Property Tax Protection and Municipal Bond Valuation *

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April 23, 2026

Abstract

I investigate how downside protection of local government revenue affects municipal bond yield spreads and human capital investment. Some states provide downside protection for school district revenue through lower-bound caps that limit declines in property taxes for school districts but not for counties. I show that this revenue protection reduces school district bond spreads and, in turn, improves student achievement and the teaching environment. Using staggered difference-in-differences estimation, I show that this protection decreases property tax volatility by 9% for school districts relative to other states and non-school issuers. Following periods of high house price volatility, protected districts experience an 11-basis-point reduction in bond yield spreads and an 11% increase in bond issuance. These lower borrowing costs translate into fiscal and nonfiscal improvements. Teacher salaries increase by 5%, and student dropout rates decline by 8%. Overall, this study highlights a borrowing channel in which revenue protection improves local fiscal capacity and human capital investment.

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Given the historical stability of the $4 trillion municipal bond market in the U.S., even a marginal increase in yields can signal a significant decline in underlying credit quality. For example, while 10 basis points may seem marginal, in the municipal bond market, it represents a three-notch downgrade from AAA to AA−.[1] To prevent such downgrades, state and local governments regulate their taxing authority and pledge their full faith to repay general obligation debt.

Although property taxes are the largest source of revenue for local governments, they are understudied in municipal bond valuation. My paper shows that property tax is first order in pricing municipal bonds. Local governments levy property taxes by applying mill rate (i.e., tax rate per $1,000) to assessed value. A common assumption is that property taxes are proportional to house prices. This assumption is true only if assessed values are proportional to house prices and mill rate is fixed. However, assessed values diverge from house prices, and mill rates vary across municipalities over time, suggesting that this assumption is inappropriate. Prior studies attribute this divergence to household wealth disparities (Amornsiripanitch, 2020), racial inequalities (Avenancio-León and Howard, 2022), political partisanship (Kalda et al., 2024), appeal behavior (Weber and McMillen, 2010), and assessors' home bias (Cohen and Chen, 2024). In the data, the average mill rate varies from 0.05% to 5%, and the cross-sectional interquartile range spans from 3 to 9%.[2]

Fiscal volatility in local government revenue affects the municipal bond market and has spillover effects on the local economy. If house price volatility translates into property tax volatility, municipal bond yield spreads and borrowing costs increase, which adversely affects local real outcomes. To prevent these effects, some states provide downside protection for local government revenue by protecting property tax from house price volatility, and thereby reducing local fiscal volatility. These property tax policies differ across and within states. Given that municipal bond risks are more pronounced during downturns, revenue protection mitigates default risk and, in turn, lowers borrowing costs.

I investigate how downside protection of local government revenue affects municipal bond

[1] 10-year bond yield spread; Bloomberg Professional Terminal (2024)

[2] See the Appendix Figure A.1.

yield spreads and human capital investment. To examine this question, I identify thirteen states that enact lower-bound caps that limit declines in property taxes for school districts, hereinafter referred as *floors*, between 2000 to 2021. These floors uniquely provide downside protection on school district revenue and do not apply to counties. Some states restrict declines by dollar amount, while others impose percentage caps.[3] I hypothesize that the property tax floor reduces local fiscal volatility for school districts, which in turn lowers their bond yield spreads. On the other hand, there are alternative hypotheses that suggest that floor can increase municipal bond yield spreads or has limited effects. First, the floor may generate a moral hazard problem in that it encourages careless spending of local governments. Because the floor reduces downside risk, local governments may internalize the floor and spend carelessly. This moral hazard amplifies default risk and in turn increases bond spreads (Cornaggia et al., 2024a; Gao et al., 2019).[4] The second alternative hypothesis is that property tax may not be the first-order determinant in municipal bond pricing. If this is true, then property tax effects are limited.[5]

Using a quadruple staggered difference-in-differences (DiD) estimation, I show that the property tax floor lowers municipal bond yield spreads. I compare bond yield spreads of school districts before and after the state-wide enactment of floor compared to non-school issuers, to the non-floor states, and to the municipalities with low house price volatility over the time period. Following a rise in house price volatility, the protected school districts show a decrease in property tax volatility

[3]For example, Kentucky restricts declines by requiring a minimum tax effort of 30 cents per $100 of property taxes for school districts to qualify for state funding. Nevada limits year-over-year declines in assessed values to no more than 2% and tax rates to no more than 0.75% (i.e., 7.5 mills). Although these policies vary in forms, they have a common goal: to ensure the adequacy of state education systems and provide equalization to every student regardless of the district's wealth (Lafortune et al., 2018).

[4]In the municipal bond literature, Gao et al. (2019) raises the concern that a moral hazard problem exists in states that allow unconditional access to bankruptcy in that municipalities carelessly issue bonds. In similar spirit, Cornaggia et al. (2024b) raises a concern that municipal bond insurance may encourage the insured issuers to engage in ex post riskier behavior because the negative consequences are covered by the insurance.

[5]When valuing municipal bonds, economists have focused on credit ratings (Adelino et al., 2017; Cornaggia et al., 2018, 2017), default risk (Poterba and Rueben, 2001; Schwert, 2017), bankruptcy risk (Gao et al., 2019), climate and environmental risks (Acharya et al., 2022; Auh et al., 2022; Cornaggia and Iliev, 2023; Goldsmith-Pinkham et al., 2023; Huang and Kumar, 2021; Painter, 2020), migration risk (Cornaggia et al., 2024; Gustafson et al., 2023), political risk (Chava et al., 2024; Gao et al., 2019), data breaches (Curti et al., 2023), market fragmentation (Babina et al., 2021; Fleckenstein and Longstaff, 2023; Longstaff, 2011; Poterba and Verdugo, 2011), and liquidity risk (Ang et al., 2014; Kidwell and Trzcinka, 1982). See literature review section for more details.

by 9% and an increase in bond issuance by 11%. Consistent with this stabilization, school district bond yield spreads decline by 11 bps compared to control bonds, which is a 7.6% decline relative to the total average of 143 bps. This effect is economically substantial and comparable to the upgrade of credit ratings from AA− to AAA.[6] The reduction in municipal bond yield spreads is greatest among low-income districts, showing a 33 bp decline in the spreads. These results speak to how revenue protection on the downside reduces borrowing costs.

A causal interpretation of the results relies on the identification assumption that municipal bond yield spreads are priced by property tax floors and house prices, not any other spuriously correlated factors. In my estimation, floors and house prices are both endogenous to bond spreads, because they are correlated with spurious factors (i.e. personal income) that directly affect the bond spreads. In support of this identification strategy, I show that there is no meaningful difference between school districts that eventually received the protection and those that did not prior to the enactment, suggesting that the parallel trend assumption holds in the staggered DiD estimation. Furthermore, I show that the adoption of the floor had limited predictability, showing that economic and demographic variables did not predict its statewide enactment. These characteristics include state-wide population, income, housing prices and volatility, crime rates, presidential electoral outcomes, homeownership rates, and poverty rates.

I provide three additional pieces of empirical support for this ultimately untestable assumption. First, I exploit the fact that four states enacted the floor through close elections. Property tax volatility falls by 7%, and bond spreads decline by 12 bps for protected districts in the states where the floor passed by a narrow margin. These results are remarkably similar with the main results. To be clear, while my sample includes all U.S. states, I use the close-election estimation as

[6] For comparison, Cornaggia et al. (2024) estimate 15 bps change in hospital bond spreads due to the adoption of remote product delivery. Cornaggia et al. (2018) show 15-22 bps change in spreads due to Moody's recalibration. Gao and Murphy (2022) find that the Medicaid expansion lowers rural hospital bond yields by 25 bps. Cornaggia et al. (2022) show that opioid crisis increased spreads of GO municipal bonds by 17 bps. Huang and Kumar (2021) report that a discovery of PFAS contamination increased municipal bond yields by 13 bps. Finally, Painter (2020) shows that a 1% increase in flood risk increases yields by 23 bps for long-term maturity municipal bonds.

a robustness check to strengthen the causal interpretation of the floor.[7]

Second, to address endogeneity concerns related to house prices, I instrument local house prices using the interaction between housing supply elasticity and national house price changes. Conditional on demand shocks, supply elasticity is orthogonal to shocks to housing productivity and other supply-side factors, as documented in prior studies (e.g., Baum-Snow and Han (2024), Guren et al. (2021), and Saiz (2010)). I use property tax revenue per capita to remove the mechanical correlation between housing stock size and property tax revenue. This estimation follows many studies that instrument house prices using supply elasticity conditional on demand shocks (e.g., Chaney et al. (2012), Chetty et al. (2017), Chakraborty et al. (2018), Corradin and Popov (2015), Couture et al. (2024), Cvijanović (2014), and Graham and Makridis (2023)).

Third, I conduct two placebo tests and show that economic variables unrelated to property taxes have limited effects on municipal bond yield spreads in this estimation. Specifically, I examine rental prices instead of house prices in the main estimation and show that the floor has limited effects. Because renters do not pay property taxes, this test reinforces that the results operate through the property tax channel rather than through other unobserved municipal characteristics. Also motivated by Butler et al. (2025), I examine revenue bond yield spreads and find limited effects, which reinforces the causal interpretation because revenue bonds are not directly backed by property taxes and are more closely tied to specific projects.

Going back to the results, I show that reduced borrowing costs have real effects on student achievement and teaching environment. I find that this borrowing channel increases teacher salaries per pupil (*pp*) by 5%, employee benefits *pp* by 10% and decreases student dropout rates by 3 percentage points. Explaining these results, I find that the floor increases current expenditure on education by 3.8%, on instruction by 5%, support services by 2.7%, and school lunch by 7.8%. When school districts secure an obligation over their revenue, their incentives to invest in learning

[7] An inevitable weakness of the close-election design is the reduction in sample size. While I acknowledge this limitation, I use this specification not as the sole identification strategy, but as additional evidence to strengthen the causal interpretation of the results.

environment increase. Therefore, the property tax floor and borrowing channel gives important policy implications on public school instruction by enhancing teaching environment and student achievement.

Contribution to the Literature. This paper provides novel insights for several strands of literature. First, my work advances the understanding of how property tax institutions of state and local governments affect municipal bond pricing. When valuing municipal bonds, economists have focused on credit ratings (Adelino et al., 2017; Cornaggia et al., 2018, 2017), default risk (Poterba and Rueben, 2001; Schwert, 2017), bankruptcy risk (Gao et al., 2019), climate and environmental risks (Acharya et al., 2022; Auh et al., 2022; Cornaggia and Iliev, 2023; Goldsmith-Pinkham et al., 2023; Huang and Kumar, 2021; Painter, 2020), migration risk (Cornaggia et al., 2024; Gustafson et al., 2023), political risk (Chava et al., 2024; Gao et al., 2019), data breaches (Curti et al., 2023), market fragmentation (Babina et al., 2021; Fleckenstein and Longstaff, 2023; Longstaff, 2011; Poterba and Verdugo, 2011), local investor's aversion to tax payment (Ambrose et al., 2025), opioid crisis (Cornaggia et al., 2022), bond insurance (Cornaggia et al., 2024b), investor attention (Cornaggia et al., 2022), liquidity risk (Ang et al., 2014; Kidwell and Trzcinka, 1982), and ESG labeling (Garrett et al., 2025). A closely related study, Goldsmith-Pinkham et al. (2023), finds that uncertainty from the sea-level rise increases municipal bond yield spreads. They control for house prices to proxy for local economic conditions and to distinguish sea-level rise uncertainty from those conditions. While Goldsmith-Pinkham et al. (2023)'s main variable of interest is not property tax,[8] the difference with my study is that I focus on property tax institutions and further show that the property tax choices are the first order in municipal bond valuation.

Although property taxes are the largest source of revenue for local governments, they are understudied in municipal bond valuation. The vast heterogeneity in property taxation across and within states has historically posed a significant empirical challenge. To overcome this challenge, I

[8]For example, baseline results in Goldsmith-Pinkham et al. (2023) exclude California, which is a state that caps annual increase in assessment values to no more than 2%. When Goldsmith-Pinkham et al. (2023) include California in their sample, the sea level rise has smaller effects on municipal bond yield spreads, as presented in the third column of Table 8.

hand-collect detailed property tax regulations directly from state statutes and administrative codes. Ultimately, building on the framework of Schwert (2017), I study the fundamental drivers of municipal default risk by focusing on local governments' ability to raise property tax revenue, which is shaped by housing market conditions and state regulations.

Furthermore, my paper contributes to the literature that examines the difference between house prices and property taxes. Researchers attribute assessment gap – the gap between assessment values and market values of properties – to household wealth disparity (Amornsiripanitch, 2020), racial inequalities (Avenancio-León and Howard, 2022), political partisanship (Kalda et al., 2024), appeal behaviors (Weber and McMillen, 2010), and assessor's home bias (Cohen and Chen, 2024). Berry (2021b) and McMillen and Singh (2023) find that property taxes are regressive, which means that properties in wealthier (poorer) neighborhoods are under (over)-assessed. Studies document that municipal equity improves after a comprehensive reassessment of properties (Berry, 2021a; Hou et al., 2023; Kim and Hou, 2024). Other studies focus on state-level caps on property taxation such as Massachusetts' Proposition $2\frac{1}{2}$ (Bradbury et al., 2001), Texas (Benson and Marks, 2010; Plummer and Pavur, 2009), and Illinois (Dye and McGuire, 1997; Dye et al., 2005). The difference with my study is that I focus on municipal bonds and risk premia. While prior studies examine the reasons for the gap between assessment values and market values of properties, my study shifts attention to how these gaps explain cross-sectional variation in municipal bond yield spreads. I contribute to this literature by studying how property tax, which diverges from house prices, affects the municipal bond yield spreads and the investments on human capital in public school districts.

Finally, my study relates to municipal bond studies that investigate school districts, public education, and universities. School district serves as the main sample in some municipal bond studies (Garrett, 2024; Goldsmith-Pinkham et al., 2023). Dougal et al. (2019) show that racial discrimination affects municipal bond pricing and find higher underwriting spreads paid by historically Black colleges and universities. Cellini et al. (2010) find that issuance of school district bonds increases house prices because home buyers are willing to pay premium for capital spending on public education. Binfare and Zimmerschied (2024) find that public universities have lower borrowing cost than private universities in the municipal bond market. While existing work has shown that higher demand for attractive school districts causes an increase in local house prices and capital spending (Bayer et al., 2020; Cellini et al., 2010; Lafortune et al., 2018), less is known about the effects on school district borrowing channel and even less is known about how the borrowing channel shapes student achievement and teaching environment. In the setting I study, floor protects revenue of school districts on downside and lowers municipal bond yield spreads, which highlights the borrowing channel. I contribute to this literature by using a floor regulation that shields revenue of school districts from economic downturns and showing that this regulation shields revenue from downside volatility, which in turn decreases municipal bond yield spreads.

The remainder of the paper is as follows. Section 1 provides institutional background. Section 2 describes the data sources and sample construction. Section 3 outlines empirical design and presents baseline results. Section 4 presents real effects on education outcomes. Section 5 concludes.

1 Institutional Background

School districts and counties levy property taxes by applying mill rate to assessed value of a property, net of applicable exemptions. Although counties and school districts overlap geographically, they are distinct taxing authorities that set property tax rates independently. Property taxes are levied separately by each jurisdiction, and the floor regulation applies only to school districts. This institutional feature allows non-school issuers within the same geographic area to serve as a control group.

The average gap between house prices and assessed values is approximately $60,000 across properties, which is a 35% gap relative to the $230,000 house price. Economists have attributed the gap to economic factors such as racial inequality (Avenancio-León and Howard, 2022), political partisanship (Kalda et al., 2024), wealth inequality (Berry, 2021b; McMillen and Singh, 2023), and budget deficit pressure (Cohen and Chen, 2024). These assessment gaps are salient across the country, including overtaxation of properties in Detroit (Berry, 2021b; Hodge et al., 2017; MacDonald, 2020)[9], under-taxation due to infrequent reassessment in New Orleans before 2021 (Berry, 2021b) and under-taxation in Wisconsin before a comprehensive reassessment 2022 (Wisconsin Policy Forum, 2023). More recently, Kerr County plans to increase assessment values following the July 2025 Central Texas floods by authorizing an emergency 8% cap, more than double the usual 3.5% limit.[10] Controversies on assessment values may lead to serious consequences. For example, local media criticized Chicago's property tax system, suggesting that the city's wealthier neighborhoods are under-assessed and poor neighborhoods are over-assessed (Grotto, 2017). Following high media coverage on this issue, assessors raised campaigns on reforms in elections.

