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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, this study uses dynamic regression discontinuity models to estimate effects of passing a bond on academic achievement among low- and high-SES students. Results consistently suggest that passing a bond measure increases achievement among low- but not high-SES students. However, these benefits for low-SES students are delayed and emerge 6 years after an election.

Introduction

Debates about the efficiency of education funding for student achievement have continued at least since the 1966 Coleman Report (e.g., Hanushek 1989, 1996; Burtless 1996; Greenwald et al. 1996; see Biddle and Berliner 2002 and Baker 2016 for reviews), including contemporary evidence of 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 (Lafortune, Rothstein, and Schanzenbach 2016; Jackson, Johnson and Persico 2016; Candelaria and Shores 2017).

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 students, for example, due to fewer opportunities for academic learning outside of school (e.g., Lareau 2003). Alternatively, the effects of 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 (Rickard et al. 2012; Southgate and Roscigno 2009; Kinney 2008; Costa-Giomi 2004). Similarly, local school facilities funds – to maintain or improve school buildings, grounds, or equipment – could hold relatively little benefit for daily school district operations or programs and may hold less potential to increase average achievement (e.g., Martorell et al. 2016). However, if achievement among socioeconomically disadvantaged students is more dependent on school context (e.g., 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 the effects of California school district general obligation bond election measures on academic achievement by socioeconomic status (SES) from 1999 to 2013. California presents a particularly 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 still cap local district revenue (Taylor 2013). School district election measures before 2013 were "essentially the only source of local discretion" (Cellini et al. 2010:218). Given the limited ability of California districts to increase their revenue in other ways, California school district election measures allow more precise estimates of local funding than Texas, where Martorell and colleagues (2016) found null effects. The period from 1999 to 2013 excludes years after the LCFF, a policy change that could change the relationship between local funding elections and student achievement.

The next section reviews research on the relationship between local funding and achievement. Methods and results sections are followed by a discussion of the implications of the findings, including potential policy implications.

Theoretical and Empirical Background

Local School Revenue and Achievement by Socioeconomic Status

Recent estimates of the effect of school funding on achievement have focused on changes in state funding (Lafortune, Rothstein, and Schanzenbach 2016; Jackson, Johnson and Persico 2016; Candelaria and Shores 2017). 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 other sources of funding (Baker and Weber 2016; Baker et al. 2017; Education Law Center 2013).

In contrast, districts distribute local funding more unequally than state or federal funding (Timar and Roza 2010; Condron and Roscigno 2003). Local revenue comes with fewer spending restrictions, such as categorical funds, than state revenue. When distributing unrestricted (non-categorical, including local) funds, evidence suggests that districts favor schools with more advantaged students (Heuer and Stullich 2011; Timar and Roza 2010; Roza and Miles 2002). In fact, Roza and colleagues (2007) examine the distribution of non-categorical funds and find that funding inequality within districts is greater than funding inequality 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). Unlike state revenue, therefore, local revenue increases may hold limited ability to increase achievement, particularly among low-SES students. Due to its more unequal distribution within districts, local revenue may only increase achievement among high-SES students.

Facilities Funding as a Type of Local Revenue

Partly because of endogeneity concerns about the relationship between local revenue and achievement, estimates of the effects of local revenue on achievement are relatively rare. Research on this topic tends to focus on funding for school facilities (e.g., 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. This is because 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 (CDIAC 2008). Since Proposition 39, bond measures can be approved under the 55 percent (rather than two-thirds) threshold, but must meet additional requirements.

Although these funds are limited to specific expenses, the ballot measure may not specify how they are distributed within the district. As with other local revenue, therefore, districts could use local facilities funds to improve context more in schools with higher-SES students (Roza 2010; Condron and Roscigno 2003). This is consistent 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% eligibility for free or reduced-price lunch, the average school with at least 75% eligibility is 6 years older and 12 percentage points more likely to require expenditures to achieve good condition (Alexander and Lewis 2014:10, 20).

The quality of school facilities is related to teacher satisfaction and retention (Buckley et al. 2004; Johnson et al. 2012), school social climate (Maxwell 2016; Uline and Tschannen-Moran 2008), and student attendance (Maxwell 2016; Duran-Narucki 2008). 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).

Partly due to teacher quality and retention, social climate, and attendance, some research finds a positive relationship between facility quality and student achievement (Maxwell 2016; Uline and Tschannen-Moran 2008; Duran-Narucki 2008). Crowding, technology, air quality, temperature, lighting, morale, or local housing values offer other potential explanations for a link between school facilities and student achievement (e.g., 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 Hanushek 1997 and Gunter and Shao 2016). Research using a variety of methods (including instrumental variables, regression discontinuity, and difference-in-differences models) finds that school facility improvements or capital expenditures increase student achievement (Conlin and Thompson 2017; Hong and Zimmer 2016; Neilson and Zimmerman 2014). Individual fixed effects models estimating the effect of moving to a new school after Los Angeles Unified School District built 130 new facilities also find positive effects on achievement (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 that 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 (Hanushek 1997; Gunter and Shao 2016).

Heterogeneous effects by SES could account for some of the inconsistent findings. Context is critical for student achievement (Sharkey 2010; Sharkey and Elwert 2011; Sharkey et al. 2014) 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 (Alexander et al. 2007; Lareau 2003; Entwisle et al. 1998), achievement among low-SES students may depend more strongly on school facilities, such as technology, air quality, lighting, and space (absence of crowding) (e.g., 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 (e.g., Maxwell 2016; Duran-Narucki 2008). Quality facilities also provide better teacher working conditions, which can help attract and retain effective teachers in districts with a high proportion of low-SES students (Buckley et al. 2004; Horng 2009; 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 high-SES students? Existing evidence is consistent with the possibility of stronger effects of facilities funding 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. Further, 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 has explicitly investigated whether effects of local funding, including capital investments, 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. Furthermore, this pattern would suggest that investments in school facilities could be more efficiently directed to poor rather than wealthy districts.

<u>Local Revenue in the California Context</u>

To improve understanding of the implications of local funding, this study explicitly examines 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 election 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% from 1998 to 2006; Brunner 2007:2). General obligation bonds also represent the vast majority (76%) 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), while revenue from a tax election measure may be more flexible, depending on the wording of the ballot measure.

Similar to existing work (Kong and Zimmer 2016; Martorell et al. 2016; Cellini et al. 2010), I use a regression discontinuity (RD) approach to address the research questions, 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. While 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.

Based on the above review, I identify the following hypotheses. Because achievement among low-SES students should depend more on school context than high-SES students, *hypothesis 1* is 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 (e.g., Roza 2010; Timar and Roza 2010), then passing a bond measure could increase achievement more among high-SES than low-SES students in a district. Based on arguments about inequality of within-district revenue distribution, *hypothesis 2* is that passing a school district bond measure increases achievement among high-SES but not low-SES students. In either scenario, election measures secure future funding for district improvements. Therefore, effects should take several years to emerge because districts must first collect, plan, and then spend the revenue. However, if families and school personnel anticipate the future 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 "Bond 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 only be used for facility improvements, *hypothesis 3* is 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

This study examines 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 includes a large number of school districts (approximately 1,000 in 2013) with many close 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 revenue limits and most funding is determined by the state. School district election measures are a unique mechanism allowing local revenue in California (Jennison 2017; Cellini et al. 2010). The limited ability of districts 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 1999-2013 for all and low-SES students. API scores are based on tests taken in the spring of each academic year and 1999 scores represent testing completed in spring 1999. Consistent with that timing, year indicates spring of the academic year throughout this paper. For example, 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 socioeconomic status throughout the paper.

Ranging from 200 to 1000, 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 API calculation varies over time, but all models include year fixed effects to account for state-wide changes in scores over time.

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 the district-level means in later years.

Calculating district-level high-SES achievement requires an additional step, because it is not included in the CDE data. First, to calculate the school-level mean API for high-SES students in years 1999-2002, I use Equation 1 and the following values, which are included in the CDE data: mean API score for all students ($\text{Mean}_{\text{Total}}$), mean score for low-SES students, total number of students tested, number of low-SES students tested, and number of high-SES students tested.

$$\text{Mean}_{\text{High-SES}} = \frac{\left(\text{Students}_{\text{Total}} * \text{Mean}_{\text{Total}} - \text{Mean}_{\text{Low-SES}} * \text{Students}_{\text{Low-SES}}\right)}{\text{Students}_{\text{High-SES}}} \tag{1}$$

Based on these school-level scores, I calculate the district-level mean score for high-SES students as a weighted average of school-level mean high-SES scores (where the weights are the number of high-SES students tested in each school in the district).

In years 2003-2013, the CDE data provide district-level achievement for all and low-SES students (i.e. district-level low-SES averages do not need to be calculated from school averages in those years). I calculate district-level mean scores among high-SES students in years 2003-2013 using Equation 1.

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 or jurisdiction, excluding county and city elections and elections that are related to community college funding.

The CEDA data do not have NCES identification numbers to facilitate merging. I create a crosswalk between CEDA measure id and NCES id numbers to allow merging of 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 support from only 55% of voters if they meet certain requirements (Ed Data 2017; CDIAC 2008). (Thirteen measures require 50%, but results are the same when excluding these districts.) Using the CEDA data, 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 regression discontinuity 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 multi-county election results determine whether a measure passes. CEDA data include multi-county data, which are used to calculate the running and treatment variables when elections include multiple counties.

School District Spending and Demographics: In order to understand the potential mechanisms for the relationship between bond election outcomes and student achievement, I use NCES id to link the above election 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 1999-2013. Specifically, I calculate total spending per pupil, instructional spending per pupil, and capital outlays per pupil. All currency is adjusted for inflation to 2014 dollars.

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

Analytic Strategy

I use regression discontinuity (RD) analyses to understand the implications of an increase in local facilities funding on achievement. Specifically, I use outcomes of district general obligation bond election measures 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 passage of a ballot measure, 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 (Lee and Lemieux 2010; Imbens and Lemieux 2008). Key assumptions include: 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 (% of voters supporting a measure), which is controlled in the regression (Lee and Lemieux 2010:287). Districts where elections 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 Estimates: Despite key strengths, however, 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 previous failed election measures 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 (see Cellini et al. 2010 for additional details). 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 2 years before and 10 years after the election (*t*-2 to *t*+10). For example, if a district holds an election in 2000, I include district data for years 1998 through 2010. I combine or "stack" these data around each individual election from 1999 to 2013 into one data set, which can include the same district-years if districts hold multiple elections.

Using these stacked panel data, I estimate effects of passing a bond measure using Equation 2, 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 percent 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.

$$Score_{its} = \beta_s Pass_{it} + V_s(Vote\ share_{it}) + \delta_s + \mu_t + \pi_{it} + \varepsilon_{its} \qquad (2)$$

The coefficients of interest (β_s) estimate the intent-to-treat (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 1-6 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 tradeoff between internal validity and power. Primary analyses use a bandwidth selected based on the data (using rdbwselect in Stata, Calonico et al. 2014). However, I vary the width of the window in sensitivity analyses (Appendix Table S4 and Figure S3) to assess robustness.

Treatment-on-the-Treated 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” treatment-on-the-treated (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 above). 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.

$$Score_{it} = \sum_{s=0}^{s}(\beta_s Pass_{it-s} + \theta_s Election_{it-s} + V_s(Vote\ share_{it-s})) + \mu_t + \pi_i + \varepsilon_{it} \quad (3)$$

In Equation 3, 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 percent of votes for the bond measure centered at the cutoff in each previous year, with fixed effects for calendar year (μ), and district (π). 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 1-6 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.

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 (presented in the Appendix) all suggest null effects of these 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 that 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 that 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 here. The Appendix includes graphs of the forcing variable density and of several demographic measures (enrollment, and proportions of Black, Hispanic, Native American, and free lunch eligible students) by the forcing variable. Due to concern that likelihood of passing could differ by the proposed bond amount, the Appendix also includes a graph 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 to estimate the effects of passing an election measure 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 election measures. Results of these analyses are presented in the Appendix.

Results

Descriptive Statistics and Balance

Table 1 provides descriptive statistics of school district bond election measures from 1999 to 2013 in the CEDA data. The number of bond elections varies substantially over time along with the pass rate, which varies from 50% in 2009 to 87.5% in 2011, the two years with the fewest bond measures (4 and 8, respectively). In contrast, vote share shows less variation, ranging from 51% in 2009 (the lowest year by 8 percentage points) to 69.6% in 2000.

Table 1 shows that the proportion of bond measures with a 55% (as opposed to 2/3) pass threshold jumps from 0% in 1999-2001 to 86.5% in 2002 and remains at or above 45% thereafter. Figure 1 compares the distribution of vote shares for bond measures with 55% and 66.7% cutoffs. Vote shares are lower for bonds requiring 55% 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 would drop out).

Table 2 provides descriptive statistics for district-year observations in the stacked panel data used for the ITT analyses (which includes observations 2 years before and 10 years after the bond measure). On average, achievement among low-SES students is 109 points (14%) 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 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 also 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 only significant for capital outlays per pupil ($p<0.01$) and suggests that 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<0.05$). Overall, these comparisons suggest that 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. The preferred models control for district enrollment in analyses below.

