

What Works and For Whom? Effectiveness and Efficiency of School Capital Investments Across the U.S.*

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Abstract

This paper identifies which investments in school facilities help students and which are valued by homeowners. Using novel data on school district bonds, test scores, and house prices across 29 U.S. states and a research design based on narrowly decided elections with staggered timing, we find that increased capital spending in schools significantly improves test scores and is efficient on average. However, the effects vary widely depending on the type of project and the characteristics of the school district. Investments in essential infrastructure, such as HVAC systems or pollutant removal, yield notable improvements in student performance, while expenditures on athletic facilities show no measurable academic benefit. Socio-economically disadvantaged districts gain disproportionately from capital investments, even after accounting for project type, yet these districts typically underinvest in such projects. Our findings suggest that closing the spending gap between high- and low-SES districts and prioritizing high-impact investments could reduce the achievement gap between these districts by as much as 25%.

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1 Introduction

In the 2019-20 academic year the U.S. spent \$90 billion—\$1,760 per student—on the construction and renovation of school facilities. Despite these large sums spent, facilities differ dramatically across districts, and many students attend schools in poor conditions. Several studies have investigated how capital expenditures impact students (as measured by test scores) and homeowners (as measured by house prices) using data from individual districts or states. However, these studies have reached remarkably different conclusions. Some, such as [Neilson and Zimmerman \(2014\)](#), find large positive effects on both student test scores and house prices; others, such as [Cellini et al. \(2010\)](#), find small effects on house prices and almost no effects on test scores. While a recent meta-analysis has identified positive impacts of higher school capital spending on students on average ([Jackson and Mackevicius, 2023](#)), there is little evidence on what causes these disparate effects.

There are at least two possible explanations for these disparities. First, it is possible that not all capital projects are the same. Capital outlays can fund a wide array of projects: Fixing a leaking roof and building a football stadium are very distinct types of investments and could produce vastly different effects. Second, capital outlays may benefit certain students more than others. For example, if there are diminishing marginal returns from spending time in safe and comfortable facilities throughout the day, socio-economically disadvantaged students may gain more from attending schools in better conditions ([Rauscher, 2020](#); [Enami et al., 2021](#)).

Understanding what types of projects matter and for whom is crucial for policymakers to decide what projects to prioritize. As [Handel and Hanushek \(2022, p. 33\)](#) write, it is exactly the variations in these effects that “are central to any interpretation and policy use. Indeed, [they] may provide insights into the mechanisms that could lead to larger impacts [...] by ensuring that funds were used in the most productive way”. Yet, a lack of nationwide data and information on fund allocation has so far made it difficult to address this question. As a result, the debate on the effectiveness and efficiency of school capital spending is still wide open.

This paper brings new data and evidence to this debate. We study not only whether capital investments matter for students and homeowners in a large sample of U.S. states, but also *what* matters and *for whom*. Our analysis makes use of a particular feature of the funding of capital outlays in the U.S.: the use of bonds, subject to electoral approval in local referenda and repaid with

revenues from local property taxes (Biasi et al., 2021). To estimate the effect of bond authorization, we compile a novel dataset with information on school bond referenda (including the text of the ballot), student test scores, and house prices for 29 states. We apply a research design to these data that exploits variation from close bond elections, while allowing for the presence of repeated elections as well as dynamic and heterogeneous treatment effects.

We find that, on average, bond authorization raises both test scores and house prices. Yet, impacts vary widely across types of projects and districts. Spending on infrastructure renovation or upgrades, such as HVAC systems or roofs, and on the removal of toxic materials from buildings raises test scores but not house prices. Conversely, spending on athletic facilities increases house prices but not test scores. In addition, bond authorization is most beneficial in districts with more disadvantaged student populations. In part, this occurs because these districts prioritize bonds that improve learning and are valuable to taxpayers, and because they have spent less on capital in the past (hence, they are likely to have facilities in worse condition). However, the benefits for disadvantaged students are larger even holding spending categories and prior spending amounts constant. These results offer direct guidance to policymakers on how to maximize the impact of school capital investments. They also help reconcile the conflicting findings from previous state-level studies. Replicating these studies, we show that differences in test score impacts across states can be primarily explained by differences in spending items and student characteristics.

Our analysis is made possible by a newly assembled panel dataset of school districts. We begin by collecting information on bond referenda from various state offices. These data include the share of votes in favor of the proposal, the proposed investment amount, and the text of each ballot measure that describes the proposed use of the funds. Using textual information from each ballot, we group bonds into eight categories of projects. These include classroom space; infrastructure such as plumbing, roofs, and furnaces; heating, ventilation, and air conditioning (HVAC) systems; IT facilities and labs (STEM); building adjustments to comply with health and safety standards; athletic facilities; purchases of land; and purchases of transportation vehicles, such as school buses.

We link information on bond elections to records from a novel dataset with average test scores of students in each district and year. Our starting point to compile this dataset is the Stanford Education Data Archive, compiled by Fahle et al. (2021) by collecting results of state standardized exams for 2009-18 and normalizing them to a national scale using the National Assessment of Edu-

cational Progress (NAEP). We extend this dataset to earlier years, gathering district-level test score averages from each state's education department and from the National Longitudinal School-Level State Assessment Score Database (NLSLSASD), maintained by the U.S. Department of Education (McLaughlin, 2005). We then use the procedures in Reardon et al. (2017) and Reardon et al. (2021) to harmonize scores across states and years. The resulting database includes district-level test score averages from as early as 1994 for some states, and for 2003-2019 for nearly all states. We further link bond and test score data to a house price index, constructed by Contat and Larson (2022) at the Census tract level and aggregated to the district level, and to enrollment, expenditures, and revenues from the National Center for Education Statistics (NCES). Our analysis dataset covers 12,370 bond elections in 29 states and 4,353 districts.

We use these data to estimate the causal effect of bond authorization. The standard approach to do so is a dynamic version of the regression discontinuity (RD) design around close bond elections, first developed by Cellini et al. (2010) (henceforth CFR) and later used by Martorell et al. (2016), Hong and Zimmer (2016), Rauscher (2020), and Baron et al. (2022), among others. This dynamic RD (DRD) design accounts for the presence of multiple elections and the correlation between bond proposals and authorizations over time. A recent literature on dynamic difference-in-difference models with staggered treatment timing has highlighted the importance of properly accounting for heterogeneity in effects across treatment cohorts (including Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Borusyak et al., 2021; Wooldridge, 2021). Since our treatment is also staggered, to ensure that our estimates are robust to the presence of this heterogeneity we refine CFR's approach by restricting attention to treated units and "clean controls." Namely, we match each district that approved a bond in a given year (or "cohort") with all the districts that also propose (but do not approve) a measure in the same cohort *and* do not approve any other measure after the year of interest. We then stack cohorts of treated and control units and estimate the dynamic RD model on this stacked dataset, controlling for cohort fixed effects.¹ Our results are robust to a host of alternative estimation methods, including the use of differently defined clean controls, the estimator proposed by Wooldridge (2021), and the original CFR estimator.

Our results indicate that bond authorizations raise capital outlays sharply by about \$1,500 per pupil in the five years following bond authorization. Test scores gradually increase after an autho-

¹This design is similar to that used by Cengiz et al. (2019) and Deshpande and Li (2019) in a difference-in-differences context; here, we extend it to a DRD context.

rization, reaching a 0.1 district-level standard deviations (sd) higher level after eight years. Two-stage least squares (2SLS) estimates imply that, accounting for the long life of capital projects and a standard rate of depreciation, a \$1,000 increase in capital spending over five years leads to a 0.05 sd increase in district-level test scores. We also find that a bond authorization leads to changes in the socio-demographic composition of school districts, likely due to household sorting across districts. However, these compositional changes can only account for roughly a third of the increase in test scores that we document.

These average impacts, though, mask dramatic differences in the effectiveness of capital investments across bonds, districts, and students. *What* the money is spent on plays a crucial role in determining the size of the effects. Category-specific estimates, obtained by adapting our stacked DRD design to only compare districts that propose bond measures in the same category, reveal that only some types of expenditures increase test scores: HVAC, safety and health improvements, STEM equipment, infrastructure, and classroom space. For example, authorizing a bond that finances HVAC increases test scores by over 0.20 sd three to six years after the election. The effectiveness of capital spending also depends on *who* is exposed to the spending increase. The positive impacts of bond authorization on test scores are concentrated in districts with a large share of free- and reduced-price lunch eligible students (FRPL, a proxy for low socio-economic status) or minority (black and Hispanic) students. These districts see larger spending increases after an authorization and tend to prioritize learning-enhancing spending categories. In addition, they tend to have invested less in their capital stock in the thirty years prior to a bond authorization. However, disadvantaged districts benefit more from bond authorization even when accounting for differences in spending amounts and categories. In addition, differences by districts' socio-demographic composition persist conditioning on capital stock and are most pronounced among districts with low capital stock. 2SLS estimates confirm that a \$1,000 increase in spending over five years increases test scores only in more disadvantaged districts. This indicates that capital spending is most effective in those districts.

Next, we examine the impacts of bond authorization on house prices, to obtain a measure of taxpayer valuation of these investments and to test for spending efficiency. House prices increase by about 9% eight years after an authorization, indicating that homeowners value school capital investments more than the increase in property taxes they are asked to sustain. Assuming no fiscal

externalities, this increase would indicate that capital investments are inefficiently low. In our context, though, it appears to be largely driven by the presence of aid given by states to school districts to supplement local funds. This aid drives a wedge between the amount of money homeowners contribute to the project in the form of property taxes and the actual spending increase, which gets capitalized in the local housing market. In fact, 2SLS estimates on house prices using only locally financed spending as the explanatory variable suggest that homeowners' valuation is similar to this spending increase, which in turn implies that spending levels are on average efficient.

As for test scores, though, we find large differences in efficiency across spending categories and types of districts. Following a bond authorization, house prices increase for spending on classrooms and athletic facilities but not for other test score-increasing categories. 2SLS estimates confirm that spending on these categories is inefficiently low. This implies that learning-enhancing spending does not necessarily increase house prices and investments that capitalize in the housing market do not necessarily enhance learning. This finding contradicts the widespread notion that housing market effects of school capital outlays are primarily due to academic benefits to students, and it suggests instead that they may materialize on the basis of other amenities these investments provide to homeowners. Capital spending is also inefficiently low in districts with a large share of FRPL or minority (black and Hispanic) students.

Taken together, our results indicate that both the types of projects funded by capital outlays and their beneficiaries shape the impact of increased spending on school facilities. Differences across spending categories, baseline capital stock, and districts' socio-demographic composition are essential in effectively designing and targeting the allocation of funds for school facilities across the U.S; ignoring these differences can lead to misguided conclusions about the returns to educational investments. Back-of-the-envelope calculations suggest that closing the spending gap between high-SES and low-SES districts *and* targeting the additional funds towards HVAC and safety/health (the categories with the highest impacts) could reduce the initial gap in test scores among these districts by up to 25%.

Contribution to the literature. Our paper primarily relates to a literature, spurred by the Coleman report ([Coleman et al., 1966](#)), on whether investing more money in schools improves student outcomes. While older studies expressed skepticism towards resource-based policies (e.g., [Hanushek](#),

1997), more recent research has shown that increasing spending and equalizing it across districts can improve educational outcomes (e.g., Candelaria and Shores, 2019; Jackson et al., 2016; Hyman, 2017; Lafontaine et al., 2018; Jackson, 2020), labor market outcomes (Jackson et al., 2016), and inter-generational mobility (Biasi, 2023). As we do, some of these studies have used variation from close elections to identify the effects of increased current and operational spending (Abbott et al., 2020; Baron, 2022). We contribute to this literature by showing that, across the U.S. as a whole, increased spending on capital projects can improve student outcomes and is valued by homeowners. We also demonstrate empirically that properly accounting for the use of funds and the characteristics of the students who experience the funding increase is crucial to establishing whether and how money matters.

Our work is also related to a set of studies, pioneered by Cellini et al. (2010), that have estimated the effects of school capital expenditures on students and the real estate market, reaching conflicting conclusions. Most of these studies use data from individual states (Cellini et al., 2010; Goncalves, 2015; Hong and Zimmer, 2016; Conlin and Thompson, 2017; Rauscher, 2020; Enami et al., 2021; Baron et al., 2022) or school districts (Neilson and Zimmerman, 2014; Lafontaine and Schönholzer, 2022). The only exception is Brunner et al. (2022), who use variation in revenues from wind energy installations and test score data from the NAEP to study impacts across states. Most of these studies (including Brunner et al., 2022) find small (and often imprecise) effects of capital spending, whereas Neilson and Zimmerman (2014) and Lafontaine and Schönholzer (2022) find larger and positive effects. This paper reconciles this literature by showing that capital spending can have profoundly different impacts depending on the types of projects and the characteristics of the students that benefit from them. More broadly, our results also speak to a literature that has estimated the capitalization of school quality into house prices (see Black and Machin, 2011, for a review).

Lastly and most importantly, this paper brings empirical evidence to the recent debate over the drivers of the observed differences in the impacts of school spending, particularly evident in the context of capital outlays (Handel and Hanushek, 2022; Jackson and Mackevicius, 2023). Since the precise use of capital funds is generally not recorded in district administrative data, prior studies have been unable to distinguish the impacts of different spending items beyond operational and capital spending. Using newly collected data on the text of ballots from bond referenda, we are able to classify spending into much finer categories. With these data, we uncover large differences

in impacts across categories and across districts serving different populations of students. Our findings can be used by state and district officials to understand how to best target school capital investments, with the ultimate goal of maximizing the returns for students and taxpayers.

2 School Capital Expenditure Across The US

U.S. public school districts allocate about 10% of their budget each year to capital outlays (Cornman et al., 2021).² Funding for capital projects is governed by specific rules that set it apart from other school expenditures. Roughly three-fourths of capital outlays are funded locally (Filardo, 2016), compared to only 45% of current spending (U.S. Department of Education, 2023).³ In addition, while state-level school finance reforms have led to a more equal and progressive distribution of current expenditures across school districts, the distribution of capital outlays has remained unequal. Spending on capital outlays varies substantially both across and within states (Biasi et al., 2021, and Appendix Figure A1). The precise rules school districts must follow to raise funds for capital projects differ across states.⁴ We summarize them here.

Local bond elections. In most U.S. states, capital outlays are primarily funded using bonds issued by each school district. In every state except Hawaii, Kentucky, and Massachusetts, bonds must be approved by voters in local referenda, usually (but not always) held during a primary or general election.⁵ States differ, though, in the majority required for a bond proposal to pass. Thirty-six states require a simple majority, i.e., 50% of those who turn out to vote.⁶ The remaining 11 states in which referenda are held require a supermajority, ranging from 55% in California to 67% in Idaho (Appendix Figure A2). With the exception of New Hampshire (which reduced its required majority from two-thirds to 60% in 1999), and California (which reduced it from two-thirds to 55% in 2001),

²87% of the remaining budget goes to current operations (i.e., staff salaries and benefits, instructional material, and maintenance); 3% goes to debt service; and 1% goes to programs such as community services, adult education, and community colleges.

