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Voter-induced Municipal Credit Risk*

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Abstract

Policy shocks that increase the costs of public goods should simultaneously reduce voter support for them. We study the extent to which this creates a corresponding voter-induced municipal credit risk. We exploit changes to state and local tax (SALT) deductions from the Tax Cut and Jobs Act as a policy shock to the costs of local public goods. In a difference-in-differences framework with repeated bond trades, we find that jurisdictions with a higher share of residents who ceased deducting SALT (and therefore face a higher marginal cost of local public goods) experienced a significant increase in municipal bond yields, with an average treatment effect of 8.3 basis points, or 3.0% of the unconditional average yield spread. This effect is concentrated in areas where residents have a direct impact on municipal financing decisions via ballot initiatives. This study thus demonstrates the importance of voters in the pricing of public debt and extends the discussion of political and policy uncertainty, and therefore coordination and agency problems, to the municipal context.

JEL classification: G1, G5, H2, H3, H7.
Keywords: Municipal Bond Risk, Voting, Local Public Finance, Tax Cuts and Jobs Act, Principal-agent Coordination.

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Creditors face risk due to their lack of control over borrowers' economic decisions (Jensen and Meckling, 1976). Absent strict contractual protections and assurances regarding their enforcement, this lack of control exposes creditors to the risk that debtors may not repay the loan (Smith Jr and Warner, 1979). In personal loan markets, lenders attempt to control this risk, often associated with "borrowers' character", by using personal credit scoring models that evaluate borrower credit history (Adelino et al., 2016). In the public debt context, this risk manifests via potential changes in voter support, which, either directly or through elected officials, is a prerequisite for public investments, taxation, and financing. Yet, there is little evidence on the extent of this *voter-induced* risk as most studies feature models based on the financial ability of governments to repay their debt (Graham et al., 1962).[1] This void in the literature is particularly surprising, given anecdotal evidence suggesting that residents' support to fund public goods or raise taxes directly affects bondholder payouts.[2] Consistent with these anecdotes, "The Fundamentals of Municipal Finance" (Temel, 2001, page 172) highlights that taxpayer mood is *as important as* the financial ability of issuers.

In light of this gap in the literature, we study how changes in residents' support for public goods impact government credit risk. We motivate our study with the intuition that the quantity demanded for public goods will decline as the cost that residents face to finance them rises. Thus, any shock that increases the cost that residents must pay for public goods should simultaneously reduce support for public investment, including, for instance, tax increases or new bonds to payoff existing debtholders. To study the link between local support for public goods and the cost of municipal debt, we exploit a shock to residents' cost of local public goods that emerges from the change in tax rules embedded in the 2017 Tax Cut and Jobs Act (TCJA), which reduced residents' incentives to deduct state and local taxes (SALT) from their federal taxable incomes.[3] We further

[1]More recent studies on public debt risk based on models of ability to repay debt include Joslin et al. (2014); Andrade et al. (2023); Augustin et al. (2022) and Schwert (2017).

[2]For instance, in June 2015, citizens of Greece rejected the bailout plan laid out by the European Commission and the International Monetary Fund, accentuating the Greek debt crisis. At the local level, the opposition of California's Orange County voters in a local tax referendum in 1995 has left the county with no choice but to file for bankruptcy (Baldassare, 1998).

[3]These SALT deductions occur when a taxpayer itemizes their tax returns instead of opting for the

examine whether any observed relation between this policy-induced shock and municipal credit spreads is concentrated in areas with more voter involvement in the municipal financing process.

To conceptualize the intuition motivating our analysis, we depict in Figure 1 the change in voter preferences for the consumption bundle of public and private goods before and after the shock. Before the enactment of the TCJA, SALT could be deducted from federal income taxes, so the cost of local public goods (p_{pub}) was subsidized for the over 30% of residents who itemized their federal taxes. For them, an additional dollar spent on local public goods costs $p_{pub} \times (1 - \tau)$, where τ is their federal tax rate. Following the TCJA, the number of residents itemizing their deductions dropped by 62.6%. For these residents, the cost of an additional dollar of local public goods rose by $\tau(1 - \tau)^{-1}$, or between 11.1% and 65.5% given 2017 federal tax rates. As a result, voters who no longer deduct SALT should be more likely to vote against new local government spending, and, to the extent that local spending is financed, the repayment risk will increase for existing municipal bonds.

Along with the insights in Temel (2001), this conceptual framework, more formally discussed in section 1.1, links the change in aggregate demand for local goods and services to the risks associated with municipal bonds that are tied to these local provisions. Thus, the first empirical prediction is that a rise in the cost of public goods associated with the decline in SALT deduction will result in higher credit spreads for outstanding bonds since debt payoffs often depend on newly levied taxes or the rolling over of existing debt. In addition, recognizing variation in the degree of residents' political empowerment, the second prediction suggests a differential sensitivity of changes in SALT itemization on credit spreads based on the cross-sectional heterogeneity in state laws governing requirements for voter approval of new issues. Specifically, the theory predicts that a shock to local public goods support will be most relevant in determining the credit risk of outstanding

standard deduction. With marginal tax rates ranging from 10% to 37%, residents who deduct SALT effectively pay 63% to 90% of the dollars paid to local governments. Thus, this fiscal subsidy implicitly lowers the cost of local public goods for those residents who itemize their deductions.

public debt in jurisdictions where residents have more influence over fiscal and bonding decisions. Evidence of such voter-induced credit risk would enrich the literature on the impacts of political and policy uncertainty on asset prices (Kelly et al., 2016; Brogaard et al., 2019; Gao et al., 2019) and corporate investment (Julio and Yook, 2012; Jens, 2017) and relates to the large literature on stakeholder coordination (Coase, 1937; Jensen and Meckling, 1976; Shleifer and Vishny, 1997).

To empirically quantify the importance of residents' support for public investment in determining government credit risk, we exploit the heterogeneous exposure of local jurisdictions to the change in the share of residents deducting SALT following the TCJA. County-level exposure to this decrease in the implicit federal subsidy is substantial and highly variable with a difference in treatment of 16.6 percentage points (p.p.) between the 95th and 5th percentiles. Our proposition relies on the assumption that a reduction in SALT deductions decreases the demand for local public services, with the magnitude of this decrease being proportional to the share of residents no longer deducting SALT (Ambrose and Valentin, 2024a).

In a continuous treatment difference-in-differences framework with bond fixed effects to force within bond comparisons, we find an increase in yields of bonds in jurisdictions with a higher share of treated residents compared to bonds in other jurisdictions. Secondary market bond spreads for the typical county, whose treatment averages 15 p.p., increase by approximately 8 basis points (bps) or 3.0% of the unconditional average yield spread. This magnitude falls within average bond yields response to local economic shocks (Cornaggia et al., 2022b; Goldsmith-Pinkham et al., 2023). We then show that the dynamics with which the effect emerges support a causal effect in determining the relative spreads.

We propose that the municipal bond market reaction to residents' cost of local public goods is driven by uncertainty regarding the approval of future taxes or bond issues. To test this proposition, we examine whether the elevated yield spreads relate to residents' involvement in the municipal financing process by hand-collecting measures that reflect the level of required voter approval for tax changes or municipal bond issuance. We add a triple-interaction term to our main specification and find positive significant coefficients for jurisdictions that require residents to approve local bonds and tax increases. In states where local governments can issue bonds without voter approval, the shock to residents' demand for local public goods does not significantly affect municipal bond yields. Notably, we see no evidence of a trend in the relation between the treatment intensity variable and yields before the TCJA announcement in either subsample.

We also conduct a state-border pair regression analysis focusing on jurisdictions along state borders to control for potential differences in economic conditions between voter approval and non-approval states. The results from this exercise show that the treatment effects are significantly larger for jurisdictions on the more stringent side of the border (i.e., where residents have more local political power), further confirming the role of voter involvement in determining credit risk. Consequently, the combination of results highlights that in a shock to residents' support for local public investment, investors require higher yields to hold bonds issued by jurisdictions whose residents are more politically empowered.

We provide a battery of robustness checks to confirm our findings. We first note that the state-month fixed effects of our main specification absorb the within-state average treatment effect of the TCJA on municipal credit spreads. More generally, our treatment effect is not sensitive to additional controls for other first-order changes induced by the TCJA, such as changes in marginal tax rates or the removal of tax exemption on advance refunding bonds. Our estimated treatment effect is also insensitive to controls for potential second-order effects of the TCJA, such as changes in the current value of the tax base (i.e., housing values) or forward-looking economic indicators (i.e., building permits). The stability of our estimate combined with the high r-squared makes it unlikely that our estimates are significantly biased by omitted variables (Oster, 2019). Our findings are also robust to using the pre-TCJA share of SALT deducters as a measure of treatment and to using entropy-balanced samples in which treated and untreated jurisdictions are similar along dimensions such as income per capita and homeownership rate.

Our study complements the current literature in several ways. First, we expand the discussion of credit risks in the municipal bond market. For instance, Cornaggia et al. (2024) document an average effect of 15 bps on hospital bond spreads caused by telehealth parity law adoptions, Gao et al. (2022) report a 25 bps average effect of Medicaid expansion on rural hospital bond yields, Cornaggia et al. (2022b) report a 17 bps effect of opioid abuse on GO bond offer yields, Cheng et al. (2023) report a 7–11 bps increase in secondary market spreads following the passage of medical marijuana laws, Cornaggia et al. (2018) report 19–33 bps decrease in spreads for upgraded municipal bonds, Painter (2020) estimates a 23 bps increase in yields in response to a 1% increase in flood risk, and Goldsmith-Pinkham et al. (2023) reports an increase of 5.3 bps for a standard deviation increase in exposure to sea-level rise.[4] Our 8.2 bps median effect not only aligns with the literature but also provides direct evidence on how the elimination of SALT deductions impacts municipal financing costs, highlighting residents' support to fund local public goods as a driver of municipal bond prices.

Second, our findings link the municipal credit risk literature to studies on how federal fiscal policies affect local economies (Hanson, 2012; Sommer and Sullivan, 2018; Valentin, 2024), and in turn, the related literature on the effects of investments in public goods for the local economy (Donaldson and Hornbeck, 2016; Baum-Snow et al., 2017; Adelino et al., 2017; Agarwal et al., 2023). Existing studies show that fiscal deductions affect property prices (Hilber and Turner, 2014; Li and Yu, 2022), residents' demand for local public goods (Pevzner et al., 2022; Ambrose and Valentin, 2024a), and the location of economic activity (Albouy, 2009; Coen-Pirani and Sieg, 2019; Fajgelbaum et al., 2019). The only studies, to our knowledge, investigating the effect of SALT on local governments are Feldstein and Metcalf (1987), which shows that jurisdictions whose residents benefit

[4]Because we focus on fiscal policies, our study also relates to research studying the implication of the preferential tax treatment for municipal bonds (Green, 1993; Ang et al., 2010; Longstaff, 2011; Kueng, 2018; Babina et al., 2021; Cortes et al., 2022; Garrett et al., 2023). However, we focus on fiscal subsidies to residents and its impact on municipal credit risk and does not focus on investors' fiscal subsidies emerging from the non-taxation of municipal bond interest.

less from the SALT deductions rely more heavily on business taxes than on deductible taxes, and Holtz-Eakin and Rosen (1990), which shows that they have lower tax rates on deductible taxes. Thus, our study offers evidence of a link between residents' loss of SALT subsidies that emerged from the TCJA and municipalities' costs of finance.

Third, our study contributes novel evidence to the literature connecting voters and local public finance. For example, our results complement Matsusaka (1995) and Matsusaka (2000), who show that voters' involvement in local public decisions impacts local governments' revenues and spending. Yu et al. (2022) use close elections for the approval of municipal bonds as a shock to identify a positive relation between leverage and yields, while Miller (2023) shows that democratization increases financial risk because voters in democracies prefer redistribution policies. We further connect with studies documenting the effect of policy and political uncertainty on asset prices. For instance, Pástor and Veronesi (2012) theoretically demonstrate that despite a positive expected effect on firm profitability, stock prices fall at the announcement of a policy change because of an increase in risk premia, with larger effects during economic downturns (Pástor and Veronesi, 2013). Such policy risk-premium impacts the prices of stocks (Brogaard et al., 2019; Liu et al., 2017), options (Kelly et al., 2016), and municipal bonds (Gao et al., 2019). In contrast to contemporaneous working papers linking municipal bond yields and the political environment (Gao et al., 2019; Dagostino and Nakhmurina, 2023), we leverage a federal-induced shock to residents' preferences to highlight a risk premium that emerges due to a lack of coordination between elected officials and voters.

Thus, our results connect voter approval requirements with municipal credit risk and hence parallel the type of coordination and agency problems often reserved for discussion in corporate contexts. For instance, classic articles that focus on stockholder-bondholder conflicts, such as Fama and Miller (1972), Black and Cox (1976), and Smith Jr and Warner (1979), recognize the impact of coordination problems on corporate debt contracts. More recently and more directly related to our setting, coordination appears in the shareholder voting literature. For example, Gillan and Starks (2000) find that shareholder coordination leads to more successful shareholder proposals, and Crane et al. (2019) finds that coordination among shareholders increases votes against low-quality managerial proposals. Our study extends the impact of coordination to the municipal setting where voters are principals and elected officials are the agents.

The paper is organized as follows. In Section 1, we describe the connection between residents, their demand for local public goods, and the cost of public debt. In Section 2, we explain our data and methodology. In Section 3, we discuss the main results of our policy experiment and provide evidence of voter-induced credit risk. In Section 4, we discuss additional empirical tests to better establish the robustness of our empirical strategy. In Section 5, we conclude.

1 Municipal Finance & Voter Preferences

The extent of voter support for public projects is a significant but understudied risk to municipal bondholders, who rely on cash flows generated from local tax policy and financing decisions. In its "U.S. municipal bond defaults and recoveries" report, covering years from 1970 to 2020, Moody's (2021) discusses several instances of a direct link between voters' approval and local government solvency. For example, Pontiac City School District, MI improved its solvency because of voters' approval: *"In March 2016, voters renewed the district's local operating tax, providing predictability for a core operating revenue and approved a new sinking fund millage that will raise funds for capital expenses and relieve spending pressure on the General Fund"* (Moody's, 2021, page 84). On the other hand, Cardinal Local School District, OH struggled because of a lack of residents' support: *"The district's operations were pressured by enrollment declines, decreased state aid, rising special education expenditures and limited voter support for new operating levies"* (Moody's, 2021, page 88). In some areas, voters even have authority over the existence of the issuing jurisdiction. For example, Dallas County Schools, TX noted that *"[...] if Dallas voters did not vote "yes," the District would be dissolved by September 1, 2018"* (Moody's, 2021, page 92).

Although these anecdotes do not provide clear predictions regarding the effect of voter preferences on municipal credit spreads, they do suggest that a shock to voters' support to finance local public goods represents a risk to municipal bond investors.[5] Furthermore, if elected officials do not perfectly internalize voter preferences, this risk may increase in the extent of voters' empowerment in the financing decision process. If voters no longer support the public goods financed by the bond, they may put up more resistance to tax changes and new financing activities that would either prevent default or facilitate bondholders' recovery from default.

1.1 A Shock to Voters' Support for Local Public Services

We empirically analyze the extent of voter-induced municipal credit risk by exploiting a shock to residents' support for local public goods that emerges from the change in tax rules embedded in the TCJA. In the United States, the tax code offers indirect subsidies to local governments by allowing residents to deduct all taxes paid to lower-level governments from their federal taxable incomes. These SALT deductions occur when a taxpayer itemizes their tax returns instead of opting for the standard deduction. With marginal tax rates ranging from 10% to 37%, residents who deduct SALT effectively pay 63% to 90% of the dollars collected by local governments. Thus, this fiscal subsidy implicitly lowers the cost of local public goods for those residents who itemize their deductions.

In 2018, the passage of the TCJA significantly reduced taxpayers' incentive to itemize their deductions on their tax returns and, in turn, to deduct SALT. Many individuals stopped itemizing their deductions because of the combination of (1) the substantial increase in the standard deduction, (2) the capping of the SALT deduction to \$10,000, and (3) the capping of the mortgage interest deduction to loans below \$750,000 (Ambrose et al., 2022). Consequently, the share of residents who itemize dropped from 31% in 2017

[5] In the absence of a shock to voter preferences, the relation between voter involvement and municipal credit spreads is an empirical question. Jurisdictions with more voter involvement could potentially operate more efficiently (Matsusaka, 2005), possibly leading to lower municipal bond yields. Alternatively, the risk of future shifts in voter preferences may lead to higher municipal credit spreads.

to 11.5% in 2018, which radically increased the net-of-deductions cost of deductible items such as the SALT and contributed to a more than $90 billion increase in implicit federal tax revenue (Department of the Treasury). To further underscore the significance of the shock for local public finances, we note that several states unsuccessfully sued the federal government, arguing that the SALT deduction was essential for maintaining their taxation and fiscal policies (Hutchins, 2018; Erb, 2018).

To conceptualize this point, we depict in Figure 1 the change in the consumption bundle of public and private goods before and after the shock. For a resident who stopped deducting SALT, the net-of-deduction price of public goods increases from $p_{pub} \times (1 - \tau)$ to p_{pub}, where p_{pub} is the price of a unit of local public goods, and τ is the average tax rate on federal taxable income. The cost increase equals $\tau(1 - \tau)^{-1}$ and ranges from 11.1% to 65.5% given the tax rates schedule prevalent in 2017. With a counter-clockwise shift in the budget constraint and under Cobb-Douglas preferences, the consumed bundle of public-private goods moves from point E to E', reflecting a lower demand for local public goods. In contrast, for a resident not impacted by the policy, the consumption bundle remains at point E (always itemizers) or E' (never itemizers).

Aggregating this resident-level shock to a jurisdiction-level shock yields an average 2.7 to 8.0% increase in the cost of public goods, with higher increases in jurisdictions with greater exposure. Assuming a Cobb-Douglas utility function, this cost increase leads to a decrease in demand for local public goods ranging from 3.0 to 9.1%. This, in turn, reduces future support for public project financing, consistent with Ambrose and Valentin (2024a) who show that following the TCJA, California school districts experienced an increase in residents opposing additional local spending as evidenced by their voting against local public goods referendums.

Although the fiscal changes were national, local governments vary in their exposure to this potential risk in two important ways. First, there is substantial variation in the share of residents that change their itemization status across jurisdictions. Second, jurisdictions vary in the extent to which voters can exert influence on the municipal financing process. Putting these two ideas together, to the extent that voter-induced municipal credit risk is economically meaningful, we expect (1) a relative increase in municipal credit spreads following the TCJA in the areas where the largest share of residents stops itemizing their federal taxes and (2) a more significant sensitivity of municipal credit spreads to changes in itemization in areas with the highest levels of voter involvement in the municipal financing process.

1.2 Measuring Jurisdiction Exposure

We measure jurisdictions' exposure to voter-induced credit risk in two stages. First, we measure the extent to which areas are treated with a change in their residents' itemization status in response to the TCJA. Second, we use laws governing voters' participation in the municipal financing process to measure sensitivity to a given treatment.

Our measure of an area's itemization treatment is the difference between the ratios of residents itemizing deductions in 2017 and 2018

$$Chg.Itm_j = \frac{Itemizers_j^{2017}}{Taxpayers_j^{2017}} - \frac{Itemizers_j^{2018}}{Taxpayers_j^{2018}}, \tag{1}$$

where j represents the local government unit. Defining *treated* residents as those who stopped itemizing, this measure is thus equivalent to the share of treated residents in jurisdictions j ($Chg.Itm_j = \frac{1}{n}\sum_{r=1}^{n} Treated_r$). We construct the measure at the state, county, and school district levels using data from the Statistics of Incomes (SOI) of the Internal Revenue Service (IRS). The SOI publishes statistics from household tax returns (Form 1040 and Schedule A) for all zip codes and counties with more than 100 taxpayers. To circumvent potential reverse causality concerns, we also provide results using the share of itemizers pre-TCJA as a proxy to treated residents, which has a correlation of 0.93 with our treatment intensity variable.

Panel (a) of Figure 2 shows our shock intensity variable $Chg.Itm_j$ at the county level, and Tables A1 and A2 show the distribution of $Chg.Itm_j$ at different jurisdiction levels.

