Appendices to
"The Value of School Facility Investments:
Evidence from a Dynamic Regression Discontinuity Design"

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Appendix A: Data

Our empirical analysis relies on a data set that merges information on bond elections, school district finances, home prices, student outcomes, and district demographics. We obtain these from a variety of sources:

Fiscal Data

Our data set is built around a panel of school districts derived from the Common Core of Data (CCD), an annual census of school districts. We use data from 1995 through 2006. Some districts merge or split in two during our sample period. For example, there are several cases where overlapping elementary and high school districts merged to form a single unified district.[1] In these cases, we combine pre-merge (or post-split) districts to form a consistent unit over time.

The CCD is also our source for school district finance data. We extract measures of total spending, capital and current expenditures, revenues, and long-term debt between 1995 and 2006. The data also report total enrollment; we use this variable to convert both district finance data and bond proposals (discussed below) to per-pupil measures. We exclude from all analyses observations on districts with total enrollments below 50.

Bond Data

Our election data are drawn from a database of school district bond and parcel tax referenda obtained from the California Education Data Partnership. For each election, we observe the election date, the referendum vote share, the vote share required for passage (55% or 66.7% for bonds), and the voter turnout. For bonds, we also observe the size of the proposed bond issue and a summary of the intended purpose. Our primary sample consists of all general obligation bond measures sponsored by school districts between 1987 and 2006. (We exclude the two measures voted on in 1986.) In some specification checks, we also examine parcel tax elections during the same period.

In a very few cases, we observe simultaneous elections in overlapping elementary and secondary districts. If these districts later merge, we treat them as the same district throughout our sample, leading us to observe two elections on the same date in the same district observation. When this happens, we retain the GO bond with the highest vote share and discard any others.

Housing Data

We obtained data on housing transactions assembled from public records by Dataquick. The data come to us in the form of summaries at the level of the census block group by calendar

[1] Most students (over 70%) in California attend unified school districts, though there are a large number of very small elementary districts.

year, covering the entire state of California from 1988 to 2005. For each block group in each year, we observe the number of transactions and the average over these transactions of the sale price, the square footage, and the lot size.

We use GIS mapping software to assign census block groups to school districts. When school district boundaries and block group boundaries do not line up we use population weights based on the proportion of a block group located within a school district.

After assigning block groups to districts, we average them to obtain district-by-year data. As discussed in the text, we weight block groups within districts by their year-2000 populations. This ensures that changes in the distribution of transactions across block groups within districts do not produce spurious changes in district average sale prices. To further control for changes in the composition of transacted homes, we include in our house price models controls for the average square footage and lot size of transacted homes and for the number of sales, all averaged to the district level. The results are insensitive both to weighting block groups by the number of transactions and to excluding the house characteristic controls, as shown in Table VI (row (9)).

Academic Achievement

California has changed its testing regime several times since the 1980s. It is thus impossible to construct a long district-level panel of student achievement that uses consistent tests and samples. We therefore merge results from several different tests. We focus on 3rd and 4th grades, as these were fairly consistently tested. We use 3rd grade reading and math scores from the California Achievement Tests (CAT) from 2003-2007; 3rd grade scores from the Stanford-9 exam from 1998-2002; and 4th grade scores from the California Learning Assessment System (CLAS) from 1993 and 1994. Though developed by different publishers and therefore not directly comparable, both the CAT and Stanford-9 exams are nationally-normed multiple-choice exams.

To account for differences in exams across years, we standardize the scaled scores each year using school-level means and standard deviations. We use the resulting standardized scores for all analyses. To convert estimated effects on these standardized scores to student-level standard deviation effects (as discussed in Section VII), we use the ratio of school-level to student-level standard deviations on the 2007 California Standards Tests (CST), the only test for which student-level statistics could be obtained.

Demographic Characteristics

We assemble a variety of measures of the demographic characteristics of district residents over time. First, we construct the racial composition and average family income of homebuyers in each district between 1992 and 2006 from data collected under the Home Mortgage Disclosure Act (HMDA). For each owner-occupied home loan – we exclude refinancings – the HMDA data report the census tract, the race and household income of the buyer, and the year of the transaction. We use a GIS matching procedure similar to the one implemented for house prices to map the tract-level HMDA data to school districts, then average the data to create a district-by-calendar year panel. We treat our measure as characterizing in-migrants to the district, though we are unable to exclude intra-district movers from the calculation. Note also that renters are not represented in these data.

