

Quid Pro Quo? Corporate Returns to Campaign Contributions

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Abstract

Scholars, pundits, and political reformers have long worried that corporations distort public policy and subvert the will of the electorate by donating to politicians. Well-publicized anecdotes notwithstanding, whether and how much corporations actually benefit from supporting political candidates remains unknown. To systematically address this question, we utilize two complementary empirical approaches that isolate the monetary benefits a company derives from a favored candidate winning office. First, we use a regression discontinuity design exploiting close congressional, gubernatorial, and state legislative elections. Second, we leverage *within-campaign* changes in market beliefs about the outcomes of U.S. Senate races. We find no evidence that corporations benefit from electing their favored candidate, and we can statistically reject effect sizes greater than 0.4 percent of firm value. Contrary to the concerns of many observers, corporate campaign contributions do not appear to buy significant political favors.

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Scholars, pundits, and political reformers often allege that corporate money corrupts politics. For instance, U.S. Supreme Court Justice John Paul Stevens contends that “[c]orporations with a large war chest to deploy on electioneering may find democratically elected bodies becoming much more attuned to their interests” (2010, p. 65). Sociologist Fred Block claims that “[c]orporate campaign contributions still go disproportionately to Republicans who offer tax cuts, antilabor policies, and reduced regulation as a quid pro quo” (2007, p. 16). Political scientists Martin Gilens and Benjamin Page offer a similarly bleak assessment. “[M]ajorities of the American public actually have little influence over the policies our government adopts... [I]f policymaking is dominated by powerful business organizations and a small number of affluent Americans, then America’s claims to being a democratic society are seriously threatened” (2014, p. 577). The predominant view on the role of corporations in the electoral process is one of distress and disapproval. Two concerns stand out in particular. First, corporate influence might alter election results in favor of pro-business candidates and away from the preferences of the public. Second, corporate donations might serve as a quid pro quo, affecting the policy choices of candidates, who, once elected, may feel indebted to their benefactors or hope to receive similar contributions in the future.

In this paper, we examine the relevance of both concerns by asking whether and how much firms benefit when a candidate they supported wins office. After all, if either mechanism is substantively important, then corporations ought to profit from their political investments. To measure and monetize the benefits a company derives from its favored candidate holding office, we turn to stock prices, which reflect the best available information about a firm’s value, present and future. Thus, to the extent that corporations derive material advantages from contributing to

political candidates, we expect these to manifest themselves in the value of the firm itself, i.e., its share price.

Our analysis begins by identifying donations from the political action committees (PACs) of publicly traded companies to candidates running for Congress, governor, or state legislator between 1980 and 2010. Corporations almost never support more than one candidate in any given race, allowing us to easily determine firms' preferred politicians. Causal inferences, however, are complicated by selection into "who gives to whom." Firms that are *ex ante* more successful are more likely to support winners, meaning that a naïve comparison of firms that gave to winning and losing candidates would produce spurious results. To carefully account for selection, we rely on two complementary empirical approaches. Our main analysis leverages the quasi-random outcomes of very close elections through a regression discontinuity (RD) design. Our second design uses *within-campaign* variation in market beliefs—as measured by betting odds—about the outcomes of U.S. Senate elections from 2004 to 2010. Identification comes from high-frequency changes in the probability that a corporation's preferred candidate wins the race. Both research designs yield substantively identical results. An electoral victory of the supported candidate does *not* significantly benefit the firm.

Our results are not easily attributable to low statistical power. *Ex ante* power analyses suggest that we would have reliably detected any effect greater than about a quarter of a percent. *Ex post*, our confidence intervals allow us to statistically reject any purported effect greater than 0.4 percent of firm value. In addition, we find no sign that a genuine impact is masked by heterogeneity. In fact, we detect little variation across offices, time periods, the size of firms, the size of donations, or economic sectors. If companies benefit from donating to political

candidates, then these effects must be small on average, and they are not detectable even in the settings where we would most expect them.

Since the corporations in our sample are extremely large relative to the typical donation, we cannot dismiss the possibility that campaign contributions are a worthwhile investment for the firm—albeit a modest one in absolute terms. We can rule out, however, that political donations are good investments for individual executives, and even the largest of our estimates are too small to support the jeremiads of pundits and reformers. In sum, our results suggest that contributing firms, at most, “give a little and get a little” (Ansolabehere, de Figueiredo, and Snyder 2003; p. 126).

Some readers are likely to be surprised by the finding that companies do not derive significant benefits from a supported politician holding office. How can our results be so different from public perception and well-known journalistic accounts? We suspect that much of the answer is due to correlational evidence. Large, successful corporations often support politically aligned incumbents, which invites suspicions of questionable behavior without offering a proper counterfactual. Another potential explanation is that our results are based on a broad sample of firms that supported many different politicians. If scholars and pundits focus on a few nefarious cases in which corporations appear to have benefitted from political quid pro quos, they will conclude that the perverse consequences of money in politics are severe, even if they are negligible on average.¹ By conducting a large-scale study that aggregates over thousands of different firms and elections, our findings speak to the typical effect of a supported

¹ Furthermore, multiple testing and selective reporting mean that a seemingly perverse finding need not reflect a genuine phenomenon (Franco, Malhotra, and Simonovits 2014; Simmons, Nelson, and Simonsohn 2011). Regardless, existing anecdotes suggest that corporate money *can* have perverse consequences, not that these consequences are large on average.

candidate rising to office rather than extreme cases. Evidence on *both* is necessary to sensibly discuss corporate influence in American politics.

Before proceeding, we should clarify what we mean by *corporate campaign contributions*. Since the Tillman Act in 1907, corporations are prohibited from donating directly to political candidates. However, firms can set up and fund the operation of PACs, which, in turn, raise money from individuals—often managers and shareholders of the firm—and give it to candidates. We use the term *corporate campaign contributions* as shorthand for contributions from corporate PACs to political candidates and their campaigns.² Since *Citizens United* and *SpeechNow v. FEC*, corporations can draw on their treasuries to give unlimited amounts to independent expenditure-only committees, which in turn advocate for or against particular candidates. Although these independent committees are officially prohibited from coordinating with campaigns, candidates are often alleged to solicit large sums for the groups that support them (Lee, Ferguson and Earley 2014). On one hand, *Citizens United* makes the results of this paper all the more important because corporate electioneering expenditures are at an all-time high. On the other hand, one might worry that recent changes in the political environment limit the generalizability of our results to the present era. Nonetheless, our limited data for the post-*Citizens United* period are consistent with our overall findings. Both before and after this landmark decision, an additional supported candidate winning office does not detectably benefit the firm.

² Naturally, individual managers can also donate directly to political candidates. Previous scholars have argued that money channeled to candidates through corporate PACs is used by companies to seek access (e.g., Fourniaies and Hall 2014; Barber 2016), while individual contributions may be due to various other motivations, even when carried out by CEOs (Bonica 2016, Barber, Canes-Wrone, and Thrower 2017). We, therefore, rely on the contribution patterns of corporate PACs rather than individual donations to identify firms' preferred candidates.

Related Literature

Political economists have extensively theorized about the role of corporate campaign contributions in the policymaking process. Many models conceptualize elections as a competitive market for private benefits, whereby firms or other special interests can give money to curry favor with politicians (e.g., Baron 1989; Denzau and Munger 1986; Grossman and Helpman 2001). Since elected officials have many levers through which to provide benefits to a firm, companies have an incentive to identify sympathetic politicians and increase their chances of being elected. A corporation might also use campaign contributions to ingratiate itself with a candidate who is likely to win anyway. Such a connection could be valuable because elected officials may propose grants and procurement contracts that can directly benefit the firm, change their roll-call vote on an important piece of legislation, alter the content of that bill through amendments or committee work, or pressure the bureaucracy to achieve favorable regulatory outcomes.

In light of these theoretical concerns, empiricists have actively looked for evidence on the perverse influence of political donations. The most common strategy involves regressing the roll-call votes of legislators on campaign contributions. One study, for instance, asks whether members of Congress who received contributions from dairy producers are more likely to vote in favor of a dairy subsidy (Welch 1982). Despite damning anecdotes, the evidence as a whole is mixed (Ansolabehere, de Figueiredo, and Snyder 2003). Even if the correlations between campaign contributions and subsequent legislator behavior were clear cut, these regressions do not necessarily uncover causal relationships. If, as predicted by theory, corporations support politicians who already agree with them, then campaign contributions and roll-call votes will be correlated regardless of whether the former have any impact on the latter. Consistent with this

account, the estimated effects of corporate contributions on roll-call votes tend to disappear with the inclusion of district or member fixed effects (Ansolabehere, de Figueiredo, and Snyder 2003; Wawro 2001). Furthermore, case studies of individual bills are insufficient to assess the overall prevalence and severity of corporate influence.³

Taking a contrarian stand, Tullock (1972) and Ansolabehere, de Figueiredo, and Snyder (2003) argue that there is not nearly as much corporate money in politics as one might expect if public policy were actually for sale. If dairy producers could really generate \$1 billion in price supports through \$1.3 million in campaign contributions, why would the dairy industry not give two, three, or even ten million dollars? And why can members of Congress be bought so cheaply?⁴ Since the vast majority of corporate interests give less than the statutory limit on campaign contributions, legal restrictions on political donations are unlikely to be the answer to this puzzle.

Other scholars have looked beyond roll-call votes (e.g., Hall and Wayman 1990). Recently, Gordon and Hafer (2005) argue that firms contribute in order to signal their willingness to fight regulators and thereby achieve more favorable treatment from government agencies. Gordon, Hafer, and Landa (2007) find that executives whose compensation is more closely tied to corporate earnings are more likely to contribute to political candidates, and they suggest that contributions are best understood as purchases of good will. Kalla and Broockman (2016) report that activists are more likely to secure a meeting with a senior staffer of a member of Congress when they reveal themselves to be donors rather than merely constituents.

³ This is especially true when evidence of nefarious behavior is more likely to be published than null results.

⁴ Our characterization of this argument is, of course, simplistic. Presumably, Congress had already been planning to provide some price supports, but by spending \$1.3 million in campaign contributions the dairy industry might have been able to get a more generous subsidy. The *marginal* return on investment might, therefore, be considerably lower—and therefore more plausible—than the numbers above suggest.

Though fascinating, we view this type of evidence as indirect. Even if political donations help to secure meetings with legislators, the ultimate consequences of these meetings remain unknown. If for-profit companies make campaign contributions to curry favor, and if these favors actually affect policy, then their value ought to be reflected in the financial worth of the firm. Thus, for publicly traded companies, we can utilize stock prices to gauge the pecuniary value of *quid pro quos*.

Outside the U.S., politically connected firms trade at a premium relative to unconnected ones (Faccio 2006; Fisman 2001). Within the U.S., Do, Lee, and Nguyen (2015) report that corporations profit from having a director who went to school with a governor, and Goldman, Rocholl, and So (2009) find benefits from appointing former politicians and high-ranking bureaucrats to the company's board. Similarly, Brown and Huang (2017) argue that corporate executives' meetings with key White House staff lead to positive abnormal returns. While political connections of this kind do appear to be valuable, they are not the kinds of connections that can simply be bought with campaign contributions.⁵

The studies most closely related to our own are Cooper, Gulen, and Ovtchinnikov (2010), Boas, Hidalgo, and Richardson (2014), and Akey (2015). Cooper et al. find that stock returns increase after corporations give money to candidates, but they acknowledge that their evidence is correlational rather than causal. Using a regression discontinuity design, Boas et al. find that public-works firms in Brazil receive more government contracts when their favored candidates rises to office. Akey (2015) analyzes thirteen close U.S. congressional races and reports that the

⁵ There also exists a sizeable literature on the impact of presidential election results on different sectors of the economy as well as the stock market as a whole (e.g., Snowberg, Wolfers, and Zitzewitz 2007; Wolfers and Zitzewitz 2016; Mattozi 2008). Knight (2007), for instance, defines a group of politically sensitive firms and uses prediction market data to show that presidential policy platforms are capitalized into equity prices. Our second empirical approach mirrors this research design, but we find no evidence that the election of senators affects the firms that supported them. One potential explanation for the discrepancy is that the president wields much more influence than individual legislators.

stock price of a firm increases by about 3 percent when a candidate to which it contributed wins. However, relying on our own, independently collected data for the same set of elections, we fail to replicate Akey's results.⁶ Given the small number of candidates and firms in Akey's analysis, the point estimates are not only imprecise but also of varying sign (see Appendix G for details).

Thus, whether corporations benefit from campaign contributions in a mature democracy like the U.S. remains unknown. Our subsequent analyses provide systematic evidence on this question by drawing on data from thousands of closely contested elections for governor, Congress, and state legislatures.

Why Give?

Why do corporations donate to political candidates? For some, the fact that corporations contribute anything at all is sign enough that there must be perverse effects. After all, why would for-profit companies give if they are not getting something valuable in return? We can think of at least six different reasons a corporation might contribute money to political campaigns, with each one corresponding to varying degrees of concern about the health of democracy.

First, corporations might give with the goal of altering election outcomes. If one candidate is more likely to support policies that would benefit a firm, that company has an incentive to contribute in order to help the aligned candidate win. Many studies show that campaign spending can influence vote shares, although the substantive size of these effects is typically small (e.g., Green and Gerber 2015). Experimental evidence from get-out-the-vote studies suggests that each additional vote costs between 100 and 200 dollars, so influencing the outcome of most large elections would be expensive. For example, consider the 2014

⁶ We have attempted to resolve these discrepancies through private correspondence but were unsuccessful because the author refused to share any code or data.

gubernatorial election in Illinois, which, *ex ante*, was thought to be an extremely competitive race. Ultimately, Bruce Rauner (R) won by over 142,000 votes. Had special interests wanted to tip the scale in favor of Rauner's opponent, Pat Quinn (D), they would have had to spend at least \$14 million, even if there was no equilibrium response from contributors on the other side. Since most campaign contributions are three or four orders of magnitude smaller and go to candidates in less competitive races, one may question whether influencing election outcomes is firms' primary motivation.

Second, companies might give to influence the behavior of a candidate who would have been elected regardless of their help. Perhaps, once in office, elected officials offer quid pro quos to companies that supported them, or they might dole out favors in order to secure similar contributions in future campaigns. This mechanism appears to be the most prominent one in the academic literature as well as the one that especially troubles observers.

Third, corporations might give with the goal of obtaining information from elected officials. Even if contributions affect neither elections nor public policy, they might create a connection between a firm and an official that benefits the company in other ways—say, by enabling it to better anticipate regulatory changes. Companies may be willing to pay for such information, even if they cannot directly affect policy choices.

Fourth, corporations might give to sitting incumbents in order to change their behavior *before* the next election. Analogous to Hall and Deardorff's (2006) theory of lobbying, campaign contributions may serve as legislative subsidies that free up an official's time, which, in turn, enables her to pursue her policy goals. This mechanism is consistent with the observation that firms tend to target aligned incumbents. However, the fact that most contributions come toward the end of an official's term—when it is too late to enact meaningful policy change before the

next election—casts doubt on such an explanation. Similarly, few corporations make contributions in support of incumbents that are expected to lose.

Fifth, corporations might give to signal their type. In the model of Gordon and Hafer (2005), firms have an incentive to signal their willingness to fight regulation. In the model of Schnakenberg and Turner (2016), corporations use campaign contributions to signal compliance with the law and thereby reduce their chances of being audited.

Sixth and last, corporations might give for consumption purposes without receiving any tangible benefits in return. This is the preferred explanation of Ansolabehere, de Figueiredo, and Snyder for individual contributions, and, perhaps, the same argument applies to corporations. While it may seem implausible that a company itself derives consumption value from political contributions, individual managers may personally know candidates running for office, enjoy attending glamorous fundraisers, or derive utility from supporting their preferred candidates. Agency problems within large firms might allow the same executives to (mis)use some of the company's resources for political purposes. Given that the typical donation is minuscule relative to the operating budgets of even medium-sized corporations, and as evidenced by the mixed results in the academic literature, it would be difficult for shareholders to determine whether campaign contributions are, in fact, a good investment.

To the extent that any of these mechanisms operate, our subsequent research designs will estimate the *combined* impact of the first three. Put differently, the benefits a company derives from a supported candidate winning the election include the value of having someone hold office who would intrinsically pursue policies that benefit the firm, the value of any political quid pro quos, as well as the value of insider information. To the extent that any of these mechanisms are substantively important, the successful election of a favored candidate should increase a firm's

stock price. The last three mechanisms are not reflected in our estimates. The fourth mechanism produces benefits in the immediate term, which do not directly depend on electoral outcomes. In the signaling theories, the value of campaign contributions is independent of election results. In fact, to signal their type companies might rationally support the candidate that is *ex post* worse. And again, in the consumption account, who wins is irrelevant to the firm because there are no meaningful direct effects.

In our view, the last explanation need not be problematic for the health of democracy. If some corporations or their executives spend shareholder money to indulge their own political preferences—and if shareholders do not find it necessary to curb these expenses—then so be it. At the very least, this form of giving is not more damaging than personal campaign contributions of similar size. For the other five explanations, there are normative concerns—though, perhaps, to varying degrees. Our impression is that most of the worry about corporate influence in politics is due to the first two possibilities, i.e., its effects on election results and political quid pro quos. Since our empirical tests capture most of what observers find concerning about corporate contributions, our results directly contribute to the current normative debates about money in politics.

