Electoral Entrenchment and Municipal Bond Prices

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Abstract

Political parties often initiate policies designed to entrench themselves and limit the power of the opposition. I show that municipal bond investors value electoral entrenchment. An increase in electoral entrenchment lowers municipal bond spreads, regardless of which party benefits. I explore potential sources for this phenomenon. The evidence indicates that rising agency frictions between constituents and their representatives drive the effect, particularly in states with substantial pension underfunding. My results suggest that a reduction in politicians' responsiveness to the will of the voters, caused by electoral entrenchment, allows politicians to make unpopular public finance decisions that bondholders value.

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1 Introduction

Controlling political parties may enact policies to entrench themselves and thus disadvantage the opposition party in an election. Electoral entrenchment can take many forms depending on the party in power and their supportive constituents. Examples include limiting polling locations, voter ID laws, campaign finance regulations, limiting the political activity of unions or religious organizations, changing voter registration rules, and gerrymandering. Despite the varied methods, the goal of electoral entrenchment is the same: to insulate the sitting government from the changing will of their constituents. In a principal-agent framework, the voter is the principal, and the politician is the agent, and electoral entrenchment increases the agency frictions. How do state debt holders respond to an increase in this agency friction? I show that tax-adjusted yield spreads decrease with greater levels of electoral entrenchment. Furthermore, I empirically demonstrate a potential mechanism: increased agency friction allows elected officials to make politically unpopular public finance decisions that can benefit bondholders.

I measure electoral entrenchment in two ways: the cost voting index (COVI) from Li et al. (2018) and the level of partisan gerrymandering. Li et al. (2018) construct COVI using principal component analysis on state election laws and performance after every presidential election since 1996, with higher values corresponding to greater voting costs. Partisan gerrymandering is the practice of constructing voting districts to ensure that a political party has an advantage in maintaining control of the government. Redistricting to favor a political party has a long history in the U.S., but the issue has greater salience in the 21st century as the prevalence and magnitude have increased (McGhee (2020)). By gerrymandering state legislative districts, a party can nearly guarantee themselves future control of the legislature. For example, in the 2018 Wisconsin election, Democrats won all state-wide elections and 53% of the vote for the state assembly but got only 36% of the seats.[1] I utilize the most commonly used measure of gerrymandering, partisan bias, from Gelman and King (1994b).

I find that a one-standard-deviation increase in the COVI results in a 6.0 basis point decrease in tax-adjusted state bond spreads. Similarly, a one-standard-deviation increase in state legislative district partisan bias is reflected in a 6.7 basis point decrease in spreads. I show a similar effect, regardless of which party benefits from the partisan bias. A 7.4 (7.0) basis point decrease in spreads is observed with

[1] Gilbert (2018)

a one-standard-deviation increase in Democratic (Republican) partisan bias. The coefficients have no statistically significant difference depending on who the bias favors.

In addition to my main results, I explore potential mechanisms. I show that the reductions in spreads due to gerrymandering are greatest when a state has a poorly funded public pension system. I interact gerrymandering with the pension debt ratio and show that a state with a pension debt ratio one standard deviation above the average and low levels of gerrymandering will see an increase in spreads of 23 basis points. In contrast, a state with the same pension underfunding level but high gerrymandering levels will only see a decrease in spreads of 3 basis points.

I also document that greater gerrymandering correlates with better public pension system health and the likelihood of undertaking reforms to improve funding. To support the idea that gerrymandering allows state governments to execute unfavorable financing policies, I show that higher gerrymandering is reflected in increased taxes following an adverse shock to government budgets.

I choose to focus on gerrymandering as my preferred measure of electoral entrenchment for two reasons. First, it is a more direct measure of electoral entrenchment. I calculate the partisan bias of legislative district maps after every lower house legislative election, which happens every two years for most states. COVI is only calculated after presidential elections and requires a principal component analysis to convert binary and continuous data on election procedures to a single value. The second reason is identification. COVI does not distinguish which party the election laws benefit. During my sample period, the majority of laws passed that increase COVI, such as strict voter ID laws and reductions in polling locations, were put in place by Republicans. Partisan bias allows me to separate which party benefits from the bias.

Republicans and Democrats engage in electoral entrenchment. I make sure to be careful about controlling for political parties in all of my regressions to ensure I capture the effect of electoral entrenchment on the outcome rather than proxying for which party is in control. I include a dummy variable for which party controls the state legislator and governorship. Additionally, when using gerrymandering, I can disentangle which party the state legislative district map benefits. Regardless of which party the district lines benefit, the effect is reduced bond spreads, and there is no statistically significant difference in the coefficients.

In my baseline regressions, I include bond and year-month fixed effects along with several controls related to the state's fiscal health and economic condition alongside standard bond characteristic controls. Additional political controls include the competitiveness of the state legislature and a dummy for the controlling part of the state. I control for the competitiveness of the state legislature to rule out the possibility that partisan bias or COVI is just capturing how much one party controls a state. One party can dominate a state, but this may result from a party aligning with the will of their constitutions as opposed to maintaining control through electoral entrenchment.

Despite the number of controls, there may still be concern about omitted variable bias affecting my results. To offer more casual evidence, I run a difference-in-differences regression around the 2012 election, with treatment being single-party control of redistricting based on the 2010 Census. The political science literature has established that single-party control of redistricting is a strong predictor of higher gerrymandering(Stephanopoulos (2018)). The magnitude of the gerrymandering based on the 2010 Census took many by surprise (Keena et al. (2021)). While the lines were established before the 2012 election, the 2012 election offered the first test of their successes in gerrymandering. Using within-bond identification, I show that bonds whose state districts were drawn by a single party experienced lower tax-adjusted spreads post the 2012 election. I also establish that, regardless of which party drew the lines, the effect is negative and significant in the post-period.

In the second part of the paper, I explore potential mechanisms. I focus on two avenues that could explain the phenomenon. First, I test the channel of policy uncertainty. Previous literature has demonstrated that investors value political stability (Gao et al. (2019), Kelly et al. (2016)). The hypothesis is that electoral entrenchment reduces policy uncertainty because investors know which party will be in control, resulting in lower borrowing rates. I test this hypothesis using the measure of state-level policy uncertainty provided by Baker et al. (2022). I find that gerrymandering does not correlate with policy uncertainty while COVI does. Including policy uncertainty in the regressions on the tax-adjusted spread results in the coefficients on partisan bias and COVI remaining essentially unchanged.

The second channel I test is increased agency friction between politicians and their constituents. Similar to a firm in which shareholders elect board members to represent their interests, constituents within a state elect politicians to represent their views. Like my setting, firms with entrenched boards are less responsive to shareholders' preferences, but their debt financing costs are not immune to the opinions of bond market participants. Cremers et al. (2007) and Klock et al. (2005) find that reducing shareholder rights decreases the yield spreads of the firms' bonds. Chava et al. (2009) focus on bank loans and conclude that firms with more shareholder rights have higher spreads on their loans. In regards to corporate decisions, Berger et al. (1997) finds that greater managerial entrenchment is associated with taking on less debt.

Although it is not the focus of this study, results in the board entrenchment literature also echo political science research on the effects of electoral entrenchment on the principal. Gompers et al. (2003) demonstrate that firms with weaker shareholder rights had lower firm value, profits, and sales growth. Bebchuk and Cohen (2005) provide suggestive causal evidence that entrenched boards reduce firm value through their actions. Concerning politicians and citizens, measuring the cost of increased agency friction to the principal is not as easy as measuring reductions in shareholder value; however, the political science literature has identified a range of domains such as education, health care, and public safety (McGhee (2020)) where reducing politicians responsiveness to the voters is associated with worst outcomes for the constituents.

Why would state legislators prefer actions that align with bondholders' interests when their responsiveness to voters declines? Municipal bond markets are an essential resource for state and local governments (Adelino et al. (2017), Yi (2021)). Access to cheaper funding will allow entrenched politicians with partisan goals more of an ability to fulfill their policy objectives. Elected officials are more confident that they will be in control in the future and can take advantage of cheaper funding to meet their goals. Electoral entrenchment weakens the ties between voters and their representatives, but the government's actions are still reflected in the bond market.

I demonstrate a relationship between the divergent views of voters and their representatives and gerrymandering using latent policy and public opinion conservatism measures on economic and social issues provided by Caughey and Warshaw (2021). I take the difference between a state's policy conservatism and public opinion conservatism to measure how much more conservative policy is relative to public opinion. First, I show that the state bond spreads are reduced with higher economic conservatism but are unchanged by social conservatism. Next, I demonstrate that partisan bias and COVI correlate with more economic policy conservatism than economic public opinion conservatism. Additionally, I show that economic policies are more conservative than public opinion regardless of which party gerrymandering benefits. The same is not true for social issues in which the party who benefits from the bias determines if the policies are more or less conservative than expected. The regressions do not show an effect of legislative competition on public opinion and policy divergence for economic or social issues. The insignificant coefficient on legislature competitiveness could be because a lack of legislative competition may result from the sitting government meeting the public's desires.

As of 2021, state and local governments have unfunded pension obligations worth a market value of between \$6 and \$7 trillion trillion (Giesecke and Rauh (2023)). The scale of unfunded liabilities implies that state and local governments will have to increase taxes or cut spending to meet the required payments. Likely, voters do not favor funding legacy pension obligations over other government services.[2] All else equal, a state whose government is more able to increases taxes or cut spending (i.e., states with high levels electoral entrenchment) to meet pension obligations should experience lower spreads.

Except for Vermont, U.S. states have balanced budget requirements (Poterba and Rueben (2001)). This fact has limited the ability of researchers to use U.S. states as a laboratory for exploring theories of political economy and government debt.[3] I argue that pension funding, which state governments have more discretion over, is a mechanism for exploring the political incentives of debt accumulation.

I find that a one standard deviation increase in electoral entrenchment as proxied by gerrymandering correlates with a pension debt ratio, which is 1% lower. While this effect is small, the sitting government inherits the pension debt ratio, and it is hard to change quickly. Therefore, a better measure of a government's commitment to a fiscally responsible pension system is the percentage of annual required contribution payments made and the undertaking of pension reforms to reduce future liabilities. I do not find an effect on annual payments, but a probit regression shows that a one standard deviation increase in gerrymandering increases the probability of pension reform by 25%.

To better understand how gerrymandering affects a state's willingness to make unpopular public finance choices, I focus on shocks to government finances. I explore how gerrymandering relates to a state's ability to respond to budget shocks. I construct the measure of state budget shocks from Poterba (1994) and Clemens and Miran (2012) for my sample period and show a correlation between states with higher gerrymandering and raising taxes in response to deficit shocks.

[2] https://www.monmouth.edu/polling-institute/reports/monmouthpoll_nj_060916/

[3] See Alesina and Passalacqua (2016) for a helpful overview of theories on government debt and political economy

2 Related Literature

This paper contributes to the literature on political economy in asset pricing and public finance.

There is an extensive literature on political uncertainty and asset prices. Most of these studies have examined the effect of political uncertainty on asset returns, showing that investors require compensation for being exposed to political uncertainty (Belo et al. (2013),Kelly et al. (2016),Pástor and Veronesi (2013),Pastor and Veronesi (2012)). Using gubernatorial elections, Gao et al. (2019) demonstrate that political uncertainty is priced into municipal bonds. This literature was the catalyst for exploring whether electoral entrenchment operates through a political uncertainty channel.

The political economy asset pricing literature, closest to my work, considers the risk of citizens' preferences being translated into government policy. Miller (2023) examines how democracy increases the risk of governments instituting redistributive policies. Dasgupta and Ziblatt (2022) and Hansen (2023) find that less democratic countries have lower sovereign debt spreads. The results of Hansen (2023), which finds that countries' bond spreads benefit more from being an autocracy if they are financially vulnerable, echo my finding that the bond spreads of states with poorly funded pension systems benefit more from an increase in electoral entrenchment. Ambrose et al. (2023) demonstrate that cities and counties where fiscal policies are put directly to a constituent vote have higher tax-adjusted municipal spreads. My paper differs from previous work because I focus on state-level electoral entrenchment to measure responsiveness to citizens' preferences. My measures have the advantage of changing over time within states.