Floor Legislation in the U.S. While the initial 1960s "equity" wave focused on aggregate funding levels, this subsequent "adequacy" phase shifted toward reducing resource disparities and supporting low-income districts (Lafortune et al., 2018). Following the second wave of school finance reforms in the 1990s, state governments increasingly regulated property taxation for school districts. Despite being a landmark effort to promote educational equality following the desegregation era, the impact of these structural reforms on municipal bond market remains under-explored. This gap in the literature is surprising given that school districts are the most frequent and largest issuers in the U.S. municipal bond market (Merritt Research Services, 2022).[11]

To fill this gap in the literature, I construct a novel dataset of state-level property tax mandates by collecting evidence from state constitutions and statutes. As shown in Figure 1, thirteen states have implemented floor regulations and insulate school districts from revenue volatility by imposing a percentage cap on downward adjustments. The goal of this policy is to establish a standardized system to ensure that every student receives at least a minimum level of financial support

[9]For example, home sold for $2,300 in 2010 was still assessed at $42,000, resulting in overtaxation (MacDonald, 2020). Hodge et al. (2017) show a comprehensive analysis that Detroit's assessment system was highly regressive, with similar homes receiving significantly different assessments, while different homes were given similar assessments.

[10]Texas Public Radio, 2025.

[11]UIC GFRC; Vanguard, 2024; Acuity Knowledge Partners, 2025

regardless of the property wealth of the district. These floors vary significantly across states, taking forms such as minimum mill rate requirements and caps on the level and growth of property taxes. For example, per Georgia's Quality Basic Education Act, every school district in Georgia should maintain a minimum mill rate of 5 mills. The state uses "Local Five Mill Share," where a district's local contribution is calculated as 0.005 multiplied by its property tax digest since 2001. The minimum millage rate increased to 14 mills in 2012, before being reduced to 10 mills in 2024.

Some states impose lower-bound percentage caps on level of property tax revenue, ensuring a baseline dollar amount is generated. Kentucky's system operates through a floor on the level of property tax revenue, defined as a mandatory amount of funding proportional to the current tax base. The Support Education Excellence in Kentucky (SEEK) formula requires local effort of 3 mills for school districts to participate in state funding. The state subtracts this 30-cent local contribution from the guaranteed per-pupil funding amount to determine state aid. Lastly, other states impose lower-bound percentage caps on growth of property taxes. For example, Nevada imposes growth-based floors, where NRS 361.4722 limits year-over-year declines in assessed values to no more than 2% and tax rates at 7.5 mills.

As shown in Figure 1, two states enacted the floor in 2000; Three states enacted it from 2001 and 2005, and six more states between 2006 and 2019, followed by two additional states after 2016. School districts' property taxes are protected from downside risk, whereas counties and cities are not. It provides a clean source of identification: school districts form a treated group, while cities and counties form a control group. The regulation varies across states and is often decided through general elections, enacted by state legislatures, or established by state supreme court rulings. Four states, which are Alabama, Georgia, Missouri, and Nevada, went through close elections that approved this law by a close margin. For example, Nevada had a general election for the passage of the Nevada Revised Statute (NRS), where 54% had voted yes, and 46% had voted no. On the other hand, in Florida, 76.52% of voters supported the adoption, while 23.48% opposed it; therefore, it was not a close election.[12] The other seven states adopted the floor through legislative action rather than general elections. For example, Minnesota Legislature enacted a series of school-finance reform, most notably the Education Finance Act, that established a guaranteed local effort system in early 1990s and enacted the floor in 2005.[13]

I show that the property tax floor is binding. Ex-ante, as shown in Figure A.2, property taxes face substantial downside risk. Around a quarter of local governments show a decline in property tax revenue compared to a year earlier, and the average decline rate was 12%. Ex-post, school districts in floor states experienced smaller declines in property tax levels and lower volatility compared to those in non-floor states. Figure 2 shows that at the onset of the Global Financial Crisis, school districts in non-floor states showed a decline in property tax per capita by 27% between 2006 and 2009, and required seven additional years to recover. However, school districts in floor states showed a smaller decline of 15% in 2009 and recovered more quickly, reaching its pre-recession level within three years. Additionally, Panel B shows that school districts in non-floor states show substantially higher property tax revenue volatility than those in floor states. This gap remains large for eight years.

2 Data

This study relies on several data sources. First, I use transaction-level municipal bond data from Municipal Securities Rulemaking Board (MSRB) and Bloomberg. Second, I use local government-level property tax data on residential properties provided by the Census Bureau's Annual Survey of State and Local Government Finances data and supplemented by the CoreLogic Tax Deeds. Third, I use county-level house price data from the Zillow Home Value Index and the district-level

[12] Florida Statutes 1011.71 states that under the Constitution, Florida school districts are protected by ten mills.

[13] During the late 1980s and early 1990s, the Minnesota Legislature debated school-finance reforms aimed at reducing disparities in educational funding while maintaining local fiscal effort. These debates took place within the education and tax committees of both chambers, where lawmakers designed a guaranteed local effort system linking state aid to a required minimum property tax levy by school districts. The legislation was passed by the House and Senate, reconciled through conference committees, and signed into law by the governor as part of broader education finance bills.

house price data aggregated from the tract-level data provided by the Federal Housing Finance Agency (FHFA). Fourth, I obtain data on state-level property tax regulations from state statutes, administrative codes, and regulatory amendments. Lastly, I use district-level data on education outcomes from the National Center for Education Statistics' (NCES) Common Core of Data (CCD).

2.1 Data Sources and Sample Construction.

Municipal Bond Data. I use transaction-level data on municipal bonds from the Municipal Securities Rulemaking Board (MSRB), which reports trade date, yield, price, maturity, and coupon rate for all municipal bond transactions since 1998.[14] I merge these transactions with municipal bond characteristics from Bloomberg using CUSIP and transaction date. Bloomberg provides information on whether a bond is a general obligation (GO) or revenue bond, whether it is callable, insured, tax-exempt or not, and whether it has fixed or variable coupon, and information on issuance size and transaction amount. Critically, Bloomberg reports the issuer's name and type, and state code for each transaction (e.g., Monroe County, Indiana). To identify school districts, I follow Goldsmith-Pinkham et al. (2023) and classify issuers using keywords such as "vocational," "technical," and "elementary," including common abbreviations, and manually match remaining issuers. Following Dougal et al. (2019), I exclude issuers corresponding to two-year and junior colleges (e.g., "community college" and "junior college"), which are typically small and often issue bonds jointly with other educational entities.

I estimate fundamental prices of municipal bonds by averaging the highest price on customer sales and the lowest price on customer purchases each day following prior studies (Curti et al., 2023; Fleckenstein and Longstaff, 2023; Green, 1993; Schwert, 2017). If customer sales and purchases are not done in one day, then I use the mean price of interdealer trades.

To focus on the direct role of property tax revenue, I use general obligation (GO) municipal bonds and school district bonds in testing my hypothesis. GO and school district bonds are directly

[14]The MSRB mandates dealers to report all municipal bond transactions beginning in 1998.

backed by property taxes, whereas revenue bonds are typically secured by cash flows from specific projects, such as utilities, sewage systems, or highway tolls. Consistent with this distinction, I show that the estimated results are more pronounced for bonds directly linked to property taxes, such as GO and school district bonds. I use revenue bonds as a placebo test to show that bonds that are less tied to property taxes show smaller changes in yield spreads, which reinforces causal interpretation of the estimates.

Municipal bond investors benefit from income tax exemptions on earnings from bonds issued within their state of residence. For example, if an Indiana resident buys a municipal bond issued in Indiana, she does not pay income tax on interests earned from the bond.[15] Babina et al. (2021) show that this exemption results in market fragmentation and limited risk-sharing across states. Investors are local residents who benefit from these income tax exemptions. Fleckenstein and Longstaff (2023) find that local investors are willing to pay a premium for this exemption. Garrett et al. (2023) show that the reduction of this exemption raises bidder markups and depresses competition, which more than offsets the tax savings to the government.

Municipal Bond Yield Spreads.

$$Spread_{bit} = \frac{\text{yield}_{bit}}{(1-\tau_t^{\text{fed}})(1-\tau_{s,t}^{\text{state}})} - r_t^f, \tag{1}$$

The main dependent variable in my study, $Spread_{bit}$ is the yield spreads of municipal bond b issued by a local government i and traded in year t. The maturity-matched risk-free rate, r_t^f, is the constant maturity zero-coupon yield curve of Treasuries, compiled by Liu and Wu (2021).[16] To isolate the effect of income tax exemption on yields, I adjust the nominal yield by dividing it with $(1-\tau_t^{\text{fed}})(1-\tau_{s,t}^{\text{state}})$, where τ_t^{fed} is the top federal income tax rate, and $\tau_{s,t}^{\text{state}}$ is the top income tax rate

[15] There are four states that do not provide income tax exemptions: Illinois (IL), Oklahoma (OK), Wisconsin (WI), and Iowa (IA). In these states, tax-adjusted yield spreads are larger because investors do not benefit from income tax exemptions.

[16] Thanks to the authors for releasing the data at their website https://sites.google.com/view/jingcynthiawu/yield-data.

in state s at time t.[17] Equation (1) is identical to Schwert (2017)'s measure and follow-up studies (e.g., Cornaggia et al., 2024; Curti et al., 2023; Fleckenstein and Longstaff, 2023; Gustafson et al., 2023). The main findings are robust with alternative measures of yield spreads, including nominal yields, Garrett et al. (2023)'s tax adjusted yield spreads, Schwert (2017)'s tax adjusted yield less Gürkaynak et al. (2007)'s risk-free rate, and yield spreads with different maturities.

Legal Sources. My primary source of policy variation is the staggered adoption of state-level legislation governing floors. I manually collect data on two categories of property tax regulations from the primary legal documents, including state statutes, administrative codes, and regulatory amendments, for each state.

For each state and each regulatory category, I record the calendar year in which the relevant legislation became effective, which serves as the treatment date in my staggered DiD framework. States that had not enacted a given regulation by the end of my sample period are coded as never-treated for that regulatory category.

Property Tax Data. I use the U.S. Census Bureau's Annual Survey of State and Local Government Finances data to obtain municipality-level property tax revenue. Pierson et al. (2015) provides data on property tax revenue for each county, city, and school district in the U.S., along with data on Federal and state transfers. They offer a single, comprehensive database of revenue and expenditure statistics, including detailed fiscal data from states, counties, cities, and school districts for the years 1967 through 2022, processed to make it user-friendly. The data includes detailed information on both tax and non-tax revenue, intergovernmental transfers, and expenditure. Furthermore, I obtain mill rates for each county, city, and school district from the Lincoln Land Institute of Policy. I use the headline mill rate provided by each municipality.

The Census property tax data includes not only residential properties but also commercial, industrial, and other real properties. This feature becomes a weakness when I estimate the ex-post effect of the floor on municipal bond spreads by interacting the floor and school indicators with

[17] Federal and state income tax rates are collected from Tax Foundation.

house price volatility. A potential concern is that the coefficient in equation (3) is overestimated when the ratio of residential to total property tax is low. The best way to address this concern is to proxy residential property tax revenue for each municipality. Because the Census does not provide data exclusively on residential property tax, I use CoreLogic Tax Deeds to obtain property-level data on tax bills for residential properties paid to each type of municipality. This dataset reports the total tax bill, assessment year and value, exemptions, property type (e.g., residential or commercial), and geographic identifiers, including ZIP code, county, city, and state. The dataset covers property tax records and represents the near-universe of single-family homes in the United States. I exclude commercial and industrial properties because they are subject to different assessment practices. I follow Avenancio-León and Howard (2022) and restrict the sample to residential properties, including single-family homes, condominiums, duplexes, and apartments. I further drop transactions that are classified as partial sales, involve multiple sales on the same day, or report a transaction value of zero. I exclude years with missing assessment data, as well as properties with assessment values below 1% or above 300% of market house prices. Following Bernstein et al. (2019), I drop properties with transaction values lower than \$50,000 and higher than \$10,000,000. I aggregate the property-level tax for residential properties to the municipality level using the geographic identifiers and information on which municipality (i.e., county, city, and school district) the tax was paid to.

House and Rental Price, and House Supply Elasticity Data. I use house price data for cities and counties from the Zillow Home Value Index (ZHVI) and school districts from FHFA house price index. I employ county-level series for all single-family residences and condominiums, which provides the geographic resolution required by my empirical strategy. The ZHVI is constructed using a two-stage procedure. In the first stage, Zillow generates an individual property-level estimated market value for more than 100 million residential properties in the U.S. In the second stage, the ZHVI aggregates property-level Zestimates into a geographic index using a repeat-Zestimate methodology that is conceptually analogous to a repeat-sales index. Furthermore, as in Bayer et al.

(2020), I measure average house prices within school district boundaries using the tract-level FHFA house price index, which is derived from mortgage transactions on single-family properties securitized by Fannie Mae and Freddie Mac. To obtain rental price, I use data from the Zillow Observed Rent Index (ZORI), which is a monthly, repeat-rent index measuring changes in asking rents over time while controlling for compositional shifts in the available rental stock. The index is dollar-denominated, capturing the mean of listed rents in the 35th-to-65th percentile range for a given geography, and is available at the national, metropolitan statistical area, county, city, and five-digit zip code levels for regions.

I measure housing supply elasticity to assess the extent to which local housing markets can accommodate changes in demand through new construction. My primary measure is the tract-level housing supply elasticity of Baum-Snow and Han (2024), which estimates the responsiveness of local housing supply to price signals at the neighborhood level. Baum-Snow and Han (2024) construct elasticity estimates at the tract level by combining granular land availability data—measuring the share of developable land within each zip code not yet converted to residential use—with local regulatory stringency and the existing density of development. This approach allows for substantial within-MSA heterogeneity in supply constraints, which is particularly important in large metropolitan areas where inner-city zip codes facing severe land scarcity may coexist with suburban zip codes that retain significant developable land. Because my empirical strategy exploits county, city, and district-level variation in house prices, this is the preferred proxy for local supply constraints.

Education Outcomes. I obtain district-level data from the National Center for Education Statistics' (NCES) Common Core of Data (CCD), specifically from America's Public Schools. The CCD consists of two components: the fiscal CCD and nonfiscal CCD. The fiscal CCD reports information on student enrollment, measured as fall membership (i.e., the number of students enrolled on a specific fall count date, typically October 1), as well as detailed financial variables, including revenues from federal, state, and local governments; total and current expenditures on instruction, support, and other services; capital outlays for construction and equipment; salaries for instructional and support staff; employee benefits; and both short-term and long-term debt. The nonfiscal CCD reports the location of the school district, number of schools, teachers, students, and support staff, and student dropout rates. I merge the two datasets using the NCES 7-digit local education agency identifier (i.e., LEAID) and school year. I then merge this dataset with transaction-level municipal bond data by matching school district names within the same county and state. As noted above, the school district names from municipal bond issuers are obtained from Bloomberg and cleaned following Goldsmith-Pinkham et al. (2023) and Dougal et al. (2019).

Sample Construction. This paper constructs a dataset combining municipal bond transactions, floor regulations, property taxes, housing markets, and education outcomes. Table A.1 presents the number of bonds and trades in each step of the sample construction.[18] I first merge the GO and school district bond transaction sample with data on residential property tax revenue per capita for each county and school district using the municipality's name.[19] I merge this data with house prices for each county provided by the ZHVI and for school district constructed from the tract-level FHFA house price index. I then merge this dataset with housing supply elasticity for each county and school district from Baum-Snow and Han (2024).[20] Finally, I merge district-level data on education outcomes from the NCES CCD using the NCES 7-digit local education agency identifier. In sum, my sample is a comprehensive dataset that includes municipal bond transactions, county and school district property taxes, house prices, and supply elasticity, and state-level floor regulations.

I follow prior studies, such as Curti et al. (2023), Fleckenstein and Longstaff (2023), Gao and

[18]This table follows Schwert (2017)'s presentation of Table 1 that summarizes each step of the sample construction in his study.

[19]As aforementioned, Bloomberg reports the issuer's name and type, and state code for each transaction (e.g., Monroe County, Indiana). To identify school districts, I follow Goldsmith-Pinkham et al. (2023) and classify issuers using keywords such as "vocational," "technical," and "elementary," including common abbreviations, and manually match remaining issuers. Following Dougal et al. (2019), I exclude issuers corresponding to two-year and junior colleges (e.g., "community college" and "junior college"), which are typically small and often issue bonds jointly with other educational entities.

[20]Because Baum-Snow and Han (2024)'s supply elasticity is provided in tract-level, I aggregate it to county, city, and school district-levels using household weight. Equation (4) in Section 3 shows results.

