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ABSTRACT

This paper investigates the impact of social-network connections to politicians on firm value. We focus on the networks of university classmates and alumni among directors of U.S. public firms and congressmen. Using the Regression Discontinuity Design based on close elections from 2000 to 2008, we identify that a director's connection to an elected congressman causes a Weighted Average Treatment Effect on Cumulative Abnormal Returns of -2.65% surrounding the election date. The effect is robust and consistent through various specifications, parametric and nonparametric, with different outcome measures and social network definitions, and across many subsamples. We find evidence to support the hypothesis that firms benefit more when connected politicians remain in state politics than when they move to federal office. Overall, our study identifies the value of political connections through social networks and uncovers its variation across different states and between state and federal political environments.

Keywords: Social network; political connection; close election; regression discontinuity design; firm value.

JEL Classifications: D72, D73, D85, G3, G10, G11, G14, G30, C21

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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, potentially distorting incentives in politics and markets in many parts of the world (see Fisman 2001, Johnson and Mitton 2003, Khwaja and Mian 2005, Faccio 2006, Faccio et al. 2006, Bertrand et al. 2008, Bunkanwanicha and Wiwattanakantang 2009, among others). In the United States, the evidence of the value of political connections is mixed, with positive estimates (Jayachandran 2006, Knight 2007, Goldman et al. 2009), as well as estimates equal to zero (Fisman et al. 2006).

In spite of numerous significant results, strong and convincing empirical evidence on the impact of political connections remains scant. One potential explanation is the endogenous nature of political connections, which prevents the precise identification and quantification of their impact. In addition, the existing literature's focus on direct links between firms and politicians (based on a politician's ownership, directorship, or management positions in firms) raises questions about the generalizability of the results, particularly in the U.S., where these direct links are relatively rare. Few papers study the broader political connections from a social network perspective, or address the questions of if and why these connections are important.

In this paper we study the impact of political connections in the U.S., where, purportedly, institutions rank among the best, and the line between politics and business is the clearest. We broaden the definition of political connections by following a social network approach to define links between politicians and corporate directors in non-contractual social relations based on their educational backgrounds. We propose an empirical strategy using the Regression Discontinuity Design of close elections to overcome three major challenges in any investigation of the social network of politicians and directors. Our empirical strategy addresses several major challenges.

The first challenge is the measurement of connections in a social network.¹ While 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

¹ See the surveys on social networks by Marsden (1990), Rauch (2001), Ioannides and Loury (2004), Jackson (2009), and Allen and Babus (2009) for more network definitions and measurements.

prohibitively costly to apply in our context. Instead, we focus on the social network of former classmates and alumni, as proposed by Bertrand et al. (2008), Cohen et al. (2008), and Nguyen (2011). This network can be clearly and unambiguously defined based on publicly available information on educational backgrounds of all politicians and directors, and is expected to strongly predict true friendship in real life. Moving beyond specific connections of certain politicians, this network's coverage is broad enough to be representative of the population of politicians and directors, making it possible to generalize the empirical results.

The second major challenge is the identification problem related to 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 any effect we want to attribute to social network connections. In specific contexts, event studies using arguably exogenous news and event probabilities from prediction markets may provide partial solutions to this issue (see, for instance, Snowberg et al. 2007, or Fisman 2001). However, as we will argue in the next section, the answer to the political connection identification problem remains elusive.

In this paper, we propose a novel approach: we identify the effect of social connections of politicians and directors by using close elections of the politicians. Lee (2008) showed that close elections can be considered a Regression Discontinuity Design (RDD), a natural experiment that produces near-randomized-trial identification with extremely good internal validity. That is, a connection to a politician elected to office by a small margin is nearly identical to a connection to one defeated by a small margin, and can be considered as a randomized experiment around the threshold. Moreover, Lee and Lemieux (2010) also show that the estimated effect is a Weighted Average Treatment Effect (WATE), thus being generalizable to the sample of all politicians with a nonzero chance of experiencing a close election. While this empirical design has been widely used in labor, political, and development economics (see Lee and Lemieux 2010), its application in corporate finance has unfortunately been limited to a handful of papers (exceptions include Chava and Roberts 2008, , Cuñat et al. 2010, Kerr et al. 2010).

The remaining challenge is the identification of social networks, or the confoundedness of homophily. Coined by sociologists,² “homophily” refers to the phenomenon that people sharing the same characteristics are more likely to join the same network, thus confounding the

² See McPherson, Smith-Lovin, and Cook (2001).

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 introduce school fixed effects, thus identifying the effect of political connections by variations over time (school fixed effects are unidentifiable 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 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. On the director side, we 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,³ and link each pair to the stock performance of the firm 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 or loss status of the candidate who shares education background with a director of the firm.

This regression equation provides an estimate of the stock-market value of a new connection to a politician in Congress. In this context, the treatment is one that suddenly puts a firm's connected politician into Congress, as opposed to leaving him where he is. As shown in Lee and Lemieux (2010), the Regression Discontinuity Design in close elections produces a consistent, unconfounded estimate of the effect of the treatment. 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, instead of running regressions trying to control for all relevant covariates, we can focus our empirical work 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

³ 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.

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 well-identifies a treatment effect that can shed light on social connections between Congressmen and corporate directors.

The RDD estimates a treatment effect of negative 2.65% during the event window from one day before to five days after the election. The effect is 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. This result means that having a connected politician in Congress significantly decreases the firm's value by 2.65% on average.

To interpret the results, our hypothesis is 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. As a result, when he moves to federal office, the firm is expected to get less benefit. We empirically test and confirm three implied predictions: (i) the value loss effect should be stronger for politicians coming from state, rather than from federal, politics; (ii) the effect is stronger for states with lower institutional quality; and (iii) firm activities should decrease in elected politicians' states. 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.

Our paper makes two main contributions to the literature on political connections. The first contribution is the solution to the identification problem. The most successful approach in the existing literature is perhaps the study of political events that happen arguably independently of political connections. Knight (2007), Goldman et al. (2008, 2009), and Mattozzi (2008) exploit close elections in presidential races in the U.S.; Roberts (1990), Jayachandran (2006) and Fisman et al. (2006) 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. Many other important papers such as 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 thus prone to biases induced by time-varying characteristics of firms or politicians/political parties.

While all of those papers have carried out extensive robustness checks to verify the causality channel, few treat the endogeneity of connections. 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. 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. The discovered results are therefore also externally valid. That is, it is possible to generalize the conclusions to the population of all firms and politicians.

The second important contribution is our finding of a negative estimated value of connection to politicians holding office. While this result 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 this finding is consistent with the incidence of the firm's lost benefits when the connected politician moves away from state politics. This result is consistent with Fisman et al. (2006), who find that, on average, firms do not enjoy financial benefits from their connections to Vice President Dick Cheney while he is in office. The originality of our result points to the remarkable difference in the institutional

environments between the federal and state levels in the U.S., implying very different values of political connections.

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

2. 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 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.

The endogeneity bias is best eliminated with a randomization of the assignment of a politician to office: if the politician is chosen randomly, there is no concern of either the reverse causation of firm value changes or the influence of some omitted variables. In practice, it is hard to find a randomized experiment on political connection.

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 randomized between the top two runners as though in a randomized experiment. 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. In other words, conditional on the election being close, the incidence of winning or losing is independent of all observable and unobservable characteristics of the politician before the election. The RDD thus allows an estimation of the average treatment effect of connections to elected politicians versus defeated politicians without any reverse causation or omitted variable bias, ensuring the internal validity of the results.

