Regression Discontinuity Estimates of the Effects of Winning School Bond Elections on School Spending, Test Scores, and Teacher Retention

Ethan Krohn

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Abstract

California school districts spent over $6 billion in capital spending in 2014 and voted on 1,966 school bond measures between 1995 and 2016 meant to finance capital spending, yet it is unclear from the literature what effect capital spending has on student outcomes or whether there are other effects caused by passing school bond measures. I expand this literature by looking at the effect of passing school bond measures on standardized test scores in California using a dynamic regression discontinuity design. I find considerable increases in capital spending after a bond measure passes that is largely explained by spending on construction. I also find a large positive and immediate divergence in test scores and in proficiency rates between the districts close to the vote-share threshold that pass and that fail to pass school bond measures. This divergence starts the year of the elections and is difficult to explain based on the change in capital spending. Furthermore, this effect lasts several years. I further explore this effect by looking at the effects on teacher and staff turnover.

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1 Introduction

California school districts spent over $6 billion in capital investments in the 2013-2014 school year (Cornman et al. 2018). The United States as a whole spent more than $53 billion on capital spending, amounted to $1029 per student during the 2014-2015 school year (Cornman et al. 2018; NCES 2018). The large majority of capital spending by California school districts is financed by passing school bond measures in local elections (Lopes and Ugo 2017). California school districts voted on 1,966 school bond measures between 1995 and 2016. Yet mixed findings from the previous literature leave the public unclear whether these bond measures impact student achievement or other outcomes.

This paper uses a dynamic regression discontinuity design (RD) and the outcomes of these school bond election measures to provide causal evidence of effects on capital spending, student achievement, and teacher and staff turnover. Unlike the previous literature using school bonds measures, I look at test scores the year of the election, before most spending would begin, to look for morale effects in addition to any effects from increases in capital spending itself.

As expected, I find a large increase in per-student capital spending during the six years after a bond measure passes, which is almost completely explained by increased spending on construction. I do not find evidence that would indicate that the increased capital spending benefited student test scores in the long-term, though I cannot rule it out. I do, however, find that school districts that pass a bond measure significantly outperform school districts that fail one during the year that the bond measures passed and the two years following. This effect is 0.11 of a standard deviation in both the year of the election and the following year and 0.093 of a standard deviation two years after the elections in my main specification. I also find increases in the proficiency rate for several years following the elections. Because this takes place before most of the increase in spending and because the spending is largely on construction, which may take some time to have an impact, it suggests that the passage of a bond measure might have impacts other than those operating through capital spending. I support this by showing an effect on teacher retention following elections on school bond measures. I find no effect on the demographics of the student body that could explain the effects on test scores.

This paper fits into two large areas of study on student achievement, the effects of capital spending as well as the impact of school climate and morale. The literature on capital spending includes several high quality studies but has not reached a clear consensus. Some papers find large positive effects and others finding no effects (Duyar 2010; Hanushek 1997; Greenwald, Hedges, and Laine 1996; Bowers and Urick 2011; Jackson 2018). Papers on large capital spending projects also find differing effects immediately after construction begins. Some papers find positive effects starting immediately and some find negative effects during construction (Neilson and Zimmerman 2014; Goncalves 2015; Conlin and Thompson 2017). Papers in the capital spending literature have put forth several mechanisms for how capital spending could affect student achievement in the long-term including improved lighting, indoor air quality, climate control, building quality, noise levels, as well as science laboratories and libraries (McGuffey and Brown 1978; Uline and Tschannen-Moran 2008).

I am aware of six studies that use school bonds as a source of variation in capital spending to estimate the impact on standardized test scores. All of them also use a dynamic regression discontinuity design. The first paper to use this approach, by Cellini, Ferreira, and Rothstein, also looks in California (Cellini, Ferreira, and Rothstein 2010). They find sizable and immediate increases in house prices and capital spending after the passage of a school bond but found only a limited effect on test scores. Cellini, Ferreira, and Rothstein only look at third and fourth grade scores between 1998 to 2007, finding small changes in third grade test scores that only appear six years after the bonds were passed. Another paper by Hong and Zimmer looks at fourth and seventh grade student proficiency levels in Michigan using a fuzzy RD extension to the method used by Cellini et al. It finds initial negative effects for the first two years followed by positive effects four to eight years after a bond is passed (Hong and Zimmer 2016). A study by Martorell, Stange, and McFarlin studies the effect of school bonds in Texas (Martorell, Stange, and McFarlin Jr 2016). This study finds no effect on test scores. A working paper by Enami uses referenda in Ohio that are not earmarked solely for capital spending but is able to separate out the effects for capital spending (Enami 2017). He looks at math proficiency levels and does not find an effect from capital spending. A working paper by Rauscher has significant overlaps with my own (Rauscher 2019). She also looks at California from 1999 to 2013, but looks specifically at differences between high and low SES students. She uses Academic Performance Index scores, which draws from a variety of content areas. She sticks closely to the specifications used in the previous literature with the exception of using a longer window. She finds positive effects for only low SES students that emerge six years after an election. Finally, a working paper by Baron looks at both referenda and school bond measures in Wisconsin and finds no effects from capital spending (Baron 2019). These papers show that there does not seem to be a consistent effect across states, demographics, and time periods. A paper by Kyung-Gon Lee and Solomon Polachek also uses close elections to look at the effect of school spending, but it does not focus on capital spending in particular (Lee and Polachek 2018).

My paper is a contribution to the literature in a number of ways. None of these papers look closely for an effect the year of the election. Enami and Hong and Zimmer both report findings for the year of the election, but I make improvements over the methods of both papers. Though Cellini et al. and Rauscher also look at California, I use a more recent data set than Cellini et al. that includes test scores from more grades and covers more years of test scores, and I do not focus on heterogeneity by SES status like Rasucher. I also bring more recent non-parametric RD approaches and modern bandwidth selection to bear.

There is also a large literature on school climate and morale's effect on academic performance (Thapa et al. 2013). This literature largely exploits cross-sectional or panel correlations between morale and test scores. As far as I am aware, no studies have looked at school morale effects from school bond measures, and I am not aware of any studies using natural experiments to look at school morale's influence on student achievement. Despite this, many US government agencies are convinced that safe and positive school morale is vital to learning (Youth.gov 2019; D. o. Education 2016).

Given that I find significant test score effects immediately after the election, it is important to explore the various mechanisms that could lead to these effects. There is anecdotal evidence of school bond measures succeeding or failing having impacts on morale. Passing a school bond measure may send a message to educators that the community supports them. In interviews afterward successful bond measures, educators often say that the measure will increase morale (Williamsm 2018; Wheeland 2019). The opposite can happen when a bond measure fails. After the failure of some bond measures in the Tuscon area, a superintendent of one of the districts was quoted as saying, "The toughest part of this loss is the (impact on) morale. We were hoping for some sort of light at the end of the tunnel. Some sort of signal that the community hasn't forsaken us" (Stephenson 2017). I also include outcomes such as teacher turnover and am planning to analyze teacher and student satisfaction.

In section 2 of this paper, I provide a background on school bond measures in California. In section 3, I present the data used in my analysis and show the summary statistics. In section 4, I explain my empirical strategy and show the results. In section 5, I go through several robustness tests. In section 6, I explore several possible mechanisms. Finally, I conclude in section 7.

2 Overview of School Bond Measures in California

In 1984, California passed Proposition 46, which allows school district to issue general obligation bonds in order to finance capital spending. The bonds must be approved by a local referendum which specifies out how the funds that they raise can be spent. Almost always, the bonds are secured with a promise to levy property taxes in an unlimited amount as needed to repay the debt. Under Proposition 46, bond measures were required to pass with two-thirds of the vote. In 2000, California passed Proposition 39, which allows most bonds to pass with 55% of the vote instead. In order to pass at the 55% level, the bond measures must meet some additional criteria not needed for the bonds facing the two-thirds threshold. All of the funds raised by bonds are required to be used for capital spending and explicitly cannot be used to fund teacher or administrator salaries or operating expenses. Here is an example ballot measure for a school bond that passed in the Davis Joint Unified School District:

> To build two new elementary schools and one new junior high school to relieve overcrowding; repair, rehabilitate, and expand all elementary, junior high, and high schools; improve access to technology; accommodate class size reduction; replace aging electrical systems, roofs, plumbing and sewer lines; and acquire needed land; shall the Davis Joint Unified School District issue bonds in the amount of $55,200,000 with an interest rate not to exceed the legal limit?

This example is typical of a bond measure in California. They often list a few specific large projects such as purchasing land or building new schools and then list broad uses for capital spending that can range from building new schools to paying for repairs and modernizing the campuses. Interestingly, the descriptions often list retaining qualified teachers among the purposes of the bonds. They do not normally mention that the bonds will have to be payed back through property taxes.

3 Data

I obtain election data from the California Election Data Archive, which is generated through a partnership between the Sacramento State University and the California Secretary of State. The data set covers all local elections in California between 1995 and 2016. It includes the number of votes in favor and against each proposition as well as the vote threshold needed to pass. It also includes the wording or how the measures appear on the ballot. I assign each election to a school year. Following the nomenclature used in the other data, I refer to a school year by the calendar year in that it ends, so the 2003-2004 school year is referred to as the 2004 school year.

Bonds were assigned to a school year using July 1st as the cutoff as that is the date that the school budgets are due. Unlike the previous literature, if a district had multiple elections the same year I dropped the district from my data. The previous literature, as far as I can tell, used the larger bond measure if a district had more than one.[1] This leaves 728 districts that had at least one election between 1995 to 2016.

The school district spending data come from the National Center for Education Statistics Common Core of Data. This data set contains a breakdown of how the expenditures are used including the amount spent on capital spending, construction, and other equipment. For this study, I use spending data covering the years from 2000 to 2016.

The test score data come from the California Standards Test, which is part of the STAR program. This data set covers the 2003 school year to the 2013 school year. I use test data at the school year-grade-district level. They include the number of students who are designated as economically disadvantaged. The CDE defines economically disadvantaged students as "students eligible for the free and reduced priced meal program (FRPM), foster youth, homeless students, migrant students, and students for whom neither parent is a high school graduate" (Torlakson 2015). From the STAR data, I also use the number of students eligible to take the STAR test as my measure of enrollment. For this project, I use only test scores from the English Language Arts test that is administered from second grade through eleventh and test scores from the Mathematics test for second through sixth grade. I do not include the math test for higher grades as after sixth grade students are able to take different math tests based on what class they are enrolled in. I then normalize the test scores by grade, school year, and test subject to mean zero and standard deviation one.

I also build a measure of staff turnover using the Staff Demographics Data from the California Department of Education. For the 1998 through 2012 school years, this data set gives demographic information for each staff member at each district. This includes how many years they have been at the district. Unfortunately, staff members are not linked across years of the data set so I must infer teacher retention from the counts of teachers of particular types.

The test score data were identified by school year and school district IDs called LEAIDs. The spending data were identified by school year and by a different NCES ID. The NCES IDs were matched to LEAIDs using Public Schools and Districts data file found on the California Department of Education's website. When necessary, the CDE's California School Directory was used to match them by hand. This was mainly for school districts that are no longer active. The election data set was more difficult to match to the district data. It was merged to the funding data first based on the name, county, and year. When this was not enough they were merged by hand.[2]

Table 1 lists the number of elections held by month. Most elections take place in June and

[1] This is definitely the case for Martorell, Stange, and McFarlin (Martorell, Stange, and McFarlin Jr 2016)

[2] School district names were often abbreviated in different ways in the election data. Several typos were also found.

November when there would be general elections. Table 2 lists the number of districts by the number of elections that they had during the sample.[3] The large majority of districts that voted on bond measures held elections more than once. Table 3 gives summary statistics about the bond measures. As mentioned in the previous section, all bonds required a two-thirds majority before 2001. There is a noticeable increase in the pass rate after the required vote share is decreased. Interestingly, there is no clear increase in the number of elections. Almost all school district held elections at the fifty-five percent threshold rather than the two-thirds threshold once that option became available. Table 4 reports summary statistics for the school districts. It splits up the districts into districts that had no school bond measures, districts that did not pass all school bond measures during the sample, and districts that did pass all measures that they voted on. The districts that did not vote on any school bond measures tend to be much smaller, already have much higher revenue per student and expenditure per student, they perform worse on standardized tests, and have a greater portion of students that receive free or reduced price meals. This could indicate that districts need to be a certain size to be able to organize a school bond measures or that districts that have less funds per student are more likely to find it worth while to put in the effort to hold an election for a school bond measure. The districts that failed to pass some measures and the ones that passed all measures are much more similar to each other.

4 Empirical Strategy and Results

The numerous elections for school bond measures in California lend themselves well to a regression discontinuity approach. This design is founded on the idea that differences between districts other than those caused by barely passing or failing to pass bond measures shrink as you approach the election cutoff needed for the measures to pass. An RD regresses the outcomes on a flexible but smooth function of the running variable, possibly some controls, and an indicator variable for whether the bond measure passed. Then, under assumptions about continuity, which side a bond measure ends up on is like a coin flip near the threshold. The coefficient on the indicator variable is the local estimate of the differential effects of passing versus failing.

The main challenge when looking at school bonds is that most districts have multiple elections creating the need to separate out effects from multiple treatments. We may care about two different effects. One would be the effect of passing a single election and not controlling how that may impact the timing of future bond measures. For instance, a district that passes a bond measure may be

[3] Some elections were not used for a variety of reasons. Some I was unable to match. All elections that took place in April or May were thrown out for the year of the election because of uncertainty in when the tests took place. Finally, some were thrown out if a district had multiple elections during the same year.

unlikely to hold another immediately afterwords. I follow the previous literature in calling this the intent to treat effects (ITT). The other effect would be the effect of passing a single bond measure when keeping the future elections constant. I call this the treatment on the treated effects (TOT). I present three specifications; a parametric ITT specification, a non-parametric ITT specification, and a parametric TOT specification.

4.1 Sample Construction

Unlike the previous literature, I put more focus on looking for an effect the year of the election as well as afterward. Most of the previous literature considered only the years after the election as they were more focused on the effect of capital spending. By looking at the year of the election, I can examine the effect of the election itself. In order to do this, I assign elections to a school year using June first as the cutoff, so that an election that takes place after June first is assigned to the following school year and one that takes place before is assigned to the previous one. This is different than the Martorell et al. paper, which used September first as the cutoff (Martorell, Stange, and McFarlin Jr 2016). I did this because if I look at the effect during the year the bond passed, I need to make sure that the elections are not being held after the tests were administered. All schools take the test during the spring, but I do not have data on when each school district actually takes it. Almost all elections take place in February, March, June, or November. This can be seen in Table 1. Based on anecdotal evidence, I believe that most schools take the test after the March elections. So, I drop the year of the election for any school districts that have elections during April and May as the elections may have taken place after the tests were taken. This is 344 observations for 76 elections.

In the case that a district held elections on multiple bond measures during the same year, I discard the district completely from my sample. There are 70 districts that do this. This is unlike the Martorell et al. study, which uses the characteristics of the largest bond in a district that voted on multiple measures during the same year or the Hong and Zimmer study, which used the election with the largest vote-share (Martorell, Stange, and McFarlin Jr 2016; Hong and Zimmer 2016). I do this because I because I believe that even a small bond measure could have morale or other effects.

4.2 Testing for Endogenous Sorting

One concern for regression discontinuity identification is endogenous sorting around the vote threshold. If districts could select which side of the threshold they fall on, then one would worry that district on either side of he threshold were no longer similar on observable or unobservable characteristics. This is less of a concern for this setting. First, districts should always want to pass the bond measures as they choose whether to hold elections on them. Second, outside the possibility of election fraud, we would expect only the possibility of partial manipulation, where the districts may have some control of the running variable but there is still an idiosyncratic element. In the setting of this paper, districts can choose when to hold an election and whether to fulfil the requirements to have a fifty-five percent threshold, but there is still idiosyncratic behavior from the voters. Partial manipulation is generally not a concern for identification (McCrary 2008). Figure 1 shows a kernel density estimate of the difference between the vote share and the threshold needed to pass, and figures 2 and 3 show histograms of several school district characteristics by distance from the threshold. In Table 5, I test several covariates for differences at the cutoff. I use a local linear regression following the method described by Calonico, Cattaneo, Farrell, and Titiuni (Calonico et al. 2017). My running variable is the distance from the threshold. I find no significant differences at threshold for these characteristics.

I perform a density test of the full sample using a method by Cattaneo et al., and fail to reject no manipulation (Cattaneo, Jansson, and Ma 2018). The p-value of the test is 0.3623. The graph of that test can be seen in Figure 4. I also perform tests for the two thresholds separately. The test of the elections with a fifty-five percent threshold has a p-value of .0793 and can be seen in Figure 5. This is significant at the ten percent level and could indicate that districts are strategic about whether they choose to hold an election on a school bond proposition or possibly about the type of threshold they qualify for. The test of the elections with a two-thirds threshold has a p-value of 0.9672 and can be seen in Figure 6. This is not significant and could indicate that these districts are less sophisticated. As a robustness test, I also run this test for elections with a fifty-five percent threshold from districts that failed to pass at least one election in Figure 7. Here, the test finds no evidence of sorting. I will show in the next section that my results are robust to limiting myself to only that sample.

4.3 Parametric ITT Specification

The simplest RD approach is to run separate RDs for individual years relative to the elections. This means running an RD for the year of the elections, and then more RDs for each subsequent year relative to the elections. This does not control for the possibility of multiple elections, and so can be thought of as an intent to treat effect. It can be interpreted as the effect of manipulating the outcome of a single election without controlling how that could affect future bond measures. Because of this, it may not be accurate for beyond the first few years after the elections as a measure of the effect of an exogenous change in capital spending. In their paper, Cellini et al. select a window of years around each election and then include focal election fixed effects in their ITT RDs (Cellini, Ferreira, and Rothstein 2010). They justify this as it being necessary to account for time-invariant district characteristics. They use a window that goes from two years before the elections to six years after. Martorell et al. and Rauscher use a window from two years before to ten years after (Martorell, Stange, and McFarlin Jr 2016; Rauscher 2019). Hong and Zimmer use a window that goes from two years before to thirteen years after (Hong and Zimmer 2016). Enami is less clear but seems to use a window from the year of the election to ten years after (Enami 2017). I follow Cellini et al. in picking a window from two years before the elections to six years after. This window is collected for each election, and then a data set is made by appending these windows together. When the windows of multiple elections in the same district overlap duplicate observations are included. Each observation is uniquely identified by the triplet j, t, and τ, where j identifies the district, t identifies the year of the focal election, and τ identifies the year relative to that election. I then regress the outcomes on $Pass_{jt}$, an indicator whether a bond measure passed in the year of the focal election, $P_g(VotesShare_{jt}, \gamma_\tau)$, a polynomial of the vote-share for the focal election, and fixed effects for relative years, calendar years, election threshold and focal elections.

