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All Economics Is Local: How Macroeconomic Trends Affect the Political Economy of Neighborhood Schools

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All Economics Is Local:

How Macroeconomic Trends Affect the

Political Economy of Neighborhood Schools

A dissertation submitted in partial satisfaction of the

requirements for the degree of Doctor of Philosophy

in Economics

by

Owen Foley Hearey

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ABSTRACT OF THE DISSERTATION

All Economics Is Local:

How Macroeconomic Trends Affect the

Political Economy of Neighborhood Schools

by

Owen Foley Hearey

Doctor of Philosophy in Economics

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Professor Leah Michelle Boustan, Chair

This dissertation uses the institutions of public schooling in the U.S. as a lens to study how broad economic trends – rising income inequality and the business cycle – affect household residential choice, neighborhood composition and popular support for local public goods.

The first chapter explores the consequences of rising neighborhood inequality for public schools. Income inequality across neighborhoods more than doubled in the U.S. between 1970 and 2010. This spatial reallocation may affect public schools through changes to the distribution of peers and support for local taxes. I find that rising neighborhood inequality within a school district increases local school funding, but also depresses human capital investment, primarily due to a widening gap between low- and high-income neighborhoods. These results are robust to instrumenting for changes in neighborhood incomes with the ini-

tial allocation of households interacted with differential national trends in household income growth by percentile.

The second chapter proposes an economic model to explain these findings. Public schools are customarily funded by a district-wide property tax, yet school quality varies considerably within districts due partly to neighborhood differences in student preparedness. In response, high-income households may choose to cluster in a few neighborhoods, lowering the average income of households in the other neighborhoods. The district's median voter, to compensate for a decline in peer quality in her own neighborhood, may elect to raise the district-wide tax rate. Consistent with the implications of this model, I find empirically that declining income in the median voter's neighborhood is associated with increasing local tax revenue per household.

The third chapter examines the business cycle dynamics of public school quality valuation. While the value of school quality improvements is critical to human capital investment decisions and education policy, little is known about how it varies with the business cycle. We apply a hedonic pricing model to data on home sales in Los Angeles County between 2000 and 2013 to study changes over time in homeowners' valuations, exploiting elementary school attendance boundaries to provide identifying variation. We find that homeowners' valuations are countercyclical – they value quality improvements more during "busts" than in "booms."

The dissertation of Owen Foley Hearey is approved.

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2016

Dedicated to the teachers and staff

of the Shaker Heights City School District

Contents

List of Figures

List of Tables

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Chapter 1

The Effects of Rising Neighborhood Inequality on Public Schools

1.1 Introduction

Income inequality has been rising in the United States since the 1970s. Increasingly, this inequality is expressed spatially, as growing differences in average income across neighborhoods (Watson, 2009; Reardon and Bischoff, 2011).[1] Some neighborhoods face concentrated poverty while others, like gated communities in some suburbs, are uniformly affluent. Because one's neighbors appear to play an important role in many areas – in education, public health, and crime, to name a few – the rise in income inequality across neighborhoods could have far-reaching effects.

This paper shows that rising income inequality across neighborhoods within school districts has affected local funding for public schools, the enrollment decisions of young adults, and the gap in enrollment between low- and high-income neighborhoods. As income sorting may be a cause or a consequence of variation in neighborhood schools, I construct a shift-share, Bartik-style instrument for across-neighborhood income inequality by interacting the initial allocation of households with differential national trends in household income growth by percentile. I find that the rise in income inequality across neighborhoods between 1970 and 2010 caused real local tax revenue per household to increase 11.2 percent, explaining about 23 percent of the growth in local funding for public schools during this period. But the rise also depressed enrollment rates in any school, public or private, among young adults by 6.3 percentage points overall, and served to widen the gap in enrollment between low- and high-income neighborhoods by 1.8 percentage points.

[1] Between 1970 and 2010, the Theil index of household income inequality grew from 0.26 to 0.35, while the component explained by average differences across Census tracts more than doubled, growing from 0.04 to 0.09.

To understand the mechanisms driving these findings, I propose and test an economic model of household residential choice, voting and public education within a school district. The key insight of the model hinges on the observation that public schools are usually funded by a district-wide property tax, but school quality varies considerably across neighborhoods due, in part, to differences in student preparedness. As a result, high-income households may choose to cluster in certain neighborhoods, lowering the average income of residents in the median voter's neighborhood. Consistent with the model's predictions, I find empirically that the median voter substitutes a higher property tax rate for declines in neighborhood peer quality.

This paper offers three main contributions to the literatures on urban economics, local public finance and the economics of education. First, I develop a new source of identification to study the causal effects of the spatial dimensions of income inequality. This method can be applied at different levels of geography and for different outcomes (e.g. crime). Second, I use this new instrument to provide the first evidence that across-neighborhood income inequality affects local public finance and education. Third, I provide a model that highlights the interaction between neighborhood-level changes in income and voter preferences in setting district tax policy. Existing work on the effect of income inequality at the local level has instead emphasized the "tax price" of public goods – as the income distribution widens, the median voter elects to raise revenue from the rich – while ignoring changes in the distribution of income across neighborhoods (Corcoran and Evans, 2010; Boustan et al., 2013).

The results suggest that rising income inequality across neighborhoods increases local school funding, but also fosters a less productive allocation of students to neighborhoods which blunts the marginal benefit of additional district expenditures. These findings help account for the persistent gap in educational attainment between children of low- and high-socioeconomic status families, as documented by Reardon (2011), despite steadily rising per-pupil expenditures. They provide an explanation for the puzzling result given in Jackson, Johnson, and Persico (2014), that endogenous increases in local school funding tend to be less productive than state court-ordered exogenous ones. More broadly, they illuminate causal linkages between three correlates of low mobility areas identified by Chetty et al. (2014): household income inequality, income segregation, and poor school quality.[2]

The remainder of the paper is structured as follows. Section 1.2 gives a brief overview of recent important trends in income inequality and public education in the U.S. Section 1.3 details the empirical approach, and illustrates the construction of the instrumental variable for across-neighborhood income inequality. Section 1.4 describes the panel of school districts, while section 1.5 presents the estimated causal effects of across-neighborhood income inequality. To elucidate the mechanisms underlying these results, the median neighborhood model is sketched in section 2.1, and tested empirically in section 2.2. The final section concludes.

[2]My results were generated independent of de Frahan and Sloane (forthcoming), which finds that increases in metropolitan area-level wage inequality cause community college enrollments to fall. I come to a similar conclusion about income inequality's effect on human capital investment, despite using a different level of analysis, employing a broader measure of enrollment, and taking a different approach to construct a shift-share instrument for local income inequality.

1.2 Background

Increasingly, rising household income inequality has manifested as spatial differences in income, particularly across neighborhoods within school districts. Since the 1970s, household income inequality has grown appreciably in the U.S. Figure 1.1 shows two measures of household income inequality, the Gini coefficient and Theil index. Both measures range from zero, representing perfect equality, to one, representing perfect inequality, and both show a steady upward trend, with limited pullback over the most recent decade. While both measures capture the rise in income inequality, the Theil index offers the analytical advantage of being geographically-decomposable. Figure 1.2 exhibits the portion of household income inequality explained by differences across Census tracts, a small geographical area akin to a neighborhood encompassing about 4,000 individuals on average. The component explained by differences across tracts has more than doubled over the period, growing from 0.043 in 1970 to 0.088 in 2010, as households sort themselves increasingly by income, and as pre-existing sorting by income becomes more pronounced with the growth in household income inequality. Moreover, most of the explanatory power of geography comes from household sorting across tracts within school districts, not from Tiebout-style sorting across school districts within a metropolitan area, or from the emergence of "superstar cities" within the U.S. (Gyourko, Mayer, and Sinai, 2006).

Over the same span, school funding has increased dramatically, while the gap in educational attainment between children of low- and high-income families has remained fairly steady – despite concerted efforts by state and federal governments to close it. The rise in funding for public K-12 education is charted in Figure 1.3. Real total revenue per pupil has increased steadily, more than doubling over the 1970 to 2010 period; by 2012, 18 percent of all local and state government expenditures went toward K-12 education, more than any other single category (Barnett et al., 2014). Nonetheless, the gap in educational attainment by socioeconomic status has not shrunk markedly (Reardon, 2011). Figure 1.4 shows the share of those aged 16-19 enrolled in either public or private school, roughly capturing young adults' tendency to complete high school and make the jump to post-secondary education. While neighborhoods above the 80th percentile for mean family income in their school district enjoyed enrollment rates rising from 83 to 90 percent, those below the 20th percentile lagged somewhat behind, with enrollment rates ranging from 68 percent to 82 percent.[3] These facts have raised important questions about the marginal productive benefit of additional education funding, and the efficiency of public school operations (Hanushek, 1986).

This paper asks to what extent the rise in income inequality across neighborhoods can account for these trends. That is, holding the distribution of household income constant within a school district, how do increases in average income differences across neighborhoods (either due to increases in household sorting by income, or divergent trends in household income) affect local public finance and education?[4] The character, financially and socially,

[3]To assess education performance, previous work has considered student-to-teacher ratios, teacher salaries, length of school year, test scores, and adult labor market outcomes, among other metrics (Card and Krueger, 1992; Bayer, Ferreira, and McMillan, 2004; Jackson, Johnson, and Persico, 2014). While these are all informative, enrollment rates offer the distinct advantage of being available widely and consistently over a long time horizon at a disaggregated level, permitting analysis not just of average district outcomes but also of within-district dynamics. And among common measures of school performance, Hanushek and Raymond (2002) highlights drop-out, retention and graduation rates for having a "strong" relationship to student achievement – all outcomes reflected in enrollment rates.

[4]In light of their taxing and budgeting powers and on the strength of Tiebout sorting arguments, school districts have historically been the *de rigueur* unit of analysis for those interested in school finance and its effect on education outcomes. But increasingly researchers are noting spending inequities within districts

of a child's primary and secondary education is determined in large part by her neighbors. While public schools are financed by all levels of government, during the period 1970 to 2010, 44 to 48 percent of total funding came from local sources, predominantly property tax revenue. Moreover, 88 to 90 percent of K-12 students were enrolled in a public school, the vast majority in a local neighborhood school (U.S. Department of Education, 2015a).[5] As a result, changing neighborhood circumstances – incomes rising in one neighborhood while falling in another, with attendant changes to home prices, student populations, and households' willingness to pay for improvements to school quality – could impact both the district's financial standing and the production of public education.[6]

1.3 Empirical Approach

1.3.1 Reduced form

The school district-level, reduced-form estimating equation for local tax revenue is:

$$ln(r) = \beta_0^r + \beta_1^r \cdot T_n + \boldsymbol{\beta_2^r} \cdot \boldsymbol{X} + \boldsymbol{\beta_3^r} \cdot \mathbb{1}_{\boldsymbol{T}} + \boldsymbol{\beta_4^r} \cdot \mathbb{1}_{\boldsymbol{D}} + \epsilon^r \tag{1.1}$$

(Roza et al., 2004), with some states going so far as to account for them in school funding aid formulae (Odden, 1999). Neighborhood differences in income in particular, are gaining notice in the courts, as racial desegregation efforts meet stiffer resistance (Kahlenberg, 2006).

[5] Even by 2010, only 6.5 percent of those enrolled in public K-12 schools were in charter or magnet schools (U.S. Department of Education, 2015b).

[6] Murnane (2013) details how the decision of young adults to arrest their studies, in particular, may be affected by both school spending and peer composition.

where r is local tax revenue per household, T_n is across-neighborhood income inequality, and $\boldsymbol{X}$, $\mathbb{1}_{\boldsymbol{T}}$ and $\mathbb{1}_{\boldsymbol{D}}$, are vectors of controls, time dummies, and fixed effects respectively. For district-level enrollment, we have:

$$e = \Phi\left\{\beta_0^e + \beta_1^e \cdot T_n + \boldsymbol{\beta_2^e} \cdot \boldsymbol{X} + \boldsymbol{\beta_3^e} \cdot \mathbb{1}_{\boldsymbol{T}} + \boldsymbol{\beta_4^e} \cdot \overline{\boldsymbol{X}} + \epsilon^e\right\} \tag{1.2}$$

where e is the enrollment rate of those aged 16-19 in any school, public or private, and time averages of all right-hand side variables ($\overline{\boldsymbol{X}}$) have replaced the fixed effects to accommodate the incidental parameters problem (Papke and Wooldridge, 2008).

Estimating time dummies removes any common time trend, while accounting for fixed effects takes out any time-invariant unobservable heterogeneity across school districts.[7] Controlling for a host of socioeconomic characteristics eliminates any time-varying, but observable, confounding factors (chief among them, changes in average household income).

I also regularly include a pair of additional control variables specifically to test the validity of various alternative hypotheses. The first two theories concern the provision of local public goods. The tax price hypothesis, originally put forth by Meltzer and Richard (1981), posits that the median voter raises the local property tax rate in response to increases in household income inequality however expressed, spatially or not; this occurs because as household income inequality grows, property value inequality grows as well, and the tax price of additional government services falls for the median voter. By including within-neighborhood income inequality, the component of the household income inequality Theil index *not* ex-

[7] For instance, some school districts could operate more efficiently than others (Downes and Pogue, 1994).

plained by cross-neighborhood differences in average incomes, I can directly test whether both "across" and "within" components of income inequality affect local taxes.

Another hypothesis, advanced by Alesina, Baqir, and Easterly (1999) among others, suggests that more ethnically fragmented communities support lower government expenditures on local public goods such as education. The addition of the black/white neighborhood dissimilarity index as a control variable in the local tax revenue regressions accounts for the possibility that changes in racial segregation, rather than rising income differences across neighborhoods, are driving any apparent results.[8]

Finally, an extensive literature has debated in what ways, and to what extent, peers play a role in education outcomes.[9] In particular, Chetty et al. (2014) finds that metropolitan areas with higher household income inequality are often also areas with poorer school quality, while Cutler and Glaeser (1997) shows that blacks in more racially segregated cities tend to have lower graduation rates. Including controls for both within-neighborhood income inequality and black/white dissimilarity in the enrollment rate regressions permits me to separate the causal effect of increasing neighborhood income differences on school participation from that of increasing household income inequality *per se* or racial segregation.

[8]The dissimilarity index is an evenness index, ranging from zero (perfect integration) to one (perfect segregation). The index may be interpreted as the share of one race that would have to switch neighborhoods to completely integrate the school district.

[9]See, for instance, Hoxby (2000), Guryan (2001), Ding and Lehrer (2007) and Jackson, Johnson, and Persico (2014).

1.3.2 Instrumental variables

Identifying causal relationships in this setting can be quite difficult, however. Consider first the local tax revenue specification, equation 1.1. Corcoran and Evans (2010) and Boustan et al. (2013) found that increasing household income inequality caused local government expenditures to increase, so our prior may be that rising income inequality across neighborhoods will exhibit a similar effect (i.e. $\beta_1^r > 0$). But consider the reverse – how would across-neighborhood income inequality respond to a positive shock to tax revenues (resulting, say, from a regional boom in home prices)? If the exogenous revenue increase reduces the incentive for households to sort by income across neighborhoods, then this reverse causality would bias the estimate of β_1^r toward zero.

Or consider the enrollment specification, equation 1.2. In line with Chetty et al. (2014), we may expect school districts with higher across-neighborhood income inequality to have poorer quality schools (i.e. $\beta_1^e < 0$). But, if we are correct that rising across-neighborhood income inequality also leads to increases in local funding, which should (all else equal) raise enrollment rates, then this omitted variable would bias the estimate of β_1^e toward zero.

An instrumental variable providing exogenous variation in T_n permits unbiased estimation of causal effects. This paper extends the insight of Boustan et al. (2013) to advance a Bartik-style, shift-share approach for studying the spatial dimensions of inequality. It exploits the fact that the national trends in household income growth (shown in Figure 1.5) would be expected to predict changes in neighborhood income even absent further household sorting. And because different school districts have different initial populations, both compositionally and in the degree of neighborhood segregation by income, the national trends in household income growth offer different predictions for changes in across-neighborhood income inequality for each district.

Construction of the instrument is depicted in Figure 1.6, which shows a hypothetical school district split into two neighborhoods at three points in time – a baseline period ($t = 0$) and two periods of interest ($t = 1, 2$). The top images show the "actual" evolution of the distribution of households over time. The bottom images show the predicted evolution. The predictions ($\widehat{T_n}$) are calculated by:

1. Fixing the distribution of households as of the baseline period;
2. Identifying each household's percentile in the national income distribution; and then
3. Projecting each household's income forward according to the national growth trend for its percentile.

In this case, three low-income and two high-income households are identified at $t = 0$, and their income growth is projected forward using national trends for their respective income percentiles. The projected changes in neighborhood income inequality from $t = 1$ to $t = 2$ are used as instruments for the actual changes.

The instrument is valid if it is sufficiently correlated with the endogenous variable but uncorrelated with the error term. The former may be satisfied by construction – particularly, the more informative the distribution of households at $t = 0$ is about the distribution of households at $t = 1, 2$. This suggests that reducing the length of time between the baseline period and the periods of interest will produce a stronger instrument.

The exclusion restriction is satisfied so long as the instruments have no direct effect on the outcome of interest; that is, the projected change is only correlated with the outcome of interest through its correlation with the actual change. Changes in the national income distribution alone should be expected to have little to no direct effect on individual school districts, but it remains possible that the interaction of those changes with the initial distribution of households may. This could occur, for instance, if a district with a concentration of low-income households at $t = 0$ would be expected *ex ante* to have declining tax revenue per household between $t = 1$ and $t = 2$. Such an "adjustment period" may be common empirically, as it may take many years for property values and tax rates to reflect changes in the district population.

In constructing the instrument, the approach of this paper is to use decadal data, varying the baseline period for later periods of interest; that is, the distribution of households in 1960 is used as a baseline to project the change from 1970 to 1980, the distribution of households in 1970 is used as a baseline to project the change from 1980 to 1990, and so on. This method ensures that the baseline period remains sufficiently informative about the periods of interest throughout, while giving districts at least ten years to adjust to the circumstances of the baseline period. And to further safeguard against the potential direct effect of lagged initial conditions, the regressions which use the instrument often include the baseline period's across-neighborhood income inequality as an additional control variable.

1.4 Data

Decennial tract-level counts of household income (of families, by income bin) as well as of persons, households, school-aged children and housing units are provided by NHGIS for 1960-

2010.[10] Tracts are assigned to districts (neighborhoods) using ArcGIS.[11] The Census only specified tracts for major metropolitan areas in 1960, but additional areas were demarcated each decade until 1990, when tracts for virtually the entire country had been defined. The panel of school districts used in estimation is unbalanced as a result.

Decennial district-level tax revenue data for 1970-2010 are provided by the National Center for Education Statistics (NCES).[12] School district boundaries and school attendance areas as of the 2010-11 school year are provided by the School Attendance Boundary Identification System (SABINS).[13] While school district boundaries are readily available for the entire country, school attendance areas are not. Only some states feature complete coverage (e.g. Delaware, Minnesota, Oregon), and the resulting sample may not be representative of the nation as a whole.

To address this concern, this paper considers three methods of defining neighborhoods. First, for the subset demarcated by SABINS, neighborhoods may be defined so that households in each neighborhood were in the same elementary, middle and high school attendance areas in 2010. Second, the tracts as defined in each Census year may be used as neighborhoods. While these definitions may not comport with actual school attendance patterns, they are available whenever tract-level Census data are and, since they tend to be smaller,

[10]Tract-level counts of household income for 1960-1970 are not available. Results using household income for 1980-2010 are broadly similar to those using family income.

[11]Some tracts overlap multiple districts (neighborhoods). In this case, the tract population is allocated proportionally to the overlapping districts (neighborhoods), assuming a uniform distribution of the population throughout the tract.

[12]Data are available via the Elementary and Secondary General Education System (ELSEGIS) for 1970-1980, the Common Core of Data (CCD) for 1990-2010.

[13]School district consolidation or fragmentation is rare in the U.S., particularly since 1970 and particularly among larger, urban districts (Kenny and Schmidt, 1994). For a recent discussion of school district mergers, see Gordon and Knight (2009).

can measure evidence of neighborhood income inequality even in small school districts with only one attendance area. But one complication with using concurrent tract boundaries is that they tend to change over time. While this does not pose a significant problem for observing changes in school districts (as districts usually encompass several smaller tracts), observing changes in a particular neighborhood demands a consistent geographic definition. As a third and final approach, I use the 2010 tract boundaries to create a panel of static, well-defined neighborhoods. Because this method yields the most comprehensive sample and permits estimation of both district- and neighborhood-level outcomes, the static-tract panel is my preferred sample for estimation – though results using all three methods are provided for comparison.

