Integrating Schools Through Affordable Housing : The Role of Progressive Public Finance *

Kenneth Whaley†

Abstract

This paper studies the provision of public schools facing income integration during a dynamic education policy environment. Using campus level data for 3,923 K-12 schools in Texas, I first show that school spending has become substantially more progressive over time, in line with national trends. To fill a gap in the literature regarding the consequences of regressive and progressive school spending, I ask the following counterfactual question. How would the school spending response to neighborhood change differ, had the policy environment in Texas remained regressive? Neighborhood composition shocks are identified by affordable housing construction that increases enrollment at the average school by 72 students (7%), of which ≈ 60 are predicted to be income qualifying for subsidized lunch. The composition shock depends on initial neighborhood income, and at previously low need (high income) schools, the share of high need students increases by 10-12 basis points. When regressive education policy links school spending to neighborhood income, my results establish that sorting of high income households away from affordable housing causes a near 35% decline in school spending. Put another way, progressive education policy is required to ensure adequate support for high need students that move to low need schools.

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*Thank you to Texas Department of Housing and Community Affairs and Texas Education Agency.
†Department of Economics, University of South Florida. kennethwhaley@usf.edu

1 Introduction

Residential segregation combined with increasing income inequality poses a threat to the equitable provision of public services, like education (Reardon and Bischoff 2011, Boustan and Margo 2013, Reardon 2013). Considering the role of public resources in neighborhood quality and lifetime outcomes, economists have conducted large scale policy experiments to understand how access to higher income neighborhoods translate to a better set of opportunities (Katz et al. 2001, Ludwig et al. 2008, Chetty et al. 2016, Aliprantis 2017). In practice, the Low-Income Housing Tax Credit Program (LIHTC) spends over $8 billion annually and is the primary policy tool supporting construction of affordable rental housing in a wide variety of neighborhoods (Hollar 2019, Keightley and Stupak 2020).

The introduction of new residents to a neighborhood via housing policy allows us to think more broadly about the consequences of neighborhood change. Building on two core public finance predictions, when decentralized public services play a role in where people choose to live, households need not have strong preferences for income segregation for a market to end up in a segregated state (Tiebout 1956, Schelling 1971, Card et al. 2008). With existing evidence that neighborhood income is linked to public good quality (Boustan 2013, Banzhaf and Walsh 2013) and education spending (Ajilore 2013), there is a gap in understanding how sorting responses to neighborhood change affect public services for residents who remain in the neighborhood, and new residents that arrive.

This paper asks how changes to neighborhood income composition affect the provision of local public schools. Neighborhood change is predicted to impact public schools in part because parents care about the peer group of students, which may induce sorting (Caetano and Maheshri 2017, Caetano and Maheshri 2023). Further, school spending in many states remains tied to local property tax revenues inherently linked to neighborhood demand and house prices (Brueckner 1982, Bayer et al. 2020b, Bayer et al. 2020a). Research in the discussion of equity in public school finance includes how property taxes are used for school spending, and shows that the US has generally become more progressive over time (Wilson et al. 2006, Gordon and Reber 2022, Rauscher and Shen 2022). Such policy changes are potential forms of omitted variables bias in this setting, as neighborhood change is a dynamic process and the policy environment is not fixed over time. While some features of a decentralized public school system may go largely unobserved, the increasing availability of school level data allows for deeper insight into the nature of local school financing (Wilson et al. 2006, Chingos and Blagg 2017, Blagg et al. 2022).

In this vein I employ data describing per-pupil spending, student income composition, enrollment and teacher counts of 3,923 K-12 schools in Texas from 2000-2020. Campus level data is advantageous to district level data for this study as I can model within-district spending variation as an outcome of within-district income variation across schools. Estimating the relationship between per-pupil spending and the share of students that income qualify for free or reduced price lunch, I find the curve to be inverse U-shaped in 2000 (regressive), and strictly increasing by 2020 (progressive). I argue that this relationship is the empirical approximation to school district funding formulas that determine spending for each school in a given year. Although this increasing progressivity reflects Texas specific changes in education policy, I am able to advance the literature through insights about the school spending response to neighborhood change, in the general case of regressive and progressive policy environments.

I next construct counterfactual measures of spending for each school×year observation in my sample. To do so I take the observed share of income qualifying students in a given year to predict per-pupil spending based on the aggregate spending curves obtained in the within-district analysis for the year 2000 (regressive) and 2020 (progressive). I am left with three 21-year panels of per-pupil spending data for each school. One measures the observed spending at a school in a given year. The second measures the level of spending for a school in a given year, had the policy environment remained fixed to the regressive structure from 2000. The third measures the level of spending at a school in a given year, had the most progressive structure of 2020 been in place for the entire sample period. The two counterfactual spending measures allow me to test for the spending response to neighborhood change, holding the policy environment fixed.

For identification of neighborhood change I take the announcement of LIHTC project approval as a source of variation in neighborhood composition. Since one-third of all US LIHTC units are estimated to house at least one child under 18 (Hollar 2019), and program incentives increase in the share of units reserved for income-qualifying tenants (Keightley and Stupak 2020), there is reason to believe new developments will impact schools. The literature also suggests that the dynamic effect of LIHTC is to introduce new residents through rental housing take-up and subsequent sorting (Baum-Snow and Marion 2009,Diamond and McQuade 2019). Mapping new Texas LIHTC units to school attendance boundaries, my econometric strategy is to compare outcomes for 838 schools that receive at least one new LIHTC development from 2000-2020 to those that do not. This count of LIHTC during my sample only includes developments where the majority of units are designated for families, following the Atkins and O'Regan (2014) classification method that distinguishes family from senior designated housing.

Using an event-study design that takes LIHTC announcement as the event year, I find statistically significant increases in enrollment (7% or 72 students), teacher headcounts (3-4% or 3 teachers) and income qualifying enrollment share (3 basis points or 61 students) in the years following new LIHTC approval. When categorizing schools above a 50% share of income qualifying students as a high need school, there is no distinguishable difference in the effect of LIHTC on enrollment and teacher counts relative to low need schools. I explore this margin of heterogeneity further, and document substantial differences in the composition change following LIHTC. The share of income qualifying students increases 10-12 basis points at previously low need schools, meaning the new batch of students in the years after LIHTC have a larger share of high need children. The opposite pattern is true for initially high need schools as the income qualifying share declines. The analysis of school composition shocks by LIHTC are corroborated by a literature describing similar heterogeneity in changes to overall neighborhood composition and house prices (Baum-Snow and Marion 2009, Diamond and McQuade 2019).

I turn to estimating the model taking the observed per-pupil spending measure as an outcome. The challenge is that time-varying policy changes will bias estimates of the spending response to LIHTC in the form of cohort effects (Sun and Abraham 2021, Goodman-Bacon 2021). In this setting I find a null estimate for the observed per-pupil spending response to LIHTC in my event-study model. With evidence that LIHTC changes neighborhood composition, which translates to changes in school composition, one might expect increased federal funding that follows income-qualifying students. While it is entirely possible that federal funding crowds out state and local funding on average, I argue that the null result for the spending response is more likely due to attenuation bias.

Next, the counterfactual regressive spending measure is taken as an outcome in the event-study model. Recall that the regressive spending measure maps observed income-qualifying shares at each school in a given year to an inverse U-shaped curve relating per-pupil spending to income composition. Thus the composition shocks from LIHTC are akin to movements along the curve. Further, there exists a threshold income qualifying share in which a small increase would cause per-pupil spending to decline. The threshold is at roughly 50% for the year 2000, so I continue to explore heterogeneity in the spending response for low need and high need schools. Since initially low need schools are expected to have identifiable increases to the income qualifying share, at least some of those schools would have predictable spending declines. I find that low need schools will incur substantial declines in school spending under regressive funding environments. High need schools, which have a smaller composition shock in the opposite direction, would have no identifiable changes in school spending.

My results show that initially low need schools exposed to neighborhood income shocks have averted substantial declines in school spending purely because of progressive policy changes. By taking the difference of the progressive and regressive spending measures, I am able to analyze who gains the most from policy changes over time. In doing so I find that policy changes caused school spending to be 35% higher at low need schools exposed to LIHTC, relative to a world where the policy environment was held constant. High need schools, where the LIHTC composition shock was small, did not incur any differential spending effects caused by policy change. If anything, because LIHTC is predicted to increase local incomes near high need schools, per-pupil spending would potentially decrease in progressive environments as less students of need enroll.

By first tracing out differential composition changes to schools that receive new LIHTC, I extend a literature that explores housing options as access mechanisms to middle and high income schools (Di and Murdoch 2013, Ihlanfeldt and Mayock 2019). Unique to affordable housing construction in high income (low need) areas is that subsequent increases in per-pupil spending are required to support a new batch of students with higher needs. My contribution is to show such a spending response only occurs in progressive policy environments, because the sorting of high income households away from LIHTC development would otherwise cause per-pupil spending to decline. Put another way, the idea that housing policy can move students to opportunity is undermined in regressive policy environments, as the resources available to new students upon arrival will begin to decline. With empirical evidence that school spending matters (Hoxby 2001, Jackson et al. 2016, Lafortune et al. 2018), and households are willing to pay for better financed schools Barrow and Rouse 2004, Cellini et al. 2010, Bayer et al. 2020a), housing policy combined with regressive school financing can have negative long-run consequences for the quality of a neighborhood.

Where to build affordable housing is a fundamental question in LIHTC program evaluation (Baum-Snow and Marion 2009). The breadth of the program has drawn economists to explain how LIHTC affects neighborhoods through private real estate markets (Ellen et al. 2007, Eriksen and Rosenthal 2010), and a variety of neighborhood amenities (Freedman and Owens 2011, Freedman and McGavock 2015). Di and Murdoch (2013) are among the only studies of school quality responses to the construction of LIHTC, finding that school ratings on average do not change using data from Texas during the first half of my sample period.

In computing the net welfare gains from LIHTC, Diamond and McQuade (2019) finds large welfare gains to low-income areas, and relatively small welfare losses in high-income areas following the construction of LIHTC. My results suggest increased per-pupil spending from the construction of new LIHTC is likely backed by Federal, not local revenue increases. However, I am not able to rule out additional local income required for absolute spending increases that come from larger enrollments requiring more teachers.

The remainder of this paper is as follows. In Section 2 I describe the data and show the presence of regressive and progressive policy environments using raw data. In Section 3 I describe the research design in detail and develop the counterfactual spending measures. In Section 4 I illustrate the school composition shocks from LIHTC using event-study models, and in Section 5 I trace out the school spending response to neighborhood change. In both Sections 4 and 5 I conduct placebo tests using the timing of senior designated LIHTC construction, which is presumed to have no effect on schools. I conclude the paper and summarize the policy implications in Section 6.

2 Background and School Data

2.1 Funding Formulas

The headline for US public schools shows expenditures top a whopping $700 billion a year to educate over 50 million K-12 students(Hussar et al. 2020). Federal funding comprises the smallest share of revenue for a large majority of districts, and the reliance on state or local revenues for the largest share varies by state. In Texas, local property taxes provide 50% of school district revenue, 44% comes from the state, and the remaining 6% from the federal government.[1] School district reliance on property taxes has long been a point of contention, sparking a wave of state finance reforms to address disparities in potential district revenue based on property values (Hoxby 2001, Jackson et al. 2016,Lafortune et al. 2018, Bayer et al.

[1] 2020 Texas Public School Finance Overview: Annual Report

2020b).

Districts manage federal, state and local tax revenues by allocating dollars for schools to spend. The substantial variation in per-pupil spending and across schools in the same district has been well documented (Roza et al. 2004, Owens et al. 2016, Chingos and Blagg 2017). The allocation process is idiosyncratic but can be described by a funding formula that considers enrollment counts, a base allotment for each student, and additional funding for student populations requiring more resources. This includes non-native English speakers, students in poverty, special education students, and those in gifted and talented programs. In Texas, the base allotment depends on grade level and is adjusted upwards for students requiring more resources and downwards if average daily attendance decreases.[2] Once a school is in receipt of funds, the principal and school administrators have discretion over how the money is spent.

2.2 School Data

The first major component of my data is a sample of K-12 schools at various grade levels in Texas from 2000-2020. The primary source of school level data is publicly available through the Texas Education Agency Academic Excellence System (2000-2012) and Academic Performance Reporting (2013-2020). The data is at the campus level, allowing me to exploit rich variation in school spending and student composition across neighborhoods within a school district. For each school I observe total spending, instructional spending, enrollment and racial demographics for annually beginning in 2000. Instructional spending is defined as activities that deal directly with the interaction between teachers and students. Salaries for teachers and classroom aides are included along with technology to deliver remote learning. All expenditure values are deflated to the year 2015 and are expressed in per-pupil terms by simply dividing the the total by the enrollment each year.

The school-level data is augmented with teacher counts and counts of students receiving

[2] Appendix Figure B3 is an example of this process for Houston Independent School District, one of the largest school districts in Texas.

free or reduced lunch provided by the National Center of Education Statistics. Throughout the analysis of this paper I use the fraction of students receiving free or reduced lunch as a measure of school income composition. The benefit of this measure is the annual frequency, which allows me to observe the relationship between school spending and neighborhood changes within the boundary over time. While the measure is not a perfect representation of neighborhood income, a contribution of this paper is to link school demographic change to local neighborhood changes.

