Housing Disease and Public School Finances

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Abstract: We propose a new mechanism to explain part of the large increase in expenditures per student in U.S. public schools in the 1990s and 2000s: housing disease is a fiscal externality from housing markets in which unexpected booms generate extra revenues that school administrators have incentives to spend. We establish the importance of housing disease by (i) assembling a novel microdata set containing the universe of housing transactions for a large sample of school districts; and (ii) using the timelines of school district housing booms to disentangle the effects of housing disease from reverse causality and changes in household composition. We find housing price elasticities of per-pupil expenditures of 0.16-0.20, which in turn account for 20 percent of the growth in public school spending for districts with at least one housing boom. School districts primarily spent the extra resources on instruction and capital projects, not on administrative expenditures, suggesting that the cost increase was accompanied by improvements in the quality of school inputs.

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I. Introduction

Median expenditure per student in U.S. public schools grew from $9,131 in 1990 to $12,407 in fiscal year 2008/9, a real change of 36%. Around the same time period, house prices in U.S. school districts skyrocketed, with the median district having prices increasing from $169K in 1993 to $284K by the end of 2007 – see both times series in Figure 1. Could the housing boom have caused those major changes in school expenditures per pupil? The answer is "no" according to the leading local public finance models (Oates 1969) because house prices are just a function of local taxes and amenities,[1] and therefore are thought to have a muted effect on local finances. This intuition comes from Tiebout (1956), who posited that local expenditures solely reflect household sorting and preferences for public goods.[2] Moreover, local government finances can be determined by other factors, such as fiscal federalism,[3] local governmental decisions and transfer schemes[4], local autonomy and competition[5], and more recent "mandates" such as pension benefits and special education.[6]

In this paper we re-examine this standard answer. We propose a new mechanism – housing disease – to explain some of the changes in local school expenditures during the 1990s and 2000s. Housing disease is a spillover from housing markets. First, housing booms generate unusually high growth rates of housing prices. That triggers a growth in school district revenues given that local governments raise a share of their funds via property or land.[7] In turn, school district administrators may have incentives to spend the extra revenues without consulting voters due to complicated budget rules, frictions in re-optimizing tax rates, or pure rent-seeking. The

[1] This property allows researchers to recover willingness to pay for local public goods and to test whether those goods are provided at efficient levels. See Bayer, Ferreira and McMillan (2007) on how to estimate willingness to pay for school quality using housing prices, and Brueckner (1979), Barrow and Rouse (2004), and Cellini, Ferreira and Rothstein (2010) on how to test for efficiency in the provision of local public goods.

[2] A long literature shows the importance of household preferences and sorting for determining the quality of public education, such as Epple and Sieg (1999), Fernández and Rogerson (2001), Hilber and Mayer (2009), and Epple, Romano and Sieg (2012).

[3] Reviews of the fiscal federalism literature can be found in Oates (1999, 2005).

[4] For the impact of local politics see Ferreira and Gyourko (2009) and more recently Macartney and Singleton (2017). For the effects of equalization and transfer schemes in education see Murray, Evans and Schwab (1998), Hoxby (2001), Bradbury, Mayer and Case (2001), Card and Payne (2002), and more recently Jackson, Johnson, and Persico (2016) and Lafortune, Rothstein and Schanzenbach (2018).

[5] See Hoxby (2000), Rothstein (2007), Hoxby (2007), and Clark (2009).

[6] See Novy-Marx and Rauh (2009, 2011, and 2012) and Brinkman, Coen-Pirani, and Sieg (2018)

[7] Local governments generally apply a property tax rate over assessed house values in order to calculate property tax bills. Each local government has its own set of rules for determining and updating assessed house values and tax rates. We discuss the details of the decentralized U.S. systems of property taxes in the next section.

end result is an increase in education expenditures without a corresponding shift in local preferences.

This type of mechanism is not unprecedented in the economics literature. In fact we use the word "disease" to emphasize its similarity to Baumol and Bowen (1966)'s cost disease, a canonical example of a spillover to the cost of public education stemming from conditions in a separate market. The primary difference is that, whereas Baumol and Bowen's cost disease originates in the labor market, the housing disease's genesis is the housing market.

The first challenge in estimating the importance of housing disease is that house prices are endogenous to school quality and household composition. We deal with this issue by using the timeline of housing booms in each school district in our sample. The variation from local housing booms has two features that are key to our research design: a) different school districts began to boom across a decade-long period from mid-1990s to the mid-2000s, some of them multiple times, allowing us to remove the impact of national macroeconomic factors; b) housing booms in the last cycle were associated neither with changes in school quality nor with widespread changes in household composition. In Section IV we show how to estimate the timeline of local booms using time series methods developed by Ferreira and Gyourko (2021) and empirically validate the research design by directly testing the two key features above.[8]

The second challenge is that housing data is generally not available for a large sample of school districts. We solve this problem by amassing the most recent version of the CoreLogic universe of housing transactions from 1993 to 2013 and mapping each home to school district boundaries. Our sample covers more than 2,000 school districts with almost 60% of all total enrollment in public schools. The micro data provide us with the additional advantage of allowing us to use a split-sample approach, such as in Card, Mas and Rothstein (2008), to deal with specification search bias that arises when the same time series is used to estimate both the timeline and magnitude of a housing boom (Leamer 1983).

We find that school district house prices are nearly 20 percent larger by the end of the fifth year of a housing boom, when compared to the pre-boom year (net of other housing booms

[8] Charles, Hurst, and Notowidigdo (2018) and DeFusco et al (2018) use a similar methodology to estimate the impact of housing booms on investments in human capital and on price increases in nearby metro areas, respectively.

in the same district, and net of time and district effects). Expenditures per pupil creep up with a one to two-year lag, turning statistically significant at year 3 and becoming 3 percent larger by the fifth year of a boom. With those magnitudes in hand we can back out the house price elasticity of public school finances. We find an elasticity between 0.16 and 0.20, with our favorite specification resulting in 0.18. This relatively small elasticity is justified by the fact that a large fraction of school district revenues now come from state and federal transfers, especially for low income districts.[9] We also find slightly larger elasticities for local housing busts, but our research design is only internally valid for housing booms – housing busts are usually accompanied by drops in income and employment that are difficult to disentangle, and homeowners may be more likely to appeal their house value assessments (McMillen (2013)).

The estimates are robust to a number of tests based on samples that include subsets of school districts, by type (unified, elementary, and secondary) or by the level of independence. The estimates are also robust to using different definitions of housing breakpoints, and to the inclusion of a number of demographic characteristics as controls. We further estimate econometric models in levels (as opposed to using logs) in order to recover the portion of an additional dollar in the housing tax base that flows to general school expenditures. We find that a $100 increase in the local housing tax base leads to a $1 increase in school expenditures, which is a reasonable approximation of effective property tax rates in the United States. This also implies that local school districts are not engaging in major changes in statutory tax rates after the begin of a housing boom. In the final robustness test we estimate the same econometric models for U.S. municipalities. Those local jurisdictions also raise part of their revenues via property taxes, and we find a similar housing disease effect.

Our main estimates imply that housing disease, for districts with at least one housing boom, can account for 20 percent of the median growth in public education spending during the 1990s and 2000s. These results imply a breakdown of the theoretically efficient choices made by Tiebout-type households. Since the optimal level of expenditures equates marginal costs (local taxation) with marginal benefits (school services), additional spending induces costs that exceed

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[9] Those transfers now correspond to more than 50% of total revenues, but this number is difficult to properly measure given that the data may not distinguish between the jurisdiction that collects taxes versus the jurisdictions that actually has control over taxes – see Hoxby (1996).

its benefits. This inefficiency is the cost of housing disease.[10] Empirically though, it is natural to ask whether these additional resources are really being wasted.

We address this question by using detailed school district finance data to measure how the additional expenditures generated via housing disease are spent. Total expenditure per pupil increases by $350 five years after a housing boom begins, with $163 assigned to current expenses and $178 allocated to capital expenditures, an almost equal split. Instruction corresponds to almost the totality of the increased current expenses; and we do not find new resources being used to increase administrative expenses, ruling out the worst type of rent seeking.

Among the instructional expenses we also find that pupil-teacher ratios, a proxy for educational quality, improve but at a fairly small rate (less than 1% reduction in pupil-teacher ratio). We also find a large increase in average salaries and benefits: 4.6% and 5.1% respectively by the end of the fifth year of a housing boom. Those changes in school wages occur in contrast to average MSA level changes in personal income that remain flat during the housing booms we study. These wage results could reflect either increases in school personnel quality/productivity or rent-seeking, a possibility raised by Brueckner and Neumark (2014) and Diamond (2017).

Despite this potential ambiguity in the interpretation of the wage effects, the fact that pupil-teacher ratios decreased, capital budgets grew, and administrative expenses remained flat suggests that housing disease is accompanied by improvements in the quality of school inputs, and that bureaucrats are not capturing the increased resources. To check for effects on educational output, we also provide test score estimates based on National Assessment of Educational Progress (NAEP), though this analysis suffers from additional shortcomings, noisy measurements, and limited statistical power. Not surprisingly, almost none of the test score estimates are statistically different from zero.

Overall, this research contributes to the understanding of the increase in public education spending of 1990s and 2000s. While the large amount of resources devoted to public education still sparks a debate over whether money matters for improving school quality,[11] we focus on

[10] Details of school finances and the Tiebout model are discussed in Section II.
[11] Some key studies on this topic include Coleman et al. (1966), Hanushek (1986), Card and Krueger (1992), Krueger (1999), and Hanushek and Rivkin (2006).

understanding why the recent growth happened in the first place. We propose and test a new mechanism that is generally not taken into account by standard theory and is difficult to study given data and design limitations. Housing disease may in fact become more relevant in the near future because extreme price fluctuations are becoming a feature of the system as opposed to a one-time bug. Housing markets are now characterized by many local housing booms and busts (Ferreira and Gyourko (2021), Sinai (2013)), fueled by both behavioral and financial factors (Shiller (2005), Mian and Sufi (2009), Favara and Imbs (2015)), and exacerbated by regulations that limit the supply of new housing (Glaeser and Gyourko (2018)).

The remainder of the paper is organized as follows: Section II reviews how school district finances work and the potential for housing disease; Section III then describes the data sources and sample construction; Section IV then describes our empirical framework and tests the validity of our research design; Section V presents our estimates; and Section VI concludes.

II. Public School Finances and Housing Markets

School districts in the United States are funded by a mix of local, state, and federal revenue. In 2014, States and localities provide 46% and 45% of total public school revenues, respectively, with federal spending contributing the final 9%.[12] State and federal transfers are generally redistributive in nature. At the state level, movements to reduce inequality in district resources gained traction in the 1970s and accelerated after a series of court cases in the 1990s.[13]

Property taxes are the dominant source of local revenue. Our empirical analysis focuses on districts with independent taxing authority, i.e. those with the power to levy taxes in order to fund local schools. Mechanisms for selecting property tax rates vary by jurisdiction. Annual budgets, with associated tax rates, are proposed and administered by district officials, and, in some cases, must be approved by voters. District officials have varying levels of accountability to their residents; superintendents and schoolboard members may be directly elected, appointed

[12] It is important to note that the distinction between state and local revenues is not always clear, due to the complexities of state revenue-sharing policies. Hoxby (1996) highlights the importance of distinguishing between the entity that collects revenue – an accounting concept – and the entity that decides how to spend it. For example, California has a system in which school districts collect taxes locally even though revenue rules are determined almost entirely by the state.
[13] Hoxby (2001), Jackson, Johnson, and Persico (2016) and Lafortune, Rothstein, and Schanzenbach (2018) provide analyses and more detailed overviews of these reforms.

by other political officers, or a mix of both. In certain cases, citizens may directly vote on school spending measures (Cellini, Ferreira, and Rothstein 2010).