My paper also contributes to the literature on risks priced into municipal bonds. Schwert (2017) illustrates that between 74% to 84% of tax-adjusted municipal bond spread is due to default risk. Examples of priced risks identified in the literature include: elderly population (Butler and Yi (2022)), increased heat stress (Acharya et al. (2022)), rising sea levels (Painter (2020), Goldsmith-Pinkham et al. (2023)), pension under-funding (Novy-Marx and Rauh (2012),Boyer (2020)) and exposure to the opioid crisis (Cornaggia et al. (2022)). The growing literature on municipal bond pricing and politics is pertinent to my study. In addition to Ambrose et al. (2023) and Gao et al. (2019), Dagostino and Nakhmurina (2023) show that cities that have mayors who come from different political parties of their state's governors experience higher spreads. Cestau (2018) show that between 2009 and 2012, states with Republican governors experienced lower spreads, and Hong and Nadler (2016) show that states with a higher proportion of Democrats in state legislators during the 2008 financial crisis had increased borrowing costs. Cestau (2018) and Hong and Nadler (2016) highlight the importance of controlling for political parties in my analysis.

Gao et al. (2020) document that increased informational frictions result in higher bond spreads. More significant informational frictions likely make politicians less responsive to their voters, as the voters would not be as well informed about their representative's policy decisions. Their work shows that local newspaper closures increased municipal borrowing costs for areas covered by the paper. They argue that the increased spread is due to the loss of government monitoring by the papers. One could imagine a similar story of gerrymandering, which allows one party to maintain control, resulting in representatives choosing the quiet life and shirking budget management responsibilities.

Additionally, this paper adds to the literature on political economy and public finance.[4] Alt and Lowry (1994) demonstrate that divided state governments, in which one party controls the legislator and another the governorship, are slower to respond to revenue shocks. Besley and Case (1995) and Escaleras and Calcagno (2009) find that several public finance outcomes are affected by gubernatorial term limits. Besley et al. (2010) provide evidence that low political competition results in higher taxes and lower capital spending at the state level. My contribution to this literature is to show another channel through which political incentives affect public finance.

Finally, I contribute to the literature on the effects of gerrymandering. McGhee (2020) provides an overview of the current state of the literature. In his review, he highlights how early empirical work found a minor role in gerrymandering shaping politician institutions (Gelman and King (1994a), Born (1985)). These initial results were reevaluated in the 21st century as partisan gerrymandering increased. Renewed interest in the topic resulted in an influential series of papers that documented the extent of gerrymandering after the 2010 Census (McGhee (2014),Stephanopoulos and McGhee (2015)). With well over a decade of data in hand since what has been dubbed the "The Great Gerrymander of 2012" by Wang (2013), researchers have recently turned their attention towards the downstream effects of gerrymandering on political institutions and policy(Stephanopoulos and Warshaw (2020),Caughey et al. (2017)). As far as I am aware, I am the first to consider the effects of gerrymandering on municipal bond spreads and public finance decisions.

[4] Persson and Tabellini (2002) provide a textbook introduction and survey of the field

3 Background

This section details the measures of electoral entrenchment, partisan bias and cost of voting index.

3.1 Partisan Bias

Gelman and King (1994a) first proposed partisan bias, which has become the most frequently used measure of gerrymandering by political scientists (Stephanopoulos and Warshaw (2020)). Part of the measure's appeal is intuitively capturing an idea of fairness between the parties. If one party received x% of the state-wide votes and got y% of seats, then had the other party gotten x% of the state-wide votes, they also should have got y% of seats. The mapping between vote and seat share is known as the seat-votes curve. The seat-votes curve can be highly responsive in the extreme, where if one party wins 51% of the votes, they get 100% of the legislative seats; or it could also be proportional where 51% of the votes result in 51% of seats, or anywhere in-between.

Partisan bias measures how asymmetric the curve is around 50%. More formally, using the notation of Katz et al. (2020), let the seat-vote function be defined as S(V), where S(·) maps the vote share V to the number of legislative seats. After an election, only a single instance of V and S(V) is observed, so to construct the full curve, an assumption of uniform partisan swing is made. This assumption states that if the state-wide vote share changes, the change is proportional across districts. Once the counterfactual vote totals for each district have been set, the hypothetical winners of each seat can be established to come up with a seat total for the new vote share. A district map is fully symmetric if $S(V) = 1 - S(1 - V) \ \forall \ V \in [0, 1]$.

Katz et al. (2020) advise estimating partisan bias as an average over a range of vote shares between 45% and 55%. Partisan bias is then calculated as follows:

$$\text{Partisan Bias} = \frac{1}{11} \times \left(\sum_{i=45}^{55} \frac{S(i\%) - (1 - S(1 - i\%))}{2} \right) \tag{1}$$

A partisan bias measure of -0.05 states that Democrats receive 5% fewer seats in the House than they would if the maps were symmetric. I take the absolute value of partisan bias as in Grumbach (2022). I also deconstruct the measure based on which party it benefits. Democratic bias is equal to 0 if the measure is positive and the absolute value of the measure if it is negative. Similarly, Republican bias is equal to

0 if negative and the measure's value if positive.

A hurdle for calculating partisan bias for state legislative districts is that there can be many uncontested elections. Uncontested elections pose a challenge because to calculate seat share, S(V), for a counterfactual state-wide voter share V, the researcher needs to know what vote share each party received in all the districts, which is unavailable in uncontested elections. I discuss how I handle uncontested elections in the data section.

A limitation of partisan bias is that it does not capture the extent to which there was an intent to give a party an advantage. Due to a state's population distribution, they may unintentionally draw lines that favor one party over another Chen et al. (2013). For the sake of my study, it does not matter if gerrymandering is an intention or not; only one party has an advantage in gaining legislative seats.

3.2 The Cost of Voting Index

The cost of voting index is a measure constructed by Li et al. (2018). The idea behind the measure is that time is a cost, so the longer and more involved it is to vote, the higher the cost. To create an index, the authors collect information on voting laws such as early voting, ID laws, poll hours, and online registration. The authors verify that the index captures an increased cost of voting by demonstrating that an increase in the measure is reflected in lower voter turnout.

Li et al. (2018) group election laws into seven issue areas: registration deadline, Voter registration restrictions, registration drive restrictions, pre-registration laws, voting inconvenience, Voter ID laws, and poll hours.[5] Next, the authors reduce the value to a single measure using principal component analysis. The greater the cost of voting index, the more costly it is to vote. The authors produce the measure for each state for every election year between 1996-2020.

4 Data

4.1 Municipal Bond Data

Municipal bond data contains issuance data from FTSE Russell and trade data from the Municipal Securities Rulemaking Board(MSRB). The sample spans from 2005-

[5] Table A2 in the appendix contains the complete list of items included in the index and how individual laws are aggregated into the seven categories.

2021 and contains bonds that are backed by U.S. states.[6] I restrict the sample to general obligation bonds to focus on bonds that have the full faith and credit of the state behind them.[7] I collect data on years to maturity, embedded options, issue size, coupon rate, insurance backing, and credit bureau rating. The credit rating is the latest S&P assigned rating; if an S&P rating is unavailable, I use Moody's.

Trade data from MSRB is aggregated to a year-month level using the average of customer buy transactions for a given bond. I apply the standard filters from the municipal bond literature (Schwert (2017), Gao et al. (2020),Ambrose et al. (2023), Acharya et al. (2022)). The filters include removing bonds with over a 20% coupon, a price less than 50 or greater than 150, within three months of their issuance date and less than one year to maturity. Additionally, I trim at the 1% level based on spread to remove the effect of outliers. Finally, I remove bonds that have a credit guarantee or are pre-refunded to remove bonds without credit risk.

I calculate the spread of each bond over a maturity-matched treasury rate from Gürkaynak et al. (2007). The spread is tax-adjusted using the highest marginal federal and state tax rates provided by NBER TaxSim Feenberg and Coutts (1993). I follow Garrett et al. (2023) and Ambrose et al. (2023) to calculate the tax rate used to adjust the spread. The equation for the tax rate accounting for state and federal taxes is:

$$\tau_{s,t}^{SF} = \tau_t^{\text{Federal}} \left(1 - \tau_{s,t}^{\text{state}} \times \mathbb{I}[t < 2018]\right) + \tau_{s,t}^{\text{state}} \times \mathbb{I}\left[\text{ State Exemption }\right]_{s,t}$$

Where $\tau_{s,t}$ is the tax rate for state s at time t, τ_t^{Federal} is the federal tax rate, and $\tau_{s,t}^{\text{state}}$ is the state tax rate at time at time t. $\mathbb{I}[\text{ State Exemption }]$ is an indicator of if state municipal bond income is exempt from taxation. $\mathbb{I}[t < 2018]$ is an indicator for if it is before 2018, after which investors can not deduct state taxes from their federal taxes.

The state and federal tax adjusted spread for bond i with maturity m is then calculated as:

$$\text{Spread}_{i,s,t,m}^{SF} = \frac{\text{Yield}_{i,s,t,m}}{\left(1 - \tau_{s,t}^{SF}\right)} - r_{m,t}^{f} \tag{2}$$

Where $r_{m,t}^{f}$ is the maturity-matched treasury rate. The federal tax adjust spread

[6]Special thanks to Tuomas Tomunen for sharing his mapping to identify state-backed bonds

[7]Using revenue bonds, the results are directionally the same but weaker.

Spread $^{F}_{i,s,t}$ follows the same equation except $\tau^{SF}_{s,t}$ is equal to $\tau^{\text{Federal}}_{t}$. The non-tax adjusted spread is given as spread$_{i,s,t}$ and is simply the yield to maturity minus a maturity-matched treasury rate.

Summary statistics for the municipal bond sample are provided in Table 1. The average yield in the sample is 285.3 basis points, with a non-tax adjusted spread of 37.52 basis points. As shown by studies (Green (1993)), adjusting for taxes significantly affects spreads; within my sample, the average state and federal tax adjust spread is 232.04 basis points, and the federal tax adjust spread is 197.35 basis points. Only 3% of the bonds are taxable, and no tax adjustment was applied to the spreads of those bonds.

4.2 State Level Controls

Fiscal data on the states comes from Pierson et al. (2015). The state unemployment rate comes from the Bureau of Labor Statistics. Data on state-level public pension funds is from the Public Plans Database from the Center for Retirement Research at Boston College and covers the years 2000-2021 for most plans. States will often have a pension plan for different professions (e.g., police, teachers, general employees), which I aggregate up to a state level by summing their reported liabilities and assets.

In my tests, I also control for which party is in control of the state and the level of competition within the state legislature. I get data on party legislative and gubernatorial party composition from the National Conference of State Legislatures. I calculate a party as controlling a state if a single party holds both legislative chambers and the Governor's office. The only exception is if a single party has a veto-proof majority in both legislative chambers, in which case they are in control regardless of the Governor's party. If neither party has control, then the state is considered divided.

The level of legislative competition comes from Besley and Case (2003). It is defined as:

$$\text{Leg. Competitiveness} = -1 * |.5 - S_{Dh}| * |.5 - S_{Ds}| \tag{3}$$

Where S_{Dh} is the share of Democratic seats in the House and S_{Ds} is the share of Democratic seats in the Senate. Greater numbers correspond to higher levels of legislative competition.

4.3 Electoral Entrenchment

I use two related samples for measuring partisan bias. The first sample comes from Stephanopoulos and Warshaw (2020) (SW), which covers elections from 1982 to 2016. I filled out partisan bias by year and month until the next election. That means I can cover the SW sample up to October 2018. This sample covers a shorter period but benefits from a more robust uncontested vote total estimation and multi-district state estimations.

SW collects presidential results along with other state-wide elections and maps them onto state legislative districts to estimate a model of what each party would have received had an election been contested. Their extensive data collection and mapping also allows them to estimate states with multi-member districts, which most other studies drop.[8] I use the SW sample as a robustness test and in the difference-in-differences regressions because their sample falls within the windows.

I construct my primary sample using state legislative election results from Klarner (2010). The sample goes from 1968 to 2022. I keep only states with single-member districts. As is standard in the literature Katz et al. (2020), I assume a constant vote share for uncontested elections. I assume that uncontested elections are won 66% to 33%. As in the SW sample, I fill the measure of gerrymandering forward to the next election. Unsurprisingly, the two samples highly correlate where they overlap with a correlation coefficient of 0.92.