Murphy (2022), and Schwert (2017) to eliminate data errors. I first drop bonds that are transacted fewer than 10 times, have missing coupon or maturity information, coupon rates greater than 20%, maturities exceeding 100 years, bond prices below 50 or above 150, and trades occurring after the bond's maturity. I then match the transaction data with the U.S. Census Bureau's Annual Survey of State and Local Government finances, which leaves 173,183 bonds. I then drop municipal bonds that have variable coupon rates, bonds that are pre-refunded, taxable, and subject to the alternative minimum tax. This leaves 111,786 bonds in the sample. Following Schwert (2017), I use bonds with similar contract terms. That is, I use non-insured bonds and non-callable bonds in the main sample to ensure that property tax risk is not mixed with the risk from insurance and callability. I confirm that the main findings are robust including callable and insured bonds. This leaves the final sample of 109,235 bonds with 9,831,150 trades. Following these steps, the final sample includes municipal bonds issued by 2,270 counties and 3,606 school districts in the U.S. from 2000 to 2022. The dataset includes 3,133,784 trades involving 41,234 GO bonds and 6,697,366 trades involving 68,001 school district bonds.

2.2 Descriptive Statistics.

Panel A of Table 1 and Figure 3 presents descriptive statistics on municipal bond. The main dependent variable $Spread_{bit}$ equals the yield minus the maturity-matched risk-free rate, with the yield adjusted for income taxes as in equation (1). The tax-adjusted yield spread has an average of 143 bps and stays positive across all time.[21] Figure 3 shows time-series variation of average municipal bond yield spreads from 2000 to 2022. There are two implications from this figure. First, given that the yield is always higher than the risk-free rate, municipal bond is risky debt, although it is well known for safety. Second, there is a cross-sectional dispersion of yield spreads across issuers, as the light blue shade shows, showing that local governments face different levels of risks. The

[21]This value corresponds well with prior studies. In my study, the average tax-adjusted yield is 4.79% from 2000 to 2022, which is similar with the same variable that shows an average of 4.32% from 1998 to 2015 reported in Schwert (2017). The average is similar with that in Fleckenstein and Longstaff (2023), where the state-issued municipal bond yield spreads show an average of 1.4% in 14 states.

Appendix Table A.2 shows average yields of local governments in each state across different maturities for each state and the number of trades. California and Texas have the largest trades, followed by Florida, Pennsylvania, Virginia, and Kentucky.

Panel B of Table 1 reports volatility of property taxes and house prices for counties, cities, and school districts. School district shows $632 of three-year rolling volatility of property tax revenue per capita. County shows volatility of $2,069 and city shows $222. The ten-year volatility is $1,380 for school districts, $5,305 for counties and $589 for cities. The Appendix Table A.3 shows descriptive statistics on additional variables. The average property tax revenue per capita is $3,222, with a standard deviation of $1,555. The growth of property tax per capita is 6% for county, 9% for city, and 3.8% for school district. The total amount of property tax revenue was $46 million for counties, $6 million for cities, and $14 million for school districts.

3 Empirical Strategy and Baseline Results

3.1 Triple Staggered DiD Estimation

As shown in Figure 1, thirteen states enacted a floor from 2000 to 2021, which requires school districts to levy property taxes above the minimum threshold. Using triple staggered DiD estimation, I compare school districts before and after the state-wide enactment of floor compared to non-school issuers and to the states that never enacted the floor over the time period. This identification relies on two sources of variation: the presence of a floor policy and differences across issuer types (school districts versus counties), because the floor applies only to school districts.

$$\begin{aligned} \text{Spread}_{bit} = \beta\left(\text{Floor}_{st} \times \text{School}_i\right) + \pi_0 \text{Floor}_{st} + \pi_1 \text{School}_i \\ + \pi_2 C_{it} + \pi_3 Z_{bit} + \mu_s + \delta_t + \varepsilon_{bit} \end{aligned} \tag{2}$$

for municipal bond b, issuer i, state s, and year t. Floor_{st} equals one if a state s mandates a minimum threshold for property taxes in year t and zero otherwise. School_i equals one if an issuer i of a municipal bond b is a school district, and zero if it is county. Although counties and school districts overlap geographically, they are distinct taxing authorities that set property tax rates independently. Property taxes are levied separately by each jurisdiction, and the floor regulation applies only to school districts. This institutional feature allows non-school issuers within the same geographic area to serve as a control group. Spread$_{bit}$ is the yield spread of municipal bond b, measured as the income-tax-adjusted nominal yield minus the maturity-matched risk-free rate, following Schwert (2017), as shown in equation (1). μ_s denotes state fixed effects. δ_t denotes year fixed effects. The coefficient of interest is β, which captures the ex-ante effect of floor on yield spreads of bonds issued by school districts.

The specification includes an extensive set of control variables related to local governments (C_{it}) and municipal bonds (Z_{bit}). I control for issuer-level characteristics (C_{it}), including growth of resident income, local employment, population, and GDP. I control for local firm risk using Hassan et al. (2019)'s data and natural hazards and disasters using EM-DAT dataset. I control for sales and income tax revenue of local governments and control for migration risk including both authorized and unauthorized migration (Cornaggia et al., 2024; Zimmerschied, 2024). Following prior studies (e.g., Acharya et al. (2022); Goldsmith-Pinkham et al. (2023)), I control for municipal bond characteristics (Z_{bit}), including the remaining time to maturity, trading volume relative to the outstanding amount, credit rating, and bond-month standard deviation of transaction prices. I control for liquidity risk measured with Amihud (2002)'s price of impact, Feldhütter (2012)'s imputed round-trip cost, and Roll (1984)'s effective bid-ask spread. The Appendix Sections A and B present definitions of control variables.

Panel A of Table 3 shows that the main coefficient of interest β is -0.06 (t-stat: -1.92), implying that school district bonds in floor states carry yield spreads that are 6 bps lower than control bonds. This estimate reflects an ex-ante pricing effect, which means that a floor provides a regulatory shield that reduces the perceived default risk of school districts and therefore lowers municipal bond yield spreads by 6 bps. Explaining these results, I find that the municipal bond issuance increases by 11% for school districts in floor states, as shown in column (3) in Panel A of Table 3. These results speak that the floor facilitates the borrowing channel ex ante, increasing bond issuance and decreasing the costs.

A potential concern is that spuriously correlated characteristics are lowering the bond spreads, not through the floor. While I deal with the endogeneity concerns in many ways, I first show that state-wide characteristics do not predict the enactment of the floor. Panel B of Figure 1 shows the estimated coefficients from regressions of floor adoption timing on a range of observable characteristics, including lagged population size, income levels, housing prices and growth, crime rates, presidential electoral outcomes, homeownership, and poverty. In all specifications, the coefficients are statistically indistinguishable from zero and economically negligible, indicating that these characteristics do not strongly predict the state-wide adoption of floors.

3.2 Quadruple Staggered DiD Estimation

I then show how the floor affects property tax volatility and municipal bond yield spreads following a rise in house price volatility. Ex post, protection on property taxes becomes binding after a rise in house price volatility. To estimate this effect, I use a quadruple-DiD regression where I compare school districts before and after the state-wide enactment of floor compared to non-school issuers and to the states that never enacted the floor and lastly, to municipalities that have low house price volatility over the time period.

$$
\begin{aligned}
y = {} & \gamma \left(\text{Floor}_{st} \times \text{School}_i \times \sigma_{it}(\text{House Price}_{it})\right) \\
& + \pi_0 \text{Floor}_{st} + \pi_1 \text{School}_i + \pi_2 \sigma_{it} \left(\text{House Price}_{it}\right) \\
& + \pi_3 \text{Floor}_{st} \times \text{School}_i \\
& + \pi_4 \text{Floor}_{st} \times \sigma_{it}(\text{House Price}_{it}) \\
& + \pi_5 \text{School}_i \times \sigma_{it}(\text{House Price}_{it}) \\
& + \pi_6 C_{it} + \pi_7 Z_{bit} + \mu_s + \delta_t + \varepsilon_{it}
\end{aligned} \tag{3}
$$

There are two outcome variables y, $\log \sigma_{it}(\text{Property Tax}_{it})$ and Spread_{bit}. I measure property tax volatility (σ_{it} $(\text{Property Tax}_{it})$) as three-year rolling volatility of change in property tax revenue per capita. I normalize property tax revenue to per-capita to remove the mechanical correlation between housing stock size and property tax revenue. Spread_{bit} is the bond-level yield minus maturity-matched risk-free rate issued by local government i and traded in year t, measured as in equation (1). In my estimation, I measure house price risk ($\sigma_{it}(\text{House Price}_{it})$) using the logarithm of the three-year rolling volatility of house prices for each municipality.

The parallel trend assumption holds. As shown in Figure 4, the pre-treatment trends for protected and unprotected bonds are parallel, which means that that the yield spreads of bonds under the floor have moved in parallel to similar bonds without such floor. I find no significant differences in yield spreads between treated and control bonds in 5 years prior to the staggered adoption floors. Following the regulatory change, municipal bond yield spreads decline by 10 bps in the first year, followed by an additional 5 bps decrease in five years.

I then show the average treatment effects and event studies. As shown in Panel B of Table 3, when $y = \sigma_{it}$ $(\text{Property Tax}_{it})$, the coefficient γ on the triple interaction is -0.087 (t-stat: -3.02), showing that volatility of property tax revenue falls by 8.7%. Panel A of Figure 4 shows event study with annual coefficients for column (3) of Panel B of Table 3. Property tax volatility declines by 7% a year after the adoption, and by 10% four years after the adoption. Furthermore, when $y = \text{Spread}_{bit}$ the coefficient γ on the triple interaction is -0.11 (t-stat: -2.31), which means that school district bonds in floor states experience an 11 basis point reduction in yield spreads when house price volatility increases, as shown in Panel B of Table 3. Panel B of Figure 4 shows that the decline in spreads is persistent over the five years following the adoption of the floor.

It is important to examine the economic magnitude of the decline in municipal bond yield spreads. The effect of 11 bp decline is a 7% decrease in the spreads relative to the total average of 143 bps. Given the safety of municipal bonds, this effect is economically significant, substantial, and comparable to the upgrade of credit ratings from AA$-$ to AAA. For comparison, Cornaggia et al. (2024) estimate 15 bps change in hospital bond spreads due to the remote product delivery. Cornaggia et al. (2018) find 15 bps change in municipal bond yield spreads due to the Moody's recalibration. Cornaggia et al. (2022) show that the opioid crisis increased the spreads by 17 bps. Huang and Kumar (2021) report that a discovery of PFAS contamination increased yields by 13 bps. Painter (2020) shows that a 1% increase in flood risk increases yields by 7 bps for short-term maturity municipal bonds.

3.3 Endogeneity Concerns

There are two endogenous variables in my estimation, floor and house prices. A floor raises endogeneity concerns, as it may be correlated with the economic and demographic conditions of a state government, which in turn may affect municipal bond yield spreads. House prices are correlated with other spurious factors like income of residents, which would directly lower municipal bond yield spreads, rather than through property tax channel.[22] An exclusive restriction assumes the 11 bp decline in bond spreads is caused by the property tax institutions, rather than any other spuriously correlated factors. I have already showed the parallel trend assumption holds and the

[22]There are two related concerns. First, municipal bond yield spreads estimated as in equation (1) are low in states with high income tax rates. Second, high personal income of local governments would on average increase income tax revenue of local governments, which in turn reduces municipal bond yield spreads.

state-wide characteristics have limited predictability on the floor enactment. In this subsection, I provide several evidence to support this ultimately untestable assumption.

First, to address the endogeneity concern of house prices, I instrument house price volatility with the interaction between national house price volatility and supply elasticity from Baum-Snow and Han (2024), Guren et al. (2021), and Saiz (2010) of the following form:

$$\text{House Price}_{it} = \phi \left(\text{House Price}_t^{National} \times \text{supply elasticity}_i \right) + \nu C_{it} + \mu_s + \delta_t + \epsilon_{it} \tag{4}$$

I support relevance condition by showing that this instrument is strongly correlated with local house price growth. During national house busts, municipalities with lower supply elasticity experience larger volatility in local house prices, while more elastic municipalities show muted responses, shown by negative and statistically significant estimate of ϕ in Panel B of Table 3. In practice, the DiD estimation shows a large first-stage F-statistic (greater than 10), confirming the relevance condition of the instrument. To support exclusive restriction, I use property tax revenue per capita as the dependent variable in equation (3). The purpose of this normalization is to remove the mechanical correlation between the size of the housing stock and the level of property tax revenue, and therefore the instrument (i.e., supply elasticity) is not endogenous to the outcome variable (i.e., property tax revenue). Additionally, as evidenced by Baum-Snow and Han (2024), Guren et al. (2021), and Saiz (2010), conditional on demand shocks, supply elasticity affects property tax revenue only through its impact on local house prices. There is no remaining direct channel linking supply elasticity to property tax revenue after accounting for demographic changes and local demand shifts. Taken together, these studies argue that conditional on demand shock and appropriate controls, their shock is orthogonal to housing productivity and other supply related factors.[23]

Second, I use an empirical fact that four states, which are Alabama, Georgia, Missouri, and Nevada, adopted property tax floor for school districts through general elections that were decided by narrow margins. In elections decided within a narrow margin around the 50% passage threshold, passage versus failure induces a discontinuous increase in the likelihood that a school issuer operates under a binding floor. To isolate the causal effects of floors on municipal bond spreads, I leverage close elections and estimate the following regression:

$$y = \beta\left(\text{Pass}_s \times \text{Close}_s \times \text{School}_i \times \text{Post}_{st}\right) + \mu_s + \delta_t + \epsilon_{ist} \tag{5}$$

for municipal bond b, issuer i, state s, and year t. There are two outcome variables, volatility of property tax per capita and municipal bond yield spreads. Pass_s equals one if state s adopts the floor policy and 0 otherwise. Close_s equals one if the vote margin for the floor policy is within a close margin in state s, and zero otherwise. This variable captures the close elections in which policy adoption is plausibly exogenous. Post_{st} equals one for years following the adoption of the floor policy in state s, and zero otherwise. The coefficient of interest, β, measures the differential change in the municipal bonds yield spreads in states where the floor narrowly passed versus failed. For brevity, equation (5) omits all lower-order interaction terms. Control variables are the same with equation (3).

Table 5 shows that property tax volatility declined by 6.7% for school districts in floor states compared to control municipalities, which is similar in magnitude to the 8.7% decline in the main results. Furthermore, school district bond yield spreads decline by 12 bp in floor states than in con-

[23]In precise, Baum-Snow and Han (2024) measure the neighborhood-level supply elasticity, which is more granular than the MSA level as in Saiz (2010). They provide supply elasticity across about 50,000 census tracts in 300 U.S. metropolitan areas, decomposing the supply response into its constituent parts such as land development and housing units. They reveal substantial variation within MSAs. They construct an instrument using Bartik (1991) style labor demand shock constructed from a residential location to commuting destination. They discount the commute time with the sum of employment in each commuting destination from a residential location and adjust it with labor supply competition effects from other residential locations. Furthermore, Guren et al. (2021) measure supply elasticity using residual variations of house prices. They estimate historical elasticity of local house prices to regional housing cycles and then interact these historical elasticity estimates with today's shock to regional house prices.

trol bonds, which is again similar to the 11 bp decline in the main results. The remarkably similar magnitudes support a causal interpretation of the results. By comparing school districts in states where the policy narrowly passed to those where it narrowly failed, I exploit quasi-random variation from close elections, further supporting the causal interpretation. An obvious caveat of this estimation is the substantial reduction in sample size, which leads to weaker statistical significance. While this limitation, also noted in prior studies (De Magalhães et al., 2025; Imbens and Lemieux, 2008; Stommes et al., 2023), is acknowledged, I use this specification not as the sole identification strategy, but as additional evidence to strengthen the causal interpretation of the results.

Third, I do two placebo tests. The first placebo test uses rental price instead of house price volatility in the spirit of Bayer et al. (2020) and Jackson et al. (2016) when estimating the effect of floor on municipal bond yield spreads. I use rental price for each county and city, provided by Zillow Observed Rent Index from 2010 to 2020. Columns (1) and (2) in Table 6 show that γ from equation (3) is economically small and statistically insignificant, when I use municipal bond yield spreads as the outcome variable and replace house price with rental price. Because renters do not pay property taxes, the small effect of rental prices on municipal bond yield spreads suggests that the decline in spreads operates through the property tax channel in equation (3), rather than through spuriously correlated factors.

