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

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Abstract

Public investments in repairs, modernization, and construction of schools cost billions. Yet little is known of the nature of infrastructure investments and the subsequent causal impacts on student outcomes. Because capital investments take many forms that can vary within particular school districts, it could operate to close (or widen) achievement gaps. This paper characterizes capital spending resulting from successful bond elections and evaluates its impact on student performance by exploiting spending variation generated from close school bond elections. School districts with successful and unsuccessful bond measures in close elections are similar in initial spending levels and predetermined attributes but starkly different in capital investments following elections, suggesting that close elections generate spending variation that mimics randomization. We find that bond passage leads to tangible improvements in facility conditions at older campuses. Overall, we find modest increases in school attendance and student achievement, primarily among poor students. These gains occur at existing campuses, suggesting that renovations (not merely the construction of new schools) can improve student achievement.

1. Introduction

States and local school districts experiment with diverse approaches aimed at improving student experiences and learning environments. Prominent examples include restricting class size, reorganizing large schools into smaller schools, and altering grade configurations.[1] While these systemic education reforms garner considerable attention from researchers and policymakers, investments in physical school facilities remain a primary, yet often overlooked, means of improving educational environments. State and local governments spend an enormous amount of resources on such investments, with annual expenditures on school facilities about $66 billion (or $1344 per student; NCES, 2011).[2] Despite the magnitude of these investments, many students attend schools that are in a state of disrepair. One estimate suggests that $300 billion in deferred maintenance is needed to bring U.S. schools into "good" condition (ASCE, 2009) and that one-quarter of U.S. schools report needing major repairs (NCES, 2000).

The prevalence of schools in need of repair is concerning because poor physical environments may impede students' ability to learn. Such effects may exist if students learn more easily in safe, clean, and controlled physical environments.[3] Schools in poor repair may also contribute to educational disparities since inadequate school facilities disproportionately serve low-income and minority students (Filardo et al., 2006). At the same time, however, the effect of school facility investments on student outcomes remains unclear. Furthermore, relatively little is known about how school facility spending is allocated within districts, how it actually affects the physical condition of schools, and the ways in which it affects student outcomes.

In this paper, we examine these questions using eighteen years of administrative data from Texas public schools. Our research design exploits variation in school facility spending arising from the electoral outcomes of school bond referenda. Like many states, school districts in Texas approve capital investments through voter referendum, where a simple majority can approve the issuance of school bonds to finance capital projects. We use a difference-in-difference approach that examines differential changes in districts

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[1] A majority of states have policies limiting class size, although budgetary pressure resulting from the Great Recession has led some to relax these requirements (Sparks, 2012). Shah et al. (2009) examine small school reforms in Oakland, California and Shakrani (2008) argues small schools benefit high school students. Schwerdt and West (2013) and Rockoff and Lockwood (2010) show that student achievement falls in the year students move to middle school and infer that schools configured to serve students through 8th grade are preferable to having separate elementary and middle schools.

[2] The scope of investments in school facilities 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).

[3] 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).

where school bond elections were passed and rejected, and we refine this analysis by focusing on close elections using regression discontinuity methods.

This study builds on an earlier study by Cellini, Ferreira, and Rothstein (2010) that used a similar research design to analyze the effects of school bond passage on housing prices and test scores of third graders in California.[4] Our contributions relative to this earlier study are four-fold. First, we examine how school bond passage affects actual physical environments. We estimate effects on the likelihood that schools are in good physical condition as well as on school openings and closings. We also consider how facilities investments affect disparities in the probability that a student attends a school in good condition. Second, we use rich student-level data that allows us to examine effects for students most at risk of attending schools in disrepair as well as effects on achievement gaps. These finer-grained data have substantive benefits since we find some evidence that impacts on test scores are larger among poor students. Third, the data we use also allow us to look at potential mechanisms for student achievement effects such as improved attendance. It also allows us to consider, and rule out, changes in student mobility as an explanation for impacts on test scores. Finally, the combination of student-level data and a longer time series on student achievement outcomes yields estimated impacts on student achievement that are considerably more precise than those reported in CFR.

We find clear evidence that bond passage leads to large increases in capital investment that are concentrated in the first two years following the bond election. Crucially, we find no evidence of any effects on operating spending or on average class size, suggesting the funds raised through the bonds are not reallocated away from capital spending. The investments in school facilities resulting from bond passage lead to tangible improvements in school conditions. We find that schools in districts where bond measures narrowly passed experience greater increases in the probability of being in good physical condition relative to schools in districts where voters narrowly rejected bond measures. We also find that bond passage reduces overcrowding (as measured by the ratio of enrollment to physical capacity) as well reported maintenance needs. These effects appear to be driven by improvements in the conditions of existing older schools as well as through closing older schools and replacing them with new schools. These results constitute the first causal estimates of the impact of school bond passage on the physical condition of school buildings. This is

[4] Hong and Zimmer (2014) also use this approach using data from Michigan. They find that bond passage leads to improved student achievement about six to seven years after the bond election. Earlier studies find a positive relationship between student achievement and measures of school facilities investments (Crampton, 2009; Jones and Zimmer, 2001; Earthman and Lemasters, 1996; Picus et al., 2005; Blincoe, 2009), although unobserved factors (e.g. residents' taste for education) may drive both capital spending and student performance. Neilson and Zimmerman (2011) and Welsh et al. (2012) find large positive impacts on student outcomes of large-scale, sustained construction programs in New Haven and Los Angeles, respectively. In contrast, the school bonds studied here finance facilities investments under normal conditions for a broader range of districts. These school bonds are also used to finance renovations of existing facilities (in addition to construction of new schools). This is important because voters strongly prefer to renovate existing schools rather than build new facilities (Zimmer et al., 2011).

notable, as prior work has not demonstrated that the incremental spending is not just used for non-educational purposes such as athletic and arts facilities.

Turning to effects on student outcomes, we find evidence of positive effects of bond passage on math and reading achievement that materialize six years after bond passage and that are stronger for poor students. While modest in magnitude, these estimates suggest that learning may be impeded by attending a school in disrepair. The point estimates are also similar in magnitude to those reported in CFR, although our estimates are substantially more precise. Notably, the results are similar for schools that existed prior to bond passage, suggesting that renovations to existing campuses (not merely the construction of new ones) can improve student performance. While we cannot pin down all the possible mechanisms that could drive these test score results, we do find that bond passage improves student attendance. Given that poor school facilities have been implicated as a cause of school absences (Earthman, 2002), we view increased attendance rates as one plausible mechanism for improved student achievement. We find no evidence that bond passage impacts student inflows or outflows to districts, which suggests changes in student composition are unlikely to drive the test score effects.

This paper proceeds as follows. The next section describes the context of facilities funding in Texas and its implications for student outcomes. Section III describes our method and data sources in great detail, as well as presents evidence on the validity of our research design. Section IV describes how school district spending and resources are altered following successful bond passage. This section also describes how districts target their investments between campuses and how quickly improvements are made. Our main findings about the effect of bond passage on student achievement are contained in Section V. Section VI investigates effects on attendance and migration, plausible channels through which capital infrastructure could impact achievement. Section VII concludes.

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 come from district funds. 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).[5] Thus, modernization, renovations, and repairs of Texas public educational facilities are financed primarily through local property taxes with minimal state support, a setting typical of most states.

[5] Texas has a well-known school finance program, the Foundation School Program (FSP), developed to address historical disparities in per-pupil funding across districts. This policy determines the amount of state and local funding for school districts and also determines the allocation of state funds to local districts. FSP aims to ensure that all districts receive "substantially equal access to similar revenue per student at similar tax effort" taking into account all state and local tax revenues of districts, student and district cost differences, and differences in property wealth (Texas Education Code, §42.001(b)). However, FSP mainly covers operational expenditures; responsibility for facility spending falls primarily on school districts.

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. In 2010, total outstanding debt from bonds issued by Texas school 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.[6] 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 in 1997 anticipated a total of $9 billion in repairs, renovations, and new construction over the next 5 years, with critically needed repairs costing $4.1 billion (TCPA, 1998). Needs tended to be greater in heavily minority districts. In a 2006 survey, 6 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.[7]

These disparities and the overall prevalence of schools in poor condition in Texas are worrisome to the extent that physical school environments affect student outcomes. There are several reasons why such effects may exist. For instance, schools 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

[6] 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.

[7] 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.

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 presented in this paper will shed light on whether this association reflects a causal relationship.

3. Empirical Strategy

A. Regression Discontinuity with Panel Data

The ideal research design would be to randomly allocate capital spending across districts and schools. Since variation in spending would not be related to other determinants of outcomes, any association between spending and subsequent student outcomes could be interpreted as due to the spending. Of course, capital spending is not randomly allocated. As discussed in Section II, large disparities exist in levels of capital spending, with spending higher (and school facilities better) in wealthier areas. While randomizing capital investments is not feasible, our empirical strategy exploits variation in capital spending levels generated by the outcomes of close bond elections that mimic randomly varying capital spending across districts. Although on average districts in which a bond measure passes are likely to be very different from districts where bond measures fail, these differences shrink as the comparisons focus on close elections. As long as there is some randomness in the vote share in favor of bond passage, whether a bond is approved or rejected in a narrowly decided election is a randomly determined event.

Our research design builds on this insight. Just as one could attribute outcome differences between districts randomly assigned different levels of capital spending to this spending, we will attribute outcome differences between students who reside in districts where bond measures narrowly pass and fail to the post-election variation in capital spending. Our regression discontinuity analysis thus uses vote share in favor of bond passage as the "running variable" and where the cutoff that determines "treatment" status is the 50 percent vote share necessary to approve the measure.[8]

Suppose that outcome Y (such as student test scores) is observed t years after a bond election was held in district j in year t. A model for the effect of bond passage is given by:

$$Y_{j,t+\tau} = \theta_\tau Pass_{j,t} + u_{j,t+\tau} \qquad (1)$$

[8] Our approach outlined below provides estimates of the effect of bond passage. In future work we will explore using the variation in capital expenditures induced by the outcome of these close bond elections as an instrumental variable (Angrist and Imbens, 1994) that will then be used to isolate the causal effect of capital expenditures on student outcomes.

where $Pass_{j,t}$ is an indicator for whether the bond measure passed and $u_{j,t+t}$ represents other factors influencing the outcome. This 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 "τ" subscript on q). Thus, we can examine the possibility that bond passage might not have immediate effects on student outcomes or that the effects might eventually fade.

In general, districts that approve bond-funded projects might be different from those that do not in ways that are related to the outcomes of interest. For example, districts that have bond-funded school construction may serve higher-income families. Since family income is a strong predictor of achievement, simple comparisons between districts that do and do not have bond-funded school construction would provide misleading inferences about the effect of these bonds. However, Lee (2008) notes that as long as there is some randomness in the outcome, then the outcome of a "close" election is "as good as" random. This implies that in a narrow range around the vote share margin needed for passage, comparisons of the outcomes of districts that have and do not have bond-funded construction can yield unbiased estimates of the effect of $Pass_{j,t}$. Formally, we modify Equation (1) by decomposing $u_{j,t+t}$ into a flexible function of the vote share $v_{j,t}$ and other factors that affect Y, $e_{j,t+t}$:

$$Y_{j,t+\tau} = \theta_\tau Pass_{j,t} + f_\tau(v_{j,t}) + \varepsilon_{j,t+\tau} \quad (2)$$

Provided that $e_{j,t+t}$ and $Pass_{j,t}$ are uncorrelated, unbiased estimates of q_t can be obtained by regressing Y on $Pass_{j,t}$ and a flexible function of $v_{j,t}$, which is permitted to differ with time since bond passage.[9] In practice, this assumption means that districts in which voters narrowly approve bond measures are not systematically different from those in which voters narrowly reject bond measures. Below we show evidence consistent with this condition.

Following CFR (2010), we estimate (2) 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 chose 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 three different sets of windows: t-2 through t+6, t-2 through t+10, and t-10 through t+10. Narrow windows have the benefit

[9] This discussion glosses over two important details. First, the effect of bond-funded construction is likely to be heterogeneous rather than constant. The regression discontinuity approach estimates the effect for districts close to the bond passage threshold (formally, it will be a weighted average effect of $Pass_{i,t}$ where the weights are increasing in the probability that a district has a close election (Lee, 2008)). In analysis available upon request, we find that districts in close elections are quite similar to all districts that held elections. Second, the function $f(v_i)$ hasn't been specified. As described below, we follow Imbens and Lemieux (2008) and Lee and Lemieux (2010) and use parametric and local linear regression methods.

of using more balanced panels, though larger windows permit us to examine effects and pre-trends over longer periods of time.[10]

To improve precision, our preferred specification alters (2) by controlling for fixed-effects that account for heterogeneity across districts and over time. In particular, we estimate a model of the form:

$$Y_{j,t+\tau} = \theta_\tau Pass_{j,t} + f_\tau(v_{j,t}) + \mu_{j,t} + \alpha_{t+\tau} + \delta_\tau + \omega_{j,t+\tau} \qquad (3)$$

where a_{t+t} and d_t are calendar and relative year effects, respectively, $m_{j,t}$ is a district-election fixed-effect, and $w_{j,t+t}$ is a random error term. Note that $m_{j,t}$ will control for fixed differences across districts. While this is not necessary to eliminate bias, district-election fixed effects should improve estimate precision and will control for changes in sample composition due to the unbalanced panel. 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 tnot vary within an elnd passage is constrained to zero in the pre-election period. In addition, we will also estimate equation (3) 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. Thus models without vote share controls can be thought of as a "difference-in-differences" or interrupted time-series model.

B. Multiple Elections and "Treatment on the Treated"

The method described above will uncover the causal effect of bond passage in a given year on outcomes in subsequent years. However, since districts can (and do) hold elections in multiple years, this "intention to treat" (ITT) combines both a direct and indirect effect (via subsequent election outcomes). Another way of saying this is that some of the "control" districts (those whose bond measure does not pass) are eventually "treated," thus our setting is akin to that of a "fuzzy" 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.[11]

$$Y_{j,t} = \sum_{\tau=0}^{\bar{\tau}} \left(\theta_\tau Pass_{j,t-\tau} + \emptyset_\tau Elect_{j,t-\tau} + f_\tau(v_{j,t-\tau}) \right) + \mu_j + \alpha_t + u_{j,t} \qquad (4)$$

[10] Since multiple observations per district are included, we adjust all standard errors for clustering at the district level.
[11] Vote share is set to zero for observations in which no election was held.

This model is estimated on a standard district-year panel, including all years from 1994 to 2011.[12] 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 the ITT estimates, though at times present TOT estimates for comparison.

C. Campus-level Analysis

We conduct three variants of this RD approach using campus-level data. The first is a cross-sectional analysis using data from a 2006 survey of public school facility condition (described below). Specifically, we use data on all elections held prior to 2006 and estimate the model:

$$Y_{cj,2006} = \theta Pass_j + f_{2006}(v_j) + \varepsilon_{cj2006} \quad (5)$$

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. 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 in the same district based on school age.

The second campus-level model is designed to distinguish between the effect of bond passage operating through renovations of existing schools and the opening of new schools. Specifically we estimate the model:

$$Y_{cgj,t+\tau} = \theta_\tau Pass_{j,t} + f_\tau(v_{j,t}) + \mu_{cgj,t} + \alpha_{t+\tau} + \delta_\tau + \omega_{cgj,t+\tau} \quad (6)$$

on a dataset of comprised of cells defined by the interaction of campus, grade, and low-income, where c again indexes campuses and g indexes grade x low-income cells. The term $\mu_{cgj,t}$ is a fixed effect for each campus-grade-economic cell. Since campuses built after the election (possibly as a consequence of the election) will have no pre-election data, they will not contribute to the estimation of θ_τ. Thus in this model, the identification of θ_τ will be driven entirely by the effect of bond passage on the renovation of existing schools.

Third, since we expect renovations to be more likely to occur at older campuses, we stratify campuses by age at baseline and test for differential effects of bond passage by campus age. To implement this, we estimate a variant of (6) where $Pass_{j,t}$ is interacted with groupings for campus age at the time of the election.

4. Data

A. Data Sources

Our analysis draws on four sources of data at the student, district and campus levels.

[12] CFR (2010) also present an alternative "recursive" estimator of the TOT effects. In practice, the one-step and recursive estimates are quite similar, though the former is more precise.

