Public Bond Issuance and Education Inequality*

NICOLE BOYSON
Northeastern University

WEILING LIU
Northeastern University

This draft: May 2, 2022

ABSTRACT

We show that less wealthy school districts have more difficulty borrowing money through municipal bond markets, which may exacerbate education inequality. When school districts issue more bonds to finance education infrastructure, they experience higher future growth in both test scores and home prices. Yet, despite these tangible benefits, for each standard deviation decrease in a school district's wealth, its education bonds are 47% less likely to be approved via public vote. In addition, less wealthy school districts pay higher yields and agent fees to issue their bonds, even controlling for factors like maturity, coupon, and credit quality. Compared to districts in the wealthiest quintile, the average school district pays $134,190 more to borrow an average-sized bond issue ($61,290 for issuance yields, $48,600 for adviser fees, and $24,300 for underwriting fees). Finally, we identify three contemporaneous constraints that can explain these higher costs: property tax limits, bond marketability, and urgency of funding.

JEL Classification: G18, G51, G14, H74, H75.

Key words: fixed income, municipal bonds, cost of education, market frictions.

* Acknowledgements: We would like to thank Kim Cornaggia, Chris Parsons, Livia Yi (discussant) and the participants at the American Finance Association, the Northeastern Finance Brown Bag, the Northfield Research Seminar, and the QWAFAFEW. We would also like to thank Alex Chen for excellent research assistance.

Please send correspondence to Nicole Boyson (Northeastern University, 412C Hayden Hall, Boston, Massachusetts 02115, phone: 617.373.4775, email: n.boyson@northeastern.edu), and Weiling Liu (Northeastern University, 414F Hayden Hall, Boston, Massachusetts 02115, phone: 617.373.4739, email: w.liu@northeastern.edu).

Conflict-of-interest disclosure statement

Nicole M. Boyson

I have nothing to disclose.

Weiling Liu

I have nothing to disclose.

Section I. Introduction

Schools in the United States borrow over $1.1 trillion for education infrastructure (US Department of Education), yet over half of American schools need significant repairs (GAO 2020). These two seemingly contradictory facts co-exist because in the United States, local property taxes finance most education infrastructure and school districts receive disparate levels of funding. Between 1994 and 2004, schools from wealthy zip codes spent three times more on capital investments than schools from less wealthy zip codes (National Council on School Facilities 2016), and Figure 1 shows that this gap has worsened over time. In addition, less wealthy schools require funding more urgently: wealthy districts often raise funds to enhance their schools, while less wealthy districts use funds for critical repairs, such as repairing roofs or removing asbestos. In this paper, we study the following two questions. First, do financial market frictions keep less wealthy schools from borrowing for education infrastructure? If so, does improving capital market accessibility enhance real outcomes for these districts?

Using a sample of school bond measures from California, we find that lower issuance rates and higher issuance costs amplify the gap in education financing and outcomes between wealthy and poor districts. We begin by showing that after districts successfully issue bonds, both student test scores and district home prices rise. However, less wealthy districts have more difficulty issuing bonds and therefore do not always reap these benefits. Requiring school district voters to approve education bond measures often precludes less wealthy districts from even accessing public markets.[1] Further, when bond ballot measures are approved, less wealthy districts pay higher yields and third-party fees to issue bonds. We identify three constraints that help explain these higher costs and guide future policies: property tax limits, bond marketability, and funding urgency.

Our paper focuses on the sizable education bond market in California. During the 1995-2020 sample period, California school districts authorized over $140 billion in education bonds.

[1] In California, as in most states, residents of school district voters must approve education bond measures. See Figure 2 for more detail.

California is an ideal laboratory for our research question, because regulatory changes in bond voting requirements during the sample period allow us to implement a quasi-experimental design to identify a causal effect of bond issuance on education outcomes. Since most states have similar bond voting and issuance structures to California (see Figure 2), our findings may also be relevant nationally.

We begin by examining the factors that predict (1) education bond proposals (i.e. the likelihood that a district will place a bond measure on the ballot) and (2) voter approval. Not only do large and wealthy districts propose more bond measures, but also, contingent on being proposed, large and wealthy districts are more likely to successfully pass these bonds. A one standard deviation increase in district wealth, proxied by local property taxes per pupil, predicts a 18% increase in the odds of holding a bond election and a 47% increase in the odds of winning voters' approval.

We next study the real effects of bond passage. While approving bond ballots directly leads to more funding for schools, these bonds may not always fund value-improving projects. We first employ a regression discontinuity design around the voting cutoff and find that when districts pass bonds by a small margin, both home prices and capital expenditures increase. To complement this analysis, we employ a quasi-experimental design based on a regulatory event (the passage of Proposition 39) that loosened credit constraints and improved school district access to the municipal bond market. Proposition 39 allowed school districts to issue general obligation bonds (GOBs) with the approval of 55% of school district voters, lower than the prior threshold of 67%. After Proposition 39 passed, the likelihood of bond passage increased by 46%.

Comparing school districts that issue bonds right after to right before the law change, school districts increase local capital spending, improve standardized test scores, and increase average home prices over the next one to seven years. While school districts across all wealth levels benefit from this law change, the most and least wealthy districts benefit most. This analysis provides strong evidence that municipal bonds support value-improving projects.

Less wealthy school districts pay higher bond costs, partially explaining why these voters are less likely to approve education bonds. Even after controlling for credit rating, coupon, and maturity, school districts in the least wealthy tax quintile pay 7 basis points more for underwriters and 11 basis points more for credit enhancements than the median district. By contrast, schools in the wealthiest quintile pay yields that are 28 basis points lower than the median school district. Turning to third-party costs, the wealthiest districts pay 18 basis points below the median for financial advisers, 9 basis points below median for underwriters, and 8 basis points below median for credit enhancements.

Having documented higher costs for less wealthy districts, we next identify three complementary explanations: property tax constraints, bond marketability, and funding urgency. We begin with property tax constraints. California limits the percent of annual property taxes that may be used for debt service. This constraint is often binding for lower wealth districts, making it difficult to both propose and gain voter approval for bond measures. To circumvent these tax constraints, less wealthy districts frequently structure bonds to defer principal repayment as long as possible. Specifically, less wealthy districts regularly issue zero-coupon bonds (called Capital Appreciation Bonds, or CABs) and long maturity bonds, both of which have significantly higher interest costs but which conform to annual property tax constraints imposed by California.

Next, to offset lower marketability, less wealthy districts more often pay for credit guarantees and offer bonds via negotiated offerings with a chosen underwriter. Underwriters in negotiated offerings guarantee a fully subscribed bond issuance, while underwriters in competitive offerings do not provide the same guarantee. This guarantee is costly, as competitive offerings tend to garner lower yields and have lower costs.

Last, we examine funding urgency. Following approval from voters, less wealthy districts issue bonds more quickly, because their schools more urgently need repairs. Conversely, wealthy districts spread bond issuances over time, first approving a large bond measure and then spending it slowly over several years. This approach allows wealthier districts to wait for a more favorable interest rate environment, leading to lower yields. These constraints are particularly troubling when considering that municipal bond interest is state and federal tax-exempt for bond buyers, because this nationwide tax subsidy disproportionately benefits wealthy schools.

State funding is a potential substitute for local funding. However, we show that state funding is sparse and does not significantly benefit less wealthy schools. In California, state funding falls into two categories: new construction and modernization. New construction funding is distributed evenly across districts, but modernization funding is disproportionally distributed to wealthier districts. The first-come-first-served nature of California modernization funding exacerbates this problem: wealthy districts are better equipped to hire financial advisers to navigate the complex state funding system and line up for this limited funding first.

Our work highlights frictions in the public funding market that have important policy implications. For example, one potential solution to the concern that less wealthy districts have trouble obtaining funding is for states to allow their most disadvantaged districts to pool their resources together and issue via a local consortium. Such a consortium would allow these districts to have a larger tax revenue base as well as more negotiation power with agents like underwriters and advisers. Eliminating the first-come-first-served nature of state modernization funding might also help level the playing field. The next section reviews related literature.

Section I.a. Related Literature

This paper contributes to three strands of literature: studies that identify frictions in municipal financing, studies that examine inequality in financial markets, and studies which analyze the costs and benefits of education funding. To our knowledge, we are the first to examine the impact of voting on the municipal market, and the first to link issuance costs to voter approval, and ultimately, to changes in real outcomes. We further expand upon our findings by showing three funding constraints that disproportionally lead to higher costs for less wealthy districts.

First, we contribute to a growing literature that studies frictions in public bond markets. Examining call options, prior work finds that municipals often advance refund bonds (Ang et al. 2017) or delay current refundings (Cohen et. al. 2022) at a present value loss. Another strand of literature highlights high underwriting costs due to several factors including: political corruption (Butler et. al. 2009; Gao et. al. 2019); local opioid abuse (Cornaggia et. al. 2022); conflicts of interest when an underwriter also serves as adviser (Garrett 2020); and insurance guarantees that may not actually increase bond marketability (Cornaggia et. al. 2021). Finally, Joffee (2016) shows that municipal financing issuance fees are high, opaque, and vary widely across districts, amounting to over $3 billion annually. Our paper builds upon past work by showing that high costs discourage voters and may cause useful local projects to go unfunded.

Second, we add to the literature studying how financial markets and mechanisms can exacerbate inequality. In the area of consumer finance, examples include Bertrand and Morse (2011); Melzer (2011); and Agarwal et. al. (2009), which study the implications of payday loans for disadvantaged individuals. Examining the retail banking industry, Taylor (2021) finds that government interventions to promote lending in areas with disadvantaged borrowers lead to worse quality financial services. Looking at entrepreneurs, Fairlie et. al. (2020) find that minority-owned start-ups have a harder time accessing capital. Most closely related to our paper, Dougal et. al. (2019) also study the municipal market and find that Historically Black Colleges and Universities (HBCUs) pay higher underwriting fees to issue tax-exempt bonds. We add to the existing literature by showing that voters in wealthy districts approve education bonds more often than in less wealthy districts. Once approved, bonds from wealthier schools have lower costs.

Finally, we add to a rich body of literature that aims to study how education is funded, and relatedly, whether funding leads to significant economic improvements for school districts. For example, additional education investments are linked with higher rates of graduation and future wages (Jackson et. al. 2016); higher pupil proficiency levels (Hong and Zimmer 2016); and higher home values (Cellini et. al. 2010). In addition to showing that municipal bond issuance and subsequent funding improves test scores and home prices; we find that the most and least wealthy districts benefit more than the median district. Finally, we build upon past findings by showing that funding constraints and issuance fees play a key role in education funding decisions.

Section I.b. Background on the Education Bond Market

Part 1. School District General Obligation Bonds

In 38 of 50 states, including California, school district voters must approve future issuance of school infrastructure general obligation bonds (GOBs) via public ballot. GOBs are municipal bonds backed by the credit and taxing power of the issuer. Like most municipal bonds, GOBs issued by school districts are exempt from federal and state taxes. GOBs fund new construction and modernization infrastructure projects that are too large for ongoing property tax revenues (which cover operating costs such as salaries, supplies, and utilities), and hence, GOBs are the primary source of infrastructure funding for most schools.

To place a GOB measure (referendum) on the ballot, the school board creates a budget and often hires an external consultant to help structure the bond *referendum*. Next, given information about the size and sometimes the costs of borrowing, school district residents vote on the referendum.[2] If the referendum passes, the school district may issue bonds in the future, up to the amount authorized by the ballot. Most bond measures are issued over time in several bond *series*, each of which typically contains dozens of *individual bonds* (each with its own CUSIP number) with varying maturities. For each series, the district hires underwriters and/or advisers to determine the terms of the individual bonds comprising the series.

Part 2. Key Regulatory Events for California School District Bonds

This section provides a timeline of legislative events for California's school district GOB process: Proposition 13 in 1978, Senate Bill 872 in 1993, and Proposition 39 in 2000 (implemented in 2001). In 1978, California passed Proposition 13, The Tax Limitations Initiative, having two key components. First, local property taxes may not exceed 1% of assessed home values. Second, assessed home values are indexed to home values in 1976, with a maximum 2% annual increase. A change in ownership can trigger a reassessment, although this rule has numerous exceptions. As Brunner (2006) notes:

> The passage of Proposition 13 shifted the primary responsibility for financing new school construction and modernization from local school districts to the state. By prohibiting property tax overrides to fund local general obligation bonds, Proposition 13 eliminated the primary source of local revenue for new school construction and modernization. Consequently, in the aftermath of Proposition13, school districts were forced to turn to the state to meet their school facility needs.
> – Brunner, p. 6.

[2] Figure 3 presents a sample bond measure.

This local funding drought lasted until 1986 when voters passed Proposition 46 allowing local districts to issue GOBs with 66 2/3% voter approval and lifting the 1% ceiling on property taxes. These GOBs must comply with a limit on the total outstanding debt for each school district. This limit is 1.25% of assessed value for elementary and high school districts and 2.5% of assessed value for Unified (combined elementary and high school) districts.

In 1993, Senate Bill 872 was signed into law. This bill allowed school districts to issue capital appreciation bonds (CABs). CABs are zero-coupon bonds: they are sold at a deep discount, make no intermediate interest payments, and pay back the initial amount borrowed plus accreted interest at maturity. School district boards that wish to issue bonds immediately but defer bond expenses to a later date find CABs particularly attractive. Because CABs make no periodic interest payments, they do not require an immediate increase in property taxes. Instead, CABs rely on projected growth in property values to fund current projects, many of which may be obsolete when the debt is paid off.

In 2000, California voters approved Proposition 39, the Smaller Classes, Safer Schools and Financial Accountability Act. This act allows districts, under certain conditions, to issue GOBs with the approval of 55% of voters. This act does not replace existing law allowing bond measures to be approved with a 66 2/3% vote, but rather, gives districts a choice between 55% bonds and 66 2/3% bonds. Special conditions to issue 55% bonds apply: 1) the local ballot measure to include a list of the specific school facilities to be addressed, 2) performance and financial audits by the governing board, 3) limits on the amount of additional property tax imposed for debt service, and 4) that the bond measure appear during a general or primary election and not a special election. Importantly, after passage of Proposition 39, districts rarely issue bonds requiring a 66 2/3% vote; since 2002, *97.5% of bond measures conform with Proposition 39 and require a 55% vote*.

Section II. Data

We combine five main data sets: bond ballot, bond issuance, school district characteristics, outcomes (i.e., test scores and home prices), and local tax collections.

We collect bond ballot data from the California Secretary of State's website from 1995 to 2019.

In California, voters approve general obligation bond (GOB) issuances via a referendum, and once approved, districts borrow the total authorized amount over time in separate issuances (series). For example, if in 2003, voters in authorize $100 million, the district might issue as follows: $30 million in 2004 (bond series 1), $45 million in 2007 (bond series 2), and $25 million in 2009 (bond series 3). Each series is comprised of several individual bonds (average number is 20), each with a unique identifier (i.e., the CUSIP number), face value, coupon, yield, price, and maturity date. We examine bonds at all three levels: the total authorized level, the bond series issuance level, and the individual CUSIP level. After voters authorize a bond, school districts have discretion over both the timing of series issuance and the terms of the bonds within the series.

Table 1 Panel A summarizes election data (referendums) for 1,256 separate bond proposals. The average authorized amount is $111 million with a median of $40 million. Most proposals were authorized after 2001, when Proposition 39 decreased the required passing cutoff to 55%. Voters approve most bond proposals during the sample period, with an average win percentage of 68%. Further, bond proposals were much more likely to pass after Proposition 39.

Turning to *series* level data, Table 1 Panel B reports data for 2,591 unique bond series. We hand match bond series data to voting data. The mean total issuance per series is $27 million (about 25% of the $111 million authorized from Panel A), with a median of $14 million. The average yearly interest cost is 4.5%. About 35% of series include at least one capital appreciation bond (CAB). About 65% of series include at least one insured bond. The median series has high credit quality with a Standard & Poor's rating of "A" (not tabulated). Total fees for each series average roughly 2.61% of face amount, or $382,000; underwriting fees are the largest cost, followed by adviser, counsel, and credit enhancement fees.

Table 1 Panel C summarizes school district characteristics, based upon data released by the state of California. The average school district has 7,212 pupils, with a median of 3,301. About 42% of districts are classified as Unified: a combined high school and elementary school district. Of the remaining districts, 11% are high school districts and 47% are elementary school districts. Another way to classify districts is by city (46%), suburb (21%), town (14%) or rural (19%). These definitions are based on economic activity, geographic dimension, and population density, as determined by the National Center for Education Statistics. The average school district in the sample provides free or reduced-price lunches to one-third of its pupils, and just over half its pupils are non-white. These characteristics vary greatly across districts. For example, the 25th and 75th percentile of free/reduced meals are 9.3% and 53.7% respectively.

Table 1 Panel C also reports outcomes data: home prices from Zillow and test scores from the state of California. The median home price is $378,186, while the 75th percentile is much higher at $616,518. Table 1 also reports several variables related to standardized test scores. As background, the Academic Performance Index (API) is a transformed number based on the California Standards Test (CST) and California High School Exit Exams (CAHSEE). Instead of averaging raw test scores, the API normalizes scores by comparing a school's performance to its peers based on pupil demographics like race and parental income. All schools have a target index of 800, and the score ranges from a low of 200 to a high of 1000. Since California stops reporting API data in 2013, we collect other proxies of pupil achievement beginning in 2014, including the fraction of pupils who meet state minimum achievement standards. The average API in our sample is 744, and the average percent of pupils who do not meet standards is 30.7%.

Finally, we report data for property taxes, infrastructure spending, and debt per pupil collected from the state of California. Similar to enrollment, the distribution of property taxes is right-skewed with a mean of $3,456 per pupil per year but a median of $2,557. Property taxes comprise about one-third of district capital outlay, averaging $1,154 per pupil per year. Local funding makes up most of the outlay with an average of $1,119 per pupil per year compared to just $289 for state funding. For state funding, new construction is $167 per pupil per year and modernization spending is $113 per pupil per year. Finally, long-term debt averages $5,825 per pupil per year.

As a first look at bond pass rates, we split school districts into five groups based upon past bond ballot success. Table 2 Panel A provides means of key variables across these groups. Schools that always succeed in approving bond measures are large and wealthy, with average enrollment of 8,006 pupils and home price of $554,917; whereas schools that never propose bond ballot measures ("Never try") have average enrollment of just 2,551 pupils and average home price of

$317,191. Schools that always succeed also perform better academically, with an average API score of 749, while those that never try score 733.

Table 2 Panel B calculates differences in means comparing school districts have at least one bond issuance (the sum of columns 2 and 3 from Panel A) to school districts that never issue a bond (the sum of columns 4 and 5 from Panel A). Generally, bond-issuing districts have many more students (6,600 difference) and are more likely to be Unified (19% difference) or classified as a city (30% difference). Bond-issuing districts have more valuable homes (difference of $200,000), higher test scores (API difference of 9 points), and fewer pupils that do not meet minimum standards (4% fewer). Finally, bond-issuing districts collect more property taxes ($1000 more per pupil) and have higher capital expenses ($342 more per pupil), most of which comes from local spending. These statistics provide preliminary evidence that that local public financing provides the bulk of infrastructure funding for schools and that schools districts with more public financing are associated with better outcomes.

Overall, these findings indicate inequality in both capital spending and outcomes that depends on the wealth and size of school districts. We next corroborate this inequality, explore the potential mechanisms that hinder schools' access to public funding markets, and investigate why state-level funding has been ineffective in closing the gap.

Section III. Empirical Findings

Section III.a. Wealth and Access to Funding Markets

Figure 1 shows that the gap in capital funding between less and more wealthy districts has widened substantially over time, amounting to almost $35,000 in per pupil spending over the last 20 years (roughly $1,750 per pupil per year). In this section, we examine the relationship between a district's wealth and its probability of asking voters to approve a new education infrastructure bond. We perform the following Logit regression analysis using bond authorization elections at the school district and year level. For each school district i and year t, we estimate:

$$Election_{it} = \tau_t + \beta_1 \ln enrollment_{it} + \beta_2 wealth + \beta_3 X_{it} + \varepsilon_{it} \tag{1}$$

where τ_t are year fixed effects, ln $enrollment_{it}$ is log of total pupil enrollment, $wealth$ is property taxes per pupil[3], and X_{it} is a vector of district characteristics including: expenses per pupil, debt per pupil, demographics such as percent of non-white pupils, and geographic traits such as a dummy for whether the school is in a rural area. We normalize wealth using pupil enrollment, since it is the most reliable measure of size at the district level[4]; but results hold if we normalize by county-level population instead[5]. The key outcome variable is $Election$, a dummy equal to 1 if the school district has at least one bond measure on the ballot that year.

Columns (1) to (3) of Table 3 Panel A show our findings. Column 1 includes year fixed effects and district size; column 2 adds additional geographic controls; and column 3 shows the full specification with all controls. Consistent across specifications, large and wealthy school districts are more likely to initiate bond measures. To interpret the 0.219 coefficient on wealth in column 3, a one standard deviation increase in log of property tax (0.75) predicts 1.18 higher odds (an 18% increase in the likelihood of holding an election). Other characteristics also predict the probability of bond election; for example, schools in rural areas have a 39% lower likelihood of an election. Overall, wealthy and large schools in non-rural areas hold the most bond elections.

To test whether a school district's wealth affects its ability to *win* a bond election, we conduct the following Logit regression for bond elections in school district i and year t:

$$Pass_{it} = \tau_t + \beta_1 \ln enrollment_{it} + \beta_2 wealth + \beta_3 X_{it} + \varepsilon_{it} \qquad (2)$$

Each observation is a bond election, so we drop schools and years with no education bond ballot. The independent variables are the same as in Equation (1), and the outcome variable is a dummy set to 1 if the bond measure passes. Table 3 Panel B summarizes findings. Column 1 includes year fixed effects and the log of total pupil enrollment, and columns 2 and 3 add district characteristics. Conditional on initiating a bond measure, we find that larger and wealthier schools win more often.

---

[3] It is likely that wealth is correlated with omitted measures of quality, such as student intelligence, that also affect the outcome variable. While we cannot measure quality directly, we can approximate it by controlling for standardized test scores (shown in Internet Appendix Table A3.1), and our findings do not qualitatively change.

