Political Connections and Firm Value: Evidence from the Regression Discontinuity Design of Close Gubernatorial Elections

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ABSTRACT

Using the regression discontinuity design of close gubernatorial elections in the U.S., we identify a significant and positive impact of the social networks of corporate directors and politicians on firm value. Firms connected to elected governors increase their value by 3.89%. Political connections are more valuable for firms connected to winning challengers, for smaller and financially dependent firms, in more corrupt states, in states of connected firms' headquarters and operations, and in closer, smaller, and active networks. Post-election, firms connected to the winner receive significantly more state procurement contracts and invest more than do firms connected to the loser.

Keywords: Social networks; political connection; firm value; regression discontinuity design; close gubernatorial election; corruption; procurement.

JEL Classifications: G3, G28, G30, G34, G38.

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1. INTRODUCTION

Do political connections impact firm value? The answer to this question yields crucial implications for shareholder value, corporate governance, institutional design, and incentive mechanisms for politicians and firms. Although abundant anecdotal evidence in the media suggests positive benefits of political connections, conducting research to prove, disprove, or generalize the relationship between political connections and firm value remains challenging. As with many topics in corporate finance, studies of political connections have to overcome the endogeneity issue, which prevents the precise identification and quantification of the impact of these connections.[1] In the U.S., the rarity of direct links of ownership or concurrent employment between corporations and politicians—resulting from the strict regulations and disclosure regime, as well as a high level of transparency—makes identifying potential political connections and gathering sufficient data and observations for empirical studies even more challenging.

Our paper attempts to address these challenges by investigating the value of political connections in the U.S., where institutions rank among the best, and the line between politics and business is among the clearest.[2] Any significant result we might find should, therefore, represent an underestimation of the value of political connections in other parts of the world that have a lower quality of institutions and governance.

We define political connections broadly by following a social network approach, as proposed by Bertrand et al. (2008), Cohen, Frazzini, and Malloy (2008), Fracassi and Tate (2012), Nguyen (2012), and Shue (2013). A firm is connected to a politician if one of its directors shares the same educational background with a politician. This definition represents a few advantages. First, the connections from the network of classmates and alumni are clearly and unambiguously defined based on publicly available information on educational backgrounds of all politicians and directors. Second, this network's coverage is broad enough to be representative of the population of politicians and directors and to avoid obvious and specific political connections that are subject to latent conflicts of interest, making it possible to generalize the empirical results. Third, alumni and classmate networks also play a particularly important role in American society. Educational institutions received as much as $41.67 billion in 2010, or 14% of all

[1] See Roberts and Whited (2013) for an overview of the endogeneity issues in empirical corporate finance and their solutions, including the regression discontinuity design.

[2] The United States ranks in the first decile in control of corruption, rule of law, regulatory quality, and government effectiveness in the World Bank's World Governance Indicators (Kaufman, Kraay, and Mastruzzi 2011) in 2000-2008.

charitable donations, second only to religious organizations (Giving USA Foundation, 2011.)[3] Our classmate-based connections might draw questions about the realistic nature of such connections, as, in actuality, not all classmates are friends (e.g., Leider et al. 2009). This factor, however, should not alter the significance of our results. Any measurement errors in this instance imply that the effect of real friendships is nuanced by many non-friend classmate social ties, thus producing an attenuation bias that reduces the absolute size of the estimate and its statistical significance. The effect of real friendships can thus be larger than that 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 the conditions for common communication and mutual trust as well as common access to the same social network (e.g., Shue 2013). Former classmates are thus more likely to later develop a strong connection, even if they were not close friends while in college or graduate school.

To identify the value of social network-based political connections, we propose a new empirical strategy. We study a sample of firms connected to candidates in close gubernatorial elections. Lee (2008) shows that close elections can be considered a Regression Discontinuity Design (RDD), a natural experiment that produces near-randomized-trial identification with great internal validity. That is, a connection to a politician elected to office by a small margin is at the limit identical to a connection to one defeated by a small margin. Their comparison in RDD identifies a consistent estimate that accounts for all potential confounding factors, be they observable or unobservable. This empirical design has gained popularity in labor, political, and development economics (see Lee and Lemieux 2010), but only recently in corporate finance (examples include Chava and Roberts 2008; Cuñat, Gine, and Guadalupe 2012; and Kerr, Lerner, and Schoar 2014.)

The RDD specification provides a treatment effect estimate of the stock-market value of a new connection to a governor, where the treatment is one that exogenously elects a firm's connected politician as governor, as opposed to not electing him. Given the identification strength, we vary the subsample of RDD by covariates to understand the value of governor

[3] We abstract from connections based on political contributions (e.g., Cooper, Gulen, and Ovtchinnikov 2010, Akey 2013) because clear links between firms and specific politicians are difficult to establish. Before 2010, firms cannot make direct contributions to politicians; they can only initiate an independently run political action committees (PAC), which channels individual donations from shareholders and employees to both major parties and their committees, other PACs and Super-PACs. Its contribution to a single specific candidate's committee is limited at $2,600 a year. Hence for most candidates, campaign finance that is identified as coming from a specific firm's associated PAC is only a tiny fraction of total campaign expenses. In our study, we control for total campaign contributions from all sources, with no effect on the estimates.

connections across different firms, industries and states. The strength of RDD also offsets a potential weakness of traditional event studies, in that we correctly estimate the value of connection even if the market misestimates the probability of event. Event-study techniques are still used in our approach only to improve estimation efficiency, and are not essential to the results.

Following Lee and Lemieux (2010), the specification estimates a Weighted Average Treatment Effect (WATE), where each politician's weight is his relative propensity to experience a very close election. While some politicians are less likely to have that experience than others, the inclusion of highly visible politicians such as Janet Napolitano in our sample suggests that our estimate can cover a broad share of the population of politicians, and is therefore generalizable to the sample of all politicians with a nonzero chance of experiencing a close election.

To further clarify the impact of the social networks of corporate directors and politicians on firm value, we study the robustness of our results to potential network homophily. Homophily, as first defined in sociology, refers to the phenomenon that people sharing the same characteristics are more likely to join the same network, thus confounding the effect of connections with the effect of shared characteristics (McPherson, Smith-Lovin, and Cook 2001.) Close election RDD still has a caveat in dealing with homophily, because potential beneficial policies may arise from common characteristics of the connected firm, director, and politician. We propose a new approach to tackle potential homophily bias by introducing interactions of school fixed effects with a dummy for schools with a winner, election year fixed effects, and winning industry fixed effects, in order to control for such potential beneficial policies. We find sensibly similar results, and ascertain that the discovered effects come from political connections, not shared characteristics. We also find stronger effects of connections among former classmates versus among alumni from far-apart graduation years (similar to results in Cohen, Frazzini, and Malloy 2008; Fracassi 2009; Nguyen 2012), and stronger effects in years of alumni reunions (as first tested in Shue 2013). Taken together, our identification strategy can shed light on social connections between governors and corporate directors.

We obtain data on gubernatorial elections from 1999 to 2010 from the Federal Election Commission, from which we filter in only elections of a winning margin within 5% between the winner and the loser. 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, and link each pair to the stock performance of the firm around the date of the politician's close election.[4] 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 an educational background with a director of the firm.

Our study provides a number of findings. First, political connections positively and significantly impact firm value. Firms connected to the winner in a close gubernatorial election experience a positive and significant average cumulative abnormal return (CAR) of 3.89% over and above the CAR of firms connected to the loser. The effect is robust to alternative specifications in the event windows and in the market models, and across sub-samples. Second, the value of political connections varies with candidate characteristics, state-level corruption, and firm and network characteristics. Political connections are more valuable for firms connected to winning challengers, for winning challengers without federal experiences before elections, in challenger-only elections, in states with higher levels of corruption, in states of connected firms' headquarters and corporate operations, in smaller firms, and in firms dependent on external finance. Political connections are more valuable when the connections are closer, more exclusive, and fresh from network reunions. Third, political connections exert real impact on firms. Following close elections, firms connected to the winner receive significantly more state procurement contracts and invest more than do firms connected to the loser.

While our paper is not the first to ask the question of the value of political connections, nor the first to try to tackle their underlying endogeneity, we contribute to the literature along several lines. First, we propose a new approach to measuring political connections based on the social networks of candidates to governorship and directors of listed firms. This approach does not exclude potential direct political connections in the prior literature and allows us to have a relatively sizable and fairly representative sample of politically connected firms, even in the context of the U.S., making our results generalizable.[5] Our paper thus contributes to the growing literature on the impact of social ties on various finance topics (Cohen, Frazzini, and Malloy

[4] We do 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.

[5] While the number of close elections is not large due to the nature of our experiment, our estimates' precision also comes from the number of firms. The number of connected firms and close elections in our sample compares favorably to prior papers (i.e., Faccio 2006, Goldman, Rocholl, and So 2009). Our results remain consistent to all possible levels of clustering, including double-clustering (Cameron, Gelbach, and Miller 2011), and to the exclusion of outliers.

2008; Hochberg, Ljungqvist, and Lu 2007, 2010; Allen and Babus 2009; Fracassi 2009; Kuhnen 2009; Jackson 2009; Engelberg, Gao, and Parsons 2012; Nguyen 2012; Lerner and Malmendier 2013; Shue 2013; and Engelberg, Gao, and Parsons, forthcoming, among others.)

Second, we propose a robust identification to the endogenous relationship between political connections and corporate outcomes. Extant literature studies extensively the value of political connections through events that happen independently of political connections. Knight (2007) and Goldman, Rocholl, and So (2009, 2013) exploit close elections in presidential races in the U.S.; Roberts (1990), Jayachandran (2006), Fisman et al. (2006), and Blanes i Vidal, Draca, and Fons-Rosen (2012) 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 (2011) study political events in Indonesia, Malaysia, Thailand, Nazi Germany, and Taiwan. This strategy avoids the direct reverse causation channel in which political connections result from politicians' power and strong politicians are connected to strong firms' directors, and both become successful. However, as discussed by Snowberg, Wolfers, and Zitzewitz (2007), many caveats persist, notably that of the unobservable 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; it thus restricts the scope and undermines the generalizability of such analysis.

Other studies using non-political firm-related events including appointments of directors (Faccio 2006; Goldman, Rocholl, and So, 2009), bailouts (Faccio, Masulis, and McConnell, 2006), and IPOs (Fan, Wong, and Zhang, 2007) are subject to the endogeneity concern that these events are partly triggered by certain unobservable characteristics of the firms. Other papers relying on fixed effects and difference-in-difference strategies, such as Khwaja and Mian (2005), Dinç (2005), Leuz and Oberholzer-Gee (2006), Bertrand et al. (2008), Claessens, Feijen, and Laeven (2008), and Li et al. (2008), are prone to biases induced by time-varying characteristics of firms or parties. While prior papers undertake various robustness checks to verify the causality channel, few treat the endogeneity of connections. The possibility of unobserved firm and elected politicians' characteristics affecting both corporate outcomes and political connections remains extremely hard to rule out.

The potential problems related to the measurement of political connections and various forms of endogeneity and reverse causality, mentioned above, might explain the inconclusive insights from the prior literature. While a large number of papers find that political connections increase firm value in a specific country (e.g., Fisman 2001 on Indonesia; Johnson and Mitton

2003 on Malaysia; Khwajia and Mian 2005 on Pakistan; Goldman, Rocholl, and So 2009 on the U.S.) or in a cross-country sample (e.g., Faccio 2006 and Faccio, Masulis, and McConnell 2006), other papers do not find any significant impact of political connections on firm value (i.e., Fisman et al. 2006; Roberts 1990).