Table 1 shows the summary statistics of my primary sample for partisan bias; Democrats have a slight disadvantage on average, with the full sample having a mean partisan bias measure of -0.01. Democrats receive 1% fewer seats compared to a fully symmetric district plan. The mean of the absolute value of partisan bias is 0.08, which affirms that both parties gerrymander, and much of each side cancels out when averaging over the raw value.

I use analysis of covariances to deconstruct the variance of partisan bias; 85% is between states, and 15% is over time. The variation over time is due to the redrawing of district lines every ten years, migration, and changing voting patterns between redistricting. Demographic change between redistricting years can be substantial; for example, Using Census data for 2016 and 2018 in Massachusetts. The voting age population grew by 1.6%, but the largest increase for a district was 8.6%, and the largest decrease for a district's voting population was 4.4%.

[8] States with multi-member districts are Arizona, Idaho, Maryland, New Hampshire, New Jersey, North Dakota, South Dakota, Vermont, and Washington.

The summary statistics for COVI during my sample period are also reported in Table 1. The mean value is 0.14, with a standard deviation of 0.63. The lowest COVI value in my sample is -2.061 from Oregon's 2016 election. The highest value is 1.302 from Mississippi's 2016 election. An analysis of covariance test shows that 98% of the variance of COVI is between states while only 2% is over time. Given that I control for state or bond fixed effects in all my regressions, the lack of between-year variation may explain why COVI is not significant in some of my regressions, particularly the ones that are on a yearly as opposed to a monthly level.

Panel A of Table 2 presents the raw correlations between the relevant variables. Competitive legislatures are negatively correlated with partisan bias, which may be explained by the fact that gerrymandering can be used as a tool to reduce competition within the legislature. While both COVI and gerrymandering capture electoral entrenchment, they measure distinct avenues. The correlation coefficient between the two is only 0.21. Given that my regressions control for either bond fixed-effects, which nests state fixed effects. A more relevant test is correlation conditional on state and year-month fixed effects. Panel B of Table 2 shows that most of the correlations are dampened once I condition on state and year-month.

5 Electoral Entrenchment and Bond Spreads

This section presents the main results of my paper, that bond spreads are reduced when electoral entrenchment is higher. The first section presents the results of regressing spreads onto measures of electoral entrenchment with a number of fixed effects and controls. The second section teases out causality by exploiting single party redistricting after the 2010 U.S. Census.

5.1 Baseline Specification

I begin by running a regression of different measures of spread on electoral entrenchment. The regression is:

$$\begin{aligned}\text{Spread}_{i,t} =& \alpha_0 + \alpha_1 * \text{Electoral Entrenchment}_{s,t} + \\ &+ \alpha_2' \text{State Controls}_{s,t} + \alpha_3' \text{ Bond Controls}_{it} + \eta_i + \eta_t + \varepsilon_{i,t}\end{aligned} \tag{4}$$

Where, η_i and η_t are bond and year-month fixed effects. The bond fixed effect absorbs time-invariant features of the bond and issuing state. I control for bond maturity and liquidity. To measure liquidity, I follow Goldsmith-Pinkham et al. (2023) and use monthly trading volume and the standard deviation of the price. I include credit rating times year-month fixed effects to allow for time variation in the valuation of credit rating buckets. In addition to the state political controls, I control the debt over GDP, pension debt, which is pension liabilities over assets, and state-level unemployment rate. I cluster at the state times the election year in which the measure is calculated because that is the level of variation of my variable of interest.

The spread measure in the baseline regression will be either federal and state, federal or not tax adjusted. The reason for providing the three different spread measures is to demonstrate that it is not just a story of changing tax rates.

The results for the regression in Equation 4 with partisan bias as a proxy for electoral entrenchment are in Table 3. The spreads are in basis points, and all non-dummy variables are standardized to have mean zero and variance one. Column one of Table 3 shows that a one standard deviation increase in partisan bias results in a 6.5 basis point reduction in spreads. With an average state and federal tax-adjusted spread of 238, a 6.5 basis point reduction in spreads is 2.7% of the mean. The fact that partisan bias is significant in all three specifications implies that the effect is not driven solely by changes in tax rates.

Table 4 contains the results of Equation 4 with COVI as a proxy for electoral entrenchment. Column 1 of the Table shows that a one standard deviation increase in the cost of voting index corresponds to a 7.25% decrease in spreads adjusted for state and federal taxes. The effect remains significant in column 2, which only adjusts spreads for federal taxes. The effect size for both electoral entrenchment proxies is in line with previous municipal bond literature, which typically finds a single or low double-digit effect on municipal bond spreads (Li (2024)).

Table 10 includes both partisan bias and COVI. The COVI and partisan bias coefficients stay essentially unchanged when run in the same regression. It is also worth highlighting in this table along with Tables 3 and 4 that divided governments result in higher spreads. Divided governments increasing spreads is result that has been documented by Basu et al. (2024). Divided governments are tangentially related to electoral entrenchment because it indicates that a party could not lock itself into power at both the legislative and executive levels. There are a host of other reasons why divided governments can increase spreads, such as slower responses to budget shocks Alt and Lowry (1994) or delays in designing budgets Andersen et al. (2012), so there is not a direct line of divided governments increasing spreads through a lack of electoral entrenchment.

Using the benefit that partisan bias can be divided between which party in benefits, Table 6 breaks down partisan bias into Democrat and Republican bias. No matter which party the bias favors, the effect on spreads is negative. An F-test shows that the null hypothesis that the effect of Democrat bias is equal to Republican bias fails to be rejected. The point of Table 6 is to alleviate the concern that the effect on my measure of partisan bias is just capturing the fact that one party has been more successful during my sample period.

Robustness Appendix B contains the results of my robustness tests. Column 1 of Table B1 contains the baseline regression results but with issuance fixed effects rather than bond fixed effects. This specification includes time-invariant bond controls, such as coupon rate, option feature dummies, and bond issuance size. Column 2 of Table B1 contains the baseline specification without adjusting for time-varying changes in credit rating. Only credit ratings are included in this specification. Column 3 is the same regression from Column 1 of Table 10 except the standard errors are clusters at the issuance and year-month level. Finally, Column 4 restricts the baseline regression to bonds with at least six observations in the sample.

Table B2 contains the baseline results with differing controls. Due to the potentially large number of uncontested state elections, it may be a concern that this is driving the results. Column 1 contains the results controlling for the percent of uncontested seats in the House. The coefficient on the percent of uncontested seats is insignificant. Column 2 contains the result without controlling for legislative competitiveness, while Column 3 does not control for any political variables. Finally, Column 4 shows the results not controlling for state economic controls.

Lastly, Table B3 uses the SW sample to estimate the baseline specification. The sample from Stephanopoulos and Warshaw (2020) only goes up to 2018, but it benefits from including multi-district states and a more robust estimation of hypothetical uncontested election results.

The first column of Table B3 contains the specification from Column 1 of Table 10 but uses the SW sample. The second column uses partisan bias estimates using presidential election results from Stephanopoulos and Warshaw (2020). When using state legislative results to estimate partisan bias, the concern is that candidate quality may affect voting behavior. The idea is that a party might not put forth a strong candidate if they know they are unlikely to win the district, making the districts look more biased if a strong contender had been running. Although this is a potential concern, Stephanopoulos and Warshaw (2020) do not find that their results change if they estimate gerrymandering with presidential election returns, and neither do my results.

Column 3 of Table B3 uses the mean-median measure of gerrymandering. The mean-median is the difference between the mean and median vote share. For fair maps, the mean and median should be roughly equal. One of the benefits of the mean-median measure is that a hypothetical seat-vote share curve does not need to be estimated. Column 3 shows that the mean-median measure of gerrymandering is significant and similar in magnitude to partisan bias.

5.2 Difference-in-Differences

One concern for identification is that there might be time-varying state economic factors that affect bond spreads and correlate with changes in electoral entrenchment. The literature has not identified any time-varying state economic factors that correlate with electoral entrenchment, but that does not rule out that any may exist. To address potential concerns, I utilize partisan bias and run a difference-in-differences around the 2012 election with single-party control of redistricting as the treatment effect.

The treatment date is November 2012 because that is the first election with the new districts. District lines had been established, and publicity was known before the election, but their full magnitude likely was not realized until after the election. I set the window around the post date to be two years, from 2010 to 2014. Given that I am using a period contained within the Stephanopoulos and Warshaw (2020) sample, I utilize their sample to expand the cross-section.

I use party control of 2010 census redistricting provided by Keena et al. (2021) as my treatment variable.[9] As highlighted in their study, there was barely any news coverage about redistricting in the run-up to the 2010 election, making it unlikely that voters selected their candidates based on redistricting.

Column one of Table 7 confirms previous studies identifying party control of redistricting as a salient factor. The coefficient of 0.03 means that if a state had single-party control of redistricting in 2010-2011, then partisan bias measured after the 2012 election was 0.03 higher. The number of observations reflects the sample

[9] Table A1 in the appendix shows who controlled redistricting during this cycle.

used in the difference-in-differences and includes states for which gerrymandering and bond spreads are measured.

The difference-in-differences model is written as:

$$\begin{aligned} \text{Spread}_{i,t}^{SF} =& \alpha_0 + \alpha_1 * (\text{Post}_t \times \text{Same Party Draw}_s) + \\ &+ \alpha_2' \text{State Controls}_{s,t} + \alpha_3' \text{ Bond Controls}_{it} + \eta_i + \eta_t + \varepsilon_{i,t} \end{aligned} \tag{5}$$

The coefficient of interest is α_1. The coefficients on Post_t and Same Party_s the fixed effects absorb. Table 7 shows the results of regression 5. Column 2 contains the result over a two-year window and $\alpha_1 = -11.48$, which is significant at the 5% level. This effect is larger than my baseline analysis, which may be explained by the fact that the difference-in-differences period is at the start of a redistricting period, which means there will be another ten years before redistricting. My baseline is over the entire sample period, including years close to redistricting.

To establish no pre-trends, Figure 1 shows the coefficients of running a dynamic difference-in-differences with quarter-year dummies. The vertical dotted black line indicates the treatment date near the end of quarter four in 2012. There do not appear to be pre-trends in the data as the coefficient is not significant at the 10% level until after the treatment date.

In order to highlight that the treatment effect is not due to just one party having outsized control over the redistricting processes, Column 3 in Table 7 breaks down the treatment to Democrats or Republicans having control of the redistricting processes. In both instances, the coefficient is negative and significant. The coefficient on Democrats controlling redistricting times the post period is lower than if the Republicans controlled redistricting, but the coefficients are not statistically different from one another.

To show robustness Columns 4 and 5 repeat the exercise of the difference-in-differences except with a one year window. In both instances the coefficients remain negative and significant.

I also provide the results for three placebo tests in Table 8. The first column uses a two-year window with the same treatment as the baseline line regressions, except the treatment period is now the midterm elections in November 2010. The second column similarly uses a two-year window with the same treatment but for the midterm election in November 2014. In both instances, the coefficient of treatment times post is insignificant. Finally, column three contains the same window and post period from the baseline regressions in Table 7, but now the treatment is defined as single-party control of the state government after the 2010 census. This treatment differs from the same party draw treatment because, in some states, a single party will not control redistricting even if one party controls all branches of government. The treatment times post-period coefficient is insignificant in this regression.

6 Mechanism Results

After establishing a relationship between electoral entrenchment and bond spreads, the next logical question is why the relationship exists. This section will explore two possibilities: political uncertainty and agency frictions.

6.1 Policy Uncertainty

I use the state-level economic policy uncertainty (EPU-S) measure provided by Baker et al. (2022). They provide a measure of monthly economic policy uncertainty for each state constructed from extracting text from local newspapers related to policies. As in their original paper, I provide an inverse hyperbolic sine transformation to their measure.

The regression I run is:

$$\begin{aligned} \text{EPU-S}_{s,t} =& \beta_0 + \beta_1 * \text{Electoral Entrenchment}_{s,t} + \\ & + \beta_2' \text{State Controls}_{s,t} + \eta_s + \eta_t + \varepsilon_{s,t} \end{aligned} \tag{6}$$

Within my sample the inverse hyperbolic sine transformation of EPU-S has a mean of 4.77 and a standard deviation of .84. The hypothesis is that the sign of β_1 is negative. The idea behind the hypothesis is that a party protecting themselves from removal will result in less policy uncertainty because investors will know which party will likely remain in control.

