Does Investing in School Capital Infrastructure Improve Student Achievement?

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Abstract: Within the research community, there is a vigorous debate over whether additional educational expenditures will lead to improved performance of schools. Some of the debate is an outgrowth of the lack of causal knowledge of the impacts of expenditures on student outcomes. To help fill this void, we examine the causal impact of capital expenditures on school district proficiency rates in Michigan. For the analysis, we employ a regression discontinuity design where we use the outcomes of bond elections as the forcing variable. Our results provide some evidence that capital expenditures can have positive effects on student proficiency levels.

Keywords: Economics of Education, Capital Expenditure, Regression Discontinuity Design

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1 Introduction

Capital expenditures represent a significant investment into education. The U.S. Department of Education reports that as much as $70 billion are spent in a year on public school construction and repairs (U.S. Department of Education, 2012, Table 205). Despite this substantial investment, many argue that further investments into facilities are needed as many students are educated in inadequate and crumbling facilities, especially in urban districts (Dejong and Clover, 2003).[1] In some cases, urban districts and state legislators have answered this call by investing billions of dollars to improve school facilities in places like Los Angeles, New Haven, and urban districts across New Jersey. In many other districts and states, policymakers have made more modest, but substantial investments into maintaining or augmenting existing facilities.

Despite the magnitude of these investments, little is known about the effectiveness of capital expenditures. The question of what impact these capital expenditures have on student outcomes is part of the larger debate of the impact of expenditures on student outcomes in general. Some argue that past investments into education have not led to significant returns. For instance, Eric Hanushek and Alfred Lindseth note that between 1960 and 2005, inflation-adjusted spending per pupil in the U.S. increased from $2,606 to $9,910, but was not accompanied by substantial improvement in national test scores, graduation rates, or the U.S.'s relative rankings on student outcomes among developed countries (Hanushek and Lindseth, 2009, pp. 45-46). Hanushek (1986, 1996, 2003) found, in a series of literature reviews, no consistent relationship between increased inputs and student performance on test scores. These findings could lead one to question whether the U.S. should continue to invest more resources in education.

[1] Advocates for greater investment includes 21st Century School Fund. See: http://www.21csf.org/csf-home/best/best.asp

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However, some researchers argue that much of the literature that Hanushek relied upon was not always rigorous (Ferguson and Ladd, 1996) and has not controlled for the possibility of reverse causality—i.e., policymakers often invest more in schools performing poorly in hopes of improving outcomes.[2] Advocates for additional educational resources often point to evaluations of the Tennessee STAR experiment which used a causal research design to randomly assign students to larger or smaller classes and found an inverse relationship between class size and student outcomes (Kruger, 1999; Kruger and Whitmore, 2001). While the Tennessee STAR experiment does provide some causal evidence of one input, the broader literature has provided little causal estimation of the relationship between student outcomes and educational inputs, including capital expenditures.[3]

Research on the impact of capital expenditures only emerged in the last 10 to 15 years. An initial question is why we expect an impact from capital expenditures. Some have speculated that capital investments would lead to safe and clean school environments, free from overcrowding with good lighting—all of which could make it easier to concentrate and lead to greater student and teacher morale and effort (Jones and Zimmer, 2001; Filardo et al., 2006). In addition, research has suggested that teachers put a premium on school facilities when making an employment decision (Horng, 2009), which suggests that schools with better facilities may be able to recruit better teachers. With these hypotheses in mind, researchers set out to examine the relationship between capital expenditures and student outcomes.

Much of the early research, while providing initial insights, did not fully account for the endogenous expenditures levels within districts—i.e., unobserved factors such as the communities'

[2] On the flip side, it is possible that districts that are willing to spend more have families and students engaged in the educational production process, and these unobservable characteristics are not captured in these same studies, leading to an upward bias in the estimated relationship.
[3] Other studies that provide credible results are Guryan (2001), Papke (2005), Papke (2008), and Hyman (2013).

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taste for education, which can drive both capital spending and student outcomes (Jones and Zimmer, 2001; Schneider, 2002; Picus et al., 2005; Blincoe, 2009). However, researchers are beginning to employ more causal designs. Two recent papers employed difference-in-differences approaches to examine the impact of large-scale construction projects of new schools in New Haven (Neilson and Zimmerman, 2011) and Los Angeles (Welsh et al., 2012) finding strong positive effects. Both papers provide strong insights into capital projects to replace old and decaying school buildings, but provide limited implications for smaller-scale projects, including maintenance and additions.

A recently published paper by Cellini, Ferreira, and Rothstein (2010) (henceforth, CFR) better encompasses the range of capital expenditures using a regression discontinuity design (RDD) to examine outcomes of districts in which capital expenditures are narrowly approved relative to districts in which capital expenditures narrowly fail. Theoretically, districts that marginally pass a bond should be similar both in observed and unobserved ways to those that marginally fail a bond. CFR's paper primarily focuses on the impact of capital expenditures on housing values, but also examines test scores and finds modest positive effect on student outcomes, but with some delays as the effects generally do not appear until six years after bond passage.

In this paper, we build on the CFR econometric approach and employ a "dynamic" RDD to obtain an unbiased estimate of capital expenditures on student outcomes in Michigan by examining bond referenda that narrowly pass and fail.[4] However, our paper takes a number of steps previously not taken to ensure the complexity of issuing bonds does not create any manipulation. Manipulation could occur for a variety of reasons, but the most important concern is whether the

[4] In an contemporaneous working paper, Martorell, McFarlin, and Stange (2015) examines whether successful passage of a bond leads to improvements in facility conditions, student attendance, and student achievement in Texas; they find no effects on student achievement.

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district learns from failed election and tries again with a modified proposal, or breaks up a bond proposal into multiple elections on the same day in hopes of gaining support for at least some of the capital expenditures. In the analysis, we take into account these possibilities by extending CFR dynamic sharp RDD to a dynamic fuzzy RDD. The fuzziness comes from the fact that districts which fail a bond can propose another one in the same year. Therefore, our paper adds to this literature by not only exclusively focusing on the impact of a wide array of capital expenditures on student achievement, but by also more explicitly discussing possible threats to the RDD approach. Focusing on the findings from our fuzzy RDD approach, we find that passing a bond increases reading proficiency by 2 to 6 percent five to seven years after passage.

2. Issuing Bonds in Michigan

Prior to 1994, Michigan primarily relied upon local property taxes to fund public education (Courant et al., 1995). In 1994, school districts began to rely more heavily on state funding of operating expenditures in schools with new legislation. Despite this policy change, funding of capital remained a local responsibility through a bond referendum in which a district must receive 50 percent approval from the electorate to approve a bond (Zimmer and Jones, 2005; Zimmer et al., 2011). Because Michigan school districts rely upon a local referendum to approve capital expenditures, we are able to use an RDD as an identification strategy to estimate the impact of capital expenditures on student outcomes. We summarize the referendum elections between 1996 and 2009 included in our data in Table 1. In the table, we provide some general trends in the number of bonds voted on per year, the average number of voters, percent of bonds passed, average bond amount, and for a subset of years in which we have the data, average repayment length millage rate, and whether the bond was financing a new building. As the table indicates, the number of proposed measures decreased substantially over the years, which may be the result of the decline

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in Michigan's economy since the early 2000s. The table also suggest that the typical bond vote share is near 50 percent as the average vote share hovers around 50 percent across the years. Finally, the average bond amount per pupil voted on is substantial ranging from a low of $6,087 per pupil in 2009 to $9,981 in 2005 (of course, these expenditures are amortized over a number of years as the bond is payed off).

Table 1: School Bond Measure Summary Statistics

Year	**No. of Bond**	**Average No. of Voters**	**Pct. of Passed**	**Avg. Vote Share (%)**		**Avg. Bond Amount per Pupil ($)**		**Avg. Repayment Year**	**Avg. Millage Rate (Amount per $1000)**	**Pct. of New Building**
				Mean	**SD**	**Mean**	**SD**			
1996	164	2536	51%	49.4	11.9	7312	5423			
1997	149	2497	43%	48.3	11.2	7764	5670			
1998	107	2644	41%	48.5	10.7	9472	7965			
1999	117	2097	48%	49.8	11.7	8348	6388			30%
2000	117	2364	49%	49.8	12.9	7694	5835	24.4	2.97	36%
2001	108	2469	63%	53.3	12.6	7487	6093	24.7	2.89	27%
2002	83	2560	59%	52.3	14.8	7882	6440	25.2	2.79	
2003	70	3361	39%	46.7	14.7	9820	10590	26.0	3.01	
2004	71	3034	63%	53.9	15.3	9243	6661	24.4	2.80	
2005	58	2558	40%	48.9	11.8	9981	10131	26.0	2.84	
2006	59	3740	44%	48.0	11.5	7771	6827			
2007	68	2660	47%	48.2	13.4	8033	8179			
2008	44	2320	57%	51.6	12.6	7598	4791			
2009	50	5386	70%	54.7	12.2	6087	6375			
Total	1265	2722	50%	50.0	12.7	8123	6907	25.0	2.89	32%

Note: Sample includes bonds with non-missing values in both passage and vote share. Date is the number of days from the first day of each year. Average bond amount per pupil is measured in constant year 2000 dollars.

We should also note that one of the features that make Michigan appealing for our analysis is that Michigan school districts are generally city/town based and are small relative to southern and western states, which often have countywide school districts. Because of the relatively small districts, with many districts only having one elementary, middle, and high school, the link between passage of a bond referendum and student achievement is more direct. These attributes make Michigan a strong place to evaluate the impact of capital expenditures.

3. Data and Analytical Strategy

3.1 Data Description

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We use data from the state of Michigan Department of Education and Treasury Department as well as the Common Core Data (CCD) from the National Center for Education Statistics (NCES).[5] These sources include information on bond election outcomes as well as district expenditures, demographics, and math and reading proficiency. We only examine school district elections and exclude any countywide elections because our outcome measure of student performance is at the district level.[6] In total, there are 577 districts holding 1,265 elections. In terms of student achievement we have 8,065 district-year observations.

