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Assessing the Effectiveness and Efficiency of Local Education Spending

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Abstract: This thesis examines the economic return and efficiency of local education spending by studying its impact towards measurable school outcomes, namely on student performance and teacher quality, and house prices. To obtain causal estimates, the main analysis uses a regression discontinuity design by comparing close winners and losers of local school bond elections as a source of random, exogenous variation in school expenditures. The results indicate that education spending in Texas is inefficiently low as house prices increase by 6-11% after two years following the authorization of an average bond. Moreover, bond-financed spending have a detrimental effect on teacher quality, whereas the effect on student performance remains positive, yet small.

Table of Contents

I. Introduction

The efficiency of public goods provision is a fundamental concern in public economics. With multiple local jurisdictions, Tiebout (1956) showed that a market solution exists as local governments compete for residents by designing public good provision that best responds to the demand of their local constituencies. An efficient allocation of public goods is attained as households locate to the jurisdiction that optimally reflects their preferences of public goods, taxes, and house prices given their budget. Despite Tiebout's convincing theoretical predictions, opposing arguments persist to reason how inefficient provision may be prevalent. First, much of the public choice literature highlight the potential of Leviathan governments that maximizes their budget beyond the socially optimal level (Brennan & Buchanan, 1980; Niskanen, 1968). On the other hand, regulations have constraint the ability for local governments to respond to residents' demand, such as by limiting tax increases or requiring referendums to engage in bond financing, leading to inefficient under-provision of public goods. In the United States (US), however, the latter currently appears to be the more likely scenario.

Every year, more than $400 billion is spent on public infrastructure in the US. However, annual spending regularly fails to meet capital and maintenance requirements, resulting to a deterioration of the country's capital assets. In 2017, the American Society of Civil Engineers reported that inadequate infrastructure accounts to a loss of almost $4 trillion in GDP and 2.5 million in jobs through 2025. As a result, an additional $2.1 trillion must be spent through 2025 to meet demand and reduce negative impacts in the economy. A similar picture arise in the context of school facility investments, which is undeniably amongst the most important public investments. Although more than $55 billion was spent on capital outlay in 2016, this amount was still 24% lower per pupil relative to the start of the century. Based on survey responses, 53% of public schools require, on average, $4.5 million to fund repairs, renovations, and modernizations of its onsite buildings to acquire a good level of overall condition and address the major obstacles to instruction.

Therefore, this thesis aims to assess the efficiency and study the overall impact of increasing public good provision, namely school capital investments. Two barriers of identification are central to overcome in this thesis. Firstly, estimates may not fully capture the effects of capital expenditures, which particularly appear in unobservable, non-academic outcomes that are equally valued by residents, such as school aesthetics or amenities. Pioneered by Oates (1969), house prices have been frequently used to address this issue. When homebuyers value a local project more than the amount of taxes they must pay to finance the project, an increase in spending will be accompanied by an increase in house prices. In this case, public good provision is inefficiently low. However, a simple OLS regression of local property values on spending do not address the challenge of obtaining causal estimates if jurisdictions are free to endogenously determine their spending levels. This leads to the second issue of identification.

The second problem concerns the potential endogeneity bias in the estimates. Capital expenditures are likely to be confounded with local factors, such as the preferences of residents or the effectiveness of governments, that also determine outcomes. To address this, I implement a new dynamic research design established in the work of Cellini, Ferreira, and Rothstein (2010) that isolates exogenous variations in school spending. In the US, school capital projects must be financed by local school district bond authorizations that must be approved through a local referendum. The approved bond proposals are then repaid through the district's property tax revenues. Districts that issue a bond proposal may be inherently different to those that do not. However, when looking at close elections, a district where a proposed bond is approved by one vote is likely to be similar in characteristics to a district who rejects a bond proposal by the same margin; hence, a causal relationship can be estimated through a RD design. In addition, Cellini et al. (2010) extend the RD design to be dynamic in nature. Bond effects may occur with lags since bond-financed facilities materialize several years after the initial authorization and sticky housing markets respond slowly to information.

By addressing the two problems, this thesis aims to assess the efficiency of public education provision through the following main research question:

To what extent do house prices respond to exogenous changes in education spending?

In addition, I further study the overall impact of education spending on several measured outcomes, leading to the following sub-question:

To what extent does exogenous changes in education spending affect measurable school outcomes on student performance and teacher quality?

Through answering these two questions, I aspire to contribute to the growing body of literature on the effectiveness of schooling expenditures. I will examine the extent to which bond-financed facility investments are valued by local residents beyond observable school outcomes. Moreover, as this thesis draws heavily to Cellini et al. (2010), I reevaluate their findings by applying their empirical strategy to a different geographical context and observable school outcomes.

I apply the methodology to a rich data set that combines almost two decades of information of Texas school bond referendums with annual, district-level measures of expenditures, house prices, student performance, teacher quality, and demographics. I find that bond authorizations increases house prices by up to 11% by the sixth year following elections. Regarding the effect towards observable outcomes, the results tend to indicate that the spike in capital expenditure is at the expense of teacher quality, while still maintaining a positive, but small, effect towards student performance. Thus, the results suggest that school districts in our sample underinvest in school facilities.

The remainder of this thesis is organized as follows. First, Section II presents a literature review in two parts: the theoretical work evaluating public good efficiency through property value capitalization and the empirical work surrounding the effects of public school expenditure. Next, Section III describes the institutional background of the school finance system in Texas and also elaborates the data sources used for the empirical analyses. Section IV introduces the two empirical research design of this thesis: the OLS specifications as baseline estimates and the dynamic RD design for causal interpretations. Section V presents the estimates. Here, I also assess the validity of the research design through a series of robustness checks. Section VI is a discussion relating the results to the previous literature and proposing potential policy implications. Finally, Section VII concludes.

II. Literature Review

This section aims to synthesize current literature on the various effects of school expenditure through two parts. First, I formulate the main hypothesis concerning the efficiency of public school spending through the causal channels that have been identified in the theoretical literature. In the second part, I provide an overview of the methodology and results of previous empirical studies that aims to capture the effects of school expenditure towards school outcomes and house prices. In addition, I further highlight how the various empirical studies mentioned contribute to decisions made in the thesis' empirical strategy.

Theory— Basic theoretical models on local public goods, property values, and residential location was first introduced by Tiebout (1956). His model on local expenditures proposes that, if local communities offer varying baskets of public goods at different of tax rates, individuals with heterogenous personal valuations of public goods will move towards the local community that maximizes their personal utility. The model that will be used in this thesis draws from an alternative extension of Tiebout's model with household voting based on Brueckner (1979). Notable variations of the theoretical model is studied by Epple and Sieg (1999) and Epple, Gordon, and Sieg (2010). In summary, households choose their location based on house prices, amenities, taxes, and their budget constraint. Households have the ability to vote for higher taxes for an increase in local public goods provision. When the level of public goods in a community change, housing values are likely to change as some households re-optimize their locational decisions. Following is the formal derivation of the theory applied to schools as public goods.

