Delayed Benefits: Effects of California School District Bond Elections on Achievement by Socioeconomic Status

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Abstract

Contradictory evidence of the relationship between education funding and student achievement could reflect heterogeneous effects by revenue source or student characteristics. This study examines potential heterogeneous effects of a particular type of local revenue—bond funds for capital investments—on achievement by socioeconomic status. Comparing California school districts within a narrow window on either side of the cutoff of voter support required to pass a general obligation bond measure, I use dynamic regression discontinuity models to estimate effects of passing a bond on academic achievement among low- and high-socioeconomic-status (SES) students. Results consistently suggest that passing a bond increases achievement among low- but not high-SES students. However, these benefits for low-SES students are delayed and emerge six years after an election. Effects are larger in low-income districts and in small districts, where benefits of capital investments are experienced by a larger proportion of students.

Keywords

funding of schools, regression discontinuity, socioeconomic inequality, local school district revenue, school facilities

Scholars and policymakers have been debating the efficiency of education funding for student achievement since at least the Coleman et al.'s (1966) report (Burtless 1996; Greenwald, Hedges, and Laine 1996; Hanushek 1989, 1996; for reviews, see Baker 2016; Biddle and Berliner 2002), including contemporary evidence that shows no relationship between funding and achievement (Morgan and Jung 2016). However, recent estimates of the effects of court-ordered school finance reforms find evidence that students (particularly in low-income districts) benefit from state funding increases for K–12 education (Candelaria and Shores 2017; Jackson, Johnson, and Persico 2016; Lafortune, Rothstein, and Schanzenbach 2016).

Contradictory evidence of the relationship between education funding and achievement could reflect heterogeneous effects. Education funding could be more efficient for achievement among socioeconomically disadvantaged (low-socioeconomic-status [SES]) students, due to fewer opportunities for academic learning outside of school (Lareau 2003). Alternatively, the effects of

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funding could vary by source or type. For example, local school funding could be used primarily to support elective programs, such as extracurricular activities or music programs, which have important benefits outside the classroom but perhaps limited ability to increase student achievement (Costa-Giomi 2004; Kinney 2008; Rickard, Bambrick, and Gill 2012; Southgate and Roscigno 2009). Similarly, local school facilities funds—to maintain or improve school buildings, grounds, or equipment—could provide little benefit for daily school district operations or programs and hold little potential to increase average achievement (Martorell, Stange, and McFarlin 2016). However, if achievement among low-SES students is more dependent on school context (Sharkey 2010) or if facilities funding frees resources to spend on academic programs (i.e., money that would otherwise be used for temporary classrooms or maintenance of facilities in poor condition; Zimmer and Jones 2005), these local investments could improve achievement.

To inform understanding of the effects of local funding on achievement, this study examines effects of California school district general obligation bond election measures on academic achievement by SES from 1999 to 2013. California presents an interesting context because school districts have local revenue limits, and most funding is determined by the state since Proposition 13 in 1978. Revenue limits were raised under the 2013 Local Control Funding Formula (LCFF), but local district revenue is still capped (Taylor 2013). Before 2013, school district election measures were "essentially the only source of local discretion" (Cellini, Ferreira, and Rothstein 2010:218). Given districts' limited ability to increase their revenue in other ways, California school district election measures allow more precise estimates of local funding than in Texas, where Martorell and colleagues (2016) found null effects. The 1999-to-2013 period excludes years after the LCFF, a policy that may have changed the relationship between local funding elections and student achievement.

THEORETICAL AND EMPIRICAL BACKGROUND

Local School Revenue and Achievement by SES

Recent estimates of the effect of school funding on achievement have focused on changes in state

funding (Candelaria and Shores 2017; Jackson et al. 2016; Lafortune et al. 2016). State revenue frequently comes with rules about how districts can spend the funds (Parker and Griffith 2016; Smith et al. 2013) and tends to be distributed more equally within districts than are other sources of funding (Baker et al. 2017; Baker and Weber 2016; Education Law Center 2013).

Local funding, in contrast, tends to be distributed more unequally by districts than state or federal funding (Condron and Roscigno 2003; Timar and Roza 2010). Local revenue comes with fewer spending restrictions, such as categorical funds, than state revenue. When distributing unrestricted (noncategorical, including local) funds, evidence suggests districts favor schools with more advantaged students (Heuer and Stullich 2011; Roza and Miles 2002; Timar and Roza 2010). In fact, Guin and colleagues (2007) examine the distribution of noncategorical funds and find that funding inequality is greater within than between districts. For example, districts may provide more resources (particularly, higher salaries for more experienced teachers) to schools in neighborhoods with more high-SES students (Roza 2010). Due to its more unequal distribution within districts compared to state revenue, local revenue increases may be less likely to increase achievement among low-SES students.

Facilities Funding as a Type of Local Revenue

Estimates of the effects of local revenue on achievement are relatively rare, partly due to endogeneity concerns. Research on this topic tends to focus on funding for school facilities (Cellini et al. 2010; Hong and Zimmer 2016; Neilson and Zimmerman 2014). School facilities funds, in contrast to most other local funds, often have strong constraints on how they can be spent. School district bond measures that provide much of the revenue for facility improvements require approval from local voters, and spending must match the ballot question. In California, for example, general obligation bonds have been allowed since Proposition 46 in 1978 but only to acquire or improve real property, including schools (California Debt and Investment Advisory Commission [CDIAC] 2008). Since Proposition 39, bond measures can be approved under the 55 percent (rather than two-thirds) threshold, but they must meet additional requirements.

Bond funds are limited to use for specific expenses, but the ballot measure may not specify distribution within the district. As with other local revenue, districts could use local facilities funds to improve schools with higher-SES students (Condron and Roscigno 2003; Roza 2010). This would align with existing evidence that schools in poor areas are in worse condition than those in more economically advantaged areas. Compared to schools with less than 35 percent eligibility for free or reduced-price lunch, the average school with at least 75 percent eligibility is six years older and 12 percentage points more likely to require expenditures to achieve good condition (D. Alexander and Lewis 2014:10, 20).

The quality of school facilities is related to teacher satisfaction and retention (Buckley, Schneider, and Shang 2004; S. Johnson, Kraft, and Papay 2012), school social climate (Maxwell 2016; Uline and Tschannen-Moran 2008), and student attendance (Duran-Narucki 2008; Maxwell 2016). In fact, a survey of elementary school teachers in California found that school facilities are the most important workplace condition—more important than salary or student characteristics—in deciding where to teach (Horng 2009). Teacher quality and retention, social climate, attendance, crowding, technology, air quality, temperature, lighting, morale, and local housing values offer potential explanations for a link between school facilities and student achievement (Jones and Zimmer 2001; Neilson and Zimmerman 2014; Welsh et al. 2012).

Although theory predicts a relationship between the quality of school facilities and achievement, empirical evidence is inconsistent (for reviews, see Gunter and Shao 2016; Hanushek 1997). Some research finds a positive relationship between facility quality or capital expenditures and student achievement (Conlin and Thompson 2017; Duran-Narucki 2008; Hong and Zimmer 2016; Maxwell 2016; Neilson and Zimmerman 2014; Uline and Tschannen-Moran 2008; Welsh et al. 2012). However, other research finds no relationship between achievement and facility quality (Picus et al. 2005), even when applying regression discontinuity methods and examining lagged effects (Martorell et al. 2016). Indeed, literature reviews suggest most studies find a null relationship between school facility quality and achievement, and those that do not are fairly evenly split, with slightly more evidence for a positive than a negative relationship (Gunter and Shao 2016; Hanushek 1997).

Heterogeneous effects by SES could account for some of the inconsistent findings. Context is critical for student achievement (Sharkey 2010; Sharkey et al. 2014; Sharkey and Elwert 2011), and improving school facilities could provide a better learning environment, which may be particularly beneficial for achievement among low-SES students. For example, compared to higher-SES students who receive more academic input at home (K. Alexander, Entwisle, and Olson 2007; Entwisle, Alexander, and Olson 1998; Lareau 2003), achievement among low-SES students may depend more strongly on school facilities, such as technology, air quality, and space (Welsh et al. 2012). These factors may enhance student attention and attendance by reducing distractions, making the school more appealing, or reducing health problems, such as asthma (Duran-Narucki 2008; Maxwell 2016). Quality facilities also provide better working conditions for teachers, which can help attract and retain effective teachers in districts with a high proportion of low-SES students (Buckley et al. 2004; Horng 2009; S. Johnson et al. 2012).

