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School desegregation and urban change: Evidence from city boundaries

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Abstract: I examine changes in the city-suburban housing price gap in metropolitan areas with and without court-ordered desegregation plans over the 1970s, narrowing my comparison to housing units on opposite sides of district boundaries. The desegregation of public schools in central cities reduced the demand for urban residence, leading urban housing prices and rents to decline by six percent relative to neighboring suburbs. The aversion to integration was due both to changes in peer composition and to student reassignment to non-neighborhood schools. The associated reduction in the urban tax base imposed a fiscal externality on remaining urban residents.

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I. Introduction

Outside of the South, the desegregation of public schools fundamentally changed the bundle of public goods available to many central city residents. Before desegregation, the typical white student attended a local public school with predominately white peers. In the early 1970s, the Supreme Court ruled that non-southern school districts could be obligated to redress *de facto* racial segregation arising from historical patterns of residential location. As a result, students in some urban districts were exposed to cross-race peers for the first time, often by being reassigned to a school outside of their immediate neighborhood.

Previous work demonstrates that school desegregation led to improvements in educational outcomes for black students.[1] However, as this paper shows, court-ordered desegregation also generated considerable costs for central cities and their residents. Following the implementation of desegregation plans, white enrollment in urban schools fell as some households relocated to the suburbs and others opted for private schooling (Reber, 2005; Baum-Snow and Lutz, 2008). I show that falling demand for urban living resulted in a six percent decline in urban housing prices and rents relative to neighboring suburbs. The associated reduction in the urban tax base imposed a fiscal externality on the remaining residents of central cities. Although the federal government provided some monetary support for the direct cost of desegregation through the Emergency School Aid Act, these funds were not sufficient to fully compensate for the costs of the program, both psychic and real.

[1] Guryan (2004) and Ashenfelter, Collins and Yoon (2006) document that cohorts of black students who attended high school after the implementation of desegregation plans have lower dropout rates and higher earnings later in life. Weiner, Lutz and Ludwig (2009) and Johnson (2010) show that these cohorts are also less likely to be arrested or incarcerated. Reber (2010) demonstrates that, in the South, the net effect of desegregation on black educational attainment was due, in large part, to the equalization of school resources between blacks and whites.

This paper offers the first estimate of the effect of school desegregation on housing prices in a large sample of metropolitan areas.[2] Housing prices offer the possibility of estimating a precise metric of the marginal resident's willingness to pay to avoid school desegregation. In comparing the estimated effect of desegregation to related hedonic estimates in the literature, I conclude that two-thirds of the aversion to desegregation plans was due to the advent of racially integrated classrooms and any associated changes in peer quality (Kane, Riegg and Staiger, 2006).[3] The remainder can be attributed to the fact that desegregation plans required some children to be assigned to schools outside of their immediate neighborhood (Bogart and Cromwell, 2000).

Willingness to pay estimates also allow for a comparison of the responses to school desegregation in southern and non-southern areas. Many southern school districts were encouraged to desegregate through financial incentives embedded in Title I of the 1965 Elementary and Secondary Education Act. Cascio, et al. (2010) show that the average southern district required $1000 (in 2000 dollars) of federal funding per pupil per year to move beyond token levels of desegregation. After converting my housing price estimate into equivalent units, I show that the marginal resident of a non-southern city was up to three times less resistant to desegregation than was the median southern voter. Studying the behavior of these "typical" residents allows the history of desegregation to move beyond case studies that overemphasize the most vocal and organized members of society.

[2] Clotfelter (1975) compares housing prices across high school attendance areas following the desegregation of Atlanta schools.

[3] Angrist and Lang (2004) find no evidence of negative peer effects on existing students in school districts that accept minorities as part of Boston area's voluntary METCO busing program, despite the fact that the average METCO student has lower test scores than the average in the receiving districts. See also Hoxby and Weingarth (2005).

This paper focuses on 81 city-suburban school district pairs outside of the South, 29 of which were placed under court order to desegregate in the 1970s. In the 1950s and 1960s, case law focused on the legal (*de jure*) separation of schools by race in the South.[4] In the 1973 *Keyes v. Denver* decision, the Supreme Court ruled that school districts could also be required to desegregate if their school assignment policy reinforced residential segregation patterns, leading to *de facto* segregation (Clotfelter, 2004). For example, many northern and western districts were accused of gerrymandering their school attendance areas on the basis of race.

However, despite the fact that much residential segregation takes place between cities and suburbs, the Court established stringent conditions for extending desegregation remedies across district lines (*Miliken v. Bradley*, 1974). A district was considered segregated if the racial composition of individual schools was out of balance with the district as a whole. By this definition, an all-white district was not considered segregated. Because suburban districts had few, if any, black residents, only a small handful of suburbs were required to participate in desegregation activity.

Motivated by this legal history, my research design takes the form of a difference-in-differences estimation. The first difference considers the change in the city-suburban housing price gap over the 1970s in metropolitan areas whose central city faced mandatory desegregation. In these areas, neither the city nor the suburb were under court order to desegregate in 1970, while the city had fallen under court order to desegregate by 1980. The second difference incorporates city-suburban pairs in which neither the city nor the suburb (or, alternatively, both districts) underwent desegregation during the period. This comparison accounts for national trends that may have reduced the demand for urban residence over the

[4] 50 percent of large southern districts that desegregated through the courts received their court order in 1970 or before, compared to only 18 percent of northern and western districts (Guryan, 2004).

1970s, including the suburbanization of employment opportunities or fiscal mismanagement in central cities. Reassuringly, I do not find a differential trend in the city-suburban housing price gap between treatment and control borders in the previous decade (1960 to 1970).

In the ideal experiment, the city-suburban housing price gap would be measured by comparing housing units that are identical in all respects except for their location. However, in reality, city and suburban housing differ in many ways including age of the unit, lot size, and so on. I approximate the experiment of interest by comparing neighboring housing units on opposite sides of city-suburban school district boundaries, a method that has been used in other contexts to study the willingness to pay for school quality.[5] Because residents of the border area sample are predominately white, we should interpret the resulting estimate as the willingness to pay to avoid school desegregation among whites in northern and western cities. The estimated price response to desegregation in the district as a whole is twice as large as at the border, suggesting that a focus on the border area may control for important omitted variables.

The remainder of the paper is organized as follows. The next section introduces the estimation equation relating housing prices to the presence of a desegregation order. Section III describes the original data set combining Census blocks along school district borders with information on the timing and content of desegregation plans. In Section IV, I present the main effect of desegregation on housing prices and rents, while Section V considers alternative specifications. Section VI interprets the estimates in the context of the history of school desegregation. Section VII concludes.