Bond election data. From the Texas Bond Review Board, we acquired data on the 2,277 separate school bond propositions put up for a vote by Texas public school districts from 1996-2009. This data contains election date, bond amount, purpose (e.g., school building, renovations) and result (passed or failed). Via public information requests, we then collected and hand-entered vote share data from 812 school districts (98% of districts holding elections), along with supplementary documentation (e.g. School board minutes). About 20 percent of the time, these districts held multiple elections on the same date. 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. Whenever there were multiple elections, 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, and these elections form the basis of our analysis sample. Between 1996 and 2009, there were 1,737 such elections, so that on average districts held elections about twice during our study period. Table 1 provides descriptive statistics about these elections. Voters approved 80% of these bond measures, with an average vote share of 64%. The mean bond amount was $11,000 per student (in $2010).

District- and campus-level longitudinal data. From Texas Education Agency's Academic Excellence Indicator System (AEIS) data system, we obtained a number of district- and campus-level characteristics for each year from 1994 to 2011. District-level measures include 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. From similar campus-level data, we also constructed district-level measures of the interquartile range of student-teacher ratio by campus type. To this, we merged annual data on school finances (e.g. capital outlays and instructional expenditures per student) at the district-level from the Common Core Data. Unfortunately, campus-level measures of capital investment are not available from any data source we are aware since capital spending is budgeted and spent by districts, even if it is targeted at specific campuses. However as we explain below, we use campus-level markers of facility conditions to examine which types of campuses were targeted by capital investment.

Student achievement and attendance data. Our primary outcomes are standardized test scores and school attendance that come from student-level TEA records.[13] We focus on reading and mathematics scores for students in grade 3 to 8, as these are available for the entire study period. 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 by grade and year. To examine attendance, we calculate the fraction of days each student is in attendance in each academic year.

[13] In the future we will also examine high school graduation and indicators for various types of disciplinary actions taken for the same student population. The student-level data come from the administrative records of the University of Texas at Dallas' Texas Schools Project database.

Campus-level cross-sectional data on school facilities. Our final data source is detailed information on school facilities conditions at a single point in time (2006). This data come from a voluntary survey conducted by Texas Comptroller of Public Accounts. For each separate facility, districts were asked to provide information about the general condition (Excellent, Good, Fair, Poor, needs replacement), enrollment, year built, year of most recent major renovation (if ever), square footage, number and square footage of portable buildings, and total student capacity. This survey was obtained from 302 districts including 3548 instructional facilities (accounting for about half of the state's student population), though we focus on the subset of districts holding school bond elections prior to 2006.

B. Analysis Sample and Summary Statistics

The microdata underlying our analyses includes individual-level test score and attendance data for all 3rd through 8th graders tested from 1994 to 2011. We aggregate these outcomes to the campus and district level in various ways to incorporate in our analysis, though data disclosure concerns require us to take certain precautions when constructing these aggregates. We calculate the mean, standard deviation, and number of observations for student groups defined by campus X grade (3rd through 8th) 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 (overall and for various subgroups) 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.[14] Separately, we use the full individual-level microdata to construct measures of test score and attendance deciles in each district and year to assess how the full distribution of outcomes is altered by capital investment. These distributions combine students from all grades and economic status group, but are only reported for districts with at least 100 tested students. For some specifications, we compare estimates using our cell-aggregate outcomes and those using the full individual microdata.

For our district- and campus-level longitudinal data, the unit of observation is the election-year level. We construct the time series for each of the 1,737 unique elections by merging all district and campus-level data from years before and after the election. The longitudinal dataset is then formed by "stacking" all of the election-specific time series. Since elections can be held in the same district in different years, a district will sometimes appear in the longitudinal data multiple times in the same year, as the lead for an earlier election held in that district or as a lag for a later election held in the district. Consequently, district-level regressions will typically have 1737 observations for each year of the panel.

[14] We do not obtain the district-level mean as that would potentially allow us to back out the mean for a non-disclosed group.

Summary statistics. Table 2 summarizes our data. Means and standard deviations of district characteristics and outcomes at baseline (year prior to election) are presented for the 1737 elections overall and separately by the bond election outcome. 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. The final column depicts characteristics for the entire panel dataset, which includes two years prior and up to six years after each election.

C. 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 would have happened in the districts in which a bond measure narrowly passes had instead the measure been rejected by voters. This assumption would be violated if there was some manipulation whereby districts were able to directly affect whether a close bond measure narrowly passed or failed. 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 noted by McCrary (2008), if it is not, then that would suggest that bond measure outcomes are not random at the cutoff. To assess whether there was any such manipulation, we implanted the test proposed by McCrary. The estimated discontinuity (the difference in the log of the kernel density estimate) is 0.227 with a standard error of 0.164, which is not statistically different from zero at conventional levels of statistical significance. Moreover, the graphical evidence in Figure 1, which shows the histogram of the vote shares, does not reveal any discontinuities at the 50 percent cutoff.

Next, we investigated whether district-level covariates trended smoothly through the 50 percent cutoff. Again, the presence of abrupt changes in these characteristics at the cutoff would suggest that there are systematic differences between the districts where the bond measures narrowly passed and those where the measures narrowly failed. To test for any discontinuous changes in district characteristics at the threshold we estimated equations (1), (2) and (3) using various pre-election characteristics as the outcome. The results (presented in Table 3) generally are consistent with the claim that baseline covariates trend smoothly through the 50 percent threshold.

The first three columns of Tables 3A and 3B report estimates of equations (1) and (2) using data just from the year prior to each election (thus these columns have 1737 observations). Few covariates have a discontinuity that is statistically significant once a polynomial (linear or cubic) of the vote share is controlled for. The next five columns include observations for two years prior and six years after each election and include interactions between bond passage and year relative to election. Unlike our main outcome analysis in the following sections, here we do not constrain the coefficient on the passage X year-prior-to-election interaction to zero. This table reports the estimate of this interaction. Specification (6) includes controls for bond election fixed-effects and a linear function in the vote share (the preferred specification in the main analysis). The estimated discontinuities are mainly small and statistically insignificant. 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.[15] Importantly, pre-election differences in all our main outcomes are small and insignificant. Below we also examine wider analysis windows in order to identify any spurious pre-trends.

5. Nature and Timing of Capital Investment

One of the main holes in existing knowledge on this topic is that there exists little systematic evidence on how capital spending is allocated. Thus we begin the analysis of the effect of capital investment by examining how bond passage affects the allocation of expenditures and resources. We focus on two types of investment – new school construction and renovations to existing schools – and also examine intervention timing, as this has implications for impacts on student outcomes.

A. District-level spending

Figure 2 depicts our main findings on district-level spending. This figure plots the average spending in various categories before and after each election, separately by vote share. The left 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 not much relationship between pre-election spending and the vote share. In the year following the election, however, capital spending is about $2000 per student higher in districts where the bond barely passed compared to those in which it barely was rejected. Moreover, we see no such relationship between bond passage and spending for other categories.

Table 4 presents estimates of equation (3) and confirms the visual evidence seen in Figure 2. In Panel A, we find that 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 in the second year after the election but are small and statistically significant thereafter, suggesting that increased capital investments occur shortly after the election. These results hold across various specifications. The impact of bond passage on capital spending might be larger in small school districts since there are fewer schools to which the funds can be directed than there are in larger districts. We find evidence consistent with this hypothesis. For districts with four or fewer schools in the year

[15] In future analysis we will probe the robustness of our findings to controls for these covariates.

of the election (typically 2 elementary schools, one middle school, and high school), the estimates in the last row of Table 4 suggest that bond passage results in a larger increase in per-pupil capital spending than occurs in larger districts. This is in spite of the fact that average per-pupil capital spending is similar in the larger and smaller districts.

Although the school bonds are explicitly targeted for capital investments, bond passage could increase spending on other school expenditure categories. Estimates in Panel B suggest minimal effect on instructional spending. Bond passage has no impact on instructional spending per student, immediately or longer term. This suggests strong flypaper effects: bond passage increases capital spending (as intended) but does not spill over into other forms of spending.

One interesting result in Table 4 is that, in some specifications, it appears that bond passage leads to lower levels of capital spending six years after the election. One reason for this could be that bond passage today leads to a reduction in the likelihood of bond passage at some point in the future. To examine this possibility, Figure 3 presents estimates of the effect of current bond passage on the likelihood of holding and passing a subsequent election. As expected, districts whose elections are successful are much less likely to hold or pass an election within four years, but the effect dissipates after that. Consequently, the ITT estimates presented in Table 4 likely understate the capital investment that follows successful bond passage. Figure 4 compares the ITT and TOT estimates ten years following bond passage. These results suggest that, holding subsequent elections constant, bond passage today has a positive effect on capital spending through year 4, essentially zero effect in years 5-8, and a negative (although imprecisely estimated) effect in years 9 and 10.

B. <u>School openings, closings, and student-teacher ratio (at district level)</u>

To examine how this capital is invested, the first outcomes we examine are indicators for whether the district opened or closed a new/old elementary, middle, or high school in the year and average student-teacher ratio. Panel A of Table 5 reports estimates of equation (3) for school openings and closings. We report estimates from our preferred specification that uses data for two years before and six years after the election, includes a linear function of the vote share, includes district-election fixed effects, and constrains coefficients for pre-election years to be zero. We see that bond passage is associated with a 13 percentage point increase in the likelihood of opening a new school of any type (on a base of 23%) in the second year after the election and an increase of 9 percentage points of a school opening in the third year after the election. These effects are strongest for elementary schools. Opening rates return to their pre-election levels by year 4. Though not shown, this finding is robust to the inclusion of a cubic in the vote share. The closing rate does not seem to be much affected by bond measure passage, though the closure of elementary schools does increase three years after bond passage (suggesting that some old schools are replaced with new buildings). Since the effect on openings is consistently greater than the effect on closings, this implies that bond passage is associated with a net increase in the number of schools. Figure 5 compares these ITT estimates to the TOT estimates and over a longer time horizon. Accounting for subsequent elections suggests that building construction lasts slightly longer than suggested by the ITT estimator.

In the bottom panel of Table 5, we examine one measure of crowding: the ratio of students to teachers. We find no evidence that bond passage is associated with a reduction in district-level class size. This same pattern is true for elementary, middle, high school and various polynomials in vote share. 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.[16]

C. School conditions, crowding, and use of portable classrooms

Using school-level data on the condition of nearly 3,000 of the state's public schools in 2006, we now assess the effect of bond passage on several more specific measures of the facility environment. Before examining the effect of bond passage per se, we observe that there is strong relationship between facility age and whether it is in at least fair condition or at least good condition (as of 2006). "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." We believe these are important markers of the facility's ability to either hamper or support student learning. Figure 6 plots the fraction of buildings that are in "Fair" (left panel) and "Good" (right panel) condition as a function of facility age. The solid line in each panel depicts all schools in the sample. General building conditions deteriorate rapidly as buildings become more than about 20 or 25 years old.

Figure 6 also provides suggestive evidence that bond passage has a positive effect on facility condition. Among schools that are the exact same age, those in districts that had previously passed a bond measure were in much better condition. This claim finds further support in Table 6. Old schools in districts whose bond election was successful are about 19 percentage points more likely to be in at least fair condition and 20

[16] We also find no effect of bond passage on student-teacher ratios when we examine effects separately for schools whose student teacher ratio was greater than the within district median (of the same type) at baseline and by whether a school's share of economically disadvantaged students was greater than the within district median (of the same type) at baseline. We also find little evidence of effects on the student-teacher ratio gap between 75-25 percentile of among schools in the district. However, we do find that middle and high schools with a large fraction of disadvantaged students experienced a decrease in average class size 3-4 years post-election (and the effect for middle schools is significant).

percentage points more likely to be in at least good condition than old schools with unsuccessful bond elections (column (3)). There appears to be no effect of bond passage for younger schools. This relationship is very robust to various polynomials in vote share and is similar for elementary, middle, and high school (though less precise). In specification (8) we control for district fixed effects, which assesses whether the condition difference between old and new schools in the same district is smaller in districts that passed a bond election. The magnitude of the effect is smaller, yet still sizable and significant for "Good" condition. Using a similar specification, Table 7 examines the effect of bond passage on other attributes of schools. For older schools, bond passage is associated with a greater likelihood of having a major renovation, a reduction in effective age (years since last renovation), and a reduction of maintenance needs. There is no obvious relationship between passage and fraction of space in portables or square foot per student, though bond passage is associated with lower occupancy rate (enrollment / capacity) for older facilities. All of these results are unaffected by inclusion of vote share polynomials (not shown). Collectively this analysis suggests that bond passage greatly improves the building conditions of schools, though this impact is concentrated in older schools.

Finally, Figure 7 examines the timing of improvements to existing campuses using the 2006 survey data described above. Though the data is cross-sectional, we exploit the fact that campuses are observed in 2006 with different lags since the most recent bond election.[17] We find that the improvement in overall building conditions, effective building age, portable use, and several measures of crowding all show the most improvement four to five years after a successful election. Comparing these results to those in Figure 5 suggests that improvements to existing facilities appear to follow school openings.

D. Summary: Bond-Passage Affects Schooling Environments with a Lag

The results in this section provide some of the first evidence demonstrating that facility investments funded by school bonds leads to tangible improvements in schooling facilities. These effects occur with some lag following the approval of school bond measures. The results depicted in Figure 4 indicate that capital investment generated by bond passage occurs in the four years following bond passage.[18] 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 (Figure 5) and the largest effects on improvement of existing conditions occurring four to five years after an election (Figure 7).

6. Achievement Outcomes

[17] The drawback of cross-sectional data is that we are not able to separate timing and period effects. Thus we cannot rule out that our timing results are due to differences in unobserved district characteristics that are correlated with the year a district held their election.

[18] It should be noted that this timing is a bit more rapid than that observed in CFR (2010).

The results in the preceding section provide some of the first evidence that facility investments funded by school bonds leads to tangible improvements in schooling facilities, but do so with a lag. Thus approval of school bonds could affect student outcomes if school facilities are an important element in the educational production function, but they also indicate that any such effects would occur with some lag. Indeed, better facilities could take time to impact student outcomes as teachers, students, and parents adjust to new settings. Because we do not know the lag structure for how improvements in school facilities affect student outcomes, in the analysis below, we permit bond passage to impact student outcomes for many years.

A. Overall achievement outcomes

Figure 8 previews our main test score results with an approach similar in spirit to our regression analysis. We plot the change in overall district student test scores before and after bond passage, separately by the vote share in favor of bond passage.[19] Test score gains following elections are quite noisy, though more positive for elections that were successful. Given the noisiness of these test score outcomes, we now turn to our regression analysis to better probe the robustness of these results. Table 8 presents our main test score results. Each row presents estimates of equation (3) for a separate district-level outcome.[20] We show overall results, as well as by free/reduced-price lunch status, and the mean test score gap by economic disadvantage status (as measured by eligibility for free or reduced price lunch). The table reports coefficients on dummies with school bond passage interacted with year relative to the election year (so that the estimates for "*t* years" reflect the impact of bond passage in the *t*^th^ year following the election). All models use a linear function of the vote share and include fixed effects for each election, relative year fixed effects, and academic year fixed effects.

Mirroring Figure 8, we find that bond passage is associated with modest reading and math scores gains, particularly for poor students after six years. By year six, bond passage is associated with 0.050 standard deviations gain in average reading scores and a 0.067 standard deviation gain in math scores for students eligible for free lunch; both are significantly different from zero at the 5% level. Test score effects overall and for non-poor students (not eligible for free or reduced price lunch) are also positive, though smaller in magnitude and not significant at conventional levels. The magnitudes we find are roughly consistent with the magnitude reported in CFR (2010) in their analysis of 3rd grade scores of students in California and in Hong and Zimmer (2014) for Michigan. Figure 9 expands our analysis time from six to ten years post-election, presenting estimates of the effect of bond passage on district-level average math scores for poor and non-poor

[19] In this figure we use the average of test scores six to eight years after an election as our post-period. This choice was informed by the lag structure of treatment effects observed in our regression results, described below.

[20] District- and district-group 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. Students in grades 3 to 8 are included.

students. Though precision decreases for later lags, the test score effects observed after six years do appear to persist through year ten. Furthermore, average gains for non-poor students continue to increase past year six and are statistically significant after year seven. Results for reading test scores are similar.

The bottom row of each panel in Table 8 presents estimates of the effect on the test score gap between poor and non-poor students. Within each district, students eligible for free lunch have test scores that are, on average, a half of a standard deviation lower than students not eligible for free or reduced lunch. If capital investment disproportionately targeted and benefited poor students in each district, this achievement gap may shrink following bond passage. Though point estimates for most lags are negative (implying poor students gain relative to non-poor students from bond passage), none is significant. Furthermore, relative to the baseline achievement gap of half a standard deviation, estimates are small in magnitude.

