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School district quality and property values: Examining differences along school district boundaries

Paramita Dhar [a], Stephen L Ross [b,*]

[a] Department of Economics, Central Connecticut State University, Robert Vance Academic Center – 452, 1615 Stanley Street, New Britain, CT 06050, United States
[b] Department of Economics, University of Connecticut, 341 Mansfield Road, Storrs, CT 06269-1069, United States

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ABSTRACT

Examining differences across school district boundaries rather than school attendance zone boundaries has several advantages. These advantages include being applicable when attendance zones are not available or less relevant to educational outcomes as arises with within district school choice and for examining the effect of factors like school spending or property taxes that do not vary within districts. However, school district boundaries have often been in place for many years allowing households to sort based on school quality and potentially creating distinct neighborhoods on either side of boundaries. We estimate models of housing prices using repeated cross-sections of housing transactions near school district boundaries in Connecticut. These models exploit changes over time to control for across boundary differences in neighborhood quality. We find significant effects of test scores on property values, but those effects are notably smaller than both OLS and traditional boundary fixed effects estimates.

1. Introduction

Many papers have examined the empirical relationship between school quality and housing prices as an indication of what parents are willing to pay for good schools.[1] Black's (1999) important paper on this topic, however, raised and documented significant concerns with much of this literature. Specifically, she suggested that existing studies might be biased because omitted information on neighborhood quality likely correlates strongly with school quality. She addressed this concern by comparing housing prices on either side of school attendance zone boundaries since those houses are presumably located in the same neighborhood. She found that her estimates of the effect of school quality fell by between 30% and 40% when she controlled for neighborhood in this manner. Since Black (1999), the boundary approach has become the gold standard for studying school quality and housing prices.[2]

However, there are many circumstances under which differences in prices across attendance zones may not be available or may be unattractive for examining the effects of school quality on housing prices. First, in some states and school districts, attendance zones may not be available digitally and may require substantial effort to transform into a usable format, and records of historical attendance zones may simply no longer exist.[3] Similarly, some boundary studies in Europe have been conducted using Local Education Authority (LEA) boundaries (Fack and Grenet, 2010; Gibbons and Machin, 2004), which are closer in structure to a school district than an attendance zone. In addition, the increase in within district choice in many US states reduces the importance of attendance zones for determining actual school attendance and so reduces the importance of these boundaries. Further, when districts are growing, attendance zones may be subject to change in the future creating uncertainty and reducing the capitalization of current school quality into property values (Cheshire and Sheppard, 2004).[4] More permanent district boundaries are less subject to such

* Corresponding author.
E-mail addresses: paramita.dhar@ccsu.edu (P. Dhar), Stephen.L.Ross@uconn.edu (S.L Ross).

[1] Some recent examples include cross-sectional analyses by Bogart and Cromwell (1997), Cheshire and Sheppard (2004), Haurin and Brasington (1996), Hayes and Taylor (1997), Taylor (2005) and Weimer and Wolkoff (2001).

[2] Recent studies that use the boundary approach to test for the impact of school quality on housing prices include Brasington and Haurin (2006), Fack and Grenet (2010), Gibbons and Machin (2004), Gibbons et al. (2009), Kane et al. (2003, 2005), and Leech and Campos (2003).

[3] See Brasington and Haurin (2006) for a recent example in the US of an analysis of capitalization using school district boundaries.

[4] Housing values should include all future benefits from belonging to a specific attendance zone and uncertainty about membership will reduce the expected benefits. Cheshire and Sheppard (2004) provide a more complete discussion on pages F403-F404 and F419-421.

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uncertainty. Finally, issues related to school finance variables, like per pupil spending and property taxes, typically do not vary across attendance zone boundaries, and so the capitalization of these factors can only be examined using variation across districts.

While focusing on school district boundaries has several potential advantages, the use of district boundaries likely has a significant impact on a key maintained assumption of boundary approaches: houses on either side of a boundary are located in the same neighborhood. Long lasting boundaries create substantial motivation and time for owner–occupants to sort onto either side of the boundary based on their preferences and endowments. Over time, this type of sorting is likely to create substantial differences in neighborhood quality between locations that are relatively close in terms of distance. For example, even at the attendance zone level, Bayer et al. (2007) find that controlling for neighborhood demographics on either side of attendance zone boundaries lowers the effect of school quality on housing prices by 50% relative to models that just control for boundary fixed effects. They conclude that substantial neighborhood quality differences exist across attendance zone boundaries. Similarly, Dachis et al. (in press) examine the effect of the imposition of a land transfer tax on property values in Toronto using data along the metropolitan boundary and find large initial differences in property values on either side of the boundary.

Therefore, in making comparisons across school district boundaries, we need a mechanism to control for the differences in neighborhood quality across those boundaries. We conduct a boundary analysis using a panel of school district boundaries, use fixed effects associated with each side of a district boundary to control for differences in neighborhood quality across those boundaries, and exploit cross time variation in school district test scores to identify the effect of scores on property values. Specifically, we make across boundary comparisons of the change in school quality over time to the change in housing prices over time (difference-in-difference approach).[5] This strategy is very similar to the approached used by Dachis et al. (in press).[6]

This study uses repeated cross-sections of Connecticut housing transactions and a panel of school district data for the period 1994–2004.[7] We limit our sample by only keeping housing transactions that are close to the district boundaries, and our analysis controls for a standard list of housing attributes plus controls for broader housing market conditions. An additional concern in studies across school district boundaries is that other local fiscal variables or public policies may differ across school district boundaries, while such variables are typically constant across attendance zone boundaries. In order to address this concern, we control for both district spending on education and property tax rates.

