Effectiveness and Efficiency 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 voting requirements and rules that differ across states. The impacts of these investments are highly debated, and existing estimates are conflicting. We assemble new data on 20,000 school bond elections and student achievement for 31 U.S. states to (i) study the impact of capital investments on student achievement and house prices in the U.S., and (ii) understand what drives differences in impacts across districts and states. Using variation from close elections in a dynamic regression-discontinuity design, we show 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, where raising funds for capital projects is more difficult. Bonds of different size and composition also produce different effects. We interpret these findings through the lens of a probabilistic voting model, in which districts are subject to different voting requirements and face strategic incentives when considering the size and composition of a bond proposal. The model suggests that the demographic composition and the institutional funding rules are crucial drivers of the effectiveness and efficiency of school capital investments.

JEL Classification: H41 H75, I22, I24, R30

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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 31 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 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), which however only contains this information from 2009 onwards. To expand this dataset and 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. Test score effects are significantly larger in districts that serve larger populations of disadvantaged and states where raising funds for capital projects is harder, such as those with a supermajority requirement. House prices increase more in these states, whereas they are zero in simple majority states; this indicates that stricter rules tend to make capital investments inefficiently low. The impact of bond approvals is also related to the size of the proposed spending amount: Both test scores and house prices effects tend to be larger for smaller bonds. The type of projects that bonds are meant to finance matter, too. For example, proposed spending increases on 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.

Differences in DRD estimates across districts with different socio-demographic characteristics and bonds of different types are useful from a descriptive standpoint, but they should be interpreted with caution. District characteristics might have a direct impact on outcomes through unobservable time-varying factors. Similarly, the size of a bond and the type of projects to be financed might be endogenously determined as a function of funding rules and district characteristics. To better interpret these estimates and understand how cross-district differences impact the returns to capital spending, we build and estimate a probabilistic voting model. Districts decide the proposed spending amount and what items to include, which can range from those that may primarily benefiting student learning, such as HVAC systems, and those that may primarily act as amenities to students and the wider community, such as athletic facilities. Voters then choose whether to approve a bond proposal of a particular size and composition.

In the model, 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 so as 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 and community attributes. 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 community attributes.

We use the model to study the impacts of more lenient funding rules, in particular a lower majority threshold or a higher state share. Comparative statics indicate that more lenient rules would increase the proposed bond size, whereas the effect on bond composition is ambiguous. Increasing state contributions would decrease bond size if voters see local and state spending as substitutes. The return to these bonds in terms of student achievement and house prices depends in turn on the concavity in the human capital production function and the valuation function.

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).[1]

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 partic-

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

ular, 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).[2] This stands in stark contrast to funding for school 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,[3] 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 A1), 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

[2]The federal government plays a trivial role in school facility funding, covering only 1% of capital outlay between 2008-09 to 2018-19.

[3]See, for example: Hoxby 2001; Koski and Hahnel 2015; Corcoran and Evans 2015; Lafortune, Rothstein, and Schanzenbach 2018

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 average 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.[4] 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.[5]

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.

[4] 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.

[5] 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 infrastucture 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 Common Core of Data (CCD) and the 1990 Census. We compile annual demographic information for school districts, including enrollment, racial/ethic composition, and the share of low-income students[6] 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.[7]

In sum, we obtained data for 40 states.[8] 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 31 states, covering 11,468 districts for 1990-2017. The earliest data available are in 1990 for six states; we have limited coverage across states until the early 2000s.[9] The final dataset includes information on election data, vote share in favor,

[6] Defined as the share of students eligible for free or reduced price school meals.

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

[8] 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.

[9] See Appendix figure A2 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.[10] 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.[11]

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

year

[10] This is also the approach of Martorell et al. (2016).

[11] 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.[12] 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)[13], 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

[12] 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.

[13] (Reardon et al., 2021)

1994 in some states.[14] 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.[15]

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.

[14] See Appendix Figure A3 for a map of the first available year of data for each state.

[15] 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 τ 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; α_j and γ_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 β_τ captures the effect of passing a bond, conditional on district and year effects, τ years after the election (when $\tau > 0$) or τ years before it (when $\tau < 0$).

Consistently estimating β_τ 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, $\mathbb{E}\left(\varepsilon_{jt}|D_{jt-\tau}\right) \neq 0$. However, as long as there is a random component of V_{js}, it is possible to estimate β_τ 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 δ_g, $P^g(V_{jt-\tau}, \delta_g)$. We can then consistently estimate β_τ via OLS using the following model:

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

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 β_τ 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 β_τ^{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:

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

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 β_τ^{ITT} via OLS on equation (1), separately for each τ; we instead jointly estimate all β_τ^{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 $2,700 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, pooling data from Math and ELA tests in grades 3 to 8 and using 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 0.027 sd higher 1 to 4 years after the election, 0.073 sd higher between 5 to 8 years after the election, and 0.067 higher 9 to 12 years after it (Table 3, column 2). The impact of bond passage is slightly higher for ELA (with a 0.081 and a 0.088 increase 5 to 8 years and 9 to 12 post election) compared with Math (a 0.067 and a 0.045 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:

$$y_{jt} = \rho K_{jt} + \alpha_j + \gamma_t + \varepsilon_{jt} \tag{3}$$

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

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 ρ 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).[16]

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 deviations later on (Table 4).

[16] 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.

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 our 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. 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 nearly 8 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.

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.[17] 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

[17] 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).

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 1.5 percentage points higher seven years after the election (2 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 4.4 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.

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 A4).

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 identify and focus on 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 Fheterogeneity 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 $1,200 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 over the five years following the election is actually higher in districts in the top tercile, and equal to $2,500. It is only $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 0.10 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 not 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 4 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 A5, 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 very similar among districts with a simple majority and those with a supermajority; in supermajority districts, though, the spending increase lasts longer. Two years after the election, spending is approximately $1,200 higher in both groups compared to pre-election levels (Figure 3, left panel (a)). Five years after the election, spending is still $600 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 $2,100 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 0.15 sd higher six years after an election compared with before (Figure 3, left panel (b)). Test scores remain instead un-

changed 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 5 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 A5, panel (c)). Following a bond approval, house prices dramatically increase in supermajority districts, reaching a 12 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 over $2,000 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 $4,100 in districts in the bottom tercile and $3,000 in districts in the bottom tercile compared with those in the top 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.

In spite of this, it is districts in the top tercile of the state share which experience the greatest increases in test scores and house prices following an election. In these districts, test scores are 0.12 d 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 5 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 A5, 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 Size and Composition

Not all school capital investments are created equal. First, they may differ in terms of size: Some projects are bigger than others and are associated to a bond with a larger amount. Second, 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 size of the spending amount (which we call "size") and type of financed projects (which we call "category").

Differences by bond size. We begin by testing whether the impact of bond passage differs across bonds of different size. We classify bonds into terciles of the distribution of the proposed spending amount per pupil, as reported on the ballot, and estimate effects separately for districts proposing bonds in the top and bottom terciles of this distribution.

First-stage RD estimates confirm that the increase in capital spending in districts that barely approve a bond, relative to those that barely fail to approve it, is significantly larger for districts with bond amounts in the top tercile compared with those in the bottom tercile. With bond amounts in the top tercile, the per year spending increase peaks at a $1,800 higher level two years after the election compared with before (Figure 4, panel (a), solid series). In districts in the bottom tercile, the increase peaks at only $300 (Figure 4, panel (a), dashed series). This difference implies that the cumulative increase in capital spending over the five yers following an election is equal to $4,500 in districts in the top tercile and $800 in districts in the bottom tercile (Figure 4, panel (a)).

In spite of the larger spending increase in districts in the top tercile, test scores experience only a small increase following a bond passage. They increase significantly more re in districts in the bottom tercile, reaching a 0.15 sd higher level ten years following the election. 2SLS estimates confirm that a $1,000 increase in spending over a time period of six years has higher returns in districts that pass smaller bonds, producing a test score increase of 0.012 standard deviations (Table 4, column 10). The return is indistinguishable from zero in districts that pass larger bonds (Table 4, column 11).

