Investing in Schools: Capital Spending, Facility Conditions, and Student Achievement

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Abstract

Public infrastructure investments in repairs, modernization, and construction of schools cost billions. However, little is known about the nature of school facility investments, how funding is allocated within school districts, whether it actually changes the physical condition of public schools, and the subsequent causal impacts on student achievement. We study achievement effects of nearly 1,400 capital campaigns initiated and financed by local school districts. To overcome the concern that school districts conducting campaigns are different from districts that do not, we exploit the fact that the bonds used to finance the campaigns require voter approval and compare districts where bond referenda in close elections narrowly pass and fail. Overall, we find little evidence that school capital campaigns improve student achievement. Further exploration reveals that although capital campaigns lead to school openings and physical improvements in school environments, the change in conditions experienced by the typical student is modest. Thus, U.S. school capital campaigns financed by local districts, the pre-dominant method through which facility investments are made, may be a limited tool for realizing substantial gains in student achievement or closing achievement gaps.

1. Introduction

The Coleman Report (1966) ignited an enduring debate on the importance of school spending by concluding that school resources play a limited role in improving student outcomes. Many empirical studies followed with some arguing that there is no systematic relationship between school resources and student outcomes (Hanushek, 1986) while numerous suggest otherwise (Greenwald, Hedges and Laine, 1996; Card and Krueger, 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 on public school facilities, with annual expenditures totaling about $66 billion (or $1344 per student; NCES, 2011).[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), with $300 billion in deferred maintenance needed to bring U.S. schools into "good" condition (ASCE, 2009). The prevalence of public schools in need of repair is worrisome because poor physical environments may impede student achievement if students learn more easily in safe, clean, controlled environments (Jones and Zimmer, 2001).[2]

Indeed, recent evidence demonstrates that new school construction projects can improve student outcomes (Duflo, 2001; Aaronson and Mazumder, 2011; Neilson and Zimmerman, 2014). These studies examined the impacts of very large construction projects financed with funds provided largely outside of the affected communities and in contexts where initial conditions of school facilities were either very poor or non-existent. For instance, Neilson and Zimmerman (2014) study a construction project that involved rebuilding almost every school campus in an urban district (New Haven, CT), which includes among the oldest buildings in the state. The project cost $70,000 per pupil and was chiefly financed through state and federal sources (Neilson and Zimmerman, 2014).[3]

What remains unclear is whether typical capital campaigns made and financed by local school districts themselves also generate appreciable benefits to students. In the U.S., school capital projects

[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 (U.S. Census Bureau, 2012).

[2] Many reasons are proposed for why physical environments could affect student outcomes. Crowded and uncomfortable conditions could dampen student morale and effort (Uline and Tschannen-Moran, 2008). In particular, inadequate lighting and climate control, chronic noise, poor indoor air quality, and too little physical space could all make it difficult for students to concentrate (Earthman, 2002; Earthman and Lemasters, 1996, 1998; Higgins et al., 2005; Schneider, 2002). Lower quality buildings could also increase student absenteeism (Schneider, 2002), particularly if they cause or exacerbate health conditions such as asthma (New York State Department of Health, 2008; Lamb, 2009). The same factors that affect students' ability to concentrate and learn could also diminish teacher morale and effectiveness, and reduce teacher retention (Buckley et al 2004).

[3] The cost of renovations to existing schools was of similar magnitude as that of new construction and only 23% of it was paid for with local funds, including City of New Haven general funds.

(both renovations and new construction) are primarily financed locally through the issuance of voter-approved bonds that are repaid with property taxes.[4] The per-pupil size of the typical capital campaign in Texas, the state we study in this paper, is about $7,500, an order of magnitude smaller than the New Haven project (Neilson and Zimmerman, 2014).[5] Although Cellini, Ferreira and Rothstein (2010; henceforth CFR) find that school bond passage in California increases housing prices, suggesting that districts underinvest in school facilities, they find modest but very imprecisely estimated effects on student achievement. The confidence intervals include both very large and zero effects of bond passage on student achievement. Thus, achievement effects remain unclear.

In this paper we provide the most comprehensive assessment of achievement effects from capital campaigns initiated and financed by local school districts. In doing so, we study 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 (CFR, 2010) to compare school districts where bond referenda narrowly pass to those that narrowly fail. We examine the impact of capital campaigns on student outcomes using information on all tested students in the state over this time period, which includes all 3rd through 8th graders and 10th or 11th graders that take the state's high school exit exam.[6] Access to student microdata allows us to examine effects for student subgroups, effects on achievement gaps, and mechanisms such as improved school attendance. Importantly, the microdata allow us to discern whether capital campaigns encourage in-district student migration and whether quality facilities help districts retain experienced teachers. In addition, we use a difference-in-differences approach to examine differential changes between districts with successful and unsuccessful capital campaigns. This approach produces more precise estimates, albeit with stronger identifying assumptions.

We find clear evidence that locally-funded campaigns lead to large increases in capital investment that are concentrated in the first two post-election years. Crucially, we find no evidence of any effects on operating spending or on average class size, suggesting that funds raised through bonds "stick" to the capital account and are not reallocated to operating costs. We also find little evidence that capital campaigns attract students into school districts or help local districts retain teachers.

For student achievement, we generally find little evidence that school capital campaigns generate better student outcomes. Our point estimates for grades 3 to 8 are a small 0.016 and 0.030 standard

[4] In the U.S., 88% of funding for capital investment comes from local school districts.
[5] The school bond size in Texas is not unusually small. For example, the California school bonds examined by Cellini, Ferreira, and Rothstein (2010) were about $6,300 per-pupil.
[6] In contrast, CFR must construct a sporadic panel of test scores spanning several different tests for third and fourth graders.

deviation increase for reading and math, respectively, in year six (p-values = 0.438, 0.269).[7] The 95 percent confidence intervals exclude effects as large as 0.06 and 0.08. For our discontinuity-based estimates, we find some evidence that capital campaigns increase test scores for poor students by 0.052 and 0.069 standard deviations for reading and math after six years (p-values = 0.041, 0.027) but discount these findings, as they are sensitive to alternative specifications of our empirical model. We find no effect of capital campaigns on non-poor students or on older students taking high school exit exams.

These results may seem surprising given the findings of recent work (e.g., Neilson and Zimmerman, 2014) that suggests large-scale capital campaigns can have important impacts on student achievement. One possible explanation is that the capital campaigns we examine do not generate meaningful improvements in the school facilities students experience. We evaluate this possibility by examining the impact of the capital campaigns on several measures of the school facility quality students face. To our knowledge, this analysis is the first to look at the causal effect of bond-funded capital campaigns on the actual schooling environments of students. In general, we find that locally financed capital campaigns lead to measurable, yet remarkably modest changes in school facility conditions. Three years after bond passage, average district-wide campus age decreases by merely 2.74 years (an 8 percent reduction), time since last major renovation or building construction decreases by 5.39 years (a reduction of 39 percent), 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. However, the overall district-level impacts are modest and not statistically significant. Our results demonstrate that the incremental capital spending is used largely for educational purposes but typical campaigns only generate modest improvements in the condition of facilities for which a typical student is exposed. Most students simply do not attend newer schools following district-financed capital campaigns.

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. Yet if districts are able to increase the scale of capital campaigns, exposing more students to substantially better conditions, might achievement effects increase proportionately? We offer two pieces of evidence on this question. First, we note that our overall test score estimates, though insignificant, are similar to those associated with $10,000 of school construction spending in the New Haven campaign (Neilson and Zimmerman,

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

2014). While this raises the possibility that effects of school bond passage might have larger effects if scaled up, we actually find no relationship between bond size and the estimated impact on achievement. We show that this relationship appears to be driven by a lack of association between bond size and pre-existing building conditions, as we find little evidence that the size of the capital campaign mediates the effect of bond passage on several measures of facility quality. Altogether, our findings suggest that the districts that are in need of large-scale building projects are not able to finance such initiatives through school bonds, the standard method for raising capital investments for school facilities in the United States. An external funding mechanism for school facilities – such as including capital spending in school finance equalization plans or direct school investments by states and the federal government (as under the American Recovery and Reinvestment Act of 2009) – might be necessary to address the inadequate conditions of many U.S. schools.

The paper proceeds as follows. The next section describes the context of facilities funding in Texas and its implications for student outcomes. Sections 3 and 4 describe our data sources and method and presents evidence on the validity of our research design. Section 5 describes how school district spending changes following successful bond passage. Our main findings about the effect of capital campaigns on student achievement are contained in Section 6. Section 7 quantifies how the physical school setting experienced by the typical student is affected by spending and Section 8 discusses and interprets our results. We conclude in Section 9.

2. School Facility Spending in Texas and Its Potential Effects on Student Outcomes

In 2008, total funding for Texas public schools was $10,600 per student, of which $1,280 (12 percent) was spent on school facilities. The vast majority of these funds are raised internally by local school districts. State property tax revenue and federal funding each account for about 10 percent of facility spending, with the remainder coming from districts (U.S. DoEd, 2010; Table 181; Filardo et al., 2010).[8] 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.

[8] 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.

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 vote to issue school bonds and the associated, concurrent increase in property taxes. An example of a ballot proposition for one Texas school capital campaign is for Ector school district:

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

The language is typical of school ballot propositions calling for bond financing for a capital campaign to construct and renovate schools but also calls for providing funds for land acquisition and purchase of new school 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 (U.S. Census Bureau, 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.[9] A 1990 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" (TEA, 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

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

construction over the next 5 years, with critically needed repairs costing $4.1 billion (TCPA, 1998). Needs tended to be greater in heavily minority districts. In a 2006 survey, 6 percent of districts reported that their instructional facilities were in "poor" condition or warranted replacement (TCPA, 2006). Also, a substantially higher rate of instructional portable space was reported in use in districts with many economically disadvantaged students. In summary, although the Texas school financing system helps equalize operational spending across districts, wide disparities in facilities conditions and capital investments remain.[10]

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 that are too small 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, 2004). Substandard facilities may thus result in chronic distractions and missed school days (Earthman, 2002). Older schools, which have not been renovated or building systems not retrofitted, may not have the infrastructure to support the latest technology (Lyons, 1999) or could lack modernized labs for science education. Low-quality educational facilities could dampen enthusiasm and effort on the part of teachers (Uline and Tschannen-Moran, 2008), thereby affecting teacher retention, which could in turn affect student performance (Buckley, Schneider, and Shang, 2004; Ingersoll, 2001; Loeb, Darling-Hammond, and Luczak, 2005). Consistent with these claims, student achievement has been shown positively associated with district-level capital spending (Crampton, 2009; Jones and Zimmer, 2001). The analysis in this paper will shed light on whether this association reflects a causal relationship.

3. Data Sources and Summary Statistics

Our analysis draws on four sources of data at the student, district, and campus levels, which are then aggregated to the district-year level.

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

[10] It is difficult to directly compare conditions in Texas with those in other states. However, a few national surveys suggest that Texas school facilities are roughly comparable to those across the country. A 1999 survey of 903 public schools found the average age of instructional buildings was 40 years with a functional age of 16 years. Older schools were more likely to report unsatisfactory conditions (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 functional age of 9 years.

school districts from 1997 to 2010.[11] We collected vote share data from 812 school districts (98% of districts holding elections) along with supporting documentation via public information requests. Whenever there were multiple propositions considered during the same academic year, we used the characteristics (size, vote share, result) for the largest proposition (by bond amount) as our "focal" election for that district in that year.[12] 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% of these bond measures, with an average vote share of 64%. The mean (median) bond amount was $11,086 ($7,756) per student (in $2010).

