Property Tax Protection and Municipal Bond Valuation *

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July 15, 2026

Abstract

I study how downside protection for local property tax revenue affects local governments' ability to commit to bondholders, and in turn their borrowing costs and human capital investment. To identify these effects, I exploit statewide regulations that limit declines in the property tax revenue of school districts, but not of counties. Using a staggered difference-in-differences estimation, I find that these regulations increase school district bond issuance by 4% and decrease the school district bond yield spreads by 13 basis points compared to unprotected school districts and counties. Protection could encourage wasteful spending and generate moral hazard problems. However, I find that these regulations improve educational outcomes rather than encouraging wasteful spending, as evidenced by lower student dropout rates and higher teacher salaries in protected school districts. These benefits for school districts come at the expense of counties, showing an increase in county borrowing costs.

Keywords: Revenue Protection; Local Borrowing Costs; Municipal Bonds; Human Capital
JEL: H74, H71, I22, G12

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Property taxes are the largest source of tax revenue for local governments, but they are understudied in the municipal bond literature. From the perspective of bondholders financing municipal bonds, it is important that local governments can keep their tax revenue stable during local recessions to repay the debt. However, after issuing bonds, local governments might not have the incentives or ability to increase property taxes when needed, due to political and legal constraints. For example, local governments face political resistance from taxpayers (Berry, 2021), and some states legally restrict increases in property taxes (Avenancio-León and Howard, 2022a). These constraints can then limit the ability of local governments to raise funding and invest in public projects.

In this paper, I study how downside protection on local property tax revenue affects local governments' borrowing costs and human capital investment. Ex ante, it is unclear whether and how much protection affects borrowing costs of local governments. On the one hand, protection can decrease local borrowing costs if these governments credibly commit to their bondholders to repay the debt. On the other hand, protection can increase local borrowing costs if it generates moral hazard problems. Local governments can internalize protection and increase wasteful spending, which ultimately increases municipal bond yields (Cornaggia et al., 2024; Gao et al., 2019; Persson and Tabellini, 1996).[1] At the same time, protection can have limited effects if other determinants dominate local borrowing costs, such as recalibration in credit ratings (Adelino et al., 2017; Cornaggia et al., 2018), climate risk (Goldsmith-Pinkham et al., 2023; Painter, 2020), market fragmentation (Babina et al., 2021), and political uncertainties (Gao et al., 2019). The effect of protection on borrowing costs is ultimately an empirical question. I find that revenue protection decreases local borrowing costs using statewide regulations that limit declines in local property tax revenue.

Public school districts provide a useful setting to learn about whether and how much revenue protection affects borrowing costs for three particular reasons. First, statewide regulations limit declines in the property tax revenue of school districts, but not of counties. This institutional fea-

[1] In the municipal bond literature, Cornaggia et al. (2024) addresses a concern that bond insurance can encourage insured issuers to engage in ex post riskier behavior. Relatedly, Gao et al. (2019) addresses the concern that creditor protection may encourage municipalities to issue bonds carelessly.

ture provides a natural comparison between school districts and counties across states that provide protection and those that do not. Second, much of the funding for public schools comes from local property taxes, creating a direct link between these taxes and ability to repay debt. Third, school district is a useful setting to test whether revenue protection encourages wasteful spending or improves human capital investment. If this protection has subsequent effects on student participation, it has real effects rather than spending wastefully.

Student participation and instruction compensation are the real outcomes I use to test whether revenue protection improves human capital investment or leads to wasteful spending. Prior research on school finance reforms shows mixed results on educational outcomes. On the one hand, Lafortune et al. (2018) finds that state transfers to school districts lead to efficient spending, which in turn improves student achievement in low-income school districts in the long run. On the other hand, Zhao (2023) finds that these transfers lead to wasteful spending and that school districts do not invest in needed facilities. Cellini et al. (2010) finds that even though school districts invest in needed facilities, these do not improve student test scores. A missing piece in much of the existing literature is the borrowing channel through which school districts' own-source revenue protection affects human capital investment.[2] My paper studies that margin explicitly and shows a borrowing channel through which own-source revenue protection improves real educational outcomes.

My paper refers to statewide regulations that limit declines in property tax revenue of school districts as *floors* and the states that enacted floors as *floor states*. Between 2000 and 2021, fifteen states independently developed, enacted, and amended their own floors that regulate the downside protection on property tax revenue of school districts. The objective of the floor policy is to enhance public education by guaranteeing minimum funding for school districts and ensuring financial support for students and teachers. Statewide floor regulations limit decline in property tax revenue of school districts, but not of counties. So, I employ a staggered triple-difference design and compare school districts in floor states (i) before and after the floor enactment; (ii) to

[2]Closely related studies, such as Yang (2024) and Lee (2025), examine debt supported by state aid and subsidies instead of school district's own-source of revenue. See literature review for details.

school districts in non-floor states; and (iii) to counties. This approach provides a clean source of identification to isolate the effect of revenue protection on local borrowing costs.

Using this staggered triple-difference estimation, I find that school districts in floor states face lower borrowing costs post the enactment; compared to school districts in non-floor states; and to counties. Explaining these results, school districts in floor states experience an average decline of 13 basis points (bps) in bond yield spreads. This effect is economically large, accounting for a 9% decline compared to average 143 bps. The reduction in borrowing costs is even larger for school districts that need protection on the own-source revenue. I find that school districts with low income and those receiving low state transfers experience average declines of 33 bps and 27 bps in bond yield spreads, respectively. Moreover, this protection by floor supports supply-side expansion and increases the bond issuance of protected school districts by 4%. The fact that floor limits the decline in property taxes is particularly important in the municipal bond market, where credit risk is more pronounced during economic downturns. By limiting declines, the floor reduces downside credit risk and, in turn, lowers local borrowing costs.

I then find that the floor improves real educational outcomes rather than encouraging careless spending. When school districts gain secure protection over their revenue, their incentives to invest in the future, including in their students, increase. In the setting I study, I show that school districts in floor states show better student participation and instruction compensation than school districts in non-floor states post the enactment of the floor. Specifically, student dropout rates decline by 0.38 pp over the long run, which is an 8.6% decline relative to the mean of 4.4%. Student-teacher ratio also decreases by 4% relative to the mean, suggesting that the average class size becomes smaller for each teacher. Regarding the instruction outcomes, teacher salaries per pupil increase by 3% and employee benefits per pupil increase by 4%. These real effects are more pronounced in school districts with low income and those that receive less state transfers. Floor has larger effects on real educational outcomes in school districts that need protection. Lastly, school spending increases in categories like education, instruction, support services, and school lunch. I use these results to support that the floor leads to efficient spending rather than wasteful spending and generating moral hazard problems.

However, these benefits to school districts come at the expense of counties. I find a decline in county property tax revenue and an increase in county borrowing costs in floor states compared to counties in non-floor states. School districts and counties overlap geographically and draw property tax revenue from a shared local property tax base. When local property tax revenue declines, the floor prevents school district property tax revenue from falling. Because the floor does not protect counties, a larger share of the decline in the local property tax base is borne by counties, reducing their property tax revenue. I find that counties face higher borrowing costs following the enactment of the floor. Specifically, county bond yield spreads increase by an average of 5 bps and county bond issuance decreases by 2.5% in floor states relative to counties in non-floor states. The protection provided to school districts comes at the cost of deteriorating borrowing conditions for counties.

A causal interpretation relies on the assumption that local borrowing costs and educational outcomes change because of floors not other spuriously correlated factors. I provide four pieces of empirical evidence to support this ultimately untestable assumption.[3] First, I show that the parallel trend assumption holds. There are no meaningful differences in property taxes, municipal bonds, and educational outcomes between treated and control municipalities prior to treatment. Second, I show that enactment of floors is not predicted by state economic and demographic characteristics or by state taxes and expenditures. These statewide characteristics include house price volatility, residents' income, state transfers, the share of residential property in total property tax revenue, and the share of votes cast for the Democratic and Republican presidential candidates. Third, I do a falsification test on the adoption of the floor. I find no meaningful pre-existing differences in municipal bonds between future-treated issuers and never-treated issuers prior to the treatment. Fourth, I use revenue bonds as a placebo test following Butler and Yi (2022) and show limited

[3]Section 4.3 discusses these estimations in detail.

effects of floors on revenue bond issuance and yield spreads.

The remainder of the paper is as follows. Section 1 provides literature review. Section 2 provides institutional background. Section 3 describes the data sources and sample construction. Section 4 outlines empirical design and presents main results. Section 5 presents real effects on education outcomes. Section 6 presents heterogeneous effects and robustness checks. Finally, Section 7 concludes.

1 Literature Review

This paper provides novel insights for several strands of literature. First, my work relates to the literature on municipal bond valuation and advances the understanding of how property tax revenue protection affects municipal bonds. The literature has previously considered determinants of municipal bond yields including changes in credit ratings unrelated to the fundamentals (Adelino et al., 2017; Cornaggia et al., 2018), bankruptcy protection (Gao et al., 2019), climate and environmental risks (Acharya et al., 2022; Auh et al., 2022; Goldsmith-Pinkham et al., 2023; Huang and Kumar, 2021; Painter, 2020), changes in authorized immigration (Zimmerschied, 2024), unauthorized immigration (Cornaggia et al., 2024), and pandemic-induced migration (Gustafson et al., 2023), political uncertainty from gubernatorial elections (Gao et al., 2019), data breaches (Curti et al., 2023), market fragmentation induced from income-tax exemption (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., 2024), investor attention (Cornaggia et al., 2022), liquidity risk (Ang et al., 2014; Kidwell and Trzcinka, 1982), and ESG labeling (Garrett et al., 2025). Although property taxes are the largest source of revenue for local governments, they are understudied in the municipal bond literature. I add to this literature by primarily focusing on property tax revenue and showing that its downside protection is a first-order determinant that lowers municipal bond yield spreads.

Second, my study contributes to the literature that examines municipal bond markets for public school education. Several studies examine borrowing costs for public education and identify non-fiscal determinants of these borrowing costs. For example, Cornaggia et al. (2026) uses partisan school bond elections in North Carolina and shows that political partisanship lowers bond yields. Dougal et al. (2019) finds that historically Black colleges and universities pay higher underwriting spreads of bonds, showing that there is significant racial bias. I add to this literature by identifying a fiscal determinant of borrowing costs of public school education, namely the protection of school districts' own-source property tax revenue. Moreover, I examine whether these lower borrowing costs translate into improvements in educational outcomes, including student participation and the teaching environment.

Third, my study relates to the literature on financial resources of public school education. Cellini et al. (2010) uses school district bond referenda for identification and finds that good public education increases local residents' willingness to pay premium, which is capitalized into higher local house prices. Adding on this study, Yi (2024) shows that the increase in local house prices is weaker in old districts that benefit less from future public school education. My study differs from these papers because I focus on local borrowing costs−measured by municipal bond yields− not the capitalization in local house prices. Through rules on protecting the local revenue, local governments' commitments are credible even when local economic conditions deteriorate, which ultimately lowers local borrowing costs.

In the public policy literature, two studies closely related to my paper, Yang (2024) and Lee (2025), examine how school finance reforms affect educational outcomes. Yang (2024) focuses on credit enhancement program, which ensures a last-resort fund for a state to use if a school district goes default. She shows that this program lowers school district bond yields and has limited effects on student test scores. Moreover, Lee (2025) finds that state transfers for instructional facilities increase student test scores and graduation rates in Texas. The difference with my paper is that my central question is not how school finance reforms and subsequent state transfers affect educational outcomes. Instead, I study how the protection of a major local own-source revenue affects commitment to bondholders and changes in local borrowing costs. My paper differs from these studies in two ways. First, I focus on own-source revenue of school districts, which is local property tax, unlike the school finance reforms that changed state transfers. Second, I study both school districts and counties, in which protection applies only to school districts, not to counties. This setting enables me to examine how the floor policy redistributes fiscal resources between school districts and counties, benefiting schools while imposing costs on counties. Benefits to school districts come at the expense of counties. I show that costs of protection are borne by counties, and county bond issuance decreases and county bond yield spreads increase in floor states than non-floor states.

Finally, my paper relates to the literature that examines frictions in the collection of local property taxes. Researchers show that property tax collections are not equal across properties due to household wealth (Amornsiripanitch, 2020), racial inequalities (Avenancio-León and Howard, 2022b), political partisanship (Kalda et al., 2024), appeal behaviors (Weber and McMillen, 2010), and assessor's home bias (Cohen and Chen, 2024). Berry (2021) and McMillen and Singh (2023) find that property taxes are regressive, which means that properties in wealthier (poorer) neighborhoods are under (over)-assessed. Some studies show that fairness improves after a reassessment of properties (Hou et al., 2023; Kim and Hou, 2024). The difference between my study and theirs is that I focus on municipal bonds and examine how lower local borrowing costs improve human capital investment, rather than studying the reasons for the gap between assessed values and house prices.

2 Institutional Background

School districts and counties levy property taxes by applying mill rates to assessed values of properties. Mill rates are property tax rates applied per $1,000 of assessed values, expressed in millage points. Although counties and school districts overlap geographically, they are distinct taxing authorities and make spending decisions independently. Counties and school districts are separate local governments with different responsibilities. Counties fund broad local services, including law enforcement, roads and transportation, parks and libraries, and disaster response. School districts use tax revenue to fund K-12 public education including instruction salaries and benefits, instructional materials and technology and school buildings and maintenance.

Floors limit the decline in the total property tax revenue of school districts compared to the previous year. Specifically, floor states impose a lower-bound percentage limit on changes in total property tax revenue from the previous year. If assessed values drop, then school districts increase mill rates to meet the minimum threshold. For example, Georgia's Quality Basic Education Act ensures that every public school district in Georgia must not decrease local property tax revenue more than 0.5% compared to the previous year. The minimum rate increased to 1.4% in 2012 and lowered to 1% in 2024. Nevada Revised Statute 361.4722 limits declines in both assessed values and mill rates. School districts' total assessed values may decline by no more than 2%, and mill rates by no more than 7.5 mills (i.e., 0.75%). Lastly, the Support Education Excellence in Kentucky (SEEK) requires public school districts to maintain a minimum local effort of 3 mills (i.e., 0.3%) to participate in state funding.

As shown in Figure 1, fifteen states enacted the floor between 2000 and 2021. Four states enacted the floor in 2000. Three states enacted it from 2001 and 2005. Six more states between 2006 and 2019, and two more states after 2016. The enactment process varied across states, mainly by voter referendum, legislative actions, or state court mandate. For example, Florida adopted the floor with a voter referendum on the passage of statute. Around 54% of voters voted in favor of the measure, while 46% voted against it. Second, other states enacted floors through legislative actions. In Minnesota, for instance, the floor policy was developed through committee deliberations in the state legislature, passed by both the House and Senate, and ultimately signed into law by the governor as part of a broader school-finance reform policy. As another example, in Texas, the Supreme Court declared the existing school-finance system unconstitutional and directed the Legislature to adopt a more equitable funding framework.

Despite differences in their enactment process, these floor policies share a common objective:

ensuring a minimum level of own-source revenue for public school education. The floor achieves this objective by limiting declines in school districts' primary own-source revenue, property taxes. The fact that it limits the decrease is particularly important in the municipal bond context in that municipal bond risks are more pronounced in downturns. The floor protects against revenue declines, which reduces the downside risk, and in turn lowers municipal bond yield spreads.

School Finance Reform was arguably the biggest reform in the U.S. that changed the revenue and spending of K-12 school districts (Hoxby, 2001). State legislatures and courts sought to reduce disparities in educational funding through changes to school finance systems. There are two broad phases of the reform (Bayer et al., 2020; Lafortune et al., 2018). The first phase is "equity", in which the reform aimed at weakening the relationship between the quality of educational services and the district's local fiscal capacity. The second phase is "adequacy", in which constitutional provisions require states to provide an adequate level of public education. Per-pupil spending became a central issue during this period. Low levels of spending deprived students of the resources necessary to receive an adequate education and thus violated state constitutions.