³States contributed 22% of funding for capital expenditures on average, while the federal government only covered 1%.

⁴See Biasi et al. (2021) and Blagg et al. (2023) for a summary of these rules.

⁵The exact election timing is a decision of each school district. In some states, districts can also call referenda to increase local property tax rates to fund operational expenses. We do not consider these elections in our analysis.

⁶Data on turnout rates for local elections are generally unavailable. Recent calculations suggest rates in the ballpark of 20% (Bowers et al., 2010).

required majorities did not change between 1995 and 2017, the time period we study.⁷

In local bond referenda, ballots outline the proposed use of the funds, typically reflecting the greatest capital needs of the district. Ballots also summarize project costs and mention the projected increase in local property taxes. For example, voters in the Templeton Unified School District, CA were called to vote on the following bond proposal in 2012:

"To update classrooms with modern computers/technology; replace portable classrooms; expand vocational education facilities including welding, engineering, medical/science technology, and construction trades; and renovate, upgrade, equip, acquire and construct classrooms/school facilities throughout the District; shall Templeton Unified School District be authorized to issue \$35,000,000 of bonds with interest rates below legal limits, an independent citizen oversight committee, no money for administrator salaries, and all funds spent locally and not taken by the State and spent elsewhere?"

In this referendum, 57.8% of all voters approved the measure. Since California has a required majority of 55%, the proposal was authorized and the district was able to issue bonds. Over the next several years, the district's schools were modernized with these funds.

Districts may propose and pass several bond measures over time. Districts who fail to approve a measure may also choose to hold another election shortly thereafter.⁸ This issue is crucial for our empirical strategy. We return to it in Section 4.

State aid. State aid represents less than 30% of funds for capital projects on average, and less than 5% in about half of the states. Yet, states such as Alaska, Hawaii, Maine, Massachusetts, New Hampshire, New York, Rhode Island, and Wyoming primarily fund capital with state dollars.

States that support school capital projects typically do so with grants to school districts. Twenty-seven states use conditional grants, contingent on a district's ability to raise funds locally. For example, California funds between 50% and 60% of the cost of eligible capital projects to districts

⁷Forty states limit the amount of debt districts can issue, from 2% of assessed property valuation in Indiana to 35% in Louisiana.

⁸In general, districts that succeed in authorizing a measure may choose to either fully exhaust their bonding capacity up to the limit approved by the voters or to do so gradually. Districts may also choose to propose several small bonds in short succession (rather than a single large bond) to fund a given project. The Los Angeles Unified school district adopted this strategy in the late 1990s and early 2000s, passing several bonds to fully fund a \$25 billion, multi-decade infrastructure renewal project ([Lafortune and Schönholzer, 2022](#)).

that raise local funds.⁹ Other states use unconditional grants, funded through sales taxes, state bond revenues, and general fund appropriations. While conditional grants tend to be regressive, some unconditional ones are progressive and allocate larger sums to districts with lower property tax bases. Ohio, for example, distributes funds to districts based on local property wealth and household income.¹⁰ While our main analysis focuses on capital outlays funded by bonds, we consider the role of state aid when we examine the efficiency of capital spending in Section 8.

3 Data

Our analysis uses a new panel dataset of U.S. school districts with information on bond elections, school district finances, enrollment and demographics, test scores, and house prices. We link it to the funding rules in place in each district and year, summarized in [Biasi et al. \(2021\)](#). In this section we describe each set of variables and their respective sources. We refer to each academic year with the calendar year of the Spring semester (e.g., 2017 for 2016-17).

3.1 School Bond Elections

We created a unified nationwide database of school bond referenda by combining information from various sources. In most states, records of the most recent local elections are published on the websites of agencies and offices such as the Secretary of State, the Department of Education, and the Department of Elections. We collected online records and complemented them with those obtained through formal public data requests. We further added data on bond elections for nine states from [Abbott et al. \(2020\)](#).

The resulting database covers elections in 41 states. Most records include the date of the referendum, the share of votes in favor of the measure, the proposed bond amount, and the ballot text. We discard data from eight states because they are incomplete¹¹ and from three additional states

⁹California's School Facility Program (SFP) relies on state-issued bonds (voted on in statewide elections) to fund 60% of project costs for modernization of aging facilities and 50% of costs for new school constructions. Because this program relies on matching grants with only limited funding for low-wealth districts with fiscal "hardship", districts need to first raise their own funding to secure state funds. This results in a regressive distribution of local and state funds for school modernization ([Lafortune and Gao, 2022; Brunner et al., 2023](#)).

¹⁰The Ohio School Facilities Commission (OSFC) was formed after a 1997 Ohio Supreme Court ruling to direct state fiscal support for school capital infrastructure, mainly via state general obligation bonds (for an evaluation of this program, see [Goncalves, 2015; Conlin and Thompson, 2017](#)).

¹¹For example, data from New Jersey do not report vote shares. Data from Illinois, Kansas, Montana, New Mexico, South Carolina, and South Dakota only contain vote shares for a small number of elections.

because they fail to pass statistical tests required for the validity of our empirical strategy (we discuss these in Section 4). We also exclude Massachusetts because it does not have mandatory voting on bonds. The availability of data, their sources, and the process of construction of our final sample are described in Appendix Table B1.

To match the rest of our data, we aggregate election information at the district-year level; if a district has multiple bonds in a year, we consider either the largest bond (for bonds with information on the spending amount) or the bond with the vote share closest to the majority (for bonds with no information on the amount).¹² The resulting dataset is an unbalanced panel of 10,146 districts in 29 states, enrolling about 71% of all U.S. students and described in column 1 of Table 1. The panel begins as early as 1990 for some states and 2003 for all states, and it ends in 2017.¹³ In these data 4,683 districts (46%) propose at least one bond measure over the sample window (column 2); our stacked approach (which we describe later) uses information from 4,353 of these districts (column 3). Each year, 19% of these districts propose a measure and 77% of these proposals are authorized (Table 1). Bonds are comparable in terms of interest rates, with a standard deviation of 0.66% (Appendix Figure A3).¹⁴

Classifying bonds into categories. We assign each bond to one or more spending categories using information from the ballot text. To obtain a list of categories that is both informative and contained, we rely on the classification produced by the website *SchoolBondFinder.com* (SBF), created and managed by The Amos Group.¹⁵ SBF groups bonds into six categories (construction and renovation, capital improvements, safety and health, technology, transportation, and others) using a proprietary algorithm. We modify this list by (i) dividing capital improvements into investments in heating, ventilation, and air conditioning (HVAC, shown by Park et al., 2020, to be crucial for student learning) and investments in other infrastructure (plumbing, roofing, and furnaces); (ii) dividing facility constructions and renovations into those pertaining to classroom space and those pertaining

¹²This approach follows Martorell et al. (2016).

¹³The earliest data available are in 1990 for six states; we have limited coverage across states until the early 2000s. Appendix Figure A4 shows the number of states with district bond data and the number of bonds in each sample year.

¹⁴Appendix Figure A3 uses data from the Mergent Municipal Bonds Database and plots coupon yield rates of school district bonds issued between 1997 and 2017, removing fixed effects for the issuance calendar, the maturity year, and the type.

¹⁵The Amos Group is a private-sector company that offers consulting services for school district capital investments. *SchoolBondFinder.com* provides information on recently proposed and passed school bonds to vendors. The underlying database contains approved (but not rejected) bonds since 2014.

to athletic facilities; and (iii) adding land purchases as a separate category. Excluding “others”, this leaves us with eight categories: classroom construction and renovation (or “classroom” in short); HVAC; other infrastructure; safety and health; technology, IT, and laboratory spaces (or “STEM”); athletic facilities; land purchases; and transportation.

To classify bonds into categories, we perform a simple search of the keywords listed in Appendix B.2 in the text of each ballot. Manual checks on a random sample of 200 ballots indicate that this procedure does an excellent job in allocating bonds to categories, with an error rate of less than 1%. We successfully assign 75% of the 14,000 bonds in our final data to at least one category based on the presence of each category’s keywords in the ballot text (the details are in Appendix B.2.1).¹⁶ It is important to notice that our category indicators are not mutually exclusive, so each bond could be linked to more than one category.¹⁷ We describe the distribution of bonds across categories more in detail in Section 6.

3.2 District Finances, Enrollment, and Demographic Information

Data on district finances are from the Annual Survey of School Districts of the National Center of Education Statistics (NCES) and from the Census of Governments. We use information on districts’ total expenditure, expenditure by category (capital, current instructional, and current non-instructional), and revenues by source starting from 1995, measured in 2020 US dollars per pupil. We also use demographic information for each school district and year from the NCES Common Core of Data (CCD). These include enrollment, the racial and ethnic composition of the student body, and the share of low-income students (defined as those eligible for free or reduced-price school meals).

3.3 Student Achievement

Our analysis requires measures of student achievement in all districts across all years. In the U.S., all students in grades 3 to 8 take state standardized exams in math and either reading or English Language Arts (ELA). These exams differ across states and years, making comparisons along these

¹⁶The majority of unassigned bonds have uninformative or incomplete ballot texts, mostly due to apparent recording mistakes.

¹⁷Appendix Table A1 shows the distribution of the number of categories, across all bonds and separately for bonds containing each of the eight categories we consider.

dimensions very challenging. A notable exception is the NAEP, taken annually by a sample of grade 4 and 8 students in a subset of all districts. By design, NAEP scores are not available for all districts.

In an attempt to create a comprehensive district-level panel of student achievement, [Reardon et al. \(2017\)](#) and [Fahle et al. \(2021\)](#) converted state test scores into a uniform national scale, normalizing them across states and years using moments from the distribution of the NAEP. They applied this method to data from all standardized state exams for 2009-2018. The resulting normalized scores are publicly available as part of the Stanford Education Data Archive (SEDA).

We extend this panel backwards by adding data from various sources. For years 2001-2005, we use data from the National Longitudinal School-Level State Assessment Score Database (NLSLSASD), originally maintained by the U.S. Department of Education and now discontinued. The NLSLSAD contains average test scores at the school-subject-grade-year level for nearly every state.^{[18](#)} For 2006-2008, we collected test score averages from each state's education department either via direct download from the departments' websites or via public data requests. These data are available at the district or at the school level, separately by grade, subject, and year. We aggregated data from NLSLSASD and state departments at the district-subject-grade-year level and normalized it using the same procedure as SEDA, described in Appendix [B.1.1](#).

By combining data from these three sources, we were thus able to build a panel dataset of standardized average test scores in math and reading/ELA for grades 3-8, for 10,146 districts with bond election data, going back to 2003 for all states and as early as 1994 for some. Because test scores are standardized, all estimates are expressed in district-level standard deviations.^{[19](#)} On average, these districts enroll 71% of all students in the nation.

3.4 House Prices

We measure changes in the real estate market with a house price index (HPI), constructed by [Contat and Larson \(2022\)](#) using a repeat-sales approach applied to data from Fannie Mae and Freddie Mac, the Federal Housing Administration, and county recorder rolls provided by CoreLogic. This

¹⁸See Appendix Figure [A5](#) for a map of the first available year of data for each state.

¹⁹On average, school and district-level standard deviations are smaller than student-level standard deviations ([Kraft \(2020\)](#)). Using a subset of our data, we estimate that student-level standard deviations are on average about 2.86 times larger than district-level ones; accordingly, our estimates would be roughly 2.86 times smaller if they were estimated using student-level data (though this scale factor varies across states, grades, subjects, and years).

HPI is available for a balanced panel of 63,122 Census tracts for 1989-2021.²⁰ It is normalized to a value of 100 in 1989 for each tract and grows according to repeat-sales estimates in the tract or nearby tracts. To aggregate the data to the school district level, we map Census tract centroids to 2010 school district boundaries from the NCES Education Demographic and Geographic Estimates Program (EDGE) and calculate the average house price index for each school district and year. This procedure yields a balanced panel of 4,679 school districts with bond election information for the period 1990-2017, enrolling 61% of all students.

4 Estimating Heterogeneous Causal Effects of Bond Authorization

Our goal is to estimate the causal effect of bond authorization, allowing it to be dynamic over time and to differ across bonds. In this section we set up a research design that allows us to do this. Our starting point is a simple comparison of outcomes over time between “treated” districts (i.e., those that succeed in authorizing a bond), and “control” districts that also propose a bond in the same year, but fail to authorize it.

There are three challenges in pursuing this goal. First, treated and control districts may differ in ways that are unobservable to us. Second, districts may propose and pass multiple bonds in our time period of analysis, and the likelihood of proposing a new bond may be related to both the success and the impacts of previous proposals. Third, because districts that fail to pass a bond in a given year might successfully do so in the future, comparing treated and control districts may result in “forbidden comparisons” ([Borusyak et al., 2021](#)), which have been shown to yield biased estimates if treatment effects are correlated with treatment timing. The first two challenges have already been recognized and addressed in the literature, starting with [Cellini et al. \(2010\)](#). The third, particularly relevant for our research question, has not. We therefore review solutions to challenges #1 and #2 and propose a solution to challenge #3.

4.1 Addressing Challenge #1: Exploiting Variation from Close Elections

We begin by considering the simple case of districts that propose and authorize at most one bond measure over the period of study. Let V_{jt} be the share of votes in favor of a bond measure proposed

²⁰These tracts are based on the 2010 Census tract geography.

by district j in year t , v the required share of favorable votes to authorize the measure, and $D_{jt} \equiv \mathbb{1}(V_{jt} \geq v)$ an indicator for bond authorization in t . The effect of bond authorization on an outcome Y_{jt} measured k years after the authorization, denoted as β_k , is the difference in outcomes between treated districts and their counterfactual had they failed to pass a bond in that year. A standard difference-in-difference setup around the timing of bond authorization would then have the form

$$Y_{jt} = \alpha_j + \gamma_{s(j)t} + \sum_{k \neq 0} \beta_k D_{jt-k} + u_{jt}, \quad (1)$$

where α_j and $\gamma_{s(j)t}$ are district- and state-by-year fixed effects and u_{jt} is an error term. We normalize β_0 to be zero. If $\mathbb{E}(u_{jt}|D_{jz}) = 0$ (or, in other words, if outcomes of treated and control units would have been on similar trends in the absence of the treatment), OLS estimates of β_k capture the effects of bond authorization.