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 1-4 predict characteristics the year before the election and Models 5-8 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 pre-election 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 election ($p<0.05$). In Model 5, districts that passed a bond experienced a 2% decrease in the proportion of tested students who were low-SES from year t-2 to t-1 ($p<0.05$). 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 two years before to ten years after the focal bond measure. The sample in these analyses is limited to elections within the RD sample on vote share (+/-3.4% 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 pre-election differences between districts that passed or failed a bond measure. Estimates in these models are therefore consistent with balance before the election by outcome.

Intent-to-Treat Estimates

Table 4 presents results of the ITT analyses. Models 1-3 predict low-SES achievement and models 4-6 predict high-SES achievement. The sample is limited to districts within a narrow window on either side of the pass cutoff (+/-3.4% 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 ($p<0.10$) to 48 points ($p<0.05$), and is larger when controlling for higher polynomials of vote share. These estimates amount to 0.40-0.57 standard deviations or 5-7% of the mean low-SES API score and 29-41% 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 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 and Figure 3 is limited to districts that narrowly passed the first bond measure in the observed time range (1999-2013). 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 post-election.

Part of the delayed effects could reflect spending patterns. Table 5 provides 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 spending by $2,840 per student and total spending by $2,950 per student with a delay: two years after the measure ($p<0.05$). No other coefficients achieve significance at the 95% level, suggesting that narrowly passing a bond measure does not influence instructional spending, number or percent of students tested, numbers of low- or high-SES students tested, or the percent 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 pre-election differences in Tables 2 and 3). After the election, mean capital spending remains stable one year after the election, but increases sharply two years after the election. Mean capital spending continues to increase in years three and four 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. Together, the results suggest that districts spend bond revenue with a delay after the election.

<u>Treatment-on-the-Treated Estimates</u>

TOT estimates are provided in Table 6. These analyses use standard district panel data and control for holding and passing a bond election, pass cutoff threshold, and vote share measures in each previous year (with vote share measures set to zero in years without an election). Because districts can have multiple past elections, these analyses do not allow limiting the sample to districts within a narrow window of passing an election. Therefore, to reduce concern about differences between districts, all models include district (and year) fixed effects and a 1-year lag measure of the dependent variable. District demographic controls are added after the baseline model.

The TOT estimates support evidence of no effects on high-SES achievement and delayed benefits for low-SES achievement. Consistent with ITT results, TOT estimates support *hypothesis 1* and contradict *hypothesis 2*. Specifically, controlling for previous elections and outcomes in a district, passing a bond measure increases low-SES achievement by about 8 points ($p<0.05$) five years after the election. TOT estimates suggest the benefits for low-SES students emerge five years after the election, in contrast to the six-year delay in the ITT analyses.

However, the pattern of delayed benefits for low-SES achievement is consistent in both ITT and TOT analyses.

In contrast to the ITT results, the TOT estimates suggest passing a bond measure slightly increased low-SES achievement one year later. However, these estimates are only marginally significant ($p<0.10$). One potential explanation for this difference is that TOT estimates are less noisy than the ITT estimates because they account for potential indirect effects of previous elections in a district. After accounting for other elections, some students could enjoy an initial boost with passage of a bond measure. For example, many election measures include technological upgrades (often in addition to larger-scale physical improvements). These technology investments could increase achievement more among low-SES students if they enjoy less access to these learning tools outside of school or if they allow teachers to target the learning needs of low-SES students more effectively. Alternatively, district administrators or teachers may be pleased about potential future school improvements signaled by passing a bond. This enthusiasm could spill over to students and initially improve achievement for some students. However, the results are less consistent with this explanation. Enthusiasm should influence both low- and high-SES students, but the marginal achievement gains are only experienced by low-SES students.

Sensitivity Analyses

A series of sensitivity analyses are conducted to assess robustness. First, Appendix Table S1 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, though the coefficients for six years post-election are slightly smaller and in Models 1 and 3 they only reach marginal significance ($p<0.10$).

Second, Table S2 provides 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 post-election is only marginally significant ($p<0.10$) when predicting capital outlays. Thus, effects on spending are not robust to including a cubic in vote share.

Third, I repeat ITT estimates in Table 4 using several placebo checks as falsification tests. 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. Results of these placebo tests are presented in Table S3. Panel A includes vote share squared and Panel B includes vote share cubed. All estimates indicate null effects using these false cutoffs and support the validity of estimates at the actual cutoff.

Fourth, I repeat the ITT estimates varying the RD bandwidth from 2.4% to 4% above and below the pass threshold. Results of these analyses are presented in Table S4. Panels A and B show estimates predicting low-SES achievement and Panels C and D show estimates of high-SES achievement. Results consistently support evidence of delayed benefits for low-SES achievement six years after the election and no effects on high-SES achievement. Figure S3 graphically summarizes the estimates for six years after the election by SES.

Fifth, I estimate effects of passing a bond measure by the voteshare 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 (1132 vs. 1689), 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 voteshare 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% threshold may reduce high-SES achievement two years after the measure. However, this is not found in the other analyses. Overall, therefore, results are broadly consistent at both pass thresholds, but one approach suggests null effects at the lower threshold.

Sixth, 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 are presented in Tables S5 and S6 and suggest that narrowly passing a bond measure initially reduces achievement among low-SES but not high-SES students. Table S7 shows estimates when varying the RD bandwidth and shows that 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 when limiting the sample to districts above and below the median values of district percent 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 higher free lunch eligibility. In support of these expectations, results shown in Table S8 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 districts, but not in high-income districts. Effects in high-free lunch districts are larger than in the full sample (though coefficients are not significantly different) and emerge both five and six years after passing a bond measure ($p<0.05$).

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 6 years after the election in all models limited to districts with 9 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 5 and 6 years after the election measure. This pattern is consistent with the possibility that bond measures increased 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 California Department of Education. 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 suggest 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 measure. 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% level. Shown in Appendix Tables S9 and S10, these analyses suggest limited effects of passing a bond measure on teacher quality. However, the data include 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 less than 5 schools. The effects of passing a bond measure should be stronger in districts with few schools, where benefits of capital investments are experienced by a larger proportion of students. These results are presented in Table S11 and include coefficients for years 1-9 after the election measure. Results suggest that passing a bond measure increases low-SES achievement 6 and 7 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 in Model 3 in those years and in all models for years 5 and 8 after the election).

Models 4-6 in Table S11 show estimated effects on capital outlays per pupil in districts with fewer than 5 schools. Results are consistent with those predicting capital outlays in the full sample and suggest that capital spending increases significantly 2 years after a bond passes. However, these estimates are larger when limited to districts with less than 5 schools ($4,260 per pupil compared to $2840 in the full sample), suggesting a larger treatment dose (investment per pupil) in smaller districts. Overall, these results suggest that 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 9 years after the bond measure.

Conclusion

Existing research provides contradictory evidence about the effects of education funding on student achievement (e.g., Jackson, Johnson and Persico 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 (Alexander et al. 2007; Lareau 2003; Entwisle et al. 1998). Furthermore, local revenue may be distributed more unequally within districts than state or federal revenue (Timar and Roza 2010; Condron and Roscigno 2003), 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 paper 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% 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 6 years after passing a bond measure is 0.29 (from Table 4, Model 3, 47.77 x 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 5 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 from Table S11, Model 3). Chetty and colleagues (2014) find that a 0.2 standard deviation increase in student achievement yields a 2% increase in annual lifetime earnings. These effects (0.04 or 0.08 SD for each $1,000 invested per pupil) are around the 50^{th} 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 S5-6), 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 (e.g., 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 (Alexander et al. 2007; Lareau 2003; Entwisle et al. 1998), 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 among low-SES students is more sensitive to context 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 5 schools where a larger proportion of students are treated (and with a larger dose) suggest larger effects, which persist for up to 9 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 (after the LCFF; 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 of less constraint on local revenue. Second, analyses examine achievement by SES within districts, rather than state- or nation-wide achievement. Evidence suggests that inequality within districts is at least as critical as inequality between districts (Lafortune et al. 2016; Guin et al. 2007; 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, results inform understanding of the relationship between education funding and achievement. First, results suggest that the effects of education funding vary by students characteristics. Specifically, education funding does not increase aggregate achievement and seems to impact 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) and consistent with other studies (Hong and Zimmer 2016; Cellini et al. 2016), results suggest that elections related to facilities funding influence achievement. However, results in California suggest that 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 effect 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 measure. If the temporary disruptions could be reduced, the gains of 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 (Taylor 2015; Brunner 2007), school facility investments may be more efficiently directed to poor rather than wealthy districts.

Research Ethics

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

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Tables and Figures

Table 1: Descriptive Statistics of School District Bond Election Measures

Year	Bond Measures (N)	55% Pass Cutoff (%)	Pass (%)	Votes in Favor (%) Mean	Votes in Favor (%) Std Dev
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 1999-2013, limited to general obligation bond measures in districts with achievement data for low- and high-SES students.

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/Pupil ($1k)	1.78	1.71	1.81	11.81	11.31	-0.50	
Capital Outlays/Pupil ($1k)	5.77	5.65	5.83	1.82	1.31	-0.51	**
Instructional Spending/Pupil	8.45	8.34	8.49	5.87	5.79	-0.08	
Enrollment (log)	11.81	11.40	11.98	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 1999-2013 and California Elections Data Archive 1999-2013, limited to district-year observations with achievement data for low- and high-SES students. Observations for certain measures are smaller: Enrollment N=13549; Spending measures N=13516.

Two-tailed t-tests indicate significant mean differences by election outcome. * p<0.05, ** p<0.01

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted Outcome Measure	Year Prior to Election (*t*-1)				Change Prior to Election (*t*-2 to *t*-1)			
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/Pupil ($1k)	-0.41**	-0.44**	3.48	3.71	-0.05	-0.10	2.72	2.82
Total Spending/Pupil ($1k)	-0.41	-0.66*	3.49	3.62	0.16	-0.18	2.66	2.80
Instructional Spending/Pupil ($1k)	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
% of Tested Students Who Are Low-SES	-0.03	-0.04	-0.03	-0.04	-0.02*	0.00	-0.01	-0.01
% of Students Tested	-0.33	-13.27	7.66	6.23	15.74	7.25	-35.46	-31.30
Year Fixed Effects & Vote Share[2]	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share[3]	N	Y	Y	Y	N	Y	Y	Y
Includes Multiple Years & 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 1999-2013, California Elections Data Archive 1999-2013, and F-33 Census data. Currency is measured in thousands of 2014 dollars.

Baseline models (1 & 5) include fixed effects for calendar year and controls for pass threshold, vote share, and vote share squared. Models 2 & 6 add vote share cubed. Models 3 & 7 include district-year observations 2 years before to 10 years after the focal bond election and within the RD sample on the running variable (+/-3.4% from the pass cutoff) and include controls for enrollment (log), number of schools (log), % free lunch-eligible, % Black, and % Hispanic students in the district. Models 4 & 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, & 8 are interactions between indicators for passing a bond election and the year before the election measure (*t*-1).

Robust standard errors are adjusted for district clustering. + p<0.10, * p<0.05, ** p<0.01

Table 4: Estimated Effects of Passing a Bond Election on Achievement by SES

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Low-SES Achievement			High-SES Achievement		
1 Year After Election	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 Years After Election	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 Years After Election	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 Years After Election	-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 Years After Election	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 Years After Election	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, & 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-squared	0.93	0.93	0.93	0.86	0.86	0.86

Source: California Department of Education Academic Performance Index (API) data 1999-2013 and California Elections Data Archive 1999-2013, 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 RD sample on the running variable (+/-3.4% 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), % free lunch-eligible, % Black, and % 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 election and years since the election.

Robust standard errors adjusted for district clustering in parentheses. + p<0.10, * p<0.05, ** p<0.01

Table 5: Estimated Effects of Passing a Bond Election on District Spending and Students Tested

VARIABLES	(1) Capital Outlays/ Pupil ($1k)	(2) Total Spending/ Pupil ($1k)	(3) Instructional Spending/ Pupil ($1k)	(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
1 Year After Election	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 Years After Election	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 Years After Election	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 Years After Election	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 Years After Election	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 Years After Election	-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, & 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-squared	0.32	0.61	0.86	0.93	0.21	0.95	0.54	0.66

Source: California Department of Education Academic Performance Index (API) data 1999-2013, California Elections Data Archive 1999-2013 and F-33 Census data, 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 RD sample on the running variable (+/-3.4% 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, % free lunch-eligible, % Black, and % 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 election and years since the election.

