Do Bondholders Prefer Republican Governors?*

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Abstract

Past research has largely ignored the effects that political parties have on states' default risk. This paper addresses this question by analyzing the response of credit spreads to poll data from gubernatorial elections. The findings are that political affiliation has a significant effect on states' credit risk. Between 2009 and 2012, electing Republican governors reduced the default risk regardless of which party controlled the state legislature. I find larger political affiliation effects when gubernatorial elections were closely contested. An analysis of the candidates campaign promises suggests that stronger positions against tax increases were associated with lower default risks.

Keywords: State debt, opinion polls, gubernatorial elections, political affiliation, municipal bonds, credit risk.

Classification: H3, H7, G1.

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1. Introduction

Is political affiliation relevant for holders of state debt? In the run-up to every election, the popular press is often filled with reports on whether Republicans or Democrats are positive for state finances. Unfortunately, this popular interest has not been matched by significant academic research. This paper attempts to fill this gap by estimating the political affiliation effect on states' credit risk.

I proceed like Knight (2006). Knight (2006) measures the effect that political platforms have on stock prices by analyzing the responses of daily stock returns to daily changes in the probability of a Bush administration during the Bush/Gore 2000 election. Similarly, I analyze the responses of states' credit risk to changes in the probability of electing a Republican governor during 17 gubernatorial elections between 2009 and 2012. I obtain the time series of state authorities' credit risk (i.e., without including state municipalities) from Credit Default Swaps (CDS)[1] data. The regular fees paid in CDS contracts, known as CDS spreads, are theoretically equal to the state bonds credit spreads (Duffie 1999). I obtain the time series of the probability of electing a Republican governor from recurring poll data provided by 136 pollsters. In this sample, electing a Republican candidate decreases the state credit spread by six percent. Such a reduction is equivalent to 93 million dollars of savings every 5 years for the average state. This result is robust to two alternative ways of calculating the independent variable of interest (i.e., probability of electing a Republican governor), different dependent variable measures (i.e., CDS contracts of different terms), several functional forms, and several sets of control variables selected from existing models.

The methodology employed in this paper (and Knight (2006)) has never been used to measure the effect that political affiliation has on fiscal outcomes, but I argue that it is suitable for several reasons. First, high frequency financial data provide a high number of observations. Second, it reduces the possibility of obtaining biased estimators. Omitted variable bias (OVB) could be only caused by omitting high-frequency variables, which are

[1] A CDS is an insurance contract. In case of a default event the protection seller buys the defaulted bonds at par value from the protection buyer, who pays a quarterly fee known as CDS spread for this insurance.

typically market-determined and observable. Further, it is less likely to have variables that jointly determine CDS spreads and voters' preferences on a daily or weekly basis than it is with four-year election cycles and fiscal outcomes (in fact, a placebo analysis and other tests do not find evidence of a bias in the estimator). Third, the "no arbitrage" condition in financial markets implies that all observations are independent (otherwise, investors could earn infinite profits by predicting pricing errors from past errors). This means it is not necessary to cluster the data by election, which, from a statistical perspective, means that only the total sample size matters when making statistical inferences.

It also means that one election would suffice to estimate a "political affiliation" effect if the candidates were perfect representations of their parties, and states equal. However, individual candidates deviate from their parties, and some states could be more sensitive to political fluctuations. Thus, one election is not sufficient, but not many are required either. Because candidates self-select into parties that are aligned with their values, because parties select candidates aligned with their values, and because candidates may attract more party-aligned voters by being aligned with the party values, groups of 17 Republican candidates and 17 Democratic candidates should represent (as groups) the values of their respective parties. In addition, I show that the results are not driven by individual elections. Thus, the effect is a "political affiliation" one. I also support this assertion with two empirical analyses. First, I provide evidence that anti-tax-increment commitments have a positive effect on CDS spreads, and these type of commitments (or the lack thereof) are typically associated with political affiliations. Second, I show that, in fact, none of the candidates in the sample have deviated from the typical tax commitments of their parties; all and solely Republican candidates promised to oppose tax increases. To round up the analysis, I also show that once we use other candidate classifications, the effect disappears: it is not a legislative branch control effect, an incumbent candidate effect, or an incumbent party effect.

The results in this paper are important for both demonstrating that political affiliation matters in government debt markets and quantifying the magnitude of the effect. They are also relevant for understanding the effects that political parties have on fiscal outcomes, when fiscal conditions are worrisome and fiscal policies are relevant. This work is valuable from an interdisciplinary perspective by bringing knowledge of financial instruments to the study of issues of importance in Economics and other social sciences. It is also valuable from a methodological perspective because it highlights the advantages of using Knight's (2006) methodology to measure the effects of political affiliation on fiscal outcomes. Finally, this work uses a novel database of state CDS spreads and constructs a unique poll database of gubernatorial elections.

