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When Money Matters Most: Unpacking the Effectiveness of School Spending

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Targeted school funding is a potentially valuable policy lever to increase educational equality by race, ethnicity, and income, but it remains unclear how to target funds most effectively. We use a regression discontinuity approach to compare districts that narrowly passed or failed a school funding election. We use close tax elections in 9 states to identify effects of operating funds and close bond elections in 8 states to identify effects of capital funds. Results indicate positive achievement returns to spending, especially for math achievement and for operating funds. We find similar returns to spending by race, ethnicity, and income (not statistically different), but we find significantly larger returns for students in low-resource districts than in high-resource districts, including larger returns for Black, Latinx, and low-income students. Mediation analyses suggest spending on teacher salaries and counselors may be particularly effective mechanisms to increase achievement among Black and low-income students.

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When Money Matters Most: Unpacking the Effectiveness of School Spending

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Abstract
Targeted school funding is a potentially valuable policy lever to increase educational equality by race, ethnicity, and income, but it remains unclear how to target funds most effectively. We use a regression discontinuity approach to compare districts that narrowly passed or failed a school funding election. We use close tax elections in 9 states to identify effects of operating funds and close bond elections in 8 states to identify effects of capital funds. Results indicate positive achievement returns to spending, especially for math achievement and for operating funds. We find similar returns to spending by race, ethnicity, and income (not statistically different), but we find significantly larger returns for students in low-resource districts than in high-resource districts, including larger returns for Black, Latinx, and low-income students. Mediation analyses suggest spending on teacher salaries and counselors may be particularly effective mechanisms to increase achievement among Black and low-income students.

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Introduction

Long-term systematic discrimination by race, ethnicity, and income (REI) have generated structural biases and inequalities in education (Darity and Mullen 2020; Domina et al. 2017; Massey 2008; Brown 2021; Faber 2020; Desmond 2016). Despite multiple school finance reforms in most states that have increased equality of funding by income (Shores et al. 2022), REI inequality in educational opportunities remains: on average, Black, Latinx, and low-income students attend schools with fewer resources than white or high-income students (Baker et al. 2020; Corcoran and Evans 2015; Darling-Hammond 1998; Condron 2009). Reflecting these unequal resources, REI inequality exists in every measure of educational opportunity, including achievement, grades, HS dropout and graduation rates, and college entrance and completion rates (Jeynes 2015; Reardon 2011; NCES 2017; Orfield 2004; Milner 2012; Ho and Kao 2018; Owens 2016). These inequalities intersect, with Black and Latinx students more likely to attend high poverty schools and low-income Black and Latinx students falling further behind their higher-income counterparts (Carnoy and Garcia 2017).

Targeted school funding is a potentially valuable policy lever to counteract this systemic inequality and increase REI equality. Schools and other social institutions form the structural roots of inequality and schools build community, provide multiple services, work with children for about half their waking hours, and can influence demand to live in a community (Heers et al. 2016; Lafortune and Schonholzer 2022; Biasi et al. 2024). Evidence shows that low-income students in low-income and Black communities benefit more from school funding (Biasi et al. 2024; Lafortune, Rothstein, and Schanzenbach 2018; Rauscher and Shen 2022; Rauscher 2020a; Jackson et al. 2016). Yet it remains unclear how to target funds most effectively to increase equality by race, ethnicity, and income.

This study provides evidence about how school funding increases can most effectively reduce educational inequalities. Using quasi-random increases in school operating and capital funds, this study addresses three research questions:

1) How much does student achievement benefit from increases in school operating versus capital funds?
2) Do effects differ by student or district race, ethnicity, or income?
3) What are the mechanisms and do mechanisms differ by race, ethnicity, or income?

Using a regression discontinuity (RD) approach, we compare districts that narrowly passed or failed a school funding election to identify how to target school funds to increase equality in educational outcomes by race/ethnicity and income. We use close tax elections to identify effects of operating funds and close bond elections to identify effects of capital funds. To preview our results, we find consistently larger achievement returns to operating than capital funds and larger achievement returns to school funding in districts with low resources (low previous spending, low income, and high poverty districts). Within districts, we do not find significantly different returns to funding by race, ethnicity, or income on average, but we find significantly larger returns to achievement among Black students in districts with low Black or high White enrollment, compared to districts with high Black or low White enrollment.

Education is increasingly important for children's life chances (Goldin and Katz 2008; Fonseca et al. 2020; Goldman and Smith 2011; Rauscher and Rangel 2020). Our results identify structural changes – namely increased operating expenditures – to increase educational equality. Targeting spending to low resource districts could most effectively increase achievement among racially, ethnically, and economically marginalized students. Mediation analyses suggest spending on teacher salaries and counselors may be particularly effective mechanisms to increase achievement among Black and low-income students.

Background

Variation by Funding Type: Evidence consistently shows that increased school funding improves student achievement (Jackson et al. 2016; Candelaria and Shores 2019; Jackson and Mackevicius 2024). Less is known about whether specific types of funding have larger benefits and how effects differ by student and district characteristics. A recent meta-analysis shows that a $1,000 increase in school spending per pupil leads to about a 0.03 standard deviation increase in average test scores, with larger effects among low-income students, but similar effects for operating and capital funds (Jackson and Mackevicius 2024). However, of the 31 studies reviewed, only one examines effects of both operating and capital funds. Baron (2022) uses district elections in Wisconsin and finds that operating funds increase achievement, but capital funds do not. When including additional states, it remains unclear whether effects differ by funding type.

Operating funds – for daily operating expenses, salaries, benefits – are crucial for student outcomes because they keep schools adequately staffed and can reduce class size (Boyd-Zaharias 1999; Baker 2017). In addition to teacher salaries and benefits, operating funds also cover curriculum improvements, instructional materials, support staff and instructional aides, counselors, social workers, transportation, and extracurricular activities. Capital funds are earmarked for spending on buildings, grounds, or equipment. They can only be used for capital expenses and may not directly affect achievement (Martorell et al. 2016) but can affect achievement indirectly by improving health or air quality or reducing crowding (Maxwell 2016). We expect operating and capital funds to improve achievement. However, because operating funds are more directly related to achievement, we expect to find larger effects of operating funds than capital funds on achievement.

Hypothesis 1: School funding increases achievement.
Hypothesis 2: Operating funds increase achievement more than capital funds.

Variation by Student and District Characteristics: Growing evidence of varying effects of school spending by student and district characteristics raises theoretical questions with policy implications. Why do effects vary? Targeted school funding for low-income schools (progressive funding) is typically justified based on need: low-income students and communities have higher needs and, as a result, it costs more to achieve a given level of achievement (Baker 2017). This rationale may be politically useful in the short-term, but perpetuates the perception of deficits and suggests lower effectiveness of school spending among low-income students and communities. Claims that progressive or equitable funding is inefficient (Okun 1975) make it easier to maintain racially and economically biased school funding policies (Nalani et al. 2021). Recent evidence of higher efficiency of school funding in low-income, Black communities provides a compelling argument for school funding targeted to those communities: an efficiency argument that counters deficit-based rationales, highlights student and community strengths, and appeals across political ideologies (Rauscher and Shen 2022).

Growing evidence shows that students with fewer financial resources benefit more from school funding (Jackson et al. 2016; Lafortune et al. 2018; Candelaria and Shores 2019; Rauscher 2020a, 2020b; Jackson and Mackevicius 2024). Higher funding increases student achievement in financially disadvantaged districts and narrows test score gaps (Card and Payne 2002; Lafortune et al. 2018; Roy 2011; Guryan 2001; Papke 2005; Downes et al. 2009). Recent work also finds larger educational returns to school funding among Black and low-income students in low-income, Black communities (Rauscher and Shen 2022). Yet we know little about why. Until we learn why the effects of school funds vary and when funds are most effective, the

case for targeted school funding to reduce educational inequality remains potentially powerful yet relatively weak in practice.

Diminishing returns theory suggests the benefits of investment decline at higher levels (Kohli and Singh 1989; McFadyen and Cannella 2004; Potter and Watts 2011). For example, time investments in children's achievement have diminishing returns: children with more previous time invested benefit less from additional time (Walberg and Tsai 1984). Similarly, school spending may have less educational benefit for children whose families or communities already spend a lot on them. Diminishing returns theory predicts higher returns to school spending among low-income students and in low-resource districts. Low-resource districts include those with low initial spending and high poverty.

Funding shapes perceptions: Investing more funds in a district can improve how individuals perceive the area, increase property values, increase demand to teach or live in a district (Neilson and Zimmerman 2014), and enhance perceived school quality. Because racism creates artificially negative perceptions of Black and Latinx communities and artificially positive perceptions of White communities, school funding should improve perceptions more and yield larger educational benefits in districts with a high proportion of Black or Latinx students. For example, school facilities funding can increase the desire to teach in a school and improve teacher quality through satisfaction and retention (Buckley et al. 2004; Johnson et al. 2012). If funding improves achievement by influencing perceptions, we expect higher returns to school spending in districts with a high proportion of Black or Latinx students and in districts with a low proportion of White students.

Funding can empower and engage students from racially marginalized backgrounds: Higher funding allows districts to recruit and retain better quality teachers – trained to value and

teach students from all backgrounds – which can increase student empowerment and school engagement (Villegas 1991; Padron et al. 2002). Higher funding allows schools to expand and improve extracurricular opportunities, which help students to engage in school despite experiencing academic alienation and devaluation (Carter 2012; Jordan 1999). For example, a band teacher may connect with and empower Black or Latinx students or alienated students may look forward to connecting with peers in band after school, despite the anger and devaluation they may experience in classes (e.g., a whitewash American history class). By improving engagement, we expect funding to increase achievement more among Black and Latinx students than White students.

> *Hypothesis 3:* School funding increases achievement more among low-income, Black, and Latinx students compared to high-income or White students.
> *Hypothesis 4:* School funding increases achievement more in high-Black, high-Latinx, low-White, and low-resource districts compared to low-Black, low-Latinx, high-White, and higher-resource districts.

Contributions: This study makes several contributions to research on the effects of school funding. Most research examines either operating funds or capital funds, but not both (Cellini et al. 2010; Rauscher 2020a; Abott et al.2020). To our knowledge, only one study based on districts in Wisconsin provides causal effects of both operations and capital funds (Baron 2022). We build on that study by estimating causal effects of operations and capital funds in more states, examining heterogeneity and mediation similarly for the two funding types. To aid comparison, we provide cost effectiveness estimates per $1,000 additional funds per pupil for operating or capital expenses.

Second, we systematically compare effects by both race and income. Growing evidence finds larger benefits of school funding for low-income students and low-resource communities (Biasi et al. 2024; Lafortune, Rothstein, and Schanzenbach 2018; Rauscher 2020a, Rauscher and

Shen 2022). School funding may also have larger benefits for Black and Latinx students by influencing student engagement or community perceptions, with the largest benefits among Black and Latinx students in low-resource districts. Despite the potential policy implications for improving racial equality, little research has examined variation in the effects of school funding by race or by race and income.

Third, to understand how funding matters and why effects vary, we quantify the role of multiple potential mechanisms. Results will advance knowledge about how funding matters and how to increase educational equality.

Methods

We use a regression discontinuity (RD) approach to estimate effects of school spending on achievement. We build on Abott and colleagues' (2020) study to compare effects of operating and capital funds and examine variation by race and income. Using referenda data from Abott et al. (2020), we estimate effects of operating funds by comparing districts that narrowly passed or failed a tax election to increase local revenue for operating expenses in 9 states (AR, CA, LA, MI, MO, OH, PA, TX, WI). We estimate effects of capital funds by comparing districts that narrowly passed or failed a bond election to increase local revenue for capital expenses in 8 states (CA, IA, LA, MO, OH, OK, TX, WI). We estimate effects of funding on achievement by race, ethnicity, and income and provide comparable estimates for a $1,000/pupil increase in operations and capital funds in standard deviation units. We test for heterogenous effects and conduct sensitivity analyses to assess validity of the RD approach. We also estimate effects on multiple potential mechanisms at the district-level, including spending, staffing, and instructional salaries.

Data: We use data on district tax and bond elections in years 1998-2018 from Abott et al. (2020) and from the California Elections Data Archive (CEDA). Abott and colleagues provide tax elections in Arkansas, Louisiana, Michigan, Missouri, Ohio, Pennsylvania, Texas, and Wisconsin, excluding 2007-08 elections in Pennsylvania about a state tax policy change and excluding elections in districts that include multiple counties due to difficulty in accurately measuring election outcomes from county records. We add California elections from CEDA, which includes accurate results from multi-county districts. Elections data include the date, percent of votes in favor of the tax increase, the cutoff required to pass, and outcome. Main analyses estimate effects of elections in years 1998-2018, which allows examining achievement measures from five years before to a maximum of ten years after the election. We repeat analyses limited to 2000-2015 for consistency with Abott et al.(2020) and results are similar.

Data on district-level achievement are from the Stanford Education Data Archive (SEDA 5.0; Fahle et al. 2024). These data include district achievement test scores for each year 2009-2019 and each grade 3-8. Year refers to spring of the academic year throughout the paper. Within each district, SEDA also provides achievement separately by race/ethnicity (non-Hispanic Black, non-Hispanic White, and Hispanic students) and income (economically disadvantaged and not economically disadvantaged, based on each state's definition of "economic disadvantage").

We link the referenda and SEDA data using NCES id to other district characteristics for years 1998-2020 from multiple sources, including the Public Elementary-Secondary Education Finance Data from the Census Finance Survey (F-33 data) on spending, National Center for Education Statistics (NCES) data on district enrollment, composition, and staffing, and Census Small Area Income and Poverty Estimates (SAIPE).

The California Department of Education (CDE) provides annual district-level achievement data separately by both race/ethnicity and income, including achievement among low-income Black, low-income Hispanic, and low-income White students. These data are only available in years 2015-2022 and in California, but provide a rare opportunity to examine effects by both student race and income. Data on student engagement (chronic absenteeism, suspension, expulsion, arrest) are gathered from the Civil Rights Data Center (CRDC), but are only available for 6 years (every other year from 2010-2021).

Measures: *Outcome:* SEDA provides annual district-level measures of achievement, separately by race/ethnicity and by income in standard deviation units, relative to the standard deviation of student scores in the national reference grade and cohort (Fahle et al. 2024:29). We aggregate across grades 3-8 separately for English Language Arts (ELA) and Math and we calculate an average of ELA and Math for a combined achievement measure for each group. Race/ethnicity categories are mutually exclusive throughout the paper; for brevity, we refer to Black and White without specifying non-Hispanic ethnicity. Low-income is based on each state's definition of economic disadvantage. Supplementary analyses use California achievement scores 2015-2022, where achievement is measured on the original scale (2000-3000).

District Funding Elections: The referenda data provide information about each district election measure 1998-2018, including the date, type (tax, bond), percent of votes in favor, and the threshold required to pass. The treatment is an indicator for whether a proposed tax or bond election passed. Voteshare (the forcing variable in RD analyses) is calculated as the percent votes in favor of each election centered at the pass cutoff.

District Spending: We calculate annual spending measures per pupil from F-33 data, including total current spending and spending on capital, instruction, salaries, instructional

salaries, benefits, support services, and debt interest. We adjust all currently for inflation to 2020 dollars based on the academic fiscal year using Consumer Price Index from the St. Louis Federal Reserve Bank (FRED) data repository (Candelaria and Shores 2020).

District Demographic Characteristics: We measure the proportion of students who are Black, Latinx, White, and eligible for free/reduced price lunch from NCES data. NCES data also provide the number of full-time equivalent teachers and other staff measures. SAIPE data provide district child poverty rate. We use the same controls as Abott et al. (2020) to address potential variation in district characteristics before an election. Specifically, we control for the proportions of students who are Latinx, Black, and eligible for free/reduced price lunch, current spending per pupil (logged), and enrollment (logged), all measured in the year before the election.