I make several small changes to the specifications used in the other papers. I interact the polynomial of the vote-share with the cutoff required to pass for each election.[4] I believe that this is good practice as it takes into account that the vote-share may have different implications based on the threshold. I add a control for the percentage of the student body that is classified as economically disadvantaged. This is similar to Rauscher who also adds a number of district demographic controls (Rauscher 2019). I include grade fixed effects which were included in some of the other papers such as Martarell et al. when combining outcomes from multiple grades (Martorell, Stange, and McFarlin Jr 2016). I also include test subject fixed effects.

This gives the specification:

$$\begin{aligned} y_{jtgs\tau} = {} & Pass_{jt}\theta_\tau^{ITT} + P_g(VoteShare_{jt} * FiftyFive_{jt}, \gamma_\tau) + \\ & P_g(VoteShare_{jt} * TwoThird_{jt}, \psi_\tau) + \eta EconDis_{jt\tau} + \alpha_\tau + \kappa_{t\tau} + \rho_g + \omega_s + \lambda_{jt} + e_{jt\tau} \end{aligned} \tag{1}$$

where $FiftyFive_{jt}$ and $TwoThird_{jt}$ are indicators for the vote-share needed for the election to pass. $EconDis_{jt\tau}$ is the percentage of students classified as economically disadvantaged. α_τ,

[4] Specifications with the polynomial and the required vote-share not interacted can be found in the appendix and have similar results.

κ_t, ρ_g, ω_s, λ_{jt} are relative year, school year, grade, test subject, and focal election fixed effects. I cluster by district.

The results for the ITT effects on per-student spending can be seen in Table 6. I find large increases in capital spending of about two thousand dollars per student the two years after an election. I also find a modest increase in spending on equipment of about fifty-three dollars per student three years after the election. Strangely, I also find an increase in spending on instruction the year of the election that does not persist. In Table 7, I show the estimated ITT effects on normalized test scores. Here, I find substantial positive effect on test scores for two years after the elections. Interestingly, this increase starts the year of the election, before I see an effect on spending. The effect loses significance after two years, but point estimates remain high, and I cannot rule out a persistent effect.

4.4 Non-Parametric ITT Specification

I also try a non-parametric approach to finding the ITT Effects. This was not done by the other papers in this literature. I use a more flexible local linear RD using the method described by Calonico, Cattaneo, Farrell, and Titiuni (Calonico et al. 2017). This is done by running separate RDs for individual years relative to the elections. When using this approach I cannot use a separate polynomial for the two election thresholds. Instead, I use the distance between the vote-share in favor and the required vote-share for the measures to pass as the running variable.

In Table 8, I show the results for the ITT effects on per-capita spending the year of the elections and the following six years. For the local linear RDs, I use the default specification with the triangular kernel and order one polynomial. In the first section of the table, I show the RD with no controls for district characteristics. In the second section, I switch to running the RD on the difference between each year and the year before the election. I subtract the per-student capital spending for the year before the election from the per-student capital spending of the same district during each of the later years. By taking the difference I am able to control for some unobserved district characteristics similar to a difference in differences. In the third section, I add covariates for the percentage of students that are classified as economically disadvantaged and the percentage needed for the bond measures to pass. I obtain fairly similar estimates across the three specifications for the year of the election and the next two years. The results are not as significant as in the parametric specification. This is to be expected as this is a more flexible approach and has less data points on which to estimate the time invariant district characteristics.

In Table 9, I run the same progression of RDs for test scores the year of the election with the exception that for the third section, I also add grade and subject fixed effects. I get very different results when not using the difference method. This is a cause for some concern. It could indicate that there are important differences between districts on either side of the threshold. The effect sizes this method finds three to six years after the elections are not reasonable. In the specifications where I subtract the score in the year before the elections, I find a sizable effect in the year of the election of about 13% that is largely inline with the findings of the parametric approach. In Figure 10, I graph the local linear regression for the year of the election with the covariates. This is the same as in the first column of the third section of Table 9.

4.5 Parametric TOT Specification

In order to find the TOT effects, I again follow the design laid out by Cellini et al. (Cellini, Ferreira, and Rothstein 2010). I assume that the effects are additive so that if a district had an election two years ago and an election five years ago, the total effect on the district from the elections would be the effect of being two years after an election plus the effect of being five years after. I also assume that previous elections effects on future elections depends only on the time between them. So for instance, I assume that having a surprisingly close election one year will not affect the size of the bond measure asked for in future elections.

I focus on a one-step dynamic RD very similar to that used in the study by Martorell, Stange, and McFarlin (Martorell, Stange, and McFarlin Jr 2016). This brings dynamics to an otherwise standard RD specification with a polynomial of the running variable by summing over the positive relative years. In other words, I have dummies for whether there was a bond measure in a district in every relative year, for instance one year after and 3 years after. I then also have a indicator variables for each relative year for whether the measure passed. These are the variables of interest. I also include in the summation a polynomial of the vote-share interacted with election threshold. This again is different than the Cellini et al., which just used a single polynomial and an indicator for one of the thresholds. I also include district, calendar year, grade, and test subject fixed effects.[5] Unlike the previous literature, I do not use the full panel for my main specification, as this could lead to my district fixed effects being less precise if districts are changing over time. Balance issues could also become larger when using the full panel. I do show the effects are robust to this choice. Instead, for my preferred specification, I run the one-step estimator on a data set that includes only observations within a window two years before to six years after each election but does not include duplicate observations.

[5]The grade and test subject fixed effects were not in the Cellini et al. or Hong and Zimmer papers as they only looked at two individual grades and test subjects.

This specification becomes:

$$Y_{jgts} = \sum_{\tau} [\beta_\tau Elect_{j\tau} + \omega_\tau Pass_{j\tau} + P_g(VoteShare_{j\tau} * FiftyFive_{j\tau}, \gamma_\tau) + P_g(VoteShare_{j\tau} * TwoThird_{j\tau}, \psi_\tau)] + \delta EconDis_{jt} + \alpha_t + \rho_g + \phi_j + \omega_s + \epsilon_{jgts} \quad (2)$$

where $Elect_{j\tau}$ is the indicator variable that an election was held in district j in relative year τ. $Pass_{j\tau}$ is the variable of interest and is an indicator that an election passed in district j in relative year τ. $Voteshare_{j\tau}$ is the percent of the vote in favor of a measure held in district j and in relative year τ. It is interacted respectively with $FiftyFive_{j\tau}$ and $TwoThird_{j\tau}$, which are dummies for the vote-share thresholds needed for different elections. $P_g()$ is a third power polynomial with coefficients γ and ψ. $EconDis_{jt}$ is the percent of students in district j during year t that are classified as economically disadvantaged. α_t, ρ_g, ω_s, and ϕ_j are year, grade, test subject, and district fixed effects. I cluster by school district. Like the ITT specification, I do not use a balanced panel of school districts, so some years may be missing for some elections.

The results for TOT effects on spending per student can be seen in Table 10 and Figure 8. I find large and very significant increases in per-capita capital spending the three years after a bond is passed, with the largest increases one and two years after the election. Almost all of this increase is in the form of spending on construction. This is largely similar to what was found in the previous literature in size of the difference in spending in any one year, but I find that the effect lasts longer. This could indicate that they are focused on even longer term projects. I do see a barely significant effect on instructional spending. This could indicate that there is some change in teacher composition or payment the year of the election, but this results is not robust to changes in the specification. That we do not see an effect on instructional spending in the years after the elections is consistent with the school districts using the fund raised from the bond measures to fund capital spending as the law requires.

The results of the TOT effects on test scores can be seen in Table 11 and Figure 9. This specification is a dynamic RD that includes a third power polynomial of the vote share, the threshold needed to pass each school bond measure, the percent of students that receive free or reduced price meals, and fixed effects for calendar year, grade, and school district.

Similar to the ITT effects, I find an immediate increase in test scores. The year that the bond measures passed, I find an increase of about 10% of a standard deviation rather than 13% that I found in the ITT specification. Theoretically, there should be no difference between the ITT effect and the TOT effect during the year of the election, so this change is likely being driven by using a polynomial of the vote-share instead of a local linear or using district fixed effects that cover more years than is possible when calculating the ITT. It is important to point out that the scores are normalized at the school district-test-grade level, not at the student level. This means that this 10% to 13% increase is quite sizable. This effect seems to be present mostly in middle and elementary school grades, though this could be driven by having more observations from those grades. The other papers that looked at the effect of test scores on standardized test grades did not use regression designs that allowed them to examine the effect that the election had the year it was on the ballot. I find that this effect appears to persist to the year after the election with an increase of 8%.

The other main result is that I find no evidence of a lasting increase in test scores that would be associated with this increase in capital spending. At the same time, I cannot rule them out. This is consistent with the Cellini, Ferreira, and Rothstein paper as well as with the Martorell, Stange, and McFarlin and Enami papers, but not with the Rauscher and Hong and Zimmer papers (Cellini, Ferreira, and Rothstein 2010; Martorell, Stange, and McFarlin Jr 2016; Enami 2017; Rauscher 2019; Hong and Zimmer 2016).

I look at proficiency rates as well. Table 12 shows the estimates of the ITT effects. I find marginally significant effects the year after the elections of about 1.4%. In Table 13, I show the estimates of the TOT effects. I find that there are significant, though small, effects the two years after the elections of between one and two percent. I also find slightly significant effects the two years after that. This could indicate that the quick positive effect from a school bond does last a few years. In Figure 12, I show the ITT effect one year the elections.

School districts that vary in income levels may increase spending in different ways leading to different results. Thus, I split up the districts into high and low districts in Table 14. I do this using districts that are on the top or bottom half based on the percent of their students designated as economically disadvantaged in the year before their election. Neither group shows long-run increases in test scores, and the immediate effects seem similar. This also differs with what Rauscher finds although she looked specifically at the test scores of high and low SES students rather than schools that had high and low percentages of low SES students (Rauscher 2019).

5 Robustness Checks and Placebo Tests

5.1 Robustness Checks

I perform several robustness checks for the TOT specifications. I try limiting my sample of districts in a couple of ways in Table 16. Almost all school districts hold their elections at the fifty-five percent threshold, which indicates that there is unlikely to be endogenous choice of the threshold. Even so, I look more closely at this in the first column by separating out the districts that had only elections with the 55% threshold. I use the 55% threshold as there were far more districts that only had elections with that threshold than districts that only had elections with a two-thirds threshold. When I do this, I still get large and significant effects the year of the election and the following year. I also get borderline significant effects two years after the election. This lines up well with the results on proficiency rates and could indicate that there is an effect that lasts for a number of years after the elections. In column 2, I loosen the assumption that the bonds are only effected by the other bonds based on how the length of time between them by removing districts that have elections within two years of each other. When I do this, the estimate for the effect the year of the elections remains similar to that found in the main TOT specification. We do lose the significance of the effect the year after the elections.

I next explore a balanced panel of districts. In order to maximize the number of elections that I in the regression, I use only schools that are present in the data for two years before to four years after the election. When looking at spending, I find similar sized increases in capital spending one and two years after the election. I also do not see the increase in instructional spending. These results are in Table 17.

The test score data only covers from 2003 to 2013, meaning that even if I look only two years before to four years after the elections, I can only use elections that took place between 2005 and 2009. This puts the time frame centered around the recession. Still, I do get results that are similar to my main specification. These results are shown in Table 18.

I compare my results in the main TOT specifications, in which I included only observations between two year before to six years after at least one election, to a specification where I use the full panel of data. I include the variables that are at the relative year level for all possible relative years after the elections. This means I have variables for relative years zero through twenty-one. The results for this comparison can be seen in Tables 19 and 20. They are broadly similar to what I found in the main TOT specification, though less significant.

In Table 21, I restrict myself to districts that fail at least one election. I do this as the density tests find no evidence of sorting for this sub-sample, even when I restrict myself to elections at the fifty-five percent threshold. It may be the case that by failing a bond measure these districts reveal that they are less capable of estimating the vote-share that they will receive or are less sophisticated in timing their elections. I find that my results are quite robust to this change.

In Table 25, I switch to using September first as the cutoff for assigning the elections to a school year. I then progressively undue some of the differences between my specification and that used by Martorell et al. In the first column, I show my results with only changing the cutoff. In the second column, I use only one polynomial instead of one for each election threshold. In the third column, I stop using the year of the election, so that that year will just go into the fixed effects. I also drop the control for the percentage of students classified as economically disadvantaged. I find that my results are not sensitive to using a September cutoff or to only using one polynomial. When I drop the variables in column three, my results become more similar to the null results found in Martorell et al and Celini et al. This is likely because of the change in putting the year zero effects into the district fixed effects.

In Tables 26 and 27, I show the TOT results when I vary the fixed effects. I find that the effects on per-student capital spending are fairly consistent across the changes in fixed effects, though including district fixed effects does increase the effects. The effect on normalized test scores are dependent on including district fixed effects. This lines up with my finding that the effects only appear when using the difference in the non-parametric ITT specification. This is somewhat worrisome, as it could indicate that there is some difference between districts across the threshold even though I see no changes in the covariates at the threshold.

5.2 Placebo Tests

I try including the relative years before a bond is passed in a regression. These years act as placebo tests where we expect not to find effects. As we would expect, I find no effects during the years before an election for spending. These results are relative to the year before the election. The results can be seen in Table 22. In Table 23, I look at the placebo years on normalized test scores. In the first column, I run the same specification as I did on spending, with the exception of grade and test subject fixed effects, and see no effects in both the pre-years and the years following the elections. In the second column, I show that I still find some effect on proficiency rates. In table 24, I test whether there is an effect either two or three years before the elections in a placebo version of the ITT effects with the difference between the observations three years before the elections minus the scores the year before the elections. I do not find an effect.

6 Mechanisms

The increase in test scores starts the year of the election, which is before the district should have access to the money from the bonds, and most of the increase in capital spending is spent on construction, which may take some time to complete. It is possible that there are capital improvements made that are both quick and persist for a number of years. This could include fixing air conditioning or making other relatively inexpensive and quick improvements that could have sizable effects. I also do find an increase in instruction spending the year of the election. This does not persist, and so is probably not a mechanism for the effect we see in later years. There could also be a change in student composition after an election. The speed of the effects lead me to suspect a possible morale effect. As mentioned in the introduction, there is at least anecdotal evidence that school officials expect a morale effect.

In an attempt to look at the effect on morale directly, I obtained access to private data from the Institute for Education Sciences School and Staffing Surveys (SASS). SASS was conducted seven times between 1987 and 2011. It is a national sample of schools, teachers, and principles that asked about many topics including staff satisfaction, number of computers, and quality of facilities. Because of the national sample design and because of inconsistencies between years about what questions were asked, I was not able to run a causal analysis on this data using an RD methodology. I do however show that there are strong correlations between teacher satisfaction and computer availability and facility quality. In Table 28, I regress whether public school teachers report that they have adequate materials on the number of computers per student, whether they report a lack of space, and whether they are generally satisfied. Both whether they agree that they have adequate materials are answered on a scale of one to four with one being strongly agree and four being strongly disagree. Whether they lack space is answered as either one for yes or two for no. I find strong correlations between having more computers per student and reporting not having a lack of space with reporting having adequate materials. This indicates that teachers likely have an accurate idea of the capital resources that districts have. I also find a strong correlation between agreeing that they have adequate materials and reporting that they are generally satisfied. This suggests that teachers both are aware of the outcomes of capital spending and care about not having enough. This supports the possibility that teachers could have a morale effect if they anticipate future capital spending because of a bond measure passing.

I attempt to find some supporting evidence for it being a morale effect by looking at staff retention. Surprisingly, it is not clear from the literature that teacher morale and teacher retention are positively linked (Cohen et al. 2009; Kukla-Acevedo 2009; Liu and Meyer 2005). In fact, some studies find that teachers that are more likely to leave also report being more satisfied. This could be because they are more detached from issues at their schools. It is also possible that passing school bond measures could lead to construction hassles, which could affect staff retention. Finally, teacher retention could in itself be a mechanism for the effect on test scores.

My teacher retention data comes from the California Department of Education Staff Demographic files from the 1997-1998 school year to the 2011-2012 school year. The files are not linked across years, but they do include data for each staff member on what school district they work at, whether they are a teacher, an administrator, or a pupil facing staff member, and the years they have worked at the district and the years they have been in education service. Because of the inadequacies of the data set, I can only form an imperfect measure of turnover. I build my measure by subtracting the number of staff members who are not in their first year at the district from the number of staff members from the year before and then dividing by the number who were there the previous year. This should give the percentage of staff members who were there the previous year who are still there during the current year.

There are several concerns with this approach. The first is that I cannot distinguish between those who retire, let go, or quit. Also, from 2009 to 2011, 30% of districts in California offered financial incentives to retire early (Williams 2015). Research by PPIC shows that many teachers quit for a few years and then come back (Reed, Rueben, and Barbour 2006). These teachers may show up in my measure as negative turnover. There are some possible problems with the data as I get several negative numbers. This could be, at least partially, from people coming back to the districts after a leave of absence. These numbers seem to be concentrated in 2011, so I look only at the years predating then.

In Table 29, I show the result of running my main specification on this measure of staff turnover. I find a positive effect on staff turnover the year of the election and the year after of about 3% that seems to be concentrated in teachers. This could be because construction creates disruptions that encourage some teachers to leave. In Table 30, I evaluate turnover by whether teacher have less than or more than or equal to fifteen years of experience. I find that the effect on turnover is only present in the more experienced teachers. This means that the effect is could largely be driven by teachers retiring. Looking at staff characteristics largely supports this finding. In Table 31, I examine the effect on the percent of staff with graduate degrees, the percent of staff with tenure, the percent of staff in their first year, the average number of years in the district, and the average number of years teaching. While most of these results are insignificant, the consistent signs suggest that there may be a move towards more first year teachers and fewer tenured teachers.

It is unclear from the literature that encouraging teachers to retire would affect test scores. Multiple studies link teacher early retirement incentives to increases in student achievement (Fitzpatrick and Lovenheim 2014; Williams 2015). On the other hand, a study by Mahler finds that effective and ineffective teachers retire at similar rates when they become eligible for pensions (Mahler 2018).

Next, I test whether compositional changes in the student body could be driving the effect on test scores. First, I look at whether there is a change in enrollment that could point to students moving to the district from other districts or private schools because of the elections. Next, I look at the percentage of the student bodies that are designated as economically disadvantaged that would indicate more affluent students moving to the districts' schools. The results from these regressions can be seen in Table 32. I do not find any evidence that compositional changes in the student body account for the change in test scores the year of the election. There is an increase in the percentage of economically disadvantaged students two years after the election. I cannot tell if this is caused by a change in student composition or more students signing up for the program. This increase could explain part of why I don't see a long-run change in test scores caused by the increase in capital spending, but it does not explain the positive effect that we see in the first years.

Until I can obtain better data, the mechanism to explain the effects that I observe is hard to pin down. While capital spending is important to teachers, we see an increase in teacher turnover following the elections. This could be part of the effect if it is ineffective teachers that are leaving, but does not provide good evidence of an effect on teacher morale. There is an increase in the percentage of students classified as economically disadvantaged, but we can not tell how this is affecting the results. This is partly because we not be certain if the school composition is changing or if there is greater take up of free and reduced price meals. I am in the process of applying for the California School Climate, Health, and Learning Surveys, which may allow me to better answer these questions.

