Desegregation and urban change: Evidence from city boundaries

Leah Platt Boustan*
UCLA and NBER
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Abstract: In the early 1970s, courts mandated that many urban school districts outside the South desegregate by race while exempting the surrounding suburbs. I estimate the marginal willingness to pay to avoid desegregation by comparing housing values in city districts, some of which faced court-ordered desegregation, to their adjacent suburbs over the 1970s, focusing on areas close to the school district boundary. The average desegregation plan reduced urban housing prices and rents by 3-7 percent. Aversion to desegregation is due both to increased exposure to cross-race peers and to student reassignment to non-neighborhood schools.

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I. Introduction

Desegregation fundamentally changed the nature of public education in many urban school districts, increasing exposure to cross-race peers and, in many cases, leading to the reassignment of students to non-neighborhood schools. Before desegregation, the typical white student – even in a large central city – attended a local school with predominately white peers. This paper focuses on large urban districts outside of the South, many of which were ordered to desegregate following the 1973 *Keyes v. Denver* decision.[1]

In the *Keyes* decision, the Supreme Court ruled that districts were responsible not only for dismantling any officially-sanctioned separation of schools by race but also for correcting *de facto* segregation arising from racial residential patterns. However, despite the fact that a large portion of residential segregation occurs between a city and its suburbs, the Court declared that desegregation remedies could not extend across district lines (*Miliken v. Bradley*, 1974). Because most suburbs had few, if any, black residents, suburban districts themselves were often not considered to be segregated and thus were not required to participate in desegregation activity.

The court's restrictive definition of segregation ensured that many large urban districts desegregated over the 1970s while their suburban counterparts did not. This legal position indirectly reduced the demand for residence in central cities relative to their neighboring suburbs. In this paper, I consider two measures of housing demand: housing prices for owner-occupied units and monthly rent for rental units. I show that the typical desegregation plan reduced urban housing prices and rents by three to seven percent. Desegregation plans had no effect on the size

[1] Early case law focused on *de jure* segregation in the South. 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). Many small southern districts began the process following the 1965 Elementary and Secondary Education Act, which included financial incentives to desegregate (Cascio, et al., 2009).

or composition of the housing stock, suggesting that these indicators represents an unambiguous fall in demand.

The results in this paper are based on a comparison of 92 city-suburban pairs in 1970 and 1980. I begin by comparing housing values in cities that underwent desegregation to their neighboring suburbs before and after the court-order. I then contrast this change in relative prices with city-suburban pairs in which neither the city nor the suburb faced a court-order to desegregate (or both did). This third difference controls for national trends that may have depressed demand for urban areas, such as the suburbanization of employment opportunities or fiscal mismanagement in many central cities.

Cities that were required to desegregate in the 1970s were larger and had a higher black population share at the beginning of the decade than did their exempted counterparts. These characteristics may have led cities undergoing desegregation to have deteriorated relative to their suburban neighbors for reasons other than school policy – for instance, due to events like the urban riots (Collins and Margo, 2007). I address these differences by restricting my comparison to neighboring Census blocks that fall on opposite sides of city-suburban school district boundaries.[2] The presence of a court-order to desegregate changes discontinuously at school borders but many unobserved confounding factors that may be correlated with desegregation (such as property damage due to riot activity) change more continuously through space.

My estimates may reflect both a distaste for exposure to cross-race peers and concerns about reassignment of students to non-neighborhood schools. Existing estimates of the willingness to pay to avoid black classmates suggest that exposure to cross-race peers can

[2] 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.

explain around 50 percent of the observed decline in housing price. Bogart and Cromwell (2000) estimate that loss of a neighborhood school reduces housing prices by 7.5 percent. Given their results, the remaining decline in housing prices implies that around 30 percent of households were reassigned to distant schools under the typical desegregation plan.

Cascio, et al. (2009) studies the response to desegregation among small districts in the South. These districts were encouraged to begin the desegregation process through the financial incentives embedded in Title I in the 1965 Elementary and Secondary Education Act. Cascio, et al. find that the average southern district required $1000 per pupil per year ($2007) to engage in some desegregation. Converting my price estimate into dollars per child implies a $320 payment per pupil per year, suggesting that the median southern voter was three times as resistant to desegregation as the marginal resident in the North. Despite the differences in these approaches, this comparison creates a useful metric to compare regional differences in resistance to desegregation and allows us to move beyond case studies that overemphasize the most vocal and organized members of society.

