House Prices and School Choice: Evidence from Chicago's Magnet Schools Proximity Lottery

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Abstract

Studies of open school policies predict house prices to rise in areas that gain access to high-quality schools. However, excess demand may limit access to high-quality schools. We take advantage of changes in Chicago's schools admissions policies to test whether a higher probability of admission to magnet schools for students living within 1.5 miles leads to higher house prices. Results indicate that the 1997 and 2009 reforms increased house prices for homes within the 1.5-mile radius by about 4.0% and 12.6%, respectively. The premium is higher for relative low-priced, large homes in areas with multiple magnet schools. Effects are significantly higher for more highly ranked magnet schools. The higher probability of admission for black students after a consent decree was vacated in 2009 led to a significant increase in prices in predominantly African-American areas on the south side.

1. Introduction

School choices are typically tied to residential location decisions in the United States. Particularly at the elementary school level, enrollments are apt to be restricted to students living within relatively small neighborhoods near the school. One result of these restrictive enrollment policies is that households who value education will be willing to pay a premium to live in districts with high-quality schools. The empirical literature on the capitalization of school quality into house prices is sufficiently large to have generated two recent reviews (Machin, 2011 and Nguyen-Hoang and Yinger, 2011), both of which conclude that households are willing to pay a significant premium to live in neighborhoods with schools whose students have high test scores.

In contrast to these closed enrollment policies, many districts offer a form of open enrollment. Students may have the option to attend any school within their district or sometimes even in another school district. The theoretical literature on open enrollment suggests that house prices will rise in areas with lower-quality schools, while house prices decline in areas whose schools receive large numbers of outside students. Reback (2005) found evidence supporting both predictions in a study of Minnesota school districts following the adoption of an inter-district open enrollment policy. Brunner, Cho, and Reback (2012) also find evidence supporting these predictions using data from 12 states that had adopted inter-district choice programs as of 1998. Analyses of intra-district open enrollment policies reach similar conclusions: house prices rise in areas with low-quality schools and house prices fall in areas with high-quality schools (Machin and Salvanes, 2016; Schwartz, Voicu, and Horn, 2014).

The question addressed in these papers is whether the presence of school choice is capitalized into house values. Choices are more valuable if they can clearly be granted. In some districts, access to high-quality schools is limited by enrollment caps. A mechanism is required to allocate enrollment when there is excess demand for a school. Local students typically get priority, while out-of-district admissions may be determined by some form of lottery. The only study to directly address the issue of the probability of admission to schools on house values is Andreyeva and Patrick (2017). However, their study focuses on charter schools rather than public schools.[1]

In this study, we analyze the effects of school choice on house values within a single, large school district, the Chicago Public School (CPS) district. Chicago designated a set of magnet schools in response to a desegregation order in 1980. Although any Chicago student could potentially enroll, a citywide lottery system was used to grant admission. The lottery included minority quotas, but students living near a school were not given priority. A reform affecting only elementary schools was introduced in December 1997 assigning higher probabilities of admission to students who lived within 1.5 miles of a magnet elementary school. Higher probability of admission was also granted to students with a sibling already attending the school. Another round of reform was introduced in December 2009 after the desegregation order was rescinded. The 2009 reform increased the percentage of seats that could be assigned based on proximity to the school from 30% to 40%, and it removed the restriction on the number of seats that could be assigned to siblings. Perhaps most significantly, the 2009 reform removed a bias that

[1] The empirical strategy in Andreyeva and Patrick (2017) focuses on the timing and location of charter school openings and the location of priority zones that confer different admission probabilities, both of which may cause endogeneity problems if they are influenced by parents or school boards. In contrast, the 1.5-mile rule introduced in the Chicago's magnet school system is clearly exogenous to these decisions.

increased the probability of admission for white students. The effect of these reforms was to significantly increase the probability of admission for a student living within 1.5 miles of a magnet elementary school.

These two reforms serve as a natural experiment allowing us to determine whether the higher probability of potential admission is capitalized into house prices. Following an approach taken by Black (1999) and by a host of subsequent authors, we compare house prices on either side of the 1.5-mile boundary to determine whether the reforms altered home values significantly.[2] In addition to a standard set of housing characteristics, we include a full set of census tract fixed effects to control for unobserved neighborhood characteristics. We find that the 1997 reform increased property values within the 1.5-mile zone by approximately 4.0%. The 2009 reform had an even larger effect of about 12.6%, although in this recessionary period the main effect was to greatly reduce the amount by which prices fell. Price increases are higher for large homes in locations with multiple magnet schools. We also find that the increases in prices are higher for relatively low-priced homes, conditional on housing characteristics, time of sale, and location. In particular, the 2009 reform led to significant increases in house prices for magnet schools in primarily African-American neighborhoods on Chicago's south side.

Although our study's main contribution to the literature is to establish that geographically-based admission probabilities have a significant effect on house prices, our results also have implications for the literature on school quality. Magnet schools are

[2] Example of studies using geographical discontinuities to estimate causal effects of schools on house values include Agarwal, et al. (2016); Andreyeva and Patrick (2017); Bayer, Ferreira, and McMillan (2007); Bogart and Cromwell (2000); Fack and Grenet (2010); Gibbons and Machin (2003); Gibbons, Machin, and Silva (2013); Ries and Somerville (2010); and Schwartz, Voicu, and Horn (2014).

relatively high-quality schools, and although admission is not restricted to high-achieving students, the students who are attracted to magnet schools are those who place a high value on education. Thus, our study supports recent work by such authors as Barrow and Rouse (2004); Brasington and Haurin (2006); Clapp, Nanda, and Ross (2008); Kane, Rieg, and Staiger (2006); and Kane, Staiger, and Samms (2003) showing that school quality and academic performance affects property values. Studies such as Andreyeva and Patrick (2017) and Chung (2015) find greater capitalization in areas with low-performing public schools. Similarly, we find larger effects for relatively high-quality magnet schools, although we also find strong effects in locations with relatively highly ranked public schools.

The study proceeds as follows. First, we summarize the history of the admissions policies for Chicago's magnet schools. Next, we present the data, our base empirical approach, and the results of standard difference in differences hedonic regressions. We then present results using a repeat sales modeling approach. Next, we examine the results of placebo tests using the difference in difference hedonic approach. We then show how the results vary across individual magnet schools. Finally, we present the results of quantile regression versions of the difference in difference estimates.

2. Chicago's Magnet Schools

The history of Chicago's magnet school system is discussed in Allensworth and Rosenkranz (2000). The CPS established Chicago's magnet schools in response to a 1980 desegregation consent decree signed with the federal government. The original goal of the decree response was to increase the percentage of white students in the CPS from its level in 1980 (less than 20%) by establishing a set of high-quality schools that would attract white students. Since Chicago's neighborhoods are highly racially segregated, neighborhood-based school admissions produce racially segregated schools. The magnet schools had racial quotas ranging from 15% - 35% white. "The hope was that by offering special schools, children from all over the city would be attracted to them. Thus, a multi-racial student body could be achieved in some schools in a system that had far too many racially isolated schools due to the housing pattern segregation that existed (and still exists) in Chicago" (Allensworth and Rosenkranz, 2000, p. 7).

Elementary schools classified as "regular magnets" were created in direct response to the consent decree and were subject to the racial quotas. Another set of schools was created that was not subject to the quotas, including "scholastic academies", "regional gifted centers", "classical schools", and "academic centers". Of these, all but the scholastic academies were limited to high-achieving students. The categories of magnet high schools are similar: "traditional magnets" were subject to racial quotas, while "regional college preparatory schools" and "international baccalaureate programs" had no quotas but were limited to high-achieving students. As admissions policies for categories of magnets other than regular elementary magnets have not changed significantly over time and continue to be citywide, only regular magnets are included in our empirical analysis, and the remainder of the section focuses on this category of magnet school.

Prior to 1997, a general, citywide lottery was conducted to allocate admissions for all magnet schools, including the regular magnets. A result of the excess demand for magnet school enrollment was that students living near a school might be denied enrollment. In response, the CPS established a "proximity lottery" that reserves a portion of the enrollment slots for students living within 1.5 miles of a regular magnet school. The proximity lottery was announced in December 1997 and was implemented for the 1998 – 1999 school year. In the first school year, 15% of the enrollment slots were reserved for the neighborhood. The percentage has been 30% since the 2000 – 2001 school year. The neighborhood is defined using straight-line distance from the student's address to the school.

