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School quality as a catalyst for bidding wars and new housing development

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Abstract

We provide new evidence of the demand for better schools as manifested in bidding wars and changes to the built environment. Using repeat sales before and after a redistricting, we exploit shocks to school quality arising from the continuous, unexpected redistricting of school attendance boundaries in Atlanta. We find that houses redistricted to higher (lower) quality schools are more (less) likely to be involved in a market-driven bidding war. Similarly, undeveloped, redistricted parcels that receive a positive (negative) school quality shock are more (less) likely to be developed. School quality shocks also have a causal effect on house prices and time-on-market.

KEYWORDS
bidding wars, construction, housing demand, housing development, school quality

1 | INTRODUCTION

> *And so it was that middle-class families across America have been quietly drawn into an all-out war…Their war has received little coverage in the press and no attention from politicians, but it has profoundly altered the lives of parents everywhere, shaping every economic decision they make. Their war is a bidding war…[in which] millions of parents joined in the search for a house on a safe street with a good school nearby. Over*

time, demand heated up for an increasingly narrow slice of the housing stock…[with parents] competing furiously with one another for the most important possession: a house in a decent school district.

- Elizabeth Warren and Amelia Tyagi (2016)

An ongoing theme in Warren and Tyagi's (2016) book is that the pressure to send one's children to a good school creates a "furious competition" for housing within those schools' attendance boundaries. This study examines how the furious competition for housing in high-quality school zones affects housing demand. We use a repeat-sales approach that exploits exogenous shocks to school quality arising from the continual redistricting of elementary school attendance boundaries. We show demand for the same house increases (decreases) immediately after it is redistricted to a higher (lower) quality school. We document this relationship using two new measures (bidding wars and residential construction) in addition to two traditional measures (house prices and time-on-market) of housing demand.

To establish a causal relationship, we construct a repeat-sales transaction dataset that spans 14 years across three large school districts in Atlanta, Georgia. The numerous school redistricting events that our study analyzes stand in contrast to the one-time redistricting events used in previous studies. We argue that an increase (decrease) in the probability that a bidding war occurs around the time a house is redistricted to a higher (lower) quality school is consistent with the notion that demand for the house increases (decreases) as a result of the change in school quality from redistricting. The empirical results support our conjecture. We find that a one-standard-deviation positive shock to school quality almost triples the likelihood of a market-driven bidding war. This finding highlights the competition for houses within the highest quality school zones, providing new evidence that parents value better schools.

Another contribution of our study is that we establish a causal link between school quality and the built environment. We first document how school quality relates to the built environment. We find that undeveloped parcels in the highest quality school zones are nearly all developed by the end of our study period. To test for causality, we examine the probability that an undeveloped parcel is subsequently developed after it is redistricted to a different school zone. We find that redistricted parcels are more (less) likely to be developed soon after they receive a positive (negative) shock to school quality. This finding provides new evidence that school quality affects housing demand, of which newly built housing satisfies a portion.

The idea that parents value better schools is well established. Numerous studies find that school quality as proxied by standardized test scores affects house prices using either a border discontinuity design (BDD) (e.g., Black, 1999; Dhar and Ross, 2012) or a redistricting boundaries approach (e.g., Ries and Somerville, 2010; Collins & Kaplan, 2017).[1] Our study differs in that we examine the effect of school quality on previously unexplored facets of the housing market: bidding wars and residential development. We also document significant price and liquidity effects associated with the redistricting of school attendance boundaries.

We find a one-standard-deviation positive (negative) shock to school quality results in a 3% increase (decrease) in house price. The price effect represents an economically significant

[1] There is a rich literature, showing that school quality is capitalized into house prices. See Andreyeva and Patrick (2017), Bonilla-Mejía et al. (2020), and Beracha and Hardin III (2021) for recent examples in the United States and Agarwal et al. (2016) and Chan et al. (2020) for examples abroad. Recent studies also find school quality affects single-family and multifamily housing rents (Beracha & Hardin, 2018; Gabe et al., 2021). We direct readers to Nguyen-Hoang and Yinger (2011) and Turnbull and Zheng (2021) for comprehensive reviews of the literature.

windfall of approximately $8500 to existing homeowners that is capitalized into house prices. We confirm that the price effect is not the product of preexisting trends. Redistricting school attendance boundaries also affects liquidity. Using time-on-market as a measure of liquidity, we find that a one-standard-deviation positive (negative) shock to school quality decreases (increases) time-on-market by approximately 5 weeks.

Previous studies focus on the houses redistricted to existing school zones but do not examine spillover effects on incumbent housing. This is a potential concern because Brummet (2014) finds the closing and displacement of students from lower to higher performing schools creates modest negative spillover effects on the incumbent students' achievement. We examine whether the redistricting process negatively affects the demand for incumbent housing within the receiving school zone. We do not find any spillover effect on demand for incumbent housing in redistricted school zones.

Our results not only support the conjecture that parents value better schools but also indicate that school quality has a causal impact on housing demand. Furthermore, we show that the competition for houses in the highest quality school zones has direct and indirect effects not only on housing market outcomes but also on the built environment. Our use of repeat sales before and after a house is redistricted mitigates concerns that unobserved time-invariant house and neighborhood attributes bias our results. We also include textual information from the public remarks section of the Multiple Listing Service (MLS) to control for the possibility that houses redistricted to higher (lower) quality schools may have undergone more (less) improvements prior to their postredistricting sale. We thus reaffirm the results in the extant literature while simultaneously providing new insights into the demand for better schools.

2 | DATA OVERVIEW

Our study area includes three school districts (Atlanta Public, DeKalb County, and Fulton County) in the two counties (DeKalb and Fulton) that represent the metropolitan core of Atlanta. The dataset combines several data sources including school attendance boundaries, standardized test scores, parcel-level land use records, and transaction data. The following subsections describe the datasets individually and provide summary statistics for the merged dataset.

2.1 | School attendance boundaries

The Board of Education for a school district determines the official attendance boundaries for each elementary school. The primary residence of their custodial parent/legal guardian determines which school students attend. We obtain the elementary school attendance boundaries for every school year from 2000–2001 through 2013–2014 from each school district's planning department.[2],[3] Using the school attendance boundaries, we assign every residential parcel in the three school districts to the appropriate elementary school. The assignment process includes both

[2] Going forward, we use the terminal year of a school year to refer to the school year. For example, SY2001 refers to school year 2000–2001 and SY2014 refers to school year 2013–2014.

[3] Atlanta Public School and DCSD provided shapefiles for SY2009–SY2014. We recreated the shapefiles for SY2001–SY2008 based on boundary descriptions and maps provided by the school districts. FCSD provided shapefiles for every school year (SY2001–SY2014). The school attendance boundaries for SY2001 and SY2014 are provided in the Internet Appendix.

developed and undeveloped residential parcels regardless of whether the parcel transacts during the study period.

School boards typically redistrict school attendance boundaries for one of two reasons: underutilization or overutilization. Redistricting is necessary when the school-age population within a school attendance zone falls short of (i.e., underutilization) or outgrows (i.e., overutilization) the occupancy capacity of the school that serves it. Consolidation often occurs for underutilized schools. In contrast, overutilized schools either build a replacement school with additional capacity or open an additional school.

Redistricting of school attendance boundaries occurred numerous times in Atlanta from SY2001 to SY2014. In fact, redistricting affected over 31% of the parcels in this study. The reason for the redistricting differs by school district. Table 1 tabulates the elementary school closings and openings across the three districts. The number of elementary schools in the Atlanta Public Schools (APS) declined from 67 in SY2001 to 48 in SY2014, corresponding to a decrease in elementary enrollment from 31,787 to 28,179 over the same period. The number of elementary schools in the DeKalb County School District (DCSD) also declined from 75 in SY2001 to 67 in SY2014, although its elementary school enrollment increased moderately from 50,484 in SY2001 to 51,377 in SY2014. In contrast, the number of elementary schools in Fulton County School District (FCSD) increased from 45 in SY2001 to 58 in SY2014, corresponding to an increase in elementary enrollment from 33,858 to 45,412 over the same period.

We briefly describe the redistricting process here. The Internet Appendix provides a more detailed overview of the process for all three school districts. In most cases, the school district informs the public of potential changes to attendance boundaries 1 year prior to their implementation. Soon after making the announcement, the district holds public meetings to collect feedback. The district then makes its final decision, with implementation occurring the following school year. For example, APS announced that they would close CW Hill Elementary School in the fall of 2008. Then, APS held public meetings to collect feedback in January 2009. In March 2009, APS announced that CW Hill would close at the end of the school year.

Changing attendance boundaries is a sensitive issue that requires input from the community. Accordingly, redistricting decisions are intrinsically political and could be endogenous to variations in housing demand. For example, developers will naturally lobby to redistrict their parcels to higher quality schools. Similarly, homeowners want their houses redistricted into the highest quality school zones, and affluent families may be more successful at exerting pressure on local authorities. School closures or openings, which necessitate the redistricting of school zones, can be endogenous for the same reasons. These factors can potentially bias the estimated effect of redistricting on housing demand.

In selecting schools to be closed, studies examining the effect of school closures on student achievement (e.g., Engberg et al., 2012; De Haan et al., 2016; Steinberg and MacDonald, 2019) note that academic performance is typically the most heavily weighted criterion. For example, Brummet (2014) notes that "policymakers have suggested shutting the lowest-performing schools and shifting students to higher-performing schools as a way to increase student achievement [...but that] community leaders and teachers unions often vehemently oppose these school closings." Our setting is ideal. We observe positive and negative shocks to school quality resulting from the school closings and openings discussed above.

The district-specific overviews in the Internet Appendix demonstrate that the redistricting of school attendance boundaries provides a plausibly exogenous shock to school quality. When this shock is included in our quasi-experimental research design, it allows us to estimate school

TABLE 1 School closings and openings

School Year	Combined districts			Atlanta public			DeKalb County			Fulton County		
	# Schools	Closed	Opened	# Schools	Closed	Opened	# Schools	Closed	Opened	# Schools	Closed	Opened
2001	187			67			75			45		
2002	188	3	4	65	3	1	75	0	0	48	0	3
2003	188	2	2	64	2	1	75	0	0	49	0	1
2004	187	5	4	62	5	3	75	0	0	50	0	1
2005	186	7	6	58	7	3	76	0	1	52	0	2
2006	186	0	0	58	0	0	76	0	0	52	0	0
2007	187	0	1	58	0	0	76	0	0	53	0	1
2008	189	1	3	57	1	0	78	0	2	54	0	1
2009	183	9	3	55	3	1	73	6	1	55	0	1
2010	185	1	3	54	1	0	73	0	0	58	0	3
2011	187	0	2	55	0	1	73	0	0	59	0	1
2012	180	7	0	55	0	0	67	6	0	58	1	0
2013	173	8	1	48	8	1	67	0	0	58	0	0
2014	173	0	0	48	0	0	67	0	0	58	0	0

Note: This table tabulates the total number of elementary schools in operation each school year by school district. The second column of each section displays the number of schools closed at the end of the previous school year. The third column of each section displays the number of schools that are opened at the beginning of the current school year. The difference between the two columns represents the net change in the number of schools in operation for that school year.