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 firms included in our sample, as we will show in Section 3 that our sample of firms is very similar to the Compustat universe.

2.2. EMPIRICAL SPECIFICATIONS

We follow Lee and Lemieux (2010) in designing two main econometric specifications to estimate the effect of political connection. In our context, 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 corresponding firm’s stock price cumulated abnormal return in a window around the election day that year. The treatment variable is the 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 the 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 our robustness checks, we also include a cubic polynomial of the vote shares, as well as other levels of clustering.

The second specification uses nonparametric regressions of the outcome variable on the treatment variable on two separate subsamples, of elected politicians and of runners-up. Predictions of the outcome variable are calculated at the threshold of 50% 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 $\hat{F}_+(50\%) - \hat{F}_-(50\%)$.⁴

2.3. OTHER ISSUES

By defining connections by all pairs of classmates, we may raise doubts about the realistic nature of those connections, as most people have only a small number of friends even among classmates (see, for instance, Leider et al. 2009). Yet this should not be a concern to the significance of our results. The measurement errors in this case imply that the effect of real friendships is nuanced by many non-friends classmate connections, thus 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 those found in this paper. On the other hand, classmate connections can be primordial in the development of relationships after college or graduate school by providing common ground in communication and mutual trust as well as common access to the same social network. In that sense, former classmates are much more likely to later develop a strong connection, even if they not close friends while in college or graduate school. In fact, several recent papers have shown the strength of this measurement of connections in many contexts (Cohen et al. 2008, Nguyen 2011, Fracassi 2009).

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 (e.g., Cohen et al. 2008), still tolerates the problem of homophily (McPherson et al. 2001). Accordingly, future politicians and directors sharing similar characteristics and preferences may have been drawn together at the same university; decades later the elected politician may enact policies in favor of these same characteristics, on which the director's firm can profit, without passing through the social network channel. In essence, unobservable factors could determine both these connections, politician's preferences, firm's activities, and market reaction to elections (i.e., value is only

⁴The 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.

affected when the similar politician is elected). For example, if a politician and a director went to a university that specializes in military studies, then the election of the former has the potential to affect the latter's firm value through new defense policies, rather than through the social network. In sum, identification problems of the effect in question emerge when certain unobservables influence both the outcomes at the firm level and the explanatory variable of political connection. While the RDD does identify the effect of "political connection" as we define it, this effect may not be the fruit of social network mechanisms but may instead result from common characteristics.

Our setup allows for a simple solution: the 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.

In summary, our research design correctly identifies and 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, and all firms in Compustat.

3. DATA DESCRIPTION

We assemble our sample using data from a few sources. First, 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 contains 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 in our sample. The average Win/Loss margin across all election 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 annual number of elections is 26 (with a maximum of 36, and

minimum of 15). Our sample elections involve on average 89 politicians per year, with a maximum of 112 and minimum of 61. The average number of connected firms per year is 362.

[Insert Table D1 Here]

We hand-collect the biographical record of these elections using Marquis *Who's Who* 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. Most of the biographies for our sample are available in *Who's Who*. To complete our biographies, 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.

Next, we obtain biographical information and past education history for directors and senior company officers from BoardEx of Management Diagnostics Limited. The data details the relational links among board directors and senior company officers for both active and inactive firms by cross-referencing these directors' and officers' employment history, educational background, and professional qualifications. In particular, the data contains current and past roles of each official in a company with start and end date (year), all undergraduate and graduate degrees attained, the year in which those degrees were awarded, and the awarding institution. We restrict our sample to board directors in U.S. publicly listed firms.

We construct our social network measure through educational institutions. 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. We thereby match institutions and degrees on *Who's Who* biographies and BoardEx. 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 year of graduation. 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 important to the connection of 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 insignificant in connecting politicians to 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 characteristic of firms in our sample and compares them to firms in the Compustat universe. The sample's firm average market capitalization is \$2.05 billion, with a maximum of \$58.64 billion and a median of \$0.38 billion, which are fairly comparable to Compustat average firms (\$2.35 billion, \$467.09 billion, and \$0.24 billion, respectively). Our average firm has a market-to-book ratio of 4.61 and age of 8.62 years, as compared to a market-to-book ratio of 4.28 and age of 8.08 years for an average Compustat firm.

4. EMPIRICAL RESULTS

In this section, we report main empirical results of our regression discontinuity design, with additional results on alternative outcome variables and alternative windows. We also present results from alternative, non-parametric estimations, as well as the results on the impact of political connections across many sub-samples.

4.1. ESTIMATIONS OF THE IMPACT OF POLITICAL CONNECTIONS USING A REGRESSION DISCONTINUITY DESIGN

Table 1 presents our estimation of the impact of political connection on firm value by relating stock price Cumulated Abnormal Returns (CAR) of connected firms around the election day to the win/lose status of the connected politician. 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). For every connected firm, we obtain daily stock returns for the 6-day event period (from day -1 to day 5), as well for a 255-day pre-event period (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. We follow a conventional event study method to calculate the abnormal returns resulting from close elections by assuming a

single-factor model with the beta estimated from the pre-event window (the results are not sensitive to the method of estimation of the abnormal returns). We exploit the RDD of close elections by limiting the sample to elections in which the vote share between the top two candidates is between 48.5% and 52.5% (i.e., within a 5% vote share margin), and by controlling for the vote shares separately for winners and losers, as suggested by Lee and Lemieux (2010), to obtain the effect at the exact threshold of 50%.

[Insert Table 1 Here]

We find a strongly negative and statistically significant effect of connection to a close election winner on CARs. Column (2) shows the 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, producing an estimate of -2.65%, significant at 1%. Column (3) controls additionally for quartic polynomials of winners' and of losers' vote shares, so as to single out the effect exactly at the threshold of 50% vote share (as suggested as a robustness check by Lee and Lemieux 2010), and reports an even larger effect of -4.07%, significant at 1%. Alternatively, we limit the sample to vote margins of 1% or less in column (1), and again find a strong, 5%-significant effect of -3.12%. These specifications indicate that being connected to a congressman in office decreases a firm's value by around 3% on average.

Columns (4) to (8) further show that the results are unaffected by "irrelevant covariates." Indeed, when the treatment is comparable to a randomized experiment, any additional control variable must be independent of the treatment, thus its inclusion should not significantly alter the estimate of the treatment effect. Column (4) controls for characteristics of the politician (dummy variables for the party, gender, incumbency, Senate/House race), and column (5) for market capitalization (a proxy for firm size), producing estimates very close to the benchmark in column (2) and all significant at 1%. In a similar vein, unobservable characteristics of the election year or the industry are also 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 -2.91%, significant at 1%. It implies that network homophily is relatively irrelevant to our treatment, and shared school characteristics are not the factor behind the negative estimate of the value of connection reported in Table 1.⁵

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 -2.18%.

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. No existing results find a negative estimate of having an additional political connection.

In summary, Table 1 provides evidence that firms connected to the winner in a close election to the U.S. Congress between 2000 and 2008 experience significant loss in firm value, as compared to firms connected to the runner-up. The results are robust and consistent when we control for politicians' characteristics, firm size, election year-, industry- and school-fixed effects. Our results imply that political connections affect firm value, and there is a significant variation in the value of political connections. In the following sections, we further investigate the sources of variation and interpret our results.

4.2. ALTERNATIVE SPECIFICATIONS AND ROBUSTNESS CHECKS

In this section, we explore alternative specifications with different event windows and calculations of the CARs. Table 2 summarizes this exercise.