Part of the increase in test scores could be attributed to changes in the composition of the student body. In districts in the bottom tercile, the share of non-disadvantaged students increases slightly by 3 percentage points six years after an election, while it remains unchanged in districts in the top tercile (Figure 4, panel (c)). The increase in test scores in the top tercile, though, remains visible even if we control for the share of students in the various demographic groups (Appendix Figure A5, panel (d)). Changes in house prices following an election mirror changes in test scores. House prices increase rapidly in districts in the bottom tercile, reaching a 20 percent higher level 9 years after an election. They instead do not chanfe in districts in the top tercile (Figure 4, panel (c)).

Differences by bond composition. Next, we test whether the impact of a bond approval on test scores and house prices depends on the bond's composition, i.e., the specific capital projects that

the bond will finance. We focus on six categories of spending: athletic facilities, new buildings, HVAC systems, roof repairs, furnishing, and building replacement. To account for heterogeneity by spending category, we enrich the model in equation (2) as follows:

$$y_{jt} = \sum_{c \in \mathbb{C}} \sum_{\tau=0}^{\tau=t+m} \xi_{c,\tau} D_{jt-\tau} * \kappa_c \sum_{\tau=t-n}^{\tau=t+m} \left[\beta_{\tau}^{TOT} D_{jt-\tau} + \phi_{\tau} M_{it-\tau} + P^{g}(V_{jt-\tau}, \delta_g) \right] + \alpha_j + \gamma_t + \varepsilon_{jt} \quad (5)$$

where $\mathbb{C}$ is the set of all spending categories and κ_c are category fixed effects. The specification above allows for the impact of bond approval to differ among categories in the years following the election; the parameters $\xi_{c,\tau}$ capture the differential change in the outcome variable τ years since the election for bonds in category c, relative to uncategorized bonds. We estimate two versions of equation (5) using test scores and house prices as the dependent variable, respectively. To maximize precision, we present and discuss linear combinations of the parameters $\xi_{c,\tau}$ for τ between 6 and 10.

Estimates from this equation indicate that the passage of bonds targeted to fund HVAC systems are associated with the largest increase in test scores, equal to 0.14 sd (Figure 5, orange bars). This is consistent with existing research showing that air conditioning can mitigate the learning losses caused by heat (Park et al., 2020). Furnishing replacements and the construction of new buildings are also associated with increases in learnings of 0.10 and 0.08 sd, respectively. The construction of athletic facilities and the renovation of existing facilities are associated with smaller increases, while roof repairs are not associated with any test score effects.

Notably, the bond categories followed by the largest test score changes are different from the categories followed by the largest house price increases. Most notably, bonds that fund HVAC systems—followed by the large test score increases—are not associated with any changes in house prices, whereas bonds that fund roof repairs—followed by virtually no test score changes— are associated with the largest increase in house prices, at 12 percent (Figure 5, blue bars).

These differences suggest that the extent to which a community may value a school capital investment goes above and beyond any improvements in learning. These estimates, though, cannot be interpreted as causal. The types of projects districts choose to pursue may be related to the funding rules, the composition of the district's community, and the characteristics of its student body—which we argued can have a direct impact on outcomes. To illustrate these relationships and better understand the sources and implications of differences in bond impacts, we resort to a

model.

6 Explaining Differences: Governance of Capital Investments

6.1 Preliminaries

We model the determination of bond characteristics and their relationship to outcomes and demographics as a game between a single district and heterogeneous households who vote on whether to pass or fail the district's bond proposal. The key tradeoff involves the benefits of larger bonds in terms of higher student achievement and other amenities against the costs of higher taxes. In addition to comparative statics, the model provides a foundation for a structural estimation of the bond determination process and of counterfactual supermajority and state share rules. We describe technical details in the Appendix.

Bond size and composition. We consider a single district with a vector w that captures demographic and fiscal characteristics as well as other features of the environment such as the state of school infrastructure and the timing and success of recent bond elections. The district considers issuing a bond whose investments may improve both student achievement $s(x, c)$ as well as the quality of amenities $a(x, c)$ in the district. Here, $x \geq 0$ is the bond's per-pupil size in dollars and $c \in [0, 1]$ is its composition. The composition of a bond captures the tradeoff between capital investments that improve student achievement and those that may serve as amenities to the wider community. Specifically, $c = 0$ indicates the bond is entirely geared towards learning, whereas a bond with $c = 1$ is a pure amenity bond. Bonds with values of $c \in (0, 1)$ have a mix of learning and amenity effects. For example, upgrading HVAC infrastructure may have a large effect on student achievement but provides little amenity value, and hence a bond consisting mostly of HVAC investments would have a composition close to $c = 0$. In contrast, a football stadium provides a valuable amenity to the district but may not improve student achievement, in which case $c = 1$.

We assume that, for a given composition, a larger bond (i.e. increasing x) improves both student achievement and amenities at diminishing rates. These rates of return may both implicitly depend on w. By definition, for a given size, changing the composition of a bond to be more amenity-focused (i.e. increasing c) lowers the impact on student achievement but increases the value of

amenities.

State funding contribution. Districts receive a transfer from the state that matches a share λ of a bond's size x. This funding rule may depend on district characteristics w. For now, 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 district.

Household preferences. There is a unit interval of households indexed by i in the district who value both amenities and student achievement. Improving these outcomes 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 $u_i(x, c)$ for each household, which depends only on bond size and composition. For a given composition, utility rises with bond size (i.e. higher x) up to a point at which the loss in private consumption outweighs the gains from higher investments, after which utility falls with bond size. For a given size, utility rises with the amenity mix of the bond (i.e. higher c) until the marginal utility from student achievement is larger than that of amenities, after which utility fall with a larger focus on amenities.

Instead of parameterizing household utility directly, it is convenient to assume that household preferences can be adequately described by the distribution of the largest bond $\tilde{x}_i(c)$ household i would accept for a given composition c. Specifically, the *maximally acceptable bond* for a given composition is given by

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

If the size of a bond were exactly equal to a household's maximally acceptable bond, $x = \tilde{x}_i(c)$, then the household would be indifferent between this proposed bond and no bond at all, $x = 0$, holding c fixed. If the bond were any larger, the household would be worse off. We assume that maximally acceptable bonds are Normal distributed with mean $\mu(c, w, \lambda)$ and standard deviation $\sigma_x(w)$. Given that the distribution is symmetric, the mean is also the preference of the median household in the district. The mean 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 objective function. Just like households, the district values both amenities and student achievement, but potentially with different weights than households. Unlike households, districts always prefer larger bonds. We capture these objectives with a flexible function $\Pi(x, c)$ which is monotonically increasing in x, and weakly concave in x and c, and with weak complementarity between x and c. 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 Process of Bond Determination

Bonds are determined in three stages. In the first stage, the district decides whether or not to propose a bond at all. If it decides to do so, it moves on to the second stage. In the second stage, the district determines the size and composition of the bond. Finally, in the third stage, households vote on whether to accept the bond proposal, and if a sufficient majority agrees, the district issues the bond and implements the planned investments. We now describe these stages in reverse order.

Voting stage. For simplicity, we assume that every household turns out to vote. Given a bond proposal (x, c), each voter i uses the following decision rule: accept the bond (i.e. vote yes) if the bond proposal is no larger than her maximally acceptable bond, perturbed by a shock to preferences: $x \leq \tilde{x}_i(c) + \eta$, where $\eta \sim \mathcal{N}(0, \sigma_\eta(w))$ captures aggregate uncertainty on election day, which could be positive or negative. For example, η could be a news shock about a corruption scandal involving district officials, which may lead to a negative shift in maximally acceptable bonds across all voters; or the discovery of asbestos in school houses may induce a positive shift. While designing the bond, the district only knows knows the distribution of η but not its realization. Hence, the stochastic share of yes votes is

$$V(x,c) = \Pr\left(x \leq \tilde{x}_i(c) + \eta\right) \tag{6}$$

$$= \Phi\left(\frac{\mu(c,w,\lambda) - x + \eta}{\sigma_x(w)}\right) \tag{7}$$

and the probability of passage is

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

where $\Phi(\cdot)$ is the cumulative density function of a standard Normal random variable and $v \in [0.5, 1)$ is the (super-)majority requirement in the state. As one would expect, a bond is more likely to pass if mean household preferences for bonds are larger, if the bonds is smaller, and if the supermajority requirement is lower.