The proximity lottery still leaves excess demand for many regular magnet schools. To assure that siblings can attend the same school, beginning in 1997 45% of the enrollment slots were set aside for siblings of students who already attend the school. Finally, 5% of the enrollment slots were reserved for allocation at the principal's discretion. The combination of the proximity lottery and the provision for siblings provides a strong incentive for families with children to choose a home in an area within 1.5 miles of a regular magnet. Moreover, some regions fall within the requisite 1.5 miles of as many as 4 magnets. These regions are especially valuable because families can enter the proximity lottery for all schools with 1.5 miles of their home and the lotteries are independent.

In September 2009, a federal court decision vacated the desegregation consent decree.[3] In December 2009, the Chicago Board of Education approved a new admissions policy for magnet schools. The most significant changes were the elimination of race-based admissions criteria and the removal of the restriction that no more than 45% of the seats were reserved for siblings. After all siblings are enrolled, 40% of the remaining seats are now reserved for students living within 1.5 miles of a regular magnet school, regardless

[3] According to the CPS web site, whites currently account for 9.4% of enrollment. Hispanics now form the largest group, with 45.6% of total enrollment. African-Americans comprise 39.3% of total enrollment, which stood at 234,679 for elementary schools and 112,029 for secondary schools in Fall 2014. These figures are drawn from http://cps.edu/About_CPS/At-a-glance/Pages/Stats_and_facts.aspx.

of race. Proximity lotteries are conducted if the number of neighborhood applicants exceeds 40% of the available seats. However, race and ethnicity continues to matter. According to the *Chicago Public Schools Policy Manual*, "In an effort to ensure ongoing diversity in these programs, if more than 50% of the entire student body, according to the 20th day file, is comprised of students within the proximity and if more than 50% of the student body is any one racial or ethnic group, no proximity lottery will be held for that school." [4]

Students who are not admitted as a sibling or via the proximity lottery can still gain admission to a magnet school through the Citywide SES Lottery. "SES" is an acronym for "socio-economic status." A score for socio-economic status is assigned to each census tract based on six criteria – median family income, adult educational attainment, the percentage of single-parent households, the percentage of home ownership, the percentage of the population that speaks a language other than English, and a school performance variable. The school performance variable is based on ISAT scores for schools with attendance areas in the census tract. The *Chicago Public Schools Policy Manual* includes the following summary of the Citywide SES Lottery":

> "Lotteries will be conducted within each of the four SES tiers and applicants will be ranked in lottery order within each tier. If there are insufficient applicants within a tier to fill the allocated number of seats in that particular SES tier, the unfilled seats will be divided evenly and redistributed across the remaining tier(s) as the process continues. A sufficient number of offers will be made in lottery order for each SES tier to fill the seats allocated to this lottery process. The remaining applicants will be placed on an applicant wait list by SES tier. http://policy.cps.k12.il.us/download.aspx?ID=82, p. 4.

[4] http://policy.cps.k12.il.us/download.aspx?ID=82.

The new admission policy was implemented for the 2010-2011 school year. The effect of removing the racial quotas, increasing the proximity lottery share to 40%, and expanding the number of seats allocated to siblings is to provide a strong incentive for families with children to live in areas that are within 1.5 miles of one or more magnet schools. Even if a student ultimately attends a private or parochial school, owning a home near a magnet school may serve as a form of insurance for parents that their child will not be forced to attend a low-quality school. Thus, house prices can be expected to rise for homes that are within 1.5 miles of a magnet school.

3. Data

Our primary data source is the Illinois Department of Revenue, which provided data on house sales for Chicago for 1993-2012. Information on additional variables such as lot size, building area, house age, and the number of rooms comes from the Cook County Assessor's Office. Most of our analysis focuses on two sub-periods encompassing the reform dates – 1995-2000 and 2007-2012. Focusing on these sub-periods helps to isolate the effects of the reforms and avoids complications arising from the booming housing markets of 2003-2006.

We restrict the sample to sales of Class 2 homes that are within 3 miles of an elementary regular magnet school that had opened before 1998. Cook County defines Class 2 properties as residential buildings with 6 units or fewer. Condos are also excluded from the analysis because we do not have data on structural characteristics for them. Table 1 provides the list of magnet schools included in the analysis, and Figure 1 shows their locations within the city along with 1.5 mile radius circles around them. Figure 1 also shows the Chicago Public School System's ranking for the quality of the magnet school, with red indicating high, black indicating median, and blue indicating low quality. Only 2 of the 22 magnets schools are listed as low quality, while 13 are identified as high quality. The same color coding is used for the tracts indicated in Figure 1, which represent the catchment zones for regular public elementary schools.

Descriptive statistics for the two sub-periods of data are presented in Table 2. Averages for most variables are similar for homes in areas affected by the reforms and for homes located more than 1.5 miles from a magnet school.

Table 1: List of Magnet Schools Operating in 1995-2012

Albert R Sabin Elementary Magnet School
Andrew Jackson Elementary Language Academy
Burnside Elementary Scholastic Academy
Edward Beasley Elementary Magnet Academic Center
Frank W Gunsaulus Elementary Scholastic Academy
Franklin Elementary Fine Arts Center
Galileo Math & Science Scholastic Academy Elementary School
Hawthorne Elementary Scholastic Academy
Inter-American Elementary Magnet School
Jensen Elementary Scholastic Academy
John H Vanderpoel Elementary Magnet School
LaSalle Elementary Language Academy
Leif Ericson Elementary Scholastic Academy
Maria Saucedo Elementary Scholastic Academy
Mark Sheridan Elementary Math & Science Academy
Ole A Thorp Elementary Scholastic Academy
Robert A Black Magnet Elementary School
Stone Elementary Scholastic Academy
Turner-Drew Elementary Language Academy
Walt Disney Magnet Elementary School
Walter L Newberry Math & Science Academy Elementary School
William Bishop Owen Scholastic Academy Elementary School

Figure 1: Magnet School Locations

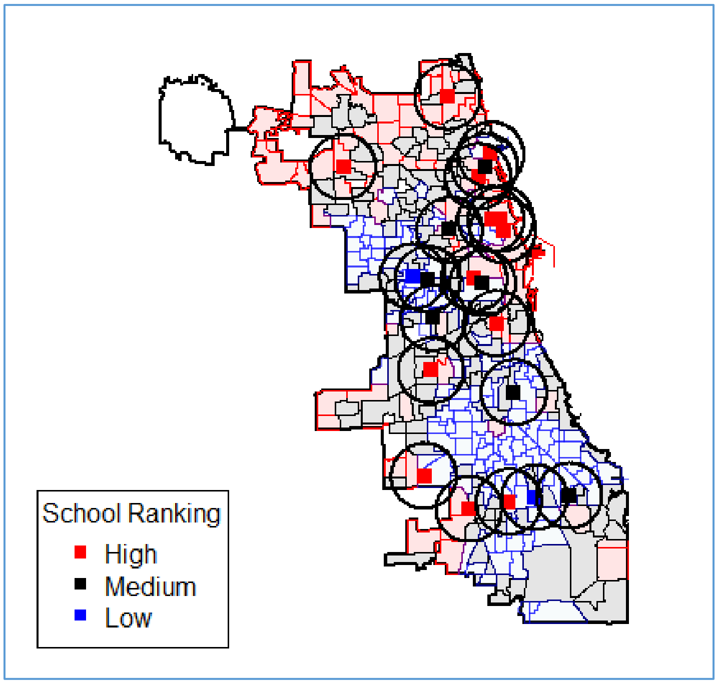


Table 2: Descriptive Statistics

	Within 1.5 Miles of a Magnet School		1.5 – 3 Miles from a Magnet School	
	1995-2000	2007-2012	1995-2000	2007-2012
Log of Sale Price	11.867 (0.620)	12.318 (1.040)	11.731 (0.512)	11.982 (0.984)
Log of Building Area	7.296 (0.401)	7.340 (0.428)	7.232 (0.394)	7.251 (0.414)
Log of Lot Size	8.162 (0.376)	8.102 (0.429)	8.263 (0.270)	8.245 (0.302)
Age	73.569 (31.410)	73.563 (35.490)	74.533 (23.774)	75.532 (25.541)
Rooms	6.623 (2.238)	6.684 (2.205)	6.394 (2.170)	6.412 (2.163)
Bathrooms	1.680 (0.712)	1.758 (0.777)	1.536 (0.620)	1.560 (0.648)
Bedrooms	3.368 (1.103)	3.392 (1.134)	3.281 (1.089)	3.305 (1.110)
Central Air Conditioning	0.233	0.247	0.159	0.157

Attic	0.394	0.375	0.429	0.410
Basement	0.698	0.655	0.724	0.703
Brick	0.635	0.637	0.599	0.590
Fireplace	0.130	0.156	0.078	0.088
1-Car Garage	0.255	0.251	0.296	0.304
2+ Car Garage	0.490	0.483	0.490	0.468
Number of Observations	30,840	19,901	32,850	19,794

Note. Standard deviations are in parentheses for continuous variables.

