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

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This paper studies the effect of school quality on residential and commercial building construction and development. 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.

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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 households sorting into neighborhoods based 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. I consider both residential and commercial development, as measured by building construction permits. I also consider both the extensive and intensive margins of development, distinguishing between new construction and the development of existing properties.

To move beyond correlations, this study takes advantage of a major school reform in Charlotte-Mecklenburg that redrew residence-based assignment zones. Following a lengthy court battle, Charlotte-Mecklenburg was ordered in the

[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).

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. To diminish the amount of re-segregation impact of the reform, the district introduced an assignment plan that granted parents a greater amount of school choice.[2]

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

[2]Previous research has studied the Charlotte-Mecklenburg reform to examine the effect of student demographics on teacher sorting into schools (Jackson, 2009) and student participation in crime (Billings et al., 2013). Other work has considered the school choice component of the reform to study its effect on student outcomes (Hastings et al., 2005; Hastings and Weinstein, 2008).

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 neighborhood and 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 determined access to schools. Once these boundaries were eliminated, however, 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 gentrifying neighborhoods as part of 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., 2013).[3] This paper's contribution is to study the effect of school quality on the *dynamics of neighborhood development*, which is analyzed through a focus on the extensive and intensive margin of building construction and renovation. This paper emphasizes, and provides direct evidence, that school policies act as neighborhood policies through their effect on development. 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).[4] Black (1999) introduced a regression discontinuity design that exploits

[3]There is also a rich literature studying the reverse-causal question, asking about the importance of neighborhoods for educational development and social behavior (Aaronson, 1998; Bowen and Bowen, 1999; Crowder and South, 2011; Wodtke et al., 2011; Chetty et al., 2016).

[4]This strategy has been used in contexts outside the education setting: see, e.g. Chen

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.[5]

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.

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et al. (2013); Lavy (2006); Lalive (2007); Pence (2006).

[5]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 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, the results, and 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.[6] The district adopted 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.[7] In De-

[6]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).

[7]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

cember 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.[8]

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 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

the desegregation order for CMS.

[8]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., 2013; Weinstein, 2014). The school choice component of the reform has been studied extensively as well (Hastings et al., 2005; Hastings and Weinstein, 2008).

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. (2013) 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

There are two main data sets used for this analysis.

The first is the database of all commercial and residential building permits issued in Mecklenburg county for 1994-2007. This data is in the public record and includes information on the issue date, completion date (if completed), site address, total construction cost, and other characteristics of the building site and construction, e.g. whether it is a new construction or an alteration, whether it's a single family or multifamily residence. Each permit includes data on heated and unheated area, the number of baths, number of bathrooms and bedrooms.[9]

[9] Mecklenburg county building permits can be viewed and downloaded from the county's Integrated Data Store: http://dwexternal.co.mecklenburg.nc.us/ids/RptGrid01.aspx?rpt=Daily_Building_Permits_Issued

The second database includes 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 also 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.[10] 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

[10]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.

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 of 2002 to allow some time for information about changes in school quality to transmit to home buyers and sellers.[11] 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.[12]

I match each residential address with neighborhood characteristics 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

[11] The results are not sensitive to this choice, and the results based on alternative choices of this window can be found in Appendix TBW.

[12] 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.

are many more in the district to provide useful variation in school quality; second, mounting empirical evidence suggests that early childhood education generates 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.[13]

3.1 Defining new and destroyed boundaries

I now define the boundary samples 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.[14]

To generate the new boundaries, I take the post-reform boundaries and I eliminate any sections that overlap with the pre-reform boundaries.[15] 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 approach ensures that in the regression the identification is based on houses that share a

[13]For robustness, I present the main results using other measures of school quality. TBW

[14]We might consider a third category of boundary files: those that are *unchanged* as a result of the reform. While this might seem a useful comparison group, a difficulty with interpreting results across unchanged boundaries is even though the school assignments themselves are unchanged, school quality and the composition of students will change as a result of the school's other boundary lines changing. Because this limits its usefulness as a comparison group, I do not report unchanged boundaries in the main analysis.

[15]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.

pre-reform school assignment. In total there are 106 new boundary dummies.

To generate the destroyed boundaries, I take the pre-reform boundaries and eliminate the sections that overlap with post-reform boundaries; the boundaries that remain are those that 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 ensures that the identification is based on 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. (TBW)

3.2 Summary statistics

Permits summary stats. Table 1 and Table 2.

Table TBW presents summary statistics for new construction of single family homes in Mecklenburg county during the sample period, 1998-2006.[16] The samples are divided into pre- and post-periods based on their sales date, and

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

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 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.[17]

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.[18] 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

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

[18]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.

boundaries are less affluent and have a higher fraction of black residents, compared to those houses not along boundaries.

4 Empirical strategy and results

This section presents the empirical strategy used to estimate the extent school quality affects the extensive and intensive margins of neighborhood development. The econometric challenge is that school quality is not randomly assigned to neighborhoods: causality may run in both directions; moreover, the correlation between school quality and neighborhood development may be driven by other factors such as the provision of public goods that affects the sorting of households into neighborhoods and improves school quality.

The empirical design presented here exploits newly drawn school assignment boundaries that separate houses that previously were assigned to the same school. Such boundaries create local discontinuities in school quality between houses that previously did not exist. We can then measure the evolution of neighborhoods across the discontinuities, before and after their appearance. Figure 2 depicts one such new boundary used in the identification strategy; it divides a set of houses that previously had the same school assignment, creating a school quality discontinuity where there previously was none.

4.1 Graphical illustration

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.[19]

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[19] 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 neighbor-

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.

4.2 Extensive margin

We first address the question of whether school quality affects the *extensive margin* of neighborhood development: whether it affects the number of new building permits per unit area, or the number of building renovation permits

hoods are being redistricted into better schools.

per unit area.[20] The unit of observation for this analsyis is a *boundary-side* (bs), each boundary has a high- and low-test score side, which remains constant throughout the sample period. I consider construction within a 0.2 mi buffer for each boundary-side. Denote $perm_{bst}^{(j)}$ as the number of building permits of type j per square kilometer for boundary-side bs in year t. Permits are indexed by j because we are interested in several permit types: residential and commercial, and, within those categories, new building permits and renovation permits.

