

# Effectiveness, Efficiency, and Governance of School Capital Investments Across the U.S.\*

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## Abstract

School capital investments, a major component of US school spending, are mostly financed through bonds issued by the school districts, with rules that vary in stringency across states. The impacts of these investments are highly debated, and existing estimates are conflicting. We reconcile this puzzle by showing that the stringency of funding rules and the demographic makeup of school districts affect the types of projects approved in equilibrium and, in turn, the impacts of these projects on students and communities. Using new data on 20,000 school bond elections and student achievement for 31 U.S. states and exploiting variation from close elections in a dynamic regression-discontinuity design, we find that approving a bond raises achievement and house prices on average. These impacts, though, are concentrated in districts with a high share of economically disadvantaged students and states with tighter financing rules. This is partly due to differences in the size and composition of financed projects: Investments on HVAC produce large test score effects but don't impact house prices, whereas investments on athletic facilities only raise house prices. These findings are consistent with a probabilistic voting model in which districts, facing different rule stringency, choose the size and composition of a bond proposal to maximize the chances of electoral approval.

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# 1 Introduction

Capital expenditures are a major component of total U.S. public school spending. Yet, large differences exist among school districts in amounts spent, types of projects financed, and the resulting condition of school facilities. Between 1990 and 2019, the average school district spent \$1,213 per pupil annually on capital projects (approximately 12% of total spending). The school district of Detroit, MI, at the 10th percentile of the distribution, spent only \$107; the school district of New Orleans, LA, at the 90th percentile, spent \$2,207. These spending disparities translate into a 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.

Just like capital spending and facilities differ across school districts, the laws regulating the financing of these investments also vary. In some states, stricter rules make it much harder for districts to raise money for capital projects. In others, state tax revenues supplement local funding, often via matching grants, especially in low-income districts (Biasi et al., 2021). Funding rules could affect the type of projects districts choose to prioritize, the marginal value of a dollar in terms of student outcomes, and its valuation by the district's community. The value of these investments could also differ across districts serving different communities and populations of students.

Due to the scattered landscape of school financing in the U.S. and the absence of comparable measures of student achievement across states, existing studies on the effects of capital spending have looked at the experience of individual states (such as Cellini et al., 2010; Martorell et al., 2016) or districts (such as Neilson and Zimmerman, 2014; Lafortune and Schönholzer, 2022) in isolation. Notably, these works have reached conflicting conclusions. The debate on the effectiveness and efficiency of school capital spending is thus still open; in particular, it is unclear whether certain projects yield higher returns than others and whether funding rules matter.

This paper contributes to this debate by (i) estimating the average impact of school capital spending on a large sample of U.S. states, and (ii) investigating what drives difference in returns across states, with emphasis on differences in the demographic composition of school districts, the funding rules, and the size and type of projects that are financed. To perform this analysis, we assemble a novel dataset on school bonds and student outcomes for 29 U.S. states. We identify the

impact of spending increases by exploiting variation from close bond elections in dynamic regression discontinuity (DRD) models (Cellini et al., 2010), which compare outcomes between districts that marginally approved a school bond and those that marginally rejected it, in a given year. On average, the approval of a bond increases student outcomes by 0.06 of a standard deviation ten years after an election. House prices also increase by 7%, indicating that communities value these investments more than they are required to pay for them. These effects, though, vary significantly across states. They are much larger in states that require an electoral supermajority to approve school bonds and in districts serving larger proportions of disadvantaged students. We interpret these findings in a probabilistic voter model, in which school districts have strategic incentives when considering the size and composition of a bond proposal and are subject to different funding rules. Using our data, we estimate key model parameters and examine counterfactual scenarios in which districts face lower supermajority thresholds and increased state contributions.

In the U.S., the financing of public school capital expenditures is largely local and follows rules set by the states, which differ on a number of aspects. Districts fund capital outlays by issuing debt in the form of bonds; in all states except Hawaii, Kentucky, and Massachusetts, the issuance of these bonds is subject to voter approval. Voting requirements, though, vary substantially. For example, 37 states (including Texas, New York, and Illinois) only require a simple majority to pass a bond. California requires a supermajority of 55%; Oklahoma, Tennessee, and Washington require 60%, and Idaho requires two-thirds. Most states also contribute towards district capital spending, to an extent that ranges from nearly zero in Missouri and Michigan to close to half in Connecticut, Wyoming, and Delaware. Supermajority requirements and lower state aid make it more difficult for districts to raise money for capital projects.

Our nationwide analysis of the effects of capital spending on outcomes is made possible by newly collected data on test scores, school bonds, and house prices at the district level. To better understand what districts spend money on, we collected data on district-level school bond elections for over two-thirds of U.S. states. In these elections, voters are called to choose whether to raise local levies to finance school capital projects. We have information on the text of each ballot, the vote share, and the proposed spending increase. In nearly all elections, ballots describe the proposed use of the funds. For example, the ballot of the June 2022 election in the Fremont Union High School District, CA, stated that the district intended 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". This information allows us to categorize bonds into classes of projects, by applying natural-language-processing (NLP) techniques to the text of each ballot.

We link information on bond elections to the test scores of all students in each district. Measures of student learning that are comparable across states and over time are generally unavailable because states measure achievement using different standardized tests, and these tests have changed over time. A notable exception is the Stanford Education Data Archive (Reardon et al., 2021). However, this dataset only contains this information from 2009 onwards. To cover earlier years, we collected school- and district-level test score data from state departments of education and combined it with an earlier national databases of school-level state assessments. Following the approach of Reardon et al. (2017) and Fahle et al. (2021), we then converted 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). With this procedure, we built a novel district-level database covering nearly all states from 2002 through 2019. We further link these data to a measure of the house price index constructed by Contat and Larson (2022) using a repeated-sales method.

In the first step of our analysis, we use DRD models to estimate the effect of passing a bond on capital spending, student learning, and the real estate market. Our empirical strategy compares outcomes between districts that narrowly approved a bond proposal and districts that marginally rejected one in the same year, by controlling directly for the vote margin. To account for the fact that proposing a bond in a given year might depend on whether a bond was proposed and/or passed in the previous years, we follow Cellini et al. (2010) and further control for whether the district proposed a bond in the past or will do so in the future and for the vote margins in each of these elections. Our estimates thus capture treatment-on-the-treated (TOT) effects of bond approvals.

We find that passing a bond leads to a large and significant average increase in capital outlays, student achievement, and house prices. In the five years following an election, spending on capital projects increases cumulatively by about \$3,500. These investments are beneficial for students: While test scores are on a flat trend in the years leading to a successful election, they increase by 0.08 standard deviations (sd) eight years after a bond passage. These estimates imply that a \$1,000

increase in capital spending over five years increases test scores by about 0.012 sd four to ten years later. The increase in spending also leads households to sort across districts, reducing the share of disadvantaged and minority students. Test score effects, though, are present even accounting for these compositional changes. Following a bond approval, house prices increase by 7% ten years after the election. This indicates that the community values school capital investments more than they are asked to pay for it, in the form of increased local property taxes. In turn, this implies that the average district spends too little on school capital projects.

These average effects, though, mask important differences across districts and states. We distinguish between two types of heterogeneity. The first, which we call “treatment effect heterogeneity,” stems from the fact the same project may yield different returns depending on the characteristics of a district’s students, such as the share of low-socioeconomic status (SES) or minority students. We find evidence of it: Both test score and house price effects are almost exclusively present in districts with high shares of students who are low-SES or minority. The second type of heterogeneity is “treatment heterogeneity,” and it stems from the fact that bonds with different compositions of spending items may also have different impacts. We show that spending on infrastructure, such as HVAC systems, are followed by large increases in test scores but not house prices, whereas spending increases on athletic facilities are followed by an increase in both test scores and house prices.

Why do different districts prioritize different types of spending projects? We highlight a crucial role for the stringency of the funding rules in this process. The intuition is that, when funding rules are stricter (for example, a larger majority is required to pass a bond), districts have larger incentives to “please” voters and will propose bonds that match voters’ preferences. We illustrate this mechanism with a probabilistic voting model, in which districts decide the proposed size and composition of the spending increase, and voters choose whether to approve such an increase or not. The probability that a bond of a given size and composition passes is a function of the attributes of both the community and the funding rules. We assume that districts choose bond size and composition to maximize an a priori unknown objective function, extending previous research on the political economy of school district spending ([Romer and Rosenthal, 1979](#)). Intuitively, the district’s optimality condition posits that a marginal increase in the size of a proposed bond increases total spending if the bond passes, but it also reduces the probability that it passes; the importance of each of these two forces depends on bond composition, voter preferences, and community at-

tributes. Similarly, a district seeks to add amenity items such as athletic facilities to a bond to the point at which it maximizes the electoral return, which again depends on voter preferences and community attributes.

Using the model, we derive a set of theoretical predictions on the effects of more lenient funding rules, such as a lower majority threshold, on bond size and composition. Comparative statics indicate that a lower majority threshold would increase the size of the spending increase and shift its composition to be more in line with the preferences of the voters. In line with these predictions, our data indicate that states with a simple majority requirement pass larger bonds, but with a stronger amenity focus, compared with supermajority states. As a result, they see smaller increases in test scores and house prices relative to supermajority states following the passage of a bond.

Taken together, the results from our analysis indicate that, on average, capital investments can be an effective way to boost student learning. However, not all increases in spending are equally productive and circumstances matter. In particular, funding rules—such as a required supermajority or a low state share of total spending—can create significant frictions and keep investment amounts at a level that is too low, with significant implications for students and the overall community.

**Literature contributions.** Our paper contributes to a body of works that have estimated the effects of school capital expenditure on student outcomes. All 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 even school districts (Neilson and Zimmerman, 2014; Lafortune and Schönholzer, 2022) and reach conflicting conclusions. 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 reconcile these studies by offering the first near-nationwide analysis of the effect of capital spending on students and house prices. In addition, we document the relationship between funding rules, bond amounts and spending items, and district characteristics in shaping the effects and investigate how changes in funding rules might affect student outcomes.

More broadly, our paper relates to a broad literature, spurred by the Coleman report (Coleman et al., 1966), that has tried to understand 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 show that, on average across the U.S., increased spending on capital projects can improve student outcomes and is valued by the community. However, we also caution that the magnitudes of these impacts crucially depend on funding rules, district characteristics, and the specific projects that are funded.

Finally, we contribute to a more broad literature on the valuation of public investments. Our empirical results and model simulations provide evidence that the fiscal institutions that govern local public good investments can have a significant impact on their efficacy 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 Expenditure Across The US

In 2018-19, roughly \$73 billion was spent on capital outlay for public K-12 schools in the United States ([Cornman 2021](#)). This includes expenditures for construction of new buildings, renovations of existing buildings, land purchases, and equipment; expenditures on repairs, routine maintenance, and debt service is not included in this figure. Capital outlay comprises just under 10% of total education spending; the vast majority (87%) of K-12 expenditures go towards current operations (i.e. staffing, materials, and maintenance).<sup>1</sup>

While a relatively small share of total K-12 expenditures, funding for capital infrastructure is unique in a number of ways. Most notably, it is largely financed through local revenues, in particular, local bonds finance through property tax increases, and fees on new real estate developments. Nationally, 77% of funding for capital outlay from 2008-09 to 2018-19 was locally funded, compared to only 22% through state dollars ([Filardo, 2021](#)).<sup>2</sup> This stands in stark contrast to funding for school

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<sup>1</sup>Debt service is roughly 3% of spending, and another 1% goes towards other programs such as community services, adult education, and community colleges.

<sup>2</sup>The federal government plays a trivial role in school facility funding, covering only 1% of capital outlay between

operations, which rely more heavily on state support. Furthermore, while state-level school finance reforms led to equalization and in many cases more progressive funding for low-income and low-wealth school districts,<sup>3</sup> funding for capital outlay has generally been higher for higher-income and higher-wealth school districts within states in most years since 1990 (Biasi et al., 2021). Moreover, spending on capital outlay varies significantly both across and within states (Appendix figure A.1), which may drive in heterogeneity in school facility conditions in different districts.

## 2.1 State-level Institutions for Capital Outlay

The reliance on local revenues varies considerably across states. In a handful of states, capital outlay is primarily funded with state dollars (Alaska, Hawaii, Maine, Massachusetts, New Hampshire, New York, Rhode Island, and Wyoming), while roughly half of states contribute less than 5% of overall funding. The structure of state support – when provided – also differs across states. 27 states provide some sort of matching grants; the structure and matching rate of these programs varies across states. For example, 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 state funds for school modernization (Lafortune and Gao, 2022).

Conversely, state funding in Ohio is more progressive: 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. Funds are distributed to districts based on a ranking determined by local property wealth and household income (for an evaluation of this program, see Goncalves (2015) and Conlin and Thompson (2017)). Other states provide funding not contingent on local revenues, funded through sales taxes, state bond revenues, and/or general fund appropriations.

Beyond providing financial support for capital outlay, many states impose restrictions on the ability of local governments to finance school capital. Property tax limits exist in 22 states, on av-

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2008-09 to 2018-19.

<sup>3</sup>See, for example: Hoxby 2001; Koski and Hahnel 2015; Corcoran and Evans 2015; Lafortune, Rothstein, and Schanzenbach 2018



erage capping property tax revenues to 2% of assessed values, though voter approval can override these restrictions in some cases; these also affect the ability of local districts to raise operational funding in many states. Debt limits are imposed in 40 states, with an average limit of 11% of assessed values.

## 2.2 School Bond Elections

With limited state support, local bonds are the key source of capital funding in most states.<sup>4</sup> In 37 states, voter approval is required for school districts to issue bonds (including Texas, New York, and Illinois) . Notably, 10 states require a supermajority of voters to approve local bond issuances. These supermajority thresholds range from 55% (California) to a two-thirds majority (Idaho).

Bond elections are held at different times of the year; districts may hold a bond election during a primary or general election—or during an “off-cycle” election year—with the decision to do so based on the strategic behavior of local school districts. Importantly, the district decision to propose a bond to local voters is dynamic; districts who lose elections may choose to hold another election shortly thereafter, while other districts may choose to propose several smaller bonds in short succession to fully fund a broader infrastructure program.<sup>5</sup>

In the typical bond election, a district proposes to issue bonds of a given amount, to be paid for by increases in local property tax rates for a specified number of years. Districts that succeed and see their proposed bond approved can then issue bonds, and may choose to fully exhaust bonding capacity up to the limit voters approved, or they may choose to do so gradually. Funds are then used for capital infrastructure projects, which may vary in scope, timing, and location within the district.

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<sup>4</sup>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.

<sup>5</sup>This was the strategy employed by Los Angeles Unified in the late 1990s and early 2000s, which passed several bonds to fully fund a \$25 billion, multi-decade infrastructure renewal project (Lafortune and Schönholzer, 2022).

## 3 Data

### 3.1 District Financial and Enrollment Data

Data on district demographics and finances come from the National Center of Education Statistics (NCES) Common Core of Data (CCD) and the 1990 Census. We compile annual demographic information for school districts, including enrollment, racial/ethnic composition, and the share of low-income students<sup>6</sup> at each grade level starting in 1991. We use district level finance data from the NCES annual survey of school districts and the Census of Government starting in the 1989-90 school year. Finance data include variables on total revenues, total spending, and spending by category; starting in 1995, our data also includes district-level records on revenue received from the state for capital outlay. Data on district mean and median home values and household income are drawn from the 1990 Census. We convert all dollar figures in the paper to 2020 dollars per pupil.