Bond design stage. The district chooses the size and composition of the bond in anticipation of the outcomes at the voting stage. Specifically, we assume that 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

$$\sigma_\eta^{-1}(w)\psi(u/\sigma_\eta(w)) = \pi_x(x,c) \tag{8}$$

and

$$-\mu_c(c,w,\lambda)\sigma_\eta^{-1}(w)\psi(u/\sigma_\eta(w)) = \pi_c(x,c), \tag{9}$$

where we call $u \equiv \mu(c,w,\lambda) - x - \sigma_x(w)\Phi^{-1}(v)$ the *wedge* between supermajority-adjusted mean preferences and the proposed bond size. $\psi(u) \equiv \phi(u)/\Phi(u)$ is a monotonically decreasing func-

tion capturing the marginal probability of passage. The terms $\pi_x(x,c) \equiv \Pi_x(x,c)/\Pi(x,c)$ with $\Pi_x(x,c) = \partial\Pi(x,c)/\partial x$ and $\pi_c(x,c)$ are *district elasticities* with respect to size and composition. Finally, $\mu_c(c,w,\lambda)$ is the *voter elasticity* with respect to composition.

Equation (8) captures the balance between household and district preferences over equilibrium bond size. Concretely, the district balances the marginal costs (left-hand side) due to the threat of failed passage at the ballot box against the benefits of marginally larger bond (right-hand side). Marginal costs are lower when aggregate uncertainty (σ_η) plays a larger role and when the wedge between proposed size and voter-desired size is large. The intuition for the former is that increasing bond size is less risky when the election outcome is primarily driven by unpredictable features that are not related to bond characteristics. The latter is explained by the intuition that if the district allows for less "wiggle room" (i.e. a smaller wedge), then increasing bond size is more likely to derail bond passage. The wedge itself depends on how much appetite households have for larger bonds. Since $\pi_x(x,c)$ falls with x (due to concavity), small marginal costs are associated with large equilibrium bonds.

The equilibrium bond will be generally smaller than the mean preference for bond size $\mu(c,w,\lambda)$ such that the wedge is positive, $u > 0$. This is due to two features of the environment: first, the district hedges against the aggregate uncertainty by; 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. However, although the equilibrium bond is below the median household's maximally acceptable bond, whether it is too small from an efficiency point of view depends on how much lower the median household's optimal spending level would be.

Equation (9) captures the balance between competing preferences of households and the district regarding the composition of the bond. Strikingly, we can see that the district and voter elasticities with respect to composition have the opposite sign in equilibrium. This means 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 voter or the district have a stronger taste for amenities.

Figure 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 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).

Proposal stage. Finally, turning to the proposal stage, the district decides to propose a bond only if the expected benefits of doing so, $P(x,c)\Pi(x,c)$, are larger than the stochastic costs of proposing a bond, $\chi(w) \sim F(\cdot|w)$. These costs could be due to logistic or reputational considerations. Let $D \in \{0,1\}$ indicate whether the district proposes a bond or not and $\rho(w) = \Pr(D=1|w)$. The conditional probability of bond proposal is then

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

We can see that a district is more likely to propose a bond if the expected benefits are high, but it depends 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 λ? Starting with bond size and composition, the equilibrium bond size is lower if the supermajority requirement is higher $\partial x^*/\partial v < 0$ (see the Appendix for a proof). 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 learning) 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 λ 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.

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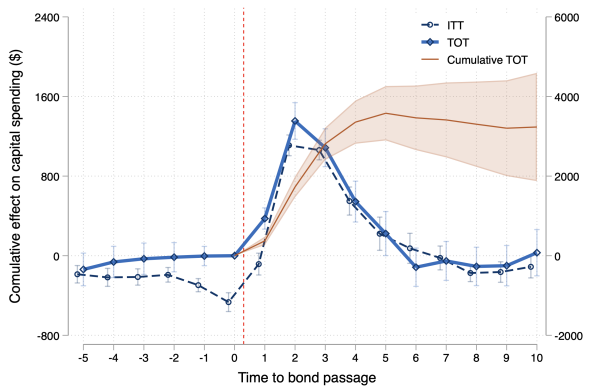
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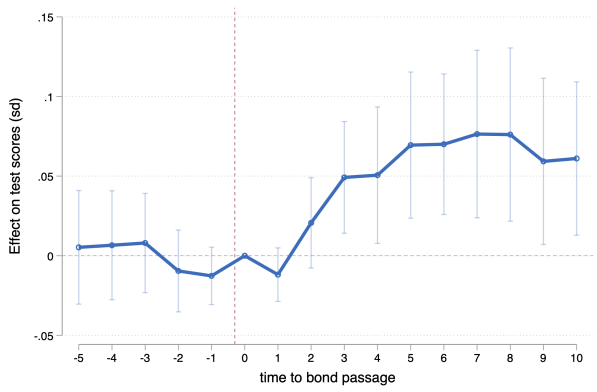
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Figure 1: Effects of Bond Passage on Capital Spending, Test Scores, and House Prices

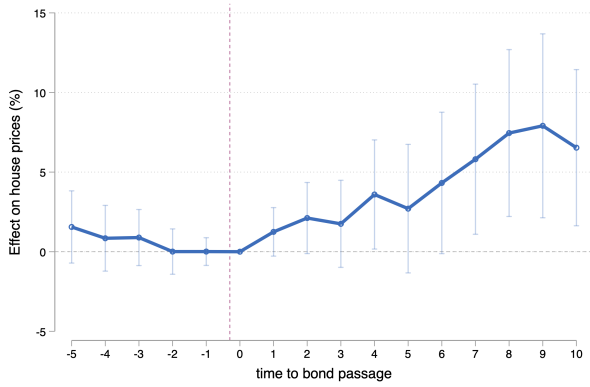
(a) Panel (a): Capital spending



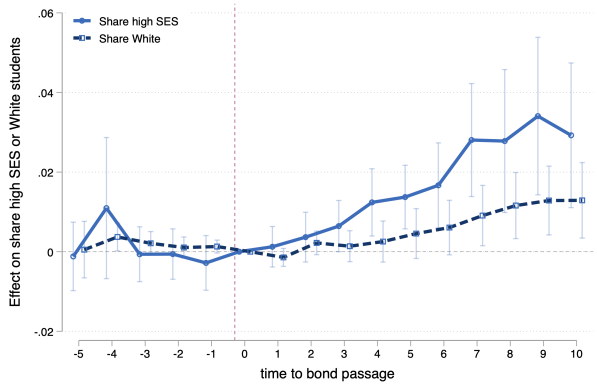
(b) Panel (b): Test scores



(c) Panel (c): House prices



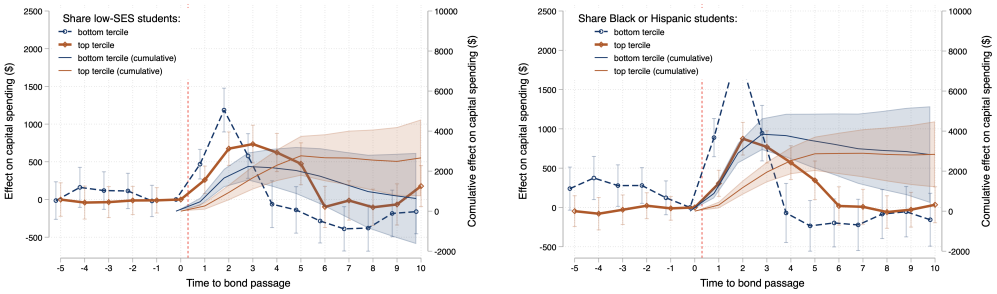
(d) Panel (d): Share of non-disadvantaged students