Table 9 contains the results of the regression from Equation 6. Column 1 shows that the coefficient on partisan bias is insignificant, although the sign is in the direction expected. Column 2 contains the results using COVI as a proxy for electoral entrenchment. Here, the sign is negative and significant at the 10% level. Column 3 contains both measures, with the coefficients being negative and only COVI being significant at the 5% level. All three columns show that policy uncertainty increases when the government is divided.

What could explain the fact that for my two measures of electoral entrenchment, partisan bias is insignificant, and COVI is significant? One potential explanation might be that policy uncertainty, as measured by newspaper articles, may operate more through the gubernatorial office. Gerrymandering only affects the state legislature, not state-wide elections. The party controlling the state legislature may remain the same, but uncertainty may not be reduced if the Governor's office changes parties. The positive coefficient on divided government supports this hypothesis: policy uncertainty increases when different parties control the legislature and Governor's office. The cost of voting applies to all elections, even state-wide ones, which could provide more predictability in controlling parties of the state.

Next, I include the measure of policy uncertainty in the regression from equation 4 with state and federal tax-adjusted spreads as the outcome variable. If I control for credit ratings, the significance of the coefficient of state policy uncertainty drops out, suggesting that rating agencies consider policy uncertainty when assigning ratings. Table 10 contains the regression results without credit rating fixed effects for all columns. Column 1 shows that the coefficient on EPU-S has the expected sign. A one standard deviation increase in EPU-S corresponds to a one basis point increase in spreads. Column 2 removes EPU-S and contains the regression results with just partisan bias and COVI. Finally, column 3 re-introduces EPU-S alongside partisan bias and COVI. In this specification, the coefficients on partisan bias and COVI are mostly unchanged and the coefficient on EPU-S has decreased slightly and lost significance.

6.2 Agency Frictions

Next, I test whether agency frictions explain my core results. Typically, agency frictions are associated with sub-optimal outcomes, but they lead to more responsible public finance management in this context. One note of caution is that this analysis only looks at responses to agency frictions from the bondholder's perspective. Absent is any welfare analysis from the constituents' point of view. Additionally, this analysis only considers agency friction's effects on public finance, and one could imagine other domains, such as education, health care, and public safety, where having increased agency friction between voters and their representatives could be particularly detrimental.

This section contains two sub-sections. The first section looks at how electoral entrenchment is related to pension funding, as unfunded pensions are a future stress to state budgets. The following section examines how electoral entrenchment interacts with shocks to state budgets.

6.2.1 Liability Interactions

My motivation for exploring public pensions as a potential channel is provided by Table 11, which runs the regression from Equation 4 but with an interaction between electoral entrenchment and either the pension debt ratio, which is liabilities over assets, or the debt outstanding over GDP. Column 1 shows the regression with partisan bias, and the pension debt ratio interacted. The higher the pension debt ratio the more partisan bias matters.

Figure 2 graphically illustrates this result. I scaled partisan bias and the pension debt ratio to a mean zero and a variance of one. The blue line has the worst-funded pension system, the red line has the best-funded pension system, and the green line is in between. The y-axis is the total effect on the spread. The x-axis is partisan bias. As partisan bias increases, the total impact decreases for states with the worst-funded pensions but remains relatively flat for states with the best-funded pensions. The interaction effect supports the agency friction hypothesis because a state with a well-funded pension system may not have to make difficult public finance decisions that go against the public's wishes and would not benefit as much from elected officials insulated from public sentiment.

Further supporting the agency frictions is Column 2 of Table 11, which shows the interaction of partisan bias with debt to GDP. The interaction is insignificant. The balanced budget requirements in most states could explain the insignificant interaction, as they limit the political gamesmanship of debt issuance.

Finally, Columns 3 and 4 show that COVI has no significant interaction between the pension debt ratio or debt to GDP. The fact that partisan bias interacts with the pension debt ratio while COVI does not may reflect a difference in how the cost of voting affects incentives to fund the pension, but I hesitate to conclude because much of the variation in COVI is between states which is absorbed through the use of bond fixed effects. The lack of significance in the interaction between COVI and the pension debt ratio may then be the result of too much variation being absorbed through fixed effects.

6.2.2 Pension Funding

Next, due to the fact that gerrymandering interacted with pension health has an impact on spreads, I explore whether states with higher gerrymandering engage in more responsible pension management. First, I run a regression of the pension debt ratio for individual funds on partisan bias. In addition to controls used in previous regressions, I include the fund's yearly return and the discount rate used for their liabilities. Column one of Table 12 shows that a one standard deviation increase in partisan bias is reflected in a -0.018 decrease in the pension debt ratio, which averages 1.36 across the sample with a standard deviation of 0.26. This effect is relatively small.

The sitting government largely inherits the pension debt ratio, and it is hard to change quickly. Therefore, a better measure of a government's willingness to fund the pension system is the percent of annual required contribution (ARC) payments made. The annual required contribution percent is how much the state has paid off the accrued pension benefits for that year plus the required amount for their amortization of unfunded actuarial accrued liability. An annual required contribution paid percent of one means the government has made the payments required to ensure their pensions are adequately funded. Column 2 of Table 12 shows that the coefficient are partisan bias is insignificant.

Next, I see if higher partisan bias correlates with an increased likelihood of making pension reforms. Pension reforms take the form of increased contribution rates or decreased benefits. Pension reform can be politically challenging because, with proper salary adjustments, reducing benefits or increasing costs to workers can reduce the quality of the public service workforce. Based on data collected from the National Association of State Retirement Administrators, I define pension reform as a binary variable, depending on whether or not they have reformed their pension.[10] Column 3 of Table 12 shows the results of running a probit regression on whether a state had a pension reform that year or not. Calculating the marginal effect, a one-unit increase in partisan bias is associated with a 4.14% increase in the probability of a pension change, holding all other variables constant

The seemly extensive effects presented in Table 11 and the minor effects presented in Table 12 point to the possibility that gerrymandering's ability to allow future governments to fund the pension system as opposed to the current funding of the pension system which is priced into the bonds. The following section will explore

[10] https://www.nasra.org/pensionreform

how gerrymandering affects states' ability to respond to budget shocks.

6.2.3 Deficit Shocks

In this section, I examine how shocks to the state government budgets interact with higher levels of gerrymandering. When hit with a shock that causes expenses to exceed revenue, states can either increase taxes, cut spending, or draw down from their rainy day fund. Raising taxes or reducing government services can be politically unpopular. I hypothesize that the less responsive elected officials are to their constituents, the more willing they are to raise taxes or reduce spending when hit with a budget shock.

I use the methodology proposed by Poterba (1994) to construct shocks to state government budgets. The National Association of State Budget Officers (NASBO) surveys states for their projected revenue and expenses for the coming fiscal year and the realized revenue and expenses for the previous fiscal year. Additionally, states report any changes in taxes or spending cuts during the year.

I collect NASBO surveys for each year in my sample and first calculate a revenue shock as follows:

$$\text{Revenue Shock}_{st} = \text{Actual Revenues}_{\,st} - \Delta\text{Tax}_{st} \quad - \text{Forecast Revenues}_{\,st} \tag{7}$$

The actual revenues the states report include tax changes made during the fiscal year. I subtract tax changes made during the year to uncover the true revenue shock. An expense shock is calculated as:

$$\text{Expense shock}_{\,st} = \ \text{Actual Outlays}_{\,st} - \Delta\,\text{Spending}_{st} \text{ - Forecast Outlays}_{\,st} \tag{8}$$

Similar to the revenue shock, I subtract changes in spending that occurred during the fiscal year to recover the expense shock. Finally, the deficit shock is then calculated as:

$$\text{Deficit Shock}_{\,st} = \ \text{Expense Shock}_{\,st} - \ \text{Revenue Shock}_{\,st}. \tag{9}$$

The deficit shock is defined as a positive value being a shortfall. The values are per capita and are in 2022 dollars to make the measure comparable across time and states.

Next, I run regressions on tax and spending changes to determine what drives this dampening effect. Following Poterba (1994) and Clemens and Miran (2012), who run similar tests, I focus on the subset of 27 states that have annual budgets. The other 23 states are on two-year budget cycles. Annual budget states are more pertinent than two-year budget cycles because the tax and spending changes are recorded yearly.

To get a sense of the different reactions to the direction of the shock, I break down the deficit shock into positive (budget shortfall) and negative shocks (budget surplus). The regressions I run are then :

$$\begin{aligned}\Delta_{s,t} =& \alpha_0 + \alpha_1 * \text{Deficit Shock} > 0 \ + \alpha_2 * \text{Deficit Shock} < 0 \\ &+ \alpha_4' \text{Controls}_{st} + \eta_s + \eta_t + \varepsilon_{st}\end{aligned} \tag{10}$$

Deficit Shock > 0 is the value of deficit shock if it is greater than 0 and 0 otherwise. Similarly, Deficit Shock < 0 is the value of deficit shock if it is less than 0 and 0 otherwise. $\Delta_{s,t}$ is either tax or spending changes.

Columns 1 and 2 of Table 13 display the regression results from equation 10. These columns confirm the results of Poterba (1994) and Clemens and Miran (2012), which show that when there is a budget shortfall, states reduce government services or increase taxes.

The subsequent regression I run includes an interaction for partisan bias. The equation is the same as equation 10 except for adding interaction terms between partisan bias and deficit shocks. Column 3 shows the interaction regression with budget cuts. The insignificance of the interaction coefficients points towards electoral entrenchment not affecting budget cuts during budget shortfalls or surpluses.

Column 4 of 13 shows the interaction regression with tax changes as the outcome variable. In this regression, the interaction term between partisan bias and a budget shortfall has a positive coefficient. The coefficient on a budget shortfall is 13.59, and the interaction term with partisan bias has a coefficient of 3.42, which means that a one standard deviation increase in partisan bias is reflected in a 25% higher tax increase relative to a state with an average amount of partisan bias, ceteris paribus.

Columns 5 and 6 show the interaction regressions on budget cuts and tax increases. In all cases the interaction terms are insignificant. The insignificant interactions could be due to the lack of variation in the cost of voting or the costs of voting not giving politicians enough coverage to raise taxes or cut budgets in response to shortfalls.

6.2.4 Policy-Public Opinion Gap

The final tests of agency frictions will attempt to proxy for agency friction using the gap between public opinion and policy. Caughey and Warshaw (2018) use policy and survey data to construct latent policy and public opinion conservatism measures for each state from 1936 to 2019. The authors use item response theory to construct a latent conservative measure at the policy and public opinion level. The authors create these measures for social policies and economic policies.

To construct a measure of the policy-public opinion (PPO) gap, I take the difference between policy conservatism and public opinion conservatism for economic and social issues. Formally:

$$\text{PPO Gap}_{st}^{\{E,S\}} = \text{policy conservatism}_{st}^{\{E,S\}} - \text{public opinion conservatism}_{st}^{\{E,S\}} \quad (11)$$

Where s is the state, and t is the year. $\{E,S\}$ indicates if the measure is either economic(E) or social(S). Greater values mean that policy is more conservative than public opinions of the state.

I choose to focus on the economic policies as those are more likely to be relevant for state bond holders.[11] Table 14 regresses the federal and state tax-adjusted, federal tax-adjusted, and non-tax-adjusted spreads on economic and social policy conservatism. As all three columns show, more conservative economic policy is correlated with lower spreads while the level of conservatism of social policy has an insignificant effect on spreads. In the case of federal and state tax-adjusted spreads, a one standard deviation increase in conservative economic policy corresponds to a 31 basis point decrease in spreads.

Using the SW sample from 1982-2018, I regress the PPO gap on partisan bias.

[11] Investors may care about social policies to the extent that they affect the municipality's economy. See Lu and Ye (2023) for an example of a social issue, abortion rights, being translated into municipal bond risk due to lower net migration.

Column 1 of Table 15 shows that the coefficient on partisan bias is positive, which means an increase in gerrymandering is reflected in economic policy being more conservative than expected given public opinion. Column 2 of Table 15 shows that COVI has the same directional effect.