District- and campus-level longitudinal data. From Texas Education Agency's (TEA) Academic Excellence Indicator System (AEIS) data system, we measure the number of campuses by type (elementary, middle, secondary, both), number of schools opening/closing by type, student-teacher ratio by campus type, and average student demographics for 1994 to 2011. We also construct the share of enrollment in new schools (opened in the past year or four years) annually. Annual data on expenditures per student at the district-level was obtained from the Common Core Data.[13]

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 all campuses in several hundred districts in 1991 and 2006. The 1991 data come from a facilities engineering assessment of all public school buildings commissioned by TEA. We have successfully digitized this data for all campuses contained in 228 districts, 181 of which held bond elections during our analysis window. The 2006 data come from a voluntary survey conducted by the Texas Comptroller of Public Accounts with responses from 302 districts, including 3548 instructional facilities (accounting for about half of the state's student population). The 1991 and 2006 data was combined with AEIS data on school openings to calculate the building age and time since last renovation for each campus in each year, which is then aggregated to the district-level.[14] The 2006

[11] 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.
[12] 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.
[13] 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
[14] Campus age is available for all years for the 228 digitized districts (181 that held bond elections), but time since last renovation is only available through 2006 as we do not have information on renovations occurring since 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 the 2006 survey.

survey also includes information about the general condition (Excellent, Good, Fair, Poor, needs replacement), square footage, number and square footage of portable buildings, and total student capacity.

Student achievement, attendance, migration. Our primary outcomes are standardized test scores and attendance records from student microdata for all 3rd through 8th graders tested from 1994 to 2011 and high school exit exam scores for the same period.[15] We focus on reading and mathematics scores for students in grade 3 to 8 and high school exit exam scores for these two subjects, as these are available for the entire study period. Exit exams are typically taken in the 10th or 11th grade. Since the tests are not comparable across grades within a year and since there were changes in the tests used over time, we standardize raw scores in the micro data by grade and year. To examine attendance, we calculate the fraction of days each student is in attendance in each academic year. Microdata are aggregated to district-year means (overall and for various subgroups) and deciles to assess how the full distribution of outcomes is altered by bond passage and subsequent capital investment.[16] We also use the micro data to calculate the share of students (2nd through 12th grade) that are new to the district in each year.

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

4. Empirical Strategy

Our regression-discontinuity research design is motivated by concern that school districts that carry out capital campaigns might be different from those that do not in unobserved ways that are related to the student outcomes of interest. For example, districts that have capital campaigns may serve higher-income families. Because family income is a strong predictor of student success, simple

[15] The student-level data come from the administrative records of the University of Texas at Dallas' Texas Schools Project database.

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

comparisons between districts that do and do not have such campaigns would be uninformative about the causal impact of capital spending.

We sidestep this concern by exploiting variation in capital spending generated from close school bond elections. Although on average, districts in which a bond measure passes are likely to be different from districts where bond measures fail, these differences shrink as comparisons focus on close elections. Lee (2008) notes that as long as there is some uncertainty in vote share in favor of bond passage, then the outcome of a close election is "as good as" random. (We show evidence on the validity of this assumption in a later section.) We can therefore attribute outcome differences between students who live in districts that narrowly pass and fail to post-election variation in capital spending.

A. Regression Discontinuity with Panel Data

Our regression-discontinuity analysis is complicated by the dynamic nature of bond passage, because a district that holds a bond election that barely fails may hold a new election later that passes. We therefore use dynamic regression-discontinuity methods developed by Cellini, Ferreira, and Rothstein (2010) to address these concerns. For our model, suppose that outcome *Y* (such as student test scores) is observed τ years after a bond election was held in district *j* in year *t*. A model for the effect of bond passage is given by:

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

where $Pass_{j,t}$ is an indicator for whether the bond measure passed and f is a flexible function of the vote share $v_{j,t}$ that is permitted to differ with time since bond passage. Other factors influencing the outcome is captured by $\varepsilon_{j,t+\tau}$. 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). Thus, we can examine the possibility that bond passage might not have immediate effects on student outcomes or that the effects might eventually fade.

Following CFR (2010), we first estimate (1) on a panel dataset constructed in the following way. First, for each district *j* that has an election in year *t*, we "stack" all district-year observations for this district in some window around *t*. For instance, if we choose a window from *t-2* through *t+6*, a district holding an election in 2004 will include all observations for the period 2002-2010. Second, we combine the stacked datasets for each separate election into one large panel dataset covering the entire study period. We present evidence using different sets of windows. Narrow windows have the benefit of using more balanced panels, though larger windows permit us to examine effects and pre-trends over longer periods of time.[17]

To improve precision, our preferred specification alters (1) by controlling for fixed-effects that account for heterogeneity across districts and over time. In particular, we estimate a model of the form:

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

where $\alpha_{t+\tau}$ and δ_τ are calendar and relative year effects, respectively, $\mu_{j,t}$ is a district-election fixed-effect, and $\omega_{j,t+\tau}$ is an error term. Note that $\mu_{j,t}$ will control for fixed differences across districts. While unnecessary to eliminate bias, the district-election fixed effects should improve precision and control for changes in sample composition due to the unbalanced panel.[18] We also estimate equation (2) without controlling for a function of the vote share, thus comparing the change in outcomes (pre- vs. post-election) between districts with successful election and those with unsuccessful elections. This difference-in-differences model will yield more precise estimates, yet requires the additional identifying assumption that changes in unobserved determinants of outcomes are unrelated to bond passage.

B. Validity of the Experiment

The key assumption underlying our approach is that districts in which a bond measure narrowly fails provide an accurate counterfactual for what happens in districts with capital campaigns financed by bond measures that are narrowly approved by voters. This assumption could be violated if school districts could control election outcomes in close elections, such that close winners and losers differ on unobservables. We present two pieces of evidence suggesting this type of manipulation is unlikely. First, we examined whether the density of the bond measure vote share is "smooth" at the 50 percent threshold, as there would be with no systematic sorting around the simple majority threshold (McCrary, 2008). The graphical evidence in Appendix Figure 1 and estimates from a formal test of the smoothness of the density indicate little evidence of discontinuities in the distribution of vote share at the 50 percent threshold.[19]

[17] Since multiple observations per district are included, we adjust all standard errors for clustering at the district level.
[18] 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 is constrained to zero in the pre-election period.
[19] The point estimate of the discontinuity in the density obtained from using the procedure proposed in McCrary is 0.227 with a standard error of 0.164.

Second, we estimated discontinuities in the mean of district-level covariates at the 50 percent cutoff. Specifically, we estimated equation (1) and (2) using many pre-election characteristics as the outcome. The results (presented in 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). However, given the number of covariates we examine it is not surprising to see some differences due to random chance. Importantly, pre-election differences in all our main outcomes are small and insignificant.

C. Multiple Elections and "Treatment on the Treated"

The method described above uncovers the causal effect of launching a capital campaign on student outcomes in subsequent years. However, because districts can (and do) hold elections in multiple years, this "intention to treat" (ITT) estimate combines both a direct and indirect effect (via subsequent election outcomes). In other words, some "control" districts (those whose bond measures do not pass) are eventually "treated," thus our setting is akin to that of a "fuzzy" regression discontinuity (RD). In order to uncover the direct effect of bond passage (and capital investment) holding subsequent election outcomes constant, the "treatment on the treated" (TOT), we follow the "one-step" method proposed by CFR (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.[20]

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

This model is estimated on a standard district-year panel among districts holding elections, including all years from 1994 to 2011.[21] 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 present ITT estimates in the appendix.

D. Campus-level Analysis

[20] Vote share is set to zero for observations in which no election was held.
[21] 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. CFR (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 on the one-step estimator. Results using the recursive estimator are available from the authors.

We conduct a variant of this RD approach using cross-sectional data from the 2006 survey of public school facility condition (described below). Specifically, we use data on the first election held prior to 2006 and estimate a standard cross-sectional RD model:

(4) $$Y_{cj,2006} = \theta Pass_j + f_{2006}(v_j) + \varepsilon_{cj2006}$$

where c indexes campuses within district j, and $Y_{cj,2006}$ represents a characteristic of school facilities in 2006. Because the "treatment" variation in this model is at the district level, we adjust standard errors for clustering at the district level. For the same reason, and 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 variants of this 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.

5. Nature and Timing of Capital Investments

We begin the analysis by examining how and when capital campaigns affect school capital investments. Figure 1 presents graphical evidence that bond passage results in a large, immediate increase in capital spending. The first panel shows that spending in the year prior to the election is similar for elections where the bond measure barely passed and failed. In fact, there is no relationship between pre-election spending and the vote share. In the year following an election, however, capital spending increases more than \$2000 per pupil in districts where the bond barely passed compared to those in which it barely failed. The spending increase persists though year two but reverses by year six.[22] The top panel of Table 3 presents ITT estimates of the effect of close bond passage on annual capital outlays per student and cumulative capital spending since the election, using our baseline specification that controls for election fixed-effects and a linear function of the vote share (with varying slopes on each side of the vote share threshold). Bond passage results in a \$2333 increase in capital spending per student (2010\$) in the year following the election, which represents a doubling of per-pupil capital outlays. The estimated impacts of bond passage on capital outlays are also large and positive in the second year after the election but are negative and statistically insignificant thereafter, suggesting that increased capital investments occur shortly after the election. Panel B of Table 3 presents our TOT estimates, which account for the possibility that bond failure today increases the

[22] 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.

likelihood of bond passage in the future, understating the capital investment that follows successful bond passage.[23] Holding subsequent elections constant, bond passage today has a positive effect on capital spending through year 3, though the increase is concentrated in the first two years following bond passage. Bond passage results in an increase in cumulative spending over the 6-year period following an election of about $5,000 per pupil.

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 provide little indication that bond passage affects instructional inputs. In the first four years after the election, bond passage has a very small and statistically insignificant effect on instructional spending per student. We find a small but statistically significant increase in instructional in years 5 and 6, but the magnitudes – about 3 percent of the sample mean – are very small and this result is not robust to alternative specifications (Appendix Table A2). We also found no evidence of effects on student-to-teacher ratios, another important marker of instructional resources. This pattern holds for elementary, middle, high school and various polynomials in vote share (not reported). This is not surprising since we saw no increase in operational expenditures (which would be necessary to reduce student-teacher ratios). Nonetheless, it is important to recognize that student-teacher ratios are an imperfect measure of school crowding. In particular, school crowding may manifest itself as an excessive number of classrooms (via portable buildings) for the existing campus structure, rather than as an excessive number of students in each classroom. Graphical evidence in Figure 2 provides further support for the lack of impact of bond passage on instructional inputs. We interpret these findings as strong evidence of "flypaper effects," implying that money sticks in the capital account.

Our estimated effects on spending and educational inputs are quite robust across different specifications, both qualitatively and quantitatively.[24]

6. Impact of School Capital Campaigns on Student Achievement

Table 4 shows TOT estimates of the impact of bond passage on test scores and attendance rates. Overall, we find very little evidence that bond passage generates improvements in overall student achievement or attendance, a conclusion that is echoed in the graphical evidence (Figure 3). For grades 3-8, the point estimates are initially close to zero and inconsistent in sign. By year 6, the

[23] 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.

[24] 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.

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. 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. Difference-in-differences models that include district fixed effects but no control for vote share generate similarly small estimates that are precise enough to rule out test score effects greater than 0.03 and 0.05 standard deviations for reading and math, respectively. Thus we are able to rule out the imprecise point estimates found by CFR, of a roughly 0.067 and 0.077 student-level standard deviation improvement for 3rd grade reading and math scores from capital investments of comparable magnitude.

