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

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This paper identifies which investments in school facilities help students and are valued by homeowners. Using novel data on school district bonds, test scores, and house prices for 29 U.S. states and a research design that exploits close elections with staggered timing, we show that increased school capital spending raises test scores and house prices on average. However, impacts differ vastly across types of funded projects. Spending on basic infrastructure (such as HVAC) or on the removal of pollutants raises test scores but not house prices; conversely, spending on athletic facilities raises house prices but not test scores. Socio-economically disadvantaged districts benefit more from capital outlays, even conditioning on project type and the existing capital stock. Our estimates suggest that closing the spending gap between high- and low-SES districts and targeting spending towards high-impact projects may close as much as 25% of the observed achievement gap between these districts.

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 are valued by homeowners. Using novel data on school district bonds, test scores, and house prices for 29 U.S. states and a research design that exploits close elections with staggered timing, we show that increased school capital spending raises test scores and house prices on average. However, impacts differ vastly across types of funded projects. Spending on basic infrastructure (such as HVAC) or on the removal of pollutants raises test scores but not house prices; conversely, spending on athletic facilities raises house prices but not test scores. Socio-economically disadvantaged districts benefit more from capital outlays, even conditioning on project type and the existing capital stock. Our estimates suggest that closing the spending gap between high- and low-SES districts and targeting spending towards high-impact projects may close as much as 25% of the observed achievement gap between these districts.

JEL Classification: H41, H75, I22, I24, R30

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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 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 state). However, disadvantaged students benefit more from increased spending 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 impacts across states can be 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. Applying text-analysis techniques to this corpus of text, 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 the average test scores of students in each district. Our starting point 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 Educational 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 same procedure as Reardon et al. (2017) to harmonize scores across states and years. The resulting novel 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 final dataset covers approximately 14,000 bond elections in 29 states and 10,146 districts, enrolling 71% of all students in the U.S.

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 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 $1,500 per pupil in

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[1]This 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.

the five years following bond authorization. Test scores gradually increase after an authorization, reaching a 0.08 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.

House prices also increase by about 9% nine years after an authorization, indicating that home-owners 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 home-owners' valuation is similar to this spending increase, which in turn implies that spending levels are on average efficient. We also find that a bond authorization leads to small changes in the socio-demographic composition of school districts, likely due to household sorting across districts. This is evident from an increase in the shares of non-low SES and White students in the districts. However, this compositional change only accounts for a small share of the increase in test scores and house prices.

These average impacts on test scores and house prices, though, mask dramatic differences in the effectiveness and efficiency 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 esti-mates, 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 but has no significant effect on house prices. In contrast, spending on athletic facilities, land purchases, or buses does not impact learning but raises house prices. This im-plies 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.

The effectiveness and efficiency of capital spending also depend on *who* is exposed to the spending increase. The positive impacts of bond authorization on both test scores and house prices are concentrated in districts with a large share of low socio-economic status (SES) or minority (black and Hispanic) students. These districts see larger spending increases after an authorization and tend to prioritize learning-enhancing and price-increasing 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 and house prices only in more disadvantaged districts. This indicates that capital spending is most effective in those districts and, at baseline, is provided at an inefficiently low level.

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 helps students thrive. 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, 2015; Jackson et al., 2016; Hyman, 2017;

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Lafortune et al., 2018; Jackson, 2020), labor market outcomes (Jackson et al., 2016), and intergenerational 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 (Abott 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 study is also related to a set of studies, pioneered by Cellini et al. (2010), that have estimated the effects of school capital expenditure on students and the real estate market, reaching conflicting conclusions. Most of these studies leverage evidence from single 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 individual school districts (Neilson and Zimmerman, 2014; Lafortune 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 Lafortune 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.

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 (Jackson and Mackevicius, 2023; Handel and Hanushek, 2022). 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-seven states require a simple majority, i.e., 50% of those who turn out to vote.[6] The remaining 10 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), 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 Fremont Union High School District, CA

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[2]87% 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.

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

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

[5]The 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.

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

[7]Forty states limit the amount of debt districts can issue, from 2% of assessed property valuation in Indiana to 30% in Arizona (Appendix Figure A3)

were called to vote on the following bond proposal in June 2022:

> *"To upgrade classrooms, science labs, and facilities for technology, arts, math, and career technical education; improve ventilation systems; provide essential seismic safety and accessibility upgrades; and, construct and repair sites and facilities, shall the measure authorizing $275 million in Fremont Union High School District bonds at legal rates, raising an estimated $18.2 million annually until approximately 2052, at projected rates of 1.5 cents per $100 of assessed valuation, with citizen's oversight and all funds staying local, be adopted?"*

In this referendum, 55.7% 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 will be 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 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

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[8]In 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).

[9]California'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).

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 5.3.

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 Abott 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 incomplete[11] and from three additional states 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

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[10]The 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).

[11]For 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.

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. 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. Each year, 6% of all districts propose a measure and 75% of these proposals are authorized (Table 1). The share of authorized bonds is higher (77%) among districts with a higher share of low-SES students. Bonds are comparable in terms of interest rates, with a standard deviation of 0.66% (Appendix Figure A4).[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 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.

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]

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

[13]The earliest data available are in 1990 for six states; we have limited coverage across states until the early 2000s. Appendix Figure A5 shows the number of states with district bond data and the number of bonds in each sample year.

[14]Appendix Figure A4 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.

[15]The 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.

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

We describe the distribution of bonds across categories more in depth 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 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 dimensions very challenging. A notable exception is the NAEP, taken annually by a sample of grade 4-8 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.[17] 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 avail-

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[17]See Appendix Figure A6 for a map of the first available year of data for each state.

able 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 1995 for some. Because test scores are standardized, all estimates are expressed in district-level standard deviations.[18] 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 HPI is available for a balanced panel of 63,122 Census tracts for 1989-2021.[19] 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

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[18]On 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).

[19]These tracts are based on the 2010 Census tract geography.

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 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

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

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.[20]

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 a measure more than once. 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 year. Districts that authorize a bond propose and authorize new bonds 3.9 and 4.0 years later, respectively (Appendix Figure A7).

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

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[20]Close elections tend to occur in districts that spend less on average, in states with a supermajority requirement, and for larger bonds (Table A1). Following CFR, to improve power we retain all data in estimation, rather than focusing on observations around the cutoff v.

equation (1) that includes controls for a district's history of bond proposals:

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

where M_{jt-k} equals one if the district proposed a bond measure k years prior to t.[21] In practice, controlling for M_{jt-k} ensures that we estimate treatment effects by comparing districts with the same bond history. This estimator is consistent if effects are (i) additive across bonds, (ii) heterogeneous across time elapsed since the election, and (iii) uncorrelated with the timing of the election.[22] 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 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.[23]

4.3 Addressing Challenge #3: Stacked Dynamic Regression Discontinuity

Having to assume that treatment effects are uncorrelated with treatment timing is not ideal for our context. For example, this assumption could be violated if bond impacts vary across spending categories and bonds in different categories have a different propensity to be proposed and passed over time. 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

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[21]Equation (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).

[22]On 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".

[23]A number of works, 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.

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.[24] We then estimate the following equation via OLS on the resulting dataset:

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

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

Two details of this strategy are worth stressing. 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 year* as the treated districts (due to the inclusion of leads and lags of M_{jct-k}), but *barely fail* to approve them (due to the inclusion of leads and lags of $P^g(V_{jct-k}, \delta_k^g)$, which helps isolate variation from close elections). Clean controls therefore remain untreated after c for reasons that are as good as random.[25] 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*

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[24]This "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).

[25]The sample would indeed be endogenously selected if we were to consider as clean controls those districts that never *propose* any bonds after c, because proposing a bond is an endogenous choice of each district.

bond history).[26]

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 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.[27] We present estimates obtained using these strategies in Sections 5, 6, and 7. Our results are largely robust to the use of these approaches.

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 A8).[28] We therefore exclude these states from our analysis.

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[26]This 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.

[27]To 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.

[28]These 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. This issue does not seem to occur in other states.

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 low-SES 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 A9 and A10).

Third, we examine pre-election differences in outcomes between treated units and their clean controls, captured by β_k for $k < 0$ in equation (3). The absence of significant differences (which we show and discuss later) suggests that 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   Average Effects of Bond Authorization

We begin by estimating the average effects of bond authorization on three main outcomes: capital spending per pupil (the first stage), test scores (a measure of spending effectiveness), and house prices (a measure of efficiency). 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).[29] 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).

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[29]Six 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 receive a small amount of state aid to compensate for the lost bond revenues.

Bond authorization does not affect non-capital (i.e. "current") spending (Table 2, column 2 and Appendix Figure A11).[30] 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 and house prices as the effect of increased capital spending. We present these effects next.

5.2 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:

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

where Y_{jrwct} 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 weigh 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 0.09 standard deviations (sd) higher level 8 years post-election (Figure 3, panel (a)).

Table 3 summarizes the impact of bond authorization on test scores. In districts that marginally approve a bond proposal, test scores are 0.041 sd higher on average one to four years after an election, 0.079 sd higher five to eight years after, and 0.073 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 0.095 increase nine to twelve years post election) compared with Math (a 0.050 increase nine to 12 years after an election), Table 3, columns 2 and 3).

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

Effect of a $1,000 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 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 spending over the previous decade, denoted as $\sum_{\ell=1}^{10} K_{jct-\ell}$:

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

In this model, ρ is the per dollar effect of changes in cumulative spending 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 increase in spending over a time span of 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%. 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.[31]

Taken together, these average estimates indicate that increasing spending on capital projects is an effective way to raise achievement. A possible explanation for this effect is an improvement in students' learning experience and classroom conditions. We revisit this hypothesis later in the paper, when we examine heterogeneity in effects by category of spending.

5.3 Effects on House Prices

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

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[31]Jackson 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: 0.08 sd, 0.07 sd, and 0.06 sd for depreciation rates of 4%, 6%, and 8%, respectively.

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). 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.[32]

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 trends between districts that approve a measure in c 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)). This indicates that households value increases in school capital spending more than the additional local taxes they are asked to contribute.

A possible interpretation for this finding is that the level of spending on school facilities is on average inefficiently low. However, reduced-form estimates do not 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

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[32]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).

prices, equal to 0.8% after accounting for the project life and depreciation (Table 4, panel (b), column 1). We can thus conclude that, on average, school capital spending is efficient across the U.S. In Section 7, however, we show that inefficiencies exist in some contexts.

5.4 Effects on Student Sorting and Implications for Student Achievement

Household sorting after an increase in school capital spending may change the composition of a district's student body.[33] If this change is large enough, it could be responsible for part or all of the observed increases in test scores. While the absence of student-level data prevents us from tracking students over time, we can assess district-level changes in the share of students belonging to various socio-demographic groups in the aftermath of a bond approval. To this end, we re-estimate equation (4) with the shares of white and high-SES students as dependent variables.

We find that the approval of a bond measure is followed by small changes in the composition of the student body. In districts that approve a bond measure, the share of high-SES students is 3 percentage points higher seven years after the election relative to before (a 4 percent change relative to an average share of 0.73, Appendix Figure A13, panel (a) and Table 3, column 7). The share of white students and total enrollment are instead largely unaffected (Table 3, columns 5 and 6). These small compositional changes, though, cannot explain the increase in test scores and house prices: Stacked DRD estimates are only slightly attenuated when we control for the share of students in each socio-demographic group in each district and year (Appendix Figure A13, panels (b) and (c)).

5.5 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.

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[33]Evidence 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).

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 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 Matters? Differences in Impacts Across Spending Categories

The results presented so far indicate that the approval of school capital bonds leads to increases in test scores and house prices. 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 and house prices.

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)). 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:

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

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. 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). 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. For robustness, in Appendix Figure A27 we also present estimates obtained controlling for indicators for other categories present in the same bond, interacted with state-by-cohort-by-year fixed effects.

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 0.1 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)).

Notably, the categories that most increase test scores are not the same as those that most increase house prices. In fact, 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 categories, including HVAC and safety/health. The correlation between test score and house prices estimates is -0.07. This suggests that households 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.[34] Estimates are largely robust to the use of alternative estimation procedures (Appendix Figure A29).

Since many bonds contain more than one category but do not specify how revenues will be split across categories, we cannot estimate the dollar impacts of bonds by category with the 2SLS model we used in Section 5. Yet, with the exception of transportation bonds (which tend to be smaller), we do not find significant differences in spending increases across bonds in different categories (Appendix Figure A28, panel (c)).[35] This suggests that differences in spending amounts are unlikely

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[34]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)).

[35]Appendix Figure A28 shows linear combinations of estimates of $\beta_{k,p}$ in equation (6), using per pupil capital spending as the dependent variable, separately by category p.

to drive the heterogeneity in impacts.

It is possible, though, that districts that approve bonds in different categories may 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; Lafortune 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 ("high SES" and "low SES," respectively). In our data, 8% of all low-SES districts propose a bond each year and 73% of these proposals are authorized. By contrast, 6% of all high-SES districts propose a bond each year, 77% of which are authorized (Table 1).

We find large differences in the impact of bond authorization across high- and low-SES districts. While test scores are on a flat trend in both groups prior to an election, they increase rapidly in low-SES districts after the election, reaching a 0.13 sd higher level after 7 years (Figure 5, left panel (a)). In high-SES districts, test scores remain unchanged post-election.

In low-SES districts, bond authorization also produces a 15% increase in house prices 7 years after an election (Figure 5, left panel (b)). In high-SES districts, instead, the effect of bond authorization on house prices is indistinguishable from zero.

There are at least three reasons why bond impacts may be larger in low-SES 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 and house prices. 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 that low-SES 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 low-SES districts, and by $800 in high-SES districts (Figure 5, left panel (c)). Yet, these differences are not enough to explain the heterogeneity in bond impacts. 2SLS test score estimates indicate that a $1,000 increase in cumulative spending increases test scores by 0.08 sd in low-SES districts, whereas it does not produce any detectable changes in high-SES districts (Table 4, panel (a), columns 3 and 2, respectively). 2SLS estimates on house prices are also larger in low-SES districts (2.1% compared with 0.4% in high-SES districts), despite being imprecisely estimated (Table 4, panel (a), columns 3 and 2, respectively). This suggests that school capital investments are inefficiently low in low-SES districts.

Low-SES districts are also slightly more likely than high-SES 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 high-SES), safety and health (14% compared to 9% for high-SES), and STEM (16% compared to 14%). They are also more likely to invest in classroom space, which raises both test scores and house prices (29%, compared to 20% for high-SES, Figure 6).

Yet, differences in bond composition alone are unable to explain the differences in impacts between high- and low-SES districts. Low-SES 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 low-SES and high-SES districts.[36] For example, authorizing a bond to fund HVAC systems increases test scores by 0.27 sd in low-SES districts but leaves them unaffected in high-SES districts (with an estimate of -0.3 sd in panel (a), indistinguishable from zero). Bonds that

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[36]In Appendix Figure A35 we omit the transportation category due to a small number of observations (among high-SES districts, it only includes 5 observations in the control group).

fund other infrastructure and STEM equipment also have larger impacts in low-SES districts (equal to 0.32 sd and 0.21 sd, respectively) compared to high-SES districts (-0.05 and 0.1, both indistinguishable from zero). Similarly, low-SES districts see larger increases in house prices following the authorization of bonds that fund athletic facilities (a 30% increase, compared with 10% for high-SES districts, panel (b)), STEM equipment (a 27% increase, compared with 0.1%), land purchases (a 27% increase, compared with 5%), and classroom space (a 22% increase, compared with 5%). These results indicate that the larger mean effects of bond authorization on low-SES districts are not uniquely driven by bond composition, but also by low-SES students benefiting more from a given investment and house prices responding more in these districts.

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 7 years (Figure 5, right panel (a)). They also see a 15% increase in house prices (right panel (b)). Instead, "low-minority" districts (with a share in the bottom tercile) experience much smaller increases in both outcomes (0.04 sd in test scores and 3% in house prices 7 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 on both test scores and house prices is much larger in high-minority districts (Table 4, columns 4 and 5, both panels). High-minority districts approve more bonds with items such as HVAC and safety and health, which increase test scores, and classroom space, which also increases house prices (Figure 6). However, they also experience larger returns within those categories, both on test scores and house prices (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 low-SES districts tend to spend less on average (Table 1). They are also more likely to be located in urban

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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-SES and low-SES districts.

To explore the role of initial facility conditions, we would ideally like to observe detailed information on each district's school buildings. 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 low-SES 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 low-SES students in the top and bottom tercile. The results of this exercise are shown in Figure 7. 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 low-SES districts compared with high-SES ones (with a 0.1 sd versus a 0.05 sd increase five years after an election, Figure 7, panel (a)). Among districts with below-median capital stock, instead, test score effects are much larger in low-SES districts, with a 0.2 sd increase 7 years after an election (Figure 7, panel (b)).

We find similar results on house prices, but large differences by student SES even in districts with above-median stock. In these districts, house prices increase by over 10% four years after an election in low-SES districts, whereas they do not change (and, if anything, they decline slightly) in high-SES districts (Figure 7, panel (c)). Among districts with below-median stock the impact

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is larger for low-SES districts, and equal to 12% and 10% four and eight years after an election, respectively (Figure 7, panel (d)).

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-SES 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.

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). Differences in impacts between these states appear to be driven by the composition of the student body and the spending categories. For example, Ohio has a significant portion of bonds in districts with high shares of low-SES students and funding infrastructures. Texas, on the other hand, has a small share of bonds in districts serving low-SES students and a large share of bonds that fund classrooms and athletic facilities (Appendix Figure A38, panel(b)).

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 low-SES 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 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).[37] 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). 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 5 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.

8 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 0.08 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

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[37]On 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.

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. As a result, low-SES and minority students see the largest benefits from bond authorization.

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 the same data as this paper.

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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)
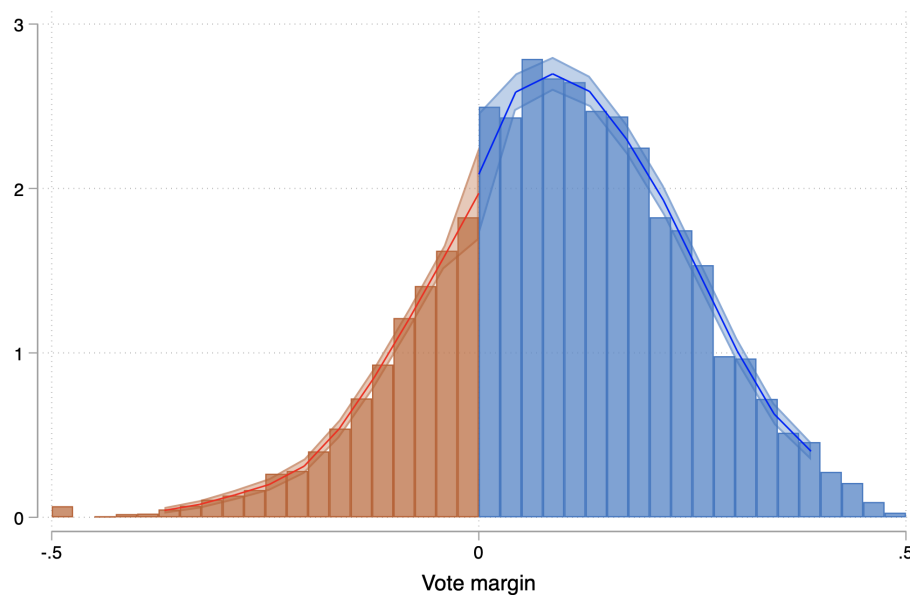
(b) Stacked data (P-value of McCrary test = 0.24)
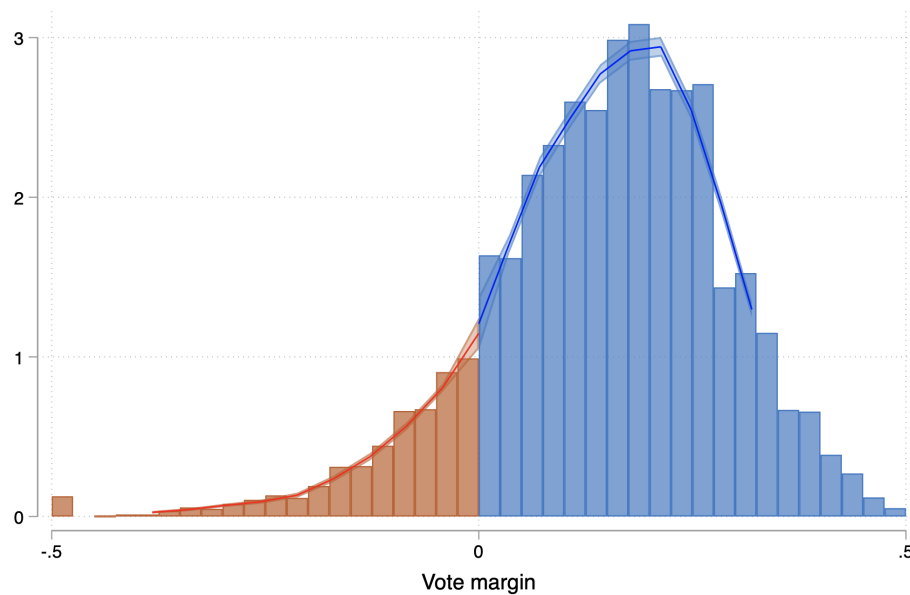
Notes: Histograms of the vote margin, defined as the difference between the share of votes in favor of the proposed measure and the required majority in each state. The lines and confidence intervals visually show the result of a McCrary (2008) test for the discontinuity in the density function at zero, 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
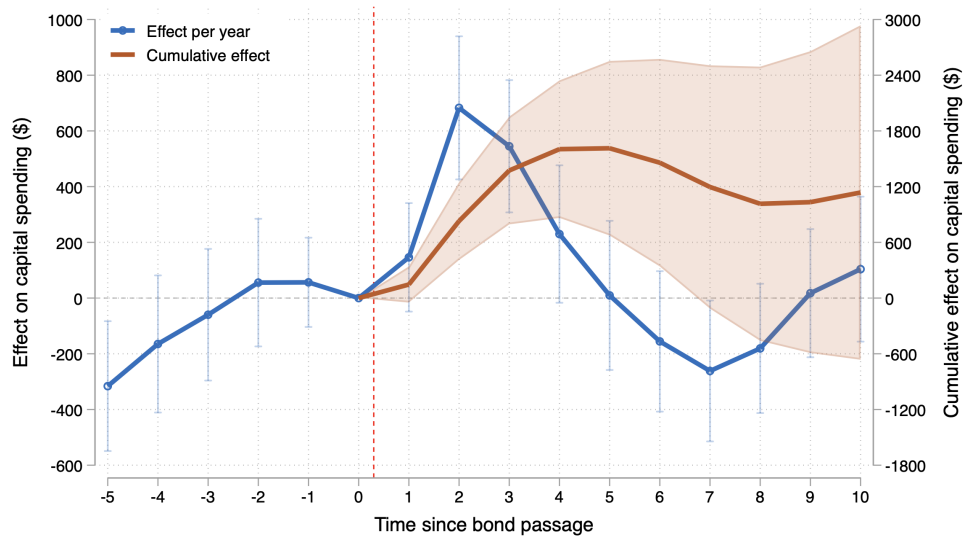
Notes: The blue line with circle markers shows estimates and confidence intervals of the parameters β_k in equation (3), obtained using capital spending per pupil as the dependent variable. The orange continuous line shows cumulative effects, calculated as the running sum of coefficients since time 0. 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
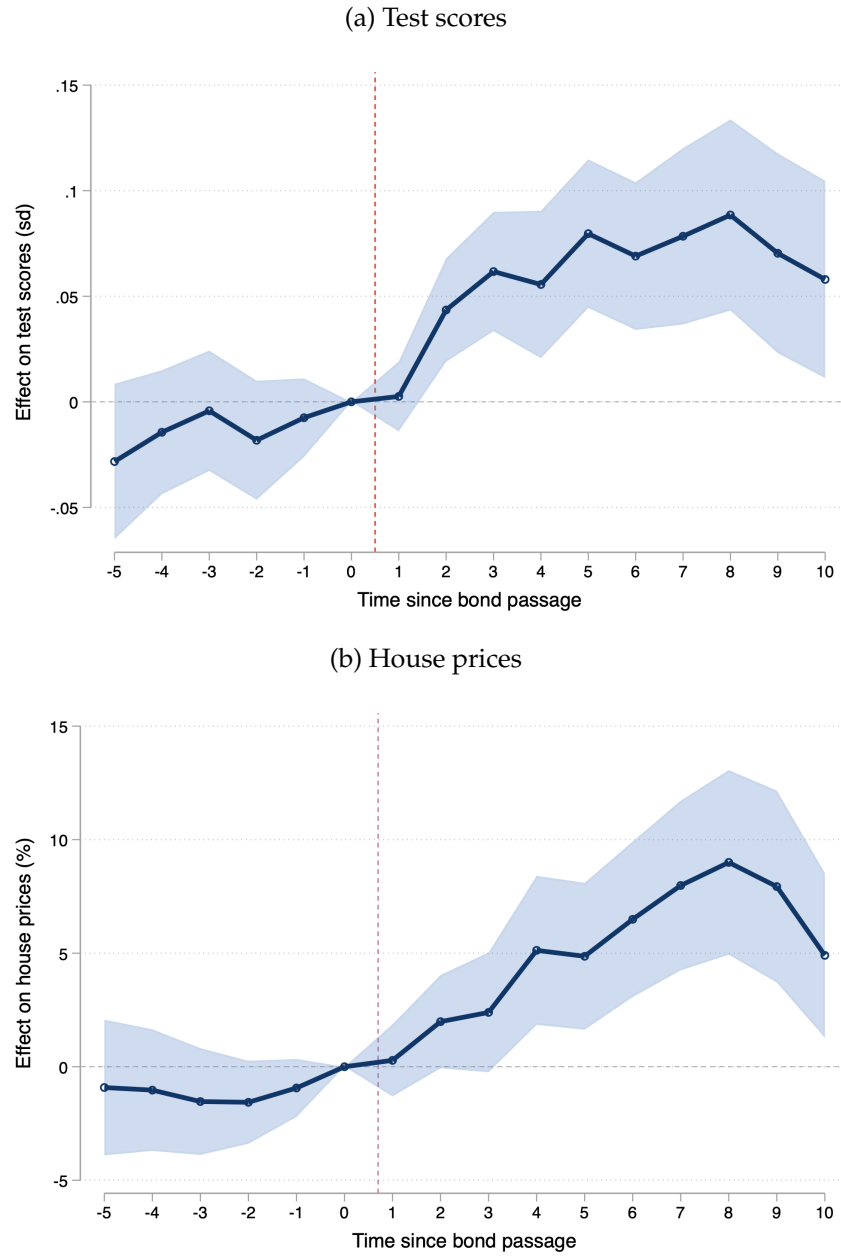
(a) Test scores

