# Voter-induced Municipal Credit Risk*

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**Abstract**

Coordination between local officials and voters is critical to the functioning of the municipal bond market. We exploit a policy-induced shock to residents' demand for local public goods to compare yield spreads of the same bonds before and after the announcement. We find a positive relation between jurisdictions with a higher share of treated residents and credit spreads. The average treatment effect is approximately 10.5 basis points and is concentrated in jurisdictions whose residents are more politically empowered in the municipal financing process. Thus, our findings offer evidence of a voter-induced premium in municipal bond credit spreads.

***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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Stakeholder coordination and principal-agent problems are of primary importance in finance since the seminal articles of Coase (1937) and Jensen and Meckling (1976).[1] However, little research exists on the coordination costs between elected officials (agents) and voters (principals) in the $4 trillion municipal bond market (SIFMA). This void in the literature is surprising because the ability to raise revenues to fund municipal bond payments often depends on voter approval. To the extent that the incentives of elected officials and voters differ (i.e., a lack of coordination), voter involvement in the municipal financing process should be a source of credit risk, especially when voters' preferences for local public services change.

We motivate our study with the intuition that the quantity of public goods demanded by residents drops as their financing cost rises. Our first research question is whether a decreased demand for public goods induced by an increase in cost is reflected in municipal credit spreads. We then examine whether residents' empowerment in the municipal financing and bonding process contributes to increased municipal risk in the face of a policy-induced shock to the cost of local public goods. This voter-induced municipal credit risk would be consistent with various industry reports, with one arguing that the *"mood of taxpayers can prove as important for the value of the [municipal] bonds as the issuer's financial ability to pay"* (Temel, 2001, page 172) and also more generally with the literature connecting political uncertainty with corporate investment (Julio and Yook, 2012; Jens, 2017) and asset prices (Kelly et al., 2016; Brogaard et al., 2019).

To examine the importance of voter-induced municipal credit risk, we conduct a policy experiment surrounding the Tax Cuts and Job Acts (TCJA). The key variable in our policy experiment follows the intuition in Ambrose and Valentin (2023a) in measuring the extent residents of a jurisdiction bear a cost increase for financing local public goods. Specifically, a jurisdiction's treatment exposure relates to the decrease in its share of residents deducting their state and local taxes (SALT). This treatment represents a large

[1]For example, research on corporate governance and the design of contracts to mitigate conflicts between managers (agents) and owners (principals) is extensive. See, for example, Andrei and Robert (1997)'s survey on corporate governance.

increase in the cost of public goods because residents who deduct SALT only pay 63% to 90% of the tax dollars collected. 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 $95^{\text{th}}$ and $5^{\text{th}}$ percentiles.

In a continuous treatment difference-in-differences framework with bond fixed effects to force within bond comparisons, we consistently find that yields of bonds issued by jurisdictions that have a higher share of treated residents increase following the announcement of the TCJA. Secondary market bond spreads for the typical county, whose treatment averages 15 p.p., increase by 8 to 12 basis points (bps) or approximately 3.0% to 4.6% of the unconditional average yield spread. We then show that the dynamics with which the effect emerges support a causal effect of the share of SALT deducters in determining the relative spreads.

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 importantly, we specifically show that neither first-order changes, such as changes in marginal tax rates or the removal of tax exemption on advance refunding bonds, nor second-order effects, such as changes in the current value of the tax base (i.e. housing values), impact our treatment effect estimates. Our findings are also robust to using the pre-TCJA fraction of SALT deducters as a measure of treatment or to using entropy-balanced samples in which treated and untreated jurisdictions are similar along dimensions such as income per capita and homeownership rate.

We propose that the municipal bond market reaction to residents' cost of local public services is driven by uncertainty regarding future willingness to fund local public projects. 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. Adding a triple-interaction term to our main specification, we 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' deductions intensity 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 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 setting credit risk.

Our paper complements the current literature in four ways. First, it expands the discussion of credit risks in the municipal bond market and the factors that are priced by municipal bond investors. This stream of literature has documented the effects of several local socioeconomic outcomes on municipal bond yields such as the extent of opioids abuse (Cornaggia et al., 2022), the aging of the population (Butler and Yi, 2022), the closure of newspapers (Gao et al., 2020), climate risks (Painter, 2020; Goldsmith-Pinkham et al., 2023), local subsidies to firms (Chava et al., 2023), the timing of trade reporting (Chalmers et al., 2021), or the state-level creditor protection regime (Gao et al., 2019a).[2] We add residents' involvement in the local public finance process as an important driver of municipal bond prices.

Second, our study provides 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, lending credence to the fear that if voter preferences

[2]Because we focus on fiscal policies, our paper also relates to research studying the implication of the preferential tax treatment for municipal bonds (Green, 1993; Babina et al., 2021; Garrett et al., 2023; Kueng, 2018; Ang et al., 2010; Longstaff, 2011). However, our paper focuses on residents' fiscal subsidies that increase risk of municipal bonds through increased voter-induced uncertainty and does not focus on investors' fiscal subsidies emerging from the non-taxation of municipal bond interest.

change, the risk to bondholders may increase through higher uncertainty. Voting impacts finance in other contexts as well. For example, using 200 years of data from 90 countries, Miller (2022) 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, Pastor 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., 2019b). Perhaps the closest paper to ours is Yu et al. (2022), which shows that the approval of municipal bonds by voters increases the risk for other bonds outstanding in the same jurisdictions, except when the bonds are largely approved by the voters. Our paper however differs from these studies by providing more direct evidence of voter-induced risk premia by leveraging a shock to residents' policy preferences.

Third, our paper complements the literature on the effect of federal fiscal policy on the local economy (Poterba, 1994; Hanson, 2012; Sommer and Sullivan, 2018; Li and Yu, 2022), and in turn, we also connect to the related literature documenting how investments in public goods impact the local economy (Donaldson and Hornbeck, 2016; Baum-Snow et al., 2017; Agarwal et al., 2023). Existing studies show that fiscal deductions affect property prices (Li and Yu, 2022; Hembre and Dantas, 2022; Valentin, 2023), residents' demand for local public goods (Pevzner et al., 2022; Ambrose and Valentin, 2023a), and the location of economic activity (Albouy, 2009; Coen-Pirani and Sieg, 2019; Fajgelbaum et al., 2019). The only papers, to our knowledge, investigating the effect of SALT on local governments are Feldstein and Metcalf (1987), which shows that jurisdictions whose residents benefit 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. We show that the loss of SALT deductibility subsidies for residents increases their municipality's costs of finance.

Finally, our study connects the importance of voter approval with municipal credit risk in a way that parallels the type of coordination and agency problems often reserved for discussion in corporate contexts (Jensen and Meckling, 1976). For instance, the classic literature on stockholder-bondholder conflicts recognizes the impact of stockholder-bondholder coordination problems on corporate debt contracts (Fama and Miller, 1972; Black and Cox, 1976; Smith Jr and Warner, 1979). More recently and more directly related to our setting, coordination has been discussed 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 potential effects on local public finance. In Section 2, we explain our data and methodology. In Section 3, we discuss the main results of our policy experiment. In Section 4, we provide evidence of voter-induced credit risk. Section 5, we discuss additional empirical tests to better establish the robustness of our empirical strategy. In Section 6, we conclude.

# 1 Municipal Finance & Voter Preferences

## 1.1 Residents' Preferences and Municipal Bonds Risk

Often, state and local governments must seek voter approval to issue general obligation (GO) bonds, which are issued to fund public-purpose projects and are repaid by taxes imposed on residents and firms of the issuing government. This voter approval implies that the political inclination of the voters can impact local governments' finances and in turn investor credit risk exposure. In its *"U.S. municipal bond defaults and recoveries"* report, covering years from 1970 to 2020, Moody's (2021) relates several instances connecting voters' approval and local government solvency. For instance, 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 make no clear predictions regarding the effect of voter involvement on municipal credit spreads, they do suggest that a shock to voters' inclination to finance local public goods represents a risk to municipal bond investors and that this risk increases in the extent of voter involvement.[3] Voters' involvement in the local public finance process can induce uncertainty regarding the future ability of local governments to finance public expenditures. 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. We thus anticipate that bond yields will reflect the heightened uncertainty regarding the ability to raise taxes in the future, with that heightened uncertainty being proportional to the shock to residents' inclination for financing local public goods and the extent of voter involvement in the municipal financing process.

[3] 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. Our empirical study does not investigate this question, rather we focus on how voter involvement affects the impact of a change in residents' preference for financing local public goods on municipal credit spreads.

## 1.2 A Shock to Voters' Demand for Local Public Services

We exploit a shock to residents' preferences for local public goods that emerges from the change in tax rules embedded in the TCJA. In the U.S., the tax code offers indirect subsidies to local governments by allowing residents to deduct from their taxable incomes all taxes paid to lower-level governments. These State and Local Taxes (SALT) deductions occur when an individual itemizes their tax returns, as opposed to opting for the standard deduction. With marginal tax rates ranging from 10% to 37%, residents who deduct SALT only 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 residents' incentive to itemize their deductions on their tax returns, and in turn, to deduct SALT. Many taxpayers stopped itemizing their deductions because of the substantial increase in the standard deduction to $24,000 for married filers. In addition, the revised tax code limits the SALT deduction to $10,000 and caps the mortgage interest deduction to loans below $750,000, further reducing the marginal use of the deductions (Ambrose et al., 2022). Consequently, the share of residents who itemize dropped from 31% in 2017 to 11.5% in 2018, increasing radically the net-of-deductions cost of deductible items such as the SALT. In turn, it decreased the cost of these non-collected taxes for the federal government from $104.1 billion in 2017 to $10.4 billion in 2019 (Department of the Treasury) which highlights the substantial drop in this form of revenue sharing between federal and local governments.[4]

Although the fiscal changes were national, local government exposure to the change in their residents' incentive to itemize is spatially heterogeneous. We exploit this spatial heterogeneity to test whether jurisdictions whose residents experience a significant decrease in the SALT deduction subsidy face a corresponding increase in financing costs. Our proposition relies on the assumption that a reduction in SALT deductions decreases

[4]Recognizing the importance of SALT for their local finances, several states unsuccessfully sued the federal government in response to the TCJA, arguing that the SALT deduction was important to maintain their taxation and fiscal policies (Hutchins, 2018; Erb, 2018).

the demand for local public services, with the magnitude of this decrease being proportional to the share of residents no longer deducting SALT. To conceptualize this point, we depict in Figure 1 the change in the consumption bundle of public and private goods demanded by residents. 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 $\tau$ is the average tax rates on federal taxable income.[5] 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 $E'$, indicating a lower demand for local public goods. In contrast, for a resident not impacted by the policy, the consumption bundle remains at point $E$.[6]

Other provisions in the TCJA include the removal of the tax exemption on advanced refunding municipal bonds, decreases in individual tax rates, adjustments to the Alternative Minimum Tax, and the reduction in corporate tax rates. In theory, these changes could affect municipal credit risk. However, we show in Section 5.2 that these concurrent changes are orthogonal to our shock and do not confound our main effect. Nevertheless, it is reasonable to expect an increase in overall disposable income since many taxpayers experienced a decrease in tax liability. We show in Figure A1, 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. If anything, the income effects that would result from a change in tax liability are thus limited in comparison to the cost change of $\tau(1-\tau)^{-1}$. We theoretically illustrate the possibility of a change in income in combination with a counterclockwise shift in the budget constraint in Figure A2. Unless the income effect is in the magnitude of the change in prices implied by the removal of the SALT deduction, the decrease in demand resulting from the removal of SALT deductions outweighs the increase in demand resulting from the income effect. Fur-

[5] That cost increase equals $\tau(1-\tau)^{-1}$ and ranges from 11.1% to 65.5% given the prevalent tax rates of 10% to 39.6% in 2017.

[6] This assumption also aligns with existing literature. Feldstein and Metcalf (1987) show that state and local governments with a higher proportion of residents benefiting from the SALT deductions rely more heavily on fiscal instruments that are deductible for individual taxpayers. Holtz-Eakin and Rosen (1990) show that federal deductibility of property tax payments has a positive impact on the property tax rate itself. A recent working paper by Ambrose and Valentin (2023a) specifically uses referendum results in California and shows that following the TCJA, residents of school districts oppose additional spending by voting against local public goods referendums.

thermore, even if income effects were as substantial as the $\tau(1-\tau)^{-1}$ price increase, which is improbable, our empirical approach focuses on the relative differential between treated households, who experience a slope change in their budget constraint, and non-treated households, who do not. This means that, regardless of the size of the income effects (indicated by the horizontal shift of the budget constraint on the Private goods axis), the decrease in demand for local public goods must be larger for treated residents. Thus, we do not expect the income effect to play a significant role in reshaping the municipal bond markets in comparison to the change in cost for providing local public goods.

### 1.3 Measuring the Fiscal Exposure

Our measure of local exposure to the fiscal shock 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 U.S. zip codes and counties with more than 100 taxpayers.[7] To circumvent potential reverse causality concerns, we also provide results using the itemizers pre-TCJA as a proxy to treated residents.