The estimation samples include only those school districts with:

- both primary and secondary schools, providing education for Kindergarten through 12th grade;[14]
- at least two constituent Census tracts;
- at least two-thirds of the school district enumerated;
- at least 200 households and 200 students in each district;
- non-zero reported enrollments, revenues and expenditures.

Figure 1.7 shows the final set of school districts included under the static-tract neighborhood definitions. (The concurrent-tract panel spans a similar set.) Likewise, Figure 1.8 shows the sample under SABINS definitions. In 2010, the static- and concurrent-tract panels represented more than 280 million people, while the SABINS panel, despite it's dramatically smaller geographic scope, still represented nearly 130 million people.

[14]Significant portions of a few states (notably California, Illinois and New Jersey) relied on separate school districts in 2010. As Corcoran et al. (2004) point out, elementary- and secondary-only school districts likely have different cost structures than unified ones, making direct comparison problematic.

Summary statistics for the static-tract panel of school districts are provided in Table 1.1, while Table 1.2 presents a comparison of the three panels described above. First, note that the distributions of both outcomes of interest are not approximately Normal. Local tax revenue per household is skewed right while enrollment rates are (necessarily) bounded by zero and one, with a significant point mass at the upper bound. Consequently, local tax revenue per household will be estimated in logs, while enrollment rates will be estimated by fractional probit regression, as recommended by Papke and Wooldridge (2008). Further, note the appreciable skewness of the districts' size distribution. Though the median school district in the static tract panel had only 11,012 residents and eight constituent tracts, the largest school district (New York City) has over 8 million residents and 2,243 constituent tracts. As my principle interest is in people, not school districts themselves, my preference is to estimate the school district specifications weighting by population, but unweighted estimates will be presented as well.

1.5 Causal Estimates

1.5.1 Local tax revenue

The results suggest that across-neighborhood income inequality and local school revenue are positively related. Table 1.3 presents ordinary least squares estimates of the fixed effects model for the static-tract panel of school districts. Across a range of specifications, the estimates suggest that a one-standard deviation increase in across-neighborhood income inequality (all else equal) is associated with an approximate 4.2 to 5.1 percent increase in local tax revenue per household. In fact, as column 3 shows, after controlling for the component of household income inequality explained by cross-neighborhood differences, the remainder is negatively related to tax revenues – contrary to the prediction of the tax price hypothesis. The black/white dissimilarity index also exhibits a negative relationship with the outcome of interest, lending support to the notion that racially heterogeneous communities may have more difficulty building consensus on public policy.

The positive relationship between neighborhood income inequality and local school revenue is consistent across alternative estimation approaches. The most exacting specification from Table 1.3 is replicated in Table 1.4 for every combination of panel and regression weighting approach. These estimates suggest that a one-standard deviation increase in neighborhood income inequality (all else equal) is associated with an approximate 1.1 to 6.3 percent increase in local tax revenue per household. The relationship is more pronounced in the regressions which weight by district population, signaling that neighborhood differences are more impactful in larger, more populous school districts. But across all specifications there is no debate as to its sign or statistical significance. Within-neighborhood income inequality shows a consistent negative relationship with local tax revenue, while the black/white dissimilarity index is regularly (though not often significantly) negatively related.

While these results are suggestive of a meaningful relationship between across-neighborhood income inequality and local school revenue, the shift-share instrumental variable approach is required to confirm that growing neighborhood differences have a truly causal effect. Table 1.5 shows the first-stage results for the static-tract panel of school districts – both without, and with, the baseline period's across-neighborhood income inequality as an additional control. The consistent, significant and positive sign on the instrument, as well as the sufficiently large Kleibergen-Paap F statistics, provide encouraging evidence that the instrument is sufficiently strong to identify causal effects.[15]

Estimates using the instrumental variable approach confirm a positive causal effect of across-neighborhood income inequality on local tax revenues for education. Ordinary and two-stage least squares estimates of the fixed effects model are presented in Table 1.6. The OLS estimate suggests that a one-standard deviation increase in income inequality across neighborhoods is associated with an approximate 5.0 percent increase in local tax revenue per household, while the 2SLS estimates suggest that a one-standard deviation increase is associated with an approximate 16.3 or 7.5 percent increase. Because across-neighborhood income inequality increased by about one-and-a-half standard deviations nationally between 1970 and 2010, the most robust result suggests that increasing neighborhood income differences caused real local tax revenue per household to increase by 11.2 percent on average over this period, or approximately 23 percent of the secular rise during this time.

The difference in magnitude between the OLS and 2SLS estimates is consistent with the reverse causality concern articulated in section 1.3, whereby exogenous increases in local tax revenues depress household incentives to sort into neighborhoods by income, biasing the OLS estimate toward zero. But it is also consistent with the possibility that the 2SLS procedure may be estimating a (larger) local average treatment effect. That is, because the instru-

[15]The Kleibergen-Paap F statistic is a Wald statistic based on the rk statistic proposed by Kleibergen and Paap (2006), which is robust to clustered errors. In the absence of alternatives, Baum, Schaffer, and Stillman (2007) suggest comparing the test statistics to the Stock and Yogo (2005) critical values, even though they assume i.i.d. errors. Because the critical value for a maximum test size of 0.10 with one exactly identified endogenous regressor is 16.38, the null hypothesis of weak instruments may be rejected for both specifications.

ment assumes each neighborhood's composition of household income percentiles is fixed, the 2SLS estimate may reflect the fact that increases in neighborhood differences have a more pronounced effect on local tax policy in less-dynamic districts with fewer household moves, spatially or economically. Alternately, the instrument may be correcting for measurement error present in the actual Theil index. In any case, the consistently positive and significant estimate supports the conclusion that rising across-neighborhood income inequality drives up local school funding.

1.5.2 Enrollment

Concerning enrollment of those aged 16-19 in any school, public or private, across-neighborhood income inequality appears to have a modest negative relationship with the tendency of young adults to stay in school. Table 1.7 presents quasi-maximum likelihood estimates of the fractional probit model for the static-tract panel of school districts; to ease interpretation, the table reports both coefficient estimates and average partial effects. While there is no clear relationship between income inequality across neighborhoods and enrollment rates when only controlling for district-wide average income, adding controls for other time-varying socioeconomic characteristics reveals a slight negative relationship. For the second and third specifications, the estimates suggest that a one-standard deviation increase in income inequality across neighborhoods (all else equal) is associated with an approximate 0.3 percentage point fall in the enrollment rate. Among alternative hypotheses, the component of household income inequality not explained by cross-neighborhood differences has no significant relationship with the enrollment rate, while the black/white dissimilarity index also exhibits a negative relationship with young adults' participation in school.

The negative relationship between across-neighborhood income inequality and enrollment rates is somewhat consistent across alternative estimation approaches. The most robust specification from Table 1.7 is replicated in Table 1.8 for every combination of panel and regression weights; to facilitate presentation, only average partial effects are reported here (and hereafter). These estimates suggest that a one-standard deviation increase in across-neighborhood income inequality (all else equal) is associated with an approximate 0.3 to 0.8 percentage point decrease in enrollment rates (though two of the six specifications imply no significant relationship). Within-neighborhood income inequality shows some evidence of a significant positive relationship with enrollment rates, while the black/white dissimilarity index is often (though not always significantly) negatively related.

Two-step instrumental variable estimation affirms that across-neighborhood income inequality has a negative causal effect on enrollment rates. Table 1.9 provides the first-stage results, while Table 1.10 compares the results estimated directly by quasi-maximum likelihood estimation with those that treat across-neighborhood income inequality as an endogenous variable. The quasi-MLE estimate suggests that a one-standard deviation increase in income inequality across neighborhoods is associated with an approximate 0.3 percentage point decline in enrollment rates, while the two-step IV estimates suggest that a one-standard deviation increase is associated with an appreciably larger approximate 1.4 or 4.2 percentage point decline. Because across-neighborhood income inequality increased by about one-and-a-half standard deviations nationally between 1970 and 2010, the most stringent specification suggests that increasing neighborhood income differences held back the growth in enrollment rates of those aged 16-19 by 6.3 percentage points on average during this period.

The difference in magnitude between the direct quasi-MLE and two-step IV estimates is consistent with the omitted variable bias concern articulated in section 1.3, whereby rising across-neighborhood income inequality is accompanied by rising local tax revenues, contributing (all else equal) to improvements in enrollment rates, which in turn bias the OLS estimate toward zero. And as with the public finance results, it is also consistent with the possibility of a (larger) local average treatment effect or measurement error in the Theil index. But once again, whatever the explanation for the difference, the consistent negative and significant estimate supports the interpretation that rising income inequality across neighborhoods serves to lower young adults' enrollment rates on average.

The deleterious effect of rising across-neighborhood income inequality on enrollment may not be bourn equally across neighborhoods, however. Because school enrollment is measured at the tract-level, and because the static and SABINS panels have consistent neighborhood boundaries over time, it is possible to test whether and to what extent low- and high-income neighborhoods respond differently to changes in across-neighborhood income inequality. Table 1.11 replicates the preceding two-step IV estimation, but at the neighborhood-level, now featuring the interaction of across-neighborhood income inequality and neighborhood income as an additional (endogenous) regressor.[16] The consistent and positive estimate on the interaction term reflects the fact that while rising neighborhood income differences lower enrollment rates on average, that effect is concentrated in low-income neighborhoods, and may not even be present in high-income neighborhoods.

[16]The critical value for a maximum test size of 0.10 with two exactly identified endogenous regressors is 7.03, so the null hypothesis of weak instruments may be rejected.

The impact of rising income inequality across neighborhoods on the gap in enrollments between low- and high-income neighborhoods is illustrated in Figure 1.9, which shows the estimated partial effect of a one-standard deviation increase in across-neighborhood income inequality for typical neighborhoods at different points in the distribution of neighborhood income within a school district. While a low-income neighborhood at the 10th percentile should expect a 1.3 percentage point fall in its enrollment rate as a result of a one-standard deviation increase in income inequality across neighborhoods, a high-income neighborhood at the 90th percentile should expect no significant change. All told, the results suggest that the rise in across-neighborhood income inequality nationally between 1970 and 2010 worked to forestall the closing of the gap in enrollment rates between low- and high-income neighborhoods by 1.8 percentage points during this period.

1.5.3 Summary of findings

To briefly summarize the findings so far, rising income inequality across neighborhoods causes local tax revenue per household to rise substantially, enrollment rates of those aged 16-19 in any school, public or private, to fall modestly, and the gap in enrollments between low- and high-income neighborhoods to widen. Because enrollment rates are found to fall in low-income neighborhoods despite a run-up in school funding, the results suggest that the loss of peers from higher-income households has a negative effect on the enrollment decisions of young adults from low-income households. Moreover, contrary to the predictions of the tax price mechanism, the component of household income inequality not expressed as neighborhood income differences is often negatively related with local tax revenue. A model of the median neighborhood, which can explain these observed results, is presented next.

Figure 1.1: Household Income Inequality Has Risen Steadily Since 1970

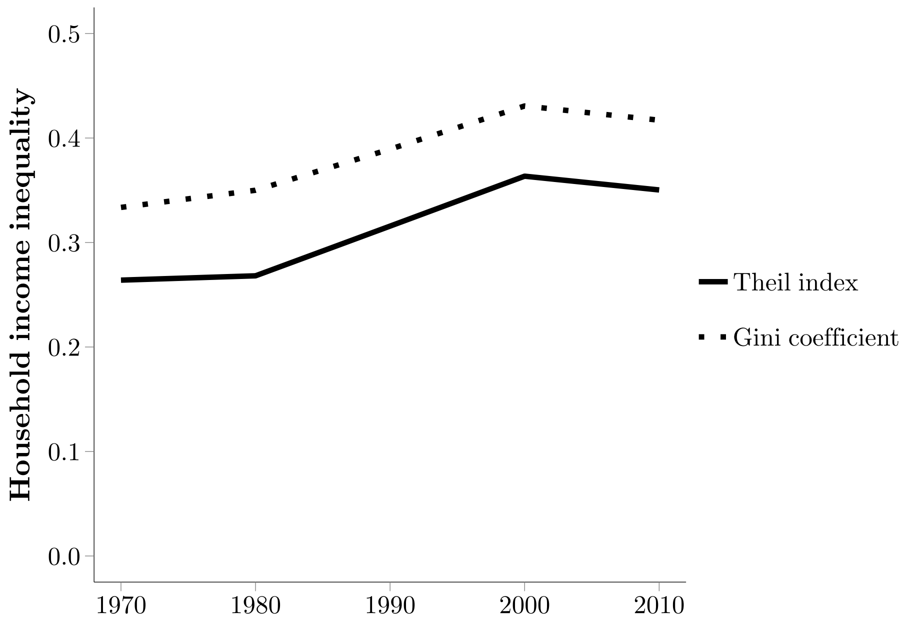


Figure 1.2: But Income Inequality Across Neighborhoods Has Grown Even More Rapidly

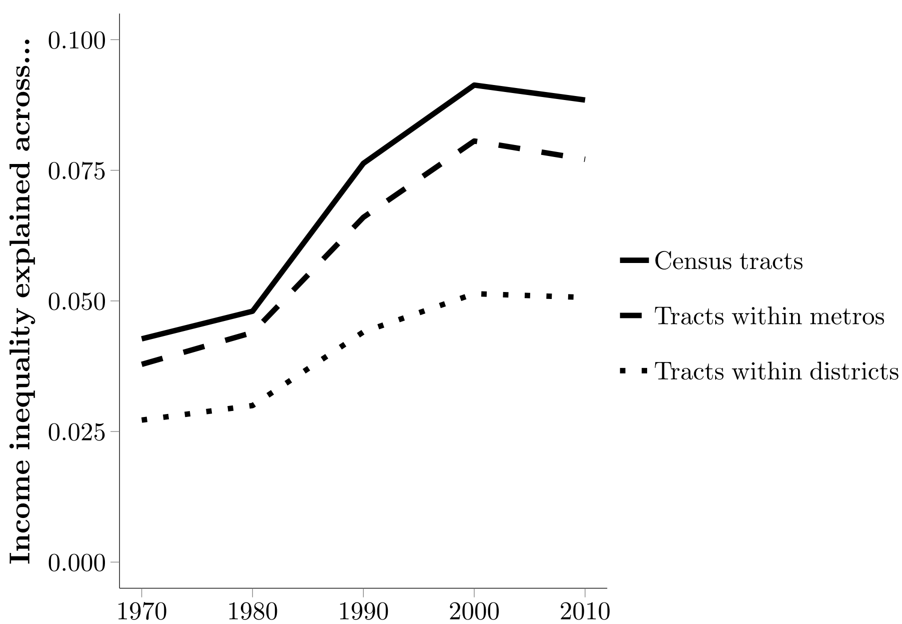


Figure 1.3: K-12 Funding Per Pupil Has More Than Doubled Since 1970

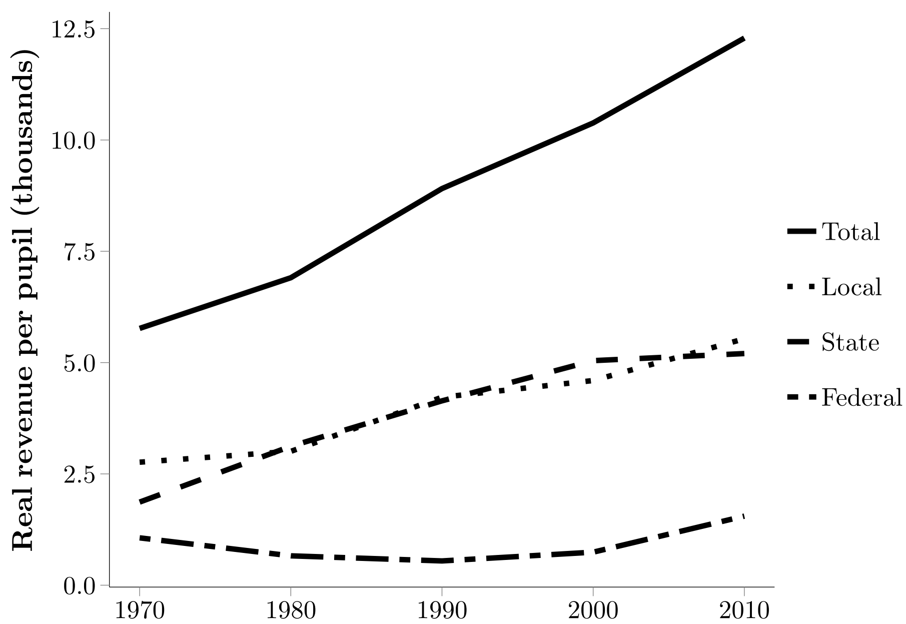


Figure 1.4: Yet the Gap in Educational Attainment by Neighborhood Income Percentile Has Largely Persisted

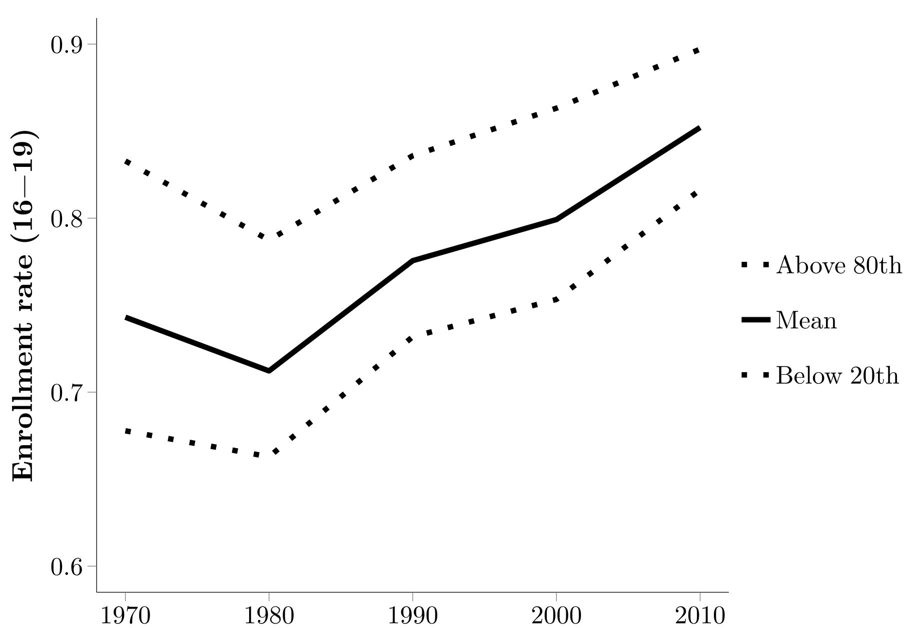


Figure 1.5: Differential Trends in Household Income Growth
Provide a Source of Exogenous Variation

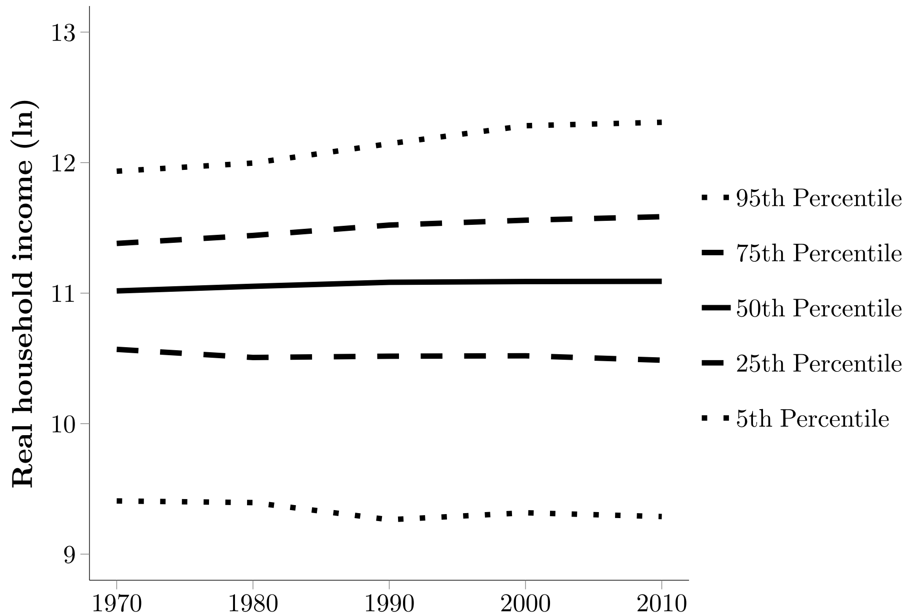


Figure 1.6: Constructing an Instrument for Income Inequality Across Neighborhoods