II. Data

A. Block-level variables

I combine data from multiple sources to estimate the effect of desegregation on housing prices. I begin by using Census maps to identify pairs of neighboring city and suburban school districts for which block level data is available in 1970 and 1980. To be included in the analysis, the border must not be obstructed by a body of water, industrial land, or 4-lane highway to improve the plausibility of the identifying assumption that neighborhood characteristics change in a continuous fashion across district borders. Furthermore, school districts on either side of the

border must have 10,000 or more residents to ensure the availability of demographic and socio-economic data.[3]

The dataset contains 92 city-suburban boundaries in 30 northern and western metropolitan areas. I omit the South for two reasons. First, many southern districts desegregated in the 1960s. In 1960, the Census Bureau only assigned blocks to central cities and a few large suburban areas and so I cannot observe pre-desegregation outcomes along most southern borders. In addition, many southern school districts contain an entire county, including both a central city and its suburban neighbors. County districts often end at the agricultural periphery of a metropolitan area, an area that was not subdivided into Census blocks in 1970.

Table 1 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. Large, fragmented cities with populous suburbs are slightly over-represented. Los Angeles-Orange County and New York-Northern New Jersey account for 30 percent of the sample while they contained only 24 percent of the non-southern metropolitan population in 1970.[4]

The unit of observation in the main analysis is a Census block. In particular, the estimation relies on comparing neighboring blocks that fall on opposite sides of city-suburban school district boundaries. Census blocks were not digitally mapped in 1970 or 1980. Instead, I

[3] The number of borders in the sample may seem small relative to the total number of divisions in urban areas. For the 15 metropolitan areas in the sample anchored by a large city (that is, one of the 50 largest cities in 1970), the average number of city-suburban borders is 10.5, 6.7 of which had 10,000 or more residents and 4.9 of which were clear of any obvious obstruction. Because this sample also includes 15 metropolitan areas anchored by smaller cities, the average number of borders for each city is 3.1 (median = 2.0).

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

code blocks by hand according to their distance from the border. I define blocks that are themselves adjacent to the boundary as being the first block "tier."

The block-level dataset contains information on distance from the jurisdiction border, housing prices and housing quality measures. The data on housing values and rents used in this study are taken from the Census of Housing. Housing price variables include the mean value of owner-occupied units and the mean rent for rental units.[5] Due to confidentiality concerns, housing prices (rents) are 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 · interest rate).[6]

Interpreting prices and vacancy rates as indicators of demand relies on the assumption of a fixed housing stock. The housing stock itself could expand through new construction or through conversion of owner-occupied units into rentals (or vice versa). Measures of the housing stock include the number of units on the block , the number of units that are owner-occupied or rented, and the average number of rooms per owner-occupied unit.

The sample is disproportionately composed of white neighborhoods. In 1970, 5.6 percent of residents on the average block were black. However, as Figure 1 makes clear, the distribution of black population share is heavily skewed toward zero. Over 80 percent is the sample is made up of blocks that have no black residents. White and black households may have had different preferences for school desegregation. While white parents may have worried about a decline in

[5] Housing values are based on owner self-reports. Kain and Quigley (1972) argue that owner reports are reliable. However, self-reports may vary across jurisdictional borders if some towns assess properties more regularly, thus providing owners with updated information.

[6] I assume an interest rate of 6 percent, which is slightly lower than the average interest rate over the 1970s.

average peer quality, black parents may have welcomed desegregation as an opportunity to improve the average level of preparation of their child's peers.[7] I report the effect of desegregation on housing prices in the full sample and for the white sub-sample, blocks that are at least 98 percent white. Given the degree of polarization in the data, results from the sub-sample are robust to other cutoffs, including blocks that are entirely white or blocks that are at least 95 percent white.

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 each judicial decision and enumerates each action that a district was required to take to counteract desegregation. 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. 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 (*PLAN*). In alternative specifications, I also consider the relationship between housing values and the number of remedial actions required by the court-order or the years since the case was decided.