This assumption, though, is unlikely to hold in our context. Districts that succeed in authorizing a bond may differ from those that fail in many unobservable ways. For example, they may be more successful in securing funds for capital expenditures through state grants; they may have a different history of capital investments; or they may serve a different body of students. All these differences could place treated and control districts on different trajectories, violating the parallel-trends assumption of differences-in-differences (i.e., $\mathbb{E}(u_{jt}|D_{jz}) \neq 0$). OLS estimates of β_k in equation (1) would then be biased.

To overcome this challenge, CFR propose a dynamic RD framework that exploits close elections. The intuition is as follows: Since the probability of authorizing a bond jumps discontinuously at the cutoff v , if unobserved characteristics (captured by $\mathbb{E}(u_{jt}|V_{jz})$) are continuous around v , then districts that fail to authorize a bond proposal by a thin margin are a good counterfactual for districts that succeed by a thin margin. Under this milder assumption, we can thus consistently estimate β_k via OLS on an augmented version of equation (1), which controls for polynomials of the vote margin in the years preceding and following t . We denote these polynomials with $P^g(V_{jt-k}, \delta_k^g)$, where g is the order of the polynomial and δ_k^g are its coefficients. As CFR argue, this framework identifies the effects of bond authorization from close elections.²¹

²¹ Close elections tend to occur in districts that spend less on average, in states with a supermajority requirement, and for larger bonds (Table A2). Following CFR, to improve power we retain all data in estimation, rather than focusing on observations around the cutoff v .

4.2 Addressing Challenge #2: Controlling for Bond Histories

In reality, matters are more complicated than this simple case because districts can propose and authorize multiple bond measures over time. On average, in our sample period 66% of all districts that propose at least one measure do so more than once; among these, 49% authorize more than one measure. This implies that, for each election year or “cohort” of treatment, both treated and control districts may have been treated in the past or may become treated in the future. When this occurs, estimates of equation (1) capture both the direct effect of bond passage and the indirect effect of past and future bonds. Proposals and authorizations may also be correlated over time. In our data, districts that fail to authorize a bond at time t on average propose a new bond after 2.9 years and authorize one after 3.1 years. Districts that authorize a bond propose and authorize new bonds 3.9 and 4.0 years later, respectively (Appendix Figure A6).

CFR extensively discuss this issue and propose a solution: a treatment-on-the-treated (TOT) estimator that captures the effect of bond authorization against the counterfactual of *never* authorizing a bond in the foreseeable future. The TOT corresponds to the OLS estimator of β_k in a version of equation (1) that includes controls for a district’s history of bond proposals:

$$Y_{jt} = \alpha_j + \gamma_{s(j)t} + \sum_{k \neq 0} [\beta_k D_{jt-k} + P^g(V_{jt-k}, \delta_k^g) + \phi_k M_{jt-k}] + u_{jt}, \quad (2)$$

where M_{jt-k} equals one if the district proposed a bond measure k years prior to t .²² In practice, controlling for M_{jt-k} ensures that we estimate treatment effects by comparing districts with the same bond history.²³ This estimator is consistent if effects are (i) additive across bonds, and (ii) heterogeneous across time elapsed since the election but homogeneous across bonds.²⁴ The latter assumption is required because the TOT uses late-treated districts as controls for early-treated districts. Early-treated districts may continue to experience the effects of bond passage by the time

²²Equation (2) replicates the “one-step” TOT estimator from CFR (equation (12)). CFR also define a “recursive” estimator, which solves for the TOT recursively from the estimated ITT effects. Dynamic RD studies that rely on the CFR methodology typically use the “one-step” estimator (e.g. Martorell et al., 2016; Rauscher, 2020; Baron, 2022).

²³The term M_{jt-k} for $k < 0$ may seem like a “bad control” (i.e., a control that is also affected by the treatment) if future bond elections are affected by the outcome of the focal election. Yet, not including these terms implies that our RD estimates would capture the effects of authorizing a bond in the focal year *and* in future years, which is not what we want to measure. Reassuringly, whether or not we include variables for future bond elections has little impact on our estimates, as we explain in Section 5 (see Appendix Figures A21, A22, A29, and A33).

²⁴On page 229, CFR state that “We assume (as has been implicit in our notation thus far) that the TOT effects of bond authorization on later authorizations and outcomes depend only on the time elapsed since the focal treatment (τ) and not on the time at which the treatment occurred or on the treatment history”.

late-treated districts pass a bond. If treatment effects vary depending on the timing of the election, these long-run effects are not properly accounted for and estimates could be biased.²⁵

4.3 Addressing Challenge #3: Stacked Dynamic Regression Discontinuity

Assuming homogeneous treatment effects is not ideal in our context. For example, this assumption could be violated if bond impacts vary across spending categories. To relax it, we build on a recent literature (including Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Borusyak et al., 2021; Wooldridge, 2021; Goodman-Bacon, 2021) that has dealt with estimating treatment effects when treatment timing is staggered and effects are correlated with the timing of the treatment. The intuition is to compare units that become treated in t with units that are never treated after t , avoiding comparisons with units that become treated at a later time. In our context, this requires comparing districts that (barely) authorize a bond in a given year (or “treatment cohort”) with districts that also propose a bond in the same cohort, but fail to authorize it *and* never authorize any bonds in the future (either because they do not propose any bonds or because they propose but fail to pass them). We implement this strategy with the following steps.

1. For each treatment cohort c , we select treated districts and untreated ones which (i) also propose a measure in c but do not authorize it, and (ii) do not authorize any bond in the ten years following an election (regardless of whether they propose any, or not). For all these districts, we only retain data for the five years preceding c and the ten years following it.
2. We “stack” data for each cohort to form a larger dataset, where one observation corresponds to a district, year, and cohort.²⁶ We then estimate the following equation via OLS on the resulting dataset:

$$Y_{jct} = \alpha_{jc} + \gamma_{s(j)ct} + \sum_{k \neq 0} [\beta_k D_{jct-k} + \phi_k M_{jct-k} + P^g(V_{jct-k}, \delta_k^g)] + u_{jct}. \quad (3)$$

This strategy addresses all the three challenges mentioned above. First, the inclusion of leads and

²⁵A number of studies, including Goodman-Bacon (2021), De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), Callaway and Sant'Anna (2021), and Borusyak et al. (2021) have discussed this issue and proposed solutions in the context of difference-in-differences and event studies with staggered treatment.

²⁶This “stacked” approach has become popular in the context of difference-in-differences with staggered timing. Two of the earliest examples are Cengiz et al. (2019) and Deshpande and Li (2019).

lags of polynomials of the vote margin implies that the parameters β_k are identified and estimated using quasi-random variation from close elections. Second, the inclusion of M_{jct-k} holds fixed any factors that may induce districts to propose a measure at a given point in time. Lastly, the comparison of treated districts with clean controls (which avoids forbidden comparisons) ensures that our estimates are consistent even when treatment effects are heterogeneous across units (Dube et al., 2023). In the estimation, we weight observations by the number of test score takers (for test scores) or district enrollment (for capital spending or house prices) in the first year of the sample.²⁷

Two details of this strategy are worth emphasizing. First, our definition of clean controls as units that remain untreated over time does not introduce endogenous selection in the sample. Clean controls are districts that propose bonds in the *same years* as the treated districts (due to the inclusion of leads and lags of M_{jct-k}), but *barely fail* to approve them for reasons that are as good as random (due to the inclusion of leads and lags of $P^g(V_{jct-k}, \delta_k^g)$), which helps isolate variation from close elections in the focal year, in the past, and in the future). To further ensure that there is no selection on unobservables in the group of districts that barely fail in c and continue to propose a bond in the future (those that share a bond history with treated districts), we also estimate a version of our model where we do not control for M_{jct-k} for $k < 0$ (i.e., we do not control for a district's *future* bond history).²⁸

Second, our strategy considers all the elections of a given district as separate events and does not restrict the number of authorizations for treated units in each relevant time window (in other words, the sum of D_{jt-k} across all k need not sum to one for each observation in the dataset). We thus maintain the assumption of additive separability of the effects of subsequent authorizations.

Alternative designs. We probe the robustness of our results to five alternative research designs. First, we implement the TOT estimator of CFR. Second, we estimate a version of our stacked DRD

²⁷We do this for two reasons. First, ideally we would want to estimate the impact of bond authorization on each student affected by capital investments. With aggregate data, we only have averages of student test scores and house prices at the school district level. Not weighting by enrollment, we would give the same weight to the very large school districts in our data (such as Los Angeles, CA enrolling over 600,000 students) as we do to the small ones (such as Gorman, CA enrolling less than 100 students). Second, district-level test score averages and the house price index are noisy estimates of true parameters. By weighting by enrollment, we are downweighting less precise estimates (i.e., those obtained from smaller samples). Estimates remain robust when we do not use weights.

²⁸This instance could occur, for example, if districts that barely fail to authorize a bond and continue to propose one have larger potential effects from bond authorization or if authorizing a bond changes the electorate in a way that affects the presence and outcome of future elections. Appendix Figure A10 shows that the characteristics of the electorate remain fairly stable following a bond authorization.

design where we define clean controls as districts that do not approve any bonds in the entire $[c - 5, c + 10]$ time window. Third, we again estimate a version of our stacked DRD design where we define clean controls as districts who have the same bond history as at least one treated district. Fourth, we estimate our stacked DRD not controlling for future bond history (i.e., excluding M_{jct-k} for $k < 0$ from equation (3)). Lastly, we implement the extended two-way fixed effects (ETWFE) estimator proposed by Wooldridge (2021), which allows β_k in equation (2) to vary by cohort.²⁹ We present estimates obtained using these strategies in Sections 5, 6, 7, and 8. Our results are largely robust to the use of these approaches.³⁰

4.4 Testing The Validity of The Research Design

Our empirical strategy requires electoral outcomes to be as good as randomly distributed among districts with close elections. We examine the plausibility of this assumption in three ways. First, we perform a McCrary (2008) test of smoothness of the density of the vote share around the electoral cutoff. A discontinuity could indicate endogenous sorting around the cutoff, which would violate the RD assumption. State-specific histograms of the vote margin (the difference between the vote share and the required majority) show discontinuities at zero in Arkansas, Missouri, and Oklahoma but not in other states (Appendix Figure A7).³¹ We therefore exclude these states from our analysis. The resulting pooled density function is smooth around zero both in the main data and in the stacked data used for estimation (Figure 1, panels (a) and (b), respectively).

Second, we test for the smoothness of pre-election district covariates around the cutoff. We consider average household income, the share of people with at least a college degree, the shares of FRPL and white students in the district, enrollment in private schools, total revenues, state revenues, and total expenditure. All these variables are smooth around the cutoff, both in the main and in the stacked dataset (Appendix Figures A8 and A9).

Third, we examine pre-election differences in outcomes between treated units and their clean

²⁹To implement this estimator accounting for the fact that each district may have more than one bond (and thus belong to more than one cohort), we reproduce observations for districts with more than one election as many times as there are elections. To ease computation, we present test scores results obtained averaging test scores across grades and subjects using the number of test score takers as weights.

³⁰The only exception are effects on house prices estimated as in CFR. This is likely due to the fact that, as the authors themselves argue, their estimator is not robust to the presence of heterogeneity in treatment effects across election cohorts, whereas the other estimators are.

³¹These discontinuities appear to be driven by a lack of mass to the left of the cutoff. This could be due to the unavailability of election data for failed referenda. Alternatively, it could occur if all referenda pass in these states.

controls, captured by β_k for $k < 0$ in equation (3). The absence of significant differences (which we show and discuss later) suggests that trends in clean controls are a good counterfactual for treated units. Taken together, these tests support the assumption of quasi-random assignment to treatment among districts with close elections.

5 Effectiveness of School Capital Spending

We begin by estimating the effectiveness of bond authorization for student learning. We first present effects on capital spending per pupil (the first stage); then, we turn to our main measure of effectiveness, test scores. We also briefly discuss the consequences of bond authorization on inter-district household sorting.

5.1 First Stage: Effects on Capital Spending

Capital expenditures increase sharply after a bond authorization. Estimates of β_k in equation (3), using per pupil capital spending as the dependent variable, indicate that the difference in spending between districts that barely authorize and those that barely reject a bond measure in year c is on a relatively flat trend in the three years preceding an election. It then increases sharply by \$700 per year at $c + 2$ and \$590 at $c + 3$, returning to pre-election levels 5 years after the election (Figure 2, connected line).³² Five years post-election, cumulative spending is \$1,650 higher in districts that authorized a bond compared to the years before the election (Figure 2, continuous line).³³

Bond authorization does not affect non-capital (i.e. “current”) spending (Table 2, column 2 and Appendix Figure A11).³⁴ This is unsurprising, as revenues from capital bonds may typically not be used to fund current spending. We can thus safely interpret the impact of bond authorization on test scores as the effect of increased capital spending. We present these effects next.

³²Six to eight years after an election, spending is actually lower in treated districts (although this difference is indistinguishable from zero; Table 2, column 1). A possible explanation for this is that control districts manage to increase their capital spending by drawing money from alternative sources to compensate for the lost bond revenues.

³³In line with the presence of small state aid, state revenues earmarked for capital projects also increase after a bond authorization (Appendix Figure A12).

³⁴Appendix Figure A11 shows estimates of β_k in equation (3) using current spending (panel (a)) and instructional spending (panel (b)) as the dependent variables.

5.2 Mean Effects on Student Achievement

To estimate the impact of bond authorization on test scores, we pool data from Math and ELA tests taken in grades 3-8. We adapt equation (3) as follows to accommodate this feature of the data:

$$Y_{jrwt} = \alpha_{jc} + \gamma_{s(j)rwct} + \sum_{k \neq 0} [\beta_k D_{jct-k} + \phi_k M_{jct-k} + P^g(V_{jct-k}, \delta_k^g)] + u_{jrwt}, \quad (4)$$

where Y_{jrwt} is the standardized average student test score of district j and cohort c for all students in grade r , subject w , and year t . $\gamma_{s(j)rwct}$ contains state-by-grade-by-subject-by-cohort-by-year fixed effects. Everything else in the equation is as before. We weight observations by the number of test-takers and cluster standard errors at the district level.

Estimates of β_k show a significant improvement in student achievement after a bond authorization. The difference in test scores between districts that marginally approve and those that marginally reject a bond measure is constant in the five years leading to an election. It then starts to increase two years after the election, reaching a 0.1 standard deviations (sd) higher level 8 years post-election (Figure 3, panel (a)).³⁵

Table 3 summarizes the impact of bond authorization on test scores. In districts that marginally approve a bond proposal, test scores are 0.04 sd higher on average one to four years after an election, 0.09 sd higher five to eight years after, and 0.087 sd higher nine to 12 years after, relative to districts that marginally reject it (Table 3, column 1). The impact of bond passage is slightly higher for ELA (a 0.10 increase nine to twelve years post election) compared with Math (a 0.07 increase nine to 12 years after an election), Table 3, columns 2 and 3).