TABLE 2 Descriptive statistics for school quality measures

	Test score measure			Correlation		
	Avg	Min	Max	Avg	Min	Max
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Single year						
Normalized raw	41.21	4.08	99.11	0.40	−0.78	1.00
R&S	4.88	−57.87	72.83	0.42	−0.94	0.95
Panel B: Three-year rolling average						
Normalized raw	41.35	3.76	99.31	0.75	−0.62	0.99
R&S	1.74	−65.40	71.52	0.74	−0.83	0.99

Note: This table provides descriptive statistics for the school quality measures used in the empirical analysis. The school quality measures in Panels A and B are the same except for the length of the term used in their construction. Panel A is a single-year measure, whereas Panel B uses a three-year rolling average. Columns (1)–(3) display the average, min, and max of the school quality measure by school. Columns (4)–(6) display the average, min, and max year-over-year correlation by school. The first school quality measure in each panel represents the average normalized raw test score on the Math and Reading sections of the CRCT. The second school quality measure (R&S) represents the difference in the percent of students who "ES" relative to the percent of students who "DNMS" of the CRCT.

quality's causal effect on housing demand. Although school utilization and standardized test score information provide a signal that redistricting *might* occur in the future, when, where, and if redistricting will actually occur is unknown ex-ante. Discussions with members of each district's planning department and anecdotal evidence in the Internet Appendix suggest that the public is frequently "caught off guard" by redistricting initiatives.

To further address endogeneity concerns, we identify and run subsample analyses on a highly exogenous subset of redistricted transactions. Specifically, we exploit the fact that opening and closing schools often affects attendance boundaries in neighboring school zones. We show that our findings are robust using a subset of houses that were redistricted to a different school zone even though their existing school remained open.

2.2 | School quality measures

We construct school quality measures using Criterion-Referenced Competency Test (CRCT) scores from the Georgia Department of Education. Introduced in SY2000, the CRCT measures student achievement of state-mandated content standards. The CRCT was administered in late spring each year and the results were released before the end of the school year. In SY2015, the Georgia Milestones Assessment System replaced the CRCT.

We create two distinct measures of school quality to demonstrate the robustness of our empirical findings. Panels A and B of Table 2 display descriptive statistics for the school quality measures using a single-year and three-year rolling average, respectively. Columns (1)–(3) display the average, minimum, and maximum of the school quality measures and columns (4)–(6) display the average, minimum, and maximum year-over-year correlations by school. We construct both school quality measures using only the Math and Reading sections of the CRCT.

Like Black (1999), the first school quality measure, "Normalized Raw," uses normalized raw test scores. The second school quality measure uses the percent of students who "did not meet the standards" (DNMS), "met the standards" (MS), or "exceeded the standards" (ES) of the CRCT.

The second measure is identical to the measure in Ries and Somerville (2010). It represents the difference in the percent of ES students relative to the percent of DNMS students. We provide additional information about the construction of the school quality measures in the Internet Appendix. Hereafter, we refer to this school quality measure as the R&S measure.

The descriptive statistics in Table 2 show that performance varies widely across elementary schools. The single-year measures are noisy relative to the 3-year rolling averages. This result is consistent with the finding of Kane and Staiger (2002) that most year-to-year changes in test scores are nonpersistent. Following the previous literature (Bayer et al., 2007), we use a 3-year rolling average of test scores in our empirical analysis to mitigate the random variation (i.e., noise) in the single-year measures.

We use standardized test scores as our school quality measure given prior research shows that parents use this information to select schools. For example, Hastings and Weinstein (2008) provide school test score information to a randomly selected subset of lower income families in a public-school choice plan. They find that receiving the test score information significantly increases the fraction of lower income families that choose higher performing schools. Similar findings are documented in studies analyzing household preferences in school choice programs (e.g., Abdulkadiroğlu et al., 2014; Dobbie & Fryer Jr, 2014; Jackson, 2010; Pop-Eleches and Urquiola, 2013). Standardized test scores have the added benefit that they are publicly available and frequently covered in local newspapers.[4] We do not consider alternative school quality measures such as value-added (e.g., Hanushek et al., 2007; Imberman and Lovenheim, 2016; Rothstein, 2010) because these measures are unavailable for the entire study period. Furthermore, these alternative measures are not routinely disseminated to the public via the press. Similarly, we do not use state-administered school grades (e.g., A, B, C) because they are only available from the Georgia Department of Education from 2012 onward (Figlio & Lucas, 2004).

2.3 | Parcel-level land use

We use tax assessor data from DeKalb County and Fulton County. Tax assessor data include parcel-level information for every residential parcel in the two counties, regardless of whether the parcel is developed or listed for sale during the study period. We assign every residential parcel to the appropriate school zone for each school year using the attendance boundary files. The assignment process allows us to identify new development and track the housing supply available in each elementary school zone over time. We use the "year built" field in the county tax assessor files to determine when an undeveloped parcel is developed. There were approximately 56,100 undeveloped residential parcels in the two counties at the end of calendar year 2001. During our study period, 79.5% of these parcels were developed and 39.5% were redistricted. The Internet Appendix provides additional insights at the school district level.

2.4 | Transaction data

The transaction data include single-family detached houses listed for sale in the Georgia Multiple Listing Service (GAMLS) from July 2001 through June 2015. The GAMLS

[4] For example, the Atlanta Journal-Constitution (AJC), which is the major daily newspaper in Atlanta, was the first to flag irregularities in CRCT test score data. We discuss the irregularities later in this section.

data contain detailed information about the property's location, age, structural characteristics, and distressed sales conditions. The GAMLS data also include listing (list date, off market date, list price, etc.) and transaction (sales price, seller concession, etc.) information that we use to calculate the time-on-market and identify houses involved in bidding wars.

Since the CRCT test scores are typically released in June of the year they are administered, we link the test scores for each school year to transactions in the subsequent four quarters. For example, we link the test scores released in SY2001 (SY2002) with transactions in the third and fourth quarters of 2001 (2002) and the first and second quarters of 2002 (2003). If a property is redistricted to a preexisting school, it is immediately associated with the test score for that school. Properties redistricted to new schools are associated with their old school's test score until the following year, when the new school's test score is publicly available. We run several specifications that include or exclude transactions that are redistricted to new schools. The results are similar across specifications.

Prior to running the empirical analysis, we impose several restrictions on the data. We geocode every record using the property address listed in the MLS and tax assessor datasets. Using the geocoded address, we assign the property to the appropriate elementary school zone for each school year and create a unique identifier that allows us to link listing and sales activity over time. Property addresses that are not geocoded are dropped. We also drop records where a variable of interest is missing or contains an invalid value. To eliminate outliers and minimize data errors, we filter the data on several distressed sales conditions and physical characteristics. We provide a complete list of the filters in the Internet Appendix. Summary statistics for the filtered transaction dataset and a subsample of houses that sold at least twice are reported in Panels A and B of Table 3.

Approximately 15% of the repeat-sales transactions represent houses that sold before and after being redistricted to a different school. Table 4 displays descriptive statistics for the change in school quality associated with the redistricted repeat-sales sample. Column (1) displays the number of unique redistricted transaction pairs, whereas columns (2)–(5) provide descriptive statistics for the shock to school quality (in percentage terms) associated with the redistricting. We calculate the shock to school quality as $S^* = S_{zt} - S_{z't'}$ where S represents one of the two school quality measures. The shock is only nonzero for houses that are redistricted from school zone z' to school zone z at times t' and $t = t' + 1$. Columns (6)–(8) display the fraction of repeat-sales pairs in which school quality improves ($S^* \geq 5\%$), declines ($S^* \leq -5\%$), or remains the same ($5\% > S^* > -5\%$) as a result of the redistricting.

The first row of both panels includes the entire redistricted repeat-sales sample regardless of how long after the redistricting the postredistricting transaction occurs. Since the shock to school quality should have a stronger effect the closer it occurs to the redistricting event, the second row in both panels removes repeat-sales pairs where the postredistricting transaction occurs more than 3 years after the house is redistricted. The third row includes two additional filters. We drop every repeat-sales pair in the APS district because there was a large-scale cheating scandal during the study period. We also drop the first year of transactions in new school zones, given that new school zones do not have a publicly available test score. However, transactions in the second and third years of the new school zone (i.e., when a test score is publicly available) are included in the third row. The descriptive statistics in Table 4 show that the shock to school quality is positive on average. The fraction of houses positively (negatively) affected by redistricting varies with the school quality measure employed.

TABLE 3 Summary statistics for transaction data

	Panel A: All sales					Panel B: Repeat sales				
	Mean	p25	p50	p75	SD	Mean	p25	p50	p75	SD
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Sales Price	278,962	138,900	228,000	362,500	201,699	296,428	166,000	256,500	380,000	187,914
Seller Concession	2,203	0	0	4,000	3,046	2,285	0	0	4,448	3,092
Transaction Price	276,759	135,930	225,000	360,000	201,556	294,143	164,423	254,000	379,380	187,804
1st Original List Price	307,409	147,900	240,000	387,500	331,021	328,045	175,000	270,000	400,000	374,466
Original List Price	302,525	145,000	239,900	379,900	319,412	324,365	174,900	269,900	399,900	369,888
Terminal List Price	289,128	139,999	234,900	375,000	213,499	306,561	169,900	264,900	395,000	198,083
Time-on-Market (days)	93.4	21.0	57.0	125.0	105.8	87.0	19.0	54.0	116.0	99.3
Age (years)	32.3	12.0	26.0	50.0	24.2	34.2	14.0	29.0	52.0	24.2
Living Area (sqft 000s)	2.2	1.5	2.0	2.8	0.9	2.2	1.5	2.0	2.7	0.9
Lot Size (sqft 000s)	17.4	8.7	13.1	19.8	15.5	16.6	8.7	13.1	18.8	13.3
Bedrooms	3.7	3.0	4.0	4.0	0.9	3.6	3.0	3.0	4.0	0.9
Bathrooms	2.7	2.0	2.5	3.0	1.0	2.6	2.0	2.5	3.0	0.9
Normalized Raw (%)	55.9	25.3	53.9	93.6	33.1	61.4	28.6	66.7	95.3	32.4
R&S (%)	17.9	−8.3	14.7	47.7	32.7	22.4	−4.2	22.6	52.1	32.0
Bidding War	0.02					0.02				
Atlanta Public	0.19					0.18				
DeKalb County	0.44					0.46				
Fulton County	0.37					0.36				
Observations	99,302					23,753				

Note: This table provides summary statistics for the entire filtered transaction dataset in Panel A and a subsample of houses that sold at least twice during the study period (i.e., repeat sales) in Panel B. Houses that are redistricted are only included in Panel B if they have a transaction prior to and after the redistricting. The 1st Original List Price variable represents the Original List Price for houses that are relisted. If a house is not relisted, then the 1st Original List Price equals Original List Price. The Terminal List Price is the list price in MLS at the time of sale. We calculate time-on-market using both sold and unsold listings where properties that are taken off the market and relisted within 90 days are treated as a continuous listing. The normalized raw and R&S school quality measures represent the 3-year rolling test score average. The bidding war variable represents unintentional, market-driven bidding wars only.