We also use two measures of the demographic composition of the students in the district. We obtain from the California STAR data set the average education of parents at the school. This is constructed from surveys sent home with students each year, and is available from 1998 to 2007. We obtain measures of the racial composition of students from the school-level component of the CCD. Racial breakdowns are available at the school level from 1987 to 2005 and at the school-by-grade level from 1998 to 2005. We aggregate these to the district-year and district-grade-year levels. Note that both the STAR and CCD data pertain only to public school students.

Merging calendars

Our school district finance and demographic data describe academic years. We treat bond elections between May of calendar year t and April of calendar year t+1 as occurring during the t-t+1 academic year. Thus, for elections occurring between May 1, 1994 and April 30, 1995 we use the 1994-5 academic year as relative year 0, 1995-6 as relative year 1, and so on.[2]

Our housing data are at the level of the calendar year. As most housing transactions happen over the summer, relative year 0 for our housing analysis is the calendar year containing the first summer after the election. Thus, for elections occurring between October 1, 1994 and September 30, 1995, transactions during calendar year 1995 are assigned to relative year 0, those during 1996 are relative year 1, and so on. Note that this means that some transactions in relative year 0 may have happened before the election (i.e. transactions in January 1995 when the election was in June), and there are even a few transactions in relative year -1 that occurred after the election (i.e. a transaction in November for one of the rare elections in September).

Appendix B: Additional Results

We present in this appendix additional descriptive statistics and results that are omitted from the main paper due to space constraints.

Bond Dynamics

Figure A.1 presents an analysis of the number of bond measures considered (left panel) or passed (right panel) over the four years following an initial election, by the margin of victory or defeat in the initial election. As in Figures II and IV, we group measures into two-percentage-point bins defined by the vote share relative to the required threshold. There is a clear discontinuity at measure passage. When the initial measure passed, even by a small amount, only about 10% of districts considered another measure shortly thereafter. (Even these primarily reflect districts that would merge at some point in the future; there were few follow-up elections after initial victories in districts with constant boundaries over our panel.) By contrast, the typical district where a measure failed narrowly considered approximately one additional measure (and passed 0.6) within four years.

Effects of Bond Authorization on Fiscal Outcomes

Figures A.2 through A.4 present additional results on the fiscal impact of bond authorization. Here, we plot the average long term debt (A.2), average capital expenditures (A.3), and average current expenditures (A.4), all in per pupil terms, as functions of the vote share. In each figure, one series shows the fiscal measures the year before the election, while the other shows the measures three years afterward. There is no sign of a discontinuity in any of the measures the year before the election. We see large discontinuities at the passage threshold in the stock of long term debt and the flow of capital spending three years afterward. There continues to be no discontinuity in current spending, consistent with our discussion in the text about how the bond funds appear to stick in the capital account.

[2] The most common month for bond elections is November, accounting for just under half of our sample. March and June are also common, with 20-25% of elections occurring in each month. Just under 10% of elections are in April. We observe at least one election in every month except July.

Figure A.5 presents additional evidence on this point. Here, we plot point estimates and confidence intervals from our recursive and one-step estimators for the TOT effect of bond authorization on current spending. This is analogous to the identical analyses of total and capital spending in Figure II. We see no evidence that bond authorization has any effect on current spending in any future year.

Pre-Election Price Trends

Table III of the main paper shows that there is no significant relationship between bond authorization and the pre-election level or one-year change in our various outcome variables, once the election vote share is controlled flexibly. However, in the models for the year-to-year change in house prices before the election the point estimates remain non-trivial even with flexible controls. This raises a question about the validity of our estimates: if election passage is correlated with the pre-election trend in housing prices, post-election changes may not be interpretable as causal effects.

Table A.1 presents several specifications intended to get at this issue. Row (1) presents the estimates of the effect of bond passage on the first-difference of pre-election housing prices from columns (5) and (6) of Table III. The first specification includes indicators for the calendar year of the election and for a vote share threshold of 55%; the second adds a cubic in the election vote share. Estimates indicate that measure passage is associated with a 1.5 percentage point higher rate of annual appreciation in the year before the election without vote share controls, or 2.0% (not significant) with controls.

The remaining rows examine effects on house price growth over longer periods before the election. Row (2) shows the estimated effect of bond authorization on the two-year pre-election change (from three years before the election to one year before the election). Coefficients here are slightly larger than those for the one-year change, but the difference is far smaller than the doubling that would be expected were the coefficients simply capturing differences in trends. In rows (3) and (4) we look at appreciation over even longer windows; point estimates are quite similar to those for the two-year change.

