Policy Uncertainty, Political Capital, and Firm Risk-Taking *

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Abstract

We document a new "policy sensitivity" channel of corporate political contributions. Firms that are highly sensitive to government policy uncertainty have a stronger incentive to contribute to political candidates, and these firms' risk-taking and performance should be more affected by the gain or loss of a political connection relative to less-sensitive firms. We verify these patterns in the data using a sample of close U.S. congressional elections. We first show that policy-sensitive firms donate more to candidates for elected office than less-sensitive firms. We then show that plausibly exogenous shocks to policy-sensitive firms' political connections produce larger subsequent changes in these firms' investment, leverage, firm value, operating performance, CDS spreads, and option-implied volatility relative to less-sensitive firms. Our results represent the first attempt in the literature to disentangle the effects of policy sensitivity and political connectedness on firms' risk-taking and performance and suggest that many existing results in the political connections literature are driven by policy-sensitive firms.

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1 Introduction

Why do corporations give money to politicians? Prominent explanations in the literature have focused on various forms of rent extraction such as government contracts, bailouts, and increased access to financing. In this paper, we explore a new hypothesis linked to firms' exposure to government policy uncertainty. Greater uncertainty over government policy outcomes can reduce the willingness of firms to make long-term investments. If political connections can help firms to achieve a favorable policy outcome or can reduce policy-related uncertainty, firms and industries exposed to significant policy risk will have a strong incentive to contribute to politicians even in the absence of direct rents. Under this logic, a firm's stock of political connections or political "capital" would be an increasing function of the firm's sensitivity to government policy uncertainty. Moreover, the gain or loss of a political connection would have a stronger effect on the subsequent risk-taking and performance of highly "policy-sensitive" firms.

Anecdotal evidence suggests that policy uncertainty has a significant effect on firms' political contribution activity. For example, a 2013 survey by PwC found that U.S. CEOs were more worried about policy uncertainty than about any other type of uncertainty, and at the same time, aggregate corporate political contributions reached the highest levels ever recorded during the 2013-2014 U.S. congressional election cycle.[1] As another example, the health care industry has faced unprecedented policy uncertainty in recent years, and at the same time, the industry's political contributions have increased at an unprecedented rate.[2] However, these anecdotal illustrations are merely suggestive and fall far short of causal evidence. In addition, little is known about how the gain or loss of a political connection differentially affects the risk-taking and performance of policy-sensitive firms.

Our paper examines the links between policy uncertainty, political capital, and firm risk-taking and performance using a sample of close Congressional elections in the United States. We develop and test three main hypotheses. First, we hypothesize that the marginal value of an extra political connection will be larger for policy-sensitive firms than for otherwise-similar "policy-neutral" firms.[3]

[1] Campaign contributions data are sourced from OpenSecrets.org. CEO survey data is from PwC, *16th Annual Global CEO Survey*, January 2013, http://www.pwc.com/gx/en/ceo-survey/2013/pdf/us-ceo-survey-2013.pdf.

[2] According to OpenSecrets.org, Political Action Committee ("PAC") contributions linked to the health care industry expanded by 150% between the 2000 and 2010 election cycles, compared with increases of 79% for energy and natural resources firms, 65% for oil companies, 55% for the finance industry, and -27% for tobacco companies during the same period.

[3] While there are no theories (to our knowledge) that link together policy uncertainty, political connections, and firm risk-taking, we can appeal to the literature on hedging to support this argument (see, e.g., Holthausen (1979)). In the presence of financing frictions, taxes, bankruptcy costs, or other types of frictions, a positive shock to uncertainty will increase the demand for hedging holding the firm's production function constant. All else equal, this implies that the marginal value of an extra hedging unit will be larger for firms exposed to greater levels of uncertainty.

Intuitively, a firm more exposed to government policy uncertainty should place a higher value on the influence or informational advantages that may stem from having direct connections to government policy-makers. If this is true, it implies that policy-sensitive firms should be more likely to make or increase campaign contributions relative to otherwise-similar policy-neutral firms.

Second, we argue that, *holding firms' policy sensitivities fixed*, a firm experiencing a "lucky" political capital shock from a candidate's close-election victory will respond differently than a firm experiencing an "unlucky" political capital shock from a candidate's narrow loss. For example, firms experiencing a "lucky" shock might increase investment, while firms experiencing an "unlucky" shock might decrease investment. Following the existing literature on political connections, we do not take a stand on whether firm risk or performance measures should increase or decrease following a "lucky" political capital shock. However, we hypothesize that after controlling for firms' ex-ante policy uncertainty sensitivities, the *differences* in responses between firms experiencing "lucky" and "unlucky" political capital shocks are likely to be statistically and economically large.

Third, we hypothesize that, *holding firms' political capital shocks fixed*, policy-sensitive firms' responses to a given election outcome should be larger in magnitude than policy-neutral firms' responses to the same election outcome. For example, if Pfizer is more sensitive to future government policies than Merck, then even if Merck and Pfizer receive the *same* close-election political capital shock, we hypothesize that Pfizer's risk-taking and performance will respond more "sharply" than Merck's risk-taking and performance following the outcome of the election.

To test these conjectures, we begin by sorting firms into "policy-sensitive" and "policy-neutral" categories based on their pre-election stock return sensitivities to the Economic Policy Uncertainty index created by Baker, Bloom, and Davis (2015). We then use firm-driven operating and performance variables (investment, leverage, R&D spending, margins, and sales growth) and market-driven outcome variables (option-implied volatility, CDS spreads, Tobin's Q) to examine whether politically-active firms alter their risk-taking behavior following federal elections in the United States. Since political donation decisions are not random, we limit our sample to firms that donated money to candidates in "close" elections during each federal election cycle. Within each election cycle, we then define the magnitude of the ex-post political capital shock for firm i as the difference between the number of ultimate winners and losers that the firm supported in close elections during that cycle. For example, Coca-Cola donated to two winning candidates and five losing candidates in close elections during the 2004 election cycle, so we compute the shock to Coke's political capital during the 2004 cycle as 2 - 5 = -3. In contrast, Coke supported

seven close-election winners and four close-election losers during the 2006 election cycle, so Coke's political capital shock in the 2006 cycle would be defined as 7 - 4 = 3.

Our primary identifying assumption is that election outcomes at the time of firms' donations are plausibly exogenous in our sample of close elections. As shown in the Coca-Cola example above, most firms do not appear to be able to predict the winners of close elections with significant accuracy. Consistent with this assumption, we find that the median net political capital shock across all election years in our sample is zero.[4] We then define "lucky" firms during each election cycle as those firms with positive net political capital shocks (i.e. those firms who donated to candidates that ultimately won more close elections than they lost). Using a differences-in-differences framework, we then examine how these "lucky" shocks to firms' political capital bases affect firms' subsequent behavior and performance.[5] In a series of triple-difference specifications, we also examine whether the effects we observe for "lucky" winners are more pronounced among policy-sensitive firms than among policy-neutral firms. Hence, in total, we are able to isolate the effects of political capital shocks on firm risk-taking for four different types of firms: "lucky" policy-sensitive firms, "unlucky" policy-sensitive firms, "lucky" policy-neutral firms, and "unlucky" policy-neutral firms. This decomposition allows us to directly test the relationships between policy uncertainty, political capital, and firms' subsequent risk-taking and performance.

Our analysis yields four main results. First, we find that policy sensitivity has a first-order effect on firms' political contributions. Specifically, we find that policy-sensitive firms increase their political contributions by approximately 8-13% relative to policy-neutral firms, particularly to candidates in close elections. This finding is consistent with the hypothesis that the marginal value of an extra political connection is larger for firms that are highly sensitive to the broader economic policy environment.

Next, we find that "lucky" political capital shocks are associated with an improvement in firms' operating performance (as measured by variables such as sales growth and ROA) and market reactions consistent with lower perceived firm risk (as measured by variables such as implied volatility and CDS spreads). We also find that "lucky" political capital shocks are associated with an increase in firm value (as measured by Tobin's Q). These effects are opposite in sign but are roughly

[4]Interestingly, it is relatively rare for firms to "hedge" each election outcome by donating to multiple candidates within the same election – this only occurs in around 5% of all firm-election pairs. We conjecture that politicians may simply not provide as much access to a firm that also supported the politician's election opponent(s).

[5]Standard differences-in-differences designs contain a treatment group and a control group. Here, both groups are treated: one experiences a positive shock while the other experiences a negative shock. As noted by Cook and Campbell (1979), this experimental design is arguably better suited than standard differences-in-differences designs for causal inference due to its high construct validity.

symmetric in magnitude for "lucky" versus "unlucky" firms, supporting our identifying assumption that close election outcomes were unknown at the time of firms' campaign contributions. These results are consistent with the existing literature on political connections and extend this literature by providing evidence on additional firm performance metrics.

Third, holding firms' ex-ante policy sensitivities fixed, we find that differences in post-election outcomes between "lucky" and "unlucky" *policy-sensitive* firms are larger in magnitude than the differences we observe between "lucky" and "unlucky" *policy-neutral* firms. The economic magnitudes of these differences are significant: for example, we observe a 10% relative difference in investment levels, a 2% relative difference in leverage, a 13% relative difference in Tobin's Q, a 12% relative difference in one-month option-implied volatility, and a 10% relative difference in one-year CDS spreads. These findings confirm our intuition that policy-sensitive firms respond more sharply to political capital shocks relative to policy-neutral firms. We also find that the differences in outcomes between "lucky" and "unlucky" *policy-neutral* firms are often economically and statistically small, while the differences in outcomes between "lucky" and "unlucky" *policy-sensitive* firms are economically and statistically large. These results suggest that many of the average effects documented in the political connections literature on variables such as firm value and sales growth are driven primarily by policy-sensitive firms.

Our final set of tests examines the effects of policy uncertainty sensitivity on firm risk-taking and performance holding firms' political connections (or, more precisely, shocks to these connections) fixed. We find that unlucky political capital shocks hurt policy-sensitive firms *particularly* badly relative to their policy-neutral peers: unlucky policy-sensitive firms have lower investment, higher leverage, lower Q, worse operating performance, and higher implied volatility and CDS spreads than policy-neutral firms hit with a similarly unlucky shock. Similarly, in some specifications, we find that lucky political capital shocks help policy-sensitive firms more than policy-neutral firms.[6] These results suggest that policy-sensitive firms respond more strongly to the resolution of political uncertainty than policy-neutral firms.

One possible concern with these results is that we have treated all political elections as being equally important. In reality, some elections clearly matter more than others (such as elections involving Senators and members of powerful congressional committees). Indeed, this is exactly

[6] In other specifications, we find that the effects of policy uncertainty on risk-taking and performance are asymmetric: policy-sensitive firms hit with a *bad* political capital shock suffer greatly (relative to similarly-unlucky policy-neutral firms), while policy-sensitive firms hit with a *good* political capital shock are still negatively impacted, though to a lesser degree. We discuss this potential asymmetry in Sections 4 and 5.

what we observe: we find that all of the effects documented above are more powerful for political capital shocks involving Senators and members of powerful committees. For example, shocks to the membership of the Senate Committee on Energy and Natural Resources have a particularly strong effect on *policy-sensitive* oil and gas firms relative to policy-neutral oil and gas firms as well as policy-sensitive and policy-neutral firms in other industries.

A second concern is that our results may be picking up firms' exposure to other sources of uncertainty such as general macroeconomic uncertainty. However, all of our results go through when we control for general uncertainty using the VIX index and the macroeconomic uncertainty index created by Jurado, Ludvigson, and Ng (2015). To mitigate concerns about our use of the Baker, Bloom, and Davis (2015) index, we also construct a firm-level definition of policy uncertainty based on firms' 10-K disclosures and find similar results.

A final concern is that our results may be picking up firms' specific *policy* exposures rather than their exposure to policy-related *uncertainty*. However, the first moment and second moment are both economically relevant in our setting: some firms may make political contributions as a hedge against uncertainty, while others may make political contributions to opportunistically influence policy outcomes, even if these outcomes do not yield direct rents to the firm. In both cases, however, the marginal value of a political connection should be greater for policy-sensitive firms. As such, both cases are consistent with our proposed "policy sensitivity" channel of corporate political activity. Indeed, the fact that we find stronger results for policy-sensitive firms using multiple definitions of policy uncertainty suggests that our policy sensitivity classifications are likely picking up meaningful variation in firms' exposure to both moments of the government policy distribution.

Our results point to a "policy sensitivity" channel of political capital accumulation that is distinct from the channels that have been previously documented in the political connections literature. For example, a growing literature points to firms' abilities to secure government funds through "bailouts" (Faccio, Masulis, and McConnell (2006); Duchin and Sosyura (2012)) or through various forms of government spending (Brogaard, Denes, and Duchin (2015); Schoenherr (2015)) as a significant channel through which firms benefit from political connections. However, we find that risk-taking declines and operating performance improves following a positive political capital shock. These results are consistent with our policy sensitivity channel, but are less consistent with the "increased government handouts" channel. Another channel argues that politically-connected firms benefit from increased credit availability through loans made by politically-connected banks (see, e.g., Khwaja and Mian (2005) and Claessens, Feijen, and Laeven (2008)). However, our finding

that firms' average leverage decreases following positive political capital shocks is not consistent with the "increased credit availability" channel. In short, our results line up most closely with the idea that corporations cultivate political connections in an attempt to influence policy outcomes on issues that are particularly relevant to the firm.

Our paper also represents the first attempt (to our knowledge) to bring together the existing literatures on uncertainty and political connections. Both literatures use elections for identification, but they do so in different ways: the literature on uncertainty uses elections as a shock to aggregate uncertainty (see, e.g., Julio and Yook (2012); Kelly, Pástor, and Veronesi (2015)), while the literature on political connections uses elections as a shock to firms' political connectedness (see, e.g., Claessens, Feijen, and Laeven (2008); Cohen, Coval, and Malloy (2011)). Importantly, the existence of two types of election-related shocks – one aggregate, one firm-specific – makes it difficult to identify which shock(s) are driving firms' post-election behavior. For example, do firms respond differently to aggregate election outcomes based on shocks to their own political capital? Do firms differ in their pre-election exposure to aggregate policy uncertainty, and if so, how does this affect their post-election risk-taking and performance in the cross-section? Are political connections more valuable when a firm is highly sensitive to potential changes in government policy? Our paper is the first to provide answers to these questions.

2 Related Literature

Our paper is related to three strands of the existing literature. First, a small but growing literature examines the effects of aggregate political uncertainty on firm outcomes and asset prices (Durnev (2010), Boutchkova, Doshi, Durnev, and Molchanov (2012), Julio and Yook (2012), Pástor and Veronesi (2012, 2013), Brogaard and Detzel (2015), Gulen and Ion (2015), Kelly, Pástor, and Veronesi (2015), Jens (2016)).[7] These papers generally find that aggregate risk-taking is reduced during periods of high uncertainty. To date, however, this strand of the literature has not looked at how economic policy uncertainty interacts with firms' political connections, and most of the analysis in this literature is focused on the time series rather than the cross-section of firms. Our paper contributes to this literature by linking policy uncertainty to firms' political activities and by examining how policy uncertainty sensitivity affects risk-taking and performance within the

[7] A related literature examines the relationship between political factors and stock returns – see, e.g, Kim, Pantzalis, and Park (2012), Belo, Gala, and Li (2013), Addoum, Delikouras, Ke, and Kumar (2014) and Cohen, Diether, and Malloy (2013).

cross-section of firms.

Our paper is also related to two strands of the literature on political connections. One strand focuses on the link between political connections and firm value. A long list of papers including Fisman (2001), Faccio (2006), Faccio and Parsley (2009), Jayachandran (2006), Ferguson and Voth (2008), Cooper, Gulen, and Ovtchinnikov (2010), Do, Lee, and Nguyen (2013), Goldman, Rocholl, and So (2009), Akey (2015), Acemoglu, Hassan, and Tahoun (2015), Borisov, Goldman, and Gupta (2015), Schoenherr (2015), and Acemoglu, Johnson, Kermani, Kwak, and Mitton (2016) all find evidence that stronger political connections are associated with increases in firm value.[8] Consistent with this literature, we find that unexpected positive shocks to firms' political capital stocks are associated with increases in firm value (as measured by Tobin's Q). We add to this literature by documenting that the link between political connections and firm value appears to be largely driven by policy-sensitive firms.

A second strand of the political connections literature is focused on identifying why firms establish connections with politicians in the first place. One view is that firms benefit from political connections through increased government spending. Faccio, Masulis, and McConnell (2006) and Duchin and Sosyura (2012) show that politically-connected firms are more likely to receive government bailouts than non-connected firms. Another set of papers finds that politically-connected firms have higher sales and/or receive more government procurement contracts (Amore and Bennedsen (2013), Goldman, Rocholl, and So (2013), Tahoun (2014), Akey (2015), Brogaard, Denes, and Duchin (2015), Schoenherr (2015)). Schoenherr (2015) finds that these contracts perform poorly and Brogaard, Denes, and Duchin (2015) suggest that these connections may stifle innovation, similar to Cohen and Malloy (2014)'s findings that government-dependent firms (who are likely to be politically-connected) have lower investment, lower R&D spending, and lower sales growth than non-government-dependent firms. Relatedly, Kim (2015) finds that firms with strong political connections have lower investment, lower R&D spending, and lower patent citations (but higher government sales) relative to firms with weak political connections.[9] A second view is that politically-connected firms benefit from increased credit availability and a potential reduction in financial constraints (Khwaja and Mian (2005), Claessens, Feijen, and Laeven (2008)). Hanouna, Ovtchinnikov, and Prabhat (2014) find that CDS spreads on average tend to be lower for politically-

[8] Agarwal, Meshke, and Wang (2012) and Coates IV (2012) find that political connections may indicate agency problems in connected firms. However, the overwhelming majority of studies has found that political connections have a large and positive impact on firm value.