Whether through context, teacher quality, or some other mechanism, do local financial investments in school facilities boost achievement more among low-SES than among high-SES students? Existing evidence suggests facilities funding will have stronger effects for low-SES students. For example, despite overall null findings, Martorell and colleagues (2016) find some evidence of benefits for students from low-SES backgrounds. Furthermore, in some cases evidence of positive effects is based largely on relatively poor or low-SES districts (Conlin and Thompson 2017; Welsh et al. 2012).

Despite the theoretical possibility of heterogeneous effects, little research explicitly investigates whether effects of local funding vary by student SES. State policies tend to favor capital investments in wealthy districts. For example, although California largely constrains revenue for operating expenses, California school districts have large discrepancies in their ability to raise local revenue for capital improvements (given unequal property values; Brunner 2007), and the state awards funding for new construction and modernization on a first-come, first-served basis, which favors districts with more resources (Taylor 2015). If high-SES students benefit less from facilities, and wealthier districts are more likely to invest in facilities partly because a lower tax rate is required

to achieve the same capital investment, then average effects may be biased toward zero. This pattern suggests school facilities investments could be more efficient in poor rather than wealthy districts.

Local Revenue in the California Context

To further our understanding of the implications of local funding, I examine potential heterogeneous effects of California school district bond elections by SES, in a period when other sources of district revenue were constrained. Specifically, I pose the following research questions: (1) Does passing a local school district general obligation bond increase student achievement? (2) Do effects differ by student SES?

I focus on general obligation bond elections for two reasons. First, they account for the majority of school facility funds in California (54 percent from 1998 to 2006; Brunner 2007:2). General obligation bonds also represent the vast majority (76 percent) of school district funding elections in California from 1999 to 2013. Second, examining various types of elections would make interpretation difficult because multiple factors could explain the results. Bond revenue has strong requirements on how it is spent (CDIAC 2008), whereas revenue from a tax election measure may be more flexible, depending on the wording of the ballot measure.

Similar to existing work (Cellini et al. 2010; Hong and Zimmer 2016; Martorell et al. 2016), I use a regression discontinuity (RD) approach, taking advantage of variation in the proportion of votes for a bond measure within a narrow window around the threshold required for the measure to pass. This study departs from previous work by explicitly comparing effects on achievement by SES. Whereas some work focuses on relatively poor districts (e.g., Conlin and Thompson 2017), this study includes any district with a bond measure, providing more generalizability.

The above review leads me to three hypotheses. Because low-SES achievement should depend more on school context, Hypothesis 1 states that passing a school district bond measure increases achievement among low-SES but not high-SES students (e.g., due to teacher quality, improved facilities). However, if school districts distribute local revenue unequally (Roza 2010; Timar and Roza 2010), then passing a bond measure could

increase achievement more among high-SES than among low-SES students in a district. Based on arguments about inequality of within-district revenue distribution, Hypothesis 2 states that passing a bond measure increases achievement among high-SES but not low-SES students. Election measures secure future funding for district improvements. Therefore, effects should take several years to emerge because districts must collect, plan, and then spend the revenue. However, if families and school personnel anticipate the changes outlined in the ballot measure (e.g., temporarily closing a school for renovation), effects on achievement could emerge earlier.

Since Proposition 46 in 1986, general obligation bonds in California were allowed "only for the acquisition or improvements of real property (e.g., fire and police stations, schools, streets and various public works projects)" (CDIAC 2008:10). Proposition 39 stipulated that "[b]ond proceeds can be used only for construction, rehabilitation, equipping school facilities, or acquisition/lease of real property for school facilities" (CDIAC 2008:10). Because bond revenue may be used only for facility improvements, Hypothesis 3 states that passing a bond measure increases spending, particularly capital spending. However, because construction planning takes time, these effects emerge with a delay after the election.

RESEARCH METHODS

Data and Measures

I examine school districts in California for several reasons. First, achievement tests are typically conducted at the state level, making cross-state comparisons challenging. The California Department of Education (CDE) provides annual achievement information for each school district separately by SES, allowing comparisons across districts. Second, California contains a large number of school districts (approximately 1,000 in 2013), many of which had closely contested general obligation bond measures (678 within 10 percentage points of the threshold required to pass from 1999 to 2013). Third, since Proposition 13, California school districts have had revenue limits, and most funding is determined by the state. School district election measures are a unique mechanism allowing local revenue in California (Cellini et al. 2010; Jennison 2017). Districts' limited ability to

increase their revenue in other ways makes school district election measures in California particularly useful for estimating local funding effects.

Achievement. Annual CDE data include school-level Academic Performance Index (API) scores from 1999 to 2013 for all students and low-SES students. API scores are based on tests taken in the spring of each academic year; 1999 scores represent testing completed in spring 1999. Consistent with that timing, years indicate spring of the academic year throughout this article (i.e., 1999 represents the 1998–1999 academic year).

The CDE defines low-SES students as those who are eligible for free or reduced-price lunch *or* whose parents both have less than a high school diploma. Because this categorization depends on income and education, I refer to the distinction as SES throughout the article.

Ranging from 200 to 1,000, API provides a single measure of performance, drawing on assessments of multiple content areas. API scores are calculated for any district, school, or student group with at least 11 valid scores (CDE 2013). Thus, separate measures by SES are available in schools or districts with at least 11 low- and high-SES students. The relative weighting of various assessments in the API calculation varies over time, but all models include year fixed effects to account for statewide changes in scores over time, and results are consistent when predicting annual district SES achievement gaps (which allows testing for different effects by SES in the same district year).

The primary dependent variables are the annual mean API scores among low-SES and high-SES students in each school district. The CDE provides annual district-level baseline API scores for low-SES students from 2003 to 2012 (and growth scores on the same scale in 2013). From 1999 to 2002, I calculate the district-level mean low-SES API score as a weighted average of school-level mean low-SES scores, where the weights are the number of low-SES students tested in each school in the district. This creates a district-level mean score for all low-SES students in the district that is comparable to district-level means in later years. For 2003 to 2013, CDE data provide district-level achievement for all students and low-SES students (i.e., district-level low-SES averages do not need to be calculated from school averages in those years). Details

about calculation of district-level high-SES achievement are provided in the online appendix.

School-district funding elections. The achievement data are merged to data on school district election outcomes. The California Elections Data Archive (CEDA) provides the following information about each school district ballot measure from 1999 to 2013: date, type (e.g., general obligation bond), proportion of votes in favor, threshold required to pass, and outcome (passed/failed). I limit analyses to elections at the school district level, excluding elections related to community college funding. The CEDA data do not have National Center for Education Statistics (NCES) identification numbers to facilitate merging. I created a crosswalk between CEDA measure ID and NCES ID numbers to merge California achievement and election data.

District ballot measures require support from a fixed proportion of voters to pass. General obligation bonds required support from two-thirds of voters until 2001 and have since allowed districts to require 55 percent support if they meet certain requirements (Ed Data 2017; CDIAC 2008) (13 measures required 50 percent, but results are the same when excluding these districts). I center the proportion of votes in favor of each measure at the pass cutoff by subtracting the threshold required to pass. This is the running or forcing variable in RD analyses. Election measures that passed (the treatment variable) are those with a higher proportion of votes than the cutoff required to pass. In instances where a school district spans multiple counties, the multicounty election results determine whether a measure passes. CEDA includes multicounty data, which I use to calculate the running and treatment variables when elections include multiple counties.

School district spending and demographics. To understand potential mechanisms for the relationship between bond election outcomes and student achievement, I use NCES IDs to link elections and achievement data to Public Elementary-Secondary Education Finance Data from the Census Finance Survey (called F-33 data), which include annual expenditure details for each district from 1999 to 2013. Specifically, I calculate per-pupil total spending, instructional spending, and capital outlays. All currency is adjusted for inflation to 2014 dollars.

Finally, I link annual district-level characteristics from the Common Core of Data, including total enrollment, number of schools, the proportion of students who are eligible for free or reduced-price lunch, and the proportion of black and Hispanic students.

Analytic Strategy

I use RD analyses to examine implications of an increase in local facilities funding on achievement. Specifically, I use district bond election outcomes as a source of exogenous variation in local funding. By examining school districts within a narrow range of the threshold of voter support required for a ballot measure to pass, I estimate effects of an exogenous increase in school district funding on student achievement by SES.