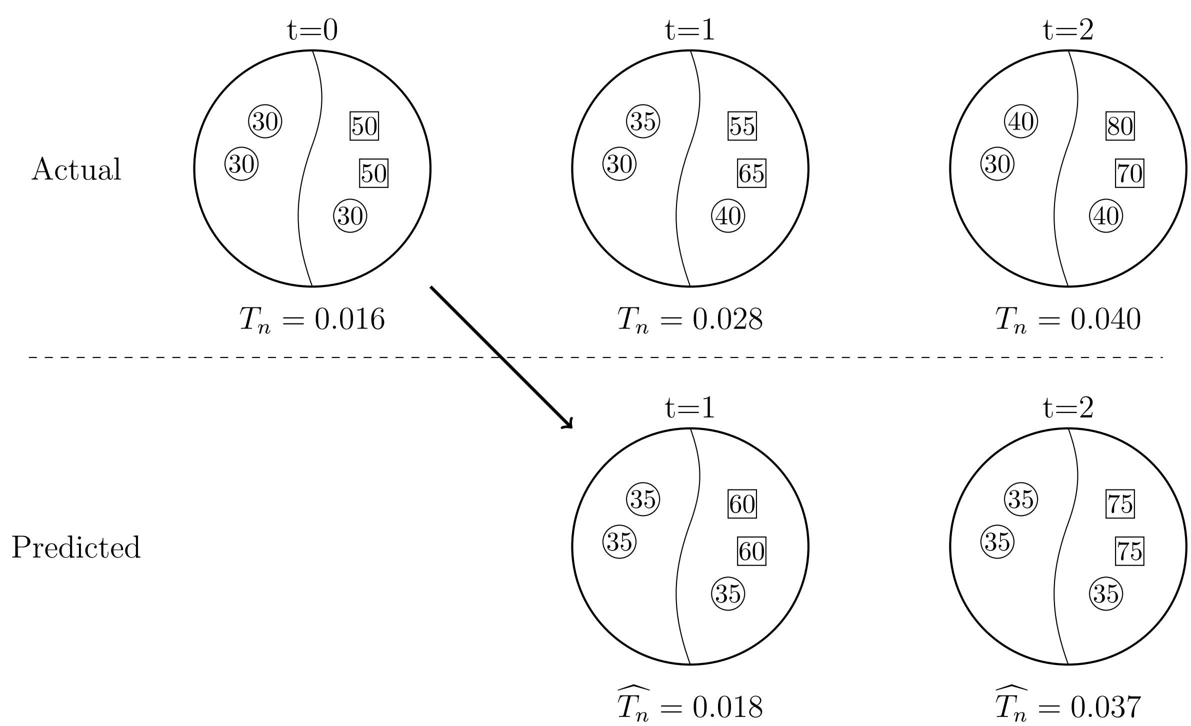


Figure 1.7: Static-Tract Panel

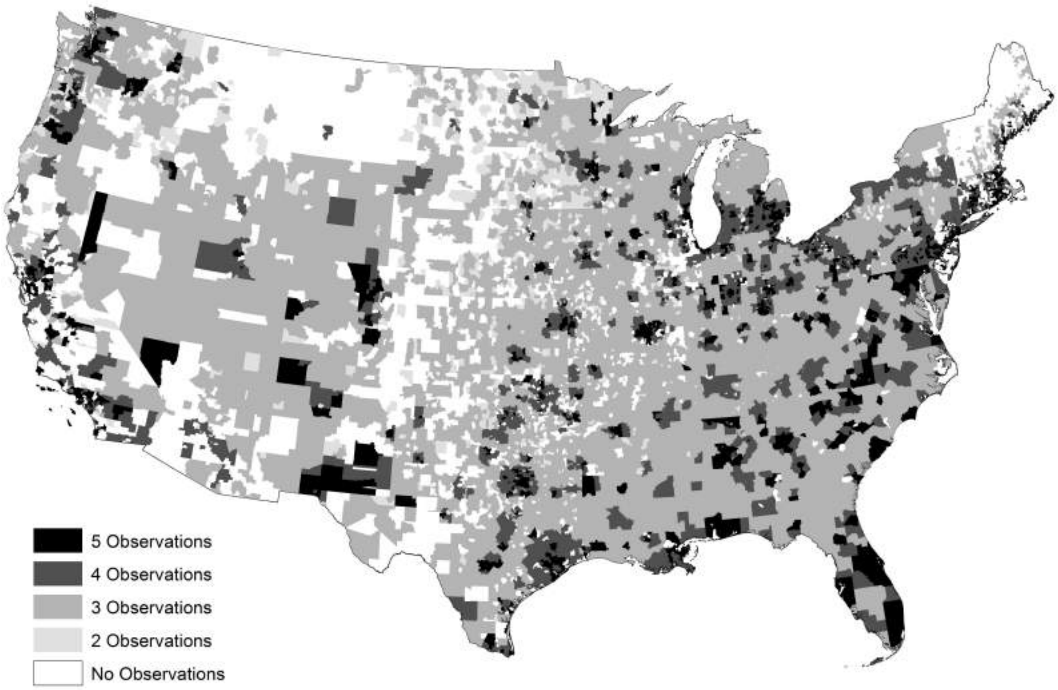
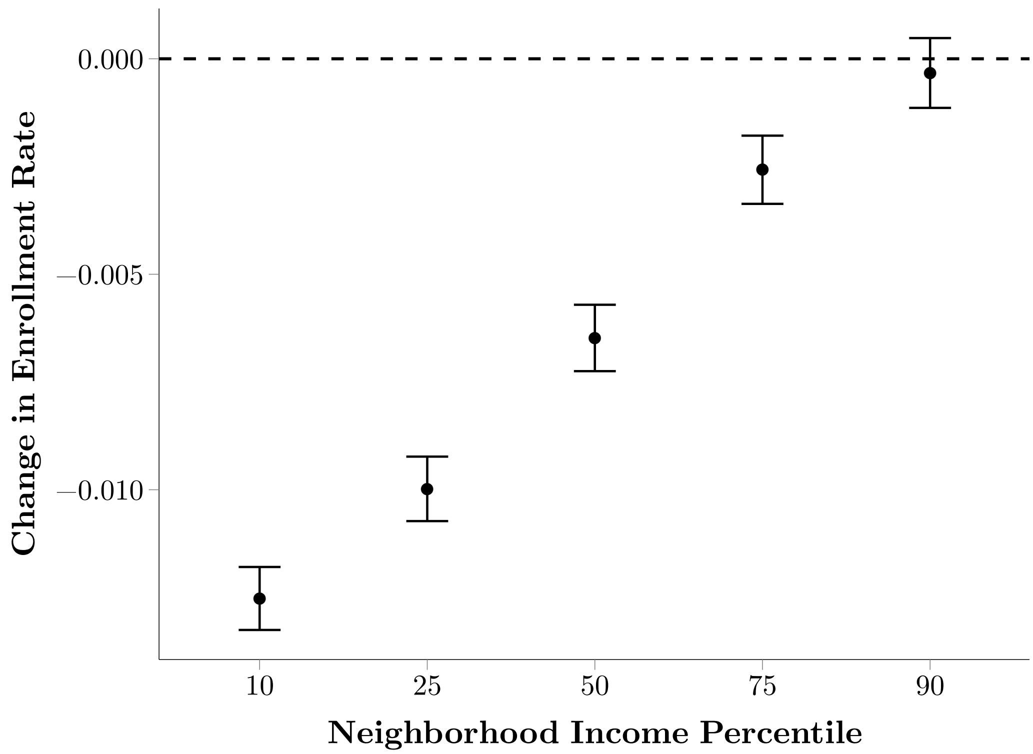


Figure 1.8: SABINS Panel

Figure 1.9: Across-Neighborhood Income Inequality Causes Gap in Enrollment Rates by Neighborhood Income to Grow



Note: Partial effects of a one-standard deviation increase in neighborhood income inequality calculated using the estimates in Table 1.11. 95 percent confidence intervals, indicated by the error bars, are the result of 100 bootstrap replications.

Table 1.1: Summary Statistics for Static-Tract Panel

	Mean	Median	SD	Min	Max
Outcomes of interest					
Local tax revenue per household ($)	2922	2446	2219	0	61095
Enrollment rate (16-19)	0.80	0.80	0.09	0.01	1.00
Principal variable of interest					
Across-neighborhood income inequality	0.011	0.005	0.016	0.000	0.249
Socioeconomic controls					
Family income ($)	72643	67484	22153	19497	292290
House value ($)	97547	75321	83284	6	1417310
Population	30313	11012	134294	701	8199221
Population per mi^2	766	90	1947	0	52273
Households with own children	0.39	0.39	0.09	0.09	0.89
Black	0.06	0.01	0.12	0.00	0.98
Hispanic	0.06	0.02	0.13	0.00	1.00
High school graduate (25+)	0.77	0.79	0.12	0.17	1.00
College graduate (25+)	0.18	0.15	0.11	0.01	0.84
Housing units owned	0.74	0.76	0.11	0.01	0.97
Children in poverty	0.15	0.13	0.10	0.00	0.76
Alternate hypotheses					
Within-neighborhood income inequality	0.25	0.24	0.06	0.10	0.67
Black/white neighborhood dissimilarity	0.33	0.31	0.20	0.00	1.00
Districts	9425				
Observations per district	3.58	3	0.78	2	5
Tracts per observation	13.46	8	37.66	2	2243

Note: All socioeconomic controls, except family income, house value, population, and population density are measured in shares. All dollar values are adjusted for inflation.

Table 1.2: School District Panel Comparison

	Static	Concurrent	SABINS
Outcomes of interest			
Local tax revenue per household ($)	2922	2939	2951
Enrollment rate (16-19)	0.80	0.75	0.79
Principal variable of interest			
Across-neighborhood income inequality	0.011	0.011	0.018
Socioeconomic controls			
Family income ($)	72643	72570	82070
House value ($)	97547	97100	122533
Population	30313	29782	154140
Population per mi^2	766	757	1561
Households with own children	0.39	0.39	0.41
Black	0.06	0.06	0.09
Hispanic	0.06	0.06	0.10
High school graduate (25+)	0.77	0.77	0.78
College graduate (25+)	0.18	0.18	0.23
Housing units owned	0.74	0.74	0.68
Children in poverty	0.15	0.15	0.13
Alternate hypotheses			
Within-neighborhood income inequality	0.25	0.25	0.24
Black/white neighborhood dissimilarity	0.33	0.34	0.30
Districts	9425	9716	708
Observations per district	3.58	3.58	4.33
Tracts per observation	13.46	13.31	47.64

Note: Means are reported. All socioeconomic controls, except family income, house value, population, and population density are measured in shares. All dollar values are adjusted for inflation.

Table 1.3: Across-Neighborhood Income Inequality and Local School Funding Are Positively Related

Dependent variable: Local tax revenue per household (log $)

	(1)	(2)	(3)
Principal variable of interest			
Across-neighborhood income inequality	2.61***	3.15***	3.16***
	(0.84)	(0.60)	(0.62)
Socioeconomic controls			
Family income (log $)	0.85***	0.91***	0.94***
	(0.06)	(0.26)	(0.26)
House value (log $)		-0.18	-0.19
		(0.12)	(0.12)
Population (log)		-0.14	-0.14
		(0.40)	(0.41)
Population per mi^2 (log)		0.15	0.13
		(0.40)	(0.41)
Households with own children		0.29**	0.28**
		(0.13)	(0.13)
Black		0.24	0.28
		(0.20)	(0.20)
Hispanic		0.67***	0.65***
		(0.23)	(0.23)
High school graduate (25+)		1.14***	1.05***
		(0.18)	(0.18)
College graduate (25+)		0.13	0.23
		(0.20)	(0.20)
Housing units owned		0.26	0.26
		(0.21)	(0.21)
Children in poverty		-0.71***	-0.51**
		(0.19)	(0.21)
Alternate hypotheses			
Within-neighborhood income inequality			-0.62***
			(0.20)
Black/white neighborhood dissimilarity			-0.07*
			(0.04)
District fixed effects	✓	✓	✓
Year dummies	✓	✓	✓
Observations	33433	33433	33433
Adjusted R^2	0.34	0.36	0.36

Note: All regressions use the static tract panel of school districts and are estimated by OLS, weighting by district time-average population. All socioeconomic controls, except family income, house value, population, and population density are measured in shares. All dollar values are adjusted for inflation. Standard errors, given in parentheses, are clustered at the district-level.

Table 1.4: Positive Relationship Is Consistent Across Panels and Weights

Dependent variable: Local tax revenue per household (log $)

	Static		Concurrent		SABINS	
	Wgt.	Unwgt.	Wgt.	Unwgt.	Wgt.	Unwgt.
Principal variable of interest						
Across-neighborhood income inequality	3.16***	0.67*	3.00***	0.56	3.95***	3.69***
	(0.62)	(0.37)	(0.59)	(0.36)	(0.92)	(0.89)
Alternate hypotheses						
Within-neighborhood income inequality	-0.62***	-0.24**	-0.60***	-0.23**	-0.51	-0.62
	(0.20)	(0.10)	(0.21)	(0.09)	(0.47)	(0.41)
Black/white neighborhood dissimilarity	-0.07*	-0.01	-0.06	-0.01	-0.11	-0.09
	(0.04)	(0.01)	(0.04)	(0.01)	(0.12)	(0.09)
Socioeconomic controls	✓	✓	✓	✓	✓	✓
District fixed effects	✓	✓	✓	✓	✓	✓
Year dummies	✓	✓	✓	✓	✓	✓
Observations	33433	33433	34103	34103	3061	3061
Adjusted R^2	0.36	0.20	0.36	0.20	0.49	0.31

Note: All regressions are estimated by OLS. Standard errors, given in parentheses, are clustered at the district-level.

Table 1.5: Instrument Is a Strong Predictor of Changes in Across-Neighborhood Income Inequality

Dependent variable: Across-neighborhood income inequality

	(1)	(2)
Instrument for...		
Across-neighborhood income inequality	1.20***	1.08***
	(0.08)	(0.12)
Socioeconomic controls	✓	✓
Alternative hypotheses	✓	✓
Baseline period control		✓
District fixed effects	✓	✓
Year dummies	✓	✓
Observations	28550	28550
Adjusted R^2	0.77	0.77
Kleibergen-Paap F	248.97	80.67

Note: All regressions use the static tract panel of neighborhoods, and are estimated by OLS, weighting by district time-average population. Standard errors, given in parentheses, are clustered at the district-level.

Table 1.6: Across-Neighborhood Income Inequality Has a Positive Causal Effect on Local School Funding

Dependent variable: Local tax revenue per household (log $)

	(1) OLS	(2) 2SLS	(3) 2SLS
Principal variable of interest			
Across-neighborhood income inequality	3.11***	10.16***	4.67*
	(0.63)	(1.35)	(2.73)
Alternate hypotheses			
Within-neighborhood income inequality	-0.64***	-0.62***	-0.34
	(0.22)	(0.22)	(0.24)
Black/white neighborhood dissimilarity	-0.07	-0.08*	0.01
	(0.04)	(0.04)	(0.06)
Socioeconomic controls	✓	✓	✓
Baseline period control			✓
District fixed effects	✓	✓	✓
Year dummies	✓	✓	✓
Observations	28550	28550	28550
Adjusted R^2	0.37		
Kleibergen-Paap F		248.97	80.67

Note: All regressions use the static tract panel of neighborhoods, and are weighted by district time-average population. Standard errors, given in parentheses, are clustered at the district-level.

Table 1.7: Across-Neighborhood Income Inequality and Enrollment Rates Are Negatively Related

Dependent variable: District enrollment rate (16-19)

	(1)		(2)		(3)	
	β	APE	β	APE	β	APE
Principal variable of interest						
Across-neighborhood income inequality	0.07	0.02	-0.65***	-0.18***	-0.65***	-0.18***
	(0.23)	(0.06)	(0.19)	(0.06)	(0.19)	(0.05)
Socioeconomic controls						
Family income (log $)	0.15***	0.04***	-0.13***	-0.04***	-0.14***	-0.04***
	(0.03)	(0.01)	(0.04)	(0.01)	(0.05)	(0.01)
House value (log $)			-0.08***	-0.02***	-0.07***	-0.02***
			(0.01)	(0.00)	(0.01)	(0.00)
Population (log)			0.06	0.02	0.07	0.02
			(0.12)	(0.03)	(0.12)	(0.03)
Population per mi^2 (log)			0.01	0.00	0.00	0.00
			(0.12)	(0.03)	(0.12)	(0.03)
Households with own children			0.04	0.01	0.04	0.01
			(0.05)	(0.01)	(0.05)	(0.02)
Black			-0.18***	-0.05**	-0.19***	-0.05***
			(0.07)	(0.02)	(0.07)	(0.02)
Hispanic			0.09	0.03	0.08	0.02
			(0.07)	(0.02)	(0.07)	(0.02)
High school graduate (25+)			0.33***	0.09***	0.33***	0.09***
			(0.07)	(0.02)	(0.08)	(0.02)
College graduate (25+)			1.31***	0.37***	1.29***	0.36***
			(0.10)	(0.03)	(0.10)	(0.03)
Housing units owned			0.73***	0.21***	0.74***	0.21***
			(0.09)	(0.03)	(0.09)	(0.03)
Children in poverty			0.25***	0.07***	0.22**	0.06***
			(0.06)	(0.02)	(0.09)	(0.02)
Alternate hypotheses						
Within-neighborhood income inequality					0.07	0.02
					(0.12)	(0.03)
Black/white neighborhood dissimilarity					-0.03	-0.01
					(0.02)	(0.00)
Time averages	✓		✓		✓	
Year dummies	✓		✓		✓	
Observations	33433		33433		33433	
Scale factor	0.29		0.28		0.28	

Note: All regressions use the static tract panel of school districts, assume a probit fractional response model and are estimated by quasi-maximum likelihood, weighting by district time-average population. All socioeconomic controls, except family income, house value, population, and population density are measured in shares. All dollar values are adjusted for inflation. Standard errors for coefficients, given in parentheses, are clustered at the district-level. Standard errors for average partial effects are the result of 100 bootstrap replications.

Table 1.8: Negative Relationship Is Somewhat Consistent Across Panels and Weights

Dependent variable: District enrollment rate (16-19)

	Static		Concurrent		SABINS	
	Wgt.	Unwgt.	Wgt.	Unwgt.	Wgt.	Unwgt.
Principal variable of interest						
Across-neighborhood income inequality	-0.18***	-0.05	0.46	-0.47***	-0.28***	-0.36***
	(0.06)	(0.05)	(0.81)	(0.17)	(0.10)	(0.11)
Alternate hypotheses						
Within-neighborhood income inequality	0.02	0.08***	0.51*	0.13***	-0.06	0.03
	(0.03)	(0.02)	(0.30)	(0.04)	(0.10)	(0.05)
Black/white neighborhood dissimilarity	-0.01*	-0.00	0.01	-0.01*	-0.00	-0.01
	(0.00)	(0.00)	(0.03)	(0.01)	(0.02)	(0.01)
Socioeconomic controls	✓	✓	✓	✓	✓	✓
Time averages	✓	✓	✓	✓	✓	✓
Year dummies	✓	✓	✓	✓	✓	✓
Observations	33433	33433	34103	34103	3061	3061
Scale factor	0.281	0.276	0.329	0.313	0.291	0.283

Note: All regressions assume a probit fractional response model and are estimated by quasi-maximum likelihood. Only average partial effects are reported. Standard errors, given in parentheses, are the result of 100 bootstrap replications.

Table 1.9: Instrument Is a Strong Predictor of Changes in Across-Neighborhood Income Inequality

Dependent variable: Across-neighborhood income inequality

	(1)	(2)
Instrument for...		
Across-neighborhood income inequality	0.79***	0.59***
	(0.06)	(0.06)
Socioeconomic controls	✓	✓
Alternative hypotheses	✓	✓
Baseline period control		✓
Time averages	✓	✓
Year dummies	✓	✓
Observations	28550	28550
Adjusted R^2	0.69	0.75
Kleibergen-Paap F	248.81	80.61

Note: All regressions use the static tract panel of neighborhoods, are estimated by OLS, weighting by district time-average population. Standard errors, given in parentheses, are clustered at the district-level.