To the best of our knowledge, our paper is among the first to use the regression discontinuity design of close gubernatorial elections to address the endogeneity and reverse causality of political connections. As detailed in the following section, our framework deals with both the endogeneity of the connected politician and the potential 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. This property reinforces the external validity of the paper's findings, making it reasonable to generalize the conclusions to the population of all firms and politicians.

Our third contribution is the finding of a consistent and positive impact of political connections on firm value in the U.S., particularly at the state level, and the variation of the value of connections across different states, firms, and network characteristics. Our statistically robust and economically significant results complement international evidence in extant literature on value-enhancing political connections (e.g., Fisman 2001, Faccio 2006, Faccio, Masulis, and McConnell 2006) and enriches evidence from the U.S. that focused mainly on the benefits of political connections to parties and at the federal level (e.g. Goldman, Rocholl, and So 2013). In a recent paper, Do et al. (2014) apply a similar method to U.S. Congress elections to show that the value of political connection is higher for state-level politicians, compared to new congressmen.

The remaining paper is organized as follows. Section 2 details the methodology. Section 3 describes the data. Section 4 reports the empirical results. Section 5 explores possible interpretations and channels of the results. Section 6 reports robustness checks. Section 7 concludes.

2. IDENTIFICATION AND EMPIRICAL DESIGN

2.1 REGRESSION DISCONTINUITY DESIGN OF CLOSE ELECTIONS

An estimation of the impact of political connections on firm value should overcome a reverse causation channel when a well-performing firm may be able to help its connected politicians win elections, or an omitted variable bias when connected firms and politicians are

affected by the same unobservable factor, such as a shift in public opinion. The reverse causation and endogeneity bias are best eliminated with a randomization of the assignment of a politician to office. If the politician is chosen randomly, no concern exists about either the reverse causation of firm value changes or the influence of some omitted variables. It is, however, extremely difficult to find a randomized experiment on political connection. Lee's (2008) pioneering work on Regression Discontinuity Designs (RDD) shows that the event of winning close to the vote threshold of 50% is randomized between the winner and the loser as in a randomized experiment, and that, 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. Results from the RDD are also externally valid and generalizable. Lee and Lemieux (2010) point out that the RDD estimate is not only informative for close elections but also for all elections. The estimate can be interpreted as a weighted average treatment effect of being politically connected, where each politician's weight is her ex ante likelihood to be in a close gubernatorial election, which is nontrivial for most American politicians. Even very powerful politicians can be subject to close gubernatorial elections, as Arizona's Janet Napolitano experienced in 2002.[6]

Our identification strategy has a key advantage in comparison with event studies. Traditional event studies rely on the event's exogeneity and the accuracy of the market's prior beliefs, unavailable except in prediction markets (see discussions in Fisman 2001, and Snowberg, Wolfers, and Zitzewitz 2007). In contrast, our design is always valid even if the market's prior belief is largely incorrect. Indeed, suppose that the market believes in a winning probability of 65% instead of the correct probability of 50%. For $100 of perceived value of winning, the pre-event connection will be priced by the market, incorrectly, at $65. The post-event market reaction to a realized win is $35, and that to a realized loss is negative $65. An event study focused on election wins may report the underestimated value of $35.[7] However, RDD

[6] In political science, Snyder (2005), Caughey and Sekhon (2011), and Grimmer et al. (2012) raise the concern of potentially predictable (non-random) sorting of winners and losers in close U.S. House elections after World War II. However, in a thorough and extensive examination of more than 40,000 close elections obtained from a longer period in the U.S. and from other countries, Eggers et al. (2015) provide systemic evidence of no sorting, and claimed that the mentioned concern likely happened by pure chance. Our Tables 10 and 11 provide extensive robustness checks on randomness and RDD required conditions.

[7] A more sophisticated study may report $35/50% = $70, assuming a market's prior belief at 50%. Without knowledge of the market's belief, no event study could estimate the correct value of $100.

estimation still produces, correctly, the difference of \$35-(-\$65) = \$100, exactly the right value of having a connection to an elected politician. (See the appendix and Lee and Lemieux 2010 for more details.)

In addition to the cross-sectional identification by RDD, time-series identification from event-study market models is used to calculate stock price's Cumulated Abnormal Returns (CARs). However, while the use of CARs improves estimation efficiency by reducing market noises, it is not essential to our results, thanks to the near-random nature of RDD assignments.

We design two main econometric specifications to estimate the effect of political connection. Each observation represents a connection between a close-election's top-two candidates and a connected firm's director through a specific university program for a given election year. The dependent variable is a connected firm's cumulated abnormal return (*CAR*) in a window around the election day. We thus combine the strength of event studies with RDD to reduce market noise in stock returns. The treatment variable is an indicator for whether a firm is connected to the winner in a close race.

Following Lee and Lemieux (2010), the first specification that we mainly use is an OLS regression of the outcome variable (*CAR*) on the treatment variable (*Winner*), controlling for the vote shares of elected politicians and defeated politicians, where the sample is limited to all races with a vote margin smaller than 5%. That is, we obtain the OLS estimate $\hat{\beta}$ in the following equation, where VS_i stands for vote share:

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

The treatment variable *Winner* is an indicator equal to one if a firm is connected to the winner and to zero if a firm is connected to the loser. Standard errors are calculated from the OLS regression, and are clustered at the politician level for each election. In our robustness checks, we include a cubic polynomial of the vote shares, as well as other levels of clustering.

We also perform robustness checks using 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 run nonparametric local (cubic) polynomial regressions of the equation:

$$CAR_i = F(VoteShare_i) + \varepsilon_i \quad (2)$$

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 $\hat{F}_+(50\%) - \hat{F}_-(50\%)$.[8]

Our connections based on all pairs of classmates might draw questions about the realistic nature of those connections, as most people actually have only a small number of friends among classmates (see, e.g., Leider et al. 2009). Classmate connection levels, however, 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-friend classmate connections, producing an attenuation bias that reduces the absolute size of the estimate and its statistical significance. The effect of real friendships can thus 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 the conditions for common communication and mutual trust as well as common access to the same social network. Former classmates are thus more likely to later develop a strong connection, even if they were not close friends while in college or graduate school. Several recent papers have shown the strength of this measure of connections in many contexts (Cohen, Frazzini, Malloy 2008; Fracassi 2009; Nguyen 2012; and Shue 2013).

2.2 POTENTIAL ISSUE OF HOMOPHILY

While firms' links to elected governors are identified as an almost-random treatment in our context, the empirical strategy so far still tolerates the potential alternative interpretation of homophily of social networks (people are more likely connected because they share the same characteristics.) This interpretation works as follows. 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 connected firms can profit. Our RDD framework will identify the effect of connection on stock prices, but it comes as a result of homophily via shared policy interests, not from the social network channel. For a concrete example, suppose that a politician and a director are both ardent students of military studies, and graduate from a university with strength in that discipline. The election of the politician then has the potential to affect the director's firm's value through new defense policies, rather than through the social network.

[8] The standard error of this estimate is calculated as the standard error of the difference of two independent variables, $\hat{F}_+(50\%)$ and $\hat{F}_+(50\%)$, as the two subsamples are completely separate from one another. The standard errors for $\hat{F}_+(50\%)$ and $\hat{F}_-(50\%)$ come from nonparametric regressions.

We address this issue with an extensive set of interacted fixed effects to address this issue. First, we use a set of interactions between school fixed effects δ_s, fixed effects of the number of decades since graduation θ_{it} and a dummy $WinSchool_{st}$ equal to one if school s is the alma mater of any winner in the corresponding election, zero otherwise. This specification controls for a school's specific interests passing into policies via the election of another politician graduating from the same school. The additional identification provided by $WinSchool_{st}$ comes from the comparison of former classmates with alumni. The flexibility in δ_s allows different levels of homophily for different schools, and that of θ_{it} allows for time-varying homophily effect over different decades. With θ_{it}, we compare different years within a decade; as we cannot use year fixed effects in this interaction.[9]

Second, we use a set of interactions between industry fixed effects χ_j and a dummy $WinIndustry_{jt}$ equal to one there is any winning politician in an election connected to a firm in that industry, zero otherwise. This specification controls for an industry's receiving certain specific favorable policies thanks to connected winners different from the firm's own connected politician. The identification provided by $WinIndustry_{jt}$ comes from the comparison among firms within an industry connected to some winner(s). The flexibility in χ_j allows different effects on policies for different industries. Furthermore, one may speculate that even within the same industry, large and small firms may benefit differently from enacted favorable policies. To control for that, we further interact with fixed effects of the quintiles of firm size. Finally, we combine both types of controls into a single specification.

In summary, our research design identifies and consistently estimates the WATE of being connected to a candidate in a gubernatorial election, 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 the Compustat database.

3. DATA DESCRIPTION

We construct our sample using data from several sources. First, we collect the gubernatorial election results from the Federal Election Committee (FEC) website. For each

[9] We can even strengthen this specification by replacing $WinSchool_{st}$ with an election year fixed effect γ_t in those interactions. The interactions $\delta_s\gamma_t$ absorb $\delta_s WinSchool_{st}$, and allow for different homophily effects based on, say, the number of alumni winning in that election. We choose to leave these specifications out for simplicity. All results remain very similar, and available upon request.

election, we identify the candidates finishing first (the winner) and second (the loser) and calculate the margin of votes between them. A close election is specified by a margin of votes of less than 5%.

Panel A of Table 1 reports the time series of close gubernatorial elections. The average annual number of gubernatorial elections is 13.08 (with a maximum of 37 and minimum of 2). The average annual number of close gubernatorial elections is 2.92 (with a maximum of 11 and minimum of 0). Out of 157 gubernatorial elections in the U.S. between 1999 and 2010, we identify 35 close ones. No trend appears in the relationship between the number of elections and the number of close elections. The average vote margin across all close elections is 2.70%.

Panel B of Table 1 reports characteristics of our sample of connected firms and compares them to firms in the Compustat universe in the same period. Our sample includes 63 firms per year on average, with a maximum of 263 firms, and a minimum of 1 firm, and represents 1.01% of the total number of listed firms and 2.15% of the total market capitalization in the Compustat dataset. The sample firm's average market capitalization is $2.98 billion and the median is $0.52 billion, which are fairly comparable to average Compustat firms ($2.41 billion and $0.25 billion, respectively). Our average firm has a market-to-book ratio of 3.95 (Tobin's Q of 2.03) and age of 9.90 years, as compared to a market-to-book ratio of 4.79 (Tobin's Q of 2.28) and age of 8.32 years for an average Compustat firm.

[*Insert Table 1 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) *World Almanac of U.S. Politics*, and (iii) *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 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 histories,

educational backgrounds, and professional qualifications. In particular, the data contain 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 both 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 of Table 1 reports the distribution of common educational backgrounds of corporate directors and gubernatorial candidates in our sample. Degrees for undergraduate studies seem to be the most important to the connection of directors and politicians: 69.12% of politicians and 86.94% of directors are connected through their undergraduate studies, having graduated from the same school/university within one year. The figures are 16.18% and 4.81% for law school; 5.88% and 6.02% for business school. Doctoral degrees appear to be insignificant in connecting politicians to directors. Only 2.94% of politicians and 1.37% of directors are connected through Ph.D. programs.