7 Conclusion

The passage of school bond measures results in large increases in capital spending for a number of years after the elections. The treatment on the treated effects on spending found in this study align remarkably well with the other studies in this literature. For each of the first two years after the elections, bond measures lead to an increase in capital spending between two and three thousand dollars with smaller increases in following years. The large majority of this spending is categorized as construction with a much smaller effect on the purchase of equipment three to four years after an election. I find no change in instructional spending outside the year of the election, and that effect is not robust to changes in the specification. Funds raised by school bond measures are required to be spent on capital, and the school districts appear to follow this rule. This is not surprising as the majority of school bond measures, all those that pass at the fifty-five percent threshold, require outside auditing.

I find a large and immediate impact on student achievement on standardized tests after the passage of school bond measures. This can be seen in both the ITT and TOT measures and in both normalized mean test scores and in proficiency rates. These results differ from what other papers using this approach have found. Interestingly, these effects began the year of the election.

Further research is needed to explore the mechanism of the effects on student achievement. The speed of the effect and that it takes place before the large majority of the spending suggests the possibility of a morale effect, in which teachers or parents are more motivated, resulting in better instruction. Anecdotal evidence quoted in the introduction does show that teachers and school officials believe that passing and failing school bond measures can have a large influence on teacher morale. Using data from the School and Staffing Surveys, I show that there is a strong correlation between equipment and facility quality and teacher morale. I find, however, an increase in teacher turnover following the passage of school bond measures. It is not clear from the literature how well teacher turnover is linked to teacher morale. The effect on teacher turnover could be because some teachers do not want to deal with the inconveniences of construction or because teachers band together after a negative morale shock when a bond fails. I find no evidence of the effects being driven by a change in student composition. It is possible that some of the effect is driven by the purchasing of equipment, which is inexpensive and has a large, quick effect. I would expect that if this were the case I would see a larger effect on spending the year of the election. I hope to further explore the possibility of a morale effect when I can obtain data from the California School Climate, Health, and Learning Surveys. I also hope to test specifications that allow me to see whether the results are driven by positive effects on the districts that pass school bonds or negative effects on the districts where they fail. I will do this using a difference-in-differences approach.

While I do not find evidence of long-run gains to student achievement following bond measures, I also cannot rule them out. The effects are not statistically significantly different from the effects I find in the earlier years. More power is needed to look at this question.

This study leaves several other open questions that would benefit from additional research. I plan to test whether the effects vary with the size of the bond measures or the amount of turnout. I believe that much more work can be done to see how districts respond to the results of previous bond measure elections. For instance, I will test whether districts shrink the size of the bond they ask for after failing to pass their last one, or whether they change the timing of their elections in order to get a more favorable portion of the electorate. It would be insightful to see if the bonds have different effects on different types of students, such as English learners and those in special education. Rauscher is looking at differing effects by SES in her working paper, though I find no different effects when splitting districts by the percentage of their students classified as economically disadvantaged (Rauscher 2019). Another issue that I hope to investigate is how well voters understand that the bonds will need to be paid back through property taxes and to look at how the districts vary in terms of debt. Finally, I think it would be insightful to see how the timing of bond measures relates to when the districts hold elections on property taxes.

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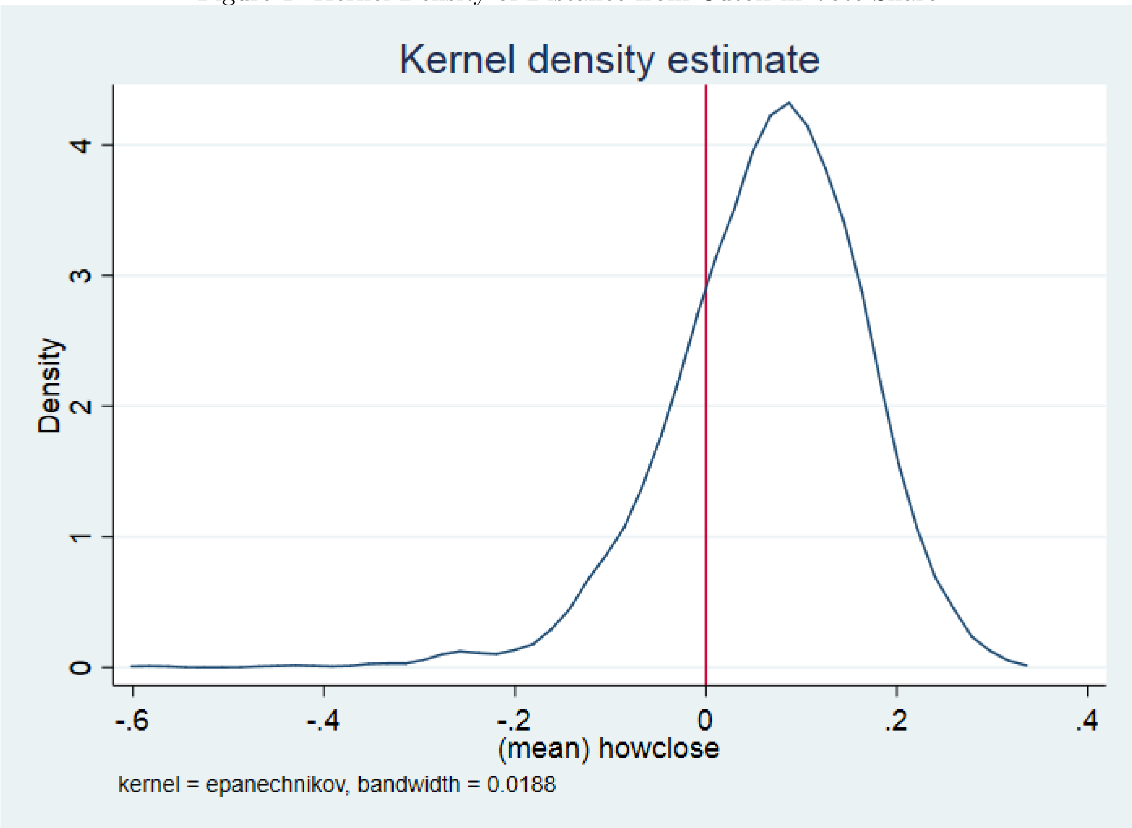
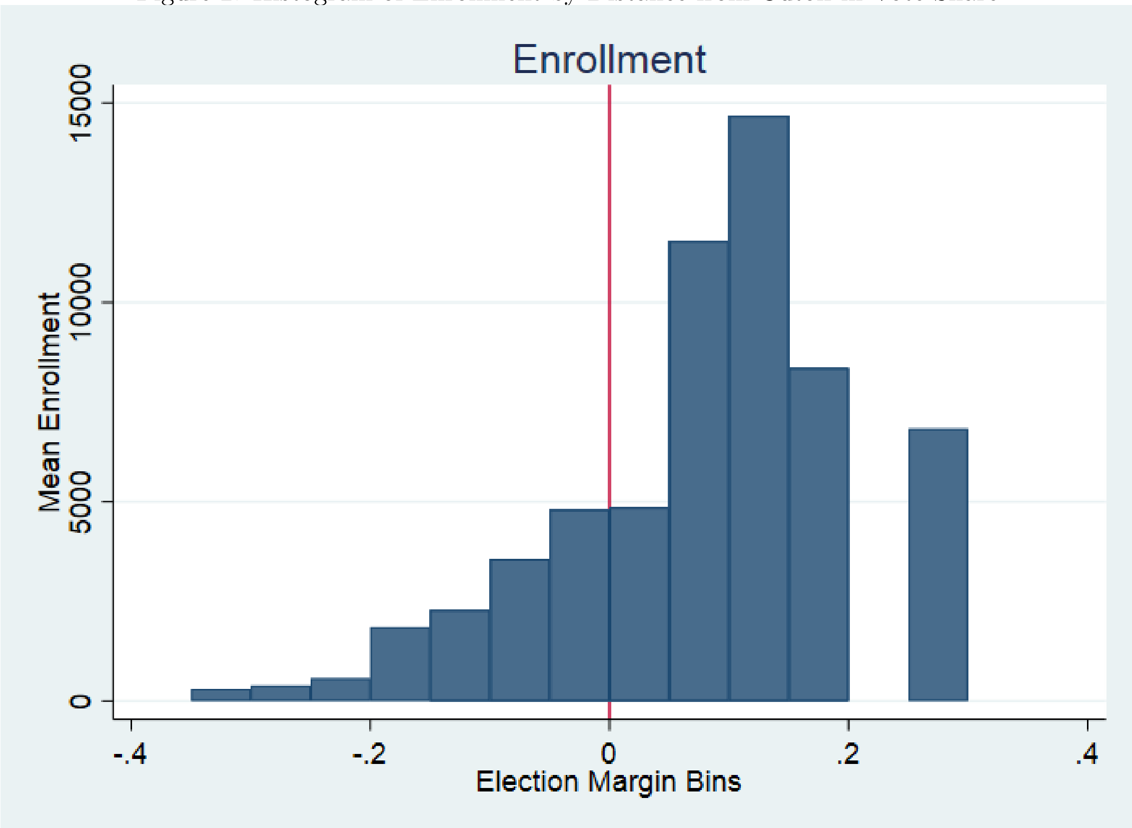
Figure 1: Kernel Density of Distance from Cutoff in Vote Share



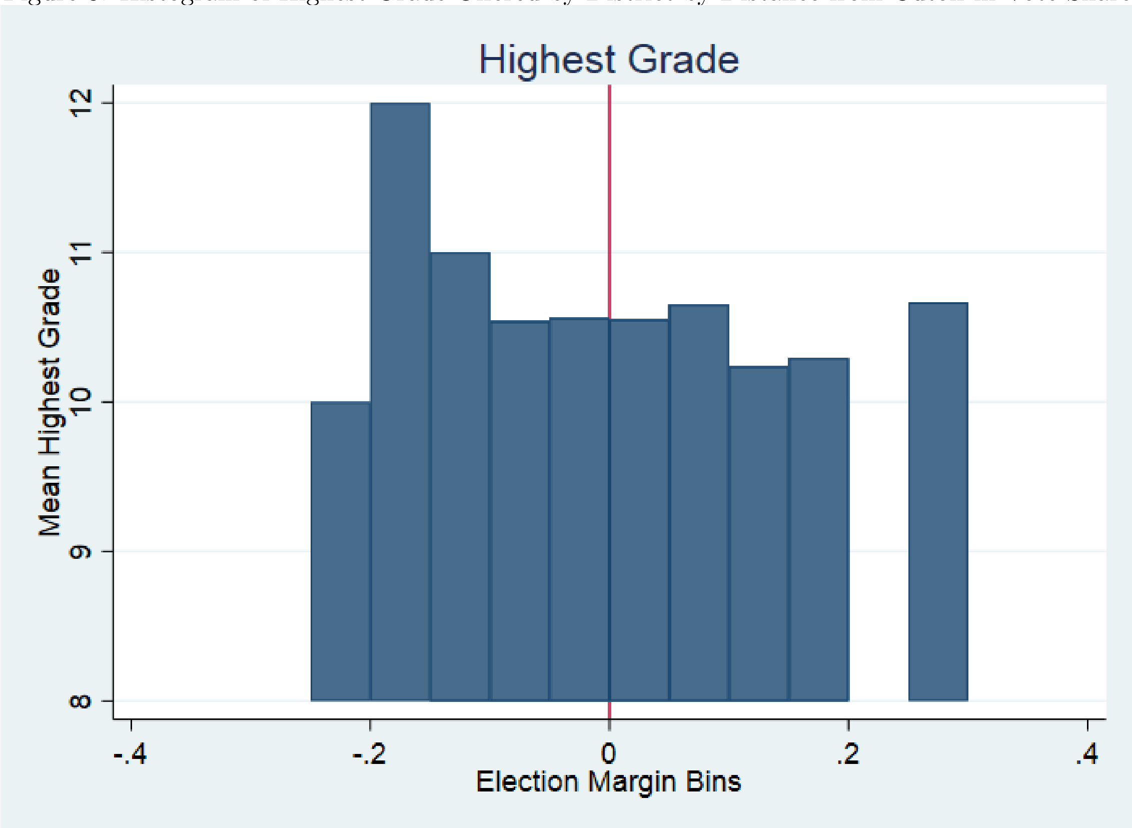
Notes: Figure shows the kernel density of the difference between the vote share and the threshold needed for the measures to pass. The vertical line represents the vote share necessary to pass the measures. The election data is from the California Election Data Archive.

Figure 2: Histogram of Enrollment by Distance from Cutoff in Vote Share



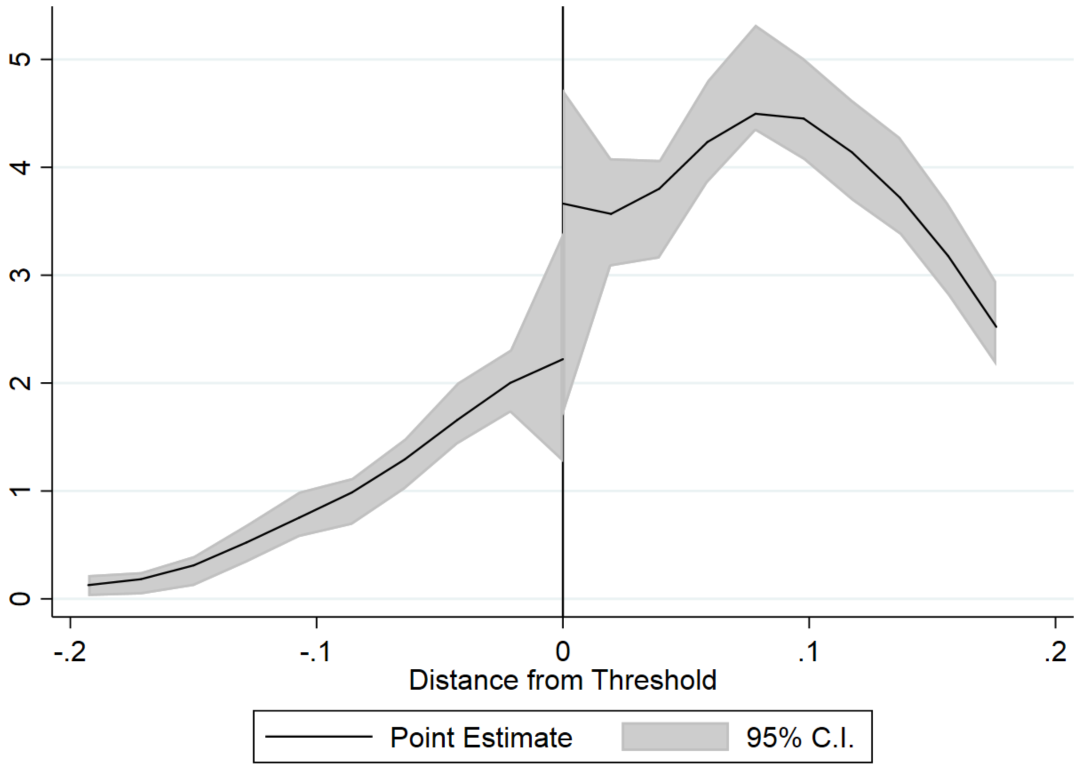
Notes: Figure shows the average enrollment in bins by distance of the vote share from the threshold needed to pass. Enrollment data is from the California STAR Test Data. Election data is from the California Election Data Archive.

Figure 3: Histogram of Highest Grade Offered by District by Distance from Cutoff in Vote Share



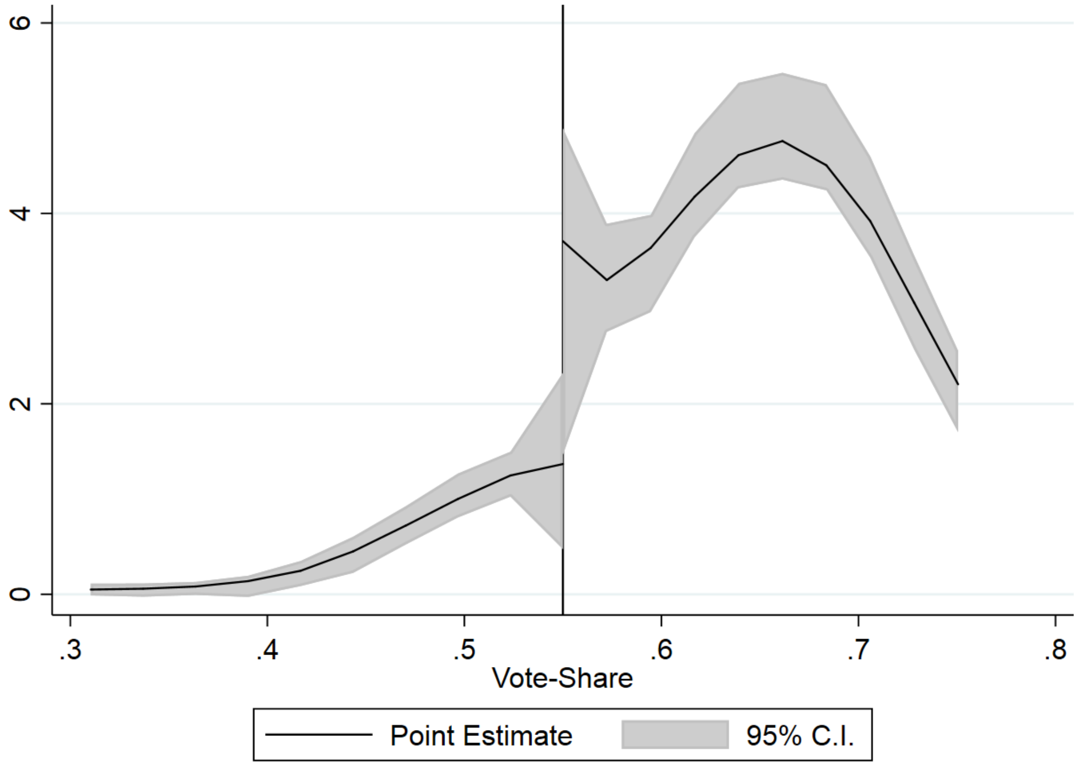
Notes: Figure shows the average highest grade offered in each district in bins by distance of the vote share from the threshold needed to pass. Grades are assigned there numeric value so that if a district offers twelfth grade then this measure will be 12. Kindergarten and pre-k are assigned the values -1 and -2 respectively. This measure appears to be smooth through the cutoff.

