Effectiveness and Efficiency of School Capital Investments Across the U.S.[*]

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Abstract

This paper studies the impact of capital projects on student learning and the real estate market, using nationwide data on U.S. school districts and focusing on what investments work and on whom. We use newly collected data on school capital bonds, test scores, and house prices for 28 U.S. states and a new research design that identifies the causal impact of bond authorizations in the presence of dynamic and heterogeneous treatment effects. On average, bond authorization significantly raises test scores and house prices. Yet, there are large differences across bonds and districts. Spending on infrastructure renovation and upgrades, such as HVAC or roofs, raises test scores but not house prices; conversely, spending on athletic facilities increases house prices but not test scores. Bond authorization is most beneficial in districts with more disadvantaged student populations, in part because these districts prioritize bonds that improve learning. We find suggestive evidence that capital funding rules drive differences in bond impacts.

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1 Introduction

U.S. public school districts spend a considerable portion of their budgets on the renovation and construction of school facilities. Between 2000 and 2019, the average school district spent $1,250 per pupil on capital each year (approximately 10% of total spending). Yet, large differences exist among districts, even within states, in amounts spent and the state of school facilities. For example, in 2000-2019 the school district of Amarillo, enrolling 30,000 students in northern Texas, spent about $500 per pupil and year; the school district of Frisco, enrolling 20,000 students in the suburbs of Dallas, spent $6,500. There is also dramatic variation in facility conditions across districts, with some featuring modern athletic facilities and state-of-the-art HVAC systems and others contending with dilapidated buildings and makeshift classrooms.

The large size of school capital investments and the poor conditions in some schools have pushed researchers to study whether spending money on school capital benefits students (as captured by test scores) and is valuable for taxpayers (as captured by real estate prices). Due to the scattered landscape of school financing in the U.S. and the absence of a national comprehensive dataset on school achievement, nearly all existing studies look at the experience of individual states or school districts. These studies have reached strikingly conflicting conclusions, ranging from positive effects on both test scores and house prices (Neilson and Zimmerman, 2014; Lafortune and Schönholzer, 2022), to effects only on house prices (Cellini et al., 2010) to no effects at all (Martorell et al., 2016; Brunner et al., 2022). One possible explanation for these findings is that prior studies are underpowered to detect meaningful effects, particularly given the long time frame of capital investments (Jackson and Mackevicius, 2023). Another possible interpretation is that capital spending is effective only under some circumstances. What these circumstances are, though, remains unclear. It could be *what* the money is spent on that matters—whether a bond is used to fix a leaking roof or to build a football stadium; or, it could be *who* it is spent on—for example more or less disadvantaged students. It could also be both. The debate on the effectiveness and efficiency of school capital spending is thus wide open (Handel and Hanushek, 2022).

This paper contributes to this debate by studying the impact of capital projects on student learning and the real estate market, using new nationwide data on U.S. school districts and focusing on the *what* and the *who* of school capital investments. To do so, we use newly collected data on school capital bonds, test scores, and house prices for 28 U.S. states and a new research design that identifies the causal impact of bond authorizations in the presence of heterogeneous treatment effects. With this toolkit, we first estimate the impact of school capital expenditures in the U.S. as a whole; then, we produce estimates (i) by spending category (the *what*) and (ii) by student body characteristics (the *who*). While bond authorization raises test scores and house prices on average, there are large differences across bonds and districts. Spending on infrastructure renovation or upgrades, such as HVAC or roofs, 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 because these districts prioritize bonds that improve learning and are valuable to taxpayers.

Our analysis leverages a few distinctive features of the funding of school capital projects in the U.S. Capital spending is primarily (although not exclusively) locally financed through debt, in the form of bonds issued by the districts and repaid with local funds. The rules for the funding of capital projects are state-specific, and they differ across states along at least two important dimensions. First, in all but three states bond issuance is contingent to electoral approval in local referenda, which require a simple voting majority in some states and a supermajority in others. Second, 27 states impose limits to the amount of outstanding district debt. Lastly, states support district spending with various forms of grants. In our analysis, we focus on capital expenditures financed through debt funded locally, thus abstracting from state aid.

Our nationwide analysis of the effects of capital spending on outcomes is made possible by newly collected data on bonds, test scores, and house prices at the district level. To better understand what districts spend money on, we collected information on bond referenda, including the text of each ballot, the share of votes in favor, and the proposed investment amount. In nearly all elections, ballots describe the proposed use of the funds. Applying natural-language-processing (NLP) techniques to the text of each ballot, we categorize bonds into eight categories of projects: classroom space; infrastructures such plumbing, roofs, and furnaces; HVAC systems; IT facilities and labs (STEM); buildings adjustments to comply with health and safety standards; athletic facilities; purchases of lands; and purchases of vehicles, such as school buses. These categories balance the ease of extracting the information from the ballot text against the granularity of capital project types.

We then link information on bond elections to test scores of students in the district. Measures of student learning that are comparable across states and over time are generally unavailable because states measure achievement using different standardized tests, which have also changed over time. A notable exception is the Stanford Education Data Archive (Reardon et al., 2021), which contain data for 2009-2018. To cover earlier years, we collect school- and district-level test score data from each state's education department and from a discontinued national database. Following the approach of Reardon et al. (2017) and Fahle et al. (2021), we then convert scores from different tests (and thus on different metrics) to a uniform scale and normalized them across state-years to a common scale using the National Assessment of Educational Progress (NAEP). This procedure yields a novel district-level test score database starting in 1993, and covering nearly all states from 2002-2019. We further link these data to a house price index based on a repeated sales (Contat and Larson, 2022) and to district enrollment, expenditures, and revenues from the National Center for Education Statistics (NCES). Our final sample covers approximately 15,000 elections in 28 states, enrolling approximately 70% of all students in the U.S.

The standard approach to estimating the causal impact of school capital spending is a dynamic version of the regression discontinuity (RD) design around close bond elections (Cellini et al. 2010, henceforth CFR). This "dynamic RD treatment-on-the-treated" (DRD-TOT) estimator compares outcomes over time of districts that narrowly passed a bond to those that failed to do so, controlling for districts' bond histories. Under the assumption that treatment effects are constant across elections, this estimator accounts for the fact that districts may hold multiple elections over time and that the probability of proposing (and passing) future bonds is affected by their past success in passing bonds. However, the assumption of constant treatment effects is highly unappealing in our context: Differences in the impact of bonds across elections are at the center of our research question. To allow for treatment effect heterogeneity, we thus combine CFR's estimator with a stacked estimator based on "clean controls", an approach we label "stacked DRD." Namely, we match each district that approved a bond in a given year with all districts that also propose (but do not approve) a measure in the same year, do not approve any other measure in a (pre-determined) time window of interest, *and* share the same history of bond proposals. We then stack treated and control units for each election year and estimate CFR's model on this stacked dataset, controlling for fixed effects for each "cohort" of treated and control districts. This approach is similar to that used by Cengiz et al.

(2019) to estimate the impact of minimum wage hikes on employment in a difference-in-differences setting. We extend it to a DRD context; Monte Carlo simulations indicate that it recovers true treatment effects in the presence of treatment heterogeneity, whereas the standard approach does not always do so.

Our stacked DRD estimator confirms that capital spending increases by a total of $2,500 in the five years following bond authorization. This increase helps student learning and is valued by taxpayers. Test scores gradually increase after authorization, reaching a 0.08 standard deviations (sd) higher level after 8 years. 2SLS estimates imply that a $1,000 increase in capital spending over five years leads to a 0.05 sd increase in test scores (similar to what found by Jackson and Mackevicius, 2023, via a meta-analysis). House prices also increase by about 7%, indicating that taxpayers value school capital investments more than they are asked to pay for it. This discrepancy, though, appears to be driven by the presence of state aid (which creates a wedge between local taxes and amounts spent) rather than by inefficiencies in the level of spending. We also find a small decrease in the share of disadvantaged students after bond authorization. Yet, this small observable change cannot account for the increase in test scores.

Moving beyond these average impacts, differences between bond effects may stem from *who* gets treated (i.e. treatment *effect* heterogeneity) or *what* the treatment entails (i.e. *treatment* heterogeneity). Concerning *what*, we find that not all capital projects are created equal. Using information on spending categories from the text of each ballot, we estimate category-specific effects by adapting our stacked DRD design to only compare districts that propose bond measures in the same category. These estimates reveal that the effects of bond authorization on test scores are concentrated on a few categories such as HVAC, infrastructures, and STEM facilities. Spending on athletic facilities, land purchases, or buses instead does not impact learning. Importantly, we also find that spending on learning-enhancing categories does not necessarily increase house prices, and that the categories that most increase prices are athletic facilities and the expansion of classroom space.

Turning to the question of how bond impacts depend on *who* gets treated, the second distinction we make is among districts that serve different populations of students. We find that the impacts of bond authorization on test scores and house prices is concentrated in districts with a large shares of low-SES or minority (black and Hispanic) students. Differences in impacts by student demographics cannot be explained by the size of the spending increase, nor by differences in the existing capital stock. In part, they appear due to a heavier focus on learning-enhancing and price-increasing items among bonds approved in in low-SES and high-minority districts. Even conditioning on spending categories, though, low-SES and minority districts benefit more from bond authorization. 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, it is provided at an inefficiently low level.

In the last part of the paper, we explore the ultimate causes of differences in impacts by studying differences across states with different funding rules. We focus on two rules: the requirement of a majority greater than 50% (supermajority) to authorize a bond and the presence of a limit to the amount of outstanding debt. Each of these rules imposes an additional constraint for districts to raise money for capital projects. This, in turn, may impact the size and composition of the bonds they propose and authorize, and the marginal return to each dollar invested. We find that districts in supermajority states see the largest increases in test scores following bond authorization, with no effects on house prices. These districts tend to pass larger bonds focused on items that primarily increase test scores. Districts in states with a debt limit see slightly larger effects on test scores, but smaller effects on house prices. This occurs because presence of a debt limit reduces districts' spending on capital projects, but this reduction primarily affects projects that predominantly increase house prices (rather than test scores).

Taken together, our results indicate that considering what the money is spent on and who benefits is crucial when evaluating the impact of increased school spending. Ignoring differences in the use of funds and the way they are distributed across students can lead to misguided conclusions about the returns to educational investments. Our findings also indicate that the stringency of a state's school funding rules may play a role in shaping what projects districts end up carrying on, as well as the marginal benefit of the investment for students and taxpayers. In a companion paper, we study the role some these rules play in shaping capital investments and how changes to these rules could improve the allocative efficiency of school capital finance.

Contribution to the literature. Our paper primarily relates to a broad literature spurred by the Coleman report (Coleman et al., 1966) on the question of whether money matters in education. While older works expressed skepticism towards resource-based policies (e.g., Hanushek, 1997), more recent studies of state-level school finance reforms have 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; Lafortune et al., 2018; Jackson, 2020), labor market outcomes (Jackson et al., 2016), and intergenerational mobility (Biasi, 2023). Like we do, some of these studies have used variation from close elections to identify the effects of increased current and operational spending (Baron, 2022; Abott et al., 2020). 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 the community, and by demonstrating that properly accounting for the use of funds and the characteristics of the students who experience the funding increase is crucial to establish if and how money matters.

Our study also contributes to a recent body of works, pioneered by Cellini et al. (2010), that have estimated the effects of school capital expenditure on students and the real estate market, often reaching conflicting conclusions. Most of these studies leverage evidence from single states (Cellini et al., 2010; Martorell et al., 2016; Hong and Zimmer, 2016; Conlin and Thompson, 2017; Baron, 2022) or individual school districts (Neilson and Zimmerman, 2014; Lafortune and Schönholzer, 2022), with one cross-state study relying on wind energy revenues(Brunner et al., 2022). Most of these studies find small and often imprecisely estimated effects of capital spending, whereas Neilson and Zimmerman (2014) and Lafortune and Schönholzer (2022) find larger and positive effects effects. We contribute to these studies by offering the first near-nationwide analysis of the effect of capital spending on students and house prices. In addition, we reconcile their findings by documenting how the returns to school capital investments depend on the circumstances under which they occur and the specific projects that are financed.

Lastly, we contribute to a more broad literature on the valuation of public investments. Our empirical results provide evidence that the fiscal institutions that govern local public good investments can have a significant impact on their effectiveness and efficiency. In particular, constraints on raising local funding can lead to inefficiently low levels of spending, evidenced by the robust positive effects for marginal projects under these regimes.

2 School Capital Expenditures Across The US

In 2018-19, U.S. public K-12 schools spent roughly $73 billion on capital outlay, including on the construction of new buildings, renovations of existing buildings, land purchases, and equipment (Cornman et al., 2021).[1] This figure amounts to just under 10% of total education spending.[2]

Funding for capital infrastructure is unique in a number of ways. Most notably, it largely comes from local bonds, financed through property tax increases and fees on new real estate developments. Between 2008-09 and 2018-19, 77% of U.S. school capital expenditures were locally funded (Filardo, 2016).[3] This stands in stark contrast to the funding of school operations, which relies more heavily on state support. Furthermore, while the state-level school finance reforms of the past 50 years have led to the equalization (and in many cases the progressivity) of funding for operational expenditures across school districts (Hoxby, 2001; Corcoran and Evans, 2015; Lafortune et al., 2018; Biasi, 2023), funding for capital outlay has remained higher for higher-income and higher-wealth school districts in each state since at least 1990 (Biasi et al., 2021). More generally, capital spending varies significantly both across and within states (Appendix Figure A1), and so does the state of school facilities (Alexander and Lewis, 2014; Nowicki, 2020).

2.1 State Rules for The Funding of School Capital Projects

States have autonomy in setting the rules that determine how school districts can raise money for capital projects.[4] We summarize them in this section and provide additional information on bond elections, at the core of our empirical strategy.

State aid. The reliance on local revenues varies considerably across states. On the one hand, a few states (Alaska, Hawaii, Maine, Massachusetts, New Hampshire, New York, Rhode Island, and Wyoming) almost uniquely fund capital investments with state dollars. On the other hand, roughly half of all states contribute less than 5% of overall funding. When provided, state support is also

[1]Repairs, routine maintenance, and debt service are not included in this figure.

[2]The vast majority (87%) of K-12 expenditures go towards current operations (i.e. staffing, materials, and maintenance). Debt service is roughly 3% of spending, and another 1% goes towards other programs such as community services, adult education, and community colleges.

[3]States contribute 22% of funding for capital expenditures on average. The federal government plays a trivial role in school facility funding, covering only 1% of capital outlay between 2008-09 to 2018-19.

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

determined in ways that differ across states. Twenty-seven of them provide matching grants with varying rates and structures. For example, California funds between 50% and 60% of capital project costs; to access these funds, districts need to first raise funds locally.[5] Other states provide grants not contingent on local revenues, funded through sales taxes, state bond revenues, and general fund appropriations, and often designed in a progressive way. Ohio, for example, distributes funds to districts based on a ranking determined by local property wealth and household income.[6]

Local bond elections. With limited state support, local bonds are the key source of capital funding in most states.[7] In all but three states (Hawaii, Kentucky, and Massachusetts), voter approval is required for school districts to issue bonds. Voting takes place in local referenda, held either during a primary or general election or during an "off-cycle" election; the exact timing is a decision of each school district. Importantly, states require different majorities for the authorization of local bonds, defined as the share of favorable votes among those who turn out to vote.[8] Thirty-seven states require a simple majority of 50%. Ten states require a supermajority, ranging from 55% in California to 67% in Idaho (Appendix Figure A2). With the exception of a reduction in the required majority from two-thirds to 60% in New Hampshire in 1999 and from two-thirds to 55% in California in 2001, states' required majorities did not vary between 1995 and 2017, our time period of analysis.

Ballots generally outline the proposed use of the funds, project costs, and the projected increase in local property taxes. For example, voters in the Fremont Union High School District, CA 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*

[5] 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, resulting in a regressive distribution of local and state funds for school modernization (Lafortune and Gao, 2022; Brunner et al., 2023).

[6] 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).

[7] In this paper, we only focus on bond elections for capital outlay, as opposed to elections to increase local property tax rates to fund school operational expenses.

[8] While data on turnout rates for local elections are generally unavailable, recent calculations suggest rates in the ballpark of 20% (Bowers et al., 2010).

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. The district was thus able to issue bonds, the state matched 60% of local funds via a matching grant, and over the next five years the district's schools will be modernized with these funds. 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 propose and pass several bond measures over time, and districts who fail to approve a measure may choose to hold another election shortly after. Districts may also choose to propose several small bonds in short succession (rather than a single large bond) to fund a given project.[9] We return to this issue in Section 4, because it is crucial for our empirical strategy.

Debt limits. In 40 states, school districts face limits to the amount of outstanding debt they can have at a given point in time. These limits are usually expressed as a share of assessed property valuation, and they range from 2% in Indiana to 30% in Arizona (Appendix Figure A3). The presence of these limits reduces, in some cases severely, the ability of districts to raise funds for capital projects. In 2017, districts in Arizona spent $1,600 on average on capital, whereas districts in Indiana spent $1,000 and districts in states with no debt limit spent $1,500.

3 Data

Our empirical analysis uses a novel panel dataset of U.S. school districts. To construct it, we link information on school district finances, enrollment and demographics, bond elections, test scores, and house prices. We also use information on the funding rules in place in each district and year, from Biasi et al. (2021). We now describe each set of variables and the related sources more in detail. We refer to each academic year with the calendar year of the Spring semester (e.g., 2017 for 2016-17).

[9] 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).

3.1 District Finances, Enrollment, and Demographic Information

Data on district finances and socio-economic characteristics are from the National Center of Education Statistics (NCES) Common Common Core of Data (CCD). Finance data come from the NCES's Annual Survey of School Districts and the Census of Government and is available since 1990. We use data from 1995, including districts' total expenditures and revenues, expenditure by category (capital, current instructional, and current non-instructional) and, since 1995, revenues by source; all these variables are measured in 2020 dollars per pupil. We also use NCES's annual demographic information for each school district, including enrollment, the racial and ethic composition of the student body, and the share of low-income students (defined as those eligible for free or reduced price school meals).

3.2 School Bond Elections

There is no comprehensive national database of local bond election outcomes. To overcome this gap, we constructed a novel database of district-year school capital bond elections using records of states and counties. In most states, local election data are recorded and maintained by either the state's secretary of state office or the department of education, and they are often available online. We compiled the available data on capital bond elections from all states where it was readily available and obtained part of the remaining ones through formal data requests.[10] Nearly all election records include the date, the share of votes in favor of the measure, the proposed bond amount, and the text of the ballot; in some cases, they also report keywords or purpose descriptions.

We were able to find bond information for 40 states.[11] Out of these, we excluded some states due to data limitations, such as reporting election data only for approved (but not rejected) bonds, not reporting vote shares, or reporting them only for a minority of all elections. We were also forced to discard data from three additional states because the assumptions of our empirical strategy are likely unmet there (Section 4 discusses this issue in more detail). If a district proposed or approved multiple bonds in the same year, we follow Martorell et al. (2016) and keep the largest bond (for elections with information on the bond amount) or the bond with the vote share closest to the

[10] We are grateful to Stéphane Lavertu for sharing bond election data for several states.