We first conduct a standard cross-sectional style boundary analysis using our school district boundary sample, and compare these estimates to those arising from a model that only controls for standard neighborhood observables. Unlike Black (1999), we obtain large estimates for the effect of school quality on housing values, on the order of a 10% increase in prices for a one standard deviation change in test scores.[8] These boundary estimates are similar to estimates that arise when we do not control for school district boundary fixed effects. However, when we control for neighborhood differences across the boundary using additional fixed effects, the estimated effects in our baseline model and key extensions fall from between 8.2 and 10.0 percentage points in the OLS models to between 4.1 and 6.5 percentage points. These estimates represent a decline of between 21% and 59% relative to our traditional boundary estimates, which is comparable to Black's decline of 30–40%. Notably, the estimates associated with only controlling for boundary fixed effects are comparable to the OLS estimates at between 7.2 and 9.6 percentage points. The bias associated with neighborhood quality differences across school district boundaries is between 2.8 and 5.3 percentage points, which is larger than the 2 percentage point bias associated with quality differences across attendance zones in Bayer et al. (2007).

Our findings suggest that school district boundaries can be used to obtain estimates of the effect of test scores on property values that are comparable to estimates obtained using attendance zone boundaries. Our findings are consistent with earlier studies that find that estimates of test score capitalization are biased upwards by omitted neighborhood attributes even when using boundaries that are relatively permanent. Further, we find substantially larger bias from omitted neighborhood attributes across school district boundaries than have been found across attendance zone boundaries. Therefore, our findings suggest that boundary fixed effects are not sufficient to control for unobserved neighborhood attributes across school district boundaries, and our paper provides an alternative methodology that can be exploited when a panel of school districts is available.

The remainder of the paper is organized as follows: Section 2 develops the methodology, Section 3 describes the data and the empirical specifications, Section 4 presents the empirical results, and Section 5 summarizes and discusses the implications of the findings.

2. Methodology

The hedonic price equation with boundary fixed effects as described by Black is as follows:

$$\ln(P_{isb}) = \beta X_{isb} + \delta Z_s + v_b + \varepsilon_{isb} \quad (1)$$

where the dependent variable is the logarithm of the sales price of house i located in school attendance zone s on the boundary between b zone s and a second zone. Z_s is the vector of the school attributes specifically school quality as evaluated by average standardized test scores.[9] X_{isb} denotes the characteristics of houses sold along the boundary, and v_b is a boundary fixed effect that controls for neighborhood quality in a spatial area that encompasses housing on both sides of the boundary. For example, Fig. 1 contains three districts I–III with three boundaries between those districts. Boundary I–II will be affiliated with a single fixed effect, e.g. $v_{\text{I–II}}$,

[5] Several studies have used cross-time variation to control for neighborhood quality and other time invariant factors in assessing the effect of school quality on house prices, such as Bogart and Cromwell (2000), Clapp et al. (2008), Downes and Zabel (2002), and Reback (2005), but none have used cross-time variation in conjunction with a boundary analysis. Also see Clapp and Ross (2004) who examine the dynamic relationship between housing prices and school district quality.

[6] Admittedly, Dachis et al. (in press) have the added advantage of an apparently exogenous policy change on which to base their difference-in-difference analysis. Also see Brunner and Imazeki (2010), Dube et al. (2010), Holmes (1998), and Rohlin (2011) for examples of analyses in other contexts that are identified by examining variation both over time and across boundaries.

[7] Our time series in 2004 is interrupted due to a substantial change in the standardized tests used by the State of Connecticut.

[8] Very similar estimates arise when we estimate boundary models using samples that cover much sorter periods of time and contain no cross-time variation in test scores.

[9] Test scores are almost always used in analyses of school quality capitalization. Admittedly, test scores capture only a small part of the school environment experienced by students and potentially valued by parents, but test scores are readily observable by both parents and researchers and allow for comparisons to the previous literature. We know of one study that has examined the effect of school "value-added" contributions to tests scores on property values (Brasington and Haurin, 2006), but that study does not find any evidence that "value-added" affects values. Clapp et al. (2008) do find that student demographics affect property values, but they also find that the effect of demographics diminishes over time being in part replaced by increasing effects of test scores. Historically, prior to the availability of standardized test scores, many studies used school spending to capture quality (Ross and Yinger, 1999).

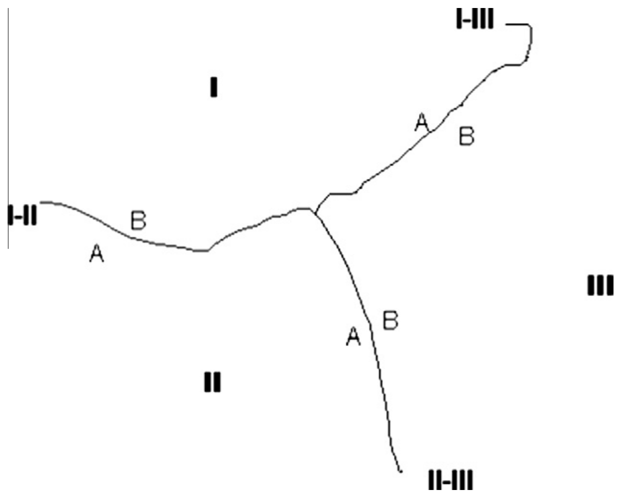

Fig. 1. Illustrative district boundaries.

and the observations along this boundary will contribute to the identification of δ based on the difference between Z_I and Z_{II}.