To further investigate pre-election trends, we look at changes over periods ending in *t-2* (i.e. two years before the focal election). Rows (5) through (7) show estimates for price changes over windows of increasing length ending in *t-2*. Point estimates are generally small and show no consistent pattern.

Taken together, the estimates suggest that the point estimates for the effect of bond authorization on the once-lagged annual rate of housing price appreciation are not reflecting a correlation between the election outcome and the long-run trend in the district, but rather a small (and insignificant) correlation with transitory shocks to prices in year t-1. This is unlikely to confound the interpretation of the estimated effects of bonds on post-election house prices.[3]

Effects of Authorizing \$1 in Bonds

Panel B of Table V presents estimates of the effects of authorizing \$1 in bonds per pupil on house prices. These are estimated slightly differently than the effects of approving a proposed bond that we report elsewhere in the text. In the ITT and the one-step estimates of the reduced form, we simply replace the bond authorization indicators in equations (7) and (12) with

[3] We have also estimated a specification analogous to the pooled specification from column (7) of Table III, including more pre-election observations in the sample and allowing for bond authorization effects on house price growth in t-2 and t-3 as well as on prices in t-1. The t-1 coefficient (0.019; SE 0.010) closely resembles that from Table III, but the t-2 coefficient is 0.005 (SE 0.013) and the t-3 coefficient is -0.008 (SE 0.012). Again, these offer no evidence of an association between bond authorization and the trend in home prices in the district.

continuous measures of the value of bonds authorized (set to zero if no bond is authorized). The resulting equations are estimated via IV, using the original bond authorization indicators as instruments for the dollar value of bonds.

Modification of the recursive estimator is slightly more complex. We redefine the π_τ coefficients from Section IV to be the (ITT) effect of authorizing \$1 in bonds in year t on the size of the bond authorized in year t+τ (again defining the absence of a bond authorization as equivalent to \$0 worth of bonds authorized). Table A.2 presents the π estimates, first for our initial definition (the effect of approving a bond issue today on the probability of approving a bond issue in the future) in column (1) and then for the redefinition in Column (2). The recursion formula (11) (or (14) for forward-looking prices) for the TOT applies directly, using the redefined πs and the ITT estimates of the effect of \$1 in bond authorization for the βs.

Table A.3 presents estimates of the effect of authorizing \$1 in bonds on fiscal outcomes in years 1 through 6. The coefficients here are interpretable as the increase in spending in each category per dollar of bonds authorized. Thus, for each dollar of bonds authorized, our recursive TOT estimator indicates that \$0.06 is spent in year 1, \$0.18 is spent in year 2, and so on. The total impact on spending in years 1 through 6 is \$1.24. We cannot reject the hypothesis that this sum is \$1, corresponding to spending 100% of the additional funds by year 6. The one-step estimator yields very similar estimates. As expected, the ITT analysis indicates somewhat smaller spending effects, particularly in years 3-5.

Appendix C: Willingness to Pay

This appendix describes how the effects of bond passage on house prices, as reported in Panel A of Table V, can be converted to obtain estimates of the marginal homebuyer's willingness to pay for school spending.

We make a number of assumptions in the calculations. We assume that the interest rate on bonds is 4.6%, that a rental unit is equivalent to 0.6 owner-occupied units, and that the tax burden is divided evenly among all of the (owner-equivalent) housing units in the district. We consider a bond issue of \$6,309 per pupil, with a 30-year maturity, in a district with average housing price of \$236,433 and 2.4 owner-equivalent units per pupil.[4] We consider two discount rates, 7.33% and 5.24%, both taken from Barrow and Rouse (2004).

We begin with the willingness to pay for additional spending. We assume that marginal homebuyers have exactly one school-aged child per household and that the price effects reflect the willingness to pay for a per-pupil share of the average bond. The estimates in column (3) of Table V indicate that bond passage raises prices by 3.02% in the first year, or \$7,136 for the average house. Property taxes must rise by \$163 per year per house in order to pay off the bond. The present discounted value of these taxes is \$1,952 (ρ_1 = 7.33%) or \$2,431 (ρ_2 = 5.24%). Thus, the total cost is \$7,136 + \$1,952 = \$9,087 (ρ_1) or \$7,136 + \$2,431 = \$9,567 (ρ_2). With 2.4 homes per pupil, a single home's share of the average bond issue is \$2,629. If we assume that marginal

[4] The bond amount and housing price are the means among districts with measures that pass or fail by less than 2%. The equivalence of a rental unit with 0.6 owner-occupied units is computed by comparing the present discounted value of the California mean monthly rent for a renter-occupied unit in 2000 (using a 5.24% discount rate) with the mean value of owner occupied homes. The count of housing units per pupil is the average in our sample; the average for districts with close elections is similar.

homebuyers have exactly one school-aged child per household, the implied WTP for $1 in spending on the household's children is $1.44 ($\rho_1$) or 1.52 ($\rho_2$).