B. Pre-trends and Robustness

Figure 10 plots estimates from the ITT model using a window that includes more pre-election observations in order to examine whether there are longer-term trend differences between districts with successful and unsuccessful bond elections. We see no obvious evidence of trend differences prior to the bond election, though estimates are admittedly imprecise over this long window.

Figure 11 examines the robustness of our main test score results to various functional forms for the vote share function. Our main finding, that average test scores of poor students increase by about 0.05 standard deviations six years after bond passage, is generally robust across various vote share polynomials (though estimates using more flexible functions are less precise). Table 9 includes point estimates and standard errors for all these specifications. Excluding fixed effects for each election generally reduces our point estimates substantially and decreases our precision. Thus inclusion of election fixed effects is an important specification decision.[21] The last row of each panel restricts analysis to the 809 elections (492 unique districts) held by districts with four or fewer campuses in the year of the election. Point estimates for economically disadvantaged students are somewhat larger for reading and smaller for math scores than the full sample, yet are less precise due to the smaller sample size. Finally, Figure 12 presents estimates of TOT effects, estimated with equation (4). Point estimates and significance are actually quite similar between ITT and TOT.

C. Impact on District-level Test Score Distributions

To assess whether capital investment benefits high- and low-achieving students similarly, Table 10 presents estimates of effects on various other moments of the distribution of test scores. For any district-year in which

[21] In future analysis, we will more formally determine the appropriate degree of polynomial by testing for the significance of higher order terms and using AIC statistics. We will also explore robustness to changing the bandwidth around bond passage. Furthermore, we will try to better understand the importance of controlling for election fixed effects by, for instance, looking for any outliers and controlling for observed (fixed) district characteristics in place of district fixed effects.

at least 100 students are tested (combining grades 3 through 8), we construct deciles of the test score distribution, as well as the gap between the 90th and 10th percentiles of the distribution and use these moments as our outcomes. We find suggestive evidence that achievement gains are greatest at the bottom of the distribution, thus bond passage (and the subsequent capital investment) modestly closes the 90-10 test score gap after six years. However, the point estimate are modest: students at the 10th percentile gain only about 0.05 standard deviations in math (0.03 in reading) relative to those at the 90th percentile and the math effect is only approaching significance ($p = 0.085$). Longer-term estimates in Figure 13 suggest that gaps continue to close in subsequent years, though the magnitudes remain small.

D. New versus Existing Campuses

Improvements in overall district-level test scores can be decomposed into test score improvements at existing schools and the test score gains associated with the opening of new campuses. Since capital investment is used for both purposes, the combined effect does not tell us whether it is more effective to renovate existing schools or open new ones. Previous research on school capital investment is silent on this issue, as it either focuses exclusively on students attending new campuses (Neilson and Zimmerman, 2012) or the combined effect of investments in new and existing schools (Cellini, Ferreira, Rothstein, 2010). To address this, we take advantage of our access to sub-district test score data. Table 11 presents estimates from models similar to the district-level analysis reported above, but using campus-grade-economic group cells as the unit of analysis.[22] All regressions are weighted by the share of total district enrollment represented by the cell in the given year, so that each district receives equal weight regardless of the number of cells (or campuses) included in it. When weighted in such a way, this regression is mathematically equivalent to the previous district-level regressions. The first row of each panel replicates our main district-level results for the purpose of comparison (albeit estimated using cell-level data), including a linear vote share and election fixed effects.[23]

The second row of each panel includes a separate fixed effect for each campus-grade-economic cell. This specification isolates the test score gains of existing facilities because only those cells with observations both before and after a given election will contribute to the identification of the effect of passage. For both test scores and economic groups, these estimates are comparable to the overall effect, suggesting test score gains for disadvantaged students even at campuses that existed before and after an election. The final two rows of each panel provide an alternative approach. They estimate similar models, but on the sample of cells that

[22] More specifically, each cell represents an election X campus X grade X economic group, since a given campus X grade X economic group cell can be included multiple times for the same year for districts that hold multiple elections in close proximity. In this discussion we omit the "election" aspect of the cell for expositional ease.

[23] Though point estimates from district- and cell-level analysis are identical, standard errors are very similar, though not identical. Generally the cell-level analysis provides standard errors that are just slightly more precise than those from the district-level analysis, which we are investigating.

existed in the year of the election. Again test score gains are very comparable for this restricted sample. Together these results suggest that the modest test score gains for low-income students we observe following bond measure passage are not driven entirely by the opening of high-achieving schools or the closing of low-achieving ones. Rather, test score improve for these students even at existing schools.

E. Attendance Outcomes and Student Migration

School attendance is one important channel through which physical school environments may influence student achievement. Faulty heating and ventilation systems may cause school closures, old building materials may cause student health problems, or students may be more likely to skip school if the building is in disarray (which may only be relevant for middle school students in our sample). Table 12 and Figure 14 both show that bond passage is associated with a narrowing of the attendance gap between poor and non-poor children (though this is not significant) and a narrowing of the attendance gap between the top and lowest decile of students by about 3 percent (0.23 percentage points).

Finally, we rule out selective migration by students as a major explanation for our results, Figure 15 depicts the effect of bond passage on in-migration rates. If bond passage attracted more students to the district or compelled some to leave, our test score results could be driven by changes in the composition of students. We find no evidence that the in-migration rate changed appreciably following bond passage, overall (not reported) or separately by economic subgroup.

7. 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 did 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 barely were 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 the educational facility conditions experienced by Texas' public school students. The money goes towards the opening of new campuses quickly (within 2 or 3 years of bond passage), sometimes in conjunction with campus closings. These changes appear to have very little impact on student-teacher ratios overall, by baseline school characteristic, or by subject. However, overall building conditions improve considerably within four or five years after bond passage, particularly for older schools.

These improvements do appear to translate to modest increases in student achievement. The test scores of low income students improve 0.05 to 0.07 standard deviations within six years of bond measure passage and these gains appear to persist for several years. Since non-poor students also benefit modestly from bond passage, the achievement gaps between poor- and non-poor students and between the highest- and lowest-achieving students close only minimally. At the same time, we find no indication that these gaps widen despite the possibility that bond-funded capital investments could serve to exacerbate disparities in schooling environments. These modest achievement gains are observed even at existing campuses, so the gains are not driven entirely by changes in the composition of schools, such as by the opening of high-achieving schools or the closing of low-performing ones. Furthermore, we find that bond passage reduces the gap in daily attendance between poor and non-poor students but does not impact cross-district migration.

How the return to capital investments compares to other uses of school spending remains an open question. However, our results suggest that capital investments can have real positive impacts on the achievement of students.

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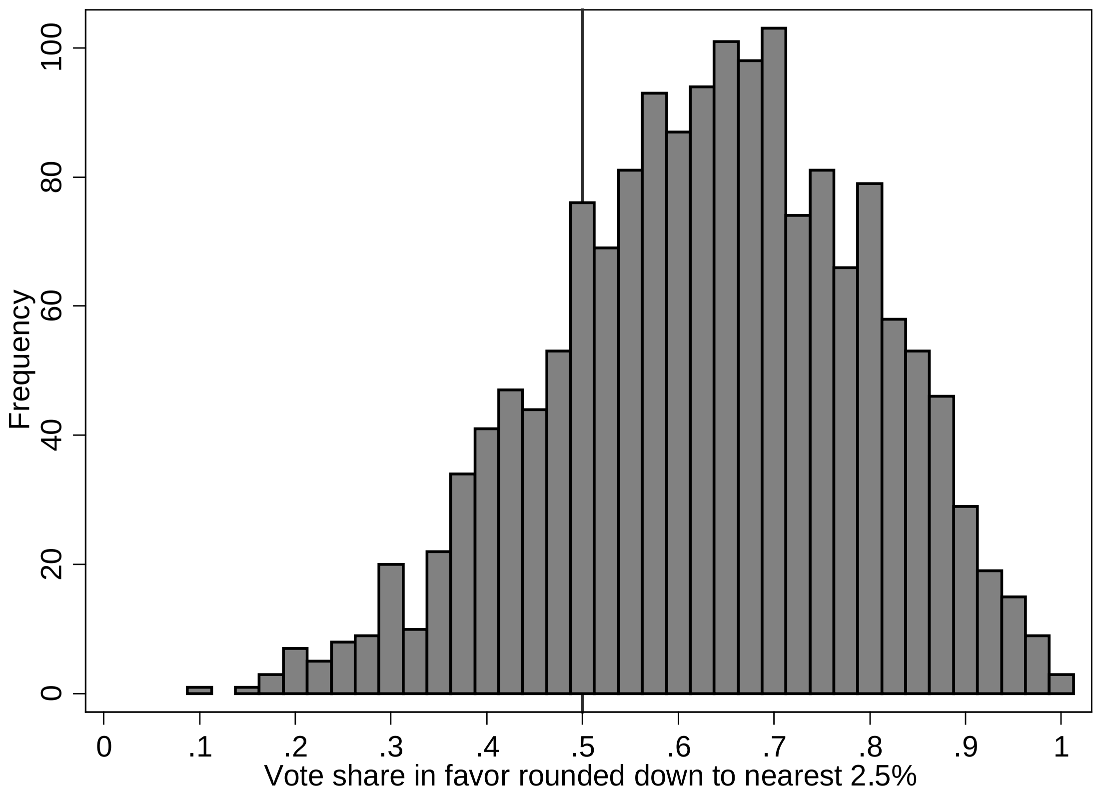
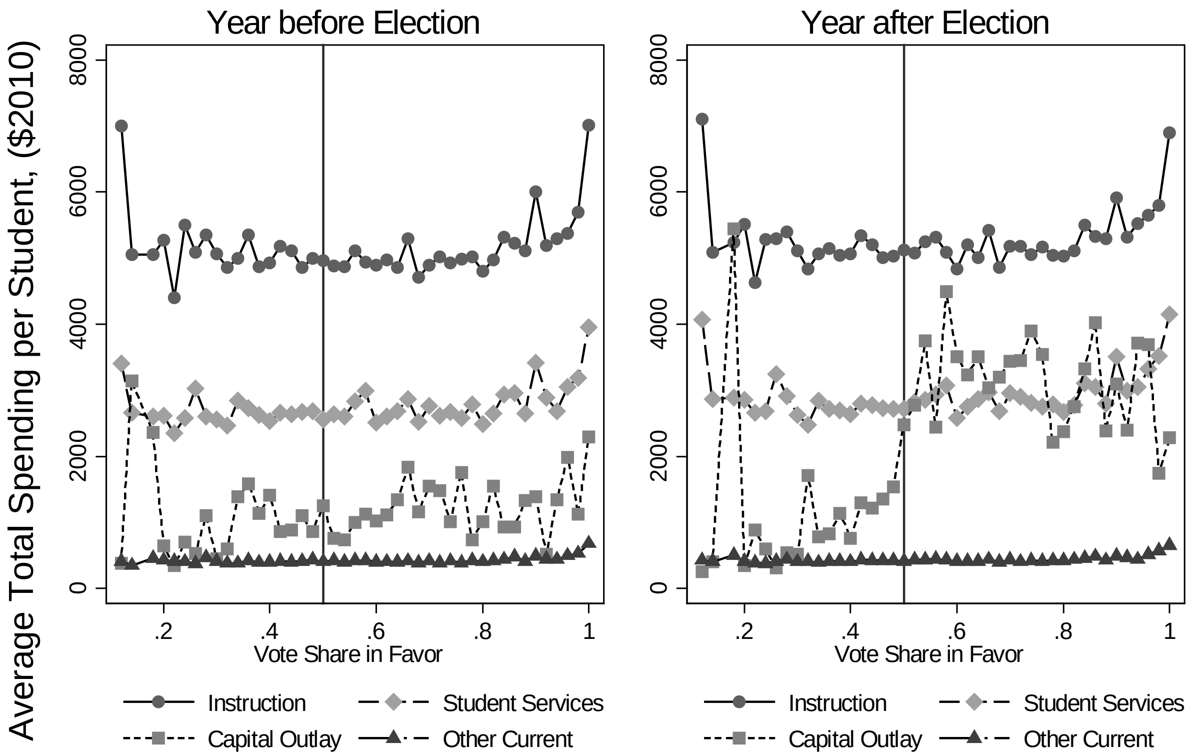
Figure 1. Histogram of Vote Shares



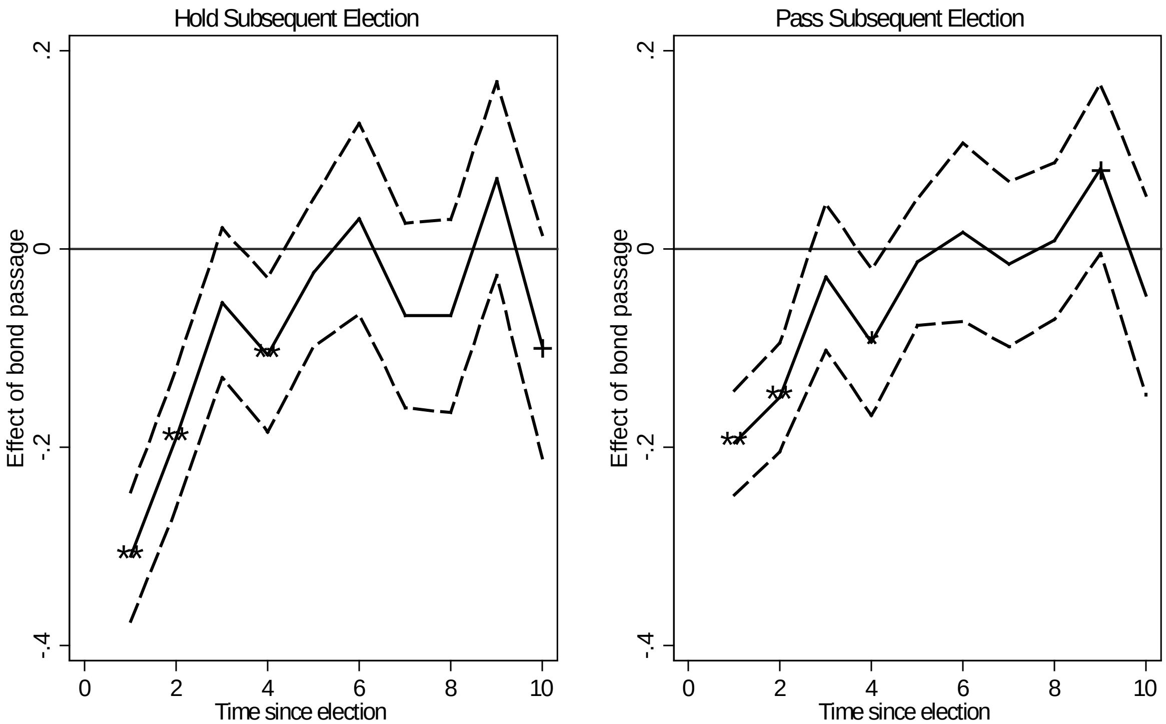
Notes: Graphs frequency of election, where elections are grouped in 2.5 point bins of vote share. Includes data for 1737 elections and 812 districts.