[Insert Table 2 Here]

In Panel A, we vary the event window of (-1,5) used in Table 1. If the election results are priced in stock values immediately, the effect reported in the previous section should be

⁵We do not include company fixed effects, as there is very little variation within companies across years, with many companies appearing only once, thus omitted from such a fixed-effect regression.

detectable only in a small window over the event day, and not before the event. On the other hand, the presence of the effect days after the event implies that the market takes time to fully react to this form of information.

Columns (1) and (2) of Panel A report the results of regressions using CARs from different pre-event windows, from day -7 to day -1 and from day -1 to day 0. The coefficient of interest is not statistically significant and always very small in size. This verifies that the treatment has not been predicted by the market prior to the event, as expected from the close elections design.

While column (3) reports the benchmark result for the window from day -1 to day 5, as in column (2) of Table 1, columns (4) and (5) use different starting days for the event window, namely beginning on the event day (day 0) and day 1, and ending on day 5. Interestingly, we find negative and significant coefficients on the Win/Lose dummy, of about 70% the size of the benchmark estimate in column (3). 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, knowing all the results of elections, shorting on firms connected to closely elected politicians and longing on those connected to closely defeated ones, with equal weights on firm connections (i.e., a firm's stock is counted twice if it is connected to two different politicians). From day 1 to day 5, this portfolio yields a risk-free return of 1.85%.

Beyond our benchmark window, such as from day 6 to day 20 after the election day, as reported in column (6), we find 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 much additional noise, 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 can be 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, and politician's state, 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 1, does not solve the issue, because of a potential censoring bias. A different

approach consists of normalizing each stock's CAR by its standard error derived from the market model within the event window. Panel B of Table 2 presents Table 1'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.⁶

In other tests of robustness reported in Appendix Table A1, we calculate the CARs using different methods, including Fama-French's three-factor model and the three-factor model with momentum (Fama and French 1993, and Carhart 1997). We also use the cumulative daily stock (raw) returns without a market model as the outcome variable. We find estimates mostly similar to those reported in Table 1, either including or excluding school fixed effects. In Appendix Table A1, we also report 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. Table A1 shows very similar results.

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

[Insert Table 3 Here]

Our RDD has so far exclusively focused on the vote share threshold of 50%. In columns (6) to (9), we further test the robustness of our result by applying the same method to “placebo”

⁶ Other robustness checks include the verification of the near-randomness of winning or losing a close election (as previously verified by Lee 2008). They are available upon request.

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. Because such a placebo threshold does not separate winners from losers in reality, 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 not statistically significant at 10%.

Figure 1 visualizes the numerical results presented in Table 3, where each half of the graph represents the fitted local polynomial of degree 3 for vote shares greater or less than 50% (for elected or defeated politicians, respectively). We see a large gap at exactly 50% of vote share, whereas the gradient of the graph is relatively small elsewhere, as already tested with placebo thresholds in Table 3. Furthermore, there is (visual) evidence of a “Z” shape of CAR with respect to vote share: as vote share increases around 50%, CAR first increases, then drops sharply at the threshold of 50%, and then increases again. As explained by Cuñat et al. (2009), this Z shape is predictable in a model where the market internalizes available information before election and anticipates the gap at 50% if the prior probabilities of winning or losing are markedly different from 50%. For instance, for an election resulting in vote shares of 52%-48%, it is likely that the market’s prior probability of the first candidate’s winning is notably larger than 50%, hence part of the gap at 50% has already been incorporated in market prices even before the election. Therefore, we do not see a large difference between the CARs at 48% and at 52% on the graph in Figure 1.

However, the robustness of the Z shape depends on the relatively strong hypothesis that no confounding factors can possibly bias the non-parametric estimation in the whole range of vote shares between 48.5% and 52.5%. This hypothesis is not necessary for the consistency of RDD, which depends only on the lack of full manipulation at exactly the threshold of 50%. That is, if one thinks that elections of 4-5% margin cannot be considered close and may present endogeneity problems with respect to the identity of the winner or loser, then such endogeneity can significantly affect the Z-shape, but it cannot invalidate the RDD result obtained from the 50% threshold.

In summary, Tables 2 and 3 (and A1) show that our results are very robust to different methodological specifications. Furthermore, they are found only in specifications where the treatment matters, and not in tests with irrelevant event windows or irrelevant vote share thresholds. Consequently, political connection must be the causal factor behind these results.

4.3. EFFECTS BY GROUPS

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

[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, we 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 divide our samples into two subsamples of firms depending on whether the close election is for the Senate or the House, rerun the benchmark regression in column (2) of Table 1, 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: in both the Senate and the House, firms connected to the winner experience significant loss in firm value. In addition, firms connected to the winner in a close Senate election appear to experience a greater loss of firm value than do firms connected to the winner in a close House election (-4.24% against -2.14%).

We also explore whether a candidate's position as incumbent or challenger in a close election and her prior political experience affect our results, by partitioning the sample accordingly. Regression results from columns (3) and (4) show that firms connected to the winner who is a challenger experience a significant loss of value, while the effect is not significant among incumbents. In columns (5) and (6), we further explore the sample of Democrats and of Republicans. In both cases, the effect is statistically significant at 5%, with a slightly larger size for Republicans than for Democrats (-2.86% versus -2.43%).

Our measure of social networks is based on the network between directors and politicians. As independent directors and executive directors are supposed to assume different tasks, we repeat our tests in subsamples of connections through independent directors and executive directors. We find, as reported in columns (7) and (8), that firms connected to a politician through one of its independent directors experience a significant loss of value (-2.76%), while the impact is not significant in firms connected to a politician through an inside

(executive) director. In the latter sample, the estimate at -1.84% is still negative and sizeable, and the lack of significance could be attributed to the small sample size.

The impact of political connection on firm value may also depend on the nature of the connection. We investigate this direction by checking the estimates across different school networks. We sort the educational institutions by the number of observations in the sample, as it is important to look at the number of prominent graduates that rise to the top in business and politics, and not just at any graduate from the same year. Intuitively, when a network is better represented in the sample, its links are arguably stronger in Granovetter's (1974) sense, in that each pair share more common connections. Such a network has a higher measure of network closure, according to Karlan et al. (2009), and is more conducive to agreements that require commitments between pairs in the network. In contrast, Karlan et al. (2009) show that a low closure network is provides better incentives for information sharing.

Through this exercise, Harvard and Yale come out as the two most represented universities (if we had looked at the number of graduates each year, large state universities would have dominated). Column (9) reports the estimate of -3.92% for the subsample of connections based on those two networks, while columns (10) and (11) show the results for the subsamples of universities that are below and above the median number of observations, respectively at -2.45% and -2.55%. The effect is markedly stronger for Harvard and Yale as compared with the average, yet little difference between the subsamples above and below median. This result is consistent with an explanation that network strength and network closure matter only at the very top schools, and that political connections matter mostly as commitment devices for deals, rather than for information sharing purposes. However, we cannot rule out some alternative explanations, such as that the media pay more attention to the educational background of graduates from Harvard and Yale, or that there are other unobserved elements very specific to these universities that help strengthen this effect.

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 to the type of election (Senate vs. House), as well as across several subsamples and subgroups.