[4] In most cases, population is reported at county level, since households can fall into multiple districts. California enacted a policy of open enrollment in 2010 allowing families to select their school district regardless of geography.

[5] These results, reported in Internet Appendix Table A3.2, comport with the main findings. They help alleviate the concern that large districts with only a few students may bias our proxy for wealth.

Interpreting the coefficient on wealth in column 3, a one standard deviation increase in log property tax (0.75) predicts a 47% increase in the likelihood of a bond measure winning. Passage rates do not differ among rural, city, or suburban districts. Overall, results from Tables 2 and 3 provide strong evidence that large and wealthy districts access bond markets more, because they both propose and win more bond measures than other districts.

We next study how an important 2001 regulation affected bond issuance outcomes. In November 2000, voters in California approved Proposition 39, lowering the voting threshold to approve school district GOBs from 66 2/3% to 55%[6]. Evidence suggests that the passage of this law was uncertain, and thus, not fully anticipated. Local surveys found that "a few weeks before the election, a bare majority of voters said they would vote 'yes' on the bill." (Baldassare, 2002), and a similar version of this law (Proposition 26) was struck down by voters just nine months earlier. In November 2000, Proposition 39 narrowly passed with 53% of voter support.

We investigate whether Proposition 39 1) led to more bond measure proposals and/or 2) led to more bond measures passing. Table 3 Panel A column 4, estimates the Logit regression:

$$Election_{jt} = \tau_t + \beta_1 \ln enrollment_{jt} + \beta_2 wealth + \beta_3 X_{jt} + \beta_4 Post2001_t + \varepsilon_{jt} \qquad (3)$$

The dependent and control variables are as in Equation (1), and $Post2001_t$ is a dummy set to 1 if the bond election uses the 55% threshold. If looser voting requirements led schools to initiate more referenda, then β_3>0. Table 3 Panel B column (4) estimates the Logit regression:

$$Pass_{jt} = \tau_t + \beta_1 \ln enrollment_{jt} + \beta_2 wealth + \beta_3 X_{jt} + \beta_4 Post2001_t + \varepsilon_{jt} \qquad (4)$$

The key outcome variable, $Pass,$ is a dummy set to one if the bond measure passes. If looser voting requirements made approval more likely, then β_3>0.

We find that Proposition 39 increases a bond measure's likelihood of passing, although it does not increase the likelihood of a school district proposing a bond measure. Specifically, in Panel A,

[6]After Proposition 39, some districts initiate bond measures at 66.67%, because the 55% threshold set limits on property taxes levied to repay the bond. *However, 97.5% of measures use the lower 55% threshold*. Hence, we do not count the 2.5% of measures with a 66 2/3% threshold as "treated." Results do not change if we drop these measures.

column 4, the coefficient on *Post2001* is positive but insignificant.[7] This finding suggests that schools generally request bond market access when needed, and they do not try to "game the system" after the regulation passed. Next, Table 3 Panel B column (4) examines the likelihood that bond measures will pass: here, the coefficient on *Post 2001* is positive and statistically significant at the 1% level. This result provides strong evidence that Proposition 39 loosened credit constraints by allowing more bond measures to pass after 2001. Next, in Section III.b., we use Proposition 39 as a quasi-experiment to examine how loosening borrowing constraints affect educational outcomes.

Section III.b. The Effect of Bond Passage on Real Outcomes

Section III.b.1. Winning Margins and Close Votes

So far, we find that less wealthy districts have more difficulty accessing public borrowing markets. This finding would not be worrisome if, for instance, less wealthy districts are likely to propose unnecessary or wasteful infrastructure projects. If this conjecture is true and voters understand this, then their tendency to reject bond ballots more frequently would be both rational and optimal. We test this conjecture in two ways. First, in this section, we compare outcomes in districts with failed bond ballots to districts with successful bond ballots, and we focus on narrow margins around the winning threshold using a regression-discontinuity design. However, since there are not too many bond ballots in California that narrowly pass or fail, we have a limited number of observations and cannot control for all district characteristics. Thus, in the next section, we corroborate this analysis by using Proposition 39 as a shock that loosened borrowing constraints, and we find similar outcomes, *even controlling for school district fixed effects*.

To estimate a regression-discontinuity design, we estimate the following for each bond measure in district i and time t:

$$Y_{it+k} = \alpha + \beta_1 pass_{it} + \beta_2 margin_{it} + \beta_3 pass_{it} * margin_{it} + \beta_4 X_{it} + \beta_5 Y_{it} + \varepsilon_{it} \qquad (5)$$

[7] Proposition 39 did not have a differential effect with respect to wealth (Internet Appendix Table A 2.3). While wealthy schools pass more measures, both wealthy and non-wealthy schools propose marginal bonds that would not have passed before 2001.

$$\text{where } -b < margin_{it} < b;\ b \in \{30\%, 20\%, 10\%\}$$

The outcome variable Y is one of the following, measured k years later: local capital spending, API (test scores), or home prices. $margin$ is the difference between the percent of yes votes received and the threshold for passing; we restrict our sample to bond measures using the 55% cutoff for uniformity. For example, if a bond measure passed with 60% yes votes, it would have a margin of 5%. $pass$ is a dummy equal to one if a bond passed, or its $margin > 0$. For simplicity, we use a rectangular kernel (no weighting) and use three different bandwidths around the cutoff: in our first sample, we study bonds which passed within a 30% difference from the cutoff; in the second sample, within a 20% difference; and finally, in our tightest estimate, within a 10% difference from the voting cutoff. If passing a bond leads to significant improvements, then we would expect $\beta_1 > 0$. β_2 captures the relation between bond ballot popularity and outcomes; and β_3 captures the *differential* relation between ballot popularity and outcomes *for just the ballots that have passed.*

Table 4 shows results. In each panel, columns (1)-(7) correspond to outcomes measured 1 to 7 years after the vote. In Panel A, when a measure is passed, the school district's capital expenditures increase over the next four years. Examining the smallest band (10%) around the voting cutoff of 55%, expenditures increase for one to three years after bond passage. This finding is intuitive: bond ballots directly approve more capital and it takes a few years to issue bonds and obtain funding. In addition, *margin* has a positive coefficient, coupled with a negative coefficient of similar magnitude on the interaction term. This suggests that while there is no effect for districts that pass bonds ($\beta_2 + \beta_3 = 0$), districts that fail to pass bonds (*margin*<0) have a decline in local expenditure three, four, and five years later. This finding implies that districts continuously rely on bond capital and reduce funding when bond referenda do not pass.

Next, in Panel B, we examine school districts' change in test scores following the passage of a bond ballot. In the widest bandwidth (30%), we see significant improvements in API scores two to four years following bond approval, suggesting positive effects. However, as we narrow in around the voting threshold, this effect diminishes, and we find a small positive effect three years following ballot passage (20% bandwidth) or no effect (10% bandwidth). This may not be too surprising, because our measure of test scores, the API, is standardized each year by the state of

California based on a set of student demographics and its interpretation varies between school districts. Thus, it may not be a fair comparison to compare API score growth *across* school districts. To remedy this, the following section, we will complement our analysis with a *within-district* analysis, where we do find positive and significant effects of bond passage.

Finally, Panel C examines the effect of bond passage on the home prices in the district. School district quality is an important factor in housing values. Thus, if residents expect bond ballot approvals to increase the quality of their school district, then it should be reflected in higher home prices. Across all three bandwidths considered, we find evidence supporting this hypothesis, although the positive effect of bond passage gets smaller as we narrow in on a small set of close votes around the voting threshold. Looking at the tightest margin (10%) we find that home prices grow two and four years following bond ballot passage. We also find that the coefficient on vote margin is negative, while the coefficient on the interaction term is positive and similar in magnitude. This suggests that, while there is no effect for districts that pass bonds ($\beta_2 + \beta_3 = 0$), districts that fail to pass bonds ($margin < 0$) experience an increase in home prices. While this result may seem puzzling at first, it may be explained by the fact that voters are less likely to approve bond borrowing if they are already content with their district and expect it to develop well in the future, which is reflected in the higher home prices.

Overall, our findings suggest that bond passage has positive, tangible effects such as higher capital expenditures and home values. Since we have a limited number of observations in this regression discontinuity design, we will next complement our findings by next using Proposition 39 as a pseudo experiment and compare within school districts to identify the effects more sharply.

Section III.b.2. Proposition 39 and Loosening Borrowing Constraints

In this section, we use Proposition 39 to identify the effects of loosening constraints and improved bond market access, *controlling for unobservable characteristics of the school district using district fixed effects.* The most straightforward way to measure the effect of funding access on real outcomes is to regress outcomes on a dummy variable for a bond measure passing. However, many districts anticipate future borrowing needs and request bond approval long before they need it. As a result, even after a bond measure passes, outcomes may take a long time to change. Many districts also smooth their issuance so that the average amount of bonds *issued* every year is relatively smooth, even though the amount *authorized* by a yes vote can be staggered and lumpy.[8] Due to smoothing, a simple OLS regression may not accurately capture how regular, repeated access to debt markets affects educational outcomes.

Proposition 39 helps circumvent the above issues and identify the effect of improved bond market access, because it provides an unanticipated regulatory event that increased districts' borrowing abilities. In other words, Proposition 39 had a measurable effect because it suddenly allowed districts to pass bond measures that would have been marginally rejected in prior years, and as a result, to gain access to increased levels of future capital funding. To test its effects, we estimate the following regression at the district-year level:

$$Y_{it+k} = \tau_t + \alpha_i + \beta_1 pass_{it} + \beta_2 post2001 + \beta_3 pass_{it} * post\ 2001 + \beta_4 X_{it} + \beta_5 Y_{it} + \varepsilon_{it} \quad (6)$$

For district i in year t, the outcome variable Y is one of the following: local capital spending, API (test scores), or home prices. Since it may take years for bonds to be issued after passing, and there may also be a lag in the timing with which infrastructure projects affect educational outcomes, we examine outcomes over seven years. $Pass$ is a dummy variable equal to 1 if the bond measure passes, and 0 otherwise. $Post\ 2001$ is equal to one if the year is 2001 or later, when Proposition 39 came into effect. τ_t and α_i are year and school district fixed effects, respectively. Including school district fixed effects means that the regression compares *school district outcomes in a given year to the same district's outcomes in past years*—a "within-district" effect. Effectively, we examine whether passing marginal bonds adds value to schools by improving outcomes over time. If additional capital funding leads to better outcomes, then we expect $\beta_3 > 0$.

We begin by examining how Proposition 39 affects local capital spending.[9] In Table 5 Panel A, columns (1) through (7) show the change in local capital spending from one to seven years after bond passage. Since bond passage directly leads to more local funding, we expect a positive

---

[8] Figure A2.1 in the Internet Appendix demonstrates how school districts smooth bond issuance over time using two examples of large districts: Stockton City Unified (Panel A) and San Francisco Unified (Panel B).

[9] Our data for local capital spending begins in 1998, reducing the number of observations compared to the full sample period of 1995-2019. Including school district fixed effects requires at least two bond issuances per district over the sample period, which excludes a small number of districts that pass only one bond (or no bonds).

relation. Indeed, the coefficients on *Pass bond* indicate that local capital spending increases two to three years after a bond measure is passed. This unconditional effect is relatively small, perhaps because many school districts smooth spending by issuing approved bonds over time. Next, since Proposition 39 allowed marginal bond proposals to pass, if these marginal proposals were larger on average, then we would expect the relation between bond passage and local capital spending to be stronger after 2001. The coefficient on the interaction term indicates that after Proposition 39, districts borrowed more overall and increased spending over the next one to six years. The magnitude of this effect is almost twice the size of the baseline.

In summary, Proposition 39 loosened school district capital constraints; not only are districts able to obtain voter approval for referenda, but also, they begin improving infrastructure quickly and in higher amounts, potentially because they anticipate borrowing more easily in the future.

Next, Table 5 Panel B directly tests the effect of increased bond market access on educational outcomes using standardized test scores (API). Unconditionally, the coefficient on *Pass bond* indicates that – for a given school district – test scores do not change significantly over the next seven years when a bond is passed. However, the coefficient on the interaction variable indicates significant increases in test scores 1, 2, and 5 years after the bond passes (and nearly significant increases in years 4 and 6). This result implies that not only do education bonds benefit schools by improving infrastructure and increasing pupils' test scores, but also that a voting threshold of 66 2/3% may have kept good projects from passing. Marginally passed capital projects after 2001 that would have been rejected prior to 2001 lead to good educational outcomes.

Finally, Table 5 Panel C examines the effect of school districts' improved bond access on home prices—a tangible measure of value that accounts for perceived quality of schools in the district.[10] Similar to API results, passing bonds has no unconditional effect on home prices. However, after 2001 – within a given school district – home prices increase a few years of bond passage; prices rise significantly between the next five to seven years. This result suggests that it takes some time for housing markets to internalize school infrastructure spending. Overall, marginal education

[10] Cellini, Ferreira, and Rothstein (2010) focus on bonds that passed by narrow margins and find that California school districts underinvest in facilities since passing a referendum causes an increase in home prices.

infrastructure projects that would have been rejected prior to 2001 appear to positively affect school districts and improve future home values. These changes in home prices do not indicate a general rise in prices over this period since the regressions include year fixed effects.

We investigate whether improvements in test scores and home prices vary depending on the wealth of the school district, and we find that less wealthy districts benefit *more* than the median school district from improved bond market access[11]. Ultimately, at all different wealth levels, school districts significantly benefit from passing a bond. Taken together, results from this section provide strong support that school districts benefit from accessing debt markets, and do not support the idea that easier access to credit incentivizes districts to invest in unnecessary projects. Less wealthy districts have similar improvements in test scores and home prices as more-wealthy districts, but they spend significantly less capital overall, indicating a better return on investment.

Section III.c. Bond issuance costs

This section examines bond issuance costs as a potential explanation for why less wealthy districts propose and pass fewer bond measures.

Section III.c.1. Issuance Yields

Issuance yield is a key determinant of a bond's borrowing costs. Controlling for all observable factors, including credit quality, we examine differences in yield between more and less wealthy districts. Table 6 columns (1)-(2) estimate the following regression at the bond CUSIP (individual bond) level:

$$Yield_{it} = \tau_t + \beta_1 property\ tax_{it} + X_{it} + \mu_{it} + \varepsilon_{it} \qquad (7)$$

For bond i issued in year t, *property tax* (log of tax per pupil) measures district wealth. *X* is a vector of district characteristics. μ is a vector of observable bond characteristics, including rating category, maturity, coupon, amount issued, and bond issue process (negotiated or competitive bid). τ are issuance year fixed effects. If less wealthy districts pay higher bond yields, then β_1<0.

[11] We show the regression results in Internet Appendix Table A3.4 and we discuss these results in detail in Section A1.2.

Table 6 column 1 shows that a one standard deviation decrease in wealth (0.75) is associated with a 7 basis point increase in yield. However, this relation no longer holds when controlling for school characteristics (column 2). This result suggests that the relation between wealth and bond costs may be nonlinear. In addition, district size (log of pupil enrollment) is strongly correlated with both wealth and issuance yields; removing the size variable renders the coefficient on property taxes per pupil negative and significant.

To investigate the potential nonlinear relation between district wealth and issuance yields, columns 3-4 of Table 6 replace the continuous measure of property tax from Equation (7) with dummies for five quintiles of property taxes. Property tax quintile dummy 1 is equal to one if a district is in the bottom 20% of the wealth distribution based on average property taxes, and property tax quintile dummy 5 is equal to one if a district is in the top 20%. The missing dummy is for the middle quintile. If less wealthy districts pay higher yields to issue bonds, then the coefficient on quintile 1 should be higher than the coefficient on quintile 5.

Considering this non-linearity, the highest wealth districts pay the lowest yields. Column 3, which controls for bond characteristics, shows that schools in the top quintile of the wealth distribution pay roughly 27.5 basis points lower issuance yields than the median district. Column 4 adds district-level controls and shows that schools in the top two quintiles of wealth have significantly lower yields (15.4 basis points lower for second wealthiest and 22.7 basis points lower for the most wealthy). Since the average bond issue is about $27 million, the wealthiest schools save about $61,290 per issue compared to the median district. Further, the differences between the least and most wealthy districts are statistically and economically significant.

One potential explanation for the discrepancy in yields is that residents of wealthier districts may be more likely to invest in their own bonds, which increases lending demand and lowers borrowing costs. Another possibility is that since less wealthy districts tend to issue bonds less often and in lower amounts their bonds are less liquid which is reflected in primary market prices.

These findings corroborate prior work and bolster the argument that bond investors may discriminate against municipal issuers for reasons other than creditworthiness.[12]

Section III.c.2. Issuance Fees

Districts pay a variety of third-party fees to issue their bonds. The largest and most common include underwriter, financial adviser, and legal counsel fees. Hence, Table 7 tests whether these fees are higher for less wealthy districts at the bond CUSIP level:

$$Fee_{it} = \tau_t + \beta_1 property\ tax\ quintile_{it} + X_{it} + \mu_{it} + \varepsilon_{it} \quad (8)$$

For bond i issued in year t, *Fee* measures issuance fees as a percent of bond principal associated with one of the following: underwriter, financial adviser, legal counsel, or total costs. All other variables are as described in Table 6. If less wealthy districts pay higher issuance fees, then β_1 for quintile 1 will be higher than the β_1 for quintile 5.

Table 7 Panel A regresses issuance fees on property tax quintile dummies and year fixed effects and finds that districts in the bottom wealth quintile pay more (compared to the median district) for every fee except advising. Conversely, districts in the top wealth quintile pay less in every type of fee except the "other" category. The differences between low and high wealth districts are statistically significant for all types of fees.

Table 7 Panel B controls for bond and district characteristics. Compared to the median district, less wealthy districts pay 7 basis points more in underwriting fees, while more wealthy districts pay 18 basis points lower financial adviser fees and 9 basis points lower underwriting fees. Since the average bond issuance is $27 million, more wealthy districts save about $48,600 per issue for advisers and $24,300 per issue for underwriters. Finally, Table 7 Panel B reports the spread in fees between low and high wealth districts: least wealthy districts pay roughly 18 basis points more for advisers and 16 basis points more for underwriters than the most wealthy districts.

The results of this section imply that relative to high wealth districts, less wealthy districts must offer higher bond yields and pay higher fees for bonds with the same credit quality. These

[12] See, for example, Dougal, Gao, Mayhew, and Parsons (2019) who show that historically black college and universities pay higher bond fees and have less liquidity than similar credit quality bonds from other universities.

higher costs mean higher taxes for voters. Hence, if voters in less wealthy districts are price sensitive, they may reject bonds that would have likely been beneficial for their school districts. The next section test voters' sensitivity to perceived tax increases.

Section III.c.3. Do Voters Internalize Bond Costs?

The finding that less wealthy districts pay higher fees and yields does not necessarily explain why these measures are less likely to pass since voters likely do not know future bond fees when voting. To find fee information, voters must either (1) prior to voting, review detailed reports which *sometimes* include estimated fees, or (2) infer costs based on the project tax increases *if* described in the bond referendum. For most of the sample period, districts are not required to report estimated tax costs in bond referenda. However, sometimes districts do voluntarily specify "no projected tax increase" in the referendum, presumably to attract yes votes. Using these data, Table 8 tests whether including this statement in the bond referendum influences voter behavior:

$$\%\, Yes\ Votes_{it} = \tau_t + \beta_1 no\ tax\ increase_{it} + X_{it} + \varepsilon_{it} \qquad (9)$$

For each bond ballot i in year t, $\%\, Yes\ Votes$ measures the ballot's approval rate using the percent of all votes which were in favor of the ballot passing. The main explanatory variable is $no\ tax\ increase$, which is a dummy equal to one if the ballot explicitly states that the bond issuance will not incur foreseeable tax increases. Other explanatory variables include time fixed effects τ and school district characteristics X. If voters are price sensitive, then bond ballots promising no tax increases should be more popular, or $\beta_1 > 0$.

Table 8 finds that ballots promising no tax increases are more popular with voters, corroborating the idea that voters internalize costs and are price sensitive. Column 1 reports the baseline specification and column 2 includes district characteristics. Ballots that promise no tax increases receive 2.5 to 3.0% additional yes votes, a significant improvement. Next, columns 3 and 4 test whether school districts in the bottom wealth quintile (column 3) or top wealth quintile (column 4) are more price sensitive, by interacting the no tax increase dummy with a dummy for wealth quintile. Overall, we find no evidence that the least or most wealthy schools pay more attention to projected tax increases than the median school district.

Section III.d. Why do less wealthy districts pay more?

Results thus far show that less wealthy districts face a double hurdle when passing bonds. First, they have difficulty convincing voters to pass bond measures, despite significant improvements to real outcomes that result from bond issuance. Second, they pay higher yields and direct issuance fees, which reduce proceeds and may cause voter hesitation. This section examines why less wealthy districts pay more. We demonstrate three types of constraints: regulatory limits on property taxes, marketability concerns, funding urgency. This section finds strong evidence that all three constraints appear to bind for less wealthy school districts.

Section III.d.1. Regulatory limits on property taxes

California regulations limit the annual amount of property taxes a district may use for debt service. Table 9 examines two ways to structure bonds that reduce or defer annual service costs. First, districts can issue capital appreciation bonds (CABs). CABs are zero coupon bonds that require no periodic interest payments. Therefore, until the principal comes due, the property tax devoted to debt service is effectively zero. Figure 5 provides an example of a series of CABs issued by the Poway Unified School District in 2010. Poway district voters approved the issuance of $179 million in GOBs in 2007. Poway will make no periodic interest payments to debtholders, instead paying back the initial principal plus accrued interest in a series of payments beginning in 2033 and ending in 2051 for an aggregate amount of about $1.075 billion on $105 million borrowed.[13]

Second, districts can issue longer maturity bonds, which delays repayment of principal. We estimate the following regressions using bond (CUSIP) level observations:

$$CAB_{it} = \tau_t + \beta_1 property\ tax\ quintile_{it} + X_{it} + \mu_{it} + \varepsilon_{it} \quad (10)$$

$$Maturity_{it} = \tau_t + \beta_1 property\ tax\ quintile_{it} + X_{it} + \mu_{it} + \varepsilon_{it} \quad (11)$$

For bond i issued in year t, *CAB* is a dummy variable equal to one if the bond a capital appreciation bond. Property tax quintile is a vector of five dummy variables for district wealth.

