School District Borrowing and Capital Spending: The Effectiveness of State Credit Enhancement*

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Abstract

School districts in the United States often borrow on the municipal bond market to pay for capital projects. Districts serving economically disadvantaged communities tend to receive lower credit ratings and pay higher interest rates. To remedy this problem, 24 states have established credit enhancement programs that promise to repay district debt when a district cannot do so, thereby enhancing the district's credit rating. With a generalized difference-in-differences approach, I rely on cross- and within-district variations to estimate the effect of state enhancement on district bond interest rate, per-pupil capital spending, and student performance. State enhancement reduces district bond interest rates by 6% and increases per-student capital spending by 2% to 7%. It also reduces the disparity in the interest rate and capital spending across districts serving lower- and higher-income families, with no discernible effect on test scores. I find no evidence that the amount of enhanced school debt is associated with significant changes in interest rates paid by state governments. Districts in states without such programs could have achieved cost savings in the range of $383 million to $1 billion from 2009 to 2019 had the states adopted similar programs.

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1 Introduction

Investments in public K-12 school facilities, including construction, renovation, and equipment purchase, represent the largest infrastructure spending in the United States besides highways (Filardo, 2016). In 2016-2017, public school districts spent $1,266 per student on capital projects, about 30 percent of which was paid by states. In contrast, the per-student operational spending was $12,794 and the states picked up a larger share. Two unique characteristics of school capital spending are relevant here. First, even though the average annual capital spending is smaller than operational spending, capital outlays are "lumpy" in that a one-time, large upfront expense is often required. Accumulating savings to finance large capital projects is practically difficult and violates the principle that those benefiting from a project should pay for it. As a result, districts often borrow money by issuing a bond on the municipal bond market, using the bond proceeds to finance the capital project, and repaying the bond over the lifespan of the project. Second, because school districts are more self-reliant for capital spending than for operational spending, the disparity in capital investment across districts may be more pronounced. The bond market assesses districts' creditworthiness based on their financial conditions and management practices, as well as the underlying economic and demographic factors. Consequentially, districts serving socioeconomically disadvantaged areas may be perceived as less creditworthy and thus pay higher interest rates. Even when the state provides grants for school capital needs, such disparities in interest rates may persist because districts issue bonds in their own names.

To address this disparity, 24 states provide credit enhancement to school district debt, by committing themselves to take some actions, such as intercepting state aid or temporarily using state funds for debt repayment, when a district fails to pay. This paper provides the first nationwide, comprehensive evaluation of state credit enhancement of school district debt, specifically its effects on the interest rate and capital spending of districts. Two other policies are available in some states to facilitate school district capital spending and borrowing, and to reduce disparities. The first is direct state aid for capital spending, which can be used for reimbursing capital outlays or repaying

debt.[1] The second is low-interest loans from states to districts.[2] State enhancement programs differ significantly from the other two types of state policies because they typically do not require initial state investments, but only a promise to act if districts faced debt repayment problems.

I first provide descriptive evidence that school districts serving higher-poverty communities receive lower credit ratings and pay higher interest rates. This suggests that the differing costs of debt may partially explain why districts in more affluent areas incur higher capital spending. Biasi, Lafortune, and Schönholzer (2021) show that the gap in per pupil capital spending between the top and bottom income quintiles has narrowed in recent years. I demonstrate that state enhancement may be one factor contributing to this narrowing gap. After the demise of the private bond insurance industry following the Great Recession, all district borrowers lose the option to receive very high enhanced ratings through purchasing insurance. Consequently, interest cost rises and capital spending falls. The notable exception is high-poverty districts located in states providing credit enhancement, which are more likely to receive state enhancement. Their capital spending has remained relatively stable in contrast to the decline experienced by other districts.

The main challenge in examining whether state credit enhancement causes a change in districts' bond interest rates and capital spending is that state enhancement is not randomly assigned. In the preferred specification, I adopt a generalized difference-in-differences approach by using district fixed effects and examining within-district variations in state enhancement. Specifically, I compare observations within the same district that are enhanced by the state versus those not enhanced. An alternative specification examines across-district variations in state enhancement. I compare, within the same state, districts whose bonds are always enhanced by the state with districts that are never enhanced. I argue later in the paper that the two identification strategies may bias the estimate in different directions if such biases exist, and one would be able to identify a likely range that the true effect lies within.

Relying on the two sources of mutually exclusive variations, the analyses generate remarkably similar effect estimates of state enhancement. Bonds enhanced by the state enjoy an interest rate

[1] According to Duncombe and Wang (2009), among the 38 states providing such aid, 7 offer lump-sum aid that does not depend on local contribution, 22 offer matching aid where the local share may or may not be adjusted for local property wealth level, and the rest offer a combination of the two approaches.

[2] At least nine states provide such loans(Duncombe & Wang, 2009). The loaned funds come from the state's own-source revenue or from a state entity called the bond bank, which borrows money by issuing pooled-financing bonds. Bond banks issue bonds in their own names and may achieve lower interest rates than individual districts due to economies of scale (Robbins & Kim, 2003).

reduction at around 0.14 percentage points or 14 basis points (roughly a 6 percent decrease). The effects are heterogeneous: the reduction is 12.5 basis points for districts in the lowest baseline poverty tercile but 18 basis points for high-poverty districts. High-poverty districts on average receive lower underlying credit ratings and thus benefit from a larger degree of rating elevation through receiving the enhanced rating from the state programs.

Due to the lack of data on the full history of state enhancement for each district and the fact that most capital investments happen within a few years after bond issuance, I focus on the four years following the first state enhancement during 2009-2019 when examining the impact on capital spending. During each of the four years, districts increase per-pupil capital spending by $337 to $349. When smoothed over the range of useful lives of capital projects, this represents a 2% to 7% increase in annual capital spending. The state enhancement-induced increases are larger in districts with higher baseline poverty rates. Event study analysis results show support for a parallel pre-trend, and districts issuing state-enhanced bonds do not have statistically different levels of capital spending prior to the bond issuance, lending credibility to the causal interpretation of the effect on capital spending.

Using the Stanford Education Data Archive (SEDA) data on district-level, standardized test scores administered in third through eighth grade in mathematics and Reading Language Arts (Reardon et al., 2021), I do not find any effect of receiving state enhancement on test scores. This suggests a null effect of the marginal capital spending increases on student performances.

In sum, state credit enhancement has the potential to offer significant interest savings to school districts, which may spur additional educational infrastructure investment. Using the effect estimates from the empirical analysis and school bond issuance data, I calculate the potential savings bond-issuing districts in 17 nonprogram states could have achieved from 2009 to 2019, had credit enhancement programs existed in these states. Under different assumptions about program setup, districts had the potential to realize $383 million to $1 billion in net present savings or up to 2 percent of the principal of school debt issued. Given the low interest rate environment during the analysis period, one may expect the borrowing cost reduction impact to be even larger if market rates were high. I find no evidence that the amount of school bonds enhanced is associated with interest rates paid by states on their own general obligation (GO) debt. I discuss how the design of state enhancement programs and the rarity of school district defaults may have contributed to

the lack of a "moral hazard" in pledging state support to local school debt, which relates to the literature on the political economy of fiscal federalism (Oates, 2005; Weingast, 2009). In decentralizing fiscal decision-making to localities, higher-level governments are often concerned about how serving as a backup payer may lead to moral hazard and excessive local spending. However, when combined with policies toward a hard budget constraint, such as preconditions for and limitations on bailout, ongoing monitoring, and market discipline, the moral hazard problem may be mitigated.

The paper also adds to the literature on school resource disparity. While much of this literature has focused on operational spending, our understanding of the disparity in capital investment is more limited. Studies by the U.S. Government Accountability Office (GAO) find that over half of public school students attend school in a building that needs at least one or more major building components or features extensively repaired, especially in schools serving high-poverty communities (GAO, 1995, 2020). The 21st Century School Fund estimates that additional spending of $46 billion per year is needed to maintain, renew, and construct school facilities to modern standards (Filardo, 2016). Using data from California, Brunner, Schwegman, and Vincent (2021) find that capital outlays vary across districts where districts with lower assessed property value per pupil, a larger share of disadvantaged students, and a top-quintile share of Black and Hispanic students issue less GO bonds. Using nationwide data, Biasi et al. (2021) find that the capital spending gap between districts from the highest and lowest within-state income quintiles has largely disappeared after 2009, due to decreased spending by richer districts. This paper shows that the cost of credit is an important contributing factor to capital spending disparity, and policies that reduce interest rates faced by high-poverty districts may narrow the gap in school infrastructure investment.

Finally, the paper is related to the literature on the impact of school capital spending. While research provides mixed evidence on whether capital spending affects student test scores (Brunner, Hoen, & Hyman, 2022; Martorell, Stange, & McFarlin Jr, 2016; Conlin & Thompson, 2017; Neilson & Zimmerman, 2014), some studies find that school capital investments lead to housing price increases, suggesting other benefits beyond improvements in test scores (Cellini, Ferreira, & Rothstein, 2010; Conlin & Thompson, 2017; Neilson & Zimmerman, 2014). Moreover, specific types of capital investments, such as new facilities to reduce crowding, air conditioning, and ventilation, are positively associated with academic performances (Lafortune & Schönholzer, 2019; Park, Goodman, Hurwitz, & Smith, 2020; Haverinen-Shaughnessy, Shaughnessy, Cole, Toyinbo, & Moschandreas,

2015). While I find a null effect on test scores, the marginal increase in capital spending induced by state enhancement may be too small to cause statistically detectable changes in test scores.

2 Policy Background and Related Literature

2.1 State Credit Enhancement

State and local governments, including school districts, borrow money by issuing bonds on the municipal bond market. Most states require school districts to obtain voter approval before issuing bonds. In those cases, successful bond elections must occur before a bond is sold and the interest rate is determined by the bond market. Investors evaluate the creditworthiness of an issuer government and demand an interest rate reflective of how likely the debt will be repaid. In other words, a district perceived to have a lower capacity to service its debt, such as those with lower revenue, pays a higher interest rate, holding all else equal. Private credit rating agencies operate in the municipal bond market, as they do in other financial markets, by assigning a rating representing their assessments of the creditworthiness of a bond. Such ratings are assigned only if paid for by the bond-issuing governments.

Districts can pursue two types of credit enhancement to lower interest rates. The first comes from private bond insurance companies. By paying for an insurance policy, a borrower demonstrates to investors that the insurance company will step in when the borrower itself cannot repay the debt.[3] Effectively, bond insurance allows the borrower to wrap the bond with the "enhanced" rating of the insurance company, which is often higher than the bond's "underlying" rating. Private insurance became increasingly popular since its inception in the municipal bond market, peaking in 2005 when the majority of issued volume was enhanced by private insurance (Cornaggia, Hund, & Nguyen, 2020). However, the Great Recession of 2007 caused downgrades to the entire private insurance industry. Bond insurance providers no longer carry a high rating that makes their enhancement attractive to many issuers. As a result, the use of private bond insurance declined precipitously afterward (Cornaggia et al., 2020).

[3]Another related, but distinct, type of credit enhancement is letters of credit offered by a commercial bank. The bank commits to pay principal and interest if the issuer is unable to do so and may be reimbursed by the issuer later. Letters of credit are more often used for variable rate demand obligations than fixed-rate bonds. The main purpose is liquidity support as opposed to credit enhancement.

State credit enhancement programs, most of which were adopted in the 1970s to 1990s, provide a second source of enhancement and a substitute for private bond insurance. These programs commit and leverage existing state resources to service district debt in case districts have trouble repaying their debt. Credit enhancement represents a non-financial, or "in-kind," state assistance to districts because it does not require actual state spending but only the commitment to intervene in local defaults. The strength of the state commitment depends on the sufficiency and certainty of the committed resources and can be categorized into three types accordingly (Ely, 2012; Kozlik, 2016; Demarco, 2020). Table 1 presents information on the 24 states with credit enhancement programs for school districts, the type of program based on the strength of the state pledge, and each program's credit rating at the end of 2019.

[Table 1 about here]

First, in an intercept program, the state pledges to intervene by intercepting or withholding state aid due to the school district and forwarding them to bondholders.[4] This credit backing is generally the weakest type of state enhancement because it does not create new financial resources for districts but rather prioritizes existing state aid for debt repayment. Nevertheless, intercept is the most common type of state enhancement, existing in 14 states. Intercept programs carry ratings that are zero to three notches below the state's rating (Kozlik, 2016), reflecting the fact that the enhancement pledge is narrower than the state's general taxing power.

Second, the state government may commit its general revenue in case of a pending district default. Three states make an appropriation pledge that funds will be appropriated by the state legislature. Six states take one step further and statutorily guarantee that state funding, often from a specific source of tax or fund, will be used without the need to pass an appropriation. The "guarantee" programs usually carry the same rating as the state's rating and "appropriation" programs have the same rating as the state or a notch lower.

Lastly, three states have dedicated funds, akin to endowments, for supporting school districts and their debt. As fiduciary funds, these programs differ from other state enhancements because the fund trustees have an exclusive obligation to protect the interests of their beneficiaries (Ely,

[4]Funds available for intercept differ across states. The strongest type of intercept carries a pledge that the state would divert any state funds already committed to the district and funds that will be appropriated in the future. The weakest type pledges only a particular type of aid that has already been allocated to the district that year.

2012). That is, the funds provide a source of funding exclusively for certain purposes, including backing school district debt, and do not need to compete with other spending priorities of the state. Therefore, the credit ratings of permanent fund programs are often excellent and are not dictated by the state rating. As with appropriation/guarantee programs, if the permanent fund is tapped for repaying school debt, school districts must later repay the state with interest and penalty.

Each enhancement program carries a credit rating that becomes the enhanced rating for participating school bonds. Committing the state's own-source revenue, through appropriation/guarantee or permanent funds, provides more security to investors than only pledging state aid intercepts and thus is often associated with higher credit ratings. The ratings of enhancement programs also depend on a state's financial position and may change over time. For example, despite providing a dedicated School Bond Reserve for debt guarantee, the New Jersey Bond Reserve program had an "A-" rating because the state's rating was only in the single A range and the fund had about $70 million to secure close to $7 billion in district debt. On the other hand, states such as Missouri and Indiana had intercept programs with very high ratings, because the states had triple-A ratings. Therefore, districts with an underlying rating higher than the state-enhanced rating may not participate in the enhancement program, especially when time and money are required in securing the enhancement.[5] This provides a source of variation in state enhancement *across* districts.

Appendix Table D1 provides additional details about the state programs, including citations of the authorizing laws.[6] Many intercept programs are automatically applied to eligible bonds. In other programs, districts must first apply to a state agency for approval. The state agency collects information about the district and the bond to be issued. It then decides whether to grant an approval, based on assessments that may include the district's fiscal condition, level of debt outstanding, and past default record. Such an approval process may serve as a disciplinary tool for districts to prudently plan their debt. Among the programs requiring an application, some collect

[5]Data show that about one percent of bonds enhanced by state programs have an underlying rating higher than the enhanced rating. As explained later, districts are automatically eligible for enhancement in some programs; in these cases, the cost to claim the enhanced rating is minimal.