Next, we modify the traditional boundary analysis to analyze repeated cross-sections of housing transactions around school district boundaries and control for time invariant neighborhood quality fixed effects on either side of each boundary. Given that school district boundaries have often been in place for many decades, we anticipate that important, time invariant (at least in the short run) neighborhood quality differences would have developed over time between the neighborhoods on either side of a district boundary, and our boundary side fixed effects will capture these differences.

The model specification used for this purpose is

$$\ln(P_{isbt}) = \beta X_{isbt} + \delta Z_{st} + \sigma_{sb} + v_{bt} + \varepsilon_{isbt} \tag{2}$$

where all terms from equation (1) are the same except that they are now designated by transaction time t. The new term σ_{sb} is the fixed effect on the side of boundary b that is located in town s, which captures time invariant differences in neighborhood quality across the boundary. For our example in Fig. 1, the model will control for six fixed effects: one each for sides A and B of each of the three boundaries. For example, the boundary side fixed effect associated with transactions in district I on the boundary between I and III, side A, is designated $\sigma_{\text{II-III}}$. Finally, in order to limit the informational requirements of our model, we assume that v_{bt} follows a linear time trend so that $v_{bt} = \gamma_b t$ where any intercept in v_{bt} is subsumed into σ_{sb}. Note that the boundary side fixed effect also captures the time invariant aspects of school quality and property prices in school district s.

Therefore, the effect of schools on house prices is identified by comparing changes in school quality to changes in housing prices on either side of a district boundary. The estimation technique to be applied here is a difference-in-difference approach. The difference across boundaries is captured by the boundary side fixed effect σ_{sb}, which removes the level variation associated with the school district and neighborhood on each side of the boundary. The second effect, v_{bt}, captures boundary specific changes over time, and causes the parameters in our model to be identified by comparing changes on one side of the boundary to changes on the other side of the boundary. In this way, the estimation approach conducts across boundaries comparisons of differences in school district attributes over time to differences in housing prices over time. The maintained assumption is that the neighborhoods on either side of the boundary are fairly similar (but not the same) and share the same location so that they experience very similar changes over time.

Table 1
Data filters.

Factors for sample determination	Number of observations
Raw data set	430,318
Boundary dataset (<2500 ft)	126,523
Filter for house sales price <10,000	751
Filter for unit age >100 years	5225
Filter for missing bed, bath and internal square Footage, non-residential property	52,259
Final regression sample	68,288

Notes: The boundary dataset row is the size of the initial boundary sample and all following rows list the number of observations removed from the sample for various reasons. The dataset is a sample of housing transactions of one-to-four family homes in CT from 1994 to 2004. The dataset is proprietary and purchased by the University of Connecticut from Bankers and Tradesman.

3. Data description and empirical specifications

Sales price, the dependent variable in our analysis, is taken from a sample of home purchase transactions provided by Banker and Tradesman for the State of Connecticut.[10] It is a sample of sales of owner-occupied properties having one-to-four units from 1994 to 2004. The dataset contains detailed information about the unit address, sales price, sales date, and detailed unit characteristics, e.g. internal square footage, number of rooms, bedrooms, bathrooms, building age and lot size. The model also includes a large number of fixed effects for Labor Market Areas by year and month in order to control for differences in market circumstances. The latitude and longitude of each property is identified, and the property is geocoded to a specific town and census tract.

Our key measure of school quality is a 3 year moving average of the 8th grade mathematics test scores, which are standardized for the state by year. A 3 year moving average is used to minimize measurement error,[11] and the 8th grade test score is used to maintain uniformity across all houses in most towns since there is often only one middle school/junior high in Connecticut school districts. Another valuable feature of our data is that in Connecticut across school district choice is extremely limited and charter schools are few and far between so that the vast majority of public school students residing in a school district attend school in that district as well.[12] Finally, our sample contains several districts in which two small towns have combined to serve students from both towns in a single high school and a single middle school. These districts have common 8th grade test scores and per pupil student spending, but their own property tax rates and elementary schools. Again, all substantive results are robust to excluding these districts.

In addition, given that we are looking across school district boundaries, we must be concerned that property taxes and per pupil spending differ on either side of the boundary.[13] The property tax rate or "mill rate" is obtained for every town in each year from The State of Connecticut's Office of Policy Management. Then an equalized or effective mill rate is calculated which is the product of statutory mill rate and the average of the ratio of the assessed value to the sales price for all transactions in each year. The present

[10] This sample was also used by Clapp et al. (2008) for their district level study. They also examine the effect of demographic attributes, but this study focuses only on math test scores and local government variables to provide a specification that is comparable to Black and other boundary studies.

[11] See Clapp et al. (2008), Gibbons and Machin (2003), and Kane et al. (2003).

[12] In addition, over 90% of all students in Connecticut attend public schools based on the American Community Survey.