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



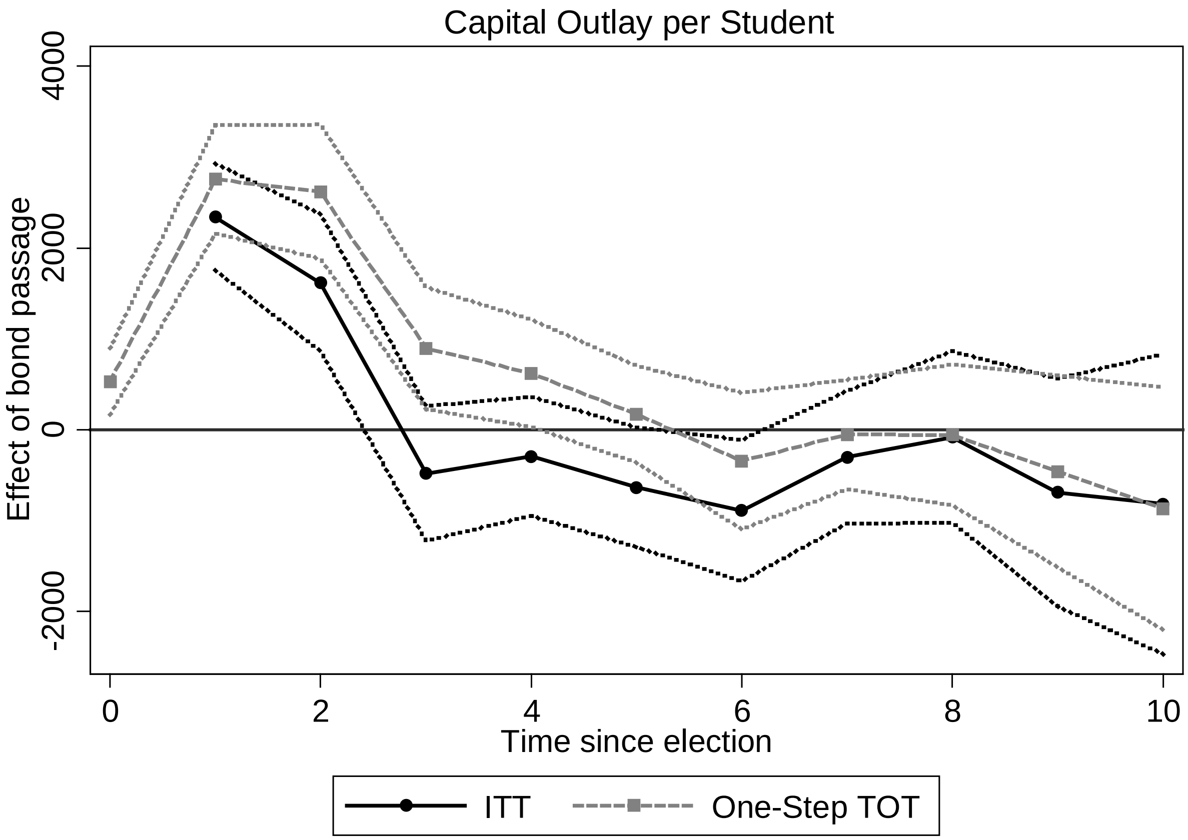
Notes: Graphs plot average district spending in each category, separately by the vote share in favor of bond passage. Elections were grouped in 2.5 point bins of vote share. Includes data for 1737 elections and 812 districts. Spending data is from the NCES Common Core.

Figure 3. Effect of Bond Passage on Likelihood of Holding or Passing Subsequent Election



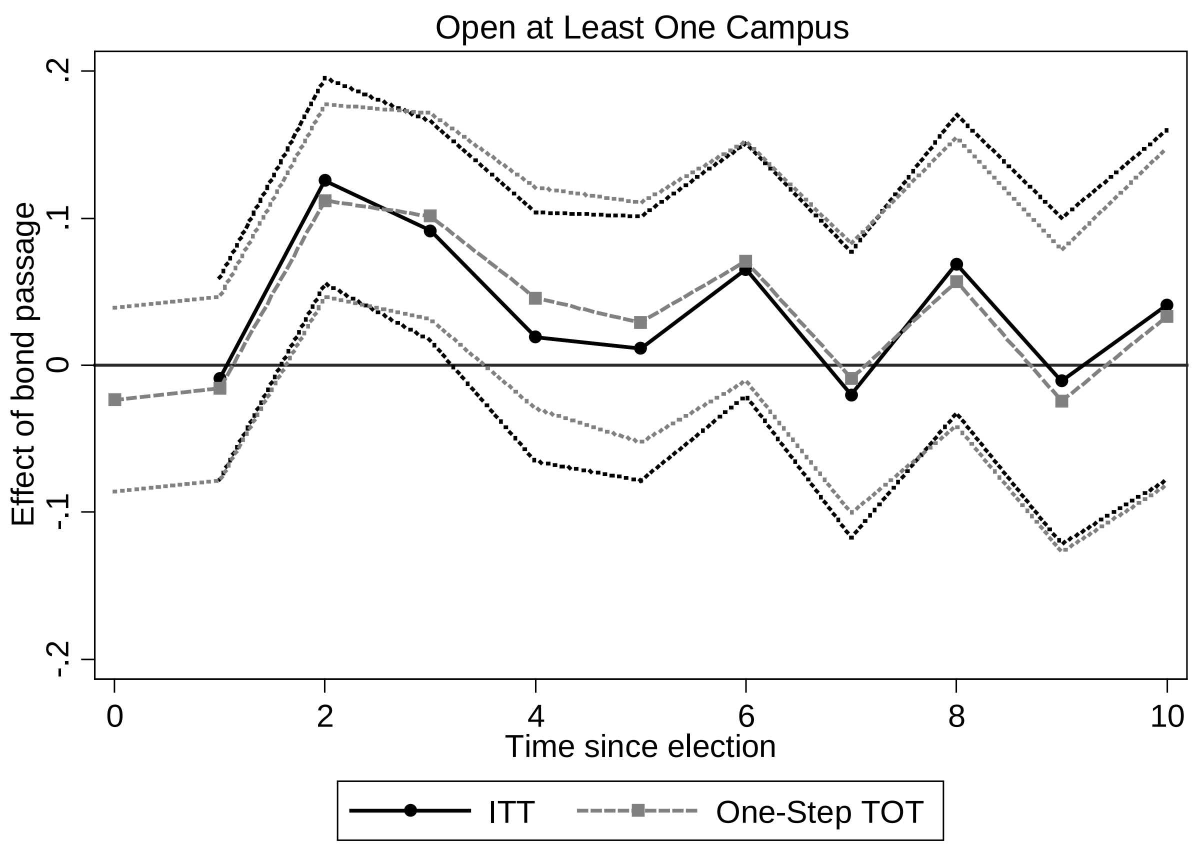
Notes: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on indicator for holding (passing) another bond election one through ten years following bond passage. Specification pools observations two years before through ten years after each bond election and includes fixed effects for each separate election and a linear function of the bond measure vote share, as described in Section IIIA and equation (3). 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.

Figure 4. Effect of Bond Passage on Capital Spending: ITT vs. TOT



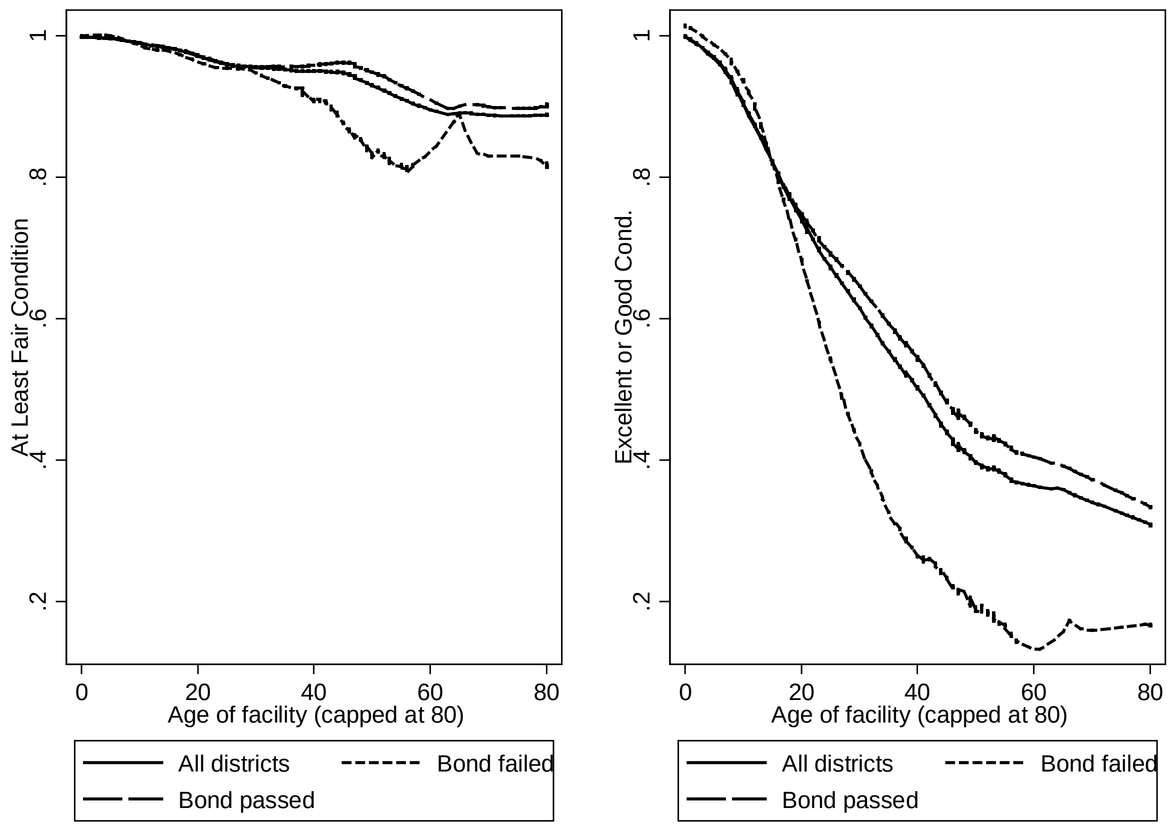
Notes: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on capital outlay per student one through ten years following bond passage. ITT specification pools observations two years before through ten years after each bond election and includes fixed effects for each separate election and a linear function of the bond measure vote share, as described in Section IIIA and equation (3). One-step TOT estimates pool all years of data in the district-year panel and includes indicators for holding and passing bond election during the current and all previous years, the vote share (linearly) for elections held in all previous years, and fixed effects for districts and years.

Figure 5. Effect of Bond Passage on Opening a New Campus



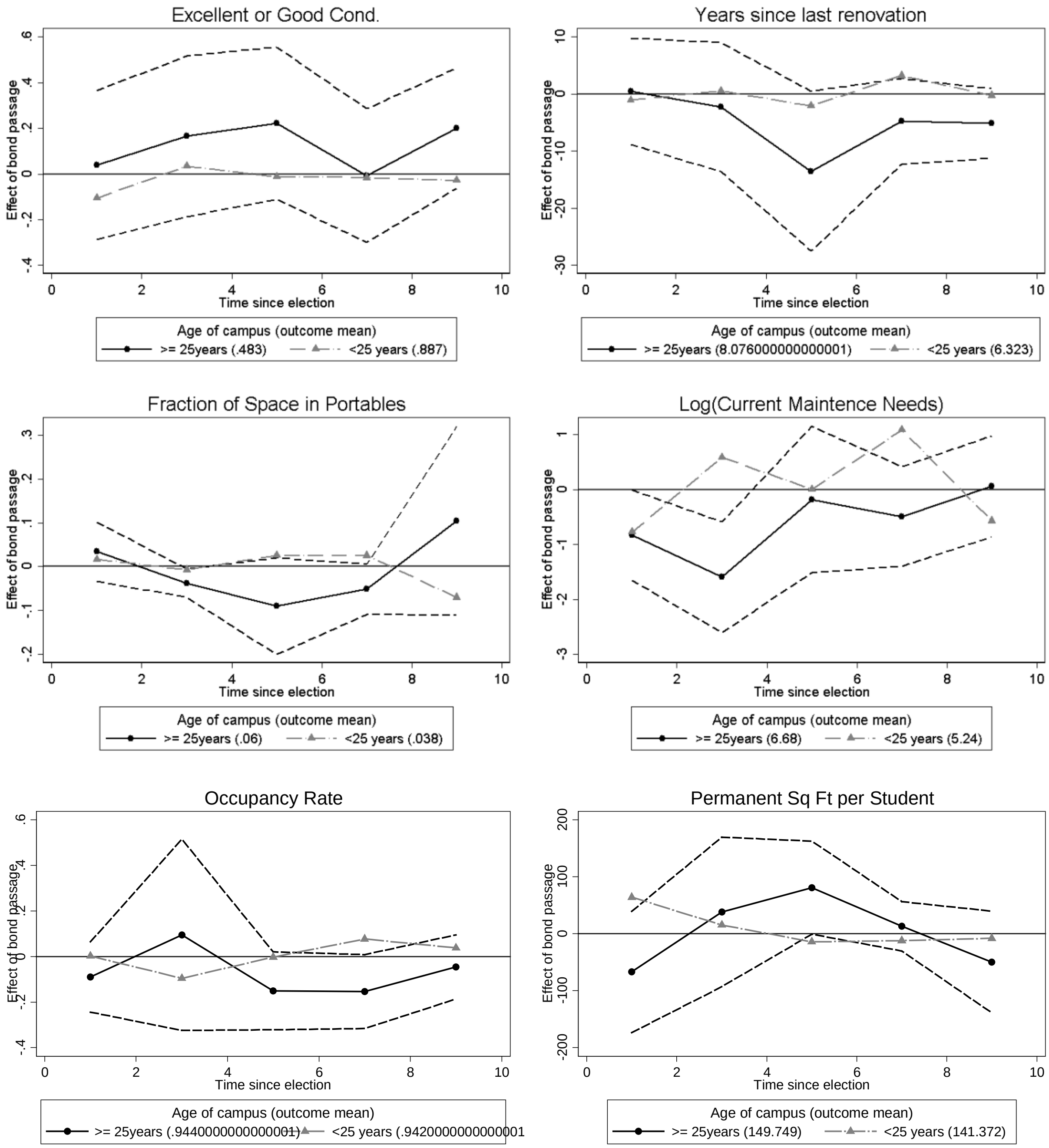
Notes: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on likelihood of opening at least one campus in the district one through ten years following bond passage. ITT specification pools observations two years before through ten years after 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 IIIA and equation (3). One-step TOT estimates pool all years of data in district X year panel and includes indicators for holding and passing bond election during the current and all previous years, the vote share (linearly) for elections held in all previous years, and fixed effects for districts and years.

Figure 6. Overall Facility Condition, by Age of Building and Earlier Election Outcome



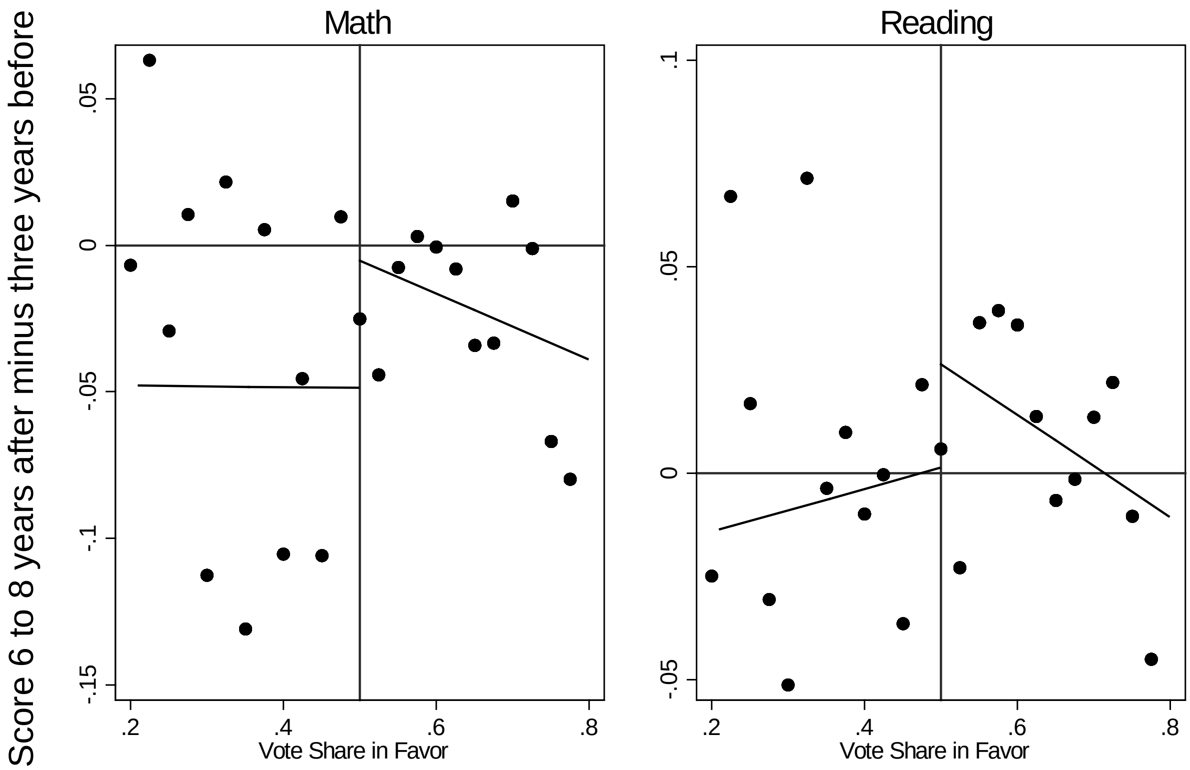
Notes: Graphs plot lowess estimates of the relationship between building condition and facility age. Dashed lines separate relationship by whether the earlier school bond passed or failed. Includes 204 unique districts, 573 unique bond elections, and 2,895 unique campuses.

