Property Tax Protection and Municipal Bond Valuation *

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Abstract

I study how downside protection for property tax revenue affects local governments' borrowing costs and human capital investment. I exploit staggered, statewide adoption of floors, which limit declines in property tax revenue of school districts, but not of counties. Revenue protection reduces school district bond yields and increases school district bond issuance. This protection could encourage wasteful spending by school districts and generate moral hazard problems. However, I find that revenue protection improves student engagement and instruction compensation with more pronounced effects in low-income school districts. Counties bear the cost of this protection, facing an increase in their bond yields and a decline in their bond issuance. Overall, interest savings for school districts exceed interest costs borne by counties by 18%.

Keywords: Revenue Protection; Local Borrowing Costs; Municipal Bonds; Property Tax; 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. It is important that local governments can keep their property tax revenue stable during local recessions to repay their debt to municipal bondholders. However, after issuing bonds, local governments might not have the incentives or ability to raise property taxes when needed, due to political and legal constraints. Local governments face political resistance from taxpayers, and some states legally restrict increases in local property taxes (Avenancio-León and Howard, 2022a). These constraints lead to a commitment issue to the bondholders that can then limit the ability of local governments to raise funding and invest in public services.

In this paper, I study how downside protection for property tax revenue can help local governments address this commitment issue reducing their borrowing costs and increasing human capital investment. Statewide regulations provide this protection by limiting declines in property tax revenue of local governments. Ex ante, it is unclear whether and how much revenue protection affects borrowing costs of local governments. On the one hand, revenue protection can decrease local borrowing costs if local governments credibly commit to their bondholders to repay the debt. On the other hand, revenue protection can increase local borrowing costs if it generates moral hazard problems where local governments internalize revenue protection and increase wasteful spending (Cornaggia et al., 2024; Gao et al., 2019; Persson and Tabellini, 1996).[1] For example, the Allen school district in Texas issued $60 million of bonds to finance an 18,000-seat football stadium for a high school with 5,200 students (ESPN, 2012; Texas Monthly, 2014). This stadium had low attendance and closed two years after opening (Bleacher Report, 2014). After the stadium closed, this school district faced an increase in borrowing costs.

The effect of revenue protection on local borrowing costs is ultimately an empirical question.

[1] In the municipal bond literature, Cornaggia et al. (2024) raises and addresses a concern that bond insurance can lead to moral hazard problems where local governments engage in ex post riskier behaviors. Gao et al. (2019) shows that moral hazard problems happen in states that provide assistance to distressed municipalities. Persson and Tabellini (1996) shows that the downside protection provided by proactive states can generate moral hazard problems where local governments do not have fiscal disciplines.

I answer it by exploiting staggered, state-level adoption of *floor* regulations, which limit declines in the property tax revenue of school districts but not of counties. I find that a floor lowers school district borrowing costs. Between 2000 and 2021, fifteen states independently developed, enacted, and amended their own floors for school districts, each of which limits the year-over-year percentage decline in a school district's property tax revenue. Georgia, for example, restricts declines in school district property tax revenue to no more than 0.5% relative to the previous year. In floor states, when property value declines, school districts must increase their property tax rates to keep property tax revenue at or above the minimum threshold. School districts are nested within counties and draw property tax revenue from shared properties. But, school districts and counties set their own tax rates and issue their own bonds to support their services (Cornaggia et al., 2026). The floor regulation does not limit declines in county property tax revenue. Counties are not required to raise their rates if property values fall and therefore receive no comparable protection.

The main threat to identification is that school district borrowing costs may change due to confounding factors that are correlated with the presence of the floor. To address this concern, I exploit the fact that school districts and their counties are exposed to the same local economic shocks, while the floor limits the decline in property tax revenue of school districts but not of counties. I use triple-difference estimation, which compares the change in the difference between school district bonds and county bonds in floor states around the statewide adoption of the floor with the change in the difference between school district bonds and county bonds in non-floor states over the same time period. This estimation first differences school district bond yields against county bond yields to remove local shocks, and then differences the resulting school district–county spread in floor states against the same spread in non-floor states to remove common time trends. The resulting coefficient therefore measures the net effect of revenue protection on borrowing costs, isolated from confounding trends.

My first main result is that school districts in floor states face lower borrowing costs compared to counties in which these school districts are located and to school districts in non-floor states. The

floor limits declines in property tax revenue of school districts, but not of counties. In floor states, school districts show an average decline of 13 basis points (bps) in bond yield spreads, which represents a 9% decline compared to the total average of 143 bps. Furthermore, school districts increase bond issuance by 4% in floor states. I find that the reduction in borrowing costs is larger for (i) low-income school districts, showing a 33 bp decline in bond yield spreads; and (ii) for school districts that receive less state aid, showing a 27 bp decline in bond yield spreads.

The next step of this study is to examine whether and to what extent revenue protection for school districts has real effects on improving human capital. To proxy for human capital investment, I measure student engagement using student dropout rates and average class size. I first show that revenue protection increases school district spending. But, it is ex ante unclear whether an increase in school district spending is wasteful or not. So, I use student engagement as a real outcome to examine whether revenue protection leads to wasteful spending or instead improves educational outcomes. If the floor generated entirely wasteful spending, it would not lead to better student engagement. Conversely, if the floor enhances student engagement, I can interpret this as evidence that revenue protection improves educational outcomes rather than generating entirely wasteful spending.

My second main result is that school districts in floor states show better student engagement and instruction compensation than school districts in non-floor states.[2] School districts increase capital spending by 5% per pupil and operational spending by 3% per pupil in floor states.[3] This increase in operational spending includes an increase in instruction salaries by 3% per pupil and employee benefits by 4% per pupil. Importantly, I find that the student dropout rates decline by 0.38 percentage points in floor states over the long run, which is an 8.6% decline relative to the mean dropout rate of 4.4%. Lastly, the average class size becomes smaller in the floor states, with the student-teacher ratio declining by 4% relative to the mean. These real effects on student en-

[2] I exclude counties from this analysis because the outcome variables are school district-level educational outcomes.

[3] Capital spending refers to outlays used to acquire, build, or upgrade long-term physical assets, such as new construction and renovation. Operational spending covers current expenditures such as utilities, maintenance, security and safety, and support services.

gagement are more pronounced in low-income school districts and in school districts that receive less state assistance. When these school districts gain secure protection over their revenue, their incentives to invest in the future, including in their students, increase.

However, the benefits to school districts come at the expense of counties. The floor limits declines in property tax revenue of school districts, but not of counties. During local downturns, counties can increase their tax rate, hold it flat, or lower it. The floor regulation does not dictate which action counties take. I show that counties in floor states hold their rates flat or lower them, effectively redistributing property tax revenue to school districts. Counties support school districts when the floor binds, and this support is priced into county borrowing costs. Counties in non-floor states, on the other hand, face no such pressure to accommodate school district revenue needs and therefore provide a counterfactual for county borrowing costs absent the obligation to support school districts. I find that counties in floor states face higher borrowing costs compared to the counties in non-floor states, showing higher bond yield spreads by 5 bps and lower bond issuance by 3%. Overall, school districts benefit from interest savings, whereas counties bear higher interest costs, and I show that the gains to school districts exceed the costs borne by counties by 18%.[4]

The floor is important in the municipal bond market for four particular reasons. First, the floor limits declines in property tax revenue, providing downside protection for the bad state of the world where the municipal credit risk is most pronounced. Second, data show that declines in property tax revenue are common. From 2000 to 2022, property tax revenue declines in an average of 35% of school districts in non-floor states and 31% of counties, neither of which is protected by the floor. Third, I show that even for the school districts that do not see a decline in property tax revenue, the floor lowers their borrowing costs. The floor remains valuable for these school districts because it is a downside buffer against expected default risk ex ante before the default

[4]Section 4.1 shows this estimation in detail.

risk is realized.[5] Fourth, the floor protects local property tax revenue against volatile local prices of residential and commercial real estate. Prior studies show that municipal bondholders price the volatility of the underlying tax base into credit risk (Goldsmith-Pinkham et al., 2023; Painter, 2020).[6] I show that the floor reduces the volatility of property tax revenue of school districts without lowering the level of this revenue below the floor.

A causal interpretation of the results relies on the identification 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.[7] First, 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, statewide economic and demographic characteristics have limited predictability on statewide enactment of floors, including house price level and volatility, income, and election results, or by state taxes, transfers, and spending. Third, I do a falsification test on the adoption of the floor. There is no meaningful pre-existing differences between future-treated issuers and never-treated issuers prior to the treatment that predict a decline in local borrowing costs. Fourth, I use revenue bonds as a placebo test following Butler and Yi (2022) and show limited effects of the floor on revenue bond issuance and yield spreads. The main sample uses general obligation and school district bonds, which are directly backed by property taxes, whereas revenue bonds are backed by specific projects and less related to property taxes.

The remainder of the paper is as follows. Section 1 provides a 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

[5] A realized default is costly for local governments. Between 2000 and 2022, twenty-five general-purpose governments defaulted (Moody's Investors Service, 2023), including those that entered bankruptcy such as Jefferson County in AL, Stockton in CA, and Detroit in MI.

[6] Painter (2020) shows that sea-level rise is priced into municipal bonds. Goldsmith-Pinkham et al. (2023) studies sea-level rise and theoretically disentangles the effects of sea-level rise on the level and volatility of local tax base. By holding underlying asset values constant, they show that higher volatility of the tax base increases municipal bond yields.

[7] Section 4.3 discusses these estimations in detail.

school districts and counties. Finally, Section 6 concludes.

1 Literature Review

This paper provides novel insights for several strands of literature. First, my work relates to the literature on determinants of municipal bond yields and advances the understanding of how property tax revenue protection affects municipal borrowing costs. 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), 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 migration (Cornaggia et al., 2024, 2026; Gustafson et al., 2023), political uncertainty (Gao et al., 2019), data breaches (Curti et al., 2023), exemption on income tax for in-state bondholders (Babina et al., 2021; Fleckenstein and Longstaff, 2023; Garrett et al., 2023; Longstaff, 2011; Poterba and Verdugo, 2011), changes in SALT deduction (Ambrose et al., 2025), opioid crisis (Cornaggia et al., 2022), 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, their effects on local borrowing costs are understudied. I add to this literature by focusing on property tax revenue and showing that its downside protection is a first-order determinant that lowers local borrowing costs.

In the municipal bond literature, my paper particularly relates to two groups of studies. First, my paper relates to a few studies that examine financing for public education. Cornaggia et al. (2026) uses school bond elections in North Carolina and shows that political partisanship lowers bond yields. Dougal et al. (2019) identifies racial bias and finds that historically Black colleges and universities pay higher underwriting spreads. I add to this literature by identifying a fiscal determinant of borrowing costs of public school education, focusing on the property tax revenue and spending needs. I also examine whether revenue protection has real effects on improving educational outcomes. Second, my paper relates to studies that examine downside protection for

municipal bonds. Gao et al. (2019) shows that state assistance lowers yields of bonds issued by distressed municipalities in proactive states. Cornaggia et al. (2024) shows that downside protection provided by bond insurance has minimal effect on lowering borrowing costs after the global financial crisis. I add to this literature by identifying protection on own-source revenue of school districts, and how the floor benefits school districts at the expense of counties.

Furthermore, my study relates to the public policy literature that investigates the financial resources of public school education. The prior literature has not reached a consensus on whether financial resources improve educational outcomes. On the one hand, Cellini et al. (2010) finds that school district capital spending financed through bonds does not improve student test scores in California.[8] Similarly, Yang (2024) shows that a credit enhancement program does not improve student test scores. On the other hand, Biasi et al. (2025) and Lafortune et al. (2018) find that school district capital spending improves student test scores in low-income and socioeconomically disadvantaged school districts over the long run. Likewise, Lee (2025) finds that state transfers for instructional facilities increase student test scores and graduation rates in Texas.

There are three missing pieces in the existing literature that studies the effects of financial resources on educational outcomes. First, prior studies use school district bond referenda for identification purpose and do not examine local borrowing costs as the main outcome variable. Second, prior studies focus on state transfers or aid to school districts, not the own-source revenue that school districts and counties share, the property tax revenue. Third, prior studies examine school districts only and do not investigate redistribution of property tax revenue between school districts and counties. My paper studies these margins explicitly and studies the effects of property tax floor on local borrowing costs, measured by municipal bond yield spreads and issuance amount. In or-

[8]Cellini et al. (2010) uses school district bond referenda for identification and finds that improvements in public education are capitalized into higher local house prices. Building on this finding, Yi (2024) shows that the increase in local house prices is weaker in old school districts as they benefit less from future public education. Bayer et al. (2020) studies the efficiency of school spending using the housing market as a laboratory and shows that school spending is efficient if it does not change local house prices. My study differs from these papers because I examine the effects of property tax protection on local governments' borrowing costs, measured by municipal bond yields and bond issuance, not the capitalization into local house prices.

der to learn about this, I use school districts as the prime setting, and student engagement as a real outcome to test whether revenue protection has real effects. I then show that the the floor does not generate new property tax revenue, but redistributes it from counties to school districts, and results in an increase in county borrowing costs.