Table 1.10: Across-Neighborhood Income Inequality Has a Negative Causal Effect on Enrollment Rates

Dependent variable: District enrollment rate (16-19)

	(1) Quasi-MLE	(2) Two-Step IV	(3) Two-Step IV
Principal variable of interest			
Across-neighborhood income inequality	-0.19***	-0.89***	-2.64***
	(0.06)	(0.14)	(0.48)
Alternate hypotheses			
Within-neighborhood income inequality	-0.00	0.06***	0.07***
	(0.04)	(0.02)	(0.02)
Black/white neighborhood dissimilarity	-0.01*	-0.00	0.00
	(0.01)	(0.00)	(0.00)
Socioeconomic controls	✓	✓	✓
Baseline period control			✓
Time averages	✓	✓	✓
Year dummies	✓	✓	✓
Observations	28550	28550	28550
Kleibergen-Paap F		248.81	80.61

Note: All regressions use the static tract panel of neighborhoods, assume a probit fractional response model and are weighted by district time-average population. Only average partial effects are reported. Standard errors, given in parentheses, are the result of 100 bootstrap replications.

Table 1.11: Across-Neighborhood Income Inequality Has a Causal Effect on Enrollment Rates that Varies Directly with Neighborhood Income

Dependent variable: Neighborhood enrollment rate (16-19)

Main variables of interest	
Across-neighborhood income inequality	-213.51***
Across-neighborhood × family income (neighborhood)	18.55***
Alternate hypotheses	
Within-neighborhood income inequality	-0.28***
Black/white neighborhood dissimilarity	0.07***
First-stage residuals	
Across-neighborhood income inequality	220.64***
Across-neighborhood × family income (neighborhood)	-19.21***
Socioeconomic controls (neighborhood)	✓
Socioeconomic controls (district)	✓
Baseline period control	✓
Time averages	✓
Year dummies	✓
Observations	220326
Scale factor	0.283
Kleibergen-Paap F	28.71

Note: All regressions use the static tract panel of neighborhoods, assume a probit fractional response model, and are estimated by two-step instrumental variables. Only average partial effects are reported. Standard errors, given in parentheses, are the result of 100 bootstrap replications.

Chapter 2

The Median Neighborhood Model

2.1 Defining the Model

This section describes a static model of household neighborhood choice, voting and public education within a school district, designed to elucidate the key intuition concerning how rising income inequality across neighborhoods within a school district affects local public finance and education. It builds on the lengthy theoretical literature on residential location choice and community public good provision dating back to Tiebout (1956), with Nechyba (1997) providing a more contemporary foundation.[1] The model identifies two key factors in determining how neighborhood income differences affect local schools: the average income of the median-voting household's neighborhood, and whether neighborhood income and tax revenue are substitutes or complements in education production.

2.1.1 Framework

Consider a municipal school district with a population of households and a fixed supply of identical housing, both of measure one. The district is divided geographically into n equally-sized neighborhoods. Each neighborhood has its own elementary school, middle school and high school, so that school-age children attend the appropriate public school in their neighborhood.[2]

The school district funds its operations via a proportional property tax (t), chosen by

[1] de Bartolomé (1990), Fernandez and Rogerson (1996), and Calabrese et al. (2006) also influenced the treatment of neighborhood peer effects.

[2] Private, charter and magnet schools do not feature in this model. For more on private schools, see Epple and Romano (1996, 1998), and on school choice, see Nechyba (1999), Ferreyra (2007), and Avery and Pathak (2015).

a direct referendum of the households.[3] Consistent with the median voter hypothesis, the median household's preference is assumed to win the referendum. If home prices in each neighborhood are p_n, district tax revenue is:

$$r = \sum_n \frac{1}{n} \cdot t \cdot p_n \tag{2.1}$$

$$= t \cdot \overline{p} \tag{2.2}$$

The school district is assumed to run a balanced budget, so total expenditures on education are equivalent to total revenues.

Unitary households, indexed by m, are distinguished by their income (i_m) and the strength of their preference for school quality (θ_m). Household incomes are drawn from a continuous distribution with support $(0, \infty)$, while preference parameters are drawn independently from a continuous distribution with support $(0, 1)$.[4] Households inelastically demand one unit of housing, and enjoy utility from consumption of a private good (c) and the quality of the public education in their neighborhood (e_n). They derive no utility from housing per se – only through access to the neighborhood's schools. The home price is thus simply a neighborhood entry cost.[5] Assuming strictly quasi-concave preferences and normalizing the price of the private consumption good to one, households choose a private

[3]State and federal funding for K-12 education do not appear explicitly in the model.

[4]The independence assumption is strong, but conservative. The model is able to generate an increase in neighborhood income differences through exogenous changes in household income alone. Permitting preferences to change – or be correlated – with changes in income would only intensify the model's predictions.

[5]For simplicity, the proceeds from home sales are assumed to be collected by an absent developer – though to close the model without affecting its implications, the district government could just as well collect all proceeds and make equal lump-sum transfers to all households as in de Bartolomé (1990).

consumption level and a neighborhood to maximize their utility subject to their budget constraint:

$$\max_{c,n} u(c, e_n; \theta_m) \tag{2.3}$$

$$\text{s.t. } c + (1+t)p_n = i_m$$

taking neighborhood school qualities, neighborhood home prices and the district property tax rate as given. Households then vote for the district tax rate that maximizes their household utility.[6] As there are many households, each household votes truthfully and takes equilibrium outcomes (i.e. home prices, neighborhood income) as given, consistent with the myopic voter model of Calabrese et al. (2006).[7]

The quality of education delivered in each neighborhood is a function of district-wide revenue and the neighborhood's average income, $\overline{i_n}$.[8,9] Assuming a district-specific production

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[6]Households are not permitted to change districts. While this is a strong modeling assumption, empirically most family moves are local. Hanushek, Kain, and Rivkin (2004) found that, of students aged 9-14 in the NLSY79 who moved between 1994-96, only 30 percent moved across school districts (usually within the same metropolitan area). Moreover, Epple and Romer (1991) shows that while the threat of high-income households' out-migration constrains local tax policy somewhat, significant local redistribution is still common.

[7]For more on the political economy of school budget referenda specifically, see Romer, Rosenthal, and Munley (1992).

[8]While there remains some discussion as to how much, and in what contexts, "money matters" in education (Hanushek, 1986; Hedges, Laine, and Greenwald, 1994; Krueger, 2002; Jackson, Johnson, and Persico, 2014), few would argue it never matters.

[9]Neighborhood strength of preference for school quality does not enter into education production directly; without relaxing the assumption of independence between household income and preference, it enters only through its possible correlation in equilibrium with neighborhood income. As with the independence assumption, this zero-restriction serves only to highlight the potential role for neighborhood income; relaxing it does not materially change the model's predictions.

function non-decreasing in all inputs, neighborhood school quality is:

$$\begin{aligned} e_n &= f\left(r, \overline{i_n}\right) \\ &= f\left(t \cdot \overline{p}, \overline{i_n}\right) \end{aligned} \tag{2.4}$$

Neighborhood income may affect education production directly or indirectly. Directly, children of high-income households may easier to educate because they are less likely to come from families in poverty, to have limited English proficiency, or to live with a single parent (Duncombe and Yinger, 2008). Indirectly, for instance, these children may attract better qualified, more experienced classroom teachers, who prefer working in high-socioeconomic status environments (Koski and Hahnel, 2008; Clotfelter, Ladd, and Vigdor, 2011). By including neighborhood-level mean household income, the production function accommodates both "neighborhood effects" pathways.[10]

A spatial equilibrium in the model is characterized by:

a. Household neighborhood allocations, $\{n\}$

b. Neighborhood home prices, $\{p_n\}$

c. District property tax, t

such that:

1. No household would prefer to live in a different neighborhood
2. The housing market in each neighborhood clears, and
3. The median voter's preference is the property tax.

[10]Nechyba (1999), Calabrese et al. (2006) and Ferreyra (2007) also use mean household income as a proxy for neighborhood-level peer effects in education.

Nechyba (1997) provides a proof of existence and uniqueness (up to the ordering of neighborhoods) of an equilibrium of this type, with neighborhood home prices increasing with neighborhood income. This paper focuses on comparative statics: how does the equilibrium change as household income inequality grows?

2.1.2 Neighborhoods in equilibrium

First, consider the allocation of households to neighborhoods. In equilibrium, neighborhoods are distinguished only by the quality of their public schools and their home prices. In the degenerative case where no household income inequality is present, all neighborhoods have the same income regardless of the allocation of households. This pooling allocation – characterized by a symmetric distribution of household incomes, equal home prices, and identical school quality across neighborhoods – is illustrated in Panel a of Figure 2.1, where households are assigned to three neighborhoods (trivially) by their optimal level of education expenditures (x^*).

However, when household income inequality is present, household income and optimal education expenditures are correlated. As a result, higher income neighborhoods are not just desirable (because neighborhoods with higher income households have better schools) but potentially sustainable (in that many of the households living there are willing to pay more for that public benefit than other households). This separating allocation – characterized by an asymmetric distribution of household incomes, differences in home prices, and variation in school quality across neighborhoods – is illustrated in Panel b of Figure 2.1, for the special case of three possible income levels (i.e. low- (L), middle- (M) and high- (H) income households).

Note that as household income inequality grows in magnitude, differences across neighborhoods grow as well. For instance, consider a shift in the household income distribution such that income growth for high-income households outpaces that of other households. Because high-income households resided disproportionately in the high-income neighborhood before the shift, the differences in income, and thus school quality, across neighborhoods will be expected to increase even absent household resorting. This is the "partial equilibrium" effect of increasing household income inequality.

The shift in the income distribution will prompt some households to move as well. In particular, more high-income households will prefer the high-income neighborhood (both because the school quality has improved significantly in the high-income neighborhood, and because high-income households now have more income to spend on education). Consequently, the rent premium for the high-income neighborhood must increase for housing markets to clear. This contributes an additional compositional shift, as the share of high-income households in the high-income neighborhood grows (and correspondingly, falls in the other neighborhoods). This is the additional "general equilibrium" effect of increasing household income inequality. (The new allocation of households displaying a "rich cluster" is illustrated in Panel c of Figure 2.1.)

Together, these effects explain how increasing household income inequality leads to growing neighborhood differences. A full accounting of the effect of income inequality on equilibrium tax rates and school quality, however, requires a discussion of school district voting mechanics.

2.1.3 Voting in equilibrium

The household's first-order condition for a maximum does not permit a closed-form solution for t in general. Consider then the special case where household preferences are Cobb-Douglas and education production assumes a generalized CES functional function:

$$\begin{aligned} e_n &= \left(\alpha \cdot r^{\rho} + (1-\alpha)\overline{i_n}^{\rho}\right)^{\frac{\kappa}{\rho}} \\ &= \left(\alpha(t \cdot \overline{p})^{\rho} + (1-\alpha)\overline{i_n}^{\rho}\right)^{\frac{\kappa}{\rho}} \end{aligned} \tag{2.5}$$

where the shape parameters α and ρ determine the relative importance and substitutability, respectively, of tax revenue and neighborhood income in education production, and κ specifies the overall returns to scale. Then it may be shown using the implicit function theorem that a household's optimal tax rate is monotonic in household income, strength of preference for public education, and own-home price.[11] So all else equal, the median-voting household, denoted by ν, is:

1. The household with the median income
2. The household with the median strength of preference for school quality
3. The household in the median income neighborhood

How a household's optimal tax rate relates to own-neighborhood income, moreover, depends on the shape of the education production function. If tax revenues and neighborhood income are substitutes, then the optimal tax rate is decreasing in own-neighborhood income. If

[11] In particular, if revenue and neighborhood income are substitutes in education production, then the optimal tax rate is strictly increasing in income and strength of preference for school quality, and strictly decreasing in own-home price.

they are complements, then the converse holds; the optimal tax rate is increasing in own-neighborhood income.

For instance, suppose that the household income distribution shifts as depicted in Panel c of Figure 2.1, so that the income growth of high-income households outpaces that of other households. Whether the median-voting household is approximated as the household with the median income, the household with the median strength of preference for school quality, or the household in the median income neighborhood, the neighborhood income of the median-voting household has fallen. (Denoting the median-voting household's neighborhood by η, we have that $\Delta\overline{i_\eta} < 0$.) If, in addition, tax revenue and neighborhood income are complements in education production, the median-voting household will accommodate the lower neighborhood income by choosing a less generous tax rate. If tax revenues and neighborhood income are substitutes, on the other hand, the median-voting household will compensate for the loss of high-income households by "soaking the rich" and selecting a more burdensome tax rate.

On the other hand, suppose that the household income distribution shifts such that income growth for low-income households lags behind that of the other households. Now rather than high-income households becoming concentrated in the high-income neighborhood, the low-income households will be "selected out" and concentrated in a "poor cluster" as depicted in Panel d of Figure 2.1. As a result, the neighborhood income of the median-voting household, however approximated, has likely improved substantially (i.e. $\Delta\overline{i_\eta} > 0$). If tax revenues and neighborhood income are complements in education production as well, the median-voting household will greet the improvement in own-neighborhood income by choosing a higher tax rate. But if tax revenues and neighborhood income are substitutes, the median-voting household will substitute the gain in high-income households for tax revenues by selecting a lower tax rate.

2.1.4 Schools in equilibrium

The implications of increasing household income inequality for district-wide school quality are indeterminate. The predicted changes in neighborhood income and the adjustment by the median-voting household interact to produce neighborhood-specific effects on schools, which may be positive or negative in the aggregate.

The model does offer predictions for the distribution of these neighborhood-specific effects, however. Whether a "rich cluster" or a "poor cluster" forms, the neighborhood income of the low-income neighborhood will have declined relative to that of the high-income neighborhood. The difference in school quality between the two will likely grow as well, unless tax revenues rise (fall) and the education production function exhibits sufficient decreasing (increasing) returns to scale; in this case, the gap in school quality between the poor and rich neighborhoods may shrink.

The model's predictions are summarized below:

	Median Voter's Neighborhood Income...	
	Declines	Improves
Substitutes	Tax Rate ↑, School Quality Gap ↑	Tax Rate ↓, School Quality Gap ↑
Complements	Tax Rate ↓, School Quality Gap ↑	Tax Rate ↑, School Quality Gap ↑

Because the implications of the model for school quality are considerably more ambiguous than those for the tax rate (at least without imposing additional functional assumptions), the empirical tests that follow focus on the predictions for local tax policy.

2.2 Testing the Model

2.2.1 Estimating equation

According to the model, the equilibrium district property tax rate is determined by the median-voting household. The median voter selects the tax rate as a function of household (income, strength of preference), neighborhood (income, home price), and district (average home price, education production technology) factors:

$$t = f\left(i_\nu, \theta_\nu, \overline{i_\eta}, p_\eta, \overline{p}, \alpha, \rho, \kappa\right) \tag{2.6}$$

The identity of the median voter may be approximated as the household in the median-income neighborhood.[12] Demographic and economic characteristics (presence of school-age children, educational attainment, race, etc.) of the median voter's neighborhood proxy for the median voter's strength of preferences for school quality. And a (possibly overlapping) set of socioeconomic factors known to affect the production of education (population size, poverty income, ethnicity, etc.) proxy for the unknown functional parameters.

Multiplying both sides of (2.6) by $\overline{p}$ and taking the log of both sides yields, to a first-order

[12] Because separately approximating the income of the median voter (i_ν) and the average income of the median voter's neighborhood ($\overline{i_\eta}$) is impossible with this method, the former is dropped from the estimating equation.

approximation:

$$ln(r) = \beta_0^r + \beta_1^r \cdot ln(\overline{i_\eta}) + \beta_2^r \cdot ln(\bar{i}) + \beta_3^r \cdot ln(p_\eta) + \beta_4^r \cdot ln(\overline{p}) + \boldsymbol{\beta_5^r \cdot X_\eta} + \boldsymbol{\beta_6^r \cdot X} + \epsilon^r \quad (2.7)$$

where $\boldsymbol{X_\eta}$ and $\boldsymbol{X}$ are vectors of socioeconomic characteristics in the median voter's neighborhood and district-wide, respectively, and ϵ^r is an error term distributed approximately $N(0, \sigma^r)$ arising from possible misspecification of the proxies. Finally, adding time dummies and district fixed effects to take full advantage of the panel data, the district-level estimating equation for local tax revenues is:

$$\begin{aligned} ln(r) = \beta_0^r + \beta_1^r \cdot ln(\overline{i_\eta}) + \beta_2^r \cdot ln(\bar{i}) + \beta_3^r \cdot ln(p_\eta) + \beta_4^r \cdot ln(\overline{p}) + \boldsymbol{\beta_5^r \cdot X_\eta} + \boldsymbol{\beta_6^r \cdot X} \quad (2.8) \\ + \boldsymbol{\beta_7^r \cdot \mathbb{1}_T} + \boldsymbol{\beta_8^r \cdot \mathbb{1}_D} + \epsilon^r \end{aligned}$$

β_1^r is the key parameter of interest. A positive estimate is consistent with the implication of the model that the median-voting household complements increases in own-neighborhood income with higher property tax rates. On the other hand, a negative estimate is consistent with the implication that the median voter substitutes for declines in own-neighborhood income with higher property tax rates. An estimate indistinguishable from zero can be considered a rejection of the model.

2.2.2 Results

In total, the results are consistent with a model in which the median voter substitutes higher property tax revenue for declines in own-neighborhood status (and vice-versa). The central finding is established in Table 2.1, which presents ordinary least squares estimates of the fixed effects model for the static-tract panel of school districts. Across a range of specifications, the estimates suggest that a 10 percent increase in household income of the median voter's neighborhood (all else, particularly district-wide household income, held constant) is associated with an approximate 3.5 to 4.4 percent decline in local tax revenue per household. In fact, as column 3 shows, neighborhood income is one of the only characteristics of the median voter's neighborhood that appears to have a significant relationship to local tax policy. And after controlling for the income of the median voter's neighborhood, the component of household income inequality not explained by cross-neighborhood income differences is negatively related to tax revenues (as with the findings in section 1.5, opposite the prediction of the tax price hypothesis).

The main result is broadly robust to estimation using other panels and to dropping population weights. The most exacting specification from Table 2.1 is replicated in Table 2.2 for every combination of panel and regression weighting approach. These estimates suggest that a 10 percent increase in mean household income of the median income neighborhood (all else equal) is associated with an approximate 1.6 to 8.6 percent decline in local tax revenue per household; the relationship seems to be stronger in more populous school districts, but there is little disagreement across specifications as to its sign or statistical significance.[13]

[13]Attendance areas for middle and high schools in the U.S. often encompass those of several elementary

Within-neighborhood income inequality shows a consistent negative relationship to local school funding, while the degree of black/white segregation is regularly (though not always significantly) negatively related.

The specifications presented so far assume that the relationship between the median voter's neighborhood income and local tax revenue is symmetric across neighborhood paradigms – that, for instance, the intensification of a "rich cluster" should induce the same policy response as the softening of a "poor cluster," since both imply declines in the median voter's neighborhood income. To test this directly, sub-samples were constructed for each neighborhood paradigm; school districts are assigned to the "rich cluster" ("poor cluster") sub-sample if the median voter's neighborhood income consistently lags (outpaces) district-wide average household income throughout the period of obseration. Summary statistics of these sub-samples are given in Table 2.3. While school districts exhibiting the "rich cluster" paradigm are nearly twice as common, and on average four times more populous, than those exhibiting a "poor cluster," the sub-samples are otherwise remarkably similar.[14]

Parameter estimates produced using the neighborhood paradigm sub-samples are consistent with the assumption of a symmetric response, though only the "rich cluster" sub-sample estimate is statistically significant alone. Table 2.4 suggests that a 10 percent increase in household income of the median voter's neighborhood (all else equal) is associated with an approximate 3.8 percent decline if the "poor cluster" paradigm is present and a 7.1 percent

schools. Consequently, smaller, less populous school districts are less likely to have multiple middle or high schools, mitigating the importance of neighborhood income differences in education production.

[14]The empirical preponderance of the "rich cluster" neighborhood paradigm is consistent with the model, which predicts the increasing isolation of high-income households as their income growth outpaces that of low- and middle-income households. Indeed, this has been the dominant national trend since the 1970s, so it stands to reason that it would drive neighborhood dynamics among the majority of school districts as well.

decline if the "rich cluster" paradigm is present. And while the former estimate is not statistically significant, the null hypothesis that the two estimates are different cannot be rejected at any conventional significant level (the t-statistic is 0.75).