Although these results provide little indication that school bond passage leads to appreciable impacts on overall student outcomes, an important question is whether bond passage reduces achievement gaps, as might be the case if the resulting investments disproportionately benefit students from disadvantaged backgrounds within districts. We investigate this issue by estimating effects on the gap between the 10th and 90th percentile of the individual test score and attendance distributions within districts. We find no evidence that bond passage narrows test score gaps; the precision of the estimates permits us to rule out very small effects on the test score distribution. For attendance, the estimates suggest bond passage might reduce disparities in attendance rates, but the estimates imply very small practical effects. We also assessed the robustness of these findings by examining the estimates across a variety of specifications for the vote share polynomial as well as the ITT estimates using alternative bandwidths.[25] These results (reported in Appendix Tables A3 and A5) are consistent with the main substantive message in Table 4 that there is little indication that bond passage narrows test score gaps.

Another way of investigating if capital campaigns reduce disparities is to see if the impacts vary by student socioeconomic status. Table 5 presents TOT estimates for test scores separately for students that receive free lunch and those that are not economically disadvantaged.[26] For the non-free

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

lunch recipients, the estimates are all very close to zero and we can rule out effects larger than 0.06 standard deviations. For the free lunch sample, however, the estimates tend to be positive and by year 6 are statistically significant for both math and reading. Nonetheless, a careful examination of this finding under alternative modeling choices leads us to discount this result somewhat, as the magnitude and significance is sensitive to specification. In Appendix Table A3, we see that the point estimates tend to reduce with more flexible polynomials in vote share and the difference-in-differences estimates are much smaller and insignificant (reading) or only marginally significant (math) compared to our baseline 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).[27]

To address the possibility that changes in the student population offset impacts of capital spending on student achievement, Panel E of Table 4 reports estimates on the overall migration rate of students into the district. The point estimates are small, but positive, for the first four years, then negative thereafter. Though the point estimate in year 2 is marginally statistically significant, this result is not persistent and generally not robust to alternative specifications (not reported).

7. How Much Do Capital Campaigns Improve School Environments?

We find little evidence that capital campaigns improve student achievement. This finding is surprising considering that studies in other contexts have found that large capital campaigns generated sizable impacts on student achievement (Neilson and Zimmerman, 2014). One possible reason our results could differ from those of other studies is that the capital campaigns financed by local districts do not generate substantial improvements in school environments. We explore this possibility in this section. In addition to examining how physical environments change, we also explore the importance of teacher retention – a key correlate of student achievement – for capital campaigns. Recent evidence suggests that school facility quality is an important factor in teachers' decision to remain at schools. We thus test for changes in teacher retention as a consequence of capital campaigns as a mechanism for improving student environments.

In Table 6, we show estimated impacts of capital campaigns on several dimensions of facility quality students face. The regression estimates and graphs in Figure 4 offer some evidence that capital campaigns improve the quality of school buildings through the opening of new schools. For instance,

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

bond-funded school capital campaigns increase the likelihood of a district opening at least one campus by 11 percentage points in the second year after the election. Capital campaigns also double the share of students attending schools opened in the past year and within the past 4 years. Despite these large proportionate increases, the number of students actually exposed to new schools is small: three years after an election, capital campaigns increase the fraction of students enrolled in a school opened within the last 4 years by less than 4 percentage points. This new construction reduces the enrollment-weighted campus age by 2 to 3 years within three years of initiating the capital campaign. Consequently, the change in average building condition predicted by campus age is positive and small, but only statistically significant for the third year following the bond election.[28] This can be seen in Figure 4, though the limited sample of districts for these analyses makes it difficult to observe discontinuities in campus age or average room condition predicted by campus age. The evidence is stronger for the claim that capital campaigns increase exposure to renovated schools. 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.[29] The graphical evidence in Figure 4 confirms that time since renovation is lower in districts where bond measures were narrowly approved, though this sample is even smaller and thus difficult to see visually.

Further evidence on the impact of capital campaigns on facility conditions comes from a cross-sectional regression discontinuity analysis of the 2006 Survey of school conditions, depicted in Figure 5. 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. 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 5 and in Panel A of Table 7, bond passage causes modest increases in the likelihood that school facilities are in at least fair or at least good condition, and lower effective age and maintenance needs, although the estimates are not statistically different from zero for districts overall.[30] However, capital campaigns are associated with

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

[29] Results on campus renovations at long lags should be interpreted cautiously, as estimates are based on a small number of elections (126 elections with 17 failures after 6 years vs. 263 elections with 54 failures after 2 years).

[30] In the 2006 survey, 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" condition is defined as "Some repairs may be beneficial, but the facility is structurally and educationally sound." Appendix Figure A7 plots the fraction of buildings that are in "Fair" and "Good" condition

closing gaps in school facility conditions between older and newer buildings: 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 campaigns also reduce the effective age of school facilities by roughly 7 years, and this effect is statistically significant.[31] The estimates in Panel B suggest that capital campaigns may increase building capacity and reduce overcrowding, but none of the estimates is statistically significant.

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.

In sum, these results suggest that capital campaigns lead to renovations of existing schools and increases in student exposure to renovated schools. We also find evidence that capital campaigns improve the quality of building conditions in older schools. The results suggest that campaigns increase school openings considerably (from a low baseline), but relatively few students are affected by such changes. The results in this section provide some of the first evidence demonstrating that capital campaigns funded by school bonds lead to tangible improvements in schooling facilities, yet the number of students materially affected by the most intensive improvements – new school openings – is modest. Capital investment generated by bond passage occurs in the four years following bond passage.[32] Moreover, alterations to the actual learning environment experienced by students may follow investment with some lag. We find that school opening lags investment by about one year, with the largest rates of opening in years two and three after a successful election.

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. Indeed, recent evidence suggests that low facility quality is an important factor in the decision-making of teachers to leave their current position (Buckley, Schneider, and

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. .

[31] 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.

[32] It should be noted that the timing of capital investments occurs more rapidly in Texas than California (CFR, 2010).

Yang, 2005). In the final row of Table 6, we find that capital campaigns have minimal impact on the fraction of teachers that leave schools (either to another school, out of the district, or out of the profession). Thus the only modest impact on school conditions for the typical student do not translate to measureable effects on teacher retention.

8. Can districts bring bond-funded capital campaigns to scale?

We study the impact of capital campaigns on student achievement by exploiting variation in capital spending from close school bond elections. Our results suggest that the typical school capital campaign has, at most, limited effects on student outcomes. This result stands in sharp contrast to the findings in recent studies that find substantial effects of large school construction programs. We believe our findings can be reconciled with those of recent studies by noting that the interventions examined in other studies were much larger in scope. Indeed, while we find that bond passage does generate appreciable improvements in a variety of measures of school facilities, the effects on student exposure to new schools that are in good condition are modest. That capital campaigns appear to have limited effects has considerable significance given that district-originated school bonds are the primary mechanism for raising funds for investing in school facilities.

We now consider whether the current system of locally financing school construction makes it unrealistic for school districts to undertake bond-funded capital campaigns that are as large as the capital investments examined in recent studies. The median bond proposed to voters in our study period was for $7,580 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 observed in New Haven.[33] To assess whether the typical campaign could be scaled up and have larger effects, in Table 8 we report the effect of bond passage on educational inputs and student achievement separately by bond amount. This analysis modifies our basic TOT model by interacting bond passage with bond amount and displaying the implied predicted effects for several bond amount percentiles. Differences in the impact of capital outlays per student for districts in the 25th and 75th percentiles are stark. As reported in Panel A, two years following bond passage we can observe the impact on cumulative capital outlays per student to be $2,400 at the 25th percentile and $6,500 for districts at the 75th percentile.

Surprisingly, these large differences in cumulative spending by bond amount correspond to very modest differences in effects on school facility conditions. Larger capital campaigns have only a

[33] In fact, the New Haven campaign would be in the 99.6 percentile of all bond elections proposed by school districts in Texas between 1997 and 2010.

slightly larger impact on the share of students attending new schools, the average age of district schools, and the average time since last renovation. Consequently, we find little evidence that larger bond campaigns have proportionately larger impacts on student achievement, even for poor students. One reason larger bonds appear not to generate proportionately larger improvements in school conditions is that they tend to be proposed by wealthier districts with fewer poor students, smaller class sizes, and who are already spending more on instruction and capital investments (Table 9). Districts with older school buildings do propose larger bond amounts, though the explanatory power of this variable beyond other resource measures is low.

This finding is echoed in Table 10, where we repeat our analysis but permit bond passage effects to vary by whether the district has above-median share of economically disadvantaged students at the start of our analysis window (1997). Despite having worse building conditions at baseline, poorer districts make lower levels of capital investment and experience more modest building improvements due to bond passage. Consequently, test score improvements following bond passage are non-existent for poor districts. This analysis likely understates district-level disparities in investment as we just examine districts that hold at least one bond election. Given existing school finance structures, districts with the greatest need for substantial school facility investment are simply not the ones making it.

9. Conclusion

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

This paper provides such empirical evidence. We do so using statewide data from Texas and a research design based on comparisons of districts where referenda on bonds to finance school construction barely were approved and rejected by voters. We find that school bond passage is associated with substantial increases in capital expenditure per student, with no spillover effects on other types of spending. This spending generates 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 or 3 years of bond passage) and renovating older ones.

These facility improvements do not appear to translate into measurable improvements in student achievement. This finding stands in contrast to recent work examining the effects of large-scale, sustained school construction campaigns (Duflo, 2001; Neilson and Zimmerman, 2014; Aaronson and Mazumder, 2011). We argue that a straightforward way of reconciling our findings with these studies is that school bonds are far too small to generate the scale of infrastructure projects examined in those other studies. This is critical because school bonds are the predominant method of financing school construction projects. To the extent that poor school facilities hamper student outcomes and contribute to achievement gaps, bond-funded capital campaigns financed by districts are unlikely to be an adequate mechanism to remedy these detrimental effects.