[6]A overwhelming majority of the programs are pre-default programs, meaning that a district or its bond trustee must inform the state if they detect a potential debt service deficiency before the default actually occurs. This adds to investor confidence that payments of principal and interest will not be delayed. Only in the New York program does the state intercept funding after default has occurred.

a nominal application fee, which is then used to defray the cost of administering the programs.[7]

States often place limits on the type or amount of school bonds that can be enhanced. For example, some allow only GO debt. Although not shown in the table, some states place a cap on the debt level or projected debt service coverage ratio for a district to be eligible, while some others give the responsible state agency regulatory discretion in assessing a district's existing debt burden when approving enhancement.[8] Therefore, some bonds of a district may be enhanced while others of the same district are not, providing a source of variation in state enhancement *within* a district.

To our knowledge, state enhancement of school bonds has never been triggered in recent years except for the Penn Hills School District in Pennsylvania.[9] Through what the state auditor described as "stunning financial mismanagement and illogical business decisions," the district had significant operating deficits and issued large amounts of bonds for facility construction without adequately budgeting for related debt payments (Burton, 2016). The state stepped in to make payments from 2015 to 2020. Monetary defaults, i.e. the failure to pay principal or interest, are very rare among school district bonds. From 1970 to 2020, Moody's reports only two defaults among school bonds rated by the agency and two among the unrated. The two rated bonds and one unrated bond were insured but not enhanced by state programs. The other unrated bond is issued by Munster School District and enhanced by the Indiana Intercept Program. The district cured the deficiency with an emergency loan and avoided triggering the state intercept. One may observe few defaults precisely because state enhancement programs are effective at preventing such defaults. However, through searching the Electronic Municipal Market Access (EMMA), a website for municipal bond disclosure, I identify no actual use of state enhancement other than Penn Hills.[10]

[7] Fixed fees are the most common. The programs in Idaho and Oregon vary the fee amount to be 0.02% to 0.05% of total debt service. Anecdotal evidence suggests that school districts have complained about the fees being tied to the size of issuance. High fees may constitute a barrier to taking advantage of the state program. However, the fee levels appear to be lower than the average premium on private bond insurance.

[8] For example, Nevada limits bonds subject to its program to $60 million per district. Missouri limits the total maximum annual debt service of a district to be less than two-thirds of state aid payments.

[9] Although not issued directly by a school district, a bond issued by Mason County, Washington for school purposes missed interest payments on June 1, 2021, due to a technical connectivity problem. Per the state guarantee program, the state treasurer made the payment on time and was reimbursed by the county later that same day.

[10] It should be noted that the federal regulator cannot require issuer disclosure, and thus, the reporting on EMMA may not be complete. I supplement the search with information directly collected from state agencies and identify no other cases of state enhancement activation.

2.2 Literature Review

The literature on state credit enhancement is scarce.[11] Most empirical studies focus on select states and periods before 2000 (Bland, 1987; Bland & Yu, 1988; Hsueh & Kidwell, 1988). They find that some but not all state programs are associated with interest savings on local bonds. By comparing enhanced bonds to other bonds issued in the same state, Bland and Yu (1988) find that the state program is associated with a 12-basis point and a 33-basis point saving in Michigan and Texas, but no statistically significant savings in Indiana and New Jersey. Given that the credit ratings of programs in Michigan and Texas were higher, the finding aligns with the expectation that the magnitude of savings depends on the strength of the state program's credit backing. Similarly, focusing on only the Texas Permanent Fund Program, Hsueh and Kidwell (1988) find that districts achieve interest cost savings ranging from 40 to 98 basis points for bonds rated single-A and Baa. However, the bond market does not treat bonds in the enhancement program—thus carrying triple-A enhanced rating—to be as creditworthy as a natural triple-A bond. Consequently, district bonds rated double-A or above do not achieve any interest rate reductions. The authors interpret this finding to mean that, although the state program ensures the principal and interest payment, it does not eliminate all default-related costs, such as transaction costs of reinvestment and interruption to investment plans.

Although no empirical studies examine the impact of state enhancement on school district capital investment or the heterogeneity in the impacts, the literature suggests that state enhancement likely assists districts most in need of capital investment. Because private insurers pursue profits, they may "cream skim" by insuring only those least likely to default and charging a high premium on those with low creditworthiness (Bland, 1987). Districts that would benefit more from insurance are less likely to be insured. In contrast, state governments do not have a profit motive. Some exert oversight through the enhancement program to encourage fiscally prudent decisions by local governments, such as maintaining a reasonable debt service coverage ratio. States like Texas explicitly prioritize less affluent districts for enhancement.

States need to consider the fiscal risk of providing credit enhancement if the enhancement pledge

[11] The related literature on private bond insurance is relatively large (Nanda & Singh, 2004; Cornaggia et al., 2020), so is the literature on contingent liabilities of sovereign nations that guarantee sub-national and private debt (Mody & Patro, 1996; Polackova, 1998).

lowers the perceived creditworthiness of state debt by investors and leads to higher state borrowing costs. While scholars have hypothesized this negative impact on states (Bland, 1987; Ely, 2012), no empirical study has examined this issue. There could be a moral hazard problem, akin to the issue of states authorizing localities to pursue bankruptcy (Yang, 2019). School districts may behave fiscally irresponsibly if the state guarantees and will ultimately pay back their debt. Some states limit enhancement to only GO bonds, cap the amount of enhanceable bonds, or require application and approval for participating in state programs. These requirements may limit the risk exposure of the state's own credit.

3 Data and Descriptive Statistics

3.1 Data

A proprietary database covering all newly issued school district bonds from 2009 to 2019 comes from Ipreo Muni Analytics.[12] Each bond issue may consist of multiple serial bonds with differing maturities, meaning that some serial bonds within the issue will be paid back earlier while others later. The serial bond structure enables the issuer to spread the principal payment over time. Each serial bond, consequently, pays a different interest rate at the time of issuance, also known as the initial offering yield. While maturity and yield differ across bonds within a series, the entire series shares some commonalities, such as credit enhancement and ratings. The bond issuance data include information on both series-level and bond-level characteristics. I include only bonds with a maturity longer than one year as short-term bonds are often issued for cash flow purposes instead of capital projects.

School district data come from the National Center for Education Statistics (NCES) Common Core databases for the 2009-2019 period and the year 2000 for baseline characteristics. The analyses include only regular local school districts. First, the Local Education Agency (LEA) Universe Survey provides data on district characteristics, such as enrollment level and racial composition of students. Second, data on district finances come from the LEA Finance Survey, also known

[12] Although the data set includes bonds issued prior to 2009, it only shows the enhancement providing the highest enhanced rating during the early period. Since bond insurance always provided a triple-A rating and was widely used before 2009, state enhancement information was likely missing. For this reason and to exclude the Great Recession period, I start the analysis in 2009.

as the F-33 survey. The Small Area Income and Poverty Estimates (SAIPE) Program provides estimates of the poverty rate among school-age children residing in the district. Using the 2000 poverty estimates, I calculate the baseline poverty percentiles.[13] Lastly, because the SEDA data cover only up to the 2017-2018 school year, the number of observations is smaller when examining test score outcomes. I normalize the data based on the 2009 test scores, and thus the effects are measured in units of standard deviations.

The NCES LEA identifier is not available in the bond data. Instead, each bond is identified by a commonly used financial security identifier called CUSIP. I rely on the district names to establish a crosswalk between the bond data and the district data. A fuzzy text matching algorithm provides suggestive matches linking the LEA identifier with CUSIP. I manually comb through to fill in for nonmatches and correct for mismatches. All variables measured in dollar amounts are inflation-adjusted to 2019 dollars.

3.2 Descriptive Statistics

Appendix table D2 presents the bond summary statistics. A total of 326,438 serial bonds have been issued by school districts from 2009 to 2019, representing 33,272 series. The average interest rate paid on these bonds is 2.35 percent. Bonds enhanced by state programs on average pay a lower rate at 2.21 percent, as compared to those from the same state but are not enhanced by the state. Enhanced bonds are also different in other aspects: on average, they have longer maturities, are larger, and are more likely to be competitively sold, GO, callable, and refunding bonds. State-enhanced bonds are more likely to not have an underlying rating; this may be because the need to pay for an underlying rating is diminished by the availability of a state-provided, enhanced rating. On the other hand, having an underlying rating could provide additional information that is appreciated by investors in an opaque market like the municipal bond market.

Figure 1 further sheds light on the rating differences between state-enhanced bonds, privately insured bonds, and unenhanced bonds. The x-axis represents the rating scale, where 0 is unrated and 16 is triple-A. The shaded bars represent the underlying rating. Unenhanced bonds have

[13] Specifically, the percentiles are based on the districts in the 2000 sample, weighted by a stable enrollment count calculated as the geometric mean of each district's enrollment in all available years. An alternative measure of community affluence is the median housing value for households with children from the 2000 census. This measure may proxy for the strength of a district's tax base. Although not reported, the results are robust to using median housing value instead of the child poverty rate.

better underlying ratings. In other words, credit enhancements, including private insurance and state enhancement, are more likely to be used by districts with lower underlying creditworthiness because the enhanced ratings (represented by hollow bars) push the bonds up the rating scale. The distribution of underlying ratings is more spread out among state-enhanced bonds than privately insured bonds. This is because private insurers did not have very high ratings for much of the period, and thus, could not provide value for district with relatively high underlying ratings. In contrast, a few state programs had triple-A ratings, providing value-added to even districts whose own ratings were very high.

[Figure 1 about here]

The upper panel of Figure 2 shows that after the Great Recession, state programs have overtaken private insurance as the primary source of credit enhancement for school district bonds, enhancing 40 percent of new bonds since 2010; in contrast, only about 20 percent are insured. A caveat and limitation of the data is that, for each bond, only the enhancement providing the highest rating was reported and thus I do not know the prevalence of double enhancement.

[Figure 2 about here]

Table 2 presents the baseline statistics on district annual revenue, spending, and other characteristics from the academic year 1999-2000. Based on all 11,150 districts, the first column shows the averages, followed by the standard deviations in parentheses in the second column. Among them, 7,406 districts have issued bonds during the 2009-2019 period (the "issuer" column).[14] Comparing all districts to the subsample of bond issuers, issuers had, on average, slightly larger enrollment in the baseline. This is in line with our understanding of the municipal market: issuing bonds requires significant transaction costs, such as fees paid to financial advisors and legal counsels, and thus, makes more sense to large governments with big-ticket capital projects.

[Table 2 about here]

I further break the issuer sample into districts that have never received state enhancement versus those that have received enhancement on at least one series. In the baseline, the "ever

[14] For the summary statistics, I link the bond data to the district-year data based on the school year during which a bond's sales date falls into.

enhanced" group had a higher share of Black and Hispanic students and a higher poverty rate, received less own-source revenue but more intergovernmental transfers per pupil, and spent less on operation but more on capital projects. This suggests that districts serving a larger share of socioeconomically disadvantaged students are more likely to receive state enhancement.

To explore why that might be the case, I categorize issuer districts by the highest underlying rating they have received from 2009 to 2019 in the last four columns of Table 2. The underlying rating represents rating agencies' assessment of the creditworthiness of a bond, without considering any credit enhancement. Because, holding all else equal, worse ratings are associated with higher interest rates, comparing the four columns shows which districts are more likely to be disadvantaged in the municipal bond market. The baseline poverty rate is negatively correlated with the underlying rating. The poverty rate was 16 percent among unrated and 14 percent among lowly rated districts but only 8 percent among districts with a high rating. Because income and property wealth are related, districts serving high-poverty areas likely generate less property tax revenue. Indeed, own-source revenue per pupil was the smallest among unrated and lowly-rated districts. Although they received more federal and state transfers, these transfers did not make up for the low level of locally collected revenue. As a result, unrated and lowly-rated districts incurred less operational and capital spending. These baseline statistics suggest that not all school districts are created equal in terms of borrowing on the municipal bond market. Districts serving poorer areas are more likely to receive a low rating on average. Because lower ratings are related to higher interest rates, these districts likely pay more on their debt. In contrast, the share of Black and Hispanic students did not differ greatly based on the types of underlying ratings but was smaller for unrated districts.

Taken together, the descriptive statistics show that districts serving a larger share of socioeconomically disadvantaged students are more likely to participate in state credit enhancement programs, probably because they are more likely to benefit from the enhancement given their relatively low underlying ratings. Ultimately, one would want to know whether the state enhancement leads to lower interest rates and thus more capital spending for these districts. The lower panel of Figure 2 contrasts the real per pupil capital spending between districts in the lowest and highest 2000 poverty quintiles, first in states offering credit enhancement and then in states without such programs. Because capital expenditures exhibit natural fluctuations over the cycle of a capital project, I follow Biasi et al. (2021) and present a five-year moving average of per-pupil capital

spending. In nonprogram states, districts in the lowest poverty quintile spend consistently more than those in the highest poverty quintile, and the gap is stable over time. The drop starting in 2010 likely reflects the impact of the Great Recession when tax revenue declined and triple-A-rated bond insurance disappeared. In states offering credit enhancement, districts in the lowest poverty quintile are likely to carry good underlying ratings and not need state enhancement.[15] Indeed, the trend in their capital spending closely mimics that of districts in nonprogram states. However, high-poverty districts in states offering enhancement exhibit a unique trend: they have increased spending on capital projects following the Great Recession, and have surpassed the low-poverty districts in per-student capital spending between 2011 and 2017. These graphs provide one possible explanation for the finding in Biasi et al. (2021) that the capital spending gap between high- and low-income districts has disappeared in recent years: state credit enhancement may have maintained access to the credit market for districts serving economically disadvantaged students when the overall bond market is negatively affected by the demise of the private insurance industry.

4 Impact on School Districts

Within a state offering enhancement, the variation in bond enhancement comes from two sources: between- and within-district. Some districts consistently have high underlying ratings, relative to the enhanced rating the state program provides. Therefore, these districts have little incentive to participate in the state program. Contrasting never-enhanced with ever-enhanced districts explains three-quarters of the variation in bond enhancement. The rest of the variation comes from the fact that some bonds of a district participate in the state program while others of the same district do not. This is most likely due to eligibility rules requiring the total enhanced debt of a district or the projected debt service coverage ratio to stay below a cap. Unfortunately, because I do not have detailed data on the projected debt service of each district, and due to the regulatory discretion of state agencies in granting enhancement, it is not possible to precisely measure whether

[15] About 61 percent of districts with a 2000 child poverty rate in the lowest quintile have received state enhancement at least once from 2009 to 2019, while 85 percent of districts in the highest poverty quintile have done so. One may interpret the graph as a visualization of the intent-to-treat effects of state enhancement.

each bond is eligible for a state enhancement program or not.[16]

As state enhancement is not randomly assigned, I focus on controlling for endogenous factors correlated with both state enhancement and the outcomes of interest through the use of fixed effects (FE) or a generalized difference-in-differences approach, while being cognizant of the identifying variation underlying the empirical analysis. The first approach controls for state-level FE using only observations from districts that always or never obtain state enhancement for bond issuance, effectively comparing "enhanceable" versus "nonenhanceable" districts within a state.[17] This approach uses the between-district variations in state enhancement to identify its effect. The second and preferred approach uses district FE, which fully absorbs the between-district variation. That is, the effect of state enhancement is identified by comparing bonds (when examining the impact on interest rate) or district-years (when examining the impact on capital spending and test scores) with enhancement to others of the same district. The advantage of the district FE approach is that all time-invariant, district-specific factors, such as underlying socioeconomic conditions and institutional history, are controlled for.