Our measures of academic achievement are the 1996-2009 4th and 7th grade district-level reading proficiency rates.[7] Proficiency rates are the metric by which schools and districts are held accountable under No Child Left Behind, but they do have certain disadvantages. First, changes in proficiency rates do not necessarily capture changes in performance across all students as the change only captures the performance of those students who switch from non-proficient to proficient status or vice versa. Second, we prefer to use the proficiency rate at the actual school which experiences a change in capital expenditures rather than a district-wide measure. However, as noted previously, in Michigan, most districts contain only a few schools—in many cases—one elementary, one middle, and one high school. Therefore, there should be a strong link between an approved capital bond and capital expenditures for individual schools within the district.

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[5] Math and reading proficiency scores came from Michigan Department of Education: http://www.michigan.gov/mde/0,1607,7-140-22709_31168_31530---,00.html. Bond vote totals, amount, type, and date come Michigan's Treasury Department, which lists all bonds issued through the state bond loan program: https://treas-secure.state.mi.us/apps/findschoolbondelectinfo.asp. NCES CCD includes district financial and demographic data: http://nces.ed.gov/ccd/bat/

[6] There are very few countywide elections in the data and this exclusion should have little impact on our analysis.

[7] We do not have a consistent measure of math proficiency over the entire time horizon, and therefore, we focus exclusively on reading proficiency. However, for some our validity checks of RDD, we use baseline math proficiency as a check.

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Collectively, this suggests that a district-wide proficiency rate serves as a strong proxy for student achievement in the district.

3.2 Analytical Approach

In this section, we first describe the implementation of the RDD and then examine possible threats to our approach through a number of validity checks in section 4. We first display a formal model assuming a naïve approach with no possible endogeneity:

$y_{jt} = b_{j,t-\tau}\theta_{\tau} + e_{jt}$, for all τ (1)

where y_{jt} is the outcome (proficiency rates) in school district j observed in time t. It should be noted that the proficiency rates as an outcome could be a function of the current election or a previous elections. $b_{j,t-\tau}$ indicates whether the bond passed τ years ago in district j, and e_{jt} captures other factors affecting outcome at time t. If e_{jt} and $b_{j,t-\tau}$ are uncorrelated, θ_{τ} is identified as the effect of the bond passed τ year ago for each τ (the time period measured in years between bond passage and the year in which the outcome is measured). The model allows for proficiency rates to be a function of the length of time between bond passage and the period in which the outcome is measured, as indicated by subscript τ on θ. In other words, the model incorporates the possibility of lagged effects rather than immediate effects. In addition, this approach allows for the possibility of additional bonds after the initial bond referendum between the current time period, t and the time period in which the bond election occured $(\mathrm{t} - \tau)$. Therefore, the effect can be characterized as an intent-to-treat (ITT) effect, which provides insights into the policy decision of gaining successful passage of an initial referendum, not necessarily the effect of the capital expenditures solely associated with the passage of a single bond referendum as there could be later capital expenditures approved through a later referendum.

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However, because issuing a bond is unlikely to be exogenous (i.e., b is correlated with the error terms), we need to develop an identification strategy to address the possibility of endogeneity. Ideally, we would randomly assign capital expenditures to districts. However, this is impractical in this context as districts are required by law to pass a referendum by majority vote. Fortunately, elections present an opportunity to gain causal effects on capital expenditures using an RDD. As Lee (2008) notes, as long as there is some randomness in the outcome of the forcing variable determining treatment—in this case, election outcomes—then a close election can mimic random assignment. Therefore, using a panel data set with vote share as the forcing variable, we employ an RDD as the identification strategy. Using an RDD and controlling for a function of vote share, we can theoretically assume that the unobserved factors remaining in the error terms are uncorrelated with bond issuance at the 50% threshold and estimate equation (2).

In the analysis, we create stacked sample similar to CFR (2010) and Martorell et al. (2015). More specifically, we estimate Equation (2) using a stacked panel dataset. For each district j that has an election in time period t, we stack all observations across years for district j in a window around time period t. Then, for each election, we combine the stacked dataset into one large dataset covering the entire time period for the study (1996-2009).[8] Formally, equation (2) is specified as:

$$y_{jt\tau} = \sum_{\tau=-2}^{13} \left(\theta_\tau g^\tau b_{jt} + \omega_\tau g^\tau f\left(v_{jt}\right)\right) + \alpha_\tau + \beta_{t\tau} + \gamma_{jt} + e_{jt\tau}, \qquad (2)$$

where $y_{jt\tau}$ is the outcome in district j at time $t + \tau$, which is τ years after bond election b_{jt}. g^τ is a dummy variable indicating year gap τ, from -2 to 13. b_{jt} is a dummy variable indicating whether the election held in district j at time t was passed. Using an interaction between g^τ and b_{jt}, we are

[8] In the stacked panel dataset, a calendar year observation may be used more than once if the corresponding district holds multiple elections. For example, if a district held elections in 2000 and 2001, the observation in 2002 is in both windows of the two elections.

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able to identify the gap-specific effect of bond passage as θ_τ. $f(v_{jt})$ is a cubic function of vote share v_{jt} and ω_τ is its coefficient. α_τ is the year-gap fixed effect, $\beta_{t\tau}$ is the calendar-year fixed effect, γ_{jt} is the bond fixed effect, which captures any district-specific variation, and $e_{jt\tau}$ is the error term. θ_{-2}, θ_{-1}, ω_{-2} and ω_{-1} are restricted to zero.[9] By doing this, we assume that passing a bond does not have any effect in previous years relative to the time when the corresponding outcomes are measured.

While the above RDD approach has intuitive appeal, there are, nevertheless, threats to validity in the context of a bond referendum.[10] For instance, a savvy school district could break up a bond proposal into multiple elections on the same day in the hopes of gaining support for at least some of the capital expenditures (e.g., instead of including a new elementary school and athletic fields in one bond referendum, a savvy school district could put up two bond elections in the same day, one for the new elementary school and one for new athletic fields in the hopes that separate elections increases the odds that at least some of the expenditures are approved). In addition, there could be multiple elections that occur on different days—i.e., a district could have elections on separate days resulting from an initial referendum failing which could lead a district to alter the amount and type of capital to gain successful passage in a subsequent election.[11]

[9] Later in a balance check, we measure observable characteristics one year prior, for which only θ_{-2} and ω_{-2} are set to be zero.

[10] Furthermore, there is a technical issue of whether two elections with a similar difference in the number of yes and no votes should be treated differently. To illustrate, assume there are two elections, one with 19 voters with 10 voting yes and 9 voting no and second election with 999 voters with 500 voting yes and 499 voting no. Both elections would have a difference of one vote, but because voting share is typically used as the forcing variable in a RDD, these elections would be treated very differently as the vote share in the first election would 52.6 percent of voters voting yes while the second election would have 50.05 percent. A theoretical question is whether these elections should be treated differently.

[11] For example, assume a school district proposed a bond with a number of capital expenditures and narrowly missed passing the bond. The district then takes out one of the capital expenditures in a second proposed referendum a few months later and because the tax burden has been slightly reduced, the bond narrowly passes.

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As a result, we adopt CFR's dynamic framework to address multiple elections *across* years. We first examined the approach CFR used as a guideline for our own analysis. In the case of multiple elections within years, CFR uses the election with the largest vote share. While this approach allows for a sharp RDD, it can be problematic if some districts are strategic in their bond proposal, including the use of multiple elections as described above. To explore this possibility, we first compare the observable district characteristics of districts with single and multiple elections (Table 2).[12] Districts with multiple measures do have a smaller percentage of free-and-reduced lunch students, a lower current and total expenditure per pupil, and a higher percentages of white students—suggesting that districts that hold multiple elections are different from districts that do not, which we take into account when applying our analytical approach.

Table 2: School Districts Descriptive Statistics

	Proposed single measure only (1)	**Ever proposed multiple measures (2)**	**Ever proposed same day measures (3)**
Current expenditures per pupil	7312 (1754) [3536]	7002 (3901) [2520]	7002 (4227) [2128]
Total expenditures per pupil	8977 (3010) [3536]	8635 (4977) [2520]	8673 (5312) [2128]
Percent point, free lunch program participants	0.51 (1.75) [3489]	0.35 (0.54) [2468]	0.34 (0.58) [2128]
Construction capital outlay per pupil	684 (1737) [3536]	737 (1785) [2520]	760 (1781) [2128]
Instruction equipment capital outlay per pupil	43 (61) [3536]	40 (52) [2520]	39 (50) [2128]
Land and structure capital outlay per pupil	92 (454) [3536]	104 (505) [2520]	100 (422) [2128]
Total students	3663 (10204)	2653 (2914)	2909 (3089)

[12] Current expenditures per pupil, total expenditures per pupil, construction capital outlay per pupil, instruction equipment capital outlay per pupil and land and structure capital outlay per pupil are measured in constant year 2000 dollars.

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	[3538]	[2520]	[2128]
Percent of white students	85.60 (21.04) [3536]	91.16 (10.68) [2520]	91.58 (10.03) [2128]
4th grade math proficiency	66.65 (15.70) [931]	67.55 (14.47) [685]	68.92 (13.39) [574]
7th grade math proficiency	57.57 (16.41) [928]	59.78 (13.89) [684]	60.97 (13.13) [573]
4th grade reading proficiency	69.82 (18.48) [3389]	71.40 (17.72) [2481]	72.16 (17.24) [2090]
7th grade reading proficiency	60.77 (19.05) [3391]	63.39 (17.92) [2476]	64.18 (17.54) [2087]

Note: Statistics are based on the standard panel dataset, pooling together observations across years. Standard deviations are in parentheses and numbers of observations (district-year) are in brackets.

