Does school quality affect neighborhood development? Evidence from a redistricting reform.*

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August 17, 2015

This paper studies the effect of school quality on housing construction. I exploit a redistricting reform in Charlotte-Mecklenburg, in which the district redrew a large number of residence-based school assignment zones. Redistricting leads to the abrupt creation of school quality discontinuities along new assignment boundaries. Using a regression discontinuity design along new boundaries, I find relatively minor and insignificant differences in the size and building quality of housing construction before the reform, but once school quality differences go into effect, housing construction on the high test score side of a new boundary is larger and higher quality.

JEL No. H0, I28, R0, R23, R31

*This is a revised version of the first chapter of my dissertation. I thank Marianne Bertrand, Matthew Gentzkow, Gary Becker, and Brent Hickman for their advice and support. I thank seminar participants in the University of Chicago Micro Lunch, the Family Economics Workshop, the Federal Reserve Bank of Chicago, and Enghin Atalay, Pietro Biroli, Gita Khun Jush, Joaquin Lopez, Sebastian Sotelo, and Adriaan Ten Kate for helpful comments and suggestions. Special thanks to Joe Brown in Mecklenburg country for providing the housing sales data and to Christine Hamlett for sharing the geocoded shapefiles of CMS assignment zones. Funding from the Margaret G. Reid Dissertation Fellowship is gratefully acknowledged. All errors are my own.

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1 Introduction

Many public school districts in the U.S. rely on residence-based assignment, which creates strong sorting of households into both neighborhoods and schools. While recent reforms in student assignment, including school choice and the expansion of charter schools, allow students to attend schools outside their assigned school, the composition of a school's students and the residents of its surrounding neighborhood remain strongly correlated.[1]

Empirically it is a challenge to move beyond correlations to identify a causal effect of school quality on the development of neighborhoods from the reverse causal effect. The correlation may simply be a consequence of household sorting on characteristics such as income, education, or preferences. In this paper I exploit a redistricting reform in Charlotte-Mecklenburg, North Carolina, to estimate the causal effect of an improvement in a neighborhood's assigned school on the development of new housing construction, including the building quality, square footage, number of bedrooms, and bathrooms of new housing.

Following a lengthy court battle, Charlotte-Mecklenburg was ordered in the fall of 2001 to dismantle its desegregation-based student assignment plan and redistrict its school assignment zones. The district complied, and, beginning in the 2002-2003 school year, introduced new assignment zones in which approximately half of students were assigned a different school.

The empirical strategy of this paper exploits the fact that inherent in the process of redistricting, some new school assignment boundaries will be cre-

[1] Parents exhibit strong parental preferences for nearby schools and school choice plans often grant priority to neighborhood residents (Hastings et al., 2005; Dur et al., 2013).

ated. New boundaries generate abrupt, local discontinuities in school quality between houses that previously shared a schooling assignment. Prior to the court decision, new assignment boundaries can be analyzed as "phantom" boundaries, since they have neither been announced nor have they taken effect. I perform a regression discontinuity (RD) along phantom boundaries to formally test whether boundaries are drawn endogenously to separate housing stocks that differ in house prices or physical attributes. The results suggest that there are slight but statistically insignificant differences in housing size or construction quality across new boundaries before they are announced. Once these boundaries go into effect, however, and local discontinuities in school quality are introduced between houses, an RD regression shows housing construction on the high test score side are 165 to 184 square feet larger, are rated 0.9 to 0.1 standard deviations higher in quality by the tax assessor, and are 2 to 3 percent more likely to have a brick facade. These results offer evidence that the stock of housing responds to changes in school quality, as the housing market anticipates changes in the composition of residents.

I perform a similar analysis of housing construction along boundaries that were eliminated as a result of the redistricting reform. Prior to the reform, these boundaries divided access to schools. When these boundaries were eliminated, the discontinuities in school quality delineated by these boundaries vanished along with them. The analysis of housing characteristics along these "destroyed" boundaries offers some, relatively weak, evidence for convergence in the characteristics of homes once school quality discontinuities disappear. The standard errors are large, making it difficult to draw more nuanced conclusions.

The identification strategy of this paper relies on the assumption that opposite sides of new boundaries do not differ in preexisting trends in new construction patterns. A concern for identification is that the school district may have drawn new assignment boundaries to incorporate developing neighborhoods into the high test score side. I explore the validity of this assumption with several empirical checks. First, I include additional controls that allow for differential time trends related to baseline neighborhood characteristics, and interacted fixed effects of boundaries with census block groups. Second, I include controls that allow flexibly for differential trends related to baseline boundary-side housing characteristics. Third, I test whether boundary-side pre-trends have predictive power for explaining whether houses are on the high test score side of a future boundary, which is important for the local randomness assumption underlying the RD design. These checks all lend support to the empirical design, and the point estimates are remarkably stable across robustness specifications.

This paper contributes to two distinct literatures. The first is the empirical literature studying the effect of school quality on neighborhoods. Prior work has studied the effect of school quality on house prices, the composition of neighborhood residents, and the propensity of students to commit crime (Clotfelter, 1975; Gill, 1983; Kane et al., 2006; Weinstein, 2014; Baum-Snow and Lutz, 2011; Deming, 2011; Lochner and Moretti, 2004; Billings et al., 2012). This paper's contribution is to study the effect of school quality on housing construction, which represents an important proxy for neighborhood development. This paper emphasizes, and provides direct evidence, that school policies act as neighborhood policies through their effect on housing construction. In a school district in which residence guarantees access to schools, a reassignment of school quality across houses will lead household preferences over housing attributes to interact with supply to generate changes in the housing stock.

This paper also contributes to the boundary RD design used in prior empirical work (Black, 1999; Kane et al., 2006; Bayer et al., 2007; Fack and Grenet, 2010).[2] Black (1999) introduced a regression discontinuity design that exploits discontinuities in school quality along attendance zone boundaries to estimate the implicit price paid for a better school assignment. This empirical strategy attempts to hold fixed unobserved neighborhood characteristics that are shared by houses along the same boundary, as school quality discontinuously jumps across the boundary line. This RD design faces two sources of potential endogeneity – first, boundary lines themselves may be drawn to separate neighborhoods that differ in unobservables; second, residents with a better school assignment may invest more in unobserved housing characteristics. Both of these concerns will bias estimates because unobservables will not be smooth across boundaries and will be correlated with both school quality and housing values.[3]

[2]This strategy has been used outside the education setting: see, e.g. Chen et al. (2013); Lavy (2006); Lalive (2007); Pence (2006).

[3]Studies beginning with (Black, 1999) have attempted to address the first concern by restricting the sample to boundaries that do not coincide with major roads or highways, but it has not been empirically tested whether this strategy reduces or eliminates bias. To my knowledge, no studies have empirically studied the second concern, but it has been raised before by Bayer et al. (2007).

The contribution of this paper is to use new and destroyed boundaries to test for these two sources of bias. An RD along new boundaries before they are announced represents a test of boundary lines being drawn to separate neighborhoods based on preexisting unobserved characteristics. Similarly, an RD along destroyed boundaries that are no longer effective represents a test of unobserved housing investments that are correlated with prior treatment status. Intuitively, this paper exploits both spatial and temporal shocks to school quality induced by boundaries appearing and disappearing, instead of using only cross-sectional variation in school quality across space.

The rest of the paper is organized as follows. Section 2 describes the redistricting reform in Charlotte-Mecklenburg. Section 3 describes the data and presents descriptive statistics. Section 4 presents the identification strategy. Section 5 presents the results along with several robustness checks, Section 6 discusses the results, and Section 7 concludes.

2 Redistricting in Charlotte-Mecklenburg

From 1971 through the fall of 2002, student assignment in Charlotte-Mecklenburg (CMS) was supervised by a federal court, which required the district to take active steps to maintain racial balance in its schools.[4] The district adopted

[4]The CMS desegregation plan was put in place after the 1971 U.S. Supreme Court ruling in *Swann v. Charlotte-Mecklenburg Board of Education*, which mandated that CMS take concrete steps such as busing to achieve racial integration in schools. The Court's decision had repercussions for school districts throughout the U.S. since it required districts to actively desegregate schools. Since the 1960s, hundreds of school districts have followed court-supervised student assignment plans, and many of these plans are in full or partial effect, although in recent years courts have been steadily dismantling these programs (Reardon et al., 2012).

a residence-based assignment plan in which school zones were gerrymandered across neighborhoods to achieve integration targets, and students were often bused long distances to attend their assigned school.

In 1997, parents of a white student in CMS sued the district, arguing that their child was denied admission to a magnet school because of her race. This suit prompted a lengthy battle in the courts, eventually leading to a September 2001 U.S. Court of Appeals ruling that declared CMS "unitary" and ordered it to redraw student assignment boundaries without regard for race.[5] In December 2001, the school board voted and approved new student assignment zones, redrawn to largely coincide with schools' surrounding neighborhoods. To offset anticipated resegregation, the district also approved a district-wide school choice plan, which began along with the new assignment zones in the 2002-2003 school year.[6]

Figure 1 depicts the old and new assignment zones. Approximately 50 percent of students received a new school assignment. The figure also highlights an example of an elementary school zone that was redistricted, Nathaniel Alexander elementary, whose assignment zone consisted of two disjoint regions that was redrawn to coincide with its surrounding neighborhood.

The school choice component of the assignment plan adopted by CMS

[5]This legal action was recorded as *Capacchione v. Charlotte-Mecklenburg Schools*. An appeal by the district to the U.S. Supreme Court was denied in April 2002, effectively ending the desegregation order for CMS.