To the extent that omitted variables cause biases, the two approaches suffer biases in different directions and may generate upper and lower bounds that contain the true estimate. With state FE comparing never- and always-enhanced districts, districts with lower intrinsic creditworthiness are more likely to obtain enhancement; meanwhile, they are more likely to pay a higher interest rate and incur less capital spending per pupil. Therefore, the state FE approach may bias the estimate for bond interest rate upwards and that for capital spending downwards. Within a district across bonds/years, a district is more likely to receive enhancement when its debt outstanding is low, which may be associated with a lower interest rate on new bonds and more potential for new

[16]There are other reasons why a district obtains state enhancement for some bonds but not others. First, many states allow only some types of school bonds, mainly GO bonds, for enhancement. Among districts ever receiving state enhancement, 70% of their GO bonds are enhanced while only 12% of their nonGO bonds are. However, this is unlikely to be the prevailing reason for changes in enhancement status within a district, given that only 7 percent of ever-enhanced districts have issued both GO and nonGO bonds. Second, a district may not obtain state enhancement for some bonds because it perceives little need for enhancement in those circumstances. For example, when the district's intrinsic creditworthiness improves or the state program's rating decreases, the district may consider the diminished value of state enhancement to not justify the costs of obtaining enhancement. Only about 3% of ever-enhanced districts have had underlying rating change from being lower to being higher than the state program rating or vice versa. Lastly, some ad hoc reasons may explain the within-district variation of state enhancement. For example, a district may be in a rush to issue a bond to take advantage of favorable market rates and choose not to apply for state enhancement.

[17]This drops roughly 9 percent of all districts, or 14 percent of issuer districts, that have experienced changes in state enhancement status in bond issues.

capital spending. Therefore, the district FE may bias the estimate for interest rate downwards and that for capital spending upwards. I present below the empirical specifications and results, along with robustness checks to further support a causal interpretation of the estimates.

4.1 Impact on School Bond Interest Rate

I first examine the impact of state enhancement on school district bond interest rates using bond-level data. Specifically, I estimate the following regression for district i with bond c issued in year T on date t:

$$yield_{ict} = \theta Enhanced_{ct} + \delta X_{ct} + \lambda_T insured_{ct} + \rho D_{it} + \mu_{i/s} + \psi_t + \epsilon_{ict} \tag{1}$$

where $yield_{ct}$ is the duration-matched yield spread, calculated as the difference between the municipal bond yield and the same duration U.S. Treasury yield.[18] Moreover, $Enhanced_{ct}$ represents whether the bond receives state credit enhancement. A negative estimate for θ would suggest that state enhancement is associated with a reduced interest rate.[19]

The regression also controls for bond characteristics X_{ct} commonly known to affect bond pricing, including log principal amount, maturity, whether a bond is GO, callable, competitively sold in the underwriting process, bank qualified, refunding, exempted from federal income taxation, and subject to tax credit provisions. I also include a series of indicator variables representing whether a particular underlying rating is assigned. The underlying rating controls are essential because the intrinsic creditworthiness of the bond is related to both the use of state enhancement and interest rate. Because the role of private insurance has been evolving after the Great Recession, λ_T allows the coefficient of bond insurance to differ each year. One can further control for district-level

[18] The rationale for using the yield spread as opposed to the initial offering yield is that the yield curve may change over time. That is, the relationship between bond characteristics and yield may be time-varying and time fixed-effects are insufficient at controlling for such changes. Following Gürkaynak, Sack, and Wright (2007), I calculate the yield on "synthetic" Treasury securities with maturity dates and coupon payments matching the municipal bond of interest. The spread is then computed relative to the yield on the synthetic Treasury security. Other studies, such as Gao, Lee, and Murphy (2019) and Yang (2019), take a similar approach in estimating the impact on municipal bond interest rates.

[19] Recent literature on two-way fixed effects (TWFE) suggests that the resulting estimate may not represent the true "average treatment effect" if the effect is heterogeneous over time or across units. With interest rate as the outcome, investors are unlikely to consider whether a previously issued bond of the same issuer is enhanced by the state. That is, state enhancement has only instantaneous effects but no dynamic effects on interest rates. However, heterogeneous effects across units as opposed to over time remain an issue. The DID_M estimator proposed by De Chaisemartin and d'Haultfoeuille (2020), which uses the not-yet-treated but not the already-treated as the comparison, generates similar findings. See Appendix B for more details.

characteristics D_{it} that may influence how investors perceive a bond's creditworthiness, including log enrollment, per-pupil revenue, expenditure and debt outstanding, as well as the child poverty level and the share of Black and Hispanic students in the district from the school year preceding the bond sale. Lastly, ψ_t are issue date fixed effects. Standard errors are clustered at the state level with state FE μ_s or at the district level with district FE μ_i.

Table 3 presents the results on bond interest rate. Column 1 reports the baseline results from equation 1 with state FE. State enhancement is associated with a reduction of 0.145 percentage points, or 14.5 basis points, in interest rate. Column 2 replaces the state FE with state-by-year FE, more flexibly allowing for state-specific trends in school bond interest rates. The point estimate decreases slightly in magnitude to -13.8 basis points. Column 3 includes district FE instead of state FE but excludes district covariates, generating a coefficient estimate of -13.4 basis points. Column 4 is the preferred specification, with district FE and controls. Receiving state enhancement is associated with a 14.1-basis point reduction in interest rate, which is about 6 percent of the average rate paid on school bonds during the period. This finding is robust to including GO bonds only (column 5), excluding refunding bonds (column 6), or excluding bonds issued by Texas school districts, which represent one-third of districts ever enhanced by state programs and consistently receive a triple-A enhanced rating (column 7). Taken together, the results suggest that state enhancement helps school districts secure substantial interest savings.

Moreover, the state FE and district FE approaches generate very similar estimates, lending support to a causal interpretation of the finding. One threat to the preferred identification strategy with district FE is that if a district has too much debt outstanding and becomes ineligible for state enhancement, unobserved factors causing excessive debt may affect interest rates as well. The potential endogenous causes of within-district variation in state enhancement, such as excessive use of debt, should be reflected in the underlying rating. Therefore, I conduct a robustness check by including only districts experiencing no change in the underlying rating.[20] Appendix Table D3 shows that the findings remain unchanged.

[20]This approach still suffers from a few problems. District may possess information that is unknown to rating agencies. This is less of a problem for testing the impact on interest rates, as investors likely also lack such information, but could lead to omitted variable bias when examining district spending and test scores. Moreover, rating agencies only assign a rating when districts issue bonds. I may fail to exclude a district not because it experiences no changes in creditworthiness, but because it does not issue bonds often.

[Table 3 about here]

The descriptive statistics have shown that districts serving economically disadvantaged communities have lower underlying ratings on average. State enhancement programs are more likely to provide a rating elevation for them, which potentially leads to more interest savings. Column 8 adds to the baseline specification two interaction terms: first between the state enhancement indicator and a dummy for the medium tercile in baseline poverty, and then between state enhancement and high-poverty tercile. State enhancement leads to a 12.5-basis point reduction in the interest rate paid by districts in the lowest poverty tercile. The interaction with the high-poverty tercile is statistically significant at 5% level and suggests an additional reduction of 5.5 basis points for high-poverty districts. In terms of interest cost savings, state enhancement is most beneficial to districts serving the most economically disadvantaged communities.

To further explore the impact of rating elevation, I calculate the difference between enhanced and underlying ratings indexed to a scale same as the x-axis in Figure 1, and based on this continuous difference, create a series of indicator variables in column 9. For example, "diff1" means that the enhanced rating is one notch higher than the underlying rating. The last indicator "diffmax" equals one when a bond does not have an underlying rating. The "enhanced" variable now represents the impact on the left-out group: bonds enhanced by state programs and have an underlying rating equal to or higher than the enhanced rating. The coefficient estimate for this variable is statistically indistinguishable from zero, suggesting that state enhancement does not affect interest rates when the enhanced rating does not exceed the underlying rating. The interactions between state enhancement and rating-elevation indicators show that, as the enhanced rating increasingly exceeds the underlying rating, state enhancement is associated with increasing interest savings. A one-notch rating elevation relates to a yield reduction of 6.4 basis points, and all the way up to a 39-basis point decrease for rating elevations in the range of 5 to 9 notches. The only exception to this monotonic relation between rating elevation and interest rate reduction comes from bonds without an underlying rating. For this group, the state enhancement is associated with a 27.5-basis point decrease, placing it between the effects of 3- and 4-notch rating elevations. Lastly, column 10 shows that intercept programs, as the left-out group, are associated with an 8-basis point decrease in school bond interest rate, while the other three types of programs provide additional reductions.

All permanent fund programs have triple-A ratings and unsurprisingly generate the largest interest rate reductions. On average, guarantee and appropriation programs offer similar enhanced ratings as intercept programs; therefore, the additional interest savings associated with guarantee and appropriation programs may suggest that committing state revenue other than educational aid is perceived favorably by bond investors beyond rating considerations.

4.2 Impact on Capital Spending

While state credit enhancement is associated with lowered interest rates, the reduction in interest rate may not necessarily lead to increased capital spending, as factors beyond borrowing cost considerations may influence districts' decision to invest in capital projects. In this section, I explore the impact of state credit enhancement on district capital spending. Specifically, for district i located in state s in year T, I estimate the following regression using district-year panel data:

$$CapitalExpense_{iT} = \beta Enhanced_{i,T \geq T_0} + \sum \zeta_\tau Issued_{i\tau} + \alpha D_{i,2000} + \nu_i + \pi_{sT} + e_{iT} \qquad (2)$$

where T_0 represents the first year the district receives enhancement on its bonds issued between 2009 and 2019. Thus, $Enhanced_{i,T \geq T_0}$ equals 1 for all district-year observations following the first state enhancement and I focus on the overall effects if a district issues multiple enhanced bonds.

A positive estimate of β would suggest that state enhancement leads to a higher per-pupil capital spending than bond issuance not enhanced by a state program.[21] In an alternative event-study specification, I allow the coefficient of $Enhanced$ to vary based on $\tau^* = T - T_0$, to capture any time-varying effects of state enhancement. When $\tau^* < 0$, the coefficients of $Enhanced_i \tau^*$ represent a pre-trend test, that is, whether the capital spending of districts that later receive state enhancement evolves differently from those not enhanced.[22] For each $\tau^* \geq 0$, the coefficient estimates the impact of receiving state enhancement τ^* years prior. A caveat is that I have only detailed data on state

[21] Again, the TWFE estimator is problematic when it compares newly-treated to already-treated units and when the effect is heterogeneous over time and across units. The already-treated units, which are still actively responding to the treatment, are not valid counterfactuals for what would have happened in the absence of the treatment. For example, districts issue enhanced bonds to pay for specific capital projects, and this may cause an increase in capital spending that only lasts until the capital project is completed. When the spending resumes to pre-treatment levels, the decrease is subtracted from a TWFE estimate, biasing the estimate upwards. Appendix B shows that, using the decomposition method in Goodman-Bacon (2021), little of the TWFE estimate is driven by the problematic comparison with already treated units; the alternative DID_M estimator generates similar results.

[22] I combine $\tau^* < -5$ to one indicator.

enhancement starting in 2009. For example, when the year of observation T is 2009, I do not know whether a district's bond issued in 2008 is enhanced by the state or not. Therefore, I limit the sample to $\tau^* <= 3$ and $T >= 2012$. While I will only be able to estimate short-term effects within four years following the first state-enhanced bond issuance, all post-treatment indicators are observed. I choose the three-year window because (1) the larger τ^* is, the smaller the sample and thus statistical power will be; and (2) capital projects are usually completed within four years after the corresponding bond is issued.

Regarding the controls, the vector $Issued_{i\tau}$ represents whether the district has issued bonds in the past, where τ is the difference between the year of observation T and the year of bond issuance.[23] Bond issuance history affects capital spending, regardless of whether the bonds are enhanced. The vector $D_{i,2000}$ controls for baseline district characteristics in 2000 including log enrollment, revenue, spending, and debt per pupil, child poverty rate, and the share of Black and Hispanic students. While the baseline district characteristics are absorbed by district FE ν_i in the preferred specification, they are controlled for in the specification with only state-by-year fixed effects π_{sT}. Standard errors are clustered on the district (state) level with (without) district FE. The regression is weighted by each district's average enrollment over time.

Before presenting the estimation results from equation 2, I conduct analyses to understand which factors are correlated with the within-district variation in state enhancement. I limit the sample to district-year observations from the year prior to a bond issuance in districts that have ever received state enhancement. I regress each district characteristic on an indicator of receiving state enhancement the year after, while controlling for issuance history that may impact the observed district characteristics, district FE, and state-by-year FE. If on average, districts are more likely to obtain state enhancement when possessing certain characteristics, one would observe a statistically significant relationship between later enhancement and these district characteristics. Table 4 shows that districts have statistically significantly lower debt outstanding prior to issuing enhanced than unenhanced bonds. This confirms information from state officials and documents that some states do not grant enhancement if a district's existing debt or projected future debt service costs are too high. Importantly, none of the other district characteristics, including the outcome of interest,

[23] While state enhancement status is available only starting in 2009, the data on bond issuance is available since 2000. Therefore, τ may be different from τ^*.

per pupil capital spending, is predictive of the subsequent receipt of state enhancement. This lends support to the conditional exogeneity of state enhancement when examining capital spending outcomes with the preferred district FE specification.

[Table 4 about here]

The pre-enhancement coefficients from the event study, as presented in the upper panel of Figure 3, are statistically indistinguishable from zero, providing reassurance to the identification strategy. The figure also shows that, after receiving state enhancement, districts spend more on capital projects. The effect increases from the year of bond issuance to the following two years and then decreases afterward. Capital projects take time to complete after capital is raised from bond issuance, but once completed, capital spending resumes to a lower level.

[Figure 3 about here]

Table 5 presents the results on district capital spending based on equation 2. Columns 1 and 2 compare always and never enhanced districts within a state: column 1 includes state FE and year FE, while column 2 includes state-by-year FE. State enhancement is associated with an average increase of $309 to $337 in annual per-pupil capital spending in the four years following enhancement. The rest of the columns include district FE, thus identifying the impact of state enhancement by comparing enhanced and unenhanced bonds of the same district while controlling for state-specific changes in average school capital spending through the inclusion of state-by-year FE. Column 3 shows that, after receiving state enhancement, districts spend on average $349 more in per-pupil capital spending in each of the following four years. Note that the coefficient for state enhancement estimated with state FE is smaller in magnitude than those estimated with district FE. Because more affluent districts are more likely to spend on capital projects and less likely to participate in state enhancement programs, any omitted variables arising from controlling for state FE bias the estimate toward zero. On the other hand, because districts in years of low existing debt are more likely to obtain state enhancement but also more likely to increase capital spending, any omitted variables arising from controlling for district FE bias the estimate upwards. As a result, the true estimate is likely between $309 and $349.

[Table 5 about here]

Some quick calculations are required to interpret the magnitude of the estimate. One shall not compare the estimate directly to the average annual capital spending because capital spending concentrates in the years immediately following bond issuance while the physical assets acquired last for many years. With an estimated annual effect of $309 to $349, the total increase in per-pupil capital spending during the four years post-enhancement period is $1,236 to $1,396. The average maturity of a bond issue is 15.6 years, and debt management best practices suggest that the useful life of the capital asset should equal the maturity of the bond. Jackson and Mackevicius (2021) assume a useful life of construction projects to be 50 years and other projects to be 15 years. Taking the range of 15 to 50 years, I spread the total increase in capital spending associated with state enhancement over the capital asset's lifespan, generating an annual increase of $26 to $88 per pupil. That represents a 2% to 7% increase in annual capital spending induced by state enhancement.