Moreover, property tax rates are usually applied to assessment values as opposed to market prices. The assessed value is simply an administrative valuation assigned to each property annually for property tax purposes. Each local government (normally a county) has a distinct set of rules to determine assessed values. While assessment values are generally correlated with house prices – and therefore the property tax is thought to be a progressive tax - recent research shows that such assessments may also vary within counties, with low income and minorities paying relatively higher taxes than non-minorities, just because their assessment houses were relatively higher (Avenancio-Leon and Howard (2021), Berry (2021)). In the empirical work below we focus on the effect of changes in actual house prices – the real economic base – as opposed to the effect of changes in assessed values and/or property tax rates.[14]

Regardless of the variation in accountability measures and tax rules, households are free to "vote with their feet" by moving to another district if local tax and spending policies stray too far from the household's preferences. This intuition underlies the Tiebout (1956) model and the extensive literature which follows.[15] While these models vary in their description of the policy levers available to local governments, they almost uniformly treat house prices based on market-clearing conditions in the housing market – as opposed to allowing house prices to determine a part of education expenditures. And even if a district experiences an unexpected housing boom, tax rates would be costlessly adjusted, and the district would restore the initial allocation by a proportional reduction in the property tax rate.

However, given the education finance system discussed above, changing tax rates can be a costly process. School district administrators may also hesitate to change tax rules because

[14] Despite the growing literature about property taxes, panel data sets with annual changes in property tax rates, assessments, and many other rules, such as appeals, discounts for seniors and homeowners, etc. are still unavailable to researchers.

[15] Examples include Epple and Sieg (1999), Fernández and Rogerson (2001), Hilber and Mayer (2009), Epple Romano and Sieg (2012), and Calabrese, Epple, and Romano (2012). Note that in Tiebout's original model, districts use head taxes rather than property taxes to screen residents, but in practice, districts cannot use head taxes and instead raise most of their revenue from property taxes. Hamilton (1975) notes that local jurisdictions can still achieve efficient sorting and expenditure policies by combining property taxes with zoning. Lot size restrictions establish a minimum house price in each jurisdiction, mimicking the screening mechanism of Tiebout's head tax.

they may not be able to distinguish housing disease from other mechanisms that produce increases in prices and revenues, such as gentrification or local productivity shocks. If, after property tax choices are codified, the district learns that prices are actually higher, then revenues will exceed expectations and must be spent (in part because many states and districts have rules that prevent schools from keeping large amounts of rainy day funds). Since the policy variables were likely chosen to equate marginal costs with marginal benefits, the additional spending may induce costs that exceed its benefits. This inefficiency is the cost of housing disease.[16]

If unexpected increases in house prices are small and idiosyncratic, there would be no reason to worry about this mechanism. One of the most salient features of housing markets, however, are strong boom-and-bust cycles. Moreover, these large swings in prices are difficult to generate in models in which prices depend solely on fundamentals. Glaeser and Nathanson (2015) review models that allow prices to depart from fundamentals, for reasons such as uncertainty about long-run supply, limited rationality, search-and-matching frictions, and lapses in credit standards. Housing disease starts with these departures from competitive equilibrium prices. More precisely, we use the term to refer to the influence of unexpected price increases – i.e. those unrelated to local fundamentals like amenities and productivity – on school district revenues and expenditures.

Another issue involves the possible uses for the revenue windfall, independent of the level of efficiency in a given district. District officials, for example, could allocate the windfall to sources that benefit them personally, such as their own salaries. Diamond (2017) and Brueckner and Neumark (2014) provide evidence that local officials sometimes use their positions to extract rents in this manner. This effect is more likely if voters pay less attention to tax revenue increases that result from unexpected windfalls as opposed to politically salient increases in rates. We explicitly test for the presence of this type of rent-seeking in our empirical work. We also test if districts spend the additional revenues on instruction and/or capital projects.[17]

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[16] This idealistic setting assumes, of course, that districts were in equilibrium and spending the optimal level of revenues prior to a housing boom. But if district spending was inefficiently low prior to the housing shock (because of frictions such as state level regulations described in Cellini, Ferreira, and Rothstein (2010)), then housing disease could actually improve efficiency.

[17] Alternatively, district leaders could save the increased revenues and return them to voters in subsequent years via lower taxes. In some cases, however, districts may have explicit incentives to avoid this behavior, as unspent funds may crowd out future transfers.

To account for a full range of possible dynamic effects, our empirical specifications allow prices and expenditures to evolve flexibly over a period of five years following a housing boom. Before turning to our empirical specification though, the next section reviews the school and housing data.

III. Data

School District Data

Our primary data source for school district finances is the School District Finance Survey (often referred to as the F-33 survey), which the National Center for Education Statistics (NCES) has administered annually since 1995. The datasets report detailed revenue and expenditure categories for all school districts in the United States.[18] School district boundaries are not constant over time, as districts merge and split with some regularity. We contacted all state education agencies to request details of the history of district boundary changes. Ultimately we received this information from 36 states, allowing us to create constant-boundary district definitions for most of our sample. We restrict our final analysis sample to districts that have independent taxing authority, "unified" districts that include both elementary and high school students, and districts that never merged or split during that time period. However, we also show that our results are robust to relaxing these restrictions.[19]

We supplement the revenue and expenditure data with demographic and staffing information from the District and School Universe Surveys, part of the NCES' Common Core of Data. These datasets provide a several useful descriptors for our analysis. First, they report the racial background of enrolled students and the number of students eligible for free or reduced-price lunches. These measures allow us to check whether changes in local housing prices might reflect changes in the composition of local students or residents. The files also provide detailed staffing information, which we use to construct measures of average salaries and employment levels for various categories of workers.

[18] The survey also includes charter school operators, which we do not include in any part of our analysis.
[19] We use per-pupil expenditure and revenue measures throughout our analysis. One shortcoming of the NCES data is that it records "snapshot" enrollment as of October 1st of each schoolyear, which may not reflect district size as accurately as other measures, such as average daily attendance. We are unaware of an annual, national dataset that records districts' average daily attendance or a similar measure, however.

Finally, we obtained microdata from the National Assessment of Educational Progress (NAEP) to assess whether changes in spending translated into short-term changes in student achievement. We make use of the State NAEP sample, which contains scores from a national, consistently-normed test administered biannually to a randomly selected subset of students in participating states.[20] We average student scores to the district-year-test level to construct a summary measure of student performance. More precisely, we use NAEP's reported "plausible values" in lieu of raw test scores, which are not included in the microdata. See Lafortune, Rothstein, and Schanzenbach (2018) and Jacob and Rothstein (2016) for useful discussions of the possible biases that may arise when using model-derived measures of student ability in external analyses.[21] Another limitation of the NAEP is limited coverage in early parts of the sample. Between 1996 and 2002, each biennial testing cycle offered only math or reading – never both. Furthermore, participation was optional, and between 41 and 45 states participated in each year during this period. Participation has been mandatory since 2002, however, and the change in sample composition likely explains the sudden change in math scores apparent in Appendix Figure 1A.

Housing Transactions Data

Our house price data come from CoreLogic, a private data vendor that aggregates public deeds records from county recorder's offices in markets across the country. Houses are pre-assigned to their Census block group, which we then match to school district boundaries using Census block relationship files.

We focus attention on districts with sufficient data to at least calculate a continuous quarterly price series between 2000:Q1 and 2007:Q1 (we use data from outside of this time period when it is available).[22] The resulting dataset includes 2,785 school districts and over 28 million transactions. To eliminate bias from specification search (Leamer 1983), we randomly split the sample in half and compute constant-quality hedonic price indices for each sample

[20] We are grateful to Julien Lafortune for providing code to link the NAEP microdata to NCES district identifiers.
[21] Fortunately, our results are virtually identical when using NAEP plausible values, ability measures estimated from item-response models, or residualized versions of these measures that control for individual student demographics, suggesting that such biases are not likely to be an important factor in our results.
[22]Specifically, we only include districts that report at least 16 observations in all quarters during this period, though we also include periods outside of this window

independently.[23] One sample is used to identify and test for the existence of structural breaks, and the other is used to estimate how prices change in the periods surrounding the break.

Figure 2A plots the 90th, 50th, and 10th percentiles of the resulting district-level price indices. The boom period of the recent cycle is apparent at each part in the distribution, with the median price increasing by 98% during that period. Figure 2B plots annual growth rates of the same series. To remove the effects of seasonality in the housing market, we calculate growth rates as year-over-year changes in the quarterly series, i.e. $(P_t-P_{t-4})/P_{t-4}$. While the national housing bust starting in early 2005 is immediately apparent, there is no visual evidence of a sudden break during the previous boom period. This fact is essential to our identification strategy. While most markets experienced a sudden onset of rapid growth, there is considerable cross-sectional variation in the timing of the booms.

Sample Restrictions and Representativeness

Table 1 reports some basic summary statistics and demonstrates how the sample composition changes as we add restrictions. The first column reports summary statistics for the entire sample of school districts in the F-33 dataset. Moving to the right, we add restrictions one by one until arriving at our main regression sample in column (5). The final column summarizes data for districts in the regression sample that we are able to match to test score data.

The most stringent sample restriction is the availability of historical housing transactions data. While the CoreLogic sample covers more than 90% of U.S. counties in 2016, we require sufficient transaction volume to estimate quarterly price indices starting no later than the year 2000. Hence, the merge to the housing sample immediately reduces our sample by 80%. Unsurprisingly, the districts that survive the merge to the housing data tend to be larger than the national average; enrollment in the breakpoint sample (10,221 students per district) is nearly three times that of the average district (3,459), corresponding to almost 60% of the total

[23] We estimate hedonic models because their data requirements are much less stringent than repeat-sales methods, particularly when working with small geographies. In practice, hedonic and repeat-sales estimates are very similar when both are computationally feasible. We construct our hedonic indices by regressing log prices on the square footage of the home (and its square), the number of bedrooms, the number of bathrooms, the age of the home, and an indicator for condominiums. Ferreira and Gyourko (2021) and DeFusco et al (2018) show that this model closely approximates the Case-Shiller index when estimated at the MSA level.

enrollment in public schools. These districts also have larger minority populations, higher student teacher ratios, and smaller portions of the population eligible for free or reduced-price lunch, an indicator of family income. Somewhat reassuringly, revenue per pupil is similar in the housing sample ($11,047/student) as in the overall sample ($11,158/student).

Columns (3) through (5) show the effects of restricting the sample to unified districts only (as opposed to districts specific to elementary schools or high schools); districts with independent taxing authority; and districts with constant borders and no missing financial data over our sample period. Enrollment, average revenue, student teacher ratios, and average demographics are largely unaffected by these restrictions. Our favorite sample is based on Column 5, and it represents 42% of all public school students.

IV. Empirical Framework and Validity of Research Design

Identifying Structural Breaks

This paper estimates the existence and timing of local booms at the school district level based on whether and when there was a structural break in each area's price appreciation rate series. This strategy is motivated by the dynamic urban spatial equilibrium model developed in Glaeser et al. (2014). Their framework implies that, in steady state, each local district will exhibit constant and continuous growth paths for house prices, new construction and population. Empirically, this suggests that house prices in a given area will grow at a (roughly) constant rate unless there is a shock to amenities or expectations, in which case we would then observe a discrete jump in the appreciation rate for that market. Informally, our approach defines the beginning of the housing boom in a local market as the point at which house price growth rates exhibit this type of sharp change.[24] In the discussion below we closely follow the breakpoint identification and inference methods described in Ferreira and Gyourko (2021) and DeFusco et al. (2018), and also provide extra details in the Online Appendix.

