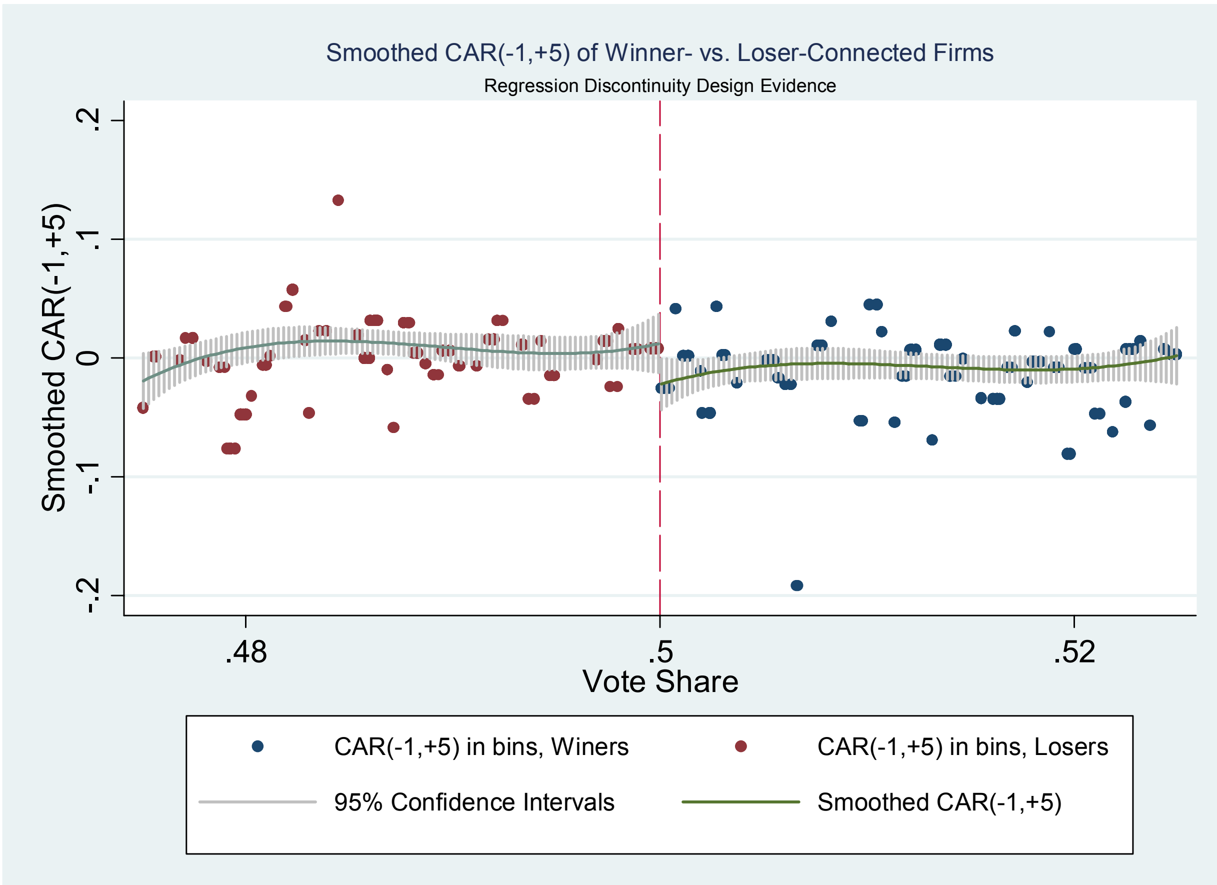

FIGURE 1.B: DISCONTINUITY EFFECT WITHOUT BINS OF OBSERVATIONS

This figure shows the RDD with Cumulative Abnormal Returns (CAR) of connected firms around close elections to US Congress between 2000 and 2008. The x-axis represents vote share between the top two candidates, and the y-axis represents CARs of firms with directors having graduated from the same university program within a year of the politician. CARs are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. The lines are predicted CARs from local cubic polynomial regressions on samples of vote shares above or below 50%, with 95%-confidence intervals in shaded areas.