However, the floor differs from other types of school finance reforms in an important way. The floor protects own-source revenue of school districts, particularly local property taxes, whereas most other reforms rely on transfers from states to local districts. To illustrate this difference, I follow the framework outlined in Jackson et al. (2014) and Jackson et al. (2015) and compare the floor with five other types of school-finance reforms, namely flat grants, equalization plans, categorical aid, foundation plans, and credit enhancement. To begin with, state provides flat grants to districts on a per-pupil basis regardless of district wealth and spending levels. Equalization plans, meanwhile, provide transfers to low-wealth districts, typically measured by local property wealth. Moreover, categorical aid gives funding to school districts if their residents are liquidity constrained or their children are unusually expensive to educate (Hoxby, 2000). Foundation plans, on the other hand, establish a foundation level of per-pupil spending. States provide transfers to districts where expected local contribution falls short of the foundation level of spending. Lastly, states provide credit enhancement in which they ensure last-resort funds for state to use and repay debt to bondholders if a school district fails to repay (Yang, 2024). In contrast, the key feature of the floor is that it operates through the protection of districts' own-source revenue rather than through state transfers, fund, or aid.

3 Data

This study relies on several data sources. First, I use municipal bond data from Municipal Securities Rulemaking Board (MSRB) in Wharton Research Data Services (WRDS) and Bloomberg. Second, I use property tax revenue data provided by the Census Bureau's Annual Survey of State and Local Government Finances data (Pierson et al., 2015). Third, I use county-level house price data from the Zillow Home Value Index (ZHVI) and district-level data aggregated from the Federal Housing Finance Agency (FHFA). Fourth, I collect property tax regulations from state statutes, administrative codes, and regulatory amendments. Lastly, I use educational outcomes data from the National Center for Education Statistics' (NCES).

3.1 Data Sources and Sample Construction

Municipal Bond Data. MSRB in WRDS reports CUSIP, trade date, yield, price, maturity, and coupon rate for municipal bond transactions. I merge this transaction-level data with the municipal bond characteristics obtained from Bloomberg using CUSIP and trade date. Bloomberg provides information on whether a bond is a general obligation (GO) or revenue bond, whether it is callable, insured, tax-exempt, and whether it has fixed or variable coupon. Bloomberg also provides 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 use 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 entities and often issue bonds jointly with other educational entities. Thus, a typical issuer name for a school district is "Monroe County Community School Corporation" in Indiana, or "Dallas Independent School District" in Dallas in Texas.

To focus on the direct role of property tax revenue, I use general obligation (GO) and school district bonds to test my hypothesis. GO and school district bonds are directly backed by property taxes, whereas revenue bonds are secured by cash flows from specific projects, such as utilities, sewage systems, or highway tolls. Consistent with this distinction, I show that the effects of floor 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 the bonds that are less tied to property taxes show smaller changes in yield spreads.

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. Babina et al. (2021) show that such exemptions result in market fragmentation and limit risk-sharing across states. Fleckenstein and Longstaff (2023) find that local investors are willing to pay premium for this exemption. Garrett et al. (2023) show that the reduction of this exemption raises bidder markups and depresses competition.

Accounting for this exemption, I calculate the municipal bond yield spreads as:

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

for a municipal bond b, issuer i, and year t. r_t^f is the maturity-matched Treasury rate, provided by Liu and Wu (2021). τ_t^{fed} is the top federal income tax rate, and $\tau_{s,t}^{\text{state}}$ is the top income tax rate in state s in year t.[4] Equation (1) is identical to Schwert (2017)'s measure and is used in influential studies in the literature (e.g., Cornaggia et al., 2024; Curti et al., 2023; Fleckenstein and Longstaff,

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

2023; Gustafson et al., 2023). However, I show that the findings are robust with alternative measures of yield spreads, including nominal yield spreads, Garrett et al. (2023)'s tax adjustment, 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 on floors. I manually collect this data from primary legal documents, including state statutes, administrative codes, and regulatory amendments. 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 property tax revenue data Census Bureau's Annual Survey of State and Local Government Finances data. Pierson et al. (2015) offers a single and comprehensive database of local government revenue and expenditure. I use county, city, and school district-level data, provided from 1967 to 2022.[5] This data includes detailed information on both tax and non-tax revenue, intergovernmental transfers, and expenditure.

Educational Outcomes. I obtain district-level data from the National Center for Education Statistics (NCES)' Common Core of Data (CCD) for America's Public Schools. The CCD consists of two components: the fiscal CCD and nonfiscal CCD. The fiscal CCD reports information 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 student enrollment, measured as fall membership (i.e., the number of students enrolled on a specific fall count date, typically October 1). Furthermore, the nonfiscal CCD shows the location of the school district, number of schools, teachers and support staff, and student dropout rates. I merge the two datasets using the NCES 7-digit local education

[5]Pierson et al. (2015) provides data on state, county, municipal government, township, special district, and school district levels.

agency identifier (i.e., LEAID) and school year.

Sample Construction. This paper combines municipal bond transactions, floor regulations, property taxes, housing markets, and educational outcomes. I first merge the GO and school district bond transaction sample with property tax revenue 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. Finally, I merge this data with district-level educational 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 tax revenue and spending, house prices, district-level educational outcomes, and state-level floor regulations.

Table A.1 presents the number of bonds and trades in each step of the sample construction.[6] I follow influential studies in the municipal bond literature, such as Curti et al. (2023), Fleckenstein and Longstaff (2023), Gao and 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. Next, I 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. The dataset includes 41,234 GO bonds and 3,133,784 GO trades and 68,001 school district bonds and 6,697,366 school district bond trades. The final sample includes 109,235 bonds with 9,831,150 trades issued by issued by 2,270 counties and 3,606 school districts

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

in fifty states from 2000 to 2022.

3.2 Descriptive Statistics

Table 1 presents descriptive statistics for municipal bonds, property tax and house prices, and educational outcomes. The main dependent variable, municipal bond yield spread $Spread_{bit}$ is average 143 bps and stays positive across all time.[7] There are two implications from Figure 2. First, municipal bond yields are always higher than risk-free rate. So, municipal bond is risky even though it is well known for it safety. Second, there is cross-sectional dispersion municipal bond yield spreads every year, as shown by the light blue shade. So, local governments face different amounts of risk. The Appendix Table A.2 shows average yields of local governments in each state with different maturities. 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. 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.

[7] This value corresponds well with prior studies (Fleckenstein and Longstaff, 2023; Schwert, 2017).

4 Empirical Strategy and Results

4.1 Effects of Floor on Municipal Bonds

Statewide regulations impose floors that limit declines in property tax revenue of school districts, but not of counties. This institutional feature provides a natural comparison between school districts and counties across floor and non-floor states. To investigate how the statewide floor regulations affect municipal bond yield spreads, I employ a staggered triple-difference estimation as follows:

$$y_{bit} = \beta\,(\text{Floor}_{st} \times \text{School}_i) + \pi_1 \text{Floor}_{st} + \pi_2 C_{it} + \pi_3 Z_{bit} + \mu_i + \delta_t + \varepsilon_{st}, \tag{2}$$

for municipal bond b, issuer i, state s, and year t. There are two types of issuers, school districts and counties. The outcome variables are Spread_{bit} and $\log(\text{Issuance}_{bit})$. Spread_{bit} is the yield spread of municipal bond b issued by an issuer i, measured as yield minus maturity-matched risk-free rate from Liu and Wu (2021) and adjusted with income-tax exemption as in Schwert (2017), as presented in equation (1). $\log(\text{Issuance}_{bit})$ is the logarithm of issuance amount, measured in dollars, of bond b issued by school district i in year t. The sample underlying equation (2) includes bonds issued by school districts and counties across floor and non-floor states.

The treatment Floor_{st} equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. Floor applies only to school districts, not to counties. So, I add an additional treatment School_i, which equals one if an issuer i is a school district, and zero if it is a county. Therefore, this identification compares bonds issued by school districts in floor states (i) before and after the enactment of the floor; (ii) to bonds issued by school districts in non-floor states; and (iii) to bonds issued by counties. μ_i and δ_t denote issuer and year fixed effects, respectively.

The specification includes an extensive set of control variables related to issuers (C_{it}) and municipal bonds (Z_{bit}). I control for issuer-level characteristics (C_{it}), including changes in local income, employment, population, and GDP. I also control for changes in sales and income tax revenue of local governments. I control for municipal bond characteristics (Z_{bit}) following prior studies (e.g., Acharya et al. (2022); Goldsmith-Pinkham et al. (2023)), which includes the remaining time to maturity, credit rating, and bond-month standard deviation of transaction prices. I also control for liquidity risk measured with Amihud (2002)'s price of impact. The Appendix Section A.1 presents definitions and measures of control variables. Standard errors are clustered at the state-level to account for within-state correlation as the floor enactment occurs at the state level.

Panel A in Table 2 shows the effect of floor on school district bonds compared to county bonds estimated using a staggered triple-difference approach in equation (2). School districts in floor states increase bond issuance by 3.8% in floor states compared to school districts in non-floor states and counties. Moreover, school district bond yield spreads are 13 bps lower in floor states than school districts in non-floor states and counties. This effect is a 9% decrease in the spreads relative to the total average of 143 bps. This effect is economically significant and comparable to other studies. For example, Cornaggia et al. (2024) shows 15 bps change in hospital bond spreads due to remote product delivery. Cornaggia et al. (2018) shows 15 bps change in municipal bond yield spreads due to the Moody's ratings recalibration. Cornaggia et al. (2022) shows that the opioid crisis increased the spreads by 17 bps. Lastly, Painter (2020) shows that a 1% increase in flood risk increases yields by 7 bps for short-term maturity municipal bonds.

Panel A in Figure 3 plots event-study estimates of the average treatment effect on treated issuers at each horizon, spanning five years before and five years after treatment. The staggered triple-difference approach in equation (2) supports parallel trend assumption. There is no meaningful difference in yield spreads of bonds issued by school districts and counties across floor and non-floor states prior to the treatment. Post the treatment, the decline in school district bond yield spreads is persistent over years. Following the floor adoption, school district bond yield spreads decline by 6 bps in the first year, followed by an additional 5 bps decrease in the next five years.

Moreover, figure 4 plots the corresponding event-study estimates for bond issuance using the staggered triple-difference approach. As with yield spreads, there is no meaningful difference in issuance of bonds issued by school districts and counties across floor and non-floor states prior to the treatment, which supports the parallel trend assumption. Post the treatment, school district bond issuance increases relative to county bond issuance.

Appendix Figure A.1 illustrates how the floor affects borrowing costs across different local governments using Dallas as an example. Panel A maps Dallas County, the fourteen school districts located within the county, and the city of Dallas, which is also located within the county. These municipalities overlap geographically and draw revenue from the same local tax base. But, only school districts are protected by the floor. Therefore, I observe and show in Panel B that following the enactment of the floor, district bond yield spreads decline by between 1 and 15 bps. However, yield spreads increase by 7 bps for Dallas County and 4 bps for the city of Dallas following the enactment of the floor.

The average treatment effect—a 13 bp decline in school district bond spreads in floor states—masks substantial heterogeneity. Because the floor protects the own-source revenue of local governments, low-income school districts should benefit the most, as they face greater downside risk in property tax revenue. If the floor is binding, its effect on bond spreads should be larger for low-income school districts. Panel A of Figure 5 shows this indeed. School districts in the lowest-income group experience a 33 bp decline in bond spreads. This effect is substantially larger than the 18 bp decline for middle-income school districts. In contrast, high-income school districts experience a small and statistically insignificant change in bond spreads. Overall, these findings suggest that the floor primarily benefits low-income school districts.

State transfers are another major source of revenue for public school districts, accounting for 20–30% of school district revenue on average. As described in Section 2, state transfers have long been a key component of school finance reforms designed to support low-income school districts. These reforms take various forms, including categorical aid and foundation plans, but share the common objective of providing additional resources to low-income school districts. The floor differs from these reforms because it protects school districts' own-source revenue rather than providing additional state transfers. The floor decreases school district bond spreads by 27 bps in districts with low state transfers and by 14 bps in districts with mid-level transfers, but by a smaller and statistically insignificant 9 bps in districts with high transfers, as shown in Panel B of Figure 5. This pattern indicates that state transfers and the floor act as substitutes: when a district receives less state support, the additional downside protection from the floor is more valuable. Taken together, the income and state-transfer results show that the floor is most valuable for the most fiscally vulnerable school districts—those with low income and limited state support.

So far, I compared school districts versus counties across floor and non-floor states. A simpler comparison would be to examine school districts in floor and non-floor states. To investigate the effect of floor on school district bonds, I include school district bonds only in the sample and estimate the effect using a staggered difference-in-differences approach as follows:

$$y_{bit} = \beta \left(\text{Floor}_{st} \right) + \pi_1 C_{it} + \pi_2 Z_{bit} + \mu_i + \delta_t + \varepsilon_{st}, \tag{3}$$

for municipal bond b, school district i, state s, and year t. In equation (3), I include bonds issued by school districts only.[8] In equation (3), outcome variables are Spread_{bit} and $\log(\text{Issuance}_{bit})$. Spread_{bit} is the yield spread of municipal bond b issued by school district i, measured as yield minus maturity-matched risk-free rate from Liu and Wu (2021) and adjusted with income-tax exemption as in Schwert (2017), as presented in equation (1). $\log(\text{Issuance}_{bit})$ is the logarithm of issuance amount, measured in dollars, of bond b issued by school district i in year t. The treatment Floor_{st} equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. So, this analysis compares yield spreads and issuance amount of bonds issued by school districts before and after the enactment of the floor, as well as neighboring states that never enacted the floor over the time period. μ_i and δ_t denote issuer and year fixed effects, respectively. Standard errors are clustered at the state-level to account for within-state correlation

[8]In Section 4.3, I will include bonds issued by counties only.

as the floor enactment occurs at the state level. As described earlier, I include control variables related to issuers (C_{it}) and municipal bonds (Z_{bit}). The Appendix Section A.1 presents definitions and measures of control variables.

Panel B in Table 2 reports results on the effect of floor on school district bonds estimated using equation (3) bonds issued by school districts only. I use school district bond yield spreads as the outcome variable and find that β is -0.07 (t-stat: -2.22). This result means that the school district bonds carry yield spreads that are 7 bps lower in floor-states than non-floor states. Moreover, I use school district bond issuance as the outcome variable and find that school districts increase bond issuance by 3% in floor states compared to non-floor states. Panel B of Figure 3 plots event-study estimates of the average treatment effect on treated issuers at each horizon, spanning five years before and five years after treatment. This figure shows that the parallel trend assumption holds. I find that there are no meaningful differences in yield spreads between treated and control bonds prior to the enactment of the floor, which supports the causal interpretation of the main estimates.

4.2 Effects of Floor on Property Taxes

Ex ante, local governments face downside risk in property tax declining, as shown in Figure A.3. 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 6 compares districts' property taxes in floor and non-flor states. As shown in Panel A, at the onset of the Global Financial Crisis, school districts in non-floor states showed a decline in property tax revenue 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.

To investigate the causal effects of floor on property taxes, I estimate a staggered triple-difference approach as follows:

$$y_{it} = \beta \left(\text{Floor}_{st} \times \text{School}_i \right) + \pi_1 \text{Floor}_{st} + \pi_2 C_{it} + \mu_i + \delta_t + \varepsilon_{st}, \tag{4}$$

for issuer i, state s, and year t. There are two types of issuers, school districts and counties. Two outcome variables are levels and volatility of property tax revenue. First, I use the logarithm of property tax revenue, i.e., $\log(\text{Property Tax}_{it})$. Second, I use the logarithm of volatility of property tax revenue, i.e., $\log(\sigma_{it}^{\text{Property Tax}})$. I measure this volatility, $\sigma_{it}^{\text{Property Tax}}$, as the three-year rolling standard deviation of changes in property tax revenue.

The treatment Floor_{st} equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. Floor applies only to school districts, not to counties. So, I add an additional treatment School_i, which equals one if an issuer i is a school district, and zero if it is a county. Therefore, this identification compares property taxes of school districts in floor states (i) before and after the enactment of the floor; (ii) to property taxes of school districts in non-floor states; and (iii) to property taxes of counties. I include control variables related to issuers (C_{it}) as described in the Appendix Section A.1. Standard errors are clustered at the state-level to account for within-state correlation as the floor enactment occurs at the state level.