I estimate the following differences-in-differences regression with boundary fixed effects:

$$perm_{bst}^{(j)} = \gamma_0 + \gamma_1 q_{bs}^{post} + \gamma_2 (q_{bs}^{post} \cdot post_{bs}) + n_{bs}^{'} \gamma_n + \gamma_{0t} f(t) + \theta_b^{new} + \epsilon_{bst} \quad (1)$$

where n_{bs} are the (mean) neighborhood attributes for boundary-side bs, q_{bs}^{post} is the average test score in the post-reform period, $f(t)$ is a polynomial in time, and θ_b is a vector of new boundary dummies. The coefficient γ_2 represents the effect of school quality on the number of building permits per square kilometer in the post-reform period (when the boundaries are in effect) minus the pre-period (when the boundaries are not in effect), for the high test score side of the new boundary relative to the low test score side of the new boundary. The coefficient γ_1 represents the pre-reform difference across the boundary, before the school quality discontinuity goes into effect. Hence

[20]The building permits data includes demolitions as well, and empirically the number of demolitions in the data is tiny; hence building permits for new construction can be interpreted as a net change in the housing stock. (TBW)

γ_1 provides an estimate of baseline differences in neighborhood development across the boundary before it takes effect. The estimate of γ_1 will provide insight into whether school boundaries are drawn to separate neighborhoods that differ in pre-existing measures of development.

Equation 1 is estimated on the sample of boundaries for which there is at least one construction project on either side of boundary b in the 13 year sample period, and for which the length of the boundary line is at least 200 meters. Neighborhood characteristics n_{bs} are constructed by averaging the block-group characteristics of permit addresses along the boundary-side.[21]

Table TBW 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.

Destroyed boundaries

We can perform a similar analysis on the sample of boundaries that were destroyed during the reform:

$$perm_{bst}^{(j)} = \delta_0 + \delta_1 q_{bs}^{pre} + \delta_2 (q_{bs}^{pre} \cdot post_{bs}) + n_{bs}\delta_n + \delta_{0t} f(t) + \theta_b^{dest} + {}_{bst} \qquad (2)$$

The key distinction with Equation 1 is that we estimate it on the sample

[21] In the event that a boundary side bs has zero building permits throughout the sample (and therefore no neighborhood information), I impute the neighborhood characteristics based on those of the opposite side of the boundary, so that effectively $n_{bs} = n_b$ for these boundaries. This imputation does not affect the results in a meaningful way and the unimputed regressions are reported in Appendix XXX.

of boundaries that were destroyed. Note that the school quality measures are based on *pre*-reform measures of school quality, when those boundaries were in effect; after the reform, the boundaries disappear and houses on either side of destroyed boundaries have the same school assignment. The coefficient δ_1 represents the pre-reform difference across the boundary, while the school quality gap is in effect; δ_1 represents the long-run equilibrium difference in neighborhood construction patterns across school quality discontinuities. The estimate of δ_2 reflects the difference in neighborhood construction on the high v. low test score side, after the gap in school quality disappears relative to before.

Identification strategy: discussion

The identification strategy has an advantage over prior studies that use a boundary fixed effects 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 boundary line, which is the key assumption necessary for identification.

Despite this advantage, there are three aspects of the identification meriting discussion. The first is a consideration of *why* particular boundaries were redrawn while others were not.[22] For instance, suppose the district only redrew boundaries in local areas where residents have weaker preferences for public school quality. Note that this concern will not bias estimates from Equation 1;

[22]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).

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 other studies implementing the boundary 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, as the neighborhood on the high test score side improves, the desirability of living on the low test score side and making housing investments may increase. In this case we might fail to detect or 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 key challenge to identification, is whether boundaries are drawn to separate areas with differential trends in home construction. For example, district officials may want to encourage areas they see as already developing by including them in the assignment zone of the better school. This assumption is considered in detail in Section 5.

4.3 Intensive margin

The intensive margin analysis follows largely the same approach as the extensive margin analysis presented above. The key distinction is that now the unit of analysis is now the *permit*, indexed by h, which has a vector of characteristics $y_h^{(j)} \in \{y_1, \cdots, y_J\}$.

The regression implemented is:

$$y_{hst}^{(j)} = \gamma_0 + \gamma_1 q_{hs}^{post} + \gamma_2 (q_{hs}^{post} \cdot post_{hs}) + n_{hs}\gamma_n + \gamma_{0t} f(t) + \theta_{hb}^{new} + {}_{hst} \tag{3}$$

We are interested in whether permits differ in their attributes; for instance, whether they differ in the cost of the project, the size and features of the structure, or the building quality.

5 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 $x_{bs,0}^{(k)}$ which are the pre-period averages. I then interact these characteristics with a linear time trend and estimate the following for each characteristic x, in the post-period:

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

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 TBW.

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.[23] 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.