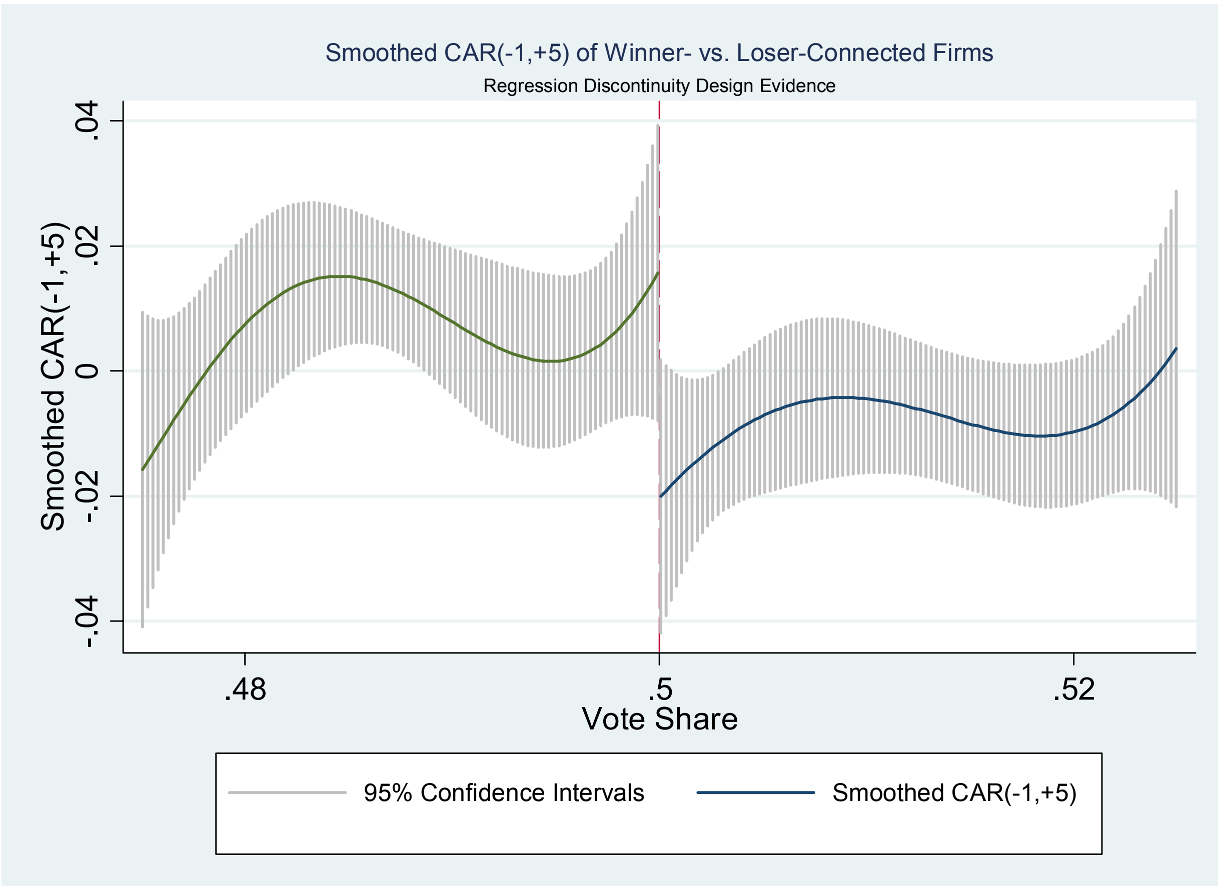

TABLE D1: SUMMARY STATISTICS

This table reports descriptive statistics of our sample. Panels A and B report vote margin distribution and summary statistics of elections and politicians per year, respectively. Federal election results are collected from the Federal Election Committee (FEC) website. For each election, we calculate the margin of votes between the top two candidates. A close election is specified by a margin of votes of less than 5%. Panel C reports the distribution of common educational backgrounds of directors and politicians in our sample. We define a political connection as a link between a firm's director and an election candidate who graduate from the same university program within a year. Following Cohen, Frazzini, and Malloy (2008), we group the education 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. Panel D reports characteristic of firms in our sample and compares them to firms in the Compustat universe.