Figure 7. Timing of Facility Improvements Following Bond Passage



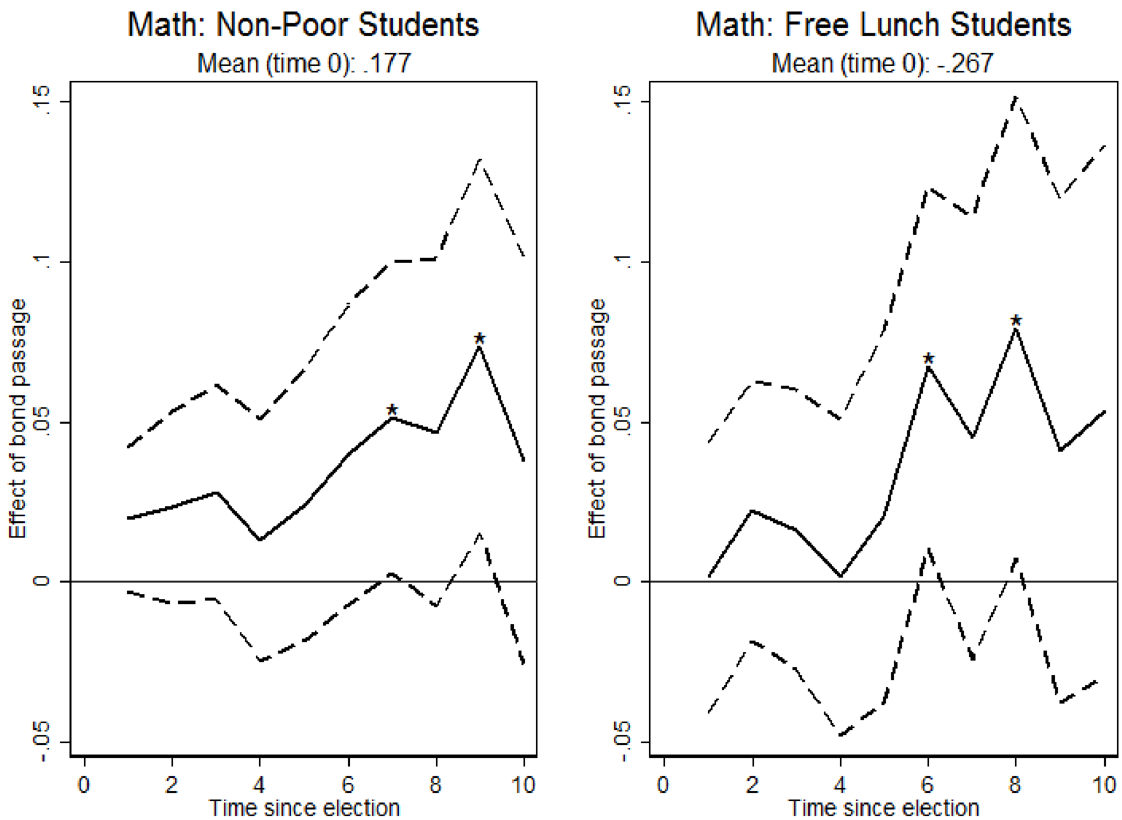
Notes: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on four measures of building condition one through nine years following bond passage. Effect is permitted to vary between old campuses (at least 25 years old at time of election) and newer 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.

Figure 8. Changes in Overall District Student Test Scores, by Vote Share



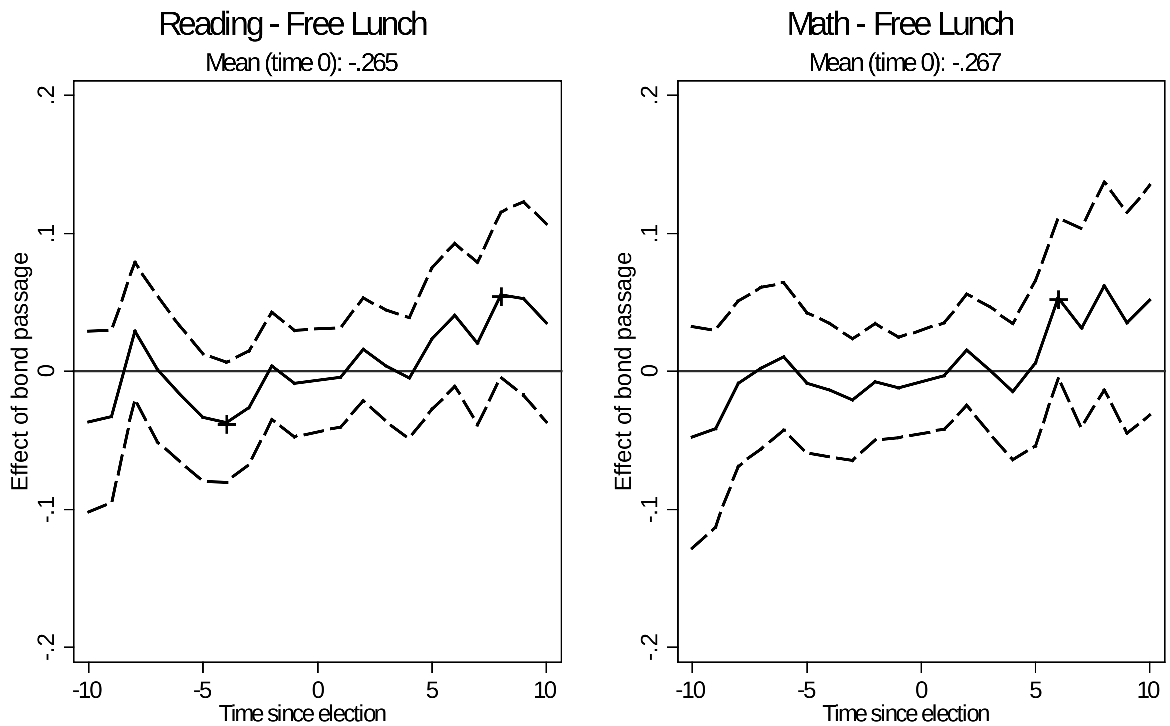
Notes: Graphs plot change in overall district student test scores before and after bond passage, separately by the vote share in favor of bond passage. Pre-period includes year election was held and prior two years; post-period includes test scores averaged across six, seven, and eight years after the election. For scatter plot, elections were grouped in 2.5 point bins of vote share. Linear fit is estimated on panel of election-year observations, not on the scatter points and thus gives more weight to vote share groups that include more observations. Includes data for 1737 elections and 812 districts.

Figure 9. District-level Estimates of Effect of Bond Passage on Math Test Scores



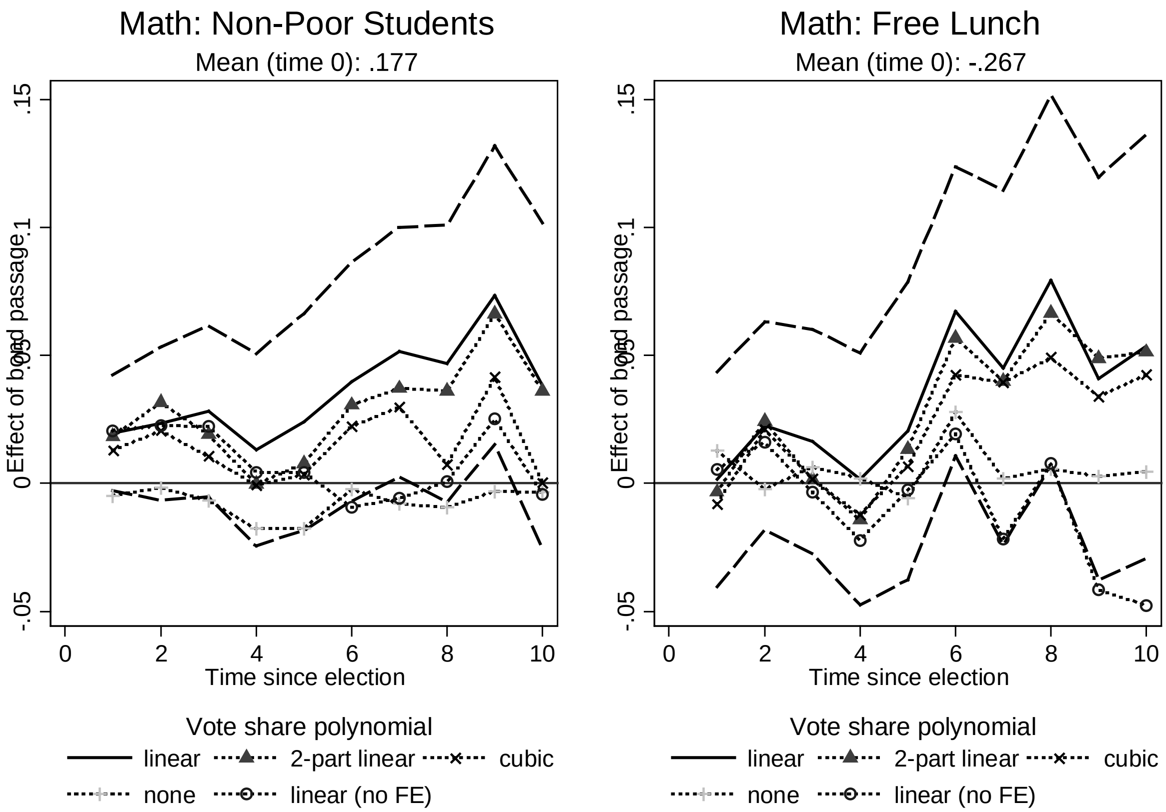
Notes: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on average district math scores for poor (eligible for free lunch) and non-poor students one through ten years following bond passage. Specification pools observations two years before 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 IIIA and equation (3). * Indicates significantly different from zero at a 5% level.

Figure 10. Pre-treatment Effects of Bond Passage on Achievement of Poor Students



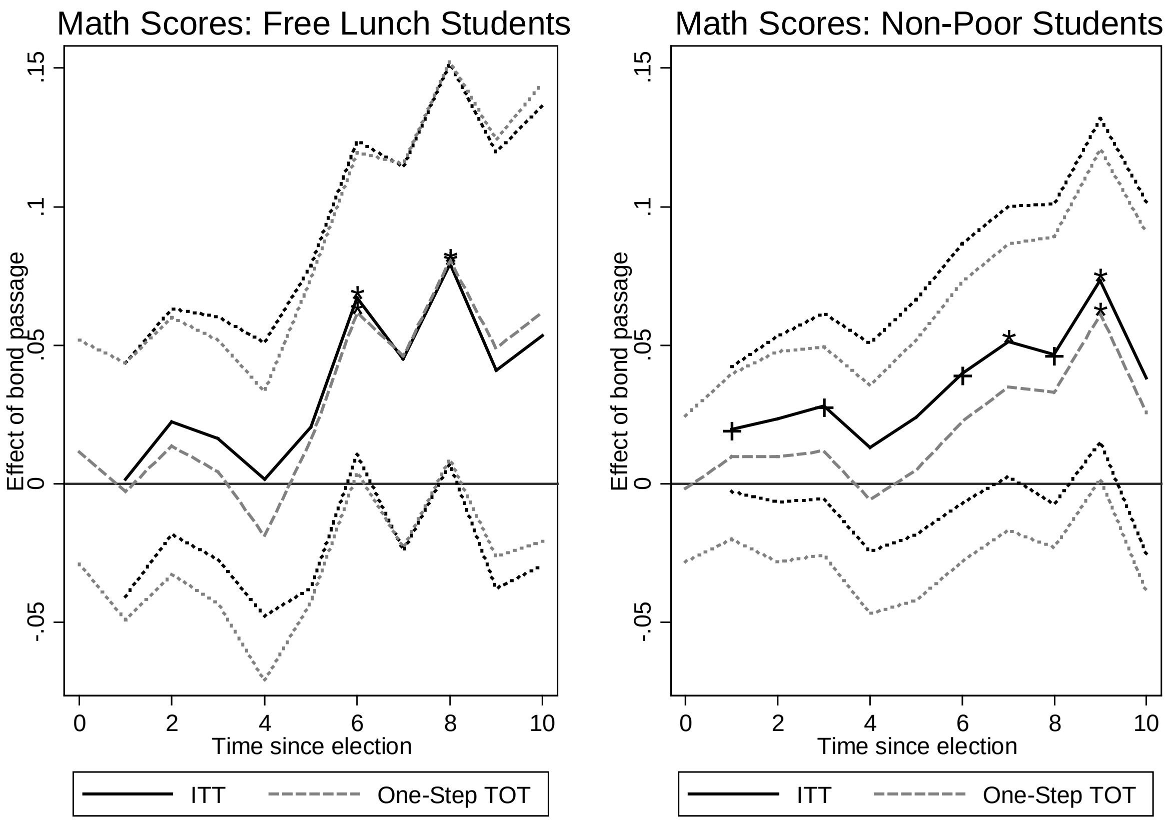
Notes: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on average district reading and math scores for poor (eligible for free lunch) students ten years prior through ten years following bond passage. Effect is normalized to zero in the year of the election. Specification pools all observations in this window around 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 IIIA and equation (3). + Indicates significantly different from zero at a 10% level.

Figure 11. District-level Estimates of Effect of Bond Passage on Test Scores, Robustness



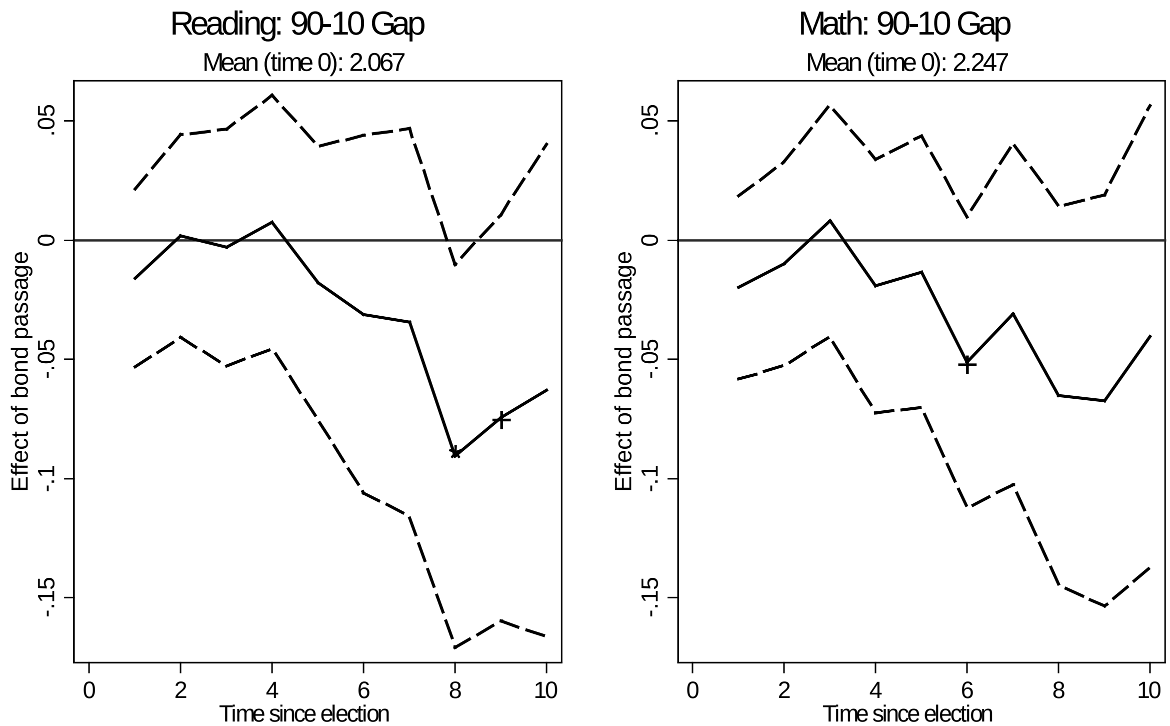
Notes: Graphs plot regression discontinuity point estimates for effect of bond passage on average district math scores for poor (eligible for free lunch) and non-poor students one through ten years following bond passage. Specification pools observations two years before through ten years after each bond election and includes and (where indicated) fixed effects for each separate election and a polynomial of the bond measure vote share, as described in Section IIIA and equation (3). Dashed lines indicate 95% confidence interval for base specification (linear vote share with fixed effects).