[5] This border discontinuity method was pioneered by Black (1999), who studied the willingness to pay for school quality across school catchment area boundaries. See also Kane, Staiger and Samms (2003) and Figlio and Lucas (2004). Boustan (2007) compares housing prices across city-suburban boundaries to study the willingness to pay to live in a suburb with wealthy co-residents.

II. Estimation Strategy

The goal of this paper is to estimate the effect of court-ordered school desegregation on housing prices in a school district. If the marginal homebuyer has a distaste for integration, I expect that housing prices in urban districts that were required to desegregate will be lower than in their neighboring suburbs. I estimate the effect of school desegregation on housing prices by exploiting variation across metropolitan areas and over time. First, I evaluate changes in the city-suburban price gap between 1970 and 1980 in metropolitan areas anchored by a central city that faced mandatory desegregation. Then, I consider borders in which neither the city nor the suburban district (or, in some cases, both districts) underwent court-ordered desegregation over this period. Finally, my difference-in-differences specification compares changes in the city-suburban housing price gap over the 1970s in metropolitan areas that were subject to court-ordered desegregation and those that were not.

I begin with the sub-sample of metropolitan areas whose central city were required to desegregate in the 1970s. Pooling data from 1970 and 1980, I estimate:

$$\ln(\text{PRICE})_{isbt} = \beta_{\text{PLAN}}(\text{CITY} \times \text{T}) + \text{S} + \text{T} + (\text{B} \times \text{T}) + \varepsilon_{isbt} \qquad \textbf{(1)}$$

where PRICE indicates the mean value of owner-occupied housing units on block *i* at time *t*. My preferred specification limits attention to blocks on either side of school district boundaries in order to minimize differences in housing quality between the city and suburban housing units.

Equation 1 groups neighboring school districts into border areas, each containing one central city and one adjacent suburb. Side of the border fixed effects (S) are distinct dummy variables for blocks on the city or the suburban side of each border, respectively. These side of the border effects absorb long-standing differences in school quality or housing attributes across

borders. Border area fixed effects (B) capture neighborhood attributes that are shared by houses on either side of the border. Although the main effect of the border area variables are subsumed by the side of the border indicators, I include an interaction between the border area fixed effects and a dummy variable for the 1980 Census year (B *x* T). This interaction allows a common neighborhood trend as the border area gentrifies or deteriorates over time.[6]

The variable of interest in equation 1 is the interaction between CITY, an indicator for blocks on the city side of the border, and the 1980 Census year. In this sub-sample, all city blocks were exposed to desegregation over the 1970s. The coefficient β_{PLAN} identifies how the difference in housing prices between the city and suburban side of the typical border changed with the implementation of a desegregation plan. My hypothesis is that $\beta_{PLAN} < 0$, or that the price of city housing declined over the 1970s relative to its neighboring suburb as the city underwent a process of school desegregation.

For comparison, I estimate a corresponding equation for the portion of the sample in which the city did not undergo court-ordered desegregation over the 1970s (or, both the city and suburb did). I estimate:

$$\ln(PRICE)_{isbt} = \beta_{NOPLAN}(CITY \; x \; T) + S + T + (B \; x \; T) + \varepsilon_{isbt} \qquad \textbf{(2)}$$

Although I do not have a strong prediction about the sign of β_{NOPLAN}, the coefficient will be less than zero if other policy changes or events reduced the value of central city residence over the 1970s.

[6] Some school districts contribute observations to two or more border areas in the sample. For example, the north side of Chicago adjacent to Evanston, IL is part of one border area, while the west side of Chicago next to Oak Park, IL forms another border. Therefore, border area fixed effects are more flexible than school district effects, allowing for local differences in school quality within a district.

The difference-in-differences specification combines data from the full set of borders, both those that received court-orders to desegregate and those that did not, and estimates:

$$\ln(\text{PRICE})_{ibst} = \beta_{D\text{-}D}(\text{PLAN} \times \text{CITY} \times \text{T}) + \gamma(\text{CITY} \times \text{T}) + \text{S} + \text{T} + (\text{B} \times \text{T}) + \varepsilon_{ibst} \qquad \textbf{(3)}$$

The variable of interest is now the interaction between location in a central city, being in the post-desegregation era, and receiving a court-ordered desegregation plan over the 1970s (PLAN). A negative value of $\beta_{D\text{-}D}$ indicates that housing prices fell over time in cities that experienced desegregation over the 1970s relative to their suburban neighbors, as compared to pairs that did not undergo desegregation. The interaction term (CITY *x* T) controls for general trends that may have reduced the demand for city residence over the 1970s.[7] In all regressions, standard errors are clustered by school district and observations are weighted by the number of owner-occupied (or rental) units on the block.

Note that, for all borders in the sample, the school district and municipal borders coincide. Therefore, the main threats to identification in this framework are other events or changes in local policies over the 1970s that may be correlated with the implementation of a desegregation plan. Given that relative city-suburban housing prices are measured at the border, we need only be concerned about factors that change discretely as one crosses from one jurisdiction to the next. Table 1 demonstrates that, in 1970, urban districts that fell under court-order over the next decade were larger and had a higher black population share than other urban districts.[8] However, treated districts were indistinguishable in terms of median income, poverty

[7] Note that the other two double interactions – (PLAN *x* T) and (PLAN *x* CITY) – are subsumed by the border area-by-time and the side of the border fixed effects, respectively.

[8] Differences in size and racial composition are consistent with the legal strategy of groups like the National Association for the Advancement of Colored People (NAACP), which targeted populous districts first in order to use most efficiently their limited legal resources.

rate and the share of the population with a college degree. Therefore, the most likely sources of bias are other changes that are associated with initial differences in city size and racial composition. For example, cities with a higher black population share were more likely to experience a race-related riot in the late 1960s, which may have reduced relative housing prices in the central city over the 1970s (Collins and Margo, 2007). I show below that the estimates are robust to controlling for a measure of riot intensity.

The generalizability of the price response to desegregation estimated at district borders depends on whether residents of border areas reflect the preferences of other city and suburban residents. Baum-Snow and Lutz (2008) show that households living near the city limits were more likely to respond to desegregation by moving to the suburbs, while centrally located households were more likely to shift to private schooling. However, the fact that different sub-populations relied on different adjustment mechanisms does not imply that one group was more accepting of integration than the other. Both of these responses imply that the urban public schools bundled with urban housing services lost value with school desegregation; falling demand for urban public schools should be reflected in urban housing prices.

III. Data

A. Block-level variables

Estimating the effect of desegregation on housing prices requires a combination of data from multiple historical sources. I begin by using Census maps to identify pairs of neighboring city and suburban school districts for which block level data on housing values are available in the Census of Housing in 1970 and 1980. To increase the likelihood that housing and neighborhood attributes are shared by units on either side of the border, I eliminate borders that

are obstructed by a body of water, industrial land, a railroad or a four-lane highway. Furthermore, I restrict my attention to school districts with at least 10,000 residents to ensure the availability of the necessary demographic and socio-economic variables. I also omit southern districts for two reasons. First, much of the school desegregation activity in the South began in the 1960s, before the Census Bureau began sub-dividing suburban areas into blocks. Second, many southern school districts cover an entire county, incorporating both a central city and its suburban neighbors.