The state whose residents are the least impacted is South Dakota ($Chg.Itm = 13.0$p.p.) while the most impacted are residents of Connecticut ($Chg.Itm = 26.6$ p.p.). The variation in $Chg.Itm_j$ increases moving from states to smaller jurisdictions. For instance, at the county level Ohio's Delaware County has the highest decrease ($Chg.Itm = 34.7$ p.p.), while some other counties experienced no change. The median $Chg.Itm_j$ at the county level is 15.1 p.p., and the difference between the 95$^{\text{th}}$ and 5$^{\text{th}}$ percentiles is 16.6 p.p.. We additionally show for each county the change in the share of itemizers from 2016 to 2017, and from 2017 to 2018 in Panel A of Figure A1. We note that there were no significant changes in the share of itemizers across counties between 2016 and 2017 but a substantial reduction in the share of itemizers following the enactment of the TCJA.

To put the impact of a 15-percentage-point change in the share of itemizers into context, we collected local referendum results from the replication files of Kogan et al. (2018) and show in Figure A2 the distribution of winning margins for school district bonds and tax referendums in four U.S. states. We note that 78.6% of bonds fall within 15 percentage points of the passing threshold. Thus, a shift causing 15% of residents to change their support for local public good spending could significantly alter election outcomes, posing a potential risk to municipal bondholders.

The degree to which the risk associated with a shock to public good demand manifests itself might thus depend on the political power residents have to alter local public financing decisions. To shed light on their roles in the pricing of municipal bond risk, we, in a second step, compare our treatment effects across jurisdictions requiring voter approval for local taxes and bond issuance to others that do not. In the spirit of Matsusaka (1995), we separate jurisdictions with voters' empowerment in local public finance compared to representative jurisdictions. Each state constitution has unique features regarding the issuance of debt for state and local government bonds (Kiewiet and Szakaty, 1996), that we classify using various sources. Given the complexity of state constitutions regarding bond, tax, and levy limit rules for various types of jurisdictions, this discrete categorization might be subject to interpretations. We, however, provide the sources that we use for each state in Appendix D and show the map of the variation in that measure in Panel (b) of Figure 2.[6] We note that 13 states and Washington D.C. do not require voter approval, 27 states require a simple majority, while 10 require a supermajority (threshold is set at 55%, 60%, or 66.6%) with several within-state exceptions.

2 Empirical Framework

2.1 The Municipal Bonds Data

We connect GO bond secondary market trades from the Municipal Securities Rulemaking Board (MSRB) to bond characteristics from the Mergent Municipal Fixed Income database. The sample comprises secondary market transactions for bonds issued before the TCJA announcement and traded between January 2015 to December 2019. This short window around the TCJA announcement, which focuses on bonds issued before the shock, mitigates the possibility that uncertainty surrounding the December 2025 expiration of the TCJA provisions could materially affects our results (Gleckman, 2023; Shaw, 2022; Wamhoff et al., 2023). We drop transactions that fall within two weeks of issuance because they correspond to primary market issuance transactions (Schultz, 2012). We also remove transactions for bonds with a remaining time to maturity of less than one year as small price deviations on short maturity bonds can lead to large changes in yield (Schwert, 2017). We compute yields at the bond-month level using the size-weighted average yield across all transactions. We also construct an issuance sample, comprising bonds issued between 2015 to 2019 that represent new borrowing with positive offering yield, amount, and coupon rate (Cornaggia et al., 2022b). For both samples, we focus on tax-exempt municipal bonds and exclude bonds offered via unconventional channels (e.g. limited offerings, private placements, and re-marketing) and bonds issued by U.S. territories.

For each bond issue or trade, we estimate its credit spread using yields on maturity-

[6]To the extent that heterogeneity exists within the state groupings, our simple, discrete classification of states produces a downward measurement error bias in our estimated coefficients.

matched treasury bonds adjusted for the tax-exemption benefits (Spreen and Gerrish, 2022) by recognizing that interest incomes from municipal bonds are exempt from federal taxes and, in most states, from state income taxes. To account for both exemptions, we follow Garrett et al. (2023) and construct the spread of bond i, issued by jurisdiction j, and traded at time t as

$$Spread_{i,j,t} = \frac{Yield_{i,j,t,m}}{(1 - \tau_{j,t})} - r^f_{m,t} \tag{2}$$

where $r^f_{m,t}$ is the yield of treasury bill of maturity m issued at time t and $\tau_{j,t}$ is the marginal tax rate of the marginal municipal bondholder. Specifically, $r^f_{m,t}$ is the time t yield on the 1-, 2-, 3-, 5-, 10-, 20-, or 30-year treasury bond with maturity closest to the remaining maturity on bond i.

To ensure that imperfect maturity matching does not affect our inferences, we also provide results using the spread to maturity-matched yield on the Municipal Market Advisors AAA-rated curve (MMA curve), a tax-exempt benchmark available from Bloomberg since 2001. The marginal tax rate is defined as

$$\tau_{j,t} = \underbrace{\tau^{Federal}_t \left(1 - \tau^{state}_{j,t} \times \mathbb{I}[t < 2018]\right)}_{\text{Fed. tax exemption}} + \underbrace{\tau^{state}_{j,t} \times \mathbb{I}[Exemption^{state}]_{j,t}}_{\text{State tax exemption}}. \tag{3}$$

The first term reflects the tax exemption provided by the federal government where $\tau^{Federal}_t$ is the marginal tax rate on federal incomes that we adjust by $(1 - \tau^{state}_{j,t})$ because state income taxes are deductible on federal tax incomes, and $\mathbb{I}[t < 2018]$ is an indicator for the pre-TCJA period. This latter term becomes one for post-TCJA years because the deductibility of state income taxes is limited for high-income earners following the introduction of the cap on SALT deductions. Thus our spread adjustment captures the changes in investors' tax benefits that emerged from the TCJA: change in marginal tax rates from 39.6% to 37.0% and the elimination of SALT deduction for high-income earners. The second term reflects the income tax exemption at the state level. Because some states do not provide the exemption for their residents, we include the indicator $\mathbb{I}[Exemption^{state}]_{j,t}$ that equals one if a state provides the exemption using the data from Babina et al. (2021). Because municipal bonds are mostly held by high-income taxpayers, we use the highest marginal tax rates on federal and state incomes provided by the TaxSim of the NBER (Feenberg and Coutts, 1993) to calibrate this tax adjustment. We winsorize both spread measures at the 0.5% level to remove outliers of potentially problematic records.

We match each bond to its county using the first six digits of the bond's 9-digit CUSIP that uniquely identifies the issuer. We then match bond data to county-level characteristics using the Federal Information Processing Standard codes. We collect population, employment, and income statistics from the Bureau of Economic Analysis, and labor force participation from the Bureau of Labor Statistics. Unless otherwise stated, we match our fiscal shock measure $Chg.Itm_j$, computed at the county level, with the bond in which the issuer is located.

Table 1 shows the summary statistics of the bond trades normalized by their inverse frequency of trade.[7] The average yield spread over the maturity-matched treasury bond at issuance is 273.9 bps. There is, however, a large variation in spreads with an interquartile range of 274.0 bps. We observe that municipal bonds traded in counties with High $Chg.Itm_j$ have on average 6.4 bps higher tax-adjusted spread, however, this difference is non-significant with county and month levels double-clustered standard errors. These bonds are similar on most bond characteristics (except bond amount and maturity) but come from wealthier and more populated counties with better economic outcomes, with nevertheless no differences in house price and housing permit growths. The within-bond variation in secondary market yields mitigates the impact of these differences on our estimates of interest. However, we also show that our results are robust to using a matched sample that controls for differences in economic conditions.

[7]The weights ensure across bonds summary statistics rather than across trades. For example, if bond A is traded twice at 100 bps and 200 bps, and bond B is traded once at 250 bps; the mean spread reported is 200 bps.

2.2 Within-bond Identification

We test whether a decrease in residents' support for local public goods (i.e., a reduction in the fraction of residents that itemize) affects their jurisdiction's cost of financing. We regress the spread of bond i, issued by a jurisdiction in county j, and traded at time t using the following specification

$$Spread_{i,j,t} = \alpha_{st} + \lambda_i + \delta \left(Post_t \times Chg.Itm_j \right) + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}, \tag{4}$$

where α_{st} are month-by-state fixed effects that absorb time-varying trends in bond yields within states, such as variation in investor demand arising from the issuance of other bonds within the state. λ_i are bond fixed effects that force within-bonds comparison implying that the coefficients on $Post_t$ and $Chg.Itm_j$ are absorbed by these fixed effects.

Our treatment variable is the interaction between the decrease in the share of itemizers between 2017 and 2018 and $Post_t$, an indicator variable equaling one for t in or after July 2017 and zero otherwise. We set the event date to July 2017 to reflect the date at which information about the TCJA's impact on the personal income tax system was likely incorporated into investors' information set.[8] Although the details of the TCJA were finalized in mid-December 2017 (Long, 2017), the provisions related to the change in itemizing rules –including the doubling of the standard deductions– were revealed in the middle of 2017 (Gopal and Light, 2017; Puzzanghera, 2017; Associated Press, 2017).[9]

We follow the latest literature and include in our baseline specification standard control variables shown previously to affect municipal bond yields (Harris and Piwowar, 2006; Gao et al., 2020; Cornaggia et al., 2022b). Specifically, $X_{i,t}$ is a vector of time-varying bond level controls including (1) the maturity-match treasury yields, and (2) the

[8]Although taxpayers may have themselves not incorporated the changes in their voting behavior before filing their 2018 taxes in early 2019 (Ambrose and Valentin, 2024a), we implicitly assume that the financial market can price this source of risk before the enactment (Wagner et al., 2018a).

[9]In Appendix B, we further discuss and show the timeline of TCJA provisions along with the treatment dates from selected studies that use this shock in their empirical strategy. The differences in treatment dates reflect variations in the timing of information releases regarding changes in the corporate versus personal tax codes and the presumed speed at which different economic agents incorporate information.

inverse maturity.[10] To control for local economic factors that could impact municipal bond risk, we also include a vector of lagged county characteristics ($Z_{j,t-1}$) including (1) log population, (2) per capita income, (3) one-year population growth, (4) one-year employment growth, and (5) labor force participation. Finally, some specifications control for an area's house prices and forward-looking economic conditions via the extent of building permit growth. All standard errors are double-clustered at the county and month level to account for potential spatial and temporal correlation in the error structure (Cornaggia et al., 2022b; Gao et al., 2020).

An important identifying assumption is that the intensity of treatment (the decrease in the share of SALT itemizers) is exogenous to trends in municipal bond spreads. Although the bond and state-by-month fixed effects included in most of our analyses help alleviate many concerns regarding unobserved confounders, it is possible that the composition of traded bonds may change for treated and untreated areas after the TCJA in a way that impacts our results. We address this issue in-depth in Appendix C, where we provide evidence alleviating this concern. In short, while some types of bonds, such as insured or negotiated bonds, become relatively less likely to trade, we find little evidence that this shift is related to a jurisdiction's exposure to our itemization treatment (or that this relation to our treatment changes after the TCJA). For instance, as expected, larger bonds are more likely to trade each month, but this increased propensity to trade is not related to our treatment, the post-TCJA period, or their interaction.

2.3 Testing for the Voter's Channel

In the second part of our analysis, we study whether the extent of an area's voter involvement in municipal financing affects the relations between municipal credit spreads and our itemization treatment. To link voters' approval requirements and the effects of a shock to residents' demand for local public goods on municipal yield spreads, we

[10] We start with a vector of bond controls including coupon rate, bond maturity and its inverse, log bond size, the maturity-match treasury yields, and indicator variables for whether the bond is callable, insured, reoffered, negotiated, and four variables denoting the use of proceeds. Those time-invariant (e.g., size, or callable) and perfectly colinear (e.g., maturity) covariates are dropped in the specification with bond and month fixed effects.

include a triple interaction of our main treatment effect ($Chg.Itm_j \times Post_t$) with the voter involvement indicators as well as all identified lower-level double interactions:

$$\begin{aligned} Spread_{i,j,t} = \;& \delta\,(Post_t \times Chg.Itm_j) + \delta^{vote}\,(Post_t \times Chg.Itm_j \times Approval_j) \\ & + \alpha_{st} + \lambda_i + \beta X_{i,t} + \gamma Z_{j,t-1} + \eta(Post_t \times Approval_j) + \varepsilon_{i,j,t}. \end{aligned} \tag{5}$$

where $Approval_j$ is a categorical variable whether voters' approval is required in jurisdiction j. Thus, δ^{vote} captures the incremental effect of a change in residents' support for local public goods for jurisdictions with voters more involved in the local public finance process. A positive δ^{vote} would be consistent with the joint hypothesis that voter-induced credit risk contributes to our estimated treatment effect and that elected officials do not act as a perfect conduit for voter preferences.

3 Results

3.1 Baseline Itemization Treatment Effect

Our main analysis focuses on yield spreads of bonds issued before the TCJA's announcement that trade in the secondary market both before and after the passage of the TCJA. The focus on secondary market trades mitigates the potential selection issue inherent in the primary market analysis. The inclusion of bond fixed effects forces comparison within the same bond and absorbs baseline effects of bond features such as credit quality, maturity, insurance, or callability on spreads.

In Table 2, we report the coefficients δ of Equation (4) while Table A3 shows the full set of coefficients. The first column reports the regression results with only bond and state-by-month fixed effects. The coefficient estimate on the explanatory variable $Post_t \times Chg.Itm_j$ is 67.1 bps, significant at the 5% level. Column (2) shows that the results are qualitatively similar but approximately 15% smaller in magnitude after the inclusion of time varying county-level economic controls, most of which are themselves significant predictors of municipal credit spreads (see Table A3). The inclusion of time-varying controls absorbs the effect of changes in the economic landscape over time. This allows us to isolate the causal effect of the change in the cost of local public goods for residents, but it may absorb certain mechanisms through which the legislation affects credit spreads. For instance, if the the itemization treatment affects local economic growth, which in turn impacts municipal credit spreads, then controlling for economic growth over time will understate the treatment effect.

The coefficient estimate of approximately 55 in column (2) corresponds to an 8.3 bps increase in yield spread for the typical county with a 15 p.p. treatment. This magnitude suggests that the policy shock we study impacts municipal credit spreads in line with the other determinants documented in the literature discussed in the introduction.[11] This yield increase represents a 3.0% increase in the cost of capital. With $145.5 billion of GO bonds issued in 2022 (SIFMA), this effect represents up to $120.4 million of the additional annual cost.

The estimates in columns (2) through (4), as well as in columns (3) through (5) of Table A4, show that aside from the inclusion of time-varying county-level controls, other control variables have little effect on our estimated treatment effect. We obtain similar estimates after controlling for bond characteristics (column (3)), house prices and building permit activity (column (4)), or bond rating fixed effects (column (4) of Appendix Table A4). This stability of our estimated treatment effect helps mitigate the possibility that it is driven by correlated omitted variables, especially given the high R^2s of about 0.93 in our estimations (Altonji et al., 2005; Oster, 2019). For instance, Oster (2019) notes "if the R^2 values from the regression with controls were always very large—say, always close to 1—then the coefficient movements would be enough" (Oster, 2019, page 190).[12]

Of particular relevance is the robustness of our results to controlling for house price

[11] See Cornaggia et al. (2024); Gao et al. (2022); Cornaggia et al. (2022b); Cheng et al. (2023); Cornaggia et al. (2018); Painter (2020); Goldsmith-Pinkham et al. (2023).

[12] The limited applicability of Oster's test in high R^2 settings, such as ours, is also consistent with the list of studies she reviews that depict lower R^2 values and thus are settings where potential unobservable variables can play a critical role in ruling out treatment effects. We, however, report the treatment effect adjusted for unobservable bias for various pairs of regressions in Table A5 and find that the share of variation explained by unobservables needs to be 11.0 to 35.5 times as large as the share of variation explained by observables to reduce the treatment effect to zero.

level and house price growth using the ZHVI data. As we show in Table A6 and as has been previously documented in the literature (Hembre and Dantas, 2022; Li and Yu, 2022; Ambrose and Valentin, 2024b), house prices are related to our treatment. Specifically, our estimates suggest that house prices will exhibit a relative 2% decline for the median zip code with a $Chg.Itm_j$ of 17.6 p.p. To the extent that the effect of the residents' TCJA fiscal shock on municipal bond yields is explained by deteriorating current fundamentals as manifested via house price changes, the coefficient on the interaction $Post_t \times Chg.Itm_j$ should attenuate as these controls are added. In contrast to this, the coefficient $\delta = 56.4$ bps in column (4) is virtually unchanged and remains statistically significant. Moreover, column (4) also adds controls based on housing permit data from the Census Bureau's Building Permit Survey, mitigating the possibility that forward-looking economic conditions are a first-order driver of our results.[13]

Several additional tests support the robustness of the treatment effect we estimate. Column (1) of Table A4 shows that our findings are not sensitive to the specific benchmark from which we compute spreads as we observe a similar percentage decline in spreads over the MMA curve. We also see similar results in Column (2) of Table A4 that reports the results using the share of itemizers in 2017 rather than the change in the ratios of itemizers, which is akin to an intent to treat analysis designed to circumvent potential reverse-causality or simultaneity bias. We also take several steps to ensure that there are no relevant differences in the types of bonds that trade before and after the TCJA that may influence our results. Column (5) of Table 2 does this by reestimating our main results after weighting each observation so that every bond takes on an equal weight in the pre- and post-TCJA periods. Appendix C and specifically Table C1 go a step further and show that there is no significant relation between our treatment measure and either the probability of a bond trading more often post-TCJA or the relative characteristics of the treated and untreated bonds being traded after the TCJA shock.

[13]The results of Table A7 and Table A8, show that the estimated coefficients across a wide range of building permit controls are typically statistically insignificant. Importantly, our treatment effect is unaffected by the inclusion of these controls even in the cases where building permit measures do significantly predict municipal credit spreads.

3.2 Dynamic Illustration

Our identification strategy relies on the assumption that in the pre-period the intensity of treatment, $Chg.Itm_j$, does not predict differential trends in the outcome variable, $Spread_{i,j,t}$. To test this assumption, we replace $\delta Post_t \times Chg.Itm_j$ in Equation (4) with $\sum_{n=1}^{20} \delta^n Quarter_n \times Chg.Itm_j$ where n represents each quarter in the sample period from Q1-2015 to Q4-2019. Thus, we separate the treatment effect into quarter effects where all estimates are relative to Q2-2017, the quarter of the TCJA announcement.

We present the coefficient estimates in Figure 3 and observe that coefficients are not significantly different from zero at the 95% confidence level in the quarters leading to the treatment period. Since these coefficients are estimated relative to the quarter before the announcement, this supports the parallel trends assumption. Five of these pre-period estimates are negative and four are positive, showing no evidence of a linear pre-trend (Roth, 2022). In contrast to the lack of a pre-trend, the coefficients become uniformly positive and significant at least at the 95% level after the event. The effect rises in the three quarters after the policy announcement and then remains steady for the remainder of the sample period.

As with any event study, our coefficient estimates reflect any changes in the market's expectations surrounding TCJA adoption. As discussed in Section 2.2, to the extent that the market had formed expectations regarding the TCJA prior to July 2017 and embedded them into prices, our estimates may not perfectly map to the causal effect of the TCJA treatment. However, the robustness of our results to dropping the quarters around July 2017 shown in Table A9 mitigates the possibility that this is a primary driver of our results. If anything, these results suggest that the estimated treatment effect may be 10% to 15% greater than the effect suggested by our full sample estimates.

We also provide the results of placebo tests using different years for *Post* and a 4-year rolling window of GO bond trades in Table A10. Using our preferred specification with bond-fixed effects, we note that the coefficients on the interaction between $Post_t$ and $Chg.Itm_j$ are non-significant for all placebo samples. These results suggest no evidence of a prior diverging trend that would have impacted jurisdictions that differ in $Chg.Itm_j$ in years before 2015.

The evidence in Figure 3 coupled with the stability of our estimates as controls are added in a high R^2 setting help mitigate the possibility that there is some omitted factor driving our results. In particular, the stability of our estimates in response to the inclusion of house price controls and measures of forward-looking activity highlight that the TCJA shock affects yield spreads through a non-fundamental channel.

3.3 Examining the Voting Channel

Thus far, we have documented that municipal credit spreads respond to increases in residents' costs of public goods. We posit that heightened uncertainty regarding future voter support for public goods spending contributes to this increased credit risk.

