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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 such investments actually change the physical condition of public schools, and the subsequent causal impacts on student achievement. We study the achievement effects of nearly 1,400 capital campaigns initiated and financed by local school districts, comparing districts where school capital bonds were either narrowly approved or narrowly defeated by district voters. Overall, we find little evidence that school capital campaigns improve student achievement. Our event-study analyses focusing on students that attend targeted schools and therefore are exposed to major campus renovations also generate very precise zero estimates of achievement effects. Thus, locally financed school capital campaigns—the predominant method through which facility investments are made—may represent a limited tool for realizing substantial gains in student achievement or closing achievement gaps.

JEL Classification Codes: I22, I24, H75

Key Words: School facilities, student achievement, school financing, school bonds

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The Coleman Report (Coleman et al. 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 (Card and Krueger 1996; Greenwald, Hedges, and Laine 1996; Jackson, Johnson, and Persico 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 in public school facilities, with annual expenditures totaling about $66 billion (or $1,344 per student; NCES 2012).[1] 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), and $300 billion in deferred maintenance is 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 nonexistent suggests that new school construction projects can improve student outcomes (Aaronson and Mazumder 2011; Duflo 2001; Nielson and Zimmerman 2014). For instance, Nielson and Zimmerman 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

[1] The scope of these investments can also be seen by noting that $407 billion in outstanding taxpayer-supported bond debt is attributed to school facilities (Dixon 2012).

urban district (located in New Haven, Connecticut). However, this type of capital campaign is atypical in the United States where school capital projects (both renovations and new construction) are primarily financed locally through the issuance of voter-approved bonds that are repaid with property taxes.[2] For instance, the average per-pupil size of capital campaigns in Texas, the state we study in this paper, is about $7,800. The achievement effects of investments of this magnitude remain unclear. Cellini, Ferreira, and Rothstein (2010) 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 to date 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 1,400 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, Ferreira, and Rothstein 2010) to compare school districts where bond referenda narrowly pass to those where they 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 third through eighth graders and tenth or eleventh graders that take the state's high school exit exam.[3]

We find clear evidence that locally funded campaigns lead to large increases in capital investment that are concentrated in the first two postelection 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

[2] In the United States, 88 percent of funding for capital investment comes from local school districts.
[3] In contrast, Cellini, Ferreira, and Rothstein construct a sporadic panel of test scores spanning many different tests for third and fourth graders.

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 percentage points on a base of 6 percent. 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 regression discontinuity(RD) point estimates for grades thrree to eight 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.[4] 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 the 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

[4] Student sorting does not drive the findings, as we find little evidence that school capital campaigns encourage in-district migration among students.

issue, the second part of the study directly measures the effect of capital investment on students actually exposed to it by analyzing more than 1,300 major campus renovations and 250 campus openings using an event-study research design. 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 that we can rule out positive effects larger than about 0.013 for math and 0.016 for reading for the first four years following a campus renovation. Thus, capital spending on campus renovations has achievement effects an order of magnitude smaller than class-size reductions with similar cost. The study results for campus opening events are more imprecise and sensitive to the sample used, but we do not find consistent evidence of achievement gains resulting from building new schools.

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.

We describe the context of facilities funding in Texas and its implications for student outcomes in the next section. Following that, we describe our data sources and methods, respectively. Next we present our main RD results for district spending, school conditions, and student achievement, and event-study estimates of the effect of campus renovations and openings. We then interpret the magnitudes and cost-effectiveness of capital interventions and provide concluding remarks.

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 $1,280 (12 percent) was spent on school facilities. The vast majority of these funds are raised internally by local school districts. State and federal funding each account for about 10 percent of facility spending, with the remainder coming from districts (U.S. Department of Education 2010, Table 181; Filardo et al. 2010).[5] 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.

In Texas, local districts are fiscally independent and have taxing authority with which to raise funds for capital improvements, principally by issuing bonds. 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 to pass the associated, concurrent increase in property taxes. An example of a ballot proposition for one Texas school capital campaign is for the Ector County 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 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*

[5] Texas has a well-known school finance program, the Foundation School Program (FSP), 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.

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 it also calls for providing funds for land acquisition and the purchase of new school buses. 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 (Dixon 2012).

Although the state supports districts' ability to raise capital inexpensively through a variety of loan assistance programs, large school infrastructure needs still exist, particularly in poor districts.[6] 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 five 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 percent of districts reported that their instructional facilities were in "poor" condition or

[6] Examples of state programs to facilitate school bond issuance include the Guaranteed Bond program, the instructional Facilities Allotment program, and the widely used Existing Debt Allotment. See Clark (2001) for a history of Texas facilities funding.

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 facility conditions and capital investments remain.[7]

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 whose building systems have not been 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, Schneider, and Shang 2004; Loeb, Darling-Hammond, and Luczak, 2005). Consistent with these claims, student achievement has been shown to be 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.

[7] 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 that the average age of instructional buildings was 40 years, with a remaining functional age of only 16 more years. Older schools were more likely to report unsatisfactory conditions (NCES 2000). A 2005 survey found that 15 percent of schools were overcrowded (NCES 2007). In comparison, the average age of facilities in Texas in 2006 was 34 years, with a remaining functional age of nine more years.

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.

Bond election data. From the Texas Bond Review Board, we acquired data on the election date, bond amount, and result for 2,277 separate school bond propositions put up for a vote by Texas public school districts from 1997 to 2010.[8] We collected vote share data from 812 school districts (98 percent 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.[9] In our analysis window there were 1,737 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 percent of these bond measures, with an average vote share of 64 percent. The mean (median) bond amount was $11,086 ($7,756) per student (in 2010 dollars).

District- and campus-level longitudinal data. From the Texas Education Agency's 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

[8] 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.

[9] 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.

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 of Data.[10]

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 the Texas Education Agency. 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.[11] The 2006 data come from a voluntary survey conducted by the Texas Comptroller of Public Accounts; responses came from 302 districts (228 that held elections), including 3,548 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

[10] Campus-level measures of capital investment are not available from any standard sources since capital spending is budgeted and spent by districts, even if it is targeted at specific campuses.

[11] A small number of campuses were not successfully digitized because original data sources were lost or damaged.

then aggregated to the district level.[12] Information on year built and year last renovated was also directly used to identify major renovations and campus openings for the event-study analysis.

Student achievement, attendance, migration. Our primary outcomes are standardized test scores and attendance records from student microdata for all third through eighth graders tested from 1994 to 2011 and high school exit exam scores for the same period.[13] We focus on reading and mathematics scores for students in grade three to eight 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 tenth or eleventh 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 microdata 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 (both overall and for various subgroups) and deciles to assess how the full distribution of outcomes is altered by bond passage and subsequent capital investment.[14] We also use the micro data to calculate the share of students (second through twelfth grade) that are new to the district in each year. Finally, the disaggregated student-level microdata are also used in event-study analysis of campus renovations and school openings.

[12] 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.

[13] Student-level data come from administrative records of the University of Texas at Dallas's Texas Schools Project.

[14] To preserve data richness while complying with data confidentiality requirements, the aggregation to district-level outcomes is done as follows. From the microdata we calculate the mean, standard deviation, and number of observations for student groups defined by campus × grade (third through eighth or exit) × 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 nonzero standard deviation. These cells are then aggregated to district-level means using the cell size as weights. Since some cells are missing because of small samples, the district average will reflect the average for nonmissing 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 nondisclosed 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.

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.

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.

Regression Discontinuity with Panel Data

The 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 postelection variation in capital spending.

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:

$$(1) \qquad Y_{j,t+\tau} = \theta_\tau Pass_{j,t} + f_\tau\left(\upsilon_{j,t}\right) + \varepsilon_{j,t+\tau} \ ,$$

where $Pass_{j,t}$ 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, Ferreira, and Rothstein (2010), we first estimate Equation (1) on a panel data set 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 data sets for each separate election into one large panel data set covering the entire study period.[15]

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

(2) $Y_{j,t+\tau} = \theta_\tau Pass_{j,t} + f_\tau\left(\upsilon_{j,t}\right) + \mu_{j,t} + \alpha_{t+\tau} + \delta_\tau + \omega_{j,t+\tau}$,

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 Equation (1) is that the district-election fixed effects improve precision and control for changes in sample composition when we have an unbalanced panel.[16] We also estimate Equation (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 it requires the additional identifying assumption that changes in unobserved determinants of outcomes are unrelated to bond passage.

Equation (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

[15] Since multiple observations per district are included, we adjust all standard errors for clustering at the district level.

[16] 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 preelection period.

present two pieces of evidence consistent with this condition. First, as shown in Appendix Figure 1, the density of the bond measure vote share is "smooth" at the 50 percent threshold, and a formal test (McCrary 2008) fails to reject that the density is continuous.[17] Second, we find little evidence of discontinuities in the mean of district-level covariates at the 50 percent cutoff when estimating Equation (2), using many preelection characteristics as the outcome.[18]

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 did 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 outcome 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—i.e., the "treatment on the treated" (TOT)—we follow the "one step" method proposed by Cellini, Ferreira, and Rothstein (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.[19]

(3) $Y_{j,t} = \sum_{\tau=0}^{\bar{\tau}} \left(\theta_\tau Pass_{j,t-\tau} + \varnothing_\tau Elect_{j,t-\tau} + f_\tau\left(\upsilon_{j,t-\tau}\right) \right) + \mu_j + \alpha_t + u_{j,t}$.

[17] The point estimate of the discontinuity in density from the McCrary test is 0.227, with a standard error of 0.164.

[18] 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 because of chance. Importantly, preelection differences in all our main outcomes are small and insignificant.

[19] Vote share is set to zero for observations in which no election was held.

This model is estimated on a standard district-year panel among districts holding elections, including all years from 1994 to 2011.[20] The coefficients on lagged bond election passage, θ_τ, provide an estimate of the causal effect of bond passage holding subsequent election outcomes constant. In this paper we primarily focus on TOT estimates, though we present ITT estimates in the appendix.

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 and new school openings. This approach offers potentially sizable power gains relative to the district-level RD since it focuses on students actually exposed to capital investment.[21] This approach approximates that used in Nielson and Zimmerman's (2014) analysis of school constructions in New Haven, but we use statewide data on a much larger number of facility investment events.

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

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

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[20] This TOT estimator could potentially be subject to bias, as it controls for outcomes (bond elections, vote share, and bond passage) subsequent to a given election. Cellini, Ferreira, and Rothstein (2010) also present an alternative "recursive" estimator of the TOT effects, which is not subject to this form of bias. In practice, the one-step and recursive estimates are quite similar, though the former is much more precise; thus our focus is on the one-step estimator. Results using the recursive estimator are available from the authors.

[21] The power gain afforded by focusing on students actually affected by capital investments comes not only from improved precision of the estimates, which has to do with the number of renovations or constructions relative to the number of close bond elections. It also relates to the bond election treatment being diffuse relative to renovations or constructions, which make effect size much smaller in the RD analysis. We return to this issue later.

where Y_{igst} is the outcome for student i in grade g attending campus s in year t, and 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}). Prerenovation differences are captured by these parameters for $p < 0$ while post-renovation differences are captured for $p > 0$.

In order to mitigate sample selection bias, we estimated these models on a sample of campuses that were open for the full panel and that had renovations during our study period.[22] 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 1,354 renovated schools in 235 districts serving fourth through eighth graders. We also conduct an analysis on schools where the renovations appear to have generated large changes in school quality. Specifically, for this analysis we focus on renovations where, before the renovation, the campus's average room condition was in the bottom two quintiles of campuses in the 1991 school facility census, but after the renovation, the campus was rated as "good" or "excellent" in the 2006 survey of school facilities.

School openings are more difficult to analyze, both conceptually and empirically, since there is not an obvious "pretreatment" group with which to compare students attending the new school. We modify Equation (4) in two ways to accommodate school openings. First, we match each new school to the existing school that the majority of students at the new school would have attended had the new school not opened, based on the empirical feeder patterns that existed prior

[22] 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.

to the school opening (see Appendix B for details on how these matches were done). The campus fixed effects μ_s in Equation (4) are then replaced with fixed effects for the combination of new and matched existing school (the "school group"). Second, since only some of the students in the school group attend the new school, we interact the D_{st}^{p} dummy variables with the share of students attending the new school for all $p > 0$. This specification nests situations where an existing campus is completely replaced by a new campus, which would be treated exactly like a major renovation in Equation (4). School opening estimates are relative to the year prior to the opening. Our sample contains 258 campus openings for which we could identify a suitable counterfactual school, though some analysis, in order to mitigate selection effects, focuses on a subset of these where the matched school accounts for a large share of counterfactual enrollment and for which there was little change in overall enrollment in the school group.

For both the renovation and construction event study analyses, the assumption needed for the 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 openings or if the composition of students changed following the event. We address these possibilities by controlling for lagged student test scores (a "value added" specification) and by examining trends leading up to renovations and openings. As we discuss in our results, we see little evidence of preevent outcome trends, which lends support to the causal interpretation of our estimates.

REGRESSION DISCONTINUITY RESULTS

Nature and Timing of Capital Investments

Figure 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 $2,000 per pupil in districts where the bond barely passed compared to those in which it barely failed. The spending increase persists though Year Two but reverses by Year Six.[23] 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, which 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 ($2,333) of capital spending per student (in 2010 dollars) 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 Three and results in an increase in cumulative spending over six years of about $5,000 per pupil.[24]

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 Figure 2 provide little indication that bond passage affects instructional inputs. In the first four years after the election, bond passage has a very small and

[23] Figure 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.

[24] As shown in Appendix Figure 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.

statistically insignificant effect on instructional spending per student. We find a small but statistically significant increase in instructional spending in Years Five and Six, but the magnitudes—about 3 percent of the sample mean—are very small, and this result is not robust to alternative specifications (Appendix Table A2).[25]

School Environments

How bond-funded capital campaigns actually alter the facility environments faced by students has not been established in prior literature (Cellini, Ferreira, and Rothstein 2010; Hong and Zimmer 2014). Table 4 and Figure 3 show 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 they 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 four 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.[26] The evidence is stronger for the claim that capital campaigns increase exposure to renovated schools.

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[25] 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.

[26] To construct a time-varying measure of average building condition, we regress overall building condition in 2006 (using a five-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 percent level or better.[27]

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 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.[28] Results are depicted in Figure 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 Figure 4, bond passage causes modest increases in the likelihood that school facilities are in at least fair or at least good condition, although the estimates are not statistically different from zero for districts overall.[29] 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). Capital

[27] 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 six years vs. 263 elections with 54 failures after two 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.

[28] 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. We also estimate a model that includes an interaction between $Pass_j$ and campus age at baseline and also district fixed effects. This specification assesses whether bond passage differentially affects schools of different ages in the same district.

[29] 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 Figure A7 plots the fraction of the 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.

campaigns also reduce the effective age of old school facilities by roughly seven years, and this effect is statistically significant.[30]

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 that relatively few students are affected by such changes. We find that school opening lags behind investment by about one year, and that the largest rates of opening occur in Years 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 school 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, Schneider, and Shang 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 teach at another school in the district, to move to another district, or to leave the profession). Thus, the only modest impact on school conditions for the typical student does not translate to measureable effects on teacher retention.

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

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[30] 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 Figure 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.

achievement or attendance, a conclusion that is echoed in the graphical evidence (Figure 5). For grades three through eight, the point estimates are initially close to zero and inconsistent in sign. By Year Six, 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 Figure 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 Cellini, Ferreira, and Rothstein, of a roughly 0.067 and 0.077 student-level standard deviation improvement for third 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 Two 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 tenth and ninetieth percentile of the individual test score and attendance distributions within districts. We find no evidence that bond passage 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. Using alternative bandwidths, 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.[31] 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 whether 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.[32] 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 Six they are statistically significant for both math

[31] 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 our section on methods, 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.