[23] 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 TBW 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 TBW 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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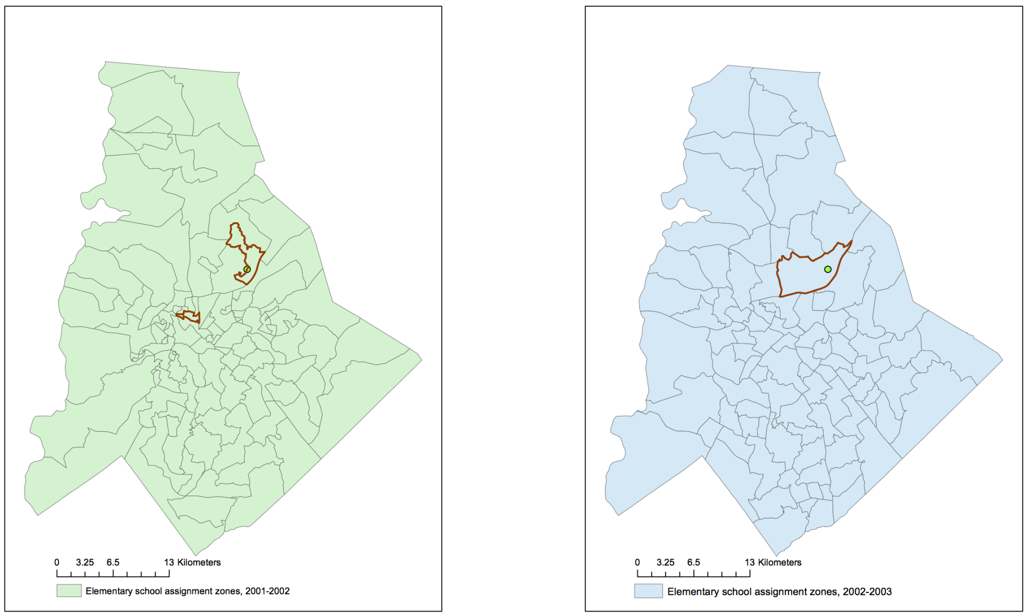
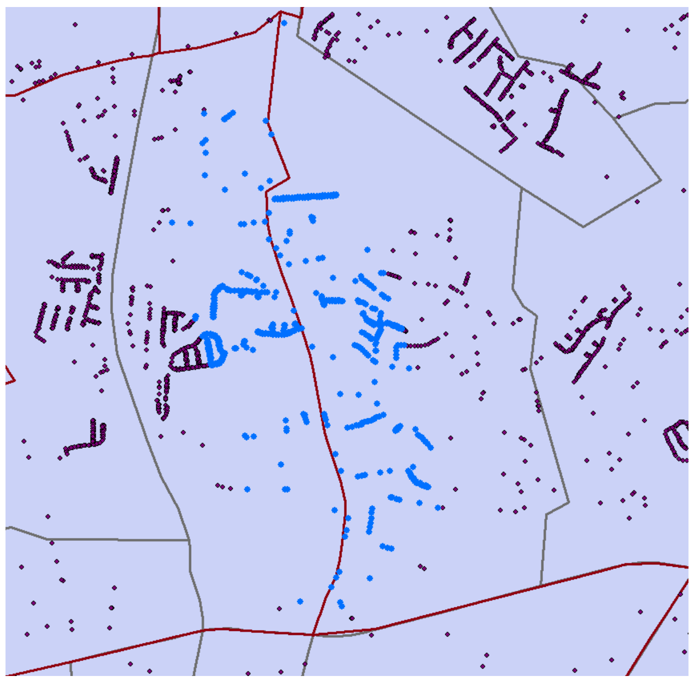
Figure 1: Charlotte-Mecklenburg student assignment maps, before and after redistricting



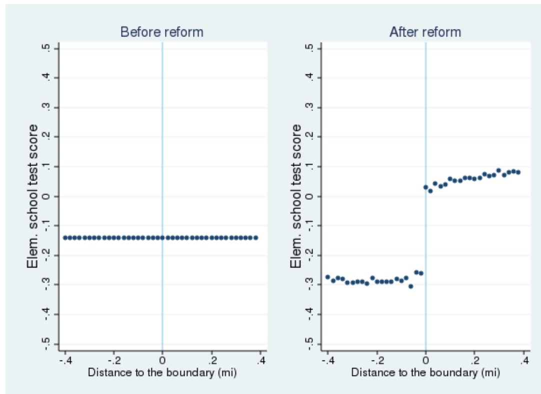
The left panel shows the map of school assignment zones for the 2001-2002 school year, while the right panel presents the map for the 2002-2003 school year, following the redistricting reform. The figures highlight one example school, Nathaniel Alexander elementary, whose boundaries were redrawn. The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

Figure 2: Illustrating the identification strategy: new boundaries

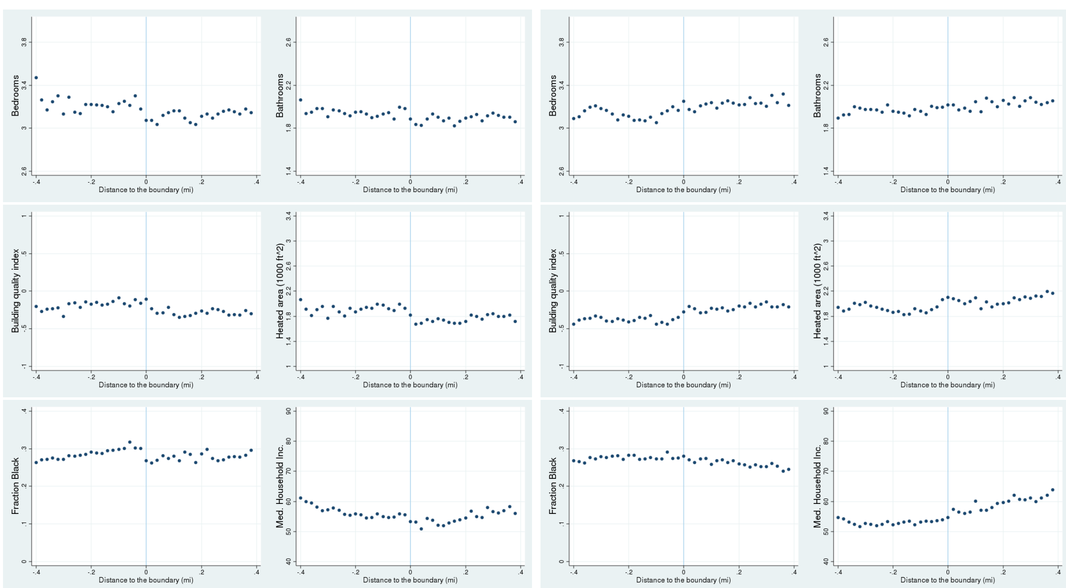


The figure above illustrates the identification strategy. The red boundary line down the middle represents a new boundary bisecting an old school assignment zone. The blue dots represent the sample of building permits within a .2 mi bandwidth of the new boundary. Prior to the reform, these permits share an old school assignment, but after the reform have different school assignments. In the pre-period this new boundary represents a "phantom boundary," which is used to test for pre-existing differences; in the post-period, the new boundary represents a local discontinuity in school quality.

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.