Analyses: Building on previous research (Abott et al. 2020; Cellini et al. 2010; Rauscher 2020a), we use a dynamic regression discontinuity (RD) approach to estimate effects of a quasi-random funding increase. By comparing districts within a narrow range around the cutoff required to pass a funding election, RD provides a causal estimate of the treatment effect among otherwise similar districts (Lee and Lemieux 2010; Imbens and Lemieux 2008). When limiting analyses to a narrow window on either side of the pass cutoff, the RD approach assumes that meaningful unobserved differences between districts are eliminated and that other covariates related to the outcome vary continuously over the forcing variable, which is controlled in the model (Lee and Lemieux 2010:287). Student achievement may differ between districts that pass or fail an election, but we limit analyses to a narrow window on both sides of the cutoff to include districts that should be similar, except for observed (and controlled) differences in the forcing variable. Close parcel tax elections (to raise operating funds) and bond elections (to raise

capital funds) allow us to estimate effects separately by funding type, using a comparable approach.

We anticipate delayed effects of school funding based on previous evidence (Rauscher 2020a; Cellini et al. 2010). To address this, we create stacked panel data around each individual election, including observations 5 years before and 10 years after the election (*t*-5 to *t*+10), into one dataset. These data can include multiple observations of the same district-years if districts hold multiple elections. To allow for effects that emerge multiple years after a funding election, we estimate separate effects in years 0-5 before the election (where effects should be null) and in 5-year periods after the election (years 1-5 and years 6-10 after the election).

We use the same bandwidth as Abott et al. (2020) and limit the sample to districts within 10 percentage points of the cutoff required to pass an election. We assess robustness to other bandwidths from one to 15 percentage points on either side the cutoff.

Equation 1 estimates effects of passing a funding election where achievement of students in district *i* in calendar year *t* is predicted with an indicator for whether the election passed, voteshare centered at the cutoff, an interaction between pass and voteshare to allow the relationship to vary, fixed effects for each calendar year (μ_t), and controls (X_{it}) for pre-election characteristics (percent Latinx, Black, and eligible for free/reduced price lunch, log spending per pupil, log enrollment, child mortality).

$$\text{Achievement}_{it} = \beta_1 \text{Pass}_{it} + \beta_2 \text{Voteshare}_{it} + \beta_3 \text{Pass}_{it} * \text{Voteshare}_{it} + \mu_t + \varepsilon_{itk} \qquad (1)$$

Robust standard errors are adjusted for district-level clustering. The coefficient of interest (β_1) estimates the effect of narrowly passing a funding election on achievement, accounting for differences in voteshare, pre-election differences between districts on multiple characteristic, and changes over time. We show estimates from models limited to years 0-5 before the election and

to years 1-5 and 6-10 after the election. Estimates should be null before the election. Positive β_1 coefficients in years after the election would support *Hypothesis 1* and suggest higher school funding increased achievement.

To examine variation by funding type, we test for significant differences (Clogg et al. 1995) between β_1 coefficients from separate models estimating effects of passing a tax election (operating funds) or a bond election (capital funds). For example, we calculate $z = (\beta_O - \beta_C)/\sqrt{SE_O^2 + SE_C^2}$, where β_O indicates β_1 when estimating the effect of passing a tax election and β_C indicates β_1 when estimating the effect of passing a bond election. A positive and significant z statistic would support *Hypothesis 2*, that operating funds increase achievement more than capital funds. We use a similar approach to test for variation by student demographics. Larger coefficients when predicting achievement among low-income, Black, and Latinx students compared to high-income or White students would support *Hypothesis 3*, that these students benefit more.

To examine variation by district demographic characteristics, we stratify the sample to identify districts above and below the median value in the year before the election. For example, to compare effects by previous district spending, we run analyses when limiting the sample to districts that are above (or below) the median spending per pupil in the year before the election. We test for significant differences between β_1 coefficients from separate high- and low-spending models, by calculating $z = (\beta_H - \beta_L)/\sqrt{SE_H^2 + SE_L^2}$, where β_H indicates β_1 (the effect of passing an election) when predicting achievement in high-spending districts and β_L indicates β_1 when predicting achievement in low-spending districts. A negative and significant z statistic would support *Hypothesis 4*, that achievement benefits of school funding are larger in low-resource districts. The one exception is analyses by percent Black enrollment: we compare estimates

above and below 2% Black enrollment (~60th percentile) because there are not enough observations of Black student achievement in districts below the median for percent Black enrollment.

Mean demographic characteristics differ significantly between districts in these stratified samples (see Appendix Table A2). As expected, district racial, ethnic, and income composition are significantly different by percent Black, Latinx, White, and free/reduced-price lunch composition and by district poverty rate. Spending is higher in districts with more economically and racially marginalized students in the tax election sample, but that pattern is generally reversed in the bond election sample. This could reflect changing or growing district enrollment in districts with a bond election. Comparing districts with high and low pre-election spending also reveals difference by the type of funding election. In the tax election sample, high-spending districts have higher proportions of low-income, Black, and Latinx students compared to low-spending districts. The opposite is observed in the bond election sample, with more economically and racially marginalized students in low-spending districts.

Cost Effectiveness: SEDA achievement measures are in standard deviation units, so β_1 estimates the change in achievement from passing a funding election in standard deviation units. We divide the β_1 coefficients by the estimated change in per pupil spending (in thousands of 2020 dollars) to calculate the average effect per $1,000 investment per pupil. We also calculate the cost per unit increase in the outcome (i.e. the cost to achieve a 1 standard deviation increase in achievement). We interpret costs in comparison to other studies using Kraft's (2020) guidelines.

Sensitivity Analyses: We examine the extent to which this approach satisfies the RD standards developed by What Works Clearinghouse (2022), including integrity of the forcing

variable, low sample attrition, continuity of the relationship between the forcing variable and the outcome, and functional form and bandwidth selection. We conduct conventional and robust density tests, separately for tax and bond elections, which are not statistically significant and consistent with the statistical integrity of the forcing variable (see Figure A1; McCrary 2008). Consistent with institutional integrity, district tax and bond elections require support from a fixed proportion of voters to pass, the pass cutoff is fixed prior to the election, votes are collected and recorded by county election offices, and districts have no opportunity to change votes.

Because we use district-level data, sample attrition is relatively low. We limit our analyses to years with SEDA achievement measures (2009-2019) and to districts with elections in years 1998-2018. The total attrition rate for district-year observations missing achievement or any other variable in the analysis in any year post-election is 11.6% for tax elections and 11.0% for bond elections. The most common missing measure is achievement, which likely reflects the number of test scores available and SEDA data suppression rules (Fahle et al. 2024). Attrition rates are similar in districts that passed or failed a tax election (11.5% vs 11.8%) or bond election (11.0% vs 11.0%). These differences are not statistically significant at the 95% level. We examine the consistency of our results in districts with at least 9 years of complete data and find substantively similar results.

We examine continuity of the outcome across the distribution of the forcing variable. Figure A2 does not indicate discontinuities in the outcome-forcing variable relationship at values of the forcing variable other than at the cutoff. We also examine and test for differences in pre-election covariates at the cutoff of the forcing variable. Table A3 shows estimated differences between district that passed or failed the tax election on multiple district measures one and three years before the election. Estimates show limited pre-election differences by election outcome.

The only significant differences are in the percent of students eligible for free or reduced-price lunch in years 1 and 3 before a bond election (two out of 104 tests of difference).

The main analyses use a sample within a predetermined bandwidth of voteshare (following Abott et al. 2020). We assess robustness to varying the bandwidth and choice of functional form for the forcing variable. We also conduct placebo checks by assigning false pass cutoffs at points above and below the actual cutoff required to pass. Specifically, we estimate effects at the median value of voteshare above and below the true cutoff. These estimates (shown in Appendix Table A4) are null and provide further evidence of continuity in the outcome-forcing variable relationship at values of the forcing variable other than the pass cutoff.

Mediation Analyses: We use Equation 1 to estimate effects of funding on multiple potential mechanisms. These measures include: number of teachers and staff; student-teacher ratio; average teacher salary; spending categories; and student composition. To better understand mechanisms, we conduct mediation estimates using paramed in Stata (Emsley and Hanhua 2013). We examine mediation separately by race, ethnicity, and income to see whether mechanisms differ.

Results

Descriptive statistics (see Appendix Table A1) show substantial differences in achievement by race, ethnicity, and income. In both the tax and bond election samples, the average Black achievement in the sample districts is more than 0.4 standard deviations below the mean and average Latinx achievement is more than 0.2 sd below the mean, while White achievement is slightly above the mean. Differences by income are also notable, with low-income achievement about 0.2 sd below the mean and high-income achievement about 0.3 sd above the mean in both samples. Pre-election differences in student composition, enrollment, and

spending indicate the importance of controlling for these pre-election measures in regression analyses. The number of observations is much lower for Black and Latinx achievement compared to other measures, resulting in lower power for regressions predicting Black and Latinx achievement.

Table 1 shows estimated effects of narrowly passing a tax or bond election on achievement by time since the election. Most estimates indicate no significant differences between districts that narrowly passed or failed a funding election in years 0-5 before the election. The one exception is significantly higher ELA achievement in districts that passed a tax election. Estimates predicting ELA achievement after the election are not significant, suggesting most of the impact of funding is on Math achievement.

In years 1-5 and 6-10 after the election, estimates show significantly higher achievement in districts that narrowly passed a tax election to increase school funding. Compared to districts that failed a tax election, districts that passed had achievement scores about 0.03 sd higher among all, White, and low-income students. Estimates predicting Math achievement are slightly higher: narrowly passing a tax election increased achievement by 0.04 sd units among all, White, and high-income students and by 0.03 sd units among low-income students. Comparing estimates using *z*-tests (Clogg et al. 1995), coefficients do not differ significantly at the 95% level by student income, race, or ethnicity. Thus, evidence suggests that narrowly passing a tax election increases achievement, consistent with *Hypotheses 1*. However, results do not support *Hypothesis 3*, that effects are larger among low-income, Black, or Latinx students.

Estimates of the effect of narrowly passing a bond election are generally positive, but not significant. However, no post-election estimates for tax and bond elections are significantly different at the 95% level, suggesting effects of operating and capital funds are not significantly

different. This is not consistent with *Hypothesis 2*, that operating funds improve achievement more than capital funds.

Estimates predicting Black and Latinx achievement are positive, but do not reach significance at the 95% level, which could reflect the lower number of observations for those groups. We repeat analyses limited to districts with achievement data for all student race, ethnicity, and income groups. These estimates (Table A5) are consistent with the main analyses, but do not reach significance at the 95% level. Again in this sample, coefficients do not differ significantly at the 95% level by student income, race, or ethnicity or by election type. Thus, estimates with achievement information for all groups do not support *Hypotheses 2 or 3*.

Table 2 shows cost effectiveness estimates based on coefficients in Table 1. Cost effectiveness estimates for tax elections are calculated as the average effect on achievement in years 1-5 and 6-10 after the election divided by the estimated increase in total current spending (in thousands of 2020 dollars) in years 1-5 after the election. Bond elections have more delayed effects (Rauscher 2020a), so cost effectiveness estimates for bond elections are calculated as the effect on achievement in years 6-10 after the election divided by the estimated increase in capital spending in years 1-5 after the election. Standard errors are calculated by subtracting the cost effectiveness estimates one standard above and below the point estimate. Shown in Figure 1, these estimates suggest a 0.1 sd increase in achievement for a $1,000 per pupil increase in current spending. These estimates are consistent across student race, ethnicity, and income, with slightly higher effectiveness among Black students than other groups. Estimates are lower for bond elections, with a 0.01 sd increase in achievement for a $1,000 per pupil increase in capital spending, but the differences by funding type are not statistically significant. Cost effectiveness for capital spending is highest among Black students (0.06 sd per $1,000/pupil) compared to

Latinx and White students (0.03 and -0.01 sd per $1,000/pupil, respectively) and is slightly higher among low-income than high-income students (0.03 vs 0.00 sd per $1,000/pupil, respectively). Cost effectiveness estimates are slightly smaller for ELA and higher for Math achievement.

Cost effectiveness is slightly higher in more diverse districts that have achievement data for all student groups, with a 0.16 sd increase in achievement for a $1,000 per pupil increase in current spending and a 0.02 sd increase for a $1,000 per pupil increase in capital spending. (Appendix Table A6 and Figure A6 show the cost effectiveness estimates). In this sample, estimates for current spending are slightly larger among White and high-income students, but estimates for capital spending remain larger for Black and low-income students.

Overall, cost effectiveness estimates suggest somewhat higher returns to operating funds than capital funds, but the differences are not significant, providing limited support for *Hypothesis 2*. The results also indicate slight but not significant differences in spending returns by race and income, which does not support *Hypothesis 3*. Estimates for current spending fall in the medium effect size at moderate cost in Kraft's (2020) guidelines and suggest it would cost $10,000/pupil to increase achievement by one standard deviation. Estimates for capital spending generally fall in the small effect size at moderate cost and suggest it would cost ten times that amount to increase achievement by a single standard deviation.

The main analyses are limited to districts within 10% of the voteshare required to pass. We repeat analyses when varying the bandwidth of voteshare around the cutoff from one to 15 percentage points. We also repeat analyses when using the optimal bandwidth identified for each dependent variable by rdbwselect in Stata (Calonico, Cattaneo, and Farrell 2021). These estimates (shown in Appendix Figures A3-A5) are consistent with the main analyses.

Heterogeneous Effects: Table 3 shows estimates by district characteristics in years 6-10 after the election, with models fit separately for districts that were above and below the median value for each district measure in the year before the election. We test for significant differences between coefficients using *z*-tests (Clogg et al. 1995). Previous research found evidence of larger returns in low-resource areas (Rauscher and Shen 2022) and our results further support this. Comparing estimates by total current spending per pupil in the year before the election, most estimates are larger in low-spending than high-spending districts. The achievement benefits of passing a tax election are significantly larger among Latinx and low-income students in low-spending districts.

Passing a bond election did not increase achievement in the full sample, but in low-spending districts passing a bond significantly increased achievement among Black, Latinx, low-income, and all students. Estimated effects of passing a bond are significantly larger for all groups except White students in low-spending compared to high-spending districts. Some estimates are also significantly larger in districts with high eligibility for free/reduced-price lunch and high child poverty. This evidence is consistent with *Hypothesis 4*, that effects are larger in low-resource districts. Furthermore, results indicate that spending in low-resource districts is particularly beneficial for achievement among low-income, Black, and Latinx students, because estimates are significantly larger for these groups in low-spending than in high-spending districts. (See Appendix Table A9 for additional intersectionality analyses by race, ethnicity, and income using data from California.)

Table 4 shows cost effectiveness estimates by district characteristics. These estimates are calculated as the effect on achievement in years 6-10 after the election divided by the estimated increase in total current spending (for tax elections) or capital spending (for bond elections) in

years 1-5 after the election. Estimates indicate consistently larger benefits of current spending for low-income, Black, and Latinx students in districts with low spending and low income (high eligibility for free/reduced-price lunch). The benefits of both current and capital spending are larger for all groups in low-spending districts. Cost effectiveness estimates in low-spending districts range from 0.18 to 0.37 sd per $1,000 in current spending and from 0.05 to 0.17 sd per $1,000 in capital spending. These are large and medium effect sizes at moderate cost (Kraft 2020) and suggest it would cost about $5,500/pupil in current spending and about $12,500/pupil in capital spending to increase achievement among all students by one standard deviation in low-spending districts. Figure 2 shows cost effectiveness estimates for Math, which range from 0.23 to 0.41 sd per $1,000 in current spending and from 0.09 to 0.23 sd per $1,000 in capital spending.

Estimates for Black achievement by district Black and White composition indicate an unexpected pattern. When districts narrowly pass a tax election, we find significantly larger increases in Black achievement in low-Black and high-White districts. Estimates predicting Black achievement are still positive in high-Black and low-White districts, but the benefit is significantly smaller. Cost effectiveness estimates suggest a 0.52 sd increase in Black achievement in low-Black districts and a 3.42 sd increase in high-White districts per $1,000/pupil spending increase. Black students in low-Black and high-White districts may be a relatively small and select group, with higher income and parental education. However, these estimates suggest potentially larger achievement returns to spending for Black students in low-Black or high-White districts.