[6]The redistricting component of the Charlotte-Mecklenburg reform has been exploited as quasi-experimental variation in school quality by several studies, estimating the effect on teacher supply, long-run criminal behavior, and the race composition of neighborhoods (Jackson, 2009; Billings et al., 2012; Weinstein, 2014). The school choice component of the reform has been studied extensively as well (Hastings et al., 2005; Hastings and Weinstein, 2008).

followed closely the existing intra-district choice plans in place in New York and Boston. Under the CMS plan, students were guaranteed a seat at their zoned school assignment. If parents preferred an alternative school they could rank up to 3 schools in the district, including magnet schools. Parents could list any school in the district, but were provided transportation only to those schools within one of four transportation regions. CMS anticipated a high demand for seats at particularly desirable schools, and increased the capacities of schools to try to accommodate parents' preferences. Schools, nonetheless faced capacity constraints, and oversubscribed schools admitted students by centralized lottery.

In the first year of its implementation, 65 percent of white parents chose their residence-based assignment as their first choice, compared to 40 percent of non-white students (Hastings et al., 2005). About 13 percent of students who won the lottery to attend their first choice school subsequently decided to attend their assigned school instead (Hastings et al., 2005). Using residential address data of students, Billings et al. (2012) reports that approximately 65 percent of students attended their assigned school prior to the reform, which dropped to 57 percent in 2002-2003, and which subsequently rose to 65 percent by 2005-2006.

3 Data

The main data set used in this analysis consists of all residential home sales transactions in Mecklenburg county – including each home's exact residential address, sales date and price – over the period 1998-2006. This data is collected by the Mecklenburg county Tax Assessors office and is in the public record. I merge this data with detailed parcel data characterizing the property, also maintained by the Tax Assessors office, which uses this data for the assessment of property taxes. This includes details on building quality, land use, the exterior material of the home, number of bedrooms and bathrooms, air conditioning, and square footage of heated area. The Tax Assessor rates building quality into the following categories: below average, average, good, very good, excellent, and custom. I assign each rating category an integer number from 1 to 6, in increasing order of building quality; I then standardize them to have mean 0, standard deviation 1 in the sample.[7] Exterior material of the house includes aluminum/vinyl, brick, masonite, stucco, hardiplank, etc. For new housing construction, the most common materials include aluminum/vinyl, brick, and masonite. Since brick commands the highest average sales price among those categories, I use an indicator for brick facade as one of the outcome measures of interest in the analysis.

The main analysis uses sales of units that are classified as single family residential under land use type, and that are described as a residence under parcel description. I divide the housing sales sample into the pre-reform period, 1998-2001, and post-reform period, 2002-2006. Because the court decision was in September 2001 and the new boundary announcement was in December 2001, I drop housing sales in the 4th quarter of 2001 and in the 1st quarter

[7]The rating category "custom" represents only 0.78 percent of all home sales. Because it has the highest average sales price of all the categories, I assign it a value of 6 in the numerical counterpart of building quality.

of 2002 to allow some time for information about changes in school quality to transmit to home buyers and sellers.[8] I define new housing construction in the pre-reform period as those constructed within 1996-2001, inclusive; I define these houses in the post-reform period as those built from 2002-2007 inclusive.[9]

I match each residential address with characteristics of its surrounding neighborhood using data from the 2000 U.S. census. Each residence is linked with its U.S. census block group using geographic shapefiles available on the census website. I use these block group identifiers to merge neighborhood characteristics at the census block-group level from the 2000 census and the 2005-2009 American Community Survey. Block-group level characteristics include the population fraction in each race category, median household income, and the average educational attainment for adults over 25.

I link each residential address with its elementary school assignment zone in both the pre- and post-reform periods using geographic shapefiles of school attendance boundaries provided by the school district. I use elementary school boundaries, instead of middle or high school boundaries, for two reasons: first, elementary schools have much smaller student populations and hence there are many more in the district to provide useful variation in school quality; second, mounting empirical evidence suggests that early childhood education generates

[8]The results are not sensitive to this choice, and the results based on alternative choices of this window around the announcement date are available upon request.

[9]A large number of houses in the sales data have a year built date that postdates the year of sale. I use an alternative method to define new construction as houses whose "age", defined as the year sold minus the year built, is between -1 and 2 years. I present the results using this alternative definition in the Supplementary Appendix.

large and persistent effects into adulthood compared to education in adolescence (Cunha et al., 2006), and, hence, the elementary school environment is particularly important for residential choice and house prices.

For each home, I locate the nearest school attendance zone boundary based on straight-line distance from the residential address, and record its nearest neighbor as the school zone on the opposite side of the boundary. Each boundary dummy is defined as a continuous border joining two school zones, or, equivalently, as a distinct pairing of the two schools sharing a border.

I supplement the school assignment data with student- and school-level data obtained from the North Carolina Education Data Research Center. This data includes student microdata on the universe of public school students in North Carolina, with demographic data including free-lunch eligibility, race, parental education, and student outcomes data such as test scores. The school-level data comprises data on teacher education and licensing, student dropout rates, and crime incidence reports. Under North Carolina state law, all public school students in grades 3-8 must take the statewide End of Grade exams, which measure student reading and math achievement. These are the primary measures of school quality used in the analysis. School addresses are from the Common Core Data available from the National Center of Education Statistics.

Test scores as a measure of school quality

Throughout the empirical work I use school-level average test scores on the North Carolina statewide End of Grade exams as a proxy measure of school quality. Test scores are a proxy for student achievement at the school, and can be viewed as a function of student, teacher, and school characteristics, including student ability, socioeconomic background and education of parents, teaching quality, and facilities and resources available to the school.

I use test scores as a proxy for three reasons. First, to provide comparability with the substantial literature estimating household willingness to pay for school quality (Black, 1999; Bayer et al., 2007). Since one of the contributions of this paper is methodological – testing the identification assumption underlying the boundary RD design used widely in the literature – proxying for school quality with test scores allows for comparison with earlier studies.

Second, empirical evidence suggests that parents have strong preferences for student achievement when they select schools. These results are robust to whether one examines survey responses of parents (Lee et al., 1994), revealed preferences (Jacob and Lefgren, 2007), or willingness to pay in the housing market (Kane et al., 2006; Bayer et al., 2007; Black, 1999).

Third, for the purposes of the question addressed in this paper, the ideal measure of school quality is one that captures many dimensions of the school environment that parents value when choosing a school, which includes both the achievement and family background of peers and the value-added of the learning environment. Student test scores, while by no means a perfect measure, are a parsimonious proxy for this multi-dimensional environment, and they are easily observable to both parents and researchers.

3.1 Defining new and destroyed boundaries

I now describe how I construct the boundaries used in the analysis. Using the pre- and post-reform assignment boundary shapefiles, I construct two separate boundary files: (1) boundaries that are present in the post-period but not in the pre-period – these are the *new* boundaries; (2) boundaries that are present in the pre- period but are not in the post-period – these are the *destroyed* boundaries.

To generate the new boundaries, I take the post-reform boundaries and I "erase" the part that overlaps the pre-reform boundaries; the boundaries that remain after this erasure are labeled new because these lines are not used as boundaries in the pre-period.[10] It is possible for a portion of the boundary to be new while another portion overlaps with the old boundary. For these cases, I simply redefine the boundary as two boundaries, consisting of the portion that is new and the portion that is common, and I link houses to the nearest boundary of this finer partition. To create the new boundary dummies used in the regression analysis, I interact the new boundary identifiers with the pre-reform school assignment; this is done to ensure that in the regression we are comparing houses that shared a pre-reform school assignment. In total there are 106 new boundary dummies.

To generate the destroyed boundaries, I take the pre-reform boundaries and I "erase" the part that overlaps with the post-reform boundaries; the

[10] In ArcGis, there is literally an "erase" geoprocessing tool that allows one to do this. In practice, I erase a buffered version of the pre-reform boundaries (buffered at .1 km on each side), so that small changes in a boundary line, say, a one-block lateral move of a boundary would not count as a "new" boundary.

boundaries that remain are labeled destroyed because these lines are no longer used as boundaries in the post-period. To create the destroyed boundary dummies used in the regression analysis, I interact the destroyed boundary identifiers with post-reform school assignment; this is done to ensure that in the regression we are comparing houses that share a post-reform school assignment. There are 139 destroyed boundary dummies.

There were small year-to-year changes in boundaries that took place in CMS that were not part of the major 2001 reform. These were minor adjustments that were made to accommodate a new school opening or closing, or to respond to population shifts or overcrowding. To reduce the likelihood that these small variations are interfering with the analysis, I drop boundaries that had small changes within either the pre- or post-reform time periods. In addition, I restrict the analysis to boundaries that are at least 200 meters in length.

3.2 Summary statistics

Table 1 presents summary statistics for new construction of single family homes in Mecklenburg county during the sample period, 1998-2006.[11] The samples are divided into pre- and post-periods based on their sales date, and are divided by boundary type (new or destroyed).

The mean sales price of new housing construction in Mecklenburg country was $219,560, in 2000 USD, during the pre-period, substantially more than

[11]Supplementary Table A.2 presents summary statistics for all housing sales transactions over the sample period.

the average sales price of all sales ($197,870). During the pre-period, housing construction along new boundaries had a mean sales price of $141,170, while housing construction along destroyed boundaries had a mean sales price of $171,720. On average, homes along boundaries lines sold for less than those not on boundaries, reflecting the reality that boundary lines appear more frequently in densely populated, urban areas, which are less affluent than suburban areas.[12]

It is evident that both the volume of sales and house prices increased substantially in Mecklenburg county over the sample period, reflecting the nationwide housing boom between 1998 and 2006. Housing construction increased in sales price by 3.9 percent (from $219,560 to $228,440). Along new boundaries, prices of new homes increased 19.8 percent, while along destroyed boundaries they increased 12.6 percent.[13] The average sales price of all housing increased 11.8 percent in the county over this period.

Levels and trends of other housing characteristics reflect a similar pattern as house prices. The lower section of the table presents average census block group level characteristics for housing sales. Houses along new and destroyed boundaries are less affluent and have a higher fraction of black residents, compared to those houses not along boundaries.