[13]CABs were popular among school districts; from 1995-2010 about 51% of all bond issuances included at least one CAB. In 2013, California passed Assembly Bill 182 (AB 182), which tightly restricted CAB offerings. Following this, fewer than 9% of bonds issuances included CABs.

Table 9 columns 1-3 finds that the lowest wealth districts are more likely to issue CABs. The most stringent specification with full controls in column (3) indicates that districts are roughly twice as likely to issue a CAB compared to the median district. Furthermore, the difference in coefficients between districts in the bottom versus top wealth quintile is highly significant across specifications. CABs allow school districts to issue bonds without paying periodic coupons or increasing property taxes in the short run; thus districts with cash or property tax increase constraints find them attractive. However, because CABs have long maturities and high price risk, the total interest expense for CABs is generally much higher than for similar yielding coupon bonds. Adding insult to injury, CABs also have higher direct fees (not tabulated).

Table 9 columns 4-6 finds that districts in the lowest two property tax quintiles also issue longer maturity bonds. Longer maturity bonds are attractive to price sensitive constituents because these bonds allow the district to delay repayment of principal. However, long maturity bonds have more price sensitivity, pay interest for a longer time, and almost always carry higher yields. The full specification in column 6 shows that districts in the lowest two quintiles of property taxes issue bonds with longer maturities. For example, compared to the median school district, the least wealthy districts issue bonds that repay their principal 1.3 years later on average. Conversely, compared to the median district, the wealthiest districts repay their debt 0.9 years faster on average. The difference in coefficients across wealth levels is significant in all three specifications.

Section III.d.2. Credit and Marketability Constraints

Two related constraints that might hurt a district's ability to issue debt are lower inherent credit quality and lower marketability. Hence, potential investors in less wealthy school district bonds may prefer bonds that carry an insurance company guarantee, since these bonds are likely less liquid and potentially more difficult to sell on the secondary market. A guarantee raises the credit rating of the bond and ensures that the holder of the bond can safely hold it to maturity, thus increasing its attractiveness. Second, since less wealthy districts issue smaller amounts less frequently, they may not be able to sell their entire bond issue using a competitive process. Instead, these districts may prefer the less risky but more expensive negotiated bid process, where they select an underwriter in advance and the underwriter agrees to purchase the entire bond issue.

The following two Logit regressions test the relation between district wealth and marketability using bond (series) issuance level observations:

$$Guaranteed_{it} = \tau_t + \beta_1 property\ tax\ quintile_{it} + X_{it} + \mu_{it} + \varepsilon_{it} \qquad (12)$$

$$Negotiated_{it} = \tau_t + \beta_1 property\ tax\ quintile_{it} + X_{it} + \mu_{it} + \varepsilon_{it} \qquad (13)$$

For bond series i issued in year t, *guaranteed* is a dummy variable set to one if some of the bonds in the series are guaranteed. *Negotiated* is a dummy variable set to one if the bond was issued through a negotiated bid as opposed to a competitive bid. Property tax quintile is a vector of five property tax quintile dummy variables.

Supporting this hypothesis, less wealthy districts issue guaranteed bonds and use negotiated bids significantly more often than wealthier districts. Table 10 estimates Equation (12) in Columns 1-3 and Equation (13) in columns 4-6. Columns 1 and 4 conduct baseline regressions controlling for issuance year effects, while columns 2 and 5 add bond characteristics. Columns 3 and 6 includes time fixed effects, bond characteristics, and school district characteristics. Examining the full specification in column 3, the most wealthy districts have a 71% lower likelihood of issuing guaranteed bonds compared to the median. Hence, wealthy districts save on guarantor costs and lower their total costs of borrowing. For the full specification in column 6, less wealthy districts have an 85% higher likelihood of using a negotiated bid while wealthier districts have a 52% lower likelihood of using a negotiated bid. This finding implies that more wealthy districts further reduce their issuance costs by using a competitive bid structure, responding to strong investor demand, which is far less common for less wealthy districts.

Section III.d.3. Urgency Constraints

Finally, we examine the relation between a school district's wealth and its funding urgency. If less wealthy districts require more urgent funding to repair their schools, they have less flexibility to wait for favorable interest rates; they also have less time to negotiate pricing with financial agents. We use two measures of urgency. The first is time to issue: the log of the number of days between bond approval and the first bond series issuance. The second is the proportion of the total authorized debt amount issued in the first bond series. If less wealthy districts have urgency constraints, then compared to the median district, their time to issue should be shorter and the proportion of authorized amount issued in the first bond series should be higher.

Table 11 estimates the following regressions using observations at the bond series level:

$$Time\ to\ issue_{it} = \tau_t + \beta_1 property\ tax\ quintile_{it} + X_{it} + \mu_{it} + \varepsilon_{it} \tag{14}$$

$$Proportion\ of\ face_{it} = \tau_t + \beta_1 property\ tax\ quintile_{it} + X_{it} + \mu_{it} + \varepsilon_{it} \tag{15}$$

For bond series i issued in year t, *Time to issue* is the log number of days between the approval date and the first bond series issuance. *Proportion of face* is the ratio of the bond series face value and the total amount of borrowing authorized. Property tax quintile is a vector of five property tax quintile dummies. X is a vector of district characteristics and μ is a large vector of observable bond characteristics. τ are issuance year fixed effects.

Compared to the median district, less wealthy districts issue bonds more quickly, suggesting they cannot time interest rates nor price-shop for financial agents. Less wealthy districts also issue a higher proportion of the voter-authorized amount right away. The full specification with all control variables in column 3 indicates that the wealthiest districts wait significantly longer (roughly 0.14 standard deviation) than the median district to issue voter-approved bonds; the issuance amount is 8.7% lower relative to the authorized amount in the first series. On the other hand, as column 6 shows, relative to the median district, least wealthy districts spend 9.2% more of the authorized amount in the first series.

Section III.e. State Funding as a Complementary Funding Method

A potential source of funding that might address unequal access to local funding markets is state-issued GOBs. However, in California, state funding is sparse, especially in the latter half of the sample period. Between 1995 to 2005, state voters authorized \$30 billion in funding. However, from 2006 to present, voters approved just \$14 billion. As of early 2022, California's state funds are depleted, with a \$15.5 billion bond measure on the ballot in 2022. (Internet Appendix A1.1 has more detail). However, we test whether California state funding helps fill the funding gap.

California distributes state aid on a first-come first-served basis, with additional funding allocated to districts with financial hardship. Prior research argues that this allocation approach favors wealthier districts (Vincent 2012, Brunner and Vincent 2018). We expand this finding by focusing on the two largest programs offered by California: new construction and modernization, which together comprise about 78% of state funding (Brunner and Vincent 2018). Over the sample period, state-provided new construction funding totaled about $26 billion (in 2016 dollars) while modernization funding totaled about $18 billion.

We test whether state funding helps address the funding gap using a set of data reported by the state of California for 1999 to 2018[14]. Table 12 estimates the following regressions:

$$Log\ of\ State\ Funding = \tau_t + \beta_1 property\ tax\ quintile_{it} + X_{it} + \mu_{it} + \varepsilon_{it} \quad (16)$$

$$Log\ of\ New\ Construction\ Funding_{it} = \tau_t + \beta_1 property\ tax\ quintile_{it} + X_{it} + \mu_{it} + \varepsilon_{it} \quad (17)$$

$$Log\ of\ Modernization\ Funding_{it} = \tau_t + \beta_1 property\ tax\ quintile_{it} + X_{it} + \mu_{it} + \varepsilon_{it} \quad (18)$$

We find mixed evidence. Columns (1) and (2) estimate Equation (16). Less wealthy districts receive slightly more state funding than either median or more wealthy districts. Columns (3) and (4) examine new construction funding, finding strong evidence that less wealthy districts receive more funding than either the median or more wealthy districts. However, this result reverses for modernization funding. Notably, columns (5) and (6) show that less wealthy districts receive significantly less funding than median districts and more wealthy districts; in contrast, wealthy districts receive significantly more modernization funding than the median district. Hence, state funding does a mediocre job of funding the gap between the least and most wealthy school districts. Consistent with prior literature, more wealthy schools appear to take advantage of the first-come first-served funding allocation process for modernization funding.

[14] Figure A2.2 in the Internet Appendix plots average spending by tax quintile for New Construction (Panel A) and Modernization (Panel B).

Section IV. Conclusion

This paper finds that unequal access to local municipal bond funding contributes to the growing gap in education financing and outcomes in California. The main alternative, state education funding, does not fill this gap. While all schools benefit from infrastructure funding, less wealthy school districts have trouble accessing this market. A one standard deviation (0.75) decrease in district wealth predicts a 47% decrease in the odds of obtaining voter approval to issue bonds. Less wealthy districts have difficulty convincing voters to access the public bond market.

Next, we show that voters are less likely to approve bonds in less wealthy districts, possibly because voters are price-sensitive and less wealthy district bonds are more expensive. Compared to the median district, wealthy districts pay about 23 fewer basis points for issuance yields, 18 fewer basis points for financial advisers, and 9 fewer basis points for underwriters. We identify three constraints impacting less wealthy districts: property tax constraints, marketability, and funding urgency. These three constraints come at the cost of significantly higher yields and issuance fees for less wealthy districts.

In summary, our findings suggest that school districts' reliance on public bond markets to fund infrastructure may exacerbate inequality rather than bridge the wealth gap. This finding has important policy implications. For example, one policy that might mitigate this inequality is to bolster state funding for new construction (and possibly eliminating the first-come, first-served approach for modernization funding), since new construction benefits both disadvantaged and wealthy school districts. In addition, since smaller districts have difficulty passing bonds and pay more to issue bonds, smaller and less wealthy school districts may benefit from banding together and issuing bonds. This suggestion would be particularly helpful for rural districts, which are geographically dispersed and have lower population density. Finally, another way to raise funds would be to tax some education bonds, which are currently tax-exempt and thus subsidized by all taxpayers and apply the proceeds towards state funding to help the most disadvantaged districts in the state.

References

Ang, A., Green, R. C., Longstaff, F. A., and Xing, Y. (2017). “Advance refundings of municipal bonds.” *The Journal of Finance*, 72(4):1645-1682.

Agarwal, Sumit, Paige Marta Skiba, and Jeremy Tobacman. "Payday loans and credit cards: New liquidity and credit scoring puzzles." American Economic Review 99.2 (2009): 412-17.

Baldassare, Mark. *A California state of mind: The conflicted voter in a changing world.* Univ of California Press, 2002.

Begley, Taylor A., and Amiyatosh Purnanandam. "Color and credit: Race, regulation, and the quality of financial services." Journal of Financial Economics 141.1 (2021): 48-65.

Bertrand, Marianne, and Adair Morse. "Information disclosure, cognitive biases, and payday borrowing." The Journal of Finance 66.6 (2011): 1865-1893.

Brunner, Eric J. *Financing school facilities in California.* Governor's Committee on Education Excellence, 2006.

Brunner, Eric J., and Jeffrey M. Vincent. "Financing school facilities in California: A ten-year perspective." *Getting Down to Facts II* (2018).

Butler, A. W., Fauver, L., and Mortal, S. (2009). Corruption, political connections, and municipal finance. *The Review of Financial Studies*, 22(7):2873-2905.

Chen, Huaizhi, Lauren Cohen, and Weiling Liu. *Calling all issuers: The market for debt monitoring.* No. w29790. National Bureau of Economic Research, 2022.

Cellini, Stephanie Riegg, Fernando Ferreira, and Jesse Rothstein. "The value of school facility investments: Evidence from a dynamic regression discontinuity design." T*he Quarterly Journal of Economics* 125.1 (2010): 215-261.

Dougal, Casey, et al. "What’s in a (school) name? Racial discrimination in higher education bond markets." *Journal of Financial Economics* 134.3 (2019): 570-590.

Fairlie, Robert W., Alicia Robb, and David T. Robinson. *Black and white: Access to capital among minority-owned startups*. No. w28154. National Bureau of Economic Research, 2020.

Gao, P., Murphy, D., and Qi, Y. “Political uncertainty and public financing costs: Evidence from US gubernatorial elections and municipal bond markets.” Available at *SSRN 1992200* (2019).

Garrett, Daniel G. “Conflicts of Interest in Municipal Bond Advising and Underwriting.” *Working paper* (2020).

Cornaggia, Kimberly, John Hund, and Giang Nguyen. "The price of safety: The evolution of municipal bond insurance value." Available at SSRN 3266890 (2021).

Cornaggia, Kimberly, John Hund, Giang Nguyen, and Zihan Ye. "Opioid crisis effects on municipal finance." *The Review of Financial Studies* 35, no. 4 (2022): 2019-2066.

Hong, Kai, and Ron Zimmer. "Does investing in school capital infrastructure improve pupil achievement?." *Economics of Education Review* 53 (2016): 143-158.

Jackson, C. Kirabo, Rucker C. Johnson, and Claudia Persico. "The effects of school spending on educational and economic outcomes: Evidence from school finance reforms." *The Quarterly Journal of Economics* 131.1 (2016): 157-218.

Joffee, Marc. "Doubly bound: The costs of issuing municipal bonds." *Haas Institute Research Brief.* (2016).

Melzer, Brian T. "The real costs of credit access: Evidence from the payday lending market." *The Quarterly Journal of Economics* 126.1 (2011): 517-555.

National Council on School Facilities, 2016, *The State of Our Schools: America’s K-12 Facilities*.

United States Government Accountability Office, 2020, *K-12 Education: School Districts Frequently Identified Multiple Building Systems Needing Updates or Replacement*.

US Department of Education. (2021, June 15). Federal role in Education. Home. Retrieved February 11, 2022, from https://www2.ed.gov/about/overview/fed/role.html

Van der Heijden, Eline, et al. "Framing effects and impatience: Evidence from a large-scale experiment." *Journal of Economic Behavior & Organization* 84.2 (2012): 701-711.

Figure 1. Cumulative Real Education Spending over Time

The figures below depict cumulative and inflation-adjusted education spending across the state of California from 1998 to 2018. Panel A reports local (orange line) and state-level (blue line) spending. Panel B further splits school districts into quintiles by wealth and shows local (dashed lines) and state (dotted lines) spending for the lowest wealth (green lines) and highest wealth (blue lines) districts. Local property taxes collected per pupil is the measure of district wealth. Source: State of California.

Panel A. Local versus State Spending

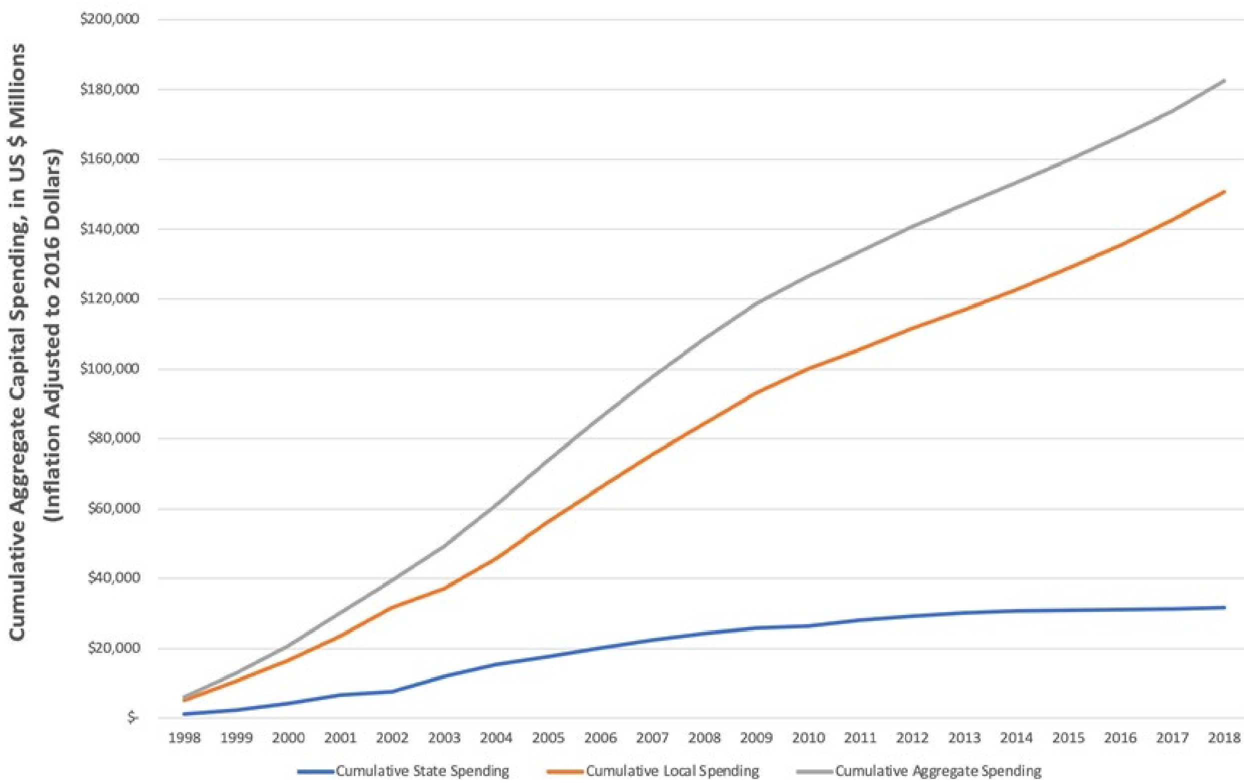


Figure 1 Continued

Panel B. Tax Quintiles and Local Versus State Spending

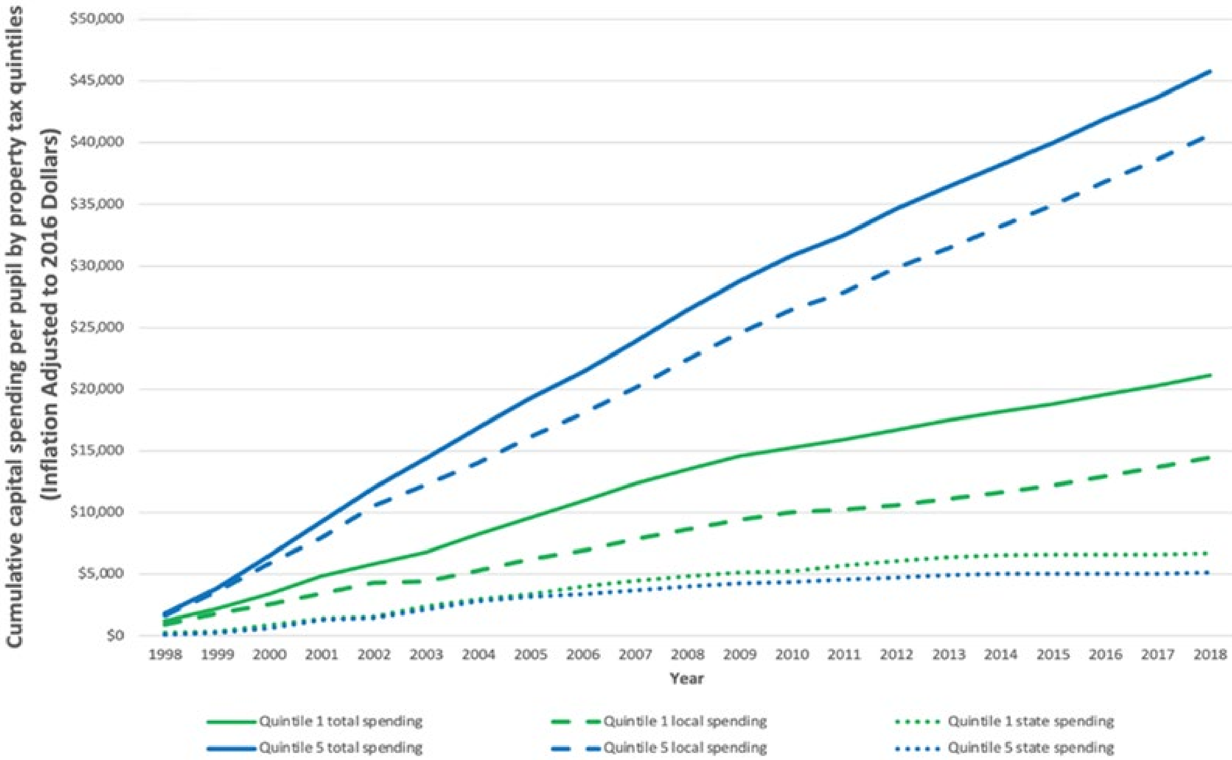


Figure 2: Plot of States that Require Vote to Pass Education Bonds

This figure shows states that require a public vote for a school district to issue general obligation bonds (shown in dark blue) and states that do not (shown in light blue). The underlying data is provided by individual state election websites.