While Table 2 suggests districts with multiple elections are different than districts that do not, it does not necessarily mean that multiple elections within the data creates bias. To further explore this issue and whether we can use the same approach as CFR as a means of addressing the multiple election issue in the context of using Michigan data, we examine the distribution of the elections with the largest vote share in our data set as displayed in Figure 1. The figure indicates a peak right above the cutoff, which could be an indication of manipulation. To more formally examine this possibility, we use a McCrary's density test (McCrary, 2008). The test provides a discontinuity estimate of 0.43 with a standard error of 0.16, which suggests the CFR approach would likely lead to biased estimates using Michigan data.

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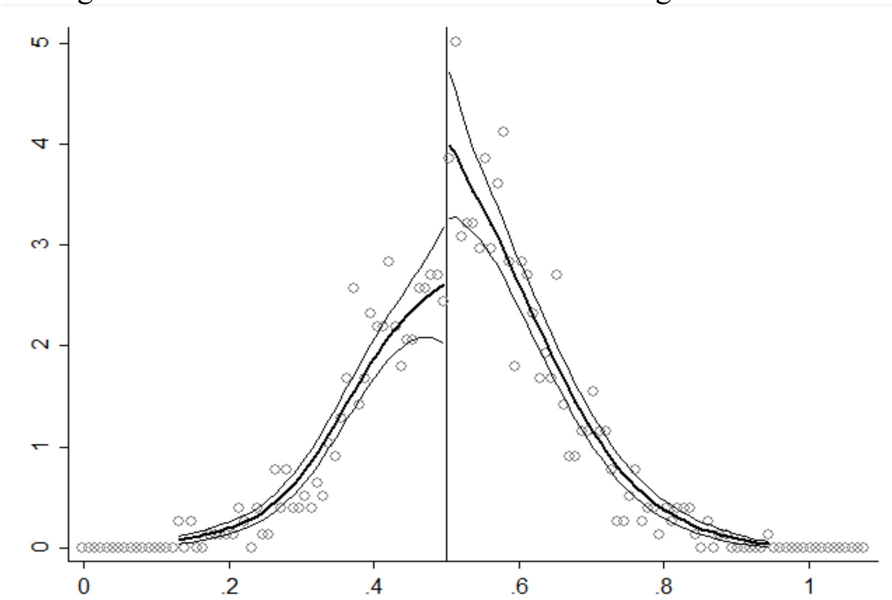
Figure 1: Distribution of Elections with the Largest Vote Share



Note: Sample includes only the election with the largest vote share if there are multiple elections in one year. The number of elections is 941. The x-axis represents vote shares and the y-axis represents density.

Therefore, we develop an alternative approach—i.e., we use the outcome of the first election in each year as our running variable. We argue using the first election is more likely to lead to unbiased estimates as districts have not had the opportunity to gain information from a first failed election.[13] To test this hypothesis, we again employ a McCrary's density test, which provides an estimate of 0.19 with a standard error of 0.15. From the test, we conclude that there is no evidence of unbalance in density around the cutoff using the first election vote share as the running variable.[14] Overall, this suggests that we cannot use the maximum vote share approach as our running variable. Instead, we use the alternative approach of the first election for within year multiple elections.[15]

[13] If there are multiple first elections on the same day, we choose the one with the largest vote share.
[14] The source of the discontinuous distribution in Figure 1 is from pooling multiple elections on different dates, and then selecting the one with the highest vote share. An alternative solution is choosing the first election during the whole study period, and then selecting the one with the highest vote share if there are multiple elections on the same day. In appendix Figure A2 we show that McCrary's density test also shows continuous density around the cutoff. We do not choose this alternative as it leaves us insufficient sample size.
[15] It should be noted that our sample size reduces a bit with this approach to 941 elections.

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4. Further Checks of the Validity of the Regression Discontinuity Design

4.1 Balance Check of Background Variables

To further examine the validity of the RDD approach, we conduct a balance check using pairwise comparisons. This balance check is analogous to randomized control trial studies that examine whether the randomization created a true random sample of treatment and control groups (Abdulkadiroglu et al., 2009; Cullen et al., 2006; Engberg et al., 2014, Zimmer and Engberg, in press). In these cases, researchers examine whether there are statistically significant differences in observable characteristics. If these observable characteristics are not statistically different more than what would be expected by random chance, then researchers typically conclude that the treatment and control samples are "balanced" both on observable and unobservable characteristics.

In Table 3, we first examine *all* districts in which bonds passed and failed, not just those near the cutoff. We do not expect the complete population of districts that passed and failed bonds to be similar. In fact, differences in observable characteristics among this population would suggest that a naïve estimate of the differences in outcomes across these two populations would be biased. Columns (1) and (2) provide means of observable district characteristics at baseline, i.e. one year prior to a bond passing (1) or failing (2). Column (3) compares the means of the two samples with a t-test. In general, the results in Columns (1), (2) and (3) suggest that districts that passed an election were more advantaged in most aspects (including student performance), which is not surprising as one would expect that more advantage districts are likely to pass a bond referendum. We next turn to the more critical population — those districts near the cutoff. Column (4) presents coefficient estimates from estimating Equation (2) treating each observable district characteristic as dependent variable. The results suggest that the RDD approach has balance among all of the observable characteristics, which provides support for our approach.

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Table 3: Pre-Estimation Balance Check, Background Variables

	Passed a measure (time t-1) (1)	Failed a measure (time t-1) (2)	Difference (t test) (3)	Year before measure (t-1) (4)
Current expenditures per pupil	6929 (1159) [468]	6765 (994) [357]	161** (77)	-75 (72) [9816]
Total expenditures per pupil	8103 (1792) [468]	7569 (1427 [357]	534*** (116)	-88 (148) [9816]
Construction capital outlay per pupil	332 (961) [468]	157 (714) [357]	175*** (61)	-26 (109) [9816]
Instruction equipment capital outlay per pupil	51 (66) [468]	56 (61) [357]	-6 (4)	-1 (8) [9816]
Land and structure capital outlay per pupil	86 (404) [468]	29 (125) [357]	58*** (22)	16 (42) [9816]
Percent point, free lunch program participant	0.49 (2.11) [457]	0.35 (0.48) [351]	0.14 (0.11)	0.15 (0.11) [9625]
Total students	3523 (6016) [468]	2700 (3321) [357]	823** (354)	-38 (48) [9820]
Percent of white students	89.13 (15.95) [468]	89.25 (14.89) [357]	-0.13 (1.09)	0.91 (0.96) [9816]
4th grade math proficiency	68.82 (13.81) [163]	65.73 (15.86) [179]	3.08* (1.61)	0.02 (2.30) [1635]
7th grade math proficiency	60.42 (14.85) [164]	56.98 (14.53) [178]	3.43** (1.59)	-1.48 (2.70) [1632]
4th grade reading proficiency	68.05 (17.40) [451]	61.80 (19.40) [352]	6.25*** (1.30)	-0.02 (1.00) [9652]
7th grade reading proficiency	58.75 (17.42) [452]	53.41 (18.46) [351]	5.34*** (1.27)	0.40 (1.06) [9650]

Note: Estimation are conducted using the stacked data set. Numbers in parentheses are standard deviations in Column 1 and 2, standard errors in Column 3, and clustered standard errors by school district in Column 4. Sample sizes for columns 1, 2 (number of elections) and 4 (number of district-year observations) are highlighted at the bottom of each cell. *, ** and *** indicate statistical significance at 90%, 95% and 99% confidence level, respectively.

As an additional step to create confidence in the RD approach, we conduct a falsification test in which we examine whether there is any difference in observed district characteristics the year before an election relative to the outcomes two and three years after the election. While we do not display the results here to conserve space, in each case, as expected, we get similar results

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as Table 3; for most outcomes, we found no significant jump at the threshold in the year before the election or any jump relative to two and three years after the election, providing support for the validity of our research design.[16]

4.2 Other possible threats

So far, we have examined baseline demographic, financial, and proficiency levels of the districts near the threshold and discussed and addressed the possible threat of multiple elections, but have not discussed other possible threats. In addition to deciding whether to split a capital project into multiple referenda or altering previously failed bond referendum, districts have control over (1) the amount of the bond, repayment years, and millage rate, (2) type of capital expenditure, e.g. new facilities, maintenance, add-ons, or (3) when the election actually occurs (is the referendum part of general election such as a presidential election or special election). Each of these characteristics could affect the outcome of an election. However, the district would have to be able to precisely estimate the number of votes needed for passage to invalidate the RDD. Nevertheless, we want to provide assurance that these threats do not apply. Therefore, in this section, we examine the bond amount per pupil and repayment time (i.e., length of the bond), which along with the taxable local property, determine the tax burden. We also examine the type

[16] The results are available upon request. To conduct the analysis, we create figures where the outcomes are plotted by the vote share on the x-axis. The 50% threshold is normalized to 0 and each unit bin represents 5% relative percentage of yes. There are 20 bins, and half of them are on the left side of the threshold. The y-axis represents the average of corresponding outcomes conditional on year fixed effects. The coefficients of bins (β) are estimated in the following regression:

$$y_i = \alpha_t + \sum_{j=1}^{20} \beta_j b_j + \varepsilon,$$

where α_t is the year fixed effect, b_j is the jth bin. y_i is the outcome in i relative year to the election.

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of proposed construction (e.g., new building, maintenance, add-ons) as well as the referendum year, election date and number of voters.[17]

Table 4 presents the balance check of bond characteristics for *all* bonds in columns 1-3. Again, because this is a balance check of all elections, differences would be expected. It is clear that a bond with a smaller amount per pupil, shorter repayment time and smaller millage rate is more likely to be passed, which suggests that districts can alter these characteristics of bonds to gain passage of a referendum. However, when we check the RDD estimates (Column 4) using Equation (2), there is no systematic difference between passed and failed bonds. Again, this provides some support for our RDD approach.