Table 1: Summary Statistics: All Residential Permits

	1996-98	1999-2000	2001-02	2003-04	2005-07
Permit characteristics					
Total const. cost (1000s)	109.18	110.10	103.24	104.57	99.22
	(89.56)	(92.18)	(88.10)	(89.44)	(90.81)
Heated square feet (1000s)	1.73	1.62	1.55	1.58	1.42
	(1.19)	(1.19)	(1.16)	(1.22)	(1.23)
Unheated square feet (1000s)	0.38	0.38	0.35	0.34	0.30
	(0.35)	(3.07)	(0.37)	(0.39)	(0.41)
Bathrooms	1.94	1.93	1.87	1.83	1.69
	(1.37)	(1.36)	(1.35)	(1.33)	(1.45)
New single-family home	0.73	0.72	0.70	0.67	0.59
	(0.45)	(0.45)	(0.46)	(0.47)	(0.49)
New multi-family home	0.00	0.00	0.01	0.00	0.01
	(0.04)	(0.05)	(0.08)	(0.06)	(0.07)
Residence alteration	0.27	0.28	0.29	0.33	0.41
	(0.44)	(0.45)	(0.46)	(0.47)	(0.49)
Project completed	0.84	0.85	0.85	0.82	0.81
	(0.36)	(0.36)	(0.36)	(0.38)	(0.39)
Days to complete	189.50	171.84	166.07	212.72	206.66
	(128.16)	(143.96)	(179.28)	(257.39)	(221.00)
Neighborhood characteristics					
Black households	0.18	0.18	0.18	0.17	0.22
	(0.21)	(0.20)	(0.20)	(0.20)	(0.23)
Asian households	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	0.03	0.04	0.03	0.04
	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)
Fraction college	0.48	0.45	0.42	0.43	0.43
	(0.19)	(0.19)	(0.19)	(0.19)	(0.21)
Median hh income (1000s)	73.59	70.40	67.45	68.42	65.03
	(28.10)	(27.02)	(25.20)	(26.20)	(28.81)
Observations	23,523	18,517	18,538	19,997	10,090

The table above reports the sample mean and standard deviations of the residential permits data. Dollar amounts are expressed in 2000 USD.

Table 2: Summary Statistics: All Commercial Permits

	1996-98	1999-2000	2001-02	2003-04	2005-07
Total const. cost (1000s)	338.05	288.59	225.29	290.72	269.01
	(1,197.28)	(1,021.98)	(780.63)	(1,928.23)	(1,390.14)
Heated square feet (1000s)	5.65	5.66	6.07	5.82	6.37
	(26.68)	(30.26)	(78.94)	(32.76)	(26.22)
Unheated square feet (1000s)	1.24	1.20	0.99	1.43	1.66
	(18.95)	(14.93)	(14.92)	(29.86)	(21.65)
New non-residence	0.24	0.17	0.17	0.23	0.17
	(0.43)	(0.38)	(0.37)	(0.42)	(0.37)
Non-residence alteration	0.42	0.46	0.41	0.50	0.54
	(0.49)	(0.50)	(0.49)	(0.50)	(0.50)
New residence	0.34	0.37	0.43	0.27	0.29
	(0.47)	(0.48)	(0.49)	(0.44)	(0.46)
Project completed	0.83	0.80	0.82	0.84	0.83
	(0.38)	(0.40)	(0.39)	(0.37)	(0.38)
Days to complete	239.85	224.31	229.08	224.76	284.15
	(322.94)	(287.44)	(267.39)	(263.21)	(287.80)
Neighborhood characteristics					
Black households	0.26	0.27	0.27	0.25	0.29
	(0.24)	(0.27)	(0.26)	(0.25)	(0.26)
Asian households	0.03	0.03	0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Other race households	0.05	0.04	0.04	0.04	0.04
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Fraction college	0.40	0.39	0.39	0.40	0.40
	(0.19)	(0.19)	(0.19)	(0.20)	(0.20)
Median hh income (1000s)	52.42	52.44	52.69	53.77	48.40
	(22.91)	(22.43)	(22.42)	(23.40)	(21.84)
Observations	13,312	10,827	11,001	7,291	5,314

The table above reports the sample mean and standard deviations of the commercial permits data. Dollar amounts are expressed in 2000 USD.

Table 3: Residential Permits: Summary Statistics of the Boundary Sample

	Before reform			After reform		
	All permits	Dest. Bnd.	New Bnd. (phantom)	All permits	Dest. Bnd. (phantom)	New Bnd.
Total const. cost (1000s)	139.37	130.21	112.55	138.49	132.98	116.65
	(80.06)	(77.01)	(79.24)	(79.15)	(73.67)	(68.30)
Heated square feet (1000s)	2.15	2.04	1.82	2.14	2.06	1.87
	(0.92)	(0.85)	(0.87)	(0.94)	(0.88)	(0.80)
Unheated square feet (1000s)	0.45	0.48	0.48	0.40	0.35	0.30
	(2.26)	(5.14)	(6.81)	(0.39)	(0.33)	(0.26)
Bathrooms	2.56	2.51	2.41	2.60	2.59	2.50
	(0.99)	(0.98)	(1.05)	(0.77)	(0.77)	(0.57)
New single-family home	1.00	1.00	1.00	0.99	0.98	0.99
	(0.06)	(0.06)	(0.06)	(0.08)	(0.12)	(0.08)
New multi-family home	0.00	0.00	0.00	0.01	0.02	0.01
	(0.06)	(0.06)	(0.06)	(0.08)	(0.12)	(0.08)
Residence alteration	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Project completed	1.00	1.00	1.00	1.00	1.00	1.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Days to complete	176.58	175.32	165.45	181.27	183.76	160.30
	(120.62)	(123.41)	(101.91)	(182.28)	(197.15)	(141.68)
Neighborhood characteristics						
Black households	0.17	0.21	0.23	0.20	0.25	0.27
	(0.18)	(0.22)	(0.22)	(0.19)	(0.24)	(0.21)
Asian households	0.03	0.04	0.03	0.03	0.04	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Other race households	0.03	0.04	0.04	0.04	0.04	0.05
	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)
Fraction college	0.45	0.46	0.38	0.39	0.40	0.32
	(0.17)	(0.18)	(0.20)	(0.17)	(0.19)	(0.19)
Median hh income (1000s)	71.60	67.64	63.01	64.33	62.09	57.16
	(23.71)	(22.44)	(23.90)	(21.35)	(21.95)	(20.61)
Observations	34,470	6,530	3,724	24,942	5,091	3,968

The table above reports the sample mean and standard deviations of the residential permits data, comparing all residential permits to the regression sample that lies along new and destroyed boundaries. Here permits are restricted to either new residence permits or residential alternations permits that were completed projects. Dollar amounts are expressed in 2000 USD.