[12] This gap in house prices between boundary and non-boundary areas is reported in studies of other urban school districts (Bayer et al., 2007).

[13] It is important to emphasize that the identification strategy used in this paper does not require similar levels or trends between houses on new boundaries and those not on boundaries. What is required is that the trends on *opposite sides* of new boundaries would be the same in the absence of the reform, and likewise for destroyed boundaries.

4 Identification strategy

When school quality is reassigned to housing stock, demand over the vector of housing attributes will interact with supply to alter the equilibrium composition of housing stock. This section presents an empirical strategy to estimate the extent school quality affects the development of neighborhood housing stocks.

An ideal study might take the form of a field experiment that randomly assigns school quality to neighborhoods, after which we observe the evolution of neighborhoods, comparing the housing construction in neighborhoods assigned high quality schools to those assigned low quality schools. In practice it is difficult to implement such an experiment, but we might think of a natural experiment in which new school assignment boundaries are redrawn to separate houses that previously had access to the same school. Such boundaries create local discontinuities between houses that were previously absent. We can then observe how the neighborhoods evolve across the discontinuity, before and after its appearance. Figure 2 depicts one such boundary used in the identification strategy; it separates a neighborhood that previously had the same school assignment, creating a school quality discontinuity where there previously was none.

Graphical illustration of the RD design

To show that new boundaries indeed create new discontinuities in school quality, I take the sample of new boundaries and regress school test scores on boundary fixed effects and on .02 mile band distance-to-the-boundary dummy variables. Negative distances indicate the "low" test score side of the new boundary. I perform this regression for both the pre- and post-reform periods separately, using the entire sample of housing sales, including both new and existing housing stocks. The coefficients on the distance bins reflect the conditional average test score at a given distance to the boundary. Figure 3 plots these regression coefficients. We see clearly that houses that previously had the same school quality experience a discontinuity between them. New boundaries introduce about 0.3 standard deviations in school-level average test score between houses, a sizable shock.

I repeat the exercise with different house and neighborhood characteristics as the dependent variable and plot the coefficients, presented in the bottom two panels of Figure 3. The coefficients for the pre-reform period reveal to what extent, if any, new boundaries are being drawn to separate housing stock of differing pre-existing characteristics. There does not appear to be a visually noticeable difference in the quality of housing stock prior to the new boundaries. If anything, some characteristics appear *less* desirable on what will become the high test score side of the new boundary. For instance, heated area and building quality index appear to be lower. Neighborhood characteristics, which are based on the 2000 census, reveal little discontinuity across phantom boundary lines. Overall, the bottom-left panel suggests the boundaries were locally drawn in a way that does not separate housing based on pre-existing levels.[14]

[14]Note that this does not rule out the possibility that globally, i.e. within the district as a whole, poor neighborhoods are being redistricted into worse schools, while good neighborhoods are being redistricted into better schools.

The coefficients for the post-reform period (bottom-right panel of Figure 3) show how housing stock differs across boundaries once they go into effect. Note these estimates include the full sample of housing sales (sales of existing stock plus sales of new construction), which make it difficult to distinguish patterns in new construction. It does appear, however, that there is a positive trend in building quality suggesting that houses on the good school side are higher quality once the boundaries go into effect.

Neighborhood characteristics in the post period are based on 2005-2009 ACS data. What is remarkable is the pattern observed for median household income. Before the boundaries going into effect there appears little, if any, increase in household income when crossing from low to high test score side of phantom boundaries. Once the boundaries go into effect in the post-period, however, the panel reveals that residents have substantially higher neighborhood income on the high test score side of the boundaries. This evidence strongly suggests in-migration of higher income residents to the high test score side of the boundary once the school quality discontinuity is introduced. When examined by race, this pattern is less pronounced.

Figure 4 performs the same exercise using destroyed boundaries. I regress school test scores on destroyed boundary fixed effects and on .02 mile band distance-to-the-boundary dummy variables. The top panel of Figure 4 clearly illustrates that once boundaries vanish school quality discontinuities vanish as well.

Consider now the bottom two panels of Figure 4. The bottom-left panel suggests that housing and neighborhood characteristics do not have a strong pattern of observables being correlated with the high test score side of the boundary, although Bayer et al. (2007) reports a positive relationship between boundary side and housing characteristics. What is striking, however, are the neighborhood characteristics once the boundaries disappear. Once the school quality discontinuity vanishes, the fraction black is much higher on what was previously the high test score side. Similarly, the neighborhood household income is relatively lower on what was previously the high test score side. Again, this finding suggests that neighborhood residents from the high test score side relocate once once boundaries disappear.

Estimation equation: new boundaries

To estimate the effect of school quality on new housing construction, I estimate the following regression:

$$x_{ht}^{(k)} = \gamma_{0t} + \gamma_1 high_h^{new} + n_{ht}^{'}\gamma_n + \theta_{bh}^{new} + \epsilon_{ht} \tag{1}$$

where $x_{ht}^{(k)}$ represents housing characteristic k of house h in time t. Characteristics used in the analysis include the number of bedrooms, bathrooms, the number of square feet of heated area, building quality, a dummy for a brick facade, and sales price. The vector n_{hk} represents neighborhood characteristics (measured and the census block-group level) including the percent of residents who are black, Hispanic, and Asian, average educational attainment, and median household income. The vector θ_{bh}^{new} represents a full set of new boundary dummies interacted with pre-reform school assignment. It is important to interact boundaries with pre-reform school assignment so that conditional on

θ_{bh}^{new}, all houses along the j-th boundary have no difference in the quality of their assigned school prior to the new boundary. The j-th element of θ_{bh}^{new} takes a value of 1 if house h is within a specified narrow band on either side of the j-th boundary. The variable $high_h^{new}$ is a dummy indicating that house h is on the high test score side of the new boundary. The residual ϵ_{ht} represents all other attributes of h that affect its market price but are unobserved to the researcher.

Equation 1 is estimated using only the sample of homes that lie along a new school assignment boundary.[15] For this analysis I include the full sample, including houses that report zero sales price; zero-price sales indicate bequests, trades, swap-outs, or other transactions the land use department deems not conventional sales.[16]

I estimate Equation 1 separately for the pre- and post-reform periods. In the pre-period, the estimate of γ_1 represents the effect of being on the high side of a *future* boundary, on house characteristic $x^{(k)}$. Because the pre-period precedes the discussion or announcement of the redistricting the will occur following the court decision in September 2001, there is little reason to suspect that households anticipate where the new boundaries will be locally drawn.

[15] This creates the usual tradeoff between efficiency and bias when selecting a bandwidth, which specifies the maximum distance a house can be from the boundary and still be included in the sample. A smaller bandwidth shrinks the radius around the boundary and reduces concern for unobserved differences between houses across the boundary line, but it also reduces the sample size and decreases precision in the estimates. In the results section, I follow Bayer et al. (2007) and report results for 0.2 mile and 0.1 mile subsamples. Black (1999) reports results estimated for the subsamples of houses within 0.35, 0.2, and 0.15 mile of a boundary. Kane et al. (2006) use the subsample of houses within 2,000 feet, 1,000 feet, and 500 feet from a school boundary.

[16] In the Supplementary Appendix, I reproduce the main results using a sample that excludes these zero-price sales.

Since there is no difference between high and low sides of the boundary in terms of school quality in the pre period, if we observe a positive and significant estimate for γ_1 in the pre-period it would suggest that boundary lines are being drawn endogenously by school officials to separate pre-existing differences in neighborhoods. Moreover, a positive and significant estimate would bring suspicion to the boundary RD design used by many researchers in the education literature because it would suggest boundary lines are not locally random.

In the post-period estimation of Equation 1, the coefficient γ_1 represents the effect of being on the high test score side of the new boundary, once the boundary is in effect and the discontinuity in school quality separates houses across the boundary lines. This is the main estimate of interest of this paper, and it represents the effect of being on the high test score side on housing characteristic $x^{(k)}$. This coefficient represents the short-run response – over the 2002-2006 period – to the school quality changes brought about by the redistricting reform, which was announced in December 2001.

Destroyed boundaries

I perform an analysis to see how new housing construction is affected when school quality gaps are eliminated. To do this, we consider the sample of new housing construction along destroyed boundaries and estimate:

$$x_{ht}^{(k)} = \delta_{0t} + \delta_1 high_h^{dest} + n_{ht}^{'}\delta_n + \theta_{bh}^{dest} + \epsilon_{ht} \tag{2}$$

We estimate Equation 2 for both pre-reform and post-reform periods so that we see the evolution of housing construction along destroyed boundaries once school quality gaps are eliminated.

In the pre-period, the coefficient δ_1 represents the effect of being on the high test score side of a boundary, while the boundary is in effect, on new housing construction characteristic $x^{(k)}$. Note that this coefficient represents a long-run equilibrium in the housing market, since all or most of these boundaries had been in effect since the 1970s.

In the post-period, the coefficient δ_1 represents the effect of being on the *formerly* high test score side following the elimination of the boundary and the school quality discontinuity that separated houses on opposite sides of the boundary. If we were to find a positive and significant δ_1, it would reflect that boundaries were separating housing stocks of different unobserved quality that persists after the school quality gap is eliminated. This result would suggest that some unobserved feature of the neighborhood is driving construction patterns instead of school quality. If we were to find a negative coefficient on δ_1, it would reflect that housing characteristics are converging between high and low test score sides once the school quality gap is eliminated. Hence, a negative coefficient on δ_1 in the post-period would reflect that neighborhood housing stock and/or the distribution of new residents is responding to the school quality shock.

Assessing the identification strategy

The identification strategy has an advantage over prior studies that use a boundary RD design because there is temporal variation in the boundaries.

This temporal variation allows us to explicitly test whether the outcome variable varies discontinuously across the RD threshold, which is the key assumption necessary for identification in an RD setting.