Table 4: Pre-Estimation Balance Check, Bond Characteristics

	Passed a measure (time t) (1)	**Failed a measure (time t) (2)**	**Difference (t test) (3)**	**Year of measure (t) (4)**
Year of referendum	2001.4 (4.0) [530]	2000.7 (3.9) [411]	0.7*** (0.3)	0.54 (0.38) [2110]
Bond amount per pupil	8062 (5889) [530]	11377 (7629) [411]	-3315*** (441)	239 (1405) [2110]
Repayment time	24.5 (5.06) [216]	26.47 (3.91) [136]	-1.97*** (0.51)	-2.51 (3.50) [855]
Millage rate	2.90 (1.74) [216]	4.24 (1.89) [135]	-1.35*** (0.20)	-0.21 (1.06) [854]
New building	0.34 (0.48) [108]	0.43 (0.50) [91]	-0.09 (0.07)	-0.16 (0.29) [437]
Election date	187.8 (85.36) [530]	169.4 (86.70) [411]	18.43*** (5.65)	15.37 (26.83) [2110]
Number of voters	2748 (5767) [530]	2659 (3121) [405]	89.6 (317.1)	418.0 (338.4) [2096]

Note: Estimation are conducted using the stacked data set. Numbers in parentheses are standard deviations in Column 1 and 2, standard errors in Column 3, and clustered standard errors by school district in Column 4. Sample sizes for columns 1, 2 (number of elections) and 4 (number of district-year observations) are highlighted at the bottom of each cell. *** indicates statistical significance at 99% confidence level.

[17] Districts can propose multiple types of construction in one bond proposal. We should note that we only have the types of expenditures for three years of our data. Election date is measured as the number of days from Jan. 1 of each year.

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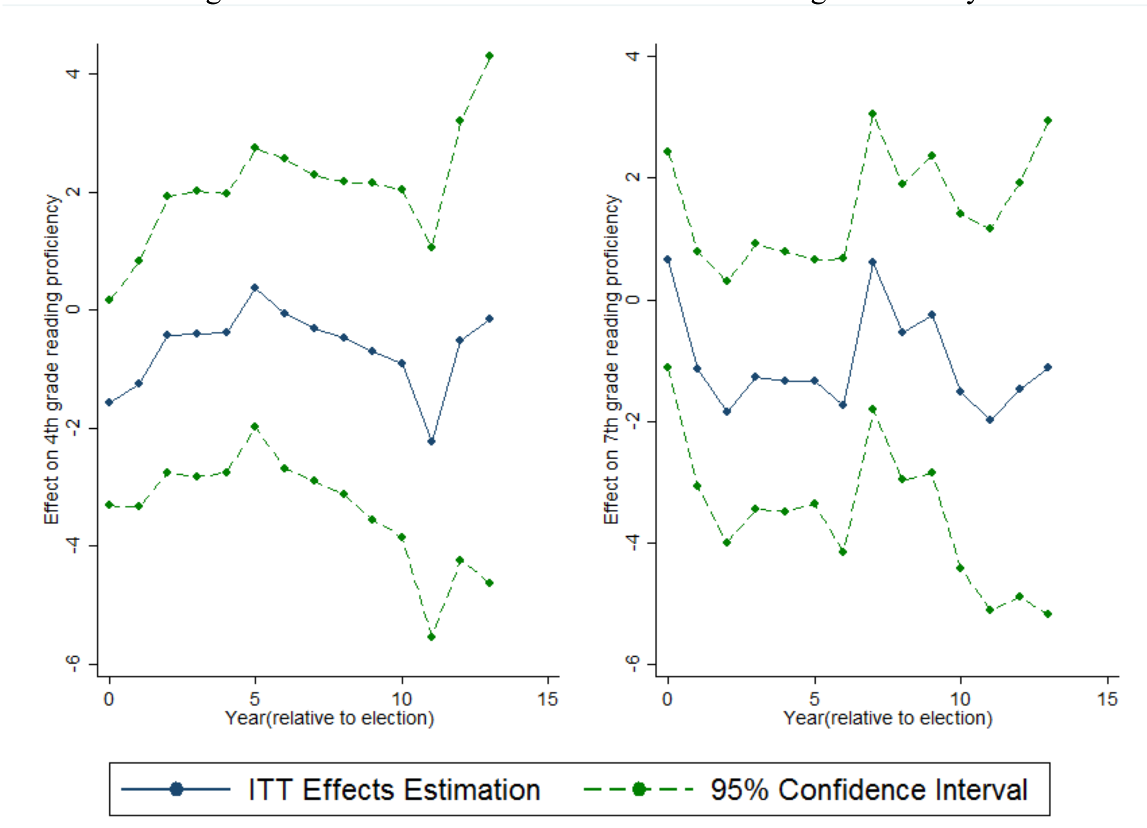
5 Effect of Passing Bond on Achievement

5.1 ITT Estimation of Initial Bond Passage

For our analysis, we first estimate our ITT model, which gives us the effect of passing the first bond in a year without controlling for subsequent elections, using Equation (2) with observations $-2 \leq \tau \leq 13$.[18] Standard errors are clustered by school district. Figure 2 presents the estimated ITT effects on 4th and 7th grade reading proficiency.

Figure 2: ITT Effects on 4th and 7th Grade Reading Proficiency



Note: Graphs show coefficients by years since election, which are specified as θ_τ in Equation (2), and the corresponding 95% confidence intervals. CIs are based on clustered standard errors by school district.

Overall, the analysis suggests little effect of passing a bond on subsequent reading proficiencies. This indicates that the policy of investing in infrastructure via referendum in itself

[18] We also fit the model with all observations $-13 \leq \tau \leq 13$ and get similar results.

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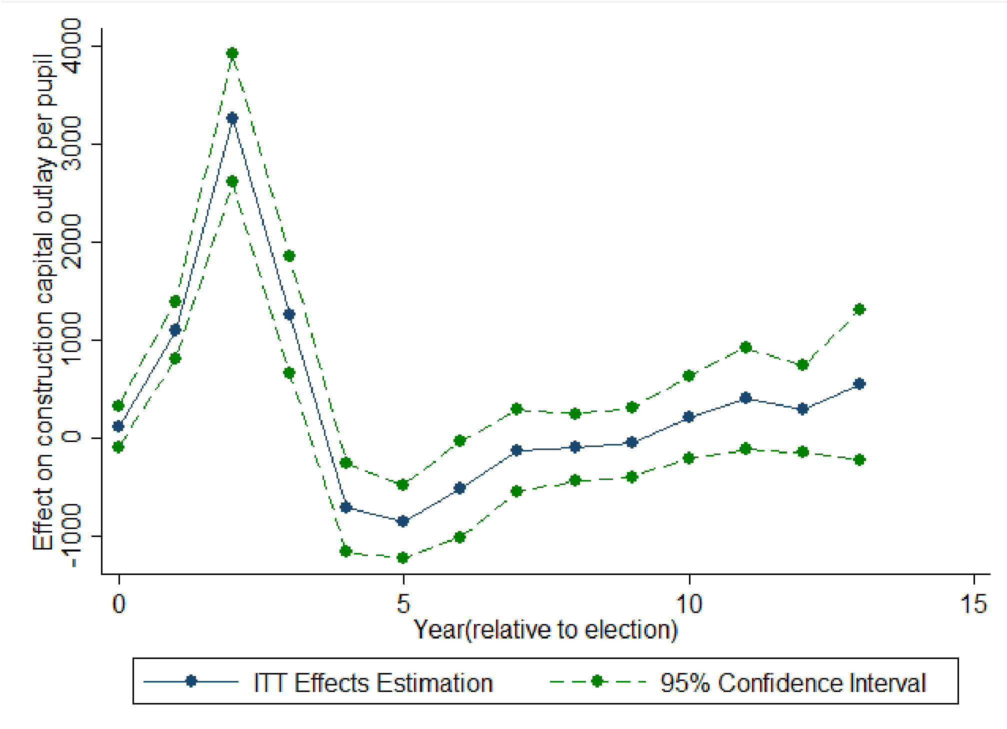
will not necessarily lead to improved student achievement. One possible reason is that the passed initial bond does not increase the capital expenditure as proposed. To explore this possibility, we estimate the ITT effect of passing a bond on the construction capital expenditures using the same estimation strategy. Figure 3 shows that the construction capital expenditures do increase significantly in the short run.

Figure 3: ITT Effect on Construction Capital Expenditure per Pupil



Note: Graphs show coefficients by years since election, which are specified as θ_τ in Equation (2), and the corresponding 95% confidence intervals. CIs are based on clustered standard errors by school district.

Another possible reason is that passing an initial bond may reduce the likelihood of passing a subsequent bond. We use the same specification as Figure 3 to estimate the ITT effect of passing a bond on the possibility of passing subsequent bonds, which is shown in Figure 4. As the figure suggests, a school district passing an initial bond is less likely to pass another bond in the short term. This result is also consistent with the negative effects on construction capital expenditure from year 4 to 8 in Figure 3.