Effect of a \$1,000 per pupil increase in capital spending on test scores: 2SLS. The literature on the impact of school resources has focused on changes in outcomes per dollar of increased spending as a policy-relevant parameter (see [Jackson and Mackevicius, 2023](#), for a review). We transform our reduced-form stacked DRD estimates into a per dollar impact using a 2SLS model. In the first stage, we use the stacked DRD of equation (4) to predict per pupil capital spending in each district, cohort, and year K_{jct} . In the second stage we express test scores in year t as a function of cumulative

³⁵These results could likely underestimate the true effects of capital spending, since only a subset of students in each district may be affected by the investments in practice.

spending per pupil over the previous decade, denoted as $\sum_{\ell=1}^{10} K_{jct-\ell}$:

$$Y_{jrwt} = \alpha_{jc} + \mu_{s(j)rwct} + \rho \sum_{\ell=1,\dots,10} K_{jct-\ell} + \sum_{k \neq 0} [\psi_k M_{jt-k} + P^g(V_{jct-k}, \pi_k^g)] + u_{jrwt}. \quad (5)$$

In this model, ρ is the per dollar effect of changes in cumulative spending per pupil on test scores. We estimate equation (5) via OLS using the predictions from the first-stage stacked DRD as explanatory variables. To account for the two-step procedure, we report bootstrapped standard errors clustered at the district level.

These estimates indicate that a \$1,000 cumulative increase in per pupil spending over ten years increases test scores by 0.017 standard deviations (Table 4, panel (a), column 1). To account for the long life of capital projects, we follow the literature (Neilson and Zimmerman, 2014; Jackson and Mackevicius, 2023, for example) and amortize the spending increase over time, assuming a project life of 30 years and a depreciation rate of 9%.³⁶ Under these assumptions, a \$1,000 increase in the flow value of capital spending increases test scores by 2.8 times as much, or 0.048 sd.

5.3 Effects on Student Sorting and Implications for Student Achievement

A possible explanation for the test score effects of bond authorization is that capital investments improve learning. An alternative explanation is household sorting in response to capital investments.³⁷ This could change the composition of a district's student body and affect districts' average achievement, even in the absence of a direct effect of spending on learning.³⁸

While the absence of student-level data prevents us from tracking students over time, we can quantify the extent to which our test score effects are driven by sorting by measuring changes in the demographic composition of each school district after a bond approval. Appendix Figure C1 shows estimates of equation (4) obtained using, as dependent variables, a set of district observables,

³⁶Jackson and Mackevicius (2023) use a life span of 50 years and a depreciation rate of 4.7% for buildings, and 15 years and 16.5% for non-building investments. We select an average of these values. Assuming a lower depreciation rate would yield larger impacts in our context.

³⁷Evidence of household sorting following changes in school district spending and local taxes has been found in some contexts, such as Michigan (Chakrabarti and Roy, 2015).

³⁸Sorting could affect the interpretation of test scores and house prices in different ways. For test scores, sorting implies that the students for whom we observe test scores after a bond authorization are different from before. We could thus estimate positive effects of bond authorization even if capital investments do not affect learning, but simply attract students who perform better for other reasons. For house prices, sorting could be a channel through which effects arise. If some people value certain projects, carrying them out will increase the demand for housing in those districts, pushing prices up. The compositional change may occur, for example, if only more advantaged people can sustain these higher prices, or only those people really value those changes.

including the share of students who are Black, Asian, Hispanic, special-education, English-learner, or entitled to a free or reduced-price lunch; the district’s poverty rate for people aged 5 to 17; rates of enrollment in private schools; and the student-teacher ratio.³⁹ These estimates show some evidence of small compositional changes following an authorization, particularly a small but significant 7% decline in the share of students who are Hispanic, a 20% decline in the share of English-Learners, and a 4% decline in the share of FRPL students.

We assess the extent to which these compositional changes can explain our test score effects using three tests described in detail in Appendix C. The first aims at measuring the portion of our test score and house price effects that can be attributed to changes in these characteristics (Appendix C.1). The second predicts the changes in test scores and house prices that we would have expected just by the change in district observables (Appendix C.2). The third assesses the importance of changes in districts’ unobservable characteristics using the method of Oster (2019) to construct bounds for treatment effects that account for changes in unobservables (Appendix C.3); the estimated bounds are fairly tight.⁴⁰ Taken together, these tests indicate that, while household sorting following a bond authorization is non-negligible, it likely explains at most one-third of the observed increase in test scores following a bond authorization. This implies that we would find important effects of capital spending on test scores even in the absence of sorting.

5.4 Robustness

Higher-order polynomials. Our main stacked DRD estimates are obtained using a linear polynomial of the vote margin, with a constant slope on either side of the cutoff ($P^g(V_{jct-k}, \delta_k^g) = \delta_{1,k} V_{jct-k}$). Appendix Figure A14 shows estimates using a linear polynomial with different slopes ($P^g(V_{jct-k}, \delta_k^g) = \delta_{L,k} V_{jct-k} + \delta_{R,k} V_{jct-k} D_{jct-k}$) and a quadratic polynomial ($P^g(V_{jct-k}, \delta_k^g) = \delta_{1,k} V_{jct-k} + \delta_{2,k} V_{jct-k}^2$). All estimates are robust to the choice of polynomials. For computational feasibility, in the rest of the paper we use a linear polynomial with constant slope.

Alternative designs. Our estimates are robust to the use of alternative designs, including the TOT estimator of CFR (Appendix Figures A15 and A16); our stacked DRD design where we define clean

³⁹Changes in the demographic composition of a district’s student body can occur if students move from or to private schools.

⁴⁰Our preferred estimate for these bounds imply that unobservables explain 4% of the test score effects and 22% of the effects of house prices.

controls as districts that do not approve any bonds in the entire $[c-5, c+10]$ time window (Appendix Figures A17 and A18); our stacked DRD design where we define clean controls as districts that have the same bond history as at least one treated district (or, in other words, we match treated and control districts in each cohort based on their bond history; Appendix Figures A19 and A20); our stacked DRD design obtained without controlling for future bond histories (Appendix Figures A21 and A22); and the ETWFE estimator of Wooldridge (2021) (Appendix Figures A23 and A24).

6 What Works? Differences in Effectiveness Across Spending Categories

The results presented so far indicate that the approval of school capital bonds leads to increases in test scores. These average estimates, though, may mask important differences in impacts across districts and types of bonds. These differences could explain why some of the existing studies, using data from individual states, have found much more muted effects of capital spending on test scores.

To begin unpacking this heterogeneity, in this section we study whether bond impacts depend on *what* is financed, i.e., the categories of projects that each bond funds. We view this as a form of *treatment* heterogeneity: Different projects may have profoundly disparate impacts on student learning and be valued differently by taxpayers.

6.1 Bond Spending Categories: Summary Statistics

As described in Section 3, we classify bonds into eight spending categories. These include the expansion, renovation, and construction of classroom space (52% of all proposed bonds and 45% of approved ones); infrastructure such as plumbing, furnishing, and roofs (28% and 27%); the acquisition or upgrade of IT equipment and the furnishing of laboratories (which we denote as STEM, 27% and 28%); the purchase of transportation vehicles (24% and 31%); the construction and renovation of athletic facilities (19% and 17%); modifications to improve building safety and health standards, such as the removal of pollutants (18% and 20%); land purchases (14% and 13%); and the installation and replacement of HVAC systems (10% and 12%, Table 1 and Appendix Figure A25, panel (a)). More than two thirds of all bonds with at least one category are assigned to more than one category, with a mean of 2.9 categories for all proposed bonds and 3.2 categories for authorized bonds,

respectively (Appendix Figure A25, panel (b), and Table A1). Some categories are more frequently bundled together in a single bond compared to others. For example, among all proposed bonds that include classrooms, 16.9% also include HVAC and 26.3% include athletic facilities (Appendix Figure A26).

6.2 Category-Specific Impacts of Bond Authorization

To study whether the impact of a bond authorization differs depending on the category of financed projects, we estimate the following equation:

$$Y_{jct} = \alpha_{jc} + \gamma_{s(j)ct} + \sum_{k \neq 0} \left[\beta_{k,p} D_{jct-k,p} + \phi_{k,p} M_{jct-k,p} + P^g(V_{jct-k}, \delta_{k,p}^g) \right] + u_{jct}, \quad (6)$$

where p denotes a spending category, $D_{jct-k,p}$ equals one if district j of cohort c authorized a bond within category p in year $t - k$, and $M_{jct-k,p}$ equals one if the same district proposed a bond within category p in year $t - k$. We obtain estimates of the model parameters separately for each category by retaining, in each cohort, only treated and control districts that propose a bond within that category. Under the assumption that the returns from investing in a given category do not differ systematically across districts that propose that category and those that do not, we can interpret the parameters $\beta_{k,p}$ as causal and compare them across categories.⁴¹ For simplicity, we present linear combinations of the parameters $\beta_{k,p}$ for $k \in [3, 6]$ separately for each p (we report fully dynamic estimates in Appendix Figure A28).⁴² As before, we cluster standard errors at the district level and weigh observations by district enrollment. When estimating effects on test scores, we pool data for multiple grades and districts and include state-by-grade-by-subject-by-cohort-by-year fixed effects in all specifications, weighing observations by the number of test takers. Due to the presence of multiple categories in a given bond, each of these category-specific estimates correspond to weighted averages over bundles that also include that category. These can be interpreted as the causal ef-

⁴¹While we can never test whether districts that propose a category and those that do not differ on the basis of unobservables, we can compare them on the basis of a rich set observable characteristics. OLS estimates of models with an indicator for districts proposing a bond with a given category in a given year as the dependent variable and a set of district observables as the explanatory variables yield a F-statistics for the joint significance of districts' observable characteristics smaller than or equal to 5 (Appendix Table A3). Results are similar if we estimate these specifications on our stacked dataset, controlling for district-by-cohort and state-by-cohort-by-year fixed effects (Table A4).

⁴²The estimates in Appendix Figure A28 reveal positive and significant (although small) increases in test scores in years 1-2 only for categories such as other infrastructures and STEM, which likely take little time to complete. Projects such as classrooms and athletic facilities, some of which require construction and therefore a longer completion times, yield effects between 3 and 6 years after an authorization but not before.

fect of a given category under the assumption that the impact of that category is uncorrelated to whether the bond also contains other categories.⁴³

We find large differences in bond impacts across categories. Authorizing bonds that fund HVAC systems produces the largest increase in test scores, equal to 0.2 sd (Figure 4). This is consistent with recent evidence on the learning losses caused by excessive heat (Park et al., 2020) and the negative productivity impacts of high temperatures (for example LoPalo, 2023), as well as the detrimental consequences of air pollution for student achievement (Gilraine and Zheng, 2022).

Health and safety bonds have the second largest impact at 0.15 sd, in line with recent findings on the negative consequences of toxic materials in schools (such as lead or asbestos) on students' cognitive outcomes (Sorensen et al., 2019; Gazze et al., 2021; Ferrie et al., 2012). Bonds that fund renovations of plumbing systems, roofs, furnaces, and STEM equipment also have a sizable test score impact, equal to 0.15 sd. Bonds for the expansion of classroom space increase test scores by 0.11 sd.

Other categories of bonds, though, do not produce any detectable effects on test scores. Bonds for athletic facilities have an estimated positive, but statistically insignificant impact of 0.08 sd. Bonds for land purchases and transportation have an effect very close to zero. Time-specific estimates indicate that the impact of HVAC bonds peaks 3 to 5 years post-election but fades out quickly, whereas the impact of bonds for other infrastructure, safety and health, and STEM fades out much more slowly. The impact of classroom bonds persists 10 years post-election (Appendix Figure A28, panel (a)).

2SLS of capital spending increases by category. The estimation of 2SLS effects of capital spending increases on test scores by category is complicated by the fact that many bonds contain more than one category, but do not specify how revenues will be split across categories. To overcome this challenge, we estimate models in equation (5) separately for each category, using *total* per pupil spending increase as the endogenous explanatory variable.⁴⁴

⁴³For robustness, in Appendix Figure A27 we also present estimates of equation (6) obtained controlling for indicators for other categories present in the same bond, interacted with state-by-cohort-by-year fixed effects. To the extent that the impact of other categories on outcomes is constant across bonds within each state and year, these specifications should account for any correlation between category-specific effects and bond composition. Estimates barely differ between Figures 4 and A27.

⁴⁴Panel (b) of Figure 4 shows increases in capital spending following the authorization of bonds in each category, obtained estimating equation (6) using capital spending as the dependent variable.

2SLS estimates of the impact of spending on test scores indicate that a cumulative \$1,000 increase in per pupil capital expenditures raises test scores by 0.09 sd if spent on HVAC, 0.06 sd if spent on other infrastructure, and 0.04 if spent on classrooms (Figure 5, orange series). This largely confirms our reduced-form evidence on the test score effects of bond authorization by category.

Taken together, although the confidence intervals of category-specific estimates often overlap, our estimates suggest that the impact of bond authorization differs depending on the spending categories. A possible explanation for this result is that districts that approve bonds in different categories serve different populations of students. For example, more disadvantaged districts could prioritize spending on HVAC systems, while more advantaged districts prioritize building stadiums. If this occurs, differences in impacts across categories could be partly driven by differences in bond impacts by student background. We explore this possibility next.

7 Who Benefits The Most? Differences by Student Background

If marginal returns to educational investments are concave, investments in school facilities may be particularly beneficial for students from more disadvantaged backgrounds. On average, these students receive much smaller private educational investments (Heckman, 2008) and attend districts with lower total spending per year (Table 1). Investments in school facilities can also reduce school absenteeism, which disproportionately affects students from more disadvantaged backgrounds (Baron et al., 2022; Lafourture and Schönholzer, 2022) and has direct implications for achievement. To investigate differences in the impact of bond authorization by student background, we test for the presence of *treatment effect heterogeneity* by estimating equation (3) separately for groups of districts serving different student populations.

7.1 Students' Socio-Economic Status

We begin by grouping districts according to their share of students eligible for free or reduced-price meals, a proxy for low socio-economic status (SES). We focus on districts in the bottom and top terciles of the distribution of this share across all U.S. districts in 1995.

We find large differences in the impact of bond authorization across high-FRPL and low-FRPL districts. While test scores are on a flat trend in both groups prior to an election, they increase

rapidly in high-FRPL districts after the election, reaching a 0.13 sd higher level after 8 years (Figure 6, left panel (a)). In low-FRPL districts, test scores are unchanged 8 years post-election.

There are at least three reasons why bond impacts may be larger in high-FRPL districts. First, these districts may propose and authorize larger bonds, leading to larger increases in spending. Second, they could prioritize spending categories that increase test scores. Third, students in these districts may have higher returns on school capital investments, even conditioning on the size of the spending increase and the spending categories.