Panel A in Table 3 shows the effect of floor on property tax revenue of school districts compared to counties estimated using equation (4). As shown in column (1), school districts in floor states do not show a decline in the level of property tax revenue compared to school districts in non-floor states and counties. In fact, it increases by a small amount, showing an increase in property tax revenue by 2% under 10% significance level. Panel A in Figure 7 shows that post treatment, property tax revenue cannot decline in school districts in floor states, supporting that the treatment was a binding law. Like other outcome variables, the parallel trend assumption holds. Moreover, the volatility of property tax revenue declines by 9% in school districts in floor states compared to school districts in non-floor states and counties. As shown in Panel B of Figure 7, property tax volatility shows an immediate effect of 5% decline in the first year, and keeps declining until five years.

4.3 Endogeneity Concerns

A causal interpretation of the results relies on the identification assumption that municipal bonds and property taxes change because of the floor rather than spuriously correlated factors. To support this ultimately untestable assumption, I have already shown that the parallel trend assumption holds for municipal bonds and property taxes. In this subsection, I provide four pieces of additional empirical evidence.

First, I verify that floor regulations are not predictable by state characteristics. I consider a variety of state characteristics in the prior year, including economic and demographic variables and tax and expenditures. My regression specification is:

$$\text{Floor}_{st} = \alpha + \beta X_{s,t-1} + \delta_t + \epsilon_{st}, \tag{5}$$

for state s and year t. Floor_{st} equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. $X_{s,t-1}$ is a state characteristic in the prior year. δ_t is year fixed effect. The coefficient of interest, β, measures whether and how much lagged state characteristics predict the enactment of floor regulations.

Figure 8 plots the estimated coefficients β from bivariate regressions of a floor on a range of observable state characteristics using equation (5). Panel A presents state economic and demographic characteristics, including house price volatility and resident income, and political preferences, proxied by the statewide vote shares of Democratic and Republican presidential candidates. In all specifications, the 95th confidence intervals span zero. The coefficients are statistically insignificant and economically negligible. House prices, income, election results, and other state variables have limited predictability on the statewide enactment of floors. Moreover, Panel B presents state tax, expenditure, and transfers, including the share of residential property taxes in total property tax revenue, mill rates, assessed values of properties, and state transfers to local governments. States that will adopt the floor in the future did not show higher or lower expenditure and state transfers compared to states that never adopted the floor. These states also do not meaningfully differ in mill rates, assessed values, and other sources of tax revenue, including sales and income tax revenue. Altogether, these tests show that there is no systematic relationship between observable statewide characteristics and enactment timing, which supports the validity of the identification.

Second, I confirm the validity of the control states. As my estimation uses never-treated states as control group, I show that never-treated states are valid counterfactuals to not-yet treated states who will eventually be treated. To do so, I assign a falsified treatment to not-yet treated states and compare them with never-treated states. For illustration, consider two states A and B in years t and $t-1$. State A adopted the floor in t and thus does not yet have a floor in $t-1$. State B never adopts the floor. I assign a falsified treatment to state A in year $t-1$, despite the fact that state A is not yet treated until year t. By doing so, I can compare states A and B in year $t-1$ and see if there are meaningful pre-existing differences that drive changes in municipal bonds. Formally, my regression specification is as follows:

$$y_{bit} = \beta\left(\text{False Floor}_{s,t-h}\right) + \pi_1 C_{it} + \pi_2 Z_{bit} + \mu_i + \delta_t + \varepsilon_{st}, \tag{6}$$

for municipal bond b, issuer i, state s, and year t. I include bonds issued by school districts only in equation (6). Outcome variables are Spread_{bit} and $\log(\text{Issuance}_{bit})$. I assign a falsified treatment, in which $\text{False Floor}_{s,t-h}$ equals one if a state s has not yet adopted the floor in $t-h$ but will do so in t for $h = 0, 1, \ldots, T$ (i.e., not-yet-treated states). $\text{False Floor}_{s,t-h}$ equals zero if a state s never adopts the floor (i.e., never-treated states). Thus, this analysis compares school district bonds in not-yet-treated states before the adoption of the floor with school district bonds in never-treated states. To be clear, the sample underlying equation (6) includes all years prior to the statewide adoption of the floor, unlike the two-year example illustrated above.

The coefficient of interest, β, measures whether there are pre-existing differences in school districts bonds between not-yet-treated and never-treated states prior to the treatment. If pre-existing differences are driving the decline in municipal bond yield spreads, the coefficient β will be negative and statistically significant in equation (6). However, the results presented in Panel A in Table 6 show that β is small and insignificant, even slightly positive, for bond spreads. Moreover, if pre-existing differences in not-yet-treated and never-treated states are driving the increase in bond issuance, then β will be positive and significant on bond issuance. However, β is small and insignificant, even slightly negative, for school district bond issuance. Altogether, there is no meaningful difference in pre-existing characteristics that are driving the changes in municipal bond yield spreads and issuance, which suggests that the measure is accurately identified.

Third, now that I have confirmed the validity of control states, I further confirm the validity of control issuers. There are two types of issuers, school districts and counties. The previous test in equation (6) uses school districts only as issuers and validation was at state-level. Because the floor applies only to school districts and not to counties, an issuer-level validation is necessary. The objective of this validation is to see whether there are pre-existing differences between school districts and counties across floor and non-floor states that drive changes in municipal bonds. To do so, I do a falsification test in triple-difference approach, in which I interact the False Floor$_{s,t-h}$ with the school indicator as follows:

$$y_{bit} = \beta\left(\text{False Floor}_{s,t-h} \times \text{School}_i\right) + \pi_1 \text{False Floor}_{s,t-h} + \pi_2 C_{it} + \pi_3 Z_{bit} + \mu_i + \delta_t + \varepsilon_{st}, \quad (7)$$

for municipal bond b, issuer i, state s, and year t. There are two types of issuers, school districts and counties in equation (7). The outcome variables are Spread$_{bit}$ and $\log(\text{Issuance}_{bit})$. I assign a falsified treatment, in which False Floor$_{s,t-h}$ equals one if a state s has not yet adopted the floor in $t-h$ but will do so in t for $h = 0, 1, \ldots, T$ (i.e., not-yet-treated state). I assign False Floor$_{s,t-h}$ as zero if a state s never adopts the floor (i.e., never-treated states). The additional treatment, School$_i$ equals one if an issuer i is a school district and zero if it is a county. So, the treated group in equation (7) is school districts in not-yet-treated states (i.e., False Floor$_{s,t-h} = 1$, and School$_i = 1$). There are two control groups: (i) school districts in never-treated states (i.e., School$_i = 1$, and False Floor$_{s,t-h} = 0$), and (ii) counties (i.e., School$_i = 0$, and False Floor$_{s,t-h} \in \{0, 1\}$). This analysis compares school district bonds in not-yet-treated states before the adoption of the floor with school district bonds in never-treated states and counties.

If there are pre-existing differences between school districts and counties prior to the floor enactment that drive a decline in municipal bond yield spreads, then β should be negative and statistically significant in equation (7). However, Panel B in Table 6 shows that β is small, insignificant, and slightly positive for municipal bond yield spreads. Moreover, if school districts had been issuing more bonds than counties prior to treatment, then β should be positive and statistically significant for bond issuance. However, Panel B shows that β is small, negative, and insignificant for municipal bond issuance. Altogether, there are no meaningful pre-existing differences between school districts and counties that drive the change in bond yield spreads and issuance, which reinforces the causal interpretation of the results.

Finally, motivated by Butler and Yi (2022) and Butler et al. (2025), I use revenue bonds instead of GO and school district bonds as a placebo test. Revenue bonds are not directly tied to property taxes, whereas GO and school district bonds are directly 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. Table 7 indeed shows this. The estimate β is statistically insignificant and economically negligible for revenue bond yield spreads and issuance.

5 Real Effects and Costs of Floor

I have shown that the floor has causal effects on decreasing school district bond yield spreads. I find that lower borrowing costs are through revenue-protection channel, in which the floor is a local government's commitment to bondholders that it will repay the debt. In this section, I show how the floor has real effects on educational outcomes and benefits public school education. Moreover, I show the tradeoff of floor in that the benefits for school districts come at the expense of counties.

5.1 Real Effects of Floor on Human Capital Investment

In this section, I study the real effects of floors on subsequent human capital investment. To what extent might floors on school district property tax revenue affect real educational outcomes? The literature on school finance has not reached a consensus on whether financial resources improve educational outcomes. Some studies have shown that financial resources do not improve student test scores in California (Cellini et al., 2010), whereas others have shown that they do in a randomized experiment in Tennessee (Hanushek, 1997) and also in low-income districts nationwide in the long run (Lafortune et al., 2018).

In my study, I argue that, in the context of revenue protection, student participation—measured by student dropout rates and the student-teacher ratio—is a better proxy for educational outcomes than student achievement, which is typically measured by test scores. Standardized tests that are uniform across states and over time may not measure student participation in the context of the benefits of the floor. Revenue protection, provided by the floor, can improve student safety, arts and athletic programs, and other non-academic dimensions of the school environment that are not necessarily reflected in standardized test scores. So, the outcome variables that I use, such as student dropout rates and student-teacher ratio, measure student participation and engagement in the context of studying how the floor affects real educational outcomes.

More broadly, I use these student dropout rates and student-teacher ratio to assess whether school districts efficiently allocate financial resources. If school districts are already in the most efficient allocation of revenue and spending, then additional revenue may lead to wasteful spending and does not meaningfully improve student engagement. However, if the allocation of resources is not yet efficient, more resources can improve the teaching environment and student participation. Under this view, floors can improve student engagement and teacher compensation by making revenue streams more stable.

I estimate difference-in-differences effect of floor on district-level educational outcomes as follows:

$$\text{Educational Outcomes}_{it} = \beta\,\text{Floor}_{st} + \pi C_{it} + \mu_i + \delta_t + \epsilon_{st} \tag{8}$$

for school district i, state s, and year t. I examine three sets of district-level outcome variables. First, I use student participation, measured by the student dropout rate and the student-teacher ratio. Second, I use total compensation and employee benefits per pupil (pp) in a school district i in year t. Third, I use expenditure pp, focusing on expenditure on education, student services, and school lunch programs. μ_i and δ_t are school district and year fixed effects, respectively. Standard errors are clustered at the state-level to account for within-state correlation as the floor enactment occurs at the state level.

Table 4 presents the effect of floor on district-level educational outcomes estimated using equation (8). Panel A presents the effect of floor on student participation. Student dropout rate declines by 0.38 pp in school districts in floor states, which is an 8.6% compared to the average of 4.4%. Accordingly, student completion rate increases by 0.42 pp, which is naturally similar with a change in dropout rates. Second, the student-teacher ratio decreases by 1, which is a 4% decrease compared to the average ratio of 25. A smaller average class size $-$lower student-teacher ratio$-$ suggests better experience for student participation and engagement (Hanushek, 1997).

In unreported results, I find that the floor does not improve student test scores, consistent with Cellini et al. (2010) in that their study finds that bond issuance does not improve test scores. Test scores may depend on a student's subject preference, aptitude, and test style. I argue that student dropout rates and the student-teacher ratio are better measures for student participation and engagement in schools in the context of revenue protection and lower borrowing costs.

In Panel B, I find that the floor increases total salaries pp by 1.6% in school districts in floor states than non-floor states. Total salaries include not only instruction salaries but also support staff salaries and administration salaries. When I focus on instruction salaries *pp*, the salaries increase by 3% in school district floor states relative to non-floor states. The number of teachers increases by only 1.7% on average. So, the rise in instruction salaries is unlikely to be driven solely by a larger size of teachers, but also by salaries per teacher.[9] The employee benefits *pp* increases by 4% in floor states relative to non-floor states. These employee benefits are the non-salary compensation that school districts provide to employees, such as health and unemployment insurance and retirement and pension contributions.

Finally, Panel C shows effect of floor on expenditure *pp*, particularly focusing on education, instruction, support services, and school lunch. 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 9 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, the student dropout rate declines by 0.2 to 0.4 pp in school districts in floor states compared to those in non-floor states post the adoption of the floor. In Panel B, 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. Altogether, parallel trend assumption holds well, supporting the causal link between the floor and enhancement in student achievement and the teaching environment.

[9]The NCES data provides data in district-level, not teacher-level.

6 Heterogeneous Effects and Robustness Checks

6.1 Heterogeneous Effects

Real estate price volatility. Floors are more valuable in school districts where local prices are volatile. A floor mitigates the transmission of house price volatility to property tax revenue, and thereby stabilizes local fiscal capacity during downturns. Therefore, a natural heterogeneity test is to compare the effects of floors between high- and low-volatility areas. I test this using the staggered quadruple-difference design as follows:

$$
\begin{aligned}
y_{it} = \beta &\left(\text{Floor}_{st} \times \text{School}_i \times \sigma_i^{\mathbf{RE}}\right) \\
&+ \pi_1 \text{Floor}_{st} \\
&+ \pi_2 \text{Floor}_{st} \times \text{School}_i \\
&+ \pi_3 \text{Floor}_{st} \times \sigma_i^{\mathbf{RE}} \\
&+ \pi_4 C_{it} + \mu_i + \delta_t + \varepsilon_{st},
\end{aligned}
\tag{9}
$$

for issuer i, state s, and year t. Outcome variables are the logarithm of property tax revenue for issuer i in year t (i.e., $\log(\text{Property Tax}_{it})$) and the logarithm of its volatility (i.e., $\log(\sigma_{it}^{\text{Property Tax}})$), where the volatility is measured as the three-year rolling standard deviation of changes in property tax revenue. The treatment Floor_{st} equals one if state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise, and School_i equals one if issuer i is a school district and zero if it is a county. Moreover, I add a new treatment σ_i^{RE}, which equals one if the issuer i's local price volatility is above the median, and zero otherwise. The volatility is measured as the standard deviation of local real estate prices. Altogether, this identification compares property taxes in school districts with high-local volatile prices in floor states (i) before and after the enactment of the floor; (ii) to school districts in non-floor states; (iii) to counties; and (iv) to school districts with low-local volatile prices in floor states. The last variation is the new part in the staggered quadruple-difference approach compared to the staggered triple-difference approach.

There are two types of real estate properties, residential and commercial. Residential properties are single-family residential, condos, townhouses and apartments. Commercial properties are non-residential properties such as offices, buildings, retail, and industrial properties. The share of residential property in tax base is on average 71% across counties and districts, as shown in the Appendix Figure A.6.

Panel B in Table 3 shows the effect of floor on property tax revenue of high-volatile school districts compared to low-volatile school districts and to counties estimated using equation (9). Following a rise in local price volatility, school districts in floor states benefit more from downside protection. School districts show declines in property tax volatility whereas they do not experience declines in the level of property tax revenue. The volatility falls by 14% following a rise in local house price volatility and by 5% following a rise in local commercial real estate price volatility. It makes sense that the magnitudes become smaller for commercial real estate property because the share in commercial property is on average 30%, which is much smaller than the 70% in residential. Importantly, these school districts do not experience declines in the level of property tax revenue. In fact, the level increases by 2% in areas with high local house price volatility and 1.2% in areas with high local commercial real estate price volatility. Altogether, these results suggest that the volatility declines without a decline in the level of property tax revenue.

Reliance on property tax revenue. The impact of floor on bond spreads is larger for local governments that depend more 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. Panel D of Figure 5 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 a smaller effect of 7 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 bond spreads is substantially stronger for long-term bonds than for short-term bonds. As shown in Panel C of Figure 5, the floor lowers yield spreads of long-term bonds by 15 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 bond spreads is not driven by a reduction in short-term rollover risk, but instead by a fundamental exposure to fiscal policy.[10] Long-term bonds embed expectations about fiscal fundamentals over extended horizon, whereas short-term bonds 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.

Liquidity risk. 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 8 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. 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. 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

[10] 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.

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 9, 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 9.

6.2 Robustness Checks.

I conduct a battery of robustness checks and confirm that the main results are robust under alternative measures and specifications.