36 of the borders in the sample divide a city district that faced a desegregation court-order from a suburban district that did not, while 45 borders contain districts that did not experience desegregation and 11 contain districts that both underwent desegregation. Borders in

[7] Guryan (2004) and Ashenfelter, Collins and Yoon (2006) show that cohorts of black students who attended high school after the implementation of desegregation plans had lower dropout rates and higher earnings later in life.

which both districts faced desegregation include Los Angeles-Pasadena, CA; New York City-Yonkers, NY; and St. Louis-University City, MO.

Desegregation plans were intended to increase interracial contact in public schools. One measure of the efficacy of this plans is the exposure index, which measures the share of the student body at the average white student's school who are black. School-level data on the racial composition of the student body was collected by the Office of Civil Rights in 1970 and 1980.[8] The exposure index for district *d* is defined as:

$$E_d = 1/W_d \, \Sigma_{s=1\ldots n} \, w_{sd} \cdot b_{sd}/t_{sd} \qquad \textbf{(1)}$$

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.[9] 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}). W_d indicates the number of white students in the district as a whole.

While the exposure index measures the average change in white contact with black students, 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 with school desegregation. To capture this variation, I measure the initial black enrollment share at the nearest high school in 1970 for every block in the sample. Without access to historical attendance area boundaries, I assume that students would have been assigned to their

[8] I thank Sarah Reber for generously providing access to the digitized Office of Civil Rights data.
[9] The dissimilarity index, another common measure of racial integration, is not well-suited for this context because it requires the existence of many sub-units (schools) within the larger entity (districts). Many suburban districts only have a single high school and would thus appear to be perfectly integrated by the dissimilarity index.

nearest public school (as the crow flies).[10] 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. Racial composition in 1970 is taken from the school-level Office of Civil Rights data described above. The mapping procedure is outlined in the Data Appendix.

III. Estimation Strategy

The goal of this paper is to estimate the effect of court-ordered school desegregation on housing prices. The empirical approach can best be understood as a triple difference. The first set of contrasts occurs in metropolitan areas whose central city engaged in desegregation. In 1970 (the pre-period), neither the city nor the suburb in these areas were under court order to desegregate their schools. By 1980, the city underwent a desegregation treatment while the suburb's school system remained unchanged. I add a third difference with metropolitan areas in which neither the city nor the suburb underwent desegregation to adjust for other differences in urban areas over the 1970s –for example, due to highway construction or the suburbanization of employment opportunities (Baum-Snow, 2007; Boustan and Margo, 2009).

While this design controls for national trends in urban change, urban districts that fell under court-order to desegregate may have faced a different trajectory over the 1970s for reasons other than desegregation. Table 2 compares the initial characteristics for the 48 urban districts in the sample.[11] Districts that were required to desegregate by 1980 were twice as large as those that were not and had a 6.6 percentage point higher black population share (on base of 11.5 percent). Differences in size and racial composition are consistent with the legal strategy of

[10] If school boards gerrymandered school attendance areas before desegregation in order to prevent racially-mixed classrooms, my measure of initial black enrollment share will be inaccurate.

[11] Many of the cities in the sample border on more than one suburb. Together, these 48 urban areas compose the city side of the 92 city-suburban pairs in the data set.

groups like the National Association for the Advancement of Colored People (NAACP), which targeted populous districts first in order to use their limited legal resources efficiently. Urban districts undergoing desegregation are statistically indistinguishable from their exempted counterparts along other dimensions, including median income, poverty rate and the share of the population with a college degree. However, differences in size and, especially, racial composition may have set these cities on a different trajectory over the 1970s. 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 urban housing prices relative to their suburban neighbors (Collins and Margo, 2007).

I address this source of bias by comparing housing units on opposite sides of city-suburban school district boundaries. The presence of school desegregation changes discretely at school district borders. The necessary identifying assumption is that other changes to housing and neighborhood quality over the 1970s occurred in a more continuous fashion. For example, in the case of a riot, the assumption requires the destructive path to decay with distance from the epicenter of the violence, rather than changing discontinuously at the district border.

Are homeowners willing to pay more for the same housing unit if it is located in a school district that avoided court-ordered desegregation? I pool block-level data from 1970 and 1980 and estimate:

$$\ln(PRICE)_{ibdt} = \alpha_1 + \beta_1(PLAN)_{dt} + X_{dt}\Gamma_1 + D + T + B + (B \cdot T) + (B \cdot D) + \varepsilon_{ibdt} \qquad \textbf{(2)}$$

where *PRICE* measures the mean value of housing units on block *i* in school district *d* at time *t*. Pairs of adjacent city and suburban school districts are grouped into border areas, which are indexed by *b*.