RD exploits a cutoff, such as that created by a school district election, as leverage to approach a causal estimate. By examining the difference between districts that narrowly passed or failed a measure, RD provides a causal estimate of the treatment effect among otherwise similar districts (Imbens and Lemieux 2008; Lee and Lemieux 2010). Key assumptions include the following: meaningful unobserved differences between districts within a narrow window on either side of the cutoff are eliminated, and other factors related to the outcome vary continuously over the forcing variable (percentage of voters supporting a measure), which is controlled in the regression (Lee and Lemieux 2010:287). Districts where bonds pass may differ from those where they fail, but limiting analysis to a narrow window on both sides of the cutoff leaves districts that should be similar, except for observed (and controlled) differences in the forcing variable.

Intent-to-treat (ITT) estimates. Despite key strengths, a traditional RD application would not take advantage of the panel nature of the election and achievement data and would lose valuable information. Delays in collecting and spending bond revenue make it likely that effects will emerge over time. Furthermore, districts may learn from failed election attempts and propose a new measure shortly after a failed measure (Cellini et al. 2010; Hong and Zimmer 2016), potentially manipulating themselves around the cutoff. To account for these characteristics of bond measures, I follow previous work and use a dynamic

RD approach (Cellini et al. 2010; Martorell et al. 2016).

The dynamic RD design includes district-level panel data and estimates the effect of passing a bond measure in multiple years following the election (for additional details, see Cellini et al. 2010). This allows for delayed effects. Panel data are included for all elections to allow for multiple elections in each district. Specifically, I create stacked panel data for each election, including a window two years before and 10 years after the election ($t - 2$ to $t + 10$). For example, if a district holds an election in 2001, I include district data for years 1999 through 2011. I combine or "stack" these data around each individual election from 1999 to 2013 into one data set, which can include multiple observations of the same district years if districts hold multiple elections.

Using these stacked panel data, I estimate effects of passing a bond measure using Equation (1), where the mean test score in district i in calendar year t and s years from the focal election is predicted by whether the focal election passed, a polynomial of the percentage of votes for the bond measure centered at the cutoff, with fixed effects for each year relative to the focal election (δ), each calendar year (μ), and each focal election (π). Vote share and pass measures are set to zero before the election, which allows inclusion of election fixed effects, and are interacted with years since the election to allow estimates to vary with time since the bond measure. Robust standard errors are adjusted for district-level clustering in all models.

$$\text{Score}_{its} = \beta_s \text{Pass}_{it} + V_s(\text{Vote share}_{it})$$
$$+ \delta_s + \mu_t + \pi_{it} + \varepsilon_{its} \qquad (1)$$

The coefficients of interest (β_s) estimate the ITT effect of passing a bond measure s years after the election, accounting for stable differences between elections (and therefore between districts) as well as changes over calendar years and years relative to the election. These coefficients (β_s) are the interactions between the indicator for pass and year since the election. Models include various functional forms of vote share, up to a cubic. I report estimates one to six years after the election for consistency with previous studies and because intervening changes make estimates in later years less precise (Cellini et al. 2010; Martorell et al. 2016).

Building on existing studies, I apply this dynamic RD technique when limiting analyses to elections within a narrow window on either side of the cutoff required for a bond measure to pass. This narrow window should leave districts that are similar, except for observed (and controlled) differences in vote share (the forcing variable). The width of the RD window creates a trade-off between internal validity and power. Primary analyses use a bandwidth selected based on the data (using rdbwselect in Stata; Calonico, Cattaneo, and Titiunik 2014). I vary the width of the window in sensitivity analyses (Table S4 and Figure S3 in the online appendix) to assess robustness.

Treatment-on-the-treated (TOT) estimates.

The above estimates represent the average effect of each election, regardless of other elections in the same district. However, districts can propose multiple bond measures and can therefore have multiple possible outcomes—representing the control group in some years and the treatment group in others. The above estimates are therefore noisy because they include effects of the focal election but also potential indirect effects of other elections in the same district. To provide alternative estimates that take previous elections into account, I use the "one-step" TOT approach developed by Cellini and colleagues (2010).

The TOT analyses are applied to standard district panel data from 1999 to 2013, where each district is represented once (rather than each election being represented once as in the ITT analyses). To account for previous elections, the TOT estimates include indicators for holding a bond election, indicators for whether it passed, and a polynomial of the vote share measured in each previous year.

$$\text{Score}_{it} = \sum_{s=0}^{s}(\beta_s \text{Pass}_{it-s} + \theta_s \text{Election}_{it-s}$$
$$+ V_s(\text{Vote share}_{it-s})) + \mu_t + \pi_i + \varepsilon_{it} \quad (2)$$

In Equation (2), the mean test score in district i in calendar year t is predicted by indicators for whether the district passed a bond measure in each previous year, indicators for whether the district had a bond election in each previous year, and a polynomial of the percentage of votes for the bond measure centered at the cutoff in each previous year, with fixed effects for calendar year (μ_t)

and district (π_i). Vote share is set to zero in district years with no election. Robust standard errors are adjusted for district-level clustering in all models.

The coefficients of interest are β_s, which estimate the effect of passing a bond measure s years since the election, accounting for district election proposal and pass history as well as vote share history. As in the ITT analyses, I report estimates one to six years after the election.

A concern with the TOT analyses is that controls for previous election outcomes could introduce endogeneity in the model and bias estimates. For example, the outcome of a previous bond election in a district could influence the likelihood of passing a later bond measure. ITT estimates are less precise, but they are preferred (over the TOT estimates) because they avoid this potential bias. TOT results are in the online appendix to conserve space.

Sensitivity analyses and validity checks.

I use several placebo checks in the ITT analyses as falsification tests. That is, I assign false cutoffs for an election measure to pass (5 and 10 percentage points above and below the actual cutoff required to pass) and estimate effects using those alternative pass thresholds. Results of these placebo tests (see the online appendix) all suggest null effects at false cutoffs, supporting the interpretation of the effects of bond passage at the actual cutoff.

To check the validity of the RD approach, I look for discontinuities in the density of the forcing variable, which could suggest districts manipulated themselves around the cutoff (McCrary 2008). For example, districts may have been more likely to propose a ballot measure if they expected it to pass. Discontinuities in other variables that should be unrelated to the forcing variable could suggest the RD assumptions do not hold. I do not find evidence of sharp discontinuities in the forcing or other variables. Furthermore, both conventional and robust density tests are not statistically significant, which supports the validity of the RD approach. The online appendix includes graphs (Figures S1, S2, S4–S9) of the forcing-variable density and of several demographic measures (enrollment; proportions of black, Hispanic, and Native American students; and proportion of students eligible for free lunch) by the forcing variable. Due to concern that likelihood of passing could differ by the proposed bond amount, the online appendix also includes a graph

Table 1. Descriptive Statistics of School District Bond Election Measures.

				Votes in favor (%)	
Year	Bond measures (*n*)	55% Pass cutoff (%)	Pass (%)	M	SD
1999	75	0.00	58.67	69.06	9.57
2000	79	0.00	62.03	69.61	9.02
2001	47	0.00	76.60	66.91	12.02
2002	156	86.54	80.77	63.72	8.48
2003	20	45.00	60.00	63.96	11.94
2004	106	94.34	82.08	64.95	8.59
2005	34	58.82	85.29	65.59	6.40
2006	124	97.58	71.77	60.70	7.66
2007	19	47.37	57.89	65.38	12.02
2008	155	94.84	81.94	63.49	8.14
2009	4	50.00	50.00	51.04	14.97
2010	86	94.19	73.26	59.50	11.97
2011	8	100.00	87.50	63.19	7.54
2012	136	97.06	81.62	63.11	9.23
2013	10	80.00	70.00	65.93	13.52
Mean		63.05	71.96	63.74	
Total	*N* = 1,059				

Source: California Elections Data Archive 1999 to 2013.
Note: Limited to general obligation bond measures in districts with achievement data for low- and high-socioeconomic-status students.

(Figure S10) of variation in the proposed bond amount per pupil (in 2014 dollars) by the forcing variable. These graphs do not show discontinuities at the cutoff required to pass.