Table 4: Commercial Permits: Summary Statistics of the Boundary Sample

	Before reform			After reform		
	All permits	Dest. Bnd.	New Bnd. (phantom)	All permits	Dest. Bnd. (phantom)	New Bnd.
Total const. cost (1000s)	605.40	531.15	492.60	767.26	756.71	632.10
	(1,716.78)	(1,148.25)	(1,373.78)	(3,327.79)	(2,356.85)	(2,013.99)
Heated square feet (1000s)	12.57	15.94	9.33	10.48	11.26	10.68
	(124.14)	(242.08)	(38.63)	(34.91)	(29.24)	(25.66)
Unheated square feet (1000s)	4.87	2.48	1.46	4.77	5.74	2.93
	(37.52)	(19.86)	(8.23)	(41.41)	(47.04)	(20.21)
New non-residence	1.00	1.00	1.00	1.00	1.00	1.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Non-residence alteration	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
New residence	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Project completed	1.00	1.00	1.00	1.00	1.00	1.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Days to complete	252.16	250.38	251.35	249.70	266.77	285.32
	(354.91)	(247.41)	(425.82)	(274.72)	(280.11)	(318.25)
Neighborhood characteristics						
Black households	0.27	0.34	0.33	0.26	0.31	0.34
	(0.25)	(0.30)	(0.27)	(0.26)	(0.33)	(0.29)
Asian households	0.03	0.03	0.03	0.03	0.03	0.03
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
Other race households	0.05	0.06	0.06	0.04	0.04	0.05
	(0.06)	(0.07)	(0.06)	(0.05)	(0.05)	(0.06)
Fraction college	0.37	0.33	0.33	0.39	0.38	0.34
	(0.20)	(0.22)	(0.20)	(0.19)	(0.23)	(0.22)
Median hh income (1000s)	53.22	50.88	47.73	54.77	51.96	50.37
	(23.58)	(23.89)	(19.63)	(22.66)	(22.91)	(22.03)
Observations	4,611	1,054	736	3,076	677	378

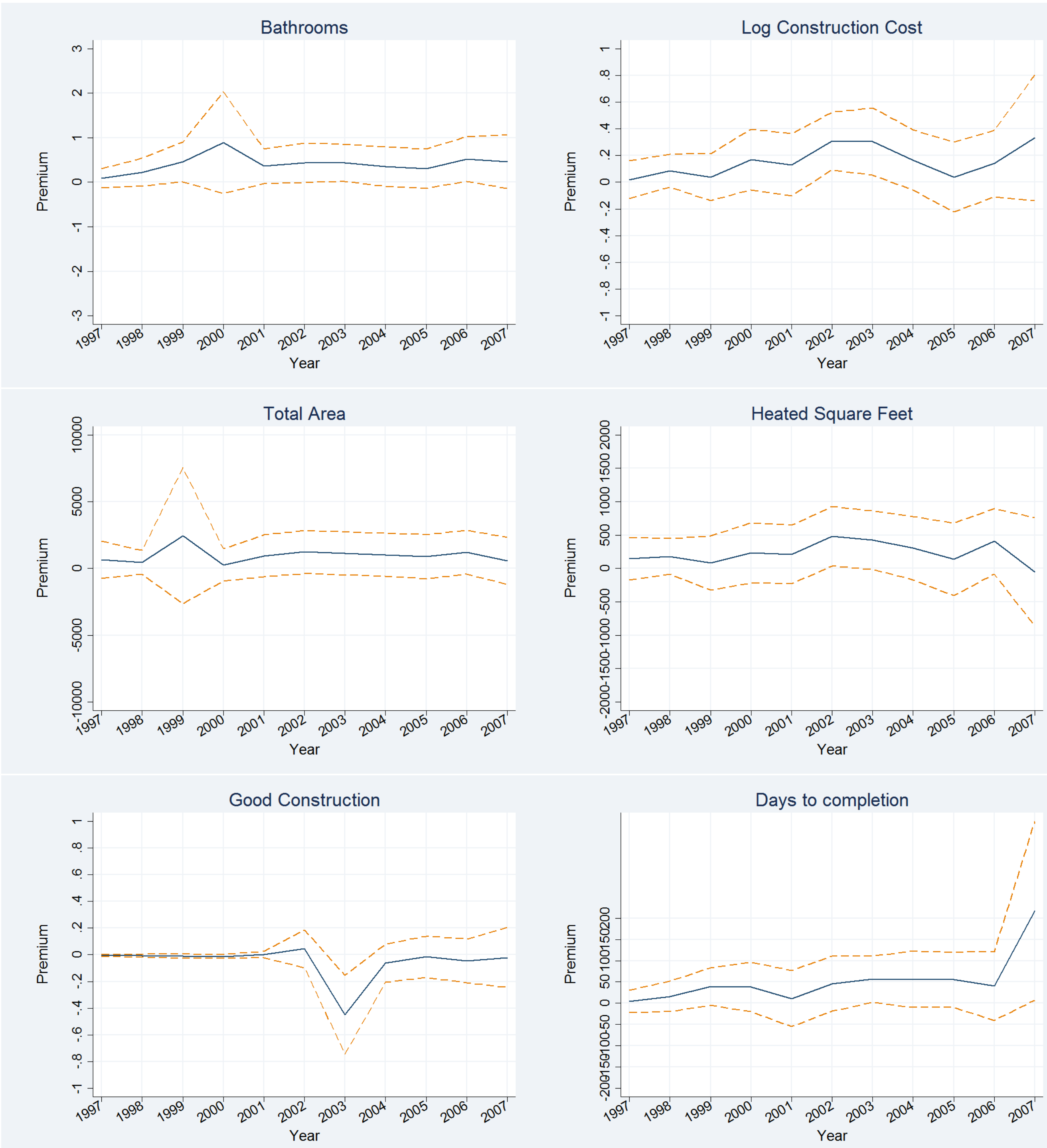
The table above reports the sample mean and standard deviations of the commercial permits data, comparing all commercial permits to the regression sample that lies along new and destroyed boundaries. Here permits are restricted to either new non-residence permits that were completed projects. Dollar amounts are expressed in 2000 USD.