Data and Research Design

Our regression discontinuity (RD) analysis relies on general election results for the U.S. Senate, U.S. House, governor, and state legislatures from 1980 to 2010.⁷ Information on campaign contributions over this period comes from the Database on Ideology, Money in Politics, and Elections (Bonica 2013). We identify corporate PACs in these data, match them to their publicly

⁷ These data were kindly provided by Jim Snyder and represent an extended version of the data set used in Ansolabehere and Snyder (2002).

traded parent companies, and aggregate all contributions (within a particular election cycle) from the same corporation to the candidates in these elections (see Appendix F for details). Supplemental data on firm size, profitability, sector, etc. come from the CRSP/Compustat Merged Database. Our final data set includes 2,819 corporations and 147,599 firm-candidate-election pairings—our unit of observation—across 16 two-year election cycles with 18,744 individual races.⁸

To construct our outcome variable, we use daily stock returns from the Center for Research in Security Prices. Following standard practice in the finance literature, we rely on the market model to compute cumulative abnormal returns (CARs) for each firm (e.g., Campbell, Lo, and MacKinlay 1996). Intuitively, CARs adjust stock returns for the performance of the entire stock market over the same period. More precisely, the daily abnormal return of firm i on day t is defined as $AR_{it} = r_{it} - \hat{\alpha}_i - \hat{\beta}_i m_t$, where r_{it} denotes the realized return of the company's stock on day t , and m_t is the market return on the same day. $\hat{\beta}_i$ and $\hat{\alpha}_i$, respectively, denote the sensitivity of the firm's stock to overall market movements and its usual risk-return performance.⁹ Residualizing stock returns in this fashion yields more precise estimates by accounting for market-wide forces that are out of companies' control as well as the fact that some firms are more responsive to overall market conditions than others. With the definition of AR_{it} in hand, the abnormal return of a firm over a period of multiple consecutive days is given

⁸ As we explain below, we restrict our sample to firm-election pairs in which more than 90% of the contributions from a particular company went to one candidate. As a consequence, we discard 1.79% of observations.

⁹ Similar to Acemoglu et al. (2016), we use a window from 230 to 30 trading days before the election to estimate $\hat{\beta}_i$ and $\hat{\alpha}_i$. Note, the efficient markets hypothesis implies that $\hat{\alpha}_i$ should be equal to zero. In our sample, enforcing this theoretical restriction produces a slightly tighter distribution of CARs and subsequently more precise estimates. The substantive difference, however, is negligible, which is why we have opted for the approach that more closely mirrors standard practice in the finance literature.

by $CAR_i(t_1, t_2) = (\prod_{t=t_1}^{t_2} [1 + AR_{it}]) - 1$.¹⁰ For our main analyses, we calculate CARs from the day before the election to the day after, i.e., $CAR(-1,1)$, but we also test for longer term effects. Reassuringly, sensible alternative ways of constructing our outcome variable result in qualitatively equivalent conclusions. In particular, we obtain nearly identical point estimates if we use simple, unadjusted returns instead (see Appendix A).

Our goal is to estimate the effect of electing a supported candidate on a firm's value. To do so, we would like to approximate the following hypothetical experiment. Suppose campaigns proceed as usual, but election result are secretly determined by a coin flip.¹¹ If who wins the election is random, then we can estimate the effect of a firm's supported candidate (rather than her opponent) rising to office by simply comparing the mean stock return of firms that support winners with that of companies that support losers. Since such an experiment is not feasible, we attempt to replicate it as closely as possible by focusing on close elections and implementing a regression discontinuity (RD) design.

To fix ideas, let $Y_{ij}(1)$ be the return for company i if its favored candidate in election j wins, and let $Y_{ij}(0)$ stand for the outcome if the firm's preferred candidate loses. X_{ij} denotes that candidate's vote margin. We are interested in $E[Y(1) - Y(0)]$, the average effect of the favored candidate winning. However, since the same firm's stock price is never observed in both states of the world simultaneously, it is generally not possible to estimate average treatment effects without imposing strong assumptions. We can, however, estimate $\lim_{x \rightarrow 0+} E[Y(1)|X]$ and $\lim_{x \rightarrow 0-} E[Y(0)|X]$, and we can calculate their difference. If potential outcomes are continuous at the electoral threshold, i.e., $\lim_{x \rightarrow 0-} E[Y(1)|X] = \lim_{x \rightarrow 0+} E[Y(1)|X]$ and $\lim_{x \rightarrow 0-} E[Y(0)|X] =$

¹⁰ Simply summing the abnormal daily returns would be a good approximation for small returns and for a small number of days, but it would be inaccurate for larger returns or over longer periods of time.

¹¹ The coin flip must be secret because candidates and firms need to behave normally, believing that their campaign efforts influence election results.

$\lim_{X \rightarrow 0^+} E[Y(0)|X]$, then $\lim_{X \rightarrow 0^+} \widehat{E}[Y(1)|X] - \lim_{X \rightarrow 0^-} \widehat{E}[Y(0)|X]$ gives us an unbiased estimate of $E[Y(1) - Y(0)|X = 0]$. In words, our RD design recovers a *local* average treatment effect, i.e., the impact of an electoral victory on firm value for the subset of companies that contributed in virtually tied races.

Because many corporations give to multiple candidates in the same electoral cycle, our RD estimate should be interpreted as the return to the firm from one additional supported candidate rising to office. To see this, let O_{ij} denote the number of *other* office holders to whom the company contributed—either in previous electoral cycles or as a result of its donations in concurrent races. Given the continuity assumptions above, $\lim_{X \rightarrow 0^+} E[O_{ij}|X] = \lim_{X \rightarrow 0^-} E[O_{ij}|X]$. Hence, on average, firms that supported bare winners should be connected to one more office holder than those backing bare losers.

We implement our RD design by estimating the following equation:

$$(1) \quad CAR(-1,1)_{ijt} = \beta Victory_{ijt} + \gamma X_{ijt} + \lambda X_{ijt} Victory_{ijt} + \epsilon_{ijt},$$

where $Victory_{ijt}$ is an indicator variable for whether firm i 's preferred candidate won election j . The coefficient of interest is β . It captures the present value of a company's favored candidate rising to office as a fraction of the overall value of the firm. We cluster standard errors by election cycle to allow for almost arbitrary forms of correlation in the residuals across firms and elections. For our main results, we restrict attention to elections in which the two-party vote share fell between .45 and .55, and we present robustness checks to demonstrate that our conclusions are insensitive to alternative bandwidths.

We settled on this particular specification after conducting *ex ante* power simulations. In each simulation, we randomly select a new date for each election year to serve as a placebo Election Day. Taking election outcomes, the pattern of corporate contributions, as well as actual

stock returns around the placebo date as given, we add in a hypothetical treatment effect of a known magnitude and implement our empirical strategy (see Appendix D for details). Figure 1 shows the results of our simulations for the specification in equation (1). These simulations suggest that if the true effect is at least 0.2 percent, we would detect it with almost 90 probability.

As mentioned above, our primary specification relies on CARs from the day before the election to the day after. This strategy maximizes statistical precision but it relies on the market to quickly internalize the effect of election results on the value of firms—even before the newly elected officials take office. There is an enormous literature on the efficiency of financial markets, which typically concludes that markets are close to efficient but not perfectly so (see, e.g., Fama 1970; Shiller 1981). We are hesitant to take a strong stand on how fast the market can internalize the impact of election results. Instead, we present a range of specifications, allowing for longer lags until prices accurately reflect all effects of elections. In particular, we present results for as much as 100 trading days after Election Day—when the winners have taken office and begun enacting their policy agendas. Although increasing standard errors make quantitative comparisons speculative, if anything, these specifications suggest that firms benefit even less from their preferred candidate’s electoral triumph than implied by our main results.

Our RD design serves several purposes. First, it directly addresses selection into who gives to whom. If the results of very close races are, indeed, quasi-random, then firms supporting the candidate who, *ex post*, barely won are, in expectation, identical to companies donating to the candidate who barely lost. Second, our research design ensures that the market is surprised by the outcomes that drive our inferences. For elections that are easily predictable, stock prices will already reflect the value of any potential quid pro quo *before* Election Day. Returns realized on

or after Election Day would, therefore, be uninformative about the benefits accruing to the firm. In very close elections, however, both candidates should have an *ex ante* realistic chance of winning, which implies that the market receives new information when the votes are tallied.¹²

Our RD results are, of course, local to close elections. That is, we estimate the monetary value of the benefits accruing to a firm when its supported candidate narrowly wins rather than narrowly loses. If we are interested in the mechanism whereby firms benefit from campaign contributions by changing the result of an election, this is exactly the quantity of interest. After all, close races are the ones in which firms could potentially affect the outcome. If, however, we are more interested in the quid pro quo or informational mechanisms, then our approach has both advantages and drawbacks. On one hand, the most powerful and influential politicians might be electorally safe, which could give them more leeway to hand out favors relative to their counterparts who live under electoral threat. On the other hand, politicians may be more willing to engage in quid pro quos precisely when their (re)election prospects are uncertain—simply because campaign contributions are more valuable when the race is expected to be close. Although our primary reason for focusing on close elections is to cleanly identify a causal relationship, we believe there are also good theoretical reasons to be interested in these cases.

¹² One potential concern with our approach is that market participants may not have the opportunity to incorporate the effects of political contributions into their valuations of firms if they are unaware of who gave to whom. Fortunately for our purposes, PAC contributions are public record, and due to FEC reporting requirements, any contributions made by mid-October of the election year are publicly disclosed before Election Day. Traders can, therefore, know which firms gave to which candidates, and they can use this information if they deem it valuable. Another potential concern is that close elections are subject to recounts and court cases, meaning that there is lingering uncertainty about the outcome even after Election Day, which, in turn, would attenuate our estimates. Although recounts and court cases do occur, it is extremely rare for the initial vote tally to be reversed. Hence, the amount of residual uncertainty is likely small. Furthermore, this concern becomes less relevant as we examine longer time horizons, or when we conduct a donut RD design that ignores the closest of elections—those most likely to be affected by recounts and legal battles.

Descriptive Facts

Several descriptive facts help to motivate our analysis. First, elections that are *ex post* close were not *ex ante* predictable, meaning that the market received genuinely new information on Election Day. To support this assertion, we turn to betting market data for 119 U.S. Senate elections between 2004 and 2010 that were listed on Intrade.¹³ For each election and each point in time, the betting price provides the market's implied belief about the probability that a particular candidate will win (see, e.g., Wolfers and Zitzewitz 2004).

In Figure 2, we plot these beliefs the day before the election against the realized two-party vote share. As expected, there is a strong relationship between the *ex post* Democratic vote share and the *ex ante* probability that the Democratic candidate wins. More importantly, for the races that were, indeed, very close—i.e., the ones driving our subsequent inferences—the market still appears to be uncertain about the outcome the day before. Likewise, races expected to be close actually ended up being toss-ups. Although betting market data is only available for a small portion of elections in our sample, these are among the most salient races to which markets should pay close attention. Yet, even in these instances, the outcomes of very close elections are not easily predictable.

Second, almost no corporations give to both candidates in a given race. Figure 3 illustrates this point with a histogram of the proportion of a firm's contributions going to the Democratic candidate. Virtually all the mass is at the extremes. In only 1.87 percent of all cases did corporate PACs contribute to both the Democrat and the Republican. This observation is important because our research design requires us to clearly identify firms' preferred candidates. The fact that the vast majority of companies support at most one candidate in any given race makes this task straightforward. In practice, we exclude from our analysis the small number of

¹³ These data were kindly provided by Koleman Strumpf.

observations where one candidate received less than 90 percent of a firm's contributions pertaining to their respective race. The pattern in Figure 3 is also interesting because it implies that corporations do not generally hedge their bets. If campaign contributions buy political favors, one might expect companies to donate to all candidates who may conceivably win. Figure 3 further shows that corporate contributions skew toward Republicans but not dramatically so. Corporate PACs support Democratic candidates in about 40 percent of the firm-elections in our data, and Republicans in the remaining 60 percent.

Another relevant fact is that, conditional on contributing, most corporations give to winners. Figure 4 depicts the distribution of the two-party vote shares of companies' favored candidates across all firm-election pairs in our data. Most firms support candidates running in uncompetitive, or even uncontested, elections. If corporations attempted to influence election outcomes, we would expect them to disproportionately contribute to candidates in races that are close. Importantly for our purposes, Figure 4 does not depict a clear discontinuity around the electoral threshold. Both visual inspection as well as formal density tests (McCrary 2008) suggest that corporate contributors are about equally likely to target bare winners and bare losers. That is, there is no evidence of a discontinuity in the running variable of our RD design (see also Appendix C).

While Figure 4 shows that most firms support winners, Table 1 demonstrates that the most successful corporations are especially likely to back winning candidates. Put differently, there is strong selection into "who gives to whom." Firms giving to winners are over 50 percent more valuable than those giving to losers, they are more profitable, and they have higher price-earnings ratios, suggesting that market participants expect faster profit growth in the future. Further, corporations that give to winners support a greater number of other political candidates

in the same electoral cycle and they experience slightly higher abnormal returns in advance of Election Day—although the latter difference is not statistically significant. Given that the variables in Table 1 are predetermined with respect to the outcomes of the elections in which companies contributed, simple, cross-sectional comparisons based on observational data would likely produce spurious results. The patterns in Table 1 may, in fact, explain why there are so many troubling anecdotes about corporate influence in American politics. Firms that support eventual office holders really are doing better. Without a proper counterfactual, observers have little way of discerning how much of this correlation is due to selection and how much of it arises from nefarious effects of campaign contributions.

As explained above, our RD design addresses the issue of selection bias by juxtaposing the returns of firms that gave to bare winners with those that supported bare losers. In order to yield causal estimates, this approach requires that potential outcomes are continuous at the electoral threshold. In Table 2, we assess the plausibility of this assumption by estimating the specification in equation (1) using all variables shown in the previous table as outcomes. That is, we test for discontinuities in pre-election covariates at the electoral threshold using our RD sample and specification. Importantly, there is no evidence of an imbalance in cumulative abnormal returns leading up to Election Day, incumbency status of the supported candidate, or overall firm performance. There is, however, a difference in the number of other candidates that the firm supported. While the imbalance is much smaller than in the full sample, it is statistically distinguishable from zero.

Taken at face value, the imbalance with respect to firms' giving in other races may be concerning. If corporations giving to bare winners are connected to more candidates and, as a result, more *ex post* winners, then our RD estimates would *overstate* the impact of an additional

supported candidate rising to office. One potential explanation for the observed pattern is that some companies are, in fact, good at identifying and targeting bare winners. Another, perhaps more compelling, explanation is that the estimated discontinuity is due to chance. As Eggers et al. (2015) point out, even when the identifying assumption of an electoral RD design is sound, if one tests enough covariates then any finite sample will exhibit some statistically significant imbalances. To account for multiple hypothesis testing, we proceed nonparametrically. Under the null hypothesis of no systematic differences, we would expect the p -values from multiple independent tests to be jointly uniformly distributed over the unit interval, whereas a non-uniform distribution would indicate that at least some of the purported differences are genuine. To test the null hypothesis of a uniform distribution, we implement a Kolmogorov-Smirnov test. The resulting joint p -value is .548, meaning that the patterns in Table 2 do not contradict the identifying assumption in our RD design. By contrast, the same test applied to Table 1 resoundingly rejects the null of no systematic differences ($p < .001$).

Nevertheless, to demonstrate that our subsequent results are not contaminated by sorting around the electoral threshold, we complement the main RD analysis in several ways. First, we implement a donut RD design in which we remove the closest of elections, i.e., the ones for which one might be especially worried about sorting. Second, we present results from an alternative research design that exploits variation in market beliefs over the course of a campaign. This approach does not rely on the continuity assumptions that are required in an RD design. Third, in Appendix A, we conduct ancillary robustness checks, such as excluding the largest firms or the ones giving to the most candidates. In each case, the evidence leads to substantively identical conclusions.

RD Results

Focusing on abnormal stock returns right around Election Day, Figure 5 presents our main result. For illustrative purposes, we average CARs across all observations within one-quarter-percentage-point-wide bins of the vote share, and we display fitted values based on the regression model in equation (1). The size of the estimated discontinuity is .0005, which implies that a firm's value increases by approximately 0.05 percent in response to a supported politician winning office. Given that the 95%-confidence interval associated with our point estimate ranges from $-.003$ to $.004$, we cannot reject the null hypothesis that the true effect equals zero. We can, however, statistically reject any purported impact greater than about 0.4 percent.

To interpret this finding, consider the following back-of-the-envelope calculations. The median firm in our data is valued at about \$1 billion. Taking our point estimate at face value, this would mean that the median contributing firm is about \$500,000 more valuable as a result of its favored candidate winning. While this may seem like an insignificant amount relative to the size of the company or relative to the cost of all but the smallest public works projects, it comes at a low cost. The median firm spends only \$3,750 on campaign donations per cycle and supports three winners.¹⁴ Hence, there is a clear rationale for companies to contribute to political candidates from their own treasuries. This does not mean, however, that a company's executives or shareholders have an economic incentive to give, and most PAC contributions are ultimately financed by individual managers. According to the best estimates in the literature, CEOs personally receive about \$1 for every \$100,000 of firm value that they create (see Murphy 1999). Thus, executives within the company would value the same benefits at only \$5. Even if we took

¹⁴ Calculating the exact return is challenging because corporations typically spend a lot more money on electioneering beyond their direct contributions to candidates. For example, companies make independent expenditures, contribute to party committees, and sometimes try to mobilize and persuade their employees (see, e.g., Bombardini and Trebbi 2011).

the upper bound of our 95% confidence interval as the true effect size, individual managers can still expect a negative return on their political investments.