### 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, collected through state- and county-level records. In most states, local election data are recorded and maintained by a statewide office, often freely available online. We compiled the available data on school-district capital bond elections from all states where it was easily available, and submitted formal data requests to those states for which data were not accessible. In most states, these data were provided by the state's secretary of state office or department of education.<sup>7</sup>

In sum, we obtained data for 40 states.<sup>8</sup> We exclude some states due to data limitations or concerns: states that only report election data for winning elections, states that do not report vote shares, and states where we do not observe the vote share for 50% or more of total bond elections. With these restrictions, we are left with a dataset of 29 states, covering 10,613 districts for 1990-2017. The earliest data available are in 1990 for six states; we have limited coverage across states until the early 2000s.<sup>9</sup> The final dataset includes information on election data, vote share in favor,

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<sup>6</sup>Defined as the share of students eligible for free or reduced price school meals.

<sup>7</sup>We are grateful to Stéphane Lavertu for sharing bond election data for several states.

<sup>8</sup>Among the ten missing states, three do not hold bond elections, and for seven we were not able to find systematic records of bond elections.

<sup>9</sup>See Appendix figure A.2 for the number of states with district bond data and the number of bonds in each sample

proposed bond amount, and some textual information such as ballot texts, keywords, or purpose descriptions.

Notably, there are some cases where a district proposed and/or approved multiple bonds in the same year. To collapse the bond data to a district-year panel we select the largest bond the district proposed in a given year.<sup>10</sup> When bond amounts are missing, we instead choose the most “marginal” bond – the bond with the vote share closest to the passage threshold in the given state-year. We exclude bond elections with missing vote share. With these restrictions, this leaves us with a final bond dataset consisting of 16,393 unique district-year bond elections, across 5,418 districts. There are 6,050 districts with no bond election during our sample window.<sup>11</sup>

### 3.2.1 Classifying Bond Expenditures

**SchoolBondFinder.com.** The administrative bond election data contain information on proposed investments primarily in the form of unstructured texts, mostly from ballot texts. Given heterogeneity in the specific uses of a bond—for example, spending to improve classroom spaces may be expected to have differing impacts than spending on athletic facilities—this lack of data has been a limitation in the prior literature. To overcome this, we obtained systematically categorized bond-level data from 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 nationally. Data on bond expenditure categories and subcategories allow us to classify bond expenditures from elections we can link to these data. Notably, these data are not comprehensive: they cover only passed (not failed) bond elections primarily in the period from 2014 onwards.

SBF consists of 14,137 bonds whose planned investments have been manually categorized into six primary categories with one to seven secondary categories each, for a total of 27 categories. The information used to categorize bonds comes from district websites, local newspapers, and other sources. The primary categories are capital improvements, construction/renovation, safety/security, technology, transportation, and other investments. We were able to match 4,065 SBF records to

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year

<sup>10</sup>This is also the approach of Martorell et al. (2016).

<sup>11</sup>In total, we collected 37,079 bonds from 40 states. From these bond data, we construct two datasets: a unique district-year panel of 16,393 bonds (53% of all bonds) with complete information on share yes votes that we use for our event study estimates; and a bond panel with 12,100 observations with additional information on bond size and composition.

bonds in our administrative dataset that have at least some textual information, which is about 14% of our bonds.

**Supervised learning of bond categories.** To categorize bonds in our dataset, we used a supervised learning procedure to predict a bond’s SBF categories based on the unstructured text in our administrative data. 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.<sup>12</sup> 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 for which we also have information on the yes share and the proposed bond size, we have a total of 12,100 bonds.

### 3.3 Student Achievement

We rely on test scores in grade 3-8 in Math and English Language Arts (ELA) / reading as our primary measure of student achievement. Because achievement tests vary across states and years, we rely on multiple data sources and 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. We first rely on data from the Stanford Education Data Archive (SEDA)<sup>13</sup>, which begins in the 2008-09 school year. SEDA data are standardized across states and years to match moments from the National Assessment of Educational Progress (NAEP), a nationally-normed exam administered in grades 4 and 8 to a representative sample of students in each state, roughly biannually.

We combine the SEDA data with test score data collected in the early 2000s under the National Longitudinal School-level State Assessment Score Database (NLSLSASD). The NLSLSASD data contain school-level test score data by grade, subject, and subgroup for nearly every state, up to the 2004-05 school year. Data for most states begins around 2000; data is available as far back as

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<sup>12</sup>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.

<sup>13</sup>[\(Reardon et al., 2021\)](#)

1994 in some states.<sup>14</sup> For the years 2005-06 to 2007-08, we supplement with our own original data collection. We were able to collect data for nearly every state for these years via direct downloads from state websites and public data requests. In many cases, we were only able to collect district and not school-level test score data.

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, as for some years we do not have access to data disaggregated below the district level.<sup>15</sup>

### 3.4 House Prices

**Contat-Larson house price index.** We use a house price index (HPI) relying on repeat-sales aggregates 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 CoreLogic, for a total of 63 million same-unit purchase pairs. The data is available for a balanced panel of 63,122 census tracts between 1989-2021 based on 2010 census tract geography. The HPI 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 between 1989-2021.

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<sup>14</sup>See Appendix Figure A.3 for a map of the first available year of data for each state.

<sup>15</sup>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).

## 4 Mean Effects on Student Achievement and House Prices

We begin our empirical analysis by studying the average effect of the passage of a school district bond across US states and districts. We first show first-stage estimates on capital spending per pupil; then, we present reduced-form estimates on student achievement, house prices, and student sorting.

### 4.1 Empirical Framework and Research Design

Our goal is to measure the dynamic impact of bond passage on outcomes. To isolate these effects, we compare outcomes across districts that passed a bond and those that did not, in the years preceding and following an election. We estimate the following model, separately for each  $\tau$  between  $-10$  and  $5$ :

$$y_{jt} = \beta_{\tau} D_{jt-\tau} + \alpha_j + \gamma_t + \varepsilon_{jt}$$

where  $y_{jt}$  is district  $j$ 's outcome in year  $t$ ;  $\alpha_j$  and  $\gamma_t$  are district and time fixed effects; and  $D_{js} = \mathbb{1}(V_{js} \geq v)$ , where  $V_{js}$  is the share of favorable votes received by a bond proposal in the district in year  $s$  and  $v$  is the required majority to pass a bond. In this model, the parameter  $\beta_{\tau}$  captures the effect of passing a bond, conditional on district and year effects,  $\tau$  years after the election (when  $\tau > 0$ ) or  $\tau$  years before it (when  $\tau < 0$ ).

Consistently estimating  $\beta_{\tau}$  is challenging because districts that pass a bond are likely to differ from those that do not, on the basis of unobservable characteristics correlated with the outcome; in other words,  $\mathbb{E}(\varepsilon_{jt} | D_{jt-\tau}) \neq 0$ . However, as long as there is a random component of  $V_{js}$ , it is possible to estimate  $\beta_{\tau}$  by exploiting narrowly decided elections in a regression discontinuity (RD) framework. The intuition is that, since the probability of passing a bond jumps discontinuously at the (arbitrary) vote share cutoff  $v$ , districts that barely failed to pass a bond are a good control for districts that barely passed it, as long as unobservable district characteristics are continuous around the cutoff.

To implement RD, we follow [Cellini et al. \(2010\)](#). We retain all the data in the sample and absorb variation from non-close elections by using flexible controls for the vote share. Assuming

that  $\mathbb{E}(\varepsilon_{jt}|V_{jt-\tau})$  is continuous, we can approximate it with a polynomial of order  $g$  with parameters  $\delta_g, P^g(V_{jt-\tau}, \delta_g)$ . We can then consistently estimate  $\beta_\tau$  via OLS using the following model:

$$y_{jt} = \beta_\tau D_{jt-\tau} + P^g(V_{jt-\tau}, \delta_g) + \alpha_j + \gamma_t + \varepsilon_{jt}. \quad (1)$$

**Intent-To-Treat vs Treatment-on-the-Treated effects** It is possible that the odds that a given district passes a bond in  $t$  might be related to whether the district proposed or passed a bond in years prior to  $t$ ; as a result, districts in the treatment and control groups might have very different bond histories. Since the specification in equation (1) does not control for a district’s bond history, estimates of  $\beta_\tau$  should be interpreted as the combination of the effect of passing a bond in  $t - \tau$  and the effects of the district’s bond history before and after  $t - \tau$ . We refer to these as “intent-to-treat” (ITT) effects and refer to the corresponding parameters as  $\beta_\tau^{ITT}$ .

ITT estimates are useful if one is interested in the overall impact of a particular bond proposal. Alternatively, one might want to isolate the impact of bond passage on spending and outcomes holding fixed a district’s bond history, to better quantify the returns to these types of investments. We refer to these as “treatment-on-the-treated” (TOT) effects. To estimate them, we modify equation (1) as follows:

$$y_{jt} = \sum_{\tau=t-n}^{\tau=t+m} [\beta_\tau^{TOT} D_{jt-\tau} + \phi_\tau M_{it-\tau} + P^g(V_{jt-\tau}, \delta_g)] + \alpha_j + \gamma_t + \varepsilon_{jt} \quad (2)$$

where  $M_{it-\tau}$  equals one if the district held an election in  $t - \tau$ , regardless of whether the bond proposal passed or not.

In what follows, we present both ITT and TOT estimates of bond passage on capital spending, and we focus on TOT when studying the impact on student achievement and house prices. We estimate  $\beta_\tau^{ITT}$  via OLS on equation (1), separately for each  $\tau$ ; we instead jointly estimate all  $\beta_\tau^{TOT}$  using equation (2). We define  $P^g(V_{jt-\tau}, \delta_g)$  to be a linear polynomial of vote share margin. Our results are robust to the use of a quadratic or cubic polynomial. All our estimates are obtained weighting observations by student enrollment.

## 4.2 First-Stage Estimates: Impact of Bond Passage on Capital Spending

School districts that passed a bond significantly increased spending on capital projects in the years immediately following the election. ITT estimates indicate that, in the years leading to an election, spending per pupil was on similar trajectories in districts where a bond narrowly won an election (the treatment group) and those where it narrowly lost (the control group; Figure 1, panel (a), dashed series). Following the election, spending rapidly increased in districts where the bond narrowly passed relative to those where it narrowly failed, peaking at \$1,100 two years after an election and declining afterwards, to reach zero 7 years after the election.

Fully accounting for a district's bond history yield a similar picture, although with slightly different estimates. TOT estimates indicate that the difference in spending between the treatment and the control was equal to zero throughout the five years leading to an election. Following the election, spending in the treatment group rose quickly, reaching a peak of \$1,350 additional dollars per pupil relative to the control group two years after an election (Figure 1, panel (a), solid series and Table 2, column 2). The difference then declined, returning to zero six years post election. A consequence of these estimates is that the cumulative difference in spending per pupil between the two groups rose rapidly in the five years following an election and then plateaued from year 6, at approximately \$3,500 dollars per pupil (Figure 1, panel (a), thin series).

It is worth noting that, due to the rules governing current and capital spending in U.S. school districts, increases in capital spending following the approval of a bond are not accompanied by increases in other types of spending (Table 2, columns 4 and 6). This ensures that the effects of bond approval on test scores and house prices implied by the RD approach are not influenced by other changes in spending. We present these estimates next.

## 4.3 Effects on Student Achievement

The increase in capital spending following the passage of a bond may improve a student's learning experience, raising achievement. We test this hypothesis by estimating the TOT of bond passage on student test scores. We pool data from Math and ELA tests in grades 3 to 8 and augment the model in equation (2) to also control for grade-subject-state-year fixed effects. We use the number of test takers as weights.



These estimates indicate that test scores were on similar trends in districts that narrowly passed a bond proposal and those that narrowly failed it, in the years leading to an election. Following the election, test scores gradually increase in treated districts relative to the control group, peaking at a 0.07 standard deviations (sd) higher level six years after the election (Figure 1, panel (b)). On average, test scores are 0.025 sd higher 1 to 4 years after the election, 0.068 sd higher between 5 to 8 years after the election, and 0.063 higher 9 to 12 years after it (Table 3, column 2). The impact of bond passage is slightly higher for ELA (with a 0.074 and a 0.084 increase 5 to 8 years and 9 to 12 post election) compared with Math (a 0.065 and a 0.043 increase 5 to 8 years and 9 to 12 post election), Table 3, columns 3 and 4).

**Two-stages least squares effects of spending increases on test scores.** We can use the reduced-form estimates of the impact of a bond approval on capital spending and test scores to calculate the per-dollar test score returns of this type of investment. We consider the following two-equations model:

$$y_{jt} = \rho K_{jt} + \alpha_j + \gamma_t + \varepsilon_{jt} \quad (3)$$

$$K_{jt} = \sum_{\tau=t-n}^{\tau=t+m} [\gamma_{\tau}^{TOT} D_{jt-\tau} + \psi_{\tau} M_{it-\tau} + P^g(V_{jt-\tau}, \eta_g)] + \alpha_j + \gamma_t + \omega_{jt} \quad (4)$$

where  $K_{jt}$  is cumulative spending on capital in school district  $j$  over the years  $t - 10$  to  $t - 6$  (measured in \$1,000) and everything else is as before. In this model, test scores are a function of cumulative spending and cumulative spending depends on the timing of the reform. The parameter  $\rho$  captures the test score impact of a \$1,000 increase in capital spending over five years. Our choice of calculating cumulative spending over the period  $t - 10$  to  $t - 6$  is guided by the dynamics of the effects shown in panels (a) and (b) of Figure 1: Following an election, capital spending raises immediately and peaks at  $t + 2$ , whereas test scores peak at  $t + 8$ ). Other papers in this literature have made similar assumptions (Jackson and Mackevicius, 2021).<sup>16</sup>

We estimate the parameters of this model using a two-stages least squares (2SLS) approach, applying the same RD design as in equation (2) to the first stage. 2SLS estimates indicate that a \$1,000 increase in spending over a time period of five years increases test scores by 0.011 standard

<sup>16</sup>Jackson and Mackevicius (2021) argue that, because capital projects may entail disruptions to student learning due to renovation and construction works, it is sensible to capture outcomes six years after the increase in spending.

deviations later on (Table 4).

#### 4.4 Effects on House Prices

The approval of a bond in a district is usually followed by an increase in local property taxes, whose revenues are earmarked to repaid a portion or all of the debt. Changes in local taxes and in the quality of a local public good, such as school facilities, may lead households to “vote with their feet” (i.e., move in or out of the district, [Tiebout, 1956](#)) depending on their preferences for the good and their budget constraint. This sorting can affect the real estate market. Importantly, changes in house prices can be used to infer the efficiency of public good provision from the taxpayers’ perspective ([Brueckner, 1979](#); [Cellini et al., 2010](#)).<sup>17</sup> In our context, an increase in house prices following an increase in school capital spending implies that taxpayers value the capital project more than what they are asked to contribute. In this situation, spending on the public good is inefficiently low. On the contrary, the absence of a change in house prices implies taxpayers value the project just as much as they are asked to contribute. amount of money taxpayers have to contribute. Notably, taxpayers’ valuation is not limited to the impact of the project on student achievement; rather, it encompasses any benefits to the schools and the overall community.