Table V indicates that the price effects are larger a few years after the election than in the first year. By this point, some of the taxes will already have been paid, but (assuming that planning and executing a construction project takes several years) none of the benefits will have been received. Moreover, because the arrival of benefits is approaching, these benefits will be discounted to a lesser degree. Thus, a constant WTP would imply rising price effects over this period. The estimated WTP (based on our one-step estimates) is 1.63 in year 2, 2.12 in year 3, and 1.70 in year 4 (for ρ_1; ρ_2 implies 1.76, 2.31, and 1.89, respectively). It is not possible to extend these estimates beyond year 4, as the assumption that all of the benefits arrive after that date is untenable. Recursive estimates yield somewhat larger WTP, rising from 1.38 in year 0 to 2.35 in year 4 (with ρ_1). The forward-looking estimates imply still larger WTP but much smaller increases over time, from $2.60 in year 0 to $2.74 in year 6 (with $\rho = 7.33\%$). This suggests that the dynamic pattern of tax payments can account for nearly all of the slope seen in Figure VI.

The calculations thus far omit two tax considerations. First, because California freezes valuations at the original purchase price, new homebuyers pay a disproportionate share of property taxes. Using Ferreira's (2007) tabulation of effective property tax rates in California, we compute that property tax increment that raises $1 per house will cost a new homebuyer about $1.35 and a long-term resident only about $0.58. This implies that the above calculations understate the tax burden borne by new buyers. Offsetting this, however, is the income tax deductibility of property tax payments and mortgage interest payments. When we incorporate both of these features into the calculation, the estimated WTP falls by about 20%. Even with this adjustment, our one-step estimates imply that marginal homebuyers are willing to give up at least $1.35 ($\rho_1$) or $1.50 ($\rho_2$) in consumption in order to provide $1 per pupil to the school district's capital budget.[5] The recursive estimates indicate larger WTP, around $2 in our forward-looking calculation and $1.80 otherwise.

The estimates in Panel B of Table V provide an alternative method of calculating the willingness to pay. The willingness to pay for an additional $1 in school spending per pupil equals the effect of $1 in bond authorization on house prices (as reported in Panel B of Table V) plus the present discounted value of the additional property tax payments that will be needed to pay off the bond. Ignoring issues of income tax deductibility and unequal tax shares, the assumptions above imply that the PDV of tax payments needed to finance $1 per pupil in bonds is $0.31 per house. Adding this to the year-0 TOT effects from Panel B of Table V, we estimate the WTP for $1 in school spending per pupil as $1.70 (using our one-step estimates) to $2.10 (using the recursive estimates). These are quite comparable to those discussed above. As earlier, estimates are larger when we use price effects several years after a bond is authorized.

Appendix D: Comparisons to Earlier Work

Bradbury, Mayer, and Case (2001) and Hilber and Mayer (2004) report the elasticity of home prices with respect to annual school spending rather than the WTP. To convert our estimates to be comparable to theirs, we first compute the size of the annuity that yields the same

[5] These WTP figures are based on the year-4 effects. We assume that new homebuyers will have tax shares 30% above average over the life of the bond, that they finance 80% of the increment to home prices using a 30-year mortgage with a fixed 8% interest rate, and that interest is fully deductible with a marginal income tax rate of 32%.

discounted value as the temporary spending increase made possible by the bonds. This is $462 ($\rho_1$) or $330 ($\rho_2$). Next, we divide this by annual per pupil spending (in the year of the election, averaged over all districts where a measure passed or failed by less than two percentage points), $6,767. The bond measures therefore represent an increase in the flow of spending of 6.8% (ρ_1) or 4.9% (ρ_2). Comparing these to the one-step housing price effects from Table V, Panel A, we obtain elasticities of year-0 prices with respect to spending of 0.44 or 0.61, respectively. Elasticities of year-4 prices are 0.68 and 1.03, again depending on the discount rate.