Robust standard errors adjusted for district clustering in parentheses. + $p<0.10$, * $p<0.05$, ** $p<0.01$

Table 6: Treatment-on-the-Treated Estimates of Passing a Bond Measure on Achievement by SES

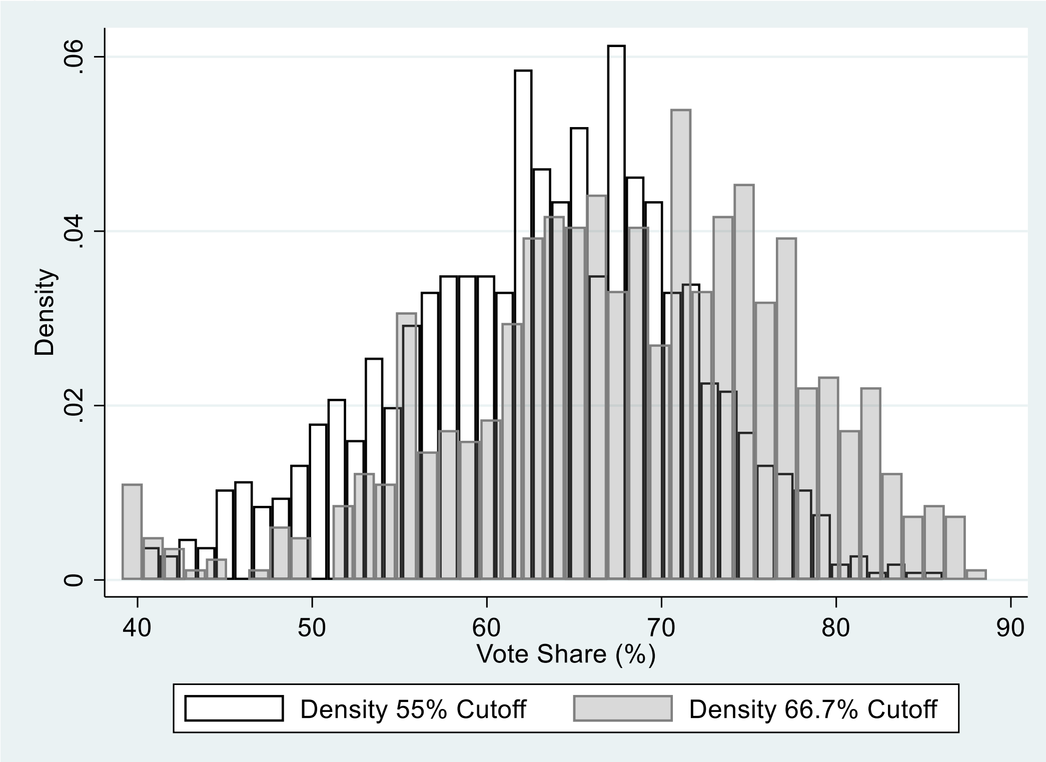
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Low-SES Achievement			High-SES Achievement		
1 Year After Election	5.59+	5.91+	5.88+	-2.69	-2.60	-2.40
	(3.22)	(3.19)	(3.22)	(4.46)	(4.58)	(4.71)
2 Years After Election	2.14	2.72	3.26	0.95	1.63	5.12
	(3.51)	(3.55)	(3.61)	(4.67)	(4.71)	(4.73)
3 Years After Election	1.65	1.91	1.74	-4.96	-4.23	-4.60
	(3.51)	(3.43)	(3.39)	(6.17)	(6.22)	(6.30)
4 Years After Election	-0.90	0.24	0.39	5.29	5.72	6.07
	(3.63)	(3.64)	(3.76)	(5.73)	(5.83)	(6.60)
5 Years After Election	7.83*	8.20*	7.96*	-0.77	-1.43	-0.85
	(3.84)	(3.82)	(3.90)	(5.71)	(5.69)	(5.91)
6 Years After Election	5.07	4.91	4.54	5.29	5.02	4.90
	(4.08)	(4.17)	(4.34)	(5.48)	(5.54)	(5.80)
Constant	295.31**	271.12**	255.57**	582.33**	515.07**	527.61**
	(11.88)	(21.96)	(24.52)	(20.56)	(37.80)	(43.95)
District & Year Fixed Effects	Y	Y	Y	Y	Y	Y
Vote Share^2	Y	Y	Y	Y	Y	Y
Demographic Controls	N	Y	Y	N	Y	Y
Observations	11,456	11,378	10,363	11,373	11,296	10,292
R-squared	0.85	0.85	0.85	0.48	0.48	0.48
Number of Districts	1,009	1,005	1,000	1,006	1,002	998

Source: California Department of Education Academic Performance Index (API) data 1999-2013 and California Elections Data Archive 1999-2013, limited to district-year observations with low- and high-SES achievement information.

All models include district and year fixed effects, 1-year lag measure of the dependent variable, and the following controls measured in each prior year (1-18): having a bond measure, passing a bond measure, pass cutoff threshold, vote share raw and squared. Model 2 adds controls for enrollment (log) and number of schools (log). Model 3 adds controls for % free lunch-eligible, % Black, and % Hispanic students in the district. Coefficients for passing a measure 1-6 years before the current year are reported.

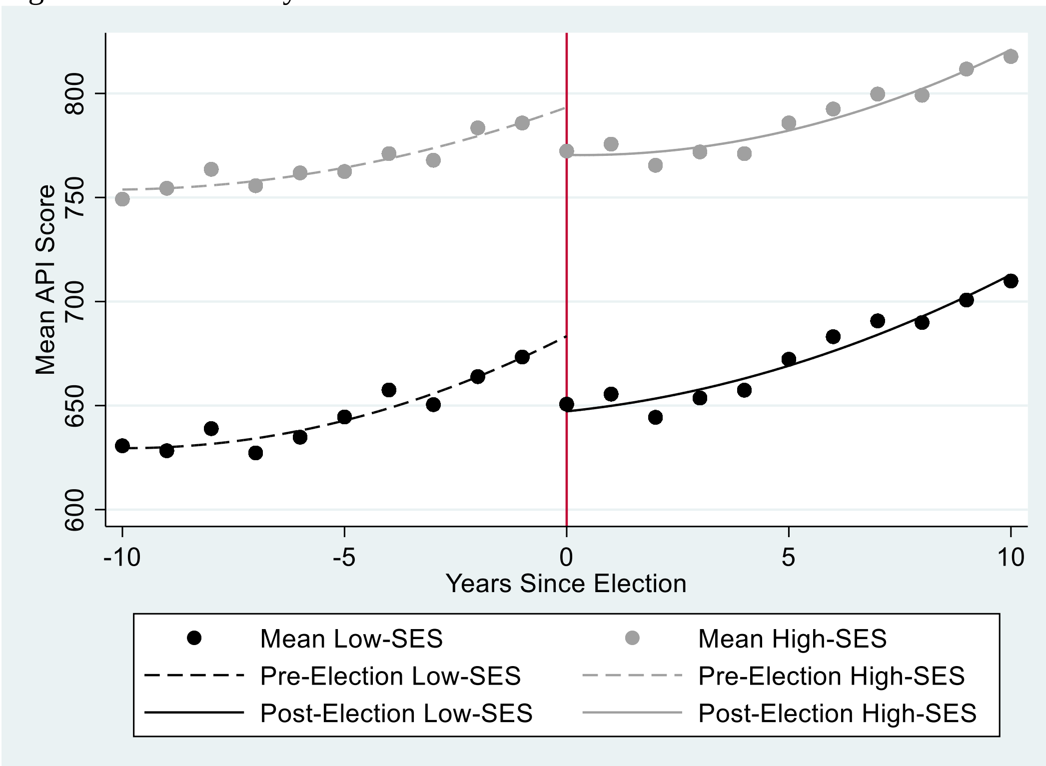
Robust standard errors adjusted for district clustering in parentheses. + p<0.10, * p<0.05, ** p<0.01

Figure 1: Vote Share Distribution by Threshold Required to Pass



Source: CEDA 1999-2013. Limited to general obligation bond measures with vote shares 40-90%. Depicts the distribution of the percent of votes for a general obligation bond measure by cutoff required to pass the measure.

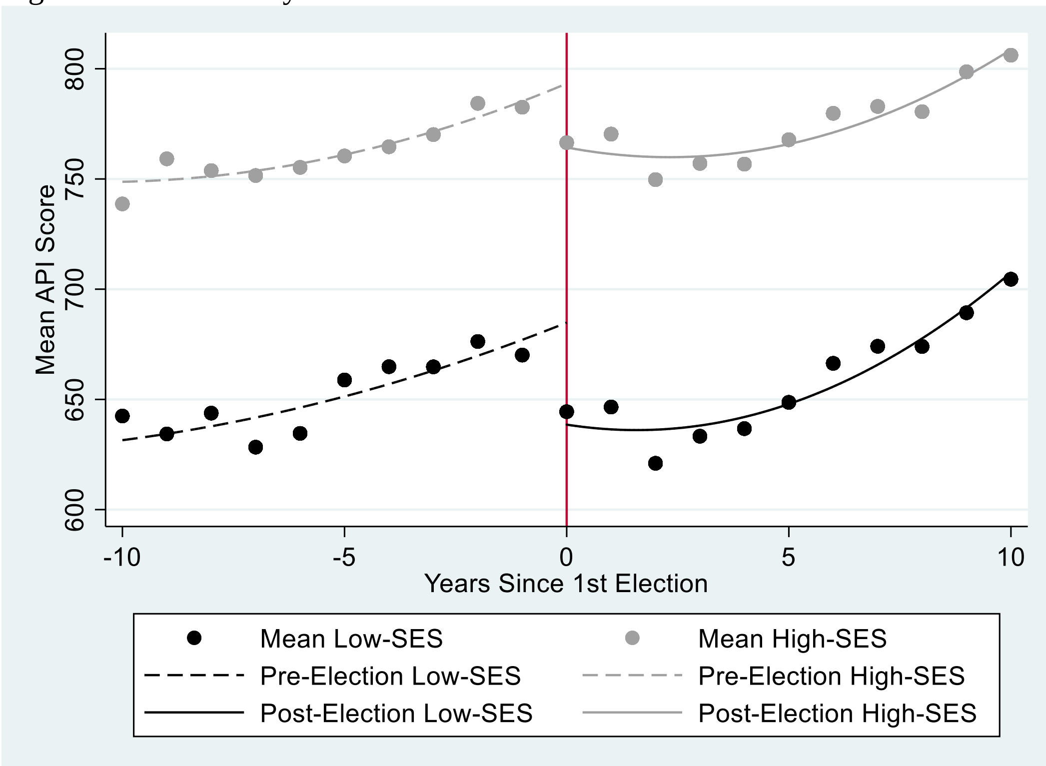
Figure 2: API Score by SES and Time Since Election Measure



Source: California Department of Education Academic Performance Index (API) data 1999-2013 and California Elections Data Archive 1999-2013, limited to district-year observations with achievement information for bond measures that narrowly passed within the RD window of vote share (+/-3.4% from the cutoff).

Quadratic trendlines illustrate a greater drop in low-SES API and steeper increase than high-SES API after the bond election.

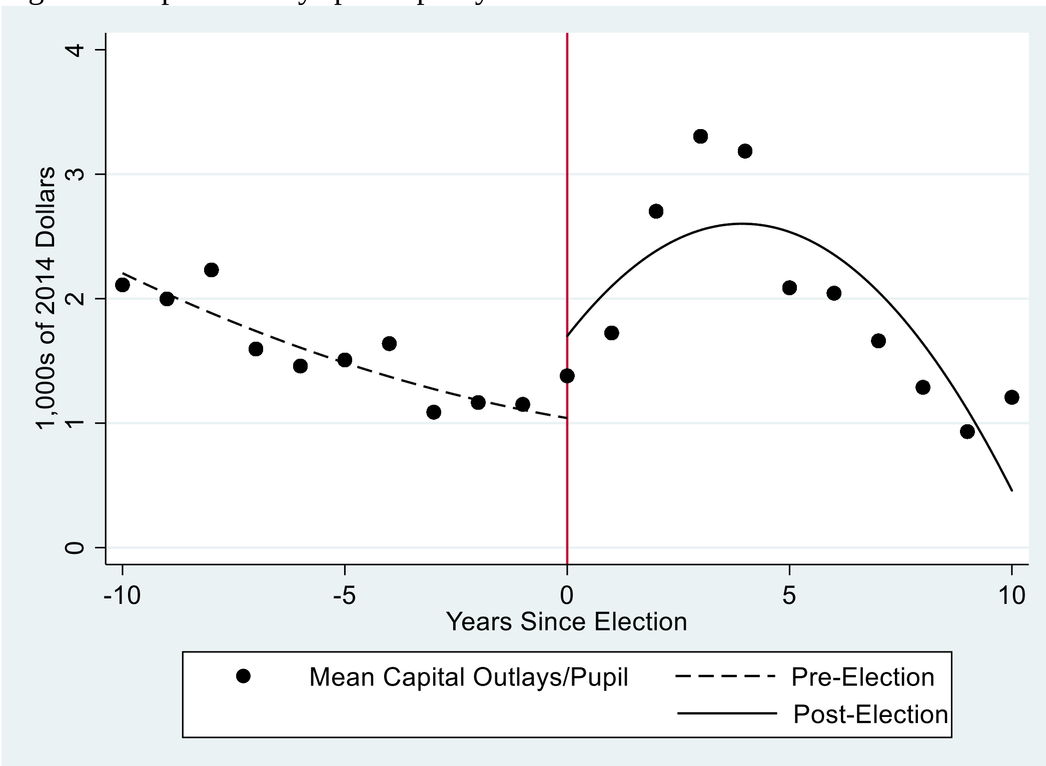
Figure 3: API Score by SES and Time Since First Election Measure



Source: California Department of Education Academic Performance Index (API) data 1999-2013 and California Elections Data Archive 1999-2013, limited to district-year observations with achievement information for districts that narrowly passed the first bond measure from 1999 to 2013 within the RD window of vote share (+/-3.4% 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.

Figure 4: Capital Outlays per Pupil by Time Since Election Measure



Source: California Department of Education Academic Performance Index (API) data 1999-2013 and F-33 Census data, limited to district-year observations with achievement measures in districts that passed an election and within the RD window of vote share (+/-3.4% from the cutoff).

Quadratic trendlines illustrate a large, delayed increase in capital outlays after the bond election.