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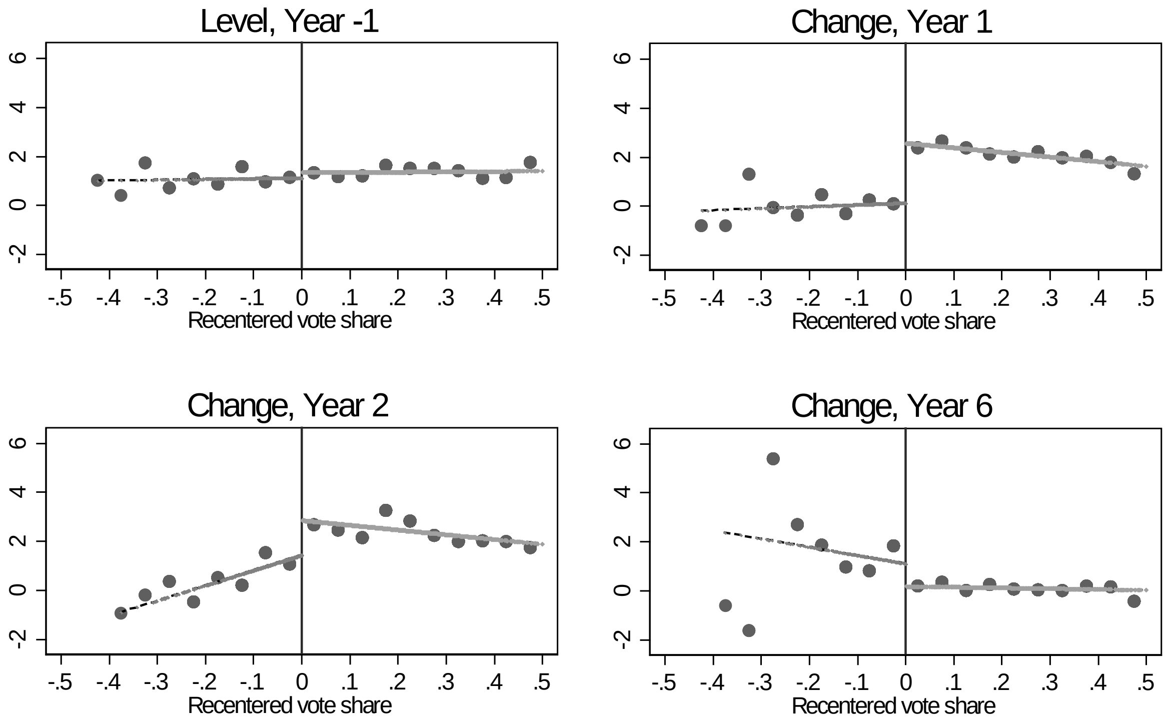
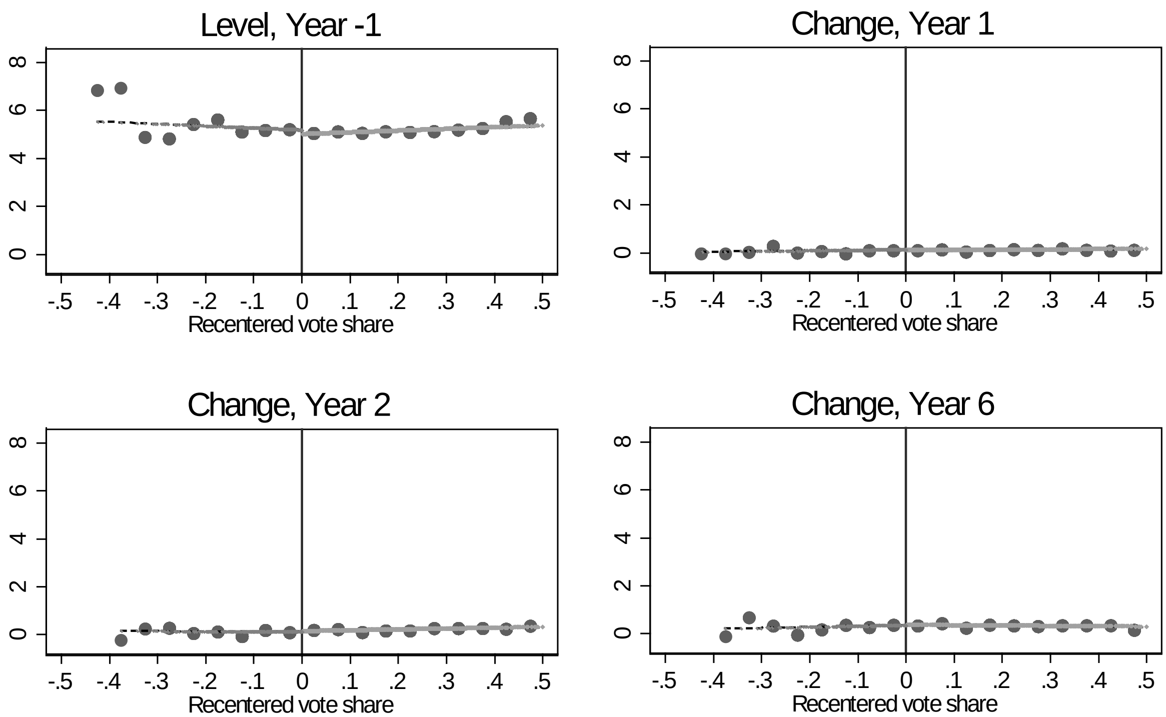
Figure 1. Level and Change in Capital Spending by Vote Share, Before and After Bond Election



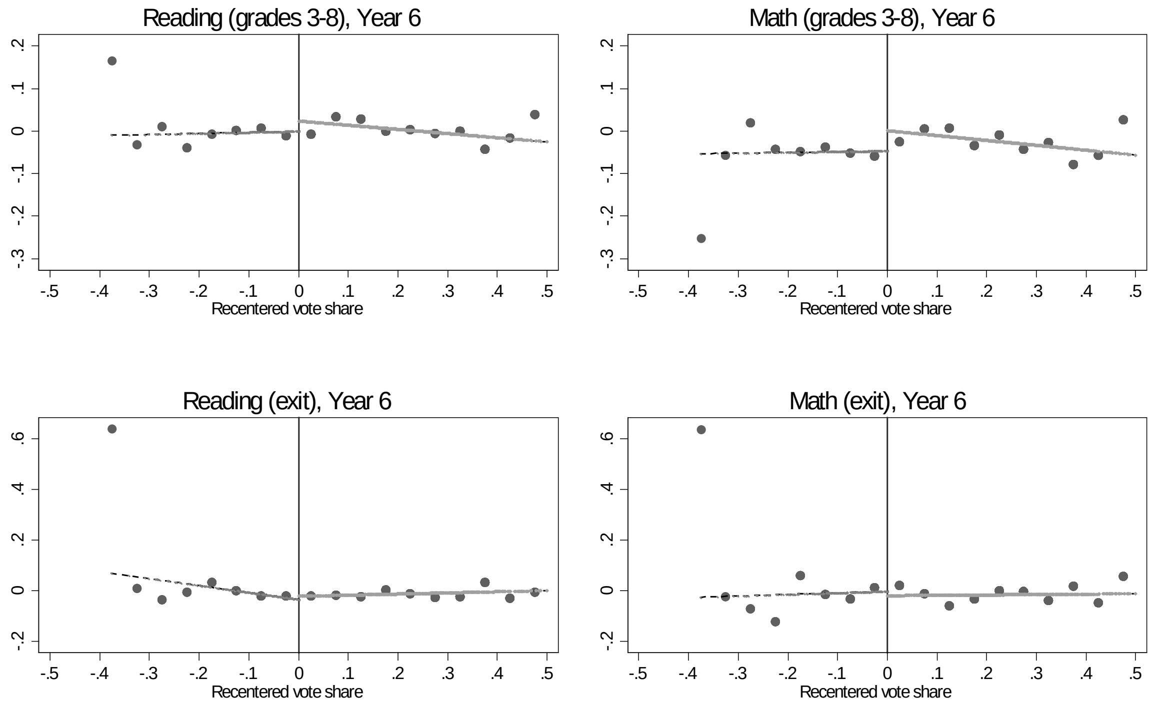
Notes: Graphs plot average district capital spending (in $1000) or change in average district capital spending (relative to years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5 point bins of vote share. Includes data for 1737 elections and 812 districts.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

Figure 2. Instructional Spending by Vote Share, Before and After Bond Election



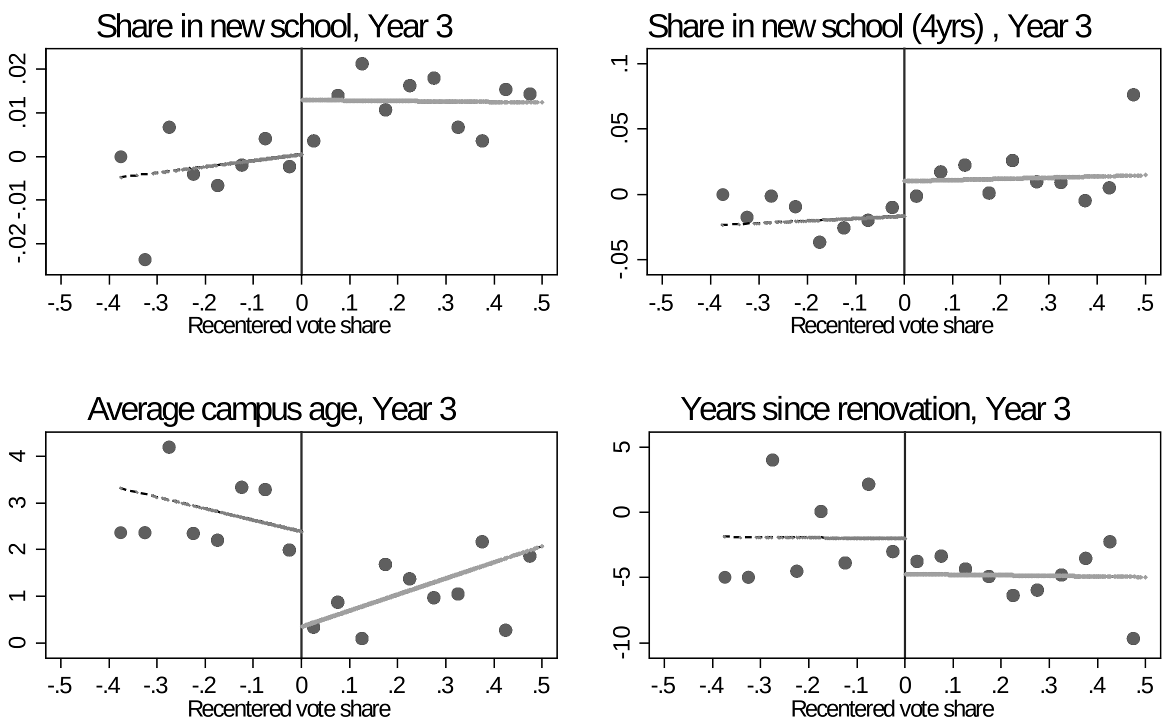
Notes: Graphs plot average district instructional spending or change in average district instructional spending (relative to years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5 point bins of vote share. Includes data for 1737 elections and 812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

Figure 3. Achievement Test Scores by Vote Share, Change Since Bond Election



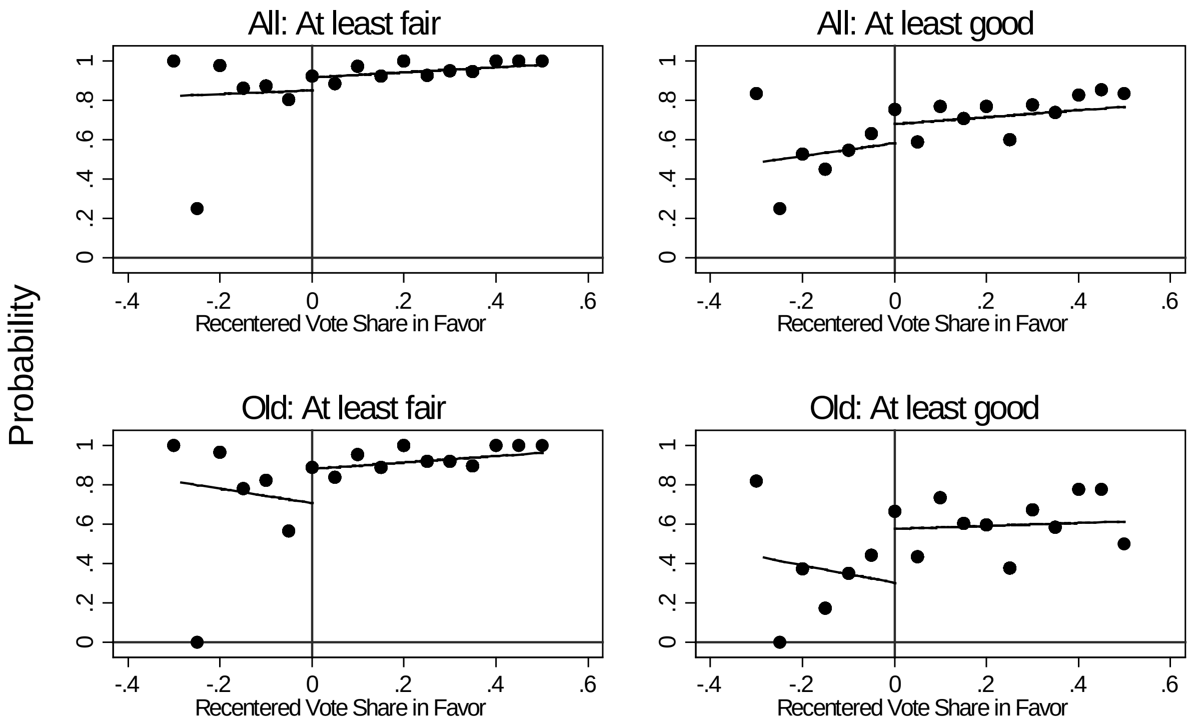
Notes: Graphs plot change in average district test scores (relative to two years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5 point bins of vote share. Includes data for 1737 elections and 812 districts.