4. EMPIRICAL RESULTS

In this section, we report main empirical results from our RDD framework as well as the results of the impact of political connections on firm value across many sub-samples.

4.1 POLITICAL CONNECTIONS AND FIRM VALUE IN A REGRESSION DISCONTINUITY DESIGN

Table 2 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 winning status of the connected politician. Each observation pairs a firm's director to a candidate in a close gubernatorial election, both of whom graduate from the same university program within a year (Cohen, Frazzini, and Malloy 2008). We calculate CAR for every connected firm during a standard 3-day event period, from day -1 to day +1. The event day (day 0) is the election day reported by the Federal Election Commission, which is always a trading

day. We first follow a conventional event study method to calculate the CARs resulting from close elections by assuming a single-factor model with the beta estimated from the pre-event window, and later use other methods of CAR estimation as robustness checks. 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) window. We exploit the RDD of close elections in which the winning margin is within a 5% vote share. We control for the vote shares separately for winners and losers, as suggested by Lee and Lemieux (2010) and specified in equation (1) in Section 2 above, to obtain the effect at the exact threshold of 50%.

[*Insert Table 2 Here*]

Results from Table 2 show an overall average significant and positive effect of connection to a close election's winner on firm value. Controlling for state and year fixed effects, column 1 reports that firms connected to the winners exhibit CARs which are 3.89% higher than CARs of firms connected to the losers at the 5% winning margin. The estimate coefficient is statistically significant at 1%. Political connections are thus highly valuable for firms at the state level. Column 1's regression will be used as our benchmark regression throughout the paper.

In regressions reported in columns 2 to 5, apart from state and year fixed effects, we control for industry fixed effects, politicians' characteristics (age, gender, election turnout of the politician, party's affiliation), directors' characteristics (age, gender)), and firm characteristics (logarithm of market capitalization, Tobin's Q, return on asset, and leverage), respectively. We obtain positive coefficient estimates of 4.19%, 2.18%, 4.20%, and 3.56%, significant at 1% or 5%, which, except for the result from column 3, are of comparable magnitude and statistical significance to our benchmark estimate in column 1.

While the quality and the timing of pre-election polls are the subject of long-lived debates in political science,[10] if polls contain additional relevant information, then including them should improve the precision of our results. Column 6 repeats our main regression, as in column 1, controlling for poll predictions in all elections. We obtain a coefficient of 3.73%, significant at 1%, on the *Winner* dummy, which is comparable to the estimate of 3.89% in column 1. Thus, the inclusion of poll predictions does not seem to affect our RDD results.

The large variation in the cross-sectional distribution of CARs might introduce outlier-biases to our results. As a check, we exclude all CARs exceeding 10% in absolute value from our

[10] See, e.g., Enns and Richman (2013) on the variability of election polls.

sample. As the results in column 7 show, we obtain an estimate of 2.74%, significant at 1%, which is smaller than the estimate of 3.89% in column 1. The outliers thus wield influence on, but do not significantly change our results.

It is worth noting that regressions in columns 2 to 7 of Panel A, with the exception of column 3, generate comparable results to those in column 1 after controlling for state, election, and industry fixed effects and various factors. This similarity in the magnitude of estimates is expected from the RDD framework in which the main estimate should not be affected by "irrelevant covariates," and RDD can account for all observable and unobservable characteristics. Indeed, when the treatment is comparable to a randomized experiment, any pre-treatment control variable must be independent of the treatment, thus its inclusion should not significantly alter the estimated magnitude of the treatment effect. Therefore, observed and unobservable characteristics of the election year, the industry and the firm are irrelevant covariates and do not alter much our main estimate.

Regressions in columns 1 to 7 show the difference in CARs between firms connected to winners and firms connected to losers. To examine the CARs of these firms separately, we ran separate regressions of CARs on subsamples of firms connected to the winners and of firms connected to the losers against the respective vote shares of the winners and losers. Columns 8 and 9 of Table 2 report the regression intercepts of +1.14% and -1.27%, both significant at 5%. This result shows that firms connected to the winners in close gubernatorial elections experience significant positive gain in value (+1.14%), while firms connected to the losers experience significant loss of value (-1.27%).

In summary, Table 2 provides evidence that firms connected to the winner in a close gubernatorial election between 1999 and 2010, as compared to firms connected to the loser, experience significant gain in firm value. Our RDD results are robust and consistent when we control for politician, director, and firm characteristics; year, state, and industry effects; and poll prediction margin. Our estimated average CAR of 3.89% for our sample of U.S. firms appears to be higher in magnitude than that found by Faccio (2006), who reports an average CAR of 1.43% from a cross-country sample of firms experiencing an event of new political connections. Meanwhile, our estimate is significantly smaller in magnitude than the estimate from Goldman, Rocholl, and So (2009), who report a difference in CARs of 8.97% between Republican-connected and Democrat-connected firms following the 2000 U.S. presidential election.

4.2 CANDIDATE CHARACTERISTICS AND THE VALUE OF POLITICAL CONNECTIONS

The previous section provides evidence of an overall positive impact of network-based political connections on firm value at the state level. In the following sections, we investigate if this effect varies on various subsamples along the lines of candidate, state, firm, and network characteristics. We run the benchmark regression as in column 1 of Table 2 for each of the subsamples and compare the estimates.

Prior literature in political science, economics, and finance has extensively studied whether and how politicians' characteristics such as incumbency and experience (i.e., Lee 2008, Caughey and Sekhon 2011) provide them with advantages to win elections. We thus conjecture that candidate characteristics determine the value of political connections. We test this conjecture by dividing our samples into subsamples of firms, depending on characteristics of politicians. Table 3 summarizes our results.

[*Insert Table 3 Here*]

We first 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. Regressions in columns 1 and 2 show that firms connected to the winner experience significant gain of value, independently of whether the winner is a challenger or an incumbent. Political connections are thus generally valuable. However, the effect appears larger for challengers (4.91%) than for incumbents (2.25%). The difference in coefficient estimates is significant at the 5% level. In column 3, we run a regression on a sub-sample of firms connected to the two candidates in a close election who are both challengers (the incumbent might have retired or moved to another office), we find an estimate coefficient of 4.91%, significant at 1%. This indicates that, in close elections that involve two new candidates, firms connected to the winning challenger experience an increase in firm value of 4.91% in comparison to firms connected to the losing challenger. Connections to the winning challengers are more valuable than to the winning incumbents, and connections to winning challengers in challenger-only close elections are even more valuable for firms.

Challengers with or without political and professional experience at the federal level might contribute differently to firm value. We collect information on the positions that candidates have held up to election and classify two categories of challengers: those whose main occupation in the election year was in a public office at federal level, and those whose main occupation was not. Columns 4 and 5 of Table 3 report the benchmark estimates by the corresponding subsamples that distinguish between firms connected to challengers coming from positions at the federal level (for instance, in a senator's office) and others. We find positive

estimates of 1.03% and 4.93%, both significant at 1%, respectively. The difference in coefficient estimates is significant at the one percent level. The results indicate that the magnitude of the value of political connections is higher for challengers who were not holding federal office and who are mainly from local politics.

In summary, Table 3 shows that our finding—that connections to the winning politician in a close election induce a significant gain in firm value—appears to be consistent and robust across several subsamples of firms connected to candidates with different characteristics. While the impact of political connections on firm value is generally positive and significant, the magnitude seems to be larger for firms connected to challenger candidates in comparison to firms connected to incumbents, for firms connected to winning challengers in challenger-only close elections, and for firms connected to challengers without federal work experience.

4.3 STATE CORRUPTION AND THE VALUE OF POLITICAL CONNECTIONS

The prior literature suggests that state characteristics impact the value of political connections at the state level through local regulation and corruption. For example, Glaeser and Saks (2006) show that the level of corruption varies across the states in the U.S. Providing evidence on the importance of state politics, Eggers and Hainmueller (2013) report that congressmen do not enjoy informational advantage for their own portfolio trade at the federal level, but rather at the local level. They disproportionately invest in local firms and in local firms that contribute to their campaign. States with better checks and balances should thus be associated with lower value of political connections. We test this conjecture and report results in Table 4. To allow state-level institutional quality to vary, regressions in Table 4 include only year fixed effects, not state fixed effects.

[*Insert Table 4 Here*]

While the RDD correctly identifies the value of political connections, ascertaining that the variation in this value across states is caused by the differences in institutional quality is more difficult. Even when we avoid direct reverse causation by using some measures calculated before 2000, the results are still exposed to endogenous selection by unobservables, such as historical or cultural factors, that may affect both institution quality and the value of political connections across states. In columns 1 and 2 of Table 4, we control for this problem by using respectively subsamples of firms above and below the median of the ALD (Average Logarithm of Distance to capital city) 1970 score of the isolation of the state capital, computed from the 1970 census. As reported by Campante and Do (2014), this measure is strongly predictive of state-level

corruption across American states (high ALD score indicates more isolated state capital, which implies lower media coverage of state politics and, therefore, more corruption). This measure is highly persistent over time and, arguably, not directly affected by unobservable determinants of corruption. Results from columns 1 and 2 support our conjecture. The estimated effect is positive (4.66%) and statistically significant at 1% among states with higher-than-median isolation of the capital city, and is positive and insignificant for other states. The difference in coefficient estimates is significant at 5%. Political connections are thus significantly more valuable in a more severely corrupt state.

Columns 3 and 4 report results on an alternative measure of corruption which was also used by Campante and Do (2014). We use the dataset of all newspapers gathered in Newslibrary.com to search for the word "corruption" close to the state name, as is similar in method to Saiz and Simonsohn (2013). We find that the value of political connections is positive and significant in more corrupt states, which are defined as the ones with higher frequency of the use of the word "corruption" in local newspapers, while this value is non-significant in less corrupt ones. The estimate on more corrupt states is 2.90%. The difference in coefficient estimates is significant at 10%.

Columns 5 and 6 divide states by the most commonly used measure of state-level corruption constructed by 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 convicted corruption cases, averaged from 1976 to 2002 to remove periodical noises. Results also support our intuition: political connections are more valuable in more corrupt states than in less corrupt states. The effect is clearly stronger in magnitude and statistically significant (4.07%, significant at 1%) in more corrupt states, and positive but insignificant in less corrupt states. The difference in coefficient estimates is highly significant at the 1% level.

In sum, Table 4 provides strong and consistent evidence that the value of political connections varies as a function of state corruption. The estimated value of political connection is larger in more corrupt states.

4.4 Firm characteristics and the value of political connections

Prior literature has explored extensively how politics and political connections impact corporations. For example, Khwaja and Mian (2005) study how political connections impact corporate access to finance; Faccio, Masulis, and McConnell (2006) show that politically

connected firms are more likely to receive financial bailout; and more recently, Cohen, Coval, and Malloy (2011) show that changes in important congressional committee chairmanships reduce investments of firms in the states of the congressmen. Following the literature, we study firm characteristics as potential determinants of the value of political connections, and detail results in Table 5.