Supplemental Online Appendix

Table S1: Estimated Effects of Passing a Bond Election on Achievement by SES – Including 1-Year Lag of Dependent Variable

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Low-SES Achievement			High-SES Achievement		
1 Year After Election	4.58	4.33	14.02	0.93	0.90	-2.43
	(10.80)	(10.71)	(17.19)	(7.98)	(8.16)	(11.71)
2 Years After Election	7.10	7.23	5.11	1.79	1.96	10.40
	(10.35)	(10.07)	(15.57)	(10.92)	(11.10)	(17.03)
3 Years After Election	11.22	11.50	10.98	-0.08	0.13	0.41
	(9.69)	(9.49)	(14.94)	(8.64)	(8.67)	(12.66)
4 Years After Election	-2.79	-1.19	14.67	9.46	9.32	23.89
	(10.93)	(10.16)	(15.74)	(19.33)	(18.74)	(28.20)
5 Years After Election	9.96	10.20	19.47	-2.37	-3.05	4.01
	(10.77)	(10.51)	(15.73)	(10.59)	(10.68)	(14.28)
6 Years After Election	24.17+	26.74*	33.13+	4.26	5.02	6.45
	(12.75)	(12.80)	(18.96)	(11.50)	(11.64)	(15.61)
Constant	210.29**	215.15**	209.96**	581.77**	586.99**	587.63**
	(61.14)	(60.50)	(59.73)	(80.81)	(81.79)	(81.98)
Fixed Effects for Election Measure, Calendar Year, & 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	2,652	2,652	2,652	2,650	2,650	2,650
R-squared	0.96	0.96	0.96	0.89	0.89	0.89

Source: California Department of Education Academic Performance Index (API) data 1999-2013 and California Elections Data Archive 1999-2013, 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 RD sample on the running variable (% of votes for the election measure).

Models are the same as those in Table 3, but control for a one-year lag (year *t*-1) measure of the dependent variable (low-SES achievement in columns 1-3, high-SES achievement in columns 4-6). 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), % free lunch-eligible, % Black, and % Hispanic students in the district. District demographic measures 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 election and years since the election.

Robust standard errors adjusted for district clustering in parentheses. + p<0.10, * p<0.05, ** p<0.01

Table S2: Estimated Effects of Passing a Bond Election on District Spending and Students Tested

VARIABLES	(1) Capital Outlays/ Pupil	(2) Total Spending/ Pupil	(3) Instructional Spending/ Pupil	(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
1 Year After Election	0.06	-0.30	-0.19	0.27	-16.47	0.04	215.23	-66.65
	(1.41)	(1.63)	(0.20)	(0.21)	(23.51)	(0.03)	(1,543.59)	(743.05)
2 Years After Election	2.52+	2.23	-0.33	0.44	4.73	0.02	2,497.26	820.07
	(1.30)	(1.36)	(0.21)	(0.27)	(31.82)	(0.03)	(3,263.76)	(1,012.91)
3 Years After Election	1.08	0.81	-0.31	0.03	-47.45	0.02	3,814.42	-366.93
	(1.63)	(1.71)	(0.23)	(0.27)	(33.19)	(0.03)	(3,241.68)	(1,082.46)
4 Years After Election	-2.22	-2.30	-0.03	0.01	7.49	-0.01	3,179.15	-1,065.78
	(1.62)	(1.82)	(0.24)	(0.28)	(36.06)	(0.04)	(3,630.66)	(1,262.41)
5 Years After Election	-0.89	-1.39	-0.21	0.13	14.65	0.02	3,282.66	-26.26
	(1.15)	(1.31)	(0.26)	(0.28)	(38.08)	(0.03)	(3,497.31)	(1,205.65)
6 Years After Election	0.31	-0.08	-0.07	0.02	-9.97	0.04	2,778.79	-1,049.96
	(1.36)	(1.48)	(0.31)	(0.32)	(37.10)	(0.04)	(3,905.43)	(1,410.15)
Constant	20.49*	53.15**	12.44**	3.27	-366.91+	0.36+	1,789.50	-4,570.45
	(9.92)	(15.22)	(3.64)	(2.04)	(219.21)	(0.20)	(21,191.29)	(8,919.05)
Fixed Effects for Election Measure, Calendar Year, & Year Since Election	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^2	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^3	Y	Y	Y	Y	Y	Y	Y	Y
Observations	2,826	2,826	2,826	2,838	2,630	2,838	2,838	2,838
R-squared	0.35	0.63	0.89	0.93	0.21	0.96	0.54	0.67

Source: California Department of Education Academic Performance Index (API) data 1999-2013, California Elections Data Archive 1999-2013 and F-33 Census data, 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 RD sample on the running variable (% of votes for the election measure).

Models are the same as those in Table 4, but include a cubic in vote share. 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, % free lunch-eligible, % Black, and % 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. Currency is measured in thousands of 2014 dollars.

Coefficients are interactions between indicators for passing a bond election and years since the election.

Robust standard errors adjusted for district clustering in parentheses. + p<0.10, * p<0.05, ** p<0.01

Table S3: Placebo Tests – Estimated Effects of False Pass Thresholds on Achievement by SES

Panel A: Including Vote Share Squared

	Low-SES				High-SES			
VARIABLES	-10%	-5%	+5%	+10%	-10%	-5%	+5%	+10%
1 Year After Election	25.04	2.2	-16.59	-0.05	13.68	-13.64	11.23	-0.14
	(16.52)	(12.06)	(10.08)	(8.18)	(19.55)	(14.84)	(11.94)	(9.75)
2 Years After Election	25.29	13.28	-3.24	-0.22	34.42	-6.92	11.62	12.52
	(20.53)	(13.90)	(12.75)	(10.74)	(23.75)	(17.02)	(12.55)	(15.13)
3 Years After Election	-2.11	10.36	-3.92	6.11	18.99	-18.84	13.65	-5.05
	(22.36)	(14.10)	(12.91)	(10.60)	(19.52)	(17.52)	(10.80)	(11.45)
4 Years After Election	10.11	2.04	-5.16	-0.8	16.32	-13.74	0.84	-12.33
	(23.67)	(17.09)	(15.44)	(11.99)	(19.02)	(19.22)	(13.38)	(19.28)
5 Years After Election	20.29	7.47	-7.12	2.62	31.48	-17.43	11.77	15.29
	(22.51)	(17.67)	(15.17)	(12.36)	(21.05)	(20.98)	(12.08)	(19.44)
6 Years After Election	40.98	-9.41	4.59	0.52	38.17	-37.59	14.53	14.64
	(23.68)	(16.78)	(17.35)	(14.07)	(22.38)	(21.32)	(14.09)	(18.67)
Fixed Effects for Election Measure, Calendar Year, & Year Since Election	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^2	Y	Y	Y	Y	Y	Y	Y	Y
Observations	885	1,847	3,430	3,313	885	1,847	3,430	3,313
R-squared	0.93	0.94	0.93	0.92	0.77	0.84	0.88	0.86

Panel B: Including Vote Share Cubed

	Low-SES				High-SES			
VARIABLES	-10%	-5%	+5%	+10%	-10%	-5%	+5%	+10%
1 Year After Election	15.2	-3.01	-11.11	2.13	7.33	-13.64	13.21	1.51
	(20.07)	(16.07)	(14.85)	(11.47)	(23.22)	(14.84)	(15.48)	(13.87)
2 Years After Election	10.98	1.45	-6.87	-6.11	20.01	-6.92	14.48	26.71
	(23.43)	(17.87)	(18.40)	(14.47)	(30.49)	(17.02)	(16.80)	(23.96)
3 Years After Election	-5.21	5.49	-3.82	2.31	4.48	-18.84	11.94	-10.3
	(27.48)	(17.24)	(18.03)	(13.98)	(23.76)	(17.52)	(15.04)	(16.65)
4 Years After Election	-3.77	6.84	-2.79	-6.74	-4.57	-13.74	4.28	-16.34
	(35.08)	(20.01)	(20.63)	(15.97)	(25.89)	(19.22)	(16.85)	(27.06)
5 Years After Election	-9.06	9.64	-6.69	-6.66	10.83	-17.43	8.57	14.68
	(32.34)	(19.24)	(21.14)	(15.72)	(26.51)	(20.98)	(15.45)	(29.72)
6 Years After Election	17.43	-10.4	-0.32	-21.75	7.45	-37.59	8.44	24.66
	(33.44)	(21.16)	(23.93)	(16.47)	(33.78)	(21.32)	(19.27)	(27.20)
Fixed Effects for Election Measure, Calendar Year, & Year Since Election	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^2	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^3	Y	Y	Y	Y	Y	Y	Y	Y
Observations	885	1,847	3,430	3,313	885	1,847	3,430	3,313
R-squared	0.93	0.94	0.93	0.92	0.77	0.84	0.88	0.86

Source: California Department of Education Academic Performance Index (API) data 1999-2013 and California Elections Data Archive 1999-2013, 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 RD sample on the running variable (% of votes for the election measure).

Models are the same as those in Table 4, but use a false threshold required to pass a bond measure. The false thresholds are 5 and 10 percentage points above and below the actual pass threshold. 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), % free lunch-eligible, % Black, and % Hispanic students in the district. District demographic measures 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 election and years since the election.

Robust standard errors adjusted for district clustering in parentheses. * p<0.05, ** p<0.01

Table S4: Estimated Effects of Passing a Bond Election on Achievement by SES: Varying RD Bandwidth
Panel A: Low-SES Achievement Part 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Low-SES Achievement by RD Bandwidth (+/- % of vote share from pass threshold)								
Years Post-Election	2.4%	2.5%	2.6%	2.7%	2.8%	2.9%	3.0%	3.1%	3.2%
1 Year	24.34	18.88	16.53	16.96	20.62	19.13	19.36	19.68	15.24
	(24.32)	(23.12)	(22.81)	(22.16)	(20.98)	(21.10)	(20.18)	(19.37)	(18.98)
2 Years	33.10	22.76	19.53	20.33	27.00	25.03	22.26	21.56	16.42
	(27.56)	(26.50)	(26.20)	(25.61)	(23.52)	(23.38)	(22.46)	(21.68)	(21.57)
3 Years	40.82	31.27	27.96	25.16	32.36	31.72	33.98	33.96	28.45
	(26.81)	(25.72)	(25.41)	(24.56)	(22.63)	(22.54)	(21.68)	(20.82)	(20.56)
4 Years	30.39	21.23	21.44	24.94	32.31	34.37	34.87	36.34	33.82
	(28.67)	(27.61)	(27.66)	(27.50)	(25.09)	(25.01)	(24.15)	(23.30)	(23.17)
5 Years	41.73	32.43	28.44	27.91	36.08	37.73	37.76	38.87+	35.52
	(29.22)	(28.23)	(28.26)	(27.20)	(24.70)	(24.38)	(23.56)	(22.69)	(22.66)
6 Years	**62.26***	**52.89+**	**47.12**	**48.03**	**57.95***	**60.09***	**60.65***	**59.09***	**57.40***
	(31.48)	(30.24)	(30.37)	(29.34)	(27.82)	(27.40)	(26.40)	(25.41)	(24.91)
Constant	205.73+	221.94+	255.44*	226.90+	227.30+	213.14+	210.94+	212.18+	244.71*
	(122.28)	(122.00)	(121.06)	(119.01)	(118.51)	(116.19)	(114.50)	(111.41)	(101.29)
Fixed Effects for Election Measure, Calendar Year, & Year Since Election	Y	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^2	Y	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^3	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,911	2,011	2,065	2,202	2,267	2,301	2,423	2,509	2,615
R-squared	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93

Panel B: Low-SES Achievement Part 2

	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
	Low-SES Achievement by RD Bandwidth (+/- % of vote share from pass threshold)							
Years Post-Election	3.3%	3.4%	3.5%	3.6%	3.7%	3.8%	3.9%	4.0%
1 Year	9.61	10.97	14.28	11.26	14.27	15.56	15.03	14.55
	(18.78)	(18.86)	(18.16)	(17.99)	(17.57)	(16.87)	(16.48)	(16.23)
2 Years	10.70	10.08	15.16	13.92	16.95	18.80	20.44	20.20
	(21.43)	(21.24)	(20.52)	(20.28)	(19.82)	(19.35)	(18.69)	(18.38)
3 Years	20.47	20.10	27.62	25.40	25.85	27.85	29.56	29.40
	(20.83)	(20.66)	(20.71)	(20.23)	(19.71)	(19.34)	(18.58)	(18.35)
4 Years	25.01	25.83	27.50	24.27	23.18	23.90	23.48	22.95
	(23.29)	(22.90)	(22.46)	(22.15)	(21.63)	(21.09)	(20.36)	(20.25)
5 Years	29.82	30.57	33.40	30.53	30.39	32.09	33.19+	32.75
	(22.58)	(21.96)	(21.71)	(21.50)	(21.26)	(20.84)	(20.07)	(20.14)
6 Years	49.49*	47.77*	51.35*	49.07*	50.97*	52.79*	54.68*	55.79*
	(24.50)	(23.74)	(23.27)	(22.90)	(22.74)	(22.41)	(21.66)	(21.86)
Constant	253.49*	287.08**	324.83**	332.45**	332.55**	325.25**	323.07**	330.52**
	(102.94)	(100.91)	(97.20)	(95.79)	(91.45)	(90.78)	(88.76)	(87.16)
Fixed Effects for Election Measure, Calendar Year, & Year Since Election	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^2	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^3	Y	Y	Y	Y	Y	Y	Y	Y
Observations	2,745	2,833	2,952	3,015	3,096	3,161	3,211	3,294
R-squared	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93