The final sample is restricted to non-charter, non-open enrollment schools with available attendance zone boundary information. [3] This yields a panel of 3,385 schools in Texas from 2000 to 2020. 2,594 schools in the sample (75%) are elementary or middle schools, and the remainder are high-schools.[4] Summary stats are reported in Table 1. Column one of Table 1 describes all schools in the sample, a total of 71,080 school $\times$ year observations. Column two includes observations in which less than half of enrollment is income qualifying, or *low need* schools. Column three includes schools with greater than 50% of the enrollment as income qualifying, or *high need* schools.

high need schools on average spend about 10% more than high need schools during the sample period. While this is potentially a byproduct of urban geography, rows 2 and 3 of Table 1 shows that high need schools have smaller enrollments on average and three times the share of income qualifying students. Class sizes are statistically indistinguishable across school type, but high need schools serve two times the share of Latino students and a larger share of Black students as well. As a border state, Texas population is estimated to be 39% Latino in 2015, well above the national average of 17.9% in the same year. Second, over 72% of people in Texas reside in or around 6 cities, with the Dallas and Houston metro areas containing approximately half of the total population.[5]

[3] I also exclude any schools that experienced LIHTC construction prior to the year 2000. This sample restriction removes less than 40 schools. I detail the LIHTC data in section **??**

[4] The elementary and middle school category are those where the maximum grade level is 8th grade. All others are categorized as high schools.

[5] Source: Texas.gov. Texas Demographics 2015

3 Research Design

This section details how I leverage the data to analyze the school spending response to neighborhood composition changes. The baseline relationship of interest is

$$S_{ijt} = g(F_{ijt}, X_{ijt}), \tag{1}$$

where S_{ijt} is per-pupil spending and F_{ijt} is the share of students that receiving free or reduced lunch subsidies through income qualification. X_{ijt} is a vector of other observed predictors of per-pupil spending that include enrollment size and the share of Latino students as a proxy for second language learning services that may correlate with higher school spending.

Figure 1 lays out the raw variation in school spending across the distribution of F_{ijt}, with left panel displaying data for the year 2000 and the right panel displaying the year 2020. On the x-axis, schools are binned into equal-interval groups based on the continuous F_{ijt} measure, and mean per-pupil spending in each bin is plotted along with the standard error. The raw data plot in Figure 1 highlights spending patterns that change over time. At the start of the sample the distribution is in concave, with per-pupil spending increasing up to about 40% income qualifying enrollment share and decreasing throughout the rest of the curve. By the end of the panel, per-pupil spending is linear and strictly increasing in F_{ijt}.

In Section 3 I unpack the relationship between per-pupil spending and neighborhood income composition by first estimating a naive OLS model of S_{ijt} as an outcome of F_{ijt}, X_{ijt} and two-way fixed effects. In Section 3.1 I discuss two hurdles in obtaining causal effects of neighborhood change on school spending. In Section 3.2 I present a conceptual framework for empirical estimation of latent school funding formulas, and discuss the estimates in the context of changing education policy environment in Section 3.3.

3.1 Bias in Naive OLS

There are several reasons to believe a relationship between S_{ijt} and F_{ijt} exists, but the direction of the relationship is ambiguous. On one hand, an increase in F_{ijt} may increase spending as Federal dollars are released under the broad umbrella of Title 1 programs. Now, Federal funding could potentially crowd out state and local spending and mitigate any predicted increases. Alternatively, when school finance is tied to local property taxes, increases in F_{ijt} may be correlated with decreases in home values that will in turn decrease per-pupil spending. In Texas, 40% of revenues for schools come from property taxes, amongst the highest in the nation.

The response of Federal spending, potential for crowd-out, and the link between property taxes and school spending are all aspects of the policy environment. As a whole, such characteristics are largely unobserved and it is reasonable to assume that the policy environment changes over time. This particular form of endogeneity will bias OLS estimates, and attenuate two-way fixed effects estimates as the source of endogeneity is time varying. I report the OLS estimates in Table 2 to examine the nature of the endogeneity.

In column 1 of Table 2 are results for a pooled OLS regression taking data from all years as independent observations. The model predicts that if F_{ijt} increases by 0.1, S_{ijt} increases by 1.69% on average. This raw correlation between the two variables is potentially caused by fixed characteristics of a school, for example urban schools may spend relatively more per-pupil than suburban and rural schools as teacher salaries and other costs are systematically higher for urban schools. Those same schools may also have higher shares of income qualifying students than other geographic locales.

In columns 2 through 4 of Table 2 I add school and year fixed effects to the OLS model. Moving from columns 1 to 2 I show that school by year fixed effects absorb much of the variation in column 1, and the relationship between within-school composition changes and per-pupil spending becomes noisy and inconsistent. In column 3 I include a quadratic term to account for non-linearity and in column 4 I add additional campus level descriptive variables that may predict F_{ijt} and S_{ijt} simultaneously. Together the models are inconsistent and insignificant estimates for the effect of a change in F_{ijt} on S_{ijt}.[6]

3.2 Computing Unbiased Measures of School Spending

In each year t, school district j sets a base-level of spending for each student in the district, $\underline{\mathrm{S}}_{jt}$. There are spending adjustments made for a given school i, based on the composition of student incomes, F_{ijt}, or idiosyncratic unobserved reasons u_{ijt}. The spending increase or decrease associated with compositional differences is a byproduct of the policy environment in a given year, as such the expectation for per-pupil spending S at school i, in district j, year t can be expressed as

$$E[S_{ijt}] = (1 - F_{ijt})\underline{\mathrm{S}}_{jt} + F_{ijt}(1 + \underbrace{\omega_t}_{\text{Policy Weight}})\underline{\mathrm{S}}_{jt} + u_{ijt}. \tag{2}$$

The policy weight ω_t is year-specific, exogenous to any particular school, and conceptually a mixture of decisions made at the district, state and federal level. Core to my research design is the estimation of empirical weights for each year, $\widehat{\omega}_t$. It is ω_t that systematically determines the shape of the school spending curve, ie variation in spending across the income distribution. Since Figure 1 suggests the spending curve to be quadratic for some school years, I approximate the spending curve by exploiting the within-district variation in S_{ijt} and F_{ijt}. Employing district fixed effects θ, my model takes the form

$$Log(S_{ijt}) = \alpha_{0t} + \alpha_{1t}F_{ijt} + \alpha_{2t}F_{ijt}^2 + X_{ijt}\beta_t + \theta_{jt} + v_{ijt}. \tag{3}$$

Given that the policy environment is predicted to change over time, I estimate Equation 3 separately for each year, indicated by the subscript t on each of the policy parameters. X_{ijt}

[6] I estimate a similar model taking the headcount of high need students as the explanatory variable of interest. However, the log-log specification has less power as places with no income qualifying students are exlcuded from the sample. Similarly inconsistent results are shown in Appendix Table A1

is a vector of covariates that may explain within-district differences in S_{ijt}. Conditional on X_{ijt}, the change in spending due to a marginal increase in F_{ij} is

$$\widehat{\omega}_t = \widehat{\alpha}_{1t} + 2\widehat{\alpha}_{2t}F_{ijt}. \tag{4}$$

Equation 4 says the funding weight $\widehat{\omega}$ for the marginal low-income student introduced to any school depends on the incumbent proportion of low-income students. The district specific base spending level for each school, $\underline{\mathrm{S}}_{jt}$, can also be obtained from Equation 3 as

$$\underline{\mathrm{S}}_{jt} = \underbrace{\widehat{\alpha}_{0t} + \overbrace{\widehat{\theta}_{jt}}^{\text{District Heterogeneity}}}_{\text{State Base Spend}}. \tag{5}$$

Substituting Equations 4 and 5 into Equation 2 we get predicted school spending as a deterministic function of the income composition,

$$\widehat{S}_{ijt} = [\widehat{\alpha}_{0t} + \widehat{\theta}_{jt}][1 + \widehat{\alpha}_{1t} + 2\widehat{\alpha}_{2t}F_{ijt}^2]. \tag{6}$$

Equation 6 assigns each school a predicted spending level based only on the year-specific policy parameters $\widehat{\alpha}_{0t}, \widehat{\theta}_{jt}, \widehat{\alpha}_{1t}, \widehat{\alpha}_{2t}$. Under the assumption that S_{ijt} is either diminishing or constant in F_{ijt}, the predictions for the signs are $\widehat{\alpha}_1 > 0$ and $\widehat{\alpha}_2 \leq 0$. For the model in Equation 6 to be considered an exogenous predictor of school spending, the requirement is

$$Cov(u_{ijt}, v_{ijt}) = 0. \tag{7}$$

Since Equation 3 estimates the funding formula for the average district in the state for each year t, Equation 7 says that unobserved characteristics of any given school must be idiosyncratic and do not themselves predict the state funding environment for a given year. Given that district funding formulas are subject to state oversight and made publicly available, it is likely that no one school tilts the state average formula one way or another in a given year.

With the policy parameters in hand for each school year I am able to ask a series of counterfactual questions that highlight the effects of policy. The first is to fix the school spending curve to the least progressive policy environment, allowing only the school income composition to change over time, and predict the regressive counterfactual spending level

$$\widehat{S}^R_{ijt} = [\widehat{\alpha}^R_0 + \widehat{\theta}^R_{jt}][1 + \widehat{\alpha}^R_{1t} + 2\widehat{\alpha}^R_{2t}F^2_{ijt}]. \quad (8)$$

By contrast I can fix the spending curve to the most progressive policy environment and predict a progressive counterfactual spending level $\widehat{S}^P_{ijt}$. I now turn to estimating the policy parameters and identifying the regressive and progressive policy years to compute $\widehat{S}^R_{ijt}$ and $\widehat{S}^P_{ijt}$.

3.3 Estimating the Policy Parameters

Estimates for the coefficients of interest in Equation 3 are presented in Table 3. The results in all columns include schools of all grade levels, pooling all years of data in columns 1-3 and adding fixed effects, enrollment controls, and time trends moving left to right. In columns 4 and 5 are the results including all schools in the sample for only the years 2000 and 2020, respectively, tracing out the spending curves underlying the raw data plots of Figure 1. In each specification, standard errors are clustered at the district level.

The first row of estimates suggest that a school with no current income qualifying students will experience an increase in per-pupil spending for a small increase in F_{ijt}. The estimate is robust to district fixed effects and the inclusion of additional controls and time trends. Quantitatively the OLS quadratic model tests if $\widehat{\alpha}_1$ is constant across the distribution of F_{ijt}. Qualitatively, the second row of estimates describe the shape of the spending curve and is negative across all models and years. The headline takeaway is that per-pupil spending is increasing but diminishing in the income qualifying share of students during the sample period.

Since funding requirements may differ for schools serving different grade ranges, I split the data into primary and secondary schools then estimate the within-district model on each subset. Appendix Table A2 show that model predictions in Table 3 reflect funding curves for primary schools. Given the sample size, results for the secondary school subset are inconsistent when estimated separately and cannot be statistically distinguished from the elementary school results.

To determine the progressivity of the policy environment in a given year, I set $\widehat{\omega}_t = 0$ in Equation 4 to compute $\tilde{F}_t$, the threshold income qualifying enrollment share in which a marginal increase would cause per-pupil spending to decline. The threshold is the standard OLS polynomial turning point, simply $\tilde{F}_t = -\frac{1}{2}\frac{\widehat{\alpha}_{1t}}{\widehat{\alpha}_{2t}}$. Estimates of $\tilde{F}_t$ are present in Figure 2 by year. A higher value of $\tilde{F}_t$ indicates a more progressive funding environment as schools do not experience decreases in spending on the basis of having a higher share of students in poverty.

The general trend in Figure 2 shows an increase in the threshold $\tilde{F}_t$ over time. The positive linear trend can be interpreted as the policy environment in Texas becoming more progressive over time. By definition no school can have a income qualifying share above 1, however there are at least 3 years in the sample where the threshold is above the upper horizontal dashed line. In such years the spending curve can be described as strictly progressive, since no increase in F_{ijt} will cause a decrease in spending for the average school. During my sample period the Texas policy environment was most progressive in 2020, and least progressive in 2000.

As Appendix Figures A1 and A2 show, by holding the policy environment fixed, changes to F_{ijt} within a particular school are akin to moving along each spending curve. Thus there are predictable school spending responses to neighborhood change now that the spending curves are pinned down. I next turn to identifying exogenous shocks in the school income composition to test the predictions of my conceptual framework.

4 Affordable Housing As A School Composition Shock

Models put forth by Caetano and Maheshri (2017) predict a sharp household response to any school demographic change, and I follow a literature that documents neighborhood change caused by affordable housing development. Empirical studies show that LIHTC changes neighborhood demographics through household sorting, documenting differential effects of LIHTC when categorizing neighborhoods by income before new units are built (Baum-Snow and Marion 2009, Dillman et al. 2017,Diamond and McQuade 2019). Taking the timing of LIHTC approval as exogenous, in this section I show LIHTC predictably changes enrollment, teacher counts, and income composition for a school. Tracing out the composition changes using event-study methods, I then analyze heterogeneity in the effects of LIHTC based on the pre-period level of F_{ijt}. I conclude Section 4 with a placebo test to be sure the results are not purely a byproduct of my empirical design.

4.1 The LIHTC Program

Created as part of the Tax Reform Act of 1986 and managed by the Internal Revenue Service, the general goal of the LIHTC program is to increase the supply of rental housing in the US. I identify no less than five stakeholders in the development of housing through LIHTC. The federal government, which allocates tax credits to state housing authorities that manage the application process and distribute tax credits to selected real estate developers. The developers sell tax credits to passive investors in exchange for operational cash flow at an estimated 75 cents for every dollar of tax credit, and investor tax benefits are realized over a ten-year period post investment (Eriksen 2009). Finally the renters, who are only subject to income limits when applying for units held for reduced rent. Developers may choose to rent all or a fraction of units in a complex at below market rent.