2. Literature Review

Credit risk is related to fiscal outcomes, and the effects of political affiliation on fiscal outcomes have been widely studied in the literature in Political Economy. On the spending side, Rogers and Rogers (2000), and Besley and Case (2003) find that a greater fraction of Democrats in the state House is associated with larger expenditures. Alt and Lowry (1994) assert that Democratic state governments are inclined to produce higher public spending as a share of per capita income. According to Reed (2006), Democratic control leads to a government size increase on the order of 3-5% compared to Republican control. Previous research finds more mixed effects. Garand (1988) finds inconclusive evidence that party control affects public spending. Besley and Case (1995b) find that Democratic governors increase public spending in their last terms. Gilligan and Matsusaka (1995) find small party effects on spending at the state and local levels. On the tax side, Besley and Case (1995a and 1995b) find that Democrats facing term limits increase taxes. Knight (2000) shows that taxes fall with Republican control of the state legislative branch, but that the opposite is true for Democratic control. Caplan (2001) finds that higher Democratic representation in either state legislative Houses is associated with tax increases. Alt and Lowry (1994) also contend that Democrats increase state taxes as a share of per capita income. Overall, budget planning is considered highly partisan (Krehbiel 2010), and it remains an important partisan subject (Clynch and Lauth 1990). Lowry, Alt, and Ferree (1998) show that voters use these differences to form expectations about future budget outcomes. Nevertheless, and to the best of my knowledge, the question of whether political parties are associated with fiscal policies with different risk profiles has not been addressed.

3. Empirical Strategy

I estimate a first-difference panel regression model in which the dependent variable is the weekly change in the five-year state CDS spread and the explanatory variable is the weekly change of the probability of electing a Republican governor.[2] The panel model is specified as follows:

$$\Delta S_{s,t} \;=\; \alpha_{s,y} \;+\; \beta_0 \Delta P(rep)_{s,t} \;+\; \Delta X'_{s,t}\theta \;+\; \gamma_t \;+\; \varepsilon_{s,t} \tag{1}$$

where "s" indicates the state, and "t" denotes time. The dependent variable $\Delta S_{s,t}$ denotes the weekly change in the five-year CDS spreads in state "s" at time "t". The independent variable $\Delta P(rep)_{s,t}$ denotes the weekly change in the probability of electing a Republican governor in state "s" at time "t". $\Delta X_{s,t}$ is the weekly change in a vector of control variables, $\alpha_{s,y}$ are state-year fixed effects, γ_t are time fixed effects, and $\varepsilon_{s,t}$ is an error term. Because the dependent variable is a traded financial asset, $\varepsilon_{s,t}$ are independent across states and time, but they might be heteroskedastic. The political affiliation effect on credit risk is given by β_0, and θ is a vector of parameters to be estimated.

3.1. *Control Variables*

Based on Ang and Longstaff (2013), Pan and Singleton (2008), Longstaff, Pan, Pedersen, and Singleton (2011), and related literature on the determinants of CDS spreads, I include a set of domestic and international economic variables that can be divided into four groups: economic performance variables (S&P500, DAX, and VIX), firms' CDS indices (North America Investment grade CDX, Europe Benchmark iTraxx, Asia Excluding Japan Investment Grade iTraxx, and Japan iTraxx), sovereign CDS indices (Emerging Market CDX), and individual

[2] It is standard in the literature to model first difference CDS spreads. CDS spreads and the probability of a Republican administration are non-stationary variables, since augmented DickeyFuller tests do not reject a unit root null hypothesis. First differences are stationary on the other hand(p=.99).

sovereign CDS (G-8 + BRIC). It is possible that not every listed variable is a relevant predictor of state CDS spreads. Therefore, in the empirical sections, I estimate three variations of Eq.(1) that differ in the number and type of control variables included.

The aforementioned market data are only available at the national level. Based on the existing literature, this fact should not pose a limitation on the predictive power of the CDS regression in Eq.(1) because CDS spreads are more responsive to global factors than local ones. Pan and Singleton (2008) indicate that the credit spreads for Mexico, Turkey, and Korea are mostly responsive to global factors such as the U.S. stock market and the stock volatility index (VIX). Longstaff, Pan, Pedersen, and Singleton (2011) reach similar conclusions using a sample of 26 nations. Moreover, they find that a single principal component explains more than 75% of the CDS spread variation during the financial crisis. The above cited and other related studies find that CDS spreads have an important commonality factor, that can be almost entirely explained by U.S. market variables. Theoretically, this relation between sovereign CDS spreads and U.S. market variables should be stronger for state CDS spreads. Ang and Longstaff (2013) find that state CDS spreads have a high level of commonality and are mostly dependent on U.S. federal CDS spreads.

State CDS are also determined by local economic variables, such as state annual reports, state debt, employment rate, unemployment rate, per capita income, tax receipts, budget deficits, growth, inflation, and news. The aforementioned variables are not updated on a weekly basis; thus, they are sources of the error term in Eq.(1). Therefore, they are not to be included in the CDS regression, and they are not a cause of OVB because they are not omitted. OVB can only be caused by omitted high-frequency variables, which are typically market-determined, and are thus included in Eq.(1). To complement the analysis, section 7.1 presents three tests that do not find evidence of OVB. Finally, I also include yearly state fixed effects, and time dummy-variables to capture unobservable national trends.

3.2. *Orthogonality Conditions*

It is possible that the low-frequency variables that are sources of the error term in Eq.(1) also shape voters' preferences. However, even then, it would not mean that the explanatory variable of interest is correlated with the error term. I show that the estimator in Eq.(1) is unbiased as long as the low-frequency variables that increase CDS spreads do not systematically increase or decrease the Republican prospects.