Alternative measures of floor: Distance to the floor. Property tax regimes evolve over time. For example, Georgia established a minimum rate of 5 mills in 2001. Over time, Georgia increased the minimum effort requirement to 14 mills in 2012 and reduced it to 10 mills in 2024.[11] More importantly, the floor may bind only when tax bases and mill rates approach the relevant constraint. The binary indicator for the floor may not fully capture the effective protection for local governments. To address this concern, I use a continuous-treatment DiD specification and employ the gap between the issuer's effective mill rate and the statewide minimum mill rate instead of the binary floor indicator in equations (3) and (2). A smaller distance indicates that a district rate is closer to the floor and therefore more likely to be affected by the protection. As presented in Table 10, results are robust using this alternative measure of floor. As shown in Panel A, school district bond yield spreads decline by 5 bps and issuance increases by 8%. As shown in Panel B, the staggered triple-difference estimation shows that district bond yield spreads decline by 10 bps and issuance increases by 10% compared to unprotected districts and counties.

Excluding economic recessions. Municipal bonds are often viewed as safe assets and attract

[11] HB824 (2012); HB987 (2024)

investors during periods of heightened uncertainty. Therefore, there is a significant peak in municipal bond yield spreads during economic recessions. Because municipal default risk is pronounced in economic recessions, one might be worried that the floor effects are centered around economic recessions only. However, I exclude the COVID-19 pandemic and the Global Financial Crisis from the analysis and finding robust results. As shown in Table 11, the declining effect remains more than 8 bps even when either event is excluded. Tellingly, results still hold in that bond spreads decline by 9 bps when both recessions are excluded.

Bad controls: credit ratings. My main specification follows prior studies in controlling for an issuer's credit rating. However, Iliev and Vitanova (2024) caution that credit ratings are a "bad control" in regressions of municipal bond yield spreads, because agencies construct credit ratings from many of the same local fiscal fundamentals that also determine municipal bond yields. A credit rating is therefore a co-determined outcome that partly reflects the variation researchers aim to study, rather than a clean control. Any portion of the effect of floor that is also reflected in credit ratings is absorbed by the rating control, attenuating the estimated floor coefficient. For this reason, excluding the credit rating control is expected to produce a larger decline in spreads. Table 12 presents these results. Excluding the credit rating control from the staggered triple-difference specification in equation (2) produces a larger estimated decline in school district bond yield spreads of 20 bps, compared to 13 bps in my baseline. At the same time, the adjusted R^2 stays similar, at 0.255 with the rating control and 0.221 without it. This indicates that the credit ratings measure the effect of the floor only to some extent rather than fully subsuming it.

Alternative measures of municipal bond yield spreads. There are three groups of states that need attention when measuring municipal bond yield spreads using the definition in Schwert (2017), as shown in equation (1). First, there are eight states that do not impose state individual income tax.[12] Second, there are six states that do not provide statewide income-tax exemptions on

[12] These states are Alaska, Florida, Nevada, South Dakota, Tennessee, Texas, Washington and Wyoming. New Hampshire taxes interests and dividend income only.

GO bond interest earned by in-state investors.[13] Third, some states do not allow exemption of federal income taxes paid. For these states, bond spreads defined in equation (1) will be mechanically higher than those in states with otherwise similar states that impose income taxes and provide tax exemptions for municipal bonds.

To mitigate these concerns, I use alternative measures of municipal bond yield spreads in testing equations (3), (2), and (9). First, I use nominal yields minus maturity-matched risk-free rate, in which the risk-free rate is provided by Liu and Wu (2021). Second, I use nominal yields minus maturity-matched risk-free rate, in which the risk-free rate is provided by Gürkaynak et al. (2007). These two measures do not have income-tax adjustment. Third, I use Garrett et al. (2023)'s adjustment in the yields. Garrett et al. (2023) considers (i) variation in state tax rates and whether states exempt municipal bond interest income from taxation; and (ii) changes in federal tax rates. If the results are robust to this measure, I interpret this as evidence that changes in federal and state income taxes, including the Bush tax cuts in 2012, do not materially affect the estimated effects of property tax floors on school district bond yield spreads.

Table 13 shows the results from estimating equations (3), (2), and (9) using alternative measures of municipal bond yield spreads. The estimates are consistent with the baseline results, showing declines in bond yield spreads ranging from 8 to 15 bp, compared to 13 bp decline in the baseline staggered quadruple-difference estimation. It is reasonable that the nominal yield spread generates smaller effects than the tax-adjusted spread because tax adjustments convert municipal yields into taxable-equivalent yields and change borrowing costs. Overall, the results are quantitatively similar across alternative measures, suggesting that the choice of yield spread measure does not materially affect the conclusions.

Local residents' race. Second, 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, 2022b) and makes underwriting

[13]These states are Illinois, Iowa, Nebraska, Oklahoma, Utah, and Wisconsin. These states may exempt taxes for certain specified obligations. For example, Utah exempts interest income from bonds issued to finance highway construction.

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 14, 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 are no significant differences in the bond spread change between local governments with more or less Black and Hispanic populations.

7 Conclusion

This study examines how the downside protection on local revenue affects local borrowing costs and human capital investment. To study this question, I employ a statewide floor regulation that protects property tax revenue of school districts but not of counties. I use a staggered triple-difference estimation, which not only accounts for staggered treatment of floor but also whether the issuer is the school district or county. I find that the school district bond yield spreads decrease by 13 bps in floor states compared to school district bonds in non-floor states and counties in all states. The bond issuance amount also increases by 4%. Lower borrowing costs may encourage moral hazard problems and increase wasteful spending. However, I find that the floor increases teacher salaries, and also decreases student dropout rates and student-teacher ratio. These improvements in educational outcomes support that the revenue protection translates into human capital investment rather than wasteful spending of school districts. Importantly, these better educational outcomes at a cost. Because school districts and counties overlap geographically, they share property tax base. Therefore, in downturns, a floor that prevents decline in school district property tax revenue has an adverse effect to the counties in that it decreases county property tax revenue. I find that the floor increases county bond yield spreads by average 5 bps. Protection that enhances student achievement and the teaching environment comes at the expense of counties in that county borrowing costs increase.

While my study focuses on the floor, there are many other regulations on local property collections. To provide a couple of examples, there are caps that restrict increases in property taxes and the variation in reassessment cycles of properties. A promising avenue for future research is to examine how these regulations, including their interactions with the floors, affect the fiscal adjustment of local governments and, in turn, influence municipal bonds.

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8 Figures and Tables

Figure 1: **Floor Legislation in the U.S.**
This map shows the enactment years of state policies that impose a floor on property tax revenue of school districts. The floor limits the decline in property tax revenue compared to the previous year. States impose floors on both assessed values and tax rates and thereby protects the total local property tax revenue from declining. Floor applies only to school districts, and not to counties. I collect statewide property tax mandates on floors from state statutes, administrative codes, and regulatory amendments. Section 2 describes institutional background in detail.

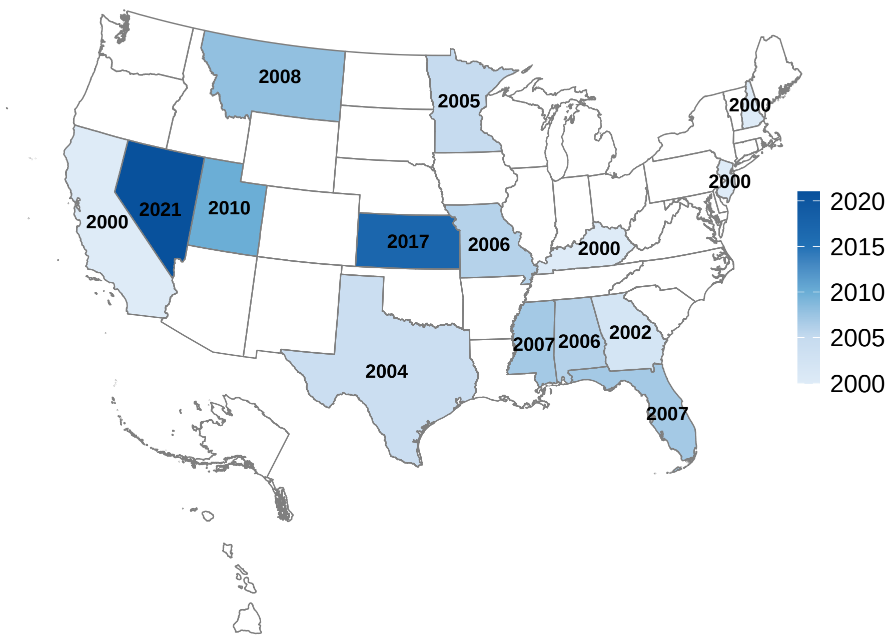


Figure 2: **Average Municipal Bond Yield Spreads**
This figure shows the average and cross-sectional interquartile range of municipal bond yield spreads from January 2000 to December 2022. The yield spreads are measured as yield minus maturity-matched Treasury rate and adjusted with income-tax exemption as in Schwert (2017), as presented in equation (1). The gray shaded areas show economic recessions.

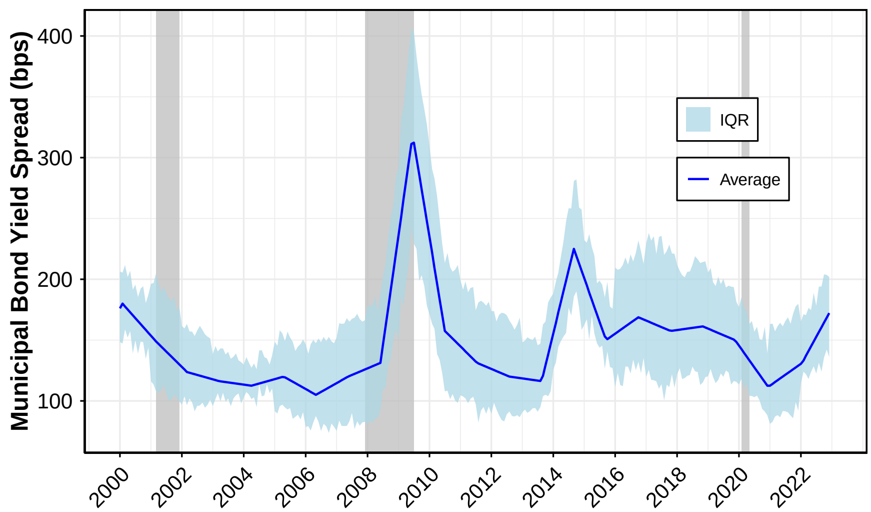


Figure 3: **Effect of Floor on Municipal Bond Yield Spreads**
This figure plots difference-in-differences estimates of the effect of floor on municipal bond yield spreads, in which yield spreads are measured as yield minus maturity-matched Treasury rate. The treatment Floor$_{st}$ equals one if state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. Panel A presents effect of floor on school district bonds relative to county bonds estimated using a staggered triple-difference approach. In Panel A, there are two types of issuers, school districts and counties. So, I add a treatment School$_i$ that equals one if an issuer i is a school district and zero if it is a county. Panel B presents effect of floor on school district bond yield spreads estimated using a staggered difference-in-differences approach and including school districts only as issuers. Panel C presents effect of floor on county bond yield spreads estimated a staggered difference-in-differences approach and including counties only as issuers. The horizontal axis denotes event time relative to the last period before treatment $(t = 0)$, with negative values corresponding to pre-treatment periods. Point estimates reflect the average treatment effect on the treated at each horizon, and the vertical bars report 95% confidence intervals. Section 4.1 explains estimation in greater detail.

Panel A. School District Bond Yield Spreads Compared to County Bond Yield Spreads

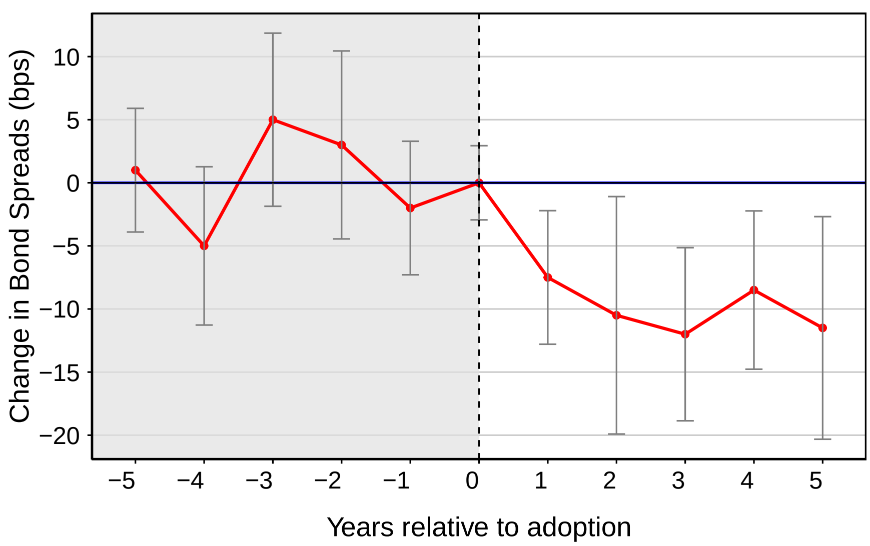
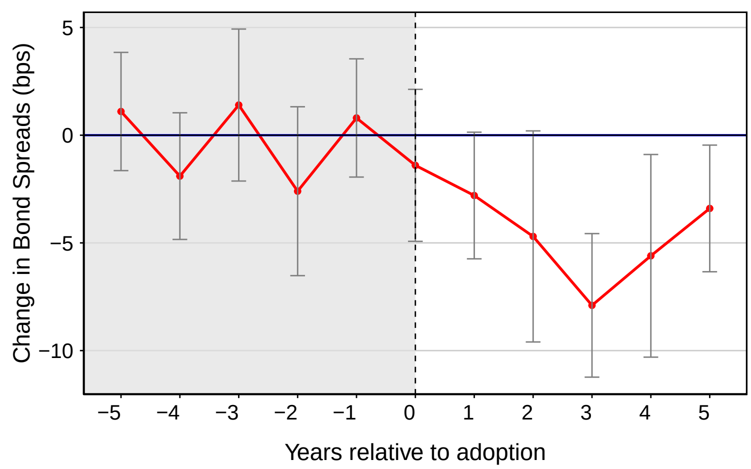


Figure 3: **Effect of Floor on Municipal Bond Yield Spreads (continued)**
This figure plots difference-in-differences estimates of the effect of floor on municipal bond yield spreads, in which yield spreads are measured as yield minus maturity-matched Treasury rate. The treatment Floor_{st} equals one if state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. Panel A presents effect of floor on school district bonds relative to county bonds estimated using a staggered triple-difference approach. In Panel A, there are two types of issuers, school districts and counties. So, I add a treatment School_i that equals one if an issuer i is a school district and zero if it is a county. Panel B presents effect of floor on school district bond yield spreads estimated using a staggered difference-in-differences approach and including school districts only as issuers. Panel C presents effect of floor on county bond yield spreads estimated a staggered difference-in-differences approach and including counties only as issuers. The horizontal axis denotes event time relative to the last period before treatment ($t = 0$), with negative values corresponding to pre-treatment periods. Point estimates reflect the average treatment effect on the treated at each horizon, and the vertical bars report 95% confidence intervals. Section 4.1 explains estimation in greater detail.

Panel B. School District Bond Yield Spreads



Panel C. County Bond Yield Spreads

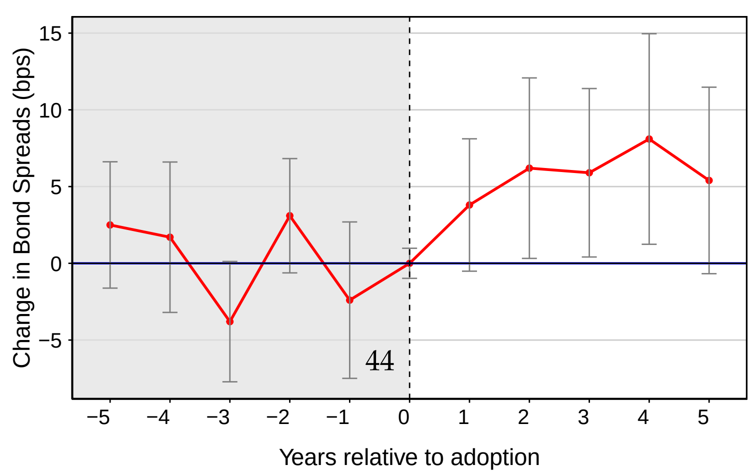


Figure 4: **Effect of Floor on Municipal Bond Issuance**
This figure plots difference-in-differences estimates of the effect of floor on municipal bond issuance, in which issuance is measured as the logarithm of the bond issuance amount in dollars. The treatment Floor_{st} equals one if state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. Panel A presents effect of floor on school district bonds relative to county bonds estimated using a staggered triple-difference approach. In Panel A, there are two types of issuers, school districts and counties. So, I add a treatment School_i that equals one if an issuer i is a school district and zero if it is a county. Panel B presents effect of floor on school district bond issuance estimated using a staggered difference-in-differences approach and including school districts only as issuers. Panel C presents effect of floor on county bond issuance estimated a staggered difference-in-differences approach and including counties only as issuers. The horizontal axis denotes event time relative to the last period before treatment $(t = 0)$, with negative values corresponding to pre-treatment periods. Point estimates reflect the average treatment effect on the treated at each horizon, and the vertical bars report 95% confidence intervals. Section 4.1 explains estimation in greater detail.