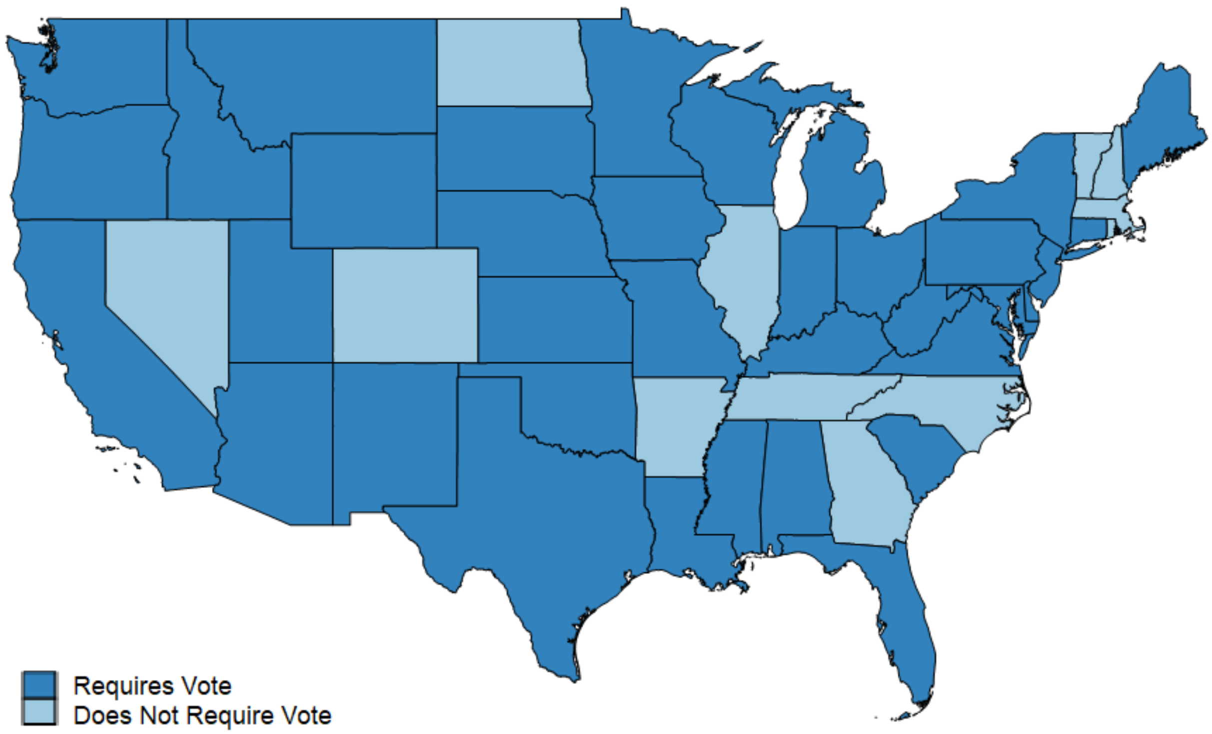


Figure 3: Sample Bond Referendum

The figure below shows an example of a ballot question related to school funding, taken from the March 3, 2020 election of Ukiah Unified School District in California. This ballot proposed $75,000,000 in total bond issuance. This ballot measure, which requires a 55% approval rate, passed by a slim margin, with 55.15% of votes. Source: Ballotopedia.

Ballot question

The ballot question was as follows:[1]

> “To improve the quality of Ukiah Unified schools; repair or replace leaky roofs; make health and safety improvements; and modernize/construct classrooms, restrooms and school facilities; shall Ukiah Unified School District issue $75,000,000 of bonds at legal rates, generating on average $4,000,000 annually as long as bonds are outstanding at a rate of approximately 5 cents per $100 assessed value, with annual audits, independent citizens' oversight committee, NO money for salaries and no money taken by the State? [2]”

Figure 4. Number of Bonds Passed versus Failed by Year

The figure below shows the total number of successful (blue bars) and failed (orange bars) bond measures between 1995 and 2019 for the state of California. While most bond measures required a 66.67% approval rate or higher to pass prior to 2001, Proposition 39 allowed school districts to propose and pass a bond measure with only 55% approval rate or higher (as long as they followed specified provisions) starting in 2001. Source: State of California.

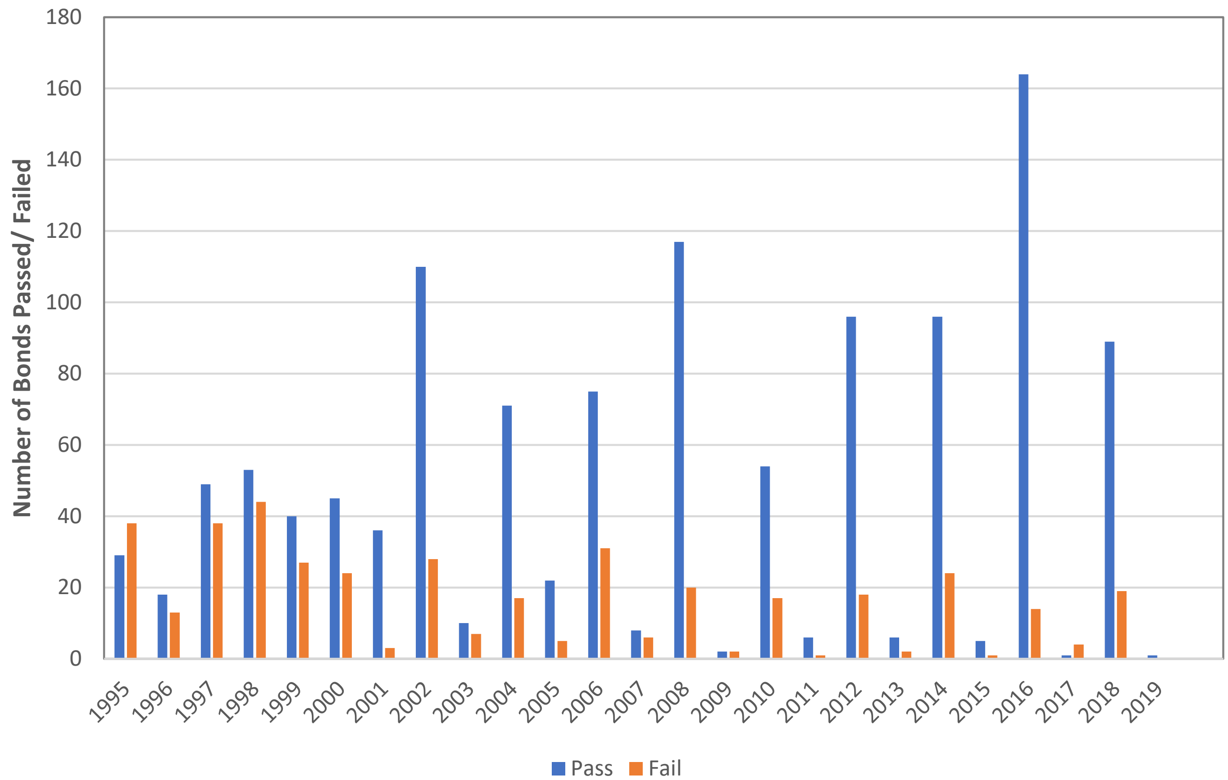


Figure 5: Example of Capital Appreciation Bond (CAB)

The following CAB series was issued by Poway Unified School District in 2010 and approved in the general bond election in November 2007. The total principal amount is $105,000,150, a portion of the $179,000,000 approved in the 2007 election. The total amount to be repaid beginning in 2033 and ending in 2051 is $1,075,195,000.

$105,000,149.70
GENERAL OBLIGATION BONDS OF
SCHOOL FACILITIES IMPROVEMENT DISTRICT NO. 2007-1 OF THE
POWAY UNIFIED SCHOOL DISTRICT, 2008 ELECTION, SERIES B
(SAN DIEGO COUNTY, CALIFORNIA)

MATURITY SCHEDULE
Base CUSIP® No. 738850†
$68,104,545.50 Capital Appreciation Series B Serial Bonds

Maturity (August 1)	Original Principal Amount	Maturity Value	Yield to Maturity	Accretion Rate	CUSP® No.†
2033	$6,570,615.00	$30,500,000	6.560%	7.110%	QY5
2034	9,192,225.60	46,680,000	6.640	7.200	QZ2
2035	8,803,904.00	48,320,000	6.720	7.230	RA6
2036	8,305,119.90	49,770,000	6.750	7.300	RB4
2037	7,923,383.30	51,010,000	6.760	7.300	RC2
2038	7,522,497.40	52,030,000	6.770	7.300	RD0
2039	7,107,169.80	52,810,000	6.780	7.300	RE8
2040	6,607,225.80	53,340,000	6.790	7.340	RF5
2041	6,072,404.70	53,610,000	6.800	7.400	RG3

$22,909,566.40 Original Principal Amount of Term Capital Appreciation Series B Bonds due August 1, 2046
Maturity Value $315,385,000 – Yield to Maturity 6.97% – Accretion Rate 7.64% – CUSP® No.† 738850RH1

$13,986,037.80 Original Principal Amount of Term Capital Appreciation Series B Bonds due August 1, 2051
Maturity Value $321,740,000 – Yield to Maturity 7.12% – Accretion Rate 8.00% – CUSP® No.† 738850RJ7

Source: http://cdiacdocs.sto.ca.gov/2010-1369.pdf

Table 1. Summary statistics

This table reports summary statistics for key variables over the period 1995-2020. Panels A, B, and C summarize the election, bond series, and district datasets respectively. In each panel, the sample is divided into four categories. The maximum number of observations (district-year) is 23,761 and the total number of districts is 730. The table drops elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes cast.

Panel A. Summary of Election Data

	Mean	25th percentile	Median	75th percentile	Standard deviation
Election data (Number: 1,256)					
Authorized amount in US $ millions	111	15	40	98	356
Total votes	22,324	3,605	8,333	20,162	60,007
Percent of yes votes relative to total votes	0.68	0.63	0.68	0.73	0.07
Indicator: initiative requires two-thirds vote to pass	0.23	NA	NA	NA	NA
Indicator: initiative requires 55% vote to pass	0.77	NA	NA	NA	NA
Number of days between election date and bond issuance date	762	156	512	1087	855

Panel B. Summary of bond series issuance data

	Mean	25th percentile	Median	75th percentile	Standard deviation
Bond issue data (Number: 2,591)					
Principal amount in US $ millions	27	6	14	30	38
Interest cost in percent	4.50	3.71	4.47	5.14	1.25
Indicator: includes at least one capital appreciation bond	0.35	NA	NA	NA	NA
Indicator: includes at least one insured bond	0.65	NA	NA	NA	NA
Indicator: negotiated bid	0.73	NA	NA	NA	NA
Indicator: competitive bid	0.27	NA	NA	NA	NA
Fees as a percent of principal: (in %)					
Underwriting fee	0.93	0.50	0.85	1.20	0.59
Financial advisor fee	0.59	0.00	0.26	0.73	0.91
Counsel fee	0.54	0.17	0.33	0.67	0.59
Credit enhancement fee	0.33	0.00	0.21	0.37	0.66
Total fees	2.61	1.19	1.91	3.23	2.21
Fees in dollars (in US $ thousands)					
Underwriting fee	181	54	110	225	231
Financial advisor fee	53	18	60	75	43
Counsel fee	60	42	55	74	35
Total fees	382	186	269	434	489

Table 1. Summary statistics, cont.
Panel C. Summary of School District Data

	(1) Mean	(2) 25th percentile	(3) Median	(4) 75th percentile	(5) Standard deviation
	District characteristics				
Enrollment	7,212	1,200	3,301	9,120	9,732
Enrollment growth (1 yr) %	0.70	-1.68	0.28	2.4	0.49
Unified district dummy %	42.4	NA	NA	NA	NA
High school district dummy %	11.2	NA	NA	NA	NA
Elementary district dummy %	46.4	NA	NA	NA	NA
City dummy %	45.7	NA	NA	NA	NA
Suburb dummy %	20.7	NA	NA	NA	NA
Town dummy %	14.4	NA	NA	NA	NA
Rural dummy %	19.1	NA	NA	NA	NA
Prop. free and reduced price meals	33.3	9.3	31.3	53.7	25.9
Proportion non-white pupils	55.8	31.4	54.7	81.1	27.5
	Home prices and test scores				
Home price (US dollar)	490,587	237,110	378,186	616,518	374,179
API (range 200-1000)	744.2	686.9	747.9	805.9	92.6
Do not meet standards dummy %	30.7	21.4	31.0	40.3	12.9
Meet standards dummy %	25.0	21.4	25.3	28.8	5.2
Above standards dummy %	18.4	9.4	14.5	23.4	12.9
Meet or above standards dummy %	43.4	31.0	40.5	53.0	16.5
	Property taxes, capital spending & debt per pupil (in US $)				
Property taxes	3,456	1,618	2,557	4,254	2,983
Capital outlay	1,154	257	614	1,406	1,538
Local capital outlay	1,119	222	625	1,433	1,660
State capital outlay	289	0	0	29	869
State funding; new construction	167	0	0	0	961
State funding; modernization	113	0	0	0	488
Long term debt	5,825	103	3,631	8,160	7,314

Table 2. Summary Statistics Categorized by District Bond Issuance History

This table reports means for key variables for different school districts over the period 1995-2020. The table divides school districts into four categories based upon bond issuance history in Panel A. As a baseline, Column 1 shows key variables' means over the full sample. The table separately reports means for school districts that: always succeed in passing bonds (Column 2), successfully pass at least one bond (Column 3), never attempt to pass a bond (Column 4), and attempt to but never succeed in passing a bond (Column 5). Panel B aggregates columns 2 and 3 and columns 4 and 5 and performs t-tests for differences in means. The table drops elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes cast.

Panel A: Means for the full sample by district's bond issuance success

	(1) Full sample	**(2) Always succeed**	**(3) Sometimes succeed**	**(4) Never try**	**(5) Try, never succeed**
Max observations	18782	10346	5271	2551	614
Number of districts	730	400	200	106	24
	District characteristics				
Enrollment	7,212	8,006	8,972	1,402	2,826
Enrollment growth (1 yr) %	0.70	0.60	0.70	0.90	0.7
Unified district dummy %	42.4	40.7	55.4	22.5	42.7
High school district dummy %	11.2	12.1	14.3	2.5	6.4
Elementary district dummy %	46.4	47.3	30.4	75.0	51.0
City dummy %	45.7	51.4	49.3	21.4	19.4
Suburb dummy %	20.7	22.9	24.2	8.7	5.5
Town dummy %	14.4	14.7	14.1	13.8	16.9
Rural dummy %	19.1	11.1	12.4	56.2	58.1
Prop. free and reduced price meals	33.3	32.3	32.8	39.0	29.9
Proportion non-white pupils	55.8	58.6	55.4	48.8	39.8
	Home prices, test scores, dropouts				
Home price	490,587	554,917	460,021	317,191	311,722
API (range 200-1000)	744.2	749.2	739.0	732.5	752.3
Do not meet standards dummy %	30.7	29.4	31.3	34.5	30.4
Meet standards dummy %	25.0	25.2	24.9	23.8	26.3
Above standards dummy %	18.4	20.2	17.9	13.2	14.9
Meet or above standards dummy %	43.4	45.4	42.9	37.2	41.2
Dropout rate (HS only) %	1.4	1.3	1.6	1.2	0.9
	Capital spending & debt per pupil (in US $)				
Property taxes	3,456	3,757	3,401	2,447	3,216
Capital outlay	1,154	1,213	1,209	853	931
Local capital outlay	1,119	1,210	1,139	786	814
State capital outlay	289	283	322	242	307
State funding; new construction	167	147	201	153	276
State funding; modernization	113	119	115	92	83
Long term debt	5,825	6,925	6,158	1,375	2,832

Table 2. Summary Statistics Categorized by District Bond Issuance History, cont.
Panel B: Districts that issue at least one bond versus districts that never issue a bond

	(1) Full sample	(2) Issues least one bond	(3) Never issue a bond	(4) Difference: (2) – (3)
Max observations	18,782	15,617	3,165	NA
Number of districts	730	600	130	NA
	District characteristics			
Enrollment	7,212	8,332	1,679	6,653***
Enrollment growth (1 yr) %	0.70	0.62	0.86	0.24
Unified district dummy %	42.4	45.6	26.4	19.2***
High school district dummy %	11.2	12.8	3.2	9.6***
Elementary district dummy %	46.4	41.6	70.4	-28.8***
City dummy %	45.7	50.7	21.0	29.7***
Suburb dummy %	20.7	23.3	8.1	15.2***
Town dummy %	14.4	14.5	14.4	0.1
Rural dummy %	19.1	11.5	56.6	-45.1***
Free and reduced price meals %	33.3	32.5	38.2	-5.8***
Proportion non-white pupils %	55.8	57.6	44.8	12.8***
	Home prices, test scores, dropouts			
Home price	490,587	522,906	316,091	206,815***
API (range 200-1000)	744.2	745.7	736.4	9.3***
Do not meet standards dummy %	30.7	30.0	33.7	-3.7***
Meet standards dummy %	25.0	25.1	24.2	0.9***
Above standards dummy %	18.4	19.4	13.6	5.8***
Meet or above standards dummy %	43.4	44.6	37.9	6.7***
Dropout rate (HS only) %	1.4	1.4	1.1	0.3***
	Capital spending & debt per pupil (US $)			
Property taxes	3,456	3,635	2,592	1,043***
Capital outlay	1,154	1,211	869	342***
Local capital outlay	1,119	1,185	792	393***
State capital outlay	289	296	255	41**
State funding; new construction	167	165	176	-11
State funding; modernization	113	117	90	27**
Long term debt	5,825	6,666	1,658	5,008***

Table 3. Likelihood of Bond Ballot Election and Passing

This table reports results from a Logit model in which the dependent variable is either set to 1 if the school district attempts to authorize a bond via ballot (Panel A), or conditional on attempting to authorize a bond, successfully passes a bond measure (Panel B). The outcome variable is set to 0 otherwise. All school district-year observations, including districts that never issue bonds, are included the regression model. The regressions include year fixed effects and several control variables. Standard errors are clustered by school district, and t-statistics are reported in parentheses. The table drops elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes cast.

Panel A. Dependent Variable=1 if Bond Election is Held

	(1)	(2)	(3)	(4)
Post 2001 dummy				0.214
				(1.10)
Log of enrollment	0.306***	0.224***	0.286***	0.286***
	(14.74)	*(7.43)*	*(8.01)*	*(8.01)*
Log of property tax per pupil			0.219***	0.219***
			(3.77)	*(3.77)*
Local capital expense per pupil			-0.126***	-0.126***
			(-5.33)	*(-5.34)*
State capital expense per pupil			-0.018	-0.018
			(-1.23)	*(-1.23)*
Long-term debt per pupil			-0.051***	-0.051***
			(-4.80)	*(-4.81)*
Free meal ratio			-0.056	-0.056
			(-0.28)	*(-0.28)*
Percent non-white pupils			0.207	0.207
			(1.12)	*(1.12)*
Unified district dummy		0.292**	0.351**	0.351***
		(4.66)	*(4.87)*	*(4.86)*
High school district dummy		0.310**	0.389**	0.389***
		(3.71)	*(3.87)*	*(3.85)*
Rural dummy		-0.218**	-0.332***	-0.333***
		(-1.94)	*(-2.84)*	*(-2.84)*
City dummy		0.046	0.020	0.020
		(0.47)	*(0.17)*	*(0.17)*
Suburb dummy		0.074	0.032	0.032
		(0.78)	*(0.30)*	*(0.30)*
Number of observations	18779	18779	13953	13953
Includes time dummies?	Yes	Yes	Yes	Yes

Table 3. Likelihood of Bond Ballot Election and Passing, cont.
Panel B. Dependent Variable=1 if Bond Measure is Passed

	(1)	(2)	(3)	(4)
Post 2001 dummy				1.425***
				(3.33)
Log of enrollment	0.183***	0.108	0.220**	0.220***
	(2.94)	*(1.40)*	*(2.18)*	*(2.18)*
Log of property tax per pupil			0.517***	0.517***
			(2.89)	*(2.88)*
Local capital expense per pupil			-0.038	-0.038
			(0.54)	*(0.53)*
State capital expense per pupil			-0.001	-0.001
			(-0.02)	*(-0.02)*
Long-term debt per pupil			0.007	0.007
			(0.28)	*(0.28)*
Free meal ratio			-0.136	-0.136
			(-0.26)	*(-0.26)*
Percent non-white pupils			1.581***	1.581***
			(3.46)	*(3.46)*
Unified district dummy		-0.574***	-0.789***	-0.789***
		(-3.09)	*(-3.85)*	*(-3.83)*
High school district dummy		-0.431*	-0.700**	-0.700**
		(-1.65)	*(-2.38)*	*(-2.38)*
Rural dummy		-0.540*	-0.482	-0.482
		(-1.79)	*(-1.49)*	*(-1.49)*
City dummy		0.681**	0.367	0.367
		(2.50)	*(1.24)*	*(1.24)*
Suburb dummy		0.426	0.354	0.354
		(1.44)	*(1.12)*	*(1.12)*
Number of observations	1594	1594	1326	1326
Includes time dummies?	Yes	Yes	Yes	Yes

Table 4. Education Outcomes after Bond Passage using Close Votes

This table reports estimates from a regression discontinuity design using the 55% threshold for a bond ballot passing. Each observation is one bond ballot, and the variable Winning vote margin captures the difference between the percent of yes votes received versus the cutoff required to pass; Pass bond dummy is a variable equal to one if the bond was passed, and it captures the effect of a bond ballot being approved; finally, the interaction term allows for different slopes on either side of the cutoff point. We show results from three different samples, each narrowing in on the effect of bond passage: samples 1, 2, and 3 contain ballots within a 30%, 20%, and 10% margin around the vote cutoff point, respectively. In all analyses, our control variables include school district characteristics as well as the current state of the outcome variable of interest. The dependent variables include: local capital spending in Panel A, test scores in Panel B, and home prices in Panel C. In columns 1-7, the dependent variable is measured for each of the seven years after the bond is authorized. We drop elections with authorized issuance of less than $10 million dollars, fewer than 1,000 total votes cast, and (due to the importance of having a uniform cutoff point) elections which did not use a 55% cutoff point. T-statistics are reported in parentheses.

