The Value of School Facility Investments:

Evidence from a Dynamic Regression Discontinuity Design[*]

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Abstract

Despite extensive public infrastructure spending, surprisingly little is known about its economic return. In this paper, we estimate the value of school facility investments using housing markets: standard models of local public goods imply that school districts should spend up to the point where marginal increases would have zero effect on local housing prices. Our research design isolates exogenous variation in investments by comparing school districts where referenda on bond issues targeted to fund capital expenditures passed and failed by narrow margins. We extend this traditional regression discontinuity (RD) approach to identify the dynamic treatment effects of bond authorization on local housing prices, student achievement, and district composition. Our results indicate that California school districts under-invest in school facilities: passing a referendum causes immediate, sizable increases in home prices, implying a willingness to pay on the part of marginal homebuyers of $1.50 or more for each $1 of capital spending. These effects do not appear to be driven by changes in the income or racial composition of homeowners, and the impact on test scores appears to explain only a small portion of the total housing price effect.

I. Introduction

Federal, state, and local governments invest more than $420 billion in infrastructure projects every year, and the American Recovery and Reinvestment Act of 2009 promises substantial increases in capital spending in the years to come.[1] School facilities may be among the most important public infrastructure investments: $50 billion is spent on public school construction and repairs each year (U.S. Department of Education 2007), yet many of the more than 97,000 public elementary and secondary schools in the United States are in need of renovation, expansion, and repair. A third of public schools rely on portable or temporary classrooms and a quarter report that environmental factors, such as air conditioning and lighting, are "moderate" or "major" obstacles to instruction (U.S. Department of Education 2007, Table 98).

Despite the importance of capital spending, little is known about the overall impact of public infrastructure investment on economic output,[2] and even less is known about the effects of school facilities investments.[3] Two central barriers to identification have been difficult to overcome. First, resources may be endogenous to local outcomes. Variation in capital spending is typically confounded with other factors (e.g., the state of the local economy or the socioeconomic status of students) that also determine outcomes.[4] Second, even causal estimates of the effects of investments may miss benefits that do not appear in measured output. This is likely to be a particular problem for school facilities, which may yield difficult-to-measure non-academic benefits like aesthetic appeal or student health and safety.

Housing markets can be used to overcome the challenge of measuring outputs. If homebuyers value a local project more than they value the taxes they will pay to finance it,

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[1] Council of Economic Advisers (2009), Table B-20. This includes gross investment in structures, equipment, and software for both military and non-military uses.

[2] Aschauer (1989a, 1989b, 1989c) is an early participant in this literature. Reviews by Gramlich (1994) and Munnell (1992) highlight a number of unresolved endogeneity issues. Pereira and Flores de Frutos (1999) and Pereira (2000) address some of the endogeneity issues and find sizable returns to infrastructure investments.

[3] See Jones and Zimmer (2001); Schneider (2002); and Uline and Tschannen-Moran (2008). Also closely related is the long literature on the effects of school spending more generally. Hanushek (1996) reviews more than 90 studies and concludes that "[s]imple resource policies hold little hope for improving student outcomes." Card and Krueger (1996) dispute Hanushek's interpretation of the literature. See also Hanushek (1986, 1997); Card and Krueger (1992); and Heckman, Layne-Farrar and Todd (1996).

[4] Angrist and Lavy (2002) and Goolsbee and Guryan (2006) exploit credibly exogenous variation in school technology investments. Neither study finds short-run effects on student achievement.

spending increases should lead to increases in housing prices.[5] Indeed, in standard models, a positive effect of tax increases on local property values is direct evidence that the initial tax rate was inefficiently low. But this strategy does not avoid the challenge of obtaining *causal* effects, which can be difficult when localities are free to endogenously choose their spending levels.

In this paper we implement a new research design that isolates exogenous variation in school investments. School capital projects are frequently financed via local bond issues, repaid from future property tax receipts. In many states, bonds can be issued only with voter approval. Although school districts that issue bonds are likely to differ in both observable and unobservable ways from those that do not, these differences can be minimized by focusing on very close elections: a district where a proposed bond passes by one vote is likely to be similar to one where the proposal fails by the same margin, though their "treatment" statuses will be quite different. Thus, a regression discontinuity (RD) framework can be used to identify the causal impact of bond funding on district outcomes.[6]

Several previous papers have used elections as sources of identification in RD models.[7] Our analysis is complicated by the dynamic nature of the bond proposal process. A district that narrowly rejects an initial proposal is likely to consider and pass a new proposal shortly thereafter. Moreover, bond effects may occur with non-trivial and unknown lags, both because new bond-financed facilities do not come online until several years after the initial authorization and because sticky housing markets may respond slowly to new information.

Traditional experimental and quasi-experimental analytical techniques cannot fully accommodate the presence of both types of dynamics, in treatment assignment and in treatment effects.[8] When treatment dynamics are important, researchers usually either restrict treatment effects to be constant or focus on the so-called "intent-to-treat" (ITT) effects of the initial treatment assignment. We develop methods for identifying dynamic "treatment-on-the-treated"

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[5] See Oates (1969); Dee (2000); Palmon and Smith (1998); Starrett (1981); and Bradbury, Mayer and Case (2001). A related body of research uses housing markets to measure household willingness to pay for school *quality*. See Rosen (1974) for an economic interpretation of the hedonic model, and Black (1999); Bayer, Ferreira, and McMillan (2007); Kane, Riegg, and Staiger (2006); and Rothstein (2006) for empirical applications.

[6] Early discussions of the RD design include Thistlethwaite and Campbell (1960) and Cook and Campbell (1979). See also Hahn, Todd, and Van der Klaauw (2001); Imbens and Lemieux (2007); and Lee and Lemieux (2009).

[7] See, e.g., Lee (2001, 2008); Lee, Moretti, and Butler (2004); DiNardo and Lee (2004); Cellini (forthcoming); Pettersson-Lidbom (2008); and Ferreira and Gyourko (2009).

[8] Ham and LaLonde (1996) model the dynamic treatment effects of job training in experimental data. They focus, however, on the impact of initial treatment assignment (i.e. of the intention to treat) and do not exploit noncompliance with this assignment. See also Card and Hyslop (2006).

(TOT) effects in the presence of dynamics in treatment assignment. To our knowledge, our proposed estimators are new to the literature. They might be fruitfully applied in a variety of other settings.[9]

We apply our estimators to a rich data set combining information on two decades of California school bond referenda with annual measures of school district spending, housing prices, district-level demographics, and student test scores. We focus on California because it provides a large sample of close elections, but it is important to emphasize that California's school finance system is unique. Nearly all school spending in California is determined centrally, and the "general obligation" bonds we study are essentially the only source of local discretion. As in other states, bond revenue is restricted to capital projects. Although the theoretical literature emphasizes the futility of restricted funding (see, e.g., Bradford and Oates, 1971a,b), it seems to be effective in our data: as we show below, bond revenues remain in the capital account. We therefore interpret the impact of bond passage on home prices and test scores as reflecting the effects of school facility investments.

We find that passage of a bond measure causes house prices in the district to rise by about six percent. This effect appears gradually over the two or three years following the election and persists for at least a decade. Our preferred estimates indicate that marginal homebuyers are willing to pay, via higher purchase prices and expected future property taxes, $1.50 or more for an additional dollar of school facility spending, and even our most conservative estimates indicate a willingness to pay of $1.13.

We find little evidence of changes in the income or racial composition of local homebuyers following the passage of a bond. Estimated effects on student achievement are extremely imprecise and provide, at best, ambiguous evidence for positive effects at long lags. Even our largest point estimates for the achievement effects are too small to fully explain the impact of bond authorization on housing prices, however. Evidently, prices reflect dimensions of school output that are not reflected in student test scores. This highlights the importance of using housing markets—rather than simply test score gains—to evaluate school investments.

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[9] Examples in the RD literature include studies of the effect of incumbency on electoral outcomes (Lee 2001, 2008); the effect of unionization on employer survival and profitability (DiNardo and Lee 2004; Lee and Mas 2009); the effect of passing a high school graduation exam (Martorel 2005; Ou 2009); and the effect of access to payday loans (Skiba and Tobacman 2008). It would also be straightforward to extend our strategy to experimental and quasi-experimental settings where agents have multiple opportunities to be assigned to treatment.

While much of the public choice literature emphasizes the potential for over-spending by "Leviathan" governments, our results suggest that the opposite is the case. They provide clear evidence that our sample of school districts under-invest in school facilities even with (limited) local control.[10] Caution is required, however, in attempting to generalize this result beyond our sample. Returns to marginal school spending may be lower in districts where the referendum election is not close or in states that allow for more local control.

The remainder of the paper is organized as follows: Section II describes the California school finance system; Section III develops simple economic models of resource allocation and capitalization; Section IV describes our research design and introduces our estimators of dynamic treatment effects; Section V describes the data; Section VI validates our regression discontinuity strategy; Section VII presents our estimates; and Section VIII concludes.

II. California School Finance

California was known in the post-war era for its high quality, high spending school system. By the 1980s and 1990s, however, California schools were widely considered underfunded. In 1995, per-pupil current spending was 13 percent below the national average, ranking the state 35th in the country despite its relatively high costs. Capital spending was particularly stingy, 30 percent below the national average.[11] California schools became notorious for their overcrowding, poor physical conditions, and heavy reliance on temporary, modular classrooms (see, e.g., New York Times 1989).

Much of the decline in school funding has been attributed to the state's shift to a centralized system of finance under the 1971 *Serrano v. Priest* decision and to the passage of Proposition 13 in 1978. In the regime that resulted, the property tax rate was fixed at 1% and the state distributed additional revenues using a highly egalitarian formula.[12] Districts were afforded no flexibility and there was little provision for capital investments. In 1984, voters approved Proposition 46, which allowed school districts to issue general obligation bonds to finance capital projects.[13] Bonds are proposed by the school district board and must be approved by a

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[10] This is consistent with Matsusaka's (1995) conclusion that public spending is lower in states with initiatives.
[11] Statistics in this paragraph are computed from U.S. Department of Education (2007), Table 174, and U.S. Department of Education (1998), Tables 165 and 42.
[12] See Fischel (1989); Shapiro and Sonstelie (1982); and Sonstelie, Brunner, and Ardun (2000) for further details and discussion of California's school finance reforms.
[13] Non-educational public entities (e.g., cities, sanitation districts) can also issue general obligation bonds using a similar procedure. An alternative source of funds is a parcel tax, which also requires voter approval but imposes

local referendum.[14] Initially, a two-thirds vote was required, but beginning in 2001 proposals that adhered to certain restrictions could qualify for a reduced threshold of 55%. Brunner and Reuben (2001) attribute 32% of California school facility spending between 1992-93 and 1998-99 to local bond referenda. The leading alternative source of funds was state transfers.

Authorized bonds are paid off over 20 or 30 years through an increment—typically 0.25 percentage points—to the local property tax rate. Under Proposition 13 assessed home values are based on the purchase price rather than the current market value. As property values in California have risen substantially in recent decades, homeowners with long tenure face low tax shares and recent homebuyers bear disproportionate shares of the burden.

Districts must specify in advance how the bond revenues will be spent. The ballot summary for a representative proposal reads:

> *Shall Alhambra Unified School District repair, upgrade and equip all local schools, improve student safety conditions, upgrade electrical wiring for technology, install fire safety, energy efficient heating/cooling systems, emergency lighting, fire doors, replace outdated plumbing/sewer systems, repair leaky rundown roofs/bathrooms, decaying walls, drainage systems, repair, construct, acquire, equip classrooms, libraries, science labs, sites and facilities, by issuing $85,000,000 of bonds at legal rates, requiring annual audits, citizen oversight, and no money for administrators' salaries?* (Institute for Social Research 2006)

Anecdotally, bonds are frequently used to build new permanent classrooms that replace temporary buildings (e.g., Sebastian 2006), although repair, maintenance, and modernization are common uses as well.

629 of the 1,035 school districts in California voted on at least one bond measure between 1987 and 2006. The average number of measures considered (conditional on any) was slightly more than two.[15] Elections were frequently close, with 35% decided by less than five percent of the vote. Table 1 shows the number of measures proposed and passed in each year, along with the average bond amount (in $1000 per pupil), the distribution of required vote shares for bond approval, and the mean and standard deviation of observed vote shares.