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 with a $Chg.Itm$ equals to 13.0

[7] 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. The School District Geographic Reference Files is 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), available at https://nces.ed.gov/programs/edge/Geographic/RelationshipFiles.

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 school district level the Bay Village City School District in Cleveland, OH has the highest change ($Chg.Itm = 41.8$ p.p.), while some school districts have 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 A3. 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 confirm the appropriateness of the treatment intensity variable, we test whether $Chg.Itm_j$ is associated with a decrease in house value which serves as the tax base for many local governments. We present in Table A3 the estimates of a continuous-treatment difference-in-differences framework using Zillow's home value index (ZHVI) as the dependent variable regressed on $Chg.Itm_j$ measured at the zip code level. The two-way fixed effects (TWFE) estimate in Column (5) shows that a decrease of 10 p.p. in the share of residents itemizing deductions is associated with a 1.1% decrease in house value, significant at the 1% level. For the median zip code with a $Chg.Itm_j$ of 17.6 p.p., this point estimate corresponds to a 2.0% decrease in house values in line with the estimates of Li and Yu (2022), or Hembre and Dantas (2022).

Our measure, although correlated with demographic characteristics such as income, house prices, and homeownership rates, exhibits a non-monotonic relationship with these factors.[8] To mitigate the possibility that these differences affect our identification assumption, we primarily investigate changes in spreads for the same bond. We also provide robust evidence of our results using an entropy-balanced weighted sample in which areas with high and low itemization rates are similar along other observable dimensions.

[8]In a non-tabulated multivariate regression, we find that income, homeownership rate, and house values are indeed positively correlated with $Chg.Itm_j$. But, even after accounting for these controls and state fixed effects, 15.7% of the variation in $Chg.Itm_j$ remains unexplained.

Despite the within-bond identification, understanding the correlation between our treatment intensity variable and income is critical since we want to avoid capturing variations impacting municipal bondholders (and thus variations in the cost of holding municipal bonds) as they often are state residents with higher incomes (Babina et al., 2021). As evidence of a disconnect between our main treatment intensity variable and high-income individuals, we show in Panel B of Figure A3 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. The non-linear relation between the share of treated taxpayers and income is also evident in Table 5 of Ambrose et al. (2022), with the most impacted group being homeowners within the $100,000-$150,000 bracket. The share of treated taxpayers diminishes at higher incomes. In addition, according to Longstaff (2011), the implied average marginal tax rate of municipal bondholders is very close to the top marginal tax rates. In 2017, the top marginal tax rate started on income greater than $470,701 of annual income, implying that the treatment variable we use relates to less affluent residents.

# 2 Empirical Framework

## 2.1 The Municipal Bonds Data

We use GO bond secondary market trades from the Municipal Securities Rulemaking Board (MSRB) that we connect to bond characteristics from the Mergent Municipal Fixed Income database. The trade sample comprises secondary market transactions for bonds issued before the TCJA announcement and traded between January 2015 to December 2019 or in some more restrictive specifications from 2016 to 2018. Although the TCJA provisions related to individuals are scheduled to expire in December 2025 (Gleckman, 2023), we do not expect this repeal to impact our results for several reasons. First, there is uncertainty regarding whether the repeal will take place and which provisions are likely to be continued or discontinued as some politicians recently introduced legislation to make some provisions permanent (Shaw, 2022; Wamhoff et al., 2023). Second, our analysis uses a short window around the TCJA announcement with a focus on bonds issued before the shock. Thus, if investors view the TCJA as temporary, this should work against us finding any results.

We drop transactions that fall within two weeks of issuance because they correspond to primary market issuance transactions (Schultz, 2012) and transactions with remaining time to maturity of less than one year because, with short maturity small price deviations lead to large changes in yield (Schwert, 2017). For transactions with missing yields, we compute trade yields from prices. Specifically, we calculate 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., 2022). For both samples, we focus on tax-exempt municipal bonds and exclude bonds offered via unconventional channels (e.g. limited offerings, private placements, and remarketing) and bonds issued by U.S. territories.

For each bond issue or trade, we estimate credit spreads from yields by constructing spread to maturity-matched treasury bonds adjusted for the tax-exemption benefits (Spreen and Gerrish, 2021). Interest incomes from municipal bonds are exempt from federal taxes and in most states from state income taxes too. To account for both exemptions, we follow Garrett et al. (2023) and construct the spread of bond $i$, issued (or traded) by jurisdiction $j$, 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 bond of maturity $m$ issued at time $t$, and $\tau_{j,t}$ is the marginal tax benefits from investing in municipal bonds such that:

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

The first term reflects the tax exemption provided by the federal government where

$\tau_t^{Federal}$ is the marginal tax rate on federal incomes that we adjust by $(1-\tau_{j,t}^{state})$ because state income taxes are deductible on federal tax incomes. This latter term becomes one for post-TCJA years because the deductibility benefits are limited for high-income earners given 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 to calibrate this tax adjustment in line with Longstaff (2011). Specifically, we use the marginal tax rates provided by the TaxSim of the NBER (Feenberg and Coutts, 1993). 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. 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, using information from Bloomberg. Then, we match bond data to county-level characteristics using the Federal Information Processing Standard (FIPS) codes. We collect population, employment, and income statistics from the Bureau of Economic Analysis (BEA), and labor force participation from the Bureau of Labor Statistics (BLS). 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.[9] The average yield spread over the maturity-matched tax-exempt treasury bill at issuance is 273.9. There is however large variation in spreads with an

[9]The weights ensure that we present means across bonds rather than means across trades. For example, if bond A is traded twice in the sample at 100 bps and 200 bps, and bond B is traded once at 250 bps; the mean reported is 200 bps.

interquartile range of 274.0 bps. We note that 45% of the GO bonds in the sample are from school districts, that 57% of the issues are callable, and that 31% carry bond insurance. 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 do not differ on most bond characteristics (except bond amount and maturity) but do come from wealthier and more populated counties with better economic outcomes. 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 in Section 5.1.

## 2.2 Within-bond Identification

We test whether the jurisdictions' exposure to residents' change in local public good preferences affects their cost of financing by estimating the yield spread change to residents' exposure to the TCJA shock. We regress the spread of bond $i$, issued or traded at time $t$ by a jurisdiction in county $j$ using the following specification

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

where $\alpha_{st}$ are month of trade by state fixed effects that absorb time-varying trends in bond yields within states, and $\alpha_j$ are bond fixed effects that force within-bonds comparison. Because of such inclusion, the coefficients on $Post_t$ and $Chg.Itm_j$ are absorbed by the granular fixed effects in our specification.

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. Although the details of the TCJA were finalized only in mid-December 2017 (Long, 2017), the provisions related to the change in household itemizing rules –including the doubling of the standard deductions– were revealed in mid-year 2017 (Gopal and Light, 2017; Puzzanghera, 2017; The Associated Press, 2017). Thus, we set the event date to July 2017 to reflect the date at which information about the TCJA's impact on individual taxpayers was likely incorporated into investors' information set. To exclude potential confounding events likely to appear further away from the announcement date, such as migration (Hageman et al., 2021), we also provide results using a shorter-time window around the shock (12 months before and after), in which we drop the second half of 2017.

Consistent with existing literature, our baseline specification includes standard control variables shown previously to affect municipal bond yields (Harris and Piwowar, 2006; Gao et al., 2020; Cornaggia et al., 2022). Specifically, $X_{i,t}$ is a vector of bond level controls including (1) coupon rate, (2) bond maturity and its inverse, (3) log bond size, (4) the maturity-match treasury yields, and indicator variables for whether the bond is (5) callable, (6) insured, (7) reoffered, (8) negotiated, and (9) four variables denoting the use of proceeds. 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. 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., 2022; Gao et al., 2020).

# 3 Main Results

## 3.1 Fiscal Shock and Municipal Credit Risk

Our main analysis focuses on secondary market trades of municipal GO bond yields. The sample includes bonds issued before the TCJA's announcement. The comparison of interest is thus the relative change in yields for bonds issued by jurisdictions heterogeneously impacted by the fiscal shock to their residents. By focusing on the secondary market, we mitigate the potential selection issue inherent in the primary market analysis.

In Table 2, we report the coefficients $\hat{\delta}$ of Equation (4) using the tax-adjusted spread over treasury yields as the dependent variable. Table A4 shows the full set of coefficients. We show the robustness of results using two sample periods – a longer window of 2015 to 2019 in Panel A and a shorter window spanning bond trades one-year around the announcement/enactment of the TCJA in Panel B. We report the results of our baseline regression that includes county and state-by-month fixed effects in Panel A, Column (1). The coefficient estimate on the explanatory variable $Post_t \times Chg.Itm_j$ is 59.1 bps, significant at the 1% level. This estimate translates into an 8.9 bps increase in yield spread for the typical county whose mean treatment averages 15 p.p., representing almost 3.2% additional cost of capital. In Column (2), we reestimate the regression with observations weighted in proportion to the pre-period trading frequency. This specification guarantees that each traded bond carries the same weight in the pre- and post-periods, alleviating the potential concern that certain types of bonds are traded more frequently after the shock. The coefficient $\hat{\delta} = 83.6$ bps, and is significant at the 1% level. The higher magnitude suggests that trading frequency is not homogeneous amongst bonds that are impacted heterogeneously by the residents' fiscal shock.

Because our sample bonds are issued by different types of local governments (counties, cities, school districts, and special purpose districts) with various risk profiles, variations in risk by types of local governments may confound the effect. We thus add bond fixed effects to force comparison within the same bond that traded both before and after the policy changes (Column [3]). We obtain consistently positive and significant results ($\hat{\delta} = 54.0$ bps). The specification using bond fixed effects as well as weighting the observations by the trading frequency pre-TCJA (Column [4]) leads to a significant estimate of 82.6 bps, significant at the 1% level.

Next, we examine the shorter window sample around the announcement of the TCJA and report analogous specification estimates in Panel B. This analysis comprises bonds traded between July 2016 to December 2018, excluding trades from July to December 2017, which allows us to better control for time-variant economic factors that could confound our main effect further away from the announcement date or within the announcement and enactment dates. We find consistent results of similar magnitude for $\hat{\delta}$, ranging from 37.2 to 77.11 bps and significant at least at the 10% level.

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 the bond fixed effects with issuer fixed effects using the first 6-digits of the CUSIP to force comparison within issuers. 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. Therefore, 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. The lack of quantity effects aligns with the extended period observed between the decision to issue GO bonds and their issuance, as documented byAdelino et al. (2022). More importantly, we do not observe changes that are correlated with our explanatory variable.

The results are presented in Column (5) of Table 2 using bonds issued between 2015 and 2019 (Panel A), or for the shorter-window sample (Panel B). After controlling for bond and county-level characteristics and including state-by-month and Cusip 6-digits (i.e. issuer) fixed effects, we find that an increase in $Chg.Itm_j$ leads to higher yields. In Panel A (B), $\hat{\delta} = 63.1$ (112.6), with p-values of 0.13 (0.08), highlighting a lack of power. Because the average decrease in the share of itemizers is approximately 15 p.p., this first estimate translates to an average increase in the cost of capital of 9.5 bps; or an annual \$2,272 additional interest payment for a bond issued in a typical county.[10] With \$145.5 billion of GO bonds issued in 2022 (SIFMA), this effect could represent up to \$137.7 million of the additional annual cost.

[10]The calculation is 9.5 bps $\times$ \$2.4 million, where we use the mean GO bond amount.

## 3.2 Dynamic Illustration

Our identification strategy relies on the assumption that in the pre-period the extent of treatment (i.e., $Chg.Itm_j$) 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 before the TCJA was announced.

We present the coefficient estimates in Figure 3. We 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, offering 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 effect rises in the three quarters after the policy announcement and then remains steady for the remainder of the sample period.

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 A5. Using the bond-fixed effects specifications, we see 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 prior to our main GO sample time period.

# 4 Examining the Voting Channel

## 4.1 Variation in Residents' Approval Requirement

The results of the policy experiment shown in the previous section relate a change in residents' indirect fiscal subsidy for local public goods to the bond yields issued by the jurisdictions in which they live. We attribute this credit risk to the heightened uncertainty regarding the ability of local jurisdictions to service their debt given the change in residents' willingness to fund local public services.

To further highlight the role of resident voters, we now compare jurisdictions that require residents' approval for local taxes and bond issuance to others that do not. In the spirit of Matsusaka (1995), we separate jurisdictions with voters' involvement in local public finance compared to purely representative jurisdictions. Because each state constitution has unique features regarding the issuance of debt for state and local government bonds (Kiewiet and Szakaty, 1996), we aggregate different sources and classify jurisdictions. We detail the sources for each state in Appendix B and show the map of the variation in that measure in Figure 4. 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.

Because of these longstanding institutional arrangements, we do not expect municipal bonds issued by local jurisdictions in states where voters are involved in the local public finance process to be similar to bonds from jurisdictions where elected officials have full autonomy in raising taxes and issuing debt. In Table A6, we report the summary statistics for all traded bonds before July 2017 split between jurisdictions based on whether voter approval is required. These differences are consistent with the finding of Kiewiet and Szakaty (1996), which finds that states that require supermajority approvals issue more guaranteed debt than those that require a lower approval level. We observe that 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, have longer maturity, and exhibit lower economic well-being, but do not differ regarding the treatment variable $Chg.Itm_j$. The higher yields and elevated guarantees are consistent with the market pricing added risk in areas with more voter involvement in the municipal financing process.