Since partisan bias is an absolute value measure, it does not differentiate which party the bias benefits. Column 3 of Table 15 separates the bias between Republicans and Democrats. No matter which party the bias favors, the effect is positive. Column 4 of Table 15 shows the same regression with social policy residuals. When looking at social policies, Democrat bias results in social policies being less conservative, and Republican bias results in more conservative social policies. Column 4 supports what has been identified in previous literature: gerrymandering moves policy towards the party that benefits from it (Caughey et al. (2017),Caughey and Warshaw (2021)). Taken together, the regression results from columns 3 and 4 suggest that the direction of the gap between policy and public opinion and the party that benefits from gerrymandering may depend on the specific policy area.

Finally, it is worth highlighting the insignificant coefficient on legislative competitiveness. A hypothesis might be that a non-competitive legislature could act with impunity against the public's wishes. The results from Table 15 indicate this may not be true. A potential explanation for this result is that even though the legislation is dominated by one party, individual legislatures still face competitive elections which prevent them from deviating from the pubic opinion.

7 Conclusion

In this article, I establish a robust relationship between electoral entrenchment and the spread of municipal bonds. The relationship lends credence to the notion that politicians' responsiveness to their constitutions is vital for understanding public finance decisions.

I explore two potential mechanism avenues. Using the cost of voting index, I find support for policy uncertainty. With partisan bias, I find support for the effect operating through the willingness to fund public pensions and agency frictions, allowing for unpopular public finance choices.

Constitutional hardball is defined as breaking established norms and stretching the limits of the law by a political party to give them an advantage in elections or legislative negotiations(Tushnet (2003)). Constitutional hardball has become an increasing phenomenon in U.S. politics (Fishkin and Pozen (2018),Bernstein (2018)). With changing norms come new incentive structures and agency frameworks. Given that the state's financial conditions appear to be getting worse before they get better due to growing legacy pension and other health benefit obligations, it is vital to understand the relationship between electoral entrenchment, political responsiveness, and public finance decisions.

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8 Figures and Tables

Figure 1: This figure shows the results of a dynamic difference-in-differences regression the specification from Column 2 of Table 7. Each period is quarter year indicator variable. The time period is from 11/2010-11/2014.The black line is the event period which is 11/2012. The blue bars correspond to standard errors at the 10% level.

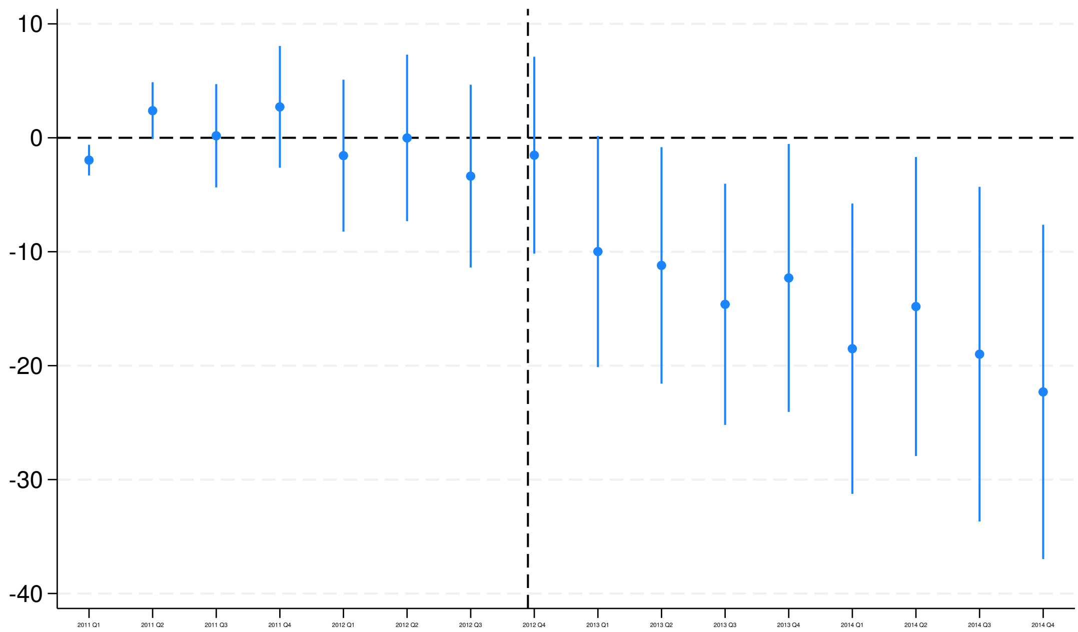

Figure 2: This figure illustrates the interaction effect of the pension debt ratio and partisan bias on state and federal tax adjusted spreads. The estimates are from Column 1 of Table 11. Both variables have been scaled to have mean zero and variance one. The red line is for states with a pension debt ratio one standard deviation above the mean. The green line is for states with a mean level of pension debt. The blue line is for states with a pension debt ratio one standard deviation below the mean. A higher pension debt ratio, the worst funded the pension system is. The shading for each line corresponds to a 5% level standard error on the total effect.

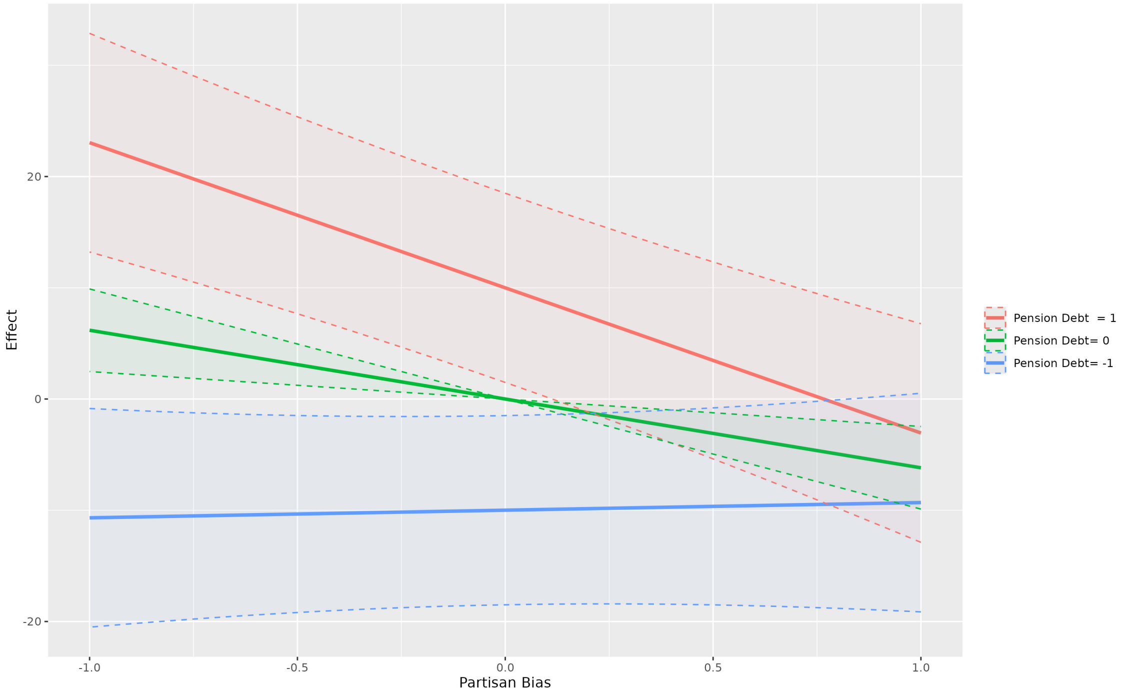

Table 1: This table presents the summary statistics of the variables used in the main analysis. The municipal bonds is at a bond year-month level. The political variables and state economic variables are at a state year-month level. All variable definitions appear in Appendix A.

	Mean	Median	Std. Dev.	P25	P75	N
Municipal Bonds						
Years to Maturity (Years)	8.51	7.11	5.89	3.83	11.99	548,843
Issue Size ($ mil)	36.74	16.71	165.91	8.11	35.00	548,843
Yield to Maturity (bps)	284.84	300.62	135.51	171.19	390.04	548,843
Spread (bps)	37.07	22.40	92.58	-29.03	93.10	548,843
State and Federal Tax-Adjusted Spread (bps)	238.09	212.30	162.72	100.44	349.66	548,843
Federal Tax-Adjusted Spread (bps)	200.44	179.34	142.37	79.93	299.39	548,843
Callable	0.54	1.00	0.50	0.00	1.00	548,843
Taxable	0.03	0.00	0.16	0.00	0.00	548,843
Std. Dev. Price	0.39	0.10	0.63	0.00	0.61	548,843
Trading Volume	0.03	0.01	0.71	0.00	0.02	548,843
Political Variables						
Raw Partisan Bias	-0.01	-0.02	0.09	-0.08	0.05	6,717
Partisan Bias	0.08	0.07	0.05	0.03	0.10	6,717
Democrat Control	0.27	0.00	0.44	0.00	1.00	6,717
Divided Control	0.37	0.00	0.48	0.00	1.00	6,717
Republican Control	0.37	0.00	0.48	0.00	1.00	6,717
Legislative Competitiveness	-0.03	-0.01	0.04	-0.03	-0.00	6,717
Cost Of Voting Index	0.14	0.26	0.63	-0.13	0.58	6,717
State Economic Variables						
State Unemployment	6.10	5.50	2.41	4.40	7.40	6,717
$\frac{Debt}{GDP}$	0.07	0.07	0.04	0.05	0.09	6,717
Pension Debt Ratio	1.36	1.31	0.26	1.17	1.53	6,717

Table 2: This table presents the summary statistics of the variables used in the main analysis. The variables are at the state month level. Panel A shows the raw correlations. Panel B shows the correlations conditioning on state and year-month. All of the regressions in my analysis include year-month and either state or bond fixed effects making Panel B the more relevant correlations.

Panel A: Unconditional Correlations

	Partisan Bias	Leg. Comp.	Democrat	Divided	Republican	Unemployment	$\frac{Debt}{GDP}$	Pension Debt Ratio	COVI
Partisan Bias	1.00								
Leg. Competitiveness	-0.61	1.00							
Democrat	0.09	-0.24	1.00						
Divided	-0.29	0.24	-0.46	1.00					
Republican	0.21	-0.02	-0.46	-0.58	1.00				
Unemployment	-0.18	0.11	0.06	0.01	-0.07	1.00			
$\frac{Debt}{GDP}$	0.09	-0.42	0.36	0.04	-0.38	0.11	1.00		
Pension Debt Ratio	0.05	-0.22	0.17	-0.01	-0.14	0.14	0.45	1.00	
COVI	0.21	-0.03	-0.36	0.01	0.32	0.11	-0.12	0.10	1.00

Panel B: Conditional Correlations

	Partisan Bias	Leg. Comp.	Democrat	Divided	Republican	Unemployment	$\frac{Debt}{GDP}$	Pension Debt Ratio	COVI
Partisan Bias	1.00								
Leg. Competitiveness	-0.55	1.00							
Democrat	0.06	-0.07	1.00						
Divided	-0.15	0.24	-0.68	1.00					
Republican	0.13	-0.25	-0.18	-0.60	1.00				
Unemployment	-0.11	-0.01	0.05	0.03	-0.10	1.00			
$\frac{Debt}{GDP}$	-0.07	-0.16	0.05	-0.13	0.13	0.13	1.00		
Pension Debt Ratio	-0.06	0.27	0.06	0.07	-0.17	-0.08	-0.02	1.00	
COVI	0.06	-0.03	-0.21	0.09	0.11	-0.00	0.11	-0.10	1.00