The model is nearly saturated by a full set of geography and time fixed effects. School district dummy variables (D) capture long-standing differences in school quality. Border area fixed effects (B) absorb neighborhood attributes that are shared by houses on both sides of the border – such as the presence of a nearby park, a bus line, or a commercial strip. The interaction term (B · T) allows border area effects to change over time if, say, the neighborhood gentrifies or deteriorates over the 1970s. Some school districts belong to two or more border areas. 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. The interaction term (B · D) permits variation in the school district fixed effect by border area to account for local differences in school quality.

The effect of a desegregation plan on housing prices is identified by the triple interaction (D · B · T), the specific price trajectories for each of the school districts in a border area over the 1970s.[12] A negative value of β_1 indicates that housing prices fell over time in cities that experienced desegregation over the 1970s relative to their suburban neighbors and compared to other city-suburban pairs that did not undergo desegregation. I also control for the black population share and the logarithm of population in the school district and a limited set of block-level housing quality measures.

IV. Results

This section estimates the effect of court-ordered school desegregation on the demand for urban residence by comparing housing prices and rents on blocks adjacent to school district boundaries. Because the block sample is disproportionately composed of white neighborhoods,

[12] In theory, I could also control for the interaction between school district and time (D · T), identifying β from districts that fall into multiple border areas. However, few sample districts meet this criteria.

the estimates will recover the willingness to pay to avoid school desegregation for the marginal white homeowner or renter. White households may dislike school desegregation because of direct concerns about mixed-race classrooms, indirect concerns about peer quality or objections to sending their children to non-neighborhood schools. After obtaining the estimated price response to desegregation, I will decompose the effect into concerns about cross-race peers and non-neighborhood schools.

A. Desegregation and exposure to cross-race peers

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

Table 3 presents coefficients from a regression estimating the relationship between the implementation of a desegregation plan during the 1970s and the change in average white exposure to black peers in a school district. In 1970, the typical white student in the average school district attended a school with a 9.1 percent black enrollment share. The advent of desegregation increased exposure to black peers by 10.9 percentage points, doubling the average exposure to black peers.

The rest of the paper examines the effect of desegregation plans themselves, rather than the realized changes in exposure to black peers, on housing outcomes. Because the change in exposure to black peers may be correlated with the loss of neighborhood schools, we could not

interpret a relationship between changes in exposure and housing prices as the direct willingness to pay to avoid black classmates.

B. The effect of desegregation on the housing stock

Before turning to the housing value results, I begin by showing that the housing stock and the racial composition of border neighborhoods do not change after the implementation of a desegregation plan. This exercise helps to validate the identifying assumption that neighborhood quality does not differentially change on one side of the border with desegregation.

The first column of Table 4 presents summary statistics for blocks with at least five owner-occupied units for which there is available housing value data in 1970. The typical block has 44.5 housing units with an average of 5.8 rooms. 72 percent of these units are owner-occupied. In the second and third column, I regress these housing stock characteristics on the presence of a desegregation plan in the full sample and for a sub-sample of blocks that are at least 98 percent white. Desegregation had no effect on the number of housing units on the block, which is not surprising because the central cities and inner-ring suburbs in the sample were already "built up" by the 1970s. At most, desegregation led to the construction of one additional housing unit per block – a three percent increase on a base of 45 units – but this effect is imprecisely estimated. The imposition of a desegregation court-order also had no effect on the share of the units that were owner-occupied versus rented.

While desegregation did not affect the number of housing units in an area, there is mild evidence that it reduced the *quality* of the housing stock. In districts undergoing desegregation, the average number of rooms per housing unit declined by 0.13 of a room over the 1970s relative to units across the border; this coefficient is significant at the ten percent level in the full sample

and statistically insignificant in the white sub-sample. To interpret this estimate, imagine two blocks with 45 units each, 35 of which have six rooms and 10 of which have five rooms in 1970 (average number of rooms = 5.78). One scenario that could generate a 0.13 room gap between the two blocks by 1980 is if the block undergoing desegregation remained unchanged and 6 of the 10 five-room units on the neighboring block added an extra room (new average = 5.91). In other words, if all renovations involve adding a single room, the coefficient suggests that desegregation reduces the share of homeowners engaging in renovations by 13 percent (= 6 renovations/45 units). It is important to note that if desegregation changed the financial return from investing in one's home, it may also be negatively correlated with other forms of renovation or maintenance that remain unobserved in the Census data.