[32] The smaller groups of students who received reduced price lunch (but not free) represent an intermediate category and were excluded from this discussion, though they are included when examining district-level mean outcomes.

and reading. Nonetheless, a careful examination of this finding under alternative specifications leads us to discount this result somewhat, as the magnitude and significance are 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).[33]

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 $7,756 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.[34] To test for dosage effects, we look at differences by several baseline (preelection) characteristics likely to be associated with the treatment intensity. We implement this by interacting bond passage in Equation (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

[33] In results available from the authors, we also find that economically disadvantaged students do not experience larger-than-average improvements in campus conditions following bond passage, as measured by average campus age and the share of students enrolled in new schools.

[34] In fact, the New Haven campaign would be in the ninety-ninth percentile of all bond electrions proposed by school districts in Texas between 1997 and 2010.

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 six-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.[35]

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 or newly opened using an event-study model with student-level microdata.

Figure 7 depicts our main event study estimates for school renovations. These models include campus fixed effects, year-grade fixed effects, and control for lagged test scores. 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 0.013 for math and 0.016 for reading for the first four years following the renovation. The bottom row of Figure 7 isolates renovations likely to be associated with large facility condition improvements by limiting the sample to schools that were in poor condition in 1991 (measured by being in the bottom two quintiles of average room condition) but that were in

[35] 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 that have fewer poor students, smaller class sizes, and 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.

good or excellent condition by 2006. Again, we find no indication that these renovations lead to improved student achievement in math or reading. Here, the flat preexisting 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 (not reported) are very similar. Table 7 presents these estimates and also includes a specification that does not control for lagged test scores. Importantly, controlling for lagged test scores does not meaningfully effect estimated point estimates (though it does improve precision considerably). This 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 presents event-study estimates of the effect of school openings. We find some suggestive evidence that campus openings are associated with test score improvements, though the estimates are not robust to various sample restrictions and differ between math and reading. In the full sample, math scores begin to improve two years after a new campus opens, increasing by 0.10 of a standard deviation after more than six years. However, focusing on school groups whose total enrollment did not change by more than 25 percent (column 2) and those with a clearly identifiable counterfactual school (column 3) alters or eliminates these patterns. Reading test scores follow a similar pattern, though point estimates are much smaller in magnitude. Also, it is worth noting that point estimates without controls for lagged test scores are twice as large (not reported), suggesting advantaged changes in student composition following campus openings. Given the lack of robustness, differences between math and reading, and changes in observed student composition (as measured by lagged test scores), we put less confidence in our estimates of campus openings than for major renovations.

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. Estimates for school openings are less robust and more subject to sample selection bias, leaving open the possibility that new school openings could improve student achievement. 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.

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, which we address in this section. 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, an estimate of the impact of instructional spending on achievement can be obtained from results for the Project STAR class size reduction experiment. Project STAR increased contemporaneous student achievement by about 0.20 standard deviations for a 50 percent increase in instructional spending (Chetty et al. 2011, Krueger 1999; Schanzenbach

2006). Mean annual per- pupil instructional spending and the capital expenditures resulting from school bond passage are roughly the same (about $5,000), implying that a bond-funded capital campaign is comparable to about a 100 percent increase in instructional spending.[36] Assuming the effect of class size is linear, the average school bond could fund a year of class size reduction that would generate improvements equal to 0.40 of a standard deviation. Our RD analysis can rule out such large effects, which suggests that spending on school facilities improves contemporaneous student achievement by less than increasing instructional spending by the same amount. But how much additional instructional spending would be required to generate the improvements in student achievement implied by the smallest effect sizes ruled out by our confidence intervals? From Figure 6, 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. These effect sizes are about 15 and 20 percent, respectively, of the achievement gain (0.40) that would be generated by class size reductions that cost the same as the typical capital campaign in Texas. Difference-in-difference estimates (i.e., from models with no vote share controls) imply an even smaller range, 7.5 to 12.5 percent.

Event study analysis of renovations allows us to rule out smaller effects both because the scale of investment is larger than for district-level bond passage and because point estimates are more precise.[37] 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 (TCPA 2014).[38] Again, assuming linearity of treatment effects, an instructional spending increase of this

[36] While the capital spending is an investment that pays out over a number of years and the capital depreciates over time, the first cohort exposed to the capital spending benefits by an amount that does not depend on the rate of depreciation.

[37] We do not explicitly address the effects of new school openings in this calculation, as the estimates and implied confidence regions were inconsistent and generally not more informative than our RD estimates.

[38] According to the report, the average elementary school construction project cost $17,461 (76 percent of projects) and the average middle school project cost $21,473 (24 percent of projects). These figures include both

magnitude would lead to student achievement gains of 1.44 standard deviations. From Figure 7, we can rule out impacts of school renovations several years after the renovation of about 0.02 of a standard deviation, or about 1.4 percent of the achievement gains associated with an increase in instructional spending of comparable cost. Thus, a dollar spent on school renovations has a smaller impact on contemporaneous student achievement than a $0.014 investment in class size reduction.

From the perspective of contemporaneous achievement effects, capital spending has a much smaller impact than spending on instruction. However, several factors make capital spending more cost-effective than the above calculations imply. Most importantly, capital spending is durable. A newly constructed school could continue to benefit students well after the initial investment, whereas smaller classes for one cohort should not benefit future cohorts. Thus, capital spending could still be cost-effective even with very small treatment effects. The upper limit of our confidence interval for the effects of renovations implies that capital spending would need to last for more than 70 years before it would be as cost-effective as class size reduction at improving student achievement.[39] The condition of campuses in our study appears to deteriorate much more rapidly than this. Capital investment may also be easier to scale up than class-size reduction (see the discussion in Schanzenbach 2006), as it does not require the hiring of additional teachers, though we do not have a way to quantify how this would alter our calculations.

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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).

[39] Since a typical renovation has (at most) an effect on achievement that is 1.4 percent of what would result from an equally costly class size reduction program, it would need to benefit 71 cohorts (71 = 100/1.4) to have the same impact on achievement minus years per dollar spent.

CONCLUSION

School facility spending represents one of the largest educational investments in the United States, 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 percent of the impact of a comparable increase in instructional spending, while difference-in-differences and event study estimates allow us to rule out much smaller achievement effects (12.5 and 1.4 percent 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, Ferreira, and Rothstein (2010) and the campus-level event-study approach of Nielson and Zimmerman

(2014), though the latter study investments targeting schools in much worse condition than the more typical investment we consider.[40]

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 (CFR, 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 area of future inquiry.

[40] Cellini, Ferreira, and Rothstein (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 percent CI reported in Figure 6 excludes their point estimate for reading but not math, but our difference-in differences 95 percent CI excludes their estimates for both subjects. Neilson 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 percent CI from the top panel of Figure 7 by four excludes the 0.11–0.12 standard deviation increase they observe for reading, but not the 0.04–0.05 standard deviation increase they observe for math.

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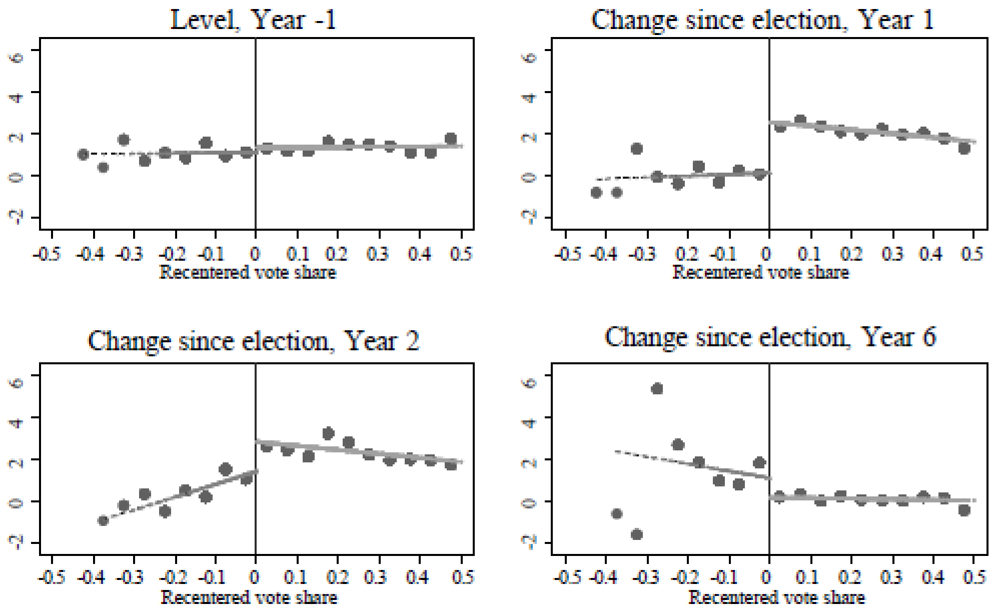
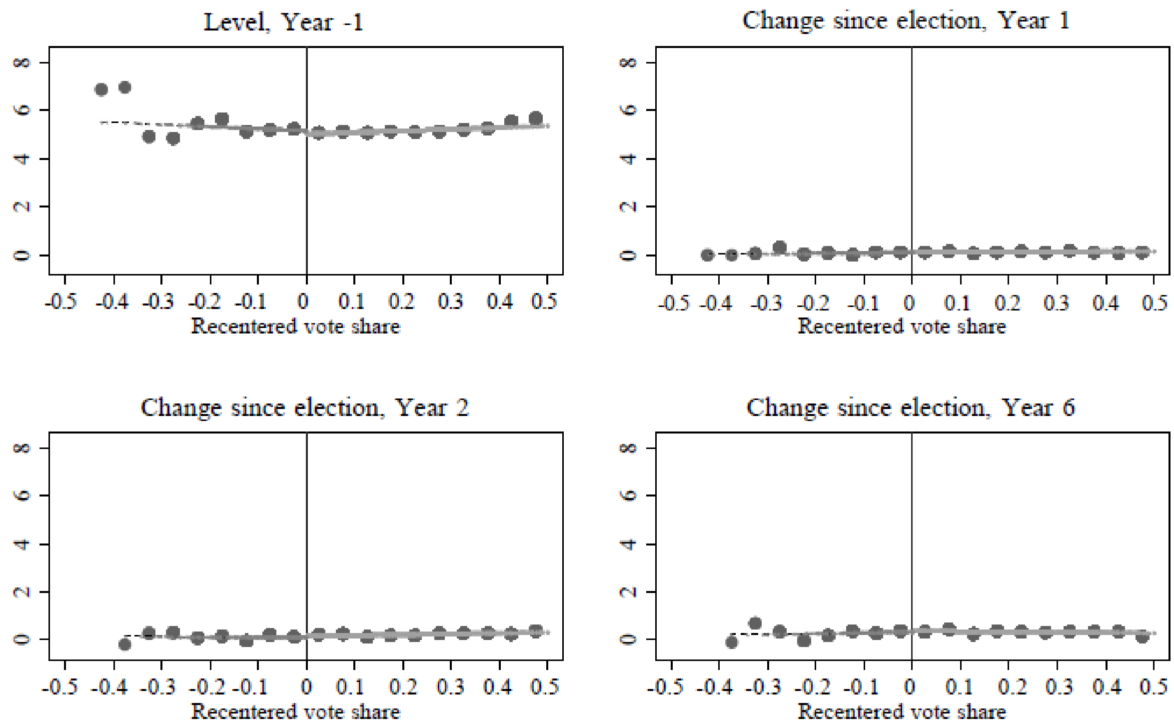
Figure 1 Level and Change in Capital Spending by Vote Share, before and after Bond Election



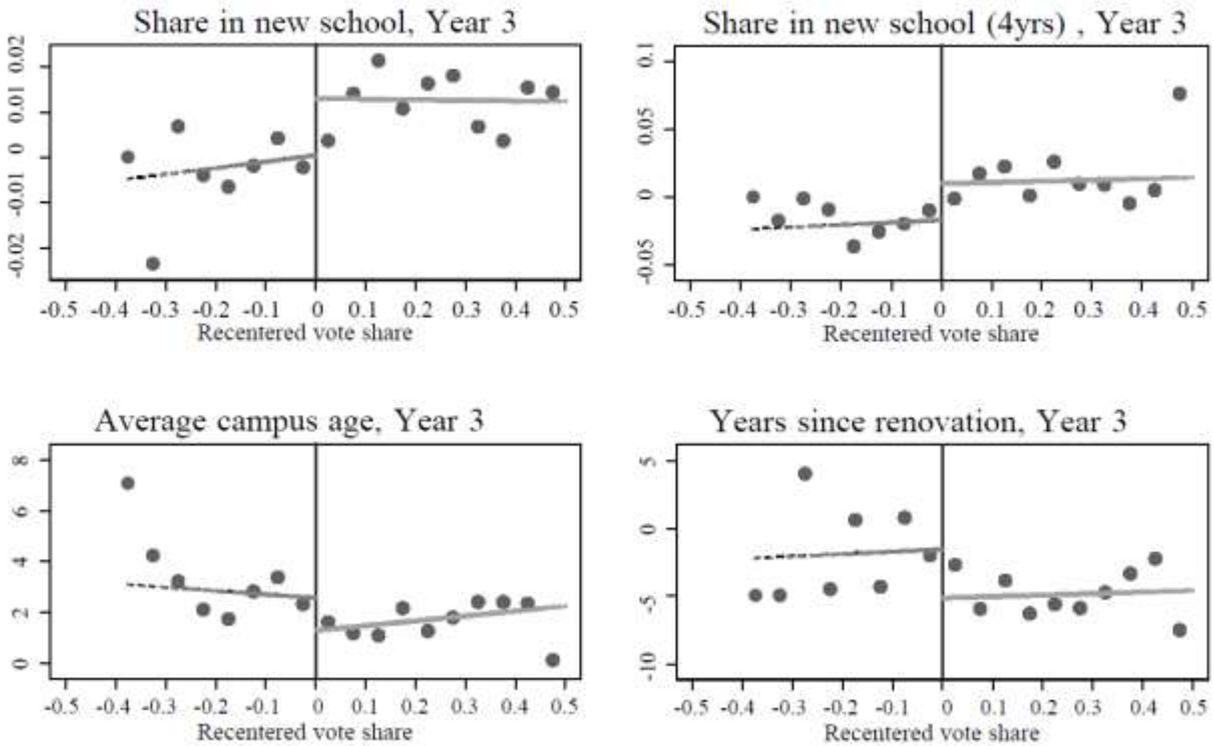
NOTE: Graphs plot average district capital spending (in $000s) or change in average district capital spending (relative to years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5-point bins of vote share. Includes data for 1,737 elections and 812 districts.
SOURCE: Spending data is from the NCES Common Core.