There are D school districts with different levels of public school spending and quality A_d. The districts have different housing costs p_d and taxes τ_d. Ceteris paribus, households prefer districts with greater level or quality of schooling; however, as public goods are funded by the district-specific tax τ_d, high public good communities are costly in terms of tax burdens and housing costs. Moving costs are assumed to be zero. Hence, when choosing where to live, household h maximizes utility over a set of d districts:

$$Max_{\{d\}}\, U_h(A_d(\tau_d), X_d, c_h)$$

subject to the budget constraint:

$$w_h \geq c_h + p_d + \tau_d$$

Here, X_d is a vector of other locational amenities that are not supplied through taxation. c_h represents the numeraire consumption good and w_h is household income. As shown in the utility function, the level of public school spending and quality depends on tax revenues, $A_d = A_d(\tau_d)$, and the property of the first derivative is positive, ${A_d}'(\tau_d) > 0$. When the local government is productively inefficient, ${A_d}'(\tau_d)$ is small.

From the maximization problem, households choose to live in a district with the locus of prices and public goods according to their preferences. The indirect utility function of household h can be defined as $U_h(A_d(\tau_d), X_d, w_h - p_d - \tau_d)$. A household's maximum bid for housing in a school district d is expressed as $g_{hd} = g_h(\tau_d, X_d)$, where $g_h(\cdot)$ is defined implicitly by $U_h(A_d(\tau_d), X_d, w_h - g_h(\tau_d) - \tau_d) = \max_{k \neq d} U_h(A_k(\tau_k), X_k, w_h - p_k - \tau_k)$. Hence, a household will choose district d if $p_d \leq g_{hd}$.

Because housing supply in each district is assumed fixed, equilibrium in the housing market is achieved when supply and demand are equal in each district through the adjustment of prices and re-sorting of households such that no household are made better off by moving. This is defined as a set of optimal taxes and house prices (τ_d^*, p_d^*) for all D school districts. In a situation of excess demand, house prices p_d will rise above the maximum bid of some household resulting them to move to another district.

Because households' preferences for public school provision A_d and local amenities X_d is heterogenous, it is possible for a household to prefer a higher tax rate than the equilibrium, $\tau_d > \tau_d^*$, even when d is their preferred district. Hence, the N_d residents of school district d is able to vote for the authorization in increasing τ_d to better fit their preferences. If the authorization is approved through a local election, households will relocate among the D districts under utility maximization. In turn, this impacts house prices. The following equation represents how housing bids respond to tax changes by differentiating the implicit function representing the household's maximum bid for housing g_h:

$$\frac{\partial g_h(\tau_d, X_d)}{\partial \tau_d} = \frac{\frac{\partial U_h}{\partial A_d} \cdot \frac{\partial A_d}{\partial \tau_d}}{\frac{\partial U_h}{\partial c_h}} - 1 = \frac{\frac{\partial U_h}{\partial A_d} \cdot \frac{\partial A_d}{\partial \tau_d} - \frac{\partial U_h}{\partial c_h}}{\frac{\partial U_h}{\partial c_h}}$$

As shown from the equation above, following an increase in taxes, housing bids will increase if the product of the marginal utility of public goods, $\frac{\partial U_h}{\partial A_d}$, and the efficiency of the local government to provide public goods, $\frac{\partial A_d}{\partial \tau_d}$, exceeds the marginal utility foregone for consumption $\frac{\partial U_h}{\partial c_h}$. Hence, there exist an inverse-U relationship between house prices and public good levels. Public goods are efficiently provided when $\frac{\partial g_h}{\partial \tau_d} = 0$, which means they are unresponsive to changes in tax rates. When the level of public goods is inefficiently low, increases in taxes will cause house prices to increase, $\frac{\partial g_h}{\partial \tau_d} > 0$; the opposite case occurs for inefficiently high public goods provision.

Now, I add a time dimension to introduce dynamics to the model, which was previously omitted for simplification. The increased level of public goods funded by a tax increase generates value through amenities that accrue for residents. The marginal willingness to pay (MWTP) of living in district d is the present value of the infinite stream of school amenities discounted at rate i:

$$MWTP_{dt} = \sum_{t=1}^{\infty}(A_d) \cdot (1+i)^{-(t-1)}$$

Even though current owners benefit from increases in the amenity value, households pay higher taxes to continue to reside in district d. If current owners and potential buyers are indifferent between staying and moving, the change in house value (capitalization) from increases in public good level and taxes, τ^*_{dt} to $\tau^{*\prime}_{dt}$, is the difference between the MWTP and the present value of future tax burden, $FTB_t(\cdot)$:

$$Capitalization_{dt} = MWTP_{dt} - FTB_{dt}(\tau^{*\prime}_{dt} - \tau^*_{dt})$$

Thus, the relationship between capitalization and efficiency in public goods provision is described below:

$$\boldsymbol{If\ Capitalization_{d1}\begin{Bmatrix} > 0 \\ = 0 \\ < 0 \end{Bmatrix}\ then\ A_d(\tau^*_d)\ is\ \begin{Bmatrix} ineffeciently\ underprovided \\ efficiently\ provided \\ inefficiently\ overprovided \end{Bmatrix}}$$

Here, the motivation in adding time subscripts is as follows: while most improvements in public goods, such as hiring better quality teachers, require a permanent tax increase, other cases, such as land acquisition, require a finite investment. This is consistent with bond financing, which is mostly used in school districts all over the US and will be further explained in Section III. With this, a school district pays an upfront cost and repays according to term period. If a referendum passes at $t = 0$, taxes are first increased at $t = 1$ and $FTB_t(\cdot)$ decreases over time until zero if tax increases are term periods. The temporal evolution of tax payments are unknown, however the total tax bill is immediately known after election results through the bond authorization amount and equals the present value of future tax burden at $t = 1, FTB_1$. The impact on house prices may therefore be immediate once information have been disclosed in election day. Hence, it should be sufficient to evaluate capitalization at $t = 1$ to test for public goods efficiency using the equation above. Yet, in reality, due to sticky house prices and imperfect information among homebuyers, it may take several years for house prices to fully reflect the impact of school bond authorizations. Therefore, the methodology developed in Section IV aims to capture the dynamic treatment effects of increases in public school spending.

As shown, the testable predictions regarding efficiency and capitalization over time depend on estimating the change in house prices. This relationship between public good provision in education and house prices will be the primary focus of the thesis. In the next subsection, I explore other relevant effects of education spending that are actively studied in the empirical literature and evaluate the methodology used.

Empirics— Before examining the efficiency of school expenditure through house prices, I also study whether school expenditure effectively influence measurable school outcomes. This subsection reviews the relevant empirical studies that aims to identify the causal effect of school expenditure towards observable school outcomes and also housing market outcomes, emphasizing on the results and evolution of the literature's methodology. In addition, I explore the growing literature that utilize a similar RD setup in the context of close elections, which is the primary methodology of this thesis.

(a) School outcomes

The most widely studied school outcome throughout the literature is the impact of education spending towards student performance. Firstly, the general conclusion regarding this relationship suggest that greater resources for education *do* lead to better student performance through improved test scores. This is confirmed through a number of meta-analysis studies, which are quantitative summaries of existing estimates in the literature (Glass and Smith, 1978; McGiverin, Gilman, and Tillitski, 1989; Hedges and Stock, 1983; Hanushek, 1996). Early studies, such as Kiesling (1967), utilized a multiple regression controlling for socio-economic attributes and school district growth found a weak relationship between expenditure per pupil and student performance. The causal interpretation for such methodology remains a debate since other sources of endogeneity bias are not controlled for. Succeeding studies, such as one by Jones and Zimmer (2001), addressed potential simultaneity bias in the relationship by applying a 2SLS, where education expenditures, proxied by school debt, are predicted by student socioeconomic characteristics, school characteristics, and other debt identifiers. Here, a significant positive association was found, where capital spending improved scores on Michigan's standardized tests. More recent studies exploits exogenous variations of different school capital inputs through an instrumental variable (IV) approach or a regression discontinuity (RD) design; however, these studies did not exploit exogeneous variations to school expenditures in general. Angrist and Lavy (2002) instrumented computer availability by a lottery program sponsoring new computer installation. Although they found an increase in computer-aided instruction, these increased capital inputs do not translate to higher student test scores. Furthermore, Goolsbee and Guryan (2006) used a RD strategy as California internet subsidy programs were dependent on school characteristics. Looking at a small neighborhood around the threshold, which approximates a randomized experiment, the authors did not find significant effects of school internet accessibility, which represents higher quality capital, towards student performance.