Panel A: Close Elections at 5%-Vote Margin

	Senate		House of Representatives		Total	
Election Year	Number of Close Election	Average Margin	Number of Close Election	Average Margin	Number of Close Election	Average Margin
2000	8	2.76%	18	2.28%	26	2.43%
2002	4	2.03%	19	2.94%	23	2.79%
2004	5	3.01%	10	2.92%	15	2.95%
2006	3	1.83%	33	2.27%	36	2.23%
2008	3	1.63%	25	2.74%	28	2.62%
Sample	23	2.42%	105	2.57%	128	2.54%

Panel B: Time Series (Biannual Observations, 2000-2008)

	Mean	Median	Minimum	Maximum	Q1	Q3	Std. Dev.
Elections per year	26	26	15	36	23	28	8
% of elections	5.45	5.51	3.21	7.68	4.93	5.94	1.62
% of reps	4.82	4.39	2.31	7.57	4.11	5.71	1.96
% of senators	13.64	11.76	9.09	23.53	9.09	14.71	6
Politicians per year	89	84	61	112	82	108	21
% of elections	6.24	6.14	4.47	7.78	5.95	6.85	1.22
% of reps	4.87	4.99	2.18	7.21	4.39	5.60	1.84
% of senators	17.11	14.81	11.19	27.12	11.98	20.47	6.67
Firms per year	362	372	200	588	260	392	149
% of stocks	4.97	4.63	2.89	8.39	3.57	5.40	2.14
% of total market value	13.09	11.79	8.12	20.99	10.97	13.60	4.84
Academic institutions per year	49	50	32	71	40	54	15

Panel C: Distribution of Degree and Graduation Years

Degree	Politicians	Directors	Graduation Year	Politicians	Directors
Business School	7.6%	4.6%	<1950	3.6%	17.6%
Medical School	0.4%	0.1%	1950-59	4.8%	4.1%
Graduate	6.8%	4.2%	1960-69	21.2%	32.6%
PhD	0.8%	0.7%	1970-79	42.8%	32.6%
Law School	9.6%	3.6%	1980-89	20.0%	11.4%
Undergraduate	74.8%	86.8%		7.6%	1.7%

Panel D: Firm Characteristics

	Sample					Compustat Universe				
	Min	Mean	Median	Max	Std. Dev.	Min	Mean	Median	Max	Std. Dev.
Market Cap (in $million)	2.3	2131.5	395.4	58638.2	5389.2	0.0	2288.7	232.8	467092.9	11085.5
Common Equity (in $million)	1.0	855.5	164.0	52817.0	2898.9	0.0	985.8	118.0	224234.3	5449.4
Market to Book Ratio	0.1	4.5	2.3	246.1	11.7	0.0	4.3	1.9	7071.4	59.0
Capital Expenditure (in $million)	0.0	88.1	9.6	3023.0	275.7	0.0	144.6	6.7	31574.4	896.2
Age	0.1	8.6	8.4	40.6	5.7	0.0	8.1	7.2	59.7	6.2
Leverage	0.0	0.3	0.2	1.0	0.3	0.0	0.3	0.2	1.0	0.3
Tobin's Q	0.3	2.4	1.6	29.5	2.4	0.1	2.1	1.4	111.5	2.7
Payout	0.0	80.8	1.2	2601.0	238.4	0	81.1	0.2	16019.0	505.5
Tangibility	0.0	0.2	0.1	0.9	0.2	0.0	0.2	0.1	1.0	0.2
Return on Assets	-2.7	0.0	0.0	0.6	0.3	-43.7	-0.1	0.0	3.3	0.6
Research & Development	0.0	0.1	0.1	2.5	0.2	0.0	0.1	0.1	7.5	0.2
Cash Reserve Ratio	0.0	0.3	0.2	1.0	0.3	0.0	0.2	0.1	1.0	0.3

Notes:
(1) Corresponding Compustat universe includes all firms within Compustat in 2000, 2002, 2004, 2006 and 2008.
(2) Negative Book value of equity, Capex, Share outstanding, Price at fiscal year end, and Firm Age are removed.

TABLE 1: VALUE OF POLITICAL CONNECTION IN THE SUBSAMPLE OF INCUMBENT CONGRESSMEN

This table reports RDD regressions of Cumulative Abnormal Returns (CAR) of connected firms around close elections to US Congress between 2000 and 2008 for the subsample of incumbents. Each observation pairs a listed firm's director to an incumbent Congressman in a close election in which the vote margin between top two candidates is within 5%, if they graduate from the same university program within a year. CARs are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. Each RDD regression controls separately for vote shares of winner and loser. Column (1) groups all incumbent candidates. Columns (2) and (7) group all incumbent senators and incumbent representatives, respectively. Columns (3) and (4) use the subsamples of senators with more or less than 4.2 years of average seniority. Column (5) considers senate committees related to agriculture and natural resources (see appendix for detailed classification). Column (6) considers senate committees overseeing the economy, budget and public finance issues. Column (8) and (9) show results for representatives with more or less than 3 years of average seniority. Standard errors in brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