Using municipal bonds issued before July 2017, Table A7 displays results that suggest that bonds issued in approval states have higher yields. Because we cannot include spatial fixed effects to identify the *Approval* variable, the inferences we make here are only 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* into indicators for majority and non-majority states, we observe respectively an additional 4.5 bps and 18.2 bps yields, compared to jurisdictions with no voters requirement. The results suggest no particular differences when subsetting to uninsured bonds only (Columns [3-4]).

## 4.2 Identification Strategy

To link voters' approval requirements and the effects of a shock to residents' demand for local public goods on municipal yield spreads, we focus on the secondary market trade sample of bonds issued before the TCJA's announcement and traded in both periods. Like in the main analysis, we provide results for the full and shorter-window samples that alleviate confounding effects further away from the policy shock. We 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:

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

where $Approval_j$ is a dichotomous variable depicting the level of voting approval required in jurisdiction $j$. Thus, $\delta^{vote}$ captures the incremental effect of the fiscal shock for jurisdictions that have voters more involved in the local public finance process beyond the baseline effect captured by $\delta$. We hypothesize that $\delta^{vote}$ is positive, highlighting municipal bond investors' concern that voters who are hit with a higher implicit cost of public goods will put the finance of the jurisdiction at greater risk when they can approve or disapprove future municipal financing decisions.

### 4.3 Main Results on Voting Channel

In Table 3, we report in the first two columns the results of our test by separating the jurisdictions based on whether voters are required to approve local spending and bonds.[11] We observe that the coefficients on the $Post_t \times Chg.Itm_j$ is only positive and significant in jurisdictions where voters are involved in the local public finance process. In states where voters have a say in the level of taxation, the result of Column (2) suggests that the average treatment relates to a spread increase of 9.5 bps.

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 (Column [4]) showing an estimate of 112.4 bps. This result thus suggests that the main effect is more pronounced for local governments that face higher constraints for increasing debt financing.

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.[12] The coefficients in Column (5) suggest that for the average impacted county, the municipal bond yields increase by 7.5 bps and 26.4 bps for jurisdictions with majority and supermajority requirements, respectively. The results with regression weights (Column [6]) consistently show increasing coefficients, although they lack statistical power. The monotonically increasing patterns confirm that the additional risk is borne by investors who hold bonds in states with higher levels of residents' voting power in local public finance.

Because the coefficients using the shorter time period have smaller magnitudes (Table A8), which may indicate slow price adjustment, we provide in Figure 5 the treatment

[11] Table A8 reports the short-window sample results.

[12] Because most variation in our treatment measure is concentrated in the East of the U.S., we also verify the main results excluding jurisdictions to the West of the Mississippi which show similar results.

effect dynamics by computing the quarter treatment effects and separating the bonds based on the *Approval* indicator. We observe that the 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 are non-significant. These results point toward an increase in municipal bond yields when residents have both endured an increased cost for financing local public goods and are involved in the process of determining taxes and bond issuance.

## 4.4 A Border Analysis

To refine the interpretation of these state-level tests, and to address the potential differences between states that require different levels of voter support for municipal policies, 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 thus 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. We provide in Figure A4 the map of the counties paired within one unit. In this framework, we define treated units as bonds in counties that are on the side of the border that have more stringent voters approval rules. We regress this treated 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. Thus, we conduct a difference-in-differences analysis on a border-restricted sample where treatment is the side of the border with more voter inclusion in the municipal financing process.

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 first include state border group x year-month fixed effects. This absorbs any effects that are common to the local economy within a state-border pair over time that may impact municipal bond yields. For instance, this absorbs potential confounding effects related to the enactment of the TCJA at the local level. However, especially in urban or suburban areas, the economies are likely to differ somewhat on either side of the border, although not necessarily in a way that will systematically correlate with our voting treatment in a large sample. Nevertheless, to average out of this effect across our 25 state border pairs, we weigh each bond- (or bond-month-) level observation by one over the number of observations in the state border pair group in the pre- and post-periods. Thus, each bond within a group carries the same weight regardless of the frequency of trade within- and across-state pairs. This weighting scheme ensures that the largest areas, which may differ more across the state border do not disproportionately drive our estimates.

We report in Table 4 the results. In the first column, we show the baseline results using all border pairs and weighting trades such that each state-border pair has the same weight. The coefficient $\delta$ representing the main treatment effect is non-significant while the coefficient $\delta^{vote}$ is positive and significant at the 10% level. Its magnitude indicates that on the side of the border that has more stringent approval rules, the average treatment of 15 p.p. of $Chg.Itm_j$ increases bond yields by 37.7 bps in comparison to the yields on the other side of the border. We also note that adding a weighting restriction to ensure that each bond within a pair has the same weight in the identification (Column [2]) does not change the magnitude nor the significance of the results. In the last two columns, we restrict the sample to state-border pairs that include one side being a "no election" state. The magnitude of the coefficients in these specifications more than doubles, further confirming the role of voter involvement in setting risk. Compared to pure representative jurisdictions, localities that must seek voter approval for spending experienced an increase in bond yields following the shock to residents' subsidy in financing local public goods.

# 5 Robustness Checks

In the previous two sections, we provide evidence for a voter-induced municipal credit risk. We report in this section, several additional analyses to better establish the robustness of our empirical setting.

We start by estimating our main specification using spread over the MMA-curve as the dependent variable in Table A9. We observe smaller estimates on the interaction between $Post_t$ and $Chg.Itm_j$ in every specification, ranging from 29.6 bps to 47.5 bps in the longer window sample. Because the spread over the MMA-curve is on average lower, these estimates depict similar proportional effects of $Chg.Itm_j$ on the cost of debt for local governments. Next, in Table A10 we report the results using the share of itemizers in 2017 rather than the change in the ratios of itemizers to circumvent potential reverse-causality or simultaneity bias. All coefficient estimates are significant at the 1% level and depict consistent positive treatment effects ranging from 31.1 to 49.1 bps (mean share of itemizers in 2017 is 22.2%).

We also 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 SALT above the cap. This zero-marginal benefit for non-treated residents is not directly captured by our variable $Chg.Itm$ (although it is by the share of residents itemizing pre-TCJA of Table A10). We construct $Wasted.SALT_j$, defined as the dollar amount of SALT that could not be deducted because of the cap normalized by the number of tax returns in county $j$. Table A11 reports the results of our main specification using this measure interacted with $Post_t$ as alternative treatment variables. The results in the first two columns provide evidence that a larger wasted SALT deduction increases bond yields in a way that parallels our main proposition. In the next two columns, 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 paper dominate and are not linked to the intensive margin change emerging from the cap.

In Panel A of Table A12, we also show that our findings are similar in the absence of county-level controls. 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 TCJA provisions affect local economic growth then controlling for economic growth over time will understate the main effects as it will not capture this indirect effect. Because the coefficients are, on average, 6 bps larger when we exclude these controls, we can attribute 8-10% of the main effect to other provisions of the TCJA operating through post-policy impacts on local economic variables. In Panel B of A12, we include *Rating* as fixed effects in the main specification to verify whether part of the yield increase can be explained by a change in Rating. The coefficients $\hat{\delta}$ are all significant and of the same magnitude as the coefficients of the main regression.

In Table A13, we interact our main treatment intensity variable with an indicator for pre-TCJA high rating (Columns [1] and [2]) or a continuous measure of rating (Columns [3] and [4]). Consistent with the idea that the effects are in bonds that have some initial risk level, we find that the residents' shock is more pronounced in low-rating bonds. This result is also consistent with Pástor and Veronesi (2013), who find that policy-induced risk is greater during worse economic conditions.[13]

## 5.1 A Matching Approach

Because jurisdictions with a large share of treated residents differ on socio-economic dimensions from jurisdictions that are less impacted (as shown in Table 1), selection could bias our continuous treatment difference-in-differences estimates. This possibility is less likely because of the within-bond comparisons, however, it is still possible for these differences to violate the "strong" parallel assumption (Callaway et al., 2021).

To address this concern, we employ an entropy balancing procedure to weight juris-

[13] In non-tabulated results, we test whether municipal bond ratings are related to our treatment exposure variable using a continuous measure of bond rating as the dependent variable. Our analysis does not show any statistically significant changes in ratings related to the shock measure $Chg.Itm_j$.

dictions based on their pre-TCJA socio-economic variables to achieve covariate balances across groups (Hainmueller, 2012).[14] We define the treated group as bonds issued by jurisdictions with $Chg.Itm_j$ greater than the national mean of 19.5 p.p.. 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, employment, and labor participation growth rates.

We then estimate our preferred regression specification using the entropy balancing weights that correct for differences in pre-TCJA covariates means and report the results in Table 5. 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 coefficients are positive and significant at the 5% level at 5.1 bps and 4.1 bps with county and bond fixed effects, respectively. The last two columns show the estimated coefficients using the continuous treatment variables, which are also positive and significant at least at the 10% level. The coefficients, which are of lower magnitude than the main results, range from 38.2 and 47.0 bps.

## 5.2 Potential Confounding Effects Linked to TCJA

The TCJA included various fiscal changes for individuals such as reductions in tax rates across the income distribution, caps on mortgage interest and SALT deductions, and also an important reduction in corporate tax rates. Correlations between jurisdiction exposure to our treatment variable $Chg.Itm_j$ and their exposures to other provisions could

[14] Several studies in economics and finance use this methodology to correct for unbalanced covariates across groups (Guriev et al., 2021; Arifin et al., 2020; Pan et al., 2022; Hasan et al., 2021; Colak and Öztekin, 2021).

pose a threat to our proposition. We examine the extent of these potential confounding explanations in this subsection.

First, we examine potential endogeneity issues regarding the change in marginal tax rates that emerge from the TCJA. The marginal tax cuts change might be a threat to our identification if investors that benefited the most from the cuts also live in jurisdictions that score high on our treatment variable $Chg.Itm_j$, as a decrease in investors' marginal tax rates increases bond yields (Babina et al., 2021; Garrett et al., 2023).[15] Given the inclusion of state-year fixed effects, this confound would emerge only to the extent that investors invest more in their home counties relative to other parts of the state. To formally address this concern, we compute the change in the mean tax rates by dividing aggregate income taxes by taxable income in each county and note a correlation between the change in average tax rates and our $Chg.Itm_j$ measure of 0.08. Using this change in tax rates as a treatment variable, we test whether it explains the change in secondary market yields. In the first two columns of Panel A of Table A15, we introduce the interaction between change in mean tax rates and $Post_t$ and observe no significant effect. We then add both our main treatment variables 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-significant while the coefficients on the main treatment remain positive and significant at a similar magnitude to the main effects.[16]

Second, the TCJA also eliminated the tax exemption on advance-refunding bonds. Jurisdictions heavily reliant on advance-refunding bonds could face increased financing costs relative to others post-TCJA. This change could affect our results if jurisdictions inclined to use 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 reliance on advanced refunding bonds. This measure

[15] It is worth reemphasizing that the tax adjustment outlined in Equation (3) accounts for the tax advantage based on annual top-marginal tax rates and state deduction rules.

[16] To correct for possible measurement bias because we use average tax rates rather than marginal ones, we also provide robust evidence using mean tax rate changes for households earning more than $100,000 annual income, the likely bond investors. The coefficients, presented in Panel B of Table A15, depict similar results.

is constructed by dividing the total amount of advance-refunding bonds issued from 2005 to 2016 by the total amount of GO bonds. This measure averages 11.6% with large variations across counties (standard deviation = 13.9%). In Columns (1) and (2) of Table A16, 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 columns (3) and (4), we observe that the risk premium associated with the change in the share of itemizers does not change.[17]

Finally, we examine whether the support for President Trump confounds our main findings. We collect the share of votes for Trump at the county level 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 for Trump with the *Post* indicator and show in Table A17 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. However, we observe no significant differences between our main effects and the effects depicted with this additional treatment included.

### 5.3 A Fundamental Deterioration in Tax Base?

To the extent that the capitalization of the SALT deductions increases the jurisdictions' current tax base, residents will demand lower wages to compensate for the monetary benefits of the subsidy (Rosen, 1979; Roback, 1982), or buy properties at higher prices (Ambrose and Valentin, 2023b). As documented by previous research and shown in Table A3, housing values decreased in jurisdictions where residents were the most impacted by

[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.

the loss of fiscal deductions induced by the TCJA (Ambrose et al., 2022; Hembre and Dantas, 2022; Li and Yu, 2022). This change may lead to higher bond yields because property taxes represent 46% of the general own revenue sources for U.S. local governments (U.S. Census, 2020). With that said, the magnitude of the house price-municipal bond yield relation may be dampened due to tax assessments typically lagging market property values.