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fewer restrictions (Orrick, Herrington & Sutcliffe, LLP, 2004). These are comparatively rare. Although we focus on general obligation bonds in the analysis below, we present some specifications that incorporate parcel taxes as well.
[14] Balsdon, Brunner, and Rueben (2003) model the board's decision to propose a bond issue.
[15] These data come from the California Education Data Partnership. More details are provided in Section V. 264 districts had exactly one measure on the ballot between 1987 and 2006, while 189 districts had 2, 99 districts had 3, 53 districts had 4, and 30 districts had 5 or more measures. The maximum was 10 measures.

III. Theoretical Framework

Education researchers and reformers often cite overcrowded classrooms; poor ventilation, indoor air quality, temperature control, or lighting; inadequate computer hardware or wiring; and broken windows or plumbing as problems that can interfere with student learning. Mitigating these types of environmental conditions may bring substantial gains to student achievement in the short run by reducing distractions and missed school days.[16] It may also benefit teachers by improving morale and reducing absenteeism and turnover, with indirect impacts on student achievement (Buckley, Schneider, and Shang, 2005).

However, student achievement is not the only potential benefit of improved infrastructure. Capital investments may also lead to enhancements in student safety, athletic and art training, the aesthetic appeal of the campus, or any number of other non-academic outputs. A full evaluation of investment decisions must capture all of these potential impacts. But rather than investigating each outcome separately, one can use parents' location decisions to identify their revealed preferences over spending levels. Any shift in the desirability of a district—along either academic or non-academic dimensions—will be reflected in equilibrium housing prices.

Bond-funded investments are accompanied by an increased tax burden with an approximately equal present value. Thus, if funds are misspent or simply yield smaller benefits than the consumption foregone due to increased taxes, bond authorization will make a district less attractive, leading to reduced pre-tax housing prices. By contrast, if the effect on school output is valued more than the foregone consumption, home prices will rise when bonds are passed. It can be shown that the efficient choice of spending levels will equate the aggregate marginal utilities of consumption and school spending (Samuelson, 1954), so positive effects on prices indicate inefficiently low spending.

We sketch a simple model to support this intuition.[17] We assume that the utility of family i living in district j depends on local school output A_j, exogenous amenities X_j, and other consumption c_i: $u_{ij} = U_i(A_j, X_j, c_i)$. The family has income w_i and faces the budget constraint $c_i \leq$

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[16] See Earthman (2002), Mendell and Heath (2004), and Schneider (2002) for reviews.
[17] The basic model is due to Tiebout (1956). We draw heavily on Brueckner (1979, 1982, 1983). Barrow and Rouse (2004) provide an accessible discussion. Epple, Filimon, and Romer (1984, 1993) present more involved models. Bergstrom and Goodman (1973) examine the relationship between spending levels and district income.

$w_i - r_j - p_j$, where r_j represents taxes and p_j is the (rental) price of local housing. Service quality depends on tax revenues, $A_j = A(r_j)$; if districts use funds inefficiently, $A'(r)$ will be low.[18]

We consider first the household location decision with predetermined spending. A family chooses the community that provides the highest utility, taking into account housing prices, taxes, and service quality. Writing the family's indirect utility in district j as $U(A(r_j), X_j, w_i - r_j - p_j)$, the implicit function theorem yields the family's bid for housing in district j as a function of amenities and taxes, $g_{ij} = g_i(X_j, r_j)$.[19] Holding prices, amenities, and tax rates in all other communities in the family's choice set constant, community j will provide higher utility than any alternative community if $p_j < g_{ij}$.

The family's willingness to pay (WTP) for a marginal increase in r_j in its chosen district is $\partial g_i(X_j, r_j)/\partial r_j$. It can be shown that

(1) $\partial g_i(X_j, r_j)/\partial r_j = (\partial U/\partial c)^{-1}[A'(r_j) * (\partial U/\partial A)] - 1$.

This WTP is positive if the marginal product of school revenues multiplied by the marginal utility of school outputs (in brackets) exceeds the marginal utility of consumption. Ignoring momentarily the effect of spending on local housing prices, the family's optimal tax and service level satisfies $\partial g_i(X_j, r_j)/\partial r_j = 0$. If $\partial g_i(X_j, r_j)/\partial r_j > 0$, the district's spending is below the family's preferred level; if $\partial g_i(X_j, r_j)/\partial r_j < 0$, the family would prefer that taxes and services be cut.

In equilibrium, the price of housing in district j, $p_j = p^*(X_j, r_j)$, equals the bid of the marginal consumer, who must be indifferent between this district and another alternative. Thus, p_j will respond positively to increases in r_j if and only if the prior level of school spending was below the preferred level of the marginal resident.

Tax changes are not exogenous, but depend on election outcomes. Many models of voting focus on landlords who are not local residents. Because they do not directly consume services, these absentee landlords will vote to maximize net-of-tax housing rents. At the maximum, the first-order effect of an exogenous change in tax rates will be zero for net rents and one for gross rents. Sale prices of rental units should reflect the present-discounted value of net rents, so will be invariant to the tax rate change.

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[18] If residents do not trust district management, A'(r) may be larger for restricted bond funds – which require that the projects that will be funded are specified before the bond referendum – than it would be for other forms of revenue. If so, bonds will have larger price effects than would unrestricted tax increases.

[19] $g_i()$ is defined implicitly by $U(A(r_j), X_j, w_i - r_j - g_i(X_j, r_j)) = \max_{k \neq j} U(A(r_k), X_k, w_i - r_k - p_k)$.

But absentee landlords do not vote. Residents do, and many will not vote to maximize the rental values of their homes. Most obviously, any renter who values spending less than the marginal resident – for whom $\partial g_i(X_j, r_j)/\partial r_j < \partial p^*(X_j, r_j)/\partial r_j$ – will vote against a proposed spending increase, as the utility she will derive from higher spending will not compensate her for the increased rent that she will pay. Similarly, a homeowner who does not wish to move will vote on the basis of her own bid-rent, not the community's price function, and will oppose a tax increase if $\partial g_i(X_j, r_j)/\partial r_j < 0$. This group may be particularly important in California: under Proposition 13, "empty nesters" face incentives to remain in their houses after their children are grown (Ferreira, 2007). These families derive little direct utility from school spending and, if they do not plan to move, will not be motivated by the prospect of increased home values. Thus, in general we should expect that even price-increasing proposals will attract some opposition, and therefore that $\partial p^*(X_j, r_j)/\partial r_j$ may be larger than zero even in political equilibrium.[20]

A final issue concerns timing. Capital projects take time to plan, initiate, and carry out, so bonds issued today will take several years to translate into improved capital services. Direct measures of school outputs will reflect the effects of bond passage only with long lags. House prices reflect the present discounted value of all future services less all future taxes, so should rise or fall as soon as the outcome of the election is known. This may happen well before the election if the outcome is easy to predict, but when the election is close there is likely important information revealed on Election Day. Price effects may therefore be immediate. However, if house prices are sticky or homebuyers have imperfect information, it may take a few years for prices to fully reflect the impact of bond passage. We are thus interested in measuring the full sequence of dynamic treatment effects on each of our outcomes.

IV. Empirical Research Design

In this section we describe our dynamic regression discontinuity design in six steps. First, we show in a cross-sectional framework how a regression discontinuity (RD) design approximates a randomized experiment. Second, we extend the framework to incorporate the presence of multiple elections in the same district. We also discuss two interpretations of the causal effect of measure passage, corresponding to the intent-to-treat (ITT) and treatment-on-the-

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[20] Exogenous increases in r may increase prices even if the pivotal voter's WTP is one (as must be the case for close elections in the median voter model) or negative (as in Romer and Rosenthal's [1979, 1982] agenda-setter model), if marginal homebuyers' preferences diverge sufficiently from those of inframarginal residents.

treated (TOT) effects that arise in experiments with imperfect compliance. Third, we describe our implementation of the RD estimator for the ITT, which exploits panel data to enhance precision. Fourth, we describe our two new estimators for the dynamic (TOT) treatment effects of bond authorization. Fifth, we discuss complications that arise in analyses of housing prices. Finally, we discuss how estimates of the effect of bond passage can be interpreted in terms of the marginal willingness to pay for \$1 of school facilities investment.

A. Regression Discontinuity in Cross Section

Suppose that district j considers a bond measure and that this proposal receives vote share v_j (relative to the required threshold v^*). Let $b_j = 1(v_j \geq v^*)$ be an indicator for authorization of the bond. Suppressing time-related considerations, we can write some outcome y_j (capital spending or the price of local houses at some later date, for example) as

$$(2) \qquad y_j = \kappa + b_j \, \theta + u_j,$$

where θ is the causal effect of bond authorization and u_j represents all other determinants of the outcome (with $E[u_j]=0$).[21]

In general, the election outcome may be correlated with other district characteristics that influence spending, so $E[\, u_j \, b_j \,] \neq 0$. If so, a simple regression of y_j on b_j will yield a biased estimate of θ. However, as Lee (2001, 2008) points out, as long as there is some unpredictable random component of the vote, a narrowly-decided election approximates a randomized experiment. In other words, the correlation between the election outcome and unobserved district characteristics can be kept arbitrarily close to zero by focusing on sufficiently close elections. One can therefore identify the causal effect of measure passage by comparing districts that barely passed a measure (the "treatment group") with others that barely rejected a bond measure (the "control group").

We focus on an implementation of the RD strategy that retains all of the data in the sample but absorbs variation coming from non-close elections using flexible controls for the vote share.[22] Assuming that $E[u_j \mid v_j]$, the conditional expectation of the unobserved determinants of y given the realized vote share, is continuous, we can approximate it by a polynomial of order g

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[21] When y_j is district spending, one might expect that θ would equal the size of the authorized bond. But this need not be so, as districts where the proposal fails may make up some of the shortfall via other means. In practice, however, the appropriately estimated θ turns out to be quite close to the average proposed bond amount.

[22] For a detailed comparison of this approach with an approach that uses data only from close elections, see Imbens and Lemieux (2007).

with coefficients γ_u, $P_g(v_j, \gamma_u)$, and the approximation will become arbitrarily accurate as $g \to \infty$. Under this assumption we can rewrite (2) as:

$$(3) \qquad y_j = \kappa + b_j \, \theta + P_g(v_j, \gamma_u) + u_j{}',$$

where $u_j{}' \equiv u_j - P_g(v_j, \gamma_u) = (u_j - E[u_j \mid v_j]) + (E[u_j \mid v_j] - P_g(v_j, \gamma_u))$ is asymptotically uncorrelated with v_j (and therefore with b_j). A regression of realized outcomes on the bond approval indicator, controlling for a flexible polynomial in the vote share, thus consistently estimates θ.[23]

B. Panel Data and Multiple Treatments

We now extend the framework to allow for multiple bond measures in the same district. We redefine b_{jt} to equal one if district j approved a measure in calendar year t and zero otherwise (i.e., if there was no election in year t or if a proposed bond was rejected). We assume that the partial effect of a bond authorization in one year on outcomes in some later year (holding bond issues in all intermediate years constant) depends only on the elapsed time. We can then write spending in any year t as a function of the full history of bond authorizations:

$$(4) \qquad y_{jt} = \sum_{\tau=0}^{\infty} b_{j,t-\tau}\theta_\tau + u_{jt}.$$

There are two sensible definitions of the causal effect of a measure passage in t-τ on spending in year t, corresponding to different potential interventions. First, one can examine the effect of exogenously authorizing a bond issue in district j in year t-τ and prohibiting the district from authorizing bonds in any subsequent year. By equation (4), this is θ_τ, since we are controlling for all other bond measures. It is commonly known as the effect of the "treatment on the treated," or TOT, and we hereafter refer to it as θ_τ^{TOT}. By isolating the impact of \$1 of debt authorization with no subsequent changes in the district's budget constraint, estimates of the TOT effect on house prices will allow us to examine homebuyers' willingness to pay for additional school spending.

Alternatively, one can focus on the impact of exogenously authorizing a bond issue and thereafter leaving the district to make subsequent bond issuance decisions as its voters wish. This interpretation, known as the "intent-to-treat" (ITT) effect, incorporates effects of $b_{j,t-\tau}$ operating through the intermediate variables $\{b_{j,t-\tau+1}, \ldots, b_{jt}\}$. It is arguably the effect of interest for evaluations of a particular bond proposal. The ITT effect of $b_{j,t-\tau}$ on y_{jt} is

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[23] If there is heterogeneity in θ across districts, the RD estimator identifies the average of θ_j among districts with close elections (Angrist and Imbens 1994).

$$(5) \quad \theta_\tau^{ITT} \equiv \frac{dy_{jt}}{db_{j,t-\tau}} = \frac{\partial y_{jt}}{\partial b_{j,t-\tau}} + \sum_{h=1}^{\tau} \left(\frac{\partial y_{jt}}{\partial b_{j,t-\tau+h}} * \frac{db_{j,t-\tau+h}}{db_{j,t-\tau}} \right)$$

$$= \theta_\tau^{TOT} + \sum_{h=1}^{\tau} \theta_{\tau-h}^{TOT} \pi_h$$

where $\pi_h \equiv db_{jt-\tau+h}/db_{j,t-\tau}$ represents the effect of authorizing the first bond on the probability of authorizing another bond measure h years later. We show in Section VI that districts that approve a bond are less likely to propose and approve other bonds in the next few years: $\pi_h < 0$ for $h \leq 4$ and $\pi_h = 0$ for $h > 4$. Assuming that $\theta_{\tau-h}^{TOT} \geq 0$ for all h, this implies that $\theta_\tau^{ITT} < \theta_\tau^{TOT}$.