With these restrictions in place, the dataset contains 81 city-suburban boundaries in 29 northern and western metropolitan areas.[9] Table 2 lists the metropolitan areas in the dataset and the number of borders that each area contributes to the sample. The sample is evenly divided between the Northeast, the Midwest and the West but slightly over-represents large, fragmented metropolitan areas with populous suburbs. Los Angeles-Orange County, CA and New York City, NY-NJ, for example, together contain a quarter of the non-southern metropolitan population in 1970 while accounting for a third of the sample.[10]

This study focuses on blocks that are adjacent to the school district boundary. A Census block is roughly the size of a square city block and extends back from the border around the length of one football field. Because Census blocks were not digitally mapped in 1970 or 1980, I code blocks by hand according to their distance from the border. The block-level dataset contains information on housing prices and rents and a small set of housing quality measures from the

[9] The number of district borders in the sample may seem small relative to the total number of such divisions in the typical urban area. The 15 metropolitan areas in the sample anchored by a large city (that is, one of the 50 largest cities in 1970) had an average of 10.5 borders, 6.7 of which had 10,000 or more residents and 4.9 of which were clear of any obvious obstruction. The average number of useable borders by metropolitan area in the sample is only 3.1 (median = 2.0) because the sample also includes areas anchored by smaller cities.

[10] Many Ohio counties are unaccountably missing from the 1970 electronic block data. I limit coverage of Ohio to borders for which electronic data is available in 1970 and 1980.

Census of Housing. Compared to transactions data, the benefit of Census data is that it covers the full housing stock, rather than a selected sample of units that have been put up for sale. However, the drawback is that housing values are based on owner self-reports.[11] Due to confidentiality restrictions, the mean housing value (rent) is only available for blocks containing at least five owner-occupied (rental) units. Because desegregation may also affect the tenure decision, I also create a measure of the average "user cost" of housing on the block. The user cost is calculated as a weighted average of the annual rent paid by renters and the borrowing cost paid by homeowners (= home value *x* interest rate).[12]

Table 3 presents summary statistics of these housing measures for the full border sample and for blocks on the city or suburban side of sample borders. Blocks on either side of the city-suburban border have typically "suburban" characteristics. Nearly two-thirds of units were owner-occupied and residents on these blocks are disproportionately white. The racial composition of sample blocks (8 percent black) more closely resembles the average suburban district in the sample (5.5 percent black) than the average city (14.5 percent black). The distribution of black population share is bimodal, with over 80 percent of the blocks in the sample had no black residents at all. The mean value of owner-occupied units was slightly over $100,000 (in 2000 dollars) on both sides of the border and mean monthly rents were around $550, figures that fall between the city and suburban means. Although blocks on either side of the border are more similar to each other than they are to either the typical city or suburban area, there are still discernable differences between them. In particular, housing values were 5.7

[11] Kain and Quigley (1972) validate the owner self-reports in the Census data. However, self-reports may vary across district borders if some districts assess properties more regularly, thus providing owners with updated information.

[12] I use an interest rate of 9 percent, which was the national average contract mortgage interest rate over the 1970s. I accessed data on historical mortgage rates here: http://mortgage-x.com/trends.htm.

percent higher on the suburban side of the border in 1970; this difference is statistically significant.

B. School district variables

I collect data on the presence of desegregation court-orders by school district from the *State of Public School Integration* website (Logan, 2004). The site contains the full text of judicial decisions and enumerates each action that a district was required to take to counteract desegregation. In the main specification, I measure the presence of a desegregation plan with a dummy variable equal to one if the court required the district to engage in at least one remedial step over the 1970s (*PLAN*). I associate each plan with the date of the court order, even if the case was later appealed to a higher court. For example, the Denver plan is coded as being handed down in 1969, even though the Supreme Court ruled on the case in 1973. In alternative specifications, I also consider the number of remedial actions required by the court-order or the years elapsed since the case was decided. Actions include steps like redistricting school attendance areas, mandatory busing of students between schools, and the creation of magnet schools. While the median court-order required that the school district engage in two remedial steps, the number of steps ranges from one to ten.

The treatment group is made up of the 29 borders in the sample that divide an urban district that faced a desegregation court-order in the 1970s from a suburban district that did not. The other 52 borders constitute the control group. Of these, 40 borders did not receive a court-order to desegregate on either side before 1980, 7 borders contain districts that were both required to desegregate over the 1970s, and 5 borders desegregated by early court-order in the

1960s. For robustness, I later try dropping the borders that faced early desegregation or that contain districts that were both required to desegregate.

Desegregation plans were intended to increase interracial contact in public schools. One measure of the efficacy of these plans is the exposure index, which measures the share of the student body at the average white student's school that is black (or vice versa). The Office of Civil Rights collected school-level enrollment data by race for all school districts in 1970 and a sample of districts in 1980. The exposure index for district *d* is defined as:

$$E_d = (\Sigma_{s=1\ldots n} [w_{sd} \cdot b_{sd}/t_{sd}]) / W_d \quad \textbf{(4)}$$

where *s* indexes schools in the district. (b_{sd}/t_{sd}) measures the share of students at a given school who are black or the number of black students divided by the total number of students enrolled at that school. E_d calculates a weighted average of these black enrollment shares where the weights are the number of white students at the school (w_{sd}) and W_d indicates the number of white students in the district as a whole.

The effect of desegregation on exposure to black peers may vary substantially across households. Households living in school attendance areas whose local public school had a large black enrollment share before desegregation may experience little increase in exposure to black peers even with the implementation of a desegregation plan. In a later specification, I estimate heterogeneous effects of desegregation plans on housing prices according to the black enrollment share at the nearest high school in 1970. Without access to historical attendance area boundaries, I assume that students would have been assigned to their nearest public school (as the crow flies)

within the relevant school district.[13] I employ GIS software and school addresses from the 1970 Elementary and Secondary General Information System (ELSEGIS) to match Census tracts to the nearest high school in the district. The mapping procedure is outlined in the Data Appendix.

IV. Results

This section estimates the effect of court-ordered school desegregation on the demand for urban residence by examining changes in the city-suburban housing price gap over the 1970s in metropolitan areas with and without a court-ordered desegregation plan. White households may have disliked school desegregation because of anxieties about mixed-race classrooms, concerns about changes in peer quality, or objections to sending their children to non-neighborhood schools. Because the block sample is disproportionately made up of white neighborhoods, the estimates will recover the willingness to pay to avoid school desegregation for the marginal white homeowner or renter.