This thesis would like to further study the impact of education spending on school outcomes, especially regarding school teachers. Issues of teacher shortages have been prevalent in policy discussions for decades. Increased capital spending that improves a school's environmental condition, such as mitigating plumbing problems, inadequate lighting and lack of temperature controls, may also benefit teachers by improving morale and reducing absenteeism, which are critical for a school's achievement (Buckley, Schneider, and Shang, 2004). Surprisingly, empirical studies actually show limited impact of capital improvements, even though the causal interpretation of the methodology remains questionable. Hanushek, Kain, and Rivkin (2004) show through applying a multiple regression that teacher mobility is more strongly affected by student characteristics, particularly race and achievement, compared to salary. In another study, one conducted by Bempah, Kaylen, Osburn, and Birkenholz (1994), the authors attributed teacher mobility to home ownership access in a school district and leadership style of school administration. The study used a simultaneous equations model in the form of a probit that involves qualitative and limited dependent variables.

In conclusion, this thesis would like to reevaluate whether capital spending of school districts result to better student performance and teacher quality. I will extend the current literature by using a variety of different measures on student performance and teachers, which will be explained in Section III.

(b) Housing market outcomes

The traditional method in identifying the effects of a school district's spending towards property values is through a hedonic multiple regression, where the house price is regressed on several community variables based of some form utility function specified. One such study was conducted by Brasington (2002) and found a positive relationship that house prices are most responsive to changes in school quality than other community variables. This finding confirms prior studies of Goodman and Thibodeau (1998) and Haurin and Brasington (1996). The pioneering, seminal work by Oates (1969) further used a 2SLS estimation to account for simultaneity bias and found similar results. Here, the first-stage predicts expenditure with a number of predetermined variables such as population density, change in population, and the like. More recent studies such as Barrow and Rouse (2004) used an IV approach in the 2SLS to capture exogenous variations in education expenditure with state aid as the instrument. A positive relationship of public school spending and property values was found with the IV procedure, while their OLS results show a negative correlation. Finally, boundary RD designs have been used to estimate the effect of school quality, based on student achievement instead of the school's expenditure, towards house prices (Black, 1999; Gibbons, Machin, and Silva, 2013). Both studies found that better-achieving school districts increases the willingness to pay for housing and, thus, property values. Overall, in a variety of geographical settings, the results of a diverse set of methodologies indicate a positive relationship between school spending and house prices, which this thesis aims to reevaluate using an alternative identification strategy similar to Cellini et al. (2010). This will be further elaborated in (c). Following the hypothesis developed in the theory section, the previous literature indicate that there is a tendency for society to underinvest in education, which opposes the public choice literature that emphasizes on overspending by "Leviathan" governments.

(c) RD design with vote shares

This thesis primarily uses the empirical strategy first developed by Cellini et al. (2010). It encompasses a RD design that compares close winners and losers of local school bond elections as a source of random, exogenous variation in education spending. Furthermore, for better precision, the authors developed an extension of the framework to a dynamic setting as panel data is used. With this, the partial effects of election results in one year towards outcomes on later years depend on the years elapsed. This dynamic RD setup was then applied to evaluating other local public goods such as open spaces (Lang, 2018). However, the basic, static RD setup comparing close winners and close losers of elections have been widely used, especially in the field of empirical political economics. In that field, narrow margin elections are used to study the effectiveness of parties in implementing policies (Lee, Moretti, and Butler, 2004; Ferreira and Gyourko, 2009; Pettersson-Lidbom, 2008).

All in all, this thesis is heavily inspired by the work of Cellini et al. (2010), but incorporating notable extensions. First, I apply and replicate their empirical framework to a different state, Texas, while the original authors applied the dynamic RD design to California. In addition, this thesis will study the impact of education on additional and different measurable outcomes regarding school outcomes and housing market outcomes.

III. Institutional Background and Data

Institutional Background— This subsection aims to give a brief overview of the school system in the US and then explain their primary method of funding and finance.

Public schools in the US are organized by each of their own school districts. In Texas, there are currently 1029 public school districts. These are legally separate entities both corporate and politic. Because they are independent in nature, school districts can be viewed similarly to local governments, such as towns or counties, who possess taxing authority and eminent domain, the right to acquire land for public use. These decisions are proposed by school boards, who are normally elected through popular vote, and must be approved through a local referenda. Because district residents determine the policy outcomes, it is natural to oblige children to attend public schools belonging to a school district where they result. Hence, it is the goal of this thesis to study if decisions of school districts affect observable school quality outcomes and the surrounding housing market.

There are two primary methods for financing school districts. First, districts receive state funding for operational use based on student attendance. Second, districts hold local bond elections to primarily finance its capital expenditures such as construction projects, major repairs, technology upgrades, acquisition of land and the like. These local elections approve the authorization of a bond, which is the maximum amount of bonds that districts are allowed to sell without another election. Bonds that have been sold are retired by raising revenue from the school district's tax rate. Through this financing system, school district policies are unique as they choose their own routes of implementation. Each district have their local goals, priorities and needs, which is based on the district's demographics, population, wealth, educational performance, and other preference-defining characteristics. Therefore, the local bond election system to finance capital projects is essential for the empirical analysis of the thesis. Essentially, by comparing close winners and losers around the

50% voter approval threshold, there is an unpredictable component of the election that approximates a randomized experiment. This, arguably, eliminates the sample selection bias generated by differences in inherent district characteristics and address the endogeneity bias in identifying the causal effects of schooling capital expenditures. Section IV gives a more detailed explanation of the empirical strategy of the thesis.

Data— The empirical analyses requires information on Texas' bond selections, school districts, and housing market. Here, I describe the data sources used to obtain such necessary information.