[13] Attendance zone studies implicitly control for local government by making comparisons within jurisdictions or school districts and so do not need to include controls for property tax or public spending. In Connecticut the towns and school districts share the same boundaries so the property tax measure for each school district is the town property tax.

Table 2
Number of boundaries in the transaction sample.

Number of boundaries with	>5 trans	>10 trans	>25 trans
2500 ft	218	171	114
1500 ft	167	133	84
1000 ft	134	107	63

Notes: The columns show number of boundaries with at least 5, 10 or 25 transactions within 2500, 1500, or 1000 ft of each side of a school district boundary during our sample period.

value of the property tax obligation is imputed using the following formula

$$PV = \log(r + m)$$

where r is the discount rate, which we set to 0.03, and m is the effective mill rate divided by 1000. The coefficient on this present value captures the capitalization rate of property tax differences.[14]

Table 1 provides the information on the various types of filters applied on the raw data to delete error prone, invalid, or anomalous observations or observations that are not relevant to our analysis. Observations are dropped that are not located within 2500 ft from a school district boundary. We also eliminate large multi-family properties (greater than four units) that are almost certainly not owner-occupied,[15] as well as properties that do not have any information on room, bathroom and internal square feet since in that case no information is available on which to base a comparison of housing prices across units. Where only some hedonic attributes are missing, dummy variables were created to allow for unique intercepts for observations with missing information.

Each house is associated with only one boundary. There are 218 district boundaries with at least five transactions on each side of the boundary. We also create two other datasets by limiting our sample to housing transactions within 1500 and 1000 ft of a boundary. Table 2 shows the number of boundaries where there are at least 5, 10, or 25 transactions on either side of the boundary illustrating that there are a significant number of boundaries with sufficient observations to conduct our analysis.[16]

Table 3 summarizes the data. The average house price over the period of study is $235,000 with a standard deviation of $322,000. It should be noted that the mean house price increases slightly when the sample is restricted to the boundary, but overall the three boundary samples are fairly similar. The mean of the standardized test score is negative for all the samples because the number of houses located in central city districts is larger than that of the suburbs and the former tend to have lower test scores. The mean of per pupil spending is approximately $8600 for both the full sample and the boundary samples. The boundary sample has similar graduation rates, but not surprisingly is more concentrated in large school districts with more densely populated boundaries. Therefore, the boundary sample is more concentrated on boundaries with central city school districts and along districts with higher shares of free lunch eligible and English as a second language students.

We estimate the OLS, boundary fixed effect, and boundary side fixed effect models for the pooled sample of all repeated cross-sections of transactions using moving averages of the school and town attributes. The dependent variable is the natural logarithm of house price for each housing transaction located along a school district boundary. All of our models control for three district/town attributes: the test score, the property taxes, and per capita student spending. All the specifications also include variables to capture the hedonic attributes, and year and month by Labor Market Area fixed effects. The OLS model also includes standard census tract characteristics to control for neighborhood quality. The standard errors are clustered at the tract level for the OLS and at the boundary level for all boundary models.

4. Results

Table 4 compares the results from a traditional hedonic regression (OLS) controlling for observed neighborhood quality to the results from regressions using a traditional boundary fixed effect model following Eq. (1) and our boundary side fixed effect model as described in Eq. (2). The first panel presents our baseline model. The resulting estimates of test scores on property values for the OLS regression are 0.082 or an 8.2 percentage point effect of a one standard deviation in test scores. The estimate actually rises to 0.093 with the inclusion of boundary fixed effects. On the other hand, the inclusion of boundary side fixed effects (our difference-in-difference approach) reduces the estimated effect of test scores to 0.065. The property tax estimates with boundary side fixed effects is −0.559% or 56% of the difference in property taxes are capitalized into property values compared to OLS and boundary fixed effects estimates of −0.655 and −0.312. The school expenditure effect estimates are 0.058 with boundary side fixed effects as opposed to 0.107 and 0.036 for the OLS and boundary fixed effect estimates.

The next three panels of Table 4 consider methodological variations on our baseline model in Panel A. First, we recognize Fack and Grenet's (2010) concern that boundary approaches can compare transactions that are far from each other even though the transactions are on the same boundary. Fack and Grenet (2010) address this concern by developing control groups for each transaction based on the inverse distance between the transaction and other transactions on the opposite side of the boundary. This approach is not viable using our difference-in-difference strategy. Instead, we developed weights for our regression based on Fack and Grenet's (2010) inverse distance strategy. Specifically, each observation on a boundary is weighted by the sum of the inverse distances to all transactions on the other side of the boundary. In this way, observations with the most proximate transactions to their comparison transactions on the other side of the boundary get the largest weight. Specifically, observation i on the kth side of the boundary between jurisdictions j and k (j–k) have the following weight

$$w_{ikj-k} = N_{kj-k} \sum_{s \in jj-k} 1/d_{is} \Big/ \sum_{i \in kj-k} \sum_{s \in jj-k} 1/d_{is} \tag{3}$$

where N_{kj-k} is the number of transactions on side k of the boundary, the index s is used to sum over all transactions on the other or j side of the boundary, and the index i is used to sum over all transactions on the k side of the boundary. The resulting estimates are 0.100, 0.072 and 0.041 for OLS, the boundary fixed effect and the boundary side fixed effect models. These estimates are also consistent with upward bias in the OLS and traditional boundary fixed effect models in a sample of school districts.