[24] This insight has led to a recent empirical literature exploiting these sharp changes to understand how changes in house values affect other economic variables (Ferreira and Gyourko (2021), DeFusco et al. (2018), Charles, Hurst, and Notowidigdo (2018)).

To formalize this idea, we start with the following reduced form model of house price growth in school district i at time t:

$$(1)\ PG_{i,t} = d_{i,t} + \epsilon_{i,t}, \qquad t = 1, \dots, T,$$

where $PG_{i,t}$ represents year-over-year price growth in district i measured in quarter t. Glaeser et al. (2014) implies that $d_{i,t} = d_{i,0}$ for all t if the district is on its steady-state growth path. However, if there is a positive shock at time t then the price growth rate will exhibit a discrete jump in that period. The beginning of a local housing boom can thus be identified by testing for the existence of one or more structural breaks in the parameter $d_{i,t}$. To carry out this test we follow established methods in the time series literature for estimating such breaks.

Borrowing heavily from Estrella's (2003) notation, the null hypothesis is that $d_{i,t}$ is constant for the entire sample period

$$(2)\ H_0: d_{i,t} = d_{i,0}, t = 1, \dots T.$$

The alternative is that $d_{i,t}$ changes at some proportion, $0 < \pi_i < 1$, of the sample which marks the beginning of a housing boom in market *i*. Specifically the alternative hypothesis is

$$(3)\ H_1:\ d_{i,t} = \begin{cases} d_{i,1}(\pi_i), & t = 1, \dots, \pi_i T \\ d_{i,2}(\pi_i), & t = \pi_i T + 1, \dots T. \end{cases}$$

For any given π_i it is straightforward to carry out this hypothesis test. However, it is slightly more complicated when π_i is unknown and the determination of its value is the primary interest. Following Estrella (2003), the estimated break point is the value t^* from the set S_i that maximizes the likelihood ratio statistic from a test of H_1 against H_0. That is, for every $t \in S_i$ we construct the likelihood ratio statistic corresponding to a test of H_1 against H_0 for that value of t, and we take the t that produces the largest test-statistic as our estimated break point for school district *i*.

This procedure will select a candidate breakpoint regardless of whether the break is statistically significant. Because we select the break that maximizes the likelihood ratio, critical values for testing must be derived from the distribution of the supremum of the likelihood ratio statistic. Andrews (1993) and Bai (1997) derive exact formulas for this distribution, and Estrella (2003) describes numerical methods to calculate p-values efficiently. We follow Estrella (2003) to calculate exact p-values for the estimated break point in each school district in the sample. We use these p-values to determine if the breakpoint is statistically significant or if it is not statistically different from zero.

So far we described a method of estimating one breakpoint per school district, but in practice some districts may have two or more breaks. To fix this issue we also test for the existence of two breaks against the null hypothesis of only one, for all locations where we do find evidence of a statistically significant break point. To do so, we closely follow Bai (1999) and Bai and Perron (1998). We identify candidate breakpoints in the two-break model by looping over all possible pairs of breaks in a districts' price growth series. Importantly, we do not require that the original breakpoint is selected as a candidate breakpoint in the two-break model. When the two-break model is statistically significant, we discard the estimates from the one-break model and keep the estimates from the two-break model. Similarly, we also estimate and test for the significance of three breaks relative to two breaks.

Estimating the Magnitude of Price Changes around Structural Breaks

The breakpoint method above provides unbiased estimates of the timing of the breakpoints, but it may not provide an unbiased estimate of the magnitude of the change in price growth rates after a breakpoint. Under the null hypothesis that there is no break point, the estimate of price changes has a nonstandard distribution and OLS estimates of its magnitude will be upwardly biased in absolute value. This could lead to an increased chance of falsely concluding that the magnitude of the breakpoint is larger than the true value, and is a form of specification search bias arising from the fact that the same data is being used to estimate both the timing and the magnitude of the structural break (Leamer, 1983).

Several approaches for adjusting the estimate of the magnitude of the price changes at the structural break have been suggested and are typically based on simulations under the null hypothesis of no break point (Andrews, 1993; Hansen, 2000a,b). Our approach to correcting the estimates of $d_{i,t}$ follows the method used by Card, Mas, and Rothstein (2008) of randomly splitting the underlying micro data of housing transactions into two subsamples: One subsample for estimating the timing of the boom (dates of breakpoints), and the other subsample for estimating the magnitude of price changes around that time. The idea is that if the two subsamples are independent, then estimates of $d_{i,t}$ from the second sample, which was not used to estimate the location of the break point, will have a standard distribution even under the null hypothesis of no structural break in the first sample. In practice, we randomly split our sample of unique houses (within districts) in two subsamples and create a separate price growth series for each random subsample of houses. The first price series is used to estimate the timing of the boom following the method just discussed, while the second price series is used to analyze the magnitude of price changes following housing booms in neighboring markets.

With breakpoint dates in hand – and using the split-sample approach – we proceed with estimating the magnitude of price changes during the timeline of local housing booms. The framework in equation (4) below is a type of non-parametric event study model, where the estimated coefficients represent the average effect of a housing boom in a district, in a year relative to the beginning of the housing boom. We do not estimate a different effect size for each district. Instead, we only allow heterogeneity in price effects by type of breakpoint, i.e., booms, busts, and non-significant breaks. Denote by $Y_{i,t}$ the log of the house price index in district *i* and quarter *t*, $t^*_{i,b}$ the quarter of the *b*th breakpoint in a district, and B_i the number of breakpoints estimated for district *i*:

$$(4)\ \ Y_{i,t} = \alpha_i + \kappa_t + \sum_{b=1}^{B_i} \sum_{\substack{\rho=-6 \\ \rho\neq 0}}^{6} \theta_\rho \mathbf{1}\left[\left\lfloor \frac{t - t^*_{i,b}}{4} \right\rfloor + 1 = \rho\right] + \varepsilon_{i,t}$$

where α_i and κ_t are district and time fixed effects, respectively.[25] The term on the right-hand side of equation (4) with the multiple summation signs contains the primary variables of interest. We construct a series of “relative year” indicator variables which identify whether the current quarter occurs ρ years before or after a quarter in which a school district experienced a structural break in its house price growth series.[26] This model is more flexible than just estimating different price changes before and after the breakpoint, in the sense that it allows the estimation of average price changes for each year relative to the breakpoint date. In practice, this

[25] We also experiment with adding county fixed effects interacted with a time linear trend, in order to allow for district-specific time trends. Those results are shown in the Online Appendix Table 6 and are similar to our main estimates reported in the paper.
[26] The notation $\lfloor x \rfloor$ is used to represent the floor function, which is the largest integer smaller than x. We need to divide the number inside the bracket by 4 because the underlying data is by quarter, but we estimate the effects in terms of relative years.

parameterization allows for flexible dynamics in the magnitude of the break's effects. Each θ_ρ measures the average change in the outcome variable ρ years before or after the break, relative to the year immediately prior to the break (note that we omit the dummy variable for relative year zero, and we also truncate the relative year coefficients at -6 and +6, so that θ_6 represents the average effect on outcomes six or more years after the breakpoint relative to period zero). Negative values of ρ target the "effects" of future breaks, allowing us to test for the existence of pre-trends that might confound our research design. The other controls included in equation (4) guarantee that the average housing boom effects will be estimated net of calendar effects, school district fixed effects, and also net of other booms and busts that happened in the same school district.

The model estimates separate effects for positive breaks, non-significant breaks, and negative breaks, as we are primarily interested in understanding the effects of housing booms – i.e. positive structural breaks. Even though all empirical specifications will estimate the effect of housing busts, the validity of such estimates are less credible since many markets begin to decline at essentially the same time, complicating efforts to separate the effects of bust-induced price variation from the national macroeconomic downturn.

Breakpoint Results and Validity of Research Design

For illustrative purposes, each panel of Figure 3 plots price growth rates for a separate district, with estimated breakpoints marked in red. The top left panel shows an example of a school district with only one positive and statistically significant breakpoint, which we call a boom. The top right panel has a district with two statistically significant breaks. The bottom left panel has a district with two booms and one bust in the same district, and finally, the bottom right panel shows the example of a district with one break that is not statistically different from zero. Those examples make the obvious point that the number of breaks we detect depends both on severity of the change in trend as well as the level of idiosyncratic variance in the series.

The three panels of Figure 4 show the full distributions of breakpoint timing for positive breaks, negative breaks, and non-significant breaks. Crucially for our identification strategy, the positive breaks are well distributed between 1998 and 2005. Cross-sectional variation in the timing of housing booms allows us to separate shocks to the local housing market from national trends and changes to the macroeconomy. Negative breaks, on the other hand, are concentrated largely during the onsets of economic downturns in 2001 and 2006. Overall, the 1,725 district time series in our favorite regression sample produce 1,107 booms, 541 busts, and 405 non-significant breaks.

Figure 5A then shows that school district housing booms are not preceded by changes in total expenditures per pupil, pupil-teacher ratios, and mathematics and reading test scores. That is not a surprise given that quality of school amenities is not part of the list of causes of the housing boom. Figure 5B then turns to the demographic composition of school districts. First, there is no evidence of changes in racial composition around booms. Second, while it appears that use of free lunch is lower in the post-boom period, the magnitude of the change is quite small compared to the size of the price effect. To confirm that shifts in demographics are not driving our results, in the next section we report results from models that control for %white, %black, %Hispanic, and % free lunch as a robustness check. Their inclusion does not impact the estimation of the house price elasticity of expenditures per pupil.

Finally, while annual income data are not available at the school district level, Ferreira and Gyourko (2021) estimate how average personal incomes vary during MSA level housing booms using data from the Bureau of Economic Analysis (BEA). These estimates are available in Online Appendix Table 1, and show that personal incomes did not change before or after the housing boom, which is a known feature of that housing cycle. While all tests above corroborate our main assumptions about demographics, at the end of Section 5 we will also discuss other possible mechanisms through which booms could alter unobserved demographic composition.

V. Results

House Prices and School Expenditures

The first three columns of Table 2 report how house prices evolved before and after the start of a school district housing boom, bust, or non-significant breakpoint. Prior to beginning of the housing boom, house prices are flat for three years. Before relative year minus three there is a small reduction in prices - not an upward trend, which would be more worrisome for identification purposes.

More importantly, house prices jump 4.8% in the first year of a boom, and keep growing in the following years, reaching 20.1% above the baseline in relative year 5. Busts have a symmetric result with cumulative price reductions of 12.0% by relative year 5. Districts that did not boom or bust had negligible price increases.

Estimates for expenditures per pupil are shown in Columns 4, 5 and 6. Expenditures show no pre-trends, and start to creep up in the second year of a housing boom, become statistically significant in year 3, and reach a peak of 3.3% in relative year 4. Busts again have a mirrored pattern of reductions in expenditures. None of the estimates are significant for school districts with non-significant breaks.

Figure 6 plots the impact of local housing booms on prices and expenditures together. Both show no trends prior to the beginning of the boom. But while prices immediately respond to the beginning of a boom, expenditures respond with a lag – matching the institutional features of school district finances discussed in Section II. Finally, the size of the price effect is an order of magnitude higher than the expenditure effect.