Before directly testing this idea by estimating Equation 5, we first descriptively study the differences between municipal bonds in voter approval and non-approval states, prior to the TCJA treatment. Consistent with evidence in Kiewiet and Szakaty (1996) and the idea that voter approval significantly affects the composition of municipal bonds, Table A11 indicates significant differences between bonds in voter approval and non-approval states. Insured bonds represent 33% of the sample in the approval states compared to 20% in the non-approval states. We also observe that they carry higher yields, have lower ratings, longer maturity, and are issued by less affluent counties.[14] Notably, voter approval and non-approval states are similar with respect to the treatment variable $Chg.Itm_j$.

In Table 3 we present the results from estimating Equation 5, using the classifications of jurisdictions' voter involvement depicted in panel (b) of Figure 2. The first two columns

[14]Using municipal bonds issued before July 2017 to avoid TCJA's confounds, Table A12 displays regression results that corroborate the yield difference between bonds in voter approval and non-approval states. Because we cannot include spatial fixed effects to identify the $Approval_j$ variable, the inferences we make here can only be suggestive. After conditioning on bond characteristics and month of issuance fixed effects, we observe that bonds in jurisdictions with approval requirements carry a bond premium of 7.2 bps (significant at the 1% level). Splitting $Approval_j$ into indicators for majority and non-majority states, we observe an additional 4.5 bps and 18.2 bps yields, respectively, compared to jurisdictions with no voter requirement.

report the results of our test by separating the jurisdictions based on whether voters are required to approve local spending and bonds. The coefficient on the $Post_t \times Chg.Itm_j$ is positive and significant only in jurisdictions whose voters are involved in the local public finance process. In states where voters have a say in the level of taxation, the results of Column (2) imply a spread increase of 9.7 bps at the county average treatment intensity. In contrast, the treatment effect in states with no voter approval is virtually zero.

In the next two columns, we use the entire sample of trades and add triple interactions between the level of voter involvement and our measure of residents' fiscal shock as depicted in Equation (5). The coefficient on the triple interaction is positive and significant compared to the non-significant coefficient on $Post_t \times Chg.Itm_j$. The result is robust to the inclusion of bond weights showing an estimate δ^{vote} of 99.7 bps (Column [4]). This result suggests that the risk premium associated with a change in residents' support for local public goods is more pronounced when voters are more involved in the municipal financing process.

In the last two columns, we further split the approval variable into majority and supermajority jurisdictions that we interact with our main treatment variable. We observe that the effect is monotonically increasing as we move toward states where voters have more power in the level of local taxation. The coefficients in Column (5) suggest that for the average impacted county, the municipal bond yields increase by 7.7 bps and 25.3 bps for jurisdictions with majority and supermajority requirements, respectively. The results with regression weights (Column [6]) consistently show increasing coefficients confirming that investors require higher yields for holding bonds issued by jurisdictions with higher levels of residents' voting power in local public finance.

We also provide in Figure 4 the treatment effect dynamics separately for bonds issued in jurisdictions requiring voters' approval or not. We observe that the δ coefficients leading up to the second quarter of 2017 are not significant in either jurisdiction type. However, after that, the coefficients for *Approval* jurisdictions are positive and significant while the coefficients for non-approval jurisdictions remain non-significant. These results point toward an increase in municipal bond yields when residents have experienced an increased cost for financing local public goods and are involved in the process of determining taxes and bond issuance.

3.4 A Border Analysis

To refine the interpretation of the state-level tests and to control for potential differences between states that require different levels of voter support, we employ a state-border pair research design. For this analysis, we consider a state-border pair as a unit of observation and focus on units that have differential voter involvement in municipal policies on each side of the border. Our sample consists of 21 state-border pairs where one side has no election and the other has either majority or supermajority election requirements. An additional 25 state-border pairs have majority election requirements on one side of the border and supermajority requirements on the other. Figure A3 provides the map of the counties forming the units of analysis. We define $Treated_j$ as bonds issued in counties with more stringent voter approval rules. We regress this variable interacted with $Post_t \times Chg.Itm_j$ on $Spread_{i,j,t}$ to test whether our main effect is more pronounced on the treated side.

Ideally, the economy on either side of a state border should be equivalent. However, this requirement will not always be met as there is some noise in our ability to perfectly match economies even at the state-border level. To alleviate this issue we include, in addition to the standard state-by-month fixed effects, border-by-period fixed effects, which absorb any effects that are common to the local economy within a state-border pair over the pre- and post-periods. For instance, this absorbs potential confounding effects related to the enactment of the TCJA at the local level.

We report the results in Table 4. In the first column, we show the baseline results using all border pairs. The coefficient δ representing the main treatment effect is non-significant while the coefficient δ^{vote} is positive and significant at the 10% level. Its magnitude indicates that on the side of the border that has more stringent approval rules, every one percentage point increase in treatment intensity increases bond yields by 0.87 bps in comparison to the yields of bonds issued on the other side of the border. We also note that adding weights to the regression to ensure that each bond has the same weights in the pre and post-period (Column [2]) increases the coefficient δ^{vote} to 111.22 bps, significant at the 10% level.

In the last two columns, we restrict the sample to units that include one side being a "no ballot" state, thus exacerbating the difference in voter requirement captured by the $Treated_j$ indicator. As expected, the magnitude of the coefficients increases, further confirming the role of voters' involvement in determining municipal bond risk. The result of this border test thus confirms that compared to pure representative jurisdictions, jurisdictions that must seek voter approval for spending experienced an increase in bond yields following the shock to residents' support for local public goods.

3.5 Threats to Identification

We argue that our empirical design plausibly isolates voter-induced credit risk from other more widespread effects of the TCJA. As discussed in Section 1, we accomplish this in two ways. First, we note that variation in jurisdictions' post-TCJA change in the share of itemizers primarily represents a shock to residents' cost of local public goods. Second, we exploit cross-sectional variation in an area's exposure to this shock using data on voters' involvement in the municipal financing process. In this section, we discuss other possible interpretations of our measure and explain why these alternative interpretations are unlikely to have empirical relevance in the context of our study.

3.5.1 Potential Confounding Effects Linked to the TCJA

It is important to acknowledge that the TCJA included a variety of other provisions that may affect municipal credit spreads. These provisions include the removal of the tax exemption on advanced refunding municipal bonds, reductions of personal income tax rates, adjustments to the Alternative Minimum Tax, and the reduction in corporate tax rates. We note that the correlation between a jurisdiction's exposure to our treatment variable $Chg.Itm_j$ and their exposures to these other provisions could affect the interpretation of our results. For example, these effects likely led to increased disposable income since many taxpayers experienced a decrease in tax liability. We show in Figure A4 the change in tax liability as a percentage of household income using the estimates in Table (4) of Ambrose et al. (2022). The income effect averages 1.5-2.0% of gross income, and is, if anything, limited in comparison to the public goods' cost change of $\tau(1-\tau)^{-1}$. Thus, any income effect is likely outweighed by the decrease in demand for public goods resulting from the removal of SALT deductions.[15] Importantly, the concurrent changes to the tax environment are for the most part unrelated to our itemization shock, and thus unlikely to substantially impact our interpretation of the estimated treatment effect. Nevertheless, there are some connections between the change in itemization and other aspects of the TCJA, which we elaborate on below.

First, we examine potential endogeneity issues regarding the change in marginal tax rates. The change in tax rates might be a threat to our identification if investors who benefited the most also live in jurisdictions that score high on our treatment variable $Chg.Itm_j$. This is a potential threat because municipal bonds are tax-exempt and a decrease in investors' marginal tax rates increases bond yields (Babina et al., 2021; Garrett et al., 2023). Although the tax adjustment outlined in Equation (3) accounts for the tax exemption based on top-marginal tax rates and state deduction rules, this confound could emerge to the extent that investors invest more in their home counties relative to other counties in their own state, given the inclusion of state-year fixed effects. To formally address this concern, we compute the change in the mean tax rates in each county by dividing aggregate income taxes by taxable income and note a correlation between the change in average tax rates and $Chg.Itm_j$ measure of 0.08. In the first column of Table 5, we interact this measure with $Post_t$ and note a non-significant coefficient. We then add our main treatment variable to verify whether the changes in marginal tax rates attenuate our effects. We find that the interaction between change in tax rates and $Post_t$ is non-

[15] We theoretically illustrate the possibility of a change in income in combination with a counterclockwise shift in the budget constraint in Figure A5.

significant while the coefficients on the main treatment remain positive and significant.[16] To correct for possible measurement bias resulting from the use of average tax rates rather than marginal ones, we reproduce this analysis using mean tax rate changes for households earning more than $100,000 annual income, the likely bond investors. The non-tabulated results depict similar results.

Second, the TCJA eliminated the tax exemption on advance-refunding bonds. This change could affect our results if jurisdictions heavily reliant on these bonds are in essence the same as the jurisdictions impacted by the $Chg.Itm_j$ shock. To alleviate this potential confound, we construct a county-level variable that captures each county's historical reliance on advanced refunding bonds. This measure is constructed by dividing the total amount of advance-refunding bonds issued from 2005 to 2016 by the total amount of GO bonds. By using this historical measure, we implicitly assume that issuers' additional cost of debt related to the elimination of this tax subsidy correlates with its historical use of these bonds. This measure averages 11.6% with large variations across counties (standard deviation = 13.9%). In Columns (3) of Table 5, we replace our shock variable with this reliance on the use of advanced-refunding bonds variable and find non-significant results. When we add our main explanatory variables in Column (4), we note that the risk premium associated with the change in the share of itemizers does not change.[17]

Third, we investigate whether the $10,000 cap on SALT deductions further explains our main results. Because residents who continue deducting SALT are subject to the cap, their marginal benefit is zero for the SALT amount above the cap. This zero-marginal benefit for non-treated residents is not directly captured by our variable $Chg.Itm$; although it is captured by the share of itemizers pre-TCJA with results shown in Column (2) of Table 5. We construct an additional variable, $Wasted.SALT_j$, defined as the dollar amount of SALT that could not be deducted because of the cap normalized by the

[16]The results presented in Table A13 also confirm that the positive coefficient on the triple interaction with $Approval_j$ holds conditional on these TCJA-confound changes.

[17]Ang et al. (2017) show that financially constrained municipalities use tax-exempt bonds to circumvent cash-flow issues. Thus, the removal of the tax exemption on advance refunding municipal bonds should impact financially constrained municipalities more intensively. In non-tabulated results, we add proxies for county-level "financial constraint" interacted with $Post$ and note non-significant results either.

number of tax returns in county j. In Column (5), we report the results of our main specification using this measure interacted with $Post_t$ as an alternative treatment variable. The positive and significant coefficient provides evidence that a larger wasted SALT deduction increases bond yields in a way that parallels our main proposition. At the mean of this variable ($1,858), it indicates an increase in bond yields of 1.49 bps. In the next column, we add our primary interaction of $Chg.Itm_j \times Post_t$ and observe that our main effect is robust to the inclusion of the $Wasted.SALT_j$ variable with the significance and magnitude remaining similar to our main results. Although there might be an additional effect due to the cap on SALT deductions, we conclude that the primary results of our study dominate and are not linked to the intensive margin change emerging from the cap.

Finally, we examine whether the electoral support for President Trump confounds our main finding. We collect the share of votes for Trump in the 2016 presidential election from MIT Election Data and Science Lab (2018) and note a negative correlation of 0.36 with our treatment intensity variable. Thus, the threat to our proposition could arise if investors would perceive jurisdictions that voted more intensively for the sitting president as less risky around the passage of the TCJA, thus explaining the higher yields for jurisdictions with high $Chg.Itm_j$. We interact the share of votes for Trump with the *Post* indicator and show in Column (7) the results of our main specification using this additional potentially confounding treatment. We observe that, if anything, places with higher votes for Trump in 2016 exhibited lower bond yields after the passage of the TCJA. For the average county with an average vote share for Trump of 42.5%, it would imply a decrease in bond yields of 4.77 bps. However, we observe no significant differences between our main effects and the effects depicted with this additional treatment included.

3.5.2 Correlation with demographic characteristics

We also recognize that our treatment, which is the post-TCJA change in itemizers, is related to demographic characteristics such as income, house prices, and homeownership rates. However, because these relations are non-monotonic, it provides residual variations in our treatment variable after such controls.[18] As we discuss in Appendix C, even after accounting for these controls and state fixed effects, we find similar results exploiting the remaining 15.7% of the variation in $Chg.Itm_j$. Examples of residual treatment are non-linearities in the income distribution, the extent of family-based deductions, charitable giving, etc. Nevertheless, to mitigate the possibility that these differences affect the interpretation of our treatment, we show in this section the robustness of our results to using an entropy-balanced weighted sample in which areas with high and low itemization rates are similar along other observable dimensions.

Because jurisdictions with a large share of treated residents differ on socio-economic dimensions from jurisdictions that are less impacted, unobserved characteristics could bias our continuous treatment difference-in-differences estimates despite the inclusion of bond fixed effects and county-level demographic controls. To achieve covariate balances across groups, we employ an entropy balancing procedure to weight jurisdictions based on their pre-TCJA socio-economic variables (Hainmueller, 2012), a methodology used in several studies in economics and finance (Arifin et al., 2020; Guriev et al., 2021; Hasan et al., 2021; Colak and Öztekin, 2021; Pan et al., 2022).

We define the treated group as bonds issued by jurisdictions with $Chg.Itm_j$ greater than the national 19.5 p.p. shock. We then implement the entropy balancing procedure to match the bond-level pre-TCJA mean values for spread and for county-level median income per capita and homeownership rates. We use income and homeownership rates because they are the largest predictors of itemizers. The entropy balancing procedure produces weights for untreated units creating a balanced sample mimicking the moments of the treated groups, therefore removing the significant differences in the socio-economic characteristics of the two groups. We present the summary statistics for each group weighted by the entropy balancing weights in Table A14. With the weights, jurisdictions with high and low shares of treated residents have similar income levels, population,

[18] For instance, we show in Panel B of Figure A1 that our measure affects households across the income distribution in a non-linear manner. The shock is most substantial within the $100,000-$200,000 income bracket, and it decreases within the top income group, which contains the typical municipal bondholder (Longstaff, 2011; Babina et al., 2021).

employment, and labor participation growth rates.

We then estimate our preferred regression specifications using the entropy balancing weights that correct for differences in pre-TCJA covariates and report the results in Table 6. The first two columns report the results using the interaction between the treated units indicator and $Post_j$ as the procedure balances covariates based on that measure. The coefficient on the interaction term with $Post_t$, replicating the main policy-experiment study, is positive and significant at the 5% level at 4.66. The triple interaction with $Approval_j$ also shows a consistent positive estimate indicating higher credit risk in jurisdictions with politically implicated residents. The last two columns show the estimated coefficients using the continuous treatment intensity variable. The sign, magnitude, and significance of the coefficients are consistent with the main results.

4 Additional Analyses

4.1 Heterogeneity by Debt Maturity and Credit Rating

In Table A15, we interact our main treatment intensity variable with an indicator for pre-TCJA low rating (Columns [1]) and with long-maturity bonds (Columns [3]). Consistent with the proposition that the effects manifest themselves more intensively in bonds that have some initial risk level, we find that the residents' shock is more pronounced in low-rating and long-maturity bonds. The maturity result suggests that the shock is likely a long-term risk whereby investors price the possibility that public support will change enough (nudged along by this shift in support for public good spending) to induce default in otherwise very safe assets. Not surprisingly, our results also concentrate in lower-rated bonds where there is a more meaningful probability of default, however even in these bonds we see no significant concentration of the effect in shorter maturity bonds (Column [5]). This result is also consistent with Pástor and Veronesi (2013), who find that policy-induced risk is greater during worse economic conditions. In Columns (2) and (4), we replicate these regressions focusing on the robustness of our voting results using preferred specifications (Column [3]) of Table 3. The similar estimates after the inclusion of interactions between our treatment measures and maturity and ratings mitigate the possibility that our voting results are in part driven by differences in the underlying bonds' maturity or credit quality in voting and non-voting states. Overall, the voting results are independent of the main differences in bond types across voting and non-voting states.

4.2 Credit-Risk Premium at Issuance

We next examine the impact of the residents' fiscal shock on the cost of capital in the primary issuance market. The results from this analysis provide a direct estimate of issuance cost, conditional on a municipality issuing bonds given the prices they face. With that said, there are a few caveats. First, we cannot include bond fixed effects to absorb any unobserved features of municipal bonds. Thus, we replace in the most stringent specification the bond fixed effects with issuer fixed effects using the first 6-digits of the CUSIP to force comparison within the same issuer. Second, the composition of new issuers may endogenously change with that change being correlated with our residents' fiscal shock measure. For example, the most adversely affected jurisdictions could choose not to issue in the post-period due to the greater borrowing costs. However, we look at the change in issuance amount and issue probability by county every month and we do not find that the issuance behavior changed significantly after the shock in such a short time window. The lack of quantity effects aligns with the extended period observed between the decision to issue GO bonds and their issuance (Adelino et al., 2023).

The results are presented in Table 7. In Column (1), after controlling for bond and county-level characteristics and including month and county fixed effects, we note that the coefficient on the interaction between $Chg.Itm_j$ and $Post_t$ is positive and significant at the 1% level. Adding issuer fixed effects (Column [2]), or controlling for the frequency of issuance (Column [3]) depict similar estimates. At the average treatment, these coefficients relate to an additional spread of 23.8-25.0 bps. In Columns (4) and (5), we report the results including the triple interaction with $Approval_j$, the specification analog to the results shown in Table 3. The coefficients on the interaction are positive and significant at the 10% level, indicating an increased risk premium also at issuance.

4.3 Revenue Bonds

We next reproduce our analysis using revenue bonds, with the results presented in Table A16. The coefficients on the treatment variable $Post_t \times Chg.Itm_j$ are non-significant throughout the different specifications which we attribute to two reasons. First, these non-significant results confirm that the change in $Chg.Itm_j$ has a minimal fundamental impact on the local economy. Because the connection between residents and revenue bond payments is slim, these results reinforce the shock to local public good cost channel, as opposed to the potential economic-fundamental channel which appears insignificant.

Second, these non-significant coefficients further reinforce the voting channel because, unlike GO bonds, revenue bonds do not require voter approval, except for some state-level bonds that are not in our sample.[19] Thus, these non-significant results for revenue bonds are consistent with the voting results of Column (1) Table 3 when we subset to jurisdictions with no voting requirement.

4.4 Jurisdiction Types

Finally, in this section, we study whether the bond yield responses to the decreased demand for local public goods extend to states or school districts. State bonds are not in our main sample and are typically funded through non-property tax sources. Thus, evidence of effects in that sample would cast doubt on the already unlikely simple property value pass-through explanation. The first two columns of Table A17 present results using GO bonds issued by states with $Chg.Itm_j$ computed at the state level. We find large and significant estimates on the interaction between $Post_t \times Chg.Itm_j$. The coefficient δ varies from 191.9 to 247.7 bps. The lower variation of $Chg.Itm_j$ at the state level (standard deviation of 0.031) explains a larger magnitude of the estimates. The magnitude implies

[19] See the Tax Policy Center report "What are municipal bonds and how are they used?" for details on the differences between revenue bonds and GO bonds.

that bonds issued by Connecticut, the state the most impacted residents' loss of SALT deductions, experience a yield increase of 26.1-33.7 bps compared to the bonds issued by South Dakota (the least impacted state); likely explaining the lower support for the TCJA in the high-income and high-taxed states (Hutchins, 2018; Senator Chuck Schumer Newsroom, 2021; Becker, 2021). Because states do not use property taxes in financing their public goods, this result also highlights that housing price shock is likely not the main channel.

In the last two columns, we show the results of school district bonds that we associate with $Chg.Itm_j$ computed at the school district level.[20] The results using GO bonds issued by school districts show a smaller magnitude than the main results of Table 2. With the bond fixed effects, we observe a positive and significant estimate of 17.5 bps (Column [3]), significant at the 1% level. The positive effect persists after weighting the observations by the number of pre-TCJA trades (Columns [4]) with a coefficient of 16.6 bps also significant at the 1% level. The smaller magnitude can be explained in part by the higher variation in $Chg.Itm_j$ within school districts and in another part, likely by the lower willingness to cut spending on schooling compared to other types of local public spending.