Panel A. Local Capital Spending

	(1) **Log Local cap. pp, yr t+1**	(2) **Log local cap. pp, yr t+2**	(3) **Log local cap. pp, yr t+3**	(4) **Log local cap. pp, yr t+4**	(5) **Log local cap. pp, yr t+5**	(6) **Log local cap. pp, yr t+6**	(7) **Log local cap. pp, yr t+7**
	Sample 1. 30% Bandwidth around Winning Threshold						
Pass Bond Dummy	0.571***	1.164***	1.285***	0.600***	0.250	0.216	-0.044
	(4.05)	*(7.67)*	*(7.99)*	*(3.35)*	*(1.16)*	*(0.93)*	*(-0.16)*
Winning Vote Margin	0.713	1.502	2.735	5.207***	5.275**	0.871	-3.751
	(0.46)	*(0.90)*	*(1.61)*	*(2.71)*	*(2.31)*	*(0.36)*	*(-1.30)*
Pass Bond x Winning Margin	-0.258	-1.209	-2.460	-3.306	-2.175	1.776	6.007*
	(-0.16)	*(-0.68)*	*(-1.33)*	*(-1.59)*	*(-0.88)*	*(0.66)*	*(1.90)*
Number of Observations	873	839	682	679	558	557	450
	Sample 2. 20% Bandwidth around Winning Threshold						
Pass Bond Dummy	0.635***	1.193***	1.340***	0.638***	-0.002	0.053	-0.135
	(4.11)	*(7.10)*	*(7.70)*	*(3.24)*	*(-0.01)*	*(0.21)*	*(-0.43)*
Winning Vote Margin	-0.813	0.980	1.236	3.292	8.450***	3.275	-3.233
	(-0.42)	*(0.45)*	*(0.57)*	*(1.32)*	*(2.70)*	*(0.96)*	*(-0.70)*
Pass Bond x Winning Margin	1.165	-0.910	-0.963	-0.946	-3.683	0.398	6.422
	(0.56)	*(-0.40)*	*(-0.41)*	*(-0.35)*	*(-1.10)*	*(0.11)*	*(1.32)*
Number of Observations	804	772	638	636	519	519	419
	Sample 3. 10% Bandwidth around Winning Threshold						
Pass Bond Dummy	0.667***	1.132***	0.947***	0.454	-0.388	-0.144	-0.527
	(3.30)	*(5.41)*	*(3.90)*	*(1.63)*	*(-1.28)*	*(-0.43)*	*(-1.28)*
Winning Vote Margin	-1.789	2.072	13.261***	11.055**	22.396***	11.185*	4.904
	(-0.52)	*(0.57)*	*(3.09)*	*(2.26)*	*(4.09)*	*(1.85)*	*(0.66)*
Pass Bond x Winning Margin	2.092	-3.051	-14.168***	-11.232**	-18.844***	-7.870	3.287
	(0.53)	*(-0.74)*	*(-2.89)*	*(-2.01)*	*(-3.02)*	*(-1.134)*	*(0.39)*
Number of Observations	443	425	365	365	291	288	230
	Common Control Variables						
Current Cap. Exp.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School District Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 4. Education Outcomes after Bond Passage using Close Votes, cont.
Panel B. Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Mean API, yr t+1	**Mean API, yr t+2**	**Mean API, yr t+3**	**Mean API, yr t+4**	**Mean API, yr t+5**	**Mean API, yr t+6**	**Mean API, yr t+7**
	Sample 1. 30% Bandwidth around Winning Threshold						
Pass Bond Dummy	3.122	4.701*	8.554**	7.454*	3.988	5.976	3.424
	(1.43)	*(1.74)*	*(2.49)*	*(1.94)*	*(0.91)*	*(1.31)*	*(0.67)*
Winning Vote Margin	-1.212	-4.197	-68.170*	-29.797	89.211	74.837	73.064
	(-0.05)	*(-0.15)*	*(-1.96)*	*(-0.75)*	*(1.59)*	*(1.26)*	*(1.18)*
Pass Bond x Winning Margin	-17.853	-11.998	62.203	22.242	-102.050*	-81.233	-80.084
	(-0.70)	*(-0.38)*	*(1.62)*	*(0.51)*	*(-1.72)*	*(-1.30)*	*(-1.22)*
Number of Observations	604	594	474	459	411	391	282
	Sample 2. 20% Bandwidth around Winning Threshold						
Pass Bond Dummy	3.042	4.756	6.450*	6.867	3.319	5.134	2.615
	(1.26)	*(1.62)*	*(1.68)*	*(1.60)*	*(0.70)*	*(1.04)*	*(0.46)*
Winning Vote Margin	18.025	-5.027	-12.222	-13.683	97.304	105.403	81.537
	(0.57)	*(-0.13)*	*(-0.25)*	*(-0.24)*	*(1.42)*	*(1.46)*	*(1.04)*
Pass Bond x Winning Margin	-45.012	-7.877	6.143	7.754	-103.303	-114.072	-79.314
	(-1.34)	*(-0.19)*	*(0.12)*	*(0.13)*	*(-1.43)*	*(-1.51)*	*(-0.96)*
Number of Observations	567	558	445	430	391	371	265
	Sample 3. 10% Bandwidth around Winning Threshold						
Pass Bond Dummy	3.066	3.103	-0.098	-0.320	-1.763	-0.633	0.241
	(0.95)	*(0.87)*	*(-0.02)*	*(-0.06)*	*(-0.31)*	*(-0.11)*	*(0.03)*
Winning Vote Margin	16.366	-30.464	121.596	99.523	136.582	181.324*	35.269
	(0.29)	*(-0.48)*	*(1.45)*	*(1.09)*	*(1.41)*	*(1.79)*	*(0.27)*
Pass Bond x Winning Margin	-41.731	64.237	-71.675	-40.093	-55.879	-139.290	18.704
	(-0.64)	*(0.88)*	*(-0.74)*	*(-0.38)*	*(-0.49)*	*(-1.18)*	*(0.13)*
Number of Observations	324	318	252	240	221	208	152
	Common Control Variables						
Current API Score	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School District Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 4. Education Outcomes after Bond Passage using Close Votes, cont.
Panel C. Home Prices

	(1) **Log mean house price, yr t+1**	(2) **Log mean house price, yr t+2**	(3) **Log mean house price, yr t+3**	(4) **Log mean house price, yr t+4**	(5) **Log mean house price, yr t+5**	(6) **Log mean house price, yr t+6**	(7) **Log mean house price, yr t+7**
	Sample 1. 30% Bandwidth around Winning Threshold						
Pass Bond Dummy	0.020	0.055*	0.106**	0.121***	0.125**	0.128***	0.117**
	(1.12)	*(1.87)*	*(2.53)*	*(2.68)*	*(2.51)*	*(2.75)*	*(2.36)*
Winning Vote Margin	-0.156	-0.902***	-1.760***	-1.969***	-2.276***	-2.292***	-2.084***
	(-0.79)	*(-2.79)*	*(-3.88)*	*(-4.01)*	*(-4.34)*	*(-4.71)*	*(-3.98)*
Pass Bond x Winning Margin	0.370*	1.323***	2.380***	2.709***	3.282***	3.252***	3.010***
	(1.76)	*(3.83)*	*(4.92)*	*(5.19)*	*(5.79)*	*(6.18)*	*(5.33)*
Number of Observations	982	980	887	883	723	715	604
	Sample 2. 20% Bandwidth around Winning Threshold						
Pass Bond Dummy	0.035*	0.079**	0.130***	0.154***	0.152***	0.148***	0.121**
	(1.80)	*(2.45)*	*(2.81)*	*(3.11)*	*(2.81)*	*(2.95)*	*(2.25)*
Winning Vote Margin	-0.424*	-1.304***	-2.090***	-2.483***	-2.570***	-2.464***	-1.937***
	(-1.77)	*(-3.29)*	*(-3.67)*	*(-4.03)*	*(-3.93)*	*(-4.07)*	*(-2.79)*
Pass Bond x Winning Margin	0.595**	1.637***	2.576***	3.076***	3.370***	3.227***	2.695***
	(2.32)	*(3.86)*	*(4.25)*	*(4.70)*	*(4.79)*	*(4.95)*	*(3.66)*
Number of Observations	913	911	818	814	678	670	565
	Sample 3. 10% Bandwidth around Winning Threshold						
Pass Bond Dummy	0.041	0.076*	0.103	0.115*	0.110	0.106	0.073
	(1.55)	*(1.80)*	*(1.65)*	*(1.71)*	*(1.51)*	*(1.57)*	*(1.05)*
Winning Vote Margin	-0.244	-0.971	-1.641	-1.929*	-2.037	-1.891*	-1.699
	(-0.58)	*(-1.40)*	*(-1.57)*	*(-1.67)*	*(-1.62)*	*(-1.65)*	*(-1.38)*
Pass Bond x Winning Margin	0.142	1.088	2.118*	2.650**	3.126**	2.918**	3.163**
	(0.29)	*(1.36)*	*(1.79)*	*(2.05)*	*(2.20)*	*(2.25)*	*(2.27)*
Number of Observations	508	506	450	448	387	381	318
	Common Control Variables						
Current House Price	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Distric Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 5. Education Outcomes after Bond Passage using 2001 Regulation Change

This table reports results from school district fixed effects regressions around the enactment of Proposition 39 in 2001. Proposition 39 allowed school districts to issue bonds with a 55% voting requirement rather than the prior 66.7%, as long as the bonds meet certain criteria. Each panel examines education outcomes (local capital spending in Panel A, test scores in Panel B, and home prices in Panel C) following bond passage before and after the date of the regulatory change. The dummy variable Post 2001 is set to one for all years after 2001 and 0 for prior years. The dummy variable Pass bond is set to 1 if a bond is authorized during the year and zero otherwise. The table also includes an interaction of Pass bond and Post 2001. The dependent variable is measured for each of the seven years after the bond is authorized. The regressions include school district fixed effects, year fixed effects, and control variables. Standard errors are clustered by school district, and t-statistics are shown in parentheses. The table drops elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes cast.

Panel A. Local Capital Spending

	(1) Log local cap. pp, yr t+1	(2) Log local cap. pp, yr t+2	(3) Log local cap. pp, yr t+3	(4) Log local cap. pp, yr t+4	(5) Log local cap. pp, yr t+5	(6) Log local cap. pp, yr t+6	(7) Log local cap. pp, yr t+7
Pass bond dummy	0.020	0.288***	0.235**	0.031	-0.038	-0.204**	-0.173*
	(0.32)	*(3.68)*	*(2.38)*	*(0.29)*	*(-0.33)*	*(-1.98)*	*(-1.87)*
Pass bond dum. x Post 2001 dummy	0.351***	0.571***	0.651***	0.519***	0.341**	0.253*	-0.168
	(3.34)	*(4.68)*	*(4.16)*	*(3.34)*	*(2.10)*	*(1.66)*	*(-1.10)*
Fifty-five percent dummy	-0.082	0.015	0.112	0.211*	0.103	0.032	0.112
	(-1.04)	*(0.17)*	*(0.99)*	*(1.84)*	*(0.96)*	*(0.27)*	*(0.89)*
Log of property tax per pupil	0.139**	0.015	-0.139	-0.274**	-0.349***	-0.420***	-0.416***
	(2.14)	*(0.16)*	*(-1.29)*	*(-2.36)*	*(-2.84)*	*(-3.67)*	*(-3.71)*
Log of local cap. exp. per pupil	0.416***	0.185***	0.073***	-0.028*	-0.129***	-0.174***	-0.188***
	(24.64)	*(10.91)*	*(4.98)*	*(-1.73)*	*(-7.87)*	*(-10.12)*	*(-10.37)*
Log of state cap. exp. per pupil	0.072***	0.039***	0.012**	-0.006	-0.012**	-0.020***	-0.013**
	(16.67)	*(7.90)*	*(2.34)*	*(-1.18)*	*(-2.13)*	*(-3.11)*	*(-1.96)*
Log of enrollment	-0.030	-0.251**	-0.423***	-0.601***	-0.631***	-0.585***	-0.503***
	(-0.36)	*(-2.21)*	*(-3.35)*	*(-4.13)*	*(-4.16)*	*(-3.81)*	*(-3.28)*
Long-term debt per pupil	-0.002	-0.022***	-0.046***	-0.056***	-0.065***	-0.066***	-0.051***
	(-0.45)	*(-3.00)*	*(-5.94)*	*(-7.06)*	*(-7.14)*	*(-7.33)*	*(-5.24)*
Free meal ratio	0.170	0.027	0.167	0.331	0.071	0.189	0.566
	(0.83)	*(0.11)*	*(0.65)*	*(1.13)*	*(0.25)*	*(0.64)*	*(1.56)*
Percent non-white non-Asian pupils	-0.238*	-0.233*	-0.160	-0.209	-0.509	-0.693	-0.691
	(-1.77)	*(-1.69)*	*(-1.16)*	*(-1.35)*	*(-1.07)*	*(-1.41)*	*(-1.62)*
Rural dummy	0.117*	0.072	0.020	0.071	0.117	-0.117	-0.126
	(1.73)	*(0.78)*	*(0.20)*	*(0.65)*	*(1.01)*	*(-1.05)*	*(-1.16)*
City dummy	0.069	0.104	0.031	0.003	-0.034	-0.144	-0.186**
	(1.22)	*(1.32)*	*(0.34)*	*(0.04)*	*(-0.32)*	*(-1.43)*	*(-1.93)*
Suburb dummy	0.089	0.090	0.081	0.076	0.082	0.018	-0.044
	(1.47)	*(1.07)*	*(0.84)*	*(0.75)*	*(0.77)*	*(0.17)*	*(-0.41)*
Number of observations	12581	11877	11217	10535	9835	9308	8725
Includes school district dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes time dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 5. Education Outcomes after Bond Passage using 2001 Regulation Change, cont.
Panel B. Test Scores

	(1) Mean API, yr t+1	(2) Mean API yr t+2	(3) Mean API, yr t+3	(4) Mean API, yr t+4	(5) Mean API, yr t+5	(6) Mean API, yr t+6	(7) Mean API, yr t+7
Pass bond dummy	-2.934	-3.708	-1.276	-2.327	-4.630	-2.550	-2.517
	(-1.13)	*(-1.42)*	*(-0.47)*	*(-0.86)*	*(-1.89)*	*(-0.97)*	*(-1.05)*
Pass bond dum x Post 2001	5.232*	6.706**	1.967	4.607	6.874**	4.755	4.028
	(1.83)	*(2.30)*	*(0.63)*	*(1.44)*	*(2.39)*	*(1.47)*	*(1.33)*
Fifty-five percent dummy	-1.751	-1.823	0.440	-1.547	-1.995	-1.871	-0.408
	(-1.41)	*(-1.44)*	*(0.29)*	*(-0.91)*	*(-1.37)*	*(-0.98)*	*(-0.22)*
Mean API	0.761***	0.564***	0.398***	0.268***	0.192***	0.142***	0.122***
	(83.40)	*(42.24)*	*(21.62)*	*(12.78)*	*(8.24)*	*(5.76)*	*(5.23)*
Log of property tax per pupil	-0.029	-3.098*	-4.908**	-5.418**	-6.467**	-6.947**	-7.322**
	(-0.02)	*(-1.76)*	*(-2.27)*	*(-2.24)*	*(-2.23)*	*(-2.25)*	*(-2.47)*
Log of local cap exp. per pupil	0.355**	0.824***	0.796***	0.749***	0.563*	0.903***	0.563*
	(2.24)	*(3.18)*	*(2.82)*	*(2.61)*	*(1.75)*	*(3.27)*	*(1.83)*
Log of state cap. exp. p/pupil	-0.037	0.043	0.122	0.150*	0.198**	0.175**	0.165**
	(-0.61)	*(0.53)*	*(1.57)*	*(1.74)*	*(2.22)*	*(2.11)*	*(2.00)*
Log of enrollment	2.233	1.525	3.697	4.349	6.933	9.776	11.698**
	(1.01)	*(0.30)*	*(0.84)*	*(0.82)*	*(1.04)*	*(1.53)*	*(2.39)*
Long-term debt per pupil	0.024	0.100	0.111	0.129	0.176	0.101	0.074
	(0.23)	*(0.71)*	*(0.68)*	*(0.75)*	*(0.95)*	*(0.58)*	*(0.44)*
Free meal ratio	3.556	14.57***	15.539**	16.208**	17.098**	14.107**	14.141
	(1.13)	*(3.15)*	*(2.50)*	*(2.34)*	*(2.47)*	*(1.93)*	*(1.62)*
Percent non-white non-Asian	-15.36***	-15.30**	-10.190	-7.462	0.603	5.374	6.740
	(-3.05)	*(-2.02)*	*(-1.12)*	*(-0.69)*	*(0.04)*	*(0.32)*	*(0.37)*
Rural dummy	0.835	1.289	1.962	1.118	0.301	0.114	-0.956
	(0.71)	*(0.66)*	*(0.91)*	*(0.43)*	*(0.11)*	*(0.04)*	*(-0.31)*
City dummy	-2.081**	-2.143	-2.845	-3.990*	-3.913*	-2.359	-0.630
	(-2.07)	*(-1.29)*	*(-1.45)*	*(-1.77)*	*(-1.69)*	*(-1.09)*	*(-0.29)*
Suburb dummy	-1.662*	-1.459	-1.847	-1.939	-1.957	-0.118	2.100
	(-1.68)	*(-0.87)*	*(-0.91)*	*(-0.83)*	*(-0.81)*	*(-0.05)*	*(0.94)*
Number of observations	8610	7941	7279	6636	5966	5559	4834
Includes sch. dist. dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes time dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 5. Education Outcomes after Bond Passage using 2001 Regulation Change, cont.
Panel C. Home Prices

	(1) Log mean house price, yr t+1	(2) Log mean house price yr t+2	(3) Log mean house price, yr t+3	(4) Log mean house price, yr t+4	(5) Log mean house price, yr t+5	(6) Log mean house price, yr t+6	(7) Log mean house price, yr t+7
Pass bond dummy	0.001	0.001	-0.008	-0.012	-0.020	-0.022	-0.029**
	(0.14)	*(0.17)*	*(-0.85)*	*(-1.13)*	*(-1.57)*	*(-1.59)*	*(-2.08)*
Pass bond dum x Post 2001	0.012	0.005	0.020	0.024	0.029*	0.030*	0.029*
	(1.04)	*(0.36)*	*(1.32)*	*(1.44)*	*(1.74)*	*(1.82)*	*(1.76)*
Fifty-five percent dummy	-0.012	-0.007	-0.011	-0.011	-0.010	-0.006	0.003
	(-1.12)	*(-0.51)*	*(-0.94)*	*(-0.93)*	*(-0.92)*	*(-0.58)*	*(0.34)*
Log of mean house price$_t$	0.848***	0.662***	0.466***	0.266***	0.112***	0.017	-0.024
	(52.13)	*(29.27)*	*(16.00)*	*(7.71)*	*(3.03)*	*(0.47)*	*(-0.70)*
Log of property tax per pupil	-0.018***	-0.023***	-0.013	-0.002	0.011	0.021	0.029**
	(-4.27)	*(-2.99)*	*(-1.22)*	*(-0.13)*	*(0.77)*	*(1.46)*	*(2.07)*
Log of local cap exp. per pupil	0.000	-0.001	-0.001	-0.002	-0.001	0.000	0.000
	(-0.49)	*(-0.67)*	*(-1.19)*	*(-1.52)*	*(-0.85)*	*(-0.29)*	*(0.21)*
Log of state cap. exp. p/pupil	-0.001***	-0.002***	-0.002***	-0.002***	-0.002***	-0.001**	-0.001
	(-2.91)	*(-3.84)*	*(-3.88)*	*(-3.09)*	*(-2.63)*	*(-2.00)*	*(-1.29)*
Log of enrollment	-0.002	0.000	0.005	0.008	0.008	0.011	0.019
	(-0.39)	*(0.01)*	*(0.40)*	*(0.51)*	*(0.49)*	*(0.69)*	*(1.17)*
Long-term debt per pupil	0.000	0.000	-0.001	-0.001	0.000	0.000	0.000
	(0.17)	*(-0.74)*	*(-0.61)*	*(-0.49)*	*(-0.22)*	*(0.05)*	*(0.03)*
Free meal ratio	-0.018	-0.038**	-0.063**	-0.093***	-0.120***	-0.126***	-0.091***
	(-1.49)	*(-1.97)*	*(-2.36)*	*(-2.96)*	*(-3.47)*	*(-3.57)*	*(-2.72)*
Percent non-white non-Asian	-0.015*	-0.036***	-0.059***	-0.079***	-0.083***	-0.092***	-0.202***
	(-1.86)	*(-3.34)*	*(-4.27)*	*(-4.62)*	*(-4.37)*	*(-4.81)*	*(-2.60)*
Rural dummy	0.013***	0.023***	0.025**	0.029***	0.025**	0.025**	0.016
	(3.21)	*(3.21)*	*(2.47)*	*(2.57)*	*(2.13)*	*(2.19)*	*(1.40)*
City dummy	0.013***	0.026***	0.035***	0.042***	0.041***	0.036***	0.025**
	(3.90)	*(4.54)*	*(4.40)*	*(4.38)*	*(3.81)*	*(3.30)*	*(2.29)*
Suburb dummy	0.016***	0.039***	0.056***	0.070***	0.076***	0.076***	0.066***
	(4.76)	*(6.64)*	*(6.97)*	*(7.13)*	*(6.91)*	*(6.77)*	*(5.77)*
Number of observations	12937	12920	12243	11553	10872	10193	9524
Includes sch. dist. dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes time dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 6. Offering Yields

This table presents OLS regressions at the CUSIP level in which the dependent variables the bond's yield at issuance based upon price sold. Standard errors are clustered by school district. The key variables of interest are average property tax and property tax quintile dummy variables. The regressions include year fixed effects and several bond as well as school district control variables. Standard errors are clustered by school district, and t-statistics are shown in parentheses. The table drops issued as a result of elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes.

Dependent Variable is Bond Offering Yield (%)

	(1)	(2)	(3)	(4)
Property taxes per pupil	-0.092**	-0.040		
	(-2.18)	*(-0.35)*		
Property tax quintile 1 dummy			-0.101	-0.135
			(-1.20)	*(-0.99)*
Property tax quintile 2 dummy			0.034	-0.021
			(0.18)	*(-0.17)*
Property tax quintile 4 dummy			-0.129	-0.154*
			(-1.53)	*(-1.70)*
Property tax quintile 5 dummy			-0.275***	-0.227*
			(-2.96)	*(-1.68)*
Coupon rate	-0.281***	-0.280***	-0.281***	-0.280***
	(-15.97)	*(-16.28)*	*(-14.95)*	*(-15.38)*
Years to maturity	0.048***	0.045***	0.049***	0.045***
	(7.41)	*(6.74)*	*(7.40)*	*(6.71)*
Log of principal issued	0.117***	0.142***	0.117***	0.144***
	(3.26)	*(4.20)*	*(3.19)*	*(4.46)*
Dummy for callable	0.946***	0.939***	0.943***	0.934***
	(-19.31)	*(19.32)*	*(18.42)*	*(18.28)*
Dummy for bank-qualified	0.424***	0.294***	0.430***	0.296***
	(3.39)	*(3.22)*	*(3.28)*	*(3.35)*
Dummy for insured	0.039	-1.145	0.016	-1.149
	(0.19)	*(-0.88)*	*(0.07)*	*(-0.89)*
Negotiated bid dummy	0.037	0.021	0.034	0.020
	(0.61)	*(0.14)*	*(0.55)*	*(0.13)*
Guarantor dummy		1.309		1.293
		(1.05)		*(1.07)*
Log of enrollment		-0.076**		-0.085**
		(-2.22)		*(-2.30)*
Long-term debt per pupil		-0.017		-0.016
		(-0.49)		*(-0.48)*
Free meal ratio		-0.082		-0.135
		(-0.46)		*(-0.62)*
Percent non-Wh non-Asian		0.196		0.240
		(0.48)		*(0.58)*
Unified dummy		0.135		0.129
		(1.01)		*(0.97)*
High school dummy		-0.168*		-0.168*
		(-1.94)		*(-1.93)*
Number of observations	13031	13031	13031	13031
Includes rating dummies?	Yes	Yes	Yes	Yes
Includes city/suburb/rural dummies?	Yes	Yes	Yes	Yes
Includes time dummies?	Yes	Yes	Yes	Yes

Table 7. Third party fees and district wealth

This table presents OLS regressions in which the dependent variables are various vendor fees scaled by the size of the bond issue. Panel A includes only election-year dummies as control variables, while Panel B includes several additional controls. Standard errors are clustered by school district. The key variables of interest are property tax quintile dummy variables. The regressions include year fixed effects and several control variables. Standard errors are clustered by school district, and t-statistics are shown in parentheses. The table drops bonds issued as a result of elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes.

