



Investing in schools: capital spending, facility conditions, and student achievement☆



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ABSTRACT

Public investments in repairs, modernization, and construction of schools cost billions. However, little is known about the nature of school facility investments, whether it actually changes the physical condition of public schools, and the subsequent causal impacts on student achievement. We study the achievement effects of nearly 1400 capital campaigns initiated and financed by local school districts, comparing districts where school capital bonds were either narrowly approved or defeated by district voters. Overall, we find little evidence that these school capital campaigns improve student achievement. Event-study analysis focused on the students actually affected by large campus renovations also generates very precise zero estimates of achievement effects. Thus, U.S. school capital campaigns financed by local districts – the predominant method through which facility investments are made – may be a limited tool for realizing substantial gains in student achievement or closing achievement gaps.

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1. Introduction

The Coleman Report (1966) ignited an enduring debate on the importance of school spending by concluding that school resources play a limited role in improving student outcomes. Many empirical studies followed with some concluding that there is no systematic relationship between school resources and student outcomes (Hanushek, 1986) and others concluding the opposite (Greenwald et al., 1996; Card and Krueger, 1996; Jackson et al., 2015). While these studies typically examine the impacts of instructional resources (e.g., teacher compensation

and class size), the physical condition of school buildings is another important component of school resources.

State and local governments invest an enormous amount on public school facilities, with annual expenditures totaling about \$66 billion (or \$1336 per student; Snyder and Dillow (2011)) and \$407 billion in outstanding taxpayer-supported bond debt attributable to school facilities (U.S. Census Bureau, 2012). Despite the magnitude of such investments, many students, especially those from disadvantaged backgrounds, attend schools that are in a state of disrepair (Filardo et al., 2010), with \$300 billion in deferred maintenance needed to bring U.S. schools into “good” condition (ASCE, 2009). The prevalence of public schools in need of repair is worrisome because poor physical environments may impede student achievement if students learn more easily in safe, clean, controlled environments (Jones and Zimmer, 2001).

Indeed, recent evidence on the impacts of very large construction projects in contexts where school facilities were either in very poor condition or non-existent suggests that new school construction projects can improve student outcomes (Duflo, 2001; Aaronson and Mazumder, 2011; Neilson and Zimmerman, 2014). For instance, Neilson and Zimmerman (2014) find positive effects on reading achievement of a construction project financed through state and federal sources that cost \$70,000 per pupil and involved rebuilding almost every school campus in an urban district (New Haven, CT). However, this type of capital campaign is atypical in the U.S. where school capital projects (both renovations and new construction) are primarily financed locally through the issuance of voter-approved

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bonds that are repaid with property taxes. For instance, the average per-pupil size of capital campaigns in Texas, the state we study in this paper, is about \$7800. The achievement effects of investments of this magnitude remain unclear. Cellini et al. (2010; henceforth CFR) find that school bond passage in California increases housing prices, but they only find modest and imprecisely estimated effects on student achievement.

In this paper we provide the most comprehensive assessment of achievement effects from school facility investments initiated and financed by local school districts. The first part of the analysis examines the impact of nearly 1400 capital campaigns initiated by 748 school districts in the state of Texas over a 14-year period. To address the concern that districts conducting such campaigns are different from those that do not, we use dynamic regression-discontinuity methods (Cellini et al., 2010) to compare school districts where bond referenda narrowly pass to those that narrowly fail. We examine the impact of capital campaigns on student outcomes using information on all tested students in the state over this time period, which includes all 3rd through 8th graders and 10th or 11th graders that take the state's high school exit exam.

Texas is an interesting state in which to conduct this analysis for several reasons. First, it is a large and diverse state with a combination of small rural districts and very large urban ones, many of which conducted capital investment campaigns during our study period. Second, the institutional context for funding facility investments in Texas differs from that in California, and our analysis thus provides a useful counterpoint to CFR's study in California. California has a number of institutional constraints (such as Proposition 13) that make it difficult for districts to raise funds to finance school facility improvements.¹ Capital spending could therefore be lower than the value preferred by local residents, which may help explain the CFR finding that school bond passage increased housing prices. In contrast, similar constraints do not exist in Texas. However, even in this context, there still may be positive effects of capital investments on student achievement. This is because in districts where residents choose to have low levels of spending for new school facilities (e.g., in poor districts that do not have the tax base to sustain a high level of capital spending), facilities may be in poor condition. Indeed, a third reason why TX is an interesting setting for this analysis is that a significant number of schools in the state are in need of repair. In these cases, facility investments could generate improvements in student outcomes if school building conditions exert a causal effect on student outcomes.

We find clear evidence that locally-funded campaigns lead to large increases in capital investment that are concentrated in the first two post-election years. Crucially, we find no effects on operating spending or on average class size, suggesting that funds raised through bonds “stick” to the capital account and are not reallocated to operating costs. We also find little evidence that capital campaigns attract students into school districts or help districts retain teachers. We also find that locally financed capital campaigns lead to measurable, yet modest changes in facility conditions. To our knowledge, this analysis is the first to look at the causal effect of typical bond-funded capital campaigns on the actual schooling environments of students. Three years after bond passage, average district-wide campus age decreases by merely 1.4 years; time since last major renovation or building construction decreases by 6.5 years; and the share of students enrolled in schools opened in the past four years increases by 3.6% age points on a base of 6%. Capital campaigns increase the likelihood that older schools are in at least fair or good condition; they also alleviate overcrowding in older schools (although overall district effects are insignificant).

Despite the investment, we find little evidence that school capital campaigns improve student outcomes. Our main RD point estimates for grades 3 to 8 are a small 0.016 and 0.030 standard deviation increase for reading and math, respectively, in year six (p -values = 0.438, 0.269) and we can rule out effects as large as 0.06 and 0.08. Estimates are

smaller or negative prior to year six. Difference-in-differences models (comparing districts before and after bond passage or failure) can rule out achievement effects greater than 0.03 and 0.05 for reading and math, respectively. The comparability of RD and difference-in-difference estimates suggests that effects of bond passage for marginal and inframarginal elections are similar, so the effects do not obviously vary with the support for bond passage.

Given that typical capital campaigns deliver only modest facility improvements for the average student, it may be unsurprising that overall achievement effects are also small. Most students simply do not attend schools that received large capital investments. To address this issue, the second part of the study directly measures the effect of capital investment on students actually exposed to it by analyzing more than 1300 major campus renovations. Controls for lagged individual test scores permit us to address changes in student composition resulting from capital investment, analogous to “value-added” models of teacher effectiveness. With or without this adjustment, we find no evidence of achievement effects of major campus renovations, even for renovations that appear to have generated large improvements in school facility conditions. Our estimates are sufficiently precise such that we can rule out positive effects larger than about 0.02 for math and 0.01 for reading for the first four years following a campus renovation.

Taken together, our analysis of capital campaigns and major renovations suggests that the typical school facility investments initiated and financed by local school districts do not generate appreciable improvements in student achievement. However, even with small effect sizes, school facility spending could still be a worthwhile use of resources since facilities are durable and can benefit many cohorts of students. To address this issue, we conducted a cost-effectiveness analysis comparing the cumulative test score impacts of facility investments implied by the largest effect size consistent with our event study estimates to the test score impact of a comparable increase in instructional spending to reduce class size. The results suggest that under reasonable assumptions regarding the durability of school facilities, school facility investments are unlikely to generate cumulative test score gains as large as those that could be obtained by reducing class size.

We describe the context of facilities funding in Texas and its implications for student outcomes in the next section. Sections 3 and 4 describe our data sources and methods, respectively. Section 5 presents our main RD results for district spending, school conditions, and student achievement. Event-study estimates of the effect of campus renovations and openings are presented in Section 6. We interpret the magnitudes and cost effectiveness of capital interventions in Section 7 and conclude in Section 8.

2. School facility spending in Texas and its potential effects on student outcomes

In 2008, total funding for Texas public schools was \$10,600 per student, of which \$1280 (12%) was spent on school facilities. The vast majority of these funds are raised internally by local school districts. Texas' well-known school finance equalization program, the Foundation School Program (FSP), was developed to address historical disparities in per-pupil funding across districts. This policy determines the amount of state and local funding for school districts and also determines the allocation of state funds to local districts. FSP aims to ensure that all districts receive “substantially equal access to similar revenue per student at similar tax effort” taking into account all state and local tax revenues of districts, student and district cost differences, and differences in property wealth (Texas Education Code, §42.001(b)). However, FSP mainly covers operational expenditures; responsibility for facility spending falls primarily on school districts. State and federal funding each account for about 10% of facility spending, with the remainder coming from districts (Table 181; Filardo et al., 2010). Thus, modernization, renovations, and repairs of Texas public educational facilities are financed primarily through local property taxes with minimal state support, a setting typical of most states.

¹ Proposition 13, passed in 1978, capped the property tax rate at 1% and has been blamed for the decline in school spending in California (Sonstelie, Brunner, and Ardon, 2000).

In Texas, local districts are fiscally independent and have taxing authority with which to raise funds for capital improvements, principally by issuing bonds. This is in contrast to the California context studied in CFR, which has policies such as Proposition 13, which place strong limits on the ability of school districts to raise property tax revenue. A share of property tax revenue is then used to pay debt service costs (principal and interest). Voters must approve bond referenda by a simple majority to issue school bonds and the associated, concurrent increase in property taxes. An example of a ballot proposition for one Texas school capital campaign is for the Ector school district:

Shall the Board of Trustees of Ector County Independent School District be authorized to issue bonds of the District as authorized by law at the time of the issuance thereof, in one or more series, in the aggregate principal amount not to exceed \$129,750,000, for the construction and renovation and equipping of high school facilities, the construction and equipment of elementary school facilities and the acquisition of any necessary school sites and new school buses, with any surplus proceeds with to be used for the construction, renovation and equipping of other school facilities in the District; with the bonds to mature, bear interest, and be issued and sold in accordance with law at the time of issuance; and shall the Board of Trustees be authorized to levy and pledge, and cause to be assessed and collected, annual ad valorem taxes, on all taxable property in the District, sufficient, without limit as to rate or amount, to pay the principal of and interest on the bonds and the cost of any credit agreements executed in connection with the bonds?

The language is typical of school ballot propositions calling for bond financing for a capital campaign to construct and renovate schools but also calls for providing funds for land acquisition and purchase of new school busses. Recent evidence suggests that Texas capital campaigns targeting renovations as opposed to new construction are more likely to be approved. Also, districts with larger fractions of Hispanics and fewer persons 65 and older are more likely to approve bonds (Bowers and Lee, 2009). In 2010, total outstanding debt from bonds issued by Texas districts for school facilities was \$63 billion (U.S. Census Bureau, 2012).

Although the state supports districts' ability to raise capital inexpensively through a variety of loan assistance programs (Clark, 2001), large school infrastructure needs still exist, particularly in poor districts. A 1991 census of all school facilities indicated that Texas districts had significant unmet needs, with the cost of meeting them between \$2 and 3 billion (1990 dollars), including replacing space rated below "fair" condition, relieving overcrowding and portable space use, and adding space for science labs and libraries. Furthermore, "buildings in poor districts are in worse condition than those in wealthy districts" (Texas Education Agency, 1992).