Finally, a key implication of the model is that there should be no monotonic relationship between local tax revenue and income inequality across neighborhoods *per se*; while the intensification of a "poor cluster" or a "rich cluster" would both be associated with increases in across-neighborhood income inequality, the model has divergent predictions for tax policy. The consistently positive relationship between neighborhood income inequality and local tax revenue revealed in section 1.5 then, must be a consequence of the dominance of the "rich cluster" paradigm in the data.

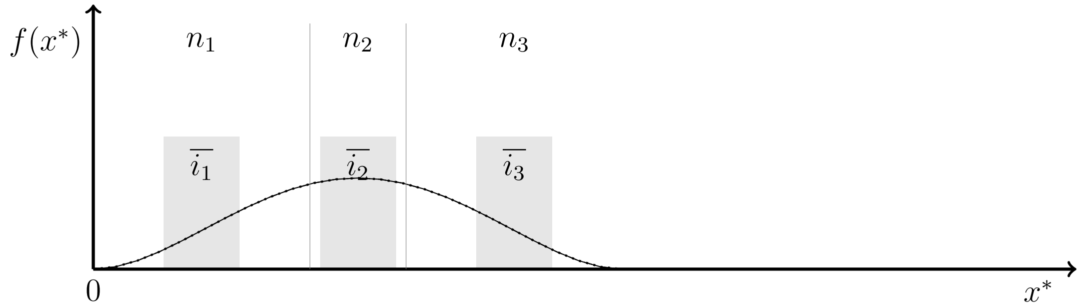
This hypothesis is confirmed by reevaluating the relationship between local tax revenue and across-neighborhood income inequality , now using the neighborhood paradigm subsamples. Table 2.5 shows that, consistent with the model's predictions, an increase in income inequality across neighborhoods has a different relationship under different paradigms; a one-standard deviation intensification of a "poor cluster" ("rich cluster"), accompanied by a rise (fall) in the median voter's neighborhood income, is associated with an approximate 3.5 percent fall (5.8 percent rise) in local tax revenue. Both estimates are statistically significant, and we cannot reject the null hypothesis that the estimates are of equal magnitudes but opposite signs (t-statistic is 0.97). So the causal estimates presented in section 1.5 may be interpreted as the effects of rising across-neighborhood income inequality on balance, with the dynamics of the "rich cluster" neighborhood paradigm prevailing.

2.3 Conclusion

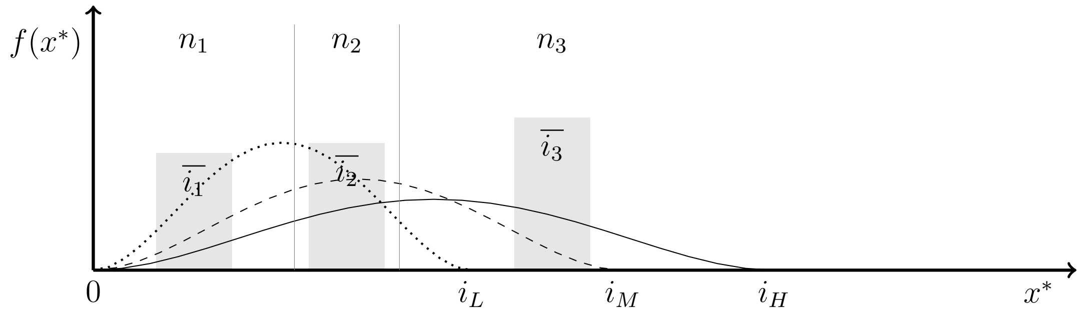
This paper shows how rising income inequality across neighborhoods within school districts has had a profound effect on local public finance and education in the U.S. Because the "rich cluster" neighborhood paradigm is dominant, rising across-neighborhood income inequality in the U.S. caused school districts' real local tax revenue per household to increase 11.2 percent, explaining about 23 percent of the secular growth in local funding for public education over this period. Nonetheless, the rise also depressed enrollment rates among those 16-19 years old in any school, public or private, by 6.3 percentage points overall, and worked to widen the gap in enrollment between low- and high-income neighborhoods by 1.8 percentage points. The results are robust to various estimation samples and specifications, and to instrumenting for changes in across-neighborhood income inequality using a shift-share, Bartik-style approach. And they are consistent with the predictions of the median neighborhood model, wherein the median voter substitutes increases in local tax rates for declines in own-neighborhood income – rather than for declines in the tax price of government expenditures.

Altogether, the results suggest that rising income inequality across neighborhoods increases local school funding, but also fosters a less productive allocation of students to neighborhoods which blunts the marginal benefit of additional district expenditures. These findings help account for the persistent gap in educational attainment between children of low- and high-socioeconomic status families, as documented by Reardon (2011), despite steadily rising per-pupil expenditures. They provide an explanation for the puzzling result presented in Jackson, Johnson, and Persico (2014), that endogenous increases in local school funding tend to be less productive than state court-ordered exogenous ones. More broadly, they illuminate causal linkages between three correlates of low mobility areas identified by Chetty et al. (2014): household income inequality, income segregation, and poor school quality.

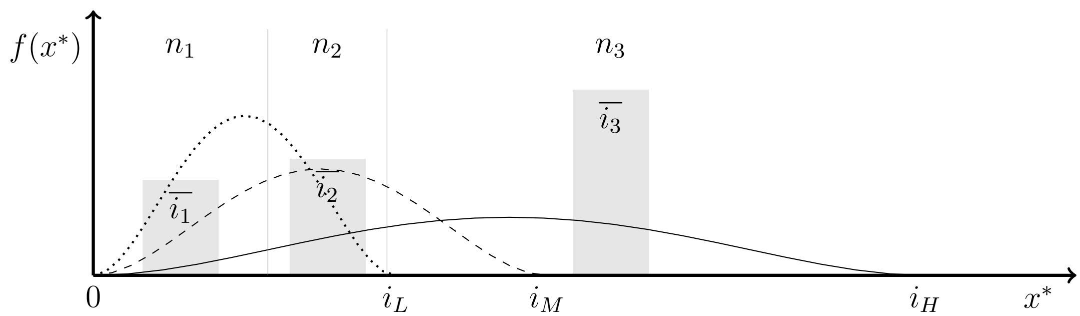
Figure 2.1: Household Income Inequality and Neighborhood Income



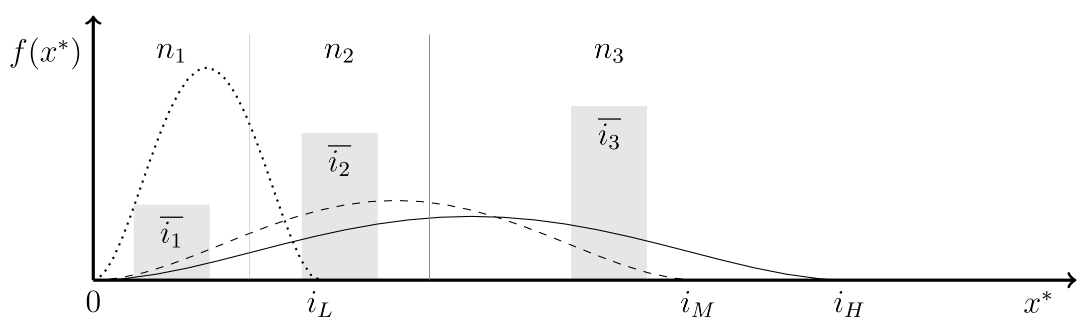
Panel a: Equal household incomes beget identical neighborhoods.



Panel b: Household income inequality begets neighborhood income differences.



Panel c: Rising incomes for high-income households create a “rich cluster”.



Panel d: Stagnating incomes for low-income households create a “poor cluster”.

Table 2.1: Median Voter Substitutes Higher Taxes for Loss of Neighborhood Income

Dependent variable: Local tax revenue per household (log $)

	(1)	(2)	(3)
Principal variables of interest			
Family income, median voter's neighborhood (log $)	-0.44***	-0.37***	-0.35***
	(0.14)	(0.13)	(0.13)
Family income, district (log $)	1.47***	1.34***	1.46***
	(0.19)	(0.25)	(0.22)
House value, median voter's neighborhood (log $)	-0.00	-0.00	-0.01
	(0.01)	(0.01)	(0.01)
House value, district (log $)	-0.11	-0.18	-0.22**
	(0.12)	(0.13)	(0.10)
Socioeconomic controls (median voter's neighborhood)			
Population (log)			-0.00
			(0.01)
Population per mi^2 (log)			0.00
			(0.01)
Households with own children			0.07
			(0.11)
Black			0.17**
			(0.07)
Hispanic			-0.07
			(0.10)
High school graduate (25+)			0.11
			(0.18)
College graduate (25+)			-0.07
			(0.11)
Housing units owned			0.03
			(0.07)
Children in poverty			-0.21*
			(0.12)
Alternate hypotheses			
Within-neighborhood income inequality			-0.65***
			(0.20)
Black/white neighborhood dissimilarity			-0.06
			(0.04)
Socioeconomic controls (district)		✓	✓
District fixed effects	✓	✓	✓
Year dummies	✓	✓	✓
Observations	33433	33433	33433
Adjusted R^2	0.33	0.35	0.36

Note: All regressions use the static tract panel of neighborhoods and are estimated by OLS, weighted by district time-average population. Characteristics of the median voter's neighborhood approximated by those of the median-income neighborhood. All socioeconomic controls, except family income, house value, population, and population density are measured in shares. All dollar values are adjusted for inflation. Standard errors for coefficients, given in parentheses, are clustered at the district-level.

Table 2.2: Median Voter's Response Is Consistent Across Panels and Weights

Dependent variable: Local tax revenue per household (log $)

	Static		Concurrent		SABINS	
	Wgt.	Unwgt.	Wgt.	Unwgt.	Wgt.	Unwgt.
Principal variable of interest						
Family income, median voter's neighborhood (log $)	-0.35***	-0.19***	-0.34**	-0.16***	-0.86***	-0.30
	(0.13)	(0.06)	(0.14)	(0.06)	(0.27)	(0.19)
Alternate hypotheses						
Within-neighborhood income inequality	-0.65***	-0.23**	-0.65***	-0.23**	-0.33	-0.55
	(0.20)	(0.10)	(0.22)	(0.09)	(0.50)	(0.41)
Black/white neighborhood dissimilarity	-0.06	-0.01	-0.02	-0.01	-0.09	-0.08
	(0.04)	(0.01)	(0.04)	(0.01)	(0.13)	(0.09)
Socioeconomic controls (district)	✓	✓	✓	✓	✓	✓
Socioeconomic controls (median voter's neighborhood)	✓	✓	✓	✓	✓	✓
District fixed effects	✓	✓	✓	✓	✓	✓
Year dummies	✓	✓	✓	✓	✓	✓
Observations	33433	33433	34103	34103	3061	3061
Adjusted R^2	0.36	0.20	0.38	0.20	0.49	0.30

Note: All regressions are estimated by OLS. Characteristics of the median voter's neighborhood approximated by those of the median-income neighborhood. Standard errors for coefficients, given in parentheses, are clustered at the district-level.

Table 2.3: Summary Statistics for Static-Tract Panel,
Neighborhood Paradigm Sub-Samples

	All	Poor	Rich
Outcomes of interest			
Local tax revenue per household ($)	2922	3177	2836
Enrollment rate (16-19)	0.80	0.81	0.80
Principal variables of interest			
Across-neighborhood income inequality	0.011	0.007	0.015
Family income, median voter's neighborhood (log $)	71963	76696	66807
Socioeconomic controls (district)			
Family income (log $)	72643	73472	70620
House value ($)	97547	100460	92167
Population	30313	12321	50008
Population per mi^2	766	424	879
Households with own children	0.39	0.38	0.39
Black	0.06	0.05	0.06
Hispanic	0.06	0.05	0.07
High school graduate (25+)	0.77	0.79	0.77
College graduate (25+)	0.18	0.18	0.17
Housing units owned	0.74	0.76	0.73
Children in poverty	0.15	0.14	0.16
Alternate hypotheses			
Within-neighborhood income inequality	0.25	0.25	0.25
Black/white neighborhood dissimilarity	0.33	0.31	0.34
Districts	9425	1384	2392
Observations per district	3.58	3.32	3.49
Tracts per observation	13.46	8.15	18.88

Note: Means are reported. Characteristics of the median voter's neighborhood approximated by those of the median-income neighborhood. All socioeconomic controls, except family income, house value, population, and population density are measured in shares. All dollar values are adjusted for inflation.

Table 2.4: Median Voter's Response Is Consistent in Sub-Samples

Dependent variable: Local tax revenue per household (log $)

	All	Poor	Rich
Principal variable of interest			
Family income, median voter's neighborhood (log $)	-0.35***	-0.38	-0.71**
	(0.13)	(0.31)	(0.31)
Alternate hypotheses			
Within-neighborhood income inequality	-0.65***	-0.39	-0.25
	(0.20)	(0.31)	(0.43)
Black/white neighborhood dissimilarity	-0.06	-0.01	-0.11
	(0.04)	(0.06)	(0.08)
Socioeconomic controls (district)	✓	✓	✓
Socioeconomic controls (median voter's neighborhood)	✓	✓	✓
District fixed effects	✓	✓	✓
Year dummies	✓	✓	✓
Observations	33433	4531	8240
Adjusted R^2	0.36	0.31	0.45
Different coefficients? (t-statistic)		0.75	

Note: All regressions use the static tract panel of neighborhoods and are estimated by OLS, weighted by district time-average population. Characteristics of the median voter's neighborhood approximated by those of the median-income neighborhood. Standard errors for coefficients, given in parentheses, are clustered at the district-level.

Table 2.5: No Evidence of Monotonic Relationship Between Across-Neighborhood Income Inequality and Local School Funding

Dependent variable: Local tax revenue per household (log $)

	All	Poor	Rich
Principal variable of interest			
Across-neighborhood income inequality	3.16***	-2.16*	3.63***
	(0.62)	(1.29)	(0.79)
Alternate hypotheses			
Within-neighborhood income inequality	-0.62***	-0.35	-0.33
	(0.20)	(0.31)	(0.40)
Black/white neighborhood dissimilarity	-0.07*	-0.02	-0.12
	(0.04)	(0.06)	(0.08)
Socioeconomic controls (district)	✓	✓	✓
District fixed effects	✓	✓	✓
Year dummies	✓	✓	✓
Observations	33433	4531	8240
Adjusted R^2	0.36	0.30	0.44
Different coefficients? (t-statistic)		3.84	
Different magnitude? (t-statistic)		0.97	

Note: All regressions use the static tract panel of neighborhoods and are estimated by OLS, weighted by district time-average population. Standard errors for coefficients, given in parentheses, are clustered at the district-level.

Chapter 3

The Countercyclical Dynamics of Public School Quality Valuation

3.1 Introduction

The perceived value of school quality improvements is central to many educational decisions. Parents weigh it when deciding where to send their children to school, and often in choosing where to live as well. Educators use it to make pedagogical choices, and policymakers consider it when formulating budgets and debating possible educational reforms. The extant literature accepts that this value may vary along various dimensions (e.g. race, sex or age), but the possibility of variation over the business cycle has received scant attention (Altonji and Dunn, 1996; Arias and McMahon, 2001; Johnson, 2013b). Yet because these decisions have broad ramifications for trends in human capital investment, education production and, ultimately, economic growth, any aggregate pattern in school quality valuations is worth understanding.

One possible explanation for this gap in the literature is the difficulty in studying it. While techniques for examining education production functions and estimating hedonic pricing models are fairly well established, approaching this particular question requires both unparalleled data and a unique setting (Card and Krueger, 1992; Hanushek and Raymond, 2002; Ries and Somerville, 2010). But the rise of the data-driven school accountability movement, the now broad availability of transaction-level data on home sales and, most fortuitously, the recent volatility provided by the "Great Recession" provide an ideal research opportunity.

In this paper, we use the experience of Los Angeles County over the period 2000 to 2013 to examine variation over the business cycle in homeowners' valuations of public school quality.[1] Because typical hedonic pricing approaches are likely to overstate the implied value

[1] The course of the Los Angeles regional economy was a simulacrum of the nation's during this period

of observable local amenities, we follow the lead of Black (1999) and exploit elementary school attendance area boundaries to provide identifying variation in public school quality. Together with comprehensive transaction-level data on sales of homes and annual school-level data on student achievement, this dynamic spatial regression discontinuity approach permits us to study how homeowners' valuations vary with broader economic conditions year-to-year.

Broadly we find that homeowners' valuations are countercyclical – they value improvements in public elementary school quality more during "busts" than in "booms." For instance, between the peak of the local housing market in 2007 and its nadir in 2009 we find that the implied valuation of a five-percent improvement in school quality increased from a level indistinguishable from zero to approximately 1.8 percent of a home's sales price. Our results are robust to the choice of bandwidth, the inclusion of various structural, educational, socioeconomic and geographic control variables, as well as to various specification checks – and they do not appear to be driven by selection of homes across years. Rather they are consistent with two explanations: first, that homeowners "trade down" from private schools during economic contractions; and second, that the option value of public school quality increases in periods of housing market tightness. These conclusions thus link our findings to a growing literature on the microeconomic ramifications of macroeconomic crises (Mian and Sufi, 2011; Benmelech, Meisenzahl, and Ramcharan, 2014; Jaimovich, Rebelo, and Wong, 2015).

The remainder of the paper is organized as follows. Section 3.2 details the econometric model, the dynamic spatial regression discontinuity approach. Section 3.3 describes the data

(Figure 3.1), but the local housing market faced disproportionately pronounced swings from "boom" to "bust" (Figure 3.2).

sources, sample selection method, and estimation samples. Section 3.4 discusses the main results, specification checks and potential interpretations. The final section concludes.

3.2 Econometric Model

The typical hedonic pricing model explains the sales price of a home as a function of its characteristics, both structural and geospatial (notably, for our purposes, the quality of the local public elementary school). Under these assumptions, the price of home m in location n can be modeled as:

$$\ln(p_{m,n}) = \beta_0 + \beta_q \cdot q_n + \boldsymbol{\beta_s} \cdot \boldsymbol{s_m} + \boldsymbol{\beta_g} \cdot \boldsymbol{g_n} + \epsilon_{m,n} \tag{3.1}$$

where $p_{m,n}$ is the sales price, q_n is a measure of local public school quality, $\boldsymbol{s_m}$ is a vector of structural characteristics and $\boldsymbol{g_n}$ is a vector of other geospatial characteristics. In this formulation, the primary coefficient of interest, β_q, represents the approximate marginal percent change in the sales price of a home due to a unit change in local public school quality.[2]

But unbiased estimation of this coefficient relies on the assumption that the included observables completely explain the observed variation in home prices. If repeated sales of homes (or, failing that, repeated sales in each neighborhood) are observed however, identification

[2]It should be stressed that any given household need not have children attending the local elementary school for the housing market to assign a value to its quality. Even if the household has no school-age children (as 81.0% of Los Angeles County households did in 2010), or if they do, but they attend a private, charter or magnet school (as 10.8%, 4.8% and 3.6%, respectively, of Los Angeles County students in Kindergarten through 12th grade did in 2009-10), the household pays the market price for the home, which accounts for the preferences of all potential owners.

may be improved by including house (or neighborhood) fixed effects to non-parametrically control for unobserved but time-invariant variation in factors affecting the sales price. In this neighborhood fixed effects model:

$$\ln(p_{m,n,t}) = \beta_0 + \beta_q \cdot q_{n,t} + \boldsymbol{\beta_s} \cdot \boldsymbol{s_{m,t}} + \boldsymbol{\beta_g} \cdot \boldsymbol{g_{n,t}} + \boldsymbol{\beta_n} \cdot \mathbb{1}_{\boldsymbol{n}} + \boldsymbol{\beta_t} \cdot \mathbb{1}_{\boldsymbol{t}} + \epsilon_{m,n,t} \tag{3.2}$$

$\mathbb{1}_{\boldsymbol{n}}$ is a vector of neighborhood fixed effects, and $\mathbb{1}_{\boldsymbol{t}}$ is a vector of time dummies accounting for a secular time trend in home prices.[3] If variation in $q_{n,t}$ across time is uncorrelated with changes in unobserved factors in neighborhood n which also affect sales prices, β_q can be identified through variation in $q_{n,t}$ across time within neighborhoods.