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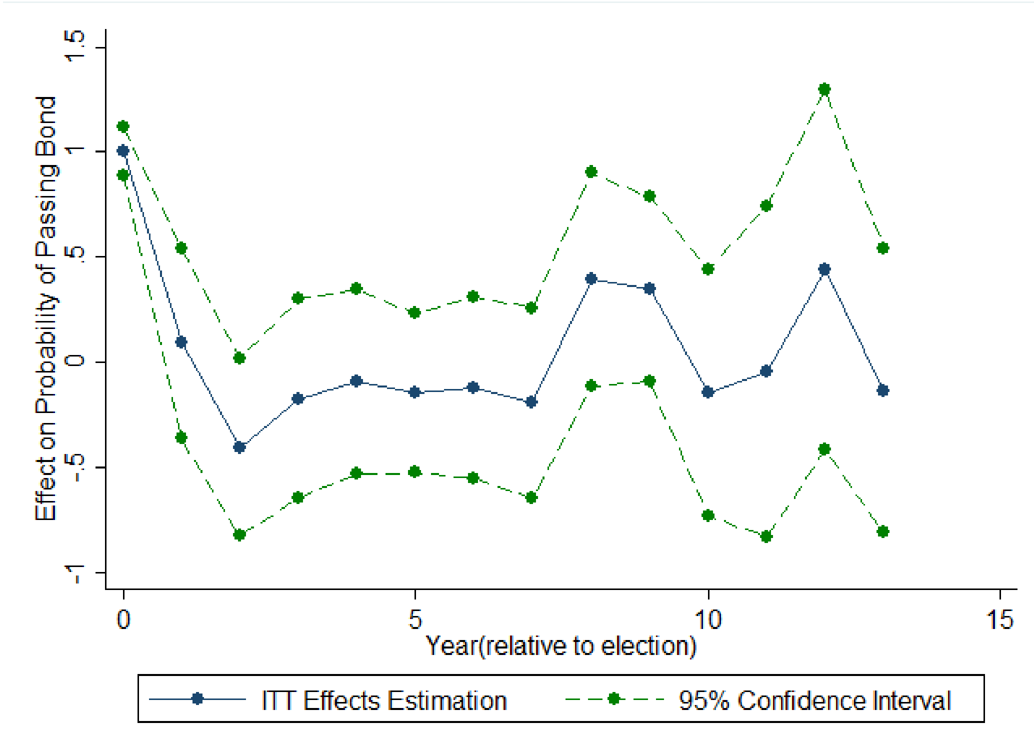
Figure 4: ITT Effect on Possibility of Passing Subsequent Bonds



Note: Graphs show coefficients by years since election, which are specified as θ_τ in Equation (2), and the corresponding 95% confidence intervals. CIs are based on clustered standard errors by school district.

We next explore the effect of passing each individual bond rather than the initial bond by taking into account the effect of the initial bond on the probability of passing subsequent bonds, which gives us a greater sense of the actual effect of capital expenditures.

5.2 Effect of Bond Passage Controlling for Subsequent Elections

In our ITT analysis, we use initial election results to define treatment and control groups. However, there are districts that fail to pass an original election, but eventually pass a bond in a subsequent election. In other words, there are districts within the control group that eventually are "treated". Therefore, it is useful to estimate the effect of bond passage on outcomes, taking into account the possibility of subsequent capital expenditures. To do this, we conduct an analysis that is analogous to a "fuzzy" RDD approach, where treatment is not fully determined by the election

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outcome and can be described as treatment-on-the-treated (TOT). More specifically, we follow CFR's one step approach in which the bond passage is defined as a district having at least one bond referendum passed in one year, which would be highly correlated with whether the vote share of the first election in the same year is greater or equal to 50%. However, this relation is not deterministic—i.e., we define "bonds" as passed with vote share less than 50% when a later bond passed in the same year, which gives the analysis its "fuzzy" nature.

To estimate the relationship between student achievement and passing each individual bond in the TOT framework rather than the initial bond in the ITT framework, y_{jt} can also be expressed as a function of all bond passages before t and other relevant variables.

$$y_{jt} = \sum_{\tau=0}^{\bar{\tau}} b_{j,t-\tau}\theta_\tau + \sum_{\tau=0}^{\bar{\tau}} e_{j,t-\tau} \quad (3)$$

where τ and $b_{j,t-\tau}$ are defined in the same way as before, and $e_{j,t-\tau}$ captures all of the other factors affecting outcome τ years ago. If $b_{j,t-\tau}$ is uncorrelated with $\sum_{\tau=0}^{\bar{\tau}} e_{j,t-\tau}$, θ_τ is identified as the TOT effect. It captures the "pure" effect of bond $b_{j,t-\tau}$, conditional on bond issuance history from $\tau = 0$ to $\tau = \bar{\tau}$. More specifically, we estimate the following model:

$$\begin{cases} b_{j,t-\tau} = 1\{v_{j,t-\tau} \geq v_{j,t-\tau}^*\}\phi_\tau + m_{j,t-\tau}\beta_\tau + f\left(v_{j,t-\tau}, \omega_{t-\tau}\right) + \eta_j + \varphi_t + \varepsilon_{jt}, & (4) \\ y_{jt} = \sum_{\tau=0}^{\bar{\tau}} \left(b_{j,t-\tau}\theta_\tau + m_{j,t-\tau}\alpha_\tau + f\left(v_{j,t-\tau}, \omega_{t-\tau}\right)\right) + \lambda_j + \kappa_t + \mu_{jt}, & (5) \end{cases}$$

where $m_{j,t-\tau}$ and $b_{j,t-\tau}$ indicate whether at least one bond was proposed and passed τ years ago. $1\{v_{j,t-\tau} \geq v_{j,t-\tau}^*\}$ is the indicator of whether the vote share of the first election τ years ago exceeds the threshold. Equations (4) and (5) jointly identify the TOT effect of passing at least one bond τ years ago for the district that would not pass any election in a year when the first one was failed. On top of the ITT effect identified by Equation (2), the effect identified by Equations (4) and (5) takes into account the nondeterministic relation between the first bond and the treatment of passing

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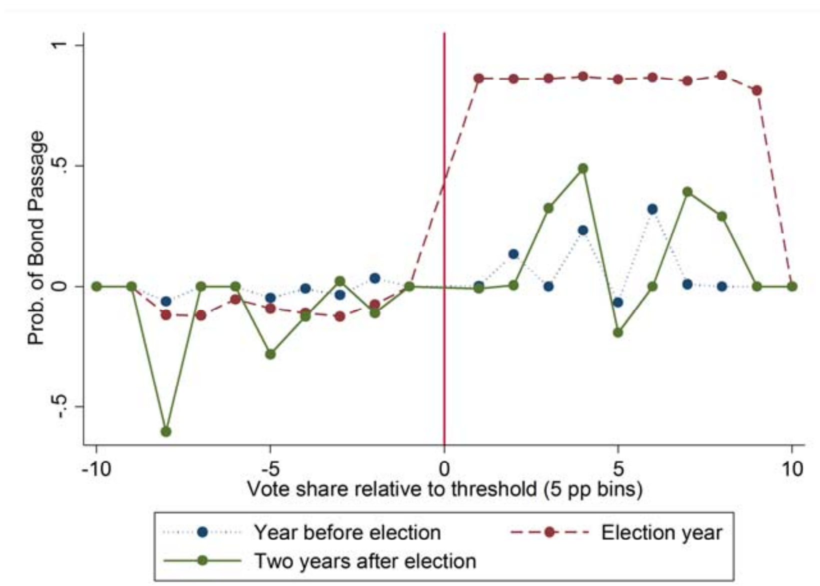
at least one bond in a year, and also controls for elections during subsequent years. In this model, the coefficient of θ_τ is the estimate of bond passage, holding subsequent elections constant.

To examine whether the relationship between indicator $1\{v_{j,t-\tau} \geq v^*_{j,t-\tau}\}$ and bond passage $b_{j,t}$ is deterministic, we graph whether the probability of passing a bond discontinuously changes with a magnitude less than 1 across the threshold of the vote share. Figure 5 shows that the probability of passing a bond jumps substantially at the threshold, but the magnitude is less than 1, which provides some support for the fuzzy RDD approach. In addition, while not shown here, we also conducted a falsification test to examine whether there was a discontinuity either one year earlier or two years later and found no such discontinuity, again providing support for the fuzzy RDD approach.

Figure 5: Graphic Checks of Probability of Passing an Election



Note: Elections with vote share of 50.001% to 55% are in bin 1, elections with vote share of 45.001% to 50% are in bin -1, etc. Each point represents the average of corresponding variable conditional on year fixed effects. The value of bin -1 is normalized to 0. So, for instance, if the estimated average value for 45-50% bin is a .2 probability, then any probability less than .2 would receive a negative value.

For the TOT analysis, we first verify that a successful bond passage actually leads to increased capital expenditures by examining whether a passing bond indeed increased the

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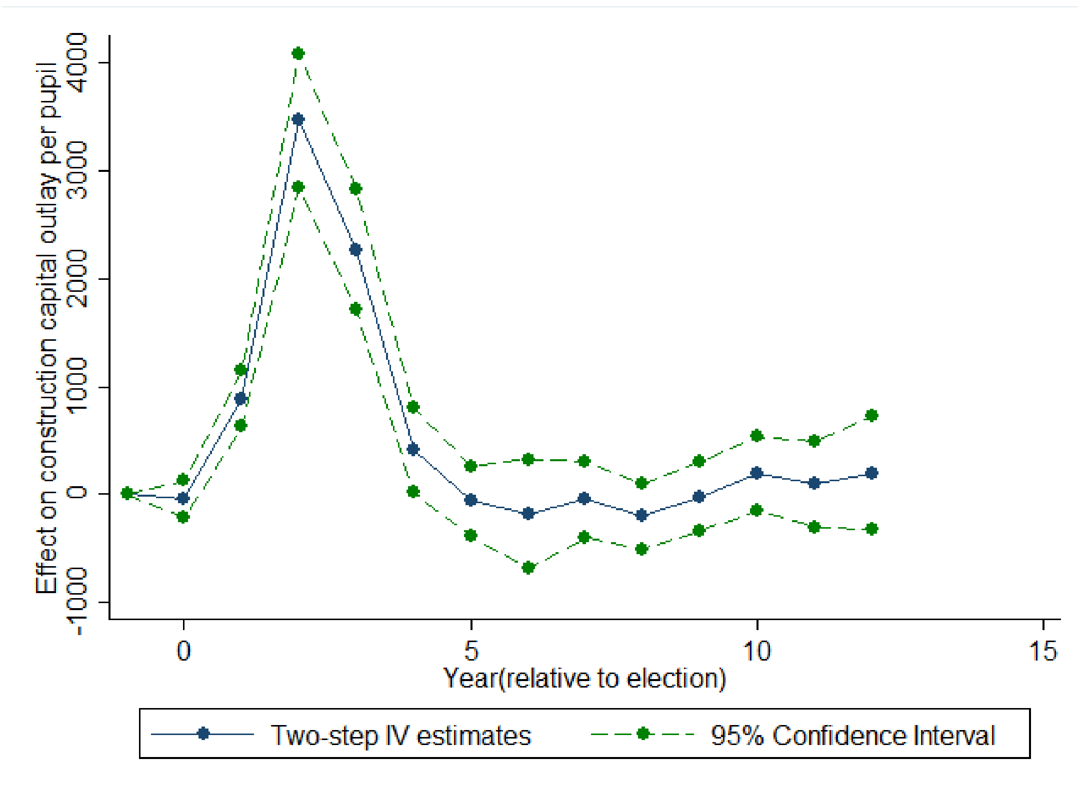
construction capital in the subsequent years. Figure 6 presents the TOT effect of passing a bond on aggregate construction capital outlay per pupil, which is the sum of construction capital and land capital outlays per pupil.[19] Although not shown here, the impact of bond passage on total expenditure shows a similar trend with the impact on aggregate construction capital in Figure 6. Overall the authorized bond suggests that a bond passage does increase total expenditure and capital expenditures per pupil, as expected.