Figure 4: Density Test of Endogenous Sorting



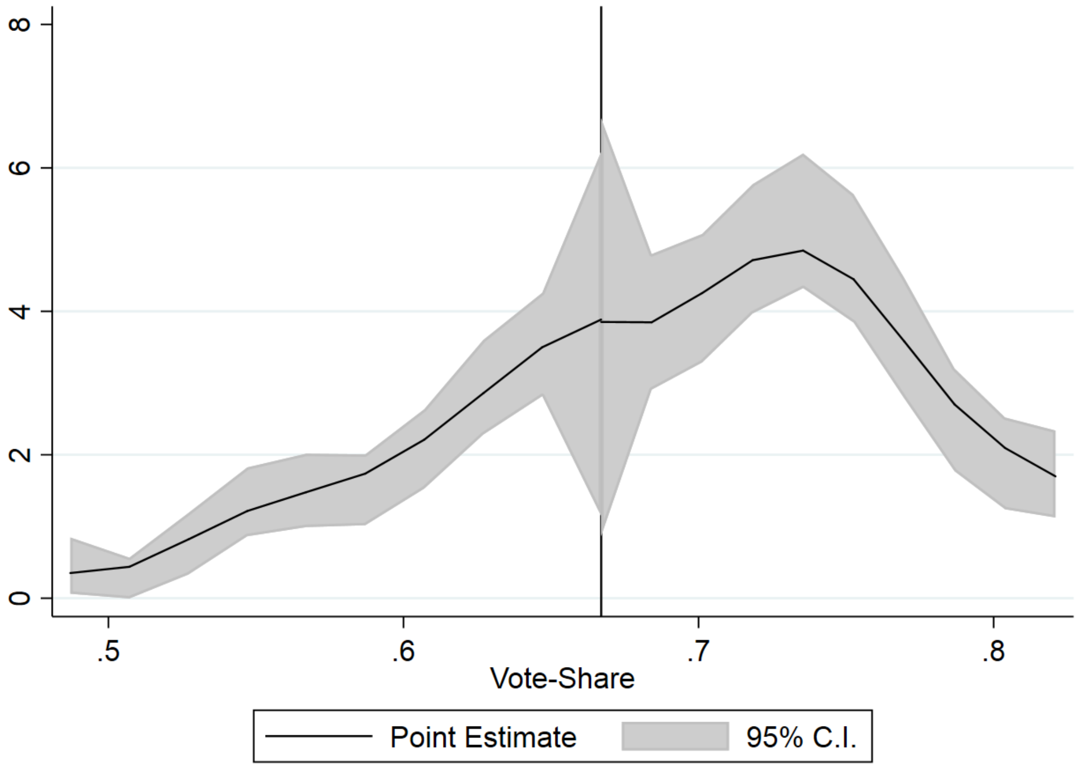
Notes: Here I perform a density test of the distance from the threshold. Data comes from the California Election Data Archive.

Figure 5: Density Test of Endogenous Sorting: 55% Threshold Elections



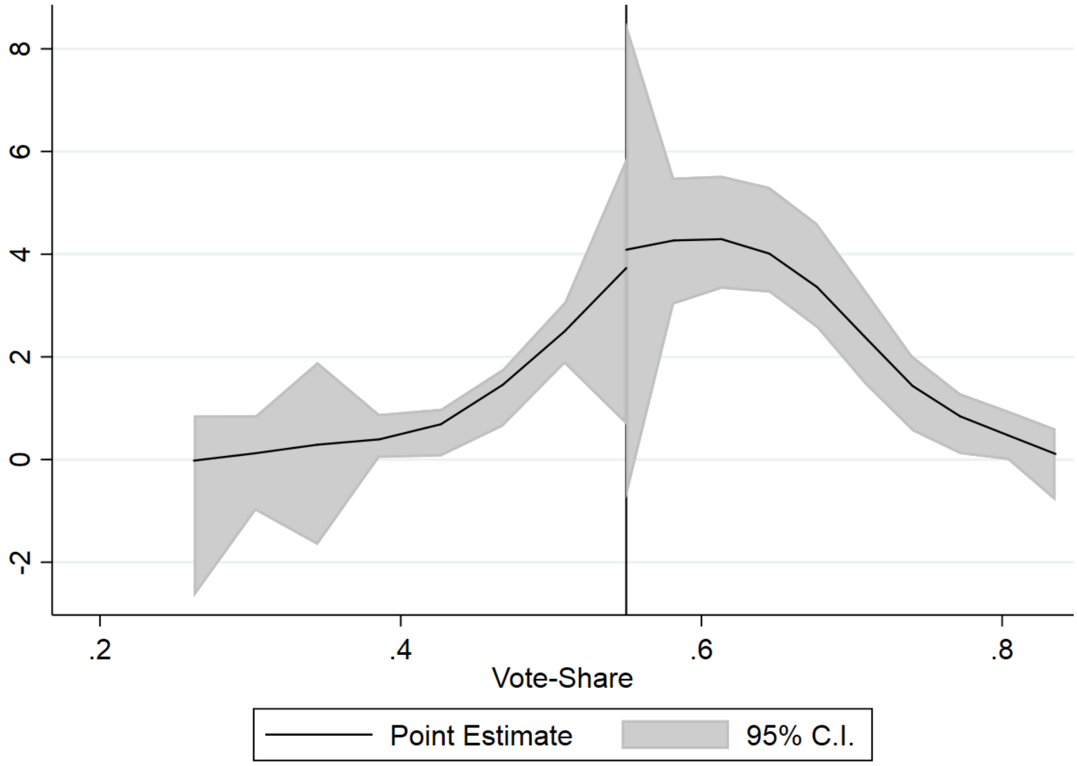
Notes: Here I perform a density test of the distance from the threshold for only elections with a 55% Threshold. Data comes from the California Election Data Archive.

Figure 6: Density Test of Endogenous Sorting: Two-Thirds Threshold Elections



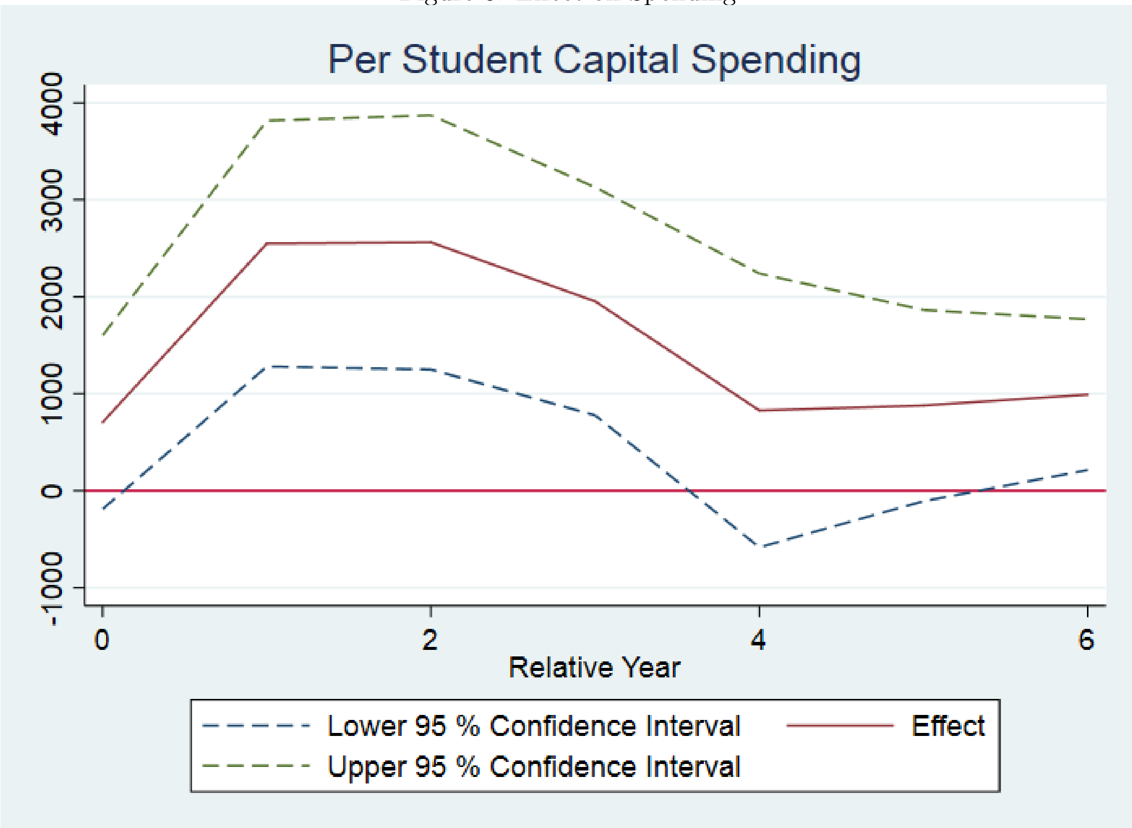
Notes: Here I perform a density test of the distance from the threshold for only elections with a two thirds threshold. Data comes from the California Election Data Archive.

Figure 7: Density Test for 55% Threshold Elections for Districts that Failed Some Elections



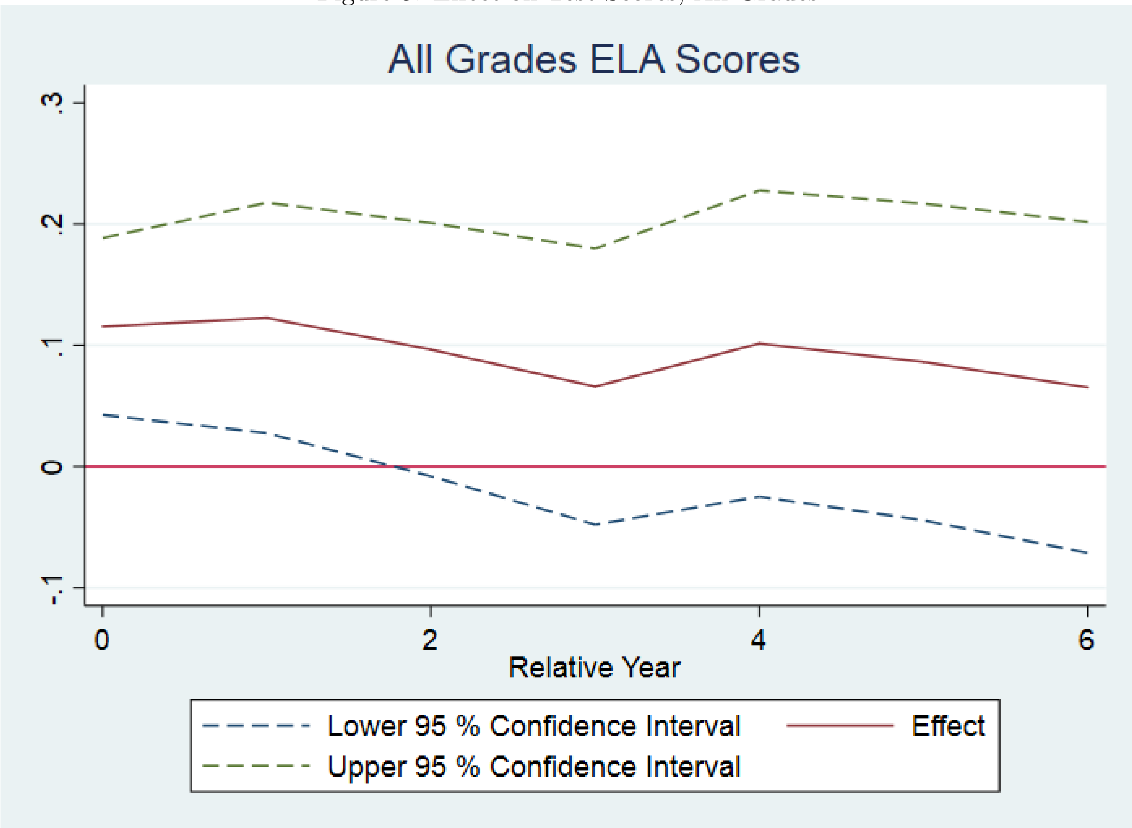
Notes: Here I perform a density test of the distance from the threshold for only elections with a fifty-five percent threshold in districts that failed to pass at least one election. Data comes from the California Election Data Archive.

Figure 8: Effect on Spending



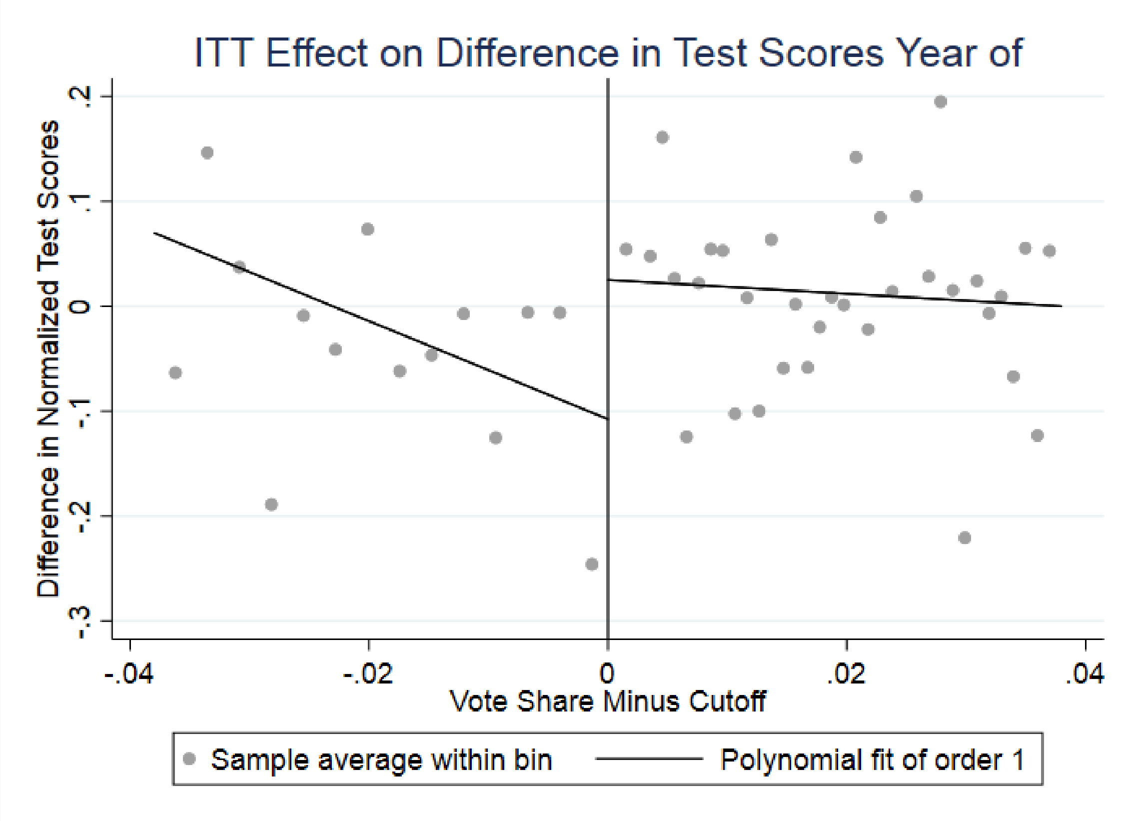
Notes: Figure shows results from the main specification for the effect on capital spending per student. I connect the pointwise estimates and 95% confidence intervals. This specification uses a dynamic regression discontinuity design. Regression are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, the required vote share for each measure to pass, and a third level polynomial of the vote share as well as fixed effects for calendar year and school district.

Figure 9: Effect on Test Scores, All Grades



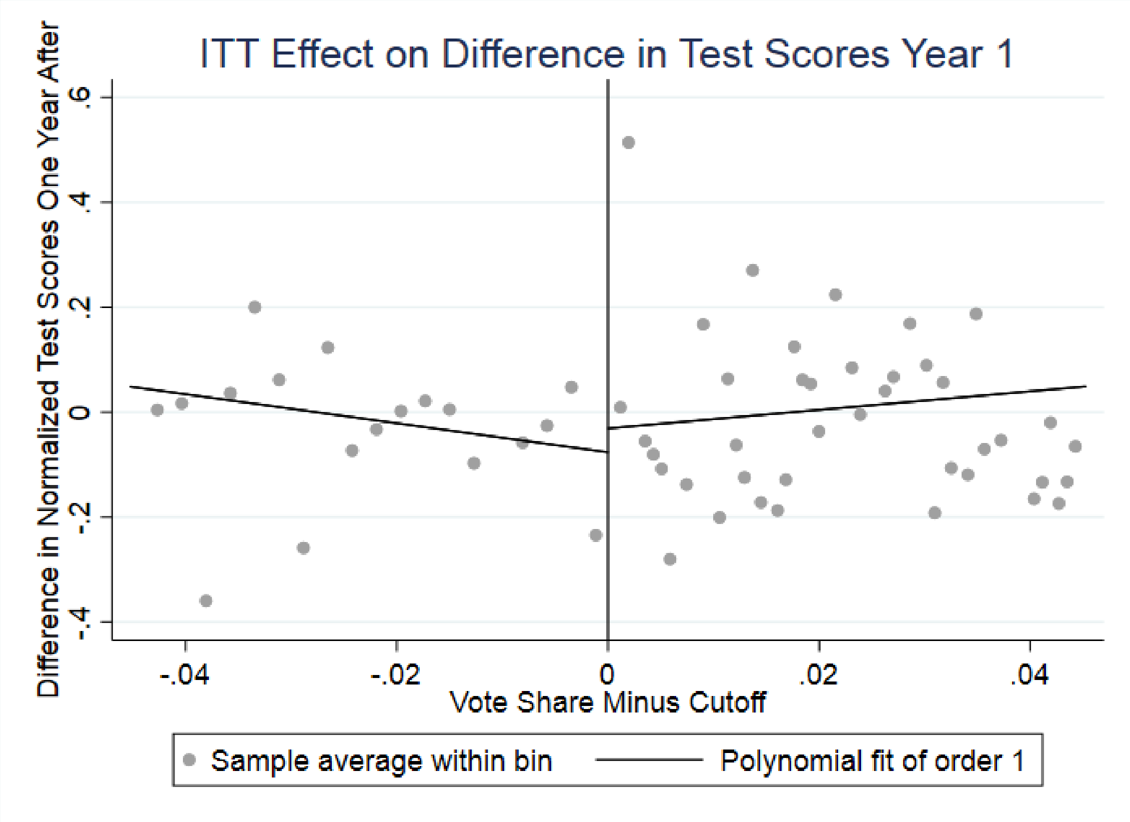
Notes:
Figure shows results from the main specification for the effect of passing school bond measures on student standardized test scores. I connect the pointwise estimates and 95% confidence intervals. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test. This specification uses a dynamic regression discontinuity design. Regression are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, the required vote share for each measure to pass, and a third level polynomial of the vote share. Test scores are normalized by grade, test subject, and school year.