To assess the average efficiency of capital investments in the US, we estimate equation (2) using a district-level house price index as the outcome variable. Differences in house prices between the two groups are on a flat trend in the years leading to an election. Following an election, though, the house price index progressively increases in districts where a bond narrowly passed, relative to those where it narrowly failed. House prices are 7 percent higher 9 years after an election (Figure 1, panel (c)) and 6.5 percent higher on average 9 to 12 years after. This indicates that, on average, taxpayers value school capital investments more than what they are asked to contribute in the form of increased local taxes, and that spending on capital projects across districts tends to be inefficiently low.

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<sup>17</sup>[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.

## 4.5 Effects on Student Sorting and Implications for Student Achievement

The sorting patterns induced by an increase in local property taxes might change the composition of the student body in each district.<sup>18</sup> If changes in composition are large, they could explain part or even all of the increases in test scores found above. To examine this possibility, the ideal test would track students over time and across school districts in the aftermath of a bond passage and quantify the prevalence of district changes. In the absence of the student-level data necessary to perform this test, we re-estimate equation (2) using as the dependent variable the share of students in various socio-demographic groups. If household sorting were prevalent, it could lead to systematic changes in the share of students belonging to each of these groups and in enrollment.

These estimates confirm that the passage of a school bond in a district is followed by a gradual compositional change in the socio-demographic composition of the student body. Nine to 12 years following an election, the share of White students is 1.1 percentage points higher seven years after the election (1.5 percent relative to an average share of 0.73) in districts that barely approved a bond compared with those that barely rejected it (Figure 1, panel (d), and Table 3, column 6). Similarly, the share of high-socioeconomic status (SES) students is 3 percentage points higher (5 percent relative to an average share of 0.58, Figure 1, panel (d), and Table 3, column 7). Notably, total enrollment remains unchanged (Table 3, column 5). These patterns are in line with the hypothesis that increases in school capital spending lead to household sorting.

Since different socio-demographic groups of students tend to have different achievement, it could be the case that part or all of the test score effects presented above are driven by these compositional changes. To assess whether this is the case, we re-estimate equation (2) using test scores as the dependent variable and controlling for the share of White and high-SES students in each district and year. Accounting for compositional changes leads to a more muted impact of bond passage on test scores, equal to 4.2 sd in the 5 to 8 years following an election (Table 3, column 9). This effect is about 60 percent of the estimate we obtained not controlling for composition (Table 3, column 2). Compositional changes thus can account for part, but not all, of the test score increases occurring in a district in the aftermath of a bond passage.

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<sup>18</sup>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).

## 4.6 Robustness

**Stacked difference-in-differences** Our main estimates are obtained from a dataset in which one observation is a district in a given year. As an alternative, we also perform a stacked-regression analysis following the approach used by [Cengiz et al. \(2019\)](#). This approach consists of the following steps: (i) we create cohort-specific sub-datasets, one per election year, each containing all the districts with a successful election in that year (treated) and all the districts with an unsuccessful election in the same year and who did not have a successful election in the years surrounding the reference election year (“clean” controls); (ii) we stack these datasets, lining them up according to the relative time indicators; (iii) we re-estimate equation (2) on this stacked dataset, interacting all fixed effects and controls with sub-dataset indicators and using only years for which we have a clean control. Estimates obtained with this approach indicate that our results are robust to the use of stacked datasets and clean controls (Appendix Figure A.4).

## 5 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 an improvement in test scores and an increase in house prices. These findings, though, are obtained by pooling together data from districts that serve different populations of students and face disparate rules that govern how money can be raised and spent. Because of these differences, mean effects could mask important heterogeneity across districts and types of bonds. Notably, this heterogeneity could explain why some previous studies (such as [Cellini et al., 2010](#); [Martorell et al., 2016](#); [Hong and Zimmer, 2016](#); [Conlin and Thompson, 2017](#); [Baron, 2022](#)), which have used data from individual states, have found much more muted effects of capital spending on test scores and house prices.

We define, and test for, two types of heterogeneity. The first is *treatment effect heterogeneity*: An investment of the same size and on the same items could have different returns in different contexts. For example, it could depend on the socio-demographic composition of the student body, if some students benefit more than others from capital investments. It could also depend on the existing funding rules, which may make it more or less difficult for a district to raise money for a capital project. The second is *treatment heterogeneity*: Bonds may differ greatly in size (i.e., the

proposed spending amount) and composition (i.e., the items to be financed). These differences could lead bonds to have disparate impacts on students and the real estate market. Importantly, these two sources of heterogeneity are strictly intertwined. The student population a district serves and the funding rules it faces may directly affect the types of bond it chooses to propose.

In this section, we explore these sources of heterogeneity in more detail by re-estimating the effects of bond passage on groups of districts with different characteristics and proposing bonds of different type. We also discuss the interpretations of our findings from a causal inference standpoint.

## 5.1 Treatment Effect Heterogeneity: Student Socio-demographics and Funding Rules

**Student socio-demographics.** We begin by examining whether the impact of bond approval differs across districts that serve different populations of students. We group districts according two characteristics: the share of economically disadvantaged students (defined as those who are eligible for a free or reduced-price lunch) and the share of minority students (Black and Hispanic), both measured in 1995. To explore heterogeneity, we estimate equation (2) separately for districts in the top and bottom terciles of the distribution of these two shares. We first estimate effects on capital spending (the first stage); then, we estimate effects on test scores, the house price index, and the share of non-disadvantaged students as a measure of sorting.

In the short run (i.e., two years after an election), capital spending increases by over \$1,500 in districts in the top tercile of the share of disadvantaged students and only \$700 in districts in the bottom tercile (Figure 2, left panel (a)). Although smaller in the short run, the spending increase in the bottom tercile is longer-lived. Four years after an election, spending is still \$600 higher in these districts compared to pre-election levels, whereas it has returned to pre-election levels in districts in the top tercile. These time patterns imply that the cumulative spending increase is the same across the two districts in the five years following an election and higher in districts in the top tercile in the following ten years, equal to \$3,500 (compared with \$2,000 in districts in the bottom tercile).

In spite of similarly sized spending increases, the impacts of bond passage on student achievement and the real estate market differ vastly across these groups of districts. Following an election, test scores increase rapidly in districts with a share of disadvantaged students in the top tercile, reaching a 0.12 higher sd eight years after an election (Figure 2, left panel (b)). Instead, test scores

do not change in districts in the bottom tercile. 2SLS estimates imply that a \$1,000 increase in spending over a time period of five years increases test scores by 0.013 standard deviations in districts in the top tercile (Table 4, column 7), while it does not produce any detectable changes in test scores in districts in the bottom tercile (Table 4, column 6). Mirroring this pattern, house prices increase steadily in districts in the top tercile, reaching a 12 percent higher level eight years after an election (Figure 2, left panel (c)). They instead remain unchanged in districts in the bottom tercile.

The changes in student composition are also more prevalent among districts in the top tercile, where the share of non-disadvantaged students is 10 percentage points higher eight years following an election but does not change in districts in the bottom tercile (Figure 2, left panel (d)). This implies that part of the increases in test scores is due to changes in the composition of student the student body, rather than to improved learning. Controlling for the shares of students in the various demographic groups, the increase in test scores in districts in the top tercile is equal to 0.10 eight years after an election (Appendix Figure A.5, panel (b)).

All these findings are largely unchanged if we bin districts into terciles according to the share of minority students (Figure 2, right panels). Taken together, these results indicate that the impact of school capital spending on student learning is much larger in districts serving less advantaged populations of students. These investments are also valued more in these districts, generating sizable increases in house prices.

**Funding rule I: Electoral majority requirements.** The impact of passing a bond may also differ depending on the funding rules each district faces. Stricter rules, such as a supermajority electoral requirement, may increase the value of the marginal investment in terms of learning and house prices, for example by leading districts to prioritize projects with higher returns. We now explore differences in the impact of bond approval across districts that require a simple majority and those that require a supermajority.

Our data indicate that, in the short run, the capital spending increase following the approval of a bond is higher in simple-majority states; in supermajority states, though, the spending increase lasts longer. Two years after the election, spending is approximately \$1,600 higher in simple-majority states and \$900 higher in supermajority states, compared to pre-election levels (Figure 3, left panel (a)). Five years after the election, spending is still \$1,000 higher in supermajority districts, whereas it

is indistinguishable from pre-reform levels in simple-majority districts. As a result, the cumulative spending increase in the five years following an election is \$4,000 in supermajority districts and only \$2,800 in simple-majority districts.

The larger spending increase experienced by supermajority districts translates into a much larger improvement in student learning. In these districts, test scores are 0.17 sd higher six years after an election compared with before (Figure 3, left panel (b)). Test scores remain instead unchanged in simple-majority districts. Rescaling these estimates by the increase in capital spending in the five years post election implies that a \$1,000 spending increase leads to a 0.012 sd increase in test scores in supermajority districts (Table 4, column 3) and a smaller and insignificant 0.006 increase in simple-majority districts six years after an election (Table 4, column 2).

A portion of the increase in test scores experienced by supermajority districts can be attributed by changes in the composition of the student body: In these districts, the share of non-disadvantaged students increases by 10 percentage points eight years after an election (Figure 3, left panel (d)). The increase in test scores in these districts, though, remains visible even if we control for the share of students in the various demographic groups (Appendix Figure A.5, panel (c)). Following a bond approval, house prices dramatically increase in supermajority districts, reaching a 10 percent higher level ten years post election (Figure 3, left panel (c)). They instead remain unchanged in simple-majority districts.

**Funding rule II: State share of total spending.** Another funding detail that might affect the impact of bond approval is the share of total capital spending that is born by the state. Districts that receive larger transfers from the state to pay for capital projects face a looser budget constraint for a given amount of local spending. On the one hand, this may induce them to undertake bigger projects with higher per-dollar returns. On the other hand, it may also lead them to invest money on projects that are less essential for student learning. The relationship between the state share of capital spending and the test score and house price increases following a bond approval is thus ambiguous ex ante. To investigate it, we re-estimate equation (2) separately on districts in the top and bottom terciles of the distribution of the state share.

RDD estimates indicate that districts in the bottom tercile experience a much greater increase in capital spending following the passage of a bond. In these districts, spending is \$2,300 higher two

years after the election compared with before and it quickly returns to pre-election levels by year 5 post election (Figure 3, right panel (a)). Spending is instead only \$750 higher in districts in the top tercile two years after the election, although it returns to zero more gradually. Cumulatively, the spending increase in the five years following an election is \$5,500 in districts in the bottom tercile and \$3,000 in districts in the bottom tercile. A possible explanation for this finding is that districts that do not receive large transfers from the state make up for this by proposing larger bonds and, as a result, capital investments are “lumpier” in these districts and more spread out over time in districts with a high state share.

In spite of this, it is districts in the top tercile of the state share that experience the greatest increases in test scores and house prices following an election. In these districts, test scores are 0.14 sd higher eight years following an election (compared with 0.5 sd higher in districts in the bottom tercile). 2SLS estimates imply that a \$1,000 increase in spending 6 to 10 years prior leads to a 0.013 increase in test scores in districts in the top tercile, whereas it does not produce any test score changes in districts in the bottom tercile (Table 4, columns 4 and 5).

A portion of the increase in test scores can be attributed by changes in the composition of the student body: In districts in the top tercile, the share of non-disadvantaged students increases by 4 percentage points eight years after an election (Figure 3, right panel (d)), whereas it declines slightly in districts in the bottom tercile. The increase in test scores in these districts, though, remains visible even if we control for the share of students in the various demographic groups (Appendix Figure A.5, panel (d)). Similarly, house prices are nearly 10 percent higher in districts in the top tercile ten years after an election compared with before, while they are—if anything—slightly lower in districts in the bottom tercile (Figure 3, panel (c)).

Taken together, these findings indicate that the stringency of the funding rules affects the returns from passing a bond. These returns are significantly larger in districts which face more stringent rules, such as a supermajority electoral rule and more oversight from the state due to a higher state share. A possible explanation for this heterogeneity in impacts is that districts facing different rules spend money on different capital projects, which have different returns. We examine this possibility next.



## 5.2 Treatment Heterogeneity: Bond Composition

Not all school capital investments are created equal. For example, they may be targeted towards different projects, such as the construction of a new school building, the development an athletics facility, or the renovation of existing facilities. These differences could generate disparities in the impact of bond approval on achievement and house prices. We explore this possibility by allowing the impact of a bond to vary by type of financed projects (which we call “composition”). We focus on seven spending items: the construction and renovation of classrooms and buildings (including those made to alleviate overcrowding); the purchase of land; improvements to HVAC systems and to other types of infrastructures (including plumbing, furnishing, and roofs); improvements to school health and safety (such as the removal of asbestos or lead paint and the upgrade of fire and earthquake safety systems); the acquisition or upgrade of IT equipment and the furnishing of laboratories (which we denote as STEM); and the construction or renovation of athletic facilities.

To estimate the impact of bond passage specific to each category, we compare outcomes of districts which proposed and marginally approved a bond with a spending item  $k$  in election year  $c$  with outcomes of districts who (i) also proposed a bond with item  $k$  at any point in time between  $c - 5$  and  $c + 12$ , but did not pass it; and (ii) did not pass any other bonds between  $c - 5$  and  $c + 12$ . This strategy allows us to compare our RDD design with the “clean controls” approach proposed, among others, by [Cengiz et al. \(2019\)](#) and [Dube et al. \(2022\)](#). We perform this comparison by constructing a stacked dataset, in the spirit of [Cengiz et al. \(2019\)](#). We combine all “treated” districts which passed a bond with item  $k$  in cohort  $c$  with their clean controls, observed between  $c - 5$  and  $c + 12$ , and stacking all the treated and control districts for each  $k$  and  $c$ . On this dataset, we then estimate the following model, separately for each item  $k$ :

$$y_{jct} = \sum_{\tau=t-12}^{\tau=t+5} \left[ \zeta_{k\tau} D_{jct-\tau}^k + \phi_{\tau}^k M_{ict-\tau} + P^g(V_{jct-\tau}, \delta_g^k) \right] + \alpha_{jc} + \gamma_{s(j)ct} + \varepsilon_{jct} \quad (5)$$

where  $\alpha_{jc}$  denotes cohort-specific district fixed effects and  $\gamma_{s(j)ct}$  are cohort-specific state-by-year fixed effects. In this model, the parameters  $\zeta_{k\tau}$  capture the differential change in the outcome variable  $\tau$  years since the election for districts which barely approved a bond that includes item  $k$ , relative to districts that barely failed to approve a bond that includes  $k$ . We estimate two versions of equation (5) for each item, using test scores and house prices as the dependent variable. To max-

imize precision, we present and discuss linear combinations of the parameters  $\xi_{k\tau}$  for  $\tau$  between +5 and +12.