[9] In contrast to this view, Do, Lee, and Nguyen (2013) finds that politically-connected firms invest more in physical capital.

connected firms, which is consistent with this view. Collectively, these findings are largely consistent with the "rent seeking" theoretical predictions of Murphy, Shleifer, and Vishny (1993) and Shleifer and Vishny (1994).[10] However, none of these papers examines the effects of policy uncertainty on political capital and firms' subsequent risk-taking.[11]

Ovtchinnikov, Reza, and Wu (2014) find that firms' innovation increases following positive political capital shocks, which they argue is due to a reduction in policy uncertainty amongst politically-connected firms. However, Ovtchinnikov, Reza, and Wu (2014) do not explicitly test whether policy uncertainty is directly impacting political connections or risk-taking. Furthermore, in contrast to Ovtchinnikov, Reza, and Wu (2014), we find no relationship between political capital shocks and R&D expenditures in our sample. Nevertheless, the key message of their paper – that politically-connected firms may benefit from a reduction in policy uncertainty – complements the main findings of our study.

3 Economic Setting and Identification Strategy

3.1 Economic Setting and Testable Implications

Our goal is to study the interaction between firms' sensitivity to economic policy uncertainty, their subsequent political activities, and their ultimate response to the resolution of policy uncertainty following U.S. federal elections. Our basic argument consists of four main components. First, some firms are more exposed (or sensitive) to economic policy uncertainty than other firms. For example, financial institutions and auto manufacturers may have been more exposed to policy uncertainty ahead of the 2008 elections than, say, textile producers. We label firms with a high exposure to policy uncertainty as "policy-sensitive" firms.

We next conjecture that policy-sensitive firms are more likely to make (or increase) political campaign contributions than similar policy-neutral firms. In particular, we argue that the marginal value of a political connection is higher for policy-sensitive firms than for policy-neutral firms – a hypothesis that is consistent with the literature on firms' hedging behavior under uncertainty in the presence of market frictions. This hypothesis leads immediately to our first testable prediction,

[10] Johnson and Mitton (2003) find that politically-connected firms benefit from foreign capital controls and suffer when these controls are removed, consistent with the predictions of Rajan and Zingales (1998).

[11] Our paper is also related to large empirical literatures on the effects of government spending on the economy (see, e.g., Cohen, Coval, and Malloy (2011) and Nakamura and Steinsson (2014)) and the effects of general uncertainty on firm risk-taking and performance (see, e.g., Bloom, Bond, and Reenen (2007); Kellogg (2014)). We omit the long list of relevant citations in these two literatures for brevity.

which is that policy-sensitive firms will contribute more to candidates for elected office than policy-neutral firms within a given election cycle.

We next link firms' donation choices to the outcomes of U.S. federal elections. Economic policy uncertainty tends to rise before U.S. federal elections and decline following these elections (Baker, Bloom, and Davis (2015)). One interpretation of this finding is that a significant proportion of policy uncertainty is related to political elections themselves and is resolved by the outcomes of these elections. Under this interpretation, the uncertainty faced by policy-sensitive firms should decline following elections, and should decline *more so* than for policy-neutral firms. As such, we posit that the previously-documented tendency for firms to "wait" on the outcomes of elections (Julio and Yook (2012), Pástor and Veronesi (2013), Kelly, Pástor, and Veronesi (2015)) will lead to sharper post-election changes in firm operating behavior for policy-sensitive firms relative to policy-neutral firms, holding political capital shocks constant. This is another prediction that is directly testable in our sample.

Election outcomes resolve two types of uncertainty: uncertainty related to future government policies, and uncertainty regarding a firm's stock of political connections or political capital. As such, firms' responses to the resolution of policy uncertainty may depend in part on whether the firm's own stock of political capital has been strengthened or weakened. Under the assumption that firms' campaign contributions are linked to its policy objectives, this implies that firms experiencing a "lucky" election draw (i.e. an election where many of the firms' contributions went to victorious candidates) will respond differently to the resolution of political uncertainty relative to firms whose contributions primarily went to losing candidates. For example, "winning" firms may decide to increase investment, while "losing" firms may decide to postpone investment.[12] Furthermore, policy-sensitive firms should be expected to increase or decrease investment (or other variables) *even more* than their non-policy sensitive brethren. These two predictions are also directly testable and arguably represent the main contribution of this paper.

3.2 Identification and Empirical Approach

Estimating the effect of political capital shocks on ex-post firm outcomes is challenging for a number of reasons. First, firms endogenously choose whether to be politically active and which politicians

[12]The expected signs of these effects are theoretically ambiguous. For example, moral hazard arguments suggest that stronger political connections should be linked to an increase in firm risk-taking. In contrast, government contracting considerations may cause firms to decrease risk-taking following positive political capital shocks either due to a desire to reduce distress probabilities or a desire to live the "quiet life" given guaranteed future income streams.

to form connections with. Second, certain types of firms may be more likely to donate to certain types of candidates who are themselves more or less likely to be elected (for example, powerful incumbents). Third, the results of most elections are effectively determined months before the actual election date, making it difficult to isolate the timing of political capital shocks on market prices or firm outcomes. Fourth, the causality could go in the other direction; that is, firms' operating decisions or riskiness may affect the outcome of elections and/or create shocks to the firm's political capital ledger.[13] Finally, other sources of unobserved heterogeneity may account for any observed relationship between political capital shocks and firms' riskiness and operating decisions. For example, a disruptive technology shock may jointly affect firms' operating decisions as well as the outcome of political elections in the state(s) most affected by the change.

To overcome these challenges, we focus on a subset of firms that donate to candidates in "close" U.S. congressional elections from 1998-2010. Our primary identifying assumption is that election outcomes at the time of firms' donations are plausibly exogenous in our sample of close elections. Our claim of plausible exogeneity requires two key conditions to be met: first, firms cannot accurately predict close-election winners at the time of their donations, and second, firms' donations themselves cannot materially affect a candidate's chances to win an election. While neither of these assumptions are directly testable, anecdotal evidence strongly supports the view that election outcomes in our sample are plausibly random conditional on firms' donation decisions. In particular, we find that the median firm in our sample supports *exactly the same* number of close-election losers as close-election winners during each congressional election cycle. Consistent with this fact, Figure 1 shows that the distribution of firms' *net* close-election political capital shocks is centered around zero, is effectively unimodal, and has relatively symmetric tails. Under the assumption that firms would rather donate to winning candidates than losing candidates, these results suggest that election outcomes are largely unpredictable in our sample of close elections and that a given firm's donations are not sufficient to sway election outcomes. Furthermore, by looking *within* the set of firms that made close-election donations, we are able to effectively control for the fact that donation patterns are not random, since all of the firms in our sample felt that it was optimal (for whatever reason) to donate to one or more close-election candidates. Finally, close elections are generally decided on election day (or very soon before), making it easier to isolate the timing associated with market and firm responses to political capital shocks.

[13] For example, financial institutions' behavior prior to the recent crisis may have affected the outcome of elections and/or the firms' political capital.

To identify the effects of political capital shocks on firm outcomes, we first need to define a firm- and election cycle-specific measure of close-election political capital shocks. We begin by defining $Close\ Wins_{i,t}$ ($Close\ Losses_{i,t}$) as the number of close-election winners (losers) that firm i donated to during election cycle t. For example, since Coca-Cola donated to two close-election winners and five close-election losers during the 2004 election cycle, we would set $Close\ Wins = 2$ and $Close\ Losses = 5$ for Coke during the 2004 cycle. We then define $Net\ Close\ Wins_{i,t}$ as the difference between $Close\ Wins$ and $Close\ Losses$. This variable captures a firm's *overall* political capital gain in close elections during a given cycle. For Coke in 2004, this variable would be defined as $Net\ Close\ Wins = 2 - 5 = -3$. We also create a dummy variable ($Close\ Election\ Dummy$) that takes the value of one if firm i's overall political capital gains are greater than the sample median of zero (i.e. where $Net\ Close\ Wins_{i,t} > 0$) during a given election cycle, and takes the value of zero otherwise. For example, since Coke donated to more close-election losers than winners in 2004, we would set $Close\ Election\ Dummy$ equal to zero for Coke in 2004.

The variables $Close\ Wins$, $Close\ Losses$, $Net\ Close\ Wins$, and $Close\ Election\ Dummy$ form our primary measures of political capital shocks. We will use all four variables in our subsequent tests. Panel A of Table 1 presents summary statistics for each of these measures (with the exception of $Close\ Election\ Dummy$, which is an explicit function of the sample median).

With our primary political capital measures in hand, we next turn to our empirical framework. We employ a differences-in-differences framework to estimate the effects of a political capital shock on firm outcomes. Specifically, we estimate the following model:

$$\begin{aligned} Outcome_{i,t} =& \alpha + \beta_1 Post\,Election_t + \beta_2 Post\,Election_t \times Capital\ Shock_{i,t} \\ &+ \Gamma' Controls_{i,t} + Firm \times Election\,Cycle\,FE + \epsilon_{i,t}\ , \end{aligned} \tag{1}$$

where i indexes firms, t indexes time, and $Capital\ Shock_{i,t}$ represents a political capital shock measure as described above. The granularity of our data allows us to include firm-election cycle fixed effects (which sweep away the $Capital\ Shock_{i,t}$ variable, which is defined at the firm-election cycle level). As such, our results can be interpreted as looking *within a firm and given election cycle*. We also perform a variety of tests to ensure that the "parallel trends" assumption holds in our analysis and to ensure that the effects we observe do not occur when we randomly reassign the event window (i.e. "placebo tests"). For example, Figure 2 presents the parallel trends graph for CDS spreads. While we do not report additional results for the sake of brevity, all of our tests

suggest that the standard conditions for inference in a differences-in-differences design are met within our sample.

Our primary coefficient of interest is β_2 in the equation above. If β_2 is positive, this signifies that a "lucky" (net) political capital shock for firm i is associated with an increase in the outcome variable of interest relative to another firm j that experienced an "unlucky" (net) political capital shock during the same election cycle.

During each election cycle, we also identify a subset of firms that are particularly sensitive to economic policy uncertainty during that cycle (our procedure for identifying such firms is listed below). We define an indicator variable, *Policy Sensitive*, to take a value of one if the firm is policy-sensitive and zero otherwise. We then use a triple-difference framework to study whether the effects of political capital shocks differ for firms that are more sensitive or less sensitive to policy uncertainty. Formally, we estimate the following model:

$$\begin{aligned} Outcome_{i,t} = \alpha &+ \beta_1 Post\,Election_t + \beta_2 Post\,Election_t \times Capital\ Shock_{i,t} \\ &+ \beta_3 Post\ Election_t \times Policy\,Sensitive_{i,t} \\ &+ \beta_4 Post\ Election_t \times Policy\ Sensitive_{i,t} \times Capital\,Shock_{i,t} \\ &+ \Gamma' Controls_{i,t} + Firm \times Election\,Cycle\,FE + \epsilon_{i,t}\ . \end{aligned} \tag{2}$$

In this specification, the coefficient β_4 captures the differential effect of being policy-sensitive on outcomes given the *same* political capital shock. If, for the sake of argument, both β_2 and β_4 are negative, than policy sensitive firms had an even larger negative reaction in the outcome to the same political capital shock than their policy-neutral peers. Since the $Capital\ Shock_{i,t}$ and $Policy\ Sensitive_{i,t}$ variables are invariant across a given firm-election cycle pair, these variables are swept away by the inclusion of firm-election cycle fixed effects.

3.3 Data

3.3.1 Political connections data

Firms contribute money to political candidates in the United States through legal entities known as Political Action Committees (PACs). PACs solicit contributions from employees of the sponsoring firm and donate these contributions to one or more political candidates.[14] Rather than donating

[14]Decisions regarding which candidates to support are typically left to one or more officers of the sponsoring company and frequently to a political specialist such as the PAC chair.

money directly to candidates' personal accounts (which is illegal in the United States), firms' PACs typically donate money to another PAC set up by a candidate for elected office (known as "Election PACs"). As such, we use Firm PAC contributions to Election PACs as our measure of a firm's political connectedness.[15]

We obtain election contribution and election outcome data from the U.S. Federal Election Commission (FEC) for all federal elections from 1998-2010.[16] We restrict our sample to general elections for the House of Representatives and the Senate, which occur on the first Tuesday of November in even-numbered years. In particular, our tests focus on *close* election outcomes, which we define as elections where the vote-share difference between the winning and runner-up candidates is 5% or less (following Do, Lee, Nguyen, and Nguyen (2012), Do, Lee, and Nguyen (2013), and Akey (2015)). In a typical two-candidate race, this means that we restrict our sample to elections where the winning candidate received less than 52.5% of the vote and the losing candidate received more than 47.5% of the vote. Panel A of Table 1 presents summary statistics for our political contributions data.

The maximum amount that a Firm PAC can contribute to an Election PAC is legally capped at $10,000 per election cycle. Given that $10,000 represents a trivial amount of money for the large, publicly-traded firms in our sample, it is reasonable to wonder what exactly a firm might expect to receive in return for such a small donation. However, the literature has found that PAC donations are often bundled with other types of political activity (such as lobbying) as part a much larger operation by firms to build connections to specific politicians (Austen-Smith (1995), Milyo, Primo, and Groseclose (2000), Ansolabehere, Snyder, and Tripathi (2002), Borisov, Goldman, and Gupta (2015), Akey (2015), Fremeth, Richter, and Schaufele (2016)). Unfortunately, lobbying data does not identify the specific politicians that a firm is attempting to influence through its lobbying activities, and hence, this data cannot be used to identify direct links between firms and politicians. However, under the assumption that campaign contributions and other activities such as lobbying are directed at a similar set of politicians, our use of campaign contributions data to identify political connections should not systematically bias any of our results.

[15]Firm employees may also donate money individually to candidates for election office. However, it is not possible to disentangle whether an individual donation reflects the individual's preferences or the preferences of their employer (see Akey (2015) for additional details). As such, we focus on Firm PAC donations as our measure of a firm's political connectedness.

[16]FEC data is transaction-level data organized by election cycle. Political contribution data is available from the FEC, the Center for Responsive Politics, or the Sunlight Foundation. The latter two organizations are non-partisan, non-profit organizations who assemble and release government datasets to further the public interest.

3.3.2 Economic policy uncertainty data

We use the Economic Policy Uncertainty index developed by Baker, Bloom, and Davis (2015) as our primary measure of economic policy uncertainty.[17] The Baker, Bloom, and Davis (2015) index is an aggregate time-series index that is based on (i) the frequency of articles in 10 major U.S. newspapers containing words which indicate uncertainty about economic policy, (ii) the prevalence of expiring tax provisions, and (iii) dispersion in analysts' forecasts regarding policy-relevant macroeconomic indicators. Additional details on the construction of this index can be found in Baker, Bloom, and Davis (2015).

3.3.3 Options data

We obtain daily option-implied volatility data from OptionMetrics from 1997-2011. OptionMetrics computes implied volatility from at-the-money call options using the Black-Scholes model. We use data for call options with 1 - 6 month maturities. We also obtain data on put options with similar maturities. Panel A of Table 1 presents summary statistics for our implied volatility data. All of our implied volatility tests use daily data from six months prior to federal election dates to six months after the election takes place.

3.3.4 Credit default swap data

We obtain daily CDS data from Markit from 2001 to 2011. Since CDS spreads are not available prior to 2001, all tests involving CDS spreads only focus on election cycles from 2002 to 2010. We focus on 1-year, 5-year, and 10-year CDS spreads on senior unsecured U.S.-dollar-denominated debt. Following Hanouna, Ovtchinnikov, and Prabhat (2014), we take the natural log of the CDS spread for each firm and use this as a dependent variable in our tests. Panel A of Table 1 presents summary statistics for our (untransformed) CDS data. All of our CDS tests use daily data from six months prior to federal election dates to six months after the election takes place.

[17] Other measures of economic policy uncertainty exist as well. For example, Whited and Leahy (1996) and Bloom, Bond, and Reenen (2007) examine the link between general uncertainty and investment and use share price volatility as a firm-specific measure of uncertainty. However, this measure seems to be too general to capture policy-specific uncertainty as opposed to other types of uncertainty. Several authors also use elections to measure time periods when policy uncertainty is high (see, e.g., Gao and Qi (2013) and Julio and Yook (2012)), but this measure cannot be used to produce ex-ante (i.e. *pre*-election) cross-sectional variation in policy uncertainty sensitivity at the firm level.

3.3.5 Balance sheet data and other data

We also obtain quarterly accounting data from COMPUSTAT, daily stock returns from CRSP, VIX data from the CBOE, and stock return factors from Ken French's website. Definitions of all variables used in our tests are contained in Appendix A. All of our balance sheet tests use quarterly data from one year prior to federal election dates to one year after the election takes place.

4 Results

4.1 Estimating Firms' Sensitivities to Economic Policy Uncertainty

To identify policy-sensitive firms, we run OLS regressions of the Baker, Bloom, and Davis (2015) index on each firm's monthly stock returns in the 18 months prior to each election in our sample. We run a separate regression for each firm and each election cycle, so our measure of policy sensitivity is defined at the firm-election cycle level. We then extract the p-value of the regression coefficient on the Baker, Bloom, and Davis (2015) index. We define a firm as being sensitive to economic policy uncertainty during a given election cycle if the p-value is less than or equal to 0.1. In other words, we define a firm as being policy-sensitive if its loading on the Baker, Bloom, and Davis (2015) index is statistically significant, regardless of whether the loading is positive or negative.

Panels B, C, and D of Table 1 present summary statistics regarding the fraction and type of firms that are policy sensitive according to our policy-sensitivity measure described above. Panel B shows that 18% of the firm-years in our sample appear to be policy sensitive. Panel B also shows that there is significant time series variation in the fraction of firms that are defined as sensitive to economic policy uncertainty: for example, the 2008 and 2004 political cycles have the largest proportion of sensitive firms (48% and 22% respectively) while 2010 has the lowest proportion (8%).