Despite this advantage, the identification strategy used here demands scrutiny in three specific areas. The first is a consideration of why particular boundaries were redrawn while others were not.[17] For instance, suppose the district only redrew boundaries in local areas where residents have weaker preferences for public school quality. Note first that this concern will not bias estimates from Equation 1; it only suggests that the results using the sample of housing stock along new boundaries may not be generalizable to *all* houses in the county, an external validity challenge shared by all studies that use a boundary RD design.

A second consideration is the presence of spillovers in housing characteristics from the high test score side of the boundary to the low test score side. For instance, neighbors on the low test score side may wish to make new housing investments as they observe their neighbors on the high test score side moving into bigger houses. In this case we might fail to detect, or we might underestimate, the effects of school quality on housing construction, and our estimates would represent lower bounds of the true effect.

A third consideration, and the most serious threat to identification, is whether boundaries are drawn to separate areas with differential trends in home construction – for example, that district officials want to encourage ar-

[17]School boundary debates tend to be intensely controversial because boundaries affect not only access to education for children but also housing values for residents who have no school-aged children. See, for example, coverage of the recent battle in Washington, D.C. over school redistricting (Brown, 2014).

eas they see as booming by including them in the assignment zone of the better school. This assumption is considered in detail in Section 5.1.

5 Results

I now present the main results of the paper: the effect of school quality on new housing construction. To do this, I restrict the sample to new housing construction along new boundaries. The goal is to see if new construction differs in observable characteristics on opposite sides of new boundaries before and after the school quality gap appears between the two sides.

Table 2 presents estimates of Equation 1 with several alternative house characteristics as the dependent variable. Column 1 estimates Equation 1 with new boundary dummies but without any additional neighborhood controls. Column 2 adds census block-group controls from the 2000 census: fraction black, Hispanic, and Asian, fraction of population over 25 with college-degree, and median household income. Column 3 adds these neighborhood controls interacted with a linear time trend, and Column 4 adds boundary-by-census-block-group dummies. For Columns 1-4 of Panel (A), the coefficient on high side dummy represents the difference in bedrooms on the high side of the phantom boundary compared to the low side. The coefficient is relatively stable – between .04 and .05 more bedrooms on the high side – but is not statistically significant. Columns 5-8 of Panel (A) show that once boundaries go into effect, the point estimate does not change dramatically, and it is statistically significant at the 10 percent level only in Column 5.

Panel (B) shows that new housing construction does not appear to have more bathrooms on the high test score side of phantom boundaries. Once boundaries go into effect, however, housing construction has between 5 and 7 percent more bathrooms, and these estimates are statistically significant. Panel (C) presents the results for square feet, again showing that new housing sales are not statistically different before the new boundaries are announced. Once boundary lines go into effect, however, new housing construction on the high test score side of the boundary have 162 to 184 more square feet.

Panel (D) suggests that building quality is between .02 and .035 of a standard deviation higher on the high test score side, *prior* to the announcement of boundaries, with a statistically significant coefficient in columns 1 and 4. This suggests that new construction is slightly higher quality on the high test score side of the phantom boundary. Once boundaries go into effect we see larger differences in building quality, ranging from .09 to .1 of a standard deviation between the high and low test score side of the boundary, with statistical significance in all columns at the 5 percent level. Panel (E) shows that brick facades are not more likely on the high test score side, prior to the reform. One the school quality gap goes into effect, new housing construction is between 2 and 3 percent more likely to have a brick facade on the high test score side.

The evidence presented thus far presents somewhat limited evidence that boundary lines are drawn endogenously to separate housing stock, since newly constructed homes do not appear statistically different on opposite sides of phantom boundaries. The limited evidence for differences across boundaries in the pre-reform period also suggests that households do not have private information or anticipate the location of the new boundaries in advance of the court ruling, otherwise new construction would reflect these new boundaries prior to their announcement. The post-reform results of Table 2, however, present strong evidence that increases in school quality generate larger and higher quality housing construction.[18]

Table 2 presents mixed support for the boundary discontinuity strategy introduced by Black (1999) and used widely in the literature. In favor of the methodology is the result that housing construction does not appear to reflect school quality differences prior to the boundaries going into effect. Against it is the result that even though boundary lines may be drawn in a way that is locally random, housing construction evolves in a way that is correlated with school quality.

Table 3 presents the regressions with log sales price (2000 USD) as the dependent variable in Equation 1. We lose observations in the regression because it excludes those with sales price of 0, which represent bequests, swap-outs, and other non-conventional sales. We see housing construction having between 6.6 percent and 8.6 percent higher price prior to the boundaries going into effect, although these estimates are not statistically significant. These estimates increase to between 7.9 and 9.2 percent premium, once the boundaries go into effect, which is statistically significant at the 10 and 5 percent levels in columns 6-8. Note that these regressions are not conditional on observable house characteristics and hence represent the unconditional house price premium of on

[18] The Supplementary Appendix reproduces Table 2 with .15 mi and .10 mi bandwidths and the results are qualitatively similar. Supplementary Appendix Table A.3 reports the results excluding zero-price sales in the data and the results again quite similar.

the high test score side of the boundary.[19]

We have shown above that school quality affects the *intensive* margin of housing construction, in terms of the size and quality of new construction. Next I consider whether school quality affects the *extensive* margin, represented by the fraction of home sales that are new construction. Note that a higher fraction may reflect either an increase in total housing stock, or it may simply reflect that a greater share of sales are new construction.

I estimate

$$newhouse_{ht} = \gamma_{0t} + \gamma_1 high_h^{new} + n_{ht}^{'}\gamma_n + \theta_{bh}^{new} + \epsilon_{ht} \tag{3}$$

where $newhouse_{ht}$ is an indicator for the house being new construction, $post_{ht}$ is an indicator for a post-reform sale, $high_h^{new}$ is an indicator for being on the high side of the new boundaries, x_{ht} includes sales year and sales quarter dummies, and θ_{bh}^{new} is a vector of new boundary dummies. The sample includes all housing sales (new construction and existing housing stock). Hence the coefficient γ_1 represents the fraction on the high test score side that are new housing construction, relative to the fraction on the low test score side.

Table 4 presents the estimates. The point estimates do not change much between pre- and post-periods, although the standard errors shrink in the post-period and the estimates become significant due to the presence of more observations. This table suggests that the housing construction estimates re-

[19]The regressions conditional on house characteristics are presented in Supplementary Appendix Table A.9. The results of the conditional analysis suggest that the unobserved component of house prices is larger on the high test score side of the phantom boundaries in the pre-period, relative to the low test score side, although it is statistically no different from zero.

ported in Table 2 are not due to changes in the "extensive" margin – i.e. increases in new housing as a share of total sales – but rather due to changes in the "intensive" margin, i.e. the quality of the new housing stock that is built.

Destroyed boundaries

Table 5 presents results from estimating Equation 2. What we see is that in the pre-period, while the boundaries are in effect, new housing characteristics are not significantly different on the high test score side of the boundary. The point estimates are somewhat large, however. For example, in square feet, houses on the high test score side are 102 to 168 square feet larger, building quality is .07 to .14 of a standard deviation higher, and houses are 3 to 6 percent more likely to have a brick facade while the boundaries are in effect. None of these estimates are statistically significant, however. Once boundaries are eliminated, the side of the boundary that once had, but no longer has, the high test score assignment has slightly smaller and lower quality housing construction, suggesting slight convergence in housing construction once boundaries are eliminated and school quality differences vanish. We can see this clearly by examining square feet, whose point estimates range between -200 and 0 feet larger on the high test score side, once school quality differences vanish.

Table 6 presents the results from estimating Equation 2 with log sales price as the dependent variable of Equation 2. These regressions present the unconditional premium, i.e. without any other house characteristics included as controls. The results show a premium of 7.9 to 9.4 percent while the boundaries are in effect, although when we add neighborhood controls interacted with a linear time trend or boundary-by-census dummies – columns 3 and 4 – we lose statistical significance. These effects decrease to between -9.3 and and 5.6 percent once the boundaries are eliminated and the school quality gap vanishes. The effects are still economically significant after the boundaries are eliminated.[20]

5.1 Robustness

We just saw that the estimates are robust to the inclusion of a wide range of controls, including baseline neighborhood covariates interacted with a linear time trend. I now present further robustness checks to increase our confidence in the results. Recall that an important requirement for our estimates to be unbiased is that the high test score side of the boundary may not have differential trends in housing construction relative to the low test score side.

To allow for differential trends on opposite sides of the boundary, I perform the following exercise. For both high and low sides of each boundary dummy I estimate boundary-side averages of new housing characteristics during the pre-period (over 1998-2001). For each boundary-side, indexed by bs, and each housing characteristic $x^{(k)}$, I construct $x^{(k)}_{bs,0}$ which are the pre-period averages. I then interact these characteristics with a linear time trend and estimate the

[20]The sales price regressions conditional on house characteristics are presented in Supplementary Appendix Table A.4. The results of the conditional analysis suggest that most of the observed premium is due to observed characteristics of housing, although the large standard errors make it difficult to come to more nuanced conclusions.

following for each characteristic x, in the post-period:

$$x_{ht}^{(k)} = \gamma_0 + \gamma_1 high_h^{new} + \lambda_1 x_{bs,0}^{(k)} t + \theta_{bh}^{new} + \tau_t + \epsilon_{ht} \tag{4}$$

This regression allows opposite sides of boundaries to have differing underlying time trends related to their pre-reform characteristics. The estimates are reported in Table A.7 and are remarkably similar in magnitudes to the baseline estimates of Table 2.

To further assure us that pre-trends are not behind the estimates, I test whether pre-trends have any predictive power for explaining whether a house is on the high side of a future boundary. Again, this is to reassure us of the local randomness assumption of the RD design.[21] To do so, I estimate the following regression over 1998-2001 period:

$$high_{hs}^{new} = \delta_0 + \delta_1 x_{bs,0}^{'} t + \theta_{bh}^{new} + \tau_t + \chi_{bs} \tag{5}$$

The regression allows us to test whether baseline house characteristics, on opposite sides of phantom boundaries, predict which side of the boundary a house is on once the boundary goes into effect. Table A.8 of the Supplementary Appendix reports the regression results, which shows that estimates of the vector δ_1 shows small and statistically insignificant effects. This result supports the identification assumption that high and low test score sides of new boundaries share common trends absent the redistricting reform.