Table 3: This table presents the estimates from the regression of Equation 4 with partisan bias used as the measure for electoral entrenchment. The outcome variable is the spread as calculated from Equation 2. Spread ^{SF} is the spread adjusted for state and federal taxes. Spread ^{F} is adjusted for federal taxes. Spread from Column 3 has no tax adjustments. The regressions are at the bond year-month level. I calculate Partisan Bias for states with non-multi-member districts. All bonds are general obligation. Credit rating is the most recent Moody's credit rating when available, otherwise the S &P is used if available. Bond Controls include years to maturity, standard deviation of prices for the month and monthly trading volume. The bond fixed effects absorb all time invariant bond characteristics such as coupon rate, optionally, issue size and use of proceeds. Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The years of the sample are from 2005-2021. Standard errors are clustered at the state × election year in which partisan bias is calculated. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1)	(2)	(3)
	Spread ^{SF}	Spread ^{F}	Spread
Partisan Bias	-6.458***	-6.257***	-3.344***
	(-3.32)	(-3.46)	(-2.92)
Leg. Competitiveness	-6.956	-5.861	-3.371
	(-1.12)	(-1.04)	(-1.04)
Divided	9.400**	8.117**	4.316*
	(2.30)	(2.17)	(1.87)
Democrat	8.212	5.174	1.778
	(1.41)	(0.98)	(0.57)
Unemployment Rate	1.967	2.182	1.162
	(0.67)	(0.83)	(0.70)
$\frac{Debt}{GDP}$	28.16***	26.72***	17.39***
	(2.99)	(3.10)	(3.48)
Pension Debt Ratio	10.92**	9.809**	5.207*
	(2.21)	(2.15)	(1.91)
Observations	513859	513859	513859
Adjusted R Squared	0.854	0.845	0.848
CUSIP FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Credit Rating × Year-Month FE	✓	✓	✓
Bond Controls	✓	✓	✓

Table 4: This table presents the estimates from the regression of Equation 4 with COVI used as the measure for electoral entrenchment. The outcome variable is the spread as calculated from Equation 2. Spread ^{SF} is the spread adjusted for state and federal taxes. Spread ^{F} is adjusted for federal taxes. Spread from Column 3 has no tax adjustments. The regressions are at the bond year-month level. COVI comes from Li et al. (2018). All bonds are general obligation. Credit rating is the most recent Moody's credit rating when available, otherwise the S &P is used if available. Bond Controls include years to maturity, standard deviation of prices for the month and monthly trading volume. The bond fixed effects absorb all time invariant bond characteristics such as coupon rate, optionally, issue size and use of proceeds. Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The years of the sample are from 2005-2021. Standard errors are clustered at the state × election year in which COVI is calculated. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) Spread ^{SF}	(2) Spread ^{F}	(3) Spread
COVI	-7.258**	-6.160**	-2.091
	(-2.15)	(-2.08)	(-1.23)
Leg. Competitiveness	-0.0511	0.900	0.338
	(-0.01)	(0.15)	(0.10)
Divided	10.04**	8.698**	4.570*
	(2.34)	(2.14)	(1.85)
Democrat	8.093	5.229	2.041
	(1.36)	(0.94)	(0.61)
Unemployment Rate	1.524	1.716	0.863
	(0.43)	(0.54)	(0.43)
$\frac{Debt}{GDP}$	35.41***	33.55***	20.78***
	(3.15)	(3.24)	(3.15)
Pension Debt Ratio	10.55*	9.444	5.009
	(1.67)	(1.62)	(1.42)
Observations	513859	513859	513859
Adjusted R Squared	0.854	0.844	0.848
CUSIP FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Credit Rating × Year-Month FE	✓	✓	✓
Bond Controls	✓	✓	✓

Table 5: This table presents the estimates from the regression of Equation 4 with COVI and partisan bias used as the measure for electoral entrenchment. The outcome variable is the spread as calculated from Equation 2. Spread ^{SF} is the spread adjusted for state and federal taxes. Spread ^{F} is adjusted for federal taxes. Spread from Column 3 has no tax adjustments. The regressions are at the bond year-month level. COVI comes from Li et al. (2018). I calculate Partisan Bias for states with non-multi-member districts. All bonds are general obligation. Credit rating is the most recent Moody's credit rating when available, otherwise the S &P is used if available. Bond Controls include years to maturity, standard deviation of prices for the month and monthly trading volume. The bond fixed effects absorb all time invariant bond characteristics such as coupon rate, optionally, issue size and use of proceeds. Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The years of the sample are from 2005-2021. Standard errors are clustered at the state × election year in which COVI is calculated. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) Spread ^{SF}	(2) Spread ^{F}	(3) Spread
Partisan Bias	-6.386***	-6.197***	-3.323***
	(-3.30)	(-3.42)	(-2.86)
COVI	-7.136**	-6.041**	-2.027
	(-2.16)	(-2.09)	(-1.19)
Leg. Competitiveness	-7.444	-6.274	-3.509
	(-1.02)	(-0.94)	(-0.95)
Divided	9.739**	8.405**	4.412*
	(2.22)	(2.03)	(1.74)
Democrat	6.837	4.010	1.388
	(1.11)	(0.71)	(0.41)
Unemployment Rate	2.260	2.430	1.245
	(0.64)	(0.76)	(0.62)
$\frac{Debt}{GDP}$	29.75***	28.07***	17.84***
	(2.67)	(2.73)	(2.84)
Pension Debt Ratio	10.94*	9.823*	5.212
	(1.77)	(1.72)	(1.50)
Observations	513859	513859	513859
Adjusted R Squared	0.854	0.845	0.848
CUSIP FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Credit Rating × Year-Month FE	✓	✓	✓
Bond Controls	✓	✓	✓

Table 6: This table presents the estimates from the regression of Equation 4 with partisan bias broken down in to Democrat and Republican bias used as the measure for electoral entrenchment. Democrat Bias is equal to partisan bias if partisan bias is positive and zero otherwise. Republican bias is equal to negative one times partisan bias if partisan bias is negative and zero otherwise. The outcome variable is the spread as calculated from Equation 2. Spread ^{SF} is the spread adjusted for state and federal taxes. Spread ^{F} is adjusted for federal taxes. Spread from Column 3 has no tax adjustments. The regressions are at the bond year-month level. I calculate Partisan Bias for states with non-multi-member districts. All bonds are general obligation. Credit rating is the most recent Moody's credit rating when available, otherwise the S &P is used if available. Bond Controls include years to maturity, standard deviation of prices for the month and monthly trading volume. The bond fixed effects absorb all time invariant bond characteristics such as coupon rate, optionally, issue size and use of proceeds. Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The years of the sample are from 2005-2021. Standard errors are clustered at the state × election year in which partisan bias is calculated. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) Spread ^{SF}	(2) Spread ^{F}	(3) Spread
Democrat Bias	-7.103*	-6.287*	-4.058*
	(-1.77)	(-1.74)	(-1.91)
Republican Bias	-6.775***	-6.844***	-3.150**
	(-3.19)	(-3.37)	(-2.30)
Leg. Competitiveness	-6.955	-5.783	-3.323
	(-1.12)	(-1.02)	(-1.02)
Divided	9.279**	7.918**	4.341*
	(2.31)	(2.16)	(1.91)
Democrat	7.937	4.723	1.854
	(1.39)	(0.91)	(0.59)
Unemployment Rate	1.911	2.116	1.122
	(0.65)	(0.81)	(0.67)
$\frac{Debt}{GDP}$	28.31***	27.47***	16.99***
	(2.90)	(3.07)	(3.23)
Pension Debt Ratio	10.92**	9.802**	5.192*
	(2.20)	(2.14)	(1.89)
Observations	513859	513859	513859
Adjusted R Squared	0.854	0.844	0.848
F-Test Democrat Bias= Republican Bias	0.01	0.02	0.13
P-Value	0.94	0.89	0.72
CUSIP FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Credit Rating × Year-Month FE	✓	✓	✓
Bond Controls	✓	✓	✓

Table 7: This table presents the estimates from the regression of Equation 5. The first column outcome variable is partisan bias post 2012 election using the SW sample. The outcome variable for columns 2 through 5 is the spread as calculated from Equation 2. Spread ^{SF} is the spread adjusted for state and federal taxes. Post is an indicator variable for if it is after the 2012 election. Same Party Draw is equal to one if a single party was in control of redistricting after the 2010 census. Republican Draw is equal to one if Republicans were in control of redistricting after the 2010 census. The regressions are at the bond year-month level. Democrat Draw is equal to one if Democrats were in control of redistricting after the 2010 census. Fixed-effects absorb the post and treatment variables. The regressions are at the bond year-month level. All bonds are general obligation. Credit rating is the most recent Moody's credit rating when available, otherwise the S &P is used if available. Bond Controls include years to maturity, standard deviation of prices for the month and monthly trading volume. The bond fixed effects absorb all time invariant bond characteristics such as coupon rate, optionally, issue size and use of proceeds. Divided and Democrat are dummy variables with Republican being the dropped dummy. State controls include state unemployment rate, debt to GDP ratio and pension debt ratio. Column one is a cross section regression using 2012 data. Columns 2 through 5 have their time window listed at the bottom of the table. Standard errors are clustered at the state and year-month level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) Partisan Bias 2012	(2) Spread ^{SF}	(3) Spread ^{SF}	(4) Spread ^{SF}	(5) Spread ^{SF}
Same Party Draw	0.0301*				
	(2.00)				
Post× Same Party Draw		-11.48**		-10.34***	
		(-2.39)		(-3.00)	
Post× Republican Draw			-10.68**		-10.29***
			(-2.15)		(-2.98)
Post× Democrat Draw			-14.48**		-10.61**
			(-2.41)		(-2.47)
Leg. Competitiveness	-0.0153**	8.348**	7.812*	2.001	1.913
	(-2.66)	(2.03)	(1.86)	(0.59)	(0.56)
Divided	0.000167	-11.75	-11.60	-17.66**	-17.63**
	(0.01)	(-1.54)	(-1.53)	(-2.56)	(-2.57)
Democrat	-0.0511***	-11.18	-11.15	-17.62**	-17.60**
	(-3.62)	(-1.28)	(-1.28)	(-2.17)	(-2.18)
Partisan Bias 2011	0.282				
	(1.33)				
Observations	37	156693	156693	79408	79408
Adjusted R Squared	0.445	0.919	0.919	0.957	0.957
F-Test Post × R Draw = Post × D Draw			0.62		0.01
P-Value			0.43		0.90
CUSIP FE		✓	✓	✓	✓
Year-Month FE		✓	✓	✓	✓
Credit Rating × Year-Month FE		✓	✓	✓	✓
Bond Controls		✓	✓	✓	✓
State Controls		✓	✓	✓	✓
Years	2012	2010-2014	2010-2014	2011-2013	2011-2013

Table 8: This table presents the placebo tests for difference-in-differences model. The outcome variable is the spread as calculated from Equation 2. Spread ^{SF} is the spread adjusted for state and federal taxes. Post is an indicator variable for if it is after the 2012 election. Same Party Draw is equal to one if a single party was in control of redistricting after the 2010 census. Same Party Control is equal to one if a single party was in control of all three branches of government between 2010 and 2012. Fixed-effects absorb the post and treatment variables. The regressions are at the bond year-month level. All bonds are general obligation. Credit rating is the most recent Moody's credit rating when available, otherwise the S &P is used if available. Bond Controls include years to maturity, standard deviation of prices for the month and monthly trading volume. The bond fixed effects absorb all time invariant bond characteristics such as coupon rate, optionally, issue size and use of proceeds. Divided and Democrat are dummy variables with Republican being the dropped dummy. State controls include state unemployment rate, debt to GDP ratio and pension debt ratio. Column one is a cross section regression using 2012 data. Columns 2 through 5 have their time window listed at the bottom of the table. Standard errors are clustered at the state and year-month level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) Spread ^{SF}	(2) Spread ^{SF}	(3) Spread ^{SF}
Post$\times$ Same Party Draw	5.846	-4.475	
	(1.45)	(-0.89)	
Post$\times$ Same Party Control			1.869
			(0.29)
Divided	13.56***	21.99**	-10.41
	(3.30)	(2.47)	(-1.26)
Democrat	8.295*	12.86	-2.939
	(1.78)	(1.32)	(-0.34)
Leg. Competitiveness	5.279	6.334	10.64**
	(1.04)	(1.46)	(2.63)
Observations	151468	144573	156697
Adjusted R Squared	0.899	0.941	0.919
CUSIP FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Credit Rating FE $\times$ Year-Month	✓	✓	✓
Bond Controls	✓	✓	✓
State Controls	✓	✓	✓
Years	2008-2012	2012-2016	2010-2014