Panel A. School District Bond Issuance Compared to County Bond Issuance

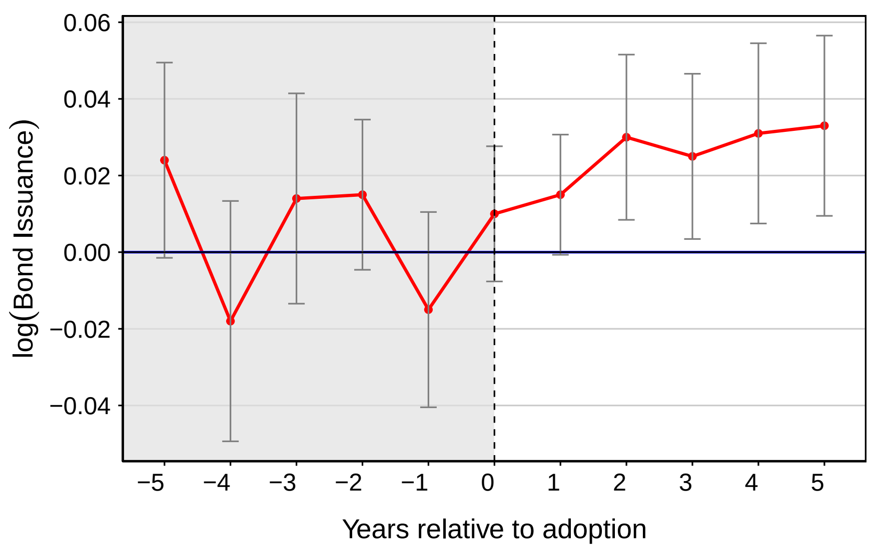
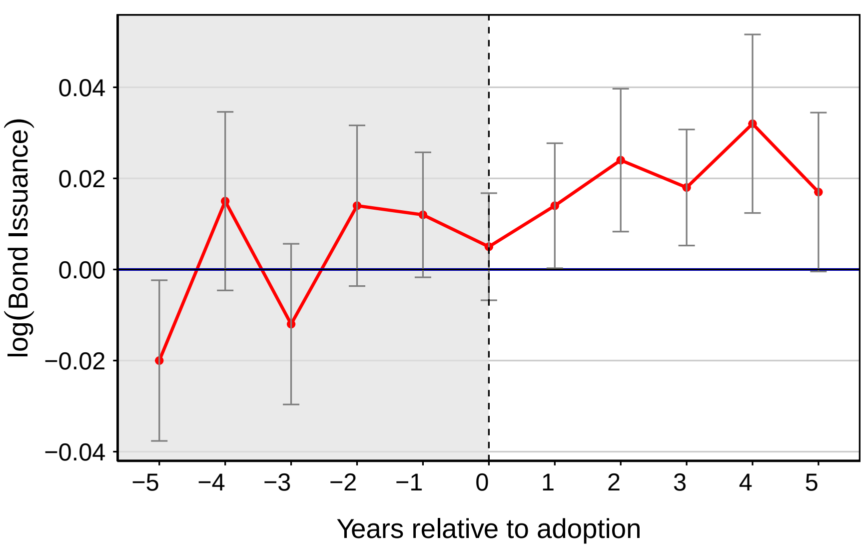


Figure 4: **Effect of Floor on Municipal Bond Issuance (Continued)**
This figure plots difference-in-differences estimates of the effect of floor on municipal bond issuance, in which issuance is measured as the logarithm of the bond issuance amount in dollars. The treatment Floor_{st} equals one if state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. Panel A presents effect of floor on school district bonds relative to county bonds estimated using a staggered triple-difference approach. In Panel A, there are two types of issuers, school districts and counties. So, I add a treatment School_i that equals one if an issuer i is a school district and zero if it is a county. Panel B presents effect of floor on school district bond issuance estimated using a staggered difference-in-differences approach and including school districts only as issuers. Panel C presents effect of floor on county bond issuance estimated a staggered difference-in-differences approach and including counties only as issuers. The horizontal axis denotes event time relative to the last period before treatment $(t = 0)$, with negative values corresponding to pre-treatment periods. Point estimates reflect the average treatment effect on the treated at each horizon, and the vertical bars report 95% confidence intervals. Section 4.1 explains estimation in greater detail.

Panel B. School District Bond Issuance



Panel C. County Bond Issuance

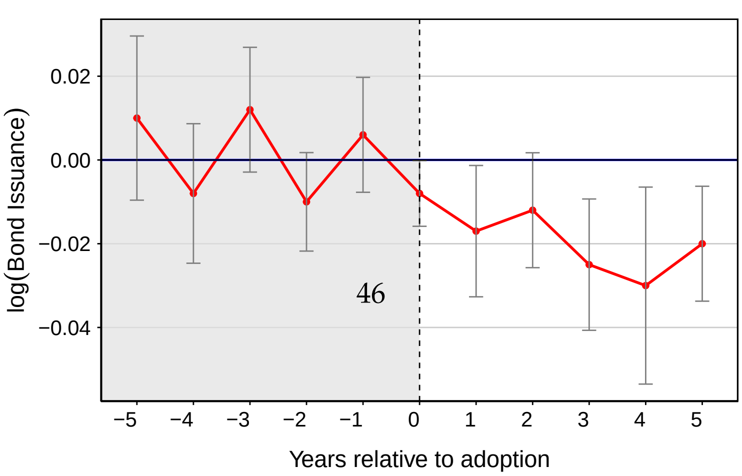


Figure 5: **Effect of Floor on Municipal Bond Yield Spreads by Heterogeneity**
This figure presents effect of floor on municipal bond yield spreads by issuer and bond characteristics. Panel A presents the effects based on school district income, in which low, middle, and high-income groups are sorted in terciles based on 2011. Panel B presents effects based on state transfers to the school district, in which low, mid, and high-transfer groups are sorted in terciles based on 2011. Panel C presents effects by municipal bond maturity, in which short-, medium-, and long-term bonds are defined as having maturities of 1–4 years, 5–9 years, and 10 or more years, respectively. Panel D presents effects based on the share of property tax revenue in total tax revenue for the school district, in which low, mid, and high-reliance groups are sorted in terciles based on 2011.

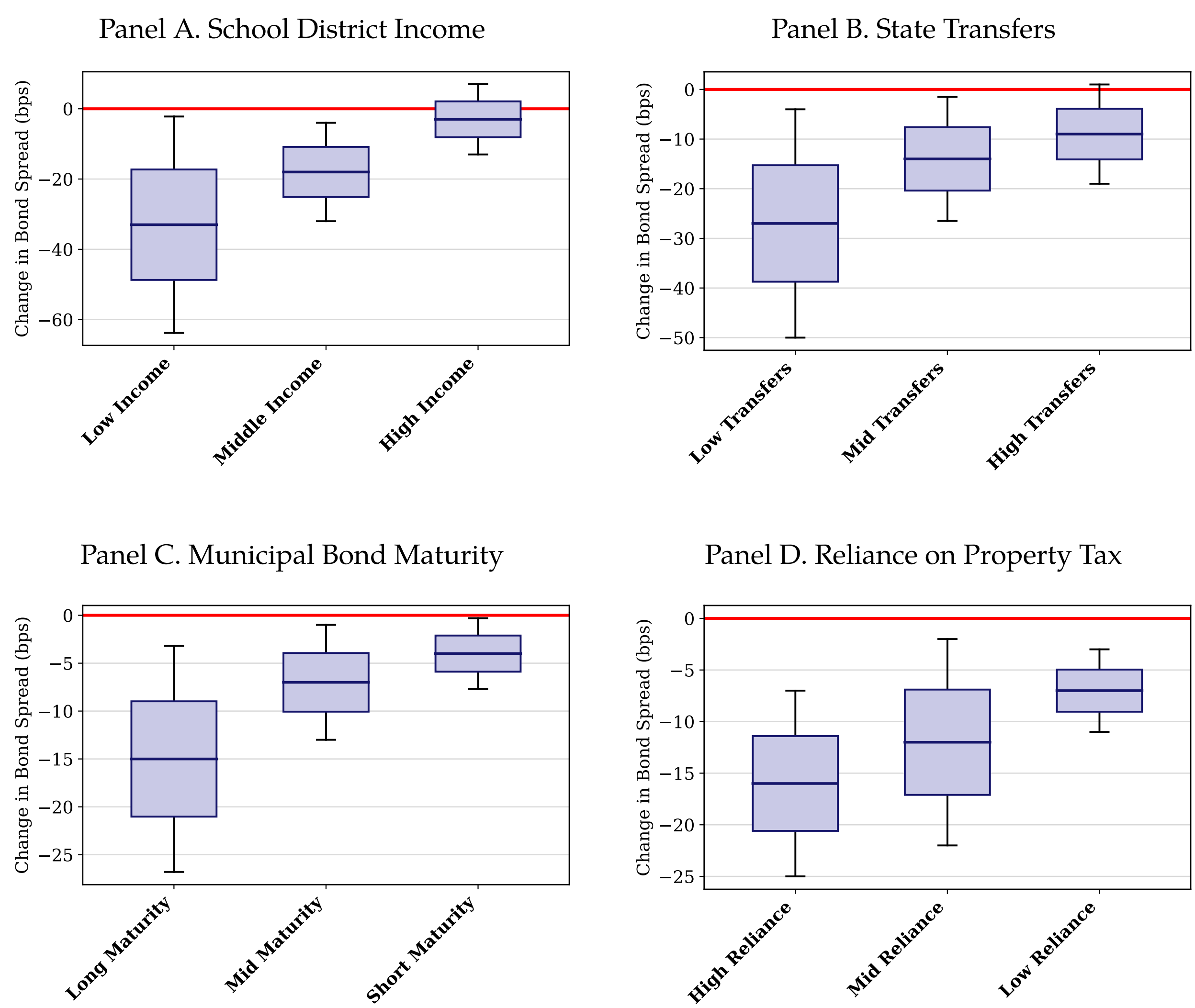
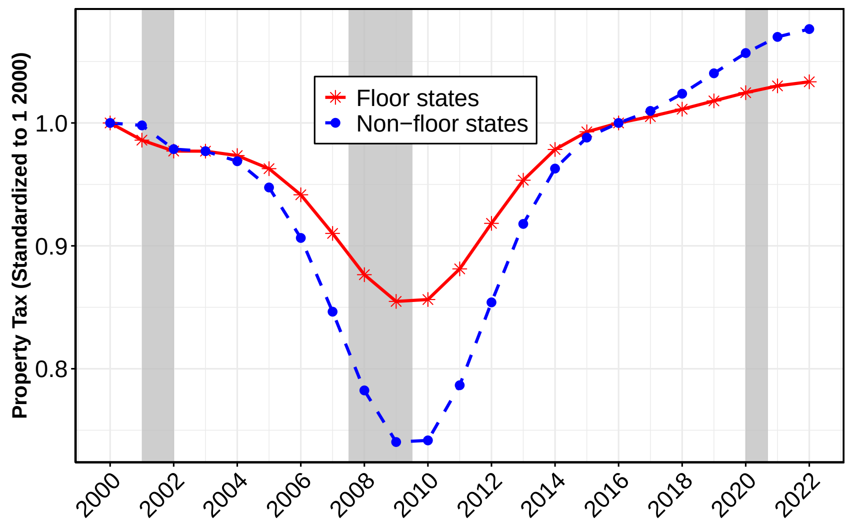


Figure 6: **Property Tax Trends in Floor and Non-Floor States**
This figure plots the levels and volatility of property tax revenue of school districts across floor and non-floor states. These states are presented in Figure 1. In panel A, property tax revenue levels are normalized to one in 2000. In panel B, property tax revenue volatility is calculated as the three-month rolling standard deviation of log changes in property tax revenue. The gray shaded areas show economic recessions.

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



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

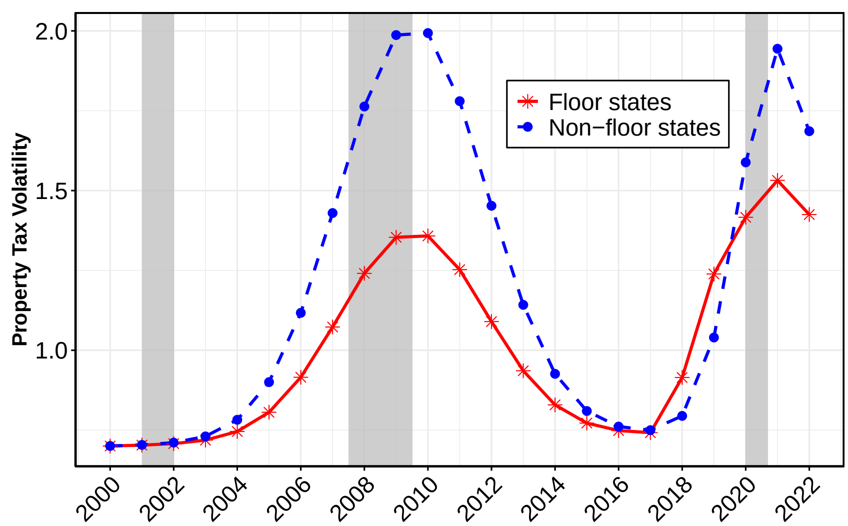
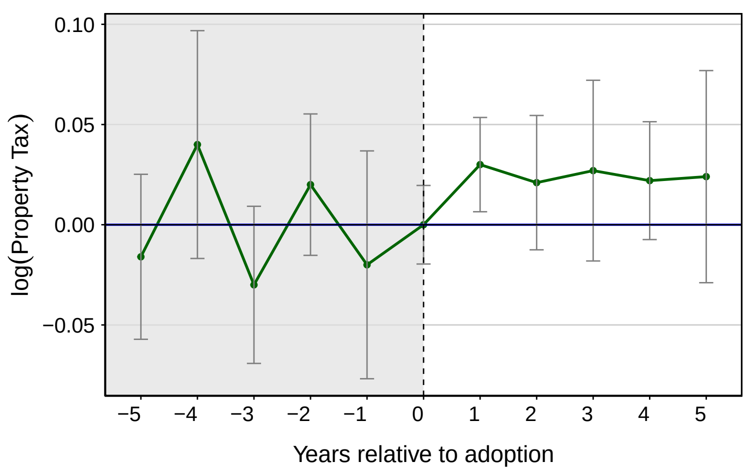


Figure 7: **Effect of Floor on Property Tax Revenue**
This figure plots difference-in-differences estimates of the effect of floor on property tax revenue. The treatment Floor_{st} equals one if state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. Floor only applies to school districts, not to counties. School_i is an additional treatment that equals one if an issuer i is a school district and zero if it is a county. Panel A presents effects of floor on the logarithm of property tax revenue and Panel B presents the effects of floor on the logarithm of property tax revenue volatility. The volatility is measured as the three-year rolling standard deviation of changes in property taxes. The horizontal axis denotes event time relative to the last period before treatment $(t = 0)$, with negative values corresponding to pre-treatment periods. Point estimates reflect the average treatment effect on the treated at each horizon, and the vertical bars report 95% confidence intervals. Section 4.2 explains estimation in greater detail.