4. Empirical Approach

Following Black (1999) and much of the subsequent literature, we use a differences-in-differences approach to estimate the effect of the admission reforms on house prices. Letting lnP_{hct} represent the log sale price of home *h* in census tract *c* at time *t*, the basic estimating equation is

$$lnP_{hct} = \gamma_1 Treat_{ht} + \gamma_2 Treat_{ht} x Reform_t + X_{hc}\beta + \mu_c + \rho_t + u_{hct} \qquad (1)$$

for either sub-period used to evaluate the reforms (i.e., 1995-2000 or 2007-2009). We include fixed effects for the quarter of sale and the census tract, and standard errors are clustered at the tract level.[5] The estimating equations also include controls for structural characteristics, including log building area; log lot size; building age; the number of rooms, bathrooms, and bedrooms; and dummy variables for central air conditioning, an attic, a basement, brick construction, a fireplace, and a one or two car garage.[6]

Two reforms took place during our sample period, one at the end of 1997 and the other at the end of 2009. As the geographic area covered by the reforms does not differ

[5] The results are similar when elementary school districts are used as the basis for geographic fixed effects rather than census tracts. Census tracts are smaller than school districts: for our sample of sales of homes that are within 1.5 miles of a magnet school, there are 817 census tracts and 339 elementary school districts.
[6] Implicitly, the estimating equations also include controls for the *Reform97* and *Reform09* variables. However, these variables are not separately identified from the controls for quarter of sale.

over time, the *Treat* variable is the same for both reform times: *Treat* = 1 if a home is within 1.5 miles of a magnet school. To evaluate the December 1997 reform, we estimate models using data for 1995 – 2000, while we use data from 2007 – 2012 to analyze the December 2009 reform. For the 1995 – 2000 data, $Reform_t$ = 1 for $t \geq$ 1998:1, while $Reform_t$ = 1 for $t \geq$ 2010:1 for the 2007 – 2012 data.

As geographic coding can be imprecise – e.g., addresses can be measured from the center of a lot, at the street, or even in the center of the street fronting the house – we omit observations lying within a buffer of ε miles around the 1.5 mile mark. We set the buffer to $\varepsilon = 1/16$, which is half the length of a standard block in Chicago. This buffer leads to the exclusion of observations for which the distance (d) between the home and the nearest magnet school lies in the range $1.4375 < d < 1.5625$. For our base model, an observation has *Treat* = 1 if the distance from the home to the nearest magnet school lies in the range $d \leq 1.4375$, while *Treat* = 0 for observations in the range $1.5625 \leq d \leq 3$.

We vary the size of the bands around the critical 1.4375 and 1.5625 mile marks by estimating models with the sample restricted to sales of homes located in *Treat* = 1 bands of $1.5 - \delta \leq d \leq 1.5 - \varepsilon$ and Treat = 0 bands of $1.5 + \varepsilon \leq d \leq 1.5 + \delta$. In addition to our base estimates for which $\delta = 1.5$, we test three smaller bandwidths: $\delta = 1$, 0.5, and 0.25. Figure 2 illustrates this process. Smaller values of δ lead to fewer observations for both the treatment and control groups as the sample is restricted to a narrower band around the $1.5 - \varepsilon$ and $1.5 + \varepsilon$ distances. Figure 3 shows the observations in the sample for 1995 – 2000 for $\delta = 1.5$ and $\delta = 0.25$ (the results are similar for 2007 – 2012). Observations are likely to be more similar across the treatment and control groups for narrower bandwidths.

The housing characteristic variables and census tract fixed effects control for heterogeneity introduced by having the larger samples produced by wider distance bands.

A sizable portion of the sample is located in areas of the city that are within 1.5 miles of more than one magnet school. Table 3 shows the number of observations that are within a 1.5 miles of 0-4 schools for each of the four distance bands. Even when δ= 0.25, a sizable share of the treatment observations is within 1.5 miles of 2 or more magnet schools. To measure the effects of treatment intensity – the number of nearby magnet schools – on house prices, we add separate $Treat$ and $Treat\ x\ \ Reform$ variables for observations that are close to 2 or 3-4 schools.

Figure 2: Treatment and Control Definitions

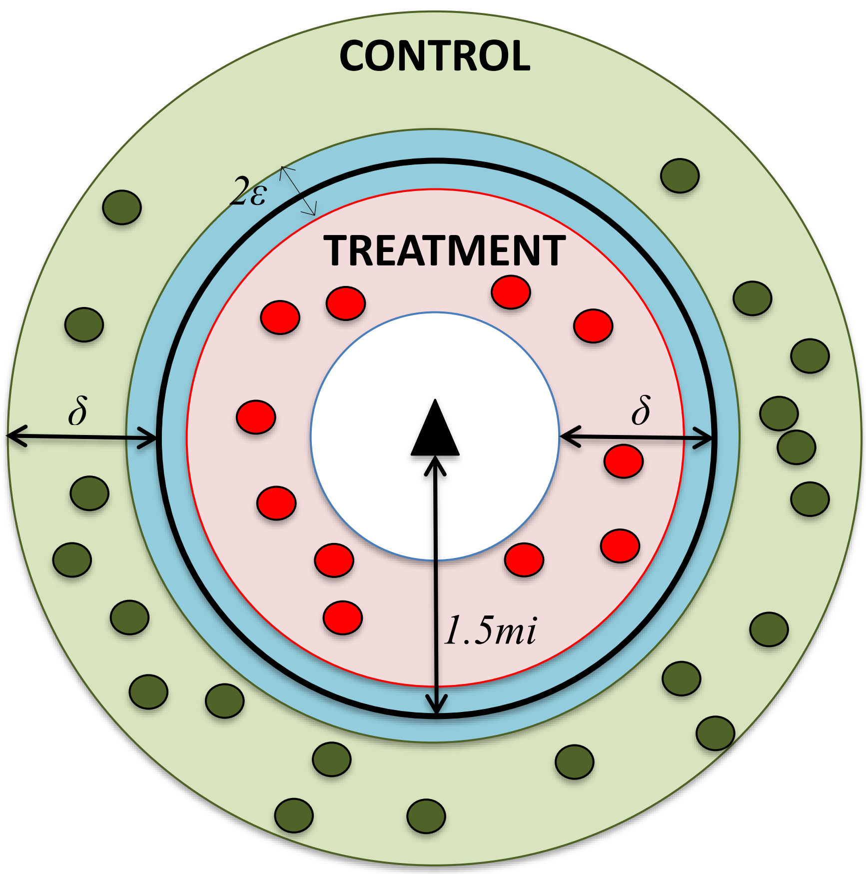
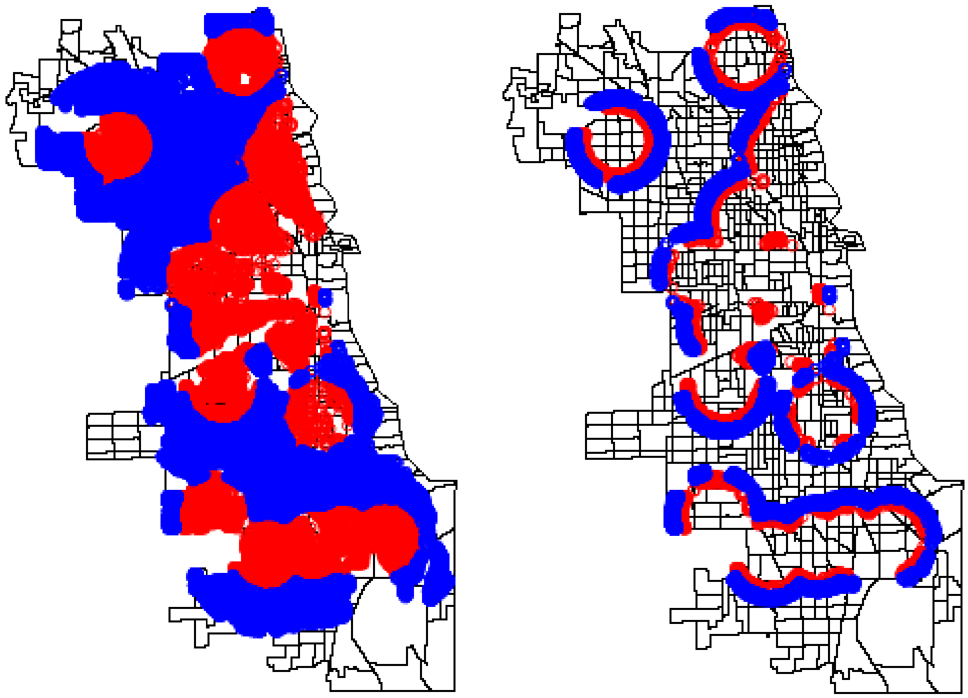