C. Intent-to-Treat Effects

We begin by describing how the RD strategy can be used to identify the ITT effects, then return to the TOT effects in subsection D. Recall that the ITT corresponds to the effect of experimentally manipulating one election outcome without controlling the district's behavior in subsequent years. The non-experimental RD analogue is straightforward: we simply examine outcomes in later years for districts that pass or fail a specified initial election, controlling flexibly for the vote share in that election but not for any subsequent votes or other variables.

It is most natural to re-orient our time index around the focal election. Thus, consider a district j that had an election in year t. We can write the district's outcome τ years later as

$$(6) \quad y_{j,t+\tau} = b_{jt} \theta_\tau^{ITT} + P_g(v_{jt}, \gamma_\tau) + u_{j,t+\tau}',$$

where $P_g(v_{jt}, \gamma_\tau)$ is a polynomial in v_{jt} with coefficients γ_τ, and $u_{j,t+\tau}' = u_{j,t+\tau} - P_g(v_{jt}, \gamma_\tau)$. By the logic in subsection A, $u_{j,t+\tau}'$ asymptotes to $u_{j,t+\tau} - E[u_{j,t+\tau}|v_j]$, which is uncorrelated with b_{jt}.

In practice, equation (6) is inefficient. This is because the error term $u_{j,t+\tau}'$ has an important component that varies at the district level but is fixed within districts over time. Even though the RD strategy ensures that this component is uncorrelated with b_{jt} conditional on the vote share controls, it nevertheless reduces precision. More precise estimates of the θ_τ^{ITT} parameters can be obtained by pooling data from multiple τ (including $\tau < 0$, corresponding to periods preceding the focal election) and including controls to absorb district-level heterogeneity.

To implement this, we begin by identifying each (j, t) combination with an election. We then select observations from district j in years $t-2$ through $t+6$. Where a district has multiple elections in close succession, the same calendar year observation is used more than once. For example, if a district had elections in 1995 and 1997, the $[t-2, t+6]$ windows are [1993, 2001]

and [1995, 2003] respectively, and the 1995-2001 observations are included in each.
Observations in the resulting data set are uniquely identified by the district, j, the date of the
focal election, t, and the number of years elapsed between the focal election and the time at
which the outcome was measured, τ. We use this sample to estimate the following regression:

$$(7) \qquad y_{jt\tau} = b_{jt}\, \theta_\tau^{ITT} + P_g(\nu_{jt},\, \gamma_\tau) + \alpha_\tau + \kappa_t + \lambda_{jt} + e_{jt\tau}.$$

Here, α_τ, κ_t, and λ_{jt} represent fixed effects for years relative to the election, for calendar years,
and for focal elections, respectively. Note that the λ_{jt} effects absorb any across-district variation.
$P_g(\nu_{jt}, \gamma_\tau)$ is a polynomial in the focal election vote share. Both the γ_τ and θ_τ^{ITT} coefficients are
allowed to vary freely with τ for $\tau \geq 0$, but are constrained to zero for $\tau < 0$. We cluster standard
errors by district (i.e., by j) to account for dependence created by the use of multiple (j, t)
observations in the sample or by serial correlation in the $e_{jt\tau}$.[24]

D. Treatment-on-the-Treated Effects

In traditional experimental designs with a single opportunity for randomization and
imperfect compliance, the TOT is readily identified by using the random treatment assignment as
an instrument for the actual treatment status. The "fuzzy" RD design (Hahn, Todd, and Van der
Klauuw 2001) uses the same strategy: even when some subjects with $\nu_{jt} < \nu^*$ are treated and/or
subjects with $\nu_{jt} > \nu^*$ are untreated, the discontinuous indicator for measure passage, b_{jt}, can be
used as an instrument for the realized treatment status.

In our study, each election is a sharp RD, but the possibility of dynamics in the b_{jt}
variable introduces fuzziness: a district in the "control" group – one where the focal election
narrowly failed – might approve a bond in a subsequent election and therefore be treated.
However, the usual fuzzy RD strategy cannot be applied. With dynamic treatment effects, a
bond authorization in year $t+h$ does not have the same effect on the outcome in $t+\tau$ as the initial
authorization in t would have.

Our "recursive" estimator for the TOT effects extends the fuzzy RD design to the case of
dynamic treatment effects. We assume (as has been implicit in our notation thus far) that the
TOT effects of bond authorization on later authorizations and outcomes depend only on the time
elapsed since the focal treatment (τ) and not on the time at which the treatment occurred or on

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[24] In the empirical application we also include an indicator for a measure with a 55% (as opposed to 2/3) threshold.

the treatment history. That is, while $\partial y_{j,t+\tau}/\partial b_{jt}$ and $\partial b_{j,t+\tau}/\partial b_{jt}$ depend on τ, they do not depend on t or on $\{b_{j1}, \ldots, b_{j,t-1}, b_{j,t+1}, \ldots, b_{j,t+\tau-1}\}$.

Recall that equation (5) related θ_τ^{ITT} to $\{\theta_h^{TOT}, \pi_h\}_{h=1,\ldots,\tau}$. We can simply invert that equation to obtain recursive formulas for the TOT effects in terms of the ITTs and the πs:

(8) $\quad \theta_0^{TOT} = \theta_0^{ITT}$;

(9) $\quad \theta_1^{TOT} = \theta_1^{ITT} - \pi_1 \theta_0^{TOT}$;

(10) $\quad \theta_2^{TOT} = \theta_2^{ITT} - \pi_1 \theta_1^{TOT} - \pi_2 \theta_0^{TOT}$; and, in general,

(11) $\quad \theta_\tau^{TOT} = \theta_\tau^{ITT} - \sum_{h=1}^{\tau} \pi_h \theta_{\tau-h}^{TOT}$.

Our recursive estimator thus proceeds in two steps. First, we estimate the coefficients θ_τ^{ITT} and π_τ using the methods discussed in subsection C.[25] Second, we solve for the dynamic TOT effects using the recursive equation (11). Standard errors are obtained by the delta method.

This recursive strategy has an important drawback. Equation (11) indicates that θ_τ^{TOT} depends on θ_τ^{ITT} as well as on $\theta_{\tau-h}^{TOT}$ and π_h for all $1 \le h \le \tau$. As a result, the estimates become extremely imprecise at long lags. Our second TOT estimator obtains greater precision by applying additional restrictions on the election dynamics. We return to equation (4), which specifies the outcome in year t as depending on the full history of bond authorizations in the district. An OLS estimate of (4) would yield biased estimates of the TOT effects, as bond authorizations (both past and current) are likely to be correlated with other determinants of outcomes. Under the standard RD assumption that measure passage is as good as randomly assigned conditional on a smooth function of the measure vote share, however, this endogeneity can be absorbed via the inclusion of a flexible polynomial in that vote share. Thus, to bring the RD methodology to the "structural" equation (4), we augment each of the lagged bond authorization indicators $b_{j,t-\tau}$ with an indicator for the presence of a measure on the ballot in year $t-\tau$, $m_{j,t-\tau}$, and a polynomial in the vote share, $P_g(v_{j,t-\tau}, \gamma_\tau)$.[26] Both the $m_{j,t-\tau}$ coefficient and the

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[25] Note that π_τ is defined as an ITT, so can be estimated via equation (7) by simply redefining $y_{jt\tau}$ to equal $b_{j,t+\tau}$. We modify the approach discussed in subsection C in two ways. First, we use all available relative years: τ ranges from -19 to +18 rather than just from -2 to +6. This permits us to estimate the TOT over a longer post-election period. Second, to obtain the covariance between the θ^{ITT} and π parameters we stack observations (for each election and each relative year τ) on the outcomes $y_{jt\tau}$ and $b_{jt\tau}$ and fully interact (7) with an indicator for the outcome type. As before, we cluster standard errors at the district level.

[26] We set $v_{j,t-\tau} = 0$ if district j did not hold an election in year $t-\tau$.

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polynomial coefficients are allowed to vary freely with τ (for $\tau \geq 0$). We also add fixed effects for each district and for each calendar year. The estimating equation then becomes:

$$(12) \qquad y_{jt} = \sum_{\tau=0}^{\bar{\tau}} (b_{j,t-\tau} \theta_{\tau}^{TOT} + m_{j,t-\tau} \alpha_{\tau} + P_g(v_{j,t-\tau}, \gamma_{\tau})) + \lambda_J + \kappa_t + u_{jt} .$$

We estimate this on a conventional panel of school districts over calendar years, with each observation used exactly once. Standard errors are clustered on the school district.

It is instructive to compare this "one-step" estimator with the recursive approach. Where the recursive strategy extends experimental techniques to accommodate dynamic treatment effects, the one-step estimator imports the RD strategy for isolating exogenous variation into an observational analysis. With the inclusion of controls for the election and vote share history in (12), the θ_{τ}^{TOT} coefficients are identified from the contrast between districts where an election in $t-\tau$ narrowly passed and those where the election narrowly failed but the sequence of prior and subsequent elections, votes, and bond authorizations is similar.

An important limitation to the one-step estimator is that it involves controlling for intermediate outcomes. The RD design does not permit a causal interpretation of the α_{τ} or γ_{τ} coefficients in (12). If the outcome of an initial election influences m or v in subsequent years, biases in their coefficients relative to the true causal effects will lead to bias in the estimated bond authorization effects θ_{τ}^{TOT}. For example, the one-step estimator will be inconsistent if the outcome of an initial election affects the composition of the electorate in subsequent elections. We will see below that the one-step estimator yields quite similar estimates to those obtained from the recursive estimator, which does not suffer from the intermediate outcomes problem. Moreover, the one-step estimates are substantially more precise.

E. Forward-Looking Housing Prices

We have not yet specified the "outcome" variable. Below, we present estimates for school district spending, student test scores, and district demographics, but our primary dependent variable is the average sale price of homes in the district. This outcome adds some complexity, as prices depend in part on expectations of future events. If the discount rate is r, standard no-arbitrage conditions ensure that the discounted TOT effect of a bond issue that will be authorized (with probability one) in period $t+h$ on prices in t is tied to the TOT effect of an

authorization in period t: $\theta_h{}^{TOT} = \theta_0{}^{TOT}(1+r)^{-h}$. Moreover, uncertainty about future election outcomes is priced at the expected value. Thus, we can write house prices in year t as

$$(13) \quad y_{jt} = \sum_{\tau=0}^{\infty} b_{j,t-\tau}\theta_{\tau}^{TOT} + \sum_{h=1}^{\infty} E_t[b_{j,t+h}]\theta_0^{TOT}(1+r)^{-h} + u_{jt}$$

where $E_t[]$ is the expectation as of date t and the second summation reflects the influence of the expected future path of the b_{jt} series.

We assume homebuyers cannot predict future election outcomes any better than can we. With this assumption, $dE_t[b_{j,t+h}] / db_{j,t-\tau} = \pi_{h+\tau}$. The ITT effect of $b_{j,t-\tau}$ on y_{jt} then becomes:

$$(14) \quad \theta_{\tau}^{ITT} = \theta_{\tau}^{TOT} + \sum_{h=1}^{\tau} \pi_h\theta_{\tau-h}^{TOT} + \sum_{h=1}^{\infty} \pi_{\tau+h}\theta_0^{TOT}(1+r)^{-h}.$$

Again, the final summation reflects the portion of the effect of the t-τ treatment that operates through its influence on the expectation of post-t treatments. Our recursive estimator is readily modified for this case. We present these "forward-looking" housing price estimates in addition to the recursive and one-step estimates below.

F. Willingness to Pay for School Spending

We have described methods for identifying the causal effect of authorizing a bond in one year on house prices in future years. Our estimators identify the bond authorization effect based on the discontinuity in the relationship between later house prices and the election vote share at the threshold required for passage. They therefore are local to close elections, and can be interpreted as the average effect of the bonds for which the elections are close. In our sample (discussed below), the average proposal that passed by less than two percentage points was for a bond issue of $6,309 per pupil, so the effect per dollar of bonds authorized in τ is simply $\tilde{\theta}_{\tau}^{TOT} \equiv \theta_{\tau}^{TOT} / 6309$.