Panel C examines the potential persistence of policy sensitivity within firms. In particular, it may be that some firms are policy-sensitive in every election cycle, whereas other firms are never policy-sensitive in any election cycle. However, Panel C shows that this does not appear to be the case; in fact, there are slightly *fewer* cases of "persistent" policy sensitivity than we would expect even if policy sensitivity were i.i.d. across firm-election cycle pairs. Similarly, Panel D shows that there is also very little persistence across industries: firms in the most policy-sensitive industry (real estate) are only policy-sensitive approximately 22% of the time, while firms in the least policy-sensitive industry (agriculture) are still policy-sensitive around 11% of the time. The lack of persistence documented in Panels C and D may seem strange since some firms (such as defense

contractors) should nearly always be sensitive to the government policy environment. However, we are sorting firms into policy-sensitivity buckets based on their *returns*. As such, our identification strategy is ultimately based on *shocks* to policy uncertainty sensitivity, which explains the lack of persistence in policy sensitivity among firms whose businesses depend closely on the government.[18]

We next examine how policy-sensitive firms differ from policy-neutral firms along observable dimensions. Table 2 contains the results of our tests. Panel A examines univariate differences in firm characteristics such as size, leverage, investment, asset intensity, firm profitability, and Tobin's Q (as proxied for by the M/B ratio). The panel shows that policy-sensitive firms tend to be larger, have higher leverage, and have lower asset intensity (PP&E/assets) relative to policy-neutral firms. However, while these results are statistically significant, their economic magnitudes are quite small. For example, policy-sensitive firms have leverage and asset intensity levels that are around 3% and 5% higher and lower than less-sensitive firms, respectively. Hence, while policy-sensitive firms are not identical to less-sensitive firms along every dimension, neither group stands out as being substantively different from the other along most observable measures.

We test this proposition more formally in Panel B. This panel presents the results of a logit regression where the dependent variable is a binary variable taking the value of one if a given firm is policy-sensitive in a given election cycle, and zero otherwise. Our independent variables are the same firm characteristics that we studied in Panel A. Panel B shows that with the exception of book leverage, none of the variables in Panel A appear to be strongly correlated with whether or not a firm is policy-sensitive in a given election cycle. We speculate that the differences we observe in leverage between policy-sensitive and policy-neutral firms are due to the importance of tax policy uncertainty within the Baker, Bloom, and Davis (2015) index.

The results we have presented thus far indicate that policy uncertainty sensitivity varies both within election cycles and within firms. In particular, the lack of persistence within firms and the relatively similar observable characteristics of sensitive versus non-sensitive firms suggest that policy sensitivities most commonly represent distinct "shocks" that are specific to a given election cycle. While policy sensitivities are not determined randomly, this evidence suggests that it is unlikely that the effects we document elsewhere are purely driven by "fundamental" differences between policy-sensitive and policy-neutral firms.

[18] Consistent with this interpretation, we can identify numerous instances in the data where groups of firms become policy-sensitive at exactly the time when the government is considering large-scale regulation changes for that industry. For example, nearly 25% of the firms that we identify as policy-sensitive during the 2004 cycle are utility companies, and data from the U.S. Department of Commerce shows that utilities regulation spiked significantly during the 2004 election cycle.

4.2 Policy-Sensitive Firms and Campaign Contributions

Having documented that some firms are more sensitive to policy uncertainty than others, we now turn to testing our primary hypotheses. We begin by testing the idea that policy-sensitive firms should donate more to candidates to elected office relative to policy-neutral firms. We examine this proposition formally in Table 3, where we regress firms' policy uncertainty sensitivities on their total political contributions and the number of candidates that the firm donates to within a given election cycle. The main variable of interest is *Policy Sensitive*, which is a binary variable that takes the value of one if a firm is policy-sensitive in a given election cycle, and is zero otherwise.

Consistent with our hypothesis, Table 3 shows that policy-sensitive firms donate more to political candidates than policy-neutral firms. Columns (1) – (4) document that policy-sensitive firms' total campaign contributions are 7 – 20% higher than the contributions made by policy-neutral firms. In columns (5) and (6), we further split each firm's political contributions into contributions made to candidates in close elections and contributions made to candidates in other (non-close) elections. These columns shows that policy-sensitive firms contribute more to both types of races, including the close election races we use in our subsequent tests. As a robustness check, we also reconstruct our policy sensitivity measure using 18 months of data ending in July of each election cycle. We then examine firm contributions from August to the end of October. Column (7) shows that policy-sensitive firms still donate more than policy-neutral firms, even when policy sensitivity is defined in an ex-ante fashion relative to donations. Finally, in column (8), we examine the *number* of politicians that firms donate to in a given election cycle and find that policy-sensitive firms donate to a larger number of candidates than policy-neutral firms. Collectively, the results in columns (1) through (8) are consistent with the hypothesis that the marginal value of an extra political connection is larger for policy-sensitive firms, and hence, these firms are more likely to donate to candidates for elected office.

One might be concerned that policy-sensitive firms may be able to forecast election outcomes more accurately than policy-neutral firms. However, columns (9) and (10) of Table 3 show that policy-sensitive firms do not appear to have better forecasting power than their policy-neutral peers when it comes to predicting the winners of close U.S. congressional elections. This result suggests that the outcomes of close elections are still veritable "coin flips" regardless of a firm's sensitivity to economic policy uncertainty.

4.3 Implied Volatility

We are now ready to explore the link between political donations, election outcomes, and firms' subsequent risk-taking and performance.

We begin by examining the implied volatility of politically active firms' at-the-money options following political capital shocks. A number of recent studies have examined the impact of political capital shocks on firms' stock returns (see, e.g., Cooper, Gulen, and Ovtchinnikov (2010), Do, Lee, and Nguyen (2013), Goldman, Rocholl, and So (2009), Acemoglu, Johnson, Kermani, Kwak, and Mitton (2016), Addoum, Delikouras, Ke, and Kumar (2014), Akey (2015), Acemoglu, Hassan, and Tahoun (2015), Borisov, Goldman, and Gupta (2015), and Schoenherr (2015)). Nearly all of these studies find that positive political capital shocks are associated with higher subsequent firm returns. Furthermore, Kelly, Pástor, and Veronesi (2015) examine the time series of implied volatility around political elections and show that implied volatilities are higher just before elections. To our knowledge, however, no one has examined how political capital shocks affect the cross-section of implied volatility.

Table 4 contains the results of our tests. We report results for implied volatility on one-month, three-month, and five-month at-the-money call options, though our results obtain for all option maturities in the OptionMetrics database. Panel A of the table shows that implied volatility decreases following elections for firms receiving "lucky" political capital shocks relative to firms receiving "unlucky" political capital shocks. Columns (1), (3), and (5) contain the results from our baseline differences-in-differences setup, while columns (2), (4), and (6) add the underlying firm's daily stock return and the stock return on the firm's value-weighted industry as control variables.[19] Collectively, columns (1) - (6) show that the relative drop in idiosyncratic volatility for "lucky" firms is quite large; for example, the implied volatility on one-month call options declines by approximately 12% following elections for "lucky" firms ($Close\ Win\ Dummy = 1$) relative to "unlucky" firms ($Close\ Win\ Dummy = 0$). Columns (7) - (9) repeat the analysis from columns (2), (4), and (6) using a continuous measure of a firm's political capital shock ($Net\ Close\ Wins$), with similar results. Finally, column (10) decomposes the $Net\ Close\ Wins$ variable into $Close\ Wins$ and $Close\ Losses$ for one-month implied volatility and confirms that the two variables produce effects of similar magnitude but with opposite sign. Similar results hold for all other option maturities, which gives us comfort that markets are indeed responding to close election shocks (as opposed to

[19]Industry returns are computed using three-digit SIC codes. All of our results are robust to other industry definitions such as one-digit or four-digit SIC codes, Fama-French industry definitions, or GICS definitions.

some other variable) and that we are not simply capturing a "general election effect."

We next use a differences-in-differences-in-differences design to examine how implied volatility changes differ between firms that are sensitive to policy uncertainty versus those that are not. Panel B of Table 4 presents this analysis. The primary coefficients of interest are the triple-difference terms ($Post \times Policy \times Close\,Win\,Dummy$ and $Post \times Policy \times Net\,Close\,Wins$, respectively), which capture the difference in treatment effects between "lucky" versus "unlucky" *policy-sensitive* firms and "lucky" versus "unlucky" *policy-neutral* firms. The triple interaction terms are negative and highly significant in all specifications, indicating that the magnitude of the "wedge" between lucky and unlucky outcomes is larger for policy-sensitive firms than for policy-neutral firms.[20] The magnitudes of the triple-difference coefficients are also much larger than the magnitudes of the difference-in-difference coefficients in all specifications, suggesting that a large fraction of the reduction in implied volatility comes through better connections to politicians in times when the firm is more sensitive to policy uncertainty. For example, in the case of five-month option-implied volatilities, the political capital effect for policy-sensitive firms is 2.5 larger than for policy-neutral firms (-.0572 vs. -.0220).

We also use the results in Panel B to examine the more general relationship between policy uncertainty and firms' implied volatilities. In particular, we compare implied volatilities across firms that have the *same* political capital shocks and face the *same* general election shock, but that differ in their ex-ante policy sensitivities. This allows us to infer the effects of policy uncertainty sensitivity on implied volatilities by comparing differences in outcomes across policy-sensitive versus policy-neutral firms experiencing the same political capital shock. We begin by examining the average shocks to policy-sensitive and policy-neutral firms. In particular, the coefficients on $Post\,Election$ and $Post \times Policy\,Sensitive$ in specifications $(7) - (9)$ of Panel B can be interpreted as the average trends for policy-neutral firms and the differential effect for policy-sensitive firms, respectively, holding political capital shocks constant. Interestingly, we find that implied volatilities typically move *higher* for policy-sensitive firms relative to policy-neutral firms in the post-election period. For example, in specification (7), the average effect for policy-neutral firms is -0.00503, but the average effect for policy sensitive firms is 0.1097.

[20] The triple-difference term measures the quantity $(\Delta Lucky\ PS - \Delta Unlucky\ PS) - (\Delta Lucky\ PN - \Delta Unlucky\ PN)$, where PS stands for policy-sensitive firms, PN stands for policy-neutral firms, and Δ indicates the difference between post-election and pre-election implied volatilities. Since relative post-election implied volatilities go down for "lucky" firms and go up for "unlucky" firms (regardless of policy sensitivities), both of the terms in parentheses are negative. Hence, the negative loading on the triple-difference term indicates that the term inside the first parenthesis is *more* negative than the term inside the second parenthesis, which in turn indicates that the "wedge" between lucky and unlucky outcomes is larger in magnitude for policy-sensitive firms.

Our empirical design also allows us to identify whether the link between policy uncertainty and implied volatility varies based on whether the firm in question obtained a *positive* or *negative* political capital shock. In particular, specification (2) of Panel B shows that policy-sensitive firms that experience "unlucky" draws seem to fare particularly poorly relative to similarly unlucky policy-neutral firms: indeed, their post-election changes in implied volatility are 33.2% higher than similarly unlucky policy-neutral firms (0.534 vs. 0.400). This accords with intuition: when faced with the same general election shock and the same (negative) political capital shock, policy-sensitive firms' implied volatilities respond more sharply upwards relative to similar policy-neutral firms. However, the converse is not true for positive political capital shocks: specification (2) shows that policy-sensitive firms that have a "lucky" draw have an implied volatility that is *still* 20.9% higher than their policy-neutral peers (0.455 vs. 0.370). In other words, the effect of policy uncertainty on implied volatilities asymmetrically depends on the firm's political capital shock: "unlucky" shocks hurt policy-sensitive firms particularly strongly, but "lucky" shocks do not help policy-sensitive firms to the same degree.[21] However, in Section 5, we also show that this asymmetry disappears when macroeconomic control variables are added to our regressions. Hence, it appears that the effects we document that are related to *policy* uncertainty are symmetric across lucky and unlucky political capital shocks for both types of firms (policy-neutral and policy-sensitive).

4.4 Credit Default Swaps

Our second measure of firm riskiness is credit default swaps. Our CDS tests proceed in the same fashion as our tests for implied volatility – we first conduct a difference-in-difference analysis for firms that have "lucky" election draws and firms that have "unlucky" election draws and next perform a triple-difference analysis to capture differences in these effects between policy-sensitive and policy-neutral firms. An increase in CDS spreads indicates an increase in the (expected) credit risk associated with a firm, while a decrease in CDS spreads indicates a decline in expected credit risk. Panel A of Table 5 presents our difference-in-difference results. Consistent with our results on implied volatility, we find that "lucky" shocks to political capital are associated with lower ex-post credit risk. The first six columns in the table examine the effects of political capital shocks on one-

[21] The asymmetry we document may potentially be related to the asymmetry documented by Acemoglu, Hassan, and Tahoun (2015), who examine stock price reactions to firms connected to different political groups during a period of extreme political turmoil in Egypt during 2011. Acemoglu, Hassan, and Tahoun (2015) find that, when one political group appears to be more likely to take power, stock prices of firms connected to this political group do not rise significantly relative to politically-unconnected firms, but stock prices of firms connected to *rival* political groups fall significantly in value. They argue that the asymmetry they observe may reflect differences in expectations about the ability of firms to seek rents following major political change.

year, five-year, and 10-year CDS spreads. Columns (1), (3), and (5) contain the results from our baseline difference-in-difference specification, while columns (2), (4), and (6) add a host of control variables to the specification. All six columns shows that CDS spreads drop significantly for "lucky" firms in the six months following U.S. federal elections. The drop in firms' expected credit risk for "lucky" firms is substantial; for example, one-year log CDS spreads decline by more than 30% for "lucky" firms ($Close\ Win\ Dummy = 1$) relative to "unlucky" firms ($Close\ Win\ Dummy = 0$). As with implied volatility, we also decompose the *Net Close Wins* variable into close wins and close losses. Columns (7)–(9) show that the loadings on the close wins and close losses variables are symmetric in magnitude and opposite in sign.

Panel B presents the results of our triple-difference analysis. Consistent with our analysis of implied volatility, we find that the wedge between "lucky" and "unlucky" outcomes is larger for policy-sensitive firms than for policy-neutral firms. Furthermore, the differences in economic magnitude between the triple-difference terms and the difference-in-difference terms are even larger than the effects we found for implied volatility: the *smallest* relative difference between the triple-difference and difference-in-difference terms is about 3.5 (specification (3)), while the largest difference is 7.5 (specification (2)). These results strongly suggest that most of the reduction in CDS spreads occurs among the subset of firms that are significantly exposed to policy uncertainty.

We also use the results in Panel B to examine the more general relationship between policy uncertainty and CDS spreads. As with implied volatilities, we compare CDS spreads across firms that have the *same* political capital shocks and face the *same* general election shock, but that differ in their ex-ante policy sensitivities. We begin by examining the average shocks to policy-sensitive and policy-neutral firms. As with implied volatilities, the coefficients on $Post\,Election$ and $Post \times Policy\,Sensitive$ in columns (7) − (9) of Panel B can be interpreted as the average trends for policy-neutral firms and the differential effect for policy-sensitive firms, respectively, holding political capital shocks constant. For example, column (8) shows that the average change in five-year CDS spreads is -9.4% for policy-neutral firms, but +39.8% for policy-sensitive firms. Hence, as with implied volatilities, we find that CDS spreads on average *increase* for policy-sensitive firms following an election.

We next examine whether this general increase in CDS spreads for policy-sensitive firms varies based on whether firms obtained a positive or negative political capital shock. In particular, column (4) of Panel B shows that policy-sensitive firms that experience "unlucky" political capital shocks again seem to fare particularly poorly relative to similarly unlucky policy-neutral firms: their CDS

spreads increase by 46% versus a decrease of 8% for policy-neutral firms. However, we again find an asymmetry in responses: policy-sensitive firms that receive a "lucky" political capital shock see their CDS spreads rise by 16%, while similarly "lucky" policy-neutral firms experience a 14% drop in CDS spreads. However, this (again) appears to be a product of macroeconomic uncertainty rather than policy uncertainty, as we show in Section 5.

4.5 Firms' Operating Decisions and Performance

Tables 4 and 5 suggest that market-driven proxies for firm risk-taking decline following positive political capital shocks, and decline particularly strongly (in magnitude) in the case of policy-sensitive firms. However, these tables do not shed light on *how* firms' risk-taking might be changing following positive political capital shocks. To address this question, Tables 6 and 7 examine how firms' investment, leverage, R&D spending, Q, profitability, and operational performance respond to "lucky" political capital shocks. As in tables 4 and 5, we begin by examining the differential response of "lucky" winners versus "unlucky" losers following the outcomes of close elections. We then further split our sample based on whether firms are particularly sensitive to economic policy uncertainty during a given election cycle.

Table 6 examines how firms' investment, leverage, and R&D spending behavior respond to political capital shocks. The results in Panel A suggest that firms do not appear to significantly adjust their investment, leverage, or R&D spending policies in response to a political capital shock: the interaction term between the post-election and close-election dummy variables is statistically zero in every specification. The fact that we do not find a differential post-election change in leverage between "lucky" and "unlucky" firms contrasts with Khwaja and Mian (2005) and Claessens, Feijen, and Laeven (2008), who find that leverage is positively associated with political connections. Our findings of no differential effects on investment and R&D also contrast with Do, Lee, and Nguyen (2013), Ovtchinnikov, Reza, and Wu (2014), Kim (2015), and Schoenherr (2015), who report evidence that political capital shocks have significant effects on investment and innovation (though in different directions).

However, when we segment our sample further based on firms' differing sensitivity to economic policy uncertainty, we find that policy-sensitive firms' investment and leverage *do* respond strongly to political capital shocks (though we still find no effects on R&D spending). In particular, Panels B and C show that policy-sensitive firms respond to a "lucky" political capital shock by *increasing* investment and *decreasing* leverage relative to similar firms experiencing an "unlucky" shock. Our

investment and leverage results are economically large: holding all else equal, "lucky" policy-sensitive firms' investment increases by about 9% and leverage decreases by about 2% relative to "unlucky" firms that are also sensitive to economic policy shocks. In contrast, there are virtually no differences in investment or leverage between "lucky" policy-neutral firms and "unlucky" policy-neutral firms. Hence, consistent with the idea that the marginal value of a political connection is greater for policy-sensitive firms, we find that policy-sensitive firms respond to election outcomes far more sharply (in magnitude) than policy-neutral firms: overall, we observe a 10% relative difference in investment levels and a 2% relative difference in leverage. These results also suggest that previous findings in the literature on variables such as investment may be driven by policy-sensitive firms.