4.4. ALUMNI NETWORK

We have so far identified the social connections between a board director of a firm and a politician by the criterion that the politician and the director graduate within one year from the same university, campus, college, or professional school. In this subsection, we study the impact

of a politician's alumni network by relaxing the restriction on year of graduation. Columns (1) to (9) in Table 5 replicate the same tests from Table 1 and report the results. Column (10) reruns the benchmark non-parametric test in column (1) of 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 is statistically significant at 5% but much smaller than the corresponding estimate of -2.65% for classmate networks, as reported in column (2), Table 1. The non-parametric test reported in column (10) produces a larger coefficient of -1.38%, statistically significant at 1%. Across the columns of Table 5, the negative and significant estimates of the value of alumni-network political connection on the CARs remain consistent, with coefficient sizes much smaller than in Table 1. (In columns (1) and (6), the results are no longer significant at 10%, though they are negative.)

The smaller estimates in Table 5, as compared with Table 1, can be explained in two different ways. First, one should expect the links between alumni who are not classmates to be less important than the links between classmates. Because our result is an average effect over all pairs of connected individuals, the estimate 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, the presence of measurement errors will likely imply an attenuation bias on our estimates. As more measurement error is probable in the alumni networks than in classmate networks, the attenuation bias will be more important for the alumni networks, leading to smaller estimates, as found in Table 5.

Overall, results from Table 5 show that our main results remain consistent when we relax our measure of social networks to alumni networks. In our context, we still find a social network effect even in a sample constructed with a less stringent definition of social network.

5. EXPLANATION OF THE RESULTS

The robust finding that connections to politicians in Congress reduce firm value appears in contrast to the existing literature on political connections. In this section, we propose and

verify an explanation for this apparently counter-intuitive phenomenon: politicians in fact bring more value to connected firms before, rather than after, being elected to the federal level. Before their elections to the Capitol, most politicians have had political experience at the state level, which has probably already resulted in benefits for connected firms. If the politicians win their congressional elections and move to federal politics, those firms' benefits of connections can be much harder to maintain. On the one hand, an elected politician will probably have much less time and focus for specific state matters that relate to their connected firms. On the other hand, the strong checks and balances in federal politics in the United States may already block all 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, 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 offers several further testable predictions based on the characteristics of the state from which the politician comes, as well as those of the politician and those of the firm, as stated below:

Prediction 1: The loss of value is only present for politicians coming from high-level state politics, not from politicians previously holding federal offices, including incumbents.

Prediction 2: In states with stronger institutional checks and balances, firms receive fewer benefits from their state-level political connections through social networks.

Prediction 3: Firm activities in the connected politician's state should decline following the politician's successful election, as compared with an unsuccessful one. Other firm characteristics may also determine the value of political connections.

We will test those predictions by dividing the sample according to the determinants of benefits from political connections from those predictions, along the line of institution quality measures, of politician's career background, and of firm's size and activities, then run the benchmark regression in each subsample and compare the estimates. The following subsections will detail the corresponding results.

5.1. TESTS BASED ON POLITICIAN'S BACKGROUND

Prediction 1 provides the most direct test of our explanation that is based on the background of candidates for Congress. We collect information on the positions these candidates have held up to election and classify four categories of politicians whose main

occupation in the election year was (1) in a public office at federal level; (2) in a public office at state level, or below; (3) in a corporate environment; or (4) in other environments, including NGOs, labor unions, and independent professions, such as doctors and professors. Table 6 reports the benchmark estimates by the corresponding subsamples.

[Insert Table 6 Here]

Columns (1) and (2) respectively show the estimate for the subsample of incumbent congressmen and challengers. The challengers also include candidates in a race for an open seat from which the incumbent had retired. Consequently there are significantly more challengers than incumbents (if all races comprise an incumbent and a challenger, by the logic of close-election near-randomization, incumbents should roughly number the same as challengers). Among challengers, the estimate of -3.24% is statistically significant at 1% and stronger than the average effect found in Table 1, while the estimate is not significant among incumbents. This result suggests that removing an incumbent from office does not significantly affect a connected firm's value, while having a connected politician elected to Congress significantly reduces it.

Columns (3) to (8) consider smaller subsamples among challengers. Columns (3) and (4) distinguish between challengers coming from various positions at the federal level (for instance, in a senator's office) and others. As our explanation would predict, the effect is insignificant and close to zero for the former, while significantly negative for the latter. We further decompose the subsample in column (4) into politicians coming from public offices, from the business world and others, with results shown respectively in columns (5), (7), and (8). The estimate in column (5) is negative, while not statistically significant at 10%. A deeper investigation in this subsample shows that the type of positions held by those challengers varies widely, from mayors and district court clerks to state legislators and governors, and this diversity potentially adds much noise to the subsample in column (5). Indeed, when we refine this subsample to only those who had previously held top-level positions as governors or state legislators, the estimate comes out statistically significant at -3.28%, as shown in column (6), even for a much smaller sample size.

The estimate for candidates coming from the corporate world shown in column (7) is also strongly negative and significant, suggesting that firms connected to the candidate suffer a loss in value due to the candidate's exit from the business world. Put another way, a candidate brings more value to a connected firm as long as he or she remains in the business world. On the

other hand, for the group of candidates from non-political, non-business occupations, the estimated effect is close to zero, as reported in column (8).

Taken together, Table 6 shows that a candidate's election to Congress clearly destroys value of connected firms if the elected congressman has been sufficiently entrenched in his home state. In contrast, the value of incumbent congressmen or of congressmen coming from positions in federal office is not significantly different from zero. While this test is still subject to the problem of selection into occupations and offices prior to elections, it strongly supports our explanation that politicians bring value to firms only at state level.

Related to incumbent's background, we push prediction 1 further for incumbent candidates, using their membership in congressional committees. The results are reported in Table 7 below.

[Insert Table 7 Here]

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. Column (2) shows a particularly strong positive effect of 7.74% (statistically significant at 5%) on firm value when a congressman in the committees on appropriations in either house is defeated in a close election. The appropriations committees of both houses control the allocation of federal funds to specific projects and are often regarded as the most important committees in Congress (see, e.g., Aghion et al., 2009). This finding shows that a politician's membership in appropriation committees is indeed very valuable to connected firms.

We do not find consistent evidence of the value of connection to members of other committees in Congress. Columns (3) to (6) report results for groups of committees in both houses, classified by their relations to natural resources, energy, and agriculture in column (3); the armed forces, government, and congressional affairs in column (4); education, science, health, and labor in column (5); and economic, financial, and budgetary issues in column (6). In comparison with column (2), none of those subsamples produce a similar positive effect.

On the other hand, the subsample of Senate committees produces a large positive estimate of 8.59%, as shown in column (7). This effect is due mostly to senior members of the Senate. In fact, if we limit the sample to senators in their 4th year or beyond in committees, as reported in column (8), the effect of 10.40% is economically important; whereas in the other half of the sample of senators, shown in column (9), the effect becomes negative.⁷ This finding confirms the role of seniority in Congress as previously stressed in political science (e.g., Roberts, 1990; Kellerman and Shepsle, 2009).

Results from Tables 6 and 7 indicate that the value of connection to a congressman initially drops when the freshly elected congressman moves away from his previous position at state level, and is only restored once he becomes senior and powerful in Congress. However, given the small number of senators (by election year) in the regressions in Table 7, those results related to senators may lack robustness. Conservatively, we are most confident with the first part of the storyline, namely the drop in value when the congressman moves from state level to federal office.