Based on these calculations, we cannot dismiss the possibility that small donations are a good investment for firms. Publicly traded companies are so large and the absolute size of most contributions is so small that it is difficult to envision a research design that could statistically rule out this possibility. We can, however, statistically reject the possibility that, on average, individual executives and managers benefit from their political donations. More importantly, there is no evidence that, once elected, supported candidates engage in substantively meaningful quid pro quos.

As discussed, for our main RD analysis we rely on a bandwidth of .05—including all elections where the two-party vote share was within .05 of the .5 threshold. In Figure 6, we present estimates and confidence intervals for alternative choices ranging from .005 to .3. All of our RD estimates are substantively small—some are even negative—and for larger bandwidths we can statistically reject even medium-sized effects. In Table 3, we present robustness checks for alternative regression models, including higher order polynomials in the running variable, and race fixed effects. Reassuringly, we obtain qualitatively and quantitatively similar results.

Relying on our preferred specification in equation (1), Figure 7 presents estimates for longer time horizons. Even if financial markets are not perfectly efficient and cannot internalize the effects of close elections immediately, at some point we would expect the impact of election outcomes to be fully reflected in stock prices. We, therefore, present RD estimates for CARs ranging from the day after the election up to 100 trading days afterward—when the electoral winners have taken office and begun implementing their policy agendas. Yet, regardless of the time horizon, there is no indication that firms benefit from their preferred candidate being

elected. Our estimates are never statistically distinguishable from zero, and after about 40 trading days they actually become negative. Regardless of time horizon, there is no evidence that electoral victories benefit the firm.

We also explore the possibility of heterogeneous effects. To this end, Table 4 presents RD results for different subsamples. The first row shows our baseline estimate from Figure 5. The second row estimates the same regression model on a donut sample, i.e., a sample from which we removed all elections decided by less than 0.2 percentage points. These are the races for which one might be worried about sorting, fraud, or legal challenges, all of which may lead to non-random outcomes. Excluding these races has virtually no impact on the RD estimate.

Next, we present results separately by office. *A priori*, one might have expected the largest impact for governors. After all, governors are politically important and operate as independent executives rather than one of many members of a legislature. The actual point estimate for governors, however, is negative, and there is no statistically significant evidence of a positive effect for any other office.

In addition, we study cases in which corporations donated relatively large sums. Even in races where a firm gave more than \$2,500—roughly the 90th percentile of donations—we obtain a small, statistically insignificant point estimate. If there were political quid pro quos, say in the form of a single grant or procurement project, we would expect to find a greater effect on firm value for small rather than large, diversified companies. We test this prediction by splitting our data according to firms' market capitalization. Again, it is not possible to reject the null of no quid pro quos. The same holds true when we separately analyze different economic sectors, and when we consider donations before and after in *Citizens United*. Although the sample size for the latter period is small, there is no evidence of an effect in either era.

Table 4 further examines heterogeneity across the number of *other* winners that a firm supported in the same election cycle. If there are diminishing marginal returns to political connections, an electoral victory should be most valuable when the company did not contribute to other elected officials. However, even in these cases, the RD estimate is small and statistically insignificant. Moreover, our results do not depend on the number of candidates that the firm supported, suggesting that the sample imbalance with respect to this variable is inconsequential.

Since one might suspect that corporations giving to candidates on both sides of the aisle are especially access oriented, we separately analyze firms that donated primarily to Republicans, Democrats, or both. Our results, however, yield no evidence of effects for any of these subgroups. One may also expect that quid pro quos are more likely to arise when only a few firms contribute to a candidate's campaign. Yet, we detect no meaningful variation across the number of firms supporting the winner. Lastly, for the legislative settings in our sample, we might expect greater effects when a company's favored legislator belongs to the majority party and thus stands a greater chance of influencing policy. But again, neither majority nor minority party winners have a meaningful impact on firm value.

Broadly summarizing, our RD results imply that, on average, firms do not derive significant benefits from the electoral victory of a supported candidate. In addition, we find little evidence of heterogeneity in effect size. Even in settings that are *a priori* most likely to yield evidence of political quid pro quos, there appear to be none.

Alternative Research Design

To ameliorate any potential concerns with our RD analysis, we implement an alternative research design that relies on a different source of variation and, therefore, on a different set of identifying

assumptions. Since both empirical approaches produce similar results, we conclude that our substantive results are neither driven by the assumptions underlying our RD design nor the unrepresentativeness of close elections.

Our alternative approach uses *within-campaign* variation in market beliefs—as measured by betting odds—about the outcomes of U.S. Senate elections from 2004 to 2010. Instead of comparing returns across firms who contributed to different candidates, this approach holds firm-candidate pairs fixed. Identification comes from high-frequency changes in the probability that a corporation’s preferred candidate ends up winning the race.

The sample for this analysis consists of 3,063 firm-candidate pairs across 119 Senate races that were listed on Intrade. As explained in the context of Figure 2, the betting price provides the market’s implied belief about the probability that a particular candidate will win (Wolfers and Zitzewitz 2004). For each firm-election, we focus on betting prices and stock returns in the 40 trading days leading up to Election Day. Restricting attention to a short period before the election ensures that the vast majority of corporations have already distributed their contributions. Furthermore, this is a period of intense campaigning, with often-significant swings in polls.

The within-campaign approach complements our RD design in a number of ways. First, since it *conditions* on “who gave to whom,” the resulting estimates are not subject to the concern that firms contributing to winners may be systematically different from those supporting losers. Second, our within-campaign design leverages additional, high-frequency variation, resulting in more statistical power for any given election. Thus, if one is especially interested in recent Senate races, then this alternative approach yields more informative results than the RD estimates. Third, this design draws on *all* elections for which market beliefs fluctuated over the

final weeks of the campaign. The evidence is, therefore, not limited to elections that were *ex post* close. In sum, a within-campaign design helps to address reservations about the internal and external validity of our RD results.

An important limitation of the within-campaign approach is that rich betting market data are only available for a small subsample of elections. Additionally, this design relies heavily on market efficiency. For our inferences to be valid, financial markets must accurately respond to high-frequency changes in candidates' electoral prospects, and betting markets have to be efficient enough for these changes to be incorporated into odds. Since betting markets are thinner than financial markets, the latter assumption may be problematic. If variation in betting prices is due to noise rather than genuine information, then our subsequent estimates would be attenuated. To speak to this issue, Appendix H presents a case study of the 2006 Senate race in Virginia. At least within this particular setting, bettors are quite responsive to new information. In particular, most of the meaningful changes in betting odds are explained by gaffes, campaign events, and new polls. We also note that restricting attention to the most liquid and, therefore, least noisy contracts on Intrade has virtually no impact on the results below.

To implement the within-campaign design, we estimate the following equation:

$$(2) \quad AR_{i,\Delta t} = \alpha + \beta \Delta \text{Pr}(\textit{Favored Candidate})_{i,j,\Delta t} + \varepsilon_{i,j,\Delta t},$$

where $AR_{i,\Delta t}$ denotes the abnormal return for firm i over time period Δt , and $\Delta \text{Pr}(\textit{Favored Candidate})_{i,j,\Delta t}$ is the change in the perceived winning probability of the company's preferred candidate in election j over the same time frame. The parameter of interest is β . It measures the increase in market value that would result from an electoral victory of the firm's favored candidate, relative to a counterfactual loss.

By regressing returns (i.e., changes in stock prices) on changes in the electoral prospects of candidates, the regression model in equation (2) is akin to a first-differences design and therefore holds all firm- and election-specific factors constant.¹⁵ Since we work with abnormal rather than unadjusted returns, our results also control for overall market conditions.

The crucial assumption for estimates based on (2) to be unbiased is that changes in beliefs about the electoral prospects of a particular candidate are uncorrelated with changes in other, unobserved factors determining the value of the firms that contributed to her campaign. This assumption would be violated if, for instance, corporate scandals had spillover effects on the supported politicians, or if financially troubled companies could withdraw earlier donations.

Table 5 presents the results from estimating equation (2) by ordinary least squares. Columns (1)–(3) use daily observations, while columns (4)–(6) rely on weekly data. For the latter analysis, the dependent variable is the CAR from Friday to Friday, and the independent variable is the change in the betting market probability over the same period. If one is concerned that daily fluctuations in betting odds are noisy and only weakly related to changes in election fundamentals, then the weekly analysis will be more informative.

Our most inclusive regression models in columns (3) and (6) also include firm-election fixed effects. We, therefore, not only condition on “who gives to whom,” but we also implicitly account for linear time trends in the electoral prospects of candidates and the performance of individual stocks. Identification in these specifications comes from temporary fluctuations

¹⁵ To see why the model in (2) conditions on who gives to whom, consider the data generating process

$$\log(\text{stock price})_{i,t} = \mu_{i,j} + \beta \text{Pr}(\text{Favored Candidate})_{i,j,t} + \varepsilon_{i,j,t},$$

where $\mu_{i,j}$ is a firm-candidate specific factor that is priced into the company’s stock. Taking the difference between time t and t' gives

$$R_{i,\Delta t} = \beta \Delta \text{Pr}(\text{Favored Candidate})_{i,j,\Delta t} + \Delta \varepsilon_{i,j,\Delta t},$$

where $\Delta t \equiv t' - t$ and $R_{i,\Delta t}$ denotes the stock’s return. Above, we rely on abnormal rather than simple returns to also account for overall market conditions.

around the respective trends. Arguably, the models in columns (3) and (6) rely on weaker identifying assumptions, but they require more faith in market efficiency.

Regardless of specification, all point estimates in Table 5 are substantively small and statistically indistinguishable from zero. Taking the coefficients at face value implies that the electoral victory of a company's supported candidate increases firm value by 0.02 to 0.17 percent. The evidence from this alternative research design is, therefore, remarkably consistent with our RD estimates.

Discussion

In this paper, we provide systematic evidence on the impact of “money in politics” by studying corporate campaign contributions in over 18,000 elections for governor, Congress, and state legislatures across three decades. Our research designs isolate the present value of the benefits a company derives if its favored candidate wins rather than loses the race. Surprisingly, we find no evidence that campaign contributions produce significant benefits for the firm. Our estimates are precise enough to statistically reject meaningfully large effect sizes as well as the possibility that campaign contributions are a good investment for individual executives. Since the victory of a supported candidate does not detectably increase firm value, we conclude that political quid pro quos between companies and individual candidates are not of first-order importance.

We should emphasize that our results only speak to the impact of *one* additional supported candidate winning office. The policymaking process is complicated, and electing one additional favored candidate may not be enough to shift the overall balance of power in a complex political system. In this sense, legislative systems with diffuse powers may protect

against the influence of special interests. However, even electing a favored governor—who has greater independent influence over policy—has no detectable effect.

Our findings raise an interesting puzzle. Scholars have argued that special interests give strategically (Barber 2016; Fourinaies and Hall 2014, 2016, 2017; Grimmer and Powell 2016), even buying access (Kalla and Broockman 2016). Yet, we find no evidence that contributions actually benefit the firm. Is access not valuable? In order for contributions to meaningfully affect firm value through this channel, campaign contributions need to buy access, access has to change the behavior of elected officials, that behavior has to translate into policy, which in turn must help firms. Determining *why*, or at which point, this causal chain breaks down is an important question for future research.

If corporations do not profit from their favored candidate winning office, why do they nonetheless contribute to political campaigns? Our results help to narrow down the set of possibilities. First, as in the signaling theories of Gordon and Hafer (2005) and Schnakenberg and Turner (2016), the benefits that accrue to the company may not depend on who wins the election. Second, companies might “give a little and get a little” (Ansolabehere, de Figueiredo, and Snyder 2003; p. 126). That is, the true benefits to the firm may be so small that we are not able to statistically detect them. Third, individual managers may derive consumption value from donating, and agency problems within the company may prevent shareholders from effectively monitoring the use of these funds.

From a normative standpoint, the last explanation is more of a problem for shareholders than for the democratic process itself. The second explanation poses only a “little” problem for democracy. And the welfare consequences of campaign contributions as signals are theoretically ambiguous. To be clear, our finding that corporations do not significantly benefit from

supporting political candidates does not necessarily imply that campaign donations have no distortionary effects. Even spending driven by consumption motives may alter election results in favor of the preferences of a small class of individuals; and our analysis is silent on the number of races that might have seen a different winner in the absence of such contributions. If the political leanings of high-ranking executives are not representative of the electorate as a whole, this might be reason enough to worry about giving by corporations and other wealthy donors. However, when it comes to the larger, more contentious issue of public policy being for sale, our results call for evidence-based optimism.

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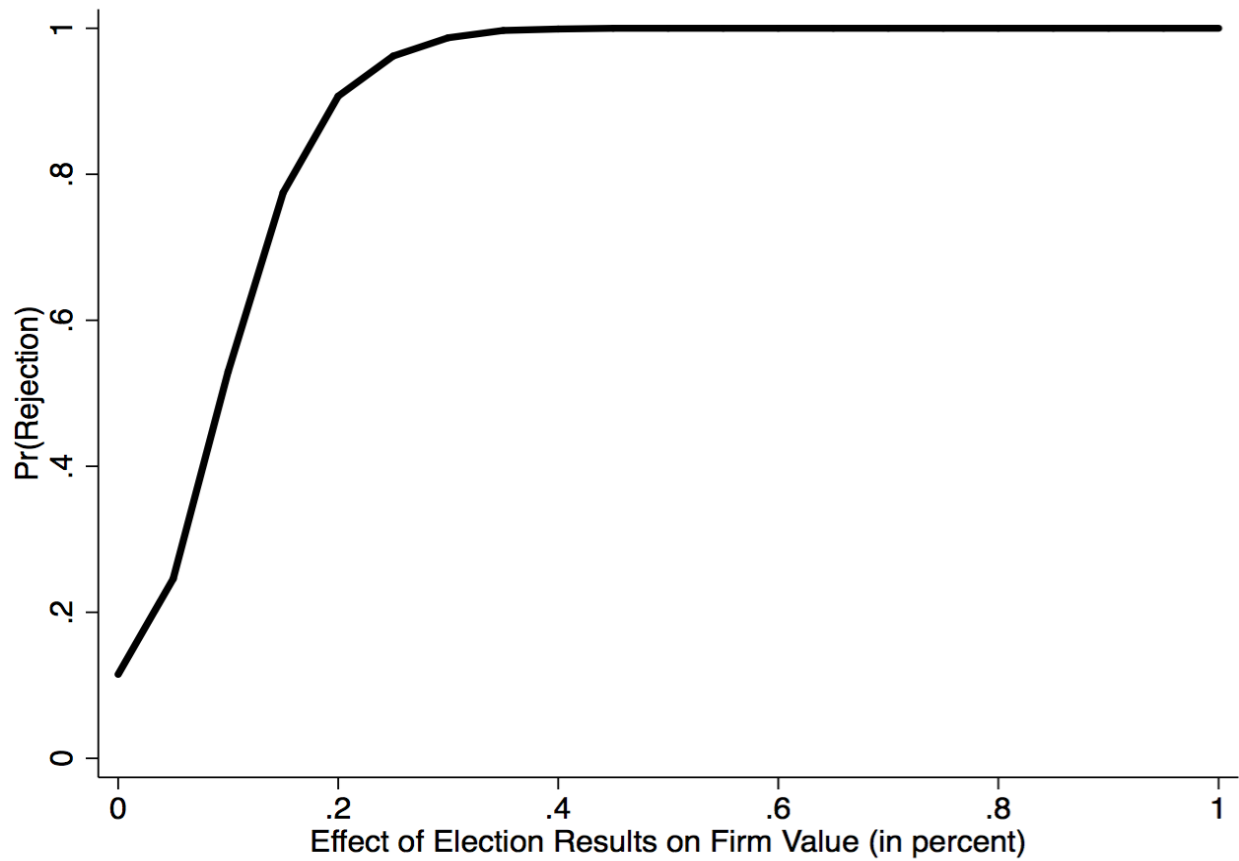
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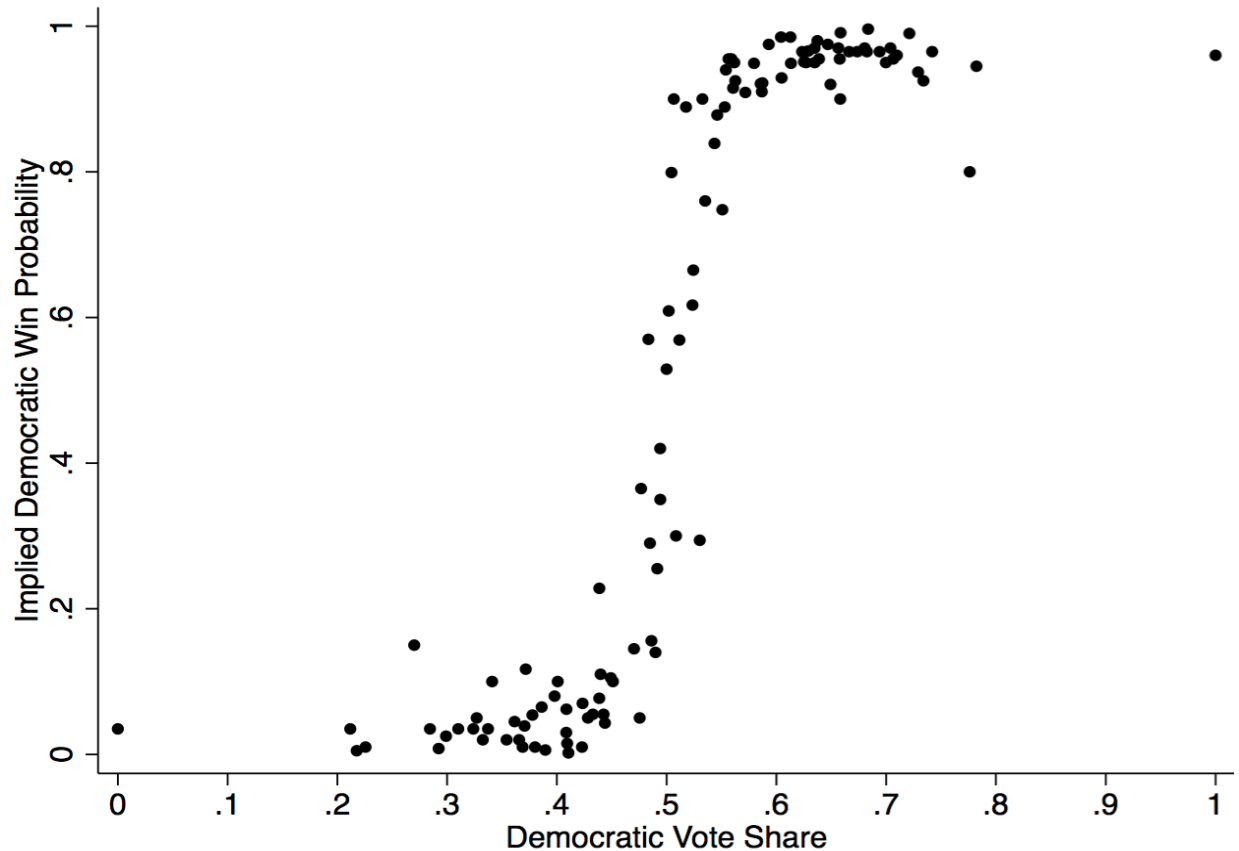
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Figure 1. Power Simulations