Figure 2 Instructional Spending by Vote Share, before and after Bond Election



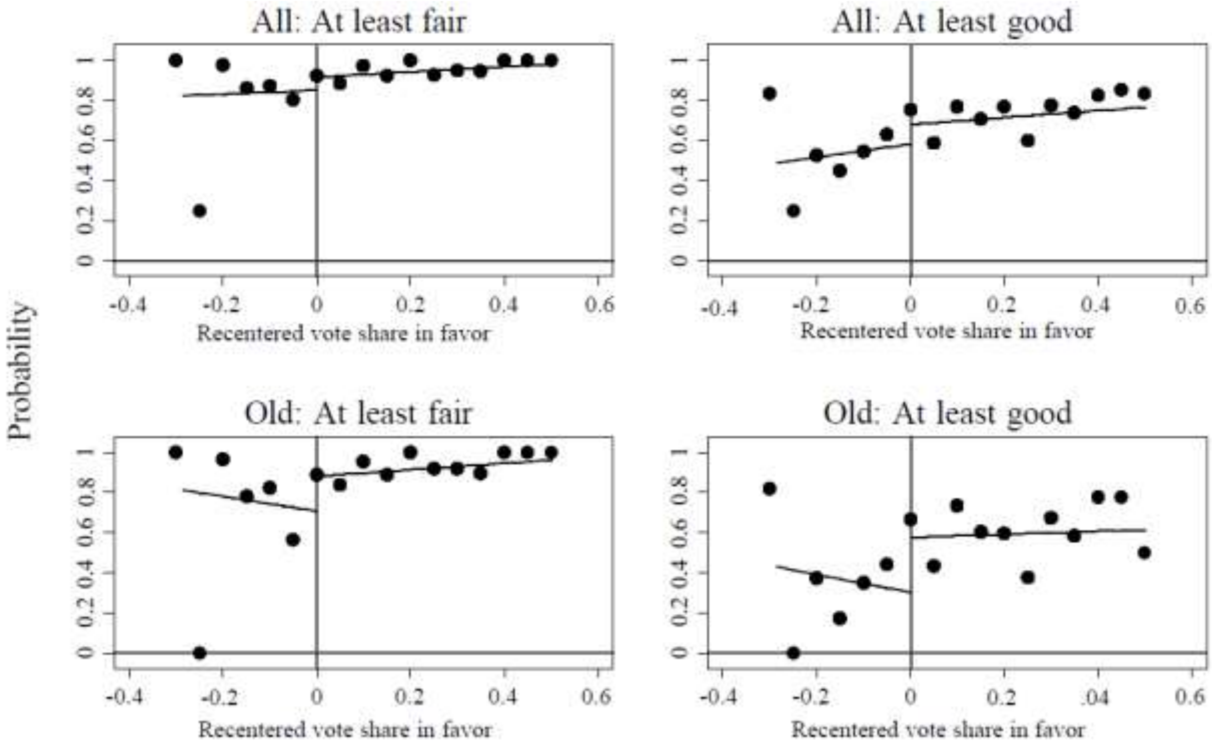
NOTE: Graphs plot average district instructional spending or change in average district instructional spending (relative to years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5-point bins of vote share. Includes data for 1,737 elections and 812 districts.
SOURCE: Spending data is from the NCES Common Core.

Figure 3 Capital Inputs by Vote Share, Change since Bond Election



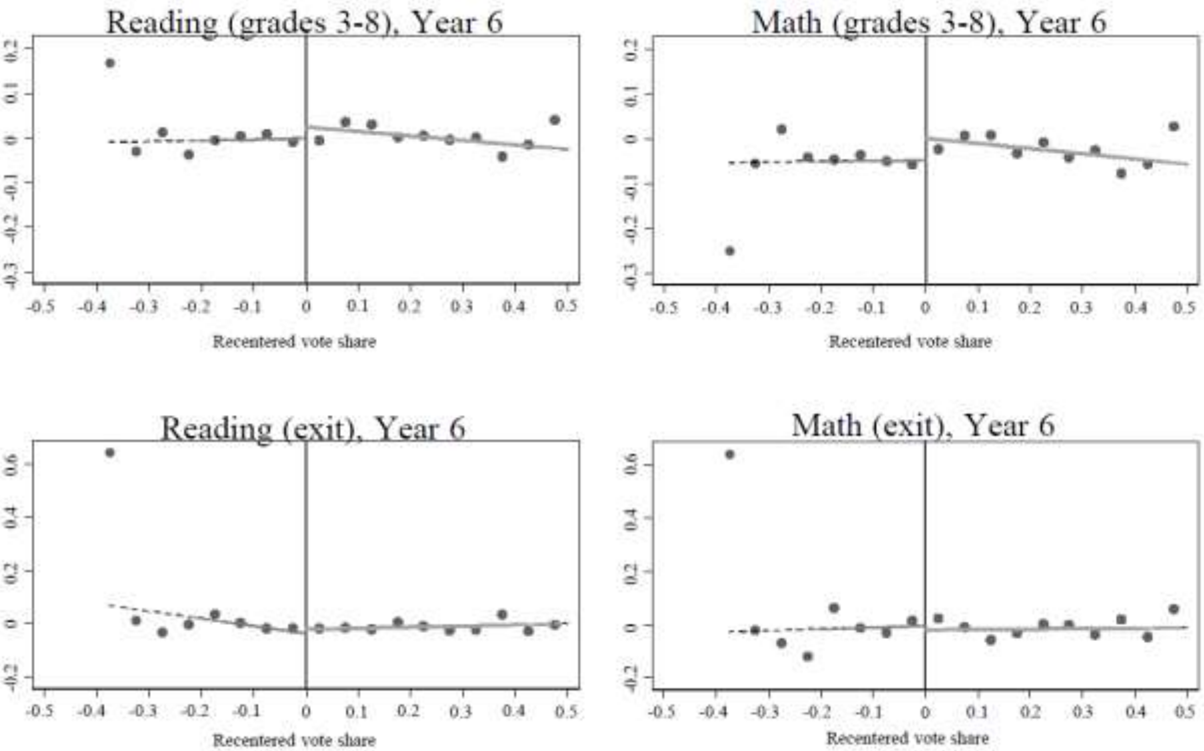
NOTE: Graphs plot change in average district building conditions (relative to two years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5-point bins of vote share. Top row includes data for 1,737 elections and 812 districts. Bottom row includes data for 804 districts and 228 districts (465 elections) for campus age and years since renovation, respectively.

Figure 4 Building Condition by Vote Share



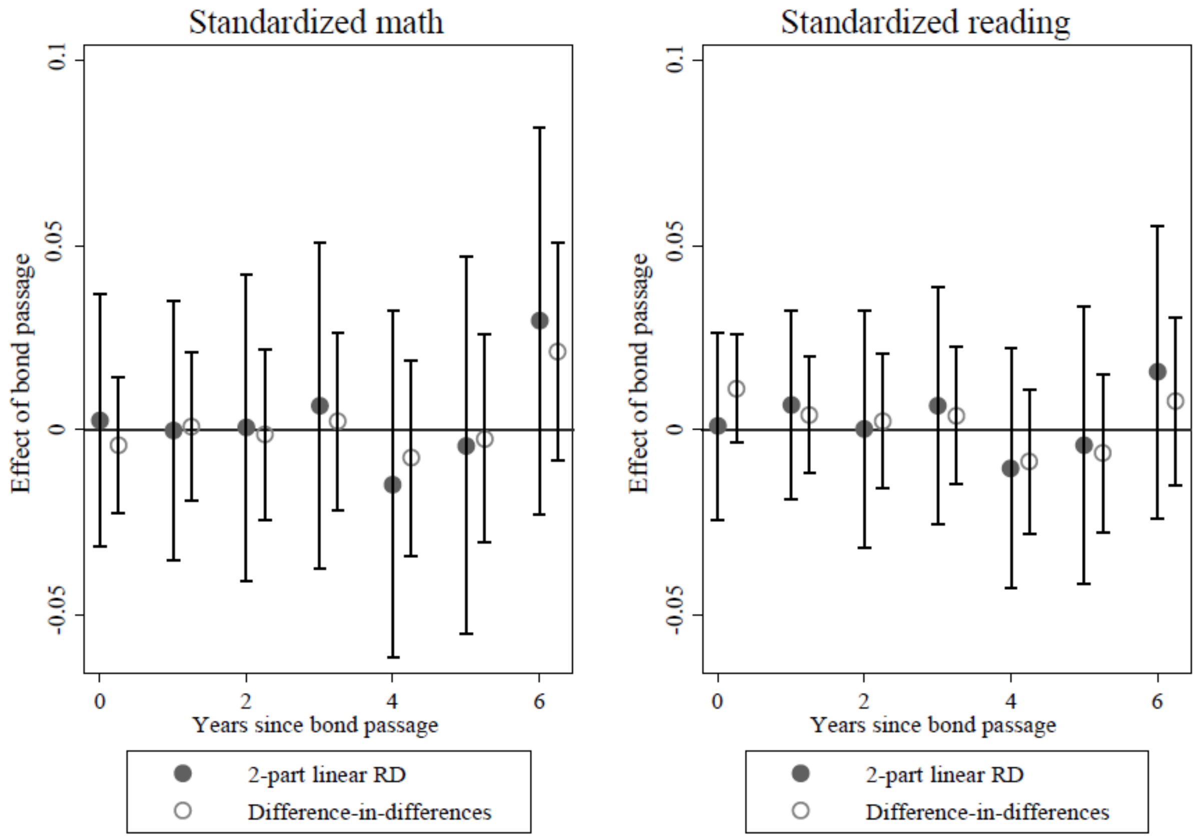
NOTE: Graphs plot fraction of district buildings in fair or good condition, separately by the vote share in favor of bond passage for first election held between 1997 and 2006. Elections were grouped in 5-point bins of vote share. Campus-level observations were weighted inversely by enrollment so that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

Figure 5 Achievement Test Scores by Vote Share, Change since Bond Election



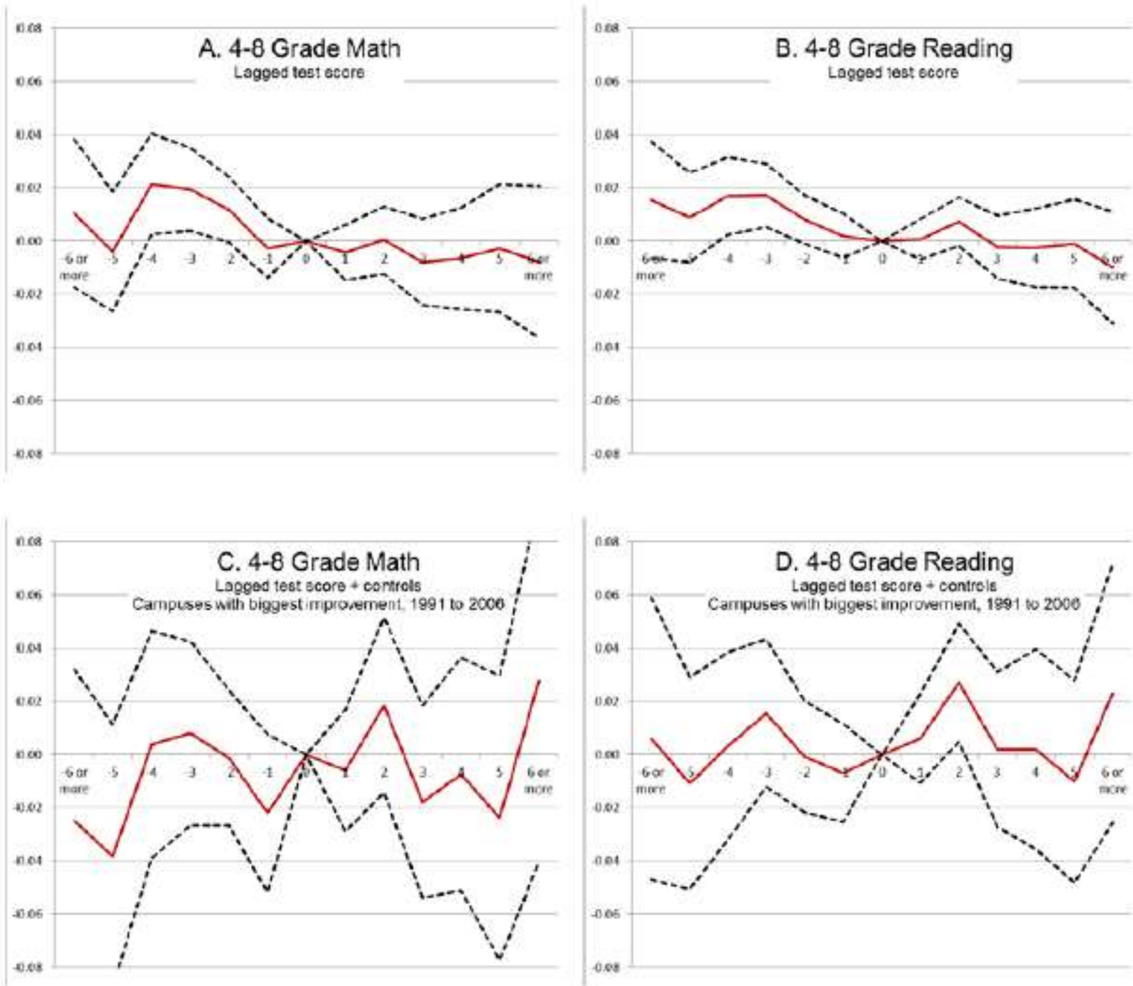
NOTE: Graphs plot change in average district test scores (relative to the two years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5-point bins of vote share. Includes data for 1,737 elections and 812 districts.

Figure 6 Effect of Bond Passage on Student Achievement, RD vs. Difference in Differences Estimates



NOTE: 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 nonpassing 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.

Figure 7 Event-Study Estimates of Effect of Campus Renovations



NOTE: Graphs plot coefficients (and 95% confidence intervals) on dummies for years prior and after a major campus renovation, estimate from Equation (4). All models include campus fixed effects, lagged test scores, and year X grade fixed effects. Panels C and D additionally control for student sex, race, and free lunch status. 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 having a "good" or "excellent" overall building condition in 2006. Sample sizes are 3.4 million student-years (1,354 campuses) for top row and 713,000 student-years (256 campuses) for bottom row. Standard errors clustered by campus.

Table 1 Summary Statistics of Capital Bond Elections

					Bond amount (millions of $2010)		Bond amount per student ($2010)		Multiple
Year	Number	Pass	Vote share	Votes cast	Mean	Median	Mean	Median	elections held
1997	36	0.86	0.69	2,003	36.1	17.7	6,913	4,884	0.19
1998	185	0.85	0.70	1,181	24.5	10.0	7,032	5,311	0.11
1999	120	0.84	0.67	3,493	59.4	13.7	8,805	6,866	0.17
2000	166	0.83	0.69	1,116	35.0	8.8	7,698	6,064	0.13
2001	121	0.83	0.68	1,636	48.3	9.5	8,962	7,576	0.21
2002	137	0.82	0.66	2,075	48.1	11.1	8,486	6,717	0.12
2003	105	0.70	0.62	3,669	70.4	18.2	10,353	7,941	0.25
2004	114	0.84	0.63	2,993	68.5	24.9	9,653	5,995	0.35
2005	95	0.69	0.60	2,849	64.1	23.1	12,433	8,689	0.31
2006	138	0.82	0.62	1,561	57.3	22.3	11,777	8,937	0.23
2007	180	0.86	0.63	3,072	56.9	21.6	14,255	11,187	0.23
2008	156	0.77	0.60	2,970	102.0	23.1	16,110	12,037	0.15
2009	85	0.73	0.58	4,723	34.6	13.9	23,135	12,783	0.25
2010	98	0.61	0.55	1,489	29.9	13.9	10,984	8,992	0.13
All	1,737	0.80	0.64	2,392	53.3	15.2	11,086	7,756	0.19

NOTE: 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 are on different dates) or largest (by bond amount) bond proposition.
SOURCE: 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).

Table 2 Summary Statistics of District Characteristics in Year Prior to Election

	Year prior to election		
	All elections	Passed	Failed
Total enrollment	6,723	7,154	4,995
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 Fraction	12.8	12.8	12.8
vocational ed Fraction	21.7	21.4	22.9
bilingual	7.6	8.0	6.2
Fraction gifted	7.3	7.3	7.0
Instructional spending per student ($2010)	5,202	5,182	5,284
Capital outlay per student ($2010)	1,305	1,354	1,107
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 (N)	812	748	279
Elections (N)	1,737	1,390	347

NOTE: Most variables are defined for the full sample of 1,737 unique elections. Enrollment-weighted average building age (years since renovation) are only available for 530 (227) districts and 1,132 (464) elections.

Table 3 Effect of Bond Passage on Educational Inputs (Two-part linear specification with election or district fixed effects.)