Figure 6: The Impact of Bond Passage on Aggregate Construction Capital Outlay per Pupil, by Years since the Election



Note: Graphs show coefficients by years since election, which are specified as θ_τ in Equation (5), and the corresponding 95% confidence intervals. CIs are based on clustered standard errors by school district.

Given that we have established a relationship between bond passage and capital expenditures, we present our TOT results of bond passage on the reading proficiencies in Column

[19] TOT is estimated as a two-step fixed effect model (Papke and Wooldridge, 2008), with $1\{v_{j,t-\tau} \geq v^*_{j,t-\tau}\}$ serving as the instruments for $b_{j,t}$ in the first stage. Standard errors are clustered by school districts for both estimations and obtained by bootstrap with 1000 repetitions.

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(1) and (2) of Tables 5. For 4th grade proficiency, we observe a statistically significant estimate starting in year 5. In subsequent years, estimates are either marginally significant or marginally insignificant with similar magnitudes of increased proficiency of 2-5 percent. For 7th grade proficiency, we find fairly consistent significant effects of 3-6 percent starting in year 7. Overall, we do find some evidence of positive effects in both 4th and 7th grade reading, but the positive effects are more consistent in 7th grade reading. So while the ITT results suggest that successfully passing a bond does not guarantee improved student performance as passing one initial bond may reduce the likelihood of passing a later bond, the actual resources generated from passing an individual bond, as estimated by TOT, is positively related to long-term effects.[20]

Table 5: TOT on 4th and 7th Grade Reading Proficiency

	Bond Passage		**Authorized Bond Amount, $1000**	
Relative year τ	**4th Grade Reading Proficiency (1)**	**7th Grade Reading Proficiency (2)**	**4th Grade Reading Proficiency (3)**	**7th Grade Reading Proficiency (4)**
0	-1.45 (1.10)	1.72 (1.28)	-0.16 (0.12)	0.18 (0.15)
1	-0.61 (1.21)	0.09 (1.29)	-0.06 (0.13)	0.01 (0.14)
2	0.56 (1.40)	-0.61 (1.50)	0.06 (0.16)	-0.06 (0.17)
3	0.73 (1.28)	0.39 (1.43)	0.09 (0.14)	0.04 (0.16)
4	1.21 (1.23)	0.75 (1.43)	0.14 (0.14)	0.08 (0.15)
5	2.43** (1.24)	1.26 (1.57)	0.27* (0.14)	0.13 (0.17)
6	2.13 (1.40)	1.44 (1.63)	0.24 (0.15)	0.15 (0.18)
7	1.47 (1.35)	3.86** (1.74)	0.18 (0.15)	0.40** (0.19)
8	2.19* (1.27)	3.53** (1.72)	0.26* (0.14)	0.40** (0.19)
9	1.87 (1.53)	3.92** (1.95)	0.22 (0.17)	0.42* (0.22)
10	2.60 (1.63)	3.96* (2.17)	0.31* (0.19)	0.43* (0.25)

[20] In Appendix Figure A1 we show the graphic analysis of test scores in a similar way as Figure 5. The figure shows that achievement is improved in districts where bond is narrowly passed in year 5, 8 and 12, which is consistent with our main results. We should note that the figure illustrates effects without controlling for the fuzziness of bond passage in a year or subsequent bond elections. Therefore, the effects controlling for them could be even larger.

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11	3.08 (1.89)	4.71** (2.15)	0.36 (0.23)	0.55** (0.27)
12	4.91** (2.33)	5.62** (2.30)	0.63** (0.32)	0.67** (0.31)
13	4.97 (3.50)	4.01 (3.24)	0.68 (0.68)	0.53 (0.60)

Note: Numbers in parentheses are clustered standard errors by school district. Sample sizes are 7244 for 4th grading reading proficiency and 7219 for 7th grading reading proficiency. * and ** indicate statistical significance at 90% and 95% confidence level, respectively.

The TOT analysis above does not provide information about the relationship between the *bond amount* and *student achievement*. Therefore, we use the same specification to estimate the TOT effect of *passing a bond* to examine the relationship between authorized *bond amount* and achievement. Columns (3) and (4) of Table 5 present the results for 4th and 7th grade reading proficiency respectively. Again, the results for 7th grade reading are more consistently significant, but overall, the analyses suggest that the authorized bond amount has a long-term effect on achievement. In terms of interpretation, a $1000 increase in authorized bond amount will increase 4th grade proficiency by 0.2-0.6 percent after 5 to 8 years and increase 7th grade proficiency by 0.4-0.7 percent after 7 to 12 years.[21]

6. Linking Capital Expenditure to Achievement

The positive effect of passing a bond on subsequent achievement is likely to be the result of more total expenditure, which can be decomposed further to construction and land acquisition. Total expenditure is an educational input that is directly affected by a passing bond. There could be other inputs that are also affected by the bond. Pooling those unobserved inputs together, we have the following decomposition:

[21] Despite the fact that we use an alternative identification strategy to CFR, our results are similar to CFR. More specifically, CFR estimate the effect of passing the bond with the highest vote share in a year for both ITT and TOT, while for ITT we estimate the effect of passing the first bond in a year and for TOT we estimate the effect of passing at least one bond in a year.

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$$\theta_\tau = \frac{\partial y_{jt}}{\partial b_{j,t-\tau}} = \sum_{\pi=0}^{\tau} (\frac{\partial y_{jt}}{\partial TE_{j,t-\tau+\pi}} \frac{\partial TE_{j,t-\tau+\pi}}{\partial b_{j,t-\tau}} + \frac{\partial y_{jt}}{\partial U_{j,t-\tau+\pi}} \frac{\partial U_{j,t-\tau+\pi}}{\partial b_{j,t-\tau}}),$$

where TE_{jt} is the total expenditures in school district j at time t and U_{jt} denotes all of the other unobserved inputs that are affected by a passing bond. What we can identify is the pooled effect of total expenditure and unobserved inputs on achievement.

One potential input/consequence of a passed bond other than total expenditure is a different demographic population attracted by the capital expenditures—e.g., better facilities attract higher performing students. To examine this threat, we examine three variables in our data: total students, percent of students enrolled in free lunch program and percent of white students, and find no significant effect of bond passage on any of these variables. Therefore, it seems unlikely that our main finding is due to demographic change. Moreover, even if there are changes in unobserved variables, we can still interpret the estimated effect as the total effect of capital expenditure, as long as those unobserved variables can only be indirectly affected by the passing bond through increased capital expenditure, which indicates

$$\frac{\partial U_{j,t-\tau+\pi}}{\partial b_{j,t-\tau}} = \sum_{\delta=0}^{\pi} \frac{\partial U_{j,t-\tau+\pi}}{\partial TE_{j,t-\tau+\pi-\delta}} \frac{\partial TE_{j,t-\tau+\pi-\delta}}{\partial b_{j,t-\tau}}$$

The estimated effect of total expenditures is the sum of direct effects of total expenditures and indirectly effects through unobserved inputs. We can decompose the effect of total expenditure in a similar way into the effect of construction capital and other expenditures.

$$\frac{\partial y_{jt}}{\partial TE_{j,t-\tau}} = \sum_{\pi=0}^{\tau} \left(\frac{\partial y_{jt}}{\partial CE_{j,t-\tau+\pi}} \frac{\partial CE_{j,t-\tau+\pi}}{\partial TE_{j,t-\tau}} + \frac{\partial y_{jt}}{\partial UE_{j,t-\tau+\pi}} \frac{\partial UE_{j,t-\tau+\pi}}{\partial TE_{j,t-\tau}} \right).$$

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Again, without any assumptions, we estimate the pooled effect of construction capital and other inputs that are also affected by a passing bond. In addition, with the assumption of no direct effect of other expenditures, we estimate the total effect of construction capital.

Previously, we showed that passing a bond increases both total expenditures and construction capital in the first four years. Therefore, we use the sums of total expenditures and construction capitals during that period as the treatment variable, respectively, and estimate the average effect of the increased total expenditures and construction capitals over four years on achievement in subsequent years when the construction is complete.[22] More specifically, we estimate Equations (5) and (6) with expenditure as the outcome in the first stage. Table 6 shows that an increase of $1,000 of total expenditure increases 4th and 7th grade proficiency by 0.2-0.7 and 0.4-0.8 percentage points respectively, although the results are most consistently significant in 7th grade reading. The results for construction capital are similar.

Table 6: Effect of Total Expenditure and Construction Capital on 4th and 7th Grade Reading Proficiency, per $1000

	4th grade reading proficiency		**7th grade reading proficiency**	
Relative year τ	**Total Expenditure**	**Construction Capital**	**Total Expenditure**	**Construction Capital**
4	0.15 (0.16)	0.17 (0.18)	0.10 (0.18)	0.11 (0.20)
5	0.31* (0.17)	0.35* (0.18)	0.16 (0.19)	0.18 (0.21)
6	0.27 (0.19)	0.30 (0.21)	0.18 (0.20)	0.20 (0.22)
7	0.19 (0.18)	0.21 (0.20)	0.49** (0.23)	0.55** (0.24)
8	0.28* (0.17)	0.31* (0.18)	0.45** (0.22)	0.50** (0.24)
9	0.24 (0.20)	0.27 (0.21)	0.50** (0.25)	0.56** (0.27)
10	0.33 (0.21)	0.37 (0.23)	0.50* (0.27)	0.56* (0.29)
11	0.39 (0.25)	0.44 (0.27)	0.60** (0.28)	0.67** (0.30)
12	0.63**	0.70**	0.72**	0.80**

[22] We assume that before the construction is finished, any precedent investment has no effect on achievement. Therefore, what we estimate is the effect of the value of a whole new infrastructure rather than the effect of investment in each year during the construction, and we can only estimate such effect in 9 subsequent years.