Columns 4 to 7 test the sensitivity of the estimate in column 3. Column 4 additionally controls for baseline district characteristics interacted with year dummies and the estimate remains similar in magnitude. Column 5 excludes bonds issued in Texas and the point estimate drops to $266 but remains statistically significant at the 0.1% level. The decrease in the estimate is unsurprising given that the Texas program offers a very high enhanced rating and thus may have a relatively large impact on capital spending. I have so far included all districts in estimating equation 2. Districts that did not issue any bonds during the period may be different from bond issuers, in terms of the relationship between observable district characteristics and capital spending patterns. Column 6 shows that the coefficient estimates are robust to the exclusion of nonissuers. Column 7 shows that the point estimate is larger when no enrollment weight is applied.[24]

Earlier results show that districts in the highest poverty tercile receive additional reductions in interest rates. I now test whether the lowered rates translate into more capital spending. Column 8 adds to equation 2 two interaction terms, respectively between $Enhanced_{i,T>T_0}$ and an indicator for the medium or highest baseline poverty tercile. Compared to the lowest poverty tercile, districts in the highest tercile increase per pupil capital spending by $335 more in each of the four years

[24]The results are robust if I include only districts experiencing no change in the underlying rating during the observation period, as shown in Appendix Table D4. Instead of state enhancement, the increase in capital spending estimated using the full sample may be due to enhanced districts experiencing relative improvement in financial condition, which should be reflected in increases in the underlying rating. By limiting the sample to only districts experiencing no changes in underlying ratings, the robustness check suggests that the increase in capital spending is unlikely due to improvement in financial condition.

following state enhancement (p value=0.065), and those serving the medium poverty tercile increase the spending by $316 more (p value=0.047). The lower panel in Figure 3 presents the event study version of this analysis. Given that, on average, higher-poverty districts spend less on per-pupil capital spending, state enhancement causes not only more capital spending in terms of dollar amounts but also a larger rate of increase among districts serving economically disadvantaged communities. To what extent does state enhancement contribute to the narrowing gap in capital spending reported in Biasi et al. (2021)? The capital spending gap in program states was $300 in 2009 and disappeared afterward. A back-of-the-envelope estimate suggests that state enhancement narrows the gap by $74, or a quarter of the total reduction.[25]

Lastly, using the specification in equation 2 with district FE, I examine whether enrollment level, student composition, and operational spending per pupil respond to state enhancement. As districts take on more debt and spend more on capital projects, they may attract different numbers or types of students; one may also be concerned about the additional capital spending crowding out operational spending. Results presented in Appendix Table D5 show no statistically significant changes in enrollment or child poverty rate in the districts after receiving state enhancement. The coefficient estimate for operational spending per pupil is $82, representing a 0.8% increase, and has a p-value of 0.104. Therefore, the additional capital spending resulting from state enhancement generates very little, if any, spillover to operational spending.

4.3 Impact on Test Scores

Increased capital investment arising from state enhancement may contribute to a more productive learning environment, leading to improvements in academic performance. In this section, I estimate equation 2 with average student test scores as the outcome variable.

Panel A of Table 6 shows that across test subjects and grades, state enhancement is not associated with statistically significant changes in test scores.[26] The magnitude of the point estimate is also small. Event study results for grades 4 and 7 in Figure 4 show no discernible pretrends

[25]During the analysis period, 46% of low-poverty districts and 56% of high-poverty districts received enhancement. Applying the point estimate in column 8, an average low-poverty district increases capital spending by $157*4 year*46%=$289. Spreading this over the 11 years of the analysis period, the annual increase is $26. Similarly, the annual increase for an average high-poverty district is (157+335)*4*56%/11=$100.

[26]The alternative DID_M estimator, which uses only the not-yet-treated as the comparison, generates similar findings. See Appendix B for details.

or over-time effects.[27] The finding is in line with evidence from studies showing the null effect of capital spending on student outcomes (Cellini et al., 2010; Martorell et al., 2016; Brunner et al., 2022; Baron, 2022). However, as suggested by Jackson and Mackevicius (2021), capital spending increases are often small, which makes it difficult to statistically identify a nonzero effect. Their meta-analysis suggests that a $1,000 annual increase in per-pupil capital spending during a four-year period increases test scores by 0.0341 standard deviations (SD). Using my upper-bound estimate of $88 increase in annual capital spending associated with state enhancement, one would expect test scores to increase by about 0.003 SD (=0.088*0.0341). This is much smaller than the effect size I could reject, given the standard errors reported in Table 6 in the range of 0.017 to 0.023.

[Table 6 about here]

[Figure 4 about here]

Other studies have shown capital spending to exert a positive impact on student outcomes in certain circumstances. First, academic performances may decrease in the construction phase but increase in subsequent years, indicating short-term disruptions in learning followed by long-term benefits (Rauscher, 2020; Conlin & Thompson, 2017). Panel B of Table 6 estimates the impact of state enhancement in the seven years post-enhancement (thus with a shorter panel). The null effect on test scores still holds. Second, capital spending may be more likely to positively impact student outcomes when it is used to construct new school facilities and to serve students in disadvantaged socioeconomic status (Neilson & Zimmerman, 2014; Lafortune & Schönholzer, 2021). While the data do not allow me to differentiate between bonds issued to finance new construction versus other uses, Table D6 shows that state enhancement does not cause consistently different improvements in test scores between districts with the amount of enhanced issuance per student in the highest quartile and those pursuing smaller capital projects. Table D7 shows that across districts in all three poverty terciles, state enhancement is not associated with changes in test scores.

[27] Capital spending might lead to other improvements in student outcomes that are not captured by math and reading test scores. Lacking comprehensive data, I do not test these potential impacts in this paper.

5 Policy Implications and Conclusion

This paper provides the first nationwide empirical analysis of state credit enhancement programs for school districts that currently exist in 24 states. The programs on average reduce the interest rates on long-term district bonds by 14 basis points. Because the market rate was notably low during the period of analysis, the interest-reduction effect may be even higher in a high-interest environment. The magnitude of reduction is directly correlated with how much higher the state program's rating is than the district's rating. The descriptive statistics show a strong correlation between a district's own rating and the local poverty rate. As a result, state enhancement causes a larger interest rate reduction for districts serving more economically disadvantaged students. State enhancement is more accessible to those districts than private insurance. Although some states require, as part of the application process, districts to provide budget projections showing the capacity to repay the debt, they do not exclude districts with low credit ratings.[28] Nor is the enhancement fee assessed based on the district's creditworthiness. In contrast, districts with low underlying ratings have to pay high insurance premiums if a private insurer is willing to insure their debt at all.

I analyze the share of bonds issued by school districts in nonprogram states from 2009 to 2019 that could have benefited had such a program existed. Among the 26 nonprogram states, only the 17 states with independent districts that have issued bonds during this period are included. The rating agencies usually assign the state's rating for a guarantee program, a rating equal to or one notch below the state's rating for an appropriation program, and a rating one to two notches below the state's rating for an intercept program. I obtain the states' GO ratings and focus on "enhanceable" district bonds with an underlying rating, and if insured, an enhanced rating, that is lower than the state's rating. Column 1 of Table 7 shows the principal amount of such bonds, scaled by the state population and expressed in thousands of dollars. The per capita enhanceable amounts range from $30 in Connecticut to $2,350 in Nebraska, and are not dramatically different from the enhanced amounts in program states reported in panel B of Appendix Table C1. The total amount of "enhanceable" bonds in the 17 states is $56 billion.

[28] Wyoming limits enhancement to bonds of investment-grade quality. This is a low bar: from 2009 to 2019, only 0.06% of school district bonds issued had noninvestment grade ratings.

[Table 7 about here]

I use the interest rate reduction effects of state enhancement to estimate the potential net-present-value savings in nonadopter states. Specifically, I calculate the notches of rating elevation if a state program were in place and use the estimates from column 9 of Table 3 to calculate the increase in bond price as a result of the rating elevation. Column 2 of Table 7 shows that implementing a credit enhancement program carrying the state's rating could save school districts in the nonadopter states up to $1 billion over the 11 years, or 2 percent of the "enhanceable" par amount. The potential savings vary across states. The saving is larger among states with many school districts and large amounts of school bond issued, such as Florida. The saving is lower among states with low ratings, such as Connecticut, because a hypothetical credit enhancement program could only provide value added to a limited set of districts with even lower ratings.

States may hesitate to commit own resources and provide a standing guarantee or appropriation for credit enhancement. Columns 3 and 4 show how the estimated savings would change if the hypothetical enhancement program's rating is set to be one or two notches below the state's rating, i.e. the likely rating for an intercept program. The estimated savings are reduced but still significant. Lastly, many of the adopter states only provide enhancement to GO bonds, likely to limit the risk exposure of the state. Column 5 of Table 7 restricts the enhanceable bonds to GO bonds and column 6 to GO bonds that are not refunding existing debt. With the most restrictive assumption, column 6 shows that enhancement programs have the potential of generating $383 million in savings among nonadopter states from 2009 to 2019.

As state enhancement reduces interest rates, school districts are able to invest more in capital projects. The empirical evidence suggests that, compared to unenhanced bonds, enhanced bonds lead to up to 7 percent more capital spending. The increase, in both percent and dollar terms, is larger for districts serving areas with higher poverty. State enhancement helps narrow the capital investment gap between districts in richer and poorer neighborhoods. However, I find no empirical evidence that state enhancement is associated with higher standardized test scores.

In sum, state enhancement programs require no direct financial assistance from the state, but can generate meaningful benefits for school districts. Is this too good to be true? I discuss below the potential costs of enhancement programs. Providing enhancement, especially through committing

own-source revenue, may expose the state to local district risk and increase the interest rate on the state's debt. As shown in Appendix C, using data on state GO bonds, I fail to establish a relationship between state GO interest rate and the amount of school bonds enhanced, even for states with relatively high levels of enhanced debt. It is useful to interpret this finding in the context of school finances in the United States. First, even though there are variations in districts' underlying ratings, both school district defaults and the triggering of state enhancement to prevent defaults are extremely rare. Providing an enhancement commitment is very unlikely to cause actual impairment to a state's finances. Second, all existing programs require school districts to pay the states back if state resources were used to prevent or cure a default. There has not been a bailout precedent. Third, school districts are "creatures of the state," subject to various oversight and monitoring by the state. Although most states do not regularly review the debt service capacity of school districts, many have statutory provisions that subject districts to state intervention or takeover if enhancement is activated (Yang, 2021).[29]

States may face other barriers to establishing enhancement programs. First, states with low credit ratings have limited potential in enacting programs that could benefit a large number of districts. While well-funded permanent funds may provide the strongest backing and carry ratings independent from the state's rating, the availability of state lands for revenue generation and the earmark of land-related revenue for educational purposes may not be present in all states. Second, interviews with state officials suggest that the program operation is not a large financial undertaking and can often be combined with other debt management programs to cut down administrative costs. However, the creation of a new program may require significant effort. Depending on the state laws, constitutional amendments, voter approval, and other legal considerations are needed (Ely, 2012). Lastly, Hsueh and Kidwell (1988) find that the Texas program has generated a negative spillover effect: because it increases the supply of triple-A debt in the state, it causes an increase in the interest rate paid by municipalities whose debt is ineligible for enhancement. Nevertheless, taking all localities in Texas as a whole, the authors estimate that the state program is associated with

[29]It is an open question whether extending state credit enhancement beyond regular school districts will endanger the creditworthiness of state governments. Accordingly to Ely (2012), seven states provide credit enhancement to county or city governments, and at least two states and the District of Columbia provide enhancement for charter schools. Expanding local debt covered under a state enhancement program could arguably increase the risk exposure of the states. For instance, charter schools that lack legacy school buildings may need state enhancement but are risky as they are dependent on charter renewal for continued operation.

an overall net saving: savings to guaranteed bonds with relatively low ratings exceed the costs to non-guaranteed triple-A bonds.

Empirical evidence in this paper shows that, if carefully designed, state credit enhancement programs can bring substantial savings to school districts without significant downsides, especially for districts serving economically disadvantaged populations that traditionally face barriers to the credit market. States with districts experiencing aging school facilities or rapid enrollment growth unaccompanied by a sufficient revenue inflow should explore such programs. Compared to other policies targeted at narrowing the school capital spending gap, such as state grants or revolving loans, credit enhancement programs require less direct spending by the state and allow for more local autonomy. The programs are increasingly important after the Great Recession and the decline of the private bond insurance industry. The 24 states currently operating enhancement programs demonstrate that a wide range of enhancements and program designs are available. Moreover, the activation of state enhancement, i.e. threat of a district default, is very rare. As the federal and state governments seek to support infrastructure investments among local governments, credit enhancement programs may be a valuable tool in the policy toolbox.

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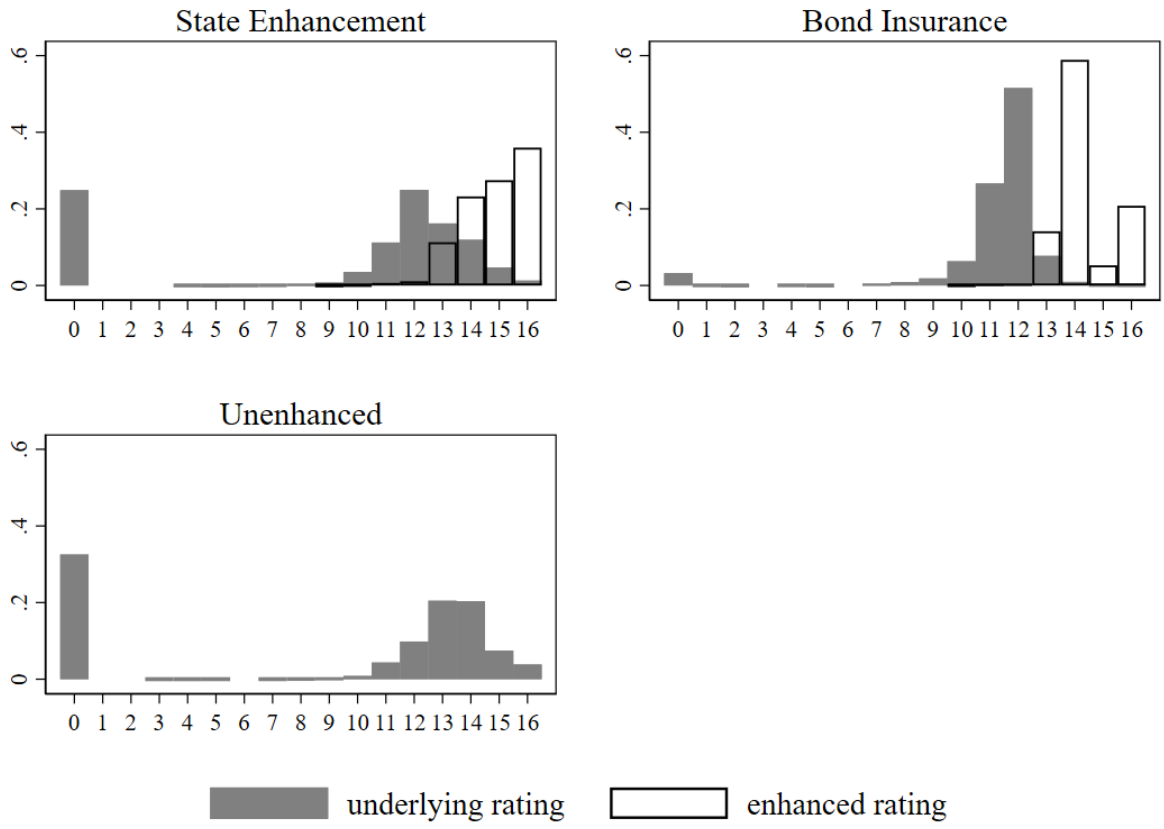
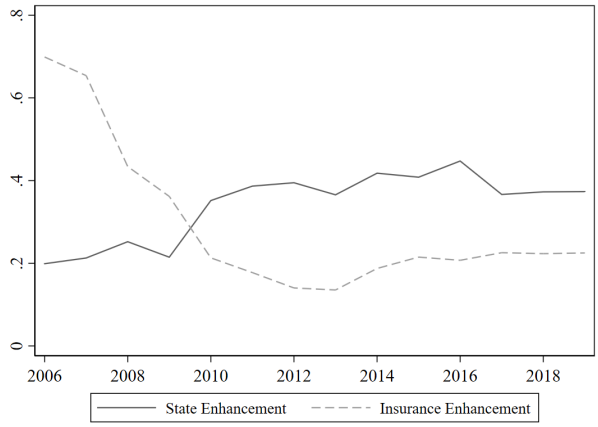
Figure 1: Distribution of Credit Ratings, by Type of Enhancement