More recent evidence suggests that unmet capital needs remain. For instance, the 614 districts responding to a 1997 survey anticipated a total of \$9 billion in repairs, renovations, and new construction over the next 5 years, with critically needed repairs costing \$4.1 billion (TCPA, 1998). Needs tended to be greater in heavily minority districts. In a 2006 survey, 6% of districts reported that their instructional facilities were in "poor" condition or warranted replacement (TCPA, 2006). Also, a substantially higher rate of instructional portable space was reported in use in districts with many economically disadvantaged students. In summary, although the Texas school financing system helps equalize operational spending across districts, wide disparities in facilities conditions and capital investments remain.²

² National surveys suggest that conditions in Texas school facilities are roughly comparable to those across the country. A 1999 survey of 903 public schools found the average age of instructional buildings was 40 years with a functional age of 16 years. Older schools were more likely to report unsatisfactory conditions (USDOE, 2000). A 2005 survey found that 15% of schools were overcrowded (USDOE, 2007). In comparison, the average age of facilities in Texas in 2006 was 34 years with a functional age of 9 years.

These disparities and the overall prevalence of schools in poor condition in Texas are worrisome to the extent that physical school environments affect student outcomes. There are several reasons why such effects may exist. For instance, schools may have overcrowded classrooms that can impede teaching and student learning (Rivera-Batiz and Marti, 1995). Another possibility is that outdated, malfunctioning building systems can lead to poor indoor air quality, ventilation, and temperature control (Mendell and Heath, 2005). Substandard facilities may thus result in chronic distractions and missed school days (Earthman, 2002). Older schools, which have not been renovated or building systems not retrofitted, may not have the infrastructure to support the latest technology (Lyons, 1999) or could lack modernized labs for science education. Low-quality educational facilities could dampen enthusiasm and effort on the part of teachers (Uline and Tschannen-Moran, 2008), thereby affecting teacher retention, which could in turn affect student performance (Buckley et al., 2004; Loeb et al., 2005). Consistent with these claims, student achievement has been shown positively associated with district-level capital spending (Crampton, 2009; Jones and Zimmer, 2001). The analysis in this paper will shed light on whether this association reflects a causal relationship.

3. Data sources and summary statistics

Our analysis draws on four sources of data at the student, district, and campus levels, which are then aggregated to the district-year level for most of the regression discontinuity analysis. Event-study analysis uses disaggregated student microdata combined with campus-level information.

3.1. Bond election data

From the Texas Bond Review Board, we acquired data on the election date, bond amount, and result for 2277 separate school bond propositions put up for a vote by Texas public school districts from 1997 to 2010.³ We collected vote share data from 812 school districts (98% of districts holding elections) along with supporting documentation via public information requests. Whenever there were multiple propositions considered during the same academic year, we used the characteristics (size, vote share, result) for the largest proposition (by bond amount) as our "focal" election for that district in that year. In these cases, there was usually a single large proposition for buildings and renovations and then one or two smaller propositions for athletic facilities or gymnasiums. In our analysis window there were 1737 district-years in which an election was held, so that on average districts held elections about twice during our study period. Table 1 provides descriptive statistics about the elections during this time period. Voters approved 80% of these bond measures, with an average vote share of 64%. The mean (median) bond amount was \$11,086 (\$7756) per student (in \$2010).

3.2. District- and campus-level longitudinal data

From the Texas Education Agency (TEA) Academic Excellence Indicator System (AEIS) data system, we measure the number of campus types (elementary, middle, secondary, both), number of schools opening/closing by type, student-teacher ratio by campus type, and average student demographics for 1994 to 2011. We also construct the share of enrollment in new schools (opened in the past year or four years) annually. Annual data on expenditures per student at the district-level was obtained from the Common Core Data.

³ We adopt the convention used by the Texas Education Agency to refer to academic year by the end year. For instance, 2000 refers to the academic year September 1999 to August 2000.

Table 1
Summary statistics of capital bond elections.

Year	Number	Pass	Vote share	Votes cast	Bond amount (millions of \$2010)		Bond amount per student (\$2010)		Multiple Elections held
					Mean	Median	Mean	Median	
1997	36	0.86	0.69	2003	36.1	17.7	6913	4884	0.19
1998	185	0.85	0.70	1181	24.5	10.0	7032	5311	0.11
1999	120	0.84	0.67	3493	59.4	13.7	8805	6866	0.17
2000	166	0.83	0.69	1116	35.0	8.8	7698	6064	0.13
2001	121	0.83	0.68	1636	48.3	9.5	8962	7576	0.21
2002	137	0.82	0.66	2075	48.1	11.1	8486	6717	0.12
2003	105	0.70	0.62	3669	70.4	18.2	10,353	7941	0.25
2004	114	0.84	0.63	2993	68.5	24.9	9653	5995	0.35
2005	95	0.69	0.60	2849	64.1	23.1	12,433	8689	0.31
2006	138	0.82	0.62	1561	57.3	22.3	11,777	8937	0.23
2007	180	0.86	0.63	3072	56.9	21.6	14,255	11,187	0.23
2008	156	0.77	0.60	2970	102.0	23.1	16,110	12,037	0.15
2009	85	0.73	0.58	4723	34.6	13.9	23,135	12,783	0.25
2010	98	0.61	0.55	1489	29.9	13.9	10,984	8992	0.13
All	1737	0.80	0.64	2392	53.3	15.2	11,086	7756	0.19

Notes: Elections were held in 812 unique school districts. Year refers to the end of the academic year (September–August). Omits 33 elections for which vote share data was not obtained. For districts that held multiple elections during the same year (typically multiple propositions on the same ballot), statistics reflect either the earliest (if elections on different dates) or largest (by bond amount) bond proposition. Sources: NCES Common Core Data (annual district enrollment), Texas Bond Review Board (bond elections held by Texas local school districts), public records requests by authors (election vote share).

3.3. Age and condition of school facilities

To better describe the impact of bond passage on building infrastructure, we obtained information about the age, time since last renovation, and room or building condition of nearly all campuses in 1991 and in a subset of districts in 2006. The 1991 data come from a facilities engineering assessment of all public school buildings commissioned by TEA. From data on the square footage, overall condition, year built, and year last renovated for each identifiable room, hallway, and other spaces at each campus, we construct the space-weighted mean of room condition and building age for each campus. We have successfully digitized this data for nearly all campuses and districts, 804 of which held bond elections during our analysis window. The 2006 data come from a voluntary survey conducted by the Texas Comptroller of Public Accounts with responses from 302 districts (228 that held elections), including 3548 instructional facilities (accounting for about half of the state's student population). This survey includes year built, year last renovated, overall condition (excellent, good, fair, poor, needs replacement), square footage, number and square footage of portable buildings, and total student capacity at the campus level. The 1991 and 2006 data were combined with AEIS data on school openings to calculate the building age and time since last renovation for each campus in each year, which is then aggregated to the district-level.⁴ Information on year built and last renovated was also directly used to identify major renovations and campus openings for the event-study analysis.

3.4. Student achievement, attendance, migration

Our primary outcomes are obtained from administrative records of the University of Texas at Dallas' Texas Schools Project. Specifically, we

⁴ Campus age is available for all years for the 804 digitized districts that held bond elections, but time since last renovation is only available through 2006 as we do not have information on renovations occurring after the 2006 survey. Furthermore, we only observe the timing of the most recent major renovation, so renovations are disproportionately clustered in the years leading up to the 2006 survey.

examine attendance rates and standardized test scores for all 3rd through 8th graders tested from 1994 to 2011 and high school exit exam scores for the same period. We focus on reading and mathematics scores for students in grade 3 to 8 and high school exit exam scores for these two subjects, as these are available for the entire study period. Exit exams are typically taken in the 10th or 11th grade. Since the tests are not comparable across grades within a year and since there were changes in the tests used over time, we standardize raw scores in the micro data by grade and year. To examine attendance, we calculate the fraction of days each student is in attendance in each academic year. For our main RD analysis, microdata are aggregated to district-year means (overall and for various subgroups) and deciles to assess how the full distribution of outcomes is altered by bond passage and subsequent capital

Table 2
Summary statistics of district characteristics in year prior to election.

	Year prior to election		
	All elections	Passed	Failed
Total enrollment	6723	7154	4995
Fraction white	57.9	57.4	59.6
Fraction black	8.5	8.1	10.3
Fraction hispanic	32.1	32.9	28.8
Fraction econ disadvantaged	47.0	46.9	47.4
Fraction LEP	8.4	8.9	6.7
Fraction special ed	12.8	12.8	12.8
Fraction vocational ed	21.7	21.4	22.9
Fraction bilingual	7.6	8.0	6.2
Fraction gifted	7.3	7.3	7.0
Instructional spending per student (\$2010)	5202	5182	5284
Capital outlay per student (\$2010)	1305	1354	1107
Close at least one campus	0.146	0.151	0.127
Open at least one campus	0.230	0.244	0.173
Student–teacher ratio – overall	13.529	13.597	13.258
Fraction of teachers leaving campus	0.228	0.228	0.230
Share of enrollment in schools opened this year	0.015	0.016	0.011
Share of enrollment in schools opened in past four years	0.060	0.062	0.051
Enrollment-weighted average age of school buildings	35.717	35.123	38.114
Enrollment-weighted average years since school last renovated	13.342	13.146	14.221
Reading test scores (grades 3 to 8)			
District-wide mean	0.027	0.030	0.016
Free lunch mean	–0.270	–0.269	–0.276
Not econ disadvantaged mean	0.200	0.200	0.199
Gap: 90–10 percentile	2.028	2.023	2.047
Gap: Not econ disadv – free lunch	0.470	0.469	0.472
Math test scores (grades 3 to 8)			
District-wide mean	0.023	0.027	0.007
Free lunch mean	–0.269	–0.265	–0.287
Not econ disadvantaged mean	0.184	0.184	0.180
Gap: 90–10 percentile	2.192	2.187	2.212
Gap: Not econ disadv – free lunch	0.452	0.450	0.463
Reading test scores (exit exam)			
District-wide mean	0.048	0.050	0.042
Free lunch mean	–0.265	–0.263	–0.272
Not econ disadvantaged mean	0.142	0.143	0.139
Math test scores (exit exam)			
District-wide mean	0.039	0.044	0.015
Free lunch mean	–0.303	–0.295	–0.332
Not econ disadvantaged mean	0.138	0.141	0.126
Attendance rate (fraction of days)			
District-wide mean	96.40	96.41	96.34
Gap: 90–10 percentile	7.86	7.86	7.88
Gap: Not econ disadv – free lunch	1.15	1.15	1.14
Student in-migration rate (all grades)	0.143	0.144	0.137
# Districts	812	748	279
# Elections	1737	1390	347

Notes: Most variables are defined for the full sample of 1737 unique elections. Enrollment-weighted average building age (years since renovation) are only available for 530 (227) districts and 1132 (464) elections.

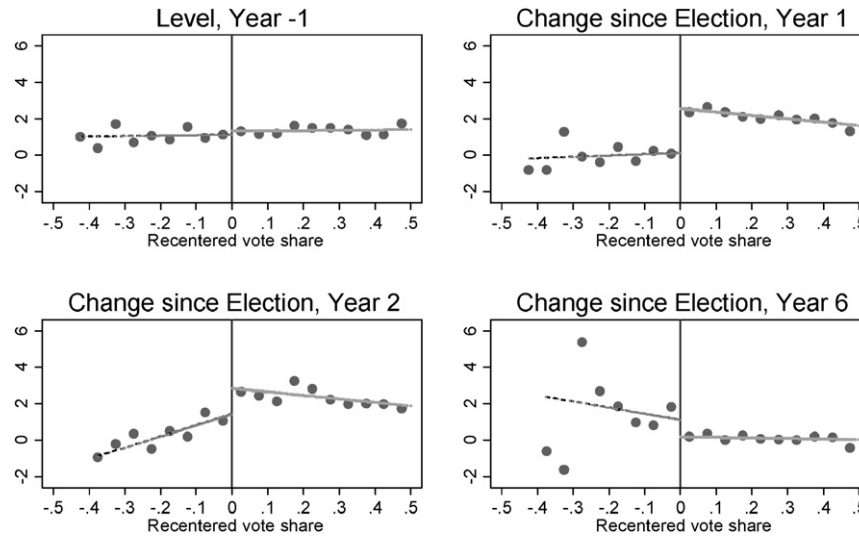


Fig. 1. Level and change in capital spending by vote share, before and after bond election. Notes: Graphs plot average district capital spending (in \$1000) or change in average district capital spending (relative to years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5 point bins of vote share. Includes data for 1737 elections and 812 districts. Spending data is from the NCES Common Core.

investment.⁵ We also use the micro data to calculate the share of students (2nd through 12th grade) that are new to the district in each year. Finally, the disaggregated student-level micro data are also used in event-study analysis of campus renovations and school openings.