Panel C: High-SES Achievement Part 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	High-SES Achievement by RD Bandwidth (+/- % of vote share from pass threshold)								
Years Post-Election	2.4%	2.5%	2.6%	2.7%	2.8%	2.9%	3.0%	3.1%	3.2%
1 Year	0.23	-0.95	-3.70	-3.53	0.28	0.40	0.39	3.35	2.63
	(16.00)	(15.88)	(15.68)	(14.93)	(14.21)	(14.11)	(13.78)	(13.46)	(13.10)
2 Years	31.09+	21.55	20.70	18.18	22.40	22.17	19.47	18.82	20.33
	(18.03)	(17.57)	(17.59)	(17.48)	(16.01)	(15.94)	(15.54)	(15.38)	(15.06)
3 Years	27.16	25.05	18.09	17.29	24.85	25.33	25.45	25.53	24.23
	(19.65)	(19.13)	(22.33)	(20.05)	(17.64)	(17.45)	(16.47)	(16.06)	(15.68)
4 Years	29.32	30.22	29.63	31.76	37.33	37.70	33.37	33.58	34.32
	(28.03)	(28.93)	(29.46)	(29.25)	(28.57)	(28.49)	(27.74)	(27.37)	(27.05)
5 Years	8.19	4.78	-3.33	-3.38	3.96	3.70	2.63	4.75	8.45
	(17.65)	(17.32)	(19.92)	(18.09)	(15.95)	(15.77)	(14.94)	(14.79)	(14.69)
6 Years	-4.71	-7.64	-19.98	-15.22	-4.15	-3.38	1.01	3.23	11.19
	(20.93)	(19.67)	(22.58)	(20.58)	(18.43)	(18.15)	(17.26)	(16.93)	(16.94)
Constant	531.68**	551.47**	597.87**	575.64**	581.31**	579.40**	590.75**	568.26**	595.74**
	(98.14)	(97.72)	(103.03)	(99.04)	(98.65)	(95.02)	(92.96)	(91.19)	(89.20)
Fixed Effects for Election Measure, Calendar Year, & Year Since Election	Y	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^2	Y	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^3	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,911	2,011	2,065	2,202	2,267	2,301	2,423	2,509	2,615
R-squared	0.85	0.85	0.85	0.85	0.85	0.85	0.85	0.86	0.86

Panel D: High-SES Achievement Part 2

	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
	High-SES Achievement by RD Bandwidth (+/- % of vote share from pass threshold)							
Years Post-Election	3.3%	3.4%	3.5%	3.6%	3.7%	3.8%	3.9%	4.0%
1 Year	-0.40	-1.95	-0.97	-2.34	4.74	3.65	4.82	4.70
	(13.51)	(13.21)	(12.86)	(12.68)	(12.58)	(12.15)	(11.74)	(11.47)
2 Years	17.08	15.20	14.15	12.40	14.61	12.43	15.41	15.68
	(15.34)	(15.18)	(15.02)	(15.02)	(14.98)	(14.85)	(14.24)	(14.04)
3 Years	19.22	16.55	16.20	14.50	17.58	17.56	21.02	20.80
	(15.64)	(15.74)	(15.44)	(15.45)	(15.36)	(15.09)	(14.47)	(14.19)
4 Years	27.57	27.57	24.44	22.05	24.66	24.13	25.46	24.34
	(26.53)	(26.08)	(25.51)	(25.25)	(24.76)	(24.33)	(23.86)	(23.10)
5 Years	4.78	7.35	6.38	4.43	7.09	8.01	9.97	9.46
	(14.47)	(14.28)	(14.32)	(14.50)	(14.47)	(14.27)	(13.82)	(13.65)
6 Years	9.16	12.36	13.37	11.41	12.21	12.59	14.75	15.93
	(16.78)	(16.59)	(16.75)	(16.70)	(16.45)	(16.25)	(15.69)	(15.66)
Constant	599.67**	602.29**	621.58**	627.42**	629.26**	627.89**	622.05**	624.07**
	(88.40)	(86.27)	(81.60)	(81.01)	(77.46)	(76.97)	(75.82)	(74.66)
Fixed Effects for Election Measure, Calendar Year, & Year Since Election	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^2	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^3	Y	Y	Y	Y	Y	Y	Y	Y
Observations	2,745	2,833	2,952	3,015	3,096	3,161	3,211	3,294
R-squared	0.86	0.86	0.86	0.86	0.86	0.87	0.87	0.87

Source: California Department of Education Academic Performance Index (API) data 1999-2013 and California Elections Data Archive 1999-2013, 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 specified RD sample on the running variable (% of votes for the election measure).

Models are the same as those in Table 3, but vary the bandwidth of vote share required to be included in the sample from +/-2.4% to +/-4.0% from the threshold required to pass. 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), % free lunch-eligible, % Black, and % Hispanic students in the district. District demographic measures 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 election and years since the election.

Robust standard errors adjusted for district clustering in parentheses. + p<0.10, * p<0.05, ** p<0.01

Table S5: Estimated Effects of Passing a Bond Election on Current Achievement by SES

Panel A: Low-SES

	(1)	(2)	(3)	(4)
VARIABLES		Low-SES API Score		
Election Measure Pass	-8.21*	-4.59	-8.15*	-8.46*
	(3.33)	(4.44)	(3.47)	(3.47)
% of Votes for Election Measure	1.68*	2.01*	1.68*	1.48+
	(0.70)	(1.02)	(0.73)	(0.82)
Pass x Vote Share	-1.28	-2.89*	-1.33	-1.03
	(1.04)	(1.41)	(1.07)	(1.16)
Constant	529.63**	529.75**	529.73**	529.78**
	(2.07)	(2.11)	(2.08)	(2.08)
District & Year Fixed Effects	Y	Y	Y	Y
Observations	11,796	11,607	11,769	11,749
R-squared	0.78	0.77	0.78	0.78
Number of Districts	1,027	1,027	1,027	1,027

Panel B: High-SES

	(1)	(2)	(3)	(4)
VARIABLES		High-SES API Score		
Election Measure Pass	-4.18	-4.97	-4.11	-5.22
	(3.51)	(4.44)	(3.70)	(3.42)
% of Votes for Election Measure	0.98	1.23	0.97	0.96
	(0.70)	(0.95)	(0.74)	(0.81)
Pass x Vote Share	-0.90	-1.30	-0.91	-0.70
	(0.99)	(1.34)	(1.04)	(1.09)
Constant	673.38**	672.91**	673.40**	673.15**
	(2.33)	(2.37)	(2.33)	(2.33)
District & Year Fixed Effects	Y	Y	Y	Y
Observations	11,796	11,607	11,769	11,749
R-squared	0.45	0.45	0.45	0.45
Number of Districts	1,027	1,027	1,027	1,027

Source: California Department of Education Academic Performance Index (API) data 1999-2013 and California Elections Data Archive 1999-2013, limited to district-year observations with low- and high-SES achievement information and within the RD sample on the running variable (+/-7.1% from pass cutoff in the traditional panel data). Estimates predict current year achievement in a traditional panel data set of district-year observations.

All models include fixed effects for calendar year and district. Robust standard errors adjusted for district clustering in parentheses. + p<0.10, * p<0.05, ** p<0.01

Models exclude: 1) election measures less than 5 years since last one; 2) election measures that are not first in the same year (Hong and Zimmer 2016 method); 3) both; and 4) observations with repeated election measures in the same year that do not have the high vote share (Cellini et al. 2010).

Table S6: Estimated Effects of Passing a Bond Election on Current Achievement by SES – Adding Polynomial Vote Share Measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Low-SES API Score				High-SES API Score			
Election Measure Pass	-11.29**	-8.21*	-12.70*	-18.21*	-6.37*	-4.18	-7.00	-10.35
	(2.84)	(3.33)	(5.36)	(7.21)	(3.00)	(3.51)	(5.09)	(6.45)
Vote Share	1.21*	1.68*	6.19*	11.26+	0.65	0.98	-0.75	-3.65
	(0.53)	(0.70)	(2.43)	(6.78)	(0.55)	(0.70)	(2.78)	(8.77)
Pass x Vote Share		-1.28	-2.51	0.28		-0.90	2.93	10.62
		(1.04)	(4.46)	(11.05)		(0.99)	(4.14)	(11.97)
Constant	529.61**	529.63**	529.77**	529.81**	673.36**	673.38**	673.31**	673.29**
	(2.07)	(2.07)	(2.08)	(2.08)	(2.33)	(2.33)	(2.34)	(2.34)
District & Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Vote Share^2	N	N	Y	Y	N	N	Y	Y
Vote Share^3	N	N	N	Y	N	N	N	Y
Observations	11,796	11,796	11,796	11,796	11,796	11,796	11,796	11,796
R-squared	0.78	0.78	0.78	0.78	0.45	0.45	0.45	0.45
Number of Districts	1,027	1,027	1,027	1,027	1,027	1,027	1,027	1,027

Source: California Department of Education Academic Performance Index (API) data 1999-2013 and California Elections Data Archive 1999-2013, limited to district-year observations with low- and high-SES achievement information and within the RD sample on the running variable (+/-7.1% from pass cutoff in the traditional panel data). Estimates predict current year achievement in a traditional panel data set of district-year observations. Non-linear vote share measures are allowed to vary by bond measure passage.

All models include district and year fixed effects. Robust standard errors adjusted for district clustering in parentheses. + $p<0.10$, * $p<0.05$, ** $p<0.01$

All models include district and year fixed effects. Those in Table S5 use various approaches to address repeated elections in the same district (excluding elections in the same district that are less than five years since the last one, those that are not the first in a calendar year, both of those types, and repeated elections in the same year without the high vote share). Estimates in Table S6 estimate the effects of passing a measure with and without including an interaction between vote share and the pass indicator, then add vote share squared and cubed. In nearly all models, results suggest that narrowly passing a bond measure initially reduces achievement among low-SES but not high-SES students. These initial effects are consistent with the pattern in Figures 2 and 3 and suggest temporary negative effects of passing a bond measure on low-SES achievement.

Table S7: Estimated Effects of Passing a Bond Election on Current Achievement by SES: Varying RD Bandwidth

Bandwidth	Low-SES			High-SES			
(+/-% from cutoff)	RD Estimate		Std. Error	RD Estimate		Std. Error	N
2	-12.42	+	6.52	-6.58		6.00	11481
3	-12.08	*	4.95	-9.37	+	4.87	11544
4	-14.07	**	4.60	-5.31		4.84	11612
5	-10.17	*	4.44	-5.25		4.34	11672
6	-8.92	*	3.83	-4.91		4.01	11727
7	-8.62	*	3.40	-4.32		3.59	11788
8	-9.57	**	3.23	-4.81		3.34	11848
9	-10.44	**	3.12	-4.51		3.16	11901
10	-10.34	**	3.12	-6.81	*	3.03	11967
11	-10.14	**	3.07	-6.49	*	2.85	12013
12	-9.21	**	3.04	-5.82	*	2.69	12058
13	-7.74	**	2.82	-6.08	+	3.12	12110
14	-7.44	**	2.71	-5.85	*	2.80	12148
15	-6.66	*	2.57	-4.67	+	2.51	12199

Source: California Department of Education Academic Performance Index (API) data 1999-2013 and California Elections Data Archive 1999-2013, limited to district-year observations with low- and high-SES achievement information and within the specified RD sample on the running variable. Estimates predict current year achievement in a traditional panel data set of district-year observations.

Each estimate is from a separate model. Models are the same as Model 2 in Table S6, but include varying RD bandwidths, ranging from 2 to 15 percentage points from the vote share threshold required to pass.