Finally, my paper relates to the literature that examines frictions in collection of property taxes. Researchers show that property tax collections are not equal across properties due to racial inequalities (Avenancio-León and Howard, 2022b), political partisanship (Kalda et al., 2024), and assessor's behavioral 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 with my study is that I focus on municipal bonds and examine how property tax affects local borrowing costs, rather than identifying the reasons for the gap between assessed values and market values of properties.

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 that issue municipal bonds and make spending decisions separately. 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 support K-12 public education and fund instruction salaries and benefits, instructional materials and technology, and school buildings and maintenance.

The floor imposes a lower-bound percentage cap that limits declines in property tax revenue of school districts, but not of counties, from the previous year. For example, Georgia's Quality Basic Education Act ensures that every public school district in Georgia must not decrease local

property tax revenue by more than 0.5% compared to the previous year. As another example, Nevada Revised Statute 361.4722 limits declines in total property tax revenue to no more than 2% for school districts compared to the previous year. Nevada also limits declines in mill rates to no more than 7.5 mills (i.e., 0.75%) for school districts.

Before states impose floors, many of them had ceilings which restrict the increase in property taxes by imposing a statutory upper-bound cap that limits the increase in property taxes (Downes and Figlio, 2008; Dye and McGuire, 1997). This ceiling is different from the floor because the ceiling imposes an upper-bound cap that restricts the increase in property taxes. Prior studies find that this ceiling can constrain spending on education due to lower per-pupil revenue. For example, Texas established a ceiling of 1.5% on the property tax rate that school districts could impose in 1993, which in turn reduced school expenditures (Plummer and Pavur, 2009). New York introduced a property tax ceiling in 2011, which led to declines in per-pupil revenues, instructional expenditures, teacher hiring, and support personnel (Nguyen-Hoang and Zhang, 2022; Sorensen et al., 2021). Illinois established a ceiling on property tax revenue in 1991, and Dye and McGuire (1997) shows this ceiling had a long-term effect of lowering instructional spending and administrative and support staff spending in school districts.

Moreover, local governments face political resistance to increasing property taxes even when the ceiling is not legally binding. Cabral and Hoxby (2012) refers to property taxes as the "most hated" tax. Prior studies show that property taxes are regressive, which means that poorer households have higher assessment values and wealthier households have lower assessment values (Berry, 2021; McMillen and Singh, 2023). Local governments face political resistance from poor households to increasing property taxes because they believe that they already face higher assessments. On the other hand, Weber and McMillen (2010) finds that rich households (i.e., those living in areas with high median home values and a larger proportion of homeowners) resisted huge spikes in their assessment values through the appeal process.

In the world where local governments face difficulties in increasing property taxes due to both

political and legal constraints, the floor was introduced to reduce disparities in educational spending (Biasi et al., 2025; Hoxby, 2001; Jackson et al., 2014, 2015; Lafortune et al., 2018). State legislatures and courts sought to reduce disparities in educational funding through changes to school finance systems. The enactment process varied across states, through voter referendum, legislative action, or state court mandate.[9] 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 to 2005. Six more states enacted it between 2006 and 2019, and two more states did so after 2016. The adopting states are geographically and politically diverse, spanning the South, West, East, and North, and including both Democratic and Republican states.

The floor redistributes property tax revenue between school districts and counties. If assessed values decline for the properties they share, then school districts must increase their tax rates to keep the property tax revenue above the minimum threshold. Evans et al. (2019) shows that school districts increase property tax revenue during local recessions because school districts respond to declining property values by increasing mill rates. The floor does not require counties to increase their mill rates. The burden of declining property tax revenue is redistributed from school districts to counties in floor states.

To illustrate, consider school districts and counties in Texas, a state that adopted the floor regulation in 2004. From 2011 to 2012, total assessed values in the Tarrant area declined from $125.9 billion to $100.4 billion.[10] The floor limits the decline in school district property tax revenue. The Fort Worth school district increased its tax rate from from 2.6 mills (i.e., 0.26%) to 3.4 mills, which increased its property tax revenue from $330 million to $337 million. The overlapping Tarrant County, on the other hand, kept its tax rate flat at 4.9 mills, so its property tax revenue declined from $615 million to $490 million, which is a 20% decline.

[9] 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. In Texas, the Supreme Court declared the existing school-finance system unconstitutional and directed the Legislature to adopt a more equitable funding framework. These floor policies share a common objective: enhance public school education.

[10] This is for the area that Tarrant County and the Fort Worth school district overlap.

In contrast, Florida does not impose a floor. The total assessed values in the Palm Beach area decreased from $127 billion to $124 billion from 2010 to 2011. In non-floor states, there is no legislation that dictates an action a county or a school district to take. Unlike school districts in floor states, Palm Beach school district decreased its tax rate from 8.9 mills to 7.7 mills, which decreased property tax revenue by 15% from $1,133 million to $961 million. Palm Beach County increased its tax rate from 6.83 mills to 6.94 mills, which increased property tax revenue from $858 million to $861 million.

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). As outlined in Jackson et al. (2014) and Jackson et al. (2015), there are five types of school-finance reforms, namely flat grants, equalization plans, categorical aid, foundation plans, and credit enhancement.[11] The common feature of these policies is that the state provides transfers, funds, or aid to public school districts. In contrast, the floor protects school districts' own-source revenue. There can be some discretion in state transfers to public school districts. For example, states tend to cut aid during economic downturns (Chakrabarti and Setren, 2015; Evans et al., 2019), precisely when municipal credit risk is most pronounced. Also, prior studies find that states provide more transfers to municipalities where they have stronger electoral support (Ansolabehere and Snyder Jr, 2006; Solé-Ollé and Sorribas-Navarro, 2008). Gao et al. (2019) finds that state assistance depends on whether the state is a Chapter 9 state or a proactive state. The floor, in contrast, applies uniformly to all school districts within a state, regardless of their political support. Therefore, the floor has an advantage in that it provides a guaranteed downside buffer against recessions for all school districts within the floor state.

[11] Flat grants provide transfers from states to school districts per pupil without considering the difference in wealth and spending needs across school districts. Equalization plans, on the other hand, provide state transfers to low-wealth school districts, typically measured by local property wealth. Likewise, categorical aid funds school districts if their residents are liquidity constrained (Hoxby, 2000). Foundation plans provide transfers to school districts where the expected contribution of school districts falls short of the foundation level of per-pupil spending. Lastly, states provide credit enhancement in which they ensure last-resort funds for the state to use and repay debt to bondholders if a school district defaults (Yang, 2024).

3 Data

This study relies on several data sources. First, I use municipal bond data from the 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 collect property tax regulations from state statutes, administrative codes, and regulatory amendments. Lastly, I use educational outcome data from the National Center for Education Statistics (NCES).

Municipal Bond Data. MSRB 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 a fixed or variable coupon, and information on issuance size and transaction amount. Critically, Bloomberg reports the issuer's name and type, and state code for each transaction (e.g., Monroe County, Indiana). To identify school districts, I follow Goldsmith-Pinkham et al. (2023) and use keywords such as "vocational," "technical," and "elementary," including common abbreviations, and manually match remaining issuers. Thus, a typical issuer name for a school district is "Monroe County Community School Corporation" in Indiana, or "Dallas Independent School District" in Texas. I include general obligation (GO) and school district bonds in my main sample to focus on the direct role of property tax revenue.[12]

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 interest earned from the bond. Babina et al. (2021) shows that such exemptions result in market fragmentation and limit risk-sharing across states.

[12] I use revenue bonds as a placebo test to show that bonds that are less tied to property taxes show no changes in yield spreads and issuance amount. 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 the floor are more pronounced for bonds directly linked to property taxes, such as GO and school district bonds.

Fleckenstein and Longstaff (2023) finds that local investors are willing to pay a premium for this exemption. To account for this exemption, I follow Schwert (2017) and 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.[13] 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, 2023; Gustafson et al., 2023). I show that the findings are robust to alternative measures of yield spreads, including yield spreads without the tax adjustment, the 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, statewide adoption of regulations 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 floor 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.

Property Tax Data. I use property tax revenue data from the Census Bureau's Annual Survey of State and Local Government Finances. Pierson et al. (2015) offers a single and comprehensive database of local government revenue and expenditure. I use county and school district-level data, provided from 1967 to 2022.[14] This data includes detailed information on both tax and non-tax revenue, intergovernmental transfers, and expenditure. Furthermore, I obtain the mill rate for each county and school district from the Lincoln Institute of Land Policy. I use the headline mill

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

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

rate provided by each municipality. Figure A.3 in the Appendix shows an example of the data for the state of Alabama. In each state and year, county and district provide a headline mill rate. exploits county and district-level variation in house prices, this is the preferred proxy for local supply constraints.

Educational Outcomes. I obtain school 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 on 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). This data also 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 agency identifier (i.e., LEAID) and school year.

Sample Construction. 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 county- and school district-level property tax revenue and spending. Finally, for the school district-level data, I merge it with school 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 property tax revenue and spending, school district-level educational outcomes, and statewide floor regulations. The Appendix Table A.1 shows the number of observations remaining after each step of cleaning the data.

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 $Spread_{bit}$, averages 143 bps and stays positive across all time. There are two implications from Figure 2. First, municipal bond yields are always higher than the risk-free rate. Second, there is cross-sectional dispersion in municipal bond yield spreads every year, as shown by the light blue shade. Local governments face different amounts of risk in municipal bonds. 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 describes property tax revenue. Property taxes are the largest source of tax revenue for the local governments in my sample, making up 32% of total tax revenue on average. County property tax revenue averages $46.3 million per year and school district revenue averages $14.3 million. As further presented in the Appendix Table A.3, property tax revenue per capita averages $3,222. Counties bear more of this downside than school districts. The three-year rolling volatility of county property tax revenue averages $2,069, more than three times the $632 for school districts, yet counties receive no comparable protection.

Lastly, Panel C of Table 1 reports school district and county spending, scaled by enrollment per pupil (pp) and per capita, respectively. On the school district side, total education spending averages $10,288 pp, and the average dropout rate is 4.4%. For counties, capital outlays, financed mainly through borrowing and thus most exposed to the decline in county bond issuance, average $141 per capita, whereas operational outlays show $1,134 per capita. Operational spending includes highway maintenance, parks and recreation, general public buildings, and libraries.

4 Empirical Designs and Results

4.1 Effects of Floor on Municipal Bonds

To investigate how the statewide floor regulations affect municipal bonds issued by protected and unprotected municipalities, I employ a triple-difference estimation as follows:

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

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

The treatment Floor_{st} is a dummy variable that equals one if a state s has imposed a floor in year t or earlier, and zero otherwise. The floor limits decline in property tax revenue of school districts, but not of counties. So, I add a treatment School_i, which is a dummy variable that equals one if an issuer i is a school district, and zero if it is a county in which the school district is located. School district is nested within counties, and using the NCES's School District Geographic Relationship Files, I link each school district to the county in which it is located. I then difference out local shocks that a school district and its county commonly face by adding issuer fixed effect μ_i, and difference out common trends that all issuers in floor and non-floor states face by controlling for year fixed effect δ_t. The issuer fixed effect absorbs the main effect of School_i. Standard errors are clustered at the state level to account for within-state correlation as floor enactment occurs at the state level.

Triple-difference identification in equation (2) compares the changes in the difference between school district bonds and county bonds in floor states before and after the statewide adoption of the floor with the changes in the difference between school district bonds and county bonds in non-floor states over the same time period. For example, I compare the changes in the difference between Warren school district bonds and Warren county bonds around Kentucky's floor adoption with school district bonds and county bonds in non-floor states over the same period. The specification controls for issuer-level characteristics (C_{it}), including changes in local income, employment, population, and GDP and municipal bond characteristics (Z_{bit}) including the remaining

time to maturity, credit ratings, trading volume relative to the outstanding amount, and liquidity risk measured with Amihud (2002)'s price impact, following prior studies (e.g., Acharya et al. (2022); Goldsmith-Pinkham et al. (2023)).

My first main result is that school districts in floor states face lower borrowing costs than counties in which school districts are located and school districts in non-floor states. As shown in Panel A in Table 2, school districts in floor states show lower bond yield spreads by 13 basis points, which 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.[15] Also, school districts in floor states increase bond issuance by 3.8% compared to counties in which these school districts are located and school districts in non-floor states.

The triple-difference approach in equation (2) supports the parallel trend assumption. Panel A of Figure 3 and Figure 4 plot event-study estimates of the average treatment effect on treated issuers at each horizon for bond yield spreads and issuance, respectively, spanning five years before and five years after treatment. There is no meaningful difference in yield spreads and issuance amount of bonds across floor and non-floor states prior to the treatment, which supports a causal interpretation of the results. Following the treatment, school district bond yield spreads decline persistently over time, falling by 6 bps in the first year and by an additional 5 bps over the subsequent five years.