Table 2 summarizes characteristics of districts in the year prior to a bond election, separately by whether the proposition was successful. Successful elections tend to be in larger districts that are spending slightly more on capital investment (and have higher rates of school openings) at baseline than unsuccessful elections. Student achievement is only slightly better at baseline in districts whose bond elections pass.

4. Empirical strategy

We employ two empirical strategies to estimate the effect of school facility investments. The first is a regression-discontinuity research design based on close school bond elections. The second is an event study analysis of the impact of school renovation and openings.

4.1. Regression discontinuity with panel data

The regression discontinuity (RD) model is based on the observation that even if districts in which a bond measure passes tend to be different from districts where bond measures fail, these differences likely shrink as comparisons focus on close elections (Lee, 2008). When this condition holds, we can attribute outcome differences between students who live in districts that narrowly pass and fail to post-election variation in capital spending.

⁵ To preserve data richness while complying with data confidentiality requirements, the aggregation to district-level outcomes is done as follows. From the micro data we calculate the mean, standard deviation, and number of observations for student groups defined by campus X grade (3rd through 8th or exit) X economic status (free-lunch eligible, reduced-price lunch eligible, not economically disadvantaged) for each year from 1994 to 2011 whenever this cell contains at least five tested students and a non-zero standard deviation. These cells are then aggregated to district-level means using the cell size as weights. Since some cells are missing due to small samples, the district average will reflect the average for non-missing groups, rather than the population of all students in the district. We do not obtain the district-level mean as that would potentially allow us to back out the mean for a non-disclosed group. District-level deciles combine students from all grades and economic status groups, but are only reported for districts with at least 100 tested students.

For an outcome Y (such as student test scores) observed τ years after a bond election was held in district j in year t , we estimate models of the form:

$$Y_{j,t+\tau} = \theta_{\tau} \text{Pass}_{j,t} + f_{\tau}(v_{j,t}) + \varepsilon_{j,t+\tau}, \quad (1)$$

where $\text{Pass}_{j,t}$ is an indicator for whether the bond measure passed and f is a flexible function of the vote share $v_{j,t}$, and $\varepsilon_{j,t+\tau}$ is a residual. The model allows the effect of bond passage at time t to have different effects on Y depending on the length of time between bond passage and the outcome (as captured by the subscript “ τ ” on θ). Following Cellini et al. (2010), we first estimate (1) on a panel dataset constructed in the following way. First, for each district j that has an election in year t , we “stack” all district-year observations for this district in some window around t . For instance, if we choose a window from $t - 2$ through $t + 6$, a district holding an election in 2004 will include all observations for the period 2002–2010. Second, we combine the stacked datasets for each separate election into one large panel dataset covering the entire study period. Since multiple observations per district are included, we adjust all standard errors for clustering at the district level.

Our preferred estimates are from models that add controls for election and time fixed-effects to Eq. (1):

$$Y_{j,t+\tau} = \theta_{\tau} \text{Pass}_{j,t} + f_{\tau}(v_{j,t}) + \mu_{j,t} + \alpha_{t+\tau} + \delta_{\tau} + \omega_{j,t+\tau} \quad (2)$$

where $\alpha_{t+\tau}$ and δ_{τ} are calendar and relative year effects, respectively, $\mu_{j,t}$ is a district-election fixed-effect, and $\omega_{j,t+\tau}$ is an error term. The advantage of this specification relative to Eq. (1) is that the district-election fixed effects improve precision and control for changes in sample composition when we have an unbalanced panel. Note that it is possible to control for these election-specific fixed-effects even though vote share does not vary within an election over time because the coefficient on bond election passage and the function of the vote share are allowed to vary with the amount of time since bond passage but are constrained to zero in the pre-election period. We also estimate Eq. (2) without controlling for a function of the vote share, which is a standard difference-in-differences specification. This difference-in-differences model will yield more precise estimates than models with vote share controls, yet requires the additional identifying assumption that changes in unobserved determinants of outcomes are unrelated to bond passage.

Table 3

Effect of bond passage on educational inputs.

Two-part linear specification with election or district fixed effects.

	Effect of bond passage after						n
	1 year	2 years	3 years	4 years	5 years	6 years	
Panel A. Capital spending (ITT)							
Capital outlays per student (mean = \$1305)	2330** (298)	1230** (394)	− 735 (449)	− 415 (413)	− 579 (361)	− 723 (516)	14,455
Cumulative capital outlays since election	2595** (290)	3950** (546)	2875** (712)	2578** (854)	2100* (1005)	1514 (1153)	10,982
Panel B. Capital Spending (TOT)							
Capital outlays per student (mean = \$1305)	2745** (288)	2368** (376)	734 + (393)	469 (368)	309 (315)	− 109 (462)	12,172
Cumulative capital outlays since election	3199** (410)	5376** (678)	5271** (857)	5319** (968)	5738** (1125)	5007** (1363)	11,360
Panel C. Instructional inputs (TOT)							
Instructional spending per student (mean = \$5202)	− 46 (65)	27 (72)	32 (74)	96 (85)	176* (87)	158 + (87)	12,172
Student–teacher ratio (mean = 13.53)	− 0.239 (0.210)	− 0.240 (0.235)	− 0.229 (0.256)	− 0.216 (0.290)	− 0.136 (0.283)	0.038 (0.298)	14,602

Notes: Each row represents a separate specification and reports effects of bond measure passage on outcomes t years later. For Panel A (ITT), the sample includes all bond elections and all outcome measures from years -2 to $+10$ relative to each election. This specification includes fixed effects for each election, a linear function of the vote share with different slopes for passing and non-passing bonds, relative year fixed effects, academic year fixed effects, and interactions between bond passage and relative year fixed effects (for relative years 1 to 10). The table reports these passage \times relative year interactions. For Panels B–C, the sample includes yearly panel data from 1994 to 2011 for all 812 districts that held bond elections. Model includes indicators for bond passage, holding election, and vote share (with different slopes for passing and non-passing bond) in the current year and each previous year up to ten. The table reports the bond passage indicators for each lag. Estimates for current period and lags greater than six are not displayed. Reported mean is for the year prior to the election. Standard errors are clustered at the district level. Significance: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Eq. (2) will deliver valid estimates of the causal effect of school bond passage if districts in which a bond measure narrowly fails do not differ systematically from districts where the bond measures are narrowly approved in ways that are related to student outcomes. We present two pieces of evidence consistent with this condition. First, as shown in Appendix Fig. 1 the density of the bond measure vote share is “smooth” at the 50% threshold and a formal test (McCrary, 2008) fails to reject that the density is continuous (the estimated discontinuity in the density is 0.227 with a standard error of 0.164). Second, we find little evidence of discontinuities in the mean of district-level covariates at the 50% cutoff when estimating Eq. (2) using many pre-election characteristics as the outcome.⁶

One complication when implementing the RD model in this case stems from the fact that districts can (and do) hold elections in multiple years. Many “control” districts (those whose bond measures do not pass) are eventually “treated”. This implies that the models above identify an “intention to treat” (ITT) effect that combines both direct effects of the current bond election and indirect effects via subsequent election outcomes. In order to uncover the direct effect of bond passage (and capital investment) holding subsequent election outcomes constant, the “treatment on the treated” (TOT), we follow the “one-step” method proposed by Cellini et al. (2010). In this approach, we include indicators for bond election passage in each prior year, indicators for holding an election in each prior year, a polynomial function of the vote share in each prior year, district fixed effects, and calendar year fixed effects.⁷

$$Y_{j,t} = \sum_{\tau=0}^{\bar{\tau}} (\theta_{\tau} \text{Pass}_{j,t-\tau} + \varnothing_{\tau} \text{Elect}_{j,t-\tau} + f_{\tau}(v_{j,t-\tau})) + \mu_j + \alpha_t + u_{j,t} \quad (3)$$

This model is estimated on a standard district-year panel among districts holding elections, including all years from 1994 to 2011. The coefficients on lagged bond election passage, θ_{τ} , provide an estimate of the

⁶ The results (Appendix Table 1) reveal that few covariates have discontinuities that are statistically significant once we control for election fixed-effects. The one exception is that districts where the bond election barely passes appear to have slightly higher rates of English-language learners (ELL) and Hispanic students (and fewer white students), but given the number of covariates examined it is unsurprising to see some differences due to chance. Importantly, pre-election differences in all our main outcomes are small and insignificant.

⁷ Vote share is set to zero for observations in which no election was held.

causal effect of bond passage holding subsequent election outcomes constant. In this paper we primarily focus on TOT estimates, though present ITT estimates in the Appendix.

4.2. Event study analysis

A key limitation of the RD analysis is that we may not have enough statistical power to detect effects of policy-relevant size. The reason is that the bond passage treatment is diffuse; funds raised by a bond may only benefit a small subset of students in a district who are difficult to identify given that we do not have campus-level capital investment information. To address these issues, we use an “event study” framework to estimate the effect of large campus renovations. This approach approximates that used in Neilson and Zimmerman’s (2014) analysis of school constructions in New Haven, but using statewide data on a much larger number of facility investment events. It also offers potentially sizable power gains relative to the district-level RD since it focuses on students actually exposed to capital investment. The power gain results not only from improved precision of the estimates, which has to do with the number of renovations relative to the number of close bond elections. It also relates to the bond election treatment being diffuse relative to major renovations, in the sense that bond elections may only affect a small proportion of students in a district that we cannot identify, whereas we can identify exactly which students benefit from a school renovation. We return to this issue in Sections 6 and 7.

To quantify the effects of renovations, we estimated models of the following form:

$$Y_{igst} = \alpha + \sum_{p=-k}^k \theta_p D_{st}^p + \rho \text{Lag} Y_{igst} + \gamma_g + \lambda_t + \mu_s + X'_{ist} \beta + \epsilon_{igst} \quad (4)$$

where Y_{igst} is the outcome for student i in grade g attending campus s in year t , D_{st}^p is a dummy variable indicating campus s was renovated p years prior to t . The terms γ_g , λ_t , and μ_s are grade, year, and campus fixed effects, respectively. Student demographic controls are included in the vector X_{ist} . The parameters θ_p are the coefficients of interest, indicating the change in outcomes p years after renovation relative to trends at schools that were not renovated during this time (we normalize to the year of renovation by omitting D_{st}^0). Pre-renovation differences are

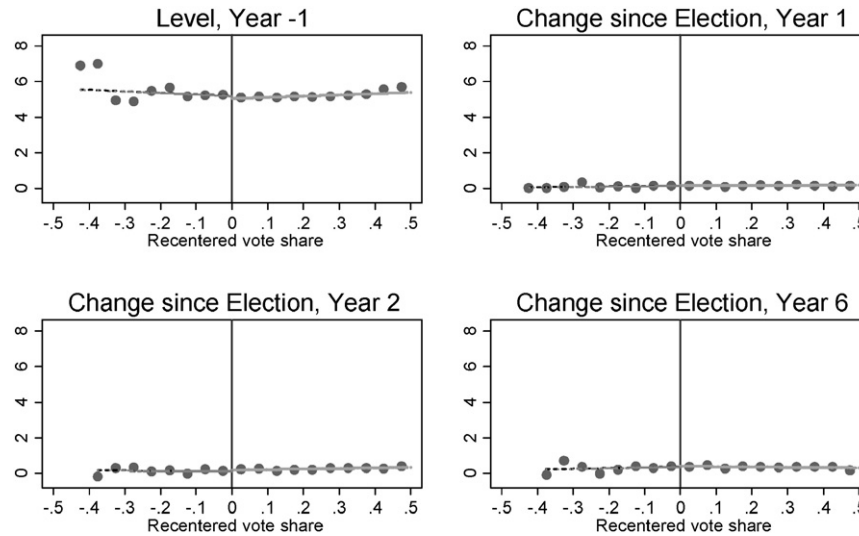


Fig. 2. Instructional Spending by Vote Share, Before and After Bond Election. Notes: Graphs plot average district instructional spending or change in average district instructional spending (relative to years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5 point bins of vote share. Includes data for 1737 elections and 812 districts. Spending data is from the NCES Common Core.

captured by these parameters for $p < 0$ while post-renovation differences are captured for $p > 0$.