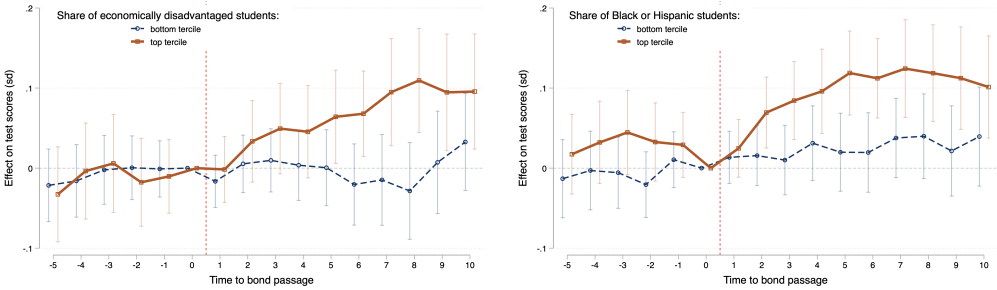
Notes: Estimates and confidence intervals of the parameters β_{τ}^{TOT} in equation (2), obtained using capital spending per pupil (panel a), test scores (panel b), house price index (panel c), and the share of non-disadvantaged students and White students (panel d) as the dependent variable. Estimates 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

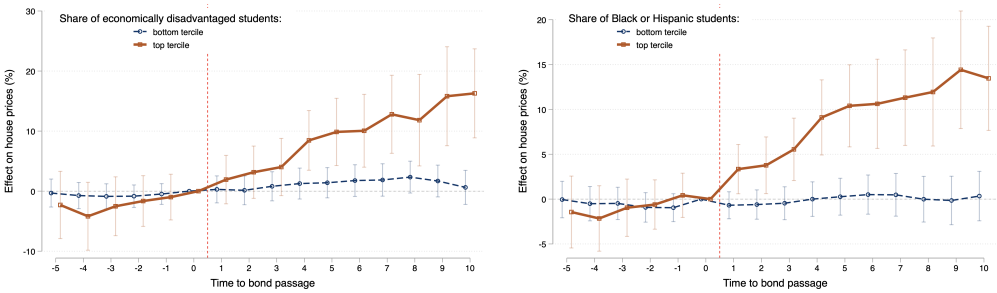
(a) Capital spending



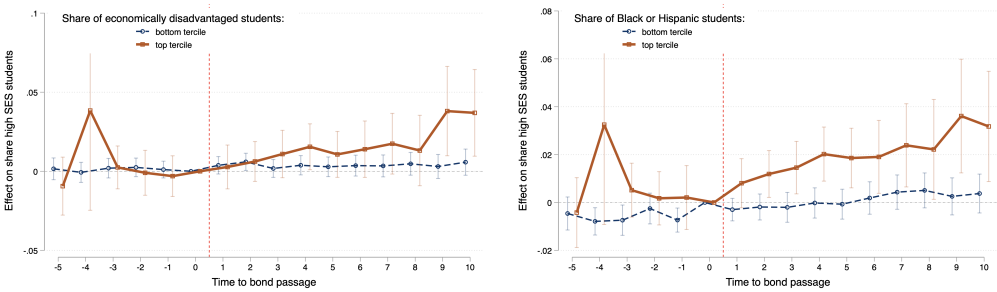
(b) Test scores



(c) House prices



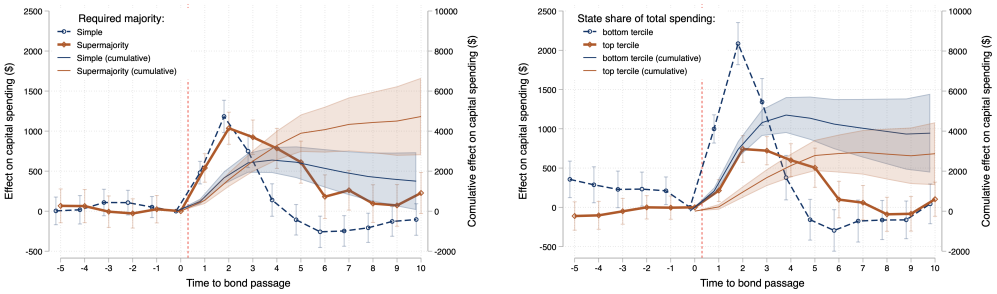
(d) Share of non-disadvantaged students



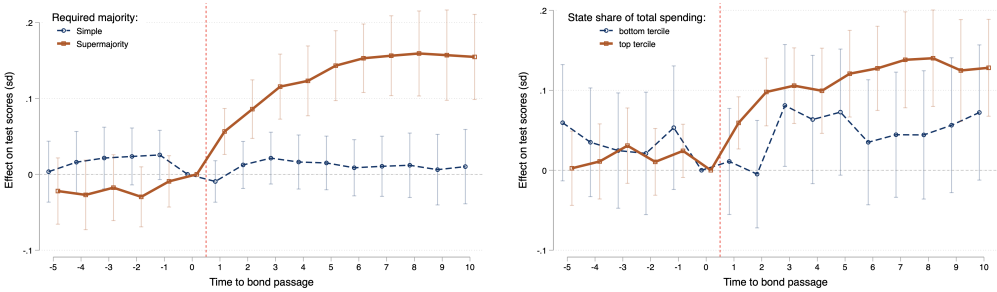
Note: Estimates and confidence intervals of the parameters β_τ^{TOT} in equation (2), obtained using capital spending per pupil (panel a), test scores (panel b), house price index (panel c), and the share of non-disadvantaged students (panel d) as the dependent variable. Figures in the left panels show 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 Funding Rules

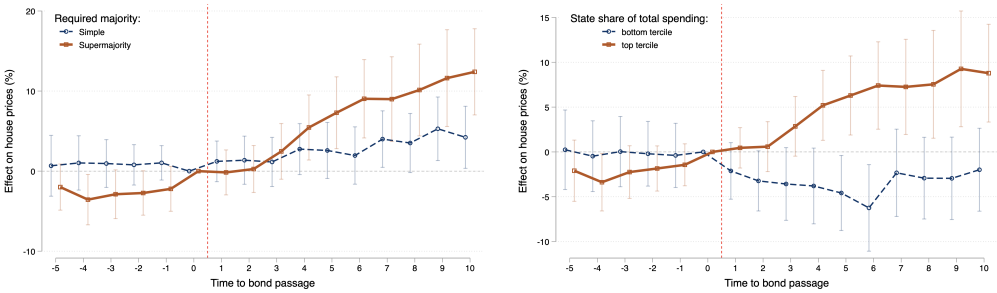
(a) Capital spending



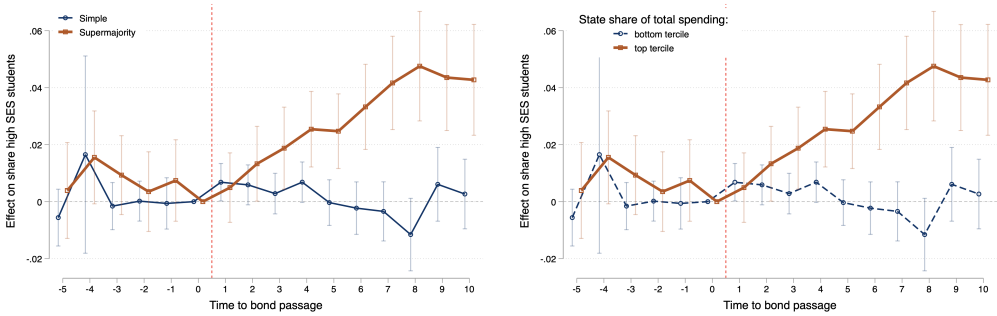
(b) Test scores



(c) House prices



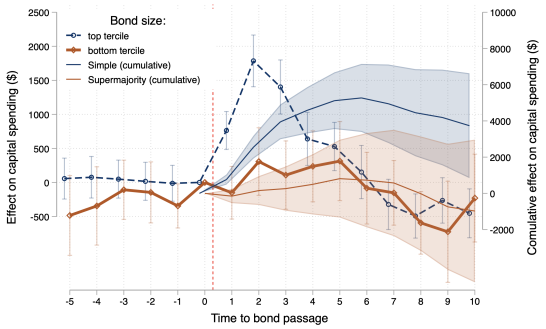
(d) Share of non-disadvantaged students



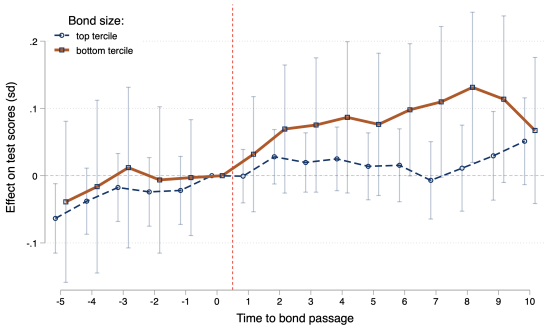
Note: Estimates and confidence intervals of the parameters β_τ^{TOT} in equation (2), obtained using capital spending per pupil (panel a), test scores (panel b), house price index (panel c), and the share of non-disadvantaged students (panel d) as the dependent variable. Figures in the left panels show estimates by required majority; figures in the right panel show estimates by tercile of the distribution of the state share of capital spending. 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 Bond Size