All models include district and year fixed effects. Robust standard errors adjusted for district clustering in parentheses. + $p<0.10$, * $p<0.05$, ** $p<0.01$

Table S8: Predicted Achievement by District Characteristics: ITT Analyses

Panel A: Predicted Low-SES Achievement Limited to Districts Above and Below Median Free Lunch Eligibility

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Low Free Lunch Eligibility			High Free Lunch Eligibility		
1 Year After Election	5.37	4.71	8.80	5.25	4.44	15.71
	(16.35)	(16.87)	(26.62)	(12.53)	(12.82)	(19.06)
2 Years After Election	8.34	8.00	-2.43	17.16	16.43	20.47
	(16.52)	(16.49)	(26.67)	(17.63)	(16.87)	(25.32)
3 Years After Election	16.46	16.82	20.02	20.76	18.46	24.47
	(18.03)	(17.70)	(27.15)	(15.57)	(14.97)	(23.31)
4 Years After Election	1.75	-0.68	41.44	6.36	5.61	32.13
	(21.93)	(21.54)	(32.96)	(15.88)	(15.03)	(24.53)
5 Years After Election	-3.78	-7.17	12.82	32.89+	32.48*	59.31**
	(19.20)	(19.59)	(26.50)	(17.50)	(15.61)	(22.01)
6 Years After Election	14.67	14.37	24.22	52.88*	53.50*	84.98*
	(20.68)	(21.24)	(29.06)	(24.49)	(22.97)	(32.60)
Constant	406.83*	479.16**	483.04**	218.09+	220.01+	211.90+
	(171.73)	(168.22)	(168.11)	(125.89)	(122.87)	(116.96)
Fixed Effects for Election Measure, Calendar Year, & 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	1,418	1,418	1,418	1,415	1,415	1,415
R-squared	0.92	0.92	0.92	0.95	0.95	0.95

Panel B: Predicted High-SES Achievement Limited to Districts Above and Below Median Free Lunch Eligibility

VARIABLES	(1) Low Free Lunch Eligibility	(2)	(3)	(4) High Free Lunch Eligibility	(5)	(6)
1 Year After Election	-2.59	-3.17	-7.88	10.78	10.34	6.94
	(9.11)	(9.06)	(14.59)	(16.25)	(15.80)	(21.39)
2 Years After Election	0.05	0.22	4.67	16.22	16.18	33.47
	(11.69)	(11.41)	(18.53)	(18.76)	(19.40)	(23.76)
3 Years After Election	-7.74	-6.80	-6.19	29.09+	28.50+	43.83
	(13.48)	(12.46)	(17.90)	(16.71)	(16.77)	(27.11)
4 Years After Election	-21.17	-22.62	11.06	32.59	31.54	58.35
	(16.38)	(16.52)	(25.71)	(35.80)	(33.72)	(51.16)
5 Years After Election	-4.30	-5.58	-5.93	7.18	6.33	31.55
	(17.57)	(17.79)	(18.51)	(18.56)	(18.59)	(26.91)
6 Years After Election	-0.60	-0.50	-7.81	9.05	9.17	34.48
	(18.63)	(18.69)	(20.45)	(21.09)	(21.41)	(30.99)
Constant	728.76**	782.28**	791.33**	539.40**	537.69**	531.12**
	(116.79)	(120.90)	(119.86)	(133.35)	(129.58)	(132.83)
Fixed Effects for Election Measure, Calendar Year, & 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	1,418	1,418	1,418	1,415	1,415	1,415
R-squared	0.88	0.88	0.88	0.84	0.84	0.85

Panel C: Predicted Low-SES Achievement Limited to Districts Above and Below Median Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES		Low Enrollment			High Enrollment	
1 Year After Election	15.31	16.90	20.77	-8.14	-9.65	-12.17
	(18.07)	(17.92)	(28.39)	(11.25)	(11.67)	(16.78)
2 Years After Election	39.33*	40.86*	23.86	-18.86	-19.21	-22.11
	(19.43)	(18.58)	(30.38)	(15.10)	(15.46)	(21.52)
3 Years After Election	40.78*	42.51*	34.66	-12.09	-12.76	7.28
	(18.49)	(17.66)	(27.36)	(17.30)	(17.31)	(24.65)
4 Years After Election	18.74	17.91	62.09+	-28.24	-27.67	-8.05
	(22.05)	(21.22)	(34.45)	(19.45)	(19.30)	(25.50)
5 Years After Election	42.71+	42.35+	68.80*	-24.53	-23.67	-2.65
	(22.58)	(21.63)	(31.37)	(19.12)	(18.89)	(24.57)
6 Years After Election	80.06**	85.95**	87.92*	-25.20	-25.41	9.35
	(24.58)	(23.53)	(33.67)	(20.05)	(20.09)	(25.51)
Constant	317.28*	302.46*	286.45*	266.96	295.51	288.13
	(131.45)	(126.24)	(123.03)	(226.58)	(231.17)	(217.17)
Fixed Effects for Election Measure, Calendar Year, & 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	1,395	1,395	1,395	1,438	1,438	1,438
R-squared	0.91	0.91	0.91	0.95	0.95	0.95

Panel D: Predicted High-SES Achievement Limited to Districts Above and Below Median Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Low Enrollment			High Enrollment		
1 Year After Election	8.17	9.55	-1.51	-8.22	-9.00	-0.65
	(13.25)	(13.16)	(19.60)	(9.39)	(8.76)	(13.38)
2 Years After Election	23.73+	24.45+	29.43	-13.43	-12.57	-5.53
	(14.33)	(13.83)	(19.30)	(13.00)	(12.43)	(18.19)
3 Years After Election	24.29+	25.90+	36.13	-12.63	-8.78	14.44
	(13.86)	(13.72)	(22.17)	(15.29)	(14.09)	(26.58)
4 Years After Election	30.18	31.14	77.70	-23.54	-20.38	3.19
	(32.25)	(31.94)	(52.74)	(15.93)	(14.68)	(20.83)
5 Years After Election	29.75+	32.03*	45.15+	-32.65*	-31.11*	-12.61
	(15.16)	(14.63)	(25.56)	(14.31)	(13.85)	(20.16)
6 Years After Election	39.56*	41.62*	36.87	-27.04	-24.74	-2.33
	(17.46)	(18.52)	(30.04)	(16.34)	(15.62)	(22.23)
Constant	685.29**	701.31**	692.33**	493.04**	531.98**	542.52**
	(111.56)	(110.93)	(110.28)	(166.15)	(172.30)	(170.70)
Fixed Effects for Election Measure, Calendar Year, & 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	1,395	1,395	1,395	1,438	1,438	1,438
R-squared	0.82	0.82	0.82	0.91	0.91	0.91

Panel E: Predicted Low-SES Achievement Limited to Districts Above and Below Median Number of Schools

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Low Number of Schools			High Number of Schools		
1 Year After Election	13.43	14.58	24.90	3.39	3.12	-0.41
	(18.32)	(18.24)	(29.75)	(10.44)	(10.64)	(14.15)
2 Years After Election	35.19+	35.97*	33.64	-4.45	-3.30	-19.46
	(18.88)	(18.10)	(30.59)	(15.02)	(14.99)	(19.01)
3 Years After Election	39.22*	40.39*	47.97+	-0.67	-0.58	1.26
	(18.43)	(17.87)	(28.51)	(16.59)	(17.00)	(22.69)
4 Years After Election	24.79	23.86	73.78*	-20.11	-18.78	-12.65
	(21.29)	(20.85)	(33.59)	(16.98)	(16.71)	(20.92)
5 Years After Election	38.16+	37.65+	74.75*	-15.43	-14.54	-12.92
	(21.59)	(20.76)	(30.59)	(17.80)	(17.93)	(23.24)
6 Years After Election	78.02**	82.63**	92.51**	-17.99	-17.77	-2.89
	(23.82)	(22.48)	(32.59)	(17.41)	(17.54)	(23.41)
Constant	284.92*	295.66*	284.78*	557.70**	568.90**	550.77**
	(127.01)	(124.38)	(120.23)	(197.45)	(202.62)	(201.43)
Fixed Effects for Election Measure, Calendar Year, & 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	1,512	1,512	1,512	1,321	1,321	1,321
R-squared	0.91	0.92	0.92	0.96	0.96	0.96

Panel F: Predicted High-SES Achievement Limited to Districts Above and Below Median Number of Schools

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Low Number of Schools			High Number of Schools		
1 Year After Election	8.15	9.56	-6.07	-4.28	-5.74	2.36
	(13.03)	(12.95)	(20.46)	(8.99)	(8.90)	(13.05)
2 Years After Election	21.93	22.85	28.31	-7.06	-5.52	-3.92
	(14.27)	(13.94)	(20.29)	(14.00)	(13.71)	(18.80)
3 Years After Election	21.51	22.93+	29.86	-4.27	-0.86	15.14
	(13.09)	(12.96)	(21.25)	(15.71)	(14.58)	(26.11)
4 Years After Election	24.26	23.68	65.73	-17.21	-13.92	6.88
	(29.90)	(29.07)	(50.43)	(15.37)	(14.57)	(18.94)
5 Years After Election	20.15	21.68	36.06	-27.03+	-26.12+	-16.57
	(15.57)	(15.09)	(24.56)	(14.42)	(14.53)	(20.00)
6 Years After Election	30.98+	32.19+	26.14	-22.75	-20.43	0.89
	(18.12)	(18.56)	(28.96)	(15.91)	(15.71)	(20.98)
Constant	668.84**	680.09**	675.72**	643.33**	678.54**	670.00**
	(121.81)	(120.86)	(121.15)	(173.52)	(176.81)	(177.44)
Fixed Effects for Election Measure, Calendar Year, & 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	1,512	1,512	1,512	1,321	1,321	1,321
R-squared	0.83	0.83	0.83	0.91	0.91	0.91

Source: California Department of Education Academic Performance Index (API) data 1999-2013 and California Elections Data Archive 1999-2013, 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 RD sample on the running variable (+/-3.4% 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), % free lunch-eligible, % Black, and % 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 election and years since the election.

Robust standard errors adjusted for district clustering in parentheses. + p<0.10, * p<0.05, ** p<0.01

Sample is limited to those below (Models 1-3) and above (Models 4-6) the median value of free lunch eligibility (34.4%, Panels A & B), total enrollment (4,931, Panels C & D), and number of schools (9 schools, Panels E & F).

Shading indicates coefficients are significantly different (Paternoster et al. 1998) in samples above and below median: indicates p<0.05; indicates p<0.10

Table S9: Predicted District Staff Characteristics: ITT Analyses

Panel A: Experience

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Mean Total Years Experience			Mean Years Experience in the District		
1 Year After Election	-0.28	-0.27	-0.32	-0.03	-0.00	0.27
	(0.51)	(0.52)	(0.85)	(0.57)	(0.58)	(0.96)
2 Years After Election	-0.34	-0.31	-0.37	-0.03	-0.03	0.26
	(0.48)	(0.48)	(0.69)	(0.50)	(0.50)	(0.70)
3 Years After Election	0.16	0.18	0.38	-0.16	-0.16	-0.27
	(0.51)	(0.50)	(0.66)	(0.56)	(0.57)	(0.75)
4 Years After Election	-0.01	0.05	0.44	-0.20	-0.16	0.03
	(0.51)	(0.51)	(0.68)	(0.59)	(0.60)	(0.80)
5 Years After Election	-0.08	-0.07	0.19	-0.34	-0.36	-0.40
	(0.60)	(0.61)	(0.85)	(0.67)	(0.69)	(0.84)
6 Years After Election	-0.26	-0.27	0.35	-0.39	-0.43	-0.28
	(0.72)	(0.72)	(1.01)	(0.77)	(0.78)	(0.94)
Constant	24.16**	24.57**	24.18**	27.51**	27.87**	27.58**
	(6.77)	(6.79)	(6.72)	(6.16)	(6.24)	(6.16)
Fixed Effects for Election Measure, Calendar Year, & 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	2,210	2,210	2,210	2,210	2,210	2,210
R-squared	0.84	0.84	0.85	0.84	0.84	0.84

Panel B: Education

VARIABLES	(1)	(2) % BA or Less	(3)
1 Year After Election	0.52	0.87	3.04
	(2.34)	(2.26)	(3.05)
2 Years After Election	3.25	3.18	2.08
	(2.58)	(2.61)	(3.63)
3 Years After Election	2.79	2.70	1.82
	(2.78)	(2.78)	(3.96)
4 Years After Election	2.25	2.11	0.02
	(2.70)	(2.63)	(3.48)
5 Years After Election	2.61	2.38	-1.49
	(3.16)	(3.04)	(3.92)
6 Years After Election	2.09	1.77	-0.83
	(3.52)	(3.40)	(4.34)
Constant	53.67*	53.55*	54.35*
	(20.69)	(20.78)	(21.34)
Fixed Effects for Election Measure, Calendar Year, & Year Since Election	Y	Y	Y
Vote Share^2	N	Y	Y
Vote Share^3	N	N	Y
Observations	2,214	2,214	2,214
R-squared	0.90	0.90	0.91

Source: California Department of Education Academic Performance Index (API) data 1999-2009, California Elections Data Archive 1999-2009, and California Basic Educational Data System data 1999-2009, limited to district-year observations with staff profile information from 2 years before to 10 years after the focal bond election, and within the RD sample on the running variable (+/-3.4% 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), % free lunch-eligible, % Black, and % 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 election and years since the election.

Robust standard errors adjusted for district clustering in parentheses. + p<0.10, * p<0.05, ** p<0.01

Table S10: Predicted District Staff Characteristics: TOT Analyses

VARIABLES	(1) Mean Total Years Experience	(2) Mean Years Experience in the District	(3) % BA or Less
1 Year After Election	-0.13	-0.09	0.15
	(0.23)	(0.22)	(1.01)
2 Years After Election	-0.40*	-0.33+	1.34+
	(0.19)	(0.18)	(0.75)
3 Years After Election	-0.37+	-0.30	1.07
	(0.21)	(0.19)	(0.87)
4 Years After Election	-0.25	-0.18	1.42
	(0.22)	(0.20)	(0.94)
5 Years After Election	-0.39+	-0.40+	0.50
	(0.23)	(0.21)	(0.97)
6 Years After Election	-0.32	-0.29	-0.02
	(0.27)	(0.24)	(1.08)
Constant	14.28**	10.40**	68.90**
	(0.27)	(0.27)	(0.84)
District & Year Fixed Effects	Y	Y	Y
Vote Share^2	Y	Y	Y
Demographic Controls	N	Y	Y
Observations	11,293	11,293	11,293
R-squared	0.02	0.03	0.06
Number of Districts	1,049	1,049	1,049

Source: California Department of Education Academic Performance Index (API) data 1999-2009, California Elections Data Archive 1999-2009, and California Basic Educational Data System data 1999-2009, limited to district-year observations with staff profile information.

All models include district and year fixed effects and the following controls measured in each prior year (1-18): having a bond measure, passing a bond measure, pass cutoff threshold, vote share raw and squared.