Let us decompose the error term $\varepsilon_{s,t}$ in Eq.(1) into the effects caused by these low-frequency variables. Let $J = \{1, ..., n\}$ index all low frequency variables, and let $j_{s,t} \in J$ denote the variable j that is updated at week "t" and state "s", where $j_{s,t}$ is random and scalar. Let $\lambda^j_{s,t}$ denote the random effect that $j \in J$ would have on $\Delta S_{s,t}$ at state "s" and date "t" in case $j_{s,t} = j$. Then, $\varepsilon_{s,t} = \sum_1^n \lambda^j_{s,t} \mathbb{1}(j = j_{s,t})$, where $\mathbb{1}(. = j_{s,t})$ takes a unit value if $j = j_{s,t}$. Let us now decompose $\Delta P(rep)_{s,t}$ in Eq.(1) into the effects caused by these low-frequency variables. Let $\delta^j_{s,t}$ denote the random effect that $j \in J$ would have on $\Delta P(rep)_{s,t}$ at state "s" and date "t" in case $j_{s,t} = j$. Then, $\Delta P(rep)_{s,t} = \sum_1^n \delta^j_{s,t} \mathbb{1}(j = j_{s,t})$. The estimator in Eq.(1) is unbiased if

$$
\begin{aligned}
E[\Delta P(rep)_{s,t} * \varepsilon_{s,t}] = 0 \quad \Leftrightarrow \quad & E[\sum_1^n \delta^j_{s,t} \mathbb{1}(j = j_{s,t}) * \sum_1^n \lambda^j_{s,t} \mathbb{1}(j = j_{s,t})] = \\
& \sum_1^n cov(\lambda^j_t, \delta^j_t) * prob(j = j_{s,t}) = \\
& E\big[cov(\lambda^j_t, \delta^j_t)\big] = 0
\end{aligned}
$$

This condition admits positive and negative correlations between the effects that low-frequency variables have on CDS and voters' preferences ($E\big[cov(\lambda^j_t, \delta^j_t)\big] = 0 \nRightarrow cov(\lambda^j_t, \delta^j_t) = 0$). There is no bias as long as it is not expected that whatever increases CDS, systematically favors (or disfavors) the candidates from one particular party. Theoretically, this is not a very strong assumption. What is negative for CDS spreads is not systematically negative for the sponsoring candidate; poor economic policies often prove to be very popular (e.g., building a wall or "Brexit"). Thus, they can correlate in very random ways. Logic suggest that we should expect "favorable" economic news to be positive for both CDS spreads and the incumbent party or candidate. Incumbency is roughly even across parties, so no party shall be systematically favored. Thus, the condition should hold. In any case, because incumbency is slightly tilted for the Democratic Party (approximately 60%), positive economic news should tend to hurt Republican candidates' prospects while decreasing the CDS spreads, which would yield the opposite effect to the one reported here. Worst case scenario, we can at least claim that the results in the paper are not driven by low-frequency variables. Empirically, Section 7.1 provides three tests that do not show evidence of bias caused by low-frequency variables. They either do not exist or do not show a tendency to favor or disfavor candidates with a certain political affiliation.

4. Data

I obtained CDS data from CMA Datavision. It offers substantial CDS data for twenty states between 2009 and 2012. CDS spreads present significant variance during the months prior to a gubernatorial election. The left panel in Table 1 provides summary statistics of the state CDS spreads during the six months prior to Election Day for states for which CDS data and poll data are available. The gap between the maximum and minimum spread during election races averages 71 basis points, equivalent to twice the average spread of the Federal Government during the worst financial crisis in 75 years. The median state standard deviation is thirteen basis points. Average CDS spreads range between 62 and 282 basis points, that is, two to seven times larger than the average CDS spread for the Federal Government. The dependent variable in Eq.(1) is defined as the Friday-to-Friday change in CDS spreads.

4.0.1. *CDS Spread Data v. Bond Yield Data*

As an alternative to CDS spreads, we may also obtain credit risk measures from bond yield data. In practice, CDS data dominate bond yield data for the following reason. First, state authorities have hundreds of bonds outstanding with many different sources of security. The credit risk of the bonds issued by a same state varies with the security sources and other

Table 1: State CDS Spreads & Poll Data

Summary statistics of 5-yr CDS spreads (left) and the Republican fraction of the combined vote intention for Republicans and Democrats (right) during the six months prior Election Day.

	CDS Spreads			Rep/(Rep +Dem)		
	Mean	S.D.	Min-Max	Mean	S.D.	Max-Min
New Jersey '09	176	52	160	54	2.5	9
California '10	282	23	103	49	2.6	9
Connecticut '10	114	13	51	45	2.7	9
Florida '10	138	23	91	50	2.7	11
Illinois '10	281	38	161	54	2.1	7
Maryland '10	62	9	37	47	2.6	8
Massachusetts '10	126	13	57	46	2.7	9
Michigan '10	230	23	97	61	2.7	10
Nevada '10	193	13	54	59	2.3	8
New York '10	222	26	99	33	5.9	17
Ohio '10	129	11	47	53	2.2	9
Pennsylvania '10	126	7	40	56	1.7	6
Texas '10	80	11	44	55	1.9	7
Wisconsin '10	118	11	53	54	1.0	4
North Carolina '12	79	10	45	56	2.1	8
Washington '12	80	11	37	49	2.1	7
Wisconsin '12	101	14	43	53	1.6	4
Median	126	13	71	51	2.5	9

features of the bonds. Second, state bonds also trade very infrequently[3], and bond yields thus carry high liquidity premiums. Further, the liquidity premium and "other" premiums also vary as a function of the features and type of the bonds issued by a same state. The problem with bond yield data is that because municipal bonds trade very infrequently, the average credit spread, liquidity premium, and other premiums may exhibit large swings that are unrelated to fundamentals due to changes in the features and types of bonds traded in two consecutive weeks. This introduces high measurement errors. For example, the spread between muni yields and after-tax treasury yields reportedly increases with longer maturities. If we observed short-term bond trades one week and long-term bond trades the following week, the observed yield spread would increase, and this change would be unrelated to fundamentals.