Figure 4. Capital Inputs by Vote Share, Change Since Bond Election



Notes: Graphs plot change in average district building conditions (relative to two years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5 point bins of vote share. Top row includes data for 1737 elections and 812 districts. Bottom row includes data for 181 districts (457 elections) and 131 districts (326 elections) for campus age and years since renovation, respectively.

Figure 5. Building Condition by Vote Share



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

Table 1. Summary Statistics of Capital Bond Elections

Year	Number	Pass	Vote share	Votes cast	Bond amount (millions of $2010)		Bond amount per student ($2010)		Multiple
					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

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

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	12.8	12.8	12.8
Fraction vocational ed	21.7	21.4	22.9
Fraction bilingual	7.6	8.0	6.2
Fraction gifted	7.3	7.3	7.0
Instructional spending per student ($2010)	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	32.385	31.317	36.682
Enrollment-weighted average years since school last renovated	13.625	13.432	14.431
Reading test scores (grades 3 to 8)			
District-wide mean	0.027	0.030	0.016
Free lunch mean	-0.270	-0.269	-0.276
Not econ disadvantaged mean	0.200	0.200	0.199
Gap: 90-10 percentile	2.028	2.023	2.047
Gap: Not econ disadv - Free lunch	0.470	0.469	0.472
Math test scores (grades 3 to 8)			
District-wide mean	0.023	0.027	0.007
Free lunch mean	-0.269	-0.265	-0.287
Not econ disadvantaged mean	0.184	0.184	0.180
Gap: 90-10 percentile	2.192	2.187	2.212
Gap: Not econ disadv - Free lunch	0.452	0.450	0.463
Reading test scores (exit exam)			
District-wide mean	0.048	0.050	0.042
Free lunch mean	-0.265	-0.263	-0.272
Not econ disadvantaged mean	0.142	0.143	0.139
Math test scores (exit exam)			
District-wide mean	0.039	0.044	0.015
Free lunch mean	-0.303	-0.295	-0.332
Not econ disadvantaged mean	0.138	0.141	0.126
Attendance rate (fraction of days)			
District-wide mean	96.40	96.41	96.34
Gap: 90-10 percentile	7.86	7.86	7.88
Gap: Not econ disadv - Free lunch	1.15	1.15	1.14
Student in-migration rate (all grades)	0.143	0.144	0.137
# Districts	812	748	279
# Elections	1,737	1,390	347

Notes: Most variables are defined for the full sample of 1737 unique elections. Enrollment-weighted average building age (years since renovation) are only available for 181 districts and 457 (326) 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)	

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

Table 4. 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)	

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

Table 5. 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)	

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

Table 6. 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
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)	
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)	
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)	
Enrollment-weighted average age of school buildings (mean = 32.4)	-1.083	-1.419	-2.737*	-3.005*	-2.501	-2.731+	3,253
	(0.967)	(1.213)	(1.364)	(1.489)	(1.638)	(1.624)	
Enrollment-weighted average years since school last renovated (mean = 13.6)	-3.030*	-5.275**	-5.392*	-9.141**	-9.671**	-10.974**	2,340
	(1.498)	(1.822)	(2.286)	(2.436)	(2.899)	(3.779)	
Building condition based on campus age	0.020	0.024	0.060*	0.046	0.040	0.048	3,253
	(0.020)	(0.025)	(0.028)	(0.030)	(0.034)	(0.035)	
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)	

Notes: Each row represents a separate specification and reports effects of bond measure passage on outcomes t years later. The sample includes yearly panel data from 1994 to 2011 for all 812 districts that held bond elections. Sample for 4th - 6th rows restricted to 181 districts for which campus age was constructed. Model includes indicators for bond passage, holding election, and vote share (with different slopes for passing and non-passing bond) in the current year and each prevoius year up to ten. The table reports the bond passage indicators for each lag. Estimates for current period and lags greater than six are not displayed. Reported mean is for the year prior to the election. Standard errors are clustered at the district level. Significance: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 7. 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 + PassXOld = 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 + PassXOld = 0 (p-val)		0.286		0.199		0.927

Notes: 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 is 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 one in the regression. Most regressions include data from 204 unique school districts. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8. Effect of Bond Passage on Educational Inputs and Test Scores, Heterogeneity by Bond Amount
TOT, Two-part linear specification with district fixed effects.

	Implied effect of bond passage after					
	1 year	2 years	3 years	4 years	5 years	6 years
A. Capital outlays per student						
Bond amount 25th percentile ($4,250 per student)	1868**	720.843	403.743	322.777	140.246	421.825
	(319)	(526)	(451)	(311)	(307)	(705)
Bond amount 50th percentile ($7,580 per student)	2315**	1607**	540+	366.077	320.965	327.502
	(275)	(364)	(320)	(278)	(305)	(619)
Bond amount 75th percentile ($12,540 per student)	2980**	2928**	744+	430.572	590.146	187.008
	(279)	(403)	(428)	(337)	(431)	(708)
B. Cumulative capital outlays per student						
Bond amount 25th percentile ($4,250 per student)	1754**	2436**	2753*	2958**	4377**	4067*
	(434)	(822)	(1,117)	(1,099)	(1,248)	(1,608)
Bond amount 50th percentile ($7,580 per student)	2509**	4056**	4281**	4456**	5384**	4881**
	(370)	(613)	(746)	(858)	(1,019)	(1,296)
Bond amount 75th percentile ($12,540 per student)	3635**	6468**	6557**	6686**	6883**	6093**
	(386)	(659)	(977)	(1,083)	(1,063)	(1,292)
C. Share of enrollment in schools opened in past four years						
Bond amount 25th percentile ($4,250 per student)	0.006	0.024*	0.031*	0.035*	0.025	0.022
	(0.011)	(0.012)	(0.014)	(0.015)	(0.016)	(0.017)
Bond amount 50th percentile ($7,580 per student)	0.006	0.023*	0.034**	0.038**	0.027+	0.023
	(0.011)	(0.011)	(0.013)	(0.015)	(0.015)	(0.016)
Bond amount 75th percentile ($12,540 per student)	0.007	0.021+	0.037**	0.041**	0.029+	0.024
	(0.010)	(0.011)	(0.013)	(0.015)	(0.016)	(0.017)
D. Enrollment-weighted average age of school buildings						
Bond amount 25th percentile ($4,250 per student)	-0.840	-0.877	-2.056	-2.849+	-2.613	-2.729
	(0.977)	(1.249)	(1.367)	(1.562)	(1.706)	(1.672)
Bond amount 50th percentile ($7,580 per student)	-1.033	-1.121	-2.361+	-2.471	-2.221	-2.323
	(0.948)	(1.203)	(1.328)	(1.535)	(1.682)	(1.652)
Bond amount 75th percentile ($12,540 per student)	-1.322	-1.483	-2.816*	-1.907	-1.636	-1.718
	(0.944)	(1.167)	(1.296)	(1.663)	(1.793)	(1.823)
E. Enrollment-weighted average years since school last renovated						
Bond amount 25th percentile ($4,250 per student)	-2.075	-4.472*	-5.543*	-8.155**	-8.983**	-10.403**
	(1.551)	(2.046)	(2.335)	(2.226)	(2.542)	(3.662)
Bond amount 50th percentile ($7,580 per student)	-2.733+	-4.802*	-5.599*	-8.713**	-9.796**	-10.720**
	(1.535)	(1.943)	(2.294)	(2.272)	(2.674)	(3.928)
Bond amount 75th percentile ($12,540 per student)	-3.713*	-5.294**	-5.682*	-9.543**	-11.008**	-11.193*
	(1.551)	(1.949)	(2.518)	(2.798)	(3.307)	(4.981)
F. Standardized math scores (grades 3-8)						
Bond amount 25th percentile ($4,250 per student)	-0.001	-0.006	-0.004	-0.018	0.000	0.022
	(0.019)	(0.022)	(0.024)	(0.027)	(0.027)	(0.028)
Bond amount 50th percentile ($7,580 per student)	-0.001	-0.003	0.001	-0.017	-0.003	0.023
	(0.018)	(0.021)	(0.023)	(0.025)	(0.026)	(0.027)
Bond amount 75th percentile ($12,540 per student)	-0.002	0.003	0.008	-0.015	-0.007	0.025
	(0.018)	(0.021)	(0.022)	(0.024)	(0.027)	(0.027)
G. Standardized math scores free lunch (grades 3-8)						
Bond amount 25th percentile ($4,250 per student)	-0.008	0.003	-0.013	-0.008	0.05	0.059+
	(0.025)	(0.028)	(0.029)	(0.033)	(0.035)	(0.034)
Bond amount 50th percentile ($7,580 per student)	0.000	0.013	-0.005	-0.002	0.042	0.062*
	(0.024)	(0.026)	(0.027)	(0.031)	(0.033)	(0.032)
Bond amount 75th percentile ($12,540 per student)	0.012	0.028	0.007	0.007	0.029	0.067*
	(0.025)	(0.026)	(0.026)	(0.030)	(0.034)	(0.031)

Notes: Each panel 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 D and E restricted to 181 districts for which campus age was constructed. Model includes indicators for bond passage, holding election, and vote share (with different slopes for passing and non-passing bond) in the current year and each prevoius year up to ten. Model also includes interaction between bond passage (in current and each previous year) and the bond amount. The table reports the implied (bond passage) + (bond amount) X (bond passage) coefficient for different bond amounts and for each lag. Estimates for current period and lags greater than six are not displayed. Standard errors are clustered at the district level. Significance: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 9. Baseline District Correlates of School Bond Size

	Dept variable: Bond amount per student at baseline (thousands) (mean = 9.78)		
	(1)	(2)	(3)
Total enrollment (thousands) (mean = 10.94)	-0.045**	-0.019	-0.019
	(0.018)	(0.020)	(0.017)
Fraction of students eligible for free lunch (mean = 49)	-0.103***	-0.087***	-0.095***
	(0.016)	(0.023)	(0.023)
Student-teacher ratio - overall (mean = 14.1)	-0.043	-1.260**	-1.122**
	(0.054)	(0.488)	(0.478)
Instructional spending per student (thousands) (mean = 5.00)	2.600***	0.850	0.745
	(0.674)	(0.867)	(0.912)
Capital outlay per student (thousands) (mean = 1.36)	1.509*	2.057	2.159
	(0.788)	(1.390)	(1.425)
Enrollment-weighted average age of school buildings (mean = 32.36)			0.088
			(0.057)
Constant	0.693	24.948***	20.895**
	(3.496)	(9.025)	(9.141)
Observations	1,737	457	457
R-squared	0.159	0.200	0.207
Sample	All elections	Elections with campus age	

Notes: 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 181 districts. District characteristics are averaged in the two years prior and year of election. Standard errors are clustered at the school district level. * p < 0.10, ** p < 0.05, *** p < 0.01