When developers apply for tax credits, site location has well-known implications for cost subsidies and affordability (Adkins et al. 2017). For one, subsidy amounts are determined as a percentage of total cost basis - applicable development costs that do no include the cost of land. Secondly, building in a qualified census tract yields higher incentives through a basis boost, an automatic increase of the total cost basis by up to 30% (Keightley and Stupak 2020). Both program attributes incentivize development in low-income neighborhoods, though there is reliable evidence that LIHTC itself does cause concentrated poverty (Ellen et al. 2007, Freedman and McGavock 2015). Lang (2012) argues that relatively lower land costs in low-income areas dominates the effect of qualfied census tract, status since land costs are not subsidized.

Who lives in LIHTC? Program guidelines require either 20% of tenants earn less than 50% of the metro area median income or at least 40% of tenants earn less than 60% area median income. Although developers are not required to rent most units to income qualified tenants, the amount of tax credits received increases with the percentage of units held below the rent limit[7] One-third of all US LIHTC units house at least one child under 18 (Hollar 2019), and if each school aged LIHTC resident attends the geographically assigned school, it is worth asking how if the timing of LIHTC development produces identifiable changes in school income composition. LIHTC developments reserved for senior housing is not predicted to influence school compositions, and I use senior housing as a placebo test to rule out spurious correlation disguised as real effects in my event-study results.

4.2 LIHTC Data

Annual LIHTC activity is maintained by the Texas Department of Housing and Community affairs. The data provides the year each housing development became board approved, the total number of rental units made available in each complex, the latitude and longitude of

[7] Rent limits are complex, but generally set 50% or 60% of the local income criteria, adjusted for household size. An area of concern is the actual affordability of LIHTC units, as the rent is determined by median income of an entire metropolitan area, while locally a neighborhood that receives LIHTC could be very poor. Housing studies have shown that the share of LIHTC units that exceed fair market rents can exceed 30% in many cities (Cummings and DiPasquale 1999).

each location, and if the development serves a specific target population.[8] Since the target population designation is inconsistent over time, I use the distribution of units by bedroom count to characterize LIHTC developments by type (Atkins and O'Regan 2014. Family targeted LIHTC is used for the main analysis of the paper, and senior designated LIHTC is used in the placebo tests.

The LIHTC sites are spatially merged to school attendance zones using the following two-step procedure. First, ArcGis software is used to geocode the latitude and longitude data for each site as a point on a map of Texas census blocks.[9] After assigning the appropriate census block to each LIHTC site, I utilize a publicly available file that matches census block to school attendance zones across the US.[10] With each housing development matched to a school zone, I aggregate annual LIHTC activity at the school level by year of project approval.[11] Details on LIHTC sample construction and the location of LIHTC development during my sample period is shown in Appendix Section B.

If LIHTC plausibly increases the share of high need students at a school, Figure 1 suggests that the school spending response will surely depend on the event year and will possibly depend on where the school lies in the income distribution. Table 4 contains descriptive information on the variation in LIHTC timing and the income qualifying enrollment share in the year LITHC received project approval. The first row of Table 4 shows 2399 LIHTC developments approved in the 838 treated school zones. The approval year for the first LIHTC build in the school zone is considered the event year, and I document substantial heterogeneity in the pre-period income composition at schools receiving affordable housing. The bottom section of Table 4 shows that 60% of LIHTC occurs near high need schools, and 40% near low need schools.

[8]Target populations include senior housing, and housing for individuals with developmental needs.

[9]The census block shapefile is provided by IPUMS National Historical Geographic Information System

[10]The School Attendance Boundary Information System : IPUMS, University of Minnesota, William and Mary.

[11]Schools exposed to LIHTC shocks prior to 2000 are excluded from the sample

4.3 Event-Study: The Effect of Affordable Housing on School Composition

Taking Y_{ijt} as the outcome of interest for school i, in district j, year t, I trace out the dynamic effects of new LIHTC housing with the event-study model

$$Y_{ijt} = \sum_{\tau=-6}^{15} \pi_\tau (D_i \times 1[\tau_t = \tau]) + \gamma_i + \gamma_t + \epsilon_{ijt}. \tag{9}$$

D_i is a dummy equal to one if the school zone ever receives family LIHTC, interacted with a set of lag and lead indicators each equal to one in the year that a school is $\tau \in [-6, 15]$ years pre or post tax credit allocation. Intuitively, school zones that are never treated have the property $(D_i \times 1[\tau_t = \tau]) = 0$ since $D_i = 0$ for all untreated years. The interacted event study instruments approximate school $i's$ exposure to LIHTC as a function of the time since the project is approved. It follows that π_τ is a set of event-study coefficients, one for each event-year τ, that estimate the dynamic treatment effect of LIHTC on the school outcomes of interest. By including the two-way fixed effects γ_i and γ_t, the π_τ estimates trace-out the within-school changes over time, caused by LIHTC availability.

The results from Equation 9 are first shown for three school characteristics: enrollment counts, teacher headcounts, and F_{ijt}. All event-study figures include the event-time coefficients π_τ plotted relative to the period prior to LIHTC approval ($\tau_t = -1$), along with 95% confidence intervals for each estimate. Figure 3 illustrates the first-order observable impacts of LIHTC on school enrollment and teacher counts, and Figure 4 displays the shock to F_{ijt}.

The upper panel of Figure 3 takes the log of enrollment as the outcome, and shows enrollment growth beginning year 2 up to a peak 7% in years 6-8 post-LIHTC. Overall the enrollment growth is gradual, however there is a small discrete jump following year 3. This may coincide with the end of the construction phase and new units coming to market, with prior sorting during the construction phase. For the average school in the sample that represents a net increase of 72 students. The funding formula for most school districts will predict more required spending as schools likely need to hire more teachers to meet class size mandates.[12] The results in the lower panel of Figure 3 show that teacher counts increase 3-4%, with the pattern of timing similar to that of enrollment. This is approximately 2 to 3 additional teachers to accommodate the enrollment increase.

Figure 4 illustrates the effect of LIHTC on school income composition, taking F_{ijt} as the outcome. The model predicts F_{ijt} gradually increases to a peak of 3 basis points by year 6-8. This means the growth rate of income qualifying students exceeds the 7% enrollment growth. In Figure 7 I take the log of high need student headcount as the outcome, and find that the net increase in income qualifying students is about 12%. For the average school receiving the LIHTC enrollment shock of 72 students, 61 are predicted to require high need funding.

4.4 Heterogeneity in the Composition Shock

I next plot heterogeneity in the changes to enrollment, teacher counts, and F_{ijt} in Figures 6 and 7 for high need and low need schools. Figure 6 shows that changes to enrollments and teacher counts follow similar trends in the post-period, however, there are differential pre-period trends. Pre-trends for high need districts receiving LIHTC show stable enrollment patterns and a subsequently smooth pre-trend for teacher counts. In contrast, enrollment growth and teacher counts at low need schools are on average lower than the event year and gradually rising. A potential qualitative explanation for this pattern is affordable housing deployments near schools that serve expanding, in-demand neighborhoods. Therefore, it tenous to claim that LIHTC near low need schools is responsible for enrollment and teacher count growth.

Figure 7 traces out heterogeneity in the LIHTC income composition shock. I find that F_{ijt} is predicted to increase 15 basis points at low need schools, and decrease by 5 basis points at high need schools. This differential effect follows the theme of Diamond and McQuade (2019),

[12] For K-4 classes in Texas there must be no more than 22 students per teacher. For grades 5-12, districts must maintain an overall ratio of 20 to one but there is no set class size cap.

in which the authors show that LIHTC causes house prices to increase in low-income (high need) neighborhoods, and decrease in high-income (low need) neighborhoods. In context of the Diamond and McQuade (2019) core argument, the decrease in F_{ijt} near high need schools occurs because LIHTC spurs neighborhood revitalization and positive income sorting. Near-low need schools, the idea that LIHTC is preceded by neighborhood demand growth appears to be supported by the pretrends in Figure 7. Prior to the event-year, F_{ijt} is decreasing, indicative of local income growth. The LIHTC effect is to reverse this trend, precisely the prediction of Diamond and McQuade (2019).

In Figure 8 I again take log of the high need headcount as the outcome to gauge the count of income qualifying students that enroll at each school type. At low need schools, the income qualifying headcount increases from 302 (of 1205) students to roughly 482. This rich variation allows me to employ counterfactual spend measures from section 3.2 that only require shocks to F_{ijt} to analyze the effects of regressive and progressive policy environments. In Section 5 I explore the policy dimension further, identifying spending responses to the change in F_{ijt} under regressive and progressive policy conditions. The heterogeneity outlined in this section allows me to identify who captures the gains from increasingly progressive public school financing in the US.

4.5 Placebo Test

In Figure A5 I take the timing of LIHTC senior designated housing as the event-year in my model. All family designated LIHTC is dropped from the sample and all never-treated schools are used as the reference group. The left panel of Figure A5 shows a null effect of LIHTC senior housing on school enrollment. Since enrollment growth is constant after receiving senior LIHTC, the center panel shows predictions that no need teachers are hired as well. Lastly, the right panel shows that senior housing creates small but statistically indistinguishable changes to school income composition. My placebo results rule out mechanical composition changes from organizing the data based on LIHTC event time. With plausibly exogenous neighborhood changes linked to school composition changes, I next test for the response of school spending in Section 5.

5 The School Spending Resonse to Neighborhood Change

Given the enrollment and teacher headcount increases, one prediction is that absolute spending will increase for a school receiving new LIHTC. The effect of LIHTC on per-pupil spending is ambiguous, and as described in Section 3.2 it will depend on the school spending structure in place. Further, when the relationship between per-pupil spending and F_{ijt} is non-linear, the school spending response to the composition shock will also depend on the preexisting school income composition. Table 4 lays out the variation in F_{ijt} for schools that receive LIHTC housing approval in the academic year.

More specifically, whether the receiving neighborhood is above or below the threshold $\tilde{F}_t$ in the period new LIHTC arrives. This is illustrated in Figure 10. The plot contains $\tilde{F}_t$ each year along with the average F_{ijt} for a school receiving LIHTC in a given year. Since per-pupil spending is predicted to rise after LIHTC if $F_{ijt} < \tilde{F}_t$, and fall otherwise, the regressive policy environment predicts the average school receiving LIHTC will be near or beyond the threshold in the majority of event years. This is illustrated in Figure 10 by substantial share of event-year points lying above the horizontal line for the regressive curve threshold.

I begin by estimating the average effect of LIHTC timing on S_{ijt}, $\widehat{S}^R_{ijt}$ and $\widehat{S}^P_{ijt}$ in Section 5.1. In Section 5.2 I document heterogeneity in the effects for high need vs. low need schools, and by exploring schools above and below the threshold $\tilde{F}_t$. In Section 5.4 I again conduct my placebo test taking per-pupil spending changes as an outcome of LIHTC senior designated housing.

5.1 Estimating The Average Response

I begin by testing the prediction that absolute spending will increase following LIHTC due to enrollment increases and teacher hiring. The results in Figure 9 show that absolute spending increases 3-5% following affordable housing development. The timing and magnitude of the absolute spending change follows tightly with the predictions for teacher headcounts in Figure 3. By contrast, estimating the model using observed per-pupil spending S_{ijt} as an outcome, the coefficients in Figure 11 vary smoothly across the event window. With no identifiable difference in pre-period and post-period spend, absolute spending growth is roughly equivalent to enrollment growth. This makes sense if the student composition remained unchanged, but an the increase in F_{ijt} post-LIHTC suggests that the new batch of students require additional support. Taking the result at face value, it appears as though equivalent education expenditures have fallen.[13]

It may also be the case that the event-study may suffer from the same policy bias as the OLS estimates in Section 3.1. Moreover, such unobserved policy changes are time-varying, a particular source of bias in event-studies that attenuate estimates to zero (Sun and Abraham 2021, Goodman-Bacon 2021). Since this bias comes through the composition channel, and not enrollment and teacher counts, I am able to identify absolute spending increases but not per-pupil spending increases tied to income qualifying enrollment share.

I next estimate the model taking my two policy-constant spend measures $\widehat{S}^R_{ijt}$ and $\widehat{S}^P_{ijt}$ as outcomes. Recall that holding the policy environment fixed and shocking F_{ijt} is akin to moving along the regressive and progressive spending curves in Figures A1 and A2. Further, Figure 10 indicates the average LIHTC receiving school is near or slightly above the regressive policy threshold, thus $\widehat{S}^R_{ijt}$ is predicted to decline. The results in Figure 12 show that $\widehat{S}^R_{ijt}$ indeed decreases after LIHTC, with spending predicted to decrease close to 10% by year 6 and falling gradually thereafter. Qualitatively, had spending curve been fixed to 2000 and

[13] Wilson et al. (2006) describe equivalent education expenditures as the amount to be spent per student (on average) if schools all faced the same costs and had the same student body composition.

never changed, the increase in F_{ijt} following new affordable housing would cause per-pupil expenditures to fall by \$720 per-pupil for the average school.

In the progressive policy environment increases to F_{ijt} will cause per-pupil spending to rise in all cases. Figure 13 contains the estimates taking $\widehat{S}^P_{ijt}$ as the outcome, and I find noisy and inconsistent estimates for the effect of LIHTC on per-pupil spending with progressive policy. Model predictions for the effect of LIHTC on $\widehat{S}^R_{ijt}$ and $\widehat{S}^P_{ijt}$ at the mean are imprecise, and in the context of the conceptual grounding in Section 3, Figures 12 and 13 will mask potential heterogeneity in the effect of income composition on school spending. This is particularly true given the sharply differential composition shocks across neighborhood type.