Table 1. School Bond Elections Summary Statistics (1996 – 2016)

Year	Number of Elections	Average Amount per Pupil	Fraction of Bonds Approved	Vote Share % in Favor	
				Mean	Standard Deviation
1996	18	2652	0.94	88.9	24.7
1997	78	4422	0.76	64.2	16.7
1998	61	5643	0.78	64.9	16.7
1999	99	4918	0.84	67.2	15.8
2000	169	6279	0.78	66.8	18.4
2001	161	5140	0.72	63.2	16.8
2002	181	6386	0.75	61.9	16.6
2003	163	5455	0.6	56.7	17.2
2004	175	4716	0.76	59.2	14.7
2005	149	10365	0.65	55.1	17.3
2006	219	7565	0.82	59.7	10.8
2007	241	13523	0.78	60.1	13.8
2008	168	9835	0.70	59.0	13.3
2009	96	11652	0.66	56.4	15.3
2010	133	8239	0.52	53.8	15.8
2011	108	8806	0.58	55.1	13.2
2012	79	12653	0.75	59.6	16.8
2013	165	12772	0.71	57.1	14.2
2014	138	18081	0.77	59.0	13.1
2015	139	13671	0.81	61.8	13.6
2016	117	15595	0.75	58.4	15.7

The Texas Bond Review Board provides data on the results of various local bond elections issued at the local water, health, or school districts, until the city or county level elections. It is the state agency responsible for debt financing to be used prudently to meet infrastructure needs and other public purposes. Hence, one of its goals is to ensure that the public has access to current information regarding the capital planning, finance, and debt management of the local government. For the purpose of this thesis, I use the dataset on school district bond election results. It contains information on the issuer district, election date, amount raised, and total votes for/against from years 1991 until 2019. Across that time span, more than 3200 elections have been held for a grand total of more than 160 billion dollars. From 1996 to 2016, there are 136 school bond elections held every year on average. The average amount authorized throughout the time period was almost $9000 per pupil. Throughout the time period, there also appears to be an increasing trend of amount authorized per pupil through bond elections, indicating the increasing importance of bond elections as a means of school finance used by district boards. By the early 2010s, school bond elections are responsible for authorizing more than $10000 of capital funds per pupil, which has increased from around $5000 per pupil during the late 1990s and early 2000s. There is a rather strong tendency for Texas residents to support increases in school expenditures since 73 percent of all bond elections was approved with 61 percent approval vote share on average from 1996 to 2016. Figure 1 shows the distribution of election vote shares throughout the time period. In addition, the average standard deviation of vote shares was 15 percent. Therefore, when evaluating close winners and losers in the RD design, this thesis approximately studies elections that are one standard deviation away from the mean. This has implications to the external validity of the analysis as the primary variation do not focus on the representative election, which will be further explained in Section IV. Compared to the California bond elections data used by Cellini et al. (2010), bond elections in Texas authorizes more funds per pupil and approve more bond passages. The average approval vote share of bond elections in Texas appears to be similar to California’s at around 60 percent, however there is greater variation of approval vote share in Texas.

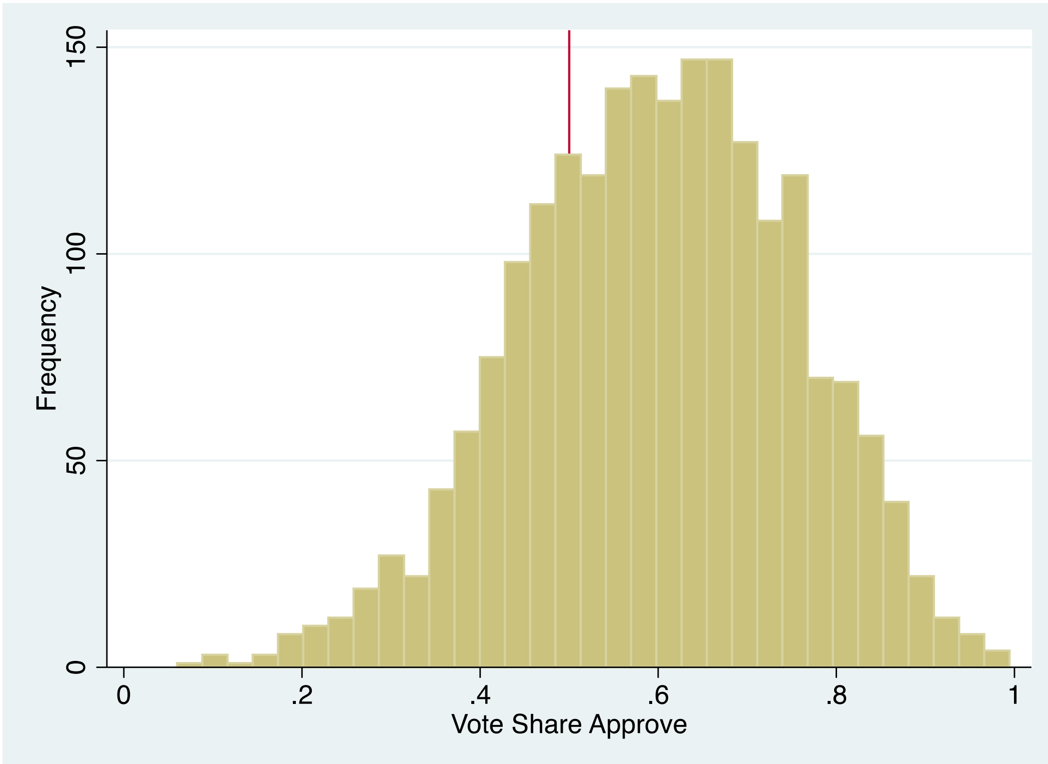

Figure 1. Frequency Distribution of Approval Vote Shares from School Bond Elections (1996 – 2016)

Detailed yearly school district information is obtained from the Texas Education Agency. One of the agency's products, Snapshot: School District Profiles, gives an overview of Texas' public education from years 1995 until 2017. From this data source, I am able to obtain information on students and teachers at school district level, which act as dependent variables of the analysis. First, it is noteworthy to analyze the impact of education investment on student performance. The dataset provides the dropout rate, percentage of students who pass college entry requirement and percentage of students passing Texas' standardized test (STAAR). Regarding teachers, I utilize the student-teacher ratio and various measures of teacher quality. The teacher quality of a school district is measured by their average years of experience and the turnover rate. To be specific, the turnover rate is defined as the percentage of teaching staff who separated from the school district throughout the year. Next, I use financial data on the total district expenditure per pupil, which is relevant to conduct the RD design. Finally, I make use of several measures of student characteristics regarding their race and socio-economic status to serve as time-varying controls for the specifications presented in the methodology, Section IV.

The main dependent variable of interest regarding the housing market must measure house prices. I use the House Price Index (HPI) as constructed by the Federal Housing Finance Agency (FHFA). The US-wide agency regulates entities in the housing market and ensures liquidity, stability and access in housing finance. The House Price Index measures the movement of single-family house prices at various geographic levels using information from repeat mortgage transactions. This means it measures price changes with repeat sales or refinancings on the same properties. This thesis uses the HPI at the census tract level that is measured annually from 1985, which serve as the base year, until 2018. Census tract level data is most preferred due to several factors. Notable reasons include its granularity and the construction of its official, exact and non-changing boundaries. For this thesis, each census tract is assigned to its corresponding school district, making house prices at the school district level as the average of the HPI of the collective census tracts.

To obtain summary statistics in Table 2, I simply regress the school district and house prices variables measured in the year before a school bond election on a dummy variable that takes value 1 if a bond authorization was approved, indicating the treatment group. The means of the outcome variables appears to be balanced as there are no significant differences between treatment and control group at pre-election. However, the treatment and control groups differ considerably in terms of the time-varying control variables measuring student characteristics. There is significantly more white students in control group districts, while there are significantly more Hispanic students in the treatment group. This may indicate that student characteristics are crucial time-varying confounders that must be controlled for in the specification of the analysis.