To measure the effect of policy uncertainty on investment and leverage, we again return to Table 6. We begin by focusing on investment and leverage differences between "unlucky" policy-sensitive firms and "unlucky" policy-neutral firms. Consistent with policy uncertainty having a significant effect on risk-taking, we find that investment decreases significantly more (-16%) and leverages increases significantly more (2.2%) for policy-sensitive firms than for policy-neutral firms following unlucky political capital shocks. However, when we examine investment differences between lucky policy-sensitive versus policy-neutral firms, we see that investment still *decreases* for policy-sensitive firms (relative to policy-neutral firms) following positive political capital shocks, while leverage changes are statistically identical for both groups of firms. That said, Section 5 again shows that this asymmetry appears to be a function of firms' exposure to macroeconomic uncertainty rather than policy uncertainty.

Table 7 extends the tests in Table 6 to examine how firms' operating performance and profitability respond to political capital shocks. Panel A present difference-in-difference results, while Panels B and C present triple-difference results. The results in Panel A suggest that "lucky" firms experience higher sales, higher returns on assets, and higher Q than "unlucky" firms following close election outcomes. These results are in line with the existing literature.[22]

However, Panels B and C show that these results are largely driven by policy sensitive-firms. We again start by examining differences between "lucky" versus "unlucky" policy-sensitive firms. We find that "lucky" policy-sensitive firms respond significantly more positively than "unlucky" policy-sensitive firms: sales are higher by 6%, ROA is higher by 0.4%, COGS is lower by 3%, profit margins improve by 4%, and firm value (as measured by Tobin's Q) increases by 15%. In

[22]For example, Amore and Bennedsen (2013), Goldman, Rocholl, and So (2013), Tahoun (2014), and Akey (2015) find evidence that sales growth increases following an increase in political connectedness.

contrast, we find much smaller differences when we examine the responses of "lucky" policy-neutral firms versus "unlucky" policy-neutral firms. Hence, as in Table 6, we find that policy-sensitive firms respond to election outcomes far more sharply (in magnitude) than policy-neutral firms. These results suggest that many existing findings in the literature – such as the findings that firms achieve higher sales and higher firm value following positive political capital shocks – appear to be largely driven by policy-sensitive firms.

We next attempt to isolate the effects of policy uncertainty on operating performance and firm value. Consistent with the results on investment, leverage, implied volatility, and CDS spreads, we find that an asymmetry exists between policy uncertainty and performance: policy-sensitive firms hit with a negative political capital shock have *really* poor performance, while we observe much smaller performance differences between "lucky" policy-sensitive and policy-neutral firms. For example, Q is 18% lower, sales are 7% lower, ROA is 0.4% lower, and COGS is 3% higher for unlucky policy-sensitive firms versus unlucky policy-neutral firms. However, most of the differences between lucky policy-sensitive and policy-neutral firms are economically small. Hence, just as in the case of investment and leverage, we find that the relationship between policy uncertainty and future operating performance is not linear: unlucky political capital shocks have a dramatic effect on policy-sensitive firms, while policy sensitivity has less of an effect on outcomes when a firm experiences a positive political capital shock. However, these results again must be tempered by our finding in Section 5 that the asymmetries we observe in Tables 4 through 7 appear to be driven by macroeconomic rather than policy uncertainty.

4.6 Policy Sensitivity and Congressional Committees

Our previous results identify the average effects of political capital shocks on firm risk-taking for policy-sensitive versus policy-neutral firms. However, some political connections may be more valuable than others. In this section, we exploit the structure of the U.S. Congress to provide further support for the idea that policy-sensitive firms react more sharply than policy-neutral firms to similar political capital shocks.

4.6.1 Senate versus House Connections

All else equal, a connection to a Senator should be more valuable than a connection to a Representative, since there are only 100 Senators (versus 435 Representatives) and Senators serve much longer terms in office (six years, versus two years for Representatives). Hence, we would expect

firms to respond more sharply to a political capital shock involving a Senate candidate, *particularly* if a firm is policy-sensitive during a given election cycle.

To test this hypothesis, we begin by defining the variable $Net\,Senate\,Wins$ as the number of winning Senate candidates that firm i supported in close elections during election cycle t minus the number of losing Senate candidates that firm i supported in close elections during the same election cycle. We define the variable $Net\,House\,Wins$ analogously. These definitions simply split the $Net\ Close\ Wins$ variable used in previous tests into Senate and House components. We then estimate the same triple-difference specification used in previous tests after substituting $Net\,Senate\,Wins$ and $Net\,House\,Wins$ for the $Net\ Close\ Wins$ variable.

Table 8 contains the results of our tests (for brevity we only report results for CDS spreads and investment; however, other firm response variables produce similar findings). For policy-sensitive firms, we find that the marginal effect of an extra political connection on firm outcomes is larger in magnitude for Senate connections relative to House connections. In particular, the triple-difference terms in columns 5-8 (Senate connections) are larger in magnitude than the corresponding terms in columns 1-4 (House connections).[23] In contrast, no clear pattern emerges for policy-neutral firms (and overall magnitudes are significantly smaller). These findings support our previous findings by suggesting that gaining or losing a *particularly* important political connection has a larger effect on firm outcomes when the firm is particularly sensitive to the overall government policy environment.

4.6.2 Powerful Senate Committees

We next examine shocks to the composition of five powerful Senate committees: (i) Appropriations, (ii) Finance, (iii) Energy and Natural Resources, (iv) Banking, Housing, and Urban Development, and (v) Commerce, Science, and Transportation. These five committees have jurisdiction over the vast majority of government policy activity that affects publicly-listed firms (in contrast to other Senate committees such as Indian Affairs, Intelligence, or Foreign Relations, whose mandates will typically affect listed companies in an indirect capacity, if at all). As such, we hypothesize that firms – and particularly policy-sensitive firms – may respond more sharply to the loss or gain of a connection to a member of one of these powerful committees relative to a general Senate candidate.

To test this hypothesis, we construct a net close-election wins variable for each firm during each election cycle for each of the five committees under study. For example, $Net\ Appropriations$

[23] As a sanity check, we can also compare the magnitudes in Table 8 against the magnitudes in Table 5 (which are based on the pooled sample of Senate and House connections). Consistent with intuition, we find that Senate magnitudes > pooled sample (Senate + House) magnitudes > House magnitudes.

measures the net number of close-election wins associated with Senate Appropriations Committee members for firm i during election cycle t. We then estimate triple-difference specifications similar to those in Table 8. We only report results for five-year CDS spreads for brevity; however, other left-hand side variables produce similar results.

Table 9 presents the results of our analysis. Consistent with our hypothesis, we find that policy-sensitive firms respond very sharply to political capital shocks associated with members of powerful Senate committees. Comparing the magnitudes of the triple-difference coefficients in Tables 8 (all Senate connections) and 9 (powerful Senate committee connections), we see that the magnitudes in Table 9 are significantly larger for policy-sensitive firms both in economic and statistical terms. However, the magnitudes for policy-neutral firms are if anything slightly *smaller* in Table 9 relative to Table 8. Hence, we find that policy-sensitive firms' CDS spreads react strongly to political capital shocks involving powerful Senate committee members, while policy-neutral firms' CDS spreads react similarly regardless of whether or not a Senator is a member of a powerful Senate committee.

4.6.3 Matching Senate Committees to Firms

Table 9 shows that policy-sensitive firms respond more sharply to political capital shocks involving members of powerful Senate committees. However, we can push the analysis in Table 9 even further by pairing powerful Senate committees with firms in the industries that these committees directly oversee. For example, we might expect an energy firm to respond more sharply to a political capital shock involving a member of the Senate Energy and Natural Resources Committee than, say, a member of the Senate Commerce Committee. Furthermore, by comparing the responses of policy-sensitive and policy-neutral firms within the *same* industry to political capital shocks involving members of the *same* Senate committee, we can arguably rule out any industry-wide trends in policy sensitivity or political donation activity within a given election cycle that might be driving our results.

As in Table 9, we test these hypotheses through triple-difference specifications where the "net close wins" variable is defined at the Senate committee level. However, unlike Table 9, we now directly match firms in specific industries with the Senate committees that oversee each industry. In particular, we match firms in the utilities and communications industries to the Commerce, Science, and Transportation Committee, firms in the energy and mining industries to the Energy and Natural Resources Committee, and firms in the banking and insurance industries with the

Finance committee.[24] This matching process allows us to test whether (i) policy-sensitive firms respond more strongly to shocks involving a member of the Senate committee that oversees their industry, and (ii) whether policy-neutral and policy-sensitive firms respond differently to political capital shocks involving members of the *same* Senate committee that directly oversees each firm's primary area of business.

We also incorporate two types of "placebo" tests into our analysis. First, we match firms from two arguably orthogonal industries (computer hardware and computer software) with political capital shocks involving the Commerce, Energy, and Finance committees. Since none of these Senate committees should play a significant role in developing policy for the computer hardware and software industries, we would expect computer hardware and software firms to respond less sharply to shocks involving members of the Commerce, Energy, and Finance committees. As a second placebo test, for each of the three Senate committees under study, we construct similar regressions using all firms *outside* of the industries that are overseen by that committee. For example, for the Senate Energy Committee, this placebo test would include all firms other than energy and mining firms. Again, we would expect non-energy firms to respond less sharply to shocks to the Energy committee than energy firms, even if the non-energy firms are themselves policy-sensitive in our sample.

Table 10 contains the results of our tests. Comparing the first three columns in Table 10 with the relevant columns in Table 9, we see that policy-sensitive firms overseen by the Commerce, Energy, and Finance committees respond far more sharply to shocks to their "primary" Senate committee relative to the general sample of policy-sensitive firms. Indeed, the point estimates we obtain in these tests are the largest point estimates we obtain out of all of our tests. In other words, policy-sensitive firms respond the most sharply to political capital shocks exactly where one would expect them to: when a close-election political capital shock involves a member of a powerful Senate committee that directly oversees the firm's activities. In contrast, we do not find the same pattern for policy-neutral firms. Hence, even when comparing policy-sensitive and policy-neutral firms *within the same industry, matched to the same Senate committee*, we find that policy-sensitive firms appear to respond more forcefully than policy-neutral firms to political capital shocks involving politicians with direct oversight of their industry.

[24] We drop the Senate Appropriations and Banking, Housing, and Urban Development committees from our analysis because these committees have broad mandates over (respectively) government spending and housing/monetary policy, which likely affect many firms across many different industries. Industries are defined using the Fama-French 49-industry classification system.

The next three columns in Table 10 contain the results of our first series of placebo tests involving firms in the computer hardware and software industries. As expected, we find that policy-sensitive firms in the computer hardware and software industries do not appear to react strongly to political capital shocks involving members of the Senate Commerce, Energy, or Finance committees. Comparing the results in the first three columns and second three columns of Table 10, we see that the triple-difference coefficients are all statistically and economically larger in magnitude for policy-sensitive firms in industries that are directly overseen by the relevant Senate committees. However, we again do not find similar results for policy-neutral firms: we find that policy-neutral firms in both relevant and irrelevant industries react similarly to shocks to the membership of the Senate Commerce, Energy, and Finance committees.

Our second set of placebo tests pairs the Senate committee responsible for overseeing industry X with all firms not in industry X. The results from these tests are reported in columns 7-10 of Table 10. In particular, column 10 shows that policy-sensitive firms in unrelated industries respond significantly less strongly to political capital shocks involving the Senate Commerce, Energy, and Finance committees than similar policy-sensitive firms in the industries that are directly overseen by these committees. In contrast, the differences in magnitudes between policy-neutral firms in related and unrelated industries are economically and statistically insignificant. Collectively, the results in Table 10 provide strong support for the hypothesis that it is *policy-sensitive* firms that respond the most sharply to a gain or loss in political connectedness.[25]

5 Robustness

We perform a variety of tests to examine the robustness of our main results to different empirical specifications. One concern with the Baker, Bloom, and Davis (2015) index is that this index may be capturing general economic uncertainty rather than policy-related uncertainty. Indeed, the correlation between the Baker, Bloom, and Davis (2015) index and the VIX index is about 0.4, suggesting that the Baker, Bloom, and Davis (2015) index may be picking up residual traces of uncertainty that are unrelated to the government policy environment.

To examine the robustness of our results to our use of the Baker, Bloom, and Davis (2015) index, we begin by re-estimating our policy-sensitivity regressions using the Fama-French factors (market, size, value, and momentum) and the VIX index as control variables.[26] This estimation procedure

[25] In untabulated tests, we replicate these splits using a quadruple-difference approach and find similar results.

[26] To further rule out general uncertainty, we also perform a similar (untabulated) analysis using the Jurado,

should allow us to better isolate policy uncertainty relative to other sources of uncertainty in the economy. We also replace our p-value-based definition of policy sensitivity with a decile-based methodology that defines a firm as being policy-sensitive in a given election cycle if its loading on the policy uncertainty index is in either the top or bottom decile. This procedure helps to ensure that a particular election cycle (such as 2008) is not driving our results. As a falsification test, we also re-estimate our main results using deciles formed from loadings on ex-ante firm return sensitivities to the VIX index (as opposed to loadings on the policy uncertainty index). If policy uncertainty sensitivity is driving our results, we would expect to find far weaker results when our triple-difference specification is estimated using VIX decile cutoffs rather than policy uncertainty decile cutoffs. Finally, as an alternative to the economy-wide Baker, Bloom, and Davis (2015) index, we define a *firm-specific* index of policy uncertainty based on firms' 10-K filings. In particular, we count the number of times the terms "government policy(-ies)" and "uncertainty" are referenced in firms' 10-K filings and classify a firm as being policy-sensitive if the number of references to these terms is in the top quintile of all firms during a given election cycle.

Table 11 presents the results of these robustness tests. We focus on CDS spreads for brevity, but our results are qualitatively similar using other dependent variables. Panel A contains the results of our decile tests (with extra controls), while Panel B contains the results of our 10-K tests. Columns (1) – (3) of Panel A show that our main results remain unchanged (and if anything are stronger) after we add control variables to our policy uncertainty sensitivity regressions and define policy sensitivity based on decile cutoffs. Columns (4) – (6) of Panel A show that our results largely go away (as expected) when we replace firms' policy uncertainty sensitivities with their sensitivities to the VIX index. Panel B shows that our main results also hold when we replace the Baker, Bloom, and Davis (2015) index with an index based on firms' references to government policy and uncertainty in their 10-K filings.

Table 11 also allows us to better understand the asymmetry between policy-sensitive and policy-neutral firms' outcomes that we documented in Tables 4 through 7. We previously found that, relative to unlucky policy-neutral firms, unlucky policy-sensitive firms had particularly poor outcomes (as expected). However, we also found that, relative to lucky policy-neutral firms, lucky policy-sensitive firms did *not* appear to have particularly good outcomes. Hence, we previously documented an asymmetry between the effects of political capital shocks on policy-sensitive versus policy-neutral firms.

Ludvigson, and Ng (2015) macroeconomic uncertainty index and find similar results.

Interestingly, Table 11 shows that this asymmetry in outcomes disappears when when we add macroeconomic control variables to our policy-sensitivity regressions. This can be seen by examining the coefficients on $Post \times Policy\,Sensitive$ in regressions where the treatment variable is $Net\,Close\,Wins$ (which includes all regressions in Table 11). For example, when we include additional macroeconomic control variables in Panel A of Table 11 (columns (1) – (3)), we find that the coefficients on $Post \times Policy\,Sensitive$ are economically and statistically insignificant, whereas these coefficients are large in magnitude, positive, and statistically significant in similar tests from Tables 4 through 7 that do not include macroeconomic controls. Similarly, Panel B shows that we do not find a statistically significant asymmetry in outcomes when policy sensitivity is defined using 10-K filings rather than using the Baker, Bloom, and Davis (2015) index. However, columns (4) – (6) of Panel A show that the previously-documented asymmetry *re*-appears when we sort firms on their sensitivity to *general* uncertainty (using the VIX index) rather than policy uncertainty. Taken together, these results suggest that the asymmetry we documented in previous tables appears to be a function of general (or macroeconomic) uncertainty rather than policy uncertainty.

We further verify that our results are robust to varying the window used to estimate policy sensitivities. We re-compute our sensitivity measures allowing for a lag of three to five months between the end of the sensitivity estimation period and the election date. Untabulated tests show that none of our results change materially, although economic magnitudes become smaller (as would be expected). We also recompute our political connection measures after excluding all contributions made in the two months leading up to each election. None of our results are materially different.

We next examine the sensitivity of our results to changes in our testing assumptions. For example, many of our empirical tests use daily data, since this allows us to include daily covariates such as firm and industry stock returns in our tests. However, following Bertrand, Duflo, and Mullainathan (2004), we verify that our results are robust to collapsing our data into one pre-event observation and one post-event observation per firm-election cycle pair. Furthermore, while our main results are clustered by firm-election cycle, we verify that clustering by firm (as recommended by Bertrand, Duflo, and Mullainathan (2004)) does not change our results. We also run a variety of parallel trends tests and placebo tests and find that all of the conditions for inference in a difference-in-difference setting are met within our sample.

It is also worth pointing out that the marginal effects we document in the paper are likely to be temporary rather than permanent in nature. In particular, the policy "state variable" will not remain constant following an election, nor will firms' stocks of political connections (or the influence

of politicians) remain constant. Indeed, the effects we estimate are likely to have a term structure (in ongoing work, we are documenting the term structure of political capital shocks). As such, in line with the arguments in Hennessy and Strebulaev (2015), our results should be thought of as capturing conditional (and possibly time-varying) marginal effects.

6 Alternative Mechanisms

Our results suggest a "policy sensitivity" channel of political capital accumulation by firms. However, the literature has proposed a number of alternative theories to explain firms' donations to politicians. One possibility is that firms establish political connections to insure themselves against future shocks — i.e., a bailout story (see, e.g., Faccio, Masulis, and McConnell (2006) and Duchin and Sosyura (2012)). A second possibility is that firms establish political connections to increase the probability of winning future government contracts or other government funding (see, e.g., Cohen and Malloy (2014)). A final possibility is that firms establish political connections in order to alleviate financial constraints by using political influence to secure additional financing (see, e.g., Khwaja and Mian (2005), Claessens, Feijen, and Laeven (2008)). While we view our results as being complementary to these channels, we nonetheless examine these potential mechanisms below to ensure that none of them can fully explain our results.