Panel A. Baseline with Election-year Dummies Only

	(1) **Total cost**	(2) **Fin. Adv.**	(3) **Underwriter**	(4) **Legal Counsel**
Property tax quintile 1 dummy	0.682***	0.148	0.143***	0.167**
	(2.63)	*(1.35)*	*(2.73)*	*(2.32)*
Property tax quintile 2 dummy	0.087	-0.012	0.067	0.004
	(0.39)	*(-0.13)*	*(1.42)*	*(0.07)*
Property tax quintile 4 dummy	-0.121	-0.083	0.035	-0.032
	(-0.50)	*(-0.86)*	*(0.66)*	*(-0.48)*
Property tax quintile 5 dummy	-0.701***	-0.238***	-0.106**	-0.149***
	(-3.07)	*(-2.78)*	*(-2.10)*	*(-2.51)*
Number of observations	2603	2233	2557	2117
Includes time dummies?	Yes	Yes	Yes	Yes
Quintile 5 – Quintile 1	-1.383***	-0.386***	-0.249***	-0.316***

Table 7. Third party fees and district wealth, continued

Panel B: Full Specification

	(1) Total cost	(2) Fin. Adv.	(3) UW	(4) Counsel
Property tax quintile 1 dummy	-0.081	-0.003	0.069*	-0.014
	(-0.53)	*(-0.03)*	*(1.73)*	*(-0.35)*
Property tax quintile 2 dummy	-0.145	-0.058	0.049	-0.049
	(-1.17)	*(-0.91)*	*(1.43)*	*(-1.57)*
Property tax quintile 4 dummy	-0.129	-0.090	0.013	-0.031
	(-1.06)	*(-1.56)*	*(0.42)*	*(-0.95)*
Property tax quintile 5 dummy	-0.220*	-0.180***	-0.090***	-0.040
	(-1.66)	*(-2.49)*	*(-2.66)*	*(-0.93)*
Log of principal issued	-1.070***	-0.364***	-0.144***	-0.325***
	(-15.71)	*(-11.66)*	*(-8.74)*	*(-16.20)*
Percent of vote	-1.007	-0.483	-0.100	-0.119
	(-1.55)	*(-1.55)*	*(-0.55)*	*(-0.64)*
AAA rating dummy	0.425**	0.045	-0.029	0.003
	(2.31)	*(0.55)*	*(-0.38)*	*(0.07)*
AA rating dummy	0.210	0.073	-0.062	0.082**
	(1.41)	*(1.12)*	*(-0.86)*	*(2.49)*
Non-rated dummy	0.134	0.075	-0.175**	0.073**
	(0.87)	*(1.15)*	*(-2.43)*	*(1.97)*
Guarantor dummy	0.441	-0.011	0.001	-0.011
	(4.00)	*(-0.21)*	*(0.03)*	*(-0.35)*
Years to maturity	0.009	-0.008*	0.006***	-0.003
	(1.09)	*(-1.92)*	*(2.68)*	*(-1.20)*
CAB dummy	0.560***	0.022	0.153***	0.017
	(6.17)	*(0.50)*	*(5.17)*	*(0.64)*
Total interest cost	0.210***	0.062**	0.064***	0.039***
	(3.95)	*(2.15)*	*(5.02)*	*(2.72)*
Log of votes, number	0.138*	0.100***	-0.027	0.031
	(1.73)	*(2.66)*	*(-1.48)*	*(1.28)*
Negotiated bid dummy	-0.062	-0.138***	-0.182***	-0.013
	(-0.71)	*(-2.98)*	*(-5.19)*	*(-0.49)*
Log of enrollment	-0.254**	-0.198***	-0.053**	-0.061**
	(-2.32)	*(-3.56)*	*(-2.29)*	*(-1.96)*
Long-term debt per pupil	-0.041***	-0.016**	-0.005	-0.010**
	(-2.76)	*(-2.08)*	*(-1.17)*	*(-2.39)*
Free meal ratio	0.670**	0.283**	0.071	0.144*
	(2.32)	*(1.98)*	*(0.97)*	*(1.85)*
Percent non-Wh non-Asian	0.289	0.091	-0.120*	0.066
	(1.17)	*(0.76)*	*(-1.88)*	*(1.04)*
Unified dummy	-0.094	-0.106**	0.026	-0.055**
	(-0.95)	*(-1.99)*	*(0.94)*	*(-1.96)*
High school dummy	-0.233*	-0.174**	0.064*	-0.050
	(-1.74)	*(-2.50)*	*(1.66)*	*(-1.27)*
Rural dummy	0.736***	0.258*	0.125**	0.107
	(2.86)	*(1.90)*	*(2.28)*	*(1.61)*
City dummy	-0.036	-0.125	0.105**	-0.052
	(-0.18)	*(-1.29)*	*(2.11)*	*(-0.93)*
Suburb dummy	-0.206	-0.190**	0.047	-0.088
	(-1.15)	*(-2.17)*	*(1.01)*	*(-1.70)*
Number of observations	2565	2215	2527	2113
Includes time dummies?	Yes	Yes	Yes	Yes
Quintile 5 – Quintile 1	-0.139	-0.177*	-0.159***	-0.026

Table 8. The Effect of No Tax Increase on Bond Voting Outcomes

This table reports results from OLS regressions at the bond election level where the outcome variable is the percent of all votes that were in favor of the measure passing. The key explanatory variable, dummy for no tax increase, is set equal to one if the bond measure explicitly states that there will be no foreseeable future tax increases associated with the passing of the bond measure. All school district-year observations, including districts that never issue bonds, are included the regression model. Regressions include year fixed effects and school district control variables. Standard errors are clustered by school district, and t-statistics are reported in parentheses. The table drops elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes cast.

	% of Yes Votes Received			
	(1)	(2)	(3)	(4)
Dummy for no tax increase	0.030**	0.025**	0.027**	0.033**
	(2.44)	(2.15)	(2.07)	(2.22)
Property tax quintile 1 dummy			-0.003	
			(-0.35)	
Dummy for no tax increase x quintile 1 dummy			-0.009	
			(-0.33)	
Property tax quintile 5 dummy				0.011
				(1.09)
Dummy for no tax increase x quintile 5 dummy				-0.026
				(-1.08)
Log of enrollment	0.007***	-0.001	-0.001	-0.001
	(2.98)	(-0.36)	(-0.37)	(-0.36)
Log of property tax per pupil		0.012**	0.011*	0.008
		(2.16)	(1.70)	(1.15)
Local capital expense per pupil		0.002	0.002	0.002
		(0.87)	(0.88)	(0.79)
State capital expense per pupil		-0.000	-0.000	-0.000
		(-0.08)	(-0.10)	(-0.02)
Long term debt per pupil		0.000	0.000	0.000
		(0.44)	(0.44)	(0.37)
Free meal ratio		0.003	0.003	0.004
		(0.20)	(0.18)	(0.23)
Percent non-white students		0.082***	0.082***	0.081***
		(5.14)	(5.15)	(5.08)
Unified district dummy		-0.022***	-0.023***	-0.022***
		(-3.39)	(-3.39)	(-3.27)
High school district dummy		-0.040***	-0.040***	-0.039***
		(-4.52)	(-4.52)	(-4.41)
Rural dummy		-0.017	-0.017	-0.017
		(-1.54)	(-1.56)	(-1.55)
City dummy		0.032***	0.032***	0.032***
		(3.16)	(3.13)	(3.12)
Suburb dummy		0.020**	0.019**	0.020**
		(2.05)	(2.04)	(2.06)
Number of observations	1,552	1,287	1,287	1,287
Includes time dummies?	Yes	Yes	Yes	Yes

Table 9. Tax constraints and district wealth

Columns (1)-(3) present logit regressions with the dependent variable is set to 1 if the district issues a capital appreciation bond and zero otherwise. Columns (4)-(6) present OLS regressions with the dependent variable is the weighted average number years to maturity for the bond issue. Standard errors are clustered by school district. The key variables of interest are property tax quintile dummy variables. The regressions include year fixed effects and several control variables. Standard errors are clustered by school district, and t-statistics are shown in parentheses. The table drops elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes cast.

	Logit: CAB issued?			**OLS: Time to maturity**		
	(1)	(2)	(3)	(4)	(5)	(6)
Property tax quintile 1 dummy	0.862***	0.516**	0.740***	1.165**	0.714*	1.329***
	(3.65)	*(2.21)*	*(2.90)*	*(2.17)*	*(1.88)*	*(3.11)*
Property tax quintile 2 dummy	-0.084	-0.394*	-0.330	0.898*	0.496	0.827**
	(-0.38)	*(-1.80)*	*(-1.50)*	*(1.67)*	*(1.27)*	*(2.09)*
Property tax quintile 4 dummy	0.031	-0.276	-0.349	0.250	0.062	0.059
	(0.13)	*(-1.10)*	*(-1.32)*	*(0.45)*	*(0.16)*	*(0.15)*
Property tax quintile 5 dummy	-0.349	-0.159	-0.353	-0.640	-0.308	-0.899**
	(-1.45)	*(-0.59)*	*(-1.22)*	*(-1.09)*	*(-0.73)*	*(-2.11)*
Log of principal issued		0.186	0.242*		2.051***	2.577***
		(1.60)	*(1.81)*		*(9.70)*	*(11.37)*
Issuance cost/principal		0.180***	0.192***		0.143*	0.053
		(3.30)	*(3.53)*		*(1.80)*	*(0.65)*
Percent of vote		-2.799**	-2.673**		-4.998***	-4.640**
		(-2.21)	*(-2.16)*		*(-2.79)*	*(-2.41)*
Time between election and issue		0.260***	0.276***		0.484***	0.573***
		(4.02)	*(4.03)*		*(5.10)*	*(6.15)*
Guarantor dummy		-0.046	0.108		0.987***	0.879***
		(-0.22)	*(0.49)*		*(3.08)*	*(2.63)*
Years to maturity		-0.003	0.000			
		(-0.17)	*(-0.02)*			
CAB dummy					-1.127***	-1.023***
					(-3.77)	*(-3.64)*
Total interest cost		1.600***	1.583***		2.851***	2.857***
		(12.49)	*(12.14)*		*(14.03)*	*(14.20)*
Log of votes, number		-0.329***	-0.429**		-1.447***	-0.603**
		(-2.82)	*(-2.44)*		*(-8.42)*	*(-2.33)*
Negotiated bid dummy		2.765***	2.812***		-0.724**	-0.772**
		(12.72)	*(12.52)*		*(-2.34)*	*(-2.50)*
Log of enrollment			-0.023			-1.377***
			(-0.13)			*(-4.95)*
Long-term debt per pupil			0.020			-0.020
			(0.80)			*(-0.51)*
Free meal ratio			0.284			1.778**
			(0.49)			*(2.22)*
Percent non-Wh non-Asian			-1.216**			-0.290
			(-2.45)			*(-0.42)*
Unified dummy			-0.123			-0.908***
			(-0.67)			*(-3.47)*
High school dummy			0.305			-0.586
			(1.04)			*(-1.25)*
Number of observations	2553	2512	2512	2565	2517	2517
Includes rating dummies?	No	Yes	Yes	No	Yes	Yes
Includes city/suburb/rural dummies?	No	No	Yes	No	No	Yes
Includes time dummies?	Yes	Yes	Yes	Yes	Yes	Yes

Table 10. Credit quality and marketability constraints and district wealth

Columns (1)-(3) present logit regressions in which the dependent variable is set to 1 if the bond has an insurance guarantee and zero otherwise. Columns (4)-(6) present logit regressions in which the dependent variable is set to one if the bond is a negotiated issue and zero otherwise. Standard errors are clustered by school district. The key variables of interest are property tax quintile dummy variables. The regressions include year fixed effects and several control variables. Standard errors are clustered by school district, and t-statistics are shown in parentheses. The table drops bonds issued as a result of elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes cast.

	Logit: Guaranteed?			**Logit: Negotiated bid?**		
	(1)	(2)	(3)	(4)	(5)	(6)
Property tax quintile 1 dummy	1.079***	0.936***	0.045	0.643**	0.332	0.613*
	(3.91)	*(2.89)*	*(0.12)*	*(2.13)*	*(1.07)*	*(1.69)*
Property tax quintile 2 dummy	0.666***	0.689***	0.269	-0.014	0.072	0.161
	(2.90)	*(2.62)*	*(0.85)*	*(-0.05)*	*(0.27)*	*(0.58)*
Property tax quintile 4 dummy	-0.107	-0.135	0.475	0.114	-0.022	-0.035
	(-0.42)	*(-0.47)*	*(1.54)*	*(0.45)*	*(-0.08)*	*(-0.13)*
Property tax quintile 5 dummy	-1.333***	-2.034***	-1.238***	-0.527**	-0.530*	-0.735***
	(-5.24)	*(-5.88)*	*(-3.17)*	*(-2.03)*	*(-1.85)*	*(-2.58)*
Log of principal issued		0.039	-0.161		0.029	0.075
		(0.28)	*(-1.06)*		*(0.23)*	*(0.57)*
Issuance cost/principal		0.336***	0.296***		-0.002	-0.045
		(3.24)	*(3.00)*		*(-0.03)*	*(-0.74)*
Percent of vote		1.643	0.780		-0.859	-0.763
		(1.13)	*(0.47)*		*(-0.66)*	*(-0.55)*
Time between election and issue		-0.375***	-0.500***		0.084	0.103**
		(-5.53)	*(-6.34)*		*(1.64)*	*(1.94)*
Guarantor dummy					-0.065	-0.178
					(-0.27)	*(-0.72)*
Years to maturity		0.061***	0.059***		-0.034**	-0.038**
		(3.79)	*(3.24)*		*(-2.18)*	*(-2.37)*
CAB dummy		-0.270	-0.013		2.571***	2.665***
		(-1.38)	*(-0.06)*		*(13.17)*	*(13.18)*
Total interest cost		-0.203**	-0.156		0.271***	0.292***
		(-2.38)	*(-1.57)*		*(2.50)*	*(2.63)*
Log of votes, number		-0.401***	-0.663***		-0.110	0.197
		(-3.55)	*(-3.26)*		*(-0.93)*	*(1.23)*
Negotiated bid dummy		-0.025	-0.356			
		(-0.10)	*(-1.31)*			
Log of enrollment			0.325			-0.437**
			(1.48)			*(-2.28)*
Long-term debt per pupil			0.000			0.015
			(0.00)			*(0.53)*
Free meal ratio			5.043***			-0.115
			(6.39)			*(-0.20)*
Percent non-Wh non-Asian			-0.296			0.395
			(-0.55)			*(0.87)*
Unified dummy			0.820***			0.374
			(2.93)			*(1.61)*
High school dummy			1.515***			-0.124
			(3.98)			*(-0.38)*
Number of observations	2458	2394	2394	2581	2495	2495
Includes rating dummies?	No	Yes	Yes	No	Yes	Yes
Includes city/suburb/rural dummies?	No	No	Yes	No	No	Yes
Includes time dummies?	Yes	Yes	Yes	Yes	Yes	Yes

Table 11. Urgency and marketability constraints and district wealth

Columns (1)-(3) present OLS regressions in which the dependent variable the log of days between the time bond is authorized and the bond is issued. Columns (4)-(6) present OLS regressions in which the dependent variable is the percentage of the authorized bond amount comprising this bond issue. Standard errors are clustered by school district. The key variables of interest are property tax quintile dummy variables. The regressions include year fixed effects and several control variables. Standard errors are clustered by school district, and t-statistics are shown in parentheses. The table drops bonds issued as a result of elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes cast.

	OLS: Time to issue			**OLS: Proportion of authorized amt**		
	(1)	(2)	(3)	(4)	(5)	(6)
Property tax quintile 1 dummy	0.054	0.117*	-0.084	0.011	-0.017	0.092***
	(0.80)	*(1.89)*	*(-1.30)*	*(0.44)*	*(-0.74)*	*(3.37)*
Property tax quintile 2 dummy	0.069	0.139***	0.055	-0.014	-0.021	0.028
	(1.10)	*(2.55)*	*(1.02)*	*(-0.72)*	*(-1.09)*	*(1.43)*
Property tax quintile 4 dummy	0.049	0.041	0.099*	0.028	0.020	-0.003
	(0.70)	*(0.69)*	*(1.65)*	*(1.19)*	*(1.04)*	*(-0.16)*
Property tax quintile 5 dummy	0.088	-0.016	0.163***	0.012	0.008	-0.087***
	(1.26)	*(-0.27)*	*(2.65)*	*(0.53)*	*(0.47)*	*(-3.68)*
Log of principal issued		-0.097***	-0.184***		0.098***	0.153***
		(-2.95)	*(-5.09)*		*(7.63)*	*(8.94)*
Issuance cost/principal		0.040***	0.049***		-0.004	-0.009***
		(3.10)	*(3.76)*		*(-1.42)*	*(-2.83)*
Percent of vote		0.576*	0.485		-0.328***	-0.223**
		(1.81)	*(1.51)*		*(-3.28)*	*(-2.16)*
Time between election and issue					-0.051***	-0.040***
					(-11.30)	*(-8.67)*
Guarantor dummy		-0.352***	-0.415***		0.017	0.041***
		(-6.04)	*(-6.84)*		*(0.87)*	*(2.54)*
Years to maturity		0.023***	0.028***		-0.002	-0.005***
		(5.29)	*(6.42)*		*(-1.23)*	*(-3.13)*
CAB dummy		0.179***	0.182***		0.014	0.013
		(3.05)	*(3.05)*		*(0.97)*	*(0.95)*
Total interest cost		-0.317***	-0.327***		-0.010*	0.000
		(-10.85)	*(-11.39)*		*(-1.81)*	*(-0.10)*
Log of votes, number		0.172***	0.024		-0.110***	-0.033*
		(6.54)	*(0.69)*		*(-12.24)*	*(-1.76)*
Negotiated bid dummy		0.085*	0.094*		0.027*	0.019
		(1.72)	*(1.88)*		*(1.68)*	*(1.22)*
Log of enrollment			0.255***			-0.134***
			(5.86)			*(-6.19)*
Long-term debt per pupil			0.003			0.001
			(0.43)			*(0.49)*
Free meal ratio			0.142			-0.022
			(1.26)			*(-0.56)*
Percent non-Wh non-Asian			0.005			-0.062*
			(0.05)			*(-1.66)*
Unified dummy			0.068			-0.056***
			(1.45)			*(-2.78)*
High school dummy			0.108*			-0.058**
			(1.70)			*(-2.20)*
Number of observations	2517	2517	2517	2495	2495	2495
Includes rating dummies?	No	Yes	Yes	No	Yes	Yes
Includes city/suburb/rural dummies?	No	No	Yes	No	No	Yes
Includes time dummies?	Yes	Yes	Yes	Yes	Yes	Yes

Table 12. State funding and district wealth

Columns (1)-(2) present OLS regressions in which the dependent variable is the log of the combined state modernization and new construction funding allocated to a district. Columns (3)-(4) present OLS regressions in which the dependent variable is the log of state new construction funding allocated to a district, and columns (5)-(6) present OLS regressions in which the dependent variable is the log of state modernization funding allocated to a district. Standard errors are clustered by school district. The key variables of interest are property tax quintile dummy variables. The regressions include year fixed effects and several control variables. Standard errors are clustered by school district, and t-statistics are shown in parentheses. The table drops issued as a result of elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes cast.

	OLS: Log of state funding		OLS: Log new constr. funding		OLS: Log of modernization funding	
	(1)	(2)	(3)	(4)	(5)	(6)
Property tax quintile 1 dummy	0.091	0.151*	0.249***	0.255***	-0.170***	-0.109*
	(0.90)	*(1.86)*	*(2.77)*	*(3.34)*	*(-2.65)*	*(-1.78)*
Property tax quintile 2 dummy	0.180*	0.082	0.195**	0.101	-0.022	-0.041
	(1.90)	*(1.09)*	*(2.37)*	*(1.41)*	*(-0.35)*	*(-0.74)*
Property tax quintile 4 dummy	0.007	0.049	-0.119	-0.087	0.081	0.117**
	(0.07)	*(0.66)*	*(-1.53)*	*(-1.25)*	*(1.19)*	*(1.97)*
Property tax quintile 5 dummy	-0.102	-0.077	-0.275***	-0.262***	0.145**	0.197***
	(-1.04)	*(-0.99)*	*(-3.43)*	*(-3.49)*	*(2.04)*	*(3.24)*
Passed a bond this year dummy		-0.123		-0.029		-0.082
		(-1.51)		*(-0.43)*		*(-1.27)*
Log of lagged local exp. p pupil		0.194***		0.115***		0.106***
		(11.91)		*(8.95)*		*(8.72)*
Log of enrollment		0.446***		0.296***		0.253***
		(15.34)		*(9.82)*		*(11.72)*
Long-term debt per pupil		0.038***		0.018***		0.022***
		(5.20)		*(3.02)*		*(4.00)*
Free meal ratio		0.016		-0.157		0.170
		(0.10)		*(-1.11)*		*(1.51)*
Percent non-Wh non-Asian		-0.065		0.102		-0.143
		(-0.46)		*(0.85)*		*(-1.34)*
Unified dummy		0.101*		0.036		0.106***
		(1.73)		*(0.64)*		*(2.67)*
High school dummy		-0.049		0.054		-0.092
		(-0.58)		*(0.69)*		*(-1.44)*
Rural dummy		0.177**		0.185***		0.033
		(2.50)		*(3.33)*		*(0.58)*
City dummy		-0.086		0.008		-0.105
		(-1.05)		*(0.12)*		*(-1.58)*
Suburb dummy		-0.275***		-0.127**		-0.175***
		(-3.56)		*(-1.96)*		*(-2.99)*
Number of observations	14037	13256	14037	13256	14037	13256
Includes time dummies?	Yes	Yes	Yes	Yes	Yes	Yes

Internet Appendix

Section A1.1. Background on California's State Issued General Obligation Bonds[15]

In addition to local school district GOBs, California voters can also approve GOBs at the state level to provide support for new school construction and modernization projects. State bond approval requires a simple majority vote of voters across the state. Since 1998, the proceeds of these bonds are distributed through California's School Facility Program (SFP). Generally, the state funds new construction with 50% state GOBs and 50% local GOBs, and modernization with 60% state GOBs and 40% local GOBs.[16] State funding totaled about $11.5 billion during the 1990s, with another $21.7 billion approved between 2000 and 2005. In 2006, voters approved about $7 billion, but did not approve additional funding (nor did the state put additional funding measures on the ballot) until 2016 when voters approved another $7 billion. Since 2014, just 5% of funding for local school district infrastructure projects has been through state GOBs. This proportion has varied significantly over time with the availability of state funding; for the period 1995 to 2014, the state proportion averaged closer to 20%. In March 2020, voters rejected a $15 billion bond proposal. A similar proposal for $15.5 billion in funding is scheduled for the March 2022 general election.