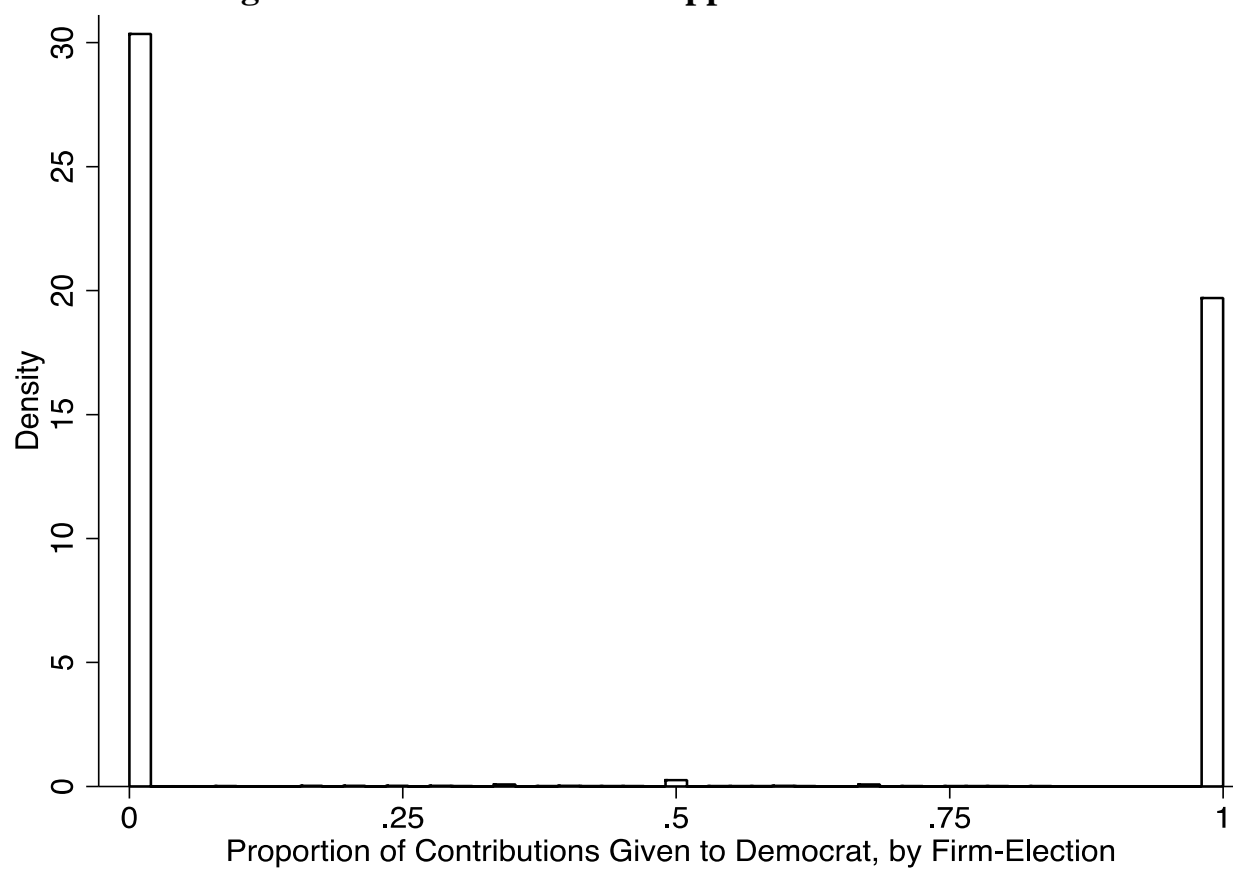
Notes: Figure shows the results of the power simulations described in the text. The horizontal axis indicates the simulated effect size, in percent. The vertical axis plots the empirical probability of rejecting the null hypothesis of no effect. For additional details on these simulations see Appendix D.

Figure 2. Close Elections were Ex Ante Unpredictable



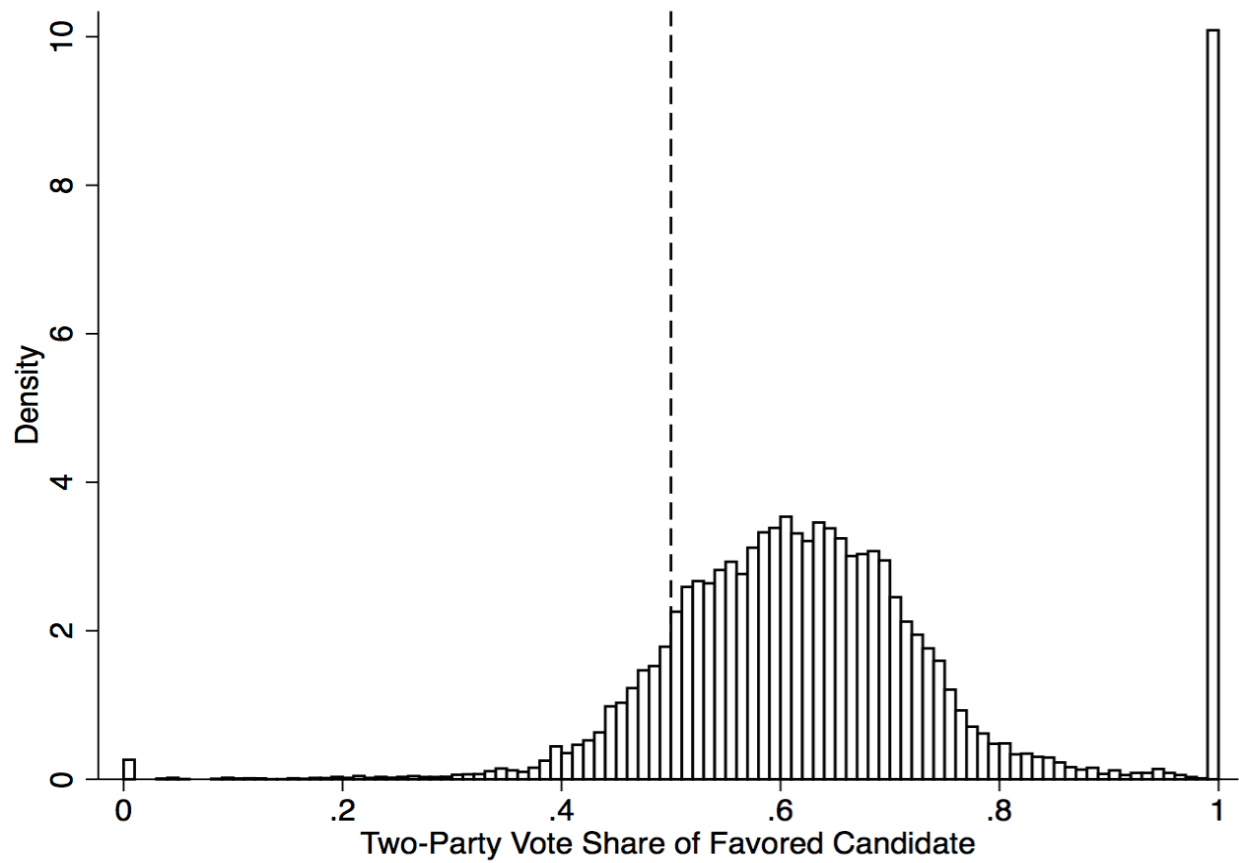
Notes: Figure plots the ex post vote share of the Democratic candidate againsts her probability of victory the day before the election, as implied by betting odds on Intrade. The sample consists of 119 U.S. Senate elections from 2004 and 2010. For additional details on these data see Appendix F.

Figure 3. Almost No Firms Support Both Candidates



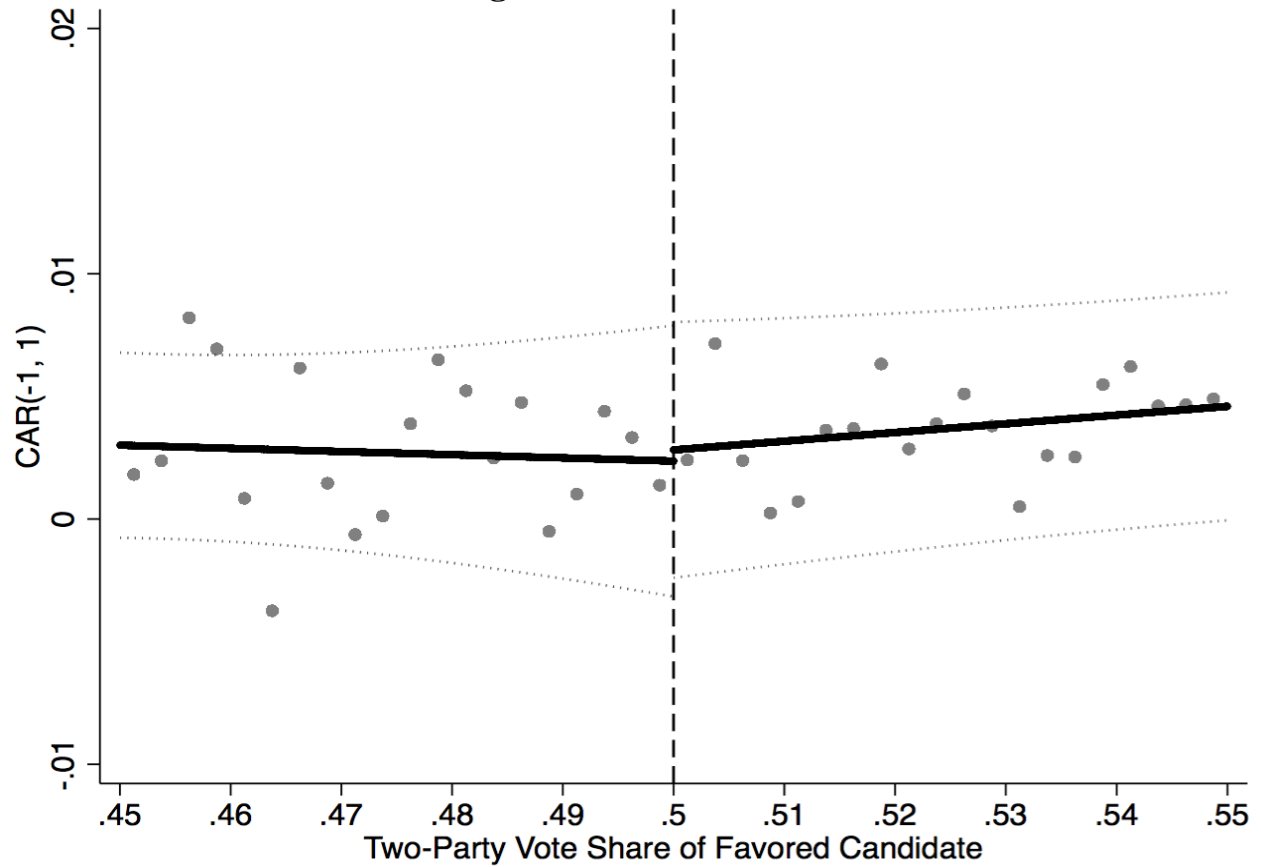
Notes: Figure depicts a histogram of the share of a company's race-specific contributions that is received by the Democratic candidate. The unit of observation is a firm-election.

Figure 4. Many (But not All) Firms Support Winners



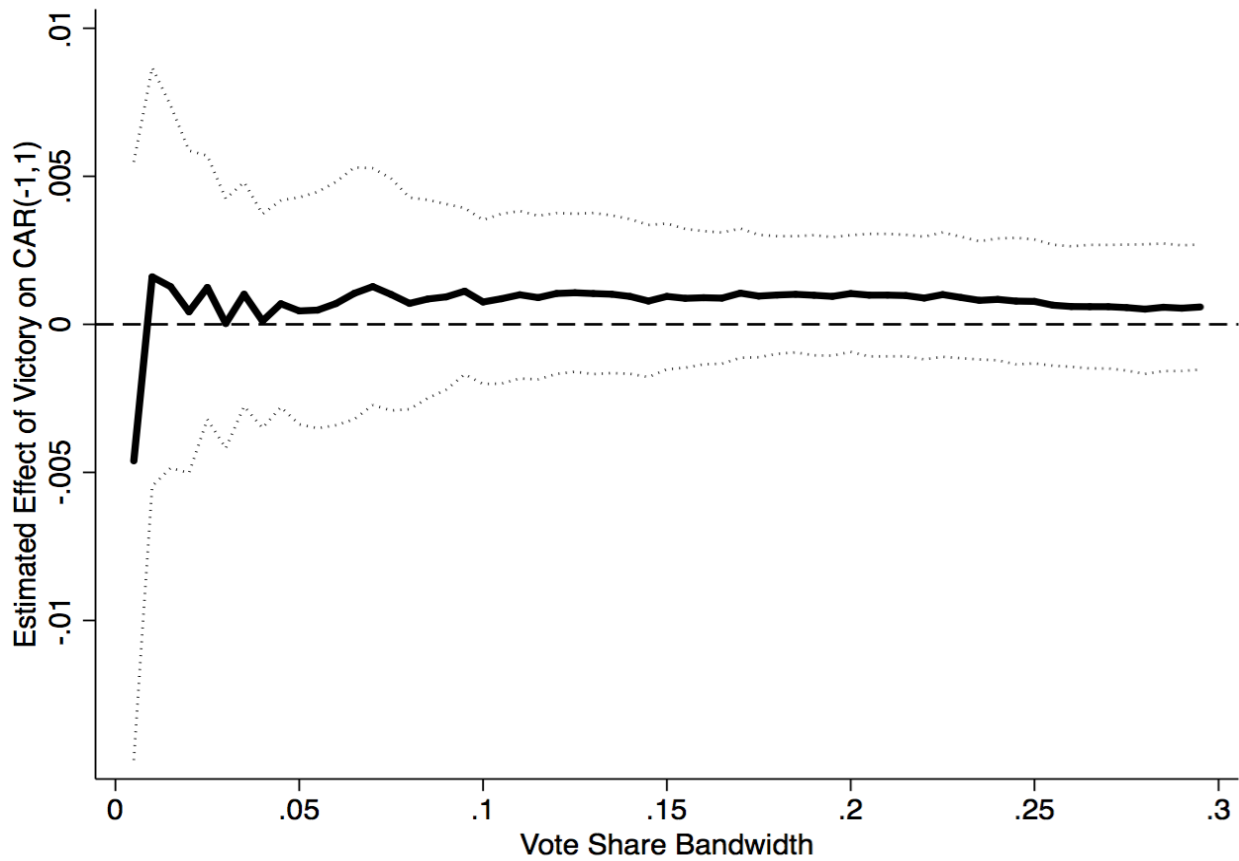
Notes: Figure shows a histogram of the realized vote share of the candidate receiving the contribution. Each observation corresponds to one firm donating to a particular candidate.