Panel A. Property Tax Revenue Levels in School Districts Compared to Counties



Panel B. Property Tax Revenue Volatility in School Districts Compared to Counties

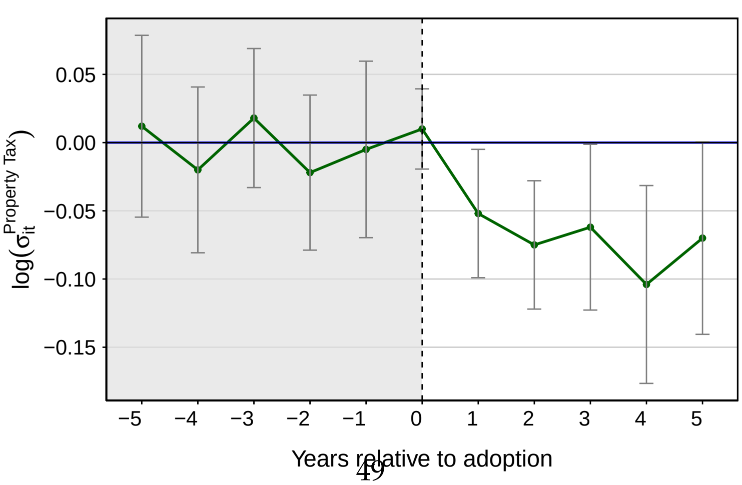


Figure 8: **Bivariate Regressions**
This figure plots the coefficients from bivariate regressions of floor enactment on the labeled state characteristics estimated from equation (5). The horizontal bandwidth reports 95% confidence intervals. The dependent variable, Floor_{st}, equals one if state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. The state characteristics are measured with a one-year lag. I standardize the covariates to have mean zero and unit standard deviation, controlling for year fixed effects. Section 4.3 discusses this estimation in detail.

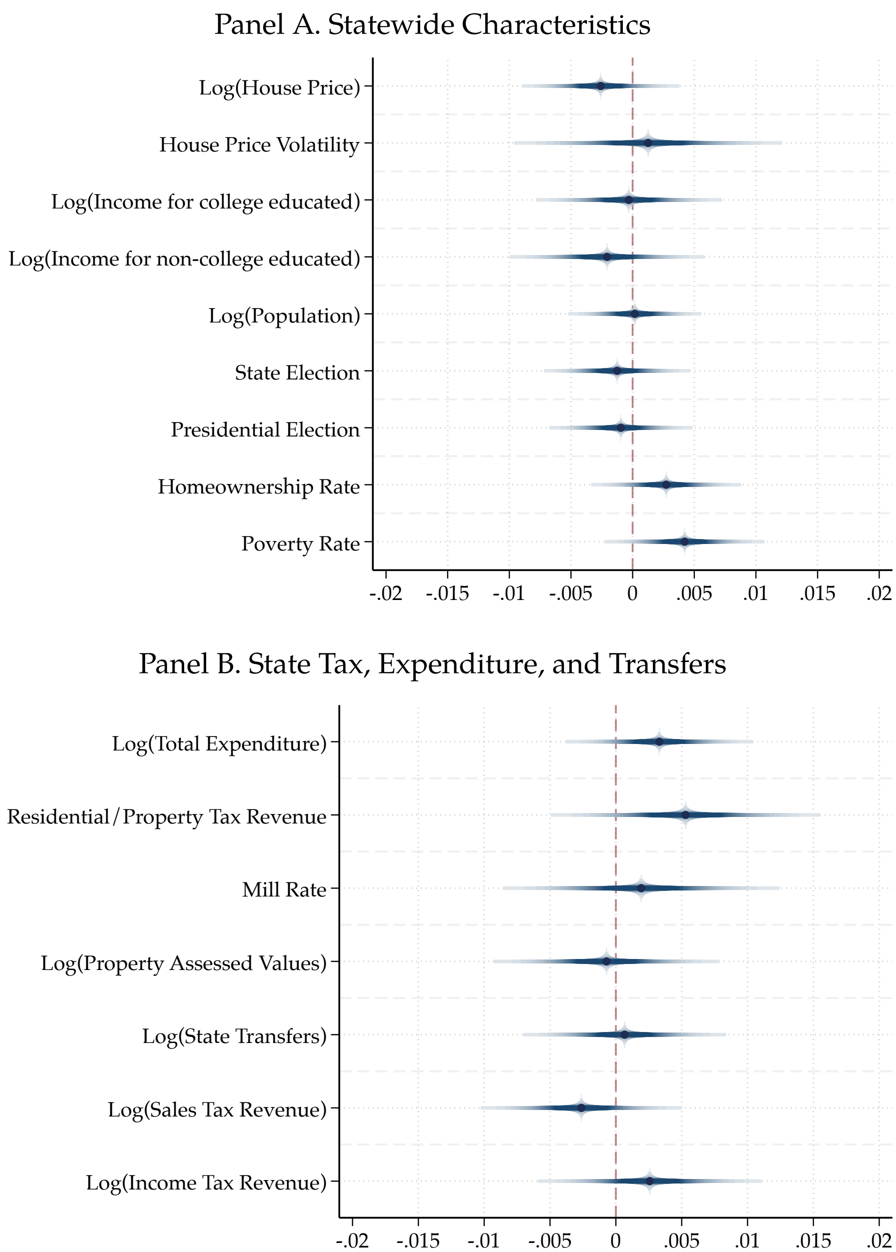
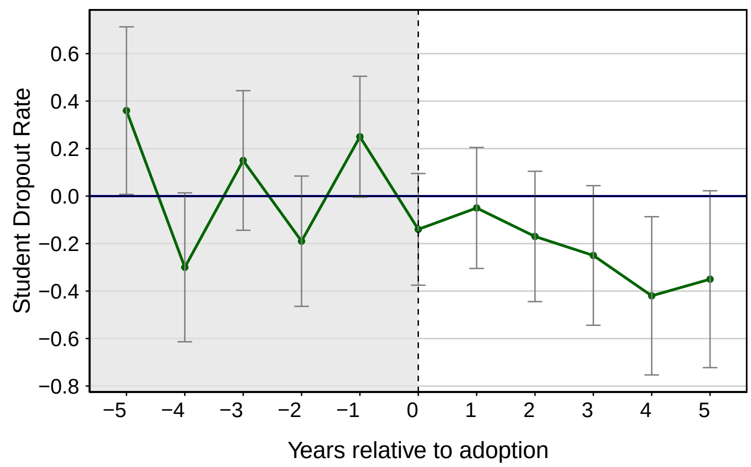


Figure 9: **Effect of Floor on Educational Outcomes**
This figure plots difference-in-differences estimates of the effect of floor on district-level educational outcomes. The treatment Floor_{st} equals one if a state s has enacted a floor regulation that limits the decline in property tax revenue in year t, and zero otherwise. The horizontal axis denotes event time relative to the last period before treatment $(t = 0)$, with negative values corresponding to pre-treatment periods. Point estimates reflect the average treatment effect on the treated at each horizon, and the vertical bars report 95% confidence intervals. Panel A presents effects of floor on student dropout rates. Panel B presents effects of floor on the logarithm of instruction salaries per pupil (pp). Section 5 explains estimation in greater detail.

Panel A. Student Dropout Rate



Panel B. Log(Instruction Salaries Per Pupil)

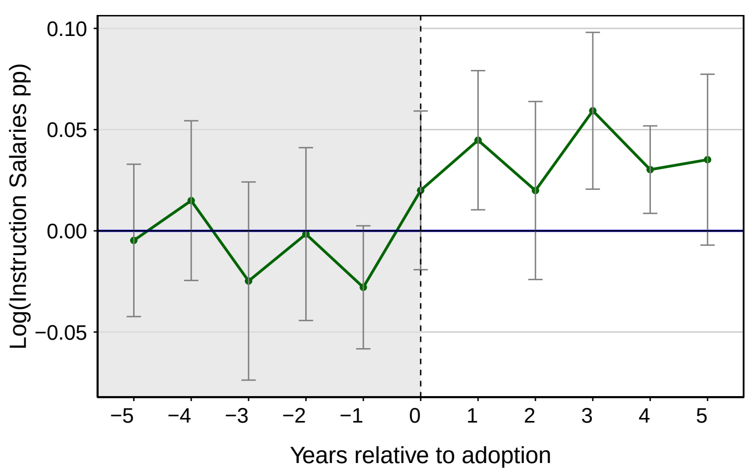


Table 1: **Descriptive Statistics**
This table reports mean, standard deviation, 1st, 25th, 75th, and 99th percentiles of key variables used in this study. Panel A presents municipal bond characteristics. The main dependent variable is tax-adjusted yield spread. This variable is measured as yield minus maturity-matched risk-free rate from Liu and Wu (2021) and adjusted with income-tax exemption as in Schwert (2017), as presented in equation (1). Panel B presents summary statistics of property taxes and house prices for school districts and counties. Panel C presents district-level outcomes on salaries, employee benefits, student dropout rate, and expenditures. Panel D presents the number of observations on issuers, bonds, and trades. The annual data span from 2000 to 2022.

Panel A. Municipal Bonds

	Mean	**SD**	**1st**	**25th**	**75th**	**99th**
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
Issue amount ($Mil)	22.4	32.5	0.23	7.5	42.32	156
Months to maturity	144.85	83.49	25	82	192	374
Coupon rate (%)	4.10	1.09	1.05	3.40	5	6

Panel B. Property Tax and House Prices

	Mean	**SD**	**1st**	**25th**	**75th**	**99th**
Property tax volatility (3-year)						
School District and County ($)	779	1,836	4	62	699	8,367
School District ($)	632	1,440	4	55	578	6,738
County ($)	2,069	4,638	20	207	1,880	21,369
Property tax volatility (10-year)						
School District and County ($)	1,688	2,989	24	181	1,732	15,065
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 ($)	44,409	53,008	5782	19,464	51,959	225,384
House price ($)	210,190	201,878	46,127	109,776	241,068	960,442
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

Table 1: **Descriptive Statistics (continued)**
This table reports mean, standard deviation, 1st, 25th, 75th, and 99th percentiles of key variables used in this study. Panel A presents municipal bond characteristics. The main dependent variable is tax-adjusted yield spread. This variable is measured as yield minus maturity-matched risk-free rate from Liu and Wu (2021) and adjusted with income-tax exemption as in Schwert (2017), as presented in equation (1). Panel B presents summary statistics of property taxes and house prices for school districts and counties. Panel C presents district-level outcomes on salaries, employee benefits, student dropout rate, and expenditures. Panel D presents the number of observations on issuers, bonds, and trades. The annual data span from 2000 to 2022.

Panel C. Educational Outcomes

	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
Number of teachers	371	1182	11	57	311	4077

Panel D: 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: **Effect of Floor on Municipal Bonds**

This table reports effect of floor on municipal bond yield spreads and issuance. The yield spreads are measured as the yield minus the maturity-matched Treasury rate, presented in equation (1). Issuance is measured as the logarithm of the bond issuance amount in dollars. The treatment Floor_{st} equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. Panel A presents effects of floor on school district bonds compared to county bonds estimated using a staggered triple-difference approach in equation (2). In Panel A, there are two types of issuers, school districts and counties. So, an additional treatment, School_i equals one if an issuer i is a school district and zero if it is a county. Panel B presents effects of floor on school district bond yield spreads estimated using equation (3) and including school districts only as issuers. Panel C presents effects of floor on county bond yield spreads estimated using equation (3) and including counties only as issuers. All specifications include district and year fixed effects and a full set of control variables described in the Appendix Section A.1.

Panel A. School District Compared to County Bonds

	(1)	(2)	(3)	(4)
	Municipal Bond Yield Spreads		**log(Municipal Bond Issuance)**	
	Coefficient	t-stat	Coefficient	t-stat
$\beta(\text{Floor}_{st} \times \text{School}_i)$	-0.130**	(-2.456)	0.038***	(2.556)
π_1 (Floor_{st})	-0.014	(-1.510)	0.045*	(1.810)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Bond Controls	Y	Y	Y	Y
Issuer Controls	Y	Y	Y	Y
N	1,231,150		1,231,150	
Adj. R^2	0.255		0.341	
Dep. Var. Mean	1.43%		16.92	
Mean ($)			$22,439,482	

Table 2: **Effect of Floor on Municipal Bonds (continued)**
This table reports effect of floor on municipal bond yield spreads and issuance. The yield spreads are measured as the yield minus the maturity-matched Treasury rate, presented in equation (1). Issuance is measured as the logarithm of the bond issuance amount in dollars. The treatment Floor_{st} equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. Panel A presents effects of floor on school district bonds compared to county bonds estimated using a staggered triple-difference approach in equation (2). In Panel A, there are two types of issuers, school districts and counties. So, an additional treatment, School_i equals one if an issuer i is a school district and zero if it is a county. Panel B presents effects of floor on school district bond yield spreads estimated using equation (3) and including school districts only as issuers. Panel C presents effects of floor on county bond yield spreads estimated using equation (3) and including counties only as issuers. All specifications include district and year fixed effects and a full set of control variables described in the Appendix Section A.1.

Panel B. School District Bonds

	(1)	(2)	(3)	(4)
	School District Bond Yield Spreads		**log(School District Bond Issuance)**	
	Coefficient	t-stat	Coefficient	t-stat
$\beta(\text{Floor}_{st})$	-0.071**	(-2.217)	0.029**	(2.220)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Bond Controls	Y	Y	Y	Y
Issuer Controls	Y	Y	Y	Y
N	697,366		697,366	

Panel C. County Bonds

	(1)	(2)	(3)	(4)
	County Bond Yield Spreads		**log(County Bond Issuance)**	
	Coefficient	t-stat	Coefficient	t-stat
$\beta(\text{Floor}_{st})$	0.050**	(2.330)	-0.025**	(-2.010)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Bond Controls	Y	Y	Y	Y
Issuer Controls	Y	Y	Y	Y
N	533,784		533,784	

Table 3: **Effect of Floor on Property Taxes**

This table presents effect of floor on property tax revenue levels and volatility. Two outcome variables are the logarithm of property tax revenue for issuer i in year t (i.e., $\log(\text{Property Tax}_{it})$) and the logarithm of volatility of property tax revenue (i.e., $\log(\sigma_{it}^{\text{Property Tax}})$), in which the volatility is measured as the three-year rolling standard deviation of changes in property taxes. The treatment Floor_{st} equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. The additional treatment, School_i equals one if an issuer i is a school district and zero if it is a county. Panel A presents effects of floor on school district property taxes relative to county property taxes using equation (2). Panel B adds local real estate price volatility (σ_i^{RE}) as an additional treatment and presents effects estimated using a staggered quadruple-difference approach in equation (9). σ_i^{RE} equals one if the issuer's local price volatility is above the median and zero otherwise. I use house prices and commercial real estate prices. All specifications include district and year fixed effects and a full set of control variables described in the Appendix Section A.1.

Panel A. School Districts Compared to Counties

	(1)	(2)	(3)	(4)
	$\log(\textbf{Property Tax}_{it})$		$\log(\sigma_{it}^{\textbf{Property Tax}})$	
	Coefficient	t-stat	Coefficient	t-stat
β ($\text{Floor}_{st} \times \text{School}_i$)	0.019*	(1.700)	-0.092***	(-2.981)
π_1 (Floor_{st})	0.018	(0.944)	0.120	(1.573)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	382,100		382,100	
Adj. R^2	0.482		0.405	

Panel B. School Districts Compared to Counties by Price Volatility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	House Price Volatility				Commercial RE Price Volatility			
	$\log(\textbf{Property Tax}_{it})$		$\log(\sigma_{it}^{\textbf{Property Tax}})$		$\log(\textbf{Property Tax}_{it})$		$\log(\sigma_{it}^{\textbf{Property Tax}})$	
	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat
β ($\text{Floor}_{st} \times \text{School}_i \times \sigma_i^{\text{RE}}$)	0.025*	(1.888)	-0.142**	(-1.999)	0.012**	(2.145)	-0.051**	(-2.078)
π_1 (Floor_{st})	0.018	(0.944)	0.120	(1.573)	0.080	(1.330)	0.105	(1.420)
π_2 ($\text{Floor}_{st} \times \text{School}_i$)	0.033**	(2.196)	-0.064**	(-2.110)	0.065**	(2.010)	-0.052*	(-1.850)
π_3 ($\text{Floor}_{st} \times \sigma_i^{\text{RE}}$)	-0.061	(-1.553)	0.253***	(2.998)	-0.095*	(-1.720)	0.210**	(2.450)
Issuer FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
N	382,100		382,100		323,423		323,423	
Adj. R^2	0.234		0.345		0.185		0.233	

Table 4: **Effect of Floor on Educational Outcomes**
This table presents effect of floor on district-level educational outcomes estimated using equation (8). The treatment Floor_{st} equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. In Panel A, the outcome variables are student dropout rate (%) and student-teacher ratio. In Panel B, the outcome variables are logarithms of total salaries per pupil (pp), instruction salaries pp, and employee benefits pp. In Panel C, the outcome variables are logarithms of expenditures on education pp, instruction pp, support services pp and school lunch pp. All specifications include district and year fixed effects and a full set of control variables described in the Appendix Section A.1.

