House Prices and School Choice:

Evidence from Chicago's Magnet Schools Proximity Lottery

Leonardo Bonilla
Department of Economics
University of Illinois
lbonill2@illinois.edu

Esteban Lopez
Department of Urban and Regional Planning
University of Illinois
lopezoch@illinois.edu

Daniel McMillen
Department of Economics
University of Illinois
mcmillen@illinois.edu

April 2016

Abstract

Studies of open school policies suggest that prices rise in areas served by low-quality schools when the students gain access to high-quality schools outside of their local district. However, excess demand may lead some students to be denied admission to high-quality schools. We take advantage of changes in admission policies to Chicago's magnet schools to test whether a higher probability of admission to high-quality schools leads to higher house prices. Chicago adopted policies that increased the probability of admission for students living within 1.5 miles of a magnet school. A comparison of house prices on either side of the 1.5-mile contour suggests that the increase in admission probabilities due to the 1997 reform increased house prices for homes within the 1.5-mile radius by about 5.4%, and the premium is still higher for homes in areas near multiple magnet schools. The 2009 reform is estimated to have produced a premium of more than 14% for homes within the 1.5-mile radius by allowing homes near magnet schools to avoid some of the dramatic drop in house prices that began at the end of 2007. Quantile estimates suggest that the reforms had the largest effect on moderately-priced homes.

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 in 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 policies (e.g., Epple and Romano, 2003; Ferreyra, 2007; and Nechyba, 2000, 2003) 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: prices rise in areas with low-quality schools and prices fall in areas with high-quality schools (Machin and Salvanes, 2010; 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 enrollment may be determined by some form of lottery.

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. 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 introduced by Black (1999) and followed 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.[1] In addition to a standard set of housing characteristics, we include a full set

[1] Example of studies using geographical discontinuities to estimate causal effects of schools on house values include 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).

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 5.4%. The 2009 form had an even larger effect of about 14%, although in this recessionary period the main effect was to greatly reduce the amount by which prices fell. Separate estimates for census tract socioeconomic status quartiles suggest that appreciation rates are highest for homes in relatively low-status areas. Quantile estimates suggest that while the 1997 reform had only a modest effect on the overall distribution of house prices, the 2009 reform produce a marked rightward shift in the price distribution, with the shift being most pronounced in the middle of the price distribution.

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 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 Brasington and Haurin (2006); Clapp, Nanda, and Ross (2008); Hoxby (2004); Kane, Staiger, and Samms (2003); Kane, Rieg, and Staiger (2006); and Rouse and Barrow (2009) showing that school quality and academic performance affects property values. The results suggest that the benefits of magnet school availability is relatively progressive in the sense that homeowners with low to moderate incomes place the highest value on higher admission probabilities.

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 low 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 multiracial 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 magnets 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 were 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 admission 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 now conducts 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 precisely using straight-line distance from the student's address to the school.

The proximity lottery 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 5 magnets. Since families can enter the proximity lottery for all schools with 1.5 miles of their home, these regions are especially valuable.

In September 2009, a federal court decision vacated the desegregation consent decree.[2] 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

[2] 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.

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 of race. Proximity lotteries are conducted if the number of neighborhood applicants exceeds 40% of the available seats. However, race and ethnicity continue 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." [3]

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.

[3] 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 have 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 DataQuick, which provided data on house sales for Chicago for 1997-2012. We then expanded the sample to include data from 1993-1996 using data from the Illinois Department of Revenue. DataQuick also provided information on lot size, building area, house age, the number of bathrooms, and indicators that the home has central air conditioning, brick construction, or a fireplace. Comparable data for 1993-1996 are drawn from the Cook County Assessor's Office. As DataQuick relies on the same data sources when constructing their sales files, the two sets of data are directly comparable. 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 a regular magnet school that had opened before 1998. The Cook County Assessor's Office 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. 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.

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

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

for either sub-period. We include fixed effects for the quarter of sale and the census tract, and standard errors are clustered at the tract level.[4] The estimating equations also include controls for structural characteristics, including log building area, log lot size, building age, the number of bathrooms, and dummy variables for central air conditioning, brick construction, and a fireplace.[5]

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 over time, the *Treat* variable is the same for both reform times: *Treat* = 1 if a home is within 1.5 miles of a

[4] 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.
[5] Implicitly, the estimating equations also include controls for the *Reform* variable. However, this variable is not separately identified from the controls for quarter of sale.

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 $t \geq$ 2009:1 for the 2007 – 2012 data. In the tables of results, the variable *Treat* is labeled as "Within 1.5 Miles of a Magnet School" and the *Treat x Reform* variable is labeled as "Within 1.5 Miles of a Magnet School, Post-Reform".

As geographic coding can 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 ε = 0.125, which is the length of a standard block in Chicago. For our base model, we exclude observations for which the distance (d) between the home and the nearest magnet school lies in the range $1.375 < d < 1.625$. An observation has *Treat* = 1 if the distance from the home to the nearest magnet school lies in the range $d \leq 1.375$ (i.e., $d \leq 1.5 - \varepsilon$), while *Treat* = 0 for observations in the range $1.625 \leq d \leq 3$. We then vary the size of the bands around the critical 1.375 and 1.625 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 δ = 1.5, we test three smaller bandwidths: δ = 1, 0.5, and 0.25.

Figure 3 shows the treatment and control observations for the four values of δ. Smaller values of δ lead to fewer observations for both the treatment and control groups as the sample is restricted to narrower bands around the critical 1.5 – ε and 1.5 + ε distances. 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 1.5 miles of 0-4 schools for each of the four distance bands. Even when δ = 0.25, more than 10% of the treatment observations are 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.

5. Results

Figure 3 shows the path of median house prices over time for the control and treatment observations for δ = 1.5. The paths look very similar for other values of δ. 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 at the end of 2012 than it had been in earlier years.[6]

[6] Figure 3 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/).

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.

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, brick construction, and a fireplace. The only anomaly is the negative sign for central air conditioning 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 5.4% 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 14.7%.

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 3.3% after the 1997 reform for homes that are within 1.5 miles of one magnet school, by 10.9% for homes that are near two schools, and by 13.1% for homes that are near three or four schools. Comparable figures for the 2009 reform are 12.9%, 8.4%, and 37.2% 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 results are quite similar when δ is reduced from 1.5 to 1, but the estimated coefficients for *Treatment x Reform* fall to 0.046 for 1995 – 2000 and 0.109 for 2007 – 2012 when δ = 0.5. The estimates remain statistically significant at the 5% level for 1995 – 2000 and at the 10% level for 2007 – 2012 for the narrowest bandwidth, δ = 0.25.