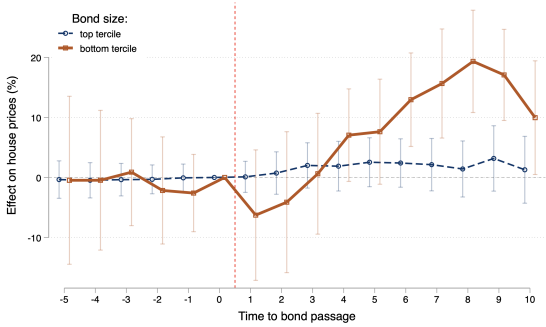
(a) Capital spending



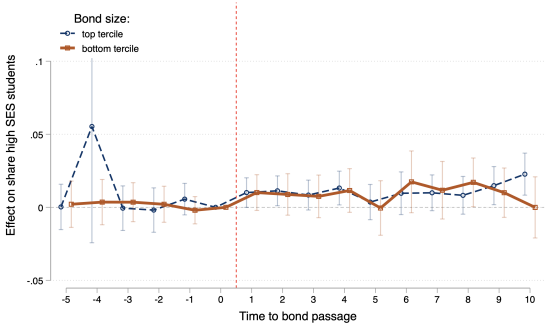
(b) Test scores



(c) House prices

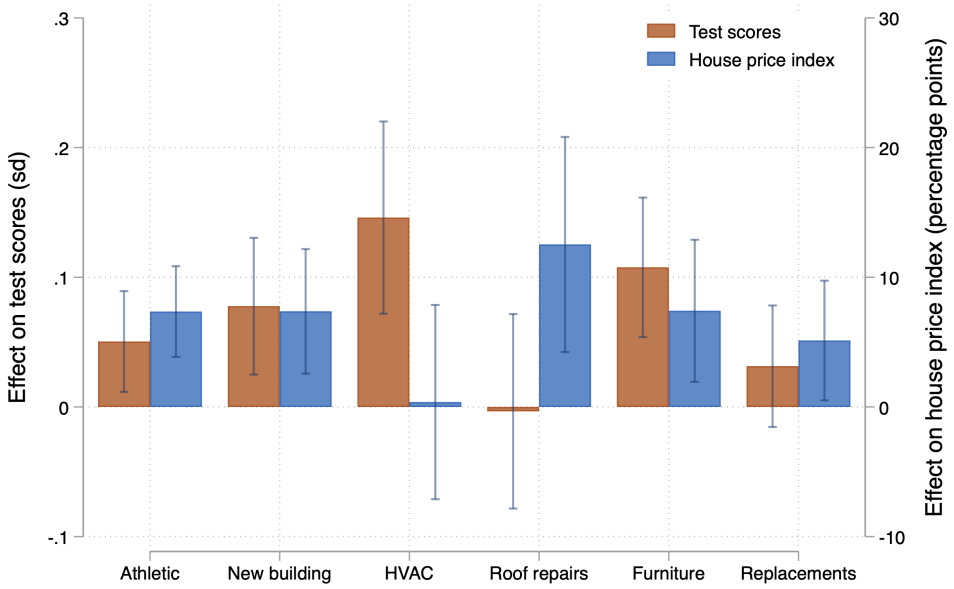


(d) Share of non-disadvantaged students



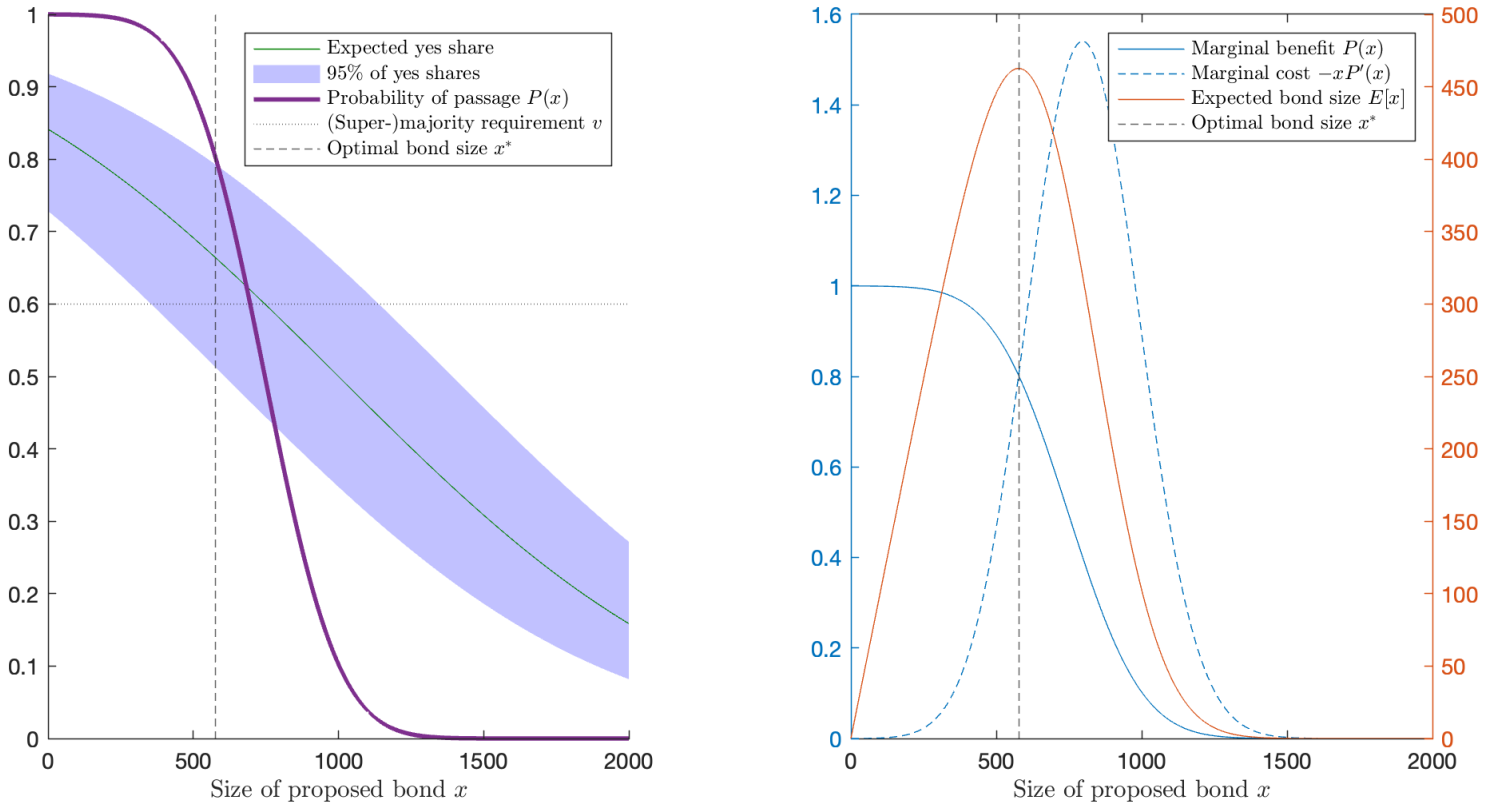
Note: Estimates and confidence intervals of the parameters β_τ^{TOT} in equation (2), obtained using capital spending per pupil (panel a), test scores (panel b), house price index (panel c), and the share of non-disadvantaged students (panel d) as the dependent variable. Estimates are obtained separately for districts in the top and bottom terciles of bond amount per pupil. 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 5: 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 τ between 6 and 10, 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 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).