Mechanisms: Table 5 shows estimated effects of narrowly passing a tax election on potential mechanisms. We use the same models as Table 1 to predict each potential mechanism.

Narrowly passing a tax election increased total current spending by about $284 per pupil in years 1-5 post-election. We find no effect of passing a tax or bond election on the number of staff or students per staff. The number of students per counselor decreased after passing a tax election (-24.4 in years 6-10 after the election), but the decline was not significant at the 95% level. However, examining categories of spending, we find significant increases in per pupil spending on instruction ($140), salaries ($147), instructional salaries ($111), and support services ($143). Passing a tax election also increased average teacher salaries (instructional salary spending per FTE teacher) by $1,176. Passing a tax election had no effect on capital or benefits spending. In contrast, narrowly passing a bond election increased capital spending by $714 per pupil and spending on debt interest by $149 per pupil in years 1-5 after the election. Capital spending decreased by $521/pupil and debt interest spending increased by $88/pupil in years 6-10 after passing a bond election.

Comparing estimates by time from the election, effects on spending emerge in years 1-5 after the election, generally before the largest effects on achievement. These results suggest that revenue increases have delayed effects on achievement, as districts take time to spend the additional funds. Estimates in years before the election suggest few pre-election differences in staffing or spending, supporting the effectiveness of the RD approach in this setting. Average salary per staff is higher before the election in districts that passed a tax election, but this is just one significant difference out of 50 tests and could have happened by chance.

To better understand mechanisms, we conduct mediation estimates for achievement using paramed in Stata (Emsley and Hanhua 2013). Mediation estimates indicate that spending on salaries, instructional salaries, and average teacher salaries are mechanisms and consistently mediate the largest proportions of the effects of passing a tax election on achievement. Figure 3

shows mediation estimates separately by student race, ethnicity, and income. For most groups, spending on instructional salaries mediates the largest share of the total effect of passing an election, explaining about 18% of the effect on Black and White achievement, about 8% of the effect on Latinx achievement, about 10% of the effect on low-income achievement, and nearly 25% of the effect on high-income achievement. Average instructional salary per FTE teacher mediates the largest share of Black achievement (nearly 20% of the total effect). The number of counselors per student also mediates about 8% of the effect on Black achievement and about 5% of the effect on low-income achievement.

The bottom rows of Table 5 show estimated effects on measures of student engagement based on data from CRDC for limited years. Narrowly passing a tax election reduced the rate of multiple out-of-school suspension by 0.2 percentage points in years 1-5 and 6-10 after the election. The rate of student arrests also declined slightly, by 0.05 percentage points, in years 1-5 after the election. Chronic absenteeism declined by 1.7 percentage points, but the estimate is not significant at the 95% level. Narrowly passing a bond election had no significant effects. These estimates should be interpreted with caution, because they are based on limited years and smaller sample sizes. However, the estimates are consistent with school funding impacting achievement partly through student engagement at school.

Sensitivity Analyses: To assess robustness, we allow the functional form of the forcing variable to vary. Specifically, we repeat the main analyses when controlling for voteshare squared and cubed, and their interactions with the pass indicator (Appendix Table A7). Estimates are not significant, but the pattern of the results is consistent with the main analyses. We also find consistent results when excluding districts with low enrollment (≤400), elections with few votes (≤200), and districts observed fewer than 15 years in the data (Appendix Table A8).

Placebo checks at false pass cutoffs (above and below the actual cutoff) indicate null effects (Appendix Table A4), which suggests continuity in achievement at values other than the pass cutoff and increases confidence in our approach.

Conclusion

We use a regression discontinuity (RD) approach to compare districts that narrowly passed or failed a school funding election. We use close tax elections in nine states to identify effects of operating funds and close bond elections in eight states to identify effects of capital funds. We systematically compare effects of spending on achievement by funding type and by student and district characteristics to learn how we can target education funds more effectively to increase student achievement for all students, and especially for students from traditionally underserved racial, ethnic, and income groups.

We find consistently larger returns to operating than capital funds, with cost effectiveness estimates of 0.10 sd increase in achievement for a $1,000 per pupil increase in current spending compared to a 0.01 sd increase in achievement for a $1,000 per pupil increase in capital spending. However, consistent with previous evidence (Jackson and Mackevicius 2024), the estimates for operating and capital funds are not significantly different at the 95% level. This suggests comparable achievement benefits from operating and capital spending.

We find the largest returns to investing in districts with low resources, consistent with diminishing returns theory and with previous research (Rauscher and Shen 2022; Biasi et al. 2024). Cost effectiveness estimates in districts with low previous spending are 0.18 sd per $1,000 increase in current spending and 0.08 sd per $1,000 increase in capital spending, with higher cost effectiveness for Black, Latinx, and low-income students. These are large and medium effect sizes at moderate cost (Kraft 2020) and suggest it would cost about $5,500/pupil

in current spending and about $12,500/pupil in capital spending to increase achievement among all students by one standard deviation in low-spending districts.

We do not find significantly different returns to funding by race, ethnicity, or income, suggesting consistent benefits of school spending within districts. However, we find larger achievement returns to funding increases among Black, Latinx, and low-income students in districts with fewer resources than in districts with more resources. Spending more in low-resource districts is therefore more cost effective and is especially more effective for students from marginalized groups. Based on this evidence, targeting spending to low-resource districts with relatively high shares of marginalized students has the potential to reduce racial, ethnic and income inequality in academic achievement.

We also find significantly larger returns to Black achievement in districts with low Black or high white enrollment. These estimates are based on relatively fewer observations and Black students in low-Black and high-White districts may have relatively higher income and parental education than Black students in other districts. However, these estimates suggest potentially larger achievement returns to spending for Black students in low-Black or high-White districts. Future research examining the effect of opportunities for Black students to attend low-Black or high-White districts could be useful in efforts to reduce racial inequality of achievement.

Mediation analyses suggest that spending on instructional salaries explains the largest share of the effect of a funding increase on achievement for most groups. This supports previous evidence that teachers play a large role in student achievement (Rockoff 2004; Rivkin et al. 2005). For Black students, average instructional salary per FTE teacher mediates the largest share of achievement (nearly 20% of the total effect). The number of counselors per student also mediates about 8% of the effect on Black achievement and about 5% of the effect on low-income

achievement. This suggests that targeting spending to teacher salaries and counselors could help improve achievement among Black and low-income students.

Estimates predicting potential mechanisms offer further evidence that the effects on achievement partly reflect increases in teacher salaries. Passing a tax election increased average teacher salary by $1,176 in the first 5 years after the election. Despite limited years of available data, estimates also suggest that the achievement effects could reflect improved student engagement at school. Passing a tax election slightly reduced the rates of out-of-school suspension and student arrests.

Limitations of this study include reliance on limited years of achievement data (2009-2019) and smaller numbers of observations of achievement among Black and Latinx students. In addition, this study includes data from a limited number of states, which improves on single-state analyses, but future work examining effects of school spending in all states would improve external validity and would provide larger sample sizes for Black and Latinx achievement. Despite limitations, we provide comparable causal effects of operations and capital funds, systematically comparing effects and mechanisms by both race and income.

Education is increasingly important for children's life chances (Goldin and Katz 2008; Fonseca et al. 2020; Goldman and Smith 2011; Rauscher and Rangel 2020). Our results identify structural changes to increase educational equality. It is most effective for achievement to invest in low-resource districts and targeting spending to low-resource districts with high shares of marginalized students could improve equality of achievement by race, ethnicity, and income. Spending on teacher salaries and counselors may be particularly effective mechanisms to increase achievement among Black and low-income students.

Research Ethics

The research in this study does not constitute human subjects research because it uses secondary data on school districts and states. Data and analysis files to replicate the study will be shared in Stata format on Open Science Framework (OSF) upon publication.

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Tables and Figures

Table 1: Estimated Effect of Passing a School Funding Election on Achievement

	Tax Elections			Bond Elections		
Dependent Variable	Years -5 - 0	Years 1-5	Years 6-10	Years -5 - 0	Years 1-5	Years 6-10
Achievement						
All	**0.0181**	**0.0263***	**0.0311***	**-0.0065**	**-0.0050**	**0.0039**
	(0.0144)	**(0.0124)**	**(0.0143)**	**(0.0213)**	**(0.0176)**	**(0.0162)**
Black	0.0394	0.0391	0.0176	0.0231	-0.0105	0.0457
	(0.0316)	(0.0287)	(0.0264)	(0.0439)	(0.0404)	(0.0329)
Latinx	0.0159	0.0116	0.0341	-0.0112	0.0046	0.0216
	(0.0466)	(0.0391)	(0.0297)	(0.0341)	(0.0277)	(0.0241)
White	0.0027	0.0176	0.0294*	0.0011	-0.0085	-0.0036
	(0.0146)	(0.0125)	(0.0145)	(0.0209)	(0.0174)	(0.0164)
Low-Income	0.0009	0.0187	0.0251*	-0.0030	0.0005	0.0210
	(0.0137)	(0.0110)	(0.0122)	(0.0202)	(0.0156)	(0.0151)
High-Income	0.0097	0.0212	0.0235	-0.0088	-0.0117	-0.0022
	(0.0139)	(0.0112)	(0.0131)	(0.0208)	(0.0166)	(0.0161)
N (All)	8591	14324	12967	5601	7652	7479
ELA						
All	**0.0281***	**0.0172**	**0.0214**	**-0.0144**	**-0.0016**	**-0.0022**
	(0.0140)	**(0.0116)**	**(0.0135)**	**(0.0199)**	**(0.0165)**	**(0.0152)**
Black	0.0397	0.0308	0.0002	0.0205	-0.0085	0.0464
	(0.0314)	(0.0284)	(0.0269)	(0.0420)	(0.0383)	(0.0298)
Latinx	0.0222	0.0069	0.0213	-0.0216	0.0200	0.0207
	(0.0422)	(0.0359)	(0.0291)	(0.0317)	(0.0253)	(0.0229)
White	0.0106	0.0072	0.0185	-0.0078	-0.0087	-0.0101
	(0.0144)	(0.0117)	(0.0140)	(0.0200)	(0.0166)	(0.0156)
Low-Income	0.0055	0.0082	0.0155	-0.0132	0.0028	0.0106
	(0.0127)	(0.0101)	(0.0113)	(0.0182)	(0.0145)	(0.0142)
High-Income	0.0200	0.0133	0.0108	-0.0144	-0.0100	-0.0116
	(0.0136)	(0.0104)	(0.0126)	(0.0201)	(0.0154)	(0.0154)
N (All)	8588	14319	12965	5600	7651	7474
Math						
All	**0.0081**	**0.0354***	**0.0407***	**0.0011**	**-0.0082**	**0.0101**
	(0.0165)	**(0.0143)**	**(0.0163)**	**(0.0247)**	**(0.0204)**	**(0.0191)**
Black	0.0393	0.0479	0.0291	0.0329	-0.0075	0.0455
	(0.0350)	(0.0309)	(0.0285)	(0.0525)	(0.0456)	(0.0396)
Latinx	0.0082	0.0221	0.0487	0.0039	-0.0044	0.0189
	(0.0564)	(0.0447)	(0.0328)	(0.0405)	(0.0329)	(0.0287)
White	-0.0059	0.0276	0.0418*	0.0106	-0.0078	0.0028
	(0.0166)	(0.0143)	(0.0163)	(0.0241)	(0.0200)	(0.0190)

Low-Income	-0.0032	0.0283*	0.0335*	0.0076	-0.0013	0.0322
	(0.0161)	(0.0131)	(0.0143)	(0.0247)	(0.0191)	(0.0185)
High-Income	-0.0004	0.0301*	0.0364*	-0.0011	-0.0116	0.0089
	(0.0162)	(0.0135)	(0.0153)	(0.0242)	(0.0200)	(0.0190)
N (All)	8580	14301	12945	5594	7647	7648

Sample includes district-year observations with a tax or bond election 1998-2018 and voteshare within 10 percentage points of the pass cutoff. Models are fit separately among observations 0-5 years before the election (column 1), 1-5 years after the election (column 2), and 6-10 years after the election (column 3). All models include year fixed effects and pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), and current spending per pupil and enrollment (years 1 & 3 before election, logged).

Robust standard errors adjusted for district clustering in parentheses. * p<0.05, ** p<0.01, *** p<0.001

Table 2: Achievement Cost Effectiveness Estimates for $1,000 per Pupil Increase

	Tax Elections (Years 1-10/Current Spending)		Bond Elections (Years 6-10/Capital Spending)	
Dependent Variable	Estimate	Std Error	Estimate	Std Error
Achievement				
All	0.10	(0.09)	0.01	(0.05)
Black	0.10	(0.19)	0.06	(0.09)
Latinx	0.08	(0.24)	0.03	(0.07)
White	0.08	(0.09)	-0.01	(0.05)
Low-Income	0.08	(0.08)	0.03	(0.04)
High-Income	0.08	(0.09)	0.00	(0.05)
ELA				
All	0.07	(0.09)	0.00	(0.04)
Black	0.05	(0.19)	0.06	(0.08)
Latinx	0.05	(0.23)	0.03	(0.06)
White	0.05	(0.09)	-0.01	(0.04)
Low-Income	0.04	(0.08)	0.01	(0.04)
High-Income	0.04	(0.08)	-0.02	(0.04)
Math				
All	0.13	(0.11)	0.01	(0.05)
Black	0.14	(0.21)	0.06	(0.11)
Latinx	0.12	(0.27)	0.03	(0.08)
White	0.12	(0.11)	0.00	(0.05)
Low-Income	0.11	(0.10)	0.05	(0.05)
High-Income	0.12	(0.10)	0.01	(0.05)

Cost effectiveness estimates are the estimated effect of passing a funding election on achievement in standard deviation units from Table 2, divided by the estimated effect of passing a funding election on spending in thousands of 2020 dollars per pupil.

Tax election cost effectiveness estimates are calculated as the average effect on achievement in years 1-5 and 6-10 after the election divided by the estimated increase in total current spending in years 1-5 after the election.

Bond election cost effectiveness estimates are calculated as the effect on achievement in years 6-10 after the election (due to delayed effects) divided by the estimated increased in capital spending in years 1-5 after the election.

Standard errors are calculated by subtracting the cost effectiveness estimates one standard above and below the point estimate.