	Dependent Variables: CAR (-1,5)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			In Senate					In House of Representative	
Subsample	All Incumbents	Senate Committees	Seniority ≥ Median	Seniority ≤ Median	Resources & Agriculture	Economy, Budget & Finance	House Committees	Seniority ≥ Median	Seniority ≤ Median
Win/Lose	-0.0129	0.0859	0.1040	0.0640	0.0766	0.0856	-0.0115	-0.0290	0.00325
	[0.0145]	[0.0170]***	[0.0278]***	[0.00624]***	[0.00601]***	[0.0165]***	[0.0184]	[0.0369]	[0.0210]
R-squared	0.006	0.016	0.019	0.058	0.031	0.017	0.003	0.012	0.000
Observations	586	127	81	54	73	119	459	230	229

TABLE 2: VALUE OF POLITICAL CONNECTION IN THE SUBSAMPLE OF CHALLENGERS

This table reports RDD regressions of Cumulative Abnormal Returns (CAR) of connected firms around close elections to US Congress between 2000 and 2008 for the subsample of challengers. Each observation pairs a listed firm's director to a challenging candidate in a close Congress election in which the vote margin between top two candidates is within 5%, if they graduate from the same university program within a year. CARs are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. Each RDD regression controls separately for vote shares of winner and loser. Column (1) considers all challengers. Columns (2) and (3) partition the challengers into those with recent federal positions and the rest. Column (4) groups all challengers with recent state level positions, and column (5) limits them to those with past positions in state's legislative bodies or as governors. Column (6) considers challengers from all other backgrounds. Standard errors in brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

	Dependent Variables: CAR (-1,5)					
	(1)	(2)	(3)	(4)	(5)	(6)
Subsample	All Challengers	Among Challengers				
		From Federal Offices	Non-Federal	From State Politics	Top State Experience	From Others
Win/Loss	-0.0324	-0.00832	-0.0350	-0.0436	-0.0389	-0.0352
	[0.0107]***	[0.0287]	[0.0104]***	[0.0142]***	[0.0180]**	[0.0255]
R-squared	0.009	0.007	0.011	0.013	0.016	0.013
Observations	1,221	199	1,022	539	341	483

TABLE 3: VALUE OF POLITICAL CONNECTION IN THE POOLED SAMPLE

This table reports RDD regressions of Cumulative Abnormal Returns (CAR) of connected firms around close elections to US Congress between 2000 and 2008. Each observation pairs a listed firm's director to a candidate (incumbent or challenger) in a close Congress election in which the vote margin between top two candidates is within 5%, if they graduate from the same university program within a year. CARs are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. Each RDD regression controls separately for vote shares of winner and loser. Column (1) reports the benchmark estimation. Column (2) controls for 4th-order polynomials of vote shares, separately for winners and losers. Column (3) controls for dummies representing party, gender, incumbency, senate/house race, log of total campaign contribution, and log of number of contributors for each connected politician. Column (4) controls for dummies representing age, gender, nationality, executive/non-executive role of the connected director. Column (5) controls for firm's market value, book value of equity, total assets, return on asset, capital expenditure and leverage. Columns (6), (7) and (8) control respectively for fixed effects of years, SIC 2-digit industries, and educational institutions. Column (9) excludes observations with CAR greater than 50%. Standard errors in brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

	Dependent Variables: CAR (-1,5)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample	5% margin	5% margin	5% margin	5% margin	5% margin	5% margin	5% margin	5% margin	No Outliers
Win/Lose	-0.0265	-0.0407	-0.0307	-0.0288	-0.0292	-0.0257	-0.0270	-0.0275	-0.0230
	[0.00853]***	[0.0137]***	[0.0112]***	[0.00928]***	[0.00973]***	[0.00835]***	[0.00926]***	[0.0110]**	[0.00774]***
Controls		4^{th} Order Poly	Politician Controls	Director Controls	Firm Controls	Year FE	Industry FE	University FE	
R-squared	0.006	0.010	0.021	0.038	0.019	0.013	0.040	0.096	0.005
Observations	1,819	1,819	1,795	1,722	1,623	1,819	1,804	1,819	1,788

TABLE 4: VALUE OF POLITICAL CONNECTIONS BY GROUPS

This table reports RDD regressions of Cumulative Abnormal Returns (CAR) of connected firms around close elections to US Congress between 2000 and 2008. Each observation pairs a listed firm's director to a candidate in a close Congress election in which the vote margin between top two candidates is within 5%, if they graduate from the same university program within a year. CARs are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. Each RDD regression controls separately for vote shares of winner and loser. Pairs of columns from (1) to (6) respectively show results on the subsamples of Senate or House races, candidates belonging to the majority or minority in the corresponding chamber, and democrats or republicans. Columns (7) to (9) examine subsamples of connections through Harvard University and institutions related to less or more than 50 individuals (sample's median) in the sample. Standard errors in brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