A. Desegregation and exposure to cross-race peers

Desegregation court-orders were intended to increase racial balance across schools. Reber (2005) demonstrates that the average desegregation plan successfully increased white exposure to black peers and vice versa. I begin by replicating this finding in my sample to show that the court-orders under study here were enforced (at least to some degree) and led to a measurable change in school policy.

Table 4 compares changes in white exposure to black peers in urban districts that fell under court-order during the 1970s with districts that avoided court supervision. At the beginning

[13] The initial black enrollment share will be measured with error if school boards were able to successfully gerrymander school attendance areas before desegregation in order to prevent racially-mixed classrooms.

of the decade, the black enrollment share at the average white student's school was slightly lower but not statistically different in districts that would fall under court-order (11.3 versus 12.6 percent), despite the fact that treated districts had a higher initial black population share. Over the 1970s, average white exposure to black peers increased by 20 point in cities under court-order but by only 5.5 points in cities that did not fall under court supervision. The difference-in-differences estimator indicates that this 14.5 point difference in the change in exposure is statistically significant and is robust to controlling for changes in total population and the black population share over the 1970s.

B. Desegregation and housing values

Table 5 explores the effect of desegregation on the value of owner-occupied housing. Column 1 begins by considering metropolitan areas whose central city received a court order to desegregate during the 1970s. In 1970, the price for units on the city side of these borders was already 4.7 percent lower than their suburban neighbors. This initial gap in housing prices could reflect pre-existing disparities in school quality or in other municipal services, like police protection. The presence of initial differences in housing prices underscores the importance of being able to measure housing prices before and after the policy change.

From 1970 to 1980, after the imposition of court-ordered desegregation, the housing price gap across these borders increased by 6.5 percentage points (equation 1). The declining relative value of city housing likely reflects an aversion to school desegregation. This conclusion is bolstered by the fact, illustrated in the second column, that the premium for suburban housing remained steady, increasing by only 0.7 points at control borders over the 1970s (equation 2). The difference-in-differences estimator indicates that the suburban price premium increased by

an additional 5.8 percentage points over the 1970s in metropolitan areas whose central city was required to desegregate (equation 3).

The estimated decline in relative city housing prices may simply be a continuation of trends from prior decades. The 1960s was a period of troubled race relations, prefaced by two decades of black in-migration to central cities and resulting "white flight" (Collins and Margo, 2007; Boustan, 2010). The final row of Table 5 examines changes in the city-suburban housing price gap across sample borders in the decade prior to the desegregation court-orders (1960 to 1970). I limit my attention to the 56 borders for which block-level data is available in 1960. Over the 1960s, the city-suburban price gap expanded by 2 percentage points both in metropolitan areas that fell under court-order in the 1970s and those that did not. The difference between these two border types is negligible and not statistically significant. Therefore, it is unlikely that the estimated change in housing prices is simply picking up long-run trends in urban demand.

For comparison, Table 6 estimates the effect of court-orders on the district-wide median housing price for the 59 borders with available data in published Census volumes.[14] In particular, the table contains estimates of equations (1) through (3) where the dependent variable is the median housing price in a city or suburban district present in the border sample. The first column demonstrates that the value of owner-occupied housing in treated cities was already substantially lower than their suburban counterparts in 1970 (18.5 percent). Over the decade, relative city prices declined by an additional 10.5 percentage points in cities subject to court-ordered desegregation, compared to a much smaller 1.7 percent decline in cities that were not (column 2).

[14] The coefficient from the 1970 to 1980 difference-in-difference regression is qualitatively similar when I restrict the sample to either the 56 borders with available block data in 1960 or to the 59 borders with available district-level data.

Altogether, relative housing prices fell by 12 percentage points more over the 1970s in cities that were subject to court-ordered desegregation.

The district-wide estimate of the willingness to pay to avoid school desegregation is twice as large as the value obtained at the city-suburban border. The disparity in these estimates may reflect differential trends in housing quality in the urban core relative to areas proximate to the suburbs, which highlights the value of restricting the main analysis to blocks adjacent to the district border. Alternatively, this gap could reflect different preferences between residents of border areas and households in other parts of the metropolitan area. However, in this case, the comparison between Tables 5 and 6 would imply that residents of border areas were *less* averse to desegregation than was the typical city household; this pattern is unlikely to hold given that households in border areas already selected to be close to the suburbs, perhaps to avoid the heavily black neighborhoods in the urban core.

C. Desegregation and other housing and neighborhood outcomes

Table 7 examines the effect of desegregation on other neighborhood outcomes, including rents, the user cost of housing, measures of housing quality and characteristics of local residents. As before, I focus on the city-suburban gap in each outcome measured at the border. For brevity, I do not present the level differences across borders in 1970 or 1980. Instead, the first column of Table 7 reports the change in the city-suburban housing price gap over the 1970s in metropolitan areas whose central city faced a desegregation court-order (equivalent to equation 1), the second column presents this change for control areas (equation 2), and the third column compares the two values (equation 3).

The monthly rent for rental units provides an additional measure of the market price of housing. However, the effect of desegregation on owner-occupied and rental housing may differ for two reasons. First, renters tend to be younger, less well-off, and less likely to have children, all of which may lead them to have different preferences for local public goods. In addition, housing prices incorporate expectations of future policy change between city and suburban school districts, while rents capture a location's value at a point in time. Given these factors, I find that desegregation had a slightly smaller effect on rents, although, given the standard errors, I cannot reject that the two estimates are the same. Over the 1970s, city rents fall by 6.6 percent relative to their suburban neighbors across treated borders. The relative decline in city rents is much more modest across control borders, resulting in a difference in differences estimate of a 4.0 percent decline in rents.

Due to data restrictions, only a subset of sample blocks have available data on average rental rates (housing values). I calculate a measure of the user cost of housing for the full sample, which is essentially a block-level weighted average of annual rents for rental units and annual borrowing costs for owner-occupied units.[15] Row 2 shows that the presence of a desegregation plan is associated with a 9.2 percent reduction in the relative annual user costs of urban housing in treated cities. Desegregation reduced both housing values and rents. Perhaps as a result, I find little relationship between desegregation and owner-occupancy rates (row 3).

Interpreting housing prices as a proxy for demand depends on the assumption that desegregation did not lead to a change in housing supply. The fourth row of Table 7 shows that there was no differential construction of new units on the city side of borders over the 1970s in either treatment or control cities. However, as prices fell, desegregation may have affected the

[15] Note that the coefficient on user costs is not itself a weighted average of the housing price and rental estimates because many blocks have both owner-occupied and rental housing.

financial return to home renovations or maintenance. The only available measure of the quality of the housing stock is the number of rooms in the typical unit. In areas under court-order to desegregate, the number of rooms in suburban housing units increased by 0.12 of a room relative to the neighboring city (row 5). If all home renovations consist of adding a single room, the difference-in-differences estimate suggests that desegregation slowed the pace of renovation by 17 percent.