[*Insert Table 5 Here*]

We first run our standard regression on two subsamples of firms whose market capitalization is respectively above or below the median in our sample. Results reported in columns 1 and 2 show coefficient estimates of 5.58%, significant at 1%, and of 1.33%, insignificant, for small and large firms, respectively. The difference in these two coefficient estimates is significant at the ten percent level. Politically connected firms that are smaller thus experience significantly greater gain of value in comparison to 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, the effect should be smaller.

An important potential benefit of political connections is easier access to finance, as shown by Khwaja and Mian (2005). We test this conjecture by investigating whether the value of political connection is associated with a firm's dependence on external finance. We construct Rajan and Zingales's (1998) measure of dependence on external finance as the industry average of (CapEx – Cashflow from Operations)/CapEx, using Fama-French 48-industry classification, then divide our sample into subsamples with above and below industry median scores. Columns 3 and 4 of Table 5 report our standard regression results on these two sub-samples. Connected firms relying more on external finance exhibit a coefficient on the *Winner* dummy of 5.33% and are significant at 1%; in contrast, for connected firms that are less dependent on external finance, the estimated effect is positive but insignificant at conventional levels. The difference in these two coefficient estimates is significant at the five percent level. Thus, firms that are financially independent seem not to be affected after election results. Meanwhile, the value of political connections is greater for financially dependent firms.

Our results show that social-network based political connections significantly enhance a firm's market value. One might ask whether investors and markets are aware of these connections. We note that our framework does not require that all investors know about the connections. Reactions from a limited number of informed investors may suffice to drive our results. In search of further evidence of the interest from investors on close elections, we divide

our sample into two subsamples of firms with below and above median abnormal trading activities around the election day, following Campbell and Wasley's (1996) approach, and re-run our benchmark regression. Results are reported in columns 5 and 6 of Table 5. In terms of CARs, among firms with a high level of abnormal trading activities, the ones connected to winners outperform the ones connected to losers by 5.51% surrounding the election window. The impact is insignificant among firms with a low level of abnormal trading activities. The difference in the two coefficient estimates among the two groups of firms is significant at the five percent level, indicating that at least a number of investors follow up with the election, react, and trade on the election results.

In summary, the examination of firm characteristics, as shown in Table 5, provides further evidence that certain firms benefit from political connections more than others.

4.5 NETWORK CHARACTERISTICS AND THE VALUE OF POLITICAL CONNECTIONS

The main objective of our paper is to identify the value of political connections through the networks of politicians and directors. We construct our proxies for social connections based on the educational ties. A growing literature in finance shows that social networks impact various financial decisions such as investment decisions and returns (Cohen, Frazzini, and Malloy 2008), venture capital funds' investment performance and competition (Hochberg, Ljungqvist, and Lu 2007, 2010), firm policies (Fracassi 2009, Shue 2013), firm financing (Engelberg, Gao, and Parsons 2012), board effectiveness (Nguyen 2012), and entrepreneurship activities (Lerner and Malmendier 2013). We thus investigate the impact of network characteristics such as distance (strength), size, and recent network interactions, and report results in Table 6.

[*Insert Table 6 Here*]

The strength of social networks is defined by how close the distance is between members. Our definition of the connection between directors and politicians based on educational backgrounds is stringent. We require directors and politicians to have graduated from the same school on the same university campus and within a year of difference. If network strength matters, we should observe that the value of political connections will be reduced when the connection is less close, i.e., when we loosen our definition of political connections. We note that when the definition of connection is loosened, the sample size is increasing as the networks include more members.

Columns 1 to 8 in Panel A of Table 6 report results of our benchmark regression on subsamples of alumni who graduate from the same university program in the same year, and

within one to five years, ten years, and twenty years of difference. Column 1 shows the strongest impact of social networks when the networks are the closest, having directors and politicians who graduate from the same school, at the same university campus, and in the same year. The coefficient estimate on the *Winner* dummy is 6.30%, significant at the one percent level. When we require that directors and politicians graduate from the same school, at the same university campus, but within one year of difference, as in our benchmark regression, we have a smaller coefficient estimate of 3.89%, significant at the one percent level. In column 3, when we require that directors and politicians graduate from the same school, at the same university campus, but within two years of difference, the coefficient estimate on the *Winner* dummy is further reduced to 1.97%, still significant at 5%. In columns 4 to 8, as expected, the coefficient estimates on the *Winner* dummy become insignificant when networks are extended to less close alumni networks.

To investigate the impact of network size on the value of networks, we sort the educational institutions by the number of observations in the sample, enabling us to look at the number of prominent graduates who 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 shares 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 provides better incentives for information sharing.

In the context of the U.S., Ivy League universities dominate networks in our sample, providing more directors and politicians than any other universities. They are thus big networks. Columns 1 and 2 in Panel B of Table 6 report the coefficient estimates of 0.54%, significant at 1%, and of 4.43%, significant at 5%, on subsamples of Ivy League and non-Ivy League connected firms, respectively. The difference in coefficient estimates is significant at 5%. Political connections thus appear to be more valuable among firms in non-Ivy League (small) networks than in Ivy League (big) networks.

Columns 3 and 4 in Panel B of Table 6 report estimates of 2.14% and 4.43%, significant at 1% and 5%, for subsamples of connections that are above (large network) and below (small network) the median number of observations, respectively. However, the difference is not statistically significant at conventional levels. The finding that small network links are more valuable, according to Karlan et al.'s (2009) theory, implies that the links bring value by providing trust and commitment in profitable agreements, rather than just sharing information.

Network ongoing and recent interactions have been reported to impact network value. Shue (2013) shows that social networks impact firm policies and that the impact is significantly stronger after alumni reunions. We empirically test this idea by running our benchmark regression on subsamples of firms with directors and connected politicians whose last alumni reunions were held in the election year and were not held in the election year. Results from columns 5 and 6 show that the value of political connections is much higher when an alumni reunion falls in the year of an election. The coefficient estimates are 3.49% and 0.91%, both significant at 1%, respectively for the two subsamples. The difference in coefficient estimates is significant at 1%.

In summary, results from Table 6 show that network characteristics such as distance (strength), size, and recent interactions impact the value of political connections. Political connections are more valuable when the networks are closer, stronger, smaller, and active.

5. CHANNELS AND INTERPRETATIONS OF THE RESULTS

Our paper attempts to identify the value of political connections, to study their potential determinants, and to investigate whether political connections change firm behavior. While we cannot provide an exhaustive list of potential channels that explain the value of political connections, we will discuss a few prominent channels suggested by the literature and provide further evidence relating to firm operation and headquarters location, corporate investment decisions, and government procurement contracts.

5.1 THE VALUE OF POLITICAL CONNECTIONS: POTENTIAL CHANNELS OF INFLUENCE

The prior literature provides several channels and mechanisms that make political connections valuable to firms, with each paper focusing on one specific channel. For example, Faccio, Masulis, and McConnell (2006) provide evidence that politically connected firms obtain preferential financing in that they are more likely to receive financial bailouts. A low or free cost of financing will certainly reduce a firm's cost of capital and thus increase firm value. Goldman, Rocholl, and So (2013) show that S&P500 firms that are politically connected to the winning party in a House, Senate, or presidential election are more likely to experience an increase in government procurement contracts.

We have indirectly investigated several channels that might explain our finding of the significant and positive value of political connections. Columns 3 and 4 of Table 5 show that the value of political connections is greater for firms more dependent on external financing (leverage). As is consistent with evidence from the literature, political connections facilitate

corporate financing and increase firm value (e.g., Faccio, Masulis, and McConnell 2006; Fan, Wong, and Zhang 2007).

State-level corruption seems to be another major channel. Columns 1 to 6 of Table 4 show that, using different proxies for corruption, the value of connections is positively related to the level of state corruption. This evidence is consistent with the widely held view in political science that state politics in the U.S. is more corrupt, thus more valuable to connected firms at the state level than at the federal level (e.g. Glaeser and Saks 2006, Eggers and Hainmueller 2013, and Campante and Do 2014). Our result hints that political connections might also create value to connected firms through corrupt means. Local politicians might have more leeway, for example, in handing local contracts to connected firms.

From a social network perspective, political connections might be valuable because of the trust-building and information-sharing roles of networks. This potential channel is corroborated by findings in columns 1 to 8 in Panel A, and in columns 1 and 2 in Panel B of Table 6, that stronger and closer (smaller) networks beget higher value. These results can be interpreted by Karlan et al.'s (2009) theory that the connections bring value in fostering trust and commitment in profitable deals, rather than simply sharing information.

Another potential channel is through trading activities. Columns 5 and 6 from Table 5 show that among connected firms that experience abnormal trading activities surrounding close elections, firms connected to the winner are associated with significantly higher CARs in comparison to firms connected to the loser. This indicates that a number of investors do pay attention to the election outcomes and trade on connected firms.

5.2 Further investigation of the channels: Corporate operations and headquarters in the election state

One might still be skeptical about why stock prices of a connected firm in one state are affected by the election outcome of a connected politician in another, faraway state. Why should these politicians care about currying favors to firms that operate outside their states?

The most direct way to address this relevant question is to look at each connected firm's operations, measured for instance by sales or employment, in the state of the connected politician. Unfortunately these detailed corporate operations by state are not readily available. We surmount this difficulty by providing a new measure of firm activities by state and year. We follow Saiz and Simonsohn (2013) and Campante and Do (2014) by searching each company's name through all local newspapers in the connected politician's state within each year, using

Newslibrary.com, and normalize the number of search hits on firms by the search hits for the neutral keyword "September." The resulting hit rate is used as a proxy of a firm's activities within a state one year before an election. We run our benchmark RDD regression as in column 1 of Table 2 on the subsample of connected firms that exhibit some state-level activities, i.e., when the hit rate is positive. Results are reported in column 1 of Table 7. We find that, among firms having some state-level activities prior to the election, firms connected to the winner in a close gubernatorial election enjoy positive and significant stock price reaction over firms connected to the loser. The coefficient estimates of the *Winner* dummy are 3.33%, significant at 1%.[11] Political connections are thus valuable for connected firms having activities in the election states (measured by the volume of hits in the news in the connected politician's state).

[*Insert Table 7 Here*]

In a recent paper, Garcia and Norli (2012) propose another proxy for state-level corporate activities. Their measure is the number of times that a state's name is cited in the SEC's 10-K forms. As they show, this proxy is robust in predicting that investment in truly local companies outperforms investment in less local firms. Using Garcia and Norli (2012) data, we construct our state presence indicator that marks all firm-state pairs where the firm has reported that state in its 10-K forms. We then augment our measure with OneSource data, from which we could identify the economic group of each firm and the state to which the entities in the firm's economic group reside. We run our benchmark regressions on the subsample of firms which have some presence in the election states (i.e., firms with a state presence indicator equal to one). As reported in column 2 of Table 7, we find a coefficient estimate of 4.70%, significant at 10%. This result indicates that among firms that have operations in the election states, the value of political connections is significantly larger for firms that are connected to the winner.

All else being equal, we expect that connected firms that are headquartered in the election states should be more directly impacted by the election outcome. We thus run our benchmark regression on a subsample of connected firms that are headquartered in the election states. Since Heider and Ljungqvist (2014) find that Compustat created data bias when backfilling firm headquarters states to actual historic headquarters states for the 1989-2011 period, we use their method to correct for Compustat's states of firm headquarters in our sample. Moreover, we also manually search for 8 firms whose information on headquarters is missing from Compustat.