6. Placebo Tests

In this section, we report the results for two sets of placebo tests. 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 $Treat\ x\ \ Reform$ or $Treat\ x\ \ Reform$ are statistically significant in either time period.

As another check on the accuracy of our models, we estimate a set of 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, with the possible exception of the largest bandwidth (δ = 1.5), where the estimated coefficient is much lower than its counterpart in Table 4 (0.054 v. 0.014).

7. 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, 2003). 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 in 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 a potential proxy for the presence of children. Table 9 shows the results of estimating separate regressions for small (≤ 1100 s.f.) and large (≥ 2250 s.f) homes.[7] The 1997 reform is estimated to have no effect on house prices. The 2009 reform does appear to have a significant on house prices for small homes. However, the estimated treatment effects are always much higher for large homes. These results are as expected if the tendency for larger homes to hold children makes their owners willing to pay a larger premium for proximity to magnet schools.

To test whether the premium for magnet schools varies with income, we take advantage of the "socio-economic status" scores produced for each census tract by the Chicago Public Schools' Office of Access and Enrollment (www.cpsoae.org). The composite score is based on median family income, adult educational attainment, the percentage of single-parent households, the

[7] These figures are approximate versions of the 25th and 75th percentiles for building areas. The exact values are 1091 and 2280.

percentage of home ownership, the percentage of the population that speaks a language other than English, and a school performance variable. The score ranges from 1 – 4, with 1 representing low socioeconomic status. The four scores divide the census tracts into approximate quartiles.

Table 9 shows the results for the four SES scores when δ = 1.5 and ε = 0.125. For both 1995 – 2000 and 2007 – 2012, the estimated coefficients for *Treat x Reform* are significantly higher for the lower two SES scores than for observations in higher-status census tracts. The estimates are statistically insignificant at the 5% level for observations in census tracts with the highest SES score. Overall, the results suggest that the benefits of the reform are progressive in the sense that the increase in the premium associated with magnet school proximity is highest for observations in low-status census tracts.

8. Quantile Regression Results

If treatment effects are highest in low-status census tracts, we might expect the distribution of house prices to shift further to the right for the left side of the distribution. In this section, we estimate a series of quantile regressions to determine whether the effects of the reforms vary across the distribution of house prices. With 817 census tracts and 24 quarters in each six-year time interval, some simplifications are necessary to make estimation feasible. We follow a two-stage estimation procedure proposed by Canay (2011) to control for census tract fixed effects. The first stage is our basic linear regression model, which includes controls for both census tracts and the quarter of sale. The first-stage regression provides estimates of the census tracts fixed effects, $D\alpha$, where D is the $n\ x$ 817 matrix of census tract indicator variables. Under the assumption that α does not vary across quantiles, the second stage is a standard quantile regression of $lnP - D\hat{\alpha}$ on

Treat, *Treat x Reform*, *X*, and controls for the time of sale. As a further simplification, we use a cubic functions to control for time in the second stage quantile regressions: $\sum_{j=1}^{3}\{\delta_j(q-q_1)^j + \lambda_j(q-q_1)^j xReform\}$, where q represents the quarter of sale. This specification allows for different time trends before and after the reform dates.

Table 10 presents the quantile regression results for the *Treat* and *Treat x Reform* variables for three quantiles, τ = 0.10, 0.50, *and* 0.90, when δ = 1.5 and ε = 0.125. For 1990-2000, the estimated coefficients for *Treat x Reform* are larger for the low and high quantiles than for the median.[8] In contrast, the estimated coefficient for *Treat x Reform* is highest for τ = 0.90 and lowest for τ = 0.10. However, limiting the estimation to three values for τ conceals much additional variation. Figure 4 shows the estimated coefficients for the *Treat* x Reform variables as τ ranges from 0.02 to 0.98 in increments of 0.01. The results suggest that the 1997 reform had its highest effect on low quantiles, while the 2009 reform's effect peaks at quantiles near 0.80. Models 3 and 4 allow the effects to vary by treatment intensity. As expected, the estimated treatment effects are higher for homes in areas that are near more magnet schools.

To illustrate the effects of the reforms on the full distribution of house prices, we use an approach similar to one proposed by Machado and Mata (2005) to estimate the effect of discrete changes in an explanatory variables on the distribution of house prices.[9] At each quantile from τ = 0.02, 0.03… 0.98, we predict the sale price at the time just after the reform using the following equation:

$$ln\hat{P} = \hat{\gamma}_1(\tau)Treat + \hat{\gamma}_2(\tau)TreatxReform + X\hat{\beta}(\tau)$$

---

[8] Standard errors are reported conditional on the first-stage estimates and are not clustered by census tract.

[9] The approach used here is discussed in more detail in McMillen (forthcoming).

$$+\sum_{j=1}^{3}\{\hat{\delta}_j(\tau)(q-q_1)^j+\hat{\lambda}_j(\tau)(q-q_1)^j xReform\}$$

with *X* set at the actual values in the data set and q = 13, i.e., the first quarter after the reform. To analyze the effects of the reform on the treated observations (*Treat* = 1), we compare the predictions for observations within 1.5 miles of a magnet school for $Reform = 0$ and $Reform = 1$. The result is a set of $n_1 x$ 97 predicted values for $Reform = 0$ and $Reform = 1$, where n_1 is the number of observations with *Treat* = 1. We then estimate kernel density functions using the full set of entries for each matrix. A comparison of the estimated kernel densities shows the shift in the full distribution of house prices for the treated observations due to the reform.

The results are shown in Figure 5. For 1995-2000, the distribution of house prices shifts to the right by a relatively small amount. The shift is somewhat larger on the left side of the distribution than at high prices. The shift is much larger for 2007-2012. The result of the 2009 reform is to shift the distribution well to the right, particularly for prices near middle-left portion of the distribution.