My second main result is that the floor lowers borrowing costs for school districts with low income and low state aid more than for school districts with high income and high state aid. I split school districts into the lowest, middle, and highest tercile of income. Panel A of Figure 5 reports that bonds issued by the lowest-income school districts show a 33 bp decline in yield spreads, compared with an 18 bp decline for bonds issued by middle-income school districts, and a 4 bp decline for bonds issued by the highest-income school districts. I then split school districts into

[15] For example, Cornaggia et al. (2024) shows a 15 bps change in hospital bond spreads due to remote product delivery. Cornaggia et al. (2018) shows a 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.

the lowest, middle, and highest tercile of state transfers instead of income. State transfers are the equalization grants and categorical aid that the school district's state provides to the school district. Panel B of Figure 5 reports that bonds issued by school districts that receive the lowest state aid show a 27 bp decline in yield spreads. School districts that receive medium and high levels of state aid show declines of 14 bps and 9 bps, respectively. The floor is more beneficial for school districts with low income and low state aid, which are the school districts that were facing larger downside risk.

So far, I compared school districts versus counties across floor and non-floor states by using a triple-difference estimation. A simpler estimation would be to compare school districts in floor states with school districts in non-floor states, and to compare counties in floor states with counties in non-floor states. This would be a staggered DiD approach estimated with either school district data only, or with county data only, 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, issuer i, state s, and year t. I run two tests with this specification. I first include school district bonds only, and then I include county bonds only. Outcome variables are Spread_{bit} and $\log(\text{Issuance}_{bit})$. Spread_{bit} is the yield spread of municipal bond b issued by 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 issuer i in year t. The treatment Floor_{st} is a dummy variable that equals one if a state s has imposed a floor in year t or earlier, and zero otherwise. I add issuer fixed effect μ_i and year fixed effect δ_t. Standard errors are clustered at the state level to account for within-state correlation as floor enactment occurs at the state level. As described earlier, I include control variables related to issuers (C_{it}) and municipal bonds (Z_{bit}).

I use this approach in equation (3) to disentangle the net borrowing costs estimated in equation

(2) into changes in school district borrowing costs and county borrowing costs. The purpose of disentangling these effects is to calculate the present value of net interest by comparing the interest savings for school districts with interest costs for counties.

Using a staggered DiD approach in equation (3), I first show that school districts in floor states face lower borrowing costs than school districts in non-floor states. As shown in Panel B in Table 2, school district bonds carry yield spreads that are 7 bps lower in floor states than in non-floor states. School districts increase bond issuance by 3% in floor states compared to non-floor states. I then show that counties in floor states face higher borrowing costs than counties in non-floor states. As presented in Panel C in Table 2, county bond yield spreads increase by 5 bps and bond issuance declines by 2.5% in floor states compared to county bonds in non-floor states. Again, the parallel trend assumptions hold in Panels B and C of Figures 3 and 4. Also, after the enactment, county bond yield spreads increase and county bond issuance decreases steadily.

Figure A.2 illustrates Dallas as an example showing how the floor affects borrowing costs of school districts and counties. This figure maps Dallas County and the fourteen school districts located within the county. These municipalities overlap geographically and draw property tax revenue from the same local tax base. But, only school districts are protected by the floor. Therefore, following the enactment of the floor, district bond yield spreads decline by between 1 and 15 bps. On the other hand, bond yield spreads increase by 7 bps for Dallas County following the enactment of the floor.

I calculate the net savings by comparing the present value of interest savings for school districts and interest costs for counties. School districts and counties issued an average of $55 million and $66 million in municipal bonds per year, respectively, from 2000 to 2022. My estimates imply that the floor leads to 7 bps lower yields on the average dollar of school district borrowing and 5 bps higher yields on the average dollar of county borrowing. Assuming municipal bonds will be outstanding until maturity, the lower yields on the $55 million issued by school districts with an average duration of 10 years lead to interest savings of $391,000 ($\approx$ \$55 million $\times$ 0.0007 $\times$ 10). The

higher yields on the \$66 million issued by counties lead to costs of \$330,000 ($\approx$ \$66 million $\times$ 0.0005 $\times$ 10). This calculation follows Gao and Murphy (2022) and is based on the intuition that duration is the scalar characterizing price changes in response to changes in yield. If I instead assume all bonds will be called on the first call date following Garrett and Ivanov (2024), the average duration is 6 years. Results then suggest that there are interest savings of \$234,000 for school districts and additional costs of \$198,000 for counties. In aggregate, the interest savings realized by school districts exceed the additional interest costs borne by counties by 18%.

Lastly, I sort school districts into terciles based on the gap between their effective mill rate and the statutory minimum rate. A potential concern with the previous estimations is that the floor may bind only when tax bases decline and mill rates approach the statutory minimum. To address this concern, I examine whether the effect varies with the proximity of the mill rate to the floor as follows:

$$y_{bit} = \beta \left(\text{Floor}_{st} \times \text{Distance}_i\right) + \pi_1 \text{Floor}_{st} + \pi_2 C_{it} + \pi_3 Z_{bit} + \mu_i + \delta_t + \varepsilon_{st}, \tag{4}$$

for municipal bond b, school district i, state s, and year t. Here, I include bonds issued by school districts only. I measure Distance_i as the gap between a school district i's effective tax rate and the statutory minimum tax rate, and assign school districts to terciles of Distance_i. There are three indicators for Distance_i, which are Near_i, Mid_i, and Far_i, and they equal one if a school district i belongs to the lowest, middle, and highest tercile, respectively, and zero otherwise.

Table 3 reports the effects of the floor on school district borrowing costs by the distance between the school district's effective tax rate and the floor rate, estimated using equation (4). There are two main findings. First, the decrease in municipal bond yield spreads and the increase in municipal bond issuance are more pronounced for school districts in which the tax rates are closer to the floor. Second, the floor reduces borrowing costs even for school districts whose tax rates are above the floor rate, which are those in the farthest tercile. This result suggests that the floor is a downside buffer and provides protection ex ante before the actual default risk is realized. Specifically, school

districts in the tercile nearest to the floor show an 11 bp decline in bond yield spreads, which is 1.6 times the average effect of 7 bps reported in Panel B of Table 2. School districts in the middle tercile show a 6 bp decline in bond yield spreads. School districts in the farthest tercile show a 5 bp decline in bond yield spreads. Moreover, bond issuance increases by 7.8% for school districts nearest to the floor, by 3.9% for the middle tercile, and by 1.5% for the tercile farthest from the floor. In the nearest school districts, the bond issuance increases by 2.7 times the average of 2.9% reported in Panel B of Table 2.

4.2 Effects of Floor on Property Taxes

Goldsmith-Pinkham et al. (2023) uses a theoretical model and shows that the volatility of future cash flows increases municipal bond yields by holding underlying asset values constant. In my paper, I show that the floor lowers the volatility of property tax revenue of school districts without reducing the level of this revenue below the floor. I use a triple-difference approach in equation (2) with issuer-level property tax outcomes and issuer-level control variables.[16] There are two outcome variables on property taxes, the levels and volatility of property tax revenue. First, I use the logarithm of property tax revenue, i.e., $\log(\text{Property Tax}_{it})$. Then, 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. This triple-difference estimation compares the changes in differences between school district property tax revenue and county property tax revenue in floor states before and after the statewide adoption of the floor with the changes in differences between school district property tax revenue and county property tax revenue in non-floor states over the same time period.

Using this triple-difference estimation, I first show that the floor lowers volatility of school district property tax revenue without reducing the level of this revenue below the floor. As shown in column (1) in Panel A in Table 4, school districts in floor states do not experience a decline in

[16] Because this estimation is at the issuer level, I exclude bond-level controls from equation (2).

the level of property tax revenue compared to school districts in non-floor states and counties in which school districts are located. In fact, it increases by 2% at the 10% significance level. Panel A in Figure 6 shows that post treatment, property tax revenue does not decline in school districts in floor states. Like other outcome variables, the parallel trend assumption holds. 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 6, property tax volatility shows an immediate effect of 5% decline in the first year, and keeps declining five years after the treatment.

I then show that the floor reduces property tax revenue volatility, particularly for school districts in which market prices of local real estate are more volatile. When local real estate prices become more volatile, the floor reduces the volatility of property tax revenue generated from these properties. Real estate properties can be classified into two categories, residential and commercial. Residential properties include single-family homes, condominiums, townhouses, and apartments, while commercial properties include non-residential properties such as offices, retail buildings, and industrial properties. As shown in Appendix Figure A.5, residential properties account for 71% of the tax base on average across counties and school districts. To examine whether the effect of the floor varies with local real estate price volatility, I estimate the following triple-difference specification:

$$y_{it} = \beta \left(\text{Floor}_{st} \times \sigma_i^{\mathbf{RE}} \right) + \pi_1 \text{Floor}_{st} + \pi_2 C_{it} + \mu_i + \delta_t + \varepsilon_{st}, \tag{5}$$

for school district i, state s, and year t. I include school districts only as issuers in this sample. The outcome variable y_{it} is 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. A new treatment σ_i^{RE} equals one if the school district i's local real estate price volatility is above the median, and zero otherwise. This volatility is measured as the standard deviation of local real estate prices. This identification compares changes in property tax revenue

volatility between high- and low-real-estate-price-volatility school districts in floor states before and after the enactment of the floor, relative to the corresponding difference between high- and low-volatility school districts in non-floor states.

The purpose of this analysis is to show that the floor stabilizes property tax revenue in school districts where local real estate prices are more volatile. Panel B of Table 4 presents the results using local residential and commercial real estate price volatility in equation (5). School districts with more volatile market prices of residential and commercial real estate experience larger declines in property tax revenue volatility following the adoption of the floor. Specifically, property tax revenue volatility declines by 14% in school districts where residential real estate price volatility is above the median. Similarly, property tax revenue volatility declines by 5% in school districts where commercial real estate price volatility is above the median.

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. I have already shown that the parallel trend assumption holds for municipal bonds and property taxes. In this subsection, I provide three pieces of additional empirical evidence to support this ultimately untestable assumption.

First, I verify that statewide economic, demographic, and budget characteristics have limited predictability on the adoption timing of statewide floor regulations. I show this by testing a bivariate regression as follows:

$$\text{Floor}_{st} = \alpha + \beta X_{s,t-1} + \delta_t + \epsilon_{st}, \tag{6}$$

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. I include house price level and volatility, income of residents, population, state and presidential election

results, home ownership and poverty rates. Presidential election results are measured as statewide vote shares of Democratic and Republican presidential candidates. In addition, I include state tax and spending, which includes share of residential property taxes in total property tax revenue, mill rates, assessed values of properties, and state transfers to local governments. δ_t is a year fixed effect. The coefficient of interest, β, measures whether and how much lagged state characteristics predict the statewide enactment of floor regulations.

Figure 7 plots the estimated coefficients β from bivariate regressions of a floor on a range of observable state characteristics using equation (6). Panels A and B present state economic and demographic characteristics, and state tax and spending. In all specifications, the 95% confidence intervals span zero. The coefficients are statistically insignificant and economically negligible. House prices, income, election results, and other economic characteristics have limited predictability on the statewide enactment of floors. Moreover, sales and income tax revenue, mill rates, assessed values, and state transfers do not meaningfully predict the statewide enactment of the floor. These tests show that there is no systematic relationship between observable statewide characteristics and enactment timing, which supports the causal interpretation of my results.

Second, I show that pre-existing differences between states that never adopted the floor and states that would adopt the floor in the future do not predict declines in borrowing costs. I test this by assigning a falsified treatment to not-yet-treated states in the years before they truly adopt the floor regulation, and comparing them to never-treated states. For illustration, consider two states A and B. State A truly adopts the floor regulation in year t_A and is thus untreated in all years before t_A. State B never adopts the floor. We then assign state A a falsified treatment date of year $t_A - h$ even though state A is not yet treated until year t_A. When $h = 5$, for example, state A is falsely coded as treated in years $t - 5$, $t - 4$, $\ldots$ and $t - 1$. I compare states A and B over this window to test if there are meaningful pre-existing differences that drive changes in municipal bond yields and issuance.

Formally, my regression is as follows:

$$y_{bit} = \beta \left(\text{False Floor}_{s,t}^{h} \right) + \pi_1 C_{it} + \pi_2 Z_{bit} + \mu_i + \delta_t + \varepsilon_{st}, \tag{7}$$

where

$$\text{False Floor}_{s,t}^{h} = \begin{cases} 1 & \text{if } t_s - h \leq t \leq t_s - 1, \\ 0 & \text{if } t < t_s - h, \\ 0 & \text{if state } s \text{ never adopts}, \end{cases} \tag{8}$$

where b indexes municipal bonds, i indexes issuers, t denotes the observation year, t_s is the true adoption year of the floor regulation in state s, and h is the number of years by which the falsification adoption date precedes the true adoption year. I falsely code an adopting state as treated in the h years before its true adoption year t_s. I code never-treated states as zero in every year. I exclude all years after the true adoption year, so that I can compare the pre-existing differences between not-yet-treated and never-treated states.