Table 3 explores a number of robustness tests. Column 1 shows our preferred estimates again to facilitate comparisons. Column 2 includes the full sample of school districts in our data, prior to restricting the sample to independent unified school districts that never experienced a split or a merge and that possess a complete panel of finance data.[27] The path of the coefficients is similar, but the point estimates are about 20% smaller - which is not surprising given the non-consistent sample. Column 3 then excludes non-independent school districts from the full sample, and the resulting point estimates for expenditures per pupil become slightly larger. Column 4 trims outliers in our preferred sample by excluding districts with expenditure growth rates in the top or bottom 1% of the sample. These estimates are only slightly smaller for house prices and similar for expenditures per pupil.

[27] We have estimated all results in this paper using the full sample of districts that we match to our housing dataset, and our findings are unaffected. The expenditure and revenue coefficients decrease slightly, as one would expect when many districts without independent taxing authority are added to the sample.

Column 5 only uses the one-breakpoint model. Price and expenditure estimates increase by the same proportion. The intuition for this result is that such model does not control for a second or third break, and therefore the effects of multiple booms are loaded into the one break. Finally, Column 6 uses our original specification with the addition of school demographics. Estimates are practically unchanged, which corroborates the validity of the research design.

House Price Elasticity of Expenditures Per Pupil

In this section we back out the house price elasticity of expenditures per pupil. One complication is that it is difficult to pin down the precise lag structure for these elasticities given the heterogeneity in school finance structures in the United States. We therefore present results from two types of Wald estimator. One divides the point estimates of expenditures per pupil in time t by the price effect in time $t-1$ (the lagged price elasticity) and one that divides the expenditure coefficient by the price coefficient from the same period (the concurrent price elasticity). Standard errors are calculated via the delta method.

Columns 1, 2, and 3 of the first row of Table 4 shows the estimated lagged price elasticities for relative years 3, 4 and 5. The estimates are remarkably stable, ranging from 0.16 to 0.20. The last column shows the estimate for a specification that bunches relative years three through five, producing a weighted average elasticity of 0.18. The next row uses concurrent estimates as opposed to the lagged structure. These concurrent elasticities are slightly smaller, with a weighted average of 0.16.[28] Next, the table reports the elasticities for the busts, showing a number that is larger than the ones for the boom but imprecisely estimated (the pooled elasticity estimate is 0.34). One possible reason for the larger elasticity is that, as we mentioned before, the busts in our sample are bunched in the onset of recessions, and therefore those results might be confounded by other factors, such as drops in employment and wages, and by appeals to assessed values.

[28] Our estimated elasticities are somewhat smaller than existing estimates of the property-tax elasticity for cities and states. Lutz (2008) estimates a value of 0.4 using national and state level time series analysis, while Vlaicu and Whalley (2011) find a 0.74 elasticity for California cities using an instrumental variable constructed from housing supply constraints.

Next we investigate if there is heterogeneity in these elasticities. We first create indicators for districts that were below and above the median expenditure per pupil in 1996, and then fully interact them with the relative year dummies. We run these models for prices and expenditures and calculate elasticities that are reported in the last two rows of Table 4. The pattern of the point estimates show that school districts with above median initial expenditures per pupil have a larger elasticity than the below median districts. However, those estimates are not statistically different from each other.

These results match a couple of important features of the American school finance system: school districts receive a large fraction of their revenues from state and federal transfers, and those transfers are disproportionally more relevant to low expenditure districts. In this setting, average elasticities should be relatively small, and high expenditure districts should have higher elasticities.

We can also use our results to back out by how much housing disease impacted the rise in public education spending in the United States during the 1990s and 2000s. We first use the model estimates from equation 4 to predict baseline expenditures per pupil for each school district in our sample. Next, we calculate another counterfactual prediction by turning off the estimated coefficients for the housing booms, i.e., we set to zero all θ^{ρ} estimates for the positive breakpoints (while negative and not-statistically significant breaks remain as part of the prediction). Comparing baseline with counterfactual predictions reveal that, on average, local housing booms explain 20% (19%) of the median (average) growth in school expenditures per pupil in the 1990s and 2000s. By construction local housing booms had no effect on expenditures for districts who never experienced a boom. These counterfactual estimates show that housing disease was an important determinant of school expenditures during the last housing cycle.

U.S. Municipalities

School districts may not be the only jurisdictions that suffer the effects of housing disease. States, counties, cities, and other special districts also rely on housing markets to raise funds. In this subsection we estimate the house price elasticity of expenditures per citizen for municipalities. We focus on these jurisdictions because the large number of U.S. cities allows us to carry out a similar empirical exercise. There are more than 35,000 cities in the United States, but we focus on cities with more than 20,000 residents because of the better data coverage.[29] Of this subset of cities, 1,528 have enough housing data to conduct the breakpoint analysis.

With the timeline of municipal housing booms in hand, we proceed with estimating Equation 4 for both house prices and expenditures per citizen. Figure 7 plots those point estimates. The pattern for price and expenditure changes is remarkably close to the price dynamics reported for school districts in Figure 6. Table 5 then reports elasticities for municipalities. Our preferred specification that pools years 3 to 5 shows a lagged price elasticity of 0.18, which is again similar to the number estimated for school districts. Elasticities for the negative breaks are noisier, presumably because of the smaller number of cities.

Housing Tax Base and School Expenditures

Thus far we have measured the effects of structural breaks in housing markets in logarithms, which facilitate the interpretation of relative magnitudes and the calculation of elasticities. Estimating the relationship in levels allows for a slightly different interpretation. Specifically, if we can obtain the causal relationship between the dollar value of all homes in the district (i.e. the residential property tax base) and total district expenditures, we can interpret the estimated magnitude as the portion of an additional dollar in housing tax base that is spent on schools.

To measure the local tax base[30], we need to measure both housing quantities and prices. We first obtain the total number of housing units in each school district in the Census years 2000 and 2010 by aggregating block level data from the Census SF1 files. For the earlier and intervening years, we interpolate linearly. We then multiply these measures with average house

[29] Annual fiscal data for municipalities come from two sources: Census of Governments and Annual Survey of State & Local Government Finances.

[30] The property tax literature commonly refers to the total value of housing in a locality as "housing wealth." In our context, we prefer the term "residential tax base" for two reasons. First, an increase in the price of housing need not increase wealth; for permanent residents, the increase in asset values is precisely offset by the increase in prices. Second, "wealth effects" have a different meaning in economics, and referring to the tax base preempts confusion.

prices recorded in the CoreLogic data to obtain a rough estimate of the value of each district's housing stock.[31]

After constructing the tax base measure, we turn to estimating its effect on expenditures and revenues, using Equation 4. Estimates of the separate effects on the tax base and total expenditures are reported in Online Appendix Table 4. With these in hand, we apply the same Wald estimators used for the elasticities and report the results in Table 6. The last column provides the concurrent and lagged estimates after pooling years three to five. Both estimates are similar, indicating that $100 dollars of additional housing tax base induced by a boom generates an additional dollar of school spending. Single-year estimates range from 0.75% to 1.20%.

We interpret these coefficients as reflecting of the marginal effective property tax rate used to raise funds for the school districts. Note that the effective rate is different than the statutory property tax rate, as the former will incorporate the influence of state transfer schemes, collections from other jurisdictions, and frictions in the re-assessment process.[32] Systematic data on property tax rates is generally not available, but our estimated magnitude is close to the national median inferred from self-reports in the American Community Survey (Harris and Moore 2013). The overall consistency likely indicates that districts do not significantly change their tax rates in the short term in response to the change in the tax base.

School District Revenues

Even though the tax base estimates above indicate that districts are not dramatically changing tax rates after a boom begins, one caveat with our study is that adjustments in tax rates, assessed values, and other local rules and regulations are not observed in the data. If districts reduce tax rates after the start of a housing boom, then we underestimate the elasticity - but can still interpret the results as a combination of the direct price effect plus the indirect political effect of potential adjustments in tax rates. The school district revenue data do not help solving

[31] Note that we are making at least two assumptions that are unlikely to hold in practice. First, home construction is cyclic, not linear. Second, we only observe average prices for houses that transact, which may not be a representative sample of the local housing stock. Both of these assumptions are born of necessity, and any associated biases are at least ameliorated by district and time fixed affects.
[32] Strictly speaking, the rate reflects the amount of funds received by districts, which would differ from the funds provided by citizens in the presence of intergovernmental transfers.

this problem because of three issues: a) it only reports total revenues as opposed to a breakdown of tax base and tax rates; b) even the breakdown by local versus state or federal transfer is muddled because it is difficult to disentangle the role of the school district as tax collector versus who in fact has control of the tax resources (Hoxby 1996); c) the revenue data is noisier than the expenditure data because of reporting standards. For example, revenues for capital projects that invest (spend) resources for five or seven years are fully recorded in the first year of the project. A similar phenomenon occurs with private donations.

Further complications arise from state policies that either restrict districts' taxing ability or redistribute revenues. Such policies are quite common; see Hoxby (2001) and Jackson, Johnson, and Persico (2016), who carefully track court cases and state legislation to evaluate the impacts of state policy changes. We are primarily interested in how such policies might mediate housing disease, not the overall impact of these policies. Accordingly, we need only focus on aspects of state formulas that respond to *changes* in the local property tax base. Note that many common formula features, such as foundation formulas or equalization policies, are not directly affected by house price growth, so their impacts are therefore absorbed by district fixed effects.

Therefore, we focus attention on state policies that restrict the growth of local property taxes by placing explicit limits on property tax growth, either by capping growth in assessments or capping revenue growth directly. We draw our classifications from Hightower, Mitani, and Swanson (2010), who surveyed all 50 states and categorized funding formulas along various dimensions.[33] In light of these issues, Table 7 reports a robustness test with magnitude estimates for total revenues and for revenue subcategories (local, state, and federal) in states with and without property tax growth caps.[34] Total revenue per pupil follows a path that is not statistically different from the path observed for total expenditures per pupil. As one would expect, local revenues respond to housing booms in uncapped states only. When property tax increases are restricted, housing booms produce small and statistically insignificant effects. State revenues show the opposite pattern: zero effect in uncapped states and positive effects in capped states.

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[33] The list of states with property tax growth caps include Arkansas, California, Colorado, Illinois, Kentucky, Michigan, Ohio, Oregon, Texas, and West Virginia. We are omitting one formula characteristic that is likely relevant: district spending caps. While not directly tied to growth, when the constraint binds – as they frequently do, in practice – they eliminate the relationship between house prices and revenue.

[34] Breakdown by local, state, and federal revenues for the whole sample is shown in Online Appendix Table 9.

But again, we caution against drawing strong conclusions from the revenue data given the many measurement issues explained above.[35]

Type of School Expenditures and Quality

How are the additional resources arising from housing disease spent by school districts? Reported school expenditures are split into three main categories: current (corresponding to 84.7% of the total expenditures during the sample period), capital (10.5%) and others (4.8%).[36] Columns 1 and 2, of Table 8 report estimates for capital and current expenditures. Current expenditures increase by 1.7% in year 5, while capital expenditures have a much larger effect, increasing 12.6% above the baseline. Converting those number into dollars by multiplying the point estimate in year 5 by the baseline average in year 0, we find an almost perfect split in the allocation of extra dollars between current and capital projects: current expenditures increase by $163 while capital expenditures increase by $179.[37] The increase in capital expenditures could be explained in part by the fact that housing supply tends to increase after house price booms (DeFusco, Nathanson, and Zwick (2020), and that in turn could increase school enrollment and the need for more capital expenditures.[38]

Next we split current expenditure into its two key categories, instruction and services, and present the estimates in Columns 3 and 4.[39] About 2/3 of the additional current expenditures (by year 5) goes directly towards instruction, while the other 1/3 is allocated to services. Finally, in Columns 5, 6, and 7 we break down the service component into instruction, pupil, and administration.[40] Instruction and pupil services concentrate the majority of the extra resources, while administrative cost point estimates are very small and not statistically different from zero.