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

				Spending rules			
		% low-SES students		Majority requirement		State share	
	Full sample	≤33 pct	>66 pct	50%	>50%	≤33 pct	>66 pct
Expenditure per pupil ($)							
Capital	1269.8	1354.4	1156.6	1337.2	1117.0	1055.1	1467.4
	(2795.0)	(2490.5)	(2859.2)	(2979.2)	(2316.9)	(2088.2)	(3339.9)
Current	6900.3	7177.2	6504.4	7236.0	6139.7	6667.9	6960.7
	(3805.1)	(2938.1)	(2637.1)	(4138.0)	(2766.7)	(2633.7)	(3253.0)
Spending rules							
State share, capital exp	0.092	0.10	0.087	0.089	0.098		
	(0.19)	(0.27)	(0.14)	(0.20)	(0.14)		
Share w/supermajority	0.31	0.20	0.41			0.31	0.35
	(0.46)	(0.40)	(0.49)			(0.46)	(0.48)
Voting requirement	0.53	0.52	0.54	0.50	0.59	0.53	0.53
	(0.048)	(0.040)	(0.052)	(0)	(0.036)	(0.046)	(0.051)
Bonds							
# proposed/year	0.063	0.081	0.057	0.066	0.056	0.056	0.062
	(0.24)	(0.27)	(0.23)	(0.25)	(0.23)	(0.23)	(0.24)
Share approved	0.76	0.74	0.79	0.75	0.76	0.69	0.76
	(0.43)	(0.44)	(0.41)	(0.43)	(0.43)	(0.46)	(0.43)
Yes share above threshold	0.096	0.078	0.12	0.11	0.066	0.054	0.097
	(0.15)	(0.13)	(0.17)	(0.16)	(0.12)	(0.13)	(0.15)
Size p.p. proposed ($1,000)	7.39	7.32	7.04	7.61	6.88	7.17	9.35
	(8.50)	(8.09)	(8.05)	(8.68)	(8.06)	(7.08)	(9.65)
Size p.p. approved ($1,000)	6.27	6.10	6.46	6.19	6.44	6.22	8.36
	(7.93)	(7.63)	(7.64)	(8.12)	(7.48)	(6.22)	(9.10)
Categories, approved bonds							
Athletic facilities	0.50	0.49	0.48	0.50	0.52	0.55	0.47
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
HVAC	0.34	0.28	0.42	0.18	0.66	0.58	0.51
	(0.47)	(0.45)	(0.49)	(0.38)	(0.47)	(0.49)	(0.50)
Furnishings	0.17	0.083	0.33	0.15	0.20	0.093	0.39
	(0.37)	(0.28)	(0.47)	(0.36)	(0.40)	(0.29)	(0.49)
New buildings	0.14	0.15	0.15	0.12	0.18	0.11	0.19
	(0.35)	(0.36)	(0.36)	(0.33)	(0.38)	(0.31)	(0.39)
Replacement buildings	0.15	0.15	0.16	0.14	0.16	0.080	0.18
	(0.35)	(0.35)	(0.36)	(0.35)	(0.37)	(0.27)	(0.39)
Any other bond items	0.19	0.18	0.24	0.11	0.40	0.25	0.23
	(0.39)	(0.39)	(0.43)	(0.31)	(0.49)	(0.43)	(0.42)
Demographics and outcomes							
Share low-SES	0.40	0.20	0.60	0.38	0.45	0.40	0.43
	(0.22)	(0.14)	(0.18)	(0.22)	(0.23)	(0.22)	(0.23)
Share Black/Hispanic	0.20	0.082	0.39	0.20	0.21	0.13	0.27
	(0.25)	(0.11)	(0.30)	(0.25)	(0.25)	(0.19)	(0.27)
ELA test scores	-0.077	0.43	-0.59	-0.055	-0.14	-0.067	-0.099
	(0.86)	(0.73)	(0.80)	(0.86)	(0.87)	(0.83)	(0.89)
Math test scores	-0.12	0.36	-0.60	-0.091	-0.21	-0.12	-0.13
	(0.86)	(0.75)	(0.81)	(0.85)	(0.87)	(0.83)	(0.88)
House price index ($1989 = 100$)	171.1	170.1	174.3	165.3	188.4	174.8	180.5
	(58.7)	(52.9)	(62.9)	(53.2)	(69.7)	(56.3)	(61.7)
Number of districts	11,468	2,700	3,078	7,993	3,475	6,929	5,986
Number of states	31	26	28	24	7	19	18

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)
1-4 years	605***	839***	7	14	-1	-0
	(47)	(69)	(19)	(25)	(2)	(2)
5-8 years	-113*	-13	9	13	5	7
	(60)	(85)	(29)	(40)	(4)	(6)
9-12 years	-87	-15	14	8	5	13
	(59)	(90)	(25)	(38)	(8)	(11)
13-16 years	-89	-17	-1	-82	-10	-9
	(80)	(127)	(38)	(57)	(6)	(10)
District FE	X	X	X	X	X	X
Year-State FE	X	X	X	X	X	X
Mean of dep. var.	1,259	1,259	6,954	6,954	502	502
Adj. R^2	0.223	0.225	0.937	0.937	0.672	0.673
N	134,640	134,640	134,640	134,640	134,640	134,640

Note: Estimates and standard errors of linear combinations of the parameters β_τ^{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	0.023*	0.027		0.023	0.033**	-0.004	0.002	0.007**	2.176	0.019
	(0.012)	(0.017)		(0.021)	(0.016)	(0.005)	(0.002)	(0.003)	(1.374)	(0.015)
5-8 years	0.050**	0.073**		0.067*	0.081***	-0.005	0.009*	0.023***	5.072*	0.044*
	(0.021)	(0.029)		(0.037)	(0.025)	(0.012)	(0.005)	(0.008)	(2.661)	(0.023)
9-12 years	0.055**	0.067**		0.045	0.088***	-0.001	0.015**	0.030***	6.507**	0.021
	(0.021)	(0.029)		(0.032)	(0.029)	(0.016)	(0.006)	(0.010)	(2.866)	(0.027)
13-16 years	0.057**	0.095**		0.057	0.126***	0.020	0.014	0.013	5.702*	0.050
	(0.025)	(0.037)		(0.041)	(0.038)	(0.020)	(0.009)	(0.011)	(3.379)	(0.036)
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^2	0.835	0.836		0.827	0.858	0.994	0.977	0.890	0.911	0.844
N	818,055	818,055		398,698	419,353	220,028	213,787	203,593	82,737	740,118

Note: Estimates and standard errors of linear combinations of the parameters β_τ^{TOT} in equation (2). The dependent variables are pooled test scores (columns 1-2 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***	0.006	0.012**	0.000	0.013**	-0.004	0.013**	0.006*	0.012**	0.008	-0.000
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.003)	(0.005)	(0.008)	(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	692,775	481,485	211,290	189,129	240,827	215,066	204,208	224,485	154,851	57,761	218,442

Note: 2SLS estimates and standard errors of the parameter ρ 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	-0.014***	-0.011***	-0.017***	-0.012***
	(0.001)	(0.002)	(0.001)	(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^2	0.31	0.54	0.36	0.57
N Districts	3,233	2,302	3,233	2,301
N Bond elections	9,916	8,966	9,915	8,964

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. Standard errors in parentheses are clustered at the district level. $*$ = 0.1; $**$ = 0.05; $***$ = 0.01.