5.2 Heterogeneity in the Spending Response

Again I divide the treated sample into low need and high need schools similar to the analysis of LIHTC shocks in Section 4.4. It is coincidental but convenient for the analysis that the threshold $\tilde{F}_t$ is roughly 0.502 for the regressive spending curve. In the results of Figure 14 I find no differential effects of LIHTC on observed spending S_{ijt} in the model, as neither school type appears to be financially affected by the timing of LIHTC. If observed spending does not increase at low need schools, where the largest increase in F_{ijt} occurs, then Federal spending tied to high need student enrollment must have crowded out state and local spending. Alternatively the model may suffer from the same bias present in the mean estimates for the change in observed spending.

In Figure 15 I present estimates of heterogenous effects of LIHTC on $\widehat{S}^R_{ijt}$, tracing out substantial heterogeneity by school type. What Figure 15 shows is that neighborhood change under regressive policy environments causes substantial decreases to spending at low need schools following LIHTC, and no spending change at initially high need schools. The differential spending response is caused by a combination of the heterogenous composition shock shown in Figure 7 and the shape of the regressive spending curve. By years 7 and 8 following LIHTC, F_{ijt} has increased by 0.1-0.15 at low need schools, but decreased 0.03-0.05 at initially high need schools. Given that any compositional shock pushing F_{ijt} past $\tilde{F}_t = 0.502$ would cause spending to decrease, the observed shock at low need schools would cause spending to decrease for any school with a pre-LIHTC F_{ijt} of 0.35 or higher.

Based on the analysis of Section 3.3, estimating the main model taking $\widehat{S}^P_{ijt}$ as the outcome is akin to asking what would be the school spending response to neighborhood change had the policy environment always been progressive? The prediction of the progressive spending curve is for any increase in F_{ijt} to be accompanied by an increase in per-pupil spending. The differential composition shocks presume that initially low need schools would experience per-pupil spending increases, and I find a 10-15% by years 7-8 after LIHTC near low need schools. The evidence shows that school spending will not change, or perhaps may decrease by a small amount, for high need schools under the most progressive school spending environments. This occurs as sorting produces a new batch of students with a lower share of income qualifying enrollees.

5.3 Who Benefits from Progressive Policy Changes?

The core argument of this paper is that unobserved policy changes dominate the omitted variable bias in the relationship between S_{ijt} and F_{ijt}. Given that the most regressive spending environment used to predict $\widehat{S}^R_{ijt}$ comes at the start of the sample period, I can estimate the effect of underlying policy changes by differencing $\widehat{\delta}_{ijt} = S_{ijt}$-$\widehat{S}^R_{ijt}$. $\widehat{\delta}_{ijt}$ measures the increase in observed spending attributed to changes in education policy during any year t. Given the differential composition shocks and predicted changes in $\widehat{S}^R_{ijt}$ following LIHTC, the interpretation of the event-study estimates taking $\widehat{\delta}_{ijt}$ as an outcome is to place a value of the spending increases caused by policy change as Texas shifts away from a regressive spending environment over time.

Results are shown in Figure 17. In the pre period, low need schools have a higher but declining per pupil expenditure level. After the composition shock per pupil spending rises because F_{ijt} increases. Taking the difference between the peak spending increase in the post period from peak spending in the pre period, I find that policy changes increased per-pupil spending by 0.8-0.4 =0.4 (40%) for low need schools following LIHTC. On the other hand, because high need schools were not subjected to much of a composition change, the observed spending response to LIHTC is not statistically distinguishable from the policy constant spending response. Thus, progressive policy changes are largely responsible for ensuring support for high need students that move to attend low need schools.

5.4 Placebo Tests

I repeat the analysis of Section 4.5 by dropping LIHTC family developments from the sample and estimating the model with LIHTC senior developments and the never treated schools. In the left panel of Figure A6 I take absolute spending as an outcome of the senior LIHTC announcement timing and find no changes to absolute spending relative to control schools. Predictably, since enrollment, teacher counts, and absolute spending remain unchanged, the right panel of Figure A6 shows that per-pupil spending changes are also not identified. Taken together the placebo tests of Section 4.5 and Section 5.4 support the findings that LIHTC creates exogenous neighborhood change, translating to differential spending responses based on the location of the shock, and the education funding environment.

6 Conclusion

Neighborhood integration is often hotly contested, and interventions in US can be traced back to the busing of Black children across school district lines to desegregate learning opportunities (Cascio et al. 2008). Today, housing market interventions to support low income families aim to avoid poverty concentration, meaning that new affordable housing development in some cases will act as a form of income integration. The findings of this study show that indeed, neighborhood composition changes caused by LIHTC rental housing changes the income composition of local public schools. Given that school funding formulas link neighborhood composition to school spending, it is reasonable to expect that identifiable composition shocks to change the way schools spend money. However, school funding formulas change over time, and the progressivity of the spending environment will largely dictate how schools respond to neighborhood change.

States that have regressive school spending environments will likely incur per-pupil spending declines following the integration of upper income neighborhoods. The primary reason is high income households sorting out, decreasing house prices and potentially lowering property tax revenues to be used for schools. The effect is compounded by the fact that following the shock, per-pupil spending would need to rise in order to meet the needs of a new batch of students. When evaluating the performance of students moving from low income to high income schools to tests for the gains from moving to opportunity, one must account for the fact that equivalent school opportunities for low income students require higher levels of spending. My results show the increase required to maintain equitable spending only occurs in progressive education funding environments.

The stratification of race and income in the United States means the left tail of the income distribution is disproportionately composed of Black and Latino individuals (Darity Jr et al. 2015), and the US has a history of restricting minority access to quality public services (Cook et al. 2023). Given the composition of who lives in LIHTC (Hollar (2019)), it is an extension for future work to understand how LIHTC changes the racial composition of schools in high income areas. I document preliminary evidence for differential neighborhood sorting by race following LIHTC in Appendix Figure A7, to the extent that such changes cause changes to the school composition. Given the simultaneity of such changes along many dimensions, the development of work by Davis et al. (2019) and Dizon-Ross (2020) is vital to extend our understanding of the racial composition shock from affordable housing policy.

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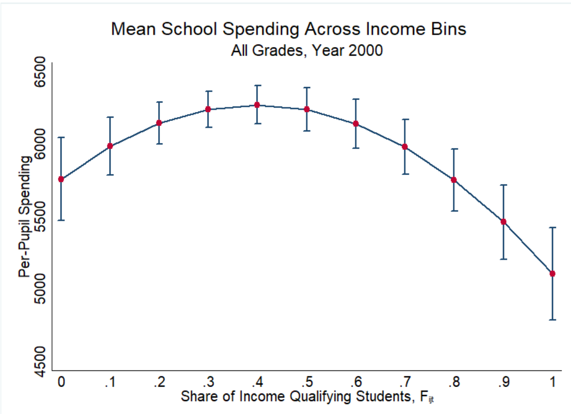
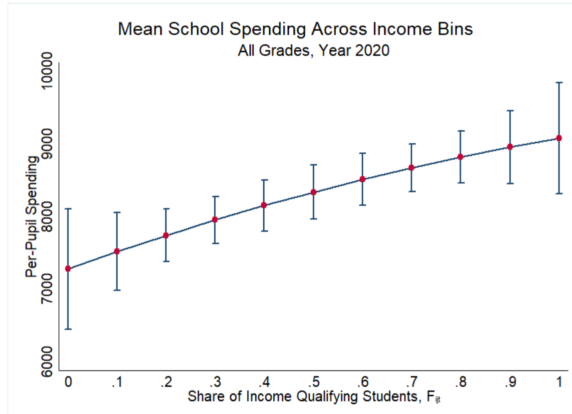
Figures

Figure 1: Mean School Spending Across The Income Distribution

(a)



(b)



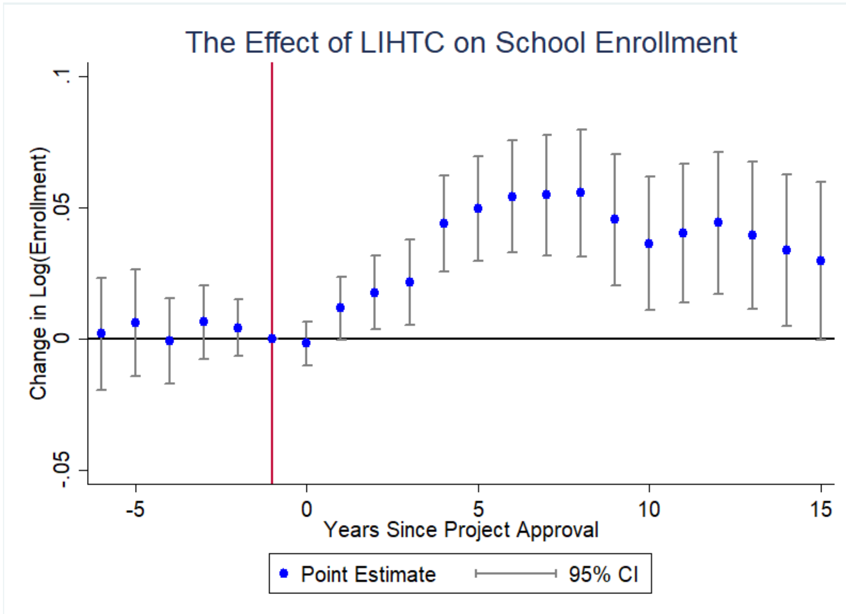
Notes: Each panel plots the mean per-pupil spending level for a sample of 3,926 K-12 schools in Texas, binned into 10 equal-interval groups based on the income qualifying enrollment share. Shown with the mean for each bin is the standard error.

Figure 2: $\tilde{F}_t$ Over Time

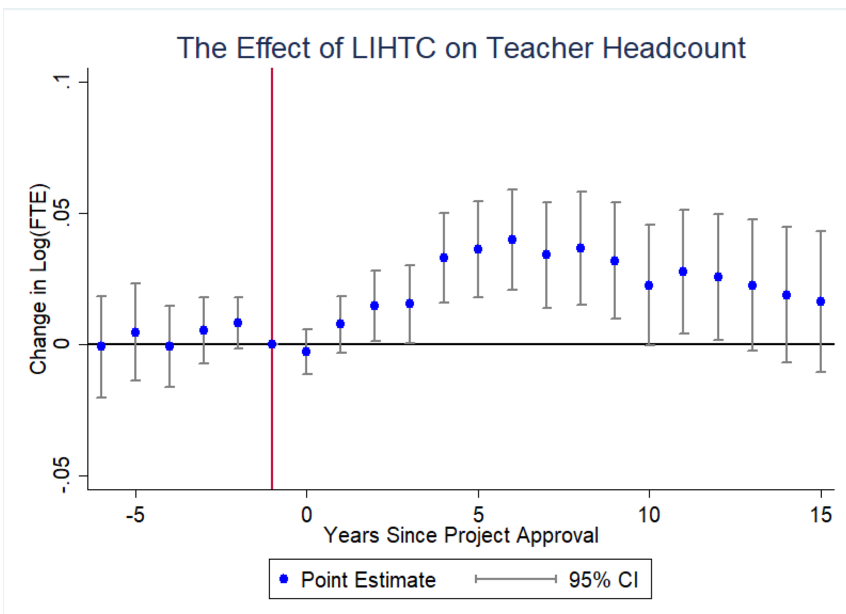
Notes: The non-linear relationship between per-pupil spending and income qualifying student share yields a threshold in which a small increase in the income qualifying share would cause per-pupil spending to decline. Figure 2 shows the change in the threshold over time.

Figure 3: Affordable Housing Increases School Size

(a)

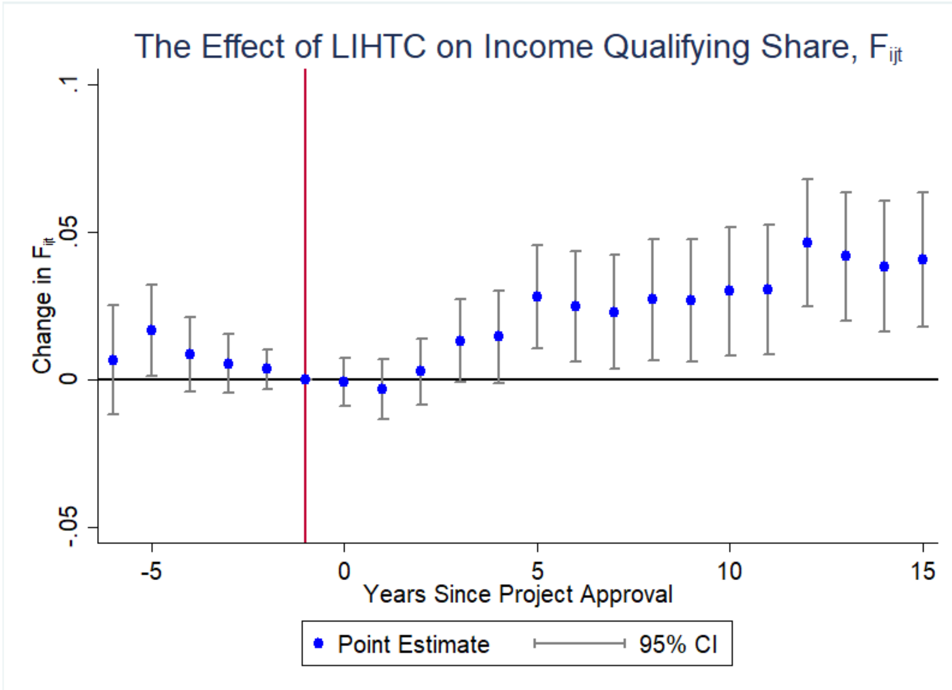


(b)