To convert this into an estimate of the willingness to pay for additional school spending, it is useful to think of a bond authorization as a bundle of several "programs." First, authorization to issue $1 in bonds per pupil means that spending in future years can rise by an amount equal in present value to $1. Second, property tax rates are raised in each of the next 30 years by an amount sufficient to pay the bond principal and interest. Assuming that the district borrows and saves at the residents' discount rate, the present value of the increment to future taxes is also $1. As homebuyers are committing to the purchase price of the house plus the

stream of future property taxes, their implied willingness to pay for the additional spending is $1 + $\tilde{\theta}_0^{TOT}$. The WTP will be greater than one if the marginal homebuyer values $1 in school spending more than $1 in other consumption.

This simplified presentation ignores many complexities: sticky house prices; the income tax treatment of property taxes, mortgage interest, and municipal bond interest; the ratio of pupils to homes; and heterogeneity in tax shares within districts can all lead $1 + $\tilde{\theta}_0^{TOT}$ to diverge from the marginal willingness to pay for $1 in school spending. We discuss a simple willingness to pay calculation in Section VII, then add the various complexities in the online appendix.

V. Data

We obtained bond data from a database maintained by the California Education Data Partnership. For each proposed bond, the data include the amount, intended purpose, vote share, required vote share for passage, and voter turnout. Our sample includes all general obligation bond measures sponsored by school districts between 1987 and 2006. We merged these to annual district-level enrollment and financial data from the Common Core of Data (CCD).

We obtained calendar-year averages, at the census block group level, of the sale price, square footage, and lot size of transacted homes from a proprietary database compiled from public records by the real estate services firm Dataquick. The underlying data describe all housing transactions in California from 1988 to 2005.[27] We used GIS mapping software to assign census block groups to school districts.

If the mix of houses that transact changes from one year to the next (for example, one might expect sales of houses that can accommodate families with children to react differently to school spending than do smaller houses), this will bias our house price measure relative to the quantity of interest, the average market value of houses in the district. We take two steps to minimize this bias. First, when we average block groups to the district level, we weight them by their year-2000 populations rather than by the number of transactions. This holds constant the *location* of transactions within the district. Second, we include in our models of housing prices controls for the average square footage and lot size of transacted homes and for the number of

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[27] The majority of housing transactions happen from May through August. We assign measures occurring after October to housing data from the following calendar year. This means that a few of the housing transactions assigned to year 0 in fact occurred before the election. To merge measures to academic year data from the CCD, we treat any measure between May 2005 and April 2006 as occurring during the 2005-06 academic year.

sales to absorb any remaining selection. These adjustments have little effect on the results, and estimates based on unadjusted data are presented in Section VII-C.

We constructed a panel of average student achievement by merging data from several different tests (listed in the online appendix) given in California at various times. We focused on 3rd and 4th graders, for whom the longest panel is available, and standardized the scaled scores each year using school-level means and standard deviations.

Finally, we obtained 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). We treat this measure as characterizing in-migrants to the district, though we are unable to exclude intra-district movers from the calculation. Renters are not represented.

The online appendix provides more detail on data and sources. Table 2 presents descriptive statistics. Column 1 shows the means and standard deviations computed over all district-year observations in our data. Columns 2 and 3 divide the sample between districts that proposed at least one bond between 1987 and 2006 and those that did not. Districts that proposed bonds are larger and have higher test scores, incomes, and housing prices, but smaller lot sizes.

Columns 4 and 5 focus on districts that approved and rejected school bonds, using data from the year just before the bond election, while Column 6 presents differences between them. Districts that passed measures had 25% higher enrollment, $206 higher current instructional spending, and $349 higher total spending. Districts that passed measures also had much higher incomes and house prices, as well as more housing transactions. However, these districts also had homes with smaller lots.

VI. Evaluating the Bond Referendum Quasi-Experiment

Our empirical strategy is to use close elections to approximate a true experiment. This requires that bond authorization be as good as randomly assigned, conditional on having a close election. In this section, we consider tests of this assumption. We also demonstrate that bond authorization in fact leads to increased capital spending in subsequent years.

A. Balance of Treatment and Control Groups

We examine three diagnostics for the validity of the RD quasi-experiment, based on the distribution of vote shares, pre-election differences in mean characteristics, and differences in

pre-election trends. Tests of the balance of *outcome* variable means and trends before the election are possible only because of the panel structure of our data, and provide particularly convincing evidence regarding the approximate randomness of measure passage.

Figure 1 shows histograms of bond measure vote shares, separately for measures that required two-thirds and 55% of the vote for approval. Discontinuous changes in density around the threshold can be an indication of endogenous sorting around this threshold, which would violate the RD assumptions (McCrary 2008). We see no evidence of such changes.

Columns 1-4 of Table 3 present regressions of fiscal, housing, and academic variables measured in the year before a bond referendum, on an indicator for whether the bond proposal was approved. The specifications in Columns 1-2 are estimated from a sample that includes only observations from the year before the election. The first column controls for year effects and the required threshold. Like Table 2, it reveals large pre-measure differences in several outcomes. The second column adds a cubic polynomial in the measure vote share. Comparing districts that barely passed a bond with districts that barely failed eliminates the significant estimates, shrinking the point estimates substantially.

Columns 3-4 turn to panels pooling observations from two years before through six years after the election, as discussed in Section IV. We generalize equation (7) by freeing the coefficients corresponding to outcomes in the year of and year before the election, and report in the table the "effect" of bond passage in the year before the election, θ_{-1}. Column 3 reports estimates from a specification without measure fixed effects (λ_{jt} in (7)), while Column 4 includes them. Pooling the data does not substantially change the estimates. The specification in Column 4, however, has much smaller (in absolute value) point estimates and standard errors, particularly for housing prices and test scores. The fixed effects evidently absorb a great deal of variation in these outcomes that is unrelated to election results.

Columns 5-7 in Table 3 repeat our three first specifications, taking as the dependent variable the *change* in each outcome between year *t-2* and *t-1*. Although the model without controls shows some differences in trends between districts that pass and fail measures, these are eliminated when we include controls for the vote share. Overall, there seems to be little cause for concern about the approximate randomness of the measure passage indicator in our RD framework. Once we control for a cubic in the measure vote share, measure passage is not

significantly correlated with pre-treatment trends of any of the outcomes we examine.[28] Further, in similar specifications (not reported in Table 3), we find no evidence of "effects" on sales volume, housing characteristics, the income of homebuyers, or other covariates.[29]

B. Intent-to-Treat Effects on School Spending

Figure 2 presents graphical analyses of mean district spending per pupil by the margin of victory or defeat, in the year before the election and three years after it. We show average outcomes (controlling for calendar year effects) in two-percentage-point bins defined by the vote share relative to the threshold.[30] Thus, the leftmost point represents measures that failed by between 8 and 10 percentage points, the next measures that failed by 6 to 8 points, and so on. The left panel shows total district spending, while the right panel shows capital outlays. As expected, there is no sign of a discontinuity in either total or capital spending in the year before the election. By contrast, in the third year after the election districts where the measure just passed spend about $1,000 more per pupil, essentially all of it in the capital account.[31]

Panel A of Table 4 presents estimates of the intent-to-treat effect of bond passage on district spending and on state and federal transfers (all in per-pupil terms) over the six years following the election, using equation (7).[32] Bond passage has no significant effect on any of the fiscal variables in the first year. We see large increases in capital expenditures in years 2, 3, and 4. These increases fade by the fifth year following the election. There is no indication of any effect on current spending in any year, and confidence intervals rule out effects amounting to

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[28] The estimated effect of bond authorization on house price changes in Columns 5-7 is reasonably large, though not significant when the vote share is controlled. If bond passage were indeed correlated with pre-existing trends in district house prices, even after controlling flexibly for the vote share, this could confound our estimates of the effect of passage on post-election prices. To investigate this issue further, we have estimated a variety of additional specifications, reported in the online appendix. The point estimates here seem to reflect a transitory blip in housing prices in year *t-1* rather than any long run trend.

[29] We have also examined other election outcomes for evidence that bond authorization is non-randomly assigned in close elections. Bond authorizations are not associated with the number of county and municipal measures that pass in the same year or in previous years nor with the probability that an incumbent mayor is re-elected.

[30] The bin corresponding to measures that failed by less than two percentage points is the category excluded from the regression used to control for year effects, so estimates may be interpreted as differences relative to that bin. Results are robust to exclusion of the year controls.

[31] It is possible that districts use bond revenues for operating expenses but report these expenditures in their capital accounts. The CCD data are not used for financial oversight, so districts have no obvious incentive to misreport.

[32] We make one modification to equation (7): We constrain the $\tau = 0$ coefficients to zero. It is not plausible that bond passage can have effects on that year's district budget, which will typically have been set well before the election. In any case, results are insensitive to removing this constraint.

more than about $100 per pupil in every year. Essentially all of the funds made available by the bond authorization are kept in the capital account.

One might be concerned that bond issues crowd out other types of educational revenues. Table 4 indicates that there is no meaningful crowding out of state or federal transfers (indeed, most of the point estimates are positive). We have also examined whether bond authorization crowds out donations to local education foundations, which often provide cash or in-kind transfers to California schools (Brunner and Sonstelie 1997; Brunner and Imazeki 2005). We find no evidence of such an effect.[33]

C. School Bond Dynamics and TOT Effects on Spending

School districts where an initial measure fails are more likely to pass a subsequent measure than districts where the initial measure passes. Figure 3 plots estimates of the π_τ coefficients, the ITT effect of measure passage in year t on the probability of passing a measure in year $t+\tau$. These are estimated via equation (7), using $b_{jt\tau}$ as the dependent variable. There are negative effects in each of the first four years, but there is no sign of any effect thereafter. The cumulative effect after bond passage is around -0.6, indicating that a close loss in an initial election reduces the expected total number of bonds ever passed by about 0.4.

As discussed in Section IV, the dynamics in treatment assignment imply that the ITT effects of bond authorization on spending understate the true TOT effects. Panel B of Table 4 presents estimates of the TOT effects from our recursive and one-step estimators. The spending effects are larger and more persistent than in Panel A, but there is still no indication that current spending or intergovernmental transfers respond to bond passage. In particular, the effects on current spending are tightly estimated zeros in every year.

Figure 4 presents estimates of the recursive and one-step dynamic treatment effects of bond passage on district spending over the longer term. Both indicate that effects on spending are exhausted by year 6. The one-step estimator indicates somewhat larger effects than the recursive estimator, but the differences are small. As expected, it also yields substantially smaller confidence intervals, particularly at long lags. When we discount all of the estimated effects from the one-step estimator back to the date of the election, using a discount rate of 7.33% as in Barrow and Rouse (2004), the effect of authorizing a bond is to increase the present

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[33] A regression of total foundation revenue per pupil in the district in 2001 on an indicator for having approved a bond proposal before 2001 (controlling for a cubic in the vote share) yields a coefficient of 15 (s.e. 42).

value of future spending by $5,671. This is quite similar to the size of the average bond proposal in close elections, $6,309.

VII. Results

A. Housing Prices

Figure 5 provides a graphical analysis of the impact of bond passage on log housing prices corresponding to the analyses of fiscal outcomes in Figure 2. Two important patterns emerge. First, housing prices in the year before the election are positively correlated with vote shares, indicating that higher priced districts are more likely to pass bond measures with larger margins of victory. Second, in districts where bond measures were approved, housing prices appear to shift upward by 6 or 7 percentage points by the third year after the election relative to the pre-election prices. There is no such shift in districts where bonds failed.

Panel A of Table 5 presents estimates of the effects of bond passage on log housing prices.[34] The first row presents the ITT analysis, using equation (7). House prices increase by 2.1% in the year of bond passage, though this is not significantly different from zero. The estimated effects rise slightly thereafter, reaching 5.8% and becoming significant three years after the election. Point estimates fade somewhat thereafter and cease to be significant.