TABLE 4 School quality shock

	Pairs (1)	Avg (2)	p25 (3)	p50 (4)	p75 (5)	Improve (6)	Decline (7)	Same (8)
Panel A: Normalized raw shock								
All	1735	1.96	−3.84	−0.29	10.70	0.38	0.23	0.39
Recent	957	3.21	−3.16	−0.29	10.70	0.43	0.19	0.37
No New orAPS	484	4.38	−3.84	4.47	14.48	0.48	0.21	0.31
Panel B: R&S shock								
All	1735	6.05	−8.42	10.49	15.74	0.61	0.31	0.08
Recent	957	7.35	−6.54	10.58	22.54	0.64	0.28	0.08
No new or APS	484	5.59	−8.01	9.29	22.54	0.55	0.36	0.09

Note: This table provides descriptive statistics for the shock to school quality in the repeat-sales transaction data. Panel A displays the shock to the normalized raw test scores and Panel B displays the shock to the R&S test score measure. Column (1) displays the number of unique transaction pairs in the repeat-sales data and columns (2)–(5) provide insight into the range of the school quality shocks associated with those transaction pairs in percentage terms. Columns (6)–(8) display the fraction of repeat-sales pairs whose test scores improve ($S^* \geq 5\%$), decline ($S^* \leq -5\%$), or remain the same ($5\% > S^* > -5\%$) after the house is redistricted to a different school. The first row in both panels displays descriptive statistics for every house that transacts before and after it was redistricted to a different school. The second row filters the data to remove repeat-sales pairs in which the postredistricting transaction takes place more than 3 years after the property is redistricted. The third row includes two additional filters that remove repeat-sales pairs that (i) are in the APS district or (ii) sold within 1 year of being redistricted to a newly opened school. Transactions in the second and third years (i.e., when a test score is available) are included.

3 | EMPIRICAL METHODOLOGY

3.1 | Background

Starting with Oates (1969), early studies use hedonic pricing techniques to link the cross-sectional variation in house prices with school quality. As with any hedonic equation, the estimates are subject to an omitted variable bias that arises from unobservable time-varying and time-invariant house and neighborhood attributes. More recent studies attempt to correct for this bias using one of two distinct approaches: BDD or redistricting boundaries.

The BDD approach that Black (1999) popularized uses border fixed effects to control for unobserved neighborhood attributes. The approach identifies houses on both sides of an attendance boundary located a short distance (e.g., less than a quarter mile) from the boundary. The underlying assumption is that the houses near the attendance boundaries are in the same neighborhood, even though they are in different school zones. The BDD approach argues that including border fixed effects isolates school quality's effect on house prices.

While the BDD approach offers substantial improvements over a simple hedonic regression, the approach cannot account for every unobserved house and neighborhood attribute. For example, Kane et al. (2006) and Gibbons et al. (2013) note that a discontinuity in prices could arise from different directional outlooks, omitted geographical variables, or location-specific amenities. The BDD approach also assumes that neighborhoods (houses) located near school attendance boundaries are similar to neighborhoods (houses) that are not located near a border. However, Zahirovic-Herbert and Turnbull (2009) note that households living near a boundary may place a lower weight on across-zone differences in school quality given their belief that the boundaries might change—as they frequently do in our study. Consequently, we provide estimates

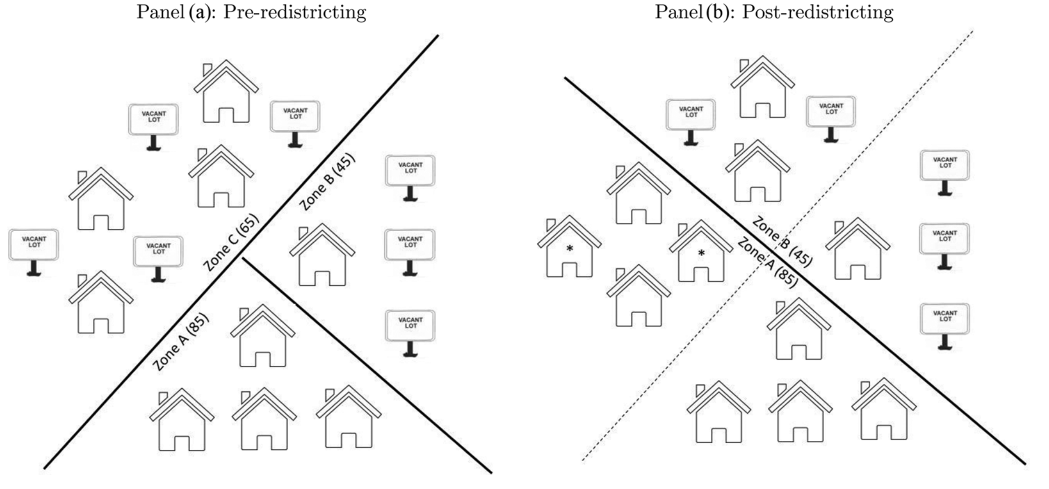

FIGURE 1 Demand for housing across school zones

Note: Panel A displays three school zones that differ only in terms of school quality and housing supply elasticity. Each school zone's average test score is displayed in brackets and its housing supply elasticity is represented by the availability of undeveloped vacant residential parcels. Panel B displays the same set of parcels after one-half of Zone C is reassigned to Zone A and the other half is reassigned to Zone B. Houses labeled with an asterisk in Panel B represent new development that occurs during the postredistricting period.

using the BDD approach as a robustness check in the Internet Appendix. We use the redistricting boundaries approach in the bulk of our analysis.

3.2 | Empirical strategy

The redistricting boundaries approach uses repeat-sales data and the redistricting of school attendance boundaries to control for unobserved house and neighborhood attributes. The approach uses the same set of houses to examine the difference in price before and after the house is redistricted. The underlying assumption is that unobserved house and neighborhood attributes are time-invariant, so using repeat sales of the same house negates their effect on transaction prices.

Figure 1 presents a visualization of our empirical strategy. In Panel A of Figure 1, there are three school zones that differ only in terms of school quality and housing supply elasticity. For each school zone, the average test score is displayed in brackets and its housing supply elasticity is represented by the availability of undeveloped residential parcels. In this simple example, we posit that Zone A has the highest level of latent demand because it has the highest test scores. We expect higher quality school zones similar to Zone A to have more bidding wars, fewer undeveloped parcels, higher average transaction prices, and shorter average time-on-market.

Although the example in Panel A suggests school quality is correlated with housing demand, it does not establish causality. To do so, we use the shock to school quality resulting from the redistricting of public-school attendance boundaries. More specifically, we show that the demand for a property increases (decreases) when the property is redistricted to a higher (lower) quality school zone. Panel B of Figure 1 presents a visualization of our approach. Properties in Zone C in Panel A are redistricted to either Zone A or Zone B. We argue that the demand for properties redistricted to Zone A (B) will increase (decrease) because they are redistricted to a higher (lower) quality school.

The redistricting in Panel B offers several distinct empirical tests of the causal effect that school quality has on housing demand. We first look at the probability that a house is involved in a market-driven bidding war before and after it is redistricted. If the probability that a bidding war occurs increases (decreases) around the time the house is redistricted to a higher (lower) quality school, then this supports the notion that the demand for the house increased as a result of the redistricting. We next examine the probability that an undeveloped parcel is built on around the time it is redistricted to a different school zone. If the probability that a new house is built on a previously undeveloped residential parcel increases around the time the parcel is redistricted, then this presents strong evidence that school redistricting increases the likelihood of development. We finally estimate the causal effect of school quality on price and liquidity.

The empirical strategy we employ is similar to Bogart and Cromwell (2000), Ries and Somerville (2010), and Collins and Kaplan (2017) except that we examine multiple redistricting events. Bogart and Cromwell (2000) use data from Shaker Heights, Ohio, that include a one-time redistricting in 1987 that reduced the number of elementary schools from nine to six. Unfortunately, every school in their study is of high quality, so they cannot estimate the effect of redistricting using schools of disparate quality. Ries and Somerville (2010) examine a one-time redistricting in Vancouver, British Columbia, in 2001. In contrast to Bogart and Cromwell (2000), their dataset includes 69 elementary schools with substantial cross-sectional variation in school quality. After controlling for price trends in surrounding neighborhoods, Ries and Somerville (2010) find that house prices do not respond to the shocks to elementary school quality associated with the redistricting. They do, however, find that prices in the top quartile respond to shocks to secondary school quality associated with the redistricting. Collins and Kaplan (2017) also examine a one-time redistricting resulting from the consolidation of Memphis City Schools and Shelby County Schools into a single unified system. Collins and Kaplan (2017) find that a one-standard-deviation increase in school quality increases house prices by 3%.

3.3 | Empirical methodology: Bidding wars, price, and liquidity

To examine the link between school quality and housing demand, we first estimate a linear fixed effects model of the form

$$d_{iznt} = S_{zt}\tau + X_{int}\beta + \psi_{nt} + \gamma_{in} + \alpha_{int} + \upsilon_{iznt}, \tag{1}$$

where d_{iznt} represents the demand for house i in neighborhood n and elementary school zone z at time t. d_{iznt} is either an indicator variable for market-driven bidding wars or a continuous variable for log of transaction price or time-on-market. S_{zt} represents one of the two test score measures, X_{int} is a vector of house and neighborhood controls, ψ_{nt} is a vector of either additively separable (Z+SY) or multiplicatively separable (Z×SY) zip code and school year fixed effects that control for house price changes over time, γ_{in} (α_{int}) is an "unobserved" time-invariant (time-varying) effect that is not captured by ψ_{nt}, and υ_{iznt} is a zero-mean error term that is uncorrelated with all of the aforementioned variables: S_{zt}, X_{int}, ψ_{nt}, γ_{in}, and α_{int}.[5] The full list of controls in X_{int} is provided in the Internet Appendix.

Equation (1) can be estimated using least squares. As noted earlier, the estimates of $\hat{\tau}$ are biased if unobserved time-invariant (γ_{in}) and time-varying (α_{int}) attributes of the house and neighborhood are correlated with school quality (S_{zt}). Although Equation (1) can establish a link between school quality and housing demand, we cannot claim that the effect is causal. The reason is that houses in higher quality school zones differ along observed (and most likely unobserved) dimensions. For example, we provide descriptive statistics by school test score decile in the Internet Appendix that show, among other things, the average size of the house and parcel increase in tandem with school test scores.

To establish a causal relationship, we use the quasi-experimental research design outlined in the preceding subsection. Using repeat sales of the same house, we estimate housing demand before and after a redistricting occurs. The underlying assumption is that the unobserved house and neighborhood attributes remain constant over time, thereby negating their effect on housing demand. The model takes the following form:

$$d_{iznt} = S^*_{zt}\tau^* + W_{int}\beta + \Omega_i + \psi_{nt} + \alpha_{int} + \upsilon_{iznt}, \tag{2}$$

which differs from Equation (1) in its use of house fixed effects, Ω_i, and the shock to school quality, S^*_{zt}. The inclusion of house fixed effects in Equation (2) means that we compare the same house in the same neighborhood. The time-invariant attributes of the house and neighborhood are differenced out for consecutive pairs of repeat transactions for house i at times t' and $t > t'$. For this reason, γ_{in} is not included in Equation (2) and W_{int} represents a subset of time-varying transaction controls in X_{int} from Equation (1).