Although all taxes are deductible conditional on itemizing deductions, these results provide evidence that the effects are not driven by a specific fiscal instrument but rather by the decrease in residents' support for local public goods due to the TCJA shock on federal tax deductions.

5 Conclusion

We use the change in residents' use of the SALT deductions, which increased the net cost for financing local public goods, to quantify the impact of resident-voters on the determination of municipal credit risk. Consistent with the proposition that changes to

[20]To compute the school district level measure of $Chg.Itm_j$, we crosswalk the data provided at the zip code level onto school districts using the School District Geographic Reference Files provided by the U.S. Census Bureau's Education Demographic and Geographic Estimates (EDGE) program on behalf of the U.S. Department of Education's National Center for Education Statistics (NCES).

residents' demand for local public services impact municipal bond credit risk premia, we show that a decrease in the share of residents deducting SALT results in a greater cost of finance for local governments. Our preferred secondary market estimates imply that for the average jurisdiction, which experienced a 15 p.p. decrease in the share of residents deducting SALT, the cost of financing increased by 7 to 10 bps, equivalent to a 2.7-to-3.7% rise in the cost of debt. The SALT deduction, which allows wealthier residents to benefit from less expensive local public services, thus additionally favors more affluent jurisdictions with a lower cost of debt.

We then investigate heterogeneous effects based on differences in residents' political influence over local fiscal and bonding policies. When separating jurisdictions based on the level of residents' required approval for the issuance of local bonds, we show that bond yields issued by jurisdictions whose residents are more politically empowered react more intensively to credit risk shocks. This result reveals a voter-induced premium in asset pricing and thus underscores coordination challenges in local public finance, complementing our understanding of the economics of governments.

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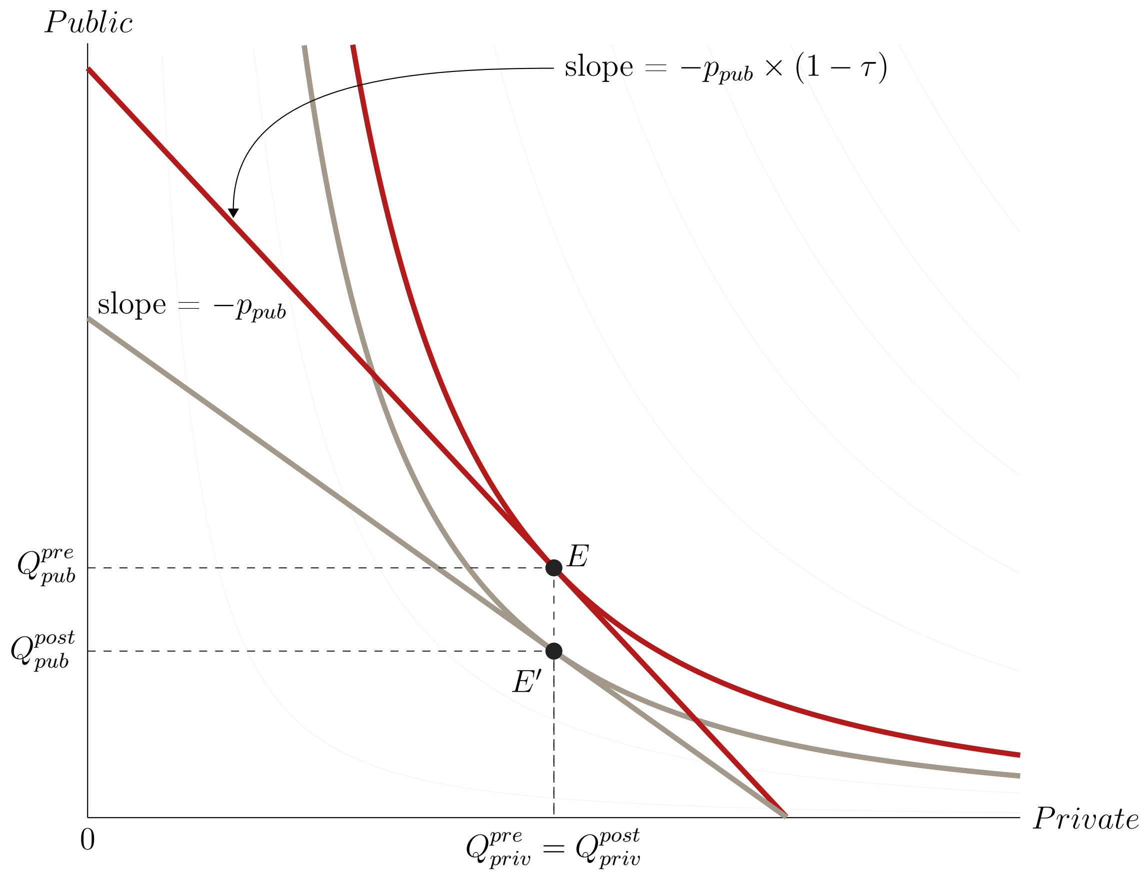
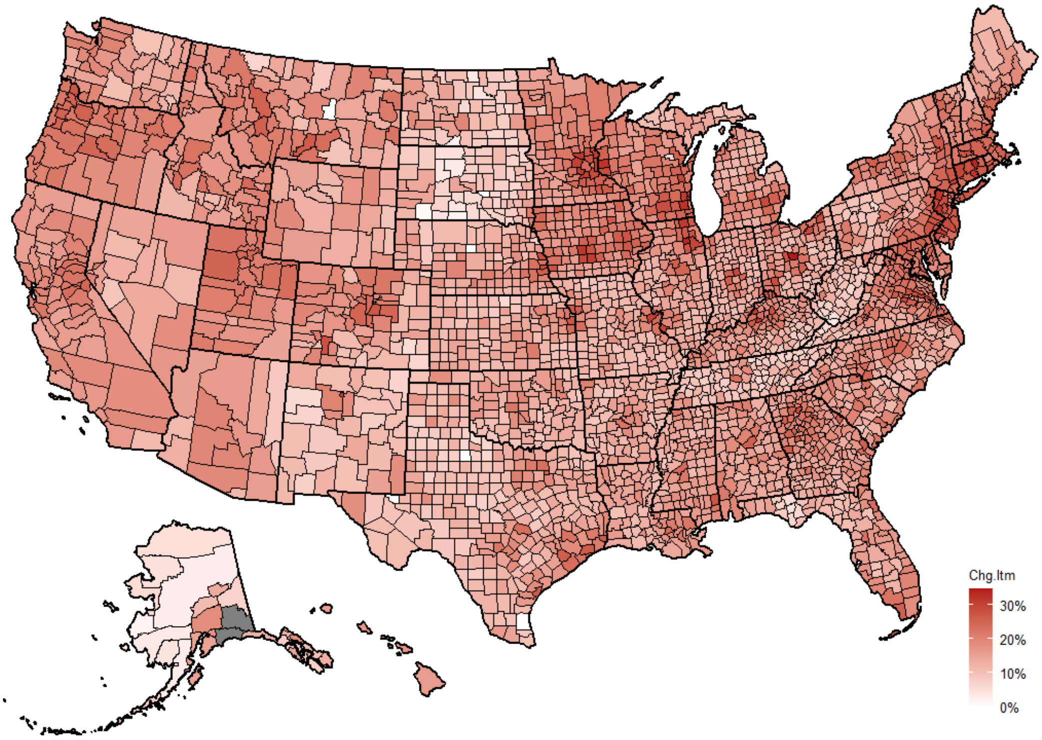
Figure 1: Stopping deducting SALT and the demand for local public goods

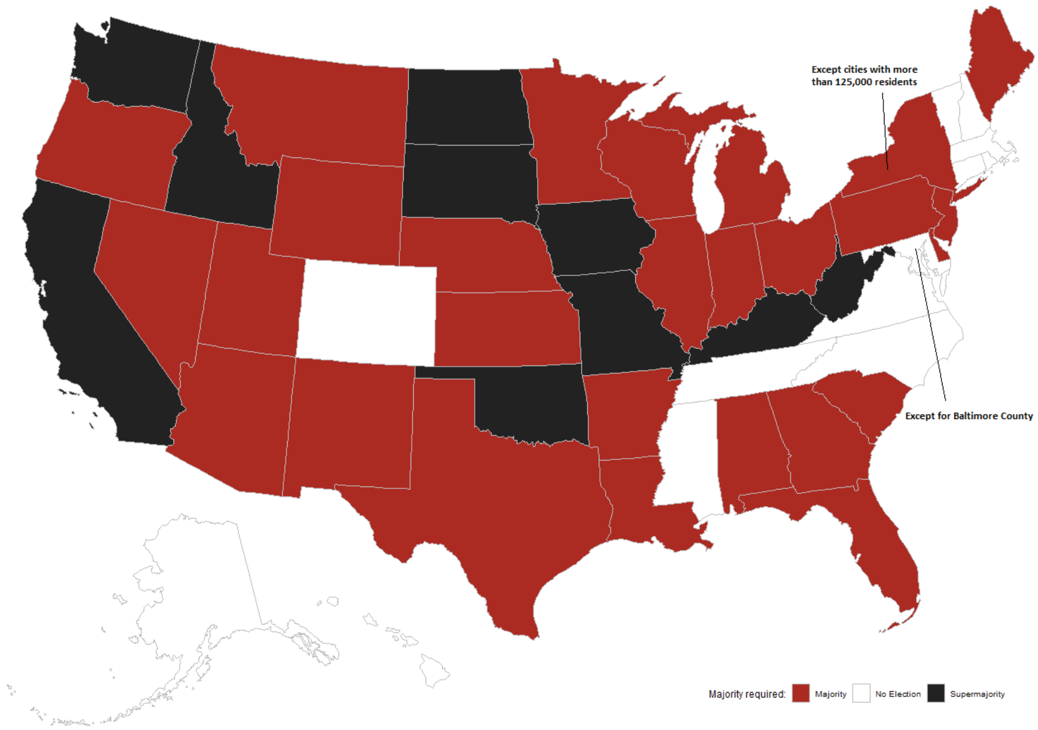


Note: This chart theoretically shows the demand for local public goods for a resident who stopped deducting their State and Local Taxes (SALT) from their federal taxable income (treated resident). The utility is over public and private goods by a Cobb-Douglas utility function. p_{pub} represents the price of a unit of public goods, and τ is the average tax rate on federal incomes.

Figure 2: Spatial variations in treatment exposure



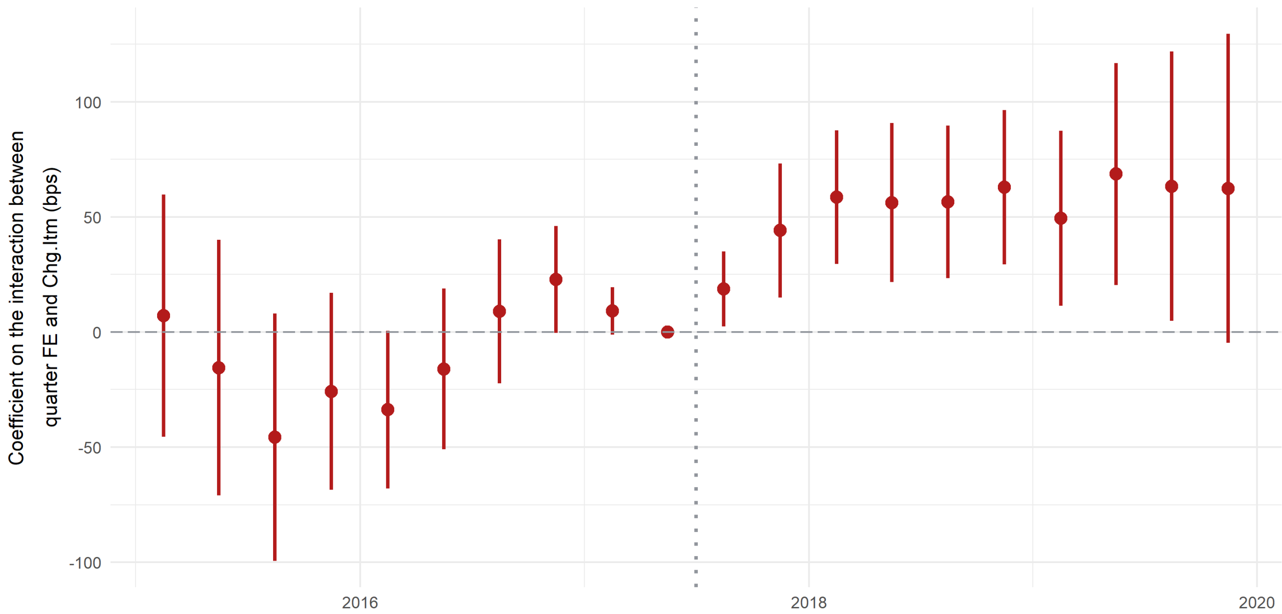
(a) Change in share of itemizers by county



(b) Residents' majority required for bond and tax referendums

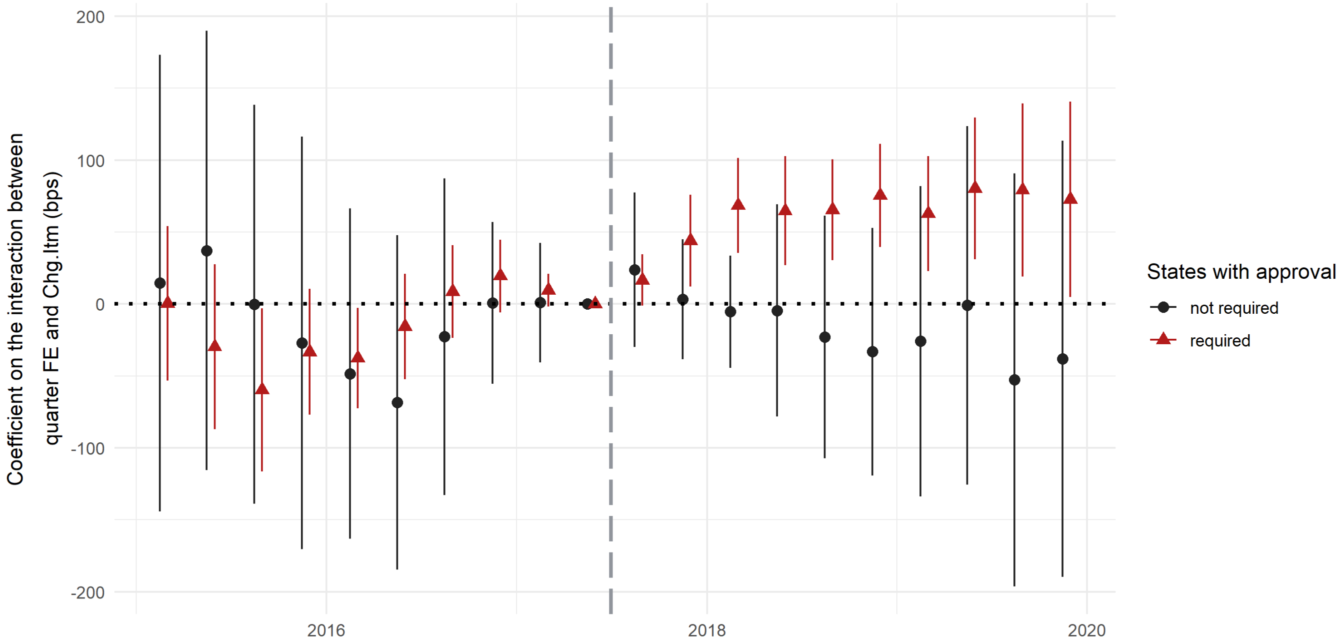
Note: Panel (a) shows the geographical distribution of the decrease in the share of itemizers by U.S. county from 2017 (pre-TCJA) to 2018 (post-TCJA). Computation from the Statistics of Incomes of the IRS. Panel (b) shows the required majority for the approval of local bonds and tax increases. Supermajority is defined as a passing threshold greater than 50%. The detailed information for each state is provided in Table D1.

Figure 3: Dynamic effects of residents' demand shock on municipal bond yields



Note: This graph shows the coefficient estimates on the interaction between quarter fixed effects and Chg.Itm using all tax-exempt GO bonds issued before July 2017 and traded from 2015 to 2019. The dependent variable is the tax-adjusted spread over treasury yields. The regressions include state-by-month and bond fixed effects, bond characteristics, and lagged county demographics. The vertical bar shows the announcement of the TCJA. The error bars show the 95% confidence interval using standard errors double-clustered at the county and trading month levels.

Figure 4: Dynamics effects by residents' political involvement in local public finance



Note: These graphs show the coefficients estimate on the interaction between quarter fixed effects and Chg.Itm using all tax-exempt GO bonds issued before July 2017 and traded from 2015 to 2019. The dependent variable is the tax-adjusted spread over treasury yields. The regressions include state-by-month and bond fixed effects, bond characteristics, and lagged county demographics. The black dots show the treatment effects for jurisdictions that do not require residents' approval and the red triangles are estimated using trades of bonds issued by jurisdictions that require residents' approval. The vertical bar shows the announcement of the TCJA. The error bars show the 95% confidence interval using standard errors double-clustered at the county and trading month levels.

Table 1: Summary statistics of the municipal bonds trades

*This table reports the summary statistics of the characteristics of the bonds traded before the TCJA shock (n = 831,288). The sample consists of tax-exempt GO bonds issued by local jurisdictions except states. All statistics are weighted by the inverse of the frequency of trades so that each of the 266,107 bonds carries the same weight. Spread is the tax-adjusted spread over the maturity-matched treasury yield, spread MMA is the maturity-matched yield on the Municipal Market Advisors AAA-rated curve, and Chg.Itm is the decrease in the ratio of itemizers in the issuer's county. Month-level house values are collected from Zillow ZHVI, and the growth is year-to-year annual housing price growth. The housing permit growth is the cumulative 3-year-horizon growth of housing permits collected from the Census Bureau's Building Permit Survey. The data is split between municipal bonds that were issued in counties with high or low Chg.Itm (below or above the county median of 15.1 percentage points). The means for the two groups are presented in Columns (4) and (5). The difference in means along the t-statistics computed via OLS with double-clustered standard errors at the county and trade month levels are shown in the last two columns. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Mean	Std. dev.	Median	High *Chg.Itm*	Low *Chg.Itm*	Difference	t-statistics
Main variables:							
Spread (bps)	273.90	168.10	242.00	274.67	268.30	6.37	1.22
Spread MMA (bps)	92.83	80.80	68.02	92.82	92.86	−0.04	−0.02
Chg.Itm (%)	0.21	0.05	0.21	0.22	0.13	0.09	26.59***
Bond-level control variables:							
Rating (notch)	18.33	1.95	18.50	18.34	18.27	0.06	0.40
Coupon (%)	3.60	1.31	3.98	3.60	3.57	0.03	0.52
Maturity (years)	8.15	5.73	6.88	8.06	8.77	−0.70	−4.22***
Amount (000s)	2,396.37	8,588.05	1,004.63	2,454.03	1,974.16	479.86	1.41
Callable	0.57	0.50	0.50	0.56	0.62	−0.06	−5.59***
Insured	0.31	0.46	0	0.31	0.30	0.01	0.36
Reoffer	0.15	0.35	0	0.15	0.14	0.01	0.73
Negotiated	0.38	0.48	0	0.38	0.34	0.05	1.66
County-level control variables:							
Income per capita (000s)	52.59	17.30	48.97	54.46	38.90	15.57	8.38***
Population growth (%)	0.01	0.01	0.01	0.01	0.004	0.005	2.95***
Employment growth (%)	0.02	0.02	0.02	0.02	0.01	0.01	3.89***
Labor participation (%)	0.75	0.06	0.75	0.76	0.71	0.05	9.62***
House Value (log)	12.36	0.59	12.29	12.44	11.73	0.71	10.86***
House value growth (%)	4.57	3.51	4.34	4.51	5.06	−0.56	−1.06
Housing permit growth (%)	0.52	1.76	0.42	0.51	0.55	−0.04	−0.36