Appendix Figures

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

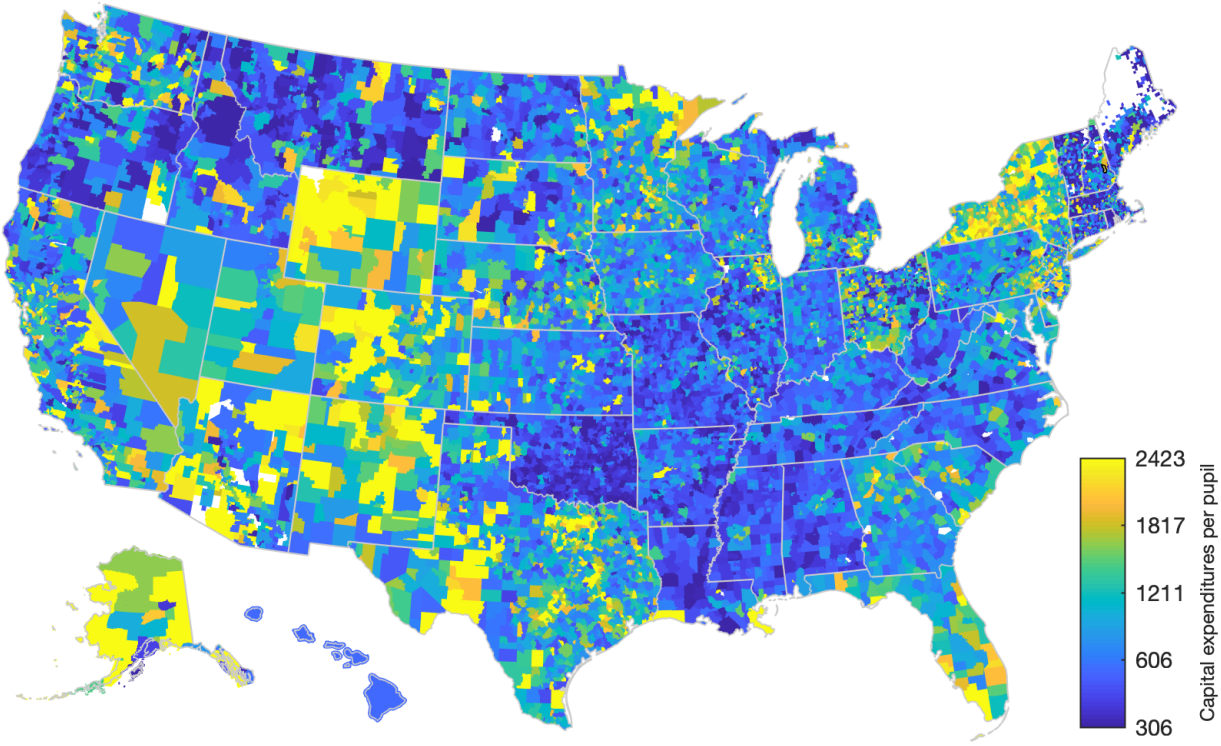
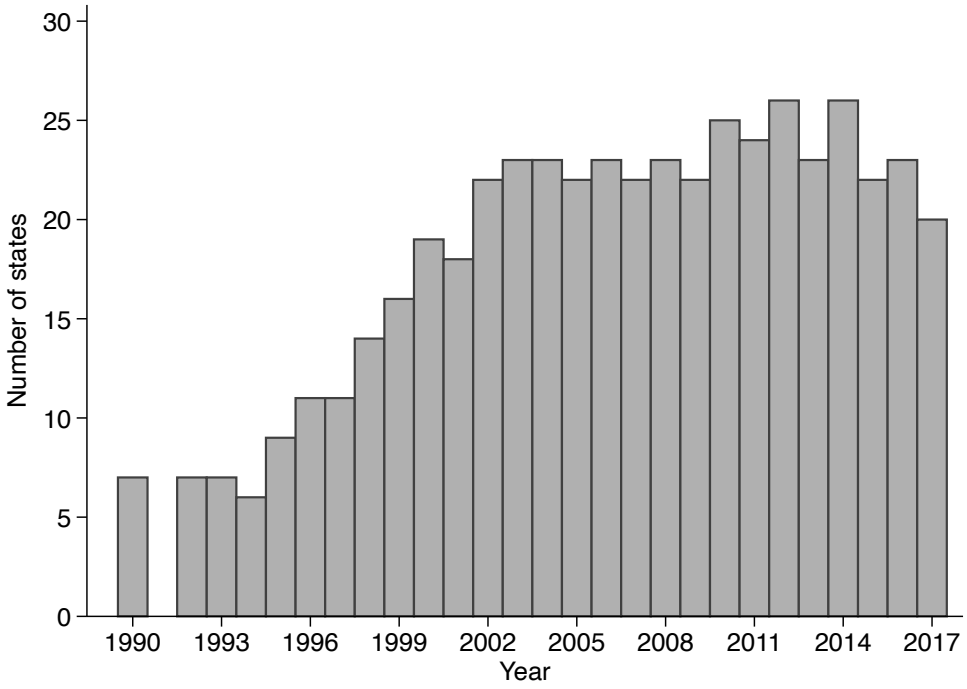
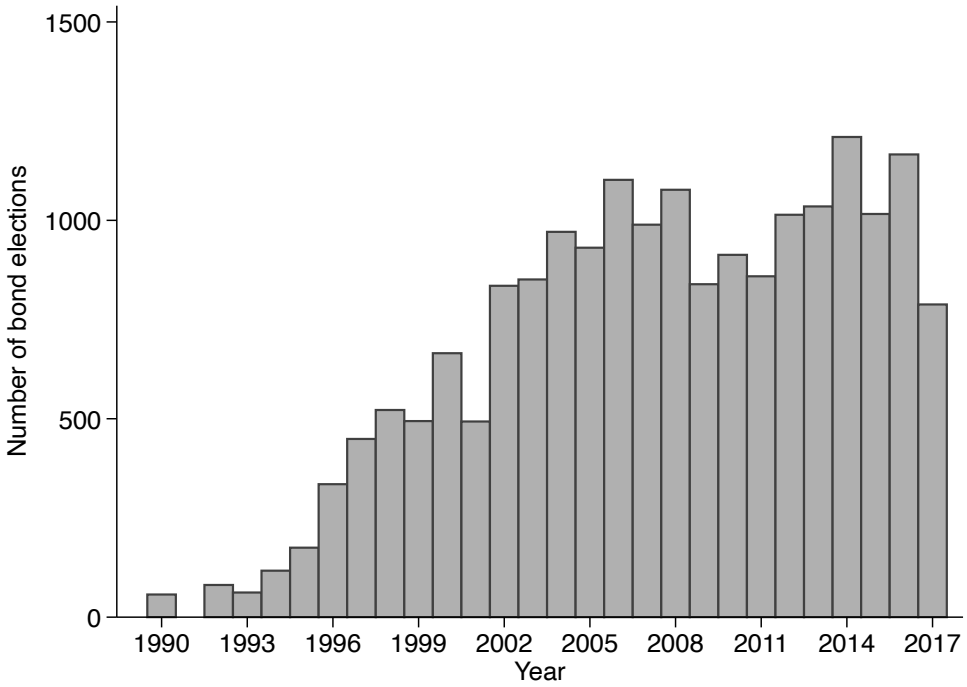