[11] In robustness checks that produce consistent (not reported) results, we refine this proxy by excluding common corporate names such as Apple and normalize by the number of citations in the *New York Times*.

We therefore obtain complete information on headquarters for all connected firms in our sample. Column 3 of Table 7 reports our result. The estimated coefficient of the *Winner* dummy is 2.81%, highly significant at the one percent level. Among firms that have headquarters in the election states, the value of firms connected to the winner in a close gubernatorial election significantly increases post-election, in comparison to firms connected to the loser. We however caution against strong interpretations of this result because the subsample of connected firms headquartered in the election states includes only 30 observations, and because corporate headquarters may not always be the place where firms conduct most of their activities.

In sum, results from Table 7 strengthen our findings from Tables 2 to 6 that political connections significantly increase firm value. Our novel measure of search hits on local newspapers and Garcia and Norli's (2012) state presence measure appear to be robust proxies for corporate operations across states.

5.3 POLITICAL CONNECTIONS AND CORPORATE OUTCOMES

A potential alternative channel that explains the value of political connections relates to the conduct of business: political connections assist connected firms, for example, in facilitating large investments (perhaps through outright financial help or cheap financing) or in obtaining more and larger state and federal procurement contracts. Although we cannot comprehensively provide evidence on every aspect of connected firms, we test this conjecture on a few important observables such as investment and state and federal procurement contracts.

We use annual growth in corporate investments as the dependent variable in our benchmark RDD specification instead of CARs. Corporate investments in a given year are measured as the sum of a firm's capital expenditure and research and development expenditure normalized by start-of-the-year total assets. Change in corporate investment in year N is defined as the difference in logarithm of corporate investment in year N versus year N-1.

Panel A of Table 8 shows the results with windows from one year before to three years after the election year. Columns 1 and 2 report that in the year before and in the election year, firms connected to winners show no difference in terms of investments, compared with firms connected to losers. This finding reaffirms the random assignment in our RD design.

[*Insert Table 8 Here*]

One year after the election, as column 3 shows, no significant change in corporate investments exists. Column 4 shows that two years after the election, firms connected to the winners invest 40.70% more than do firms connected to the losers. The effect is significant at

5%. Three years after the election, as column 5 shows, the effect remains positive, but insignificant. This finding of the positive impact of political connections on corporate investment is consistent with Cohen, Coval, and Malloy (2011), who show that powerful politicians create shocks in states' public expenditures which impacts state-level corporate capital spending.

We next investigate another potentially important channel of political influence, that is, whether connected firms are favored to win government's contracts.[12] We obtain connected firms' state-level procurement data from the Federal Procurement Data System. We use proxies for changes in state and federal procurements as the dependent variable in our benchmark RDD specification instead of CARs. Columns 1 and 2 in Panel B of Table 8 show results using 2-year before against 2-year after change in the logarithm of the dollar value of connected firms' state and federal procurement contracts and assistance, respectively. The coefficient estimate on the *Winner* dummy in column 1 is 1.86, significant at 1%. This indicates that, in comparison to firms connected to the loser, firms connected to the winner in a close gubernatorial election experience an increase of 186% in the logarithm of the dollar value of the state procurement contracts in the period of two years after election relative to the period of two years before election. However, one should caution against the small sample size of firms with state-level contracts and assistance in column 1, as the result might be sensitive to a few very large contracts. By contrast, we do not find any significant difference in the value of federal procurement contracts between firms connected to losers and firms connected to winners. The estimate in column 2 is statistically insignificant, with large standard errors despite a more sizeable sample.

Columns 3 to 4 report the effects of connections on a firm's likelihood to receive procurement contracts in the period after the election for the subsamples of firms headquartered in the election states and for other firms, respectively. Column 4 shows that, among firms headquartered in the election states, it is significantly more likely for firms connected to the winner to obtain state procurement contracts after the election. Meanwhile, among firms not headquartered in the election states, the effect is not significant. The difference in coefficient estimates between columns 3 and 4 is statistically insignificant.

[12] Goldman, Rocholl, and So (2013) document that S&P500 firms connected to the winning party in certain House, Senate, or presidential elections are likely to receive more government procurement contracts.

In sum, results from Table 8 provide further evidence that political connections affect corporate investing activities. Connected firms to the winners are also more likely to obtain state procurement contracts, and if they do, they enjoy much bigger contracts.

6. NETWORK HOMOPHILY, FALSIFICATION TESTS, AND ROBUSTNESS CHECKS

In this section, we investigate whether network homophily impacts our results; conduct various robustness checks, including nonparametric and placebo tests; and perform sensitivity checks that are testable in the RDD framework.

6.1 NETWORK HOMOPHILY

Our empirical tests show a significant and positive impact of political connections on firm value. However, as with any study on social networks, network homophily remains a potential confounding factor. In our context, a homophily bias may arise from the possibility that politicians and directors sharing the same characteristics are more likely to join the same school, and later respectively enact and benefit from similar policies, thus confounding the effect of connections through education links with the effect of shared characteristics.

As detailed in section 2.2, we use an extensive set of interactions of fixed effects to control for potential homophily bias. On top of identification by RDD, the additional identification comes from both (i) comparison between former classmates and alumni, allowing for flexible homophily effects across different schools and over time, and (ii) comparison between firms in the same winning industry, allowing for flexible homophily effects across different industries. We detail our results in Table 9.

[Insert Table 9 Here]

Column 1 reports the result from our first additional identification strategy that controls for favorable policies coming from other winners from the same school. We find a coefficient estimate of 2.64%, significant at 1%, on the *Winner* dummy. Column 2 shows the result from the second additional strategy that controls for favorable policies enacted for the same connected industry. The coefficient estimate on the *Winner* dummy is 4.10%, significant at 5%. When we further interact this set of controls with a set of fixed effects for quintiles of firm size, the estimate becomes 3.19%, significant at 1%, as reported in column 3. Finally, column 4 reports the specification that combines both strategies. The estimate is 2.53%, significant at 5%.

The estimates in Table 9 are all positive and strongly significant, and not substantially smaller than the benchmark results shown in Table 2. This similarity indicates reassuringly that our analysis and conclusions are robust to concerns of potential network homophily bias.

6.2 NONPARAMETRIC AND PLACEBO TESTS

We first perform the nonparametric tests specified in Section 2, and report the result in column 1 of Panel A in Table 10. The estimated effect is 2.27%, significant at 5%.[13]

[Insert Table 10 Here]

Our RDD relies on the vote share threshold of 50%. Lee and Lemieux (2010) suggest that RDD is falsifiable if we use "placebo" thresholds, i.e., those at which no jump in the treatment exists. In columns 2 to 5, we apply this falsification test with placebo vote share cutoffs different from 50%. For example, in the sample used for column 2, a politician is marked as hypothetically elected if his vote share is 48% or above, and marked as hypothetically defeated otherwise. We then apply the nonparametric regression around the placebo cutoff of 48% and report the corresponding estimate. Because this threshold is only hypothetical, we do not expect to find results similar to those in column 1. Columns 2 to 5 confirm our prediction: for the placebo thresholds of 48%, 49%, 51%, and 52%, the estimates are small and statistically insignificant. It is thus reassuring that hypothetical cutoffs cannot replicate the significant result with the real cutoff of 50%, as shown in column 1.

Figure 1 visualizes the outcome variable, CAR(-1,+1), against vote shares, plotted in bins by vote shares (Lee and Lemieux 2010), and with markers of bins above and below the 50% cutoff. We plot nonparametric estimates of CAR(-1,+1) as functions of vote share, where each half of the graph represents the estimated function for vote shares greater or less than 50% (i.e., for elected or defeated politicians, respectively). The bands represent confidence intervals at 95%.

[*Insert Figures 1 Here*]

We see a large gap at exactly the 50% threshold. Furthermore, visual evidence of an inverted "Z" shape of CAR with respect to vote share can be seen: as vote share increases around 50%, and as CAR first decreases, then jumps sharply at the threshold of 50%, and then decreases again. According to Cuñat, Gine, and Guadalupe's (2012) event-based explanation, this

[13] We also rerun all analyses in the paper using the nonparametric approach, which produce very similar qualitative conclusions. For simplicity, we choose to present the simpler implementation of RDD.

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%.

However, Cuñat, Gine, and Guadalupe's Z shape depends on the demanding 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 consistent with the further increase at around 52% vote share in Figure 1. Such increase is likely due to cross-sectional heterogeneities, coming from innate characteristics of elections with results at the level of 52%-48%. In traditional event studies, such heterogeneities could only be dealt with by controlling for observable characteristics. In contrast, RDD identification holds on the cross-section of stocks and thus does not require the strong hypothesis of no confounding factors.

The RDD results can be sensitive to the choice of nonparametric specification, most importantly in terms of the bandwidth chosen in the nonparametric regression (Imbens and Kalyanaraman 2012). We choose a prudent approach in examining a wide range of bandwidths in our nonparametric estimation procedure. The results are shown in Figure 2.

[*Insert Figure 2 Here*]

The estimated effect remains stable, and always significant at 5%, across all choices of bandwidth. For our benchmark choice of 0.05, the effect is 2.27%. For bandwidths smaller than 0.05, the estimate becomes noisier but also much stronger. In sum, Figure 2 clearly confirms the result that firms make significant gains in value following the elections of their connected governors.

6.3 ALTERNATIVE SPECIFICATIONS OF THE EVENT STUDY

The focus of our analysis is on the three-day event window, from -1 to +1. As this event window, specification is simply one among several possibilities; we also consider our main specification using two alternative windows (-2 to +2), and (0 to +2). Both cases produce sensibly similar results, which are available upon request.

In our paper, cumulated abnormal returns are estimated based on the one-factor market model around the election day (Day 0). The market model is estimated using daily data over a 255-day (-315,-61) window. As a further check, we calculate the CARs using different methods, including the cumulative daily stock (raw) returns, Fama-French's three-factor model (Fama and French 1993), and the four-factor model (Carhart 1997). We then use these CARs in our RDD

regressions, as in Table 2. We find estimates mostly similar to those reported in Table 2. Results are reported in columns 1 to 6 in Panel B of Table 10.

Throughout our analysis, we choose to keep the unit of observation at the most fundamental level: each observation represents a connection between a firm and a candidate's election. Columns 7 to 9 in Panel B of Table 10 examine other levels of observation, where the data are respectively aggregated by politician-year, director-year, and firm-year. Coefficient estimates are almost identical to the benchmark results.

6.4 CHECKS OF RANDOMNESS

Lee and Lemieux (2010) emphasizes RDD's advantages in that one can check the near-randomness of winning or losing a close election by applying the benchmark specification on all pre-election variables to verify that they do not exhibit any discontinuity at the threshold. We run those regressions on pre-election variables of firm-candidate connections from close U.S. gubernatorial elections between 1999 and 2010, and report supporting results in Table 11.

[*Insert Table 11 Here*]

Panel A of Table 11 reports regressions of politicians' characteristics, such as age, gender, logarithm of the vote turnout, incumbency, poll win margin, party affiliation, and federal work experience on the *Winner* dummy. Panel B examines director characteristics such as age, gender, and size of director network. Panel C considers state characteristics used in previous tables, such as ALD 1970 score (isolation of the state capital, Campante and Do 2014), corruption news (Saiz and Simonsohn 2013, Campante and Do 2014), and corruption conviction rate (Glaeser and Saks 2006). Panel D exhibits results of different firm characteristics we have used in Tables 1 to 5, such as firm size, leverage, and Tobin's Q. Almost none of the reported regressions provides a significant coefficient of the *Winner* dummy, consistent with the assumption that the considered variables do not exhibit discontinuities around the vote share threshold.