We conduct two sets of tests to examine whether a deterioration in the current jurisdiction tax base is a likely driver of our results. First, we augment our main specification with controls for house price level and house price growth using the ZHVI. To the extent that the effect of the residents' TCJA fiscal shock 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. If the coefficients remain similar, then it is likely that our findings are not primarily driven by changes in current municipal fundamentals. The results are in Panel A of Table 6. The coefficients on the interaction $Chg.Ded_j \times Post_t$ is positive, significant and of the same magnitude as our main results. If anything, their magnitude is larger. Thus, the results do not support the proposition that the downward pressure on housing prices drives our results. The lack of yield change following the house price decline can be attributed to a slow adjustment in property tax collection. This slow adjustment occurs because tax rates are often endogenously calculated, and property assessments do not occur annually. Consequently, these findings suggest that the tax base does not play a significant role in explaining our main effects.

Second, we study whether the bond yield responses to the decreased demand for local public goods extend to the state or school district levels. State bonds are not in our main sample and are typically funded through non-property tax sources. Thus, evidence of effects in that sample cast doubt on the already unlikely simple property value pass-through explanation for our findings. The first two columns of Panel B of Table 6 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 $\hat{\delta}$ varies from 250.4 to 278.0 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 of the first coefficient implies that bonds issued by Connecticut, the state the most impacted residents' loss of SALT deductions, experience a yield increase of 34.1 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 of Panel B, we show the results of school district bonds that we associate with $Chg.Itm_j$ computed at the school district level. These bonds are in our main sample, but matched to county-level changes in itemization, meaning that the connection between the issuing jurisdiction and its residents is now more perfectly aligned. 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 17.1 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 16.2 bps also significant at the 1% level. Though all types of taxes are deductible conditional on itemizing deductions, the results of this section provide evidence that the effects are not driven by a specific fiscal instrument but rather, we posit, by a decrease in residents' willingness to pay for local public goods due to the TCJA shock on federal tax deductions.

# 6 Conclusion

We use the change in residents' use of the SALT deductions, which increased their net cost for financing local public goods and services, to quantify the impact of resident-voters on the determination of municipal credit risk. Consistent with the proposition that changes to residents' demand for local public services impact municipal bond credit risk premiums, 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 the cost of financing for the average jurisdiction, which experienced a 15 p.p. increase in the share of residents deducting SALT, increased by 8 to 12 bps, equivalent to a 3.0-to-4.6% rise in the cost of debt. The SALT deduction allowance, which allows wealthier residents to benefit from less expensive local public services, thus additionally favors more affluent jurisdictions through external financing at a lower cost.

We then delve into heterogeneous effects based on differences in residents' political influence. 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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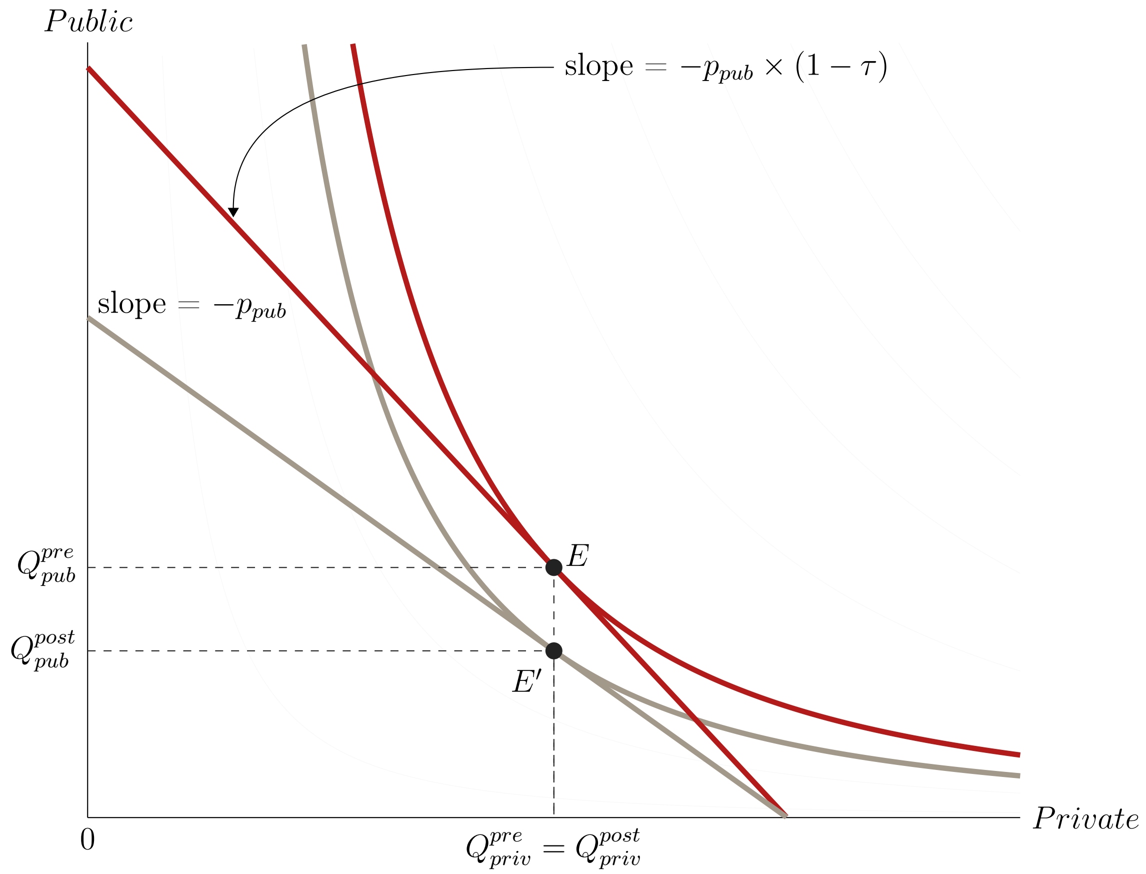
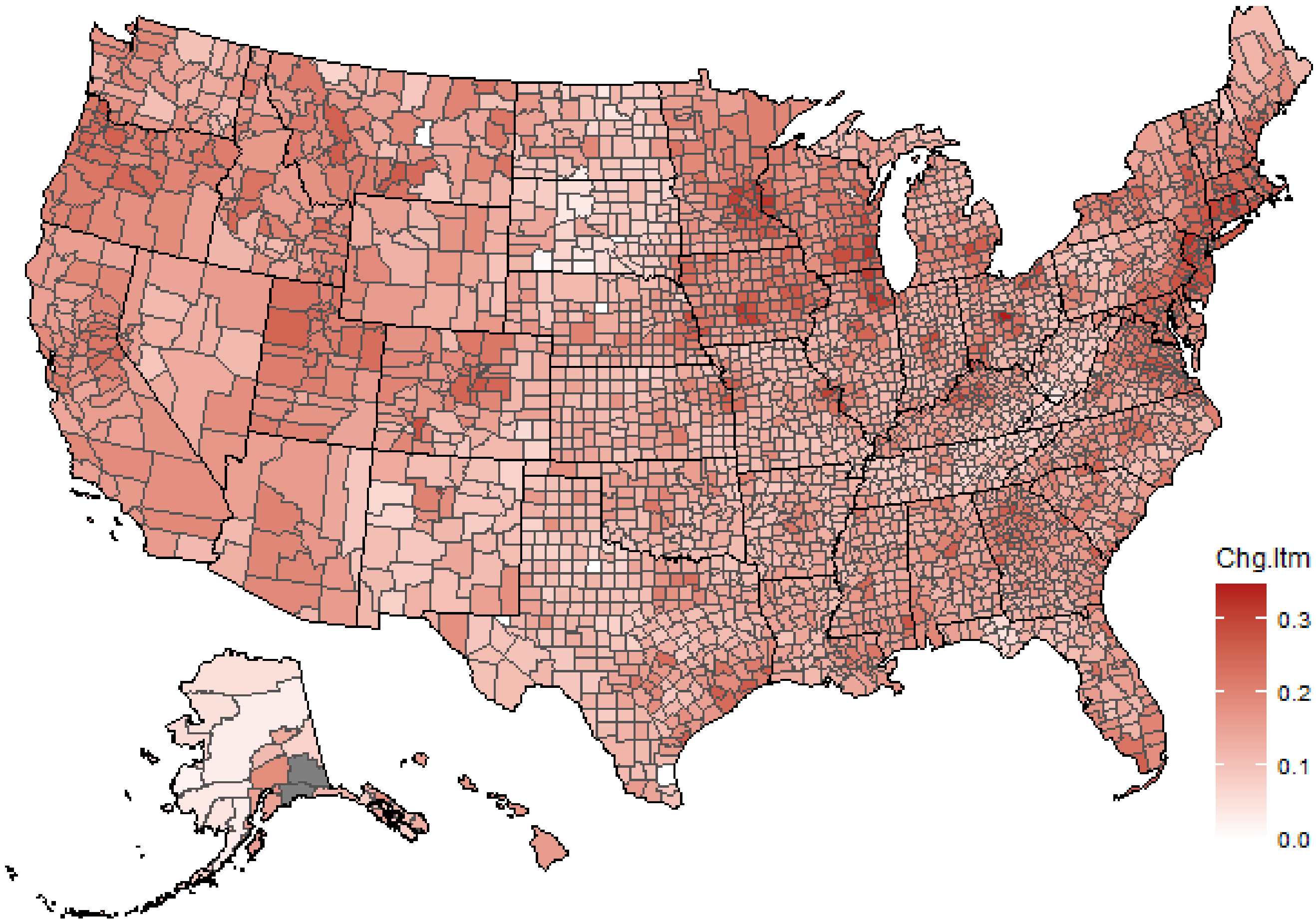
Figure 1: Change in SALT deduction status and demand for local public goods



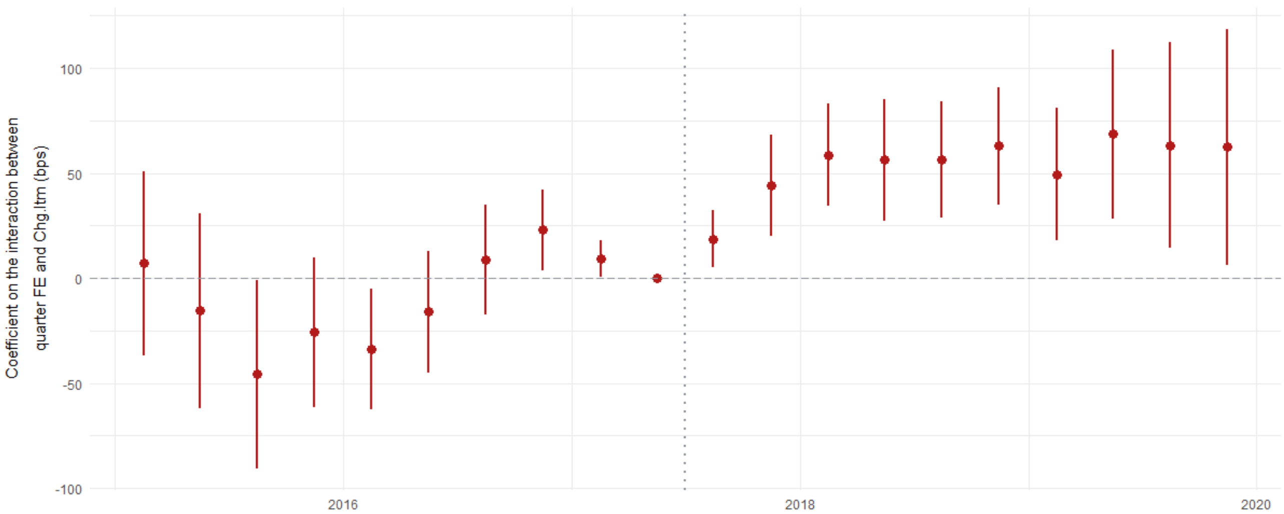

*Note: This graph theoretically shows the demand for local public goods before and after the TCJA fiscal change. It shows a change in the net-of-deduction price of local public goods of 30%. The utility function is defined by Cobb-Douglas over public and private goods with* $\alpha = 2/3$.