Estimates from this model indicate that the passage of bonds targeted to fund HVAC systems lead to largest increase in test scores, equal to 0.22 sd (Figure 4, orange bars). This is consistent with existing research showing that air conditioning can mitigate the learning losses caused by heat (Park et al., 2020). Renovations of other types of infrastructure and health and safety improvements also bring sizable increases in learning, equal to 0.12 sd each. The construction of athletic facilities, the construction and renovation of classroom and building facilities, and the upgrade of STEM facilities lead to smaller increases in test scores, equal to 0.07 sd, 0.07 sd, and 0.04 sd respectively. Lastly, spending on transportation and land purchases lead to zero or even negative (although insignificant) effects on test scores.

Notably, the bond categories that lead to the largest test score changes are different from the categories followed by the largest house price increases. The category that stands out in terms of house price increases is the construction and renovation of athletic facilities, followed by a 9 percent increase (Figure 4, blue bars). Other categories lead to smaller and imprecise increases in house prices, for example equal to 6 percent for HVAC systems and 5 percent for classroom building and renovations. Health and safety improvements and the upgrade of STEM facilities lead to zero or even negative changes in house prices.

These differences suggest that the extent to which a community values a school capital investment may go above and beyond any improvements in learning. When interpreting these results, though, it is crucial to keep in mind that the choice of proposing bonds targeting different categories may be related to the composition of the district's student body, prior history of investments, and the funding rules which—as we showed—are all sources of treatment effect heterogeneity as well. To better understand how these dimensions relate to each other in producing learning and house price changes, we resort to a model.

## 6 Explaining Differences: Governance of Capital Investments

The goal of the model is to illustrate how the characteristics of school district bonds that get approved, including amount and spending items, are determined in equilibrium given the demo-

graphic characteristics of the district and the funding rules it has to abide to. We model this process as a game between each district and heterogeneous households, who vote in favor or against the bond proposal given their preferences. From a household’s perspective, the key tradeoff involves the benefits of passing larger bonds (which are akin to a “better” public good) against the costs of higher taxes. We use the model for two purposes: (i) as a theoretical framework to examine comparative statics, and (ii) as the foundation for the structural estimation of the bond determination process, which allows us to investigate the impact of counterfactual funding rules on the characteristics of approved bonds in equilibrium and, in turn, on students and communities. We outline here the main model ingredients; technical details are described in the Appendix.

## 6.1 Setup

**Bond size and composition** A district considers issuing a bond with size (i.e., per pupil spending increase)  $x \geq 0$  and composition  $c \in [0, 1]$ . For a given  $x$ ,  $c$  summarizes the impact of the capital investment on amenities for the broader community, relative to its impact on student achievement. A bond with composition  $c = 0$  only contains spending items that improve achievement and have no impact on amenities (such as investments on HVAC systems); a bond with  $c = 1$  only contains items that improve amenities but have no impact on achievement (such as the construction of a stadium).

The passage of a bond with size  $x$  and composition  $c$  improves achievement by  $s(x, c)$  and increases amenities by  $a(x, c)$ . For a given  $c$ , a larger bond improves both student achievement and amenities at diminishing rates (i.e.,  $s_x(x, c) > 0$  and  $s_{xx}(x, c) < 0$ ;  $a_x(x, c) > 0$  and  $a_{xx}(x, c) < 0$ ). These rates of return may both implicitly depend on district characteristics. By definition, for a given size, an increase in  $c$  decreases  $s(x, c)$  and increases  $a(x, c)$ .

**Funding rules** To issue a bond, each district must receive electoral approval from at majority  $v \in [0.5, 1)$  of voters. When issuing a bond of size  $x$ , districts receive a transfer from the state that matches a share  $\lambda$  of  $x$ . This matching rate may depend on district characteristics  $w$ . We assume that the district is small relative to the state, so that the statewide taxes required to fund the state are negligible from the perspective of the households in the district.

**Household preferences** Each district contains a unit interval of households, indexed by  $i$ , who value consumption, amenities, and student achievement. Improving achievement and amenities through larger bonds comes at the cost of reduced private consumption due to higher taxes. As we show in the Appendix, this tradeoff can be represented by a household-specific indirect utility function  $u_i(x, c)$ , which depends only on bond size and composition and which we normalize by setting  $u_i(0, c) = 0$  for any  $c$ . For a given  $c$ , utility is increasing in  $x$  up to the point where the loss in private consumption outweighs the gains from higher investments; after that point, utility decreases in  $x$ . For a given  $x$ , utility rises with  $c$  as long as the marginal utility from student achievement is larger than the marginal utility of amenities (i.e., as long as  $s_c(x, c) \geq a_c(x, c)$ ), after which utility decreases with  $c$ .

We build on the political economy literature of school funding (Romer and Rosenthal, 1979, 1982; Grosz and Milton, 2022) and assume that household preferences can be summarized by the distribution of  $\tilde{x}_i(c)$ , the *maximally acceptable bond* each household  $i$  would accept for a given composition  $c$ :

$$\tilde{x}_i(c) \equiv \max \{x : u_i(x, c) \geq 0\} \sim \mathcal{N}(\mu(c, w, \lambda), \sigma_x(w)).$$

When  $x = \tilde{x}_i(c)$ , household  $i$  is indifferent between the proposed bond and no bond at all ( $x = 0$ ). When  $x > \tilde{x}_i(c)$ ,  $i$  prefers  $x = 0$ . We assume that maximally acceptable bonds are normally distributed with mean  $\mu(c, w, \lambda)$  and standard deviation  $\sigma_x(w)$ . The mean (which is also the maximally acceptable bond of the median household due to a symmetric distribution) depends on bond composition, district characteristics, and the state match rate. For example, more amenity-focused bonds may increase or decrease the mean, depending on voter preferences; and more state support may temper the appetite for locally financed bonds. The standard deviation of household preferences may depend on district characteristics as well. For example, larger districts may have a wider distribution of maximally acceptable bonds.

**District characteristics and objective** Each district is characterized by a vector  $w$ , which includes demographic and fiscal characteristics, the state of school infrastructure, and the timing and success of recent bond elections. District value both amenities and student achievement, but potentially with different weights than households. Following the literature, we assume that, unlike households, districts always prefer larger bonds as they seek to maximize their budget for a given  $c$ .

We capture a district's objective with a function  $\Pi(x, c)$ , monotonically increasing in  $x$ , weakly concave in  $x$  and  $c$ , and with weak complementarity between  $x$  and  $c$ . Notably, this function nests the special case of a district that simply seeks to maximize bond size,  $\Pi(x, c) = x$ , irrespective of its composition.

## 6.2 Bond Size and Composition in Equilibrium

Bonds are determined in three stages, which we summarize in Figure 5. In the first stage, the district decides whether to propose a bond (in which case it moves to the second stage) or not (in which case the game ends). In the second stage, the district determines the size and composition of the bond. Finally, in the third stage, households vote on the bond proposal; if a sufficient share of them agrees with the proposal, the district issues the bond, it implements the planned investments, and the achievement and amenity effects occur. We now describe these stages in reverse order.

**Stage 3: Voting** For simplicity, we assume that every household turns out to vote. Given a bond proposal  $(x, c)$ , each household  $i$  votes yes if and only if  $x \leq \tilde{x}_i(c) + \eta$ , where  $\eta \sim \mathcal{N}(0, \sigma_\eta(w))$ . In words,  $i$  votes yes if the proposed bond is weakly smaller than her maximally acceptable bond, with some uncertainty (such as a news shock about a corruption scandal involving district officials, akin to  $\eta < 0$ , or the discovery of asbestos in school facilities, akin to  $\eta > 0$ ).

Given this uncertainty, the share of yes votes is

$$V(x, c) = \Pr(x \leq \tilde{x}_i(c) + \eta) = \Phi\left(\frac{\mu(c, w, \lambda) - x + \eta}{\sigma_x(w)}\right) \quad (6)$$

and the probability of passage is

$$P(x, c) = \Pr(V(x, c) \geq v) = \Phi\left(\frac{\mu(c, w, \lambda) - x - \sigma_x(w)\Phi^{-1}(v)}{\sigma_\eta(w)}\right),$$

where  $\Phi(\cdot)$  is the cumulative density function of a standard normal and  $v \in [0.5, 1)$  is the majority requirement. Intuitively, a bond is more likely to pass if mean household preferences for bonds are larger, if the bond is smaller, and if the supermajority requirement is lower.

**Stage 2: Bond design** The district chooses the size and composition of the bond in anticipation of the outcomes at the voting stage; importantly, the district only knows the distribution of  $\eta$  but not its realization. The district maximizes the expected value of its objective function:

$$(x^*, c^*) = \arg \max_{x \geq 0, c \in [0,1]} P(x, c) \Pi(x, c),$$

where  $(x^*, c^*)$  is the equilibrium bond.

The first-order conditions (FOC) with respect to size and composition characterize the tradeoffs the district considers in designing the bond. Assuming interior solutions exist, they can be written as

$$\psi(x, c) = \pi_x(x, c) \tag{7}$$

and

$$\psi(x, c) = -\frac{\pi_c(x, c)}{\mu_c(c, w, \lambda)}, \tag{8}$$

where  $\psi(x, c) \equiv \phi(z(x, c)/\sigma_\eta(w))/\{\Phi(z(x, c))\sigma_\eta(w)\}$  is monotonically decreasing in  $z(x, c)$  and captures the marginal probability of passage; and  $z(x, c) \equiv \mu(c, w, \lambda) - x - \sigma_x(w)\Phi^{-1}(v)$  is the *wedge* between the preferences of the pivotal voter and the proposed bond size. The terms  $\pi_x(x, c) \equiv \Pi_x(x, c)/\Pi(x, c)$  and  $\pi_c(x, c) \equiv \Pi_c(x, c)/\Pi(x, c)$  are *district elasticities* with respect to size and composition. Finally,  $\mu_c(c, w, \lambda)$  is the *voter elasticity* with respect to composition.

Equation (7) captures the balance between household and district preferences over equilibrium bond size. The district balances the marginal costs of a larger bond (left-hand side), due to the threat of failed passage, against its marginal benefits (right-hand side). Marginal costs are lower when aggregate uncertainty is larger (i.e., larger  $\sigma_\eta(w)$ ). Intuitively, increasing bond size is less costly when the election outcome is primarily driven by unpredictable features that are not related to bond characteristics. Marginal costs are also lower when the wedge  $z(x, c)$  between the proposed size and the voter-desired size is larger: if the district allows for more “wobble room”, increasing bond size is less likely to derail bond passage. Notably, the wedge itself depends on how much appetite households have for larger bonds. Since  $\pi_x(x, c)$  decreases with  $x$  (due to concavity), small marginal costs are associated with large equilibrium bonds.

The equilibrium bond will be generally smaller than the median household’s maximally accept-

able bond  $\mu(c, w, \lambda)$ , so  $z(x, c) > 0$ . This is due to two features of the environment: first, the district hedges against the aggregate uncertainty by proposing a smaller bond; and second, a supermajority requirement drives the district to target a higher-quantile voter (i.e. one more skeptical towards bonds) than the median household.

Equation (8) illustrates the balance between the competing preferences of households and the district on the bonds' composition. Strikingly, district and voter elasticities with respect to composition have the opposite sign in equilibrium. This implies that, from the perspective of the median voter, the equilibrium bond is focused either too much or too little on amenities, depending on whether the voters or the district have a stronger or weaker taste for amenities.

Figure A.6 illustrates the FOC with respect to bond size for the simple case of a bond-maximizing district (i.e.  $\Pi(x, c) = x$ ). In the left panel, the equilibrium bond is lower than the pivotal voter's preferred bond size because the district hedges against the aggregate uncertainty in the share of yes votes. In the right panel, the equilibrium is determined by the crossing of the marginal benefit line (the probability of increasing the bond by one dollar) and the marginal cost line (the marginal probability of the bond getting rejected at the ballot box).

**Stage 1: Bond proposal** The district proposes a bond only if the expected associated benefits  $P(x, c)\Pi(x, c)$  are larger than the cost of proposing a bond  $\chi(w) \sim F(\cdot|w)$ . This stochastic component captures logistic, administrative, or reputation costs. Letting  $D = 1$  if the district proposes a bond and 0 otherwise and  $\rho(w) = \Pr(D = 1|w)$ , the probability of bond proposal is

$$\rho(w) = F(P(x, c)\Pi(x, c)|w).$$

Thus, a district is more likely to propose a bond if the expected benefits are high and/or when the probability of passage is high. Crucially, these benefits depend on district characteristics.

### 6.3 Comparative Statics

**Bond size and composition** How do bond characteristics and outcomes depend on the supermajority requirement  $v$  and the state match rate  $\lambda$ ? Starting with bond size and composition, the equilibrium bond size is lower if the supermajority requirement is higher  $\partial x^*/\partial v < 0$ . The intuition is simple: a higher supermajority requires the district to persuade a larger share of voters, which it

does (*ceteris paribus*) by proposing a smaller bond. Table 5 provides evidence that, indeed, larger bonds are associated with smaller yes shares, even within the same district over time.

The composition of the equilibrium bond becomes more amenity-focused with a higher supermajority requirement if voters have a stronger preference for amenities (relative to achievement) than districts. The intuition is that districts will compromise more on their preferred amenity mix so they do not have to give up as much in terms of bond size. Hence, increasing the supermajority requirement may have the consequence that the district changes its investment focus towards or away from student achievement, depending on its preferences relative to voters.

Turning to the role of state contributions, the sign of  $\partial x^*/\partial \lambda$  depends on the sign of  $\partial \mu(c, w, \lambda)/\partial \lambda$ : if voters are willing to accept larger bonds if the state match rate is higher, then equilibrium bonds will indeed be higher. Similarly, higher state contributions may increase or decrease the amenity focus of the equilibrium bond, depending on relative preferences of the district and voters.

**Student achievement and amenities** Given these comparative statics on bond characteristics, we can also study the implied impacts of changes in  $v$  and  $\lambda$  on student achievement and amenities. A higher supermajority requirement leads to lower student achievement,  $\partial s(x^*, c^*)/\partial v < 0$ , unless the equilibrium bond becomes substantially more learning-focused due to much stronger voter preferences than district preferences for achievement. The impact on amenities is also negative,  $\partial a(x^*, c^*)/\partial v < 0$ , unless equilibrium bond sizes are only slightly smaller and voters are substantially more amenity-focused than districts.

A higher state contribution improves student achievement,  $\partial s(x^*, c^*)/\partial \lambda > 0$ , if it increases voter preferences for bond size  $\partial \mu(c, w, \lambda)/\partial \lambda > 0$ . Changes in amenities depend again on relative preferences.

## 6.4 Identification and Estimation

We begin by assuming that  $\Pi(x, c) = x^{\tilde{\beta}}$  with  $\tilde{\beta} = \beta + \epsilon$  and  $\beta > 0$ , so that districts care only about the size of the bond, but the marginal return to larger bonds may be decreasing or increasing.  $\epsilon$  is a mean-zero term capturing bond-specific variation in the return to size that is unobserved to the econometrician. The key parameters of the model are then: the median voter's maximally acceptable bond  $\mu(c, w, \lambda)$ ; the dispersion of voters' maximally acceptable bonds  $\sigma_x(w)$ ; and the



marginal return  $\beta$ . We discuss what variation identifies each of these parameters in turn.