Beyond changes to the housing stock, desegregation may have induced a re-sorting of the population at the local level, with households most opposed to the plan first to move out. White households may have been more opposed to desegregation than black households because of concerns about the effect of desegregation on peer quality. In addition, households with children may have been particularly averse to living in a desegregated school district. As a result, districts undergoing desegregation may have attracted more black residents and households without children than neighboring blocks in the suburbs over the 1970s.

Despite the potential for re-sorting across borders, I find little relationship between desegregation and either the racial composition or age distribution of the population in this sample. The sixth row of Table 7 shows that the presence of a desegregation plan is associated with a small and statistically insignificant increase in the probability of having a black neighbor. The final row of Table 7 estimates the effect of desegregation on the share of residents made up of school-aged children (5 to 17 years old). In both treatment and control areas, blocks on the city side of the border experienced small relative declines in the presence of school-aged children

over the 1970s. Having a court-order did not lead to a differential decline in the size of the school-aged population at city borders.[16]

V. Alternative specifications

Table 8 presents a series of robustness checks and alternative specifications for the relationship between school desegregation and housing prices. The table's first row reproduces the baseline estimate, which finds that integration reduced housing prices by 5.8 percent. The second row addresses the main threat to identification, namely other changes to central cities over the 1970s that may have coincided with desegregation. Cities under court order were larger and had a higher black population than cities that escaped court supervision (Table 1). A natural candidate, then, for an omitted city-level variable is the incidence of race-related riot activity in the 1960s and early 1970s. I use a city-level index of riot intensity proposed by Collins and Margo (2007), which combines riot-related deaths, arrests, arsons and other forms of damage. For this application, I set the riot index equal to zero in all cities in 1970, despite the fact that many riots occurred in the late 1960s, and assign the level of total riot activity over the period to 1980. Reassuringly, I find no effect of riot activity on housing prices from 1970 to 1980, either because their consequences were already incorporated into housing prices by 1970, as Collins and Margo's results would suggest, or because the epicenter of the violence was far from the suburban border. Most importantly for this context, adding a measure of riot activity has no effect on magnitude or precision of desegregation estimate.

[16] This pattern is consistent with Baum-Snow and Lutz's (2008) finding that, outside the South, urban residents were more likely to respond to mandated school desegregation by shifting to private schooling rather than by leaving the central city. In this case, desegregation would not lead to out-migration from the city and resulting changes in household composition but would reduce the value of urban housing as the demand for the public schools bundled with city housing services falls.

The third and fourth rows of Table 8 augment the price regression with controls for the black population share and the average number of rooms in units on the block. Controlling for the local racial composition has no effect on the estimated effect of desegregation. However, as we saw in Table 7, desegregation policies are associated with slower renovation of the housing stock. Therefore, controlling for the average number of rooms reduces the housing price estimate to 4.0 percent, implying that around 30 percent of the total relationship between desegregation and housing prices can be explained by changes in the underlying housing stock. Readers may prefer these values as true "hedonic" estimates of the willingness to pay to avoid school integration because they control for other observed differences in housing and neighborhood quality. However, I view the changing incentive for home renovations as an endogenous effect of the policy and therefore consider the number of rooms to be an outcome variable in its own right.

In the next two rows of Table 8, I drop subsets of the control group, starting with the five borders that faced early desegregation plans (row 5) and then the seven borders that faced court-ordered desegregation on both sides over the 1970s (row 6). Dropping the early plans reduces the coefficient of interest to 4.9 percent, which is significant at the 10 percent level, while dropping the borders with desegregation on both sides increases the coefficient to 7.6 percent. The results are qualitatively unchanged in regressions that drop borders in Los Angeles (row 7), the metropolitan area that contributes the largest number of observations to the sample, or that weight each block or each border equally (rows 8 and 9).

In the main specification, I group all desegregation court-orders into a single category and compare cities that faced a court-order in the 1970s to those that did not. In the tenth row, I instead count the number of required remedies contained in the court-order. Remedies include actions like rezoning school attendance areas, transferring students between schools, busing

students between schools or creating a magnet school. The coefficient implies that each required step reduced housing values by 1.9 percent. According to this estimate, a desegregation plan with the median number of steps (two) would lead to a 3.8 percent reduction in housing values, which is lower than the baseline estimate. This comparison suggests that the first step in a new plan had a larger effect on housing values than did incremental steps added to an existing plan.

School districts may have phased in the reforms required by a court-order over a number of years. In this case, we may expect the effect of a desegregation plan on housing values to accumulate over time. On the other hand, as soon as a court-order is handed down, the intended policy changes can be anticipated by the public and, therefore, any effect on the demand for residence in the school district may occur immediately. The final row of Table 8 replaces the dummy variable for the presence of a desegregation plan with a continuous variable indicating the years since the court-order was handed down. Housing values decline by 1.3 percent for every year since the court order was issued. This coefficient implies that the 5.9 percent decline in housing values estimated is reached around five years after the plan is first announced.

Households living in school attendance areas whose local public school had a large black enrollment share before desegregation may experience little increase in exposure to black peers after the implementation of a desegregation plan. Table 9 allows for a heterogeneous response to desegregation on the basis of the initial black enrollment share at the nearest high school. In particular, the table contains estimates of equation 3 augmented with an interaction between the desegregation effect [(PLAN *x* CITY *x* T)] and the black enrollment share at the nearest public high school in 1970. The main effect of the black enrollment share in 1970 is absorbed by the side of the border fixed effects. This specification is conducted on a subsample of predominately white blocks on which at least 98 percent of the residents are white. The first column re-

estimates equation 3 for this subsample; the typical desegregation plan reduced housing prices by 8.4 percent.

The second column adds the interaction between the presence of a desegregation plan and the black enrollment share in the nearest high school in 1970. I find that desegregation reduces housing values by 13.2 percent in areas of the city that otherwise would have attended an all-white high school (that is, the blocks for which the local high school had a black enrollment share of zero in 1970). As the initial black enrollment share of the local high school increases, the estimated effect of desegregation on housing values declines.[17] According to these estimates, desegregation would have had no effect on housing values in areas that were assigned to a high school with a 40 percent black enrollment share in 1970.[18]

VI. Interpretation

This section highlights three implications of the relationship between school desegregation and urban housing prices. First, I argue that court-ordered desegregation reduced the tax base of central cities, imposing a fiscal externality on city residents. Second, by comparing my estimate with others from the literature, I show that the willingness to pay to avoid school desegregation can be attributed both to concerns about cross-race peers and to preferences for neighborhood schools. Finally, I argue that the housing price estimate suggests that the marginal northern homeowner was substantially less resistant to desegregation than was

[17] I find a similar effect on housing prices when I interact the presence of a desegregation order with the *difference* between the district-wide black enrollment share and enrollment at the local high school, a measure that provides a sense of how much the local school would have had to change in order to be in compliance with the court order.