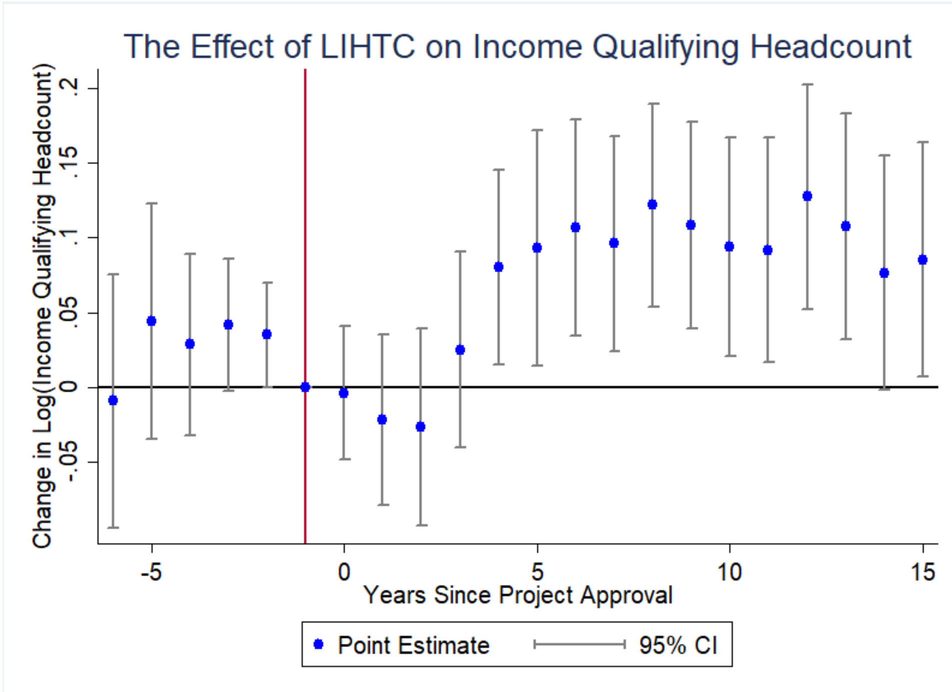
Notes:Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Figure 4: The LIHTC Composition Shock



Notes: Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

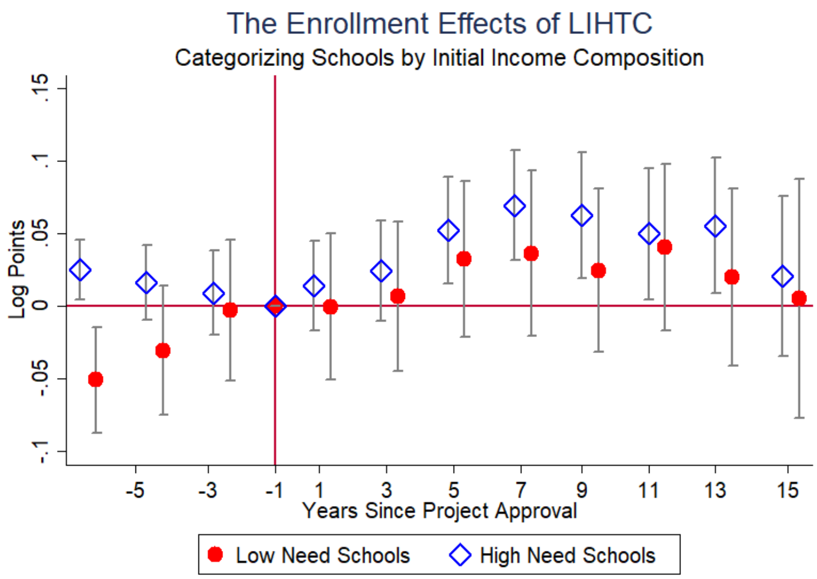
Figure 5: The LIHTC Composition Shock



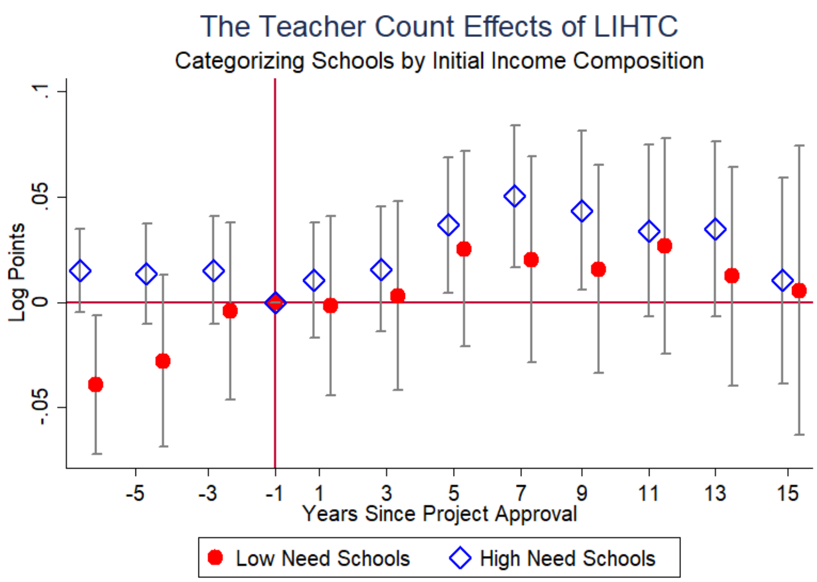
Notes: Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Figure 6: Heterogeneity In School Size Changes After LIHTC

(a)

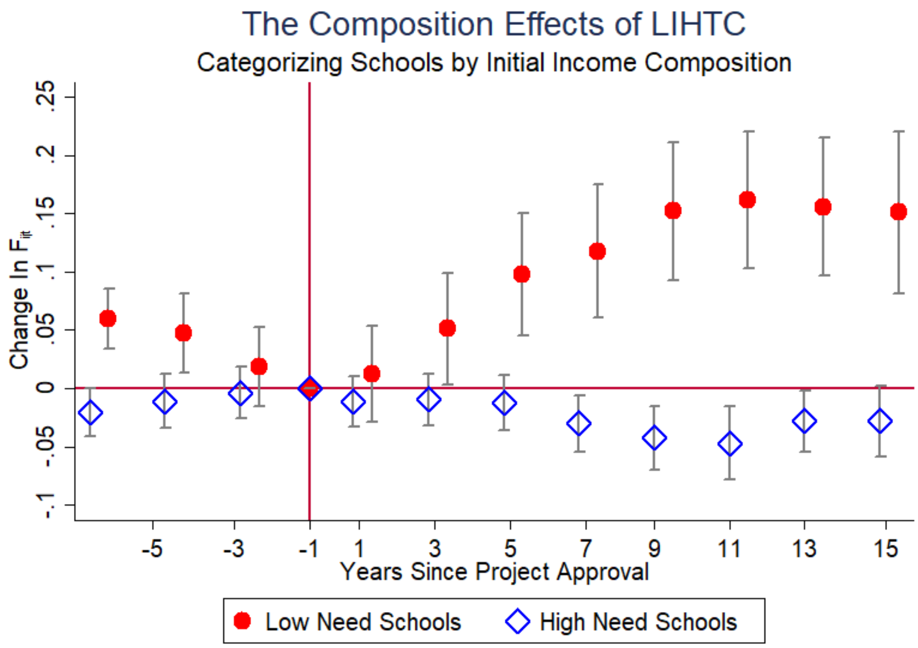


(b)



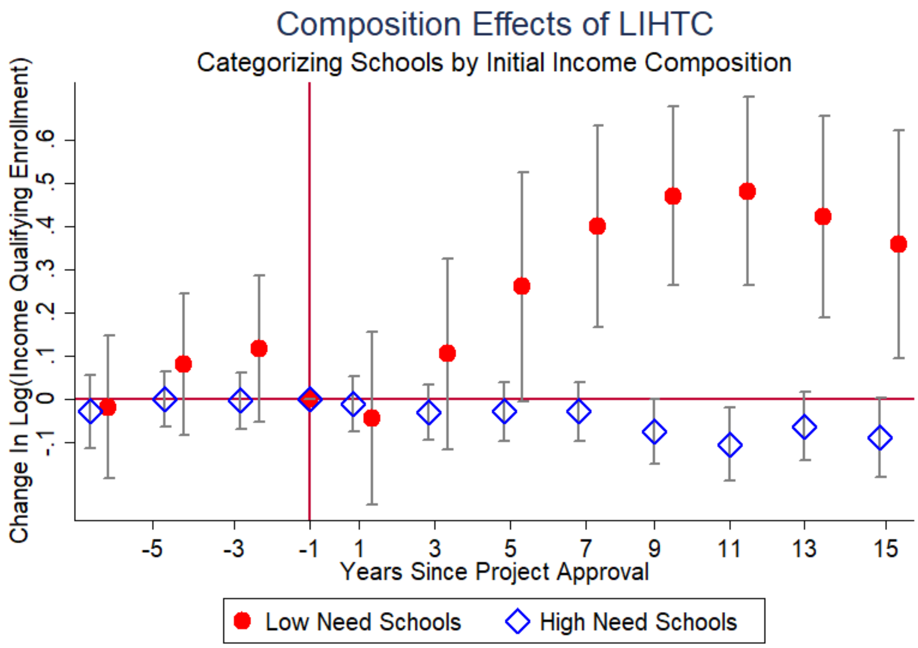
Notes: Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Figure 7: Heterogeneity in the LIHTC Composition Shock



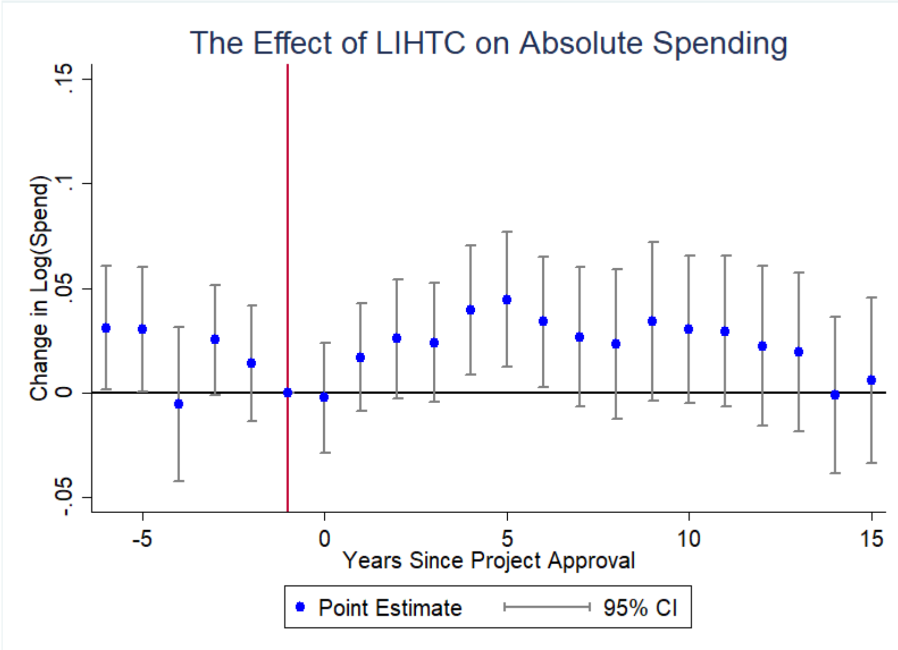
Notes: Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Figure 8: Heterogeneity in the LIHTC Composition Shock



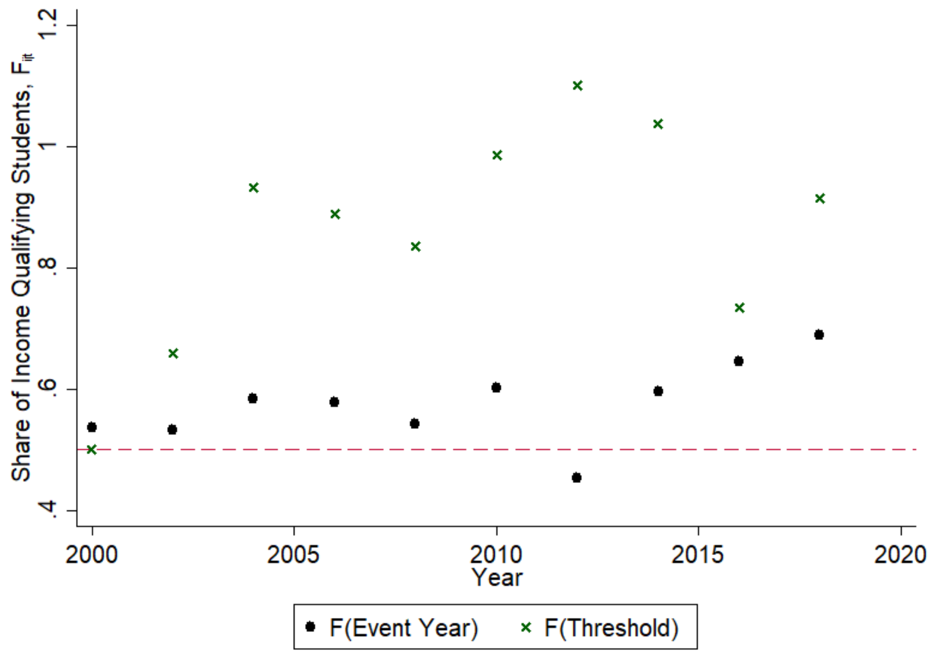
Notes: Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Figure 9: The Effect of LIHTC On Absolute Spending