[11] Three of these 10 states do not hold bond elections; for the others, we were not able to find systematic records of bond elections.

majority (for bonds with missing amount). These restrictions leave us with 17,533 district-year elections in 10,003 districts and 28 states, enrolling about 70% of all U.S. students for the period 1990-2017.[12] 4,906 districts (49%) do not propose any bond measure during our sample window. Each year, 6% of all districts propose a measure and 75% of these proposals are authorized. The share of proposed bonds is higher (8%) among districts with a lower share of low-SES students and lower (5%) in supermajority districts (see Table 1). Bonds are comparable in terms of interest rates; the standard deviation is 0.07% (Appendix Figure A4).[13]

Classifying bonds into categories. We use text information from election ballots to categorize bond measures into spending categories. Our main approach uses a NLP classification algorithm developed by The Amos Group, a private-sector company offering consulting services for school district capital investments. Their *SchoolBondFinder.com* (SBF) database curates detailed information on thousands of school bonds authorized across the whole country since 2014.[14] The company uses a proprietary algorithm to classify bonds into six spending categories and and up to seven subcategories per category.

We were able to match 4,065 SBF records to bonds in our administrative dataset that have at least some textual information, or about 14% of all our bonds. To categorize bonds in our dataset that were not linked to SBF, we used a supervised learning procedure to predict a bond's SBF categories based on the unstructured text of the ballot. Specifically, we use a neural network with twenty hidden layers on our matched SBF-administrative data to separately predict whether each of the 27 categories is absent or present.[15] We maximize out-of-sample goodness of fit using ten-fold cross-validation. The predictive accuracy of our procedure is typically between 70%-90%.

Using the parameters trained on the matched sample, we then impute all categories for the remaining 86% of our administrative bonds that we were not able to match to SBF. After restricting to bonds in the sample described in the previous paragraph, we have category information for 13,820

[12]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.

[13]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.

[14]Notice that these data are not comprehensive: they cover only passed (not failed) bond elections primarily in the period from 2014 onwards.

[15]In principle, it would have been possible to predict whether a specific bundle of categories is present. However, given the fairly large number of categories (27), this would have required predicting an outcome with more than one hundred million possible values (2^{27}), which is unlikely to produce reliable predictions.

bonds, or 79% of our sample. The most popular category is the construction, renovation, and expansion of classroom space (present in 45% of all bonds), followed by investments in IT and laboratory spaces (or "STEM", 28%), the renovation of infrastructures such as plumbing, roofing, and furnaces (27%), investments to increase buildings' health and safety standards (20%), the construction and renovation of on athletic facilities (17%), and the installation of HVAC systems (12%, Table 1). The majority of all bonds are assigned to more than one category (Appendix Figure 7).

3.3 Student Achievement

Our primary measure of student achievement are test scores in grade 3-8 in mathematics and English Language Arts (ELA) or reading. Because achievement tests vary across states and years, we gather data from multiple sources and use a normalization method developed in Reardon et al. (2017) and Fahle et al. (2021) to construct a panel dataset of district-level test scores over multiple decades. Our first source is the the Stanford Education Data Archive (SEDA Reardon et al., 2021), which begins in 2009. SEDA data are standardized across states and years to match moments from the National Assessment of Educational Progress (NAEP), a national exam administered in grades 4 and 8 to a representative sample of students in each state, roughly biannually. We combine the SEDA data with information from the National Longitudinal School-level State Assessment Score Database (NLSLSASD). NLSLSASD contain school-level test score data by grade, subject, and subgroup for nearly every state, until 2005. Data for most states begins around 2000; data is available as far back as 1994 in some states.[16]

For the years 2005-2008, we supplement with our own original data collection from each state's education department. We were able to collect data for nearly every state for these years via direct downloads from state websites and public data requests.Where applicable, we aggregate data to the district-year-grade-subject level. For some state-years, data are recorded as a count of students meeting proficiency standards. For these years, we follow the procedure used in SEDA and developed in Reardon et al. (2017) and Fahle et al. (2021) to estimate average test scores for each district-year-grade-subject cell using the proficiency count data. To make results comparable across years, we again follow SEDA and standardize scores relative to distribution on the NAEP. We then standardize test scores in district-level (rather than student-level) standard deviations, since for

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

some years we do not have access to data disaggregated below the district level.[17]

3.4 House Prices

We capture changes in the real estate market with a house price index (HPI), constructed using a repeat-sales approach developed by Contat and Larson (2022). The HPI uses data from Fannie Mae or Freddie Mac, the Federal Housing Administration, and county recorder rolls provided by Core-Logic, for a total of 63 million same-unit purchase pairs. The HPI is available for a balanced panel of 63,122 census tracts between 1989-2021 based on 2010 census tract geography. It is normalized to a value of 100 in 1989 for each census tract and grows according to repeat-sales estimates in the tract or nearby tracts. To aggregate the data to school districts, we map census tract centroids to 2010 school district boundaries from the NCES Education Demographic and Geographic Estimates Program (EDGE) and average the house price index for each school district and year. This results in a balanced panel of 7,530 school district for the period 1989-2021.

4 Estimating Heterogeneous Causal Effects of Bond Authorization

In this section we develop a research design to estimate the causal effect of bond authorization on current and future outcomes, allowing for this effect to be dynamic over time and to vary across districts. To build intuition, we first review the use of dynamic RD estimators when treatment effects are homogeneous. We then propose an alternative estimator that allows for heterogeneity in treatment effects.

4.1 Regression Discontinuity with Close Elections: Simplest Case

We begin by considering the case of districts which 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 $D_{jt} = \mathbb{1}(V_{jt} \geq v)$ an indicator for bond authorization. The effect k years after authorization is $\tilde{\beta}_k$.

[17] On average, a district-level standard deviation is smaller than a student-level standard deviation: the impacts we estimate are on average 2-3 times larger in absolute value than what we would estimate with student-level standard deviations (though this scale factor varies by state-grade-subject-year).

We can express the outcome of interest y_{jt} in year t as

$$y_{jt} = \alpha_j + \gamma_t + \tilde{\beta}_k D_{it-k} + u_{jt},$$

where α_j and τ_t are district- and year-specific components and u_{jt} is an error term.

The empirical challenge in estimating $\tilde{\beta}_k$ is that the election outcome may be correlated with other time-varying district characteristics that affect spending, such as the district's unobservable capital needs: $\mathbb{E}(u_{jt}|D_{js}) \neq 0$. To overcome this challenge, CFR use a dynamic RD framework that exploits close elections. The intuition is that, since the probability of authorizing a bond jumps discontinuously at the cutoff v, districts where a proposal barely passes are a good control for districts where a proposal barely fails. If $\mathbb{E}(u_{jt}|V_{js})$ is continuous at the cutoff, it can be approximated by a polynomial of order g with coefficients δ_g. One can then consistently estimate $\tilde{\beta}_k$ for any k via OLS using the following specification:

$$y_{jt} = \alpha_j + \gamma_t + \tilde{\beta}_k D_{jt-k} + P^g(V_{jt-k}, \delta_g) + u_{jt}. \tag{1}$$

where the continuity of $\mathbb{E}(u_{jt}|V_{js})$ ensures that u_{jt} is asymptotically uncorrelated with V_{js}.

4.2 Dealing with Repeated Elections

Reality is more complicated than this simple case because districts can propose and authorize multiple measures over time. In our data, 30% of all districts who propose at least one measure propose more than one bond during our sample window and 22% authorize more than one; the median lag between successful elections is 10 years, and 7 years between any elections (Appendix Figure A10). This makes it difficult to characterize the treatment and control group at a given point in time and to define the relevant treatment for a given district.

CFR extensively discuss this issue and define two different estimators. The first, called intent-to-treat (ITT), is the OLS estimator of $\tilde{\beta}_k$ in equation (1). It captures the effect of authorizing a measure k years ago, leaving both treated and control districts free to propose and pass measures in the future. Hence, this estimator captures the causal effect of bond authorization against the counterfactual of failing to pass a bond in that year, but possibly succeeding in the near future if and when the district tries again.

In contrast, the second estimator, called treatment-on-the-treated (TOT), captures the effect of bond authorization against the counterfactual of never authorizing a bond over the foreseeable future, rather than simply delaying authorization with some probability. It is the OLS estimate of β_k in the following specification:

$$y_{jt} = \alpha_j + \gamma_t + \sum_{k \neq 0} [\beta_k D_{jt-k} + \phi_k M_{it-k} + P^g(V_{jt-k}, \delta_g)] + u_{jt} \tag{2}$$

where M_{it-k} equals one if the district proposed a bond measure k years after authorization. With the inclusion of all leads and lags of M_{it} and $P^g(V_{jt}, \delta_g)$, the coefficients β_k are identified by comparing districts where a bond measure barely passed k years ago to those where the measure barely failed, with a similar history of bond proposals, authorizations, and votes. Including leads and lags of all variables also allows us to examine pre-trends and explore how treatment effects evolve over time.

4.3 Dealing with Heterogeneity in Treatment Effects

As CFR point out, the TOT estimator is only consistent under the assumption of constant treatment effects across elections, because it uses not-yet-treated districts as controls for treated districts.[18] This assumption is not appropriate for our context: Differences in treatment effects across districts are the very focus of our analysis. A recent literature on event studies with staggered treatment, pioneered by Cengiz et al. (2019), has shown how the assumption of constant treatment effects can be relaxed by restricting the set of controls to be "clean" (i.e., never-treated) units, and by stacking treated and control units belonging to the different treatment cohorts (in our case, election years). We adapt this strategy to incorporate the RD design and leverage variation from close elections. We proceed as follows:

1. For each election in year c, we restrict our attention to a relevant time window of analysis $\mathcal{W}_c$.

2. For each district j and election year c, we define a bond history as the set of all measures proposed by j in $\mathcal{W}_c$: $\mathcal{H}_{jc} = \{M_{js}\}_{s \in \mathcal{W}_c}$.

3. We match each district j with an approved bond measure in c with a control group of districts

[18] A number of works including 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.

$\mathcal{C}_{jc}$, which also propose a measure in c but do not authorize it, do not authorize any bond in $\mathcal{W}_c$, *and* share the same bond history as j: $\mathcal{H}_{mc} = \mathcal{H}_{jc}\ \forall m \in \mathcal{C}_{jc}$.

4. We stack observations for treated and control units in the relevant time window, for each election. We then estimate the following equation via OLS using this stacked dataset:

$$y_{jct} = \alpha_{jc} + \gamma_{ct} + \sum_{k\neq 0} \left[\beta_k D_{jt-k} + \phi_k M_{it-k} + P^g(V_{jct-k}, \delta_g)\right] + u_{jct} \tag{3}$$

A few properties of this estimator are worth mentioning. First, exclusively using clean controls and stacking observations from different elections implies that the framework is robust to the existence of heterogeneous treatment effects (Dube et al., 2023). Appropriately defining the control group to be exclusively composed by untreated units is especially important in our context, due to the dynamic and repeated nature of the treatment. Second, matching treated and untreated districts with the same bond history holds fixed any (possibly endogenous) factors that may induce districts to propose a measure in the first place. Lastly, the inclusion of leads and lags of vote margin polynomial implies that the coefficients β_k are identified using variation from close elections. It is worth stressing that we consider all the elections of a given district as separate events and do 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 implicitly assume that the effects of subsequent authorizations are additively separable. Monte Carlo simulations show that OLS estimates of β_k in equation (5.2) are unbiased in the presence of heterogeneous treatment effects and multiple bond authorizations per district; TOT estimates are biased.

4.4 Testing The Validity of The Research Design

Our empirical strategy requires electoral outcomes to be as good as randomly assigned among districts with a close election. We examine the validity of this assumption in two ways. First, we perform a McCrary (2008) test of smoothness of the density function of the running variable around the cutoff. A discontinuity could indicate endogenous sorting around the cutoff, which would violate the RD assumptions. State-specific histograms of the vote margin (the difference between the vote share and the required majority) show discontinuities at zero in Arkansas, Missouri, and

Oklahoma but not in other states (Appendix Figure A7). We therefore exclude these states from our analysis. The pooled density function in the estimation sample of 28 states, enrolling approximately 70% of all U.S. students, is smooth around zero (Figure 1).

Second, we test for the smoothness of pre-election district covariates around the cutoff. Appendix Figure A8 shows means of average household income, the population share of people with at least college degree, the composition of the student body (including the shares of low-SES and white students), enrollment in private schools, total revenues, state revenues, and total expenditures by quantiles of the vote margin. All these variables are smooth around the cutoff. Taken together, these tests support the assumption of quasi-random assignment to treatment among districts with close elections.

5 Average Effects of Bond Authorization

In this section we present the average effects of bond authorization on several outcomes, including capital spending (the first stage), test scores, and house prices. We use the latter to derive conclusions about the efficiency and effectiveness of school capital investments. We also briefly discuss the consequences of bond authorization on inter-district household sorting. Lastly, we compare our estimates with those obtained applying the TOT estimator of CFR.

5.1 First Stage: Effects on Capital Spending

Capital expenditures increase sharply after a bond authorization. Estimates of β_k in equation (5.2), using per pupil capital spending as the dependent variable, indicate that the difference in spending between districts that barely approve and those that barely reject a bond measure in year t is on a flat trend prior to t. It then increases by \$800 per year at $t+2$ and at $t+3$ and returns to pre-election levels 7 years after the election (Figure 2, connected line). Relative to the year of the election, cumulative spending is \$2,500 higher on average 5 years post election in districts that approve a measure, compared with districts that reject it, and it stays at this higher level up to 10 years post-election (Figure 2, solid line).

Bond authorization does not affect current spending (Appendix Figure A11).[19] This is unsur-

[19] Appendix Figure A11 shows estimates of β_k in equation (5.2) using current spending (panel (a)) and instructional spending (panel (b)) as the dependent variable.

prising, as current spending cannot be financed through debt and is usually with a combination of local and state revenues determined by a funding formula. This implies that we can 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 (5.2) as follows to accommodate this feature of the data:

$$y_{jgsct} = \sum_{k=t-n}^{k=t+m} [\beta_k D_{jt-k} + \phi_k M_{it-k} + P^g(V_{jct-k}, \delta_g)] + \alpha_{jc} + \gamma_{gsct} * K_j + u_{jgsct}$$

where y_{jgsct} is the standardized average student test score of district j for all students in grade g, subject j, and year t and referring to election year c. The vector γ_{gsct} contains grade-by-subject-by-cohort-by-year fixed effects, K_j equals to one if district j's capital expenditures in 1995 were above the national median, and everything else in the equation is as before. Interacting γ_{gsct} with K_j helps account for the time-varying impact of differences in capital stock on outcomes (we return to this issue in our heterogeneity analysis in Section 6.3). We cluster standard errors at the district level.

Estimates of β_k indicate that achievement improves in the aftermath of a bond authorization. The difference in test scores between districts that marginally approve and those that marginally reject a bond proposition is flat in the years leading to an election. It starts to increase two years after the election, presumably, after the capital project has been completed, and it reaches a 0.08 standard deviations (sd) higher level 5 years later (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 0.035 sd higher on average 1 to 4 years after an election, 0.075 sd higher 5 to 8 years after, and 0.069 sd higher 9 to 12 years after relative to districts that marginally reject it (Table 3, column 2). The impact of bond passage is slightly higher for ELA (a 0.093 increase 9 to 12 years post election) compared with Math (a 0.045 increase 9 to 12 years post election), Table 3, columns 2 and 3).

Effect of a $1,000 increase in capital spending on test scores: 2SLS In the literature on the impact of school resources has focused on the change in outcomes per dollar of increased spending as a policy-relevant variable (Jackson and Mackevicius, 2023). To transform our reduced-form RDD estimates into a per dollar impact, we use a two-stages least squares model. In the first stage, we use the stacked RDD of equation (5.2) to predict capital spending k_{jct}. In the second stage we express test scores in year t as a function of cumulative spending over an interval of time $[t-a, t-b]$ prior to t, denoted as $\hat{K}_{jct} = \sum_{s=t-a}^{t-b} k_{jcs}$:

$$y_{jgsct} = \rho \hat{K}_{jct} + \sum_{k=t-n}^{k=t+m} \left[\phi_k^{2SLS} M_{it-k} + P^g(V_{jct-k}, \delta_g^{2SLS}) \right] + \alpha_{jc} + \gamma_{gsct} * K_j + u_{jgsct}. \tag{4}$$

In this model, ρ is the per dollar effect of changes in cumulative spending on test scores. We estimate equation (4) via OLS using the predictions from the first-stage stacked RDD as explanatory variables and setting $a = 5$ and $b = 1$. 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 five years increases test scores by 0.011 standard deviations (Table 4). These estimates, though, do not account for the fact that spending is concentrated in the first few years following bond authorization but the life of each project is much longer. Assuming a project depreciation rate of 9% and a project life of 30 years implies that we have to multiply our estimates by 2.8 to obtain the impact of a $1,000 increase in spending. This brings the 2SLS estimate to 0.045 sd.[20]

Taken together, these estimates indicate that increasing spending on capital projects is an effective way to raise achievement on average. 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 is is useful for two reasons. First, house prices measure any benefits of school capital investments for students and communities not captured by test scores. If these benefits are valued by homeowners and homebuyers more than the taxes they pay to finance them, spending

[20] Jackson and Mackevicius (2023) consider 50 years and a depreciation rate of 4.7% and 15 years and a rate of 15.4% for building and non-building investments. We consider an average of these values.

increases should raise house prices (Cellini et al., 2010).

Second, house prices provide a test for efficiency of public spending. Simple models of optimal spending postulate that the amount of a public good 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 leave the community, 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.[21]

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 (2). Estimates of β_k are indistinguishable from zero for $k \geq t$, indicating similar pre-election trends between districts that approve a measure in t and those that reject it. After the election, house prices gradually increase in districts that approve a bond measure, reaching a 7% higher level 9 years post election (Figure 3, panel (c)). 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, these estimates do not account for the fact that several states provide districts with grants to cover part of capital expenditures. Since these grants are not financed via local taxes, they raise the marginal benefit of spending without raising marginal costs. For a test of spending efficiency in the presence of state grants, we estimate house price effects of increases in *local* capital spending by substituting $\hat{K}_{jct}$ in equation (4) with cumulative lagged local spending (defined as the per pupil amount of bonds proposed by district j between $t-5$ and $t-1$) and using the house price index as the dependent variable. These estimates indicate that a $1,000 increase in local spending produces a small and insignificant change in house prices. We can thus conclude that, on average, school capital spending is efficient across the U.S. In Section 6 we show that inefficiencies exist in some contexts.

[21] 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.

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.[22] 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 (5.2) with the shares of White and high-SES students as dependent variables.

We find that the approval of a bond measure is followed only by a small change in the composition of the student body. In districts that approve a bond measure, the share of high-SES students is 2.4% percentage points higher seven years after the election relative to before (a 3 percent change relative to an average share of 0.73, Appendix Figure A12, panel (a)). The share of White students and total enrollment are instead unaffected (Table 3, columns 5 and 6). These small compositional changes cannot explain the increase in test scores: Stacked RDD estimates remain virtually unaltered when we control for the share of students in each socio-demographic groups in each district and year (Appendix Figure A12, panel (b)).

6 Differences In The Impact of Bond Approvals

The results presented so far indicate that the approval of bonds to finance school capital projects 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 (see, for example, Cellini et al., 2010; Martorell et al., 2016; Brunner et al., 2022).

In this section we focus on two types of heterogeneity. The first is *treatment* heterogeneity: Bonds may have profoundly disparate impacts on student learning and the housing market depending on the type of projects they fund. The second is *treatment effect* heterogeneity: An investment on a given capital project may have different impacts depending on the characteristics of the students

[22]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).

enrolled in the school district and on the initial state of school facilities. We now examine each type of heterogeneity in more detail.