(b) 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
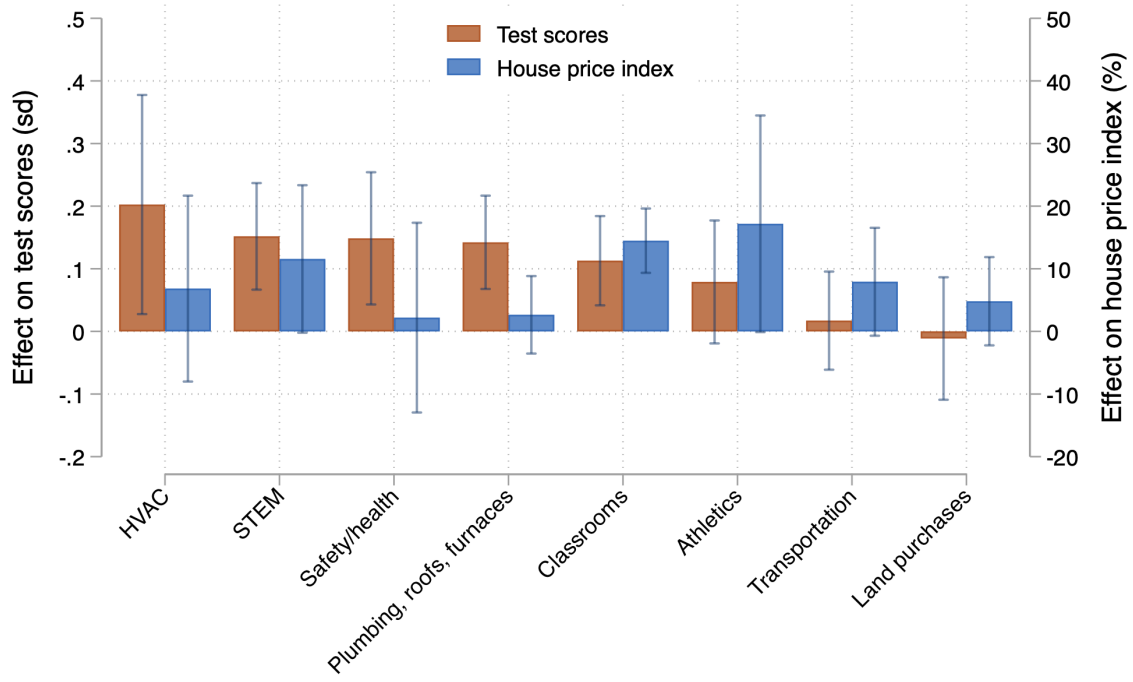
Note: Point estimates and confidence intervals of an average of the parameters $\beta_{k,p}$ in equation (6) for $k \in [3, 6]$, shown separately for each spending category p. The orange series is estimated using test scores as the dependent variable, pooled across subjects and grades, using district-by-cohort 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. Confidence intervals are calculated using standard errors clustered at the district level.

Figure 5: Effects of Bond Authorization By Student Demographics

(a) Test scores

(b) House prices

(c) Capital spending

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 disadvantaged students ("low-SES" denotes the top tercile and "high-SES" 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 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 6: Bond Composition Across Groups of Districts

(a) Categories sorted by effect on test scores

Legend: HVAC, STEM, Safety/health, Other infr, Classrooms, Athletics, Transportation, Land
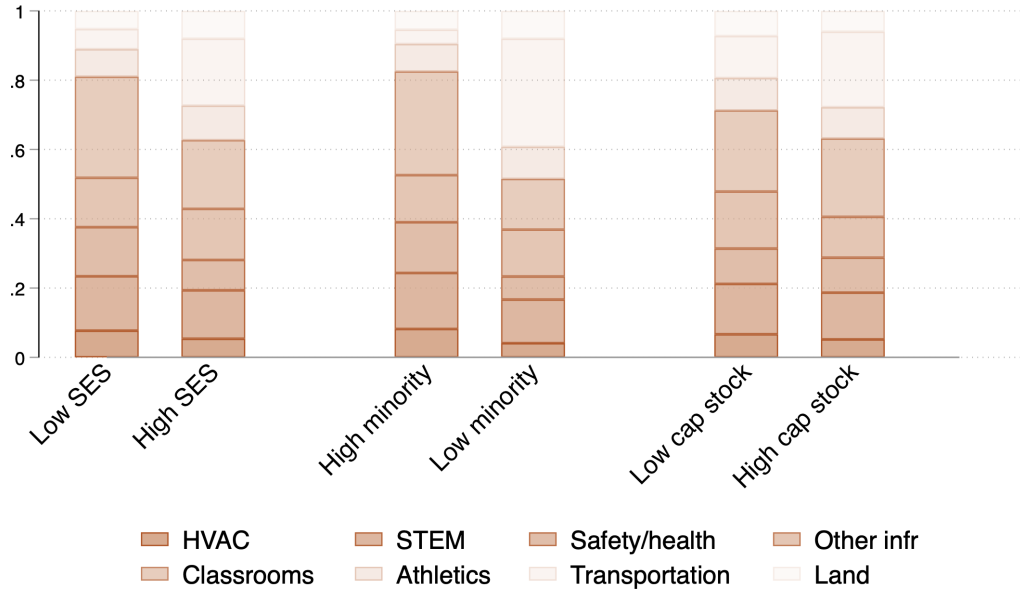
(b) Categories sorted by effect on the house price index

Legend: Athletics, Classrooms, STEM, Transportation, HVAC, Land, Other infr, Safety/health
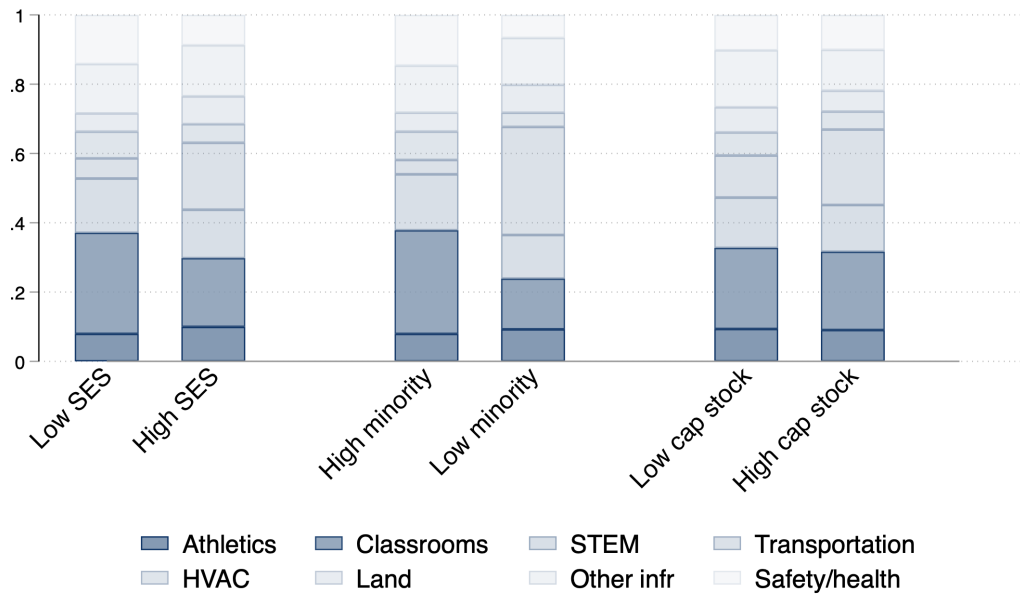
Note: Share of bonds by category and district group. Each bar refers to the group of districts labeled on the horizontal axis. Each bar portion refers to the share of all bonds in a given spending category. In the top panel, categories are ranked from the bottom (darker shade) to the top (lighter shade) 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 7: Effects of Bond Authorization By Socio-Economic Status and Initial Capital Stock
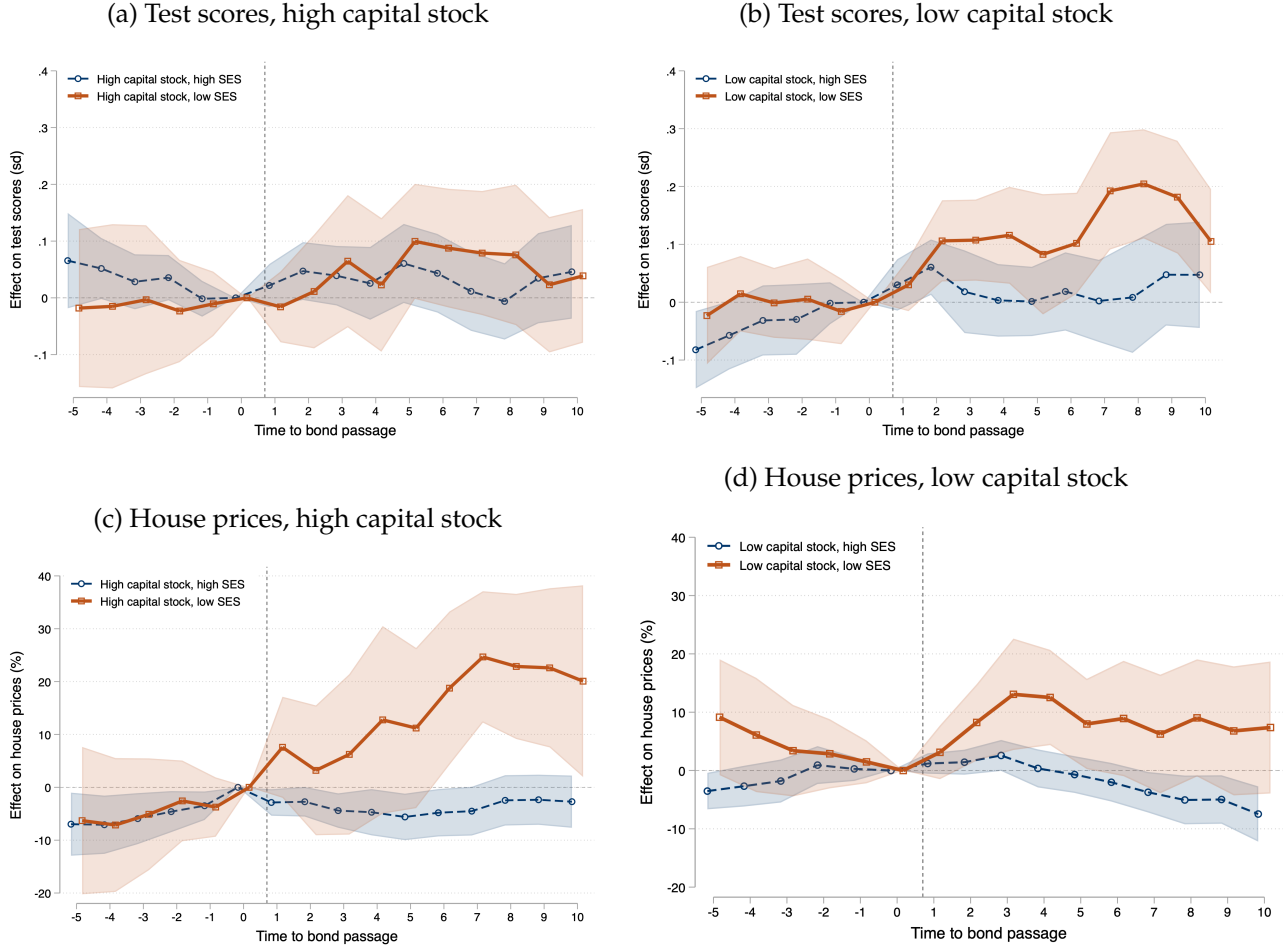
(a) Test scores, high capital stock

(b) Test scores, low capital stock

(d) House prices, low capital stock

(c) House prices, high 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 SES 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. "How-SES" and "low-SES" denote districts in the top and bottom terciles of the distribution of the share of low-SES 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 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

| | | Share of low-SES students in | |
	Full sample	bottom tercile ("high-SES")	top tercile ("low-SES")
Capital	1320.0	1366.5	1268.9
	(2895.2)	(2526.4)	(3094.7)
Current	7046.9	7256.5	6691.3
	(3940.0)	(2882.5)	(2775.0)
Spending rules			
Share w/supermajority	0.19	0.16	0.26
	(0.39)	(0.36)	(0.44)
Voting requirement	0.51	0.51	0.52
	(0.034)	(0.032)	(0.032)
Debt limit (share prop. value)	0.095	0.089	0.090
	(0.057)	(0.046)	(0.068)
Bonds			
Share proposing a bond/year	0.062	0.081	0.062
	(0.24)	(0.27)	(0.24)
Share approved	0.75	0.73	0.77
	(0.43)	(0.44)	(0.42)
Vote margin	0.099	0.076	0.12
	(0.16)	(0.13)	(0.17)
Size p.p. proposed ($1,000)	7.67	7.23	7.76
	(8.26)	(7.76)	(7.94)
Categories, approved bonds			
Classrooms	0.45	0.37	0.63
	(0.50)	(0.48)	(0.48)
Other infrastructure	0.27	0.28	0.31
	(0.44)	(0.45)	(0.46)
HVAC	0.12	0.10	0.17
	(0.32)	(0.30)	(0.37)
STEM equipment	0.28	0.26	0.34
	(0.45)	(0.44)	(0.47)
Safety/health	0.20	0.16	0.31
	(0.40)	(0.37)	(0.46)
Athletic facilities	0.17	0.19	0.17
	(0.38)	(0.39)	(0.38)
Transportation	0.31	0.36	0.13
	(0.46)	(0.48)	(0.33)
Land purchases	0.13	0.15	0.11
	(0.33)	(0.36)	(0.32)
Demographics and outcomes			
Share low-SES	0.39	0.20	0.59
	(0.22)	(0.13)	(0.18)
Share Black/Hispanic	0.22	0.083	0.44
	(0.26)	(0.12)	(0.29)
ELA test scores	-0.077	0.42	-0.63
	(0.87)	(0.73)	(0.79)
Math test scores	-0.11	0.36	-0.63
	(0.87)	(0.75)	(0.81)
House price index (1989 = 100)	168.9	170.3	174.5
	(57.5)	(53.4)	(64.2)
Number of districts	10,146	2,588	2,556
Number of states	29	25	27

Note: Means and standard deviations of variables of interest.

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	323***	-12	2
	(96)	(30)	(4)
6-8 years	-199*	-35	7
	(112)	(48)	(7)
9-10 years	61	-65	9
	(115)	(48)	(9)
District-Cohort FE	X	X	X
Year-State-Cohort FE	X	X	X
Mean of dep. var.	1,650	7,929	488
Adj. R^2	0.286	0.977	0.872
N	128,392	128,392	128,392

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 | Enrollment | | | Test | HPI |
| | Pooled | Math | ELA | | ln(Enrollment) | White | High-SES | scores | |
Average effect over:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1-4 years	0.041***	0.039**	0.043***	2.447*	0.005	0.004	0.010***	0.039***	1.713
	(0.014)	(0.017)	(0.013)	(1.343)	(0.005)	(0.002)	(0.003)	(0.013)	(1.175)
5-8 years	0.079***	0.075***	0.085***	7.085***	0.012	0.006	0.021***	0.068***	5.732***
	(0.021)	(0.026)	(0.021)	(2.097)	(0.010)	(0.004)	(0.006)	(0.020)	(1.856)
9-12 years	0.073**	0.050	0.095***	4.972**	0.024*	0.005	0.021***	0.064**	3.631*
	(0.028)	(0.031)	(0.029)	(2.013)	(0.014)	(0.004)	(0.007)	(0.028)	(1.884)
District-Cohort FE	X	X	X	X	X	X	X	X	X
Yr-St-Gr-Subj-Coh FE	X							X	
Yr-St-Gr-Coh FE		X	X						
Year-State-Coh FE				X	X	X	X		X
Enroll. shares								X	X
Adj. R^2	0.874	0.864	0.896	0.936	0.998	0.990	0.932	0.875	0.940
N	1,109,241	537,664	571,559	85,854	128,401	128,154	124,840	1,087,483	82,960

Note: Estimates and standard errors of linear combinations of the parameters β_k in equation (3). The dependent variables are pooled test scores (columns 1 and 8); math and reading/ELA test scores (columns 2 and 3, respectively); the house price index (columns 4 and 9); the natural logarithm of total enrollment (column 5); and the share of enrolled students who are white (column 6) and high-SES (column 7). All columns control for district-by-cohort and cohort-by-state-by-year effects. Columns 1 and 8 also control 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. Columns 8 and 9 additionally control for the share of white and low-SES students in each district and year. 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 low SES		Share minority		Capital stock	
Sample:	All	Low	High	Low	High	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cap spending ($1,000)	0.0172***	0.0012	0.0286***	0.0148**	0.0262***	0.0072	0.0240***
	(0.005)	(0.006)	(0.008)	(0.006)	(0.008)	(0.006)	(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 low SES		Share minority		Capital stock	
Sample:	All	Low	High	Low	High	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cap spending ($1,000)	0.283*	0.158	0.745	0.298*	0.785**	0.513*	0.054
	(0.151)	(0.193)	(0.490)	(0.149)	(0.333)	(0.286)	(0.202)
w/depreciation	0.793	0.443	2.086	0.835	2.197	1.437	0.151
District FE	X	X	X	X	X	X	X
Yr-St-Gr-Subj FE	X	X	X	X	X	X	X
N	117,072	37,193	17,424	37,058	28,182	46,799	30,451

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 (panel (b)). Column 1 is estimated on the full sample of districts; columns 2 and 3 on the subsamples of districts with a share of low-SES 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 panel (b), 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. $* = 0.1$; $** = 0.05$; $*** = 0.01$.