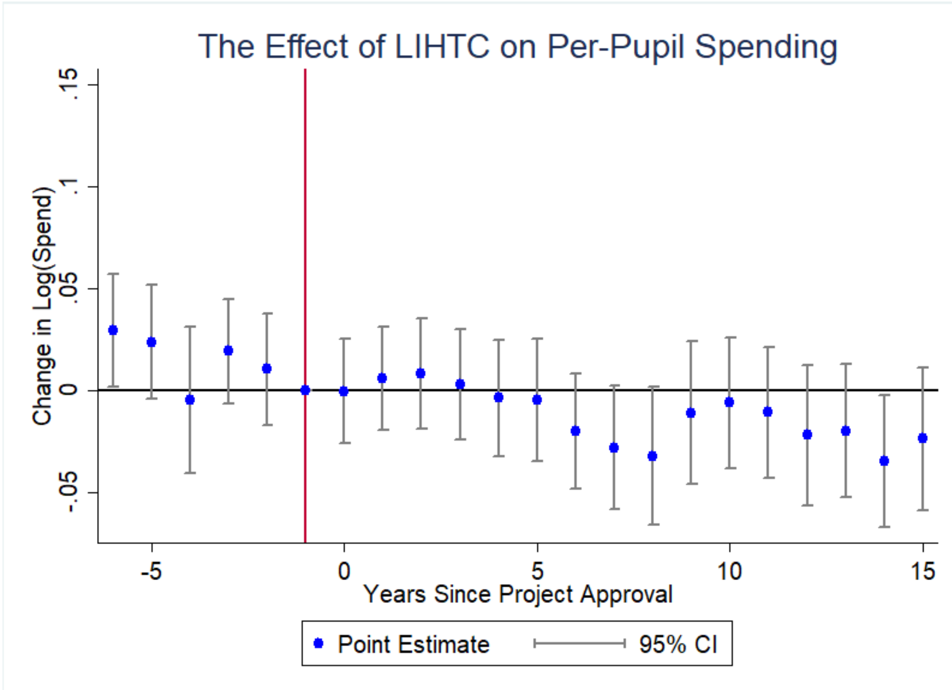
Notes: Absolute spending is calculated as total spending in a given year, divided by enrollment fixed to the initial sample year. Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Figure 10: Threshold $\tilde{F}$ Over time



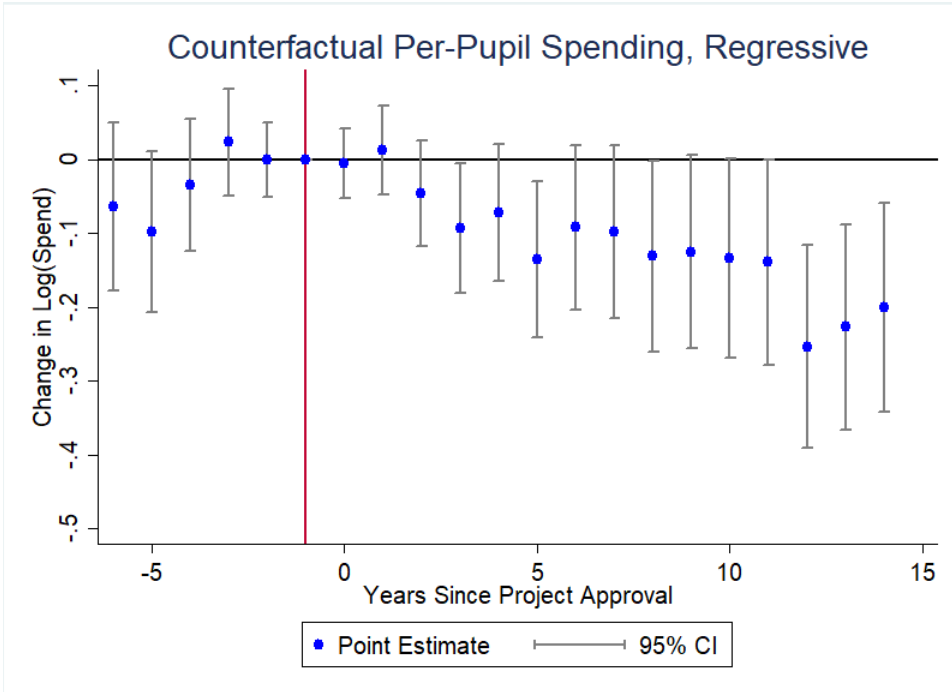
Notes: The non-linear relationship between per-pupil spending and income qualifying student share yields a threshold in which a small increase in the income qualifying share would cause per-pupil spending to decline. The image shows the change in the threshold over time. Following from Table 4, Figure 10 includes the mean income-qaulifying share at schools receiving LIHTC for the first time in each year.

Figure 11: The LIHTC Spending Response × Observed Spending S_{ijt}



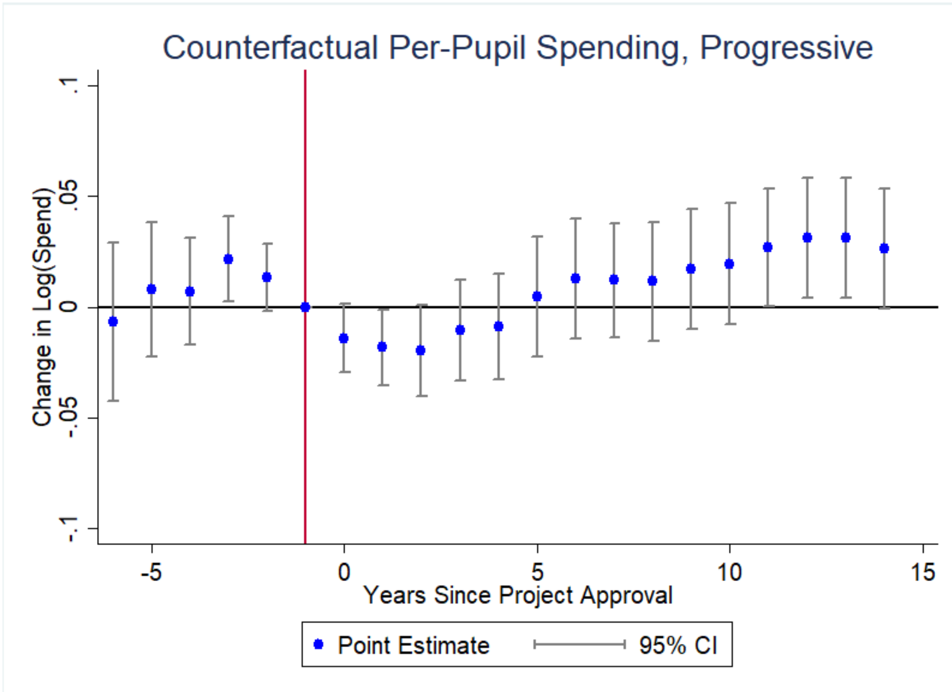
Notes: Observed spending is calculated as total spending in a given year divided by enrollment in that year. Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Figure 12: The LIHTC Spending Response × Regressive Policy $\widehat{S}_{ijt}^{R}$



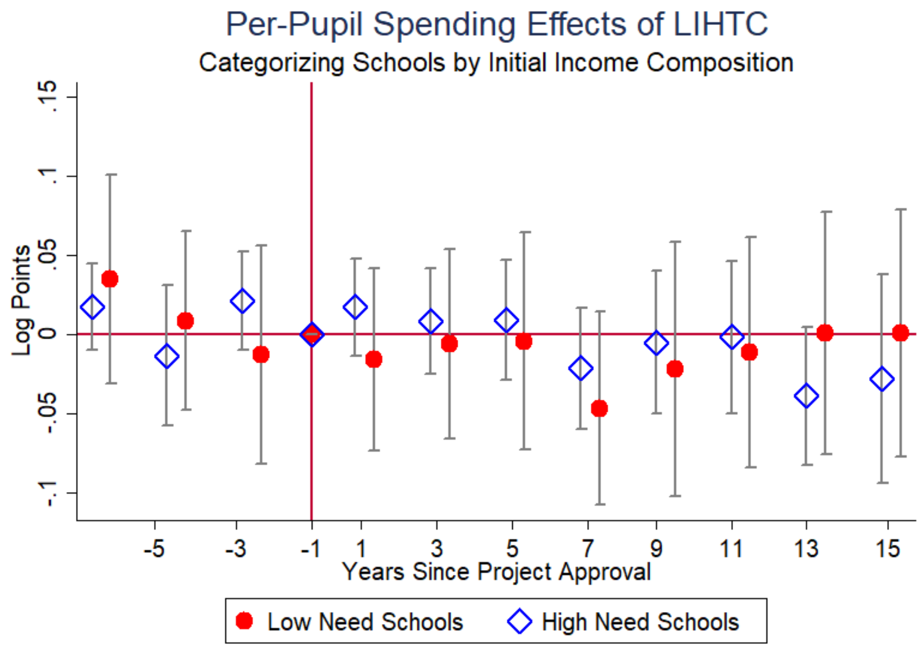
Notes: Regressive policy spending is a counterfactual measure of per-pupil spending in a given year as predicted by the model in Section 3. Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Figure 13: The LIHTC Spending Response $\times$ Progressive Policy $\widehat{S}^{P}_{ijt}$



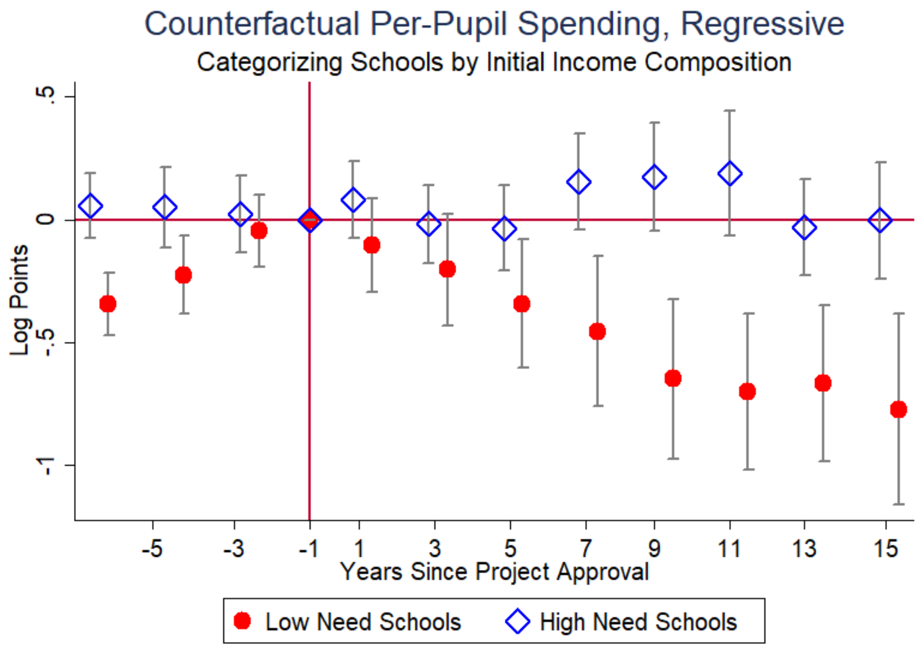
Notes: Progressive policy spending is a counterfactual measure of per-pupil spending in a given year as predicted by the model in Section 3.Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Figure 14: Heterogeneity in The LIHTC Spending Response $\times$ Observed Spending S_{ijt}



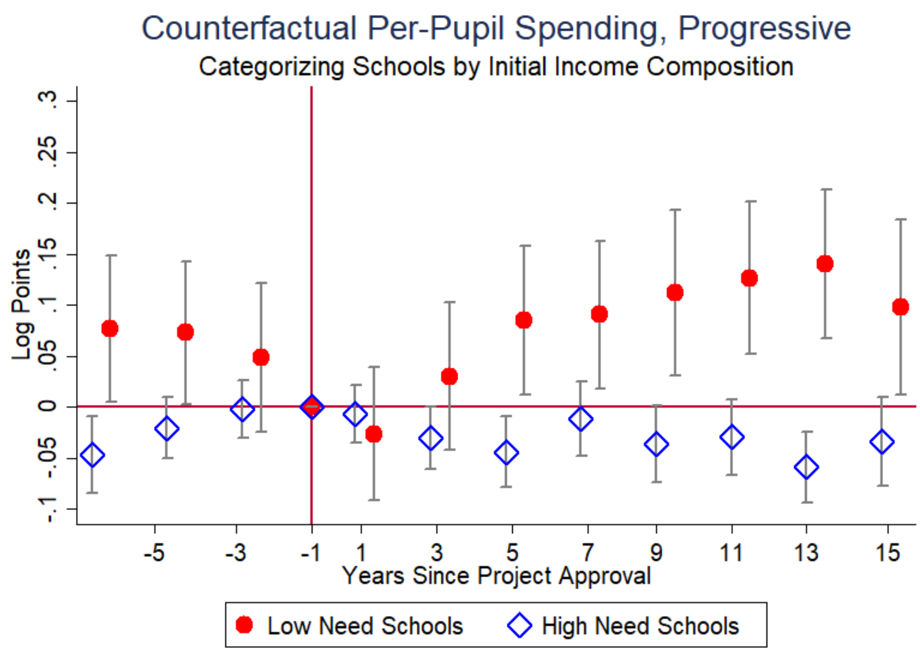
Notes: Observed spending is calculated as total spending in a given year divided by enrollment in that year. Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Figure 15: Heterogeneity in The LIHTC Spending Response $\times\ \widehat{S}^R_{ijt}$