Figure 12. District-level Estimates of Effect of Bond Passage on Math Test Scores: ITT vs. TOT



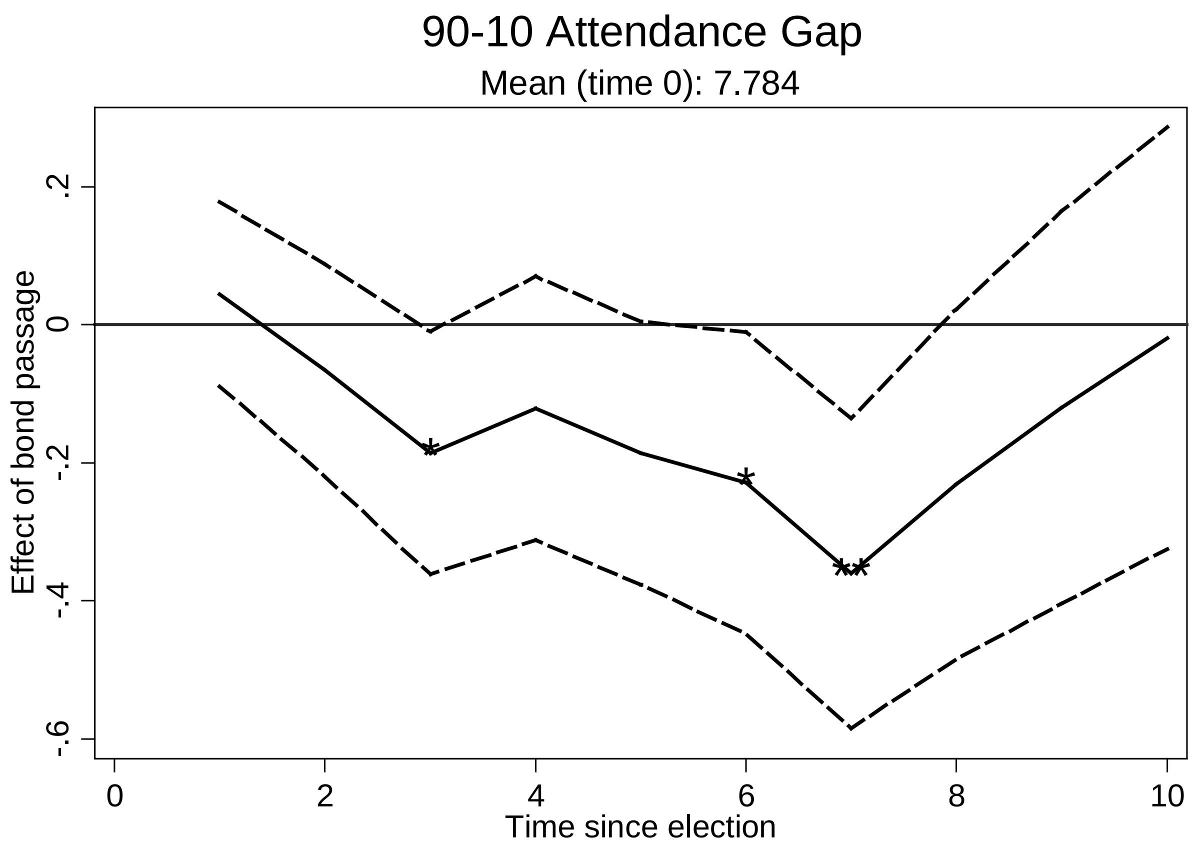
Notes: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on average district math scores for poor (eligible for free lunch) and non-poor students one through ten years following bond passage. ITT specification pools observations two years before 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 B3.a and equation (3). One-step TOT estimates pool all years of data in district X year panel and includes indicators for holding and passing bond election during the current and all previous years, the vote share (linearly) for elections held in all previous years, and fixed effects for districts and years.

Figure 13. District-level Estimates of Effect of Bond Passage on 90-10 Test Score Gap



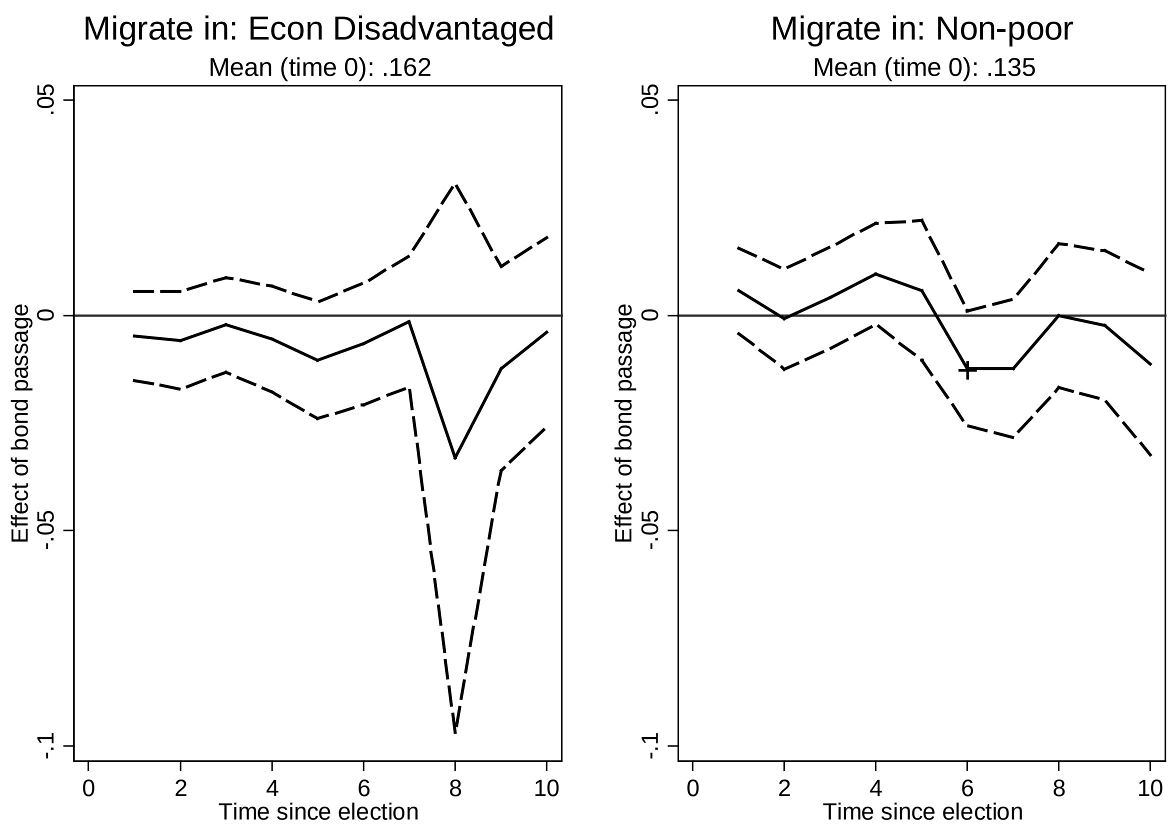
Notes: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on on the 90-10 achievement gaps one through ten years following bond passage. Specification pools observations two years before through ten years after each bond election and includes fixed effects for each separate election and a linear function of the bond measure vote share, as described in Section B3.a and equation (3). Markers indicate significantly different from zero at a 5% (*) and 10% (+) level.

Figure 14. Effects of Bond Passage on Attendance



Notes: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on the 90-10 percentile gap in attendance rate (fraction of days in attendance) ten years prior through ten years following bond passage. Specification pools all observations in this window around each bond election and includes fixed effects for each separate election and a linear function of the bond measure vote share. Markers indicate significantly different from zero at a 10% (+) and 5% (*) level.

Figure 15. Effects of Bond Passage on In-Migration Rate, by Economic Status



Notes: Graphs plot regression discontinuity point estimates and 95% confidence intervals for effect of bond passage on the in-migration rate ten years following bond passage. Migration rate is calculated as the ratio of students in grades 2 to 12 that are new to the district in year t+1 to the number of students in grades 1 to 11 in year t. Specification pools all observations in this window around each bond election and includes fixed effects for each separate election and a linear function of the bond measure vote share. Markers indicate significantly different from zero at a 10% (+) and 5% (*) level.

Table 1. Summary Statistics of Capital Bond Elections

					Bond amount (millions of $2010)		Bond amount per student ($2010)		Multiple
Year	Number	Pass	Vote share	Votes cast	Mean	Median	Mean	Median	elections held
1996	36	0.86	0.69	2,003	36.1	17.7	6,913	4,884	0.19
1997	185	0.85	0.70	1,181	24.5	10.0	7,032	5,311	0.11
1998	120	0.84	0.67	3,493	59.4	13.7	8,805	6,866	0.17
1999	166	0.83	0.69	1,116	35.0	8.8	7,698	6,064	0.13
2000	121	0.83	0.68	1,636	48.3	9.5	8,962	7,576	0.21
2001	137	0.82	0.66	2,075	48.1	11.1	8,486	6,717	0.12
2002	105	0.70	0.62	3,669	70.4	18.2	10,353	7,941	0.25
2003	114	0.84	0.63	2,993	68.5	24.9	9,653	5,995	0.35
2004	95	0.69	0.60	2,849	64.1	23.1	12,433	8,689	0.31
2005	138	0.82	0.62	1,561	57.3	22.3	11,777	8,937	0.23
2006	180	0.86	0.63	3,072	56.9	21.6	14,255	11,187	0.23
2007	156	0.77	0.60	2,970	102.0	23.1	16,110	12,037	0.15
2008	85	0.73	0.58	4,723	34.6	13.9	23,135	12,783	0.25
2009	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 start 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

	Sample Characteristics at Baseline (year prior to election)						All years pooled	
	All elections		Passed		Failed		All elections	
	mean	sd	mean	sd	mean	sd	mean	sd
Total enrollment	6,723	15,251	7,168	16,409	4,960	9,149	7,213	15,789
Fraction white	57.9	29.0	57.3	29.3	60.0	27.7	56.0	29.1
Fraction black	8.5	12.0	8.1	11.2	10.2	14.7	8.6	12.1
Fraction hispanic	32.1	29.2	33.0	29.8	28.5	26.6	33.6	29.2
Fraction econ disadvantaged	47.0	21.7	46.9	22.1	47.2	20.3	48.7	21.8
Fraction LEP	8.4	11.0	8.9	11.5	6.7	8.5	8.8	10.8
Fraction special ed	12.8	3.5	12.8	3.5	12.9	3.3	12.2	3.5
Fraction vocational ed	21.7	8.2	21.4	8.1	22.9	8.1	22.4	7.9
Fraction bilingual	7.6	10.1	8.0	10.5	6.2	8.0	8.1	10.1
Fraction gifted	7.3	3.1	7.3	3.1	7.0	2.7	7.2	3.0
Instructional spending per student ($2010)	5,202	1,116	5,182	1,149	5,283	971	5,253	1,034
Student services spending	2,889	1,006	2,891	1,055	2,883	783	2,970	993
Other spending per student	435	124	434	124	436	125	443	126
Capital outlay per student ($2010)	1,305	2,309	1,356	2,388	1,100	1,958	1,982	3,057
Open at least one campus	0.230	0.421	0.244	0.429	0.174	0.380	0.233	0.423
Close at least one campus	0.146	0.353	0.151	0.359	0.126	0.332	0.139	0.346
% campuses opened	0.038	0.098	0.041	0.103	0.028	0.077	0.040	0.113
% campuses closed	0.023	0.072	0.023	0.070	0.024	0.081	0.021	0.068
Student-teacher ratio - overall	13.529	5.581	13.599	6.166	13.253	1.967	13.372	3.122
Elementary	14.643	1.922	14.712	1.944	14.378	1.812	14.564	1.874
Middle	13.764	2.124	13.827	2.150	13.516	2.006	13.685	2.140
Secondary	12.873	11.182	12.992	12.461	12.414	2.906	12.543	4.723
Reading test scores								
District-wide mean	0.027	0.229	0.030	0.232	0.017	0.216	0.027	0.242
Free lunch mean	-0.270	0.217	-0.269	0.216	-0.276	0.220	-0.259	0.221
Not econ disadvantaged mean	0.200	0.202	0.201	0.206	0.199	0.185	0.203	0.216
1st decile	-1.155	0.462	-1.152	0.469	-1.170	0.434	-1.165	0.482
9th decile	0.873	0.090	0.872	0.091	0.877	0.085	0.889	0.093
Gap: 90-10 percentile	2.028	0.423	2.023	0.429	2.046	0.402	2.054	0.439
Gap: Not econ disadv - Free lunch	0.470	0.214	0.469	0.216	0.472	0.204	0.462	0.213
Math test scores								
District-wide mean	0.023	0.263	0.027	0.264	0.008	0.259	0.013	0.279
Free lunch mean	-0.269	0.243	-0.265	0.236	-0.287	0.269	-0.269	0.251
Not econ disadvantaged mean	0.184	0.241	0.184	0.246	0.180	0.224	0.178	0.254
1st decile	-1.207	0.419	-1.203	0.419	-1.220	0.418	-1.226	0.430
9th decile	0.985	0.141	0.984	0.145	0.989	0.123	1.008	0.143
Gap: 90-10 percentile	2.192	0.365	2.188	0.360	2.209	0.384	2.234	0.364
Gap: Not econ disadv - Free lunch	0.452	0.215	0.450	0.212	0.463	0.230	0.447	0.217
Attendance rate (fraction of days)								
District-wide mean	96.40	0.71	96.41	0.72	96.35	0.66	96.37	0.72
Free lunch mean	95.62	0.95	95.62	0.97	95.59	0.89	95.63	0.93
Not econ disadvantaged mean	96.78	0.68	96.79	0.69	96.73	0.63	96.76	0.69
1st decile	91.96	1.35	91.96	1.38	91.94	1.24	91.99	1.35
9th decile	99.82	0.27	99.82	0.27	99.80	0.28	99.78	0.29
Gap: 90-10 percentile	7.86	1.28	7.86	1.31	7.87	1.16	7.79	1.28
Gap: Not econ disadv - Free lunch	1.15	0.86	1.15	0.87	1.15	0.82	1.12	0.84
# Districts	812	812	748	748	279	279	812	812
# Elections	1,737	1,737	1,387	1,387	350	350	1,737	1,737
# Observations	1,737	1,737	1,387	1,387	350	350	13,831	13,831