6.1 Treatment Heterogeneity: Differences by Spending Categories

Bonds are used to finance different kinds of projects. Some projects involve the expansion of classroom space; others the installment and repair infrastructures such as HVAC, roofs, or plumbing; others the construction or renovation of a school's athletic facilities. These categories of projects could have profoundly different effects on students' learning experiences. They may also entail different amenity components and therefore be valued differently by taxpayers.

To explore this possibility, we now test whether the impact of a bond authorization differs depending on the category of financed projects. We use textual information from the election ballots, which describe the use of the funds, to group bonds in eight categories: (i) the construction, renovation, and expansion of buildings and classroom space (42% of all bonds, Appendix Figure A9, panel (a)); installations and repairs of (ii) HVAC systems (9%) and (iii) other types of infrastructures (including plumbing, furnishing, and roofs, 24%); (iv) improvements to the health and safety standards of a school (such as the removal of asbestos or lead paint and the upgrade of fire and earthquake safety systems, 14%); (v) the acquisition or upgrade of IT equipment and the furnishing of laboratories (which we denote as STEM, 20%); (vi) the construction or renovation of athletic facilities (15%); (vii) land purchases (10%); and (viii) transportation vehicles purchases (including school buses, 15%). 32% of all bonds belong to more than one category (Appendix Figure A9, panel (b)).

We find large differences in effects across categories. In the short run, the approval of bonds that fund HVAC systems produces the largest increase in test scores (0.17 sd, Figure 4, panel (a)). This is consistent with recent evidence on the learning losses caused by heat (Park et al., 2020). Bonds for the renovations of plumbing systems, roofs, and furnaces, STEM equipment, and health and safety improvements also produce sizable increases, equal to 0.13, 0.12, and 0.11 sd respectively. Bonds for the expansion of classroom space increase test scores by 0.07 sd. Instead, bonds for categories such as land purchases, transportation, and athletic facilities produce no detectable effects on test scores.

Notably, the categories that increase test scores are not the same as those that increase house prices. House prices only increase following the construction of athletic facilities (26% increase, Figure 4, panel (a)) and the expansion of classroom space (12%). The approval of bonds belonging to other categories (including HVAC) leaves house prices unchanged. The correlation between test score and house prices estimates is -0.6.

Panel (b) of Figure 4 shows long-run effect estimates. Six to 10 years after an election, test scores are highest after the authorization of bonds that fund STEM equipment, health and safety, and classroom space (with a 0.14, 0.12, and 0.12 sd increase). The categories that most increase house scores prices are again classroom space and athletic facilities (with a 15% and 17% effect on house prices, respectively). The correlation between long-run effects on test scores and house prices is 0.07.

6.2 Treatment Effect Heterogeneity I: Differences by Student Background

If marginal returns to educational investments are concave, investments in school facilities may be particularly beneficial for the learning experience of students from more disadvantaged backgrounds. On average, these students receive much smaller private educational investments (Heckman, 2008) and attend districts with lower total spending (Table 1). Investments in school facilities have also been shown to reduce school absenteeism, a phenomenon disproportionately affecting students from more disadvantaged backgrounds (Baron et al., 2022). To investigate differences in the impact of bond authorization by student backgrounds, we estimate equation (5.2) separately for groups of districts serving different student populations.

Socio-economic status We begin by grouping districts according to their share of students eligible for a free or reduced-price lunch, 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 US districts in 1995 ("high SES" and "low-SES," respectively).

We find large differences in the impact of spending between these two groups. In low-SES districts, the effect of a bond authorization on student learning is positive and significant: 7 years after a successful election, test scores are 0.13 sd higher (Figure 5, left panel (a)). In high-SES districts, instead, this effect is indistinguishable from zero. Test scores are on flat trends in the years before the election both in high-SES and low-SES districts, corroborating the RD assumptions.

In low-SES districts, bond authorization also produces sizeable increases in house prices. Seven years after an election, the house price index is 12X% higher in districts that approve a bond (Figure 5, left panel (b)). In high-SES districts, the effect of bond authorization on house prices is zero.

A possible explanation for these different impacts is that low-SES districts implement bigger projects. The data, though, do not support this explanation. Stacked RDD estimates of the effect of bond authorization on capital spending indicate that low-SES and high-SES districts increase spending by similar amounts ($3,800 and $3,300 5 years after an election, respectively; Figure 5, left panel (c)). In line with this finding, 2SLS test score estimates confirm that a $1,000 increase in cumulative spending increases test scores by 0.09 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 (4.4% compared with 0.3% in high-SES districts), although imprecisely estimated (Table 4, panel (a), columns 3 and 2, respectively).

Differences in impacts may also arise if low-SES and high-SES districts approve bonds with different compositions. Our data show evidence of this. Low-SES districts are more likely to approve bonds to fund items that raise test scores, such as HVAC (with 8% of all bonds in this category, compared with 5% for high-SES), safety and health (14% compared with 9% for high-SES), and STEM (16% compared with 14%). They are also more likely to invest on classroom space, which raises house prices (29%, compared with 20% for high-SES, Figure 7).

Even within categories, though, low-SES districts see larger effects of bond authorization. This is evident from Figure 8, which replicates panel (a) of Figure 4 separately for low-SES and high-SES districts.[23] For example, authorization of a bond to fund HVAC systems increases test scores by 0.27 sd 1-5 years after the election in low-SES districts, but it does not change them in high-SES districts (with an estimate of -0.03 sd in panel (a), indistinguishable from zero). Bonds that fund other infrastructure and STEM equipment also have larger impacts in low-SES districts (equal to 0.25 sd and 0.17sd, respectively) compared with high-SES districts (-0.05 and 0.05, both indistinguishable from zero). Low-SES districts also see larger increases in house prices following the authorization of bonds that fund athletic facilities (27% increase, compared with 8% for high-SES districts, panel (b)), land purchases (22% increase, compared with 3%) classroom space (20% increase, compared

[23]In Figure 8 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). Appendix Figure 8 shows average effects by category and SES 6-10 years post-election.

with 7%), and STEM equipment (17% increase, compared with 5%). These results indicate that the larger mean effects of bond authorization on low-SES districts is not uniquely driven by bond composition, but also by low-SES students benefiting more from a given investment.

Share of minority students To explore alternative measures of student background, we also group districts according to their share of Black and Hispanic ("minority") students in 1995. "High-minority" districts (with a share of minority students in the top tercile) experience a 0.12 sd increase in test scores 7 years after an election (Figure 5, right panel (a)) and a 12% increase in house prices (right panel (b)). "Low-minority" districts (with a share in the bottom tercile) experience much smaller increases (0.04 sd in test scores and 3% in house prices 7 years post-election). Spending increases significantly more in high-minority districts ($3,800 five years after a successful election) compared with low-minority districts ($1,200, Figure 5, right panel (c)). Yet, 2SLS estimates indicate that the impact of a $1,000 increase on both test scores and house prices is larger in high-minority districts (Table 4, columns 4 and 5, both panels). Differences in impacts are likely due to bond composition. High-minority districts approve more bonds with items such as HVAC and safety and health, which increase test scores, and classroom space, which increases house prices (Figure 7). However, they also experience larger returns within categories, both on test scores and house prices (Appendix Figure A14).

6.3 Treatment Effect Heterogeneity II: Differences By Existing Stock of Capital

Districts enrolling students from more disadvantaged backgrounds may have facilities in worse conditions. First, these districts spend less on on average (Table 1), in part because they serve communities with a lower tax base. Second, they tend to be located in urban areas, which typically have older buildings. The approval of a bond may produce very different effects depending on the initial conditions of school facilities. For example, the installation of an HVAC system in a school that does not have one may improve learning much more than the installation of the same system in a school that already has 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 of the relationship between bond impacts and student characteristics. While so far we have controlled for a district's position in the 1995 national distribution of capital spending in all our specifications, this variable may only inadequately capture the state of facilities at a given point in time.

To investigate differences in impact by the extisting state of school facilities, the ideal dataset would contain detailed information on school buildings. To our knowledge, this type of information is not systematically kept and is thus unavailable. To overcome this obstacle, we construct a measure of a district's stock of capital at a given point in time using historical expenditure data from the Census of Governments for the years 1967-2017. The Census contains data on local governments' spending, recorded every five years. We linearly interpolate spending values in intercensal years and aggregate them over 30 years using a 5% depreciation rate. Appendix Figure A15 shows that a district's capital stock increases following an election.[24] To explore differences in impact by the level of existing capital stock, we estimate equation (5.2) separately for districts in the top and bottom terciles of the nationwide distribution of per pupil capital stock in the year prior to the election ("low capital stock" and "high capital stock," respectively).

We find that the impact of bond authorization on test scores is larger in low-capital stock districts, and equal to 0.14 sd 8 years post-election (Figure 6, panel (a)). High-capital stock districts instead experience no increase in test scores. However, house prices do not mirror the path of test scores: They increase significantly in both groups of districts, by about 5% 7 years after an election. These findings provide suggestive evidence that high-capital stock districts value spending on school capital projects above and beyond the increase in test scores.

Differences in impacts on test scores are unlikely to be driven by differences in amounts spent. Following an election, capital spending increases by about $2,000 in both groups of districts. 2SLS estimates are imprecise, so they should be interpreted with caution; yet, they suggest that the impact of a $1,000 increase in spending has slightly larger effects in low-capital stock districts on both test scores and house prices (Table 4, columns 6 and 7).

Instead, differences in impacts across districts with different capital stocks are likely driven by bond composition. Districts with a low stock are more likely to approve bonds in spending categories that increase test scores, such as HVAC (7%, compared to 5% for high-capital stock districts) and infrastructures (19%, compared to 11%; Figure 7, panel (a)). Instead, low-stock and high-stock districts are equally likely to approve bonds that fund athletic facilities and classroom space, the

[24] Appendix Figure A15 shows estimates of the parameters β in equation (5.2), shown using a district's capital stock (calculated with data from the Census of Governments) as the dependent variable.

two categories that increase house prices (Figure 7, panel (b)).

6.4 Summary

Taken together, the results from this section indicate that the impact of bond authorization for school capital spending produces very different effects on students and communities depending on the types of financed projects. Impacts also differ across districts that serve different populations of students, in part due to the projects that high-SES and low-SES fund. These results highlight how average impacts may mask a great deal of heterogeneity and caution against drawing broad conclusions by looking at small samples of districts, students, and bonds.

7 The Role of Funding Rules

We now study whether bond authorization produces different effects depending on the institutional rules that determine how districts can raise funds for capital projects. These rules determine how difficult it is for a district to implement these projects. This, interacted with the characteristics of a community, can affect the size and composition of authorized bonds and, in turn, the bonds' impacts on students and communities. We focus on two types of rules: the required majority for bond referenda and the presence of a limit to the amount of outstanding debt.

7.1 Required Electoral Majority

Bond authorization produces larger increases in test scores in states that require a supermajority, compared to states that require a simple majority, to approve a bond. Estimates of equation (5.2) for districts in supermajority states indicate that test scores increase by 0.14 sd and 0.12 sd 5 to 8 and 9 to 12 years after a successful election, respectively (Table 5, panel (a), column 2). Test scores also increase in simple majority states, but by a smaller amount (0.05 and 0.06 sd 5 to 8 and 9 to 12 years after a successful election, respectively; Table 5, panel (a), column 1).

In contrast, supermajority states do not see any changes in house prices after a successful election; estimates of β in equation (5.2) are negative but insignificant (Table 5, panel (b), column 2). Simple-majority states instead experience an increase in house prices, equal to 4% and 7% 5 to 8 years and 9 to 12 years after a successful election (Table 5, panel (b), column 1).

These differences in impacts can be explained by differences in bond composition between the two groups of districts. Supermajority states are more likely to pass bonds that raise test scores, such as those funding HVAC (11%, compared to 4% for simple-majority states), other infrastructures (17%, compared to 31%), safety and health (18%, compared to 7%), and STEM (19%, compared to 12%; Figure 7). Simple-majority states are instead more likely to pass bonds that raise house prices, such as those funding athletic facilities (11%, compared with 5% for supermajority states).

Supermajority states tend to approve larger bonds compared with simple-majority states. Spending increases by about $500 per year 1 to 4 years after a successful election in simple-majority states, and then returns to pre-election levels from 5 years onwards (Table 5, panel (c), column 1). Spending increases significantly more in supermajority states, by $760 per year 1 to 4 years and by $580 per year 5 to 8 years after a successful election. These spending differences, though, are unlikely to explain the larger test score impacts in supermajority districts. 2SLS estimates, while imprecise, suggest that the same $1,000 increase in cumulative spending has a larger impact in supermajority states (10 sd) relative to simple-majority ones (4 sd, Table 6, panel (a), columns 1 and 2). 2SLS estimates on house prices are small and indistinguishable from zero (Table 6, panel (b), columns 1 and 2), indicating that capital spending is provided at an efficient level in both groups of districts.

Overall, these results indicate that differences in the required electoral majority to pass a bond are related to differences in bond size, composition, and impacts. A larger majority requirement should make it more difficult for districts to gather enough consensus to approve a bond. In equilibrium, this could lead districts that face this requirement to propose (and pass) bonds with smaller amounts and/or larger amenity values, in order to please voters. Alternatively, it may lead them to prioritize projects that are useful for students and ultimately only pass those that are truly essential. Our results seem to support the latter scenario: Districts in states that require a supermajority pass larger bonds focused on items that increase test scores, with positive effects on students.

7.2 Debt Limits

Bond authorization may also produce different effects depending on whether districts face limits to the amount of outstanding debt. These limits may curtail the ability of proposing a bond, with implications for the size and composition of approved bonds and, in turn, their effects on test scores and house prices.

Our data indicate that bond approval produces similar effects on test scores in districts with and without a debt limit. In the four years following a successful election, test scores increase by 0.025 sd in districts with no limit and 0.04 sd in districts with a limit. Test scores continue to increase, by 0.076 and 0.049 sd in districts with no limit and by 0.074 and 0.076 in districts with a limit 5 to 8 and 9 to 12 years following an election, respectively (Table 5, panel (a), columns 3 and 4).

Instead, the impact of bond approval on house prices differs across the two groups of districts. House prices increase by 5% and 8% in districts with no limit, but only by 4% and 4% in districts with a limit 5 to 8 and 9 to 12 years post election, respectively (Table 5, panel (b), columns 3 and 4). This discrepancy can be explained in part by differences in bond composition. Districts with no debt limit pass more bonds funding items that increase house prices, such as athletic facilities (13%, compared to 8% for districts with a limit) and classroom space (48%, compared to 19% for districts with a limit). It can also be explained by differences in the size of the spending increase. In the four years following a successful election, spending increases by more than $600 per year in districts with no limit, and by $420 per year in districts with a limit. 2SLS estimates indicate a larger impact of a $1,000 spending increase on test scores in districts with no debt limit (Table 6, panel (a), columns 3 and 4), but a larger increase in house prices in those with a debt limit (Table 6, panel (b)).

In sum, it appears that the presence of a debt limit reduces districts' spending on capital projects. However, this reduction occurs primarily on projects that predominantly increase house prices (rather than test scores). Instead, the presence of a debt limit appears to increase the dollar returns of capital investments in terms of test scores and does not impact the efficiency of spending. This suggests that school districts are able to work their way around this limit, ensuring that the projects essential for student learning take place anyway.

8 Conclusion

This paper has investigated 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 using a stacked DRD estimator that allows for dynamic and heterogeneous treatment effects, we have shown that the approval of a bond increases test scores by 0.08 sd and house prices by 7% 5-8 years after an election in the average U.S. district. Taken at face value, these estimates indicate that investing on school facilities is beneficial from students and valued by the community more than the required increased in local taxes. Using 2SLS, we have also shown 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, 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 taxpayers without school-age children. School investments that carry an amenity component and that are more visible, such as the construction or renovation of athletic facilities and the expansion of classroom space, produce instead significant increases in house prices, even if they do not have 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.

We close our analysis by examining the role of funding rules – and specifically the presence of a supermajority requirement and a debt limit – in shaping the effects of bond authorization, via bond size and composition. Even if these requirements undermine districts' ability to raise funds for capital projects, districts that face them still manage to authorize bonds in categories with the largest test score effects. Our analysis, though, does not capture the interplay between funding rules and the composition of a district's population. Considering them together is crucial to understand how changes in funding rules can impact the size and composition of authorized bonds in equilibrium, and how these would impact students and taxpayers. This question, outside the scope of the present paper, is addressed in a companion work.

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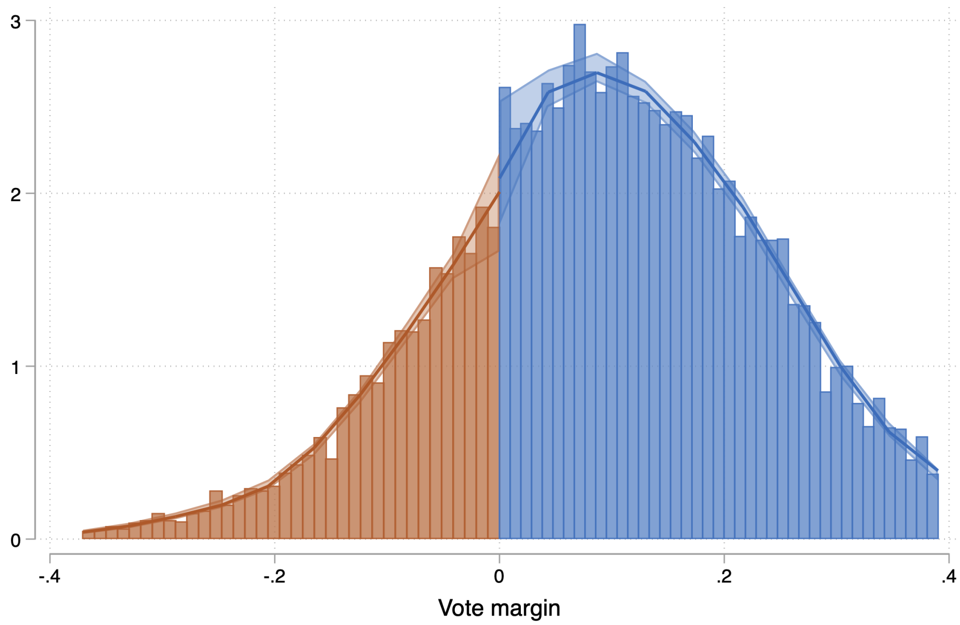
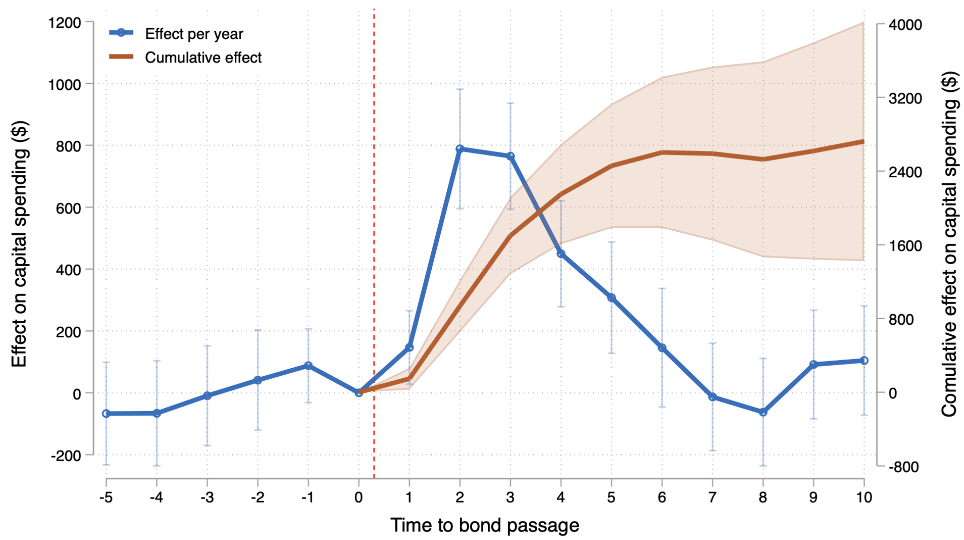
Figure 1: Smoothness of The Density Function of The Vote Margin



Notes: Histogram of the vote margin around the cutoff of zero. The vote margin is 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. The test fails to reject the null hypothesis of no discontinuity, with a p-value of 0.22. The sample includes AZ, CA, CO, CT, DE, FL, GA, ID, IN, IA, LA, MD, MI, MN, MS, NE, NV, NY, ND, OH, OR, PA, RD, TX, VA, WA, WV, WI.