Ries and Somerville (2010) highlight the difficulty in disentangling the change in neighborhood prices ($\Delta\psi = \psi_{nt} - \psi_{nt'}$) from the effect of changes in school quality ($\Delta S = S_{zt} - S_{zt'}$) and the shock to school quality related to the redistricting ($S^* = S_{zt} - S_{z't'}$). To address the issue, they compute several levels of disaggregated price indexes using subsamples of houses unaffected by the redistricting. They argue that $\Delta\psi$ is orthogonal to ΔS for houses that did not experience a shock to school quality. Thus, the shock to school quality (S^*) is only nonzero when a house is redistricted. A potential concern with this approach is that it requires the trend in housing prices to be uncorrelated with the redistricting. If the trends are correlated, which a robustness check in Ries and Somerville (2010) indicates they are, then most of their findings may be explained by preexisting differences in price trends between zoned and rezoned areas (Collins & Kaplan, 2017).

We take a similar, albeit slightly different approach. We exploit the extended time period of our study to include Z×SY fixed effects.[6] Our approach differs in that it controls for price changes in the redistricted neighborhood, instead of using price changes in the surrounding neighborhoods that are not redistricted. This might bias the magnitude of our estimates downward because it includes the redistricted transactions in the fixed effect estimate. However, we argue that including the redistricted transactions in the estimation of $\Delta\psi$ strengthens our claim that the shock to school quality has a causal effect if the coefficient estimates (τ^*) are still significant. More

[5] The "unobserved" time-invariant (γ_{in}) and time-varying (α_{int}) effects refer to relevant attributes that the researcher (i) does not observe (i.e., they are not available in the dataset) or (ii) does observe but does not include in Equation (1). A researcher might exclude an attribute if it is not readily available or easily quantifiable, or they think that it is irrelevant.

[6] The empirical results reported throughout the article are robust to the use of alternative neighborhood by time fixed effects including the original elementary school zone, new elementary school zone, and school district.

importantly, the use of Z×SY fixed effects in lieu of disaggregated price indexes in Equation (2) allows us to use the same specification to estimate the causal effect that school quality has on several distinct measures of housing demand: bidding wars, transaction prices, and time-on-market.

To demonstrate that our findings are robust and to allow for a more direct comparison, we also provide estimates using a disaggregated price index approach in the Internet Appendix. The coefficient estimates are similar to the results we report in the body of the article; they are positive and statistically significant. Not surprisingly, the magnitude of coefficient estimates is larger because the price indexes do not properly control for changes to *local* market conditions (i.e., the price indexes are aggregated at a higher level).

3.3.1 | Textual analysis

A natural concern is that houses redistricted to higher (lower) quality schools might undergo more (less) home improvements prior to their postredistricting sale. Bayer et al. (2007) express a similar concern using the BDD approach. They note that the BDD approach does not "address the possibility that the higher-income households on the higher test score side of a school boundary might be more likely to make home improvements (e.g., install granite countertops) unobserved by the researcher, in turn contributing to the higher average house prices on that side of the boundary." To address this concern, we include textual information from the agents' descriptions of the house provided in the public remarks section of the MLS. Nowak and Smith (2017) show that the remarks section of the MLS contains indicators of both time-invariant and time-varying attributes of the house and neighborhood. Thus, if houses redistricted to higher-quality schools are more likely to have newly installed granite countertops, we would capture this salient information when included in the agents' remarks.

We incorporate the textual information from the agents' remarks into Equation (2) using the double-selection LASSO procedure in Liu et al. (2020).[7] When employed in a textual analysis framework, the double-selection LASSO procedure identifies keywords and phrases (*tokens*) that predict house prices and/or postredistricting transactions. The selected tokens form a sufficient dictionary that, when included as indicator variables in Equation (2), allow for valid asymptotic inference on the parameter of interest, τ^*, associated with the variable of interest, S^*, the shock to school quality.

Including house fixed effects in Equation (2) controls for unobserved time-invariant house and neighborhood attributes. Including the sufficient dictionary of tokens in Equation (2) controls for time-varying house and neighborhood attributes (α_{int}) that are "unobserved" in previous studies (Nowak & Smith, 2020). When employed in unison, our approach allows us to isolate the causal effect of school quality on several measures of housing demand: bidding wars, transaction prices, and time-on-market.

3.4 | Empirical methodology: Development

Panel A of Figure 1 posits that higher (lower) quality school zones have fewer (more) undeveloped residential parcels. However, several other plausible explanations could exist for the

[7] LASSO is short for least absolute shrinkage and selection operator. For additional information about the double-selection LASSO procedure, see Belloni et al. (2014) and Liu et al. (2020).

development patterns in the figure—such as proximity to employment, shopping, or recreation facilities. To control for these other plausible explanations, we use the redistricting of school attendance boundaries to examine whether school quality has a causal effect on new housing development. If the shock to school quality is not correlated with these other plausible explanations, then any change to the probability that a parcel is developed that occurs soon after a redistricting is strong evidence that school quality has a causal effect on housing development.

Although our empirical strategy is the same, we use a proportional hazard model to examine school quality's causal effect on new housing development. In doing so, we contribute to the burgeoning literature that examines the "real option" associated with undeveloped land (Bulan et al., 2009; Cunningham, 2007; Grenadier, 1996; Quigg, 1993). The proportional hazard model takes the following form:

$$h(t) = h_0(t)\exp(X_t'\beta), \tag{3}$$

where a vector of covariates, X, shift the baseline hazard. The vector of covariates includes the undeveloped parcel's distance to downtown Atlanta, parcel size, and indicator variables for each school district. We also either include the shock to school quality (S^*) or lagged indicators that identify when the parcel is redistricted. One advantage of this approach is that it does not require a true "start date." We can thus estimate the hazard starting at the beginning of our study period when the undeveloped residential parcels enter the sample (i.e., SY2001) and remain until they "die" (i.e., a house is built on the parcel).

4 | BIDDING WARS

Since parents want to send their kids to the best school possible, bidding wars should occur more frequently in the highest quality school zones. Unfortunately, we are unable to observe the number of bids received for each transaction. This complicates the identification of whether a house is involved in a bidding war. Previous studies assume that any house that sells for a price above its original list price is involved in a bidding war (Bucchianeri & Minson, 2013; Han & Strange, 2014; Han & Hong, 2016). Our approach differs slightly. We identify whether the *transaction price* is greater than both the original and terminal list price. To construct our bidding war indicator variable, we define $FOLP_{it}$ as the first original list price, OLP_{it} as current original list price, LP_{it} as the terminal list price, TP_{it} as the transaction price, SP_{it} as the sales price, and SC_{it} as the seller concession for house i at time t. If a property is taken off the market and relisted within 90 days, we treat it as a continuous listing. In this case, $FOLP_{it}$ represents the original list price for the property when it was first listed. If the property is not relisted, then $FOLP_{it} = OLP_{it}$. Using this notation, $TP_{it} = SP_{it} - SC_{it}$ and the bidding war indicator variable, $Bidwar_{it}$, is constructed as follows:

$$Bidwar_{it} = \begin{cases} 0, \text{if } \max(FOLP_{it}, OLP_{it}, LP_{it}) \geq TP_{it} \\ 1, \text{if } \max(FOLP_{it}, OLP_{it}, LP_{it}) < TP_{it}. \end{cases} \tag{4}$$

Residential housing transactions frequently include a seller concession. Table 3 shows that 47.5% of the sold listings include a seller concession. The average concession is approximately 2.2% of the sales price. We subtract seller concessions because their inclusion misclassifies

3.4% of the transactions as bidding wars (11.4% using sales price vs. 8% using transaction price).

We argue that bidding wars should occur more frequently in school zones with high levels of latent demand. Those school zones, by definition, have multiple bidders waiting to purchase housing. If there is latent demand for a house, we expect the bidding war to occur immediately after it is listed. As such, we use various time-on-market cutoffs to further restrict our bidding war measure in Equation (4). Our preferred measure includes bidding wars ($Bidwar_{it} = 1$) with a time-on-market that is less than or equal to 28 days. The 28-day cutoff signifies that the listing incited immediate activity and multiple bids. Although we do not have information on the number of bids received, it is more plausible that multiple bids occur early in its listing cycle (i.e., 4 weeks or less), which push the transaction price higher compared to a house that is listed for 10 weeks. The Internet Appendix provides additional insight into the time-on-market cutoffs. The cutoff selected (28, 42, or 56 days) does not materially impact our results.

Although our dataset does not include the number of bids received for each transaction, we provide cursory evidence on the validity of our bidding war measure using a small set of Redfin transactions. The Redfin dataset contains 175 transactions in Atlanta from 2015 to 2017 in which a Redfin agent represented either the buyer or seller. More importantly, the Redfin dataset includes the number of bids received for each transaction. Using Equation (4), we classify 38 of the 175 transactions in the Redfin dataset as bidding wars. Of the 38 transactions flagged as bidding wars, approximately 80% received more than one bid. For these 30 transactions, the number of bids received ranged from 2 to 16. Although the remaining eight transactions received only one bid, the average transaction price to list price (1.030) was similar to the other 30 transactions (1.025). Moreover, the average time-on-market (6.4 days) for both transaction sets was nearly identical. Thus, we interpret the eight transactions with only one bid as preemptive bidding wars.[8]

4.1 | Intentional versus unintentional bidding wars

When a house is listed on the MLS, the seller, presumably with guidance from their agent, sets the list price.[9] The market, however, decides whether the house sells via traditional Nash bargaining (i.e., standard sequential search) or a bidding war (i.e., ascending bid auction). Although the seller does not choose the process, they can influence the likelihood of a bidding war by strategically marketing the house.[10] A low list price will attract more visitors (Han & Strange, 2016), so it has a better chance of receiving multiple competing bids that result in a bidding war. Despite a rich literature on pricing strategies, few empirical studies exist on real estate underpricing and bidding wars. Bucchianeri and Minson (2013) is an exception. The authors find little to no benefit to underpricing a house, even in hot markets.

[8] In all but one of the eight transactions, the Redfin agent represented the buyer. Consequently, it is possible that the seller received more than one offer, but it was not communicated to the Redfin agent.

[9] Barwick and Pathak (2015) provide a detailed overview of the residential brokerage industry in the United States. The authors note that listing agents are "typically involved in advertising the house, suggesting listing prices, conducting open houses, and negotiating with buyers." Although real estate auctions are typically reserved for distressed sellers in the United States, they are common in other parts of the world (Mayer, 1995). For example, leasehold sales of urban land are conducted by auctions in China (Cai et al., 2013).

[10] Chen and Rosenthal (1996) note that competition among buyers does occasionally drive prices above their ceilings (i.e., list price) in housing markets. However, they note that the occurrence of explicit competition among buyers is often outside the control of the seller.

A distinguishing feature of our study is that we carefully delineate intentional and unintentional bidding wars. We define intentional bidding wars as the product of the seller's listing strategy. This type of bidding war is not necessarily a sign of latent demand. We consider a bidding war intentional if the seller sets a list price below the expected transaction price. The low list price is meant to attract multiple buyers who bid against each other to push the transaction price above the list price. Intentional bidding wars are the primary focus of Bucchianeri and Minson (2013). In contrast, we are primarily interested in *unintentional* bidding wars. Unintentional bidding wars are market driven. They are not the product of a listing strategy. We interpret their occurrence as a strong signal of housing demand within the school zone.