Figure 10: ITT Effect on Difference in Normalized Test Scores Year of Election



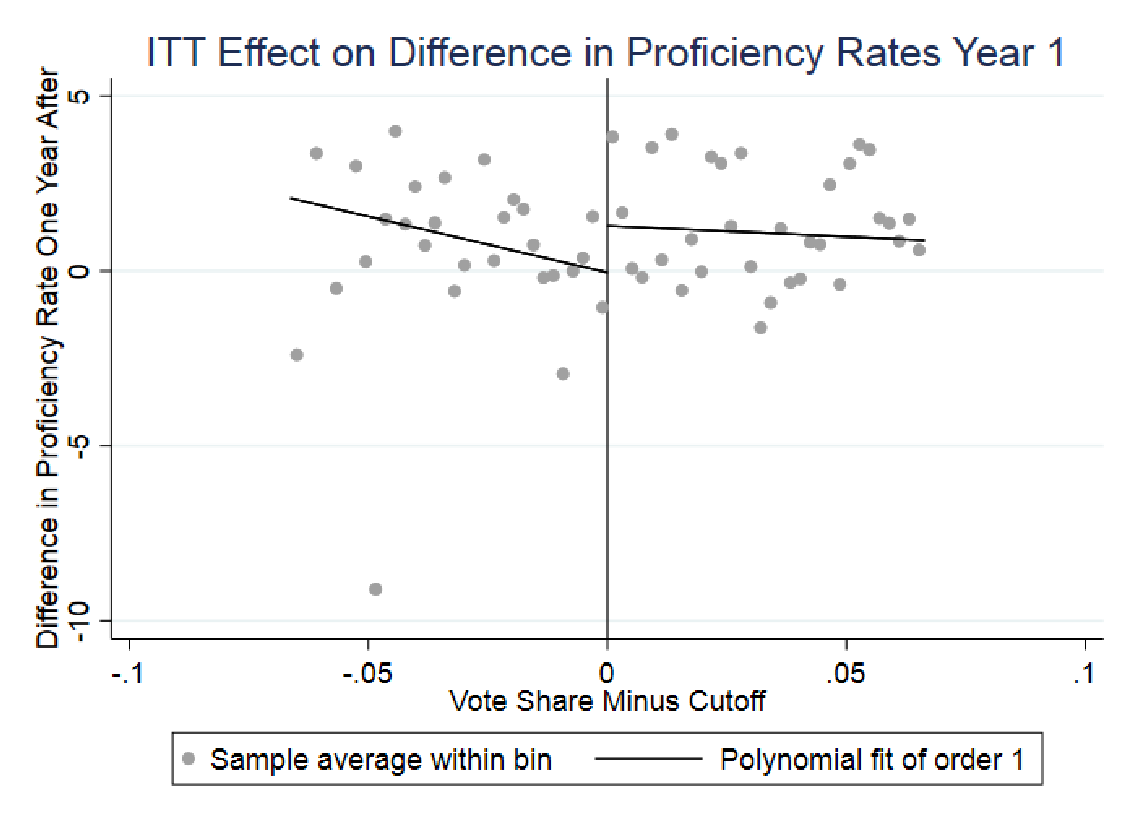
Notes: This figure shows the effect on test scores using my ITT specification. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test. I use a RD style, differences in differences. For each test score during the year of the election, I subtract the test score of the same district, grade, and subject the year before the election. I then run a local polynomial RD of that difference using the method described by Calonico, Cattaneo, Farrell, and Titiuni (Calonico et al. 2017). I use the distance between the vote-share and the cutoff for each election as the running variable. I still include controls for grade, election cutoff, percent of economically disadvantaged students, and test subject.

Figure 11: ITT Effect on Difference in Normalized Test Scores Year After Election



Notes: This figure shows the effect on test scores using my ITT specification. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test. I use a RD style, differences in differences. For each test score during the year of the election, I subtract the test score of the same district, grade, and subject the year before the election. I then run a local polynomial RD of that difference using the method described by Calonico, Cattaneo, Farrell, and Titiuni (Calonico et al. 2017). I use the distance between the vote-share and the cutoff for each election as the running variable. I still include controls for grade, election cutoff, percent of economically disadvantaged students, and test subject.

Figure 12: ITT Effect on Difference in Proficiency Rate Year After Election



Notes: This figure shows the effect on test scores using my ITT specification. The proficiency rate data come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test. I use a RD style, differences in differences. For each test score during the year of the election, I subtract the proficiency rate of the same district, grade, and subject the year before the election. I then run a local polynomial RD of that difference using the method described by Calonico, Cattaneo, Farrell, and Titiuni (Calonico et al. 2017). I use the distance between the vote-share and the cutoff for each election as the running variable. I still include controls for grade, election cutoff, percent of economically disadvantaged students, and test subject.

Table 1: Number of Bond Measure Elections by Month

January	1
February	32
March	208
April	52
May	24
June	320
July	1
August	0
September	16
October	2
November	909
December	4

Notes: Table shows the number of school bond elections that took place in each month during the period from 1995 to 2016. Election data is from the the California Election Data Archive.

Table 2: Number of Districts With Each Number of Bond Measure Elections

Number of Elections	Number of Districts
1	228
2	266
3	154
4	59
5	17
6	3
7	0
8	1

Notes: Figure shows the number of school districts that had 1, 2, 3, 4, 5 , 6, 7, or 8 elections for school bond measures during the period from 1995 to 2016. Election data is from the the California Election Data Archive.

Table 3: Summary of Bond Proposition Elections

Year	Number of Elections	Pass Rate	Percentage Requiring 55%	Percentage Requiring Two Thirds of Vote	Mean Distance From Cutoff	SD of Distance From Cutoff
1995	22	.318	0	1	-.028	.095
1996	57	.614	0	1	.001	.129
1997	55	.709	0	1	.034	.103
1998	108	.648	0	1	.022	.101
1999	77	.714	0	1	.042	.099
2000	91	.626	0	1	.028	.103
2001	58	.69	.017	.983	.03	.078
2002	91	.857	.714	.286	.074	.101
2003	86	.802	.814	.186	.078	.091
2004	61	.787	.902	.098	.071	.082
2005	55	.873	.909	.091	.121	.087
2006	32	.875	.813	.188	.069	.062
2007	98	.776	.949	.051	.068	.078
2008	43	.744	.884	.116	.066	.085
2009	105	.905	.943	.057	.094	.086
2010	3	.667	.667	.333	-.041	.249
2011	74	.784	.973	.027	.062	.11
2012	8	.875	.875	.125	.064	.074
2013	122	.861	.992	.008	.088	.089
2014	10	.7	.8	.2	.084	.126
2015	127	.835	.969	.031	.073	.087
2016	186	.946	.925	.075	.118	.088

Notes: Table shows summary statistics for the elections for school bond measures held in California between 1995 and 2016. The years are based on school years ending in in the year listed. The cutoff used for the school years in July 1st. Election data is from the the California Election Data Archive.

Table 4: Summary Statistics of Districts

Characteristic	Districts That Voted on No Measures	Districts That Did Not Pass All Measures	Districts That Passed All Measures
Enrollment	669.8041	7674.962	5604.681
Normalized ELA Score	-.3665449	.1121271	.1680753
Normalized Math Score Grades 2-6	-.3907894	.0298871	.1097117
Revenue Per Student	42217.29	13899.13	14470.29
Total Expenditure Per Student	40733.19	14103.64	14618.33
Total Capital Spending Per Student	2116.817	1971.84	1830.243
Percentage Economically Disadvantaged	.5948393	.4900185	.4974371
Number of Districts	441	233	495

Notes: Table shows summary statistics for school districts when they have been divided into categories based on whether they voted on no school bond measures, whether they failed to pass at least one measure, and whether they voted on one or more measure and passed all of them. Data comes from the California Star Test, the National Center for Education Statistics Common Core of Data, and the California Election Data Archive.

Table 5: Local Linear RD of Covariates

VARIABLES	Economically Disadvantaged	Enrollment	Capital Spending	Total Revenue	Number of Voters
RD_Estimate	0.0157	2.571	4.716e+06	1.907e+07	-10,126
Observations	595	606	966	966	1493
Robust p-value	0.991	0.887	0.149	0.530	0.425
BW Loc. Poly.	0.0558	0.0433	0.0395	0.0434	0.105
BW Bias	0.0916	0.0710	0.0702	0.0695	0.143

Notes: This table shows the results of running a local linear RD on covariates. Each column is a separate regression. The first four regressions were performed on district characteristics and were done the year before the election. The first column is the percentage of the student body classified as economically disadvantaged. The second is enrollment. The third is total capital spending. The fourth is total revenue. The last column in on the number of voters during the election. This uses a window of two years before to six years after each election. Variables include whether a bond passed in each relative year, a third level polynomial of the vote-share for each election threshold, and a control for the percentage of students classified as economically disadvantaged. Fixed effects are included for election window, relative year, and calendar year. Results are clustered at the district level. Election data comes from the California Election Data Archive. Spending data comes from the NCES Common Core of Data. Enrollment and economically disadvantaged data comes from the California STAR Test Data.

Table 6: ITT Effects on Spending: Parametric RD

	Capital Spending	Construction	Land and Existing	All Equipment	Instruction
Year of Election	442.5	407.9	27.0	7.63	733.8**
	(408.0)	(388.1)	(91.6)	(19.8)	(344.5)
1 Year Post	1994.9***	2061.6***	-60.5	-6.26	-48.6
	(528.4)	(501.1)	(143.1)	(22.1)	(284.9)
2 Years Post	1587.2***	1555.6***	33.8	-2.20	-22.1
	(555.3)	(546.7)	(101.4)	(16.4)	(292.3)
3 Years Post	642.5	594.7	-14.5	62.2***	-132.2
	(486.5)	(428.8)	(159.0)	(18.1)	(222.7)
4 Years Post	-677.0	-674.8	-9.44	7.27	116.3
	(600.0)	(584.2)	(140.8)	(13.1)	(302.6)
5 Years Post	-573.4	-386.1	-200.3	13.0	-274.0
	(408.0)	(379.5)	(150.6)	(26.9)	(199.2)
6 Years Post	-140.5	-261.4	150.0**	-29.0	-342.6*
	(331.3)	(324.6)	(72.4)	(19.8)	(199.6)
Observations	7552	7552	7552	7552	7552
R-Squared	0.37	0.35	0.56	0.31	0.86

Notes: This table shows the ITT effects on spending estimated using the parametric model. This uses a window of two years before to six years after each election. Variables include whether a bond passed in each relative year, a third level polynomial of the vote-share for each election threshold, and a control for the percentage of students classified as economically disadvantaged. Fixed effects are included for election window, relative year, and calendar year. Results are clustered at the district level. Election data comes from the California Election Data Archive. Spending data comes from the NCES Common Core of Data.

Table 7: ITT Effects on Normalized Test Scores: Parametric RD

	All Grades	High School	Middle School	Elementary
Year of Election	0.11***	-0.020	0.066	0.15***
	(0.033)	(0.050)	(0.051)	(0.045)
1 Year Post	0.11**	-0.085	0.11*	0.14**
	(0.045)	(0.069)	(0.060)	(0.061)
2 Years Post	0.093*	-0.037	0.10	0.11
	(0.052)	(0.071)	(0.072)	(0.070)
3 Years Post	0.083	-0.087	0.11	0.098
	(0.056)	(0.077)	(0.072)	(0.073)
4 Years Post	0.086	-0.038	0.088	0.098
	(0.063)	(0.085)	(0.077)	(0.080)
5 Years Post	0.085	-0.061	0.059	0.12
	(0.064)	(0.087)	(0.080)	(0.079)
6 Years Post	0.074	-0.13	0.066	0.11
	(0.065)	(0.088)	(0.084)	(0.081)
Observations	91476	13901	25717	51858
R-Squared	0.84	0.92	0.89	0.85

Notes: This table shows the ITT effects on normalized test scores estimated using the parametric model. This uses a window of two years before to six years after each election. Variables include whether a bond passed in each relative year, a third level polynomial of the vote-share for each election threshold, and a control for the percentage of students classified as economically disadvantaged. Fixed effects are included for election window, relative year, and calendar year. Results are clustered at the district level. Election data comes from the California Election Data Archive. Test scores data comes from the California STAR tests.

Table 8: ITT Effects on Per Student Capital Spending: Local Linear RD

VARIABLES	Year of the Election	1 Year Later	2 Years Later	3 Years Later	4 Years Later	5 Years Later	6 Years Later
RD_Estimate	1,135	2,368	2,169	840.8	-106.1	-1,175	-220.2
Observations	677	646	692	711	782	777	786
Robust p-value	0.0307	0.00722	0.0267	0.418	0.892	0.0938	0.533
BW Loc. Poly.	0.0508	0.0622	0.0655	0.0822	0.0629	0.0356	0.0412
BW Bias	0.0903	0.110	0.0998	0.151	0.105	0.0660	0.0955
Local Linear RD of Difference in Per Student Capital Spending							
RD_Estimate	1,211	3,756	3,029	2,983	729.5	-481.9	-2,478
Observations	396	322	314	274	270	199	169
Robust p-value	0.252	0.0362	0.232	0.426	0.805	0.841	0.570
BW Loc. Poly.	0.0456	0.0623	0.0588	0.0400	0.0303	0.0269	0.0225
BW Bias	0.0732	0.0916	0.0896	0.0828	0.0641	0.0459	0.0385
Local Linear RD of Difference in Per Student Capital Spending With Covariates							
RD_Estimate	1,141	3,656	2,331	1,106	2,159	547.6	-128.2
Observations	385	312	304	265	261	193	163
Robust p-value	0.258	0.0473	0.350	0.774	0.385	0.987	0.638
BW Loc. Poly.	0.0457	0.0603	0.0483	0.0433	0.0315	0.0303	0.0357
BW Bias	0.0756	0.0912	0.0774	0.0671	0.0663	0.0524	0.0665

Notes: The first section of this table shows the effect on per-student capital spending using a simple local linear RD of the normalized test scores. The second and third sections of this table show the effect on test scores using a local linear RD of the change in normalized test scores since the year before the elections. The third section also includes covariates for the election vote-share threshold and percentage of economically disadvantaged students. All sections use a triangular kernal with a mserd bandwidth type and second order bias. I use the method described by Calonico, Cattaneo, Farrell, and Titiuni (Calonico et al. 2017). I use the distance between the vote-share and the cutoff for each election as the running variable. Election data comes from the California Election Data Archive. Spending data comes from the NCES Common Core of Data.

Table 9: ITT Effect on Normalized Test Scores: Local Linear RD

VARIABLES	Year of the Election	1 Year Later	2 Year Later	3 Year Later	4 Year Later	5 Year Later	6 Year Later
RD_Estimate	0.00778	0.00713	-0.0470	0.230	0.206	0.0942	0.187
Observations	8186	7787	8344	8429	9263	9283	9346
Robust p-value	0.730	0.594	0.252	1.49e-06	3.20e-06	0.0459	0.000127
BW Loc. Poly.	0.0327	0.0331	0.0344	0.0321	0.0398	0.0440	0.0376
BW Bias	0.0553	0.0810	0.0644	0.0590	0.0698	0.0754	0.0608
Local Linear RD of Difference in Normalized Test Scores							
RD_Estimate	0.134	0.0468	0.0352	0.0540	0.100	0.107	0.213
Observations	7095	5611	5473	4590	4538	3323	2769
Robust p-value	0.0104	0.290	0.497	0.295	0.166	0.320	0.147
BW Loc. Poly.	0.0377	0.0486	0.0724	0.0330	0.0302	0.0273	0.0220
BW Bias	0.0760	0.0893	0.125	0.0740	0.0595	0.0473	0.0432
Local Linear RD of Difference in Normalized Test Scores With Covariates							
RD_Estimate	0.131	0.0411	0.0356	0.0700	0.145	0.223	0.129
Observations	6966	5493	5355	4470	4414	3229	2689
Robust p-value	0.0111	0.328	0.429	0.182	0.0242	0.0507	0.303
BW Loc. Poly.	0.0380	0.0456	0.0748	0.0345	0.0356	0.0197	0.0254
BW Bias	0.0747	0.0858	0.132	0.0770	0.0757	0.0343	0.0488

Notes: The first section of this table shows the effect on test scores using a simple local linear rd of the normalized test scores. The second and third sections of this table show the effect on test scores using a local linear RD of the change in normalized test scores since the year before the elections. The third section also includes covariates for the grade, test subject, election vote-share threshold, and percentage of economically disadvantaged students. All sections use a triangular kernal with a mserd bandwidth type and second order bias. I use the method described by Calonico, Cattaneo, Farrell, and Titiuni (Calonico et al. 2017). I use the distance between the vote-share and the cutoff for each election as the running variable. Election data comes from the California Election Data Archive. Spending data comes from the NCES Common Core of Data. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test

Table 10: Spending Per Student: TOT Effects

	Capital Spending	Construction	Land and Existing	All Equipment	Instr. Spending
Treatment Year of Election	707.0	651.4	43.2	12.4	830.8*
	(456.5)	(431.3)	(131.2)	(24.2)	(436.1)
Treatment 1 Year Later	2549.7***	2612.5***	-63.8	0.93	42.8
	(645.6)	(611.7)	(195.5)	(27.0)	(435.6)
Treatment 2 Years Later	2561.8***	2516.9***	41.5	3.45	120.8
	(668.0)	(647.3)	(153.3)	(19.4)	(482.9)
Treatment 3 Years Later	1953.9***	1886.5***	-7.23	74.6***	68.2
	(598.8)	(530.5)	(220.7)	(24.0)	(377.9)
Treatment 4 Years Later	828.6	796.7	11.5	20.5	189.8
	(719.2)	(690.0)	(206.7)	(17.9)	(453.4)
Treatment 5 Years Later	879.2*	1059.0**	-200.6	20.8	-66.9
	(502.4)	(463.3)	(204.1)	(29.9)	(335.9)
Treatment 6 Years Later	992.1**	860.4**	152.9	-21.2	-121.1
	(396.0)	(378.6)	(110.2)	(24.0)	(280.3)
Observations	4764	4764	4764	4764	4764
R-Squared	0.41	0.37	0.67	0.31	0.86

Notes: Table shows results from the main specification for the effect on a number of measures of spending per student. This specification uses a dynamic regression discontinuity design. Regression are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, and a third level polynomial of the vote share interacted with the required vote share for each measure to pass as well as fixed effects for calendar year and school district.

Table 11: TOT Effect on Test Scores

	All Grades	High School	Middle School	Elementary
Treatment Year of Election	0.096***	0.031	0.059	0.13***
	(0.033)	(0.048)	(0.047)	(0.045)
Treatment 1 Year Later	0.084**	0.0031	0.10**	0.097*
	(0.040)	(0.055)	(0.050)	(0.053)
Treatment 2 Years Later	0.056	0.051	0.076	0.047
	(0.041)	(0.054)	(0.054)	(0.055)
Treatment 3 Years Later	0.026	0.012	0.065	0.0077
	(0.040)	(0.056)	(0.049)	(0.051)
Treatment 4 Years Later	0.036	0.059	0.059	0.019
	(0.044)	(0.058)	(0.053)	(0.056)
Treatment 5 Years Later	0.011	0.053	0.013	-0.00010
	(0.044)	(0.056)	(0.052)	(0.055)
Treatment 6 Years Later	0.0081	-0.0019	0.029	0.0054
	(0.040)	(0.050)	(0.049)	(0.050)
Observations	57707	8370	16308	33029
R-Squared	0.83	0.91	0.88	0.84

Notes: Table shows results for the TOT effect of passing school bond measures on student standardized test scores. In this table I include math test scores for grades two through six in addition to the ELA test scores for grades two through eleven from the California Standards Test. I do not include math test scores for higher grades as students are able to take different tests. This specification uses a dynamic regression discontinuity design. Regression are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, and a third level polynomial of the vote share interacted with the required vote share for each measure to pass. I include fixed effects for grade, calendar year, and school district.