6.1 Bailout Likelihood

Policy-sensitive firms may donate to politicians in order to increase the likelihood of receiving a government bailout. If this is true, it implies that the differences we observe in risk-taking and performance between policy-sensitive and policy-neutral firms may be driven by a "tail risk" channel rather than a policy-sensitivity channel. We use the Marginal Expected Shortfall (MES) measure developed by Acharya, Pedersen, Philippon, and Richardson (2010) to test this hypothesis. MES measures a firm's expected stock return conditional on the market index experiencing an extreme negative return. Lower (more-negative) values of MES indicates higher exposure to tail risk. In Table 12, we estimate a triple-difference specification to examine how firms' MES responds to political capital shocks, and whether these responses are different for policy-sensitive versus policy-neutral firms. We find no statistically significant differences in post-election MES between policy-sensitive and policy-neutral firms receiving similar political capital shocks. As such, it is unlikely that the differences between policy-sensitive and policy-neutral firms documented elsewhere in the

paper are being driven by an increase in the probability of a government bailout.[27]

6.2 Government Contractors

A second potential channel that has been described in the literature is a "government contracting" channel. Intuitively, large government contractors have an incentive to support political candidates who can help them to earn future sales. As such, firms experiencing a positive political capital shock (and hence, a higher probability of obtaining government contracts) may simply kick back and enjoy the "quiet life," since their future earnings streams are expected to be less affected by market competition.[28] Cohen and Malloy (2014) find evidence consistent with this argument: they find that investment is lower and operating performance is weaker at government-dependent firms.

To test this story, we download segment data from COMPUSTAT and classify firms as being "government-dependent" in a given quarter if they list the U.S. Government (or a government entity) as one of their operating segments. We then examine whether our previous results on risk-taking are being driven primarily by government-dependent firms.[29] Table 13 contains the results of our tests. In the table, we have chosen to focus on CDS spreads; however, we obtain similar results for our other tests. Table 13 shows that government contractors behave much like the other firms in our sample. For example, columns (4)-(6) show that when we limit our sample to only include government-dependent firms, we still obtain qualitatively and quantitatively similar results to our prior findings. Likewise, columns (7)-(9) confirm that our main findings still obtain after excluding government-dependent firms from our sample. Hence, our results do not appear to be fully explained by the "government contracting" hypothesis.

6.3 Financial Flexibility

Another strand of the literature argues that politically-connected firms may be able to obtain "extra" debt financing (often from politically-connected banks) relative to less-connected firms (see, e.g., Khwaja and Mian (2005) and Claessens, Feijen, and Laeven (2008)). This "extra" financing may help politically-connected firms to overcome financial constraints or invest in politically-

[27] We also find that positive political capital shocks are associated with improvements in subsequent operating performance. In contrast, the existing literature on bailouts finds that politically-connected firms have poor subsequent operating performance (Faccio, Masulis, and McConnell (2006); Duchin and Sosyura (2012)).

[28] Alternatively, government-dependent firms experiencing a positive political capital shock may reduce risk-taking in order to avoid distress, which may affect the firm's ability to benefit from future government contracts.

[29] We identify government-dependent firms differently than Cohen and Malloy (2014). They examine regulatory filings to find firms who obtain more than 10% of sales from the U.S. government, whereas we simply examine firms that have a separate operating segment for government sales.

beneficial projects. However, we find that leverage *decreases* overall for policy-sensitive firms experiencing lucky political capital shocks. As such, our results do not appear to be driven by a leverage or financing channel.

6.4 Longstanding Industry-Political Party Affiliations

We also run a series of tests to ensure that we are not simply picking up industry-specific effects in our results. In particular, one possibility is that our "political capital shocks" may simply be picking up longstanding political affiliations between certain industries and certain political parties. For example, it may be that technology firms always give to Democrats, and Democrats in a given election cycle were more likely to win close elections because Democrats in general did well in that cycle. If this is true, our previous "political capital" results could be driven by general industry-political party affiliations rather than the gain or loss of a connection to a specific politician.

To rule out this hypothesis, we replace the firm-election cycle fixed effects that were used in Tables 4 through 10 with a combination of firm fixed effects and *industry*-election cycle fixed effects (where industry definitions are based on Fama and French (1997)'s 49-industry classification system). Table 14 shows that all of our main results are qualitatively and quantitatively similar when we look within industry-election cycles rather than firm-election cycles. Furthermore, the correlation between firms' net close-election wins variable (*Net Close Wins*) and the "winner" of the general election (for example, Democrats in 2006 and 2008; Republicans in 2010) is effectively zero. Finally, our Senate committee tests in Table 10 show that our main results still hold even when we look within industries at very specific political capital shocks affecting the Senate committee with oversight for that industry. Hence, the effects we document in Tables 4 through 10 do not seem to be driven by industry-specific political affiliations or general election trends.

7 Conclusion

This paper links firms' cross-sectional sensitivities to economic policy uncertainty to their subsequent political activity and post-election operating decisions and performance. Our motivation is built around three key ideas. First, we argue that the marginal value of an extra political connection should be larger if a firm is highly sensitive to economic policy uncertainty. Intuitively, a firm exposed to significant policy-related uncertainty should particularly value the influence or information offered by politicians, suggesting that policy uncertainty may be a key explanatory

factor in firms' political donation decisions. Second, we argue that firms' risk-taking and performance will systematically vary following elections based on whether the firm gained or lost political connections. Finally, we argue that shocks to a firm's political connectedness will trigger stronger responses among firms that are highly sensitive to policy uncertainty. Intuitively, if political connections are more valuable to firms that are highly exposed to government policy uncertainty, then the gain or loss of a political connection should have a larger potential impact on these firms' subsequent operating decisions and performance.

To test these ideas, we begin by classifying firms into "policy-sensitive" and "policy-neutral" categories based on the sensitivity of their stock returns to the Economic Policy Uncertainty index developed by Baker, Bloom, and Davis (2015). We then examine whether policy-sensitive firms are more likely to donate to candidates in U.S. congressional elections than their policy-neutral peers. We next exploit shocks to firms' political connectedness stemming from close U.S. congressional elections to identify the relationship between firms' policy sensitivities and their subsequent risk-taking and performance. For a wide range of risk-taking and performance measures, we compare outcomes between firms that had the *same* political capital shock but *different* ex-ante policy sensitivities. As such, we are able to estimate the marginal effects of policy uncertainty on firm outcomes holding firms' political connectedness constant. Our setting also allows us to compare outcomes between firms that have the *same* policy sensitivity but *different* political capital shocks, allowing us to more cleanly estimate the marginal effects of a political capital shock on firms' subsequent risk-taking.

Our main findings can be summarized as follows. First, consistent with the idea that the marginal value of a political connection is larger for policy-sensitive firms, we find that policy-sensitive firms are more likely to increase political campaign contributions relative to policy-neutral firms. Second, we find that the gain or loss of a political connection has a much larger effect on the risk-taking and performance of policy-sensitive firms. This result holds across a wide range of operating and performance variables including implied volatility, CDS spreads, firm value, investment, leverage, and sales, suggesting that many of the average effects documented in the literature on political connections appear to be driven by policy-sensitive firms. We also find that the differential effects of a political capital shock on policy-sensitive firms are even larger when the politician in question is a Senator or sits on a powerful congressional committee. Collectively, our findings point to a new rationale for firms' engagement in the political process and show that political connections have a greater impact on the subsequent risk-taking and performance of policy-sensitive firms.

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Appendix A - Variable Definitions

Variable	Definition	Source
Post Election	A binary variable that takes the value of 1 in all time periods following an election.	CRSP
Close Wins	The number of winning candidates involved in a close general election that a firm donated to prior to the election	Federal Election Commission and Authors' Computation
Close Losses	The number of losing candidates involved in a close general election that a firm donated to prior to the election	Federal Election Commission and Authors' Computation
Net Close Wins	Close Wins - Close Losses	Federal Election Commission and Authors' Computation
Post × Net Close Wins	Net Close Wins multiplied by Post Election	Federal Election Commission and Authors' Computation
Post × Close Wins	Close Wins multiplied by Post Election	Federal Election Commission and Authors' Computation
Post × Close Losses	Close Losses multiplied by Post Election	Federal Election Commission and Authors' Computation
Close Win Dummy	A binary variable that takes the value of 1 if Net Close Wins > 0	Federal Election Commission and Authors' Computation
Post × Close Win Dummy	Close Win Dummy multiplied by Post Election	Federal Election Commission and Authors' Computation
Policy Sensitive	A binary variable that takes the value of 1 if we identify a firm as policy sensitive as described in the text	CRSP, Baker, Bloom, and Davis (2015), and Authors' Computation
Post× Policy × Net Close Wins	Net Close Wins multiplied by Post Election and Policy Sensitive	CRSP, Baker, Bloom, and Davis (2015), and Authors' Computation
Post× Policy × Close Win Dummy	Close Win Dummy multiplied by Post Election and Policy Sensitive	CRSP, Baker, Bloom, and Davis (2015), and Authors' Computation
Ln(Total Contributions)	The natural logarithm of a firm's total contributions to all House and Senate elections in an election cycle	Federal Election Commission and Authors' Computation
Ln(Close-Election Contributions)	The natural logarithm of a firm's total contributions to all House and Senate elections that were won or lost by a margin of 5 percentage points or less in an election cycle	Federal Election Commission and Authors' Computation
Ln(Other Contributions)	The natural logarithm of a firm's total contributions to all House and Senate elections that were won or lost by a margin of greater than 5 percentage points in an election cycle	Federal Election Commission and Authors' Computation
Ln(Late Contributions)	The natural logarithm of a firm's total contributions to all House and Senate elections in the last three months of an election cycle	Federal Election Commission and Authors' Computation
Ln(Number of Candidates Donated To)	The natural logarithm of the number of Senate and House candidates that a firm donated to in an election cycle	Federal Election Commission and Authors' Computation
Government Contractor	A binary variable that takes value of 1 if a firm discloses that the US Government is a major Customer	Compustat
Ln(Mkt Cap)	The natural logarithm of a firm's market capitalization	CRSP
CAPM Beta	A firm's CAPM Beta	CRSP and Authors' Computation
CAPM Vol	The standard deviation of residuals of a from annual Beta regressions	CRSP and Authors' Computation
MES	A firm's Marginal Expected Shortfall computed following Acharya et al. (2010)	CRSP and Authors' Computation
Log1y	The natural log of a firm's 1-year CDS spread	Markit
Log5y	The natural log of a firm's 5-year CDS spread	Markit
Log10y	The natural log of a firm's 10-year CDS spread	Markit

1-month implied volatility	The implied volatility of a firm's 1-month at-the-money call options	OptionMetrics
3-month implied volatility	The implied volatility of a firm's 3-month at-the-money call options	OptionMetrics
5-month implied volatility	The implied volatility of a firm's 5-month at-the-money call options	OptionMetrics
Firm return	A firm's daily (unless otherwise noted) stock return (variable: ret)	CRSP
M/B	A firm's market capitalization divided by its lagged book value of equity (variables: prc, shrout, ceqq)	CRSP, Compustat
VW ind return	The value-weighted return on a firm's 3-digit SIC industry (weights are based on market capitalization)	CRSP
Ln(Size)	The natural log of the firm's book value of assets (variable: atq)	Compustat
Investment	Quarterly capital expenditures divided by lagged net PP&E (variables: capxy (adjusted), ppentq)	Compustat
R&D spending	Quarterly R&D expenditure divided by book assets (variables: xrdq, atq)	Compustat
Book leverage	Quarterly book value of debt divided by book assets (variables: dlcq, dlttq, atq)	Compustat
EBITDA growth	Quarter-over-quarter change in a firm's EBITDA (variable: oibdpq)	Compustat
Profit margin	EBIT divided by sales (variables: oiadpq, revtq)	Compustat
COGS	Cost of goods sold divided by sales (variables: cogsq, revtq)	Compustat
SG&A	Selling, general, & administrative expenses divided by sales (variables: xsgaq, revtq)	Compustat
Cash	Cash & cash equivalents divided by book assets (variables: cheq, atq)	Compustat
Current ratio	Current assets divided by current liabilities (variables: actq, lctq)	Compustat
Profitability (ROA)	EBITDA divided by assets (variables: oiadpq, atq)	Compustat

Figure 1: Net Close Wins Histogram

The figure below shows the distribution of $Net\,Close\,Wins$ measured from 1998-2010.

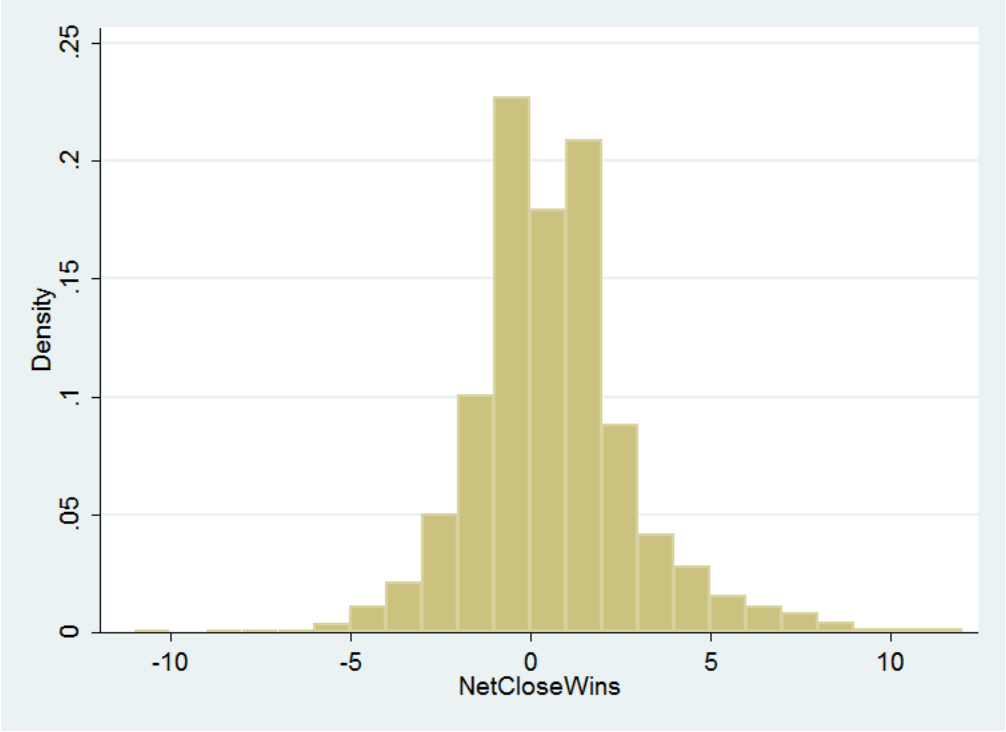

Figure 2: Parallel Trends Test: CDS Spreads

The figure below plots average log CDS spreads for "lucky" versus "unlucky" firms in the pre- and post-election periods. The figure shows that the "parallel trends" assumption appears to hold in the pre-election period.

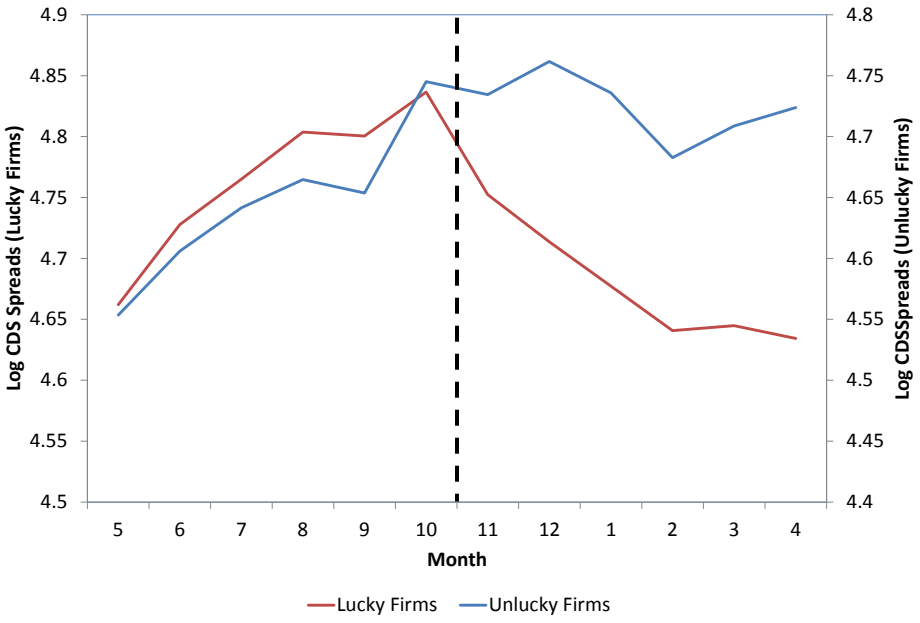

Table 1: Summary Statistics

Panel A presents summary statistics for (i) political connections data (taken from Federal Election Commission filings), (ii) firm accounting data (Compustat), (iii) implied volatility data (OptionMetrics), and (iv) CDS spreads (Markit). Variable definitions can be found in the text and Appendix A. Panels B, C, and D report summary statistics for firms that are sensitive to the Economic Policy Uncertainty (EPU) index of Baker, Bloom, and Davis (2015) based on our estimation procedure (details of which can be found in the text). Panel B reports the number and proportion of firms in each election cycle that are sensitive to the EPU index as well as the fraction of sensitive firms whose EPU sensitivities are positive and negative, respectively. Panel C reports summary statistics on the number of election cycles that a given firm is policy-sensitive according to our estimation procedure. Panel D reports summary statistics regarding the industry distribution of policy-sensitive firms across our sample period (1998-2010).