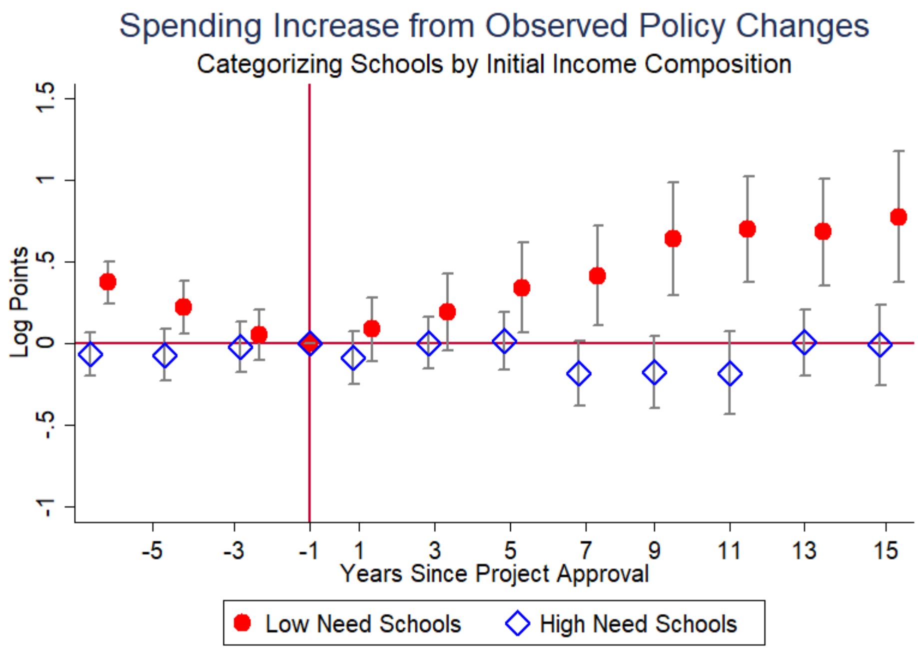
Notes: Regressive policy spending is a counterfactual measure of per-pupil spending in a given year as predicted by the model in Section 3. Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Figure 16: Heterogeneity in The LIHTC Spending Response $\times$ $\widehat{S}^{P}_{ijt}$



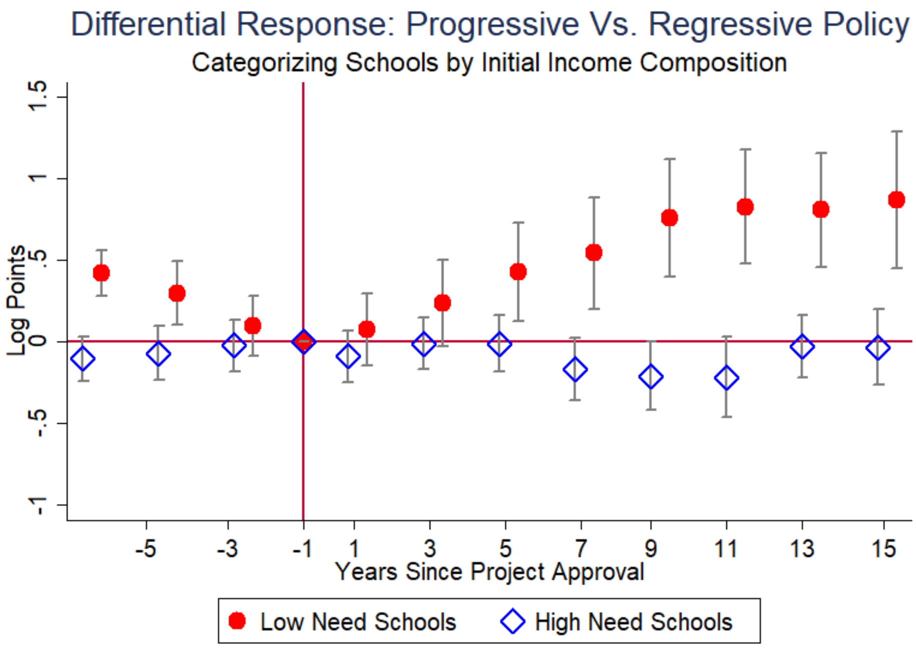
Notes: Progressive policy spending is a counterfactual measure of per-pupil spending in a given year as predicted by the model in Section 3. Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Figure 17: How Policy Shaped the Observed Spending Response, $\widehat{\delta}_{ijt}$



Notes: $\widehat{\delta}_{ijt} = S_{ijt}$ - $\widehat{S}^{R}_{ijt}$. Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Figure 18: The Boundaries of Policy Effects $\widehat{\delta}^{P}_{ijt}$



Notes: $\widehat{\delta}^{P}_{ijt} = \widehat{S}^{P}_{ijt}$ - $\widehat{S}^{R}_{ijt}$. Event-study estimates of the $\hat{\pi}_t$ coefficients from Equation 9 for the outcome variable of interest. Each regression includes school and year fixed effects, along with a linear time trend interacted with the initial period quartile of the income qualifying share. 95% confidence intervals are shown, with observations clustered at the school level.

Tables

Table 1: School Summary Statistics

	Full Sample	< 50% Income Qualifying Low-Need Schools	50%+ Income Qualifying High-Need Schools
Per-Pupil Spending	7,315.95	6,920.50	7,611.68
	(3445.7)	(3599.7)	(3295.1)
Enrollment	1,032.36	1,205.59	902.82
	(781.9)	(896.7)	(654.3)
Income Qualifying Count	511.53	301.49	668.60
	(442.2)	(304.2)	(463.6)
Income Qualifying Share	0.54	0.26	0.75
	(0.284)	(0.151)	(0.143)
Teacher Count	65.91	75.53	58.72
	(46.96)	(52.71)	(40.69)
Student-Teacher Ratio	15.30	15.48	15.16
	(3.139)	(4.029)	(2.244)
Fraction Latino	0.50	0.31	0.64
	(0.307)	(0.254)	(0.268)
Fraction Black	0.13	0.10	0.16
	(0.162)	(0.115)	(0.186)
School×Year Obs.	71,080	28,030	43,050

Table 2: Relationship Between Income and School Spending

Outcome: $Log(S_{ijt})$	(1)	(2)	(3)	(4)
F_{ijt}	0.169***	0.036	-0.069	-0.073
	(0.022)	(0.026)	(0.103)	(0.092)
F_{ijt}^2			0.103	0.060
			(0.109)	(0.098)
N	71,080	71,080	71,080	71,080
School X Year FE		×	×	×
Additional Controls				×
r2	0.029	0.536	0.537	0.583

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: Cross-Section Relationship Between Income and School Spending

	(1) All Years	(2) All Years	(3) All Years	(4) 2000	(5) 2020
$\widehat{\alpha}_1$	0.320***	0.367***	0.383***	0.291**	0.675***
	(0.091)	(0.065)	(0.074)	(0.098)	(0.126)
$\widehat{\alpha}_2$	-0.122	-0.253***	-0.276***	-0.386***	-0.339**
	(0.091)	(0.065)	(0.062)	(0.083)	(0.116)
N	71,080	71,080	71,080	3,150	3,150
r2	0.030	0.283	0.284	0.363	0.264
District FE		$\times$	$\times$	$\times$	$\times$
Additional Controls			$\times$	$\times$	$\times$

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: LIHTC Treatment at Sample Schools

Year	**2003**	**2013**	**2020**
LIHTC Builds Since 2000	522	1544	2399
Income Qualifying Share in the Event Year	Count	Percent	Cumulative
0-0.1	40	4.77	4.77
0.1-0.2	48	5.73	10.50
0.2-0.3	57	6.80	17.30
0.3-0.4	95	11.34	28.64
0.4-0.5	97	11.58	40.21
0.5-0.6	109	13.01	53.22
0.6-0.7	109	13.01	66.23
0.7-0.8	97	11.58	77.80
0.8-0.9	93	11.10	88.90
0.9-1	93	11.10	100

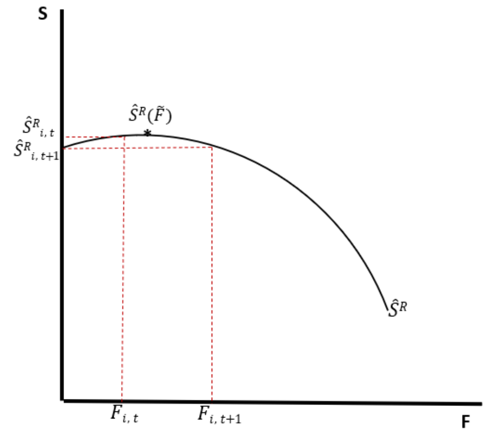
Notes: Row 1 reflects cumulative LIHTC developments in the sample districts from 2000-2020. The bottom pane contains counts of LIHTC builds approved in schools within each bin of the free-lunch share distribution. Descriptive statistics for the 838 schools with any LIHTC activity are presented in Table 1.

Supplementary Appendix

Local Housing Development and Money for Neighborhood Schools

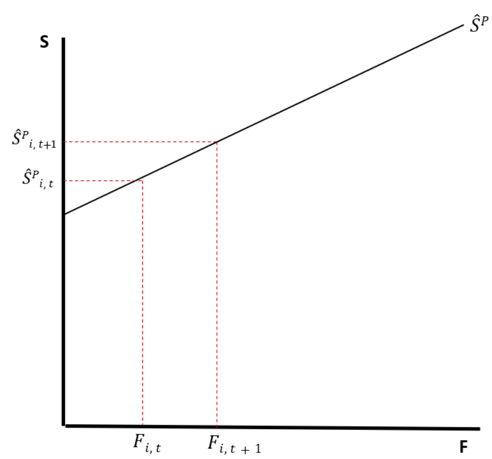
Kenneth Whaley

Figure A1: Regressive Spending Curves



Notes: School spending in a regressive policy environment. Along the spending curve, there is a point in which a small increase in F_{ijt} would cause per-pupil spending to decline. The regressive spending curve prevailed in Texas at the start of the sample period.

Figure A2: Progressive Spending Curves

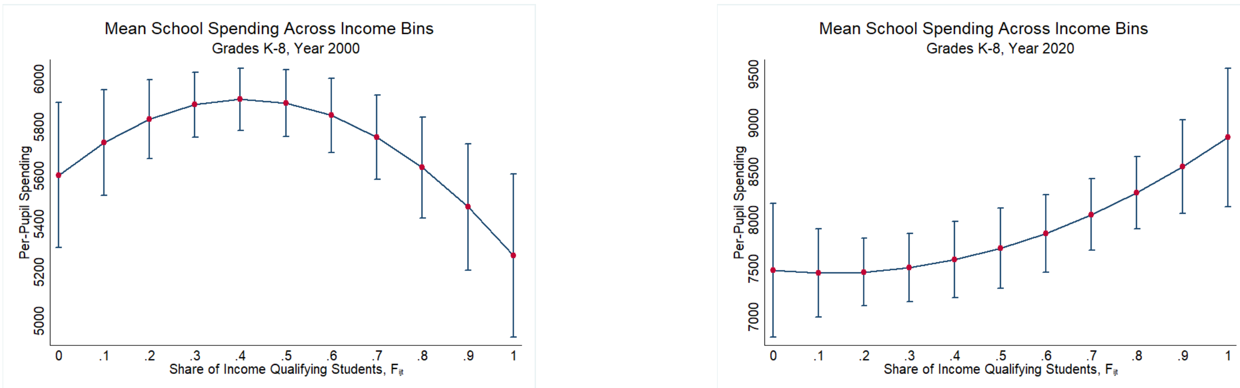


Notes: School spending in a progressive policy environment. In a progressive spending environment per-pupil spending is strictly increasing in F_{ijt}. The progressive spending curve prevailed in Texas at the end of the sample period.

A.1 Mean Spending Across Income Bins, By Grade

Figure A3: Mean School Spending Across The Income Distribution (K-8)

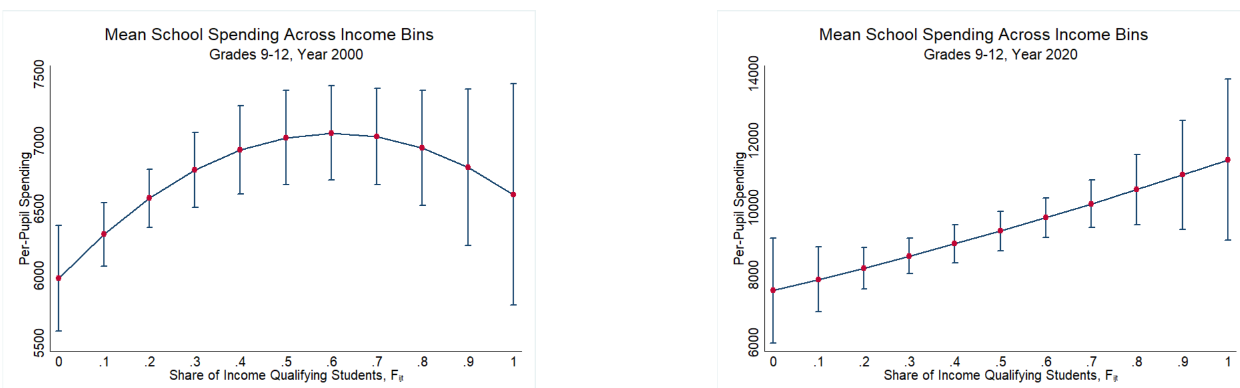
(a) (b)



Notes: Each panel plots the mean per-pupil spending level for a sample of 3,926 K-12 schools in Texas, binned into 10 equal-interval groups based on the share of students receiving free or reduced lunch. Shown with the mean for each bin is the standard error.

Figure A4: Mean School Spending Across The Income Distribution (9-12)

(a) (b)



Notes: Each panel plots the mean per-pupil spending level for a sample of 3,926 K-12 schools in Texas, binned into 10 equal-interval groups based on the share of students receiving free or reduced lunch. Shown with the mean for each bin is the standard error.