To begin with, I test equation (7) by including bonds issued by school districts only. The coefficient of interest, β, measures whether there are pre-existing differences between school districts in not-yet-treated states and school districts in never-treated states that drive a decline in school district borrowing costs. If there are such differences, the coefficient β will be significantly negative for school district bond yield spreads and significantly positive for school district bond issuance. However, the results presented in Panel A in Table 8 show that β is small and insignificant, even slightly positive for bond yield spreads, and even slightly negative for bond issuance. There is no meaningful difference in pre-existing characteristics that are driving the decline in municipal borrowing costs, which suggests that the measure is accurately identified.

I then test equation (7) by including bonds issued by counties only. If there are pre-existing differences between counties in not-yet-treated states and counties in never-treated states that result in an increase in county borrowing costs, β should be significantly positive for county bond yield

spreads and significantly negative for county bond issuance. However, Panel B in Table 8 rules out this possibility by showing statistically insignificant and economically negligible coefficients for both county bond yield spreads and issuance. I repeat this falsification test when h is one, two, and five years. Falsified treatment dates set one, two, and five years ahead of the real treatment all indicate that pre-existing differences do not meaningfully predict a decline in school district borrowing costs and an increase in county borrowing costs. These tests reinforce that the floor is accurately identified.

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. In my main results, I use GO and school district municipal bonds because they are directly backed by the property tax revenue of the issuing municipality. Revenue bonds, on the other hand, are secured by revenues generated by specific projects, such as highway tolls, airport fees, and water and sewer charges. Revenue bonds are less directly related to property tax revenue. If my estimates are driven by the floor, then the effects on revenue bond yield spreads and issuance should be small. Table 9 indeed shows this. I find that borrowing costs for revenue bonds in floor states are not significantly different from those for revenue bonds issued in non-floor states. Specifically, the floor does not decrease revenue bond yield spreads, as shown by the positive and statistically insignificant coefficient estimate. Also, the floor does not increase revenue bond issuance, as the coefficient is again statistically indistinguishable from zero. My evidence suggests that the difference between borrowing costs for school district bonds in floor and non-floor states is due to the difference in floor policies, rather than other unobserved state characteristics.

4.4 Robustness Checks

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

First, I exclude economic recessions from the main analysis. Municipal bonds are often viewed

as safe assets and attract investors during periods of heightened uncertainty. One might be worried that the floor effects are centered around economic recessions only. To alleviate this concern, I exclude the Global Financial Crisis and the Covid-19 pandemic from the analysis and find robust results. As shown in Table 10, 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.

Second, credit ratings can be viewed as bad controls (Iliev and Vitanova, 2024) because credit rating agencies construct ratings from many of the same local fiscal fundamentals that also determine municipal bond yields. Thus, Iliev and Vitanova (2024) caution that credit ratings are co-determined outcomes that partly reflect the variation researchers aim to study, rather than clean controls. Any portion of the effect of the floor that is also reflected in credit ratings can be absorbed by the rating control, which in turn attenuates the estimated floor coefficient. For this reason, excluding the credit rating control should produce a larger estimated decline in spreads. Table 11 presents these results. I exclude the credit rating control from the triple-difference specification in equation (2) and find a larger estimated decline in school district bond yield spreads of 20 bps, compared with 13 bps in the baseline specification.

Third, I use alternative measures for municipal bond yields. 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.[17] Second, there are six states that do not provide statewide income-tax exemptions on GO bond interest earned by in-state investors.[18] 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 that are otherwise similar but impose income taxes and provide tax exemptions for municipal bonds.

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

[18]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.

To mitigate these concerns, I use three alternative measures of municipal bond yield spreads in testing equations (2) and (3). First, I use yields minus maturity-matched risk-free rate, in which the risk-free rate is provided by Liu and Wu (2021), without the tax adjustment. Second, I use nominal yields minus maturity-matched risk-free rate, in which the risk-free rate is provided by Gürkaynak et al. (2007), without the tax adjustment. Third, I present results based on the different maturities of municipal bonds. Table 12 shows the effects of the floor on municipal bond yield spreads estimated with equations (2) and (3) using the first two alternative measures. The estimates are consistent with the baseline results, showing declines in bond yield spreads ranging from 8 to 11 bps, compared to a 13 bps decline in the baseline results. For the maturities, 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, which is consistent with prior studies (Gao and Murphy, 2022; Gustafson et al., 2023; Painter, 2020).[19] This maturity gradient suggests that the policy operates not through short-run rollover risk but through mitigating downside exposure to property tax revenue.

5 Real Effects of Floor on School Districts and Counties

So far, I have shown that the floor decreases school district borrowing costs, whereas it increases county borrowing costs. In this section, I show whether and how much the floor affects real outcomes of school districts and counties, particularly focusing on student engagement and instruction compensation for school districts and capital and operational spending for counties.

[19] Painter (2020) and Goldsmith-Pinkham et al. (2023) show that spreads of long-term municipal bonds are larger than those of short-term bonds due to sea-level rise. Gustafson et al. (2023) also show this effect using migration induced by the Covid-19 pandemic.

5.1 Effects of Floor on Human Capital Investment in School Districts

I measure human capital investment with three sets of outcome variables: (i) student engagement measured by student dropout rate and student-teacher ratio, (ii) instruction compensation measured by salaries and employee benefits, and (iii) school district spending on capital and operational outlays per pupil. I focus on student engagement to assess whether the floor leads to wasteful spending or instead facilitates investment in human capital. If all of this spending was entirely wasteful, then the floor would not improve student engagement. Instead, if the floor enhances student engagement, I use this result to support the interpretation that the protection has real effects on investing in human capital rather than encouraging wasteful spending. To test this, I use a staggered DiD approach in equation (3) with school district-level educational outcomes and control variables. This specification includes school districts only and excludes counties because the educational outcomes are at the school district level.[20]

My first result is that the floor increases capital and operational spending as presented in Panel A of Table 5. On the capital spending side, school districts in floor states increase infrastructure spending by 5% per pupil (pp). Spending on infrastructure includes the construction of new buildings, major renovations and remodeling, and facility repairs. On the operational spending side, total salaries increase by 1.6% pp, with instruction salaries increasing by 3% pp, and employee benefits increasing by 4% pp in floor states, as shown in Panel B of Table 5. Total salaries include support staff and administration salaries. Employee benefits include non-salary compensation such as health and unemployment insurance and retirement and pension contributions. Panel A of Figure 8 plots event studies on the effects of instruction salaries. School districts in floor states show a steady increase in instruction salaries per pupil from 2 to 6% after the adoption of the floor compared to the school districts in non-floor states.

My second result is that the floor improves student engagement, particularly reducing student dropout rate and average class size. As shown in Panel C of Table 5, student dropout rates decline

[20] Because this estimation is at the school district level, I exclude bond-level controls from equation (3).

by 0.38 percentage points in school districts in floor states, which is an 8.6% decline compared to the average of 4.4%. Panel B in Figure 8 shows that the parallel trend assumption holds, and the student dropout rate declines over the long run.[21] Naturally, the student completion rate increases by 0.42 percentage points, as shown in column (2) in Panel C of Table 5, which is similar in magnitude to the decline in dropout rates. Lastly, the student-teacher ratio decreases by 4% compared to the average ratio of 30, implying a smaller class size and an improvement in student engagement (Hanushek, 1997).

My third result is that the improvements in student engagement are more pronounced in low-income school districts and in school districts that receive fewer equalization grants and categorical aid from the state transfers. As shown in Panel A of Table 6, student dropout rates decline by 1.1 percentage points in low-income school districts, compared with a 0.22 percentage point decline in high-income school districts. Similarly, dropout rates decline by 1.2 percentage points in school districts receiving lower levels of state aid, compared with a 0.305 percentage point decline in districts receiving higher levels of state aid. As further shown in Panel B, class size decreases by a larger amount in low-income and low-aid school districts. The student-teacher ratio declines by 4 students in low-income school districts, compared to the average of thirty students per teacher. The number of teachers increases by 1.7% in floor states compared to non-floor states, as shown in Panel B of Table 5. The floor is valuable for school districts with limited access to equalization grants and foundation aid. These results suggest that revenue protection is beneficial for school districts with limited financial resources, and when these school districts gain secure protection over their revenue, their incentives to invest in the future, including in their students, increase.

5.2 Effects of Floor on County Spending

Counties support school districts by absorbing the decline in total property tax revenue in floor states, whereas counties are not obligated to support school districts in non-floor states. I have

[21] This long-term effect is consistent with Lafortune et al. (2018) who shows that state transfers improve student test scores in low-income school districts over the long run.

already shown that the floor increases county bond yield spreads by 5 bps and decreases county bond issuance by 2.5%. In this subsection, I further study whether and how much the floor alters county spending.

It is ex ante ambiguous whether and how much the floor affects county spending. On the one hand, two forces predict that counties cut spending. First, counties absorb a larger share of the decline in the local property tax base, so they collect less own-source revenue, and cut spending in real time (Dambra and Gustafson, 2025). Second, the higher borrowing costs and lower bond issuance documented above make capital financing scarcer and more expensive, which lowers capital spending because counties finance long-lived assets through debt. On the other hand, two forces could leave county spending unchanged. First, counties may benefit from state and federal transfers that offset the loss of own-source revenue. Second, the floor applies directly only to school districts, so its effect on counties operates indirectly through the shared tax base, so this second-order effect may be too small to cut spending. Whether, and how much, the floor reduces county spending is an empirical question.

To examine this question, I use a staggered DiD approach outlined in equation (3) including counties only. I compare county spending in floor states with county spending in non-floor states before and after the staggered, statewide adoption of the floor. Counties in non-floor states are valid counterfactuals for what a county's spending would be absent the obligation to support school districts. There are two types of county spending, capital and operational outlays. Capital outlays are used to build or acquire long-lived assets and infrastructure, such as constructing roads, public buildings, parks, and sewerage systems. Operational outlays are the recurring expenditures required to deliver public services and keep county operations running day to day, such as salaries, supplies, and routine maintenance. The distinction is the type of spending rather than the service: many county functions require both. A public park, for example, requires capital spending to acquire the land and install playgrounds and trails, and operational spending to run it, including groundskeeping, maintenance, and the salaries of parks staff.

Table 7 shows that counties in floor states cut spending on both capital and operational outlays. Column (1) shows that counties in floor states cut capital outlays by 6%, significant at the 1% level, while column (2) shows a 3.4% reduction in operational outlays, significant at the 5% level. The decline in capital outlays reflects the contraction in county borrowing. Capital spending, such as park land, playgrounds, and ballfields, finances long-lived assets that are often financed with bonds. The decline in operational spending reflects the revenue squeeze. With less property tax revenue to draw on, counties trim the recurring costs of keeping a park open, such as groundskeeping, maintenance, and parks staff. Spending on parks and recreation, which includes both types of outlays, falls by 5.9%. Spending on highways falls by 6%, sewerage by 3.4%, and public buildings by 9.7%. Altogether, these results show that the floor's redistribution away from counties manifests as cuts to both capital investment in and operation of county public services.

6 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 find that the school district bond yield spreads decrease by 13 bps and bond issuance increases by 4% in floor states compared to school district bonds in non-floor states and counties in all states. I further find that the floor increases teacher salaries, and decreases student dropout rates and class size. These improvements in educational outcomes support the interpretation that the revenue protection translates into human capital investment. Importantly, these better educational outcomes come at a cost. A floor that prevents declines in school district property tax revenue has an adverse effect on the counties because it decreases county property tax revenue and increases county borrowing costs. I find that the floor increases county bond yield spreads by an average of 5 bps. Protection that enhances student achievement and the teaching environment comes at the expense of counties.

While my study focuses on the floor, there are many regulations on local property tax collec-

tions. 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, affect municipal bonds.

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7 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. The 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 the institutional background in detail.