Reber (2005) shows that the typical desegregation plan resulted in a decline in aggregate white enrollment, which is an indication either of white out-mobility or of a shift from public to private schooling (see also: Baum-Snow and Lutz, 2008). A resulting change in local racial composition could confound the estimates if white households dislike black neighbors and interactions are extremely localized, occurring only with other residents of the same block and not with immediate cross-border neighbors. In the full sample, the presence of a desegregation plan is associated with a 1.8 percentage point increase in the black population share of the local area, but this relationship is not statistically different from zero. In the white sub-sample, desegregation does not change the local racial composition in any way (though this null effect is almost by construction).

Taken together, there is no evidence that desegregation leads to changes in housing supply or in the composition of the local population. However, desegregation may slow investment in the housing stock. I will control for the average number of rooms for units on the

block but other measures of housing quality are unobserved. Overall, there is little reason to expect that desegregation will lead to a decline in housing prices unless this policy change reduces the demand to live in the school district in question.

C. The effect of desegregation on housing price and rents

Table 5 presents the core result in the paper – namely, the effect of desegregation on the value of owner-occupied housing and the monthly price of rental units. At the beginning of the period, an owner-occupied housing unit on the typical sample block was worth $79,000 (in 2000 dollars) and annual rent for the average rental unit was $3,480. Following desegregation, the value of owner-occupied units fell by 5.1 percent in the full sample and by 6.4 percent in the sub-sample of predominately white blocks. Monthly rent declined by a similar 7.3 percent in the full sample and by 3.0 percent in the white sub-sample, though the last estimate cannot be statistically distinguished from zero.

Data are only available on housing values (rents) on blocks with at least five owner-occupied (rental) units. While, on average, desegregation is not associated with a shift from owner-occupancy to tenancy (Table 4), small changes in owner-occupancy can have potentially large changes in sample composition due to this data restriction. I create a measure of the average user cost of housing, which is a weighted average of annual rents for rental units and annual borrowing costs for owner-occupied units. This measure is not sensitive to the owner-occupancy rate. The presence of a desegregation plan leads the annual user costs of housing to decline by 9.6 percent in both the full sample and in the white sub-sample.

These results suggest that desegregation reduced the residential tax base of school districts and, therefore, the resources available to the average student. The average district in the

sample allocated $4,000 per pupil in 1970 (in 2000 dollars) and relied on residential property taxes for around 75 percent of total revenue. I assume that the estimated decline in housing values, which was generated from a comparison across school district borders, can be applied to all white neighborhoods in the central city. In 1970, 84 percent of the Census tracts in the median non-southern city were less than two percent black. Under various assumptions about the relationship between desegregation and housing values in black neighborhoods, we can conclude that desegregation reduced the residential tax base by 6.6 to 8.1 percent.[13] Such a decline in the tax base would translate into a $198-$243 decline in revenue per pupil. If desegregation also required new expenses such as new buses or higher teacher salaries, this value would be an underestimate of the declining resources associated with desegregation. These calculations make clear that court-ordered desegregation operated as an unfunded mandate that disproportionately applied to central city districts.

D. Alternative specifications

Table 6 presents a series of alternative methods to analyze the effect of desegregation on housing prices. The baseline specification groups all desegregation court-orders into a single category and compares districts that experienced desegregation of any kind to districts that did not. The first row of Table 6 instead counts 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

[13] For the median city, the average decline in the value of housing stock would be a weighted average between black and white neighborhoods. If housing values are unchanged in black neighborhoods, the residential tax base would decline by 8.1 percent (= 0.16 · 0.000 + 0.84 · -0.096). If, instead, housing values increased in black neighborhoods by as much as they declined in white neighborhoods, the residential tax base would decline by 6.6 percent (= 0.16 · 0.096 + 0.84 · -0.096). This calculation uses the user cost of housing estimates from Table 5, row 3.

coefficients imply that each required step reduced housing values by 1.7 percent in the full sample and by 2.2 percent in the white sub-sample. According to this estimate, a desegregation plan with the median number of steps (two) would lead to a 4.4 percent reduction in housing values in the white sub-sample. The implied effect of a desegregation plan in this specification is lower than the base estimate of 6.4 percent (Table 5), suggesting that the first step in a new plan had a larger effect on housing values than did adding incremental steps to an existing plan.