The next rows show estimates of the TOT effects from our two estimators. As expected, these are somewhat larger and are uniformly significant after year 0. The estimates indicate that the TOT effect of bond approval in year t is to increase average prices by 2.8-3.0% that year, 3.6-4.1% in year $t+1$, 4.2-8.6% in years $t+2$ through $t+5$, and 6.7-10.1% in $t+6$. Figure 6 plots the coefficients and confidence intervals from the two dynamic specifications, showing estimates out to year 15. The recursive estimator shows growing effects through almost the entire period, while the one-step estimator yields a flatter profile. Confidence intervals are wide, particularly for the recursive estimator in later periods, and a zero effect is typically at or near the lower bound of these intervals.[35]

As discussed in Section IV, the TOT estimators assume that house prices are unaffected by the likelihood of a future bond authorization. To relax this assumption, we estimate a

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[34] We augment each of our house price specifications with controls for the average characteristics of transacted homes. In contrast to the analysis in Table 4, we allow for bond effects in the year of the election, as housing markets may respond immediately to the election outcome.

[35] We have also estimated models that constrain the TOT to be constant over time. With our one-step estimator, we obtain a point estimate of 4.9% and standard error of 1.7%.

modified version of the recursive estimator that allows for perfectly forward-looking prices, as described in Section IV.E. In this specification, the immediate effect of bond passage is larger and the profile in the first few years is flatter than in our myopic specification. Point estimates in years 0 through 6 are 6.1%, 6.8%, 7.4%, 9.5%, 8.8%, 9.5%, and 10.4%, respectively. These are shown as hollow circles in Figure 6. Since our expectation is that housing markets are neither fully myopic nor subject to perfect no-arbitrage conditions, we think that the true effect is likely to lie between the two sets of estimates.

B. Willingness to Pay for School Facility Investments

As discussed in Section III, a substantial effect of bond passage on prices indicates that the marginal resident's willingness to pay (WTP) for school services exceeds the cost of providing those services and therefore that school capital spending is inefficiently low. It is thus instructive to compute the WTP implied by our estimated price effects. This calculation requires assumptions about interest and discount rates, the speed with which new facilities are brought into service, property tax shares, and the income tax deductibility of property taxes and mortgage interest payments. We outline our baseline calculations here. We describe the details and present alternative calculations in the online appendix.

The average house in districts with close elections (margins of victory or defeat less than two percent) is worth $236,433, so a 3.0% effect on house prices raises the value of the average house by approximately $7,100. The average bond proposal in close elections is about $6,300 per pupil, and there are 2.4 owner-equivalent housing units per pupil. With a typical municipal bond interest rate of 4.6%, this implies a property tax increment of $163 per house per year, for a present discounted value of about $1,950. Thus, the effect of passing a bond on the total cost of owning a home in the district, combining the house price effect with the PDV of future taxes, is approximately $9,050. That homebuyers are willing to pay this implies that their WTP for $1 in per pupil spending is about $1.44 (=9050/6300).[36] When we account (in the appendix) for the deductibility of mortgage interest and property taxes and for the higher tax share borne by new homebuyers, we can drive the WTP estimate as low as $1.13, but never down to $1.

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[36] Our comparison of the cost per home to the bond amount per pupil is appropriate if the marginal homebuyer has one school-aged child. This almost exactly matches the average number of children in owner-occupied California households in the 2000 census who moved in 1999.

As Figure 6 suggests, the WTP is generally higher when we measure the price effects several years after the election. WTPs based on the price effect in year 4, for example, range from $1.31 (one-step estimator, fully accounting for taxes) to $1.89 (recursive estimator, without taxes, using a discount rate of 5.24%). The sensitivity of WTP calculations to the year in which price effects are measured may indicate that capitalization is not immediate.[37] However, the forward-looking price estimates indicate a WTP that is largely invariant to the year in which the price effect is measured and is around $2.

Additional specifications reported in Panel B of Table 5 use an alternative strategy to identify the WTP. We re-estimate the ITT and TOT effects, this time using the dollar value of bonds authorized as the "treatment" variable and the indicator for bond authorization as an instrument for it.[38] These estimates indicate a $1.39 to $1.79 increase in house prices in the year of the election per dollar of bonds issued, with even larger estimates in later years. The implied WTP depends on assumptions about interest rates, tax shares, and income tax deductibility, but under reasonable assumptions exceeds these coefficients by around $0.31.[39]

C. Robustness

Table 6 presents a variety of alternative specifications meant to probe the robustness of the housing price results. To conserve space we report only the estimates from our one-step specification of the TOT effect of bond approval on prices four years later. Row 1 reports the baseline estimates. Rows 2-4 vary the vote share controls: Row 2 includes only a linear control; Row 3 allows for three linear segments, with kinks at 55% and 67% vote shares; and Row 4 allows separate cubic vote share-outcome relationships in the [0, 55%], [55%, 67%], and [67%, 100%] ranges. None of these yields evidence contrary to our main results.

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[37] If capitalization is immediate, a simpler WTP calculation could be based on the ITT effects of bond passage on year-0 housing prices and on the PDV of future spending. Applying this, we estimate a WTP around $1.77. But there are several drawbacks to this method, most notably that we observe a long panel of post-election spending for only the earliest referenda in our sample and that our "immediate" house price measure – average sales prices in the year of the election – may be contaminated by sales occurring before the election.

[38] To implement this, we replace b_{jt} in equations (7) and (12) with the dollar values of the authorized bonds (set to zero if the proposal is rejected) and instrument these with b_{jt}. The π coefficients in the recursion formula (11) are similarly redefined as the effect of authorizing $1 in bonds in year t on the expected value of the bond authorization in $t+\tau$. Note that this incorporates any differences in the size of initial and subsequent proposals. See online appendix for further details.

[39] The $0.31 figure reflects a ratio of 2.4 houses per pupil and a wedge between the district's borrowing rate and residents' discount rates. See the online appendix for more detail. Overall, our WTP estimates are somewhat larger than, but not out of line with, the WTPs implied by estimates of the effect of unrestricted spending on house prices from Bradbury, Mayer, and Case (2001); Hilber and Mayer (2004); and Barrow and Rouse (2004).

Rows 5-7 report estimated discontinuities at locations other than the threshold required for passage. In each of these specifications, we also allow a discontinuity at the actual threshold. In Row 5, we estimate the discontinuity in our outcomes at the counterfactual threshold, 55% when $v^* = 2/3$ and 2/3 when $v^* = 55\%$, while Rows 6 and 7 show estimates for placebo thresholds 10 percentage points above or below the true threshold. Only one of the coefficients measuring discontinuities at counterfactual thresholds is statistically significant, and it has the opposite sign from the estimated effect at the actual threshold.

Our TOT effects hold constant school bond authorizations that are subsequent to an initial authorization, but do not hold constant other forms of district responses, such as parcel taxes. If bond authorization raises the probability that other revenue increases will be approved, our calculations will overstate the WTP for \$1 in additional spending. To examine this, we add indicators for the presence of a parcel tax measure in each relative year τ and for its passage. Row 8 reports the bond passage coefficient when parcel taxes are controlled. The estimated bond effects are unchanged. The parcel tax coefficients (not reported) are statistically indistinguishable both from zero and from the bond coefficients.[40]

Finally, Row 9 reports estimates from a specification for the raw mean of the log prices of homes that transacted, without adjusting for changes in the distribution of transactions across block groups or controlling for home characteristics. The bond effect is again similar to that obtained with our preferred price measure.

D. Academic Achievement

The first two rows of Table 7 report estimates of the effect of bond passage on third grade reading and math scores from our one-step estimator.[41] The effects are small and insignificant for the first several years. This result is expected given the time it takes to execute capital projects; the flow of academic benefits (if any) should not begin for several years. However, the point estimates are generally positive and seem to gradually trend upward, at least for the first few years. This pattern is easier to see in Figure 7, which plots the point estimates and confidence intervals from the math specification. By year six, we see large, marginally significant effects, corresponding to about one-sixth of a school-level standard deviation. Point

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[40] We have also estimated the effect of bond authorization on fiscal outcomes in the district's municipality. Effects on municipal revenues and on a variety of categories of spending are precisely estimated zeros.

[41] Estimates from our other estimators are similar. See Cellini, Ferreira, and Rothstein (2008).

estimates fall back to zero thereafter, and are quite imprecise. Confidence intervals include large positive effects, but we cannot reject zero effects in every year.

The year-six point estimates correspond to effects of roughly 0.067 student-level standard deviations for reading and 0.077 for math. If taken literally, these imply that bond-financed improvements to existing facilities raise achievement by about one third as much as a reduction in class sizes from 22 to 15 students (Krueger 1999).[42] But even this maximal interpretation of the test score results can explain only a small share of the full house price effects seen earlier. Previous research on school quality capitalization (see, e.g., Bayer, Ferreira and McMillan 2007; Black 1999; and Kane, Riegg, and Staiger 2006) has found that a one school-level standard deviation increase in test scores raises housing prices between four and six percent. This implies that our estimated year-six effect on test scores would explain only about one sixth of the effect of bond passage on house prices.[43]

Third grade test scores are a limited measure of academic outcomes. School facilities improvements may have larger effects on achievement in later grades or in other subjects (e.g., science, where lab facilities may be important inputs). Nevertheless, it seems likely that a sizable portion of the hedonic value of school facilities reflects non-academic outputs. Parents may value new playgrounds or athletic facilities for the recreational opportunities they provide, enhanced safety from a remodeled entrance or drop-off area, and improved child health from asbestos abatement and the replacement of drafty temporary classrooms, even if these do not contribute to academic achievement. New facilities may also be aesthetically appealing. Any improvements in these dimensions of school output will lead to housing price effects that exceed those reflected in test scores. The potential relevance of these channels underscores the importance of using housing markets to value school investments.

E. Household Sorting

Recent empirical studies of the capitalization of school quality emphasize the importance of social multiplier effects deriving from preferences for wealthy neighbors (see, e.g., Bayer, Ferreira and McMillan 2004). If wealthy families have higher willingness to pay for school output, passage of a bond may lead to increases in the income of in-migrants to the district,

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[42] We find no evidence that bond passage affects teacher-pupil ratios, or that the results could be attributable to the construction of new, smaller schools. Results are available upon request.

[43] An increase of .185 of a school-level standard deviation in test scores multiplied by an effect of 6 percentage points would yield a price increase of just 1.1 percentage point.

generating follow-on increases in the desirability of the district, in house prices, and in test scores.

In Panel B of Table 7 we report dynamic RD estimates for the impact of bond approval on sales volumes. Volumes would be expected to rise if passage leads to changes in the sort of families that prefer the school district. The estimates show that sales volumes increase by 200-300 units per year. An analysis of log volumes indicates about a 3% increase in sales, though this is not statistically significant.[44] The next two rows show estimated effects on the average size of transacted homes and lots. The estimated effects on home size are precisely estimated zeros. Those for lot size – which is far more heterogeneous – are less precise, but offer no indication of systematic effects.

The remainder of Table 7 examines effects on population composition directly. In Panel C, we report effects on the characteristics of new homebuyers. We find no distinguishable effect on average income or on racial composition. Panel D reports effects on the student population. We find no impact on enrollment, racial composition, or average parental education.[45]

Since we have only limited data on population changes, there may be sorting on characteristics that we do not measure (e.g., tastes for education or the presence of children). Even so, sorting is not likely to account for our full price effect. The literature indicates that social multiplier effects on house prices could be as large as 75% of the direct effect of school quality (Bayer, Ferreira, and McMillan 2004). This would imply that at most 2.5 percentage points of the estimated 6% price effect in year three could be due to sorting, still leaving a large portion that must be attributable to increased school output.

VIII. Conclusion

Infrastructure investments have been and will remain important components of government budgets, yet we have few tools to assess their effectiveness. In this paper we use a "dynamic" regression discontinuity design to estimate the value of school facility investments to parents and homeowners. We identify the effects of capital investments on housing prices by

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[44] Sales volume effects could represent either an increase in the local supply of homes or an increased turnover rate of existing homes. Yearly data on housing construction is unavailable, so these cannot be disentangled.
[45] We have also looked at effects on enrollment in early grades, where composition effects may appear first. We find no change in Kindergarten or 1st grade enrollment. We do find a small, permanent increase in the fraction of white and Asian students in Kindergarten, though not in first grade even several years later. One potential explanation is that some families switched from private to public Kindergartens after bond passage; some bond proposals specify building additional classrooms to permit conversion from half-day to full-day Kindergarten.

comparing districts in which school bond referenda passed or failed by narrow margins. Unlike districts where bond referenda garnered overwhelming voter support or opposition, the set of districts with close votes are likely to be similar to each other in both observable and unobservable characteristics.