We estimated these models on a sample of campuses that were open for the full panel and that had renovations during our study period, to mitigate sample selection bias. To identify renovated schools and the timing of renovations, we used information from the 2006 facility condition survey available for 302 districts, which identifies the date a school was last renovated. Identifying variation thus comes only from differences in the timing of renovation rather than in the existence of a renovation project. After making these restrictions, we have a sample of 1354 renovated schools in 235 districts serving 4th–8th graders. We also conduct an analysis on schools where the renovations appear to have generated large changes in school quality conditions. Specifically, for this analysis we focus on renovations where the campus average room condition was in the bottom two quintiles of campuses in the 1991 school facility census (before the renovation) but the campus was rated as “Good” or “Excellent” in the 2006 survey of school facilities (after the renovation).⁸

The assumption needed for the event-study estimates to be interpreted as causal effects is that the unobserved factors that affect student outcomes cannot be systematically correlated with the timing of school renovations or openings. This assumption is stronger than what is required for the RD analysis and could be violated if student outcomes were trending upward or downward leading up to renovations or if the composition of students changed following the event. We address these possibilities by examining outcome and characteristic trends leading up to renovations and by controlling for lagged student test scores (a “value added” specification) and other characteristics. As we discuss in our results, we see little evidence of pre-event outcome trends, which lends support to the causal interpretation of our estimates. Nonetheless, we also examine models that include campus-specific linear time trends to account for the possibility of pre-renovation trends.

5. Regression discontinuity results

5.1. Nature and timing of capital investments

Fig. 1 presents graphical evidence that bond passage results in a large, immediate increase in capital spending. In the year prior to the election (first panel), spending is similar for districts where bond

measures were approved or rejected, but in the year following an election, capital spending increases more than \$2000 per pupil in districts where the bond barely passed compared to those in which it barely failed. The spending increase persists through year two but reverses by year six.⁹ The top panel of Table 3 presents ITT estimates of the effect of close bond passage on annual and cumulative capital outlays, using our baseline specification that controls for election fixed-effects and a linear function of the vote share (with varying slopes on each side of the vote share threshold). Bond passage results in doubling (\$2333) of capital spending per student (2010\$) in the year following the election, with large and positive effects in the second year as well. Thereafter, the effects are negative and statistically insignificant, suggesting that increased capital investments occur shortly after the election. TOT estimates in Panel B show that bond passage has a positive effect on capital spending through year 3 and results in an increase in cumulative spending over 6 years of about \$5000 per pupil.¹⁰

Although the school bonds are explicitly targeted for capital investments, bond passage could increase spending on other school expenditure categories. However, the estimates in Panel C and the graphical evidence in Fig. 2, provides little indication that bond passage affects instructional inputs. In the first four years after the election, bond passage has a very small and statistically insignificant effect on instructional spending per student. We find a small but statistically significant increase in instructional in years 5 and 6, but the magnitudes – about 3% of the sample mean – are very small and this result is not robust to alternative specifications (Appendix Table A2).¹¹

5.2. School environments

How bond-funded capital campaigns actually alter the facility environments faced by students has not been established in prior literature (Cellini et al., 2010; Hong and Zimmer, 2014). Table 4 and Fig. 3 show

⁹ Fig. 1 and subsequent figures use a bandwidth of 5 percentage points and plot a linear prediction estimated on the underlying election data, not the aggregated bins. Similar figures with a 2.5 percentage point bandwidth and quadratic prediction are displayed in the Appendix.

¹⁰ As shown in Appendix Fig. A2, districts whose elections are successful are much less likely to hold or pass an election within four years, but the effect dissipates in later years.

¹¹ Appendix Table A2 shows TOT estimates using linear, quadratic, and cubic polynomials in the vote share. Because the TOT specification does not lend itself to restricting the running variable bandwidth, we also show ITT estimates in Appendix Table A4 that use different bandwidths as well as alternative polynomials.

⁸ We also examined school openings, but this analysis was underpowered and inconclusive. These results are described in Appendix B.

Table 4

Effect of bond passage on capital inputs and teacher mobility.
TOT, Two-part linear specification with election or district fixed effects.

	Effect of bond passage after						n
	1 year	2 years	3 years	4 years	5 years	6 years	
(1) Open at least one campus (mean = 0.230)	−0.017 (0.036)	0.112** (0.037)	0.073 + (0.041)	0.043 (0.045)	−0.016 (0.050)	0.118* (0.047)	13,794
(2) Share of enrollment in schools opened this year (mean = 0.015)	0.003 (0.004)	0.014** (0.005)	0.015* (0.006)	−0.003 (0.010)	0.001 (0.006)	0.012* (0.006)	14,603
(3) Share of enrollment in schools opened in past four years (mean = 0.060)	0.008 (0.010)	0.021 + (0.011)	0.036** (0.013)	0.036* (0.015)	0.026 + (0.016)	0.023 (0.017)	13,791
(4) Enrollment-weighted average age of school buildings (mean = 35.19)	−0.586 (0.536)	−0.612 (0.645)	−1.431* (0.727)	−0.880 (0.877)	−0.109 (0.958)	−0.051 (1.022)	14,477
(5) Enrollment-weighted average years since school last renovated (mean = 13.4)	−3.604** (1.361)	−5.519** (1.637)	−6.524** (2.142)	−9.524** (2.302)	−9.698** (2.614)	−10.677** (3.349)	2,964
(6) Building condition based on campus age (mean = 3.77)	0.013 (0.010)	0.016 (0.012)	0.035* (0.014)	0.023 (0.017)	0.011 (0.018)	0.009 (0.019)	14,477
(7) Fraction of teachers leaving campus (mean = 0.228)	0.010 (0.010)	0.000 (0.011)	−0.013 (0.013)	0.000 (0.012)	0.003 (0.013)	0.000 (0.012)	13,654

Notes: Each row represents a separate specification and reports effects of bond measure passage on outcomes *t* years later. The sample includes yearly panel data from 1994 to 2011 for all 812 districts that held bond elections. Sample for 4th and 6th rows restricted 805 districts and 5th row restricted to 228 districts. For which campus age was constructed. Model includes indicators for bond passage, holding election, and vote share (with different slopes for passing and non-passing bond) in the current year and each previous year up to ten. The table reports the bond passage indicators for each lag. Estimates for current period and lags greater than six are not displayed. Reported mean is for the year prior to the election. Standard errors are clustered at the district level. Significance: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

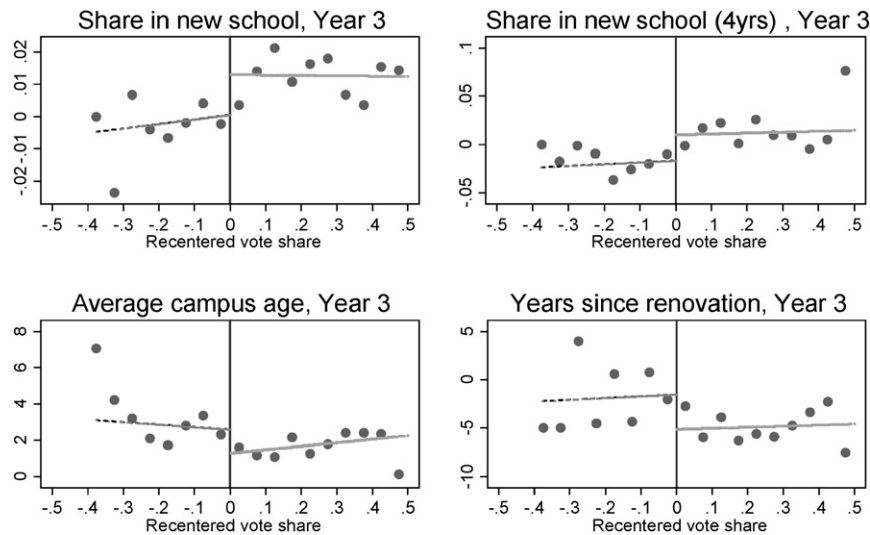


Fig. 3. Capital Inputs by Vote Share, Change Since Bond Election. Notes: Graphs plot change in average district building conditions (relative to two years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5 point bins of vote share. Top row includes data for 1737 elections and 812 districts. Bottom row includes data for 804 districts and 228 districts (465 elections) for campus age and years since renovation, respectively.

that capital campaigns improve the quality of school buildings partially through the opening of new schools: bond-funded school capital campaigns increase the likelihood of a district opening at least one campus by 11 percentage points by year two and double the share of students attending brand new schools. Despite these large proportionate increases, the number of students actually exposed to new schools is small: three years after an election, capital campaigns increase the fraction of students enrolled in a school opened within the last 4 years by less than 4 percentage points. This new construction reduces the enrollment-weighted campus age by 1.4 years within three years of initiating the capital campaign. Consequently, the change in average building condition predicted by campus age is positive and small for the third year following the bond election.¹² The evidence is stronger for the claim that capital campaigns increase exposure to renovated schools.

¹² To construct a time-varying measure of average building condition, we regress overall building condition in 2006 (5 point scale) on a cubic in campus age, then predict out of sample to all campuses and years for which campus age is available.

All estimated effects of capital campaigns on enrollment-weighted average years since a school was last renovated are negative and statistically significant at the 5% level or better.¹³

Further evidence on the impact of capital campaigns on facility conditions comes from a cross-sectional analysis of the 2006 survey of school conditions. Since the outcomes generated from the survey are only observed in a single year, we estimate standard cross-sectional

¹³ Results on campus renovations at long lags should be interpreted cautiously, as estimates are based on a small number of elections (126 elections with 17 failures after 6 years vs. 263 elections with 54 failures after 2 years). In addition to our baseline specification (which includes election fixed effects and controls for a two-part linear function of the vote share), we also estimated models using a variety of alternative specifications to assess the robustness of the effects on school conditions. Appendix Tables A2 and A4 show TOT estimates using linear, quadratic, and cubic polynomials in the vote share and ITT estimates using various bandwidths. Our estimated effects on educational inputs are quite robust across these different specifications, both qualitatively and quantitatively.