[18] The implied effect on housing prices on a block assigned to a high school that was 40 percent black in 1970 is zero (= -0.132 + [0.336 · 0.4]). Although being assigned to a 40 percent black high school is out of sample, there is substantial variation in the black enrollment share at the nearest high school across borders (mean gap = 15 pp; standard deviation = 23 pp).

the median southern voter, in the sense that she would have needed less monetary compensation in order to accept racial desegregation in local schools.

A. Tax revenue and fiscal externalities

The typical desegregation plan reduced housing values and rents in an urban school district. As a result, desegregation reduced the residential tax base in urban school districts relative to their neighboring suburbs. The average school district in the sample allocated $4,000 per pupil in educational expenditures (in 2000 dollars) and relied on residential property taxes for 75 percent of total revenue. Under various assumptions about the effect of desegregation on housing values in black neighborhoods, my estimates suggest that desegregation would have reduced the residential tax base by 4.9 to 6.0 percent.[19] A decline of this magnitude would translate into a $147 to $180 reduction in revenue per pupil assuming a constant property tax rate.[20] Over time, some portion of the funding of K-12 education has shifted from the locality to the state in many areas, reducing the long run consequences of a decline in the city tax base on school funding.

The full effect of desegregation on available resources per pupil depends on the relationship between desegregation and both tax revenues and schooling costs. If desegregation required new expenditures, such as additional buses or higher teacher salaries, the estimated

[19] The average decline in the residential tax base is a weighted average of changes in user costs in white and black neighborhoods. Given that residents on sample blocks are predominately white, I assume that my estimate indicates the change in housing prices in white neighborhoods. 84 percent of Census tracts in the median sample city were predominately white (defined as less than two percent black). If housing values were unchanged in black neighborhoods following desegregation, the residential tax base would have declined by 6.0 percent (= 0.16 · 0.000 + 0.84 · -0.071). If, instead, housing values increased in black neighborhoods by as much as they declined in white neighborhoods, the residential tax base would have declined by 4.9 percent (= 0.16· 0.071 + 0.84 · -0.071). This calculation uses the user cost of housing estimates from Table 8, row 2.

[20] Of course, cities were free to respond to this decline in the tax base by increasing the property tax rate, thereby holding constant the revenue collected per pupil. In that case, the cost of the unfunded mandate would have been borne broadly by property owners and renters, rather than only by households with school-aged children.

decline in available resources per pupil would understate the true decline. In contrast, if the policy change resulted in a net loss of student enrollments in urban schools, this value would be an overestimate. Furthermore, the research design in this paper can only identify changes in urban housing prices *relative to* neighboring suburbs. Therefore, while it is clear that school integration exacerbated inequities in school resources between cities and suburbs, we cannot conclude definitively that the urban tax base experienced an absolute decline.

B. Cross-race peers and neighborhood schools

The typical desegregation plan altered the mechanism by which students were assigned to schools. In order to comply with desegregation orders, school districts could no longer place all students in the nearest school. Rather, many white students were reassigned to schools in predominately black neighborhoods and vice versa. Comparing my results with estimates from the literature, I infer that objections to school desegregation were driven by more than just fears about cross-race classrooms or concerns about peer quality but also reflect an aversion to the assignment mechanism by which desegregation was achieved.

Kane, Riegg and Staiger (2006) compare housing prices on either side of elementary school attendance area boundaries in Charlotte-Mecklenberg, NC, while controlling for distance to school. According to their estimate, the increase in black enrollment associated with the typical desegregation plan would cause housing prices to decline by 3.8 percent.[21] By this measure, two-thirds of the estimated housing price response to school desegregation can be attributed to concern about mixed-race classrooms and associated changes in peer quality (=

[21] Kane and co-authors estimate that a 10 percentage point increase in black enrollment share leads to a 2.6 percent decline in housing prices, suggesting that the 14.5 percentage point increase in black enrollment associated with the typical plan in my sample (Table 4, row 2) would lead to a 3.8 percent decline in housing prices.

3.8/5.9). The remainder of the estimated price response is likely due to concerns about school re-assignment. Bogart and Cromwell find that assignment to a non-neighborhood school reduces housing prices by 7.5 percent. The residual change in housing prices would therefore imply that around 30 percent of sample households faced school re-assignment (= [5.9-3.8]/7.5), a value consistent with qualitative accounts of how desegregation was implemented.

C. A revealed preference approach to the history of Civil Rights

Existing histories of the Civil Rights era generalize about the popular response to school desegregation on the basis of the writings and actions of the most outspoken members of society.[22] These views – whether of angry segregationists who gathered to block the desegregation of Central High in Little Rock, AR or of crusading integrationists who marched in Selma, AL – may not be representative of the average resident. In contrast, this paper seeks to elicit *typical* attitudes toward school desegregation by studying the behavior of the marginal homeowner or renter.

In a related approach, Cascio, et al. (2010) study a large sample of southern school districts. Title I of the 1965 Elementary and Secondary Education Act provided federal funding for K-12 education nationwide. In order to be eligible for funding, school districts could not maintain segregated schools. The authors reason that, by accepting the offer of federal funding, school districts reveal the price at which their median voter was willing to forgo segregated schools. To be in compliance, districts needed to increase the black enrollment share at the average white student's school by around four percentage points. Cascio, et al. estimate that the

[22] A non-exhaustive list of the vast historical literature on responses to desegregation includes Carter, 1995; Gaston, 1998; Webb, 2005; Sokol, 2006 and Crespino, 2009.

typical southern district was willing to engage in this amount of desegregation for $1000 per pupil per year of federal funding (in 2000 dollars).

To compare my results with Cascio, et al., I convert housing prices into dollars per pupil. By my estimate, a four percentage point increase in black enrollment share is associated with a 2.6 percent decline in the user cost of housing, or a $168 reduction in annual user costs for the average housing unit ($=\$6{,}508 \cdot 0.026$).[23] Converting this value into dollars per child yields an annual payment of $303 per child, around one-third of the federal payments required to induce the typical southern school district to begin the desegregation process.[24]

By this metric, the median southern voter appears to have been four times as resistant to school desegregation as the marginal resident in the North.[25] Despite potential differences between the median voter and the marginal resident as bellwethers of "average" tastes, it appears that the typical southerner was substantially more opposed to desegregation than was the typical northerner. However, this gap is not as large as we might expect based on the case study evidence alone.