Figure 2: Change in share of itemizers by county



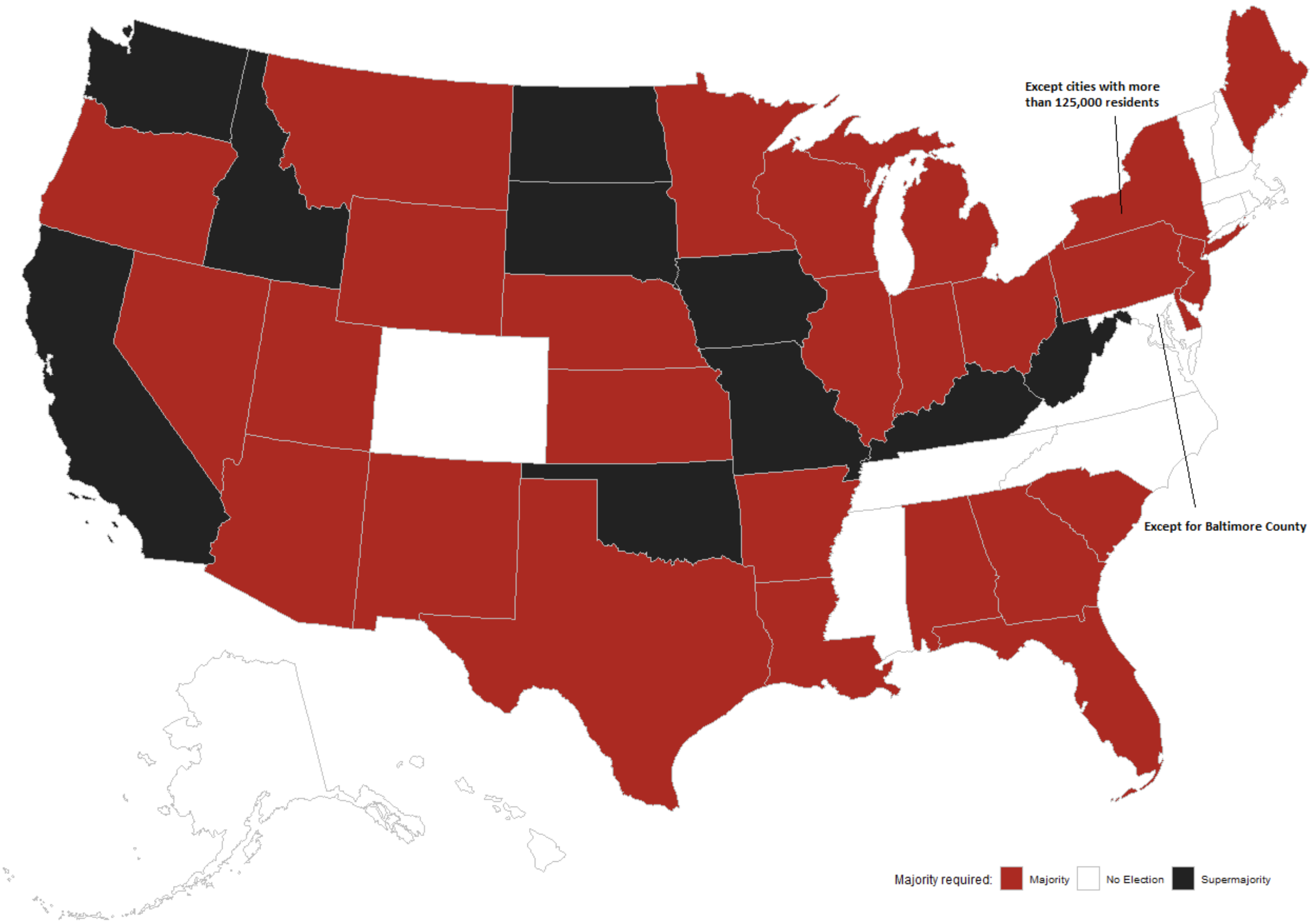

*Note: This map shows the geographical distribution of the decrease in the share of itemizers by U.S. county from 2017 (pre-TCJA) to 2018 (post-TCJA). Computation from the Statistics of Incomes of the IRS.*

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



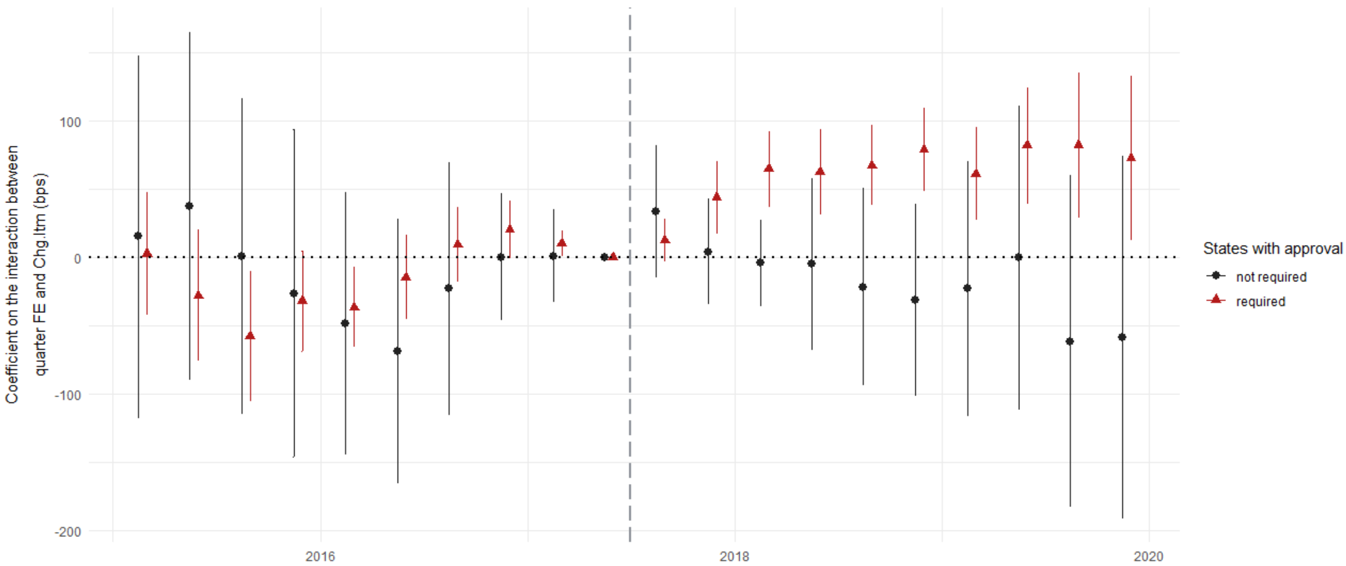

*Note: This graph shows the coefficient estimates on the interaction between quarter fixed effects and Chg.Itm on the tax-adjusted yields using all tax-exempt GO bonds issued before July 2017 and traded from 2015 to 2019. The dependent variable is the tax-adjusted spread over treasury yields. The regressions include state-by-month and bond fixed effects, bond characteristics, and lagged county demographics. The vertical bar shows the announcement of the TCJA (July 2017). The error bars show the 90% confidence interval using standard errors double-clustered at the county and trading month levels.*

Figure 4: Residents' majority required for bond and tax referendums




*Note: This map 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 B1.*

Figure 5: Dynamics effects by residents' involvement in local public finance




*Note: These graphs show the coefficients estimate on the interaction between quarter fixed effects and Chg.Itm on the tax-adjusted yields 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 effects for jurisdictions that do not require residents' approval and the red triangle dots are estimated using trades of bonds issued by jurisdictions that require residents' approval. The vertical bar shows the announcement of the TCJA (July 2017). The error bars show the 90% 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 municipal 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. 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 |
| School District bonds | 0.45 | 0.50 | 0 | 0.43 | 0.59 | −0.16 | −5.99*** |
| **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*** |

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

*This table reports the estimates of* $Spread_{i,j,t} = \alpha_{st} + \alpha_j + \delta\ Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$*.* $Spread_{i,j,t}$ *is the 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,* $\alpha_{st}$ *are state-by-month fixed effects,* $\alpha_j$ *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. The coefficient estimates for the control variables are shown in Table A4. In the first four columns, all trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used, while Column 5 uses issuance data from 2015 to 2019. In Columns (2) and (4), the observations are weighted by the number of trades for the same bond observed prior to the TCJA. The results in Panel B reproduce the results of Panel A by restricting the sample to bonds traded between July 2016 to December 2018 excluding trades between July and December 2017. 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.*

| **PANEL A:** | *Dependent variable: Spread (bps)* | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | Secondary market trades | | | | Issuance |
| $Post_t \times Chg.Itm_j$ | 59.10*** | 83.58*** | 54.03*** | 82.63*** | 63.03 |
| | (21.55) | (30.95) | (19.17) | (29.40) | (41.03) |
| State x Month FE | X | X | X | X | X |
| County FE | X | X | | | |
| Bond FE | | | X | X | |
| Cusip 6-digits | | | | | X |
| Bond characteristics | X | X | X | X | X |
| County-level control | X | X | X | X | X |
| Weighted trades | | X | | X | |
| Observations | 1,488,023 | 1,488,023 | 1,488,023 | 1,488,023 | 104,970 |
| $R^2$ | 0.64 | 0.65 | 0.93 | 0.93 | 0.92 |
| Adjusted $R^2$ | 0.63 | 0.65 | 0.92 | 0.92 | 0.92 |

| **PANEL B:** | *Dependent variable: Spread (bps)* | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | Secondary market trades | | | | Issuance |
| $Post_t \times Chg.Itm_j$ | 51.97** | 77.11*** | 37.23** | 50.59* | 112.60* |
| | (19.09) | (26.87) | (17.28) | (24.65) | (60.21) |
| State x Month FE | X | X | X | X | X |
| County FE | X | X | | | |
| Bond FE | | | X | X | |
| Cusip 6-digits | | | | | X |
| Bond characteristics | X | X | X | X | X |
| County-level control | X | X | X | X | X |
| Weighted trades | | X | | X | |
| Observations | 628,911 | 628,911 | 628,911 | 628,911 | 40,120 |
| $R^2$ | 0.64 | 0.65 | 0.96 | 0.96 | 0.93 |
| Adjusted $R^2$ | 0.64 | 0.64 | 0.95 | 0.95 | 0.93 |

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

*This table reports the estimates of* $Spread_{i,j,t} = \delta(Post_t \times Chg.Itm_j) + \delta^{vote}(Post_t \times Chg.Itm_j \times Approval_j) + \alpha_t + \alpha_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \eta(Post_t \times Approval_j) + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *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,* $\alpha_{st}$ *are state-by-month fixed effects,* $\alpha_j$ *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) |
| $Post_t \times Chg.Itm_j$ | 3.05 | 63.26*** | −31.69 | −15.19 | −28.68 | −13.72 |
| | (25.98) | (20.96) | (27.01) | (42.67) | (26.28) | (41.71) |
| ... x Approval | | | 97.84*** | 112.39** | | |
| | | | (36.44) | (55.55) | | |
| ... x Majority states | | | | | 78.82** | 103.70* |
| | | | | | (31.99) | (52.76) |
| ... x Supermajority states | | | | | 194.48* | 149.83 |
| | | | | | (107.65) | (149.98) |
| 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 | 262,699 | 1,225,324 | 1,488,023 | 1,488,023 | 1,488,023 | 1,488,023 |
| $R^2$ | 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* $Spread_{i,j,t} = \alpha_{st} + \alpha_i + \delta\ Post_t \times Chg.Itm_j + \beta X_{i,t} + \delta^{vote}(TreatedBorder_j \times Post_t \times Chg.Itm_j) + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *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,* $TreatedBorder_j$ *equals one for bonds located on the side of a state border with a higher level of residents' involvement in the local public finance process,* $\alpha_{st}$ *are state-border times month of trade fixed effects,* $\alpha_i$ *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 July 2016 to December 2018 excluding trades between July and December 2017 from counties located at a state border 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 | |
| $Post_t \times Chg.Itm_j$ | −95.59 | −80.20 | −427.64 | −320.45 |
| | (144.37) | (118.87) | (308.90) | (254.81) |
| $TreatedBorder \times Post_t$ | −59.78* | −53.34** | −137.57** | −109.94* |
| | (30.26) | (25.32) | (65.82) | (54.89) |
| $TreatedBorder \times Post_t \times Chg.Itm_j$ | 251.64* | 231.34* | 569.25* | 465.93* |
| | (140.03) | (118.27) | (290.51) | (245.61) |
| StateBorder x month FE | X | X | X | X |
| Bond FE | X | X | X | X |
| Bond characteristics | X | X | X | X |
| County-level controls | X | X | X | X |
| Weights | Border | Border + Bond | Border | Border + Bond |
| Observations | 138,167 | 138,167 | 71,649 | 71,649 |
| $R^2$ | 0.96 | 0.97 | 0.96 | 0.97 |
| Adjusted $R^2$ | 0.95 | 0.96 | 0.95 | 0.96 |

Table 5: Weighted Least Squared regression estimates with Entropy Balancing Weights

*This table reports the estimates of* $Spread_{i,j,t} = \alpha_{st} + \alpha_j + \delta\ Post_t \times Treat_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *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,* $\alpha_{st}$ *are state-by-month fixed effects,* $\alpha_j$ *are county or bond fixed effects as described in the table, 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. 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) |
| $Post_t \times Treated_j$ | 5.07** | 4.66** | | |
| | (2.21) | (2.28) | | |
| $Post_t \times Chg.Itm_j$ | | | 38.22* | 47.03** |
| | | | (22.03) | (20.40) |
| State x Month FE | X | X | X | X |
| County FE | X | | X | |
| Bond FE | | 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 |
| $R^2$ | 0.65 | 0.93 | 0.65 | 0.93 |
| Adjusted $R^2$ | 0.65 | 0.92 | 0.65 | 0.92 |

Table 6: Residents' shock, property tax base, and municipal bond yields

*This table reports the estimates of* $Spread_{i,j,t} = \alpha_{st} + \alpha_j + \delta \; Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the 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,* $\alpha_t$ *and* $\alpha_j$ *are time and spatial 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.*