The procedure can also be used to simulate the effect of the reforms for alternative treatment intensities. The $Treat$ and $TreatxReform$ variables are interacted with the *NumSchools* variables, which indicate whether the treated observation is within 1.5 miles of 1, 2, or 3-4 schools. We again estimate quantile regression for τ = 0.02, 0.03, … , 0.98. The next step is to calculate the full set of $n_1 x$ 97 predictions for $Reform = 0$ and $Reform = 1$ for *NumSchools* = 1, 2, and 3-4. The results are shown in Figure 6. For 1995-2000, the effect of the 1997 reform is now seen to be quite pronounced for homes near 2 or 3-4 magnet schools. The

2009 reform is estimated to produce a still larger rightward shift in the price distribution. For both reforms, the shifts are concentrated in the middle of the price distribution.

9. 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 Reback (2005), Cho, and Reback (2012), Machin and Salvanes (2010), 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 lower probability of being denied a seat in the desirable schools.

We take advantage of changes in admission policies to 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 at least 3% result of the 1997 reform. The premium is still higher – in the 10% - 13% range – 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 10% 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. Separate estimates for census tract socioeconomic status quartiles suggest that appreciation rates are highest for homes in relatively low-status areas, while quantile estimates suggest treatment effects are most pronounced for homes with prices in the middle of the overall distribution.

References

Allensworth, Elaine M. and Todd Rosenkranz, "Access to Magnet Schools in Chicago" (2000) http://files.eric.ed.gov/fulltext/ED446171.pdf.

Bayer, P., F. Ferreira, and R. McMillan, "A Unified Framework for Measuring Preferences for Schools and Neighborhoods," *Journal of Political Economy* 115 (2007), 588-638.

Black, Sandra, "Do Better Schools Matter? Parental Valuation of Elementary Education," *Quarterly Journal of Economics* 114 (1999), 577-599.

Bogart, William T. and Brian A. Cromwell, "How Much is a Neighborhood School Worth?," *Journal of Urban Economics* 47 (2000), 280-305.

Brasington, David and Donald R. Haurin, "Educational Outcomes and House Values: A Test of the Value Added Approach," *Journal of Regional Science* 46 (2006), 245-268.

Brunner, Eric J., Sung-Woo Cho, and Randall Reback, "Mobility, Housing Markets, and Schools: Estimating the Effects of Inter-District Programs," *Journal of Public Economics* 96 (2012), 604-614.

Canay, Ivan A., "A Simple Approach to Quantile Regression for Panel Data," *Econometrics Journal* 14 (2011), 368-386.

Clapp, John M., Anupam Nanda, and Stephen L. Ross, "Which School Attributes Matter? The Influence of School District Performance and Demographic Composition On Property Values," *Journal of Urban Economics* 63 (2008), 451-466.

Epple, Dennis and Richard E. Romano, "Neighborhood Schools, Choice, and the Distribution of Educational Benefits," in Caroline M. Hoxby (ed.), *The Economics of School Choice*, Chicago, University of Chicago Press (2003), 227-286.

Ferryra, Maria M., "Estimating the Effects of Private School Vouchers in Multidistrict Economies," *American Economic Review* 97 (2007), 789-817.

Gibbons, Steve and Stephen Machin, "Valuing English Primary Schools," *Journal of Urban Economics* 53 (2003), 197-219.

Gibbons, Stephen, Stephen Machin, and Olmo Silva, "Valuing School Quality using Boundary Discontinuities," *Journal of Urban Economics* 75 (2013), 15-28.

Fack, Gabrielle and Julien Grenet, "When Do Better Schools Raise Housing Prices? Evidence from Paris Public and Private Schools," *Journal of Public Economics* 94 (2010), 59-77.

Kane, Thomas J., Douglas O. Staiger, and Gavin Samms, "School Accountability Ratings and Housing Values," in William Gale and Janet Pack (eds.) *Brookings-Wharton Papers on Urban Affairs*, Washington, DC, Brookings Institution (2003, 83-137.

Kane, Thomas J., Stephanie K. Riegg, and Douglas O. Staiger, "School Quality, Neighborhoods, and Housing Prices," *American Law and Economics Review* 8 (2006), 183-212.

Jackson, Shawn L., (2010). *An Historical Analysis of the Chicago Public Schools Desegregation Consent Decree (1980 - 2006): Establishing Its Relationship with the Brown v. Board Case of 1954 and the Implications of Its Implementation on Educational Leadership.* Dissertation, Loyola University of Chicago (http://ecommons.luc.edu/luc_diss/129/).

Machado, J.A.F. and J. Mata, "Counterfactual Decomposition of Changes in Wage Distributions using Quantile Regression," *Journal of Applied Econometrics* (2005) 20, 445-465.

Machin, Stephen, "Houses and Schools: Valuation of School Quality through the Housing Market," *Labour Economics* 18 (2011), 723-729.

Machin, Stephen and Kjell Solvanes, "Valuing School Quality via a School Choice Reform," working paper, London School of Economics (2010).

McMillen, Daniel, "Conditionally Parametric Quantile Regression for Spatial Data: An Analysis of Land Values in Early Nineteenth Century Chicago," *Regional Science and Urban Economics* (forthcoming).

Nechyba, Thomas J., "Mobility, Targeting, and Private School Vouchers," *American Economic Review* 90 (2000), 130-146.

Nechyba, Thomas J., "School Finance, Spatial Income Segregation and the Nature of Communities," *Journal of Urban Economics* 54 (2003), 61-88.

Nguyen-Hoang, Phuong and John Yinger, "The Capitalization of School Quality into House Values: A Review," *Journal of Housing Economics* 20 (2011), 30-48.

Reback, Randall, "House Prices and the Provision of Local Public Services: Capitalization under School Choice Programs," *Journal of Urban Economics* 57 (2005), 275-301.

Ries, John and Tsur Somerville, "School Quality and Residential Property Values: Evidence from Vancouver Rezoning," *Review of Economics and Statistics* 92 (2010), 28-944.

Sander, William, "Private Schools and School Enrollment in Chicago," *Chicago Fed Letter No. 231* (2006).

Schwartz, Amy Ellen, Ioan Voicu, and Keren Mertens Horn, "Do Choice Schools Break the Link between Public Schools and Property Values? Evidence from House Prices in New York City," *Regional Science and Urban Economics* 49 (2014), 1-10.