Table 3A. Covariate Balance in Year Prior to Bond Election

		Only year prior to election			2 years prior, 6 years after election				
	Mean	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Student characteristics									
Total enrollment	6,723	2329**	3226*	1,083	2382**	3263*	60	58	-10
		(842)	(1,340)	(1,374)	(787)	(1,297)	(44)	(39)	(58)
Fraction white	57.86	-3.392*	1.985	-3.217	-3.759*	1.994	-0.271*	-0.219	-0.100
		(1.625)	(2.681)	(2.934)	(1.599)	(2.661)	(0.133)	(0.152)	(0.171)
Fraction black	8.51	-2.000*	0.340	-0.751	-1.977*	0.049	0.010	-0.001	-0.038
		(0.892)	(1.311)	(1.621)	(0.899)	(1.276)	(0.077)	(0.078)	(0.094)
Fraction hispanic	32.09	4.961**	-2.633	3.794	5.285**	-2.395	0.229+	0.198	0.157
		(1.564)	(2.701)	(2.846)	(1.539)	(2.706)	(0.125)	(0.142)	(0.158)
Fraction econ disadvantaged	46.98	0.319	-3.406+	1.865	0.390	-3.654+	0.251	0.241	0.566
		(1.208)	(1.911)	(2.058)	(1.204)	(1.896)	(0.376)	(0.359)	(0.413)
Fraction LEP	8.43	2.186**	-1.481	1.287	2.339**	-1.450	0.350**	0.334**	0.200
		(0.577)	(1.056)	(1.023)	(0.569)	(1.052)	(0.125)	(0.130)	(0.139)
Fraction special ed	12.80	-0.371+	0.091	-0.298	-0.381+	0.077	0.001	-0.034	-0.092
		(0.205)	(0.311)	(0.393)	(0.201)	(0.305)	(0.123)	(0.136)	(0.161)
Fraction vocational ed	21.66	-1.121*	-1.486*	-0.839	-1.197*	-1.563*	-0.529	-0.320	-0.562
		(0.507)	(0.757)	(1.007)	(0.511)	(0.745)	(0.469)	(0.496)	(0.624)
Fraction bilingual	7.61	1.881**	-1.625+	1.163	2.025**	-1.551	0.252+	0.266*	0.189
		(0.540)	(0.986)	(0.945)	(0.533)	(0.983)	(0.139)	(0.134)	(0.148)
Fraction gifted	7.28	0.234	0.193	-0.082	0.244	0.214	-0.072	-0.046	-0.015
		(0.176)	(0.262)	(0.318)	(0.169)	(0.255)	(0.127)	(0.139)	(0.157)
Instructional spending per student ($2010)	5,202	2	-409**	-207+	-19	-410**	-66+	-59	-47
		(62)	(99)	(113)	(64)	(100)	(38)	(43)	(50)
Capital outlay per student ($2010)	1,305	370**	215	138	409**	218	148	188	149
		(129)	(195)	(234)	(128)	(195)	(249)	(286)	(329)
Out-migration rate overall	0.143	0.000	0.006	0.006	0.000	0.005	0.003	0.004	0.003
		(0.003)	(0.004)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
In-migration rate overall	0.143	0.005+	0.004	0.005	0.005+	0.003	-0.005	-0.003	-0.005
		(0.003)	(0.004)	(0.006)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)
Close at least one campus	0.146	0.033	0.031	0.024	0.030	0.034	-0.006	-0.014	-0.017
		(0.021)	(0.031)	(0.037)	(0.021)	(0.031)	(0.041)	(0.040)	(0.049)
Open at least one campus	0.230	0.059**	0.081*	0.037	0.066**	0.088*	0.043	0.042	0.054
		(0.023)	(0.035)	(0.043)	(0.022)	(0.035)	(0.044)	(0.049)	(0.057)
% campuses closed	0.023	0.001	-0.007	-0.003	-0.001	-0.007	-0.004	-0.004	-0.003
		(0.004)	(0.006)	(0.007)	(0.005)	(0.006)	(0.008)	(0.009)	(0.010)
% campuses opened	0.038	0.012*	0.014+	0.007	0.013**	0.015+	0.026+	0.028	0.030
		(0.005)	(0.008)	(0.011)	(0.005)	(0.008)	(0.014)	(0.018)	(0.018)
Student-teacher ratio - overall	13.529	0.200	0.939*	0.350	0.235	0.919*	0.078	0.090	0.027
		(0.169)	(0.417)	(0.263)	(0.176)	(0.373)	(0.131)	(0.128)	(0.149)
Elementary	14.643	0.242*	0.451**	0.212	0.259*	0.460**	0.087	0.096	0.120
		(0.114)	(0.173)	(0.200)	(0.114)	(0.169)	(0.114)	(0.119)	(0.148)
Middle	13.764	0.207	0.457*	0.183	0.233+	0.453*	-0.056	0.066	0.114
		(0.134)	(0.208)	(0.239)	(0.133)	(0.202)	(0.129)	(0.147)	(0.160)
Secondary	12.873	0.387	1.196	0.357	0.451	1.118	0.441	0.305	-0.129
		(0.328)	(0.865)	(0.444)	(0.351)	(0.740)	(0.756)	(0.673)	(0.325)
Vote share control		None	Linear	Cubic	None	Linear	Linear	2-part Linear	Cubic
Election fixed effects		No	No	No	No	No	Yes	Yes	Yes
Max sample size		1737	1737	1737	13829	13829	13829	13829	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 to 3 includes all bond elections and outcome measures in the year prior to the election. These specifications include bond passage, academic year fixed effects, and polynomial in vote share (where indicated). The table reports the coefficient on bond passage. The sample in columns 4 to 8 includes outcomes 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. All these specifications include relative year fixed effects, academic year fixed effects, 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). Where indicated, the specification also includes fixed effects for each bond election and a polynomial in the election vote share interacted with relative year fixed effects. The 2-part linear model permits the vote share slope to differ on either side of the passing threshold. District- and district-group mean test scores and attendance 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. Test score and attendance deciles were calculated for each district overall whenever at least 100 students were tested in the district. Students in grades 3 to 8 are included. Standard errors are clustered at the district level. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 3B. Covariate Balance in Year Prior to Bond Election

		Only year prior to election			2 years prior, 6 years after election				
	Mean	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Standardized Reading									
District-wide mean	0.027	0.011	0.011	-0.004	0.010	0.015	-0.005	-0.001	0.011
		(0.014)	(0.022)	(0.025)	(0.014)	(0.022)	(0.009)	(0.010)	(0.012)
Free lunch mean	-0.270	0.013	-0.007	0.014	0.012	-0.003	-0.013	-0.007	0.013
		(0.014)	(0.021)	(0.026)	(0.014)	(0.021)	(0.016)	(0.018)	(0.020)
Not econ disadvantaged mean	0.200	0.001	0.010	-0.002	0.000	0.010	-0.009	-0.007	-0.011
		(0.012)	(0.018)	(0.020)	(0.012)	(0.018)	(0.011)	(0.012)	(0.014)
Gap: 90-10 percentile	2.028	-0.020	-0.023	0.013	-0.017	-0.029	-0.026	-0.031	-0.043+
		(0.025)	(0.039)	(0.042)	(0.024)	(0.037)	(0.019)	(0.021)	(0.024)
Gap: Not econ disadv - Free lunch	0.470	-0.008	0.017	-0.021	-0.008	0.013	0.005	0.000	-0.023
		(0.013)	(0.019)	(0.024)	(0.013)	(0.019)	(0.018)	(0.019)	(0.022)
Standardized Math									
District-wide mean	0.023	0.012	0.004	-0.001	0.012	0.008	-0.009	-0.007	0.008
		(0.016)	(0.026)	(0.031)	(0.016)	(0.025)	(0.013)	(0.014)	(0.017)
Free lunch mean	-0.269	0.016	-0.003	0.036	0.018	-0.001	-0.009	-0.006	0.024
		(0.017)	(0.025)	(0.032)	(0.017)	(0.025)	(0.019)	(0.021)	(0.024)
Not econ disadvantaged mean	0.184	-0.001	-0.004	-0.014	-0.001	-0.002	-0.017	-0.019	-0.026
		(0.014)	(0.022)	(0.026)	(0.014)	(0.021)	(0.013)	(0.015)	(0.017)
Gap: 90-10 percentile	2.192	0.000	0.022	0.029	0.000	0.011	-0.014	-0.017	-0.045+
		(0.022)	(0.033)	(0.037)	(0.022)	(0.032)	(0.020)	(0.022)	(0.025)
Gap: Not econ disadv - Free lunch	0.452	-0.013	0.006	-0.048*	-0.014	0.006	0.001	-0.004	-0.037
		(0.014)	(0.019)	(0.024)	(0.014)	(0.019)	(0.019)	(0.021)	(0.024)
Attendance									
District-wide mean	96.395	0.061	-0.099	-0.062	0.066	-0.089	0.011	0.015	0.006
		(0.042)	(0.062)	(0.077)	(0.041)	(0.060)	(0.041)	(0.045)	(0.054)
Free lunch mean	95.616	0.062	-0.175*	-0.052	0.061	-0.184*	-0.066	-0.039	0.000
		(0.058)	(0.079)	(0.095)	(0.057)	(0.077)	(0.066)	(0.070)	(0.080)
Not econ disadvantaged mean	96.777	0.053	-0.018	-0.003	0.059	-0.014	0.042	0.041	0.003
		(0.040)	(0.059)	(0.075)	(0.039)	(0.058)	(0.047)	(0.050)	(0.062)
Gap: 90-10 percentile	7.860	-0.034	0.161	0.102	-0.043	0.139	-0.095	-0.063	-0.072
		(0.076)	(0.119)	(0.141)	(0.075)	(0.115)	(0.076)	(0.080)	(0.099)
Gap: Not econ disadv - Free lunch	1.148	-0.033	0.162*	0.047	-0.027	0.173*	0.102	0.068	-0.003
		(0.054)	(0.076)	(0.089)	(0.053)	(0.075)	(0.070)	(0.074)	(0.082)
Vote share control		None	Linear	Cubic	None	Linear	Linear	2-part Linear	Cubic
Election fixed effects		No	No	No	No	No	Yes	Yes	Yes
Max sample size		1737	1737	1737	13829	13829	13829	13829	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 to 3 includes all bond elections and outcome measures in the year prior to the election. These specifications include bond passage, academic year fixed effects, and polynomial in vote share (where indicated). The table reports the coefficient on bond passage. The sample in columns 4 to 8 includes outcomes 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. All these specifications include relative year fixed effects, academic year fixed effects, 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). Where indicated, the specification also includes fixed effects for each bond election and a polynomial in the election vote share interacted with relative year fixed effects. The 2-part linear model permits the vote share slope to differ on either side of the passing threshold. District- and district-group mean test scores and attendance 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. Test score and attendance deciles were calculated for each district overall whenever at least 100 students were tested in the district. Students in grades 3 to 8 are included. Standard errors are clustered at the district level. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 4. Effect of Bond Passage on Capital and Instructional Spending per Student

			Coefficient on bond passage X eventtime interaction					
	Election FE	Voteshare Function	1 year	2 years	3 years	4 years	5 years	6 years
Panel A. Capital outlay per student ($2010)								
All districts (n = 12,212, mean = $1,982)	Yes	linear	2333**	1593**	-501	-306	-661+	-896*
			(305)	(391)	(383)	(340)	(343)	(401)
	Yes	none	2073**	1558**	-409	-51	-161	-897*
			(168)	(234)	(266)	(236)	(227)	(451)
	No	linear	2582**	1775**	-446	-241	-536+	-784*
			(287)	(346)	(341)	(315)	(300)	(383)
	Yes	2-part linear	2136**	1199**	-623	-307	-527	-675+
			(315)	(375)	(422)	(373)	(336)	(410)
	Yes	cubic	2077**	886*	-641	-103	-492	-641
			(369)	(420)	(442)	(428)	(370)	(459)
Small districts (n=5,665, mean = $2,188)	Yes	linear	3515**	2232**	-506	-438	-885	-1522*
			(533)	(707)	(742)	(592)	(593)	(685)
Panel B. Instructional spending per student ($2010)								
All districts (n = 12,212, mean = $5,253)	Yes	linear	19	-10	11	21	93	58
			(37)	(48)	(51)	(54)	(65)	(70)
	Yes	none	10	38	39	11	19	17
			(27)	(33)	(34)	(35)	(41)	(41)
	No	linear	-360**	-384**	-311**	-349**	-259**	-261*
			(99)	(96)	(86)	(100)	(100)	(104)
	Yes	2-part linear	2	10	-18	37	84	50
			(40)	(49)	(55)	(57)	(67)	(72)
	Yes	cubic	-19	37	-40	13	49	26
			(47)	(61)	(65)	(65)	(85)	(88)
Small districts (n=5,665, mean = $5,678)	Yes	linear	3	-16	19	42	185	88
			(67)	(83)	(93)	(101)	(122)	(122)

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 +6 relative to each election. Since some districts hold multiple elections in quick succession, some outcomes appear in the sample multiple times for different relative years. All specifications include relative year fixed effects, academic year fixed effects, and interactions between bond passage and relative year fixed effects (for relative years 1 to 6). The table reports these passage X relative year interactions. Where indicated, the specification also includes fixed effects for each bond election and a polynomial in the election vote share interacted with relative year fixed effects. The 2-part linear model permits the vote share slope to differ on either side of the passing threshold. Standard errors are clustered at the district level. Significance: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 5. Effect of Bond Passage on School Openings, Closings, and Student-Teacher Ratios

		Coefficient on bond passage X eventtime interaction					
	mean	1 year	2 years	3 years	4 years	5 years	6 years
Panel A. School Opening and Closing							
Open at least one campus	0.233	-0.010	0.127**	0.094*	0.022	0.013	0.067
		(0.036)	(0.036)	(0.039)	(0.044)	(0.046)	(0.044)
Elementary	0.123	-0.005	0.122**	0.070*	0.017	0.010	0.058+
		(0.025)	(0.030)	(0.035)	(0.033)	(0.032)	(0.030)
Middle	0.075	0.000	0.031	0.015	-0.001	-0.002	0.003
		(0.024)	(0.025)	(0.025)	(0.030)	(0.030)	(0.029)
Secondary	0.095	0.009	0.060*	0.022	0.052	0.000	0.033
		(0.026)	(0.030)	(0.028)	(0.032)	(0.034)	(0.037)
% campuses opened	0.040	0.006	0.037**	0.021+	0.006	0.003	0.011
		(0.010)	(0.013)	(0.011)	(0.014)	(0.013)	(0.015)
Close at least one campus	0.139	-0.078*	0.003	0.027	-0.002	-0.021	0.067
		(0.034)	(0.032)	(0.037)	(0.044)	(0.043)	(0.045)
Elementary	0.040	-0.030	0.017	0.046*	0.007	-0.003	0.038
		(0.020)	(0.020)	(0.022)	(0.028)	(0.026)	(0.027)
Middle	0.031	-0.035+	0.022	0.002	-0.021	-0.050*	0.027
		(0.019)	(0.016)	(0.018)	(0.019)	(0.024)	(0.022)
Secondary	0.070	-0.008	-0.003	0.014	0.010	0.027	0.051
		(0.026)	(0.025)	(0.027)	(0.031)	(0.028)	(0.031)
% campuses closed	0.021	-0.002	0.015*	0.010	-0.001	-0.002	0.015+
		(0.007)	(0.007)	(0.008)	(0.009)	(0.008)	(0.009)
Panel B. Student-teacher ratio							
Student-teacher ratio - overall	13.372	-0.336	-0.283	-0.329	-0.267	-0.314	-0.196
		(0.231)	(0.248)	(0.266)	(0.290)	(0.305)	(0.330)
Elementary	14.564	-0.028	-0.094	-0.071	-0.046	-0.140	-0.185
		(0.097)	(0.119)	(0.124)	(0.146)	(0.161)	(0.178)
Middle	13.685	-0.144	-0.111	0.026	0.067	0.170	0.379+
		(0.138)	(0.157)	(0.173)	(0.182)	(0.192)	(0.226)
Secondary	12.543	-0.716*	-0.529	-0.701+	-0.630	-0.637	-0.473
		(0.313)	(0.344)	(0.367)	(0.408)	(0.410)	(0.458)

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 +6 relative to each election. Since some districts hold multiple elections in quick succession, some outcomes appear in the sample multiple times for different relative years. All specifications include election fixed effects, vote share (linearly), relative year fixed effects, academic year fixed effects, and interactions between bond passage and relative year fixed effects (for relative years 1 to 6). The table reports these passage X relative year interactions. Most specifications include 13,830 observations, though some contain fewer due to logically missing data (e.g. districts without a middle school will not have middle school student-teacher ratio). 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 Facilities Condition in 2006, by Age of Facility

	All schools								Elem	Middle	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: At least fair condition											
Bond passed	0.109**	0.102**	-0.010	-0.039	-0.019	0.050	0.013		-0.0844	0.0878*	0.075
	(0.046)	(0.044)	(0.010)	(0.031)	(0.063)	(0.086)	(0.113)		(0.087)	(0.051)	(0.067)
School is old		-0.0981***	-0.253***	-0.250***	-0.262***	-0.264***	-0.260***	-0.193***	-0.262***	-0.18	-0.236*
		(0.019)	(0.068)	(0.068)	(0.068)	(0.069)	(0.069)	(0.065)	(0.083)	(0.110)	(0.130)
Bond passed*School is old			0.187***	0.186***	0.198***	0.201***	0.197***	0.0995	0.158*	0.129	0.217
			(0.070)	(0.070)	(0.071)	(0.071)	(0.071)	(0.068)	(0.086)	(0.113)	(0.135)
Panel B: At least good condition											
Bond passed	0.155**	0.130**	0.008	-0.027	-0.005	-0.117	-0.055		0.062	0.007	-0.116
	(0.060)	(0.055)	(0.051)	(0.081)	(0.100)	(0.137)	(0.191)		(0.127)	(0.178)	(0.101)
School is old		-0.325***	-0.493***	-0.489***	-0.502***	-0.496***	-0.508***	-0.461***	-0.552***	-0.445***	-0.495***
		(0.034)	(0.071)	(0.071)	(0.069)	(0.071)	(0.071)	(0.075)	(0.100)	(0.165)	(0.152)
Bond passed*School is old			0.203**	0.193**	0.206***	0.199**	0.211***	0.172**	0.218**	0.272	0.243
			(0.080)	(0.080)	(0.078)	(0.080)	(0.080)	(0.083)	(0.109)	(0.183)	(0.169)
Polynomial in vote share	None	None	None	Linear	Linear (2 part)	Quadradic	Cubic	None	Linear (2 part)	Linear (2 part)	Linear (2 part)
District fixed effects	No	No	No	No	No	No	No	Yes			
Observations	2,937	2,917	2,917	2,895	2,895	2,895	2,895	2,895	1,822	509	415

Notes: Old is an indicator for whether the facility is 25 years or older. Age is missing for some observations. 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 7. Effect of Bond Passage on Various Facility Characteristics, by Age of Facility