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

Online Appendix

Barbara Biasi, Julien Lafortune and David Schönholzer
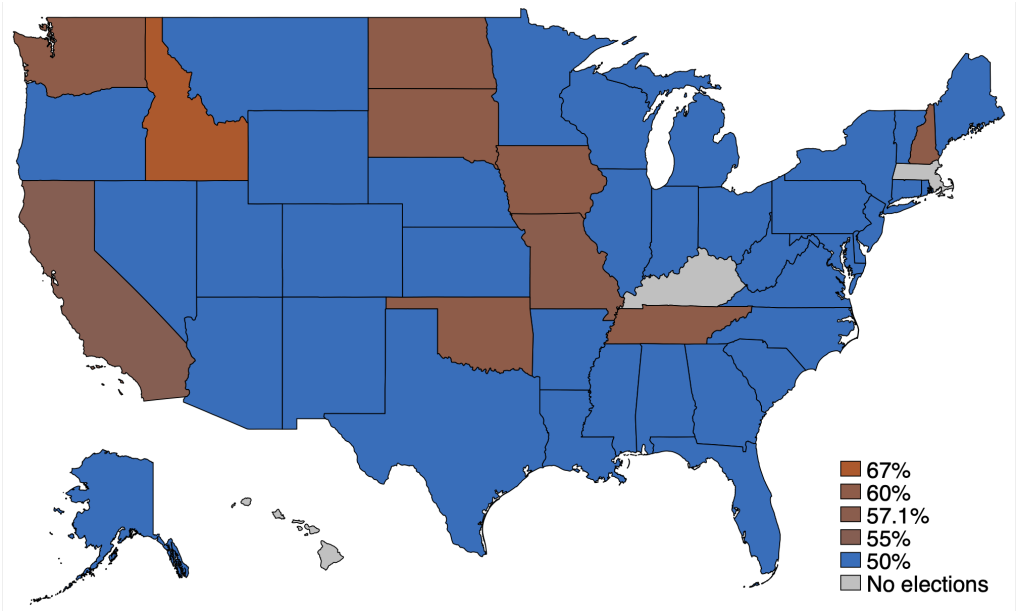
List of Tables

List of Figures

A   Additional Figures and Tables

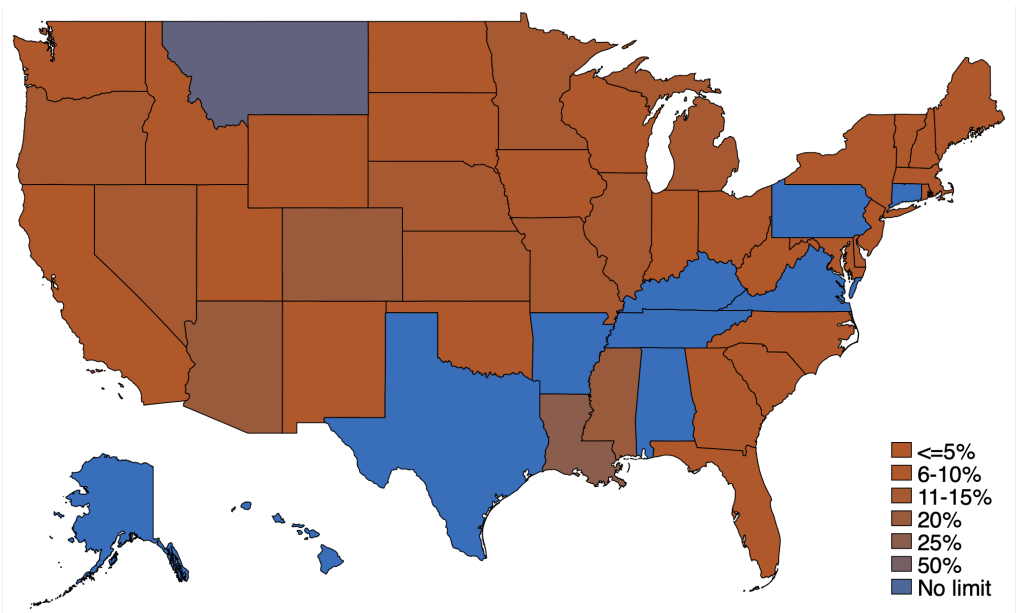Figure A1: District-level Capital Expenditures (per-pupil, 2015-16)
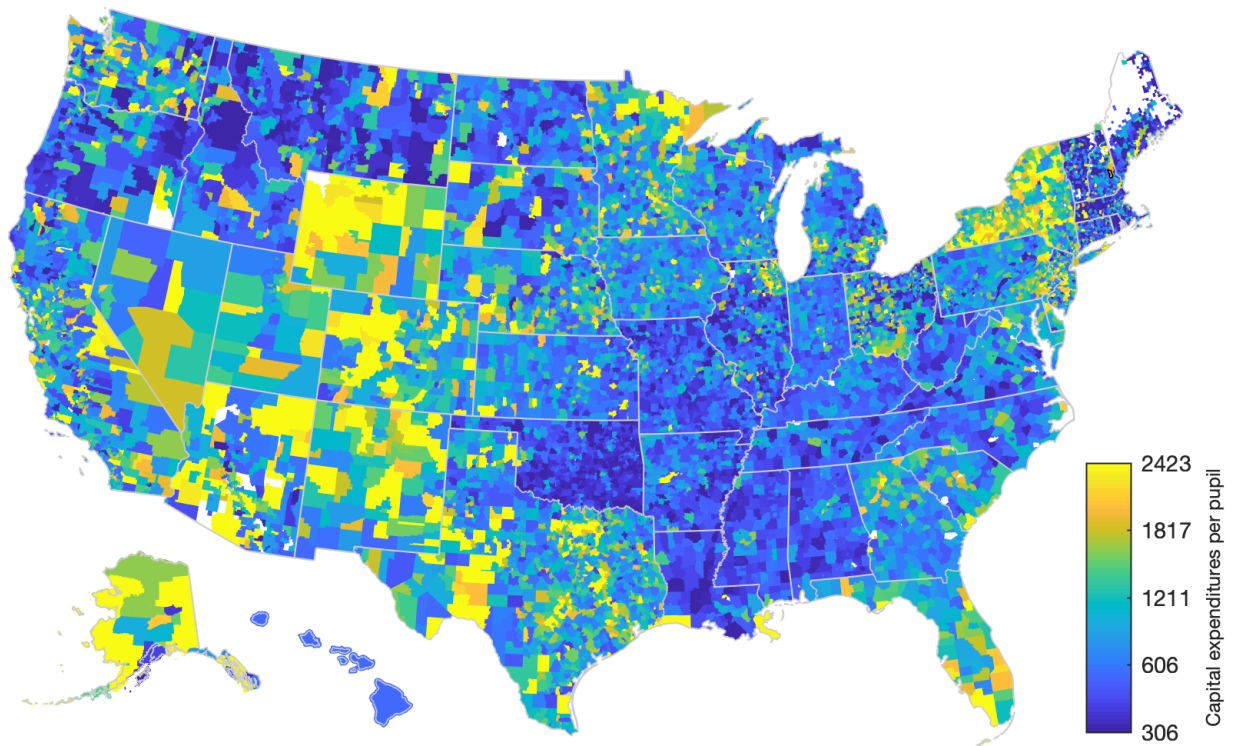
Note: Spending on school capital projects, per pupil, 2015-16. Source: National Center for Education Statistics (NCES).
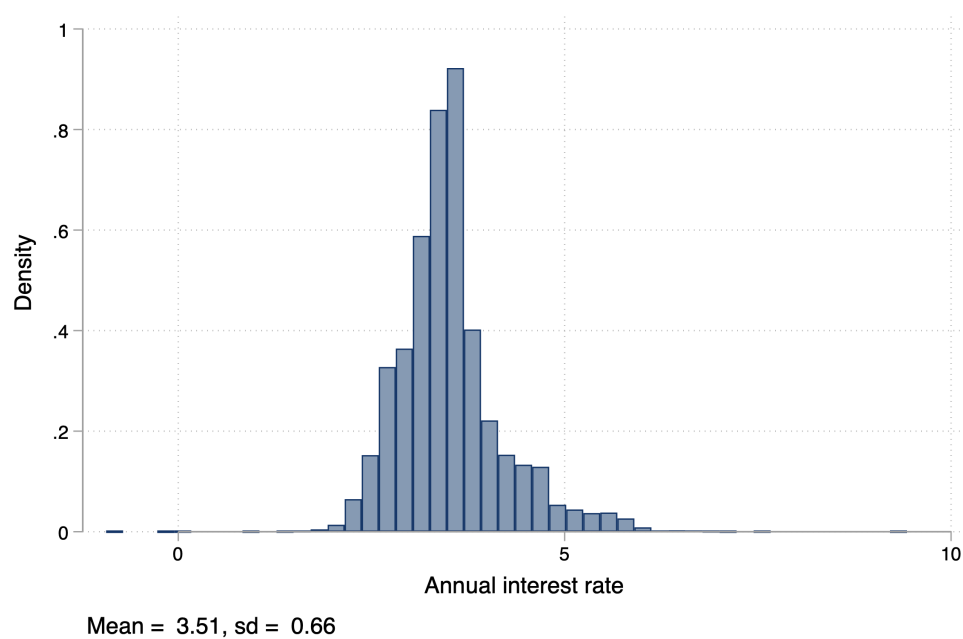
Figure A2: Majority Requirements

Note: Majority requirements refer to the share of favorable votes, among all people who vote, required for a bond measure to pass.

Figure A3: Debt Limits

Note: Debt limits are expressed as a share of total assessed property values.

Figure A4: School District Bonds Interest Rates, 1997-2017

Mean = 3.51, sd = 0.66

Note: Coupon rates on school district bonds for the years 1997-2017. Rates are shown net of fixed effects for the year of issuance and maturity and for bond type. Data from the Mergent Municipal Bonds Database.
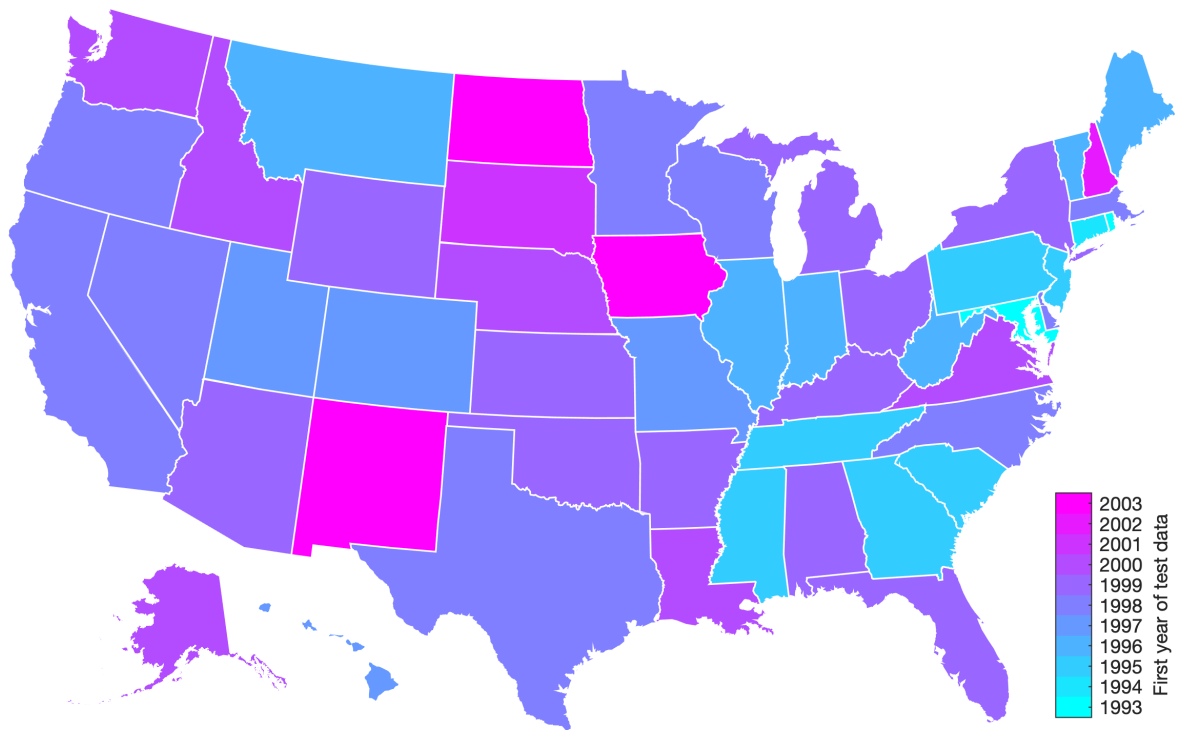
Figure A5: Bond Data Coverage, by Year
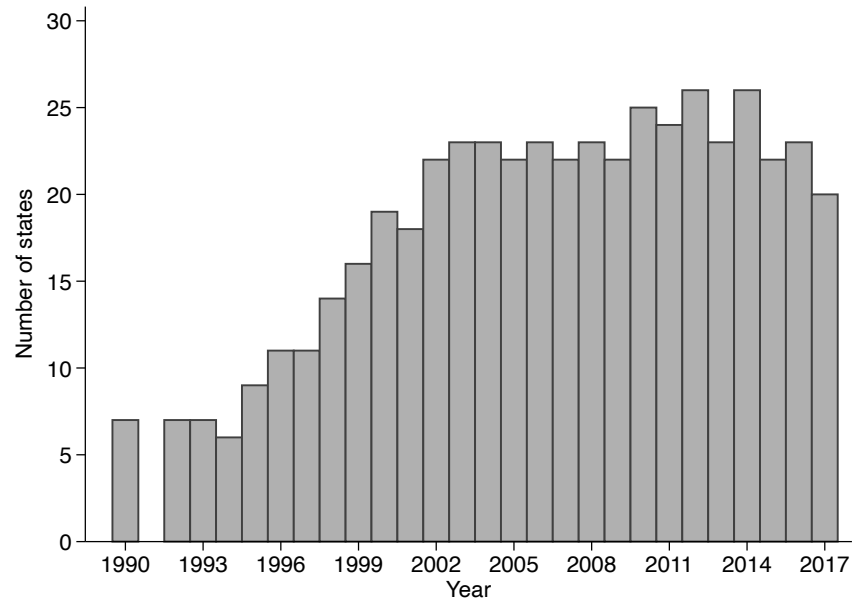
(a) Number of States with Bond Elections in a Year
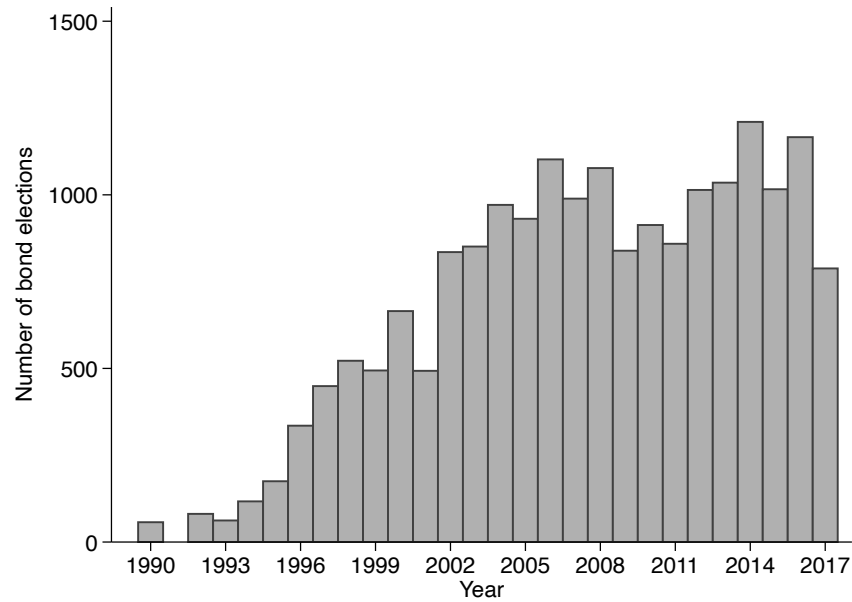

(b) Number of Bond Elections

Note: Panel (a) shows the number of states with bond election information in each year. Panel (b) shows the number of bond elections in our data in each year.

Figure A6: First Year with Test Score Data, by State

Note: First year for which we have test score data, by state

.

Figure A7: Distribution of Time Elapsed Between Subsequent Elections, by Outcome of Earlier Election

(a) Any subsequent election

(b) Successful subsequent election
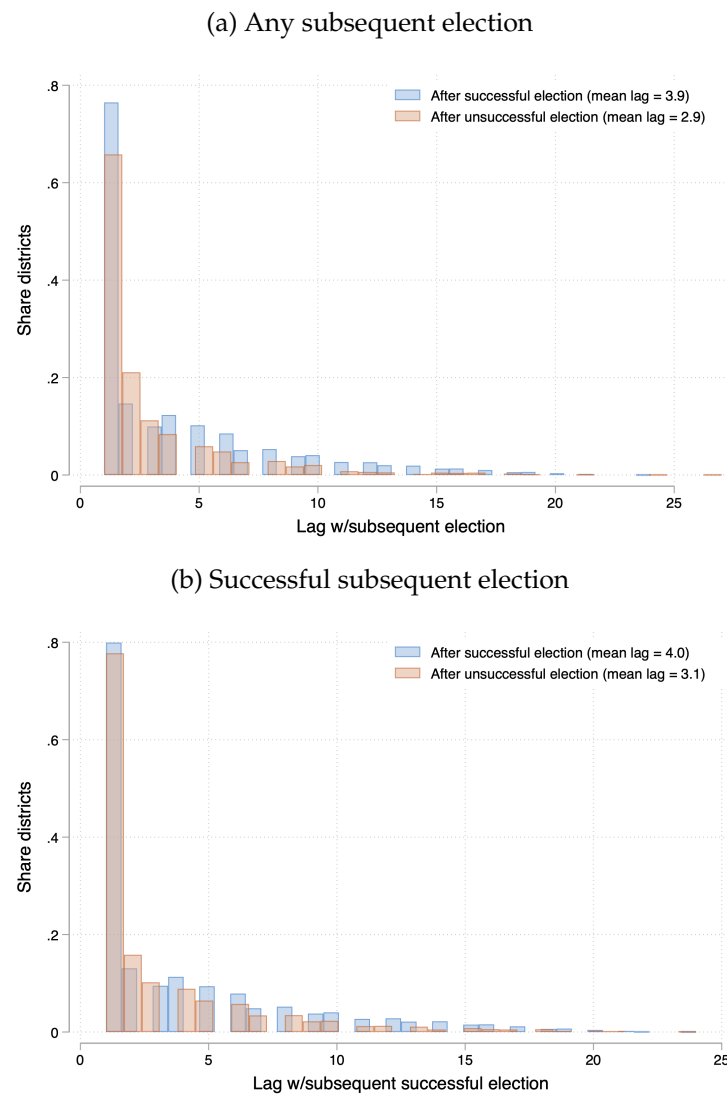
Note: Distribution of time elapsed between any two subsequent district elections, by outcome of the first election (successful or unsuccessful). Panel (a) shows the distribution all districts and elections; panel (b) focuses on successful subsequent elections.

# Figure A8: Density of Vote Margin, by State
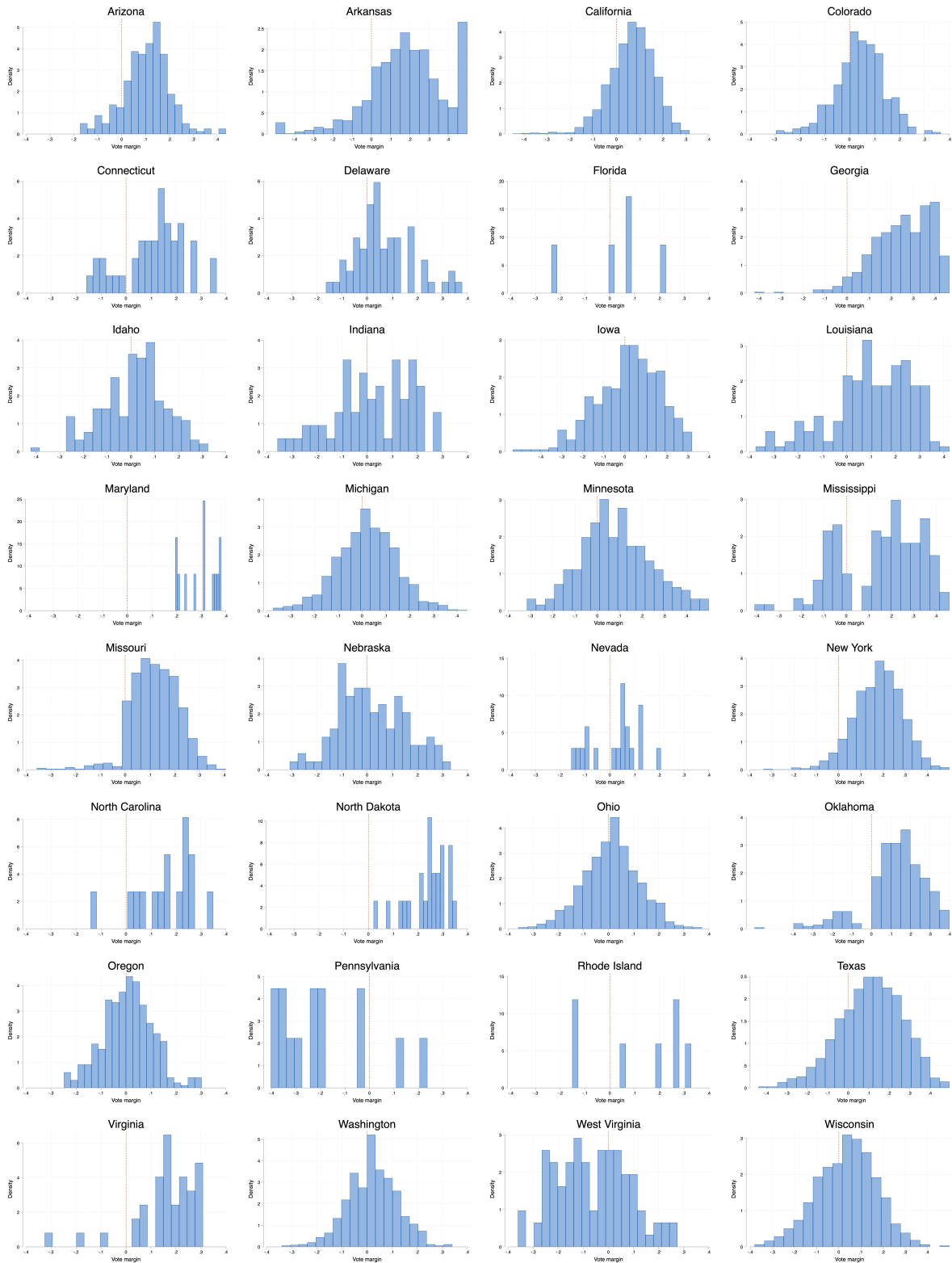
Note: Histogram of vote margins by state. The vote margin is defined as the difference between the share of votes in favor of the proposed measure and the required majority in the state.

## Figure A9: Covariate Balance Around the Vote Margin Cutoff. Main Data
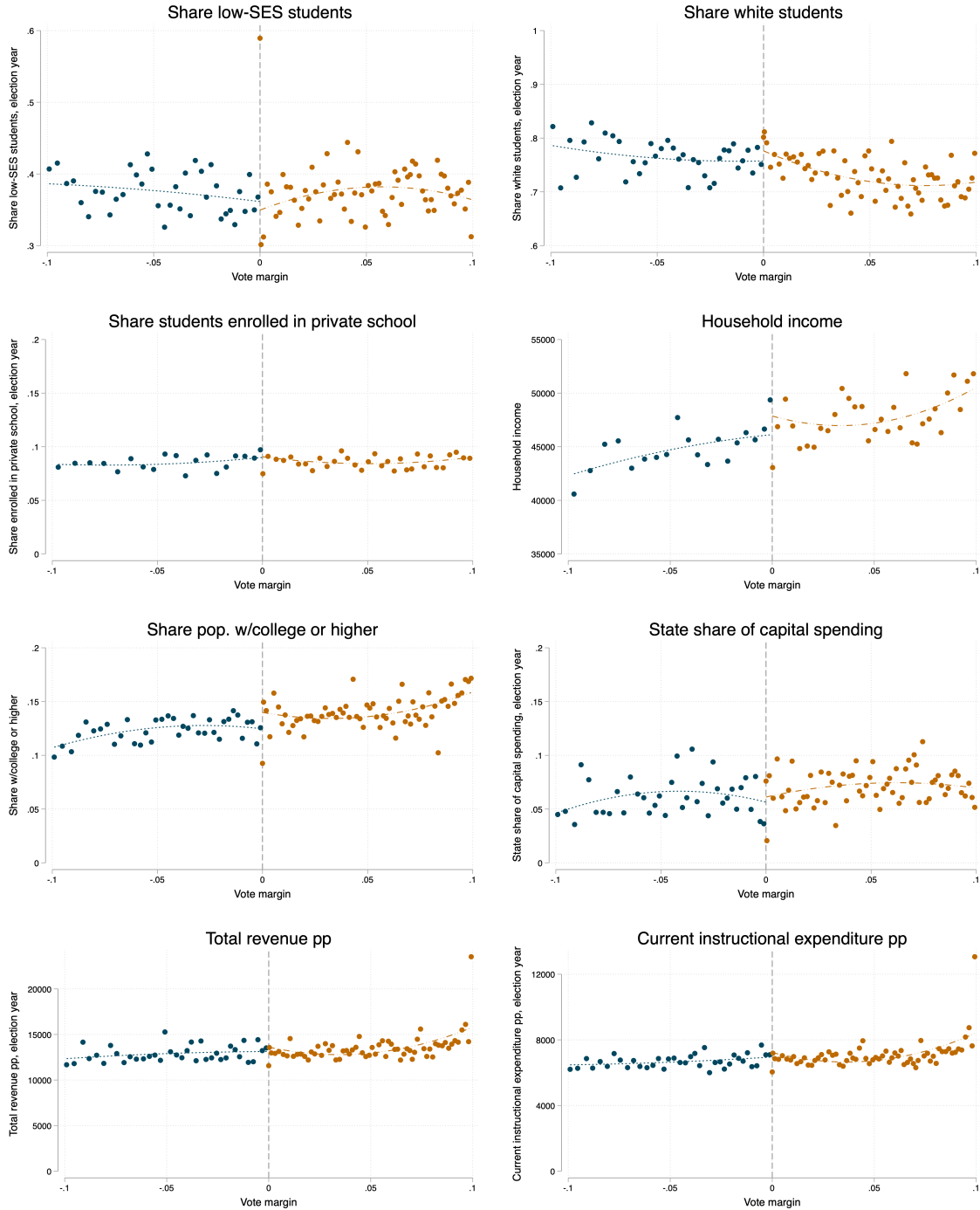
Note: Binned scatterplots of district-level covariates around the vote margin cutoff, obtained using the main data set. Positive vote margins denote successful elections. Each dot is a quantile of vote margin; the vertical axis displays the mean of each covariate in the corresponding quantile. The lines represent fitted quadratic polynomials on either side of the threshold. All variables are measured in the year of the election except for household income and the population share of people with at least a college degree, which are from the U.S. Census of Population and Housing (years 1990 and 2000) and the American Community Survey (years 2007-2012).