	Effect of bond passage after						
	1 year	2 years	3 years	4 years	5 years	6 years	*n*
Panel A. Capital Spending (ITT)							
Capital outlays per student (mean = $1,305)	2330***	1230***	-735	-415	-579	-723	14,455
	(298)	(394)	(449)	(413)	(361)	(516)	
Cumulative capital outlays since election	2595***	3950***	2875***	2578***	2100**	1,514	10,982
	(290)	(546)	(712)	(854)	(1,005)	(1,153)	
Panel B. Capital Spending (TOT)							
Capital outlays per student (mean = $1,305)	2745***	2368***	734*	469	309	-109	12,172
	(288)	(376)	(393)	(368)	(315)	(462)	
Cumulative capital outlays since election	3199***	5376***	5271***	5319***	5738***	5007***	11,360
	(410)	(678)	(857)	(968)	(1,125)	(1,363)	
Panel C. Instructional Inputs (TOT)							
Instructional spending per student (mean = $5202)	-46	27	32	96	176**	158*	12,172
	(65)	(72)	(74)	(85)	(87)	(87)	
Student-teacher ratio (mean = 13.53)	-0.239	-0.240	-0.229	-0.216	-0.136	0.038	14,602
	(0.210)	(0.235)	(0.256)	(0.290)	(0.283)	(0.298)	

NOTE: 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 nonpassing 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 X 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 nonpassing bond) in the current year and each previous year up to 10. The table reports the bond passage indicators for each lag. Estimates for current period and lags greater than 6 are not displayed. Reported mean is for the year prior to the election. Standard errors are clustered at the district level. Significance: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

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

NOTE: 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 to 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 nonpassing bond) in the current year and each previous year up to 10. The table reports the bond passage indicators for each lag. Estimates for current period and lags greater than 6 are not displayed. Reported mean is for the year prior to the election. Standard errors are clustered at the district level. Significance: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

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

NOTE: Each row represents a separate specification and reports effects of bond measure passage on outcomes *t* years later. The sample incudes 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 nonpassing bond) in the current year and each previous year up to 10. The table reports the bond passage indicators for each lag. Estimates for current period and lags greater than 6 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: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Table 6 Socioeconomic Heterogeneity in Effect of Bond Passage
(TOT, Two-part linear specification with district fixed effects.)

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

NOTE: 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 nonpassing bond) in the current year and each previous year up to 10. The table reports the bond passage indicators for each lag. Estimates for current period and lags greater than 6 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 group x district 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: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Table 7 Event-study Estimates of Effects of Campus Renovations

	Campus Renovations					
	4th-8th Grade Math			4th-8th Grade Reading		
	All campuses (1)	All campuses (2)	Large improvement, 1991-2006 (3)	All campuses (4)	All campuses (5)	Large improvement, 1991-2006 (6)
6+ years prior	0.0021	0.0106	-0.0247	0.0275	0.0156	0.0061
	(0.0272)	(0.0143)	(0.0290)	(0.0201)	(0.0112)	(0.0271)
5 years prior	-0.0101	-0.0040	-0.0382	0.0139	0.0088	-0.0108
	(0.0208)	(0.0115)	(0.0252)	(0.0156)	(0.0086)	(0.0204)
4 years prior	0.0089	0.02149**	0.0038	0.02492*	0.01693**	0.0031
	(0.0172)	(0.0096)	(0.0217)	(0.0129)	(0.0074)	(0.0180)
3 years prior	0.0189	0.01946**	0.0081	0.02691***	0.01728***	0.0156
	(0.0133)	(0.0079)	(0.0177)	(0.0101)	(0.0061)	(0.0142)
2 years prior	0.01872*	0.01177*	-0.0012	0.02155***	0.00811*	-0.0008
	(0.0096)	(0.0063)	(0.0130)	(0.0073)	(0.0047)	(0.0107)
1 year prior	0.0058	-0.0027	-0.0221	0.01022**	0.0020	-0.0069
	(0.0063)	(0.0057)	(0.0152)	(0.0049)	(0.0042)	(0.0094)
1 year after	-0.0025	-0.0043	-0.0059	0.0009	0.0007	0.0062
	(0.0065)	(0.0053)	(0.0117)	(0.0050)	(0.0039)	(0.0086)
2 years after	0.0037	0.0004	0.0186	0.0126	0.0073	0.02719**
	(0.0104)	(0.0065)	(0.0168)	(0.0079)	(0.0047)	(0.0113)
3 years after	-0.0009	-0.0079	-0.0177	0.0147	-0.0023	0.0019
	(0.0140)	(0.0083)	(0.0184)	(0.0108)	(0.0061)	(0.0149)
4 years after	-0.0068	-0.0065	-0.0074	0.0104	-0.0025	0.0020
	(0.0181)	(0.0098)	(0.0224)	(0.0137)	(0.0075)	(0.0192)
5 years after	-0.0043	-0.0026	-0.0238	0.0058	-0.0010	-0.0102
	(0.0225)	(0.0123)	(0.0273)	(0.0163)	(0.0085)	(0.0194)
6+ years after	-0.0050	-0.0078	0.0279	-0.0029	-0.0098	0.0230
	(0.0267)	(0.0145)	(0.0348)	(0.0204)	(0.0107)	(0.0248)
Lagged score	No	Yes	Yes	No	Yes	Yes
Other controls	No	No	Yes	No	No	Yes
Fixed effects	Year, Grade	Year X Grade	Year X Grade	Year, Grade	Year X Grade	Year X Grade
Observations	3,387,465	3,387,465	713,352	3,383,471	3,383,471	712,597
R-squared	0.10886	0.528	0.524	0.091	0.494	0.490
Events	1354	1354	256	1354	1354	256
Campuses	1354	1354	256	1354	1354	256

NOTE: 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) include campuses that were in the bottom 40% of average room condition in 1991 but were rated as having a "good" or "excellent" overall building condition in 2006. Standard errors clustered by campus. Significance: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

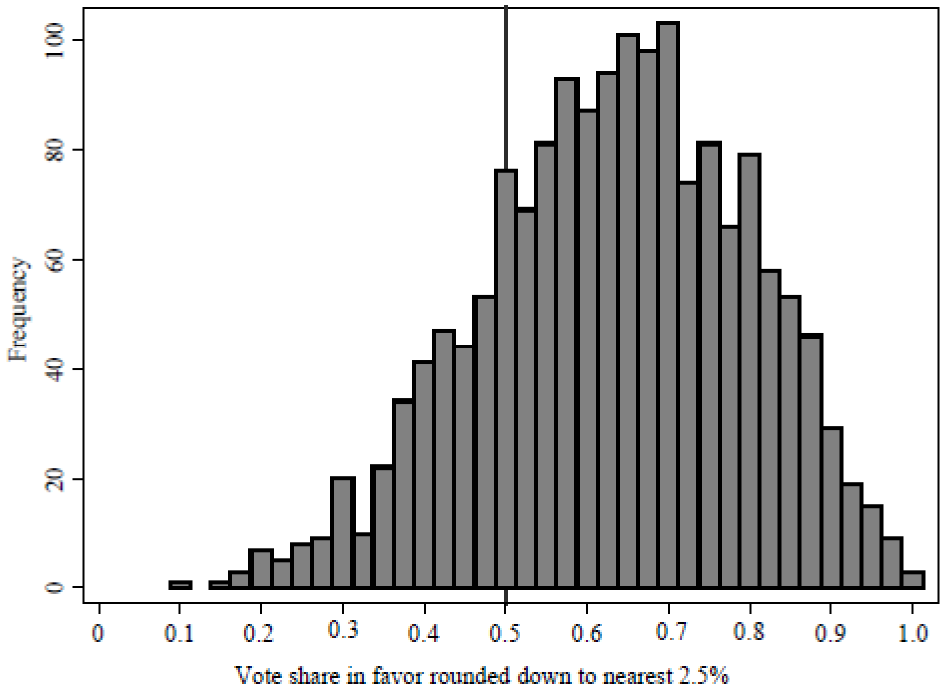
Table 8 Event-study Estimates of Effects of Campus Openings

	Campus Openings					
	4th-8th Grade Math			4th-8th Grade Reading		
	All campuses (1)	Small enrollment change in school group (2)	Counterfact. school 75% of enrollment (3)	All campuses (4)	Small enrollment change in school group (5)	Counterfact. school 75% of enrollment (6)
6+ years prior	0.00338	0.00465	0.08195	0.00418	0.00089	0.04319
	(0.0274)	(0.0353)	(0.0508)	(0.0145)	(0.0204)	(0.0283)
5 years prior	0.02205	0.03073	0.04278	0.01316	0.00607	0.00807
	(0.0212)	(0.0309)	(0.0410)	(0.0130)	(0.0196)	(0.0259)
4 years prior	-0.0005	0.00642	0.03313	-0.00984	-0.00896	-0.01315
	(0.0174)	(0.0254)	(0.0343)	(0.0098)	(0.0141)	(0.0172)
3 years prior	0.00696	0.02241	0.06510**	0.00162	0.00467	0.01252
	(0.0136)	(0.0202)	(0.0246)	(0.0085)	(0.0130)	(0.0179)
2 years prior	-0.01134	0.00269	0.01459	0.00187	-0.00134	-0.00333
	(0.0109)	(0.0148)	(0.0194)	(0.0076)	(0.0091)	(0.0149)
1 year prior	0.00427	0.0001	0.00711	0.00648	0.00169	0.00655
	(0.0154)	(0.0234)	(0.0303)	(0.0129)	(0.0200)	(0.0247)
1 year after	0.01203	-0.01019	-0.02309	0.00835	-0.00975	-0.00588
	(0.0179)	(0.0247)	(0.0317)	(0.0135)	(0.0168)	(0.0189)
2 years after	0.04043**	0.01937	0.01128	0.02162	0.00058	-0.00706
	(0.0192)	(0.0237)	(0.0309)	(0.0140)	(0.0165)	(0.0235)
3 years after	0.05477**	0.0357	-0.03254	0.02718	-0.00501	-0.03062
	(0.0267)	(0.0316)	(0.0301)	(0.0168)	(0.0220)	(0.0269)
4 years after	0.09134***	0.10909***	-0.02056	0.04834***	0.05844**	-0.01959
	(0.0311)	(0.0375)	(0.0511)	(0.0182)	(0.0227)	(0.0270)
5 years after	0.07013**	0.03637	-0.00272	0.03670*	0.01727	-0.01328
	(0.0350)	(0.0420)	(0.0541)	(0.0212)	(0.0266)	(0.0309)
6+ years after	0.09969**	0.10517*	-0.01935	0.05355**	0.05300*	0.01436
	(0.0454)	(0.0578)	(0.0693)	(0.0247)	(0.0295)	(0.0415)
Lagged score	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Year X Grade	Year X Grade	Year X Grade	Year X Grade	Year X Grade	Year X Grade
Observations	1,198,350	474,734	235,338	1,196,066	474,060	234,908
R-squared	0.524	0.538	0.527	0.489	0.501	0.508
Events	258	95	48	258	95	48
Campuses	516	190	96	516	190	96

NOTE: All specifications also include school-group fixed effects. Sample includes all test score observations from 1996 to 2006 in newly opened campuses and matched counterfactual schools with consistent information on opening date between 2006 survey and AEIS data and for which counterfactual school was identifiable. Sample further restricted to individuals for whom prior year test score is available. Specifications (2) and (5) isolate school openings in which the total 4th – 8th grade enrollment in the school group does not change by more than 25%. Specifications (3) and (6) isolate school openings in which at least 75% of students at the newly opened school are predicted to have gone to the matched counterfactual school. Standard errors clustered by school group. Significance: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

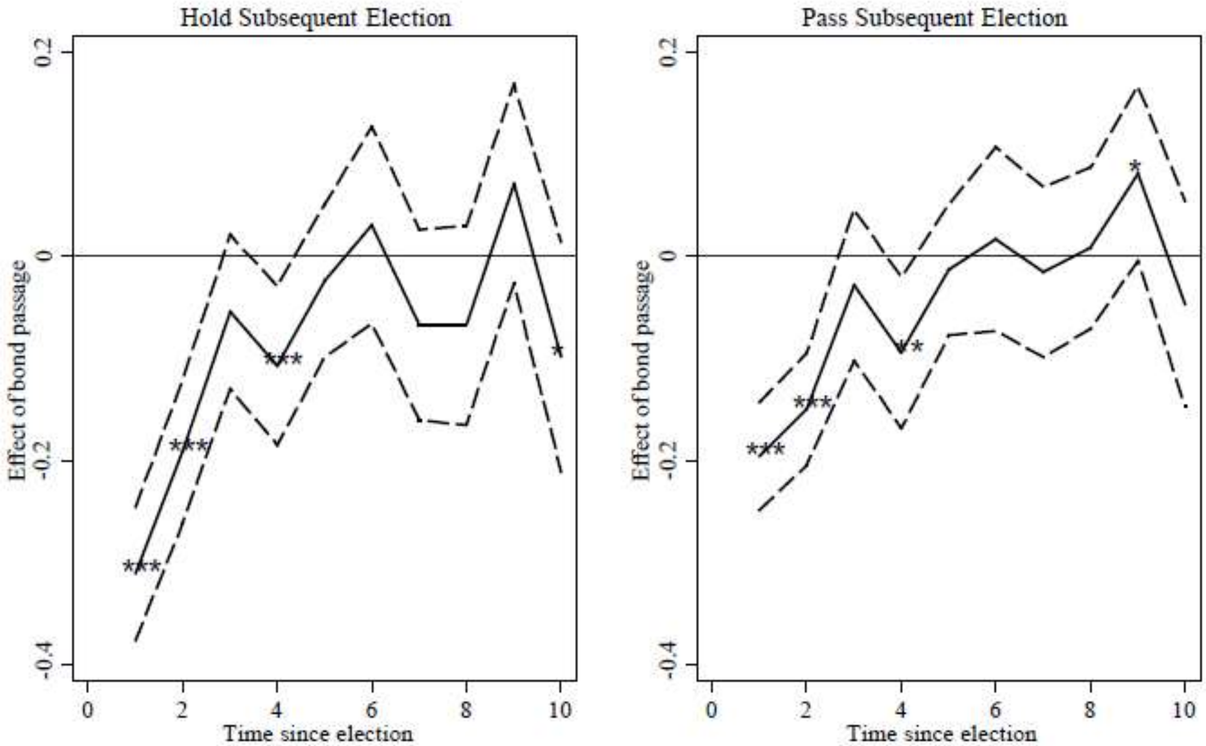
APPENDIX A: FIGURES

Appendix Figure A1 Histogram of Vote Shares



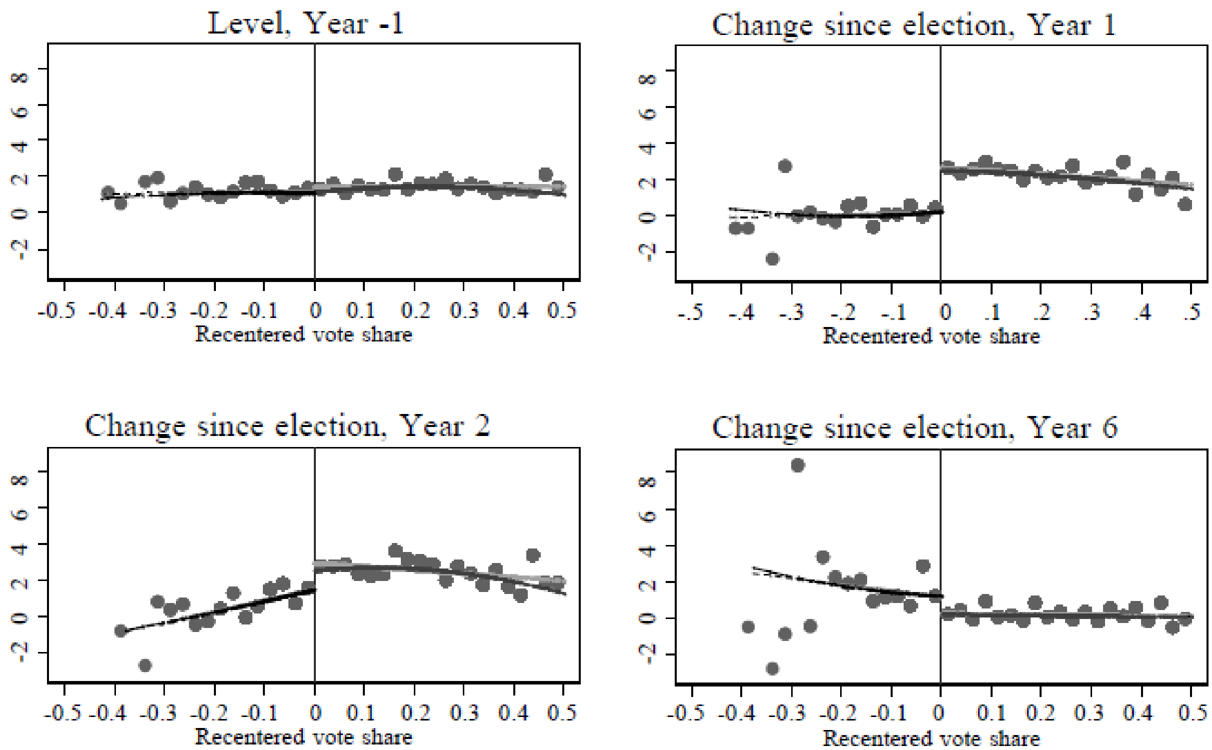
NOTE: Graphs frequency of election, where elections are grouped in 2.5-point bins of vote share. Includes data for 1,737 elections and 812 districts.