School districts may have taken a few years to implement the reforms contained in a court-order. In this case, we may expect the effect of a desegregation plan on housing values to accumulate over time. However, as soon as a court-order is handed down, the intended policy changes are made public and, therefore, any effect on the demand for residence in the school district may occur immediately. The second row of Table 6 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 6.4 percent decline in housing values estimated for the white sub-sample is only reached five years after the plan is first announced.

The main specification conducts a triple difference, comparing urban school districts undergoing desegregation to their neighboring suburbs and then contrasting these borders with urban-suburban pairs in which neither side was under court-order to desegregate. The goal of the triple difference is to use borders with no desegregation activity to control for other trends facing large urban areas during the 1970s. However, if borders with no desegregation activity are systematically different and are subject to their own idiosyncratic shocks, one may be concerned that the main results are being driven by this potentially misleading comparison. The third row of Table 6 reports estimates based on a simpler difference-in-differences strategy that contrasts

housing values in school districts undergoing desegregation and in neighboring suburbs, as before limiting the comparison to Census blocks adjacent to the school district border. The estimated effects on housing prices in this context are nearly identical to the triple-difference case in Table 5.

The last alternative specification allows for a heterogeneous response to desegregation on the basis of the initial black enrollment share at the nearest high school. While the average gap in black enrollment share across district borders in 1970 is 15 percentage points, there is substantial variation in the size of this gap (standard deviation = 23 points). One case in point: high schools on either side of the Los Angeles-Glendale, CA border have essentially no black students and the cross-border gap in black enrollment share is only 0.2 percentage points, whereas the Los Angeles high school closest to Inglewood, CA was over 95 percent black and the cross-border gap in black enrollment in 1970 was 87 percentage points. Following desegregation, Los Angeles residents on the Glendale border likely experienced a greater change in exposure to black peers than did Angelinos who already lived near Inglewood.

In the fourth row of Table 6, I interact a dummy variable for the presence of a desegregation plan with the initial black enrollment share in the nearest high school (as of 1970). The main effect of the 1970 black enrollment share is subsumed by the school district-by-border area fixed effects. In the white sub-sample, the advent of desegregation reduces housing values by 11 percent in areas of the city that otherwise would have attended an all-white high school. As the pre-desegregation black enrollment share of the local high school increases, the effect of the desegregation plan on housing values declines. The coefficients suggest that desegregation had no effect on housing values in areas that otherwise would have been assigned to a high school with a 50 percent black enrollment share (= -0.113 + 0.248 · 0.5). This pattern is robust to

excluding the borders with the largest pre-desegregation gap in black enrollment (Los Angeles-Inglewood, CA and Detroit-Dearborn, MI).

V. Interpretation

A. Exposure to cross-race peers versus preference for neighborhood schools

In order to increase exposure to cross-race peers, the typical desegregation plan changed the system by which students were assigned to schools. Rather than placing students in the nearest school, many school districts reassigned white students to predominately black schools in black neighborhoods and vice versa. Based on a study of a school redistricting plan in Shaker Heights, Ohio, Bogart and Cromwell (2000) estimate that assignment to a non-neighborhood school reduces housing prices by 7.5 percent. A portion of the estimated willingness to pay to avoid school desegregation may be due to concerns about school location, rather than concerns about peer composition *per se*. While I am unable to disentangle these two mechanisms directly, this section compares my estimates to existing values from the literature to infer the possible contribution of each channel.

To facilitate the comparison across studies, I convert the existing estimates into the implied change in housing prices for a 10.9 percentage point increase in exposure to black peers associated with the typical desegregation plan. Clotfelter (1975) compares housing prices by high school attendance area following the desegregation of Atlanta schools. According to his estimates, a 10.9 percentage point increase in black enrollment share was associated with a 5.5 percent decline in average housing values. In a more recent study, Kane, Riegg and Staiger (2006) compare housing prices on either side of elementary school attendance area boundaries in Charlotte-Mecklenberg, NC. Their estimates implies that a 10.9 percentage point increase in

black enrollment share leads to a 2.7 percent decline in average housing values. The 5.1 percent decline in housing values in my sample falls between the two existing values.