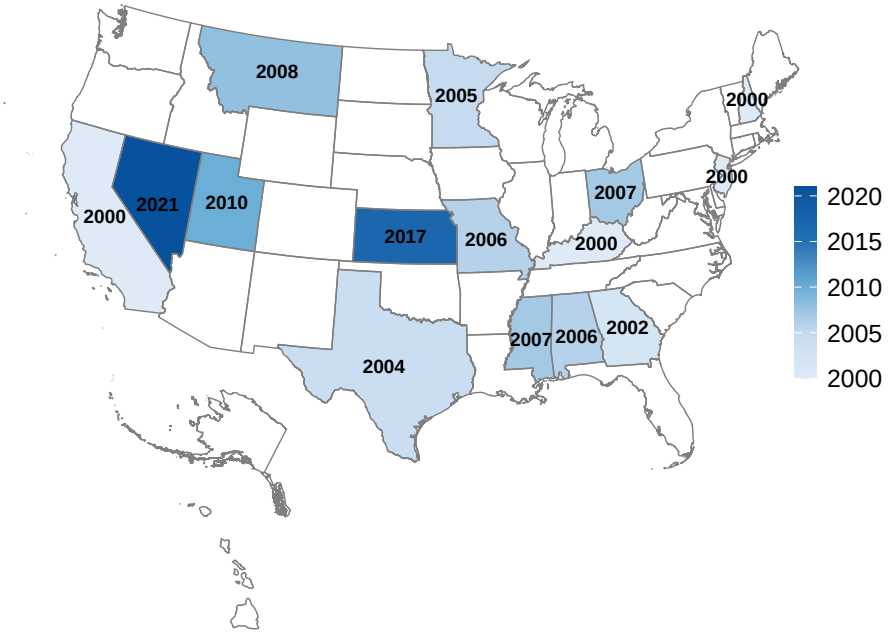


Figure 2: **Average Municipal Bond Yield Spreads**
This figure shows the average and cross-sectional interquartile range of municipal bond yield spreads from January 2000 to December 2022. The yield spreads are measured as tax-adjusted yield minus maturity-matched Treasury rate, as presented in equation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

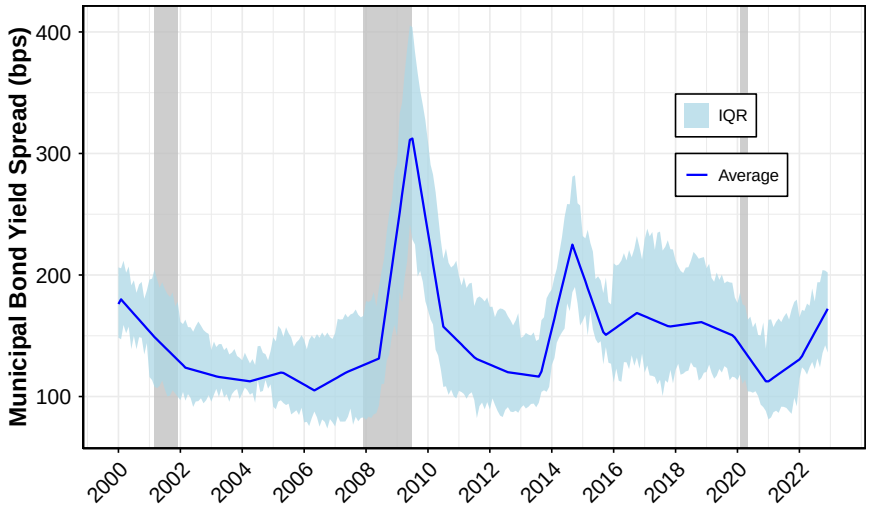


Figure 3: **Effect of Floor on Municipal Bond Yield Spreads**
This figure plots difference-in-differences estimates of the effect of the floor on municipal bond yield spreads, measured as tax-adjusted yield minus maturity-matched Treasury rate. The treatment Floor_{st} equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. 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 in which school district is located. Panel A compares the changes in differences between school district bonds and county bonds in floor states before and after the statewide adoption of the floor to the changes in differences between school district bonds and county bonds in non-floor states over the same time period. Panel B includes school districts only and compares school district bonds in floor states to school district bonds in non-floor states before and after the statewide adoption of floor. Panel C includes counties only and compares county bonds in floor states to county bonds in non-floor states before and after the statewide adoption of floor. 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 the estimation.

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

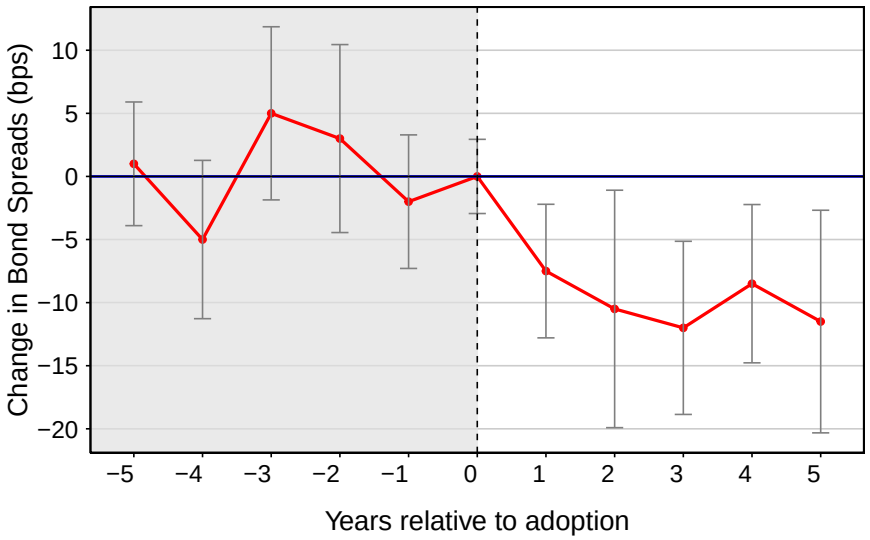
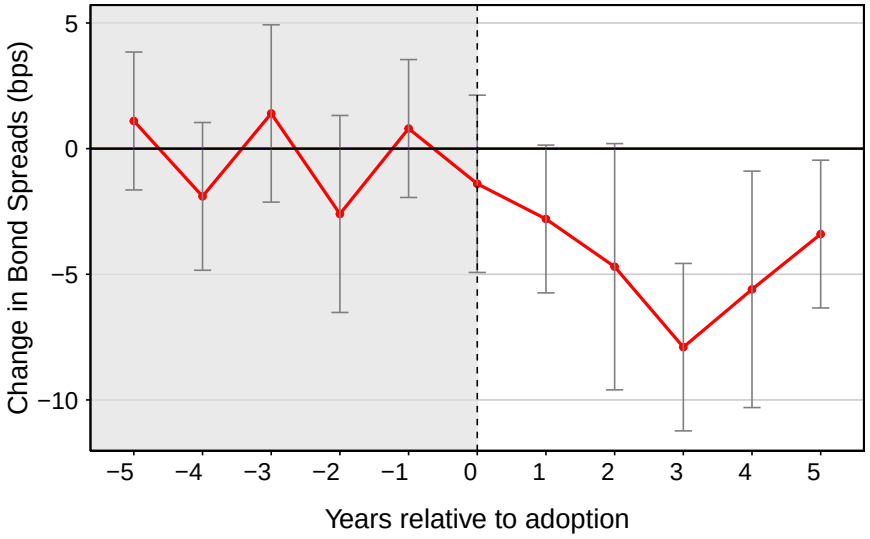


Figure 3: **Effect of Floor on Municipal Bond Yield Spreads (continued)**

Panel B. School District Bond Yield Spreads



Panel C. County Bond Yield Spreads

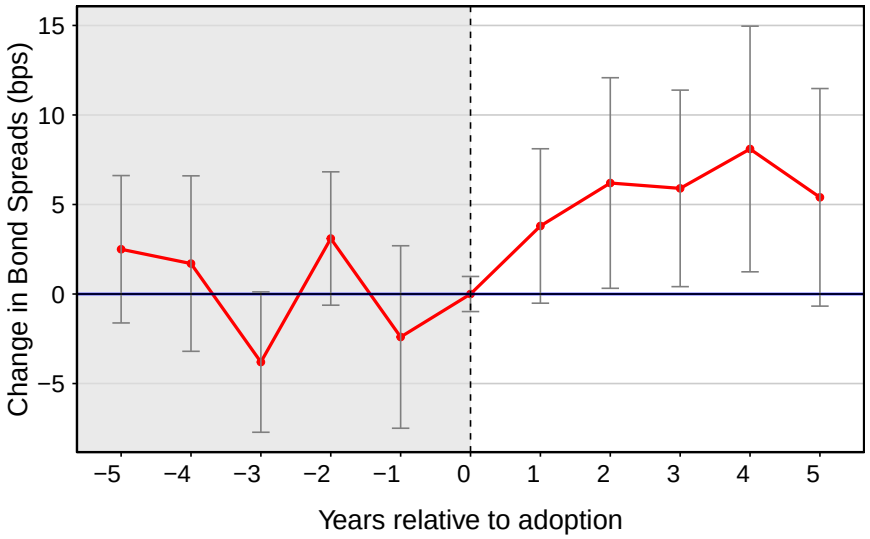


Figure 4: **Effect of Floor on Municipal Bond Issuance**
This figure plots difference-in-differences estimates of the effect of the floor on municipal bond issuance, measured as the logarithm of bond issuance amount in dollars. The treatment Floor_{st} equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. 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 in which school district is located. Panel A compares the changes in differences between school district bonds and county bonds in floor states before and after the statewide adoption of the floor to the changes in differences between school district bonds and county bonds in non-floor states over the same time period. Panel B includes school districts only and compares school district bonds in floor states to school district bonds in non-floor states before and after the statewide adoption of floor. Panel C includes counties only and compares county bonds in floor states to county bonds in non-floor states before and after the statewide adoption of floor. 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 the estimation.

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

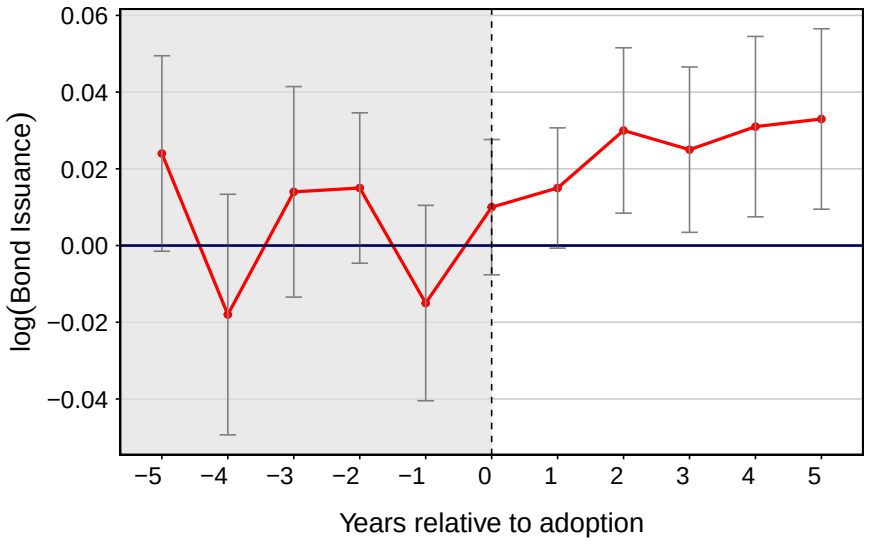
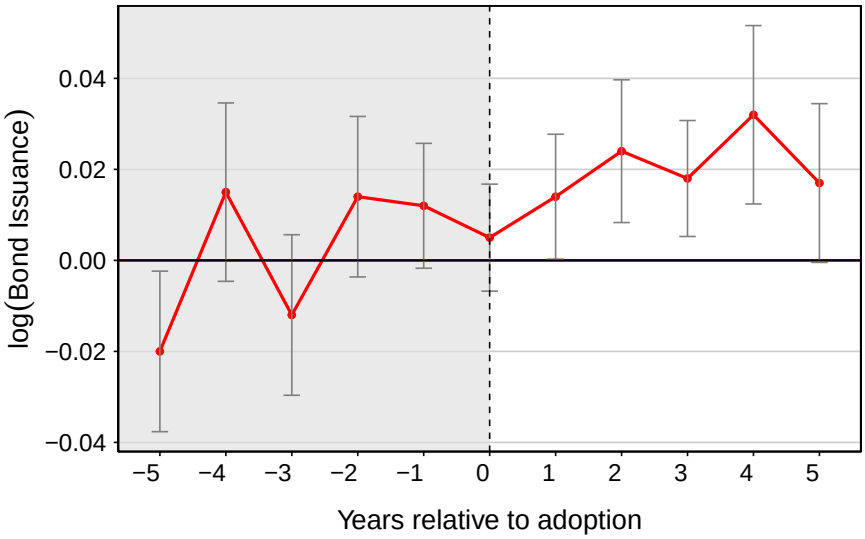


Figure 4: **Effect of Floor on Municipal Bond Issuance (continued)**

Panel B. School District Bond Issuance



Panel C. County Bond Issuance

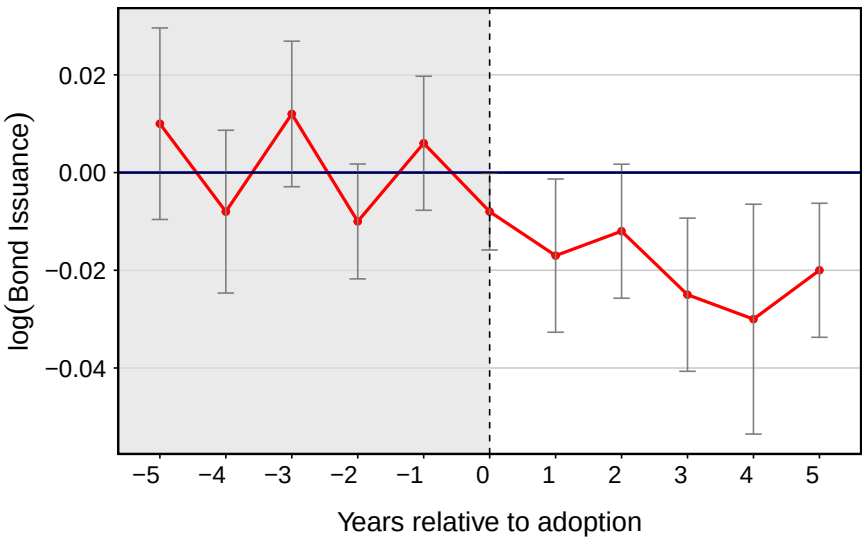


Figure 5: **Effect of Floor on Municipal Bond Yield Spreads by Heterogeneity**
This figure presents the effect of the floor on municipal bond yield spreads by the characteristics of school districts and bond maturity. I include school districts only in the sample. Panel A presents the effect of the floor on school district bond yield spreads by terciles of school district income. Panel B presents the effect of the floor on school district bond yield spreads by terciles of state transfers to the school district. These transfers are categorical aid and equalization grants. Panel C presents the effect of the floor on school district bond yield spreads by bond maturity. Short-, medium-, and long-term bonds are defined as having maturities of 1–4 years, 5–9 years, and 10 or more years, respectively. Lastly, Panel D presents the effect of the floor on school district bond yield spreads by terciles of the reliance on property tax revenue, measured as the share of property tax revenue in total tax revenue.