Finally, the second placebo test uses revenue bonds instead of GO and school district bonds, in the spirit of Butler et al. (2025). Revenue bonds are not directly tied to property taxes, whereas GO and school district bonds are backed by property taxes. If the municipal bond yield spreads decrease through the property tax channel, then the effect should be very small for revenue bonds. Columns (3) and (4) in Table 6 indeed show this. The estimate γ is statistically insignificant and economically negligible when outcome variable is revenue bond yield spreads in equation (3). Together, these results suggest that the 11 bp decline in the spreads is through the property tax channel and floor policy, rather than other related channels.

3.4 Heterogeneous Effects

School District Income. The average treatment effect—an 11 bp decline in school district bond spreads in floor states—masks substantial heterogeneity. I estimate the staggered DiD specification in equation (3), allowing the treatment effect to vary by school district income groups. I interact the floor indicator with income tercile indicators based on district-level income. Panel A of Figure 6 reports the heterogeneous treatment effects. School districts in the lowest income group experience a 35 bp decline in bond spreads following the adoption of the floor. The estimate is statistically significant at 1% level. In contrast, districts in the highest income group show small and no statistically significant change in spreads relative to control issuers. Districts in the middle income group show a decline of 19 bps in spreads, which is a larger decline than the highest income group. Taken together, the floor policy mitigates downside risk of property taxes. Because such downside risk is more pronounced in lower-income districts, the protective effect of the floor is concentrated among fiscally vulnerable school districts.

Federal transfers. On average, federal transfers account for 20–30% of local government revenue, providing an additional fiscal buffer through intergovernmental transfers. Federal transfers occur on a regular basis as part of the federal government's ongoing fiscal relationship with local governments. For example, cities of Bloomington and Martinsville in Indiana received federal funding for the construction of Interstate highway I-69.[24]

To examine the heterogeneity by federal transfers, I estimate the staggered DiD specification allowing the treatment effect to vary by terciles of federal transfers. Panel B of Figure 6 shows that when school districts have low income and received less federal transfers, they experience the largest decline in bond spreads following the adoption of the floor. The school districts with low income and low federal transfers show 41 bps decline in spreads following the floor adoption. The school districts with low income but received high federal transfers have smaller effects of floor on spreads, showing an 11 bp decline. In contrast, districts that receive high federal transfers and

[24] FHWA I-69 Section 5

have high income show little to no statistically significant response. These findings suggest two implications. First, floor serves as a protection against municipalities with the highest downside risk, which are those with low income and low federal transfers. Second, federal transfers also protect local governments from the downside risk. Therefore, the floor is a more binding and valuable form of insurance against the negative shocks.

Reliance on property tax revenue. The impact of floor on spreads is larger for local governments that depend more heavily on property tax revenue. Across local governments, property taxes account for 30% of total tax revenue on average, but with substantial variation ranging from 2% to 85%, as shown in the Appendix Figure A.6. Local governments that rely less on property taxes rely more on sales and income tax revenue. I exploit this variation by estimating heterogeneous treatment effects across terciles of property-tax reliance, defined as the share of property tax in total revenue, in equation (3). Panel D of Figure 6 shows results. Bond spreads decline by 16 bps following floor adoption for governments with high reliance on property taxes. In contrast, governments with less reliance on property tax experience smaller and statistically weaker effects of 4 bps. This pattern supports the fiscal policy channel: when property taxes are a larger share of total revenue, a floor that reduces their downside volatility has a greater impact on decreasing expected default risk.

Municipal bond maturity. The effect of the floor on spreads is substantially stronger for long-term bonds than for short-term bonds. As shown in Panel C of Figure 6, the floor lowers yield spreads of long-term bonds by 20 basis points, compared with 7 and 4 bps for medium- and short-term bonds. Following Gustafson et al. (2023), Painter (2020), and Gao and Murphy (2022), I use this evidence to show that the decline in spreads is not driven by a reduction in short-term rollover risk, but instead by a fundamental exposure to fiscal policy.[25] Long-term bonds embed expectations about fiscal fundamentals over extended horizon, whereas short-term bonds

[25] In similar spirit, Gustafson et al. (2023), Painter (2020), and Goldsmith-Pinkham et al. (2023) show that spreads of long-term municipal bonds are larger than short-term bonds due to migration risk, climate risk, and sea-level rise, respectively.

embed rollover risk. This maturity gradient suggests that the policy does not operate through short-run rollover risk. Instead, it mitigates persistent downside exposure in future revenue and expenditures.

In practical terms, I construct state-level yield curves using transaction-level data for bonds with maturities ranging from one to 30 years issued between January 2000 and December 2022. I estimate the term structure using the reformulated Nelson and Siegel (1987)'s model of Diebold and Li (2006), as shown in the Appendix Figure A.5. This estimation allows me to use yields across maturities and compare policy effects along the entire yield curve rather than relying on sparse individual bond transactions. Details on estimate are provided in the Appendix Section C.

3.5 Robustness Checks.

A potential concern with the results so far is that the bonds that are protected by the floor regulation are influenced by bond risks and issuer characteristics not captured by the rich set of controls. To alleviate this concern, I conduct a battery of robustness checks and confirm that these alternative risks are not the main drivers of the main estimates.

Liquidity risk. First, I find no significant differences in the change in spreads between bonds with high and low liquidity risks. Municipal bonds issued by local governments that are protected by the floor regulation are not simply more liquid bonds. Table 7 reports changes in bond spreads following the adoption of the floor for subsamples of municipal bonds that have high and low liquidity risks. Estimates on the change in spreads differ little between municipal bonds with high and low liquidity risks following the floor regulation. Bond liquidity is measured with the bid-ask spread (Roll, 1984), price of impact (Amihud, 2002), and dispersion of traded bond prices around the market consensus (Jankowitsch et al., 2011). Details on measurements are presented in Section B. While main results control for liquidity risk, this subsample analysis further supports that liquidity risk is not the main driver of the effect of floor on spreads.

Co-party affiliation. Second, the change in spread differs little between local governments whose governor is from the same party with the U.S. president (i.e., unified governments) and those from the different party (i.e., divided governments). A potential concern is that unified governments may adjust deficits faster than divided governments (Poterba, 1994) and therefore the decline in spreads is pronounced in unified governments. I first confirm that federal transfers to local governments do not differ significantly between unified and divided governments, as shown in Panel A of Table 8, which rules out a possibility that federal transfers are higher and therefore deficits are adjusted faster. Next, I split the bonds based on unified versus divided governments, and show that the spread change is similar in magnitude and the difference is statistically insignificant, as shown in Panels B and C of Table 8.

Excluding economic recessions. Third, results are robust excluding the Global Financial Crisis, Covid-19 Pandemic, and both. Municipal bonds are often viewed as safe assets and attract investors during periods of heightened uncertainty. Therefore, there is a significant peak in municipal bond yield spreads during the Global Financial Crisis. Because municipal default risk is pronounced in economic recessions, one might be worried that the floor effects are centered around economic recessions only. However, when I exclude the COVID-19 pandemic and the Global Financial Crisis from the analysis, the declining effect remains more than 8 bps even when either event is excluded, as shown in Table 9. Tellingly, it still holds at decline in spreads of 9 bps when both episodes are removed from the sample.

Local residents' race. Fourth, the results remain robust across municipalities with different races of local residents. Prior studies find that racial inequality creates gaps between assessment and market values of properties (Avenancio-León and Howard, 2022) and it make underwriting more expensive for historically Black colleges and universities (Dougal et al., 2019). A related concern in my study is that the floor effects may be influenced by racial inequality. However, as shown in Table 10, spreads decline by 7 bps to 13 bps across jurisdictions with varying racial compositions. This table reports estimates after controlling for racial and ethnic composition within each local government, including White, Black, Hispanic, Asian, and Non-Hispanic populations. There is no significant difference in the bond spread change between local governments with more or less Black and Hispanic populations.

4 Real Effects of Borrowing Channel on Human Capital Investment

In this section, I investigate the potential real effects of property tax floor on subsequent human capital investment via borrowing channel through lower municipal bond yield spreads.

To what extent might the floor for school district's property tax have real effects? The literature on school education has no consensus on whether financial support from the government has real effects on education outcomes. For example, Hanushek (1997) writes "State legislatures could decide how much money to invest in schools and could trust local districts to apply funds in a productive manner. But the fact that local districts do not use funds effectively complicates this picture. The clearest message of existing research is that uniform resource policies will not work as intended". By contrast, Hedges et al. (1994) shows "systematic positive relations between resource inputs and school outcomes", reviewing the (subset of) the studies in Hanushek (1997), and suggesting that a quarter of studies that shows positive and statistically significant relationship cannot be ignored.

Much of the disagreement comes from the possibility of endogeneity concerns of specifications. Earlier studies used functional forms that raise concerns of omitted variables, either at the student, class, school, or state level. To address these concerns, Krueger (1999) brings in a large-scale randomized experiment on class size, namely the Tennessee Student/ Teacher Achievement Ratio experiment, where students and teachers were randomly assigned to different size classes from kindergarten through third grade. He uses this randomized experiment and shows that the state funding increased student test scores especially for small classes, and teacher characteristics had little effect. Motivated by these findings, I investigate whether protection on property tax has real effects on human capital investments. In a similar spirit, Lafortune et al. (2018) uses post-1990 school finance reforms to suggest that state funding increases student achievement in low-income districts. However, these studies do not examine borrowing channel and municipal bond market, and also do not study property tax channel. They focus on state funding, where local governments have limited controls. The difference with my study is I focus on local government's (constrained) choice to levy property taxes, exploiting a unique set of property tax floors for school districts, and examine the borrowing channel. This channel is distinct from state funding in that local governments levy property taxes by applying mill rates to assessed values, which they spend a lot of efforts to stabilize.

Having established in Section 3 that property tax floor lowers borrowing costs through lower school district bond yield spreads, I assess whether this borrowing channel interact with floor to jointly affect human capital investments. I estimate a variation of equation (2), where I test the effects of floor on real effects using school district-level education outcomes.

$$\text{Real Effects}_{it} = \beta \ \text{Floor}_{st} + \pi C_{it} + \eta_i + \mu_s + \delta_t + \epsilon_{it} \tag{6}$$

for school district i, state s, and year t. I examine two sets of outcome variables. First, I study real effects on staff salaries and student participation, focusing on total salaries per pupil (pp), teacher salaries pp, employee benefits pp, and student dropout rate (%). Second, I study current expenditure on education, instruction, support services, and school lunch. The salaries, benefits, and expenditure are in log, thus the coefficient β shows percent change. The outcome variables use district-level data provided by the National Center for Education Systems (NCES). Consistent with equation (3), μ_s denotes state FE and δ_t is year FE. I add school district FE η_i in equation (6) to account for time-invariant characteristics of school districts.

Table 4 shows results. Property tax floor increases total staff salaries pp by 1.6% under 5% statistical significance. When I focus on instruction staff salaries, the effect is larger, showing 5% increase under 1% statistical significance. The employee benefits pp also increase by 10%. The student dropout rate declines by 3 pp. In Panel B, I show that the property tax floor increases current expenditures, particularly those related to education outcomes in school districts. Current expenditures on education increase by 3.8%, instructional expenditures increase by 5 %, expenditures on support services increase by 2.7%, and expenditures on school lunch increase by 7.8%. The results speak directly to a complementarity between the legal protection on revenue and real outcomes on outcome. The protection on school district revenue increases expenditure on education, but importantly, has real effects on salaries, teaching environment, and student achievement.

For the causal estimates to be valid, the parallel trend assumptions must hold. Figure 5 shows that in years prior to the adoption the floor, the differences between treated and control school districts in education outcomes are economically small and not statistically different from zero. In contrast, there are significant differences, statistically and economically, between treated and control school districts post the staggered adoption of the floor. In Panel A, school districts in floor states show a steady increase in instruction salaries per pupil from 2 to 6% post the adoption of the floor compared to the school districts in non-floor states. In Panel B, the student dropout rate declines by 2 to 4 percentage points in school districts in floor states compared to those in non-floor states post the adoption of the floor. Altogether, parallel trends assumptions hold well, supporting the causal link between the floor and enhancement in student achievement and the teaching environment.

5 Conclusion

While property taxes are the largest revenue source for local governments, they are understudied in municipal bond valuation. This study provides an analysis of how property tax institutions, particularly focusing on floors (i.e., lower bound caps that restrict declines in assessed values and mill rates) for school districts, affect municipal bond valuation and subsequent changes in human capital investments. By challenging the common assumption that property taxes are strictly proportional to house prices, this paper demonstrates that local governments face significant downside risk in tax collections. Property tax institutions directly dictate a local government's revenue-raising capacity and its subsequent credit risk. Floor adoption reduces revenue volatility by 9%. This stabilization leads to an 11-bp reduction in school district bond yield spreads, an effect comparable in economic magnitude to a credit rating upgrade from AA− to AAA. These reduced borrowing costs have real effects on education outcomes, resulting in an increase in instruction salaries, and a decrease in student dropout rates. Explaining these results, I find an increase in school district bond issuance and the expenditure on education and instruction post the adoption of the floor.

While this study focuses on property tax floors, they are not the only regulations governing local property taxation. Other important regulations include upper-bound caps that restrict increases in property tax revenue and variation in mandatory reassessment cycles. Unlike floors, these regulations constrain the upward adjustment of property tax revenue. A promising avenue for future research is to examine how these regulations constrain the fiscal adjustment of local governments and, in turn, influence municipal bond valuation.

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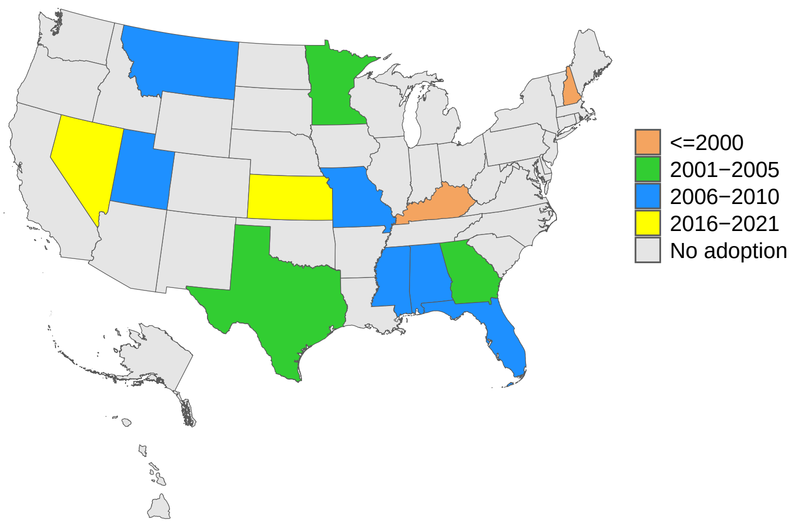
Figures and Tables

Figure 1: **Floor Legislation in the U.S.**
Panel A shows adoption years of states that mandate a floor for school districts' property tax revenue. States are partitioned into two mutually exclusive groups: level floor states and growth floor states. Panel B plots the coefficients and 95% confidence intervals from bivariate regressions of an indicator for floor adaption on the pre-adoption economic and demographic characteristics. The dependent variable equals one if a state had enacted a floor by year t. All variables are measured at the state level, standardized to have mean zero and unit variance, and lagged by one year.

Panel A. State-wide Adoption of Floors



Panel B. What characteristics predict the state-wide adoption of floors?