[23] The typical plan in my sample increased black enrollment share by 14.5 percent (Table 4) and reduced user costs by 9.2 percent (Table 8). By this estimate, the 4 percentage point increase in black enrollment share associated with Title I funding would lead user costs to fall by 2.6 percent. User costs is the most relevant metric for this calculation because it combines the preferences of home-owners and renters.

[24] The average block had 45 housing units and 25 school-aged children (5-17 years old).

[25] This comparison could understate regional differences because northern desegregation plans often required school reassignment while southern plans did not. However, the comparison could also overstate the typical southerner's distaste for integration for two reasons. First, residents most concerned about integration may have been most likely to vote in school board elections. Second, the Cascio, et al. paper generates variation in the size of federal grants by comparing rich and poor districts in states with greater and less school spending. Therefore, the marginal district that is indifferent about accepting the federal funding or not will be a rich district in an ungenerous state whose residents may have been more opposed to integration than the southern average.

VII. Conclusion

The integration of public schools by race was one of the most important changes to the American educational system in the twentieth century. The Supreme Court first required school districts to address the *de facto* school segregation associated with historical patterns of residential location by race in the mid-1970s. The Court considered extending this obligation to predominately white suburban areas, but ultimately rejected this possibility in the 1974 *Miliken v. Bradley* decision. Therefore, outside of the South, court-ordered desegregation was applied only to central cities.

As a result, the integration of public schools changed the value of urban residence in the North and West. Urban schools became more racially diverse and students were often reassigned to non-neighborhood schools in order to achieve the necessary racial mix. I show in this paper that this process of school desegregation resulted in a decline in the demand for urban residence. Housing prices and rents in cities under court-order fell by six percent relative to their neighboring suburbs. The associated reduction in the urban tax base created a fiscal externality for remaining residents of central cities.

Changes in housing prices reveal the marginal home owner's willingness to pay to avoid school desegregation. This value converts the average disapproval of school desegregation into a dollar value that can then be compared to other programs, time periods, or regions. Cascio, et al., for instance, estimate that the typical southern district would have engaged in a token amount of desegregation for a payment of $1,200 per pupil. By this measure, northern residents appear to be three times less averse to desegregation than the median southern voter. Such a revealed preference-based measure contributes to our understanding of the history of school desegregation and of the Civil Rights era more broadly.

Data Appendix

Pairing each Census block with the nearest high school proceeds in three steps:

1. 1970 street addresses for schools in sample districts are obtained from the Elementary and Secondary General Information System (ELSEGIS). I identify academic high schools as those that contain grades 9-12 or 10-12 and do not include the words "manual," "technical" or "vocational" in their name. Using GIS software, I locate these schools using the 2000 Census electronic road maps (http://www.esri.com/data/download/census2000_tigerline/). This process accurately geocoded over 90 percent of the schools in the sample. I checked the names and addresses of all unmatched schools using on-line resources. In some cases, road names had changed from 1970 to 2000 and could be edited by hand; in others, schools appears to have closed in the intervening three decades.

2. In a separate GIS layer, I map the centroid of Census tracts that contribute blocks to the sample. I then calculate the distance between Census tracts and high schools within the same district and select the high school with the minimum distance to be the assigned school for that area.

3. The Office of Civil Rights collected data on the racial composition of enrolled students by school. I match the OCR data with the ELSEGIS addresses using a cross-walk between the school identifiers. Districts with multiple tracts along one border area can match to more than one high school. In this cases, I assign the average racial composition of the two closest high schools to that area.

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Table 1: Initial characteristics of urban school districts, 1970

	Under order during 1970s	No order during 1970s	Difference
ln (population)	13.172	12.071	1.100
	(1.307)	(0.922)	(0.334)
Share black	0.189	0.130	0.059
	(0.127)	(0.136)	(0.043)
ln(median income)	10.718	10.716	0.002
	(0.139)	(0.132)	(0.043)
Share poverty	0.093	0.085	0.009
	(0.027)	(0.035)	(0.010)
Share college degree	0.122	0.104	0.017
	(0.093)	(0.056)	(0.022)

Notes: The regressions compare the 13 cities that received a desegregation court-order during the 1970s to the 36 cities that did not. Characteristics are measured in 1970.

Table 2: School district borders with available block-level data by metropolitan area

Region	Metropolitan area	Full sample	Number under court-order during 1970s
Northeast	Allentown-Bethlehem, PA	2	
	Boston, MA	3	2
	Hartford, CT	2	
	New York, NY-NJ†	10	1
	Pittsburgh, PA	2	2
	Providence, RI	1	
	Scranton, PA	1	
	Springfield-Chicopee, MA	1	1
Midwest	Akron, OH	2	
	Canton, OH	1	
	Chicago, IL†	5	
	Cleveland, OH	2	2
	Dayton, OH	1	1
	Des Moines, IA	1	
	Detroit, MI	5	5
	Grand Rapids, MI	3	
	Indianapolis, IN	1	
	Kansas City, KS-MO	4	2
	Minneapolis/St. Paul, MN	2	2
	Moline-Davenport, IL-IA	2	
	South Bend, IN	1	
	St. Louis, MO	1	
West	Denver, CO	2	
	Las Vegas, NV	1	
	Los Angeles, CA†	17	8
	Phoenix, AZ	1	
	San Bernard.-Riverside, CA	1	
	San Francisco, CA†	3	
	San Jose, CA	3	2
	TOTAL:	**81**	**29**

Notes: Metropolitan areas marked with † include secondary central cities that are now considered by the Census Bureau to anchor their own, independent metropolitan areas. These are: Newark, NJ; Jersey City, NJ; and Clifton, NJ (New York); Gary, IN (Chicago); Anaheim, CA (Los Angeles); and Oakland, CA (San Francisco).

Table 3: Summary statistics in border sample, 1970

	Full sample	City	Suburb
Average value, owner occupied	$107,083	104,079	110,125
N = 2087/1050/1037	(40,725)	(37,580)	(43,487)
Average rent	$549.40	544.16	554.79
N = 1513/767/746	(166.60)	(156.34)	(176.48)
Average user cost	$6,544.26	6,386.41	6,704.51
N = 2646/1318/1320	(2248.74)	(2065.41)	(2411.23)
Number units per block	45.046	46.371	43.721
	(53.388)	(59.178)	(46.875)
Number rooms in owned units	5.786	5.763	5.809
	(0.861)	(0.829)	(0.891)
Share owner occupied	0.629	0.625	0.633
	(0.310)	(0.313)	(0.307)
Share black	0.079	0.080	0.079
	(0.221)	(0.222)	(0.219)
Share pop, 0-4 yrs old	0.068	0.069	0.068
	(0.046)	(0.045)	(0.047)
Share pop, 5-17 yrs old	0.213	0.211	0.215
	(0.101)	(0.102)	(0.101)

Notes: The table reports means and standard deviations (in parentheses) of block-level characteristics for Census blocks adjacent to 81 city-suburban school district borders. The number of blocks underlying each statistic is reported in the left-hand column for the full sample, the city side of the border and the suburban side of the border respectively. The number reported for user costs apply to the rest of the table as well. Due to confidentiality restrictions, mean housing values (rents) are only available for blocks containing at least five owner-occupied (rental) units, while other characteristics are available for the full sample. All dollar values are reported in 2000 dollars. User cost is a weighted average of annual rent for rental units and borrowing cost for homeowners (= home value *x* 0.06 interest rate).