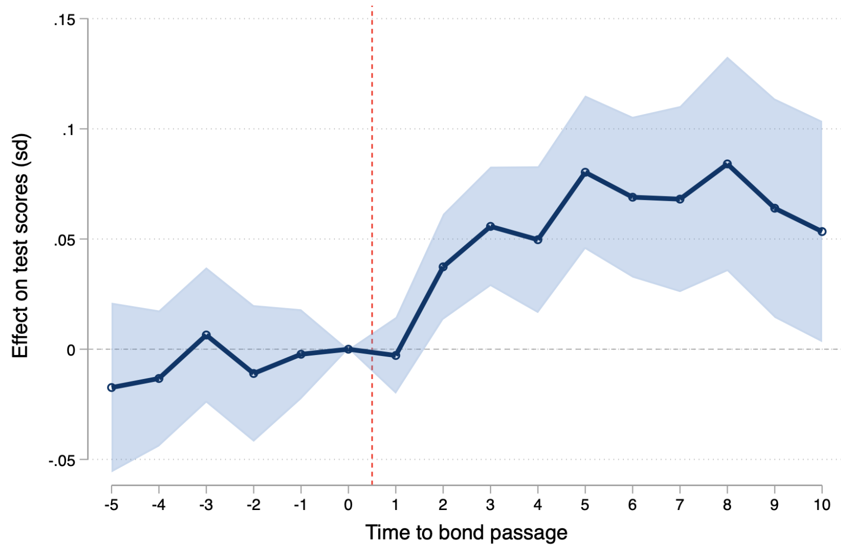
Figure 2: Mean Effects of Bond Passage on Capital Spending



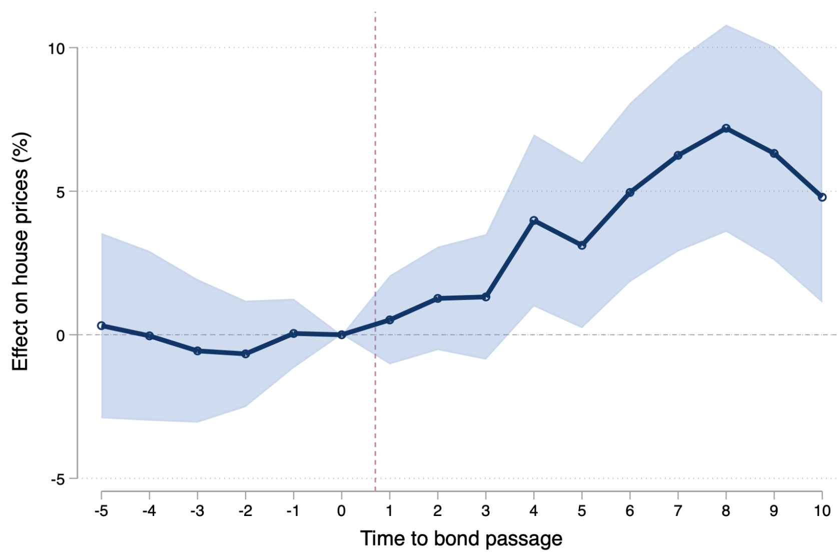
Notes: Estimates and confidence intervals of the parameters β_k in equation (5.2), obtained using capital spending per pupil as the dependent variable. The orange line shows cumulative effects, calculated as the running sum of coefficients since time 0. Estimates are obtained using state-by-year effects interacted with an indicator for above-median 1995 capital spending, and weighing observations by district enrollment. Standard errors are clustered at the district level.

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

(a) Test scores



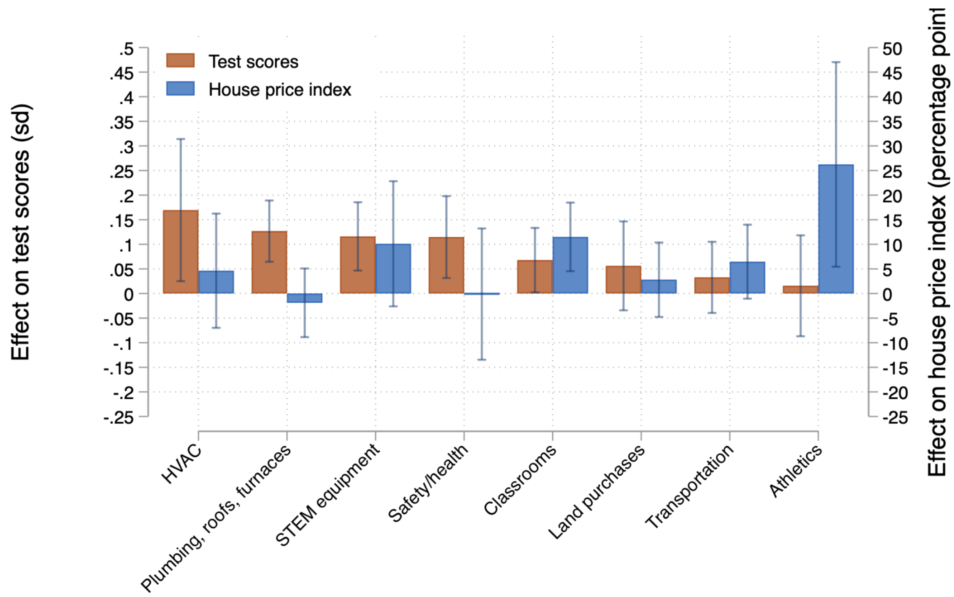
(b) House prices



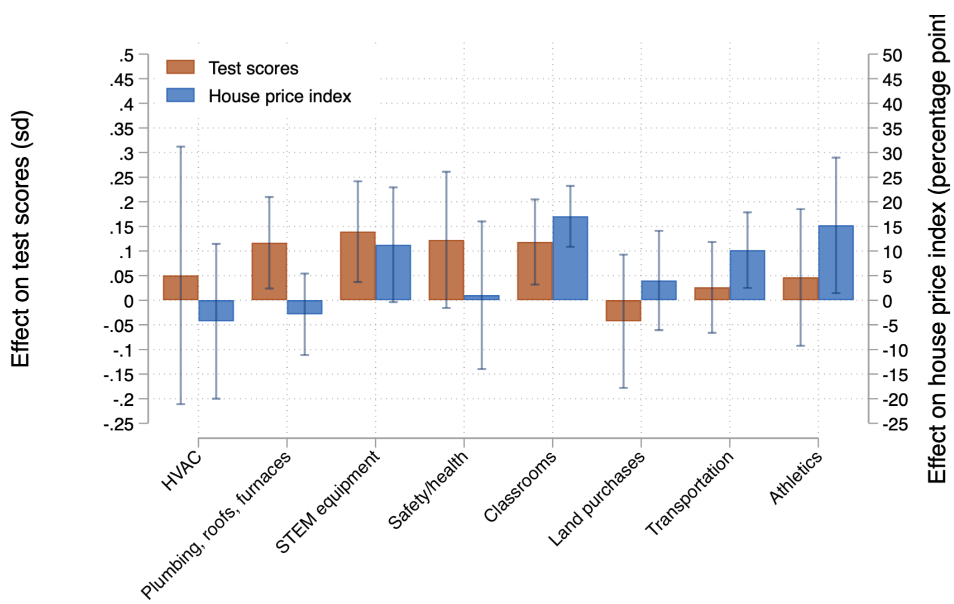
Notes: Estimates and confidence intervals of the parameters β_K in equation (2), obtained using pooled test scores (panel a) and house price index (panel b) as the dependent variable. Test score estimates are obtained pooling data across subjects and grades, using state-by-year-by-subject-by-grade effects interacted with an indicator for above-median 1995 capital spending, and weighing observations by the number of test takers. House price estimates are obtained using state-by-year effects and weighing observations by district enrollment. Standard errors are clustered at the district level.

Figure 4: Effects of Passing a Bond, By Spending Category

(a) Short-run effects: 1-5 years post-election



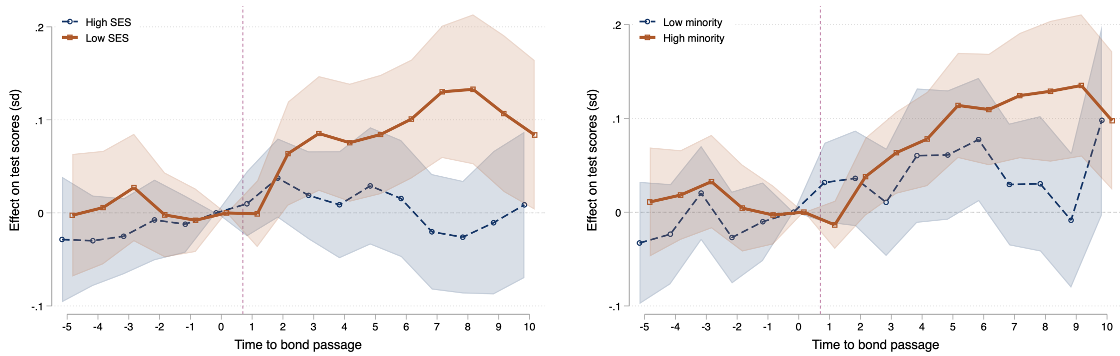
(b) Long-run effects: 6-10 years post-election



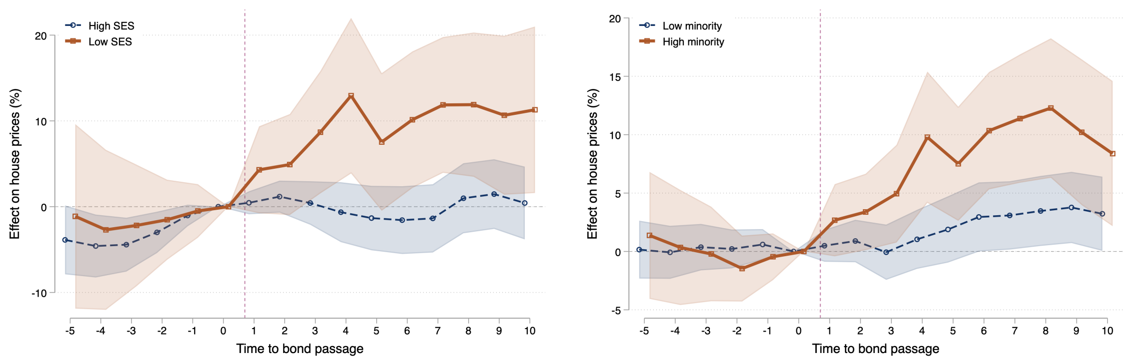
Note: Point estimates and confidence intervals of a linear combination of the parameters β in equation (5.2), obtained stacking districts by category of bond and separately for each category. Panel (a) shows estimates 1-5 years post-election; panel (b) shows estimates 6-10 years post-election. The orange series is estimated using test scores as the dependent variable, pooled across subjects and grades, using state-by-year-by-subject-by-grade effects interacted with an indicator for above-median 1995 capital spending, and weighing observations by the number of test takers. The blue series is estimated using the house price index as the dependent variable, using state-by-year effects and weighing observations by district enrollment. Confidence intervals are calculated using standard errors clustered at the district level.

Figure 5: Effects of Bond Passage By Student Demographics

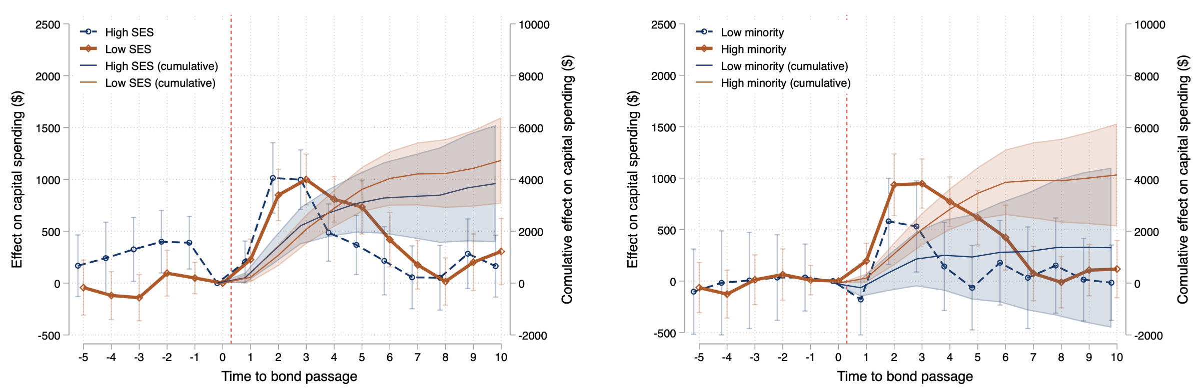
(a) Test scores



(b) House prices



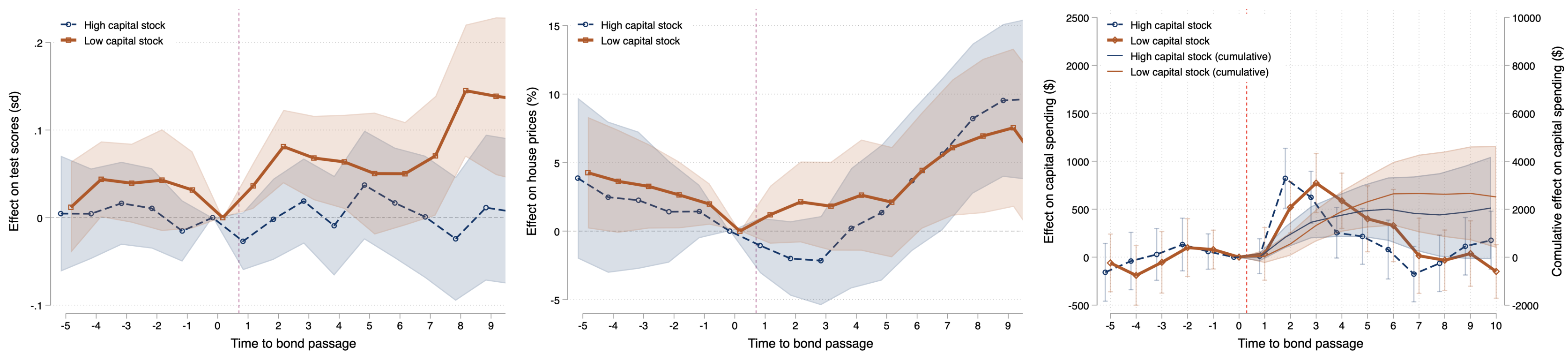
(c) Capital spending



Note: Estimates and confidence intervals of the parameters β in equation (5.2), 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 state-by-year-by-subject-by-grade effects interacted with an indicator for above-median 1995 capital spending, 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.

Figure 6: Effects of Bond Passage By Initial Capital Stock

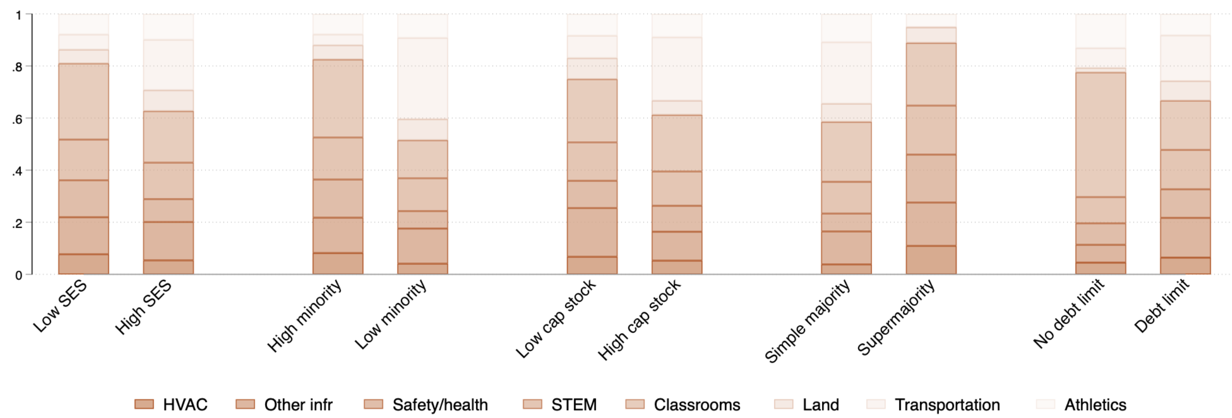
(a) Test scores (b) House prices (c) Capital spending



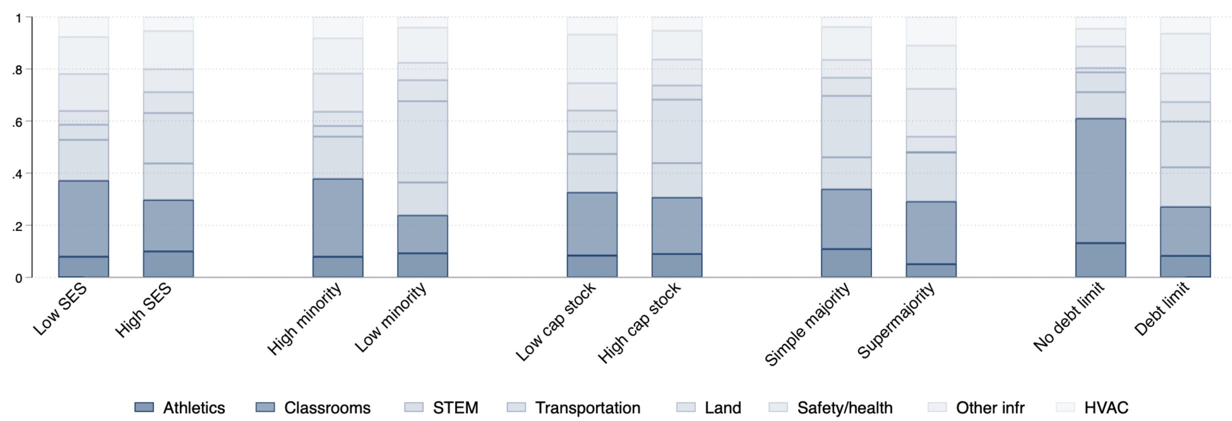
Note: Estimates and confidence intervals of the parameters β in equation (5.2), obtained using test scores (panel a), the house price index (panel b), and capital spending per pupil (panel c) as the dependent variable. Estimates are shown separately for districts in the top and bottom terciles of the distribution of capital stock in the year prior to the election ("Low capital stock" and "High capital stock," respectively). 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 state-by-year-by-subject-by-grade effects interacted with an indicator for above-median 1995 capital spending, 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.