Local school districts must apply for funding from the SFP, with funds allocated to local school districts on a first-come first-served basis.[17] The bulk of SFP funding (about 80% since inception) is allocated to two programs: new construction and modernization. The current application process

---

[15] This discussion draws heavily from the summaries in Brunner (2006) and Brunner and Vincent (2018).

[16] Modernization projects were initially funded with 80% state GOB and 20% local GOBs. This formula was changed to the current 60/40 split following the passage of Assembly Bill 16 in 2002.

[17] Prior to implementation of the SFP in 1998, the state used a priority method of funding local school district applications. Districts with year-round schooling programs and significant enrollment growth and a request for 50% of funding needs were considered "Priority 1" status while those with similar programming and growth but requesting 100% of funding needs were considered "Priority 2" status. Due to insufficient state funding, most projects below these priority statuses were not funded. In 1997, the state began allocating funds on a first-come, first-served basis.

for state funding has two parts: an application for eligibility and an application for funding. The Office of Public School Construction (OPSC) reviews eligibility applications and submits them to the State Allocation Board (SAB) for approval. Once the SAB approves the eligibility application, the district requests funding by submitting a funding application to the Office of Public School Construction (OPSC).

For new construction funding, districts must show that expected pupil enrollment over the next five years exceeds existing seating capacity. New construction funding requires three different eligibility application forms, and must be requested at the school district level. For modernization funding, districts must show that their existing facilities (that are at least 25 years old) require improvements such as air conditioning, lighting, plumbing or electrical systems. Modernization funding requires a single eligibility application form and can be requested separately for individual schools within a district, in contrast with new construction funding which is at the district level.

Once approved, districts applying for either program submit a funding application form, and state grants are determined on a per-pupil basis. The state also sponsors a financial hardship program for school districts unable to provide matching funds (50% for new construction and 40% for modernization). Under this program, districts can receive up to 100% of new funding costs. To be considered, a district must demonstrate its inability to raise matching funds via local GOBs and its inability to contribute matching funds from its current budget.

Since 2010, resulting from a low SFP balance, the SAB has apportioned funds on a streamlined basis for projects approved but not yet funded. This process is called Priority in Funding (PIF) and reduces the time permitted for districts to initiate construction while providing funding only twice a year, compared to the prior method of continuously funding projects. In 2012, the state reached its SFP bond funding capacity and began placing eligibility applications on an "Acknowledged List" rather than an "Unfunded Approval" list as in the past. Districts could apply for funding but it was not guaranteed. In 2016, California voters approved the first state bond ballot in a decade, authorizing about $7 billion in new funding. The state has been extremely slow in allocating these funds despite a very long "Acknowledged List," doling out less than $1 billion per year since 2017. As noted earlier, voters rejected a $15 billion bond proposal in 2020; as of 2021, district demand for funds exceeds state supply of funds by about $2.3 billion. The state plans to put a new bond proposal of $15.5 billion on the ballot in March 2022.

Section A1.2. Differential Effect of Loosening Voting Constraints

In Section III.b., we show that bond ballot passage after 2001 improves educational outcomes over time for a given district. In addition, we are particularly interested in how bond market access affects less wealthy districts, and therefore estimate the following amended regression:

$$Y_{it+k} = \tau_t + \alpha_i + \beta_1 pass\ bond_{it} + \beta_2 post2001 + \beta_3 pass\ bond_{it} * post\ 2001 + \beta_4 pass\ bond_{it} * post\ 2001 * low\ tax + \beta_5 pass\ bond_{it} * post\ 2001 * high\ \ tax + \beta_6 X_{it} + \varepsilon_{it} \tag{A1}$$

The regression includes the same terms as Equation (5) but also includes a triple interaction of *post 2001* and *pass bond* with a dummy variable for whether the school is in a high (or low) property tax bracket, defined as districts in the top (bottom) quintile by property tax collected per pupil per year. If $\beta_4 + \beta_3 > 0$, then less wealthy districts improve educational outcomes after passing a marginal bond. If $\beta_5 + \beta_3 > 0$, then more wealthy districts improve their educational outcomes after passing a marginal bond.

Table A3.4 Panel A reports the differential effect of bond approval post 2001 on less and more wealthy districts' future capital spending. Table A3.4 Panel A reports summed coefficients (bottom of table) to measure the aggregate effect of a bond passing after 2001. Less wealthy districts have significantly higher capital spending than the median school district when a bond passes after 2001 (Pass x post + pass x post x low > 0) for the next one to two years. Wealthy districts benefit for a longer period: they have higher capital spending than the median district (Pass x post + pass x post x high tax > Pass x post + pass x post x low tax) for five years after bond passage. Hence, both high and low wealth districts benefit from Proposition 39: they increase capital spending relative to the median district. However, wealthier districts both spend more and amortize spending over a longer period of time.

Next, Table A3.4 Panel B shows that both low and high wealth districts significantly improve pupil test scores following better bond market access. The summed coefficients at the bottom of Panel B indicate that passing bonds significantly improves pupil test scores 1, 2, and 5 years post-bond passage after 2001 compared to the median district. The results for the least wealthy districts are particularly impressive, because Panel A shows that their capital spending is lower and less prolonged than rich districts. Hence, it appears that even modest infrastructure spending by less wealthy districts can have a significant impact on school quality.

Finally, Table A3.4 Panel C examines how bond passage post-2001 affects home prices in districts of different wealth levels. The sums of coefficients at the bottom of the table indicate that less wealthy districts experience a significant rise in home prices 3 to 5 years after passage, while the wealthiest districts experience a significant rise in home prices a bit later: 4 to 7 years after bond passage (both relative to the median school district). These results may occur for two reasons. First, wealthy districts tend to spread spending over a longer period of time and more often approve bond referenda. Second, less wealthy districts may spend bond proceeds on more urgent construction projects that are more visible and thus incorporate more quickly into home prices.[18]

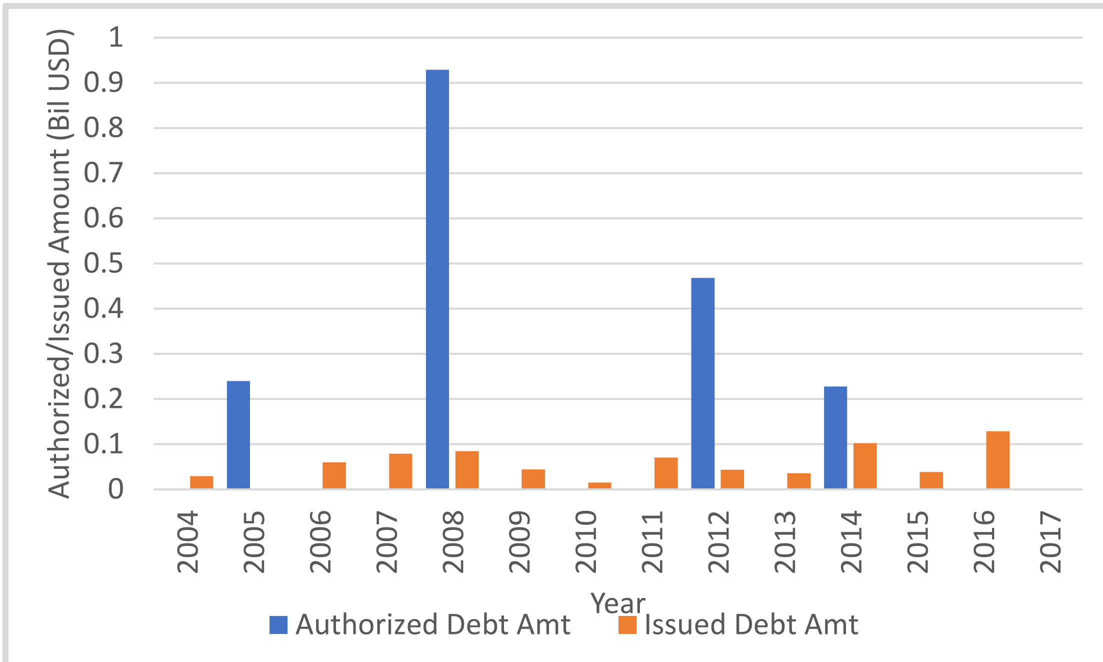
[18] We investigate this idea in more detail and provide evidence in Section III.d.

Overall, findings offer strong support that all school districts benefit from public bond issuance. Furthermore, less wealthy districts have similar improvements in test scores and home prices as more wealthy districts, but they spend significantly less capital over a shorter period of time, indicating a better return on investment.

Section A2. Internet Appendix Figures

Figure A2.1. Examples of Debt Amount Authorized and Issued by Year

Panel A. Stockton Unified School District



Panel B. San Francisco Unified School District

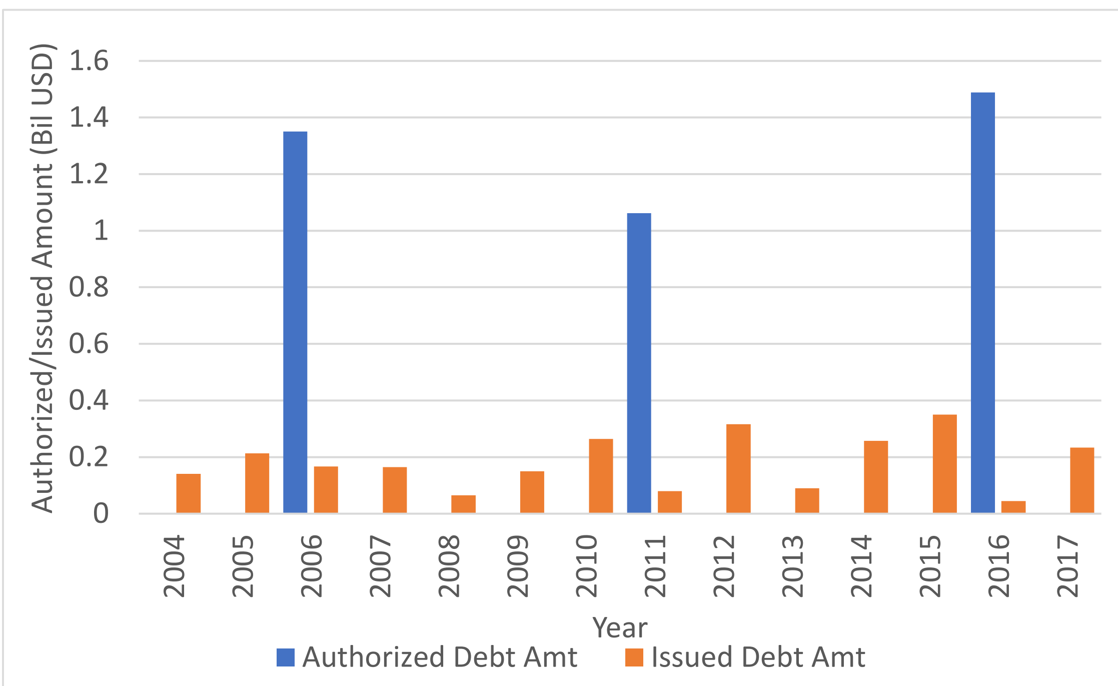
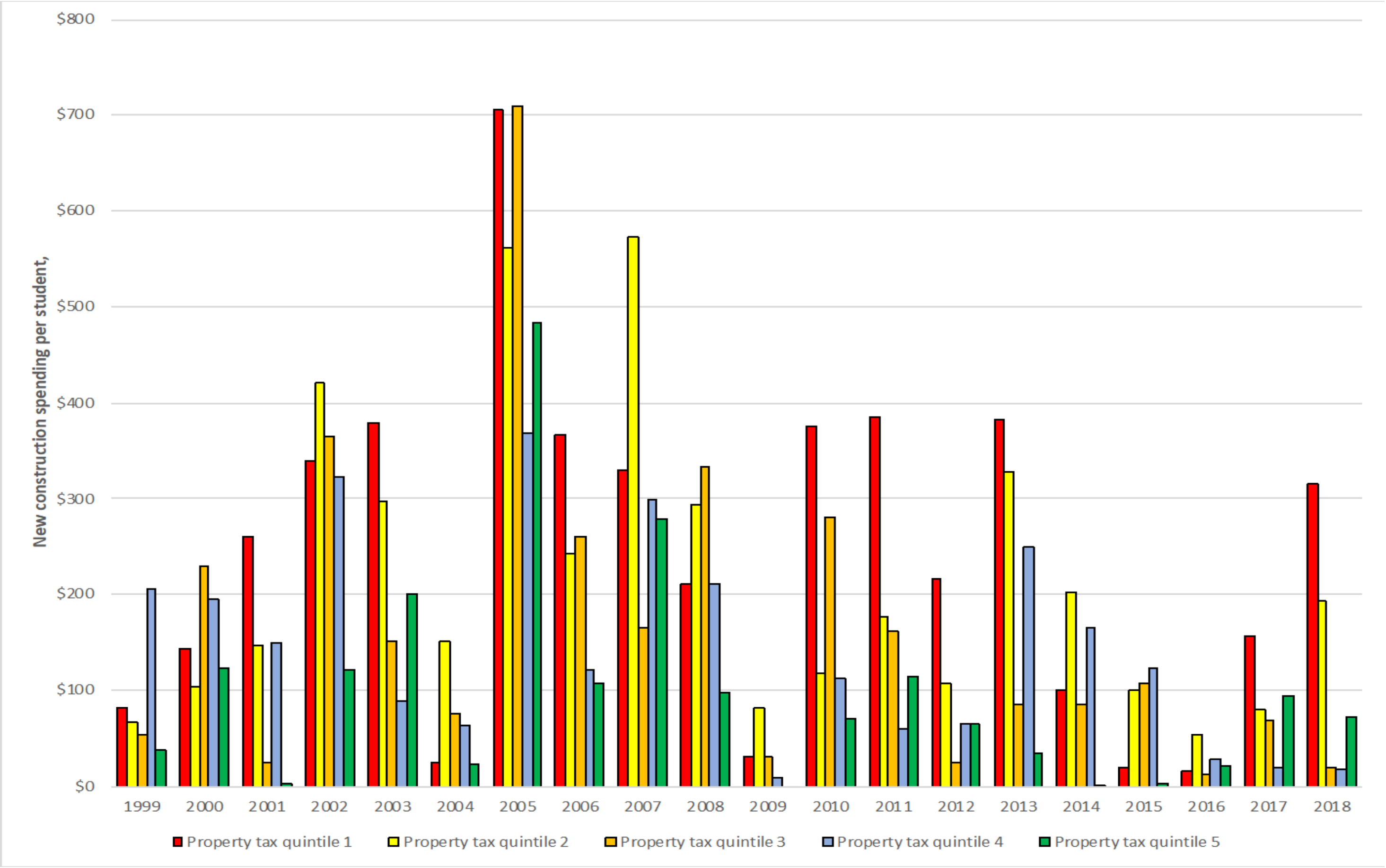


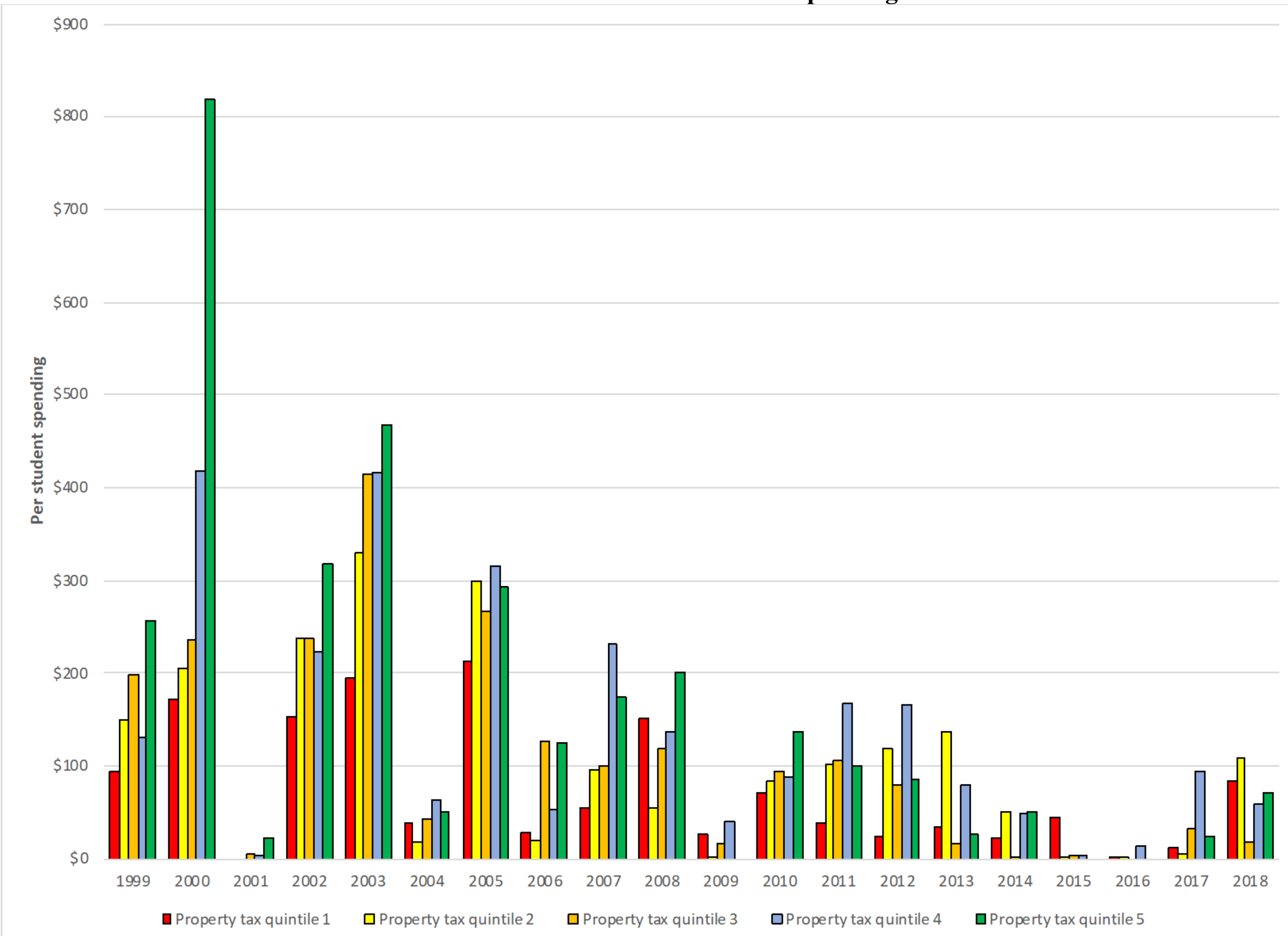
Figure A2.2. State spending per pupil by property tax quintiles

Panel A presents average new construction per pupil. We group school districts into quintiles based on their property tax per pupil per year, requiring at least 5 school districts in each bin each year. Panel B presents the modernization spending using the same methodology.

Panel A: Average new construction spending, per pupil



Panel B: Modernization spending



Section A3. Internet Appendix Tables

Table A3.1. Controlling for Test Scores

This table reports results from a logit model in which the dependent variable is either set to 1 if the school district attempts to authorize a bond via ballot (columns 1-3), or conditional on attempting to authorize a bond, successfully passes a bond measure (columns 4-6). Unlike Table 3 in the main text, we additionally control for the average API score as a proxy for school quality. All school district-year observations, including districts that never issue bonds, are included the regression model. The regressions include year fixed effects and several control variables. Standard errors are clustered by school district, and t-statistics are shown in parentheses. We drop elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes cast.