Table 2: Local fiscal shock to residents and municipal bonds spreads

This table reports the estimates of $Spread_{i,j,t} = \alpha_{st} + \alpha_i + \delta\ Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county* j *and traded at month* t, $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j, $Post_t$ *equals 1 for bonds traded after July 2017,* α_{st} *are state-by-month fixed effects,* α_i *are bond fixed effects,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics. The coefficient estimates for the control variables are shown in Table A3. All trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. In Columns (4) and (5), regressions include the log of house value, the annual housing price growth, and the 3-year-horizon growth of housing permits. In Column (5), the post-TCJA trades are weighted by the number of trades for the same bond observed before the TCJA. Standard errors in parentheses are double-clustered at the county and month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)				
	(1)	(2)	(3)	(4)	(5)
$Post_t \times Chg.Itm_j$	67.10**	55.06***	54.73***	56.40***	48.03**
	(27.53)	(19.19)	(19.16)	(19.76)	(21.81)
Bond FE	X	X	X	X	X
State x Month FE	X	X	X	X	X
County-level control		X	X	X	X
Time-varying bond control			X	X	X
Housing price controls				X	X
Weighted trades					X
Observations	1,641,662	1,641,662	1,641,662	1,622,649	1,622,649
R^2	0.93	0.93	0.93	0.93	0.93
Adjusted R^2	0.92	0.92	0.92	0.92	0.92

Table 3: Residents' political involvement, fiscal shock, and municipal bond yields

This table reports the estimates of $Spread_{i,j,t} = \delta(Post_t \times Chg.Itm_j) + \delta^{vote}(Post_t \times Chg.Itm_j \times Approval_j) + \alpha_{st} + \alpha_i + \beta X_{i,t} + \gamma Z_{j,t-1} + \eta(Post_t \times Approval_j) + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county* j *and traded at month* t, $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j, $Post_t$ *equals 1 for bonds traded after July 2017,* $Approval_j$ *is the degree of residents' involvement in the local public finance process,* α_{st} *are state-by-month fixed effects,* α_i *are bond fixed effects,* $X_{i,t}$ *are bond level controls, and* $Z_{j,t-1}$ *are lagged county-level characteristics. In Columns (1-2), transactions are split based on whether the jurisdictions require residents' approval for bond and tax increases. In Columns (3-4), a triple interaction between* $Post_t \times Chg.Itm_j$ *with the approval indicator is added. Columns (5-6) further split the approval indicator into Majority and Supermajority status. All trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. Standard errors, presented in parentheses, are double-clustered at the county and trading month level. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively*

	Dependent variable: Spread (bps)					
	No Approval	Approval	*All*		*All*	
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_t \times Chg.Itm_j$	2.83	63.92***	−31.16	−40.47	−28.18	−37.42
	(26.08)	(20.93)	(26.96)	(31.42)	(26.28)	(31.15)
... x Approval			98.02***	99.71**		
			(36.44)	(39.39)		
... x Majority states					78.82**	81.45**
					(31.99)	(37.14)
... x Supermajority states					196.00*	194.56*
					(107.94)	(102.36)
State x Month FE	X	X	X	X	X	X
Bond FE	X	X	X	X	X	X
Bonds characteristics	X	X	X	X	X	X
County-level controls	X	X	X	X	X	X
Weighted trades				X		X
Observations	284,844	1,355,551	1,640,395	1,640,395	1,640,395	1,640,395
R^2	0.93	0.93	0.93	0.93	0.93	0.93
Adjusted R^2	0.92	0.92	0.92	0.92	0.92	0.92

Table 4: Regression results with state-border variations in voting

This table reports the estimates of $Spread_{i,j,t} = \alpha_{st} + \alpha_{bt} + \alpha_i + \delta\ (Post_t \times Chg.Itm_j) + \delta^{vote}(Treated_j \times Post_t \times Chg.Itm_j) + \eta(Treated_j \times Post_t) + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county* j *traded at month* t, $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j, $Post_t$ *equals 1 for bonds traded after July 2017,* $Treated_j$ *equals one for bonds located on the side of a state border with a higher level of residents' empowerment in the local public finance process,* α_{st} *are state times month of trade fixed effects,* α_{bt} *are border times post-TCJA fixed effects,* α_i *are bond fixed effects,* $X_{i,t}$ *are bond level controls, and* $Z_{j,t-1}$ *are lagged county-level characteristics. All tax-exempt GO bonds issued before the TCJA announcement and traded from 2015 to 2019 are used. Standard errors, presented in parentheses, are double-clustered at the county and trading month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)			
	(1)	(2)	(3)	(4)
	All borders		Borders with no election	
$Post_t \times Chg.Itm_j$	31.18	12.85	7.10	−1.32
	(35.79)	(40.08)	(63.51)	(64.60)
... x $Treated_j$	87.35*	111.22*	134.29*	124.27*
	(50.97)	(57.61)	(70.13)	(73.50)
State x month FE	X	X	X	X
Border FE x post	X	X	X	X
Bond FE	X	X	X	X
Bond characteristics	X	X	X	X
County-level controls	X	X	X	X
Weights		X		X
Observations	226,529	226,529	117,410	117,410
R^2	0.93	0.93	0.93	0.93
Adjusted R^2	0.92	0.92	0.92	0.92

Table 5: Robustness to main specifications linked to other TCJA provisions

*This table reports the estimates of $Spread_{i,j,t} = \alpha_{st} + \alpha_i + \delta\ Post_t \times Other.TCJA.exposure_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county j and traded at month t, $Other.TCJA.exposure_j$ are various county-level exposure to other TCJA provisions, $Post_t$ equals 1 for bonds traded after July 2017, α_{st} are state-by-month fixed effects, α_i are bond fixed effects, $X_{i,t}$ are bond level controls and $Z_{j,t-1}$ are lagged county-level characteristics. All trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. Columns (1-2) use the percentage point change in average tax rates, Columns (3-4) use the 10-year (2005-2016) share of GO issuance that was advance refunding bonds, Columns (5-6) use the dollar amount of SALT that could not be deducted because of the cap normalized by the number of tax returns, and Columns (7-8) use the share of votes for Trump at the 2016 presidential election. Each even column includes in addition $Chg.Itm_j$, the decrease in the ratio of itemizers in county j, interacted with $Post_t$. Standard errors, presented in parentheses, are double-clustered at the county and trading month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Post_t \times Chg.TaxRate_j$	-143.14	6.58						
	(237.71)	(245.41)						
$Post_t \times Reliance.AdvRefunding_j$			0.20	-0.25				
			(14.03)	(13.67)				
$Post_t \times Wasted.SALT_j$					0.001**	0.001		
					(0.0004)	(0.0004)		
$Post_t \times Share.Trump_j$							-11.22*	-12.02**
							(5.85)	(5.63)
$Post_t \times Chg.Itm_j$		54.96**		54.80***		49.39**		58.42***
		(20.92)		(19.20)		(19.84)		(18.81)
State x Month FE	X	X	X	X	X	X	X	X
Bond FE	X	X	X	X	X	X	X	X
Bonds characteristics	X	X	X	X	X	X	X	X
County-level controls	X	X	X	X	X	X	X	X
Observations	1,640,288	1,640,288	1,640,288	1,640,288	1,637,667	1,637,667	1,635,124	1,635,124
R^2	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93
Adjusted R^2	0.92	0.92	0.92	0.92	0.92	0.92	0.92	0.92

Table 6: Weighted Least Squared regression estimates with Entropy Balancing Weights

This table reports the estimates of $Spread_{i,j,t} = \alpha_{st} + \alpha_i + \delta\ Post_t \times Treat_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county* j *traded at month* t, $Treat_j$ *is an indicator that equals one for jurisdiction with a decrease in the ratio of itemizers greater than 19.6 percentage points or the decrease in the ratio of itemizers in county* j *itself,* $Post_t$ *equals 1 for bonds traded after July 2017,* α_{st} *are state-by-month fixed effects,* α_i *are bond fixed effects, and* $X_{i,t}$ *are bond level controls. All trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. All untreated units are weighted by the entropy balancing weights algorithm matching treated and non-treated bonds based on their pre-TCJA means of (1) spread, (2) income per capita, and (3) homeownership rates. Regressions in Columns (2) and (4) also include the triple interaction with* $Approval_j$ *(and all other identified lower level interaction), the degree of residents' involvement in the local public finance process. Standard errors, presented in parentheses, are double-clustered at the county and trading month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)			
	(1)	(2)	(3)	(4)
$Post_t \times Treated_j$	4.66^{**}	-9.63^{**}		
	(2.28)	(4.41)		
... x Approval		16.24^{***}		
		(4.99)		
$Post_t \times Chg.Itm_j$			47.03^{**}	-75.21^{*}
			(20.40)	(41.72)
... x Approval				141.10^{***}
				(45.43)
State x Month FE	X	X	X	X
Bond FE	X	X	X	X
Bond characteristics	X	X	X	X
County-level control	X	X	X	X
Weighted trades EBAL	X	X	X	X
Observations	1,488,023	1,488,023	1,488,023	1,488,023
R^2	0.93	0.93	0.93	0.93
Adjusted R^2	0.92	0.92	0.92	0.92

Table 7: Local fiscal shock to residents and municipal bonds spreads at issuance

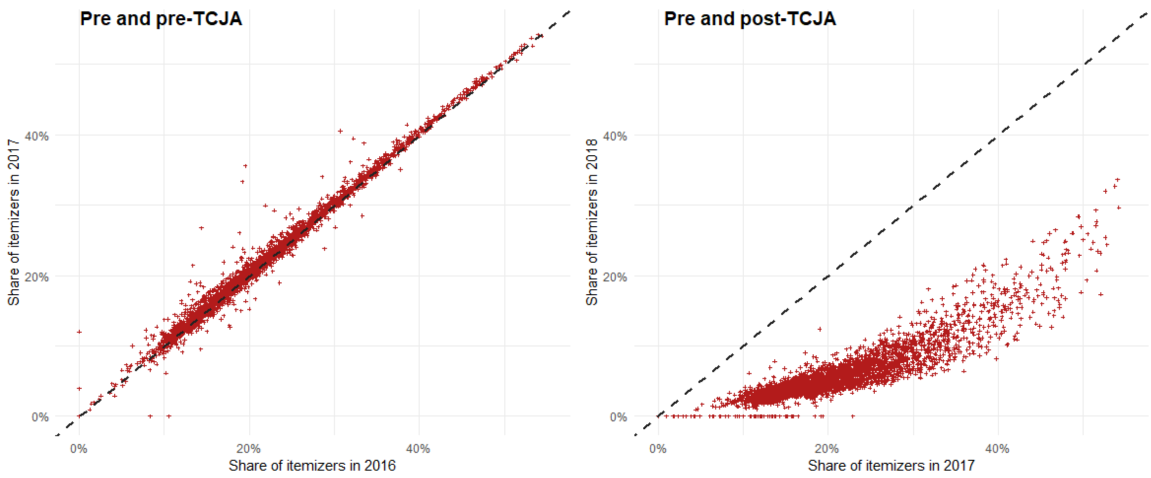
This table reports the estimates of $Spread_{i,j,t} = \alpha_t + \alpha_j + \delta\ Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the issuance municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county* j *and issued in month* t, $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j, $Post_t$ *equals 1 for bonds issued after July 2017,* α_t *are month fixed effects,* α_j *are county or issuer fixed effects as described in the table,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics. All tax-exempt GO bonds from 2015 to 2019 are used. In Columns (4) and (5), an additional triple interaction with* $Approval_j$ *(and all identified lower-level interactions), the degree of residents' involvement in the local public finance process, is added. In Columns (3) and (5), the observations are weighted by the number of GO bonds issued within the same county before the TCJA. Standard errors, presented in parentheses, are double-clustered at the county and month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)				
	(1)	(2)	(3)	(4)	(5)
$Post_t \times Chg.Itm_j$	157.63***	161.77***	165.63***	53.27	46.24
	(30.61)	(29.56)	(31.19)	(56.23)	(57.13)
... x Approval				121.84*	130.30*
				(68.41)	(65.22)
Month FE	X	X	X	X	X
County FE	X				
Issuer FE		X	X	X	X
Bond characteristics	X	X	X	X	X
County-level control	X	X	X	X	X
Weighted trades			X		X
Observations	104,970	104,970	104,970	104,970	104,970
R^2	0.86	0.91	0.92	0.91	0.92
Adjusted R^2	0.86	0.91	0.91	0.91	0.91

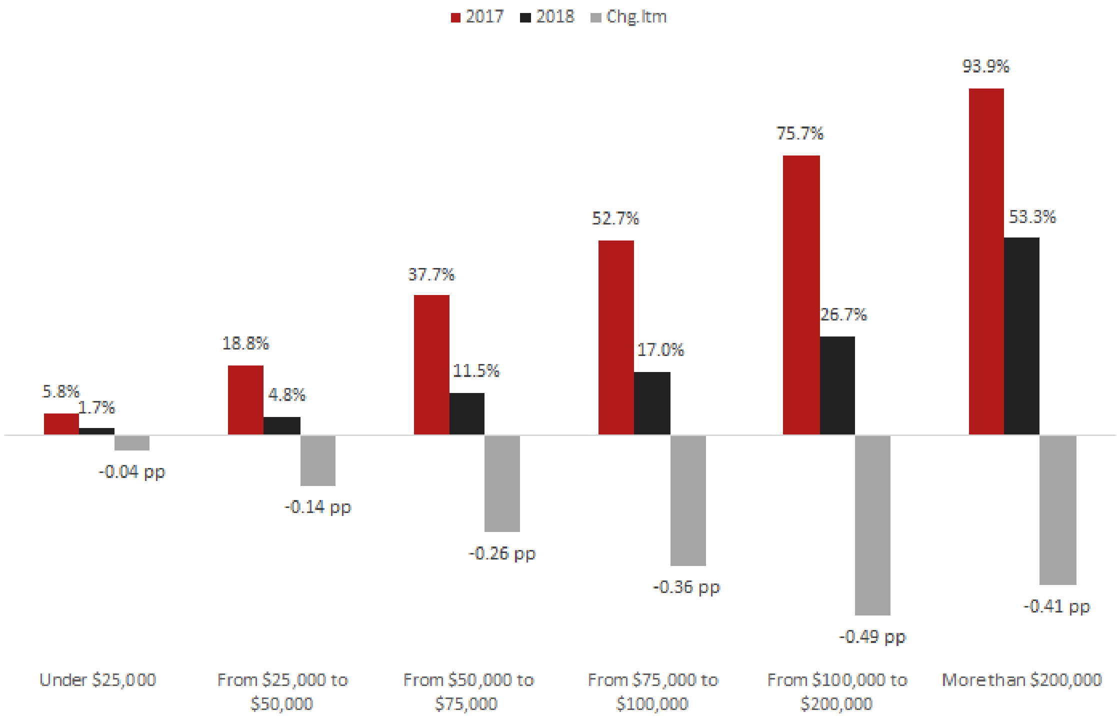
Internet Appendix

A Additional Figures & Tables

Figure A1: Change in taxpayers' itemization rates



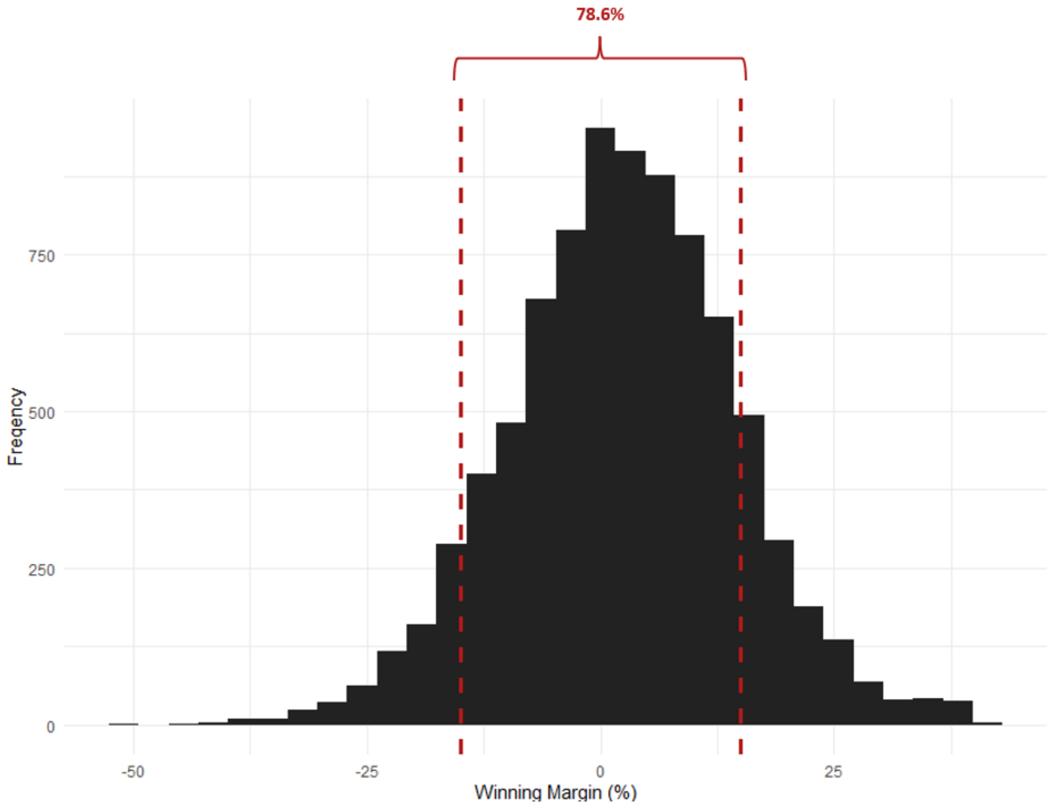
(a) Temporal variations



(b) Variation by income bins

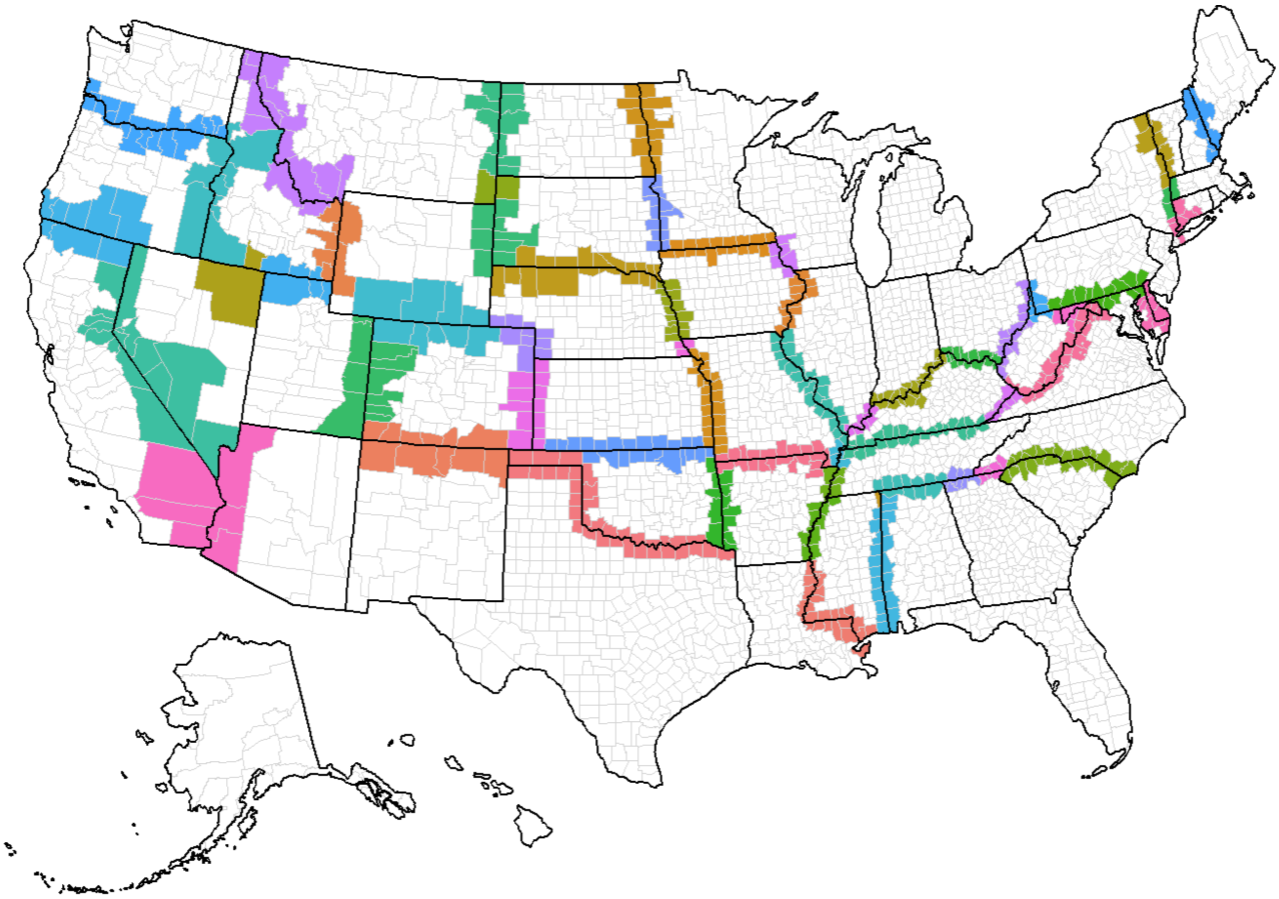
Note: The scatter plots in Panel (a) show the share of itemizers by county in 2016 versus 2017 (left) and between 2017 and 2018 (right). Each dot represents one county and both lines show the 45-degree line. In Panel (b), the bar graph shows the share of itemizers in 2017 and 2018 by income groups. The negative grey bars show the treatment variable Chg.Itm. The data comes from the Statistics of Incomes of the IRS