[35] In Online Appendix Table 7 we also show that estimated expenditure elasticities are larger in districts with a high share of local revenues. We also estimate local revenue elasticities and show that those elasticities are larger in districts with a higher share of local revenues.
[36] Other types of expenditure include interest on debt and payments to other governments or school systems.
[37] Such results do not seem to vary by subgroups of districts with high or low expenditures – see Online Appendix Table 5.
[38] We find that school district enrollment increases by 2% five years after the beginning of a housing boom.
[39] Instruction accounts for 60.9% of current expenditures most of which is teacher salaries and benefits (though instructional aides are also included in this category). Services are 33.8%. Examples of service employees include support, administrative, operations, transportation, and business staff.
[40] Instructional services are expenses related to instruction that do not involve interaction between students and teachers in the classroom; examples include staff training, curriculum development, and technological services.

Further we test if the additional instruction expenditures are allocated to the quantity of teachers – which reduces pupil-teacher ratios – or to raises in wages and benefits paid to the teachers. Estimates are shown in Table 9. We find a mix of both: large increases in average salaries and benefits in years four and five (4.6% and 5.1% respectively), and moderate decline in pupil-teacher ratios moderately after a boom (slightly less than 1%). We also report separate effects for instructional salaries, administrative salaries, and other salaries in Online Appendix Table 8 though our construction of these variables requires significant caveats.[41] These estimates corroborates the result that administrative costs are not increasing with housing disease. Ultimately housing disease increases the quality of school inputs by the combination of additional resources devoted to instruction and capital projects, while not expanding bureaucratic expenses.

Table 10 then presents independent point estimates for math and reading tests scores, for both 4th and 8th graders, in columns 1 through 4. Because the NAEP test is only administered every two years, we pool relative-year coefficients into groups of two. The estimates are noisy, in part because test scores are never available for 15% of our main regression sample and inconsistently available for other districts. But the main reason might be that, for the magnitude of expenditure changes observed in the data (3.3%), the potentially small effects on test scores would not be detectable with precision. Indeed, we see no significant effects on math scores in any specification. There is some evidence that reading scores increased for fourth graders, but not for 8th graders.

We are hesitant to over-interpret the 4th grade reading test score estimates for several reasons. First, the effects enter with a substantial lag. The estimates are driven by observations long after the boom, placing significant strain on our identification strategy. The extended lag also creates an unbalanced panel; we only observe five post-boom years for districts with positive breaks relatively early in the sample, which are observably different from the late-

Pupil support includes administrative, guidance, health, and logistical expenditures, such as counseling, speech therapy, and record maintenance. Administrative services include operations associated with the district office or the office of the school principal.

[41] We calculate average salaries by dividing total spending on salaries (obtained from the F-33 Finance file) by the number of employees (obtained from the Common Core of Data survey file). Unfortunately, these two datasets do not group employees into consistent categories, so we aggregate up to the broad groupings described here. Mapping the categories to a common definition nonetheless requires some guesswork. To reduce the influence of misclassification errors, we drop districts with fewer than ten employees in a given category, since errors in employee counts are most harmful in small samples.

breaking areas. Furthermore, it is noteworthy that we do not observe a similar increase in math scores. We are unaware of any reason to expect reading scores to respond more strongly to increased expenditures than math scores. In fact, generally speaking, math scores are more responsive to educational intervention than reading scores for school-age children (Fryer 2017). Finally, as explained in the data section, NAEP performance data is based on plausible value predictions of individual test scores, as opposed to the raw tests scores per se.

General Equilibrium Consequences

Our results assumed that sorting based on unobservables were not driving the estimates, and we corroborated this assumption by looking at how observed school demographics changed around the timeline of local housing booms. However, one could posit that housing booms may induce a higher share of high-income families or families with more school-age children to move to better school districts (or districts with higher expenditures per pupil) just because of those households' larger budget constraints. The mechanism is simple: households with higher unobserved willingness to pay for those school districts will win the bidding war for the limited supply of homes in those districts. Such effects, though, would have a minimal impact on the overall composition of households in a school district because only a small fraction of homeowners move every year. Moreover, new households likely have less influence in the local political decisions of school boards given that they are mostly newcomers. To the extent that new households do affect tax and spending decisions, one could consider these changes part of the housing disease effect. Notably, if housing disease consists of both direct price effects and any secondary effects on policy, our point estimates are still consistent, though they have a more reduced-form interpretation.[42]

Our discussion also implicitly assumes that housing wealth effects do not operate – that is, the increase in house prices does not cause households to demand higher local education expenditures. In theory wealth effects should not occur in our setting because housing consumption remains constant: homeowners would have to sell their current house to tap the new wealth, but the cost of buying a new similar home would completely offset the gains from the

[42] Those secondary effects could also include small changes in demographics and enrollment discussed in the validity of the research design, and/or changes in state revenues due to matching grants.

previous sale. Behavioral factors and refinancing activity could generate some type of wealth effect though, and the empirical literature has shown mixed results. While wealth effects may impact college enrollment and fertility decisions, as shown by Lovenheim (2011) and Lovenheim and Mumford (2013), other results in the real estate finance literature (e.g. Agarwal et al (forthcoming), DeFusco (2018)) estimate that such wealth effects may be small.

Finally, another general equilibrium consequence of housing disease depends on the degree of inefficiency of the new expenditure levels. High levels of inefficient spending should lead to lower future house prices and a reduction in expenditures. Those second order effects may happen with even longer lags though, making its estimation not suitable in our setting.

VI. Conclusion

Both housing prices and educational spending rose dramatically in the 1990s and 2000s. Traditional public finance theory views public school districts as a set of local jurisdictions that provide different degrees of school quality, and access to those benefits is capitalized into house prices. This paper shows that the reverse causal channel should not be ignored: house price increases lead to additional spending per pupil by increasing the local tax base, and local administrators have incentives to spend those extra funds. We refer to this phenomenon as housing disease, as the increase in expenditures comes from a housing market spillover rather than a political decision weighing the benefits of school spending against the costs of increased tax burdens.

The magnitude of the estimated effect can be substantial: we estimate house price elasticities of per-pupil expenditures of 0.16-0.20, implying that rising house prices can explain approximately 20% of the increase in per-pupil expenditures leading up to the Great Recession. Although housing disease is a source of inefficiency in local finances, we find that the spending increases are concentrated on student instruction and capital projects, and not administrator salaries, suggesting that improvements in school quality may have accompanied the increase in school expenditures.

Our results also have important implications for hedonic valuation methods. As noted previously, the Tiebout (1956) assumptions are often invoked to justify regressing house prices on measures of local amenities to recover homeowners' valuations of these amenities (see Bayer, Ferreira and McMillan (2007) and Hilber (2007) for discussions). Our results point to a potential reverse causality issue when these methods are used to value local public goods. Future work should take care to assess and address this potential bias.

Even after the widespread growth of state and federal revenue sharing rules in the past decades, our results show that district finances are still influenced by local housing conditions. Since there is little reason to believe that housing cycles are disappearing, housing disease will remain a relevant feature of the American landscape. It may even grow in importance, as long as local communities have the power to constrain new housing development through zoning rules. Those regulations not only magnify the housing affordability problem in the United States, but also increase the cost of local services via housing disease. In fact, an interesting area of future work relates to how individuals within a district are bearing the incidence of housing disease, and how jurisdictions interested in reducing localities' exposure to price shocks should alter their taxing framework.

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Table 1: Sample Restrictions and Representativeness

	All Districts	Merged with Housing	Unified Districts Only	Indep. Districts Only	Final District Finance Sample	Test Score Sample
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Districts	13850	2785	2070	1748	1716	1465
Enrollment	3459	10221	12334	11750	11706	13200
Revenue Per Pupil	11158	11047	10929	10696	10725	10562
Student/Teacher ratio	14.37	16.84	16.61	17.11	17.06	17.07
Percent Black (K-4)	0.07	0.10	0.10	0.10	0.10	0.11
Percent Hispanic (K-4)	0.10	0.17	0.15	0.16	0.16	0.16
Percent Free-Lunch	0.27	0.20	0.20	0.21	0.21	0.21

Notes: All variables are reported for the year 2005. Restrictions are added cumulatively; hence each column is a subset of the column directly to its left. The district finance regression sample includes only districts with constant boundaries and no missing finance data during our sample period.

Table 2: Price and Expenditure Impacts of Housing Booms and Busts

	Log Price			Log Expenditure		
	Positive	Non-Sig.	Negative	Positive	Non-Sig.	Negative
	(1)	(2)	(3)	(4)	(5)	(6)
Relative Year <= -6	0.062***	-0.012	-0.120***	0.024**	-0.021*	-0.024*
	(0.013)	(0.018)	(0.017)	(0.010)	(0.011)	(0.012)
Relative Year = -5	0.037***	0.007	-0.100***	0.005	-0.022**	-0.027**
	(0.010)	(0.013)	(0.013)	(0.009)	(0.010)	(0.012)
Relative Year = -4	0.028***	0.001	-0.102***	0.000	-0.018**	-0.019*
	(0.008)	(0.010)	(0.011)	(0.008)	(0.009)	(0.011)
Relative Year = -3	0.017***	-0.003	-0.087***	0.002	-0.009	-0.012
	(0.006)	(0.008)	(0.009)	(0.007)	(0.008)	(0.010)
Relative Year = -2	0.007	-0.001	-0.063***	-0.000	-0.014**	-0.008
	(0.005)	(0.005)	(0.007)	(0.006)	(0.007)	(0.009)
Relative Year = -1	-0.005	-0.000	-0.037***	0.007	-0.010*	-0.001
	(0.004)	(0.003)	(0.005)	(0.005)	(0.005)	(0.007)
Relative Year = 0	0	0	0	0	0	0
Relative Year = 1	0.049***	0.003	-0.015***	0.005	-0.002	0.002
	(0.004)	(0.003)	(0.004)	(0.005)	(0.005)	(0.007)
Relative Year = 2	0.117***	0.006	-0.032***	0.013**	-0.001	-0.004
	(0.006)	(0.005)	(0.005)	(0.006)	(0.007)	(0.010)
Relative Year = 3	0.170***	0.008	-0.048***	0.025***	0.005	-0.016
	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)	(0.010)
Relative Year = 4	0.197***	0.004	-0.080***	0.036***	0.004	-0.011
	(0.008)	(0.008)	(0.011)	(0.008)	(0.009)	(0.011)
Relative Year = 5	0.202***	-0.007	-0.118***	0.032***	0.010	-0.024*
	(0.009)	(0.010)	(0.013)	(0.008)	(0.010)	(0.013)
Relative Year >= 6	0.257***	-0.031**	-0.184***	0.049***	0.027**	-0.012
	(0.012)	(0.013)	(0.015)	(0.009)	(0.011)	(0.013)
R-squared	0.860	0.860	0.860	0.798	0.798	0.798
Number of observations	88534	88534	88534	25740	25740	25740
Time FEs	X	X	X	X	X	X
Area FEs	X	X	X	X	X	X

Notes: Prices are estimated using quarterly data, while expenditures are only available annually. All models are based on equation (4). The sample includes all independent, unified districts with no missing finance data, constant borders, and sufficient housing data to calculate breakpoints (see text for the precise criterion). Standard errors allow for clustering at the district level. ***, **, and * reflect statistical significance at 1%, 5%, and 10% confidence, respectively.