[14] See Yinger et al. (1988) for a derivation of this formula.

[15] See Carroll and Yinger (1994) for a discussion of the capitalization of school quality and property taxes into rents. Capitalization of these factors into the value of rental property operates simply through their effect on the discounted value of all future market rents, while capitalization for homeowners may be driven by a complex and non-linear combination of investment and consumption motives.

[16] One might be concerned about measurement error in housing prices given that our sample only contains between 10 and 25 observations on either side of many of the borders over our 11 year time frame. However, the reader should remember that housing prices are the dependent variable. Any undue variation on the left hand side of the equation caused by the small number of transactions will only influence the standard errors of our estimates (as opposed to attenuation bias from measurement error in a right hand side variable). In practice, the resulting estimates are reasonably precise even after the inclusion of a very flexible combination of fixed effects and so the number of transactions along the boundaries at any point in time is not a large issue.

Table 3
Summary statistics over all years.

	Full sample		2500 ft		1500 ft		1000 ft	
	Mean	S.D	Mean	S.D	Mean	S.D	Mean	S.D
Housing attributes								
Price (K)	235	322	254	539	257	383	259	348
Rooms	6.72	2.35	6.74	3	6.7	2.89	6.67	2.77
Bed	2.45	1.65	3.13	1.52	3.11	1.49	3.08	1.37
Bath	1.97	.881	1.78	.914	1.79	.935	1.78	.917
Age	42	24.1	43	23.7	42	23.5	42	23
Sq. ft. (K)	1.69	1.03	1.68	2.63	1.66	2.15	1.64	1.69
School district characteristics								
Test score	−.413	1.31	−.504	1.38	−.497	1.39	−.489	1.39
Taxes	−3.00	.207	−2.92	.218	−2.92	.218	−2.93	.216
Spending (K)	8.61	1.24	8.6	1.27	8.63	1.26	8.66	1.24
Graduation rate	91.07	9.05	90.47	9.77	90.59	9.72	90.7	9.63
Enrollment	6516	5716	7254	6051	7301	6146	7295	6169
Free lunch	23	23.47	25.41	25.34	25.49	25.75	25.24	25.85
Non-English	11.4	14.2	13.40	15.56	13.41	15.45	13.36	15.48
Percent central city	22.8		38		37.90		37.41	
Sample size	349,730		68,288		38,900		24,159	

Notes: The cells contain sample means and standard deviations for each subsample. Sales price, square feet, and per pupil spending are reported in 1000s. The test score is the 3 year moving average of the standardized 8th grade math score, spending is per pupil student spending in the school district, and property tax is reported as present value of the effective property tax burden based on a discount rate of 0.03 where effective property taxes are based on assessed value scaled by the district average of the ratio of assessed values to sales prices.

The results in the next two panels mirror the estimates above. Panel C reruns the baseline model replacing the moving average of 8th grade test scores with an alternative measure of school quality, the average of 4th, 6th and 8th grade test scores in the current year. Moving average test scores are often used to minimize noise by including the performance of multiple cohorts in the same school test score, but in a panel context one might worry that some of the effect of across time differences in school quality would be lost if test scores are averaged over time. The average across grades includes information on multiple cohorts without including information on multiple years. The resulting estimates are 0.085, 0.096 and 0.043 for OLS, the boundary fixed effect and the boundary side fixed effect models. Panel D reruns the baseline model replacing the linear boundary trend with a non-parametric trend for changes in neighborhood quality around the boundary. The model simply includes a vector of boundary-year fixed effects. The resulting estimates are 0.082, 0.093 and 0.050 for OLS, the boundary fixed effect and the boundary side fixed effect model.

Across these four models, the inclusion of fixed effects associated with each side of every boundary yields estimates between 4.1 and 6.5 percentage points as compared OLS estimates between 8.2 and 10.0 percentage points, a decline of between 21% and 59% as compared to Black's decline of 30–40%. Further, the estimates associated with only controlling for boundary fixed effects are comparable to the OLS estimates at between 7.2 and 9.6 percentage points. The bias associated with neighborhood quality differences across school district boundaries is between 2.8 and 5.3 percentage points, which is larger than the 2 percentage point bias associated with attendance zones in Bayer et al. (2007). The alternatives to our baseline model in Panels B-D yield somewhat smaller effects of test scores. However, the distance weighted model reduces the amount of information available, the averages across grades model does not smooth out temporary across year shocks, and the non-parametric trend model substantially reduces the information available for identification, which in each case might exacerbate downward bias from measurement error.

As with test scores, the pattern of estimates across models is the same across panels A through D for property taxes and school expenditures. The OLS estimates are larger and the boundary fixed effect estimates are smaller than the boundary side fixed effect (difference-in-difference) estimates. The difference-in-difference estimates fall in a relatively narrow range for property tax capitalization between −0.339 and −0.559, but in a wider range for per pupil expenditures of between 0.022 and 0.078.