Figure 3: Treatment and Control Observations, 1995 - 2000



$\delta = 1.5$ $\delta = 0.25$

Table 3: Number of Observations by Distance Range and Number of Nearby Magnet Schools

Nearby Schools	1995 – 2000				2007 – 2012			
	Treat = 1: $1.5 - \delta \le d \le 1.5 - \varepsilon$	Omitted: $1.5 - \varepsilon < d < 1.5 + \varepsilon$	Treat = 0: $1.5 + \varepsilon \le d \le 1.5 + \delta$	Total	Treat = 1: $1.5 - \delta \le d \le 1.5 - \varepsilon$	Omitted: $1.5 - \varepsilon < d < 1.5 + \varepsilon$	Treat = 0: $1.5 + \varepsilon \le d \le 1.5 + \delta$	Total
$\delta = 1.5$, $\varepsilon = .0625$								
0	0	1,602	31,248	32,850	0	1,023	18,771	19,794
1	20,528	1,661	0	24,189	12,808	952	0	13,760
2	3,999	20	0	4,019	3,878	21	0	3,899
3	2,325	2	0	2,327	1,999	0	0	1,999
4	303	2	0	305	242	1	0	243
Total	29,155	3,287	31,248	63,690	18,927	1,997	18,771	39,695
$\delta = 1.0$, $\varepsilon = .0625$								
0	0	1,602	22,471	24,073	0	1,023	13,716	14,739
1	18,642	1,661	0	20,303	10,965	952	0	11,917
2	3,228	20	0	3,248	3,069	21	0	3,090
3	1,250	2	0	1,252	1,158	0	0	1,158
4	303	2	0	305	242	1	0	243
Total	23,423	3,287	22,471	49,181	15,434	1,997	13,716	31,147
$\delta = 0.5$, $\varepsilon = .0625$								
0	0	1,602	11,032	12,634	0	1,023	6,933	7,956
1	10,140	1,661	0	11,801	6,171	952	0	7,123
2	1,264	20	0	1,284	1,122	21	0	1,143
3	377	2	0	379	408	0	0	408
4	88	2	0	90	62	1	0	63
Total	11,869	3,287	11,032	26,188	7,763	1,997	6,933	16,693
$\delta = 0.25$, $\varepsilon = .0625$								
0	0	1,602	4,896	6,498	0	1,023	3,121	4,144
1	4,700	1,661	0	6,361	2,819	952	0	3,771
2	358	20	0	378	329	21	0	350
3	47	2	0	49	62	0	0	62
4	8	2	0	10	3	1	0	4
Total	5,113	3,287	4,895	13,296	3,213	1,997	3,121	8,331

5. Empirical Results

Figure 4 shows the path of median house prices over time for the control and treatment observations for δ = 1.5 and δ = 0.25. The vertical lines indicate the start of the first quarter following the December 1997 and December 2009 reform dates, i.e., 1998:1 and 2010:1. Median prices start at nearly identical levels for control and treatment observations in 1993, but a wedge forms before the 1997 reform, after which median prices are significantly higher for properties closer to the magnet schools. The treatment premium does not vary greatly over the subsequent decade. The 2010 reform appears to have averted some of the collapse in house prices that began in late 2007. Prices fell much more for control properties than for homes close to magnet schools. Although prices rose again for control properties after 2010, the discount for control properties remains larger as late as 2017 as it had been in earlier years.[7]

Table 4 presents our primary regression results. All sales within 3 miles of a magnet school are included in this set of regressions, i.e., δ = 1.5. The regressions include controls for the quarter of sale and census tract fixed effects. Standard errors are clustered by census tract.

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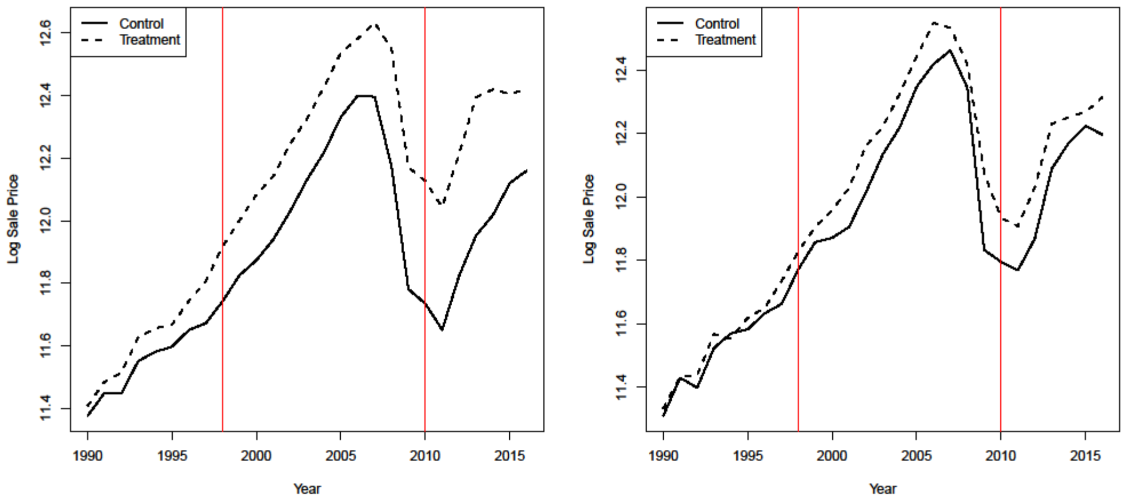
[7] Figure 4 suggests that the reforms may have been anticipated prior to their formal announcement, particularly in 2009. The Federal Court decision in September 2009 was the result of a long process, with the Desegregation Consent Decree first modified in 2004 and then amended in 2006 (Jackson, 2010). The final and decisive changes were introduced in 2009 when the Consent Decree was rescinded. As early as January 2009 CPS officials signaled a probable move from race to socioeconomic status as factors influencing admissions decisions (http://catalyst-chicago.org/2009/09/federal-judge-ends-chicago-schools-desegregation-decree/).