Instead, CDS spreads reflect the credit risk of the state authority itself; CDS spreads

[3]As reported by Green, Hollifield, Schürhoff (2007a), and Cestau, Green, and Schürhoff(2013).

measure the likelihood of the union of all possible default events, and thus give just one credit risk measure per state. Second, by definition, CDS contracts are only swapping the credit risk of the underlying bonds, not their liquidity premiums or other premiums. Therefore, they reflect only the state credit risk.[4] Finally and most importantly, CDS measures of credit risk are not subject to the measurement errors caused by changes in the mix of features of traded bonds because CDS contracts are standard and homogeneous.

4.1. *Poll data & Final Sample*

I collected poll data for every election held between 2009 and 2012 (for states with CDS data) from four different sources: FiveThirtyEight, Real Clear Politics, Ballotpedia, and Wikipedia. Four states (with CDS data) did not have significant poll data. I used a variety of methods to match and contrast poll reports across the data sources. In many instances, it required case-by-case corrections, such as with pollsters' names, which were required to avoid double-counting the same reports recorded with different names. After manually correcting the pollster's name for every poll report in every state, the total number of pollsters went from 305 to 136.[5] The right panel of Table 1 presents summary statistics of the Republican percentage of the combined vote intention for Democratic and Republican candidates during the six months prior to Election Day. I report Saturday to Friday averages to avoid outliers. Vote intentions show high variation during election campaigns. The mean Republican percentages pivot around 50%, which indicates that there was not a pronounced preference for Republican or Democratic candidates. The average difference between the maximum and minimum Republican vote intention is nine percent points. This is a significant change if we consider that going from 45.5% to 54.5% translates into losing the election with almost certainty to winning the election with almost certainty. Thus, it is not unusual for election contests to have large swings on the foreseeable winner.

The final sample contains CDS observations for twenty states. Collected poll data are restricted to six months prior to Election Day due to limited data availability beyond this

[4] Longstaff et al. (2005), Pan and Singleton (2008), Longstaff et al. (2011), and Ang and Longstaff (2013)

[5] Poll reports by the Univ. of Southern California were recorded with three different names: USC, USC/LA times, and LA Times. After correcting California's poll data the number of pollster went from 54 to 15.

point.[6] I dropped all observations without poll data during the six months preceding election contests. I also dropped all observations between twelve and six months preceding election contests (although poll data are not available, changes in candidates' prospects may still determine CDS spreads). I assumed that candidates' prospects do not change beyond twelve months preceding election contests, and thus, I did not drop these observations. Treatment is defined as a change in the average weekly vote intention during a gubernatorial election. Periods without changes in candidates' prospects serve as control observations. The final sample contains 924 observations altogether and 170 treatments. It is not necessary to cluster the data by election for the following reasons (i.e., 170 is the effective number of treatments for making statistical inferences). First, CDS are traded financial assets; thus, all CDS observations are independent by a "no-arbitrage" condition. Second, by model design, the effect (β_0) does not vary by state. A additional concern is whether I am estimating a "political affiliation" effect or a "circumstantial" effect, which I discuss in the last section.

5. From Vote Intentions to Probabilities

Let p_R denote the population percentage of Republican voters and p_D the population percentage of Democrat voters. The Republican candidate wins the election if $p_R > p_D$. Poll data provide sample percentages $\hat{p}_R$ and $\hat{p}_D$ that are normally distributed random variables of the true population parameters, with means p_R and p_D respectively. The difference $\hat{p}_R - \hat{p}_D$ is also normally distributed, with mean $p_R - p_D$ and standard deviation $\sigma_{p_R - p_D}$. That is $(\hat{p}_R - \hat{p}_D) \sim N(p_R - p_D, \sigma_{p_R - p_D})$. Given $\hat{p}_R$, $\hat{p}_D$, and $\sigma_{p_R - p_D}$, the probability of electing a Republican candidate is given by $Prob(p_R > p_D) = Prob\left(-(p_R - p_D) < 0\right) = Z\left(\frac{(\hat{p_R} - \hat{p_D})}{\sigma_{p_R - p_D}}\right)$.

The standard deviation $\sigma_{p_R - p_D}$ is determined by the sample size and the correlation between p_R and p_D. In elections with only two possible options the population percentages are perfectly negatively correlated. Given that all gubernatorial elections in the sample are approximately two-option elections, I assume that p_R and p_D are perfectly negatively correlated. Later in the paper I relax this assumption. The estimated $\sigma_{p_R - p_D}$ is given by

[6] Knight(2006) takes same criterion.

$\hat{\sigma}_{p_R - p_D} = \sqrt{\frac{p_R + p_D - (p_R - p_D)^2}{N}}$, where N denotes sample size. I convert vote intentions into probabilities and then calculate the Saturday-to-Friday average probability.[7]

6. Empirical Results

This section shows the estimation results of the CDS model in Eq.(1). Table 2 shows estimates for three different specifications of Eq.(1): one without control variables, one with domestic and CDX variables, and one with all of the control variables described in section three. Every specification includes state-year fixed effects and time fixed effects. Heteroscedastic-consistent standard errors are listed in parentheses. The first row in Table 2 shows the effect on CDS spreads of a unit change in the probability of electing a Republican. When a Democrat is elected, that probability is zero, and it is one if a Republican is elected. Thus, it is straightforward to interpret the results: a Republican governor versus a Democratic one (i.e., a discrete choice) reduces the CDS spread by an average of 6.8 basis points (approximately a six percent of the mean CDS spread and more than half of the average standard deviation in the sample). In monetary terms, such a reduction is equivalent to more than 93 million dollars in savings every 5 years to the average state in the sample.[8] Every coefficient is statistically significant at the 1% significance level, and the magnitudes do not decrease as I introduce more control variables. Moreover, they are more precisely estimated.