Table 12: ITT Effect on Proficiency Rates: Local Linear RD

Local Linear RD of Difference in Proficiency Rates							
VARIABLES	Year of the Election	1 Year Later	2 Year Later	3 Year Later	4 Year Later	5 Year Later	6 Year Later
RD_Estimate	0.501	1.350	1.445	1.505	-5.028	-1.007	-1.448
Observations	7095	5623	5473	4590	4538	3323	2769
Conventional Std. Error	0.799	0.732	0.677	0.891	1.740	2.397	2.963
Conventional p-value	0.531	0.0653	0.0327	0.0913	0.00386	0.674	0.625
Robust p-value	0.425	0.107	0.0765	0.220	0.00348	0.542	0.518
BW Loc. Poly.	0.0490	0.0657	0.0978	0.0546	0.0229	0.0211	0.0209
BW Bias	0.0855	0.110	0.165	0.110	0.0534	0.0437	0.0468
Local Linear RD of Per Student Capital Spending							
VARIABLES	Year of the Election	1 Year Later	2 Year Later	3 Year Later	4 Year Later	5 Year Later	6 Year Later
RD_Estimate	0.244	1.372	0.992	1.609	0.889	1.919	1.952
Observations	6966	5505	5355	4470	4414	3229	2689
Conventional Std. Error	0.787	0.728	0.886	0.925	1.126	2.009	2.575
Conventional p-value	0.757	0.0593	0.263	0.0818	0.430	0.339	0.448
Robust p-value	0.621	0.0990	0.435	0.135	0.659	0.323	0.460
BW Loc. Poly.	0.0492	0.0663	0.0644	0.0476	0.0376	0.0255	0.0216
BW Bias	0.0849	0.112	0.103	0.0821	0.0669	0.0505	0.0395

Notes: This table shows the effect on Proficiency Rates using a local linear RD of the change in normalized test scores since the year before the elections. The Second section also includes covariates for the grade, test subject, election vote-share threshold, and percentage of economically disadvantaged students. All sections use a triangular kernal with a mserd bandwidth type and second order bias. I use the method described by Calonico, Cattaneo, Farrell, and Titiuni (Calonico et al. 2017). I use the distance between the vote-share and the cutoff for each election as the running variable. Election data comes from the California Election Data Archive. Spending data comes from the NCES Common Core of Data. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test

Table 13: Percent Proficient

	All Grades
Treatment Year of Election	0.80
	(0.51)
Treatment 1 Year Later	1.38**
	(0.62)
Treatment 2 Years Later	1.64**
	(0.66)
Treatment 3 Years Later	1.35*
	(0.70)
Treatment 4 Years Later	1.24*
	(0.71)
Treatment 5 Years Later	0.69
	(0.72)
Treatment 6 Years Later	0.62
	(0.66)
Observations	57707
R-Squared	0.42

Notes: Here I run the main specification on the percentage of students who test as proficient in math and reading. Controls are included for relative year, economically disadvantaged percentage, and the required vote share for each measure to pass. I include fixed effects for grade, school year, test subject, and school district. The results are clustered at the school district level.

Table 14: Normalized Test Scores by Income Partition

	Low Income Districts	High Income Districts
Treatment Year of Election	0.096**	0.11**
	(0.044)	(0.044)
Treatment 1 Year Later	0.12**	0.080
	(0.059)	(0.052)
Treatment 2 Years Later	0.059	0.027
	(0.066)	(0.061)
Treatment 3 Years Later	0.027	0.0057
	(0.071)	(0.057)
Treatment 4 Years Later	0.019	0.011
	(0.076)	(0.070)
Treatment 5 Years Later	-0.021	-0.017
	(0.074)	(0.071)
Treatment 6 Years Later	-0.013	-0.027
	(0.063)	(0.070)
Observations	31503	26204
R-Squared	0.81	0.82

Notes: Table shows results for the effect of passing school bond measures on student ELA and Mathematics standardized test scores in districts with the highest and lowest percentage of students who are designated as economically disadvantaged. Districts were selected into these income categories based on the percentage of students who are classified as economically disadvantaged the year before the election. This specification uses a dynamic regression discontinuity design. Regressions are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, and a third level polynomial of the vote share interacted with the required vote share for each measure to pass. I include fixed effects for grade, calendar year, and school district. Test scores are normalized by grade, test subject, and school year.

Table 15: ITT Effects on Normalized Test Scores: Parametric RD, One Polynomial

	All Grades
Treatment Year of Election	0.090***
	(0.029)
Treatment 1 Year Later	0.094***
	(0.036)
Treatment 2 Years Later	0.044
	(0.032)
Treatment 3 Years Later	0.0038
	(0.031)
Treatment 4 Years Later	0.034
	(0.034)
Treatment 5 Years Later	0.022
	(0.033)
Treatment 6 Years Later	-0.010
	(0.030)
Observations	91637
R-Squared	0.84

Notes: This table shows the ITT effects estimated using the parametric model with only one polynomial for the vote-share. This uses a window of two years before to six years after each election. Variables include whether a bond passed in each relative year, a third level polynomial of the vote-share, and a control for the percentage of students classified as economically disadvantaged. Fixed effects are included for election window, relative year, and calendar year. Results are clustered at the district level. Election data comes from the California Election Data Archive. Spending data comes from the NCES Common Core of Data.

Table 16: Sample Tests

	55% Cutoff Elections	No Close Together Elections
Treatment Year of Election	0.13**	0.098**
	(0.053)	(0.048)
Treatment 1 Year Later	0.17**	0.053
	(0.072)	(0.052)
Treatment 2 Years Later	0.14*	0.024
	(0.078)	(0.053)
Treatment 3 Years Later	0.085	0.031
	(0.080)	(0.053)
Treatment 4 Years Later	0.11	0.011
	(0.096)	(0.062)
Treatment 5 Years Later	0.15	-0.018
	(0.099)	(0.059)
Treatment 6 Years Later	0.064	-0.022
	(0.094)	(0.057)
Observations	21846	45197
R-Squared	0.76	0.83

Notes: Here I show results for the effect of passing school bond measures on student ELA and Mathematics standardized test scores from the California Standards Test in two different samples of school districts. In the first column I look at only districts that had only elections that required a 55% threshold. In the second column, I look at only districts that did not have multiple elections within two years of each other. Regression are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, the required vote share for each measure to pass, and a third level polynomial of the vote share. I include fixed effects for calendar year, grade, and school district. Test scores are normalized by grade, test subject, and school year.

Table 17: Balanced Panel of Districts, Spending Per Student

	Capital Spending	Construction	Land and Existing	All Equipment	Instr. Spending
Treatment Year of Election	573.3	726.4	-212.0	58.9	130.4
	(818.8)	(744.4)	(194.8)	(42.4)	(471.6)
Treatment 1 Year Later	2556.0**	2776.2**	-235.2	15.1	-609.6
	(1155.4)	(1109.8)	(232.1)	(33.0)	(460.9)
Treatment 2 Years Later	3070.7***	3074.3***	-1.90	-1.67	-363.0
	(1131.1)	(1112.0)	(173.1)	(29.6)	(630.3)
Treatment 3 Years Later	1706.4*	1780.8*	-165.0	90.7**	-145.3
	(959.2)	(926.3)	(180.6)	(39.6)	(754.0)
Treatment 4 Years Later	1020.2	1021.4	-23.9	22.7	-361.9
	(1222.6)	(1216.6)	(165.6)	(25.0)	(1028.2)
Observations	2281	2281	2281	2281	2281
R-Squared	0.40	0.40	0.31	0.34	0.88

Notes: Here I run the main specification for spending with a balanced panel with only districts that are present in the data in all years from two years before the elections to four years after. The regressions are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, the required vote share for each measure to pass, and a third level polynomial of the vote share. I include fixed effects for calendar year and school district. Test scores are normalized by grade, test subject, and school year.

Table 18: Balanced Panel of Districts, Effect on Normalized Test Scores

	All Grades ELA
Treatment Year of Election	0.14***
	(0.048)
Treatment 1 Year Later	0.091*
	(0.051)
Treatment 2 Years Later	0.073
	(0.058)
Treatment 3 Years Later	0.037
	(0.067)
Treatment 4 Years Later	0.018
	(0.076)
Observations	27731
R-Squared	0.84

Notes: Here I run the main specification for test scores with a balanced panel with only districts that are present in the data in all years from two years before the elections to four years after. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test. The regressions are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, the required vote share for each measure to pass, and a third level polynomial of the vote share. I include fixed effects for calendar year, grade, and school district. Test scores are normalized by grade, test subject, and school year.

Table 19: Full Panel: TOT Effects on Spending Per Student

	Capital Spending	Construction	Land and Existing	All Equipment	Instr. Spending
Treatment Year of Election	742.6	666.9	67.1	8.60	869.4
	(487.8)	(453.0)	(143.1)	(22.9)	(669.6)
Treatment 1 Year Later	2251.8***	2332.1***	-72.5	-7.75	-177.3
	(661.2)	(624.9)	(206.9)	(27.4)	(649.6)
Treatment 2 Years Later	2710.1***	2595.9***	116.9	-2.70	-9.04
	(732.9)	(707.7)	(168.2)	(24.1)	(728.3)
Treatment 3 Years Later	2205.4***	2039.7***	90.0	75.6***	480.9
	(641.3)	(571.7)	(249.7)	(24.1)	(628.2)
Treatment 4 Years Later	1174.1	1039.7	94.4	40.0*	738.5
	(781.5)	(758.6)	(224.8)	(23.6)	(709.0)
Treatment 5 Years Later	1040.0*	1169.1**	-161.1	32.1	177.0
	(622.6)	(578.4)	(249.2)	(42.1)	(628.0)
Treatment 6 Years Later	1339.9*	894.4	465.8	-20.3	238.5
	(701.6)	(604.6)	(336.8)	(25.6)	(621.7)
Observations	7611	7611	7611	7611	7611
R-Squared	0.31	0.30	0.16	0.44	0.62

Notes: Table shows results for the TOT effect of passing school bond measures on per student spending using the full panel of data. This specification uses a dynamic regression discontinuity design. Regression are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, and a third level polynomial of the vote share interacted with the required vote share for each measure to pass. I include fixed effects for calendar year and school district. Relative year variables are included for all possible relative years after the elections 0 through 21.

Table 20: Full Panel: TOT Effects on Normalized Test Scores

	All Grades	High School	Middle School	Elementary
Treatment Year of Election	0.079*	0.0052	0.050	0.11**
	(0.043)	(0.062)	(0.060)	(0.050)
Treatment 1 Year Later	0.081*	-0.076	0.11*	0.11*
	(0.049)	(0.076)	(0.064)	(0.061)
Treatment 2 Years Later	0.057	-0.053	0.077	0.071
	(0.054)	(0.085)	(0.076)	(0.066)
Treatment 3 Years Later	0.034	-0.11	0.064	0.048
	(0.059)	(0.093)	(0.074)	(0.071)
Treatment 4 Years Later	0.051	-0.056	0.063	0.064
	(0.067)	(0.10)	(0.080)	(0.078)
Treatment 5 Years Later	0.034	-0.087	0.015	0.066
	(0.069)	(0.11)	(0.081)	(0.079)
Treatment 6 Years Later	0.039	-0.16	0.034	0.083
	(0.070)	(0.11)	(0.085)	(0.081)
Observations	91920	12629	26107	53184
R-Squared	0.81	0.89	0.87	0.81

Notes: Table shows results for the TOT effect of passing school bond measures on student standardized test scores using the full panel of data. In this table I include math test scores for grades two through six in addition to the ELA test scores for grades two through eleven from the California Standards Test. I do not include math test scores for higher grades as students are able to take different tests. This specification uses a dynamic regression discontinuity design. Regression are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, and a third level polynomial of the vote share interacted with the required vote share for each measure to pass. I include fixed effects for grade, calendar year, and school district. Relative year variables are included for all possible relative years after the elections 0 through 21.

Table 21: TOT Effects on Normalized Test Scores and Proficiency Rates for Districts that Fail to Pass at Least One Measure

	Normalized Test Scores	Proficiency Rates
Treatment Year of Election	0.12**	1.36*
	(0.060)	(0.82)
Treatment 1 Year Later	0.14**	2.83***
	(0.064)	(0.99)
Treatment 2 Years Later	0.093	2.47**
	(0.066)	(0.97)
Treatment 3 Years Later	0.024	2.07**
	(0.061)	(0.88)
Treatment 4 Years Later	0.063	2.73***
	(0.069)	(0.97)
Treatment 5 Years Later	0.096	1.73*
	(0.065)	(0.97)
Treatment 6 Years Later	0.071	1.11
	(0.051)	(0.84)
Observations	18263	18263
R-Squared	0.83	0.48

Notes: Table shows results for the TOT effect of passing school bond measures on student standardized test scores using only districts that fail at least one measure. In this table I include math test scores for grades two through six in addition to the ELA test scores for grades two through eleven from the California Standards Test. This specification uses a dynamic regression discontinuity design. Regression are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, and a third level polynomial of the vote share interacted with the required vote share for each measure to pass. I include fixed effects for grade, calendar year, and school district.

Table 22: Pre-year Placebos on Spending

	Capital Spending	Construction	Land and Existing	All Equipment	Instr. Spending
Placebo 6 Years Before	-109.6	-29.0	-111.6	31.1	-588.8
	(1026.7)	(995.8)	(157.5)	(21.3)	(411.9)
Placebo 5 Years Before	-109.7	-33.0	-83.7	7.07	-92.4
	(654.4)	(618.0)	(105.6)	(23.0)	(401.7)
Placebo 4 Years Before	-307.1	-109.8	-209.4*	12.1	-66.1
	(549.6)	(503.5)	(126.7)	(25.4)	(363.9)
Placebo 3 Years Before	-501.2	-295.6	-221.5	16.0	-159.9
	(768.9)	(747.3)	(168.0)	(22.6)	(352.0)
Placebo 2 Years Before	-507.2	-136.5	-391.9	21.1	90.0
	(496.0)	(416.5)	(264.9)	(17.6)	(340.8)
Treatment Year of Election	435.0	520.7	-105.7	20.0	749.3
	(488.3)	(460.2)	(104.8)	(26.0)	(490.4)
Treatment 1 Year Later	2190.1***	2397.6***	-220.6	13.1	-261.2
	(653.7)	(620.2)	(156.2)	(27.2)	(440.9)
Treatment 2 Years Later	2186.3***	2253.4***	-81.3	14.2	-146.4
	(649.0)	(628.5)	(122.9)	(20.1)	(478.7)
Treatment 3 Years Later	1639.4***	1685.8***	-125.9	79.5***	-241.9
	(596.7)	(522.9)	(187.0)	(23.9)	(351.1)
Treatment 4 Years Later	454.4	520.0	-91.4	25.7	-160.4
	(701.0)	(670.9)	(164.5)	(18.0)	(428.3)
Treatment 5 Years Later	496.6	762.3*	-290.3	24.6	-333.5
	(484.9)	(446.9)	(187.1)	(28.8)	(314.2)
Treatment 6 Years Later	624.1*	568.6	76.3	-20.8	-343.5
	(362.1)	(351.3)	(74.2)	(22.2)	(255.9)
Observations	6133	6133	6133	6133	6133
R-Squared	0.40	0.36	0.63	0.29	0.81

Notes: Here I run the main specification for test scores but with the years before the elections acting as placebos. If the specification is correct we should not see any effect during the years before the election. The regressions are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, the required vote share for each measure to pass, and a third level polynomial of the vote share. I include fixed effects for calendar year and school district. Test scores are normalized by grade, test subject, and school year.

Table 23: Pre-year Placebos on Test Scores

	Normalized Test Scores	Proficiency Rates
Placebo 6 Years Before	-0.020	0.89
	(0.052)	(0.81)
Placebo 5 Years Before	-0.030	1.13
	(0.049)	(0.78)
Placebo 4 Years Before	-0.061	0.12
	(0.047)	(0.78)
Placebo 3 Years Before	-0.061	0.26
	(0.048)	(0.75)
Placebo 2 Years Before	-0.022	1.11
	(0.048)	(0.70)
Treatment Year of Election	0.067	0.87
	(0.044)	(0.68)
Treatment 1 Year Later	0.045	1.18
	(0.046)	(0.73)
Treatment 2 Years Later	0.019	1.48**
	(0.043)	(0.69)
Treatment 3 Years Later	-0.0055	1.05
	(0.041)	(0.72)
Treatment 4 Years Later	0.00047	0.89
	(0.043)	(0.70)
Treatment 5 Years Later	-0.026	0.28
	(0.043)	(0.70)
Treatment 6 Years Later	-0.022	0.25
	(0.039)	(0.63)
Observations	74333	74333
R-Squared	0.82	0.40

Notes: Here I run the main specification for test scores but with the years before the elections acting as placebos. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test. The regressions are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, the required vote share for each measure to pass, and a third level polynomial of the vote share. I include fixed effects for calendar year, grade, and school district. Test scores are normalized by grade, test subject, and school year.

Table 24: Local Linear RD of Difference in Normalized Test Scores for Placebo Years

VARIABLES	Placebo Three Years Before	Placebo Two Years Before
RD_Estimate	0.0146	-0.0137
Observations	5915	6519
Conventional Std. Error	0.0486	0.0316
Conventional p-value	0.764	0.665
Robust p-value	0.637	0.800
BW Loc. Poly.	0.0502	0.0846
BW Bias	0.0877	0.151

Notes: This table shows the effect on the change in test scores the years before the elections as placebo tests using a local linear RD. I subtract the scores from the same district, grade, and test subject the year before the election from the tests scores of the relative years shown in the table. All sections use a triangular kernal with a mserd bandwidth type and second order bias. I use the method described by Calonico, Cattaneo, Farrell, and Titiuni (Calonico et al. 2017). I use the distance between the vote-share and the cutoff for each election as the running variable. Election data comes from the California Election Data Archive. Spending data comes from the NCES Common Core of Data. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test

Table 25: September School Year Comparison

	September Cutoff	One Poly.	Reducing Variables
treat0	0.088***	0.083***	
	(0.033)	(0.030)	
Treatment 1 Year Later	0.077**	0.071**	0.034
	(0.036)	(0.031)	(0.027)
Treatment 2 Years Later	0.065*	0.044	0.014
	(0.038)	(0.032)	(0.030)
Treatment 3 Years Later	0.051	0.0062	-0.022
	(0.040)	(0.032)	(0.031)
Treatment 4 Years Later	0.016	-0.029	-0.051
	(0.041)	(0.032)	(0.032)
Treatment 5 Years Later	0.0094	-0.016	-0.042
	(0.041)	(0.032)	(0.032)
Treatment 6 Years Later	0.0030	0.00066	-0.030
	(0.040)	(0.033)	(0.033)
Year 0	X	X	
Disadvantaged Control	X	X	
One Polynomial		X	X
Observations	54659	54659	55545
R-Squared	0.82	0.82	0.83

Notes: In this table I show a side by side comparison of the effects on normalized test scores using specifications that have less and less differences with the specification used by Martorell et al. and Cellini, et al (Martorell, Stange, and McFarlin Jr 2016; Cellini, Ferreira, and Rothstein 2010). All columns use a September first cutoff rather than June first for assigning elections to school years. In the first column everything is the same as my main specification except for the September cutoff. In the second column, I do not include separate polynomials for elections with a 55% and 66.7% thresholds. Instead, I have one polynomial and another variable for the threshold. In the third column, I also do not include the control for percentage of students designated as economically disadvantaged as well as leaving out variables for the year of the election. This specification uses a dynamic regression discontinuity design. Regression are clustered at the school district level. Controls are included for relative year, and a third level polynomial of the vote share. Test scores are normalized by grade, test subject, and school year. Data comes from the California Election Data Archive and the IES Common Core of Data.