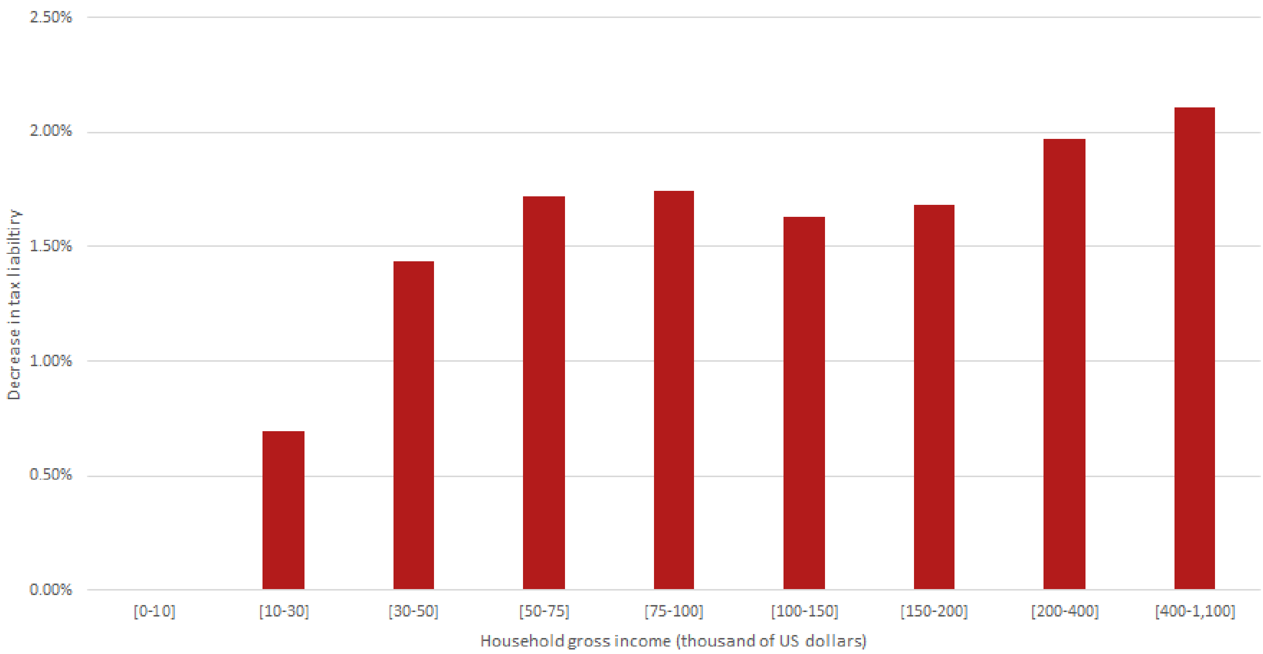
| **PANEL A:** | *Dependent variable: Spread (bps)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post_t \times Chg.Itm_j$ | 68.25*** | 88.08*** | 55.24*** | 84.72*** |
| | (21.99) | (32.30) | (19.79) | (30.30) |
| State x Month FE | X | X | X | X |
| County FE | X | X | | |
| Bond FE | | | X | X |
| House value (log) and growth | X | X | X | X |
| Bond characteristics | X | X | X | X |
| County-level control | X | X | X | X |
| Weighted trades | | X | | X |
| Observations | 1,488,023 | 1,488,023 | 1,473,124 | 1,473,124 |
| $R^2$ | 0.93 | 0.93 | 0.93 | 0.93 |
| Adjusted $R^2$ | 0.92 | 0.92 | 0.92 | 0.91 |

| **PANEL B:** | *Dependent variable: Spread (bps)* | | | |
|---|---|---|---|---|
| | State bonds | | School district bonds | |
| | (1) | (2) | (3) | (4) |
| $Post_t \times Chg.Itm_j$ | 250.39*** | 274.94*** | 17.10*** | 16.17*** |
| | (33.91) | (36.55) | (4.29) | (6.10) |
| 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 | 164,496 | 164,496 | 675,592 | 675,592 |
| $R^2$ | 0.93 | 0.93 | 0.93 | 0.92 |
| Adjusted $R^2$ | 0.92 | 0.92 | 0.92 | 0.91 |

# Internet Appendix

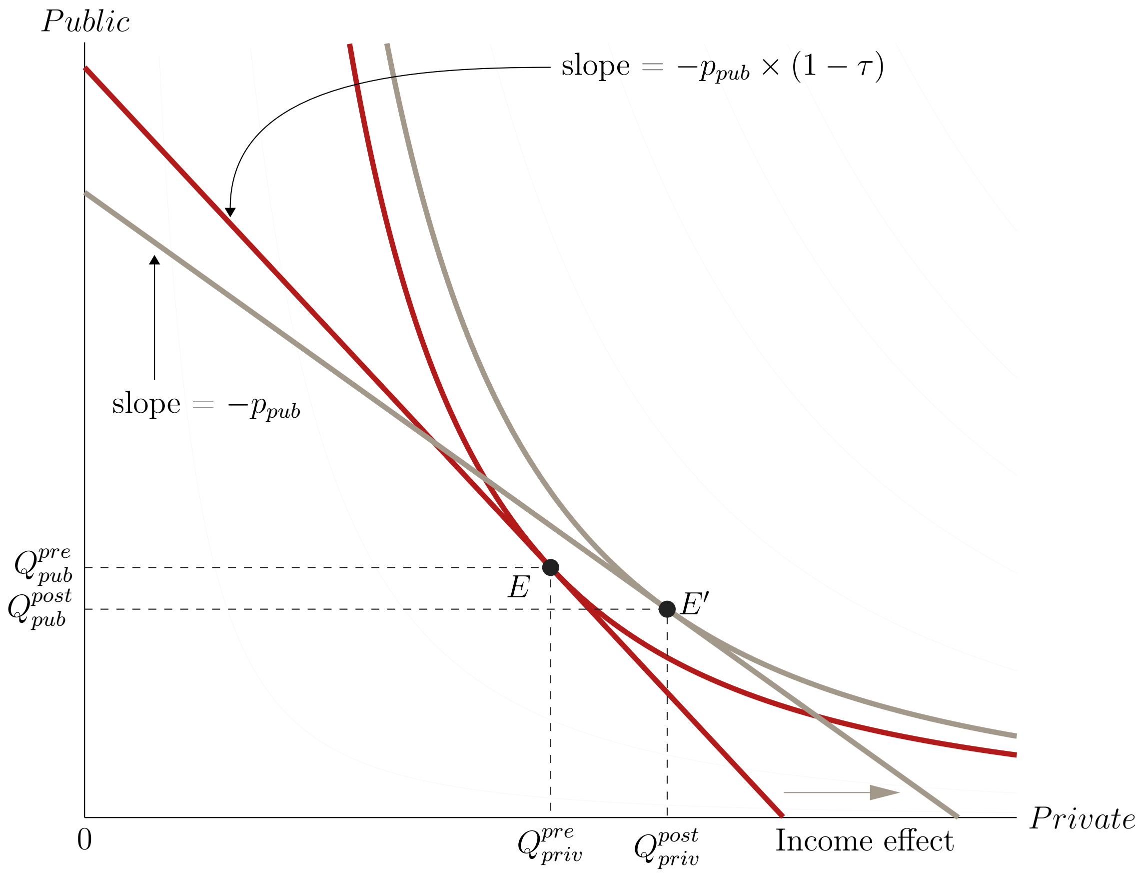
## A Additional Figures & Tables

Figure A1: TCJA implied income effects from change in tax liability



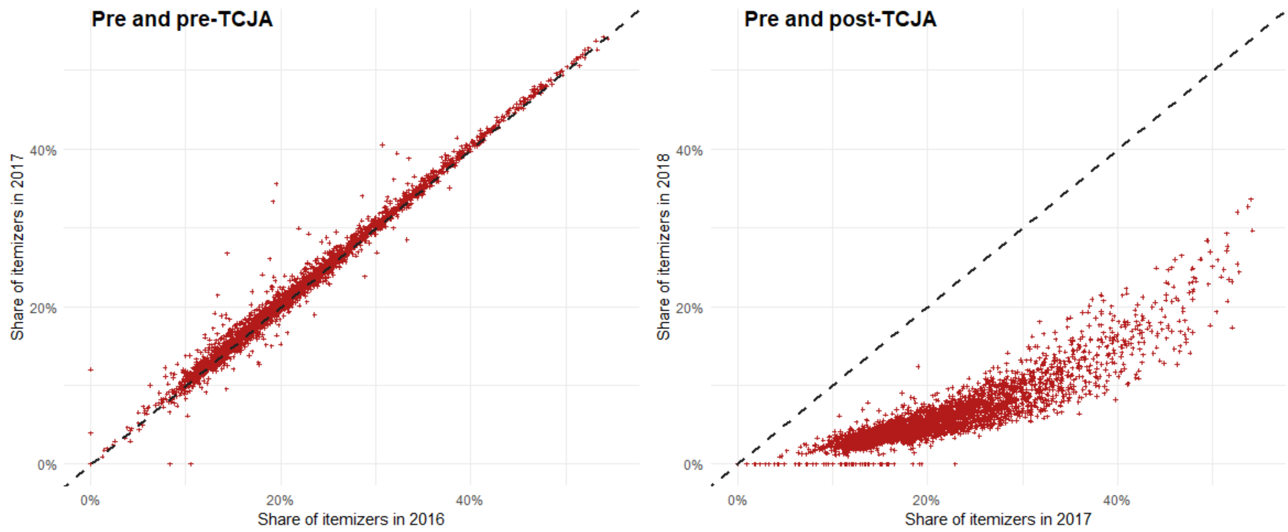

*Note: These bars show the decrease in tax liability from the TCJA in the percentage of income. The data is compiled from Table (4) of Ambrose et al. (2022).*

Figure A2: TCJA fiscal change and demand for local public goods with possible income effects

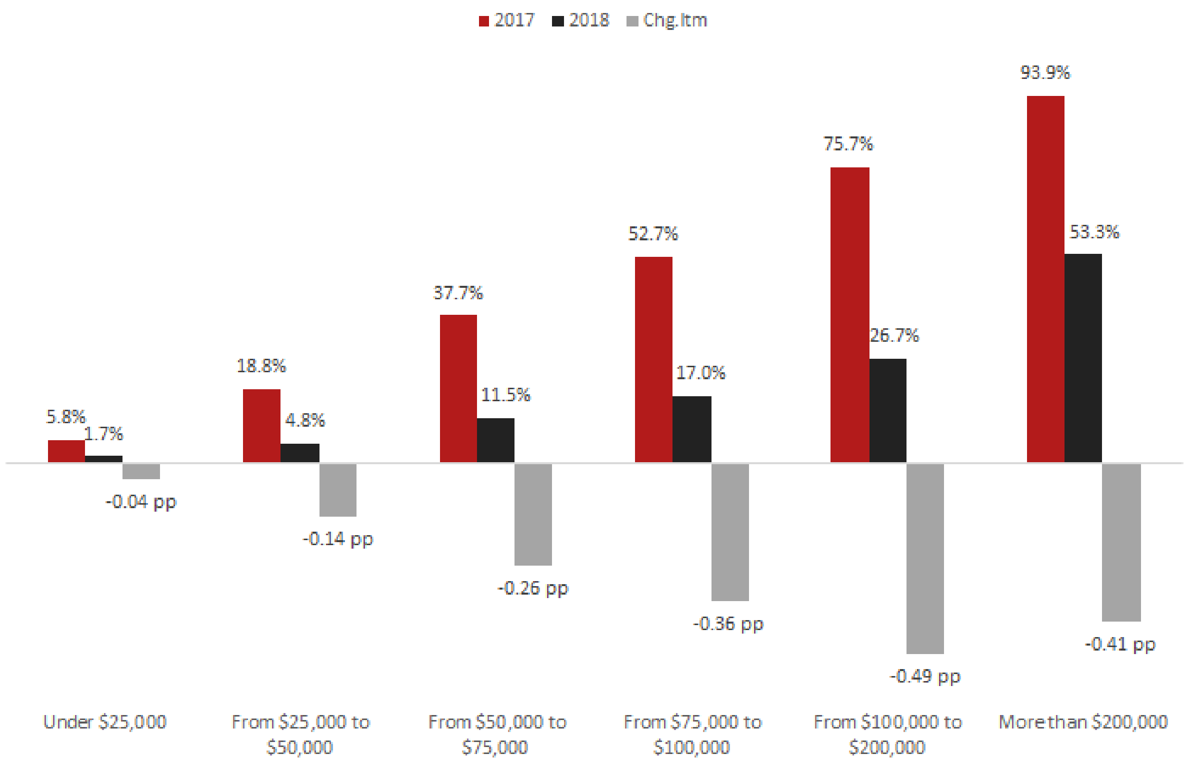



*Note: This graph theoretically shows the demand for local public goods before and after the TCJA fiscal change. It shows a change in the net-of-deduction price of local public goods of 30% and an increase in income of 20%, resulting from other TCJA provisions.*