The results shown in Table 2 stem from an ex-ante analysis in nature. The question of whether political affiliation affects credit risk ex-post is certainly interesting and has not been addressed in the literature. Nevertheless, an ex-post analysis would face two drawbacks. First, we do not observe high-frequency treatments ex-post but rather once every four years. Thus, such analysis would need to include all low-frequency variables correlated with fiscal and election outcomes (that we do not need to include now) as regressors, because they would be updated with the same (or more) frequency as the dependent variable. However, not all of them are known or observed by the econometrician. Second, it would require long

[7] It is a probability of electing a Republican governor if the election took place in such week.

[8] I obtained this figure by multiplying total bonded debt from "Comprehensive Annual Financial Reports" by the average effect. I do not include short term debt nor unfunded Pension Funds.

Table 2: Baseline Model

$\Delta S_{s,t} = \alpha_s + \beta_0 \Delta P(Republican)_{s,t} + \Delta X'_{s,t}\theta_t + \gamma_t + \epsilon_{s,t}$, where Δ denotes first difference, $S_{s,t}$ are 5-yr state CDS spreads, and "Prob. Republican" is the probability of electing a Republican governor. $X_{s,t}$ include: Domestic & CDX (ln(S&P500), ln(VIX), 5-yr CDX.EM, CDX.IG, 5-yr US CDS) and international (5-yr CDS for G-8 and BRIC countries, Itraxx Europe, Itraxx Asia, Itraxx Japan, and ln(DAX)). α_s and γ_t are state and time fixed effects respectively. Heteroscedasticity-consistent standard errors between parentheses. (*10%, **5%, ***1%)

ΔProb. Republican	-5.71***	-6.77***	-8.03***
	(1.70)	(1.61)	(1.25)
Controls:			
Domestic & CDX		Yes	Yes
International			Yes
State Fixed Effects	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes
N	924	924	924
R-sq	0.16	0.33	0.39

observation periods, but state CDS data are not available beyond 2008; thus, bond yield data must be used instead, with all of the measurement errors that doing so would entail. The methodology employed in this paper purposefully avoids these drawbacks.

7. Robustness Checks

7.1. *Orthogonality Tests*

7.1.1. *Effect by Election Contestability.*

It is reasonable to expect smaller responses of CDS spreads to changes in voting intentions when the differences between candidates are so large that investors do not perceive changes in voting intentions as meaningful changes in the probability of electing a Republican governor. However, if the reported political effect were spurious, the effect on CDS would not depend on whether the change in voting intentions is a meaningful change in the probability because they would be unrelated. On the contrary, if we observed non-linear effects, it would be very strong evidence that the reported effect in Table 2 is not spurious.[9] In this section, I

[9]The nonlinearity is implicitly captured by a linear model in probabilities.

test this non-linearity by estimating linear models in voting intentions for the sub-sample containing the eight election contests with average Republican percentages closest to 50% and the sub-sample containing the eight election contests with average Republican percentages most distant to 50%. The first row of Table 3 shows political effects for closely contested elections that are twice as large as the estimates for relatively uncontested elections. The difference between sub-samples, as measured by an identifier of the second sub-sample in columns three and four, is positive and roughly significant at 10%, which is probably due to the small sample sizes. Overall, Table 3 shows that the CDS spreads are truly responding to candidates' prospects, and that these responses are not spurious.

Table 3: Non-linear CDS response

The first two columns show estimates of a linear model in voting intentions for two subsamples: the "top 8 states" include the eight election most closely contested, and "bottom 8 states" the eight least closely contested. The "$\mathbb{1}$(bottom 8)" variable in columns 3 and 4, takes a unit value for the "bottom 8 states". The dependent is the 5-yr CDS spread. "ΔRepublican %" is the change in the Republican slice of total Republican and Democratic voting intentions. Control variables are listed in Table 2. Heteroscedasticity-consistent standard errors between parentheses.

	Levels			Difference	
ΔRepublican %	-0.703**	-0.903***	Republican %	-0.735**	-0.904***
(top 8 states)	(0.313)	(0.262)		(0.312)	(0.251)
ΔRepublican %	-0.360**	-0.371*	$\mathbb{1}$(bottom 8)*	0.371	0.509*
(bottom 8 states)	(0.161)	(0.189)	ΔRepublican %	(0.342)	(0.301)
Controls:					
Domestic & CDX	Yes	Yes		Yes	Yes
International		Yes			Yes
State Fixed Effects	Yes	Yes		Yes	Yes
Month Fixed Effects	Yes	Yes		Yes	Yes
N	821/834	821/834		902	902
R-sq	0.32/ 0.34	0.39/0.41		0.33	0.39