Figure 4: Median House Prices over Time



δ = 1.5 δ = 0.25

Table 4: Estimated Differences in Differences Effects on House Prices, δ = 1.5 and ε = .0625

Variable	1995-2000	2007-2012	1995-2000	2007-2012
Log of Building Area	0.319 (0.009)	0.332 (0.017)	0.319 (0.009)	0.331 (0.017)
Log of Lot Size	0.229 (0.012)	0.394 (0.020)	0.229 (0.012)	0.394 (0.020)
Age/10	-0.014 (0.002)	-0.031 (0.002)	-0.014 (0.002)	-0.031 (0.002)
Rooms	-0.001 (0.002)	-0.013 (0.004)	-0.001 (0.002)	-0.013 (0.003)
Bathrooms	0.018 (0.005)	0.004 (0.008)	0.018 (0.005)	0.004 (0.008)
Bedrooms	0.012 (0.003)	0.005 (0.006)	0.012 (0.003)	0.004 (0.006)
Central Air	-0.004 (0.005)	0.019 (0.008)	-0.004 (0.005)	0.019 (0.008)
Attic	-0.016 (0.004)	0.006 (0.008)	-0.017 (0.004)	0.006 (0.008)
Basement	-0.004 (0.005)	0.035 (0.009)	-0.004 (0.005)	0.034 (0.009)

Brick	0.039 (0.005)	0.055 (0.011)	0.040 (0.005)	0.055 (0.011)
Fireplace	0.027 (0.008)	0.032 (0.014)	0.027 (0.008)	0.033 (0.014)
1-Car Garage	0.054 (0.005)	0.028 (0.010)	0.054 (0.005)	0.028 (0.014)
2+ Car Garage	0.070 (0.004)	0.051 (0.009)	0.070 (0.004)	0.051 (0.009)
Within 1.5 Miles of a Magnet School	0.041 (0.020)	0.021 (0.031)		
Within 1.5 Miles of a Magnet School, Post-Reform	0.040 (0.010)	0.126 (0.028)		
Within 1.5 Miles of 1 Magnet School			0.054 (0.020)	0.028 (0.031)
Within 1.5 Miles of 2 Magnet Schools			0.012 (0.028)	0.082 (0.044)
Within 1.5 Miles of 3-4 Magnet Schools			-0.000 (0.049)	-0.126 (0.069)
Within 1.5 Miles of 1 Magnet School, Post-Reform			0.015 (0.010)	0.106 (0.028)
Within 1.5 Miles of 2 Magnet Schools, Post-Reform			0.116 (0.020)	0.053 (0.050)
Within 1.5 Miles of 3-4 Magnet Schools, Post-Reform			0.153 (0.016)	0.366 (0.047)
R^2	0.777	0.685	0.778	0.686
Number of Observations	60,403	37,698	60,403	37,698

Notes. Standard errors (in parentheses) are clustered by census tract. The dependent variable is the log of sale price. The regressions include controls for the quarter of sale and census tract fixed effects. The post-reform dates are defined as 1998 and later for the 1995 – 2000 sample and 2010 and later for the 2007 – 2012 sample.

The results for the structural characteristics are standard. Sales prices are estimated to be higher for bigger, newer homes on larger lots. Prices are also higher for homes with more bathrooms and bedrooms, brick construction, a fireplace, and a garage. The only anomalies are the negative signs for central air conditioning and the presence of a basement in the 1995-2000 regressions, but the estimated coefficients turn to the expected positive value in the later time period. The key results are listed last in Table 4. The results for 1995-2000 indicate that prices rose by approximately 4% in areas that had admission probabilities increased by the 1997 reform. The results for 2007-2012 indicate that the effect was of the 2009 was larger at 12.6%.

Table 4 also presents the results for treatment intensity. For both time periods, the interactions between the number of nearby magnets and $Treat\ x\ \ Reform$ (i.e., within 1.5 miles of a magnet school, post-reform) imply a higher treatment effect for homes that are within 1.5 miles of a larger number of magnet schools. The results for the 1995 – 2000 period imply that house prices rose by 1.5% after the 1997 reform for homes that are within 1.5 miles of one magnet school, by 11.6% for homes that are near two schools, and by 15.3% for homes that are near three or four schools. Comparable figures for the 2009 reform are 10.6%, 5.3%, and 36.6% for homes that are within 1.5 miles of 1, 2, or 3-4 schools.

Table 5 shows how the results vary as the bandwidth around the 1.5-mile mark varies. The estimated coefficients for Treatment x Reform fall from 0.040 for 1995 – 2000 when δ = 1.5 to 0.035, 0.031, and 0.020 when δ decreases to 1.0, 0.50, and 0.25. Comparable estimates for 2007 – 2012 are 0.126 for δ = 1.5, 0.138 for δ = 1.0, 0.085 for δ = 0.50, and 0.076 for δ = 0.25. The estimates remain statistically significant at the 5% level for all but the narrowest bandwidth, δ = 0.25.

Table 5: Variation in Distance Bands

Variable	δ = 1.5, ε = .0625	δ = 1, ε = .0625	δ = 0.5, ε = .0625	δ = 0.25, ε = .0625
1995 – 2000				
Within 1.5 Miles of a Magnet School	0.041 (0.020)	0.044 (0.020)	0.041 (0.019)	0.030 (0.017)
Within 1.5 Miles of a Magnet School, Post-1997	0.040 (0.010)	0.035 (0.011)	0.031 (0.014)	0.020 (0.016)
Number of Observations	60,403	45,894	22,901	10,009
2007 – 2012				
Within 1.5 Miles of a Magnet School	0.021 (0.031)	0.017 (0.032)	0.036 (0.034)	0.011 (0.037)
Within 1.5 Miles of a Magnet School, Post-2009	0.126 (0.028)	0.138 (0.031)	0.085 (0.042)	0.076 (0.052)
Number of Observations	37,698	29,150	14,696	6,334

Notes. Standard errors (in parentheses) are clustered by census tract. The dependent variable is the log of sale price. The regressions include controls for the quarter of sale and census tract fixed effects.

6. Repeat Sales

Our first robustness check takes advantage of repeat sales to control for unobserved characteristics of homes and neighorhoods that do not change over time. We let *s* denote the sale of a home at time s < t. Also, note that $Treat_{ht} = Treat_{hs}$ since treatment status is defined as being within 1.5 miles of a magnet schools, which does not change over time since we have the same number of magnet schools throughout the two sample periods. If γ_2 is constant over time, the repeat sales version of the model is simply:

$$lnP_{hct} - lnP_{hcs} = \gamma_2(Treat_h xReform_t - Treat_h xReform_s) + (\rho_t - \rho_s) + (u_{hct} - u_{hcs}) \tag{2}$$

Equation (2) is estimated by specifying a series of discrete variables D_{hct} for the house sale pairs such that $D_{hct} = -1$ if house *h* sold for the first time at *t*, $D_{hct} = 1$ if it sold for the second time at time *t*, and $D_{hct} = 0$ if it did not sell at time *t*:

$$\begin{gathered} lnP_{hct} - lnP_{hcs} = \\ \gamma_2(Treat_h xReform_t - Treat_h xReform_s) + \rho_t D_t + (u_{hct} - u_{hcs}) \end{gathered} \tag{3}$$

Equation (3) implies a parallel shift in the price index for locations near magnet schools at the time of reform. Alternatively, separate appreciation rates can be estimated for all periods for magnet and non-magnet locations by estimating the following equation:

$$\begin{gathered} lnP_{hct} - lnP_{hcs} = \\ (\theta_t - \rho_t)Treat_h D_t + \rho_t D_t + (u_{hct} - u_{hcs}) \end{gathered} \tag{4}$$

Equation (4) tests directly whether the price index differs both pre- and post-reform.

Since repeat sales estimates can be sensitive to relatively small samples in beginning and ending periods, we expand the sample to 1993 – 2014 using the Illinois Department of Revenue data. The results of estimating equation (3) and (4) using data for 1993 – 2001 are shown in Figure 5. Figure 6 shows comparable results for 2002 – 2014. The results from equation (3) are consistent with the hedonic estimates, but significantly higher in magnitude: the estimated value of γ_2 – the post-reform treatment effect – is 0.062 with a standard error of 0.008 in for 2003 – 2001 and 0.253 with a standard error of 0.014 for 2002 – 2014. The results from estimating equation (4) are shown in the right panels of Figures 5 and 6, and the t-values are shown in Figure 7. For the earlier time period, the differences in the estimated appreciation rates are not significantly different for magnet and non-magnet locations until the time of reform, after which the appreciation rates are generally significantly higher for locations within 1.5 miles of a magnet school. The results are more complex for the later time period. The estimated appreciation rates are significantly lower for locations near magnet schools toward the end of the housing boom in 2006, after which they again are close to one another in 2007 and 2008. Appreciation rates are higher in magnet school areas beginning about a year before December 2009 reform, and they remain significantly higher thereafter. Overall, the results suggest that the 1997 reform was capitalized immediately into property values, while the 2009 reform appears to have been anticipated, although we cannot rule out the possibility that the 2009 results are in part driven by differences in broad spatial trends in appreciation rates.