Figure 5. Main Result



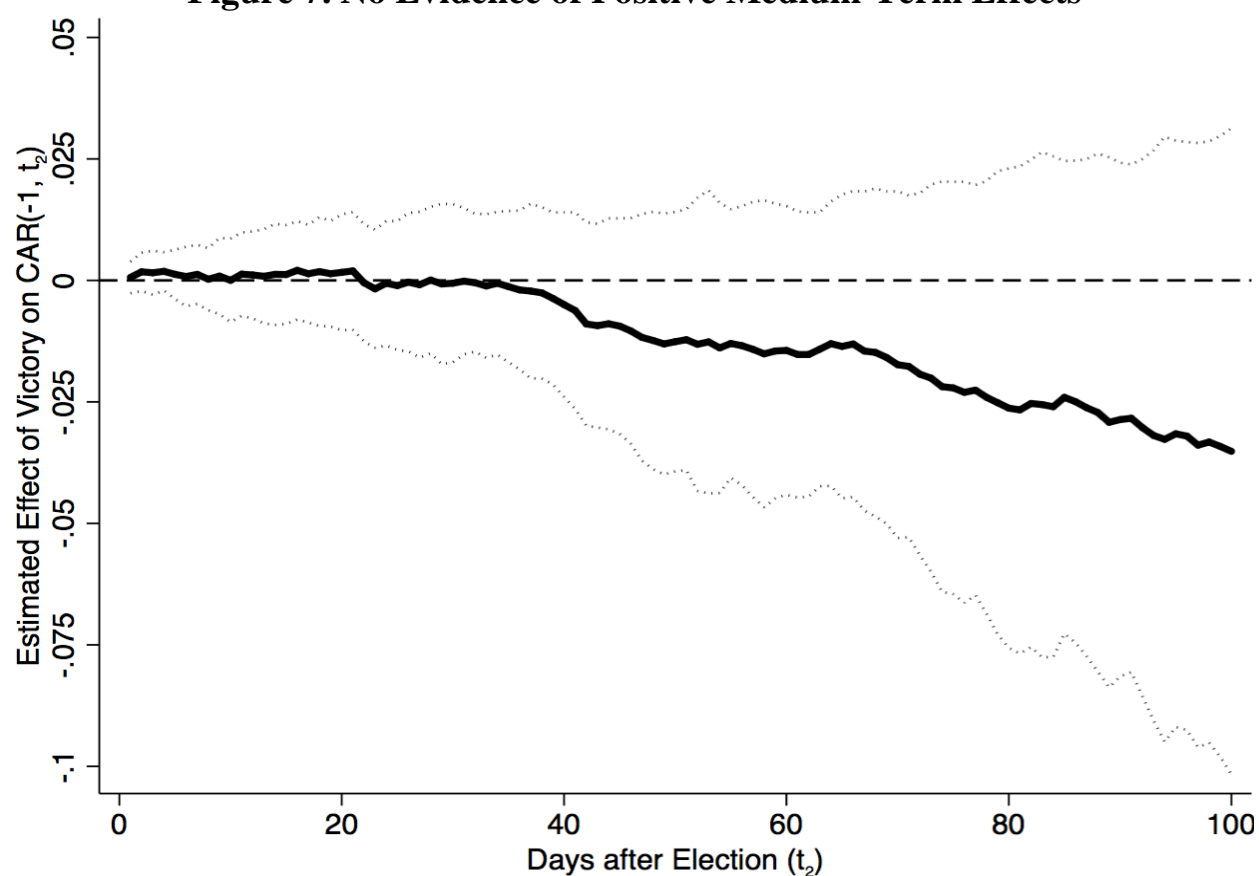
Notes: Figure depicts fitted values and confidence intervals from estimating equation (1) by ordinary least squares. Confidence intervals account for clustering by election cycle. Each dot corresponds to the mean of the dependent variable over a 0.25 percentage-point-wide interval.

Figure 6. Robustness across Bandwidths



Notes: Figure plots estimates of β in equation (1) and the associated confidence intervals for different bandwidths around the electoral threshold. Confidence intervals account for clustering by election cycle.

Figure 7. No Evidence of Positive Medium-Term Effects



Notes: Figure depicts estimated effects and confidence intervals for time horizons of up to 100 trading days after the election. Point estimates correspond to β in equation (1) with $CAR(-1, t_2)$ as dependent variable. Confidence intervals account for clustering by election cycle. The sample is the same as in Figure 5.

Table 1. Selection into "Who Gives to Whom," All Races

Variable	Mean		Winner = Loser [p-value]
	Loser	Winner	
log(Size of Donation)	6.521 [1.107]	6.423 [1.018]	.031
Donation to Incumbent	.421 [.494]	.791 [.407]	< .001
<i>Firm Characteristics:</i>			
log(Firm Value)	22.31 [2.16]	22.81 [2.10]	< .001
Return on Assets (%)	5.068 [7.468]	5.161 [7.517]	.586
log(Net Income)	19.57 [2.05]	20.02 [2.02]	< .001
Price-Earnings Ratio	20.96 [18.76]	22.13 [19.07]	.083
<i>Donations to other Candidates:</i>			
Number of Other Candidates	123.8 [183.3]	170.2 [225.9]	< .001
Fraction Winners	.83 [.14]	.89 [.10]	< .001
CAR(-7,-1)	.003 [.081]	.005 [.074]	.096

Notes: Table reports means and standard deviations of various candidate, firm, and donation characteristics for winning and losing candidates, respectively. The sample consists of all firm-candidate pairs in our data set. The rightmost column displays p -values from a t -test for equality in means, accounting for clustering by election cycle.

Table 2. Balance Tests, RD Sample

Outcome	Discontinuity		<i>p</i> -value
log(Size of Donation)	.0190	(.0539)	.729
Donation to Incumbent	-.0635	(.0445)	.175
<i>Firm Characteristics:</i>			
log(Firm Value)	.1121	(.1166)	.353
Return on Assets (%)	.1518	(.2786)	.595
log(Net Income)	.1046	(.1191)	.394
Price-Earnings Ratio	1.059	(.688)	.146
<i>Donations to other</i>			
<i>Candidates:</i>			
Number of Other Candidates	18.90	(7.854)*	.029
Fraction Winners	.0011	(.0048)	.825
CAR(-7,-1)	-.0016	(.0025)	.518

Notes: Entries are point estimates and standard errors for β in equation (1), with the variable listed in the leftmost column as outcome. Standard errors are reported in parentheses and account for clustering by election cycle. The sample consists of all close elections, i.e., races with a two-party vote share between .45 and .55. The rightmost column displays *p* -values for the null hypothesis that there is no discontinuity at the electoral threshold, i.e., $H_0: \beta = 0$. ** and * denote statistical significance at the 1%- and 5%-levels, respectively.

Table 3. Robustness Checks*A. Standard RD Specification*

Local Polynomial	Bandwidth (in p.p.)				IK-Optimal
	.5	1.25	2.5	5	
Constant	.0024 (.0023)	.0008 (.0018)	.0003 (.0012)	.0011 (.0010)	.0012 (.0008)
Linear	−.0046 (.0047)	.0014 (.0031)	.0012 (.0021)	.0005 (.0018)	.0009 (.0013)
Quadratic	−.0065 (.0061)	−.0006 (.0043)	.0005 (.0032)	.0006 (.0020)	.0006 (.0020)
Cubic	−.0049 (.0086)	−.0053 (.0045)	.0013 (.0044)	.0010 (.0034)	.0005 (.0022)
Number of Observations	3,038	7,333	14,956	29,390	--

B. RD Specification with Race Fixed Effects

Local Polynomial	Bandwidth (in p.p.)				IK-Optimal
	.5	1.25	2.5	5	
Constant	.0041 (.0024)	.0013 (.0013)	.0023 (.0019)	.0020 (.0014)	−.0006 (.0015)
Linear	.0036 (.0032)	.0052 (.0028)	.0021 (.0016)	.0018 (.0022)	−.0023 (.0038)
Quadratic	.0036 (.0032)	.0052 (.0029)	.0020 (.0016)	.0018 (.0022)	−.0007 (.0040)
Cubic	.0045 (.0056)	.0068 (.0035)	.0029 (.0030)	.0028 (.0017)	−.0219 (.0225)
Number of Observations	3,038	7,333	14,956	29,390	--

Notes: Entries are RD estimates and standard errors for the effect of a supported politician rising to office on CAR(-1,1). The upper panel modifies the regression model in equation (1) by considering polynomials in the running variable, while the lower panel also controls for race fixed effects. Standard errors are clustered by election cycle. The optimal bandwidth in the rightmost column is chosen according to the procedure of Imbens and Kalyanaraman (2011). ** and * denote statistical significance at the 1%- and 5%-levels, respectively.

Table 4. Analysis of Subsamples

	Estimate	Firm-Elections
Baseline	.0005 (.0018)	29,390
Donut Sample	.0007 (.0017)	28,786
<i>By Office:</i>		
Governors	−.0007 (.0050)	1,757
U.S. Senate	.0050 (.0037)	4,170
U.S. House	.0026 (.0021)	10,550
State Senate	−.0062 (.0032)	4,552
State House	−.0031 (.0028)	8,361
<i>By Donation Size:</i>		
≥ \$500	.0014 (.0022)	20,404
≥ \$100	.0020 (.0023)	13,204
≥ \$2,500	−.0000 (.0036)	3,938
<i>By Firm Value:</i>		
< 200 million	−.0096 (.0096)	1,827
200 million-1 billion	.0016 (.0029)	3,695
1–5 billion	−.0015 (.0035)	5,940
> 5 billion	.0026 (.0014)	13,524
<i>By Sector:</i>		
Manufacturing	.0023 (.0018)	13,378
Transportation and Infrastructure	−.0034 (.0031)	6,647
Finance, Insurance, and Real Estate	−.0028 (.0038)	4,678
Other	.0041 (.0038)	4,687
<i>Before vs After Citizens United:</i>		
Before	.0004 (.0019)	28,277
After	.0023 (.0028)	1,113
<i>By Number of other Winners Supported:</i>		
0	.0052 (.0037)	840
1–10	.0093 (.0042)*	3,181
11–100	−.0018 (.0026)	15,933
> 100	.0004 (.0017)	9,436
<i>By Number of Candidates Supported:</i>		
> 5	−.0000 (.0022)	27,440
> 50	.0002 (.0020)	17,906
<i>By Share of Same-Cycle Donations Given to Democrats:</i>		
< 1/3	.0020 (.0011)	11,370
1/3–2/3	−.0006 (.0022)	16,172
≥ 2/3	−.0024 (.0111)	1,848
<i>By Number of Firms Supporting the Winner:</i>		
1	−.0001 (.0096)	2,203
2–4	−.0037 (.0051)	10,146
5–14	.0041 (.0028)	10,853
≥ 15	−.0030 (.0041)	8,391
<i>By Control of Chamber (Post-Election):</i>		
Winner in Majority Party	.0043 (.0031)	13,741
Winner in Minority Party	−.0025 (.0026)	13,892

Notes: Entries are point estimates and standard errors for β in equation (1), estimated on different subsamples of the data. Standard errors are reported in parentheses and account for clustering by election cycle. All specifications restrict attention to close elections, i.e., races with a two-party vote share between .45 and .55. The rightmost column indicates the number of observations in a particular sample. ** and * denote statistical significance at the 1%- and 5%-levels, respectively.

Table 5. Within-Campaign Results*A. Daily Intervals*

	Abnormal Returns		
	(1)	(2)	(3)
$\Delta\text{Pr}(\text{Favored Candidate})$.0010 (.0018)	.0014 (.0021)	.0014 (.0021)
Firm-Cycle FE	No	Yes	No
Firm-Election FE	No	No	Yes
R-Squared	.000	.012	.012
Number of Observations	114,903	114,903	114,903

B. Weekly Intervals

	Abnormal Returns		
	(4)	(5)	(6)
$\Delta\text{Pr}(\text{Favored Candidate})$.0002 (.0080)	.0017 (.0086)	.0015 (.0097)
Firm-Cycle FE	No	Yes	No
Firm-Election FE	No	No	Yes
R-Squared	.000	.115	.115
Number of Observations	20,737	20,737	20,737

Notes: Entries are point estimates and standard errors for γ in equation (2). Standard errors are reported in parentheses and account for clustering by election cycle. All specifications restrict attention to the last 40 trading days prior to the Election Day. The upper panel uses daily data, whereas the lower one aggregates observations at the weekly level. ** and * denote statistical significance at the 1% and 5% levels, respectively.

Online Appendix to “Quid Pro Quo? Corporate Returns to Campaign Contributions”

A. Additional Robustness Checks

To verify the robustness of the results in the main text, we have conducted a series of sensitivity and robustness analyses. Following the best practices outlined by Imbens and Lemieux (2008) and Lee and Lemieux (2010), Appendix Tables A.1–A.3 show results with respect to different combinations of bandwidth and choice of polynomial in the running variable.

In addition, Table A.1 presents estimates using CAR(-1,5) instead of CAR(-1,1) as outcome, while Table A.2 relies on cumulative *raw* rather than abnormal returns. Table A.3 probes the robustness of our estimates with respect to including race or electoral-cycle fixed effects. Broadly summarizing, regardless of bandwidth, polynomial specification, outcome measure, or choice of fixed effects, there is no evidence to conclude that a supported candidate rising to office affects firm value. In fact, 13 out of the 132 point estimates in Tables A.1–A.3 are negative, and only 2 are statistically significant at conventional levels—less than would be expected to arise by chance if the estimates were jointly independent random variables.

Finally, Table A.4 shows that our results are robust to excluding either the biggest donors or the smallest ones, dropping very large or very small firms, as well as restricting attention to presidential, midterm or odd-year elections.

B. Calculation of Abnormal Returns

We calculate abnormal returns using the market model, which is standard in the finance literature (see, e.g., Campbell, Lo and MacKinlay 1997). The abnormal return of firm i on day d , $AR_{i,d}$, is defined as

$$(B.1) \quad AR_{i,d} \equiv r_{i,d} - (\hat{\alpha}_i + \hat{\beta}_i m_d)$$

where $r_{i,d}$ is the return of company i 's stock on that day, and m_d denotes the return of the value-weighted market portfolio. $\hat{\alpha}_i$ and $\hat{\beta}_i$ are firm-specific parameters that need to be estimated. Intuitively, β_i measures how sensitive a stock's returns are with respect to overall market

conditions, while α_i indicates whether the firm tends to under- or overperform relative the market. The abnormal return of a firm is, thus, the residual between the realized return of the stock and its predicted performance based on the stock market as a whole. Absent special circumstances affecting the value of a particular set of firms, abnormal returns ought to equal zero on average. Systematically positive or negative values, however, are suggestive of events that led market participants to revise their valuations of the company.

In line with the typical approach in the literature, we obtain $\hat{\alpha}_i$ and $\hat{\beta}_i$ by estimating

$$(B.2) \quad r_{i,d} = \alpha_i + \beta_i m_d + u_{it}$$

on data from a *pre*-period of 200 trading days. To ensure that the estimates are not confounded by the outcome of the election itself, we set the end of the pre-period to 30 days before the event.

While our point estimates are insensitive to residualizing returns in this fashion, as evidenced by the comparison of Table 1 and Appendix Table A.2, using abnormal rather than raw returns increases statistical precision because it implicitly controls for market movements.

C. Vote Share Distribution

Appendix Figure A.1 depicts superimposed histograms of the distribution of incumbent and challenger vote shares, excluding uncontested races. Unsurprisingly, the bulk of donations goes to incumbents, especially winning incumbents.

In order to rule out systematic sorting of donations around the electoral threshold, Figure A.2 shows the density of the vote share of the supported candidate. A discontinuity at 0.5 would undermine the validity of the identifying assumption in our RD design. To test for such a discontinuity, we implement the procedure of McCrary (2008) and find a small difference of 0.095 log-points.

Calculating correct p -values for the null hypothesis of no discontinuity requires us to account for nonindependence across observations (due to multiple firms giving to the candidates in a particular race). We do so using a simple permutation test. Specifically, we create 5,000 surrogate data sets in which the vote shares of the candidates in a given race are randomly switched with probability one half. Next, we estimate the discontinuity at the electoral threshold, and compute the number of instances in which the absolute value of the estimated discontinuity is bigger than that in the original data set. By construction, randomly permuting the vote shares of candidates eliminates (potential) sorting around the electoral threshold. Any observed

discontinuity must be due to small sample variability alone. Hence, *under the null hypothesis*, the fraction of placebo estimates that are greater than the observed discontinuity measures the probability of obtaining an estimate at least as large as the actual one. In our data, this probability equals 43.34, which means that it is *not* possible to reject the null of no discontinuity around the electoral threshold.

Figure A.3 replicates the preceding analysis using the vote share of the Democratic candidate as the running variable. Visual inspection shows that there is no discontinuity around the electoral threshold, which is supported by a discontinuity point estimate of 0.026 and a p -value of 87.08. In contrast with Figure A.2, more mass is concentrated to the left of the threshold, indicating that donations to winning candidates are disproportionately channeled to Republicans.

D. Power Calculations

In this section, we conduct simulations in order to assess the statistical power of our econometric machinery. The objective of these simulations is to compute how often we would reject the null hypothesis of no effect if the true impact was of a given magnitude. In addition, the simulations allow us to speak to the empirical size of our setup, i.e., the probability of rejecting the null of no effect when the null is true. Our power calculations exploit the fact that elections take place on a specific day, while stock markets operate throughout the year.