Table 9: This table presents the estimates from the regression of Equation 6. The outcome variable, EPU-S, is state level economic policy uncertainty which comes fomr Baker et al. (2022). The regressions are at the state year-month level. COVI comes from Li et al. (2018). I calculate Partisan Bias for states with non-multi-member districts. Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The years of the sample are from 2000-2021. Standard errors are clustered at the state × election year in which COVI is calculated. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) EPU-S	(2) EPU-S	(3) EPU-S
Partisan Bias	-0.0252		-0.0278
	(-1.20)		(-1.27)
COVI		-0.0482*	-0.0499**
		(-1.89)	(-2.02)
Leg. Competitiveness	-0.0449	-0.0293	-0.0541
	(-1.14)	(-0.82)	(-1.24)
Divided	0.105**	0.0944**	0.0944**
	(2.41)	(2.19)	(2.20)
Democrat	0.0116	-0.0171	-0.0177
	(0.24)	(-0.33)	(-0.34)
Unemployment Rate	0.143***	0.152***	0.147***
	(6.15)	(5.81)	(5.69)
$\frac{Debt}{GDP}$	-0.104*	-0.0984	-0.107*
	(-1.91)	(-1.57)	(-1.71)
Pension Debt Ratio	-0.0212	-0.0206	-0.0226
	(-0.83)	(-0.65)	(-0.73)
Observations	9573	9573	9573
Adjusted R Squared	0.428	0.429	0.429
Year-Month FE	✓	✓	✓
State FE	✓	✓	✓

Table 10: This table presents the estimates from the regression of state level economic policy uncertainty, EPU-S, on spreads. The outcome variable is the spread as calculated from Equation 2. Spread ^{SF} is the spread adjusted for state and federal taxes. The regressions are at the bond year-month level. Column one contains just EPU-S from Baker et al. (2022). Column two is only with partisan bias and COVI. Column 3 contains EPU-S, partisan bias and COVI. COVI comes from Li et al. (2018). I calculate Partisan Bias for states with non-multi-member districts. All bonds are general obligation. Unlike previous spread regressions this model does not contain credit ratings. Bond Controls include years to maturity, standard deviation of prices for the month and monthly trading volume. The bond fixed effects absorb all time invariant bond characteristics such as coupon rate, optionally, issue size and use of proceeds. Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The years of the sample are from 2005-2021. Standard errors are clustered at the issue ID and year-month level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) Spread ^{SF}	(2) Spread ^{SF}	(3) Spread ^{SF}
EPU-S	0.993*		0.929
	(1.72)		(1.64)
Partisan Bias		-5.655***	-5.694***
		(-3.51)	(-3.54)
COVI		-5.935**	-5.836**
		(-2.38)	(-2.34)
Leg. Competitiveness	-2.632	-9.023	-9.011
	(-0.48)	(-1.46)	(-1.46)
Divided	9.080**	9.329**	9.385**
	(2.47)	(2.52)	(2.53)
Democrat	-0.849	-2.948	-2.800
	(-0.16)	(-0.54)	(-0.52)
Unemployment	8.056**	9.382**	9.228**
	(2.23)	(2.43)	(2.40)
$\frac{Debt}{GDP}$	43.19***	40.04***	40.03***
	(3.76)	(3.65)	(3.64)
Pension Debt Ratio	13.80***	13.61***	13.65***
	(3.31)	(3.29)	(3.30)
Observations	513690	513690	513690
Adjusted R Squared	0.844	0.845	0.845
CUSIP FE	✓	✓	✓
Year-Month FE	✓[44]	✓	✓
Bond Controls	✓	✓	✓

Table 11: This table presents the estimates from the regression of Equation 4 with COVI and partisan bias used as the measure for electoral entrenchment interacted with either the pension debt ratio or debt outstanding to GDP. The outcome variable is the spread as calculated from Equation 2. Spread ^{SF} is the spread adjusted for state and federal taxes. The regressions are at the bond year-month level. COVI comes from Li et al. (2018). I calculate Partisan Bias for states with non-multi-member districts. All bonds are general obligation. Credit rating is the most recent Moody's credit rating when available, otherwise the S &P is used if available. Bond Controls include years to maturity, standard deviation of prices for the month and monthly trading volume. The bond fixed effects absorb all time invariant bond characteristics such as coupon rate, optionally, issue size and use of proceeds. Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The years of the sample are from 2005-2021. Standard errors are clustered at the state × election year in which COVI is calculated. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) Spread ^{SF}	(2) Spread ^{SF}	(3) Spread ^{SF}	(4) Spread ^{SF}
Partisan Bias	-6.182***	-6.496***		
	(-3.26)	(-3.36)		
Pension Debt Ratio	9.994**	10.94**	10.76*	10.69*
	(2.30)	(2.20)	(1.70)	(1.69)
Partisan Bias × Pension Debt Ratio	-6.868***			
	(-4.16)			
$\frac{Debt}{GDP}$	16.91*	28.81***	34.48***	35.96***
	(1.94)	(2.97)	(3.03)	(3.20)
Partisan Bias × $\frac{Debt}{GDP}$		0.830		
		(0.54)		
COVI			-7.178**	-7.265**
			(-2.16)	(-2.16)
COVI × Pension Debt Ratio			-1.811	
			(-0.64)	
COVI × $\frac{Debt}{GDP}$				3.196
				(0.90)
Leg. Competitiveness	-5.509	-6.721	-0.499	1.346
	(-0.93)	(-1.10)	(-0.08)	(0.22)
Divided	11.57***	9.482**	9.830**	10.04**
	(2.97)	(2.31)	(2.30)	(2.38)
Democrat	8.160	8.479	7.848	8.120
	(1.43)	(1.43)	(1.31)	(1.37)
Unemployment Rate	1.666	2.075	1.391	2.104
	(0.57)	(0.71)	(0.40)	(0.60)
Observations	513859	513859	513859	513859
Adjusted R Squared	0.854	0.854	0.854	0.854
CUSIP FE	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓
Credit Rating × Year-Month FE	✓	✓	✓	✓
Bond Controls	✓	✓	✓	✓

Table 12: This table presents the results from regression pension variables on partisan bias. Pension Debt Ratio is the state's aggregate pension liabilities over assets. ARC Percent is the annual required contribution paid over the required annual contribution rate for that year. Pension Reform is a dummy variable which is equal to one if a state implemented pension reforms that year. The regressions are at a state year level. Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The years of the sample are from 2000-2021. Standard errors are clustered at the state × election year in which partisan bias is calculated. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) Pension Debt Ratio	(2) ARC Percent	(3) Pension Reform
Partisan Bias	-0.0182*	0.00103	0.201*
	(-1.92)	(0.07)	(1.87)
Leg. Competitiveness	0.0250	0.0166	-0.0942
	(1.53)	(0.35)	(-0.55)
Divided	0.0382*	-0.00204	-0.0890
	(1.96)	(-0.04)	(-0.37)
Democrat	0.0389*	-0.0277	0.0872
	(1.85)	(-0.67)	(0.32)
Unemployment Rate	-0.0173	-0.00964	0.110
	(-1.45)	(-0.57)	(0.63)
$\frac{Debt}{GDP}$	0.0547**	0.0522	0.498*
	(2.53)	(0.60)	(1.70)
Pension Debt Ratio		-0.0430*	0.0800
		(-1.84)	(0.44)
Observations	796	789	550
Adjusted R Squared	0.823	0.120	0.199
Year FE	✓	✓	✓
State FE	✓	✓	✓

Table 13: This table presents the estimates from the regression of Equation 10. Budget Changes are changes in spending that occurred throughout fiscal year. Tax Changes are changes in taxation that occurred throughout fiscal year. Deficit Shock is defined in Equation 9. A positive deficit shock means there was a budget shortfall and a negative deficit shock is a budget surplus. Deficit Shock + is equal to Deficit Shock if Deficit Shock is positive and is zero otherwise. Deficit Shock - is equal to Deficit Shock if Deficit Shock is negative and is zero otherwise. COVI comes from Li et al. (2018). I calculate Partisan Bias for states with non-multi-member districts.Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The regression is at the state year level with states which have an annual budget cycle. The years of the sample are from 2000-2021. Standard errors are robust. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) Budget Changes	(2) Tax Changes	(3) Budget Changes	(4) Tax Changes	(5) Budget Changes	(6) Tax Changes
Deficit Shock +	-25.17***	15.31**	-25.65***	13.59**	-21.66***	14.65**
	(-5.78)	(2.26)	(-5.45)	(2.04)	(-4.18)	(1.98)
Deficit Shock -	-2.378	2.613	-1.866	2.277		
	(-0.92)	(1.59)	(-0.70)	(1.40)		
Divided	0.994	2.639	1.585	2.642	3.683	3.699
	(0.22)	(1.02)	(0.35)	(1.04)	(0.69)	(1.23)
Democrat	0.336	1.415	0.924	0.566	1.678	1.477
	(0.07)	(0.37)	(0.18)	(0.14)	(0.24)	(0.35)
Leg. Competitiveness	-0.739	-4.206	5.425	-5.371	-0.310	-6.383
	(-0.19)	(-1.40)	(1.07)	(-0.99)	(-0.08)	(-1.47)
Partisan Bias			6.420**	-0.909		
			(2.16)	(-0.33)		
Deficit Shock + × Partisan Bias			0.983	3.427**		
			(0.43)	(2.47)		
Deficit Shock - × Partisan Bias			-0.112	0.644		
			(-0.13)	(0.66)		
COVI					2.926	-0.564
					(0.76)	(-0.21)
Deficit Shock + × COVI					0.460	7.268
					(0.06)	(0.78)
Deficit Shock -					-0.0161	0.0231
					(-0.71)	(1.47)
Deficit Shock - × COVI					-0.000819	-0.0168
					(-0.07)	(-0.92)
Observations	671	671	671	671	522	522
Adjusted R Squared	0.498	0.223	0.501	0.241	0.512	0.216
State FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
State Controls	✓	✓	✓	✓	✓	✓
Cluster	Robust	Robust	Robust	Robust	Robust	Robust
Years	2000-2021	2000-2021	2000-2021	2000-2021	2000-2021	2000-2021

Table 14: This table shows the results of regressing spreads on latent measures of economic and social conservatism.The outcome variable is the spread as calculated from Equation 2. Spread ^{SF} is the spread adjusted for state and federal taxes. Spread ^{F} is adjusted for federal taxes. Spread from Column 3 has no tax adjustments. Economic Policy Conservatism is a latent measure of how conservative state economic polices are. Social Policy Conservatism is a latent measure of how conservative state social polices are. Both measures come from Caughey and Warshaw (2021). The regressions are at a bond year-month level. All bonds are general obligation. Credit rating is the most recent Moody's credit rating when available, otherwise the S &P is used if available. Bond Controls include years to maturity, standard deviation of prices for the month and monthly trading volume. The bond fixed effects absorb all time invariant bond characteristics such as coupon rate, optionally, issue size and use of proceeds. Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The years of the sample are from 2005-2021. Standard errors are clustered at the state × year level . *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) Spread ^{SF}	(2) Spread ^{F}	(3) Spread
Economic Policy Conservatism	-31.01***	-22.86***	-14.52***
	(-3.22)	(-2.67)	(-2.87)
Social Policy Conservatism	2.980	3.334	5.105
	(0.37)	(0.46)	(1.23)
Divided	3.074	4.228	2.208
	(0.97)	(1.55)	(1.40)
Democrat	4.338	4.529	1.248
	(1.00)	(1.18)	(0.56)
Unemployment Rate	1.582	2.337	1.979
	(0.40)	(0.65)	(0.96)
$\frac{Debt}{GDP}$	24.11***	20.97***	14.08***
	(2.85)	(2.74)	(3.23)
Pension Debt Ratio	9.217**	8.218**	4.420**
	(2.39)	(2.33)	(2.21)
Observations	541129	541129	541129
Adjusted R Squared	0.857	0.847	0.849
CUSIP FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Credit Rating × Year-Month FE	✓	✓	✓
Bond Controls	✓	✓	✓

Table 15: This table shows the results of public and policy opinion gaps as defined in 11 on electoral entrenchment. PPO Gap^E is the public and policy opinion gap on economic policies and PPO Gap^S is public and policy opinion gap on social policies. A positive public and policy opinion gap means that polices are more conservative then public opinion and a negative value means that policies are less conservative then public opinion. COVI comes from Li et al. (2018). Partisan Bias comes from Stephanopoulos and Warshaw (2020).Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The regression is at the state year level. The years of the sample are from 1982-2018. Standard errors are clustered at the state × election year in which COVI is calculated. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) PPO Gap^E	(2) PPO Gap^E	(3) PPO Gap^E	(4) PPO Gap^S
Partisan Bias	0.0624***			
	(4.60)			
Leg. Competitiveness	0.00545	0.0157	0.00328	0.0162
	(0.23)	(0.40)	(0.14)	(0.51)
Divided	-0.269***	-0.223***	-0.257***	-0.151***
	(-8.12)	(-6.52)	(-7.64)	(-3.73)
Democrat	-0.366***	-0.344***	-0.349***	-0.340***
	(-8.97)	(-8.57)	(-8.44)	(-6.82)
COVI		0.138***		
		(4.97)		
Democrat Bias			0.0475***	-0.0457**
			(2.71)	(-2.09)
Republican Bias			0.0647***	0.124***
			(4.74)	(7.07)
Observations	1546	990	1546	1546
Adjusted R Squared	0.924	0.957	0.924	0.821
State FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

A Appendix

State-Level Variables

- **Raw Partisan Bias** : Level of gerrymandering. Positive values are bias towards Democrats and negative values are biased towards Repulicans.
- **Partisan Bias**: The absolute value of Raw Partisan Bias
- **Democrat**: Indicator variable if Democrats control both legislative branches and the executive branch.
- **Republican**: Indicator variable if Republicans control both legislative branches and the executive branch.
- **Divided**: Indicator variable if neither party control both legislative branches and the executive branch.
- **Legislative Competitiveness**: The competitiveness of the legislature. Higher values correspond to a more competitive legislature
- **Cost of Voting Index**: PCA measure of the cost of voting across seven issue areas.
- **State Unemployment**: The level of unemployment for a state in a given month
- $\frac{Debt}{GDP}$: The debt outstanding of a state over the state's GDP.
- **Pension Debt Ratio**: The aggregate liabilities over the aggregate assets across a state's pension system.