Figure 7: Bond Composition Across Groups of Districts

(a) Categories sorted by effect on test scores



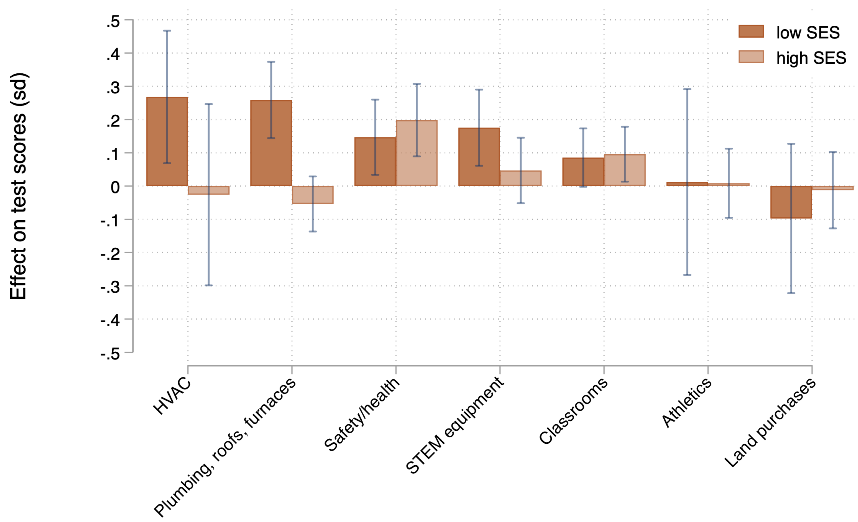
(b) Categories sorted by effect on the house price index



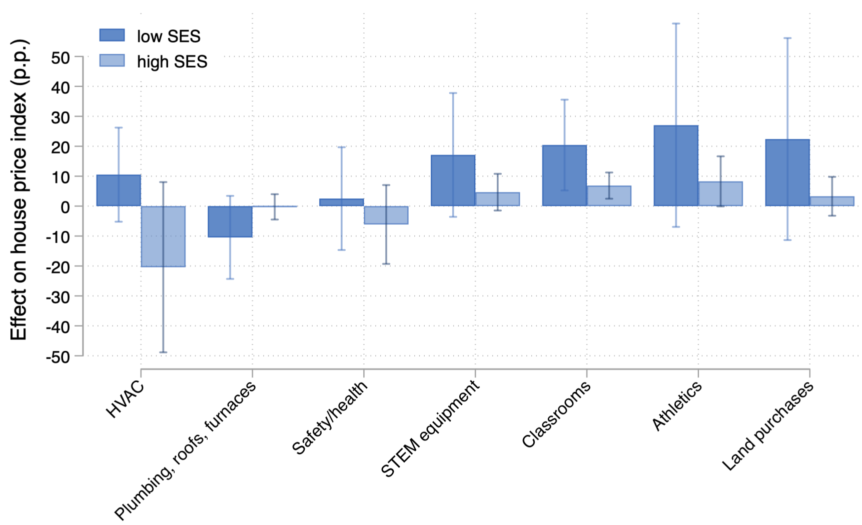
Note: Share of bonds by category and district group. Each bar refers to the group of districts labeled on the horizontal axis. Each bar portion refers to the share of all bonds in a given spending category. In the top panel, categories are ranked from the bottom to the top according to their test score impact in years 1-5. In the bottom panel, categories are ranked from the bottom to the top according to their house price index impact in years 1-5.

Figure 8: Effects of Passing a Bond, By Spending Category and Share low-SES Students

(a) Effects on test scores, 1-5 years post-election



(b) Effects on house price index, 1-5 years post-election



Note: Point estimates and confidence intervals of a linear combination of the parameters β in equation (5.2), obtained stacking districts by category of bond and separately (i) for each category, and (b) by student demographics, captured by the tercile of the share of disadvantaged students ("low-SES" denotes the top tercile and "high-SES" denotes the bottom tercile). Panel (a) shows estimates on test scores 1-5 years post-election; panel (b) shows estimates on the house price index 1-5 years post-election. Estimates in panel (a) are estimated using test scores as the dependent variable, pooled across subjects and grades, using state-by-year-by-subject-by-grade effects interacted with an indicator for above-median 1995 capital spending, and weighing observations by the number of test takers. Estimates in panel (b) are estimated using the house price index as the dependent variable, using state-by-year effects and weighing observations by district enrollment. Confidence intervals are calculated using standard errors clustered at the district level.

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

				Spending rules			
		% low-SES students		Majority requirement		Debt limit	
	Full sample	≤33 pct	>66 pct	50%	>50%	No	Yes
Expenditure per pupil ($)							
Capital	1326.7	1367.4	1280.7	1337.2	1283.5	1367.3	1313.0
	(2913.0)	(2529.9)	(3127.6)	(2979.2)	(2623.9)	(2748.8)	(2966.2)
Current	7064.1	7261.8	6707.5	7236.0	6359.5	6754.8	7168.3
	(3963.8)	(2885.1)	(2802.0)	(4138.0)	(3050.8)	(2026.0)	(4424.7)
Spending rules							
Share w/supermajority	0.20	0.16	0.27			0	0.26
	(0.40)	(0.37)	(0.44)			(0)	(0.44)
Voting requirement	0.52	0.52	0.53	0.50	0.60	0.50	0.53
	(0.045)	(0.039)	(0.050)	(0)	(0.046)	(0)	(0.050)
Debt limit (share prop. value)	0.094	0.089	0.091	0.11	0.042	.	0.094
	(0.058)	(0.046)	(0.069)	(0.055)	(0.025)	(.)	(0.058)
Bonds							
Share proposing a bond/year	0.063	0.081	0.063	0.066	0.051	0.058	0.064
	(0.24)	(0.27)	(0.24)	(0.25)	(0.22)	(0.23)	(0.25)
Share approved	0.75	0.73	0.77	0.75	0.73	0.80	0.73
	(0.43)	(0.45)	(0.42)	(0.43)	(0.45)	(0.40)	(0.44)
Vote margin above threshold	0.099	0.076	0.12	0.11	0.054	0.15	0.084
	(0.16)	(0.13)	(0.17)	(0.16)	(0.12)	(0.19)	(0.14)
Size p.p. proposed ($1,000)	7.97	7.85	7.84	7.82	8.70	11.3	7.40
	(8.27)	(8.00)	(8.07)	(8.32)	(7.98)	(9.75)	(7.85)
Categories, approved bonds							
Classrooms	0.45	0.37	0.63	0.36	0.86	0.88	0.37
	(0.50)	(0.48)	(0.48)	(0.48)	(0.35)	(0.33)	(0.48)
STEM equipment	0.28	0.26	0.34	0.19	0.67	0.18	0.29
	(0.45)	(0.44)	(0.47)	(0.39)	(0.47)	(0.39)	(0.46)
HVAC	0.12	0.10	0.17	0.061	0.39	0.083	0.13
	(0.32)	(0.30)	(0.37)	(0.24)	(0.49)	(0.28)	(0.33)
Other infrastructure	0.27	0.28	0.31	0.20	0.59	0.13	0.30
	(0.44)	(0.45)	(0.46)	(0.40)	(0.49)	(0.33)	(0.46)
Safety/health	0.20	0.16	0.30	0.11	0.65	0.15	0.21
	(0.40)	(0.37)	(0.46)	(0.31)	(0.48)	(0.36)	(0.41)
Athletic facilities	0.17	0.19	0.17	0.17	0.18	0.24	0.16
	(0.38)	(0.39)	(0.38)	(0.38)	(0.38)	(0.43)	(0.37)
Other categories	0.063	0.12	0.015	0.071	0.017	0.0072	0.081
	(0.24)	(0.32)	(0.12)	(0.26)	(0.13)	(0.085)	(0.27)
Demographics and outcomes							
Share low-SES	0.38	0.20	0.59	0.38	0.41	0.43	0.37
	(0.22)	(0.13)	(0.18)	(0.22)	(0.23)	(0.22)	(0.22)
Share Black/Hispanic	0.21	0.083	0.44	0.20	0.29	0.27	0.19
	(0.26)	(0.12)	(0.29)	(0.25)	(0.28)	(0.27)	(0.25)
ELA test scores	-0.079	0.42	-0.64	-0.055	-0.21	-0.057	-0.089
	(0.87)	(0.73)	(0.79)	(0.86)	(0.93)	(0.85)	(0.88)
Math test scores	-0.12	0.36	-0.63	-0.091	-0.25	-0.068	-0.14
	(0.87)	(0.75)	(0.81)	(0.85)	(0.94)	(0.82)	(0.89)
House price index (1989 = 100)	169.1	170.3	174.9	165.3	187.3	156.4	173.3
	(57.7)	(53.5)	(64.5)	(53.2)	(72.6)	(47.9)	(60.0)
Number of districts	10,003	2,580	2,500	7,993	2,010	5,788	5,072
Number of states	28	24	26	24	4	17	16

Note: Means and standard deviations of variables of interest.

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

Type of expenditure:	Capital	Current	Other non-instr services
Avg. effect over:	(1)	(2)	(3)
1-5 years	537***	33*	2
	(69)	(18)	(2)
6-10 years	94	20	12**
	(79)	(26)	(4)
11-15 years	37	5	13**
	(75)	(29)	(6)
District FE	X	X	X
Year-State FE	X	X	X
Adj. R^2	0.295	0.978	0.850
N	119,524	119,524	120,854

Note: Estimates and standard errors of linear combinations of the parameters β_k in equation (5.2). The dependent variables are capital spending (column 1), current spending (column 2), and spending on non-instructional services (column 3), all measured on a per pupil basis. All columns control for district and state-by-year effects; the latter are also interacted for an indicator for capital spending above the median in 1995. Standard errors in parentheses are clustered at the district level. $*$ = 0.1; $**$ = 0.05; $***$ = 0.01.

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

	Test scores			House price	Enrollment			Test
	Pooled	Math	ELA	index	Total (1,000)	Share White	Share high SES	scores
Avg. effect over:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1-4 years	0.035**	0.028*	0.042***	1.771	-0.454	0.002	0.007**	0.029***
	(0.013)	(0.016)	(0.013)	(1.173)	(0.281)	(0.002)	(0.003)	(0.008)
5-8 years	0.075***	0.064**	0.088***	5.378***	-3.433***	0.004	0.014***	0.054***
	(0.022)	(0.025)	(0.023)	(1.844)	(1.071)	(0.003)	(0.005)	(0.014)
9-12 years	0.069**	0.045	0.093***	5.884**	-2.729**	0.006	0.018**	0.065***
	(0.029)	(0.032)	(0.031)	(2.098)	(1.177)	(0.004)	(0.008)	(0.017)
District FE	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	
Enroll. shares								X
Adj. R^2	0.880	0.870	0.902	0.939	0.999	0.991	0.940	0.882
N	1,028,305	497,874	530,409	79,582	220,392	220,021	214,117	1,007,045

Note: Estimates and standard errors of linear combinations of the parameters β_{τ}^{TOT} in equation (2). 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 (column 4); total enrollment (column 5); and the share of enrolled students who are white (column 6) and non-economically disadvantaged (column 7). All columns control for district and state-by-year effects, interacted for an indicator for capital spending above the median in 1995. Columns 1 and 8 also control for state-by-year-by-grade-by-subject-by-above median 1995 capital spending, and columns 2-3 control for state-by-year-by-grade-by-above median 1995 capital spending. Standard errors in parentheses are clustered at the district level. $*$ = 0.1; $**$ = 0.05; $***$ = 0.01.

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

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)	0.016**	-0.002	0.031***	0.015	0.028***	0.009	0.011
	(0.006)	(0.008)	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)
w/depreciation	.0448	-.0056	.0868	.042	.0784	.0252	.0308
District FE	X	X	X	X	X	X	X
Yr-St-Gr-Subj FE	X	X	X	X	X	X	X
N	1,028,305	313,054	305,273	242,269	335,525	338,459	215,335
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)	0.251	0.068	0.809	0.221	0.573*	0.053	0.074
	(0.162)	(0.165)	(0.511)	(0.139)	(0.311)	(0.206)	(0.238)
w/discounting	0.702	0.191	2.265	0.619	1.603	0.148	0.207
District FE	X	X	X	X	X	X	X
Yr-St FE	X	X	X	X	X	X	X
N	79,582	33,172	16,091	20,999	24,580	29,435	13,291

Note: 2SLS estimates and standard errors of the parameter ρ in equation (4). The dependent variable is a standardized measure of test scores. Column 1 is estimated on the full sample of districts; columns 2 and 3 on the subsamples of districts with a state share of capital spending in the bottom and top terciles, respectively; columns 4 and 5 on the subsamples of districts with a share of economically disadvantaged students in the bottom and top terciles, respectively; and columns 6 and 7 on the subsamples of districts with pre-election existing capital stock in the bottom and top terciles, respectively. In panel (a), all columns control for district and state-by-year-by-grade-by-subject-by-above median 1995 capital spending fixed effects and observations are weighted by the number of test takers. In panel (b), all columns control for district and state-by-year-by-above median 1995 capital spending fixed effects and observations are weighted by district enrollment. Standard errors in parentheses are clustered at the district level. $*$ = 0.1; $**$ = 0.05; $***$ = 0.01.

Table 5: Effects of Bond Passage on Student Achievement, House Prices, and Capital Spending, By Funding Rules

Panel (a): Test scores	Majority		Debt limit	
	Simple	Supermajority	No	Yes
	(1)	(2)	(3)	(4)
1-4 years	0.022	0.088**	0.025	0.040**
	(0.014)	(0.031)	(0.020)	(0.016)
5-8 years	0.048*	0.140**	0.076**	0.074**
	(0.023)	(0.053)	(0.033)	(0.025)
9-12 years	0.057	0.118**	0.049	0.076*
	(0.033)	(0.054)	(0.047)	(0.038)
District FE	X	X	X	X
Yr-St-Gr-Subj FE	X	X	X	X
Adj. R^2	0.849	0.920	0.845	0.891
N	882,884	145,421	320,102	708,203

Panel (b): House prices	Majority		Debt limit	
	Simple	Supermajority	No	Yes
	(1)	(2)	(3)	(4)
1-4 years	1.310	-1.055	2.723	0.512
	(1.192)	(4.066)	(1.945)	(1.190)
5-8 years	4.319**	-0.226	4.987	3.801*
	(2.011)	(5.320)	(3.443)	(2.174)
9-12 years	6.671***	-5.786	8.230*	4.368*
	(2.317)	(6.355)	(4.533)	(2.181)
District FE	X	X	X	X
Yr-St-Gr-Subj FE	X	X	X	X
Adj. R^2	0.944	0.930	0.931	0.943
N	65,878	13,704	15,990	63,592

Panel (c): Capital spending	Majority		Debt limit	
	Simple	Supermajority	No	Yes
	(1)	(2)	(3)	(4))
1-4 years	494.169***	759.031***	623.106***	422.505***
	(81.480)	(106.777)	(127.952)	(82.858)
5-8 years	-6.713	582.484***	-105.306	119.020
	(92.849)	(128.369)	(163.596)	(87.753)
9-12 years	128.479	-101.179	136.237	-52.262
	(83.262)	(145.923)	(143.965)	(83.840)
District FE	X	X	X	X
Yr-St-Gr-Subj FE	X	X	X	X
Adj. R^2	0.266	0.435	0.359	
N	101,401	18,123	32,510	87,176

Note: Estimates and standard errors of linear combinations of the parameters β_{τ}^{TOT} in equation (2). Each column refers to a subsample of districts, defined according to the corresponding funding rule. The dependent variables are pooled test scores (panel (a)), the house price index (panel (b)), and capital spending per pupil (panel (c)). All panels control for district fixed effects. Panel (a) controls for state-by-year-by-grade-by-subject-by-above median 1995 capital spending, and panels (b) and (c) control for state-by-year effects, interacted for an indicator for capital spending above the median in 1995. Standard errors in parentheses are clustered at the district level. $*$ = 0.1; $**$ = 0.05; $***$ = 0.01.

Table 6: 2SLS: Effects of Increases in Cumulative Spending on Capital on Test Scores and House Prices

Panel (a): Test scores	Majority		Debt limit	
Sample:	Simple	Supermajority	No	Yes
	(1)	(2)	(3)	(4)
Cap spending ($1,000)	0.008	0.019	0.018*	0.014
	(0.007)	(0.012)	(0.011)	()
w/depreciation	.0224	.0532	.0504	.0392
District FE	X	X	X	X
Yr-St-Gr-Subj FE	X	X	X	X
N	882,884	145,421	320,102	708,203
Panel (b): House prices	Majority		Debt limit	
Sample:	Simple	Supermajority	No	Yes
	(1)	(2)	(3)	(4)
Cap spending ($1,000)	0.114	0.172	0.198	0.330
	(0.185)	(0.371)	(0.395)	()
w/discounting	0.319	0.482	0.553	0.924
District FE	X	X	X	X
Yr-St FE	X	X	X	X
N	65,878	13,704	11,403	68,179

Note: 2SLS estimates and standard errors of the parameter ρ in equation (4). The dependent variable is a standardized measure of test scores. Column 1 is estimated on the sample of districts with a required simple majority; column 2 on the sample of districts with a required supermajority; column 3 on the sample of districts without a debt limit; and column 4 on the sample of districts with a debt limit. In panel (a), all columns control for district and state-by-year-by-grade-by-subject-by-above median 1995 capital spending fixed effects and observations are weighted by the number of test takers. In panel (b), all columns control for district and state-by-year-by-above median 1995 capital spending fixed effects and observations are weighted by district enrollment. Standard errors in parentheses are clustered at the district level. $*$ = 0.1; $**$ = 0.05; $***$ = 0.01.