Table 5 presents the results of the baseline boundary side fixed effect model for variable subsamples. The first panel shows the estimates when the sample of housing transactions is restricted only to houses very close to the boundary using threshold distances of 1500 and 1000 ft. The estimated coefficient on test scores rises substantially and estimates for property taxes and per pupil spending fall moderately when the sample is restricted to transactions within 1000 ft. However, all results are qualitatively robust across the three distance thresholds.[17]

The next panel of Table 5 presents the results for subsamples restricted to single family homes only, transactions located along suburban boundaries and transactions located along the boundaries between a central city and its suburbs. The results restricting the sample to single family homes are comparable to the baseline model estimates in Table 4, and not surprisingly the suburban subsample estimates are also similar. However, the capitalization of school quality appears larger and the capitalization of taxes and spending smaller along central city boundaries. These differences may help explain the differences with a 1000 foot threshold in panel A because this requirement restricts the sample primarily to more densely populated areas where there are a substantial number of transactions very near to boundaries. Further, given that central city transactions are overrepresented in our boundary sample, the estimates for our boundary sample are likely somewhat larger than the anticipated average effect for the full sample of transactions. Note this does not affect the comparisons in Table 4 because the OLS estimates are based on the same boundary sample.

Table 6 examines the differences between the effects of 4th grade test scores in elementary school and 8th grade test scores. Column 1 presents the results from Table 4 using 8th grade test scores, column 2 presents results using 4th grade test scores and column 3 presents the results including both scores simultaneously. The coefficient estimate on the 4th grade test score is

[17] While not presented, the qualitative pattern of relatively stable test score estimates between OLS and boundary fixed effect models and lower estimates for the boundary side fixed effect model arise for all models presented.

Table 4
School quality, tax and spending capitalization estimates.

	OLS	Boundary FE	Difference-in-difference
Panel A – baseline Model			
Test	.082**	.093**	.065**
	(.007)	(.011)	(.015)
Taxes	−.655**	−.312**	−.559**
	(.049)	(.081)	(.075)
Spending	.107**	.036**	.058**
	(.006)	(.007)	(.008)
Panel B – distance weights			
Test	.100**	.072**	.041**
	(.010)	(.010)	(.019)
Taxes	−.668**	−.363**	−.517**
	(.074)	(.067)	(.134)
Spending	.100**	.042**	.077**
	(.010)	(.009)	(.010)
Panel C – average test score			
Test	.085**	.096**	.043**
	(.007)	(.011)	(.015)
Taxes	−.559**	−.239**	−.408**
	(.050)	(.072)	(.064)
Spending	.060**	.014**	.022**
	(.009)	(.004)	(.007)
Panel D – non-parametric trend			
Test	.082**	.093**	.050**
	(.007)	(.011)	(.020)
Taxes	−.655**	−.312**	−.393**
	(.049)	(.081)	(.078)
Spending	.107**	.036**	.078**
	(.006)	(.007)	(.010)
Size	65,571	65,325	65,325

Notes: Estimates are based on the pooled sample of transactions between 1994 and 2004 within 2500 ft of a boundary. The dependent variable natural log of house price, $P_{ikb(jk)t}$ of house i in district k at the boundary of districts j and k at time t. The first row contains the 8th grade standardized math test score, and the next two rows contain property tax and per pupil spending, respectively. In order, the columns present the estimates from the OLS, boundary fixed effect, and difference-in-difference with boundary side fixed effects and boundary trends models. The first panel contains the estimates for our baseline model, the second panel weights transactions based on proximity to comparison transactions across boundaries, the third panel uses the average of 4th, 6th and 8th grade test scores instead of the moving average of 8th grade test scores, and the last panel uses a non-parametric trend for changes in boundary quality over time. All the specifications also include variables to capture the hedonic attributes, and year and month by Labor Market Area fixed effects. The OLS model also includes standard census tract characteristics to control for neighborhood quality. Adjusted standard errors are in the parenthesis with tract clustering for OLS and boundary clustering for fixed effect models.
** represents significance at the 0.05 level or better.

Table 5
Estimates for different subsamples.

	2500 ft	1500 ft	1000 ft
Panel A – different boundary distances			
Test scores	.065**	.053**	.082**
	(.015)	(.018)	(.023)
Taxes	−.559**	−.531**	−.474**
	(.075)	(.086)	(.114)
Spending	.058**	.054**	.049**
	(.008)	(.011)	(.014)
Sample Size	65,325	37,240	23,110
	Single family homes	Suburbs	Central city
Panel B – suburban and urban housing			
Test scores	.051**	.057**	.079**
	(016)	(.017)	(.031)
Taxes	−.452**	−.673**	−.480**
	(.068)	(.135)	(.100)
Spending	.061**	.078**	.037**
	(.009)	(.012)	(.013)
Sample size	58,374	39,574	25,751

Notes: Estimates are based on the pooled sample of transactions between 1994 and 2004. The dependent variable natural log of house price, $P_{ikb(jk)t}$ of house i in district k at the boundary of districts j and k at time t. The first row contains the standardized math test score, and the next two rows contain property tax and per pupil spending, respectively. The three columns in the first panel present the difference-in-difference estimates for three samples based on proximity to the school district boundary, and the three columns in the second panel present results for a sample restricted to single family homes, central city transactions, and suburban transactions. All the specifications also include standard controls and cluster standard errors by boundary.
** represents significance at the 0.05 level or better.

0.023 well below the 0.065 estimate for 8th grade test score. When both scores are included, the coefficient on 8th grade test score falls to 0.047 and the coefficient on 4th grade test score is smaller at 0.032. One might be tempted to conclude that test scores in later grades are more important for explaining property values, but these differences in magnitude might also be explained by the fact that most districts have several grade schools and so the district average of 4th grade test scores does not capture the quality of the specific schools to which properties provide access.