	Dependent Variables: CAR (-1,5)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Subsample	Senators	House	Chamber Majority	Chamber Minority	Democrats	Republicans	Harvard	Small Networks	Large Networks
Win/Loss	-0.0424	-0.0214	-0.0262	-0.0286	-0.0243	-0.0286	-0.0545	-0.0245	-0.0255
	[0.0117]***	[0.0112]*	[0.0129]**	[0.0115]**	[0.0117]**	[0.0137]**	[0.0117]***	[0.0113]**	[0.00985]**
R-squared	0.015	0.004	0.008	0.008	0.004	0.008	0.012	0.005	0.009
Observations	559	1,260	893	926	1,057	762	215	1,092	727

TABLE 5: VALUE OF POLITICAL CONNECTIONS THROUGH THE SOCIAL NETWORKS OF ALUMNI

This table reports RDD regressions of Cumulative Abnormal Returns (CAR) of connected firms around close elections to US Congress between 2000 and 2008. Each observation pairs a listed firm's director to a candidate in a close Congress election in which the vote margin between top two candidates is within 5%, if they graduate from the same university program, regardless in which year. CARs are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. Each RDD regression controls separately for vote shares of winner and loser. Column (1) reports the benchmark RDD regression. Column (2) controls for a quartic polynomial in vote share, separately for losers and winners. Column (3) controls for dummy variables representing party, gender, incumbency and senate/house race information of the politician involved. Column (4) controls for firm's market value. Columns (5), (6) and (7) control respectively for fixed effects of years, SIC 2-digit industries, and educational institutions. Column (8) excludes observations with CAR of 50% or higher. Standard errors in brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

	Dependent Variables: CAR (-1,5)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	5% margin	5% margin	5% margin	5% margin	5% margin	5% margin	5% margin	No Outliers
Win/Lose	-0.0058	-0.0121	-0.0054	-0.0058	-0.0036	-0.0057	-0.0058	-0.0052
	[0.0028]**	[0.0060]**	[0.0024]**	[0.0024]**	[0.0027]	[0.0028]**	[0.0034]*	[0.0023]**
Controls		4^{th} Order Poly	Politician Variables	Market Value	Year FE	Industry FE	School FE	
R-squared	0.001	0.001	0.001	0.001	0.012	0.017	0.015	0.001
Observations	29,527	29,527	29,063	29,527	29,527	29,527	29,527	29,330

TABLE 6: ALTERNATIVE EVENT WINDOWS AROUND ELECTION DAY

This table reports RDD regressions of Cumulative Abnormal Returns (CAR) of connected firms around close elections to US Congress between 2000 and 2008. Each observation pairs a listed firm's director to a candidate in a close Congress election in which vote margin between top two candidates is within 5%, if they graduate from the same university program within a year. CARs are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. Standardized CARs are CARs normalized by volatility during the event period. Each RDD regression controls separately for vote shares of winner and loser. Standard errors in brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Panel A: Cumulative Abnormal Returns

	Dependent Variables: CAR					
	(1)	(2)	(3)	(4)	(5)	(6)
Window	(-7,-1)	(-1,1)	(-1,5)	(0,5)	(1,5)	(6,20)
Win/Lose	0.00174	-0.0155	-0.0265	-0.0182	-0.0185	0.0139
	[0.0165]	[0.00649]**	[0.00853]***	[0.00947]*	[0.00802]**	[0.0220]
R-squared	0.003	0.005	0.006	0.005	0.005	0.002
Observations	1,804	1,819	1,819	1,819	1,819	1,819

Panel B: Standardized Cumulative Abnormal Returns

	Dependent Variables: SCAR					
	(1)	(2)	(3)	(4)	(5)	(6)
Window	(-7,-1)	(-1,1)	(-1,5)	(0,5)	(1,5)	(6,20)
Win/Lose	0.032	-0.25	-0.322	-0.261	-0.290	0.0616
	[0.208]	[0.127]**	[0.125]**	[0.143]*	[0.129]**	[0.142]
R-squared	0.005	0.004	0.005	0.004	0.004	0.000
Observations	1,464	1,819	1,819	1,819	1,819	1,819