Table 26: Changing Fixed Effects for TOT Estimates of Effects on Capital Spending Per Student

	No District or Year FE	No Year FE	Both District and Year FE
Treatment Year of Election	229.6	545.6	707.0
	(344.0)	(453.6)	(456.5)
Treatment 1 Year Later	1515.9***	2322.3***	2549.7***
	(553.9)	(642.9)	(645.6)
Treatment 2 Years Later	1643.7***	2402.8***	2561.8***
	(530.4)	(659.6)	(668.0)
Treatment 3 Years Later	1732.4***	1939.5***	1953.9***
	(509.7)	(599.3)	(598.8)
Treatment 4 Years Later	660.9	745.1	828.6
	(614.9)	(732.0)	(719.2)
Treatment 5 Years Later	338.3	818.2	879.2*
	(401.9)	(503.7)	(502.4)
Treatment 6 Years Later	893.9**	1018.7***	992.1**
	(455.0)	(390.7)	(396.0)
Observations	4764	4764	4764
R-Squared	0.095	0.40	0.41

Notes: Table shows results for the effect on capital spending per-capita with differing fixed effects. The first column does not include district or school year fixed effects. The second column includes district but not school year fixed effects. The third column includes both district and school year fixed effects. This specification uses a dynamic regression discontinuity design. Regression are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, and a third level polynomial of the vote share interacted with the required vote share for each measure to pass. Test scores are normalized by grade, test subject, and school year.

Table 27: Changing Fixed Effects for TOT Estimates of Effects on Normalized Test Scores

	No District or Year FE	No Year FE	Both District and Year FE
Treatment Year of Election	-0.029	0.10***	0.096***
	(0.079)	(0.034)	(0.033)
Treatment 1 Year Later	-0.011	0.084**	0.084**
	(0.072)	(0.040)	(0.040)
Treatment 2 Years Later	-0.025	0.052	0.056
	(0.069)	(0.041)	(0.041)
Treatment 3 Years Later	0.0067	0.019	0.026
	(0.074)	(0.040)	(0.040)
Treatment 4 Years Later	-0.0045	0.033	0.036
	(0.067)	(0.044)	(0.044)
Treatment 5 Years Later	-0.092	0.0056	0.011
	(0.062)	(0.045)	(0.044)
Treatment 6 Years Later	-0.12**	0.0025	0.0081
	(0.062)	(0.040)	(0.040)
Observations	57707	57707	57707
R-Squared	0.63	0.83	0.83

Notes: Table shows results for the effect on normalized test scores with differing fixed effects. The first column does not include district or school year fixed effects. The second column includes district but not school year fixed effects. The third column includes both district and school year fixed effects. This specification uses a dynamic regression discontinuity design. Regression are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, and a third level polynomial of the vote share interacted with the required vote share for each measure to pass as well as fixed effects for grade. Test scores are normalized by grade, test subject, and school year.

Table 28: Non-Causal Estimates of Relationship Between Adequate Equipment and Satisfaction

	Computers Per Student	Lack of Space	General Satisfaction
Agree-Adequate Materials	-0.020***	-0.026***	0.23***
	(0.0042)	(0.0028)	(0.0027)
Observations	79020	40280	85330
R-Squared	0.00028	0.0022	0.077

Notes: Here I calculate the relationship between whether public school teachers report that they have adequate materials with the number of computers per student, whether they report a lack of space, and whether they are generally satisfied. Both whether they agree that they have adequate materials are answered on a scale of one to four with one being strongly agree and four being strongly disagree. Whether they lack space is answered as either one for yes or two for no. Observation numbers are rounded to the nearest ten. SOURCE: U.S. Department of Education, Institute of Education Sciences, School and Staffing Surveys for years 1999-2000 and 2003-2004.

Table 29: Staff Turnover Before 2011

	All Staff	Teachers	Administrators	Pupil Serving
Treatment Year of Election	0.031*	0.024	0.0061	0.038
	(0.016)	(0.019)	(0.072)	(0.083)
Treatment 1 Year Later	0.033**	0.036**	0.047	-0.064
	(0.016)	(0.018)	(0.053)	(0.10)
Treatment 2 Years Later	0.022	0.014	-0.030	0.020
	(0.016)	(0.019)	(0.054)	(0.076)
Treatment 3 Years Later	-0.014	-0.029	-0.028	-0.078
	(0.021)	(0.024)	(0.048)	(0.075)
Treatment 4 Years Later	0.023	0.0055	-0.017	0.077
	(0.018)	(0.022)	(0.055)	(0.072)
Treatment 5 Years Later	0.032**	0.012	0.0082	-0.032
	(0.015)	(0.015)	(0.046)	(0.075)
Treatment 6 Years Later	0.018	-0.0040	-0.055	0.090
	(0.014)	(0.016)	(0.048)	(0.086)
Observations	4427	3485	3436	3111
R-Squared	0.34	0.37	0.22	0.23

Notes: Here I run the main specification on staff turnover. I look only at years before 2011. Controls are included for relative year, economically disadvantaged percentage, and the required vote share for each measure to pass. I include fixed effects for school district. The results are clustered at the school district level.

Table 30: Teacher Turnover by Experience

	Less Than 15 Years	Greater Than or Equal 15 Years
Treatment 1 Year Later	0.028	0.14**
	(0.024)	(0.069)
Treatment 2 Years Later	0.0091	0.096
	(0.028)	(0.068)
Treatment 3 Years Later	-0.031	0.043
	(0.031)	(0.069)
Treatment 4 Years Later	-0.0056	0.19
	(0.029)	(0.19)
Treatment 5 Years Later	0.025	0.070*
	(0.023)	(0.041)
Treatment 6 Years Later	0.018	0.044
	(0.023)	(0.040)
Treatment Year of Election	0.0073	0.13**
	(0.026)	(0.063)
Observations	3475	3457
R-Squared	0.37	0.12

Notes: Here I run the main specification on teacher turnover for teachers with more and less than fifteen years of experience. I look only at years before 2011. Controls are included for relative year, economically disadvantaged percentage, and the required vote share for each measure to pass. I include fixed effects for school district. The results are clustered at the school district level.

Table 31: Staff Characteristics

	Graduate Degrees	Tenured	First Year	Years in District	Years Teaching
Treatment Year of Election	-22.1	-156.1	11.2	-0.15	-0.13
	(23.3)	(146.1)	(10.6)	(0.23)	(0.25)
Treatment 1 Year Later	-22.7	-120.8	20.4	-0.16	-0.22
	(23.2)	(131.7)	(13.2)	(0.27)	(0.30)
Treatment 2 Years Later	-13.7	-146.4	23.2*	0.013	-0.031
	(23.6)	(150.4)	(14.1)	(0.31)	(0.33)
Treatment 3 Years Later	-13.0	-113.1	10.7	0.51	0.40
	(21.4)	(107.1)	(12.3)	(0.35)	(0.36)
Treatment 4 Years Later	-4.55	-25.1	22.8	0.33	0.33
	(19.3)	(49.7)	(17.7)	(0.34)	(0.32)
Treatment 5 Years Later	16.4	-17.0	6.54	0.39	0.21
	(22.8)	(50.6)	(9.63)	(0.31)	(0.31)
Treatment 6 Years Later	51.5	121.8	30.5*	0.022	0.0031
	(54.1)	(146.2)	(18.5)	(0.26)	(0.28)
Observations	3469	3388	3487	3487	3487
R-Squared	0.99	0.78	0.91	0.87	0.86

Notes: Here I run the main specification on various staff characteristics. Controls are included for relative year, economically disadvantaged percentage, and the required vote share for each measure to pass. I include fixed effects for school district. The resulsts are clustered at the school district level.

Table 32: Effect on Enrollment and Economically Disadvantaged

	Enrollment	Econ. Dis. Percentage
Treatment Year of Election	-261.5	0.0093
	(212.4)	(0.019)
Treatment 1 Year Later	-3.90	0.0030
	(220.4)	(0.020)
Treatment 2 Years Later	314.9	0.035**
	(279.7)	(0.017)
Treatment 3 Years Later	472.7	0.015
	(369.4)	(0.021)
Treatment 4 Years Later	549.8	-0.0042
	(392.3)	(0.017)
Treatment 5 Years Later	92.9	0.0016
	(197.7)	(0.018)
Treatment 6 Years Later	-699.8	0.015
	(812.5)	(0.019)
Observations	4764	4764
R-Squared	0.99	0.92

Notes: Here I run the main specification, but with Enrollment and the percent of students who are designated as economically disadvantaged as the dependent variables. The regressions are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, the required vote share for each measure to pass, and a third level polynomial of the vote share. I include fixed effects for calendar year and district.

Appendix A Additional Tests

In Table 33, I include test scores only the ELA test and exclude the scores for the Mathematics test. Here we get similar, though slightly smaller, results.

I further explore my results by splitting up the effect during the year the bond passed by whether the bond election occurred in between June and September, between October and December, and between January and March. Remember that January to March will be closest to when the test is taken. This can be seen in Table 8. I find that the effect seems to be more significant if they take place before January. This points to it taking there being some delay before the effect kicks in. It also points to there being some persistence of the effect as we observe it even in elections that took place during the summer before.

I explore the possibility of heterogeneity in the effect on test scores by income level. I try running a regression with interactions between the treatment variable and a cubic polynomial of the percentage of students who are designated as economically disadvantaged, as well as including a non-interacted polynomial of the percentage of free and reduced lunch students. The results for this can be seen in Table 34. There does seem to be some heterogeneity in the treatment on test scores during the year of the election. It does not appear to be linear, and in fact, when I include only the linear interaction of the percentage of free and reduced price meals I do not see a significant effect from it.

To more closely look for heterogeneity in the effect, I plot the residuals from the main specification on test scores when I exclude the control for the percentage of students who are designated as economically disadvantaged in Figure 13.

To look for heterogeneity in the effect on proficiency by the percent of students near the border of proficiency I separate the school districts into thirds based on their proficiency rates two years before the election and run the TOT specification on each of those partitions. This allows me to test whether there is an effect in the school districts that have more students below the proficiency rate who may be more focused on those students. I do not find clear differences between the partitions. This can be seen in Table 35.

I try replacing the polynomial with a variety of polynomials of different orders. This gives very similar coefficients to the main specification. It can be seen in Table 36.

I explore some slightly different specifications. First, I don't interact vote-share and the election cutoff.

This specification becomes

$$Y_{igt} = \sum_{\tau}[\beta_\tau Elect_{i\tau} + \omega_\tau Pass_{i\tau} + \psi_\tau Thresholdi\tau + \gamma_\tau VoteShare_{i\tau} + \gamma_\tau VoteShare^2_{i\tau} + \gamma_\tau VoteShare^3_{i\tau}]$$

$$+\delta Econ.Dis._{it} + \alpha_t + \rho_g + \phi_i + \epsilon_{it}$$

where $Elect_{i\tau}$ is the indicator variable that an election was held in district i in relative year τ. $Pass_{i\tau}$ is the variable of interest and is an indicator that an election passed in district i in relative year τ. $Voteshare i\tau$ is the percent of the vote in favor of a measure held in district i and in relative year τ. $Econ.Dis._{it}$ is the percent of students in district i during year t that are classified as economically disadvantaged. α_t is year fixed effects, rho_g is the grade fixed effects, and ϕ_i is a district fixed effect. I cluster by school district.

I replace the polynomial of the vote share with a polynomial of the distance of the vote-share to the cutoff. This is to explore the possibility that districts that are more similar to other districts that are about as close to the vote share threshold that they need to pass bonds rather than districts with similar vote shares. I think that this is unlikely, but worth exploring. I obtain similar results using this method as can be seen in Table 38.

Using the distance from the vote share also allows me to try changing the specification in another manner. I interact the cubic polynomial of the distance from between the vote share and the threshold with whether the elections failed or passed. My specification becomes

$$Y_{igt} = \sum_{\tau}[\beta_\tau Elect_{i\tau} + \omega_\tau Pass_{i\tau} + \psi_\tau Thresholdi\tau + Pass_{i\tau} * (\gamma_\tau Howclose_{i\tau} + \gamma_\tau Howclose^2_{i\tau} + \gamma_\tau Howclose^3_{i\tau})$$

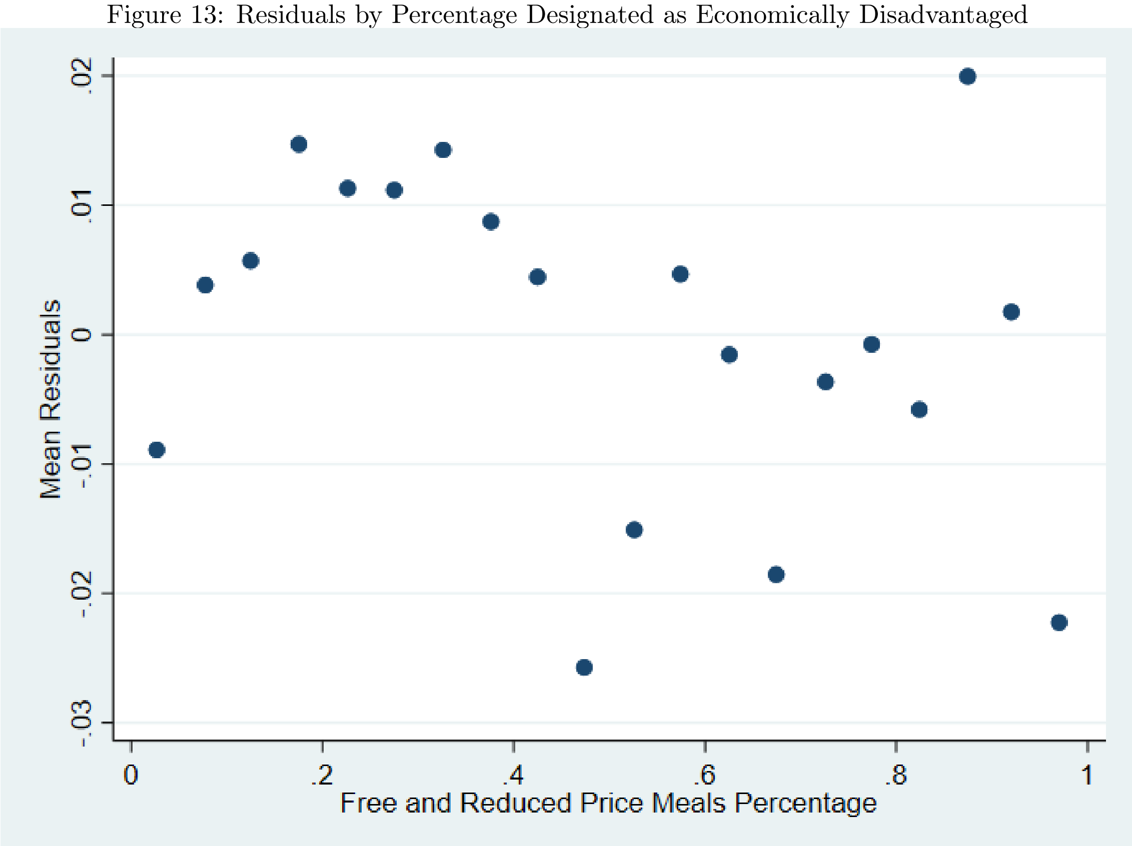
$$+Fail_{i\tau} * (\gamma_\tau Howclose_{i\tau} + \gamma_\tau Howclose^2_{i\tau} + \gamma_\tau Howclose^3_{i\tau})] + \delta FRPM_{it} + \alpha_t + \rho_g + \phi_i + \epsilon_i gt$$

where $Elect_{i\tau}$ is the indicator variable that an election was held in district i in relative year τ. $Pass_{i\tau}$ is the variable of interest and is an indicator that an election passed in district i in relative year τ. $Howclose_{i\tau}$ is the difference between the percent of the vote in favor of a measure held in district i and in relative year τ and the cutoff needed for the measure to pass. $FRPM_{it}$ is the percent of students in district i during year t that are designated as economically disadvantaged. α_t is year fixed effects, ρ_g is the grade fixed effects, and ϕ_{it} is a district fixed effect. This time I do not find the effect on test scores the year of the election. The results can be seen in Table 38 as well.

Next, I show the results if I weight the districts by their enrollment. This can be seen in Table 40. Because of extreme outliers in the size of districts, I exclude any districts that are ever in the top one percent by number of students who take one of the tests at any time in the sample period. The coefficient on the effect the year of the election remains similar to before, though it is only borderline significant. There is also a borderline significant positive effect two years after the election.

In Table A, I run the placebo test with only the pre-years and not controlling for any post year effects. Here I, unsurprisingly, find negative effects for some of the pre-years. This is likely caused by districts failing a bond measure and then passing one a few years later. In Table 42, I show the differential probability that a district passes a bond given that they recently failed to pass one. Because this is still an RD, this is the differential probability against having passed a bond measure. I do this by modifying the ITT specification so that they outcome variable is a bond passing and the treat variable is whether one failed in the focal election. I find that there is a large probability that a district will pass a bond measure in the the years following a failed measure. In the third column of Table A, I show the placebo years for districts that only had one measure. For this subgroup, there should be no chance of this happening, and indeed, I find no effect.

Figure 13: Residuals by Percentage Designated as Economically Disadvantaged



Notes:
The y-axis of this figure shows residuals from the main specification on ELA and Mathematics test scores without controlling for percentage designated as economically disadvantaged. The x-axis shows the percentage designated as economically disadvantaged. The residuals are averaged for every 5% window of the percentage designated as economically disadvantaged.