In summary, our randomness robustness checks show that our results 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.

7. CONCLUSIONS

Our paper studies the impact of the network of politicians and directors on firm value. We use the Regression Discontinuity Design (RDD) to identify the value of connections to a

politician elected to be state's governor in the U.S. in a closely contested race. The estimate of the weighted average treatment effect (WATE) during the period 1999 to 2010 shows an average positive and significant cumulative abnormal return of 3.89% surrounding the election date. The results are robust to homophily concerns, various parametric and nonparametric specifications, to different measures of outcome variables, to different definitions of social network, and across many subsamples.

We also find that political connections are more valuable for firms connected to winning challengers and challengers not from federal offices, as well as for firms in more corrupt states, firms in states of connected firms' headquarters and with corporate operations, in smaller firms, and in firms dependent on external finance. Political connections are more valuable when the network is closer, stronger, smaller, and active. After elections, firms connected to the winners receive significantly more state procurement contracts and invest significantly more than do firms connected to the losers.

Our potential contributions to the finance literature are threefold. First, we propose a new approach to measure political connections based on social networks of politicians and directors of listed firms. This broad and representative measure of political connections results in a relatively sizable sample of U.S. firms. Any potential measurement errors represent an attenuation bias that reduces the absolute size and statistical significance of the estimate.

Second, we propose a robust solution to the identification problem. Our framework deals adequately with both the endogeneity of the connected politician and the selection bias due to network homophily, providing a powerful internal validity of the empirical results. Moreover, the estimated effect is a WATE across the sample of all politicians subject to a close election, and across sampled firms, which are comparable to Compustat's universe. Our results are thus externally valid and generalizable to the population of all firms and politicians.

Third, we find a consistent, positive, economically meaningful impact of political connections on firm value in the U.S., particularly at the state level. This result complements international evidence in extant literature, and enriches evidence from the U.S. that focuses mainly on the benefits of political connections to parties and politicians at the federal level.

Overall, our study identifies the value of social network-based political connections in the United States and uncovers its variation across different states, firms, and network characteristics.

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ECONOMETRIC APPENDIX

As shown by Lee and Lemieux (2010), suppose that the cumulative abnormal returns averaged over firms connected to a candidate *i*, CAR_i, is a function of the treatment variable, namely win/lose status, all observable characteristics W_i as well as unobservables U_i. The vote share of each candidate is also a function of W_i and unobservables V_i (while we assume linearity for simplicity, the results are much more general):

$$CAR_i = Winner_i\beta + W_i\gamma + U_i,$$
$$VoteShare_i = W_i\delta + V_i.$$

Assume that conditional on W and U, the density of V is continuous. This assumption amounts to saying that each candidate cannot fully determine the exact vote share (partial influence on vote share is still allowed). Therefore, $f_{VoteShare|W,U}(x|W,U)$, the probability density of vote share conditional on W and U, is continuous. Then the joint distribution of W and U conditional on vote share is also continuous in vote share, as:

$$\Pr[W = w, U = u|VoteShare = x] = f_{VoteShare|W,U}(x|W,U)\frac{\Pr[W = w, U = u]}{f_{VoteShare}(x)}$$

Because of this continuity, all observed and unobserved predetermined characteristics will have identical distributions on either side of the threshold, $VoteShare = 50\%$:

$$\lim_{x\downarrow 50\%} \Pr[W = w, U = u|VoteShare = x] = \lim_{x\uparrow 50\%} \Pr[W = w, U = u|VoteShare = x]$$

We can thus define and estimate the treatment effect as:

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

The effect can be estimated by approximating CAR_i from both sides of the 50% threshold of vote share. This procedure controls for both observable and unobservable characteristics, using the observed vote share, not the vote share predicted by polls or markets (the observable part.)

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

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

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

Figure 1

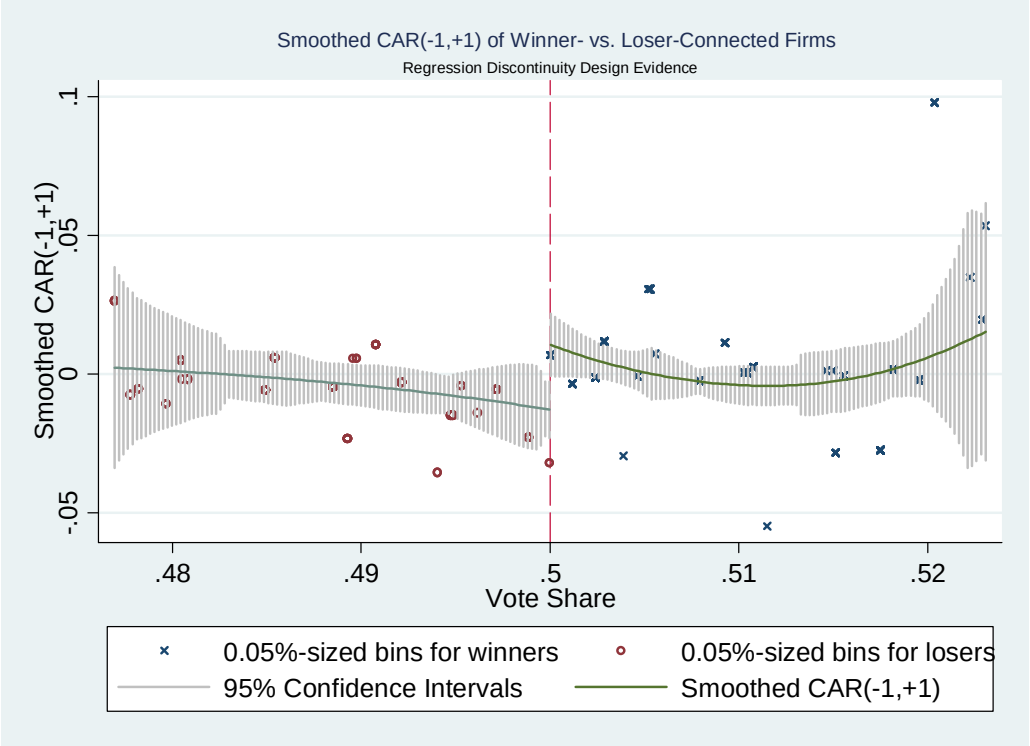

Figure 2

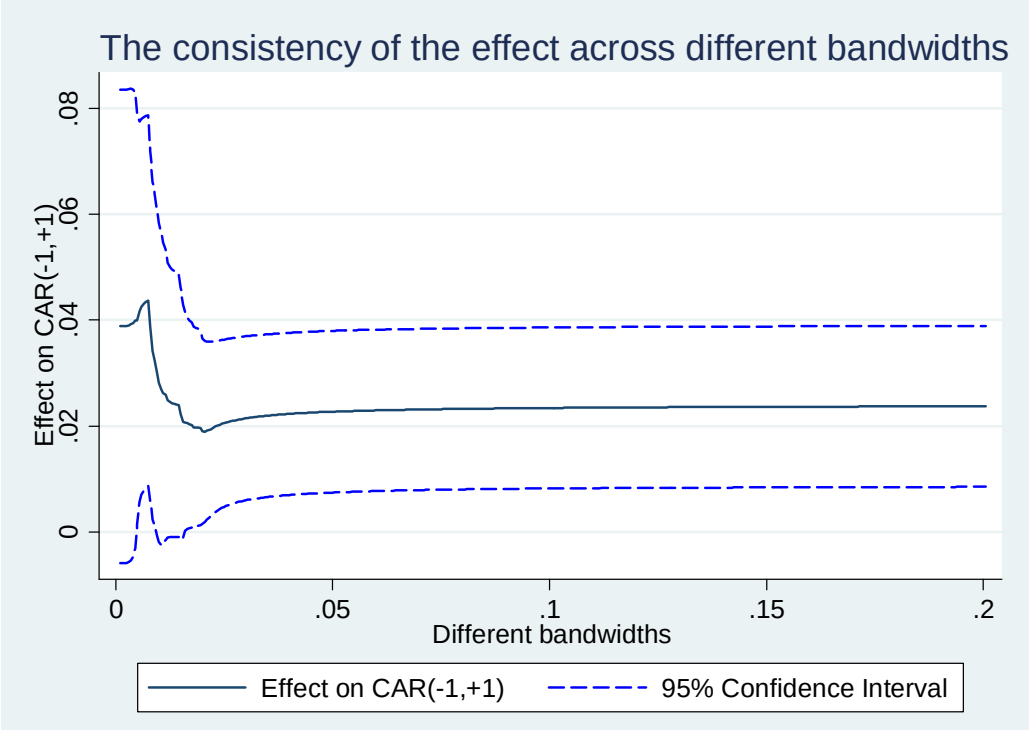

Table 1: Descriptive Statistics

This table reports descriptive statistics of our sample. Panel A shows the details and distribution of gubernatorial and close gubernatorial elections at 5% vote margin by election year. Panel B shows descriptive statistics of politically connected firms in our sample and of firms in the Compustat database. A firm is defined as politically connected if one of its directors and a close election's candidate graduate from the same university program within a year. *Market Cap* is the firm's market capitalization (in millions) measured at the fiscal year end. *Common Equity* is the firm's book value of equity (in millions). *Market to Book Ratio* is the ratio of the firm's market value of equity to book value of equity. *Capital Expenditure* is the firm's capital expenditure (in millions). *Age* is the firm's age. *Leverage* is the ratio of total debt to total assets. *Tobin's Q* is the ratio of the sum of book value of total assets and market value of equity less book value of stockholders' common equity to total assets. *Payout* is the sum of dividends paid and shares repurchased (in millions). *Tangibility* is the ratio of net property, plant, and equipment to total assets. *ROA* is the ratio of operating income before depreciation to start-of-period total assets. *RND* is the ratio of research and development expenditure to start-of-period total assets. *Cash Reserve Ratio* is the ratio of the firm's cash and short-term investments to total assets. Panel C shows the distribution of degrees of connected politicians and directors in our sample.