5.2. TESTS BASED ON STATE CHARACTERISTICS

Moving away from politicians' backgrounds, prediction 2 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 various ample support of this prediction.⁸

[Insert Table 8 Here]

Columns (1) and (2) distinguish between politicians' states having more or less than median regulations. The index of regulation by state is measured for 1999 in Clemson

⁷ To be more precise, we measure seniority by averaging over a congressman's seniority across her committees to make the subsamples in columns (8) and (9) exclusive. The choice of 4th year experience is closest to the sample median.

⁸ Those results are also confirmed by regressions, including an interaction between our main explanatory variable, Win/Lose, and a dummy variable distinguishing between good and bad institutions, measured in ways similar to those shown in Table 8. Because these regressions implicitly impose the same coefficients for the controls of vote share for each subsample, they are less preferred than our reported results, and are only available upon request.

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 strongly significant effect in states with more regulations, where the potential is greater for politicians to grant benefits to connected firms on a discretionary basis.

Instead of regulations, columns (3) to (8) attempt to 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. Columns (3) and (4) use that measure to show a more sizable and significant effect for more corrupt states.

Because 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 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. While Exalead.com conducts web-based searches, columns (7) and (8) use the dataset of all newspapers gathered in Newslibrary.com to search for the word "corruption" close to the state name, with the number of search hits normalized by the search hits for the state name alone. We can thus cover cases of corruption as reported both on the internet and in newspapers. Both measures yield satisfying results that support our intuition, as the effect is clearly stronger and statistically significant in more corrupt states, while it is indistinguishable from zero in less corrupt states.

As these measures of institutional qualities are calculated before this paper's period of study, we partly avoid the problem of direct reverse causation. However, the use of measures of corruption or regulations may expose us to the problem of endogenous selection, where certain unobserved characteristics may affect both the selection into good or bad institutional designs, and later affect the political connection that we estimate. While we still have clearly identified the WATE of political connections, we cannot ascertain that its variation across states truly comes from the differences in institutional quality. This endogeneity problem is a perennial problem in

all studies of the economics of institutions, where the exogeneity and excludability of instrumental variables are keys to the answer.

In columns (9) and (10) we attempt to check this problem by using GCISC, a measure of population concentration around the state capital city. As developed by Campante and Do (2010), this measure is highly predictive of several measures of qualities of political governance both across countries and across American states (higher concentration implies better institution quality because of the political pressure of the population). This measure is also highly persistent over time and is arguably much less directly affected by institution qualities than by historical events, such as the somewhat arbitrary choice of state capitals. As expected, our estimated effect is strongly significant among states of lower-than-median population concentration, as shown in column (9), while in column (10), the effect is practically indistinct from zero.

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

5.3. TESTS BASED ON FIRM CHARACTERISTICS

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

[Insert Table 9 Here]

Columns (1) and (2) report regression results on two subsamples of firms whose market capitalization is respectively above or below the median. The difference between those results indicates 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). Put differently, political connections are more important for small firms. Larger firms may be connected to many politicians, and the financial benefit of connection to one more politician may only represent 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 $(\text{CapEx} - \text{Cashflow from Operations})/\text{CapEx}$, then divide our sample into industries with above and below median scores. Columns (3) and (4) of Table 9 report our standard regression results on these two sub-samples. For industries relying more on external finance, the coefficient on the Win/Lose dummy is -2.99% and significant at 5%; in contrast, for the subsample of industries with less dependence 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 estimated effect appears to be particularly strong when determinants are interacted. 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%) in firm value as a result of a connection to a politician in federal office. Column (6) considers the subsample of states with higher than median corruption, using the Newslibrary.com measure as detailed in the previous subsection, for which the distance between the firm's headquarters and the politician's state is in the smallest quartile (less than 650km). Such distance is used as 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 the politician's state. The estimated effect in column (6) is much stronger than in column (7) of Table 8, and much stronger than for the sample with the limitation by distance alone.⁹ In column (7), the sample is limited to states with higher than median corruption, and to industries with higher than median reliance on external finance. As expected, the effect is strongly significant, and is much larger than both column (3) and column (7) of Table 8.

The estimation results shown in Table 9 indirectly corroborate the storyline that firms get benefit from politicians before their election to federal office. 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 unavailable.

We surmount 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

⁹ Results on the division relating to the distance between a firm's headquarters and a politician's state are available upon request.

year, using Newslibrary.com; we then normalize the number of search hits by the search hits for the neutral keyword “September” across the same set of newspapers. The resulting hit rate is used as a proxy of a firm’s activities within a state in a certain year. We further remove any firm-state fixed effect by looking at only the change in the hit rate after each year, then use this measure of changes of firm activities across various windows and subsamples as the dependent variable in our benchmark regressions, and report the results in Table 10.¹⁰

[Insert Table 10 Here]

Columns (1) to (3) focus on the subsample of challengers with top state experience that was used in column (6) of Table 6. Column (1), in particular, shows that being connected to an elected congressman clearly reduces a firm’s activities in the corresponding state from the election year (where elections are held in November) to the following year, with a coefficient of -1.54 percentage points of hit rates. Column (2) presents a placebo test in the period before the event, between election year -1 and 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 has been accomplished by 1 year after the election, as the estimated effect is close to zero for the window from year 1 to year 2.

Focusing on the main event window from the election year to the year after, columns (4) to (6) follow Table 6 in treating different subsamples of politicians, similar to the samples in columns (3), (7), and (8) in Table 6, respectively (candidates from federal offices, from corporate environments, and from other backgrounds, respectively). Reassuringly, we do not see any significant results in those subsamples, confirming the intuition 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 further 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

¹⁰ Changes in our measure of firm activities, calculated for the whole sample of all U.S. local newspapers, are highly correlated with changes in firm sales, investments, R&D, employment, and cash flows. These results, available upon request, suggest that our measure is a good proxy for firm activities at state level.

the favor is over. Taken together, the verifications of predictions 1 to 3 across Tables 6 to 10 provide a wide array of support to the explanation that politicians bring more benefits to (certain kinds of) connected firms before, than they do after, elections to federal office.

6. CONCLUSIONS

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 connection to a politician elected to the U.S. Congress in a closely contested race. The estimate of the Weighted Average Treatment Effect (WATE) during the period 2000 to 2008 shows a negative and significant cumulative abnormal return of 2.65% surrounding the election date. The results are robust to various specifications, parametric and nonparametric, 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 effectively deals with the endogenous nature of connected politicians. Our results are also externally valid, as the estimated WATE is averaged over the sample of all politicians susceptible to experience a close election. The external validity is further strengthened as firms in our sample are comparable to Compustat's universe.

Second, we find a negative estimated value of connection to U.S. congressmen. This apparently surprising estimate is consistent with an explanation that firms benefit more from political connections when the connected politician remains in state politics than when (s)he moves to the federal level. We empirically test several resulting predictions and find a wide range of evidence supporting our hypothesis.

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

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TECHNICAL APPENDIX

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

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

This estimate is well identified under the assumption that the density of *VoteShare*, conditional on all characteristics of an observation, is continuous. Such assumption is warranted if the incidence of winning cannot be perfectly manipulated by candidates. 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.