Table 1

List of Magnet Schools Operating in 1997

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

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	11.847 (0.710)	12.058 (1.108)	11.639 (0.615)	11.644 (1.085)
Log of Building Area	7.434 (0.507)	7.453 (0.505)	7.327 (0.472)	7.355 (0.467)
Log of Lot Size	8.158 (0.351)	8.114 (0.400)	8.248 (0.271)	8.219 (0.297)
Age/10	7.287 (3.187)	8.205 (3.673)	7.322 (2.462)	8.597 (2.800)
Bathrooms	2.070 (1.104)	2.207 (1.171)	1.810 (0.937)	1.919 (0.972)
Central Air Conditioning	0.238 (0.426)	0.274 (0.446)	0.159 (0.365)	0.177 (0.382)
Brick	0.611 (0.488)	0.611 (0.488)	0.557 (0.497)	0.536 (0.499)
Fireplace	0.113 (0.317)	0.101 (0.301)	0.072 (0.258)	0.067 (0.250)
Number of Observations	48,683	35,217	51,799	38,367

Note. Standard deviations are in parentheses.

Table 3

Number Observations by Distance Range and Number of Magnet Schools within 1.5 Miles

Nearby Schools	1995 – 2000				2007 – 2012			
	Treat = 1: $1.5 - \delta \leq d \leq 1.5 - \varepsilon$	Omitted: $1.5 - \varepsilon < d < 1.5 + \varepsilon$	Treat = 0: $1.5 + \varepsilon \leq d \leq 1.5 + \delta$	Total	Treat = 1: $1.5 - \delta \leq d \leq 1.5 - \varepsilon$	Omitted: $1.5 - \varepsilon < d < 1.5 + \varepsilon$	Treat = 0: $1.5 + \varepsilon \leq d \leq 1.5 + \delta$	Total
$\delta = 1.5, \varepsilon = .125$								
0	0	5,125	46,674	51,799	0	3,943	34,424	38,367
1	31,713	5,031	0	36,744	20,748	3,663	0	24,411
2	7,128	191	0	7,319	6,990	161	0	7,151
3	4,124	24	0	4,148	3,271	27	0	3,298
4	461	11	0	472	350	7	0	357
Total	43,426	10,382	46,674	100,482	31,359	7,801	34,424	73,584
$\delta = 1.0, \varepsilon = .125$								
0	0	5,125	33,262	38,387	0	3,943	25,072	29,015
1	25,983	5,031	0	31,014	17,602	3,663	0	21,265
2	5,618	191	0	5,809	5,339	161	0	5,500
3	2,287	24	0	2,311	2,014	27	0	2,041
4	461	11	0	472	350	7	0	357
Total	34,349	10,382	33,262	77,993	25,305	7,801	25,072	58,178
$\delta = 0.5, \varepsilon = .125$								
0	0	5,125	15,202	20,327	0	3,943	11,750	15,693
1	13,050	5,031	0	18,081	9,257	3,663	0	12,920
2	2,019	191	0	2,210	1,785	161	0	1,946
3	703	24	0	727	686	27	0	713
4	123	11	0	134	94	7	0	101
Total	15,895	10,382	15,202	41,479	11,822	7,801	11,750	31,373
$\delta = 0.25, \varepsilon = .125$								
0	0	5,125	5,177	10,302	0	3,943	3,883	7,826
1	4,715	5,031	0	9,746	3,176	3,663	0	6,839
2	450	191	0	641	396	161	0	557
3	86	24	0	110	104	27	0	131
4	1	11	0	12	0	7	0	7
Total	5,252	10,382	5,177	20,811	3,676	7,801	3,883	15,360

Table 4
Estimated Differences in Differences Effects on House Prices, δ = 1.5 and ε = .125

Variable	1995-2000	2007-2012	1995-2000	2007-2012
Log of Building Area	0.333 (0.007)	0.295 (0.010)	0.334 (0.007)	0.296 (0.010)
Log of Lot size	0.255 (0.011)	0.350 (0.018)	0.255 (0.011)	0.350 (0.018)
Age/10	-0.024 (0.001)	-0.044 (0.002)	-0.024 (0.001)	-0.044 (0.002)
Bathrooms	0.011 (0.003)	0.020 (0.004)	0.010 (0.003)	0.019 (0.004)
Central Air Conditioning	-0.010 (0.005)	0.059 (0.008)	-0.011 (0.005)	0.060 (0.008)
Brick	0.053 (0.005)	0.098 (0.008)	0.053 (0.005)	0.098 (0.008)
Fireplace	0.061 (0.007)	0.057 (0.011)	0.061 (0.007)	0.057 (0.011)
Within 1.5 Miles of a Magnet School	0.020 (0.032)	-0.025 (0.037)		
Within 1.5 Miles of a Magnet School, Post-Reform	0.054 (0.010)	0.147 (0.024)		
Within 1.5 Miles of 1 Magnet School			0.032 (0.032)	-0.021 (0.037)
Within 1.5 Miles of 2 Magnet Schools			0.006 (0.037)	0.069 (0.043)
Within 1.5 Miles of 3-4 Magnet Schools			-0.056 (0.046)	-0.051 (0.057)
Within 1.5 Miles of 1 Magnet School, Post-Reform			0.033 (0.011)	0.129 (0.024)
Within 1.5 Miles of 2 Magnet Schools, Post-Reform			0.109 (0.020)	0.084 (0.039)
Within 1.5 Miles of 3-4 Magnet Schools, Post-Reform			0.131 (0.013)	0.372 (0.048)
R^2	0.728	0.749	0.729	0.750
Number of Observations	90,100	65,783	90,100	65,783

Notes. Clustered standard errors are in parentheses. 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.

Table 5

Variation in Distance Bands

Variable	δ + ε = 1.5, ε = .125	δ + ε = 1, ε = .125	δ + ε = 0.5, ε = .125	δ + ε = 0.25, ε = .125
1995 – 2000				
Within 1.5 Miles of a Magnet School	0.020 (0.032)	0.022 (0.032)	0.023 (0.033)	0.024 (0.033)
Within 1.5 Miles of a Magnet School, Post-Reform	0.054 (0.010)	0.051 (0.011)	0.046 (0.014)	0.031 (0.019)
Number of Observations	90,100	67,611	31,097	10,429
2007 – 2012				
Within 1.5 Miles of a Magnet School	-0.025 (0.037)	-0.025 (0.037)	-0.017 (0.038)	-0.013 (0 .042)
Within 1.5 Miles of a Magnet School, Post-Reform	0.147 (0.024)	0.145 (0.026)	0.109 (0.036)	0.085 (0.047)
Number of Observations	65,783	50,377	23,572	7,559

Notes. Clustered standard errors are in parentheses. The dependent variable is the log of sale price. The regressions include controls for structural characteristics, the quarter of sale, and census tract fixed effects.