Bond-Level Variables

- **Years to Maturity**: Number of years left until a bond matures
- **Issue Size**: Size of the original offering
- **Yield to Maturity**: Yield to maturity without any adjustments
- **Spread**: Yield to maturity minus a maturity matched treasury rate
- **State and Federal Tax-Adjusted Spread** : Yield to maturity minus a maturity matched treasury rate adjusted for state and federal taxes

- **Federal Tax-Adjusted Spread** : Yield to maturity minus a maturity matched treasury rate adjusted for federal taxes
- **Callable**: An indicator variable for if a bond is callable
- **Taxable**: An indicator variable for if a bond is taxable
- **Std. Dev. Price** : Standard deviation of the prices of a bond within a given month
- **Trading Volume**: The amount of trading volume for a given month over the amount outstanding

Table A1: Table showing which party drew the maps and through which means, based on the 2011 redistricting cycle. This table is adapted from Keena et al. (2021).

Republicans	**Democrats**	**Both Parties**	**Independent Body**
State Legislature	*State Legislature*	*State Legislature*	*Citizen Commission*
Alabama House	Delaware House	New York House	Alaska House
Florida House	Illinois House	Oregon House	Arizona House
Georgia House	Kentucky House	*Politician Commission*	California House
Indiana House	Massachusetts House	Colorado House	Idaho House
Louisiana House	Rhode Island House	Connecticut House	Iowa House
Michigan House	Vermont House	Hawaii House	Montana House
Mississippi House	West Virginia House	Maine House	Washington House
New Hampshire House	*Politician Commission*	Missouri House	*Court*
North Carolina House	Arkansas House	New Jersey Assembly	Kansas House
North Dakota House	Maryland House	Pennsylvania House	Minnesota House
Oklahoma House			Nevada Assembly
South Carolina House			New Mexico House
South Dakota House			
Tennessee House			
Texas House			
Utah House			
Virginia House			
Wisconsin House			
Wyoming House			
Politician Commission			
Ohio House			

Table A2: Component Parts of the Cost of Voting Index. This table is taken from Li et al. (2018)

Issue area	**Cost of voting consideration**
1. Registration deadline (ratio variable)	No. of days prior to election that registration must occur
2. Voter reg. restrictions (additive indicator)	Same day registration not allowed for all elections
	Same day registration not located at poll locations
	Felons not allowed to register
	Mental competency req. for voter registration
	No online voter registration
	Same day registration not allowed in presidential election
	No automatic voter registration
3. Reg. drive restrictions (additive indicator)	Official certification required by state
	Participation in state training course required
	Group required to submit documents to state
	Penalty imposed for any violation of deadline or rule
4. Preregistration laws (0–5; Likert scale)	0 = 16-year-olds allowed to preregister
	1 = 17-year-olds allowed to preregister
	2 = 17.5-year-olds allowed to register
	3 = allowed to register 90 days prior to 18th birthday
	4 = allowed to register 60 days prior to 18th birthday
	5 = no preregistration allowed
5. Voting inconvenience (additive indicator)	No early voting
	Excuse required for absentee voting
	State sanctioned excuse required for absentee voting
	No in-person absentee voting
	No all mail voting
	No "ask once and always able to vote absentee"
	No time off from work for voting
	No time off from work with pay for voting
	Reduced number of polling stations
6. Voter ID laws (0–5; Likert scale)	0 = no ID required to cast a ballot, only signature
	1 = non-photo ID required not strictly enforced
	2 = photo ID required not strictly enforced
	3 = non-photo ID required strictly enforced
	4 = photo ID required strictly enforced
7. Poll hours (ratio variable)	Min. and max. poll hours (averaged and reversed)

B Appendix

Table B1: This table presents the robustness tests for the regression of Equation 4 with COVI and partisan bias used as the measure for electoral entrenchment. The outcome is the the spread adjusted for state and federal taxes. Column 1 contains the results of the baseline regression with issuance fixed effects. In this specification time invariant bond controls are included such as coupon rate, option feature dummies and bond issuance size. Column 2 contains the baseline specification without adjusting for time-varying changes in credit rating. Column 3 is the same regression from Column 1 of Table 10 except the standard errors are clusters at the issuance and year-month level. Column 4 restricts the baseline regression to bonds which have at least six observations in the sample. The regressions are at the bond year-month level. COVI comes from Li et al. (2018). I calculate Partisan Bias for states with non-multi-member districts. All bonds are general obligation. Credit rating is the most recent Moody's credit rating when available, otherwise the S &P is used if available. Bond Controls include years to maturity, standard deviation of prices for the month and monthly trading volume. Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The years of the sample are from 2005-2021. Standard errors are clustered at the state × election year in which COVI is calculated. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1)	(2)	(3)	(4)
	Spread ^{SF}	Spread ^{SF}	Spread ^{SF}	Spread ^{SF}
Partisan Bias	-5.347**	-7.106***	-6.386***	-6.447***
	(-2.49)	(-3.37)	(-4.02)	(-3.34)
COVI	-7.261*	-6.708*	-7.136***	-7.193**
	(-1.91)	(-1.77)	(-3.07)	(-2.17)
Leg. Competitiveness	-10.39	-8.849	-7.444	-7.490
	(-1.29)	(-1.18)	(-1.24)	(-1.02)
Divided	10.55**	5.916	9.739**	9.732**
	(2.17)	(1.16)	(2.54)	(2.21)
Democrat	8.800	-5.047	6.837	6.831
	(1.31)	(-0.60)	(1.36)	(1.10)
Unemployment Rate	4.855	4.783	2.260	2.243
	(1.17)	(1.05)	(0.77)	(0.63)
$\frac{Debt}{GDP}$	32.44***	26.76**	29.75***	29.96***
	(2.67)	(2.17)	(2.84)	(2.69)
Pension Debt Ratio	12.73**	8.237	10.94**	10.95*
	(2.02)	(1.13)	(2.59)	(1.77)
Observations	513932	513969	513859	507209
Adjusted R Squared	0.792	0.847	0.854	0.853
CUSIP FE		✓	✓	✓
Issue ID FE	✓			
Year-Month FE	✓	✓	✓	✓
Credit Rating × Year-Month FE	✓		✓	✓
Credit Rating FE		✓		
Bond Controls	✓	✓	✓	✓

Table B2: This table presents the robustness tests for the regression of Equation 4 with COVI and partisan bias used as the measure for electoral entrenchment. The outcome is the the spread adjusted for state and federal taxes. Column 1 contains the results controlling for the percent of uncontested seats in the House. Column 2 contains the result without controlling for legislative competitiveness while Column 3 does not control for any political variables. Column 4 shows the results not controlling for any state economic controls. The regressions are at the bond year-month level. COVI comes from Li et al. (2018). I calculate Partisan Bias for states with non-multi-member districts. All bonds are general obligation. Credit rating is the most recent Moody's credit rating when available, otherwise the S &P is used if available. Bond Controls include years to maturity, standard deviation of prices for the month and monthly trading volume. Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The years of the sample are from 2005-2021. Standard errors are clustered at the state × election year in which COVI is calculated. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) Spread ^{SF}	(2) Spread ^{SF}	(3) Spread ^{SF}	(4) Spread ^{SF}
Partisan Bias	-6.487***	-5.101***	-5.345***	-7.956***
	(-3.03)	(-2.93)	(-3.13)	(-3.79)
COVI	-7.078**	-6.991**	-6.776**	-6.150*
	(-2.17)	(-2.07)	(-2.00)	(-1.82)
Leg. Competitiveness	-7.316			-10.08
	(-1.01)			(-1.19)
% Uncontested	0.478			
	(0.17)			
Divided	9.783**	8.860**		9.644**
	(2.25)	(2.11)		(2.18)
Democrat	6.837	6.627		8.137
	(1.11)	(1.10)		(1.35)
Unemployment Rate	2.263	2.149	2.974	
	(0.64)	(0.60)	(0.82)	
$\frac{Debt}{GDP}$	29.75***	31.89***	30.95**	
	(2.67)	(2.65)	(2.53)	
Pension Debt Ratio	10.88*	10.17	10.11	
	(1.76)	(1.61)	(1.61)	
Observations	513859	513859	513859	513926
Adjusted R Squared	0.854	0.854	0.854	0.854
CUSIP FE	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓
Credit Rating × Year-Month FE	✓	✓	✓	✓
Bond Controls	✓	✓	✓	✓

Table B3: This table presents the robustness tests for the regression of Equation 4. The outcome is the the spread adjusted for state and federal taxes. Column 1 contains the specification from Column 1 of Table 10 but using the SW sample. Column 2 uses partisan bias estimates using presidential election results. Column 3 uses the mean-median measure of gerrymandering. The regressions are at the bond year-month level. COVI comes from Li et al. (2018). Gerrymandering estimates comes from Stephanopoulos and Warshaw (2020). All bonds are general obligation. Credit rating is the most recent Moody's credit rating when available, otherwise the S &P is used if available. Bond Controls include years to maturity, standard deviation of prices for the month and monthly trading volume. Divided and Democrat are dummy variables with Republican being the dropped dummy. Every other variable in the table are standardized to have mean zero and variance one. The years of the sample are from 2005-2021. Standard errors are clustered at the state × election year in which COVI is calculated. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The t-statistics are in parenthesis under the coefficients.

	(1) Spread ^{SF}	(2) Spread ^{SF}	(3) Spread ^{SF}
Partisan Bias	-4.365*		
	(-1.83)		
Partisan Bias Presidential		-8.337***	
		(-2.84)	
Mean Median			-4.386**
			(-2.07)
COVI	-5.538*	-6.192**	-5.653**
	(-1.93)	(-2.20)	(-2.02)
Leg. Competitiveness	1.123	5.154	1.784
	(0.20)	(1.01)	(0.32)
Divided	4.690	4.728	5.958
	(0.91)	(0.90)	(1.15)
Democrat	3.612	2.774	4.112
	(0.55)	(0.42)	(0.64)
Unemployment Rate	5.874	5.832	5.812
	(1.06)	(1.03)	(1.01)
$\frac{Debt}{GDP}$	27.76**	30.76***	27.40**
	(2.55)	(2.81)	(2.55)
Pension Debt Ratio	6.804	5.437	6.826
	(1.14)	(0.95)	(1.13)
Observations	474211	474211	474211
Adjusted R Squared	0.857	0.857	0.857
CUSIP FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Credit Rating × Year-Month FE	✓	✓	✓
Bond Controls	✓	✓	✓