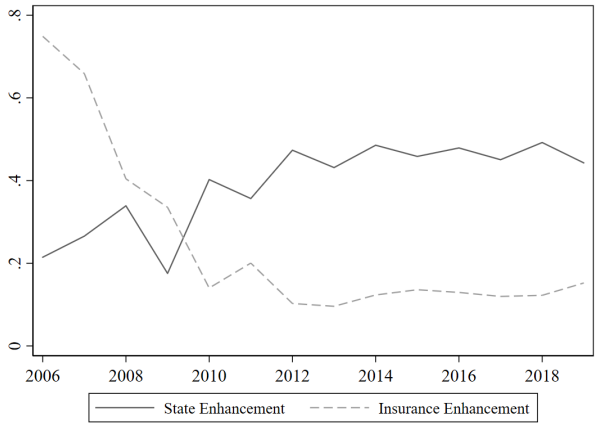
Notes: The x-axis represents the rating scale, where 0 is no rating (unrated) and 16 is triple-A rating. When two rating agencies both assign a rating and the ratings disagree, the rating shown here represents the higher of the two. The sample includes bond series issued by regular local school districts from 2009 to 2019.

Figure 2: Trends in the Share of School Bonds Enhanced and Per Pupil Capital Spending

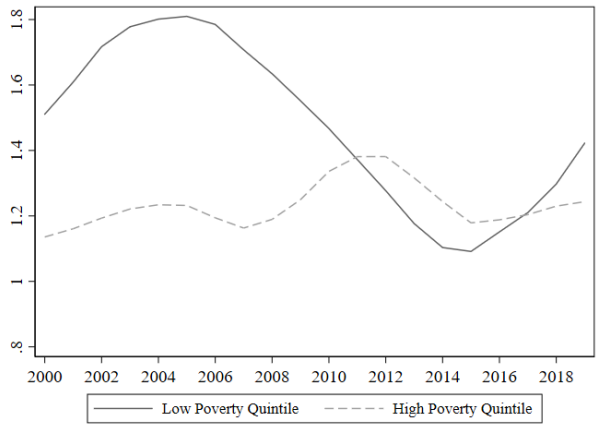
(a) Share of bond enhanced, by number of bonds



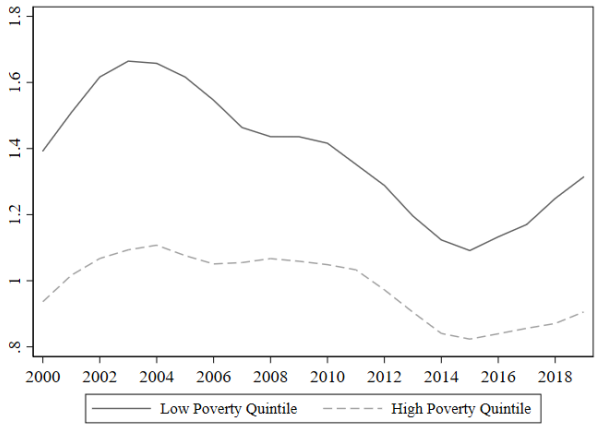
(b) Share of bond enhanced, by principal amount



(c) Capital spending in states offering enhancement



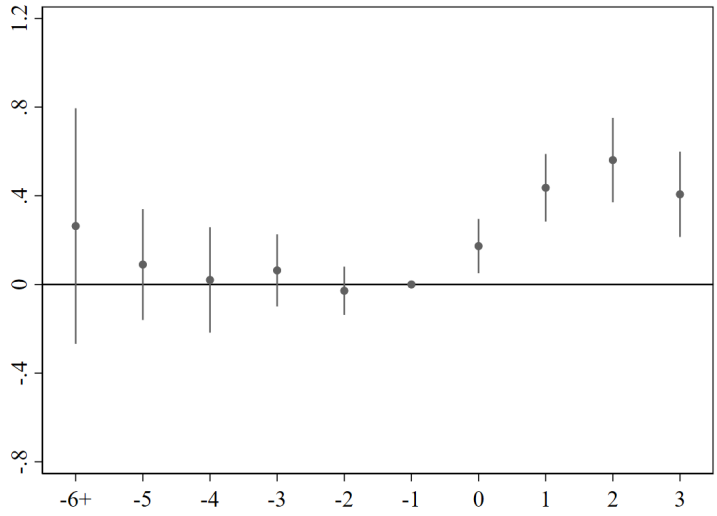
(d) Capital spending in other states



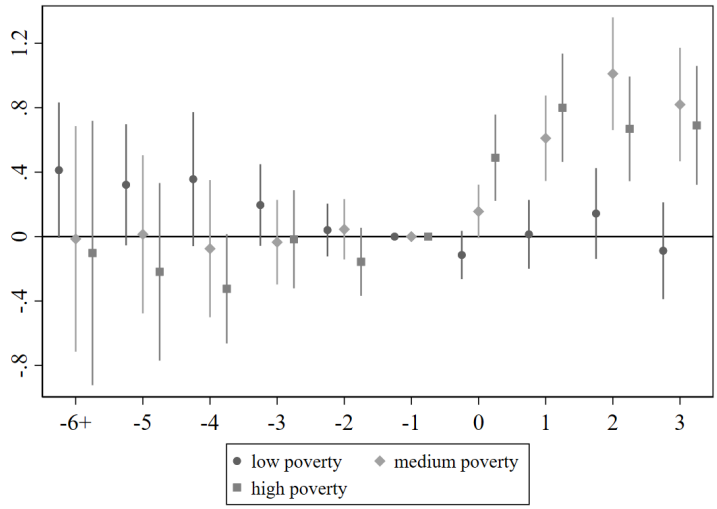
Notes: The upper panel shows the percent of school district bonds, with maturity longer than one year, issued in each year that are insured versus enhanced by state credit enhancement programs. Figure 2a calculates the share based on the number of serial bonds issued, while Figure 2b calculates the share based on the principal amount. The lower panel shows the trends in the 5-year moving average of per pupil capital spending by school districts in the highest versus lowest poverty quintile. The poverty quintiles are based on the districts in the sample in 2000, weighted by a stable enrollment count calculated as the geometric mean of each district's enrollment in all available years. Figure 2c (Figure 2d) represents the trends in states with (without) credit enhancement programs. Almost all state credit enhancement programs were adopted prior to 2000, except for New Mexico which adopted the program in 2003.

Figure 3: Impact of State Enhancement on Per Pupil Capital Spending

(a) Event study results

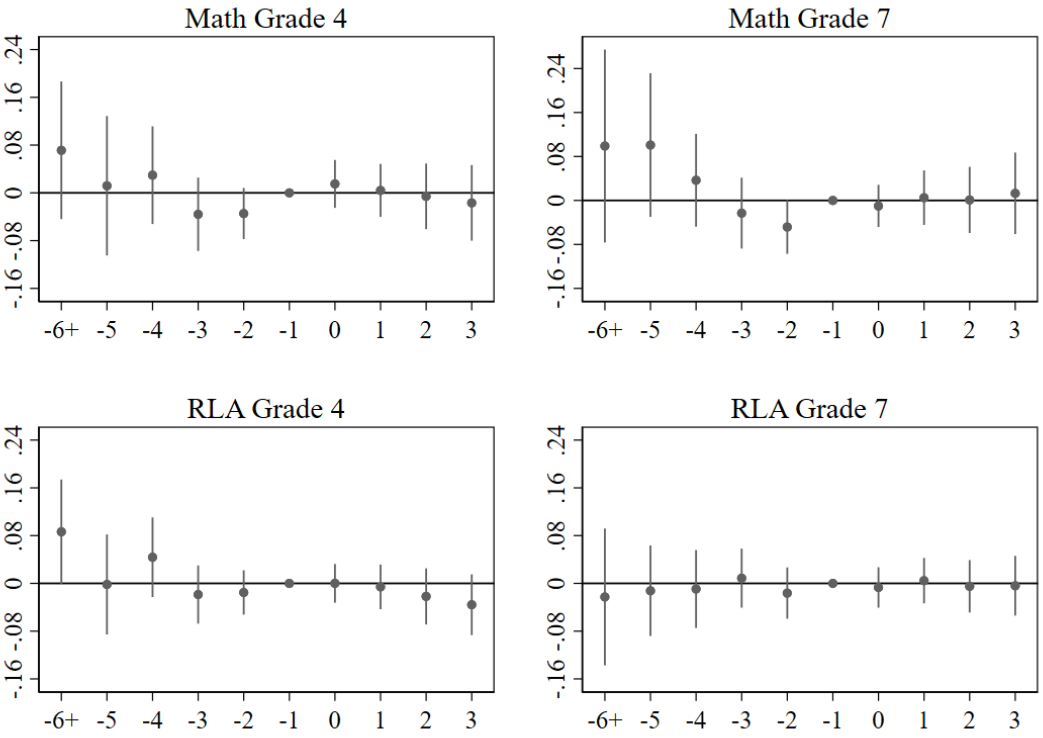


(b) Event study results, by baseline poverty tercile



Notes: The upper panel shows the event study analysis results based on the entire sample, while the lower panel shows results from districts in different terciles based on the 2000 child poverty rate. The x-axes represent year to the first state enhancement during the 2009-2019 observation period, with -6+ combining six or more years prior to the first enhancement. Per pupil capital spending is measured in thousands of 2019 dollars. The regression also controls for the full bond issuance history, district fixed effects, and state-by-year fixed effects. The lines represent the 95 percent confidence interval, with standard errors clustered at the district level.

Figure 4: Impact of State Enhancement on Test Score



Notes: The figures show the event study analysis results on math as well as reading and language art (RLA) test scores in grades 4 and 7. The x-axes represent year to the first state enhancement during the 2009-2019 observation period, with -6+ combining three or more years prior to the first enhancement. The regression also controls for the full bond issuance history, district fixed effects, and state-by-year fixed effects. The lines represent the 95 percent confidence interval, with standard errors clustered at the district level.

Table 1: State Credit Enhancement Programs for School Districts

State	Program	Type	Year Enacted	S&P	Moody's
Arkansas	School District Aid Intercept	Intercept	1997	Aa2	
Colorado	School District Intercept	Intercept	1991	AA	Aa2
Georgia	School District Intercept	Intercept	1991	AA+	Aa1
Idaho	School Bond Guaranty	Guarantee	1999	AAA	Aaa
Idaho	School Credit Enhancement	Appropriation	1999	AA+	Aa1
Indiana	School District Intercept	Intercept	1980	AA+	Aaa
Massachusetts	Qualified Bond Program	Intercept	1980	AA	Aa2
Michigan	School Bond Qualification and Loan	Guarantee	1964	AA	Aa1
Minnesota	School District Credit Enhancement	Appropriation	1993	AAA	Aa2
Missouri	Direct Deposit Program	Intercept	1995	AA+	Aa1
New Jersey	School Bond Reserve Act	Guarantee	1980	A-	A3
New Jersey	Qualified School Bond Program	Intercept	1976		Baa1
New Mexico	School District Credit Enhancement	Intercept	2003		Aa3
Nevada	School District Guarantee	Permanent fund	1998	AAA	Aaa
New York	Section-99B intercept	Intercept	1974		A1
North Dakota	School District Credit Enhancement	Intercept	2000	AA	Aa2
Ohio	School District Credit Enhancement	Intercept	1995	AA	Aa2
Oregon	School Bond Guaranty	Guarantee	1999	AA+	Aa1
Pennsylvania	Act 150 Intercept	Intercept	1975	A	A3
South Carolina	Credit Enhancement	Intercept	1976	AA	Aa1
South Dakota	State Aid Pledge	Intercept	1988	AA+	
Texas	Permanent School Fund Bond Guarantee	Permanent fund	1983	AAA	Aaa
Utah	School Bond Guaranty	Guarantee	1996	AAA	Aaa
Washington	School Bond Guarantee	Guarantee	1999	AA+	Aaa
West Virginia	State Intercept	Appropriation	1974	AA-	
Wyoming	School District Bond Guarantee	Permanent fund	1994	AAA	

Notes: The table covers state credit enhancement programs as of December 31, 2019 for K-12 school districts but not those exclusively for other types of local governments. It excludes credit enhancement programs for charter schools. It excludes programs such as bond banks and revolving loans that support local borrowing but do not issue bonds in school districts' own names. It also excludes programs that exist in law but are used on an ad hoc basis (New Hampshire) or inactive (Oklahoma). Kentucky has an intercept program but is excluded because it applies to only long-term leases. The S&P (Moody's) column represents the credit rating assigned by Standard & Poor's (Moody's) on the program as of the end of 2019.

Table 2: School District Baseline Summary Statistics

	All		Issuer: All	Issuer: Never enh.	Issuer: Ever enh.	Underlying Rating: No	Low	Medium	High
enrollment, logged	7.27	(1.30)	7.44	7.39	7.51	6.37	7.28	8.06	8.44
Black&Hispanic share	0.17	(0.24)	0.17	0.15	0.19	0.10	0.19	0.18	0.18
poverty	0.14	(0.09)	0.13	0.12	0.14	0.16	0.14	0.10	0.08
total revenue	11.98	(3.30)	11.77	11.90	11.59	10.52	11.56	12.44	13.27
federal transfer	0.77	(0.99)	0.66	0.65	0.69	0.79	0.73	0.54	0.47
state transfer	6.01	(2.26)	5.85	5.61	6.19	5.97	6.46	5.34	4.31
own-source revenue	5.21	(3.68)	5.26	5.65	4.71	3.76	4.37	6.57	8.49
total spending	12.12	(3.69)	12.00	12.03	11.96	10.51	11.76	12.85	13.62
operational spending	10.81	(3.02)	10.63	10.67	10.56	9.50	10.45	11.24	11.88
capital spending	1.31	(1.84)	1.38	1.36	1.41	1.01	1.31	1.61	1.75
debt outstanding	5.12	(6.23)	6.06	5.70	6.57	3.45	5.86	7.55	7.75
N	11,150		7,406	4,325	3,081	1,409	3,175	2,175	647

Notes: The baseline statistics are based on 2000 data. All financial variables are measured on the per pupil basis in thousands of 2019 dollars. Poverty measures the child poverty rate in the area served by the school district. The issuer sample includes only districts that have issued bonds during the period of academic year 2008-2009 to 2018-2019, which is further divided into those whose bonds are never subject to a state credit enhancement program ("Never enh.") versus those with at least one bond participating in a state program ("Ever enh."). The last four columns categorize the issuer districts based on their highest underlying ratings observed during the period. Low rating represents ratings below A+ (S&P) or A1 (Moody's); medium rating includes AA- and AA (S&P) or Aa3 and Aa2 (Moody's); and high rating includes AA+ and AAA (S&P) or Aa1 and Aaa (Moody's). When two rating agencies both assign a rating and the ratings disagree, the higher of the two is recorded.