Our data confirm that high-FRPL districts spend more on capital projects after a bond authorization. Cumulative capital spending increases by \$4,000 on average 5 years after an authorization in high-FRPL districts, and by \$800 in low-FRPL districts (Figure 6, left panel (c)). Yet, these differences are not enough to explain the heterogeneity in bond impacts. 2SLS estimates indicate that a \$1,000 increase in cumulative spending increases test scores by 0.08 sd in high-FRPL districts, whereas it does not produce any detectable changes in low-FRPL districts (Table 4, panel (a), columns 3 and 2, respectively).

High-FRPL districts are also slightly more likely than low-FRPL ones to approve bonds to fund projects that raise test scores, such as HVAC systems (8% of all approved bonds are in this category, compared to 5% for low-FRPL), safety and health (14% compared to 9% for low-FRPL), and STEM (16% compared to 14%). They are also more likely to invest in classroom space, which raises test scores (29%, compared to 20% for low-FRPL, Figure 7).

Yet, differences in bond composition alone are unable to explain the differences in impacts between high- and low-FRPL districts. High-FRPL districts see larger effects of bond authorization *within each bond category*. This is evident from Appendix Figure A35, which reproduces the estimates in Figure 4 separately for high-FRPL and low-FRPL districts.⁴⁵ For example, authorizing a bond to fund HVAC systems increases test scores by 0.27 sd in high-FRPL districts but leaves them unaffected in low-FRPL districts (with an estimate of -0.3 sd in panel (a), indistinguishable from zero). Bonds that fund other infrastructure and STEM equipment also have larger impacts in high-FRPL districts (equal to 0.32 sd and 0.22 sd, respectively) compared to low-FRPL districts (-0.05 and 0.1, both indistinguishable from zero). These results indicate that the larger mean effects of bond authorization on high-FRPL districts are not uniquely driven by bond composition, but also

⁴⁵In Appendix Figure A35 we omit the transportation category due to a small number of observations (among low-FRPL districts, it only includes 5 observations in the control group).

by high-FRPL students benefiting more from a given investment.

Share of Minority Students. We also explore whether districts serving different shares of racial and ethnic minorities see different impacts. We group districts in terciles of their share of Black and Hispanic (“minority”) students in 1995.

The patterns of effect heterogeneity resemble those found in the previous section. “High-minority” districts (with a share of minority students in the top tercile) experience large increases in test scores after an election, equal to 0.12 sd after 8 years (Figure 6, right panel (a)). Instead, “low-minority” districts (with a share in the bottom tercile) experience much smaller increases in both outcomes (0.06 sd in test scores 8 years post-election). The size of the investment and the spending categories alone cannot explain the differences in impacts: 2SLS estimates indicate that the impact of a \$1,000 increase in per pupil spending on test scores is much larger in high-minority districts (Table 4, panel a, columns 4 and 5). High-minority districts approve more bonds with items such as HVAC and safety and health, which increase test scores (Figure 7). However, they also experience larger test score effects within those categories (Appendix Figure A36).

7.2 A Possible Mechanism: Differences in Baseline Capital Stock

An additional possible reason for the larger bond impacts in districts serving disadvantaged students is that these districts might have facilities in worse conditions. Our data indicate that high-FRPL districts tend to spend less on average. They are also more likely to be located in urban areas, which typically have older buildings (Lafortune and Schönholzer, 2022).

The approval of a bond may produce very different effects depending on the initial conditions of school facilities. For example, the installation or improvement of an HVAC system in a school with a deficient system may improve learning much more than the replacement of the same system in a school that already had a functioning one. Investigating whether the impact of bond approval depends on the state of school facilities prior to an election is thus important both on its own, and as a potential driver for the differences in impacts between high-FRPL and low-FRPL districts.

To explore the role of initial facility conditions, we would ideally like to observe detailed information on each district’s school buildings over time. Unfortunately, this type of information is not available at the national level. To partly overcome this data limitation, we construct a measure of a

district's stock of capital at any given point in time and use it as a proxy for the state of school facilities. We use historical expenditure data from the Census of Governments for the years 1967-2017, which record local governments' capital spending every five years. We linearly interpolate capital spending values in inter-censal years and aggregate them over 30 years using a 5% depreciation rate. A district's capital stock is negatively correlated with its share of FRPL students, suggesting that, on average, more disadvantaged students attend schools in buildings in worse conditions (Appendix Figure A37).

To quantify differences in bond authorization impacts across districts with high and low capital stock and understand how they interact with student SES, we re-estimate equation (3) separately for districts (a) with capital stock above and below the median in the year prior to the election, and (b) with a share of FRPL students in the top and bottom tercile. The results of this exercise are shown in Figure 8. Among districts with above-median capital stock, the effects of bond authorization on test scores are positive but noisy. They are also only slightly larger in high-FRPL districts compared with low-FRPL ones (Figure 8, panel (a)). Among districts with below-median capital stock, instead, test score effects are much larger in high-FRPL districts, with a 0.2 sd increase 7 years after an election (Figure 8, panel (b)).

Taken together, our results indicate that an ex ante low capital stock and worse facility conditions may be responsible for some of the observed differences in bond impacts between high- and low-FRPL districts. Yet, these differences persist even conditioning on capital stock. This, in turn, confirms that students with different backgrounds may benefit more from attending schools in better conditions, particularly if these schools are not in a great state to begin with. Our findings also highlight how the detrimental effects of low levels of investments on low-SES students may compound over time, exacerbating achievement gaps between students with a different socio-economic background.

8 Efficiency of School Capital Investments

Beyond test scores, previous studies of the effects of school capital spending have examined impacts on house prices. This serves two purposes. First, changes in house prices capture any benefits of school capital investments for students and communities not captured by test scores. If these

benefits are valued by homeowners and home buyers more than the taxes they pay to finance them, spending increases should raise house prices ([Cellini et al., 2010](#)).

Second, house prices provide a test for the efficiency of public spending. Simple models of optimal spending postulate that public goods provision is efficient when its aggregate marginal benefit equals the marginal cost of providing it ([Samuelson, 1954](#)). When the amount is inefficient (either too high or too low), households will “vote with their feet” and re-sort across communities, with consequences for house prices ([Tiebout, 1956](#)). Existing residents may also be willing to pay a greater share of their income on housing if the marginal benefits are higher than their (tax) cost. [Brueckner \(1979\)](#) combined these two insights to suggest a simple test of efficiency of public good provision: If a spending increase raises house prices, the initial spending level was inefficiently low. Vice versa, if the spending increase lowers house prices, the initial level was too high.⁴⁶

8.1 Mean Effects on House Prices

To estimate the impact of increased school capital spending on house prices, we use a district-level house price index as the outcome variable in equation (3). This variable is normalized to 100 for each district in 1989; effects can thus be interpreted in percent over the mean for that year. Estimates of β_k are indistinguishable from zero prior to the election, indicating similar pre-election differences between districts that approve a measure in some year and those that reject it. After the election, house prices gradually increase in districts that approve a bond measure, reaching a 9% higher level eight years post election (Figure 3, panel (b)). On average, house prices are 7% higher five to eight years after a bond authorization and 5% higher nine to twelve years later (Table 3, column 4). This indicates that, on average, households value increases in school capital spending more than the additional local taxes they are asked to contribute.⁴⁷

2SLS. Panel (b) Table 4 shows 2SLS estimates of the impact of cumulative spending increases on house prices. We obtain these by predicting cumulative spending using, as first stage, the model in equation (3) and then re-estimating equation (5) using house prices as the dependent variable (we control for district-by-cohort and state-by-year-by-cohort). These estimates confirm that a \$1,000

⁴⁶[Coate and Ma \(2017\)](#) show that this kind of efficiency assessment relies on the assumption of myopic beliefs about future investment behavior of the district. A similar test has been recently used by [Bayer et al. \(2020\)](#).

⁴⁷As for test scores, these house price estimates are robust to the use of alternative designs (Appendix Figures A16, A18, A20), and A24.)

increase in spending over ten years raises house prices by 5.3% accounting for depreciation (Table 4, panel (b), column 1).

A possible interpretation for this finding is that the level of spending on school facilities is on average inefficiently low. However, neither the reduced-form estimates nor the 2SLS estimates described above account for the fact that several states provide districts with grants to partially cover capital expenditures. Since these grants are not financed exclusively via local taxes, they raise the marginal benefit of spending without raising marginal costs for households in the district.

To conduct a proper test of spending efficiency in the presence of state grants, we thus estimate house price effects of increases in *local* capital spending by substituting cumulative lagged spending in equation (5) with cumulative lagged *local* spending (defined as the per pupil bond size proposed by a district over the last decade) and using the house price index as the dependent variable. These estimates indicate that a \$1,000 increase in local spending produces only a small increase in house prices, equal to 0.8% accounting for depreciation (Table 4, panel (c), column 1). We can thus conclude that, on average, school capital spending is efficient across the U.S. However, we show below that inefficiencies exist in some contexts.

8.2 House Price Effects and Efficiency of Spending by Category

First, we obtain effects of bond authorization on house prices by re-estimating equation (6) using the HPI as the dependent variable. We find that the categories that most increase test scores are not the same as those that most increase test scores. House prices increase following the construction of athletic facilities (a 17% increase, Figure 4), the expansion of classroom space (14%), and STEM improvements (11% sd). However, they remain unchanged following the approval of bonds in other test score-enhancing categories, including HVAC and safety/health. The correlation between test score and house prices estimates is -0.07 . This suggests that home buyers value different kinds of improvements in school facilities—particularly those that are visible and have an amenity value, such as athletic facilities—over those that improve student learning.⁴⁸ Estimates are largely robust to the use of alternative estimation procedures (Appendix Figure A29).

⁴⁸ Time-specific estimates indicate that the impact of athletic facilities on house prices peaks 3 to 5 years post-election and then declines, whereas the impact of classroom bonds persists over time (Appendix Figure A28, panel (b)).

Efficiency by Category: 2SLS. To properly gauge spending efficiency by category, the blue series in Figure 5 shows 2SLS effects of bond amounts on house prices. A \$1,000 increase in (locally financed) capital spending increases house prices by 2.5 pp when allocated to safety and health or athletics, and 1.5 pp when allocated to land purchases or classrooms. These estimates indicate that spending on these categories tends to be inefficiently low. Estimates for other categories are instead indistinguishable from zero, indicating efficient spending levels.

8.3 Efficiency by Student Background

Next, we test for differences in the house price impact of bond authorization across districts serving students from different backgrounds. We find large differences in the impact of bond authorization on house prices across districts with high and low shares of FRPL districts. In high-FRPL districts, house prices increase by 15% 8 years after a successful bond election (Figure 6, left panel (b)). In low-FRPL districts, instead, the effect of bond authorization on house prices is indistinguishable from zero.

Differences in bond size or state aid are not enough to explain these differences: 2SLS estimates of bond amounts on house prices are also larger in high-FRPL districts (1.9% compared with 0.4% in low-FRPL districts), despite being imprecisely estimated (Table 4, panel (c), columns 3 and 2, respectively). This suggests that school capital investments are inefficiently low in high-FRPL districts. We find similar results when grouping districts according to their share of minority students (Figure 6, right panel (b), Appendix Figure A36, and Table 4, columns 4 and 5).

8.4 Efficiency by Baseline Capital Stock

Lastly, we explore whether differences in baseline facility conditions may explain some of the heterogeneity in the house price effects shown above. As before, we re-estimate equation (3) separately for districts (a) with capital stock above and below the median in the year prior to the election, and (b) with a share of FRPL-eligible students in the top and bottom tercile.

Among districts with above-median capital stock, house prices increase by over 10% four years after an election in high-FRPL districts, whereas they do not change (and, if anything, they decline slightly) in low-FRPL districts (Figure 8, panel (c)). Among districts with below-median stock the impact is larger for high-FRPL districts, and equal to 12% and 10% four and eight years after an

election, respectively (Figure 8, panel (d)). Overall, these results indicate that ex ante low capital stock is unlikely to explain the large house price effects of bond authorization on low-SES districts.

9 Discussion and Conclusion

This paper investigates the impact of investments in school capital on student learning and the real estate market, studying what types of investments work and under which circumstances. Using variation from closely decided referenda on school bonds and an estimator that allows for both dynamic and heterogeneous treatment effects, we show that the approval of a bond increases test scores by 0.1 sd and house prices by 7% five to eight years after an election in the average U.S. district. Taken at face value, these estimates indicate that investing in school facilities is beneficial for students and valued by the community more than the required increase in local taxes. Using 2SLS, we also show that the increase in house prices is primarily due to the presence of state aid, rather than to inefficiencies in the ex ante level of spending.

These average effects, though, mask significant variation across funded projects and across districts serving different populations of students. Investments in school infrastructure such as HVAC, safety and health, plumbing, roofs, and furnaces produce large increases in test scores, likely because they improve students' learning experiences. However, they do not produce any effects on house prices, possibly because they are not "visible" to homeowners without school-age children. School investments that carry an amenity component and that are more visible, such as the construction or renovation of athletic facilities and the expansion of classroom space, produce instead significant increases in house prices, even if they do not have as much of an impact on learning. We have also shown that districts that serve more socio-economically disadvantaged students tend to pass more bonds with larger impacts on both test scores and house prices. In part due to their focus on these types of investments (although not entirely), low-SES and minority students see the largest benefits from bond authorization.

Using our estimates to better target spending. Our estimates can directly inform policies aimed at improving achievement across the board and at closing achievement gap across districts serving different populations of students. To illustrate this point, we quantify how much of the achievement gaps between districts serving different shares of FRPL students could be closed simply by (a)

equalizing spending across districts, raising it to the level of the highest spending districts and (b) targeting funds towards projects with the largest achievement impacts.

Raising capital spending per pupil in low-SES districts (as measured by the share of FRPL students) to the level of high-SES districts would imply an increase in spending of about \$1,000 over 10 years in low-SES districts (Table 1).⁴⁹ Absent a change in the distribution of spending across categories, this spending increase would raise test scores by about 0.08 sd in low-SES districts (using the estimate in Table 4, panel (a), column 3).⁵⁰ This is akin to closing about 8% of the initial achievement gap between high- and low-SES districts, equal to roughly one sd (Table 1). However, increasing spending by the same amount *and* targeting it towards categories that are most effective in raising test scores, such as HVAC or other infrastructure, has the potential of generating increases in test scores roughly three times as large (as evidenced by comparing the average impacts for low-SES districts in Figure 6 with the category-specific ones in Figure A35). A proper targeting of resources could therefore close up to 25% of the achievement gap between high- and low-SES districts.