	At least fair condition	At least good condition	Ever major renovation	Effective age	Enrollment/ capacity	log(Maintenance needs)	Fraction of sq ft in portables	Sq ft per student
pass	-0.0394	-0.0271	-0.102	1.544	0.043	0.482	0.007	-5.295
	(0.031)	(0.081)	(0.097)	(2.050)	(0.058)	(0.451)	(0.022)	(16.570)
old	-0.250***	-0.489***	0.342***	10.82***	0.081**	1.852***	0.032	4.537
	(0.068)	(0.071)	(0.085)	(2.707)	(0.038)	(0.316)	(0.028)	(13.770)
pass_old	0.186***	0.193**	0.156*	-8.380***	-0.115**	-0.973***	-0.028	18.50
	(0.070)	(0.080)	(0.094)	(2.905)	(0.048)	(0.352)	(0.029)	(17.970)
Polynomial in vote share	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Constant	1.003***	0.879***	0.574***	5.157***	0.819***	4.383***	0.0338*	160.9***
	(0.009)	(0.051)	(0.077)	(1.067)	(0.038)	(0.291)	(0.020)	(8.160)
Observations	2,895	2,895	2,588	2,589	2,861	2,515	2,847	2,857
R-squared	0.085	0.151	0.298	0.078	0.006	0.103	0.011	0.009

Notes: Old is an indicator for whether the facility is 25 years or older. Age is missing for some observations. 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 Test Scores

	n	mean	Coefficient on bond passage X eventtime interaction					
			1 year	2 years	3 years	4 years	5 years	6 years
A. Standardized Reading								
District-wide mean	13,791	0.027	0.000	0.011	0.011	-0.002	0.000	0.025
			(0.010)	(0.013)	(0.013)	(0.014)	(0.016)	(0.017)
Free lunch mean	13,225	-0.259	-0.002	0.023	0.015	0.006	0.032	0.050*
			(0.019)	(0.019)	(0.020)	(0.021)	(0.024)	(0.024)
Reduced lunch mean	11,529	-0.049	-0.005	-0.005	0.009	0.001	0.021	0.027
			(0.021)	(0.024)	(0.024)	(0.025)	(0.026)	(0.029)
Not econ disadvantaged mean	13,687	0.203	0.015	0.015	0.017	-0.003	0.001	0.030
			(0.009)	(0.013)	(0.013)	(0.014)	(0.017)	(0.019)
Gap: Not econ disadv - Free lunch	13,123	0.462	0.000	-0.020	-0.003	-0.020	-0.041+	-0.018
			(0.018)	(0.019)	(0.020)	(0.021)	(0.024)	(0.023)
B. Standardized Math								
District-wide mean	13,791	0.013	0.002	0.010	0.017	-0.003	0.007	0.044+
			(0.015)	(0.016)	(0.018)	(0.020)	(0.023)	(0.024)
Free lunch mean	13,225	-0.269	0.003	0.024	0.017	0.004	0.023	0.067*
			(0.022)	(0.021)	(0.023)	(0.026)	(0.030)	(0.029)
Reduced lunch mean	11,525	-0.062	-0.012	-0.015	0.006	-0.004	-0.003	0.044
			(0.022)	(0.025)	(0.028)	(0.032)	(0.032)	(0.035)
Not econ disadvantaged mean	13,685	0.178	0.021+	0.025	0.029+	0.015	0.026	0.039
			(0.012)	(0.016)	(0.017)	(0.019)	(0.022)	(0.024)
Gap: Not econ disadv - Free lunch	13,121	0.447	-0.005	-0.006	0.011	0.002	-0.003	-0.015
			(0.016)	(0.017)	(0.018)	(0.019)	(0.023)	(0.023)

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 +6 relative to each election. 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, a linear function of the vote share, relative year fixed effects, academic year fixed effects, and interactions between bond passage and relative year fixed effects (for relative years 1 to 6). The table reports these passage X relative year interactions. District- and district-group 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. Students in grades 3 to 8 are included. Standard errors are clustered at the district level. Significance: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 9. Effect of Bond Passage on Test Scores, Robustness

Sample	Election FE	Voteshare Function	1 year	2 years	3 years	4 years	5 years	6 years	1 year	2 years	3 years	4 years	5 years	6 years
			Coefficient on bond passage X eventtime interaction						Coefficient on bond passage X eventtime interaction					
			Panel A. Standardized Reading, Free lunch mean						Panel B. Standardized Math, Free lunch mean					
All districts	Yes	linear	-0.002	0.023	0.015	0.006	0.032	0.050*	0.003	0.024	0.017	0.004	0.023	0.067*
			(0.019)	(0.019)	(0.020)	(0.021)	(0.024)	(0.024)	(0.022)	(0.021)	(0.023)	(0.026)	(0.030)	(0.029)
	Yes	cubic	-0.009	0.018	0.000	0.010	0.034	0.046	-0.006	0.023	0.003	-0.007	0.011	0.048
			(0.026)	(0.025)	(0.025)	(0.027)	(0.031)	(0.030)	(0.030)	(0.027)	(0.028)	(0.034)	(0.040)	(0.038)
	Yes	2-part linear	0.000	0.024	-0.001	-0.001	0.029	0.047+	-0.001	0.027	0.002	-0.010	0.018	0.060+
			(0.024)	(0.022)	(0.023)	(0.024)	(0.029)	(0.026)	(0.028)	(0.024)	(0.025)	(0.029)	(0.034)	(0.032)
	No	linear	0.000	0.017	-0.003	-0.009	0.023	0.026	0.006	0.016	-0.005	-0.022	-0.003	0.019
			(0.021)	(0.022)	(0.022)	(0.025)	(0.024)	(0.027)	(0.025)	(0.024)	(0.025)	(0.027)	(0.029)	(0.030)
	Yes	None	0.001	-0.006	0.004	0.001	-0.007	0.016	0.013	-0.002	0.005	0.003	-0.007	0.028
			(0.013)	(0.013)	(0.014)	(0.014)	(0.017)	(0.017)	(0.014)	(0.016)	(0.016)	(0.018)	(0.020)	(0.020)
Small districts	Yes	linear	0.016	0.046	0.016	0.025	0.076+	0.077+	-0.001	0.024	-0.014	-0.049	-0.010	0.062
			(0.035)	(0.032)	(0.034)	(0.037)	(0.040)	(0.040)	(0.040)	(0.038)	(0.040)	(0.043)	(0.053)	(0.052)
			Panel C. Standardized Reading, Not econ disadvantaged						Panel D. Standardized Math, Not econ disadvantaged					
All districts	Yes	linear	0.015	0.015	0.017	-0.003	0.001	0.030	0.021+	0.025	0.029+	0.015	0.026	0.039
			(0.009)	(0.013)	(0.013)	(0.014)	(0.017)	(0.019)	(0.012)	(0.016)	(0.017)	(0.019)	(0.022)	(0.024)
	Yes	cubic	0.009	0.007	0.001	-0.013	-0.022	0.004	0.013	0.022	0.011	0.001	0.005	0.021
			(0.012)	(0.016)	(0.016)	(0.018)	(0.020)	(0.024)	(0.015)	(0.021)	(0.022)	(0.026)	(0.028)	(0.032)
	Yes	2-part linear	0.014	0.020	0.014	-0.004	-0.009	0.015	0.019	0.032+	0.019	0.001	0.009	0.030
			(0.010)	(0.013)	(0.013)	(0.015)	(0.017)	(0.020)	(0.013)	(0.018)	(0.018)	(0.020)	(0.023)	(0.026)
	No	linear	0.022	0.018	0.015	-0.001	-0.005	0.001	0.020	0.022	0.021	0.004	0.004	-0.010
			(0.018)	(0.020)	(0.021)	(0.022)	(0.024)	(0.026)	(0.022)	(0.024)	(0.024)	(0.026)	(0.027)	(0.029)
	Yes	None	-0.010	-0.012	-0.012	-0.029**	-0.024*	-0.013	-0.005	-0.001	-0.007	-0.018	-0.018	-0.004
			-0.007	-0.008	-0.009	-0.009	-0.010	-0.011	(0.008)	(0.010)	(0.012)	(0.013)	(0.014)	(0.015)
Small districts	Yes	linear	0.030+	0.022	0.021	-0.012	-0.010	0.050	0.034+	0.028	0.020	-0.016	0.004	0.045
			(0.017)	(0.021)	(0.022)	(0.024)	(0.027)	(0.033)	(0.020)	(0.027)	(0.030)	(0.032)	(0.038)	(0.043)

Notes: Each row in each panel 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 +6 relative to each election. Since some districts hold multiple elections in quick succession, some outcomes appear in the sample multiple times for different relative years. All specifications include relative year fixed effects, academic year fixed effects, and interactions between bond passage and relative year fixed effects (for relative years 1 to 6). The table reports these passage X relative year interactions. Where indicated, the specification also includes fixed effects for each bond election and a polynomial in the election vote share interacted with relative year fixed effects. The 2-part linear model permits the vote share slope to differ on either side of the passing threshold. Standard errors are clustered at the district level. Significance: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 10. Effect of Bond Passage on Distribution of Test Scores

	mean	Coefficient on bond passage X eventtime interaction					
		1 year	2 years	3 years	4 years	5 years	6 years
A. Standardized Reading							
90th percentile	0.889	-0.002	0.000	0.001	-0.004	0.000	0.006
		(0.003)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
10th percentile	-1.165	0.015	0.000	0.006	-0.008	0.020	0.040
		(0.020)	(0.023)	(0.027)	(0.030)	(0.032)	(0.041)
Gap: 90 - 10 percentile	2.054	-0.017	-0.001	-0.005	0.004	-0.020	-0.033
		(0.019)	(0.022)	(0.026)	(0.027)	(0.030)	(0.039)
B. Standardized Math							
90th percentile	1.008	0.001	0.002	0.006	-0.003	0.013	0.014
		(0.007)	(0.008)	(0.009)	(0.011)	(0.012)	(0.012)
10th percentile	-1.226	0.021	0.014	-0.002	0.020	0.028	0.068+
		(0.021)	(0.023)	(0.027)	(0.031)	(0.033)	(0.037)
Gap: 90 - 10 percentile	2.234	-0.021	-0.012	0.007	-0.023	-0.015	-0.054+
		(0.020)	(0.022)	(0.025)	(0.027)	(0.029)	(0.031)

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 +6 relative to each election. 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, a linear function of the vote share, relative year fixed effects, academic year fixed effects, and interactions between bond passage and relative year fixed effects (for relative years 1 to 6). The table reports these passage X relative year interactions. Due to student privacy concerns, outcomes were constructed only for district-years with at least 100 tested students. Sample size is 12,884 observations (1,639 elections from 740 unique districts). Students in grades 3 to 8 are included. Standard errors are clustered at the district level. Significance: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 11. Effect of Bond Passage on Test Scores, Campus X Group-level Estimates

		Coefficient on bond passage X eventtime interaction						Coefficient on bond passage X eventtime interaction					
Sample	Fixed effect	1 year	2 years	3 years	4 years	5 years	6 years	1 year	2 years	3 years	4 years	5 years	6 years
		Panel A. Standardized Reading, Free lunch mean						Panel B. Standardized Math, Free lunch mean					
All cells	Election	-0.002	0.023	0.015	0.006	0.032	0.050*	0.003	0.024	0.018	0.005	0.023	0.067*
		(0.018)	(0.018)	(0.018)	(0.020)	(0.023)	(0.022)	(0.020)	(0.020)	(0.021)	(0.024)	(0.028)	(0.027)
All cells	Cell	-0.006	0.013	0.020	0.018	0.040+	0.046*	0.006	0.026	0.021	0.023	0.044	0.075*
		(0.015)	(0.018)	(0.019)	(0.021)	(0.023)	(0.023)	(0.017)	(0.020)	(0.023)	(0.025)	(0.029)	(0.029)
Existing cells	Election	-0.003	0.023	0.023	0.010	0.029	0.053*	0.000	0.026	0.019	0.010	0.026	0.071*
		(0.018)	(0.018)	(0.019)	(0.022)	(0.022)	(0.023)	(0.021)	(0.020)	(0.022)	(0.025)	(0.029)	(0.029)
Existing cells	Cell	-0.007	0.013	0.022	0.016	0.037	0.048*	0.005	0.029	0.022	0.020	0.039	0.079**
		(0.015)	(0.018)	(0.019)	(0.022)	(0.023)	(0.024)	(0.017)	(0.020)	(0.023)	(0.026)	(0.030)	(0.030)
		Panel C. Standardized Reading, Not econ disadvantaged						Panel D. Standardized Math, Not econ disadvantaged					
All cells	Election	0.015+	0.015	0.017	-0.003	0.001	0.030+	0.021+	0.025+	0.029+	0.015	0.026	0.039+
		(0.009)	(0.012)	(0.012)	(0.014)	(0.016)	(0.018)	(0.011)	(0.015)	(0.016)	(0.018)	(0.021)	(0.023)
All cells	Cell	0.015+	0.020+	0.020+	0.001	0.010	0.029	0.018	0.024	0.027	0.013	0.031	0.039
		(0.009)	(0.012)	(0.012)	(0.014)	(0.017)	(0.019)	(0.012)	(0.015)	(0.017)	(0.019)	(0.023)	(0.024)
Existing cells	Election	0.017+	0.016	0.023+	-0.002	0.000	0.038*	0.020+	0.026+	0.037*	0.013	0.031	0.045+
		(0.009)	(0.012)	(0.012)	(0.014)	(0.016)	(0.018)	(0.011)	(0.015)	(0.016)	(0.019)	(0.021)	(0.023)
Existing cells	Cell	0.016+	0.020+	0.019+	-0.001	0.004	0.037*	0.017	0.027+	0.027	0.009	0.029	0.043+
		(0.009)	(0.012)	(0.012)	(0.014)	(0.017)	(0.019)	(0.012)	(0.015)	(0.017)	(0.020)	(0.023)	(0.024)

Notes: Each row in each panel 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 +6 relative to each election. The unit of observation is the campus X grade X economic group X year cell. All specifications include vote share (linearly), relative year fixed effects, academic year fixed effects, and interactions between bond passage and relative year fixed effects (for relative years 1 to 6). The table reports these passage X relative year interactions. Where indicated, the specification also includes fixed effects for either each bond election or each cell. Regressions are weighted by the share of total enrollment in a district represented by each cell. "Existing cells" refers to cells (campus X grade X economic group) that existed in the year of the election. Standard errors clustered by district. Significance: + p < 0.10, * p < 0.05, ** p < 0.01.

Table 12. Effect of Bond Passage on Fraction of Days in Attendance

	mean	Coefficient on bond passage X eventtime interaction					
		1 year	2 years	3 years	4 years	5 years	6 years
A. District-level Means							
District-wide mean	96.37	-0.011	0.029	0.101+	0.047	0.005	0.044
		(0.037)	(0.045)	(0.058)	(0.059)	(0.055)	(0.061)
Free lunch mean	95.63	-0.070	0.039	0.148	0.083	0.041	0.183
		(0.058)	(0.074)	(0.092)	(0.086)	(0.091)	(0.114)
Reduced lunch mean	96.58	-0.010	0.132+	0.236*	0.159+	0.266*	0.275**
		(0.074)	(0.077)	(0.093)	(0.089)	(0.106)	(0.107)
Not econ disadvantaged mean	96.76	-0.023	0.027	0.023	0.041	0.017	0.027
		(0.043)	(0.046)	(0.051)	(0.061)	(0.061)	(0.064)
Gap: Not econ disadv - Free lunch	1.12	0.033	0.003	-0.130	-0.051	-0.022	-0.158
		(0.063)	(0.077)	(0.089)	(0.086)	(0.094)	(0.110)
B. District-level Distribution							
10th percentile	91.99	-0.003	0.079	0.214*	0.176+	0.172+	0.260*
		(0.072)	(0.082)	(0.096)	(0.101)	(0.103)	(0.119)
40th percentile	96.57	0.043	0.033	0.148**	0.073	0.076	0.113+
		(0.036)	(0.042)	(0.051)	(0.050)	(0.052)	(0.066)
90th percentile	99.78	0.040+	0.011	0.027	0.052+	-0.014	0.032
		(0.023)	(0.026)	(0.027)	(0.029)	(0.032)	(0.033)
Gap: 90 - 10 percentile	7.79	0.042	-0.068	-0.187*	-0.124	-0.186+	-0.228*
		(0.069)	(0.080)	(0.091)	(0.099)	(0.099)	(0.113)

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 +6 relative to each election. 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, a linear function of the vote share, relative year fixed effects, academic year fixed effects, and interactions between bond passage and relative year fixed effects (for relative years 1 to 6). The table reports these passage X relative year interactions. Due to student privacy concerns, distribution outcomes were constructed only for district-years with at least 100 tested students. Students in grades 3 to 8 are included. Standard errors are clustered at the district level. Significance: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.