Table 3: Heterogeneous Effects by District Composition

Stratified Samples	Tax Elections				Bond Elections			
(1 yr before election)	High	Low	z-test		High	Low	z-test	
By District Spending								
All	0.0102	0.0424*	-1.13		-0.0178	0.0478*	-2.04	*
	(0.0212)	(0.0191)			(0.0217)	(0.0237)		
Black	-0.0048	0.0495	-1.01		-0.0194	0.1083*	-2.01	*
	(0.0299)	(0.0450)			(0.0439)	(0.0459)		
Latinx	-0.0355	0.0842	-2.02	*	-0.0302	0.0668*	-1.98	*
	(0.0342)	(0.0484)			(0.0378)	(0.0311)		
White	0.0067	0.0425*	-1.22		-0.0204	0.0327	-1.67	
	(0.0226)	(0.0187)			(0.0223)	(0.0227)		
Low-Income	-0.0045	0.0472**	-2.10	*	0.0004	0.0620**	-2.07	*
	(0.0170)	(0.0178)			(0.0197)	(0.0223)		
High-Income	-0.0056	0.0417*	-1.82		-0.0308	0.0434	-2.33	*
	(0.0197)	(0.0169)			(0.0219)	(0.0231)		
By District % Black								
All	0.0548*	0.0215	1.12		-0.0030	0.0087	-0.36	
	(0.0244)	(0.0171)			(0.0234)	(0.0225)		
Black	0.0152	0.1489**	-2.48	*	0.0347	0.1794	-0.99	
	(0.0272)	(0.0466)			(0.0347)	(0.1416)		
Latinx	0.0305	0.0608	-0.44		0.0372	0.0060	0.60	
	(0.0322)	(0.0613)			(0.0297)	(0.0424)		
White	0.0705**	0.0125	1.86		-0.0281	0.0141	-1.30	
	(0.0264)	(0.0165)			(0.0233)	(0.0227)		
Low-Income	0.0218	0.0296	-0.33		0.0320	0.0135	0.62	
	(0.0179)	(0.0157)			(0.0210)	(0.0213)		
High-Income	0.0427	0.0154	0.98		-0.0084	0.0058	-0.44	
	(0.0232)	(0.0155)			(0.0238)	(0.0219)		
By District % Latinx								
All	0.0398	0.0191	0.72		0.0099	0.0153	-0.17	
	(0.0221)	(0.0182)			(0.0248)	(0.0211)		
Black	0.0311	-0.0202	1.08		0.0547	0.0090	0.63	
	(0.0324)	(0.0347)			(0.0411)	(0.0596)		
Latinx	0.0377	0.0058	0.39		0.0472	-0.0187	1.07	
	(0.0315)	(0.0762)			(0.0252)	(0.0562)		
White	0.0345	0.0207	0.48		-0.0177	0.0210	-1.18	
	(0.0221)	(0.0186)			(0.0248)	(0.0213)		
Low-Income	0.0185	0.0287	-0.42		0.0383	0.0282	0.34	
	(0.0191)	(0.0154)			(0.0229)	(0.0187)		
High-Income	0.0262	0.0187	0.29		-0.0324	0.0330	-2.07	*
	(0.0205)	(0.0165)			(0.0242)	(0.0204)		
By District % White								

All	0.0201	0.0394	-0.68		0.0169	-0.0042	0.67
	(0.0179)	(0.0221)			(0.0203)	(0.0241)	
Black	0.8427*	0.0156	2.50	*	0.0523	0.0539	-0.02
	(0.3300)	(0.0266)			(0.0773)	(0.0351)	
Latinx	0.0616	0.0396	0.18		0.0412	0.0246	0.30
	(0.1216)	(0.0303)			(0.0488)	(0.0258)	
White	0.0207	0.0385	-0.62		0.0150	-0.0245	1.23
	(0.0179)	(0.0225)			(0.0208)	(0.0244)	
Low-Income	0.0234	0.0283	-0.21		0.0377*	0.0166	0.73
	(0.0161)	(0.0173)			(0.0186)	(0.0219)	
High-Income	0.0224	0.0221	0.01		0.0146	-0.0199	1.08
	(0.0160)	(0.0207)			(0.0193)	(0.0253)	
By District % Free/Reduced Lunch							
All	0.0470*	-0.0066	1.97	*	-0.0053	0.0237	-0.93
	(0.0190)	(0.0195)			(0.0209)	(0.0233)	
Black	0.0095	-0.0127	0.44		0.0573	0.0390	0.28
	(0.0333)	(0.0383)			(0.0390)	(0.0533)	
Latinx	0.0715*	-0.0303	1.93		0.0395	0.0101	0.61
	(0.0335)	(0.0409)			(0.0280)	(0.0393)	
White	0.0516*	-0.0035	1.97	*	-0.0200	0.0234	-1.35
	(0.0205)	(0.0190)			(0.0219)	(0.0236)	
Low-Income	0.0332	0.0057	1.16		0.0071	0.0427*	-1.22
	(0.0174)	(0.0162)			(0.0212)	(0.0202)	
High-Income	0.0373	0.0043	1.23		-0.0062	0.0105	-0.53
	(0.0207)	(0.0169)			(0.0228)	(0.0217)	
By District Child Poverty Rate							
All	0.0351	0.0195	0.61		0.0192	-0.0122	1.01
	(0.0187)	(0.0176)			(0.0216)	(0.0223)	
Black	-0.0015	0.0149	-0.33		0.0815	-0.0086	1.41
	(0.0297)	(0.0397)			(0.0422)	(0.0482)	
Latinx	0.0913*	-0.0237	2.16	*	0.0386	0.0164	0.47
	(0.0359)	(0.0392)			(0.0305)	(0.0356)	
White	0.0434*	0.0142	1.08		0.0103	-0.0151	0.80
	(0.0211)	(0.0168)			(0.0221)	(0.0229)	
Low-Income	0.0216	0.0199	0.08		0.0190	0.0292	-0.35
	(0.0160)	(0.0159)			(0.0215)	(0.0202)	
High-Income	0.0349	0.0158	0.76		0.0008	-0.0079	0.28
	(0.0193)	(0.0161)			(0.0226)	(0.0221)	

Sample is the same as Table 1, limited to 6-10 years after the election. All models include the same controls as Table 1. Models are fit separately among districts above/below the median of composition measures 1 year before the election. Robust standard errors adjusted for district clustering in parentheses. * p<0.05, ** p<0.01, *** p<0.001
Shaded cells indicate significant difference between coefficients by district composition, p<0.05;
$z = (\beta_H - \beta_L)/\sqrt{SE_H^2 + SE_L^2}$ (Clogg et al. 1995).

Table 4: Achievement Cost Effectiveness Estimates for $1,000 per Pupil Increase

Stratified Samples (1 yr before election)	Tax Elections Effect per $1k Current Spending		Bond Elections Effect per $1k Capital Spending	
	High	Low	High	Low
By District Spending				
All	0.04	0.18	-0.03	0.08
Black	-0.02	0.22	-0.03	0.17
Latinx	-0.13	0.37	-0.04	0.11
White	0.02	0.18	-0.03	0.05
Low-Income	-0.02	0.21	0.00	0.10
High-Income	-0.02	0.18	-0.04	0.07
By District % Black				
All	0.20	0.07	0.00	0.01
Black	0.06	0.52	0.05	0.22
Latinx	0.11	0.21	0.06	0.01
White	0.26	0.04	-0.04	0.02
Low-Income	0.08	0.10	0.05	0.02
High-Income	0.16	0.05	-0.01	0.01
By District % Latinx				
All	0.11	0.09	0.02	0.02
Black	0.09	-0.09	0.10	0.01
Latinx	0.10	0.03	0.09	-0.02
White	0.10	0.09	-0.03	0.02
Low-Income	0.05	0.13	0.07	0.03
High-Income	0.07	0.08	-0.06	0.03
By District % White				
All	0.08	0.12	0.02	-0.01
Black	3.42	0.05	0.05	0.12
Latinx	0.25	0.12	0.04	0.05
White	0.08	0.12	0.02	-0.05
Low-Income	0.09	0.08	0.04	0.04
High-Income	0.09	0.07	0.01	-0.04
By District % Free/Reduced Lunch				
All	0.11	-0.05	-0.01	0.03
Black	0.02	-0.10	0.10	0.05
Latinx	0.17	-0.24	0.07	0.01
White	0.12	-0.03	-0.03	0.03
Low-Income	0.08	0.04	0.01	0.05
High-Income	0.09	0.03	-0.01	0.01
By District Child Poverty Rate				
All	0.20	0.06	0.02	-0.02
Black	-0.01	0.04	0.08	-0.01

Latinx	0.51	-0.07	0.04	0.02
White	0.24	0.04	0.01	-0.02
Low-Income	0.12	0.06	0.02	0.04
High-Income	0.20	0.05	0.00	-0.01

Cost effectiveness estimates are the estimated effect of passing a funding election on achievement in standard deviation units from Table 4, divided by the estimated effect of passing a funding election on spending in thousands of 2020 dollars per pupil. Estimates are calculated as the average effect on achievement in years 6-10 after the election divided by the estimated increase in spending in years 1-5 after the election (total current spending for tax elections, capital spending for bond elections).

Table 5: Estimated Effect of Narrowly Passing a Tax Election on Potential Mechanisms

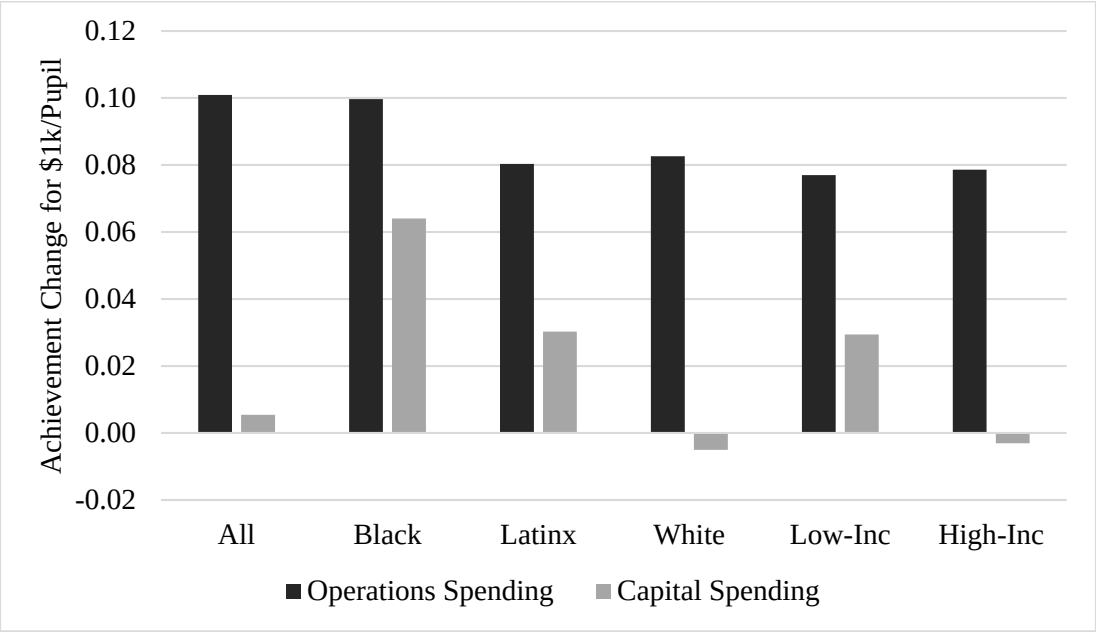
	Tax Elections			Bond Elections		
Dependent Variable	Years -5-0	Years 1-5	Years 6-10	Years -5-0	Years 1-5	Years 6-10
FTE Teachers	2.7060	4.1244	3.2000	-2.2633	-3.1739	-0.2824
	(2.3187)	(2.5882)	(2.8725)	(4.8987)	(5.2526)	(5.7702)
Total Staff	1.0115	7.6159	8.7866	-3.2428	-3.2086	-1.6319
	(4.7229)	(5.2078)	(6.0890)	(9.4720)	(9.9371)	(11.3908)
Counselors	0.1949	0.5955	0.3197	-1.5652	-1.0535	0.2622
	(0.5032)	(0.5208)	(0.4233)	(1.2046)	(1.1195)	(1.2225)
Librarians	0.0710	0.1496	0.3356	-0.9604	-0.6093	0.0592
	(0.2354)	(0.2536)	(0.2634)	(0.6632)	(0.6260)	(0.6304)
Instructional Aides	-1.1535	-0.6581	-0.9547	-8.3629	-3.3261	0.7873
	(2.9943)	(2.9269)	(3.5258)	(8.2585)	(7.4215)	(7.3303)
Administrators	-79.1077	6.6119	168.9436	-342.9807	-171.8526	56.5829
	(281.6783)	(294.1235)	(327.1504)	(706.6503)	(733.0093)	(763.2436)
Administrative Staff	-3.6645	-0.1998	-1.4694	-2.1690	-2.3939	2.8220
	(2.8824)	(2.3346)	(2.1633)	(4.9855)	(5.3618)	(4.9406)
Students per FTE	0.0024	-0.0319	-0.0238	0.1382	0.1493	0.2611
	(0.1591)	(0.1550)	(0.1410)	(0.2537)	(0.2558)	(0.2643)
Students per Staff	0.0892	0.0034	0.0355	0.0608	-0.0032	0.0853
	(0.0873)	(0.0928)	(0.0990)	(0.1350)	(0.1382)	(0.1483)
Students per Counselor	16.0015	3.1802	-24.4357	16.8087	-47.5605	-24.0364
	(23.1096)	(22.3839)	(29.8384)	(52.2013)	(43.0119)	(61.3868)
Students per Librarian	-115.4038	103.4005	-211.8639	1301.6491	562.5352	620.6006
	(114.1864)	(170.9500)	(225.6006)	(843.6432)	(315.6523)	(353.5333)
Salary per Staff	0.9554*	0.8494	1.0880	0.4662	0.8980	0.6687
	(0.4607)	(0.5497)	(0.6347)	(0.7347)	(0.8051)	(0.9104)
Instructional Salary per FTE	0.3743	1.1757*	1.2633	0.7994	1.0409	0.9325
	(0.5185)	(0.5917)	(0.7013)	(0.8431)	(0.9522)	(1.0866)
Total Current Spending/Pupil	0.0382	0.2844***	0.0646	-0.0209	0.0786	-0.0609
	(0.0207)	(0.0681)	(0.0856)	(0.0367)	(0.0812)	(0.1209)
Instructional Spending/Pupil	0.0428	0.1399***	0.0506	-0.0056	0.0220	-0.0078
	(0.0251)	(0.0402)	(0.0553)	(0.0341)	(0.0554)	(0.0735)
Salary Spending/Pupil	-0.0032	0.1468***	0.0678	0.0100	0.0574	-0.0133
	(0.0282)	(0.0403)	(0.0540)	(0.0412)	(0.0569)	(0.0702)
Inst Salary Spending/Pupil	0.0314	0.1113***	0.0748*	0.0127	0.0294	-0.0013
	(0.0212)	(0.0274)	(0.0346)	(0.0324)	(0.0403)	(0.0480)
Benefits Spending/Pupil	-0.0145	0.0230	-0.0338	-0.0194	0.0247	0.0055
	(0.0254)	(0.0312)	(0.0367)	(0.0476)	(0.0569)	(0.0635)
Support Services Spend/Pupil	-0.0065	0.1425**	0.0201	-0.0140	0.0494	-0.0634
	(0.0243)	(0.0478)	(0.0452)	(0.0307)	(0.0445)	(0.0637)
Capital Spending/Pupil	0.1039	-0.0357	0.0346	-0.0173	0.7139***	-0.5205***
	(0.0934)	(0.1098)	(0.1272)	(0.0845)	(0.1587)	(0.1345)

Debt Interest Spending/Pupil	0.0081	0.0070	-0.0057	0.0011	0.1490***	0.0876**
	(0.0171)	(0.0187)	(0.0207)	(0.0214)	(0.0267)	(0.0293)
N District-Years	24226	19616	16538	16014	13436	11028
CRDC Measures						
Chronic Absenteeism/Pupil	-0.0021	-0.0043	-0.0167	-0.0119	-0.0107	0.0007
	(0.0030)	(0.0061)	(0.0100)	(0.0116)	(0.0134)	(0.0097)
Multiple Out-of-School Suspension/Pupil	0.0012	-0.0021*	-0.0023*	-0.0031	0.0010	0.0007
	(0.0014)	(0.0010)	(0.0011)	(0.0025)	(0.0014)	(0.0011)
Expulsion/Pupil	0.0003	-0.0003	-0.0001	-0.0003	-0.0002	-0.0000
	(0.0004)	(0.0002)	(0.0001)	(0.0004)	(0.0002)	(0.0003)
Arrest/Pupil	0.0005	-0.0005*	0.0002	-0.0004	-0.0004	-0.0002
	(0.0004)	(0.0002)	(0.0001)	(0.0004)	(0.0002)	(0.0004)
N District-Years	3398	6753	7782	2402	3733	4241

Coefficients are from OLS regression models predicting each dependent variable. Sample includes district-year observations with a funding election 1998-2018 and voteshare within 10 percentage points of the pass cutoff. Models are fit separately among observations 0-5 years before the election (column 1), 1-5 years after the election (column 2), and 6-10 years after the election (column 3). All models include year fixed effects and pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), and current spending per pupil and enrollment (years 1 & 3 before election, logged). Models predicting CRDC measures have fewer observations due to limited years available. All currency is in thousands of 2020 dollars.