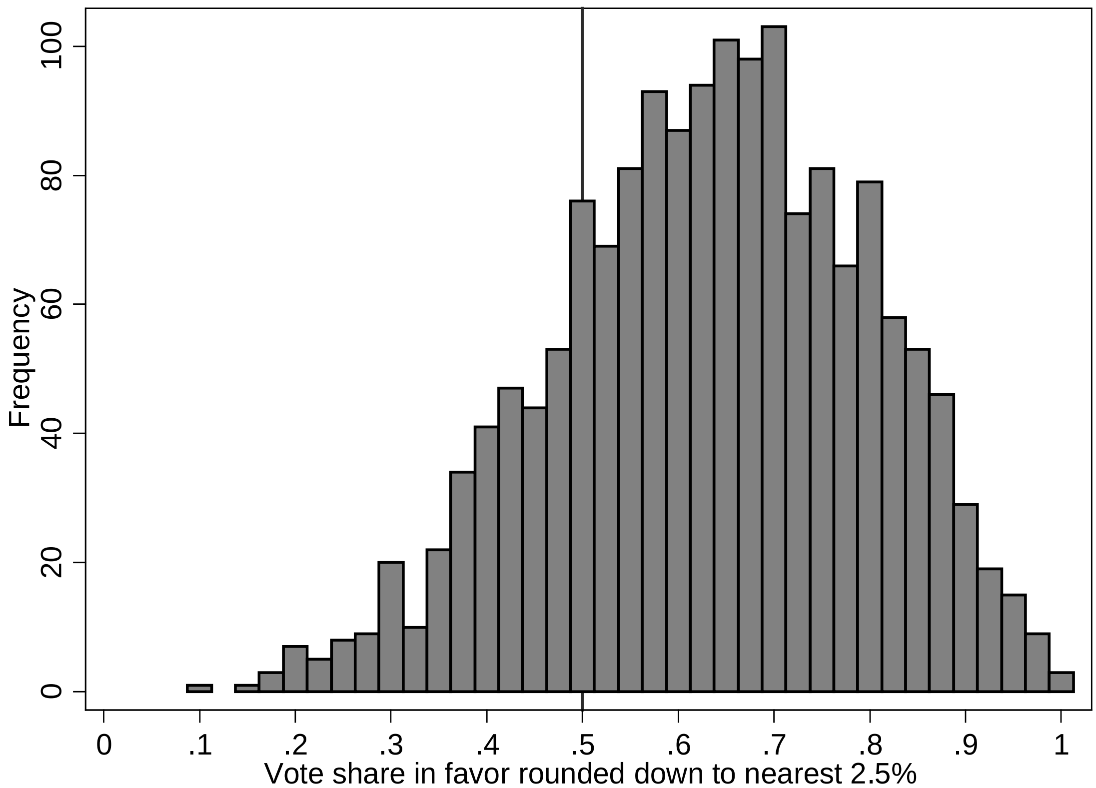
Table 10. Effect of Bond Passage on Educational Inputs and Test Scores, Heterogeneity by District Characteristics
TOT, Two-part linear specification with district fixed effects.

		Effect of bond passage after						
	mean	1 year	2 years	3 years	4 years	5 years	6 years	n
B. Cumulative capital outlays per student								
Non-poor Districts		3637**	5876**	5872**	5865**	6317**	5414**	11,360
		(435)	(706)	(872)	(1,001)	(1,177)	(1,430)	
Economically-disadvantaged Districts		2512**	4746**	4547**	4588**	5091**	4524**	
		(429)	(746)	(941)	(1,029)	(1,168)	(1,378)	
C. Share of enrollment in schools opened in past four years								
Non-poor Districts	0.058	0.009	0.024*	0.041**	0.042**	0.031*	0.026	13,791
		(0.011)	(0.012)	(0.013)	(0.015)	(0.016)	(0.017)	
Economically-disadvantaged Districts	0.037	0.007	0.017	0.029*	0.031+	0.020	0.022	
		(0.011)	(0.012)	(0.014)	(0.016)	(0.017)	(0.018)	
D. Enrollment-weighted average age of school buildings								
Non-poor Districts	32.667	-0.903	-1.544	-3.015*	-3.330*	-2.753+	-2.899+	3,253
		(1.049)	(1.282)	(1.464)	(1.548)	(1.652)	(1.623)	
Economically-disadvantaged Districts	36.651	-1.006	-1.037	-2.274+	-2.401	-2.008	-2.409	
		(0.913)	(1.154)	(1.333)	(1.602)	(1.825)	(1.754)	
F. Standardized math scores (grades 3-8)								
Non-poor Districts	0.124	0.002	0.005	0.018	-0.002	0.016	0.051+	14,520
		(0.018)	(0.021)	(0.022)	(0.023)	(0.026)	(0.027)	
Economically-disadvantaged Districts	-0.105	-0.01	-0.017	-0.02	-0.044+	-0.041	-0.011	
		(0.020)	(0.023)	(0.024)	(0.026)	(0.028)	(0.028)	
G. Standardized math scores free lunch (grades 3-8)								
Non-poor Districts	-0.220	0.012	0.026	0.011	0.020	0.062+	0.095**	13,962
		(0.025)	(0.026)	(0.026)	(0.029)	(0.034)	(0.031)	
Economically-disadvantaged Districts	-0.312	0.000	0.01	-0.023	-0.031	-0.015	0.019	
		(0.027)	(0.028)	(0.029)	(0.033)	(0.037)	(0.034)	

Notes: Each panel 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 panels D and E restricted to 181 districts for which campus age was constructed. Model includes indicators for bond passage, holding election, and vote share (with different slopes for passing and non-passing bond) in the current year and each prevoius year up to ten. Model also includes interaction between bond passage (in current and each previous year) and indicator for whether the district has above-median share of economically disadvantaged students in 1997. The table reports the implied (bond passage) + (poor) X (bond passage) coefficient for each lag.Estimates for current period and lags greater than six are not displayed. Reported mean is for the year prior to the election. Standard errors are clustered at the district level. Significance: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

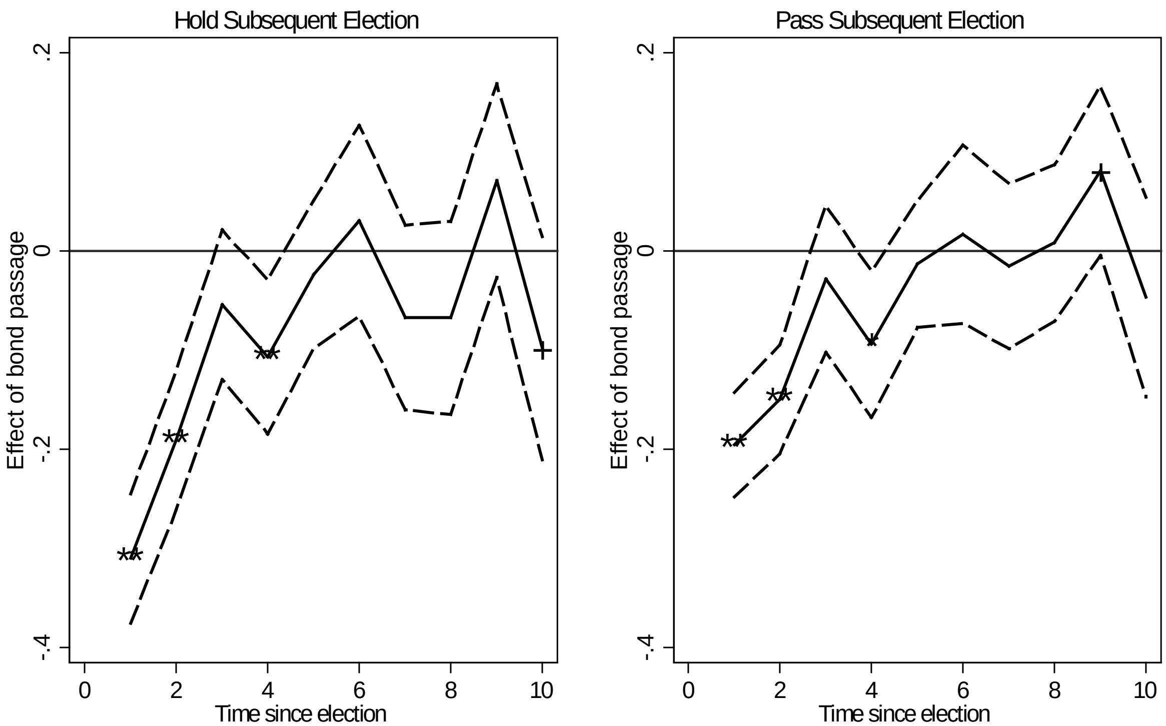
APPENDIX FIGURES

Appx Figure A1. Histogram of Vote Shares



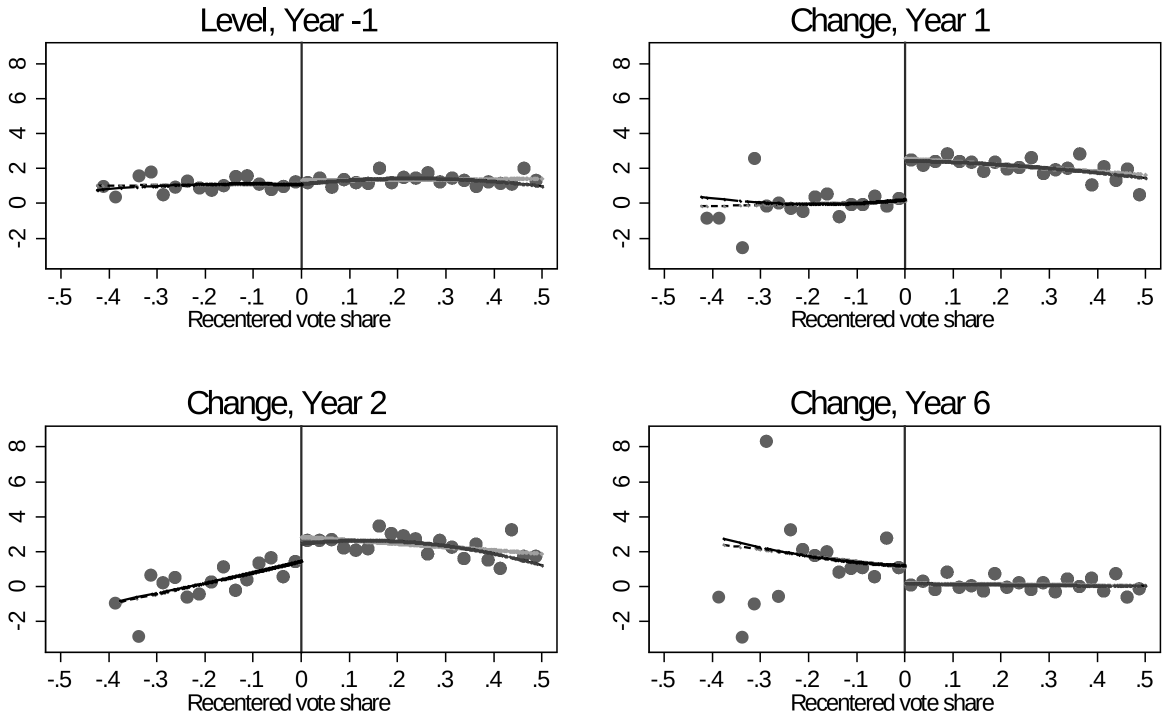
Notes: Graphs frequency of election, where elections are grouped in 2.5 point bins of vote share. Includes data for 1737 elections and 812 districts.