School Capital Expenditure Rules, Student Outcomes, and Real Estate Capitalization

Online Appendix

Barbara Biasi, Julien Lafortune and David Schönholzer

A Additional Figures and Tables

Figure A1: District-level Capital Expenditures (per-pupil, 2015-16)

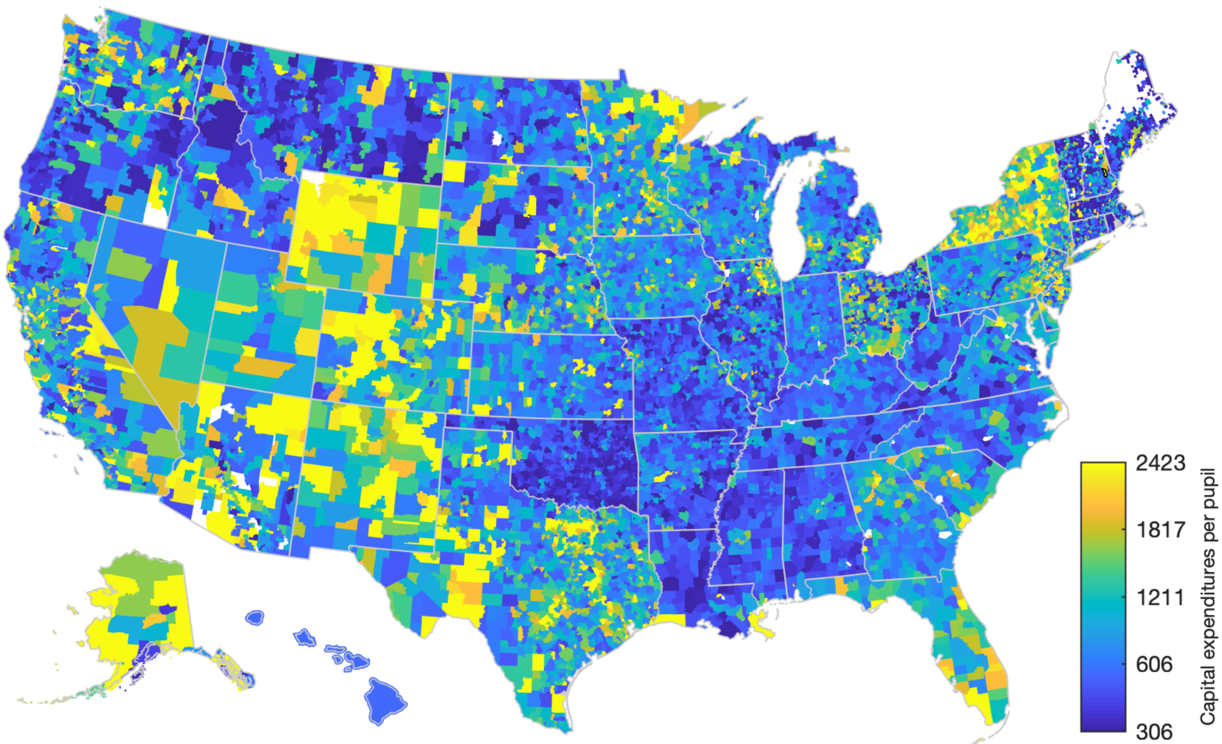
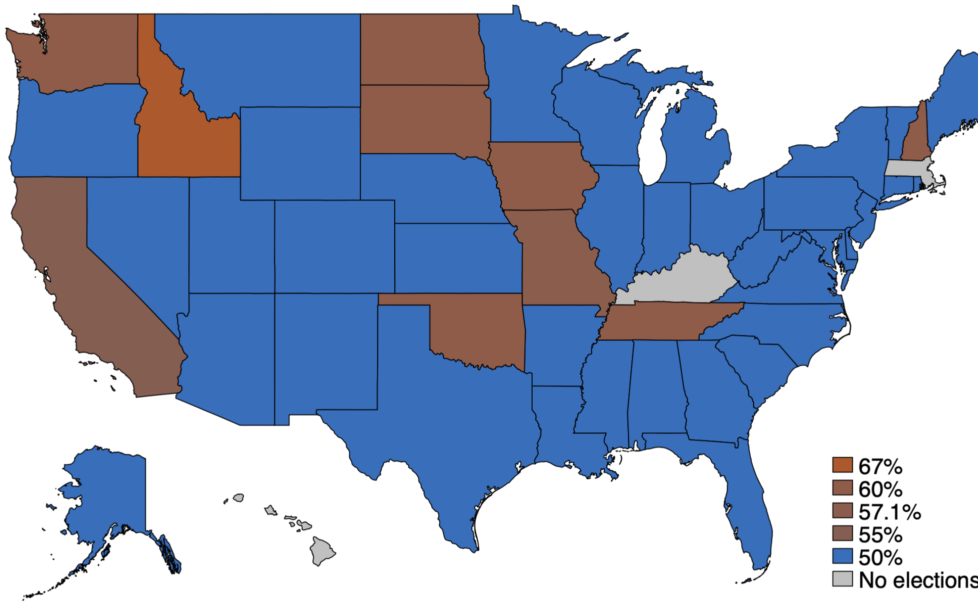
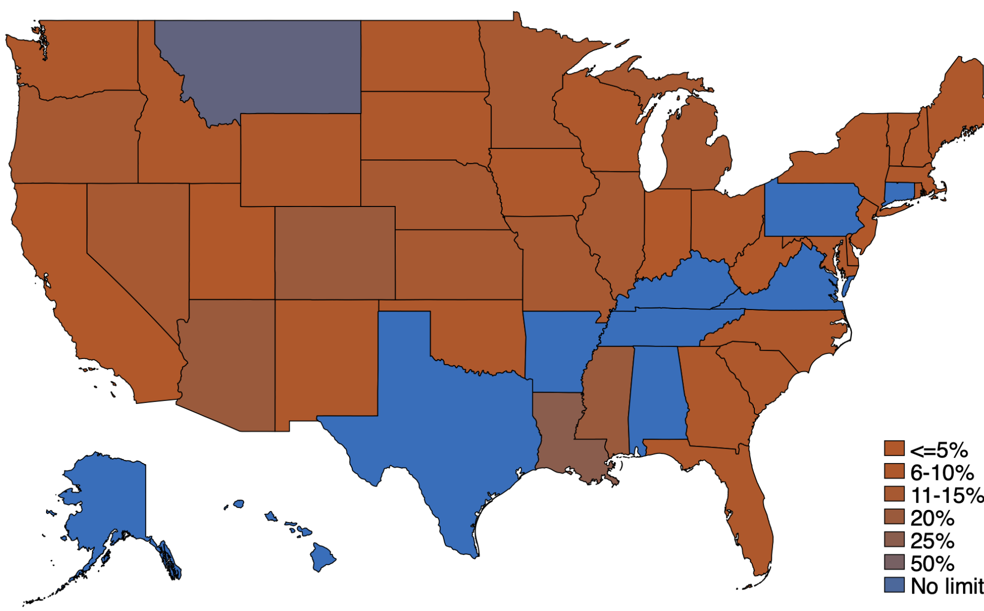


Figure A2: Majority Requirements



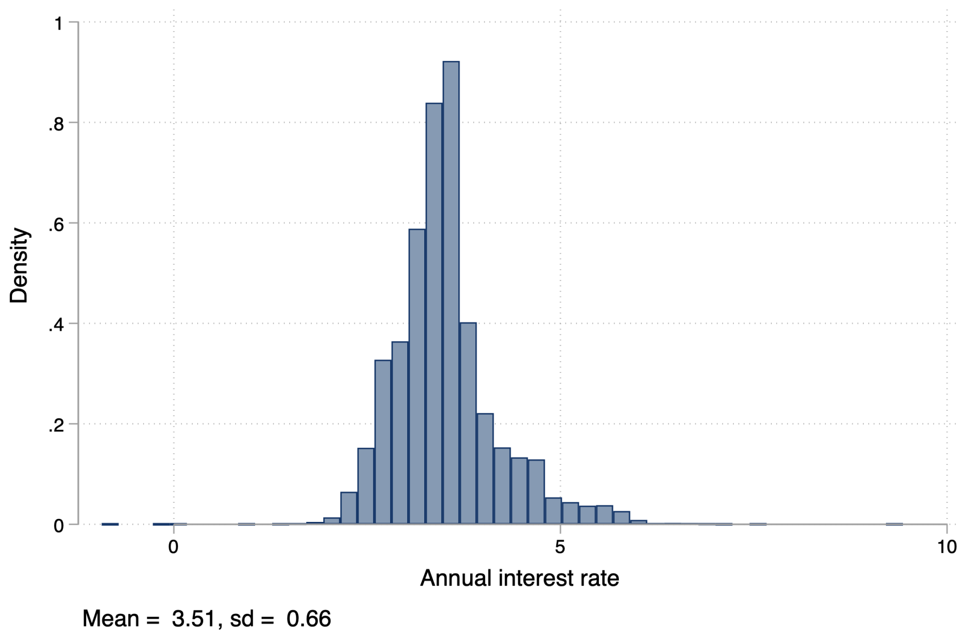
Note: Majority requirements refer to the share of favorable votes, among all people who vote, required for a bond measure to pass.

Figure A3: Debt Limits



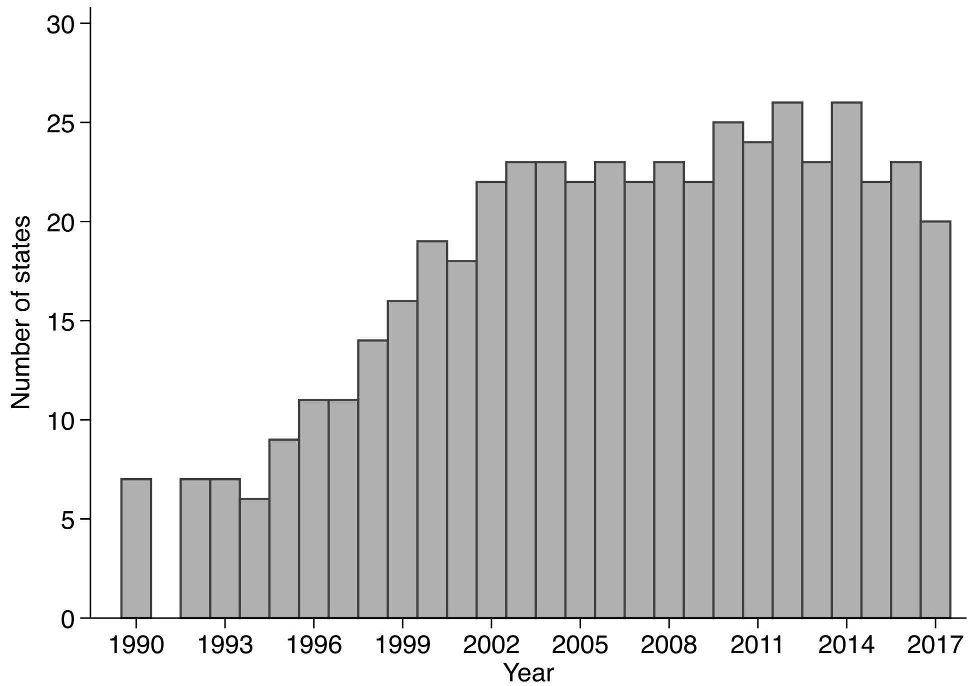
Note: Debt limits are expressed as a share of total assessed property values.

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

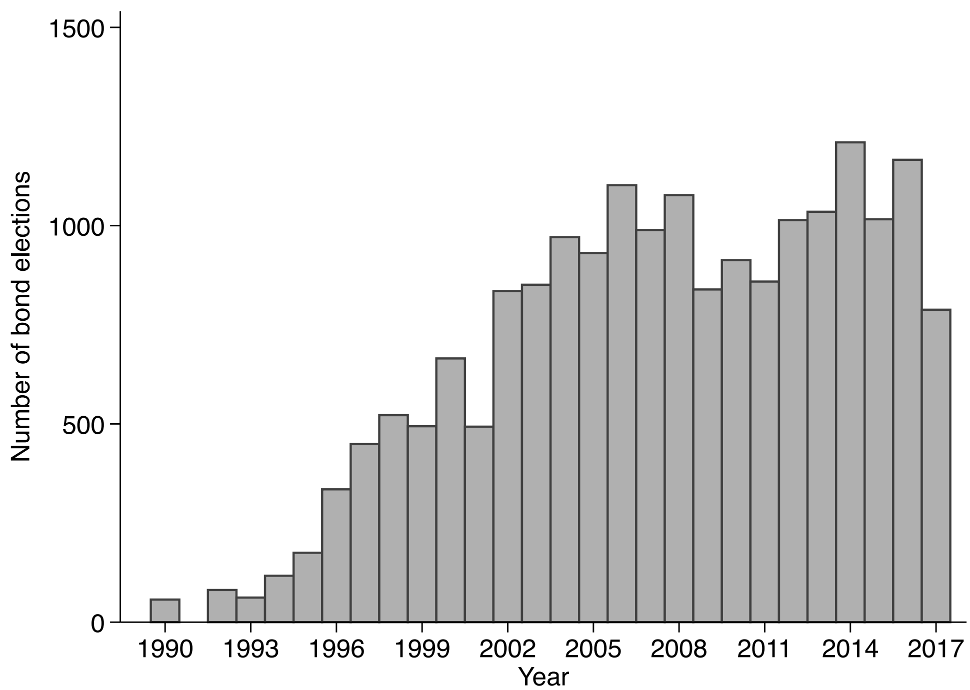


Note: Coupon rates on school district bonds for the years 1997-2017. Rates are shown net of fixed effects for the year of issuance and maturity and for bond type. Data from the Mergent Municipal Bonds Database.

Figure A5: Bond Data Coverage, by Year



(a) Number of States with Bond Elections in a Year



(b) Number of Bond Elections

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

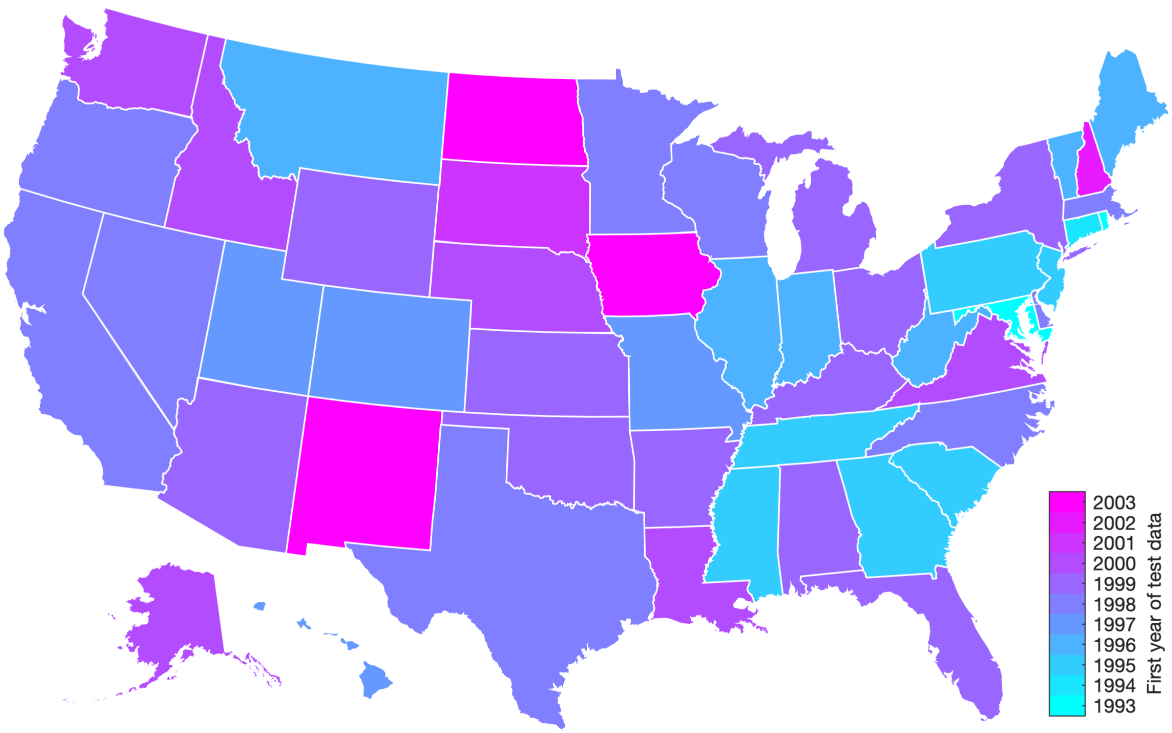
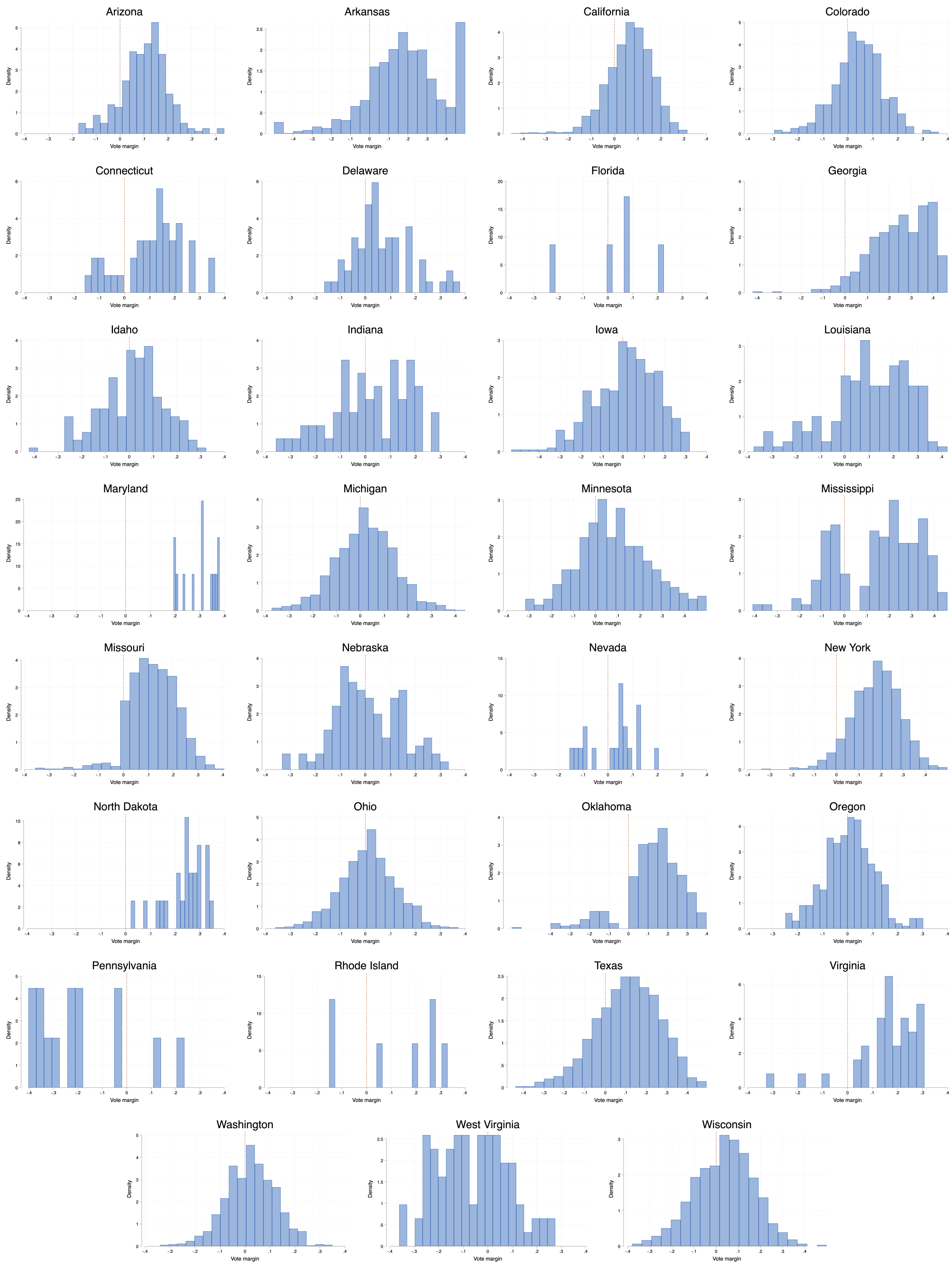
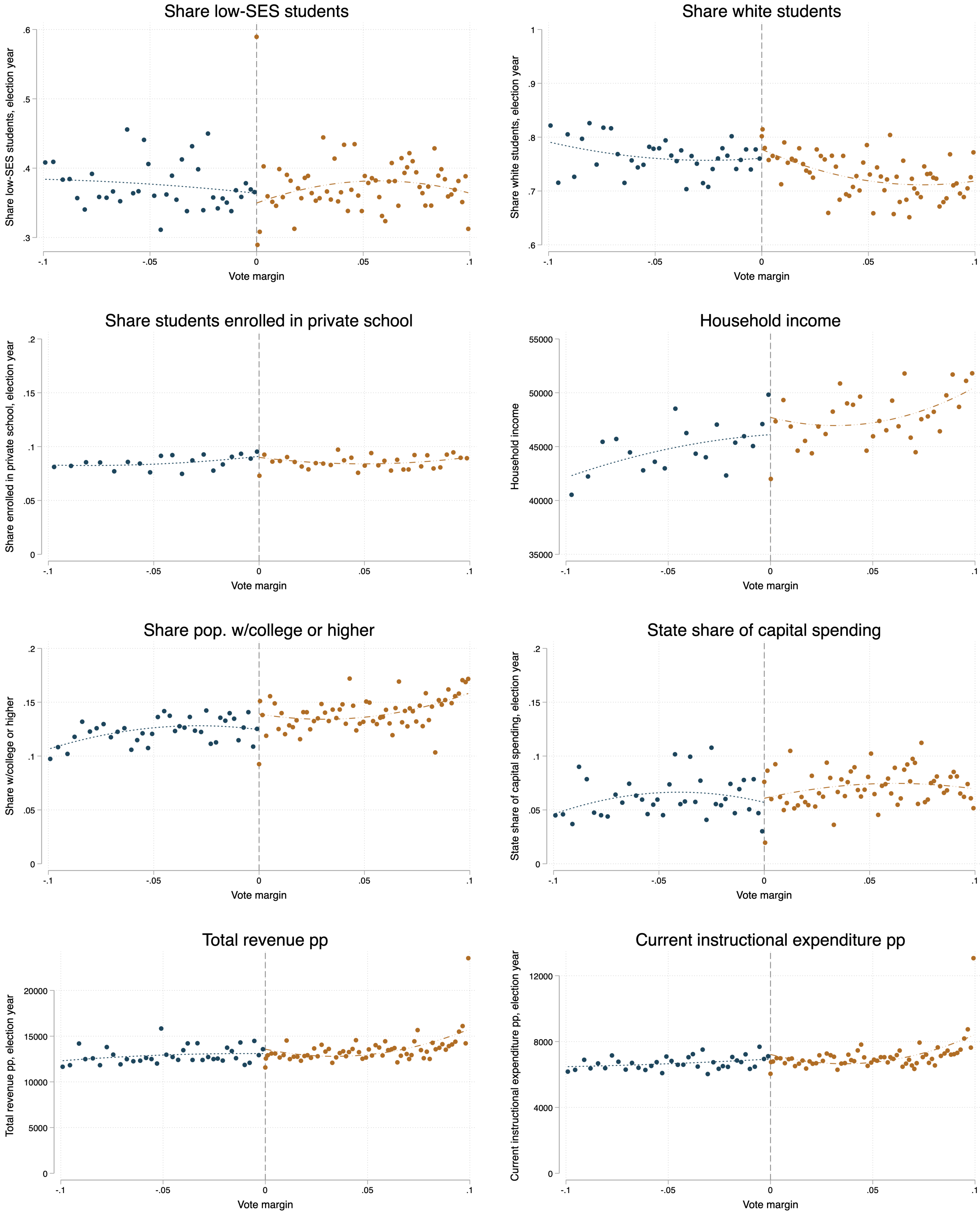