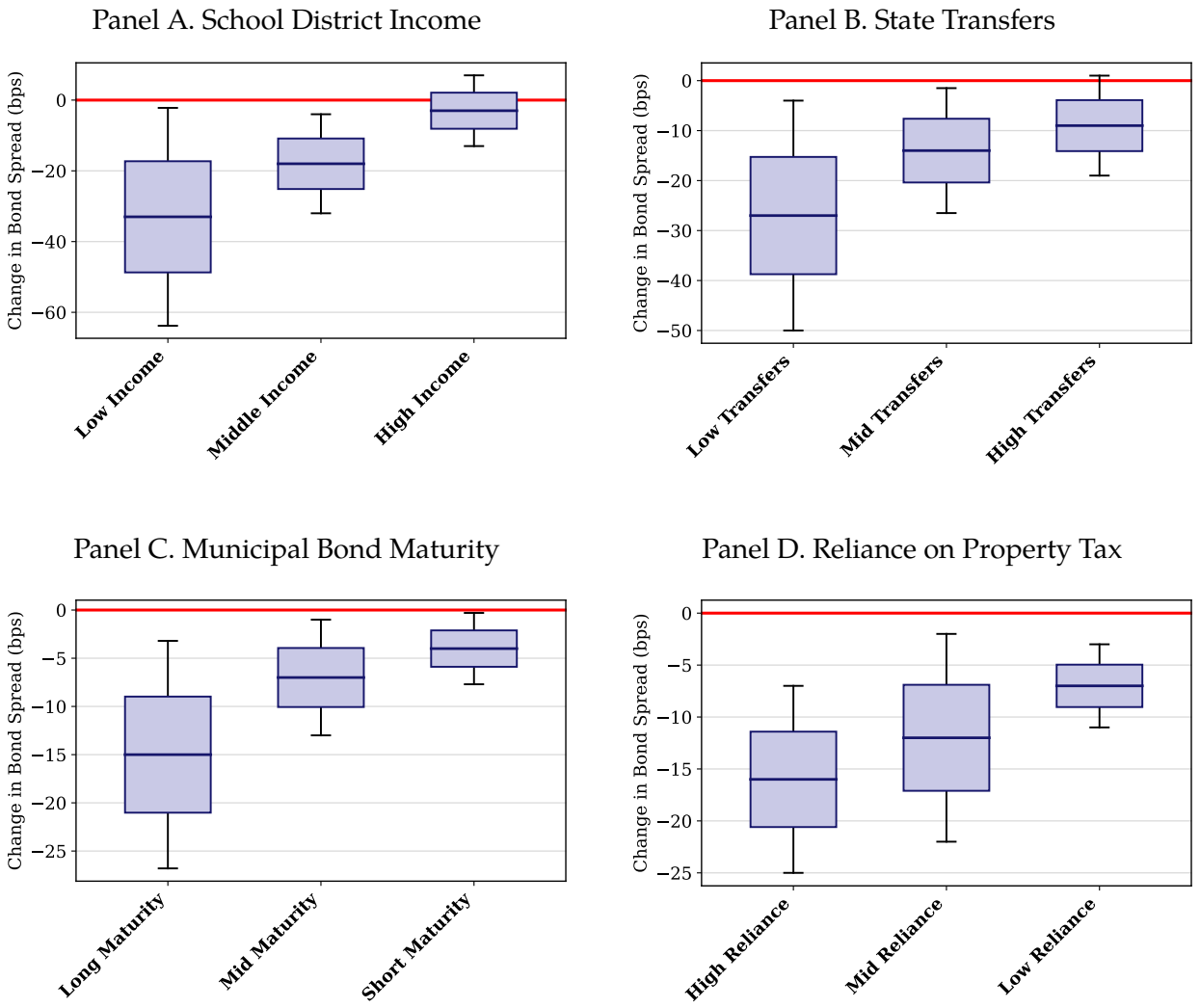
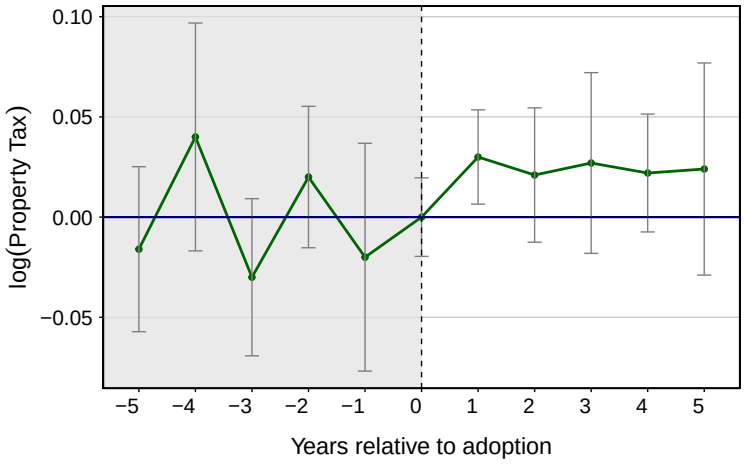


Figure 6: **Effect of Floor on Property Tax Revenue**
This figure plots difference-in-differences estimates of the effect of the floor on property tax revenue. Panel A presents the effect of the floor on property tax revenue level, measured by the logarithm of property tax revenue. Panel B presents the effect of the floor on the property tax revenue volatility, measured by the logarithm of 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. There are two types of issuers, school districts and 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 in which school district is located. 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 the estimation.

Panel A. Property Tax Revenue Levels



Panel B. Property Tax Revenue Volatility

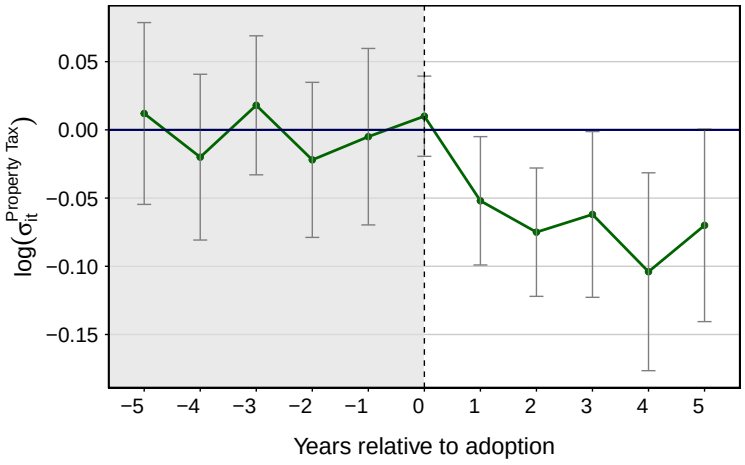


Figure 7: **Bivariate Regressions**
This figure plots the coefficients from bivariate regressions of floor enactment on the labeled state characteristics estimated from equation (6). The horizontal bars report 95% confidence intervals. The dependent variable, Floor_{st}, equals one if a state s imposes a floor that limits declines in property tax revenue in year t, and zero otherwise. The independent variables, 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.

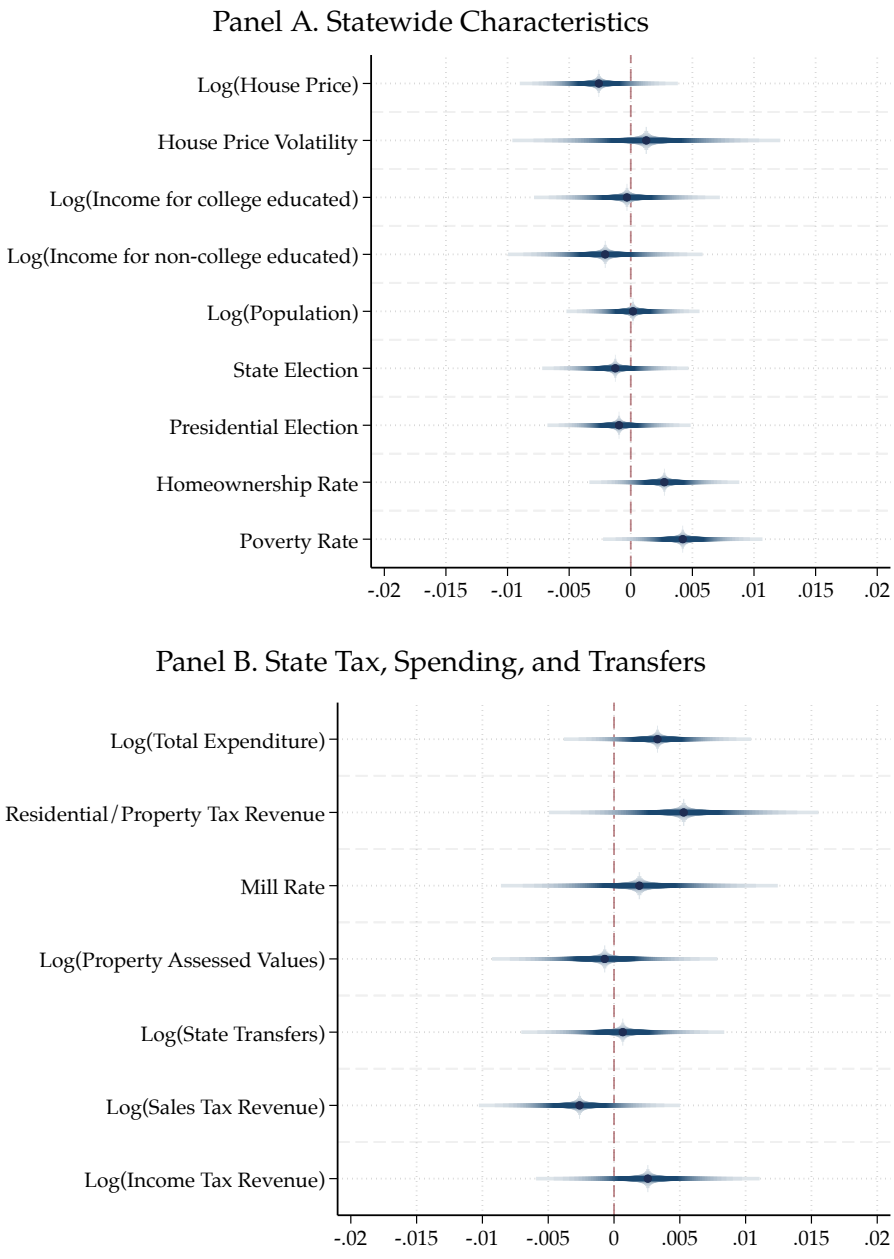
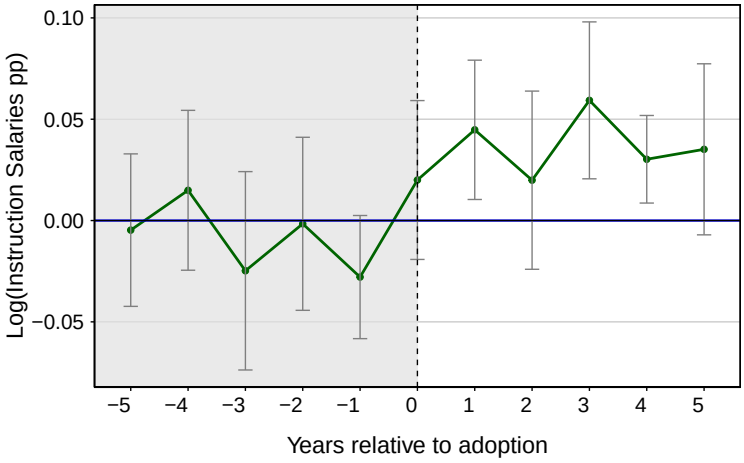


Figure 8: **Effect of Floor on Educational Outcomes**
This figure plots difference-in-differences estimates of the effect of the floor on school district-level educational outcomes. There is one type of issuer, school district. This estimation compares school districts in floor states to school districts in non-floor states before and after the floor enactment. 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 the effects of the floor on the logarithm of instruction salaries per pupil (pp). Panel B presents the effects of the floor on student dropout rates. Section 5.1 explains the estimation in detail.