# Figure A10: Covariate Balance Around the Vote Margin Cutoff. Stacked Data
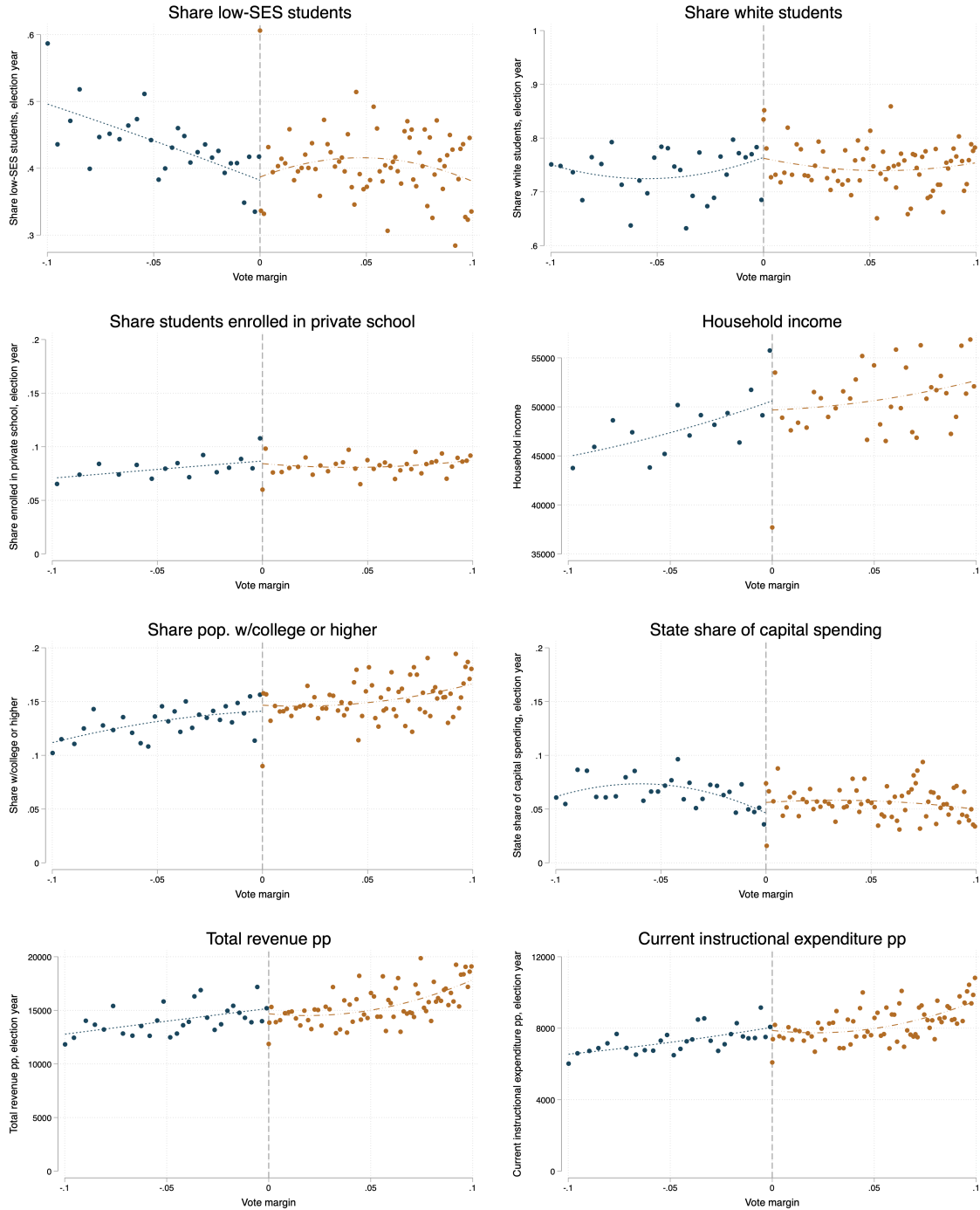
Note: Binned scatterplots of district-level covariates around the vote margin cutoff, obtained using the stacked data set used in estimation. Positive vote margins denote successful elections. Each dot is a quantile of vote margin; the vertical axis displays the mean of each covariate in the corresponding quantile. The lines represent fitted quadratic polynomials on either side of the threshold. All variables are measured in the year of the election except for household income and the population share of people with at least a college degree, which are from the U.S. Census of Population and Housing (years 1990 and 2000) and the American Community Survey (years 2007-2012).

## Figure A11: Mean Effects of Bond Authorization on Current Spending
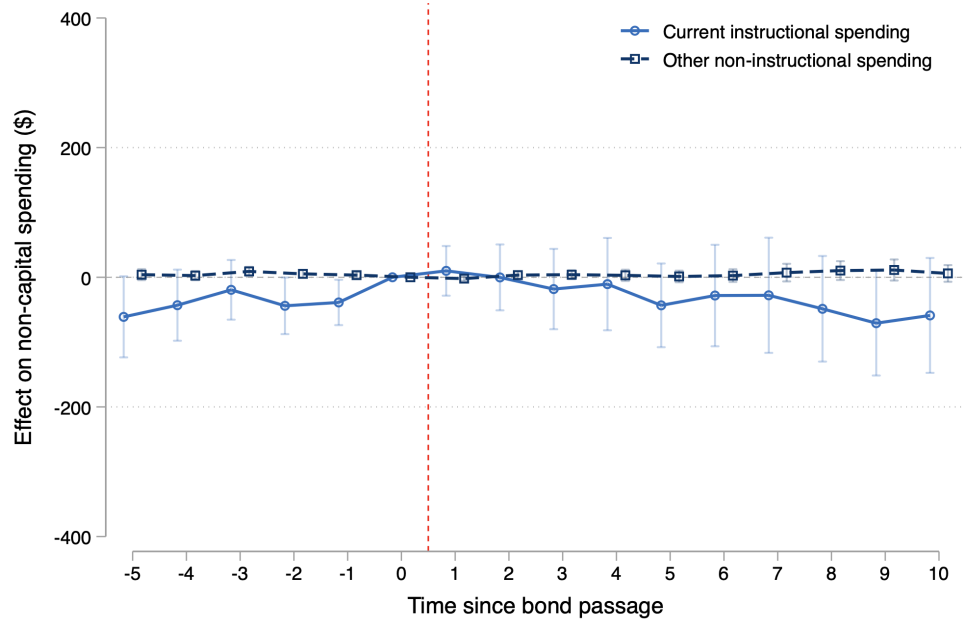
Note: Estimates and confidence intervals of the parameters β_k in equation (3), obtained using current instructional spending and other (non-instructional) spending per pupil as the dependent variables. Estimates are obtained using district-by-cohort and cohort-by-state-by-year effects; observations and weighted by district enrollment. Standard errors are clustered at the district level.

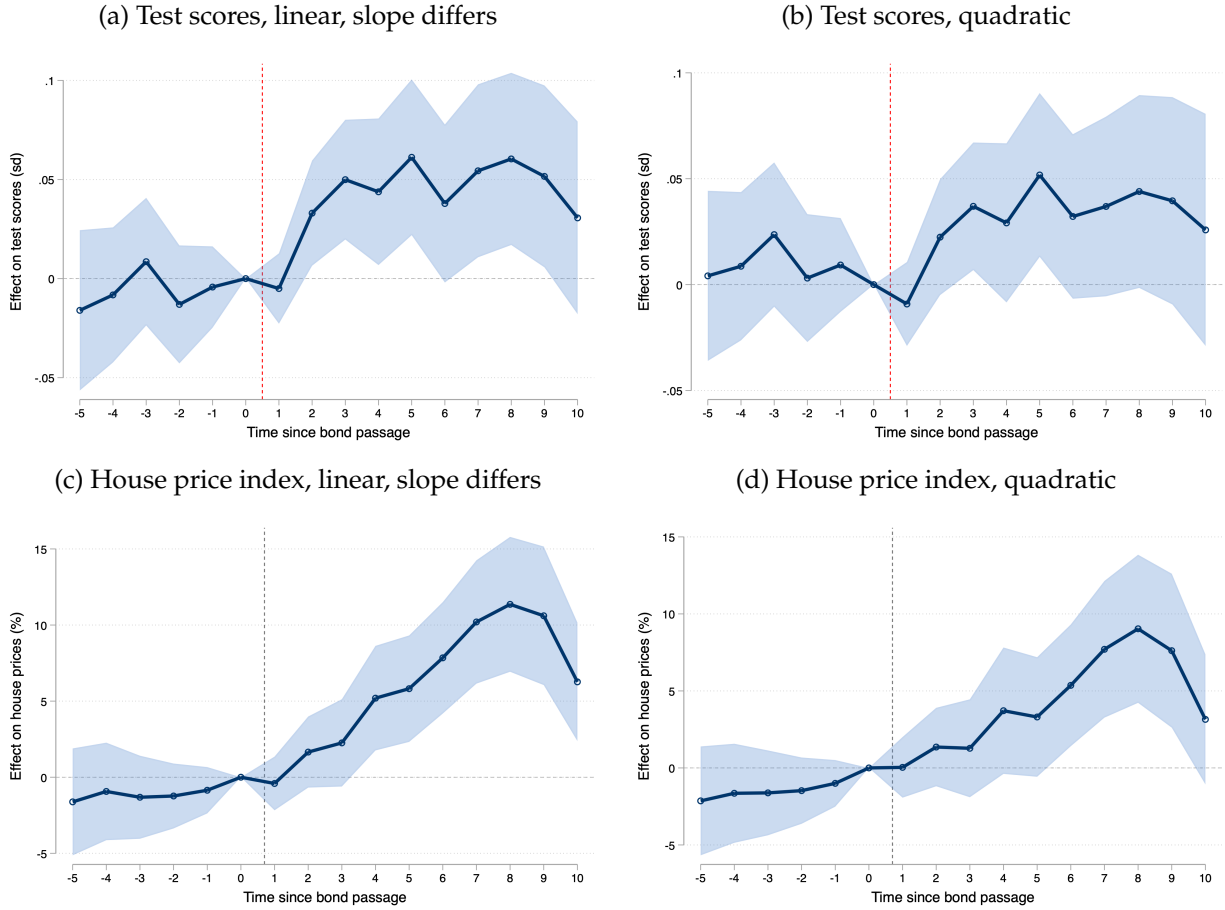## Figure A12: Average Effects of Bond Authorization on Test Scores, by Subject
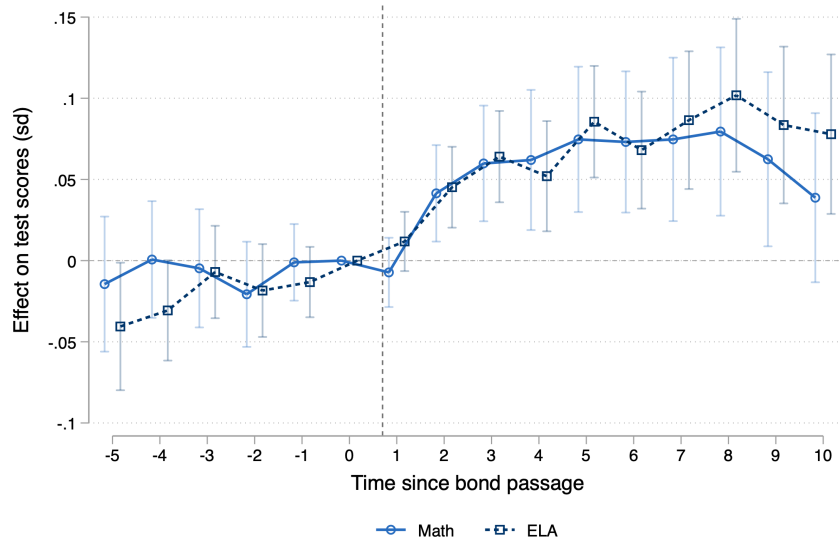
Note: Estimates and confidence intervals of the parameters β_k in equation (3), obtained using test scores (panel a) and house price index (panel b) as the dependent variable. Estimates are shown separately by subject; they are obtained pooling data across grades, controlling for district-by-cohort and cohort-by-state-by-year-by-grade effects, and weighing observations by the number of test takers. Standard errors are clustered at the district level.

**Figure A13: Average Effects of Bond Authorization on Student Body Composition**

(a) Shares of high-SES and White students

(b) Test scores

(c) House prices
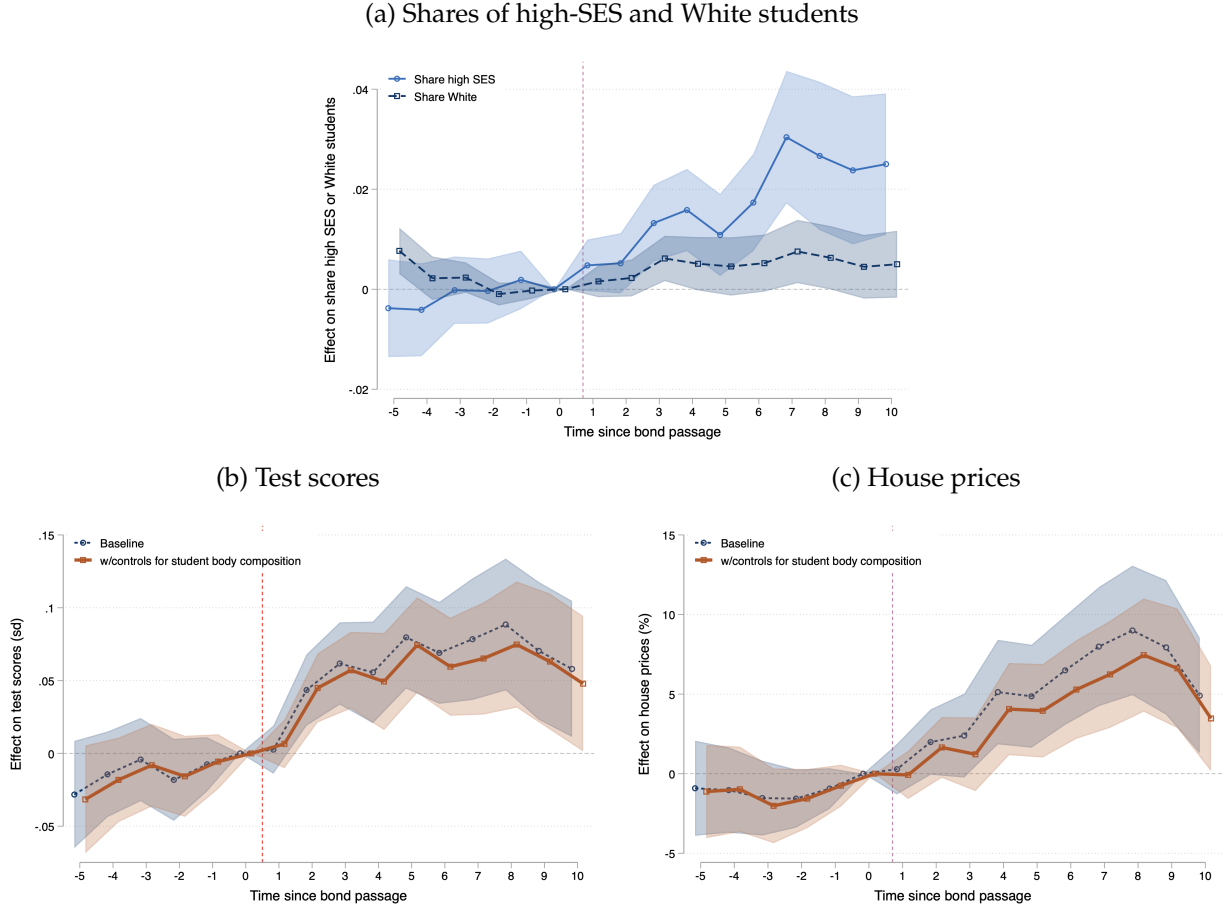
Notes: Panel (a) shows estimates and confidence intervals of the parameters β_k in equation (3), obtained using the shares of high-SES (solid line) and White students (dashed line) as the dependent variables. Panel (b) shows estimates and confidence intervals of β_k on test scores (as in panel (a) of Figure 3), obtained controlling for the share of low-SES and minority students in each district and year. Panel (c) shows estimates and confidence intervals of β_k on house prices (as in panel (b) of Figure 3), obtained controlling for the share of low-SES and minority students in each district and year. In panels (a) and (c), estimates are obtained using district-by-cohort and cohort-by-state-by-year effects and observations are weighted by district enrollment. In panel (b), estimates are obtained pooling data on multiple grades and years and using district-by-cohort and cohort-by-state-by-year-by-subject-by-grade effects, and observations and weighted by the number of test takers. Standard errors are clustered at the district level.

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Figure A14: Average Effects of Bond Authorization on Test Scores and House Prices. Using Different Polynomials of The Vote Share

(a) Test scores, linear, slope differs

(b) Test scores, quadratic

(c) House price index, linear, slope differs

(d) House price index, quadratic

Notes: Estimates and confidence intervals of the parameters β_k in equation (3), obtained using test scores (panel a) and house price index (panel b) as the dependent variable. In panels (a) and (c), we control for a linear polynomial of the vote share variable allowing for the slope to differ on either side of the threshold. In panels (b) and (d), we control for a quadratic polynomial of the vote share 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 A15: Average Effects of Bond Authorization on Capital Spending. Using Cellini, Ferreira, and Rothstein's (2010) Treatment-on-the-Treated Estimator
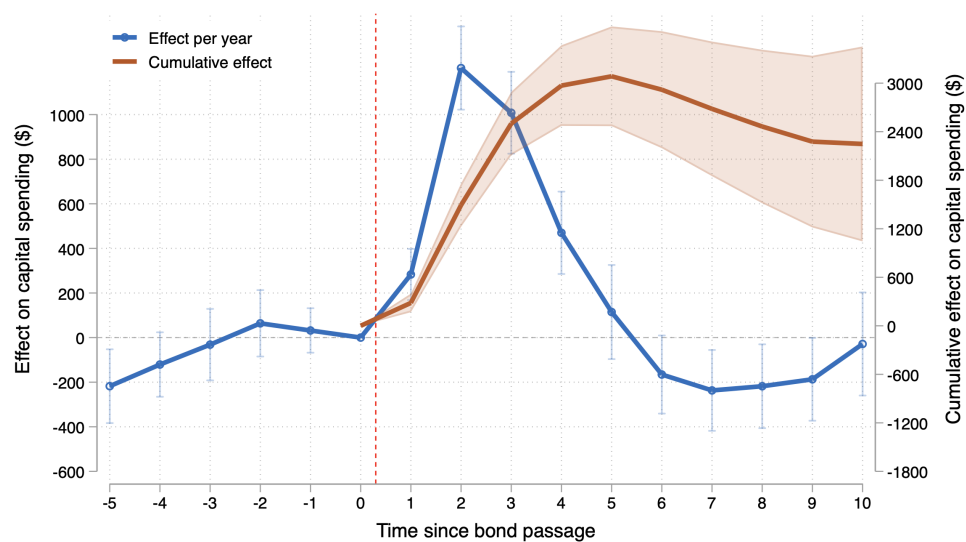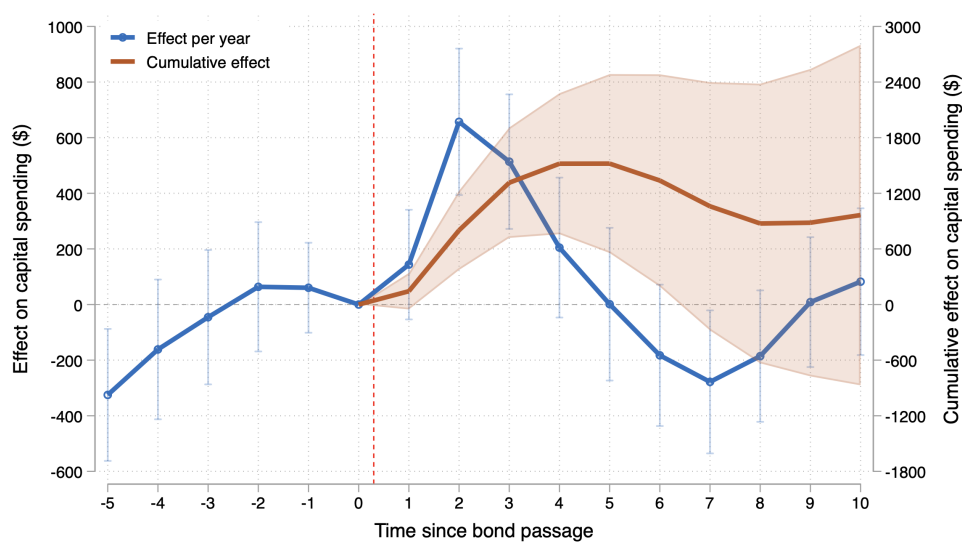

Notes: The blue line with circle markers shows estimates and confidence intervals of the parameters β_k in equation (2), obtained using capital spending per pupil as the dependent variable. The orange continuous line shows cumulative effects, calculated as the running sum of coefficients since time 0. Estimates are obtained using district and state-by-year effects; observations and weighted by by district enrollment. Standard errors are clustered at the district level.