Figure A3: Variations in fiscal shock to residents



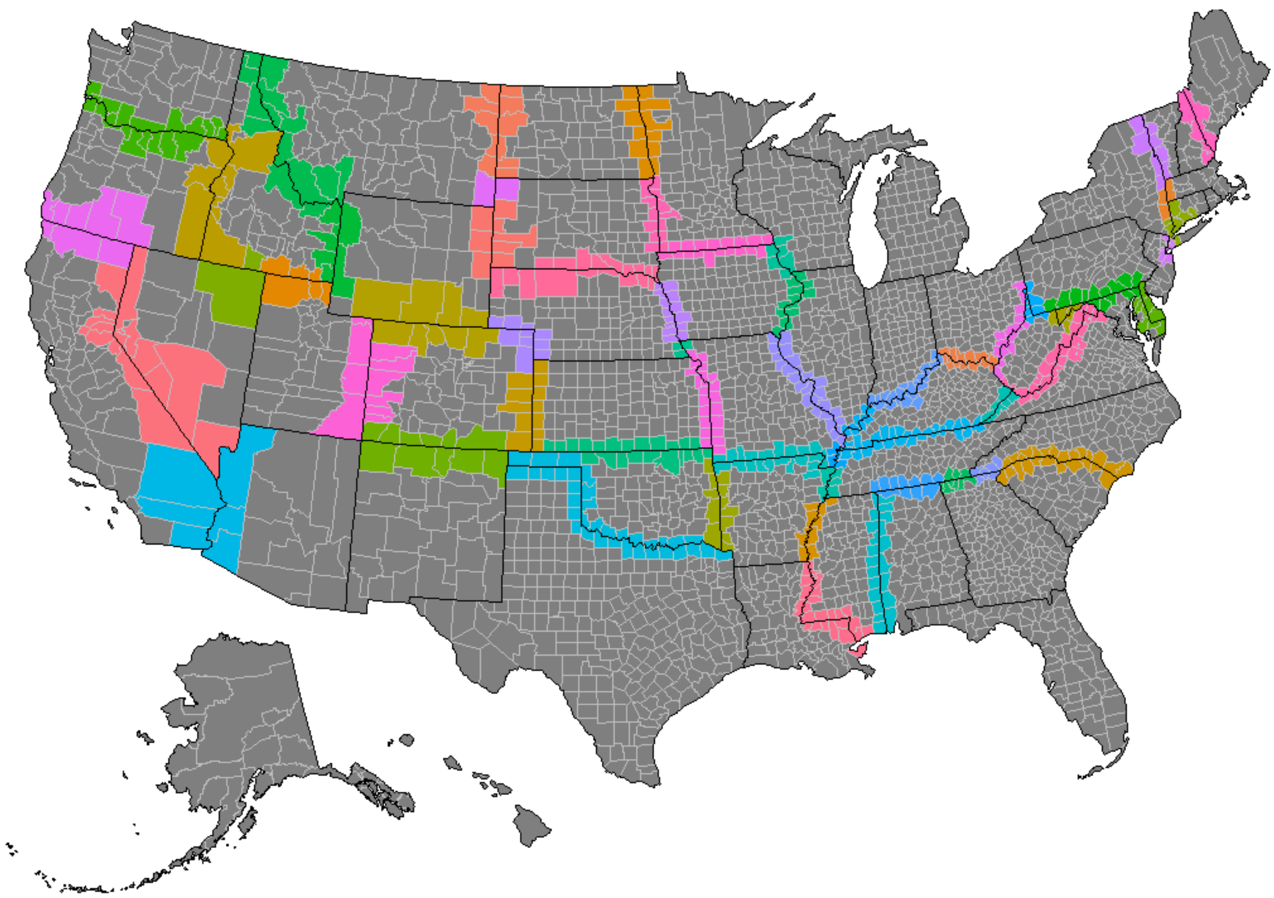

(a) Temporal variations




(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 graphs show 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 A4: 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.*

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: 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 A4: Local fiscal shock and municipal bonds spreads - full set of coefficients

*This table reports the estimates of* $Spread_{i,j,t} = \alpha_{st} + \alpha_j + \delta\ Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the 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,* $\alpha_{st}$ *are state-by-month fixed effects,* $\alpha_j$ *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. In the first four columns, all trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used, while Column 5 uses issuance data from 2015 to 2019. In Columns (2) and (4), the observations are weighted by the number of trades for the same bond observed prior to the TCJA. The results in Panel B reproduce the results of Panel A by restricting the sample to bonds traded between July 2016 to December 2018 excluding trades between July and December 2017. 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) |
| | Secondary market trades | | | | Issuance |
| $Post_t \times Chg.Itm_j$ | 59.10*** | 83.58*** | 54.03*** | 82.63*** | 63.03 |
| | (21.55) | (30.95) | (19.17) | (29.40) | (41.03) |
| Inverse maturity | 20.64*** | 15.89* | 93.84*** | 92.22*** | 10.03*** |
| | (5.76) | (8.28) | (8.36) | (10.18) | (3.42) |
| Treasury Rate | −0.20*** | −0.14*** | −0.59*** | −0.61*** | 0.04 |
| | (0.03) | (0.04) | (0.04) | (0.05) | (0.04) |
| Coupon | 14.91*** | 13.24*** | | | −11.63*** |
| | (2.03) | (2.57) | | | (1.34) |
| Maturity | 8.55*** | 8.67*** | | | 11.70*** |
| | (0.32) | (0.42) | | | (0.26) |
| Size (log) | 1.77 | 1.89 | | | −5.66*** |
| | (1.61) | (1.74) | | | (0.57) |
| Callable | 164.71*** | 167.96*** | | | 46.58*** |
| | (4.71) | (6.35) | | | (1.86) |
| Insured | 86.15*** | 85.16*** | | | 2.63 |
| | (5.20) | (6.84) | | | (2.91) |
| Reoffer | −18.75*** | −11.20* | | | −18.67*** |
| | (4.35) | (6.17) | | | (3.21) |
| Negotiated | 14.42*** | 24.18*** | | | 13.45*** |
| | (3.87) | (6.29) | | | (4.18) |
| Population (log) | −115.17*** | −180.34*** | −113.08** | −166.97** | −194.24*** |
| | (40.95) | (58.19) | (45.49) | (75.87) | (70.33) |
| Income per capita | 0.001*** | 0.001*** | 0.001*** | 0.001*** | 0.0004 |
| | (0.0002) | (0.0002) | (0.0002) | (0.0002) | (0.0003) |
| Population growth | 45.71 | 33.62 | −137.53* | −253.41** | 252.89 |
| | (89.11) | (115.36) | (76.36) | (114.29) | (168.21) |
| Employment growth | −57.88** | −60.52 | −57.19** | −76.19** | −35.97 |
| | (27.50) | (38.88) | (24.59) | (37.51) | (68.76) |
| Labor participation | 45.50 | 66.71 | 56.33 | 94.79 | −103.28 |
| | (49.52) | (71.16) | (47.64) | (74.16) | (107.08) |
| Four proceeds categorical fixed effects | X | X | X | X | X |
| State x Month FE | X | X | X | X | X |
| County FE | X | X | | | |
| Bond FE | | | X | X | |
| Cusip 6-digits | | | | | X |
| Bond characteristics | X | X | X | X | X |
| County-level control | X | X | X | X | X |
| Weighted trades | | X | | X | |
| Observations | 1,488,023 | 1,488,023 | 1,488,023 | 1,488,023 | 104,970 |
| $R^2$ | 0.64 | 0.65 | 0.93 | 0.93 | 0.92 |
| Adjusted $R^2$ | 0.63 | 0.65 | 0.92 | 0.92 | 0.92 |

Table A5: Placebo tests

*This table reports the estimates of* $Spread_{i,j,t} = \delta\,(Post_t \times Chg.Itm_j) + \alpha_{st} + \alpha_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *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 *before and after the TCJA,* $\alpha_{st}$ *are state-by-month fixed effects,* $\alpha_j$ *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 use 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 | −9.78 | −3.17 | −18.21 | −33.68 | −85.41 | 2.77 | −19.62 |
| | (19.95) | (26.56) | (20.64) | (27.05) | (38.43) | (52.95) | (25.50) | (37.95) |
| 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 | 0.86 | 0.92 | 0.91 | 0.90 | 0.89 | 0.92 | 0.91 |
| Adjusted $R^2$ | 0.84 | 0.83 | 0.90 | 0.89 | 0.88 | 0.87 | 0.91 | 0.89 |

Table A6: Differences in bonds characteristics based on required approval indicator

*This table reports the summary statistics of tax-exempt GO bonds traded from 2015 until the TCJA announcement (July 2017). 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. The means of each variable for jurisdictions that differ in their degree of resident's involvement in the local public finance are provided in Columns (5) and (6).*

| | Mean | Std. dev. | Median | Approval | Non-approval |
|---|---|---|---|---|---|
| **Main variables:** | | | | | |
| Spread (bps) | 273.90 | 168.10 | 242.00 | 279.23 | 247.27 |
| Spread MMA (bps) | 92.83 | 80.80 | 68.02 | 94.78 | 83.10 |
| Chg.Itm (%) | 0.21 | 0.05 | 0.21 | 0.20 | 0.22 |
| **Bond-level control variables:** | | | | | |
| Rating | 18.33 | 1.95 | 18.50 | 18.21 | 18.94 |
| Coupon | 3.60 | 1.31 | 3.98 | 3.55 | 3.86 |
| Maturity | 8.15 | 5.73 | 6.88 | 8.30 | 7.39 |
| Amount (000s) | 2,396.37 | 8,588.05 | 1,004.63 | 2,308.13 | 2,837.30 |
| Callable | 0.57 | 0.50 | 0.50 | 0.57 | 0.54 |
| Insured | 0.31 | 0.46 | 0 | 0.33 | 0.20 |
| Reoffer | 0.15 | 0.35 | 0 | 0.15 | 0.13 |
| Negotiated | 0.38 | 0.48 | 0 | 0.40 | 0.27 |
| School District bonds | 0.45 | 0.50 | 0 | 0.52 | 0.10 |
| Issue Year | 2,011.68 | 3.75 | 2,011.50 | 2,011.69 | 2,011.60 |
| **County-level control variables:** | | | | | |
| Income per capita (000s) | 52.59 | 17.30 | 48.97 | 50.94 | 60.87 |
| Population growth | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 |
| Employment growth | 0.02 | 0.02 | 0.02 | 0.02 | 0.02 |
| Labor participation | 0.75 | 0.06 | 0.75 | 0.75 | 0.77 |

Table A7: Municipal bond yields and residents' involvement in municipal finance

*This table reports the estimates of* $Spread_{i,j,t} = \alpha_t + Approval_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the tax-adjusted spread over the maturity-matched treasury yield,* $Approval_j$ *are indicators for jurisdiction in which residents' approval for local taxes and bonds are required,* $\alpha_t$ *are month fixed effects,* $X_{i,t}$ *are bond level controls and* $Z_{j,t-1}$ *are lagged county-level characteristics. Only issued bonds prior to the TCJA announcement (July 2017) are used. 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, presented 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 A8: Residents' involvement, fiscal shock, and municipal bond yields - Short-window results

*This table reports the estimates of* $Spread_{i,j,t} = \delta(Post_t \times Chg.Itm_j) + \delta^{vote}(Post_t \times Chg.Itm_j \times Approval_j) + \alpha_t + \alpha_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \eta(Post_t \times Approval_j) + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *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,* $\alpha_{st}$ *are state-by-month fixed effects,* $\alpha_j$ *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 or not. 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 of tax-exempt GO bonds issued before the TCJA announcement and traded from July 2016 to December 2018 excluding trades from July to December 2017 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) |
| $Post_t \times Chg.Itm_j$ | −26.53 | 50.33** | −19.84 | −8.59 | −20.81 | −12.09 |
| | (23.22) | (19.07) | (28.60) | (44.46) | (28.25) | (43.00) |
| ... x Approval | | | 65.41* | 69.82 | | |
| | | | (32.50) | (50.47) | | |
| ... x Majority states | | | | | 71.32** | 92.00* |
| | | | | | (33.22) | (51.13) |
| ... x Supermajority states | | | | | 37.24 | −20.39 |
| | | | | | (51.86) | (77.65) |
| 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 | 111,586 | 517,325 | 628,911 | 628,911 | 628,911 | 628,911 |
| $R^2$ | 0.96 | 0.96 | 0.96 | 0.96 | 0.96 | 0.96 |
| Adjusted $R^2$ | 0.95 | 0.95 | 0.95 | 0.95 | 0.95 | 0.95 |