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	(0.32)	(0.34)	(0.31)	(0.33)
13	0.63 (0.43)	0.71 (0.46)	0.51 (0.41)	0.57 (0.44)

Note: Numbers in parentheses are clustered standard errors by school district. Sample sizes are 7746 for all of the estimations. * and ** indicate statistical significance at 90% and 95% confidence level, respectively.

7. Sensitivity Analysis

To provide further support for our results, we conduct a number of sensitivity analyses. First, we check whether our main results are sensitive to the approach of dealing with multiple elections, including multiple elections on either the same or different days. In Table 7, we exclude multiple elections on different days (Columns (1) (3), (5) and (7)) and same-day elections (Columns (2), (4), (6) and (8)), which reduces the generalizability of our analysis, but also reduces the complexity multiple elections creates for our research design. Examining Columns (5) through (8), although the magnitudes are different, we consistently find no ITT effects and positive TOT effects similar to our primary results. Together, this suggests our analysis is robust to the exclusion restriction of multiple elections and same-day elections. We also find consistent results for 4th grading reading proficiency in Columns (1) through (4). Therefore, we conclude that our primary results are robust to the strategies we employ to address the multiple elections.

Table 7: Sensitivity Analysis

	4th Grade Reading Proficiency				**7th Grade Reading Proficiency**			
	ITT		**TOT**		**ITT**		**TOT**	
Relative year τ	**Single election (1)**	**No same-day election (2)**	**Single election (3)**	**No same-day election (4)**	**Single election (5)**	**No same-day election (6)**	**Single election (7)**	**No same-day election (8)**
0	-1.58 (1.32)	-2.09 (1.30)	-1.75 (1.75)	-1.36 (1.70)	1.88 (1.30)	1.81 (1.23)	1.65 (1.79)	2.31 (1.75)
1	0.35 (1.55)	-1.44 (1.53)	0.20 (1.86)	-0.47 (1.84)	0.29 (1.59)	-0.60 (1.41)	0.47 (1.81)	0.17 (1.81)
2	0.13 (1.73)	-0.91 (1.77)	0.53 (2.03)	1.14 (2.09)	1.79 (1.69)	0.42 (1.55)	1.89 (2.04)	1.26 (2.09)
3	0.56 (1.73)	-1.59 (1.93)	1.97 (1.83)	0.56 (1.95)	0.92 (1.70)	0.07 (1.52)	1.49 (1.93)	1.59 (1.90)
4	-0.33 (1.58)	-2.07 (1.90)	0.91 (1.70)	0.81 (1.85)	1.95 (1.76)	-0.14 (1.60)	3.28* (1.93)	1.99 (1.91)
5	1.55 (1.62)	-0.22 (1.87)	2.99* (1.78)	3.15* (1.89)	0.98 (1.66)	-0.43 (1.51)	3.04 (2.10)	2.80 (2.07)
6	3.51*	0.56	5.15**	4.72**	-0.19	-1.56	2.38	2.29

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	(1.89)	(2.18)	(2.22)	(2.19)	(2.12)	(1.92)	(2.27)	(2.36)
7	1.28 (1.97)	-1.58 (2.16)	2.52 (2.04)	1.85 (2.07)	2.11 (2.10)	0.20 (2.01)	5.35** (2.33)	4.90** (2.38)
8	1.90 (1.85)	-1.27 (2.18)	3.75** (1.83)	3.44* (1.95)	0.63 (2.10)	-0.97 (1.91)	3.69 (2.32)	3.61 (2.43)
9	2.40 (2.08)	-0.96 (2.36)	3.79* (2.23)	3.17 (2.36)	2.58 (2.23)	-0.64 (2.04)	6.10** (2.59)	4.96* (2.62)
10	1.13 (1.90)	-2.09 (2.33)	2.92 (2.03)	2.71 (2.33)	0.79 (2.45)	-2.65 (2.35)	3.89 (2.72)	2.79 (2.82)
11	-0.72 (2.31)	-4.74* (2.55)	3.92 (2.54)	3.15 (2.81)	2.57 (2.47)	-0.84 (2.51)	7.42*** (2.55)	7.20** (2.93)
12	0.33 (2.73)	-3.10 (2.86)	3.29 (2.53)	4.30 (3.02)	1.62 (2.21)	-1.44 (2.37)	5.68** (2.51)	5.80** (2.86)
13	-1.70 (3.56)	-4.33 (3.42)	0.25 (4.35)	2.80 (6.05)	-0.17 (3.30)	-2.13 (3.12)	2.83 (3.85)	4.39 (5.67)

Note: Numbers in parentheses are clustered standard errors by school district. Sample sizes are 4493, 5234, 4763, 5154, 4499, 5236, 4743 and 5132 respectively. In column (3) and (7) we estimate TOT effects because with single election only the relationship between vote share and bond passage is deterministic. * and ** indicate statistical significance at 90% and 95% confidence level, respectively.

We also conduct sensitivity analyses for our model specification. We first examine the functional form by estimating the effects of passing a bond with a linear control function and a quadratic polynomial, respectively. We also examine whether our main results are sensitive to introducing a sequential weighted estimation where each observation is weighted by the distance from the threshold.[23] The observation at the threshold is weighted by one and the weight decreases as the observation moves away from the threshold, which gives the greatest influence to the observations that are closest to the threshold. While we show the results in Appendix Table A1 and A2, overall, these sensitivity analyses produce similar results to our main results and suggest that our analysis is robust to the specification choices.

9. Conclusion

While many advocate for additional spending for public education, some researchers have questioned whether these additional resources will lead to improved results. Eric Hanushek wrote a series of papers in which he concludes that there is no systematic evidence that additional resources improve student outcomes (Hanushek, 1986, 1996, 2003). However, other scholars

[23] The weight function is $1/(1 + |v - 0.5|)$, where v is the vote share.

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believe it is obvious that additional expenditures improve educational outcomes and argue that simple correlations between student outcomes and expenditures do not take into account the changing demographics over time (Krueger, 1998) and that most of the current research has not dealt with unobserved district and school characteristics, which could bias their results (Ferguson and Ladd, 1996; Guryan, 2001). Nowhere is this debate more relevant than urban districts, which are grappling with chronically low-performing schools and many of these districts are considering whether they should invest in improved facilities.

We use data from Michigan school district referendum elections and employ a RDD to estimate the relationship between capital expenditures on student achievement and school expenditures. We explore alternatives to the CFR approach and estimate ITT and TOT effects. The results from these analyses suggest that capital investments are unlikely to have short-term effects on achievement, but could have long-term effects as the percent of students reaching proficiency in reading increased by 2 to 6 percent (equivalent to 0.1 to 0.3 standard deviations) five to seven years after passage of a bond referendum with more consistent significant results in 7th grade reading than 4th grade reading. We can only speculate as to why we observe a positive effect in the long-term, but not the short-term. Much of the delayed effect could be explained by the fact that it takes time to get the capital in place—it takes up to four years to get the capital in place.

While the positive long-term effect from the capital expenditures is not trivial, it is unclear whether the effect would be positive from a benefit-cost perspective as capital investments can be significant and the benefits may only materialize in the very long run—it may take five to ten years to observe positive impacts from the investment. In addition, one must wait until these students enter adulthood to observe positive benefits to society. Even with a low discount rate, the benefit-cost tradeoff may not be positive. Nevertheless, our analysis suggests that capital expenditures do

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have the potential to raise student achievement and further research is needed to analyze whether the investment is financially viable from a benefit-cost perspective.

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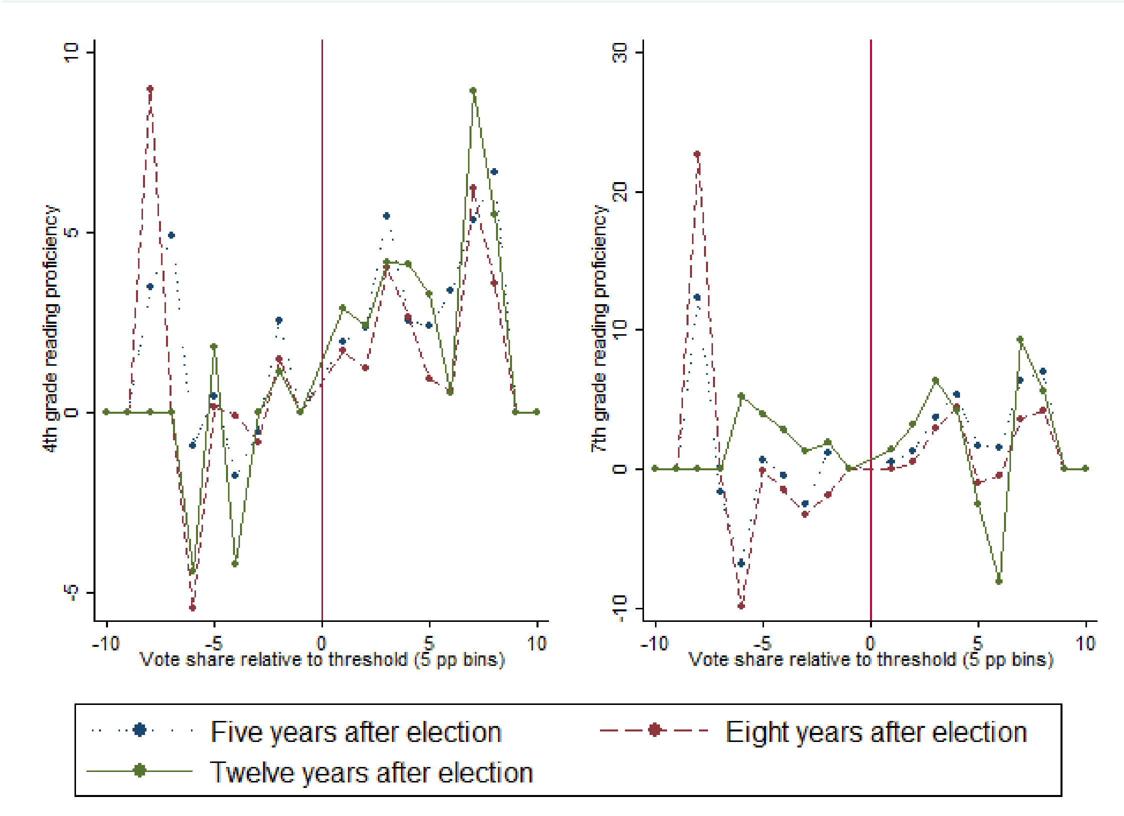
Appendix

Figure A1: Graphic Checks of 4th and 7th Grade Reading Proficiency



Note: Elections with vote share of 50.001% to 55% are in bin 1, elections with vote share of 45.001% to 50% are in bin -1, etc. Each point represents the average of corresponding variable conditional on year fixed effects. The value of bin -1 is normalized to 0.