One final concern with identifying the effect of school quality using cross-time variations is that we might simply be testing for a relationship between housing prices and short-run year-to-year variation in district outcomes that is neither easily observed by homeowners nor of long-term significance to property values. If this concern is valid, models identified by cross-time variation might understate the impact of school quality and other school or district attributes on housing prices. We address this concern by dividing our data into three or two periods and comparing changes in property values across periods to changes in test scores, taxes and spending between those periods. The smoothed town/district variables from the middle year of each period are assigned to all transactions during a period so that the only variation in the data arises from changes over several years.[18] Specifically, we divide the data into three periods 1994–1996, 1998–2000 and 2002–2004 and assign moving averages from 1995, 1999 and 2003, respectively, to these periods. For two periods, we use transactions from 1994–1998 and 2000–2004 with moving averages from 1996 and 2002. All comparisons in these samples involve changes over 4 or 6 years. These periods were chosen to have equal length and to maximize the time between periods while still exploiting most of the data in the 1994–2004 sample.

Table 7 presents the sample means for each period. The sample exhibits a substantial increase in transaction volume over time with a growing share of transactions in suburban towns and a substantially increasing average home size. No systematic trend is observed on school district demographics. Table 8 investigates the effect of these changes by first in column 2 presenting estimates that are weighted to hold the composition across school districts constant over time and then in columns 3 and 4 by presenting estimates for the beginning (1994–1999) and end (2000–2004) of our sample. The overall change in composition across towns or school districts has little impact on our estimates, but over time we observe a substantial increase in the rate of capitalization of test scores (0.044 vs. 0.084), property taxes (−0.331 vs. −0.839) and per pupil spending (0.025 vs. 0.068). This finding is consistent with earlier findings of Clapp et al. (2008) that school quality capitalization increases over time in Connecticut during this period.

Finally, Table 9 presents our estimates for our long-run models. The first column presents our baseline estimates for our full sample with year-to-year variation. The next two columns present the estimates for the effect of changes over 4 years for three different periods and the effects of changes over 6 years for two different periods. The resulting estimates are relatively stable with the test score estimates falling from 0.065 to 0.044 and 0.048, the property tax estimates falling from −0.559 to −0.508 and −0.518, and the spending estimates rising from 0.058 to 0.70 and 0.061 for the

[18] See Clapp et al. (2008) for an earlier application of this technique.

Table 6
Eighth vs. fourth grade test scores.

	Eighth grade test score	Fourth grade test score	Fourth and eighth grade test score
Test 8	.065** (.015)		.047** (.015)
Test 4		.023 (.017)	.032** (.014)
EPTR	−.559** (.075)	−.562** (.072)	−.538** (.074)
EXP	.058** (.008)	.058** (.008)	.059** (.008)
Size	65,325	66,436	64,836

Notes: Estimates are based on the pooled sample of transactions between 1994 and 2004. The dependent variable natural log of house price, $P_{ikb(jk)t}$ of house i in district k at the boundary of districts j and k at time t. The first row contains our baseline difference-in-difference estimates for standardized math test score, and the next two rows contain estimates for property tax and per pupil spending, respectively. The first column repeats our core results using the 8th grade standardized test score, the second column replaces this score with the 4th grade standardized test score and the final column includes both scores. All the specifications also include standard controls and cluster standard errors by boundary.
** represents significance at the 0.05 level or better.

Table 7
Summary statistics for the three subperiods of data.

	1994–1996		1998–2000		2002–2004	
	Mean	S.D	Mean	S.D	Mean	S.D
Housing attributes						
Price (K)	186	187	210	270	331	797
Rooms	6.73	2.75	6.87	2.77	6.70	3.18
Bed	2.91	1.50	3.15	1.38	3.21	1.63
Bath	2.07	.932	1.85	.904	1.61	.841
Age	39	24	45	24.04	44	23.39
Sq. ft. (K)	1.56	1.40	1.62	1.06	1.84	3.91
School district characteristics						
Test score	−.548	1.48	−.549	1.40	−.421	1.30
Taxes	−2.83	.268	−2.88	.217	−3.02	.142
Spending (K)	7.55	.967	7.93	1.204	9.72	.395
Graduation rate	91.03	9.07	89.95	10.08	90.87	9.46
Enrollment	7815	6107	7653	6202	6509	5764
Free lunch	23.86	23.81	26.10	25.86	25.94	26.54
Non-English	13.08	15.11	14.27	16.51	12.81	14.74
Percent central city	41.93		39.28		34.16	
Sample size	13,108		16,878		26,528	

Notes: The cells contain sample means and standard deviations for each period. Sales price, square feet, and per pupil spending are reported in 1000s. The test score is the 3 year moving average of the standardized 8th grade math score, spending is per pupil student spending in the school district, and property tax is reported as present value of the effective property tax burden based on a discount rate of 0.03 where effective property taxes are based on assessed value scaled the district average of the ratio of assessed values to sales prices.

three and two period samples, respectively.[19] Therefore, we find no evidence that our estimates of school quality capitalization were understated by focusing on year-to-year changes in test scores.