Alternately, if there are sharp geographic breaks in local public school quality (e.g. boundaries of elementary school attendance areas), one can improve identification by including fixed effects representing the nearest discontinuity for each home and restricting the estimation sample to the sales of homes nearest these discontinuities (i.e. choosing a tight "bandwidth"). In this spatial regression discontinuity model:

$$\ln(p_{m,n}) = \beta_0 + \beta_q \cdot q_n + \boldsymbol{\beta_s} \cdot \boldsymbol{s_m} + \boldsymbol{\beta_g} \cdot \boldsymbol{g_n} + \boldsymbol{\beta_b} \cdot \mathbb{1}_{\boldsymbol{b}} + \epsilon_{m,n} \tag{3.3}$$

$\mathbb{1}_{\boldsymbol{b}}$ is a vector of nearest boundary fixed effects. If variation in q_n across boundaries is not correlated with changes in unobserved factors across boundaries which also affect sales prices, β_q can be identified through variation in q_n across boundaries within nearby bandwidth

[3] If the area under study is large and geographically diverse, a separate time trend for each subregion may be more appropriate. When estimating these models in Section 3.4, we include time dummies for each of the 20 county subdivisions of Los Angeles County defined by the U.S. Census Bureau in 2010.

areas.[4]

If both repeated sales and spatial discontinuities are present however, we can combine the two approaches. This permits us to see how the market valuation of school quality varies over time – that is, to estimate a different β_q for each time t. In this dynamic spatial regression discontinuity model:

$$\ln(p_{m,n,t}) = \beta_0 + \boldsymbol{\beta_{q,t}} \cdot \boldsymbol{q_{n,t}} \cdot \mathbb{1}_{\boldsymbol{t}} + \boldsymbol{\beta_s} \cdot \boldsymbol{s_{m,t}} + \boldsymbol{\beta_g} \cdot \boldsymbol{g_{n,t}} + \boldsymbol{\beta_{b,t}} \cdot \mathbb{1}_{\boldsymbol{b,t}} + \epsilon_{m,n,t} \tag{3.4}$$

$\boldsymbol{\beta_{q,t}}$ is a vector of coefficients for public school quality (one for each time t) and $\mathbb{1}_{\boldsymbol{b,t}}$ is a vector of nearest boundary-time period fixed effects (i.e. a different non-parametric time trend for each bandwidth area). So long as variation in $q_{n,t}$ across boundaries at time t is not correlated with changes in unobserved factors across the boundaries within nearby bandwidth areas at time t which also affect sales prices, each element of $\boldsymbol{\beta_{q,t}}$ can be identified through variation in $q_{n,t}$ across boundaries within nearby bandwidth areas at time t.

Because the dynamic spatial regression discontinuity model is our preferred approach in the empirical analysis that follows, the potential threats to identification therein deserve special attention. Specifically, what factors affecting a home's sales price would be both unobserved by the econometrician and likely to vary across an elementary school boundary within a given time period? Though we account for a host of structural characteristics in our analysis, any particular set of control variables is necessarily incomplete and so it remains possible that we may attribute the positive effect of systematic, but unobserved, differences

[4] All else equal, this suggests that choosing a tighter bandwidth will provide more plausibly valid identification.

in home quality to observed differences in school quality.[5]

We may also worry that – despite anecdotal evidence that elementary school attendance area boundaries are drawn purely to apportion the school-age population to schools within school districts in such a way as to best use available resources and minimize overcrowding – the boundaries may coincide with other major breaks (e.g. a school district border, railroad tracks or an elevated highway).[6] In an effort to guard against this concern, we often exclude home sales whose nearest elementary school boundary coincides with a district boundary and always control for neighborhood characteristics using census data on socioeconomic characteristics at the block group-level.[7]

3.3 Data

Our sample draws on sales of single-family residences in Los Angeles County during the period January 2000 to December 2013. Data on residential sales and structural characteristics were provided by DataQuick (for 2000 to 2010) and CoreLogic (for 2011 to 2013).[8] Records are excluded from the sample if the sale is not an arms-length transaction, if the sales price is not reported, or if the transaction cannot be matched to the assessor's structural report.

[5] In a recent examination of the hedonic approach, Billings (2015) evaluates the magnitude of this issue by additionally controlling for residential building permits, which proxy for unobserved home renovations. The author concludes that the implied bias of unobserved structural differences in these studies is a "second-order concern."

[6] In explaining a series of attendance area boundary changes for the 2015-16 school year, the Los Angeles Unified School District cited a need to "better balance enrollments among several neighboring schools" (Los Angeles Unified School District, 2016).

[7] Our identification is on better footing the more variation we observe in these neighborhood controls near elementary school area boundaries. In our .2 mile bandwidth estimation sample, the median (mean) bandwidth area includes home sales in 4 (4.49) distinct census block groups.

[8] CoreLogic purchased DataQuick in 2013. Structural data are accurate as of the most recent assessment.

Each transaction is geocoded and assigned to a public elementary school according to attendance area boundaries as of 2002 provided by Los Angeles County.[9] We focus on elementary schools because they tend to be smaller than middle or high schools, and so they provide relatively more variation to study. Our primary measure of elementary school quality, the Academic Performance Index (API), comes from the California Department of Education. Biannually between 1999 and 2013, each school was assigned an API on the basis of student achievement in annual statewide aptitude exams – a Base API at the beginning of the school year, and a Growth API at the end. The index ranges in value from 200 to 1000, but schools were encouraged to maintain scores of at least 800. Each transaction is assigned the Growth API reported for the school year ending in the year the transaction closed.

Because the state's formula for API changes somewhat over time, magnitudes of differences across schools are only strictly meaningful within a school year. This complicates our effort to interpret changes in API over time particularly because, as Figure 3.3 shows, there has been a pronounced compression in the distribution of API within Los Angeles County over the study period. Does a year-to-year increase in API imply a meaningful improvement in school quality or simply "grade inflation" due to changing evaluation criteria? While it's impossible to empirically disentangle these two interpretations, it's important to note that the latter interpretation implies that small improvements in API should be relatively more meaningful in more recent years. So if year-to-year improvements are principally spurious,

[9]While transaction year-specific attendance area boundaries would be preferable, geospatial data on elementary school attendance area boundaries are limited, particularly in electronic formats and particularly for years past. Fortunately anecdotal evidence suggests that the boundaries don't change often, and not very quickly when they do (most often in response to major events, such as a school closing) (Black, 1999). Moreover if the boundaries we use are imprecisely measured, that will only serve to attenuate our estimates toward zero.

then small gains in API should, all else equal, become increasingly valuable to homeowners.[10]

API is an attractive summary measure of a school's effectiveness, but school-to-school differences in API are likely correlated with other differences – in school conditions, funding, tax rates, and neighborhood character. The dynamic spatial regression discontinuity design controls non-parametrically for unobservable, time-varying local characteristics that do not change across the elementary school attendance area boundary, but many features of the educational environment other than school quality *per se* do. To address this concern, we regularly include the residence's property tax bill, the school's student-teacher ratio, neighborhood-level socio-economic characteristics and two distance measures – distance to employment centers and distance to the shoreline – as additional control variables.[11]

Summary statistics are presented in Table 3.1 for the whole sample and the restricted samples of homes located within .3, .2 and .1 miles of a boundary, respectively. The restricted samples exclude sales of homes whose nearest boundary coincides with a school district border, or whose nearest boundary does not feature transactions observed on both sides.[12] While the restricted samples are only a fraction of the size of the full sample, the

[10]We would arrive at the same conclusion, incidentally, even if improvements were meaningful but homeowners only cared about a school's rank, not its absolute level of quality. We explore this possibility further in Section 3.4.

[11]The property tax for each $1,000 of assess value for fiscal year 2015 is determined by geocoding each structure into tax rate areas provided by the Los Angeles County Auditor-Controller. The student-teacher ratio as of the 2009-10 school year is calculated using data from the California Department of Education. The neighborhood-level socioeconomic characteristics as of 2000 are measured by geocoding each structure into block groups provided by the U.S. Census Bureau. The distance to employment centers is calculated as the employment-weighted average distance to the 40 ZIP code areas with the highest average employment over the period 2000-2010 according to County Business Patterns data.

[12]We also exclude homes within .01 miles of a valid boundary to guard against possible geocoding errors. The unrestricted sample, which includes variation across district boundaries, includes the school district's per-pupil expenditure level as an additional educational environment control variable. The district's per-pupil expenditure as of the 2009-10 school year is calculated using data from the National Center for Education Statistics.

compositional differences between them are slight.[13] Moreover while the number of transactions varies over time (as shown in Figure 3.4), the pattern is broadly consistent across all four samples. The trend of typical sales prices is also similar across samples (seen in Figure 3.5), and generally congruent with the Case-Shiller Home Price Index for the broader Los Angeles metropolitan area.

3.4 Results

On average during our study period, homeowners value improvements in local public school quality. Table 3.2 presents regression results where the marginal effect of an improvement in elementary school quality on sales price is *not* allowed to vary by transaction year. The estimates in the first column reflect the typical hedonic model, while the second, third and fourth columns display estimates for the spatial regression discontinuity model for homes within .3, .2, and .1 miles of a boundary, respectively. (The fifth column shows estimates for the typical hedonic model using the restricted sample of homes within .1 miles of a boundary.)[14] Estimates for all five models report a significant positive marginal effect for elementary school quality on sales price, though the estimates are appreciably smaller for the three models that exploit the geospatial discontinuity around elementary school attendance area boundaries. This does not appear to be entirely a consequence of sample selection;

[13]Elementary school attendance areas tend to be smaller in more densely-populated parts of the county. Consequently the restricted samples tend to be more representative of those areas, evidenced here by a slight tendency toward lower-priced and smaller homes, lower quality schools, and poorer, more densely-populated, and more Hispanic, neighborhoods.

[14]Unless otherwise noted, coefficient estimate standard errors are adjusted for clustering at the elementary school-year level throughout.

the estimate for the fifth model, though statistically different from that of the first (at the five-percent level), is much closer in magnitude to that than the estimates of the other three. Rather, these findings signal the value of the spatial regression discontinuity approach – that failing to control non-parametrically for local characteristics inflates estimates of homeowners' valuation of school quality improvements.

Identification with this method relies on the assumption that homes on different sides of the elementary school attendance area boundary are sufficiently similar and, to the extent that they are not, that those differences are either observed or uncorrelated with differences in school quality. This assumption cannot be directly tested, but Table 3.3 gives some suggestive evidence – the results of tests of differences in means of observable characteristics across the boundary for the whole sample as well as for each of the restricted samples. In general, the differences in characteristics other than school quality become smaller and less significant as the bandwidth decreases. Unsurprisingly, homes on the side of the boundary with better school quality tend to have higher sales prices than those on the other side. But they also exhibit more bedrooms and bathrooms, larger living spaces, younger buildings, and higher student-teacher ratios on average, as well as somewhat richer, less dense, more Asian, less Hispanic, better educated, more owner-dominant neighborhood populations – though these differences tend to attenuate as the sample becomes more restricted. The significant socioeconomic differences highlight that elementary school quality differences do influence where households choose to live. But they also suggest that, even for the most restricted sample, it may not be safe to assume that houses on either side of the boundary have identical neighborhood characteristics. All regressions that follow include the full complement of neighborhood controls as a result.

Turning to our principal matter of interest, we find that while homeowners value improvements in school quality on average, their valuation varies over time. In fact, we find that the valuation is broadly counter-cyclical; it is higher around periods of economic contraction than it is around periods of expansion. Table 3.4 displays regression results where the marginal effect of an improvement in elementary school quality on sales price is permitted to vary by transaction year. The estimates in the first column reflect the typical hedonic model, while the second, third and fourth columns display estimates for the spatial regression discontinuity model for homes within .3, .2, and .1 miles of a boundary, respectively.[15] (Figure 3.6 illustrates, for each model, the implied change in sales price (for the mean-valued home) associated with a five-percent improvement in school quality (at the mean) for each transaction year.) While the trend implied by the typical hedonic approach varies more wildly than those for the other three, all four models suggest that the valuation was relatively high around the 2001 recession, declined during the mid-aughts expansion, rose rapidly during the "Great Recession", and plateaued during the current expansion. This trend is both economically and statistically significant. Using the estimates for the spatial regression discontinuity model with the .2 mile restricted sample, we find that the implied change in sales price (for the mean-valued home) associated with a five-percent improvement in school quality (at the mean) increased from a level indistinguishable from zero to a 1.8 percent increase between the peak of the local housing market in 2007 and its nadir in 2009. And as Table 3.5 shows using the same estimates, the coefficient estimates for the period 2006-2007

[15]Because these regressions include boundary-year fixed effects, the restricted samples now exclude sales of homes whose nearest boundary does not feature transactions observed on both sides in the same year.

are indeed largely statistically different from those of the period 2000-2002 and those of the period 2009-2013.

An immediate concern is that this apparent trend is driven by selection, not the business cycle. We've already noted that the number of transactions fell significantly between 2005 and 2008, before leveling off somewhat thereafter. But were the homes sold in any given year not representative of the entire sample? Table 3.6 exhibits the results of tests of differences in means of observable characteristics across transaction years for the whole sample. Beginning with Panel A, we observe a boom-bust-recovery trend for sales prices, as well as a steady increase in API, as expected. But among observable structural characteristics, there appears to be no systematic difference between homes sold during expansions and those sold during contractions. Only building age exhibits a discernible pattern, but the trend is independent of the business cycle; as the county's housing stock ages, the average home sold grows increasingly old. Looking now at Panels B and C, we find no evidence of any variation in student-teacher ratio, per-pupil expenditures or property tax across transaction years. We do observe some evidence that homes in lower socioeconomic status neighborhoods (i.e. poorer, more Hispanic, less educated, more family-dominant) sell disproportionately in 2005, 2006 and 2009, while homes in higher socioeconomic status neighborhoods change hands disproportionately in 2007 and 2013. While these findings speak to the influence of the subprime mortgage crisis on the local housing market, they don't line up with, and thus don't appear to explain, the counter-cyclical pattern we observe in homeowners' valuations of school quality.

3.4.1 Specification checks

To explore the robustness of our main result we perform a battery of specification checks. The results are presented in Table 3.6; each represents a deviation from our baseline specification (the dynamic spatial regression discontinuity approach using the .2 mile restricted sample presented in Table 3.4). The first two checks concern sample selection. One unique feature of Los Angeles County is the predominance of the Los Angeles Unified School District (LAUSD).[16] To ensure our results are not driven by the (possibly anomalous) experience of this large district, we try excluding all homes within LAUSD from the regression. As the estimates in the first column show, the broad pattern of our findings are maintained in the remaining 68 districts.

Another feature of primary education in Los Angeles County is the prevalence of alternatives to local public school, including both private schools and public choice options such as charter or magnet schools.[17] If our model is correct and the coefficient estimates are reflective of public school quality valuations, the market price for local public elementary school quality should be relatively higher in communities where these alternatives are less prominent. We test this proposition by excluding all homes within 45 school districts (including LAUSD) in which more than 10 percent of students in Kindergarten through 8th grade enroll in private schools, or more than 10 percent employ a public choice option, from the regression. Consistent with our conjecture, as the results in the second column reveal,

[16] The second-largest school district in the country, LAUSD enrolled 43.4 percent of all students in Kindergarten through 8th grade in Los Angeles County for the 2014-15 school year.

[17] In the 2009-10 school year for instance, 10.8%, 4.8% and 3.6%, of students in Kindergarten through 12th grade in Los Angeles County enrolled in private, charter and magnet schools, respectively.

almost all of our estimates grow larger under this restriction.[18]

The last three specification checks relate to measuring school quality and divining what's salient to homeowners.[19] As we discussed in Section 3.2, one concern with using API is that the values may not be directly comparable over time, and so a given improvement in values may imply different changes in quality in different years. A related concern is that, if primary education is partially a competitive positional good, households may care more about the rank of a school's quality than its level *per se*. To speak to both of these potential issues, we try using API decile rank (by year, among elementary schools in Los Angeles County) as our measure of elementary school quality. Looking at the third column of Table 3.6, we see that the pattern of our main findings is broadly replicated using this rank-order approach.

Alternately, there may be an issue concerning non-linearities in the estimated marginal effect of API – specifically because schools can "fail" (or, in the language of the California Department of Education, be deemed in need of "program improvement") in large part on the basis of their API.[20] If schools moving in and out of program improvement status are driving our results, then it would not be appropriate to interpret our findings as representative of typical improvements in school quality. As a specification check, we include program

[18]This implies that expanding charter school options, though publicly-funded, may contribute to waning support for traditional public schools. Avery and Pathak (2015) explores the distributional effects of school choice in greater detail.

[19]The salience of API itself does not appear to be at issue. While undoubtedly more households are familiar with API as a measure of public school quality today than when it was first introduced in 1999, the non-monotonic character of our estimated trend suggests that a secular increase in public awareness of API alone cannot explain our main result.

[20]Schools are encouraged to maintain API of at least 800. Those that fall short are issued yearly growth targets. If they fail to maintain adequate progress, the state (or federal government) may intervene, placing the school in "program improvement." Schools that remain in program improvement for several consecutive years risk various sanctions, including closure.

improvement status interacted with year dummy variables as additional control variables. The results, shown in the fourth column, are largely unchanged, consistent with the interpretation that households value changes in API broadly, not simply as a means to avoid school failure.

Finally, it remains possible that homeowners don't value school quality *per se* as much as they value characteristics of schools that happen to be correlated with API – in particular, the racial composition or economic background of the student body. The tendency for higher socioeconomic status students to attend better schools (e.g. to have better qualified and more experienced teachers) is well documented (Koski and Hahnel, 2008; Clotfelter, Ladd, and Vigdor, 2011). But this very correlation makes it difficult, econometrically, to account for this alternate explanation.[21] Nonetheless we explore this possibility by calculating a "synthetic" API for each school in each year representing the portion of the school quality measure explained by – or at least correlated with – the socioeconomic characteristics of enrolled students.[22] We then include this measure, interacted with a vector of year dummies, as additional control variables, permitting us to test directly how homeowners value the portion of API *not* explained by school demographics. The results are shown in the fifth column of Table 3.6.[23] Under this strict test, the countercyclical pattern of the coefficients

[21]Our neighborhood socioeconomic controls should control for some of this effect. But the census data, measured at the block group-level, may not represent bandwidth areas well and, in any event, may not reflect the population of enrolled students.

[22]Specifically we perform year-specific regressions, using the estimation sample, of API on four school-level demographic variables: the share of enrolled students who identified as African American, as Asian, as Hispanic or Latino, and the share who qualified for free or reduced-price meals. (Due to data limitations, the share of students who qualified for free and reduced-price meals in 2001-2003 was interpolated from the shares reported in 2000 and 2004.) The R^2 of these regressions range from .65 to .81, reflecting the high degree of collinearity among these school-level factors.

[23]Due to the inclusion of a generated regressor, the coefficient estimate standard errors for this specification

is still present, but most are no longer statistically significant. So while we cannot rule out the possibility that homeowner preferences for high socioeconomic status students, and not for improved school quality *per se*, contribute to the valuations we observe, that alternative interpretation does not appear to account for our main finding of a countercyclical trend.

3.4.2 Discussion

If our finding of a countercyclical trend in public school quality valuation is robust and not driven by the selection of homes sold over time, what can account for it? At least three explanations are consistent with the observed pattern. The first is the possibility that households perceive the return to school quality to be higher during economic contractions. While there is some research linking graduate school enrollment decisions to business cycle fluctuations, comparable analysis for elementary school is scant – in part because primary education has long been universal (at least in the U.S.) and only recently have researchers been able to observe differences in quality across schools consistently (Kniesner, Padilla, and Polachek, 1978; Psacharopoulos et al., 1996; Johnson, 2013a). But because the state of the business cycle during early childhood will not have much bearing on conditions during the breadth of one's working years, it seems unlikely that parents put much stake in forecasts of economic growth more than a decade out while making decisions concerning their children's primary education.

A second possibility is that credit-constrained homeowners, in an effort to smooth consumption across the business cycle, are "trading down" from private schools to public alter-

are the result of 100 bootstrap replications.

natives during contractions.[24] We find limited circumstantial evidence that private school enrollment was procyclical during our study period. Figure 3.7 shows the share of Los Angeles County students enrolled in private school between 2000 and 2013 for two grade ranges: Kindergarten through 4th grade and Kindergarten through 8th grade.[25] The time series is noisy, but while there is a secular trend of falling private school enrollment shares over the study period, it does appear to accelerate somewhat around the business cycle downturn between 2007 and 2010 – exactly when we would expect substitution away from private school to be most pronounced.[26]

A third explanation concerns the option value of local amenities. During periods of economic contraction and home price uncertainty the housing market tightens, and households change homes less frequently than they do during periods of expansion. Consequently households may be willing to pay relatively more for local amenities (such as public school quality) during "busts" because the option value of these amenities has increased. For example, a household which may have a child (or simply another child) in the near future will put relatively more stock in the quality of the local elementary school if they expect to live in the neighborhood for a few more years. Figure 3.8, which shows the median number of years

[24]Jaimovich, Rebelo, and Wong (2015) observes similar substitution patterns among consumers in the accommodation, apparel, restaurant, home furnishing and general merchandise sectors during the 2007-2012 period.