Panel A. Distribution of Gubernatorial Elections

Election Year	Number of Elections	Number of Close Elections	Proportion of Close Election	Average Margin of Close Election
1999	3	1	0.333	0.011
2000	11	3	0.273	0.026
2001	2	0	0.000	-
2002	36	11	0.306	0.280
2003	3	1	0.333	0.039
2004	11	4	0.364	0.024
2005	2	0	0.000	-
2006	36	3	0.083	0.025
2007	3	0	0.000	-
2008	11	1	0.091	0.035
2009	2	1	0.500	0.038
2010	37	10	0.270	0.190
Total	157	35	-	-
Mean	13.083	2.917	0.213	0.027
Median	7	1	0.271	0.026
Min	2	0	0.000	0.011
Max	37	11	0.500	0.390

Panel B. Firm Characteristics

	Sample			Compustat Universe		
	Mean	Median	StD	Mean	Median	StD
Market Cap (in $million)	2,980.630	515.026	16,433.430	2,411.790	252.717	11,123.870
Common Equity (in $million)	1,170.820	201.170	4,687.690	1,039.540	120.486	5,554.220
Market to Book Ratio	3.949	2.186	8.553	4.789	1.958	278.285
Capital Expenditure (in $million)	113.184	11.171	579.865	147.541	6.655	951.273
Age	9.902	9.367	6.324	8.316	7.167	6.496
Leverage	0.260	0.224	0.255	0.274	0.207	0.272
Tobin's Q	2.029	1.469	1.673	2.279	1.414	3.344
Payout (in $million)	77.105	1.519	248.871	76.197	0.223	463.509
Tangibility	0.186	0.106	0.207	0.221	0.129	0.231
ROA	-0.018	0.026	0.226	-0.063	0.015	0.483
RND	0.130	0.061	0.172	0.124	0.068	0.203
Cash Reserve Ratio	0.273	0.180	0.264	0.245	0.145	0.254
Number of firms per year	63	24	95	3,002	2,939	476

Panel C. Distribution of Degrees of Connected Politicians and Directors

Degree	Politicians (%)	Directors (%)
Business School	5.88	6.02
Graduate	5.88	0.86
PhD	2.94	1.37
Law School	16.18	4.81
Undergraduate	69.12	86.94
Total (%)	100.00	100.00

Table 2: Political Connections and Firm Value in a Regression Discontinuity Design

This table reports OLS regressions of the cumulative abnormal returns (CARs) among the politically connected firms around close gubernatorial elections in the U.S. between 1999 and 2010. A firm is defined as politically connected if one of its directors and a close election's candidate graduate from the same university program within a year. Each observation pairs a connected firm to the connected candidate's close election. 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) window. *Winner* is a dummy variable equal to one (zero) if a politician wins (loses) a close gubernatorial election. A close election is specified by the margin of votes between the winner and the loser being less than 5%. All regressions control for the quadratic polynomials of vote share of winners and vote share of losers, as prescribed by the regression discontinuity design, and state and year fixed effects. Column (1) presents results on the standard model with the winning margin of 5%. Column (2) controls for industry fixed effects. Column (3) controls for characteristics of politicians (age, gender, election turnout, and party affiliation). Column (4) controls for director characteristics (age, gender, and inside/independent directorship). Column (5) controls for firm characteristics (logarithm of market capitalization, Tobin's Q, return on asset, and leverage). Column (6) controls for poll prediction. Column (7) excludes observations with CAR larger than 10%. Columns (8) and (9) respectively report the results on the subsamples of winners and losers.

	Dependent Variables: CAR (-1,1)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Subsample	5% Margin	5% Margin	5% Margin	5% Margin	5% Margin	5% Margin	Outliers Excluded	Winners	Losers
Winner	0.0389 [0.00833]***	0.0419 [0.0103]***	0.0218 [0.00912]**	0.0420 [0.0110]***	0.0356 [0.00878]***	0.0373 [0.0102]***	0.0274 [0.00511]***		
Intercept								0.0114 [0.0056]**	-0.0127 [0.0061]**
Vote Share (Winners) & Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	State and Year FE	State, Year and Industry FE	State, Year and Politicians' Characteristics	State, Year and Director Characteristics	State, Year and Firm Characteristics	State, Year and Poll Prediction	State and Year FE		
R-squared	0.045	0.140	0.064	0.103	0.054	0.045	0.115	0.006	0.007
Observations	586	586	586	579	507	532	547	357	229

Table 3: Candidate Characteristics and the Value of Political Connections

This table reports OLS regressions of the cumulative abnormal returns (CARs) among the politically connected firms around close gubernatorial elections in the U.S. between 1999 and 2010. A firm is defined as politically connected if one of its directors and a close election's candidate graduate from the same university program within a year. Each observation pairs a connected firm to the connected candidate's close election. 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) window. *Winner* is a dummy variable equal to one (zero) if a politician wins (loses) a close gubernatorial election. A close election is specified by the margin of votes between the winner and the loser being less than 5%. All regressions control for the quadratic polynomials of the vote share of winners and vote share of losers, as prescribed by the RDD, and state and year fixed effects. Columns (1) to (5) respectively show results on the subsamples of incumbents, challengers, challenger-only close elections, and challengers from, and not from federal offices. Standard errors in square brackets are corrected for clustering by state. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

	Dependent Variables: CAR (-1,1)				
	(1)	(2)	(3)	(4)	(5)
Subsample	Incumbents	Challengers	Challenger-only Elections	Challenger from Federal Offices	Challenger not from Federal Offices
Winner	0.0225 [0.00001]***	0.0491 [0.0120]***	0.0491 [0.0120]***	0.0103 [0.00001]***	0.0493 [0.00852]***
Vote Share (Winners) & Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes
Controls	State and Year FE	State and Year FE	State and Year FE	State and Year FE	State and Year FE
R-squared	0.104	0.045	0.047	0.034	0.063
Observations	58	528	469	92	436
Δ	-0.0266**			-0.0390***	
χ^2	5.46			22.48	

Table 4: State Corruption and the Value of Political Connections

This table reports OLS regressions of the cumulative abnormal returns (CARs) among the politically connected firms around close gubernatorial elections in the U.S. between 1999 and 2010. A firm is defined as politically connected if one of its directors and a close election's candidate graduate from the same university program within a year. Each observation pairs a connected firm to the connected candidate's close election. 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) window. *Winner* is a dummy variable equal to one (zero) if a politician wins (loses) a close gubernatorial election. A close election is specified by the margin of votes between the winner and the loser being less than 5%. All regressions control for the quadratic polynomials of the vote share of winners and vote share of losers, as prescribed by the regression discontinuity design, and year fixed effects. Columns (1) to (6) respectively show results on the subsamples of above or below median of the following measures: ALD 1970 score of isolation of the state capital (Campante and Do 2014), corruption news (Saiz and Simonsohn 2013, Campante and Do 2014), and corruption conviction rate (Glaeser and Saks 2006). Standard errors in square brackets are corrected for clustering by state. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

	Dependent Variables: CAR (-1,1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Subsample	Low ALD	High ALD	Less Corruption News	More Corruption News	Less Conviction	More Conviction
Winner	0.00668	0.0466	-0.00686	0.0290	0.0136	0.0407
	[0.00914]	[0.0152]***	[0.0195]	[0.00588]***	[0.0103]	[0.0105]***
Vote Share (Winners) & Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Year FE	Year FE	Year FE	Year FE	Year FE	Year FE
R-squared	0.006	0.045	0.008	0.018	0.009	0.057
Observations	331	250	247	236	424	162
Δ	-0.0399**		-0.0359*		-0.027***	
χ^2	5.46		3.48		9.95	

Table 5: Firm characteristics and the Value of Political Connections

This table reports OLS regressions of the cumulative abnormal returns (CARs) among the politically connected firms around close gubernatorial elections in the U.S. between 1999 and 2010. A firm is defined as politically connected if one of its directors and a close election's candidate graduate from the same university program within a year. Each observation pairs a connected firm to the connected candidate's close election. 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) window. *Winner* is a dummy variable equal to one (zero) if a politician wins (loses) a close gubernatorial election. A close election is specified by the margin of votes between the winner and the loser being less than 5%. All regressions control for the quadratic polynomials of the vote share of winners and vote share of losers, as prescribed by the regression discontinuity design, and state and year fixed effects. Columns (1) to (4) respectively show results on the subsamples in the cross-section of size (below or above median market capitalization) and with or without reliance on external finance (Rajan and Zingales 1998). Columns (5) and (6) respectively show results on subsamples of firms with below and above median abnormal trading activity (Campbell and Wasley 1996). Standard errors in square brackets are corrected for clustering by state. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Subsample	Small Firm	Large Firm	Rely on External Finance	Not Rely on External Finance	High Abnormal Trading Activity	Low Abnormal Trading Activity
Winner	0.0558 [0.0150]***	0.0133 [0.0136]	0.0533 [0.0119]***	0.0146 [0.00917]	0.0551 [0.0151]***	0.0102 [0.0167]
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes
Controls	State and Year FE	State and Year FE	State and Year FE	State and Year FE	State and Year FE	State and Year FE
R-squared	0.063	0.123	0.055	0.287	0.198	0.119
Observations	292	294	426	145	293	293
Δ	0.0425*		0.0387**		0.0449**	
χ^2	3.52		5.03		4.66	

Table 6: Social Network Characteristics and the Value of Political Connections

This table reports OLS regressions of the cumulative abnormal returns (CARs) among the politically connected firms around close gubernatorial elections in the U.S. between 1999 and 2010. A firm is defined as politically connected if one of its directors and a close election's candidate graduate from the same university program within a year. Each observation pairs a connected firm to the connected candidate's close election. 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) window. *Winner* is a dummy variable equal to one (zero) if a politician wins (loses) a close gubernatorial election. A close election is specified by the margin of votes between the winner and the loser being less than 5%. All regressions control for the quadratic polynomials of the vote share of winners and vote share of losers, as prescribed by the regression discontinuity design, and state and year fixed effects. Panel A reports the effects of connections by the strength of school networks. Columns (1) to (8) of Panel A respectively show results on the subsamples of alumni who graduate from the same university program within the same year, within one to five years, within 10 years, and within 20 years. Panel B shows school-specific effects of connections. Columns (1) to (6) of Panel B respectively present results on subsamples of connections through Ivy League and non-Ivy League networks, network size (below or above the average numbers of directors in in BoardEx universe), and alumni whose last reunion is held in election year or not. Standard errors in square brackets are corrected for clustering by state. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

Panel A. Strength of School Networks

	Dependent Variables: CAR (-1,1)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Subsample	Within 0Y	Within 1Y	Within 2Y	Within 3Y	Within 4Y	Within 5Y	Within 10Y	Within 20Y
Winner	0.0630	0.0389	0.0197	0.00768	-0.000736	-0.00388	-0.00369	-0.00427
	[0.0197]***	[0.00833]***	[0.00845]**	[0.00871]	[0.00824]	[0.00865]	[0.00474]	[0.00384]
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	State and Year FE	State and Year FE	State and Year FE	State and Year FE	State and Year FE	State and Year FE	State and Year FE	State and Year FE
R-squared	0.193	0.045	0.026	0.023	0.019	0.019	0.013	0.015
Obs	228	586	974	1,311	1,659	1,995	3,602	5,632

Panel B. School-Specific Effects, Network Size, Network Distance, and the Value of Political Connection

	Dependent Variables: CAR (-1,1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Subsample	Ivy League	Non-Ivy League	Large Networks	Small Networks	Reunion Year	Non-reunion Year
Winner	0.00540 [0.0000]***	0.0443 [0.0189]**	0.0214 [0.0000]***	0.0443 [0.0189]**	0.0349 [0.00526]***	0.00913 [0.00198]***
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes
Controls	State and Year FE	State and Year FE	State and Year FE	State and Year FE	State and Year FE	State and Year FE
R-squared	0.029	0.126	0.034	0.123	0.094	0.107
Obs	320	266	321	265	183	302
Δ	-0.0389**		0.0229		0.0258***	
χ^2	4.8		1.67		22.8	

Table 7: Firm Headquarters and Operations in the Election State and the Value of Political Connections