Methodologically, my approach is closer to the Kane, Riegg and Staiger study, which also relies on a narrow comparison between houses on either side of a school attendance border. In contrast, Clotfelter's design cannot control for unobserved differences between attendance areas that received large numbers of new black students during desegregation and those that remained relatively unchanged. Therefore, in what follows, I will compare my results with Kane, et al.

Kane and co-authors compare housing prices across school attendance areas within a single school district. As a result, we may assume that all households in the sample faced the same probability of being assigned to a non-neighborhood school. In contrast, I rely on cross-district variation in which the presence of a desegregation plan is associated with both an increase in exposure to black peers and a heightened probability of being assigned to a non-neighborhood school.

Under the assumption that the Kane, et al. parameters represent the "true" willingness to pay to avoid cross-race peers, the residual component of my estimate can be attributed to a distaste for being assigned to a non-neighborhood schools. After accounting for the change in peer composition, my estimate leaves a 2.4 percent change in housing values unexplained (= 5.1-2.7). Relying on Bogart and Cromwell's estimate of the value of a neighborhood school, this residual change in housing prices implies that 32 percent of households in the sample would have faced re-assignment to a non-neighborhood school (= 2.4/7.5). This decomposition suggests that around half of the aversion to school desegregation was due to a change in the racial composition of peers and the other half from the substantial reorganization of the school

assignment system in which many households were required to send their children to non-neighborhood schools.

B. A revealed preference approach to the history of school desegregation

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.[14] 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 local resident. Even histories that strive to collect a representative sample of popular opinion rely heavily on individual statements and self-representation rather than drawing inferences about revealed preference from choices and behavior.

In contrast to the existing histories of desegregation, this paper seeks to elicit *typical* attitudes toward school desegregation by studying the marginal homeowner or renter. Members of the marginal household may not have taken the time to express their views through political action; yet, their attitudes can be recovered by studying changes in housing prices.

Cascio, et al. (2009) take a similar revealed preference approach by studying voter preferences towards school desegregation in a large sample of southern school districts. Title I of the 1965 Elementary and Secondary Education Act authorized federal funding for K-12 education nationwide but excluded school districts that maintained segregated schools. Cascio, et al. 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

[14] 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.

to increase the black enrollment share at the average white student's school from zero to four percent. Cascio, et al. estimate that the typical southern district was willing to engage in this amount of desegregation for $1000 per pupil per year of federal funding (in 2000 dollars).

While the median voter and the marginal resident may not represent the same location in the distribution of attitudes towards desegregation, it is still instructive to compare my results with those from Cascio, et al. To facilitate this comparison, I consider the estimated effect of desegregation on the user cost of housing, a value that combines the preferences of both homeowners and renters. In my sample, a four percentage point increase in black enrollment share is associated with a 3.5 percent decline in housing prices (= -0.096 · 4.0), or a $227 reduction in annual user costs for the average housing unit (=$6,508 · 0.035). In order to convert this value into dollars per child, rather than dollars per housing unit, note that the average block had 45 housing units and 32 school-aged children (5-18 years old). Therefore, an annual savings of $227 per house translates into a $320 payment per child, which is around one-third of the federal payments required to induce the typical southern school district to begin the desegregation process. By this metric, the median southern voter appears to have been three times as resistant to school desegregation as the marginal resident in the North. While average southerners were clearly more opposed to desegregation than were average northerners, this gap is not as large as we might expect based on the case study evidence alone.

VI. Conclusion

[In progress]