Table 6

Treatment Defined as Within 0.5 Miles of a Magnet School

Variable	$\delta = 0.25$, $\varepsilon = 0$	$\delta = 0.5$, $\varepsilon = 0$
1995 – 2000		
Within 0.5 Miles of a Magnet School	-0.013 (0.015)	-0.003 (0.017)
Within 0.5 Miles of a Magnet School, Post-Reform	0.015 (0.020)	0.007 (0.019)
Number of Observations	14,417	27,531
2007 – 2012		
Within 0.5 Miles of a Magnet School	-0.021 (0.021)	-0.022 (0.023)
Within 0.5 Miles of a Magnet School, Post-Reform	0.020 (0.037)	0.044 (0.035)
Number of Observations	10,419	19,537

Notes. Clustered standard errors are in parentheses. The dependent variable is the log of sale price. The regressions include controls for structural characteristics, the quarter of sale, and census tract fixed effects.

Table 7
Reform Date Defined as 1994

	δ = 1.5, ε = .125	δ = 1, ε = .125	δ = 0.5, ε = .125	δ = 0.25, ε = .125
Within 1.5 of a Magnet School	0.040 (0.031)	0.041 (0.031)	0.051 (0.032)	0.047 (0.035)
Within 1.5 of a Magnet School, Post-1994	0.014 (0.007)	0.011 (0.008)	-0.005 (0.012)	-0.002 (0.019)
Number of Observations	59,803	44,376	20,315	6,940

Notes. Clustered standard errors are in parentheses. The dependent variable is the log of sale price. The regressions include controls for structural characteristics, the quarter of sale, and census tract fixed effects. The sample includes data from 1993-1996.

Table 8

Estimates for Small and Large Homes

Variable	$\delta + \varepsilon = 1.5$, $\varepsilon = .125$	$\delta + \varepsilon = 1$, $\varepsilon = .125$	$\delta + \varepsilon = 0.5$, $\varepsilon = .125$	$\delta + \varepsilon = 0.25$, $\varepsilon = .125$
1995 – 2000, Small Homes (≤ 1100 s.f.)				
Within 1.5 Miles of a Magnet School	0.089 (0.040)	0.086 (0.040)	0.080 (0.041)	0.061 (0.047)
Within 1.5 Miles of a Magnet School, Post-Reform	-0.005 (0.010)	-0.002 (0.011)	0.001 (0.014)	0.022 (0.024)
Number of Observations	23,910	17,955	7,991	2,796
2007 – 2012, Small Homes (≤ 1100 s.f.)				
Within 1.5 Miles of a Magnet School	0.014 (0.068)	0.018 (0.068)	0.013 (0.067)	0.015 (0.074)
Within 1.5 Miles of a Magnet School, Post-Reform	0.102 (0.031)	0.094 (0.033)	0.088 (0.044)	0.048 (0.060)
Number of Observations	16,339	12,279	5,579	1,861
1995 – 2000, Large Homes (≥ 2250 s.f.)				
Within 1.5 Miles of a Magnet School	-0.056 (0.037)	-0.052 (0.038)	-0.029 (0.040)	-0.023 (0.055)
Within 1.5 Miles of a Magnet School, Post- Reform	0.131 (0.016)	0.127 (0.018)	0.098 (0.027)	0.046 (0.039)
Number of Observations	22,808	17,335	8,004	2,698
2007 – 2012, Large Homes (≥ 2250 s.f.)				
Within 1.5 Miles of a Magnet School	-0.145 (0.058)	-0.144 (0.060)	-0.128 (0.064)	-0.143 (0.078)
Within 1.5 Miles of a Magnet School, Post- Reform	0.228 (0.039)	0.223 (0.044)	0.177 (0.064)	0.133 (0.083)
Number of Observations	17,668	13,699	6,492	2,053

Notes. Clustered standard errors are in parentheses. The dependent variable is the log of sale price. The regressions include controls for structural characteristics, the quarter of sale, and census tract fixed effects.

Table 9

Variation by Census Tract Socioeconomic Status

Variable	SES = 1	SES = 2	SES = 3	SES = 4
1995 – 2000				
Within 1.5 Miles of a Magnet School	0.020 (0.032)	0.022 (0.032)	0.023 (0.033)	0.024 (0.033)
Within 1.5 Miles of a Magnet School, Post-Reform	0.054 (0.010)	0.051 (0.011)	0.046 (0.014)	0.031 (0.019)
Number of Observations	90,100	67,611	31,097	10,429
2007 – 2012				
Within 1.5 Miles of a Magnet School	-0.025 (0.037)	-0.025 (0.037)	-0.017 (0.038)	-0.013 (0 .042)
Within 1.5 Miles of a Magnet School, Post-Reform	0.147 (0.024)	0.145 (0.026)	0.109 (0.036)	0.085 (0.047)
Number of Observations	65,783	50,377	23,572	7,559

Notes. Clustered standard errors are in parentheses. The dependent variable is the log of sale price. The regressions include controls for structural characteristics, the quarter of sale, and census tract fixed effects.