Since we are unable to identify whether a property was intentionally underpriced, we construct an underpriced indicator variable, $Underprice_{it}$, that identifies whether the seller listed house i at time t for less than the expected transaction price, $E(TP_{it})$, where

$$Underprice_{it} = \begin{cases} 0, \text{if } \max(FOLP_{it}, OLP_{it}, LP_{it}) \geq E(TP_{it}) \\ 1, \text{if } \max(FOLP_{it}, OLP_{it}, LP_{it}) < E(TP_{it}). \end{cases} \quad (5)$$

Our identification process is similar to Genesove and Mayer (2001) who estimate the expected selling price to examine loss aversion in housing markets. Following Genesove and Mayer (2001), we estimate $E(TP_{it})$ at the time of listing using a hedonic model. We use the expected transaction price to identify underpriced listings. As a robustness check, we create four additional underpriced indicator variables. The Internet Appendix provides an overview of the estimation procedure and a summary of all five measures.

Using survey data, Han and Strange (2014) find that a growing number of sales, over 30% in some markets, are involved in bidding wars. However, the authors do not delineate whether a bidding war is intentional or unintentional. Using Equation (5), we define a bidding war as intentional when $Underprice_{it} = 1$ or unintentional when $Underprice_{it} = 0$. This delineation is important because the recent rise in bidding wars gives the impression that underpricing a house to incite a bidding war is an effective listing strategy. It may, however, provide a false impression if a large portion of bidding wars are unintentional.

The descriptive statistics in Table 5 highlight several important stylized facts. First, the market share estimates of bidding wars in Han and Strange (2014) are overstated because they do not adjust the sales price for seller concessions. For example, Han and Strange (2014) estimate that 11.4% (9.0%) of all nondistressed transactions sold for more than their list price in Atlanta from 2003 to 2006 (2007–2010). In contrast, we estimate that bidding wars represent approximately 7.0% (4.8%) of the Atlanta market from 2003 to 2006 (2007–2010). Second, bidding wars' market share appears to covary with market conditions. As inventory decreases (increases) over time, bidding wars' market share increases (decreases). Third, imposing a time-on-market, $[TOM \leq 28]$, filter reduces the bidding war market share estimate by more than 50% in some years. Finally, a large fraction of the bidding wars are market driven (i.e., unintentional) rather than the product of the seller's intentional listing strategy. The Internet Appendix discusses the descriptive statistics in Table 5 in more detail.

4.2 | School quality and bidding wars

Table 6 reports the school quality coefficient estimates for 12 distinct specifications of Equation (1) where we interchange the dependent variable and school quality measures in the linear

TABLE 5 Bidding war frequency and market conditions by school year

	2001 (1)	2002 (2)	2003 (3)	2004 (4)	2005 (5)	2006 (6)	2007 (7)	2008 (8)	2009 (9)	2010 (10)	2011 (11)	2012 (12)	2013 (13)	2014 (14)
Entire sample														
Average TOM	50.2	67.6	72.4	71.9	97.0	113.3	135.9	146.9	129.5	129.6	120.3	88.2	71.6	73.3
Inventory	–	4.2	4.5	4.9	8.4	9.4	12.4	14.3	13.0	14.0	11.2	7.0	6.0	5.0
Turnover (%)	–	3.2	3.8	4.2	4.3	4.1	3.4	2.9	2.6	2.4	2.7	3.2	3.5	3.9
Premium Above LP (%)	−4.2	−4.9	−5.0	−4.5	−4.6	−5.9	−8.9	−10.3	−8.9	−10.6	−8.7	−5.4	−5.6	−5.9
Observations	5060	7054	8280	8962	9456	8529	5848	4829	4950	4510	5429	7313	8895	10,187
Panel B: % Bidding War	10.7	9.0	8.0	8.1	7.0	4.8	3.0	4.1	6.9	5.1	8.2	13.8	10.8	9.7
Average TOM	42.3	51.6	65.3	61.3	77.9	74.0	62.5	38.9	37.4	37.0	32.7	45.3	38.0	23.8
Inventory	–	4.2	5.7	5.7	9.4	9.5	14.8	12.1	10.3	13.0	9.3	6.2	6.0	4.2
Turnover (%)	–	3.2	4.0	4.4	4.7	4.4	3.6	3.2	2.8	2.3	2.8	3.0	3.4	4.0
Premium Above LP (%)	4.1	3.7	3.8	4.0	3.4	2.7	4.0	6.6	8.6	6.7	6.5	8.0	6.4	4.9
Observations	542	634	659	724	664	406	178	197	342	232	445	1,012	961	988
% Bidding War [TOM ≤ 28]	5.6	3.9	3.2	3.5	3.1	2.4	1.5	2.5	4.6	2.9	5.7	8.6	7.4	8.0
Average TOM	13.4	13.8	12.8	11.9	11.9	11.8	12.6	15.2	13.5	14.2	13.9	12.4	9.7	8.9
Inventory	–	4.0	4.5	5.7	9.3	9.4	14.3	12.0	10.7	13.4	9.1	5.9	5.5	4.0
Turnover (%)	–	3.3	4.1	4.4	4.8	4.3	3.6	3.2	2.9	2.4	2.9	3.2	3.7	4.1
Premium Above LP (%)	4.0	3.0	3.4	3.3	3.0	2.1	3.3	6.8	8.4	6.6	6.9	7.6	5.4	4.7

(Continues)

TABLE 5 (Continued)

	2001 (1)	2002 (2)	2003 (3)	2004 (4)	2005 (5)	2006 (6)	2007 (7)	2008 (8)	2009 (9)	2010 (10)	2011 (11)	2012 (12)	2013 (13)	2014 (14)
Observations	282	274	266	313	296	207	90	121	229	132	311	629	661	814
% Intentional Bidding War	2.3	1.5	1.7	2.0	1.7	1.5	0.9	1.8	3.3	2.1	3.9	5.6	3.7	4.0
Average TOM	12.0	12.2	11.7	10.8	10.9	11.0	12.8	15.0	13.7	14.8	14.7	14.3	11.1	10.6
Inventory	–	4.0	4.1	5.4	9.6	8.9	12.7	11.6	11.3	13.7	8.9	6.2	6.1	4.6
Turnover (%)	–	3.3	4.2	4.4	4.4	4.3	3.4	3.2	2.7	2.2	2.7	2.9	3.2	3.7
Premium Above LP (%)	4.2	4.0	3.5	3.4	3.3	2.0	3.8	8.5	9.9	7.7	8.4	9.8	8.1	6.7
Observations	115	108	140	183	163	126	52	86	164	95	211	410	326	410
% Unintentional Bidding War	3.3	2.4	1.5	1.5	1.4	0.9	0.6	0.7	1.3	0.8	1.8	3.0	3.8	4.0
Average TOM	14.4	14.9	14.1	13.4	13.2	13.0	12.3	15.6	13.2	12.8	12.2	9.0	8.3	7.2
Inventory	–	3.9	4.8	6.1	9.0	10.0	16.5	12.8	9.1	12.5	9.5	5.5	5.0	3.4
Turnover (%)	–	3.4	4.1	4.4	5.3	4.3	3.9	3.0	3.3	2.9	3.3	3.7	4.2	4.6
Premium Above LP (%)	3.9	2.4	3.2	3.2	2.6	2.3	2.7	2.5	4.7	4.0	3.6	3.4	2.8	2.6
Observations	167	166	126	130	133	81	38	35	65	37	100	219	335	404

Note: This table reports the mean percentage of transactions that are involved in a bidding war each school year from 2001 to 2014. The "% Bidwar" section represents the entire subsample of transactions that are involved in a bidding war. The "% Bidwar [TOM $\leq$ 28]" section includes transactions that are involved in a bidding war with a time-on-market (TOM) of 28 days or less. Inventory measures the supply of single-family detached houses available for sale in the elementary school zone. We calculate inventory as the total number of houses available for sale (i.e., listed on the MLS) during the month the house is listed divided by the average number of sales per month over the previous year. Turnover measures the demand for housing over time within the elementary school zone. We calculate turnover as the annualized average number of sales transactions over the previous 3 months divided by the housing stock.

TABLE 6 School quality and bidding wars

	Bidding war [TOM ≤ 28]		Intentional bidding war		Unintentional bidding war	
	Raw	**R&S**	**Raw**	**R&S**	**Raw**	**R&S**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Panel A						
School Quality	0.005	0.004	−0.007**	−0.007**	0.011***	0.011***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Fixed Effect	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY
Controls	✓	✓	✓	✓	✓	✓
Observations	99,302	99,302	99,302	99,302	99,302	99,302
R-squared	0.039	0.039	0.033	0.033	0.021	0.021
Panel B						
School Quality	0.006	0.009*	0.000	0.000	0.007**	0.009***
	(0.005)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)
Fixed Effect	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY
Controls	✓	✓	✓	✓	✓	✓
Tokens	✓	✓	✓	✓	✓	✓
Observations	99,302	99,302	99,302	99,302	99,302	99,302
R-squared	0.051	0.051	0.050	0.050	0.037	0.037

* $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Note: This table reports the school quality coefficient estimates from linear probability models where the dependent variable is one of three distinct bidding war variables. The dependent variable in the first section (columns (1) and (2)) represents all bidding wars with a time-on-market less than or equal to 28 days. The middle section (columns (3) and (4)) includes a subset of bidding wars that are intentionally underpriced. The final section (columns (5) and (6)) includes the complementary subset of bidding wars that are market driven (i.e., not underpriced). Coefficient estimates are reported for two school quality measures using their 3-year rolling averages. Columns (1), (3), and (5) display estimates for the normalized raw CRCT scores and columns (2), (4), and (6) display estimates for the R&S school quality measure. Panels A and B differ only in the fixed effects that are employed. Panel A uses additively separable zip code and school year fixed effects, whereas Panel B uses multiplicatively separable zip code and school year fixed effects. Every column includes the full set of time-invariant and time-varying controls listed in the Internet Appendix.

probability model. The dependent variable in columns (1) and (2) identifies all transactions involved in a bidding war that sold within 28 days. In the middle (far right) section, the dependent variable identifies a subset of transactions that were involved in a bidding war, sold within 28 days, and were (were not) intentionally underpriced. To demonstrate the robustness of the results, we estimate the coefficients using both the normalized raw (columns (1), (3), and (5)) and R&S (columns (2), (4), and (6)) measures of school quality.

The results in columns (1) and (2) suggest that bidding wars are not correlated with school quality. However, the results in the middle and far right sections suggest that the correlation is obfuscated when the type of bidding war is not delineated. The estimates in columns (3) and (4) suggest that bidding wars in which the seller intentionally underprices the listing are more (less) likely to occur in lower (higher) quality school zones. In contrast, the estimates in columns (5) and (6) suggest that market-driven (i.e., unintentional) bidding wars are more (less) likely to occur in higher (lower) quality school zones. The results lend support to our conjecture that there is latent demand for housing in high-quality school zones.