Robust standard errors adjusted for district clustering in parentheses. + p<0.10, * p<0.05, ** p<0.01

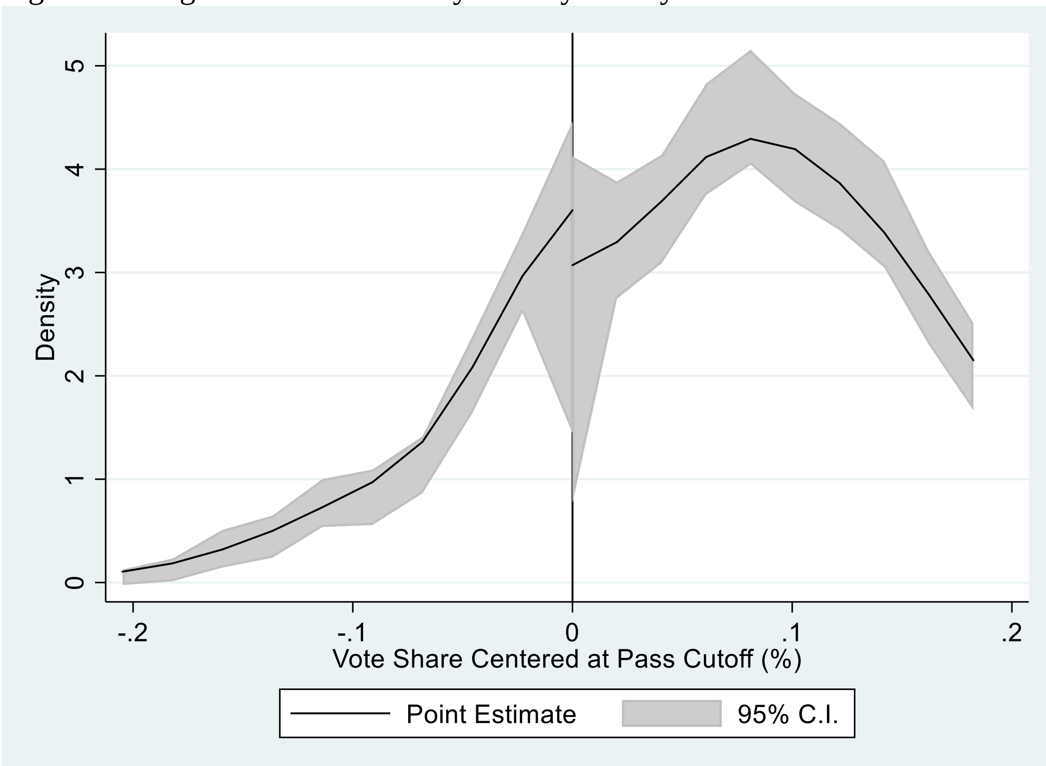
Table S11: Predicted Low-SES Achievement and Capital Outlays in Districts with Fewer than Five Schools

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Low-SES Achievement			Capital Outlays/Pupil ($1k)		
1 Year After Election	31.82	33.45	76.97	1.49	1.53	0.63
	(30.12)	(30.01)	(49.19)	(2.32)	(2.32)	(4.22)
2 Years After Election	54.03+	55.56*	69.93	4.31*	4.26+	7.56*
	(27.99)	(27.83)	(45.53)	(2.14)	(2.14)	(3.01)
3 Years After Election	60.79*	60.75*	83.46+	3.53	3.62	3.80
	(26.74)	(26.36)	(47.01)	(2.89)	(2.90)	(5.51)
4 Years After Election	20.20	19.19	113.20+	3.01	3.25	-1.37
	(38.36)	(41.03)	(64.47)	(3.20)	(3.14)	(4.93)
5 Years After Election	52.08+	53.19+	101.71+	3.58+	3.54+	1.76
	(31.21)	(31.43)	(51.85)	(1.90)	(1.82)	(3.25)
6 Years After Election	114.89**	126.11**	159.85**	-0.18	-1.85	2.02
	(32.42)	(32.31)	(47.78)	(2.30)	(2.79)	(3.02)
7 Years After Election	83.36**	85.84**	108.00**	2.11	2.10	3.17
	(24.60)	(24.22)	(38.90)	(2.44)	(2.53)	(4.04)
8 Years After Election	69.22+	73.19+	106.01+	-1.00	-1.15	5.35
	(34.80)	(36.75)	(62.69)	(2.37)	(2.55)	(4.92)
9 Years After Election	94.02**	97.09*	123.05+	-1.74	-1.53	2.80
	(35.20)	(37.60)	(62.00)	(2.46)	(2.33)	(3.29)
Constant	440.18**	476.67**	434.31**	16.88	17.10	20.75
	(165.97)	(166.92)	(159.58)	(13.74)	(14.02)	(13.80)
Fixed Effects for Election Measure, Calendar Year, & 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	689	689	689	677	677	677
R-squared	0.90	0.90	0.91	0.44	0.46	0.48

Source: CDE API data 1999-2013 and CEDA 1999-2013, 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 RD sample on the running variable (+/-3.4% from the pass cutoff). Models are the same as those in Table 4, but the sample is limited to districts with fewer than 5 schools.

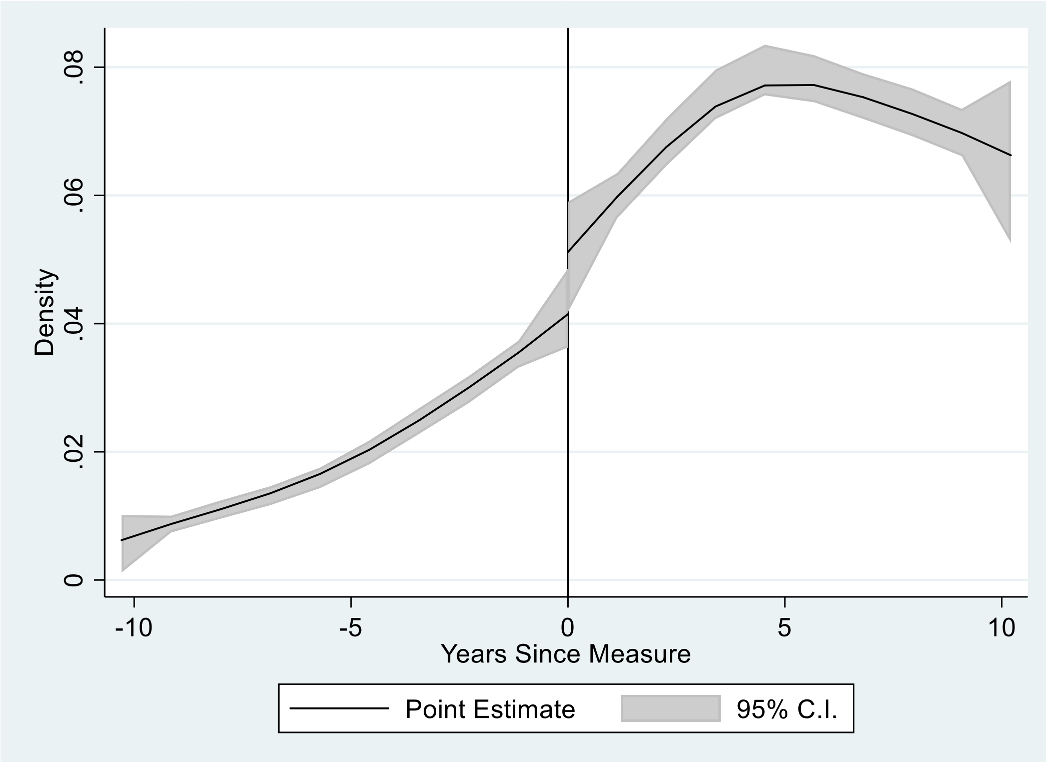
Robust standard errors adjusted for district clustering in parentheses. + p<0.10, * p<0.05, ** p<0.01

Figure S1: Regression Discontinuity Density Plot by Percent of Votes for Bond Measure



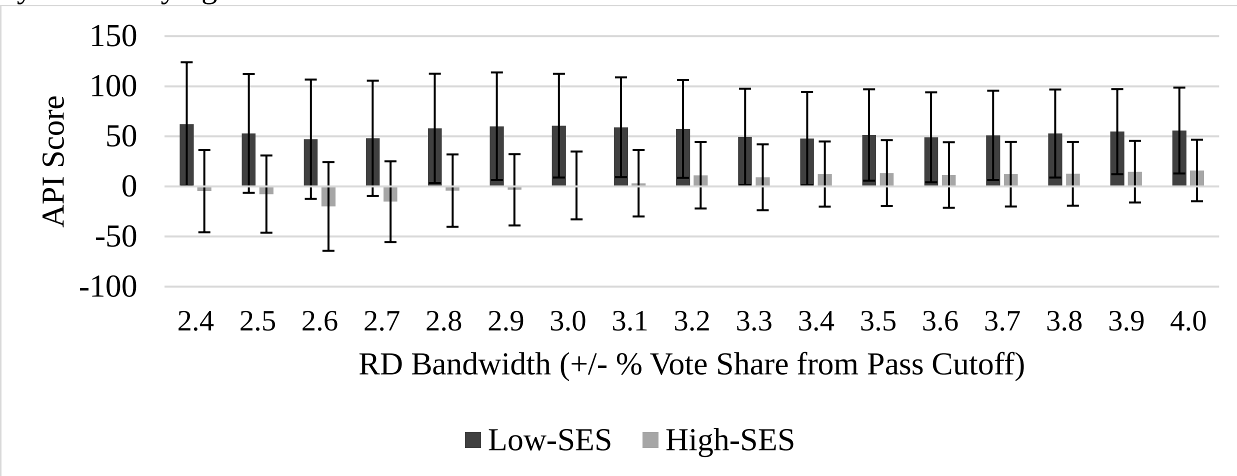
RD density plot using default settings in rddensity package in Stata 15 (Cattaneo et al. 2018). Defaults are quadratic of vote share to construct density point estimates and cubic of vote share to construct the bias-correct estimates. Conventional and robust estimates are not significant at 95% level whether including all bond measures with achievement data or the regression discontinuity sample (+/-7.1% from cutoff required to pass).

Figure S2: Regression Discontinuity Density Plot by Years Since Measure



RD density plot using default settings in rddensity package in Stata 15 (Cattaneo et al. 2018). Defaults are quadratic of years since measure to construct density point estimates and cubic of years since measure to construct the bias-correct estimates. Conventional estimate is not significant; robust estimate is significant (p<0.01).

Figure S3: Estimated Effects of Passing a Bond Measure 6-Years Post-Election on Achievement by SES: Varying RD Bandwidth



Based on estimated effects 6 years after the election in Table S4.

Figure S4: Total District Enrollment by Percent of Votes for Bond Measure

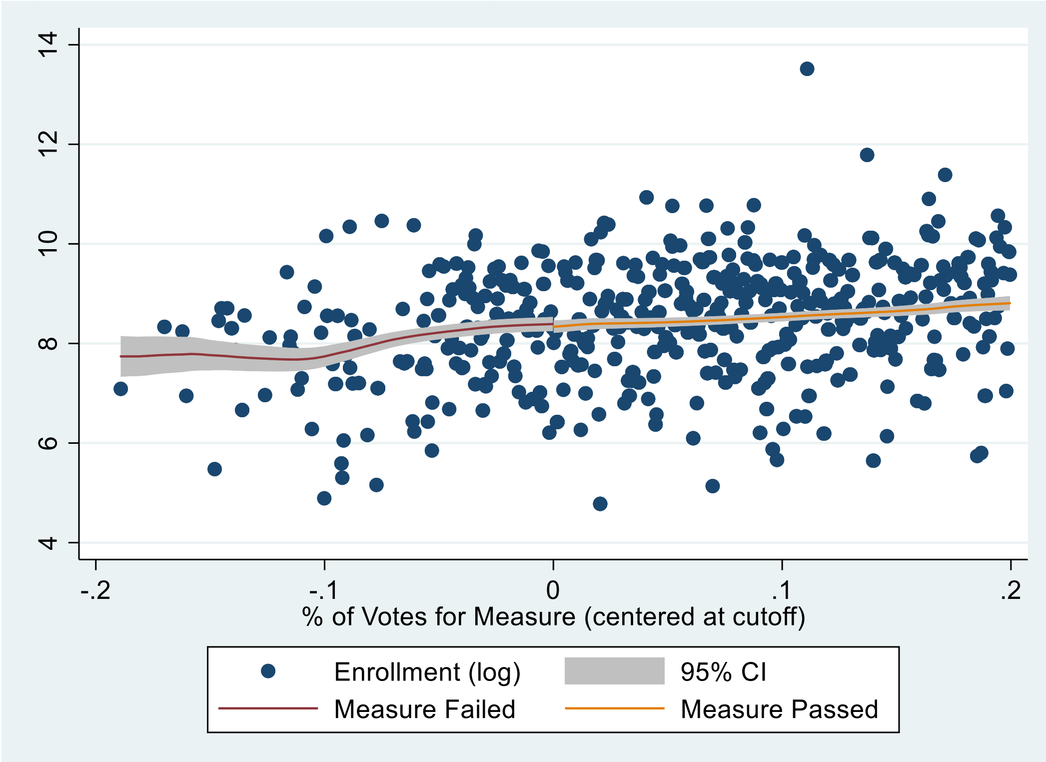


Figure S5: Annual Change in District Enrollment by Percent of Votes for Bond Measure