Panel A — Political Connections, Firm Fundamentals, Implied Volatility, and CDS Spreads					
Data Type	**Variable**	**Mean**	**Median**	**Std. Dev.**	**Number**
Political Connections	*Net Close Wins*	0.13	0	2.37	7,838
	Close Wins	2.85	2	3.22	7,838
	Close Losses	2.71	2	2.77	7,838
	Total Contributions	$118,762	39,950	231,214	5,433
	Close Election Contributions	$16,328	7,000	25,896	3,988
	Other Contributions	$107,469	35,775	210,956	5,398
Firm Fundamentals	*LnSize*	8.898	8.943	1.598	22,353
	Leverage	0.656	0.658	0.189	22,353
	M/B	2.853	2.036	2.638	21,152
	ROA	0.023	0.0203	0.024	21,913
	I_t/K_{t-1}	0.049	0.0399	0.0347	20,462
Implied Volatility	1 *month implied volatility*	0.4038	0.3495	0.2290	842,190
	3 *month implied volatility*	0.3934	0.3453	0.2123	840,089
	5 *month implied volatility*	0.3869	0.3423	0.2025	830,831
CDS Spreads	1 *year spread*	0.0183	0.0035	0.0878	355,735
	5 *year spread*	0.0214	0.0078	0.0569	388,325
	10 *year spread*	0.0216	0.0094	0.0498	359,382

Panel B — Firm Sensitivity to Economic Policy Uncertainty						
Election Cycle	**All Firms**	**Policy-Sensitive Firms**	**Fraction Sensitive**	**Std. Dev.**	**Positive Sensitivity**	**Negative Sensitivity**
1998	10,211	1,463	0.143	0.350	29%	71%
2000	9.698	1,248	0.129	0.335	41%	59%
2002	8,195	938	0.114	0.318	43%	57%
2004	7,376	1,586	0.215	0.411	93%	7%
2006	7,462	905	0.122	0.326	32%	68%
2008	7,646	3,689	0.482	0.500	7%	93%
2010	7,203	568	0.079	0.270	39%	61%
Total	57,791	10,397	0.180	0.382	35%	65%

Panel C — Number of Policy-Sensitive Election Cycles Per Firm (Sample restricted to firms present in all seven election cycles)			
Number of Cycles where Firm is Policy-Sensitive	**Firm Count**	**Empirical Distribution**	**Binomial Dist. ($p = 0.180$)**
0 cycles	840	26.7%	25.0%
1 cycle	1,317	41.8%	38.3%
2 cycles	761	24.2%	25.2%
3 cycles	195	6.2%	9.2%
4 cycles	32	1.0%	2.0%
5 cycles	4	0.1%	0.3%
6 cycles	0	0.0%	0.0%
7 cycles	0	0.0%	0.0%
Test: Actual = Binomial	Chi-Square	p-Value	N
	64.60	< 0.0001	3,149

Table 1: Summary Statistics (Continued)

Panel D — Number of Policy-Sensitive Firms Per Fama-French 49 Industry				
Industry	**Industry Number**	**Count**	**Mean**	**Std. Dev.**
Real Estate	47	304	0.224	0.417
Computers	35	773	0.210	0.407
Electronic Equipment	37	2095	0.208	0.406
Non-Metallic and Industrial Metal Mining	28	208	0.202	0.402
Measuring and Control Equipment	38	657	0.199	0.4
Communication	32	1379	0.198	0.399
Precious Metals	27	350	0.191	0.394
Machinery	21	1006	0.191	0.393
Shipbuilding and Railroad Equipment	25	69	0.188	0.394
Chemicals	14	506	0.186	0.389
Fabricated Products	20	88	0.182	0.388
Candy and Soda	3	150	0.180	0.385
Business Services	34	2365	0.179	0.383
Transportation	41	794	0.179	0.383
Electrical Equipment	22	834	0.179	0.383
Trading	48	8344	0.175	0.38
Defense	26	63	0.175	0.383
Textiles	16	132	0.174	0.381
Construction	18	401	0.172	0.378
Apparel	10	349	0.172	0.378
Restaurants, Hotels, and Motels	44	685	0.171	0.377
Insurance	46	1176	0.169	0.375
Aircraft	24	149	0.168	0.375
Computer Software	36	2549	0.167	0.373
Tobacco Products	5	60	0.167	0.376
Steel Works	19	458	0.166	0.372
Medical Equipment	12	1049	0.166	0.372
Recreation	6	271	0.162	0.369
Petroleum and Natural Gas	30	1411	0.162	0.368
Almost Nothing	49	268	0.160	0.368
Printing and Publishing	8	351	0.160	0.367
Entertainment	7	444	0.158	0.365
Automobiles and Trucks	23	457	0.155	0.363
Business Supplies	39	368	0.155	0.362
Construction Materials	17	475	0.154	0.361
Wholesale	42	1407	0.154	0.361
Utilities	31	1043	0.153	0.361
Consumer Goods	9	339	0.153	0.361
None	None	850	0.152	0.359
Pharmaceutical Products	13	1965	0.149	0.356
Healthcare	11	626	0.141	0.348
Shipping Containers	40	100	0.140	0.349
Banking	45	4130	0.140	0.347
Coal	29	80	0.138	0.347
Retail	43	1610	0.137	0.344
Beer and Liquor	4	156	0.135	0.342
Personal Services	33	404	0.134	0.341
Food Products	2	483	0.124	0.33
Rubber and Plastic Products	15	205	0.117	0.322
Agriculture	1	104	0.106	0.309

Table 2: Economic Policy Uncertainty Sensitivity and Firm Characteristics

This table presents summary statistics for firms that are sensitive to Economic Policy Uncertainty and for those that are not sensitive. Panel A presents univariate differences, while Panel B presents results from a Logit analysis with an indicator variable that takes the value of one if a firm has been classified as sensitive to economic policy uncertainty and 0 otherwise as the dependent variable. Details of this estimation procedure are found in the text. Standard errors are presented in parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A — Univariate Differences

	Ln(Size)	Book Leverage	Investment / Capital	Market / Book	Net PPE / Assets	Profit Margin	Return on Assets
Other firms	9.057	0.684	0.052	3.005	0.312	0.104	0.021
	(0.012)	(0.001)	(0.001)	(0.024)	(0.002)	(0.005)	(0.001)
	22,668	22,415	19,387	21,480	21,779	20,501	22,115
Policy-sensitive firms	9.282	0.706	0.051	2.973	0.297	0.118	0.021
	(0.027)	(0.004)	(0.001)	(0.059)	(0.004)	(0.004)	(0.001)
	4,866	4,796	4,310	4,564	4,657	4,480	4,743
Difference	0.225***	0.021***	-0.001	-0.031	-0.015***	0.014	0.000
	(0.028)	(0.004)	(0.001)	(0.059)	(0.004)	(0.011)	(0.001)

Panel B — Logit Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
	Policy-	Policy-	Policy-	Policy-	Policy-	Policy-
Variable	Sensitive	Sensitive	Sensitive	Sensitive	Sensitive	Sensitive
$ln(Size)$	0.0595**	0.0580	0.00672	0.00356	0.0284	0.0284
	(0.0282)	(0.0467)	(0.0373)	(0.0437)	(0.0484)	(0.0586)
$Book\ Leverage$	0.610**	0.634	1.003***	1.008**	1.112**	1.112*
	(0.276)	(0.439)	(0.372)	(0.453)	(0.505)	(0.603)
I_t/K_{t-1}	-0.546	-0.511	-1.224	-1.203	-1.195	-1.195
	(0.844)	(1.164)	(1.084)	(1.283)	(1.242)	(1.421)
M/B	-0.00658	-0.00906	0.0115	0.00880	0.0170	0.0170
	(0.0114)	(0.0170)	(0.0133)	(0.0162)	(0.0178)	(0.0203)
$Profit\ Margin$	0.0187	0.0149	0.0529	0.0488	0.0781	0.0781
	(0.0495)	(0.0582)	(0.0723)	(0.0753)	(0.108)	(0.112)
$Net\ PP\&E/Assets$	-0.222	-0.207	-0.223	-0.190	0.0368	0.0368
	(0.179)	(0.489)	(0.230)	(0.336)	(0.416)	(0.442)
ROA	1.460	1.857	1.410	1.552	3.147	3.147
	(2.191)	(2.510)	(2.963)	(3.182)	(3.518)	(3.475)
$Intercept$	-2.324***	-2.325***	-2.766***	-2.718***	-16.59***	-16.59***
	(0.299)	(0.546)	(0.424)	(0.549)	(3.832)	(1.205)
Fixed effects	None	None	Cycle	Cycle	FF-Cycle	FF-Cycle
Clustering	Firm	FF-Cycle	Firm	FF-Cycle	Firm	FF-Cycle
Observations	21,570	21,210	21,570	21,210	14,808	14,808
Pseudo-R squared	0.005	0.005	0.236	0.239	0.262	0.262

Table 3: Economic Policy Uncertainty Sensitivity and Political Contributions

This table documents the correlation between firms' policy sensitivity and their political contributions. The dependent variable in columns (1) – (4) is the natural logarithm of the amount a firm contributed to candidates in House and Senate races. This variable is then decomposed in Columns (5) and (6) into contributions to candidates in close elections (Column 5) and contributions to candidates in other elections (Column 6). The dependent variable in column (7) is the same as in columns (1) – (4); however, the policy sensitivity estimation period ends in the July prior to each November election and contributions are measured only from August to October. The dependent variable in column (8) is the (log) number of politicians that a firm donated to within a given election cycle. Finally, the dependent variable in columns (9) and (10) represents the net number of donation recipients who won their respective (close) elections within a given election cycle. The primary independent variable of interest is *Policy Sensitive*, which takes a value of one if a firm is defined as policy-sensitive during a given election cycle based on its return correlation with the Baker, Bloom, and Davis (2015) Economic Policy Uncertainty index. All other independent variables are defined in the Appendix. Standard errors are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Ln(Total Contributions)	(2) Ln(Total Contributions)	(3) Ln(Total Contributions)	(4) Ln(Total Contributions)	(5) Ln(Close-Election Contributions)	(6) Ln(Other Contributions)	(7) Ln(Total Late Contributions)	(8) Ln(Number of Candidates Donated To)	(9) Net Close-Election Wins	(10) Net Close-Election Wins
Policy−Sensitive	0.207***	0.194***	0.0792**	0.0749**	0.136**	0.0671*	0.0911*	0.0644**	0.066	0.102
	(0.0568)	(0.0293)	(0.0328)	(0.0352)	(0.0555)	(0.0384)	(0.0517)	(0.0289)	(0.124)	(0.136)
Ln(Size)				0.433***	0.411***	0.437***	0.486***	0.311***		0.194*
				(0.0438)	(0.0510)	(0.0470)	(0.0560)	(0.0335)		(0.104)
Leverage				-0.00583	-0.301	0.0936	-0.368**	0.105		0.0963
				(0.138)	(0.193)	(0.164)	(0.184)	(0.111)		(0.404)
Profitability				0.0257	0.0218	0.0128	0.0324***	0.0202		-0.0611*
				(0.0169)	(0.0172)	(0.0246)	(0.0059)	(0.0157)		-0.0367
M/B				0.00619*	0.00713	0.00522	0.7310	0.0042		0.0029
				(0.00362)	(0.00444)	(0.00391)	(0.458)	(0.0029)		(0.0098)
Cash				0.0348	-0.145	0.172	-0.341	0.027		-0.479
				(0.160)	(0.219)	(0.175)	(0.211)	(0.125)		(0.483)
Intercept	11.11***	11.11***	12.03***	8.293***	4.124***	8.080***	4.984***	1.581***	-0.54	-2.222
	(0.0467)	(0.00519)	(0.490)	(0.614)	(0.874)	(0.607)	(0.656)	(0.494)	(2.635)	(2.844)
Fixed effects	None	Firm	Firm, FF-Cycle	Firm, FF-Cycle	Firm, FF-Cycle	Firm, FF-Cycle	Firm, FF-Cycle	Firm, FF-Cycle	Firm, FF-Cycle	Firm, FF-Cycle
Clustering	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	27,601	27,601	27,190	23,077	23,069	22,925	23,372	23,071	27,190	23,077
R-squared	0.003	0.857	0.905	0.910	0.813	0.902	0.849	0.907	0.515	0.531

Table 4: The Impact of Political Capital Shocks on Implied Volatility

This table documents the effects of political capital shocks on implied volatility using data on firms' donations to political candidates in close U.S. federal elections. Panel A presents the difference-in-difference analysis for firms that have "lucky" political capital shocks before and after an election compared to those that had "unlucky" shocks. Panel B presents a triple difference analysis that examines how this effect varies for firms that are sensitive to economic policy uncertainty. Policy Uncertainty Sensitivity is measured using the correlation between a firm's equity returns and the Baker, Bloom, and Davis (2015) economic policy uncertainty index as defined in the text. Daily implied volatilities are taken from OptionMetrics and are computed using prices on at-the-money call options. Each regression uses daily data from six months' prior to a federal election to six months following the election. Our election data spans all biennial U.S. federal elections from 1998-2010. Specifications with control variables include daily firm stock returns and daily industry stock returns. All independent variables are defined in the Appendix. Standard errors are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A — Difference-in-Difference Analysis

	(1) 1-Month Implied Volatility	(2) 1-Month Implied Volatility	(3) 3-Month Implied Volatility	(4) 3-Month Implied Volatility	(5) 5-Month Implied Volatility	(6) 5-Month Implied Volatility	(7) 1-Month Implied Volatility	(8) 3-Month Implied Volatility	(9) 5-Month Implied Volatility	(10) 1-Month Implied Volatility
Post Election	0.0349***	0.0354***	0.0357***	0.0360***	0.0350***	0.0352***	0.0172***	0.0196***	0.0197***	0.0191***
	(0.00306)	(0.00308)	(0.00297)	(0.00298)	(0.00287)	(0.00288)	(0.00215)	(0.00207)	(0.00200)	(0.00305)
Post × Close Win Dummy	-0.0494***	-0.0495***	-0.0446***	-0.0446***	-0.0421***	-0.0421***				
	(0.00402)	(0.00403)	(0.00386)	(0.00386)	(0.00374)	(0.00375)				
Post × Net Close Wins							-0.0117***	-0.0107***	-0.0101***	
							(0.000757)	(0.000749)	(0.000728)	
Post × Close Wins										-0.0119***
										(0.00074)
Post × Close Losses										0.0113***
										(0.00100)
Intercept	0.395***	0.395***	0.384***	0.384***	0.377***	0.377***	0.395***	0.384***	0.377***	0.395***
	(0.00103)	(0.00103)	(0.000994)	(0.000996)	(0.000962)	(0.000963)	(0.00103)	(0.000990)	(0.000958)	(0.00103)
Controls	No	Yes	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Clustering	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Observations	841,514	841,169	839,403	839,068	830,153	829,818	841,169	839,068	829,818	841,169
R-squared	0.748	0.750	0.788	0.789	0.802	0.803	0.751	0.790	0.804	0.751

Table 4: The Impact of Political Capital Shocks on Implied Volatility (Continued)

Panel B — Triple-Difference Analysis									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	1-Month Implied Volatility	1-Month Implied Volatility	3-Month Implied Volatility	3-Month Implied Volatility	5-Month Implied Volatility	5-Month Implied Volatility	1-Month Implied Volatility	3-Month Implied Volatility	5-Month Implied Volatility
Post Election	0.00527**	0.00566**	0.00599**	0.00623**	0.00598**	0.00617**	-0.00503***	-0.00245	-0.00165
	(0.00267)	(0.00267)	(0.00252)	(0.00252)	(0.00242)	(0.00242)	(0.00188)	(0.00177)	(0.00171)
Post × *Policy Sensitive*	0.134***	0.135***	0.135***	0.136***	0.132***	0.132***	0.113***	0.111***	0.107***
	(0.00889)	(0.00894)	(0.00869)	(0.00873)	(0.00843)	(0.00846)	(0.00731)	(0.00713)	(0.00686)
Post × *Close Win Dummy*	-0.0299***	-0.0298***	-0.0244***	-0.0243***	-0.0220***	-0.0219***			
	(0.00351)	(0.00351)	(0.00329)	(0.00329)	(0.00320)	(0.00320)			
Post × *Policy* × *Close Win Dummy*	-0.0495***	-0.0499***	-0.0552***	-0.0555***	-0.0572***	-0.0574***			
	(0.0155)	(0.0156)	(0.0152)	(0.0153)	(0.0146)	(0.0147)			
Post × *Net Close Wins*							-0.00707***	-0.00590***	-0.00534***
							(0.000706)	(0.000691)	(0.000667)
Post × *Policy* × *Net Close Wins*							-0.0116***	-0.0129***	-0.0134***
							(0.00248)	(0.00246)	(0.00238)
Intercept	0.395***	0.395***	0.384***	0.384***	0.377***	0.377***	0.395***	0.384***	0.377***
	(0.000954)	(0.000956)	(0.000914)	(0.000916)	(0.000884)	(0.000885)	(0.000952)	(0.000912)	(0.000882)
Controls	No	Yes	No	Yes	No	Yes	Yes	Yes	Yes
Fixed effects	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Clustering	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Observations	841,514	841,169	839,403	839,068	830,153	829,818	841,169	839,068	829,818
R-squared	0.759	0.761	0.801	0.802	0.815	0.816	0.762	0.802	0.816

Table 5: The Impact of Political Capital Shocks on CDS Spreads

This table documents the effects of political capital shocks on CDS Spreads using data on firms' donations to political candidates in close U.S. federal elections. Panel A presents the difference-in-difference analysis for firms that have "lucky" political capital shocks before and after an election compared to those that had "unlucky" shocks. Panel B presents a triple difference analysis that examines how this effect varies for firms that are sensitive to economic policy uncertainty. Policy Uncertainty Sensitivity is measured using the correlation between a firm's equity returns and the Baker, Bloom and Davis (2015) economic policy uncertainty index as defined in the text. Daily CDS spreads are taken from Markit for 1-year, 5-year, and 10-year tenors. All CDS spreads are then expressed in log form. Each regression uses daily data from six months' prior to a federal election to six months following the election. Specifications with controls include daily firm stock returns, Tobin's Q, Leverage, Size, and Operating Ratio. Our election data spans all biennial U.S. federal elections from 1998-2010. All independent variables are defined in the Appendix. Standard errors are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A — Difference-in-Difference Analysis