Appendix Figure A2 Effect of Bond Passage on Likelihood of Holding or Passing Subsequent Election



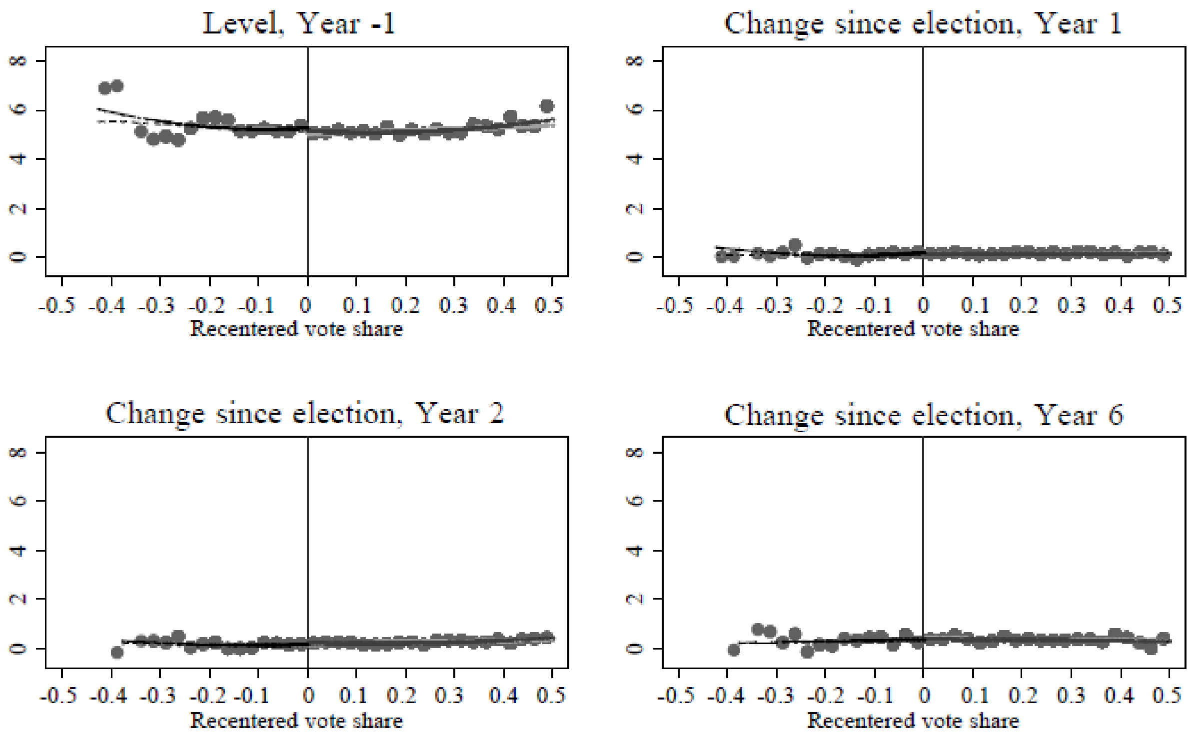
NOTE: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on indicator for holding (passing) another bond election 1 through 10 years following bond passage. Specification pools observations 2 years before through 10 years after each bond election and includes fixed effects for each separate election and a linear function of the bond measure vote share, as described in the methods section and Equation (2). Results omitting election fixed effects are indistinguishable. Observation in year of election is omitted. Markers indicate significantly different from zero at the 0.10 level (*); 0.05 level (**); and 0.01 level (***).

Appendix Figure A3 Capital Spending by Vote Share, before and after Bond Election



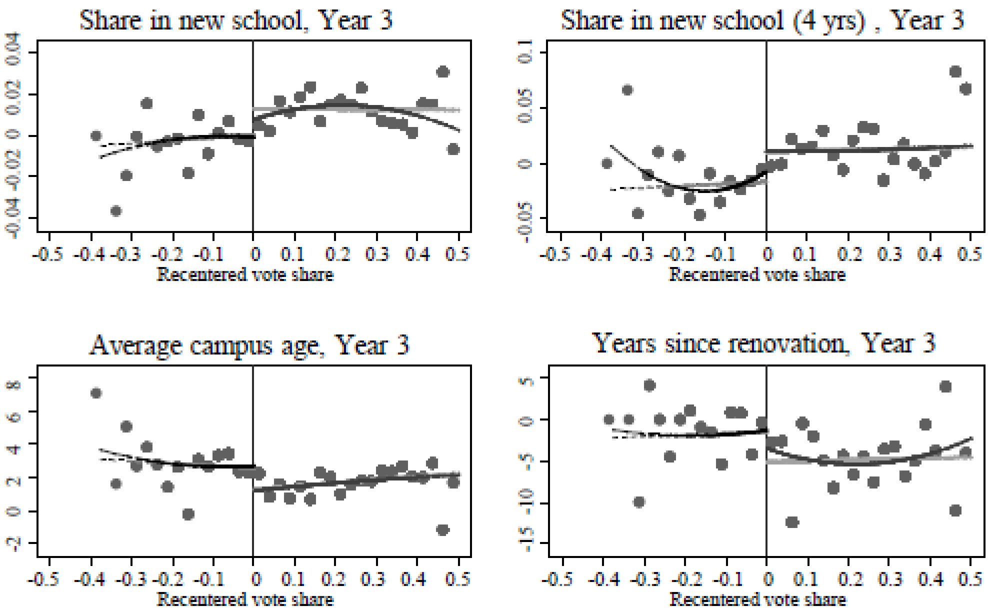
NOTE: Graphs plot average district capital spending (in $000s) or change in average district capital spending (relative to years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 2.5-point bins of vote share. Includes data for 1,737 elections and 812 districts. Spending data is from the NCES Common Core.

Appendix Figure A4 Instructional Spending by Vote Share, before and after Bond Election



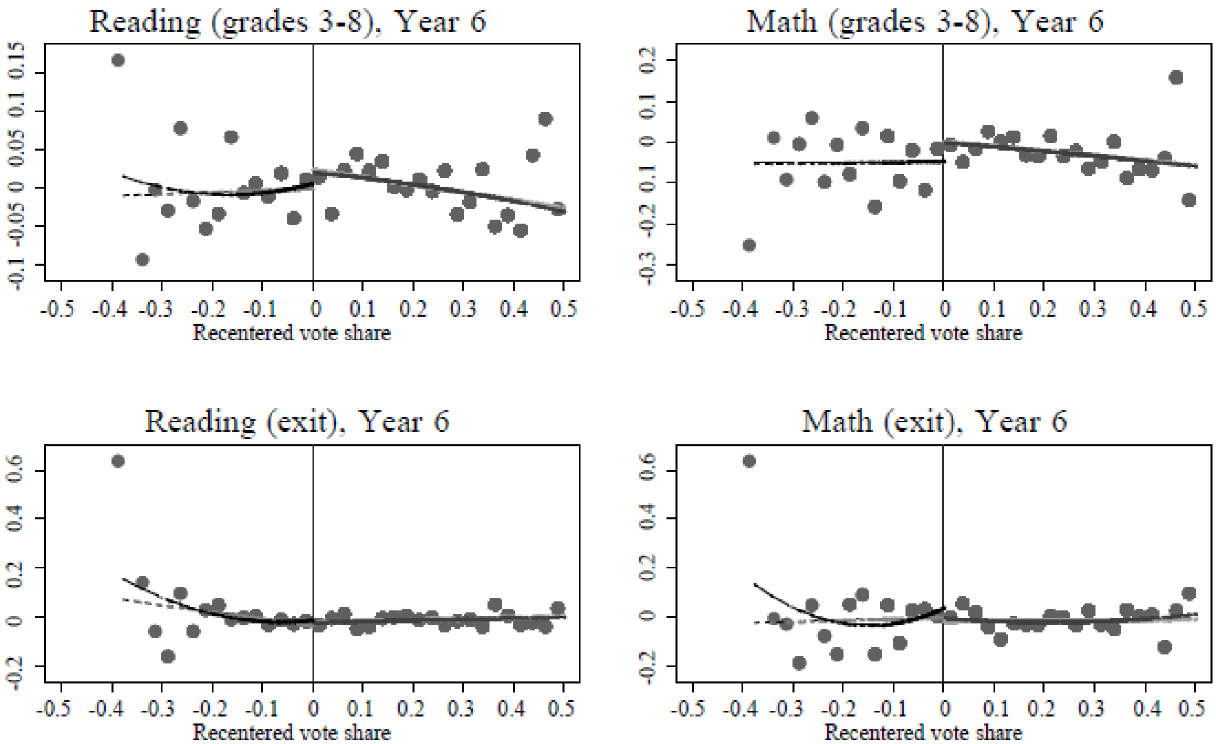
NOTE: Graphs plot average district instructional spending or change in average district instructional spending (relative to years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 2.5-point bins of vote share. Includes data for 1,737 elections and 812 districts. Spending data is from the NCES Common Core.

Appendix Figure A5 Capital Inputs by Vote Share, Change since Bond Election



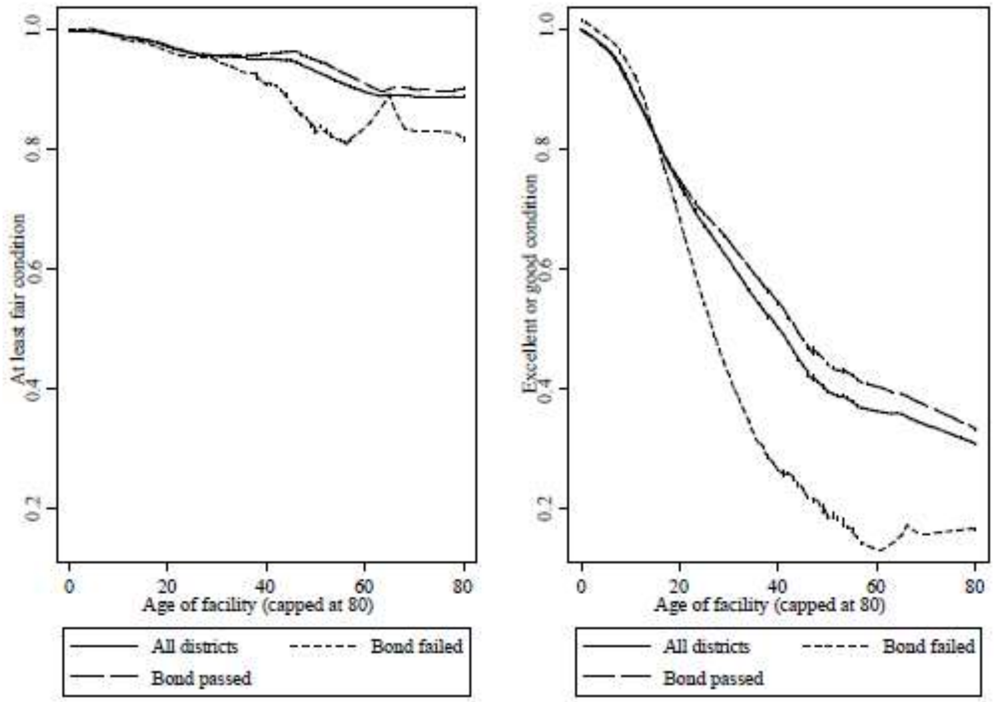
NOTE: Graphs plot change in average district building conditions (relative to two years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 2.5-point bins of vote share. Top row includes data for 1,737 elections and 812 districts. Bottom row includes data for 804 districts and 228 districts (465 elections) for campus age and years since renovation, respectively.

Appendix Figure A6 Change in Test Scores by Vote Share, Change since Bond Election



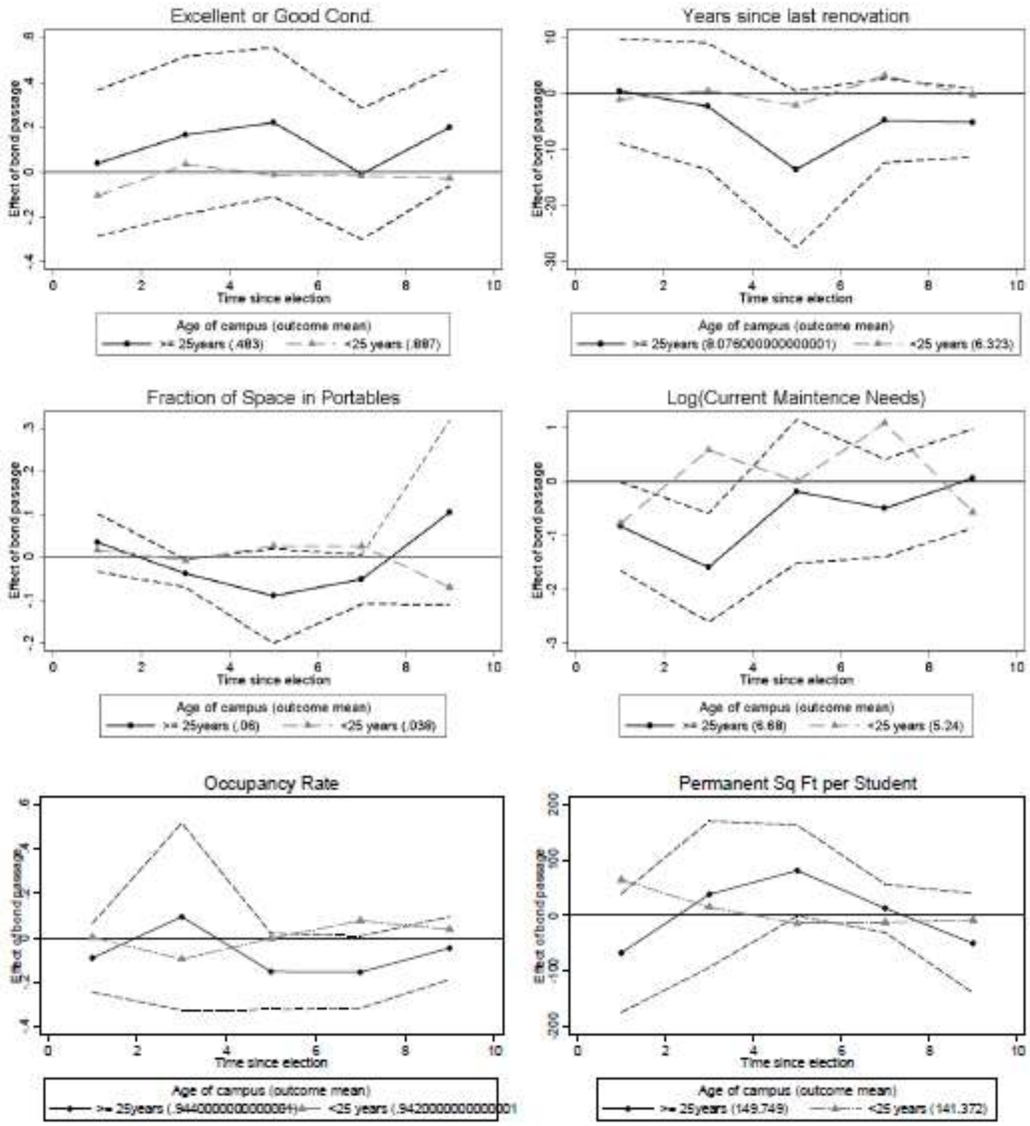
NOTE: Graphs plot change in average district test scores (relative to two years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5-point bins of vote share. Includes data for 1,737 elections and 812 districts.