[21] This approach is analogous to the robustness check of Akerman et al. (2015) and Bhuller et al. (2013), which estimate a regression to test whether baseline characteristics predict future changes in the treatment variable.

6 Discussion

Our measure of school quality – test scores – is a proxy for a vector of attributes of the school assignment. This proxy may reflect student ability, teacher ability, the ethnic composition of the student body, or other school-level variables that correlate with test scores. The data do not allow us to disentangle which of the school-level attributes is affecting housing construction.

Perhaps the most intuitive mechanism behind the results is that a positive shock to school quality generates an in-migration of richer residents, who demand larger and better quality housing. This is consistent with a hedonic framework in which households have preferences over a vector of housing attributes; as school quality is reassigned to houses, the demand and supply will interact to generate a new stock of housing (Rosen, 1974; Ekeland et al., 2004). This effect would be driven simply by school quality and housing attributes being normal goods, or being complements on the demand side.

This mechanism has some support from Figures 3 and 4. Figure 3 shows little if no difference by household income across new boundaries before they are announced. Once they go into effect, however, the high test score side is populated by residents with higher household income. Figure 4 shows a similar pattern for destroyed boundaries. While boundaries are in effect there is no clear difference by income or race. After the boundaries are eliminated, there is a clear decrease in household income on the formerly high test score side, and an increase in the fraction of black residents in the neighborhood.

7 Conclusion

It is empirically challenging to estimate the causal effect of school quality on the development of neighborhoods. This paper exploits a redistricting reform in Charlotte-Mecklenburg to study the effect of school quality on housing construction, including the size and building quality of new residences.

This paper contributes to the widely-used strategy of Black (1999) by studying new boundary discontinuities that appear disappear as a result of a redistricting reform. I find minor differences in the size and building quality of housing construction before the new boundaries go into effect, but once they do, housing construction on the high test score side of a new boundary is larger and higher quality.

Under a system in which one's residence guarantees access to public schools, school policies act as neighborhood policies, and have the potential to affect housing construction and the evolution of neighborhoods.

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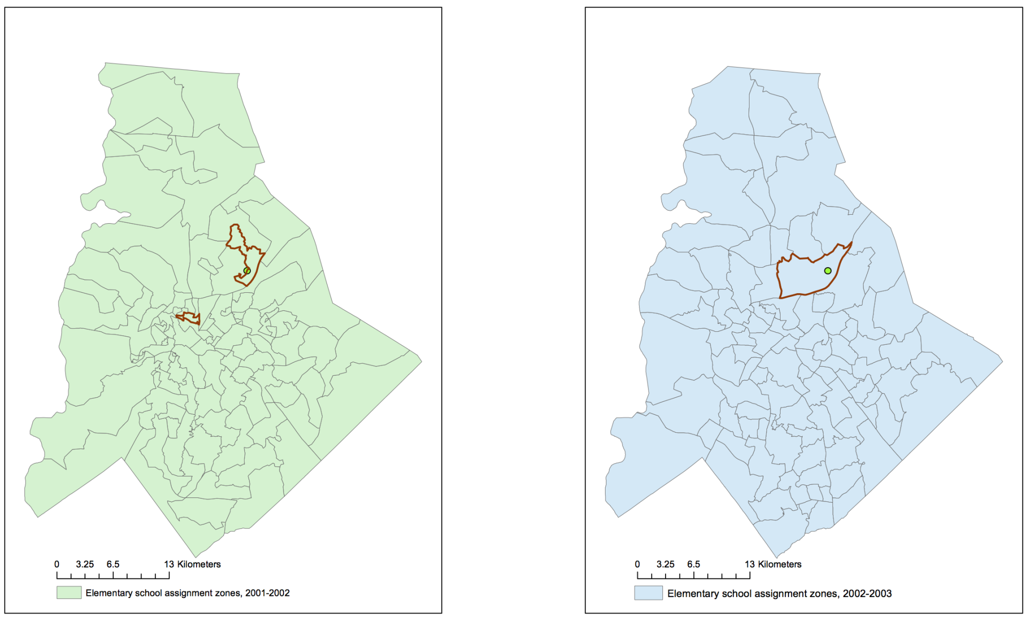
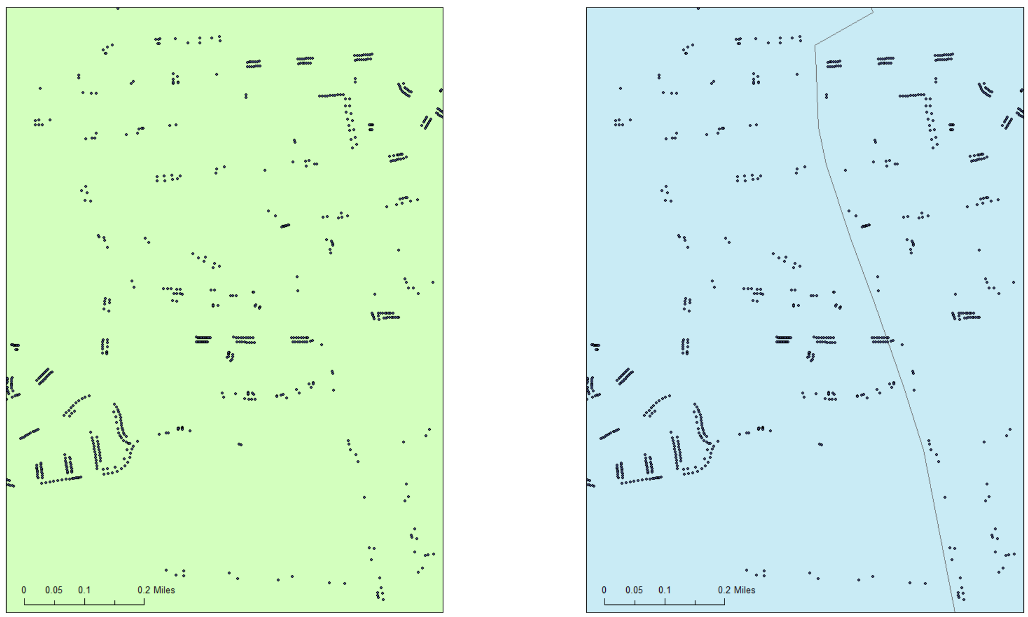
Appendix I: Tables and Figures

Figure 1: Charlotte-Mecklenburg student assignment maps, before and after redistricting



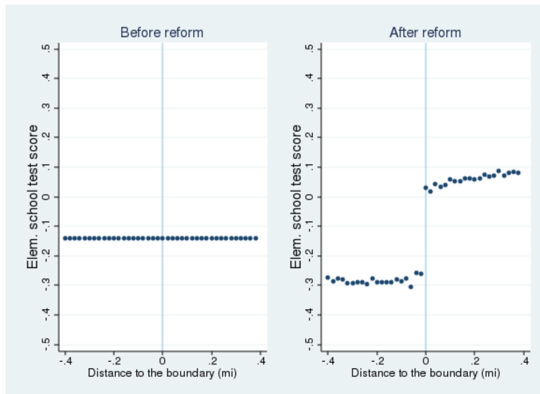
The left panel shows the map of school assignment zones for the 2001-2002 school year, while the right panel presents the map for the 2002-2003 school year, following the redistricting reform. The figures highlight one example school, Nathaniel Alexander elementary, whose boundaries were redrawn. The school's physical location is indicated by a dot and remains in the same location in both periods.

Figure 2: Illustrating the identification strategy: new boundaries

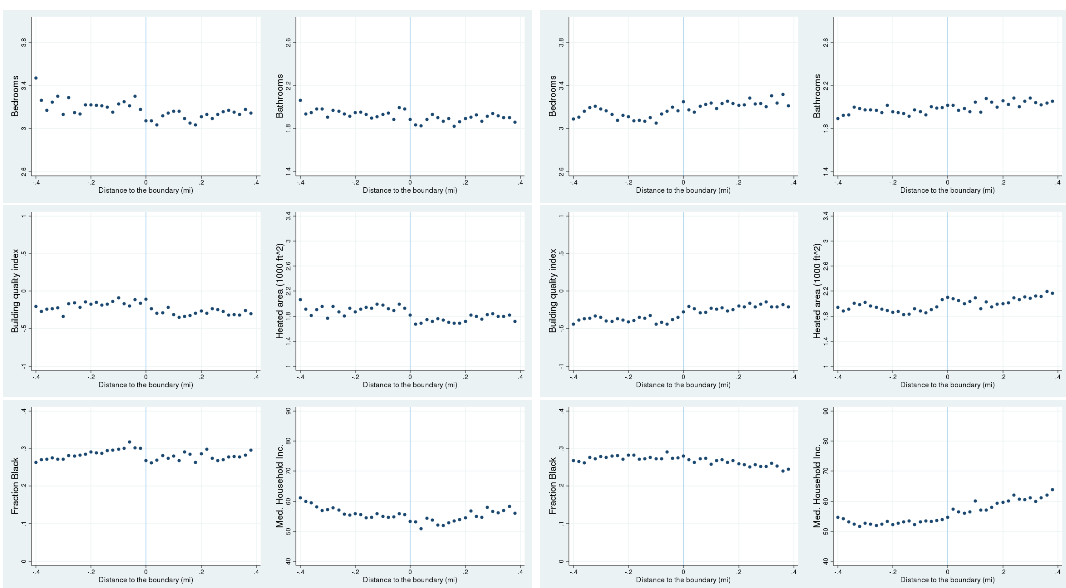


The left panel above depicts home sales in a neighborhood that will become separated by a new boundary during the reform; the same neighborhood is depicted in the right panel in the post period. The identification strategy applies an RD along new boundaries: in the pre-period this new boundary represents a "phantom boundary," which is used in the identification to test for pre-existing differences across boundary lines. In the post-period, houses are divided by a new boundary, which creates a local discontinuity in school quality; an RD in the post-period picks up the added effect of school quality discontinuities on housing construction.