	(1) 1-Year Log CDS Spread	(2) 1-Year Log CDS Spread	(3) 5-Year Log CDS Spread	(4) 5-Year Log CDS Spread	(5) 10-Year Log CDS Spread	(6) 10-Year Log CDS Spread	(7) 1-Year Log CDS Spread	(8) 5-Year Log CDS Spread	(9) 10-Year Log CDS Spread	(10) 1-Year Log CDS Spread
Post Election	0.0439	0.0561*	0.0795***	0.0838***	0.105***	0.105***	-0.0593***	0.0160	0.0474***	-0.0690**
	(0.0322)	(0.0322)	(0.0189)	(0.0188)	(0.0161)	(0.0161)	(0.0218)	(0.0128)	(0.0111)	(0.0311)
Post × Close Win Dummy	-0.309***	-0.329***	-0.187***	-0.194***	-0.161***	-0.164***				
	(0.0393)	(0.0394)	(0.0238)	(0.0235)	(0.0209)	(0.0207)				
Post × Net Close Wins							-0.0678***	-0.0420***	-0.0356***	
							(0.00647)	(0.00398)	(0.00352)	
Post × Close Wins										-0.0675***
										(0.00637)
Post × Close Losses										0.0701***
										(0.00865)
Intercept	-5.523***	-0.116	-4.747***	-1.642**	-4.545***	-2.713***	-0.0622	-1.616**	-2.697***	-0.0427
	(0.0103)	(1.299)	(0.00618)	(0.674)	(0.00542)	(0.578)	(1.246)	(0.646)	(0.559)	(1.243)
Controls	No	Yes	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Clustering	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Observations	355,735	298,451	388,325	325,090	359,382	302,068	298,451	325,090	302,068	298,451
R-squared	0.898	0.899	0.920	0.924	0.917	0.919	0.900	0.925	0.920	0.900

Table 5: The Impact of Political Capital Shocks on CDS Spreads (Continued)

Panel B — Triple-Difference Analysis									
	(1) 1-Year Log CDS Spread	(2) 1-Year Log CDS Spread	(3) 5-Year Log CDS Spread	(4) 5-Year Log CDS Spread	(5) 10-Year Log CDS Spread	(6) 10-Year Log CDS Spread	(7) 1-Year Log CDS Spread	(8) 5-Year Log CDS Spread	(9) 10-Year Log CDS Spread
Post Election	-0.235***	-0.238***	-0.0765***	-0.0822***	-0.0280*	-0.0353**	-0.257***	-0.0974***	-0.0487***
	(0.0299)	(0.0297)	(0.0175)	(0.0173)	(0.0148)	(0.0146)	(0.0209)	(0.0122)	(0.0104)
Post × *Policy Sensitive*	0.911***	0.945***	0.517***	0.539***	0.435***	0.449***	0.686***	0.398***	0.342***
	(0.0680)	(0.0678)	(0.0414)	(0.0410)	(0.0354)	(0.0352)	(0.0583)	(0.0358)	(0.0307)
Post × *Close Win Dummy*	-0.0797**	-0.0807**	-0.0667***	-0.0585***	-0.0599***	-0.0528***			
	(0.0362)	(0.0358)	(0.0217)	(0.0211)	(0.0192)	(0.0186)			
Post × *Policy* × *Close Win Dummy*	-0.560***	-0.607***	-0.260***	-0.307***	-0.200***	-0.236***			
	(0.120)	(0.125)	(0.0767)	(0.0811)	(0.0660)	(0.0690)			
Post × *Net Close Wins*							-0.0258***	-0.0175***	-0.0163***
							(0.00733)	(0.00438)	(0.00387)
Post × *Policy* × *Net Close Wins*							-0.0964***	-0.0573***	-0.0394***
							(0.0181)	(0.0117)	(0.0103)
Intercept	-5.525***	-2.248**	-4.750***	-2.858***	-4.547***	-3.796***	-2.215**	-2.834***	-3.775***
	(0.00916)	(1.116)	(0.00556)	(0.601)	(0.00490)	(0.540)	(1.104)	(0.593)	(0.535)
Controls	No	Yes	No	Yes	No	Yes	Yes	Yes	Yes
Fixed effects	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Clustering	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Observations	354,146	298,366	386,523	325,005	357,732	301,983	298,366	325,005	301,983
R-squared	0.909	0.911	0.926	0.931	0.923	0.926	0.911	0.931	0.926

Table 6: The Impact of Political Capital Shocks on Investment, Capital Structure, and R&D Spending

Panel A presents differences-in-differences tests that examine how "lucky" firms' investment, leverage, and R&D spending change following a positive shock to the firms' political capital. Panels B and C present a triple-difference analysis that examines how the effect of a "lucky" political capital shock varies for firms that are highly sensitive to economic policy uncertainty within a given election cycle. Policy Uncertainty Sensitivity is measured using the correlation between a firm's equity returns and the Baker, Bloom and Davis (2015) economic policy uncertainty index as defined in the text. Each regression is based on quarterly data from CRSP/Compustat from four quarters prior to each election date to four quarters after each election date. All independent variables are defined in the Appendix. Control variables include firm size, M/B, free cash flow / assets, and net PP&E / assets for investment and R&D regressions, plus operating profitability and the firm's current ratio for leverage regressions. Banks and firms missing industry classification codes are excluded from the sample. Investment and M/B are trimmed at the 1% and 99% percentiles. The sample is also restricted to firms with leverage ratios between zero and one (inclusive). Standard errors are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1 % levels, respectively.

Panel A — Difference-in-Difference Analysis

	(1) Investment $\frac{I_t}{K_{t-1}}$	(2) Investment $\frac{I_t}{Assets_{t-1}}$	(3) Book Leverage $\frac{Liabilities_t}{Assets_t}$	(4) Book Leverage $\frac{Debt_t}{Assets_t}$	(5) R&D Spending $\frac{R\&D\ Expense_t}{Assets_t}$	(6) R&D Spending $\frac{R\&D\ Expense_t}{Sales_t}$
Post Election	0.00002	0.000194	0.00675***	0.00114	0.000572*	0.00322
	(0.000591)	(0.000218)	(0.00172)	(0.00148)	(0.000318)	(0.00214)
Post × Close Win Dummy	0.00103	0.000001	-0.00177	-0.00162	-0.000192	-0.00745
	(0.000880)	(0.000320)	(0.00239)	(0.00235)	(0.000510)	(0.00579)
Intercept	0.156***	0.0635***	0.534***	-0.0113	0.0771***	0.728**
	(0.0224)	(0.00892)	(0.0788)	(0.0706)	(0.0270)	(0.310)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Clustered errors	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Observations	18,368	18,368	18,267	18,267	6,573	6,570
R-Squared	0.691	0.783	0.947	0.941	0.721	0.752

Panel B — Triple-Difference Analysis

	(1) Investment	(2) Investment	(3) Book Leverage	(4) Book Leverage	(5) R&D Spending	(6) R&D Spending
Post Election	0.00210***	0.000855***	0.00252	-0.00246	0.000690*	0.00430*
	(0.000674)	(0.000244)	(0.00183)	(0.00167)	(0.000403)	(0.00240)
Post × Close Win Dummy	-0.000622	-0.000511	0.00183	0.00204	-0.000146	-0.00864
	(0.000972)	(0.000353)	(0.00255)	(0.00261)	(0.000578)	(0.00727)
Post × Policy Sensitive	-0.00844***	-0.00268***	0.0177***	0.0151***	-0.000445	-0.00420
	(0.00139)	(0.000536)	(0.00438)	(0.00375)	(0.000826)	(0.00651)
Post × Policy × Close Win Dummy	0.00538**	0.00168**	-0.0134**	-0.0167***	-0.000794	0.00575
	(0.00237)	(0.000839)	(0.00671)	(0.00556)	(0.00137)	(0.0118)
Intercept	0.164***	0.0662***	0.516***	-0.0252	0.0780***	0.734**
	(0.0225)	(0.00896)	(0.0781)	(0.0706)	(0.0268)	(0.309)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Clustering	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Observations	18,368	18,368	18,267	18,267	6,573	6,570
R-Squared	0.693	0.784	0.948	0.941	0.721	0.752

Table 6: The Impact of Political Capital Shocks on Investment, Capital Structure, and R&D Spending (continued)

Panel C — Triple-Difference Analysis with Continuous Treatment						
	(1)	(2)	(3)	(4)	(5)	(6)
	Investment	Investment	Book Leverage	Book Leverage	R&D Spending	R&D Spending
Post Election	0.00187***	0.000647***	0.00325**	-0.00169	0.000629**	0.00461
	(0.000500)	(0.000175)	(0.00144)	(0.00125)	(0.000268)	(0.00287)
Post × *Net Close Wins*	-0.000127	-0.000064	0.000239	0.000378	-0.000014	-0.000533
	(0.000185)	(5.85e-05)	(0.000508)	(0.000536)	(7.75e-05)	(0.000629)
Post × *Policy Sensitive*	-0.00605***	-0.00191***	0.0120***	0.00810***	-0.000703	-0.00146
	(0.00114)	(0.000404)	(0.00334)	(0.00282)	(0.000663)	(0.00410)
Post × *Policy* × *Net Close Wins*	0.00148***	0.000451**	-0.00309**	-0.00393***	-0.000093	0.000077
	(0.000459)	(0.000201)	(0.00140)	(0.00109)	(0.000259)	(0.00190)
Intercept	0.164***	0.0662***	0.516***	-0.0257	0.0780***	0.729**
	(0.0225)	(0.00893)	(0.0781)	(0.0706)	(0.0269)	(0.309)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Clustering	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Observations	18,368	18,368	18,267	18,267	6,573	6,570
R-Squared	0.693	0.784	0.948	0.941	0.721	0.752

Table 7: The Impact of Political Capital Shocks on Operating Performance and Profitability

Panel A presents differences-in-differences tests that examine how "lucky" firms' operating performance and profitability change following a positive shock to the firms' political capital. Panel B and C present a triple-difference analysis that examines how the effect of a "lucky" political capital shock varies for firms that are highly sensitive to economic policy uncertainty within a given election cycle. Policy Uncertainty Sensitivity is measured using the correlation between a firm's equity returns and the Baker, Bloom and Davis (2015) economic policy uncertainty index as defined in the text. Each regression is based on quarterly data from CRSP/Compustat from four quarters prior to each election date to four quarters after each election date. All independent variables are defined in the Appendix. Banks and firms missing industry classification codes are excluded from the sample. M/B is trimmed at the 1% and 99% percentiles. Standard errors are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1 % levels, respectively.

Panel A — Difference-in-Difference Analysis

	(1) Sales $ln(Sales_t)$	(2) Assets $ln(Assets_t)$	(3) M/B M/B_t	(4) ROA $\frac{EBIT_t}{Assets_{t-1}}$	(5) COGS $\frac{COGS_t}{Sales_t}$	(6) SG&A $\frac{SG\&A_t}{Sales_t}$	(7) Profit Margin $\frac{EBITDA_t}{Sales_t}$
Post Election	0.0402***	0.0743***	-0.213***	-0.00243***	-0.000225	-0.00629	0.00556
	(0.00567)	(0.00532)	(0.0387)	(0.000366)	(0.0152)	(0.00778)	(0.0230)
Post × Close Win Dummy	0.0198**	-0.00955	0.169***	0.00238***	0.00389	-0.00222	-0.00440
	(0.00880)	(0.00783)	(0.0557)	(0.000521)	(0.0165)	(0.00952)	(0.0244)
Intercept	7.146***	8.864***	2.953***	0.0234***	0.688***	0.216***	0.0994***
	(0.00216)	(0.00195)	(0.0140)	(0.000132)	(0.00462)	(0.00253)	(0.00692)
Fixed effects	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Clustering	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Observations	22,296	22,353	21,152	21,913	22,190	15,837	22,184
R-Squared	0.984	0.993	0.857	0.695	0.607	0.746	0.679

Panel B — Triple-Difference Analysis

	(1) Sales	(2) Assets	(3) M/B	(4) ROA	(5) COGS	(6) SG&A	(7) Profit Margin
Post Election	0.0699***	0.0885***	-0.0953**	-0.00158***	-0.00753	-0.00971	0.0184
	(0.00592)	(0.00625)	(0.0403)	(0.000380)	(0.0194)	(0.0102)	(0.0295)
Post × Close Win Dummy	-0.00697	-0.0214**	0.0692	0.00166***	0.0124	0.000478	-0.0172
	(0.00947)	(0.00882)	(0.0589)	(0.000557)	(0.0207)	(0.0119)	(0.0310)
Post × Policy Sensitive	-0.132***	-0.0628***	-0.525***	-0.00379***	0.0323	0.0141	-0.0568*
	(0.0146)	(0.0112)	(0.106)	(0.00102)	(0.0222)	(0.0105)	(0.0316)
Post × Policy × Close Win Dummy	0.106***	0.0421**	0.365**	0.00265**	-0.0427*	-0.00717	0.0563*
	(0.0214)	(0.0188)	(0.159)	(0.00134)	(0.0239)	(0.0125)	(0.0333)
Intercept	7.146***	8.864***	2.953***	0.0234***	0.688***	0.216***	0.0994***
	(0.00212)	(0.00194)	(0.0139)	(0.000131)	(0.00462)	(0.00253)	(0.00692)
Fixed effects	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Clustering	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Observations	22,296	22,353	21,152	21,913	22,190	15,837	22,184
R-Squared	0.984	0.993	0.858	0.696	0.607	0.746	0.679

Table 7: The Impact of Political Capital Shocks on Operating Performance and Profitability (continued)

Panel C — Triple-Difference Analysis with Continuous Treatment							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Sales	Assets	M/B	ROA	COGS	SG&A	Profit Margin
Post Election	0.0666***	0.0803***	-0.0654**	-0.000933***	-0.00167	-0.00946	0.0107
	(0.00469)	(0.00452)	(0.0303)	(0.000289)	(0.0114)	(0.00626)	(0.0172)
Post × *Net Close Wins*	0.000716	-0.00397**	0.00308	0.000282***	-0.000928	-7.75e-05	0.000167
	(0.00187)	(0.00166)	(0.0113)	(0.000105)	(0.00146)	(0.000600)	(0.00162)
Post × *Policy Sensitive*	-0.0839***	-0.0446***	-0.370***	-0.00235***	0.0118	0.0103	-0.0317*
	(0.0113)	(0.00918)	(0.0803)	(0.000692)	(0.0133)	(0.00655)	(0.0185)
Post × *Policy* × *Net Close Wins*	0.0270***	0.0108***	0.104***	0.00114***	-0.00900*	-0.00248**	0.0105**
	(0.00463)	(0.00352)	(0.0371)	(0.000378)	(0.00495)	(0.00112)	(0.00493)
Intercept	7.146***	8.864***	2.953***	0.0234***	0.688***	0.216***	0.0995***
	(0.00211)	(0.00194)	(0.0139)	(0.000131)	(0.00461)	(0.00253)	(0.00691)
Fixed effects	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Clustering	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Observations	22,296	22,353	21,152	21,913	22,190	15,837	22,184
R-Squared	0.984	0.993	0.858	0.696	0.607	0.746	0.679

Table 8: Senate vs. House Connections

This table documents the effects of political capital shocks in the House of Representatives and in the Senate on CDS Spreads and Investment using a triple-difference framework to compare firms that had "lucky" shocks to those that had "unlucky" shocks across firms that are policy-sensitive and policy-neutral. Columns (1) – (3) and (5) – (8) present examine firms' log CDS spreads at various maturities using daily data spanning six months before the election to six months after the election. Columns (4) and (8) examine Investment using one year of quarterly Compustat data before and after each election. Policy Uncertainty Sensitivity is measured using the correlation between a firm's equity returns and the Baker, Bloom and Davis (2015) economic policy uncertainty index as defined in the text. Daily CDS spreads are taken from Markit for 1-year, 5-year, and 10-year tenors. All regressions include controls and firm-election cycle fixed effects. Controls include daily firm stock returns, Tobin's Q, Leverage, Size, and Operating Ratio. Standard errors are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10, 5, and 1 % levels respectively.

	(1) 1-Year Log CDS Spreads	(2) 5-Year Log CDS Spreads	(3) 10-Year Log CDS Spreads	(4) $\frac{I}{K}$	(5) 1-Year Log CDS Spreads	(6) 5-Year Log CDS Spreads	(7) 10-Year Log CDS Spreads	(8) $\frac{I}{K}$
Post Election	-0.270***	-0.0954***	-0.0452***	0.00199***	-0.279***	-0.111***	-0.0613***	0.00185***
	(0.0224)	(0.0123)	(0.0104)	(0.000513)	(0.0181)	(0.0109)	(0.00955)	(0.000485)
Post × *Policy Sensitive*	0.790***	0.449***	0.379***	-0.00695***	0.695***	0.411***	0.352***	-0.00550***
	(0.0591)	(0.0357)	(0.0306)	(0.00113)	(0.0613)	(0.0369)	(0.0312)	(0.00116)
Post × *Net House Wins*	-0.00865	-0.0201***	-0.0206***	0.000967*				
	(0.00944)	(0.00517)	(0.00440)	(0.000564)				
Post × *Policy* × *Net House Wins*	-0.0842***	-0.0408***	-0.0235*	-0.000306				
	(0.0226)	(0.0147)	(0.0128)	(0.000215)				
Post × *Net Senate Wins*					-0.0637***	-0.00941	-0.00382	0.000213
					(0.00941)	(0.00649)	(0.00593)	(0.000285)
Post × *Policy* × *Net Senate Wins*					-0.127***	-0.0976***	-0.0781***	0.00249***
					(0.0385)	(0.0232)	(0.0198)	(0.000820)
Intercept	-2.131*	-2.797***	-3.754***	0.163***	-2.310**	-2.900***	-3.838***	0.164***
	(1.132)	(0.599)	(0.531)	(0.0224)	(1.138)	(0.634)	(0.565)	(0.0224)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Clustering	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Observations	298,366	325,005	301,983	18,368	298,366	325,005	301,983	18,368
R-squared	0.909	0.930	0.926	0.693	0.911	0.930	0.926	0.693

Table 9: Senate Committee Connections

This table documents the effects of political capital shocks to powerful Senate Committees on CDS Spreads using a triple-difference framework to compare firms that had "lucky" shocks to those that had "unlucky" shocks across firms that are policy-sensitive and policy-neutral. The dependent variable is five-year log CDS spreads using daily data spanning six months before the election to six months after the election. Policy Uncertainty Sensitivity is measured using the correlation between a firm's equity returns and the Baker, Bloom and Davis (2015) economic policy uncertainty index as defined in the text. All regressions include controls and firm-election cycle fixed effects. Controls include daily firm stock returns, Tobin's Q, Leverage, Size, and Operating Ratio. Standard errors are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10, 5, and 1 % levels respectively.