In particular, we create 5,000 surrogate data sets, in which we randomly assign a placebo election date for each cycle in the original dataset. As potential election dates, we consider all trading days starting from the first Tuesday after the first Monday of February of the actual election year, and up to 180 trading days after. For each surrogate dataset we calculate abnormal returns for contributing firms around the randomly assigned placebo election date, and we increase these returns by a treatment effect of known size. We do this for 21 different effect sizes, ranging from a 0 percent effect per donation up to a 1 percent effect (in increments of 0.05 percent). Using the regression specification in equation (1), we then estimate the effect of donations on cumulative abnormal returns.

For each simulated effect size, Figure 1 in the main text plots the fraction of surrogate datasets for which we reject the null hypothesis of no effects of donations on firm value. Reassuringly, the exercise indicates that the null of no effect would have been rejected with high

probability if the true effect of donating to a bare winner on firm value had been larger than 0.2. Relatedly, the analysis of the case shows that the empirical size of our econometric setup is not too far from the theoretical size of 5 percent, as the null of no effect is rejected in only 11.50 percent of the surrogate datasets. Furthermore, about 69.6 percent of the placebo effects are larger (in absolute value) than the point estimate in the original data set (cf. Appendix Figure A.4). We, therefore, conclude that the point estimate obtained in our main analysis is well within the range of possible values that one would expect to see if the null hypothesis of no effect holds true.

E. Permutation Test

In this section, we conduct a permutation test of the effects of campaign contributions on stock market returns. Our objective is to compute the sampling variability of our main point estimate *imposing* the null hypothesis of no effect. To this end, we create 10,000 surrogate data sets, in which we randomly flip the vote shares of the two candidates in a given election. Using the regression model in equation (1), we then compute a placebo effect of donations on abnormal returns for each newly created instance of the data. By construction, under the null, the distribution of placebo estimates coincides with the sampling distribution of the estimand in (1).

Figure A.5 visualizes the results from this test. The dashed line corresponds to the estimate reported in the main text, which appears to lie well within the range of plausible values. More specifically, under the null of no effect, we would expect to obtain an estimate that is larger (in absolute value) than ours with probability 72.9. Hence, the results from this permutation test imply that it is not possible to reject the claim that the electoral victory of a supported candidates does not affect firm value.

In addition, we note that, based on the regression specification in equation (1), we would reject the null $H_0: \beta = 0$ in about 5.91 percent of instances in favor of $H_1: \beta \neq 0$. Given that this number is close to the theoretical size of 5 percent, we conclude that our clustering scheme appears to perform well.

F. Data Appendix

This appendix provides a description of all the data sources and the process that we followed to match firm contributions data to stock market data.

F.1 Firm PAC Contributions to Candidate PACs

Campaign contributions data was obtained from DIME, the Database on Ideology, Money in Politics, and Elections (Bonica 2013).

Donation Size refers to the dollar amount given by a corporate PAC to a candidate in a given election cycle.

F.2 CRSP stock returns data

Data on publicly traded firms' stock market returns were obtained from CRSP, the Center for Research in Security Prices. We extracted daily returns data from 1979 to 2010. CRSP daily returns incorporate a price adjustment that accounts for stock splits, as well as an adjustment that corrects for dividend distributions. For the market return we use the Total Return Value-Weighted Index, which is a market-capitalization weighted index of the daily returns of all firms belonging to the S&P 500.

We also extracted the SIC codes of each company from CRSP. Following the SIC Division Structure (osha.gov/pls/imis/sic_manual.html), we assigned firms into 10 different sectors (A to J). For the analysis by sector in Table 3 in the main text, we separately considered *Manufacturing* (Division D), *Transportation, Communications, Electric, Gas, and Sanitary Services* (Division E) and *Finance, Insurance and Real Estate* (Division H). The remainder of firms are grouped under the *Other* category.

F.3 General Election Returns Data

General elections data for the U.S. Senate, U.S. House, governor, and state legislatures from 1980 to 2010 was provided by Jim Snyder, and represent an extended version of the data set used in Ansolabehere and Snyder (2002).

Two-Party Vote Share of Favored Candidate is defined as the percentage of the vote obtained by the candidate receiving the contribution over the total of votes for the candidates of the Democratic and Republican parties. Notice, in our final dataset an observation corresponds to a firm-election pair.

Cycle is defined as the two-year election cycle.

Year is defined as the year that the election took place. In 97 percent of the cases the *cycle* and *year* variables coincide. The remaining 3 percent of cases, where elections take place in odd years, pertain to state legislature and gubernatorial elections in KY, LA, MS, NJ and VA.

F.4 Matching contributions data to election returns and stock market data

The DIME database of campaign contributions contains both individual and corporate donations to political candidates. Restricting attention to the latter, we assigned corporate donations to the corresponding stock ticker code by matching the corporation names in both the DIME and CRSP dataset.

Our matching procedure is based on a standard algorithm for record linkage. Specifically, after standardizing company names, we use the *relink* package in STATA, which employs a modified bigram string comparator. A bigram algorithm compares two strings using all combinations of two consecutive characters within each string. The bigram comparison function then returns a value between 0 and 1, indicating the total number of bigrams that are in common between the strings divided by the average number of bigrams in the strings. After applying the algorithm to the pair of datasets, we *manually* reviewed each match to ensure that the matches proposed by the algorithm appear to be correct.

After matching corporate PAC contributions to election PACs with the corresponding election returns and stock market data, we constructed the following variables:

Donation to Incumbent is a indicator variable that takes value one if the candidate that received the contribution was an incumbent in that specific election.

Number of Other Candidates Supported is the number of other candidates supported by the corporate PAC in the election cycle, across all electoral offices.

Number of Other Winners Supported denotes the number of other candidates supported by the corporate PAC in the election cycle that ended up winning their respective electoral races.

Fraction Winners denotes the fraction of other candidates supported by the firm in the electoral cycle that ended up winning their respective electoral races.

F.5 CRSP/Compustat Merged Database for firm characteristics

We obtained data on firm characteristics from the CRSP/Compustat Merged Database. For theoretical reasons, we focus on pre-determined variables, which means that we extracted firm characteristics for the year *prior* to the elections under consideration.

Firm Value refers to the market capitalization of the firm in the year before the election took place. This variable equals the price of the stock at the end of the year times the number of outstanding shares.

Net Income denotes the income or loss reported by a company after subtracting expenses and losses from all revenues and gains.

Return on Assets (%) is calculated as *Net Income* divided by the total value of the firm's assets.

Price-Earnings Ratio (%) is calculated by dividing the stock price of the company at the end of the year over earnings per share. Earnings per share is calculated as *Net Income* divided by the number of outstanding shares. Following standard practice in the finance literature, we exclude observations with a negative *Net Income*, as this would lead to nonsensical, negative PE ratios. Further, we exclude obvious outliers, i.e., observations with a PE ratio higher than 200. In the vast majority of cases, extremely high PE ratios are due to nearly zero earnings per share.

F.6 Partisan Control of Chamber

Table 4 in the main text contains a robustness check where the sample is split according to whether the winning candidate in the race joins the majority or minority group in the legislative chamber (we exclude races for governor). In order to perform this sample split we collected data on party control of Congress and American legislatures. For the latter, we made use of the Partisan Balance Data, from the Carl Klarner Dataverse (Klarner, 2013).

F.7 Intrade Data

Intrade data on betting market prices for U.S. Senate elections were kindly provided by Koleman Strumpf. Intrade.com was a web-based trading exchange where contracts on the realization of various events were traded. We use data on the daily betting price of 119 U.S. Senate elections between 2004 and 2010. These prices are defined relative to a basis of 100. To give a specific example that illustrates how these markets work, suppose that the contract for the IL Democratic candidate winning the 2008 Senate election trades at 25 dollars. If a bettor decides to wager 25 dollars in favor of the Democratic candidate, she obtains 100 dollars in case the Democratic candidate wins the election. The bettor loses the 25 dollars if the Democratic candidate loses the election. Wolfers and Zitzewitz (2004) show that, under mild assumptions, the betting price provides the market's implied belief about the probability that a particular candidate will win.

To perform the within-election analysis we extracted all betting prices for the Democratic and Republican candidates pertaining to the last 40 trading days before Election Day, and matched the betting prices time series to the data on election returns.

$\Delta Pr(\textit{Favored Candidate})_t$ denotes, in the case of the daily analysis, the difference between the betting price of the candidate at trading day t and trading day $t - 1$. For the weekly analysis, it denotes the difference between the betting price of the candidate on the Friday of week t and the Friday of week $t - 1$.

Our final sample does not include the following elections: IN 06, SD 10 and CT 06. In the first case, the Republican candidate, Richard Lugar, faced a libertarian, Steve Osborn. Since there was no Democratic candidate we exclude this election. In the second case, the Republican candidate, John Thune, was unchallenged. In the last case, Joe Lieberman lost the Democratic primary to Ned Lamont, but decided to run as an independent. He ended up beating both Lamont and Alan Schlesinger, the Republican candidate, in the general election. Again, we exclude this election since it does not neatly fit into a framework of two-way races.

G. Replication of Akey (2015)

In the main text, we state that we have not been able to replicate the results of Akey (2015), and we argue that estimates based on the authors' small sample are highly fragile. In what follows, we provide evidence in support of this claim.

The sample of Akey (2015) consists of donations that were made to candidates in 13 special elections to the U.S. Senate (1) and House (12). In each case, the margin of victory was smaller than 5 percentage points. For the full list of candidates participating in these elections, as well as election dates and results, see Table 3 in Akey (2015).

As the author refused to share the data and computer programs necessary to replicate the reported results, we independently obtained information on donations to the candidates in question from the Federal Elections Commissions (FEC). We manually identified all campaign contributions from corporate PACs, and matched them to the respective firms' stock market data, paying close attention to only include contributions that were made before the election took place. Following the description in Akey (2015), we exclude donations from corporate PACs that gave to both candidates in a particular race. Our replication sample consists of 286 firm-candidate pairs, which exceeds the number of observations in Akey's main analysis by 22 percent. Unfortunately, the data characterization in Akey (2015) is not detailed enough to reconcile this difference without access to the author's data.

Appendix Table A.6 attempts to replicate Akey's Table 4A, i.e., the author's main result. To this end, we report estimates based on equation (1) in Akey (2015). That is, we estimate a standard RD specification without fixed effects, and we cluster standard errors at the firm level. As in Akey (2015), the dependent variable for all specifications in this table is cumulative abnormal returns from the day before until 5 trading days after the election, i.e., $CAR(-1,5)$. We follow Akey and calculate CARs based on the Fama-French three-factor model.

Unlike Akey, we present results for different combinations of bandwidth and polynomial of the running variable. While the estimate based on a bandwidth of 2.5 percentage points and a quadratic functional form matches its counterpart in Akey (2015) reasonably closely—0.0327 vs 0.0260—the remaining entries in Table A.7 demonstrate that the results as a whole are extremely sensitive to the choice of bandwidth as well as functional form. Importantly, none of our estimates are positive and statistically significant. Ten out of sixteen coefficients are, in fact, negative.

Appendix Table A.8 provides further evidence that Akey's sample of 13 special elections yields sensitive and, therefore, ambiguous results. The estimates in this table are based on the same specification as their counterparts in the previous table, but rely on $CAR(-1,1)$ and $CAR(-$

1,5) computed according to the market model as dependent variables.¹ Again, the coefficients are very fragile.

In sum, we fail to replicate the main results of Akey (2015). Recreating the author's sample as closely as possible based on the brief description in the former paper, we obtain some estimates that are of the same sign and comparable size, while many others are highly dissimilar. We conclude that results based on this sample are not robust and, therefore, inconclusive.

H. Is Variation in Betting Market Prices Meaningful? A Case Study

For our secondary research design, we rely on within-campaign variation in market beliefs about the outcomes of U.S. Senate elections between 2004 and 2010. One potential concern is that electoral betting markets are thin, and, therefore, much of the variation in betting prices might be noise. If correct, this would attenuate our point estimates. To make the case that variation in betting odds likely reflects genuine information and to show that betting markets respond in reasonable ways to campaign events, we present a case study of one of the elections in our analysis—the 2006 U.S. Senate race in Virginia. Appendix Figure A.6 plots the implied probability of a Democratic vs. Republican victory based on betting odds for the 150 days leading up to the election. The figure also shows the results of every poll reported by Real Clear Politics, along with several key campaign events.

The 2006 U.S. Senate race in Virginia pitted Republican incumbent George Allen against Democratic challenger Jim Webb. Early in the campaign, Allen was a clear favorite. In polls, he consistently received more than 55 percent of the two party support, and the betting markets suggested that his chances of victory were more than 80 percent. On August 8, it was discovered that Allen, a pro-life conservative, owned stock in the drug company that produces the Plan B contraceptive pill. Further, on August 11, he made a major campaign gaffe when, at a campaign event, he referred to S.R. Sidarth, a volunteer for his opponent of Indian ancestry, as *Macaca*. After this gaffe, Allen lost most (but not all) of his lead in the polls, and the betting markets reduced their beliefs about his victory to about 70 percent.

During the final two weeks of the campaign, another event played out in Webb's favor. On October 26, Allen and other conservatives criticized Webb's novels for their sexually explicit

¹ Given that the correlation between CARs computed under the market model and CARs based on the Fama-French three-factor model exceeds .96, the estimates in Table A.8 would remain virtually unchanged if we used the latter.

content. Webb had been an award-winning journalist and respected author. He vehemently defended himself by pointing out that his books were inspired by real events, had been placed on the Marine Corps reading list, and were taught in college literature classes around the country. This exchange, among other factors, appeared to further harm Allen's reelection chances. For the first time, the rolling average of polls gave a very slight edge to Webb, and the betting markets similarly revised their beliefs, predicting a 50-50 tossup.

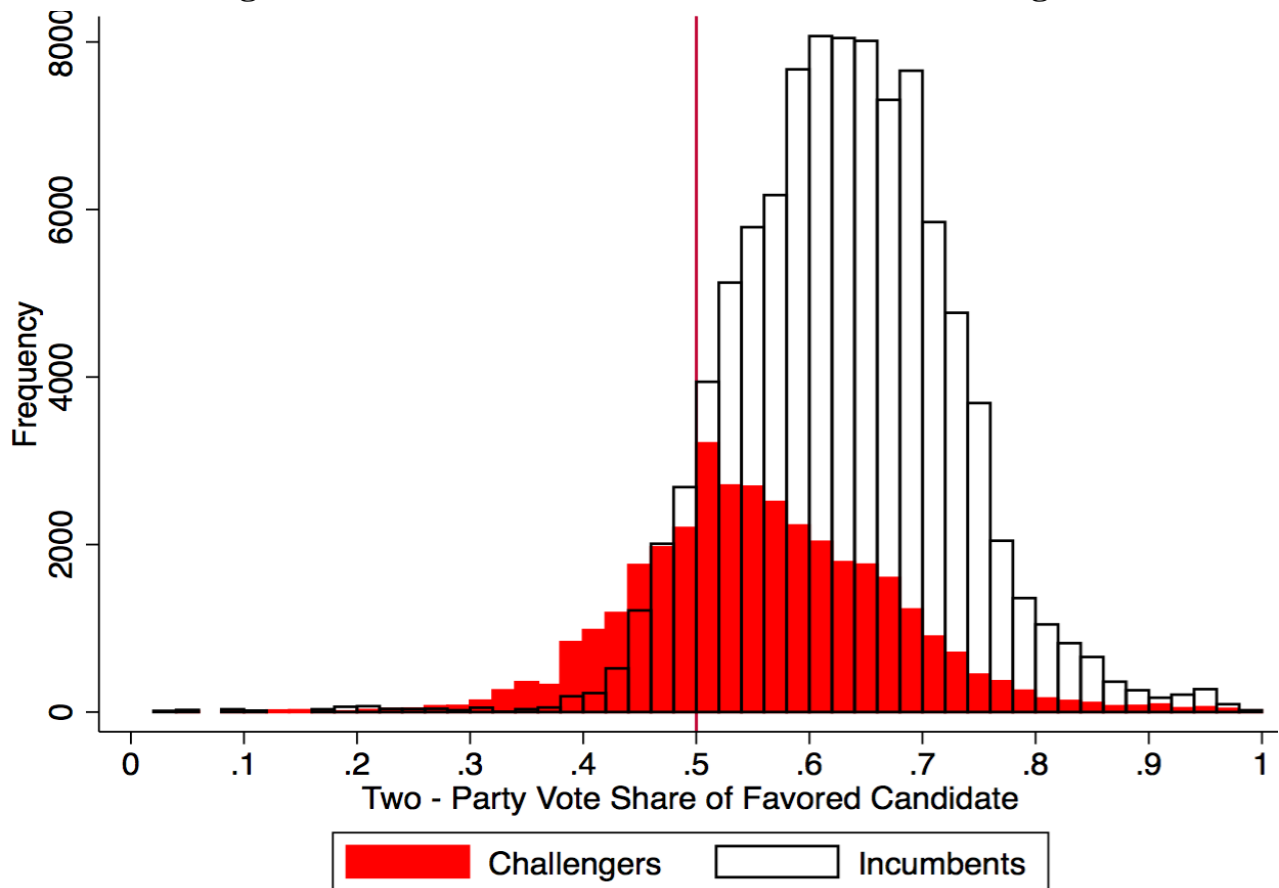
On Election Day, Webb won with 50.2 percent of the two-party vote. The election was so close that Allen did not concede until virtually all the votes were counted on November 9—almost 48 hours after the polls closed. Although Allen was holding out, almost all of the uncertainty in the betting market had already been resolved. The markets gave Webb a 50 percent chance the day before the election, a 65 percent chance on Election Day, a 95 percent chance the day after the election, and a 99 percent chance two days after the election.