Table 2. Summary Statistics of Treatment and Control Group School Districts at One Year Prior to Bond Elections

Variables	**Treatment**		**Control**		**t**
	Mean	**Standard Deviation**	**Mean**	**Standard Deviation**	
Total Expenditure per Pupil	10160	5041	9953	3369	0.87
Student to Teacher Ratio	13.64	2.12	13.13	1.83	1.27
Dropout Rate	0.64	0.85	0.6	0.88	0.83
% Pass STAAR Test	76.68	10.01	75.54	10.37	1.76
% Pass College Entry Requirement	23.28	13.02	22.76	12.99	0.62

Average Teacher Experience	12.16	2.14	12.41	2.09	-1.48
Teacher Turnover Rate (%)	16.14	6.66	16.42	7.38	-0.63
% Students African American	7.91	10.07	8.19	12.39	-0.36
% Students Hispanic	34.92	25.63	29.95	25.37	3.11
% Student White	54.25	26.12	59.43	26.37	-3.13
% Student Economically Disadvantaged	48.25	20.39	50.12	20.25	-1.47
House Price Index	151.04	43.43	146.61	36.04	1.65
Number of Pupils	5313	9678	3562	4096	1.33

IV. Methodology

OLS— A natural starting point to study the effect of educational expenditures towards school outcomes, housing market outcomes, and household locational decisions is through an ordinary least-squares (OLS) regression. This can be formulated by the following equation:

$$Y_{dt} = \beta_0 + \beta_1 X_{dt} + \kappa_t + \gamma \boldsymbol{Z}_{dt} + \upsilon_{dt}$$

Here, I regress some potential outcome Y_{dt} of district d at time t on the main independent variable, education expenditures X_{dt}. To recap, I first study the impact of education expenditures on school outcomes regarding student performance and teaching quality. The relevant dependent variables in this area are the student dropout rate, percentage passing STAAR test, percentage passing college entry requirement, student-teacher ratio, and teachers' turnover rate and average years of experience. Next, the effect on the housing market is studied with the HPI as the main dependent variable. I take the logarithm of the HPI so that the effects on house prices can be interpreted as percentage changes.

To interpret β_1 of the OLS equation as the causal effect of education expenditures on outcome Y_{dt}, the main identification assumption of conditional independence (CI) must hold. That is, the independent variable X_{dt} must be uncorrelated with the error term u_{dt}, which captures other factors that affect outcome Y_{dt}. To account for this, I will include specifications that include year fixed effects κ_t and a vector of time-varying school district characteristics $\boldsymbol{Z}_{dt}$. As mentioned in the data subsection of Section III, the time-varying controls include student racial composition and socioeconomic status, which proxy the time-varying household characteristics of a school district. I withdraw from using district fixed effects since there is lack of variation within school districts, making it harsh for the dataset to handle.

Moreover, the RD design, which is the thesis' preferred empirical strategy, will control for time-invariant, district-specific confounders when close elections are examined. Hence, the set of controls will account for time-varying factors that are both general and district-specific, through the year fixed effects κ_t and vector $\boldsymbol{Z}_{dt}$. Robust standard errors will be reported to avoid obtaining biased standard errors due to heteroskedasticity. However, it can be argued that the OLS specifications do not yield a causal estimate and the CI assumption still does not hold. For instance, other unobserved time-varying district characteristics are not included in the vector $\boldsymbol{Z}_{dt}$. Moreover, time-invariant district-specific factors may be crucial as district fixed effects are excluded. Therefore, this thesis would go a step further to address these multiple sources of endogeneity bias by employing a RD design that proxies a random experiment.

RD— The main analysis of this thesis utilize a dynamic RD model that was first developed by Cellini et al. (2010), which was mentioned in Section II. I start by describing how a simple cross-sectional, static RD framework approximates a randomized experiment. Then, to enhance precision, I extend to a dynamic model by exploiting a panel data setup where bond authorization elections affect outcomes of subsequent years. With this specification, the interpretation of the dynamic RD estimator that I will use corresponds to the intent-to-treat (ITT), which will be explained further in this subsection.

As explained in the previous subsection, the coefficients of the OLS regressions are likely to contain endogeneity bias. To address this issue, applying a RD design essentially compares school districts that just barely pass a referendum to those that just barely fail, which makes vote share serve as the forcing variable v_d. A district whose vote share in an election exceed the majority of 50% is assigned the treatment group, while elections with vote shares below 50% is assigned the control group. In a narrow bandwidth around the 50% approval mark, variations in education expenditure can be considered random, resulting to a discontinuity when a bond proposal receives majority vote. Through this identification strategy, it is reasonable to exclude district fixed effects that controls for time-invariant district-specific variables since they are randomly distributed at both sides of the threshold. In other words, the correlation between election outcomes and unobserved district characteristics are arbitrarily close to zero by focusing on close elections. As mentioned in the previous subsection, assessing the robustness of the RD estimate with district fixed effects is difficult for the data to handle due to the lack of variations in the district level.

The static RD strategy can be formalized with the following equation:

$$Y_d = \theta_0 + \theta_1 B_d + f(v_d, \rho) + \varepsilon_d$$

When district d holds a bond election, some vote share v_d is realized. $B_d = 1(v_d > 0.5)$, which serve as a dummy variable that indicates the approval of a bond authorization. Therefore, θ_1 can be interpreted as the causal effect of approving a bond proposal towards some outcome Y_d when restricting the sample to close elections. The equation additionally controls vote share with a flexible polynomial with the corresponding parameters ρ.

The main identifying assumption of the RD design is continuity in the forcing variable and no systematic differences between school districts who just approved and declined a bond election. Manipulations or the presence of other jumps in other variables apart from authorization of bonds invalidates the research design. In principle, there should not exist any manipulations around the threshold by construction because school boards or the marginal voters cannot manipulate a school district's election outcome. It must also be noted that variations captured by the authorization of a bond around the approval threshold is local in nature. This means that it uses a subsample of complier districts who are almost indifferent from increases in education expenditures through the bond proposal, meaning that they are close to the 50% approval mark. As a result, the estimates of this empirical approach corresponds to a local average treatment effect (LATE). This means that the coefficient does not necessarily identify the effect of the representative school district, but rather identify effects from the sample of districts with vote shares narrowly around the threshold. Specifically, as mentioned in Section III, the average vote shares of elections throughout the period of study is around 61% with standard deviation of 15%. Therefore, the LATE uses the subsample of school district elections that are approximately one standard deviation away from the mean vote share.

Now, I extend the framework to a dynamic model where the passing of a referendum affects outcome in subsequent years beyond the year of election. Hence, variables are now indexed by calendar year t and the dummy variable B_{dt} equals 1 if a school district approves a referendum in year t and zero otherwise. Similar to Cellini et al. (2010), I assume that the partial effect of a bond authorization in one year towards outcomes on later years depend solely on elapsed time, holding bond issues in intermediate years constant. Thus, the outcome of district d at time t is modelled as a function of all prior bond referendum history represented by the following equation:

$$Y_{dt} = \sum_{\omega=0}^{\bar{\omega}} \left[\varphi_{1,\omega} B_{d,t-\omega} + f\left(v_{d,t-\omega}, \rho\right)\right] + \kappa_t + \gamma \boldsymbol{Z}_{dt} + u_{dt}$$

where ω is the number of years prior to the outcome where a referendum might be held, κ_t is a year fixed effect, and $\boldsymbol{Z}_{dt}$ is a vector of time-varying controls that measures student socio-economic characteristics. For the main analysis, I set $\bar{\omega} = 6$ since it is similarly used in Cellini et al. (2010) study. Also, I restrict the sample to the neighborhood bandwidth around 30% to 70% approval vote share and a linear specification for vote shares to identify the discontinuity. In this dynamic setting, the coefficients $\sum_{\omega=0}^{\bar{\omega}} \varphi_{1,\omega}$ represent effect of authorizing a bond at ω years prior towards an outcome at time t. For the scope of this thesis, the coefficients in this dynamic RD setup corresponds to ITT effects because it does not control for the district's behavior in subsequent years. This interpretation incorporates and captures the effects of $B_{d,t-\omega}$ also through intermediate variables $\{B_{d,t-\omega}, B_{d,t-\omega+1}, \dots, B_{d,t}\}$. In reality, districts who narrowly reject a bond proposal will be more likely to consider and pass a bond proposal in the future. Beyond the scope of this thesis, Cellini et al. (2010) further developed this dynamic RD design to control for district elections in subsequent years. This isolates the impact of authorizing a bond without subsequent changes in the district's budget constraint, which is commonly known as "treatment-on-treated" (TOT) effects.