Reconciling the estimates from the literature. The differences in impacts across bonds and districts can rationalize the contrasting results found in previous state-level studies. Although our state-level estimates differ slightly from previous studies (likely due to different outcome variables, data aggregation, and empirical models), in panel(a) of Appendix Figure A38 we are able to replicate the positive effects found in Ohio by [Conlin and Thompson \(2017\)](#), the small positive effects found in California ([Cellini et al., 2010](#); [Rauscher, 2020](#)), and the lack of effects in Wisconsin ([Baron, 2022](#)), Michigan ([Conlin and Thompson, 2017](#)), and Texas ([Martorell et al., 2016](#)).

Importantly, differences in impacts between states and districts appear to be driven by the composition of the student body and the spending categories.⁵¹ We show this in Table 5, where we decompose the variance of district-specific test score effects into various dimensions of heterogeneity: a) across states, b) across districts serving different populations of students, c) across districts that pass bonds of different size, d) across districts that pass bonds in different categories, e) a combi-

⁴⁹On average, spending on capital per pupil differs by \$97 per year between low- and high-SES districts. Hence, closing this difference for 10 years implies a cumulative spending increase of about \$970.

⁵⁰Here, we amortize spending into the flow value of capital spending under the same assumptions detailed in Section 5.

⁵¹For example, Ohio has a significant portion of bonds in districts with high shares of FRPL students and funding infrastructures. Texas, on the other hand, has a small share of bonds in districts serving FRPL students and a large share of bonds that fund classrooms and athletic facilities (Appendix Figure A38, panel(b)).

tion of b) and d), and f) a combination of c) and d). We do so using a Shapley-Owen decomposition (Israeli, 2007; Huettner et al., 2012).⁵² The exercise shows that most of the total variation in test score effects can be explained by demographic characteristics and bond categories combined (9% of total variation, or 61% of explained variation), while other factors explain a smaller share. These results underline the importance of accounting for differences in these factors when assessing the impact of capital spending on students.

Concluding remarks. Our results highlight how accounting for differences in *what* school capital investments fund and *for whom* is essential to fully appreciate their impact on schools and communities. They also offer guidance to school district leaders when choosing which spending items to prioritize. Of course, districts' decisions (and their ability to raise funds for capital projects) may also depend on the specific funding rules – and specifically, the presence of a supermajority requirement – in place in their state, since these rules determine how easy it is for a district to raise money for a specific project. While we have abstracted from this dimension here, we believe that considering the interplay between these constraints and the composition of a district's population is crucial to understanding how changes in constraints can impact the size and composition of authorized bonds in equilibrium, and how they can impact students and taxpayers. A proper analysis of these issues is outside the scope of the present paper but is the focus of ongoing work, using similar data as this paper.

⁵²We estimate district-specific effects of bond authorization on test scores, by allowing the parameters β_k in equation (4) to be constant for $k \in [5, 10]$ and district-specific. A Shapley-Owen decomposition estimates what portion of the R^2 of a regression of district-specific effects of bond authorization on the variables in a)-f) above can be attributed to each group of variables. For each group of variables j , the portion of total R^2 it explains is given by $R_j^2 = \sum_{T \subseteq V \setminus \{j\}} \frac{|T|!(K - |T| - 1)!}{K!} [R^2(T \cup \{j\}) - R^2(T)]$, where $R^2(S)$ is the adjusted R^2 of a regression of the gap on a group of variables S , V contains all group of variables, $|T|$ is the number of groups in set T , and $K \equiv |V| = 6$ is the total number of groups of variables considered. We use the adjusted R^2 instead of the standard R^2 to account for the different number of variables in each group of variables.

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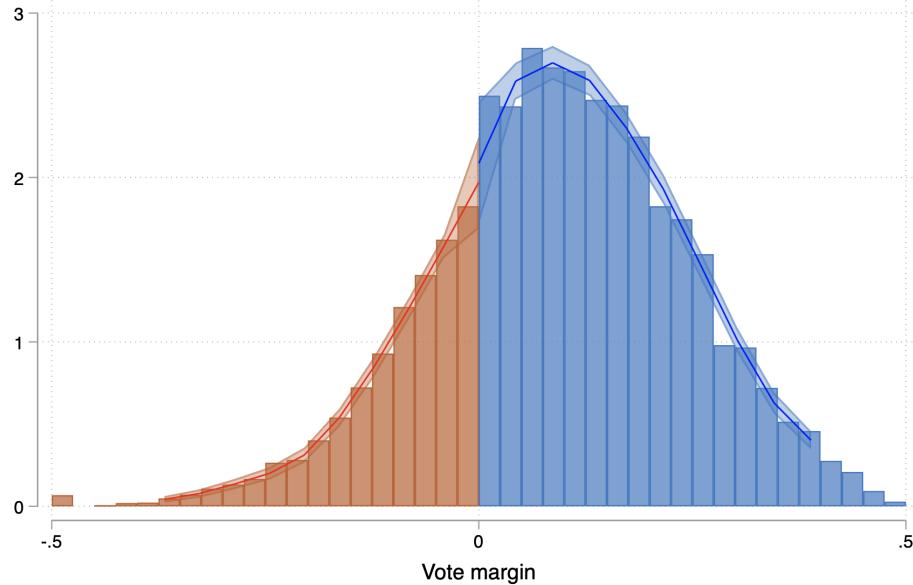
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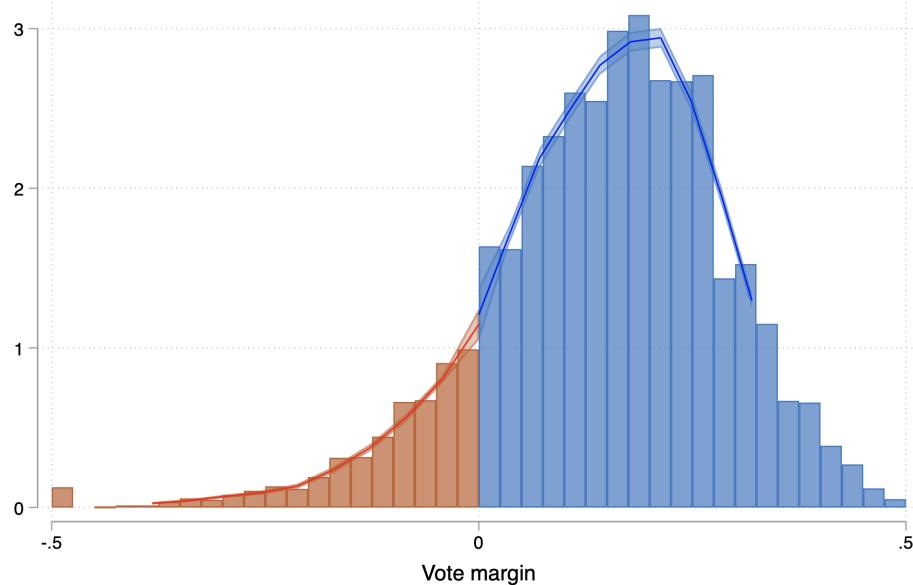
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Figure 1: Smoothness of The Density Function of The Vote Margin

(a) Main data (P-value of McCrary test = 0.59)

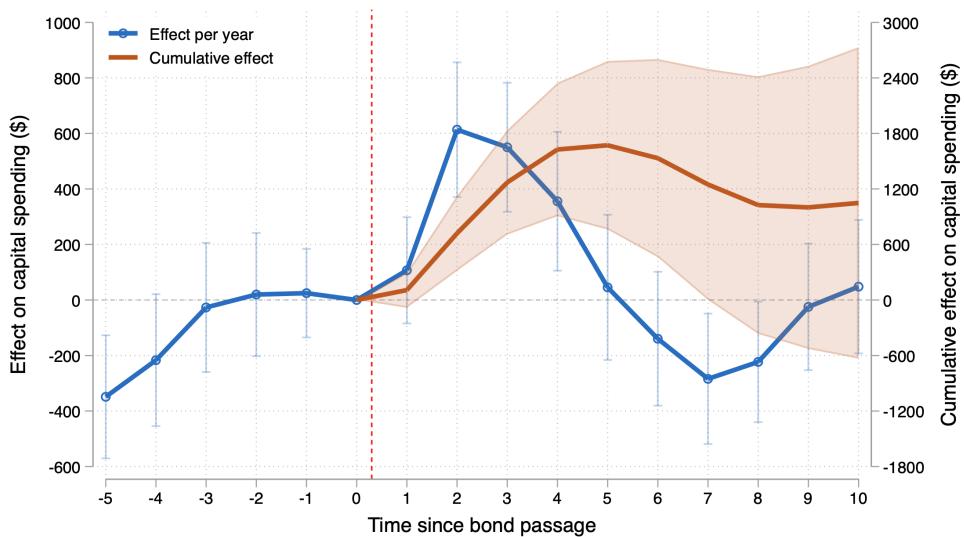


(b) Stacked data (P-value of McCrary test = 0.24)



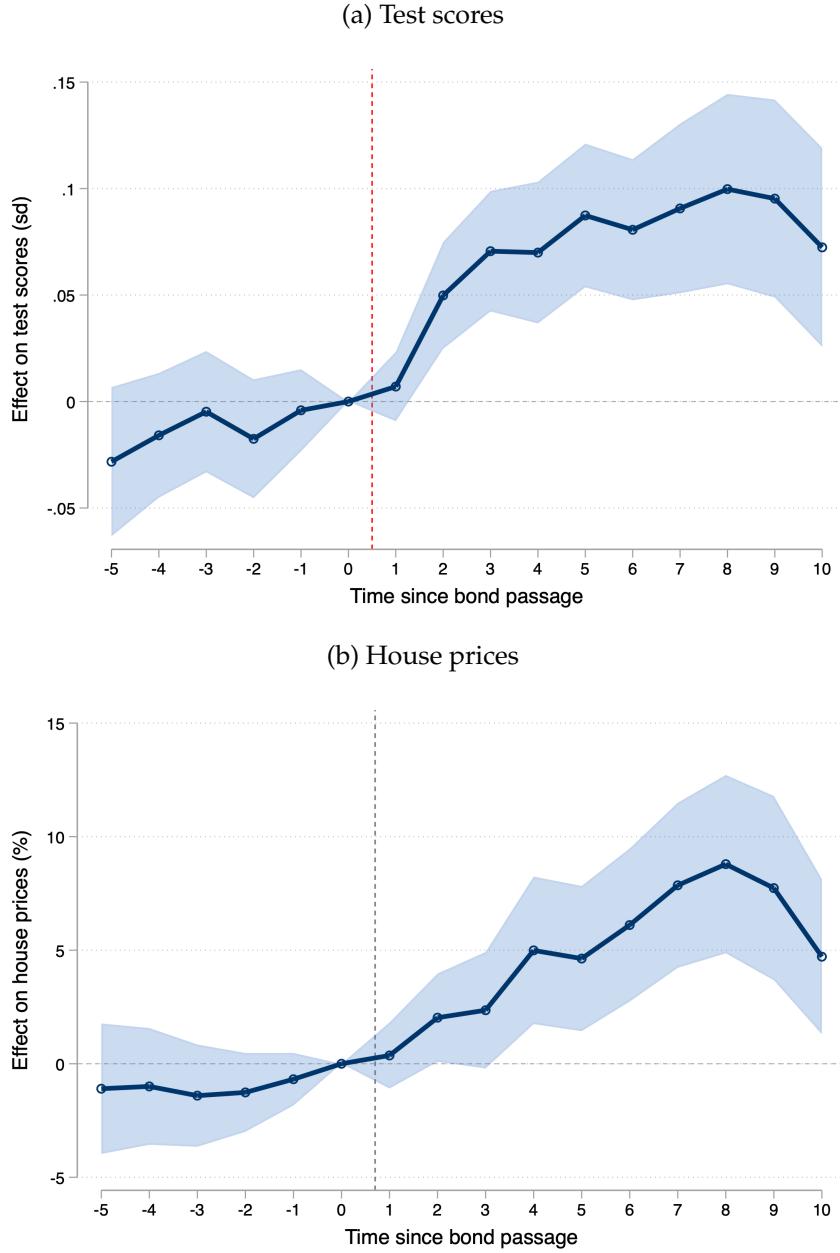
Notes: Histograms of the vote margin, defined as the difference between the share of votes in favor of the proposed measure and the required majority in each state. The lines and confidence intervals visually show the result of a McCrary (2008) test for the discontinuity in the density function at zero, using a uniform kernel and a cubic polynomial. Panel (a) is constructed using the main data set. Panel (b) is constructed using the stacked data set used in estimation. The sample includes AZ, CA, CO, CT, DE, FL, GA, ID, IN, IA, LA, MD, MI, MN, MS, NC, NE, NV, NY, ND, OH, OR, PA, RD, TX, VA, WA, WV, WI.

Figure 2: Average Effects of Bond Authorization on Capital Spending



Notes: The blue line with circle markers shows estimates and confidence intervals of the parameters β_k in equation (3), obtained using capital spending per pupil as the dependent variable. The orange continuous line shows cumulative effects, calculated as the running sum of coefficients since time 0. Estimates are obtained using district-by-cohort and cohort-by-state-by-year effects; observations are weighted by district enrollment. Standard errors are clustered at the district level.