7.1.2. *Placebo Regressions*

Suppose that there were some variables such that if they are positive news for bondholders (i.e. decrease CDS spreads), they are also positive news for Republican candidates, thus causing the negative effect documented in Table 2. If they were also correlated in the cross-section of states, whenever they were positive news for bondholders and the Republican candidate in one state, they would tend to be good news for bondholders and Republican candidates in other states. Therefore, Republican prospects in one state should be negatively correlated with CDS spreads in other states. Failing to observe such a correlation is evidence that the results are not biased by omitted or low-frequency variables correlated in the cross-section of states. I construct a placebo test using the sub-sample of 2010 elections, where I randomly interchange the entire time series of Republican probabilities across states, and replicate Table 2. I repeat this process fifty times (a total of 150 estimations). Under the null hypothesis of no confounding variables correlated in the cross-section of states, we should reject $\beta = 0$[10] by statistical chance in 10%, 5%, and 1% of the times, if we use 90%, 95%, and 99% confidence intervals, respectively. If the rejection rates were higher than 10%, 5%, and 1%, respectively, it would be evidence of bias. Table 4 compares sample rejection rates with theoretical ones. The first column reports the confidence level used to test the null hypothesis. The second column reports theoretical rejection rates under the null hypothesis and the confidence interval in column one, and the 95% confidence interval for sample rejection rates (sample rejection rates are random variables themselves). The third column shows the sample rejection rates of the placebo test using the confidence level of column 1. The sample rejection rates are almost equal to the theoretical ones and are always within the confidence intervals in column two. Table 4 shows that the baseline estimates are not biased by omitted and low-frequency variables that are correlated in the cross-section of states. For comparative purposes, panel D of Table 6 replicates the baseline model using the sub-sample of 2010 elections without interchanging poll data across states. As in Table 2, it shows negative and statistically significant effects at the 1% level, where magnitudes are also similar to those in Table 2.

7.1.3. *Lead and Lag*

Suppose that there were some variables with serial correlation, such that if they are positive news for bondholders, they are also positive news for Republican candidates (thus causing the negative effect documented in Table 2). A lead (lag) of the Republican candidate probability

[10] I assume that weekly state CDS spreads do not respond to weekly poll reports of other states.

Table 4: Placebo Tests & 2010 Elections

The table shows rejection rates of political effects in 2010 when the whole time series of poll data are randomly interchanged across states. Column one reports the confidence level used to test the null hypothesis of no effect. Column two reports theoretical rejection rates and a confidence interval of sample rejection rates under the null. The third column shows sample rejection rates for 150 simulations.

Confidence Interval	Theoretical Rejection Rates	Sample Rejection
99%	1%[+/-1.6]	1%
95%	5% [+/-3.5]	5%
90%	10% [+/-4.8]	12%

would likely be correlated with such variables because such variables would be correlated with themselves one period ahead (behind), which would, in turn, be correlated with the Republican probability in that forward (backward) period. Therefore, including a lead (lag) of the probability as a regressor should capture part of the spurious effect and curtail part of the documented effect in Table 2. Failing to observe such effects indicates that the results are not biased by omitted or low-frequency variables having serial correlation. I include a lead and a lag of the probability of electing a Republican governor as control variables in Eq.(1) and replicate Table 2. Table 5 shows the estimate results for three different specifications of Eq.(1) that include a lead and a lag of the probability of electing a Republican governor. The first row in Table 5 shows political affiliation effects that are negative and statistically significant at the 1% level. More importantly, the magnitudes are not smaller than those in Table 2. The second and third rows show coefficient estimates for the lead and lag probability variables that are not statistically significant at the 10% level for none of the three specifications. Overall, Table 5 shows that the results are not biased by omitted or low-frequency variables having serial correlation.

7.2. *Model Variants*

Panel A in Table 6 shows estimates of Eq.(1) assuming zero correlation between the population parameters p_R and p_D. Panel B shows the estimation results when I weigh poll data by pollster experience in the state. The results are not different from the baseline model in

Table 5: Lead & Lag

The dependent variable is the 5-year CDS spread. "ΔProb. Republican" is the change in the probability of electing a Republican governor. Lead ΔProb. and Lag ΔProb. are leads and lags of "ΔProb. Republican". Control variables are listed in Table 2. Heteroscedasticity-consistent standard errors between parentheses.

ΔProb. Republican	-10.35***	-9.650***	-12.13***
	(3.02)	(3.13)	(4.20)
Lag ΔProb. Republican	-0.58	-0.92	-1.39
	(6.03)	(5.69)	(6.26)
Lead ΔProb. Republican	-2.59	-2.43	-6.19
	(8.79)	(10.74)	(10.87)
Controls:			
Domestic & CDX		Yes	Yes
International			Yes
State Fixed Effects	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes
N	856	856	856
R-sq	0.15	0.31	0.38

Table 2. Both panels show negative and statistically significant coefficients at the 1% level and of the same order of magnitude. Thus, the results in Table 2 are robust to different independent variable variants. Panel C replicates Table 2 using 10-year CDS spreads. Although a 10-year horizon exceeds a governor term, any credit event during her term is included in 10-year CDS. The estimates in Panel C are negative and statistically significant at the 5% level if no control variables are included or statistically significant at the 1% level otherwise. The results in Panel C indicate that the Table 2 estimates are robust to dependent variable variants. Panel E presents the results from a linear model in voting intentions instead of in probabilities. The estimates show the response of CDS spreads to a one-percentage-point change in the Republican slice of total Republican and Democratic voting intentions. The effects are negative and statistically significant at the 1% level. Finally, Panel F shows that CDS spreads decrease on the Election Date (i.e. ignoring poll data) when Republicans win the election. The significance decreases to 10%, which was to be expected given that on Election Day some outcomes are foreseeable (from poll reports) and are incorporated into CDS spreads in advance. The results in panels E and F suggest that Table 2 estimates are robust to functional form variants.