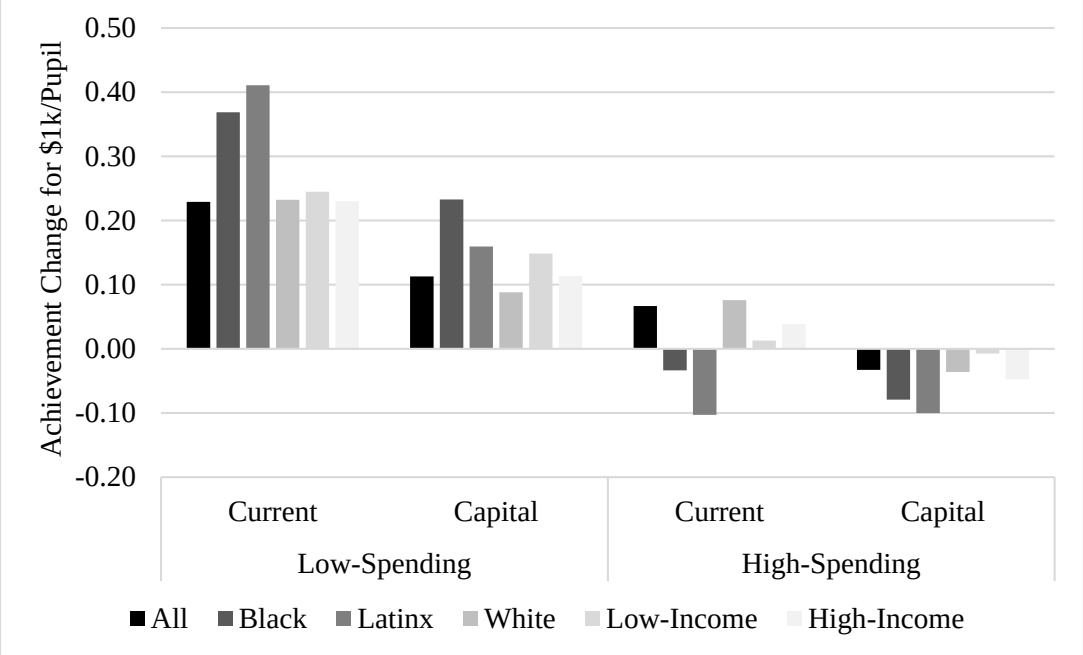
Robust standard errors adjusted for district election clustering in parentheses. * p<0.05, ** p<0.01, *** p<0.001

Figure 1: Achievement Cost Effectiveness Estimates by Spending Type and Student Characteristics



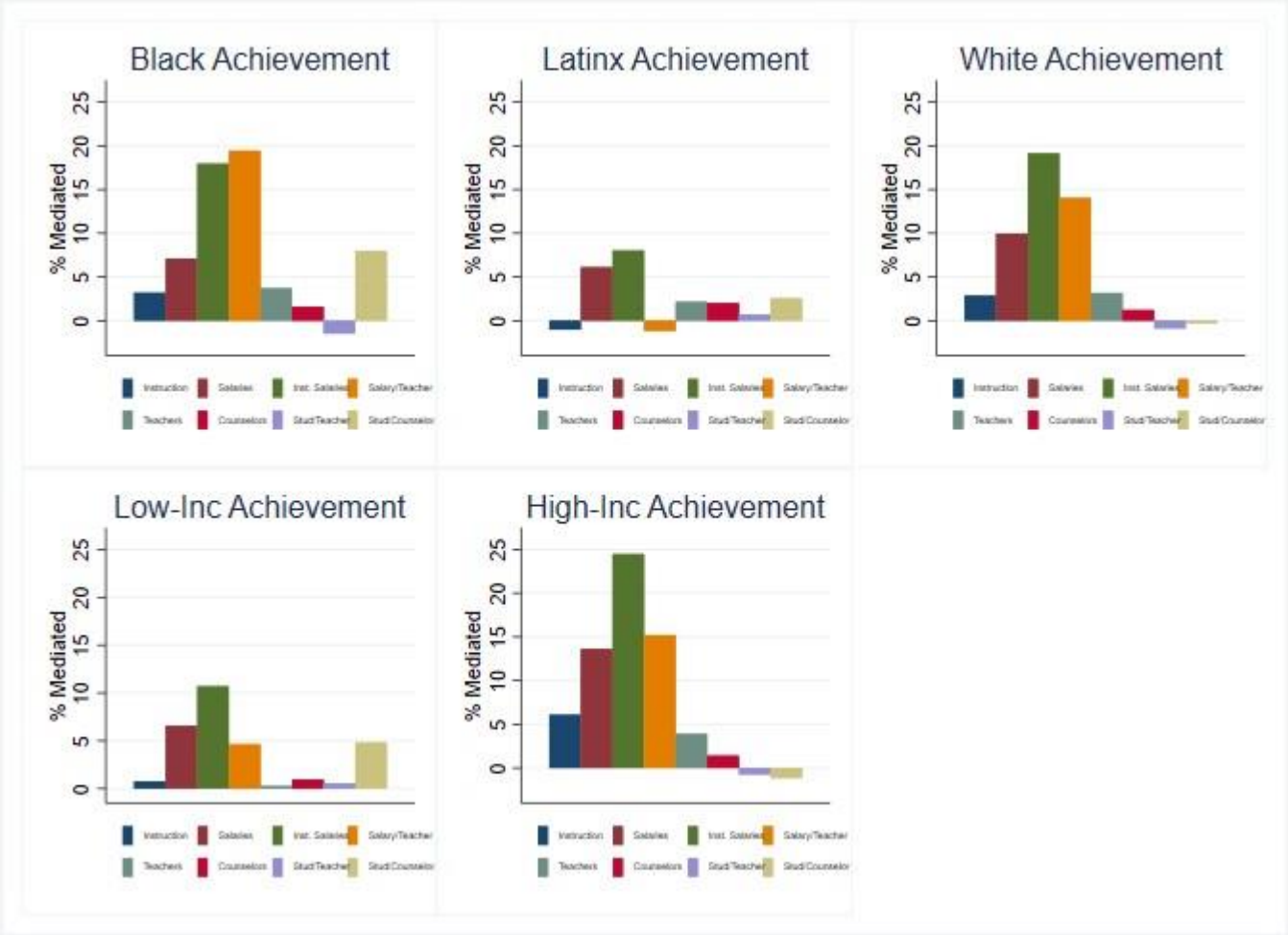
Source: Table 3.

Figure 2: Math Achievement Cost Effectiveness Estimates by Spending Type, Student Characteristics, and Previous Spending



Source: Table 5.

Figure 3: Mediation Estimates



Estimated using paramed in Stata (Emsley and Liu 2013) including district-year observations with a tax election 1998-2018, voteshare within 10 percentage points of the pass cutoff, and 6-10 years after the election. Paramed estimates use linear models, the same controls in Table 2, and allow for interaction between the treatment (passing an election) and the mediator.

Bars indicate the estimated percent of the effect of passing a tax election on achievement mediated by per pupil spending on instruction, salaries, instructional salaries; average salary per FTE teacher; number of FTE teachers, number of counselors, students per FTE teacher, and students per counselor.

Supplementary Online Appendix

Table A1: Descriptive Statistics

	Tax Elections					Bond Elections				
Variable	All	Std Dev	Failed	Passed	Diff	All	Std Dev	Failed	Passed	Diff
Achievement (sd units)	0.11	0.34	0.10	0.12	*	0.02	0.34	0.04	0.01	*
Black Achievement	-0.47	0.30	-0.47	-0.47		-0.43	0.30	-0.43	-0.43	
Latinx Achievement	-0.23	0.33	-0.23	-0.22		-0.29	0.29	-0.29	-0.28	
White Achievement	0.20	0.29	0.18	0.21	*	0.16	0.29	0.16	0.16	
Low-Income Achievement	-0.18	0.25	-0.18	-0.18		-0.26	0.25	-0.24	-0.28	*
High-Income Achievement	0.35	0.26	0.33	0.36	*	0.28	0.27	0.28	0.27	*
Passed Election	0.56	0.50	0.00	1.00	n/a	0.56	0.50	0.00	1.00	n/a
Voteshare	0.87	5.43	-4.40	4.95	*	0.88	5.49	-4.42	5.02	*
% Free/Reduced-Price Lunch (1 yr before election)	0.31	0.21	0.31	0.31	*	0.38	0.23	0.36	0.39	*
% Black (1 yr before election)	0.07	0.16	0.07	0.08	*	0.06	0.11	0.05	0.06	*
% Latinx (1 yr before election)	0.05	0.12	0.05	0.05		0.17	0.23	0.14	0.20	*
Enrollment (1 yr before election)	3402.46	5285.77	3339.89	3450.97		5244.75	16088.16	4198.84	6061.34	*
Enrollment (3 yrs before election)	3430.52	5358.24	3357.44	3487.18	*	5124.00	15582.31	4126.36	5902.91	*
Total Current Spending/Pupil (1 yr before election)	12.20	1.97	12.17	12.23	*	11.44	1.91	11.48	11.41	*
Total Current Spending/Pupil (3 yrs before election)	11.94	1.96	11.92	11.96		11.23	1.95	11.22	11.24	
Total Current Spending/Pupil	12.86	2.37	12.74	12.96	*	11.85	2.31	11.85	11.85	
Capital Spending/Pupil	1.54	3.17	1.48	1.59	*	2.17	3.80	2.06	2.26	*
Year	2014.11	3.11	2013.95	2014.23	*	2013.97	3.18	2013.83	2014.07	*
N District-Years	27,291		11,919	15,372		15,131		6,634	8,497	

Summary statistics of district-year observations 5 years before to 10 years after a funding election held in years 1998-2018 with voteshare within 10 percentage points of the pass cutoff. Two-tailed t-tests indicate whether the mean difference between observations by election outcome is significant: * $p<0.05$.

Observations are smaller for achievement by subgroups: Tax elections: Black N=6542; Latinx N=6220; White N=26475; Low-inc N=25173; High-inc N=25284. Bond elections: Black N=4061; Latinx N=7331; White N=14296; Low-inc N=13796; High-inc N=13815.

All currency is in thousands of 2020 dollars.

Table A2: Descriptive Statistics by District Characteristics in the Year Before a Funding Election

Variable (Measured 1 yr before election)	Tax Elections Low (<Median)	Tax Elections High (>Median)	Diff	Bond Elections Low (<Median)	Bond Elections High (>Median)	Diff
By District Spending						
% Free/Reduced-Price Lunch	0.26	0.30	*	0.38	0.32	*
Child Poverty Rate	0.12	0.14	*	0.15	0.14	*
% Black	0.05	0.11	*	0.06	0.05	*
% Latinx	0.04	0.05	*	0.17	0.14	*
% White	0.87	0.79	*	0.68	0.76	*
Enrollment	3069.02	4003.51	*	5841.08	4973.01	*
Total Current Spending/Pupil	10.72	13.53	*	9.92	12.53	*
Capital Spending/Pupil	1.54	1.72	*	1.20	1.00	*
N District-Years	6,766	6,201		3,444	4,035	
By District % Black						
% Free/Reduced-Price Lunch	0.23	0.36	*	0.29	0.42	*
Child Poverty Rate	0.11	0.16	*	0.12	0.16	*
% Black	0.01	0.19	*	0.01	0.12	*
% Latinx	0.03	0.07	*	0.12	0.20	*
% White	0.93	0.67	*	0.82	0.61	*
Enrollment	2035.90	5946.11	*	2573.39	8800.74	*
Total Current Spending/Pupil	11.79	12.52	*	11.45	11.17	*
Capital Spending/Pupil	1.64	1.60		0.91	1.31	*
N District-Years	8,059	4,908		4,117	3,362	
By District % Latinx						
% Free/Reduced-Price Lunch	0.26	0.30	*	0.28	0.43	*
Child Poverty Rate	0.13	0.13	*	0.12	0.17	*
% Black	0.07	0.09	*	0.05	0.06	*
% Latinx	0.01	0.10	*	0.02	0.31	*
% White	0.90	0.75	*	0.87	0.56	*
Enrollment	2543.14	4643.01	*	2801.38	8195.83	*
Total Current Spending/Pupil	11.87	12.29	*	11.61	11.01	*
Capital Spending/Pupil	1.61	1.64		0.78	1.43	*
N District-Years	6,960	6,007		3,914	3,565	
By District % White						
% Free/Reduced-Price Lunch	0.34	0.22	*	0.48	0.22	*
Child Poverty Rate	0.15	0.11	*	0.18	0.10	*
% Black	0.15	0.01	*	0.10	0.02	*
% Latinx	0.09	0.01	*	0.28	0.03	*
% White	0.69	0.97	*	0.52	0.93	*
Enrollment	5317.09	1868.70	*	8357.96	2313.99	*
Total Current Spending/Pupil	12.51	11.66	*	10.97	11.69	*
Capital Spending/Pupil	1.66	1.60		1.38	0.79	*

N District-Years	6,194	6,773		3,785	3,694	
By District % Free/Reduced Lunch						
% Free/Reduced-Price Lunch	0.13	0.46	*	0.15	0.52	*
Child Poverty Rate	0.09	0.19	*	0.08	0.19	*
% Black	0.03	0.13	*	0.02	0.08	*
% Latinx	0.03	0.07	*	0.06	0.23	*
% White	0.90	0.76	*	0.86	0.60	*
Enrollment	3093.01	4028.97	*	4213.88	6342.35	*
Total Current Spending/Pupil	11.91	12.25	*	11.54	11.14	*
Capital Spending/Pupil	1.77	1.46	*	1.18	1.01	*
N District-Years	7,108	5,859		3,407	4,072	
By District Child Poverty Rate						
% Free/Reduced-Price Lunch	0.16	0.41	*	0.20	0.50	*
Child Poverty Rate	0.07	0.20	*	0.07	0.21	*
% Black	0.03	0.13	*	0.02	0.09	*
% Latinx	0.03	0.06	*	0.08	0.23	*
% White	0.89	0.77	*	0.84	0.60	*
Enrollment	3205.74	3872.30	*	4417.98	6310.12	*
Total Current Spending/Pupil	11.95	12.19	*	11.49	11.17	*
Capital Spending/Pupil	1.80	1.42	*	1.20	0.98	*
N District-Years	6,933	6,034		3,688	3,785	

Mean values in the year before a funding election by district characteristics in the year before the election. Samples include district-year observations in years 6-10 after a funding election held in years 1998-2018 with voteshare within 10 percentage points of the pass cutoff. Low column includes districts below the median value in the year before the election. High column includes districts above the median value in the year before the election. Comparison by % Black includes districts above/below 2% Black (~60th percentile) due to limited observations with Black achievement in the low-Black sample.

Two-tailed t-tests indicate whether the mean difference between observations by district characteristics is significant: * $p<0.05$.

All currency is in thousands of 2020 dollars.