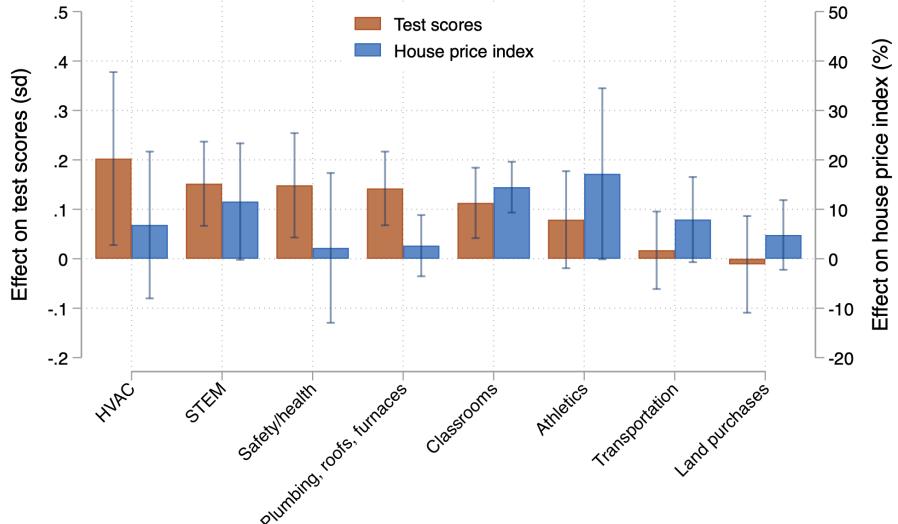
Figure 3: Average Effects of Bond Authorization on Test Scores and House Prices



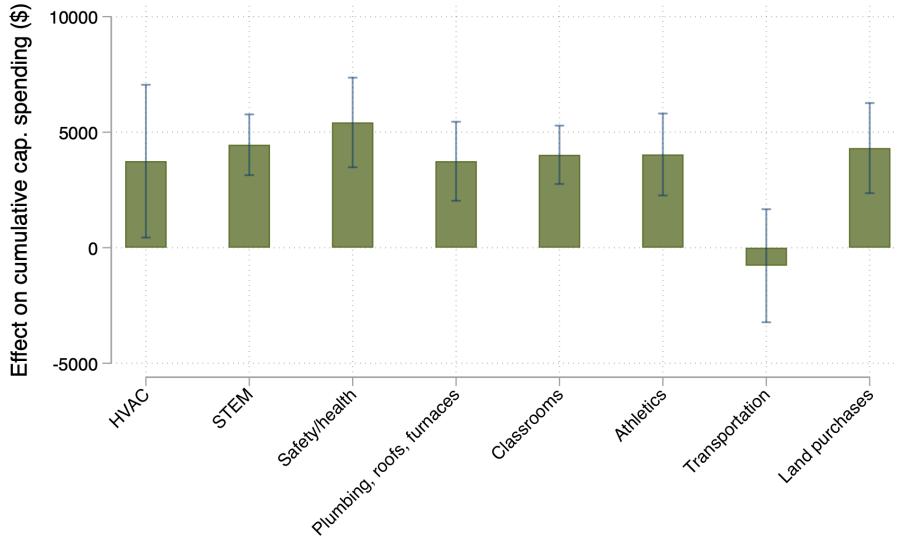
Notes: Estimates and confidence intervals of the parameters β_k in equation (3), obtained using test scores (panel a) and the house price index (panel b) as the dependent variable. Test score estimates are obtained pooling data across subjects and grades, controlling for district-by-cohort and cohort-by-state-by-year-by-subject-by-grade effects, and weighing observations by the number of test takers. House price estimates are obtained using district-by-cohort and cohort-by-state-by-year effects, weighing observations by district enrollment. Standard errors are clustered at the district level.

Figure 4: Effects of Bond Authorization By Spending Category

(a) Test scores and house prices

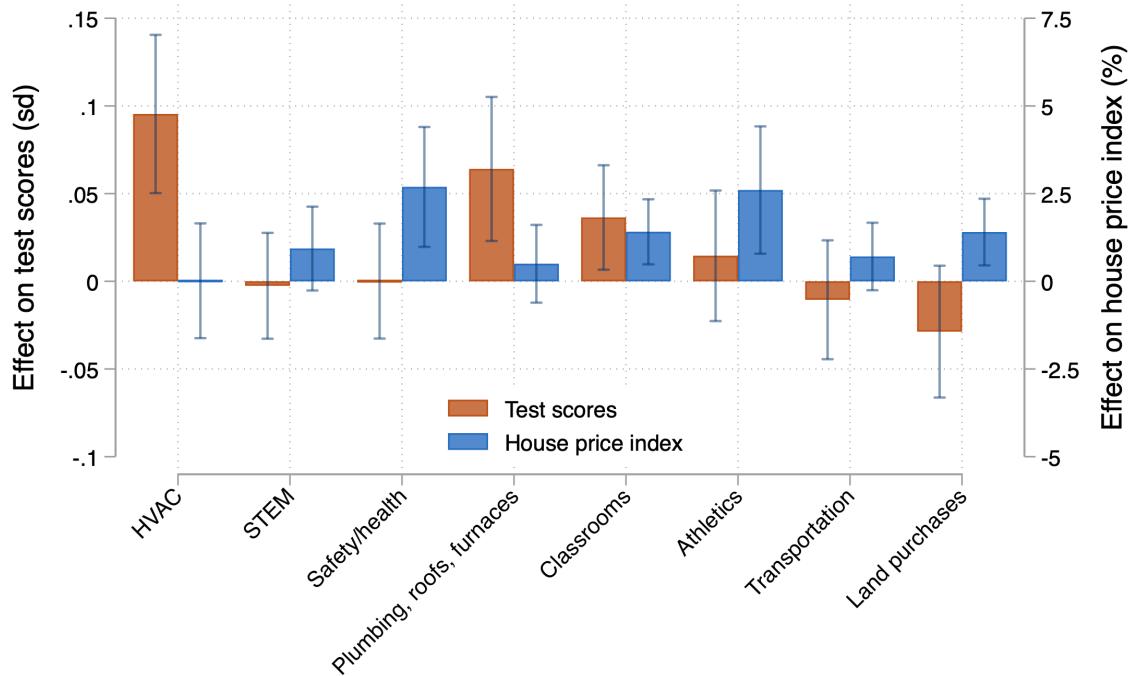


(b) Capital spending



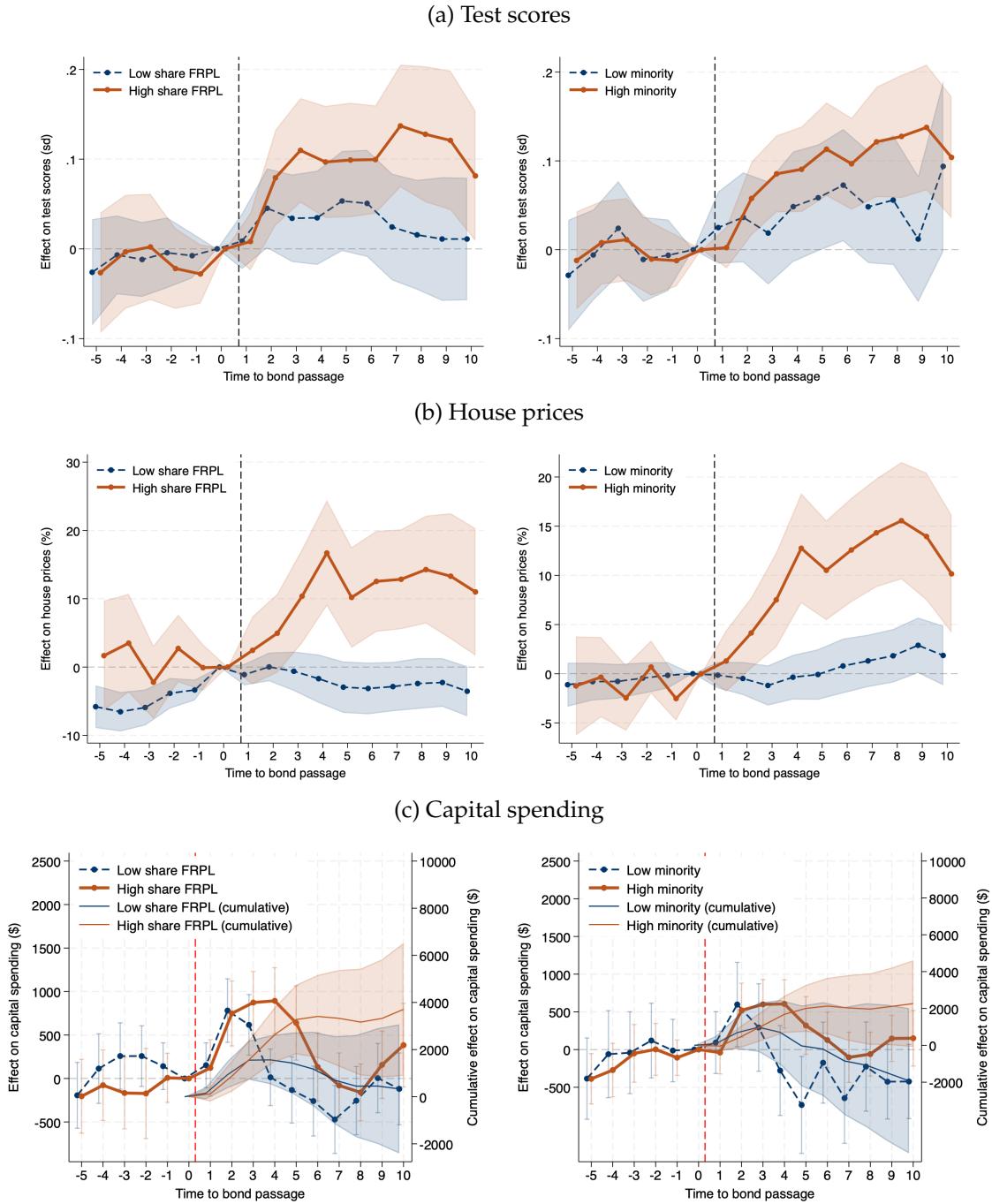
Note: Point estimates and confidence intervals of an average of the parameters $\beta_{k,p}$ in equation (6) for $k \in [3, 6]$, shown separately for each spending category p . In panel (a), the orange series is estimated using test scores as the dependent variable, pooled across subjects and grades, using district-by-cohort and state-by-year-by-subject-by-grade-by-cohort effects and weighing observations by the number of test takers. The blue series is estimated using the house price index as the dependent variable, using district-by-cohort and state-by-year-by-cohort effects and weighing observations by district enrollment. In panel (b), the green series is estimated using capital spending per pupil as the dependent variable, using district-by-cohort and state-by-year-by-cohort effects and weighing observations by district enrollment. Confidence intervals are calculated using standard errors clustered at the district level.

Figure 5: 2SLS Effects of Capital Spending Increases By Spending Category



Note: 2SLS point estimates and confidence intervals of the effect of spending increases on test scores and house prices, by category. Estimates are obtained by considering total spending increases (for test scores) and bond amounts (for house prices) in the first stage. The orange series shows effects on test scores, pooled across subjects and grades and obtained using district-by-cohort and state-by-year-by-subject-by-grade-by-cohort effects and weighing observations by the number of test takers. The blue series shows effects on the house price index, obtained using district-by-cohort and state-by-year-by-cohort effects and weighing observations by district enrollment. Confidence intervals are calculated using standard errors clustered at the district level.

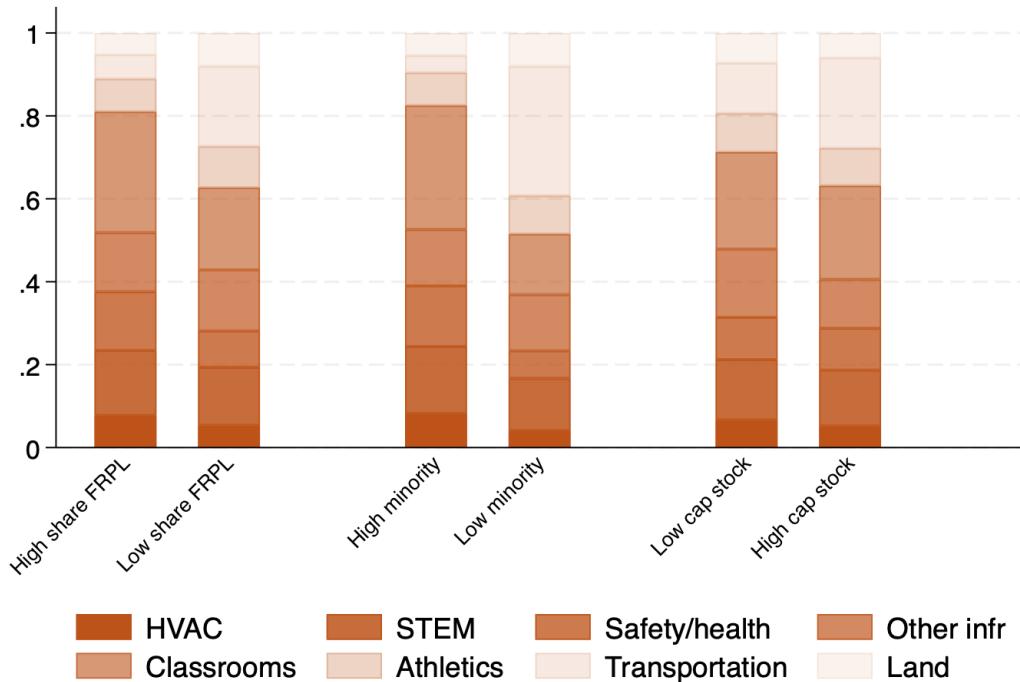
Figure 6: Effects of Bond Authorization By Student Demographics



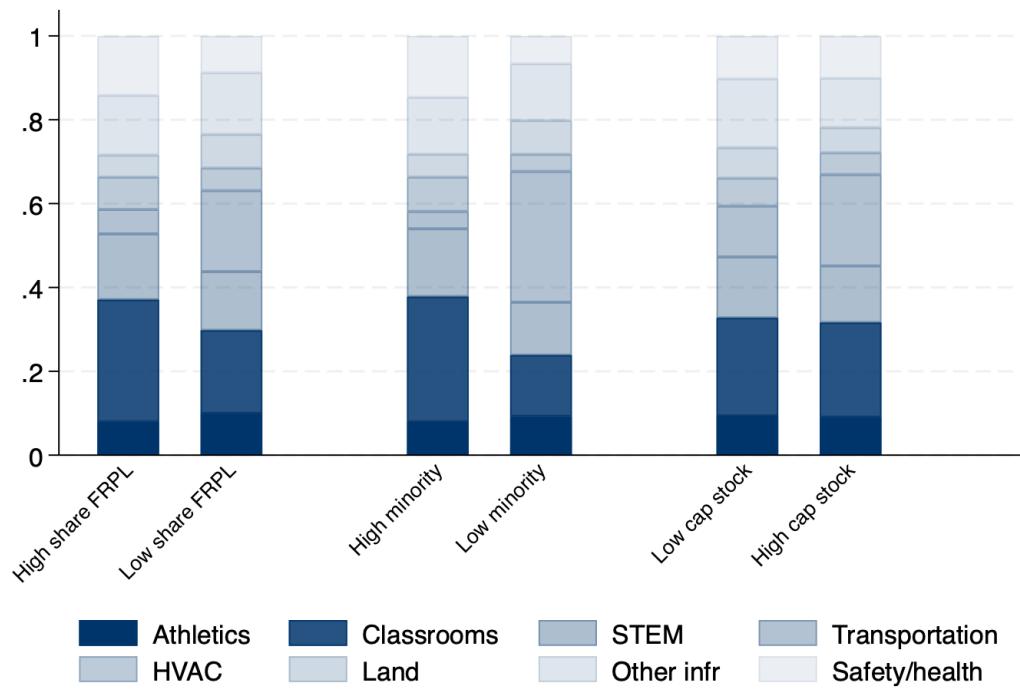
Note: Estimates and confidence intervals of the parameters β_k in equation (3), obtained using test scores (panel a), the house price index (panel b), and capital spending per pupil (panel c) as the dependent variable. Figures in the left panels show estimates by tercile of the share of FRPL students (“high share” denotes the top tercile and “low share” denotes the bottom tercile). Figures in the right panels show estimates by tercile of the share of minority students (“high-minority” denotes the top tercile and “low-minority” denotes the bottom tercile). Estimates on test scores are obtained by pooling data across subjects and grades, using district-by-cohort and state-by-year-by-subject-by-grade-by-cohort effects and weighing observations by the number of test takers. Other estimates are obtained using district-by-cohort and state-by-year-by-cohort effects and weighing observations by district enrollment. Standard errors are clustered at the district level.