Appendix Figure A7 Overall Facility Condition, by Age of Building and Earlier Election Outcome



NOTE: Graphs plot lowest estimates of the relationship between building condition and facility age. Dashed lines separate relationship by whether the earlier school bond passed or failed. Includes 204 unique districts, 573 unique bond elections, and 2,895 unique campuses.

Appendix Figure A8 Timing of Facility Improvements Following Bond Passage



NOTE: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on four measures of building condition 1 through 9 years following bond passage. Effect is permitted to vary between old campuses (at least 25 years old at time of election) and newer campuses. Time since election is grouped into 2-year bins. Confidence interval is displayed for old campuses only. Specification includes indicators for time since election (grouped into 2-year bins), bond passage and old campus interacted with these indicators separately, the interaction between passage, old, and time indicators, and a linear function of the vote share. Graphs plot the main passage effects and the old campus interactions. Outcomes are all measured in 2006, though elections are held in different years enabling the estimation of time-varying treatment effects. Includes 204 unique districts, 573 unique bond elections, and 2,895 unique campuses.

Appendix Table A1 Covariate Balance Prior to Election

	(1)	(2)		(1)	(2)
A. Educational Inputs			B. Outcomes		
Capital outlay per student ($2010) (Mean=1305)	181	83	Std test scores district mean (grades 3-8)		
	(205)	(322)	Reading (mean = 0.027)	0.022	-0.004
Instructional spending per student ($2010) (Mean=5202)	-250**	-8		(0.024)	(0.010)
	(102)	(39)	Math (mean =0.013)	0.015	-0.007
Student-teacher ratio - overall (Mean=13.529)	0.571	0.045		(0.028)	(0.014)
	(0.357)	(0.134)	Std test scores free lunch mean (grades 3-8)		
Fraction of teachers leaving campus (Mean = 0.228)	0.017	0.012	Reading (mean =-0.270)	0.005	-0.014
	(0.011)	(0.014)		(0.024)	(0.019)
Open at least one campus (0.233)	0.056	0.047	Math (mean = -0.269)	0.012	0.000
	(0.038)	(0.050)		(0.028)	(0.021)
Share of enrollment in schools opened this year (0.015)	0.007*	0.007	Std test scores not econ disadvantaged mean (grades 3-8)		
	(0.004)	(0.007)	Reading (mean = 0.200)	0.017	-0.008
Share of enrollment in schools opened in past four years (mean 0.063)	0.019*	0.008		(0.021)	(0.013)
	(0.010)	(0.008)	Math (mean = 0.184)	0.001	-0.020
Enrollment-weighted average age of school buildings (35.19)	-2.482*	-0.224		(0.024)	(0.017)
	(1.359)	(0.184)			
Enrollment-weighted average years since school last renovated (13.62)	-1.080	-1.091	Within district 90-10 test score difference (grades 3-8)		
	(1.550)	(0.698)	Reading (mean = 2.028)	-0.028	-0.027
C. District characteristics				(0.043)	(0.022)
Total enrollment (Mean=6723)	2737**	51	Math (mean = 2.234)	0.019	-0.023
	(1,306)	(41)		(0.038)	(0.023)
Fraction white (Mean=57.86)	2.332	-0.435***	Std test score on exit exam		
	(2.867)	(0.159)	Reading (mean = 0.048)	0.011	-0.017
Fraction black (Mean=8.51)	-0.345	-0.006		(0.023)	(0.026)
	(1.614)	(0.087)	Math		
Fraction Hispanic (Mean=32.09)	-2.253	0.436***			
	(2.810)	(0.147)	Attendance rate (grades 3-8)		
Fraction econ disadvantaged (Mean=46.98)	-3.250	0.393	District mean (mean = 96.37)	-0.025	-0.024
	(2.015)	(0.391)		(0.071)	(0.046)
Fraction LEP (Mean=8.43)	-0.954	0.353**	90-10 difference (mean = 7.860)	0.020	-0.083
	(0.969)	(0.147)		(0.129)	(0.083)
Fraction special ed (Mean=12.8)	0.069	-0.057			
	(0.317)	(0.158)			
Election fixed effects	No	Yes		No	Yes
Max sample size	1737	13829		1737	13829

NOTE: Each cell represents a separate specification and reports effects of bond measure passage on outcomes the year prior to election. The sample in column 1 includes all bond elections and outcome measures in the year prior to the election. This specification includes bond passage, academic year fixed effects, and bond election vote share (linearly with different slopes on each side of the passing threshold). The table reports the coefficient on bond passage. The sample in column 2 includes observations for years -2 to +6 relative to each election. Since some districts hold multiple elections in quick succession, some outcomes appear in the pooled sample multiple times for different relative years. This specification includes relative year fixed effects, academic year fixed effects, bond election vote share (linearly with different slopes on each side of the passing threshold), and interactions between bond passage and relative year fixed effects (for relative years -1 to +6). The table reports the coefficient on passage interacted with the indicator for the year prior to an election (relative year = -1). Standard errors are clustered at the district level. Significance: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Appendix Table A2 Effect of Bond Passage on Educational Inputs—Robustness (TOT, Two-part linear specification with district fixed effects.)

	Effect of bond passage after							Effect of bond passage after						
polynomial	1 year	2 years	3 years	4 years	5 years	6 years	*n*	1 year	2 years	3 years	4 years	5 years	6 years	*n*
	A. Capital outlays per student							D. Share of enrollment in schools opened in past four years						
2-part linear	2745***	2368***	734*	469	309	-109	12,172	0.008	0.021*	0.036***	0.036**	0.026*	0.023	13,791
	(288)	(376)	(393)	(368)	(315)	(462)		(0.010)	(0.011)	(0.013)	(0.015)	(0.016)	(0.017)	
None	2440***	2355***	510**	507**	290	-490		-0.001	0.012*	0.028***	0.034***	0.029***	0.022**	
	(169)	(212)	(245)	(203)	(195)	(404)		(0.006)	(0.006)	(0.007)	(0.007)	(0.008)	(0.009)	
Linear	2861***	2753***	908***	590*	143	-400		0.009	0.021**	0.036***	0.042***	0.031**	0.023	
	(303)	(382)	(351)	(313)	(283)	(399)		(0.010)	(0.011)	(0.012)	(0.014)	(0.014)	(0.015)	
Quadratic	2623***	2079***	611	480	197	-54		0.010	0.027**	0.039***	0.039**	0.028*	0.021	
	(323)	(376)	(380)	(380)	(295)	(519)		(0.011)	(0.012)	(0.014)	(0.015)	(0.016)	(0.017)	
Cubic	2464***	1944***	679*	561	147	-130		0.012	0.031**	0.043***	0.043**	0.033*	0.021	
	(364)	(439)	(398)	(408)	(323)	(465)		(0.012)	(0.014)	(0.016)	(0.018)	(0.018)	(0.018)	
	B. Cumulative capital outlays per student							E. Enrollment-weighted average age of school buildings						
2-part linear	3199***	5376***	5271***	5319***	5738***	5007***	11,360	-0.586	-0.612	-1.431**	-0.880	-0.109	-0.051	14,477
	(410)	(678)	(857)	(968)	(1,125)	(1,363)		(0.536)	(0.645)	(0.727)	(0.877)	(0.958)	(1.022)	
None	2892***	4843***	4681***	4943***	4792***	3797***		-0.233	-0.566**	-1.173***	-1.526***	-1.341***	-1.538***	
	(249)	(378)	(494)	(559)	(658)	(962)		(0.245)	(0.271)	(0.295)	(0.378)	(0.424)	(0.436)	
Linear	3199***	5725***	5700***	5814***	5743***	4859***		-1.258**	-1.278**	-2.090***	-2.009**	-1.041	-0.971	
	(407)	(658)	(796)	(907)	(1,065)	(1,333)		(0.498)	(0.596)	(0.682)	(0.799)	(0.879)	(0.964)	
Quadratic	3025***	4984***	4903***	5089***	5138***	4530***		-0.353	-0.480	-1.294	-0.863	0.059	0.170	
	(454)	(723)	(870)	(1,012)	(1,148)	(1,386)		(0.575)	(0.690)	(0.798)	(0.909)	(1.001)	(1.067)	
Cubic	2746***	4619***	4620***	4847***	4699***	4019***		-0.568	-0.829	-1.699**	-1.339	-0.428	-0.329	
	(508)	(804)	(971)	(1,160)	(1,320)	(1,525)		(0.639)	(0.736)	(0.866)	(0.979)	(1.080)	(1.156)	
	C. Instructional spending per student							F. Enrollment-weighted average years since school last renovated						
2-part linear	-46	27	32	96	175.886**	158.262*	12,172	-3.604***	-5.519***	-6.524***	-9.524***	-9.698***	-10.677***	2,964
	(65)	(72)	(74)	(85)	(87)	(87)		(1.361)	(1.637)	(2.142)	(2.302)	(2.614)	(3.349)	
None	-27	-4	20	-37	-31	-37		-3.004***	-4.307***	-4.363***	-6.408***	-6.474***	-3.319*	
	(36)	(38)	(36)	(39)	(40)	(41)		(0.742)	(0.894)	(1.085)	(1.131)	(1.158)	(1.807)	
Linear	-90	-54	30	47	150.505*	128		-3.382**	-4.848***	-5.904***	-9.112***	-9.804***	-8.068***	
	(61)	(68)	(68)	(78)	(82)	(83)		(1.364)	(1.547)	(2.116)	(2.244)	(2.535)	(3.090)	
Quadratic	-28	79	37	113	158.479*	147		-3.408**	-5.644***	-6.662***	-9.337***	-10.176***	-12.469***	
	(67)	(74)	(78)	(90)	(94)	(95)		(1.469)	(1.695)	(2.240)	(2.688)	(3.009)	(3.838)	
Cubic	-57	71	23	75	121	115		-2.290	-4.477**	-5.842**	-8.729***	-9.532***	-11.488***	
	(70)	(78)	(83)	(94)	(104)	(103)		(1.588)	(1.767)	(2.454)	(2.928)	(3.426)	(4.256)	

NOTE: 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 panel F restricted to 228 districts for which renovation data available. Model includes indicators for bond passage, holding election, and vote share (with different slopes for passing and nonpassing bond) in the current year and previous year up to 10. The table reports the bond passage indicators for each lag. Estimates for current period and lags greater than 6 are not displayed. Standard errors are clustered at the district level. Significance: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Appendix Table A3 Effect of Bond Passage on Student Achievement—Robustness

	All Students							Free Lunch Students						
	Effect of bond passage after							Effect of bond passage after						
polynomial	1 year	2 years	3 years	4 years	5 years	6 years	*n*	1 year	2 years	3 years	4 years	5 years	6 years	*n*
	A. Math scores (grades 3-8)							A. Math scores (grades 3-8)						
2-part linear	0.000	0.001	0.007	-0.015	-0.004	0.030	14,520	0.010	0.024	0.004	0.005	0.036	0.069**	13,962
	(0.018)	(0.021)	(0.022)	(0.024)	(0.026)	(0.027)		(0.025)	(0.026)	(0.026)	(0.030)	(0.034)	(0.031)	
None	0.001	-0.001	0.002	-0.008	-0.002	0.021		0.015	-0.002	0.002	0.006	0.002	0.035*	
	(0.010)	(0.012)	(0.012)	(0.013)	(0.014)	(0.015)		(0.015)	(0.016)	(0.016)	(0.017)	(0.019)	(0.019)	
Linear	-0.003	-0.004	0.009	-0.014	-0.003	0.033		-0.004	0.013	0.008	-0.002	0.023	0.067**	
	(0.018)	(0.020)	(0.021)	(0.023)	(0.025)	(0.026)		(0.023)	(0.024)	(0.025)	(0.027)	(0.031)	(0.030)	
Quadratic	-0.001	0.001	0.003	-0.012	-0.002	0.027		0.011	0.029	0.004	0.012	0.033	0.063*	
	(0.020)	(0.024)	(0.024)	(0.026)	(0.028)	(0.029)		(0.027)	(0.028)	(0.028)	(0.032)	(0.036)	(0.033)	
Cubic	-0.004	-0.002	0.003	-0.006	0.003	0.032		0.004	0.025	0.005	0.013	0.030	0.057	
	(0.023)	(0.026)	(0.027)	(0.029)	(0.032)	(0.032)		(0.030)	(0.030)	(0.031)	(0.034)	(0.039)	(0.038)	
	B. Reading scores (grades 3-8)							B. Reading scores (grades 3-8)						
2-part linear	0.007	0.000	0.007	-0.010	-0.004	0.016	14,520	0.021	0.020	0.002	0.013	0.055**	0.052**	13,962
	(0.013)	(0.016)	(0.016)	(0.017)	(0.019)	(0.020)		(0.022)	(0.024)	(0.023)	(0.023)	(0.028)	(0.026)	
None	0.004	0.002	0.004	-0.009	-0.006	0.008		0.011	0.000	0.009	0.005	0.003	0.019	
	(0.008)	(0.009)	(0.009)	(0.010)	(0.011)	(0.012)		(0.013)	(0.014)	(0.014)	(0.014)	(0.016)	(0.016)	
Linear	0.001	-0.001	0.002	-0.012	-0.006	0.020		-0.001	0.014	0.011	0.005	0.044*	0.053**	
	(0.012)	(0.015)	(0.015)	(0.016)	(0.017)	(0.018)		(0.019)	(0.021)	(0.020)	(0.022)	(0.023)	(0.025)	
Quadratic	0.008	0.002	0.006	-0.008	-0.004	0.017		0.019	0.022	0.001	0.018	0.055*	0.051*	
	(0.014)	(0.018)	(0.018)	(0.018)	(0.020)	(0.022)		(0.023)	(0.025)	(0.024)	(0.025)	(0.029)	(0.027)	
Cubic	0.007	0.002	0.006	-0.004	0.002	0.023		0.005	0.012	0.001	0.017	0.057**	0.048*	
	(0.016)	(0.019)	(0.018)	(0.019)	(0.021)	(0.024)		(0.025)	(0.026)	(0.025)	(0.026)	(0.029)	(0.029)	
	C. Math scores (exit exam)							C. Math scores (exit exam)						
2-part linear	-0.016	-0.011	0.003	-0.041	-0.039	-0.036	13,278	0.027	-0.010	0.058	-0.003	-0.020	0.007	11,339
	(0.020)	(0.026)	(0.028)	(0.031)	(0.030)	(0.031)		(0.032)	(0.038)	(0.041)	(0.052)	(0.042)	(0.048)	
None	-0.003	0.008	0.017	-0.017	-0.004	0.013		0.027	0.023	0.048**	0.020	-0.013	0.034	
	(0.013)	(0.015)	(0.016)	(0.018)	(0.018)	(0.018)		(0.020)	(0.022)	(0.023)	(0.028)	(0.027)	(0.028)	
Linear	-0.009	0.006	0.025	-0.038	-0.016	-0.025		0.012	0.014	0.064*	-0.005	0.014	0.014	
	(0.019)	(0.023)	(0.025)	(0.026)	(0.029)	(0.028)		(0.029)	(0.033)	(0.036)	(0.040)	(0.039)	(0.041)	
Quadratic	-0.014	0.009	0.008	-0.024	-0.043	-0.027		0.017	0.013	0.055	-0.009	-0.019	0.006	
	(0.021)	(0.027)	(0.030)	(0.032)	(0.032)	(0.033)		(0.033)	(0.040)	(0.043)	(0.054)	(0.045)	(0.052)	
Cubic	-0.011	0.023	0.022	-0.003	-0.027	-0.018		-0.001	0.037	0.054	-0.013	0.001	0.013	
	(0.024)	(0.029)	(0.032)	(0.033)	(0.035)	(0.036)		(0.036)	(0.043)	(0.047)	(0.054)	(0.048)	(0.053)	
	D. Reading scores (exit exam)							D. Reading scores (exit exam)						
2-part linear	-0.007	0.007	0.019	-0.001	-0.015	0.007	13,279	0.025	0.031	0.066	0.050	-0.012	0.043	11,344
	(0.019)	(0.021)	(0.028)	(0.025)	(0.027)	(0.025)		(0.034)	(0.036)	(0.049)	(0.047)	(0.047)	(0.043)	
None	0.002	0.014	0.036**	0.021	0.009	0.009		0.009	0.041*	0.035	0.050**	0.012	0.029	
	(0.012)	(0.012)	(0.015)	(0.015)	(0.015)	(0.013)		(0.021)	(0.022)	(0.025)	(0.025)	(0.026)	(0.025)	
Linear	-0.010	0.016	0.020	0.004	-0.013	-0.007		-0.005	0.043	0.046	0.048	-0.001	0.039	
	(0.018)	(0.019)	(0.021)	(0.021)	(0.023)	(0.023)		(0.031)	(0.032)	(0.038)	(0.036)	(0.039)	(0.035)	
Quadratic	-0.005	0.025	0.009	0.016	-0.005	0.011		0.024	0.052	0.061	0.070	-0.002	0.035	
	(0.020)	(0.021)	(0.030)	(0.027)	(0.029)	(0.028)		(0.036)	(0.039)	(0.051)	(0.049)	(0.049)	(0.048)	
Cubic	-0.004	0.035	0.006	0.038	0.014	0.015		0.015	0.070*	0.049	0.089*	0.025	0.037	
	(0.023)	(0.024)	(0.029)	(0.027)	(0.030)	(0.030)		(0.039)	(0.042)	(0.053)	(0.048)	(0.051)	(0.048)	