[25]The vast majority of Los Angeles County elementary schools serve students from Kindergarten through either 5th or 6th grade, but data limitations make calculating population shares for those grade ranges impractical.

[26]Much of the research on the macroeconomic consequences of the recent financial crisis has focused on the supply side, detailing how shocks to household credit drove down consumer spending (Mian and Sufi, 2011; Benmelech, Meisenzahl, and Ramcharan, 2014). But because the procyclical availability of easy credit should work to bid up the capitalization of school quality in home prices during expansions and depress it during contractions, our results seem more consistent with a demand-side explanation.

Los Angeles County householders have lived in their current residence for the years 2000 to 2013, provides some support for this explanation. While the median tenure for owners eased down from 11 to 10 years during the period 2000 to 2006, it jumped to 13 years by 2009 and had only risen a year further by 2013. All else equal we would expect this trend to drive a slow decline, a rapid increase and then a slow rise in homeowners' valuation of local public school quality – which is indeed largely what we find.

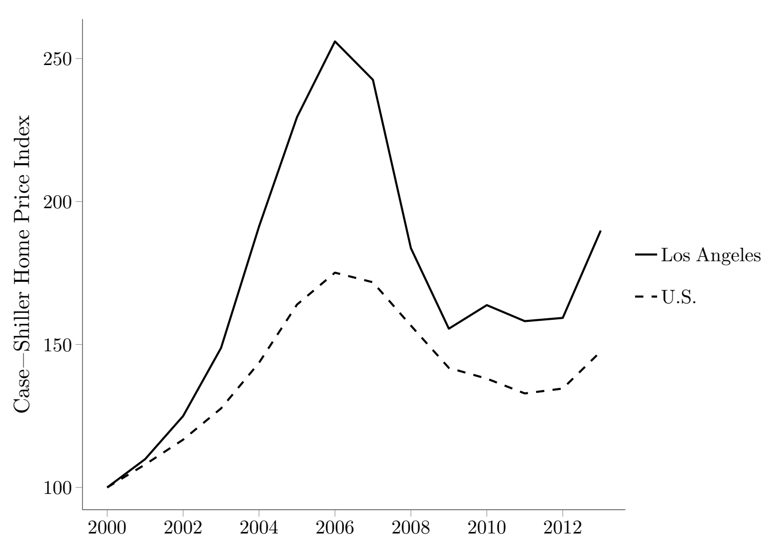
3.5 Conclusion

In this paper, we show that homeowners' valuation of local public school quality is countercyclical. Moreover this variation is economically significant; between the peak of the local housing market in 2007 and its nadir in 2009 we find that the implied valuation of a five-percent improvement in school quality increased from a level indistinguishable from zero to approximately 1.8 percent of a home's sales price. We identify this result with a dynamic hedonic pricing model, exploiting spatial discontinuities at elementary school attendance area boundaries to enhance identification. Our findings are insensitive to a variety of specification checks, and do not appear to be driven either by the selection of homes sold over time or by a secular increase in the salience of our chosen measure of school quality. Instead we suspect that the observed countercyclical pattern stems from the combination of two effects – homeowners' substitution of private for public school during expansions, and the rising option value of local public school attendance during contractions – though further research on homeowner decision-making is necessary to estimate their relative importance.

Figure 3.1: Los Angeles Metropolitan Area GDP Growth

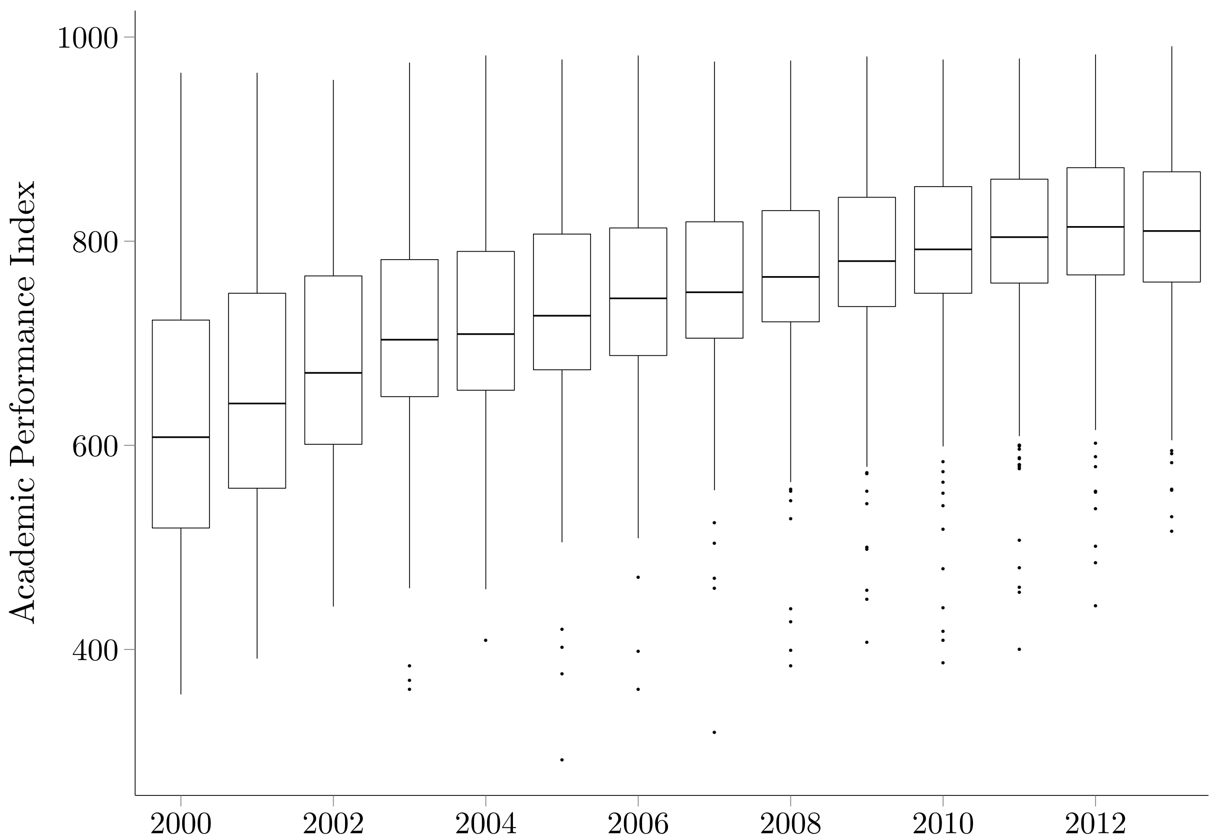
Note: Estimated annual percent changes in current dollars of GDP for the U.S. and the Los Angeles-Long Beach-Anaheim metropolitan statistical area are provided by the BEA.

Figure 3.2: Case-Shiller Home Price Index for Los Angeles Metropolitan Area



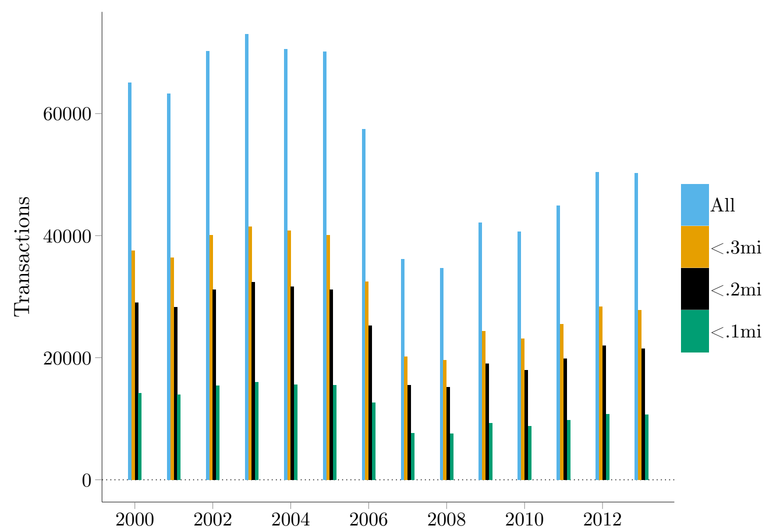
Note: The S&P/Case-Shiller Home Price Indices for the U.S. and the Los Angeles-Long Beach-Anaheim metropolitan statistical area (each normalized so that 2000 is 100) are provided by FRED.

Figure 3.3: Academic Performance Index of Los Angeles County Schools



Note: Growth API for Los Angeles County elementary schools provided by the California Department of Education. The bottom and top of the box show the first and third quartiles, respectively, while the band shows the median. The whiskers show the most extreme data points within one-and-a-half inter-quartile ranges of the box. Dots represent outliers.

Figure 3.4: Count of Transactions by Estimation Sample

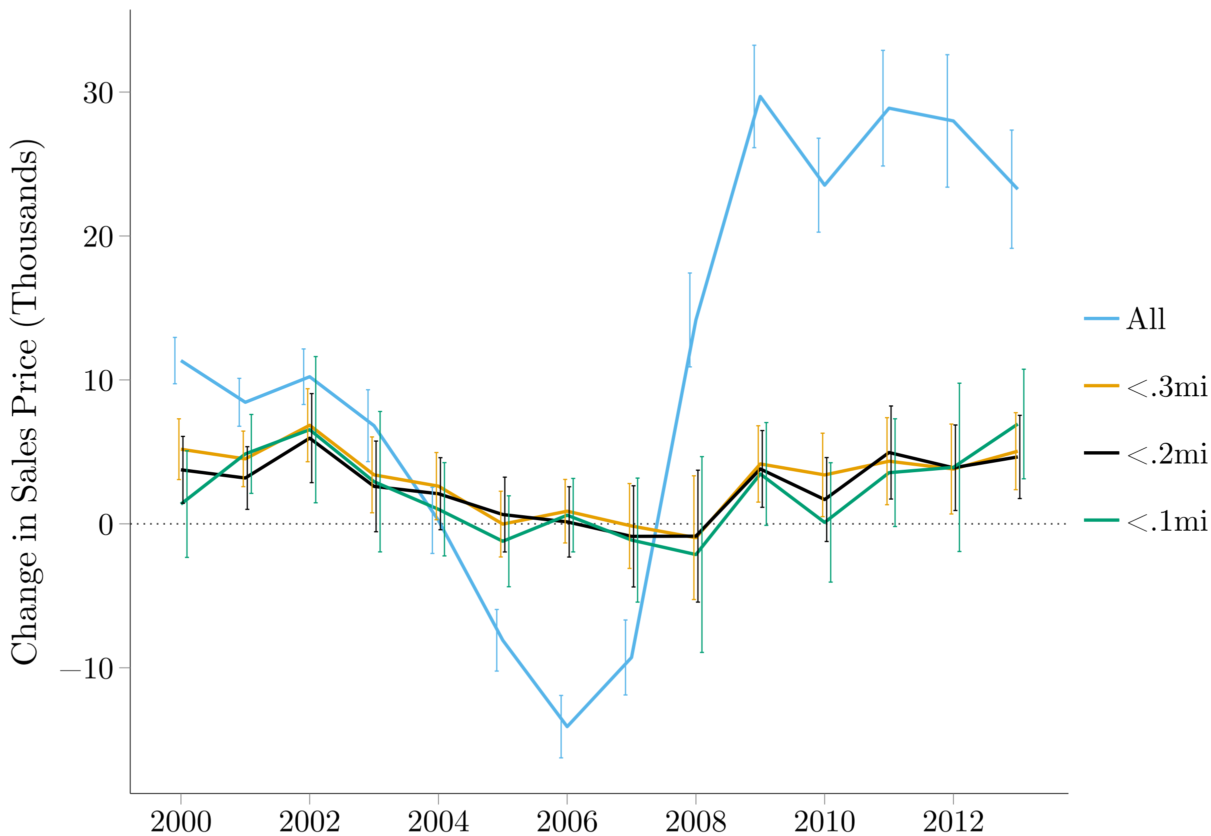


Note: Transaction data provided by DataQuick for 2000-2010 and CoreLogic for 2011-2013.

Figure 3.5: Mean Sales Price by Estimation Sample

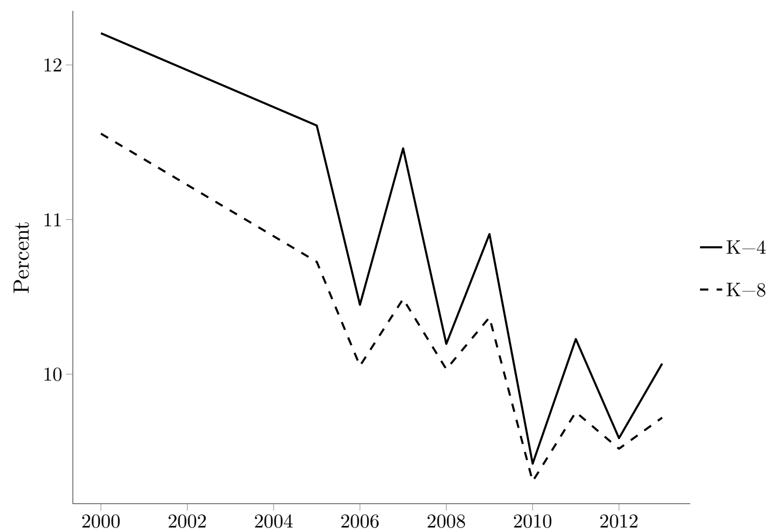
Note: Transaction data provided by DataQuick for 2000-2010 and CoreLogic for 2011-2013.

Figure 3.6: Homeowners' School Quality Valuation Is Counter-Cyclical



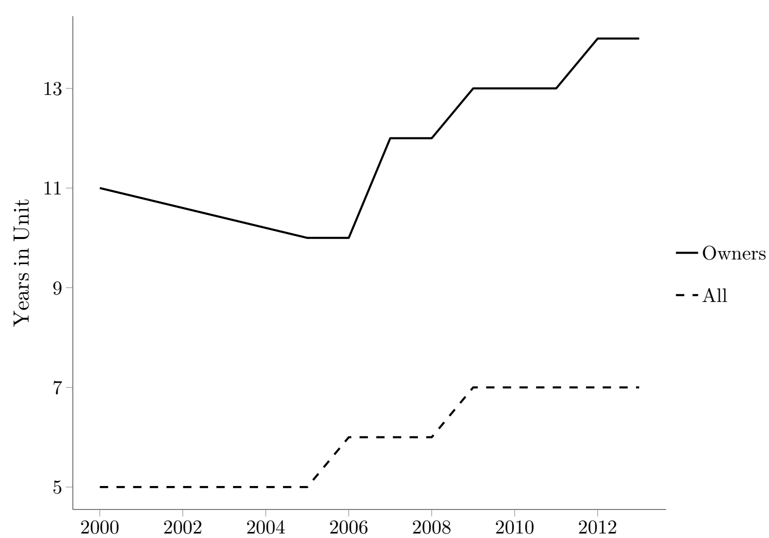
Note: The implied change in sales price (for the mean-valued home) associated with a five-percent improvement in school quality (at the mean) for each transaction year (presented with 95 percent confidence intervals) according to the regressions presented in Table 3.4.

Figure 3.7: Share of Los Angeles County Students Enrolled in Private School



Note: Enrollment data provided by the U.S. Census Bureau – the decennial census for 2000, and the American Community Survey for 2005-2013.

Figure 3.8: Median Tenure for Los Angeles County Householders



Note: Household tenure data provided by the U.S. Census Bureau – the decennial census for 2000, and the American Community Survey for 2005-2013.

Table 3.1: Summary Statistics[a]

	All	<.3mi	<.2mi	<.1mi
Real sales price ($)	570,344	527,936	516,343	504,128
	(693,435)	(645,990)	(673,663)	(504,044)
Elementary school quality[b]	7.56	7.47	7.44	7.42
	(1.08)	(1.10)	(1.10)	(1.10)
Structure controls				
Bedrooms	3.2	3.1	3.1	3.1
	(0.9)	(0.9)	(0.9)	(0.9)
Bathrooms	2.1	2.0	2.0	2.0
	(1.0)	(1.0)	(0.9)	(0.9)
Living space (square ft.)	1715.7	1630.4	1605.3	1585.9
	(893.8)	(806.3)	(786.0)	(765.1)
Building age	49.9	52.4	52.8	52.7
	(22.9)	(22.2)	(22.3)	(22.6)
Education controls[c]				
Property tax (per $1,000 assessed value)	11.8	11.9	11.9	11.9
	(0.8)	(0.9)	(0.9)	(0.9)
Student-teacher ratio	20.7	20.6	20.6	20.6
	(4.3)	(4.2)	(4.1)	(4.0)
Per-pupil expenditure ($)	11,907	12,152	12,145	12,113
	(2,489)	(2,424)	(2,419)	(2,411)
Socioeconomic controls[d]				
Median household income ($)	57,672	54,421	53,560	53,259
	(26,716)	(24,396)	(23,945)	(23,708)
Population per mi^2	8,908	9,748	10,079	10,308
	(6,323)	(6,381)	(6,518)	(6,626)
Black	0.10	0.10	0.11	0.11
	(0.17)	(0.18)	(0.18)	(0.18)
Asian	0.12	0.12	0.12	0.12
	(0.14)	(0.13)	(0.14)	(0.14)
Hispanic	0.35	0.38	0.39	0.40
	(0.27)	(0.27)	(0.27)	(0.28)
High school graduate (25+)	0.75	0.73	0.72	0.71
	(0.20)	(0.20)	(0.21)	(0.21)
College graduate (25+)	0.26	0.25	0.24	0.23
	(0.19)	(0.18)	(0.18)	(0.18)
Reside with own-children	0.40	0.40	0.41	0.41
	(0.12)	(0.12)	(0.12)	(0.12)
Own residence	0.68	0.66	0.65	0.65
	(0.23)	(0.23)	(0.23)	(0.23)
Geographic controls				
Distance to employment centers (mi.)[e]	20.8	20.0	19.9	19.9
	(9.1)	(8.5)	(8.4)	(8.3)
Distance to shoreline (mi.)	15.1	14.2	14.0	14.0
	(12.9)	(11.9)	(11.6)	(11.5)
Observations	769,140	438,278	340,098	168,016

[a] Means (standard deviations) are reported.

[b] Annual elementary school quality as measured by the California Department of Education's Growth Academic Performance Index (in hundreds).

[c] The property tax represents the tax bill per $1,000 of assessed value for fiscal year 2015. The student-teacher ratio is measured at the elementary school-level as of school year 2009-10. Per-pupil expenditure is measured at the school district-level as of school year 2009-10.

[d] Socioeconomic controls are measured at block group-level as of 2000, and (except for household income and population density) represent shares.

[e] Distance to employment centers is calculated as the employment-weighted average distance to the 40 ZIP code areas with the highest average employment over the period 2000-2010.

Table 3.2: Homeowners Value Improvements in Local Public School Quality

Dependent variable: Real sales price (ln \$)[a]

	All	<.3mi	<.2mi	<.1mi	<.1mi
Elementary school quality[b]	0.039***	0.015***	0.011***	0.008***	0.033***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Structure controls					
Bedrooms	-0.017***	-0.009***	-0.009***	-0.005***	-0.012***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Bathrooms	-0.003	-0.003	-0.017	-0.042***	-0.053***
	(0.011)	(0.008)	(0.010)	(0.005)	(0.005)
Bathrooms^2	0.005***	0.003**	0.005***	0.009***	0.013***
	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Living space (ln square ft.)	0.530***	0.488***	0.480***	0.467***	0.503***
	(0.005)	(0.004)	(0.005)	(0.006)	(0.006)
Building age	0.003***	0.001***	0.001***	0.001***	0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Building age^2	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Education controls[c]	✓	✓	✓	✓	✓
Socioeconomic controls[d]	✓				✓
Geographic controls[e]	✓				✓
Quarter-of-sale dummies	✓	✓	✓	✓	✓
County subdivision-year dummies[f]	✓	✓	✓	✓	✓
Boundary fixed effects		✓	✓	✓	
Number of boundaries		1,585	1,579	1,552	
Observations	769,140	438,278	340,098	168,016	168,016
Adjusted R^2	0.75	0.77	0.76	0.75	0.72
Memo item:					
A five-percent improvement in school quality (at the mean) corresponds to a...					
	1.43%	0.56%	0.39%	0.30%	1.19%
	\$8,283	\$3,222	\$2,267	\$1,770	\$6,935
...change in sales price (for the mean-valued home).[g]					

[a]Coefficient estimates (standard errors) are reported. All standard errors are adjusted for clustering at the elementary school-year-level.