Panel A. Student Participation

	(1)	(2)	(3)
	Student Dropout Rate (%)	**Student Completion Rate (%)**	**Student-Teacher Ratio**
β (Floor_{st})	-0.380^{***}	0.424^{**}	-1.001^{*}
t-stat	(-2.527)	(2.111)	(-1.888)
Issuer FE	Y	Y	Y
Year FE	Y	Y	Y
N	69,840	69,840	69,840
Adj. R^2	0.228	0.187	0.166
Dep. Var. Mean	4.4%	92.1%	25.4

Panel B. Instruction Outcomes Per Pupil (pp)

	(1)	(2)	(3)	(4)
	log(**Total Salaries** pp)	log(**Instruction Salaries** pp)	log(**Employee Benefits** pp)	log(**No. of Teachers**)
β (Floor_{st})	0.016^{**}	0.029^{**}	0.037^{***}	0.017^{**}
t-stat	(2.377)	(2.221)	(2.764)	(2.210)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	69,840	69,840	69,840	69,840
Adj. R^2	0.423	0.458	0.384	
Dep. Var. Mean	8.676	8.290	7.732	5.92
Mean	\$5,861	\$3,985	\$2,280	371

Table 4: **Effect of Floor on Educational Outcomes (continued)**
This table presents effect of floor on district-level educational outcomes estimated using equation (8). The treatment Floor$_{st}$ equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. In Panel A, the outcome variables are student dropout rate (%) and student-teacher ratio. In Panel B, the outcome variables are logarithms of total salaries per pupil (pp), instruction salaries pp, and employee benefits pp. In Panel C, the outcome variables are logarithms of expenditures on education pp, instruction pp, support services pp and school lunch pp. All specifications include district and year fixed effects and a full set of control variables described in the Appendix Section A.1.

Panel C. Expenditure Per Pupil (pp)

	(1)	(2)	(3)	(4)
	log(**Education** pp)	log(**Instruction** pp)	log(**Services** pp)	log(**School Lunch** pp)
β (Floor$_{st}$)	0.038***	0.051***	0.027***	0.078**
t-stat	(2.688)	(3.278)	(3.290)	(2.131)
Issuer 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: **Effect of Floor on Student Participation by Heterogeneity**
This table presents effect of floor on district-level educational outcomes by school district income and state transfers. The treatment Floor_{st} equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. In Panel A, the outcome variable is student dropout rate (%). In Panel B, the outcome variable is class size measured by student-teacher ratio. All specifications include district and year fixed effects.

Panel A. Student Participation

	Student Dropout Rate					
	School District Income			**State Transfers**		
	Low (L)	High (H)	L−H	Low (L)	High (H)	L−H
β (Floor_{st})	-0.420**	-0.220**	-0.200**	-0.401**	-0.305***	-0.096***
t-stat	(-2.149)	(-2.222)	(-2.001)	(-2.198)	(-2.511)	(-2.578)
Issuer FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
N	33,920	33,920	33,920	34,538	34,538	34,538
Adj. R^2	0.198	0.201	0.167	0.232	0.229	0.189

Panel B. Class Size

	Student-Teacher Ratio					
	School District Income			**State Transfers**		
	Low (L)	High (H)	L−H	Low (L)	High (H)	L−H
β (Floor_{st})	-2.000**	-0.545*	-1.455*	-1.110**	-1.000*	-0.110
t-stat	(-2.333)	(-1.890)	(-1.913)	(-2.189)	(-1.720)	(-1.455)
Issuer FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
N	33,920	33,920	33,920	34,538	34,538	34,538
Adj. R^2	0.153	0.156	0.188	0.201	0.202	0.200

Table 6: **Falsification Tests**

This table presents results on falsification tests. A falsified treatment, False Floor$_{s,t-h}$, equals one if a state s has not yet adopted the floor in $t-h$ but will do so in t for $h = 0, 1, \ldots, T$ (i.e., not-yet-treated state). False Floor$_{s,t-h}$ is zero if a state s never adopts the floor (i.e., never-treated states). Panel A presents effect of the False Floor$_{s,t-h}$ on school district bond yield spreads and issuance amount estimated using equation (6) including bonds issued by school districts only. Panel B presents the effect of False Floor$_{s,t-h}$ on school district bonds relative to county bonds estimated using equation (7) including bonds issued by school districts and counties. In Panel B, there are two types of issuers, school districts and counties. So, I add another treatment, School$_i$, which equals one if an issuer i is a school district and zero if it is a county. Section 4.3 explains this estimation in greater detail.

Panel A. Falsification Test in DiD

	(1)	(2)	(3)	(4)
	School District Bond Spreads		**log(School District Bond Issuance)**	
	Coefficient	t-stat	Coefficient	t-stat
β (False Floor$_{s,t-h}$)	0.028	(1.451)	-0.059	(-1.543)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Bond Controls	Y	Y	Y	Y
Issuer Controls	Y	Y	Y	Y
N	1,231,150		1,231,150	
Adj. R^2	0.255		0.341	

Panel B. Falsification Test in Triple-Difference

	(1)	(2)	(3)	(4)
	Municipal Bond Spreads		**log(Municipal Bond Issuance)**	
	Coefficient	t-stat	Coefficient	t-stat
β (False Floor$_{s,t-h}$ $\times$ School$_i$)	0.010	(0.635)	-0.012	(-0.593)
π_1 (False Floor$_{s,t-h}$)	-0.007	(-0.451)	-0.019	(-0.643)
	-0.004	(-1.135)	-0.003	(-0.664)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Bond Controls	Y	Y	Y	Y
Issuer Controls	Y	Y	Y	Y
N	1,231,150		1,231,150	
Adj. R^2	0.255		0.341	

Table 7: **Placebo Test**
This table presents results on placebo test. Motivated by Butler and Yi (2022) and Butler et al. (2025), I use revenue bonds instead of school district and GO bonds. Here, I include counties only as issuers. The treatment Floor_{st} equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. Section 4.3 explains this estimation in detail.

	(1)	(2)	(3)	(4)
	Revenue Bond Yield Spreads		**log(Revenue Bond Issuance)**	
	Coefficient	t-stat	Coefficient	t-stat
$\beta(\text{Floor}_{st})$	0.023	(1.217)	0.021	(0.980)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Bond Controls	Y	Y	Y	Y
Issuer Controls	Y	Y	Y	Y
N	932,202		932,202	
Adj. R^2	0.058		0.034	

Table 8: **Effects of Floor on Municipal Bond Yield Spreads By Liquidity Risk**
This table presents effect of floor on municipal bond yield spreads by bond liquidity risk. High and low-liquidity risk bonds are grouped based on median of liquidity risk measured using Amihud (2002)'s price of impact. The Appendix Section A.1 describes this measure in detail.

	(1)	(2)
	Municipal Bond Spreads	
	High Liquidity Risk	**Low Liquidity Risk**
β (Floor$_{st}\times$ School$_i$)	-0.164**	-0.159**
	(-2.290)	(-2.110)
Issuer FE	Y	Y
Year FE	Y	Y
Bond Controls	Y	Y
Issuer Controls	Y	Y
N	602,411	609,598
Adj. R^2	0.251	0.258

Table 9: **Effects of Floor on Municipal Bond Yield Spreads by Co-Party Affiliation**
This table presents effects of floor on municipal bond yield spreads by co-party affiliation between the state governor and the U.S. president. Unified governments are defined as states in which the governor and the U.S. president belong to the same political party, and divided governments otherwise. Panel A compares federal transfers to local governments in states that are unified versus divided. Panels B reports effects of floor on school district bond yield spreads compared to the county bonds estimated using a staggered triple-difference approach. In Panel B, the sample is split into bonds issued by local governments in states that are unified versus divided.

Panel A. Federal transfers

	(1)	(2)	(3)	(4)
	Unified (U)	**Divided (D)**	**Difference (U−D)**	**t-stat**
State governments	12677879	13290614	-2393350	(-0.566)

Panel B. Effect of Floor on Municipal Bonds by Unified vs Divided Governments

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

Table 10: **Alternative Measures of Floor**
This table reports results from a continuous-treatment DiD specification that replaces the binary floor indicator in equations (3) and (2) with the gap between the issuer's effective mill rate and the statewide minimum mill rate. Section 6.2 provides details.

Panel A. School Districts

	(1)	(2)	(3)	(4)
	School District Bond Yield Spreads		**log(School District Bond Issuance)**	
	Coefficient	t-stat	Coefficient	t-stat
$\beta(\text{Distance}_{it})$	-0.056*	(-1.888)	0.078*	(2.000)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Bond Controls	Y	Y	Y	Y
Issuer Controls	Y	Y	Y	Y
N	697,366		697,366	

Panel B. School Districts Compared to Counties

	(1)	(2)	(3)	(4)
	Municipal Bond Yield Spreads		**log(Municipal Bond Issuance)**	
	Coefficient	t-stat	Coefficient	t-stat
$\beta(\text{Distance}_{it} \times \text{School}_i)$	-0.098**	(-2.321)	0.100***	(2.714)
π_0 (Distance_{it})	-0.011	(-1.284)	0.038*	(1.742)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Bond Controls	Y	Y	Y	Y
Issuer Controls	Y	Y	Y	Y
N	1,231,150		1,231,150	
Dep. Var. Mean	1.43%		16.92	
Mean ($)			$22,439,482	

Table 11: **Effects of Floor on Municipal Bond Yield Spreads Excluding Economic Recessions**
This table presents effect of floor on municipal bond yield spreads estimated using equations (3) and (2) excluding economic recessions, namely the Global Financial Crisis (2007–2009), Covid-19 pandemic (2020–2021), and both periods. All specifications include district and year fixed effects and a full set of control variables described in the Appendix Section A.1.

Panel A. School Districts

	(1)	(2)	(3)
	Exclude Recessions		
	GFC	**Covid**	**Both**
β (Floor$_{st}$)	-0.069**	-0.049**	-0.055**
	(-2.173)	(-2.112)	(-2.061)
Issuer FE	Y	Y	Y
Year FE	Y	Y	Y
Bond Controls	Y	Y	Y
Issuer Controls	Y	Y	Y
N	956,214	1,074,903	800,267
Adj. R^2	0.303	0.248	0.262

Panel B. School Districts Compared to Counties

	(1)	(2)	(3)
	Exclude Recessions		
	GFC	**Covid**	**Both**
β (Floor$_{st}\times$ School$_i$)	-0.080**	-0.011**	-0.010**
	(-2.200)	(-2.010)	(-2.333)
Issuer FE	Y	Y	Y
Year FE	Y	Y	Y
Bond Controls	Y	Y	Y
Issuer Controls	Y	Y	Y
N	956,214	1,074,903	800,267
Adj. R^2	0.156	0.233	0.143

Table 12: **Bad Controls: Excluding Credit Ratings**

This table reports the effect of floor on municipal bond yield spreads estimated using the staggered triple-difference approach in equation (2), excluding the issuer's credit rating from the set of control variables. Iliev and Vitanova (2024) caution that credit ratings are a "bad control" in regressions of municipal bond yield spreads, because rating agencies construct ratings from many of the same local fiscal fundamentals that also determine municipal bond yields. The yield spreads are measured as the yield minus the maturity-matched risk-free rate from Liu and Wu (2021) and adjusted for the income-tax exemption as in Schwert (2017). The treatment Floor$_{st}$ equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. The additional treatment School$_i$ equals one if an issuer i is a school district and zero if it is a county. All specifications include district and year fixed effects and the full set of control variables described in the Appendix Section A.1, except for the credit rating control.

School District Bonds Compared to County Bonds

	(1)	(2)
	Municipal Bond Yield Spreads	
	Coefficient	t-stat
β(Floor$_{st}$ $\times$ School$_i$)	-0.200**	(-2.222)
π_1 (Floor$_{st}$)	-0.023*	(-1.676)
Issuer FE	Y	Y
Year FE	Y	Y
Bond Controls	Y	Y
Issuer Controls	Y	Y
N	1,231,150	
Adj. R^2	0.221	

Table 13: **Alternative Measures of Municipal Bond Yield Spreads**
This table reports the results from estimating equations (3), (2), and (9) using alternative measures of municipal bond yield spreads. First, I do not do the exemption-based adjustment and use yield minus maturity-matched risk-free rate from Liu and Wu (2021). Second, I use yield minus maturity-matched risk-free rate from Gürkaynak et al. (2007). Third, I use an alternative method for tax adjustment following Garrett et al. (2023) and use their tax-adjusted yield less maturity-matched risk-free rate from Liu and Wu (2021).

Panel A. School Districts

	(1)	(2)	(3)	(4)	(5)	(6)
	No tax adjustment		**Gürkaynak et al. (2007)**		**Garrett et al. (2023)**	
	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat
$\beta(\text{Floor}_{st})$	-0.058**	(-2.041)	-0.047**	(-2.231)	-0.079**	(-2.034)
Issuer FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Bond Controls	Y	Y	Y	Y	Y	Y
Issuer Controls	Y	Y	Y	Y	Y	Y
N	697,366		697,366		697,366	

Panel B. School Districts Compared to Counties

	(1)	(2)	(3)	(4)	(5)	(6)
	No tax adjustment		**Gürkaynak et al. (2007)**		**Garrett et al. (2023)**	
	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat
$\beta(\text{Floor}_{st} \times \text{School}_i)$	-0.081**	(-2.204)	-0.104***	(-2.947)	-0.148**	(-2.012)
Issuer FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Bond Controls	Y	Y	Y	Y	Y	Y
Issuer Controls	Y	Y	Y	Y	Y	Y
N	1,231,150		1,231,150		1,231,150	

Table 14: **Effect of Floor on Municipal Bonds Controlling for Local Residents' Race**
This table presents effect of floor on municipal bond yield spreads estimated using equations (3) and (2) controlling for local resident's racial composition. I measure racial composition as a share of the race in total population in the municipality. All specifications include year fixed effect and a full set of control variables described in the Appendix Section A.1.

	(1)	(2)	(3)	(4)	(5)
	White	**Black**	**Hispanic**	**Asian**	**Non-Hispanic**
β (Floor$_{st}\times$ School$_i$)	-0.134**	-0.076*	-0.118**	-0.069*	-0.123**
	(-2.031)	(-1.922)	(-2.253)	(-1.814)	(-2.365)
π (Floor$_{st}$)	-0.072***	-0.051**	-0.067***	-0.046*	-0.070***
	(-2.941)	(-2.083)	(-2.712)	(-1.776)	(-2.884)
Issuer FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Bond Controls	Y	Y	Y	Y	Y
Issuer Controls	Y	Y	Y	Y	Y
Adj. R^2	0.224	0.231	0.228	0.233	0.227

A Online Appendix

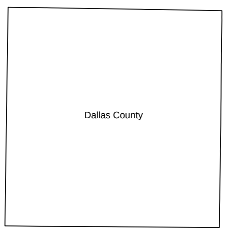
A.1 Measurements of Control Variables

Category	Description
Issuer characteristics	Growth in personal income of local residents, local employment, local population, and local GDP, all measured as annual growth from the previous year
Bond characteristics	Bond's time to maturity, trading volume relative to the outstanding amount, credit rating, and bond-month SD of transaction prices.
Liquidity risk	I use Amihud (2002)'s price of impact to measure liquidity risk of municipal bonds. The price of impact is an average return per volume of trade on a given month for a bond. $$\text{Amihud}_{b,t} = \frac{1}{N_t}\sum_{k=1}^{N_t}\frac{\left|\frac{P_k - P_{k-1}}{P_{k-1}}\right|}{Q_k}, \quad \text{(A.1)}$$ 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.
State transfers	I control for annual growth of state transfers from the state 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.