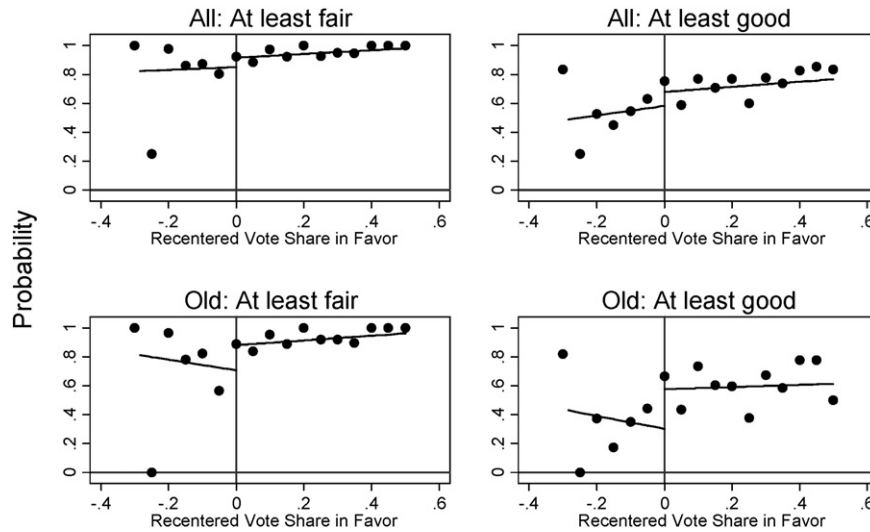


Fig. 4. Building Condition by Vote Share. Notes: Graphs plot fraction of district buildings in fair or good condition, separately by the vote share in favor of bond passage for first election held between 1997 and 2006. Elections were grouped in 5 point bins of vote share. Campus-level observations were weighted inversely by enrollment such that each district is given equal weight. Includes data for 204 districts.

RD models where the running variable is the vote share in the first bond election held by a district between 1997 and the time of the survey. To parallel our district-level panel analysis, we weight each campus observation by the inverse of the total number of schools in a district so that each district receives equal weight. Results are depicted in Fig. 4 (model

estimates are reported in Appendix Table A7). One limitation of this analysis is that we only have the survey data for one year and 302 districts (204 of which held bond elections), limiting statistical power. As seen in the top row of Fig. 4, bond passage causes modest increases in the likelihood that school facilities are in at least fair or at least good

Table 5

Effect of bond passage on district-wide student outcomes.
TOT, Two-part linear specification with district fixed effects.

	Effect of bond passage after						n
	1 year	2 years	3 years	4 years	5 years	6 years	
A. Standardized test scores (grades 3–8)							
Reading (mean = 0.027)	0.007 (0.013)	0.000 (0.016)	0.007 (0.016)	−0.010 (0.017)	−0.004 (0.019)	0.016 (0.020)	14,520
Math (mean = 0.023)	0.000 (0.018)	0.001 (0.021)	0.007 (0.022)	−0.015 (0.024)	−0.004 (0.026)	0.030 (0.027)	14,520
B. Within district 90–10 test score difference (grades 3–8)							
Reading (mean = 2.028)	−0.009 (0.032)	0.012 (0.035)	−0.017 (0.037)	−0.012 (0.038)	−0.041 (0.038)	−0.014 (0.043)	13,003
Math (mean = 2.192)	0.012 (0.031)	0.004 (0.033)	0.015 (0.033)	−0.009 (0.035)	−0.017 (0.038)	−0.012 (0.037)	13,005
C. Standardized score on exit exam							
Reading (mean = 0.048)	−0.007 (0.019)	0.007 (0.021)	0.019 (0.028)	−0.001 (0.025)	−0.015 (0.027)	0.007 (0.025)	13,279
Math (mean = 0.039)	−0.016 (0.020)	−0.011 (0.026)	0.003 (0.028)	−0.041 (0.031)	−0.039 (0.030)	−0.036 (0.031)	13,278
D. Attendance rate (grades 3–8)							
District mean (mean = 96.40)	−0.018 (0.056)	0.076 (0.064)	0.129 (0.082)	−0.012 (0.071)	0.013 (0.066)	−0.014 (0.071)	14,559
90–10 difference (mean = 7.86)	0.053 (0.096)	−0.148 (0.103)	−0.222* (0.110)	−0.160 (0.123)	−0.229* (0.114)	−0.182 (0.129)	13,329
E. Student mobility (all grades)							
In-migration rate (mean = 0.143)	0.002 (0.004)	0.007* (0.004)	0.004 (0.004)	0.003 (0.004)	−0.004 (0.005)	−0.007 (0.005)	13,765

Notes: Each row represents a separate specification and reports effects of bond measure passage on outcomes t years later. The sample includes yearly panel data from 1994 to 2011 for all 812 districts that held bond elections. Model includes indicators for bond passage, holding election, and vote share (with different slopes for passing and non-passing bond) in the current year and each previous year up to ten. The table reports the bond passage indicators for each lag. Estimates for current period and lags greater than six are not displayed. Reported mean is for the year prior to the election. District mean test scores were calculated by aggregating campus-economic-grade group means (available whenever cell size is at least 5 students) to the district-level. Thus groups with fewer than 5 students in the campus-grade are excluded from calculation of overall averages. District-years with fewer than 100 students are excluded from models examining 90–10 differences. Standard errors are clustered at the district level. Significance: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

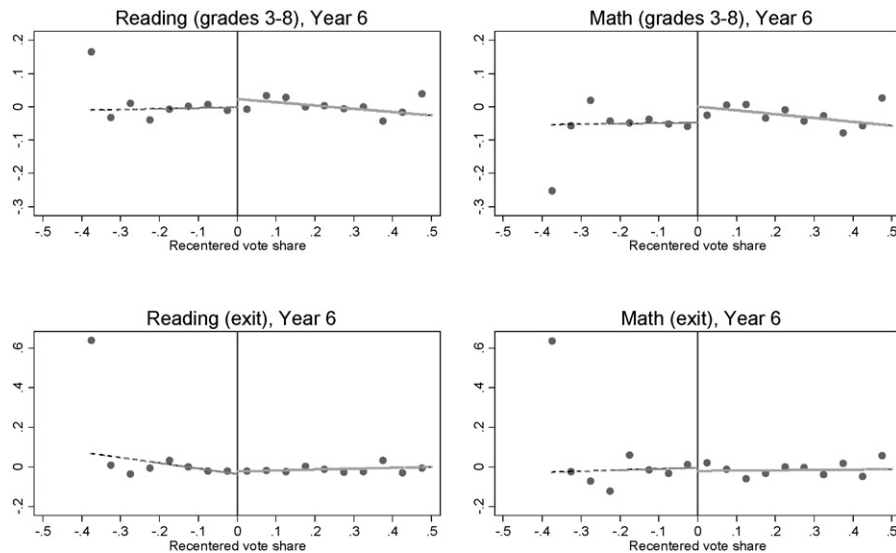


Fig. 5. Achievement Test Scores by Vote Share, Change Since Bond Election. Notes: Graphs plot change in average district test scores (relative to the two years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5 point bins of vote share. Includes data for 1737 elections and 812 districts.

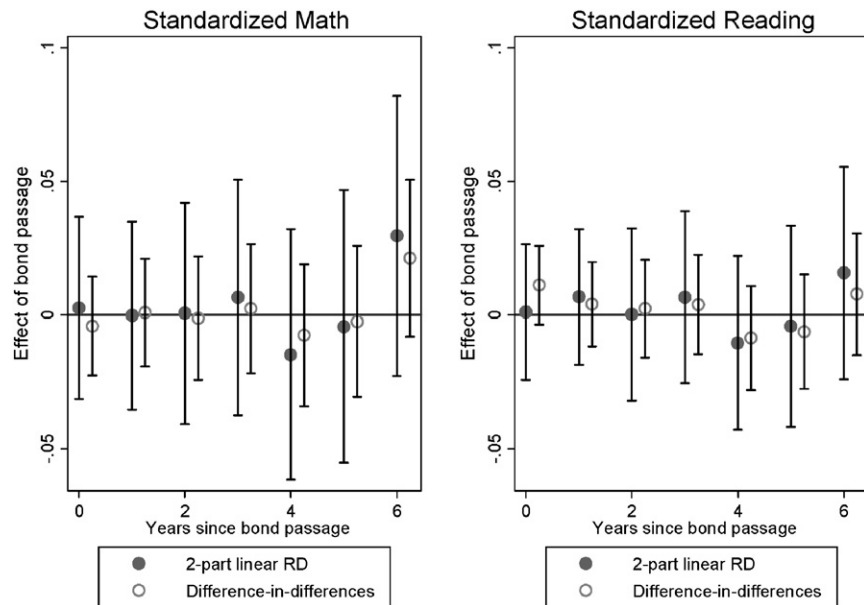


Fig. 6. Effect of Bond Passage on Student Achievement, RD vs. Difference-in-Differences Estimates. Notes: Graphs depict coefficients (and 95% confidence intervals) for main achievement test results. The sample includes yearly panel data from 1994 to 2011 for all 812 districts that held bond elections. RD model includes district fixed effects, year fixed effects, indicators for bond passage, holding election, and vote share (with different slopes for passing and non-passing bond) in the current year and each previous year. The figure reports the bond passage indicators for each lag. Difference-in-differences model omits vote share controls. Estimates for lags greater than six are not displayed.

condition, although the estimates are not statistically different from zero for districts overall.¹⁴ However, capital campaigns are associated with closing gaps in school facility conditions between older and newer buildings (bottom row): bond passage increases the likelihood that a school is in at least fair or at least good condition among old schools by about 15 to 22 percentage points (p -value 0.045, 0.018).

¹⁴ District administrators were asked to rate the physical condition of all their school buildings. “Fair” condition is defined as “Major repairs needed, but the building’s condition does not impair student learning or staff/student safety.” “Good” is defined as “Some repairs may be beneficial, but the facility is structurally and educationally sound.” Appendix Fig. A7 plots the fraction of buildings that are in “Fair” and “Good” condition as a function of facility age. General building conditions deteriorate rapidly as buildings become more than about 20 or 25 years old, though older buildings are in better condition if an earlier bond election was successful.

Capital campaigns also reduce the effective age of old school facilities by roughly 7 years, and this effect is statistically significant.¹⁵

In sum, these results suggest that capital campaigns increase student exposure to renovated schools and improve the quality of building conditions in older schools. The results also suggest that campaigns increase school openings considerably (from a low baseline), but relatively few students are affected by such changes. We find that school opening lags investment by about one year, with the largest rates of opening in years

¹⁵ These patterns are quite robust to various polynomials in vote share and the inclusion of district fixed effects. Results are similar for elementary, middle, and high school separately (though less precise). Appendix Fig. A8 exploits the fact that campuses are observed in 2006 with different lags since the first bond election to document that the improvement in overall building conditions, effective building age, portable use, and several measures of crowding seen among older campuses all show the most improvement four to five years after a successful election.

Table 6

Socioeconomic heterogeneity in effect of bond passage.
TOT, Two-part linear specification with district fixed effects.