Figure 3: New boundaries: house characteristics before and after the reform

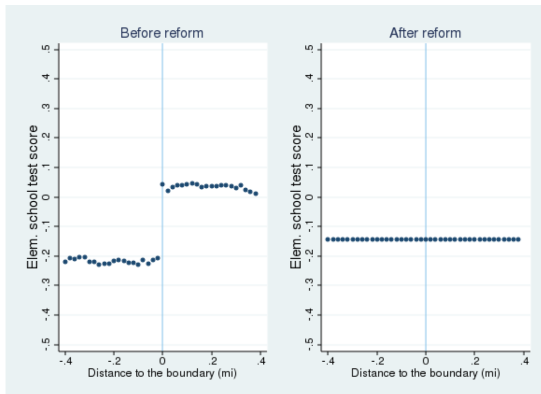


Pre-reform (phantom boundaries) Post-reform (boundaries in effect)

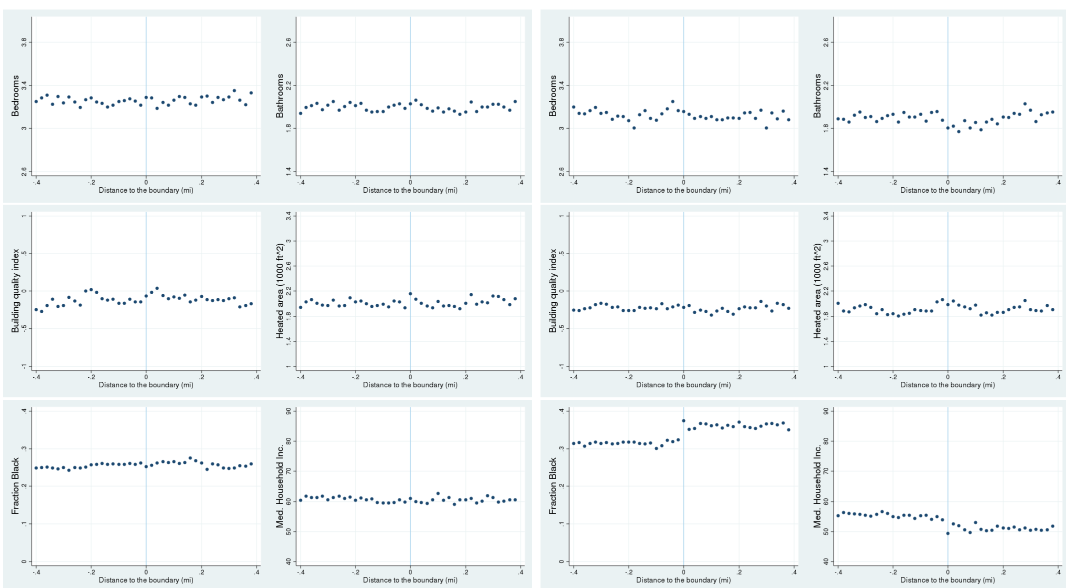


Each panel in this figure is constructed as follows: (i) regress the variable of interest on new boundary dummies and on 0.02 mi band distance-to-the-boundary dummy variables; (ii) plot the coefficients on these distance dummies. A given point in each figure represents the conditional average at a given distance to the boundary, where negative distances indicate the low test score side. The range of the y-axis is 2 standard deviations of the variable of interest, except the school test score (top) panel, which has range 1 standard deviation.

Figure 4: Destroyed boundaries: house characteristics before and after the reform



Pre-reform (boundaries in effect) Post-reform (phantom boundaries)



Each panel in this figure is constructed as follows: (i) regress the variable of interest on destroyed boundary dummies and on 0.02 mi band distance-to-the-boundary dummy variables; (ii) plot the coefficients on these distance dummies. A given point in each figure represents the conditional average at a given distance to the boundary, where negative distances indicate the "low" test score side. The range of the y-axis is 2 standard deviations of the variable of interest, except the school test score (top) panel, which has range 1 standard deviation.

Table 2: School quality's effect on characteristics of new housing

	Before reform (phantom boundaries)				After reform (boundaries in effect)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Bedrooms								
High side dummy	0.049	0.041	0.045	0.052	0.063*	0.048	0.044	0.052
	(0.056)	(0.054)	(0.055)	(0.059)	(0.036)	(0.036)	(0.036)	(0.037)
(B) Bathrooms								
High side dummy	0.048	0.023	0.022	0.002	0.070***	0.053**	0.059***	0.055***
	(0.034)	(0.028)	(0.030)	(0.029)	(0.024)	(0.021)	(0.021)	(0.018)
(C) Square feet (1000s)								
High side dummy	0.113	0.073	0.064	0.033	0.181***	0.162***	0.168***	0.184***
	(0.077)	(0.058)	(0.059)	(0.064)	(0.044)	(0.043)	(0.044)	(0.045)
(D) Building Quality								
High side dummy	0.035*	0.020	0.023	0.026*	0.107**	0.091**	0.093**	0.086**
	(0.020)	(0.015)	(0.018)	(0.013)	(0.043)	(0.041)	(0.042)	(0.043)
(E) Brick Face								
High side dummy	-0.001	-0.018	-0.021*	-0.007	0.027*	0.025*	0.026**	0.021*
	(0.009)	(0.012)	(0.012)	(0.007)	(0.014)	(0.013)	(0.013)	(0.011)
Boundary dummies	Yes	Yes	Yes		Yes	Yes	Yes	
Neighborhood controls		Yes	Yes			Yes	Yes	
Baseline neigh. * time			Yes	Yes			Yes	Yes
Boundary-by-census dum.				Yes				Yes
Observations	3,860	3,860	3,860	3,860	10,084	10,084	10,084	10,084

The sample includes all sales of new housing construction within .2 mi of a new boundary. The "before" sample, columns 1-4, are houses that were built and sold between 1996 and 2001 (inclusive), before the new boundaries were announced. The "after" sample, columns 5-8, are houses that were built and sold between 2002 and 2007 (inclusive), after the new boundaries were in effect. Each panel presents the results of estimating Equation 1 with a different dependent variable. Each housing characteristic is regressed on a dummy for whether the house is on the high test score school side of a new boundary, boundary dummies, sales quarter dummies, sales year dummies, and distance to the assigned school. Neighborhood controls are from the 2000 census and include: fraction black, Hispanic, Asian, fraction of population over 25 with college-degree, and median household income. Standard errors are clustered at the school level and are reported in parentheses. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table 1: Descriptive statistics of the regression sample: new housing construction

	Before reform			After reform		
	All sales	Destroyed Bnd.	New Bnd. (phantom)	All sales	Destroyed Bnd. (phantom)	New Bnd.
House characteristics						
Sale price (2000 USD)	219.56	171.72	141.17	228.42	196.58	176.19
	(455.23)	(115.13)	(115.60)	(378.35)	(195.10)	(183.61)
Full baths	2.25	2.15	2.08	2.25	2.26	2.11
	(0.61)	(0.49)	(0.39)	(0.63)	(0.62)	(0.50)
Half baths	0.71	0.68	0.52	0.73	0.71	0.72
	(0.49)	(0.49)	(0.51)	(0.48)	(0.51)	(0.48)
Num. bedrooms	3.46	3.40	3.23	3.37	3.33	3.19
	(0.71)	(0.63)	(0.52)	(0.77)	(0.87)	(0.71)
Year built, 1990s	0.45	0.57	0.51	0.16	0.00	0.00
	(0.50)	(0.50)	(0.50)	(0.37)	(0.00)	(0.00)
Year built, 2000s	0.55	0.43	0.49	0.84	1.00	1.00
	(0.50)	(0.50)	(0.50)	(0.37)	(0.00)	(0.00)
Heated area (1000*ft)	2.33	2.12	1.75	2.38	2.33	2.16
	(1.07)	(0.86)	(0.77)	(1.04)	(1.01)	(0.84)
Brick face	0.14	0.08	0.05	0.10	0.07	0.03
	(0.35)	(0.28)	(0.21)	(0.30)	(0.25)	(0.18)
A/C unit	1.00	1.00	1.00	1.00	1.00	1.00
	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)
Neighborhood characteristics						
Black households	0.16	0.22	0.24	0.19	0.25	0.25
	(0.16)	(0.21)	(0.19)	(0.19)	(0.25)	(0.19)
Asian households	0.03	0.04	0.03	0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Other race households	0.03	0.04	0.05	0.04	0.04	0.04
	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)
Fraction college	0.43	0.44	0.34	0.40	0.39	0.32
	(0.17)	(0.18)	(0.18)	(0.18)	(0.20)	(0.17)
Median hh income (2000 USD)	69.90	64.91	58.53	66.82	62.09	56.50
	(22.64)	(20.59)	(20.51)	(23.99)	(22.85)	(19.25)
Observations	57,943	7,125	3,860	96,761	12,581	10,084

Standard deviations in parentheses.