Table A3: Estimated Pre-Election Differences by Election Outcome

	Tax Elections				Bond Elections			
	1 Year Before Election		3 Years Before Election		1 Year Before Election		3 Years Before Election	
Variable	Pass Coefficient	N	Pass Coefficient	N	Pass Coefficient	N	Pass Coefficient	N
% Free/Reduced-Price Lunch	-0.0005	4552	-0.0076	4532	-0.0376**	3387	-0.0327*	3150
	(0.0105)		(0.0103)		(0.0144)		(0.0148)	
% Black	-0.0014	4556	-0.0026	4541	-0.0106	3368	-0.0102	3145
	(0.0085)		(0.0083)		(0.0068)		(0.0072)	
% Latinx	0.0106	4559	0.0075	4541	-0.0154	3386	-0.0150	3145
	(0.0067)		(0.0065)		(0.0148)		(0.0150)	
% White	-0.0111	4558	-0.0088	4541	0.0344*	3385	0.0265	3145
	(0.0126)		(0.0122)		(0.0175)		(0.0176)	
Total Current Spending (log)	-0.0082	4478	-0.0129	4491	-0.0033	3335	0.0060	3118
	(0.0081)		(0.0080)		(0.0113)		(0.0116)	
Enrollment (log)	-0.0483	4480	-0.0621	4491	-0.0594	3335	-0.0554	3118
	(0.0521)		(0.0515)		(0.0819)		(0.0829)	
FTE Teachers	-2.2362	4129	-2.7184	4233	-3.8588	3067	-3.8070	2877
	(6.0335)		(6.0454)		(10.9004)		(10.9970)	
Instructional Aides	-3.9109	4390	-3.9012	4406	-13.5356	2961	-12.9484	2666
	(4.0858)		(4.0173)		(13.6853)		(11.1909)	
Administrators	-353.6866	4462	-561.2925	4403	-1133.7965	3108	-619.5744	2772
	(433.9366)		(437.3708)		(1080.5176)		(1056.4810)	
Counselors	-0.0709	4350	-0.5894	4387	-1.7845	2870	-2.6377	2549
	(0.6580)		(0.6560)		(1.6134)		(1.7893)	
Librarians	-0.1978	4356	-0.2749	4383	-1.0012	2914	-1.2502	2602
	(0.3072)		(0.2984)		(0.7590)		(0.8402)	
Total Staff	-6.8011	4148	-3.5086	4254	-20.6215	3086	-27.9180	2917
	(12.2992)		(12.0365)		(21.7901)		(21.4254)	
Students per FTE Teacher	0.2620	4361	0.0832	4486	0.4331	3301	-0.2546	3106
	(0.2315)		(0.1548)		(0.3175)		(0.4654)	
Students per Counselor	2.8702	4194	37.3489	4262	-8.1408	2657	230.9987	2381

	(28.7116)		(25.0264)		(59.6954)		(118.3857)	
Revenue/Pupil	0.1694	4478	-0.0164	4491	-0.0287	3335	-0.2091	3118
	(0.1728)		(0.1682)		(0.2290)		(0.2872)	
Total Spending/Pupil	0.1152	4478	-0.1874	4491	-0.0147	3335	-0.2318	3118
	(0.2219)		(0.2134)		(0.2400)		(0.3164)	
Total Current Spending/Pupil	-0.0961	4478	-0.1433	4491	-0.0523	3335	0.0437	3118
	(0.1063)		(0.1012)		(0.1339)		(0.1362)	
Instructional Spending/Pupil	-0.0215	4478	-0.0585	4491	-0.0195	3335	0.0662	3118
	(0.0627)		(0.0596)		(0.0824)		(0.0836)	
Support Services Spend/Pupil	-0.0760	4478	-0.0852	4491	-0.0165	3335	-0.0059	3118
	(0.0544)		(0.0511)		(0.0606)		(0.0622)	
Other Spending/Pupil	0.0014	4478	0.0005	4491	-0.0163	3335	-0.0166	3118
	(0.0079)		(0.0088)		(0.0118)		(0.0121)	
Capital Spending/Pupil	0.1409	4478	-0.0853	4491	0.0422	3335	-0.2613	3118
	(0.1742)		(0.1652)		(0.1183)		(0.1834)	
Debt Interest Spending/Pupil	-0.0017	4478	-0.0119	4491	0.0140	3335	-0.0049	3118
	(0.0215)		(0.0211)		(0.0241)		(0.0262)	
Salary Spending/Pupil	-0.0611	4478	-0.1093	4491	-0.0014	3335	0.0425	3118
	(0.0647)		(0.0619)		(0.0761)		(0.0780)	
Instructional Spending/Pupil	-0.0061	4478	-0.0290	4491	0.0070	3335	0.0513	3118
	(0.0422)		(0.0404)		(0.0540)		(0.0550)	
Benefits Spending/Pupil	-0.0662	4478	-0.0663	4491	-0.0175	3335	0.0146	3118
	(0.0408)		(0.0387)		(0.0608)		(0.0616)	
Achievement (sd units)	-0.0067	1803	0.0215	1235	0.0455	1142	0.0414	864
	(0.0289)		(0.0343)		(0.0392)		(0.0461)	

Coefficients are from OLS regression models predicting each variable measured 1 or 3 years before the election. Sample includes district-year observations with a funding election 1998-2018, voteshare within 10 percentage points of the pass cutoff, and in the year of the election. All models include year fixed effects. All currency is in thousands of 2020 dollars.

Robust standard errors adjusted for district clustering in parentheses. * p<0.05, ** p<0.01, *** p<0.001

Table A4: Placebo Tests: Estimated Effect of False Pass Cutoffs on Achievement

	Tax Elections		Bond Elections	
	Low Placebo	High Placebo	Low Placebo	High Placebo
Dependent Variable	(Median Below True Cutoff)	(Median Above True Cutoff)	(Median Below True Cutoff)	(Median Above True Cutoff)
Achievement				
All	0.0282	0.0239	0.0084	0.0008
	(0.0147)	(0.0142)	(0.0163)	(0.0169)
Black	0.0112	0.0082	0.0416	0.0020
	(0.0264)	(0.0272)	(0.0340)	(0.0324)
Latinx	-0.0200	0.0460	0.0104	0.0018
	(0.0324)	(0.0307)	(0.0241)	(0.0258)
White	0.0241	0.0183	0.0006	-0.0022
	(0.0150)	(0.0142)	(0.0164)	(0.0171)
Low-Income	0.0216	0.0197	0.0150	0.0166
	(0.0122)	(0.0122)	(0.0151)	(0.0156)
High-Income	0.0242	0.0180	-0.0023	-0.0084
	(0.0134)	(0.0130)	(0.0162)	(0.0169)
N (All)	13060	12844	7518	7403

Sample includes district-year observations with a tax or bond election 1998-2018, voteshare within 10 percentage points of the pass cutoff, and 6-10 years after the election. All models include year fixed effects and pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), and current spending per pupil and enrollment (years 1 & 3 before election, logged).

Robust standard errors adjusted for district clustering in parentheses. * $p<0.05$, ** $p<0.01$, *** $p<0.001$

Placebo tests assign false pass cutoffs: the median value of voteshare above and below the true cutoff required to pass. Estimates at these false cutoffs are null and further suggest continuity in the outcome-forcing variable relationship at values of the forcing variable other than the pass cutoff.

Table A5: Estimated Effect of Passing a School Funding Election on Achievement: Limited to Observations with Achievement for Each Student Subgroup

Dependent	Tax Elections			Bond Elections		
Variable	Years -5 - 0	Years 1-5	Years 6-10	Years -5 - 0	Years 1-5	Years 6-10
Achievement						
All	0.0275	0.0521	0.0361	0.0064	-0.0018	0.0129
	(0.0331)	(0.0373)	(0.0345)	(0.0458)	(0.0374)	(0.0336)
Black	-0.0186	0.0106	0.0098	0.0187	0.0280	0.0523
	(0.0416)	(0.0417)	(0.0336)	(0.0492)	(0.0439)	(0.0358)
Latinx	-0.0419	0.0174	0.0279	0.0217	0.0323	0.0322
	(0.0531)	(0.0522)	(0.0347)	(0.0425)	(0.0353)	(0.0330)
White	-0.0651	0.0185	0.0470	0.0280	-0.0288	-0.0180
	(0.0464)	(0.0410)	(0.0374)	(0.0458)	(0.0396)	(0.0327)
Low-Income	-0.0255	0.0220	0.0297	0.0109	0.0292	0.0449
	(0.0345)	(0.0325)	(0.0265)	(0.0430)	(0.0332)	(0.0283)
High-Income	0.0358	0.0622	0.0203	0.0083	-0.0172	-0.0090
	(0.0355)	(0.0336)	(0.0335)	(0.0484)	(0.0368)	(0.0312)
N (All)	884	1529	1549	1235	1655	1776
ELA						
All	0.0394	0.0443	0.0138	0.0019	-0.0000	0.0028
	(0.0318)	(0.0348)	(0.0345)	(0.0376)	(0.0312)	(0.0292)
Black	-0.0281	0.0099	-0.0120	0.0203	0.0261	0.0449
	(0.0416)	(0.0396)	(0.0317)	(0.0463)	(0.0408)	(0.0331)
Latinx	-0.0194	0.0169	0.0021	0.0152	0.0401	0.0335
	(0.0451)	(0.0464)	(0.0341)	(0.0349)	(0.0290)	(0.0314)
White	-0.0542	0.0130	0.0300	0.0236	-0.0313	-0.0335
	(0.0498)	(0.0416)	(0.0401)	(0.0421)	(0.0347)	(0.0293)
Low-Income	-0.0152	0.0122	0.0117	0.0048	0.0310	0.0356
	(0.0309)	(0.0295)	(0.0244)	(0.0340)	(0.0270)	(0.0254)
High-Income	0.0488	0.0628	-0.0039	0.0067	-0.0137	-0.0228
	(0.0353)	(0.0320)	(0.0344)	(0.0439)	(0.0313)	(0.0270)
N (All)	884	1529	1549	1235	1655	1776
Math						
All	0.0156	0.0599	0.0585	0.0110	-0.0035	0.0231
	(0.0381)	(0.0415)	(0.0368)	(0.0571)	(0.0461)	(0.0413)
Black	-0.0149	0.0143	0.0253	0.0242	0.0367	0.0621
	(0.0452)	(0.0459)	(0.0380)	(0.0591)	(0.0504)	(0.0428)
Latinx	-0.0656	0.0251	0.0585	0.0305	0.0278	0.0343
	(0.0654)	(0.0611)	(0.0390)	(0.0555)	(0.0460)	(0.0402)
White	-0.0776	0.0246	0.0684	0.0350	-0.0263	-0.0023
	(0.0472)	(0.0427)	(0.0374)	(0.0532)	(0.0471)	(0.0395)
Low-Income	-0.0354	0.0345	0.0488	0.0189	0.0279	0.0543

	(0.0410)	(0.0375)	(0.0312)	(0.0561)	(0.0431)	(0.0363)
High-Income	0.0249	0.0645	0.0461	0.0103	-0.0203	0.0062
	(0.0400)	(0.0375)	(0.0353)	(0.0562)	(0.0448)	(0.0384)
N (All)	884	1529	1549	1235	1655	1776

Sample includes district-year observations with a tax or bond election 1998-2018, voteshare within 10 percentage points of the pass cutoff, and achievement information for each student subgroup: Black, Latinx, White, Low-Income, and High-Income. Models are fit separately among observations 0-5 years before the election (column 1), 1-5 years after the election (column 2), and 6-10 years after the election (column 3). All models include year fixed effects and pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), and current spending per pupil and enrollment (years 1 & 3 before election, logged).

Robust standard errors adjusted for district clustering in parentheses. * p<0.05, ** p<0.01, *** p<0.001

Table A6: Achievement Cost Effectiveness Estimates for $1,000 per Pupil Increase: Limited to Observations with Achievement for Each Student Subgroup

Dependent Variable	Tax Elections (Years 1-10/ Current Spending)	Bond Elections (Years 6-10/ Capital Spending)
Achievement		
All	0.16	0.02
Black	0.04	0.07
Latinx	0.08	0.05
White	0.12	-0.03
Low-Income	0.09	0.06
High-Income	0.15	-0.01
ELA		
All	0.10	0.00
Black	0.00	0.06
Latinx	0.03	0.05
White	0.08	-0.05
Low-Income	0.04	0.05
High-Income	0.10	-0.03
Math		
All	0.21	0.03
Black	0.07	0.09
Latinx	0.15	0.05
White	0.16	0.00
Low-Income	0.15	0.08
High-Income	0.19	0.01

Cost effectiveness estimates are the estimated effect of passing a funding election on achievement in standard deviation units (Table A5) divided by the estimated effect of passing a funding election on spending in thousands of 2020 dollars per pupil.

Tax election cost effectiveness estimates are calculated as the average effect on achievement in years 1-5 and 6-10 after the election divided by the estimated increase in total current spending in years 1-5 after the election.

Bond election cost effectiveness estimates are calculated as the effect on achievement in years 6-10 after the election (due to delayed effects) divided by the estimated increased in capital spending in years 1-5 after the election.

Table A7: Estimated Effect of Narrowly Passing a Funding Election on Achievement: Alternative Functional Form of Voteshare

	Tax Elections		Bond Elections	
Dependent Variable	Voteshare Squared	Voteshare Cubed	Voteshare Squared	Voteshare Cubed
Achievement				
All	0.0241	0.0247	0.0174	0.0392
	(0.0210)	(0.0279)	(0.0248)	(0.0327)
Black	0.0123	0.0254	0.0428	0.0336
	(0.0426)	(0.0588)	(0.0478)	(0.0616)
Latinx	0.0736	0.1013	0.0425	0.0466
	(0.0453)	(0.0590)	(0.0380)	(0.0553)
White	0.0365	0.0494	0.0241	0.0295
	(0.0205)	(0.0276)	(0.0246)	(0.0330)
Low-Income	0.0236	0.0269	0.0232	0.0338
	(0.0179)	(0.0236)	(0.0226)	(0.0280)
High-Income	0.0167	0.0209	0.0288	0.0574
	(0.0195)	(0.0252)	(0.0243)	(0.0335)
N (All)	12967	12967	7479	7479

Sample includes district-year observations with a tax or bond election 1998-2018, voteshare within 10 percentage points of the pass cutoff, and 6-10 years after the election. All models include year fixed effects and pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), and current spending per pupil and enrollment (years 1 & 3 before election, logged).

Models are the same as in Table 1, but include voteshare cubed and its interaction with the pass cutoff in all models, and voteshare cubed and its interaction with the pass cutoff (in the even columns).

Robust standard errors adjusted for district clustering in parentheses. * $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table A8: Estimated Effect of Narrowly Passing a Tax Election on Achievement: Alternative Samples

	Tax Elections Excluding Districts with:			Bond Elections Excluding Districts with:		
Dependent Variable	Low Enrollment	Low Number of Votes	Low Observations	Low Enrollment	Low Number of Votes	Low Observations
Achievement						
All	0.0299*	0.0309*	0.0276	0.0034	0.0052	0.0025
	(0.0145)	(0.0143)	(0.0146)	(0.0164)	(0.0164)	(0.0176)
Black	0.0152	0.0198	0.0208	0.0466	0.0429	0.0534
	(0.0258)	(0.0266)	(0.0267)	(0.0329)	(0.0326)	(0.0361)
Latinx	0.0360	0.0326	0.0272	0.0210	0.0202	0.0317
	(0.0298)	(0.0300)	(0.0302)	(0.0241)	(0.0245)	(0.0259)
White	0.0286	0.0309*	0.0237	-0.0041	-0.0021	-0.0102
	(0.0147)	(0.0145)	(0.0149)	(0.0163)	(0.0164)	(0.0177)
Low-Income	0.0256*	0.0260*	0.0239	0.0211	0.0225	0.0217
	(0.0122)	(0.0122)	(0.0126)	(0.0152)	(0.0152)	(0.0162)
High-Income	0.0229	0.0243	0.0211	-0.0025	-0.0028	-0.0062
	(0.0131)	(0.0131)	(0.0132)	(0.0158)	(0.0160)	(0.0173)
N (All)	12613	12788	12213	7148	7285	6264

Sample includes district-year observations with a tax or bond election 1998-2018, voteshare within 10 percentage points of the pass cutoff, and 6-10 years after the election. All models include year fixed effects and pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), and current spending per pupil and enrollment (years 1 & 3 before election, logged).

Sample exclusions: column 1 excludes observations with enrollment ≤400, column 2 excludes elections with votes ≤200; column 3 excludes districts observed <10 years.