TABLE 7: RDD WITH NONPARAMETRIC REGRESSIONS AND TESTS

This table reports RDD regressions of Cumulative Abnormal Returns (CAR) of connected firms around close elections to US Congress between 2000 and 2008. Each observation pairs a listed firm's director to a candidate in a close Congress election in which vote margin between top two candidates is within 5%, if they graduate from the same university program within a year. CARs are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. Each column runs a local cubic polynomial regression of the dependent variable on vote shares in the subsamples above and below the cutoff, then reports the difference between the predicted values of the dependent variable for each subsample around the cutoff. Column (1) shows the benchmark regression. Columns (2) to (5) show the results for different values of the bandwidth of local polynomial regressions. Columns (6) to (9) show results with hypothetical cutoffs. Standard errors are in brackets; *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

	Dependent Variables: CAR (-1,5)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Benchmark	Robustness to Bandwidths				Placebo Thresholds			
Bandwidth	0.05	0.04	0.03	0.02	0.01				
Cutoff	50%	50%	50%	50%	50%	48%	49%	51%	52%
Win/Lose	-0.034	-0.034	-0.0342	-0.0345	-0.0387	0.0805	0.0128	0.0465	0.0234
	[0.0168]**	[0.0168]**	[0.0167]**	[0.0168]**	[0.0180]**	[0.0235]***	[0.0207]	[0.0283]	[0.0218]

TABLE 8: STATE CHARACTERISTICS AND THE VALUE OF POLITICAL CONNECTIONS

This table reports RDD regressions of Cumulative Abnormal Returns (CAR) of connected firms around close elections to US Congress between 2000 and 2008. Each observation pairs a listed firm's director to a candidate in a close Congress election in which vote margin between top two candidates is within 5%, if they graduate from the same university program within a year. CARs are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. Columns (1) to (8) respectively show results on the subsamples of above or below median of the following measures: regulation score, corruption conviction rate in 2000 (Glaeser Saks 2006), Exalead.com 2009 search hits for "corruption" close to name of main city, normalized by hits for name of main city, and Newslibrary.com 2009 all newspapers search hits for "corruption" close to name of state, normalized by hits for name of state, and GCISC 1970 score (population concentration around the State capital, Campante Do 2012). Standard errors in brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

	Dependent Variables: CAR (-1,5)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Subsample	High Regulation	Low Regulation	More Corrupt Conviction	Less Corrupt Conviction	More Corrupt Main City	Less Corrupt Main City	More Corrupt State	Less Corrupt State	High GCISC 1970	Low GCISC 1970
Win/Lose	-0.0327	-0.0127	-0.0430	-0.0135	-0.0531	-0.00740	-0.0309	-0.0213	-0.0205	-0.0360
	[0.0123]***	[0.0113]	[0.0132]***	[0.0113]	[0.0148]***	[0.0115]	[0.0125]**	[0.0122]*	[0.0110]*	[0.0136]***
R-squared	0.012	0.003	0.011	0.004	0.014	0.004	0.009	0.004	0.005	0.008
Observations	1,166	653	852	967	872	947	1,081	738	905	914

TABLE 9: FIRM CHARACTERISTICS AND THE VALUE OF POLITICAL CONNECTIONS

This table reports RDD regressions of Cumulative Abnormal Returns (CAR) of connected firms around close elections to US Congress between 2000 and 2008. Each observation pairs a listed firm's director to a candidate in a close Congress election in which the vote margin between top two candidates is within 5%, if they graduate from the same university program within a year. CARs are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. Columns (1) to (4) respectively show results on the subsamples of below or above median market capitalization, with or without reliance on external finance (Rajan and Zingales 1998). Column (5) uses the subsample of firms below median market capitalization and with reliance on external finance. Standard errors in brackets are corrected for clustering by politicians in each election. Column (6) refers to the subsample with the distance between firm's headquarter and politician's State within the lowest quartile, and above median corruption score by Newslibrary search hits in politician's State (see Table 6). Column (7) refers to the subsample with above median dependence on external finance and above median corruption score. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

	Dependent Variables: CAR (-1,5)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Subsample	Lower Market Cap	Higher Market Cap	Rely on External Finance	Not Rely on External Finance	Lower Market Cap, Rely on External Finance	Short HQ Distance, More Corrupt	Rely on External Finance, More Corrupt
Win/Lose	-0.0656	0.000202	-0.0299	-0.0217	-0.0564	-0.0718	-0.0377
	[0.0197]***	[0.00911]	[0.0128]**	[0.0148]	[0.0198]**	[0.0223]***	[0.0185]**
R-squared	0.023	0.004	0.010	0.004	0.025	0.034	0.015
Observations	763	1,056	948	871	511	359	550