Figure 5: Repeat Sales Estimates, 1993 - 2001

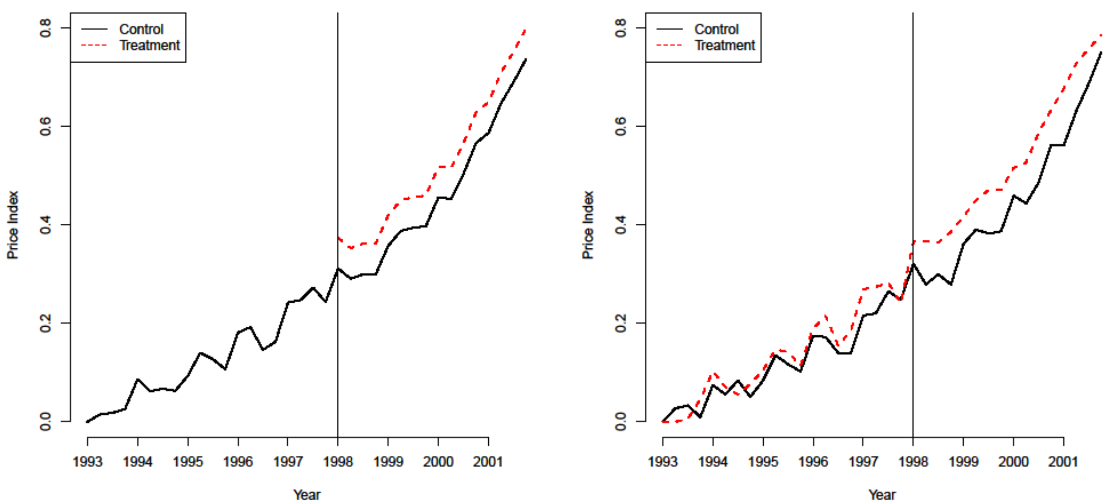


Figure 6: Repeat Sales Estimates, 2002-2014

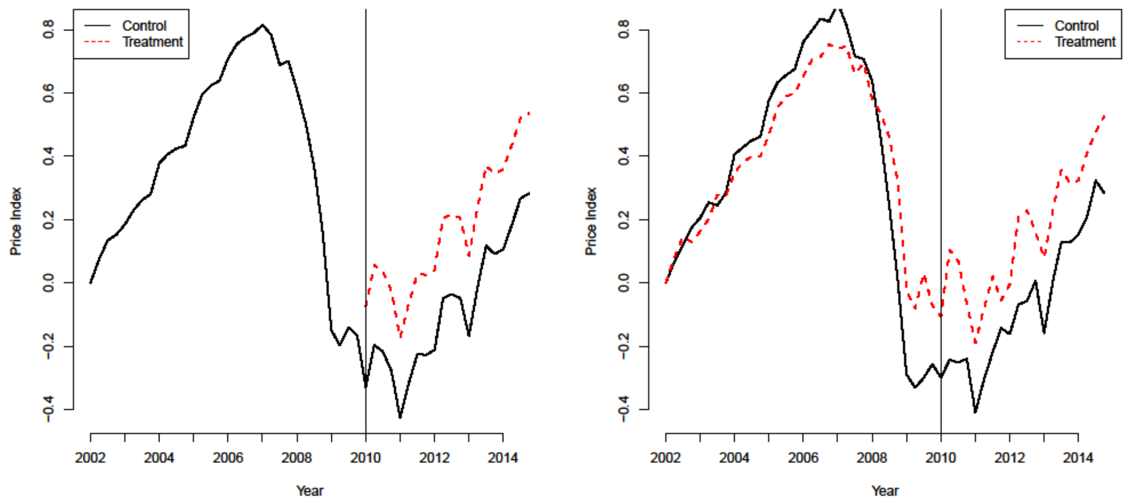
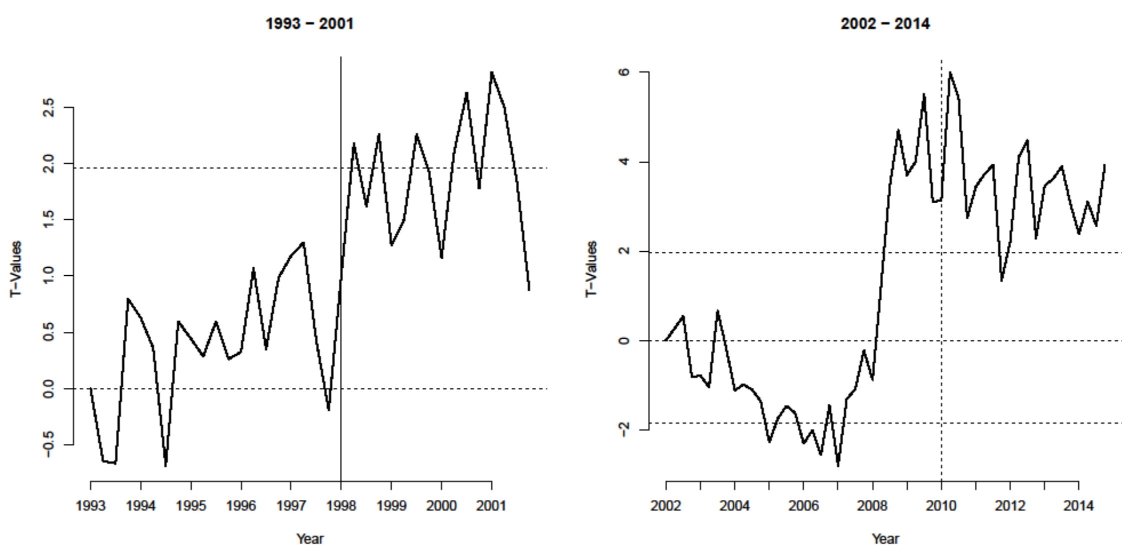


Figure 7: T-Values for Equation (4) Estimates



7. Placebo Tests

In this section, we report the results for two sets of placebo tests using the standard difference in differences hedonic approach. First, we use an incorrect definition of the treatment area: any home within 0.5 miles of a magnet school is defined as having received the treatment of a higher probability of admission. With δ set to either 0.25 or 0.5, this treatment definition means that both "treatment" and "control" observations have actually been beneficiaries of the reforms. Thus, we should not expect to find statistically significant estimates for the $Treat\ x\ \ Reform$ variables. The results are shown in Table 6. As expected, none of the estimated coefficients for or $Treat\ x\ \ Reform$ is statistically significant in either time period.

Table 6: Treatment Defined as Within 0.5 Miles of a Magnet School

Variable	δ = 0.25, ε = 0	δ = 0.5, ε = 0
1995 – 2000		
Within 0.5 Miles of a Magnet School	-0.003 (0.016)	0.003 (0.017)
Within 0.5 Miles of a Magnet School, Post-1997	-0.003 (0.019)	-0.002 (0.018)
Number of Observations	9,133	17,286
2007 – 2012		
Within 0.5 Miles of a Magnet School	0.011 (0.028)	0.020 (0.029)
Within 0.5 Miles of a Magnet School, Post-2009	-0.012 (0.044)	-0.001 (0.043)
Number of Observations	5,896	11,164

Notes. Standard errors (in parentheses) are clustered by census tract. The dependent variable is the log of sale price. The regressions include controls for housing characteristics, the quarter of sale, and census tract fixed effects.

As another check on the accuracy of our models, we estimate a set of hedonic regressions with an incorrect definition of the treatment date. We define the treatment date as December 1994 rather than December 1997 and restrict the sample to sales from 1993-1996. In this case, none of the observations has received a higher probability of admission. The results are shown Table 7. As expected, the incorrect treatment is indicated to have no effect on house prices.

Table 7: Reform Date Defined as 1994

	$\delta = 1.5$, $\varepsilon = .0625$	$\delta = 1$, $\varepsilon = .0625$	$\delta = 0.5$, $\varepsilon = .0625$	$\delta = 0.25$, $\varepsilon = .0625$
Within 1.5 of a Magnet School	0.034 (0.017)	0.035 (0.017)	0.036 (0.016)	0.022 (0.016)
Within 1.5 of a Magnet School, Post-1994	0.010 (0.007)	0.006 (0.008)	-0.016 (0.011)	-0.020 (0.015)
Number of Observations	39,293	29,725	14,877	6,690

Notes. Standard errors (in parentheses) are clustered by census tract. The dependent variable is the log of sale price. The regressions include controls for housing characteristics, the quarter of sale, and census tract fixed effects. The sample includes data from 1993-1996.