Table 4: School desegregation and white exposure to black peers

Dependent variable = White exposure to black peers

	Mean/standard deviation		
	Under court-order during 1970s	Not under court order during 1970s	Difference
1970	0.113	0.126	-0.012
	(0.067)	(0.114)	(0.034)
1980	0.313	0.181	0.132
	(0.206)	(0.119)	(0.053)
Δ 1970-1980			0.145
			(0.063)
Δ 1970-80 with controls			0.135
			(0.039)

Notes: The sample includes city districts for which there is school-level data on racial composition in 1970 and 1980. The regressions compare the 13 cities that received a desegregation court-order during the 1970s to the 24 cities with available data that did not. The difference-in-differences specification in the fourth row controls for the black population share and logarithm of total population in the district.

Table 5: School desegregation and relative city housing prices at the district border, 1960-80

Dependent variable = ln(housing value)

	Under court-order during 1970s	Not under order during 1970s	Difference
1970	-0.047	-0.026	-0.021
	(0.014)	(0.015)	(0.020)
1980	-0.097	-0.023	-0.073
	(0.028)	(0.022)	(0.035)
Δ 1970-1980	-0.065	-0.007	-0.058
	(0.024)	(0.015)	(0.028)
Pre-trend:			
Δ 1960-1970	-0.023	-0.022	-0.001
	(0.013)	(0.017)	(0.022)

Notes: Standard errors are reported in parentheses and clustered by school district. In rows 1 and 2, cells contain coefficients from regressions of block-level housing values on an indicator variable for being in the central city in a given decade (1970 or 1980). Row 3 reports coefficients for regressions of changes in housing prices from 1970 to 1980 on the interaction between being in the central city and in the 1980 Census year (equations 1-3 in the text). Row 4 conducts the same regression for the previous decade (1960 to 1970). Note that the coefficients in row 3 are not equivalent to the difference between the coefficients in rows 1 and 2 because the regressions underlying row 3 also include side of the border fixed effects. All regressions are weighted by the number of owner-occupied units on the block. For rows 1 to 3, the sample includes Census blocks adjacent to 81 city-suburban school district borders in 1970 and 1980. Data on housing values are only available for blocks containing at least five owner-occupied units. Regressions in row 3 contain 4386 observations, 2087 blocks from 1970 and 2299 blocks from 1980. Row 4 contains Census blocks adjacent to the 56 city-suburban borders with block-level data in 1960 (2495 observations, 1010 blocks from 1960 and 1485 blocks from 1970).

Table 6: School desegregation and relative city housing prices for the district as a whole

Dependent variable = ln(median housing value)

	Under court-order during 1970s	Not under order during 1970s	Difference
1970	-0.185	-0.073	-0.112
	(0.052)	(0.036)	(0.063)
1980	-0.290	-0.090	-0.200
	(0.072)	(0.050)	(0.086)
Δ 1970-1980	-0.142	-0.022	-0.120
	(0.044)	(0.020)	(0.040)

Notes: Standard errors are reported in parentheses. See the notes to Table 5 for details on the specification. The sample consists of school districts along the 59 borders for which housing price data is available in published Census volumes in 1970 and 1980.

Table 7: The effect of desegregation on other housing and neighborhood outcomes

	Court-order in 1970s *Δ 1970-80*	No order in 1970s *Δ 1970-80*	Difference Order vs. no order
ln(rent)	-0.066	-0.027	-0.040
	(0.024)	(0.021)	(0.030)
ln(user cost)	-0.125	-0.033	-0.092
	(0.031)	(0.022)	(0.037)
ln(num. units on block)	0.046	-0.010	0.055
	(0.055)	(0.030)	(0.064)
Share owner occupied	-0.014	0.002	-0.017
	(0.021)	(0.009)	(0.023)
Av. number of rooms	-0.115	0.050	-0.166
	(0.076)	(0.056)	(0.094)
Share black	0.020	0.003	0.017
	(0.019)	(0.010)	(0.021)
Share 5-17 years old	-0.008	-0.008	0.000
	(0.011)	(0.004)	(0.012)

Notes: Standard errors are reported in parentheses and clustered by school district. The sample includes Census blocks adjacent to 81 city-suburban school district borders in 1970 and 1980. See notes to Table 5 for further details on the sample and regression specification.

Table 8: Alternate specifications: Desegregation and housing values

Dependent variable = ln(housing value)

	Coefficient
(1) Baseline effect	-0.058 (0.028)
(2) Control for riot activity in city	-0.060 (0.028)
(3) Control for percent black on block	-0.059 (0.026)
(4) Control for number of rooms on block	-0.040 (0.026)
(5) Drop borders with early plans (in 1960s)	-0.049 (0.028)
(6) Drop borders with plans on both sides	-0.076 (0.026)
(7) Drop Los Angeles borders	-0.064 (0.040)
(8) Weight each block equally	-0.058 (0.027)
(9) Weight each border equally	-0.059 (0.029)
(10) RHS = Number of steps in court-order	-0.019 (0.003)
(11) RHS = Number of years since order passed	-0.013 (0.004)

Notes: Standard errors are reported in parentheses and clustered by school district. The sample includes Census blocks adjacent to 81 city-suburban school district borders in 1970 and 1980. See notes to Table 5 for further details on the sample and regression specification.

Table 9: Heterogeneous response to school desegregation: Interaction with pre-desegregation black enrollment share at nearest high school

Dependent variable = ln(housing value)

	(1)	(2)
PLAN *x* CITY *x* T	-0.084	-0.132
	(0.027)	(0.025)
PLAN *x* CITY *x* T *x* (%black nearest HS, 1970)		0.336
		(0.127)

Notes: Standard errors are reported in parentheses and clustered by school district. The sample includes the 2,815 Census blocks that were less than 2 percent black and were adjacent to 81 city-suburban school district borders in 1970 and 1980. See notes to Table 5 for further details on the sample and regression specification. The Data Appendix explains how Census blocks were paired with their nearest high school in 1970. School-level racial composition data is from the Office of Civil Rights.