4.3 | Redistricting and market-driven bidding wars

Although the results in the preceding subsection establish a link between bidding wars and school quality, we cannot claim that this relationship is causal. The reason is that houses in higher quality school zones differ along observed (and most likely unobserved) dimensions. To establish causality, we estimate Equation (2) using the quasi-experimental research design outlined earlier in our empirical strategy. In essence, we show that the change in the probability of an unintentional bidding war is directly related to the exogenous shock to school quality (S^*) associated with the redistricting of house i from school zone z' to school zone z.

Panels A and B of Table 7 use additive (Z+SY) or multiplicative (Z×SY) zip code and school year fixed effects, respectively.[11] Note, however, that the zip code is differenced out when Z+SY fixed effects are included in Equation (2). The first four columns use the shock to the normalized raw school quality measure associated with the redistricting, whereas columns (5)–(8) use the shock to the R&S school quality measure. Columns (1), (2), (5), and (6) include all redistricted transactions regardless of how long after the redistricting they occur. The only difference between columns (1) (5) and (2) (6) is that column (2) (6) includes textual information from the MLS remarks section and column (1) does not. Since the shock to school quality should have a stronger effect the closer the transaction is to the redistricting event, columns (3) and (7) drop repeat sales where the postredistricting transaction occurs more than 3 years after the house is redistricted. Columns (4) and (8) remove every transaction in the APS district and drop repeat-sales pairs in which the postredistricting transaction occurs in a new school zone during its first year of existence (i.e., when a test score is not publicly available).

Regardless of the school quality measure or fixed effects employed, the coefficient estimates are positive and statistically significant. Since the school quality measures are normalized, the coefficients represent the percent change in the variable of interest associated with a one-standard-deviation change in school quality. A one-standard-deviation positive (negative) shock

[11] The use of Z×SY fixed effects helps control for local market conditions across space and over time (i.e., market thickness, hot vs. cold), thereby allowing us to isolate the effect of a shock to school quality on the four measures of housing demand. Furthermore, the use of house fixed effects helps control for unique characteristics of the property that may influence the arrival of a buyer with a particularly strong idiosyncratic taste for that property.

TABLE 7 School quality's causal effect on unintentional (market-driven) bidding wars

	Normalized raw				R&S			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A								
School Quality Shock	0.098***	0.097***	0.108**	0.119**	0.093***	0.094***	0.115***	0.107**
	(0.032)	(0.032)	(0.048)	(0.047)	(0.028)	(0.028)	(0.042)	(0.047)
Fixed Effect	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY
House FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Tokens		✓	✓	✓		✓	✓	✓
Recent			✓	✓			✓	✓
School Districts	3	3	3	2	3	3	3	2
Observations	23,753	23,753	22,233	18,075	23,753	23,753	22,233	18,075
R-squared	0.475	0.494	0.498	0.502	0.475	0.494	0.498	0.502
Panel B								
School Quality Shock	**0.078****	**0.070****	**0.092***	**0.104****	**0.062****	**0.061****	**0.093****	**0.108****
	(0.032)	**(0.032)**	**(0.048)**	**(0.047)**	**(0.030)**	**(0.030)**	**(0.045)**	**(0.052)**
Fixed Effect	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY
House FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Tokens		✓	✓	✓		✓	✓	✓
Recent			✓	✓			✓	✓
School Districts	3	3	3	2	3	3	3	2
Observations	23,753	23,753	22,233	18,075	23,753	23,753	22,233	18,075
R-squared	0.510	0.528	0.532	0.535	0.510	0.528	0.532	0.535

* $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Note: The dependent variable in every column is an unintentional bidding war indicator variable that equals 1 when the transaction is involved in a bidding war, is not underpriced, and has a time-on-market of 28 days or less, or zero otherwise. Columns (1)–(4) use the shock to the normalized raw school quality measure that results from the redistricting and columns (5)(8) use the shock to the R&S school quality measure. Columns (1), (2), (5), and (6) include all redistricted transactions regardless of how long after the redistricting the transaction takes place. Columns (3) and (7) remove repeat-sales pairs in which the postredistricting transaction takes place more than 3 years after the property is redistricted. Columns (4) and (8) filter out transactions in the APS district and the first year of transactions for houses that are redistricted to a new school. Transactions that occur in the second and third years (i.e., when a test score is available) are included. Every column includes house fixed effects and a set of time-varying listing and sales controls. The controls are provided in the Internet Appendix. Standard errors clustered at the house level are reported in brackets.

to school quality increases (decreases) the probability of an unintentional, market-driven bidding war by approximately 3.5% in columns (4) and (8) of Panel B.[12] Since the sample average is 2%, a one-standard-deviation shock to school quality almost triples the likelihood of a market-driven bidding war.

The Internet Appendix provides a series of robustness checks that further examines the causal relationship between school quality and bidding wars. Instead of using a continuous measure of the school quality shock, the robustness check includes indicator variables for postredistricting transactions where school quality improves ($S^* \geq 5\%$) or declines ($S^* \leq -5\%$) as a result of the

[12] The normalized raw (R&S) school quality measure has a standard deviation of 32.4 (32.0).

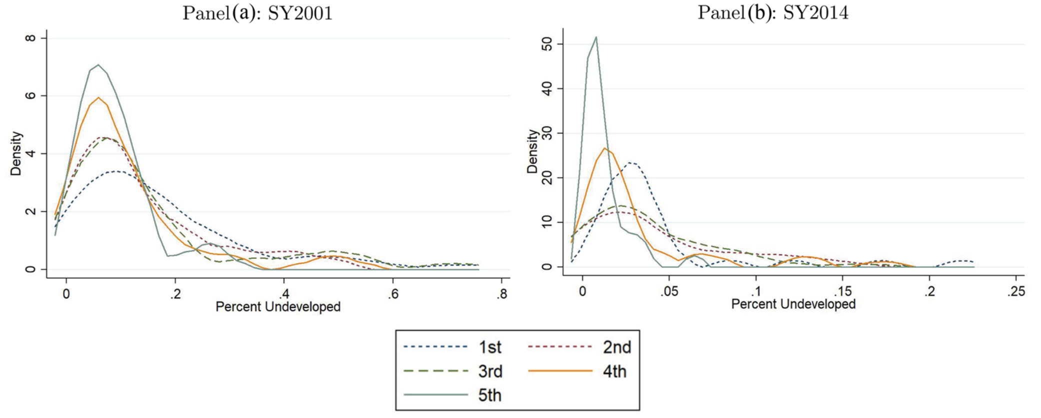

FIGURE 2 Percent undeveloped parcels by school quintile
[Color figure can be viewed at wileyonlinelibrary.com]

Note: Elementary schools are placed into a quintile based on their normalized raw test score. The schools are sorted in ascending order and placed in the appropriate quintile, so the first quintile includes the elementary schools with the lowest test scores and the fifth quintile includes the elementary schools with the highest test scores. Panel A plots the density estimates for the percent of undeveloped parcels in the school zone in 2001. Panel B provides the same plot using 2014 data. Note that the density (vertical) and percent undeveloped (horizontal) scales are different in Panels A and B.

redistricting. Similar to the results in Table 7, the coefficient estimates show that a positive shock to school quality increases the probability of an unintentional, market-driven bidding war by 2.5–3.1%. Although not the focus of this study, we also examine whether shocks to school quality have a causal effect on intentional bidding wars. The results in Table 6 suggest that intentional bidding wars are correlated with lower quality school zones. However, results in the Internet Appendix provide no evidence that a causal relationship exists between school quality and intentional bidding wars.

5 | SCHOOL QUALITY AND THE BUILT ENVIRONMENT

Housing demand can be met in one of two ways: resale of existing stock or new development. If the quality of local schools is a crucial determinant of housing demand, then neighborhoods located in higher (lower) quality school zones should be more (less) developed. Figure 2 examines the supply elasticity and amount of new residential development from the start (SY2001) to the end (SY2014) of our study period. Panel A plots density estimates of the percent of undeveloped parcels within a school zone by its school test score quintile in 2001. The first quintile represents school zones with the lowest test scores, and the fifth quintile represents school zones with the highest test scores. Not surprisingly, the fourth and fifth quintiles have a relatively lower percentage of undeveloped parcels.

Panel B displays a similar density plot for SY2014. Since the school zones and their test scores change over time, we allow the composition of the test score quintiles to change. Panel B highlights the correlation between school quality and new development during the study period. The fifth quintile has the highest density of developed parcels, indicating that a large portion of the undeveloped parcels in SY2001 were developed during the study period.

TABLE 8 The effect of school quality on timing of development

	Normalized raw			R&S			Indicator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School Quality Shock	0.981***	0.979***	1.055***	1.045***	1.043***	1.058***		
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)		
Distance to CBD	1.014***	1.013***	1.015***	1.017***	1.017***	1.017***	1.015***	1.014***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Parcel Size	0.999***	0.999***	0.999***	0.999***	0.999***	0.999***	0.999***	0.999***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Redistricted_t							0.898***	0.982
							(0.021)	(0.023)
$\text{Redistricted}_{t-1}$							1.054**	1.057**
							(0.026)	(0.027)
$\text{Redistricted}_{t-2}$							1.121***	1.143***
							(0.022)	(0.034)
$\text{Redistricted}_{t-3}$							1.180***	1.092***
							(0.027)	(0.030)
Fixed Effect	District	District	District	District	District	District	District	District
Recent		✓	✓		✓	✓		
School Districts	3	3	2	3	3	2	3	2
Observations	403,048	403,048	310,077	403,048	403,048	310,077	403,048	310,077
Log-likelihood	−464,551	−464,529	−366,492	−462,504	−463,274	−365,427	−464,649	−366,792

* $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Note: This table reports the school quality coefficient estimates from a proportional hazard model. The coefficients are reported in exponentiated form and the standard errors reported in parenthesis are bootstrapped with 50 repetitions. Coefficient estimates are reported for the shock to two school quality measures using their 3-year rolling averages. Columns (1)–(3) display estimates for the shock to the normalized raw CRCT scores as a result of the redistricting and columns (4)–(6) display estimates for the shock to the R&S school quality measure. The school quality shock variable in columns (2), (3), (5), and (6) is nonzero only if the redistricting occurs within the last 3 years. Columns (3) and (6) filter out transactions in the APS district and the first year of transactions for houses that are redistricted to a new school.

5.1 | Redistricting and housing development

Figure 2 shows that a considerable amount of new development occurred in the highest quality school zones. However, we cannot conclude new development occurred solely because the parcels are in higher quality school zones. Several other plausible explanations exist such as the distance to the central business district and lot size, that are likely correlated with high-quality schools. To control for these other plausible explanations and establish a causal relationship, we use the quasi-experimental research design outlined earlier in our empirical strategy.

Table 8 presents the results for several specifications of Equation (5). The parameter estimates in every column assume an exponential distribution that imposes a constant proportional hazard: $h_0(t) = \lambda$. Columns (1)–(3) include the raw normalized school quality shock, and columns (4)–(6) include the R&S school quality shock. We also examine the timing of the new development relative to the redistricting in columns (7) and (8). The coefficients in Table 8 are exponentiated, so a coefficient greater (less) than 1 suggests that an increase in the covariate increases (decreases) the probability of development. For example, a coefficient of 1.01 implies that a one-standard-deviation change in the covariate increases the probability of development 1%.