Barrow and Rouse (2004) estimate the effect of state aid—which they interpret as free transfers—on aggregate home prices, both measured in per pupil terms. To adapt our results to a similar metric, we first convert our effects on house prices to effects on property values per pupil. The estimates from Table V, Column 3 indicate price effects of $7,136 in year 0 and $12,348 in year 4. Multiplying this by 2.4 housing units per pupil, we find effects of $17,126 in year 0 and $29,635 in year 4. Comparing these to the average bond value, $6,309, we estimate that school spending with present discounted value of $1 per pupil raises aggregate property values by $2.71 in year 0 and $4.70 in year 4.

Barrow and Rouse's (2004) estimates represent the effect of free transfers, while the bond spending studied here must be paid for by increased property taxes. To compute the effect of tax-financed spending implied by Barrow and Rouse's estimates, we first note that they compare their results to the null hypothesis that prices rise by $1 per dollar of present discounted value of future state aid. The ratio of their estimates to the null, less one, can therefore be interpreted as the effect of $1 of state aid to be paid for with $1 in taxes (both in present value). Their main specification yields an estimated coefficient of 30.285; this corresponds to a housing price effect per dollar of bonds of 0.16 (with a discount rate of 5.24%) to 0.63 (with a discount rate of 7.3%).

While these estimates are lower than our findings, several further considerations help reconcile these results. First, Barrow and Rouse find much greater capitalization in better-educated and higher-income districts. To the extent that California's education and income levels are higher than the national average, we would expect Barrow and Rouse's estimate for all districts to underestimate the housing price effect for the state. Barrow and Rouse's estimates for the top 80% of districts by education (as measured by the share of the population without a high school diploma) yield a range of 0.42 – 0.99, while their estimates for the top quintile of districts by average household income yield 2.52-3.93. Further, the interpretation of their results is quite sensitive to the discount rate: using a 10% discount rate, their estimates of the effect on property values range from 1.1 for all districts to 5.7 for the top quintile. Finally, Barrow and Rouse's estimates reflect the value of a permanent increase in state aid, while homebuyers might be uncertain about the permanency of the policy changes they examine. If the stream of state aid is expected to persist for only 20 years, the implied effect on property values per dollar of future spending is much larger. Thus, our estimates are not out of line with theirs.

Table A.1
Tests of Pre-Election Differences in Trends between Passed and Failed Bonds

	No vote share controls	Control for cubic in vote share
	(1)	(2)
Dependent variable		
Change in prices, through t-1		
1. from t-2	**0.015**	0.020
	(0.007)	(0.010)
2. from t-3	**0.021**	0.026
	(0.010)	(0.015)
3. from t-4	**0.023**	0.021
	(0.010)	(0.018)
4. from t-5	0.018	0.032
	(0.011)	(0.020)
Change in prices through t-2		
5. from t-3	-0.013	-0.005
	(0.008)	(0.012)
6. from t-4	0.013	0.005
	(0.009)	(0.016)
7. from t-5	0.012	0.025
	(0.010)	(0.017)

Notes: Each entry represents the bond authorization effect from a separate specification. Specifications in columns (1) and (2) are identical to those in columns (5) and (6) of Table III of the main text, respectively, except that the dependent variable varies.

Table A.2
The Effect of Authorizing a Bond on the Probability of Passing a Later Bond, and the Effect of Authorizing $1 in Bonds on the Value of Later Bond Authorizations

	Effect of authorizing a bond on the probability of authorizing a later bond (1)	Effect of authorizing $1 in bonds on the dollar value of later bond authorizations (including zeros) (2)
1 year later	**-0.23**	**-0.29**
	(0.03)	(0.07)
2 years later	**-0.19**	**-0.23**
	(0.03)	(0.06)
3 years later	**-0.11**	-0.10
	(0.03)	(0.05)
4 years later	**-0.10**	-0.04
	(0.03)	(0.05)
5 years later	-0.01	0.02
	(0.03)	(0.05)
6 years later	-0.01	0.04
	(0.03)	(0.07)
7 years later	0.00	0.15
	(0.04)	(0.12)
8 years later	0.01	0.03
	(0.05)	(0.09)
9 years later	0.07	0.14
	(0.04)	(0.11)
10 years later	0.01	-0.03
	(0.04)	(0.14)
11 years later	0.03	-0.21
	(0.06)	(0.20)
12 years later	0.00	0.18
	(0.06)	(0.12)
13 years later	0.06	0.11
	(0.07)	(0.16)
14 years later	-0.08	0.08
	(0.05)	(0.12)
15 years later	-0.05	0.13
	(0.08)	(0.22)
16 years later	0.04	0.03
	(0.11)	(0.33)
17 years later	0.00	-0.13
	(0.13)	(0.24)
18 years later	-0.09	1.98
	(0.15)	(1.95)