	(1) 5-Year Log CDS Spread	(2) 5-Year Log CDS Spread	(3) 5-Year Log CDS Spread	(4) 5-Year Log CDS Spread	(5) 5-Year Log CDS Spread
Post Election	-0.121***	-0.130***	-0.116***	-0.0614***	-0.132***
	(0.0129)	(0.0114)	(0.0126)	(0.0195)	(0.0116)
Post × *Policy Sensitive*	0.476***	0.295***	0.461***	0.610***	0.356***
	(0.0393)	(0.0398)	(0.0334)	(0.0394)	(0.0506)
Post × *Net Appropriations*	-0.0154				
	(0.0125)				
Post × *Sensitive*×	-0.136***				
Net Appropriations	(0.0430)				
Post × *Net Finance*		-0.199***			
		(0.0362)			
Post × *Sensitive*×		-0.303***			
Net Finance		(0.0590)			
Post × *Net Energy*			-0.0342***		
			(0.00686)		
Post × *Sensitive*×			-0.156***		
Net Energy			(0.0214)		
Post × *Net Housing/Banking*				-0.0853***	
				(0.0155)	
Post × *Sensitive*×				-0.450***	
Net Housing/Banking				(0.0428)	
Post × *Net Commerce*					-0.0304**
					(0.0143)
Post × *Sensitive*×					-0.185***
Net Commerce					(0.0467)
Intercept	-3.155***	-3.500***	-3.584***	-3.651***	-3.306***
	(0.687)	(0.626)	(0.683)	(0.635)	(0.685)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed effects	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Clustering	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Observations	274,121	274,121	274,121	274,121	274,121
R-squared	0.927	0.931	0.930	0.932	0.928

Table 10: Senate Committee/Industry Connections

This table documents the effects of political capital shocks to powerful Senate Committees on CDS Spreads using a triple-difference framework to compare firms that had "lucky" shocks to those that had "unlucky" shocks across firms that are policy-sensitive and policy-neutral in numerous industry subsamples. The dependent variable is five-year log CDS spreads. Columns (1) – (3) present results where a Senate committee has been matched to an industry that it oversees (the Commerce committee to communication firms and utilities, the Energy Committee to mining and energy firms, and the Finance Committee to banks and insurance firms). Columns (4) – (6) perform the regressions on a "placebo industry" — computer hardware and software firms. Columns (7) – (9) present regressions on all firms that were not included in the regressions presented in columns (1) – (3). Column (10) presents tests of differences in coefficients between the results of Columns (1) – (3) and (7) – (9). All regressions include controls and firm-election cycle fixed effects. Controls include daily firm stock returns, Tobin's Q, Leverage, Size, and Operating Ratio. Standard errors are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10, 5, and 1 % levels respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable	5-Year Log CDS Spread	5-Year Log CDS Spread	5-Year Log CDS Spread	5-Year Log CDS Spread	5-Year Log CDS Spread	5-Year Log CDS Spread	5-Year Log CDS Spread	5-Year Log CDS Spread	5-Year Log CDS Spread	Test of Differences
Subsample	Utilities and Communication	Mining and Energy	Banks and Insurance	Computers and Software	Computers and Software	Computers and Software	Excluding Utilities	Excluding Mining	Excluding Banks	
Post Election	-0.179***	-0.220***	-0.106***	-0.132***	-0.114***	-0.127***	-0.124***	-0.108***	-0.134***	
	(0.0308)	(0.0400)	(0.0287)	(0.0392)	(0.0379)	(0.0418)	(0.0125)	(0.0131)	(0.0124)	
Post × Policy Sensitive	0.0749	0.670***	0.0952	0.373**	0.399***	0.322**	0.409***	0.432***	0.315***	
	(0.0954)	(0.102)	(0.0926)	(0.159)	(0.102)	(0.131)	(0.0544)	(0.0346)	(0.0431)	(1) - (7)
Post × Net Commerce	-0.0196			-0.0452			-0.0353**			0.0157
	(0.0369)			(0.0551)			(0.0152)			(0.0405)
Post × Sensitive× Net Commerce	-0.348***			-0.0388			-0.155***			-0.193**
	(0.0984)			(0.109)			(0.0508)			(0.109)
										(2) - (8)
Post × Net Energy		-0.0111			-0.0333			-0.0342***		0.0231
		(0.0163)			(0.0282)			(0.00732)		(0.0176)
Post × Sensitive× Net Energy		-0.301***			-0.0382			-0.148***		-0.153**
		(0.0496)			(0.0975)			(0.0220)		(0.0522)
										(3) - (9)
Post × Net Finance			-0.253***			-0.138			-0.187***	-0.072
			(0.0711)			(0.0866)			(0.0406)	(0.0780)
Post × Sensitive× Net Finance			-0.736***			-0.104			-0.264***	-0.472**
			(0.185)			(0.171)			(0.0625)	(0.196)
Intercept	-1.652	-2.380*	-3.805**	-1.334	-1.184	-1.326	-3.590***	-3.608***	-3.616***	
	(1.605)	(1.387)	(1.844)	(1.370)	(1.363)	(1.482)	(0.723)	(0.795)	(0.658)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Fixed effects	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	
Clustering	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	
Observations	43,553	22,914	37,490	19,380	19,380	19,380	230,568	251,207	236,631	
R-squared	0.897	0.952	0.931	0.936	0.936	0.937	0.933	0.929	0.932	

Table 11: Policy Sensitivity Robustness Tests

In Panel A, we compute firms' return sensitivities to the Economic Policy Uncertainty index after controlling for the Fama-French factors, the Momentum factor, and the VIX. For each election cycle, we define a firm as being *Policy Sensitive* if its return sensitivity is in the top or bottom decile. We likewise define a firm as being $VIX\ Sensitive$ if its return sensitivity to the VIX index is in the top or bottom decile. In Panel B, we define *Policy Sensitive* based on the number of times that a firm mentions "government policy" or "government policies" and "uncertainty" in its 10-K filing in the year preceding each election cycle (from October $t-1$ to October t). We define a firm as being policy-sensitive in cycle t if the firm is in the top quintile in terms of the number of references to government policy and uncertainty in its most recent 10-K. The symbols *, **, and *** denote statistical significance at the 10, 5, and 1% levels respectively.

Panel A — Policy Sensitivity Definitions using Deciles

	(1)	(2)	(3)	(4)	(5)	(6)
	1-Year Log CDS Spread	5-Year Log CDS Spread	10-Year Log CDS Spread	1-Year Log CDS Spread	5-Year Log CDS Spread	10-Year Log CDS Spread
Post Election	-0.0597***	0.0157	0.0473***	-0.0727***	0.00662	0.0418***
	(0.0223)	(0.0131)	(0.0114)	(0.0223)	(0.0132)	(0.0114)
Post × *Policy Sensitive*	-0.0322	-0.0211	-0.0161			
	(0.0952)	(0.0558)	(0.0443)			
Post × *Net Close Wins*	-0.0649***	-0.0402***	-0.0341***	-0.0629***	-0.0395***	-0.0340***
	(0.00651)	(0.00402)	(0.00356)	(0.00647)	(0.00402)	(0.00360)
Post × *Policy* × *Net Close Wins*	-0.121***	-0.0779***	-0.0634***			
	(0.0373)	(0.0206)	(0.0181)			
Post × $VIX\ Sensitive$				0.199*	0.163***	0.0987*
				(0.110)	(0.0621)	(0.0576)
Post × VIX × *Net Close Wins*				-0.0544**	-0.0193	-0.0130
				(0.0243)	(0.0150)	(0.0139)
Fixed effects	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Clustering	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	298,364	325,003	301,981	298,364	325,003	301,981
R-squared	0.900	0.925	0.920	0.900	0.925	0.920

Panel B — Policy Sensitivity Definitions using 10-K Policy References

	(1)	(2)	(3)	(4)	(5)	(6)
	1-Year Log CDS Spread	5-Year Log CDS Spread	10-Year Log CDS Spread	1-Year Log CDS Spread	5-Year Log CDS Spread	10-Year Log CDS Spread
Post Election	-0.0960***	-0.0024	0.0388***	-0.1070***	-0.0126	0.0284**
	(0.0272)	(0.0155)	(0.0134)	(0.0208)	(0.0130)	(0.0117)
Net Close Wins				0.0105	-0.0001	0.0027
				(0.0105)	(0.0080)	(0.0073)
Policy Sensitive				-0.0001	0.0048	0.0027
				(0.0531)	(0.0390)	(0.0345)
Post × *Net Close Wins*	-0.0495***	-0.0311***	-0.0296***	-0.0514***	-0.0310***	-0.0286***
	(0.0086)	(0.0053)	(0.0048)	(0.0078)	(0.0048)	(0.0045)
Post × *Policy Sensitive*	0.0483	0.0266	0.0056	0.0481	0.0247	0.0076
	(0.0431)	(0.0256)	(0.0223)	(0.0356)	(0.0230)	(0.0198)
Net Close Wins × *Policy*				0.0162	0.0119	0.0065
				(0.0114)	(0.0090)	(0.0083)
Post × *Policy* × *Net Close Wins*	-0.0353***	-0.0217***	-0.0113*	-0.0341***	-0.0208***	-0.0106
	(0.0125)	(0.0077)	(0.0069)	(0.0117)	(0.0072)	(0.0066)
Fixed effects	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm, FF-Cycle	Firm, FF-Cycle	Firm, FF-Cycle
Clustering	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	288,131	313,803	291,723	285,525	311,022	289,023
R-squared	0.905	0.933	0.928	0.833	0.853	0.847

Table 12: Political Capital Shocks and Marginal Expected Shortfall

This table presents regression estimates of various political connection and policy sensitivity measures on firms' Marginal Expected Shortfall (MES). MES, which is defined in Acharya, Pedersen, Philippon, and Richardson (2010), represents the conditional expected return on a stock given a left-tail market return realization. A more negative MES number indicates a greater exposure to systemic/tail risk. Specifications with controls include Ln(Mkt Cap), CAPM Beta, and CAPM Vol. All independent variables are defined in the Appendix. All regressions include firm-election cycle fixed effects. Standard errors clustered by firm are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 % levels respectively.

	(1) Marginal Expected Shortfall	(2) Marginal Expected Shortfall	(3) Marginal Expected Shortfall	(4) Marginal Expected Shortfall	(5) Marginal Expected Shortfall	(6) Marginal Expected Shortfall	(7) Marginal Expected Shortfall	(8) Marginal Expected Shortfall
Post Election	-0.00287***	-0.00218***	-0.00201***	-0.00161***	-0.00259***	-0.00236***	-0.00177***	-0.00175***
	(0.000425)	(0.000329)	(0.000323)	(0.000248)	(0.000463)	(0.000393)	(0.000343)	(0.000294)
Post × Policy Sensitive					-0.00132	0.000853	-0.00156	0.000473
					(0.00119)	(0.000902)	(0.00103)	(0.000772)
Post × Close Win Dummy	0.00232***	0.00156***			0.00232***	0.00178***		
	(0.000697)	(0.000540)			(0.000735)	(0.000586)		
Post × Net Close Wins			0.000649***	0.000473***			0.000664***	0.000559***
			(0.000154)	(0.000123)			(0.000169)	(0.000136)
Post × Policy × Close Win Dummy					-0.000968	-0.00115		
					(0.00231)	(0.00164)		
Post × Policy × Net Close Wins							-0.000328	-0.000471
							(0.000436)	(0.000319)
Intercept	-0.0265***	0.0216	-0.0265***	0.0221	-0.0265***	0.0192	-0.0265***	0.0182
	(0.000160)	(0.0209)	(0.000160)	(0.0209)	(0.000159)	(0.0213)	(0.000159)	(0.0213)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Fixed effects	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Clustering	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	7,774	7,707	7,774	7,707	7,774	7,707	7,774	7,707
R-squared	0.879	0.927	0.879	0.928	0.879	0.927	0.879	0.928

Table 13: Government Contractors and CDS Spreads

The following table presents regression estimates of various political connection measures on log CDS spreads for "government-dependent" versus "non-government-dependent" firms. Specifications (1) - (3) are run on the full sample of firms. Specifications (4) - (6) are run on the sample of Government Suppliers (see text for details), while specifications (7) - (9) exclude government suppliers. All independent variables are defined in the Appendix. All regressions include firm-election cycle fixed effects. Standard errors clustered by firm-election cycle are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10, 5, and 1 % levels respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample:	All Firms	All Firms	All Firms	Gov Firms	Gov Firms	Gov Firms	Non-Gov Firms	Non-Gov Firms	Non-Gov Firms
Dependent Variable:	1-Year Spreads	5-Year Spreads	10-Year Spreads	1-Year Spreads	5-Year Spreads	10-Year Spreads	1-Year Spreads	5-Year Spreads	10-Year Spreads
Post Election	0.022	0.072***	0.100***	0.045	0.084	0.087	0.036*	0.088***	0.109***
	(0.027)	(0.016)	(0.014)	(0.254)	(0.164)	(0.143)	(0.033)	(0.020)	(0.176)
Gov Contractor	-0.209	-0.185	-0.101						
	(0.193)	(0.116)	(0.099)						
Post × Close Election Dummy	-0.302***	-0.186***	-0.161***	-0.240	-0.227	-0.276*	-0.338***	-0.202***	-0.170***
	(0.036)	(0.022)	(0.019)	(0.308)	(0.208)	(0.165)	(0.040)	(0.024)	(0.021)
Post × Gov Contractor	0.165	0.133	0.0464						
	(0.204)	(0.122)	(0.105)						
Gov × Close Election Dummy	0.0695	0.139	0.117						
	(0.213)	(0.129)	(0.109)						
Post × Close Election Dummy × Gov	-0.193	-0.177	-0.129						
	(0.228)	(0.138)	(0.116)						
Intercept	-5.507***	-4.730***	-4.535***	-5.516***	-4.619***	-4.410***	-5.599***	-4.809***	-4.604***
	(0.010)	(0.006)	(0.005)	(0.154)	(0.102)	(0.0857)	(0.010)	(0.006)	(0.005)
Fixed effects	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Clustering	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle	Firm-cycle
Observations	395,968	438,640	401,202	9,452	10,327	9,597	304,938	333,515	308,605
R-squared	0.891	0.918	0.915	0.974	0.981	0.982	0.891	0.915	0.910

Table 14: Industry Robustness Tests

This table examines the robustness of the tests in Tables 4 - 8 to different types of industry controls. Specifically, the regressions below all include firm and industry-cycle fixed effects. As such, these tests look *within* an industry *within* a given election cycle for identification. Standard errors clustered by firm-election cycle are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10, 5, and 1 % levels respectively.

	(1) 5-year CDS Spreads	(2) 3-month Implied Volatility	(3) Investment	(4) Leverage	(5) M/B	(6) Sales	(7) Assets	(8) ROA
Post Election	-0.0761***	-0.0037**	0.000679	0.000689	-0.0706*	0.0672***	0.0868***	-0.0014***
	(0.0176)	(0.0018)	(0.000623)	(0.001687)	(0.0396)	(0.0058)	(0.0061)	(0.0004)
Close Win Dummy	0.000972	0.0030***	-0.001775*	0.003993	-0.1625**	0.0002	0.0456***	-0.0022***
	(0.0334)	(0.0009)	(0.000962)	(0.003593)	(0.0762)	(0.0160)	(0.0153)	(0.0006)
Policy Sensitive Dummy	-0.249***	-0.0555***	0.004516***	-0.003181	0.2481**	0.0553**	0.0354	0.0015*
	(0.0439)	(0.0064)	(0.001530)	(0.005225)	(0.1259)	(0.0218)	(0.0228)	(0.0009)
Post × *Close Win Dummy*	-0.0634***	-0.0060***	-0.000126	0.002555	0.0374	-0.0053	-0.0190**	0.0014***
	(0.0218)	(0.0007)	(0.000886)	(0.002551)	(0.0577)	(0.0091)	(0.0086)	(0.0005)
Post × *Policy Sensitive Dummy*	0.515***	0.1099***	-0.007280***	0.009918***	-0.4974***	-0.1299***	-0.0644***	-0.0039***
	(0.0414)	(0.0071)	(0.001258)	(0.003425)	(0.1017)	(0.0140)	(0.0109)	(0.0010)
Policy × *Close Win Dummy*	0.0393	0.0047**	-0.002455	0.005116	0.1212	-0.0183	-0.0356	0.001
	(0.0742)	(0.0022)	(0.002456)	(0.008340)	(0.2101)	(0.0330)	(0.0346)	(0.0014)
Post × *Policy* × *Close Win Dummy*	-0.267***	-0.0123***	0.005228**	-0.011221**	0.4318***	0.0941***	0.0338*	0.0025*
	(0.0772)	(0.0024)	(0.002232)	(0.005387)	(0.1573)	(0.0207)	(0.0190)	(0.0013)
Intercept	-4.706***	0.3939***	0.074579***	0.213552***	0.2173	6.8751***	8.0019***	0.0395***
	(0.0196)	(0.0459)	(0.014451)	(0.068447)	(1.1879)	(0.4740)	(0.3430)	(0.0122)
Fixed effects	Firm, FF-Cycle	Firm, FF-Cycle	Firm, FF-Cycle	Firm, FF-Cycle	Firm, FF-Cycle	Firm, FF-Cycle	Firm, FF-Cycle	Firm, FF-Cycle
Clustering	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle	Firm-Cycle
Observations	379,503	828,508	20,458	22,353	21,152	22,296	22,353	21,913
R-Squared	0.853	0.693	0.526	0.848	0.685	0.958	0.972	0.551