	Effect of bond passage after						n
	1 year	2 years	3 years	4 years	5 years	6 years	
A. Standardized reading test scores (grades 3–8)							
Free lunch eligible	0.021 (0.022)	0.020 (0.024)	0.002 (0.023)	0.013 (0.023)	0.055* (0.028)	0.052* (0.026)	13,962
Not econ disadvantaged	0.010 (0.013)	0.003 (0.016)	0.011 (0.016)	−0.006 (0.018)	−0.004 (0.020)	0.009 (0.020)	14,342
B. Standardized math test scores (grades 3–8)							
Free lunch eligible	0.010 (0.025)	0.024 (0.026)	0.004 (0.026)	0.005 (0.030)	0.036 (0.034)	0.069* (0.031)	13,962
Not econ disadvantaged mean	0.009 (0.017)	0.010 (0.021)	0.014 (0.021)	−0.001 (0.023)	0.006 (0.026)	0.009 (0.025)	14,341
C. Standardized score on reading exit exam							
Free lunch eligible	0.025 (0.034)	0.031 (0.036)	0.066 (0.049)	0.050 (0.047)	−0.012 (0.047)	0.043 (0.043)	11,344
Not econ disadvantaged mean	−0.023 (0.019)	0.001 (0.020)	0.007 (0.027)	−0.002 (0.025)	0.022 (0.028)	0.008 (0.025)	13,006
D. Standardized score on math exit exam							
Free lunch eligible	0.027 (0.032)	−0.010 (0.038)	0.058 (0.041)	−0.003 (0.052)	−0.020 (0.042)	0.007 (0.048)	11,339
Not econ disadvantaged mean	−0.034 (0.023)	−0.006 (0.026)	−0.005 (0.027)	−0.026 (0.030)	−0.018 (0.032)	−0.027 (0.031)	13,005

Notes: Each row represents a separate specification and reports effects of bond measure passage on outcomes t years later. The sample includes yearly panel data from 1994 to 2011 for all 812 districts that held bond elections. Model includes indicators for bond passage, holding election, and vote share (with different slopes for passing and non-passing bond) in the current year and each previous year up to ten. The table reports the bond passage indicators for each lag. Estimates for current period and lags greater than six are not displayed. Reported mean is for the year prior to the election. Group mean test scores were calculated by aggregating campus-economic-grade group means (available whenever cell size is at least 5 students) to the groupXdistrict-level. Thus groups with fewer than 5 students in the campus-grade are excluded from calculation of overall averages. Standard errors are clustered at the district level. Significance: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

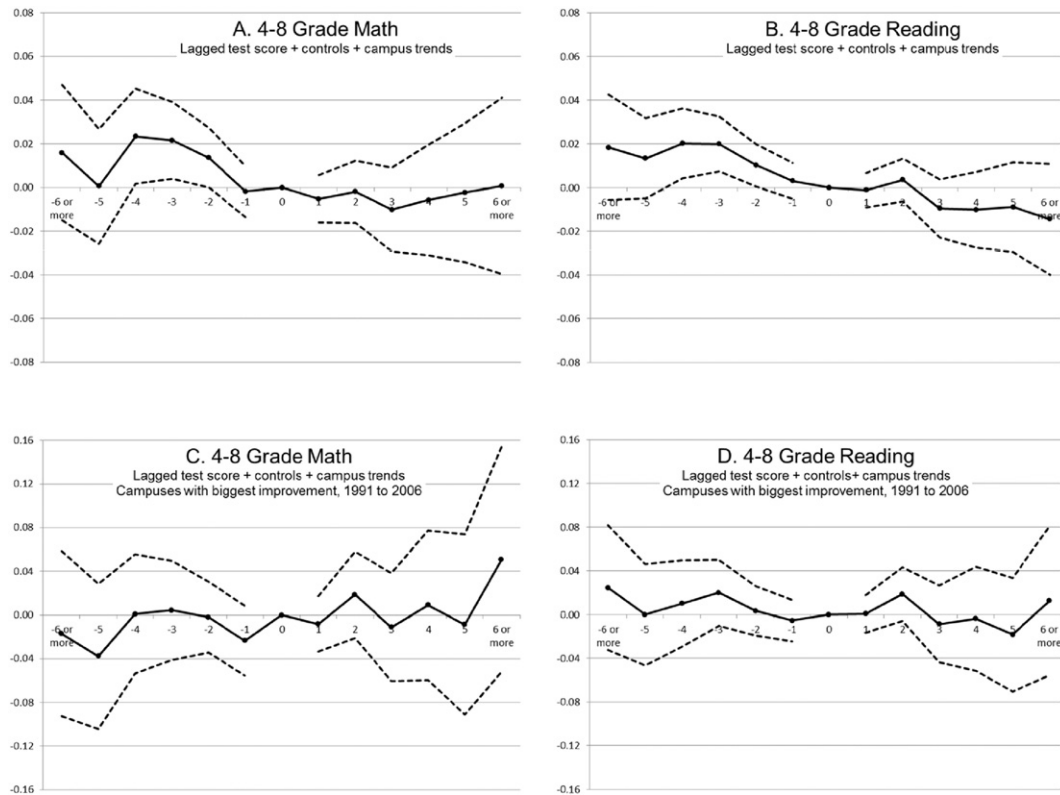


Fig. 7. Event-study Estimates of Effect of Campus Renovations. Notes: Graphs plot coefficients (and 95% confidence intervals) on dummies for years prior and after a major campus renovation, estimated via Eq. (4). All models include campus fixed effects, lagged test scores, student sex, race, free lunch status, year X grade fixed effects, and campus-specific linear time trends. Sample includes all test score observations from 1995 to 2006 in districts that participated in the 2006 facilities survey. Sample is further restricted to campuses that held a renovation and is open in all years from 1994 to 2006, and to individuals for whom prior year test score is available. Final row includes campuses that were in the bottom 40% of average room condition in 1991 but were rated as “Good” or “Excellent” overall building condition in 2006. Sample sizes are 3.4 million student-years (1354 campuses) for top row and 713,000 student-years (256 campuses) for bottom row. Standard errors clustered by campus.

two and three after a successful election. The results in this section provide some of the first evidence demonstrating that capital campaigns funded by school bonds lead to tangible improvements in schooling facilities.

Although the capital campaigns we study appear to confer only modest improvements to facilities, they may yet influence student environments through attracting and retaining high-quality teachers to a local district (Buckley et al., 2005). In the final row of Table 4, we find that capital campaigns have minimal impact on the fraction of teachers that leave schools (either to another school, out of the district, or out of the profession). Thus, the only modest impact on school conditions for the typical student does not translate to measureable effects on teacher retention.

5.3. Student achievement

Table 5 shows TOT estimates of the impact of bond passage on test scores and attendance. Overall, we find little evidence that bond passage generates improvements in student achievement or attendance, a conclusion that is echoed in the graphical evidence (Fig. 5). For grades 3–8, the point estimates are initially close to zero and inconsistent in sign. By year 6, the estimates are positive but statistically insignificant. The magnitude of the estimates is 0.016 and 0.030 standard deviations for reading and math, respectively, and we can rule out effects larger than 0.06 for reading and 0.08 for math. This finding is shown more clearly in Fig. 6, which plots coefficients and confidence intervals for our preferred RD specification along with a difference-in-differences model that does not control for vote share. Difference-in-differences point estimates are

very similar to those from the RD but are precise enough to rule out test score effects greater than 0.03 and 0.05 standard deviations for reading and math, respectively. Thus, we are able to rule out the imprecise point estimates found by CFR, of a roughly 0.067 and 0.077 student-level standard deviation improvement for 3rd grade reading and math scores from capital investments of comparable magnitude. The estimated impacts on exit exam scores and overall attendance rates are very close to zero and inconsistent in sign both across years and between math and reading. As shown in Appendix Table A3, across a variety of different specifications of the vote share function, we find very little evidence of impacts of bond passage on student performance.

To address the possibility that changes in the student population offset impacts of capital spending on student achievement, Panel E of Table 5 reports estimates on the overall migration rate of students into the district. The point estimates are small, but positive, for the first four years, then negative thereafter. Though the point estimate in year 2 is marginally statistically significant, this result is not persistent and generally not robust to alternative specifications (not reported).

Although these results provide little indication that school bond passage leads to appreciable impacts on overall student outcomes, an important question is whether bond passage reduces achievement gaps, as might be the case if the resulting investments disproportionately benefit students from disadvantaged backgrounds within districts. We investigate this issue by estimating effects on the gap between the 10th and 90th percentile of the individual test score and attendance distributions within districts. We find no evidence that bond passage

Table 7
Event-study estimates of effects of campus renovations.

	Campus Renovations					
	4–8th grade math			4–8th grade reading		
	No controls	Full controls	Full controls + campus-specific time trends	No controls	Full controls	Full controls + campus-specific time trends
	(1)	(2)	(3)	(4)	(5)	(6)
6+ years prior	0.0021 (0.0272)	0.0104 (0.0143)	0.0160 (0.0158)	0.0275 (0.0201)	0.0160 (0.0107)	0.0184 (0.0123)
5 years prior	−0.0101 (0.0208)	−0.0034 (0.0115)	0.0005 (0.0134)	0.0139 (0.0156)	0.0099 (0.0082)	0.0134 (0.0094)
4 years prior	0.0089 (0.0172)	0.02166** (0.0096)	0.02349** (0.0111)	0.02492* (0.0129)	0.01758*** (0.0072)	0.02029** (0.0082)
3 years prior	0.0189 (0.0133)	0.01953** (0.0079)	0.02159** (0.0090)	0.02691*** (0.0101)	0.01760*** (0.0058)	0.01999*** (0.0064)
2 years prior	0.01872* (0.0096)	0.01232** (0.0062)	0.01360* (0.0069)	0.02155*** (0.0073)	0.00883* (0.0046)	0.01031** (0.0050)
1 year prior	0.0058 (0.0063)	−0.0018 (0.0057)	−0.0018 (0.0060)	0.01022** (0.0049)	0.0028 (0.0041)	0.0031 (0.0042)
1 year after	−0.0025 (0.0065)	−0.0045 (0.0053)	−0.0052 (0.0056)	0.0009 (0.0050)	0.0003 (0.0039)	−0.0012 (0.0040)
2 years after	0.0037 (0.0104)	−0.0010 (0.0065)	−0.0019 (0.0073)	0.0126 (0.0079)	0.0052 (0.0047)	0.0035 (0.0051)
3 years after	−0.0009 (0.0140)	−0.0104 (0.0082)	−0.0101 (0.0098)	0.0147 (0.0108)	−0.0058 (0.0060)	−0.0096 (0.0068)
4 years after	−0.0068 (0.0181)	−0.0098 (0.0100)	−0.0057 (0.0129)	0.0104 (0.0137)	−0.0068 (0.0076)	−0.0102 (0.0089)
5 years after	−0.0043 (0.0225)	−0.0023 (0.0126)	−0.0023 (0.0163)	0.0058 (0.0163)	−0.0012 (0.0085)	−0.0089 (0.0105)
6+ years after	−0.0050 (0.0267)	−0.0063 (0.0145)	0.0007 (0.0206)	−0.0029 (0.0204)	−0.0092 (0.0105)	−0.0145 (0.0130)
Lagged score	No	Yes	Yes	No	Yes	Yes
Other controls	No	Yes	Yes	No	Yes	Yes
Fixed effects	Year, grade	Year × grade	Year × grade	Year, grade	Year × grade	Year × grade
Trends	None	None	Campus-specific	None	None	Campus-specific
Observations	3,387,465	3,387,465	3,387,465	3,383,471	3,383,471	3,383,471
R-squared	0.10886	0.533	0.536	0.091	0.500	0.501
Events	1354	1354	1354	1354	1354	1354
Campuses	1354	1354	1354	1354	1354	1354

Notes: All specifications also include campus fixed effects. Sample includes all test score observations from 1995 to 2006 in campuses contained in the 2006 facilities survey, held a renovation and is open in all years from 1994 to 2006, and to individuals for whom prior year test score is available. Standard errors clustered by campus.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

Table 8
Effects of campus renovations on student characteristics.