Figure A16: Average Effects of Bond Authorization on Test Scores and House Prices. Using Cellini, Ferreira, and Rothstein's (2010) Treatment-on-the-Treated Estimator
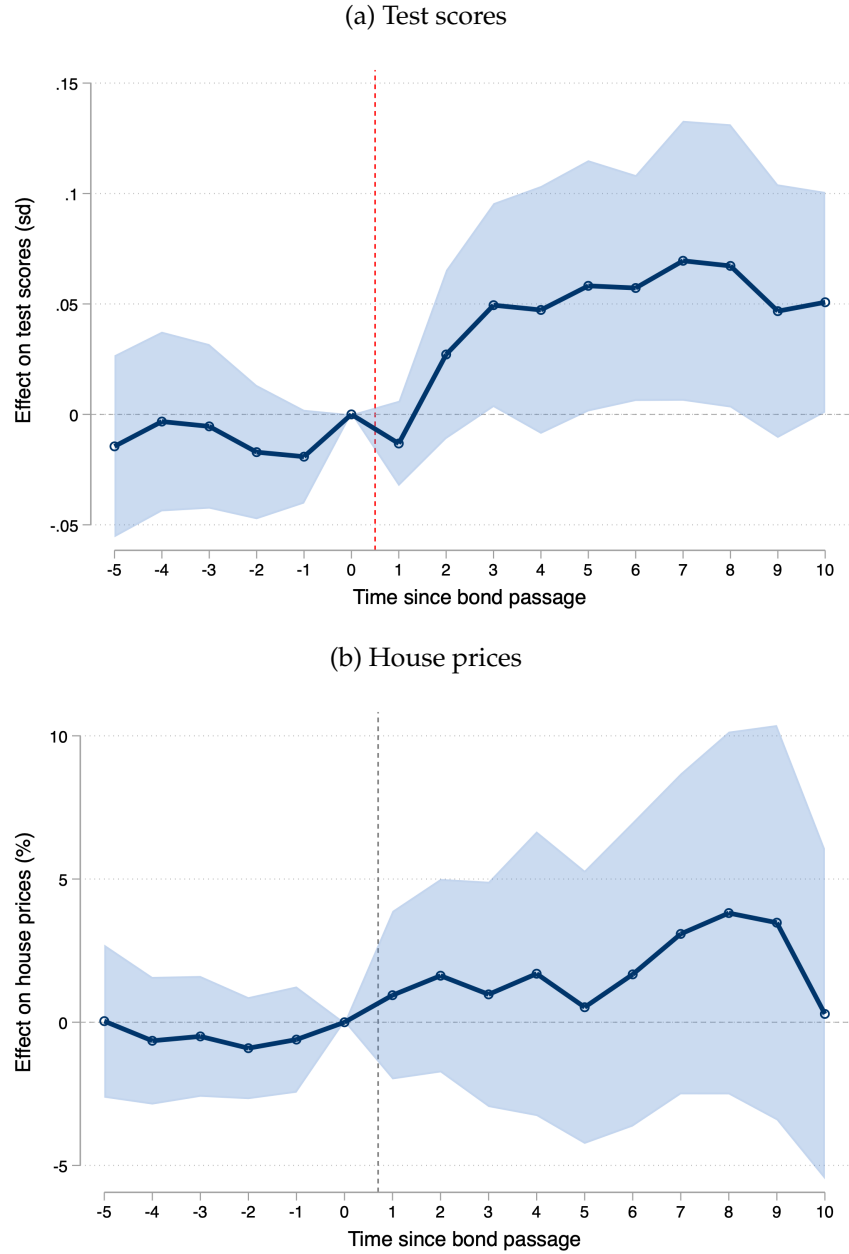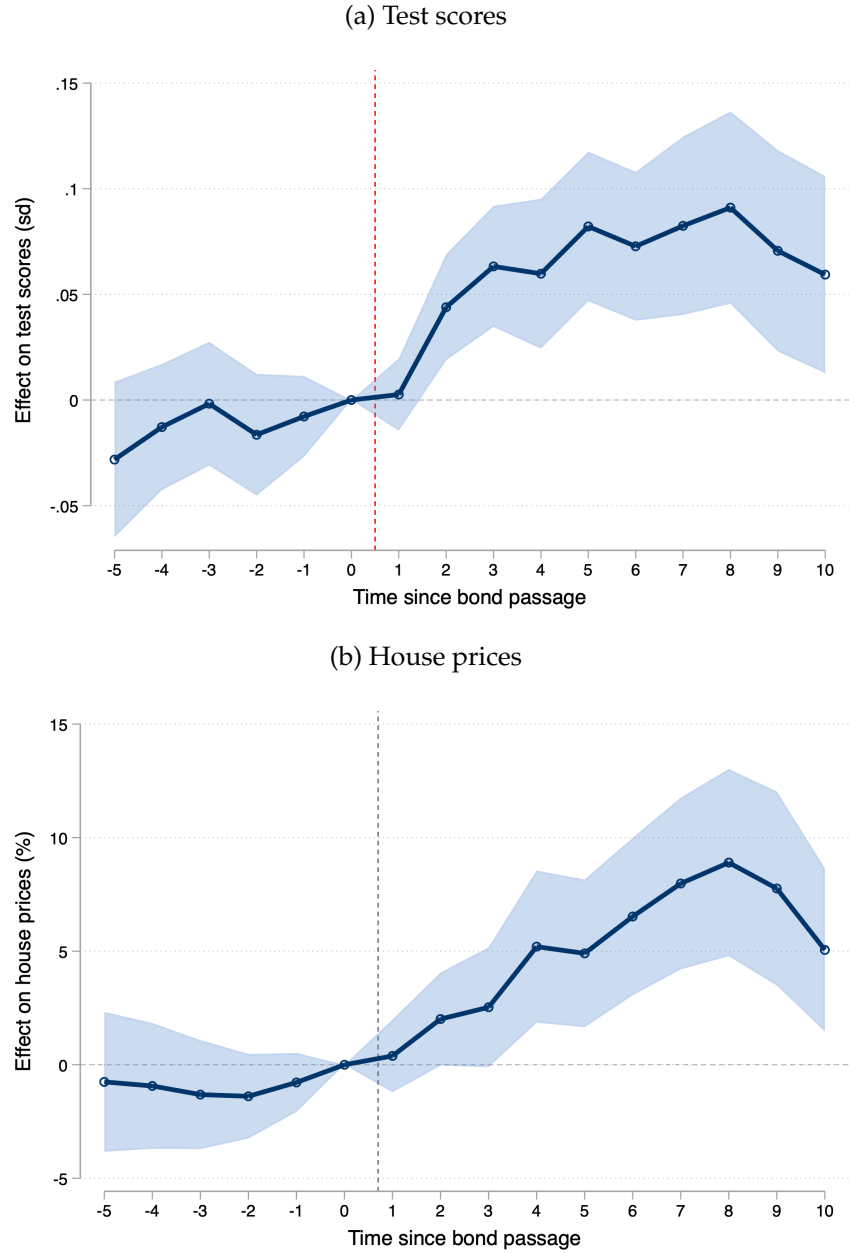
(a) Test scores

(b) House prices

Notes: Estimates and confidence intervals of the parameters β_k in equation (2), obtained using test scores (panel a) and house price index (panel b) as the dependent variables. Test score estimates are obtained pooling data across subjects and grades, controlling for district and state-by-year-by-subject-by-grade effects, and weighing observations by the number of test takers. House price estimates are obtained using district and state-by-year effects, weighing observations by district enrollment. Standard errors are clustered at the district level.

Figure A17: Average Effects of Bond Authorization on Capital Spending. Stacked Approach, Controls Never Authorize a Bond in Time Window of Analysis

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 and using as "clean controls" only districts that never authorize any bonds in the time window of analysis. 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 and weighted by by district enrollment. Standard errors are clustered at the district level.

Figure A18: Average Effects of Bond Authorization on Test Scores and House Prices. Stacked Approach, Controls Never Authorize a Bond in Time Window of Analysis

(a) Test scores

(b) House prices

Notes: Estimates and confidence intervals of the parameters β_k in equation (3), obtained using test scores (panel a) and house price index (panel b) as the dependent variable, and using as "clean controls" only districts that never authorize any bonds in the time window of analysis. 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.

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Figure A19: Average Effects of Bond Authorization on Capital Spending. Stacked Approach, Matching on Pre-Election Bond History
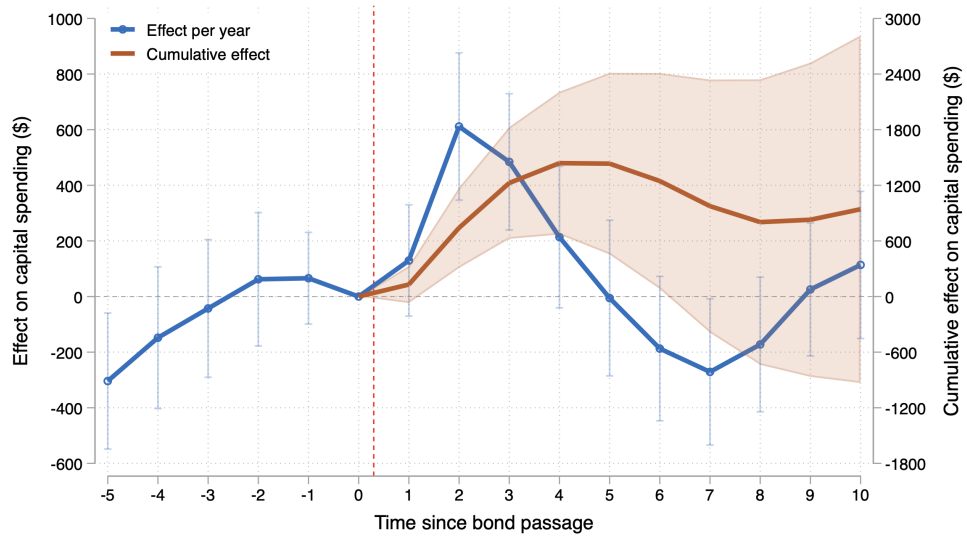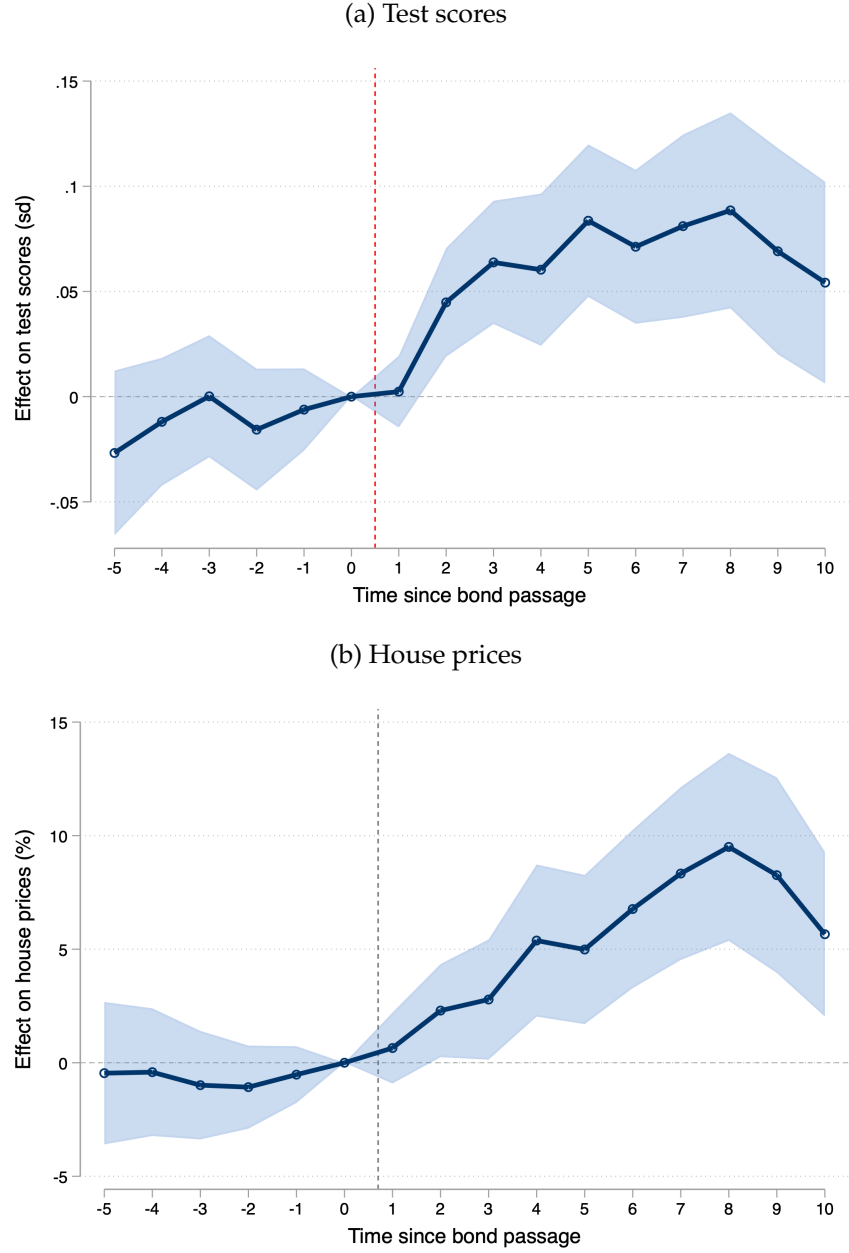

Notes: The blue line with circle markers shows estimates and confidence intervals of the parameters β_k in equation (3), obtained using capital spending per pupil as the dependent variable and using as "clean controls" only districts that share the bond history with at least one treated district in their cohort. 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 and weighted by by district enrollment. Standard errors are clustered at the district level.

Figure A20: Average Effects of Bond Authorization on Test Scores and House Prices. Stacked Approach, Matching on Pre-Election Bond History

(a) Test scores

(b) House prices

Notes: Estimates and confidence intervals of the parameters β_k in equation (3), obtained using test scores (panel a) and house price index (panel b) as the dependent variable, and using as "clean controls" only districts that share the bond history with at least one treated district in their cohort. 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 A21: Average Effects of Bond Authorization on Capital Spending. Stacked Approach, Not Controlling for Future Bond History
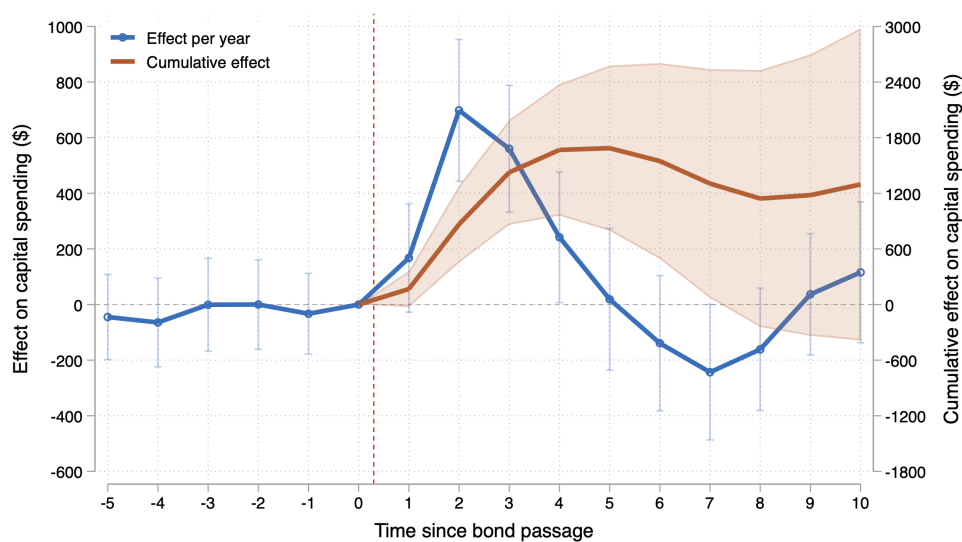

Notes: The blue line with circle markers shows estimates and confidence intervals of the parameters β_k in equation (3), obtained using capital spending per pupil as the dependent variable and not controlling for M_{jct-k} for $k < 0$. The orange continuous line shows cumulative effects, calculated as the running sum of coefficients since time 0. Estimates are obtained using district-by-cohort and cohort-by-state-by-year effects; observations and weighted by by district enrollment. Standard errors are clustered at the district level.

Figure A22: Average Effects of Bond Authorization on Test Scores and House Prices. Stacked Approach, Not Controlling for Future Bond History

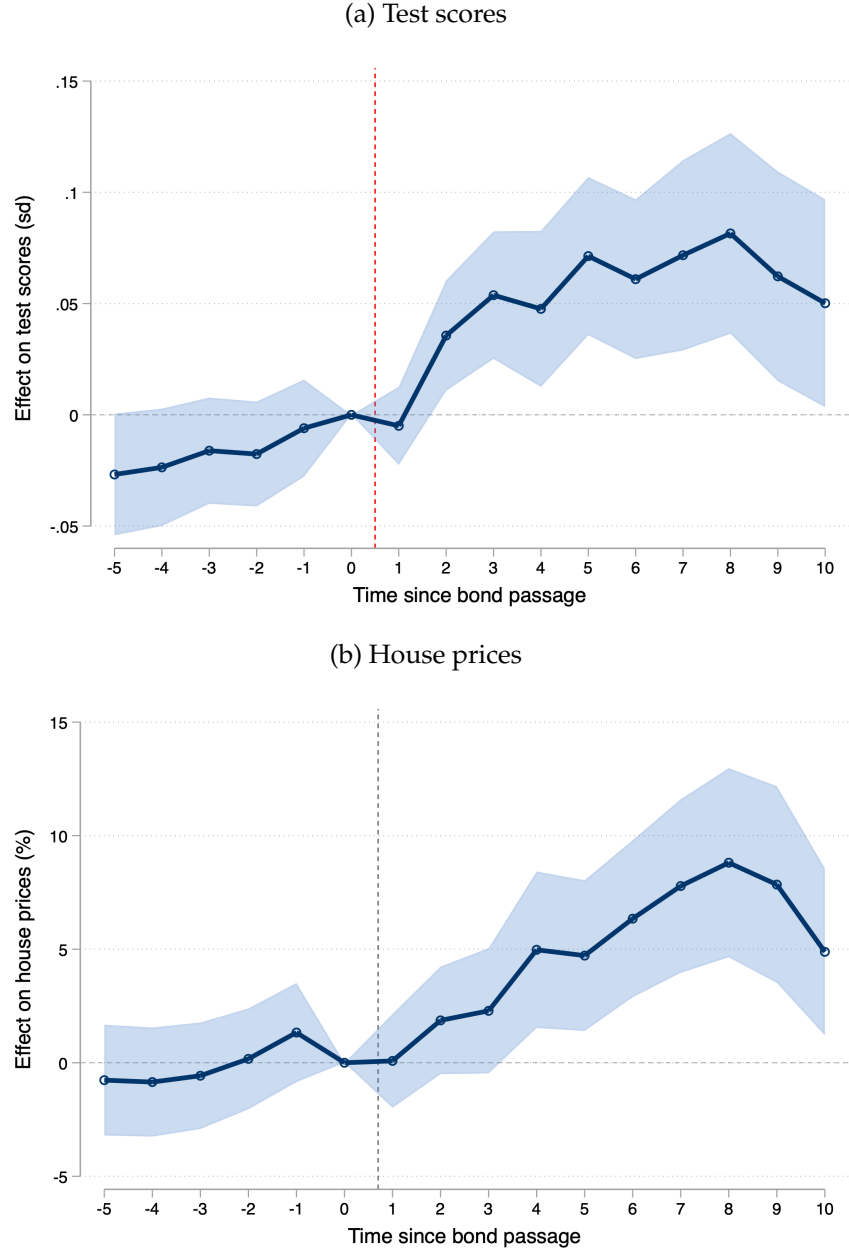(a) Test scores

(b) House prices

Notes: Estimates and confidence intervals of the parameters β_k in equation (3), obtained using test scores (panel a) and house price index (panel b) as the dependent variable and not controlling for M_{jct-k} for $k < 0$. 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 A23: Average Effects of Bond Authorization on Capital Spending. Extended Two-Way-Fixed-Effects Estimator as in Wooldridge (2021)
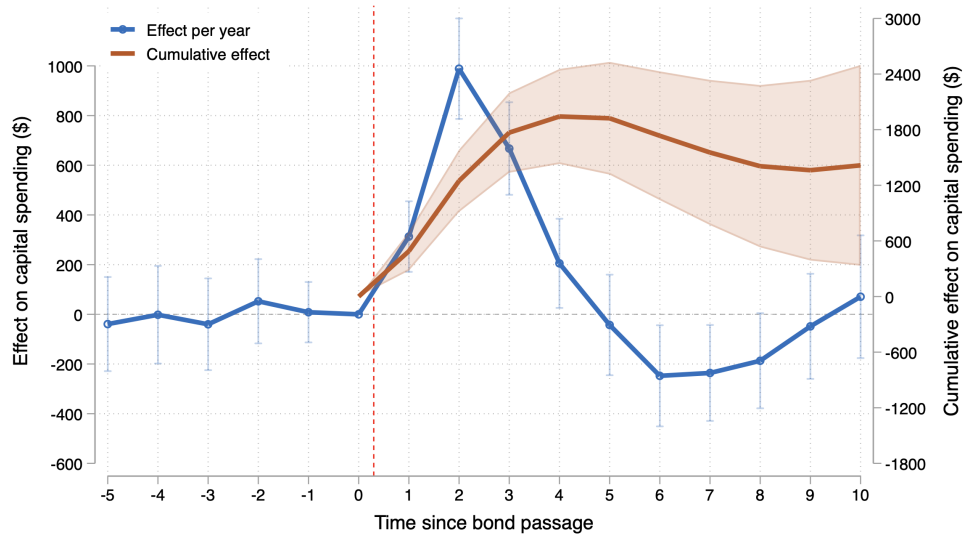

Notes: The blue line with circle markers shows estimates and confidence intervals of the parameters β_k in equation (2), obtained using capital spending per pupil as the dependent variable and allowing for the treatment effect to be heterogeneous across cohorts, as in Wooldridge (2021) (we show averages of treatment effects across cohorts). The orange continuous line shows cumulative effects, calculated as the running sum of coefficients since time 0. Estimates are obtained using district and state-by-year effects; observations and weighted by by district enrollment. Standard errors are clustered at the district level.