Our analysis is complicated by the tendency for districts where proposed bonds are rejected to propose and pass additional measures in future years and by the likely importance of dynamics in the treatment effects. We propose two new "dynamic RD" estimators that accommodate these complexities, bringing the identification power of a traditional RD design into a dynamic panel data context. These estimators are likely to prove useful in other experimental and quasi-experimental settings where there are multiple opportunities for treatment and where the treatment effect dynamics are of interest. In RD settings, the methods require that each treatment opportunity be characterized by a discontinuity in treatment probability as a running variable exceeds a threshold. Repeated referenda are an ideal example: our methods can easily be used to assess the causal effects of other policies decided by elections.

Turning back to our substantive application, our primary analyses are of the impact of passing a bond on house prices. We find treatment effects of six percent or more, and implied valuations of $1.50 or more for $1 in school capital spending. As theory predicts, most of the price effect appears well in advance of the completion of the funded projects. We find some evidence of effects on student achievement several years after bond passage, but no sign of effects on the racial composition or average incomes of district residents. The home price effects presumably reflect the anticipation of increased school output, though it appears that much of the effect derives from dimensions of output (such as safety or aesthetics) that are not captured by test scores.

Our results provide clear evidence that California districts at the margin of passing a bond are spending well below the economically efficient level, with returns to additional spending far in excess of the cost. Evidently, the referendum process erects too large a barrier to the issuance of bonds and prevents many worthwhile projects. As Hoxby (2001) argues, a loosening of California's constraints on local spending would yield substantial economic benefits. More generally, our results suggest that well-targeted funds for school construction may raise social welfare, particularly in states and localities with low levels of capital investment and highly centralized systems of school finance.

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Figure 1. Distribution of bond measures by vote share
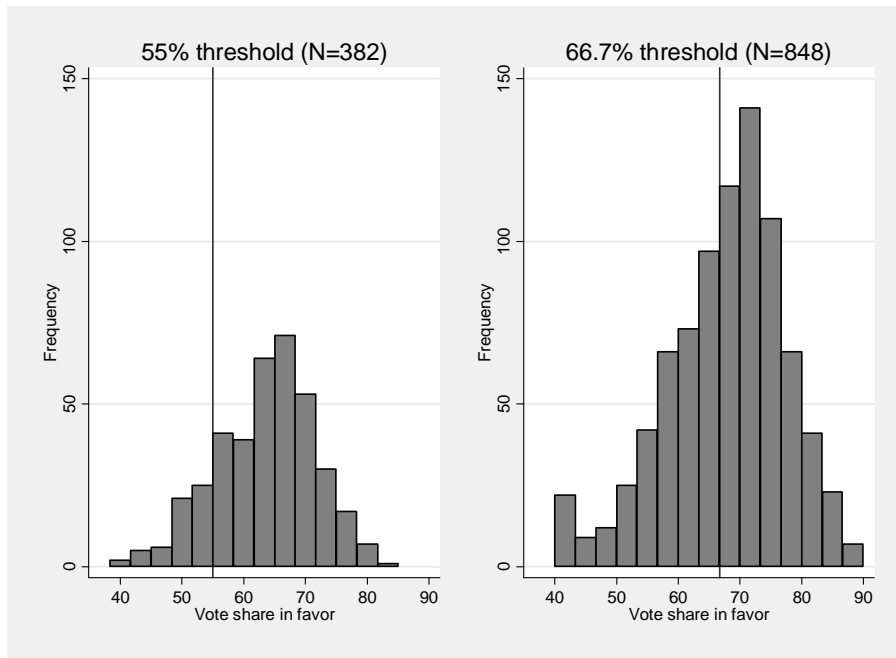
Notes: Sample includes all school district general obligation bond measures in California from 1987 to 2006. Vote shares are censored at 40 and 90.

Figure 2.Total spending and capital outlays per pupil, by vote share,
one year before and three years after election
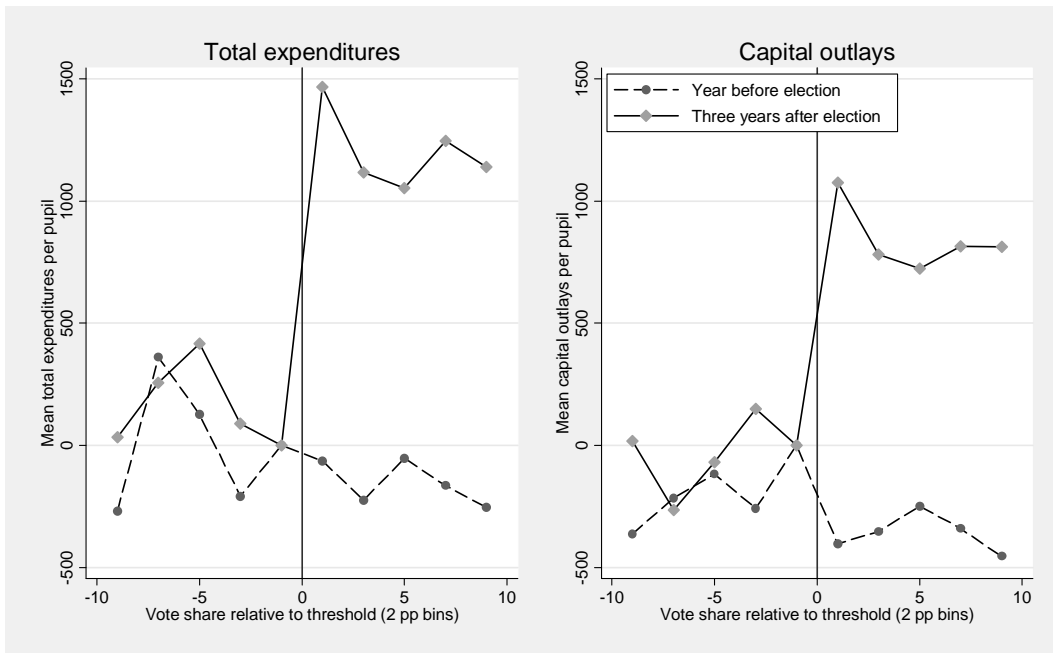
Notes: Graph shows average total expenditures (left panel) and capital outlays (right panel) per pupil, by the vote share in the focal bond election. Focal elections are grouped into bins two percentage points wide: measures that passed by between 0.001% and 2% are assigned to the 1 bin; those that failed by similar margins are assigned to the -1 bin. Averages are conditional on year fixed effects, and the -1 bin is normalized to zero.

Figure 3. Estimates of the effect of bond passage on the probability of passing a later bond, by years since the focal election
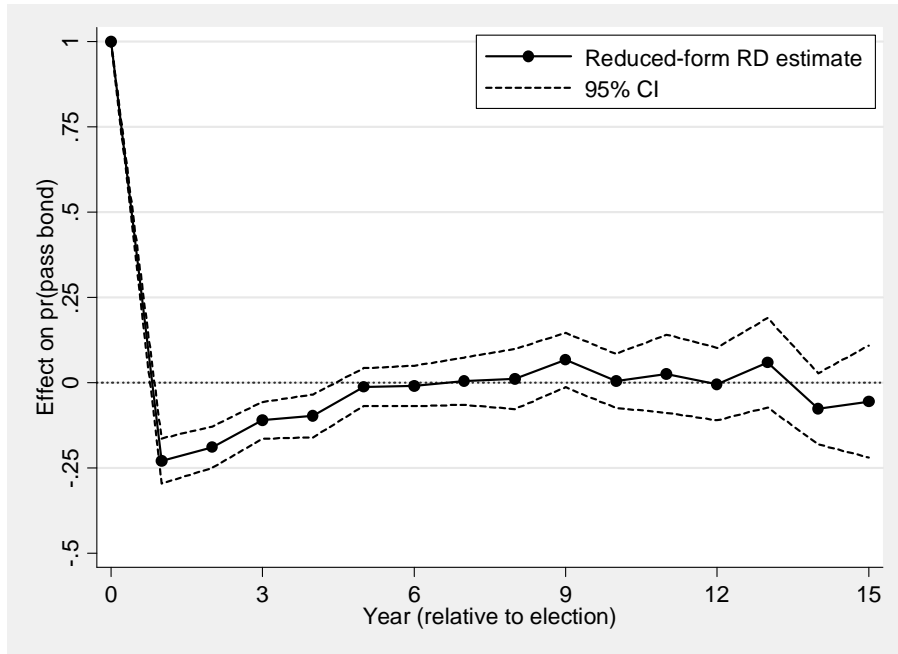
Notes: Graph shows coefficients and 95% confidence intervals for the effect of measure passage in year t on the probability of passing a measure in year $t+\tau$. The specification is the ITT regression described in equation (7). Sample includes relative years -19 through +19, excluding relative year 0 (when the effect is mechanically one).

Figure 4. Recursive and one-step estimates of dynamic TOT effects of bond passage on total expenditures per pupil, by years since election
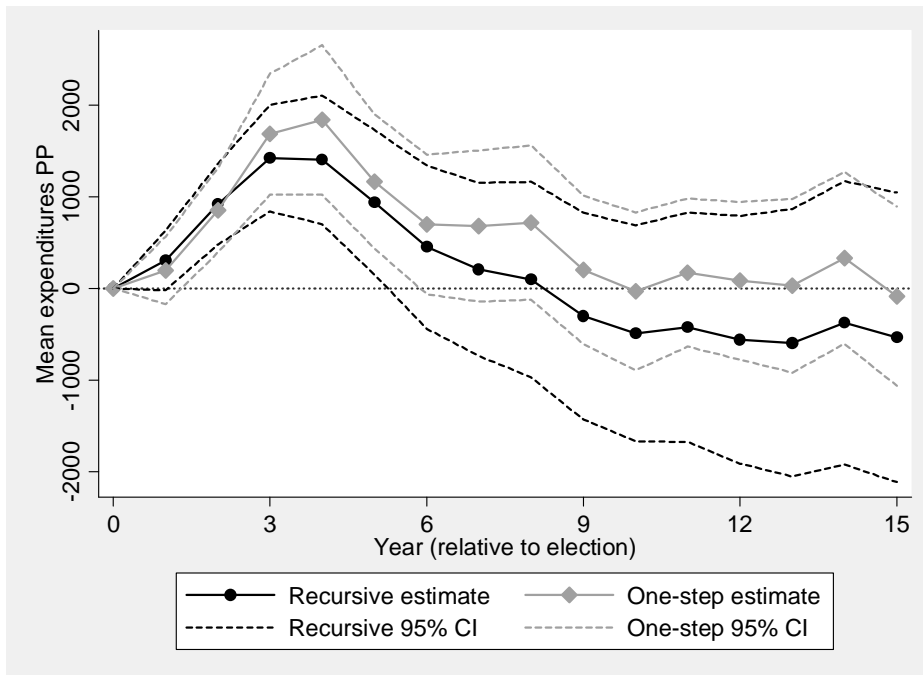
Notes: Graph shows coefficients and 95% confidence intervals for the "recursive" and "one-step" estimates of the treatment-on-the-treated (TOT) effects of measure passage at each lag on expenditures per pupil. The specifications are as in equations (11) and (12), respectively. CIs are based on standard errors clustered at the district level.

Figure 5. Log housing prices by vote share, one year before and three years after election
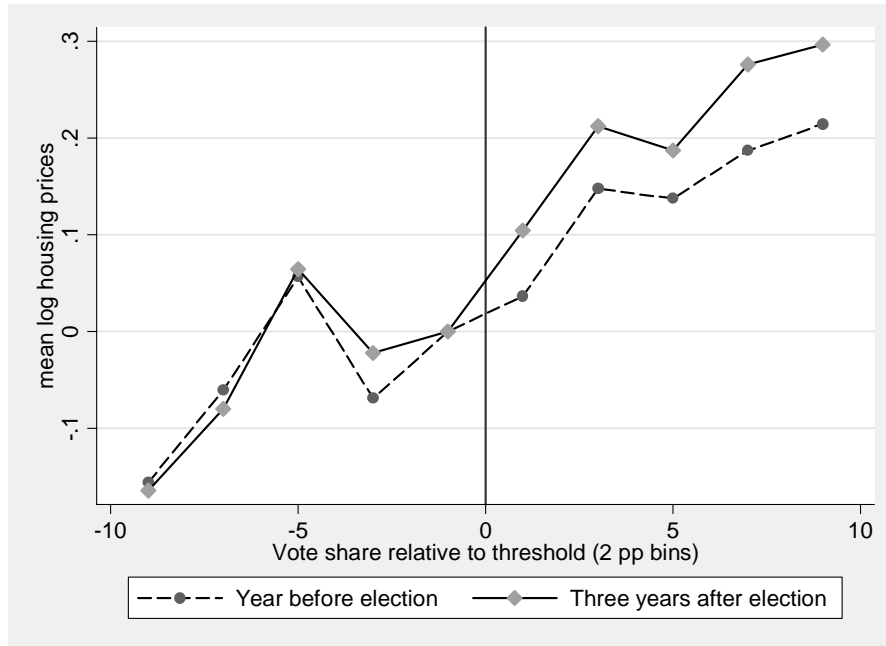
Notes: Graph shows average log housing prices by the vote share in the focal bond election. Focal elections are grouped into bins two percentage points wide: measures that passed by between 0.001% and 2% are assigned to the 1 bin; those that failed by similar margins are assigned to the -1 bin. Averages are conditional on year fixed effects, and the -1 bin is normalized to zero.