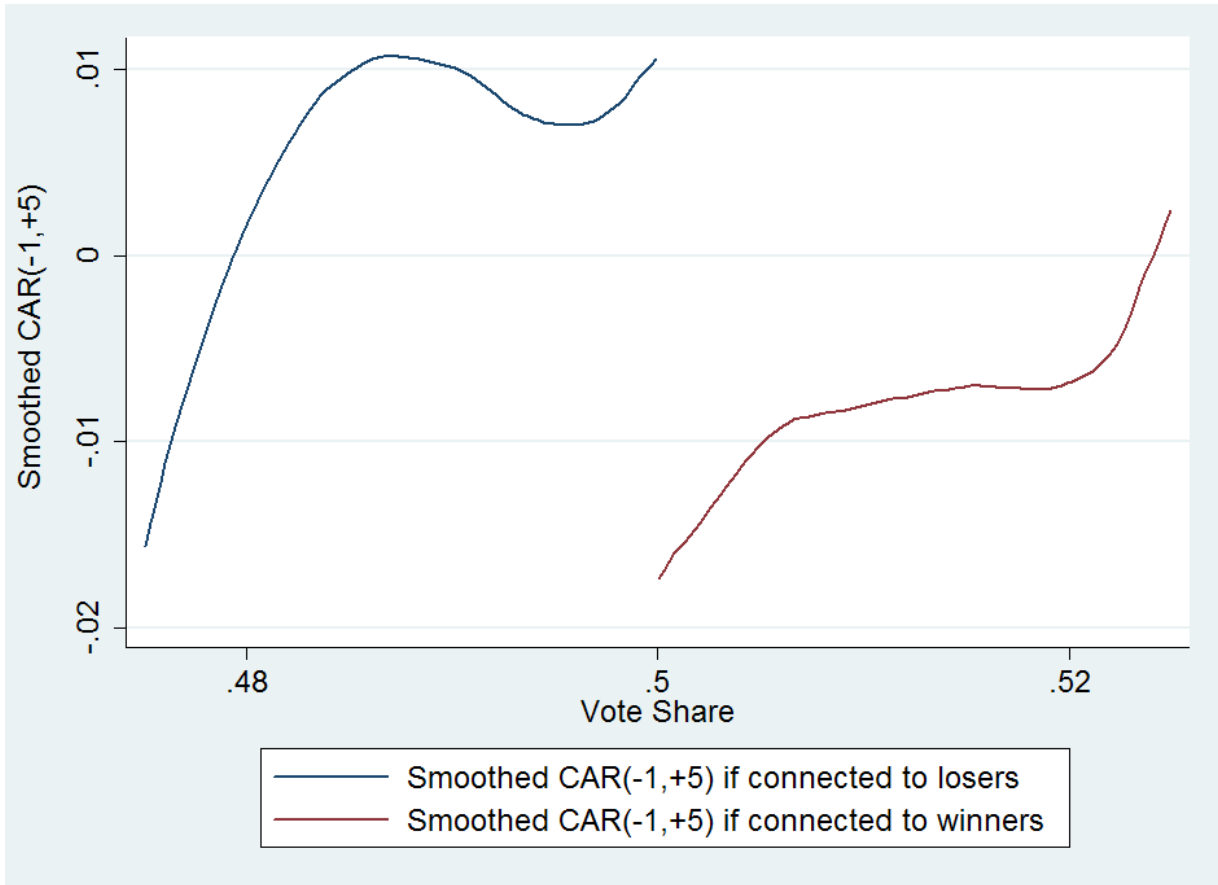


Figure 1

Table D1 Summary Statistics

A. Close Elections at 5%-Vote Margin

Election Year	Senate		House of Reps.		Total	
	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%
	23	2.42%	105	2.57%	128	2.54%

B. Time Series (Biannual Observations, 2000-2008)

	Mean	Median	Minimum	Maximum	Q1	Q3	Stdev
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

C. Distribution of Degree and Graduation Years

Degree			Graduation		
	Politicians	Directors	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%	>=1990	7.6%	1.7%

D. Firm Characteristics

	Sample					Compustat Universe				
	Min	Mean	Median	Max	SD	Min	Mean	Median	Max	SD
Market Cap (in \$ millions)	2.3	2048.9	379.1	58638.2	5341.7	0.0	2346.4	242.5	467092.9	11209.5
Common Equity (in \$ millions)	1.0	776.3	163.2	52817.0	2652.3	0.0	1014.0	121.3	224234.3	5540.5
Market to Book Ratio	0.1	4.61	2.23	246.08	12.39	0.0	4.28	1.92	7071.35	59.17
Capital Expenditure (in \$ millions)	0.0	86.78	9.50	3023.0	274.88	0.0	147.09	6.85	31574.35	900.73
Age	0.1	8.62	8.41	40.58	5.7	0.0	8.08	7.24	59.71	6.2

Notes:

- (1) Corresponding Compustat universe includes all firms within Compustat in 2000, 2002, 2004, 2006 and 2008.
- (2) Book value of equity<0, Capex<0, Share outstanding<0, Price at fiscal year end <0, Firm Age <0 are removed.

Table 1: Main Estimations

This table reports the main pooled OLS regressions of the Cumulative Abnormal Returns among the politically connected firms around close elections for US Senate and Congress between 2000 and 2008. Each observation pairs a firm's director to a candidate finishing first or second in a close election, who furthermore graduates from the same university program within a year (Cohen et al. 2008). Average abnormal returns are estimated based on the market model around the election day (Day 0). The market model is estimated using daily data over a 255-day (-315,-61) period. Win/Lose is a dummy variable equal to one if and only if a politician finishes first or second in an election. A close election is specified by the margin of votes between the top two candidates, with an x% margin referring to the subsample of elections with less than x% vote margin. Vote Share (Winners) and Vote Share (Losers) refer to the vote shares of winners and vote shares of losers, respectively. Column (3) controls for a quartic polynomial in vote share, separately for losers and winners. Column (4) controls for dummy variables representing party, gender, incumbency and senate/house race information of the politician involved. Column (5) controls for firm's market value. 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 of 50% or higher. Standard errors in square 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	1% margin	5% margin	5% margin	5% margin	5% margin	5% margin	5% margin	5% margin	No Outliers
Win/Lose	-0.0312 [0.00970] **	-0.0265 [0.00853] ***	-0.0407 [0.0137] ***	-0.0282 [0.00873] ***	-0.0266 [0.00857] ***	-0.0257 [0.00835] ***	-0.0270 [0.00926] ***	-0.0291 [0.0110] ***	-0.0218 [0.00758] **
Vote Share (Winners) and Vote Share (Losers)	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls			Full Poly	Politician Variables	Market Value	Year FE	Industry FE	School FE	
R-squared	0.020	0.006	0.010	0.008	0.006	0.013	0.040	0.085	0.004
Obs	316	1,819	1,819	1,817	1,819	1,819	1,804	2,066	1,806

Table 2 Alternative Specifications

This table reports the pooled OLS regressions of the Cumulative Abnormal Returns and Standardized Cumulative Abnormal Returns among the politically connected firms around close elections for US Senate and Congress between 2000 and 2008 for alternative event study windows. Each observation pairs a firm's director to a candidate finishing first or second in a close election, who furthermore graduates from the same university program within a year (Cohen et al. 2008). Average abnormal returns are estimated based on the market model around the election day (Day 0). The market model is estimated using daily data over a 255-day (-315,-61) period. Standardized CAR is CAR normalized by volatility during the event period. Win/Lose is a dummy variable equal to one if and only if a politician finishes first or second in an election. A close election is specified by the margin of votes between the top two candidates being less than 5%. Vote Share (Winners) and Vote Share (Losers) refer to the vote shares of winners and vote shares of losers, respectively. Standard errors in square 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)	(-1,0)	(-1,5)	(0,5)	(1,5)	(6,20)
Win/Lose	0.00278 [0.0162]	-0.00804 [0.00544]	-0.0265 [0.00853]	-0.0182 [0.00947]	-0.0185 [0.00802]	0.0139 [0.0220]
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.004	0.003	0.006	0.005	0.005	0.002
Obs	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)	(-1,0)	(-1,5)	(0,5)	(1,5)	(6,20)
Win/Lose	-0.100 [0.181]	-0.145 [0.136]	-0.322 [0.125]	-0.261 [0.143]	-0.290 [0.129]	0.0616 [0.142]
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.011	0.004	0.005	0.004	0.004	0.000
Obs	1,477	1,819	1,819	1,819	1,819	1,819