Figure A2: Winning margin at bond and tax referenda in four states



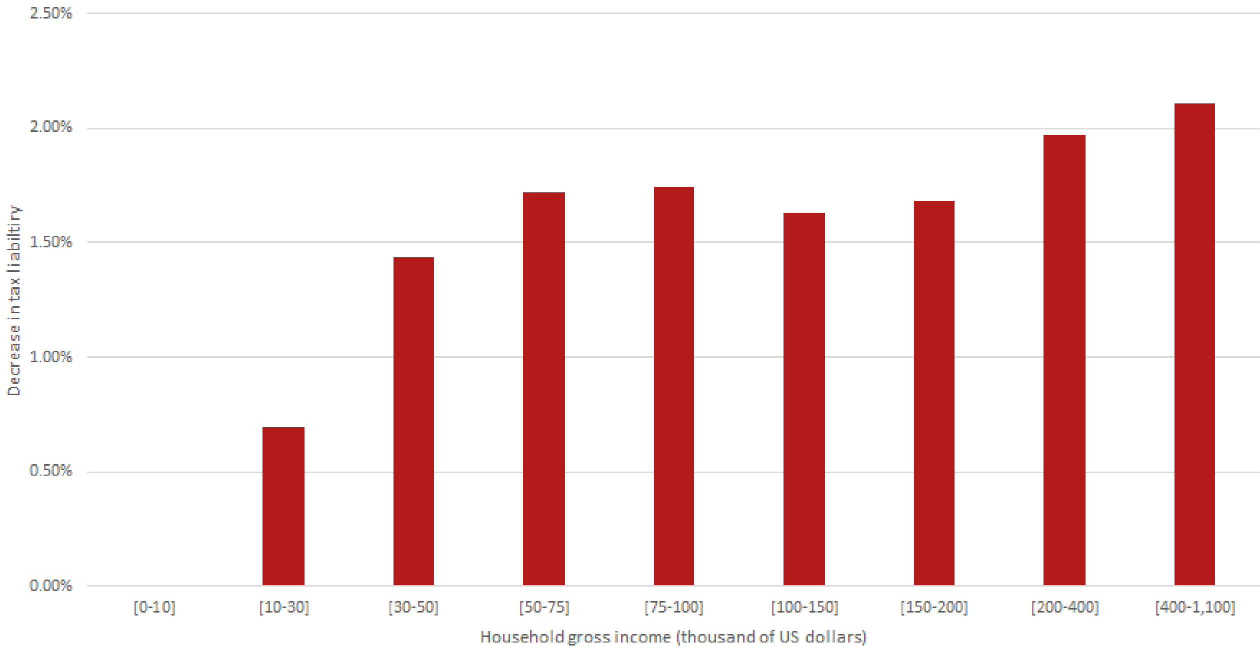
Note: This histogram shows the winning margin at school bond and tax referendums from 2000 to 2016 in California, Ohio, Texas, and Wisconsin. The data was downloaded from the replication files of Kogan et al. (2018). The vertical dashed bars show the +/- 15 percentage points away from the passing thresholds.

Figure A3: State-border pairs with distinct residents' voting status



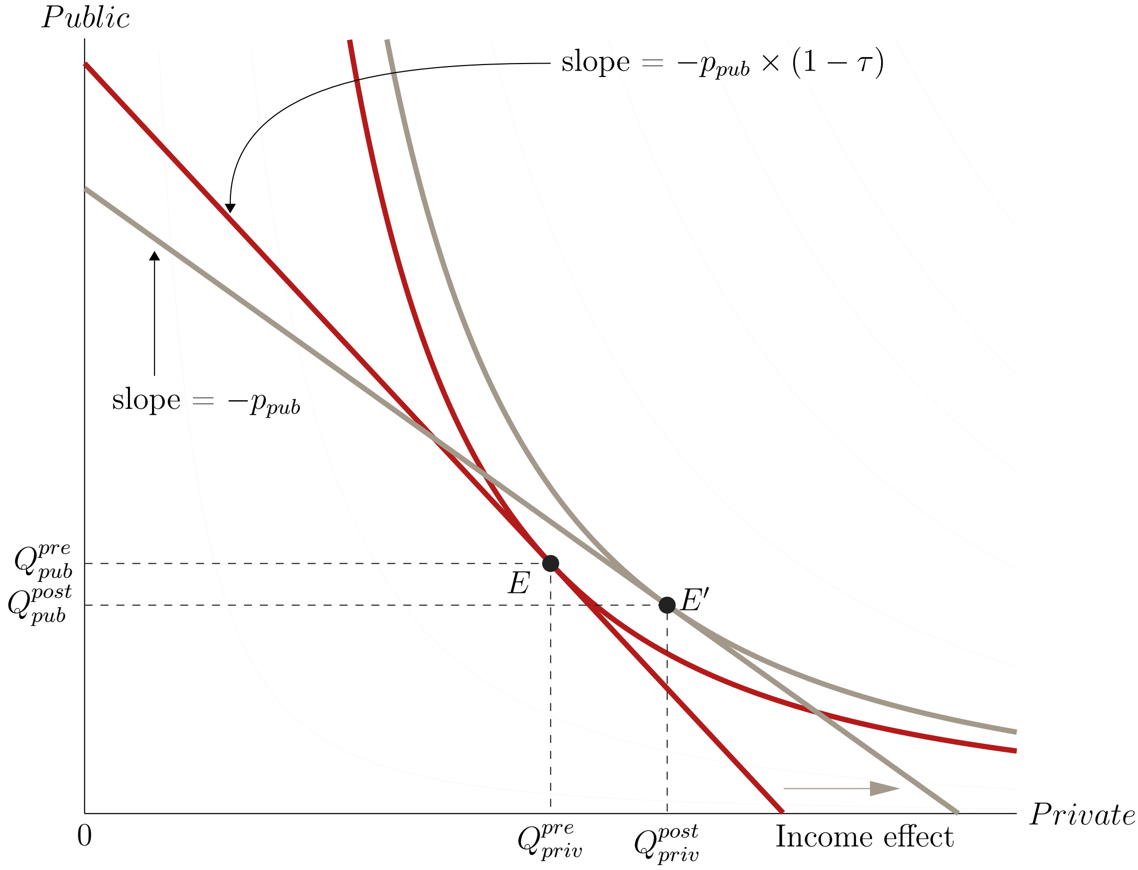
Note: This map shows the state-border pairs used in the border study. Counties are grouped together (one color) when the political involvement of residents differs from one to the other side of the state borders.

Figure A4: TCJA implied income effects from change in tax liability



Note: These bars show the decrease in tax liability from the TCJA in the percentage of income. The data is compiled from Table (4) of Ambrose et al. (2022).

Figure A5: TCJA fiscal change and demand for local public goods with income effects



Note: This chart theoretically shows the demand for local public goods for a resident who stopped deducting their State and Local Taxes (SALT) from their federal taxable income (treated resident), in combination with an increase in disposable income. The utility function is defined by Cobb-Douglas over public and private goods. p_{pub} *represents the price of a unit of public goods, and* τ *is the average tax on federal incomes.*

Table A1: Distribution of the fiscal shock measure by jurisdiction types

This table shows the distribution of the Chg.Itm variable for different jurisdiction level. The data comes from the Statistics of Income of the IRS. The school district measure is cross-walked using the The School District Geographic Reference Files provided by the EDGE program.

	Number	min	q01	q05	q25	Median	q75	q95	q99	max
State	51	0.130	0.131	0.139	0.168	0.196	0.212	0.241	0.259	0.266
County	3,141	0	0.059	0.085	0.119	0.151	0.191	0.251	0.293	0.347
School Districts	13,471	0	0.049	0.091	0.134	0.176	0.228	0.308	0.349	0.418
Zip code	27,521	0	0	0.045	0.129	0.176	0.231	0.313	0.364	0.583

Table A2: State level change in the share of itemizers pre- and post-TCJA

State	Chg.Itm (p.p.)	State	Chg.Itm (p.p.)
AL	18.17	MT	20.78
AK	15.38	NE	20.59
AZ	18.84	NV	16.73
AR	15.89	NH	21.93
CA	18.03	NJ	25.27
CO	20.11	NM	15.48
CT	26.62	NY	22.92
DE	21.18	NC	18.90
DC	18.62	ND	14.30
FL	17.13	OH	19.64
GA	20.08	OK	15.67
HI	16.65	OR	22.95
ID	20.41	PA	20.32
IL	21.24	RI	22.74
IN	17.02	SC	18.60
IA	23.27	SD	13.00
KS	18.15	TN	13.56
KY	20.08	TX	16.93
LA	16.64	UT	21.46
ME	20.02	VT	20.69
MD	22.65	VA	20.35
MA	23.07	WA	18.00
MI	19.75	WV	13.12
MN	24.23	WI	24.05
MS	16.76	WY	15.73
MO	18.97		

Table A3: Local fiscal shock and municipal bonds spreads - full set of coefficients

This table reports the estimates of $Spread_{i,j,t} = \alpha_{st} + \alpha_i + \delta \; Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$*.* $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county* j *and traded at month* t*,* $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j*,* $Post_t$ *equals 1 for bonds traded after July 2017,* α_{st} *are state-by-month fixed effects,* α_i *are bond fixed effects,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics. All trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. In Columns (4) and (5), regressions include the log of house value, the annual housing price growth, and the 3-year-horizon growth of housing permits. In Column (5), the post-TCJA trades are weighted by the number of trades for the same bond observed before the TCJA. Standard errors in parentheses are double-clustered at the county and month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)				
	(1)	(2)	(3)	(4)	(5)
$Post_t \times Chg.Itm_j$	67.10**	55.06***	54.73***	56.40***	48.03**
	(27.53)	(19.19)	(19.16)	(19.76)	(21.81)
Population		−110.38**	−114.62**	−113.88**	−110.86**
		(45.11)	(44.55)	(45.33)	(51.91)
Income per capita		0.001***	0.001***	0.001***	0.001***
		(0.0002)	(0.0002)	(0.0002)	(0.0002)
Pop. growth		−140.57*	−125.31*	−177.48**	−233.04**
		(75.80)	(73.73)	(84.35)	(99.52)
Employment growth		−56.24**	−55.78**	−59.52**	−57.93**
		(23.84)	(23.85)	(25.20)	(28.50)
Labor participation		60.54	55.51	89.85	116.03*
		(47.60)	(46.34)	(55.84)	(63.22)
Inv. maturity			91.52***	91.84***	86.36***
			(8.34)	(8.34)	(7.73)
Treasury Rate			−0.59***	−0.59***	−0.59***
			(0.04)	(0.04)	(0.04)
House value growth (%)				−0.09	−0.08
				(0.11)	(0.14)
House Value (log)				−5.90	−2.56
				(15.79)	(17.36)
Permit growth 3-year-horizon				0.06	0.03
				(0.27)	(0.43)
Bond FE	X	X	X	X	X
State x Month FE	X	X	X	X	X
Weighted trades					X
Observations	1,641,662	1,641,662	1,641,662	1,622,649	1,622,649
R^2	0.93	0.93	0.93	0.93	0.93
Adjusted R^2	0.92	0.92	0.92	0.92	0.92

Table A4: Robustness to main specifications

This table reports the regression estimates of various specifications following the baseline regression $Spread_{i,j,t} = \alpha_{st} + \alpha_i + \delta\ Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county j and traded at month t,* $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j, $Post_t$ *equals 1 for bonds traded after July 2017,* α_{st} *are state-by-month fixed effects,* α_j *are county or bond fixed effects as described in the table,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics. All trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. In Column (1), the dependent variable is replaced by* $Spread\_MMA_{i,j,t}$, *the traded municipal bond spread over the MMA-curve. In Column (2), the treatment intensity variable is* $Itm2017_j$, *the share of itemizers in county* j *in 2017. In Column (3), the regressions do not include* $Z_{j,t-1}$, *in Column (4)* $X_{i,t}$ *includes bond rating fixed effects, and in Column (5) it also includes bond rating fixed effects interacted with month of trade fixed effects. Standard errors, presented in parentheses, are double-clustered at the county and trading month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable:				
	Spread MMA	Spread (bps)			
	(1)	(2)	(3)	(4)	(5)
$Post_t \times Chg.Itm_j$	29.58***		65.13**	56.14***	45.43***
	(10.36)		(27.53)	(17.91)	(15.82)
$Post_t \times Shr.Itm17_j$		35.23***			
		(8.72)			
State x Month FE	X	X	X	X	X
Bond FE	X	X	X	X	X
Bonds characteristics	X	X	X	X	X
County-level controls	X	X		X	X
Rating FE				X	X
Rating x Month FE					X
Observations	1,488,023	1,488,023	1,488,871	1,488,023	1,488,023
R^2	0.90	0.93	0.93	0.93	0.93
Adjusted R^2	0.88	0.92	0.92	0.92	0.92

Table A5: Unobservable-bias-adjusted treatment effects

This table reports the implied treatment effect (β^*) after correcting for the possible unobservable bias following Oster (2019) methodology using R_{max}, the maximum achievable R-square should we have access to all observable and non-observable variables, of 1 and various δ parameters, the degree of selection on unobservable relative to observables. Columns (1) and (2) use the regressions results of Table 2 Columns (1) and Columns (4) compared to a baseline regression that includes only the treatment variable $Post_t \times Chg.Itm_j$ and its lower interactions ($\beta = 126.61$ and $R^2 = 0.04$). In Columns (3) and (4), we compare the results of Columns (1) and (4) of Table 2 with regressions that include Month by state fixed effects, county fixed effects, and bond characteristics ($\beta = 75.29$ and $R^2 = 0.64$). The last row shows the implied δ that would equal the treatment effects to zero (beta$^* = 0$).

	No control		Some controls	
	to some	to most	to more	to most
	(1)	(2)	(3)	(4)
$\delta = 0.5$	64.853	53.837	66.150	54.298
$\delta = 1$	62.611	51.273	65.204	52.193
$\delta = 2$	58.125	46.144	63.311	47.985
$\delta = 3$	53.640	41.015	61.419	43.776
δ^*	14.958	10.997	35.456	13.402

Table A6: Difference-in-differences estimates of the house value increase

This table reports the estimates of $log(HousePrice_{j,t}) = \alpha_1\ Post_t + \alpha_2\ Chg.Itm_j + \alpha_3\ (Post_t \times Chg.Itm_j) + \epsilon_{j,t}$ *where* $HousePrice_{j,t}$ *is the Single Family Median house price for each zip code from Zillow ZHVI for all months from January 2015 to December 2020. Post equals one for periods after the enactment of the TCJA in January 2018, and Chg.Itm is the differences between the share of itemizers in 2017 and 2018 in each zip code computing from the SOI of the IRS. Standard errors clustered at the level of the fixed effects are presented in parentheses. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable:				
	log(Median house value)				
	(1)	(2)	(3)	(4)	(5)
Post x Chg.Itm	-0.122^{**}	-0.125^{***}	-0.109^{***}	-0.112^{***}	-0.112^{***}
	(0.051)	(0.035)	(0.009)	(0.036)	(0.011)
Chg.Itm	4.822^{***}	4.824^{***}	4.022^{***}	3.856^{***}	
	(0.207)	(0.205)	(0.125)	(0.163)	
Post	0.163^{***}				
	(0.013)				
State FE	X	X			
Metro FE			X		
County FE				X	
Zipcode					X
Month fixed effects		X	X	X	X
Observations	1,887,988	1,887,988	1,887,988	1,887,988	1,887,988
R^2	0.616	0.619	0.750	0.738	0.996
Adjusted R^2	0.616	0.619	0.750	0.738	0.996

Table A7: Robustness analysis – Housing permit growth

This table reports the estimates of $Spread_{i,j,t} = \alpha_{st} + \alpha_i + \delta\ Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county* j *and traded at month* t, $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j, $Post_t$ *equals 1 for bonds traded after July 2017,* α_{st} *are state-by-month fixed effects,* α_i *are bond fixed effects,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics. In each column, the regression is supplemented by a variable proxying for the growth in housing permits in county j from the Census Building Permits Survey.For instance, in Column (1)* $Permitgrowth1year_t = Permit_t/Permit_{t-1} - 1$, *in Column (2),* $Permitgrowth2year_t = Permit_t/Permit_{t-2} - 1$, *and in Column (4)* $Permitcum.growth2year_t = (Permit_t + Permit_{t-1})/(Permit_{t-2} + Permit_{t-3}) - 1$. *All trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. Standard errors in parentheses are double-clustered at the county and month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)				
	(1)	(2)	(3)	(4)	(5)
$Post_t \times Chg.Itm_j$	55.38***	54.55***	55.75***	53.94***	54.71***
	(19.38)	(19.27)	(19.37)	(19.13)	(19.32)
Permit growth 1-year	0.05				
	(0.35)				
Permit growth 2-year		0.38			
		(0.37)			
Permit growth 5-year			−0.18		
			(0.20)		
Permit cum. growth 2-year				0.63*	
				(0.37)	
Permit cum. growth 3-year					0.05
					(0.27)
State x Month FE	X	X	X	X	X
Bond FE	X	X	X	X	X
Bond characteristics	X	X	X	X	X
County-level control	X	X	X	X	X
Observations	1,636,703	1,636,624	1,636,574	1,638,411	1,638,832
R^2	0.93	0.93	0.93	0.93	0.93
Adjusted R^2	0.92	0.92	0.92	0.92	0.92

Table A8: Robustness analysis – Housing permit growth exposure

This table reports the estimates of $Spread_{i,j,t} = \alpha_{st} + \alpha_i + \delta\ Post_t \times Chg.Itm_j + \phi\ Post_t \times Permits.Growth.Exposure_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county* j *and traded at month* t, $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j, $Post_t$ *equals 1 for bonds traded after July 2017,* α_{st} *are state-by-month fixed effects,* α_i *are bond fixed effects,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics.* $Permits.Growth.Exposure_j$ *is the growth in housing permits in 2017 over the last one, two or three years in county j computed from the Census Building Permits Survey. In Columns (4-6), we replace* $Post_t$ *by trading month fixed effects. In Columns (7-9), we transform* $Permits.Growth.Exposure_j$ *into deciles categories that we interact with month fixed effects. All trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. Standard errors in parentheses are double-clustered at the county and month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Post_t \times Chg.Itm_j$	55.67***	50.59**	52.85***	55.88***	50.25**	50.40***	52.78***	55.44***	50.61***
	(19.43)	(19.27)	(19.00)	(19.43)	(19.31)	(18.61)	(18.59)	(16.96)	(17.01)
$Post_t \times Permits.Growth.1y.Exposure_j$	1.36								
	(1.70)								
$Post_t \times Permits.Growth.2y.Exposure_j$		−2.70*							
		(1.57)							
$Post_t \times Permits.Growth.3y.Exposure_j$			−0.76**						
			(0.34)						
$Month.FE_t \times Permits.Growth.1y.Exposure_j$				X					
$Month.FE_t \times Permits.Growth.2y.Exposure_j$					X				
$Month.FE_t \times Permits.Growth.3y.Exposure_j$						X			
$Month.FE_t \times Permits.Growth.1y.Exposure.Decile_j$							X		
$Month.FE_t \times Permits.Growth.2y.Exposure.Decile_j$								X	
$Month.FE_t \times Permits.Growth.3y.Exposure.Decile_j$									X
State x Month FE	X	X	X	X	X	X	X	X	X
Bond FE	X	X	X	X	X	X	X	X	X
Bond characteristics	X	X	X	X	X	X	X	X	X
County-level control	X	X	X	X	X	X	X	X	X
Observations	1,634,007	1,634,007	1,634,007	1,634,007	1,634,007	1,634,007	1,634,007	1,634,007	1,634,007
R^2	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93
Adjusted R^2	0.92	0.92	0.92	0.92	0.92	0.92	0.92	0.92	0.92