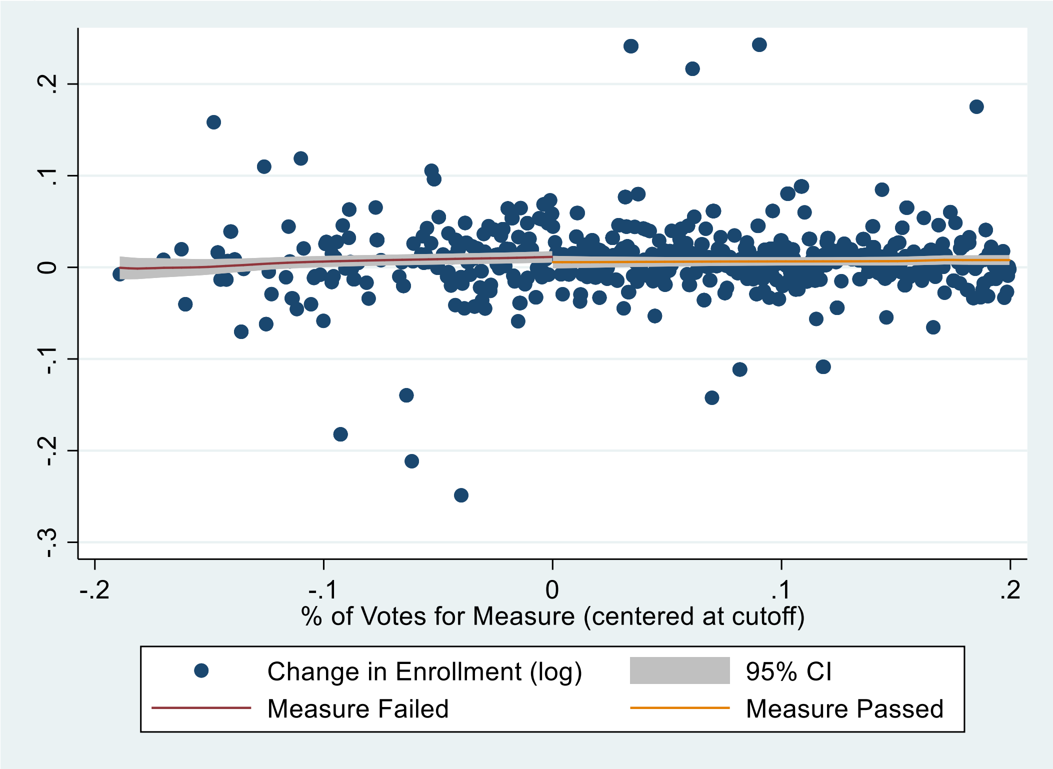


Figure S6: Percent Black Students by Percent of Votes for Bond Measure

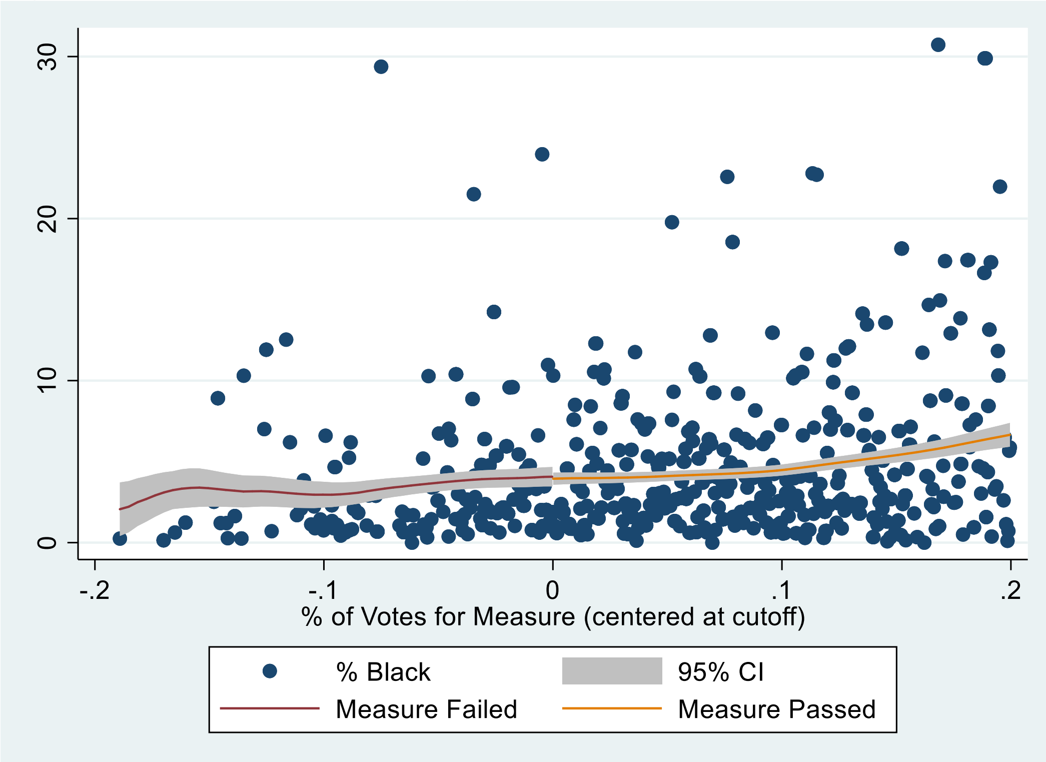


Figure S7: Percent Hispanic Students by Percent of Votes for Bond Measure

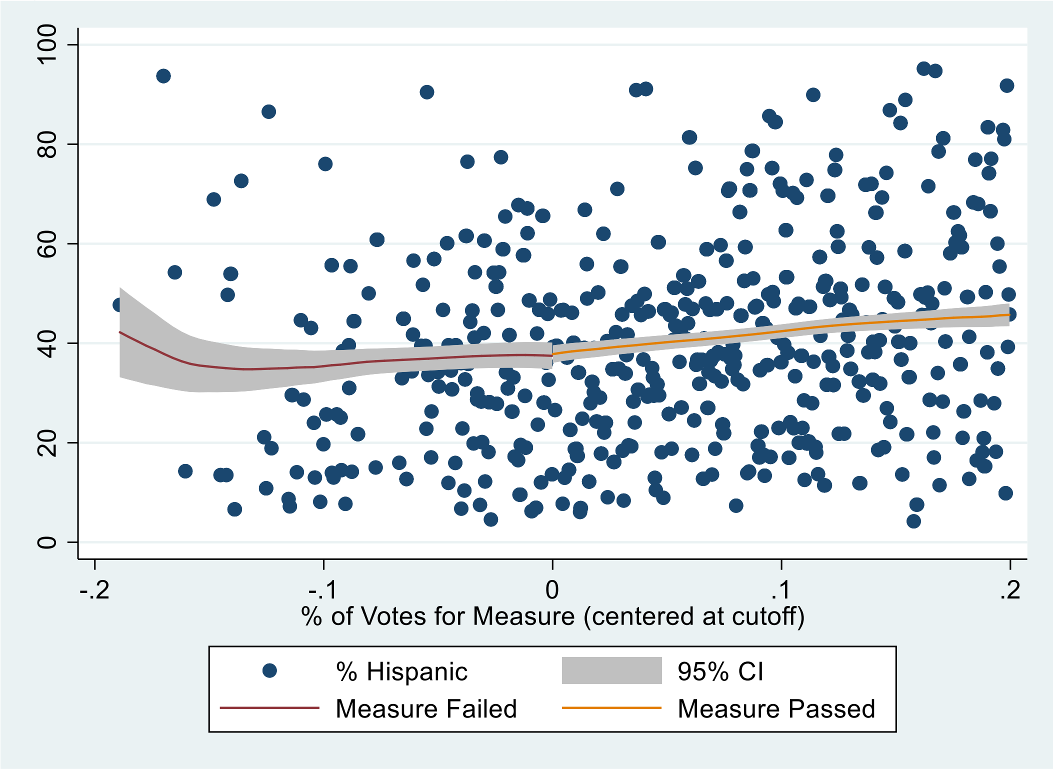


Figure S8: Percent Native American Students by Percent of Votes for Bond Measure

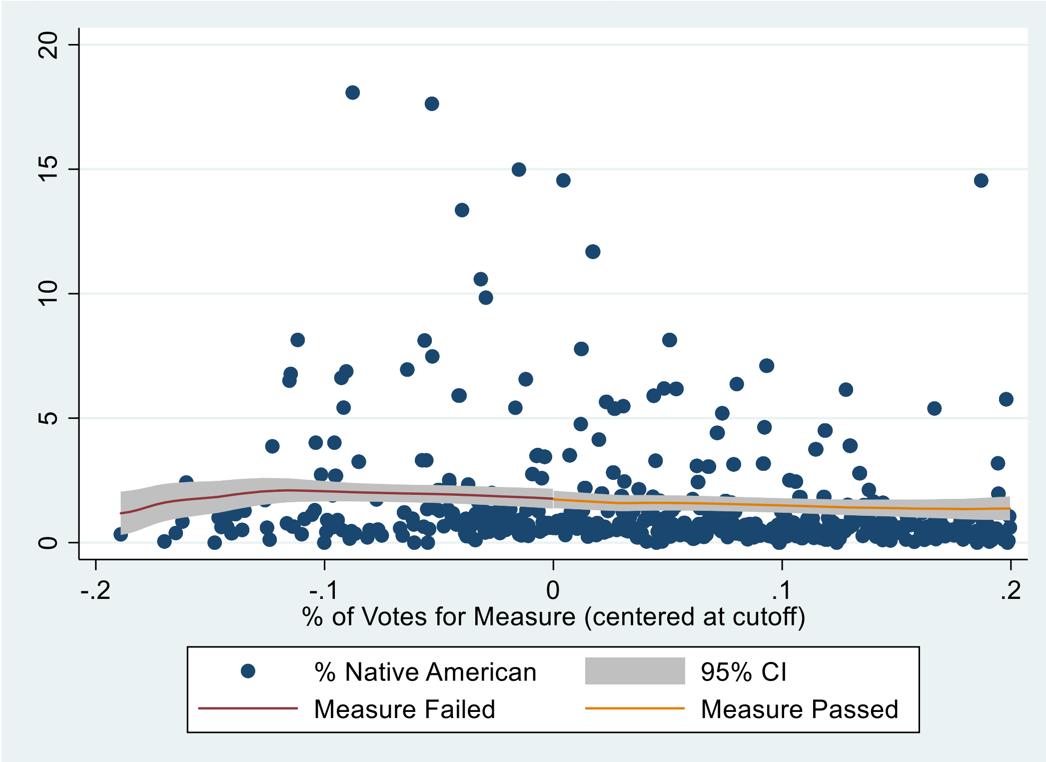


Figure S9: Percent Free Lunch-Eligible Students by Percent of Votes for Bond Measure

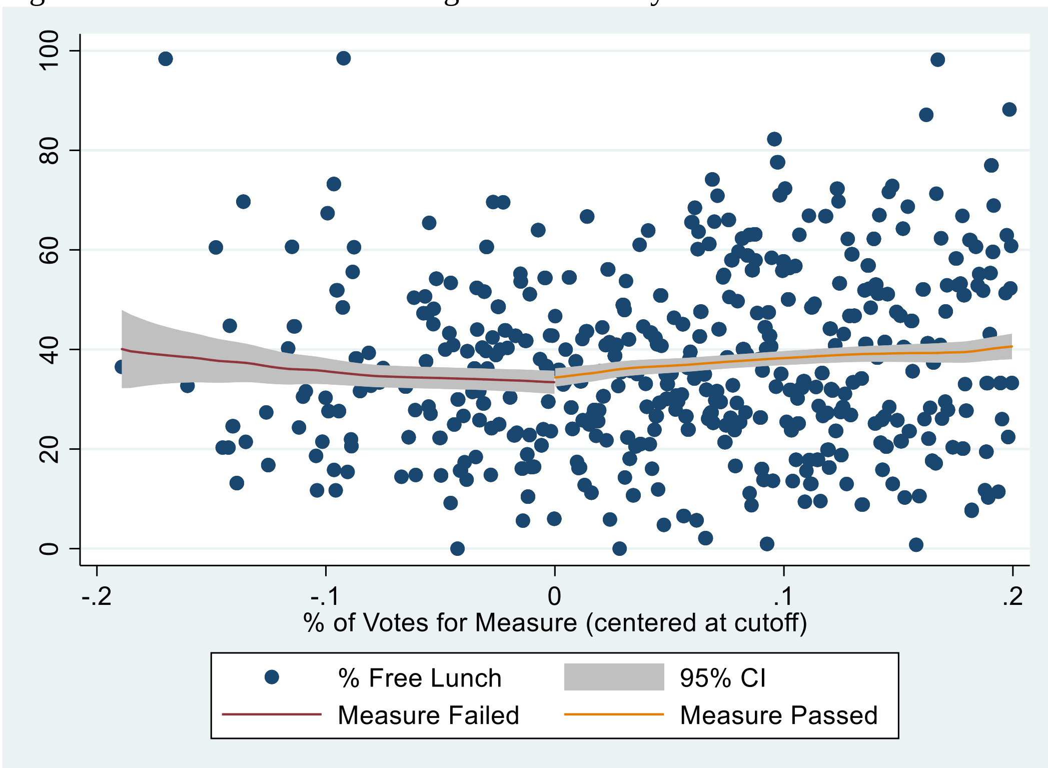


Figure S10: Proposed Bond Amount per Pupil by Percent of Votes for Bond Measure