Table 3: Impact of State Enhancement on District Bond Interest Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
enhanced	-0.145***	-0.138**	-0.134***	-0.141***	-0.118***	-0.163***	-0.121***	-0.125***	0.022	-0.081***
	(0.039)	(0.039)	(0.011)	(0.011)	(0.012)	(0.019)	(0.013)	(0.015)	(0.017)	(0.016)
enhanced x medium poverty								-0.008		
								(0.023)		
enhanced x high poverty								-0.055*		
								(0.025)		
enhanced x diff1									-0.064***	
									(0.018)	
enhanced x diff2									-0.160***	
									(0.020)	
enhanced x diff3									-0.253***	
									(0.022)	
enhanced x diff4									-0.302***	
									(0.024)	
enhanced x diff5to9									-0.392***	
									(0.030)	
enhanced x diffmax									-0.275***	
									(0.027)	
enhanced x permanent fund										-0.128***
										(0.026)
enhanced x guarantee										-0.057*
										(0.025)
enhanced x appropriation										-0.101*
										(0.041)
FE	state	state-by-year	district	district	district	district	district	district	district	district
bond covariates	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
district covariates	yes	yes	no	yes	yes	yes	yes	yes	yes	yes
nonGO bonds	yes	yes	yes	yes	no	yes	yes	yes	yes	yes
refunding bonds	yes	yes	yes	yes	yes	no	yes	yes	yes	yes
Texas bonds	yes	yes	yes	yes	yes	yes	no	yes	yes	yes
Observations	237,370	237,370	326,704	326,359	305,093	167,280	274,280	323,359	326,359	326,359

Notes: Bond interest rates are expressed in percentage points. Each regression controls for issue date fixed effects. Bond covariates include log principal amount, maturity, whether a bond is general obligation (GO), callable, competitively sold, bank qualified, refunding, exempted from federal income taxation, and subject to tax credit provisions, a series of indicator variables representing whether the bond carries a particular underlying rating, as well as the interactions between year indicator and the bond insurance indicator. District covariates include log enrollment, per-pupil revenue, expenditure and debt outstanding, the child poverty level and the share of Black and Hispanic students in the school year immediately preceding bond issuance. The "diff" variables are indicator variables representing whether the difference between the state enhanced and the underlying ratings equals to a particular number (or falls into a specific range); "diffmax" equals 1 if a bond carries no underlying rating. Lastly, "permanent fund", "guarantee", and "appropriation" equal 1 if the state enhancement program is of each of the types. The first two columns are based on the sample of always and never enhanced districts, with standard errors clustered at the state level. Standard errors are clustered at the district level otherwise. *p<0.05, **p<0.01, ***p<0.001.

Table 4: Pre-State Enhancement Balance in Covariates

	Capital spending	Debt outstanding	Logged enrollment	Total revenue	Current spending	Poverty	Black&Hispanic share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
state enhanced	-0.097	-0.877***	-0.001	-0.004	-0.033	0.000	0.001
	(0.072)	(0.184)	(0.003)	(0.043)	(0.036)	(0.001)	(0.001)
Observations	8,404	8,404	8,404	8,404	8,404	8,399	8,404

Notes: Sample consists of district-year observations from the year prior to a bond issuance in districts that have ever received state enhancement. Each regression controls for bond issuance history (whether a bond has been issued in each of the prior year), as well as school district and state-by-year fixed effects. The indicator variable "state enhanced" equals 1 if the bond issued the year after is enhanced by the state. For state enhancement to be conditionally exogenous within a district, it should not be predicative of the prior year's district characteristics. Standard errors clustered at the district level are in parentheses. *p<0.05, **p<0.01, ***p<0.001.

Table 5: Impact of State Enhancement on Per Pupil Capital Spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
enhanced	0.309**	0.337**	0.349***	0.338***	0.266***	0.325***	0.587***	0.157
	(0.108)	(0.118)	(0.069)	(0.070)	(0.072)	(0.072)	(0.070)	(0.110)
enhanced x medium poverty								0.316*
								(0.159)
enhanced x high poverty								0.335
								(0.182)
FE	state+year	state-by-year (SY)	district+SY	district+SY	district+SY	district+SY	district+SY	district+SY
baseline trend	no	no	no	yes	no	no	no	no
all states	yes	yes	yes	yes	exclude TX	yes	yes	yes
all districts	yes	yes	yes	yes	yes	issuer only	yes	yes
weighted	yes	yes	yes	yes	yes	yes	no	yes
Observations	70,097	70,097	77,310	74,488	72,456	52,340	77,310	75,639

Notes: Each regression controls for baseline school district characteristics from 2000, which are absorbed by district fixed effects (FE) but not state FE. Column 4 additionally controls for all baseline district characteristics interacted with year dummies. Bond issuance history is also controlled for. The first two columns are based on the sample of always and never enhanced districts, with standard errors clustered at the state level. Standard errors are clustered at the district level otherwise. "Issuer only" includes only districts that have issued bonds between 2009 and 2019. When a regression is weighted, the weight applied is the geometric mean of district enrollment over time. *p<0.05, **p<0.01, ***p<0.001.

Table 6: Impact of State Enhancement on Test Scores

	Math, Grade						Reading Language Arts, Grade					
	3	4	5	6	7	8	3	4	5	6	7	8
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Four years following state enhancement												
enhanced	-0.006	0.021	0.018	0.017	0.010	-0.006	0.010	0.003	-0.004	-0.006	0.001	-0.017
	(0.019)	(0.021)	(0.020)	(0.023)	(0.023)	(0.028)	(0.018)	(0.017)	(0.017)	(0.018)	(0.017)	(0.018)
Observations	51,595	51,516	50,706	50,127	44,371	40,639	51,566	51,589	51,521	51,071	50,221	49,063
Panel B: Seven years following state enhancement												
enhanced	-0.011	-0.018	0.021	0.005	-0.035	-0.020	0.045	0.020	0.038	0.012	-0.052	-0.058
	(0.029)	(0.034)	(0.032)	(0.030)	(0.036)	(0.046)	(0.031)	(0.028)	(0.029)	(0.032)	(0.031)	(0.050)
Observations	30,710	30,514	29,838	29,516	26,021	23,381	30,505	30,377	30,072	29,919	29,047	27,798

Notes: Each regression controls for bond issuance history, school district fixed effects, and state-by-year fixed effects. Regressions are weighted by the geometric mean of district enrollment over time. Standard errors clustered at the district level are in parentheses. Panel B examines a longer state enhancement history and thus replies on a shorter panel/smaller sample size. Baseline standard deviation of test scores are from 2009. *p<0.05, **p<0.01, ***p<0.001.

Table 7: Potential Use and Saving of State Credit Enhancement in Nonadopter States

	Enhanceable par, state rating	Saving, state rating	Saving, state rating-1	Saving, state rating-2	Saving, state rating-2 & GO	Saving, state rating-2 & new money GO
	(1)	(2)	(3)	(4)	(5)	(6)
Alabama	0.61	48.06	37.09	14.86	14.49	9.31
Arizona	0.13	5.60	2.21	0.69	0.69	0.64
California	0.11	107.98	98.21	91.89	91.54	58.56
Connecticut	0.03	0.52	0	0	0	0
Florida	0.65	308.56	305.37	246.41	246.41	107.15
Illinois	0.22	86.35	85.07	80.18	68.82	29.69
Iowa	1.87	134.43	133.60	115.06	114.62	89.31
Kansas	2.16	87.39	66.40	36.45	36.35	11.39
Louisiana	0.38	17.98	11.79	3.23	3.23	2.04
Maine	0.05	0.50	0.04	0.04	0.04	0
Mississippi	0.34	10.45	7.87	6.22	4.59	2.52
Montana	1.55	35.56	34.23	28.19	27.26	24.32
Nebraska	2.35	72.95	70.62	43.75	37.00	16.34
New Hampshire	0.16	1.83	0.91	0.40	0.40	0.40
Oklahoma	1.69	37.88	32.63	28.37	28.37	28.24
Tennessee	0.04	4.62	4.12	2.90	0.32	0.29
Wisconsin	0.93	40.39	23.35	9.27	9.27	2.36
All	13.29	1001.04	913.48	707.91	683.41	382.55

Notes: The table presents the total amount of school district bonds ("enhanceable par") issued between 2009 and 2019 that could have benefited if a credit enhancement program were available in the nonadopter state, scaled by the state's population and expressed in $thousand, and the related net-present-value savings ("saving"), expressed in $million. To benefit from a hypothetical state program, the school district bond must have an underlying rating–and if privately insured, an insured rating–lower than the state's rating. The net present value of savings is obtained by calculating the increase in bond prices given the yield reduction effect estimated for each level of rating enhancement in column 7 of Table 3. Columns containing "state rating-x" further limits the calculation to bonds whose underlying rating (and insurance enhanced rating if insured) is more than x notches below the state's rating. Column 5 is further limited to general obligation (GO) bonds and column 6 to nonrefunding GO bonds. Of the 26 states that do not have credit enhancement programs for school districts, 9 are not included in the table. Alaska, Hawaii, Maryland, North Carolina, and Virginia do not have independent school districts that issue bonds in their own names. Districts in Delaware and Vermont did not issue bonds during the period. One district in Rhode Island issued bonds but the insured rating is the same as the state rating. Finally, districts in Kentucky issued bonds solely through separate facility finance corporations.

Appendices

A Study Scope and Sample Construction

Charter schools are excluded from this study. The federal Department of Education provides funding to public and private entities to support programs that offer credit enhancement to charter schools. State credit enhancement programs for charter schools, such as the Charter School Credit Enhancement Program by the Massachusetts Development Finance Agency and the Charter Financing Enhancement Program by the California School Finance Authority, have received funding from the federal government.

The paper focuses on regular local school districts so that I can connect bond-issuing information to district capital spending and test score outcomes. This means the analyses do not capture all bond issuance and capital spending for school purposes. First, I exclude bonds issued by city or county governments for dependent schools, because such bonds could finance multiple capital projects beyond school purposes. Second, school capital projects may be financed by independent authorities, such as school finance authorities or health and education facility authorities. These bonds are issued in the name of the authorities and it is often difficult to connect them to a particular school district. Third, supervisory unions and regional education services may issue bonds that benefit multiple regular districts within the region and are excluded.

To prevent outliers and to address the volatility in per-pupil measures, two exclusions are made in constructing the district sample. First, I exclude districts with an average enrollment of fewer than 100 students. This removes 6% of observations, with only 0.08% of total enrollment. Next, I exclude observations where the enrollment demonstrates a 15% change from both the years prior and after; if for a district more than one-third of annual observations are excluded under this criterion, all remaining observations from the district are excluded as well. This rids 1.5% of observations, with 0.5% of enrollment.

To address extreme outliers in the financial data that likely reflect reporting or coding errors, I follow the approach of Dee, Jacob, and Schwartz (2013). I drop observations where the real total revenue or spending per pupil was greater than 150 percent of the state-specific 95th percentile value or less than 50 percent of the state-specific 5th percentile value. This effectively excludes Hawaii and the District of Columbia where the entire jurisdiction is combined into a single district. Overall, the exclusion rids 2.4% of the district-year observations, with 0.6% of enrollment.

B Two-Way Fixed Effects and Alternative Estimator

The main estimates in the paper are based on two-way fixed effects (TWFE) estimators. The TWFE estimator regresses the outcome of interest (bond interest rate, per pupil capital spending, and average test score) on a treatment indicator, that is, whether enhanced by the state, and district and time fixed effects. Recent development in the literature shows that the resulting estimate may not represent the true "average treatment effect" if the effect is heterogeneous over time (e.g. dynamic effects as shown in event study graphs) or across units (e.g. differing effects for high-poverty versus low-poverty districts) (Goodman-Bacon, 2021; De Chaisemartin & d'Haultfoeuille, 2020). This is because the TWFE estimate is a weighted average of comparisons between (1) newly-treated units and not-yet-treated or never-treated units, and (2) newly-treated units and already-treated units.[30] The second type of comparison is not a "good comparison" with heterogeneous treatment effects. The already-treated units, which are still actively responding to the treatment, are not valid counterfactuals for what would have happened in the absence of the treatment. In the context of this paper, for example, districts issue enhanced bonds to pay for specific capital projects, and this may cause an increase in capital spending that only lasts until the capital project is completed. When the spending resumes to pre-treatment levels, the decrease is subtracted from a TWFE estimate, biasing the estimate upwards. Similarly, if the effects are not heterogeneous over time but across groups starting treatment earlier versus later, then the already-treated units may be subject to a different effect than the newly-treated units and serve as bad comparisons.[31]

First, to test the validity of a TWFE estimator in the context of this paper, I examine how much of the estimate comes from comparing newly treated to already treated units. Following the decomposition method in Goodman-Bacon (2021), I estimate equation 2 with district FE and decompose the resulting estimate into different pairs of comparisons.[32] The decomposition results show that 96 percent of the TWFE estimate comes from comparing never-treated to newly-treated districts, 1 percent from comparing districts treated earlier versus later, and the remaining 3 percent from the "within" comparison due to differences in time-varying covariates across districts experiencing treatment changes at the same time. This shows that little of the TWFE estimate is driven by the problematic comparison with already treated units but much relies on comparing against never-treated units.

[30] This is the case when the treatment is permanent, meaning that once a unit is treated, it will not become untreated in the future. If the treatment is transient, then comparisons are also made between units getting off treatment and not-yet-treated units, and between units getting off treatment and already-treated units.

[31] However, data in this paper do not seem to show that among districts ever enhanced by the state programs, those with a higher baseline poverty rate are more likely to receive enhancement earlier. Further, I have no reason to believe a priori that the effect of state credit enhancement changes over calendar years.

[32] Although the Goodman-Bacon decomposition reports the effect estimates from each pair of comparisons, it can only be estimated on a balanced panel. Therefore, the estimates are not strictly comparable to the estimates reported in the paper.