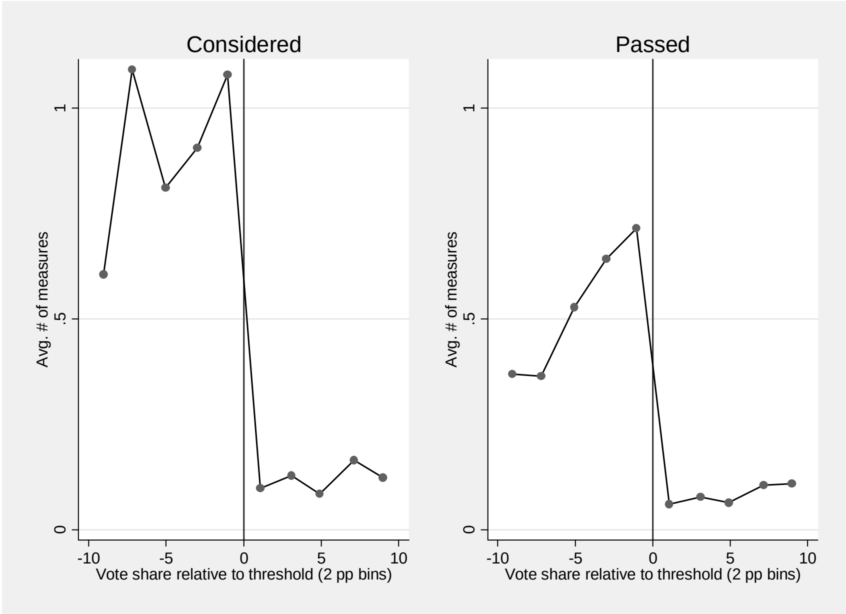
Notes: Each column represents a separate specification. Column (1) presents the π coefficients corresponding to the recursive estimates in Table V, Panel A. Column (2) presents the π coefficients corresponding to the recursive estimates in Table V, Panel B. Each controls for cubics in the focal election vote share and an indicator for a 55% threshold, each interacted with relative year, and for district and calendar year fixed effects. Standard errors, in parentheses, are clustered on the school district. Bold coefficients are significant at the 5% level.

Table A.3
The Effect of Authorizing $1 in Bonds on Fiscal Outcomes:
Intent-to-Treat (ITT) and Treatment-on-the-Treated (TOT) Effects

	1 yr later (1)	2 yrs later (2)	3 yrs later (3)	4 yrs later (4)	5 yrs later (5)	6 yrs later (6)
A. Intent-to-treat (ITT)						
Total expenditures PP	**0.08**	**0.20**	**0.26**	**0.22**	0.07	-0.05
	(0.04)	(0.05)	(0.05)	(0.07)	(0.07)	(0.08)
Capital outlays PP	0.06	**0.17**	**0.22**	**0.19**	0.07	-0.06
	(0.03)	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)
Current instructional expenditures PP	0.01	0.00	0.00	-0.01	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
State and federal transfers PP	0.02	0.01	-0.02	0.02	0.03	0.00
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)
B. Treatment-on-the-treated (TOT)						
Recursive estimator						
Total expenditures PP	**0.06**	**0.18**	**0.29**	**0.32**	**0.24**	0.13
	(0.03)	(0.05)	(0.06)	(0.09)	(0.10)	(0.13)
Capital outlays PP	0.05	**0.16**	**0.27**	**0.29**	**0.23**	0.11
	(0.03)	(0.04)	(0.05)	(0.07)	(0.09)	(0.11)
Current instructional expenditures PP	0.01	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)
State and federal transfers PP	0.01	0.00	-0.03	-0.01	0.00	-0.03
	(0.02)	(0.03)	(0.04)	(0.05)	(0.05)	(0.06)
One-step estimator						
Total expenditures PP	0.07	**0.17**	**0.30**	**0.36**	**0.21**	0.12
	(0.04)	(0.07)	(0.07)	(0.08)	(0.07)	(0.08)
Capital outlays PP	0.06	**0.15**	**0.27**	**0.32**	**0.20**	0.09
	(0.03)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)
Current instructional expenditures PP	0.01	0.00	-0.01	-0.01	-0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
State and federal transfers PP	0.02	-0.01	0.05	0.03	0.03	0.01
	(0.03)	(0.04)	(0.06)	(0.05)	(0.04)	(0.05)