Figure A7: Density of Vote Margin, by State



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.

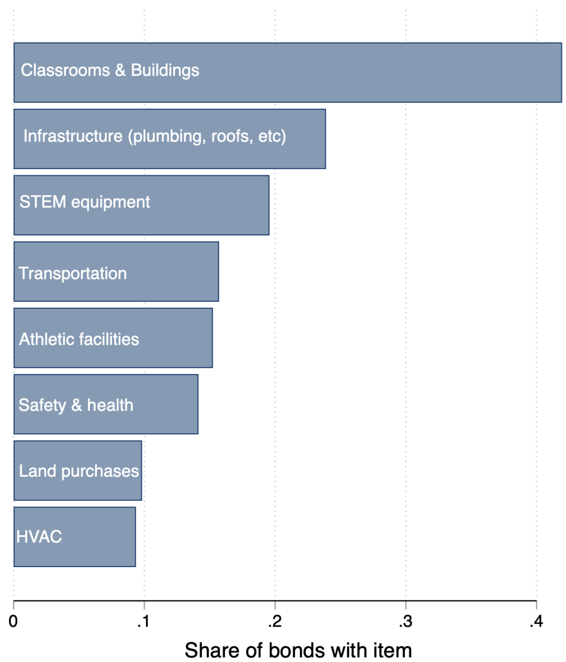
Figure A8: Covariate Balance Around the Vote Margin Cutoff



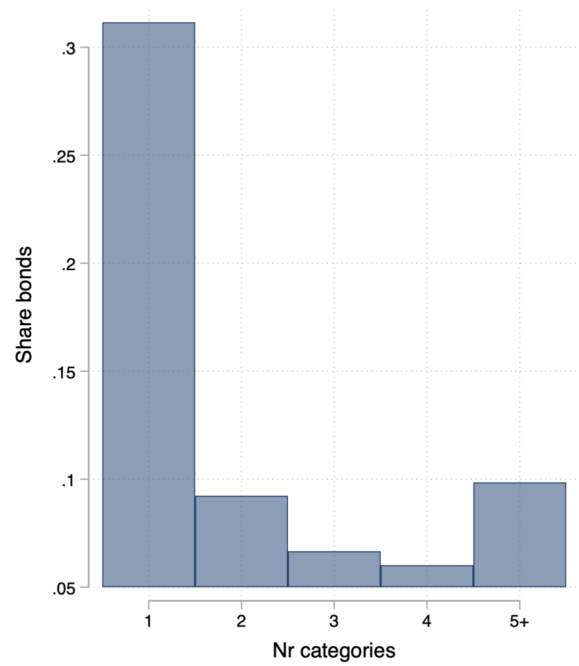
Note: Binned scatterplots of district-level covariates around the vote margin cutoff. Positive vote margins denote successful elections. Each dot is a quantile of vote margin; the vertical axis displays the mean of each covariate in the corresponding quantile. The lines represent fitted quadratic polynomials 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 XX and are measured in XX.

Figure A9: Share of Bonds by Category and Number of Categories

(a) Share of bonds in each category



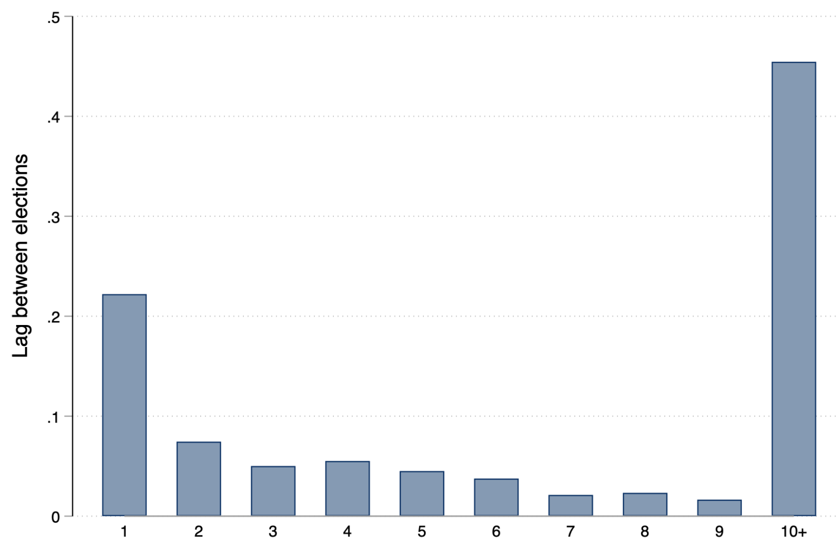
(b) Share of bonds by number of assigned categories



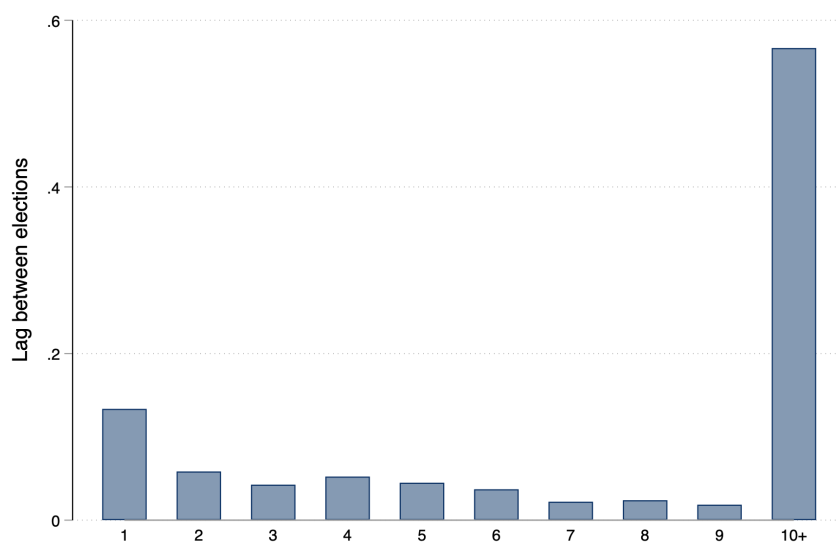
Note: Panel (a) shows the share of bonds assigned to each (non-mutually exclusive) category. Panel (b) shows the share of bonds with each number of assigned categories.

Figure A10: Multiple Elections per District: Summary Statistics

(a) Lag between subsequent elections

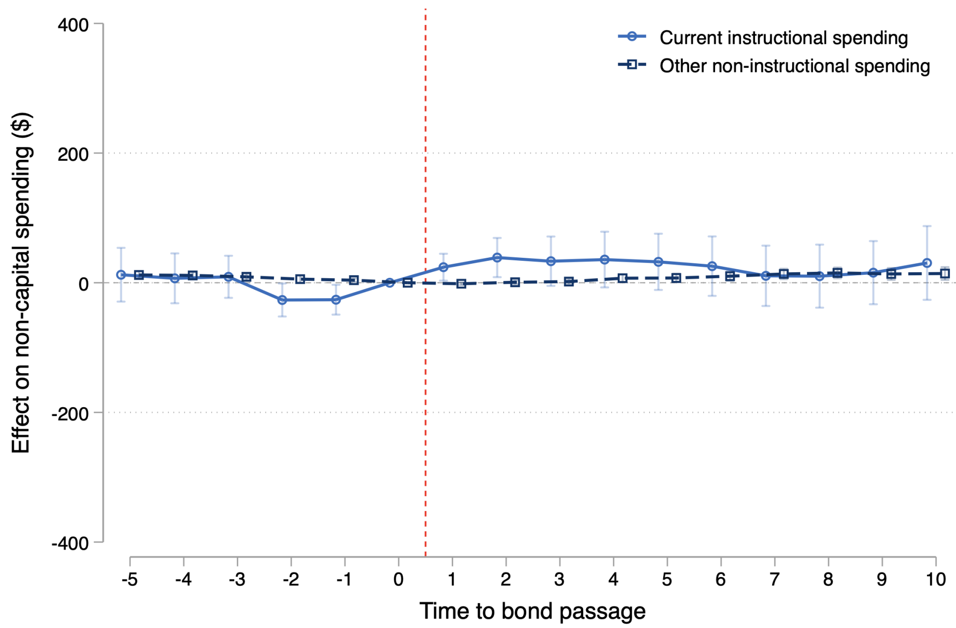


(b) Lag between subsequent successful elections



Note: Panel (a) shows the distribution of the lag between subsequent elections. Panel (b) hows the distribution of the lag between subsequent successful elections.

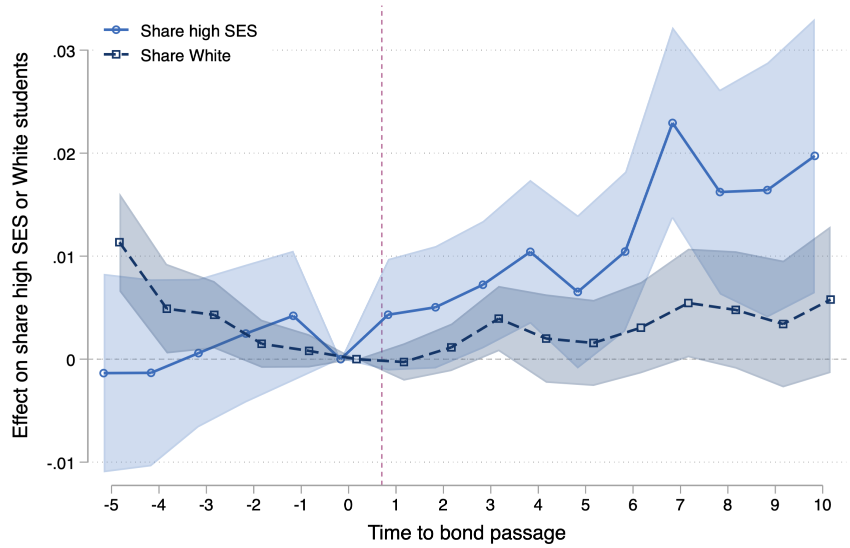
Figure A11: Mean Effects of Bond Passage on Current Spending



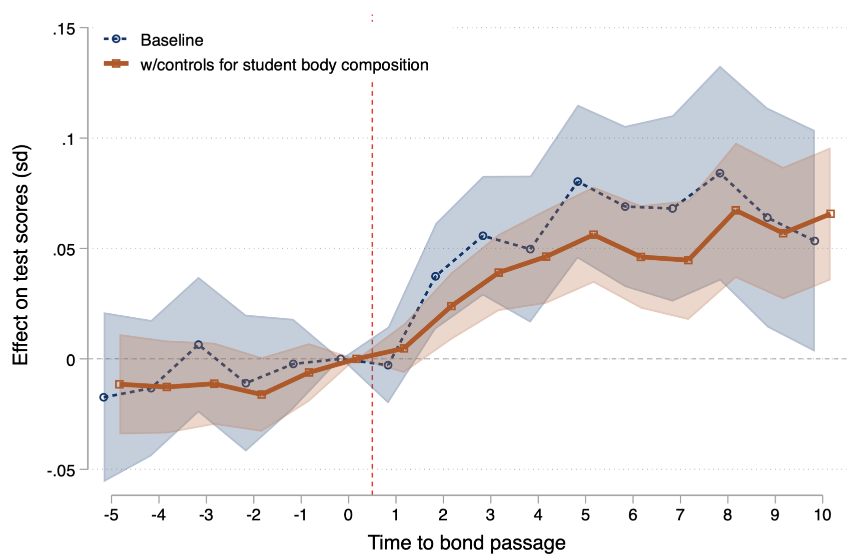
Notes: Estimates and confidence intervals of the parameters β_k in equation (5.2), obtained using current instructional spending and other, non-instructional per pupil as the dependent variable. Estimates are obtained using state-by-year effects interacted with an indicator for above-median 1995 capital spending, and weighing observations by district enrollment. Standard errors are clustered at the district level.

Figure A12: Effects of Bond Passage on Student Sorting and Test Scores, Holding District's Student Composition Fixed

(a) Share of high-SES and White students



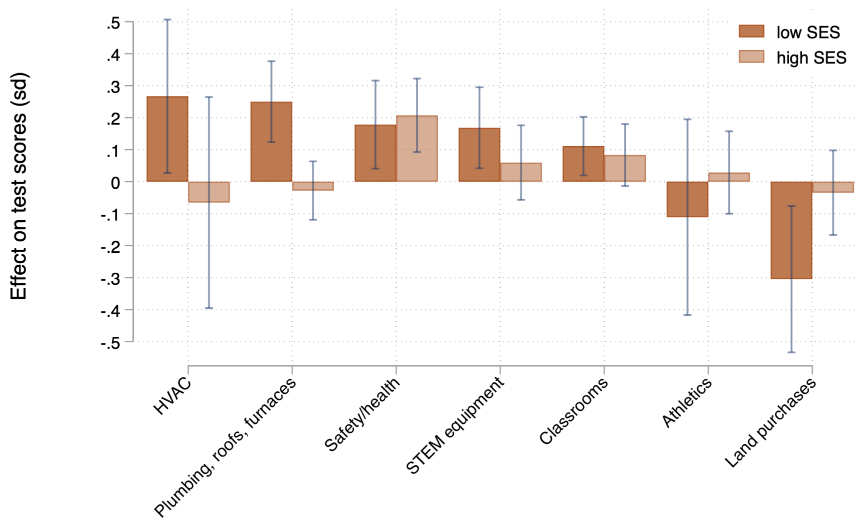
(b) Test scores, controlling for share of students in demographics groups



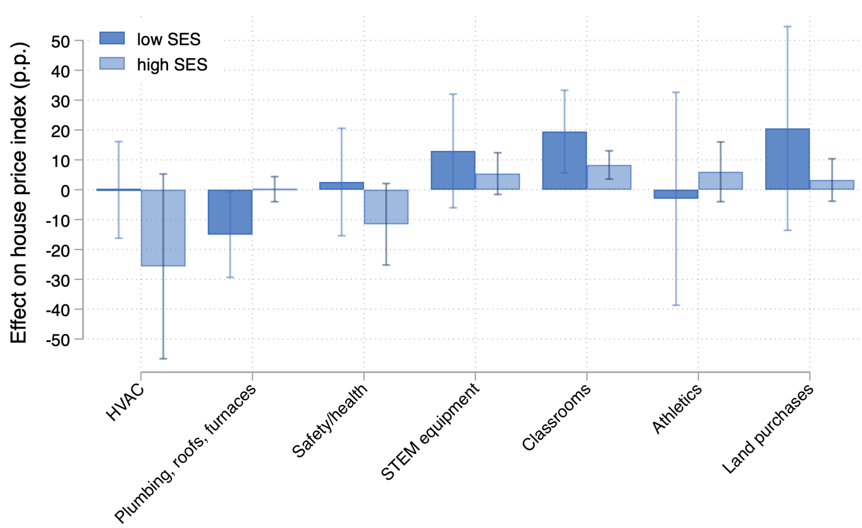
Notes: Estimates and confidence intervals of the parameters β_K in equation (2), obtained using the shares of high-SES and White students (panel (a)) and pooled test scores (panel b). Estimates in panel (a) are obtained using district and state-by-year-by effects interacted with an indicator for above-median 1995 capital spending and weighing observations by district enrollment. Estimates in panel (b) are obtained pooling data across subjects and grades, using state-by-year-by-subject-by-grade effects interacted with an indicator for above-median 1995 capital spending and further controlling for the share of low-SES, Black, and Hispanic students, and weighing observations by the number of test takers. Standard errors are clustered at the district level.