**Median voter preferences ( $\mu(c, w, \lambda)$ ) and dispersion ( $\sigma_x(w)$ )** Median voter preferences and their dispersion are identified off of voter support for bonds of varying sizes at the ballot box. Specifically, we can observe the joint distribution of the share of yes voters, bond size, and district characteristics due to equation (6). In this equation  $\sigma_x(w)$  is identified as the loss in voter support due to an increase in bond size. Inverting the Normal CDF and taking expectations, we can then rewrite it as

$$\mu(c, w, \lambda) = \sigma_x(w) \mathbb{E} [\Phi^{-1}(V(x, c))] + x,$$

where we used the fact that  $\mathbb{E}[\eta] = 0$ . This equation shows that the median voter's maximally acceptable bond size  $\mu(c, w, \lambda)$  is identified as the sum of the actual proposed bond size  $x$  and a term that captures the extent of voter support for the bond beyond the median  $\mathbb{E}[\Phi^{-1}(V(x, c))]$ , scaled by the standard deviation of median voter preferences. Intuitively, if the distribution of voter preferences in the district is very wide (i.e. large  $\sigma_x(w)$ ), then a given change in bond size sways a smaller share of voters. In this case, any vote margin beyond the median (i.e.  $\mathbb{E}[\Phi^{-1}(V(x, c))] > 0$ ) must indicate a bond preference of the median voter that is far larger than the proposed bond  $x$ .

We can estimate these parameters directly via OLS. Concretely, by inverting the Normal CDF in equation (6), we arrive at a (population) regression equation of the form

$$\Phi^{-1}(V(x, c)) = \sigma_x^{-1}(w) \mu(c, w, \lambda) - \sigma_x^{-1}(w) x + \sigma_x^{-1}(w) \eta.$$

On the left-hand side is the transformed share of yes voters, which we regress on variables capturing bond composition, district characteristics, and state characteristics corresponding to  $\sigma_x^{-1}(w) \mu(c, w, \lambda)$  as well as bond size  $x$  interacted with district characteristics.  $\sigma_x^{-1}(w) \eta$  is mean zero and can be treated as an error term.

**District preferences ( $\beta$ )** Note that the district elasticity with respect to size is given by  $\pi_x(x, c) = \beta/x$ . The parameter  $\beta$  is identified off of the risk-return tradeoff the district strikes. Define the moment

$$H(x, c) = \mathbb{E}[x\psi(x, c) - \beta], \tag{9}$$

which is zero whenever the FOC with respect to size in equation (7) holds. Hence, if we observe districts taking little risk at the ballot box (i.e. large  $\psi(x, c)$ ) and proposing large bonds, then we infer that their marginal valuation of bonds is high. Conversely, if they take substantial risk of ballot failure and propose small bonds, their marginal valuation is low.

We estimate this parameter via Generalized Method of Moments (GMM) using the moment defined in equation (9). To ensure that standard errors take into account the uncertainty from both estimation steps, we estimate bootstrap standard errors wrapped around both steps.<sup>19</sup>

## 6.5 Parameter Estimates

Table 6 shows results from the estimation of the voting stage and the bond design stage. We can see that estimates of  $\beta$  are between zero and one for all states, meaning that the return to bond size is increasing and concave. The rate of concavity, which can be interpreted as a measure of district preference for large bonds, is quite different from one state to the next. States like Michigan and Ohio have a rate near one, implying that the marginal return of increasing bond size is nearly constant. In other words, districts in these states have a strong appetite for large bonds. In contrast, the rate for New York is close to zero, meaning that districts in these states see little value in designing large bonds. Indeed, bonds in New York are very small, only \$355 per pupil on average, whereas in Michigan the average bond is more than \$6,000 per pupil.

Turning to estimated voter preferences, we can see that average preferences  $\mu(c, w, \lambda)$  also differ substantially across states: California districts have the largest maximally acceptable bonds, with an average of around \$23,300 per pupil, and Michigan has the smallest, with \$8,800. At first blush, it is surprising for a state like Idaho to have large average in estimated maximally acceptable bond. But bonds in Idaho are larger than those in Ohio, yet they still garner two-thirds majorities on average whereas those in Ohio barely achieve simple majorities.

Finally, looking at the dispersion of voter preferences,  $\sigma_x(w)$ , we see ranges between \$16,100 in Georgia and \$44,900 in California. This parameter can also be interpreted as the inverse elasticity of yes shares with respect to bond size, which implies that voters in Georgia are much more sensitive to bond size than those in California.

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<sup>19</sup>Alternatively, we could have expressed the OLS component as another set of moments and estimated a single GMM, which would be equivalent with the two-step procedure conducted here.

## 7 Conclusion

This paper has provided the first near-nationwide estimates of the impact of school capital investments on student learning and the real estate market in the U.S. Using variation from closely decided elections on school bonds, aimed at financing capital projects, we have shown that the approval of a bond increases test scores by 0.06 sd and raises house prices by 7% in the average school districts. 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 increase in local taxes, which in turn suggests that these investments tend to be too low.

We have also demonstrated, however, that these average impacts mask important variation across districts, which can be traced back to differences across districts and states in the demographics of the student body and the rules disciplining the funding of school capital projects. These differences influence the size and type of projects districts choose to finance. One striking result of our analysis is that states that require an electoral supermajority to approve bonds see the largest increases in test scores and house prices, which in turn suggests that these stringent rules keep capital spending at an inefficiently low level.

To interpret these estimates and understand how cross-district differences impact the returns to capital spending, we have used a probabilistic voting model. The model features districts deciding the proposed spending amount and the types of projects to finance, and voters choosing whether to approve a bond proposal of a particular size and composition. Using the model, we explore whether the proposed bond size, the type of capital projects that get financed, and the impacts on student learning and house prices would be different financing rules were to be made more lenient (for example by reducing the required electoral majority). Comparative statics suggest that more lenient rules would increase the size of the proposed bonds, with ambiguous effects on the type of financed projects.

An additional result from our analysis is that the increase in school capital spending following the approval of a bond changes the composition of the student body of each district via household sorting, to an extent that also depends on funding rules. In particular, an increase in spending is followed by a reduction in the share of economically disadvantaged and minority students. While these compositional changes cannot explain all the increase in test scores, they suggest that the

benefits of school spending increases might increase segregation across school districts and disproportionately benefit certain portions of the population. A rigorous quantification and a study of the implications of sorting in response to increases in local school capital spending, and the way in which these interact with funding rules, represent a fruitful area for future research

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

	Full sample	% low-SES students		Spending rules			
		≤33 pct	>66 pct	Majority requirement		State share	
				50%	>50%	≤33 pct	>66 pct
<i>Expenditure per pupil (\$)</i>							
Capital	1286.8 (2843.1)	1361.1 (2514.9)	1191.4 (2956.2)	1337.2 (2979.2)	1128.7 (2359.4)	1085.3 (2200.5)	1465.8 (3344.7)
Current	6964.2 (3884.9)	7220.7 (2886.0)	6562.9 (2696.6)	7236.0 (4138.0)	6112.8 (2788.1)	6813.3 (2693.2)	6931.8 (3250.1)
<i>Spending rules</i>							
State share, capital exp	0.096 (0.19)	0.11 (0.27)	0.094 (0.15)	0.089 (0.20)	0.12 (0.15)		
Share w /supermajority	0.24 (0.43)	0.17 (0.38)	0.36 (0.48)			0.19 (0.40)	0.29 (0.45)
Voting requirement	0.52 (0.047)	0.52 (0.040)	0.53 (0.053)	0.50 (0)	0.60 (0.041)	0.52 (0.046)	0.53 (0.048)
<i>Bonds</i>							
Share proposing a bond/year	0.061 (0.24)	0.080 (0.27)	0.059 (0.23)	0.066 (0.25)	0.046 (0.21)	0.054 (0.23)	0.056 (0.23)
Share approved	0.75 (0.43)	0.73 (0.44)	0.78 (0.42)	0.75 (0.43)	0.75 (0.43)	0.63 (0.48)	0.79 (0.40)
Vote margin above threshold	0.100 (0.16)	0.076 (0.13)	0.12 (0.17)	0.11 (0.16)	0.065 (0.13)	0.040 (0.14)	0.11 (0.15)
Size p.p. proposed (\$1,000)	7.84 (8.24)	7.83 (8.01)	7.60 (8.01)	7.82 (8.32)	7.91 (7.91)	8.47 (7.34)	9.50 (9.04)
<i>Categories, approved bonds</i>							
Classrooms	0.43 (0.50)	0.37 (0.48)	0.58 (0.49)	0.36 (0.48)	0.69 (0.46)	0.43 (0.50)	0.69 (0.46)
STEM equipment	0.27 (0.44)	0.26 (0.44)	0.31 (0.46)	0.19 (0.39)	0.53 (0.50)	0.36 (0.48)	0.36 (0.48)
HVAC	0.11 (0.32)	0.10 (0.30)	0.15 (0.36)	0.061 (0.24)	0.31 (0.46)	0.091 (0.29)	0.19 (0.39)
Other infrastructure	0.26 (0.44)	0.27 (0.45)	0.28 (0.45)	0.20 (0.40)	0.48 (0.50)	0.42 (0.49)	0.29 (0.45)
Safety/health	0.20 (0.40)	0.16 (0.37)	0.28 (0.45)	0.11 (0.31)	0.52 (0.50)	0.13 (0.34)	0.34 (0.47)
Athletic facilities	0.17 (0.37)	0.19 (0.39)	0.16 (0.37)	0.17 (0.38)	0.15 (0.36)	0.30 (0.46)	0.14 (0.34)
Other categories	0.074 (0.26)	0.12 (0.32)	0.038 (0.19)	0.071 (0.26)	0.089 (0.28)	0.16 (0.36)	0.014 (0.12)
<i>Demographics and outcomes</i>							
Share low-SES	0.40 (0.23)	0.20 (0.14)	0.60 (0.18)	0.38 (0.22)	0.45 (0.24)	0.40 (0.22)	0.42 (0.23)
Share Black/Hispanic	0.21 (0.25)	0.083 (0.11)	0.41 (0.30)	0.20 (0.25)	0.25 (0.27)	0.13 (0.19)	0.28 (0.27)
ELA test scores	-0.082 (0.87)	0.42 (0.73)	-0.61 (0.79)	-0.055 (0.86)	-0.20 (0.90)	-0.077 (0.84)	-0.11 (0.89)
Math test scores	-0.12 (0.86)	0.36 (0.75)	-0.62 (0.81)	-0.091 (0.85)	-0.25 (0.90)	-0.12 (0.84)	-0.14 (0.88)
House price index (1989 = 100)	169.1 (57.2)	170.4 (53.4)	174.8 (63.2)	165.3 (53.2)	184.4 (69.0)	175.8 (58.3)	174.4 (57.4)
Number of districts	10,613	2,618	2,860	7,993	2,620	6,379	5,683
Number of states	29	25	27	24	5	18	17

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	
	DiD	RD, linear	DiD	RD, linear	DiD	RD, linear
Avg. effect over:	(1)	(2)	(3)	(4)	(5)	(6)
1-5 years	595*** (46)	759*** (67)	7 (22)	12 (32)	2 (3)	-4 (7)
6-10 years	-106* (60)	-69 (84)	3 (32)	0 (46)	7 (5)	6 (10)
11-15 years	-62 (58)	-35 (90)	5 (28)	-12 (43)	8 (8)	12 (13)
District FE	X	X	X	X	X	X
Year-State FE	X	X	X	X	X	X
Adj. R <sup>2</sup>	0.222	0.223	0.939	0.939	0.668	0.668
N	119,479	119,479	119,479	119,479	119,479	119,479

*Note:* Estimates and standard errors of linear combinations of the parameters  $\beta_{\tau}^{TOT}$  in equation (2). The dependent variables are capital spending (columns 1-2), current spending (columns 3-4), and spending on non-instructional services (columns 5-6), all measured on a per pupil basis. *DiD* estimates (columns 1, 3, and 5) are obtained not controlling for a polynomial of the vote share; *RD, linear* estimates (columns 2, 4, and 6) are obtained controlling for a linear polynomial of vote share. All columns control for district and 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 3: Effects of Bond Passage on Student Achievement and House Prices

	DiD		RD, linear						
	Test scores				Enrollment		House Price	Test	
	Pooled	Math	ELA	Total	Share white	Share high SES	Index	Scores	
Avg. effect over:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1-4 years	0.022 (0.013)	0.025*** (0.008)	0.023 (0.023)	0.029 (0.017)	0.016** (0.007)	-0.002 (0.003)	0.006 (0.003)	2.745 (1.991)	0.019 (0.016)
5-8 years	0.045** (0.022)	0.068*** (0.010)	0.065 (0.040)	0.074** (0.027)	0.015 (0.014)	0.005 (0.005)	0.023*** (0.008)	4.616* (2.644)	0.042* (0.024)
9-12 years	0.052** (0.023)	0.063*** (0.010)	0.043 (0.034)	0.084** (0.031)	0.022 (0.018)	0.011 (0.007)	0.030** (0.011)	4.714 (2.825)	0.028 (0.028)
13-16 years	0.057** (0.026)	0.093*** (0.011)	0.057 (0.043)	0.122*** (0.039)	0.049** (0.022)	0.010 (0.010)	0.010 (0.012)	3.400 (3.499)	0.064 (0.038)
District FE	X	X	X	X	X	X	X	X	X
Yr-St-Gr-Subj FE	X	X							X
Yr-St-Gr FE			X	X					
Year-State FE					X	X	X	X	
Enroll. shares									X
Adj. R <sup>2</sup>	0.839	0.839	0.830	0.862	0.994	0.978	0.902	0.907	0.843
N	736,818	736,818	358,536	378,278	194,969	189,296	182,978	74,468	716,023

Note: Estimates and standard errors of linear combinations of the parameters  $\beta_{\tau}^{TOT}$  in equation (2). The dependent variables are pooled test scores (columns 1-2 and 9); Math and ELA test scores (columns 3 and 4, respectively); total enrollment (column 5); share of enrolled students who are white (column 6) and non-economically disadvantaged (column 7); and the house price index (column 8). *DiD* estimates (column 1) are obtained not controlling for a polynomial of the vote share; *RD, linear* estimates (columns 2-9) are obtained controlling for a linear polynomial of vote share. All columns control for district and state-by-year effects, interacted for an indicator for capital spending above the median in 1995. Columns 1-2 and 9 also control for state-by-year-by-grade-by-subject-by-above median 1995 capital spending, and columns 3-4 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: Test Score Effects of Increases in Cumulative Spending on Capital

Sample:	All	Majority		State Share		Share low-SES students		Share minority students		Bond size	
		Simple	Super	T1	T3	T1	T3	T1	T3	T1	T3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Cap spending (\$1,000)	0.011*** (0.004)	0.006 (0.004)	0.011** (0.005)	-0.003 (0.005)	0.013** (0.005)	-0.005 (0.004)	0.013** (0.005)	0.005 (0.004)	0.012** (0.005)	0.005 (0.008)	0.000 (0.004)
District FE	X	X	X	X	X	X	X	X	X	X	X
Yr-St-Gr-Subj FE	X	X	X	X	X	X	X	X	X	X	X
N	619,926	481,485	138,441	142,934	240,827	200,851	195,496	214,689	137,248	52,396	215,298

*Note:* 2SLS estimates and standard errors of the parameter  $\rho$  in equation (3). The dependent variable is a standardized measure of test scores. Column 1 is estimated on the full sample of districts; column 2 on the sample of districts with a required simple majority; column 3 on the sample of districts with a required supermajority; columns 4 and 5 on the subsamples of districts with a state share of capital spending in the bottom and top terciles, respectively; columns 6 and 7 on the subsamples of districts with a share of economically disadvantaged students in the bottom and top terciles, respectively; columns 8 and 9 on the subsamples of districts with a share of minority (Black and Hispanic) students in the bottom and top terciles, respectively; and columns 10 and 11 on the subsamples of districts passing bonds with amounts in the bottom and top terciles, respectively. All columns control for district and state-by-year-by-grade-by-subject-by-above median 1995 capital spending fixed effects. Standard errors in parentheses are clustered at the district level. \* = 0.1; \*\* = 0.05; \*\*\* = 0.01.