Table 3: School quality's effect on sales price of new housing

	Before reform (phantom boundaries)				After reform (boundaries in effect)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High side dummy	0.071	0.066	0.086	0.071	0.089	0.079*	0.087**	0.092**
	(0.062)	(0.061)	(0.061)	(0.054)	(0.055)	(0.041)	(0.042)	(0.039)
Boundary dummies	Yes	Yes	Yes		Yes	Yes	Yes	
House controls		Yes	Yes			Yes	Yes	
Neighborhood controls			Yes	Yes			Yes	Yes
Baseline neigh. * time				Yes				Yes
Boundary-by-census dum.	2,695	2,695	2,695	2,695	5,020	5,020	5,020	5,020
Observations	0.433	0.453	0.458	0.498	0.382	0.411	0.418	0.448

Each column presents estimates of Equation 1 with sales price as the dependent variable. See the notes to Table 2 for additional details on the sample and included controls. Standard errors are clustered at the school level and are reported in parentheses. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table 4: School quality and the share of home sales that are new construction

	Before reform (phantom boundaries)				After reform (boundaries in effect)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var: New house sale								
High side dummy	-0.044	-0.051	-0.052	-0.087	-0.065*	-0.058**	-0.058**	-0.042
	(0.031)	(0.032)	(0.032)	(0.054)	(0.032)	(0.028)	(0.029)	(0.026)
Boundary dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood controls		Yes	Yes	Yes		Yes	Yes	Yes
Baseline neigh. * time			Yes	Yes			Yes	Yes
Boundary-by-census dum.				Yes				Yes
Observations	11,230	11,230	11,230	11,230	21,876	21,876	21,876	21,876
R^2	0.468	0.470	0.473	0.544	0.563	0.567	0.568	0.628

Each column presents estimates of Equation 3 where the dependent variable a dummy indicating new housing. The sample includes all housing sales within .2 mi of a new boundary. Controls included but not reported include sales quarter dummies, sales year dummies, distance to the assigned school. Standard errors are clustered at the school level and are reported in parentheses. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table 5: Destroyed boundaries and new housing construction

	Before reform (boundaries in effect)				After reform (phantom boundaries)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Bedrooms								
High side dummy	-0.007	-0.016	-0.014	0.002	-0.026	-0.002	-0.003	-0.066
	(0.103)	(0.097)	(0.100)	(0.112)	(0.043)	(0.043)	(0.041)	(0.056)
(B) Bathrooms								
High side dummy	0.067	0.077	0.087	0.128	0.011	0.026	0.027	-0.065*
	(0.060)	(0.060)	(0.066)	(0.102)	(0.025)	(0.030)	(0.029)	(0.036)
(C) Square feet (1000s)								
High side dummy	0.102	0.112	0.114	0.168	-0.031	0.006	0.005	-0.202**
	(0.141)	(0.137)	(0.140)	(0.196)	(0.067)	(0.058)	(0.058)	(0.082)
(D) Building Quality								
High side dummy	0.139	0.116	0.111	0.069	0.014	0.021	0.019	-0.183**
	(0.188)	(0.161)	(0.164)	(0.191)	(0.055)	(0.066)	(0.066)	(0.091)
(E) Brick Face								
High side dummy	0.036	0.045	0.045	0.064	0.021	0.023	0.023	-0.040**
	(0.040)	(0.042)	(0.043)	(0.061)	(0.015)	(0.016)	(0.016)	(0.017)
Boundary dummies	Yes	Yes	Yes		Yes	Yes	Yes	
Neighborhood controls		Yes	Yes			Yes	Yes	
Baseline neigh. * time			Yes	Yes			Yes	Yes
Boundary-by-census dum.				Yes				Yes
Observations	7,125	7,125	7,125	7,125	12,581	12,581	12,581	12,581

The sample includes all sales of new housing construction within .2 mi of a destroyed boundary. The "before" sample, columns 1-4, are houses that were built and sold between 1996 and 2001 (inclusive), before the new boundaries were announced. The "after" sample, columns 5-8, are houses that were built and sold between 2002 and 2007 (inclusive), after the new boundaries were in effect. Each panel presents the results of estimating Equation 2 with a different dependent variable. Each housing characteristic is regressed on a dummy for whether the house is on the high quality school side of a destroyed boundary, boundary dummies, sales quarter dummies, sales year dummies, and distance to the assigned school. Standard errors are clustered at the school level and are reported in parentheses. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table 6: Destroyed boundaries and the sales price of new housing construction

	Before reform (boundaries in effect)				After reform (phantom boundaries)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High side dummy	0.094*	0.087*	0.079	0.090	0.032	0.057	0.056	-0.093**
	(0.053)	(0.050)	(0.052)	(0.070)	(0.042)	(0.038)	(0.038)	(0.044)
House controls								
Boundary dummies	Yes	Yes	Yes		Yes	Yes	Yes	
Neighborhood controls		Yes	Yes			Yes	Yes	
Baseline neigh. * time			Yes	Yes			Yes	Yes
Boundary-by-census dum.				Yes				Yes
Observations	4,885	4,885	4,885	4,885	6,707	6,707	6,707	6,707
R^2	0.428	0.431	0.433	0.466	0.471	0.479	0.482	0.552

Each column presents estimates of Equation 2 with sales price as the dependent variable. See the notes to Table 5 for additional details on the sample and included controls. Standard errors are clustered at the school level and are reported in parentheses. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Appendix II: Supplementary appendix (not for print publication)

Table A.1: Variable definitions

Variable	Description
House characteristics	
Sale price (2000 USD)	The sale price of the home converted to 2000USD using the BLS Consumer Price Index - All Urban Consumers (CUUR0000SA0) annual average.
Bathrooms	Number of bathrooms. The parcel data includes both full and half bathrooms. In the analysis I use full bathrooms.
Bedrooms	Number of bedrooms.
Year built	Year the residence was built.
Heated area	Number of square feet of heated area in the residence. In the analysis I express this variable in thousands of square feet.
A/C unit	Indicator for the presence of air-conditioning unit in the residence.
Brick face	An indicator for the building exterior wall is brick. Other common exterior wall materials include aluminum/vinyl, masonite, hardiplank, stucco, etc.
Building quality	Tax Assessor's rating of the building grade, which include the following ratings: below average (1.2% of all sales), average (70.1%), good (18.6%), very good (7.1%), excellent (2.27%), custom (0.78%). Each rating is assigned an integer 1-6, which is standardized to have mean 0, s.d. 1 in the sample.
Neighborhood characteristics	
Black households	Percent Black in the census block group.
Asian households	Percent Asian in the census block group.
Other race households	Percent Other race in the census block group.
Fraction college	Percent of the population 25 and over with a college degree in the census block group.
Median hh income (2000USD)	Median household income in the census block group, in 2000 USD.

Table A.2: Descriptive statistics of all housing sales

	Before reform			After reform		
	All sales	Destroyed Bnd.	New Bnd. (phantom)	All sales	Destroyed Bnd. (phantom)	New Bnd.
House characteristics						
Sale price (2000 USD)	197.87	160.20	147.47	221.27	196.58	176.19
	(343.38)	(156.07)	(198.16)	(380.73)	(195.10)	(183.61)
Full baths	2.09	1.94	1.93	2.10	2.26	2.11
	(0.68)	(0.69)	(0.61)	(0.70)	(0.62)	(0.50)
Half baths	0.61	0.54	0.46	0.62	0.71	0.72
	(0.52)	(0.52)	(0.52)	(0.52)	(0.51)	(0.48)
Num. bedrooms	3.35	3.21	3.14	3.30	3.33	3.19
	(0.72)	(0.74)	(0.65)	(0.76)	(0.87)	(0.71)
Year built, pre-1970	0.18	0.30	0.26	0.18	0.00	0.00
	(0.38)	(0.46)	(0.44)	(0.38)	(0.00)	(0.00)
Year built, 1970s	0.05	0.06	0.08	0.05	0.00	0.00
	(0.23)	(0.24)	(0.27)	(0.22)	(0.00)	(0.00)
Year built, 1980s	0.10	0.08	0.11	0.08	0.00	0.00
	(0.30)	(0.28)	(0.32)	(0.28)	(0.00)	(0.00)
Year built, 1990s	0.35	0.29	0.26	0.17	0.00	0.00
	(0.48)	(0.45)	(0.44)	(0.38)	(0.00)	(0.00)
Year built, 2000s	0.32	0.27	0.29	0.52	1.00	1.00
	(0.47)	(0.44)	(0.46)	(0.50)	(0.00)	(0.00)
Heated area (1000*ft)	2.16	1.94	1.78	2.20	2.33	2.16
	(1.04)	(0.94)	(0.84)	(1.20)	(1.01)	(0.84)
Brick face	0.24	0.27	0.23	0.21	0.07	0.03
	(0.43)	(0.44)	(0.42)	(0.41)	(0.25)	(0.18)
A/C unit	0.95	0.89	0.91	0.94	1.00	1.00
	(0.22)	(0.31)	(0.28)	(0.24)	(0.00)	(0.00)
Neighborhood characteristics						
Black households	0.20	0.30	0.28	0.22	0.25	0.25
	(0.22)	(0.29)	(0.24)	(0.23)	(0.25)	(0.19)
Asian households	0.03	0.03	0.03	0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Other race households	0.04	0.04	0.05	0.04	0.04	0.04
	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
Fraction college	0.41	0.37	0.34	0.39	0.39	0.32
	(0.19)	(0.21)	(0.19)	(0.19)	(0.20)	(0.17)
Median hh income (2000 USD)	65.68	57.47	55.29	63.12	62.09	56.50
	(25.25)	(24.07)	(22.07)	(25.69)	(22.85)	(19.25)
Observations	103,470	18,829	11,228	166,629	12,581	10,084

Detailed descriptions of the variables are given in Supplementary Table A.1. Standard deviations are in parentheses.