8. Treatment Heterogeneity

In this section, we relax the assumption that the effect of the reforms is the same for all households. According to the U.S. Census, 17% of Chicago's high school students were enrolled in private schools in 2003, with 2/3 of these students attending Catholic schools (Sander, 2006). Although private schools often offer some need-based scholarships, they remain costly for many lower-income households. Moreover, only 21% of Chicago's households had children under 18 in 2000. High-income households who can readily afford private school tuition may have little interest in Chicago's magnet schools except as a form of insurance, while the primary interest for childless households in the proximity of a magnet school may be its effect on the ability to sell the home in the future.

Although we do not observe any demographic data for the households represented in our sample, house size is correlated with the presence of children. Table 8 shows the results of estimating separate difference in difference hedonic regressions for small (≤ 1100 s.f. and ≤ 2 bedrooms) and large (≥ 2000 s.f. and ≥ 3 bedrooms) homes. For both time periods, the change in probability of admission to magnet schools has a significant effect on house prices for large homes, while the estimated effect for small homes is statistically insignificant. These results are as expected if the tendency for larger homes to hold more children makes their owners willing to pay a larger premium for proximity to magnet schools.

Table 8: Estimates for Small and Large Homes

	1995 – 2000		2007 – 2012	
	Small Homes	Big Homes	Small Homes	Big Homes
Within 1.5 Miles of a Magnet School	0.030 (0.027)	-0.009 (0.027)	0.057 (0.059)	-0.081 (0.053)
Within 1.5 Miles of a Magnet School, Post-Reform (1997 or 2009)	0.007 (0.011)	0.094 (0.015)	0.034 (0.035)	0.196 (0.050)
Number of Observations	12,589	14,203	7,419	9,752

Notes. Standard errors (in parentheses) are clustered by census tract. The dependent variable is the log of sale price. The regressions include controls for housing characteristics, the quarter of sale, and census tract fixed effects. Small homes are defined as having no more than 1100 square feet and 0-2 bedrooms. Large homes have at least 2000 square feet and 3 or more bedrooms.

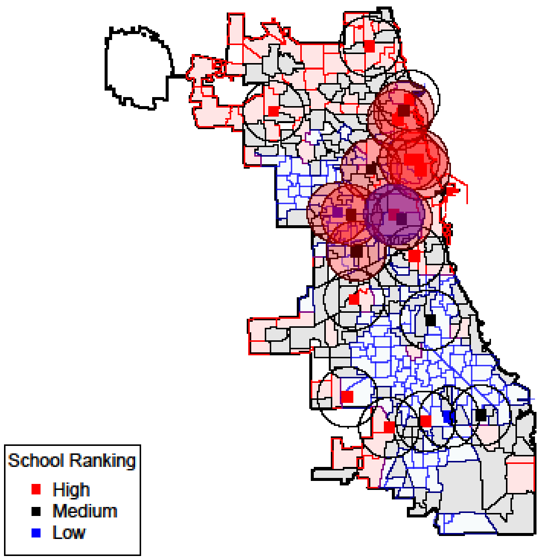
To test whether the results differ across magnet schools, we estimate separate regressions for each 3-mile circular region around the 22 magnet schools. The results are summarized in Table 9 and Figure 8. The 1.5 mile radius of a magnet school is filled with red if the estimated coefficient is statistically significant at the 5% level. For 1995-2000, the estimated coefficients for being within 1.5 miles of a magnet school after 2007 are positive and statistically significant for 10 of the 22 schools. The schools with statistically significant effects are all on the more affluent north side of the city in this earlier period. The results are much different for 2007 – 2012, with 12 of the 22 comparable coefficients estimated to be positive and statistically significant, including estimates for five south-side schools.

Table 9: Estimates for Individual Schools

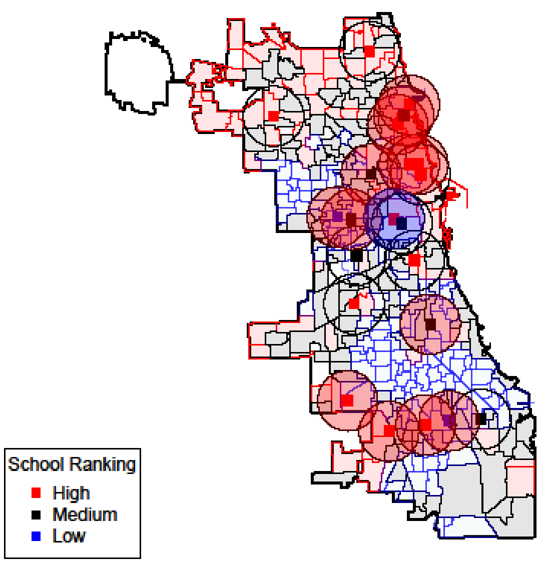
		1995 – 2000			2007 – 2012		
School	Ranking	Coef.	Std. Err.	Obs.	Coef.	Std. Err.	Obs.
Andrew Jackson	1	0.211*	0.028	4,094	-0.328*	0.069	3,689
Frank W Gunsaulus	1	0.013	0.015	10,633	-0.004	0.058	5,193
Franklin	1	0.864*	0.033	4,525	-0.079*	0.025	3,927
Hawthorne	1	0.086	0.023	8,515	0.135*	0.044	6,436
John H Vanderpoel	1	0.006	0.014	7,135	0.147*	0.071	5,110
LaSalle	1	0.260*	0.063	5,123	0.338*	0.038	4,380
Mark Sheridan	1	0.053	0.041	3,072	0.011	0.117	2,316
Ole A Thorp	1	-0.012	0.009	16,022	-0.006	0.021	7,781
Stone Elementary	1	-0.005	0.026	6,306	-0.052	0.037	3,810
Turner-Drew	1	-0.016	0.021	5,278	0.285*	0.068	5,048
Walt Disney	1	0.016	0.027	6,063	0.096*	0.046	4,573
Walter L Newberry	1	0.064*	0.026	6,047	0.291*	0.064	5,043
William Bishop Owen	1	-0.002	0.012	9,601	0.260*	0.055	4,928
Albert R Sabin	2	0.165*	0.018	9,403	0.282*	0.055	7,026
Edward Beasley	2	0.016	0.040	3,061	0.351*	0.153	3,384
Galileo	2	0.145*	0.037	3,572	-0.066	0.159	3,142
Inter-American	2	0.073*	0.026	7,422	0.142*	0.049	5,690
Jensen	2	0.151*	0.030	5,157	0.248*	0.075	4,127
Maria Saucedo	2	0.044*	0.026	5,397	-0.067	0.120	3,355
Robert A Black	2	0.009	0.021	3,861	0.096	0.074	3,445
Burnside	3	-0.035	0.021	4,919	0.151*	0.067	4,807
Leif Ericson	3	0.148*	0.027	5,376	0.15*	0.078	4,093

Notes. The regressions are estimated using observations within 3 miles of each school. The results show the estimated coefficients for being within 1.5 miles of a magnet school, post-reform. Standard errors are clustered by census tract. The dependent variable is the log of sale price. The regressions include controls for housing characteristics, the quarter of sale, and census tract fixed effects. Asterisks indicate statistical significance at the 10% level or lower.

Figure 8: Coefficients for Treatment x Post-Reform



1995-2000



2007-2012

These results for individual schools are particularly interesting because the south-side schools are located in primarily African-American neighborhoods. White students had a higher probability of admission to all magnet schools than black students in the earlier time period, both before and after the 1997 reform that increased the probability of admission for students living near the schools. Thus, it is not surprising that the 1997 effect had a bigger effect for the white neighborhoods on the north side: on the south side, African-American students were still rationed out of the schools. When this racial bias was eliminated in 2009, the higher probability of admission for black students is directly evident in the change in house prices in predominantly African-American, south-side neighborhoods.