Figure A13: Effects of Passing a Bond, By Spending Category and Share Low-SES Students

(a) Effects on test scores, 1-10 years post-election



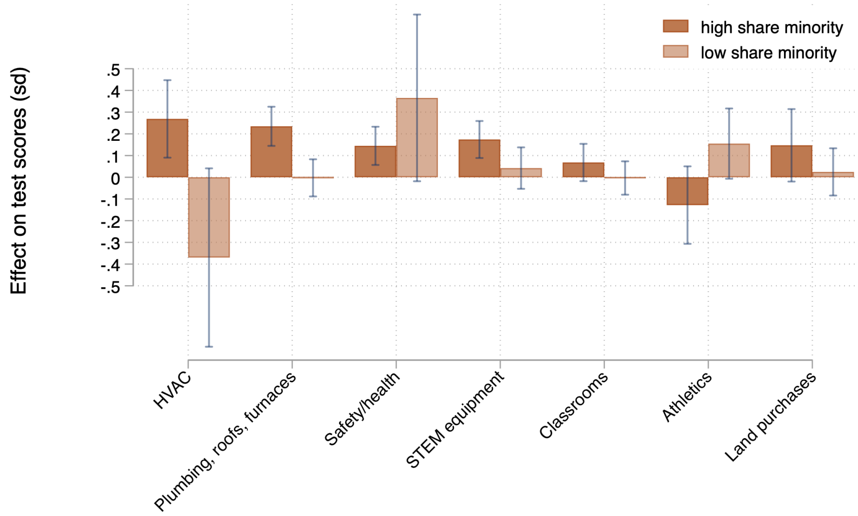
(b) Effects on house price index, 1-10 years post-election



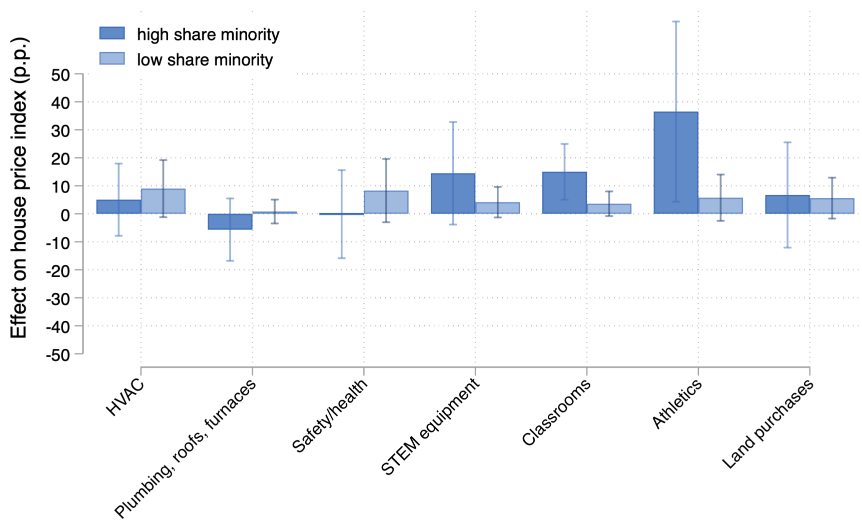
Note: Point estimates and confidence intervals of a linear combination of the parameters β in equation (5.2), obtained stacking districts by category of bond and separately (i) for each category, and (b) by student demographics, captured by the tercile of the share of disadvantaged students ("low-SES" denotes the top tercile and "high-SES" denotes the bottom tercile). Panel (a) shows estimates on test scores 1-10 years post-election; panel (b) shows estimates on the house price index 1-10 years post-election. Estimates in panel (a) are estimated using test scores as the dependent variable, pooled across subjects and grades, using state-by-year-by-subject-by-grade effects interacted with an indicator for above-median 1995 capital spending, and weighing observations by the number of test takers. Estimates in panel (b) are estimated using the house price index as the dependent variable, using state-by-year effects and weighing observations by district enrollment. Confidence intervals are calculated using standard errors clustered at the district level.

Figure A14: Effects of Passing a Bond, By Spending Category and Share Minority Students

(a) Effects on test scores, 1-5 years post-election

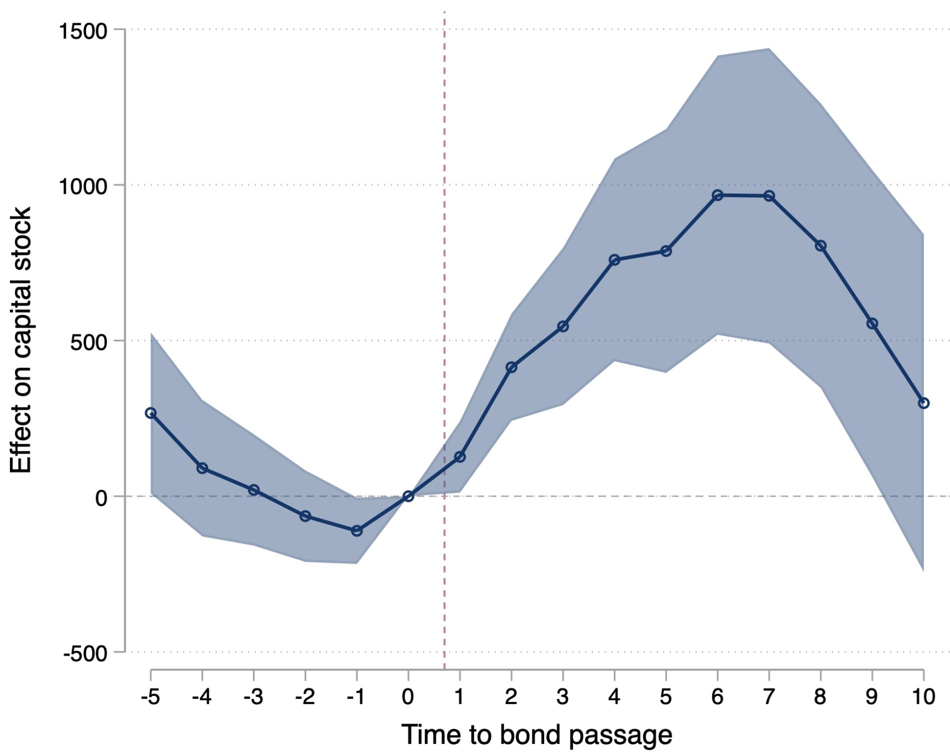


(b) Effects on house price index, 1-5 years post-election



Note: Point estimates and confidence intervals of a linear combination of the parameters β in equation (5.2), obtained stacking districts by category of bond and separately (i) for each category, and (b) by student demographics, captured by the tercile of the share of minority students ("low-minority" denotes the bottom tercile and "high-minority" denotes the top tercile). Panel (a) shows estimates on test scores 1-5 years post-election; panel (b) shows estimates on the house price index 1-5 years post-election. Estimates in panel (a) are estimated using test scores as the dependent variable, pooled across subjects and grades, using state-by-year-by-subject-by-grade effects interacted with an indicator for above-median 1995 capital spending, and weighing observations by the number of test takers. Estimates in panel (b) are estimated using the house price index as the dependent variable, using state-by-year effects and weighing observations by district enrollment. Confidence intervals are calculated using standard errors clustered at the district level.

Figure A15: Effects of Bond Passage on A District's Capital Stock



Note: Estimates and confidence intervals of the parameters β in equation (5.2), obtained using capital stock as the dependent variable. 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 are obtained using state-by-year effects and weighing observations by district enrollment. Standard errors are clustered at the district level.

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

	Non-close	Close (margin$= +/-0.1$)	Difference
Expenditure per pupil ($)			
Capital	1272.8	1063.8	208.9***
			(46.01)
Current	7945.8	6947.1	998.7***
			(75.78)
Spending rules			
Share w/supermajority	0.123	0.208	-0.0854***
			(0.00619)
Has debt limit	0.722	0.834	-0.112***
			(0.00715)
Size p.p. proposed ($1,000)	7.356	8.732	-1.376***
			(0.159)
Categories, approved bonds			
Classrooms	0.387	0.569	-0.182***
			(0.0118)
STEM equipment	0.238	0.350	-0.112***
			(0.0107)
HVAC	0.106	0.141	-0.0350***
			(0.00779)
Other infrastructure	0.212	0.377	-0.164***
			(0.0105)
Safety/health	0.184	0.242	-0.0573***
			(0.00970)
Athletic facilities	0.157	0.205	-0.0472***
			(0.00912)
Demographics and outcomes			
Share low-SES	0.418	0.376	0.0418***
			(0.00386)
ELA test scores	-0.0712	-0.0639	-0.00730
			(0.0163)
Math test scores	-0.0988	-0.0786	-0.0202
			(0.0165)
House price index ($1989 = 100$)	183.5	191.3	-7.858***
			(1.170)

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%.

B Stacked Dynamic Regression Discontinuity: Monte Carlo Simulations

In this appendix we use Monte Carlo simulations to show how well stacked-DRD and DRD-TOT (Cellini et al., 2010) can recover bond treatment effects in the presence of (i) dynamic treatment (multiple bonds per district); (ii) dynamic treatment effects; and (iii) heterogeneous treatment effects across elections. We first present a statistical model of elections and voting which determines the dynamic treatment. We then model treatment effects, letting parameters govern dynamics and heterogeneity. Lastly, we apply the two estimators and compare estimates to the true treatment effects.

Statistical model

Elections and voting Let j denote a district and t a year. Let h_{jt} be an indicator for j proposing a bond in t, v_{jt} the vote margin for that election, and $p_{jt} = \mathbf{1}(v_{jt}) > 0$ an indicator for bond passage.

The sample starts in year 0; we fix $h_{j0} \sim$ Binomial$(1, 0.1)$ and $v_{jt0}|h_{j0} = 1 \sim \mathcal{N}(\mu_v, \sigma_v^2)$ (we winsorize p_{jt0} to be between 0 and 1). In years following $t = 0$, a district's probability to hold an election is

$$h_{jt} = \mathbf{1}(\alpha_j^h + \tau_t^h + u_{jt}^h + \sum_{s=1}^{5} \rho_s^h p_{jt-s} + \sum_{s=1}^{5} \delta_s^h h_{jt-s}(1 - p_{jt-s}) > H)$$

and the vote margin in that election is

$$v_{it} = \mathbf{1}(\alpha_j^p + \tau_t^p + u_{jt}^p + \sum_{s=1}^{5} \rho_s^p p_{jt-s} + \sum_{s=1}^{5} \delta_s^p h_{jt-s}(1 - p_{jt-s}) + \sum_{s=1}^{5} \gamma_s (1 - h_{jt-s})$$

where $\alpha_j^h, \tau_t^h \sim \mathcal{N}(0, \sigma_h^2)$; $u_{jt}^h \sim \mathcal{N}(0, 1)$; $\alpha_j^p, \tau_t^p \sim \mathcal{N}(0, \sigma_p^2)$; $u_{jt}^p \sim \mathcal{N}(0, \sigma_u^2)$. We assume $\rho^h \leq 0$, $\rho^p \leq 0$, $\delta^h \geq 0$, $\delta^p \geq 0$, and $\gamma \geq 0$. In words, proposing and passing a bond is less likely if one was passed recently and less likely otherwise. Also, passing a bond is more likely if a bond was proposed but not passed recently.

Treatment effects We assume that the treatment effect at t of an election which occurred at time t_e in district j, $\beta_{jt_e t}$, is heterogeneous across treatment cohorts and correlated with the vote margin. It is also dynamic, a linear function of time since the election:

$$\beta_{jt_e t} = \beta_{0,t_e} + \beta_{l,t_e}(t - t_e) + \beta_{s,jt_e}(t - t_e)^2 + \rho v_{jt_e}$$

For simplicity and without loss of generality, we assume that β_{s,it_e} is constant across districts and election years. On the other hand, $\beta_{0,t_e} \sim \mathcal{N}(\bar{\beta}_0, \theta_1)$ and $\beta_{l,t_e} \sim \mathcal{N}(\bar{\beta}_l, \theta_2)$. In words, the intercept and linear term of treatment effects with time are heterogeneous across cohorts. This implies that there is a correlation between the timing of treatment and the magnitude of the treatment effect.

Test scores Test scores for district j in year t are denoted by y_{jt} and are given by

$$y_{jt} = \mu_{jt} + \sum_{s=1}^{T} \beta_{ist} p_{is} + \theta_1 \bar{v}_i,$$

where $\mu_{jt} \sim \mathcal{N}(0, \sigma_y^2)$ and $\bar{v}_i$ is the average vote margin for district i in the period of study, set to zero if i never holds any elections.

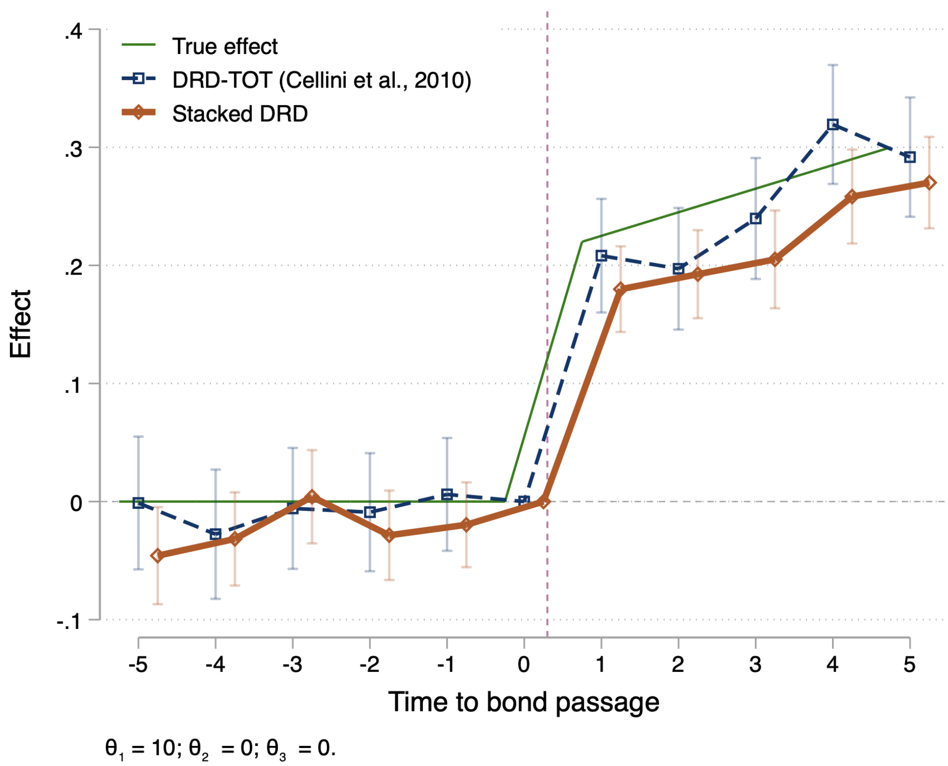
Calibrated parameters We calibrate the model's parameters to match the share of proposed and passed bonds in the data in each year and the vote margin. Values are as follows: $\mu_v = 0.05$; $\sigma_v^2 = 0.2$; $H = 1.7$; $\sigma_h^2 = 0.5$; $\sigma_p^2 = 0.08$; $\sigma_u^2 = 0.2$; $\sigma_y^2 = 0.2$; $\bar{\beta}_0 = 0.2$; $\bar{\beta}_l = 0.02$; $\rho = 10$.

Simulations

Below, I show simulations of three specifications for $\theta_1 \in \{0, 0.2, 0.5\}$ (the parameter that disciplines cohort-specific heterogeneity in the slope component of treatment effects), and $\theta_2 \in \{0, 0.1, 0.2\}$. All the estimates are based on a generated sample of 2000 districts, each observed for 20 years.

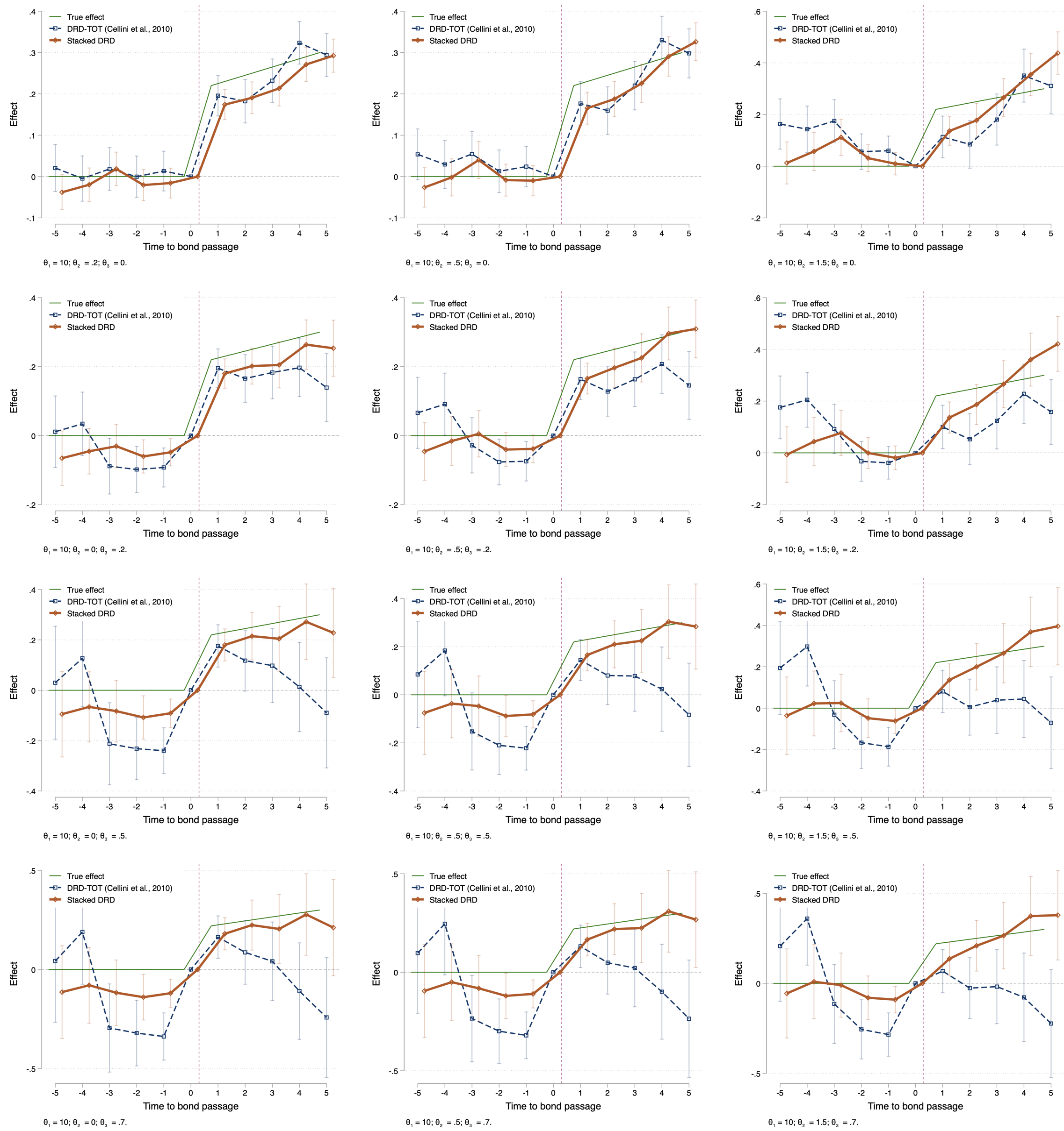
In the absence of treatment effect heterogeneity ($\theta_1 = 0$, $\theta_2 = 0$), stacked DRD and DRD-TOT deliver the same estimates (Figure B1). In the presence of treatment effect heterogeneity on the intercept and/or slope, the stacked-DRD delivers estimates closer to the true effects (Figure B2).

Figure B1: Stacked DRD vs DRD-TOT: Monte Carlo Simulation, No Heterogeneity



Note: Estimates and confidence intervals of the parameters β in equations (2) (DRD-TOT) and (5.2) (stacked DRD) on test scores generated using the statistical model above. Estimates are shown for $\theta_1 = 0$ and $\theta_2 = 0$.

Figure B2: Stacked DRD vs DRD-TOT: Monte Carlo Simulations, With Heterogeneity



Note: Estimates and confidence intervals of the parameters β in equations (2) (DRD-TOT) and (5.2) (stacked DRD) on test scores generated using the statistical model above. Estimates are shown for various values of θ_1 and θ_2.