Table A.3: School quality's effect on characteristics of new housing (excluding zero-price sales)

	Before reform (boundaries in effect)				After reform (phantom boundaries)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Bedrooms								
High side dummy	0.039	0.026	0.030	0.007	0.035	0.012	0.012	0.028
	(0.054)	(0.044)	(0.047)	(0.050)	(0.050)	(0.052)	(0.052)	(0.053)
(B) Bathrooms								
High side dummy	0.068**	0.045**	0.047**	0.025	0.068**	0.049	0.055*	0.050**
	(0.033)	(0.020)	(0.022)	(0.015)	(0.031)	(0.031)	(0.031)	(0.025)
(C) Square feet (1000s)								
High side dummy	0.065	0.024	0.020	-0.032	0.192***	0.165***	0.168***	0.189***
	(0.082)	(0.051)	(0.053)	(0.058)	(0.054)	(0.054)	(0.054)	(0.055)
(D) Building Quality								
High side dummy	0.021	0.009	0.013	0.028**	0.117**	0.102**	0.100**	0.103*
	(0.020)	(0.019)	(0.019)	(0.014)	(0.048)	(0.050)	(0.050)	(0.052)
(E) Brick Face								
High side dummy	-0.005	-0.019	-0.020	0.001	0.029*	0.029*	0.030*	0.025*
	(0.009)	(0.012)	(0.012)	(0.003)	(0.016)	(0.017)	(0.016)	(0.014)
Boundary dummies	Yes	Yes	Yes		Yes	Yes	Yes	
Neighborhood controls		Yes	Yes			Yes	Yes	
Baseline neigh. * time			Yes	Yes			Yes	Yes
Boundary-by-census dum.				Yes				Yes
Observations	2,695	2,695	2,695	2,695	5,020	5,020	5,020	5,020

This table reproduces Table 2 but excludes zero-price sales. Standard errors are clustered at the school level and are reported in parentheses. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table A.4: Conditional price effects: new boundaries

	Before reform (phantom boundaries)			After reform (boundaries in effect)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High side dummy	0.054	0.063	0.084	0.081	0.042	0.043	0.051	0.051
	(0.058)	(0.063)	(0.061)	(0.055)	(0.060)	(0.047)	(0.050)	(0.045)
Boundary dummies	Yes	Yes	Yes		Yes	Yes	Yes	
House controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood controls		Yes	Yes			Yes	Yes	
Baseline neigh. * time			Yes	Yes			Yes	Yes
Boundary-by-census dum.				Yes				Yes
Observations	2,695	2,695	2,695	2,695	5,020	5,020	5,020	5,020
R^2	0.457	0.466	0.471	0.508	0.403	0.426	0.419	0.463

This table reproduces table 3 adding an additional vector of housing characteristics as controls, including number of bedrooms, number of bathrooms, standardized building quality, heated area sq. feet, and a dummy for having a brick exterior. Standard errors are clustered at the school level and are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: School quality's effect on characteristics of new housing (.15 mi bandwidth)

	Before reform (phantom boundaries)				After reform (boundaries in effect)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Bedrooms								
High side dummy	0.102	0.098	0.115	0.111	0.043	0.034	0.031	0.050
	(0.072)	(0.071)	(0.074)	(0.073)	(0.039)	(0.040)	(0.041)	(0.040)
(B) Bathrooms								
High side dummy	0.075	0.041	0.048	0.006	0.057**	0.045**	0.049**	0.046**
	(0.054)	(0.045)	(0.051)	(0.041)	(0.021)	(0.020)	(0.020)	(0.018)
(C) Square feet (1000s)								
High side dummy	0.192**	0.153**	0.158**	0.105**	0.182***	0.171***	0.175***	0.180***
	(0.087)	(0.061)	(0.067)	(0.050)	(0.046)	(0.045)	(0.046)	(0.046)
(D) Building Quality								
High side dummy	0.030	0.019	0.021	0.010	0.105**	0.098**	0.099**	0.091*
	(0.021)	(0.022)	(0.025)	(0.011)	(0.048)	(0.049)	(0.049)	(0.050)
(E) Brick Face								
High side dummy	-0.008	-0.013	-0.018	-0.010	0.027*	0.028*	0.029**	0.023*
	(0.012)	(0.011)	(0.012)	(0.010)	(0.014)	(0.014)	(0.014)	(0.013)
Boundary dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood controls		Yes	Yes	Yes		Yes	Yes	Yes
Baseline neigh. * time			Yes	Yes			Yes	Yes
Boundary-by-census dum.				Yes				Yes
Observations	2,587	2,587	2,587	2,587	7,510	7,510	7,510	7,510

This table reproduces Table 2 with a .15 mi bandwidth. Standard errors are clustered at the school level and are reported in parentheses. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table A.6: School quality's effect on characteristics of new housing (.10 mi bandwidth)

	Before reform (phantom boundaries)				After reform (boundaries in effect)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Bedrooms								
High side dummy	0.135*	0.072	0.122*	0.061	0.022	0.021	0.021	0.039
	(0.080)	(0.059)	(0.062)	(0.058)	(0.043)	(0.044)	(0.044)	(0.043)
(B) Bathrooms								
High side dummy	0.081	0.011	0.018	0.028	0.046**	0.036*	0.042**	0.044**
	(0.063)	(0.034)	(0.041)	(0.039)	(0.021)	(0.020)	(0.020)	(0.020)
(C) Square feet (1000s)								
High side dummy	0.236*	0.050	0.068	0.081	0.171***	0.166***	0.170***	0.181***
	(0.127)	(0.051)	(0.052)	(0.058)	(0.049)	(0.050)	(0.052)	(0.051)
(D) Building Quality								
High side dummy	0.086**	0.015	0.017	0.027*	0.120*	0.116*	0.119*	0.120*
	(0.039)	(0.020)	(0.021)	(0.014)	(0.065)	(0.066)	(0.067)	(0.068)
(E) Brick Face								
High side dummy	0.007	-0.006	-0.008	-0.005	0.033*	0.033*	0.035*	0.032*
	(0.016)	(0.015)	(0.018)	(0.007)	(0.018)	(0.019)	(0.018)	(0.018)
Boundary dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood controls		Yes	Yes	Yes		Yes	Yes	Yes
Baseline neigh. * time			Yes	Yes			Yes	Yes
Boundary-by-census dum.				Yes				Yes
Observations	1,452	1,452	1,452	1,452	5,143	5,143	5,143	5,143

This table reproduces Table 2 with a .1 mi bandwidth. Standard errors are clustered at the school level and are reported in parentheses. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table A.7: Robustness check: school quality's effect on characteristics of new housing

	Before reform (boundaries in effect)				After reform (phantom boundaries)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Bedrooms								
High side dummy	0.039	0.026	0.030	0.007	0.035	0.012	0.012	0.028
	(0.054)	(0.044)	(0.047)	(0.050)	(0.050)	(0.052)	(0.052)	(0.053)
(B) Bathrooms								
High side dummy	0.068**	0.045**	0.047**	0.025	0.068**	0.049	0.055*	0.050**
	(0.033)	(0.020)	(0.022)	(0.015)	(0.031)	(0.031)	(0.031)	(0.025)
(C) Square feet (1000s)								
High side dummy	0.065	0.024	0.020	-0.032	0.192***	0.165***	0.168***	0.189***
	(0.082)	(0.051)	(0.053)	(0.058)	(0.054)	(0.054)	(0.054)	(0.055)
(D) Building Quality								
High side dummy	0.021	0.009	0.013	0.028**	0.117**	0.102**	0.100**	0.103*
	(0.020)	(0.019)	(0.019)	(0.014)	(0.048)	(0.050)	(0.050)	(0.052)
(E) Brick Face								
High side dummy	-0.005	-0.019	-0.020	0.001	0.029*	0.029*	0.030*	0.025*
	(0.009)	(0.012)	(0.012)	(0.003)	(0.016)	(0.017)	(0.016)	(0.014)
Boundary dummies	Yes	Yes	Yes		Yes	Yes	Yes	
Neighborhood controls		Yes	Yes			Yes	Yes	
Baseline neigh. * time			Yes	Yes			Yes	Yes
Boundary-by-census dum.				Yes				Yes
Observations	2,695	2,695	2,695	2,695	5,020	5,020	5,020	5,020

This table reproduces columns 5-8 of Table 2 with an additional control included in all columns: the pre-period boundary-side specific mean of the dependent variable interacted with a linear time trend.

Table A.8: Robustness check: do trends in housing construction predict the new boundary side?

	(1)	(2)	(3)	(4)
Bedrooms * t	-0.060	-0.014	-0.025	-0.003
	(0.207)	(0.196)	(0.187)	(0.040)
Bathrooms * t	0.026	-0.005	0.013	0.032
	(0.207)	(0.199)	(0.185)	(0.062)
Building Quality * t	-0.032	-0.021	0.028	0.007
	(0.079)	(0.081)	(0.065)	(0.017)
Heated Area * t	0.072	0.055	0.077	0.005
	(0.112)	(0.113)	(0.099)	(0.039)
Boundary dummies	Yes	Yes	Yes	Yes
Neighborhood controls		Yes	Yes	Yes
Baseline neigh. * time			Yes	Yes
Boundary-by-census dum.				Yes
Observations	3,860	3,860	3,860	4,148
R^2	0.611	0.631	0.658	0.756

This table reports the vector $\hat{\delta}_1$ from the estimation of Equation 5.

Table A.9: Conditional price effects: destroyed boundaries

	Before reform (boundaries in effect)			After reform (phantom boundaries)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High side dummy	0.058	0.047	0.038	0.039	0.039	0.059*	0.059*	-0.043
	(0.048)	(0.044)	(0.047)	(0.051)	(0.035)	(0.030)	(0.030)	(0.032)
House controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Boundary dummies	Yes	Yes	Yes		Yes	Yes	Yes	
Neighborhood controls		Yes	Yes			Yes	Yes	
Baseline neigh. * time			Yes	Yes			Yes	Yes
Boundary-by-census dum.				Yes				Yes
Observations	4,885	4,885	4,885	4,885	6,707	6,707	6,707	6,707
R^2	0.463	0.466	0.468	0.499	0.504	0.513	0.515	0.564

This table reproduces table 6 adding an additional vector of housing characteristics as controls, including number of bedrooms, number of bathrooms, standardized building quality, heated area sq. feet, and a dummy for having a brick exterior. Standard errors are clustered at the school level and are reported in parentheses. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.