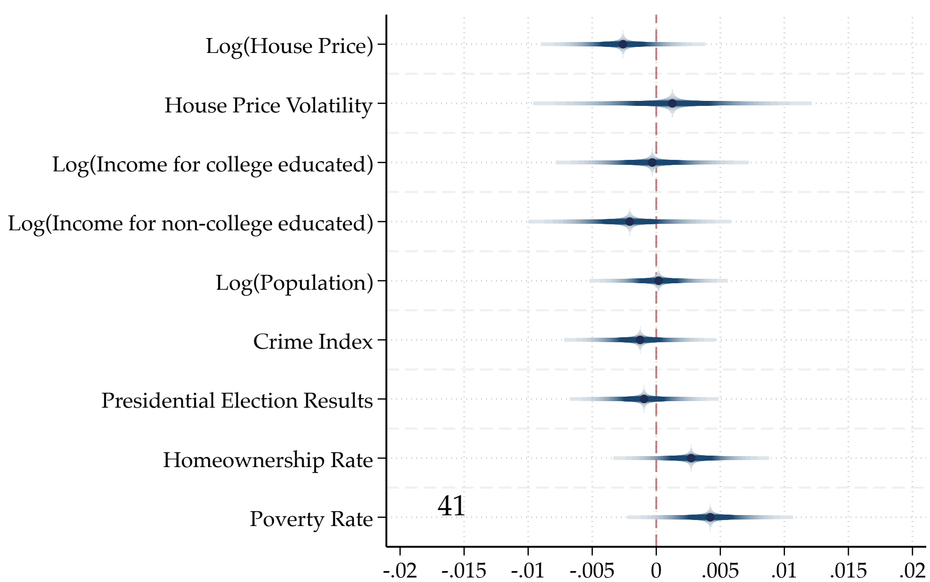
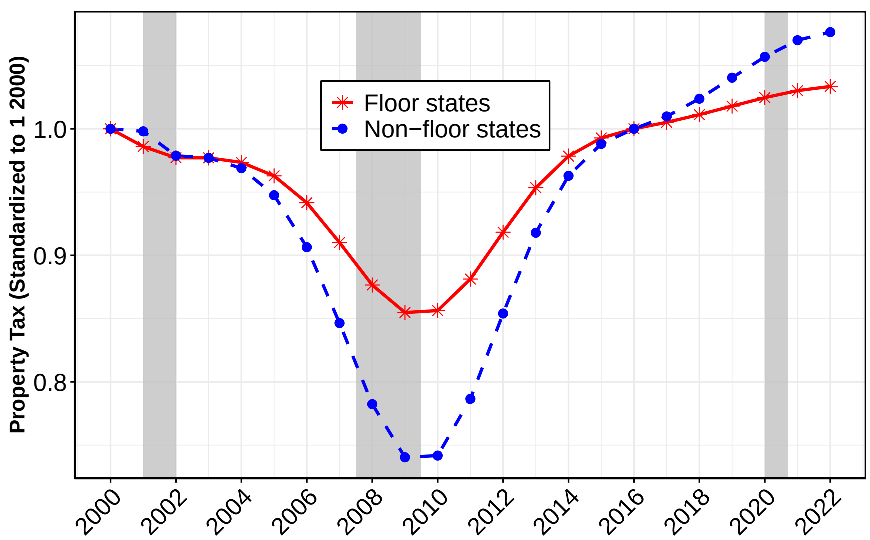


Figure 2: **Is Property Tax Floor Binding?**
This figure compares the average level (Panel A) and rolling volatility (Panel B) of property tax revenues for school districts in states with and without floors. Property tax revenue levels are normalized to one in 2000. Volatility is calculated as the annual average of the three-month rolling standard deviation of log changes in property tax revenue.

Panel A. Property Tax Levels: Floor vs. Non-Floor States



Panel B. Property Tax Volatility: Floor vs. Non-Floor States

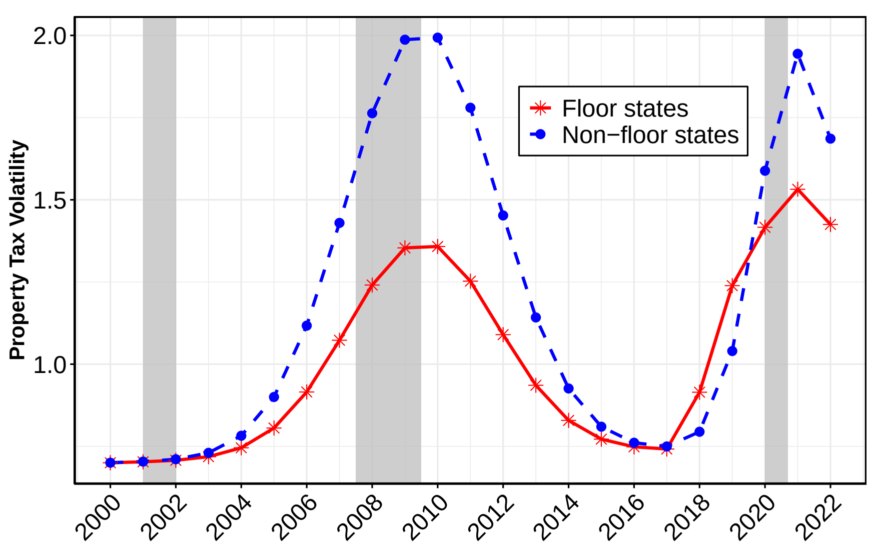


Figure 3: **Average Municipal Bond Yield Spreads.**
This figure shows the value-weighted average and the cross-sectional dispersion ($25^{th} - 75^{th}$ percentile band) of tax-adjusted yield spreads of municipal bonds the U.S. from January 2000 to December 2022.
$Spread_{bit} = \frac{\text{yield}_{bit}}{(1-\tau_t^{\text{fed}})(1-\tau_{s,t}^{\text{state}})} - r_t^f$, for municipal bond b issued by local government i in year t, and the maturity-matched risk-free rate r_t^f. This measure follows Schwert (2017) and adjusts yield spreads for tax-exempt status to account for market fragmentation. τ_t^{fed} and $\tau_{s,t}^{\text{state}}$ denote the top federal and state income tax rates, respectively.

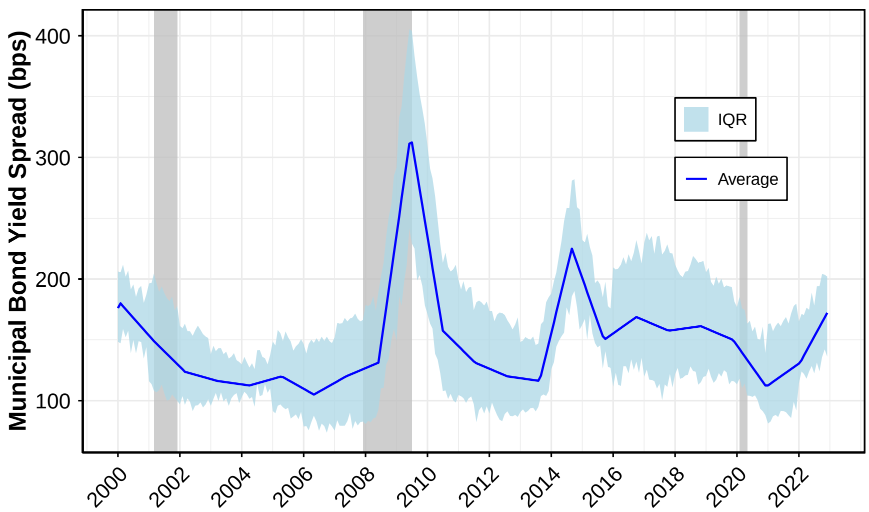
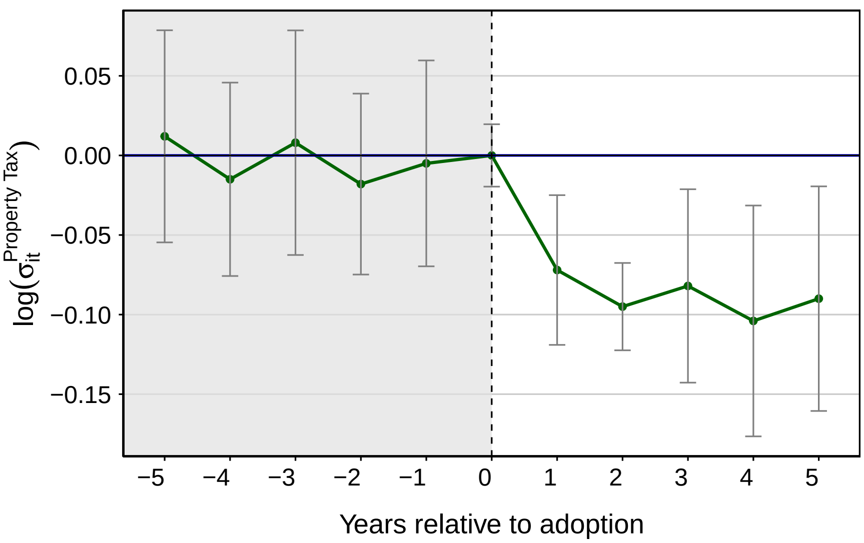


Figure 4: **Effects of Floor on Property Tax Volatility and Municipal Bond Spreads.**
This figure reports the annual effects of floor on property tax volatility and municipal bond yield spreads defined as in equation (1), estimated using equation (3). Property tax volatility, $\sigma_{it}(\text{Property Tax}_{it})$, is defined as the three-year rolling standard deviation of changes in per-capita property tax revenue. Panels A and B correspond to the event-study estimates of the coefficients reported in columns (1) and (2) of Panel B of Table 3, respectively. The bandwidth reports 95% confidence intervals. The x axis plots five years pre- and post- staggered adoption of floors.

Panel A. Ex-Post Effects on Property Tax Volatility



Panel B. Ex-Post Effects on School District Bond Yield Spreads

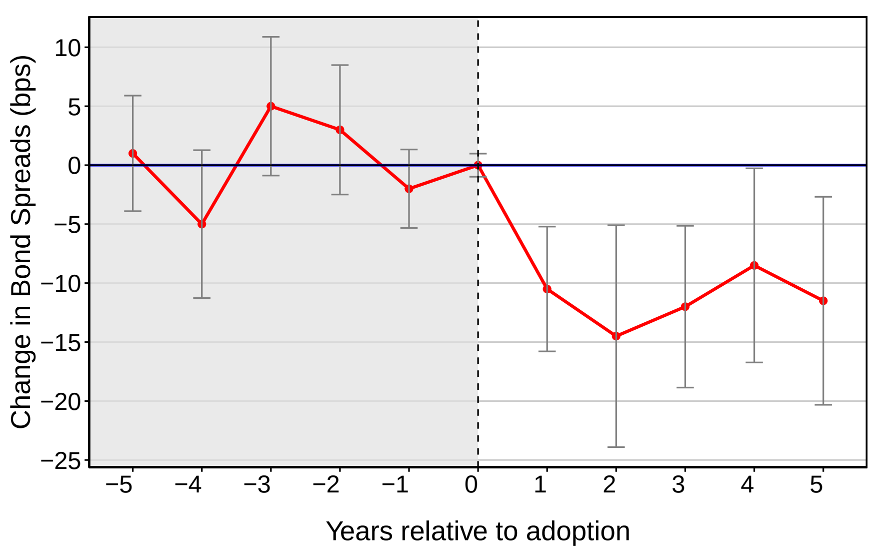
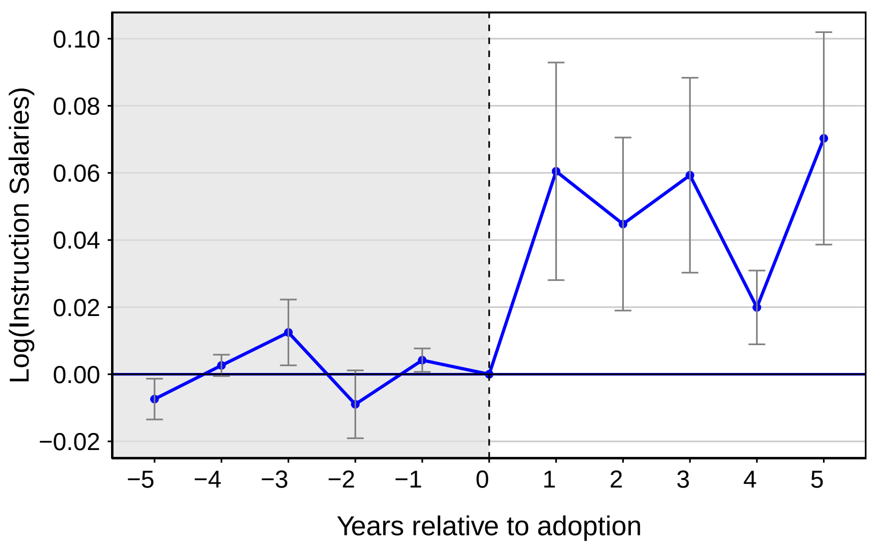


Figure 5: **Effects of Floor on Education Outcomes.**
This figure reports the annual effects of floor on instruction salaries and student dropout rates in public school districts, estimated using equation (6). Panels A and B correspond to the event-study estimates of the coefficients reported in columns (2) and (4) of Panel A of Table 4, respectively. The bandwidth reports 95% confidence intervals. The x axis plots five years pre- and post- staggered adoption of floors.

Panel A. Effects on Log(Instruction Salaries Per Pupil)



Panel B. Effects on Student Dropout Rate

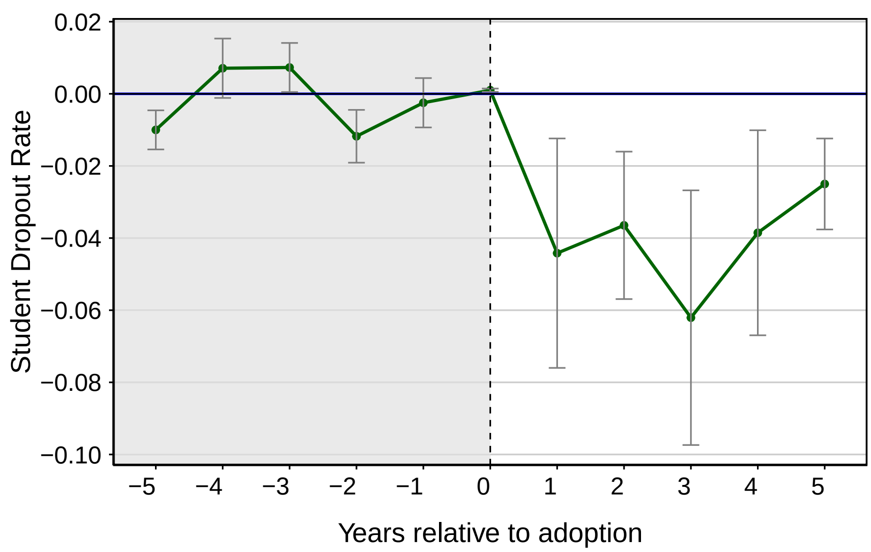


Figure 6: **Effect of floor on municipal bond spreads by heterogeneity.**
This figure presents four panels illustrating the heterogeneous effects of house price volatility interacted with the floor on municipal bond yield spreads. In Panel A, I present estimates from regressions that augment equation (3) with interaction by terciles of school district income. In Panel B, I augment two interaction terms to allow the treatment effect to vary along two dimensions—local government income (low vs. high) and federal transfers (low vs. high). In Panel C, I augment with interaction by terciles of municipal bond maturity. In Panel D, I augment with interaction by terciles of property tax-reliance, defined as the share of property tax in total revenue.

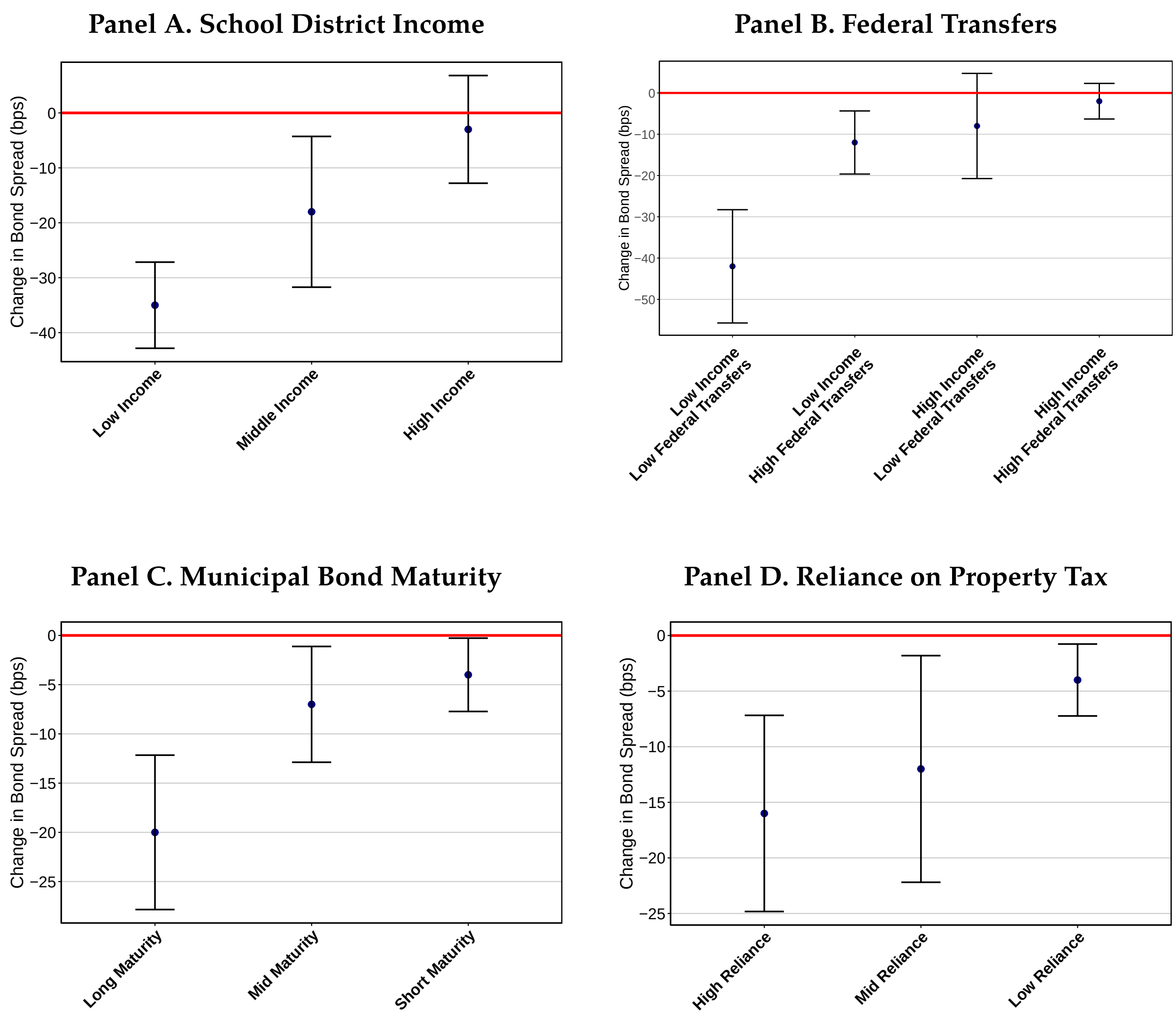


Table 1: **Descriptive Statistics on Municipal Bonds and Property Taxes.**
This table reports mean, standard deviation, 1st, 25th, 75th, and 99th percentiles of key variables used in this study. The tax-adjusted yield spread equals the yield minus the maturity-matched risk-free rate, with the yield adjusted for income taxes as in equation (1). Panel B presents summary statistics of house prices and property taxes. Data on house prices are sourced from Zillow Home Value Index and FHFA house index. Data on property tax are sourced from CoreLogic Tax-Deeds. The annual data span from 2000 to 2022. The Appendix Table A.3 provides summary statistics of additional variables.