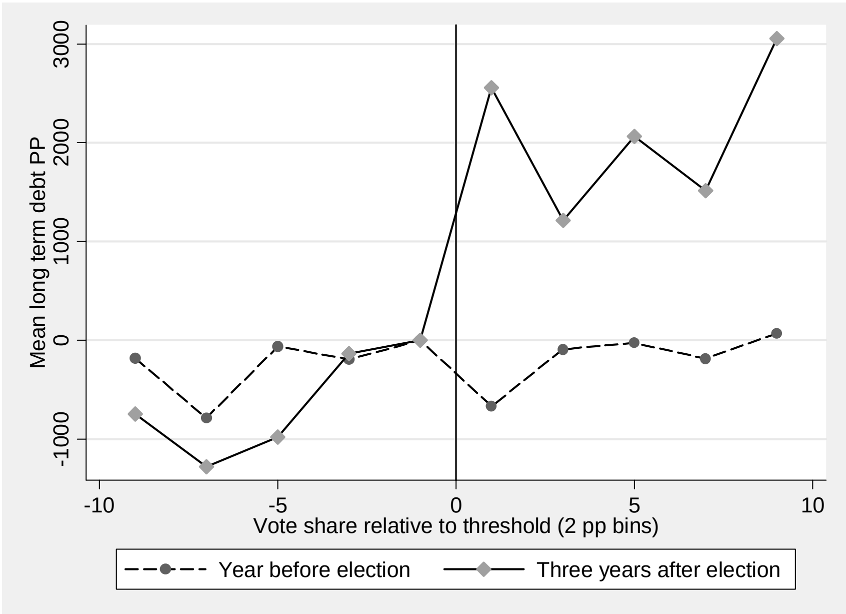
Notes: Samples and specifications correspond to those in Table IV, with one exception: where in Table IV the regressions are estimated by OLS and include indicators for measure authorization, here the estimates are via IV and the measure authorization indicators are used as instruments for the dollar value of bonds authorized (per pupil, and set to zero if no bond is authorized).

Figure A.1
Number of Additional Measures Considered and Passed within Four Years Following Bond Measure Election, by Margin of Victory/Defeat



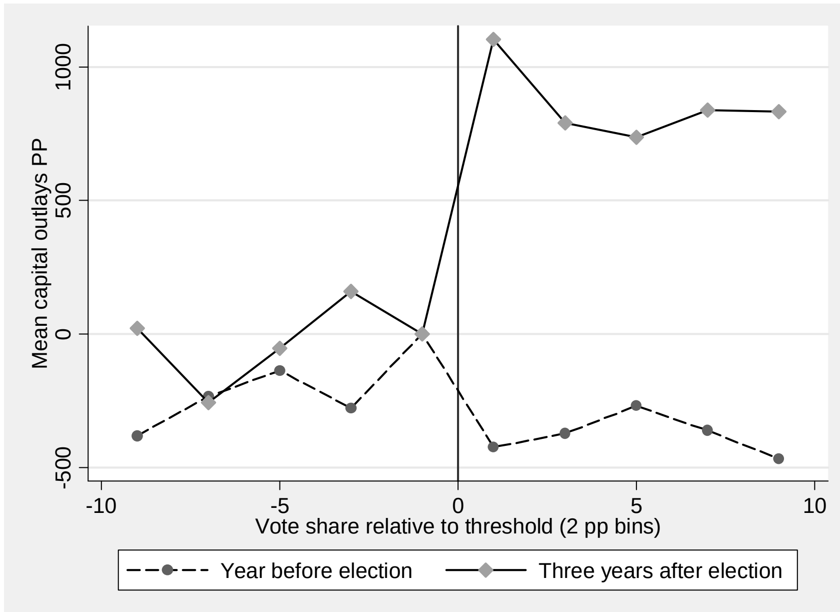
Notes: Graphs show average number of measures considered or passed in the first four years after the focal election, by the vote share in that focal election. Focal elections are grouped into bins two percentage points wide: Measures that passed by between 0.001% and 2% are assigned to the 1 bin; those that failed by similar margins are assigned to the -1 bin.