Figure 6. Recursive, one-step, and forward-looking estimates of dynamic TOT effects of bond passage on log house prices, by years since election

Notes: Graph shows coefficients and 95% confidence intervals for the "recursive" and "one-step" estimates of the treatment-on-the-treated (TOT) effects of measure passage at each lag on log house prices. Specifications are described in equations (11) and (12), respectively. The graph also shows recursive estimates of the forward-looking TOT effect of measure passage on log house prices, using the alternative recursion formula (14).

Figure 7. Recursive and one-step estimates of dynamic TOT effects of bond passage on average mathematics test scores, by years since election
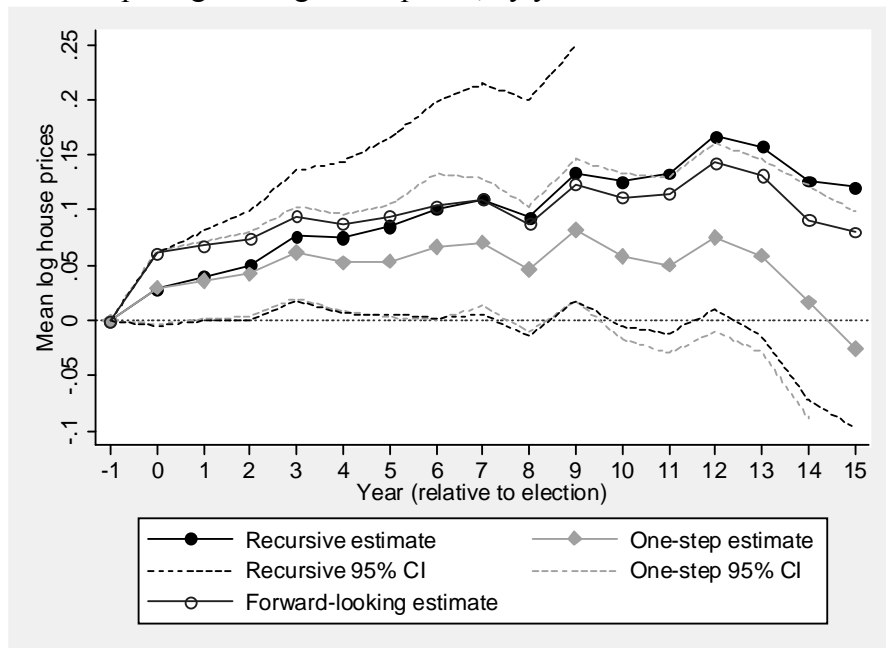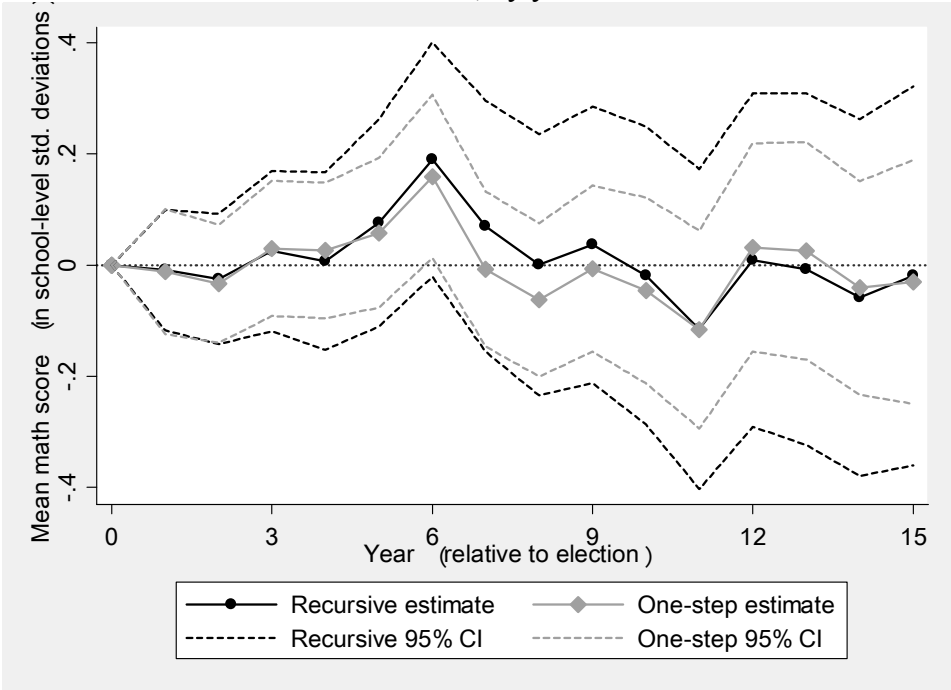

Notes: Graph shows coefficients and 95% confidence intervals for the "recursive" and "one-step" estimates of the treatment-on-the-treated (TOT) effects of measure passage at each lag on mathematics test scores. Specifications are as in equations (11) and (12), respectively. CIs are based on standard errors clustered at the district level.

Table 1. School bond measure summary statistics

Year	Number of measures	Avg. amount per pupil	Fraction 55% req.	Fraction approved	Vote share in favor (%) Mean	Vote share in favor (%) SD
(1)	(2)	(3)	(4)	(5)	(6)	(7)
1987	29	$3,134	0	0.52	64.6	12.0
1988	33	5,081	0	0.61	67.8	8.2
1989	28	3,103	0	0.50	66.4	9.7
1990	31	7,096	0	0.42	61.4	15.2
1991	55	7,612	0	0.40	64.0	10.3
1992	57	7,467	0	0.40	62.2	10.8
1993	45	7,305	0	0.47	62.1	11.7
1994	50	7,365	0	0.42	65.1	9.6
1995	84	6,266	0	0.48	65.0	10.9
1996	50	5,780	0	0.70	70.3	7.9
1997	110	7,244	0	0.64	68.9	8.7
1998	116	6,762	0	0.60	68.7	9.3
1999	82	9,425	0	0.62	69.6	9.7
2000	86	6,307	0	0.65	69.4	8.7
2001	50	8,338	0.48	0.84	68.7	9.2
2002	146	6,004	0.89	0.79	63.4	8.5
2003	18	6,542	0.50	0.56	61.6	9.6
2004	106	8,130	0.93	0.82	65.1	8.6
2005	35	10,157	0.74	0.86	64.7	6.5
2006	109	9,748	0.96	0.72	61.0	7.9

Notes: Data obtained from California Data Partnership. Sample includes all general obligation bond measures proposed by California school districts from 1987 to 2006. Dollar amounts in Column (3) are measured in constant year-2000 dollars.

Table 2. School district descriptive statistics for fiscal, housing markets, and academic variables

	All school districts	Never proposed a measure	Proposed at least one measure	Passed a measure (time t-1)	Failed a measure (time t-1)	Diff (4)-(5) (t stat)
	(1)	(2)	(3)	(4)	(5)	(6)
Number of districts	948	319	629			
A. Fiscal variables						
Number of observations	10,197	3,306	6,891	626	218	
Log enrollment	7.43	6.18	8.03	8.34	8.09	**0.25**
	[1.69]	[1.43]	[1.47]	[1.48]	[1.48]	(2.13)
Total expenditures PP	$7,466	7,410	7,493	7,290	6,941	**349**
	[2,177]	[2,293]	[2,119]	[1,898]	[1,921]	(2.32)
Capital outlays PP	$922	679	1,038	882	935	-53
	[1,100]	[905]	[1,164]	[1,005]	[1,112]	(0.62)
Current instructional expenditures PP	$3,905	4,034	3,844	3,824	3,618	**206**
	[808]	[941]	[728]	[703]	[677]	(3.82)
B. Housing market variables						
Number of observations	15,151	4,578	10,573	731	382	
House prices	$241,537	190,337	263,706	285,857	210,499	**75,358**
	[198,618]	[149,691]	[212,612]	[240,439]	[178,766]	(5.91)
Log house prices	12.16	11.95	12.26	12.33	12.08	**0.25**
	[0.65]	[0.62]	[0.65]	[0.66]	[0.55]	(6.71)
Square footage	1,603	1,572	1,615	1,625	1,637	-11
	[407]	[456]	[386]	[401]	[363]	(0.47)
Lot size	56,772	97,604	39,797	32,342	49,388	**-17,047**
	[81,652]	[111,614]	[57,266]	[48,933]	[60,891]	(4.73)
Sales volume	881	316	1,126	1,519	1,134	**385**
	[1,966]	[951]	[2,225]	[3,568]	[1,445]	(2.54)
Income of homebuyers	$96,482	84,753	101,674	107,689	90,339	**17,350**
	[59,094]	[45,204]	[63,606]	[70,382]	[58,903]	(4.36)
Log income of homebuyers	11.36	11.25	11.40	11.45	11.31	**0.14**
	[0.46]	[0.43]	[0.47]	[0.49]	[0.41]	(5.13)
C. Achievement variables						
Number of observations	9,748	3,240	6,508	460	170	
Reading, grade 3	0.17	0.10	0.21	0.16	0.19	-0.028
	[0.91]	[0.96]	[0.88]	[0.93]	[0.81]	(0.37)
Math, grade 3	0.07	-0.06	0.13	0.12	0.10	0.020
	[0.90]	[0.99]	[0.85]	[0.88]	[0.82]	(0.27)

Notes: Columns 1-5 shows averages and standard deviations (in square brackets). Column 6 reports the difference between Columns 4 and 5, with t statistics in parentheses; bold coefficients are significant at the 5% level. Samples in Columns 1, 2, and 3 include all available observations in all years. Fiscal variables are available for years 1995-2005, housing market variables for 1988-2005 (except income and log income, which are only from 1992-2006), and test scores for 1992-1993 and 1997-2006. Columns 4 and 5 include only observations for the year prior to a bond referendum. All dollar figures are measured in real year-2000 dollars. Achievement scores are standardized to mean zero and standard deviation one across schools in each year, then averaged to the district. Housing market variables are averages across all transacted homes, reweighted to match the population distribution across block groups in 2000.

Table 3. Pre-bond measure balance of treatment and control groups

	Year before election (t-1)				Change, t-2 to t-1		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Fiscal outcomes							
Total Expenditures PP	6	-363	-262	28	-10	50	98
	(123)	(191)	(187)	(177)	(102)	(149)	(151)
Capital outlays PP	**-179**	-220	-154	-44	0	54	95
	(86)	(133)	(126)	(145)	(87)	(121)	(123)
Current instructional exp. PP	**91**	-24	-12	35	-7	-6	-2
	(44)	(63)	(62)	(36)	(19)	(31)	(31)
B. Housing market outcomes							
Log house prices	**0.184**	0.043	0.040	0.013	**0.015**	0.020	0.017
	(0.029)	(0.044)	(0.043)	(0.011)	(0.007)	(0.010)	(0.010)
C. Achievement outcomes							
Reading, grade 3	-0.040	0.147	0.185	-0.010	-0.022	-0.032	-0.022
	(0.088)	(0.120)	(0.117)	(0.054)	(0.034)	(0.058)	(0.057)
Math, grade 3	0.042	0.180	**0.214**	0.054	-0.054	-0.002	0.004
	(0.089)	(0.112)	(0.109)	(0.062)	(0.039)	(0.059)	(0.056)
Year effects & threshold control	Y	Y	Y	Y	Y	Y	Y
Cubic in vote share	N	Y	Y	Y	N	Y	Y
Sample pools relative years [-2, 6]	N	N	Y	Y	N	N	Y
Bond measure fixed effects	N	N	N	Y	N	N	N

Notes: Each entry comes from a separate regression. Dollar values are measured in constant year-2000 dollars. Columns 1-4 report estimated bond effects on outcome levels the year before the election; Columns 5-7 report estimated effects on the annual growth rate that year. Samples in Columns 1-2 and 5-6 include observations from the year before each bond measure election. Samples in Columns 3, 4, and 7 consist of observations from two years before to six years after each bond election. The specification in these columns is equation (7), with indicators for each calendar year and each relative year (-2 through +6), plus interactions of the -1 through +6 relative year indicators with a cubic in the vote share, an indicator for measure passage, and an indicator for an election with a 55% threshold. The interaction between the relative year -1 indicator and the measure passage indicator is reported. Column 4 also includes measure fixed effects. Models for house prices include controls for square footage, lot size, and sales volume in all columns. Sample sizes vary with availability of dependent variable; for fiscal outcomes, N=845 in Columns 1-2, 6,970 in 3-4, 780 in 5-6, and 5,815 in 7. Standard errors (in parentheses) are robust to heteroskedasticity and, in Columns 3, 4, and 7, clustered at the school district level. Bold coefficients are significant at the 5% level.