Table 5: Share Yes Votes as a Function of Bond Size

	Share yes votes			
	(1)	(2)	(3)	(4)
Bond size	-0.011*** (0.001)	-0.011*** (0.001)	-0.007*** (0.002)	-0.008*** (0.002)
State FE	X	X		
Year FE	X	X		
District FE			X	X
State-by-year FE			X	X
Controls		X		X
Adj R <sup>2</sup>	0.31	0.36	0.55	0.57
N Districts	3,462	3,462	2,504	2,503
N Bond elections	12,100	12,099	11,123	11,121

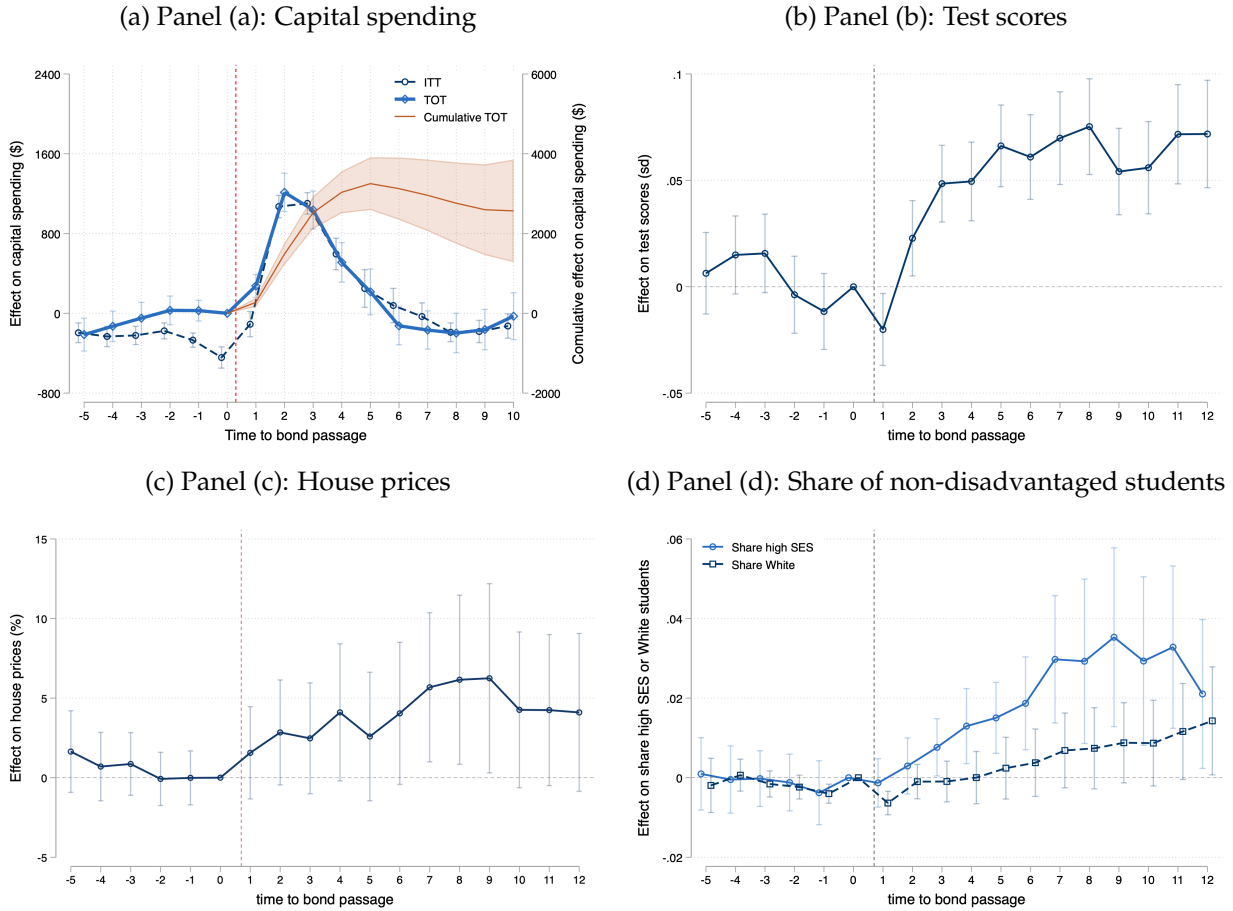
*Note:* OLS estimates and standard errors of a regression of yes share on bond size as measured by the log bond amount per pupil, including both passed and failed bonds. The regression is similar to expression (6) in the model. Controls include turnout, the number of propositions in the same year, the number of propositions in the last five years, the number of passed propositions in the last five years, log population, average household income, share college educated, share Asian, and share below 18. Standard errors in parentheses are clustered at the district level. \* = 0.1; \*\* = 0.05; \*\*\* = 0.01.

Table 6: Model parameter estimates

	Bonds	Size p.p.	Yes share	$\hat{\beta}$		$\hat{\mu}(c, w, \lambda)$		$\hat{\sigma}_x(w)$	
				Estimate	SE	Estimate	SE	Estimate	SE
California	1,174	5,252	0.64	0.308	(0.024)	23.3	(1.6)	44.9	(5.4)
Georgia	521	4,157	0.75	0.075	(0.011)	20.4	(0.8)	16.1	(1.0)
Idaho	195	4,410	0.67	0.668	(0.069)	20.3	(1.4)	25.1	(3.2)
Michigan	1,230	6,032	0.50	0.861	(0.057)	8.8	(0.3)	23.3	(2.0)
Minnesota	311	4,753	0.56	0.489	(0.043)	13.2	(0.7)	27.2	(2.5)
Missouri	951	1,686	0.70	0.305	(0.021)	18.8	(0.7)	21.9	(1.6)
New York	2,184	355	0.66	0.132	(0.009)	19.5	(1.0)	34.8	(2.5)
Ohio	936	3,607	0.51	0.703	(0.038)	9.1	(0.2)	22.9	(1.8)
Oklahoma	758	903	0.71	0.262	(0.028)	21.2	(1.5)	25.6	(2.8)
Texas	1,663	5,391	0.59	0.358	(0.027)	17.4	(1.0)	34.0	(3.9)
Wisconsin	1,508	3,055	0.52	0.643	(0.040)	9.6	(0.2)	25.8	(2.2)

*Note:* Estimates of model parameters. Number of bonds, average bond size per pupil, and average yes share. Displayed parameters are the state-specific marginal benefits to bond size,  $\beta$ ; the median voter's maximally acceptable bond,  $\mu(w)$ , averaged across states; and the standard deviation in the distribution of voter preference for bond size,  $\sigma_x(w)$ . Estimates of  $\mu(w)$  and  $\sigma_x(w)$  are in thousands of dollars per pupil. Standard errors in parentheses are computed with a bootstrap with 500 iterations.

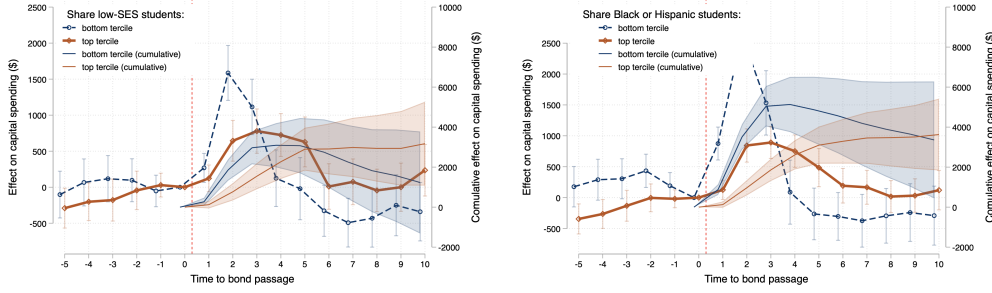
Figure 1: Effects of Bond Passage on Capital Spending, Test Scores, and House Prices



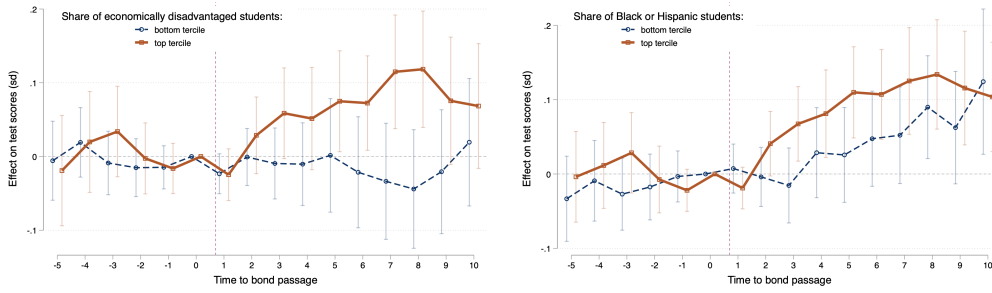
Notes: Estimates and confidence intervals of the parameters  $\beta_T^{TOT}$  in equation (2), obtained using capital spending per pupil (panel a), test scores (panel b), house price index (panel c), and the share of non-disadvantaged students and White students (panel d) as the dependent variable. Estimates on test scores are obtained pooling data across subjects and grades, using state-by-year-by-subject-by-grade effects, and weighing observations by the number of test takers. Other estimates are obtained using state-by-year effects and weighing observations by district enrollment. Standard errors are clustered at the district level.

Figure 2: Treatment Effect Heterogeneity: By Student Demographics

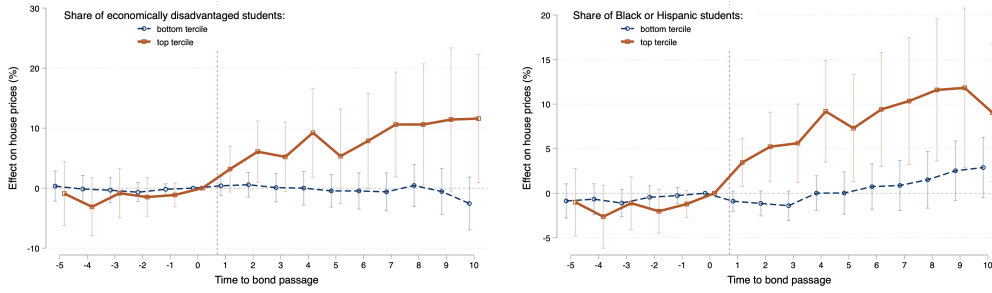
(a) Capital spending



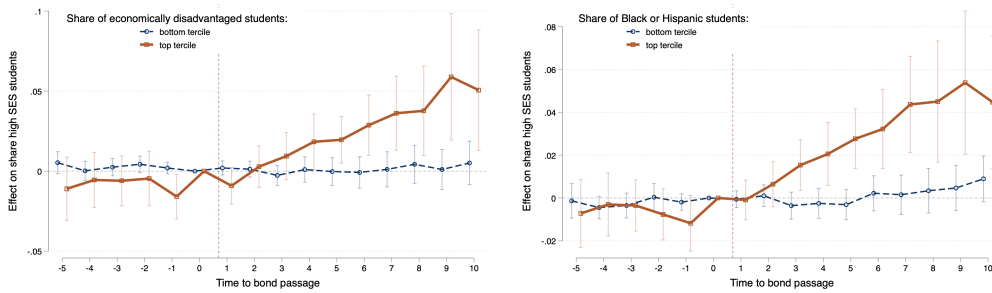
(b) Test scores



(c) House prices



(d) Share of non-disadvantaged students

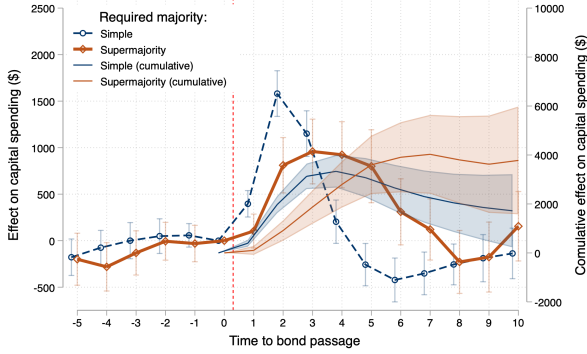


*Note:* Estimates and confidence intervals of the parameters  $\beta_T^{TOT}$  in equation (2), obtained using capital spending per pupil (panel a), test scores (panel b), house price index (panel c), and the share of non-disadvantaged students (panel d) as the dependent variable. Figures in the left panels show estimates by tercile of the share of disadvantaged students; figures in the right panel show estimates by tercile of the share of minority students. Estimates on test scores are obtained pooling data across subjects and grades, using state-by-year-by-subject-by-grade effects, and weighing observations by the number of test takers. Other estimates are obtained using state-by-year effects and weighing observations by district enrollment. Standard errors are clustered at the district level.