Table 10
Quantile Regression Results

Variable	τ = 0.10	τ = 0.50	τ = 0.90
Model 1: 1995-2000 (90,100 obs.)			
Within 1.5 Miles of a Magnet School	0.009 (0.011)	0.032 (0.003)	0.034 (0.005)
Within 1.5 Miles of a Magnet School, Post- Reform	0.060 (0.014)	0.028 (0.004)	0.043 (0.007)
Model 2: 2007-2012 (65,783 obs.)			
Within 1.5 Miles of a Magnet School	0.002 (0.011)	-0.009 (0.007)	-0.089 (0.011)
Within 1.5 Miles of a Magnet School, Post- Reform	0.104 (0.015)	0.159 (0.011)	0.165 (0.015)
Model 3: 1995-2000 (90,100 obs.)			
Within 1.5 Miles of 1 Magnet School	0.035 (0.011)	0.041 (0.003)	0.025 (0.006)
Within 1.5 Miles of 2 Magnet Schools	-0.150 (0.021)	0.035 (0.006)	0.132 (0.010)
Within 1.5 Miles of 3 Magnet Schools	-0.099 (0.025)	-0.073 (0.008)	-0.034 (0.013)
Within 1.5 Miles of 1 Magnet School, Post- Reform	0.062 (0.014)	0.008 (0.004)	0.018 (0.007)
Within 1.5 Miles of 2 Magnet Schools, Post- Reform	0.112 (0.026)	0.090 (0.008)	0.064 (0.013)
Within 1.5 Miles of 3 Magnet Schools, Post- Reform	0.142 (0.031)	0.121 (0.009)	0.120 (0.016)
Model 4: 2007-2012 (65,783 obs.)			
Within 1.5 Miles of 1 Magnet School	0.038 (0.012)	-0.007 (0.008)	-0.139 (0.012)
Within 1.5 Miles of 2 Magnet Schools	0.022 (0.017)	0.090 (0.012)	0.114 (0.017)
Within 1.5 Miles of 3 Magnet Schools	-0.038 (0.024)	-0.019 (0.017)	-0.110 (0.024)
Within 1.5 Miles of 1 Magnet School, Post- Reform	0.100 (0.017)	0.138 (0.012)	0.164 (0.017)
Within 1.5 Miles of 2 Magnet Schools, Post- Reform	-0.025 (0.026)	0.080 (0.018)	0.202 (0.025)
Within 1.5 Miles of 3 Magnet Schools, Post- Reform	0.491 (0.034)	0.378 (0.024)	0.242 (0.034)

Notes. The dependent variable is the log of sale price. The regressions include controls for structural characteristics, time trends, and census tract fixed effects.

Figure 1

Magnet School Locations

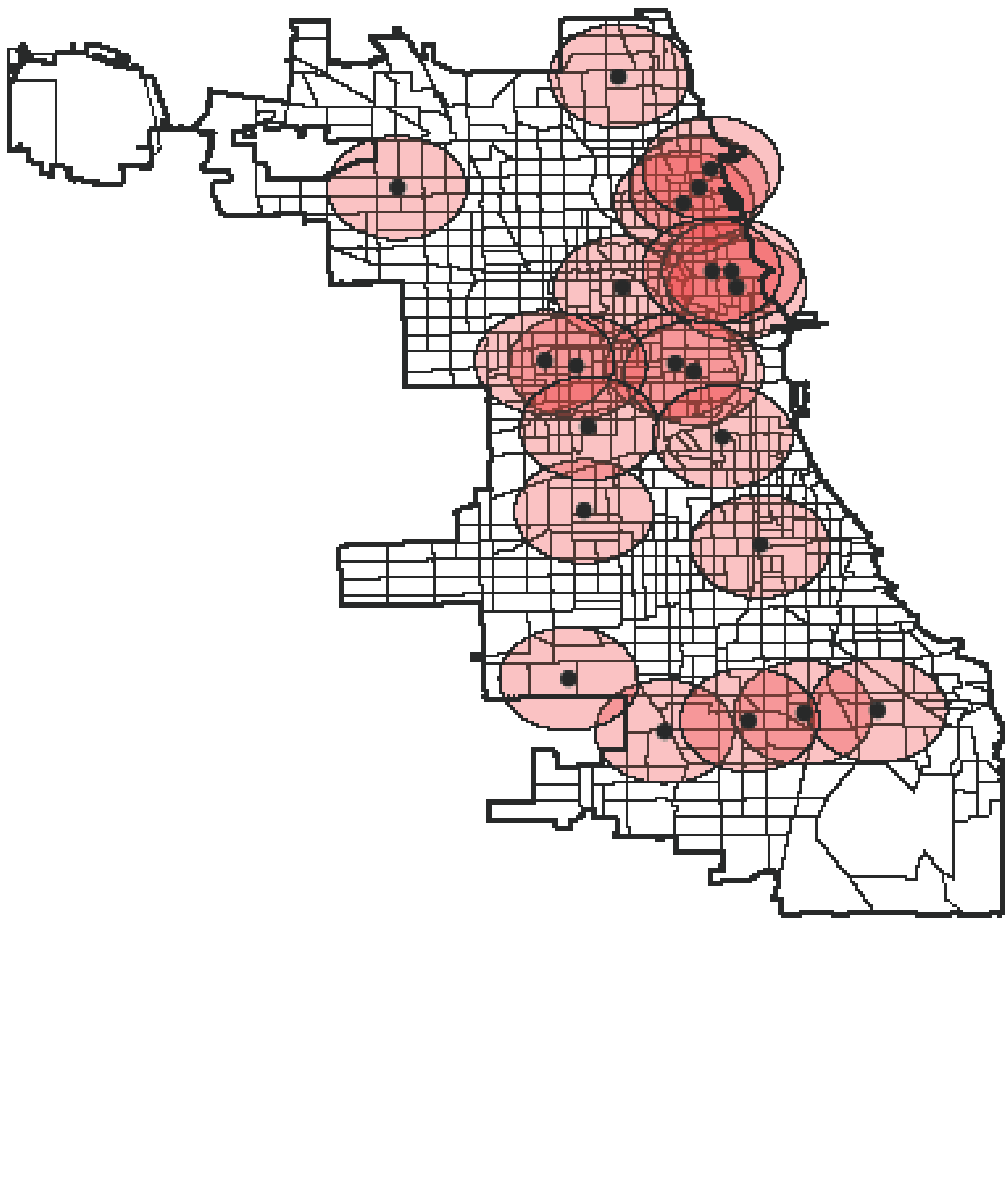
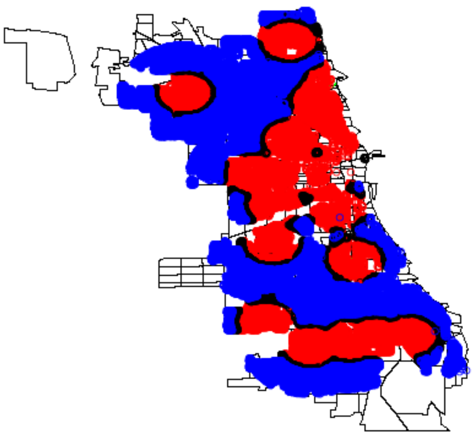
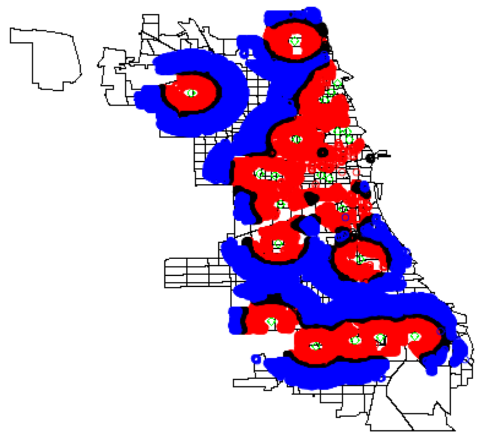