	Mean	**SD**	**1st**	**25th**	**75th**	**99th**
A: Municipal Bond Characteristics						
Tax-adjusted yield spread (%)	1.43	0.85	0.03	0.47	2.27	3.97
Tax-adjusted yield (%)	4.79	0.81	3.09	4.35	5.43	6.28
Yield (%)	2.83	0.48	1.89	2.50	3.27	3.71
Months to maturity	144.85	83.49	25	82	192	374
Coupon rate (%)	4.10	1.09	1.05	3.40	5	6
B: Property Tax and House Prices						
Property tax volatility (3-year)						
School District ($)	632	1,440	4	55	578	6,738
County ($)	2,069	4,638	20	207	1,880	21,369
Property tax volatility (10-year)						
School District ($)	1,380	2,368	23	163	1,425	11,638
County ($)	5,205	8,686	98	718	5,508	42,952
House price volatility (3-year)						
School District (Index = 1990)	17	18	2	6	22	88
County ($)	10,413	12,778	1,144	3,298	12,742	61,785
House price volatility (10-year)						
School District (Index = 1990)	34	30	7	13	46	146
County ($)	22,727	23,683	3,920	8,189	28,647	111,950
Property tax/ Total tax revenue	0.318	0.214	0.021	0.141	0.455	0.859
Federal Transfers (Mil., $)	124	1,458	0.028	3	32	1,025
Assessment value ($)	169,971	160,153	43,415	123,816	200,177	1,628,949
C: Number of Observations						
Issuers	School Districts	Counties	States			
	3,606	2,270	50			
Bonds	GO	School district				
	41,234	68,001				
Trades	GO	School district				
	533,784	697,366				

Table 2: **Descriptive Statistics on District-level Education Outcomes.**
This table reports mean, standard deviation, 1st, 25th, 75th, and 99th percentiles of key variables used in this study. The tax-adjusted yield spread equals the yield minus the maturity-matched risk-free rate, with the yield adjusted for income taxes as in equation (1). Panel B presents summary statistics of house prices and property taxes. Data on house prices are sourced from Zillow Home Value Index and FHFA house index. Data on property tax are sourced from CoreLogic Tax-Deeds. The annual data span from 2000 to 2022. The Appendix Table A.3 provides summary statistics of additional variables.

	Mean	**SD**	**1st**	**25th**	**75th**	**99th**
Total salaries per pupil ($)	5861	4230	2936	4790	7636	17639
Instruction salaries per pupil ($)	3985	2863	1985	3270	5211	12220
Employee benefits per pupil ($)	2280	1720	702	1570	3341	8448
Student dropout rate (%)	4.4	3.8	0	1.1	8.8	20.9
Education expenditure per pupil ($)	10288	5565	5270	8215	13639	30709
Instruction expenditure per pupil ($)	6148	3311	3133	4911	8158	18469
Support services expenditure per pupil ($)	3638	2369	1636	2804	4947	12055
School lunch expenditure per pupil ($)	1390	980	0	770	1980	3790
Number of students	1504	16590	86	639	3491	38122

Table 3: **Effects of Floor on Property Tax Volatility and Municipal Bond Yield Spreads.** This table reports the staggered difference-in-differences coefficients on the effect of floors on property tax volatility and municipal bond yield spreads. Panel A presents ex-ante effects on municipal bond yield spreads defined as in equation (1) estimated using equation (2). Floor$_{st}$ equals one if state s imposes a minimum threshold on school district property tax revenue in year t, and zero otherwise. School$_i$ equals one if issuer i is a school district and zero if it is a county or city. Panel B reports ex-post effects based on equation (3). Property tax volatility, σ_{it}(Property Tax$_{it}$), is defined as the three-year rolling standard deviation of changes in per-capita property tax revenue. To address endogeneity concerns, house price volatility is instrumented using the interaction between national house price volatility and local housing supply elasticity, as specified in equation (4). All specifications include state fixed effect, year fixed effect and the full set of control variables described in Appendix Section A.

Panel A: Ex-ante Effects

	(1)	(2)	(3)	(4)
	Municipal Bond Spreads		**Log(Municipal Bond Issuance)**	
	Coefficient	t-stat	Coefficient	t-stat
β Floor$_{st}$ × School$_i$	-0.062*	(-1.917)	0.118***	(2.789)
π_0 (Floor$_{st}$)	-0.007	(-0.534)	0.034*	(1.888)
π_1 (School$_i$)	0.040	(0.530)	0.020	(1.234)
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	1,231,150		1,231,150	
Adj. R^2	0.255		0.341	

Panel B: Ex-post Effects

	(1)	(2)	(3)	(4)
	σ_{it}(Property Tax$_{it}$)		**Municipal Bond Spreads**	
	Coefficient	t-stat	Coefficient	t-stat
γ (Floor$_{st}$× School$_i$ × σ_{it}(House Price$_{it}$))	-0.087***	(-3.021)	-0.110**	(-2.317)
π_0 (Floor$_{st}$)	0.004	(0.412)	0.006	(0.521)
π_1 (School$_i$)	-0.006	(-0.587)	-0.029*	(-1.742)
π_2 (σ_{it}(House Price$_{it}$))	0.331***	(3.118)	0.184***	(3.006)
π_3 (Floor$_{st}$× School$_i$)	-0.052**	(-2.214)	-0.067***	(-2.881)
π_4 (Floor$_{st}$ × σ_{it}(House Price$_{it}$))	-0.009	(-0.643)	0.011	(0.904)
π_5 (School$_i$ × σ_{it}(House Price$_{it}$))	-0.028*	(-1.694)	-0.019*	(-1.667)
ϕ (House Price$_t^{\text{National}}$ × Elasticity$_i$)	-0.130**	(-2.270)	-0.177**	(-2.003)
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	382,100		1,231,150	
F	21.900		18.530	
Adj. R^2	0.405		0.517	

Table 4: **Effects of Floor on Education Outcomes in School Districts.**
This table reports the staggered difference-in-differences coefficients on the effect of floors on district-level real outcomes estimated using equation (6). In Panel A, the outcome variables are logarithms of total salaries per pupil (pp), instruction salaries pp, and employee benefits pp. Another outcome variable is student dropout rate (%). In Panel B, the outcome variables are current expenditures on education, instruction, support services and school lunch. Floor_{st} equals one if state s imposes a minimum threshold on school district property tax revenue in year t, and zero otherwise. All specifications include state FE, school district FE, year FE and a full set of district-level controls described in Appendix Section A.

Panel A: Teacher Salaries Per Pupil (PP) and Student Participation

	(1)	(2)	(3)	(4)
	log(**Total Salaries PP**)	log(**Instruction Salaries PP**)	log(**Employee Benefits PP**)	**Student Dropo**
β $(\text{Floor})_{st}$	0.016**	0.049***	0.103***	−0.380
t-stat	(2.377)	(6.468)	(6.764)	(−5.5
District FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	69,840	69,840	69,840	69,84
Adj. R^2	0.423	0.458	0.384	0.22
Mean Dep. Var.	8.676	8.290	7.732	4.4%
Mean ($)	$5861	$3985	$2280	

Panel B: Current Expenditure Per Pupil

	(1)	(2)	(3)	(4)
	log(**Education PP**)	log(**Instruction PP**)	log(**Services PP**)	log(**Lunch PP**)
β (Floor_{st})	0.038***	0.051***	0.027***	0.078**
t-stat	(4.688)	(5.278)	(3.290)	(2.131)
School District FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	69,840	69,840	69,840	69,840
Adj. R^2	0.585	0.626	0.476	0.333
Mean Dep. Var.	9.238	8.723	8.201	7.237
Mean ($)	$10288	$6148	$3648	$1390

Table 5: **Effects of Floor Using Close Elections.**
This table reports the staggered difference-in-differences coefficients on the effect of floors on property tax volatility and municipal bond yield spreads. Close_s equals one if a state s passed a floor for school districts through general elections that were decided by narrow margins and zero otherwise. There are four states that adopted the floor through close elections, which are Alabama, Georgia, Missouri, and Nevada. The data span from 2000 to 2022.

	(1)	(2)	(3)	(4)
	σ_{it}(**Property Tax**$_{it}$)		**Municipal Bond Spreads**	
	Coefficient	t-stat	Coefficient	t-stat
$\beta(\text{Pass}_s \times \text{Close}_s \times \text{School}_i \times \text{Post}_{st})$	-0.067*	(-1.781)	-0.124*	(-1.888)
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	382,100		1,231,150	
N ($\text{Close}_s = 1$)	12,749		78,492	
F	11.010		12.987	
Adj. R^2	0.193		0.090	

Table 6: **Placebo Tests.**

This table presents placebo tests. In columns (1) and (2), I re-estimate equation (3), replacing house prices with rental prices, while retaining municipal bond yield spreads as the outcome variable. In columns (3) and (4), I use revenue bond yield spreads—rather than general obligation and school district bond spreads—as the dependent variable, and maintaining house prices in the specification. The data span from 2000 to 2022, except for rental prices, which cover 2010 to 2020 due to data limitations provided by the Zillow Home Value Index.

	(1)	(2)	(3)	(4)
	Rental Price		**Revenue Bond Spreads**	
	Coefficient	t-stat	Coefficient	t-stat
γ (Floor$_{st}\times$ School$_i$ $\times$ σ_{it}(Price$_{it}$))	-0.048	(-1.344)	-0.029	(-1.082)
π_0 (Floor$_{st}$)	0.151	(1.318)	0.131	(0.435)
π_1 (School$_i$)	0.002	(0.184)	-0.004	(-0.327)
π_2 (σ_{it}(Price$_{it}$))	0.015	(0.412)	-0.008	(-0.266)
π_3 (Floor$_{st}\times$ School$_i$)	-0.006	(-0.521)	0.003	(0.248)
π_4 (Floor$_{st}$ $\times$ σ_{it}(Price$_{it}$))	0.001	(0.093)	-0.002	(-0.144)
π_5 (School$_i$ $\times$ σ_{it}(Price$_{it}$))	-0.004	(-0.381)	0.005	(0.417)
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	51,202		932,202	
Adj. R^2	0.038		0.041	

Table 7: **Effects of Floor on Municipal Bond Yield Spreads By Liquidity Risk.**
This table examines whether the effect of floor regulation on municipal bond yield spreads differs by bond liquidity risk. The specification follows equation (3). The sample is split into bonds with high and low liquidity risk, measured using bid–ask spreads (Roll, 1984), price impact (Amihud, 2002), and dispersion of transaction prices around market consensus (Jankowitsch et al., 2011). Spread_{bit} is the yield spread of municipal bond b measured as in equation (1). Floor_{st} equals one if state s mandates a minimum threshold for school districts' property tax revenue in year t and zero otherwise. School_i equals one if issuer i is a school district and zero if it is a county or city. Property tax volatility and house price volatility are defined as three-year rolling volatilities. House price volatility is instrumented using the interaction between national house price volatility and supply elasticity as presented in equation (4). All specifications include the full set of controls described in Appendix Section A.

Panel A: Ex-ante Effects

	(1)	(2)
	High Liquidity Risk	**Low Liquidity Risk**
β ($\text{Floor}_{st} \times \text{School}_i$)	-0.064*	-0.059*
	(-1.890)	(-1.941)
State FE	Y	Y
Year FE	Y	Y
N	602,411	609,598
Adj. R^2	0.251	0.258

Panel B: Ex-post Effects

	(1)	(2)	(3)	(4)
	High Liquidity Risk		**Low Liquidity Risk**	
	Coefficient	t-stat	Coefficient	t-stat
γ ($\text{Floor}_{st} \times \text{School}_i \times \sigma_{it}(\text{House Price}_{it})$)	-0.108**	(-2.284)	-0.093**	(-2.341)
π_0 (Floor_{st})	0.005	(0.498)	0.007	(0.612)
π_1 (School_i)	-0.031*	(-1.732)	-0.027*	(-1.684)
π_2 ($\sigma_{it}(\text{House Price}_{it})$)	0.191***	(3.044)	0.179***	(2.988)
π_3 ($\text{Floor}_{st} \times \text{School}_i$)	-0.069***	(-2.901)	-0.065***	(-2.842)
π_4 ($\text{Floor}_{st} \times \sigma_{it}(\text{House Price}_{it})$)	0.012	(0.952)	0.010	(0.881)
π_5 ($\text{School}_i \times \sigma_{it}(\text{House Price}_{it})$)	-0.020*	(-1.658)	-0.018*	(-1.612)
ϕ ($\text{House Price}_t^{\text{National}} \times \text{Elasticity}_i$)	-0.174**	(-2.041)	-0.180**	(-2.087)
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	604,881		607,128	
F	13.210		17.876	
Adj. R^2	0.195		0.219	

Table 8: **Effects of Floor on Municipal Bond Yield Spreads by Co-Party Affiliation.**
This table examines whether the effects of floor regulation on municipal bond yield spreads differ by political alignment between the state governor and the U.S. president. Unified governments are defined as those in which the governor and the president belong to the same political party, while divided governments are defined as those with different party affiliations. Panel A compares federal transfers to state and local governments based on unified versus divided governments. The Difference column reports federal transfers to unified governments minus transfers to divided governments. Panels B and C report results estimated from equation (3). The sample is split into bonds issued by local governments that are unified and diversified.

Panel A: Federal transfers

	(1)	(2)	(3)	(4)
	Unified	**Divided**	**Difference**	**t-stat**
Local governments	10932451	11507005	-1980508	(-0.672)
State governments	12677879	13290614	-2393350	(-0.566)

Panel B: Ex-ante Effects

	(1)	(2)
	Unified Government	**Divided Government**
β (Floor$_{st}\times$ School$_i$)	-0.071**	-0.054*
	(-2.120)	(-1.730)
State FE	Y	Y
Year FE	Y	Y
N	296,845	315,164
Adj. R^2	0.166	0.122

Panel C: Ex-post Effects

	(1)	(2)	(3)	(4)
	Unified Government		**Divided Government**	
	Coef.	t-stat	Coef.	t-stat
γ (Floor$_{st}\times$ School$_i$ $\times$ σ_{it}(House Price$_{it}$))	-0.112**	(-2.311)	-0.107**	(-2.281)
π_3 (Floor$_{st}\times$ School$_i$)	-0.069***	(-2.880)	-0.065***	(-2.741)
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	301,112		310,897	
Adj. R^2	0.113		0.223	

Table 9: **Effects of Floor on Municipal Bond Yield Spreads Excluding Economic Recessions.** This table re-estimates the ex-post specification in equation (3) after excluding major recession episodes. Column (2) excludes the Global Financial Crisis (2007–2009), Column (3) excludes the COVID-19 pandemic (2020–2021), and Column (4) excludes both periods. All specifications include the full set of control variables. The persistence of the negative coefficient indicates that the effect of floor regulation on municipal bond yield spreads is not driven solely by recessionary periods.