Finally, to gain deeper understanding of the effects of passing a bond measure, I conduct analyses using standard district-level panel data and a traditional RD approach without addressing the dynamic characteristics of bond election measures. These analyses estimate effects in the current year and do not account for the dynamic nature or potentially delayed effects of bond measures. Results of these analyses are presented in the online appendix.

RESULTS

Descriptive Statistics and Balance

Table 1 provides descriptive statistics of school district bond election measures from 1999 to 2013. The number of bond elections varies substantially over time along with the pass rate, which varies from 50 percent in 2009 to 87.5 percent in 2011, the two years with the fewest bond measures (four and eight, respectively). In contrast, vote share shows less variation, ranging from 51

percent in 2009 (the lowest year by 8 percentage points) to 69.6 percent in 2000.

Table 1 shows that the proportion of bond measures with a 55 percent (as opposed to two-thirds) pass threshold jumps from 0 percent in 1999 to 2001 to 86.5 percent in 2002 and remains at or above 45 percent thereafter. Figure 1 compares the distribution of vote shares for bond measures with 55 and 66.7 percent cutoffs. Vote shares are lower for bonds requiring 55 percent, and in both cases, the bulk of the vote shares are above the cutoff. Both ITT and TOT analyses either control for the pass threshold or include bond measure fixed effects (so cutoff measures drop out).

Table 2 provides descriptive statistics for district-year observations in the stacked panel data used for the ITT analyses (which includes observations two years before and 10 years after the bond measure). On average, achievement among low-SES students is 109 points (14 percent) below that of high-SES students. The differences are similar in observations where the bond measure passed or failed. However, mean achievement is higher for both low- and high-SES students in observations where the bond measure passed. Specifically, mean low-SES API score is
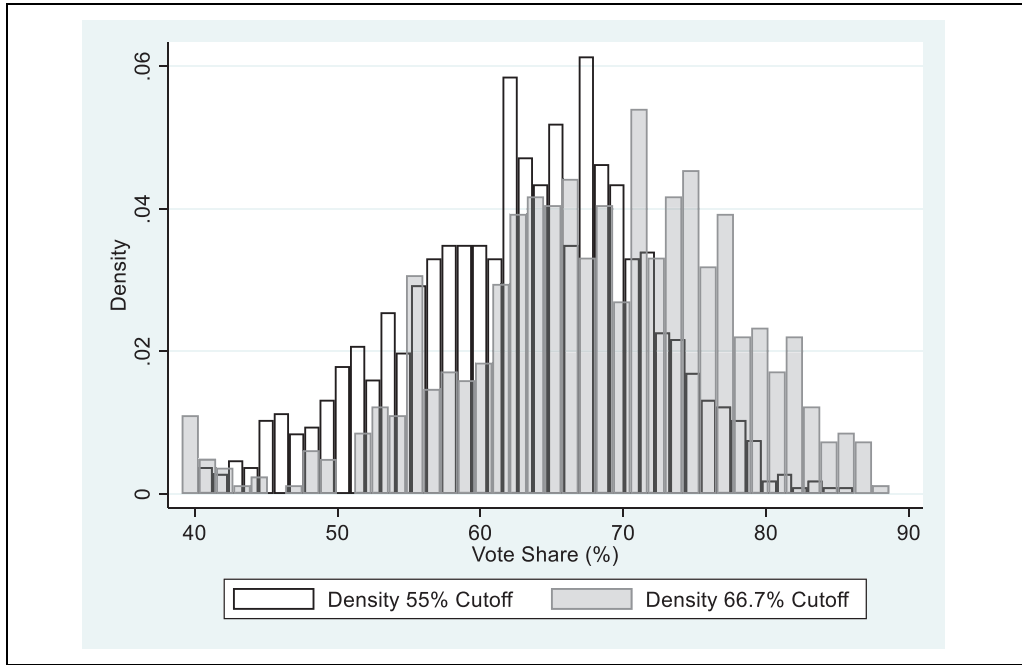
Figure 1. Vote share distribution by threshold required to pass.
Source: California Elections Data Archive 1999 to 2013.
Note: Limited to general obligation bond measures with vote shares 40 to 90 percent. Figure depicts the distribution of the percentage of votes for a general obligation bond measure by cutoff required to pass the measure.

20 points higher and mean high-SES API is 16 points higher among observations with a passed bond measure. Mean API score for all students is 19 points higher with a passed bond measure.

The last columns in Table 2 compare districts the year before the election by the outcome of the bond measure. Compared to districts that failed the bond measure, districts that passed the measure have slightly higher API scores (about 5 points) the year before the election for low-SES, high-SES, and all students. However, these differences are not statistically significant. Spending measures are slightly lower the year before the election among districts that passed the measure. This difference is significant only for capital outlays per pupil ($p < .01$) and suggests districts that passed a bond measure invested approximately $510 less per student in capital the year before the bond measure compared to districts that failed a measure. Finally, enrollment is higher the year before the election in districts that passed the bond ($p < .05$). Overall, these comparisons suggest achievement is similar before the election by bond outcome. However, capital spending is

lower and enrollment is higher the year before in districts that passed a bond. These differences could be reasons for proposing and passing a bond measure if districts are crowded and do not have enough money for facility investments. In analyses that follow, the preferred models control for district enrollment.

To further assess balance before the election, Table 3 presents results of models estimating the effect of passing a bond on district characteristics (achievement, spending, and students tested) before the election. Models 1 to 4 predict characteristics the year before the election, and models 5 to 8 predict the change in these characteristics from two years to one year before the election (year $t - 2$ to $t - 1$). The baseline model includes calendar-year fixed effects and controls for the pass threshold, vote share, and vote share squared. These models are limited to observations the year before the focal bond election to estimate preelection district characteristics. Models 2 and 6 add vote share cubed. Results of models 1 and 2 suggest districts that passed a bond had lower capital and total spending per pupil the year before the

Table 2. Descriptive Statistics of School District-Year Observations.

Variables	All districts	Failed bond measure	Passed bond measure	Failed bond measure $(t-1)$	Passed bond measure $(t-1)$	Difference pass-fail $(t-1)$
Low-SES API	673.28	658.97	679.23	661.13	665.98	4.85
High-SES API	782.49	770.97	787.29	771.49	777.27	5.78
Total API	734.00	720.48	739.62	722.57	727.94	5.37
Total spending per pupil ($1,000s)	11.81	11.40	11.98	11.81	11.31	–0.50
Capital outlays per pupil ($1,000s)	1.78	1.71	1.81	1.82	1.31	–0.51**
Instructional spending per pupil ($1,000s)	5.77	5.65	5.83	5.87	5.79	–0.08
Enrollment (log)	8.45	8.34	8.49	8.07	8.47	0.39*
N district years	13,675	4,017	9,658	254	885	

Source: California Department of Education Academic Performance Index (API) data 1999 to 2013 and California Elections Data Archive 1999 to 2013.
Note: Limited to district-year observations with achievement data for low- and high-socioeconomic-status (SES) students. Observations for certain measures are smaller: enrollment, $N = 13,549$; spending measures, $N = 13,516$.
*$p < .05$. **$p < .01$ (two-tailed t tests indicate significant mean differences by election outcome).

election ($p < .05$). In model 5, districts that passed a bond experienced a 2 percent decrease in the proportion of tested students who were low SES from year $t-2$ to $t-1$ ($p < .05$). Other estimates are not significant, suggesting balance before the election by outcome.

The other models in Table 3 use the same approach as the main ITT analyses and include district-year observations 2 years before to 10 years after the focal bond measure. The sample in these analyses is limited to elections within the RD sample on vote share (± 3.4 percent of vote share from the pass cutoff, selected based on the data using rdbwselect in Stata). Models 3 and 7 include district fixed effects and time-varying demographic controls. Models 4 and 8 add election-measure fixed effects. In these more rigorous models, there are no significant preelection differences between districts that passed or failed a bond measure. Estimates in these models are therefore consistent with balance before the election by outcome.

ITT Estimates

Table 4 presents results of the ITT analyses. Models 1 to 3 predict low-SES achievement, and models 4 to 6 predict high-SES achievement. The sample is limited to districts within a narrow window on either side of the pass cutoff (± 3.4 percent of

vote share from the pass cutoff), and all models include fixed effects for focal election measure, calendar year, and year since the election measure as well as controls for vote share and district demographic characteristics. Models 2 and 3 add vote share squared and cubed, respectively. Coefficients provide estimates of the achievement effects of passing a bond measure one to six years ago.