Panel A. Instruction Salaries



Panel B. Student Dropout Rate

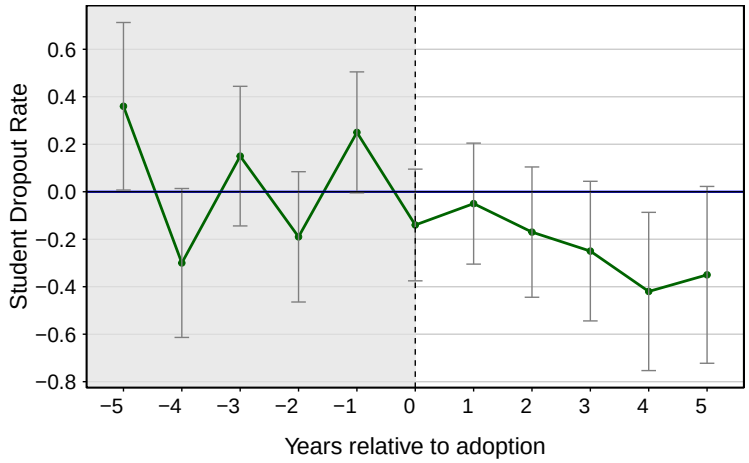


Table 1: **Descriptive Statistics**
This table reports mean, standard deviation, 1st, 25th, 75th, and 99th percentiles of key variables used in this study. Panel A presents municipal bond characteristics. Panel B presents property taxes. Panel C presents school district and county spending. Panel D presents the number of observations. 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 ($Mil.): School District	55.4	32.5	0.23	7.5	68.32	156.1
Issue amount ($Mil.): County	66.4	22.5	1.1	8,6	92.89	212.9
Months to maturity	144.85	83.49	25	82	192	374
Coupon rate (%)	4.10	1.09	1.05	3.40	5.11	6.20

Panel B. Property Tax

	Mean	**SD**	**1st**	**25th**	**75th**	**99th**
Property tax revenue: County (Mil. $)	46.31	187.98	0.33	3.51	28.28	683.44
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
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

Table 1: **Descriptive Statistics (continued)**

Panel C. School District and County Spending

	Mean	**SD**	**1st**	**25th**	**75th**	**99th**
School District Spending per pupil (pp)						
Total Education *pp* ($)	10,288	5,565	5,270	8,215	13,639	30,709
Instruction *pp* ($)	6,148	3,311	3,133	4,911	8,158	18,469
Support services *pp* ($)	3,638	2,369	1,636	2,804	4,947	12,055
School lunch *pp* ($)	1,390	980	110	770	1,980	3,790
Total salaries *pp* ($)	5,861	4,230	2,936	4,790	7,636	17,639
Instruction salaries *pp* ($)	3,985	2,863	1,985	3,270	5,211	12,220
Employee benefits *pp* ($)	2,280	1,720	702	1,570	3,341	8,448
Student dropout rate (%)	4.4	3.8	0	1.1	8.8	20.9
Number of students	1,504	16,590	86	639	3,491	38,122
Number of teachers	371	1,182	11	57	311	4,077
County Spending per capita						
Total capital outlays ($)	141.5	187.5	0.9	33.0	170.0	1,183.9
Total operational outlays ($)	1,134.2	1,038.2	81.6	451.6	1,456.4	5,595.8
Non-mandatory operational outlays ($)	185.0	195.8	4.1	72.6	218.8	1,196.1
Highways ($)	181.2	200.6	2.0	66.0	217.0	1,228.4
Sewerage ($)	41.8	61.9	0.0	3.0	54.1	340.6
General public buildings ($)	26.5	32.8	0.5	9.2	29.9	219.7
Parks & recreation ($)	24.1	39.1	0.0	3.6	27.1	251.9
Libraries ($)	16.6	18.9	0.0	4.2	22.0	109.5
Population	111,566	215,138	1,389	14,878	101,046	1,386,667

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 the effect of the floor on municipal bond yield spreads and issuance. The yield spreads are measured as the tax-adjusted yield minus the maturity-matched Treasury rate, presented in equation (1). 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. 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 in which school district is located. Panel A compares the changes in differences between school district bonds and county bonds in floor states before and after the statewide adoption of the floor to the changes in differences between school district bonds and county bonds in non-floor states over the same time period, estimated using triple-difference approach in equation (2). Panel B includes school districts only in the sample and compares school district bonds in floor states to school district bonds in non-floor states before and after the floor enactment, estimated using equation (3). Panel C includes counties only in the sample and compares county bonds in floor states to county bonds in non-floor states before and after the floor enactment, estimated using equation (3).

Panel A. School District Bonds 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)**

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 School District Bonds by Distance to the Floor Rate**
This table reports the effect of the floor on school district bond yield spreads and issuance by the school district's distance to the minimum rate. I measure Distance$_i$ as the gap between a school district i's effective tax rate and the statutory minimum tax rate, and assign school districts to terciles of Distance$_i$. The indicators Near$_i$, Mid$_i$, and Far$_i$ equal one if a school district i belongs to the lowest, middle, and highest tercile, respectively, and zero otherwise. 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 sample includes school districts only. This specification compares bonds issued by school districts in floor states that operate close to the minimum rate before and after the statewide enactment of floor; to school district bonds that operate farther from the minimum rate; and to school district bonds in non-floor states, estimated using equation (4). All specifications include issuer and year fixed effects.

	(1)	(2)	(3)	(4)
	School District Bond Yield Spreads		**log(School District Bond Issuance)**	
	Coefficient	t-stat	Coefficient	t-stat
β(Floor$_{st}$ × Near$_i$)	-0.110**	(-2.212)	0.078**	(2.100)
β(Floor$_{st}$ × Mid$_i$)	-0.063**	(-2.089)	0.039**	(2.010)
β(Floor$_{st}$ × Far$_i$)	-0.056**	(-2.332)	0.015**	(2.111)
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 (Near$_i$)		232,455		232,455
N (Mid$_i$)		231,377		231,377
N (Far$_i$)		230,597		230,597

Table 4: **Effect of Floor on Property Taxes**
This table presents effect of floor on property tax revenue level (i.e., $\log(\text{Property Tax}_{it})$) and property tax revenue volatility (i.e., $\log(\sigma_{it}^{\text{Property Tax}})$) for issuer i in year t. 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. In Panel A, an additional treatment School_i equals one if an issuer i is a school district and zero if it is a county in which school district is located. In Panel B, there is only one type of issuer, school district. Local real estate price volatility (σ_i^{RE}) equals one if a school district's local real estate price volatility is above the median, and zero otherwise. Panel B compares property tax revenue volatility between high- and low-real-estate-price-volatility school districts in floor states before and after the statewide enactment of the floor, relative to the difference between high- and low-volatility school districts in non-floor states, estimated using equation (5).

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 ($\text{Floor}_{st}\times$ 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 by Volatility of Local Real Estate (RE) Prices

	(1)	(2)	(3)	(4)
	Property Tax Revenue Volatility ($\log(\sigma_{it}^{\textbf{Property Tax}})$)			
	Residential RE Price Volatility (σ_i^{RE})		Commercial RE Price Volatility (σ_i^{RE})	
	Coefficient	t-stat	Coefficient	t-stat
β ($\text{Floor}_{st}\times$ σ_i^{RE})	-0.142**	(-2.000)	-0.051**	(-2.078)
π_1 (Floor_{st})	0.120	(1.573)	0.105	(1.420)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	196,200		134,250	
Adj. R^2	0.345		0.233	

Table 5: **Effect of Floor on Educational Outcomes**
This table presents effect of floor on school district-level educational outcomes estimated using equation (3). There is one type of issuer, school district. 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. This estimation compares school districts in floor states to school districts in non-floor states; before and after the floor enactment. In Panel A, the outcome variables are logarithms of expenditures on education per pupil (pp), infrastructure pp, support services pp, and school lunch pp. In Panel B, the outcome variables are logarithms of total salaries pp, instruction salaries pp, employee benefits pp, and the number of teachers. In Panel C, the outcome variables are student dropout rate (%), student completion rate (%), and student-teacher ratio. Section 5 discusses the estimation.

Panel A. School District Spending Per Pupil (pp)

	(1)	(2)	(3)	(4)
	log(**Education** pp)	log(**Infrastructure** 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	$16148	$3648	$1390

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	0.221
Mean	$5,861	$3,985	$2,280	371

Table 5: **Effect of Floor on Educational Outcomes (continued)**

Panel C. Student Engagement

	(1)	(2)	(3)
	Student Dropout Rate (%)	**Student Completion Rate (%)**	**Student-Teacher Ratio**
β (Floor$_{st}$)	-0.380^{***}	0.424^{**}	-1.208^{*}
t-stat	(-2.527)	(2.111)	(-1.900)
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%	30.2

Table 6: **Effect of Floor on Student Engagement by Heterogeneity**
This table presents the effect of the floor on educational outcomes by school district income and state transfers. School districts are sorted into low and high-income groups based on the median. School districts are sorted into low and high-transfer groups based on the median. State transfers include state aid programs, such as categorical aid, and equalization grants that provide additional funding to local school districts. 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 issuer and year fixed effects.

Panel A. Student Dropout Rate

	Student Dropout Rate					
	School District Income			**State Aid**		
	Low (L)	High (H)	L−H	Low (L)	High (H)	L−H
β (Floor$_{st}$)	-1.110^{**}	-0.220^{**}	-0.890^{*}	-1.211^{**}	-0.305^{***}	-1.115^{***}
t-stat	(-2.109)	(-2.222)	(-1.919)	(-2.019)	(-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 Aid**		
	Low (L)	High (H)	L−H	Low (L)	High (H)	L−H
β (Floor$_{st}$)	-4.000^{**}	-0.545^{*}	-3.455^{*}	-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 7: **Effect of Floor on County Spending Per Capita**
This table presents the effect of the floor on county spending per capita estimated using equation (3). There is only one type of issuer, county. The outcome variables are county spending on capital and operational outlays, and county spending on parks and recreation, libraries, natural resources, highways, sewerage, and public buildings. 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. This estimation compares counties in floor states to counties in non-floor states before and after the statewide enactment of floor. Section 5.2 discusses these results.

	(1)	(2)	(3)	(4)
	log(**Capital**)	log(**Operational**)	log(**Parks & Recreation**)	log(**Libraries**)
β (Floor_{st})	-0.061^{***}	-0.034^{**}	-0.059^{**}	-0.027^{**}
t-stat	(-2.883)	(-2.196)	(-2.400)	(-2.562)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	43,982	43,982	43,982	43,982
Adj. R^2	0.239	0.231	0.218	0.191
	(5)	(6)	(7)	(8)
	log(**Natural Resources**)	log(**Highways**)	log(**Sewerage**)	log(**Public Buildings**)
β (Floor_{st})	-0.016^{*}	-0.060^{**}	-0.034^{**}	-0.097^{*}
t-stat	(-1.747)	(-2.399)	(-2.175)	(-1.729)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	43,982	43,982	43,982	43,982
Adj. R^2	0.125	0.181	0.111	0.157

Table 8: **Falsification Tests**
This table presents results on falsification tests estimated using equation (7). I assign a falsified treatment, False Floor$^{h}_{s,t}$, to a state s that has not yet adopted the floor in $t-h$ but will adopt in t (i.e., not-yet-treated state). h is the number of years by which the falsification adoption date precedes the true adoption year. I falsely code an adopting state as treated in the h years before its true adoption year. False Floor$^{h}_{s,t}$ is zero if a state s never adopts the floor (i.e., never-treated state). I exclude all years after the true adoption year, so that I can compare the pre-existing differences between not-yet-treated and never-treated states. Panel A presents the effect of the False Floor$^{h}_{s,t}$ on school district bond yield spreads and issuance. Panel B presents the effect of False Floor$^{h}_{s,t}$ on county bond yield spreads and issuance.

Panel A. Falsification Test for School Districts

	(1)	(2)	(3)	(4)
	School District Bond Spreads		**log(School District Bond Issuance)**	
	Coefficient	t-stat	Coefficient	t-stat
β (False Floor$^{h=5}_{s,t}$)	0.028	(1.451)	-0.059	(-1.543)
β (False Floor$^{h=2}_{s,t}$)	0.017	(0.061)	0.023	(1.211)
β (False Floor$^{h=1}_{s,t}$)	0.002	(0.561)	-0.011	(-0.932)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	152,111		152,111	

Panel B. Falsification Test for Counties

	(1)	(2)	(3)	(4)
	Municipal Bond Spreads		**log(Municipal Bond Issuance)**	
	Coefficient	t-stat	Coefficient	t-stat
β (False Floor$^{h=5}_{s,t}$)	-0.012	(-0.593)	0.010	(0.635)
β (False Floor$^{h=2}_{s,t}$)	-0.007	(-0.451)	-0.019	(-0.643)
β (False Floor$^{h=1}_{s,t}$)	-0.004	(-1.135)	-0.003	(-0.664)
Issuer FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
N	231,150		231,150	

Table 9: **Placebo Test: Revenue Bonds**
This table presents results on a placebo test estimated using revenue bonds in the spirit of Butler and Yi (2022) and Butler et al. (2025). 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
N	932,202		932,202	
Adj. R^2	0.058		0.034	

Table 10: **Effects of Floor on Municipal Bond Yield Spreads Excluding Economic Recessions** This table presents the effect of the floor on municipal bond yield spreads estimated using equations (2) and (3) excluding economic recessions, namely the Global Financial Crisis (2007–2009), Covid-19 pandemic (2020–2021), and both periods. All specifications include issuer and year fixed effects.