Table 5: School quality's effect on residential construction: new boundaries

	(1)	(2)	(3)	(4)
(A) Log Total Cost				
Elem. test score	0.051	0.039	0.032	-0.050
	(0.045)	(0.043)	(0.042)	(0.058)
After reform * Elem. test	0.136	0.081	0.080	0.179*
	(0.122)	(0.084)	(0.090)	(0.090)
Observations	7,692	7,692	7,692	7,692
R^2	0.641	0.651	0.652	0.697
(B) Total Area				
Elem. test score	0.027	-0.026	-0.038	-0.021
	(0.113)	(0.140)	(0.135)	(0.168)
After reform * Elem. test	0.436	0.328	0.321	0.393
	(0.286)	(0.233)	(0.249)	(0.255)
Observations	7,683	7,683	7,683	7,683
R^2	0.074	0.075	0.075	0.078
(C) Heated Area				
Elem. test score	0.076	0.053	0.035	0.071
	(0.075)	(0.065)	(0.072)	(0.086)
After reform * Elem. test	0.273	0.178	0.174	0.197
	(0.227)	(0.164)	(0.180)	(0.172)
Observations	7,683	7,683	7,683	7,683
R^2	0.652	0.662	0.665	0.721
(D) Bath				
Elem. test score	0.184**	0.161**	0.146*	0.045
	(0.083)	(0.075)	(0.075)	(0.048)
After reform * Elem. test	0.013	-0.077	-0.082	0.029
	(0.170)	(0.134)	(0.133)	(0.118)
Observations	7,638	7,638	7,638	7,638
R^2	0.290	0.296	0.299	0.803
(E) Nonc. Const.				
Elem. test score	0.002	-0.001	-0.000	0.004
	(0.011)	(0.011)	(0.014)	(0.017)
After reform * Elem. test	-0.061**	-0.062***	-0.061***	-0.070***
	(0.024)	(0.022)	(0.020)	(0.020)
Observations	7,692	7,692	7,692	7,692
R^2	0.501	0.502	0.503	0.521
(F) Days Comp.				
Elem. test score	-31.078*	-31.959**	-37.001***	-49.105**
	(15.675)	(15.223)	(11.354)	(19.740)
After reform * Elem. test	45.572***	40.045***	38.648***	50.295***
	(16.275)	(15.051)	(13.290)	(18.939)
Observations	7,692	7,692	7,692	7,692
R^2	0.273	0.274	0.278	0.322

The sample includes all residential building permits within .2 mi of a new boundary. Each panel presents the results of estimating Equation 3 with a different dependent variable. 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 pre-reform school assignment and are reported in parentheses.

Figure 4: Year-by-year regressions: new boundaries



These figures present year-by-year estimates of Equation 5. The estimation equation is: $y_{ijt} = \beta_0 q_j^{post} + \beta_{95} \cdot q_j^{post} + \beta_{96} \cdot q_j^{post} + \cdots + \beta_{07} \cdot q_j^{post} + n_i \gamma + \nu_t + \theta_{ib} + {}_i$

Table 6: School quality's effect on new construction: an analysis of destroyed boundaries

	(1)	(2)	(3)	(4)
(A) Log Total Cost				
Elem. test score	0.186	0.165	0.168	0.322
	(0.153)	(0.170)	(0.158)	(0.315)
After reform * Elem. test	-0.284**	-0.295*	-0.291*	-0.709**
	(0.135)	(0.167)	(0.162)	(0.345)
Observations	11,621	11,621	11,621	11,621
R^2	0.623	0.629	0.633	0.689
(B) Total Area				
Elem. test score	0.651	0.592	0.595	1.214
	(0.393)	(0.443)	(0.420)	(0.856)
After reform * Elem. test	-0.807**	-0.848*	-0.843**	-2.257**
	(0.335)	(0.440)	(0.422)	(0.910)
Observations	11,586	11,586	11,586	11,586
R^2	0.083	0.084	0.084	0.089
(C) Heated Area				
Elem. test score	0.472	0.412	0.420	0.871
	(0.335)	(0.378)	(0.355)	(0.729)
After reform * Elem. test	-0.595*	-0.610	-0.599	-1.641**
	(0.308)	(0.389)	(0.371)	(0.775)
Observations	11,586	11,586	11,586	11,586
R^2	0.577	0.589	0.592	0.660
(D) Bath				
Elem. test score	0.200	0.179	0.194	0.708*
	(0.172)	(0.201)	(0.191)	(0.400)
After reform * Elem. test	-0.353*	-0.376	-0.384	-1.251**
	(0.192)	(0.260)	(0.243)	(0.535)
Observations	11,532	11,532	11,532	11,532
R^2	0.223	0.228	0.230	0.271
(E) Nonc. Const.				
Elem. test score	-0.008	-0.010	-0.009	-0.030***
	(0.010)	(0.012)	(0.010)	(0.010)
After reform * Elem. test	0.026	0.028	0.026	0.010
	(0.035)	(0.035)	(0.037)	(0.050)
Boundary-by-census dum.				Yes
Observations	11,621	11,621	11,621	11,621
R^2	0.522	0.522	0.522	0.539
(F) Days Comp.				
Elem. test score	58.706***	60.920***	63.938***	95.900*
	(19.476)	(20.249)	(20.390)	(52.910)
After reform * Elem. test	-27.109	-29.915	-31.363	-92.730
	(25.936)	(29.815)	(30.078)	(67.996)
Boundary-by-census dum.				Yes
Observations	11,621	11,621	11,621	11,621
R^2	0.363	0.364	0.365	0.412