Contrary to Hypothesis 2, no coefficients are significant when predicting high-SES achievement. Estimates (not shown) are similarly null when predicting achievement among all students in a district. When predicting low-SES achievement, coefficients also fail to reach statistical significance until six years after the bond measure. Among districts that narrowly passed or failed a bond measure, passing the bond increased achievement among low-SES students six years later. Depending on the functional form of vote share included, this increase ranges from 33 ($p < .10$) to 48 ($p < .05$) points, and it is larger when controlling for higher polynomials of vote share. These estimates amount to 0.40 to 0.57 standard deviations or 5 to 7 percent of the mean low-SES API score and 29 to 41 percent of the mean gap between low- and high-SES scores. Thus, consistent with Hypothesis 1, passing a bond measure increases achievement among low- but not high-SES students. However, these

Table 3. Estimated Balance of Treatment and Control Groups Prior to Bond Election Measure.

	Year prior to election (t − 1)				Change prior to election (t − 2 to t − 1)			
Predicted outcome measure	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low-SES API	2.13	−5.25	5.72	−0.79	−0.47	1.06	−9.00	−9.98
High-SES API	7.46	7.02	1.79	1.46	−6.14	−5.49	−16.09	−18.35
Capital outlays per pupil ($1,000s)	−0.41**	−0.44**	3.48	3.71	−0.05	−0.10	2.72	2.82
Total spending per pupil ($1,000s)	−0.41	−0.66*	3.49	3.62	0.16	−0.18	2.66	2.80
Instructional spending per pupil ($1,000s)	0.03	−0.08	0.06	−0.04	0.16	0.03	0.21	0.22
Students tested (log)	0.18	0.11	−0.02	0.00	0.11†	0.03	0.26	0.22
Percentage of tested students who are low SES	−0.03	−0.04	−0.03	−0.04	−0.02*	0.00	−0.01	−0.01
Percentage of students tested	−0.33	−13.27	7.66	6.23	15.74	7.25	−35.46	−31.30
Year fixed effects and vote share²	Y	Y	Y	Y	Y	Y	Y	Y
Vote share³	N	Y	Y	Y	N	Y	Y	Y
Includes multiple years and district fixed effects	N	N	Y	Y	N	N	Y	Y
Bond-measure fixed effects	N	N	N	Y	N	N	N	Y

Source: California Department of Education Academic Performance Index (API) data 1999 to 2013, California Elections Data Archive 1999 to 2013, and F-33 Census data.

Note: Currency is measured in thousands of 2014 dollars. Baseline models (1 and 5) include fixed effects for calendar year and controls for pass threshold, vote share, and vote share squared. Models 2 and 6 add vote share cubed. Models 3 and 7 include district-year observations 2 years before to 10 years after the focal bond election and within the regression discontinuity sample on the running variable (±3.4 percent from the pass cutoff) and include controls for enrollment (log), number of schools (log), percentage free-lunch eligible, percentage black, and percentage Hispanic students in the district. Models 4 and 8 add bond-measure fixed effects. Controls are allowed to vary by election passage. Vote share and pass measures are allowed to vary by time since measure but are set to zero in year t − 2. Coefficients in models 3, 4, 7, and 8 are interactions between indicators for passing a bond measure and the year before the election measure (t − 1). Robust standard errors are adjusted for district clustering. N = no; Y = yes.
†p < .10. *p < .05. **p < .01.

benefits are delayed and do not emerge until six years after the bond election.

Figures 2 and 3 support this pattern of delayed benefits for low-SES students. They show trends in mean API scores by SES before and after a bond election. Figure 2 includes district-year observations for bond elections that narrowly passed. Figure 3 is limited to districts that narrowly passed the first bond measure in the observed time range (1999 to 2013). Both figures suggest an initial decline in achievement after the election and a larger decline for low-SES students. However, in both figures, low-SES achievement increases at a faster rate than high-SES achievement after the election and is higher than the pre-election mean by six years postelection.

Part of the delayed effects could reflect spending patterns. Table 5 shows results of ITT estimates when predicting spending and student characteristics, using the same approach as models 2 and 5 in Table 4. Consistent with Hypothesis 3, narrowly passing a bond measure increases capital

Table 4. Estimated Effects of Passing a Bond Measure on Achievement by Socioeconomic Status (SES).

Variable	Low-SES achievement			High-SES achievement		
	(1)	(2)	(3)	(4)	(5)	(6)
Years after election						
1	7.31	6.71	10.97	2.05	1.87	−1.95
	(11.74)	(11.89)	(18.86)	(8.74)	(8.83)	(13.21)
2	16.41	17.06	10.08	7.47	7.80	15.20
	(13.61)	(13.46)	(21.24)	(11.13)	(11.11)	(15.18)
3	19.05	18.85	20.10	9.38	9.50	16.55
	(13.79)	(13.64)	(20.66)	(10.49)	(10.44)	(15.74)
4	−0.24	−0.17	25.83	3.58	3.04	27.57
	(15.07)	(14.91)	(22.90)	(17.71)	(17.14)	(26.08)
5	13.08	13.15	30.57	−0.35	−0.95	7.35
	(15.38)	(15.15)	(21.96)	(11.26)	(11.31)	(14.28)
6	33.20†	35.50*	47.77*	8.08	8.67	12.36
	(17.51)	(17.34)	(23.74)	(12.92)	(13.04)	(16.59)
Constant	272.48**	289.29**	287.08**	590.89**	600.83**	602.29**
	(104.45)	(102.03)	(100.91)	(85.39)	(86.11)	(86.27)
Fixed effects for election measure, calendar year, and year since election	Y	Y	Y	Y	Y	Y
Vote share^2	N	Y	Y	N	Y	Y
Vote share^3	N	N	Y	N	N	Y
Observations	2,833	2,833	2,833	2,833	2,833	2,833
R^2	0.93	0.93	0.93	0.86	0.86	0.86

Source: California Department of Education Academic Performance Index data 1999 to 2013 and California Elections Data Archive 1999 to 2013.
Note: Limited to district-year observations with low- and high-SES achievement information, from 2 years before to 10 years after the focal bond election, and within the regression discontinuity sample on the running variable (± 3.4 percent from the pass cutoff). All models include fixed effects for the focal bond election, calendar year, and year since the election as well as controls for vote share, enrollment (log), number of schools (log), percentage free-lunch eligible, percentage black, and percentage Hispanic students in the district. District demographic coefficients are allowed to vary by election passage. Vote share and pass measures are allowed to vary by time since measure. Coefficients are interactions between indicators for passing a bond measure and years since the election. Robust standard errors adjusted for district clustering are in parentheses. N = no; Y = yes.
†$p < .10$. *$p < .05$. **$p < .01$.

spending by $2,840 per student and total spending by $2,950 per student but with a two-year delay ($p < .05$). No other coefficients achieve significance at the 95 percent level, suggesting that narrowly passing a bond measure does not influence instructional spending, number or percentage of students tested, numbers of low- or high-SES students tested, or the percentage of tested students who are low SES within six years of the election.