Figure A2: Bond Data Coverage, by Year



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



(b) Number of Bond Elections

Figure A3: First Year with Test Score Data, by State

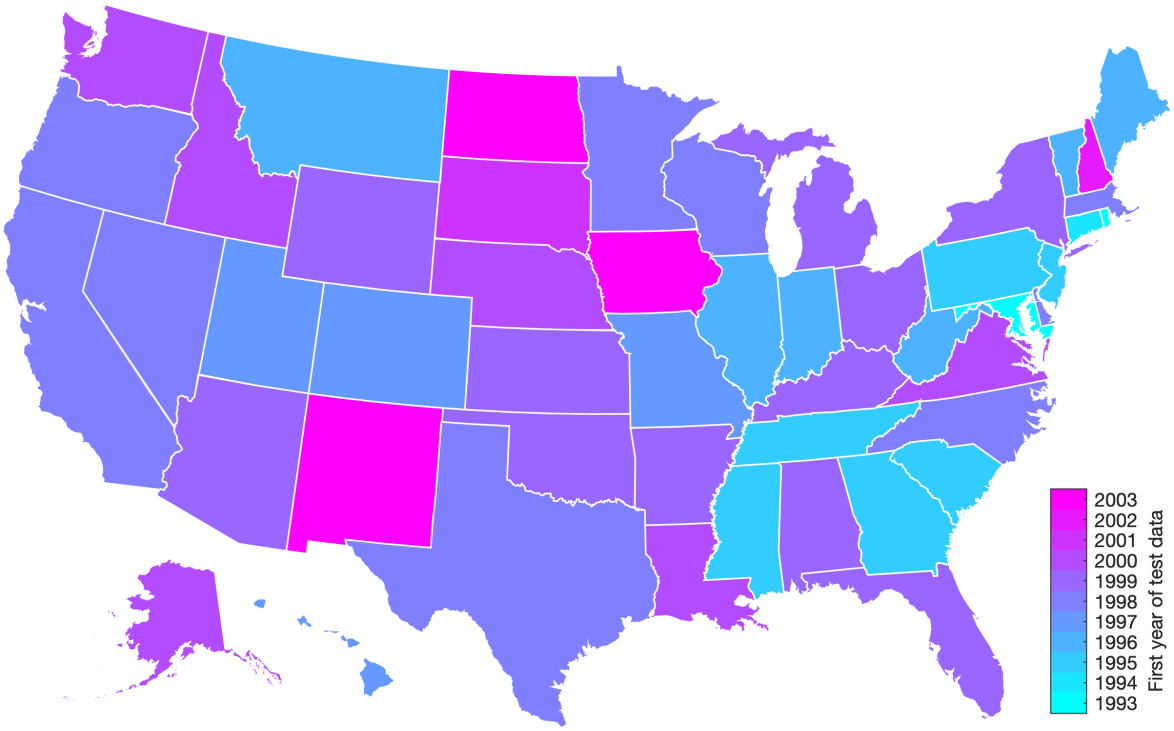
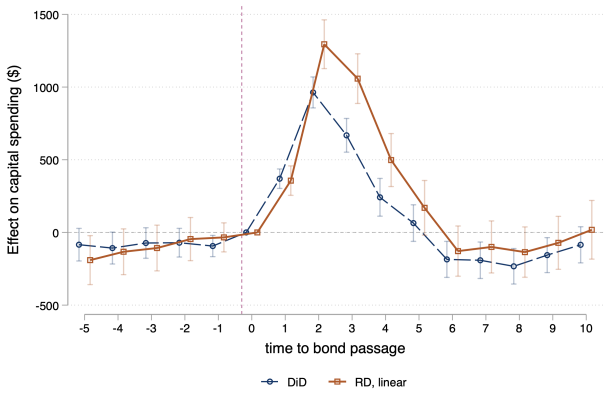
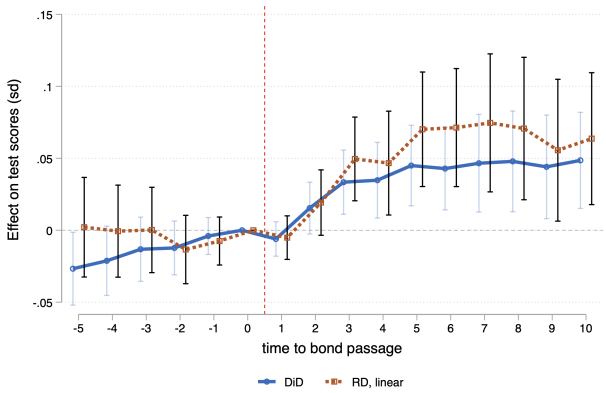


Figure A4: Effects of Bond Passage on Capital Spending, Test Scores, and House Prices: Stacked Difference-in-Differences

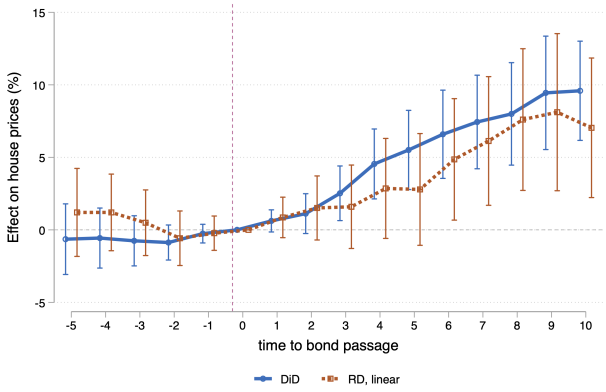
(a) Panel (a): Capital spending



(b) Panel (b): Test scores



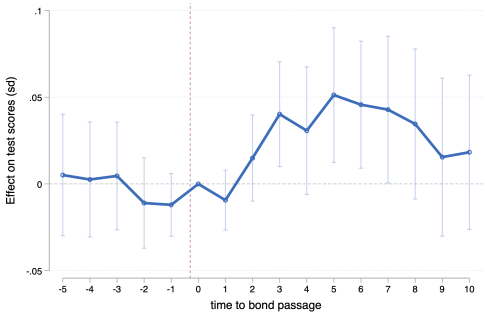
(c) Panel (c): House prices



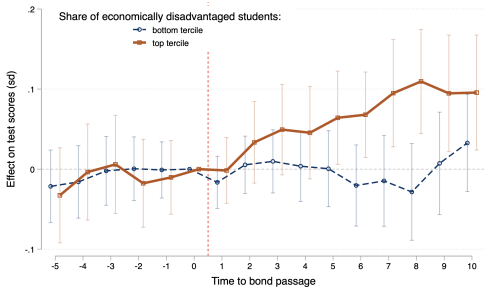
Notes: Estimates and confidence intervals of the parameters β_{τ}^{TOT} in equation (2) obtained using the stacked DiD/RD approach of Cengiz et al. (2019). Standard errors are clustered at the district level.

Figure A5: Effects of Bond Passage on Test Scores: Controlling for The Composition of The Student Body

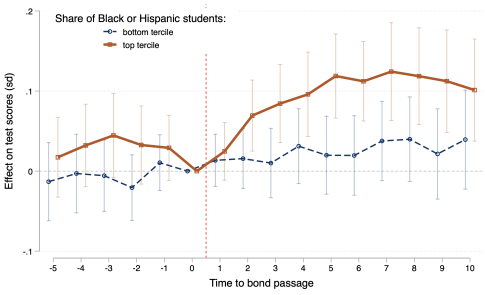
(a) Mean effects



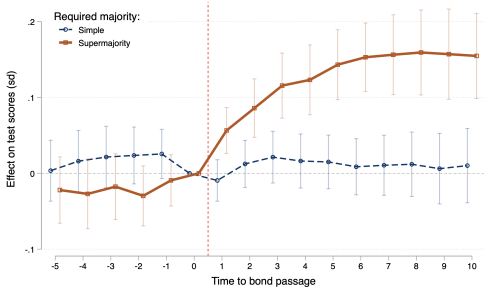
(b) By share of low-SES students



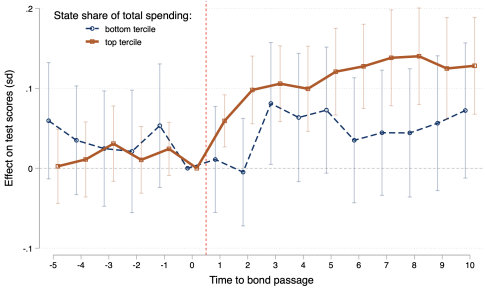
(c) By terciles of the share of minority students



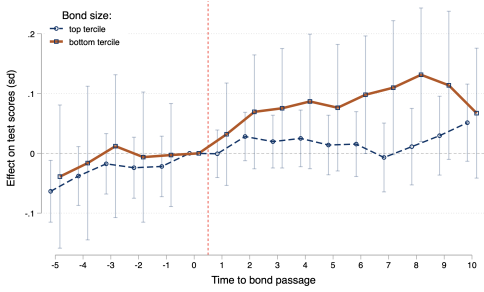
(d) By terciles of the majority requirement



(e) By state share of capital spending



(f) By terciles of bond size



Notes: Estimates and confidence intervals of the parameters β_{τ}^{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.