Table A9: Main results dropping 2017

This table reports the estimates of $Spread_{i,j,t} = \alpha_{st} + \alpha_i + \delta\ Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county* j *and traded at month* t, $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j, $Post_t$ *equals 1 for bonds traded after July 2017,* α_{st} *are state-by-month fixed effects,* α_i *are bond fixed effects,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics. All trades from 2015, 2016, 2018, and 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. In Columns (4) and (5), regressions include the log of house value, the annual housing price growth, and the 3-year-horizon growth of housing permits. In Column (5), the post-TCJA trades are weighted by the number of trades for the same bond observed before the TCJA. Standard errors in parentheses are double-clustered at the county and month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)				
	(1)	(2)	(3)	(4)	(5)
$Post_t \times Chg.Itm_j$	86.75**	69.82***	69.71***	71.12***	63.13**
	(34.29)	(24.69)	(24.54)	(25.36)	(26.70)
Bond FE	X	X	X	X	X
State x Month FE	X	X	X	X	X
County-level control		X	X	X	X
Time-varying bond control			X	X	X
Housing price controls				X	X
Weighted trades					X
Observations	1,300,572	1,299,873	1,299,873	1,284,574	1,284,574
R^2	0.93	0.93	0.93	0.93	0.93
Adjusted R^2	0.92	0.92	0.92	0.92	0.91

Table A10: Placebo tests

This table reports the estimates of $Spread_{i,j,t} = \delta\,(Post_t \times Chg.Itm_j) + \alpha_{st} + \alpha_i + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county* j *and traded at month* t, $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j *before and after the TCJA,* α_{st} *are state-by-month fixed effects,* α_i *are bond fixed effects,* $X_{i,t}$ *are bond level controls, and* $Z_{j,t-1}$ *are lagged county-level characteristics. Each pair of Columns uses 4 years of tax-exempt GO bond trades as indicated in the Columns headers and* $Post_t$ *equals 1 for the second half of the sample. Standard errors, presented in parentheses, are double-clustered at the county and trading month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively*

	Dependent variable: Spread (bps)							
	2010-2013		*2011-2014*		*2012-2015*		*2013-2016*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Post_t \times Chg.Itm_j$	−5.42	2.15	−3.17	−6.60	−33.68	−31.00	2.77	3.70
	(19.95)	(19.97)	(20.64)	(21.63)	(38.43)	(36.51)	(25.50)	(27.19)
State x Month FE	X	X	X	X	X	X	X	X
Bond FE	X	X	X	X	X	X	X	X
Bonds characteristics	X	X	X	X	X	X	X	X
County-level controls	X	X	X	X	X	X	X	X
Weighted trades		X		X		X		X
Observations	1,222,216	1,222,216	973,481	973,481	1,129,310	1,129,310	1,085,313	1,085,313
R^2	0.87	0.87	0.92	0.92	0.90	0.90	0.92	0.92
Adjusted R^2	0.84	0.84	0.90	0.90	0.88	0.88	0.91	0.90

Table A11: Differences in bonds characteristics based on required approval indicator

*This table reports the summary statistics of tax-exempt GO bonds traded from 2015 until July 2017 (n = 831,288). All statistics are weighted by the inverse of the frequency of trades so that each of the 266,107 bonds carries the same weight. Spread is bond yield over the maturity-matched tax-exempt treasury yield in basis points, spread MMA is the maturity-matched yield on the Municipal Market Advisors AAA-rated curve, $Chg.Itm_j$ is the change in the share of itemizers at the county level. Month-level house values are collected from Zillow ZHVI, and the growth is year-to-year annual housing price growth. The housing permit growth is the cumulative 3-year-horizon growth of housing permits collected from the Census Bureau's Building Permit Survey. The data is split between jurisdictions that require residents' approval for fiscal and bonding policies or do not. The means for the two groups are presented in Columns (4) and (5). The difference in means along the t-statistics computed via OLS with double-clustered standard errors at the state and trade month levels are shown in the last two columns. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Mean	Std. dev.	Median	Approval	Non-approval	Difference	t-statistics
Main variables:							
Spread (bps)	273.90	168.10	242.00	279.23	247.27	31.96	2.61***
Spread MMA (bps)	92.83	80.80	68.02	94.78	83.10	11.68	4.44***
Chg.Itm (%)	0.21	0.05	0.21	0.20	0.22	−0.02	−1.44
Bond-level control variables:							
Rating (notch)	18.33	1.95	18.50	18.21	18.94	−0.73	−3.48***
Coupon (%)	3.60	1.31	3.98	3.55	3.86	−0.32	−2.94***
Maturity (years)	8.15	5.73	6.88	8.30	7.39	0.92	2.14**
Amount (000s)	2,396.37	8,588.05	1,004.63	2,308.13	2,837.30	−529.16	−0.62
Callable	0.57	0.50	0.50	0.57	0.54	0.03	1.35
Insured	0.31	0.46	0	0.33	0.20	0.13	3.20***
Reoffer	0.15	0.35	0	0.15	0.13	0.02	1.81*
Negotiated	0.38	0.48	0	0.40	0.27	0.13	1.24
County-level control variables:							
Income per capita (000s)	52.59	17.30	48.97	50.94	60.87	−9.93	−2.66***
Population growth (%)	0.01	0.01	0.01	0.01	0.01	0.003	0.80
Employment growth (%)	0.02	0.02	0.02	0.02	0.02	0.0002	0.06
Labor participation (%)	0.75	0.06	0.75	0.75	0.77	−0.02	−1.39
House Value (log)	12.36	0.59	12.29	12.32	12.55	−0.23	−1.29
House value growth (%)	4.57	3.51	4.34	4.77	3.59	1.18	1.22
Housing permit growth (%)	0.52	1.76	0.42	0.52	0.52	−0.004	−0.04

Table A12: Municipal bond yields and residents' involvement in municipal finance

This table reports the estimates of $Spread_{i,j,t} = \alpha_t + \psi Approval_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the tax-adjusted spread over the maturity-matched treasury yield at issuance,* $Approval_j$ *are indicators for jurisdiction in which residents' approval for local taxes and bonds are required,* α_t *are month fixed effects,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics. The sample consists of all GO bonds issuance data from 2015 to July 2017. In Columns (2) and (4), we further split the approval indicator into Majority and Supermajority status. In Columns (3) and (4) use, the sample is restricted to GO bonds that are uninsured. Standard errors in parentheses are double-clustered at the county and issuing months level. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)			
	All issues		Uninsured issues	
	(1)	(2)	(3)	(4)
Approval	7.22***		7.19**	
	(2.62)		(2.78)	
Majority states		4.49		4.93
		(2.76)		(2.97)
Supermajority states		18.20***		15.70***
		(3.89)		(4.41)
Month FE	X	X	X	X
Bond characteristics	X	X	X	X
County-level controls	X	X	X	X
Observations	69,830	69,830	53,194	53,194
R^2	0.81	0.81	0.79	0.79
Adjusted R^2	0.81	0.81	0.79	0.79

Table A13: Robustness to voter specifications linked to other TCJA provisions

This table reports the estimates of $Spread_{i,j,t} = \alpha_{st} + \alpha_i + \delta\ Post_t \times Other.TCJA.exposure_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$*.* $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county j and traded at month t,* $Other.TCJA.exposure_j$ *are various county-level exposure to other TCJA provisions,* $Post_t$ *equals 1 for bonds traded after July 2017,* α_{st} *are state-by-month fixed effects,* α_i *are bond fixed effects,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics. All trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. Columns (1-2) use the percentage point change in average tax rates, Columns (3-4) use the 10-year (2005-2016) share of GO issuance that was advance refunding bonds, Columns (5-6) use the dollar amount of SALT that could not be deducted because of the cap normalized by the number of tax returns, and Columns (7-8) use the share of votes for Trump at the 2016 presidential election. Each column also includes a triple interaction (and all identified lower-level interactions) between* $Post_t$*,* $Chg.Itm_j$ *(the decrease in the ratio of itemizers in county* j*), and* $Approval_j$*) (the degree of residents' involvement in the local public finance process). Standard errors, presented in parentheses, are double-clustered at the county and trading month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)			
	(1)	(2)	(3)	(4)
$Post_t \times Chg.TaxRate_j$	−28.54			
	(239.04)			
$Post_t \times Reliance.AdvRefunding_j$		0.26		
		(13.70)		
$Post_t \times Wasted.SALT_j$			0.001*	
			(0.0004)	
$Post_t \times Share.Trump_j$				−12.23**
				(5.77)
$Post_t \times Chg.Itm_j$	−32.50	−31.17	−39.51	−25.46
	(25.81)	(26.65)	(30.43)	(25.88)
... x Approval	98.73***	98.05***	99.95**	95.53***
	(34.97)	(36.16)	(38.43)	(35.66)
State x Month FE	X	X	X	X
Bond FE	X	X	X	X
Bonds characteristics	X	X	X	X
County-level controls	X	X	X	X
Observations	1,640,288	1,640,288	1,637,667	1,635,124
R^2	0.93	0.93	0.93	0.93
Adjusted R^2	0.92	0.92	0.92	0.92

Table A14: Summary statistics weighted by Entropy Balancing Weights

*This table reports the summary statistics of the bond characteristics traded before the TCJA shock (n = 831,288). The sample consists of tax-exempt GO bonds issued by all local governments except state governments. All statistics are weighted by the entropy balancing weights that match bonds in high Chg.Itm jurisdictions to bonds in low Chg.Itm jurisdictions based on the pre-TCJA mean values for (1) spread, (2) median income per capita, and (3) homeownership rates. Spread is the tax-adjusted spread over the treasury bill, spread MMA is the maturity-matched yield on the Municipal Market Advisors AAA-rated curve, and Chg.Itm is the change in the share of itemizers at the county level. The data is split between municipal bonds that occurred in counties with high or low Chg.Itm (below or above the national shock of 19.6 percentage points). The means for the two groups are presented in Columns (4) and (5). The difference in means along the t-statistics computed via OLS with double-clustered standard errors at the county and trade month levels are shown in the last two columns. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Mean	Std. dev.	Median	High *Chg.Itm*	Low *Chg.Itm*	Difference	t-statistics
Main variables:							
Spread (bps)	271.66	174.73	227.93	267.66	275.44	-7.78	-1.50
Spread MMA (bps)	98.97	86.49	68.52	98.16	99.74	-1.58	-0.66
Chg.Itm (%)	0.20	0.05	0.19	0.24	0.16	0.08	25.77***
Bond-level control variables:							
Rating (notch)	18.42	1.94	18.50	18.52	18.33	0.20	1.05
Coupon (%)	3.57	1.26	3.98	3.61	3.54	0.07	1.67
Maturity (years)	7.34	5.53	5.96	6.89	7.78	-0.89	-5.34***
Amount (000s)	13.68	1.23	13.66	13.71	13.65	0.06	0.93
Callable	0.56	0.50	0.50	0.54	0.57	-0.03	-3.01***
Insured	0.32	0.47	0	0.30	0.34	-0.04	-1.27
Reoffer	0.12	0.32	0	0.11	0.12	-0.004	-0.52
Negotiated	0.37	0.48	0	0.32	0.42	-0.10	-2.71**
County-level control variables:							
Income per capita (000s)	55,892.47	22,231.34	49,616	57,321.28	54,538.16	2,783.12	0.58
Population growth (%)	0.01	0.01	0.01	0.01	0.01	-0.0001	-0.05
Employment growth (%)	0.02	0.02	0.02	0.02	0.02	0.003	1.07*
Labor participation (%)	0.77	0.07	0.76	0.77	0.76	0.02	1.78

Table A15: Bond pre-TCJA rating and maturity, change in itemizers, and municipal bond spread

This table reports the estimates of $Spread_{i,j,t} = \delta(Post_t \times Chg.Itm_j) + \delta^{rating}(Post_t \times Chg.Itm_j \times LowRating_j) + \alpha_{st} + \alpha_i + \beta X_{i,t} + \gamma Z_{j,t-1} + \eta(Post_t \times HighRating_j) + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county j and traded at month t,* $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j, $Post_t$ *equals 1 for bonds traded after July 2017,* α_{st} *are state-by-month fixed effects,* α_i *are bond fixed effects,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics. In Columns (1) and (2),* $LowRating_j$ *is an indicator that equals one if the pre-TCJA is lower than the median, and in Columns (3) and (4) is replaced by* $LongMaturity_j$*, an indicator that equals one if the pre-TCJA maturity is greater than the median. All trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. Columns (2) and (4) also include a triple interaction (and all identified lower-level interactions) between* $Post_t$*,* $Chg.Itm_j$*, and* $Approval_j$*) (the degree of residents' involvement in the local public finance process). Standard errors, presented in parentheses, are double-clustered at the county and trading month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)				
	(1)	(2)	(3)	(4)	(5)
$Post_t \times Chg.Itm_j$	2.54	-67.50^{**}	1.04	-79.22^{**}	-35.97
	(27.21)	(32.72)	(24.94)	(31.80)	(42.28)
... X *LowRating*	121.25^{*}	118.82^{*}			99.66
	(69.99)	(70.25)			(99.73)
... X *HighMaturity*			76.43^{***}	75.07^{***}	59.29^{*}
			(26.98)	(27.03)	(32.08)
... x High Maturity x Low Rating					23.96
					(62.56)
$Post_t \times Chg.Itm_j \times Approval_j$		80.79^{**}		92.61^{**}	
		(36.11)		(35.55)	
State x Month FE	X	X	X	X	X
Bond FE	X	X	X	X	X
Bond characteristics	X	X	X	X	X
County-level control	X	X	X	X	X
Observations	1,488,023	1,488,023	1,488,023	1,488,023	1,488,023
R^2	0.93	0.93	0.93	0.93	0.93
Adjusted R^2	0.92	0.92	0.92	0.92	0.92

Table A16: Local fiscal shock to residents and revenue bonds spreads

This table reports the estimates of $Spread_{i,j,t} = \alpha_{st} + \alpha_i + \delta\ Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded revenue bond tax-adjusted spread over the maturity-matched treasury yield located in county* j *and traded at month* t, $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j, $Post_t$ *equals 1 for bonds traded after July 2017,* α_{st} *are state-by-month fixed effects,* α_i *are bond fixed effects,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics. All trades from 2015 to 2019 of Revenue bonds issued before the TCJA announcement are used. In Columns (4) and (5), regressions also include the log of house value and the annual housing price growth. In Column (5), the post-TCJA trades are weighted by the number of trades for the same bond observed before the TCJA. Standard errors in parentheses are double-clustered at the county and month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)				
	(1)	(2)	(3)	(4)	(5)
$Post_t \times Chg.Itm_j$	-4.09	-33.21	-30.05	-15.01	-13.14
	(33.56)	(28.50)	(28.77)	(28.79)	(30.63)
Bond FE	X	X	X	X	X
State x Month FE	X	X	X	X	X
County-level control		X	X	X	X
Time-varying bond control			X	X	X
Housing price controls				X	X
Weighted trades					X
Observations	961,033	960,996	960,996	953,397	953,397
R^2	0.94	0.94	0.94	0.94	0.94
Adjusted R^2	0.93	0.93	0.93	0.93	0.93

Table A17: Residents' shock at state and school district level, and government bond yields

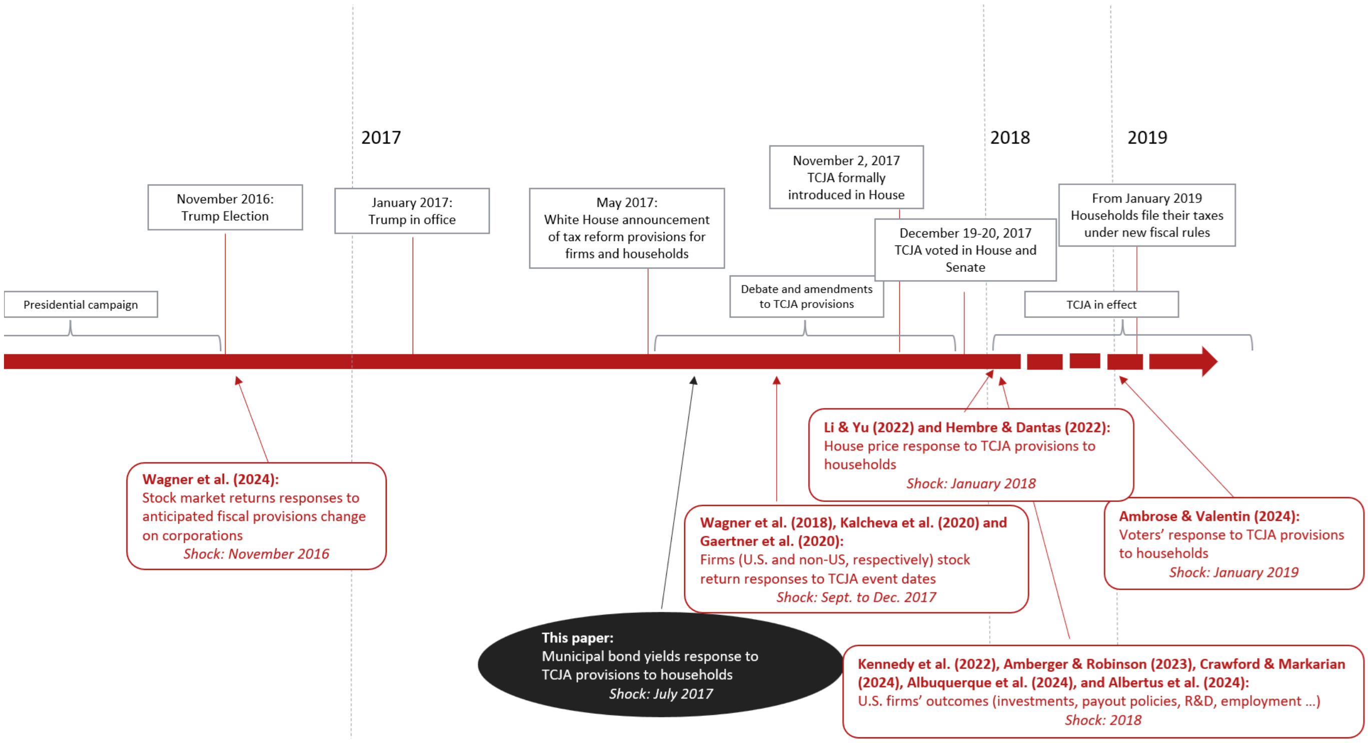
This table reports the estimates of $Spread_{i,j,t} = \alpha_{st} + \alpha_i + \delta\ Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in jurisdiction* j *traded at month* t, $Chg.Itm_j$ *is the decrease in the ratio of itemizers in jurisdiction* j, $Post_t$ *equals 1 for bonds traded after July 2017,* α_t *and* α_i *are time and bond fixed effects as described in the table,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics. All trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. Standard errors, presented in parentheses, are double-clustered at the State (Columns [1-2]) or School district (Columns [3-4]) and trading month level. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Spread (bps)			
	State bonds		School district bonds	
	(1)	(2)	(3)	(4)
$Post_t \times Chg.Itm_j$	247.74***	191.88***	17.54***	16.59***
	(33.95)	(34.16)	(4.34)	(4.79)
Month FE	X	X		
State x Month FE			X	X
Bond FE	X	X	X	X
Bond characteristics	X	X	X	X
County/State-level controls	X	X	X	X
Weighted trades		X		X
Observations	171,069	171,069	745,007	745,007
R^2	0.93	0.93	0.93	0.93
Adjusted R^2	0.93	0.92	0.92	0.91

B Discussion of Treatment Date Choice

Figure B1 shows the timeline of TCJA along with the various treatment dates from selected papers that use this shock in their empirical strategy.The existing literature supports a variety of post-TCJA treatment periods, ranging between November 2016 and January 2019, with the differences rationalized by the timing of information releases regarding changes in the corporate versus personal tax codes and the presumed speed with which different economic agents will incorporate this information. For example, Wagner et al. (2018a) uses the Trump election date (November 2016) to study the effects on stock returns since plans for corporate tax cuts were unambiguously on Trump's policy platform during the presidential campaign. Wagner et al. (2018b), Kalcheva et al. (2020) and Gaertner et al. (2020), in contrast, select various key-event dates, spanning from September to December 2017 to study non-US and US stock market reactions.[21] In contrast, Li and Yu (2022) and Hembre and Dantas (2022) use January 2018 to study the effects on home values, while Ambrose and Valentin (2024a) uses even later dates to study voting outcomes. These later dates are consistent with the fact that the TCJA proposals for changes in personal taxes were not discussed until the announcement in mid-2017, and the marginal homebuyers and voters were slower to incorporate the TCJA into their decisions.