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



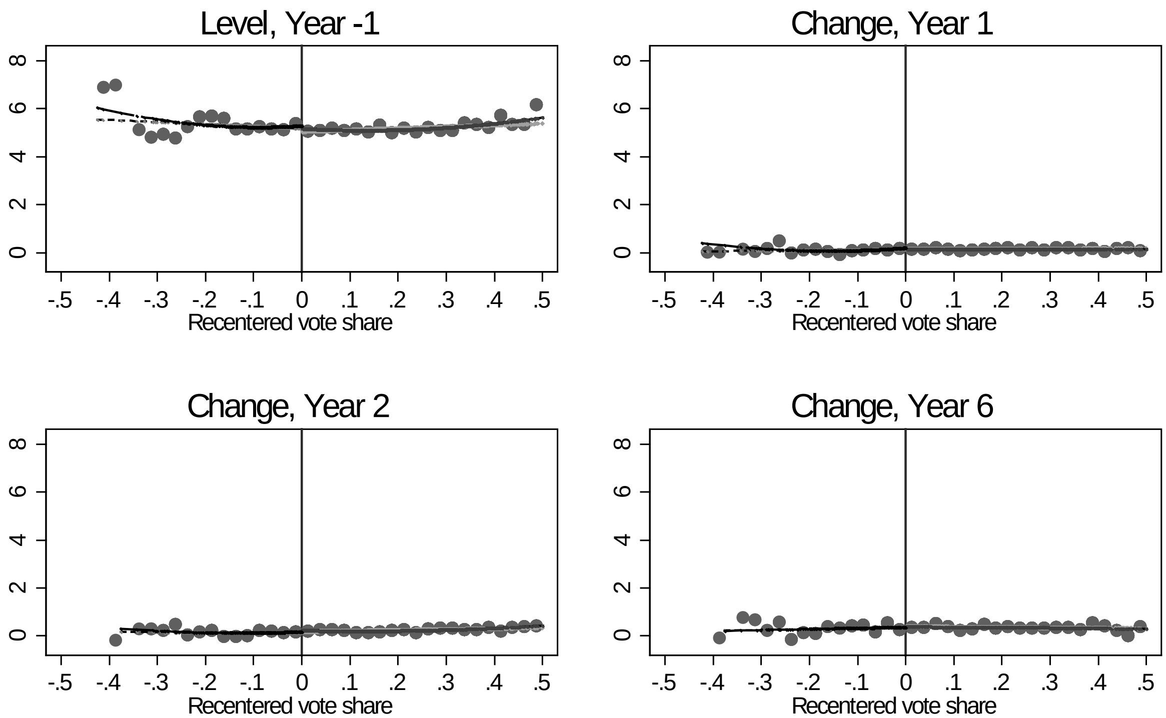
Notes: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on indicator for holding (passing) another bond election one through ten years following bond passage. Specification pools observations two years before through ten years after each bond election and includes fixed effects for each separate election and a linear function of the bond measure vote share, as described in Section 4A and equation (2). Results omitting election fixed effects are indistinguishable. Observation in year of election is omitted. Markers indicates significantly different from zero at a 10% (+), 5% (*), and 1% (*) level.

Appendix Figure A3. Capital Spending by Vote Share, Before and After Bond Election



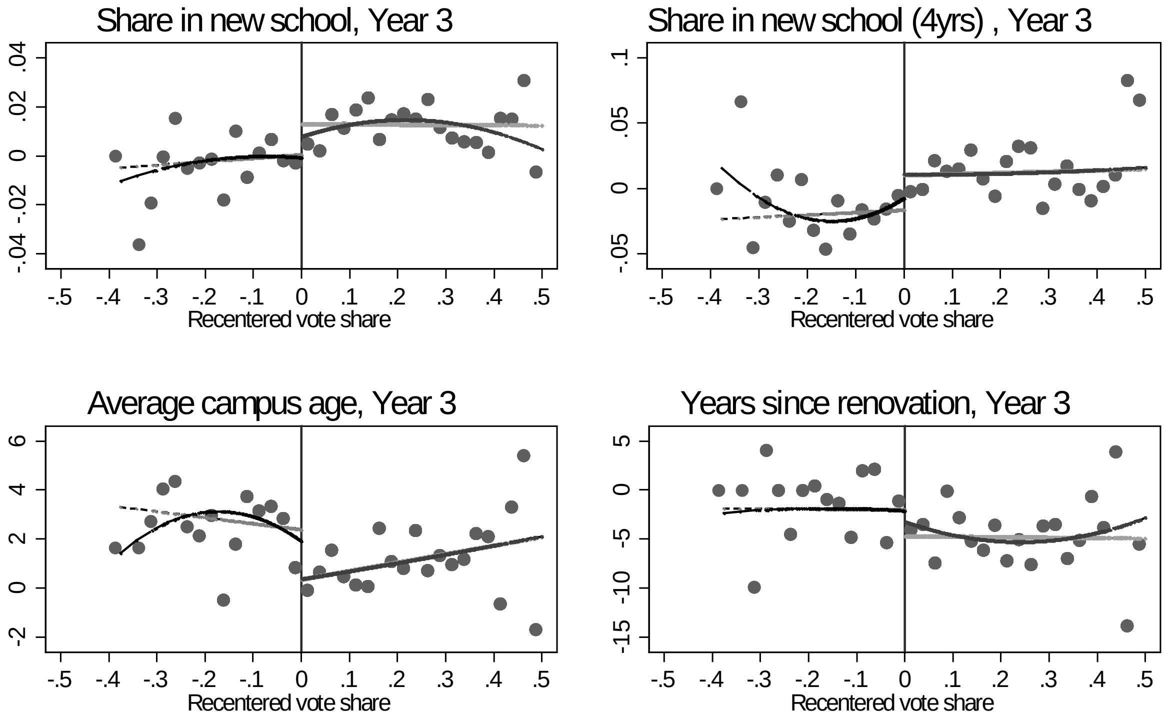
Notes: Graphs plot average district capital spending (in $1000) or change in average district capital spending (relative to years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 2.5 point bins of vote share. Includes data for 1737 elections and 812 districts. Spending data is from the NCES Common Core.

Appendix Figure A4. Instructional Spending by Vote Share, Before and After Bond Election



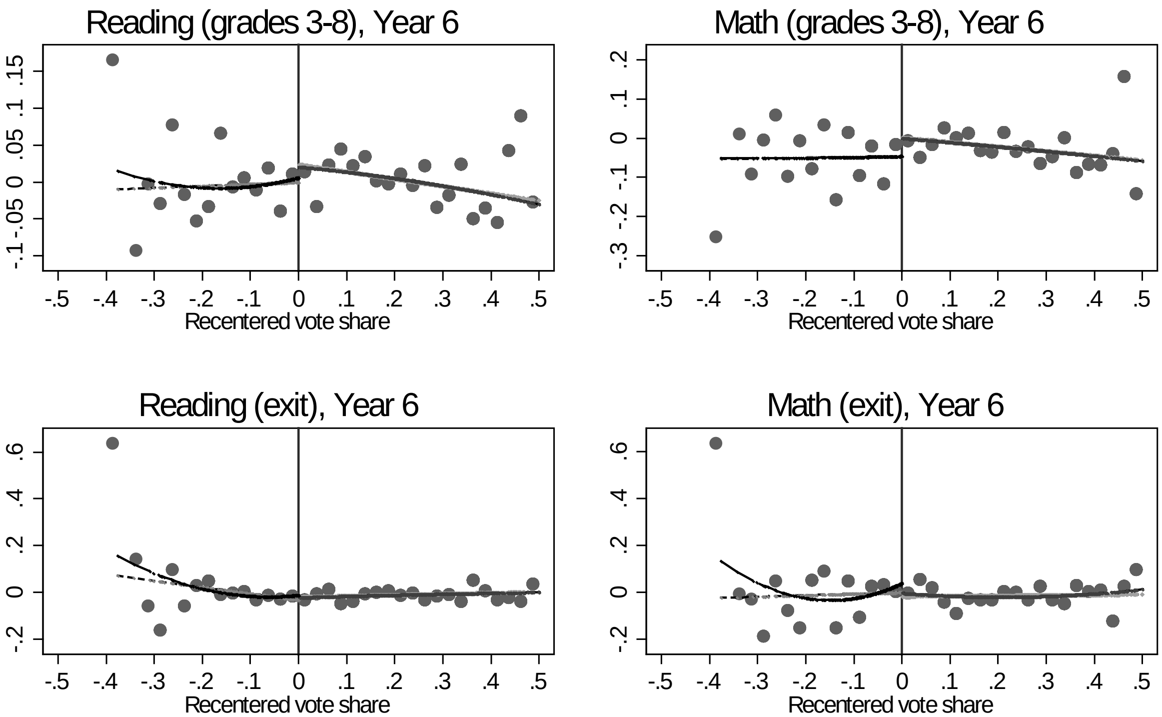
Notes: Graphs plot average district instructional spending or change in average district instructional spending (relative to years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 2.5 point bins of vote share. Includes data for 1737 elections and 812 districts. Spending data is from the NCES Common Core.

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



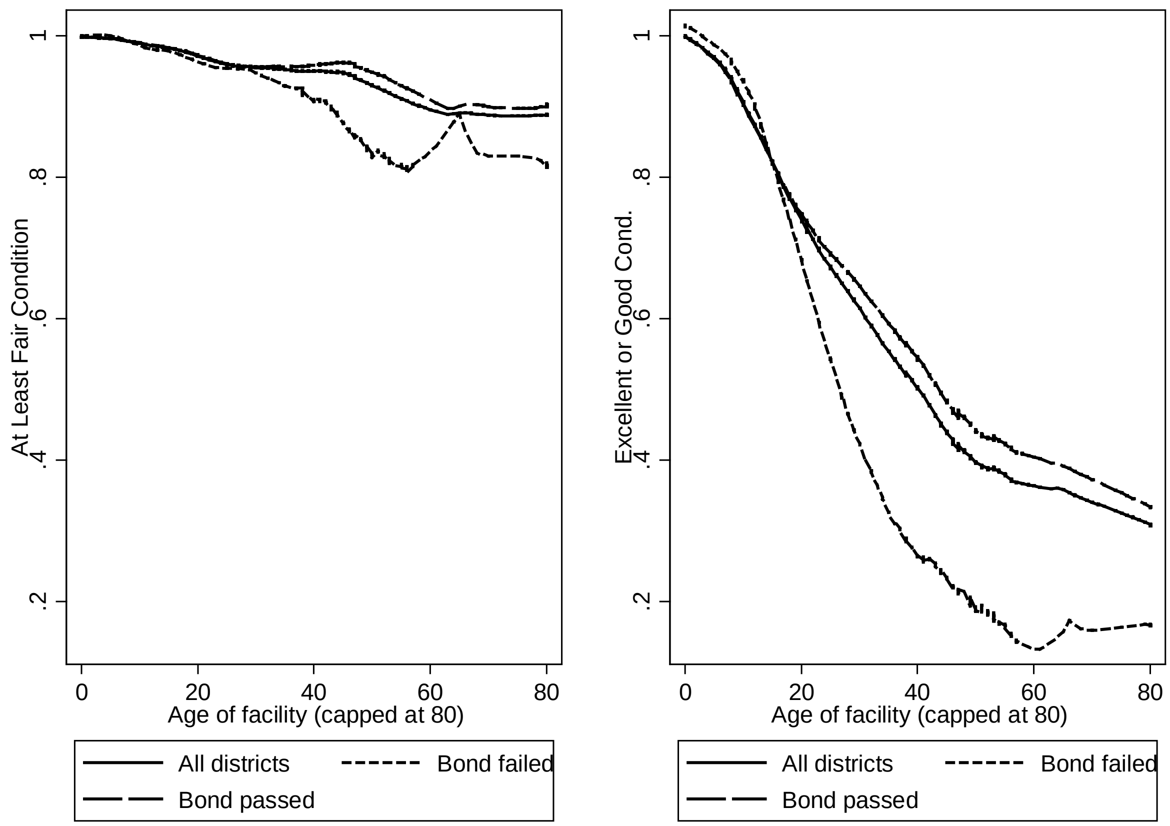
Notes: Graphs plot change in average district building conditions (relative to two years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 2.5 point bins of vote share. Top row includes data for 1737 elections and 812 districts. Bottom row includes data for 181 districts (457 elections) and 131 districts (326 elections) for campus age and years since renovation, respectively.