Figure 2

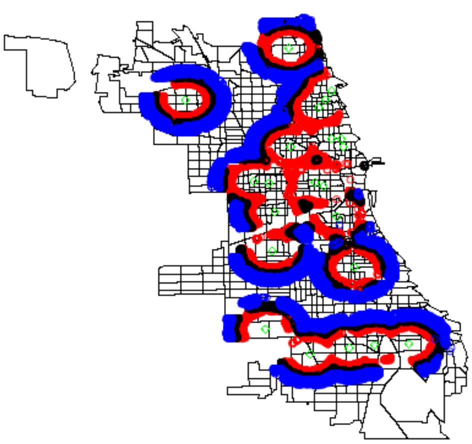
Treatment and Control Definitions



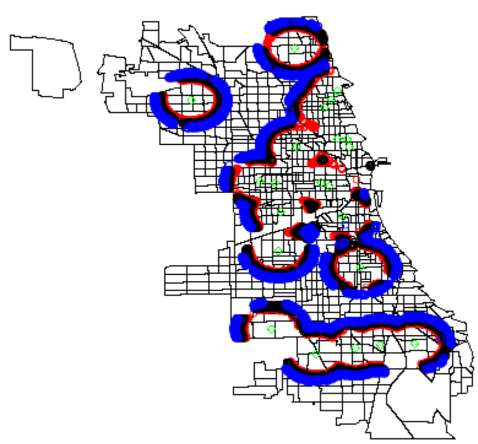
δ = 1.5



δ = 1



δ = 0.5



δ = 0.25

Figure 3

Median House Prices over Time

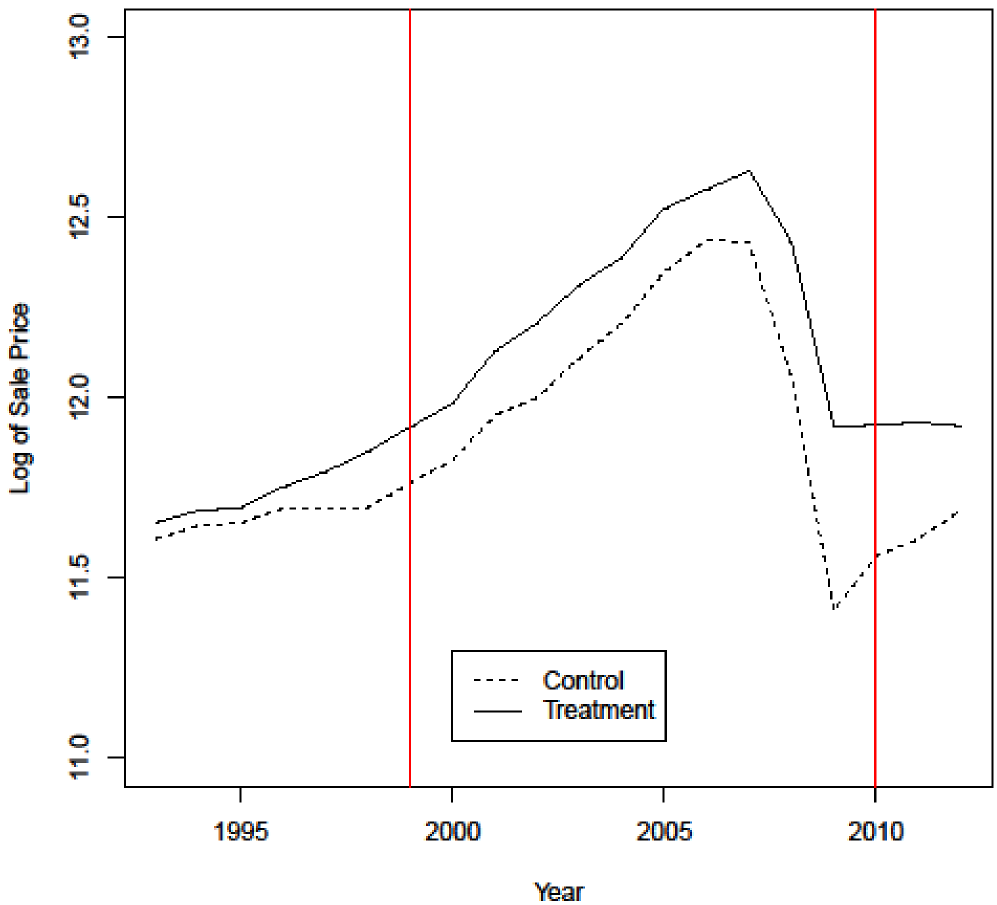
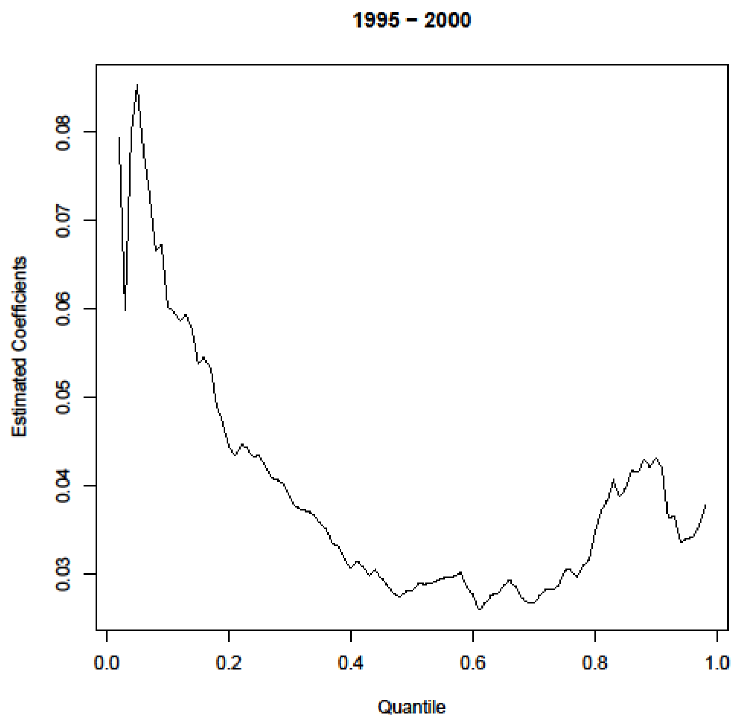