Figure B1: TCJA timeline and related papers



Note: This timeline presents the key events related to the passage of the TCJA and illustrates the different studies that have used the TCJA as a shock in their empirical analyses, along with the various dates they have chosen.

Two key institutional details support our use of a post-period beginning on or around July 2017. First, the TCJA provisions regarding households were announced in May of 2017 and were followed

[21] Other studies that focus on corporate investments among other firms' outcomes, such as Crawford and Markarian (2024), Amberger and Robinson (2024), Kennedy et al. (2022), and Albertus et al. (2024), use 2018 as the shock date. Albuquerque et al. (2024) uses January 2018 but specifically controls for the three months leading to January 2018 to avoid anticipation bias in the payout rates they study. Given our monthly data and to avoid anticipation bias (Borochin et al., 2022), we avoid using these later dates.

by several months of debate before being voted on in the House of Representatives in December of 2017. Thus, any date before May of 2017 is almost certainly too early for the purposes of our study, although in theory there could have been an earlier response that was then updated when the specific provisions of the TCJA were released.

Second, the July 2017 date is consistent with the dates chosen in the literature to the extent that the marginal investors in the municipal bond market are somewhat slower to incorporate information compared to equity market participants but faster than the typical homebuyer or voter. Although municipal bonds are traditionally viewed as being slower at incorporating information than more liquid markets (Cornaggia et al., 2022a), several recent studies support the idea that municipal bond prices have recently begun responding more quickly to new information. On a high level, evidence suggests that the municipal bond market has transformed from being illiquid to a more transparent and liquid market characterized by increased trading by informed investors (Hund et al., 2024).

C Endogeneity Concerns

We recognize that there is a potential correlation between our treatment intensity variable and trends in municipal bonds (and their premiums). Thus, we conduct several additional tests demonstrating that our analysis is robust to this concern. First, we study whether the composition of traded bonds changes around the TCJA in a way that could bias our main estimates. To do this, we construct complete panel data and estimate a linear probability model in which the dependent variable is an indicator that equals one when a bond is traded in a given month and the explanatory variables of interest are the same as those used in our main tests. Table C1 shows the results. The statistically insignificant coefficients on our main treatment variable indicate that more and less intensively treated bonds are not differential likely to be traded after the TCJA. Moreover, the non-significant interactions with other explanatory variables indicate little change in the relative characteristics of the treated and untreated bonds being traded after the TCJA shock.

Second, we acknowledge that our treatment embeds parts of any post-TCJA changes in the effect of factors such as household income, home value, and homeownership rate on municipal yield spreads. To document the effects of these three main determinants of the share of itemizers, we collect these data at the county level as of 2017 (pre-TCJA) and interact them with *Post* in regression specifications that parallel our main specification. We show the results in the first 4 columns of Table C2 noting that all these variables are standardized for ease of interpretation and comparison. As expected, these controls have a positive correlation with bond yields post-TCJA, although only the housing value exposure impacts yields significantly. For instance, the coefficients in Column (2) indicate that bond yield in a county with a housing value one standard deviation above the mean experiences a relative yield increase of 6.66 bps. Adding those three variables together depicts similar results with housing value being the only significant variable.

The last two columns of Table C2 show that our treatment operates through more than just these channels by controlling for deciles of these determinants interacted with month-fixed effects. The results of Columns (5) and (6) show that the residual variation in our treatment significantly predicts yield spreads, with possibly even a larger magnitude than the overall treatment effect we estimate. Examples of residual treatment are non-linearities in the income distribution (i.e., the change in itemizers is smallest in the bottom and top of the income distribution but concentrated in the middle and upper-middle class), the extent of family-based deductions, charitable giving, etc. The fact that these types of residual variation strongly predict post-TCJA yield is supportive of our argument because (1) it is unlikely that these features of the change in itemizers are coincidentally related to post-TCJA yield spreads through channels other than our treatment, and (2) the similarity in the point estimate when compared to our main results suggests that our baseline treatment effect estimates are not substantially biased due to the inclusion of the three forces.

Table C1: Pre- and post-TCJA probability of municipal bond trading

This table reports the estimates of $Traded_{i,j,t} = \alpha_{st} + \alpha_j + \delta \; Post_t \times Chg.Itm_j + \beta X_i \times Post_t \times Chg.Itm_j + \varepsilon_{i,t}$. $Traded_{i,t}$ *is an indicator variable that equals one if bond* i *is traded in month* t, $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j, $Post_t$ *equals 1 for bonds traded after July 2017,* α_{st} *are state-by-month fixed effects,* α_j *are county fixed effects,* X_i *are bond level controls. A complete panel data of tax-exempt GO bonds issued before the TCJA announcement and traded at least once in the period 2015-2019 is used. Standard errors in parentheses are double-clustered at the county and month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable: Traded indicator						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Post_t \times Chg.Itm_j$	0.007	0.064*	0.107	−0.003	−0.030	−0.004	−0.029
	(0.017)	(0.038)	(0.241)	(0.021)	(0.025)	(0.019)	(0.021)
Coupon		0.006					
		(0.004)					
... $\times Post_t$		0.003					
		(0.002)					
... $\times Chg.Itm_j$		0.031					
		(0.022)					
... $\times Post_t \times Chg.Itm_j$		−0.016					
		(0.012)					
Amount (log)			0.049***				
			(0.007)				
... $\times Post_t$			0.001				
			(0.004)				
... $\times Chg.Itm_j$			0.023				
			(0.036)				
... $\times Post_t \times Chg.Itm_j$			−0.007				
			(0.018)				
Callable				0.020***			
				(0.006)			
... $\times Post_t$				−0.002			
				(0.004)			
... $\times Chg.Itm_j$				−0.037			
				(0.026)			
... $\times Post_t \times Chg.Itm_j$				0.017			
				(0.021)			
Insured					−0.032*		
					(0.018)		
... $\times Post_t$					−0.040***		
					(0.011)		
... $\times Chg.Itm_j$					0.105		
					(0.069)		
... $\times Post_t \times Chg.Itm_j$					−0.014		
					(0.048)		
Reoffer						−0.064***	
						(0.010)	
... $\times Post_t$						0.076***	
						(0.011)	
... $\times Chg.Itm_j$						0.034	
						(0.048)	
... $\times Post_t \times Chg.Itm_j$						0.086*	
						(0.051)	
Negotiated							0.019**
							(0.009)
... $\times Post_t$							−0.037***
							(0.014)
... $\times Chg.Itm_j$							−0.010
							(0.034)
... $\times Post_t \times Chg.Itm_j$							0.087
							(0.067)
State x Month FE	X	X	X	X	X	X	X
County FE	X	X	X	X	X	X	X
Observations	15,966,420	15,966,420	15,966,420	15,966,420	15,966,420	15,966,420	15,966,420
R^2	0.021	0.024	0.059	0.022	0.024	0.025	0.022
Adjusted R^2	0.021	0.023	0.059	0.022	0.024	0.025	0.021

Table C2: The effects of the determinants of Itemizers on Bond Yield spread

This table reports the estimates of $Spread_{i,j,t} = \alpha_{st} + \alpha_i + \delta\ Post_t \times Chg.Itm_j + \phi\ Post_t \times Determinants.Itm.Exposure_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county* j *and traded at month* t, $Chg.Itm_j$ *is the decrease in the ratio of itemizers in county* j, $Post_t$ *equals 1 for bonds traded after July 2017,* α_{st} *are state-by-month fixed effects,* α_i *are bond fixed effects,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics.* $Determinants.Itm.Exposure_j$ *is the pre-TCJA measure of median income (Column [1]), housing value (Column [2]), and ownership rates (Column [3]) collected from the American Community Survey 5-years at the county level in 2017. All these variables are standardized for ease of interpretation and comparison. All trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. Standard errors in parentheses are double-clustered at the county and month levels. Estimates followed by ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.*

	Dependent variable:					
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_t \times Income.Exposure_j$	5.61			3.32		
	(3.75)			(3.64)		
$Post_t \times HouseValue.Exposure_j$		6.66**		8.06*		
		(3.16)		(4.38)		
$Post_t \times Ownership.Exposure_j$			0.47	3.66		
			(2.60)	(3.88)		
$Post_t \times Chg.Itm_j$					84.65***	19.01
					(26.74)	(29.26)
... $\times Approval_j$						83.60**
						(34.57)
State x Month FE	X	X	X	X	X	X
Bond FE	X	X	X	X	X	X
Bond characteristics	X	X	X	X	X	X
County-level control	X	X	X	X	X	X
Income deciles x Month FE					X	X
House Value deciles x Month FE					X	X
Ownership deciles x Month FE					X	X
Observations	1,640,395	1,640,319	1,640,395	1,640,319	1,640,395	1,640,395
R^2	0.94	0.94	0.94	0.94	0.94	0.94
Adjusted R^2	0.92	0.92	0.92	0.92	0.92	0.92

D Details on the Voter's approval variable

State	Status	Source	Details
Alabama	Majority	Ballotpedia	*"Alabama requires a ballot measure to issue new bonding or issue special school taxes "*
Alaska	No election	Ballotpedia	*"Alaska is one of nine states along with the District of Columbia that do not require elections for school bond and tax votes."*
Arizona	Majority	Ballotpedia	*"Arizona requires school districts to hold elections for issuing new bonds or to override a school district budget."*
Arkansas	Majority	Abott et al. (2020)	*"There are no limits on local property tax rates that school districts can levy, but there is a minimum of 25 mills for maintenance and operations. A majority of voters must approve increases to property tax rates beyond this minimum."*
California	Supermajority	Rueben and Cerdán (2003)	*"the passage of Proposition 218, which required that any new general tax or fee measure achieve a two-thirds majority vote, did little to aid local efforts to raise funds. For these governments, the only good news along these lines came in 2000, when the passage of Proposition 39 lowered the supermajority needed for school bond approval to 55 percent."*
		CA Secretary of State	Many elections occurring every year at every level of local governments.
Colorado	No election	Ballotpedia	*"Colorado has two different types of ballot measures that are required under two different laws. [...] This type of ballot measure has rarely been used; it is considered to be a last resort option."*
Connecticut	No election	Ballotpedia	*"In Connecticut, the voters of a school district must approve the district's budget on a annual basis. [...]. Also, Connecticut requires state approval for public school bonding and capital projects, but do not require voter approval."*

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State	Status	Source	Details
Delaware	Majority	Ballotpedia	*"Under Delaware law, all school districts must call for a special election in order to issue new bonds. Delaware requires a levy election if a school district wants to increase or decrease a tax levy."*
District of Columbia	No election	Ballotpedia	*"There are no school bond or tax elections in Washington, D.C.."*
Florida	Majority	Ballotpedia	*"referendums are required for school districts wanting to exceed the state's millage limit and to issue new bonds"*
Georgia	Majority	Ballotpedia	*"A simple majority is needed in order to pass a school levy or sales tax election"*
Hawaii	No election	Ballotpedia	*"Hawaii is one of nine states along with the District of Columbia to not have school bond and tax elections."*
Idaho	Supermajority	Idaho Constitution	*"No county, city, board of education, or school district, or other subdivision of the state, shall incur any indebtedness, [...] without the assent of two-thirds of the qualified electors thereof voting at an election to be held for that purpose, [...]"*
Illinois	Majority	Illinois General Assembly	*"however, nothing in this amendatory Act of the 98th General Assembly authorizes a taxing district to increase its limiting rate or its aggregate extension without first obtaining referendum approval as provided in this Section."*
Iowa	Supermajority	Iowa department of education	*"A bond election for school buildings and/or sites must be approved by at least 60 percent of those voting."*
Kansas	Majority	Ballotpedia	*" In Kansas, no capital outlay levy can exceed five years in length without voter approval. Also, ballot questions are mandatory in Kansas for issuing new bonds."*
Kentucky	Supermajority	Ballotpedia	*"A two-thirds super-majority vote is required to pass a bond issue in the State of Kentucky"*
Louisiana	Majority	Abott et al. (2020)	*"Parish school boards also have the authority to levy a "constitutional" property tax of up to 5 mills (13 mills in New Orleans). Districts can supplement this by obtaining voter approval to levy additional property taxes for a specific purpose relating to operations, maintenance, or capital expenses."*

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State	Status	Source	Details
Maine	Majority	Ballotpedia	*"In Maine, school districts are required to have elections to approve a budget or to issue new bonding and or bond taxes."*
Maryland	Only Baltimore	Ballotpedia	*"Under Maryland law, all new bonding for school districts and extensions to tax levies must be approved by the respective County Board of Commissioners where the district resides. The only part of the state that requires bond elections is Baltimore County. "*
Massachusetts	No election	Ballotpedia	*"Massachusetts is one of nine states along with the District of Columbia that do not require elections for school bond and tax votes."*
Michigan	Majority	Abott et al. (2020)	*"School districts must get approval from a majority of voters if they wish to exceed caps on local property taxes that the state set in 1994. In general, there is a cap of 18 mills on non-homestead property taxes. A majority of school district voters must approve millage increases for non-homestead properties and must renew these mills over time."*
Minnesota	Majority	Ballotpedia	*"Minnesota law requires a referendum for issuing new bonds that pertains to capital improvements or new construction of facilities."*
		MN secretary of states	Multiple tax and bond ballots for county, municipal, and school district.
Mississippi	No election	Ballotpedia	*"Mississippi is one of nine states along with the District of Columbia that do not require elections for school bond and tax votes."*
Missouri	Supermajority	Ballotpedia	*"There are tough super majority requirements as a bond issue requires a four-sevenths vote (57.15%) while any referendum involving exceeding the levy cap, debt ceiling levy, or a Proposition C levy referendum requires a two-thirds super majority vote (66.7%) for approval. "*
Nebraska	Majority	Ballotpedia	*"Elections are mandated for exceeding the Maximum Levy Cap, the growth rate, and issuing new bonding."*
New Jersey	Majority	Ballotpedia	*"A three-fifths (60%) super majority is required for levy limit elections while bond referendums require a simple majority."*

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State	Status	Source	Details
Nevada	Majority	Ballotpedia	*"In Nevada, a bond election is mandated if a school district needs to exceed the fifteen percent debt limit set by Nevada law. Also, if a school district wants to issue bonding to build new facilities or improve existing ones, voter approval is required"*
New Hampshire	No election	Ballotpedia	*"New Hampshire does not require school districts to seek voter approval to issue new bonding. New Hampshire is one of nine states along with the District of Columbia to not require school bond or tax elections."*
New Mexico	Majority	Article IX, New Mexico Constitution	*"No such law [] shall take effect until it shall have been submitted to the qualified electors of the state and have received a majority of all the votes cast thereon at a general election"*
New York	Majority with exception	Ballotpedia	*"Elections are not required for any city over 125,000, New York City, and Nassau County because the New York State Constitution forbids any school district from exceeding their debt limits and asking the voters to approve increases in debt limits."*
		Ballotpedia	*"A three-fifths (60%) super-majority vote is required to approve a election involving the constitutionally protected debt limit. A simple majority vote is required to pass a bond issue."*
North Carolina	No election	Ballotpedia	*"Under North Carolina law, a school district cannot take debt that exceeds two-thirds of their current debt without voter approval. The provision in the Constitution is for all local government units including school districts. However, North Carolina does not mandate elections for bond issues and exceeding levy caps. "*
North Dakota	Supermajority	Ballotpedia	*"North Dakota is one of a few states to have tough super majority requirements for voter approval. Any levy for capital improvements must have a three-fifths (60%) super-majority vote while any general fund levy election question must have a fifty-five percent super majority. A distance learning levy only requires a simple majority."*
Ohio	Majority	Coate and Milton (2019)	*"Since 1911, Ohio has limited the ability of local governments to set property tax rates, and allowed voters to approve higher taxes through referenda. [...] Approval requires a majority of votes."*

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State	Status	Source	Details
Oklahoma	Supermajority	Ballotpedia	*"Oklahoma requires a three-fifths (60%) super-majority vote to approve bond referendums while referendums involving the five mill limit only require a simple majority vote."*
Oregon	Majority	Ballotpedia	*"In Oregon, ballot questions are required when a school district if a school district wants to issue bonding, exceed the property tax cap protected by the Oregon Constitution, and exceed the Oregon Mill Rate. "*
Pennsylvania	Majority	Abott et al. (2020)	*"A 2006 law requires voter approval for any proposed tax increase that exceeds an index capturing increases in wages and employment costs for schools. "*
Rhode Island	No election	Ballotpedia	*"There are no school bond and tax elections in Rhode Island. Rhode Island is one of nine states along with the District of Columbia to not hold school bond or tax elections. "*
South Carolina	Majority	Ballotpedia	*"South Carolina requires ballot questions to issue new bonding and to exceed the fifteen mill levy limit. "*
South Dakota	Supermajority	Ballotpedia	*"South Dakota requires a three-fifths (60%) super-majority vote in order to approve a bond measure. However, South Dakota does not require elections for school districts seeking to exceed the levy cap."*
Tennessee	No election	Ballotpedia	*"Tennessee is one of eight states along with the District of Columbia that does not hold school bond or school tax referendums."*
Texas	Majority	Yu et al. (2022)	*"To issue general obligation bonds, local governments in Texas must obtain voter approval in referenda that use a simple majority rule."*
Utah	Majority	Ballotpedia	*"A simple majority is needed to pass an election involving the state mandated debt limit or a bond issue."*
Vermont	No election	Ballotpedia	*"All bond issues and requests to raise tax levies are the authority of the Vermont Educational and Health Buildings Agency. It is up to the agency to freely set the terms of all bond issues including interest, selling terms, maturity, and restrictions on successive bond issues."*

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State	Status	Source	Details
Virginia	No election	Ballotpedia	*"If a county wants to provide bonding to two or more school divisions a ballot question is required. If the request comes from a single school division or from a individual municipal government, no ballot question is required."*
		Virginia department of election	Only statewide and region bonds elections are reported since 1956.
Washington	Supermajority	MRSC	*"Many local ballot measures only require a simple majority (50% plus one) with no minimum voter turnout. However, bond measures and certain other voted revenue sources require a 60% Supermajority and may also require minimum validation (voter turnout) requirements."*
West Virginia	Supermajority	Ballotpedia	*"In order to pass a levy cap election, a simple majority is required. Any election that requires new bonding or bond taxes must pass through a super-majority of three-fifths (60%) to gain voter approval"*
Wisconsin	Majority	Abott et al. (2020)	*"Districts must obtain approval from a majority of district voters to exceed the state revenue limit."*
		Ballotpedia	*"Under Wisconsin law, a school district is required to issue a referendum for new bonds if the total costs of the bonding cause the district's debt to surpass $1,000,000 [...]. School districts are exempted from referendums if they are ordered by a state or federal court to remove hazardous substances or be in compliance with fire standards and the districts need to issue new bonds to pay for the state or federally mandated improvements. Also, no referendum is required if a new school district is created by detaching a former consolidated district or purchasing property "*
		WI Department of Public Instructions	Despite the institutional exceptions, we observe numerous yearly referendums on tax and bond elections in WI school district (e.g. 81 proposed ballot in 2022).
Wyoming	Majority	Ballotpedia	*"Wyoming has three different kinds of school finance elections which are for bond issues, creating or repaying a building fund, or to issue a special tax for adult education programs. [...] Bonding cannot be used to retire debt or pay other obligations."*