In addition, I estimate the dynamic RD model above that substitutes the dummy for approving a bond election, $B_{d,t-\omega}$, with a continuous variable of the amount of bond authorized, $Q_{d,t-\omega}$. $Q_{d,t-\omega} = 0$ if the bond proposal is declined. This reports $\sum_{\omega=0}^{\bar{\omega}} \lambda_{1,\omega}$ as the effects of authorizing $1000 of schooling expenditures per pupil in bonds, which can be compared to the baseline OLS estimates. This can be represented with the equation below:

$$Y_{dt} = \sum_{\omega=0}^{\bar{\omega}} \left[\lambda_{1,\omega} Q_{d,t-\omega} + f\left(v_{d,t-\omega}, \rho\right)\right] + \kappa_t + \gamma \boldsymbol{Z}_{dt} + \mu_{dt}$$

I apply the two dynamic RD equation above towards several outcomes. First, I check if bond authorizations result in a discontinuity in total school expenditures per pupil of school districts. Here, I expect for the magnitude of coefficients $\sum_{\omega=0}^{\bar{\omega}} \varphi_{1,\omega}$ will approximately equal the average proposed bond amount. Next, the models are applied to look at school outcomes: student-to-teacher ratio, percent of students passing Texas standardized test, percent of students passing college entry requirement, student dropout rate, average teacher experience, and teacher turnover rate. Finally, I apply the dynamic RD models to the logarithm of house price index (HPI), which is the main dependent variable of the analysis. This evaluates the efficiency of education expenditures of school districts in Texas. Similar to the OLS strategy, I calculate robust standard errors when estimating the RD coefficients to obtain a more conservative, unbiased standard errors that approaches the homoskedasticity assumption.

For the last subsection in Section V, I apply additional measures to test the validity and robustness of the RD design and its results. One common threat to identification in a RD setup is the presence of underlying nonlinearities between the forcing variable and its outcomes. To evaluate this, I explore the sensitivity of coefficients $\sum_{\omega=0}^{\overline{\omega}} \varphi_{1,\omega}$ to variations in the order of polynomial of vote shares from a first-order to third-order specification. In addition, I test the robustness to variations in the bandwidth sizes around the threshold to further explore the estimates' vulnerability from picking up underlying nonlinearities. Lastly, I use different lags of the independent variable in the dynamic RD design to check whether the estimates capture a degree of cyclicality in bond elections.

V. Results

In this section, the results of the analysis will be presented through several divisions. First, I present the relationship between school expenditure towards the relevant school outcomes and house prices using the OLS identification strategy. The OLS estimates serve as the baseline comparison for the RD setup. Then, the results of the dynamic RD design will be explored. I first show that bond elections result to higher total expenditures per pupil when comparing close winners and close losers school districts. Next, I use the same RD specifications to explore the effects of authorizing a bond towards several measurable school outcomes and house prices. As mentioned in Section IV, I investigate the effects of authorizing a bond as a dummy and the effects of authorizing an amount of $1000 in bonds.

OLS— In this subsection, I evaluate the relationship between total school district expenditure per pupil towards school and housing market outcomes using OLS. First, the correlation between the variables is shown. Then, I observe the changes in the estimates when year fixed effects and time-varying controls for student characteristics are added.

Table 3 shows the β_1 coefficients of the OLS specifications introduced in Section IV. Total school district expenditure per pupil are expressed per thousand dollars. Note that, on average, school districts spend around $10000 per pupil yearly. To recap, the relevant variables to proxy school quality will measure the student-to-teacher ratio, the performance of students (percentage passing standardized test, percentage passing college entry requirement, and dropout rate), and the quality of teachers (average teacher experience and teacher turnover rate). Overall, the relationship between school spending and the relevant outcome variables are considerably robust in terms of magnitude and significance after the introduction of both sets of controls. For brevity, I only report the coefficients of the preferred OLS specification that controls for general time trend and district-specific time-varying factors. The estimates of the preferred OLS specification tends to either be the most conservative or be in the middle relative to the specification without controls or the specification that only include year dummies.

Firstly, with regards to the student-teacher ratio, an increase of $1000 in expenditures per pupil leads to around 0.3 less students per teacher, on average. Hence, districts need to spend around $3000 to have one less student per teacher. For variables measuring student performance, an increase in school expenditures tend to result in significantly more favorable student outcomes except for college entry outcomes. However, even though statistically significant, the magnitudes of the coefficients are small to be meaningful in the real world context. An increase of $1000 in total expenditures per pupil lead to changes of less than 1% among the student performance outcomes. On average, there is a reduction in the dropout rate by 0.02% and an improvement in the percentage of students passing Texas' standardized test by 0.1% for every increase of $1000 in total expenditures per pupil. Moreover, the percentage of students that pass college entry requirement worsened by 0.1%, on average, when expenditures per pupil increase by $1000. Regarding the teacher outcomes of school districts, OLS estimates show that increasing school spending leads to improvement of teacher quality in terms of experience and turnover. Unlike the effect on student outcomes, the estimates for teacher quality are relatively large to be relevant in the real world context. An increase in $1000 of per pupil school expenditures improves the average years of experience of teachers employed by 0.1 years on average. In addition, when looking at teacher turnover, an increase in $1000 of total expenditures per pupil lead to a reduction in the turnover rate by almost 0.1% on average. In conclusion, looking at school outcomes, the OLS specification generally leads favorable student and teacher outcomes when school spending increases; though, some coefficients tend to be small to be meaningfully interpreted in the real world. This holds for all variables except for the student performance variable with regards to college entry. Nonetheless, it should be noted that these estimates should not be interpreted as causal effects since the identifying assumption of conditional independence is unlikely to be met even after controlling for general time trends and district-specific time-varying characteristics. The current set of controls may still ignore vital unobserved components, which results to biased, endogenous OLS estimates.

Finally, I also use OLS to investigate the effects of total expenditures towards log house prices, which is the main outcome variable that tests the efficiency of school district spending. The preferred OLS specification indicates that a $1000 increase in per pupil spending increases house prices by 0.009%, though the coefficient is not statistically significant. Relating back to the theory in Section II, the OLS estimates show that school expenditures are efficiently provided as residents' value of the increase in spending equals the future tax burden they incur, cancelling out the effects towards house price capitalization. Even though the OLS coefficients towards measurable school outcomes are small to be meaningfully interpreted, the increase in total expenditures from authorizing bonds may materialize in other unmeasurable criteria, such as better school aesthetic, safety, and the like, which may still be valued by district residents and be incorporated towards their willingness to pay. All in all, the estimates from the OLS specifications will serve as the baseline comparison for applying the dynamic RD design.