The sample includes all sales of building construction within .2 mi of a destroyed boundary. Each panel presents the results of estimating Equation 3 with a different dependent variable. Neighborhood controls are as in Table 5. Standard errors are clustered at the pre-reform school assignment and are

Table 7: Extensive margin: residential permits along new boundaries

	(1)	(2)	(3)	(4)
Elem. test score	6.271**	3.528	0.639	-10.895*
	(2.660)	(3.972)	(3.516)	(5.586)
After reform	-3.284	-3.284	-2.240	-2.240
	(2.461)	(2.462)	(2.188)	(2.235)
After reform * Elem. test	-5.956	-5.956	-0.178	-0.178
	(5.006)	(5.009)	(4.452)	(4.547)
Neighborhood controls		Yes	Yes	Yes
Neigh. controls * time			Yes	Yes
Boundary dummies				Yes
Observations	5568	5568	5568	5568
R^2	.0027	.0061	.008	.1
F-stat	5.984	4.597	2.572	2.731
Mean of dep. var.	8.27	8.27	8.27	8.27

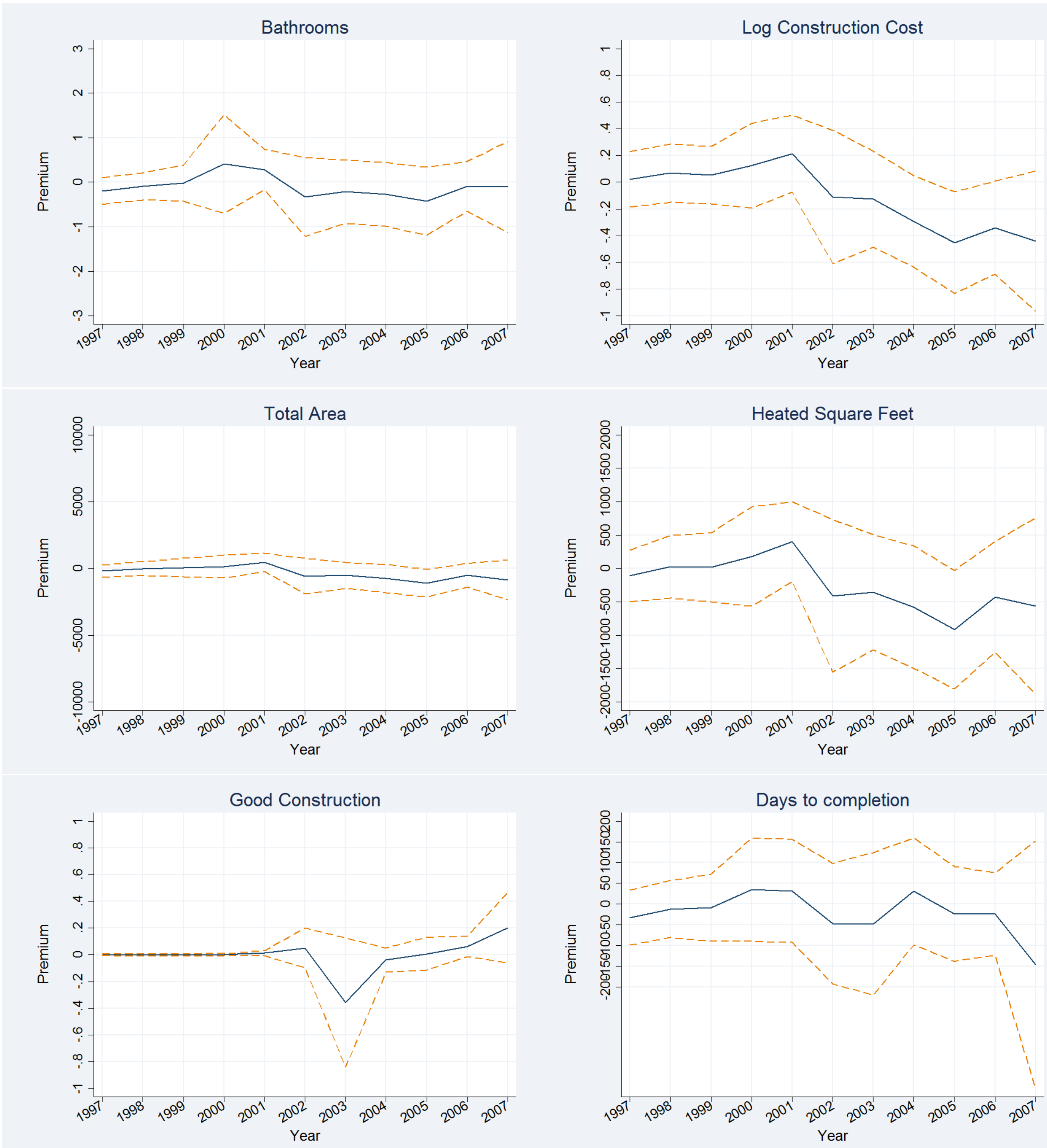
TBW. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table 8: Extensive margin by permit type: new boundaries

	Residential		Commercial	
	New Residence	Alteration	New Non-Res.	Non-Res. Alt.
Elem. test score	3.528	0.292	0.283	0.967
	(3.972)	(0.931)	(0.286)	(1.475)
After reform	-3.284	0.844	-0.271	-1.078
	(2.462)	(0.576)	(0.319)	(0.882)
After reform * Elem. test	-5.956	1.647**	0.065	-0.030
	(5.009)	(0.662)	(0.296)	(0.938)
Neighborhood controls	Yes	Yes	Yes	Yes
Observations	5,568	5,568	5,568	5,568
R^2	0.006	0.039	0.015	0.023
F-stat	4.597	6.342	11.059	4.111
Mean of dep. var.	8.27	2.93	0.93	2.36

TBW. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Figure 5: Year-by-year regressions: destroyed boundaries