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



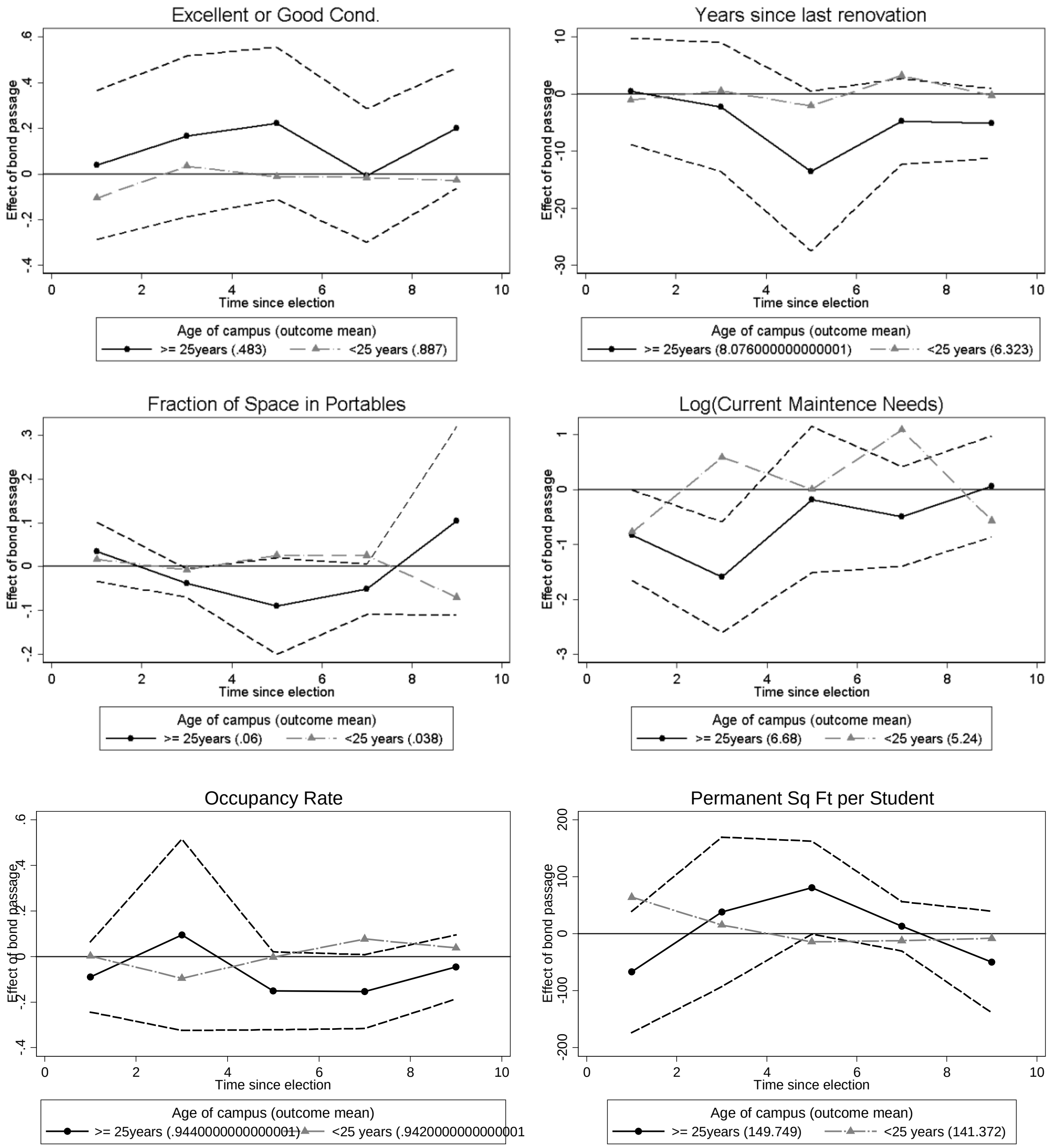
Notes: Graphs plot change in average district test scores (relative to two years prior to election), separately by the vote share in favor of bond passage. Elections were grouped in 5 point bins of vote share. Includes data for 1737 elections and 812 districts.

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



Notes: Graphs plot lowess 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



Notes: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on four measures of building condition one through nine 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 two-year bins. Confidence interval is displayed for old campuses only. Specification includes indicators for time since election (grouped into two-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 1. 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 (32.38)	-5.463+	-1.104**		(0.024)	(0.017)
	(2.800)	(0.379)			
Enrollment-weighted average years since school last renovated (13.62)	-0.765	-1.271	Within district 90-10 test score difference (grades 3-8)		
	(1.732)	(0.820)	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

Notes: Each cell represents a separate specification and reports effects of bond measure passage on outcomes the year prior to election. The sample in columns 1 includes all bond elections and outcome measures in the year prior to the election. This specifications include 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 colum 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, election 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. + p < 0.10, * p < 0.05, ** p < 0.01.

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						
polynomial	1 year	2 years	3 years	4 years	5 years	6 years	n
	A. Capital outlays per student						
2-part linear	2745**	2368**	734+	469	309	-109	12,172
	(288)	(376)	(393)	(368)	(315)	(462)	
None	2440**	2355**	510*	507*	290	-490	
	(169)	(212)	(245)	(203)	(195)	(404)	
Linear	2861**	2753**	908**	590+	143	-400	
	(303)	(382)	(351)	(313)	(283)	(399)	
Quadratic	2623**	2079**	611	480	197	-54	
	(323)	(376)	(380)	(380)	(295)	(519)	
Cubic	2464**	1944**	679+	561	147	-130	
	(364)	(439)	(398)	(408)	(323)	(465)	
	B. Cumulative capital outlays per student						
2-part linear	3199**	5376**	5271**	5319**	5738**	5007**	11,360
	(410)	(678)	(857)	(968)	(1,125)	(1,363)	
None	2892**	4843**	4681**	4943**	4792**	3797**	
	(249)	(378)	(494)	(559)	(658)	(962)	
Linear	3199**	5725**	5700**	5814**	5743**	4859**	
	(407)	(658)	(796)	(907)	(1,065)	(1,333)	
Quadratic	3025**	4984**	4903**	5089**	5138**	4530**	
	(454)	(723)	(870)	(1,012)	(1,148)	(1,386)	
Cubic	2746**	4619**	4620**	4847**	4699**	4019**	
	(508)	(804)	(971)	(1,160)	(1,320)	(1,525)	
	C. Instructional spending per student						
2-part linear	-46	27	32	96	175.886*	158.262+	12,172
	(65)	(72)	(74)	(85)	(87)	(87)	
None	-27	-4	20	-37	-31	-37	
	(36)	(38)	(36)	(39)	(40)	(41)	
Linear	-90	-54	30	47	150.505+	128	
	(61)	(68)	(68)	(78)	(82)	(83)	
Quadratic	-28	79	37	113	158.479+	147	
	(67)	(74)	(78)	(90)	(94)	(95)	
Cubic	-57	71	23	75	121	115	
	(70)	(78)	(83)	(94)	(104)	(103)	

	Effect of bond passage after						
polynomial	1 year	2 years	3 years	4 years	5 years	6 years	n
	D. Share of enrollment in schools opened in past four years						
2-part linear	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)	
None	-0.001	0.012+	0.028**	0.034**	0.029**	0.022*	
	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)	(0.009)	
Linear	0.009	0.021*	0.036**	0.042**	0.031*	0.023	
	(0.010)	(0.011)	(0.012)	(0.014)	(0.014)	(0.015)	
Quadratic	0.010	0.027*	0.039**	0.039*	0.028+	0.021	
	(0.011)	(0.012)	(0.014)	(0.015)	(0.016)	(0.017)	
Cubic	0.012	0.031*	0.043**	0.043*	0.033+	0.021	
	(0.012)	(0.014)	(0.016)	(0.018)	(0.018)	(0.018)	
	E. Enrollment-weighted average age of school buildings						
2-part linear	-1.083	-1.419	-2.737*	-3.005*	-2.501	-2.731+	3,253
	(0.967)	(1.213)	(1.364)	(1.489)	(1.638)	(1.624)	
None	-0.716+	-0.979*	-2.002**	-2.514**	-2.240**	-2.436**	
	(0.432)	(0.481)	(0.512)	(0.679)	(0.721)	(0.746)	
Linear	-1.567+	-1.704	-3.279*	-3.834**	-3.363*	-3.561*	
	(0.841)	(1.119)	(1.297)	(1.364)	(1.498)	(1.615)	
Quadratic	-0.741	-1.202	-2.590+	-2.842+	-2.274	-2.449	
	(1.075)	(1.309)	(1.510)	(1.575)	(1.782)	(1.805)	
Cubic	-0.308	-0.929	-2.669	-2.604	-2.596	-2.448	
	(1.221)	(1.481)	(1.749)	(1.745)	(2.019)	(2.230)	
	F. Enrollment-weighted average years since school last renovated						
2-part linear	-3.030*	-5.275**	-5.392*	-9.141**	-9.671**	-10.974**	2,340
	(1.498)	(1.822)	(2.286)	(2.436)	(2.899)	(3.779)	
None	-2.598**	-3.978**	-3.800**	-6.278**	-6.504**	-2.858	
	(0.769)	(0.994)	(1.171)	(1.221)	(1.228)	(1.984)	
Linear	-2.870+	-4.737**	-4.904*	-8.994**	-10.134**	-8.839*	
	(1.537)	(1.775)	(2.273)	(2.455)	(2.840)	(3.561)	
Quadratic	-3.510*	-6.065**	-6.006**	-9.440**	-10.485**	-13.564**	
	(1.569)	(1.785)	(2.310)	(2.817)	(3.292)	(4.367)	
Cubic	-3.325+	-5.784**	-5.715*	-9.279**	-10.564**	-13.187**	
	(1.833)	(1.974)	(2.595)	(3.079)	(3.819)	(5.037)	

Notes: Each row represents a separate specification and reports effects of bond measure passage on outcomes t years later. The sample includes yearly panel data from 1994 to 2011 for all 812 districts that held bond elections. Sample for panels E and F restricted to 181 districts for which campus age was constructed. Model includes indicators for bond passage, holding election, and vote share (with different slopes for passing and non-passing bond)in the current year and each prevoius year up to ten. The table reports the bond passage indicators for each lag. Estimates for current period and lags greater than six are not displayed. Standard errors are clustered at the district level. Significance: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

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	-3.199**	-4.112**	-3.533**	-5.132**	-5.537**	-1.542	2,538
		(0.568)	(0.877)	(1.057)	(1.244)	(1.560)	(2.290)	
all	2-part lin	-2.803*	-4.568**	-3.280	-6.580**	-7.070*	-7.289+	2,538
		(1.131)	(1.563)	-2.052	(2.360)	(2.916)	(4.150)	
all	quadratic	-2.842*	-4.718**	-2.910	-6.493*	-7.407*	-9.402+	2,538
		(1.163)	(1.584)	(2.112)	(3.083)	(3.555)	(4.851)	
0.15 to 0.85	2-part linear	-2.470+	-4.309**	-2.602	-6.005*	-7.203*	-7.722+	2,151
		(1.316)	(1.671)	(2.156)	(2.700)	(3.219)	(4.557)	
0.25 to 0.75	2-part linear	-2.291	-4.221*	-2.623	-6.250+	-7.688+	-9.926+	1,577
		(1.467)	(1.757)	(2.157)	(3.541)	(4.146)	(5.580)	
0.40 to 0.60	2-part linear	-0.070	-1.062	-2.238	-10.244	-10.519	-10.697	583
		(2.073)	(2.499)	(3.688)	(7.560)	(8.544)	(11.341)	

Notes: 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 voteshare 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: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

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)	

Notes: 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 voteshare 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: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Appendix Table A6. Effect of Bond Passage on Test Scores - ITT and Robustness for Economically Disadvantaged Stu

		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)	

Notes: 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 voteshare 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: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.