Table 4. The impact of bond passage on fiscal outcomes:
intent-to-treat (ITT) and treatment-on-the-treated (TOT) effect estimates

	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	335	**936**	**1,271**	**961**	200	-333
	(177)	(216)	(273)	(305)	(316)	(335)
Capital outlays PP	255	**802**	**1,121**	**841**	219	-360
	(151)	(191)	(244)	(277)	(276)	(279)
Current instructional	35	8	3	-26	-20	-19
expenditures PP	(39)	(43)	(45)	(56)	(71)	(74)
State and federal transfers PP	100	41	-98	79	157	-13
	(129)	(149)	(177)	(175)	(175)	(193)
B. Treatment-on-the-treated (TOT)						
Recursive Estimator						
Total expenditures PP	306	**920**	**1,424**	**1,405**	**940**	452
	(166)	(225)	(297)	(358)	(404)	(455)
Capital outlays PP	250	**822**	**1,303**	**1,281**	**924**	381
	(143)	(193)	(257)	(308)	(341)	(372)
Current instructional	44	13	1	-20	-20	-27
expenditures PP	(41)	(54)	(59)	(77)	(100)	(115)
State and federal transfers PP	67	-22	-142	-19	11	-148
	(120)	(148)	(190)	(207)	(227)	(261)
One-Step Estimator						
Total expenditures PP	198	**853**	**1,688**	**1,841**	**1,169**	701
	(188)	(235)	(337)	(417)	(374)	(389)
Capital outlays PP	220	**792**	**1,549**	**1,660**	**1,091**	**554**
	(157)	(228)	(299)	(308)	(268)	(267)
Current instructional	22	-28	-33	-64	-80	-82
expenditures PP	(46)	(52)	(49)	(64)	(77)	(80)
State and federal transfers PP	41	-50	184	104	91	-6
	(133)	(185)	(311)	(218)	(203)	(227)

Notes: Each row represents a separate specification, and reports effects of measure passage on outcomes 1 year later (Column 1), 2 years later (Column 2), and so on. Dependent variables are measured in constant year-2000 dollars per pupil. Panel A presents estimates of the ITT effects of bond passage. The sample consists of all bond elections and all outcome measures from years relative to the election -2 through +6. Some fiscal measures appear in the sample several times for different relative years. N=6,970. The specification corresponds to equation (7), and includes bond measure fixed effects; indicators for calendar years and years relative to the bond measure; and interactions of the relative year fixed effects (for relative years 1 through 6) with a cubic in the vote share, an indicator for passage, and an indicator for a 55% threshold. The table reports the relative year-passage interaction coefficients. Panel B presents estimates of TOT effects, first using the recursive estimator and second using the one-step estimator. The recursive estimator uses equation (11), applied to ITT estimates as in Panel A but with all available relative years included in the sample. N=13,405.   The one step estimator uses a conventional panel of districts-by-calendar years. The specification is equation (12). It includes calendar year effects; indicators for the presence of an election t years ago for t=1,...,18; indicators for measure approval t years ago; cubics in the vote shares of the election t years ago (if any); and indicators for a 55% threshold in the election t years ago. N=7,038. Standard errors (in parentheses) are clustered on the school district, and bold coefficients are significant at the 5% level.

Table 5. The impact of bond passage and bond amounts on log housing prices:
intent–to-treat (ITT) and treatment-on-the-treated (TOT) effect estimates

	Yr of elec. (1)	1 yr later (2)	2 yrs later (3)	3 yrs later (4)	4 yrs later (5)	5 yrs later (6)	6 yrs later (7)
A. Effect of authorizing a bond							
Intent-to-treat (ITT)	0.021	0.027	0.036	**0.058**	0.038	0.038	0.047
	(0.015)	(0.017)	(0.020)	(0.022)	(0.024)	(0.027)	(0.035)
Treatment-on-the-treated (TOT)							
Recursive estimator	0.028	**0.041**	**0.050**	**0.077**	**0.075**	**0.086**	**0.101**
	(0.017)	(0.021)	(0.025)	(0.030)	(0.035)	(0.041)	(0.050)
One-step estimator	0.030	**0.036**	**0.042**	**0.062**	**0.052**	**0.054**	**0.067**
	(0.017)	(0.018)	(0.020)	(0.021)	(0.022)	(0.026)	(0.034)
B. Effect of authorizing $1,000 per pupil in bonds							
Intent-to-treat (ITT)	0.0047	0.0059	0.0078	**0.0136**	0.0083	0.0081	0.0104
	(0.0032)	(0.0037)	(0.0041)	(0.0050)	(0.0050)	(0.0056)	(0.0080)
	[$1,105]	[$1,403]	[$1,853]	[$3,213]	[$1,959]	[$1,914]	[$2,460]
Treatment-on-the-treated (TOT)							
Recursive estimator	**0.0076**	**0.0090**	**0.0104**	**0.0188**	**0.0146**	**0.0153**	0.0174
	(0.0036)	(0.0044)	(0.0045)	(0.0058)	(0.0059)	(0.0070)	(0.0102)
	[$1,788]	[$2,137]	[$2,449]	[$4,441]	[$3,441]	[$3,611]	[$4,108]
One-step estimator	0.0059	**0.0076**	**0.0081**	**0.0120**	**0.0091**	**0.0095**	0.0130
	(0.0033)	(0.0036)	(0.0036)	(0.0042)	(0.0041)	(0.0048)	(0.0068)
	[$1,387]	[$1,796]	[$1,908]	[$2,841]	[$2,147]	[$2,246]	[$3,073]

Notes: Each row represents a separate specification and reports effects of measure passage on log house prices 1 year later (Column 1), 2 years later (Column 2), and so on. The dependent variable is the log of the block-group-population weighted average sale price of all transacted homes in the school district, measured in constant year-2000 dollars per pupil. Panel A presents estimates of the effect of authorizing a bond. The ITT estimates use a sample of all bond elections and all log house prices from years relative to the election -2 through +6. N=7,968. The specification is equation (7), and includes bond measure fixed effects; indicators for calendar years and years relative to the bond measure; average square footage and lot size of transacted homes; the number of transactions; and interactions of the relative year fixed effects (for relative years 0 through 6) with a cubic in the vote share, an indicator for passage, and an indicator for a 55% threshold,. The table reports the relative year-passage interaction coefficients. The recursive TOT estimates are based on equation (11) applied to ITT estimates, estimated as in row 1 but with all available relative years included in the sample. N=20,070. The one-step TOT estimates are based on a conventional panel of districts-by-calendar years. The specification is equation (12). It includes calendar year effects; indicators for the presence of an election t years ago for t=1,...,18; indicators for measure approval t years ago; cubics in the vote shares of the election t years ago (if any); indicators for a 55% threshold in the election t years ago; average square footage and lot size of transacted homes; and the number of transactions. N=10,227. Panel B reports the effect of authorizing $1,000 per pupil in bonds. All specifications are similar to Panel A, except that bond passage indicators are replaced by continuous measures of the size of the bond authorized (set to zero if no bond is authorized), with the passage indicators used as instrumental variables for the bond amounts. Numbers in square brackets represent the effect on home price levels, evaluated at the mean price in districts with close elections, $236,433. Standard errors are clustered on the school district, and bold coefficients are significant at the 5% level.

Table 6. Alternative specifications for log housing prices: one-step estimates of TOT effects

	Log housing price effects 4 yrs after election
1. Baseline (cubic in vote share)	**0.052**
	(0.022)
A. Vote share controls	
2. Linear	**0.061**
	(0.021)
3. 3-part linear	0.048
	(0.024)
4. 3-part cubic	**0.109**
	(0.042)
B. Placebo thresholds	
5. Switch 55% & 67% tresholds	**-0.078**
	(0.033)
6. Actual threshold minus 10	0.031
	(0.033)
7. Actual threshold plus 10	-0.017
	(0.034)
C. Additional specifications	
8. Including parcel tax referenda	**0.058**
	(0.020)
9. No weights and no housing controls	**0.051**
	(0.022)

Notes: Each cell represents a separate regression. Only the coefficients for housing prices four years after the election are shown. The baseline specification presents the estimated effect of measure passage in the 4th year after the election from the one-step specification in Panel A of Table 5. Remaining cells derive from slight modifications to this sample or specification. The "linear" specification replaces the cubics in the vote share of each past election with linear controls; "3-part linear" uses linear segments in the [0, 55], [55, 66.7], and [66.7, 100] ranges; and "3-part cubic" uses separate cubic segments in each range. The "placebo threshold" specification in Panel B include both the actual measure passage indicators and counterfactual indicators that reflect vote shares in excess of alternative thresholds; the coefficients shown are those on the counterfactual indicators in the 4th year after the election. In Panel C, the estimate labeled "including parcel tax measures" adds controls for the presence of a parcel tax measure on the ballot in each past year, cubics in the parcel tax vote shares, and indicators for parcel tax passage. In row 9, the dependent variable is the raw average price of houses transacted during the calendar year, without reweighting, and housing characteristic controls are excluded. All standard errors are clustered at the district level and bold coefficients are significant at the 5% level.

Table 7. The effect of bond passage on academic achievement, housing market transactions, homebuyer and school district characteristics: one-step estimates of 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)	N (7)
A. Academic Achievement							
Reading, grade 3	-0.010	-0.023	0.058	-0.026	0.039	0.103	6,660
	(0.054)	(0.051)	(0.053)	(0.058)	(0.061)	(0.064)	
Math, grade 3	-0.012	-0.034	0.030	0.026	0.058	**0.160**	6,660
	(0.057)	(0.054)	(0.062)	(0.062)	(0.069)	(0.075)	
B. Housing market transactions							
Sales volume	93	**207**	**282**	**213**	**241**	**325**	10,857
	(78)	(89)	(93)	(98)	(116)	(115)	
Log sales volume	-0.001	0.041	0.031	-0.005	0.032	0.039	10,857
	(0.056)	(0.062)	(0.066)	(0.068)	(0.070)	(0.073)	
Average square footage	-6	2	6	15	14	18	10,273
	(13)	(16)	(17)	(17)	(16)	(19)	
Average lot size	-4159	3008	3980	**10874**	3455	6586	10,579
	(3962)	(3041)	(3903)	(5198)	(5026)	(3777)	
C. Homebuyer characteristics							
Income	3245	1042	-2384	4212	1486	450	9,921
	(2624)	(2645)	(3113)	(3435)	(3218)	(3391)	
Log income	0.027	0.017	-0.005	0.035	-0.008	0.004	9,921
	(0.019)	(0.021)	(0.023)	(0.025)	(0.025)	(0.024)	
Fraction white & Asian	0.017	0.000	0.001	0.004	0.001	-0.008	9,921
	(0.009)	(0.009)	(0.010)	(0.010)	(0.011)	(0.011)	
D. School district characteristics							
Log enrollment	-0.012	-0.011	0.010	0.001	0.001	-0.007	7,038
	(0.017)	(0.020)	(0.025)	(0.039)	(0.035)	(0.042)	
Fraction white & Asian	-0.002	0.004	0.002	0.005	0.008	0.004	7,035
	(0.005)	(0.006)	(0.008)	(0.008)	(0.009)	(0.010)	
Avg. parental education	-0.060	-0.218	-0.018	-0.169	-0.163	0.005	6,978
	(0.096)	(0.170)	(0.156)	(0.218)	(0.161)	(0.149)	

Notes: Each row represents a separate specification and reports effects of measure passage on outcomes 1 year later (Column 1), 2 years later (Column 2), and so on. All regressions use the one-step TOT specification to estimate the effect of bond passage on outcomes. The specification is equation (12) in the text. It includes calendar year effects; indicators for the presence of an election t years ago for t=1,...,18; indicators for measure approval t years ago; cubics in the vote shares of the election t years ago (if any); indicators for a 55% threshold in the election t years ago. Variation in sample sizes reflects differences in availability of dependent variables. Kindergarten and 1st grade enrollments and racial shares exclude districts with grade-level enrollment below 20 in 1987. Bold coefficients are significant at the 5% level.