Table 3. OLS Estimates of School Quality Outcomes on Total School Expenditure per Pupil (000s)

Dependent Variable	Coefficient β_1		
Student-Teacher Ratio	**-0.24***** (0.098)	**-0.31***** (0.015)	**-0.27***** (0.014)
• N	27113	27113	27113
• F-stat	607.92	26.63	162.40
% Pass STAAR Test	**0.18***** (0.018)	**0.23***** (0.022)	**.10***** (0.020)
• N	27017	27017	27017
• F-stat	90.97	203.87	610.37
Dropout Rate (%)	**-0.039***** (0.003)	**-0.028***** (0.003)	**-.020***** (0.003)
• N	25268	25268	25268
• F-stat	198.27	154.19	135.21
% Pass College Entry Requirement	**-0.064***** (0.020)	**-0.13***** (0.028)	**-.11***** (0.023)

• N	22538	22538	22538
• F-stat	10.29	33.17	426.99
Average Teacher Experience	**0.13****** (0.007)	**0.16****** (0.011)	**.12****** (0.009)
• N	27145	27145	27145
• F-stat	312.14	12.44	178.05
Teacher Turnover Rate	**-0.038****** (0.017)	**-0.20****** (0.023)	**-.071****** (0.020)
• N	26984	26984	26984
• F-stat	5.00	74.71	132.38
Log House Price Index	**0.018****** (0.001)	0.0005 (0.0005)	0.0009 (0.0005)
• N	12256	12256	12256
• F-stat	225.05	361.32	347.73
Controls:			
• Year fixed effects	-	√	√
• Time-varying controls vector	-	-	√

Note. Independent Variable: Total Expenditure per Pupil (000s);
***: significant at 0.01 level;
**: significant at 0.05 level;
*: significant at 0.1 level;
Parentheses shows standard errors.

RD on Total Expenditures— Table 4 provides the ITT effects of authorizing a bond proposal towards total school expenditures per pupil using the dynamic RD equation presented in Section IV. I provide the effects over six years following an election, which is similar to the specification used by Cellini et al. (2010). As shown, there is no indication that bond elections significantly affect current total spending. However, large, significant increases in total expenditures are observed during the first, second, and third year following an election. This magnitudes of these effects are relatively robust after the introduction of year fixed effects, time-varying controls, and both simultaneously. In the first year following an authorization of a bond, per pupil expenditures of school districts optimistically increase up to $4000. In the second year, school spending further increases by $3000 per pupil. Finally, the third year following an election is the final year where the effects of bond authorization is significant. Here, total per pupil school district spending increases by around $2000. The effects on total spending fades out after the fourth until sixth year following elections. Even though the magnitudes are positive, confidence intervals rule out significant effects at the 5% level. The graphical analysis on the effect of bond authorizations towards total expenditures per pupil is shown in Figure A of Appendix 3. The figure shows the effects on total expenditures per pupil at two years after the elections, $\omega = 2$, by plotting the residuals of the RD specification with vote shares.

Table 4. The Effect of Bond Authorization on Total Expenditures per Pupil using an RD Design

Main Independent Variable (Bond Authorized)	Coefficient φ_1		
Lag 0 ($\omega = 0$)	-231.49 (404.40)	-331.42 (358.61)	-427.65 (353.21)
Lag 1 ($\omega = 1$)	**3319.35**** (734.61)	**4130.72***** (1546.65)	**3983.27***** (1463.69)
Lag 2 ($\omega = 2$)	**2947.11***** (890.44)	**3219.11***** (655.04)	**3034.11***** (650.04)
Lag 3 ($\omega = 3$)	**1822.21**** (836.49)	**2057.82***** (781.87)	**1972.84***** (746.47)
Lag 4 ($\omega = 4$)	764.86 (940.71)	772.55 (774.12)	965.68 (815.42)
Lag 5 ($\omega = 5$)	286.46 (910.84)	905.95 (894.90)	624.07 (940.50)
Lag 6 ($\omega = 6$)	470.63 (729.74)	256.29 (661.38)	484.17 (663.34)
Controls:			
• Year fixed effects	-	√	√
• Time-varying controls vector	-	-	√
Model Summary:			
• N	848	848	848
• F-stat	2.24	24.72	19.87

Note. Dependent Variable: Total Expenditure per Pupil;
***: significant at 0.01 level;
**: significant at 0.05 level;
*: significant at 0.1 level;
Parentheses shows standard errors.

Across the three dynamic RD specifications, the equation including year fixed effects and time-varying student characteristics vector consistently show the moderate estimate among the significant years. Excluding both sets of controls most often provide the conservative estimate, while only including year fixed effects show optimistic effects in many of the lags. It is important to note that the significant fiscal effects of authorizing a bond in years one to three after elections total to approximately $9000 of spending per pupil. Alluding to Table 1, the average bond amount proposed throughout the period of study is was also close the $9000. This equivalence may provide support to the validity of the dynamic RD design, which means that it is less likely for the empirical strategy to capture spurious effects. Moreover, compared to the ITT bond authorization effects on school spending found in Cellini et al. (2010) study, the significant fiscal effects I found in this thesis appear one year earlier. In the setting of California, fiscal effects are most significant in years two until four following an election, while it is during years one to three where fiscal effects are most significant in the case of Texas school bond elections. Even though the timing is different, the time span where fiscal effects are significant is similar at three years, which shows some resemblance across the two studies. All in all, this subsection shows that, using the dynamic RD empirical strategy, bond authorizations do affect fiscal outcomes of school districts. Specifically, the funds raised through approving a bond proposal appears to materialize one-to-one in total per pupil expenditures of school districts in Texas within four years after elections.

RD on School District Outcomes— Appendix A provides the ITT effects towards school outcomes when the independent variable is set as a dummy when a bond proposal is approved through an election. Appendix B shows the estimates when the independent variable is a continuous variable of the approved amount of the bond passage. Surprisingly, the magnitudes of many of the coefficients diverge across the two specifications, along with its significance. First, I highlight the general trends throughout the lags and also the coefficient estimates that are most robust. Then, I will further elaborate in detail the effects of authorizing school bonds on each specific school outcome.

Many of the effects on school outcomes are small and insignificant for the first several years. This is expected given that capital projects financed by bond elections take time to execute, hence the flow towards school outcomes tend to not begin in the first several years. The effects towards student performance tend to improve, while the effects towards teacher quality tend to worsen over the time period of study. It is important, though, to note that the statistical significance of the coefficients are limited and the magnitude are relatively small to be meaningful interpreted, which is a similar case in the OLS specifications. The most robust estimates across both independent variable specifications are at outcomes of percentage of students passing college requirement and average teacher experience. Authorizing school bonds in general significantly increase the percentage of students in a school district to pass college entry requirement by almost 10%-points. Per $1000 per pupil raised through bonds, this translates to 0.7%-points more students passing college requirement. This means that districts have to spend more than $10000 per pupil, which is twice the average expenditure per pupil, to increase the relative number of students passing college entry requirement by 7%. This finding is robust and materialize six years following an election. In absolute terms, given that the average district size in the RD sample is around 7400 pupils, districts have to raise around $19 per pupil in bonds for an additional student to pass the college entry requirement. Another similarly robust finding is the negative effect of authorizing bonds towards teachers experience. Approving bond proposals in general significantly reduce average teacher experience by around one year in the fourth and fifth year following elections, totaling up to two full years of less experience. However, when translated to per $1000 raised in bonds per pupil, the effect on teacher experience is only significant in the sixth year after elections where it decreases significantly by around 0.05-0.07 years.