4.1. Test for predictable trends across boundaries

A natural concern with our identification strategy is that certain towns, possibly the more affluent and successful towns, are systematically improving over time. Such trends would lead to simultaneously increasing school quality and rising property values. One significant mechanism for such improvements to arise would be households sorting into the districts based on expectations of

Table 8
Estimates for different time periods.

	All years	Composition weights	1994–1999	2000–2004
Math	.065** (.015)	.061** (.017)	.044** (.018)	.084** (.023)
EPTR	−.559** (.075)	−.532** (.081)	−.331** (.097)	−.839** (.095)
EXP	.058** (.008)	.050** (.009)	.025 (.014)	.068** (.010)
Size	65,325	65,325	29,500	35,825

Notes: The Dependent Variable is the natural log of house price, $P_{ikb(jk)t}$ of house i in district k at the boundary of districts j and k at time t. The first column presents the baseline difference-in-difference estimates, the second column presents resulting using weights intended to hold the composition of transactions across towns constant over time and the final two columns present the baseline model estimated for the 1994–1999 and the 2000–2004 subsamples. All the specifications also include standard controls and cluster standard errors by boundary.
** represents significance at the 0.05 level or better.

Table 9
Estimates over longer periods of time.

	All years	3 Period	2 Period
Math	.065** (.015)	.044** (.022)	.048** (.021)
EPTR	−.559** (.075)	−.508** (.080)	−.518** (.106)
EXP	.058** (.008)	.070** (.011)	.061** (.018)
Size	65,325	53,938	59,288

Notes: The Dependent Variable is the natural log of house price, $P_{ikb(jk)t}$ of house i in district k at the boundary of districts j and k at time t. The first column presents the baseline difference-in-difference estimates, and the next two columns present the same model estimated for three 3-year periods (1994–1996, 1998–2000, 2002–2004) and two 5-year periods (1994–1998, 2000–2004). All the specifications also include standard controls and cluster standard errors by boundary.
** represents significance at the 0.05 level or better.

improving test scores or neighborhood quality. If changes in location quality (possibly as captured by housing prices) are predictable, then this will provide individuals with more time and opportunity to sort.

While we cannot rule out bias from systematic improvements in unobservables, we will test whether there are predictable differences across boundaries in the trend in housing prices. Specifically, we estimate our baseline boundary side fixed effect models described in Eq. (2) dropping the controls for the time varying school district attributes, i.e. test scores, property taxes and per pupil expenditures. The resulting residuals from these estimations capture the information used to identify the effect of school district attributes on property values. We then estimate a simple linear time trend model for each side of a boundary.

These slope coefficients describe the trends in housing prices for neighborhoods on either side of boundaries during our sample period. Since this across time variation is used to identify our estimates, we would like to believe that this variation does not contain any systematic or pre-determined trends that might be driving our results. In order to test for this, we examine whether differences in these trends across boundaries can be explained by pre-determined differences between the districts on either side of the boundary. Specifically, the trends on both sides of our 382 boundaries are regressed on the three 1994 school district variables and five 1990 census tract attributes[20] associated with each boundary side while controlling for

[19] Very similar long-run estimates arise when we weight to hold the composition across towns constant over periods as was done in Table 8 across years.

[20] The variables are Income, percent black, percent Hispanic, percent owner-occupied and percent married in tract. Some observations on the same boundary and in the same town lie in different census tracts. The attributes used are an average of the tract attributes across all transactions along a boundary in a given district.

boundary fixed effects in order to test whether there exist predictable and systematic differences in housing price trends across boundaries. For the four models in Table 4, the *F*-statistics associated with the town and neighborhood attributes in the regression model were 0.26, 0.49, 0.32 and 0.14 and the likelihoods of rejecting the null of no predictable trends were 0.978, 0.862, 0.959 and 0.997 for the residuals arising from the models in panels A, B, C and D, respectively.[21] We find no evidence that our findings have been identified using variation in housing prices that is predictable based on observable neighborhood or district information.

5. Conclusion

This paper examines the effect of school quality on housing prices using differences in housing prices across school district boundaries, rather than attendance zone boundaries. Once we control for the neighborhood quality differences across boundaries, we find fairly moderate effects of test scores on property values. These findings are robust to a variety of robustness checks, and we find no evidence that our findings were driven by predictable trends in housing prices on either side of district boundaries. We also find significant effects of property taxes and school expenditures on housing prices, and as with test scores those effects appear to be overstated in models without controls for unobservable neighborhood quality.

However, as in Bayer et al. (2007), we find evidence of bias associated with neighborhood quality differences across boundaries. On average, this bias is moderately larger in our study of school district boundaries than in Bayer et al.'s (2007) study of attendance zone boundaries: 3–5 percentage point vs. a 2 percentage point decline in the standardized effect for Bayer et al. (2007). Our findings serve as a warning for studies that might implement a traditional boundary approach using school districts because those studies might face substantial bias from omitted variables across district boundaries. Obtaining valid estimates using variation across school district boundaries likely requires the availability of across time variation.

In conclusion, our paper provides a new approach for controlling for omitted neighborhood variables when school district information that varies over time is available. This approach may be especially valuable when attendance zone information is not available or meaningful and for examining school district attributes where there is no variation within school districts, such as property tax rates or school expenditures.

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[21] None of the individual *T*-statistics are statistically significant, and out of the thirty-two parameters estimates all but one are below 1.0 (vast majority below 0.5). Standard errors were clustered at the boundary level just as was done in our primary analyses.