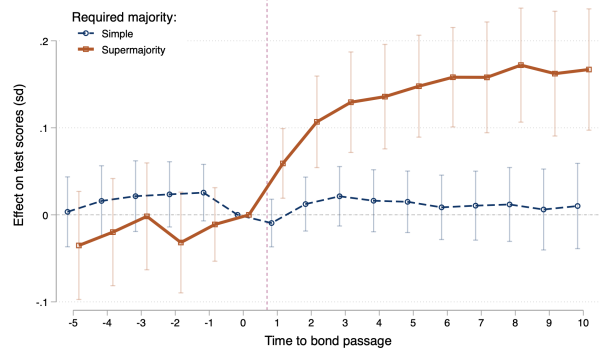


Figure 3: Treatment Effect Heterogeneity: By Required Majority Rules

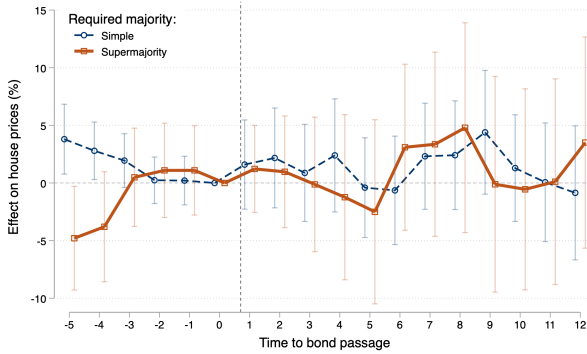
(a) Capital spending



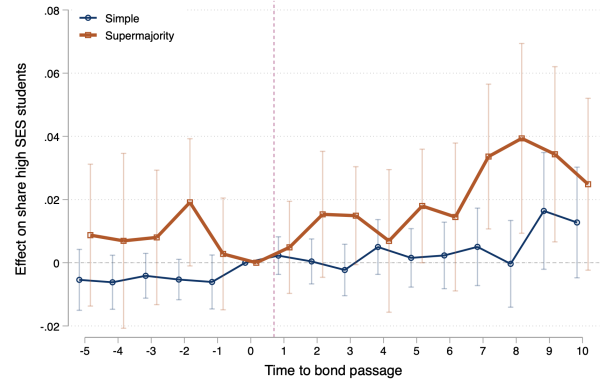
(b) Test scores



(c) House prices

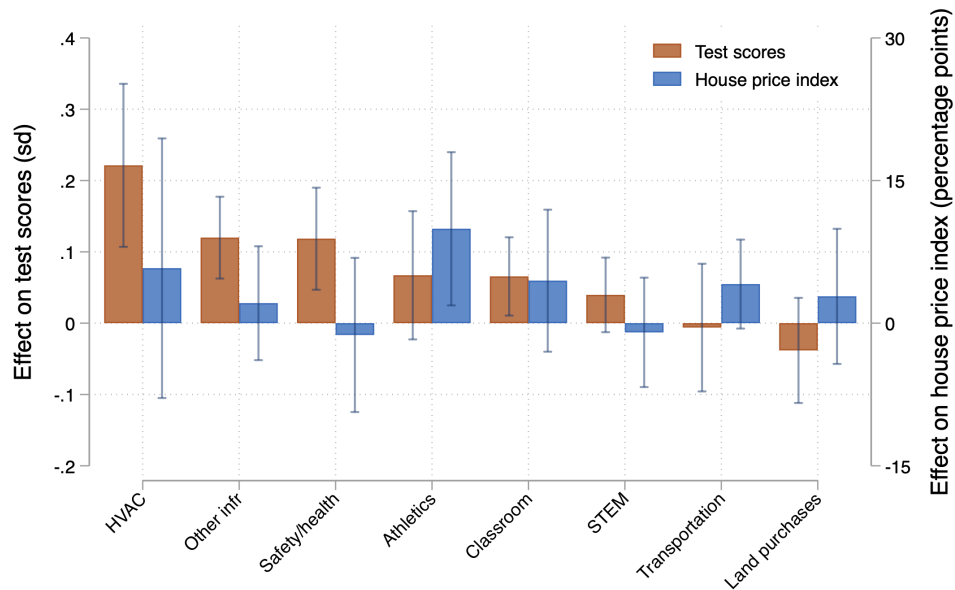


(d) Share of non-disadvantaged students



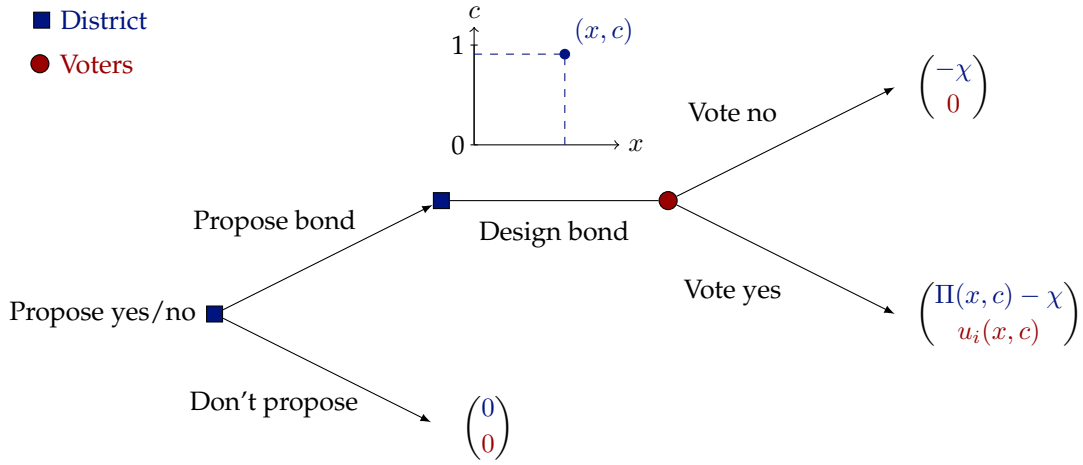
Note: Estimates and confidence intervals of the parameters  $\beta_{\tau}^{TOT}$  in equation (2), obtained using capital spending per pupil (panel a), test scores (panel b), house price index (panel c), and the share of non-disadvantaged students (panel d) as the dependent variable. Estimates are obtained separately for states with a simple majority requirement (dashed line) and those with a supermajority requirement (solid line). Estimates on test scores are obtained pooling data across subjects and grades, using state-by-year-by-subject-by-grade effects, and weighing observations by the number of test takers. Other estimates are obtained using state-by-year effects and weighing observations by district enrollment. Standard errors are clustered at the district level.

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



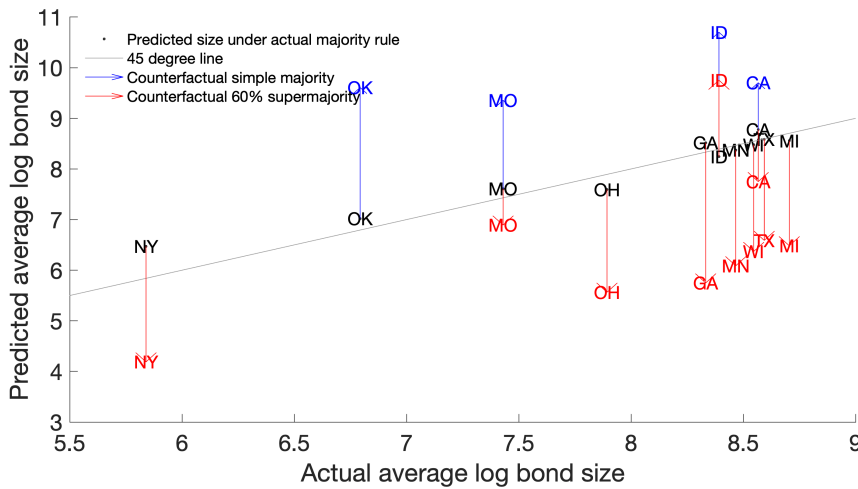
*Note:* Point estimates and confidence intervals of a linear combination of the parameters  $\xi_{c,\tau}$  in equation (5) for  $\tau$  between 5 and 12, shown separately for each category  $c$  (displayed on the  $x$ -axis). The orange series is estimated using test scores as the dependent variable, pooled across subjects and grades, using state-by-year-by-subject-by-grade effects 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: Extensive Form of Bond Proposal Game



Note: Model overview. In the first stage, the district decides whether to propose a bond or not. If it does, which comes at the stochastic cost  $\chi \sim F(\cdot|w)$ , it then designs the bond in the second stage by choosing its size  $x$  and its composition  $c$  so as to maximize its expected payoff  $\Pi(x, c)$ . Finally, in the third stage, voter  $i$  then decides whether to vote yes or no based on its utility  $u_i(x, c)$  and an aggregate election day shock. If the bond passes, investments are realized and achievement and amenity changes come into effect.

Figure 6: Policy Counterfactual: Changing Supermajority Requirements  $v$ . Impact on Bond Size  $x$



Note: Simulated impact of changing  $v$  to 0.5 (blue marks) or 0.6 (red marks) on bond size per pupil.

# Online Appendix for “School Capital Expenditure Rules, Student Outcomes, and Real Estate Capitalization”

Barbara Biasi, Julien Lafortune and David Schönholzer

## A Model Details

**Direct Valuation of Public Goods, Taxes, and Private Consumption.** We show here how to arrive at an indirect utility function  $u_i(x, c)$  that only depends on bond size  $x$  and composition  $c$  and no longer requires specifying household preferences over public goods, taxes, and private consumption. To this end, we assume households indexed by  $i$  have a utility function  $U(a, s, z_i)$ , which depends on amenities  $a$ , school quality  $s$ , and private consumption  $z_i$ . Households are subject to a budget constraint given by  $z_i \leq (1 - \tau)y_i$ , where  $y_i$  is household income and  $\tau$  is a tax rate proportional to household income (as a way to model property taxes without specifying the housing market).

The school district has a per-household budget constraint given by  $x = \tau\bar{y}$ , where  $\bar{y}$  is average household income. Public goods are produced through the functions  $a = a(x, c)$  for amenities and  $s = s(x, c)$  for school quality. Based on these assumptions, we can then write a household’s indirect utility function as:

$$u_i(x, c) = \max_{x, c} U \left( a(x, c), s(x, c), y_i \left( 1 - \frac{x}{\bar{y}} \right) \right).$$

Under the assumptions stated in the main text on the functional forms of  $a(x, c)$  and  $s(x, c)$ ,  $u_i(x, c)$  has the desired properties. In particular, utility rises with  $x$  up to some bliss point, after which utility falls; and it will rise with  $c$  until marginal returns to amenities and schooling equalize.

## B Appendix Figures and Tables

### Appendix Figures

Figure A.1: District-level Capital Expenditures (per-pupil, 2015-16)

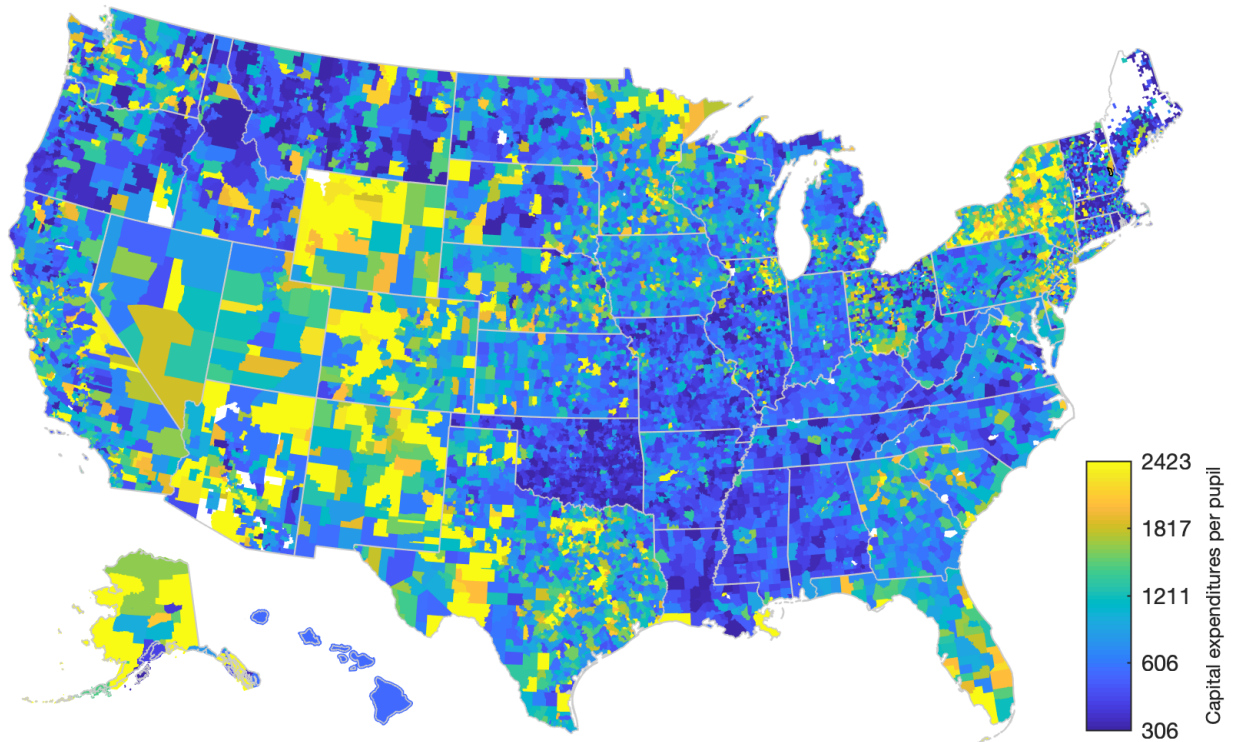
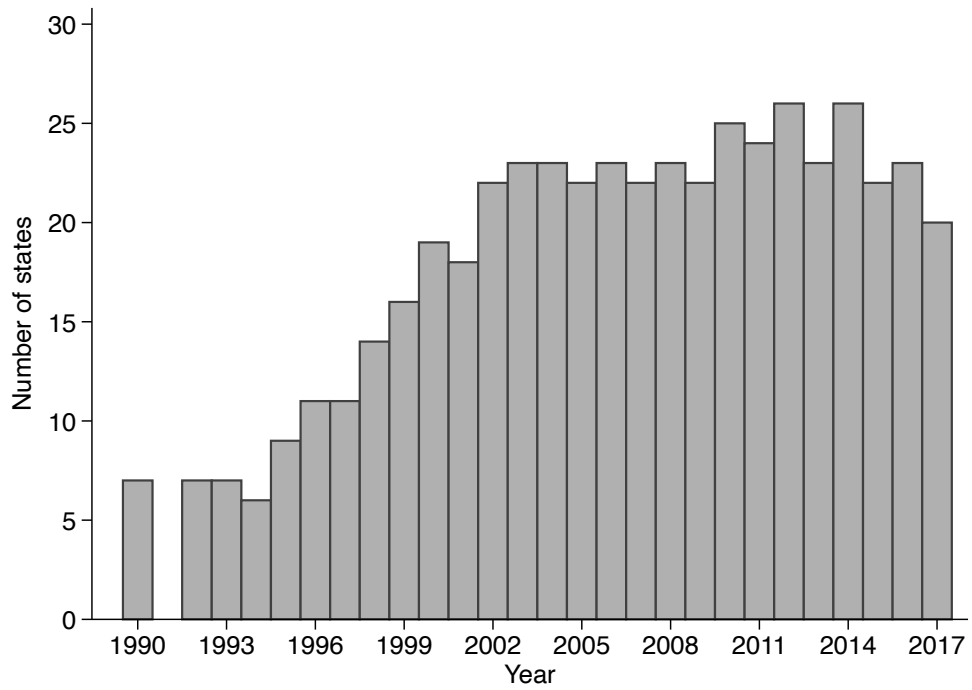
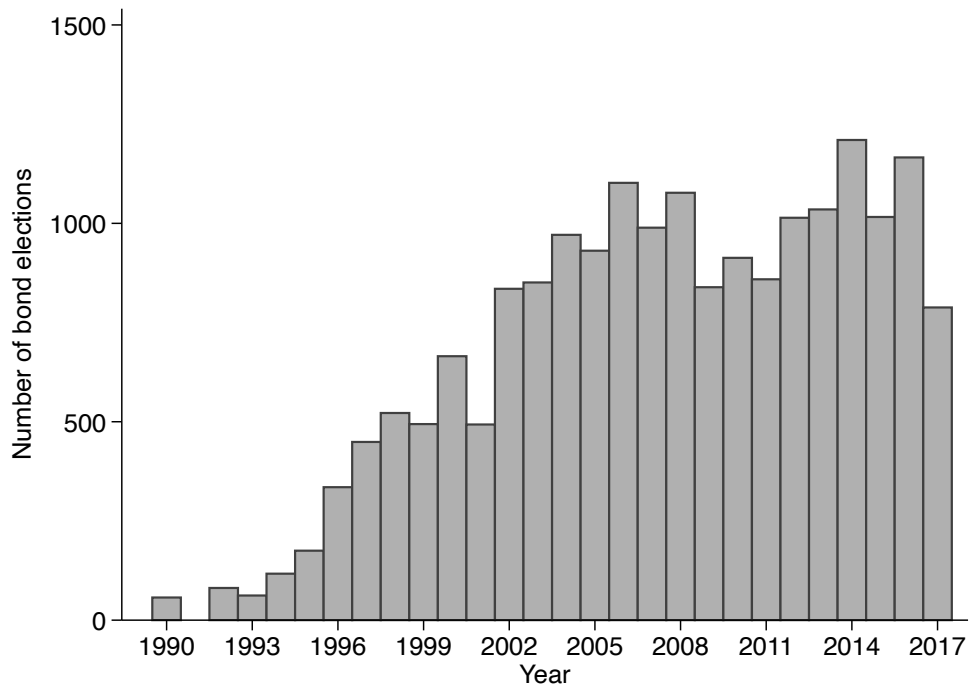