Table 6: Model Variants

The dependent variable is the 5-yr CDS spread. "ΔProb. Republican" is the change in the probability of electing a Republican governor. Control variables are listed in Table 2. In panel A I assume zero correlation between candidates' voting intention. In panel B I weight poll data by pollster experience. In panel C the dependent variable is the 10-yr CDS spread. Panel D replicates Table 2 using the sub-sample of 2010 elections. In panel E "Republican" is the change in the Republican slice of total Republican and Democratic voting intentions. In panel F "Republican" takes a unit value if the Republican candidate is elected, and a negative unit otherwise. Heteroscedasticity-consistent standard errors between parentheses.

	Panel A: Zero			Panel B: Weight		
ΔProb. Republican	-4.16***	-5.32***	-6.58***	-5.12***	-6.36***	-7.86***
	(1.34)	(1.30)	(1.13)	(1.85)	(2.28)	(1.72)
	Panel C: 10-yr			Panel D: 2010		
ΔProb. Republican	-4.03**	-4.55***	-5.51***	-6.84***	-6.33***	-4.91***
	(1.73)	(1.47)	(1.27)	(1.79)	(1.23)	(1.09)
	Panel E: Polls			Panel F: Elections		
Republican	-0.509***	-0.493***	-0.534***	-3.70*	-3.30*	-3.16*
	(0.131)	(0.135)	(0.147)	(2.00)	(1.68)	(1.75)
Controls:						
Domestic & CDX		Yes	Yes		Yes	Yes
International			Yes			Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

7.3. *Individual Gubernatorial Elections*

In this section, I show that results are not driven by individual elections. In each case I drop a different gubernatorial election from the sample and replicate Table 2. In every case, the political affiliation effects are of similar magnitudes to those in Table 2 and are statistically significant at the 1% level, except in the case where Florida and no control variables are included, where the statistical significance drops to 5%. Based on this evidence, Table 2 results are driven neither by candidates deviating from the party average nor by elections that are more sensitive to political fluctuations. [11]

[11] Compared to studies using OECD data (similar number of sovereign states as here) that also estimate constant effects across states, U.S. states are more homogenous, and the constitution and statutes that regulate the issuance and security of state bonds do not vary much from state to state.

8. *Political Affiliation Effect*

The previous sections show beyond a reasonable doubt that there is a set of characteristics that the Republican candidates in the sample share that are positive for credit risk. In the following sections, I show that these characteristics are not circumstantial and that they are typically associated with political affiliations.

8.1. *Incumbent Effect*

Political economists have found significant incumbent effects in many other applications, so it is a natural classification of candidates to test. Table 7 shows the response of CDS spreads to changes in the probability of a victory of the incumbent party and the incumbent candidate. The responses are not statistically significant in either case, which indicates that the baseline estimates are not an incumbent effect masked as a political affiliation effect.

Table 7: Incumbent Party and Candidate Effects

This table shows the response of CDS spreads to changes in the probability ("ΔProb. Incumbent") of victory of the incumbent party (left panel) and the incumbent candidate (right panel). Control variables are listed in Table 2. Heteroscedasticity-consistent standard errors between parentheses.

	Left: Incumbent Party			Right: Incumbent Candidate		
ΔProb. Incumbent	-3.42	-2.78	-3.00	1.92	4.51	7.73
	(2.40)	(2.70)	(3.26)	(5.31)	(4.81)	(4.81)
Controls:						
Domestic & CDX		Yes	Yes		Yes	Yes
International			Yes			Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	924	924	924	806	806	806
R-sq	0.16	0.32	0.39	0.15	0.33	0.41

8.2. *Legislative Branch Control*

Many economists claim that state fiscal policies are primarily determined by the legislative branch. However, in the United States, all governors can oversee the legislative branch because they all possess the faculty to veto legislative decisions and almost all of them have the power to line-veto budget bills. Both directly and indirectly, governors have power to affect fiscal policies, and candidates actively undertake political campaigns that hinge on fiscal policies. The effect of oversight is undoubtedly a political affiliation effect because Republican governors will tend to be aligned with Republican-controlled legislatures and stand in opposition to Democratic-controlled ones, and vice versa. Will the oversight effect depend on which party controls the legislative branch, and is what we observe in the data consistent with it? The answers are "no" and "yes", respectively. If bondholders prefer Republican-controlled legislatures, they will welcome Republican governors when Republicans control the state legislature because they would be aligned with their agenda and opposition from a Democratic governor would be avoided. Republican governors will also be welcome when Democrats control the state legislature because they would stand in opposition to their agenda. Conversely, Republican governors will not be welcome in either case if bondholders prefer Democratic-controlled legislatures. Therefore, the sign (not the magnitude) of the oversight effect should not change with legislative branch control. It depends only on bondholders' preferences.

I estimate the political affiliation effect in the sub-sample containing the nine elections in which the Democratic Party controls the state legislature and in the sub-sample containing the eight elections with Republican-controlled legislatures after election.[12] Columns one and two of Table 8 show negative and statistically significant coefficients at the 1% level for both groups. The estimates are larger for Democratic-controlled legislatures, but the difference between sub-samples, as measured by an identifier of the first sub-sample in columns three and four, is not statistically significant. The Table 8 results do not prove that the oversight effect (i.e., a genuine political affiliation effect) is significant, but they are consistent with it.