Table 3 Nonparametric Tests

This table reports the nonparametric regressions of the Cumulative Abnormal Returns among the politically connected firms around close elections for US Senate and Congress between 2000 and 2008. Each observation pairs a firm's director to a candidate finishing first or second in a close election, who furthermore graduates from the same university program within a year (Cohen et al. 2008). Average abnormal returns are estimated based on the market model around the election day (Day 0). The market model is estimated using daily data over a 255-day (-315,-61) period. Win/Lose is a dummy variable equal to one if and only if a politician finishes first or second in an election. Each column consists of running a local cubic polynomial regression of the dependent variable on vote shares in a subsample above the cutoff and a subsample below the cutoff, then calculating the difference between the predicted values of the dependent variable for each subsample around the cutoff. The first column shows the result for the realistic cutoff of 50%. Columns (2) to (5) show the results for different values of the bandwidth. Columns (6) to (9) show results with hypothetical cutoffs. Standard errors are in square 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 [0.0168] **	-0.034 [0.0168] **	-0.0342 [0.0167] **	-0.0345 [0.0168] **	-0.0387 [0.0180] **	0.0805 [0.0235] ***	0.0128 [0.0207]	0.0465 [0.0283]	0.0234 [0.0218]	

Table 4: Effects by Group

This table reports the pooled OLS regressions of the Cumulative Abnormal Returns among the politically connected firms around close elections for US Senate and Congress between 2000 and 2008. Each observation pairs a firm's director to a candidate finishing first or second in a close election, who furthermore graduates from the same university program within a year (Cohen et al. 2008). Average abnormal returns are estimated based on the market model around the election day (Day 0). The market model is estimated using daily data over a 255-day (-315,-61) period. Win/Lose is a dummy variable equal to one if and only if a politician finishes first or second in an election. A close election is specified by the margin of votes between the top two candidates being less than 5%. Vote Share (Winners) and Vote Share (Losers) refer to the vote shares of winners and vote shares of losers, respectively. Columns (1) to (8) respectively show results on the subsamples of Senate or House races, incumbent or challenger candidates, democrats or republicans, and independent directors or executive directors. Columns (9) to (11) examine subsamples of connections through Harvard & Yale, and institutions that are alma mater of less or more than 50 individuals (sample's median) in the sample. Standard errors in square brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Subsample	Dependent Variables: CAR (-1,5)										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Senate	House	Incumbent	Challenger	Democrats	Republicans	Indep. Directors	Exec. Directors	Harvard & Yale	Small Networks	Large Networks
Win/Lose	-0.0424 [0.0117] ***	-0.0214 [0.0112] *	-0.0129 [0.0145]	-0.0324 [0.0107] ***	-0.0243 [0.0117] **	-0.0286 [0.0137] **	-0.0276 [0.00901] ***	-0.0184 [0.0210]	-0.0392 [0.00849] ***	-0.0245 [0.0113] **	-0.0255 [0.00985] **
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.015	0.004	0.005	0.009	0.004	0.008	0.007	0.003	0.017	0.005	0.009
Obs	559	1,260	598	1,221	1,057	762	1,493	326	449	1,092	727

Table 5: Alumni Networks

This table reports the pooled OLS regressions of the Cumulative Abnormal Returns among the alumni-network politically connected firms around close elections for US Senate and Congress between 2000 and 2008. Each observation pairs a firm's director to a candidate finishing first or second in a close election, who furthermore graduates from the same university program without restriction on year of graduation (Cohen et al. 2008). Average abnormal returns are estimated based on the market model around the election day (Day 0). The market model is estimated using daily data over a 255-day (-315,-61) period. Win/Lose is a dummy variable equal to one if and only if a politician finishes first or second in an election. A close election is specified by the margin of votes between the top two candidates, with an x% margin referring to the subsample of elections with less than x% vote margin. Vote Share (Winners) and Vote Share (Losers) refer to the vote shares of winners and vote shares of losers, respectively. Column (3) controls for a quartic polynomial in vote share, separately for losers and winners. Column (4) controls for dummy variables representing party, gender, incumbency and senate/house race information of the politician involved. Column (5) controls for firm's market value. 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 of 50% or higher. Column (10) runs a local cubic polynomial regression of the dependent variable on vote shares in a subsample above the cutoff and a subsample below the cutoff, then calculates the difference between the predicted values of the dependent variable for each subsample around the cutoff. Standard errors in square brackets are corrected for clustering by politicians in each election, except in column (10) where clustering does not matter. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Subsample	1% margin	5% margin	5% margin	5% margin	5% margin	5% margin	5% margin	5% margin	No Outliers	Non-Param.
Win/Lose	-0.0029 [0.0036]	-0.0058 [0.0028] **	-0.0121 [0.0060] **	-0.0054 [0.0024] **	-0.0058 [0.0024] **	-0.0036 [0.0027]	-0.0057 [0.0028] **	-0.0058 [0.0034] *	-0.0052 [0.0023] **	-0.0138 [0.0042] ***
Vote Share (Winners) and Vote Share (Losers)	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	-
Controls			Full Poly	Politician Variables	Market Value	Year FE	Industry FE	School FE		-
R-squared	0	0.001	0.001	0.001	0.001	0.012	0.017	0.015	0.001	-
Obs	5,656	29,527	29,527	29,063	29,527	29,527	29,527	29,527	29,330	29,527

Table 6: Tests by Politicians' Previous Experience

This table reports the pooled OLS regressions of the Cumulative Abnormal Returns among the politically connected firms around close elections for US Senate and Congress between 2000 and 2008. Each observation pairs a firm's director to a candidate finishing first or second in a close election, who furthermore graduates from the same university program within a year (Cohen et al. 2008). Average abnormal returns are estimated based on the market model around the election day (Day 0). The market model is estimated using daily data over a 255-day (-315,-61) period. Win/Lose is a dummy variable equal to one if and only if a politician finishes first or second in an election. A close election is specified by the margin of votes between the top two candidates being less than 5%. Vote Share (Winners) and Vote Share (Losers) refer to the vote shares of winners and vote shares of losers, respectively. Columns (1) and (2) consider samples of incumbents and election challengers. Columns (3) and (4) divide the sample of challengers into those with recent federal positions and the rest. Column (5) group all challengers with recent state level positions, and column (6) limits them to those with past positions in state's legislative bodies or as governors. Column (7) considers challengers from corporate environment, and column (8) considers the rest (non-politician, non-corporate backgrounds). Standard errors in square brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Subsample	Dependent Variables: CAR (-1,5)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)				
	All Incumbents	All Challengers	Among Challengers		From State Politics	Top State Experience (House, Senate, Gov.)	From Business	From Others				
		From Federal Offices	Not From Federal Offices									
Win/Lose	-0.0129 [0.0145]	-0.0324 [0.0107]	***	-0.00832 [0.0287]	-0.0350 [0.0104]	***	-0.0394 [0.0282]	-0.0328 [0.0193]	*	-0.0387 [0.00840]	***	0.00518 [0.0288]
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.005	0.009	0.007	0.011	0.007	0.010	0.011	0.011	0.011	0.011	0.011	0.093
Obs	598	1,221	199	1,022	448	402	474	474	474	474	474	126