Since the estimates reported in Table 8 are all significant, we reject the null hypothesis that the covariates chosen do not affect the timing of development. Of particular interest are the coefficient estimates for the shock to school quality in columns (1)–(6). The results for the normalized raw school quality shock are mixed. The results in columns (1) and (2) suggest that a positive shock to school quality decreases the probability of development. However, after we remove transactions in the APS district, the results suggest that a positive shock to school quality significantly increases the probability of development that aligns with the R&S coefficient estimates in columns (4)–(6).

Columns (7) and (8) provide insight into the timing of development associated with the school quality shock estimates in columns (1)–(6). We find that the probability of development does not increase immediately after redistricting. This makes intuitive sense, given that the residential housing development process often takes just under a year from permit to completion. The results in columns (7) and (8) show the probability of development increases a year after redistricting. These findings suggest that the existing housing stock initially accommodates the increase in demand until new development is completed in subsequent periods.

The Internet Appendix provides a series of robustness checks that further examine the causal relationship between school quality and residential development. Instead of using a continuous measure of the school quality shock, the robustness checks include indicator variables that identify when school quality improved ($S^* \geq 5\%$) or declined ($S^* \leq -5\%$) as a result of redistricting. Consistent with the results in Table 8, the coefficient estimates suggest that a positive shock to school quality significantly increases the probability of new residential development.

6 | HOUSE PRICES AND LIQUIDITY

Tables 9 and 10 use Equation (2) to examine school quality's causal effect on house prices and time-on-market, respectively. In both tables, Panel A uses Z+SY additive fixed effects and Panel B uses Z×SY multiplicative fixed effects. The first four columns use the shock to the normalized raw school quality measure associated with the redistricting, whereas columns (5)–(8) use the shock to the R&S school quality measure. Columns (1), (2), (5), and (6) include all redistricted transactions regardless of how long after redistricting they occur. Columns (3) and (7) remove repeat-sales pairs in which the postredistricting transaction is more than 3 years after the house is redistricted. Columns (4) and (8) remove all transactions in the APS district and the first year of transactions associated with new schools (i.e., transactions with no publicly available test score). In unreported results, we also filter out every transaction that involved a house that is redistricted to a new school. The results are similar regardless of the filter.

Table 9 reports the results when the log of transaction price is the dependent variable in Equation (2). The results in Panel A suggest that a one-standard-deviation positive (negative) shock to school quality results in an 11.9% increase (decrease) in transaction prices using the normalized raw measure in column (4) and an 11.7% increase (decrease) using the R&S measure in column (8). Panel B further controls for time-varying neighborhood attributes using zip code by school year (Z×SY) fixed effects. The results in Panel B suggest that a one-standard-deviation positive (negative) shock to school quality leads to a 3.7% increase (decrease) in transaction prices using the normalized raw measure in column (4) and a 3% increase (decrease) using the R&S measure in column (8). The 3% estimate in column (8) of Panel B represents an increase (decrease) of approximately $8500 in value at the mean transaction price of $278,962.

TABLE 9 School quality's causal effect on transaction price

	Normalized raw				R&S			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A								
School Quality Shock	0.411***	0.366***	0.392***	0.367***	0.509***	0.437***	0.441***	0.367***
	(0.056)	(0.049)	(0.061)	(0.076)	(0.050)	(0.044)	(0.051)	(0.065)
Fixed Effect	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY
House FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Tokens		✓	✓	✓		✓	✓	✓
Recent			✓	✓			✓	✓
School Districts	3	3	3	2	3	3	3	2
Observations	23,753	23,753	22,233	18,075	23,753	23,753	22,233	18,075
R-squared	0.941	0.957	0.957	0.958	0.941	0.957	0.958	0.958
Panel B								
School Quality Shock	**0.043**	**0.063**	**0.147****	**0.113****	**0.047**	**0.068***	**0.167*****	**0.095****
	(0.050)	**(0.045)**	**(0.063)**	**(0.053)**	**(0.041)**	**(0.038)**	**(0.052)**	**(0.045)**
Fixed Effect	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY
House FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Tokens		✓	✓	✓		✓	✓	✓
Recent			✓	✓			✓	✓
School Districts	3	3	3	2	3	3	3	2
Observations	23,753	23,753	22,233	18,075	23,753	23,753	22,233	18,075
R-squared	0.969	0.976	0.977	0.978	0.969	0.976	0.977	0.978

* $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Note: The dependent variable in every column is the log of transaction price. Columns (1)–(4) use the shock to the normalized raw school quality measure that results from the redistricting and columns (5)–(8) use the shock to the R&S school quality measure. Columns (1), (2), (5), and (6) include all redistricted transactions regardless of how long after the redistricting they take place. Columns (3) and (7) remove repeat-sales pairs in which the postredistricting transaction takes place more than 3 years after the property is redistricted. Columns (4) and (8) filter out transactions in the APS district and the first year of transactions for houses that are redistricted to a new school. Transactions that occur in the second and third years (i.e., when a test score is available) are included. Panel A uses school year fixed effects and Panel B uses school year by zip code fixed effects. Every column includes house fixed effects and a set of time-varying listing and sales controls. Standard errors clustered at the house level are reported in brackets.

The extant literature focuses on price effects even though price and liquidity are codetermined in housing markets.[13] Table 10 reports the results when time-on-market is the dependent variable in Equation (2). Time-on-market estimates are expressed in weeks to facilitate interpretation. The results in Panel A suggest that a one-standard-deviation positive (negative) shock to school quality reduces (increases) time-on-market by approximately 4.3 (3.8) weeks using the normalized raw (R&S) measure in column (4) (column (8)). The magnitude of the estimates is similar after

[13] One exception is Zahirovic-Herbert and Turnbull (2008) who examine the effect of school quality on both house prices and liquidity in Baton Rouge, Louisiana. Although Zahirovic-Herbert and Turnbull (2008) use the shock to school quality associated with redistricting, the limited timeframe (1998–2002) of their dataset precludes the use of repeat sales. Thus, their estimates are subject to an omitted variable bias.

TABLE 10 School quality's causal effect on time-on-market

	Normalized raw				R&S			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A								
School Quality Shock	−7.158**	−7.407***	−12.213***	−13.361**	−4.320*	−4.338*	−11.503***	−11.840**
	(2.880)	(2.821)	(3.817)	(5.247)	(2.340)	(2.315)	(3.184)	(4.934)
Fixed Effect	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY	Z+SY
House FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Tokens		✓	✓	✓		✓	✓	✓
Recent			✓	✓			✓	✓
School Districts	3	3	3	2	3	3	3	2
Observations	23,753	23,753	22,233	18,075	23,753	23,753	22,233	18,075
R-squared	0.545	0.567	0.569	0.574	0.545	0.567	0.570	0.574
Panel B								
School Quality Shock	**−9.756***	**−10.406***	**−11.540***	**−15.305***	**−8.588***	**−8.765***	**−12.612***	**−14.432***
	(3.057)	**(3.061)**	**(4.399)**	**(5.701)**	**(2.618)**	**(2.639)**	**(3.746)**	**(5.423)**
Fixed Effect	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY
House FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Tokens		✓	✓	✓		✓	✓	✓
Recent			✓	✓			✓	✓
School Districts	3	3	3	2	3	3	3	2
Observations	23,753	23,753	22,233	18,075	23,753	23,753	22,233	18,075
R-squared	0.581	0.602	0.605	0.612	0.581	0.602	0.605	0.612

* $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Note: The dependent variable in every column is time-on-market (in weeks). Columns (1)–(4) use the shock to the normalized raw school quality measure that results from the redistricting and columns (5)–(8) use the R&S school quality measure. Columns (1), (2), (5), and (6) include all redistricted transactions regardless of how long after the redistricting they take place. Columns (3) and (6) remove repeat-sales pairs in which the postredistricting transaction takes place more than 3 years after the property is redistricted. Columns (4) and (8) filter out transactions in the APS district and the first year of transactions for houses that are redistricted to a new school. Transactions that occur in the second and third years (i.e., when a test score is available) are included. Panel A uses school year fixed effects and Panel B uses school year by zip code fixed effects. Every column includes house fixed effects and a set of time-varying listing and sales controls. Standard errors clustered at the house level are reported in brackets.

controlling for time-varying neighborhood attributes in Panel B. A one-standard-deviation positive (negative) shock to school quality reduces (increases) time-on-market by approximately 5 (4.6) weeks using the normalized raw (R&S) measure. These findings are consistent with the results in Zahirovic-Herbert and Turnbull (2008). Moreover, given the high search and holding costs associated with housing markets, these results suggest that focusing solely on price understates the value households place on school quality.

The Internet Appendix provides a series of robustness checks that further examine the causal relationship between school quality, house prices, and liquidity. Instead of using a continuous measure of the shock to school quality, the robustness checks include indicator variables for postredistricting transactions where school quality improves ($S^* \geq 5\%$) or declines ($S^* \leq -5\%$) as a result of the redistricting. Similar to the results in Tables 9 and 10, the coefficient

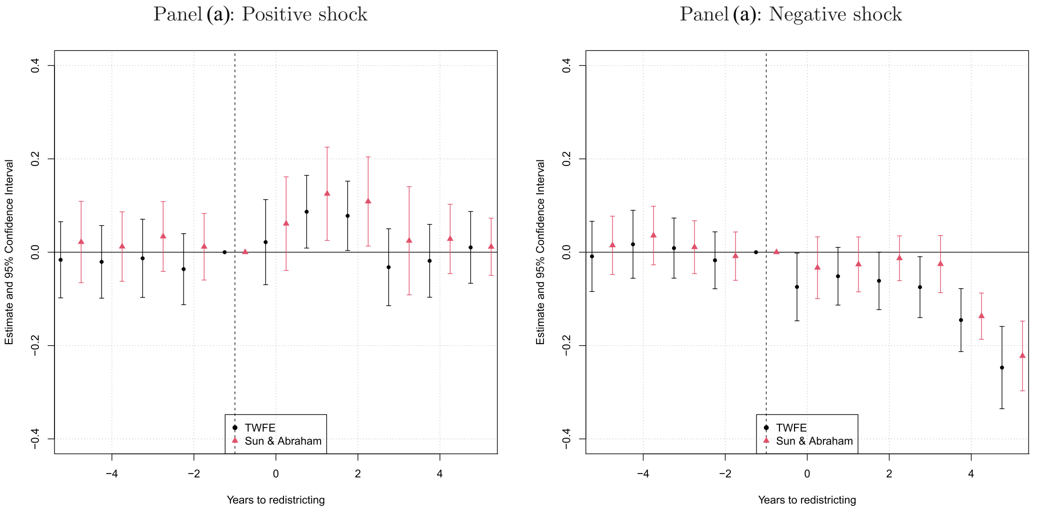

FIGURE 3 Effect of redistricting on transaction prices
[Color figure can be viewed at wileyonlinelibrary.com]
Note: Figure 3 plots event-study estimates and 95% confidence intervals for the relative-time periods of 5 years around redistricting. Panel A plots estimates for houses that received a positive school quality shock and Panel B plots estimates for houses that received a negative school quality shock. Each panel plots staggered TWFE estimators using OLS (in black with circle markers) and the approach used in Sun and Abraham (2021) (in red with triangle markers).

estimates suggest a positive shock to school quality increases house prices by 2.7–3.4% and shortens time-on-market by 3.2–4.9 weeks.[14]

6.1 | Parallel trends assumption

The results we report might be biased if the demand for houses redistricted to higher quality schools was trending differently than those redistricted to lower quality schools or not redistricted at all. For example, suppose that house prices in neighborhoods redistricted to higher quality schools were already rising before the redistricting. In that case, the coefficient for the shock to school quality (τ^*) in Equation (2) may be positive even though the redistricting per se did not cause house prices to increase. We include neighborhood by time fixed effects, which control for local market conditions in Equation (2) to partially mitigate this concern.