Appendix Table A4 Effect of Bond Passage on Educational Inputs—ITT and Robustness

		Effect of bond passage after						
Bandwidth	polynomial	1 year	2 years	3 years	4 years	5 years	6 years	*n*
		A. Capital outlays per student (all districts)						
all	none	2103***	1598***	-413	-73	-183	-941**	14,455
		(165)	(231)	(265)	(235)	(225)	(452)	
all	2-part linear	2330***	1230***	-735	-415	-579	-723	14,455
		(298)	(394)	(449)	(413)	(361)	(516)	
all	quadratic	2284***	1016***	-801*	-276	-534	-676	14,455
		(326)	(380)	(444)	(433)	(363)	(582)	
0.15 to 0.85	2-part linear	2317***	1121***	-686	-344	-455	-638	12,594
		(322)	(387)	(447)	(431)	(362)	(552)	
0.25 to 0.75	2-part linear	2338***	735	-644	39	-327	-633	9,713
		(356)	(465)	(507)	(495)	(419)	(610)	
0.40 to 0.60	2-part linear	1961***	1635**	523	508	-93	-1318*	4,002
		(571)	(737)	(755)	(799)	(554)	(789)	
		B. Cumulative Capital outlays per student (all districts)						
all	none	2479***	4179***	3610***	3759***	3665***	2556***	10,982
		(169)	(328)	(449)	(528)	(616)	(871)	
all	2-part lin	2595***	3950***	2875***	2578***	2100**	1,514	10,982
		(290)	(546)	(712)	(854)	(1,005)	(1,153)	
all	quadratic	2535***	3662***	2515***	2487***	1934**	1,317	10,982
		(323)	(570)	(671)	(835)	(972)	(1,084)	
0.15 to 0.85	2-part linear	2546***	3826***	2865***	2727***	2296**	1802*	9,479
		(318)	(568)	(685)	(832)	(964)	(1,080)	
0.25 to 0.75	2-part linear	2593***	3335***	2125***	2084**	1,415	576	7,214
		(353)	(658)	(803)	(1,033)	(1,215)	(1,554)	
0.40 to 0.60	2-part linear	2119***	3618***	3334***	2989*	2,181	-88	2,924
		(501)	(927)	(1,173)	(1,594)	(1,812)	(2,256)	
		C. Opened at least one campus						
all	none	0.036	0.113***	0.077***	0.007	0.014	0.040	17,077
		(0.023)	(0.024)	(0.027)	(0.029)	(0.031)	(0.030)	
all	2-part lin	0.001	0.140***	0.078*	0.029	-0.017	0.124**	17,077
		(0.040)	(0.040)	(0.044)	(0.052)	(0.055)	(0.050)	
all	quadratic	-0.002	0.135***	0.032	0.033	-0.013	0.093*	17,077
		(0.042)	(0.043)	(0.045)	(0.056)	(0.057)	(0.054)	
0.15 to 0.85	2-part linear	0.003	0.141***	0.068	0.038	-0.007	0.115**	15,012
		(0.043)	(0.043)	(0.046)	(0.056)	(0.057)	(0.053)	
0.25 to 0.75	2-part linear	0.006	0.158***	0.005	0.032	-0.020	0.092	11,710
		(0.049)	(0.048)	(0.050)	(0.062)	(0.065)	(0.061)	
0.40 to 0.60	2-part linear	-0.061	0.075	-0.079	0.028	-0.122	-0.009	4,896
		(0.066)	(0.071)	(0.075)	(0.091)	(0.093)	(0.093)	
		D. Instructional spending per student						
all	none	14	47	48	9	18	9	14,455
		(27)	(34)	(34)	(35)	(40)	(41)	
all	2-part lin	15	34	-5	22	61	0	14,455
		(44)	(52)	(57)	(60)	(73)	(76)	
all	quadratic	9	68	-23	13	43	-6	14,455
		(46)	(55)	(61)	(62)	(80)	(82)	
0.15 to 0.85	2-part linear	1	45	-1	19	66	-9	12,594
		(47)	(55)	(60)	(63)	(75)	(81)	
0.25 to 0.75	2-part linear	-22	53	5	-25	3	-37	9,713
		(51)	(64)	(68)	(72)	(91)	(91)	
0.40 to 0.60	2-part linear	-15	88	43	30	96	84	4,002
		(78)	(94)	(103)	(106)	(143)	(138)	
		E. Enrollment-weighted average years since school last renovated						
all	none	-3.180***	-3.963***	-3.752***	-5.090***	-5.160***	-1.731	3,191
		(0.533)	(0.777)	(0.984)	(1.130)	(1.378)	(2.015)	
all	2-part lin	-2.929***	-4.341***	-4.108**	-6.589***	-6.389**	-6.746*	3,177
		(1.005)	(1.379)	-1.982	(2.184)	(2.558)	(3.589)	
all	quadratic	-2.435**	-4.003***	-3.545*	-6.404**	-6.737**	-8.551**	3,177
		(1.061)	(1.417)	(2.063)	(2.812)	(3.150)	(4.191)	
0.15 to 0.85	2-part linear	-2.264**	-3.680**	-3.443	-6.040**	-6.549**	-7.089*	2,728
		(1.127)	(1.438)	(2.117)	(2.470)	(2.814)	(3.893)	
0.25 to 0.75	2-part linear	-1.874	-3.483**	-3.444	-6.749**	-7.758**	-9.105*	2,020
		(1.216)	(1.487)	(2.145)	(3.084)	(3.484)	(4.782)	
0.40 to 0.60	2-part linear	0.115	-1.127	-1.842	-8.734	-8.369	-8.608	739
		(1.749)	(2.237)	(3.250)	(6.278)	(6.764)	(9.172)	

NOTE: Each row represents a separate specification and reports effects of bond measure passage on outcomes *t* years later. The sample includes all bond elections and all outcome measures from years -2 to +10 relative to each election in which the vote share falls within the bandwidth. Since some districts hold multiple elections in quick succession, some outcomes appear in the sample multiple times for different relative years. All specifications include fixed effects for each election, 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 X relative year interactions. Standard errors are clustered at the district level. Significance: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Appendix Table A5 Effect of Bond Passage on Test Scores—ITT and Robustness

		Effect of bond passage after						
Bandwidth	polynomial	1 year	2 years	3 years	4 years	5 years	6 years	*n*
		A. Math scores (grades 3-8)						
all	none	0.002	0.000	0.005	-0.004	-0.006	0.016	17,030
		(0.009)	(0.012)	(0.012)	(0.014)	(0.014)	(0.016)	
all	2-part linear	0.010	0.014	0.021	0.004	0.009	0.042	17,030
		(0.013)	(0.018)	(0.020)	(0.022)	(0.025)	(0.027)	
all	quadratic	0.011	0.016	0.018	0.006	0.009	0.039	17,030
		(0.014)	(0.019)	(0.021)	(0.024)	(0.026)	(0.028)	
0.15 to 0.85	2-part linear	0.012	0.016	0.026	0.010	0.014	0.045	14,972
		(0.014)	(0.019)	(0.020)	(0.023)	(0.026)	(0.028)	
0.25 to 0.75	2-part linear	0.010	0.001	-0.009	-0.024	-0.018	0.027	11,679
		(0.015)	(0.021)	(0.023)	(0.026)	(0.029)	(0.032)	
0.40 to 0.60	2-part linear	-0.013	-0.014	-0.027	-0.039	-0.071	-0.038	4,884
		(0.026)	(0.033)	(0.036)	(0.040)	(0.048)	(0.053)	
		B. Reading scores (grades 3-8)						
all	none	-0.003	-0.003	0.001	-0.010	-0.011	0.003	17,030
		(0.007)	(0.008)	(0.009)	(0.009)	(0.010)	(0.011)	
all	2-part linear	0.010	0.006	0.015	-0.002	0.000	0.018	17,030
		(0.010)	(0.014)	(0.014)	(0.015)	(0.018)	(0.019)	
all	quadratic	0.013	0.009	0.015	-0.001	0.000	0.017	17,030
		(0.010)	(0.015)	(0.015)	(0.016)	(0.019)	(0.020)	
0.15 to 0.85	2-part linear	0.007	0.004	0.017	-0.002	-0.002	0.013	14,972
		(0.010)	(0.014)	(0.015)	(0.015)	(0.018)	(0.020)	
0.25 to 0.75	2-part linear	0.011	0.001	0.004	-0.014	-0.015	0.005	11,679
		(0.012)	(0.016)	(0.016)	(0.017)	(0.020)	(0.023)	
0.40 to 0.60	2-part linear	0.001	-0.001	-0.009	-0.027	-0.040	-0.037	4,884
		(0.019)	(0.026)	(0.023)	(0.025)	(0.029)	(0.036)	
		C. Math scores (exit exam)						
all	none	-0.011	-0.005	0.003	-0.031	-0.016	0.005	15,990
		(0.012)	(0.015)	(0.017)	(0.020)	(0.019)	(0.020)	
all	2-part linear	-0.015	-0.008	0.009	-0.039	-0.021	-0.015	15,990
		(0.022)	(0.028)	(0.030)	(0.034)	(0.031)	(0.033)	
all	quadratic	-0.007	0.021	0.024	-0.015	-0.016	-0.001	15,990
		(0.023)	(0.029)	(0.032)	(0.035)	(0.033)	(0.035)	
0.15 to 0.85	2-part linear	-0.018	-0.007	0.007	-0.028	-0.033	-0.019	14,068
		(0.023)	(0.027)	(0.032)	(0.034)	(0.032)	(0.032)	
0.25 to 0.75	2-part linear	-0.032	-0.022	0.018	-0.018	-0.023	-0.024	10,988
		(0.025)	(0.030)	(0.035)	(0.039)	(0.034)	(0.035)	
0.40 to 0.60	2-part linear	-0.066*	-0.011	-0.011	0.002	-0.051	-0.019	4,546
		(0.038)	(0.045)	(0.050)	(0.055)	(0.051)	(0.049)	
		D. Reading scores (exit exam)						
all	none	-0.003	0.008	0.028*	0.012	-0.001	-0.002	15,991
		(0.011)	(0.013)	(0.016)	(0.016)	(0.016)	(0.016)	
all	2-part linear	-0.006	0.014	0.028	0.004	0.001	0.019	15,991
		(0.019)	(0.022)	(0.028)	(0.024)	(0.026)	(0.028)	
all	quadratic	-0.002	0.033	0.021	0.022	0.012	0.022	15,991
		(0.021)	(0.024)	(0.030)	(0.026)	(0.028)	(0.031)	
0.15 to 0.85	2-part linear	-0.010	0.016	0.025	0.027	0.004	0.012	14,069
		(0.021)	(0.022)	(0.029)	(0.025)	(0.027)	(0.027)	
0.25 to 0.75	2-part linear	-0.032	0.007	0.018	0.024	0.003	0.006	10,989
		(0.022)	(0.025)	(0.031)	(0.027)	(0.028)	(0.031)	
0.40 to 0.60	2-part linear	-0.034	0.014	-0.038	0.045	-0.003	0.007	4,547
		(0.037)	(0.038)	(0.044)	(0.039)	(0.040)	(0.045)	

NOTE: Each row represents a separate specification and reports effects of bond measure passage on outcomes *t* years later. The sample includes all bond elections and all outcome measures from years -2 to +10 relative to each election in which the vote share falls within the bandwidth. Since some districts hold multiple elections in quick succession, some outcomes appear in the sample multiple times for different relative years. All specifications include fixed effects for each election, 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 X relative year interactions. Standard errors are clustered at the district level. Significance: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Appendix Table A6 Effect of Bond Passage on Test Scores—ITT and Robustness for Economically Disadvantaged Students