	Dummy= 1 if Bond Election is Held			**Dummy= 1 if Bond Measure is Passed**		
	(1)	(2)	(3)	(4)	(5)	(6)
Log of enrollment	0.298***	0.171***	0.292***	0.383***	0.310***	0.309**
	(10.96)	*(4.59)*	*(6.78)*	*(4.73)*	*(3.11)*	*(2.42)*
Log of property tax per pupil			0.315***			0.538**
			(4.57)			*(2.55)*
Mean API score	0.000	0.000	-0.002**	0.001	-0.001	0.005**
	(0.27)	*(0.51)*	*(-2.29)*	*(0.75)*	*(-0.77)*	*(2.08)*
Local capital expense per pupil			-0.080***			-0.036
			(-2.74)			*(-0.42)*
State capital expense per pupil			-0.016			0.002
			(-0.98)			*(0.04)*
Long term debt per pupil			-0.060***			-0.006
			(-4.60)			*(-0.19)*
Free meal ratio			-0.456			1.551**
			(-1.59)			*(2.09)*
Percent non-white students			-0.075			1.792***
			(-0.33)			*(3.35)*
Unified district dummy		0.416***	0.346***		-0.719***	-0.708***
		(5.12)	*(3.84)*		*(-3.02)*	*(-2.67)*
High school district dummy		0.371***	0.147		-0.503	-0.196
		(3.52)	*(1.08)*		*(-1.60)*	*(-0.48)*
Rural dummy		-0.179	-0.154		-0.399	-0.326
		(-1.27)	*(-1.04)*		*(-1.07)*	*(-0.83)*
City dummy		0.197	0.216		0.543	0.505
		(1.56)	*(1.55)*		*(1.48)*	*(1.35)*
Suburb dummy		0.250*	0.299**		0.633*	0.621*
		(1.92)	*(2.11)*		*(1.74)*	*(1.65)*
Number of observations	10,412	10,412	9,555	938	938	886
Includes time dummies?	Yes	Yes	Yes	Yes	Yes	Yes

Table A3.2. Using Total Population instead of Total Pupil Count

This table reports results from a logit model in which the dependent variable is either set to 1 if the school district attempts to authorize a bond via ballot (columns 1-3), or conditional on attempting to authorize a bond, successfully passes a bond measure (columns 4-6). Unlike Table 3 in the main text, we measure school district size using log total population (reported at county level due to issues outlined in Section III.a) instead of the log number of enrolled pupils. All school district-year observations, including districts that never issue bonds, are included the regression model. The regressions include year fixed effects and several control variables. Standard errors are clustered by school district, and t-statistics are shown in parentheses. We drop elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes cast.

	Dummy= 1 if Bond Election is Held			**Dummy= 1 if Bond Measure is Passed**		
	(1)	(2)	(3)	(4)	(5)	(6)
Ln of population (county level)	0.105***	0.049**	0.068***	0.187***	0.127*	0.066
	(4.95)	*(2.30)*	*(2.71)*	*(3.09)*	*(1.87)*	*(0.80)*
Ln of property tax per person (county level)			0.199**			1.046***
			(2.05)			*(3.41)*
Local capital expense per pupil			-0.121***			-0.032
			(-4.64)			*(-0.41)*
State capital expense per pupil			0.012			0.001
			(0.81)			*(0.03)*
Long term debt per pupil			-0.080***			0.028
			(-7.32)			*(1.00)*
Free meal ratio			0.025			-0.172
			(0.12)			*(-0.31)*
Percent non-white students			0.059			1.557***
			(0.30)			*(3.26)*
Unified district dummy		0.371***	0.485***		-0.503***	-0.530**
		(6.76)	*(7.04)*		*(-2.76)*	*(-2.50)*
High school district dummy		0.323***	0.517***		-0.404	-0.460
		(3.93)	*(5.10)*		*(-1.50)*	*(-1.41)*
Rural dummy		-0.099	-0.179		-0.750**	-0.578
		(-0.82)	*(-1.40)*		*(-2.18)*	*(-1.63)*
City dummy		0.146	0.258**		0.440	0.386
		(1.36)	*(2.10)*		*(1.43)*	*(1.12)*
Suburb dummy		0.163	0.190		0.309	0.303
		(1.46)	*(1.52)*		*(0.90)*	*(0.82)*
Number of observations	8,544	13,700	10,938	1,430	1,430	1,175
Includes time dummies?	Yes	Yes	Yes	Yes	Yes	Yes

Table A3.3. Testing for Differential Effects of Proposition 39

This table reports results from a logit model in which the dependent variable is either set to 1 if, conditional on attempting to authorize a bond, a school district successfully passes a bond measure. The main variable of interest is the fifty-five percent dummy, which is equal to one after Proposition was passed in 2001 and if the ballot had a 55% voting threshold. Unlike Table 3 in the text, we also interact this variable with our proxies for school district size and wealth, in order to understand if the law had a differential effect based on size or wealth. All school district-year observations, including districts that never issue bonds, are included the regression model. The regressions include year fixed effects and several control variables. Standard errors are clustered by school district. We drop elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes cast.

	Dummy= 1 if Bond Measure is Passed	
	(1)	(2)
Fifty-five percent dummy	1.212	5.115**
	(0.98)	*(2.12)*
Fifty-five percent dummy x Log of enrollment	*0.108*	
	(0.74)	
Fifty-five percent dummy x Log of property tax per pupil		*-0.387*
		(-1.27)
Log of enrollment	-0.181	-0.098
	(-0.80)	*(-0.47)*
Log of property tax per pupil	0.310	0.607*
	(1.42)	*(1.75)*
Local capital expense per pupil	-0.052	-0.046
	(-0.72)	*(-0.65)*
State capital expense per pupil	0.016	0.018
	(0.45)	*(0.49)*
Long term debt per pupil	0.013	0.013
	(0.49)	*(0.47)*
Free meal ratio	-0.157	-0.139
	(-0.28)	*(-0.25)*
Percent non-white students	1.842***	1.873***
	(3.66)	*(3.77)*
Unified district dummy	-0.869***	-0.885***
	(-4.04)	*(-4.13)*
High school district dummy	-0.951***	-0.952***
	(-2.91)	*(-2.93)*
Rural dummy	-0.516	-0.533
	(-1.57)	*(-1.62)*
City dummy	0.387	0.365
	(1.26)	*(1.19)*
Suburb dummy	0.162	0.158
	(0.51)	*(0.49)*
Number of observations	1,323	1,323
Includes time dummies?	Yes	Yes

Table A3.4. Education outcomes after 2001 regulation change by tax bracket

This table reports results from regressions around the enactment of Proposition 39 in 2001, for the most wealthy (high p tax) and least wealthy (low p tax) districts separately. Each panel examines education outcomes (local capital spending in Panel A, test scores in Panel B, and home prices in Panel C) following bond passage before and after the date of the regulatory change. The dummy variable Post 2001 is set to one for all years after 2001 and 0 for prior years. The dummy variable Pass bond is set to 1 if a bond is authorized during the year and zero otherwise. The table also includes an interaction of Pass bond and Post 2001. The dependent variable is measured for each of the seven years after the bond is authorized. The regressions include school district fixed effects, year fixed effects, and control variables. Standard errors are clustered by school district. The table drop elections with authorized issuance of less than $10 million dollars and fewer than 1,000 total votes cast.

Panel A: Local Capital Spending

	(1) Log local cap. pp, yr t+1	(2) Log local cap. pp, yr t+2	(3) Log local cap. pp, yr t+3	(4) Log local cap. pp, yr t+4	(5) Log local cap. pp, yr t+5	(6) Log local cap. pp, yr t+6	(7) Log local cap. pp, yr t+7
Pass bond dummy	0.020	0.288***	0.238**	0.037	-0.042	-0.192*	-0.166*
	(0.32)	*(3.67)*	*(2.40)*	*(0.35)*	*(-0.36)*	*(-1.85)*	*(-1.79)*
Pass x post	0.361***	0.607***	0.709***	0.478***	0.359**	0.325**	-0.134
	(3.26)	*(4.78)*	*(4.42)*	*(2.95)*	*(2.10)*	*(2.09)*	*(-0.83)*
Pass bond x post x low p tax	-0.082	-0.183*	-0.427***	-0.188	-0.395***	-0.307***	-0.070
	(-0.88)	*(-1.89)*	*(-3.76)*	*(-1.60)*	*(-3.32)*	*(-2.54)*	*(-0.53)*
Pass bond x post x high p tax	0.019	-0.015	0.000	0.252**	0.192	-0.141	-0.112
	(0.21)	*(-0.13)*	*(0.00)*	*(1.98)*	*(1.46)*	*(-1.05)*	*(-0.73)*
Fifty-five percent dummy	-0.083	0.013	0.121	0.218*	0.108	0.037	0.110
	(-1.05)	*(0.15)*	*(1.08)*	*(1.89)*	*(1.00)*	*(0.32)*	*(0.87)*
Property tax quintile 1 dummy	-0.115	-0.036	0.120	0.230*	0.231*	0.225**	0.132
	(-1.50)	*(-0.36)*	*(1.07)*	*(1.87)*	*(1.85)*	*(1.94)*	*(1.01)*
Property tax quintile 2 dummy	-0.067	-0.014	-0.006	0.103	0.105	0.046	0.077
	(-1.23)	*(-0.19)*	*(-0.08)*	*(1.28)*	*(1.15)*	*(0.54)*	*(0.76)*
Property tax quintile 4 dummy	0.013	-0.040	-0.077	-0.064	-0.099	-0.061	0.050
	(0.23)	*(-0.58)*	*(-0.83)*	*(-0.61)*	*(-0.94)*	*(-0.64)*	*(0.56)*
Property tax quintile 5 dummy	0.095	0.076	-0.178	-0.254	-0.220	-0.293*	-0.215
	(1.07)	*(0.62)*	*(-1.20)*	*(-1.59)*	*(-1.38)*	*(-1.84)*	*(-1.47)*
Log of lagged local cap. exp. p pupil	0.417***	0.185***	0.072***	-0.029*	-0.131***	-0.176***	-0.192***
	(24.84)	*(10.97)*	*(5.04)*	*(-1.82)*	*(-8.03)*	*(-10.13)*	*(-10.32)*
Log of state cap. exp. per pupil	0.072***	0.038***	0.012**	-0.006	-0.013**	-0.021***	-0.013**
	(16.61)	*(7.81)*	*(2.37)*	*(-1.18)*	*(-2.20)*	*(-3.15)*	*(-1.96)*
Log of enrollment	-0.073	-0.243**	-0.400***	-0.535***	-0.519***	-0.434***	-0.320**
	(-0.93)	*(-2.37)*	*(-3.54)*	*(-4.06)*	*(-3.61)*	*(-2.84)*	*(-2.01)*
Long-term debt per pupil	-0.001	-0.022**	-0.047***	-0.058***	-0.068***	-0.069***	-0.055***
	(-0.26)	*(-2.97)*	*(-6.01)*	*(-7.32)*	*(-7.46)*	*(-7.69)*	*(-5.59)*
Free meal ratio	0.164	0.022	0.171	0.329	0.063	0.184	0.559
	(0.80)	*(0.09)*	*(0.66)*	*(1.12)*	*(0.22)*	*(0.61)*	*(1.52)*
Percent non-Wh non-Asian	-0.232*	-0.239*	-0.181	-0.237	-0.535	-0.749	-0.773*
	(-1.72)	*(-1.74)*	*(-1.32)*	*(-1.55)*	*(-1.12)*	*(-1.51)*	*(-1.78)*
Rural dummy	0.119*	0.074	0.022	0.073	0.118	-0.123	-0.130
	(1.75)	*(0.81)*	*(0.22)*	*(0.66)*	*(1.02)*	*(-1.10)*	*(-1.19)*
City dummy	0.072	0.105	0.031	0.000	-0.038	-0.155	-0.192**
	(1.27)	*(1.33)*	*(0.34)*	*(0.00)*	*(-0.37)*	*(-1.54)*	*(-1.98)*
Suburb dummy	0.093	0.095	0.079	0.069	0.081	0.013	-0.041
	(1.55)	*(1.14)*	*(0.82)*	*(0.69)*	*(0.77)*	*(0.12)*	*(-0.37)*
Number of observations	12581	11877	11217	10535	9835	9308	8725
Includes school dist. dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes time dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pass x post + pass x post x low	0.279**	0.423***	0.282	0.290	-0.035	0.018	-0.203
Pass x post + pass x post x high	0.381***	0.592***	0.710***	0.730***	0.551***	0.185	-0.246

Table A3.4. Education outcomes after 2001 regulation change by tax bracket, cont.

Panel B: Test Scores

	(1) Mean API, yr t+1	(2) Mean API yr t+2	(3) Mean API, yr t+3	(4) Mean API, yr t+4	(5) Mean API, yr t+5	(6) Mean API, yr t+6	(7) Mean API, yr t+7
Pass bond dummy	-2.900	-3.629	-1.123	-2.206	-4.594*	*-2.531*	-2.503
	(-1.11)	*(-1.39)*	*(-0.41)*	*(-0.82)*	*(-1.88)*	*(-0.96)*	*(-1.04)*
Pass x post	4.848*	5.624**	1.579	4.644	7.079**	5.127	4.081
	(1.68)	*(1.93)*	*(0.50)*	*(1.43)*	*(2.44)*	*(1.58)*	*(1.30)*
Pass bond x post x low p tax	1.968	2.700**	-0.104	-1.219	-0.990	-1.179	1.293
	(1.56)	*(1.95)*	*(-0.06)*	*(-0.71)*	*(-0.56)*	*(-0.66)*	*(0.72)*
Pass bond x post x high p tax	0.349	2.785**	1.633	0.712	0.541	-0.090	-0.869
	(0.30)	*(2.13)*	*(1.04)*	*(0.38)*	*(0.33)*	*(-0.05)*	*(-0.47)*
Fifty-five percent dummy	-1.796	-1.849	0.394	-1.611	-2.055	-1.932	-0.402
	(-1.44)	*(-1.47)*	*(0.26)*	*(-0.95)*	*(-1.43)*	*(-1.02)*	*(-0.22)*
Mean API	0.761***	0.570***	0.407***	0.279***	0.205***	0.156***	0.135***
	(87.27)	*(44.14)*	*(22.77)*	*(13.43)*	*(9.05)*	*(6.45)*	*(5.92)*
Property tax quintile 1 dummy	-1.971	-1.717	0.145	2.133	3.503	3.702	4.402
	(-1.44)	*(-0.91)*	*(0.06)*	*(0.88)*	*(1.27)*	*(1.33)*	*(1.64)*
Property tax quintile 2 dummy	-0.830	-0.077	1.546	2.162	2.428	1.433	2.700*
	(-1.00)	*(-0.07)*	*(1.12)*	*(1.41)*	*(1.48)*	*(0.89)*	*(1.82)*
Property tax quintile 4 dummy	0.476	0.028	0.430	0.526	2.045	0.770	0.261
	(0.57)	*(0.02)*	*(0.27)*	*(0.30)*	*(1.17)*	*(0.44)*	*(0.14)*
Property tax quintile 5 dummy	0.560	-1.035	-0.338	0.215	1.991	-0.461	-2.130
	(0.42)	*(-0.46)*	*(-0.13)*	*(0.08)*	*(0.67)*	*(-0.17)*	*(-0.84)*
Log of local cap exp. per pupil	0.342**	0.775***	0.729***	0.691**	0.495	0.825***	0.490
	(2.18)	*(3.03)*	*(2.60)*	*(2.39)*	*(1.52)*	*(2.94)*	*(1.59)*
Log of state cap. exp. per pupil	-0.041	0.037	0.114	0.146*	0.192**	0.174**	0.167**
	(-0.69)	*(0.45)*	*(1.44)*	*(1.68)*	*(2.13)*	*(2.08)*	*(2.03)*
Log of enrollment	2.745	3.341	6.262	6.919	10.044	12.693**	14.61***
	(1.15)	*(0.67)*	*(1.48)*	*(1.38)*	*(1.59)*	*(2.09)*	*(3.03)*
Long-term debt per pupil	0.015	0.059	0.056	0.070	0.103	0.038	0.029
	(0.15)	*(0.42)*	*(0.34)*	*(0.41)*	*(0.56)*	*(0.22)*	*(0.18)*
Free meal ratio	3.469	14.58***	15.749**	16.540**	17.364**	14.587**	14.480*
	(1.12)	*(3.17)*	*(2.54)*	*(2.39)*	*(2.51)*	*(2.00)*	*(1.65)*
Percent non-Wh non-Asian	-15.72***	-1.032**	-11.212	-8.126	0.128	5.487	7.943
	(-3.18)	*(-2.19)*	*(-1.27)*	*(-0.76)*	*(0.01)*	*(0.33)*	*(0.45)*
Rural dummy	0.768	1.211	1.974	1.250	0.497	0.333	-0.664
	(0.66)	*(0.63)*	*(0.92)*	*(0.48)*	*(0.18)*	*(0.12)*	*(-0.21)*
City dummy	-2.096**	-2.115	-2.795	-3.903*	-3.750	-2.154	-0.364
	(-2.08)	*(-1.28)*	*(-1.43)*	*(-1.74)*	*(-1.63)*	*(-1.00)*	*(-0.17)*
Suburb dummy	-1.599	-1.211	-1.500	-1.594	-1.499	0.246	2.395
	(-1.62)	*(-0.72)*	*(-0.74)*	*(-0.68)*	*(-0.62)*	*(0.11)*	*(1.07)*
Number of observations	8610	7941	7279	6636	5966	5559	4834
Includes school dist. dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes time dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pass x post + pass x post x low	6.817**	8.323***	1.474	3.425	6.089**	3.947	5.374
Pass x post + pass x post x high	5.197*	8.408***	3.212	5.356	7.620*	5.037	3.213

Table A3.4. Education outcomes after 2001 regulation change by tax bracket, cont.
Panel C: Home Prices

	(1) Log mean house price, yr t+1	(2) Log mean house price yr t+2	(3) Log mean house price, yr t+3	(4) Log mean house price, yr t+4	(5) Log mean house price, yr t+5	(6) Log mean house price, yr t+6	(7) Log mean house price, yr t+7
Pass bond dummy	0.001	0.001	-0.009	-0.014	-0.022	-0.024	-0.031
	(0.14)	*(0.14)*	*(-0.95)*	*(-1.31)*	*(-1.77)*	*(-1.78)*	*(-2.28)*
Pass x post	0.012	0.002	0.012	0.012	0.017	0.017	0.021
	(1.00)	*(0.16)*	*(0.81)*	*(0.74)*	*(1.01)*	*(1.02)*	*(1.25)*
Pass bond x post x low p tax	0.008*	0.019***	0.026***	0.033***	0.018	-0.001	-0.037***
	(1.67)	*(2.51)*	*(2.97)*	*(3.61)*	*(1.44)*	*(-0.13)*	*(-4.27)*
Pass bond x post x high p tax	-0.005	-0.003	0.013	0.023**	0.033***	0.050***	0.051***
	(-1.03)	*(-0.43)*	*(1.38)*	*(2.15)*	*(2.80)*	*(4.34)*	*(4.82)*
Fifty-five percent dummy	-0.012	-0.006	-0.011	-0.010	-0.008	-0.003	0.006
	(-1.11)	*(-0.48)*	*(-0.88)*	*(-0.85)*	*(-0.74)*	*(-0.34)*	*(0.57)*
Log of mean house price	0.848***	0.662***	0.466***	0.266***	0.112***	0.016	-0.026
	(52.46)	*(29.23)*	*(15.94)*	*(7.67)*	*(3.00)*	*(0.44)*	*(-0.74)*
Property tax quintile 1 dummy	0.012***	0.020***	0.031***	0.039***	0.044***	0.046***	0.043***
	(2.89)	*(2.67)*	*(3.19)*	*(3.45)*	*(3.72)*	*(4.03)*	*(3.70)*
Property tax quintile 2 dummy	0.006***	0.009*	0.015**	0.020***	0.024***	0.026***	0.026***
	(2.12)	*(1.76)*	*(2.28)*	*(2.52)*	*(2.73)*	*(2.88)*	*(2.99)*
Property tax quintile 4 dummy	-0.008**	-0.012**	-0.021***	-0.029***	-0.032***	-0.032***	-0.033***
	(-2.26)	*(-1.98)*	*(-2.56)*	*(-3.05)*	*(-3.31)*	*(-3.40)*	*(-3.71)*
Property tax quintile 5 dummy	-0.014***	-0.028***	-0.043***	-0.055***	-0.065***	-0.063***	-0.065***
	(-2.61)	*(-2.74)*	*(-3.05)*	*(-3.27)*	*(-3.64)*	*(-3.65)*	*(-4.12)*
Log of local cap exp per pupil	0.000	-0.001	-0.001	-0.001	0.000	0.001	0.001
	(-0.64)	*(-0.71)*	*(-0.97)*	*(-1.13)*	*(-0.28)*	*(0.41)*	*(0.96)*
Log of state cap. exp. per pupil	-0.001***	-0.002***	-0.002***	-0.002***	-0.001**	-0.001	-0.001
	(-2.90)	*(-3.79)*	*(-3.72)*	*(-2.81)*	*(-2.26)*	*(-1.60)*	*(-0.84)*
Log of enrollment	0.004	0.004	-0.003	-0.012	-0.022	-0.025*	-0.021
	(0.73)	*(0.47)*	*(-0.25)*	*(-0.86)*	*(-1.58)*	*(-1.82)*	*(-1.55)*
Long-term debt per pupil	0.000	-0.001	0.000	0.000	0.000	0.001	0.001
	(-0.15)	*(-0.90)*	*(-0.53)*	*(-0.25)*	*(0.14)*	*(0.43)*	*(0.47)*
Free meal ratio	-0.017	-0.036*	-0.060**	-0.089***	-0.116***	-0.123***	-0.092***
	(-1.38)	*(-1.84)*	*(-2.23)*	*(-2.84)*	*(-3.37)*	*(-3.53)*	*(-2.80)*
Percent non-Wh non-Asian	-0.016**	-0.037***	-0.059***	-0.077***	-0.080***	-0.088***	-0.186**
	(-1.99)	*(-3.43)*	*(-4.23)*	*(-4.47)*	*(-4.16)*	*(-4.58)*	*(-2.40)*
Rural dummy	0.012***	0.022***	0.025**	0.029***	0.026**	0.027**	0.018*
	(3.05)	*(3.06)*	*(2.41)*	*(2.58)*	*(2.21)*	*(2.36)*	*(1.65)*
City dummy	0.012***	0.025***	0.034***	0.041***	0.039***	0.035***	0.024**
	(3.82)	*(4.43)*	*(4.29)*	*(4.28)*	*(3.72)*	*(3.24)*	*(2.24)*
Suburb dummy	0.016***	0.038***	0.054***	0.067***	0.071***	0.071***	0.061***
	(4.66)	*(6.46)*	*(6.73)*	*(6.84)*	*(6.53)*	*(6.37)*	*(5.33)*
Number of observations	12937	12920	12243	11553	10872	10193	9524
Includes school dist. dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes time dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pass x post + pass x post x low	0.020	0.021	0.038**	0.045***	0.034*	0.016	-0.016
Pass x post + pass x post x high	0.007	-0.001	0.025	0.035*	0.050***	0.067***	0.072***