	(1)	(2)	(3)
	Excl. GFC	**Excl. COVID**	**Excl. Both**
γ (Floor$_{st}\times$ School$_i$ $\times$ σ_{it}(House Price$_{it}$))	-0.086**	-0.113**	-0.091**
	(-2.189)	(-2.273)	(-2.000)
π_0 (Floor$_{st}$)	0.007	0.005	0.006
	(0.612)	(0.521)	(0.483)
π_1 (School$_i$)	-0.028*	-0.031*	-0.024
	(-1.724)	(-1.842)	(-1.392)
π_2 (σ_{it}(House Price$_{it}$))	0.172***	0.191***	0.158**
	(2.813)	(3.044)	(2.214)
π_3 (Floor$_{st}\times$ School$_i$)	-0.061***	-0.069***	-0.055**
	(-2.673)	(-2.912)	(-2.061)
π_4 (Floor$_{st}$ $\times$ σ_{it}(House Price$_{it}$))	0.010	0.012	0.008
	(0.834)	(0.947)	(0.668)
π_5 (School$_i$ $\times$ σ_{it}(House Price$_{it}$))	-0.018*	-0.021*	-0.015
	(-1.634)	(-1.703)	(-1.412)
State FE	Y	Y	Y
Year FE	Y	Y	Y
N	956,214	1,074,903	800,267
Adj. R^2	0.203	0.309	0.191

Table 10: **Effects of Floor on Municipal Bond Yield Spreads Controlling for Racial Composition.** This table examines whether the estimated ex-post effect of floor regulation on municipal bond yield spreads is sensitive to controlling for racial composition within local governments. Each column augments equation (3) by including the population share of the indicated racial group. Column (6) includes all racial composition variables simultaneously; one category is omitted to avoid perfect multicollinearity. γ denotes the coefficient on $\text{Floor}_{st} \times \text{School}_i \times \sigma_{it}(\text{House Price}_{it})$. House price volatility is instrumented using the interaction between national house price volatility and local housing supply elasticity, as specified in equation (4). All specifications include year fixed effect and the full set of control variables described in Appendix Section A.

	(1)	(2)	(3)	(4)	(5)
	White	Black	Hispanic	Asian	Non-Hispanic
γ ($\text{Floor}_{st} \times \text{School}_i \times \sigma_{it}(\text{House Price}_{it})$)	-0.134***	-0.076*	-0.118**	-0.069*	-0.123**
	(-2.731)	(-1.922)	(-2.253)	(-1.814)	(-2.365)
π_0 (Floor_{st})	0.006	0.004	0.005	0.003	0.006
	(0.588)	(0.402)	(0.517)	(0.301)	(0.621)
π_1 (School_i)	-0.033*	-0.021	-0.029*	-0.018	-0.031*
	(-1.823)	(-1.214)	(-1.691)	(-1.082)	(-1.742)
π_2 ($\sigma_{it}(\text{House Price}_{it})$)	0.203***	0.157**	0.188***	0.149**	0.196***
	(3.261)	(2.218)	(2.987)	(2.104)	(3.104)
π_3 ($\text{Floor}_{st} \times \text{School}_i$)	-0.072***	-0.051**	-0.067***	-0.046*	-0.070***
	(-2.941)	(-2.083)	(-2.712)	(-1.776)	(-2.884)
π_4 ($\text{Floor}_{st} \times \sigma_{it}(\text{House Price}_{it})$)	0.011	0.009	0.012	0.008	0.010
	(0.912)	(0.731)	(0.964)	(0.654)	(0.841)
π_5 ($\text{School}_i \times \sigma_{it}(\text{House Price}_{it})$)	-0.022*	-0.014	-0.019*	-0.012	-0.021*
	(-1.691)	(-1.104)	(-1.603)	(-0.982)	(-1.734)
State FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Adj. R^2	0.524	0.531	0.528	0.533	0.527

Online Appendix

A Measurements of Control Variables

Category	Description
Municipal bond characteristics	Bond's time to maturity, trading volume relative to the outstanding amount, credit rating, and bond-month SD of transaction prices.
Economic conditions of issuers	Growth in personal income of local residents, local employment, local population, and local GDP, all measured as annual growth from the previous year
Moody's recalibration	I exclude municipal bonds that changed their credit ratings after the Moody's 2010 calibration. Cornaggia et al. (2018) released the CUSIPs that changed the ratings on their website.
Liquidity risk	Amihud (2002)'s price of risk, Feldhütter (2012)'s round-trip cost and Roll (1984)'s bid-ask spread. See the Appendix Section B for measurements of these variables.
Firm risk	I value-weight the firm-level exposure to tax policies, economic policies, and budget deficits at the municipality-level. Hassan et al. (2019) provides a unique dataset on firm-level exposure to tax policy, economic policy, and budget deficits. Gao et al. (2021) provides firm's historical headquarter location from historical SEC filings.
Climate risk	I control for hazards and disasters, including climate-related events such as droughts, floods, heatwaves, and environmental contamination, using data from the EM-DAT dataset. I assign a value of 1 if a municipality experienced a hazard or disaster in a given month and 0 otherwise. The Appendix Table A.4 describes a list of these hazards from 2000 to 2022.
Federal transfers	Federal transfers refers to the transfers in dollar amount from the Federal government to a local government each year. The data are obtained from the U.S. Census Bureau's Census of Governments and Annual Survey of State and Local Government Finances.
Sales and income tax	I control for annual growth of sales tax and income tax of local governments. In this analysis, I exclude five no-sales tax states and nine no-income tax states are excluded. The data are obtained from the U.S. Census Bureau's Census of Governments and Annual Survey of State and Local Government Finances.
Migration risk	I control for migration risk using two measures. First, I use domestic net migration data for authorized immigration, provided by the Census. Second, I use national unauthorized migration population estimates provided by the Office of Homeland Security Statistics.
Local residents' race	Racial composition of each municipality in each year—specifically, the share of White, Black, Hispanic, Asian, and Non-Hispanic populations.

B Measurements of liquidity risk

A high value of the price impact measure proposed by Amihud (2002) indicates that trading a municipal bond results in a large price change. Thus, a high value of Amihud measure implies high liquidity risk. Feldhütter (2012) develops a measure of price dispersion based on the deviation of bond transaction prices from a market consensus valuation. This dispersion serves as a proxy for transaction costs: greater price dispersion implies higher round-trip costs and, consequently, higher liquidity risk in municipal bonds. Lastly, Roll (1984) develops a measure of the effective bid-ask spread based on the negative serial covariance of transaction price changes. Here, a larger Roll measure indicates more negative autocovariance in transaction prices, which in turn implies a wider effective bid-ask spread and higher liquidity risk.

B.1 Roll (1984)'s bid-ask spread.

I use Roll (1984)'s bid-ask spread to measure liquidity risk in municipal bonds. The high bid-ask spread refers to high liquidity risk in municipal bonds. Roll shows that the bid-ask spread can be measured by:

$$\text{Roll}_{b,t} = 2\sqrt{-\text{Cov}(\Delta P_k, \Delta P_{k-1})}, \tag{A.1}$$

where P_k denotes the price of a municipal bond b at trade k. I first estimate the monthly Roll measure, and then aggregate it to yearly using a median. Following Schwert (2017), I drop municipal bonds that have less than four trades within a month.

B.2 Amihud (2002)'s price of impact

I use Amihud (2002)'s price of impact to measure liquidity risk of municipal bonds following Schwert (2017). The price of impact is an average return per volume of trade on a given month for a bond. A high value of Amihud measure indicates that a small trade leads to a large price change, which implies high liquidity risk. Amihud (2002)'s price of impact is computed as:

$$\text{Amihud}_{b,t} = \frac{1}{N_t}\sum_{k=1}^{N_t}\frac{\left|\frac{P_k - P_{k-1}}{P_{k-1}}\right|}{Q_k}, \tag{A.2}$$

where N_t is the number of trades for a municipal bond b on month t, P_k denotes the bond price at trade k, and Q_k is the par amount of that trade. Following Schwert (2017), I drop bonds that have less than two transactions on a given day to estimate the Amihud measure of price of impact. I aggregate the monthly Amihud measure to yearly using a median.

B.3 Jankowitsch et al. (2011)'s price dispersion

Jankowitsch et al. (2011) proposes an alternative measure of liquidity risk, which is the dispersion of traded bond prices around the market consensus valuation. Jankowitsch et al. (2011) calculates the price dispersion as:

$$\text{Bond Price Dispersion}_{b,t} = \frac{\sum_{k=1}^{N_t}(P_k - M_t)^2 Q_k}{\sum_{k=1}^{N_t} Q_k}, \tag{A.3}$$

where N_t is the number of trades for bond b on day t, P_k is the price at trade k, Q_k is the par amount traded, and M_t is the market's consensus valuation. Jankowitsch et al. (2011) defines M_t as the volume-weighted average price for the trading day. Monthly dispersion values are computed by averaging the valid daily estimates within each month. Yearly dispersion are obtained by using the median of monthly dispersions.

C Term structure.

I use Diebold and Li (2006)'s reformulation of Nelson and Siegel (1987)'s yield curve, namely the three-factor yield curve model, providing the level, the slope, and the curvature.

$$y_{st}(h) = \underbrace{f_{st}}_{\text{level}} + \underbrace{f_{st}}_{\text{slope}} \cdot \frac{1 - e^{-h\chi}}{h\chi} + \underbrace{f_{st}}_{\text{curvature}} \cdot \left(\frac{1 - e^{-h\chi}}{h\chi} - e^{-h\chi}\right) + \varepsilon_{st}(h) \tag{A.4}$$

where $y_{st}(h)$ is the continuously compounded yield at maturity h issued by local governments in a U.S. state s in year t. The yield curve is expressed as a linear combination of the level, slope, and curvature components, where f_{1t}, f_{2t}, and f_{3t} are time-varying parameters. The loading on the level factor f_{1t} is a constant, which equally affects yields at different maturities. The loading on the slope factor f_{2t}, given by $\frac{1-e^{-h\chi}}{h\chi}$, begins at 1 when maturity h is near zero and decays monotonically toward 0 as h increases. Lastly, the loading on the curvature factor f_{3t}, expressed as $\frac{1-e^{-h\chi}}{h\chi} - e^{-h\chi}$, starts at 0, rises to a peak at medium maturities, and then decays toward 0.

This study uses equation (A.4) to estimate the yield curve related to maturities from one to thirty years. Diebold and Li (2006) estimate the factor values $\widehat{f_{1t}}$, $\widehat{f_{2t}}$, and $\widehat{f_{3t}}$ by fixing $\chi = 0.0609$, the value that maximizes the loading on the curvature factor. The parameter χ determines the maturity at which the curvature factor reaches its maximum.[26] I follow Diebold and Li (2006) and estimate equation (A.4) using a fixed value of $\chi = 0.0609$, after which I estimate f_{1t}, f_{2t}, and f_{3t} via OLS. By doing so, I obtain a panel of yields across maturities, states, and years, which allows me to examine how sensitivity influences the shape of the yield curve.

[26] Nelson and Siegel (1987)'s parsimonious yield curve is following:

$$y_{st}(h) = b_{1t} + b_{2t} \cdot \frac{1 - e^{-\chi_t h}}{\chi_t h} + b_{3t} \cdot e^{-\chi_t h} + \varepsilon_{st}(h), \tag{A.5}$$

b_{1t}, b_{2t}, b_{3t}, and χ_t are time-varying parameters. While Nelson and Siegel (1987) has a time-varying parameter χ_t, Diebold and Li (2006) treat it as fixed and suggest that there is little degradation of model fit. Furthermore, they say that the time-series variation of χ_t has little economic interpretation.

D Appendix Figures and Tables

Figure A.1: **Mill Rate Trends.**
This figure plots average and cross-sectional IQR of mill rates across counties from 2000 to 2022.

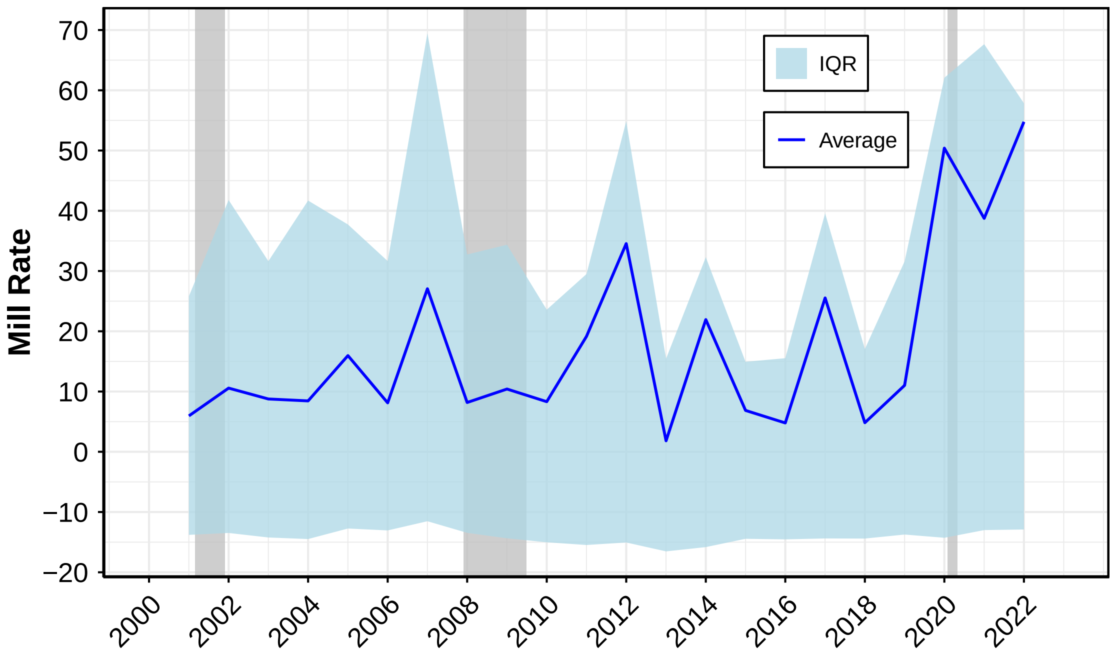
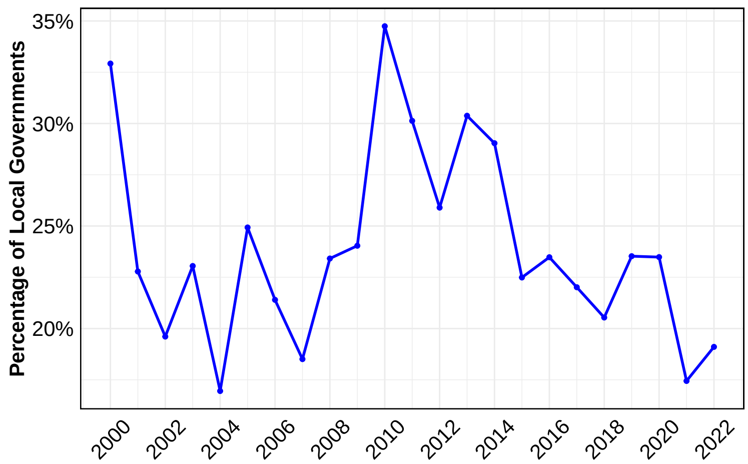


Figure A.2: **Macro Trends on Property Tax.**
This figure plots the macro trends on property tax per capita of local governments. Panel A plots the fraction of local governments that experienced a decline in property tax revenue per capita relative to the previous year. Panel B plots the average decline rate among these local governments. The Appendix Figures A.3 and A.4 show macro trends on increases and stable changes in property taxes.