Figure A24: Average Effects of Bond Authorization on Test Scores and House Prices. Extended Two-Way-Fixed-Effects Estimator as in Wooldridge (2021)
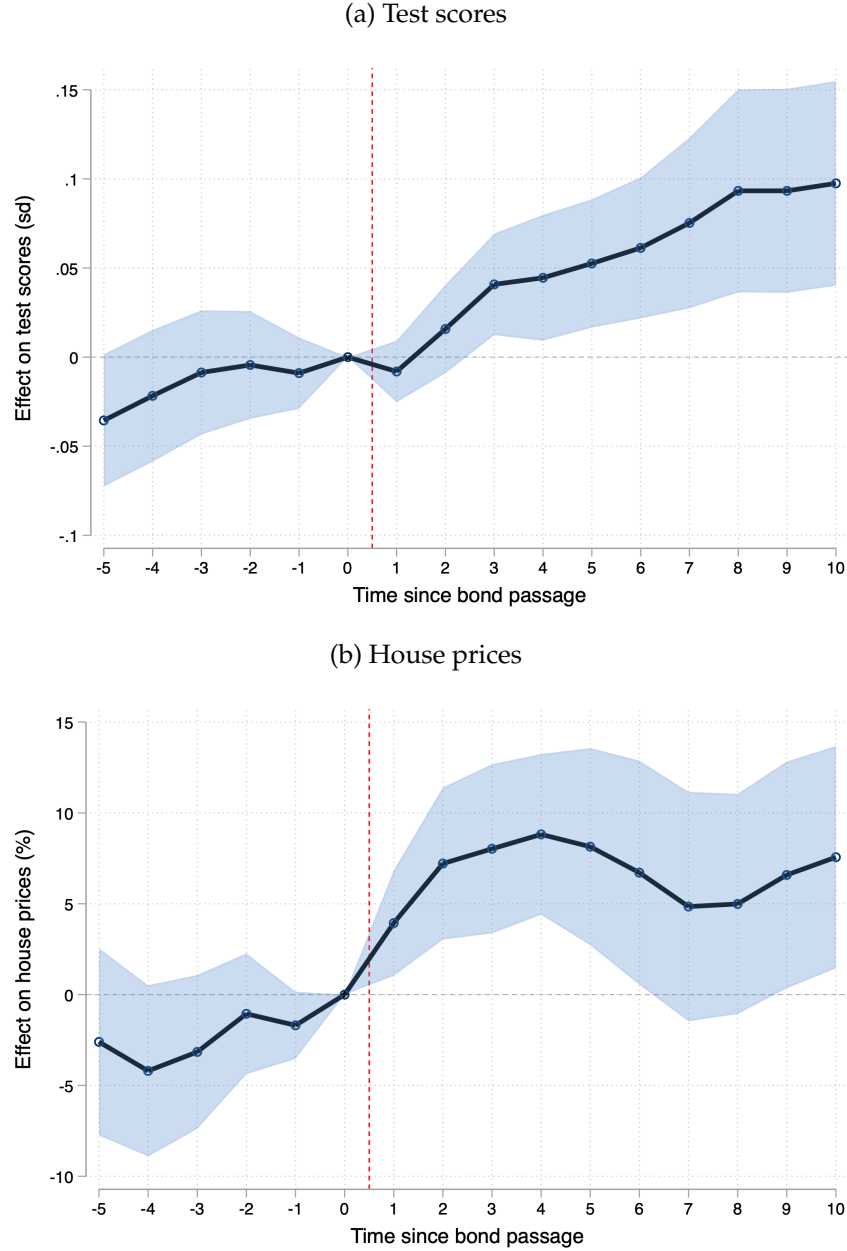
(a) Test scores

(b) House prices

Notes: Estimates and confidence intervals of the parameters β_k in equation (2), obtained using test scores (panel (a)) and the house price index (panel (b)) as the dependent variable and allowing for the treatment effect to be heterogeneous across cohorts, as in Wooldridge (2021) (we show averages of treatment effects across cohorts). We average test scores across grades and subjects within a district-year, using the number of test score takers as weights. All 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 A25: Share of Bonds by Category and Number of Categories

(a) Share of bonds in each category

(b) Share of bonds by number of assigned categories
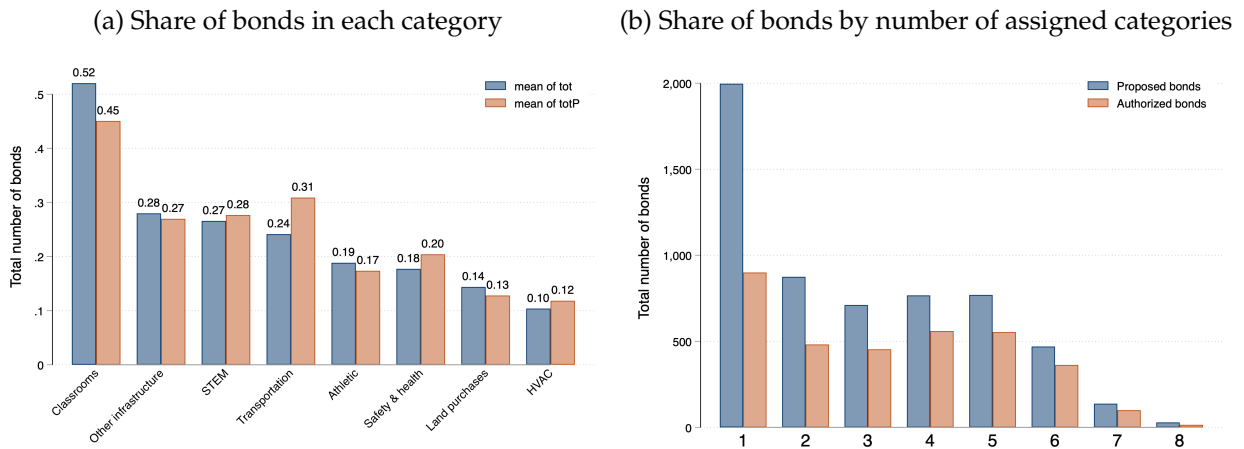
Note: Panel (a) shows the number of bonds assigned to each (non-mutually exclusive) category. Panel (b) shows the number of bonds with each number of assigned categories.

Figure A26: Bundling of Bond Categories: Shares of Bonds by Category that Also Contain Other Categories

(a) Proposed bonds

(b) Authorized bonds
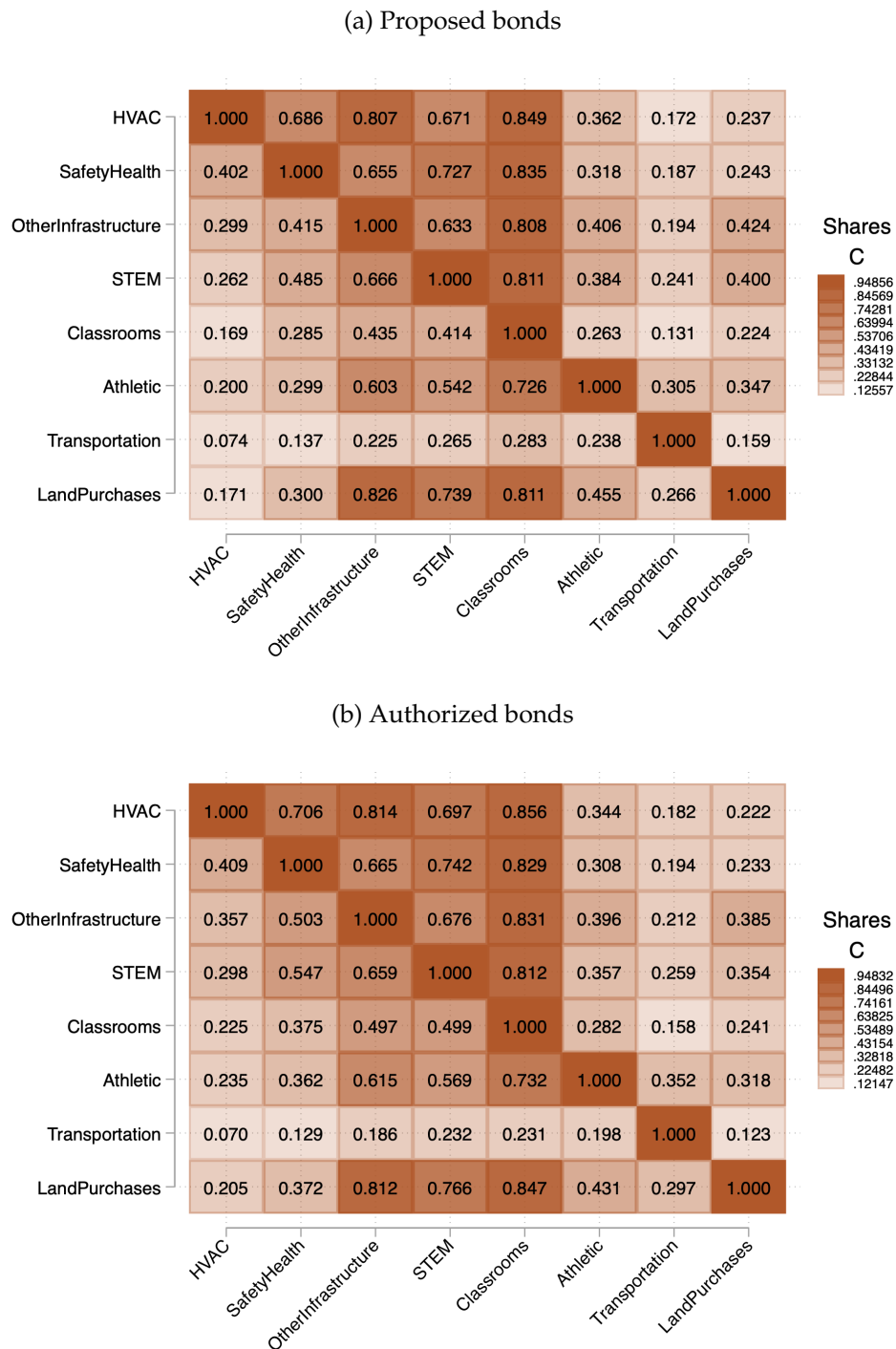
Note: Each number in the matrix corresponds to the share of bonds in the category shown on the horizontal axis, who also belong to the category on the vertical axis. For example, the number 0.237 in the top-right cell of panel (a) indicates that 23.7% of all HVAC bonds also contain land purchases. Panel (a) refers to all proposed bonds; panel (b) refers to authorized bonds.

Figure A27: Effects of Passing a Bond, By Spending Category. Controlling For Other Categories
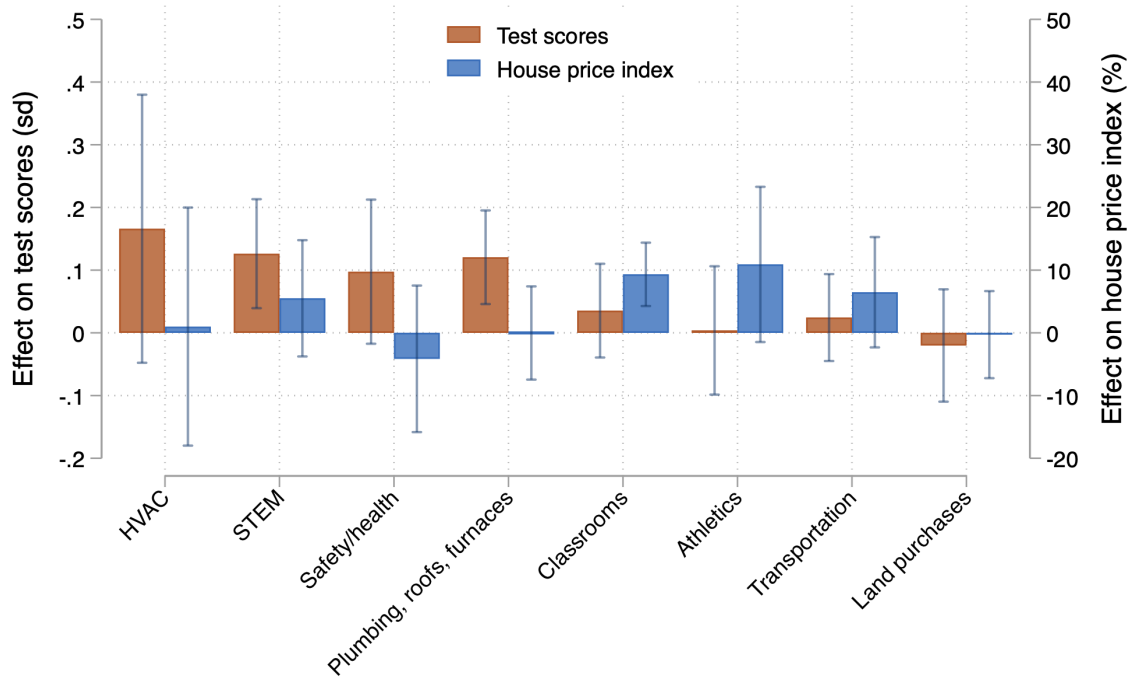
Note: Point estimates and confidence intervals of averages of the parameters $\beta_{k,p}$ in equation (6) for $k \in [3,6]$, shown separately for each spending category p. The orange series is estimated using test scores as the dependent variable, pooled across subjects and grades, using district-by-cohort 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. All specifications also control for indicators for other bond categories, interacted with state-by-year-by-cohort fixed effects. Confidence intervals are calculated using standard errors clustered at the district level.

Figure A28: Effects of Passing a Bond, By Spending Category. Dynamic Effects

(a) Test scores
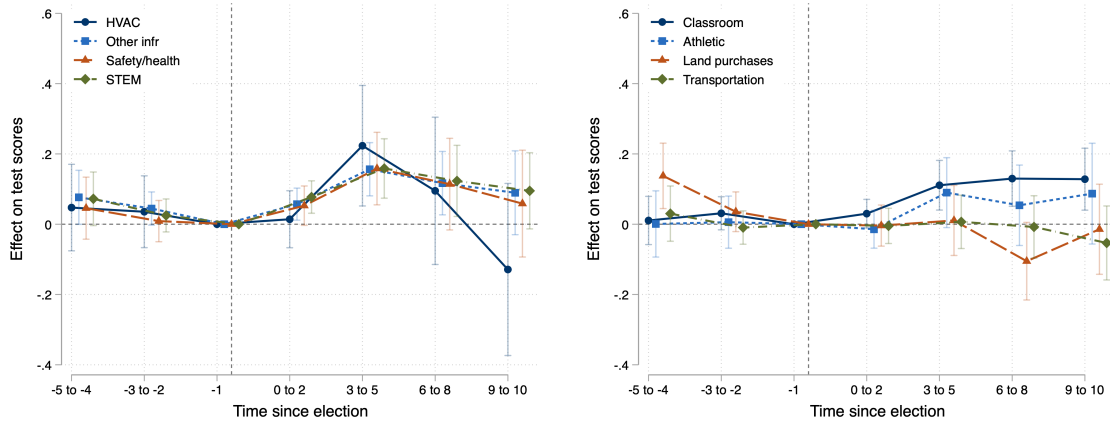
(b) House prices
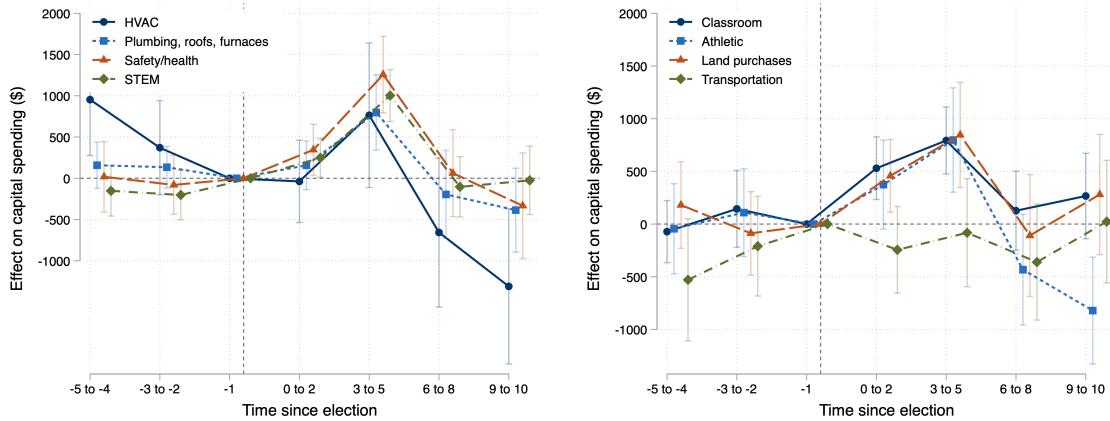
(c) Capital spending per pupil

Note: Point estimates and confidence intervals of averages of the parameters $\beta_{k,p}$ in equation (6) over different time periods, shown separately for each spending cat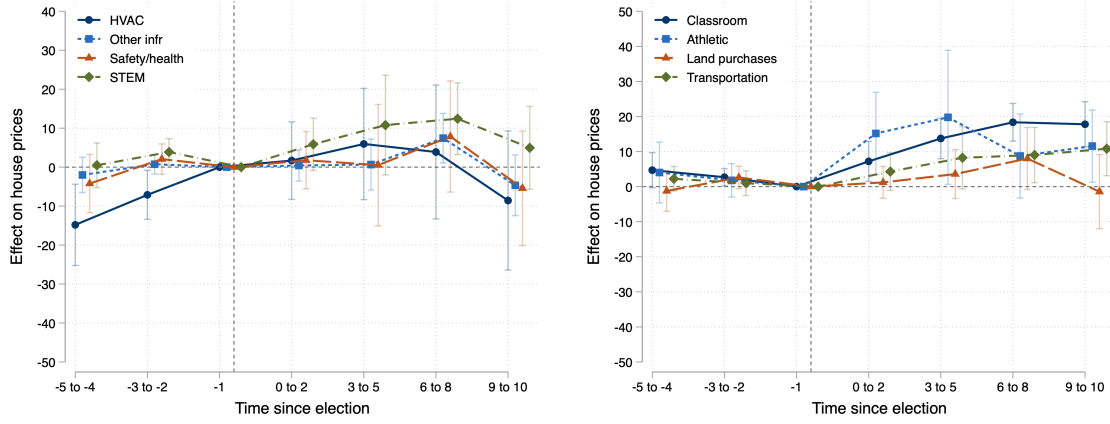egory p. The dependent variables are test scores (panel (a)), the house price index (panel (b)), and capital spending per pupil (panel (c)). Test score effects are obtained pooling data across subjects and grades, using district-by-cohort and state-by-year-by-subject-by-grade-by-cohort effects and weighing observations by the number of test takers. Capital spending and house price effects are 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 A29: Effects of Passing a Bond, By Spending Category. Alternative Estimation Approaches

(a) Stacked DRD, matching on pre-election bond history
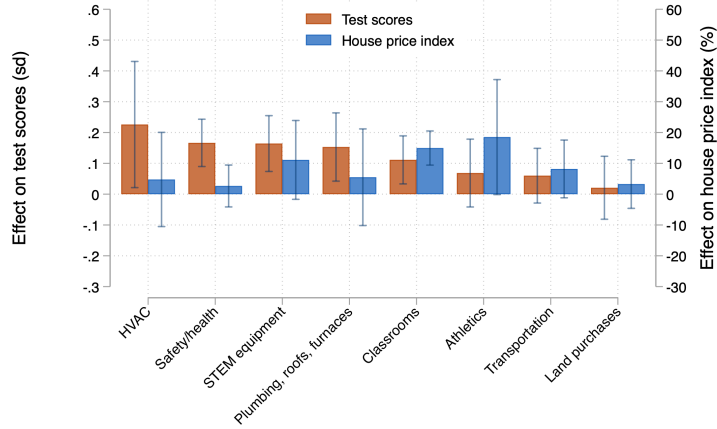
(b) Stacked DRD, controls never authorize bonds in window

(c) Stacked DRD, not controlling for future bond history
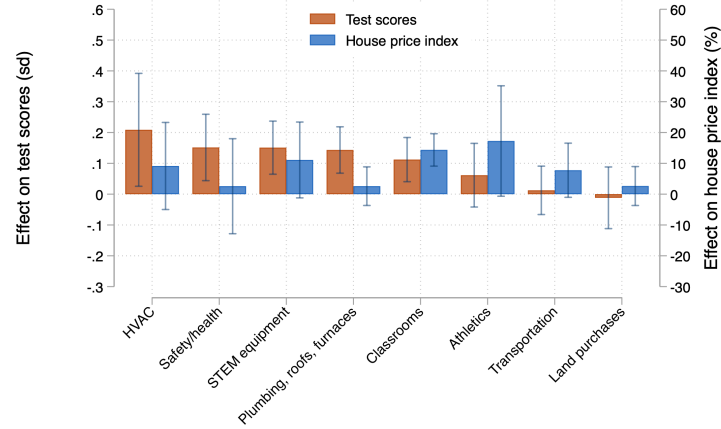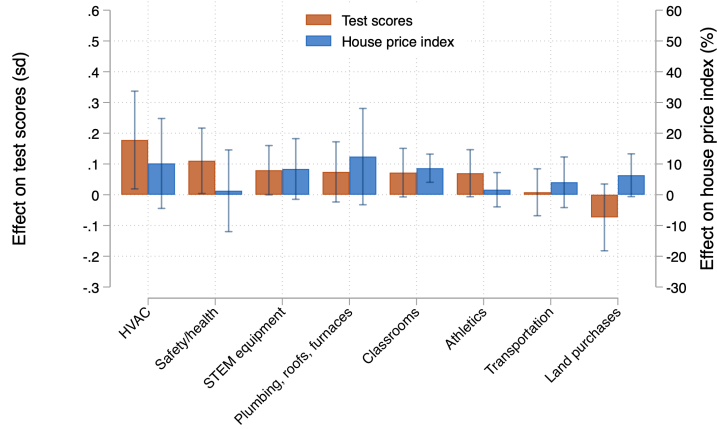
(d) Extended Two-way Fixed Effects (Wooldridge, 2021)
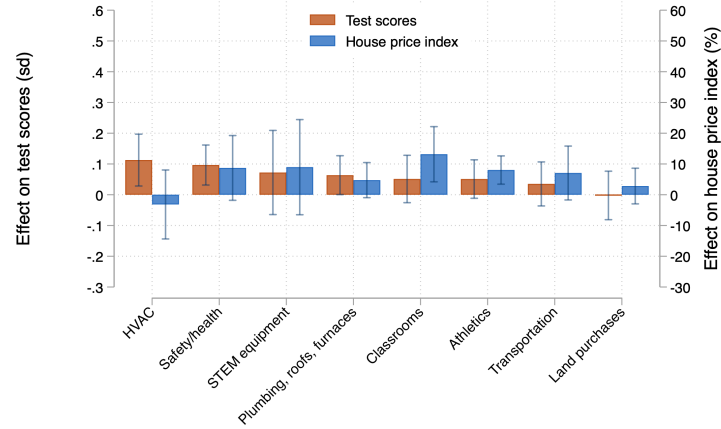
Note: Point estimates and confidence intervals of averages of the parameters $\beta_{k,p}$ in equation (6) for $k \in [3,6]$, shown separately for each spending category p. The orange series is estimated using test scores as the dependent variable, pooled across subjects and grades, using district-by-cohort 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. Estimates are obtained using the different approaches described in Section 4. Confidence intervals are calculated using standard errors clustered at the district level.

Figure A30: Effects of Bond Authorization By Student Demographics. Using Cellini, Ferreira, and Rothstein's (2010) Treatment-on-the-Treated Estimator

(a) Test scores

(b) House prices

(c) Capital spending

Note: Estimates and confidence intervals of the parameters β in equation (2), 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 disadvantaged students ("low-SES" denotes the top tercile and "high-SES" 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 pooling data across subjects and grades, using district-and and state-by-year-by-subject-by-grade effects and weighing observations by the number of test takers. Other estimates are obtained using state-by-year effects and weighing observations by district enrollment. Standard errors are clustered at the district level.

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Figure A31: Effects of Bond Authorization By Student Demographics. Stacked Approach, Controls Never Authorize a Bond in Time Window of Analysis

(a) Test scores

(b) House prices

(c) Capital spending

Note: Estimates and confidence intervals of the parameters β 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, and using as "clean controls" only districts that never authorize any bonds in the time window of analysis. Figures in the left panels show estimates by tercile of the share of disadvantaged students ("low-SES" denotes the top tercile and "high-SES" 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 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.