Table 33: Effect on Only ELA Test Scores

	All Grades ELA	High School ELA	Middle School ELA	Elementary ELA
Treatment Year of Election	0.076**	0.031	0.078*	0.098**
	(0.030)	(0.048)	(0.042)	(0.041)
Treatment 1 Year Later	0.076**	0.0031	0.12**	0.088*
	(0.036)	(0.055)	(0.049)	(0.049)
Treatment 2 Years Later	0.057	0.051	0.078	0.040
	(0.037)	(0.054)	(0.051)	(0.053)
Treatment 3 Years Later	0.034	0.012	0.079	0.0075
	(0.037)	(0.056)	(0.048)	(0.049)
Treatment 4 Years Later	0.040	0.059	0.065	0.0078
	(0.040)	(0.058)	(0.052)	(0.052)
Treatment 5 Years Later	0.017	0.053	0.016	-0.0021
	(0.040)	(0.056)	(0.048)	(0.051)
Treatment 6 Years Later	0.0092	-0.0019	0.019	0.016
	(0.036)	(0.050)	(0.045)	(0.045)
Observations	37063	8370	12180	16513
R-Squared	0.87	0.91	0.91	0.89

Notes: Table shows results from the main specification for the TOT effect of passing school bond measures on only student ELA standardized test scores from the California Standards Test. This specification uses a dynamic regression discontinuity design. Regression are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, the required vote share for each measure to pass, and a third level polynomial of the vote share. I include fixed effects for grade, calendar year, and school district. Test scores are normalized by grade, test subject, and school year.

Table 34: Heterogeneity in Income

	All Grades ELA
Treatment Year of Election	0.18***
	(0.052)
Treat 0 Linear Econ. Dis. Interaction	-0.69**
	(0.31)
Treat 0 Squared Econ. Dis. Interaction	1.42**
	(0.72)
Treat 0 Cubed Econ. Dis. Interaction	-0.83*
	(0.48)
Observations	57707
R-Squared	0.83

Notes: Here I test for heterogeneity by income. I run the main specification on ELA and Mathematics standardized test scores, but I add a third degree polynomial of the percentage designated as economically disadvantaged as well as a polynomial that is not interacted with the treat variables. Like before, regressions are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, the required vote share for each measure to pass, and a third level polynomial of the vote share. I include fixed effects for calendar year, grade, and school district. Test scores are normalized by grade, test subject, and school year.

Table 35: Percent Proficient By Proficiency Partition Two Years Before Election

	Low	Medium	High
Treatment Year of Election	0.33	0.33	0.11
	(0.57)	(0.51)	(0.52)
Treatment 1 Year Later	1.17	1.01	0.51
	(0.73)	(0.66)	(0.76)
Treatment 2 Years Later	1.81**	1.57*	1.10
	(0.91)	(0.80)	(0.87)
Treatment 3 Years Later	1.28	2.04**	1.81*
	(1.02)	(0.88)	(1.08)
Treatment 4 Years Later	1.61	2.12**	1.61
	(1.14)	(1.02)	(1.07)
Treatment 5 Years Later	0.95	1.82	1.41
	(1.41)	(1.18)	(1.22)
Treatment 6 Years Later	-0.11	1.90	1.90
	(1.45)	(1.35)	(1.51)
Observations	26847	28820	23110
R-Squared	0.37	0.37	0.31

Notes: Here I run the main specification on the percentage of students who test as proficient in math and reading but split up the school districts by whether they are in the top, middle, or bottom third of districts by proficiency percentage two years before the election. Controls are included for relative year, economically disadvantaged percentage, and the required vote share for each measure to pass. I include fixed effects for grade, school year, test subject, and school district. The results are clustered at the school district level.

Table 36: Different Power Polynomials

	Linear	2nd Power	3rd Power	4th Power	5th Power
Treatment Year of Election	0.074***	0.087***	0.096***	0.096**	0.11**
	(0.027)	(0.028)	(0.034)	(0.038)	(0.046)
Treatment 1 Year Later	0.037	0.039	0.10***	0.10**	0.10**
	(0.032)	(0.032)	(0.037)	(0.041)	(0.042)
Treatment 2 Years Later	0.024	0.035	0.063*	0.072*	0.070*
	(0.031)	(0.031)	(0.036)	(0.042)	(0.042)
Treatment 3 Years Later	-0.022	0.011	0.030	0.040	0.041
	(0.031)	(0.031)	(0.036)	(0.043)	(0.043)
Treatment 4 Years Later	-0.016	0.019	0.040	0.058	0.054
	(0.033)	(0.033)	(0.037)	(0.046)	(0.046)
Treatment 5 Years Later	-0.020	0.012	0.017	0.028	0.023
	(0.033)	(0.033)	(0.037)	(0.045)	(0.046)
Treatment 6 Years Later	-0.019	0.0062	-0.00025	0.024	0.022
	(0.032)	(0.032)	(0.034)	(0.040)	(0.040)
Observations	57707	57707	57707	57707	57707
R-Squared	0.83	0.83	0.83	0.83	0.83

Notes: Here I run the main specification with a variety of different level polynomials of the vote share. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test. Regression are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, and the required vote share for each measure to pass. I include fixed effects for grade, calendar year, and school district. Test scores are normalized by grade, test subject, and school year.

Table 37: Effect on Test Scores by Month Grouping

	All Grades
Treatment Year of Election June to Sept.	0.086
	(0.060)
Treatment Year of Election Oct. to Dec.	0.083*
	(0.043)
Treatment Year of Election Jan. to March	0.079
	(0.057)
Treatment 1 Year Later	0.083**
	(0.040)
Treatment 2 Years Later	0.055
	(0.041)
Treatment 3 Years Later	0.026
	(0.040)
Treatment 4 Years Later	0.037
	(0.044)
Treatment 5 Years Later	0.011
	(0.044)
Treatment 6 Years Later	0.0078
	(0.040)
Observations	57707
R-Squared	0.83

Notes: Table shows results for the effect of passing school bond measures on student ELA standardized test scores. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test. In this specification the year of the election is split into three periods. This specification uses a dynamic regression discontinuity design Regression are clustered at the school district level. Controls for relative year, percentage designated as economically disadvantaged, and a third level polynomial of the vote share interacted with the required vote share for each measure to pass. I include fixed effects for grade, calendar year, test subject, and school district. Test scores are normalized by grade, test subject, and school year.

Table 38: Alternative Specifications

	One Polynomial	Two Polynomials
Treatment Year of Election	0.11***	0.033
	(0.037)	(0.054)
Treatment 1 Year Later	0.13***	0.023
	(0.047)	(0.059)
Treatment 2 Years Later	0.10*	0.028
	(0.053)	(0.071)
Treatment 3 Years Later	0.058	0.033
	(0.058)	(0.075)
Treatment 4 Years Later	0.10	0.11
	(0.066)	(0.089)
Treatment 5 Years Later	0.10	0.099
	(0.068)	(0.093)
Treatment 6 Years Later	0.062	0.053
	(0.070)	(0.094)
Observations	106642	106642
R-Squared	0.83	0.83

Notes: Here I replace the polynomial of the vote share with a polynomial of the difference between the vote share and the election threshold. For the two polynomial specification, I then add another polynomial of the vote share that I interact with the treat variable. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test. The regressions are clustered at the school district level. Controls are included for relative year and and percentage designated as economically disadvantaged. I include fixed effects for grade, calendar year, and school district. Test scores are normalized by grade, test subject, and school year.

Table 39: Spending Per Student Weighted by Number of Students

	Capital Spending	Construction	Land and Existing	All Equipment	Instr. Spending
Treatment Year of Election	677.7**	677.9**	-6.76	6.63	129.6
	(322.7)	(301.9)	(142.0)	(9.03)	(119.1)
Treatment 1 Year Later	827.4	907.3*	-83.4	3.53	225.6
	(562.9)	(534.7)	(130.7)	(9.50)	(157.8)
Treatment 2 Years Later	1340.4**	1343.7***	-6.02	2.74	195.1
	(526.7)	(485.1)	(133.9)	(9.39)	(156.5)
Treatment 3 Years Later	1365.9**	1168.3**	195.2	2.37	315.0*
	(536.3)	(519.1)	(154.6)	(10.6)	(184.9)
Treatment 4 Years Later	944.6	786.9	159.8	-2.09	226.2
	(576.0)	(543.2)	(172.7)	(9.44)	(209.4)
Treatment 5 Years Later	493.8	333.2	162.2	-1.65	152.9
	(552.7)	(525.0)	(169.4)	(10.3)	(221.1)
Treatment 6 Years Later	230.6	24.0	214.8	-8.20	137.4
	(583.5)	(553.1)	(159.9)	(11.3)	(229.4)
Observations	6231	6231	6231	6231	6231
R-Squared	0.59	0.59	0.38	0.44	0.93

Notes: Here I run the main specification except that I weight the districts by their enrollment. Because of large outliers in enrollment, I exclude any districts that are ever in the top one percent of the number of students who take any test. The regressions are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, the required vote share for each measure to pass, and a third level polynomial of the vote share. I include fixed effects for calendar year and school district.

Table 40: Weighted by Student Enrollment

	All Grades
Treatment Year of Election	0.075*
	(0.041)
Treatment 1 Year Later	0.072
	(0.045)
Treatment 2 Years Later	0.086
	(0.052)
Treatment 3 Years Later	0.017
	(0.052)
Treatment 4 Years Later	0.046
	(0.054)
Treatment 5 Years Later	0.041
	(0.053)
Treatment 6 Years Later	-0.0019
	(0.056)
Observations	105088
R-Squared	0.90

Notes: Here I run the main specification except that I weight the districts by their enrollment. Because of large outliers in enrollment, I exclude any districts that are ever in the top one percent of the number of students who take any test. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test. The regressions are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, the required vote share for each measure to pass, and a third level polynomial of the vote share. I include fixed effects for grade, test subject, calendar year, and school district. Test scores are normalized by grade, test subject, and school year.

Table 41: Placebos: Only Pre-Years

	Normalized Test Scores	Proficiency	Only One Election
Placebo 6 Years Before	-0.072	0.45	0.23
	(0.060)	(0.90)	(0.16)
Placebo 5 Years Before	-0.082	0.65	0.095
	(0.055)	(0.83)	(0.14)
Placebo 4 Years Before	-0.098*	-0.13	0.055
	(0.051)	(0.83)	(0.15)
Placebo 3 Years Before	-0.100**	-0.015	0.052
	(0.049)	(0.79)	(0.10)
Placebo 2 Years Before	-0.069	0.76	-0.025
	(0.046)	(0.74)	(0.12)
Observations	44404	44404	8389
R-Squared	0.83	0.43	0.75

Notes: Here I run the main specification for test scores but with only the years before the elections acting as placebos. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test. The regressions are clustered at the school district level. Controls are included for relative year, percentage designated as economically disadvantaged, the required vote share for each measure to pass, and a third level polynomial of the vote share. I include fixed effects for calendar year, grade, test subject, and school district. Test scores are normalized by grade, test subject, and school year.

Table 42: Probability of Passing a Bond Measure if Failed One Recently

	Probability
1 Years Post Failed Election	0.34^{***}
	(0.050)
2 Years Post Failed Election	0.24^{***}
	(0.052)
3 Years Post Failed Election	0.12^{***}
	(0.034)
4 Years Post Failed Election	0.11^{***}
	(0.038)
5 Years Post Failed Election	0.050
	(0.038)
6 Years Post Failed Election	0.085^{**}
	(0.036)
Observations	7552
R-Squared	0.55

Notes: Here I run the ITT specification on whether you pass a bond measure, but replace the variables for if a bond passed with if one failed. This gives the probability that you pass a bond measure given that you failed one recently. This uses a window of two years before to six years after each election. Variables include whether a bond passed in each relative year, a third level polynomial of the vote-share for each election threshold, and a control for the percentage of students classified as economically disadvantaged. Fixed effects are included for election window, relative year, and calendar year. Results are clustered at the district level. Election data comes from the California Election Data Archive.

Table 43: ITT Effect on Per-Student Capital Spending for Only Two-Thirds Threshold Elections: Local Linear RD
Local Linear RD of Difference in Per Student Capital Spending with Covariates: Two-Thirds Threshold

VARIABLES	Year of the Election	1 Year Later	2 Year Later	3 Year Later	4 Year Later	5 Year Later	6 Year Later
RD_Estimate	4,980	7,035	4,232	3,643	5,472	3,860	5,152
Observations	74	72	72	70	68	58	55
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
BW Type	mserd	mserd	mserd	mserd	mserd	mserd	mserd
Robust p-value	0.00691	6.99e-05	0.486	0.588	0.238	0.174	0.374
Order Loc. Poly.	1	1	1	1	1	1	1
Order Bias	2	2	2	2	2	2	2
BW Below Loc. Poly.	0.0422	0.0429	0.0614	0.0589	0.0306	0.0286	0.0367
BW Above Loc. Poly.	0.0422	0.0429	0.0614	0.0589	0.0306	0.0286	0.0367
BW Bias Below	0.0674	0.0751	0.0939	0.0859	0.0598	0.0560	0.0616
BW Bias Above	0.0674	0.0751	0.0939	0.0859	0.0598	0.0560	0.0616

Notes: This table shows the effect on per-student capital spending for only elections at the two-thirds threshold using a local linear RD of the change in spending since the year before the elections. I also includes covariates for the grade, test subject, election vote-share threshold, and percentage of economically disadvantaged students. I use a triangular kernal with a mserd bandwidth type and second order bias. I use the method described by Calonico, Cattaneo, Farrell, and Titiuni (Calonico et al. 2017). I use the distance between the vote-share and the cutoff for each election as the running variable. Election data comes from the California Election Data Archive. Spending data comes from the NCES Common Core of Data. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test

Table 44: ITT Effect on Normalized Test Scores for Only Two-Thirds Threshold Elections: Local Linear RD
Local Linear RD of Difference in Normalized Test Scores with Covariates: Two-Thirds Threshold

VARIABLES	Year of the Election	1 Year Later	2 Year Later	3 Year Later	4 Year Later	5 Year Later	6 Year Later
RD_Estimate	0.193	-0.0432	-0.00926	-0.0526	0.0364	-0.622	-2.211
Observations	1058	1033	1012	951	933	823	769
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
BW Type	mserd	mserd	mserd	mserd	mserd	mserd	mserd
Robust p-value	0.0538	0.939	0.761	0.927	0.640	0.00693	0.000273
Order Loc. Poly.	1	1	1	1	1	1	1
Order Bias	2	2	2	2	2	2	2
BW Below Loc. Poly.	0.0305	0.0448	0.0464	0.0417	0.0335	0.0263	0.0162
BW Above Loc. Poly.	0.0305	0.0448	0.0464	0.0417	0.0335	0.0263	0.0162
BW Bias Below	0.0588	0.0814	0.0913	0.0788	0.0694	0.0566	0.0344
BW Bias Above	0.0588	0.0814	0.0913	0.0788	0.0694	0.0566	0.0344

Notes: This table shows the effect on normalized test scores for only elections at the two-thirds threshold using a local linear RD of the change in normalized test scores since the year before the elections. I also includes covariates for the grade, test subject, election vote-share threshold, and percentage of economically disadvantaged students. I use a triangular kernal with a mserd bandwidth type and second order bias. I use the method described by Calonico, Cattaneo, Farrell, and Titiuni (Calonico et al. 2017). I use the distance between the vote-share and the cutoff for each election as the running variable. Election data comes from the California Election Data Archive. Spending data comes from the NCES Common Core of Data. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test

Table 45: ITT Effect on Per-Student Capital Spending for Only Fifty-Five Threshold Elections: Local Linear RD
Local Linear RD of Difference in Per Student Capital Spending with Covariates: Fifty-Five Threshold

VARIABLES	Year of the Election	1 Year Later	2 Year Later	3 Year Later	4 Year Later	5 Year Later	6 Year Later
RD_Estimate	1,268	3,731	2,942	1,624	2,257	-208.3	-169.5
Observations	376	300	292	253	249	184	154
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
BW Type	mserd	mserd	mserd	mserd	mserd	mserd	mserd
Robust p-value	0.185	0.0453	0.179	0.619	0.375	0.844	0.530
Order Loc. Poly.	1	1	1	1	1	1	1
Order Bias	2	2	2	2	2	2	2
BW Below Loc. Poly.	0.0479	0.0636	0.0562	0.0297	0.0307	0.0311	0.0339
BW Above Loc. Poly.	0.0479	0.0636	0.0562	0.0297	0.0307	0.0311	0.0339
BW Bias Below	0.0775	0.0945	0.0885	0.0469	0.0679	0.0537	0.0649
BW Bias Above	0.0775	0.0945	0.0885	0.0469	0.0679	0.0537	0.0649

Notes: This table shows the effect on per-student capital spending for only elections at the fifty-five percent level using a local linear RD of the change in spending since the year before the elections. I also includes covariates for the election vote-share threshold and percentage of economically disadvantaged students. I use a triangular kernal with a mserd bandwidth type and second order bias. I use the method described by Calonico, Cattaneo, Farrell, and Titiuni (Calonico et al. 2017). I use the distance between the vote-share and the cutoff for each election as the running variable. Election data comes from the California Election Data Archive. Spending data comes from the NCES Common Core of Data.

Table 46: ITT Effect on Normalized Test Scores for Only Fifty-Five Threshold Elections: Local Linear RD
Local Linear RD of Difference in Normalized Test Scores with Covariates:Fifty-Five Threshold

VARIABLES	Year of the Election	1 Year Later	2 Year Later	3 Year Later	4 Year Later	5 Year Later	6 Year Later
RD_Estimate	0.136	0.0461	0.0438	0.0918	0.122	0.300	0.147
Observations	6764	5267	5119	4259	4200	3073	2563
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
BW Type	mserd	mserd	mserd	mserd	mserd	mserd	mserd
Robust p-value	0.00994	0.321	0.386	0.498	0.343	0.0488	0.221
Order Loc. Poly.	1	1	1	1	1	1	1
Order Bias	2	2	2	2	2	2	2
BW Below Loc. Poly.	0.0383	0.0423	0.0723	0.0259	0.0250	0.0181	0.0256
BW Above Loc. Poly.	0.0383	0.0423	0.0723	0.0259	0.0250	0.0181	0.0256
BW Bias Below	0.0737	0.0793	0.123	0.0492	0.0447	0.0317	0.0509
BW Bias Above	0.0737	0.0793	0.123	0.0492	0.0447	0.0317	0.0509

Notes: This table shows the effect on normalized test scores for only elections at the fifty-five percent level using a local linear RD of the change in normalized test scores since the year before the elections. I also includes covariates for the grade, test subject, election vote-share threshold, and percentage of economically disadvantaged students. I use a triangular kernal with a mserd bandwidth type and second order bias. I use the method described by Calonico, Cattaneo, Farrell, and Titiuni (Calonico et al. 2017). I use the distance between the vote-share and the cutoff for each election as the running variable. Election data comes from the California Election Data Archive. Spending data comes from the NCES Common Core of Data. The standardized test scores come from the ELA test for grades two through eleven and the Mathematics test for grades two through six of the California Standards Test