8.3. Tax Policy

To gain insight into the set of characteristics shared by the Republican candidates in my sample and determine whether they are circumstantial or a true political affiliation effect,

[12] New York had a divided legislature after 2010, but the Democratic Party gained control one year later.

Table 8: Divided Government

The first two columns show Eq.(1) estimates for two sub-samples: "Democratic Control", which include the eight elections with Democratic-controlled legislatures following election, and "Republican Control", which are the eight elections with Republican-controlled legislatures following election. "$\mathbb{1}$(Democratic Control)" takes a unit value for "Democratic Control". The dependent variable is the 5-yr CDS spread. "ΔProb. Republican" is the change in the probability of electing a Republican governor. Control variables are listed in Table 2. Heteroscedasticity-consistent standard errors between parentheses.

	Levels			Difference	
ΔProbc. Republican	-8.954***	-8.347***	ΔProb. Republican	-5.027***	-7.503***
(Democratic Control)	(3.20)	(2.71)		(0.85)	(1.46)
ΔProb. Republican	-4.750***	-7.474***	$\mathbb{1}$(Democratic Control)*	-4.091	-1.251
(Republican Control)	(0.89)	(1.58)	ΔProb. Republican	(3.19)	(3.03)
Controls:					
Domestic & CDX	Yes	Yes		Yes	Yes
International		Yes			Yes
State Fixed Effects	Yes	Yes		Yes	Yes
Month Fixed Effects	Yes	Yes		Yes	Yes
N	849/828	849/828		924	924
R-sq	0.33/0.33	0.40/0.39		0.33	0.39

I analyzed their campaign promises, political records, endorsements, "rankings", and individual characteristics, as provided by different sources and advocacy groups (e.g., "Project Vote Smart"). First, I found that absolutely every Republican candidate in my sample promised to oppose tax increases, whereas no Democratic candidate made such commitment. Although "Blue Dog Democrats" and "Progressive Republicans" exist, these sorts of promises are typically associated with political affiliation, and the candidates do not deviate from their party policies. Second, I find evidence that these different tax commitments between the parties explain some of the Table 2 estimates. I estimate the political affiliation effect in the sub-sample containing the elections where the Republican candidate signed the "Taxpayer Protection Pledge" and in the sub-sample containing the elections in which the Republican candidate did not sign the pledge. The "Taxpayer Protection Pledge" is a pledge to oppose increases in marginal income tax rates for individuals and businesses, sponsored by "Americans for Tax Reform"; the underlying assumption is that signers show a stronger commitment. The first row of Table 9 shows that the political effects in the sub-sample of signers are negative and statistically significant at the 1% level and are almost twice as large as the estimates in the non-signers sample, which are roughly significant at the 10% level. The difference between samples, as measured by an identifier of the signers sample in columns three and four, is negative but not statistically significant, probably due to sample sizes.

Table 9: Candidates Profile

"ΔProb. Republican" is the change in the probability of electing a Republican governor. "Pledge Signers" are elections where the Republican candidate signed the "Taxpayer Protection Pledge", and "Non Pledge Signers" the remaining elections. $\mathbb{1}$(signers) takes a unit value for "Pledge Signers" elections. The dependent variable consists of 5-year State CDS spreads. Control variables are listed in Table 2. Heteroscedasticity-consistent standard errors between parentheses.

	Levels			Difference	
ΔProb. Republican	-6.615***	-7.893***	ΔProb. Republican	-4.485	-4.978*
(Pledge Signers)	(1.69)	(1.23)		(3.76)	(2.69)
ΔProb. Republican	-4.120	-4.391	$\mathbb{1}$(signers)*	-2.701	-3.621
(Non Pledge Signers)	(3.99)	(3.20)	ΔProb. Republican	(4.11)	(2.91)
Controls:					
Domestic & CDX	Yes	Yes		Yes	Yes
International		Yes			Yes
State Fixed Effects	Yes	Yes		Yes	Yes
Month Fixed Effects	Yes	Yes		Yes	Yes
N	877/827	877/827		924	924
R-sq	0.32/0.33	0.40/0.41		0.33	0.39

Although the observed tax promises are typical party promises and might explain the Table 2 results, we could not claim that they are the only drivers or that all other drivers are party effects. Although the candidates in this sample behave as the norm and future candidates will likely continue to do so, economic contexts will vary with business cycles. We cannot claim that these tax promises would be relevant in booming economies or explain why they were relevant in worrisome fiscal conditions. It is also not obvious why such promises are believable because it is not clear that Republicans do not increase taxes ex-post or why opposing taxes could have a positive effect (multiple explanations are possible. For example, although the pledge is a tax commitment, tax commitments are effectively size commitments, and issuer size is a well-documented risk factor).

9. Conclusion

This paper provides an empirical answer to the question of whether political affiliation affects credit risk. Electing Republican candidates to governor's office has a positive effect on states' credit risk. Robustness tests do not show evidence of a correlation between the independent variable of interest and the error term in the estimation equation. The results are also robust to two alternative ways of calculating the independent variable, different dependent variables, several functional forms, and several sets of control variables. The results are not driven by any particular election and do not mask an incumbent effect. They are also consistent with a governor's oversight effect, which is undoubtedly a political affiliation effect. Candidates' tax policies are typical party policies and probably one driver of the effect (thus, a political affiliation effect), but more mechanisms might also explain the effect. Future research should aim to find these mechanisms for different economic contexts. This paper is a step in that direction.

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