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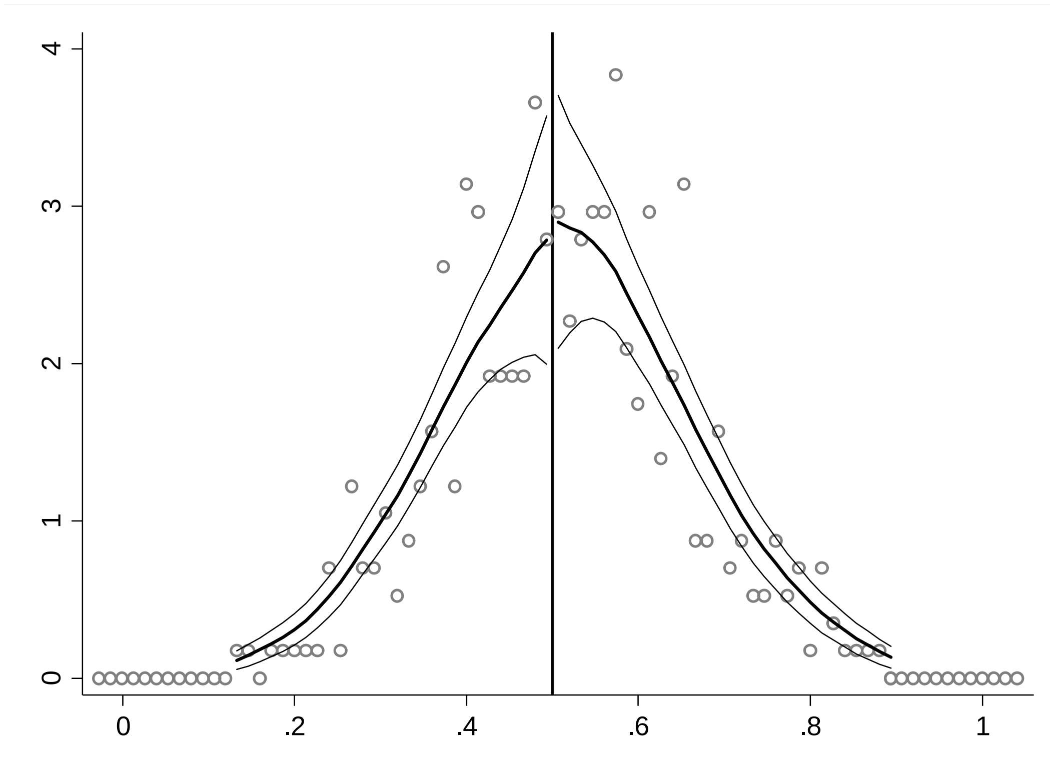
Figure A2: Distribution of Elections with the Largest Vote Share, First Election in the Whole Sample Period



Note: Sample includes only the election with the largest vote share if there are multiple elections during the whole sample period 1996-2009. The number of elections is 433. The x-axis represents vote shares and the y-axis represents density.

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Table A1: Sensitivity Analysis: Specification, 4th Grade Reading Proficiency

	Other forms of polynomial				**Weighted Estimation**	
	ITT		**TOT (LATE)**		**ITT (5)**	**TOT (LATE) (6)**
Relative Year τ	**Linear (1)**	**Quadratic (2)**	**Linear (3)**	**Quadratic (4)**		
0	-1.59 (0.97)	-1.56* (0.93)	-1.45 (1.16)	-1.52 (1.14)	-1.45 (0.89)	-1.38 (1.10)
1	-1.03 (1.14)	-1.15 (1.10)	-0.41 (1.27)	-0.58 (1.25)	-1.20 (1.07)	-0.40 (1.29)
2	-0.22 (1.29)	-0.31 (1.24)	0.78 (1.47)	0.62 (1.45)	-0.32 (1.21)	0.86 (1.46)
3	-0.52 (1.37)	-0.42 (1.30)	0.42 (1.34)	0.57 (1.32)	-0.33 (1.25)	0.95 (1.32)
4	-0.32 (1.34)	-0.33 (1.26)	1.33 (1.26)	1.22 (1.26)	-0.28 (1.21)	1.65 (1.23)
5	0.43 (1.36)	0.46 (1.27)	2.47* (1.27)	2.46* (1.27)	0.54 (1.22)	1.96 (1.22)
6	-0.01 (1.50)	0.01 (1.41)	2.25 (1.48)	2.18 (1.45)	0.01 (1.35)	2.07 (1.40)
7	-0.10 (1.65)	-0.16 (1.39)	1.68 (1.39)	1.57 (1.39)	-0.18 (1.33)	1.52 (1.35)
8	-0.53 (1.49)	-0.43 (1.42)	2.21* (1.33)	2.20* (1.31)	-0.27 (1.37)	2.16* (1.28)
9	-0.95 (1.65)	-0.73 (1.54)	1.66 (1.59)	2.80 (1.58)	-0.61 (1.47)	1.96 (1.49)
10	-1.28 (1.74)	-0.99 (1.60)	2.46 (1.65)	2.57 (1.67)	-0.70 (1.51)	2.93* (1.65)
11	-2.45 (1.82)	-2.29 (1.75)	3.39* (1.99)	3.23* (1.96)	-2.13 (1.70)	2.92 (1.97)
12	-0.62 (2.08)	-0.51 (1.99)	5.39** (2.44)	5.15** (2.42)	-0.35 (1.92)	5.66** (2.48)
13	0.08 (2.44)	0.01 (2.36)	5.75 (3.75)	5.31 (3.63)	0.02 (2.34)	4.14 (3.54)

Numbers in parentheses are clustered standard errors by school district. Sample sizes is 7244 for the ITT estimations and 9652 for the TOT estimations. * and ** indicate statistical significance at 90% and 95% confidence level, respectively.

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Table A2: Sensitivity Analysis: Specification, 7th Grade Reading Proficiency

	Other forms of polynomial				Weighted Estimation	
	ITT		TOT (LATE)		ITT (5)	TOT (LATE) (6)
Relative Year τ	**Linear (1)**	**Quadratic (2)**	**Linear (3)**	**Quadratic (4)**		
0	0.30 (1.00)	0.53 (0.95)	1.73 (1.30)	1.72 (1.31)	0.55 (0.91)	1.66 (1.29)
1	-1.45 (1.03)	-1.30 (1.01)	0.22 (1.34)	0.73 (1.33)	-1.40 (0.97)	0.12 (1.30)
2	-2.11* (1.13)	-1.97* (1.12)	-0.66 (1.52)	-0.68 (1.53)	-2.04* (1.09)	0.01 (1.56)
3	-1.51 (1.20)	-1.39 (1.16)	0.71 (1.48)	0.46 (1.47)	-1.36 (1.13)	0.73 (1.44)
4	-1.52 (1.18)	-1.45 (1.14)	1.09 (1.49)	0.82 (1.47)	-1.46 (1.10)	1.09 (1.49)
5	-1.66 (1.09)	-1.48 (1.06)	1.36 (1.64)	1.28 (1.62)	-1.42 (1.04)	0.87 (1.61)
6	-2.20* (1.31)	-1.95 (1.27)	1.45 (1.66)	1.38 (1.67)	-1.91 (1.24)	1.39 (1.62)
7	0.28 (1.37)	0.51 (1.30)	3.89** (1.79)	3.89** (1.79)	0.60 (1.26)	3.73** (1.76)
8	-0.87 (1.34)	-0.66 (1.29)	3.96** (1.77)	3.68** (1.77)	-0.67 (1.25)	2.98* (1.75)
9	-0.57 (1.47)	-0.34 (1.39)	4.12** (1.95)	4.05** (1.99)	-0.26 (1.33)	4.05** (1.93)
10	-1.83 (1.59)	-1.63 (1.54)	4.40** (2.13)	4.22* (2.20)	-1.55 (1.50)	4.28** (2.15)
11	-2.52 (1.72)	-2.18 (1.67)	4.90** (2.15)	4.89** (2.20)	-1.96 (1.63)	4.85** (2.25)
12	-1.51 (1.88)	-1.46 (1.81)	5.97** (2.40)	5.94** (2.38)	-1.50 (1.76)	6.36** (2.70)
13	-1.27 (2.24)	-1.16 (2.16)	4.22 (3.44)	4.12 (3.35)	-1.04 (2.08)	3.63 (3.35)

Numbers in parentheses are clustered standard errors by school district. Sample sizes is 7219 for the ITT estimations and 9650 for the TOT estimations. * and ** indicate statistical significance at 90% and 95% confidence level, respectively.

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