Table 3: Robustness of Price and Expenditure Effects of Housing Booms

	Main Sample	All Districts	All Indep. Districts	Trimmed Sample	Single Break	Demog. Ctrls.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Effects on ln(Price)						
Relative Year = 1	0.048***	0.041***	0.046***	0.046***	0.061***	0.050***
	(0.004)	(0.003)	(0.004)	(0.005)	(0.003)	(0.004)
Relative Year = 2	0.116***	0.106***	0.115***	0.114***	0.142***	0.117***
	(0.006)	(0.004)	(0.005)	(0.006)	(0.005)	(0.006)
Relative Year = 3	0.168***	0.154***	0.163***	0.165***	0.210***	0.166***
	(0.007)	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)
Relative Year = 4	0.196***	0.174***	0.182***	0.192***	0.254***	0.193***
	(0.008)	(0.006)	(0.006)	(0.008)	(0.009)	(0.008)
Relative Year = 5	0.201***	0.172***	0.179***	0.196***	0.281***	0.198***
	(0.009)	(0.007)	(0.007)	(0.009)	(0.012)	(0.009)
R-squared	0.859	0.874	0.871	0.864	0.857	0.859
Number of observations	88,534	144,605	126,495	74,316	88,534	86,682
Panel B. Effects on ln(Expenditures Per Student)						
Relative Year = 1	0.004	0.004	0.006	0.002	0.007	0.004
	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Relative Year = 2	0.012*	0.009*	0.012**	0.015**	0.014**	0.012*
	(0.006)	(0.005)	(0.005)	(0.006)	(0.007)	(0.006)
Relative Year = 3	0.023***	0.013**	0.015***	0.024***	0.030***	0.023***
	(0.007)	(0.005)	(0.006)	(0.006)	(0.008)	(0.007)
Relative Year = 4	0.033***	0.022***	0.024***	0.032***	0.042***	0.033***
	(0.008)	(0.006)	(0.006)	(0.007)	(0.010)	(0.008)
Relative Year = 5	0.030***	0.017***	0.018***	0.029***	0.046***	0.030***
	(0.008)	(0.006)	(0.007)	(0.007)	(0.010)	(0.008)
R-squared	0.797	0.801	0.795	0.849	0.797	0.798
Number of observations	25,740	41,678	36,578	21,405	25,740	25,274

Notes: Column (1) reproduces the results in Table 2; see Table 2 notes for details of the sample and specification. Column (2) includes all districts with sufficient housing data to estimate breakpoints (see text for the precise criterion). Column (3) restricts this sample to districts with independent taxing authority. Column (4) imposes the other restrictions in our main regression sample and also removes districts whose annual revenue growth falls in the top or bottom one percent of observed values in our sample. Column (5) follows the main regression sample but uses breakpoint results calculated from a model that allows at most one break per district. Column (6) follows the main regression sample and specification but adds controls for the percentage of minority students and the percentage of students eligible for free lunch.

Table 4: Education Expenditure Elasticities of School District House Prices

	Relative Year 3	Relative Year 4	Relative Year 5	Pooled Yrs. 3-5
	(1)	(2)	(3)	(4)
All Positive Breaks				
Lagged Price Elasticity	0.18***	0.20***	0.16***	0.18***
	(0.05)	(0.04)	(0.04)	(0.04)
Concurrent Price Elasticity	0.14***	0.19***	0.17***	0.16***
	(0.04)	(0.04)	(0.04)	(0.04)
All Negative Breaks				
Lagged Price Elasticity	0.54*	0.21	0.33**	0.34*
	(0.33)	(0.19)	(0.15)	(0.19)
Heterogeneity in Lagged Price Elasticities				
High-Expenditure Districts	0.20*	0.21**	0.17**	0.20**
	(0.11)	(0.09)	(0.08)	(0.08)
Low-Expenditure Districts	0.16***	0.20***	0.16***	0.17***
	(0.06)	(0.05)	(0.05)	(0.05)

Notes: Elasticities are the ratio of coefficients on log expenditures and log price. Lagged price elasticities divide expenditure coefficients by price coefficients from the previous year. Concurrent price elasticities divide expenditure and price coefficients from the same year. We collapse price data to the annual level to create a common estimation dataset and estimate models via seemingly unrelated regression to compute standard errors. Otherwise, the sample and specification follow the description in Table 2. High (low) expenditure districts are districts with per-student expenditures above (below) the sample median in 1996. The underlying regression results for these subsamples are reported in Online Appendix Table 2. Standard errors allow for clustering at the district level. ***, **, and * reflect statistical significance at 1%, 5%, and 10% confidence, respectively.

Table 5: Municipal Expenditure Elasticities of Local House Prices

	Relative Year 3	Relative Year 4	Relative Year 5	Pooled Yrs. 3-5
	(1)	(2)	(3)	(4)
All Positive Breaks				
Lagged Price Elasticity	0.15	0.19**	0.25***	0.18**
	(0.13)	(0.08)	(0.08)	(0.08)
Concurrent Price Elasticity	0.10	0.17**	0.25***	0.15**
	(0.08)	(0.07)	(0.08)	(0.06)
All Negative Breaks				
Lagged Price Elasticity	1.34**	0.53*	0.52***	0.64**
	(0.67)	(0.28)	(0.20)	(0.31)

Notes: These estimates show the ratio of coefficients on total expenditures and coefficients the total value of the districts' housing stocks. Hence, the reported effects can be interpreted as the increase in education spending resulting from a one-dollar increase in the value of the residential property tax base. See Online Appendix Table 3 for the underlying regression results. We measure the value of housing stocks by multiplying average transaction prices in the CoreLogic data by the number of housing units in each district. The latter are obtained from the 2000 and 2010 Censuses, and we interpolate linearly in other years. As in Table 4, lagged (concurrent) effects divide expenditure coefficients by tax base coefficients from the previous (same) year. Standard errors allow for clustering at the district level. ***, **, and * reflect statistical significance at 1%, 5%, and 10% confidence, respectively.

Table 6: Effects of Changes in the Residential Property Tax Base on Education Expenditures

	Relative Year 3	Relative Year 4	Relative Year 5	Pooled Yrs. 3-5
	(1)	(2)	(3)	(4)
All Positive Breaks				
Lagged Effect	0.0079***	0.0090***	0.0114***	0.0097***
	(0.0012)	(0.0016)	(0.0024)	(0.0016)
Concurrent Effect	0.0075***	0.0120***	0.0118***	0.0100***
	(0.0015)	(0.0030)	(0.0029)	(0.0022)

Notes: These estimates show the ratio of coefficients on total expenditures and coefficients the total value of the districts' housing stocks. Hence, the reported effects can be interpreted as the increase in education spending resulting from a one-dollar increase in the value of the residential property tax base. See Online Appendix Table 4 for the underlying regression results. We measure value of housing stocks by multiplying average transaction prices in the CoreLogic data by the number of housing units in each district. The latter are obtained from the 2000 and 2010 Censuses, and we interpolate linearly in other years Lagged price elasticities divide expenditure coefficients by price coefficients from the previous year. As in Table 4, lagged (concurrent) effects divide expenditure coefficients by tax base coefficients from the previous (same) year. We measure district the value of housing stocks by multiplying average transaction prices in the CoreLogic data by the number of housing units in each district. The latter are obtained from the 2000 and 2010 Censuses, and we interpolate linearly in other years. Standard errors allow for clustering at the district level. ***, **, and * reflect statistical significance at 1%, 5%, and 10% confidence, respectively.

Table 7: Effects on Total District Revenues and Revenue Sources

	Full Sample	No Property Tax Growth Cap			Property Tax Growth Cap		
	Log Total Revenues	Log Local Revenues	Log State Revenues	Log Federal Revenues	Log Local Revenues	Log State Revenues	Log Federal Revenues
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Relative Year = 1	0.007	-0.005	0.001	-0.008	0.014	0.018	0.024**
	(0.004)	(0.005)	(0.007)	(0.010)	(0.009)	(0.013)	(0.010)
Relative Year = 2	0.005	-0.000	-0.001	-0.026***	0.011	0.030**	0.025**
	(0.005)	(0.007)	(0.008)	(0.010)	(0.009)	(0.014)	(0.011)
Relative Year = 3	0.014***	0.020**	0.011	-0.036***	0.007	0.041***	0.002
	(0.005)	(0.009)	(0.010)	(0.012)	(0.010)	(0.014)	(0.016)
Relative Year = 4	0.025***	0.023**	0.010	-0.004	0.003	0.056***	0.014
	(0.005)	(0.010)	(0.012)	(0.014)	(0.012)	(0.015)	(0.012)
Relative Year = 5	0.012**	0.029***	-0.012	-0.004	-0.019	0.023	0.025
	(0.005)	(0.010)	(0.013)	(0.017)	(0.012)	(0.015)	(0.016)
R-squared	0.909	0.950	0.867	0.907	0.950	0.867	0.907
Number of observations	25,740	25,739	25,739	25,721	25,739	25,739	25,721
Time FEs	X	X	X	X	X	X	X
Area FEs	X	X	X	X	X	X	X

Notes: See notes to Table 2 for details of the sample and specification.

Table 8: Effects on Expenditure Subcategories

	Log Expenditure						
	Current	Capital	Current Instruction	Current Services	Service Pupil	Service Instructional	Service Administrative
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Relative Year = 1	0.003	0.004	0.006**	-0.001	-0.008	0.003	-0.005
	(0.002)	(0.036)	(0.002)	(0.003)	(0.006)	(0.008)	(0.004)
Relative Year = 2	0.010***	0.062	0.012***	0.008**	0.008	0.027**	-0.001
	(0.003)	(0.049)	(0.003)	(0.004)	(0.007)	(0.011)	(0.005)
Relative Year = 3	0.014***	0.116**	0.017***	0.012***	0.022***	0.029**	0.003
	(0.003)	(0.055)	(0.003)	(0.004)	(0.008)	(0.013)	(0.006)
Relative Year = 4	0.018***	0.175***	0.021***	0.016***	0.032***	0.045***	0.004
	(0.003)	(0.060)	(0.003)	(0.005)	(0.009)	(0.013)	(0.007)
Relative Year = 5	0.017***	0.126**	0.019***	0.016***	0.042***	0.035**	0.008
	(0.003)	(0.060)	(0.004)	(0.005)	(0.010)	(0.013)	(0.007)
R-squared	0.958	0.293	0.956	0.929	0.893	0.795	0.857
Number of observations	25,739	25,729	25,739	25,739	25,272	25,279	25,279
Mean Expenditure	9,614	1,420	5,950	3,306	538	389	725
Time FEs	X	X	X	X	X	X	X
Area FEs	X	X	X	X	X	X	X

Notes: See notes to Table 2 for details of the sample and specification.