Even though the TWFE estimator may still be valid given that the estimate relies heavily on the comparison against never-treated units, I apply an alternative estimator to test the robustness of the findings. Because I have a binary treatment (whether enhanced by state program or not) in a staggered design (although treated at different times, once treated, a district remains treated), the DID_M estimator proposed by De Chaisemartin and d'Haultfoeuille (2020) is shown to be robust to heterogeneous effects. The DID_M estimator compares, over any two periods, the period-one-to-two outcome evolution of groups going from untreated to treated and of groups untreated at both dates. With a staggered design, the latter group is the not-yet-treated group. The estimator is unbiased for the switching-in groups at period 2 under a parallel trends assumption on the untreated outcome. As a result, the estimator enables me to estimate the effect in each of the four years following the treatment.[33] Because the DID_M estimator uses the not-yet-treated to infer the trends that would have affected the newly treated if their treatment had not changed, I also perform a pre-treatment placebo test to examine the validity of this assumption. The placebo estimator compares the outcome evolution from the two periods prior to any treatment change and tests if the outcome evolution is different between the treated and the not-yet-treated groups. If the placebo estimate is not significantly different from zero, it provides confidence that groups switching treatment do not experience different trends before the switch than the comparisons used to reconstruct the counterfactual trends.

Table B1 presents the effect estimates of state enhancement on school district per pupil capital spending using the DID_M estimator. First, the three pre-treatment placebo estimates are small in magnitude and statistically insignificant, suggesting that the not-yet-treated districts provide a valid counterfactual. The estimates for the four years following state enhancement are all positive and are statistically significant at 1% level one to three years after receiving enhancement. This confirms the finding that state enhancement leads to higher per pupil capital spending. Similarly, Table B2 presents the effect estimates of state credit enhancement on average test scores using the DID_M estimator. Confirming the main findings from the paper, the estimates for post-state enhancement indicators are largely small in magnitude and statistically insignificant at 5% level.[34]

So far, I have focused the discussion regarding the validity of the TWFE estimator and estimates from

[33] Other estimators are also available, following similar practices of avoiding using already-treated units as comparisons. Callaway and Sant'Anna (2021) aggregate units into cohorts that start receiving treatment at the same period, and compare the cohort outcome to the average outcome from the same period across units that remain untreated until the period of interest ends (i.e. certain years post-treatment). With staggered adoption and a binary treatment, the DID_M estimator, also using the not-yet-treated as controls, is identical to the treatment effect obtained from Callaway and Sant'Anna (2021) (De Chaisemartin & D'Haultfoeuille, 2022). Sun and Abraham (2021) propose an estimator of cohort-and-period specific effects that use never-treated groups or last-treated groups as controls. With the former control group, this estimator is identical to that proposed by Callaway and Sant'Anna (2021) (De Chaisemartin & D'Haultfoeuille, 2022). However, Sun and Abraham (2021) do not provide proof that the estimator is robust to the inclusion of covariates or using the not-yet-treated as controls.

[34] Two estimates from the table are statistically significant at 5% level. However, this could arise out of chance given that many coefficients are reported in the table.

Table B1: Impact on Per Pupil Capital Spending, de Chaisemartin and D'Haultfoeuille Estimator

	(1) coefficient estimate	(2) standard error
enhanced, $\tau^* = 0$	0.092	(0.047)
enhanced, $\tau^* = 1$	0.550^{***}	(0.126)
enhanced, $\tau^* = 2$	0.770^{***}	(0.141)
enhanced, $\tau^* = 3$	0.420^{**}	(0.133)
enhanced, $\tau^* = -1$	-0.063	(0.039)
enhanced, $\tau^* = -2$	-0.004	(0.058)
enhanced, $\tau^* = -3$	-0.064	(0.070)

Notes: The regression controls for bond issuance history and state-by-year fixed effects. The weight applied is the geometric mean of district enrollment over time. The estimator is the DID_M estimator proposed in De Chaisemartin and d'Haultfoeuille (2020). Further, τ^* represents the difference between year of observation and the first year of state credit enhancement. Thus, when $\tau^* < 0$, the estimates show the pre-treatment placebo test results. Standard errors are clustered at the school district level and estimated based on 100 bootstrap replications. *p<0.05, **p<0.01, ***p<0.001.

the alternative estimator on district outcomes as opposed to bond outcomes. When examining bond interest rate as the outcome variable, I argue that it is unlikely for investors to consider whether a previously issued bond of the same issuer is enhanced by the state. That is, state enhancement has only instantaneous effects but no dynamic effects. If a district issues a bond not enhanced by the state but a previously issued bond was enhanced, there is no reason to assume that the current bond investors will benefit from the previous enhancement for which it does not qualify, especially when controlling for covariates that may be affected by the previous issuance history. However, heterogeneous effects across units as opposed to over time remain an issue. Using the DID_M estimator, I obtain the alternative estimate for the instantaneous impact of state enhancement on district bond interest rate at -0.169 percentage points (standard error 0.041), which is statistically significant and similar in magnitude to the TWFE estimates reported in the paper.

Table B2: Impact on Test Score, de Chaisemartin and D'Haultfoeuille Estimator

	Math, Grade						Reading Language Arts, Grade					
	3	4	5	6	7	8	3	4	5	6	7	8
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
enhanced, $\tau^* = 0$	0.009	0.005	0.013	-0.017	-0.022	-0.038	0.021	-0.008	0.030	-0.020	-0.013	-0.041
	(0.020)	(0.024)	(0.020)	(0.024)	(0.020)	(0.027)	(0.023)	(0.018)	(0.017)	(0.020)	(0.017)	(0.021)
enhanced, $\tau^* = 1$	0.040	0.021	0.012	0.027	0.003	-0.013	0.029	0.017	0.027	0.011	0.001	-0.007
	(0.028)	(0.029)	(0.026)	(0.033)	(0.034)	(0.053)	(0.025)	(0.024)	(0.025)	(0.025)	(0.023)	(0.022)
enhanced, $\tau^* = 2$	0.046	0.069	-0.008	0.003	-0.012	-0.025	0.053	0.002	0.050	0.046	-0.023	-0.022
	(0.035)	(0.041)	(0.033)	(0.041)	(0.039)	(0.061)	(0.037)	(0.032)	(0.033)	(0.033)	(0.033)	(0.030)
enhanced, $\tau^* = 3$	0.072	0.071	0.004	0.002	-0.018	-0.029	0.068	0.033	0029.	-0.010	-0.020	-0.043
	(0.046)	(0.053)	(0.042)	(0.054)	(0.047)	(0.054)	(0.039)	(0.037)	(0.038)	(0.039)	(0.037)	(0.039)
enhanced, $\tau^* = -1$	-0.037	0.035	0.009	-0.013	0.031	-0.024	0.017	0.021	-0.028	-0.030	-0.014	-0.005
	(0.027)	(0.021)	(0.024)	(0.027)	(0.031)	(0.030)	(0.022)	(0.020)	(0.021)	(0.022)	(0.029)	(0.024)
enhanced, $\tau^* = -2$	0.047	0.039	-0.023	0.004	0.001	0.070*	-0.014	0.027	0.014	-0.025	-0.013	-0.021
	(0.028)	(0.025)	(0.024)	(0.027)	(0.029)	(0.029)	(0.026)	(0.026)	(0.026)	(0.025)	(0.023)	(0.023)
enhanced, $\tau^* = -3$	0.030	-0.006	0.031	0.035	-0.069	-0.039	0.025	0.012	-0.003	0.029	0.094*	-0.035
	(0.031)	(0.040)	(0.039)	(0.039)	(0.041)	(0.037)	(0.042)	(0.049)	(0.045)	(0.050)	(0.046)	(0.034)

Notes: Each The regression controls for bond issuance history and state-by-year fixed effects. The weight applied is the geometric mean of district enrollment over time. The estimator is the DID_M estimator proposed in De Chaisemartin and d'Haultfoeuille (2020). Further, τ^* represents the difference between year of observation and the first year of state credit enhancement. Thus, when $\tau^* < 0$, the estimates show the pre-treatment placebo test results. Standard errors are clustered at the school district level and estimated based on 100 bootstrap replications. *p<0.05, **p<0.01, ***p<0.001.

C Impact on State Interest Rate

State enhancement reduces the interest rate paid by school districts and increases district capital spending. However, the provision of state enhancement, especially through not simply intercepting school aid but committing the state's own financial resources, may negatively affect investors' perception of the state's creditworthiness and increase interest rates paid by the state government. I test whether this is true through a first-difference approach, examining the association between the change in state GO bond interest rate and the change in the amount of district bonds enhanced by the state.

I first calculate the cumulative amount of district bonds enhanced within each state on a given date. Because detailed enhancement data starts in 2009, I have no information on enhanced bonds issued prior to 2009. However, the issue is partially mitigated given that I am interested in the change in, not the total, amount of enhanced bonds.[35] In this calculation, I track enhanced bonds that have matured and been paid back from 2009 to 2019 and exclude them from the cumulative total.

Second, I obtain data on all GO bonds issued by state governments. A state could have multiple GO issues on the same date and each issue consists of multiple bonds with different interest rates, due to differing maturities and other bond features. To obtain a measure of state GO interest rate on a given date, I apply an "unexplained yield" approach. Specifically, the yield spread to a duration-matched synthetic Treasury bond is regressed on bond characteristics including logged par value, maturity, and whether a bond is insured, callable, competitively sold, tax exempted, or subject to a federal tax credit. The regression also controls for issuer and sale date fixed effects. The residual from estimating the regression represents the "unexplained yield" and is averaged for each date to provide a measure of state GO interest rate on that date.

Merging the enhanced bond data with state GO interest rate data, I compile a data set for the state-level analysis.[36] Panel A of Table C1 presents the summary statistics based on the entire sample, where a state is observed more times if it issues GO bonds more often.[37] Panel B provides information on the most recent

[35] I say "partially" because enhanced bonds issued prior to 2009 could mature during the observation period and affect the change in the amount of enhanced bond outstanding, which I cannot truly observe.

[36] This means the analysis includes only states that issue GO bonds. Out of the 24 states with credit enhancement programs for school districts, 6 did not issue GO bonds during the observation period. Colorado, Indiana, and South Dakota laws largely prohibit GO bonds. Nevada only issues "limited tax" GO bonds supported by statewide property tax levies, which are subject to constitutional and statutory limitations. North Dakota and Wyoming have issued revenue bonds through various authorities but no GO bonds.

[37] For control variables that may explain state interest rates, I include state population, per capita revenue, spending, and debt outstanding, all from the Census Survey of Government Finances. The size of the rainy day fund as a percent of general fund spending, from the National Association of State Budget Officers, proxies for the level of savings a state possesses. Lastly, the coincident index is a measure of state economic condition from the Federal Reserve Bank of Philadelphia, covering data on employment and income. The coincident indexes combine four state-level indicators to summarize current economic conditions in a single measure. The four indicators are nonfarm payroll employment, average hours worked in manufacturing by production workers, the unemployment rate, and wage and salary disbursements deflated by the consumer price index. The trend for each state's index is set to match the trend of its gross domestic product.

enhanced amounts outstanding by state and demonstrates considerable variations across states. The amount ranges from $20 per state resident in New York to $3,551 in Texas. Texas also leads others regarding the total amount of enhanced bonds in dollar terms; to put the $100 billion of enhanced bonds in context, the book value of the Texas Permanent School Fund is $47 billion as of 2020. Excluding Texas, the amount of enhanced school bonds in all states are generally on par or lower than the state's own long-term debt outstanding.

I take a first-difference approach to examine the relation between changes in the "unexplained yield" between two dates of state GO bond issuance and changes in the amount of enhanced school bonds outstanding. Specifically, I estimate the following regression for state s with GO bond issuance on date t:

$$\Delta y_{st} = \Delta EnhancedAmt_{st}\gamma + \Delta D_{st}\eta + \Delta t\kappa + \Delta u_{st} \tag{3}$$

where y_{st} represents the "unexplained yield" and $EnhancedAmt_{st}$ the cumulative amount of enhanced school bonds measured on a per capita basis. If enhancing too much school debt lowers the creditworthiness of a state, the estimate for γ should be positive. Through taking the first difference, the regression differences out any time-invariant, state-specific factors that correlated with state interest rates and enhanced amounts at the same time. Further, D_{st} controls for time-varying state characteristics, including state population, state finances, and the coincident index. Through Δt, I also control for the length of time elapsed since the last state GO bond issuance. The standard errors are clustered on the state level.

Column 1 of Table C2 presents the estimate of γ without controlling for covariates, while column 2 includes controls. Although positive, both estimates are small in magnitude and not statistically significant at the 5% level. Given the standard error, I am able to reject an effect up to 2 basis points for a $100 increase in the enhanced amount per state resident.[38] Column 3 excludes states without enhancement programs and compares states offering enhancement against each other. The coefficient estimate for the enhanced amount is larger in magnitude but statistically insignificant at the 5% level. Column 4 excludes states with a permanent fund program. The permanent fund is independent of the state's general financial resources and thus the state's own creditworthiness may be insulated from providing credit enhancement using the fund.[39] The coefficient estimate is very similar to the baseline in column 2. Finally, it is possible that, while on

[38]Similar to TWFE, the first-difference estimator may suffer from the heterogeneous effect problem: states with a different level of enhanced school bonds, i.e. different intensity of treatment, may not be a good comparison. The empirical solution embedded within the DID_M estimator, however, requires some modification with the continuous treatment. With the treatment taking many values, it may be impossible to find a "good" comparison with the same level of treatment intensity in the pre-period. Therefore, I bin the value of per capita enhanced school bond into four categories and use this categorical variable as the treatment for applying the DID_M estimator. This generates a statistically insignificant estimate of -0.016 (standard error 0.052), supporting the finding that the amount of enhanced school bonds is not associated with changes in interest rates paid by the states.

[39]This expectation is in line with credit rating agencies' practices of decoupling state issuer ratings from that of the credit enhancement program supported by a permanent fund.

Table C1: State-Level Analysis Descriptive Statistics

A. Summary statistics:

	Mean	SD	Min	Max
yield	3.57	1.29	0.07	8.39
enhanced amount, per capita	0.21	0.52	0.00	3.48
logged population	15.60	1.09	13.32	17.49
own-source general revenue	4.34	1.43	2.42	24.61
intergovernmental revenue	1.99	0.55	1.05	5.10
total spending	7.36	1.92	4.15	19.77
debt outstanding	4.52	2.59	0.78	14.04
rainy day fund, share of GF spending	0.06	0.18	0.00	2.38
coincident index	106.96	15.97	79.07	151.79

B. Most recently observed enhanced amount, by state:

	Per capita ($thousand)	Total ($million)	Times state debt (times)
Arkansas	2.894	8696	1.225
Colorado	1.752	9822	0.517
Georgia	0.272	2835	0.211
Idaho	0.790	1356	0.406
Indiana	0.084	561	0.025
Massachusetts	0.036	245	0.003
Michigan	1.396	13912	0.441
Minnesota	2.502	13954	0.825
Missouri	1.119	6838	0.406
Nevada	0.081	241	0.074
New Jersey	0.310	2791	0.044
New Mexico	0.911	1902	0.263
New York	0.020	390	0.003
North Dakota	0.775	586	0.176
Ohio	0.535	6242	0.226
Oregon	2.142	8875	0.586
Pennsylvania	0.208	2668	0.055
South Carolina	1.333	6695	0.527
South Dakota	0.355	309	0.089
Texas	3.551	100496	1.891
Utah	1.250	3878	0.569
Washington	2.276	16852	0.479
West Virginia	0.040	73	0.007

Notes: Panel A is based on the entire sample of state general obligation (GO) bond issues merged with data on the outstanding amount of enhanced school district bonds (issued since 2009). Yield is the initial offering yield of state GO bonds. GF stands for general fund. Coincident index is a measure of state economic condition from the Federal Reserve Bank of Philadelphia, covering data on employment and income. Panel B is based on the most recent data point, out of the entire sample, from each state. Unless otherwise specified, all financial variables are measured in thousands of 2019 dollars and on a per state resident basis.