	No campus-specific trends		Student characteristics		
	Lagged test scores				
	Math	Reading	Free lunch (mean 0.418)	Male (mean 0.495)	Non-white (mean 0.633)
6+ years prior	−0.0079 (0.0262)	0.0247 (0.0196)	−0.0098 (0.0226)	0.0000 (0.0039)	0.0051 (0.0082)
5 years prior	−0.0045 (0.0198)	0.0151 (0.0149)	−0.0037 (0.0170)	0.0011 (0.0031)	0.0057 (0.0064)
4 years prior	−0.0151 (0.0162)	0.0160 (0.0122)	−0.0043 (0.0145)	−0.0013 (0.0026)	0.0041 (0.0053)
3 years prior	0.0013 (0.0127)	0.01659* (0.0096)	−0.0029 (0.0113)	−0.0004 (0.0021)	0.0015 (0.0042)
2 years prior	0.0127 (0.0093)	0.02308*** (0.0070)	0.0015 (0.0082)	−0.0017 (0.0017)	0.0004 (0.0029)
1 year prior	0.01588*** (0.0059)	0.01636*** (0.0047)	0.0042 (0.0050)	−0.00208* (0.0011)	0.0002 (0.0016)
1 year after	0.0014 (0.0060)	−0.0017 (0.0050)	−0.0013 (0.0059)	−0.0015 (0.0012)	−0.0020 (0.0018)
2 years after	0.0016 (0.0099)	0.0035 (0.0080)	−0.0142 (0.0095)	−0.00294* (0.0017)	−0.0041 (0.0035)
3 years after	0.0068 (0.0134)	0.02319** (0.0110)	−0.02755** (0.0137)	−0.00483** (0.0021)	−0.0054 (0.0050)
4 years after	−0.0058 (0.0169)	0.0138 (0.0134)	−0.03187* (0.0176)	−0.00598** (0.0025)	−0.0066 (0.0063)
5 years after	−0.0094 (0.0208)	0.0032 (0.0164)	0.0039 (0.0195)	−0.00543* (0.0031)	−0.0048 (0.0075)
6+ years after	−0.0067 (0.0263)	−0.0025 (0.0209)	0.0111 (0.0243)	−0.00660* (0.0038)	−0.0042 (0.0091)
Lagged score	No	No	No	No	No
Other controls	None	None	None	None	None
Fixed effects	Year × grade	Year × grade	Year × grade	Year × grade	Year × grade
Trends	None	None	None	None	None
Observations	3,505,756	3,499,164	3,505,756	3,505,756	3,505,756
R-squared	0.103	0.096	0.247	0.002	0.453
Events	1354	1354	1354	1354	1354
Campuses	1354	1354	1354	1354	1354

Notes: All specifications also include campus fixed effects. Sample includes all test score observations from 1995 to 2006 in campuses contained in the 2006 facilities survey, held a renovation and is open in all years from 1994 to 2006, and to individuals for whom prior year test score is available. Specifications (3) and (6) includes campuses that were in the bottom 40% of average room condition in 1991 but were rated as “Good” or “Excellent” overall building condition in 2006. Standard errors clustered by campus.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

narrows test score gaps; the precision of the estimates permits us to rule out very small effects on the test score distribution. For attendance, the estimates suggest bond passage might reduce disparities in attendance rates, but the estimates imply very small practical effects. We also assessed the robustness of these findings by examining the estimates across a variety of specifications for the vote share polynomial as well as the ITT estimates using alternative bandwidths.¹⁶ These results (reported in Appendix Tables A3 and A5) are consistent with the main substantive message in Table 5 that there is little indication that bond passage narrows test score gaps.

Another way of investigating if capital campaigns reduce disparities is to see if the impacts vary by student socioeconomic status. Table 6 presents TOT estimates for test scores separately for students that receive free lunch and those that are not economically disadvantaged. For the non-free lunch recipients, the estimates are all very close to zero and we can rule out effects larger than 0.06 standard deviations. For the free lunch sample, however, the estimates tend to be positive and by year 6 are statistically significant for both math and reading.

¹⁶ The TOT specification does not lend itself to restricting the running variable bandwidth, so we also show ITT estimates with various bandwidths as well as alternative polynomials. As explained in Section 4, the TOT estimates use the running variables from multiple elections for the same district in a single regression model on panel data. Restricting the vote share bandwidth would sharply reduce the number of districts we could use in the sample if the restriction applied to all the possible elections that contribute a vote share to a particular regression. It would also bias the sample to districts that hold relatively few elections.

Nonetheless, a careful examination of this finding under alternative specifications leads us to discount this result somewhat, as the magnitude and significance is sensitive to specification. In Appendix Table A3, we see that the point estimates tend to reduce with more flexible polynomials in vote share and the difference-in-differences estimates are much smaller and insignificant (reading) or only marginally significant (math) compared to our baseline RD estimates. Moreover, once the bandwidth is limited to elections where the vote share was within 25 percentage points of passage, the ITT point estimates are close to zero and much smaller than the ITT estimates that use the full range of vote shares and a linear function of the vote share (Appendix Table A6). Finally, event study estimates demonstrate no positive effects of campus renovations for poor students specifically, as we discuss in Section 6.

5.4. Dosage and heterogeneity by district characteristics

Though our main results find no measureable effect of bond-funded capital campaigns overall, it is possible that campaigns with large impacts on conditions could have bigger effects. The median bond proposed to voters in our study period was for \$7756 per student. While this represents a large increase over baseline levels of spending, it is an order of magnitude smaller than what was observed in the large-scale school construction program undertaken in New Haven, which would be in the 99.6th percentile of all bonds proposed during our study period. To test for dosage effects, we look at differences by several baseline (pre-election) characteristics likely to be associated with the

Table 9
Event-study estimates of effects of campus renovations, subgroups.

	4–8th Grade Math (with time trends)				4–8th Grade Reading (with time trends)			
	Poorest 25% districts	Free-lunch eligible students	Building in poor condition, 1991	Large improvement, 1991–2006	Poorest 25% districts	Free-lunch eligible students	Building in poor condition, 1991	Large improvement, 1991–2006
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
6+ years prior	–0.0076 (0.0234)	0.0248 (0.0216)	–0.0160 (0.0365)	–0.0170 (0.0385)	0.02924* (0.0176)	0.0273 (0.0176)	0.0051 (0.0272)	0.0246 (0.0292)
5 years prior	–0.0145 (0.0206)	0.0029 (0.0183)	–0.0316 (0.0323)	–0.0378 (0.0339)	0.02453* (0.0144)	0.02502* (0.0138)	–0.0004 (0.0214)	–0.0002 (0.0238)
4 years prior	0.0155 (0.0173)	0.03120** (0.0153)	–0.0119 (0.0273)	0.0010 (0.0279)	0.02936** (0.0122)	0.02964** (0.0120)	–0.0062 (0.0193)	0.0101 (0.0202)
3 years prior	0.0106 (0.0142)	0.02473** (0.0125)	0.0008 (0.0226)	0.0042 (0.0233)	0.02566** (0.0097)	0.03007*** (0.0093)	0.0076 (0.0153)	0.0200 (0.0155)
2 years prior	0.0117 (0.0110)	0.02029** (0.0098)	0.0126 (0.0179)	–0.0020 (0.0166)	0.0124 (0.0076)	0.01705** (0.0075)	0.0027 (0.0117)	0.0034 (0.0116)
1 year prior	–0.0030 (0.0091)	0.0024 (0.0084)	–0.0065 (0.0136)	–0.0235 (0.0164)	–0.0019 (0.0063)	0.0068 (0.0063)	–0.0011 (0.0095)	–0.0055 (0.0096)
1 year after	–0.0119 (0.0080)	–0.0096 (0.0076)	–0.0021 (0.0113)	–0.0082 (0.0129)	–0.0029 (0.0062)	–0.0009 (0.0060)	0.0026 (0.0088)	0.0006 (0.0088)
2 years after	0.0040 (0.0117)	0.0026 (0.0106)	0.0034 (0.0182)	0.0185 (0.0202)	0.0026 (0.0078)	0.01319* (0.0074)	0.0098 (0.0114)	0.0186 (0.0126)
3 years after	–0.0095 (0.0165)	–0.0222 (0.0141)	–0.0202 (0.0270)	–0.0111 (0.0253)	–0.02109* (0.0110)	–0.0132 (0.0103)	–0.0272 (0.0191)	–0.0086 (0.0180)
4 years after	0.0011 (0.0221)	–0.0114 (0.0190)	–0.0027 (0.0377)	0.0090 (0.0350)	–0.0194 (0.0147)	–0.0145 (0.0133)	–0.0226 (0.0250)	–0.0038 (0.0244)
5 years after	–0.0013 (0.0283)	–0.0160 (0.0228)	–0.0057 (0.0466)	–0.0086 (0.0421)	–0.03021* (0.0166)	–0.02563* (0.0147)	–0.0354 (0.0278)	–0.0184 (0.0265)
6+ years after	0.0300 (0.0352)	0.0020 (0.0293)	0.0265 (0.0593)	0.0509 (0.0525)	–0.0166 (0.0214)	–0.0251 (0.0189)	–0.0208 (0.0380)	0.0124 (0.0347)
Lagged score	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Year × grade	Year × grade	Year × grade	Year × grade	Year × grade	Year × grade	Year × grade	Year × grade
Trends	Campus-specific	Campus-specific	Campus-specific	Campus-specific	Campus-specific	Campus-specific	Campus-specific	Campus-specific
Observations	1,732,799	1,398,480	723,916	713,352	1,730,193	1,396,620	723,166	712,597
R-squared	0.533	0.501	0.541	0.527	0.498	0.465	0.501	0.492

Notes: All specifications also include campus fixed effects. Sample includes all test score observations from 1995 to 2006 in campuses contained in the 2006 facilities survey, held a renovation and is open in all years from 1994 to 2006, and to individuals for whom prior year test score is available. Specifications (3) and (6) includes campuses that were in the bottom 40% of average room condition in 1991 but were rated as “Good” or “Excellent” overall building condition in 2006. Standard errors clustered by campus.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

treatment intensity. We implement this by interacting bond passage in Eq. (3) with bond amount and indicators for the district having an above-median share of students economically disadvantaged (in 1997), above-median enrollment-weighted campus age (in 1997), and below-median building condition (in 1991). Though districts proposing larger bonds and with older and poor-quality buildings do indeed make larger capital investments following bond passage, the differences are not very large and we detect no differences in test score effects by these baseline characteristics. In fact, the 6-year test score point estimate is smaller for districts with greater needs for capital investment. While suggestive of minimal effect of capital campaigns on student achievement, this dosage analysis is fairly underpowered.¹⁷

6. Event-study results

A limitation of district-level RD models is that we cannot identify which students benefit from the investments generated by bond passage. Thus bond passage may be too diffuse a treatment to detect small to moderate effects on district-level outcomes. To address this issue, we estimate the effect of attending schools that have been renovated using an event-study model with student-level microdata.

¹⁷ Results from these models are reported in Appendix Table A8. Some of the imprecision of dosage effect estimates stems from the fact that bond-funded capital campaigns do not appear to be well targeted at the districts with the greatest needs. Large campaigns are proposed by wealthier districts with fewer poor students, smaller class sizes, and who are already spending more on instruction and capital investments (Appendix Table A9). Districts with older school buildings do propose larger bond amounts, though this relationship is economically small.

The top row of Fig. 7 depicts our fully-controlled event study estimates for school renovations. These models include campus fixed effects, year-grade fixed effects, and controls for student sex, race, free lunch status, and lagged test scores. They also include campus-specific linear time trends. The results provide no indication of meaningful effects on test scores, as all of the post-intervention estimates are close to zero and precisely estimated. In particular, we can rule out positive effects larger than about 0.020 for math and 0.010 for reading for the first four years following the renovation. Table 7 presents these estimates and also includes specifications that do not include student controls or campus-specific time trends. Importantly, controlling for lagged test scores (and other controls) does not meaningfully impact estimated point estimates (though does improve precision considerably). Campus-specific trends also have little impact on our estimates, though do make our estimates less precise. Fig. 7 does suggest a very modest downward trend in reading test scores for the full sample prior to renovations. However, there is no obvious improvement relative to trend in the post-renovation period, nor is there evidence of a pre-trend for math or for other subgroups (discussed below in Table 9).