Table A9: Local fiscal shock and municipal bonds spreads - Using MMA

*This table reports the estimates of* $Spread\_MMA_{i,j,t} = \alpha_{st} + \alpha_j + \delta\ Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. *$Spread\_MMA_{i,j,t}$ is the traded municipal bond spread over the MMA-curve 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, $\alpha_{st}$ are state-by-month fixed effects, $\alpha_j$ 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 Columns (2) and (4), the observations are weighted by the number of trades for the same bond observed prior to the TCJA. The results in Panel B reproduce the results of Panel A by restricting the sample to bonds traded between July 2016 to December 2018 excluding trades between July and December 2017. 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.*

| **PANEL A:** | *Dependent variable: Spread over MMA-curve (bps)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post_t \times Chg.Itm_j$ | 29.95** | 46.28*** | 29.58*** | 47.50*** |
| | (11.25) | (16.12) | (10.36) | (15.84) |
| State x Month FE | X | X | X | X |
| County FE | X | X | | |
| Bond FE | | | X | X |
| Bond characteristics | X | X | X | X |
| County-level control | X | X | X | X |
| Weighted trades | | X | | X |
| Observations | 1,488,023 | 1,488,023 | 1,488,023 | 1,488,023 |
| $R^2$ | 0.48 | 0.51 | 0.90 | 0.89 |
| Adjusted $R^2$ | 0.48 | 0.51 | 0.88 | 0.87 |

| **PANEL B:** | *Dependent variable: Spread over MMA-curve (bps)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post_t \times Chg.Itm_j$ | 24.09** | 37.70** | 16.99* | 24.93* |
| | (9.64) | (13.57) | (8.54) | (12.19) |
| State x Month FE | X | X | X | X |
| County FE | X | X | | |
| Bond FE | | | X | X |
| Bond characteristics | X | X | X | X |
| County-level control | X | X | X | X |
| Weighted trades | | X | | X |
| Observations | 628,911 | 628,911 | 628,911 | 628,911 |
| $R^2$ | 0.47 | 0.51 | 0.95 | 0.95 |
| Adjusted $R^2$ | 0.47 | 0.51 | 0.93 | 0.93 |

Table A10: Secondary market results using pre-TCJA share of itemizers

*This table reports the estimates of* $Spread_{i,j,t} = \alpha_{st} + \alpha_j + \delta\ Post_t \times Itm2017_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county j and traded at month t,* $Itm2017_j$ *is the share of itemizers in county j in 2017,* $Post_t$ *equals 1 for bonds traded after July 2017,* $\alpha_{st}$ *are state-by-month fixed effects,* $\alpha_j$ *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 Columns (2) and (4), the observations are weighted by the number of trades for the same bond observed prior to the TCJA. The results in Panel B reproduce the results of Panel A by restricting the sample to bonds traded between July 2016 to December 2018 excluding trades between July and December 2017.. 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.*

| **PANEL A:** | *Dependent variable: Spread (bps)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post_t \times Itm2017_j$ | 31.12*** | 40.27*** | 35.26*** | 49.16*** |
| | (10.14) | (13.97) | (8.71) | (12.86) |
| State x Month FE | X | X | X | X |
| County FE | X | X | | |
| Bond FE | | | X | X |
| Bond characteristics | X | X | X | X |
| County-level control | X | X | X | X |
| Weighted trades | | X | | X |
| Observations | 1,488,023 | 1,488,023 | 1,488,023 | 1,488,023 |
| $R^2$ | 0.64 | 0.65 | 0.93 | 0.93 |
| Adjusted $R^2$ | 0.63 | 0.65 | 0.92 | 0.92 |

| **PANEL B:** | *Dependent variable: Spread (bps)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post_t \times Itm2017_j$ | 21.53** | 23.80 | 21.78** | 24.42* |
| | (9.01) | (14.75) | (7.87) | (12.95) |
| State x Month FE | X | X | X | X |
| County FE | X | X | | |
| Bond FE | | | X | X |
| Bond characteristics | X | X | X | X |
| County-level control | X | X | X | X |
| Weighted trades | | X | | X |
| Observations | 628,911 | 628,911 | 628,911 | 628,911 |
| $R^2$ | 0.64 | 0.65 | 0.96 | 0.96 |
| Adjusted $R^2$ | 0.64 | 0.64 | 0.95 | 0.95 |

Table A11: Change in itemizers, SALT cap, and municipal bond spread

*This table reports the estimates of* $Spread_{i,j,t} = \alpha_{st} + \alpha_j + \delta\ Post_t \times Waster.SALT_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. *$Spread_{i,j,t}$ is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county j and traded at month t, $Wasted.SALT_j$ is the dollar amount of SALT that could not be deducted because of the cap normalized by the number of tax returns in county* j, *$Post_t$ equals 1 for bonds traded after July 2017, $\alpha_{st}$ are state-by-month fixed effects, $\alpha_j$ 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 bonds issued before the TCJA announcement are used. In Columns (2), and (4) the observations are weighted by the number of trades for the same bond observed prior to the TCJA. The results in Columns (3) and (4) additionally include the interaction $Chg.Itm_j \times Post_t$. 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) |
| $Post_t \times Wasted.SALT_j$ | 0.001** | 0.001** | 0.001 | 0.001 |
| | (0.0004) | (0.0004) | (0.0004) | (0.0004) |
| $Post_t \times Chg.Itm_j$ | | | 49.01** | 75.88** |
| | | | (19.86) | (30.78) |
| 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 | | X | | X |
| Observations | 1,485,828 | 1,485,828 | 1,485,828 | 1,485,828 |
| $R^2$ | 0.93 | 0.93 | 0.93 | 0.93 |
| Adjusted $R^2$ | 0.92 | 0.91 | 0.92 | 0.91 |

Table A12: Evaluating the impact of socio-economic factors and ratings on main results

*This table reports the estimates of* $Spread_{i,j,t} = \alpha_{st} + \alpha_j + \delta\ Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the 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,* $\alpha_{st}$ *are state-by-month fixed effects,* $\alpha_j$ *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 Columns (2) and (4), the observations are weighted by the number of trades for the same bond observed prior to the TCJA. In Panel A, the regressions do not include* $Z_{j,t-1}$*, while in Panel B* $X_{i,t}$ *includes bond rating 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.*

| **Panel A:** | *Dependent variable: Spread (bps)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post_t \times Chg.Itm_j$ | 65.08** | 83.20** | 65.13** | 90.12** |
| | (26.76) | (37.37) | (27.53) | (40.32) |
| State x Month FE | X | X | X | X |
| County FE | X | X | | |
| Bond FE | | | X | X |
| Bond characteristics | X | X | X | X |
| Weighted trades | | X | | X |
| Observations | 1,488,871 | 1,488,871 | 1,488,871 | 1,488,871 |
| $R^2$ | 0.64 | 0.65 | 0.93 | 0.93 |
| Adjusted $R^2$ | 0.63 | 0.65 | 0.92 | 0.91 |

| **Panel B:** | *Dependent variable: Spread (bps)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post_t \times Chg.Itm_j$ | 70.75*** | 111.42*** | 56.14*** | 84.00*** |
| | (21.90) | (31.28) | (17.90) | (26.32) |
| State x Month FE | X | X | X | X |
| County FE | X | X | | |
| Bond FE | | | X | X |
| Bond characteristics | X | X | X | X |
| Rating FE | X | X | X | X |
| County-level controls | X | X | X | X |
| Weighted trades | | X | | X |
| Observations | 1,488,023 | 1,488,023 | 1,488,023 | 1,488,023 |
| $R^2$ | 0.66 | 0.68 | 0.93 | 0.93 |
| Adjusted $R^2$ | 0.66 | 0.68 | 0.92 | 0.92 |

Table A13: Bond pre-TCJA rating, change in itemizers, and municipal bond spread

*This table reports the estimates of* $Spread_{i,j,t} = \delta(Post_t \times Chg.Itm_j) + \delta^{rating}(Post_t \times Chg.Itm_j \times Rating_j) + \alpha_{st} + \alpha_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \eta(Post_t \times Rating_j) + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *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,* $\alpha_{st}$ *are state-by-month fixed effects,* $\alpha_j$ *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. In Columns (1) and (2),* $Rating_j$ *is an indicator that equals one if the pre-TCJA is greater than the median, and in Columns (3) and (4) is a standardized continuous measure of pre-TCJA mean bond rating. All trades from 2015 to 2019 of tax-exempt GO bonds issued before the TCJA announcement are used. In Columns (2), and (4) the observations are weighted by the number of trades for the same bond observed prior to the TCJA. 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) |
| $Post_t \times Chg.Itm_j$ | 120.09** | 213.80** | 41.91*** | 57.34** |
| | (48.61) | (97.16) | (14.97) | (22.65) |
| $HighRating \times Post_t \times Chg.Itm_j$ | −118.73* | −236.79 | | |
| | (68.14) | (147.58) | | |
| $Rating(standardized) \times Post_t \times Chg.Itm_j$ | | | −45.06* | −81.19** |
| | | | (24.72) | (39.86) |
| 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 | | 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 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 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) | 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: Change in individual tax rates and municipal bond spread

*This table reports the estimates of* $Spread_{i,j,t} = \alpha_{st} + \alpha_j + \delta\ Post_t \times Chg.TaxRate_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county j and traded at month t,* $Chg.TaxRate_j$ *is the percentage point change over the TCJA in average tax rates in county* j, $Post_t$ *equals 1 for bonds traded after July 2017,* $\alpha_{st}$ *are state-by-month fixed effects,* $\alpha_j$ *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 Columns (2) and (4), the observations are weighted by the number of trades for the same bond observed prior to the TCJA. Panel B reproduces the results of Panel A by using the percentage point change in average tax rates calculated only for taxpayers with gross income larger than $100,000. 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.*

| **PANEL A:** | *Dependent variable: Spread (bps)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post_t \times Chg.TaxRate_j$ | −141.56 | −567.47 | 5.61 | −356.62 |
| | (237.15) | (347.61) | (244.64) | (350.74) |
| $Post_t \times Chg.Itm_j$ | | | 54.18** | 72.81** |
| | | | (20.89) | (31.32) |
| 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 | | 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 | 0.91 | 0.92 | 0.92 |

| **PANEL B:** | *Dependent variable: Spread (bps)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post_t \times Chg.TaxRate.HighIncome_j$ | 22.41 | −165.86 | 91.56 | −40.61 |
| | (171.77) | (267.67) | (167.03) | (252.91) |
| $Post_t \times Chg.Itm_j$ | | | 54.18** | 72.81** |
| | | | (20.89) | (31.32) |
| 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 | | X | | X |
| Observations | 1,487,945 | 1,487,945 | 1,487,945 | 1,487,945 |
| $R^2$ | 0.93 | 0.93 | 0.93 | 0.93 |
| Adjusted $R^2$ | 0.92 | 0.91 | 0.92 | 0.92 |

Table A16: Robustness – Reliance on Advanced Refunding Bonds

*This table reports the estimates of* $Spread_{i,j,t} = \alpha_{st} + \alpha_j + \delta\ Post_t \times Reliance.AdvRefunding_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. *$Spread_{i,j,t}$ is the traded municipal bond tax-adjusted spread over the maturity-matched treasury yield located in county j and traded at month t, $Reliance.AdvRefunding_j$ is the 10-years (2005-2016) share of GO issuance that was advance refunding bonds in county j, $Post_t$ equals 1 for bonds traded after July 2017, $\alpha_{st}$ are state-by-month fixed effects, $\alpha_j$ 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 Columns (2) and (4), the observations are weighted by the number of trades for the same bond observed prior to the TCJA. 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) |
| $Post_t \times Reliance.AdvRefunding_j$ | −3.26 | −0.47 | −0.33 | −1.08 |
| | (14.91) | (19.98) | (13.65) | (19.29) |
| $Post_t \times Chg.Itm_j$ | | | 54.03*** | 82.65*** |
| | | | (19.21) | (29.43) |
| State x Month FE | X | X | X | X |
| CUSIP | X | X | X | X |
| Bond characteristics | X | X | X | X |
| County-level control | X | X | X | X |
| Weighted trades | | X | | X |
| Observations | 1,487,970 | 1,487,970 | 1,487,970 | 1,487,970 |
| $R^2$ | 0.93 | 0.93 | 0.93 | 0.93 |
| Adjusted $R^2$ | 0.92 | 0.91 | 0.92 | 0.92 |

Table A17: Secondary market results controlling for Trump votes

*This table reports the estimates of* $Spread_{i,j,t} = \alpha_{st} + \alpha_j + \delta\ Post_t \times Chg.Itm_j + \beta X_{i,t} + \gamma Z_{j,t-1} + \varepsilon_{i,j,t}$. $Spread_{i,j,t}$ *is the 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,* $\alpha_{st}$ *are state-by-month fixed effects,* $\alpha_j$ *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 Columns (2) and (4), the observations are weighted by the number of trades for the same bond observed prior to the TCJA. 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) |
| $Post_t \times Trump.Share_j$ | $-10.87^{*}$ | $-17.94^{**}$ | $-11.68^{**}$ | $-20.55^{**}$ |
| | (5.86) | (8.42) | (5.64) | (8.12) |
| $Post_t \times Chg.Itm_j$ | | | $57.67^{***}$ | $92.88^{***}$ |
| | | | (18.81) | (28.37) |
| 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 | | X | | X |
| Observations | 1,483,059 | 1,483,059 | 1,483,059 | 1,483,059 |
| $R^2$ | 0.93 | 0.93 | 0.93 | 0.93 |
| Adjusted $R^2$ | 0.92 | 0.91 | 0.92 | 0.91 |

# B 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."* |