This case study illustrates that political betting markets appear to be quite responsive to poll results and campaign events. The evidence, therefore, suggests that variation in betting odds is meaningful. Furthermore, consistent with the assumptions of our RD design, the market was uncertain about the outcome of this close election before Election Day, and almost all of that uncertainty was resolved the day after.

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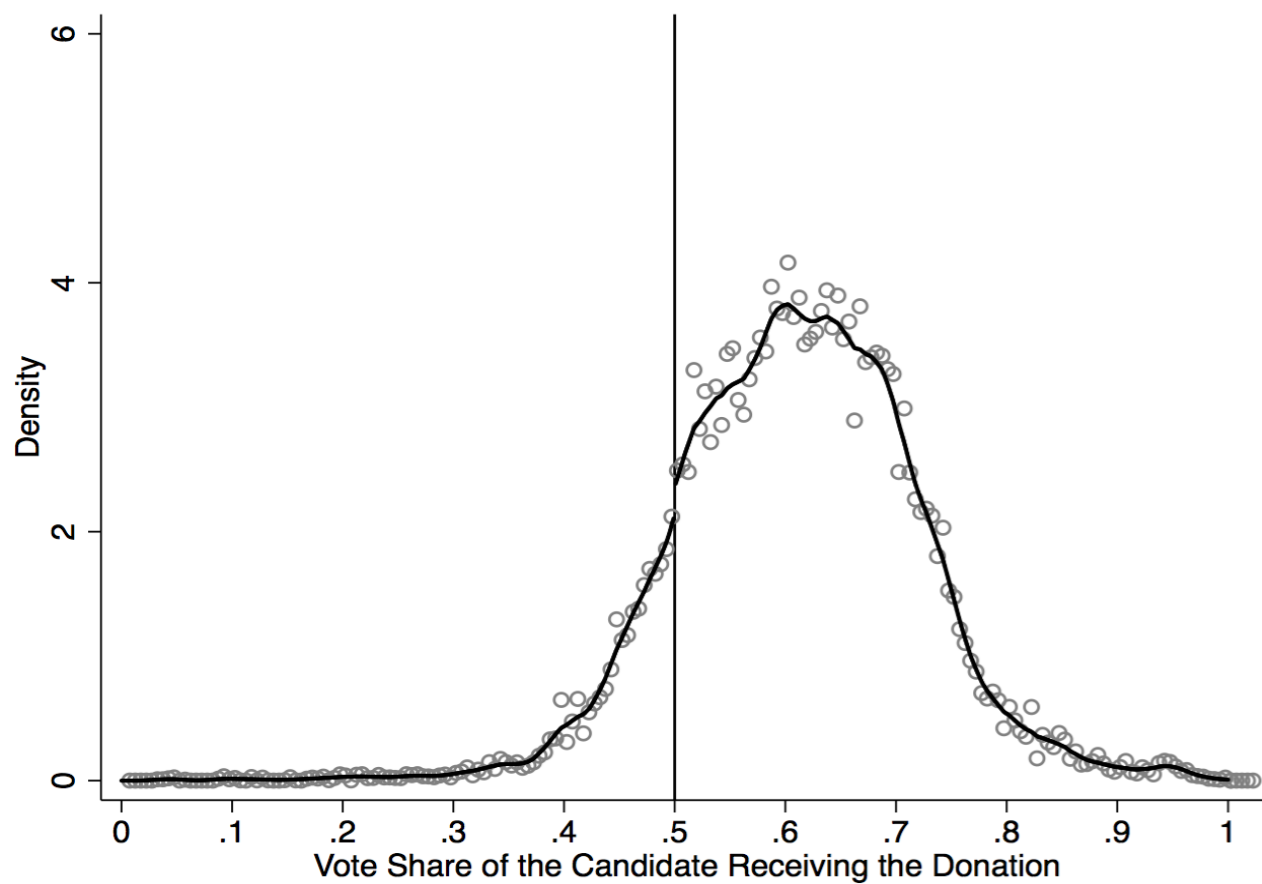
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Figure A.1. Vote Shares of Incumbents and Challengers



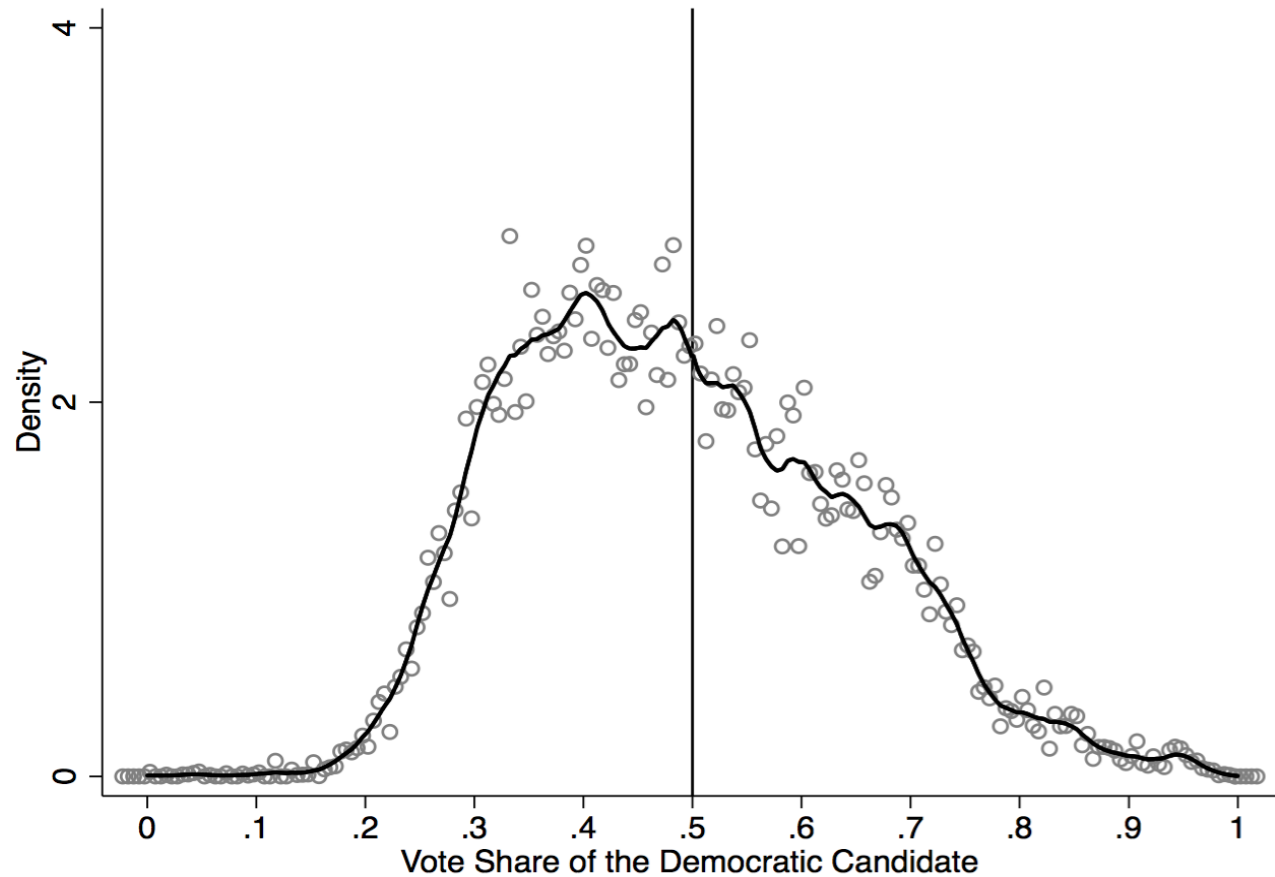
Notes: Figure depicts superimposed histograms of the vote shares of incumbents and challengers, excluding uncontested races. "Challengers" includes candidates that compete against an incumbent as well as all candidates that compete for an open seat. An observation consists of a firm-election pair, and the vote share variable refers to the two-party vote share of the candidate that received the donation.

Figure A.2. Vote Shares of Supported Candidates



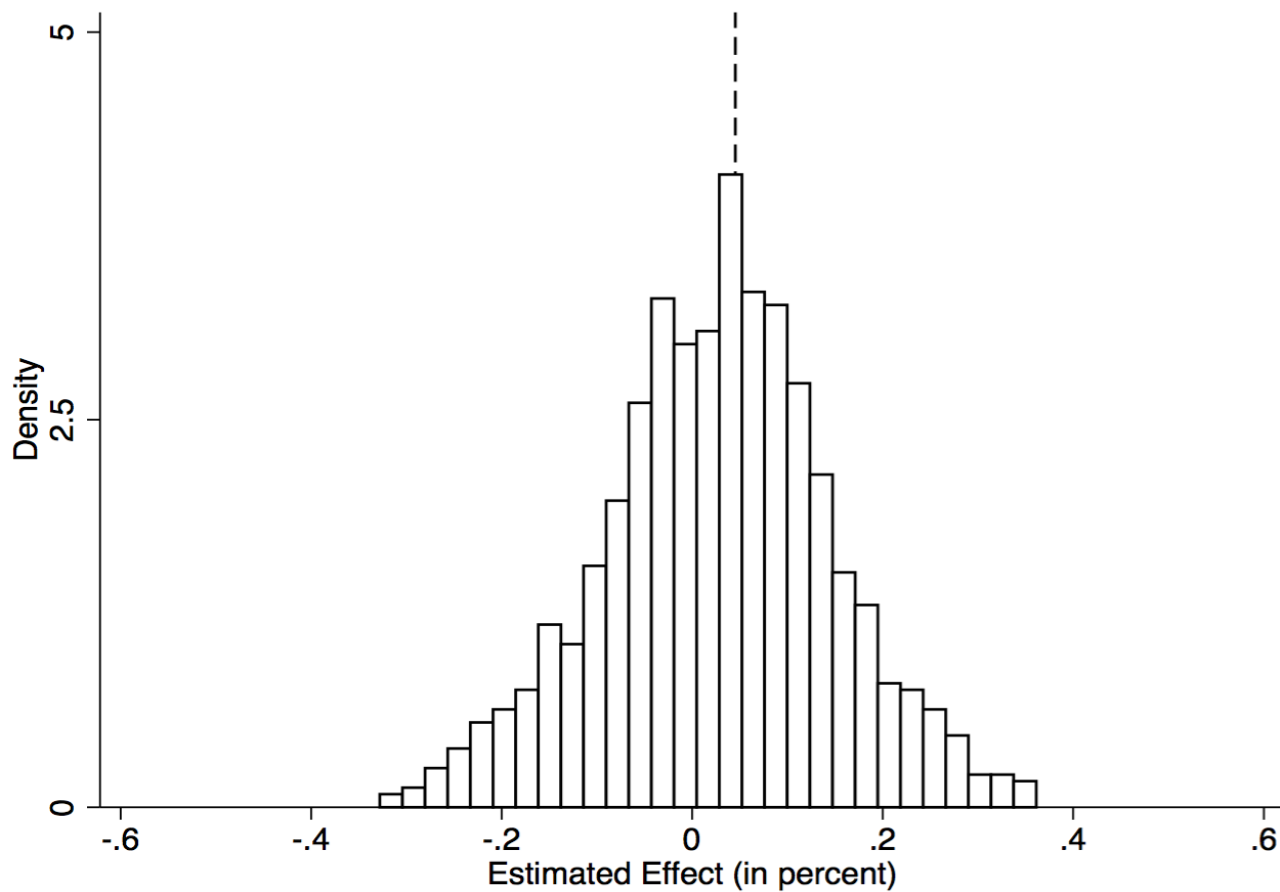
Notes: Figure depicts the density of the vote share of the candidate receiving the donation, excluding uncontested races. Circles correspond to averages over 0.5-percentage-point-wide bins. Estimates of the associated density are based on the procedure of McCrary (2008) with a bandwidth of 2.5 percentage points.

Figure A.3. Vote Share of the Democratic Candidate



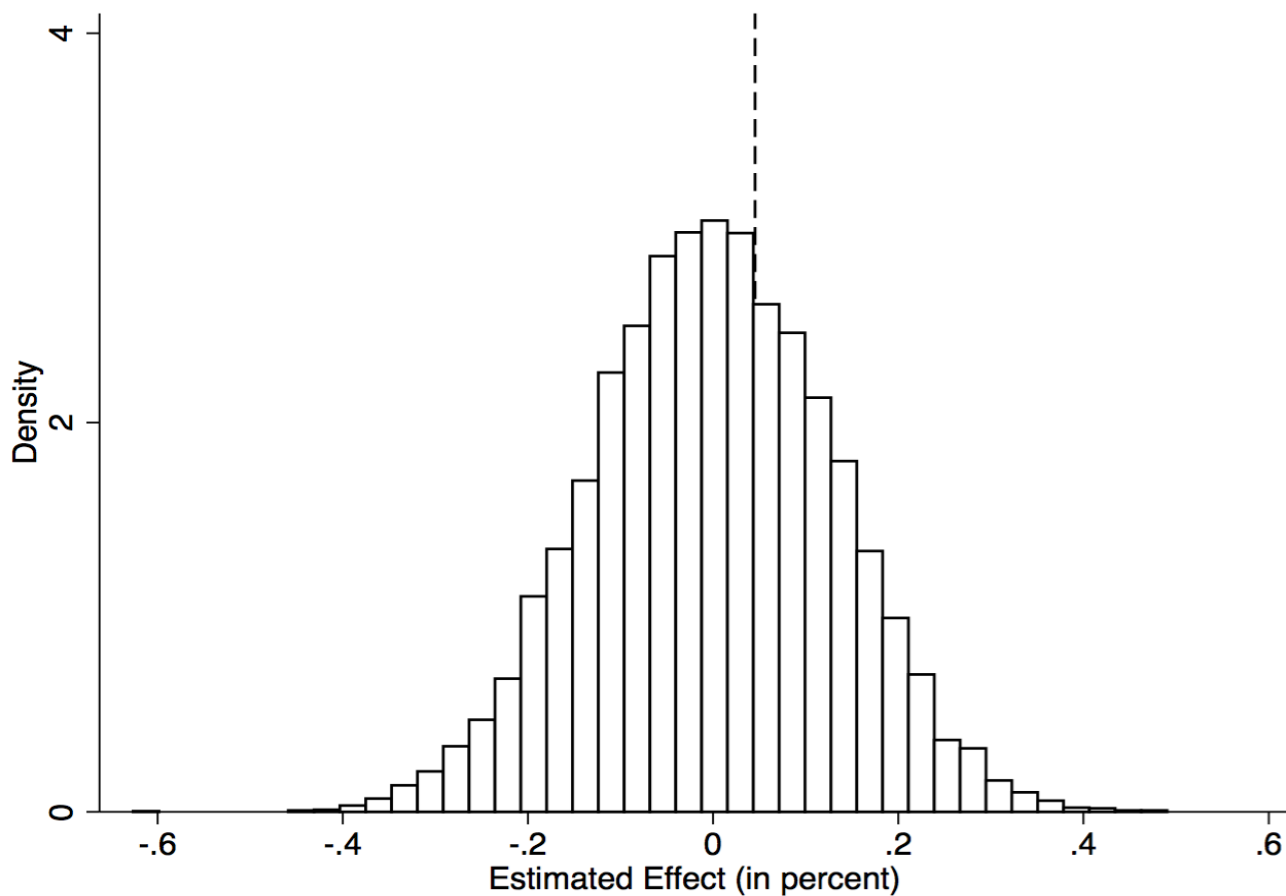
Notes: Figure depicts the density of the vote share of the Democratic candidate, excluding uncontested races. Circles correspond to averages over 0.5-percentage-point-wide bins. Estimates of the associated density are based on the procedure of McCrary (2008) with a bandwidth of 2.5 percentage points.