Besides the two variable above, college entry outcomes and teacher experience, the results towards other school outcomes diverge in terms of significance and, for some outcomes, also in terms of magnitude when comparing the two RD specifications. First, looking at teacher turnover, authorizing bonds in general increases the turnover rate by almost 3%-points in the second and third year following elections and up to 4%-points in the fourth lag. The significance of the coefficients at the fourth lag is robust to the introduction of all sets of controls, while the coefficients at the second and third lag is significant after the controlling for both year fixed effects and time-varying school district characteristics. However, when the independent variable is set as continuous, the coefficients lose statistical significance, leading to mixed results. In addition, the effect on dropout rate shows similar results. The dummy when bonds are authorized do not significantly affect students' dropout rate at all lags. However, when the explanatory variable is expressed as the amount authorized, approving $1000 per pupil in bonds decrease the dropout rate significantly at the year of the election and five years after. It is important to consider, though, that the magnitude towards dropout rate is small to be meaningful since it is affected by less than 0.01% for every $1000 bonds per pupil authorized.

Furthermore, the ITT school bond effects on outcomes of student-to-teacher ratio and percentage of students passing STAAR test appears to diverge across the two RD specifications. Appendix 1 Table A shows that authorizing bonds in general tend to result to more students per teachers, but only significant four years after elections at one extra student per teacher. However, when the independent variable is set to the amount of bond authorized, there seems to be a negative effect. Here, the specification indicates that there are 0.04 significantly less student per teacher on the election year when $1000 are authorized in bonds per pupil. Again, the magnitude is also small to be relevant beyond the statistical context. Hence, the diverging results on student-to-teacher ratio makes it difficult to construct meaningful interpretations. Finally, diverging results also appear when studying the percentage of students passing Texas' standardized test, though less polarizing. Appendix 1 Table B shows mostly statistically insignificant effects of authorizing bonds towards STAAR test results. Though, most coefficients were negative and statistically significant at 10% level in the fourth lag. On the other hand, the specification in Appendix 2 Table B shows that the percentage of students passing the standardized test increases for every $1000 of bonds authorized per pupil. The increase in the percentage is significant at the year of the election, the fourth and fifth year after elections with magnitudes ranging from 0.1-0.3%-points. Again, even though many of the coefficients are significant, the diverging result and the small magnitude also makes it irrelevant to extrapolate to the real world context.

Overall, the coefficients from the dynamic RD specifications differ considerably to the preferred OLS specifications. First, OLS overestimates the effect of expenditures towards the dropout rate, but underestimates the effect towards percentage of students passing STAAR test. In addition, the dynamic RD design presents opposite coefficients for college entry outcomes, teacher experience, and teacher turnover rate compared to OLS coefficients. The preferred RD methodology indicate that school district college entry outcomes improved, while teacher experience and teacher turnover rate worsened upon the authorization of school bonds. To summarize, school bond elections in Texas are primarily used to finance capital projects and facility investments such as building renovations, land acquisition, and the like. With this, the dynamic RD design show that the spike in capital expenditure from bond authorizations are at the expense of the district's teacher quality. This result is supported by the increased turnover rate in the second to fourth year after bond elections. Moreover, the findings show that the remaining teachers tend to have lesser years of experience. Hence, as capital expenditures increase from bond authorizations, the results show that districts tend to lay-off experienced teachers and keep or hire lower-paid, less-experienced teachers to maintain its financial wellbeing. Fortunately, however, these cost-saving measures on teacher quality do not negatively affect the student performance in the district. In fact, some measures of student performance improved upon bond authorizations, specifically on college entry outcomes and dropout rate, although the magnitudes towards these outcomes are relatively small. This may imply that the potential decrease in student performance from poor teaching quality is offset by the increase in student performance deriving from the school district's capital improvements.

RD on House Price Index— In this subsection, I apply the same dynamic RD framework towards log house price index (HPI) as the main outcome variable. Looking at Table 5, when the independent variable is set as a dummy, the most robust and significant result indicate that bond authorizations materialize in house prices only in the sixth year following an election. At the sixth lag, the HPI significantly increases by approximately 11-16% and is robust to the introduction of various sets of controls. Evaluating the magnitudes, house prices tend to decrease from the year of bond elections up until the year after, although the coefficients are not statistically significant. In the second and third year following bond elections, house prices tend to increase and the coefficients become significant in some specifications. Across the three specifications, house prices may optimistically rise to almost 7% at the second year and 14% at the third year. Moreover, the results show that house prices then stagnates at the fourth and fifth year following an election as none of the coefficients are significant throughout the different specifications. The graphical analysis on the effect of school bond authorizations towards log house prices is shown in Figure B of Appendix 3. The figure shows the effects on log house prices at two years after an election, $\omega = 2$, by plotting the residuals of the RD specification with the approval vote share of lagged two years.

Table 5. The Effect of Bond Authorization on Log House Price Index using an RD Design

Main Independent Variable (Bond Authorized)	**Coefficient φ_1**		
Lag 0 ($\omega = 0$)	-0.022 (0.032)	-0.014 (0.027)	-0.024 (0.026)
Lag 1 ($\omega = 1$)	-0.066 (0.075)	-0.003 (0.063)	-0.008 (0.063)
Lag 2 ($\omega = 2$)	0.078 (0.068)	0.029 (0.056)	**0.068*** (0.039)
Lag 3 ($\omega = 3$)	0.091 (0.079)	**0.14**** (0.065)	0.048 (0.053)
Lag 4 ($\omega = 4$)	0.029 (0.091)	0.065 (0.078)	0.024 (0.081)
Lag 5 ($\omega = 5$)	-0.016	0.011	-0.034

	(0.094)	(0.083)	(0.078)
Lag 6 ($\omega = 6$)	**0.13**** (0.063)	**0.16***** (0.052)	**0.11**** (0.042)
Controls:			
• Year fixed effects	-	√	√
• Time-varying controls vector	-	-	√
Model Summary:			
• N	755	755	642
• F-stat	1.65	15.01	11.46

Note. Dependent Variable: Log House Price Index (HPI)
***: significant at 0.01 level;
**: significant at 0.05 level;
*: significant at 0.1 level;
Parentheses shows standard errors.

Similar to the previous subsection, Table 6 can be interpreted more meaningfully as I study the house prices effects of authorizing $1000 per pupil in bonds. These coefficients can then be compared to the baselines estimates presented in the OLS subsection. Unlike on school outcomes, when the independent variable is set continuous based on the amount authorized, the results show more similarities when compared to specifications in Table 5. First, a similar result emerge where the most robust and significant effects appear in the sixth lag. Here, for every $1000 authorized in bonds per pupil, house prices increase by 0.3-0.6%, which are significant at least at the 10% level. Moreover, another robust and significant result emerge at the third lag, which did not appear when the independent variable was a dummy for authorized bonds. The result shows that, at the third year following an election, house prices increase by 0.4-0.7% for every $1000 of bonds authorized per pupil. Looking at each lag, the authorization of $1000 of bonds per pupil do not significantly affect house prices at the year of the election and one year after. Only after the introduction of year fixed effects and time-varying controls, house prices are positively affected two years after the election, which rises by up to 0.5%. Four years after an election, house prices may rise up to 0.6% on average when $1000 in bonds per pupil is authorized, however the coefficient becomes insignificant after the introduction of both sets of controls. Moreover, house prices are not significantly affected at the fifth lag, even though coefficients have negative magnitudes that relatively small.