Although no other coefficients achieve statistical significance, coefficients predicting capital and total spending remain positive until six years after the election. That is the same relative year when effects of passing a measure emerge for low-SES

achievement. Figure 4 shows changes in mean capital spending per pupil by time since the election. The graph suggests a slight downward trend before the election (consistent with preelection differences in Tables 2 and 3). After the election, mean capital spending remains stable for one year but increases sharply two years after the election. Mean capital spending continues to increase in years 3 and 4 following the election but drops by five years after the election. This pattern is consistent with expected delays because districts must collect and plan for capital investments. The results suggest that districts spend bond revenue with a delay after the election.
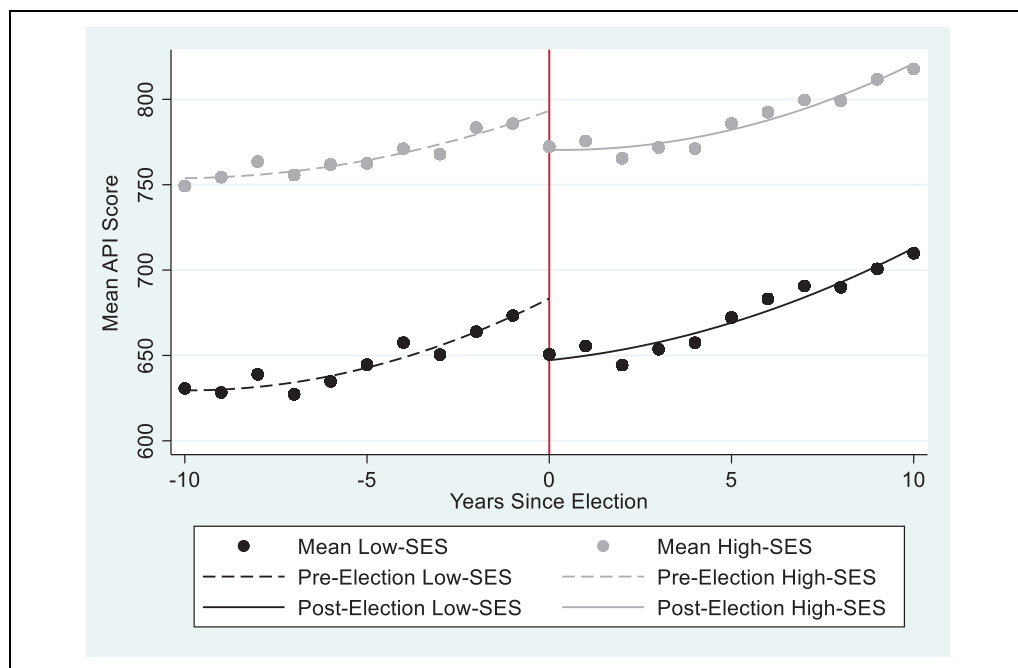
Figure 2. API score by socioeconomic status (SES) and time since election measure.
Source: California Department of Education Academic Performance Index (API) data 1999 to 2013 and California Elections Data Archive 1999 to 2013.
Note: Limited to district-year observations with achievement information for bond measures that narrowly passed within the regression discontinuity window of vote share (± 3.4 percent from the cutoff). Quadratic trendlines illustrate a greater drop in low-SES API and steeper increase than high-SES API after the bond election.

Sensitivity Analyses

I conducted a series of sensitivity analyses to assess robustness. First, results of TOT analyses are discussed in the online appendix and shown in Table S12. Overall, the TOT estimates show no effects on high-SES achievement and delayed benefits for low-SES achievement.

Second, Table S1 in the online appendix provides results of the same ITT estimates in Table 4 when controlling for a one-year lag measure of the dependent variable. Results are consistent with those in Table 4, although the coefficients for six years postelection are slightly smaller, and in models 1 and 3 they reach only marginal significance ($p < .10$).

Third, Table S2 in the online appendix shows results of the same ITT estimates in Table 5 but including vote share cubed. Results suggest null effects on most outcomes, similar to Table 5. However, effects on total spending do not reach

significance, and the coefficient for two years postelection is only marginally significant ($p <$ .10) when predicting capital outlays. Thus, effects on spending are not robust to including a cubic in vote share.

Fourth, I repeat ITT estimates in Table 4 using several placebo checks as falsification tests (see Table S3 in the online appendix). Specifically, I create false pass cutoffs 5 and 10 percentage points above and below the actual pass cutoff and estimate effects using those alternative thresholds. Whether including vote share squared or cubed, estimates indicate null effects using these false cutoffs and support the validity of estimates at the actual cutoff.

Fifth, I repeat the ITT estimates varying the RD bandwidth from 2.4 to 4 percent above and below the pass threshold (see Table S4 in the online appendix). Results consistently support evidence of delayed benefits for low-SES achievement six years after the election and no effects
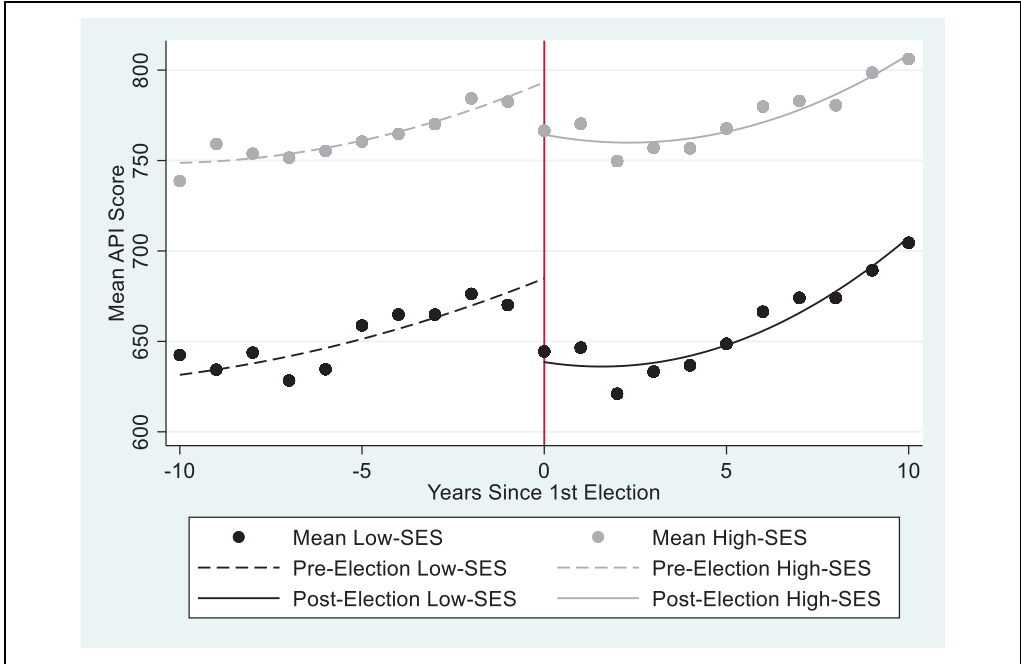
Figure 3. API Score by socioeconomic status (SES) and time since first election measure.
Source: California Department of Education Academic Performance Index (API) data 1999 to 2013 and California Elections Data Archive 1999 to 2013.
Note: Limited to district-year observations with achievement information for districts that narrowly passed the first bond measure from 1999 to 2013 within the regression discontinuity window of vote share (± 3.4 percent from the cutoff). Quadratic trendlines illustrate a greater drop in low-SES API and steeper increase than high-SES API after the first bond election.

on high-SES achievement. Figure S3 in the online appendix summarizes the estimates for six years after the election by SES.

Sixth, I estimate effects of passing a bond measure by the vote share threshold required to pass. Among low-SES students, estimates from ITT analyses do not reach significance at the lower cutoff. This could be due to the smaller sample size (1,132 vs. 1,689), differences in the election measures, or their later timing (see Table 1). Thus, ITT effects on low-SES achievement appear to be driven largely by bond measures with the higher vote share requirement. Results from the TOT analyses predicting low-SES achievement suggest the effects of passing a bond measure do not differ by the cutoff required to pass. There is some evidence that passing a bond at the 55 percent threshold may reduce high-SES achievement two years later. However, this is not found in the other analyses. Overall, results are broadly consistent at both pass thresholds, but one approach suggests null effects at the lower threshold.

Seventh, I conduct analyses using standard district-level panel data and a traditional RD approach to estimate the effects of passing an election measure on current-year achievement without addressing the dynamic characteristics of bond election measures. Results of these analyses (see Tables S5 and S6 in the online appendix) suggest that narrowly passing a bond measure initially reduces achievement among low-SES but not high-SES students. Table S7 in the online appendix shows estimates when varying the RD bandwidth: negative effects emerge for high-SES students at some bandwidths, but effects are smaller than those for low-SES students. These initial effects are consistent with the pattern in Figures 2 and 3.

Context and Mechanisms

To provide more information about the contexts in which these results hold, I repeat the ITT analyses but limit the sample to districts above and below

the median values of district percentage eligible for free lunch, total enrollment, and number of schools. Bond measures often focus on improving one or two schools in a district. I therefore expect to find stronger effects in smaller districts with fewer schools. Furthermore, if school context matters more for low-SES students, then passing a bond measure may have stronger effects in districts with more students eligible for free lunch. In support of these expectations, results in Table S8 in the online appendix suggest heterogeneous effects of passing a bond measure by each of these district characteristics.