Table 9: Effects on Wages, Benefits, and Teacher Employment

	Log Avg. Salary	Log Avg. Benefits	Log Pupil Tchr. Ratio
	(1)	(2)	(3)
Relative Year = 1	-0.013***	-0.014**	-0.001
	(0.004)	(0.005)	(0.002)
Relative Year = 2	-0.005	-0.002	-0.011
	(0.005)	(0.006)	(0.008)
Relative Year = 3	0.009	0.012	-0.009***
	(0.005)	(0.008)	(0.003)
Relative Year = 4	0.029***	0.037***	-0.007
	(0.006)	(0.009)	(0.005)
Relative Year = 5	0.046***	0.051***	-0.008**
	(0.007)	(0.011)	(0.004)
R-squared	0.793	0.866	0.831
Number of observations	24,178	24,178	24,864
Time FEs	X	X	X
Area FEs	X	X	X

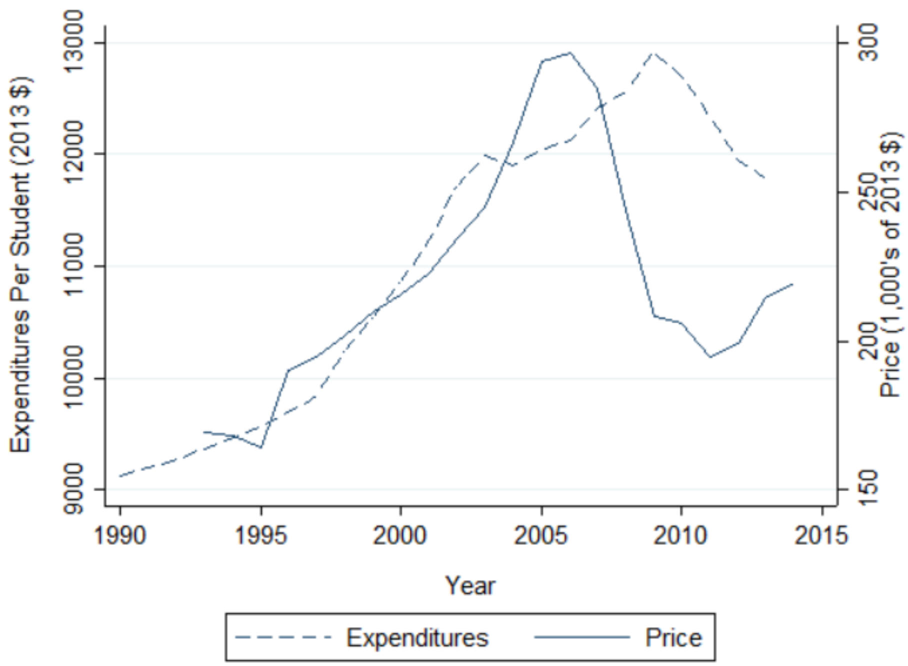
Notes: See notes to Table 2 for details of the sample and specification. All dependent variables are in logs.

Table 10: Effects on NAEP Test Scores

	Grade 4 Math	Grade 4 Reading	Grade 8 Math	Grade 8 Reading
	(1)	(2)	(3)	(4)
Relative Year in [1, 2]	0.0412	0.0322	-0.00268	0.0102
	(0.0383)	(0.0418)	(0.0338)	(0.0280)
Relative Year in [3, 4]	0.00658	0.0484	-0.00641	0.0247
	(0.0390)	(0.0453)	(0.0345)	(0.0305)
Relative Year in [5, 6]	0.0239	0.111**	0.0229	0.0558
	(0.0465)	(0.0537)	(0.0402)	(0.0346)
Observations	3,711	3,751	3,719	3,796
R-squared	0.816	0.762	0.829	0.783
Mean scores	241	224	282	260
Time FEs	X	X	X	X
Area FEs	X	X	X	X

Notes: See the notes to Table 2 for other details of the specification and sample restrictions.

Figure 1: House Prices and Expenditures Per Student for Median School District



Notes: Plot shows medians among school districts in our final regression sample (i.e. all independent, unified districts with no missing finance data, constant borders, and sufficient housing data to calculate breakpoints).

Figure 2A: School District House Price Indices

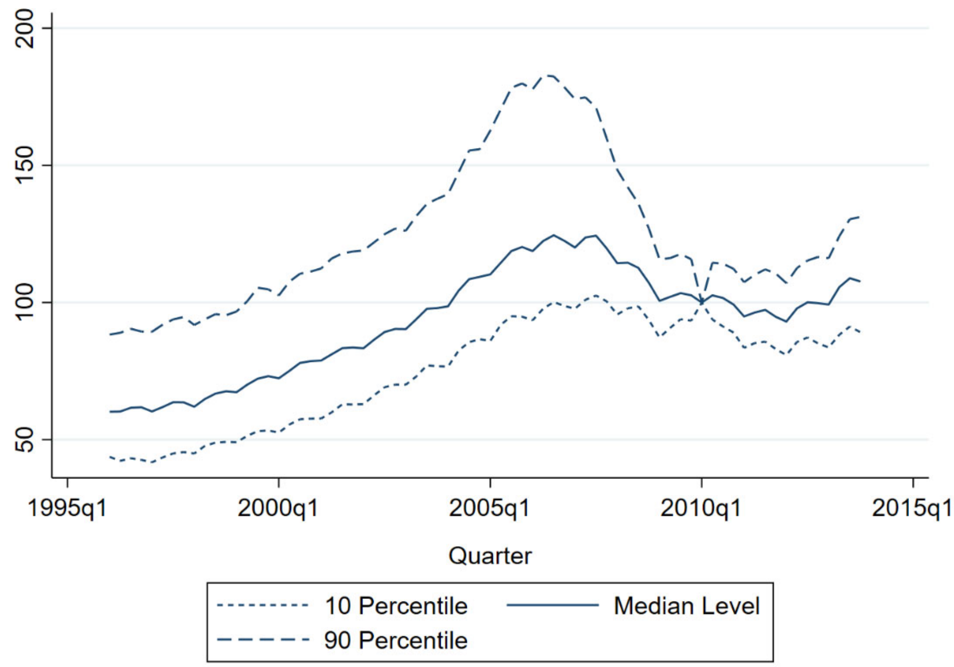
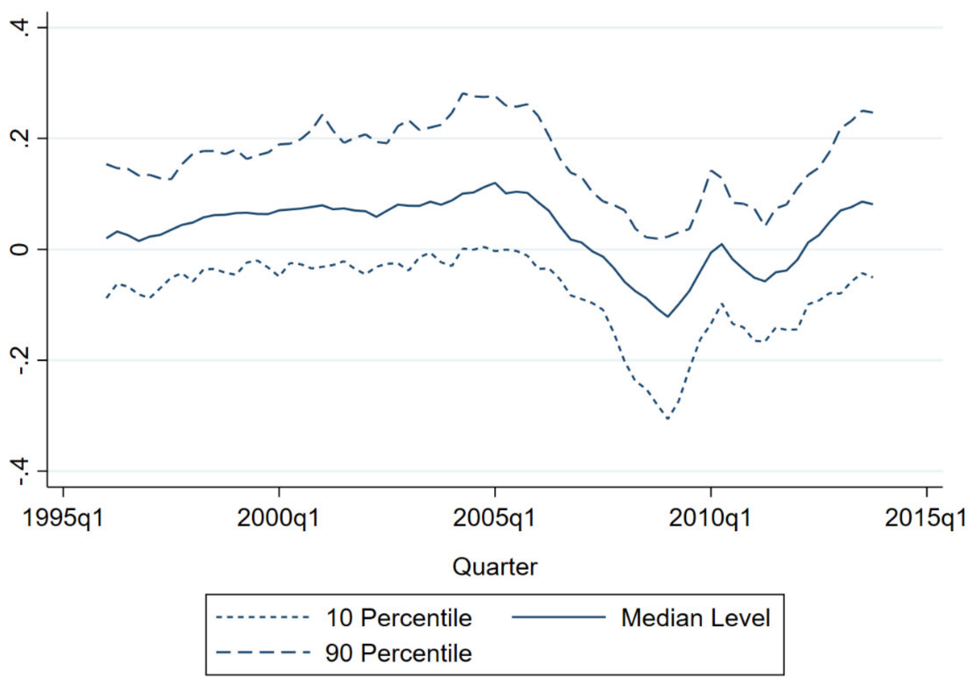


Figure 2B: School District House Price Index Growth Rates



Notes: Plots show percentiles among school districts in our final regression sample (i.e. all independent, unified districts with no missing finance data, constant borders, and sufficient housing data to calculate breakpoints).