Table 7: Tests by Incumbents' Committee Membership

This table reports the pooled OLS regressions of the Cumulative Abnormal Returns among the politically connected firms around close elections for US Senate and Congress between 2000 and 2008. Each observation pairs a firm's director to an incumbent Congressman finishing first or second in a close election, who furthermore graduates from the same university program within a year (Cohen et al. 2008). Average abnormal returns are estimated based on the market model around the election day (Day 0). The market model is estimated using daily data over a 255-day (-315,-61) period. Win/Lose is a dummy variable equal to one if and only if a politician finishes first or second in an election. A close election is specified by the margin of votes between the top two candidates being less than 5%. Vote Share (Winners) and Vote Share (Losers) refer to the vote shares of winners and vote shares of losers, respectively. Column (1) groups all incumbent candidates. Column (2) considers only members of the Appropriations Committee in both chambers. Columns (3) to (6) consider other groups of committees in both chambers, respectively related to natural resources and agriculture in (3), state, government and Congress affairs in (4), education, health, labor and sciences in (5), and economic, financial and budgetary issues in (6) (see appendix for detailed classification). Column (7) reports results from senate committees, of which the results for the subsamples of senators with committee seniority above and below 4 years are respectively reported in column (8) and (9). Column (10) shows results for house committees. Standard errors in square brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Subsample	Dependent Variables: CAR (-1,5)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All incumbents	In Both Houses					Senate Committees	Of Senate Committees		House Committees
	Appropriations Comm.	Resources & Agriculture	State Affairs	Education, Health, Labor	Economy & Budget		Seniority > 4	Seniority ≤ 4		
Win/Lose	-0.013 [0.0145]	0.0774 [0.0265]	-0.00133 ** [0.0158]	-0.0226 [0.0123]	-0.00560 * [0.0532]	-0.0200 [0.0169]	0.0859 [0.0170]	0.104 [0.0279]	-0.0555 *** [0.00264]	-0.00999 *** [0.0184]
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.008	0.026	0.005	0.016	0.003	0.011	0.016	0.019	0.081	0.004
Obs	582	56	281	331	213	469	126	80	46	456
Congressmen	70	10	30	25	23	54	11	7	4	59

Table 8: Variation by State's Regulation, Corruption and Institution Quality

This table reports the pooled OLS regressions of the Cumulative Abnormal Returns among the politically connected firms around close elections for US Senate and Congress between 2000 and 2008. Each observation pairs a firm's director to a candidate finishing first or second in a close election, who furthermore graduates from the same university program within a year (Cohen et al. 2008). Average abnormal returns are estimated based on the market model around the election day (Day 0). The market model is estimated using daily data over a 255-day (-315,-61) period. Win/Lose is a dummy variable equal to one if and only if a politician finishes first or second in an election. A close election is specified by the margin of votes between the top two candidates being less than 5%. Vote Share (Winners) and Vote Share (Losers) refer to the vote shares of winners and vote shares of losers, respectively. Columns (1) to (10) respectively show results on the subsamples of above of 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, 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 2010). Standard errors in square brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

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

Table 9: Firm characteristics as determinants of the value of political connection

This table reports the pooled OLS regressions of the Cumulative Abnormal Returns among the politically connected firms around close elections for US Senate and Congress between 2000 and 2008. Each observation pairs a firm's director to a candidate finishing first or second in a close election, who furthermore graduates from the same university program within a year (Cohen et al. 2008). Average abnormal returns are estimated based on the market model around the election day (Day 0). The market model is estimated using daily data over a 255-day (-315,-61) period. Win/Lose is a dummy variable equal to one if and only if a politician finishes first or second in an election. A close election is specified by the margin of votes between the top two candidates being less than 5%. Vote Share (Winners) and Vote Share (Losers) refer to the vote shares of winners and vote shares of losers, respectively. 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 square 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 by Newslibrary search hits in politician's State. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Subsample	Dependent Variables: CAR (-1,5)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	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 [0.0197] ***	0.000202 [0.00911]	-0.0299 [0.0128] **	-0.0217 [0.0148]	-0.0564 [0.0198] ***	-0.0718 [0.0223] ***	-0.0377 [0.0185] **
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.023	0.004	0.010	0.004	0.025	0.034	0.015
Obs	763	1,056	948	871	511	359	550

Table 10: Firms Activities in States

This table reports the pooled OLS regressions of the change in firm activities among the politically connected firms around close elections for US Senate and Congress between 2000 and 2008. Each observation pairs a firm's director to a candidate finishing first or second in a close election, who furthermore graduates from the same university program within a year (Cohen et al. 2008). Firm activities in a given state in a given year are measured as "Firms Reported In Local Newspapers" (FRILN), 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 FRILN over different event windows, with year 0 being the election year. Win/Lose is a dummy variable equal to one if and only if a politician finishes first or second in an election. A close election is specified by the margin of votes between the top two candidates being less than 5%. Vote Share (Winners) and Vote Share (Losers) refer to the vote shares of winners and vote shares of losers, respectively. Columns (1) to (3) limit the sample to challengers with past positions in state's legislative bodies or as governors, respectively with windows of one year after, one year before, and two years after the election year. The samples in columns (4) to (6) are respectively challengers coming from federal offices, from corporate environment, and from non-political, non-corporate occupations. Standard errors in square brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

	Challengers with Top State Experience			From Federal Offices	From Business	From Others
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Change in Activities	(0,+1)	(-1,0)	(+1,+2)	(0,+1)	(0,+1)	(0,+1)
Win/Lose	-0.0154 [0.00253] ***	-0.00148 [0.00616]	-0.00152 [0.00764]	-0.000822 [0.00163]	-0.0102 [0.00635]	-0.393 [0.435]
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.009	0.013	0.000	0.014	0.012	0.016
Obs	402	401	402	199	470	126

Table A1: Further Robustness Checks

This table reports robustness checks of the Cumulative Abnormal Returns (CAR) among the politically connected firms around close elections for US Senate and Congress between 2000 and 2008. In columns (1) to (6) each observation pairs a firm's director to a candidate finishing first or second in a close election, who furthermore graduates from the same university program within a year (Cohen et al. 2008). 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 model with momentum in columns (5) and (6). Those models are estimated around the electionday (Day 0) using daily data over a 255-day (-315,-61) period. Win/Lose is a dummy variable equal to one if and only if a politician finishes first or second in an election. A close election is specified by the margin of votes between the top two candidates, with an x% margin referring to the subsample of elections with less than x% vote margin. Vote Share (Winners) and Vote Share (Losers) refer to the vote shares of winners and vote shares of losers, respectively. 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 square brackets are corrected for clustering by politicians in each election. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Dependent Var:	Raw Returns (-1,5)		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	Two-way clustering Pol Year & Com Year
Win/Lose	-0.0204 [0.0190]	-0.0445 [0.0211]	-0.0228 ** [0.00774] ***	-0.0248 [0.0101] **	-0.0261 [0.00725] ***	-0.0270 [0.00949] ***	-0.0306 [0.00917] ***	-0.0287 [0.00819] ***	-0.0271 [0.0196]	-0.0261 [0.00759] ***
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
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
Obs	1,819	1,819	1,819	1,819	1,818	1,818	1,308	1,593	192	1,818