Robust standard errors adjusted for district clustering in parentheses. * p<0.05, ** p<0.01, *** p<0.001

Table A9: Heterogeneous Effects by Student Income and Race/Ethnicity: California

	Achievement (California Assessment of Student Performance and Progress Scale, 2000-3000)					
	Tax Elections			Bond Elections		
Achievement Measure	Years -5 - 0	Years 1-10	Cost Effectiveness	Years -5 - 0	Years 1-10	Cost Effectiveness
All	1.2327	12.7592	0.49	-0.5369	4.1042	0.07
	(1.3754)	(6.7812)		(2.0034)	(6.3172)	
Black	-11.2924	7.9297	0.30	0.7127	-38.7197	-0.67
	(9.6096)	(12.2411)		(7.6231)	(20.2596)	
Latinx	0.1340	14.7410	0.56	-0.5685	7.7623	0.13
	(2.2803)	(8.9581)		(2.3770)	(8.8157)	
White	3.0663	3.0364	0.12	-1.8870	0.4576	-0.35
	(2.1837)	(9.2227)		(2.3867)	(7.9841)	
Low-Income	1.2370	18.4025*	0.70	-1.4461	2.1470	0.01
	(2.8331)	(7.3126)		(2.6701)	(8.9803)	
High-Income	-0.9629	10.8212	0.41	1.0373	3.4940	0.04
	(2.0593)	(7.6497)		(2.1109)	(6.8287)	
N (All)	177	166		479	419	
Low-Income Black	7.0866	92.7769	3.54	8.0512	-11.5329	0.06
	(10.8163)	(45.5186)		(13.8401)	(30.6531)	
N	49	33		120	70	
Low-Income Latinx	2.9663	8.0615	0.31	-0.5305	-4.2407	-0.20
	(2.1994)	(9.0193)		(3.2509)	(8.0785)	
N	145	132		446	377	
Low-Income White	-2.2403	11.0378	0.42	6.5708	9.4559	-0.07
	(8.4937)	(19.4044)		(4.6476)	(10.6679)	
N	107	75		386	303	

Sample includes California district-year observations with a tax or bond election 1998-2018, voteshare within 10 percentage points of the pass cutoff, and achievement data 2015-2022. Achievement is measured in the California Assessment of Student Performance and Progress scale, 2000-3000. Models are fit separately among observations 0-5 years before the election and 1-10 years after the election. All models include year fixed effects and pre-election controls for % Latinx, % Black, and % eligible for free/reduced price lunch (year before election), current spending per pupil and enrollment (years 1 & 3 before election, logged), and the dependent variable (year before election).

Robust standard errors adjusted for district clustering in parentheses. * p<0.05, ** p<0.01, *** p<0.001

Cost effectiveness estimates are standardized coefficients for years 1-10 after the election divided by the estimated increase in spending in years 1-5 after the election in thousands of 2020 dollars per pupil (total current spending for tax elections, capital spending for bond elections).

Tests for differences between coefficients by student race, ethnicity, and income indicated no significant differences at p<0.05; $z = (\beta_H - \beta_L)/\sqrt{SE_H^2 + SE_L^2}$ (Clogg et al. 1995).

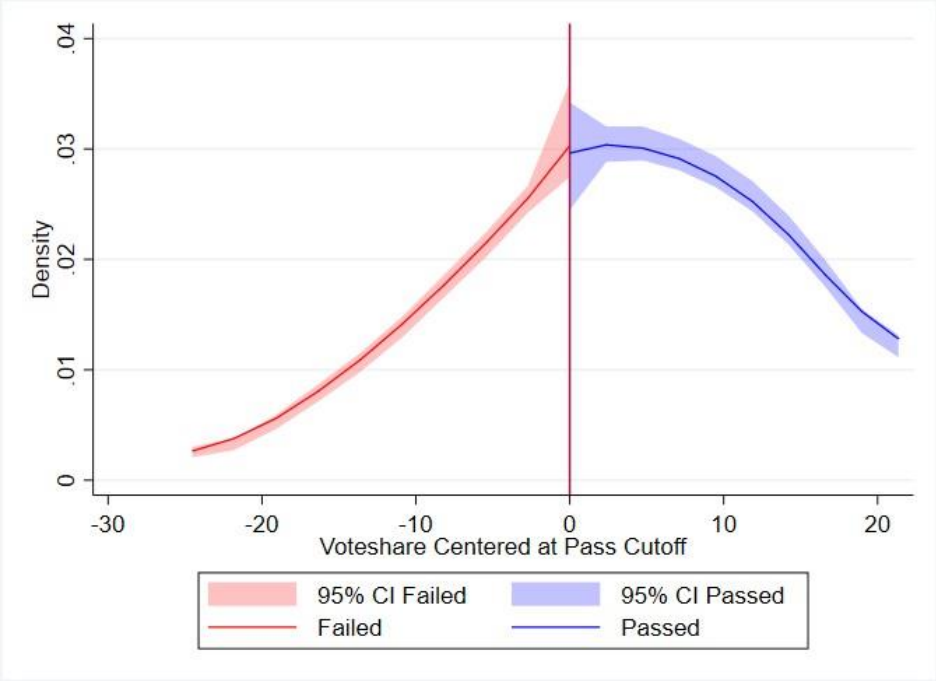
Combining diminishing returns theory with the theorized effects of funding on perceptions and engagement, school funding is expected to increase equality by race, ethnicity,

and income. We expect to find larger benefits of funding among low-income Black and Latinx students compared to White students.

To examine potential intersectionality, Table A9 shows estimated effects of narrowly passing a tax or bond election in California using achievement data in years 2015-2022, measured in the original assessment scale (2000-3000). Sample sizes are small, but still suggest a significant increase in low-income achievement from narrowly passing a tax election. No estimates differ significantly by race, ethnicity, income, or by race/ethnicity and income (e.g., low-income Black compared to White estimates). Cost effectiveness estimates (calculated by standardizing coefficients and dividing by the estimated increase in operations or capital spending per pupil) are again larger for tax elections (operations spending) and, within tax elections, are higher for Black and Latinx students than White students and higher for low-income than high-income students. The estimated cost effectiveness of current spending is particularly high for low-income Black students (3.54 sd per $1,000/pupil). This analysis is based on one state and only eight years of achievement data with separate measures by both income and race/ethnicity, but results are consistent with the main analyses. Results partially contradict *Hypothesis 3* and suggest effects of spending are not significantly different by race, ethnicity, and income.

Figure A1: Density Plot by Voteshare

Panel A: Tax Elections



Panel B: Bond Elections

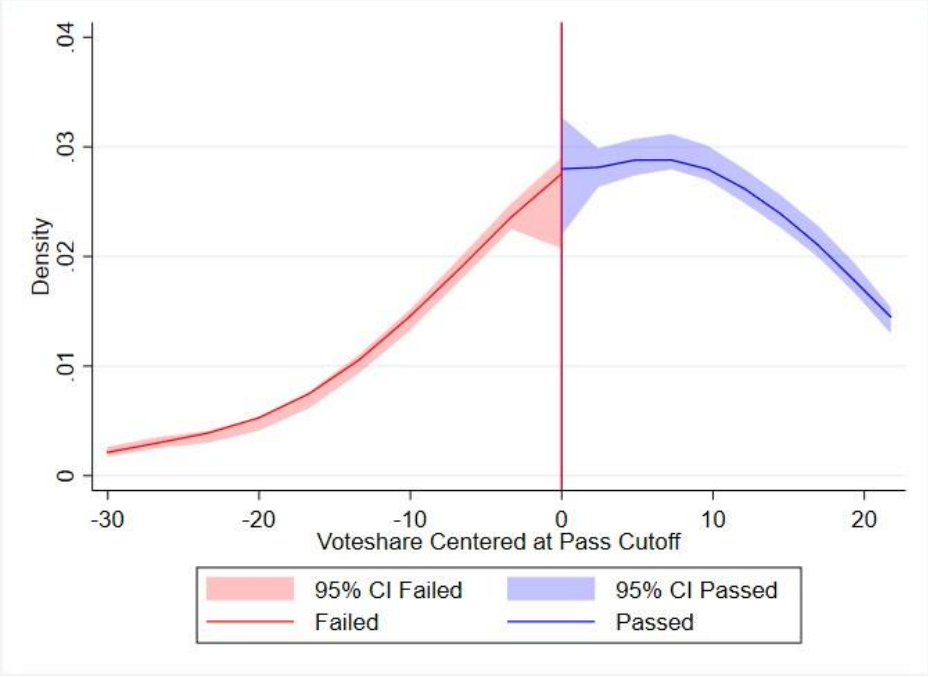
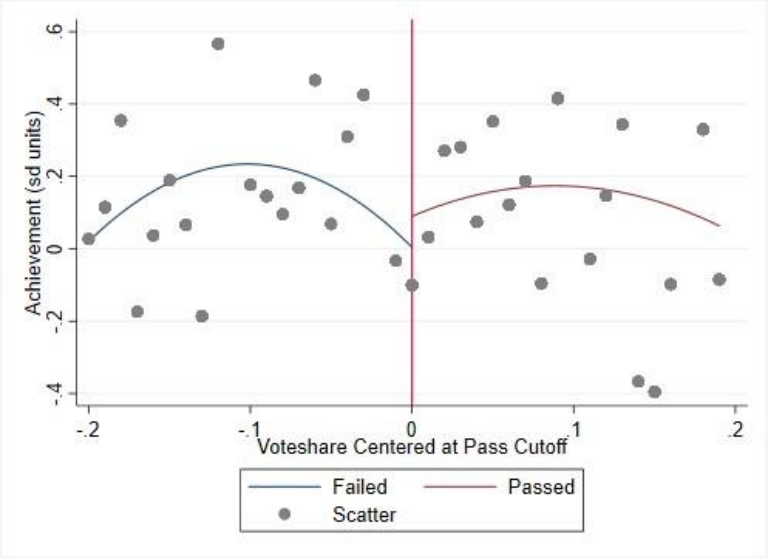
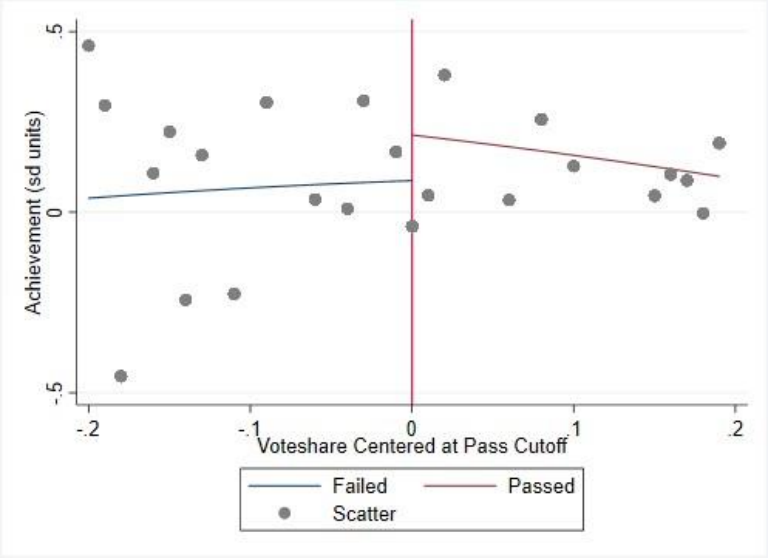


Figure A2: Achievement by Voteshare

Panel A: Tax Elections



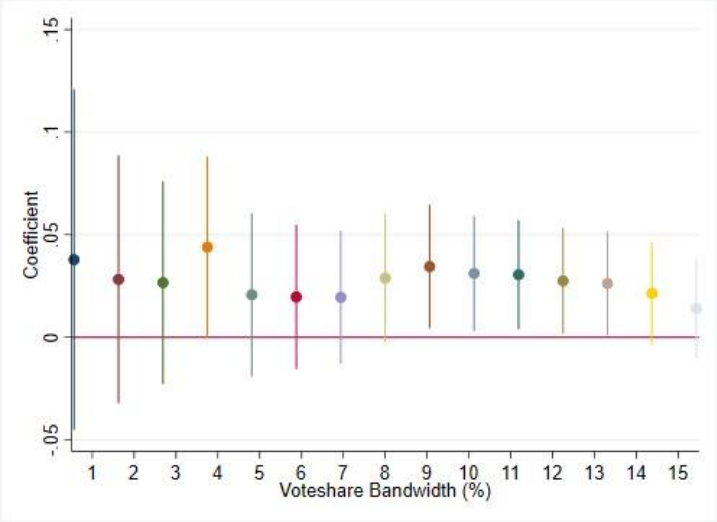
Panel B: Bond Elections



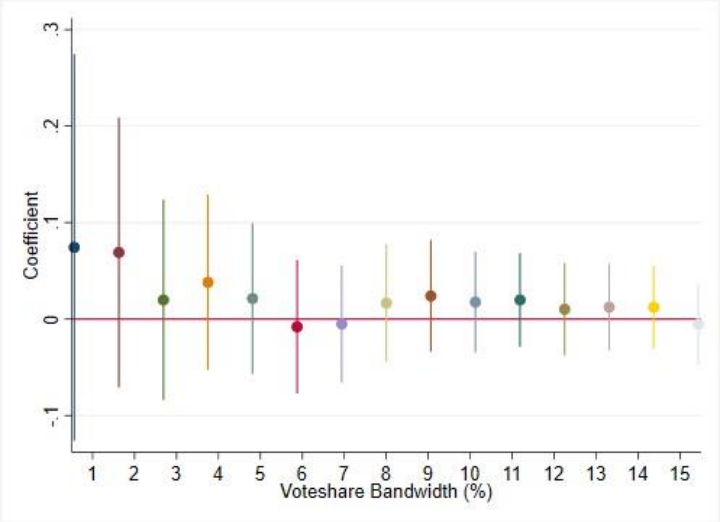
Quadratic fit lines of districts that failed and passed a tax election (Panel A) or a bond election (Panel B).

Figure A3: Estimated Effect of Narrowly Passing a Tax Election on Achievement by Voteshare Bandwidth from the Pass Cutoff