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Figure A32: Effects of Bond Authorization By Student Demographics. Stacked Approach, Matching on Pre-Election Bond History
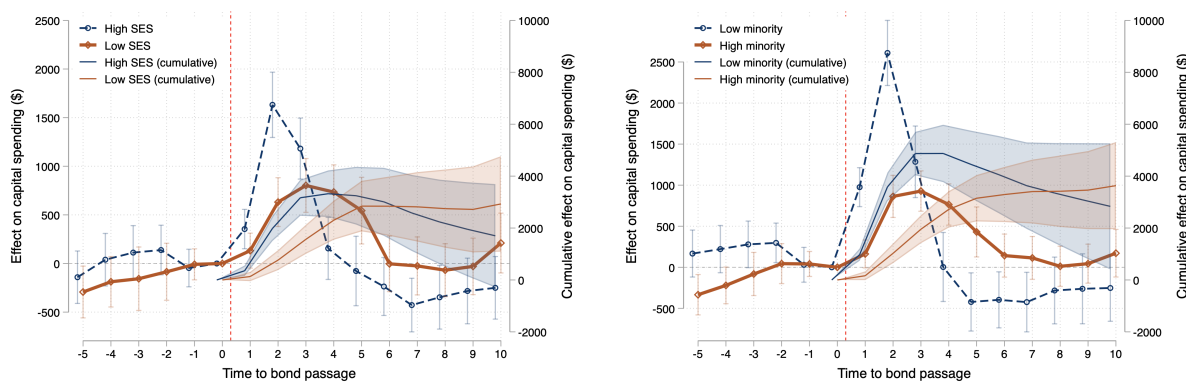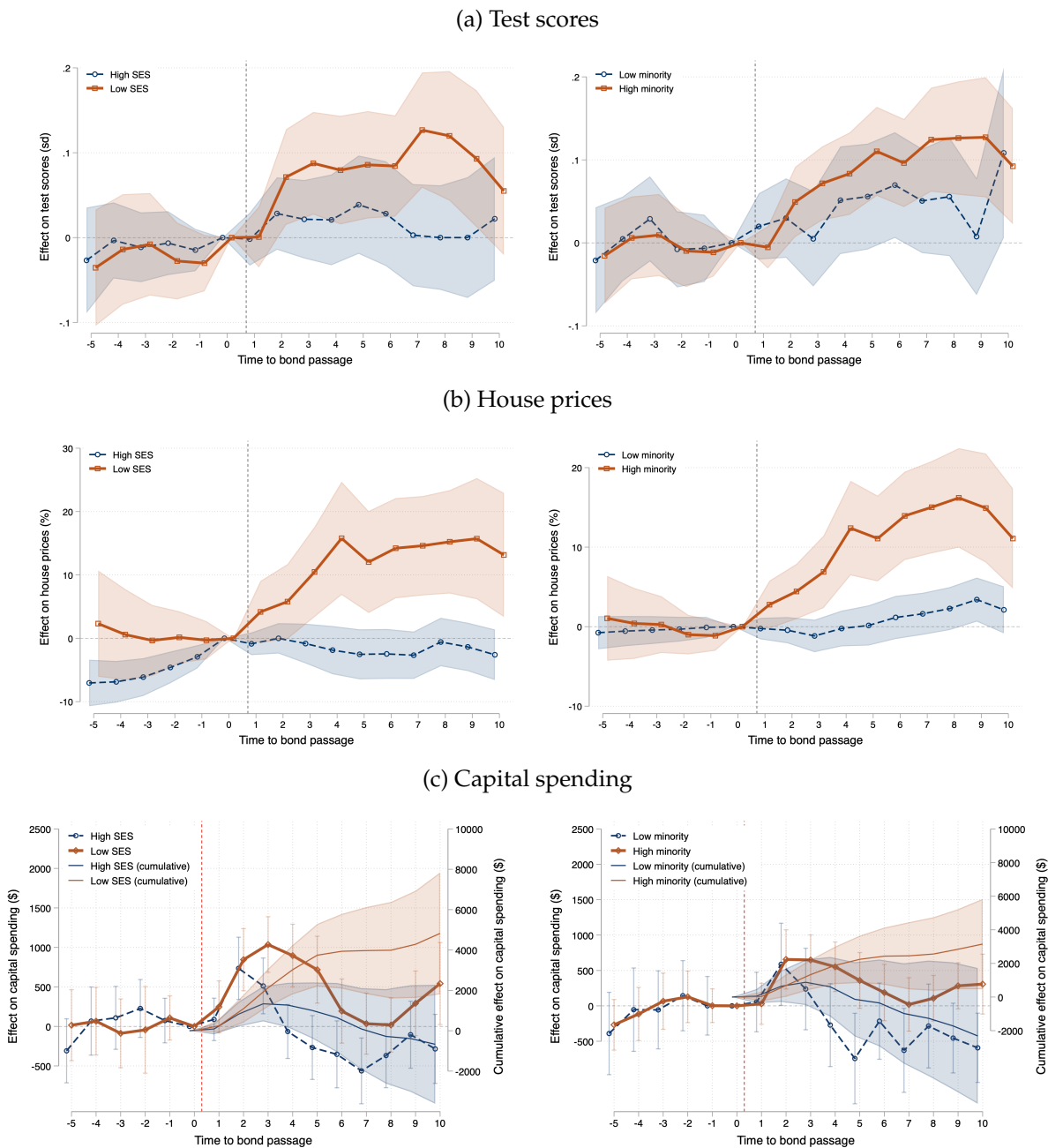
(a) Test scores

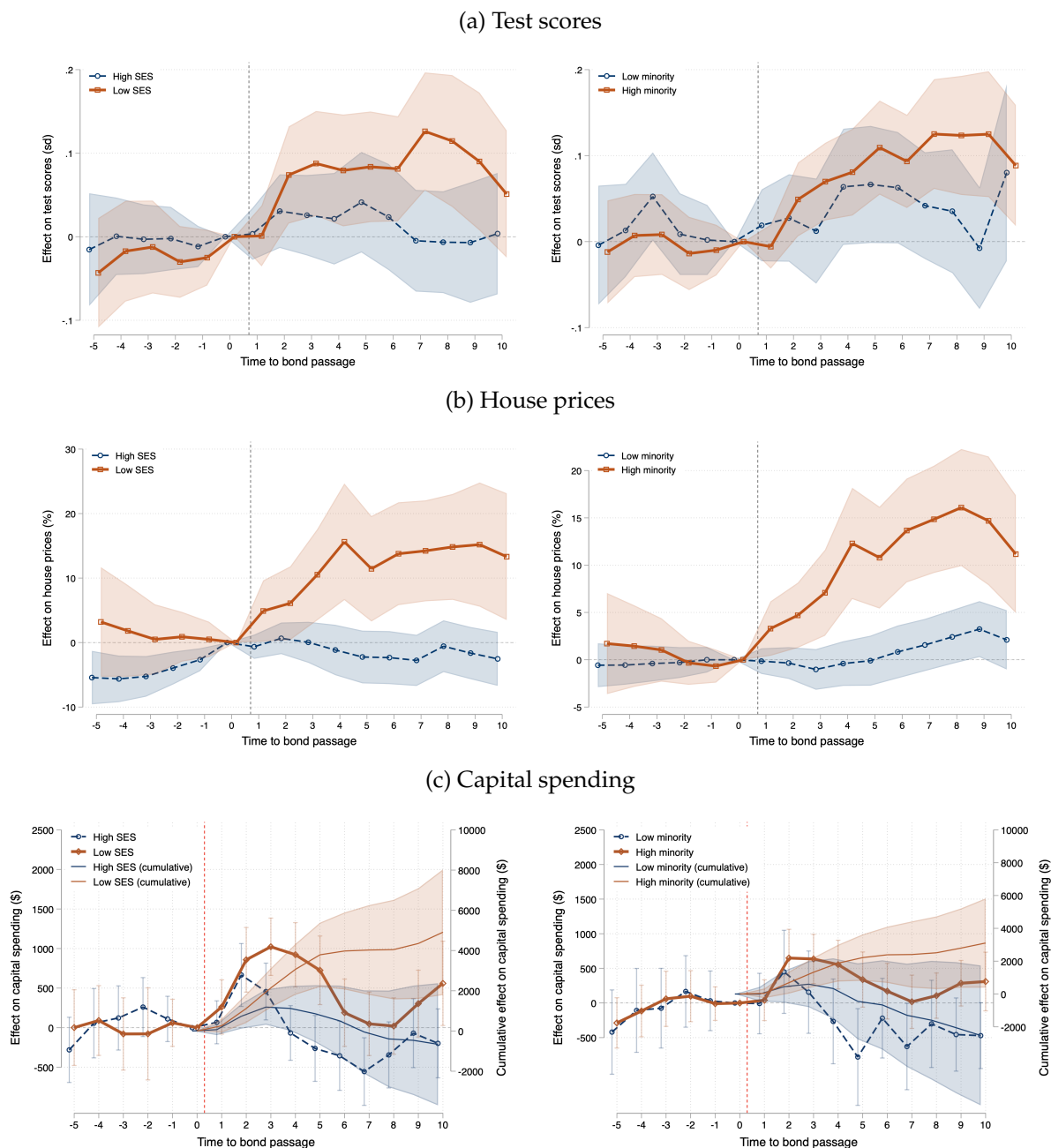

(b) House prices

(c) Capital spending

Note: Estimates and confidence intervals of the parameters β 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, and using as "clean controls" only districts that share the bond history with at least one treated district in their cohort. Figures in the left panels show estimates by tercile of the share of disadvantaged students ("low-SES" denotes the top tercile and "high-SES" 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 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 A33: Effects of Bond Authorization By Student Demographics. Stacked Approach, Not Controlling for Future Bond History

(a) Test scores
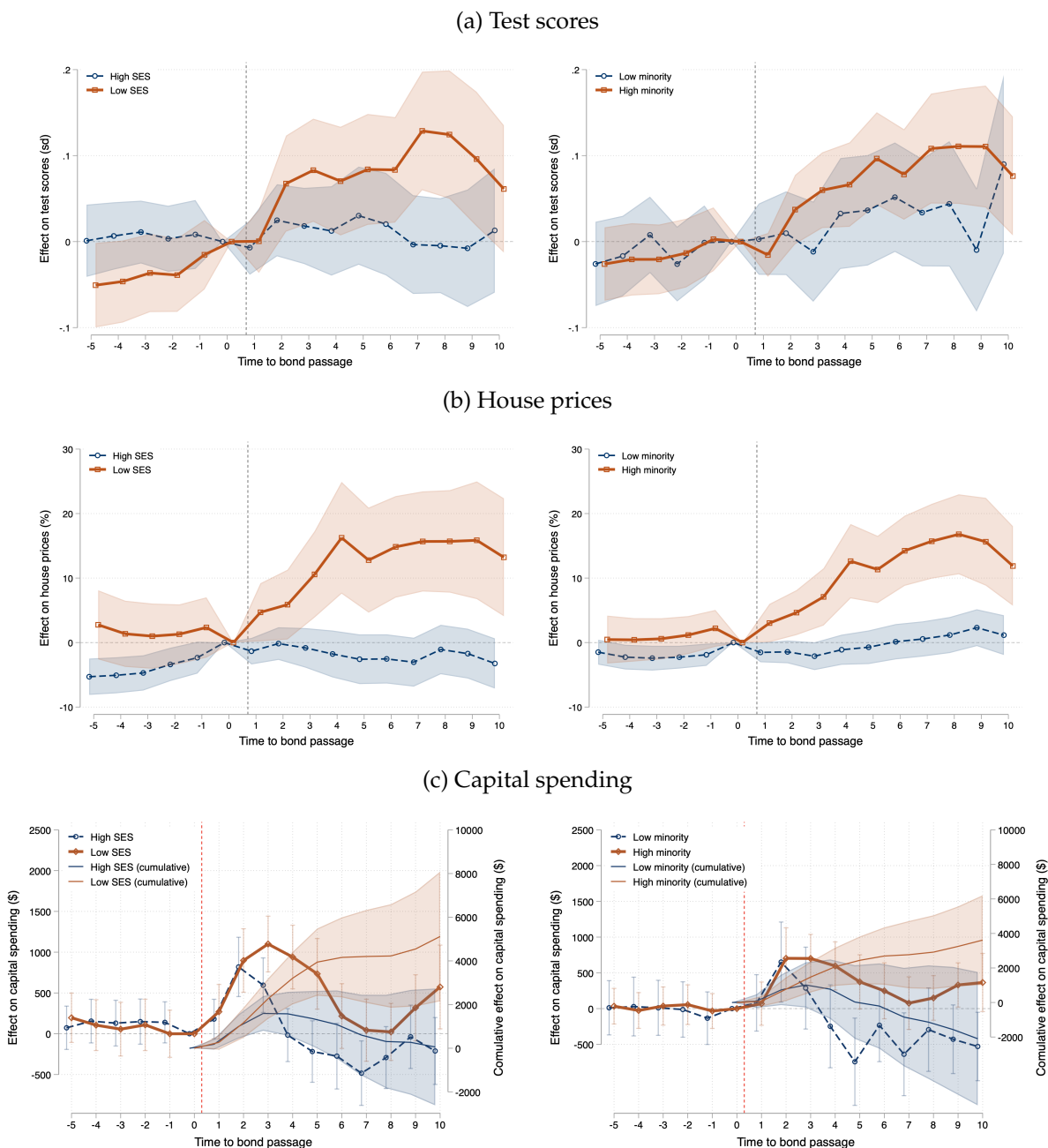

(b) House prices

(c) Capital spending

Note: Estimates and confidence intervals of the parameters β 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, and using as "clean controls" only districts that share the bond history with at least one treated district in their cohort. Figures in the left panels show estimates by tercile of the share of disadvantaged students ("low-SES" denotes the top tercile and "high-SES" 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 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 A34: Effects of Bond Authorization By Student Demographics. Extended Two-Way-Fixed-Effects Estimator as in Wooldridge (2021)

(a) Test scores

(b) House prices

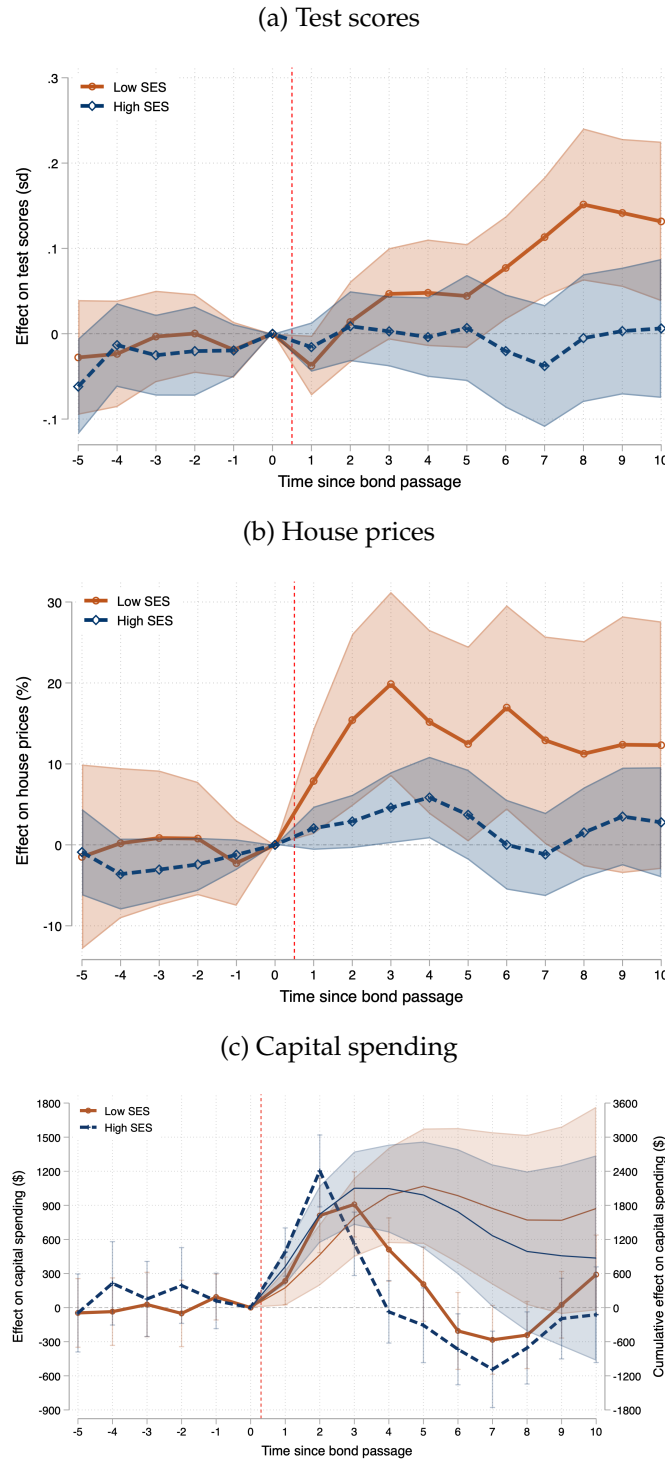
(c) Capital spending

Note: Estimates and confidence intervals of the parameters β 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, and allowing for the treatment effect to be heterogeneous across cohorts, as in Wooldridge (2021) (we show averages of treatment effects across cohorts). Estimates are shown by tercile of the share of disadvantaged students ("low-SES" denotes the top tercile and "high-SES" denotes the bottom tercile). We average test scores across grades and subjects within a district-year, using the number of test score takers as weights. All 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.

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Figure A35: Effects of Bond Authorization By Spending Category and Share of Low-SES Students
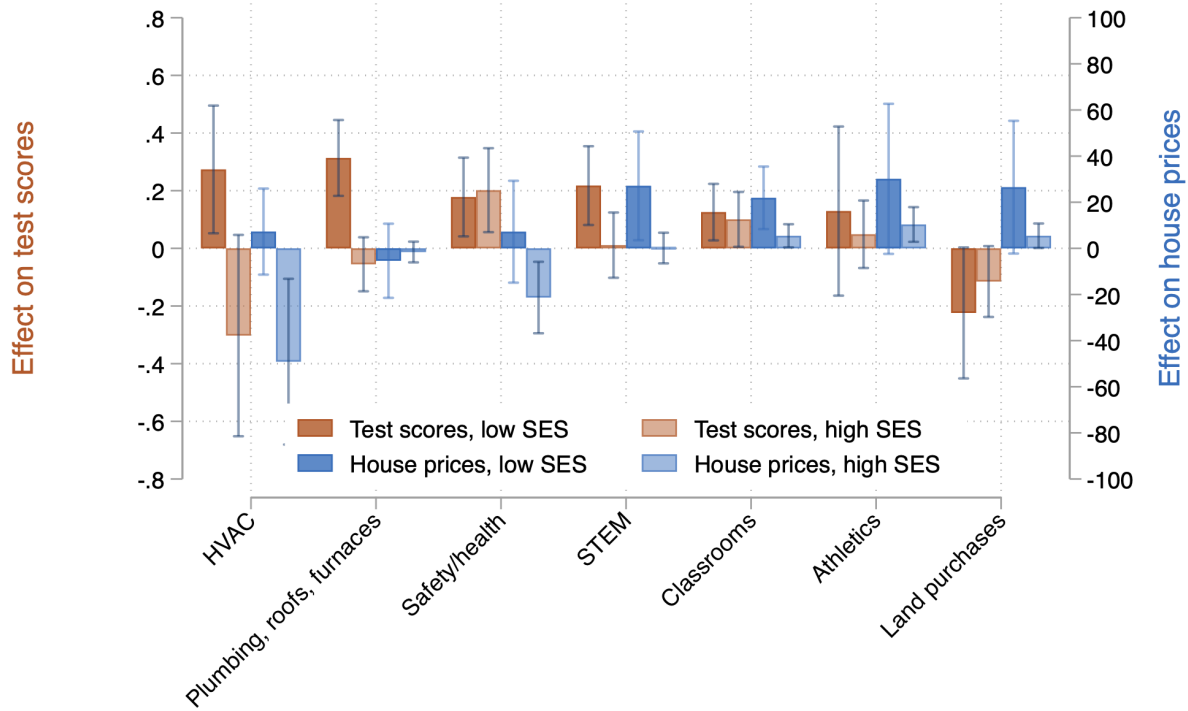
Note: Point estimates and confidence intervals of a linear combination of the parameters $\beta_{k,p}$ in equation (6) for $k \in [3,6]$, shown separately for each spending category p and estimated separately for districts in the top tercile of the distribution of low-SES students ("low-SES", darker series) and those in the bottom tercile ("high-SES", lighter series). The orange series are 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 are 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. Confidence intervals are calculated using standard errors clustered at the district level.

Figure A36: Effects of Bond Authorization, By Spending Category and Share of Minority Students
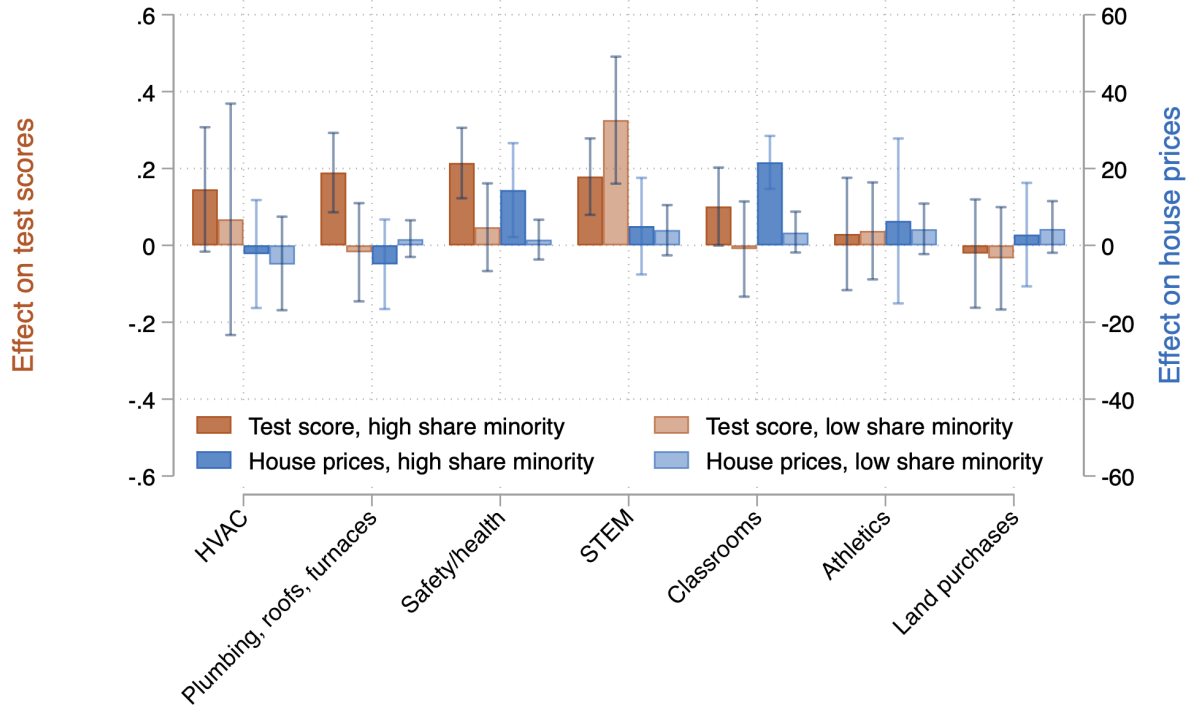
Note: Point estimates and confidence intervals of averages of the parameters $\beta_{k,p}$ in equation (6) for $k \in [3,8]$, shown separately for each spending category p and estimated separately for districts in the top tercile of the distribution of the share of Black and Hispanic students ("high minority", darker series) and those in the bottom tercile ("low minority", lighter series). The orange series are 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 are 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. Confidence intervals are calculated using standard errors clustered at the district level.