Panel A. School District Bond Yield Spreads

	(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
N	956,214	1,074,903	800,267
Adj. R^2	0.303	0.248	0.262

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

	(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
N	956,214	1,074,903	800,267
Adj. R^2	0.156	0.233	0.143

Table 11: **Bad Controls: Excluding Credit Ratings**
This table reports the effect of the floor on municipal bond yield spreads estimated using the 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. 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 in which school district is located.

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

Table 12: **Alternative Measures of Municipal Bond Yield Spreads**
This table reports the results from estimating equations (2) and (3) using alternative measures of municipal bond yield spreads. In columns (1) and (2), I use yield minus maturity-matched risk-free rate from Liu and Wu (2021) without the tax-adjustment. In columns (3) and (4), I use yield minus maturity-matched risk-free rate from Gürkaynak et al. (2007) without the tax-adjustment.

Panel A. School District Bonds Compared to County Bonds

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

Panel B. School District Bonds

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

A Online Appendix

A.1 Additional Heterogeneity Tests

Reliance on property tax revenue. The impact of the 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.5. 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.

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 A.4 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 the floor on spreads.

Co-party affiliation. The change in spread differs little between local governments whose governor is from the same party as the U.S. president (i.e., unified governments) and those from a different party (i.e., divided governments). A potential concern is that unified governments may adjust deficits faster than divided governments (Poterba, 1994) and therefore the decline in spreads

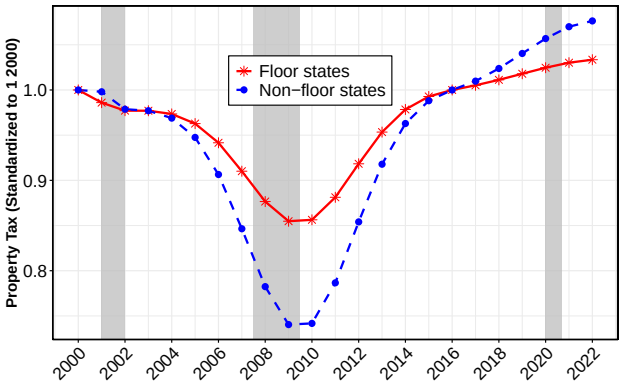
is pronounced in unified governments. I first confirm that federal transfers to local governments do not differ significantly between unified and divided governments, as shown in Panel A of Table A.5, which rules out the possibility that federal transfers are higher and therefore deficits are adjusted 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 A.5.

Local residents' race. Finally, 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 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 A.6, 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.

B Appendix Figures and Tables

Figure A.1: **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-year 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

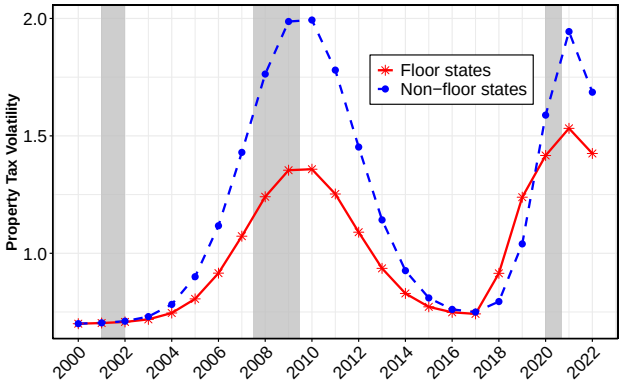


Figure A.2: **Dallas County and School Districts**
The figure shows the effects of the floor on yield spreads of bonds issued by Dallas County and school districts nested in Dallas County.



(a) County (b) School District

Figure A.3: **Example for Mill Rate**
This figure shows an example of this data for the state of Alabama. I obtain data on mill rates from the Lincoln Institute of Land Policy.

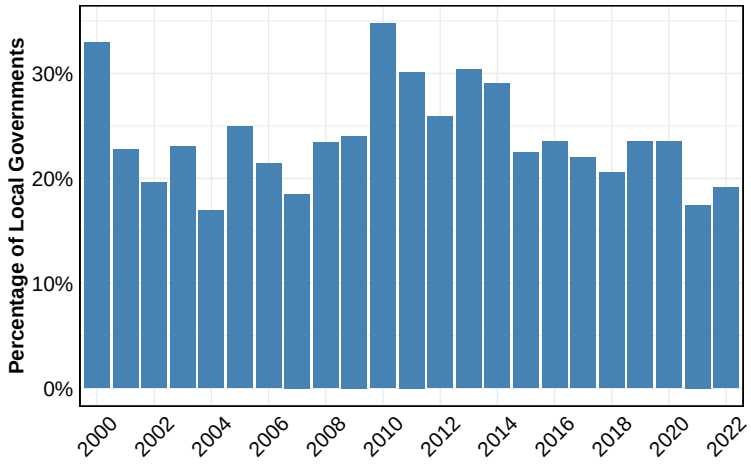
Alabama Department of Revenue

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.4: **Fraction 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 fraction of local governments that experienced an increase in property tax revenue relative to the previous year. Panel C plots the fraction of local governments that experienced a stable change in property tax revenue relative to the previous year. I classify a local government as experiencing a stable change when its property tax revenue changes by less than 0.5% from the previous year.

Panel A. Percentage of local governments with declining property tax revenue



Panel B. Percentage of local governments with increasing property tax revenue

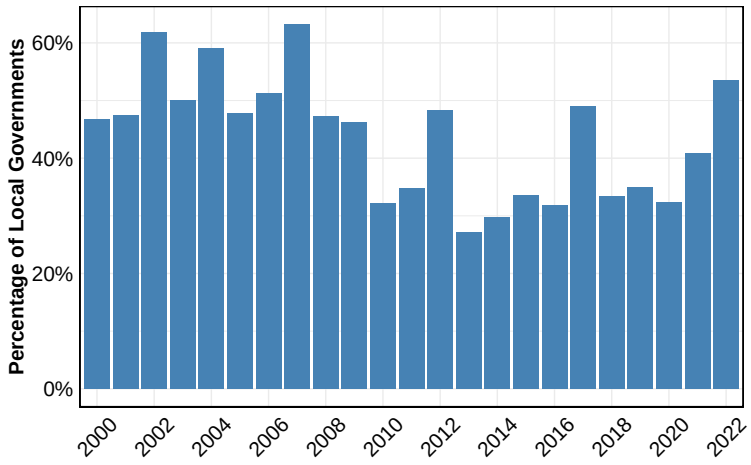


Figure A.4: **Fraction of Local Governments (Continued)**

Panel C. Percentage of local governments with stable property tax

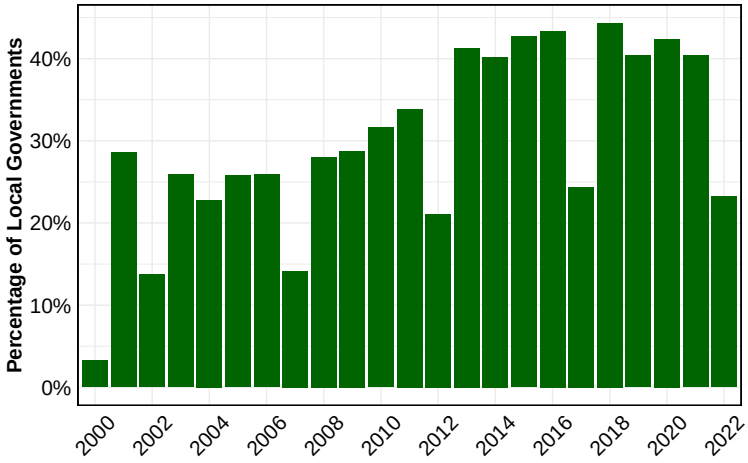
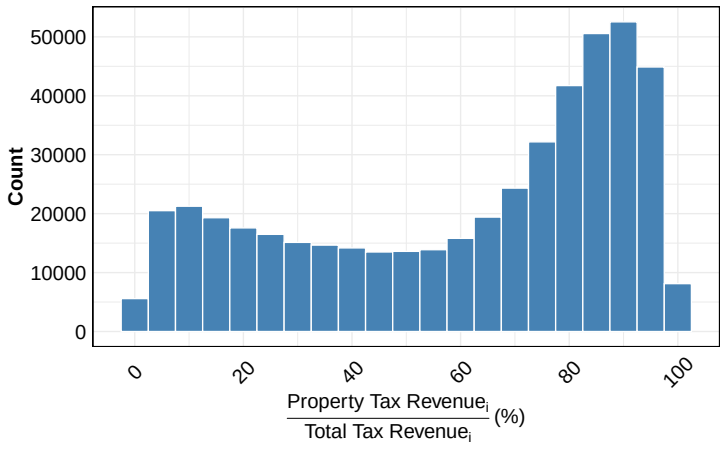


Figure A.5: **Property Tax Share**
Panel A plots a histogram on the distribution of government-year share of property tax in total tax revenue. I use data on revenue for school districts and counties from the Census. Panel B plots average share of residential property in tax base across school districts 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 Revenue in Total Tax Revenue



Panel B. Share of Residential Property Revenue in Total Tax Revenue

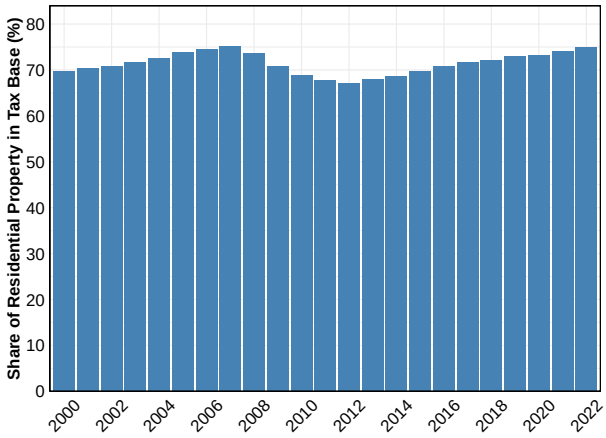


Table A.1: **Sample Construction.**
This table shows observations remaining after each step of cleaning the data, following the presentation of Schwert (2017)'s Table 1. I first restrict the sample to general obligation (GO) and K-12 school district bonds. I then remove data errors, following Schwert (2017), Curti et al. (2023), Gao and Murphy (2022), and Fleckenstein et al. (2014), and drop bonds that have coupons greater than 20%, maturities over 100 years, and fewer than 10 trades. The Census match restricts the sample to counties and school districts with annual revenues over $50 million, following Schwert (2017). See Section 3 for the remaining steps of the data cleaning.

Observations remaining after each step					
Cleaning Step	**Bonds**	**Trades**	**School Districts**	**Counties**	**States**
Initial sample	2,762,569	24,520,945	25,008	2,976	50
Restrict to GO and K-12 school districts	1,998,569	13,120,185	13,054	2,803	50
Remove data errors	825,855	8,958,324	12,417	2,749	50
Census match	173,183	7,513,357	8,163	2,611	50
Drop variable rate bonds	172,180	5,513,357	7,984	2,589	50
Drop insured bonds	131,908	4,791,217	6,472	2,514	50
Drop pre-refunded bonds	125,908	3,791,076	5,793	2,463	50
Drop taxable and AMT bonds	111,611	2,639,523	4,912	2,389	50
Drop Trades After Maturity	111,786	2,637,880	4,851	2,361	50
Drop callable bonds	109,235	1,231,150	3,606	2,270	50
Final sample	109,235	1,231,150	3,606	2,270	50

Table A.2: **Average yields by maturity of h years.**
This table shows average yields of counties and school districts in each state by bond maturity.

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.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 ($)	3,222	1555	914	2113	3950	8071
B: Top Income Tax Rates						
Federal income tax rate (%)	36.86	1.91	35	35	38.85	39.60
State income tax rate (%)	5.58	2.95	0	4.60	7	13.30
Property tax volatility (10-year)						
School District and County ($)	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

Table A.4: **Effects of Floor on Municipal Bond Yield Spreads By Liquidity Risk**
This table presents the effect of the floor on municipal bond yield spreads by bond liquidity risk. High and low-liquidity risk bonds are grouped based on the median of liquidity risk measured using Amihud (2002)'s price impact.

	(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
N	602,411	609,598
Adj. R^2	0.251	0.258

Table A.5: **Effects of Floor on Municipal Bond Yield Spreads by Co-Party Affiliation**
This table presents the effects of the 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. Panel B reports the effects of the 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.

	(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
N	296,845	315,164
Adj. R^2	0.166	0.122

Table A.6: **Effect of Floor on Municipal Bonds Controlling for Local Residents' Race**
This table presents the effect of the floor on municipal bond yield spreads estimated using equations (3) and (2) controlling for local residents' racial composition. I measure racial composition as a share of the race in total population in the municipality. All specifications include year fixed effects.

	(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
Adj. R^2	0.224	0.231	0.228	0.233	0.227