		Effect of bond passage after						
Bandwidth	polynomial	1 year	2 years	3 years	4 years	5 years	6 years	*n*
				A. Math scores (grades 3-8)				
all	none	0.013	-0.001	0.009	0.011	-0.001	0.031	16,314
		(0.014)	(0.016)	(0.016)	(0.018)	(0.020)	(0.020)	
all	2-part linear	0.013	0.030	0.010	0.013	0.032	0.069**	16,314
		(0.021)	(0.025)	(0.026)	(0.030)	(0.035)	(0.034)	
all	quadratic	0.010	0.029	0.008	0.015	0.025	0.059	16,314
		(0.022)	(0.025)	(0.027)	(0.032)	(0.037)	(0.036)	
0.15 to 0.85	2-part linear	0.019	0.036	0.017	0.021	0.040	0.072**	14,427
		(0.022)	(0.025)	(0.026)	(0.031)	(0.036)	(0.035)	
0.25 to 0.75	2-part linear	0.014	0.025	-0.015	-0.018	0.005	0.049	11,270
		(0.023)	(0.027)	(0.030)	(0.034)	(0.041)	(0.040)	
0.40 to 0.60	2-part linear	-0.021	0.020	-0.015	-0.054	-0.025	0.012	4,734
		(0.036)	(0.043)	(0.045)	(0.054)	(0.065)	(0.067)	
				B. Reading scores (grades 3-8)				
all	none	0.001	-0.008	0.005	0.003	-0.005	0.013	16,314
		(0.013)	(0.013)	(0.014)	(0.014)	(0.016)	(0.016)	
all	2-part linear	0.020	0.018	0.001	0.009	0.037	0.041	16,314
		(0.021)	(0.023)	(0.023)	(0.024)	(0.028)	(0.027)	
all	quadratic	0.018	0.018	0.000	0.014	0.036	0.038	16,314
		(0.020)	(0.024)	(0.024)	(0.026)	(0.029)	(0.028)	
0.15 to 0.85	2-part linear	0.017	0.014	0.002	0.011	0.039	0.033	14,427
		(0.021)	(0.024)	(0.024)	(0.025)	(0.029)	(0.027)	
0.25 to 0.75	2-part linear	0.020	0.013	-0.001	-0.003	0.032	0.026	11,270
		(0.023)	(0.026)	(0.027)	(0.028)	(0.033)	(0.031)	
0.40 to 0.60	2-part linear	0.018	0.020	0.024	-0.030	0.007	-0.010	4,734
		(0.036)	(0.040)	(0.036)	(0.041)	(0.046)	(0.049)	
				C. Math scores (exit exam)				
all	none	0.019	0.006	0.030	0.010	-0.023	0.024	13,992
		(0.022)	(0.025)	(0.026)	(0.031)	(0.030)	(0.031)	
all	2-part linear	0.013	-0.018	0.033	-0.025	-0.026	-0.013	13,992
		(0.038)	(0.043)	(0.045)	(0.057)	(0.050)	(0.054)	
all	quadratic	0.006	0.008	0.036	-0.024	-0.021	-0.009	13,992
		(0.040)	(0.045)	(0.047)	(0.060)	(0.053)	(0.059)	
0.15 to 0.85	2-part linear	-0.017	-0.014	0.019	-0.029	-0.027	-0.027	12,414
		(0.040)	(0.043)	(0.046)	(0.059)	(0.052)	(0.053)	
0.25 to 0.75	2-part linear	-0.014	-0.002	0.008	-0.033	-0.032	-0.022	9,726
		(0.043)	(0.048)	(0.050)	(0.065)	(0.057)	(0.060)	
0.40 to 0.60	2-part linear	-0.097	0.019	-0.048	-0.108	-0.099	-0.102	4,058
		(0.067)	(0.071)	(0.074)	(0.097)	(0.082)	(0.090)	
				D. Reading scores (exit exam)				
all	none	0.010	0.033	0.023	0.039	-0.001	0.011	13,998
		(0.022)	(0.023)	(0.027)	(0.028)	(0.030)	(0.030)	
all	2-part linear	0.039	0.048	0.082*	0.050	-0.004	0.023	13,998
		(0.037)	(0.041)	(0.048)	(0.048)	(0.050)	(0.049)	
all	quadratic	0.043	0.074*	0.085*	0.077	0.017	0.021	13,998
		(0.039)	(0.044)	(0.051)	(0.051)	(0.052)	(0.055)	
0.15 to 0.85	2-part linear	0.028	0.059	0.084*	0.084*	0.008	0.007	12,418
		(0.039)	(0.043)	(0.051)	(0.049)	(0.052)	(0.051)	
0.25 to 0.75	2-part linear	0.049	0.050	0.064	0.094*	0.014	0.013	9,730
		(0.042)	(0.049)	(0.058)	(0.054)	(0.059)	(0.060)	
0.40 to 0.60	2-part linear	0.037	0.073	0.021	0.053	-0.001	-0.018	4,060
		(0.070)	(0.074)	(0.094)	(0.084)	(0.090)	(0.087)	

NOTE: Each row represents a separate specification and reports effects of bond measure passage on outcomes *t* years later. The sample includes all bond elections and all outcome measures from years -2 to +10 relative to each election in which the vote share falls within the bandwidth. Since some districts hold multiple elections in quick succession, some outcomes appear in the sample multiple times for different relative years. All specifications include fixed effects for each election, 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 X relative year interactions. Standard errors are clustered at the district level. Significance: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Appendix Table A7 Effect of Bond Passage on Facility Condition, by Age of Facility

Panel A. Overall Building Condition and Age

	At least fair condition		At least good condition		Effective age		log(Maintenance needs)	
Pass	0.0621	-0.0327	0.104	-0.00899	-3.456	1.371	-0.0287	0.569
	(0.0525)	(0.0324)	(0.0890)	(0.0799)	(2.1020)	(2.0970)	(0.4410)	(0.4430)
Old		-0.245***		-0.520***		10.43***		1.849***
		(0.0647)		(0.0696)		(2.5360)		(0.3020)
PassXOld		0.180***		0.235***		-8.169***		-0.983***
		(0.0674)		(0.0792)		(2.7460)		(0.3400)
Constant	0.855***	1.001***	0.570***	0.877***	11.92***	5.318***	5.522***	4.345***
	(0.0424)	(0.0088)	(0.0580)	(0.0491)	(1.7170)	(1.0440)	(0.2770)	(0.2760)
Vote share	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Observations	2,873	2,855	2,873	2,855	2,562	2,556	2,507	2,500
R-squared	0.03	0.083	0.024	0.157	0.025	0.076	0.015	0.104
Pass + Pass X Old = 0 (*p*-val)		0.0448		0.0175		0.007		0.34

Panel B. Building Capacity and Overcrowding

	Enrollment/capacity		Fraction of sq ft in portables		Sq ft per student	
Pass	-0.0122	0.0575	-0.00848	0.00766	-3.55	-14.87
	(0.0494)	(0.0570)	(0.0141)	(0.0214)	(17.58)	(16.89)
Old		0.0768**		0.0341		4.961
		(0.0363)		(0.0263)		(12.9600)
PassXOld		-0.111**		-0.0299		16.8
		(0.0468)		(0.0275)		(17.3400)
Constant	0.862***	0.813***	0.0537***	0.0331*	167.5***	165.2***
	(0.0283)	(0.0372)	(0.0124)	(0.0189)	(9.0220)	(8.5160)
Vote share	Linear	Linear	Linear	Linear	Linear	Linear
Observations	2,855	2,844	2,822	2,809	2,852	2,840
R-squared	0.001	0.006	0.006	0.012	0.001	0.008
Pass + Pass X Old = 0 (*p*-val)		0.286		0.199		0.927

NOTE: "Old" is an indicator for whether the facility is 25 years or older. Bond passage and vote share from the first election held prior to 2006 are used for school districts that held multiple bond elections in our analysis window. Standard errors are clustered at the school district level. Observations are weighted by the inverse of the total number of schools in the district, so that each district receives a weight of 1 in the regression. Most regressions include data from 204 unique school districts. Significance: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Appendix Table A8 Effect of Bond Passage on Educational Inputs and Test Scores, Heterogeneity by District and Bond Characteristics (TOT, Two-part linear specification with district fixed effects.)

	Implied effect of bond passage after year						Implied effect of bond passage after year					
	1	2	3	4	5	6	1	2	3	4	5	6
A. By Bond Amount	Bond amount at 25th percentile ($4,250 per student)						Bond amount at 75th percentile ($12,540 per student)					
Cumulative capital outlays per student	1754***	2436***	2753**	2958***	4377***	4067**	3635***	6468***	6557***	6686***	6883***	6093***
	(434)	(822)	(1,117)	(1,099)	(1,248)	(1,608)	(386)	(659)	(977)	(1,083)	(1,063)	(1,292)
Enrollment-weighted average age of school buildings	-0.081	-0.181	-0.893	-0.584	0.245	0.571	-0.615	-0.627	-1.519**	-0.946	-0.304	-0.276
	(0.618)	(0.737)	(0.800)	(0.943)	(1.008)	(1.027)	(0.521)	(0.638)	(0.719)	(0.879)	(0.978)	(1.061)
Standardized math scores (grades 3-8)	-0.001	-0.006	-0.004	-0.018	0.000	0.022	-0.002	0.003	0.008	-0.015	-0.007	0.025
	(0.019)	(0.022)	(0.024)	(0.027)	(0.027)	(0.028)	(0.018)	(0.021)	(0.022)	(0.024)	(0.027)	(0.027)
B. By District Socioeconomic Status	Non-poor Districts						Economically-disadvantaged Districts					
Cumulative capital outlays per student	3637***	5876***	5872***	5865***	6317***	5414***	2512***	4746***	4547***	4588***	5091***	4524***
	(435)	(706)	(872)	(1,001)	(1,177)	(1,430)	(429)	(746)	(941)	(1,029)	(1,168)	(1,378)
Enrollment-weighted average age of school buildings	-0.826	-0.961	-1.780**	-1.347	-0.667	-0.558	-0.086	0.079	-0.716	-0.067	0.853	0.816
	(0.558)	(0.662)	(0.742)	(0.879)	(0.954)	(1.015)	(0.542)	(0.653)	(0.747)	(0.930)	(1.013)	(1.062)
Standardized math scores (grades 3-8)	0.002	0.005	0.018	-0.002	0.016	0.051*	-0.01	-0.017	-0.02	-0.044*	-0.041	-0.011
	(0.018)	(0.021)	(0.022)	(0.023)	(0.026)	(0.027)	(0.020)	(0.023)	(0.024)	(0.026)	(0.028)	(0.028)
C. By Initial Condition of Buildings in District	Districts with campuses in better condition						Districts with campuses in worse condition					
Cumulative capital outlays per student	3175***	5502***	5198***	5158***	5567***	4504***	3119***	5523***	5231***	5409***	5810***	5316***
	(422)	(677)	(878)	(996)	(1,165)	(1,397)	(456)	(753)	(939)	(1,061)	(1,240)	(1,488)
Enrollment-weighted average age of school buildings	-0.535	-0.553	-1.355*	-0.806	-0.100	-0.090	-0.615	-0.620	-1.400*	-0.787	0.095	0.162
	(0.563)	(0.678)	(0.756)	(0.912)	(0.983)	(1.034)	(0.553)	(0.652)	(0.744)	(0.902)	(1.004)	(1.097)
Standardized math scores (grades 3-8)	0.005	0.011	0.013	-0.007	0.004	0.035	0.004	-0.001	0.003	-0.02	-0.012	0.020
	(0.018)	(0.021)	(0.023)	(0.025)	(0.026)	(0.027)	(0.019)	(0.021)	(0.024)	(0.025)	(0.028)	(0.028)
D. By Initial Age of Buildings in District	Districts with newer campuses						Districts with older campuses					
Cumulative capital outlays per student	2971***	5031***	4581***	4564***	4995***	4249***	3502***	6121***	5996***	6178***	6685***	6035***
	(434)	(708)	(903)	(1,009)	(1,163)	(1,384)	(435)	(722)	(924)	(1,061)	(1,213)	(1,447)
Enrollment-weighted average age of school buildings	-0.962*	-1.062	-1.707**	-1.190	-0.534	-0.385	0.313	0.242	-0.748	-0.123	0.940	0.876
	(0.541)	(0.646)	(0.728)	(0.880)	(0.955)	(1.028)	(0.578)	(0.689)	(0.780)	(0.931)	(1.006)	(1.059)
Standardized math scores (grades 3-8)	-0.001	0.002	0.009	-0.006	0.003	0.039	-0.005	-0.005	-0.005	-0.037	-0.026	0.007
	(0.018)	(0.022)	(0.022)	(0.024)	(0.026)	(0.027)	(0.020)	(0.022)	(0.025)	(0.026)	(0.028)	(0.028)

NOTE: Each row represents a separate specification and reports effects of bond measure passage on outcomes *t* years later. The sample includes panel data from 1994 to 2011 for all 812 districts that held bond elections. Sample for panels C and D restricted to 805 districts for which baseline campus age and condition is available. Model includes indicators for bond passage, holding election, and vote share (with different slopes for passing and nonpassing bond) in the current year and each previous year up to 10. Model also includes interaction between bond passage (in current and each previous year) and the baseline characteristic reported (e.g., bond amount, district with campus worse than median, etc.). The table reports the implied (bond passage) + (characteristic) X (bond passage) coefficient for each characteristic group and for each lag. Estimates for current period and lags greater than 6 are not displayed. Standard errors are clustered at the district level. Significance: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Appendix Table A9 Baseline District Correlates of School Bond Size

	Dept variable: Bond amount per student at baseline (000s) (mean = 9.78)		
	(1)	(2)	(3)
Total enrollment (000s) (mean = 10.94)	-0.045**	-0.045**	-0.038**
	(0.018)	(0.018)	(0.016)
Fraction of students eligible for free lunch (mean = 49)	-0.103***	-0.103***	-0.110***
	(0.016)	(0.016)	(0.016)
Student-teacher ratio - overall (mean = 14.1)	-0.043	-0.044	-0.029
	(0.054)	(0.055)	(0.046)
Instructional spending per student (000s) (mean = 5.00)	2.600***	2.586***	2.345***
	(0.674)	(0.679)	(0.704)
Capital outlay per student (000s) (mean = 1.36)	1.509*	1.516*	1.558*
	(0.788)	(0.792)	(0.797)
Enrollment-weighted average age of school buildings (mean = 32.36)			0.056**
			(0.027)
Constant	0.693	0.788	0.046
	(3.496)	(3.523)	(3.387)
Observations	1,737	1,723	1,723
R-squared	0.159	0.159	0.162
Sample	All elections	Elections with campus age	

NOTE: Sample in specifications (2) and (3) includes all bond elections held by districts for which average campus age in year of election was available, which is 805 districts. District characteristics are averaged in the 2 years prior and year of election. Standard errors are clustered at the school district level. Significance: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

APPENDIX B: DESCRIPTION OF SCHOOL OPENING EVENT STUDY ANALYSIS

To identify school openings, we first restrict our sample of campuses to those contained in the 2006 Facilities Survey (approximately 300 different districts) and also to the years 1996 to 2006. Since schools may change identification numbers for various reasons, we identify "clean" school openings where the year the facility was built according to the 2006 survey is within two years of the first year the campus ID appears in the AEIS data. This resulted in 380 campuses opened between 1996 and 2006 with third–eighth graders. Fifty percent of campuses with new campus ID numbers were deemed to be "clean" openings, representing more than 70 percent of enrollment in schools with new campus ID numbers.

Identifying Counterfactual Schools

The event study analysis of school openings is complicated by the fact that the newly-constructed schools by definition have no preconstruction data. Our solution to this problem is to use data on feeder patterns to identify a "counterfactual school" that students would have attended had the newly built school not been built. The basic idea is to see which feeder schools contribute enrollment to the newly built school in the first year it opens, and then use data from the year(s) before the construction to see where students in the feeder schools attended before the newly built school was constructed.

To see how we implemented this approach, we began by identifying the F schools that students attending a school that opened in year t attended in year $t-1$. We then computed the share of students in the new school who came from each feeder school i, denoted by a_i. Next, we examined the transition patterns between the newly built school's feeder-schools and existing schools between $t-2$ and $t-1$ (i.e., prior to the new school opening). We chose the counterfactual school from the set of schools receiving students in $t-1$ who attended the newly built school's feeder schools in $t-2$. First, we calculated the share of students in $t-1$ who attended feeder school i in $t-2$ that attended receiving school j in $t-1$ (denoted by λ_j^i). Using these shares, we computed $w_j = \sum_i^F \lambda_j^i a_i$, which is the share of students who attended the feeder schools that contribute to the newly built school's enrollment in year t and who attend receiving school j in $t-1$. We selected the receiving school with the largest value of w_j to be the newly built school's counterfactual school. After identifying the matched schools, we formed the grouping between the new and matched existing school. In the event study models for school openings, we control for school-group fixed effects as explained in the main text.

In practice, we could not identify suitable counterfactual matches for all newly opened schools. One reason is because a new school's best match was not unique, meaning that there was another new school that shared the same counterfactual school. Another reason was because a school's best match was itself a school that opened during our study period. Furthermore, some opened campuses or matched counterfactual schools were dropped because their students were missing prior-year test scores. After excluding these problematic cases, we were left with 258 school openings.

Even for new schools for which we found a match, the best match may not be very good if feeder patterns are very diffuse. We used two variables to assess the quality of a match. The first is the percentage change in the total sample size in the school group in the year the school opened. If this change is large, it would suggest that there could be large compositional changes in the school group in the year before the opening, when the school group consisted only of the counterfactual match school, and the year of the opening. The second is the value of w_j for the matched school. Low values of w_j suggest that, had the new school not been built, the students attending the new school would likely have been spread evenly across a number of existing schools rather than being concentrated in one existing school. In these cases, there is less reason to think that the preopening achievement patterns in the school group are informative about the preopening achievement patterns of students who would have attended the new school had it been in existence. To assess the robustness of our results, we estimate models where we exclude openings where the sample size in the school group changed by more than 25 percent (leaving 95 openings) and another set of models where we only use openings where the value of w_j for the best match is at least 0.75 (leaving 48 openings).