Figure 3: Examples of Breakpoint Estimates

Figure 4: Timing of Structural Breaks in School District House Prices

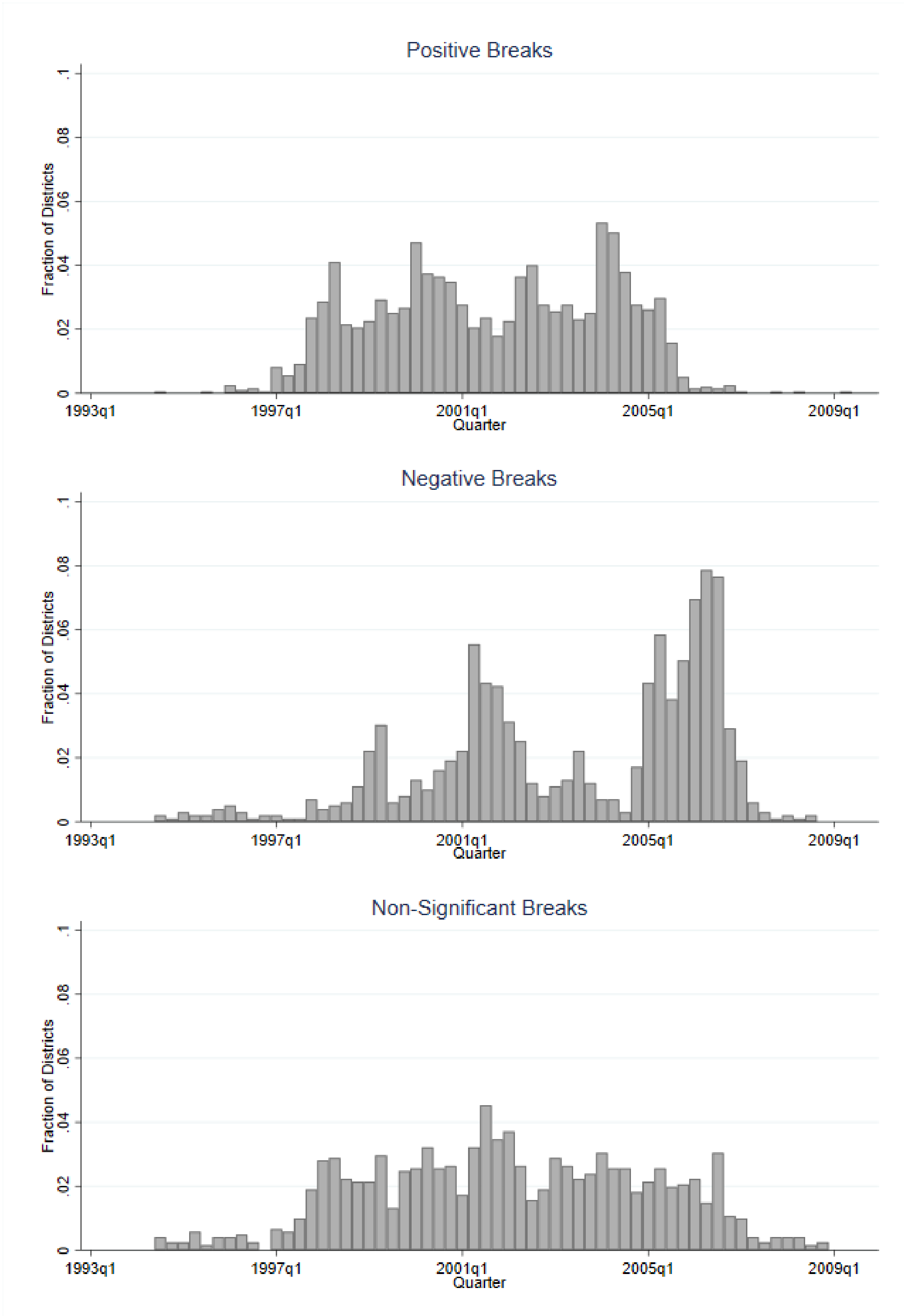
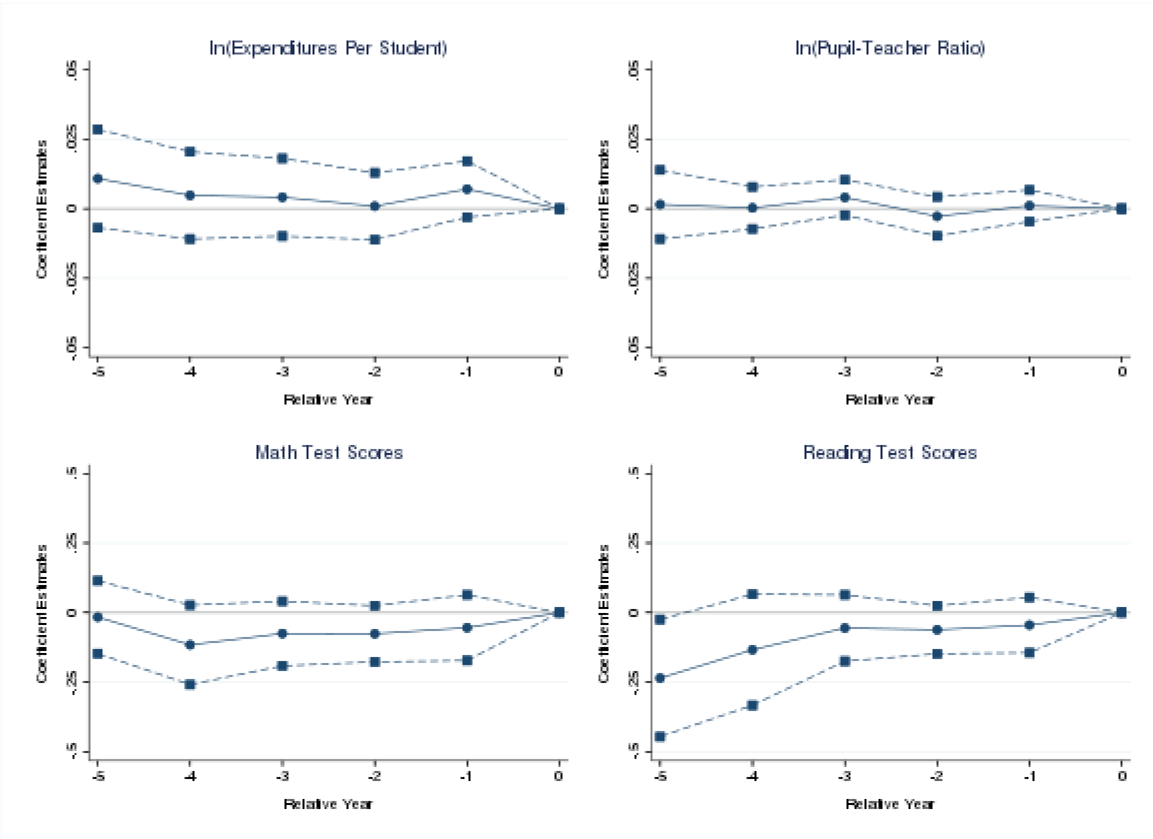
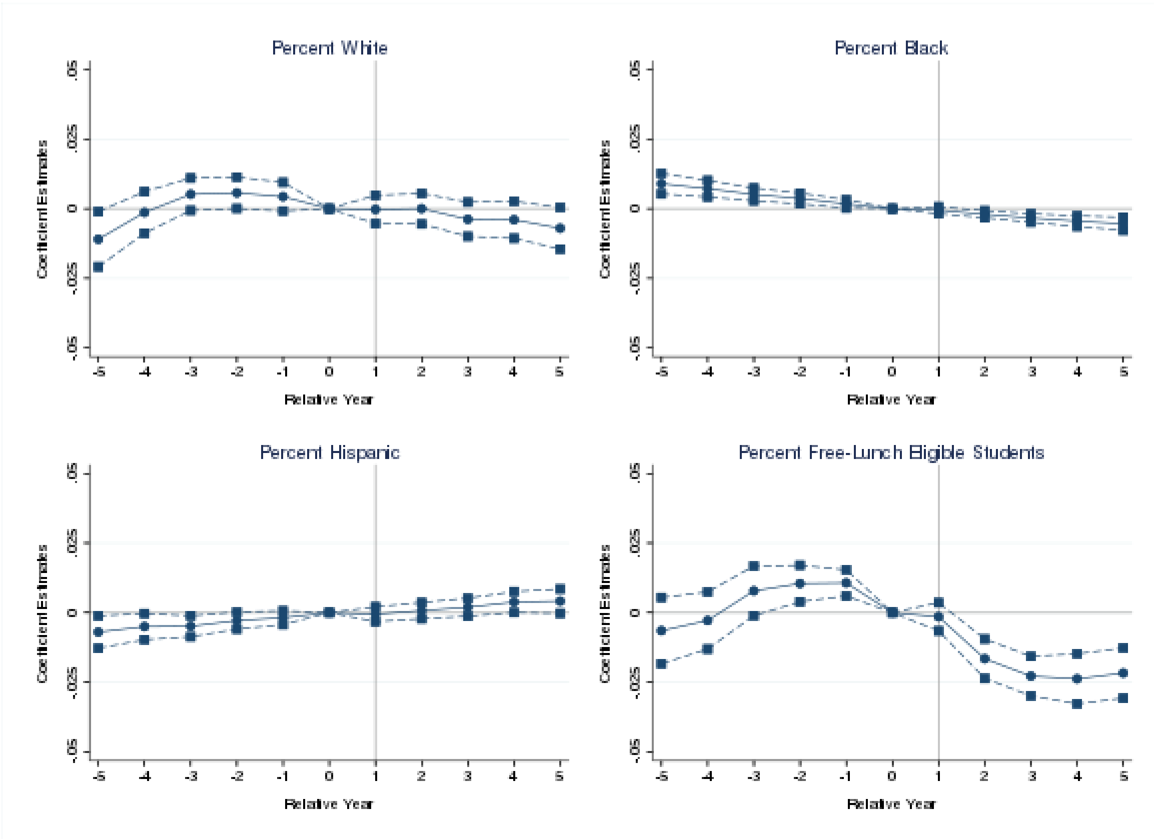


Figure 5A: Pre-Boom Trends in School Quality Proxies



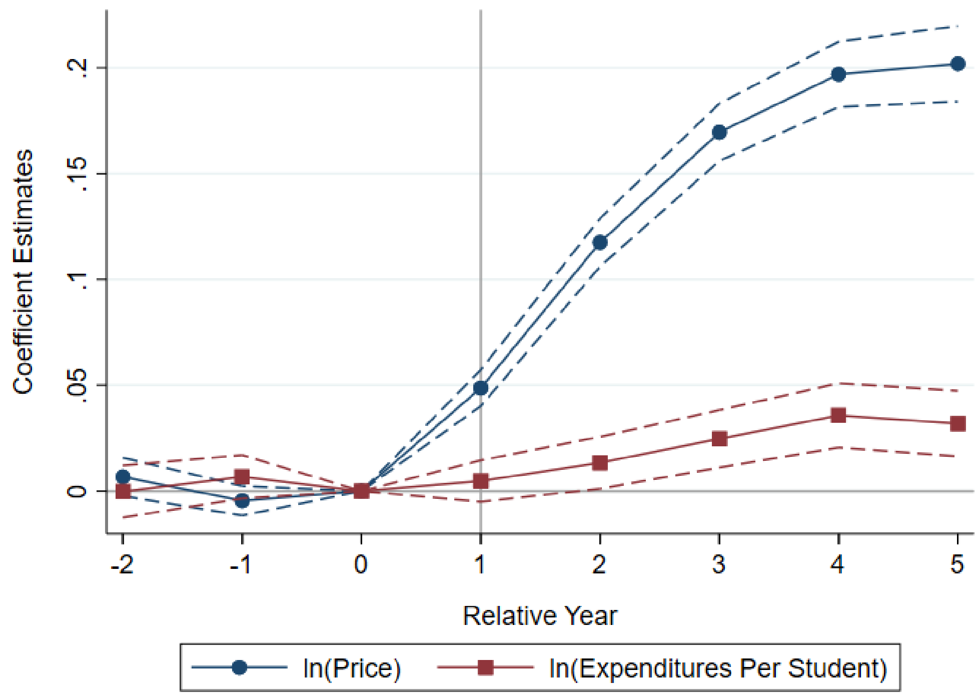
Notes: Plotted coefficients are from a model based on equation (4) – see Table 2 notes for more details. The sample includes all independent, unified districts with no missing finance data, constant borders, and sufficient housing data to calculate breakpoints. Dotted lines show 95% confidence intervals, with standard errors clustered at the district level.

Figure 5B: Demographic Shifts During Housing Booms



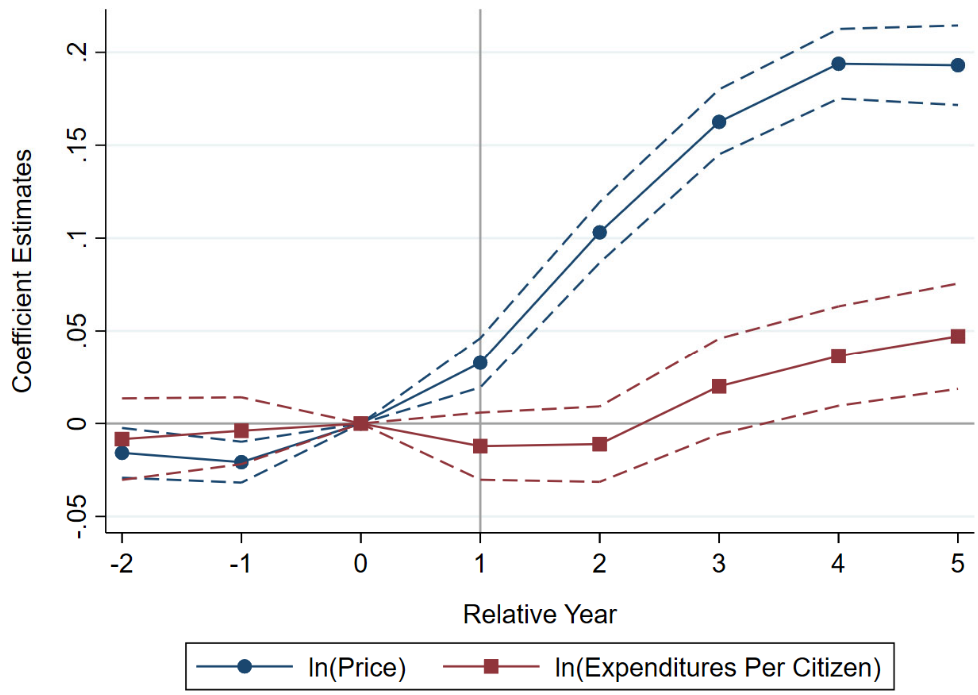
Notes: Plotted coefficients are from a model that follows equation (4) – see Table 2 notes for details. The sample includes all independent, unified districts with no missing finance data, constant borders, and sufficient housing data to calculate breakpoints. Dotted lines show 95% confidence intervals, with standard errors clustered at the district level.

Figure 6: The Effects of a Housing Boom on Prices and School District Expenditures



Notes: Plotted coefficients are based on the estimates from Table 2. The sample includes all independent, unified districts with no missing finance data, constant borders, and sufficient housing data to calculate breakpoints. Dotted lines show 95% confidence intervals, with standard errors clustered at the district level.

Figure 7: The Effects of Housing Booms on Municipal House Prices and Expenditures



Notes: Plotted coefficients are from a model based on equation (4) – see Table 2 notes for more details. Dotted lines show 95% confidence intervals, with standard errors clustered at the municipality level.