Figure A37: Capital Stock and Share of Low-SES Students: Correlation
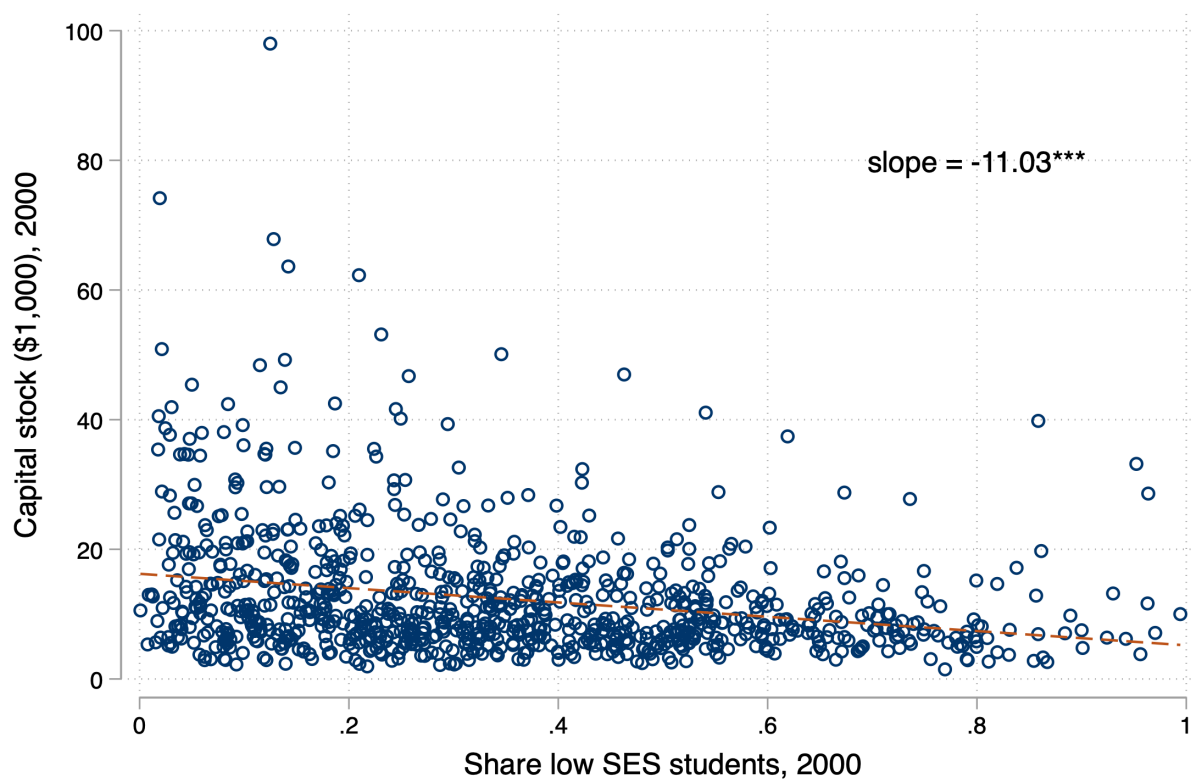
Note: Scatter plot of districts' shares of low-SES students (horizontal axis) and capital stock, calculated as each district's sum of capital spending over 30 years using a 5% depreciation rate (vertical axis). Variables are measured in the year 2000.

Figure A38: Replicating Estimates from Previous Studies

(a) Dynamic estimates, by state and study
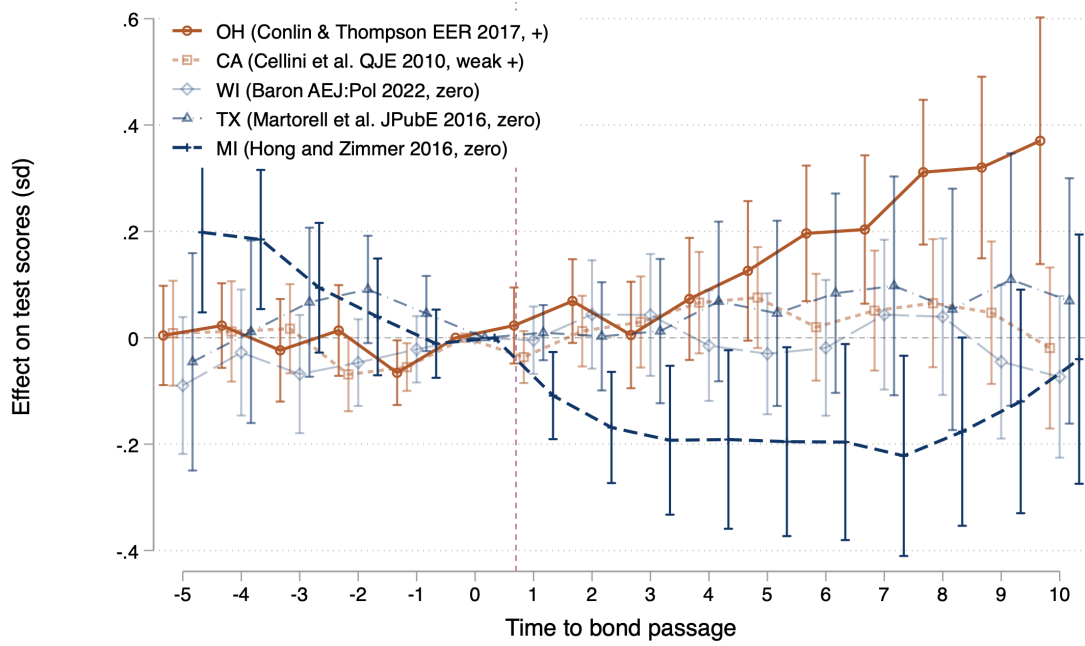
(b) Share of bonds in each category by state and district SES
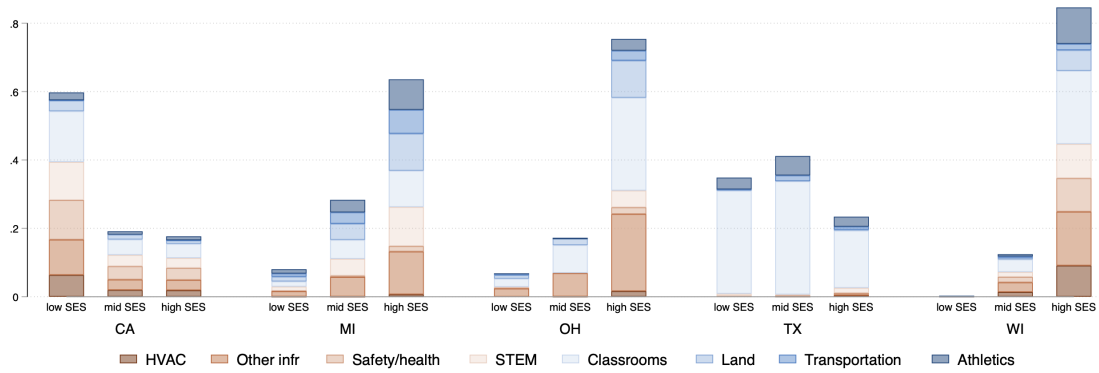
Note: Panel (a) shows estimates and confidence intervals of equation (3) obtained using data from the states and years included in each study. Panel (b) shows the share of bonds passed in each state and time period, by category and type of district. *Low SES*, *mid SES*, and *high SES* refer to districts in the first, second, and third tercile of the distribution of the share of low-SES students across the whole country.

Table A1: Close vs Non-Close Elections: District Expenditures, Bonds, and Spending Categories

	Non-close	Close (margin= $+/-0.1$)	Difference
Capital	1272.8	1061.3	211.6***
			(45.96)
Current	7942.3	6946.0	996.3***
			(75.71)
Spending rules			
Share w/supermajority	0.122	0.208	-0.0859***
			(0.00618)
Voting requirement	0.509	0.515	-0.00577***
			(0.000499)
Debt limit (share prop. value)	0.0866	0.0948	-0.00822***
			(0.00106)
Share approved	0.839	0.622	0.217***
			(0.00717)
Vote margin	0.157	0.0155	0.142***
			(0.00239)
Size p.p. proposed ($1,000)	6.858	8.270	-1.411***
			(0.173)
Categories, approved bonds			
Classrooms	0.387	0.571	-0.183***
			(0.0118)
Other infrastructure	0.212	0.379	-0.166***
			(0.0105)
HVAC	0.106	0.141	-0.0346***
			(0.00778)
STEM equipment	0.238	0.351	-0.113***
			(0.0107)
Safety/health	0.184	0.242	-0.0583***
			(0.00970)
Athletic facilities	0.157	0.205	-0.0478***
			(0.00912)
Transportation	0.368	0.196	0.172***
			(0.0110)
Land purchases	0.0955	0.190	-0.0942***
			(0.00799)
Demographics and outcomes			
Share low-SES	0.418	0.376	0.0419***
			(0.00386)
Share Black/Hispanic	0.226	0.215	0.0104**
			(0.00450)
ELA test scores	-0.0679	-0.0624	-0.00548
			(0.0163)
Math test scores	-0.0948	-0.0778	-0.0169
			(0.0164)
House price index (1989 = 100)	183.5	191.2	-7.702***
			(1.169)
Number of districts	3,446	3,085	4,683
Number of states	29	28	29

Note: Means and standard deviations of variables of interest, for close and non-close elections. Close elections are defined as those with an absolute vote margin of at most 15%.

Table A2: First Stage: Effects of Bond Authorization on School Expenditures. Stacked Approach, Matching on Pre-Election Bond History

Type of expenditure:	Capital	Current	Other non-instr services
Average effect over:	(1)	(2)	(3)
1-5 years	360***	-6	2
	(98)	(30)	(3)
6-10 years	-159	-38	5
	(116)	(46)	(7)
11-15 years	73	-109**	5
	(115)	(52)	(8)
District FE	X	X	X
Year-State FE	X	X	X
Adj. R^2	0.288	0.977	0.873
N	124,105	124,105	124,105

Note: Estimates and standard errors of linear combinations of the parameters β_k in equation (3), obtained using as "clean controls" only districts that share the bond history with at least one treated district in their cohort. 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 A3: Effects of Bond Authorization on Student Achievement and House Prices. Stacked Approach, Matching on Pre-Election Bond History

	Test scores			HPI	Enrollment			Test	HPI
	Pooled	Math	ELA		ln(Enrollment)	White	High-SES	scores	
Average effect over:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1-4 years	0.043***	0.041**	0.045***	2.777**	0.003	0.004*	0.010***	0.041***	1.999*
	(0.014)	(0.018)	(0.014)	(1.348)	(0.005)	(0.002)	(0.003)	(0.013)	(1.177)
5-8 years	0.081***	0.078***	0.087***	7.398***	0.008	0.007*	0.023***	0.071***	5.944***
	(0.022)	(0.028)	(0.022)	(2.131)	(0.011)	(0.004)	(0.007)	(0.021)	(1.888)
9-12 years	0.069**	0.046	0.092***	5.606**	0.021	0.006	0.022***	0.061**	4.190**
	(0.028)	(0.032)	(0.029)	(2.023)	(0.014)	(0.004)	(0.007)	(0.028)	(1.912)
District FE	X	X	X	X	X	X	X	X	X
Yr-St-Gr-Subj FE	X							X	
Yr-St-Gr FE		X	X						
Year-State FE				X	X	X	X		X
Enroll. shares								X	X
Adj. R^2	0.875	0.866	0.898	0.937	0.998	0.990	0.932	0.877	0.940
N	1,071,680	519,235	552,427	83,287	124,113	123,870	120,686	1,050,935	80,453

Note: Estimates and standard errors of linear combinations of the parameters β_k in equation (3), obtained using as "clean controls" only districts that share the bond history with at least one treated district in their cohort. The dependent variables are pooled test scores (columns 1 and 8); Math and ELA test scores (columns 2 and 3, respectively); the house price index (columns 4 and 9); the natural logarithm of total enrollment (column 5); and the share of enrolled students who are white (column 6) and high-SES (column 7). All columns control for district-by-cohort and cohort-bystate-by-year effects. Columns 1 and 8 also control 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. Columns 8 and 9 additionally control for the share of white and low-SES students in each district and year. Standard errors in parentheses are clustered at the district level. $* = 0.1$; $** = 0.05$; $*** = 0.01$.

Table A4: First Stage: Effects of Bond Authorization on School Expenditures. Stacked Approach, Controls Never Authorize a Bond in Time Window of Analysis

Type of expenditure:	Capital	Current	Other non-instr services
Average effect over:	(1)	(2)	(3)
1-5 years	380***	1	1
	(97)	(30)	(3)
6-10 years	-161	-24	4
	(114)	(45)	(7)
11-15 years	45	-94*	4
	(114)	(51)	(8)
District FE	X	X	X
Year-State FE	X	X	X
Adj. R^2	0.287	0.977	0.871
N	126,421	126,421	126,421

Note: Estimates and standard errors of linear combinations of the parameters β_k in equation (3), obtained using as "clean controls" only districts that never authorize any bonds in the time window of analysis. 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 A5: Effects of Bond Authorization on Student Achievement and House Prices. Stacked Approach, Controls Never Authorize a Bond in Time Window of Analysis

| | Test scores | | | HPI | Enrollment | | | Test | HPI |
| | Pooled | Math | ELA | | ln(Enrollment) | White | High-SES | scores | |
Average effect over:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1-4 years	0.042***	0.041**	0.045***	2.533*	0.005	0.004*	0.009**	0.041***	1.810
	(0.014)	(0.018)	(0.014)	(1.354)	(0.005)	(0.002)	(0.003)	(0.013)	(1.183)
5-8 years	0.082***	0.080***	0.088***	7.078***	0.011	0.006*	0.022***	0.071***	5.691***
	(0.022)	(0.027)	(0.021)	(2.117)	(0.010)	(0.004)	(0.007)	(0.020)	(1.873)
9-12 years	0.073**	0.051	0.096***	5.095**	0.024*	0.005	0.020***	0.064**	3.741*
	(0.028)	(0.031)	(0.028)	(2.006)	(0.014)	(0.004)	(0.007)	(0.027)	(1.893)
District FE	X	X	X	X	X	X	X	X	X
Yr-St-Gr-Subj FE	X							X	
Yr-St-Gr FE		X	X						
Year-State FE				X	X	X	X		X
Enroll. shares								X	X
Adj. R^2	0.874	0.864	0.896	0.936	0.998	0.990	0.932	0.875	0.939
N	1,091,678	529,101	562,559	84,634	126,429	126,182	122,894	1,070,120	81,765

Note: Estimates and standard errors of linear combinations of the parameters β_k in equation (3), obtained using as "clean controls" only districts that never authorize any bonds in the time window of analysis. The dependent variables are pooled test scores (columns 1 and 8); Math and ELA test scores (columns 2 and 3, respectively); the house price index (columns 4 and 9); the natural logarithm of total enrollment (column 5); and the share of enrolled students who are white (column 6) and high-SES (column 7). All columns control for district-by-cohort and cohort-bystate-by-year effects. Columns 1 and 8 also control 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. Columns 8 and 9 additionally control for the share of white and low-SES students in each district and year. Standard errors in parentheses are clustered at the district level. * = 0.1; ** = 0.05; *** = 0.01.

B Construction of The Dataset

B.1 Test Scores

Here, we describe the collection, compilation, and standardization of test score data across states and years. To construct our panel of test scores at the district-subject-grade-year level, we rely on multiple data sources:

1. For years 2005 and earlier, we rely on data from the National Longitudinal School Level State Assessment Score Database (NLSLSASD).[38] Test scores are at the school-grade-subject-year level, and include data from all states from 2003-2005, and a subset of states in earlier years. Most states have data from at least 1999, with the earliest state (Maryland) reporting data as early as 1993.

2. For 2006-2008, we collected data from individual states. Data were collected from state departments of education. In some states, data were publicly accessible on a state website, while other states required us to submit public data requests. Through this process we collected from 44 states and the District of Columbia; we were unable to collect data for Alabama, Alaska, Hawaii, Nebraska, North Dakota, and Oklahoma. Depending on the state, data are either at the district-subject-grade-year or the school-subject-grade-year level.

3. For 2009-2018, we rely on district-subject-grade-year test score data in math and ELA from the Stanford Education Data Archive (SEDA).[39]

We restrict only to test scores in grades 3-8; data for other grades is inconsistent across years, states, and subjects. We restrict attention only to district-level test scores. For state-years where we have school-level but not district-level scores, we take the weighted average score across schools, weighting by enrollment.[40]

For each state and year, we keep only test scores for math and English Language Arts (ELA) for the primary exam used in the state to assess educational standards. For ELA scores, we use scores from the reading, language, or literacy tests in a state. If multiple exams (e.g. reading and literacy) are available in a given year, we use only the reading exam.[41] Finally, we keep only districts with non-missing district IDs (NCES LEA IDs).

B.1.1 Standardizing Data

For the non-SEDA data, the type of test scores vary by state, subject, and year, including proficiency shares or counts, normed scale scores, percentile scores, and normal curve equivalence

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[38]We thank Sean Reardon and Jesse Rothstein for sharing NLSASASD data.

[39]We use data from SEDA version 4.0.

[40]When included in the data, we weight by the number tested. If the number tested is not available, we use school enrollment from the NCES Common Core of Data as weights to construct the mean score.

[41]In some cases, there are scores for reading exams that are not the primary state standards assessment. In these instances we use scores from the language or literacy portion of the primary state standards exam.

scores. For percentile and normal curve equivalence scores, we construct a mean test score by using the inverse normal transformation. For test score data with proficiency rates or counts, we need to estimate mean scores using the distribution of students in each proficency category.

We estimate mean scores from proficiency count data using hetoeroskedastic ordered probit models (HETOP), following the approach developed by Reardon et al. (2017) and used in the SEDA data Fahle et al. (2021).[42] When only two proficiency levels are available (e.g. above/below proficient), we estimate mean scores using homoskedasitc (HOMOP) models. Where more than two proficiency levels are available, we estimate mean scores using hetoeroskedastic (HETOP) models. We exclude roughly 3% of observations where all students in a district-subject-grade-year are in a single proficiency category.

Next, we convert scores to standard deviation units and standardize scores. To construct a consistent sample across test score data sources, we restrict attention to districts that appear at least once in the SEDA data from 2009-2018. Then, for all non-SEDA test scores we convert scores to district-level standard deviations, using the mean and standard deviation within subject-grade-year and across districts.

Finally, we convert scores to a common scale across state-years using the distribution across state-years on the National Assessment for Educational Progress (NAEP). NAEP state-level scores are generally available every other year for 4th and 8th grade math and reading. Starting in 1994, we estimate state-level mean NAEP scale scores and standard devitaions by linearly interpolating or extrapolating across grades and years, using the biannually available data. Standardized disrtict level scores are then constructed by taking the product of our district-level mean scores and the NAEP state-subject-grade-year standard deviation, and then adding the mean state-subject-grade-year NAEP score. To conform with SEDA "cohort scale" scores, we standardize these mean scores relative to the average of the NAEP mean and standard deviation of the four national cohorts in 4th grade in 2009, 2011, 2013, and 2015 (Fahle et al., 2021).[43]

B.2 Bond Data

B.2.1 Classifying Bonds into Categories

We classify bonds into eight non-mutually exclusive categories using the text of each bond's ballot. Specifically, we assign a bond to a category if its ballot text contains a related word or word substring. The assignment rules are as follows:

- *Classroom*: Text contains one among "building", "Building", "classroom", "Classroom" "school fa" "School fa" AND one among"construct", "Construct", "overcrow", "Overcrow", "const.", "renov", "Renov", "repa", "Repla", "repla", "Repa", "modern", "Modern", "improv", "Improv", "upgrad", "Upgrad", "refurb", "Refurb"

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[42]We estimate the HETOP models on our data using the `hetop` command in Stata.

[43]Because SEDA scores are standardized in student-level and not district-level standard deviation units like our other district-level data, we standardize the SEDA scores to the district level by first inverting the NAEP normalization and rescaling to district-level standard deviations, and then reapplying the NAEP normalization.

- *Other Infrastructures*: Text contains one among "plumbing" "Plumbing", "sewa", "Sewa", "sewi", "Sewi", "flush", "Flush", "Restroom", "restroom", "roof", "ROOF", "Roof", "furni", "Furni", "FURNI", "window", "Window", "Door", "door"

- *HVAC*: Text contains one among "HVAC", "hvac", "Hvac", "Cool", "cool", "COOL", "Heat", "HEAT", "heat", "air co", "Air co", "air-co", "Air-co", "vent", "Vent"

- *STEM*: Text contains one among "Lab", "lab", "career tech", "Career tech", "Career Tech", "Career tech", "Career Tech", "vocat" "Vocat", "STEM", "Comput", "comput", "COMPUT"

- *Safety/health*: Text contains one among "Safe", "safe", "SAFE", "Security", "security", "surveil", "Surveil" "Alarm", "alarm" "fire", "FIRE", "Asbes", "asbes", "ASBES"

- *Athletic*: Text contains one among "thlet", "THLET", "gym", "Gym", "GYM", "tadiu", "TADIU", "Sport", "sport", "SPORT", "field", "Field"

- *Transportation*: Text contains one among "bus", "BUS", "Bus", "Vehicle", "vehicle", "VEHICLE", "transpo", "Transpo", "TRANSPO"

- *Land purchases*: Text contains one among "land", "Land", "site", "Site" AND one among "acqui", "Acqui", "purch", "Purch"

Table B1: Bond Data: Sources, Limitations, and Inclusion in Final Sample

State	Source	Data Issues	Satisfies RD assmps	In final dataset	Has ballot text
Alabama	N/A				
Alaska	N/A				
Arizona	Stifel, Nicolaus & Company, Inc.		X	X	
Arkansas	Stéphane Lavertu's records; Division of Elections		X	X	
California	California Elections Data Archive		X	X	X
Colorado	Dept of Education		X	X	
Connecticut	Office of Secretary of State		X	X	X
Delaware	Dept of Elections		X	X	X
Florida	TaxWatch; Dept of Education		X	X	
Georgia	Secretary of State		X	X	X
Hawaii	N/A - doesn't vote				
Idaho	Secretary of State		X	X	X
Illinois	State Board of Education	Too few bonds			X
Indiana	Secretary of State		X	X	
Iowa	Dept of Education		X	X	
Kansas	Dept of Education	Too few bonds			X
Kentucky	N/A - doesn't vote				
Louisiana	Secretary of State		X	X	
Maine	N/A				
Maryland	State Board of Elections		X	X	X
Massachusetts	Dept of Elections, Dept of Revenue		X		X
Michigan	Stéphane Lavertu's records; Association of School Boards		X	X	X
Minnesota	Dept of Education		X	X	X
Mississippi	Statewide Election Management System		X	X	
Missouri	State Auditor's Office, collected by Shiloh Dutton; Stéphane Lavertu's records				X
Montana	Secretary of State	Too few bonds			X
Nebraska	Stéphane Lavertu's records; Board of State Canvassers		X	X	X
Nevada	Secretary of State		X	X	X
New Hampshire	N/A				
New Jersey	School Boards Association	No vote share			X
New Mexico	nmbonds.com	Too few bonds			
New York	Stéphane Lavertu's records		X	X	X
North Carolina	Dept of State Treasurer	X	X	X	
North Dakota	N/A				
Ohio	Stéphane Lavertu's records		X	X	X
Oklahoma	Stéphane Lavertu's records				X
Oregon	Oregon School Board Association		X	X	
Pennsylvania	Association of School Business Officials; Stéphane Lavertu's records		X	X	
Rhode Island	Secretary of State		X	X	
South Carolina	Election Commission	Too few bonds			X
South Dakota	Secretary of State	Too few bonds			
Tennessee	Individual district offices	Too few bonds			
Texas	Stéphane Lavertu's records		X	X	X
Utah	N/A				
Virginia	Department of Elections		X	X	X
Vermont	N/A				
Washington	Office of the Superintendent				
West Virginia	Secretary of State		X	X	
Wisconsin	Adam Gamoran - University of Wisconsin		X	X	X
Wyoming	N/A				
Total			28	28	23

Note: Sources, availability, and limitations of bond election records by state.