First, the main ITT effects hold in districts with high free-lunch eligibility but not in districts with low eligibility. This suggests passing a bond measure increases achievement among low-SES students (after a delay) in low-income but not high-income districts. Effects in districts with more students eligible for free lunch are larger than in the full sample (although coefficients are not significantly different) and emerge five and six years after passing a bond measure ($p < .05$).

Second, passing a bond measure has different effects by enrollment. For example, when including full controls, passing a bond increases low-SES achievement in years 5 and 6 after the election but only in small districts (i.e., below median enrollment). Effects are also significant in years 2 and 3 after the election but only in models without full controls. When limited to large districts, effects on low-SES achievement are null. In contrast to the main results, passing a bond measure also increases high-SES achievement in districts below median enrollment. These effects are smaller than those for low-SES achievement and do not hold in the model with full controls.

Third, passing a bond measure increases low-SES achievement six years after the election in all models limited to districts with nine or fewer schools (i.e., below the median number of schools). In model 3 with full controls, effects are significant in years 4, 5, and 6 after the election measure. In contrast, effects are null in districts with a larger number of schools and in models predicting high-SES achievement.

Overall, passing a bond measure increases low-SES achievement in small and low-income districts, and these effects emerge most consistently five and six years after the election. This pattern is consistent with the possibility that bond measures increase achievement by improving context more for low-SES students. That is, the effects of passing a bond measure are greater when a higher proportion of students in the district experience the capital improvement (lower enrollment and fewer schools) and are from low-income families. Future research could use data on physical school characteristics to test this mechanism directly.

To provide more information about potential mechanisms, I gathered data on teacher characteristics from the California Basic Educational Data System (CBEDS), provided by the CDE. The CBEDS Certificated Staff Profile provides information about certificated staff at each school from 1999 to 2009. Weighting by the number of staff at each school, I created district-level mean values of total years of experience, years of experience in the district, and proportion with a bachelor's degree or less. ITT estimates show no effects of passing a bond measure on teacher characteristics. TOT estimates suggest passing a bond measure reduces the average years of teacher experience two years after the election. Effects on capital outlays are also found two years after passing a bond, suggesting experienced teachers may leave or retire before construction begins. Other estimates are not significant at the 95 percent level. These analyses suggest passing a bond measure has limited effects on teacher quality (see Tables S9 and S10 in the online appendix). However, these data cover a shorter time range than the main analyses.

Finally, the main analyses suggest the effects of passing a bond measure on low-SES achievement may be limited to a single year. To address this concern, I estimate effects of passing a bond measure on low-SES achievement when limiting the sample to districts with fewer than five schools. The effects of passing a bond measure should be stronger in districts with few schools, where a larger proportion of students experience the benefits of capital investments. Table S11 in the online appendix shows the results and includes coefficients for years 1 to 9 after the election. Results suggest that passing a bond measure increases low-SES achievement six and seven years after the election in all models. Coefficients are also significant at years 3 and 9 after the election in models 1 and 2 (and marginally significant

Table 5. Estimated Effects of Passing a Bond Measure on District Spending and Students Tested.

Variable	(1) Capital outlays per pupil ($1,000s)	(2) Total spending per pupil ($1,000s)	(3) Instructional spending per pupil ($1,000s)	(4) Students tested (log)	(5) % of students tested	(6) % of tested students who are low SES	(7) Low-SES students tested	(8) High-SES students tested
Years after election								
1	1.65	1.55	−0.13	0.22†	17.24	0.03	369.43	618.42
	(1.28)	(1.40)	(0.13)	(0.12)	(19.58)	(0.02)	(920.02)	(438.18)
2	2.84*	2.95*	−0.09	0.29	29.45	0.05†	1,469.13	1,022.07†
	(1.18)	(1.29)	(0.15)	(0.17)	(30.66)	(0.03)	(2,016.65)	(601.38)
3	2.40	2.22	−0.25	0.09	0.59	0.04	4,534.47	822.61
	(1.54)	(1.73)	(0.18)	(0.20)	(24.78)	(0.03)	(3,202.56)	(916.16)
4	2.30	2.17	−0.18	0.09	−0.08	0.01	3,908.08	376.74
	(1.71)	(1.91)	(0.22)	(0.22)	(26.22)	(0.03)	(2,993.24)	(1,006.50)
5	0.56	0.11	−0.26	−0.04	23.48	0.03	2,876.87	213.79
	(1.15)	(1.37)	(0.25)	(0.24)	(24.28)	(0.03)	(2,708.48)	(950.59)
6	−0.15	−0.57	−0.36	0.00	25.79	0.02	2,708.93	−230.77
	(1.28)	(1.48)	(0.30)	(0.26)	(26.00)	(0.03)	(3,224.75)	(1,096.05)
Constant	40.65**	74.33**	10.63*	2.98	−266.79	0.38*	3,619.53	−2,142.25
	(9.66)	(15.76)	(5.32)	(2.04)	(165.54)	(0.19)	(16,799.00)	(6,869.92)
Fixed effects for election measure, calendar year, and year since election	Y	Y	Y	Y	Y	Y	Y	Y
Vote share^2	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3,264	3,264	3,264	2,962	2,728	2,962	2,962	2,962
R^2	0.32	0.61	0.86	0.93	0.21	0.95	0.54	0.66

Source: California Department of Education Academic Performance Index data 1999 to 2013, California Elections Data Archive 1999 to 2013 and F-33 Census data.
Note: Limited to district-year observations with low- and high-SES achievement information, from 2 years before to 10 years after the focal bond election, and within the regression discontinuity sample on the running variable (±3.4 percent from the pass cutoff). Currency is measured in thousands of 2014 dollars. All models include fixed effects for the focal bond election, calendar year, and year since the election as well as controls for vote share, enrollment (log), number of schools, percentage free-lunch eligible, percentage black, and percentage Hispanic students in the district. District demographic coefficients are allowed to vary by election passage. Vote share and pass measures are allowed to vary by time since measure. Coefficients are interactions between indicators for passing a bond measure and years since the election. Robust standard errors adjusted for district clustering are in parentheses. SES = socioeconomic status; N = no; Y = yes.
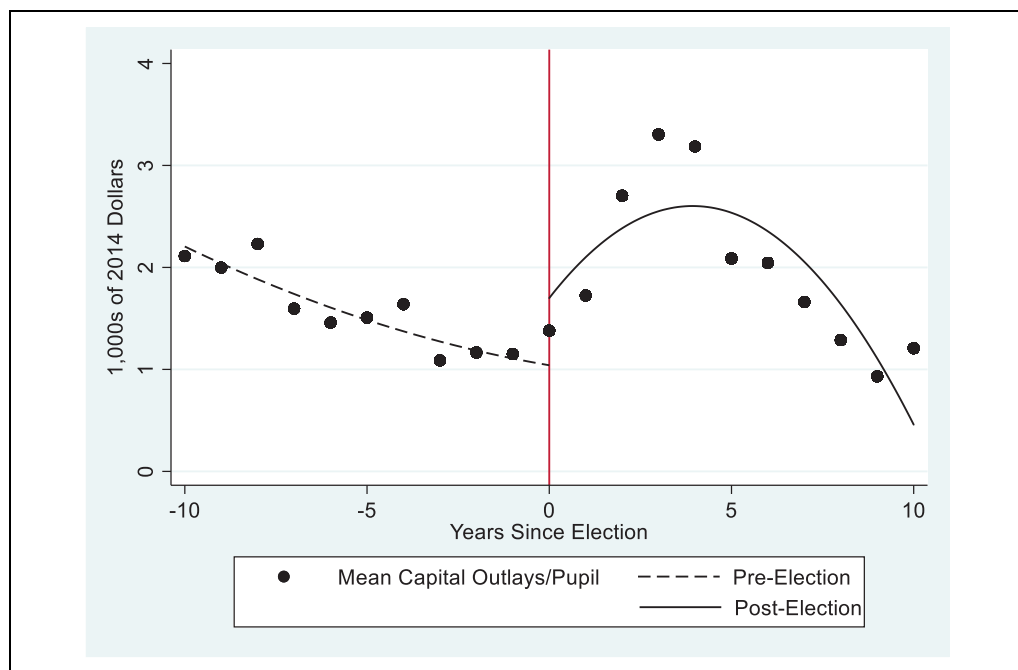†$p < .10$. *$p < .05$. **$p < .01$.