Robustness to student controls suggests little change in the composition of students following renovations, lending credibility to the key assumption that unobservable student attributes did not change following large school renovations. Table 8 examines changes in student characteristics directly. There is no consistent pattern in these results, with students in renovated schools becoming more disadvantaged following renovation by some measures and for some time periods (e.g. % free lunch eligible decreases slightly in years 3 through 4) but more advantaged for others (e.g. lagged reading score increasing in year 3).

In Table 9 we focus on districts, schools, and students that are most likely to be positively affected by campus renovations. Analyses for these subsamples is important since very poor districts may have the most difficulty raising funds for capital campaigns, so investments in these districts may target renovations with the greatest marginal benefit for students. Similarly, poor students may be in districts with difficulty raising funds and may also attend schools in worse condition at baseline. We find no evidence that student achievement improves following renovations for these districts or students. The null finding for poor students causes us to further discount the positive (though not robust) effects of bond passage on poor students found with the district-level RD (Table 6). Finally, the bottom row of Fig. 7 isolates renovations likely to be associated with large facility condition improvements by limiting the sample to schools in poor condition in 1991 (measured by being in the bottom two quintiles of average room condition) but by 2006 were in good or excellent condition. Again, we find no indication that these renovations lead to improved student achievement in math or reading. Here, the flat pre-existing trend continues after the renovation, with the point estimates neither systematically above nor below zero. Results focusing only on schools that were in the bottom quintile of room condition in 1991 (Table 9) are very similar.

In summary, we find no evidence that student outcomes improve following large school renovations and we can rule out very small achievement effects. This is true even when focusing on renovations that were likely to have caused large improvements in the physical condition of the school and for districts and students most likely to experience positive effects. These results suggest that the lack of effects of bond passage on student test scores may reflect school facility investments having little effect on student outcomes, at least in the context of our sample and time period, rather than an artifact of an overly-diffuse treatment.

7. Effect size and cost effectiveness

In order to interpret the magnitudes of the achievement effects our analysis rules out, we compare our estimates to those from increases in instructional spending of a similar amount. Given the large expenditures districts make on school facility improvements, a crucial issue for economic policy is the effectiveness of these investments relative to other uses. The discussion below misses any benefits of facility spending that are not reflected in improved student achievement. However, given the policy significance of student achievement (e.g., for school accountability programs), we believe these back-of-the-envelope calculations can provide a useful framework for thinking about the comparative effectiveness of various educational investments.

As a starting point, consider the test score impacts of \$1000 spent on a capital campaign funded by school bonds, a school renovation, and class size reduction. For school bonds, our results suggest bond passage leads to an increase of per-pupil capital expenditures of about \$5000 (see Table 3). From Table 5 (Panel A), the upper bound of the confidence interval of the impact of bond passage six years later is about 0.06 for reading and 0.08 for math. Assuming the effect of spending is linear, the estimates imply that \$1000 of per-pupil spending from a school bond would likely generate test score effects no larger than 0.012–0.02. For renovations, a recent compilation of costs for all Texas school construction projects estimates the typical elementary and middle school project costs about \$18,000 per student (Texas Comptroller of Public Accounts, 2014).¹⁸ Our estimates in Table 7 (columns 3 and

6) suggest we can rule out effects of renovations of 0.02 for math and 0.008 for reading after four years (the point estimates are similar for years 5 and 6+, but are less precise since they are based off progressively fewer renovation events). Again assuming linear effects of spending, we can rule out effects of \$1000 spent on renovations larger than 0.0011 for math and .0004 for reading. Finally, the results of the Tennessee Project STAR class size reduction experiment suggest that a 50% increase in instructional spending raised contemporaneous student achievement by about 0.20 standard deviations for (Krueger, 1999; Schanzenbach, 2006; Chetty et al., 2011). Since annual per-pupil instructional expenditures in our data are about \$5000 (see Table 2), \$1000 spent on class size reduction would generate test score gains of 0.08.

Of course these estimates of the effect of \$1000 of spending on the three interventions are not directly comparable because, unlike class size reduction, capital investments yield benefits to multiple cohorts of students. To account for the durability of capital investments, we calculate the discounted stream of test score gains resulting from such investments. We do so assuming that capital depreciates geometrically at a constant rate δ and also that capital is replaced after T years. This implies that the discounted value of test score gains from capital spending

is equal to $G(c, \delta, T, r) = \sum_{t=1}^T \frac{c(1-\delta)^{t-1}}{(1-r)^{t-1}}$, where c is the test score gain experienced by the first cohort exposed to the capital spending (i.e., before any depreciation occurs) and r is the discount rate. We show results using 0.02 and .0011 as the values for c ; these correspond to the test score impacts of \$1000 spent on capital via school bond passage or renovation, respectively, implied by the upper bound of the confidence intervals.

Table 10 shows values of G for different values of c, δ, T , and r . Our preferred calculations use $T = 50$, which is what the Bureau of Economic Analysis (2003) uses for the life expectancy of a public educational building, and has also been used in another analysis of the school

Table 10

Cumulative test score impacts of school facility investments at upper bound of confidence interval.

		Cumulative test score impact of \$1000 spent on school facilities via:			
		School bond passage (effect size = 0.02)		School renovation (effect size = 0.0011)	
		Depreciation rate (δ)		Depreciation rate (δ)	
Discount rate (r)	Building life (T)	0	0.041 (preferred)	0	0.041 (preferred)
0	10	0.200	0.167	0.011	0.009
0	20	0.400	0.277	0.022	0.015
0	50 (preferred)	1.000	0.428	0.056	0.024
3	10	0.176	0.148	0.010	0.008
3	20	0.306	0.221	0.017	0.012
3	50 (preferred)	0.530	0.282	0.029	0.016
5	10	0.162	0.138	0.009	0.008
5	20	0.262	0.193	0.015	0.011
5	50 (preferred)	0.383	0.228	0.021	0.013

Note: Cell entries depict the present discounted value of standardized test score gains resulting from \$1000 of school facility investments implied by our estimates of the impact of school renovations or school bond passage. These entries should be compared to test score gains of 0.08 from \$1000 spent on class size reduction found in prior literature. The effect size (c) used in the calculations is 0.02 for school bond passage and 0.0011 large renovations. As explained in the text, these are the test score impacts of \$1000 of spending via these treatments implied by the upper bound of the 95% confidence interval of our estimates. Specifically, we use the Year 6 estimate in Table 5 for math and the Year 4 estimate for math in Column 3 of Table 7, adjusted by the per-pupil cost of the bond and renovation treatments, respectively. Note that we use the Year 4 rather than the Year 6+ estimates for renovations because the Year 4 estimates are considerably more precise and similar in magnitude to the Year 6+ estimates (the qualitative conclusion of the cost-effectiveness analysis holds when basing it off the Year 6+ estimates). Our preferred calculations (in bold) using $T = 50$ and $\delta = 0.041$, as these are values reported in the literature and used in analyses of school capital stocks.

¹⁸ According to the report, the average elementary school construction project cost \$17,461 (76% of projects) and the average middle school project cost \$21,473 (24% of projects). These figures include both brand new schools and large renovations or expansions to existing schools, but we are not able to distinguish between them. Anecdotally, a large fraction of these are for existing campuses and thus provide a reasonable approximation for the major renovations contained in our event study analysis. National estimates for the construction costs for new elementary and middle schools were \$25,500 and \$29,959 per student, respectively, in 2010 (National Clearinghouse for Educational Facilities, 2015).

building capital stock in Texas (Taylor et al., 2005). Our preferred calculations use a depreciation rate of 4.1%, which is based on an estimate of the depreciation rate of non-residential state and local capital derived by Holtz-Eakin (1993) and is also used in an analysis of school facility financing in California (Brunner, 2007). We also show results using other values for comparison purposes. The effect sizes consistent with our estimates of the impact of school bond passage would imply that school bonds generate larger aggregate test score gains than class size reduction (0.08) even when capital depreciates quickly (low values of T and high values of δ). To be clear, we do not interpret this as implying that school bonds are a more cost-effective way of generating test score gains than class size reduction, but rather that the confidence intervals for bond passage effects are too large to rule out effect sizes that would give school bonds greater cost-effectiveness than class size reduction. On the other hand, the maximum effect size of renovations that we can rule out would imply an aggregate test score gain that is smaller than the gain in test scores from class size under reasonable assumptions about the durability of school buildings. In fact, assuming $T = 50$ and $\delta = 0.041$, the estimated impact of renovations on test scores would need to be about 0.067 to yield a total test score increase equal to what could be attained via class size reduction with a discount rate of zero, or 0.125 with a discount rate of 0.05. Effects this large are not consistent with the empirical evidence in Table 7.

8. Conclusion

School facility spending represents one of the largest educational investments in the U.S., with state and local governments spending more than \$65 billion a year on these expenditures. Despite the magnitude and ubiquity of this investment, we know surprisingly little about how this money is spent, how it is allocated within and across districts, and its impact on student outcomes. In the current era of lean public budgets, understanding the answers to these questions has considerable significance for economic policy.

This paper provides such empirical evidence. Using statewide administrative data from the state of Texas to estimate both RD models based on close school bond elections and event study models of school renovations, we find little indication that spending on school facilities generates improvements in student achievement. School bond passage is associated with substantial increases in capital expenditure per student and real improvements in educational facilities, though the number of students materially affected by the typical project is low. The money goes towards the opening of new campuses quickly (within 2 to 3 years of bond passage) and renovating older ones with no impact on operating expenditures. Our RD estimates allow us to rule out effects of school facility investments on contemporaneous achievement larger than 15 to 20% of the impact of a comparable increase in instructional spending, while difference-in-difference and event study estimates allow us to rule out much smaller achievement effects (12.5 and 1.4% of effects from class size reductions of similar cost, respectively). The confidence intervals for our estimates also exclude the point estimates found in two prior studies that use similar research designs. Namely, the district-level RD approach of Cellini et al. (2010) and the campus-level event-study approach of Nielson and Zimmerman (2014), though the latter studies investments targeting schools in much worse condition than the more typical investment we consider.¹⁹

We conclude that typical recent capital investments made and financed by local school districts themselves did not generate appreciable

improvements in student achievement. Although there may be other benefits to improving school facilities such as improving student health, teacher morale, or neighborhood amenities, these investments are unlikely to generate significant achievement gains or narrow achievement gaps. Neighborhood residents do appear to value marginal school investments (Cellini et al., 2010), but it appears that improved test scores are not the main channel. Uncovering these additional benefits and determining whether alternative-funding mechanisms such as direct federal and state investment would have a different impact are both important areas of future inquiry. Along the same lines, future research should examine whether there are benefits, including to student outcomes, of facility investments in poor areas that may have difficulty generating funds for facility improvements.

Appendix A. Supplementary data

Supplementary data to this article can be found online at <http://dx.doi.org/10.1016/j.jpubeco.2016.05.002>.

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¹⁹ Cellini et al. (2010) study bond elections that are of similar magnitude as those in our study, so our estimates are directly comparable to theirs. Our baseline RD 95% CI reported in Fig. 6 excludes their point estimate for reading but not math, but our difference-in-differences 95% CI excludes their estimates for both subjects. Nielson and Zimmerman (2014) study an intervention that is nearly four times larger than the typical renovation in Texas. Multiplying the upper bound of our 95% CI from the top panel of Fig. 7 by four excludes the 0.11–0.12 SD increase they observe for reading, but not the 0.04–0.05 SD increase they observe for math.

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