TABLE 10: POLITICAL CONNECTIONS AND FIRMS ACTIVITIES IN CORRESPONDING STATES

This table reports RDD regressions of changes in firm activities of connected firms around close elections to US Congress between 2000 and 2008. Each observation pairs a listed firm's director to a candidate in a close Congress election in which the vote margin between top two candidates is within 5%, if they graduate from the same university program within a year. Firm activities in a given state in a given year are measured as the ratio of the number of search hits for the firm's name in local newspapers and the number of search hits for the neutral keyword "September". The dependant variable is the change of firm activities over different event windows (election year is year 0.) Columns (1) to (3) consider samples of challengers with recent state level positions, respectively with windows of one year after, one year before, and two years after the election year. Column (4) further restricts the sample to those with experience in state's legislative bodies or as governors, within one year after the election. Columns (5) to (6) consider challengers coming from federal offices and from other backgrounds. Standard errors in brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Subsample	**Challengers with State Experience**			**Challengers with Top State**	**From Federal Offices**	**From Others**
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Year-to-Year Change in Activities	(0,+1)	(-1,0)	(+1,+2)	(0,+1)	(0,+1)	(0,+1)
Win/Lose	-0.00957	0.000225	-0.00284	-0.0154	0.00145	-0.0405
	[0.00435]**	[0.00559]	[0.00434]	[0.00253]***	[0.000981]	[0.0476]
R-squared	0.01	0.005	0.002	0.009	0.128	0.002
Observations	593	591	593	402	144	479

TABLE 11: DIRECTORS' REMAINING TENURE AFTER POLITICIANS' ELECTION

This table reports RDD regressions of directors' remaining tenure after connected politicians' close elections to US Congress between 2000 and 2008. Each observation pairs a listed firm's director to a candidate in a close Congress election in which the vote margin between top two candidates is within 5%, if they graduate from the same university program within a year. The dependant variable in columns (1) to (7) is the number of years the director remains with a firm after the connected politician's election. These columns control for the director's elapsed tenure. Columns (1) to (2) consider samples of incumbents and challengers, respectively. Columns (3) to (6) use samples of challengers with recent state level positions, those with experience in state's legislative bodies or as governors, those coming from federal offices and those from other backgrounds, respectively. Column (7) restricts the challengers sample exclusively to directors aged between 52 and 56 (middle quintile). Column (8) uses director's pre-election elapsed tenure as dependant variable. Standard errors in brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Dependent Variable	**Director's Remaining Years**							**Director's Elapsed Years**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Subsample	Incumbents	Challengers	Challengers from State	Challengers with Top State Experience	Challengers from Fed	Challengers from Others	Challengers 52 to 56 year-old Directors	Challengers from State
Win/Lose	-0.439	-2.075	-2.643	-3.681	-1.920	-0.275	-3.961	-1.126
	[0.992]	[0.920]**	[1.347]*	[1.502]**	[1.200]	[0.926]	[0.986]***	[0.854]
R-squared	0.013	0.074	0.073	0.095	0.166	0.127	0.175	0.004
Observations	598	1221	594	402	144	483	324	594

TABLE A1: ADDITIONAL ROBUSTNESS CHECKS

In columns (1) to (6) each observation pairs a listed firm's director to a candidate in a close Congress election (vote margin between top two candidates within 5%), if they graduate from the same university program within a year. The outcome variable is raw returns from the window (-1,5) in columns (1) and (2), CARs calculated from Fama-French model in columns (3) and (4), CARs calculated from Fama-French-Carhart model with momentum in columns (5) and (6). Those models are estimated around the election day (day 0) using daily data from day -315 to day -61. Win/Lose is a dummy variable equal to one if and only if a politician finishes first or second in an election. Columns (7) to (9) collapse the data so that each unit of observation is respectively a director, a company, or a politician. In column (10) the benchmark regression in Table 1 is estimated with two-way clustering of both Politician-Year and Company-Year (Cameron, Gelbach & Miller, 2011). Standard errors in brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Dependent Variable	**CAR (-1,5) Raw Returns**		**CAR (-1,5) from FF**		**CAR (-1,5) from FFM**		**CAR (-1,5)**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample	5% margin	5% margin	5% margin	5% margin	5% margin	5% margin	Director Level	Company Level	Politician Level	2-way clustering: Pol. & Com.
Win/Lose	-0.0204	-0.0445	-0.0228	-0.0248	-0.0261	-0.0270	-0.0306	-0.0287	-0.0271	-0.0261
	[0.0190]	[0.0211]**	[0.00774]***	[0.0101]**	[0.00725]***	[0.00949]***	[0.00917]***	[0.00819]***	[0.0196]	[0.00759]***
School FE	No	Yes	No	Yes	No	Yes				
R-squared	0.012	0.147	0.005	0.083	0.005	0.083	0.006	0.005	0.013	0.005
Observations	1,819	1,819	1,819	1,819	1,818	1,818	1,308	1,593	192	1,818