To further justify the parallel trend assumption, we run a staggered treatment two-way fixed effects (TWFE) estimation. Specifically, we regress the log of transaction price on interaction terms between an indicator for being in the redistricted treatment group and the number of years to treatment, controlling for house fixed effects and year fixed effects. We run the estimation separately for redistricting events in which houses received a positive school quality shock ($S *> 0$) and a negative school quality shock ($S *> 0$).

Panels A and B of Figure 3 present the event-study plots for houses that received positive and negative shocks, respectively. Although TWFE regressions are the standard way to test for

[14] The Internet Appendix further examines the relationship between school quality and house prices using a standard hedonic model, the BDD approach, and an approach similar to Ries and Somerville (2010). Regardless of the approach employed, the school quality estimates are positively and significantly associated with house prices.

parallel trends in staggered event-study research designs, they have been shown to deliver consistent estimates only under relatively strong assumptions about homogeneity in treatment effects (Callaway & Sant'Anna, 2021; De Chaisemartin & d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021). Consequently, we also employ the Sun and Abraham (2021) estimator approach to address concerns about heterogeneous treatment effects and variation in treatment timing in addition to plotting staggered TWFE estimators using ordinary least squares (OLS).

Overall, Figure 3 shows that the estimates are consistent with the parallel trends assumption. Regardless of the estimator used, the coefficients on the years prior to redistricting are all close to zero and exhibit no discernible pretrends. Figure 3 also sheds light on the dynamics of treatment effects: house prices increase (at least initially) when houses receive a positive school quality shock, whereas house prices decline when houses receive a negative school quality shock.

7 | HIGHLY EXOGENOUS REDISTRICTING

Since we recognize that redistricting decisions could be endogenous to variations in housing demand, we identify and run subsample analyses on a highly exogenous subset of redistricted transactions. Specifically, we exploit the fact that opening and closing schools frequently affects attendance boundaries in neighboring school zones. We find roughly 39% of the redistricted transactions involve houses that were redistricted to a different school even though the school they were previously assigned to remained open.

Table 11 examines the causal effect of school quality on the probability of an unintentional bidding war (Panel A), transaction prices (Panel B), and time-on-market (Panel C) using the highly exogenous subsample of redistricted transactions. The results indicate that a positive (negative) shock to school quality increases (decreases) house prices and the probability of an unintentional bidding war, while decreasing (increasing) time-on-market. However, the coefficient estimates are no longer statistically significant when we restrict the sample further by removing transactions in the APS district and new school zones in columns (4) and (8) of Table 11. This is probably due to the reduced number of remaining redistricted transactions.

8 | SPILLOVER EFFECTS

This section examines whether redistricting has spillover effects on the demand for incumbent housing (i.e., houses already assigned to the receiving school zone). Does the positive (negative) shock from redistricting have an adverse (advantageous) effect on incumbent houses in the receiving school zone? Ex-ante, the spillover effect is unclear. Students redistricted from lower to higher performing schools may see their academic performance increase to the level of the incumbent students. In this scenario, school quality is unaffected; redistricting likely has little to no spillover effect on demand for incumbent housing. However, redistricted students may not realize gains in scholarship necessary to raise their performance to the level of the incumbent students. In this instance, school quality decreases, and the redistricting may negatively affect demand for incumbent housing.

We identify the existing school zones that receive redistricted houses to estimate the spillover effect. We exclude all new school zones because they do not have existing housing. We also exclude repeat-sales pairs that are redistricted into the school zone (i.e., the original treatment group). Our new treatment group is incumbent houses with transactions before and after redistricting.

TABLE 11 Exogeneous subsample analyses

	Normalized raw				R&S			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Bidding wars								
School Quality Shock	0.090**	0.080*	0.091*	0.066	0.086**	0.081**	0.090*	0.074
	(0.040)	(0.041)	(0.048)	(0.052)	(0.040)	(0.040)	(0.049)	(0.059)
Panel B: Transaction price								
School Quality Shock	0.140**	0.166***	0.150***	0.081	0.098**	0.125***	0.118**	0.072
	(0.056)	(0.050)	(0.057)	(0.060)	(0.043)	(0.040)	(0.046)	(0.049)
Panel C: Time-on-market								
School Quality Shock	−12.254**	−13.082***	−13.727***	−7.623	−9.003**	−9.717***	−11.621***	−6.027
	(4.815)	(4.731)	(5.286)	(5.810)	(3.745)	(3.730)	(4.404)	(5.538)
N	21,614	21,614	21,227	17,437	21,614	21,614	21,227	17,437
Fixed Effect	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY
House FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Tokens		✓	✓	✓		✓	✓	✓
Recent			✓	✓			✓	✓
School Districts	3	3	3	2	3	3	3	2

* $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Note: Table 11 examines the effect of school quality shocks from redistricting on housing demand using a highly exogenous subsample of houses that were redistricted even though their previous school was not closed. Coefficient estimates for unintentional bidding wars, the log of transaction price, and time-on-market (weeks) are presented in Panels A–C, respectively. Columns (1)–(4) use the shock to the normalized raw school quality measure that results from the redistricting and columns (5)–(8) use the shock to the R&S school quality measure. Columns (1), (2), (5), and (6) include all redistricted transactions regardless of how long after the redistricting the transaction takes place. Columns (3) and (7) remove repeat-sales pairs in which the postredistricting transaction takes place more than 3 years after the property is redistricted. Columns (4) and (8) filter out transactions in the APS district and the first year of transactions for houses that are redistricted to a new school. Transactions that occur in the second and third years (i.e., when a test score is available) are included. Every column includes house fixed effects and a set of time-varying listing and sales controls. The controls are provided in the Internet Appendix. Standard errors clustered at the house level are reported in brackets.

Incumbent houses that do not have both a pre- and postredistricting transaction are dropped. The incumbent houses represent 7.4% of the remaining sample or 703 repeat-sales pairs. The preredistricting transaction of an incumbent house is assigned a school quality shock of zero. The postredistricting transaction of an incumbent house is assigned the school quality shock that houses redistricted into the school zone receive, thus allowing us to test for spillover effects on incumbent housing.

The results in Table 12 indicate that there is no spillover impact on the probability of an unintentional bidding war (Panel A), transaction prices (Panel B), or time-on-market (Panel C) for incumbent housing. Thus, incumbent houses in redistricted school zones are not adversely (advantageously) affected when houses from a lower (higher) quality school zone are redistricted into their school zone. This finding is not surprising. Redistricting does not change the underlying educational infrastructure the incumbent houses are assigned to. These results support Brummet (2014) finding that closing schools and displacing students to higher performing schools positively affects the displaced students' achievement but creates modest spillover effects on the receiving schools.

TABLE 12 Spillover effects on incumbent housing

	Normalized raw				R&S			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Bidding wars								
School Quality Shock	0.024	0.018	0.006	0.003	0.014	0.006	−0.004	−0.005
	(0.023)	(0.025)	(0.023)	(0.025)	(0.025)	(0.026)	(0.026)	(0.028)
Panel B: Transaction price								
School Quality Shock	0.035	0.033	0.030	0.022	0.046	0.041	0.039	0.032
	(0.030)	(0.025)	(0.026)	(0.027)	(0.035)	(0.030)	(0.031)	(0.032)
Panel C: Time-on-market								
School Quality Shock	−1.361	−0.558	−1.087	−0.516	−1.976	−0.695	−1.404	−0.963
	(2.543)	(2.577)	(2.645)	(2.839)	(2.843)	(2.899)	(2.984)	(3.268)
N	19,009	19,009	18,854	15,875	19,009	19,009	18,854	15,875
Fixed Effect	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY	Z×SY
House FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Tokens		✓	✓	✓		✓	✓	✓
Recent			✓	✓			✓	✓
School Districts	3	3	3	2	3	3	3	2

* p<0.1; $^{**}p$<0.05; $^{***}p$<0.01.

Note: Table 12 examines whether housing demand changed for incumbent housing in receiving school zones that were affected by redistricting. Coefficient estimates for unintentional bidding wars, the log of transaction price, and time-on-market (weeks) are presented in Panels (A)–(C), respectively. Columns (1)–(4) use the shock to the normalized raw school quality measure that results from the redistricting and columns (5)–(8) use the shock to the R&S school quality measure. Columns (1), (2), (5), and (6) include all redistricted transactions regardless of how long after the redistricting the transaction takes place. Columns (3) and (7) remove repeat-sales pairs in which the postredistricting transaction takes place more than 3 years after the property is redistricted. Columns (4) and (8) filter out transactions in the APS district and the first year of transactions for houses that are redistricted to a new school. Transactions that occur in the second and third years (i.e., when a test score is available) are included. Every column includes house fixed effects and a set of time-varying listing and sales controls. The controls are provided in the Internet Appendix. Standard errors clustered at the house level are reported in brackets.

9 | CONCLUSION

Research shows that skill formation is a dynamic process in which early inputs strongly affect the productivity of inputs later in the life cycle (Betts, 1995; Deming, 2009; Dobbie & Fryer Jr, 2014; Garces et al., 2002; Heckman, 2006). Therefore, it is not surprising that parents want to enroll their children in the highest quality school possible during their formative years. We show competition for housing in the highest quality school zones manifests in bidding wars. This amplified competition increases both house prices and liquidity, thereby incentivizing the development of empty parcels.

Although the extant literature generally agrees that school quality is a crucial determinant of housing demand, previous empirical results are mixed. To mitigate the fact that higher quality schools tend to be located in better neighborhoods, we use a quasi-experimental research design that exploits the continual redistricting of public-school attendance boundaries over a 14-year period in Atlanta. By identifying houses that transact before and after a redistricting event, we control for unobserved time-invariant house and neighborhood attributes. The repeat-sales approach does not, however, control for unobserved time-varying attributes. To address this concern, we incorporate textual information about the property from the remarks section of the MLS.

Using four distinct measures of housing demand, we provide new evidence that school quality directly affects not only housing market outcomes, but also the housing markets themselves (i.e., the built environment). More specifically, we find that houses are more (less) likely to be involved in a bidding war and undeveloped parcels are more (less) likely to be built on soon after they are redistricted to a higher (lower) quality school. These new findings provide strong evidence of parental demand for high-quality schools, of which newly developed housing satisfies a portion. This is the first study to provide empirical evidence that school quality acts as a catalyst for bidding wars and new housing development. We also find that a positive one-standard-deviation shock to school quality leads to a 3% increase in transaction price and a 4.6-week reduction in time-on-market. Given that price and liquidity are codetermined in housing markets, the liquidity effect we document suggests that focusing solely on price underestimates school quality's effect on housing demand.

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