Panel A. Percentage of local governments with declining property tax per capita



Panel B. Average decline rate of property tax per capita

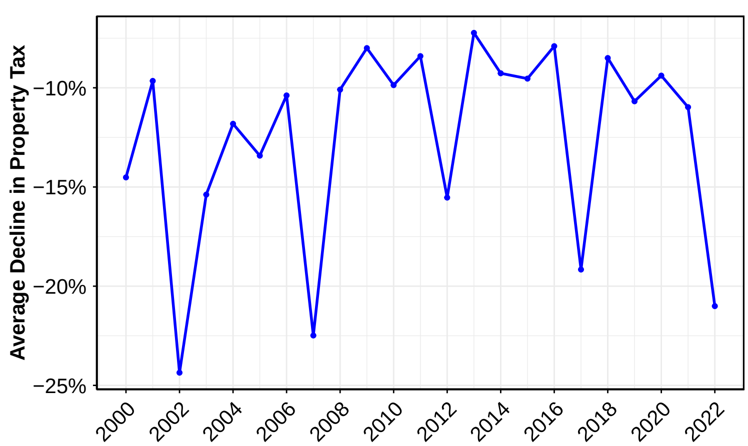
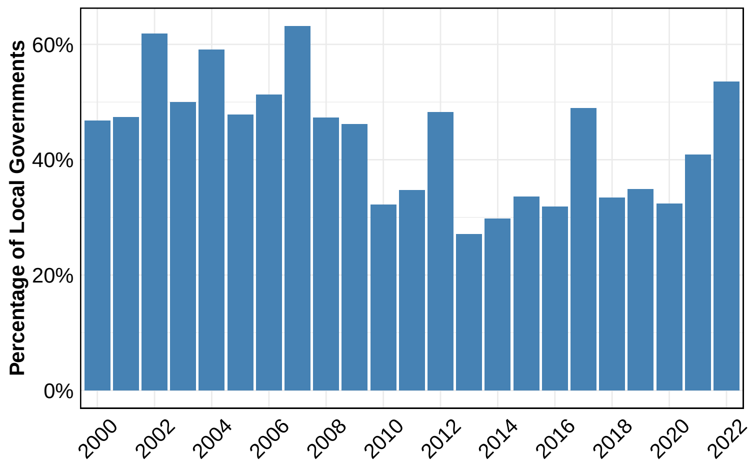


Figure A.3: **Increasing Trends on Property Tax.**
This figure plots the macro trends on property tax per capita of local governments. Panel A plots the fraction of local governments that experienced an increase in property tax revenue per capita relative to the previous year. Panel B plots the average increase rate among these local governments.

Panel A. Percentage of local governments with increasing property tax per capita



Panel B. Average increase rate of property tax per capita

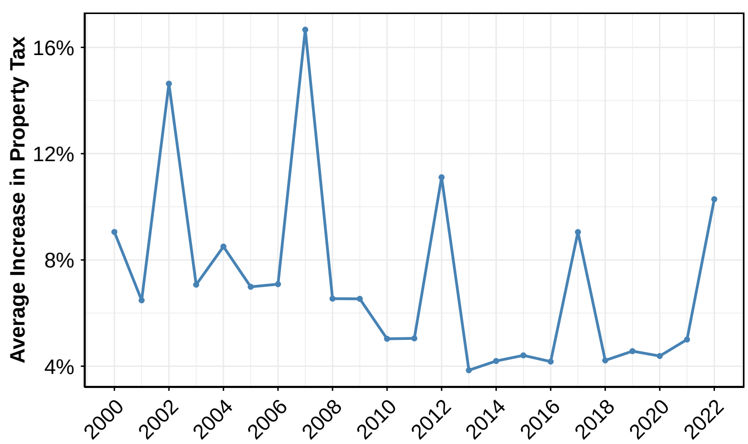
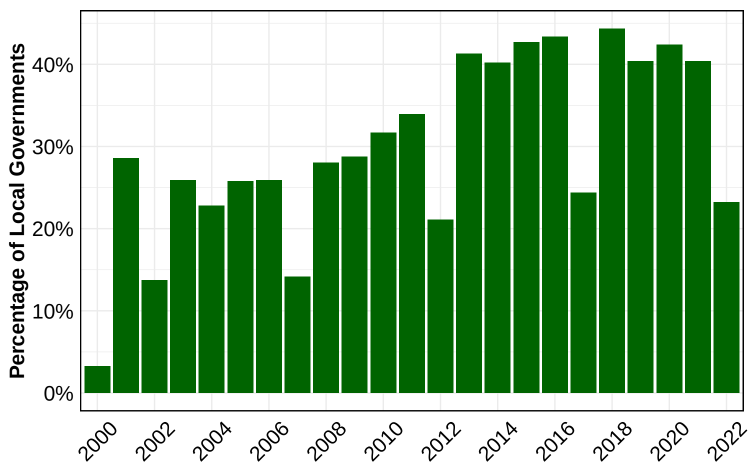


Figure A.4: **Stable Trends on Property Tax.**
This figure plots the macro trends on property tax per capita of local governments. Panel A plots the fraction of local governments that experienced a stable change in property tax revenue per capita relative to the previous year. Panel B plots the average change rate among these local governments.

Panel A. Percentage of local governments with stable change in property tax per capita



Panel B. Average rate of property tax per capita

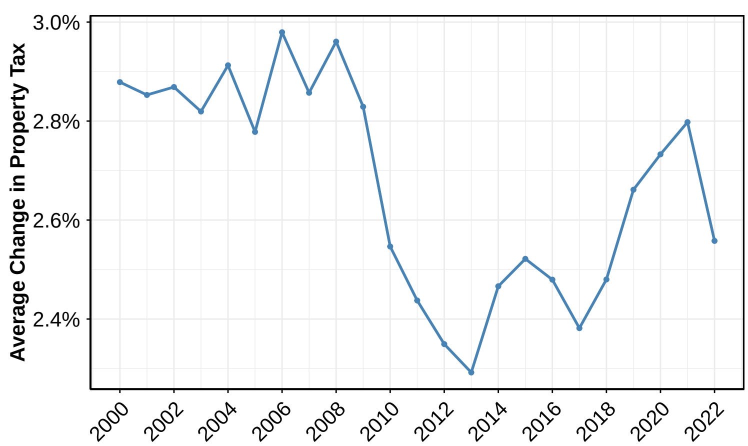


Figure A.5: **Term structure.**
This figure shows term structure of municipal bond yields across 0 to 30 years, estimated using Diebold and Li (2006)'s reformulation of Nelson and Siegel (1987)'s three-factor model, namely, level, slope, and curvature factors, as outlined in the Appendix Section C.

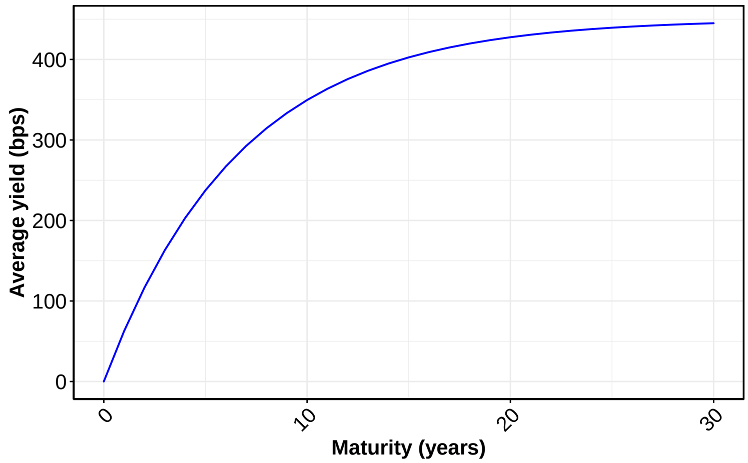
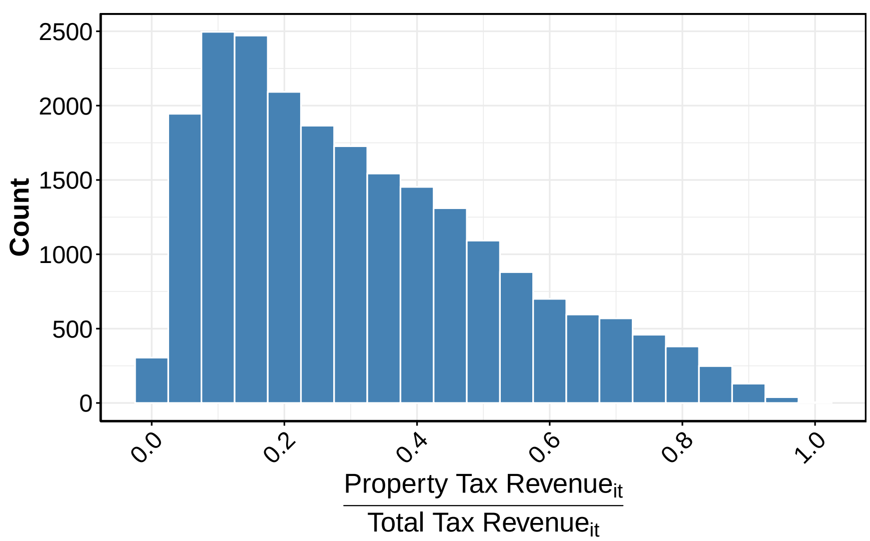


Figure A.6: **Property Tax Share**
This histogram shows the distribution of government-year share of property tax in total tax revenue.



Steps to Cleaning Transaction Data						
Cleaning Step	**Bonds**	**Trades**	**School Districts**	**Counties**	**Cities**	**States**
Full sample	2,762,569	204,520,945	25,008	2,976	20,051	50
GO and K-12 school districts	1,998,569	173,120,185	13,054	2,803	18,437	50
Remove data errors	825,855	138,958,324	12,417	2,749	17,892	50
Census match	173,183	15,513,357	8,163	2,611	15,247	50
Drop variable rate bonds	172,180	15,513,357	7,984	2,589	15,031	50
Drop insured bonds	131,908	12,791,217	6,472	2,514	14,186	50
Drop pre-refunded bonds	125,908	11,791,076	5,793	2,463	13,577	50
Drop taxable and AMT bonds	111,611	10,639,523	4,912	2,389	12,841	50
Drop Trades After Maturity	111,786	10,637,880	4,851	2,361	12,496	50
Drop callable bonds	109,235	9,831,150	4,207	2,312	11,938	50
Final sample	109,235	9,831,150	3,606	2,270	10,743	50

Table A.1: **Sample Construction.**
This table follows the presentation of Schwert (2017)'s Table 1. I first merge the GO and school district bond transaction sample with data on residential property tax revenue per capita for each county and school district using the municipality's name. I then merge this data with house prices for each county provided by the ZHVI and for school district constructed from the tract-level FHFA house price index. I merge this dataset with housing supply elasticity for each county and school district from Baum-Snow and Han (2024). I merge district-level data on education outcomes from the NCES CCD using the NCES 7-digit local education agency identifier. I drop elementary only, secondary only, specialized, administrative/non-operating, and service agency districts. I remove data errors following Schwert (2017), Curti et al. (2023), Gao and Murphy (2022), Fleckenstein et al. (2014), and drop bonds that have coupons greater than 20%, maturities over 100 years, and fewer than 10 trades. I further restrict the sample to general obligation bonds from cities and counties with annual revenues over $50 million and school district bonds. See Section 2 for details.

State	Trades (%)	y(1)	y(5)	y(20)	y(30)
Alabama	2.2	1.93	2.55	2.71	3.52
Arizona	2.3	2.19	2.77	3.85	5.13
Arkansas	1.1	2.07	2.57	2.68	3.71
California	16.3	1.46	2.09	2.72	3.43
Connecticut	1.6	1.95	2.62	3.76	5.62
Delaware	0.2	1.79	2.48	3.47	4.09
Florida	6.4	2.10	2.25	2.81	3.53
Georgia	2.7	1.51	2.03	2.63	3.56
Illinois	2.2	2.35	3.12	3.88	5.74
Kansas	1.4	1.64	1.94	2.61	3.88
Kentucky	3.1	2.06	2.61	2.67	3.53
Louisiana	1.4	1.22	2.07	2.66	3.58
Maine	0.7	1.84	1.89	2.54	3.49
Maryland	1.6	2.02	2.68	2.81	3.77
Massachusetts	2.4	1.68	1.92	2.53	3.33
Michigan	2.2	2.09	2.76	3.20	3.97
Minnesota	2.5	1.99	2.83	2.88	4.07
Mississippi	0.7	1.98	2.66	3.61	5.42
Missouri	2.6	2.23	2.85	3.28	3.99
Montana	0.4	2.12	2.77	3.86	5.81
Nevada	0.5	2.37	2.51	2.93	3.72
New Hampshire	0.4	1.86	2.61	3.44	5.74
New Jersey	3.2	2.14	2.71	2.71	3.51
New Mexico	0.8	2.03	2.17	2.59	3.62
New York	7.0	1.39	2.11	2.71	3.41
North Carolina	2.9	1.15	1.92	2.52	3.51
Ohio	3.4	1.91	2.00	2.63	3.61
Oklahoma	1.5	1.93	2.68	3.78	5.66
Oregon	1.2	1.96	2.59	2.89	3.71
Pennsylvania	4.2	2.06	2.62	2.72	3.67
Rhode Island	0.8	2.08	2.75	3.46	3.96
South Carolina	1.8	1.36	2.01	2.63	3.47
Tennessee	1.6	1.99	2.57	3.50	5.20
Texas	7.6	1.62	2.00	2.66	3.53
Utah	1.3	1.88	2.53	3.50	5.39
Vermont	0.4	1.87	2.65	3.55	4.50
Virginia	3.2	0.76	1.89	2.53	3.40
Washington	2.4	1.57	1.93	2.55	3.41
Wisconsin	1.8	1.77	1.97	2.78	4.18

Table A.2: **Average yields by maturity of h years.**
I show the average number of trades for all counties and cities in each state and year.

Table A.3: **Additional Descriptive Statistics.**
This table reports mean, standard deviation, 1st, 25th, 75th, and 99th percentiles of additional variables used in this study. The annual data span from 2000 to 2022.

	Mean	**SD**	**1st**	**25th**	**75th**	**99th**
A. House Prices and Property Tax						
House price growth: County (%)	4.18	6.61	-61.60	0.68	7.49	48.58
City (%)	4.40	7.54	-74.40	0.60	8.50	66.30
School district (%)	4.43	9.42	-63.74	-0.71	9.42	30.48
Growth in property tax per capita: County (%)	6.48	13.43	-27.92	0.74	10.37	40.59
City (%)	9.00	14.43	-23.40	0.60	22.50	28.40
School District (%)	3.80	11.03	-29.80	-0.20	8.00	29.00
Property tax per capita ($)	3,222	1555	914	2113	3950	8071
Property tax revenue: County (Mil. $)	46.31	187.98	0.33	3.51	28.28	683.44
City (Mil. $)	6.08	151.71	0.00	0.02	1.04	81.38
School District (Mil. $)	14.29	51.30	0.03	1.18	11.08	152.07
Property tax/ Total tax revenue	0.318	0.214	0.021	0.141	0.455	0.859
Federal Transfers (Mil., $)	124	1,458	0.028	3	32	1,025
Assessment value ($)	169,971	160,153	43,415	123,816	200,177	1,628,949
B: Top Income Tax Rates						
Federal income tax rate (%)	36.86	1.91	35	35	38.85	39.60
State income tax rate (%)	5.58	2.95	0	4.60	7	13.30

(1) Group	(2) Type	(3) Sub-Type
Biological	Epidemic	Infectious disease
Biological	Epidemic	Viral disease
Climatological	Drought	Drought
Climatological	Wildfire	Forest fire
Climatological	Wildfire	Land fire (Brush, Bush, Pasture)
Climatological	Wildfire	Wildfire
Geophysical	Earthquake	Ground movement
Geophysical	Volcanic activity	Lava flow
Hydrological	Flood	Flash flood
Hydrological	Flood	Flood
Hydrological	Flood	Riverine flood
Hydrological	Mass movement	Landslide
Hydrological	Mass movement	Mudslide
Industrial accident	Chemical spill	Chemical spill
Industrial accident	Explosion (Industrial)	Explosion (Industrial)
Industrial accident	Gas leak	Gas leak
Industrial accident	Oil spill	Oil spill
Meteorological	Extreme temperature	Cold wave
Meteorological	Extreme temperature	Heat wave
Meteorological	Extreme temperature	Severe winter conditions
Meteorological	Storm	Blizzard/Winter storm
Meteorological	Storm	Derecho
Meteorological	Storm	Extra-tropical storm
Meteorological	Storm	Hail
Meteorological	Storm	Lightning/Thunderstorms
Meteorological	Storm	Sand/Dust storm
Meteorological	Storm	Severe weather
Meteorological	Storm	Storm
Meteorological	Storm	Tornado
Meteorological	Storm	Tropical cyclone
Miscellaneous accident	Collapse	Collapse
Miscellaneous accident	Explosion	Explosion
Miscellaneous accident	Fire	Fire
Miscellaneous accident	Miscellaneous accident	Miscellaneous accident
Transport	Air	Air
Transport	Rail	Rail
Transport	Road	Road
Transport	Water	Water

Table A.4: **Disasters and Hazards at EMDAT**
This table lists groups of hazards and disasters that occurred in the United States from 2000 to 2022, as reported by EMDAT.