TABLE A2: RDD RANDOMNESS CHECKS

Each observation pairs a listed firm's director to a candidate in a close Congress election (vote margin between top two candidates within 5%), if they graduate from the same university program within a year. Cumulative Abnormal Returns (CAR) are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. Each column serves to show that a dependent variable's distribution is continuous at the cutoff point of 50% vote share. Panel A shows results for politician's gender, age, chamber, logarithm of campaign contribution, logarithm of number of contributors, and incumbency. Panel B considers challenger's party and different backgrounds, director's age, gender and executive/non-executive role, and social network size. Panel C displays results with different firm characteristics. Panel D reports regressions with industry's financial dependence, state's institution quality and corruption measured in different ways (see text for details). Standard errors in brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Panel A: Politician Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Pol. Gender	Pol. Age	Senate/House	Log(Campaign Contribution)	Log(Number of Contributors)	Incumbency
Win/Lose	0.0999	2.673	0.103	-0.0644	-0.370	-0.177
	[0.116]	[2.086]	[0.224]	[0.655]	[0.395]	[0.202]
R-squared	0.023	0.030	0.019	0.004	0.014	0.029
Observations	1,817	1,797	1,819	1,819	1,819	1,819

Panel B: Challenger and Director Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Challengers' Party	Challengers from State Politics	Challengers with Top State Experience	Challengers from Federal Politics	Director's Gender	Director's Age	Executive Directorship	Large Social Network
Win/Lose	-0.192	-0.382	-4.943	-0.291	-0.0300	2.685	0.0779	-0.143
	[0.268]	[0.276]	[4.730]	[0.239]	[0.0412]	[2.049]	[0.0475]	[0.194]
R-squared	0.066	0.055	0.129	0.058	0.004	0.033	0.003	0.023
Observations	1,221	1,221	1,221	1,221	1,819	1,722	1,819	1,819

Panel C: Firm Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Market Cap	Common Equities	Assets	Return on Asset	Capital Expenditure	Leverage
Win/Lose	3,291	604.8	-1,522	-0.0318	-0.0566	-0.0272
	[3,255]	[1,055]	[10,321]	[0.0225]	[0.0361]	[0.0426]
R-squared	0.003	0.002	0.001	0.002	0.002	0.000
Observations	1,786	1,751	1,752	1,690	1,688	1,745

Panel D: Industry and State Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	External Finance Dependence	GCISC 1970	Regulation	Corruption Main City	Corruption Conviction Rate	Corruption State
Win/Lose	-0.115	0.0192	-0.0621	12,339	-0.0873	4.974
	[0.709]	[0.0388]	[0.182]	[7,746]	[0.0758]	[62.35]
R-squared	0.003	0.060	0.070	0.039	0.015	0.025
Observations	1,715	1,780	1,819	1,819	1,819	1,550

TABLE A3: HETEROGENEOUS EFFECTS FROM QUANTILE REGRESSIONS FOR CHALLENGERS FROM STATE POLITICS

Each observation pairs a listed firm's director to a candidate in a close Congress election (vote margin between top two candidates within 5%), if they graduate from the same university program within a year. Cumulative Abnormal Returns (CAR) are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. Column (1) reports the estimation using median regression (50% quantile), while columns (2) to (5) report the estimation using quantile regressions at the 1st, 2nd, 3rd and 4th quantiles. Bootstrapped standard errors are reported in brackets. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

	Dependent variables: CAR(-1,5)				
	(1)	(2)	(3)	(4)	(5)
Subsample	Median 5% margin	1st quintile	2nd quintile	3rd quintile	4th quintile
Win/Lose	-0.0353	-0.0759	-0.0343	-0.0142	-0.0125
	[0.0105]***	[0.0276]***	[0.0127]***	[0.00889]	[0.0193]
Observations	594	594	594	594	594

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