Figure A.4. Placebo Test



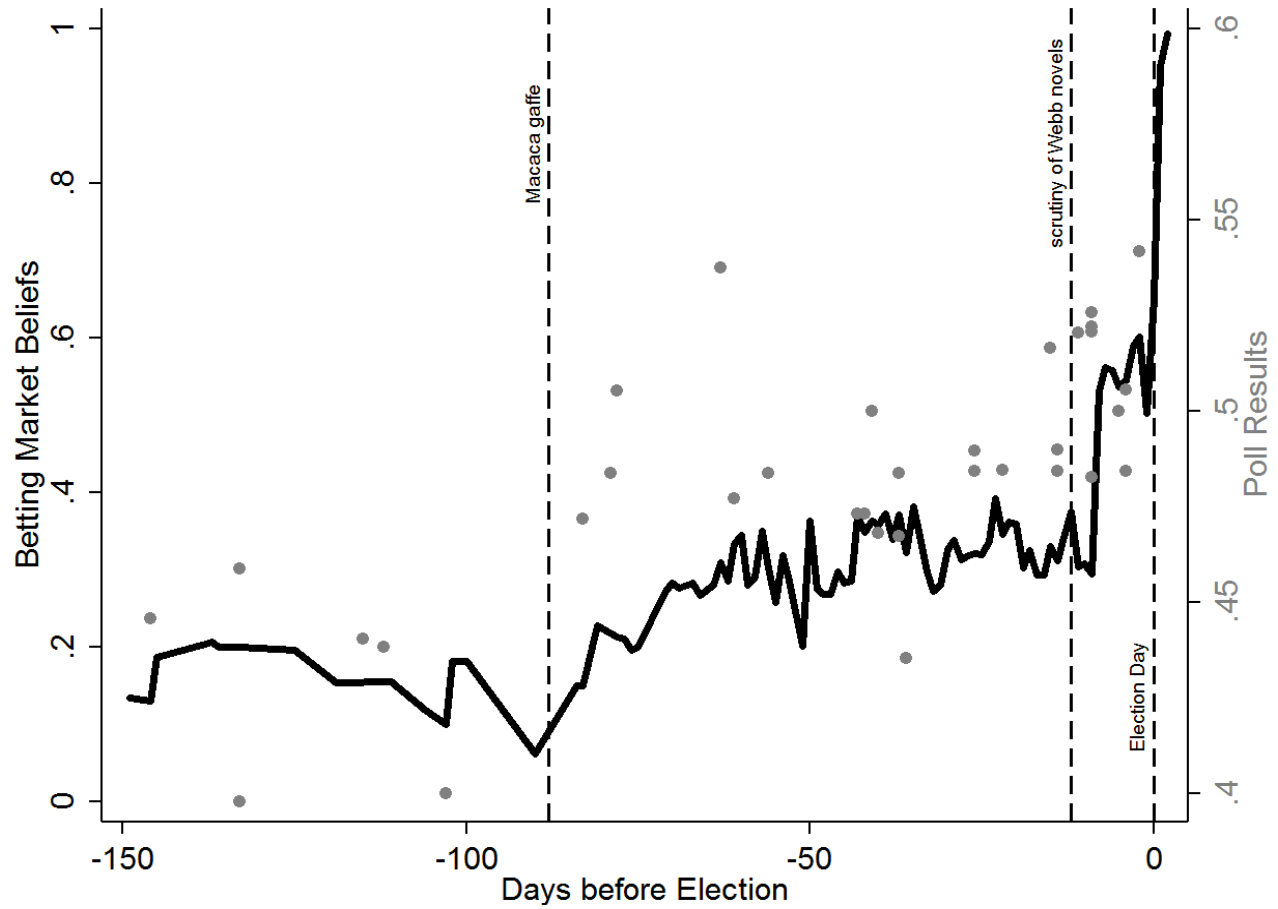
Notes: Figure shows a histogram of estimated placebo effects of a supported politician rising to office, based on 5,000 randomly drawn "Election Days," as explained in Appendix D. The vertical line indicates the point estimate in the original data.

Figure A.5. Permutation Test



Notes: Figure shows a histogram of estimated placebo effects of a supported politician rising to office, based on 10,000 randomly generated permutations, as explained in Appendix E. The vertical line indicates the point estimate in the original data.

Figure A.6. Betting Market Beliefs during the 2006 Virginia Senate Campaign



Notes: Figure depicts the implied probability of a Democratic victory based on betting odds for the 150 days leading up to the election (solid line; left axis) as well as the results of every poll reported by Real Clear Politics (dots; right axis). Key campaign events are represented by vertical lines.

Table A.1: Additional Sensitivity and Robustness Checks, I/III

Panel A: CAR(-1, 1)					
Local Polynomial	Bandwidth (in p.p.)				IK-Optimal
	0.5	1.25	2.5	5	
Constant	.0024 (.0023)	.0008 (.0018)	.0003 (.0012)	.0011 (.0010)	.0012 (.0008)
Linear	-.0046 (.0047)	.0014 (.0031)	.0012 (.0021)	.0005 (.0018)	.0009 (.0013)
Quadratic	-.0065 (.0061)	-.0006 (.0043)	.0005 (.0032)	.0006 (.0020)	.0006 (.0020)
Cubic	-.0049 (.0086)	-.0053 (.0045)	.0013 (.0044)	.0010 (.0034)	.0005 (.0022)
Number of Observations	3,038	7,333	14,956	29,390	--
Panel B: CAR(-1, 5)					
Local Polynomial	Bandwidth (in p.p.)				IK-Optimal
	0.5	1.25	2.5	5	
Constant	.0064* (.0030)	.0018 (.0021)	.0007 (.0018)	.0011 (.0015)	.0004 (.0013)
Linear	.0062 (.0049)	.0052 (.0045)	.0033 (.0026)	.0009 (.0026)	.0028 (.0021)
Quadratic	.0096 (.0083)	.0115* (.0047)	.0059 (.0046)	.0035 (.0029)	.0030 (.0025)
Cubic	.0126 (.0108)	.0099 (.0053)	.0092 (.0053)	.0066 (.0038)	.0017 (.0030)
Number of Observations	3,035	7,325	14,935	29,351	--

Notes: Entries are RD estimates and standard errors for the effect of a supported politician rising to office on cumulative abnormal returns. The upper panel uses CAR(-1,1) as outcome, while the lower one uses CAR(-1,5). Standard errors clustered account for clustering by election cycle. The optimal bandwidth in the rightmost column is chosen according to the procedure of Imbens and Kalyanaraman (2011). The difference in sample size between Panels A and B is due to missing information on stock returns in the raw data.

Table A.2. Additional Sensitivity and Robustness Checks, II/III

Panel A: CR(-1, 1)					
Local Polynomial	Bandwidth (in p.p.)				IK-Optimal
	0.5	1.25	2.5	5	
Constant	.0024 (.0014)	.0021 (.0013)	.0010 (.0011)	.0013 (.0007)	.0013 (.0008)
Linear	.0005 (.0044)	.0020 (.0026)	.0020 (.0017)	.0014 (.0017)	.0022 (.0010)
Quadratic	.0000 (.0061)	.0026 (.0031)	.0031 (.0024)	.0018 (.0018)	.0019 (.0020)
Cubic	.0041 (.0079)	.0002 (.0049)	.0021 (.0041)	.0025 (.0024)	.0010 (.0021)
Number of Observations	3,038	7,333	14,956	29,390	--
Panel B: CR(-1, 5)					
Local Polynomial	Bandwidth (in p.p.)				IK-Optimal
	0.5	1.25	2.5	5	
Constant	.0081 (.0059)	.0057 (.0048)	.0029 (.0037)	.0016 (.0032)	.0027 (.0031)
Linear	.0185 (.0118)	.0088 (.0063)	.0065 (.0045)	.0043 (.0040)	.0057 (.0047)
Quadratic	.0239 (.0170)	.0198 (.0130)	.0133 (.0094)	.0078 (.0056)	.0047 (.0045)
Cubic	.0336 (.0235)	.0214 (.0124)	.0140 (.0088)	.0129 (.0076)	.0057 (.0048)
Number of Observations	3,035	7,325	14,935	29,351	--

Notes: Entries are RD estimates and standard errors for the effect of a supported politician rising to office on cumulative *raw* returns. The upper panel uses CR(-1,1) as outcome, while the lower one uses CR(-1,5). Standard errors account for clustering by election cycle. The optimal bandwidth in the rightmost column is chosen according to the procedure of Imbens and Kalyanaraman (2011). The difference in sample size between columns A and B is due to missing information on stock returns in the raw data.

Table A.3. Additional Sensitivity and Robustness Checks, III/III

Panel A: Race Fixed Effects					
Local Polynomial	Bandwidth (in p.p.)				IK-Optimal
	0.5	1.25	2.5	5	
Constant	.0041 (.0024)	.0013 (.0013)	.0023 (.0019)	.0020 (.0014)	−.0006 (.0015)
Linear	.0036 (.0032)	.0052 (.0028)	.0021 (.0016)	.0018 (.0022)	−.0023 (.0038)
Quadratic	.0036 (.0032)	.0052 (.0029)	.0020 (.0016)	.0018 (.0022)	−.0007 (.0040)
Cubic	.0045 (.0056)	.0068 (.0035)	.0029 (.0030)	.0028 (.0017)	−.0219 (.0225)
Number of Observations	3,038	7,333	14,956	29,390	--
Panel B: Party-Cycle Fixed Effects					
Local Polynomial	Bandwidth (in p.p.)				IK-Optimal
	0.5	1.25	2.5	5	
Constant	.0009 (.0019)	.0012 (.0016)	.0005 (.0011)	.0006 (.0005)	.0007 (.0004)
Linear	−.0016 (.0038)	.0018 (.0025)	.0020 (.0018)	.0010 (.0017)	.0011 (.0008)
Quadratic	−.0038 (.0060)	.0001 (.0033)	.0014 (.0024)	.0013 (.0020)	.0011 (.0016)
Cubic	−.0038 (.0089)	−.0011 (.0038)	.0024 (.0035)	.0020 (.0024)	.0010 (.0020)
Number of Observations	3,038	7,333	14,956	29,390	--

Notes: Entries are RD estimates and standard errors for the effect of a supported politician rising to office on CAR(−1,1). The upper panel controls for race fixed effects, while the lower panel controls for party-cycle fixed effects. Standard errors account for clustering by election cycle. The optimal bandwidth in the rightmost column is chosen according to the procedure of Imbens and Kalyanaraman (2011).

Table A.4. Sample Splits

	Dependent variable				Sample Size
	CAR(-1, 1)	CAR(-1, 5)	CAR(-1, 10)	CAR(-1, 30)	
Full Sample	.0005 (.0018)	.0009 (.0026)	-.0005 (.0039)	-.0004 (.0075)	29,390
<i>By Office:</i>					
U.S. House	.0026 (.0021)	.0029 (.0032)	.0030 (.0033)	.0056 (.0066)	10,550
U.S. Senate	.0050 (.0037)	.0055 (.0038)	.0153 (.0102)	.0198 (.0138)	4,170
Governor	-.0007 (.0050)	.0066 (.0133)	.0019 (.0112)	.0150 (.0091)	1,757
State House	-.0031 (.0028)	-.0034 (.0041)	-.0091 (.0097)	-.0155 (.0166)	8,361
State Senate	-.0062 (.0032)	-.0041 (.0034)	-.0128* (.0043)	-.0198 (.0102)	4,552
<i>By Size of Donation:</i>					
1st Tercile	-.0007 (.0019)	.0020 (.0021)	-.0011 (.0040)	.0003 (.0107)	14,626
2nd Tercile	.0004 (.0023)	-.0013 (.0040)	-.0006 (.0036)	.0063 (.0052)	7,182
3rd Tercile	.0024 (.0029)	.0008 (.0035)	.0000 (.0063)	-.0082 (.0084)	7,582
<i>By Decade:</i>					
1980s	.0014 (.0019)	.0028 (.0029)	.0079 (.0064)	.0164 (.0090)	5,120
1990s	-.0023 (.0019)	.0018 (.0009)	.0006 (.0036)	-.0006 (.0065)	9,918
2000s	.0012 (.0033)	-.0009 (.0053)	-.0048 (.0072)	-.0066 (.0144)	14,352
<i>By Market Capitalization:</i>					
Below Median	-.0016 (.0032)	-.0026 (.0049)	-.0089 (.0087)	-.0083 (.0137)	12,489
Above Median	.0028 (.0014)	.0039* (.0016)	.0042* (.0019)	.0062 (.0048)	12,496
<i>By Sector:</i>					
Manufacturing	.0023 (.0018)	.0032 (.0016)	.0043 (.0029)	.0080 (.0040)	13,378
Transportation and Infrastructure	-.0034 (.0031)	-.0034 (.0043)	-.0065 (.0059)	-.0148 (.0133)	6,647
Finance, Insurance, and Real Estate	-.0028 (.0038)	-.0010 (.0047)	-.0004 (.0037)	-.0048 (.0067)	4,678
Other	.0041 (.0038)	.0023 (.0088)	-.0069 (.0167)	-.0007 (.0235)	4,687
<i>By Incumbency Status of Supported Candidate:</i>					
Incumbents	.0012 (.0019)	.0002 (.0019)	-.0001 (.0033)	-.0013 (.0088)	17,043
Challengers	-.0005 (.0021)	.0014 (.0038)	-.0016 (.0058)	.0001 (.0074)	12,347

Notes: Entries are point estimates and standard errors for β in equation (1), estimated on different subsamples of the data and for different time horizons. Standard errors are reported in parentheses and account for clustering by election cycle. All specifications restrict attention to close elections, i.e., races with a two-party vote share between .45 and .55. The rightmost column indicates the number of observations in a particular sample. ** and * denote statistical significance at the 1%- and 5%-levels, respectively.

Table A.5. Ancillary Analysis of Subsamples

	Estimate	Firm-Elections
Baseline	.0005 (.0018)	29,390
<i>Excluding Largest Firms:</i>		
< 90 percentile	.0005 (.0020)	22,866
< 75 percentile	−.0005 (.0021)	19,634
<i>Excluding Smallest Firms:</i>		
> 10 percentile	.0013 (.0015)	22,004
> 25 percentile	.0012 (.0019)	17,847
<i>Excluding Biggest Donors:</i>		
< 90 percentile	.0002 (.0018)	26,950
< 75 percentile	−.0000 (.0018)	23,177
<i>Excluding Smallest Donors:</i>		
> 10 percentile	−.0002 (.0023)	25,794
> 25 percentile	.0005 (.0020)	20,826
<i>By Years:</i>		
Presidential	.0022 (.0028)	13,451
Midterm	−.0008 (.0018)	15,212
Odd	−.0020 (.0022)	727

Notes: Entries are point estimates and standard errors for β in equation (1), estimated on different subsamples of the data. Standard errors are reported in parentheses and account for clustering by election cycle. All specifications restrict attention to close elections, i.e., races with a two-party vote share between .45 and .55. The rightmost column indicates the number of observations in a particular sample. ** and * denote statistical significance at the 1%- and 5%-levels, respectively.

Table A.6. Replication of Akey (2015), I/II

Outcome: Fama French CAR(-1, 5)				
Local Polynomial	Bandwidth (in p.p.)			IK-Optimal
	0.5	1.25	2.5	
Constant	-.0318 (.0683)	.0004 (.0190)	-.0143* (.0063)	-.0002 (.0217)
Linear	-.0318 (.0683)	.0308 (.0707)	-.0231 (.0199)	.0308 (.0707)
Quadratic	-.0318 (.0683)	-.2503 (.1981)	.0327 (.0507)	-.1421 (.1382)
Cubic	-.0318 (.0683)	-2.7276 (3.8835)	.0680 (.1171)	.0680 (.1171)
Number of Observations	24	70	277	--

Notes: Entries are RD estimates and standard errors from estimating equation (1) in Akey (2015) on our hand-collected data on firm-candidate pairs pertaining to the 13 special elections identified by Akey. As in Akey (2015), the outcome is CAR(-1,5), calculated based on the Fama-French three-factor model. The optimal bandwidth in the rightmost column is selected according to the procedure of Imbens and Kalyanaraman (2011). Results do not vary across rows in the first column because the sample for this bandwidth only includes a single election. As a result, the running variable only takes a single value at each side of the electoral threshold, and all polynomials are perfectly collinear. ** and * denote statistical significance at the 1% and 5% levels, respectively.

Table A.7. Replication of Akey (2015), II/II

Outcome: Market Model CAR(-1, 5)				
Local Polynomial	Bandwidth (in p.p.)			IK-Optimal
	0.5	1.25	2.5	
Constant	-.0116 (.0513)	-.0007 (.0134)	-.0172** (.0065)	.0073 (.0144)
Linear	-.0116 (.0513)	.0467 (.0570)	-.0212 (.0165)	.0467 (.0570)
Quadratic	-.0116 (.0513)	-.1794 (.1784)	.0417 (.0395)	-.0908 (.1188)
Cubic	-.0116 (.0513)	-2.7063 (3.8761)	.0882 (.1049)	.0882 (.1049)
Number of Observations	24	70	277	--
Outcome: Market Model CAR(-1, 1)				
Local Polynomial	Bandwidth (in p.p.)			IK-Optimal
	0.5	1.25	2.5	
Constant	-.0351 (.0214)	-.0078 (.0102)	-.0122** (.0041)	-.0117* (.0058)
Linear	-.0351 (.0214)	-.0245 (.0306)	-.0184 (.0122)	-.0245 (.0306)
Quadratic	-.0351 (.0214)	-.1462 (.0927)	-.0122 (.0268)	-.0994 (.0593)
Cubic	-.0351 (.0214)	-1.0125 (3.3741)	-.0238 (.0586)	-.0238 (.0586)
Number of Observations	24	70	277	--

Notes: Entries are RD estimates and standard errors from estimating equation (1) in Akey (2015) on our hand-collected data on firm-candidate pairs pertaining to the 13 special elections identified by Akey. The outcome in the upper panel is CAR(-1,5), calculated based on the market model; while the lower panel uses CAR(-1,1), as in the main text. The optimal bandwidth in the rightmost columns is selected according to the procedure of Imbens and Kalyanaraman (2011). Results do not vary across rows in the first column because the sample for this bandwidth only includes a single election. As a result, the running variable only takes a single value at each side of the electoral threshold, and all polynomials are perfectly collinear. ** and * denote statistical significance at the 1% and 5% levels, respectively.