Table A1: Cross-Section Relationship Between Income and School Spending

Outcome: $Log(S_{ijt})$	(1)	(2)	(3)	(4)
log(Income Qualifying Headcount)	0.023***	-0.039***	0.063***	-0.035*
	(0.006)	(0.011)	(0.018)	(0.016)
log(Income Qualifying Headcount)^2			-0.012***	0.003
			(0.002)	(0.002)
N	71,080	71,080	71,080	71,080
School X Year FE		×	×	×
Additional Controls				×
r2	0.005	0.457	0.459	0.477

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2: Cross-Section Relationship Between Income and School Spending

	Primary All Years	Primary 2000	Primary 2020	Secondary All Years	Secondary 2000	Secondary 2020
$\widehat{\alpha}_1$	0.370***	0.480***	0.466***	0.064	0.306*	0.826
	(0.078)	(0.065)	(0.082)	(0.070)	(0.114)	(0.451)
$\widehat{\alpha}_2$	-0.252***	-0.478***	-0.178*	0.081	-0.075	-0.599
	(0.074)	(0.070)	(0.076)	(0.066)	(0.140)	(0.456)
N	62684.000	2442.000	2938.000	17349.000	218.000	250.000
r2	0.312	0.445	0.443	0.509	0.784	0.552

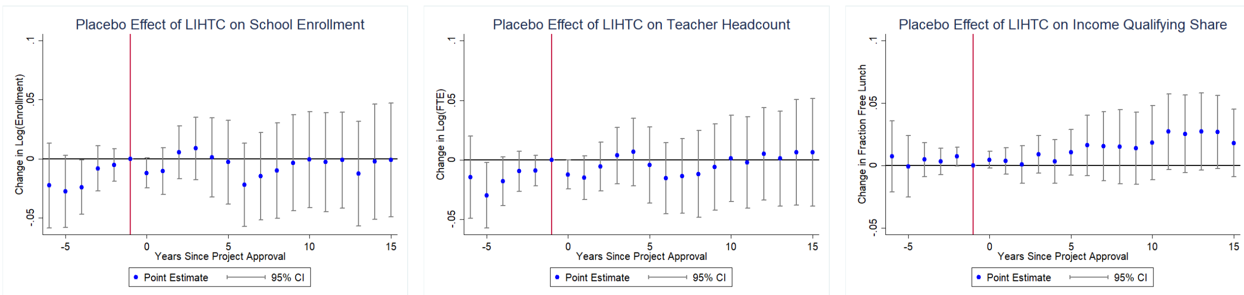
Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.2 Placebo Effects

Figure A5: Placebo Effects of LIHTC

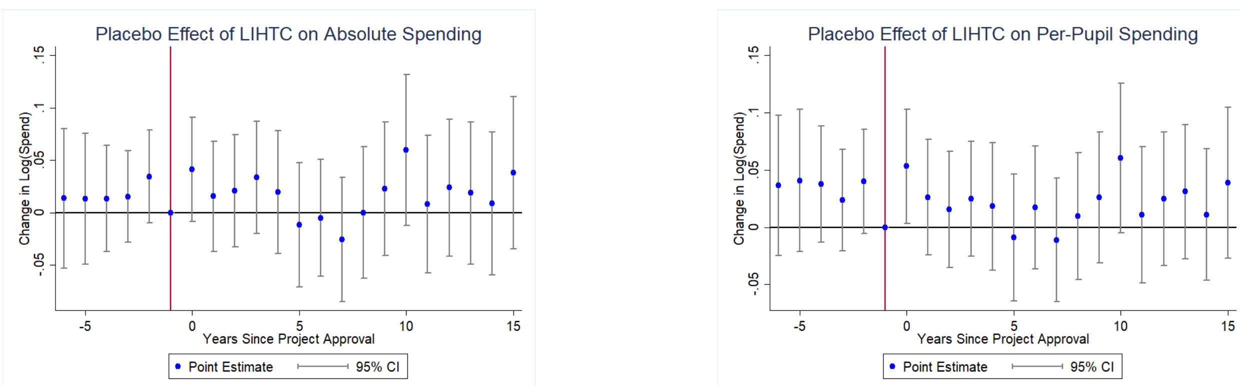
(a) (b) (c)



Notes:

Figure A6: Placebo Effects of LIHTC

(a) (b)

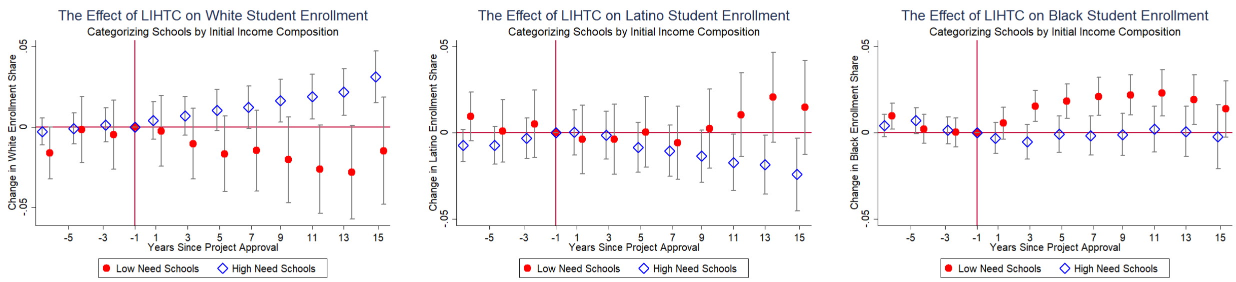


Notes:

A.3 Heterogeneity in Racial Sorting

Figure A7: Differential Sorting By Income Group

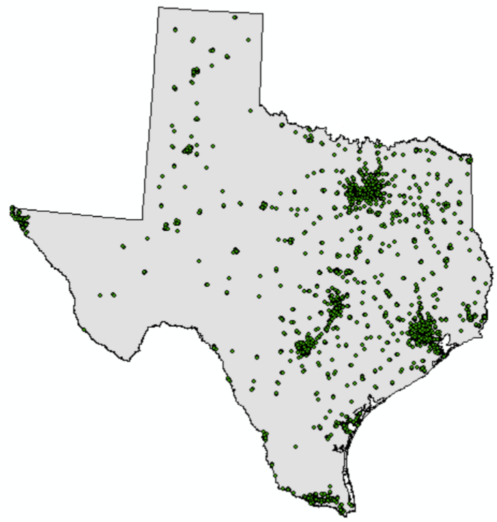
(a) (b) (c)



Notes:

B Data Appendix

Figure B1: LIHTC in Texas



Notes: Each dot marks the location of a rental housing development under LIHTC program oversight. Source: Texas Department of Housing and Community Affairs.

B.1 Assembling the Panel

In this section I provide detail of the data sources behind the school and housing panel data. The first piece of the data is the school level finance data provided publicly by the Texas Education Agency (TEA).[14] TEA provides total expenditure data and total spending for instruction dating back to 2000, along with enrollment counts and the racial composition of each school. I balance the panel for schools in 2000-2020 based on the TEA finance data.

Additional data describing teacher counts and the count of students income qualifying for lunch subsidies are provided by National Center for Education Statistics table generator for the years 2000-2020 at the campus level. The two sources have different school identifiers, so the additional data is merged to the finance data using a crosswalk of school IDs pro-

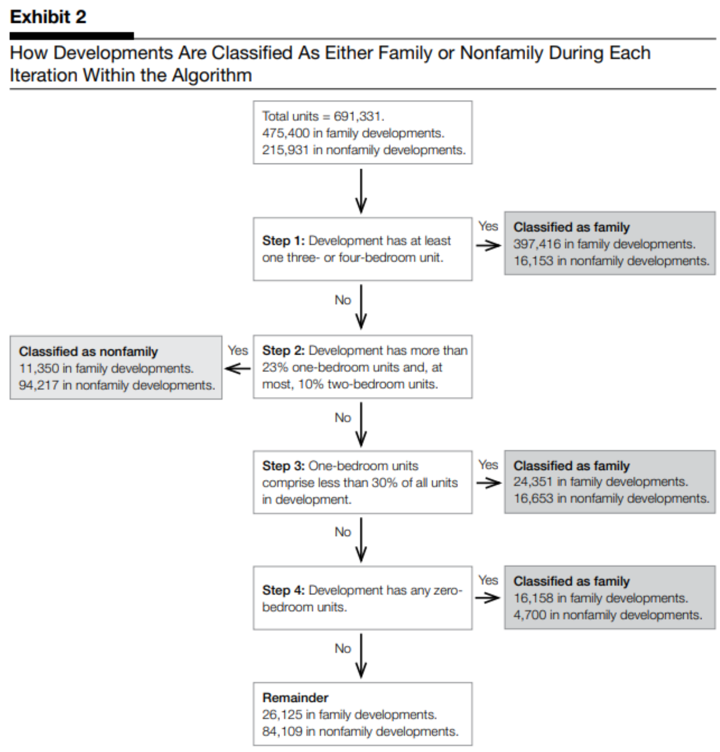
[14]The TEA managed *Texas Academic Performance Reporting* system has public databases available back to the 2013 school year. The *Academic Excellence Indicator System* housed the data prior to 2013. The data prior to 2004 is not listed on the Academic Excellence Indicator System website but remains available via the archive. Navigating to the 2004 webpage then adjusting the url with the desired year will take you to the pre-2004 data. A unique campus identifier is consistent for schools across both systems.

vided by TEA via email request. The sample is restricted to non-charter schools and those without open attendance boundaries. My analysis is limited to schools with available spatial data for the school attendance zones, which comes from The School Attendance Boundary Information System (SABINS) project for the 2009-2010 school year. The SABINS project was carried out by researchers at University of Minnesota, William and Mary, and Census IPUMS and was discontinued after the 2009-2010 school year.

I aggregate LIHTC data to school zones by first mapping each individual LIHTC complex to a census block using shapefiles loaded to ArcGIS. The LIHTC data from Texas Department of Housing and Community Affairs (TDHCA) is coded with latitude and longitude data that I use to map each LIHTC complex to a census block. Each housing observation is then merged to a school zone to be aggregated by year, using a SABINS census block to school attendance zone crosswalk publicly available through NHGIS. To facilitate identification in my event study design, I limit the sample to those schools in which no LIHTC was assigned prior to the start of the sample. This restriction, combined with the non-charter non-open boundaries restriction leaves me with 3,925 schools across 688 districts.

Of the 2901 LIHTCevents in the sample, I must classify each as family, senior, or other LIHTC types. To do this I follow the procedure below, obtain from Atkins and O'Regan (2014). 297 LIHTC observations are missing data for the quantity of units in the complex. For those observations I fill the data using the zip-code median for the entire sample. From the algorithm, 783 are classified as targeting senior aged population groups and used for placebo tests. The remainder are taken as family targeted LIHTC.

Figure B2: Denoting LIHTC as Family or Senior Housing

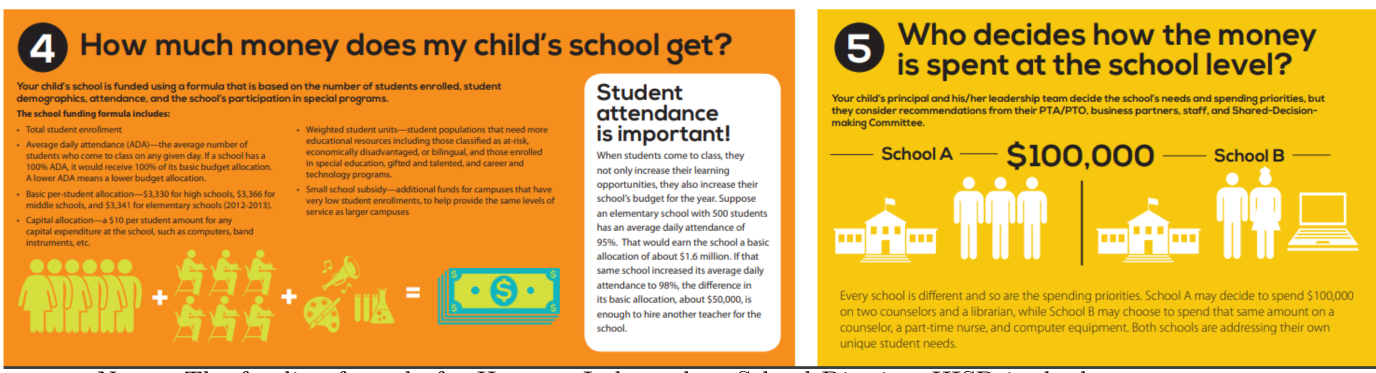


Notes: Classification of LIHTC units by type. Source: Atkins and O'Regan (2014)

B.2 Texas Funding Formulas

As with many states, although property tax rates are school district specific, the revenues themselves are "rolled" to the state level for redistribution in the name of base funding equalization. States themselves have funding formulas that assign dollar amounts to districts based on a complex funding formula. School districts in Texas themselves have a great deal of fiscal authority, charged with allocative decisions across schools in the district. District funding formulas are often presented to be much simpler in nature, and the majority of districts in Texas are transparent about underlying school funding formulas. The analysis of Section 3.2 is an empirical approximation for the average funding formula in Texas for a given year. An example of the district funding formula for Houstin ISD, the largest in Texas, is shown in Figure B3.

Figure B3: An Example of the District to School Allocation Process



Notes: The funding formula for Houston Independent School District. HISD is the largest public school system in Texas and one of the ten largest in the United States. Source: HISD Budget Basics. https://www.houstonisd.org/cms/lib2/TX01001591/Centricity/Domain/22921/Budget_BasicsRd2_rev022113b.pdf