Figure 7: Bond Composition Across Groups of Districts

(a) Categories sorted by effect on test scores

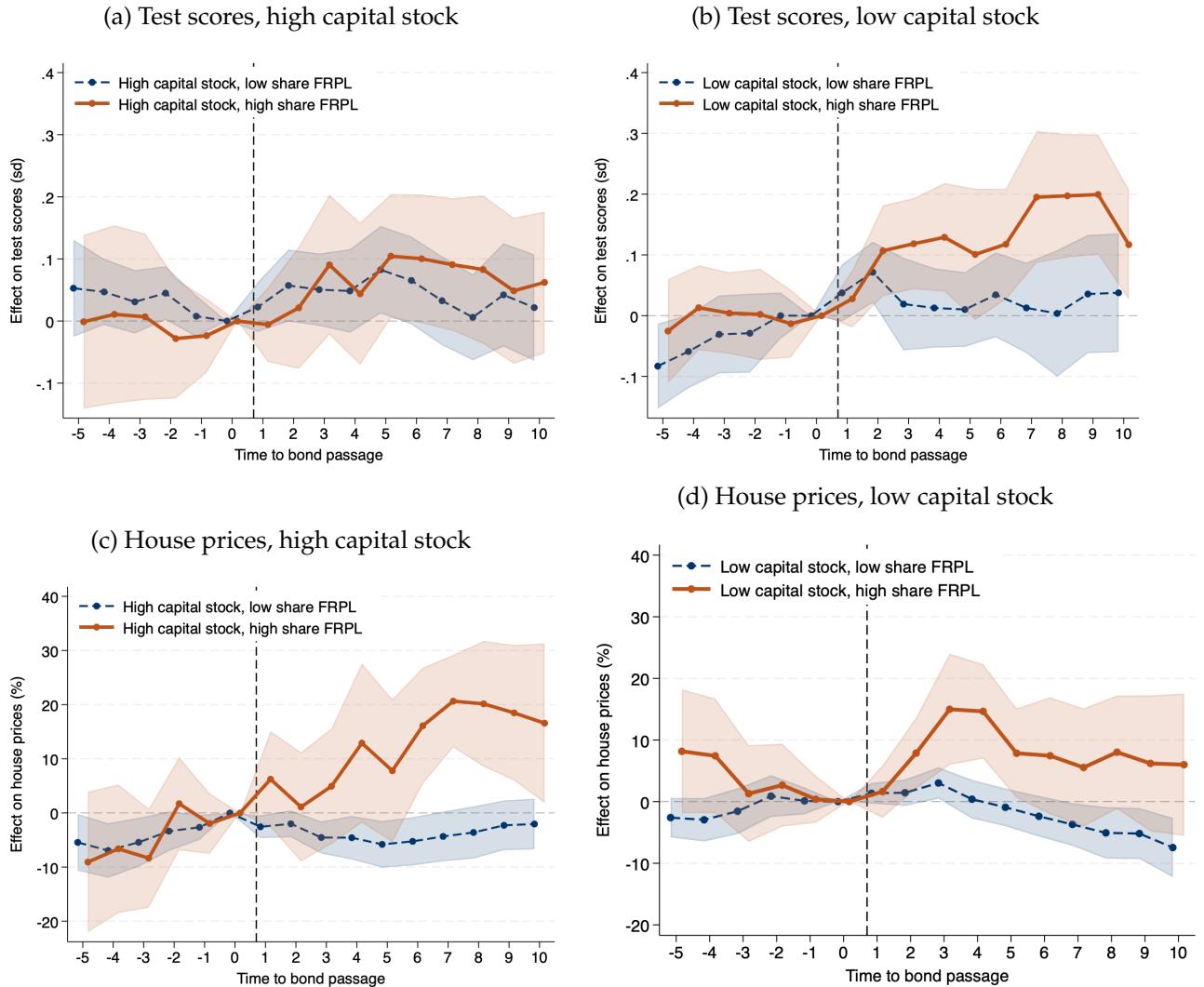


(b) Categories sorted by effect on the house price index



Note: Share of bonds by category and district group. Each bar refers to the group of districts labeled on the horizontal axis. Each bar portion refers to the share of all bonds in a given spending category. In the top panel, categories are ranked from the bottom (darker shade) to the top (lighter shade) according to their test score impact in years 3-6. In the bottom panel, categories are ranked from the bottom (darker shade) to the top (lighter shade) according to their house price index impact in years 3-6.

Figure 8: Effects of Bond Authorization By Socio-Economic Status and Initial Capital Stock



Note: Estimates and confidence intervals of the parameters β_k in equation (3), obtained using test scores (panels (a) and (b)) and the house price index (panels (c) and (d)), shown separately by capital stock and share of FRPL students. “Low capital stock” and “High capital stock” denote districts in the bottom and top 50% of the distribution of capital stock the year before the election, respectively. “High share FRPL” and “Low share FRPL” denote districts in the top and bottom terciles of the distribution of the share of FRPL students. Capital stock is calculated using data from the Census of Governments for the years 1967-2017 as the sum of capital spending over a period of 30 years, to which we apply a depreciation rate of 5%. Estimates on test scores are obtained by pooling data across subjects and grades, using district-by-cohort and state-by-year-by-cohort-subject-by-grade effects and weighing observations by the number of test takers. Effects on house prices are obtained using district-by-cohort and state-by-year-by-cohort effects and weighing observations by district enrollment. Standard errors are clustered at the district level.

Table 1: District Expenditures, Bonds, and Spending Categories: Summary Statistics

	Full sample	W/election	Analysis sample
Capital	1320.0 (2895.2)	1407.1 (3051.2)	1614.2 (3517.8)
Current	7046.9 (3940.0)	6560.1 (2597.6)	7004.5 (3025.0)
<i>Spending rules</i>			
Share w/supermajority	0.19 (0.39)	0.22 (0.42)	0.22 (0.41)
Voting requirement	0.51 (0.034)	0.52 (0.034)	0.52 (0.033)
Debt limit (share prop. value)	0.095 (0.057)	0.096 (0.058)	0.095 (0.058)
<i>Bonds</i>			
Share proposing a bond/year	0.062 (0.24)	0.11 (0.32)	0.19 (0.39)
Share approved	0.75 (0.43)	0.75 (0.43)	0.77 (0.42)
Vote margin	0.099 (0.16)	0.099 (0.16)	0.11 (0.16)
Size p.p. proposed (\$1,000)	7.67 (8.26)	7.54 (8.14)	7.66 (8.49)
<i>Categories, approved bonds</i>			
Classrooms	0.45 (0.50)	0.45 (0.50)	0.45 (0.50)
Other infrastructure	0.27 (0.44)	0.27 (0.44)	0.27 (0.44)
HVAC	0.12 (0.32)	0.12 (0.32)	0.12 (0.32)
STEM equipment	0.28 (0.45)	0.28 (0.45)	0.28 (0.45)
Safety/health	0.20 (0.40)	0.20 (0.40)	0.21 (0.41)
Athletic facilities	0.17 (0.38)	0.17 (0.38)	0.18 (0.38)
Transportation	0.31 (0.46)	0.31 (0.46)	0.34 (0.47)
Land purchases	0.13 (0.33)	0.13 (0.33)	0.13 (0.33)
<i>Demographics and outcomes</i>			
Share FRPL	0.39 (0.22)	0.38 (0.22)	0.42 (0.22)
Share Black/Hispanic	0.22 (0.26)	0.23 (0.26)	0.26 (0.27)
ELA test scores	-0.077 (0.87)	-0.084 (0.86)	-0.065 (0.86)
Math test scores	-0.11 (0.87)	-0.11 (0.86)	-0.070 (0.87)
House price index (1989 = 100)	168.9 (57.5)	175.4 (61.2)	196.1 (60.0)
Number of districts	10,146	4,683	4,353
Number of states	29	29	29

Note: Means and standard deviations of variables of interest. In all columns, the sample is restricted to states that pass the McCrary test.

Table 2: First Stage: Effects of Bond Authorization on School Expenditures

Type of expenditure:	Capital	Current	Other non-instr services
Average effect over:	(1)	(2)	(3)
1-5 years	334*** (93)	4 (30)	3 (4)
6-8 years	-216** (104)	-35 (47)	7 (7)
9-10 years	12 (110)	-65 (51)	10 (8)
District-Cohort FE	X	X	X
Year-State-Cohort FE	X	X	X
Mean of dep. var.	1,618	7,789	485
Adj. R ²	0.293	0.968	0.864
N	141,323	141,323	141,323

Note: Estimates and standard errors of linear combinations of the parameters β_k in equation (3). The dependent variables are per pupil capital spending (column 1), current spending (column 2), and spending on non-instructional services (column 3). All columns control for district-by-cohort and cohort-by-state-by-year effects. Observations are weighted by district enrollment. Standard errors in parentheses are clustered at the district level. * = 0.1; ** = 0.05; *** = 0.01.

Table 3: Effects of Bond Authorization on Student Achievement and House Prices

	Test scores			HPI
	Pooled	Math	ELA	
Average effect over:	(1)	(2)	(3)	(4)
1-4 years	0.049*** (0.013)	0.051*** (0.017)	0.049*** (0.013)	2.436* (1.292)
5-8 years	0.090*** (0.021)	0.090*** (0.026)	0.093*** (0.020)	6.847*** (2.048)
9-12 years	0.087*** (0.028)	0.070** (0.032)	0.104*** (0.028)	4.846** (1.926)
District-Cohort FE	X	X	X	X
Yr-St-Gr-Subj-Coh FE	X			
Yr-St-Gr-Coh FE		X	X	
Year-State-Coh FE				X
Enroll. shares				
Adj. R ²	0.868	0.860	0.891	0.936
N	1,048,421	507,724	540,693	85,835

Note: Estimates and standard errors of linear combinations of the parameters β_k in equation (3). The dependent variables are pooled test scores (column 1); math and reading/ELA test scores (columns 2 and 3, respectively); and the house price index (column 4). All columns control for district-by-cohort and cohort-by-state-by-year effects. Column 1 also controls for cohort-by-state-by-year-by-grade-by-subject effects, and columns 2-3 control for cohort-by-state-by-year-by-grade effects. Standard errors in parentheses are clustered at the district level. * = 0.1; ** = 0.05; *** = 0.01.

Table 4: 2SLS, Effects of Increases in Cumulative Capital Spending on Test Scores and House Prices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel (a): Test scores	Share FRPL			Share minority		Capital stock	
	All	Low	High	Low	High	High	Low
Sample:							
Cap spending per pupil (\$1,000)	0.0172*** (0.004)	0.0012 (0.005)	0.0286*** (0.008)	0.0148** (0.006)	0.0262*** (0.008)	0.0072 (0.006)	0.0240*** (0.006)
w/depreciation	0.0483	0.0033	0.0801	0.0413	0.0735	0.0201	0.0672
District FE	X	X	X	X	X	X	X
Yr-St-Gr-Subj FE	X	X	X	X	X	X	X
N	1,109,241	355,642	327,632	269,221	360,891	558,461	435,927
Panel (b): House prices	Share FRPL			Share minority		Capital stock	
	All	Low	High	Low	High	High	Low
Sample:							
Cap spending per pupil (\$1,000)	1.905** (0.650)	0.116 (0.538)	4.368** (1.604)	0.566 (0.394)	4.466*** (1.304)	2.185** (1.089)	2.763*** (0.745)
w/depreciation	5.334	0.325	12.231	1.584	12.506	6.119	7.738
District FE	X	X	X	X	X	X	X
Yr-St-Gr-Subj FE	X	X	X	X	X	X	X
N	125,868	40,328	20,075	38,697	32,041	50,994	33,883
Panel (c): House prices	Share FRPL			Share minority		Capital stock	
	All	Low	High	Low	High	High	Low
Sample:							
Bond amount per pupil (\$1,000)	0.286* (0.119)	0.149 (0.137)	0.677 (0.456)	0.297** (0.081)	0.845** (0.324)	0.484 (0.222)	0.060 (0.167)
w/depreciation	0.800	0.416	1.897	0.831	2.365	1.354	0.167
District FE	X	X	X	X	X	X	X
Yr-St-Gr-Subj FE	X	X	X	X	X	X	X
N	125,868	40,328	20,075	38,697	32,041	50,994	33,883

Note: 2SLS estimates and standard errors of the parameter ρ in equation (5). The dependent variables are standardized test scores (panel (a)) and the house price index (panels (b) and (c)). *Cap spending* denotes cumulative capital spending increases in the previous 10 years. *Bond amount* denotes cumulative amounts of authorized bond in the previous 10 years. Column 1 is estimated on the full sample of districts; columns 2 and 3 on the subsamples of districts with a share of FRPL students in the bottom and top terciles, respectively; columns 4 and 5 on the subsamples of districts with a share of minority (Black and/or Hispanic) students in the bottom and top terciles, respectively; and columns 6 and 7 on districts with a capital stock above and below the national median, respectively. In panel (a), we pool data from all grade and years and control for district-by-cohort and cohort-by-state-by-year-by-grade-by-subject fixed effects, weighing observations by the number of test takers. In panels (b) and (c), we control for district-by-cohort and cohort-by-state-by-year fixed effects, weighing observations by district enrollment. Coefficients in the row *with depreciation* are obtained considering an average life span of 30 years for capital investments and a depreciation rate of 9%. Bootstrapped standard errors in parentheses are clustered at the district level. * = 0.1; ** = 0.05; *** = 0.01.

Table 5: Shapley-Owen Decomposition of Variation in Bond Effects on Test Scores

Group of variables	Explained variation R_j^2	Share of total R^2
State	1.6%	10.6%
Demographics	2.1%	14.3%
Bond characteristics	1.6%	10.9%
$\ln(\text{Capital stock})$	0.1%	0.4%
Demographics x bond characteristics	8.9%	61.0%
Bond characteristics x $\ln(\text{Capital stock})$	0.7%	5.0%
All	14.6%	100 %

This table shows a Shapley-Owen decomposition of the adjusted R^2 of a regression of district-specific effects of bond authorization on the groups of variables indicated in the text. The method used for the decomposition is described in detail in the text. *All* reports the adjusted R^2 of a regression with all groups of variables included.