This table reports OLS regressions of the cumulative abnormal returns among the politically connected firms around close gubernatorial elections in the U.S. between 1999 and 2010. A firm is defined as politically connected if one of its directors and a close election's candidate graduate from the same university program within a year. Each observation pairs a connected firm to the connected candidate's close election. Average abnormal returns are estimated based on the market model around the election day (Day 0). *Winner* is a dummy variable equal to one (zero) if a politician wins (loses) a close gubernatorial election. A close election is specified by the margin of votes between the winner and the loser being less than 5%. All regressions control for the quadratic polynomials of the vote share of winners and vote share of losers, as prescribed by the regression discontinuity design, and state and year fixed effects. Column (1) shows results on subsamples of firms with activities in a given state in a given year, measured as the ratio of the number of search hits for the firm's name in local newspapers and the number of search hits for the neutral keyword "September," as in Campante and Do (2014). Column (2) reports results on firms with operations in the election state, based on the Garcia and Norli (2012) measure of state operational presence across time, and supplemented with OneSource data. Column (3) reports results on firms that are headquartered in the election state with Compustat's states of firm headquarters, corrected using the Heider and Ljungqvist (2014) method. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

	Dependent Variable: CAR (-1,1)		
	(1)	(2)	(3)
Subsample	Some Media Mention Prior to Election	Garcia and Norli (2012) State Presence	Same HQ-Election State
Winner	0.0330 [0.00907]***	0.0470 [0.0246]*	0.0281 [0.000001]***
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes
Controls	State and Year FE	State and Year FE	State and Year FE
R^2	0.062	0.186	0.775
Observations	409	187	30

Table 8: Political Connections and Corporate Outcomes

This table reports OLS regressions of the change in corporate outcomes among the politically connected firms following close gubernatorial elections in the U.S. between 1999 and 2010. A firm is defined as politically connected if one of its directors and a close election's candidate graduate from the same university program within a year. Each observation pairs a connected firm to the connected candidate's close election. *Winner* is a dummy variable equal to one (zero) if a politician wins (loses) a close gubernatorial election by a margin of votes between the winner and the loser of less than 5%. All regressions control for the quadratic polynomials of the vote share of winners and vote share of losers, as prescribed by the regression discontinuity design, and state and year fixed effects. Panel A reports change in corporate investments, defined as the difference in logarithm of corporate investment in year N versus year N-1. Investments are measured in a given year as the sum of capital expenditure and research and development expenditure normalized by start-of-the-year total assets. Columns (1) to (5) of Panel A show respectively results on corporate investments from one year before to three years after the election. Column (1) and (2) of Panel B show results in the change in log of the dollar value of a firm's procurement contracts and assistance in a 2-year period after election as compared to a 2-year period before election, respectively, with state-level and federal-level procurement. Columns (3) to (4) report the effects of connections on the probability to receive procurement contracts or government assistance after the election on two subsamples of firms headquartered and not headquartered in the election state. Standard errors in square brackets are corrected for clustering by state. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

Panel A: Corporate Investments

Dependent Variables:	**Δ Log(Firm Investing Activities)**				
	(1)	(2)	(3)	(4)	(5)
Window	Years: (-2,-1)	(-1,0)	(0,1)	(1,2)	(2,3)
Winner	0.0339	-0.241	-0.050	0.407	0.121
	[0.131]	[0.162]	[0.146]	[0.181]**	[0.139]
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes
Controls	State and Year FE	State and Year FE	State and Year FE	State and Year FE	State and Year FE
R-squared	0.065	0.091	0.128	0.101	0.100
Observations	468	466	480	463	442

Panel B: Procurement and Government Funding

Dependent Variable:	**Δ Log(Dollar Value)**		**Δ Indicator**	
	(1)	(2)	(3)	(4)
Subsample	State	Federal	Same HQ-Election State	Different HQ-Election State
Winner	1.863 [0.0000]***	0.573 [0.387]	0.0106 [0.0000]***	-0.0607 [0.0777]
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes
Controls	State and Year FE	State and Year FE	State and Year FE	State and Year FE
R-squared	0.793	0.184	0.566	0.112
Observations	24	211	30	556
Δ			0.0713	
χ^2			1.04	

Table 9: Homophily

This table reports OLS regressions of the Cumulative Abnormal Returns among the politically connected firms around close gubernatorial elections in the U.S. between 1999 and 2010. A firm is defined as politically connected if one of its directors and a close election's candidate graduate from the same university program within a year. Each observation pairs a connected firm to the connected candidate's close election. 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) window. *Winner* is a dummy variable equal to one (zero) if a politician wins (loses) a close gubernatorial election. A close election is specified by the margin of votes between the winner and the loser being less than 5%. All regressions control for the quadratic polynomials of the vote share of winners and vote share of losers, as prescribed by the regression discontinuity design, and state and year fixed effects. Columns (1) to (9) control for various interacted fixed effects. *WinSchool* is a dummy equal to one if any politician from a specific school wins in a specific election. *WinIndustry* is a dummy equal to one if a winning politician is connected to a firm in the industry. Industry is a firm's two-digit SIC code. Time since graduation decade is the number of decades since a politician graduates from a school. Standard errors in square brackets are corrected for clustering by state. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

	Dependent Variables: CAR (-1,1)			
	(1)	(2)	(3)	(4)
Subsample	5% margin	5% margin	5% margin	5% margin
Winner	0.0264 [0.0000]***	0.0410 [0.01053]**	0.0319 [0.01352]***	0.0253 [0.00479]**
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes
Controls	School FE x TimeGrad Decade FE x *WinSchool*, State FE, and Year FE	Industry FE x *WinIndustry*, State FE, and Year FE	Industry FE x Size Quintile FE x *WinIndustry*, State FE, and Year FE	(1) and (2)
R-squared	0.125	0.1425	0.3677	0.231
Obs	586	586	586	586

Table 10: Falsification and Placebo Tests

This table reports falsification and additional tests. A firm is defined as politically connected if one of its directors and a close election's candidate graduate from the same university program within a year. Each observation pairs a connected firm to the connected candidate's close election. *Winner* is a dummy variable equal to one (zero) if a politician wins (loses) a close gubernatorial election with a margin of votes between the winner and the loser of less than 5%. CARs are calculated around the election day (day 0), based on the market model using daily data from day -315 to day -61. Each column runs a local cubic polynomial regression of the dependent variable on vote shares in the subsamples above and below the cutoff, and reports the difference between the predicted values of the dependent variable for each subsample around the cutoff. Panel A shows several falsification tests. Column (1) shows the benchmark regression at 50% of the vote share threshold. Columns (2) to (5) show results with different hypothetical cutoffs. Panel B presents the additional tests. All regressions control for the vote share of winners and vote share of losers, as prescribed by the RD Design. The outcome variable CAR is calculated using different models: Fama-French model in columns (1) and (2); Fama-French model with momentum in columns (3) and (4); and raw returns in columns (5) and (6). Columns (7) to (8) collapse the data so that each unit of observation is respectively a director, or a company. Standard errors in brackets are corrected for clustering by state. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

Panel A: RDD with Non-Parametric Regressions and Tests

	Dependent Variables: CAR (-1,1)				
	(1)	(2)	(3)	(4)	(5)
	Benchmark	Placebo Thresholds			
Cutoff	50%	48%	49%	51%	52%
Winner	0.0227	0.0100	0.0037	-0.0066	0.1044
	[0.0113]**	[0.0326]	[0.0119]	[0.0100]	[0.0671]

Panel B: Additional Robustness Checks

Dependent Variable:	(1) FF	(2) FF	(3) FFM	(4) FFM	(5) Raw	(6) Raw	(7) Politician-Level	(8) Director-Level	(9) Company-Level	(10) Two-Way Clustering
Sample	5% margin	5% margin	5% margin	5% margin	5% margin	5% margin				5% margin
Winner	0.0386	0.0351	0.0352	0.0318	0.0419	0.0351	0.0407	0.0415	0.0373	0.0389
	[0.0087]***	[0.0000]***	[0.0074]***	[0.0000]***	[0.0101]***	[0.0000]***	[0.0222]*	[0.0087]***	[0.0077]***	[0.0136]***
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	State and Year FE	State, Year, and School FE	State and Year FE	State, Year, and School FE	State and Year FE	State, Year, and School FE	State and Year FE	State and Year FE	State and Year FE	State and Year FE
R-squared	0.047	0.136	0.040	0.113	0.095	0.184	0.667	0.052	0.039	0.045
Observations	586	586	586	586	586	586	51	439	552	586

Table 11: RDD Randomness Checks

This table reports robustness checks of the near-randomness of the win/lose treatment induced by close gubernatorial elections between 1999 and 2010. A firm is defined as politically connected if one of its directors and a close election candidate graduate from the same university program within a year. Each observation pairs a connected firm to the connected candidate's close election. *Winner* is a dummy variable equal to one (zero) if a politician wins (loses) a close gubernatorial election with a margin of votes between the winner and the loser of less than 5%. All regressions control for the quadratic polynomials of the vote share of winners and vote share of losers, as prescribed by the regression discontinuity design, and state and year fixed effects. Each column serves to show that a dependent variable's distribution is continuous at the cutoff point of 50% vote share. These dependent variables are those used as control variables in Tables 2 to 7 in the main text. Panel A shows results for politicians' characteristics (gender, age, logarithm of election turnout, incumbency, poll margin of win, and party affiliation.) Panel B reports results on director characteristics (director gender, age, executive role, and logarithm of social network size.) Panel C exhibits results on state characteristics (regulations, economic freedom, state employment, corruptions). Panel D reports regressions with firm characteristics (size, Tobin's Q, operating ROA, leverage, financial dependence, same headquarters as the election state dummy, measure of operations in election state based on Garcia and Norli (2012), state procurement, and investments). Standard errors in square brackets are corrected for clustering by state. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

Panel A: Politician Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable:	Pol. Gender	Pol. Age	Log(Turnout)	Incumbency	Poll Margin Of Victory	Party Affiliation	Federal Experience
Winner	-0.3133	1.636	-0.114	-0.111	0.007	-1.060	0.079
	[0.2862]	[4.715]	[0.244]	[0.323]	[0.0127]	[1.067]	[0.374]
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.172	0.049	0.119	0.065	0.056	0.098	0.092
Observations	51	51	51	51	51	51	51

B. Director Characteristics

Dependent Variable:	(1) Director's Gender	(2) Director's Age	(3) Log(Count of Institution)
Winer	-0.0306 [0.0902]	3.006 [4.278]	-0.577 [1.064]
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes
R-squared	0.027	0.110	0.049
Observations	439	433	439

C. State Characteristics

Dependent Variable:	(1) ALD 1970	(2) Convictions	(3) Corruption News
Winner	0.0762 [0.0582]	-1.334 [6.694]	-291.6 [162.2]*
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes
R-squared	0.045	0.036	0.187
Observations	581	586	483

D. Firm Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable:	Market Capitalization	TobinQ	Operating ROA	Leverage	Dependence on External Finance	Same HQ-Election State	Firm Activities Prior to Election	Garcia and Norli (2012) State Presence	Procurement	Investing Activities
Winner	-0.101	0.252	-0.176	0.0442	0.150	-0.0575	-0.225	-0.221	0.0224	0.453
	[0.527]	[0.225]	[0.197]	[0.0414]	[0.352]	[0.129]	[0.119]*	[0.146]	[0.0140]	[0.310]
Vote Share (Winners) and Vote Share (Losers)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.023	0.008	0.010	0.007	0.012	0.048	0.023	0.028	0.007	0.017
Observations	586	552	512	547	571	586	586	586	586	474