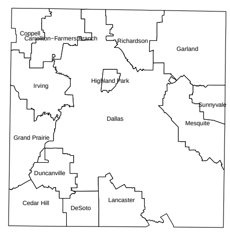
B Appendix Figures and Tables

Figure A.1: **Dallas County, School District, and City**
Panel A shows maps of Dallas County, school districts in Dallas County, and the city of Dallas. Panel B shows effects of floor on yield spreads of bonds issued by Dallas County, districts, and the city.

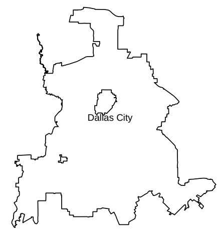
Panel A. Maps



(a) County

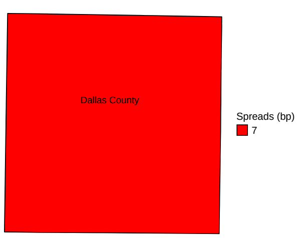


(b) School District

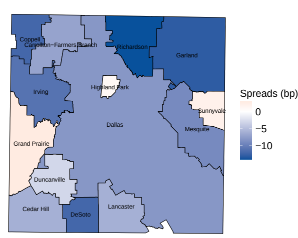


(c) City

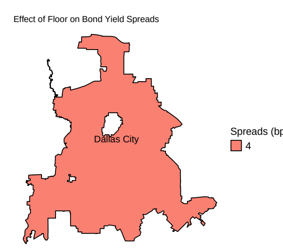
Panel B. Effect of Floor on Municipal Bond Yield Spreads



(a) County



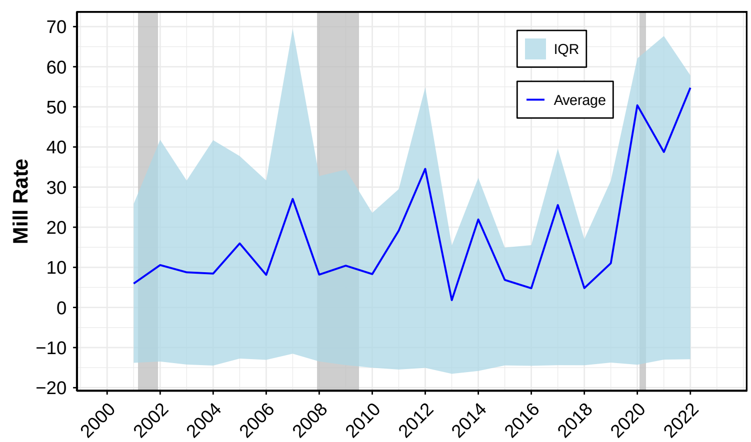
(b) School District



(c) City

Figure A.2: **Mill Rate.**
Panel A plots mean and cross-sectional inter-quartile range (IQR) of mill rates across counties and school districts from 2000 to 2022. I obtain data on mill rates from Lincoln Institute of Land Policy. Panel B shows an example of this data for the state of Alabama.

Panel A. Mill Rate Trends



Panel B. Example of mill rate in Alabama

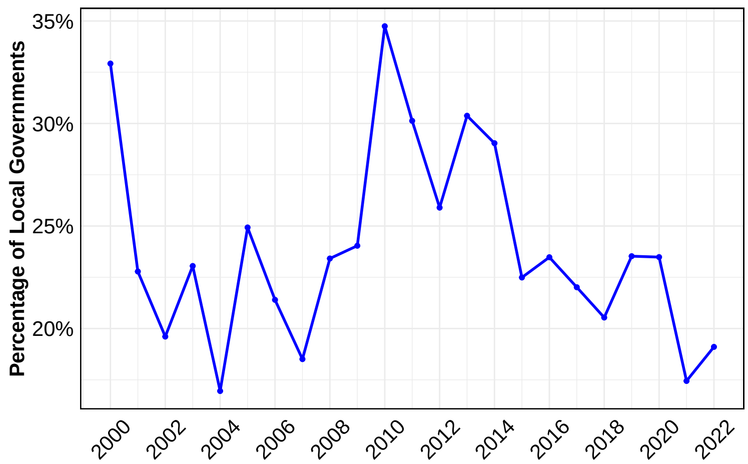
Alabama Department of Revenue

AUTAUGA - OCTOBER 2023

		MILLAGE
STATE		
PUBLIC SCHOOL	3.0	
SOLDIER	1.0	
GENERAL FUND	2.5	
TOTAL STATE		6.5
COUNTY		
GENERAL FUND	5.0	
ROAD & BRIDGE	2.5	
FIRE PROTECTION - OUTSIDE PRATTVILLE	3.0	

Figure A.3: **Decreasing Trends on Property Tax**
This figure plots the macro trends on property tax revenue of local governments. Panel A plots the fraction of local governments that experienced a decline in property tax revenue relative to the previous year. Panel B plots the average decline rate among these local governments. The Appendix Figures A.4 and A.5 show macro trends on increases and stable changes in property taxes.

Panel A. Percentage of local governments with declining property tax



Panel B. Average decline rate of property tax

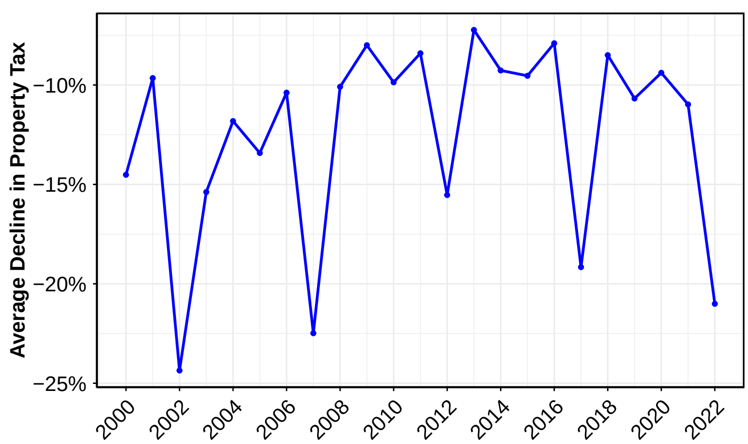
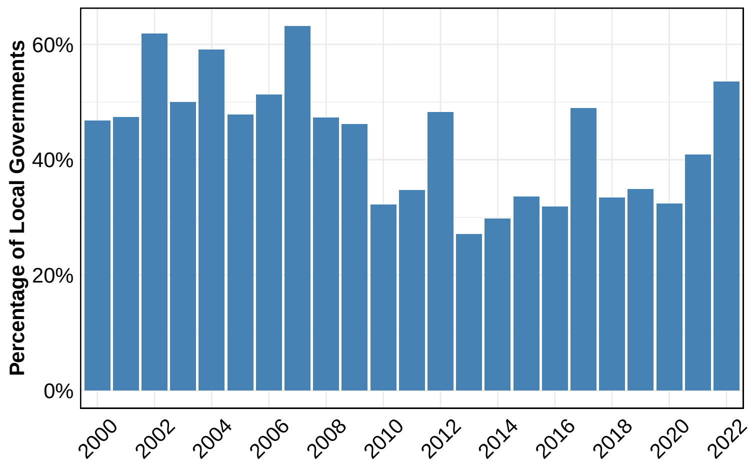


Figure A.4: **Increasing Trends on Property Tax**
This figure plots the macro trends on property tax revenue of local governments. Panel A plots the fraction of local governments that experienced an increase in property tax revenue relative to the previous year. Panel B plots the average increase rate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

Panel A. Percentage of local governments with increasing property tax



Panel B. Average increase rate of property tax

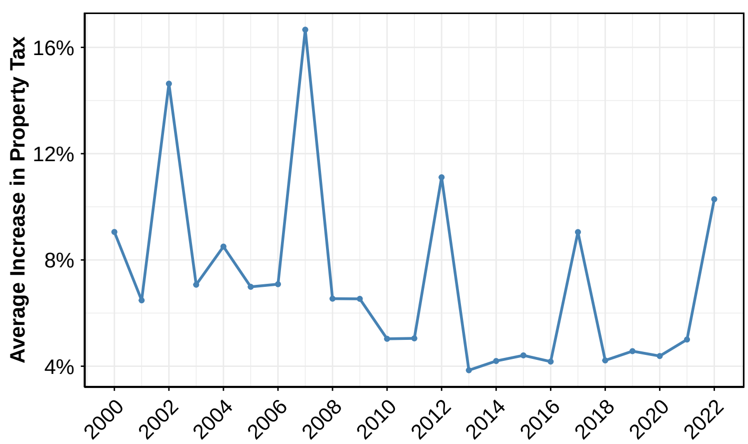
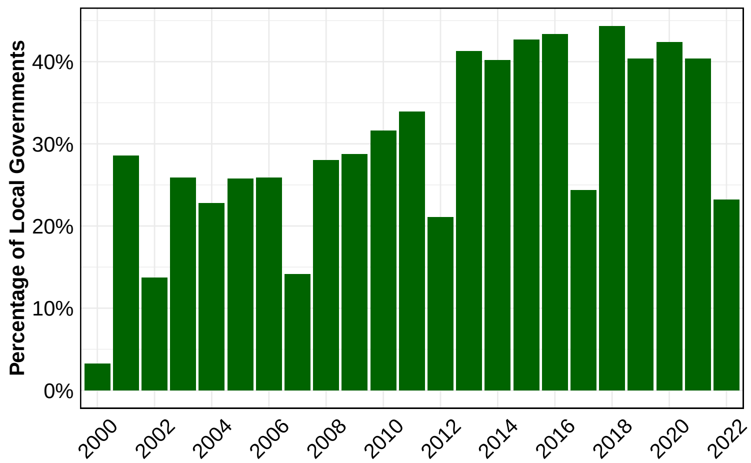


Figure A.5: **Stable Trends on Property Tax**
This figure plots the macro trends on property tax revenue of local governments. Panel A plots the fraction of local governments that experienced a stable change in property tax revenue relative to the previous year. Panel B plots the average change rate among these local governments.

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



Panel B. Average rate of property tax

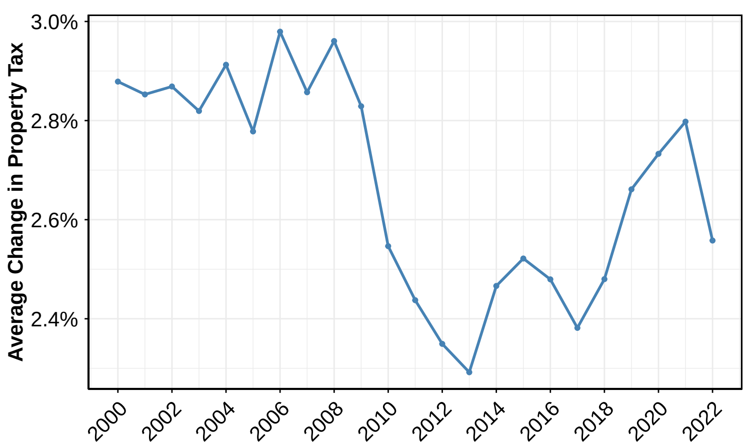
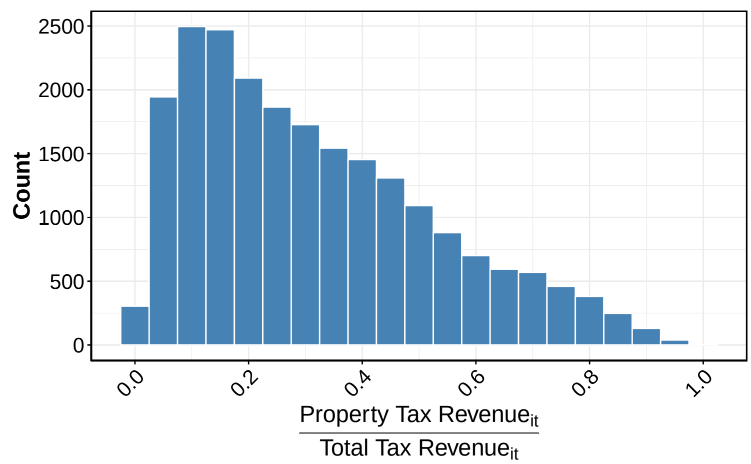


Figure A.6: **Property Tax Share**

Panel A plots a histogram on the distribution of government-year share of property tax in total tax revenue. I use data on revenue for school district and county from Census. Panel B plots average share of residential property in tax base across school districts and counties from 2000 to 2022. Residential properties include single-family residential, condos, townhouses, and apartments. Commercial properties include offices, buildings, retail, and industrial properties. I obtain data on tax bills and type of properties for each property from Corelogic TaxDeeds.

Panel A. Histogram on Share of Property Tax in Total Tax Revenue



Panel B. Share of Residential Property in Tax Base

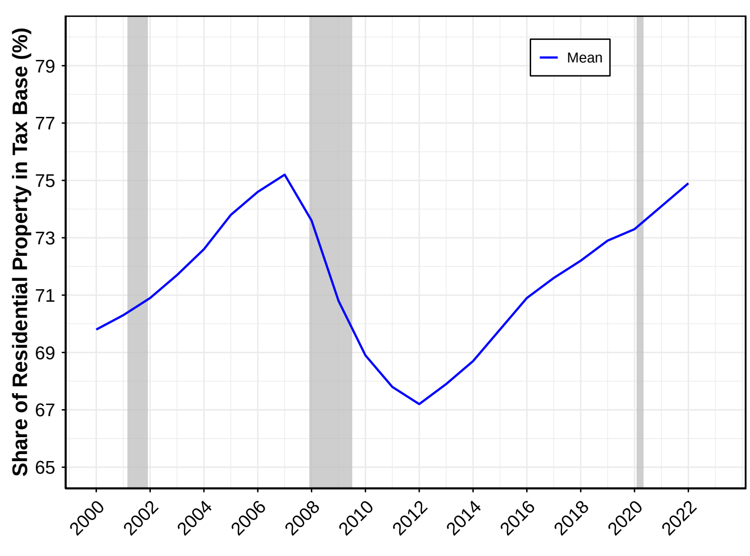
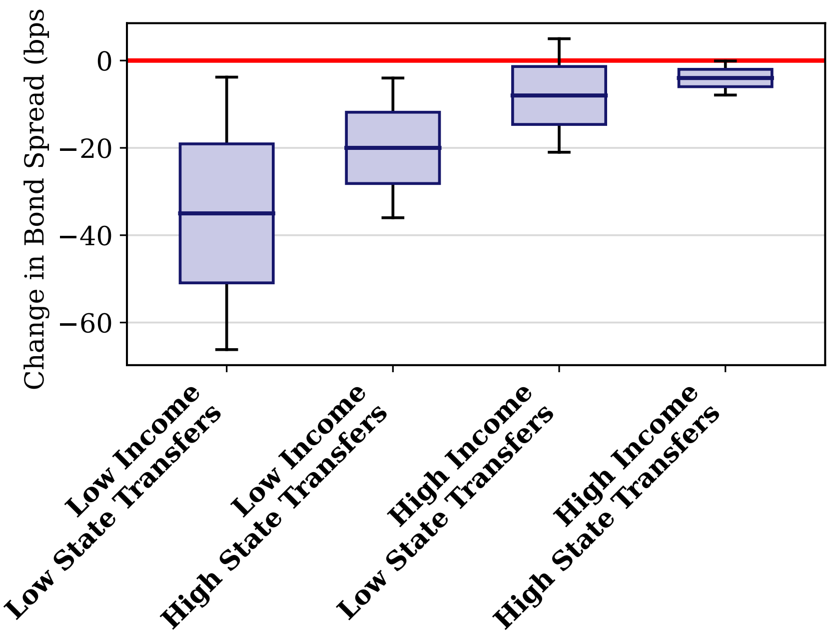


Figure A.7: **Effect of Floor on Municipal Bond Spreads by Income and State Transfers**
This figure presents the effect of floor on municipal bond yield spreads by the interaction of school district income and state transfers. School districts are double-sorted into low and high income and low and high state transfers based on 2011. The box spans the point estimate ± one standard error and the whiskers report the 95% confidence interval. The red line marks zero. This figure complements Panel B of Figure 5, which reports effects by state transfers alone.



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 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 3 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