Figure 4. Capital outlays per pupil by time since election measure.
Source: California Department of Education Academic Performance Index data 1999 to 2013 and F-33 Census data.
Note: Limited to district-year observations with achievement measures in districts that passed a bond measure and within the regression discontinuity window of vote share (± 3.4 percent from the cutoff). Quadratic trendlines illustrate a large, delayed increase in capital outlays after the bond election.

in model 3 in those years and in all models for years 5 and 8 after the election).

Models 4 to 6 in Table S11 in the online appendix show estimated effects on capital outlays per pupil in districts with fewer than five schools. Results are consistent with those predicting capital outlays in the full sample and suggest that capital spending increases significantly two years after a bond passes. However, these estimates are larger when limited to districts with fewer than five schools ($4,260 per pupil compared to $2,840 in the full sample), suggesting a larger treatment dose (investment per pupil) in smaller districts. Overall, these results suggest the effects of passing a bond measure are not limited to a single year. Rather, in small districts where students receive a relatively large dose, the effects persist up to nine years after the bond measure passed.

CONCLUSION

Existing research provides contradictory evidence about the effects of education funding on student achievement (e.g., Jackson et al. 2016; Morgan and Jung 2016). Possible explanations for these contradictory results include heterogeneous effects of funding by student characteristics and revenue source. Education funding may benefit socioeconomically disadvantaged students more than others if they receive less academic input at home (K. Alexander et al. 2007; Entwisle et al. 1998; Lareau 2003). Furthermore, local revenue may be distributed more unequally within districts than state or federal revenue (Condron and Roscigno 2003; Timar and Roza 2010), hindering achievement returns among disadvantaged students who might otherwise benefit most. For

example, some evidence suggests that one type of local revenue—facilities funding—has limited efficiency for achievement (Martorell et al. 2016). This article uses dynamic RD analyses to estimate the effects of passing a local school district bond measure on achievement among high- and low-SES students in California. By explicitly examining heterogeneity in the relationship between funding and achievement, this study moves beyond the long-standing debate about whether funding matters to examine when and for whom it matters.

Dynamic RD estimates indicate that narrowly passing an election measure has no effect on high-SES achievement but increases low-SES achievement after a delay. Specifically, I find that passing a bond measure increases low-SES achievement by around half of a standard deviation or about 6 percent of the mean. These benefits amount to approximately a third of the mean gap between low- and high-SES achievement in the time period. However, these benefits for low-SES acheivement are delayed and do not emerge until six years after the election measure.

To put the results in context, the standardized coefficient predicting low-SES achievement six years after passing a bond measure is 0.29 (from Table 4, model 3, 47.77 × 0.5/82.3). Dividing by the average per-pupil revenue at stake in close bond election measures suggests that the effect of passing a bond measure on low-SES achievement is 0.04 standard deviations per \$1,000 dollars of facilities funding for each student (0.29/8.23). In districts with fewer than five schools, the equivalent effect of passing a bond measure on low-SES achievement is 0.08 standard deviations per \$1,000 dollars of facilities funding for each student (0.89/10.88 based on Table S11, model 3). Chetty, Friedman, and Rockoff (2014) find that a 0.2-standard-deviation increase in student achievement yields a 2 percent increase in annual lifetime earnings. These effects (0.04 or 0.08 standard deviations for each \$1,000 invested per pupil) are around the 50th percentile in Kraft's (2018) guidelines for interpreting effects in light of cost.

Delayed achievement benefits are consistent with the finding that effects on capital spending emerge after a shorter delay than effects on low-SES achievement. Results suggest that narrowly passing a bond measure increases capital spending by \$2,840 per student two years after the measure. Thus, the results suggest that districts take time to spend bond revenue, possibly due to delays in

planning for capital investments and construction. Passing a bond measure increases capital outlays two years after the election and increases low-SES achievement six years after the election. This suggests achievement benefits emerge after capital investment in facilities are completed and the proverbial dust has settled.

The pattern of results is consistent with previous evidence of reduced achievement during construction (Goncalves 2015) and of temporary disruptions from capital investments in relatively poor school districts (Conlin and Thompson 2017). However, this study explicitly examines heterogeneity by SES and provides greater generalizability by including wealthier districts, as well (Taylor 2015). Particularly when estimating current-year effects (Tables S5 and S6 in the online appendix), the results are also consistent with the possibility that election measures create temporary disruptions to student learning, and low-SES achievement is most sensitive to those disruptions. For example, facility improvements may require construction, temporary relocation of a school, or teacher time or training to use new technology (Conlin and Thompson 2017; Leuven et al. 2007). Indeed, evidence from Ohio suggests test scores decline during construction (Goncalves 2015). If learning among low-SES students depends more on context compared to high-SES students, who receive more academic input at home (K. Alexander et al. 2007; Entwisle et al. 1998; Lareau 2003), temporary disruptions may reduce low-SES achievement while leaving high-SES achievement unchanged.

Following this temporary setback, low-SES achievement increases at a faster rate than high-SES achievement (see Figures 2 and 3). This pattern is consistent with the possibility that achievement is more sensitive to context among low-SES students than high-SES students. In fact, low-SES students may be more sensitive to context than estimated here, because only a subset of students in a district is influenced by a bond measure. Districts often renovate or build one school at a time, so only a proportion of students experience disruption and then improved context. Thus, the results of the main analyses are likely lower-bound estimates. Analyses limited to districts with fewer than five schools where a larger proportion of students are treated (and with a larger dose) suggest larger effects, which persist for up to nine years. Data on spending within districts are rare, but when they become available, future research could

examine effects of bond passage on within-district spending inequality and on achievement among students in schools that received the most investment.

There are several additional limitations to this study. First, analyses examine districts in California, which limits generalizability. Local tax initiatives vary by state in their flexibility and whether voters are involved. However, California presents a valuable context because education funding is largely determined by the state, and school district election measures provide almost the only local opportunity to increase school district revenue. This is particularly true during the time period examined here. This study examines years before the LCFF, when local revenue was highly constrained. Recent evidence suggests revenue may hold more potential to reduce achievement gaps when there are fewer spending constraints (i.e., after the LCFF; R. Johnson and Tanner 2018). Future research could build on this finding to examine the relationship between local funding elections and achievement by SES in contexts with less constraint on local revenue. Second, analyses examine achievement by SES within districts rather than state- or nationwide achievement. Evidence suggests inequality within districts is at least as critical as inequality between districts (Guin et al. 2007; Lafortune et al. 2016; Roza 2010). Thus, estimates identify the effects of passing a local bond measure on relatively local inequality of achievement and cannot address inequality between districts. Finally, this study cannot identify mechanisms. However, results examining effects on capital outlays are consistent with the possibility that effects are driven by the temporary disruptions and then improved context of facility improvements.

Despite these limitations, the results inform our understanding of the relationship between education funding and achievement. First, results suggest that effects of education funding vary by students characteristics. Specifically, education funding does not increase aggregate achievement and seems to affect low-SES more than high-SES student achievement, with stronger effects in districts with more low-income students. Passing a bond measure may initially reduce achievement among low-SES but not high-SES students. After this temporary setback, however, passing a bond measure has delayed benefits for low-SES but not high-SES achievement. Second, contrary to some previous evidence (Martorell et al. 2016) but consistent with other studies (Cellini

et al. 2010; Hong and Zimmer 2016), results suggest elections related to facilities funding influence achievement. Results in California suggest election measures may initially harm but then improve achievement among low-SES students after about six years. Thus, previous evidence of null effects may reflect hetergeneous effects by SES and variation of effects over time, with initial setbacks countered by longer-term benefits.

Overall, results suggest that passing a school-district bond election measure, which increases local revenue, does not affect high-SES achievement but has delayed benefits for low-SES achievement. Thus, passing a bond measure may improve equality of opportunity in the long term. Results suggest that districts or states could work to mitigate potential temporary disruptions among low-SES students following an election. If the temporary disruptions could be reduced, the gains from passing a bond measure may become more apparent, investments would be more efficient, and equality of opportunity could improve. Furthermore, because high-SES students and districts benefit less from capital investments, and wealthier districts are more likely to invest in facilities (Brunner 2007; Taylor 2015), school facility investments may be more efficiently directed toward poor rather than wealthy districts.

RESEARCH ETHICS

This study uses secondary data and no human subjects were involved.

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SUPPLEMENTAL MATERIAL

Appendices are available in the online version of this journal.

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