These figures present year-by-year estimates of Equation TBW. The estimation equation is: $y_{ijt} = \beta_0 q_j^{pre} + \beta_{95} \cdot q_j^{pre} + \beta_{96} \cdot q_j^{pre} + \cdots + \beta_{07} \cdot q_j^{pre} + n_i \gamma + \nu_t + \theta_{ib} + {}_i$

Figure 6: Extensive margin year-by-year: residential permits along new boundaries

Estimation equation: $y_{ijt} = \beta_0 q_j^{post} + \beta_{95} \cdot q_j^{post} + \beta_{96} \cdot q_j^{post} + \cdots + \beta_{07} \cdot q_j^{post} + n_i\gamma + \nu_t + \theta_{ib} + \ _i$

Table 9: Extensive margin: residential permits along destroyed boundaries

	(1)	(2)	(3)	(4)
Elem. test score	10.097***	5.025*	2.687	2.825
	(2.991)	(2.983)	(2.806)	(4.379)
After reform	-1.221	-1.221	-0.477	-0.477
	(1.443)	(1.443)	(1.360)	(1.389)
After reform * Elem. test	-6.154	-6.154	-1.477	-1.477
	(5.855)	(5.857)	(5.849)	(5.975)
Neighborhood controls		Yes	Yes	Yes
Neigh. controls * time			Yes	Yes
Boundary dummies				Yes
Observations	6648	6648	6648	6648
R^2	.013	.026	.03	.17
F-stat	10.050	6.479	5.422	4.701
Mean of dep. var.	5.64	5.64	5.64	5.64

TBW. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table 10: Extensive margin year-by-year: residential permits along destroyed boundaries

TBW. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Extensive margin by type: destroyed boundaries

	Residential		Commercial	
	New Residence	Alteration	New Non-Res.	Non-Res. Alt.
Elem. test score	5.025*	-0.321	2.251	120.988
	(2.983)	(0.858)	(1.511)	(134.029)
After reform	-1.221	0.926	-0.819	48.045
	(1.443)	(0.598)	(1.252)	(38.455)
After reform * Elem. test	-6.154	0.914	0.597	-66.941
	(5.857)	(1.383)	(0.696)	(83.914)
Neighborhood controls	Yes	Yes	Yes	Yes
Observations	6,648	6,648	6,648	6,648
R^2	0.026	0.076	0.001	0.001
F-stat	6.479	8.683	4.994	2.041
Mean of dep. var.	5.64	2.73	2.06	27.93

TBW. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

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	176.65	169.60
	(343.38)	(156.07)	(198.16)	(380.73)	(198.40)	(203.64)
Full baths	2.09	1.94	1.93	2.10	1.96	1.96
	(0.68)	(0.69)	(0.61)	(0.70)	(0.72)	(0.63)
Half baths	0.61	0.54	0.46	0.62	0.54	0.56
	(0.52)	(0.52)	(0.52)	(0.52)	(0.53)	(0.52)
Num. bedrooms	3.35	3.21	3.14	3.30	3.18	3.15
	(0.72)	(0.74)	(0.65)	(0.76)	(0.79)	(0.70)
Year built, pre-1970	0.18	0.30	0.26	0.18	0.29	0.21
	(0.38)	(0.46)	(0.44)	(0.38)	(0.46)	(0.41)
Year built, 1970s	0.05	0.06	0.08	0.05	0.06	0.06
	(0.23)	(0.24)	(0.27)	(0.22)	(0.23)	(0.24)
Year built, 1980s	0.10	0.08	0.11	0.08	0.06	0.07
	(0.30)	(0.28)	(0.32)	(0.28)	(0.24)	(0.25)
Year built, 1990s	0.35	0.29	0.26	0.17	0.13	0.10
	(0.48)	(0.45)	(0.44)	(0.38)	(0.33)	(0.31)
Year built, 2000s	0.32	0.27	0.29	0.52	0.46	0.55
	(0.47)	(0.44)	(0.46)	(0.50)	(0.50)	(0.50)
Heated area (1000*ft)	2.16	1.94	1.78	2.20	1.99	1.94
	(1.04)	(0.94)	(0.84)	(1.20)	(0.98)	(0.88)
Brick face	0.24	0.27	0.23	0.21	0.24	0.18
	(0.43)	(0.44)	(0.42)	(0.41)	(0.43)	(0.38)
A/C unit	0.95	0.89	0.91	0.94	0.87	0.91
	(0.22)	(0.31)	(0.28)	(0.24)	(0.33)	(0.28)
Neighborhood characteristics						
Black households	0.20	0.30	0.28	0.22	0.32	0.28
	(0.22)	(0.29)	(0.24)	(0.23)	(0.30)	(0.24)
Asian households	0.03	0.03	0.03	0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Other race households	0.04	0.04	0.05	0.04	0.05	0.05
	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)
Fraction college	0.41	0.37	0.34	0.39	0.35	0.32
	(0.19)	(0.21)	(0.19)	(0.19)	(0.22)	(0.18)
Median hh income (2000 USD)	65.68	57.47	55.29	63.12	55.77	53.78
	(25.25)	(24.07)	(22.07)	(25.69)	(24.45)	(20.81)
Observations	103,470	18,829	11,228	166,629	32,258	21,874

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 TBW 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)
Elem. test score	0.177***	0.089*	0.091**	-0.005	0.123*	0.079	0.079	0.016
	(0.065)	(0.045)	(0.045)	(0.045)	(0.067)	(0.057)	(0.058)	(0.077)
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	17,495	17,495	17,495	17,495	19,419	19,419	19,419	19,419
R^2	0.689	0.699	0.701	0.724	0.607	0.612	0.613	0.636

This table reproduces Table TBW 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 TBW 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 TBW 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 TBW 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)
Elem. test score	0.019	-0.009	-0.015	0.077*	-0.001	-0.007	-0.004	0.058*
	(0.024)	(0.025)	(0.025)	(0.041)	(0.029)	(0.027)	(0.026)	(0.031)
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	25,454	25,454	25,454	25,454	28,276	28,276	28,276	28,276
R^2	0.732	0.738	0.740	0.762	0.687	0.690	0.691	0.705

This table reproduces Table TBW 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.