Table C2: Impact of Enhanced Amount on State Bond Interest Rate

	(1)	(2)	(3)	(4)	(5)
enhanced amount	0.0384	0.0277	0.0971	0.0301	
	(0.1052)	(0.1075)	(0.1635)	(0.1268)	
enhanced amount, range1					-0.4033
					(0.6614)
enhanced amount, range2					0.1075
					(0.1155)
enhanced amount, range3					0.0168
					(0.1597)
state covariates	no	yes	yes	yes	yes
nonenhancement states	yes	yes	no	yes	yes
permenant fund states	yes	yes	yes	no	yes
Observations	1,237	1,237	381	1,191	1,237

Notes: The interest rate of state general obligation bond is expressed in percentage points. Enhanced amount represents the amount of outstanding school bonds issued after 2009 and enhanced by state credit enhancement programs. The amount is scaled by the state population and expressed in thousands of dollars; the cutoffs for the three ranges are 0.178 and 0.642. *p<0.05, **p<0.01, ***p<0.001.

average the amount of enhanced bonds does not affect the state interest rate, there is a relationship if a very large amount of school debt is enhanced. To test this, I estimate a linear spline regression for the three terciles of the per-capita enhanced amount variable. The coefficient estimate is the largest in the middle tercile, when the per capita enhanced amount is between $178 and $642. A $100 increase in the enhanced amount is associated with a 1.1-basis point increase in state bond interest rate, although the estimate remains statistically insignificant at the 5% level. The coefficient estimate is much smaller in magnitude for the highest tercile where the enhanced amount is above $642 per capita, suggesting that higher amounts of enhanced bonds are not necessarily associated with larger negative impacts on the state government.

In sum, I find no evidence that providing enhancement for school bonds negatively affects the creditworthiness of state governments. The descriptive statistics show that except for states with permanent fund programs, the amount of enhanced school debt is on par or lower than the state's own debt. All states have statutory requirements that if state resources were used to service a district's debt, the district must pay the state back later. Some states limit their exposure to risks associated with providing enhancement by capping the amount of enhanceable bonds and implementing an application process. These requirements, along with the fact that actual activation of state commitment is rare, may have limited the impact of enhancement on state interest rates. However, the analysis suffers from the limited observations of state GO bonds and sample size constraints. Moreover, the estimate might not be causal. For example, unobserved state-level time-varying factors, such as a state's economic condition, may affect how many school districts seek enhancement and the state's interest rate at the same time.

D Appendix Tables and Figures

Table D1: State Credit Enhancement Program Details

State	Authorizing laws	Eligible bonds	Application needed	Fees	Pre- or post-default
Arkansas	ACA 6-20-1204	all	no	NA	pre
Colorado	CRS 22-41-110	GO, lease obligation	no	NA	pre
Georgia	OCGA 20-2-480, 202-2-170	all	no	NA	pre
Idaho (guaranty)	Statute 33-53	GO	yes	\$500+0.05% of debt service	pre
Idaho (credit enhancement)	Statute 57-728	GO non-refunding	yes	\$500+0.02% of debt service	pre
Indiana	Code 20-48-1-11	GO, lease obligation	no	NA	pre
Massachusetts	Title 7 Chapter 44A	maturity 10 to 30 years	yes	no	pre
Michigan	Public Act 92 of 2005	GO	yes	\$4,000 base fee[1]	pre
Minnesota	Statute 126C.5	GO, anticipation note, COP	yes	no	pre
Missouri	Statute 360.106	GO	yes	no	pre
New Jersey (reserve act)	Statute 18A_56-17 to 21	all	no	NA	pre
New Jersey (qualified bond)	Statute 18A_24-85 to 97	maturity<30 years	yes	no	pre
New Mexico	NMSA 22-18-13	GO	no	NA	pre
Nevada	Statute 387.513 to 387.528	all[2]	yes	no	pre
New York	Statute 56-6 Section 99-B	all	no	NA	post
North Dakota	Code 6-09.4-23	all	no[3]	no	pre
Ohio	Code 3317.18	GO	yes	no	pre
Oregon	Statute 328.321 to 328.361	GO	yes	\$200+0.03% of debt service	pre
Pennsylvania	Title 24 Section 6-633	all	no[4]	NA	pre
South Carolina	Code 59-71-155	GO	no	NA	pre
South Dakota	Statute 13-19-29	GO, lease purchase[5]	yes	\$3,000-\$10,000[6]	pre
Texas	TEC 45 Subchapter C	GO[7]	yes	\$1,500	pre
Utah	Statute 53G-4-801 to 808	GO	yes	no	pre
Washington	RCW 39.98	GO	yes	\$100	pre
West Virginia	annual appropriation	GO	no	NA	pre
Wyoming	Code 9-4-1001	certain refunding[8]	yes	no	pre

Notes: Program names and types can be found in Table 1. GO stands for general obligation. COP stands for certificate of participation, a type of bond secured with revenue from an equipment or facility lease. In pre-default programs, the state intervene before the principal and interest payments are due, while in the post-default program, the state intervene after defaults have occurred.

[1] For bonds with a principal value over \$5 million, an additional \$150 is collected for each \$1 million in the principal amount over \$5 million.

[2] Correspondence with state officials suggests that effectively, only GO bonds are approved for the guarantee in practice.

[3] Districts must adopt a resolution to participate in the program.

[4] Districts must send to the state the official statement within 30 days of bond sale.

[5] The state Health and Educational Facilities Authority must be involved in the debt issuance for the bond to be eligible for enhancement.

[6] The fee is the greater of \$3,000 and 0.125% of principal, but should not exceed \$10,000.

[7] Applications are prioritized for districts with lower property wealth per average daily attendance.

[8] The program is effectively closed. Only investment-grade refunding bonds for GO debt issued before November 1, 2001 are eligible.

Table D2: Serial Bond Summary Statistics

	All		Nonprogram State	Program State	
				Not Enhanced	Enhanced
yield, percent	2.348	(1.13)	2.529	2.296	2.211
logged principal	13.269	(1.42)	13.209	13.073	13.446
maturity, years	9.539	(6.15)	9.763	8.537	9.939
whether general obligation	0.935	(0.25)	0.873	0.934	0.993
whether callable	0.830	(0.38)	0.806	0.814	0.864
whether competitively sold	0.354	(0.48)	0.308	0.376	0.385
whether tax exempted	0.950	(0.22)	0.935	0.966	0.953
whether tax credit	0.007	(0.08)	0.005	0.011	0.006
whether refunding	0.487	(0.50)	0.389	0.522	0.557
whether bank qualified	0.508	(0.50)	0.476	0.577	0.496
not rated	0.153	(0.36)	0.136	0.057	0.228
BB+(Ba1) and below	0.001	(0.02)	0.000	0.001	0.001
BBB-(Baa3)	0.001	(0.03)	0.001	0.003	0.001
BBB(Baa2)	0.003	(0.06)	0.002	0.006	0.004
BBB+(Baa1)	0.007	(0.08)	0.006	0.009	0.007
A-(A3)	0.030	(0.17)	0.025	0.032	0.034
A(A2)	0.122	(0.33)	0.123	0.135	0.114
A+(A1)	0.246	(0.43)	0.255	0.241	0.242
AA-(Aa3)	0.188	(0.39)	0.212	0.193	0.161
AA(Aa2)	0.158	(0.36)	0.148	0.216	0.131
AA+(Aa1)	0.062	(0.24)	0.050	0.082	0.061
AAA(Aaa)	0.029	(0.17)	0.042	0.028	0.017
insured	0.208	(0.41)	0.300	0.410	0.000
state enhanced	0.392	(0.49)			
N	326,438		119,974	78,113	128,351

Notes: Yield represents initial offering yield. For each underlying rating category, the Standard & Poor's rating is listed first, followed by the Moody's rating in the parenthesis. While it is practically possible to receive both private bond insurance and state credit enhancement on a bond issue, the data identifies only the source of credit enhancement providing the highest enhanced credit rating. The sample includes only school district bonds with a maturity longer than one year, and covers the period of, based on bond sale date, 2009 to 2019. The column "nonprogram state" includes bonds issued by school districts in states without credit enhancement programs. Within program states, the columns "not enhanced" and "enhanced" are based on state credit enhancement status of the bond.

Table D3: Impact on District Bond Interest Rate, Districts without Underlying Rating Change

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
enhanced	-0.162***	-0.157***	-0.128***	-0.133***	-0.110***	-0.146***	-0.122***	-0.127***	0.006	-0.078***
	(0.036)	(0.040)	(0.016)	(0.016)	(0.016)	(0.034)	(0.019)	(0.025)	(0.034)	(0.024)
enhanced x medium poverty								-0.017		
								(0.036)		
enhanced x high poverty								0.008		
								(0.035)		
enhanced x diff1									-0.127**	
									(0.040)	
enhanced x diff2									-0.116*	
									(0.046)	
enhanced x diff3									-0.238***	
									(0.043)	
enhanced x diff4									-0.193***	
									(0.044)	
enhanced x diff5to9									-0.212***	
									(0.053)	
enhanced x diffmax									-0.164***	
									(0.040)	
enhanced x permanent fund										-0.107**
										(0.037)
enhanced x guarantee										-0.063
										(0.038)
enhanced x appropriation										-0.229***
										(0.055)
FE	state	state-by-year	district	district	district	district	district	district	district	district
bond covariates	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
district covariates	yes	yes	no	yes	yes	yes	yes	yes	yes	yes
nonGO bonds	yes	yes	yes	yes	no	yes	yes	yes	yes	yes
refunding bonds	yes	yes	yes	yes	yes	no	yes	yes	yes	yes
Texas bonds	yes	yes	yes	yes	yes	yes	no	yes	yes	yes
Observations	126,701	126,701	161,088	160,922	154,266	81,384	135,734	158,850	160,922	160,922

Notes: Sample includes only bonds issued by districts without changes in underlying rating. See Table 3 for detailed footnotes. *p<0.05, **p<0.01, ***p<0.001.

Table D4: Impact on Per Pupil Capital Spending, Districts without Underlying Rating Change

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
enhanced	0.340**	0.342**	0.354***	0.340***	0.230**	0.330***	0.605***	0.216
	(0.112)	(0.119)	(0.076)	(0.078)	(0.080)	(0.079)	(0.079)	(0.134)
enhanced x medium poverty								0.095
								(0.189)
enhanced x high poverty								0.389
								(0.202)
FE	state+year	state-by-year (SY)	district+SY	district+SY	district+SY	district+SY	district+SY	district+SY
baseline trend	no	no	no	yes	no	no	no	no
all states	yes	yes	yes	yes	exclude TX	yes	yes	yes
all districts	yes	yes	yes	yes	yes	issuer only	yes	yes
weighted	yes	yes	yes	yes	yes	yes	no	yes
Observations	58,948	58,948	63,783	61,345	59,593	38,813	63,783	62,227

Notes: Sample includes only bonds issued by districts without changes in underlying rating from 2009 to 2019. Each regression controls for baseline school district characteristics from 2000, which are absorbed by district fixed effects (FE) but not state FE. Column 4 additionally controls for all baseline district characteristics interacted with year dummies. Bond issuance history is also controlled for. The first two columns are based on the sample of always and never enhanced districts, with standard errors clustered at the state level. Standard errors are clustered at the district level otherwise. "Issuer only" includes only districts that have issued bonds between 2009 and 2019. When a regression is weighted, the weight applied is the geometric mean of district enrollment. *p<0.05, **p<0.01, ***p<0.001.

Table D5: Impact of State Enhancement on Other District Characteristics

	Logged enrollment (1)	Current spending (2)	Poverty rate (3)
enhanced	0.003	0.082	-0.001
	(0.002)	(0.051)	(0.001)
Observations	76,833	76,833	76,501

Notes: Each regression controls for district and state-by-year fixed effects. Bond issuance history is also controlled for. The weight is the geometric mean of district enrollment over time, and is applied in columns 2 and 3. Standard errors are clustered at the district level. *p<0.05, **p<0.01, ***p<0.001.

Table D6: Impact on Test Scores, Small vs. Large Issuance of Enhanced Bonds

	Math, Grade						Reading Language Arts, Grade					
	3	4	5	6	7	8	3	4	5	6	7	8
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
enhanced	-0.006	0.014	0.012	0.009	0.003	-0.011	0.005	-0.002	-0.008	-0.013	-0.007	-0.025
	(0.020)	(0.022)	(0.021)	(0.024)	(0.024)	(0.029)	(0.019)	(0.018)	(0.018)	(0.018)	(0.017)	(0.019)
enhanced x large	0.007	0.080	0.063	0.086	0.122*	0.086	0.061	0.062	0.038	0.084	0.080*	0.078
	(0.047)	(0.058)	(0.047)	(0.047)	(0.050)	(0.065)	(0.040)	(0.036)	(0.036)	(0.053)	(0.035)	(0.041)
Observations	51,595	51,516	50,706	50,127	44,371	40,639	51,566	51,589	51,521	51,071	50,221	49,063

Notes: Each entry is from a separate regression that controls for bond issuance history, school district fixed effects, and state-by-year fixed effects. The variable "large" is an indicator of whether the amount of enhanced issuance per student is in the upper quartile, a proxy for enhanced bonds issued to fund large capital projects. Regressions are weighted by the geometric mean of district enrollment over time. Standard errors clustered at the district level are in parentheses. *p<0.05, **p<0.01, ***p<0.001.

Table D7: Impact on Test Scores, by Baseline Poverty Tercile

	Math, Grade						Reading Language Arts, Grade					
	3	4	5	6	7	8	3	4	5	6	7	8
Low-poverty tercile:												
enhanced	-0.019	-0.012	-0.023	0.036	0.015	-0.002	-0.010	-0.005	-0.025	0.005	0.021	-0.011
	(0.029)	(0.032)	(0.030)	(0.034)	(0.035)	(0.032)	(0.023)	(0.024)	(0.023)	(0.026)	(0.024)	(0.024)
Observations	18,135	18,089	17,818	17,573	16,287	15,319	18,110	18,101	18,025	17,812	17,384	16,874
Medium-poverty tercile:												
enhanced	-0.006	0.037	0.034	0.038	0.004	0.020	0.034	0.013	0.023	0.019	0.011	-0.009
	(0.030)	(0.031)	(0.034)	(0.038)	(0.045)	(0.056)	(0.033)	(0.028)	(0.030)	(0.030)	(0.029)	(0.032)
Observations	16,748	16,735	16,391	16,198	14,683	13,500	16,801	16,783	16,784	16,688	16,444	16,174
High-poverty tercile:												
enhanced	0.021	0.057	0.041	-0.056	-0.021	-0.093	0.024	-0.005	-0.008	-0.047	-0.038	-0.051
	(0.040)	(0.042)	(0.040)	(0.037)	(0.041)	(0.052)	(0.039)	(0.034)	(0.030)	(0.035)	(0.033)	(0.038)
Observations	15,774	15,751	15,549	15,432	12,508	10,969	15,723	15,766	15,770	15,636	15,463	15,128

Notes: Each entry is from a separate regression that controls for bond issuance history, school district fixed effects, and state-by-year fixed effects. Each sample represents a tercile based on 2000 child poverty rate. Regressions are weighted by the geometric mean of district enrollment over time. Standard errors clustered at the district level are in parentheses. *p<0.05, **p<0.01, ***p<0.001.