Our final set of hedonic regressions allows the effects of the reforms to vary by the Chicago Public Schools' ranking of the the quality of the schools. The broad ranking simply sorts schools into high, medium, and low categories. As is evident from Figure 1, only 2 of the 22 magnets school are classified as low quality, compared with 13 high-quality schools. In contrast, most of the regular public elementary schools on the south and west sides of the city are ranked as low quality. Since the ranking is from 2008, we focus on the 2007 – 2012 period.

The results of three regressions are shown in Table 10. The specification shown in (1) allows the results for locations within 1.5 miles of a magnet school to differ depending on whether the local public high school is highly ranked. The results suggest that the admssion policy reform had a larger effect on homes located within the catchment area of a highly ranked public school. This result may reflect the influence of sorting: areas that attract families who care more about school quality may sort into locations with highly ranked public schools, and these areas also experience greater appreciation when the probability of admission to a magnet school increases.

Table 10: Estimates by School Quality Ranking

Within 1.5 Miles of a Magnet School	-0.005 (0.025)	-0.023 (0.024)	-0.018 (0.024)
Within 1.5 Miles of a Magnet School with Rank = High		-0.042 (0.045)	-0.036 (0.045)
Public School Rank = High	-0.030 (0.025)		-0.019 (0.025)
Within 1.5 Miles of a Magnet School, Post-Reform	0.052 (0.032)	-0.040 (0.048)	-0.072 (0.047)
Within 1.5 Miles of a Magnet School and Public School Rank = High, Post Reform	0.174 (0.031)		0.134 (0.032)
Within 1.5 Miles of a Magnet School with Rank = High and Public School Rank = High, Post Reform		0.237 (0.045)	0.209 (0.044)

Notes. Standard errors (in parentheses) are clustered by census tract. The dependent variable is the log of sale price. The regressions include controls for housing characteristics, the quarter of sale, and census tract fixed effects. The sample includes data from 2007-2012.

The specification labeled (2) in Table 10 presents results when the estimated effects are allowed to vary by the quality of the magnet school. The estimates suggest that the effect of the reform was high for high-quality magnet schools but insignificant for relatively low-quality magnets. Finally, specifcation (3) allows for separate effects of the reform depending on the quality of both the local public school and the magnet schools. While both effects are significant, the estimated effect of being near a high-quality magnet schools is much higher than the effect of being near a high-quality public school. Thus, while sorting alone may matter, the more influential effect is to be have a higher probability of being admitted into a high-quality magnet school.

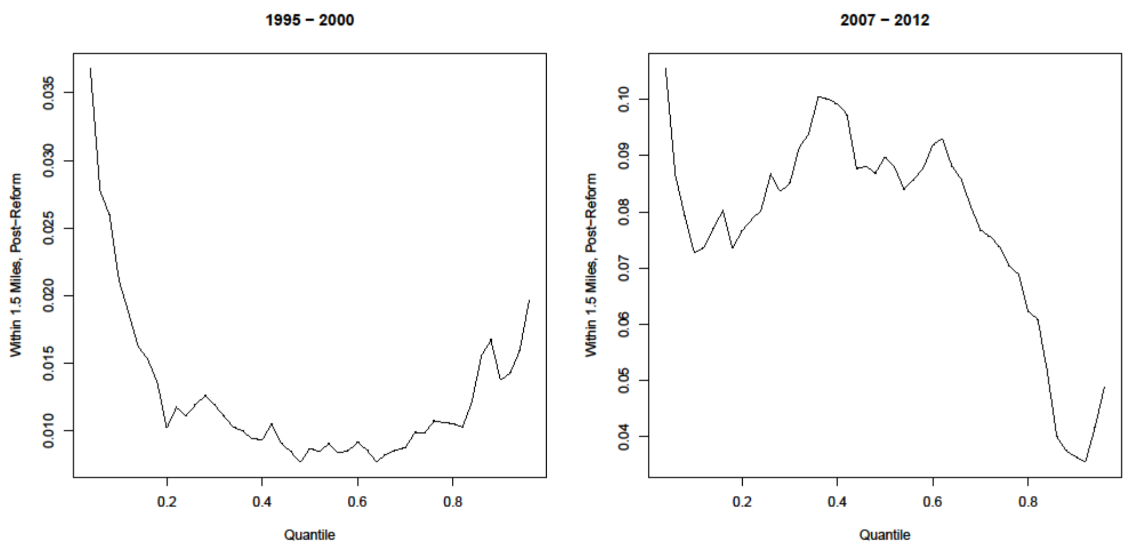
9. Quantile Regression Results

Although magnet schools help to improve the quality of Chicago's public schools, it remains true that a large percentage of Chicago residents opt for private and parochial schools. According to Sander (2006), nearly half (48%) of Chicago's white 12-year-olds attended private schools in 2000, compared with 15% of corresponding suburban residents. Comparable figures for black and Hispanic residents are 8% and 10% in Chicago and 8% and 7% in the suburbs. Since the probability of private school enrollment increases with income, we might expect that the effects of a higher probability of magnet school admission will have more effect on low to medium priced homes than on high-priced houses.

To test whether the admission reforms had a higher effect on low-priced homes, we estimate a series of conditional quantile regressions for quantiles ranging from 0.04 to 0.96 increments of 0.02. Equation (1) forms the basis for these quantile regressions. The coefficients on *Treat x Reform* indicate whether increasing the probability of admission to magnet schools increases house prices at each quantile, conditional on structural characteristics, quarter of sale, and census tract fixed effects.

The estimated quantile regression coefficients for *Treat x Reform* are shown in Figure 9. For both sample periods, the estimated coefficients are higher for low quantiles. For the earlier period, the range of high coefficients is concentrated in the region with quantiles below 0.20, while in the later period the range of relatively high coefficients continues through a quantile of about 0.60. These results suggest that the reforms shift the distribution of house prices to the right, with a greater shift at relatively low house prices than on the right side of the distribution. The reforms have much less effect on relatively high-priced homes, where children are more likely to attend private and parochial schools.

Figure 9: Quantile Regression Results



10. Conclusion

The literature on school choice has largely neglected the need to ration spaces in high-quality schools. Having the right to apply for admission to a school does not guarantee a student a seat. The option may well be valuable, as suggested by the studies by Brunner, Cho, and Reback (2012); Machin and Salvanes (2010); Reback (2005); and Schwartz, Voicu, and Horn (2014), all of which suggest that house prices rise in areas that gain open enrollment in high-quality schools. However, the premium should be higher if students have a higher probability of receiving a seat in the desirable schools.

We take advantage of changes in admission policies for magnet schools to test whether a higher probability of admission to high-quality schools leads to higher house prices. Chicago's magnet schools were created in response to a 1980 desegregation consent decree. Although regular magnet schools did not restrict admission to high-achieving students, there soon was an excess demand for seats as they gained a reputation as high-quality schools. At the end of 1997, Chicago introduced a proximity lottery that increased the probability of admission to students living within 1.5 miles of a magnet school. When the consent decree expired in 2009, Chicago again increased admission probabilities for students living within this radius by removing racial quotas, eliminating a restriction on the proportion of a school's enrollment devoted to siblings, and increasing the proportion of the seats allocated to students living within the 1.5-mile radius.

Using data on house sales for 1993 – 2012, we find strong evidence that these admission reforms increased prices for homes within 1.5 miles of a magnet school as compared to homes in neighboring areas that did not benefits from the reforms. Prices are estimated to have increased by about 4% as a result of the 1997 reform. The premium is still higher – as much as 15% – for the subset of homes in areas for which admission probabilities rose dramatically as a result of being within the 1.5-mile radius of more than two schools. The 2009 reform is also estimated to have a large effect on house prices, with homes within the 1.5-mile radius earning a premium of more than 12% over more distant housing. The 2009 appears to have helped homes within 1.5 miles of a magnet school avoid some of the dramatic drop in house prices that occurred during the late 2000s.

The effects of the 2009 reform was high for high-quality magnet schools but insignificant for relatively low-quality magnets. Additionally, we find that the estimated effect of being near a high-quality magnet school is much higher than the effect of being near a high-quality public school. The results also suggest that eliminating racial quotas led to significant increases in prices for predominantly African-American neighborhoods whose students formerly had a lower probability of admission to the local magnet schools than white students. The estimated effects are significantly higher for larger homes that are more likely to hold children, and for relatively low-priced homes where children are more likely to attend public schools.

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