Figure 4

Quantile Regression Coefficients for Treatment, Post-Reform



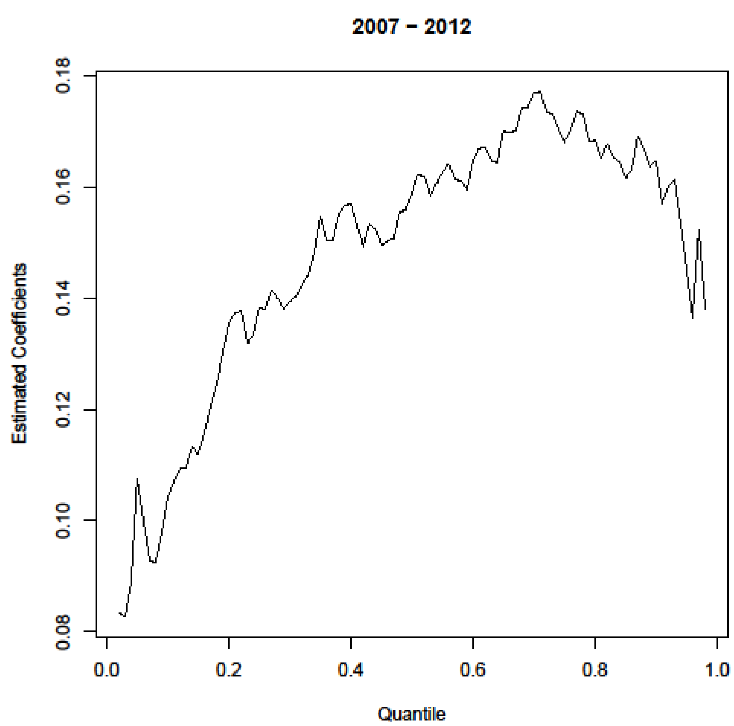


Figure 5

Quantile Estimates of Log Sale Price Distributions within 1.5 Miles of a Magnet School

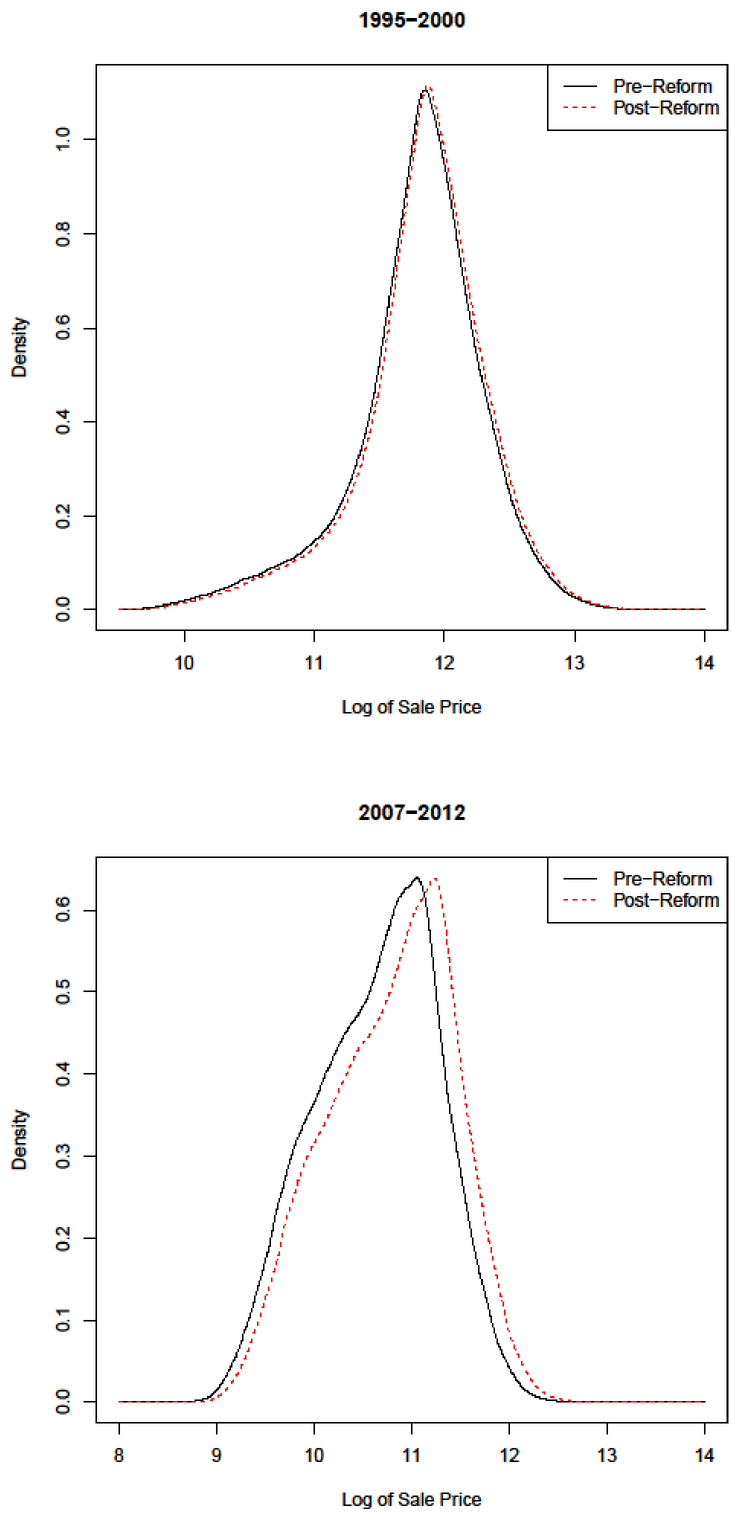


Figure 6

Quantile Estimates of Log Sale Price Distributions within 1.5 Miles of a Magnet School by Number of Nearby Schools