[b]Annual elementary school quality as measured by the California Department of Education's Growth Academic Performance Index (in hundreds).

[c]School controls include structure-level property tax assessment, elementary school-level student-teacher ratio, and (for "All" and the latter "<.1mi") school district-level expenditures per student.

[d]Neighborhood controls include block group-level household income, population density, as well as share black, Asian, Hispanic, high school and college graduate, residing with own children, and owning residence.

[e]Geographic controls include second-order polynomials in distance to employment centers and distance to the shoreline.

[f]The U.S. Census Bureau divided Los Angeles County into 20 subdivisions for statistical purposes in 2010.

[g]Between 2000 and 2013, the mean API among all schools in Los Angeles County was 726.5, and the mean sales price of a single-family residence in the estimation sample was \$580,597 (in 2015 dollars).

Table 3.3: Differences Across Elementary School Boundaries Decline With Bandwidth[a]

	All[b]	<.3mi	<.2mi	<.1mi
Real sales price (ln $)	0.046***	0.029***	0.028***	0.018*
	(0.010)	(0.010)	(0.009)	(0.010)
Elementary school quality	0.45***	0.43***	0.44***	0.44***
	(0.02)	(0.02)	(0.02)	(0.02)
Structure controls				
Bedrooms	0.033***	0.024***	0.022***	0.015*
	(0.007)	(0.007)	(0.008)	(0.009)
Bathrooms	0.061***	0.041***	0.039***	0.029***
	(0.010)	(0.009)	(0.009)	(0.010)
Living space (ln square ft.)	0.031***	0.021***	0.021***	0.015***
	(0.005)	(0.005)	(0.005)	(0.005)
Building age	-0.71**	-0.90***	-1.06***	-0.86**
	(0.33)	(0.32)	(0.33)	(0.36)
School controls				
Property tax (per $1,000 assessed value)	-0.04***	0.00	0.01	0.01
	(0.01)	(0.02)	(0.02)	(0.02)
Student-teacher ratio	0.695***	0.592***	0.565***	0.471***
	(0.101)	(0.104)	(0.102)	(0.096)
Per-pupil expenditure ($)	-176.5***			
	(41.1)			
Neighborhood controls				
Median household income ($)	2262***	1672***	1784***	1449***
	(371)	(348)	(339)	(357)
Population per mi^2	-391***	-452***	-507***	-418***
	(78.5)	(82.9)	(86.4)	(93.3)
Black	-0.009***	-0.005*	-0.005	-0.004
	(0.002)	(0.003)	(0.003)	(0.003)
Asian	0.011***	0.007***	0.006***	0.004*
	(0.002)	(0.002)	(0.002)	(0.002)
Hispanic	-0.017***	-0.015***	-0.016***	-0.011***
	(0.004)	(0.004)	(0.004)	(0.004)
High school graduate (25+)	0.019***	0.015***	0.015***	0.012***
	(0.003)	(0.003)	(0.003)	(0.003)
College graduate (25+)	0.015***	0.011***	0.011***	0.008***
	(0.003)	(0.003)	(0.003)	(0.003)
Reside with own-children	-0.003	-0.004**	-0.004**	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Own residence	0.020***	0.017***	0.016***	0.014***
	(0.003)	(0.003)	(0.003)	(0.003)
Geographic controls				
Distance to employment centers (mi.)	-0.171	-0.064	-0.081	-0.040
	(0.189)	(0.183)	(0.180)	(0.181)
Distance to shoreline (mi.)	-0.057	-0.059	-0.042	0.012
	(0.270)	(0.265)	(0.260)	(0.261)
Observations	645,694	438,278	340,098	168,016

[a]Each cell shows the coefficient estimate (standard error) from a regression of each listed variable on a dummy variable – 1 if the house is on the side of the boundary with higher measured elementary school quality, 0 otherwise. All standard errors are adjusted for clustering at the elementary school-year-level.

[b]118,829 observations included in Table 3.2 are excluded here because the nearest boundary does not feature transactions observed on both sides.

Table 3.4: Homeowners' School Quality Valuation Is Counter-Cyclical

Dependent variable: Real sales price (ln \$)[a]

	All	<.3mi	<.2mi	<.1mi
Elementary school quality[b] ×...				
2000	0.054***	0.025***	0.018***	0.007
	(0.004)	(0.005)	(0.006)	(0.009)
2001	0.040***	0.021***	0.015***	0.023***
	(0.004)	(0.005)	(0.005)	(0.007)
2002	0.048***	0.032***	0.028***	0.031**
	(0.005)	(0.006)	(0.007)	(0.012)
2003	0.032***	0.016**	0.012	0.014
	(0.006)	(0.006)	(0.008)	(0.012)
2004	0.001	0.012**	0.010	0.005
	(0.006)	(0.006)	(0.006)	(0.008)
2005	-0.038***	-0.000	0.003	-0.006
	(0.005)	(0.006)	(0.006)	(0.008)
2006	-0.067***	0.004	0.001	0.003
	(0.005)	(0.005)	(0.006)	(0.006)
2007	-0.044***	-0.001	-0.004	-0.005
	(0.006)	(0.007)	(0.009)	(0.010)
2008	0.067***	-0.005	-0.004	-0.010
	(0.008)	(0.010)	(0.011)	(0.016)
2009	0.141***	0.020***	0.018***	0.016*
	(0.009)	(0.006)	(0.006)	(0.009)
2010	0.112***	0.016**	0.008	0.000
	(0.008)	(0.007)	(0.007)	(0.010)
2011	0.137***	0.021***	0.024***	0.017*
	(0.010)	(0.007)	(0.008)	(0.009)
2012	0.133***	0.018**	0.018**	0.019
	(0.011)	(0.008)	(0.007)	(0.014)
2013	0.110***	0.024***	0.022***	0.033***
	(0.010)	(0.006)	(0.007)	(0.009)
Structure controls[c]	✓	✓	✓	✓
Education controls[d]	✓	✓	✓	✓
Socioeconomic controls[e]	✓	✓	✓	✓
Geographic controls[f]	✓	✓	✓	✓
Quarter-of-sale dummies	✓	✓	✓	✓
County subdivision-year dummies[g]	✓			
Boundary-year dummies		✓	✓	✓
Number of boundaries		18,465	18,327	17,459
Observations	769,140	423,302	328,557	162,473
Adjusted R^2	0.76	0.79	0.78	0.77

[a] Coefficient estimates (standard errors) are reported. All standard errors are adjusted for clustering at the elementary school-year-level.

[b] Annual elementary school quality as measured by the California Department of Education's Growth Academic Performance Index (in hundreds).

[c] Structure controls include number of bedrooms, living space and second-order polynomials in bathrooms and building age.

[d] School controls include structure-level property tax assessment, elementary school-level student-teacher ratio, and (for "All") school district-level expenditures per student.

[e] Neighborhood controls include block group-level household income, population density, as well as share black, Asian, Hispanic, high school and college graduate, residing with own children, and owning residence.

[f] Geographic controls include second-order polynomials in distance to employment centers and distance to the shoreline.

[g] The U.S. Census Bureau divided Los Angeles County into 20 subdivisions for statistical purposes in 2010.

Table 3.5: Estimated Counter-Cyclical Trend Is Statistically Significant

	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
2000	0.003	-0.010	0.005	0.008	0.015	0.017**	0.022**	0.022	-0.000	0.010	-0.006	-0.001	-0.004
2001		-0.013	0.003	0.005	0.012	0.014	0.019	0.019	-0.003	0.007	-0.008	-0.003	-0.007
2002			0.016	0.018	0.025***	0.028***	0.032***	0.032**	0.010	0.020**	0.005	0.010	0.006
2003				0.002	0.009	0.012	0.016	0.016	-0.006	0.004	-0.011	-0.006	-0.010
2004					0.007	0.009	0.014	0.014	-0.008	0.002	-0.014	-0.009	-0.012
2005						0.002	0.007	0.007	-0.015	-0.005	-0.020**	-0.015	-0.019**
2006							0.005	0.005	-0.017**	-0.007	-0.023**	-0.018	-0.021**
2007								-0.000	-0.022**	-0.012	-0.028**	-0.023**	-0.026**
2008									-0.022	-0.012	-0.028**	-0.023	-0.026**
2009										0.010	-0.005	-0.000	-0.004
2010											-0.015	-0.010	-0.014
2011												0.005	0.001
2012													-0.004

Note: Each cell displays the difference in the regression coefficient on elementary school quality between the row year and the column year using the estimates for the "<.2mi" specification presented in Table 3.4.

Table 3.6: Panel A – Testing the Difference of Means Across Transaction Years

	2000	2001	2002	2003	2004	2005	2006
Real sales price (ln $)	-0.39***	-0.31***	-0.18***	-0.02	0.20***	0.37***	0.44***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Elementary school quality	-1.17***	-0.85***	-0.54***	-0.19***	-0.17***	-0.03	0.08**
	(0.06)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
Structure controls							
Bedrooms	-0.00	-0.03*	0.00	-0.01	-0.01	0.00	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Bathrooms	0.00	-0.01	0.00	-0.01	-0.02	-0.02	-0.06***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Living space (ln square ft.)	0.01	-0.01	0.01	-0.01	-0.01	-0.01	-0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Building age	-5.32***	-4.48***	-3.69***	-2.49***	-2.25***	-1.82**	0.58
	(0.69)	(0.68)	(0.73)	(0.69)	(0.74)	(0.91)	(0.77)
Observations	14,370	13,938	15,365	16,093	15,730	15,367	12,272

	2007	2008	2009	2010	2011	2012	2013
Real sales price (ln $)	0.47***	0.03	-0.19***	-0.15***	-0.18***	-0.13***	0.10***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Elementary school quality	0.36***	0.41***	0.48***	0.62***	0.79***	0.86***	0.87***
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Structure controls							
Bedrooms	0.01	0.03	-0.03	-0.07***	0.05**	0.07***	0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Bathrooms	0.05	0.02	-0.07***	-0.09***	0.07***	0.10***	0.10***
	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Living space (ln square ft.)	0.03**	0.01	-0.03***	-0.03***	0.02*	0.04***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Building age	0.61	0.31	3.24***	5.47***	5.57***	6.32***	7.65***
	(0.80)	(0.84)	(0.81)	(0.72)	(0.70)	(0.70)	(0.67)
Observations	7,530	7,247	9,256	8,721	9,427	10,545	10,223

Note: Each cell shows the coefficient estimate (standard error) from a regression of each listed variable on a dummy variable – 1 if the transaction occurred in the given year, 0 otherwise – using the .2 mile restricted sample. All standard errors are adjusted for clustering at the elementary school-year-level.

Panel B – Testing the Difference of Means Across Transaction Years[a]

	2000	2001	2002	2003	2004	2005	2006
Education controls							
Property tax (per $1,000 assessed value)	-0.02	-0.05	-0.04	-0.02	0.03	0.03	0.04
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Student-teacher ratio	0.19	0.21	0.20	0.02	-0.06	-0.59	-0.41
	(0.19)	(0.19)	(0.20)	(0.21)	(0.25)	(0.36)	(0.31)
Per-pupil expenditure ($)	-3.75	-25.21	-27.04	0.62	-19.59	-93.47	11.52
	(104.40)	(106.29)	(105.48)	(105.06)	(105.83)	(111.90)	(109.44)
Socioeconomic controls							
Median household income ($)	569.14	495.74	1114.06	392.66	-1364.25*	-1788.32**	-2713.92***
	(903.84)	(855.86)	(889.40)	(898.02)	(817.03)	(808.88)	(818.74)
Population per mi^2	44.90	79.16	-46.48	-34.17	54.20	84.07	350.57
	(202.66)	(208.25)	(206.23)	(206.47)	(203.85)	(238.43)	(214.44)
Black	-0.00	-0.01	-0.01	-0.01	-0.00	0.01	0.02*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Asian	0.01	0.01	0.01	0.00	-0.00	-0.01	-0.01**
	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	14,370	13,938	15,365	16,093	15,730	15,367	12,272

	2007	2008	2009	2010	2011	2012	2013
Education controls							
Property tax (per $1,000 assessed value)	-0.02	-0.00	0.11**	0.03	-0.01	-0.02	-0.06*
	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.03)	(0.03)
Student-teacher ratio	0.09	-0.14	0.70***	-0.15	-0.09	0.02	0.15
	(0.20)	(0.29)	(0.12)	(0.24)	(0.21)	(0.21)	(0.20)
Per-pupil expenditure ($)	84.68	-203.32*	98.77	38.02	55.59	74.26	61.09
	(108.62)	(115.85)	(103.37)	(105.70)	(105.82)	(107.22)	(107.17)
Socioeconomic controls							
Median household income ($)	1962.29**	724.11	-1841.60**	-1143.37	667.92	1070.50	3135.02***
	(948.93)	(847.90)	(797.80)	(861.66)	(946.80)	(983.51)	(1027.25)
Population per mi^2	-268.03	-666.27***	213.01	314.48	74.73	-89.43	-425.39**
	(205.67)	(202.14)	(207.41)	(215.40)	(213.44)	(199.09)	(203.94)
Black	-0.01	-0.01*	0.01	0.00	0.01	0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Asian	0.00	-0.00	-0.01	-0.00	-0.01	-0.00	0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Observations	7,530	7,247	9,256	8,721	9,427	10,545	10,223

Note: Each cell shows the coefficient estimate (standard error) from a regression of each listed variable on a dummy variable – 1 if the transaction occurred in the given year, 0 otherwise – using the .2 mile restricted sample. All standard errors are adjusted for clustering at the elementary school-year-level.

Panel C – Testing the Difference of Means Across Transaction Years[a]

	2000	2001	2002	2003	2004	2005	2006
Socioeconomic controls (cont.)							
Hispanic	-0.01	-0.01	-0.01	-0.00	0.01	0.01	0.03**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
High school graduate (25+)	0.01	0.01	0.01	0.00	-0.01	-0.01	-0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
College graduate (25+)	0.01	0.01	0.01	0.00	-0.01	-0.02**	-0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Reside with own-children	-0.00	-0.00	-0.00	-0.00	0.01	0.01**	0.01**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Own residence	-0.00	0.00	0.00	-0.00	-0.01	-0.00	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Geographic controls							
Distance to employment centers (mi.)	-0.35	-0.34	-0.18	0.04	0.45	0.99*	0.37
	(0.38)	(0.37)	(0.40)	(0.40)	(0.46)	(0.55)	(0.50)
Distance to shoreline (mi.)	-0.70	-0.57	-0.30	-0.08	0.66	1.52*	0.67
	(0.53)	(0.52)	(0.57)	(0.57)	(0.66)	(0.79)	(0.71)
Observations	14,370	13,938	15,365	16,093	15,730	15,367	12,272

	2007	2008	2009	2010	2011	2012	2013
Socioeconomic controls (cont.)							
Hispanic	-0.03***	-0.01	0.03***	0.02	0.00	-0.01	-0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
High school graduate (25+)	0.02***	0.01*	-0.02***	-0.01*	-0.00	0.00	0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
College graduate (25+)	0.02***	-0.00	-0.02***	-0.01	0.00	0.01	0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Reside with own-children	-0.01***	-0.00	0.01***	0.01	-0.00	-0.01	-0.02***
	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Own residence	0.00	0.02**	0.00	-0.00	0.00	-0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Geographic controls							
Distance to employment centers (mi.)	-0.58	1.19**	-0.03	-0.06	-0.54	-0.62*	-0.62
	(0.40)	(0.52)	(0.44)	(0.43)	(0.38)	(0.38)	(0.38)
Distance to shoreline (mi.)	-0.58	1.70**	0.15	-0.18	-0.77	-0.92*	-0.82
	(0.55)	(0.74)	(0.65)	(0.60)	(0.53)	(0.51)	(0.51)
Observations	7,530	7,247	9,256	8,721	9,427	10,545	10,223

Note: Each cell shows the coefficient estimate (standard error) from a regression of each listed variable on a dummy variable – 1 if the transaction occurred in the given year, 0 otherwise – using the .2 mile restricted sample. All standard errors are adjusted for clustering at the elementary school-year-level.

Table 3.6: Counter-Cyclical Trend Is Robust to Various Specification Checks

Dependent variable: Real sales price (ln \$)[a]

	Exclude LAUSD[b]	Exclude Private or Choice >10%[c]	API Decile[d]	Program Improvement[e]	Synthetic API[f]
Elementary school quality[b] ×...					
2000	0.023***	0.038***	0.005**		0.008
	(0.007)	(0.010)	(0.002)		(0.010)
2001	0.005	0.030***	0.007***		0.016**
	(0.007)	(0.008)	(0.002)		(0.007)
2002	0.016**	0.029***	0.010***		0.022*
	(0.007)	(0.011)	(0.003)		(0.012)
2003	-0.005	0.009	0.004*	0.014*	0.005
	(0.010)	(0.014)	(0.002)	(0.008)	(0.011)
2004	0.006	0.011	0.002	0.011*	0.008
	(0.007)	(0.009)	(0.002)	(0.006)	(0.008)
2005	-0.005	0.002	0.001	0.006	-0.002
	(0.007)	(0.008)	(0.002)	(0.007)	(0.007)
2006	-0.004	0.003	-0.000	0.001	-0.003
	(0.008)	(0.008)	(0.002)	(0.007)	(0.007)
2007	0.003	0.014	-0.002	-0.001	-0.011
	(0.009)	(0.012)	(0.002)	(0.010)	(0.009)
2008	0.020	0.037**	0.000	0.004	-0.030**
	(0.013)	(0.015)	(0.003)	(0.012)	(0.012)
2009	0.024***	0.022*	0.005***	0.032***	-0.004
	(0.008)	(0.012)	(0.002)	(0.008)	(0.008)
2010	0.010	0.028**	0.003*	0.001	-0.009
	(0.010)	(0.011)	(0.002)	(0.008)	(0.008)
2011	0.018**	0.027**	0.006***	0.022**	0.002
	(0.009)	(0.011)	(0.002)	(0.009)	(0.007)
2012	0.011	0.032**	0.005***	0.016**	0.011
	(0.008)	(0.013)	(0.002)	(0.008)	(0.011)
2013	0.017**	0.029***	0.006***	0.021***	0.004
	(0.008)	(0.010)	(0.002)	(0.008)	(0.008)
Structure controls[c]	✓	✓	✓	✓	✓
Education controls[d]	✓	✓	✓	✓	✓
Socioeconomic controls[e]	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓
Quarter-of-sale dummies	✓	✓	✓	✓	✓
Boundary-year dummies	✓	✓	✓	✓	✓
Number of boundaries	9,805	4,240	18,327	14,222	18,321
Observations	181,321	72,114	328,557	242,224	328,400
Adjusted R^2	0.81	0.74	0.78	0.81	0.78

[a]Coefficient estimates (standard errors) are reported. All specification checks represent deviations from the baseline "<.2mi" specification presented in Table 3.4. All standard errors are adjusted for clustering at the elementary school-year-level (except for the last specification, where standard errors are the result of 100 bootstrap replications).

[b]This specification excludes all homes within the Los Angeles Unified School District.

[c]This specification excludes homes in 45 (out of a total of 69) school districts (including LAUSD) in which more than 10 percent of K-8 students enroll in private schools, or more than 10 percent of K-8 students employ an educational choice option (e.g. charter, magnet).

[d]This specification uses decile ranks (calculated annually among all Los Angeles County elementary schools) of the California Department of Education's Growth Academic Performance Index as the measure of elementary school quality.

[e]This specification includes the lack of "program improvement" status interacted with a vector of year dummies as additional control variables.

[f]This specification includes a "synthetic" Academic Performance Index interacted with a vector of year dummies as additional control variables. (This value is calculated from a year-specific regression, using the estimation sample, of API on four school-level demographic variables: the share of enrolled students who identified as African American, as Asian, as Hispanic or Latino, and the share who qualified for free or reduced-price meals.) The number of observations differs from that of the baseline regression due to limited cases of missing school-level demographic data.

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