Figure A.2
Long Term Debt per Pupil, by Vote Share, One Year before and Three Years after Election, by Margin of Victory/Defeat



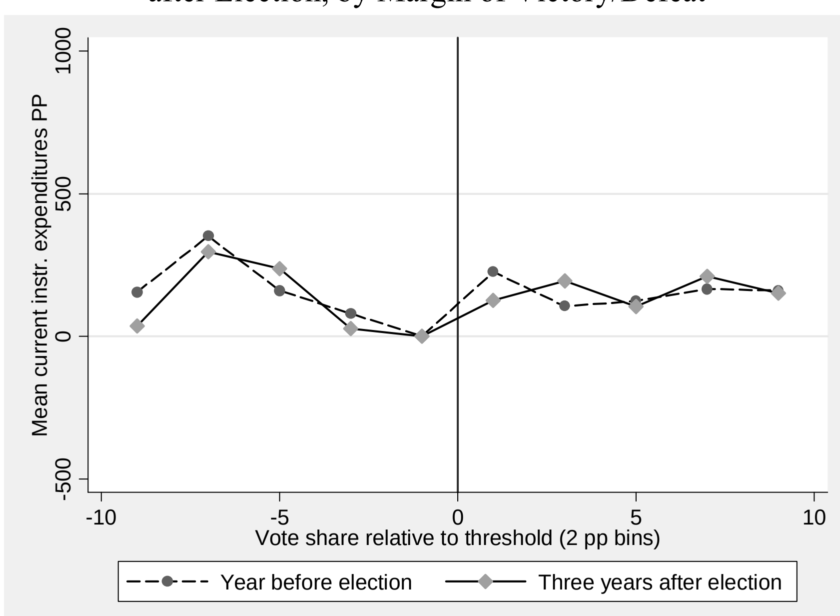
Notes: Graph shows average long term debt per pupil in each bin. Averages are conditional on year fixed effects, and the -1 bin is normalized to zero. Measures that passed by between 0.001% and 2% are assigned to the 1 bin; those that failed by similar margins are assigned to the -1 bin.

Figure A.3
Capital Outlays per Pupil, by Vote Share, One Year before and Three Years after Election, by Margin of Victory/Defeat



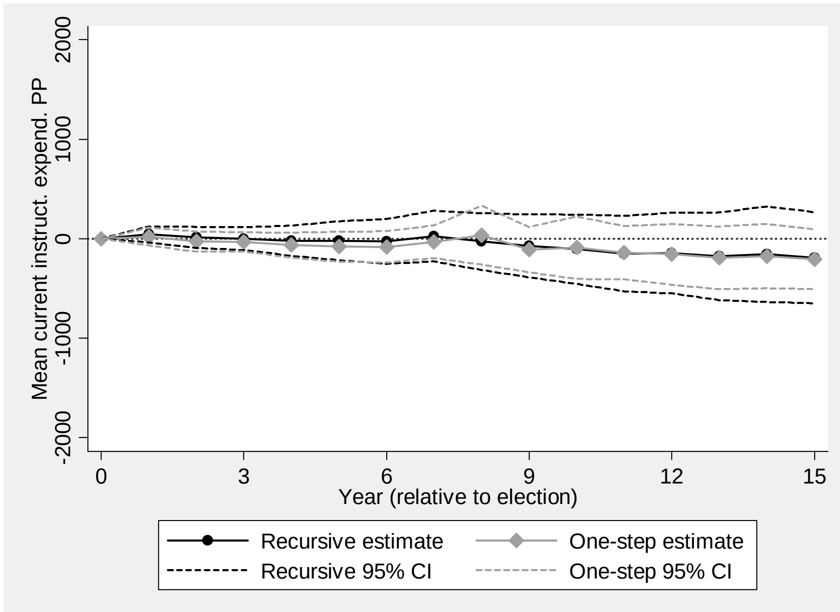
Notes: Graph shows average capital outlays per pupil in each bin in the listed year relative to the election. Averages are conditional on year fixed effects, and the -1 bin is normalized to zero. Measures that passed by between 0.001% and 2% are assigned to the 1 bin; those that failed by similar margins are assigned to the -1 bin.

Figure A.4
Current Instructional Expenditures per Pupil, by Vote Share, One Year before and Three Years after Election, by Margin of Victory/Defeat



Notes: Graph shows average current instructional expenditures per pupil in each bin in the listed year relative to the election. Averages are conditional on year fixed effects, and the -1 bin is normalized to zero. Measures that passed by between 0.001% and 2% are assigned to the 1 bin; those that failed by similar margins are assigned to the -1 bin.

Figure A.5
Recursive and One-Step Estimates of Dynamic TOT Effects of Bond Passage on Current Instructional Expenditures per Pupil, by Years since Election



Notes: Graph shows coefficients and 95% confidence intervals for bond passage effects at each lag from the "recursive" and "one-step" estimators discussed in the text. Standard errors are clustered at the district level.