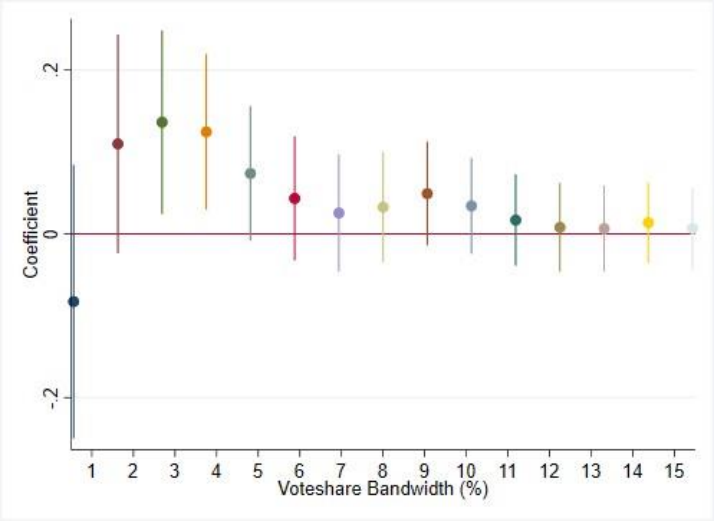
Panel A: Achievement among All Students



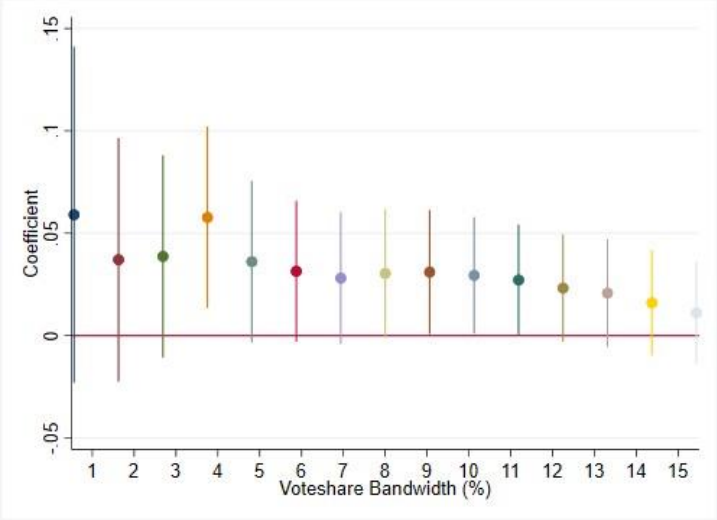
Panel B: Black Achievement



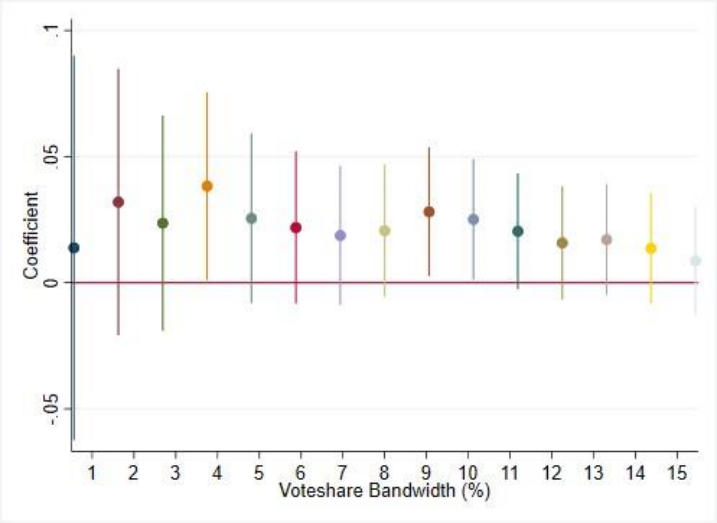
Panel C: Latinx Achievement



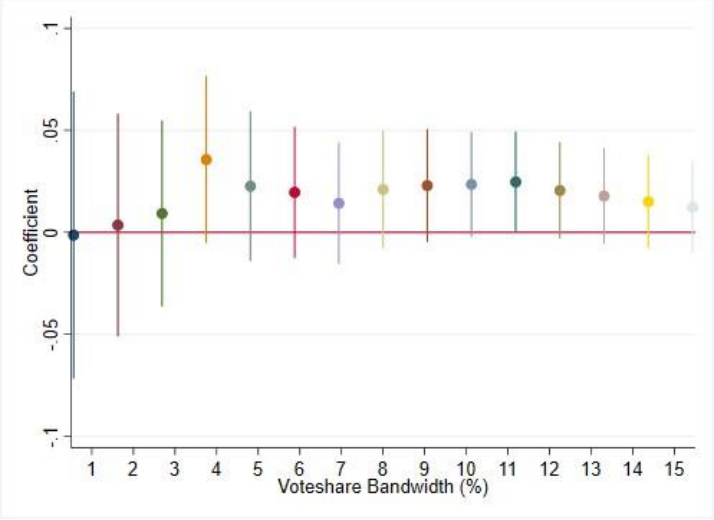
Panel D: White Achievement



Panel E: Low-Income Achievement



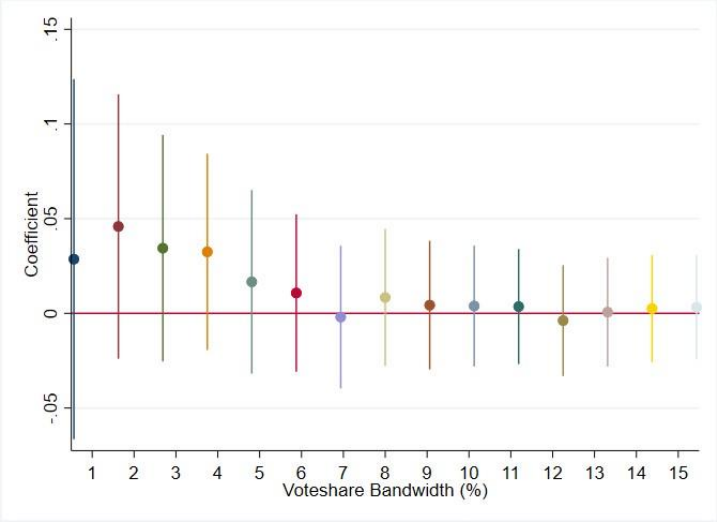
Panel F: High-Income Achievement



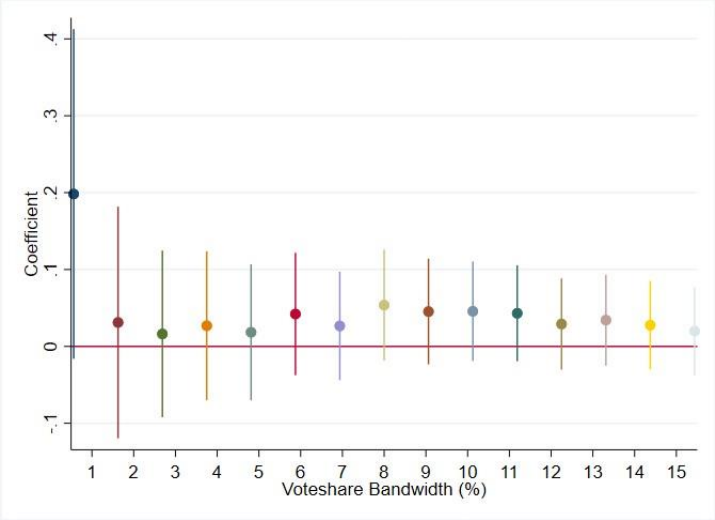
Using the same models used in Table 2 to predict achievement in years 6-10 after the election, coefficients for passing a tax election are shown from separate models when varying the bandwidth of voteshare from the pass cutoff from 1% to 15%.

Figure A4: Estimated Effect of Narrowly Passing a Bond Election on Achievement by Voteshare Bandwidth from the Pass Cutoff

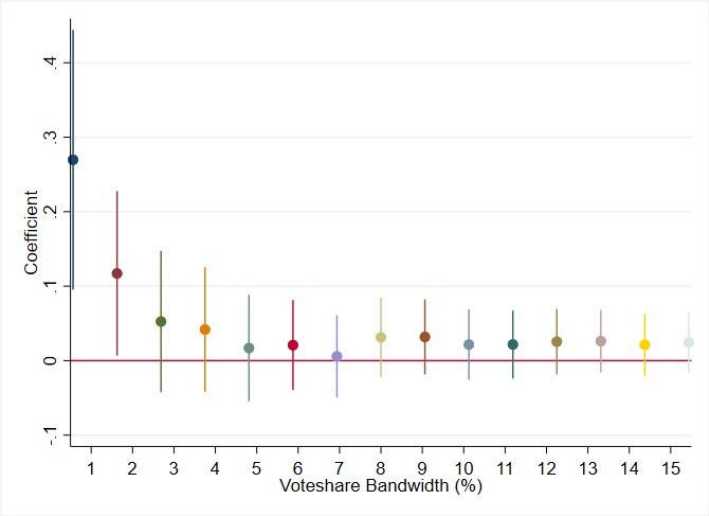
Panel A: Achievement among All Students



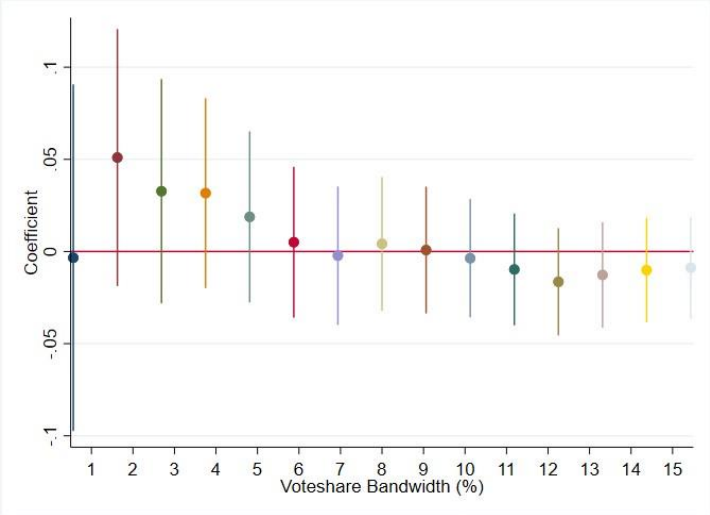
Panel B: Black Achievement



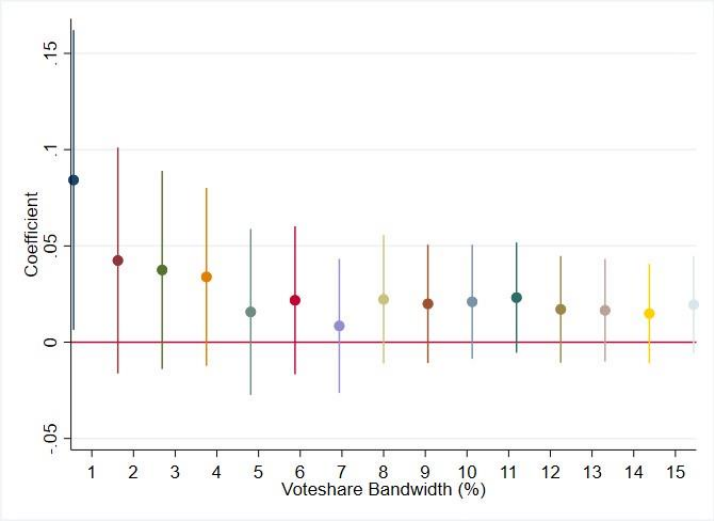
Panel C: Latinx Achievement



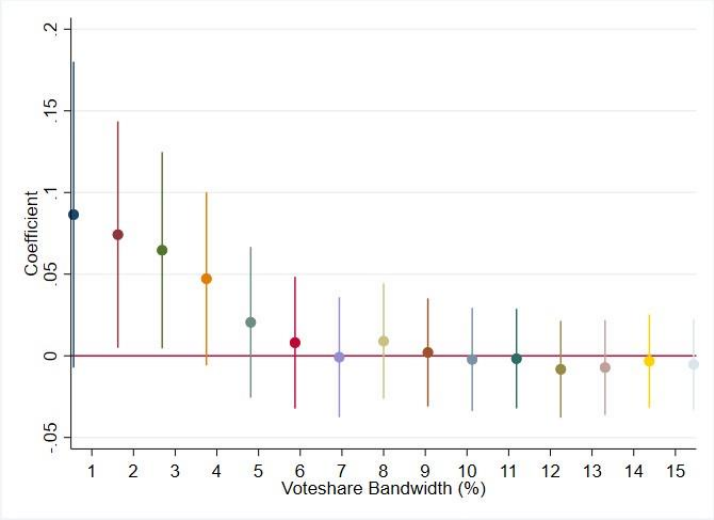
Panel D: White Achievement



Panel E: Low-Income Achievement



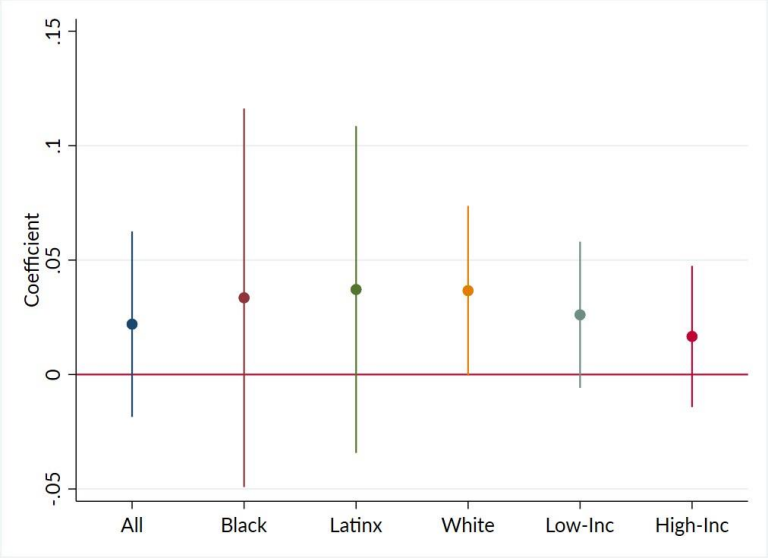
Panel F: High-Income Achievement



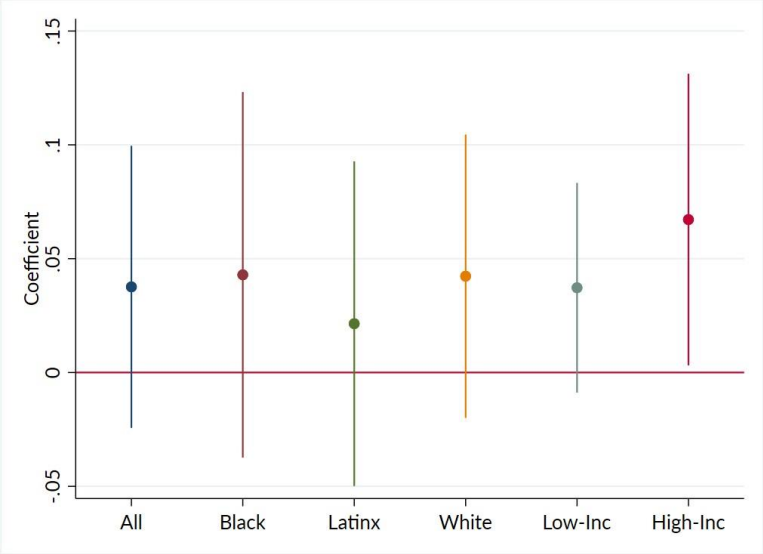
Using the same models used in Table 2 to predict achievement in years 6-10 after the election, coefficients for passing a bond election are shown from separate models when varying the bandwidth of voteshare from the pass cutoff from 1% to 15%.

Figure A5: Estimated Effect of Narrowly Passing a Funding Election on Achievement Using Optimal Bandwidth from the Pass Cutoff

Panel A: Tax Elections

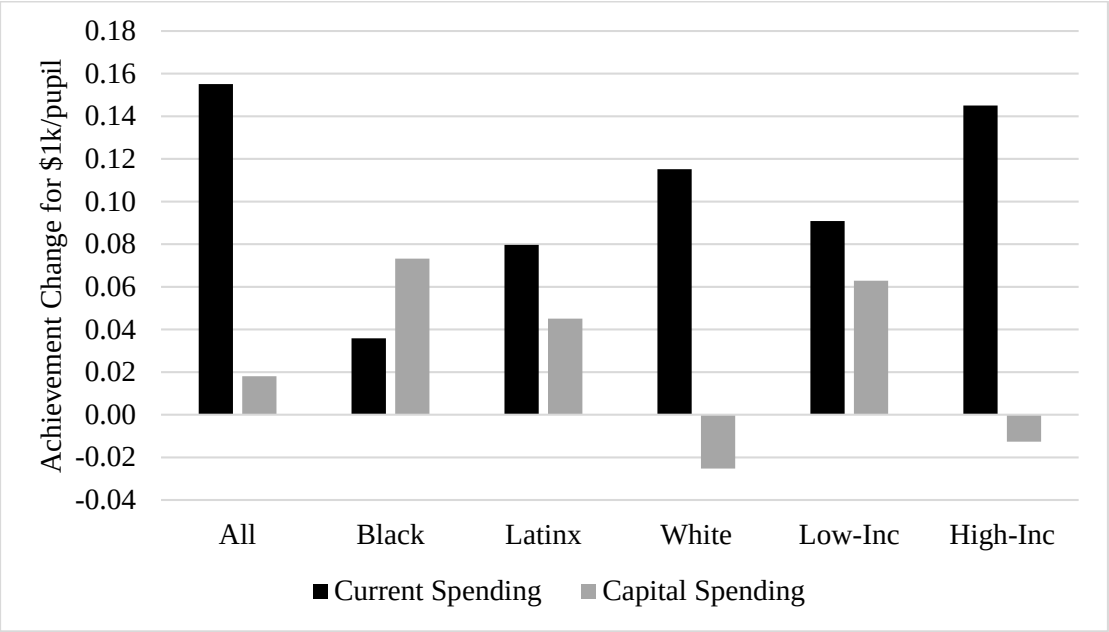


Panel B: Bond Elections



Using the same models used in Table 2 to predict achievement in years 6-10 after the election, coefficients for passing a funding election are shown from separate models when using the optimal bandwidth of voteshare from the pass cutoff selected using rdbwselect (Calonico et al. 2020). Bandwidths for each dependent variable are: Tax elections: All 4.53%; Black 4.58%; Latinx 6.60%; White 5.70%; Low-Income 5.59%; High-Income 6.65%. Bond elections: All 3.63%; Black 7.78%; Latinx 6.36%; White 3.48%; Low-Income 5.73%; High-Income 3.23%.

Figure A6: Cost Effectiveness Estimates by Spending Type and by Student Characteristics: Limited to Observations with Achievement for Each Student Subgroup



Source: Table A6.