Figure A.2: Bond Data Coverage, by Year



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



(b) Number of Bond Elections

Figure A.3: First Year with Test Score Data, by State

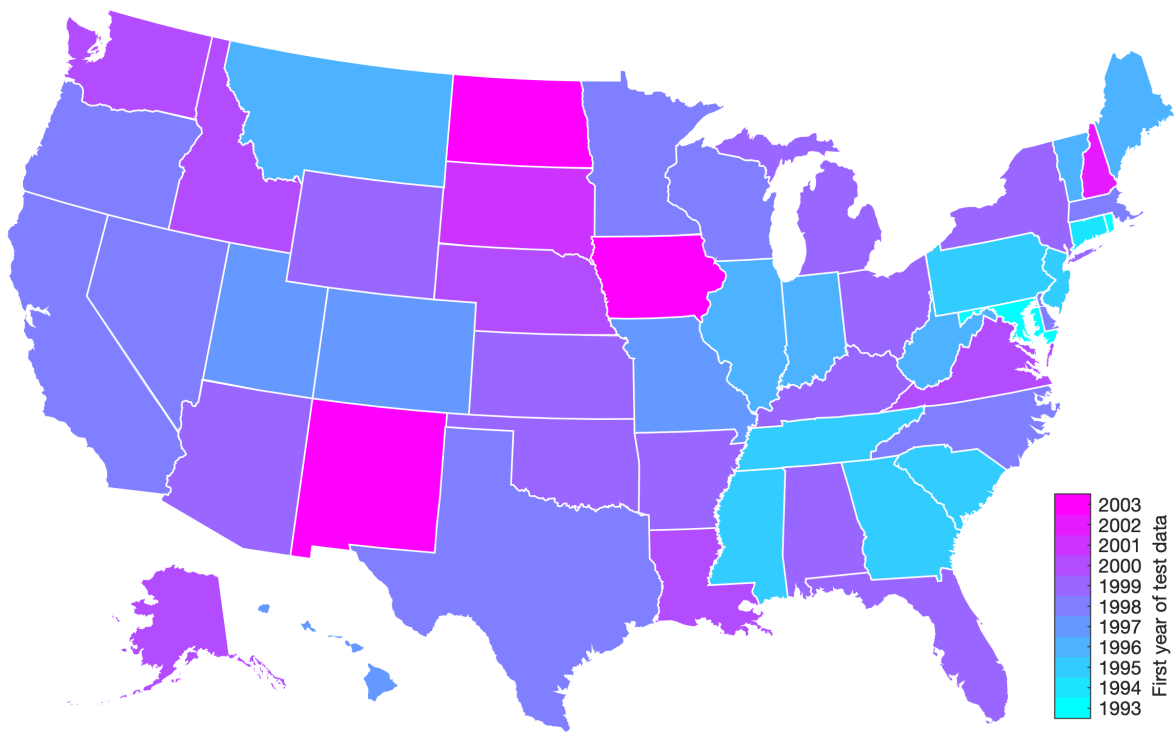
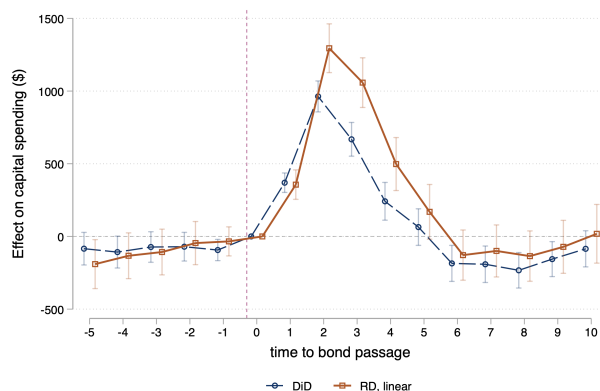
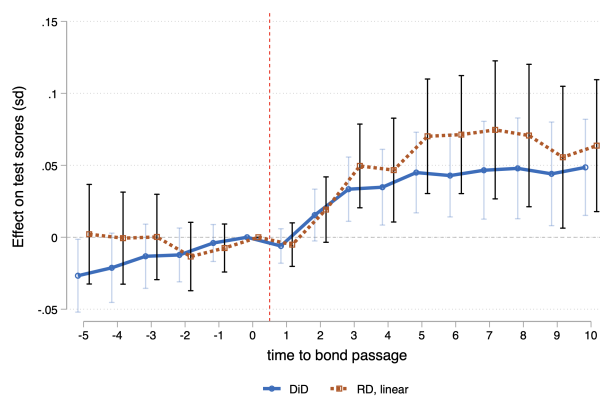


Figure A.4: Effects of Bond Passage on Capital Spending, Test Scores, and House Prices: Stacked Difference-in-Differences

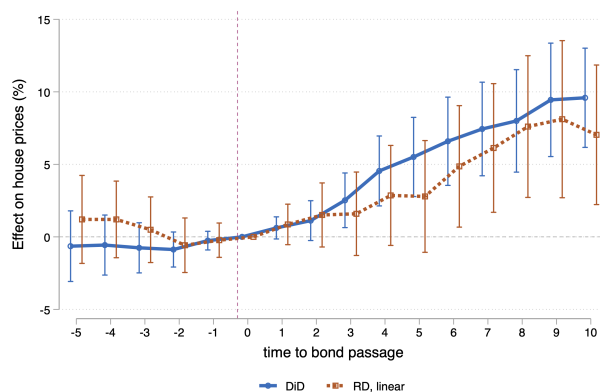
(a) Panel (a): Capital spending



(b) Panel (b): Test scores



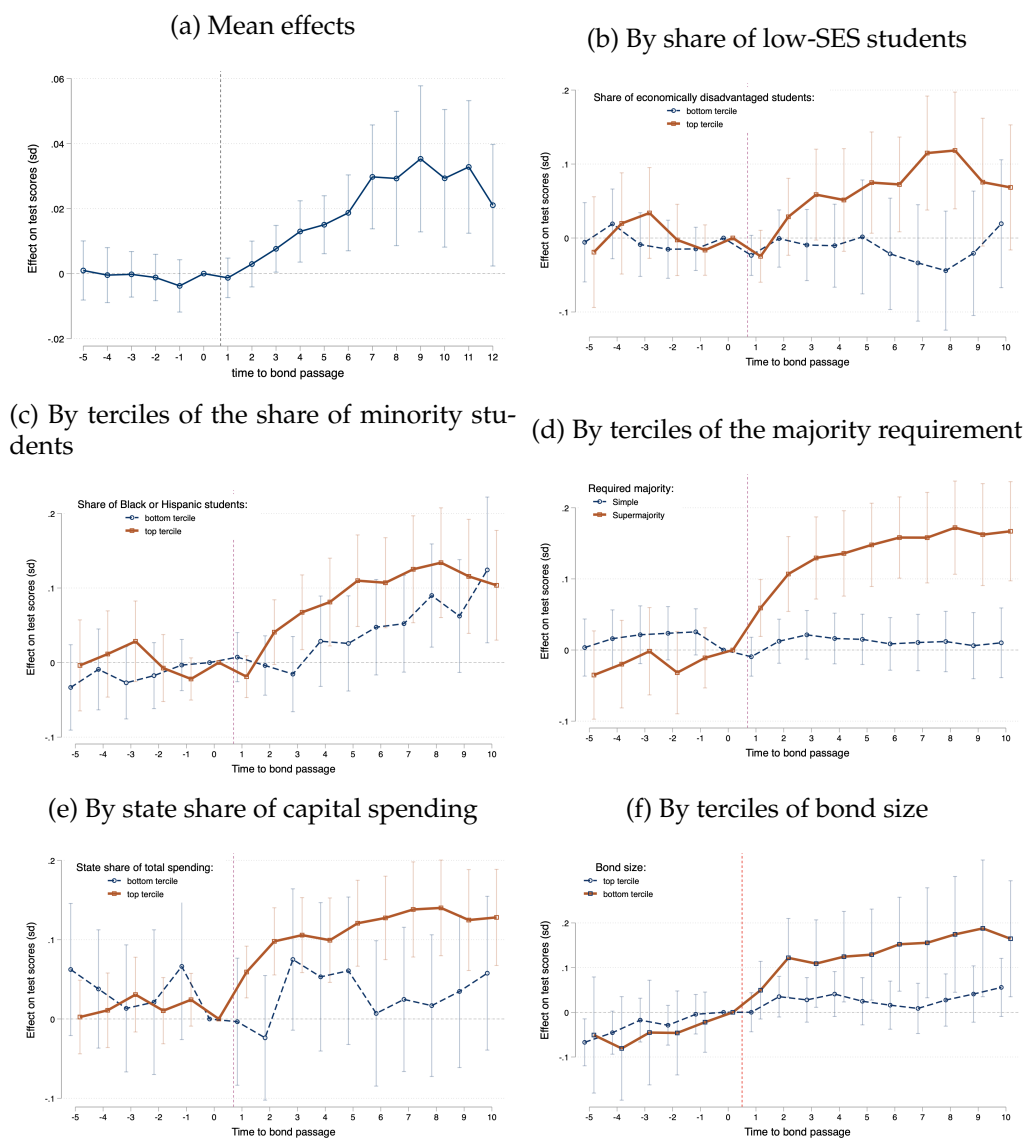
(c) Panel (c): House prices



Notes: Estimates and confidence intervals of the parameters  $\beta_{\tau}^{TOT}$  in equation (2) obtained using the stacked DiD/RD approach of Cengiz et al. (2019). Standard errors are clustered at the district level.

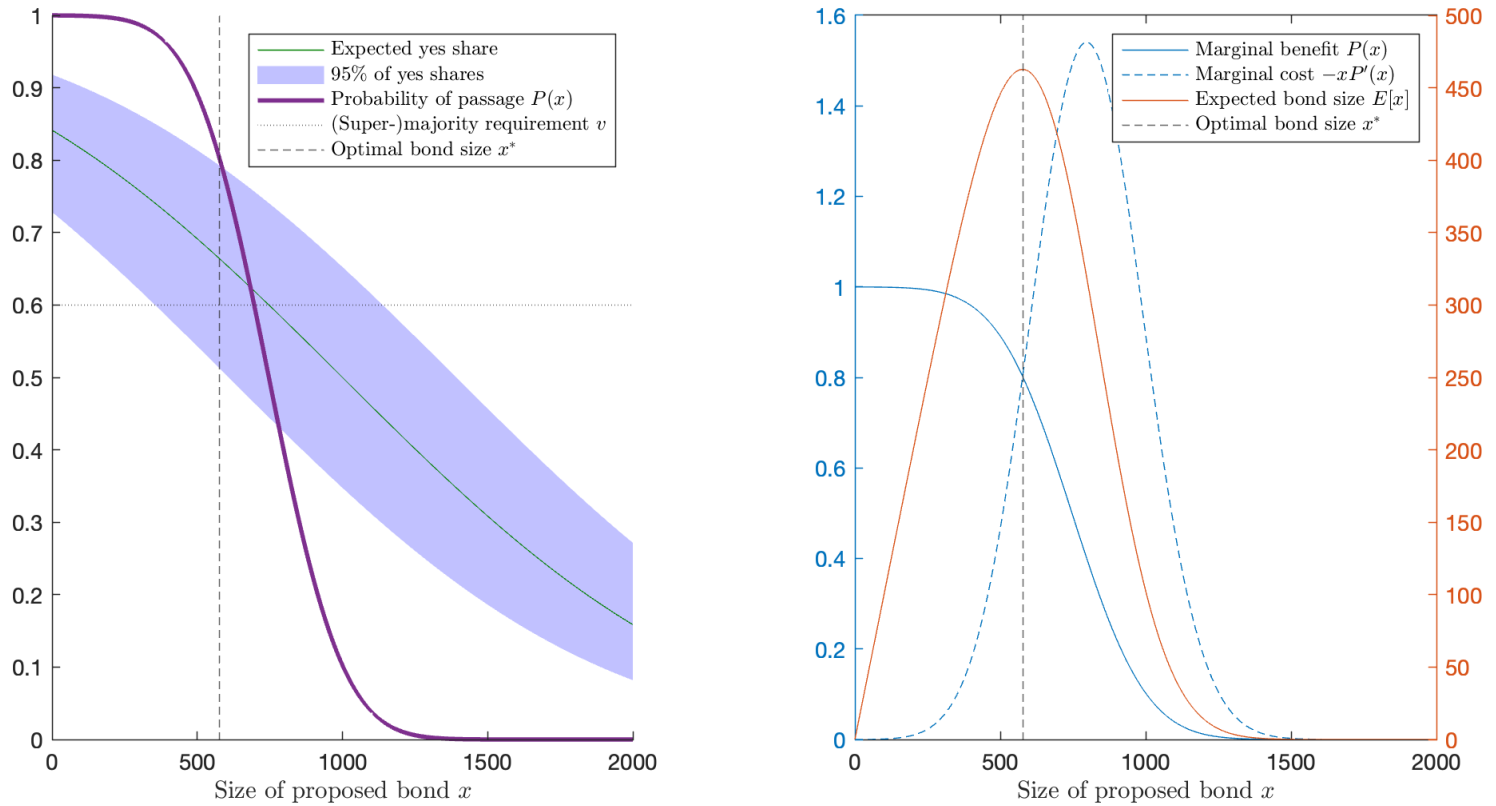


Figure A.5: Effects of Bond Passage on Test Scores: Controlling for The Composition of The Student Body



Notes: Estimates and confidence intervals of the parameters  $\beta_{\tau}^{TOT}$  in equation (2), obtained using test scores as the dependent variable and controlling for the share of White and non-economically disadvantaged students. Standard errors are clustered at the district level.

Figure A.6: Illustration of Model Tradeoffs



*Note:* Illustration of the first order condition in the model with respect to bond size, for the simple case of a bond-maximizing district. In the panel of the left, the equilibrium bond is lower than the pivotal voter's preferred bond size because the district hedges against the aggregate uncertainty in the share of yes votes. In the panel on the right, the equilibrium is determined by the crossing of the marginal benefit line (the probability of increasing the bond by one dollar) and the marginal cost line (the marginal probability of the bond getting rejected at the ballot box).