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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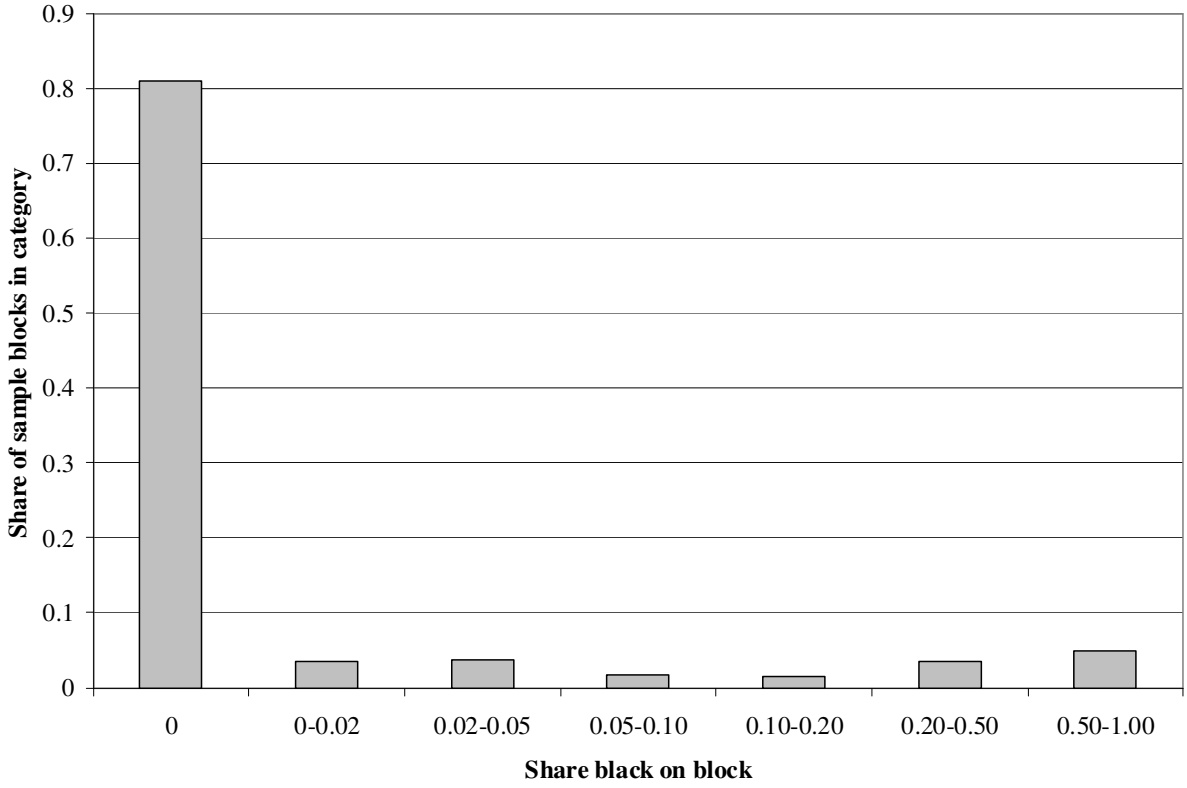
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Figure 1: Black population share on blocks adjacent to city-suburban borders, 1970



Notes: Black population share reported for Census blocks that are adjacent to one of 92 city-suburban school district borders in the data set and that have at least five owner-occupied units in 1970.

Table 1: School district borders with available block-level data by metropolitan area

Region	Metropolitan area	Full sample
Northeast	Allentown-Bethlehem, PA	2
	Boston, MA	3
	Hartford, CT	3
	New York, NY-NJ†	10
	Pittsburgh, PA	2
	Providence, RI	3
	Scranton, PA	1
	Springfield-Chicopee, MA	1
Midwest	Akron, OH	2
	Canton, OH	1
	Chicago, IL†	6
	Cleveland, OH	2
	Dayton, OH	1
	Des Moines, IA	2
	Detroit, MI	5
	Grand Rapids, MI	4
	Indianapolis, IN	1
	Kansas City, KS-MO	4
	Minneapolis/St. Paul, MN	2
	Moline-Davenport, IL-IA	2
	South Bend, IN	1
	St. Louis, MO	1
West	Denver, CO	3
	Las Vegas, NV	1
	Los Angeles, CA†	19
	Phoenix, AZ	1
	Portland, OR	1
	San Bernard.-Riverside, CA	1
	San Francisco, CA†	3
	San Jose, CA	4
	TOTAL:	**92**

Notes: Metropolitan areas marked with † contained secondary central cities in 1960 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 2: School desegregation and district-level characteristics, 1970

	ln(population)	Share black	ln(median income)	Share poverty	Share college degree
=1 if desegregated	1.341	0.066	0.012	0.007	0.019
	(0.297)	(0.040)	(0.038)	(0.010)	(0.020)
Constant	11.818	0.115	10.712	0.084	0.099
	(0.182)	(0.024)	(0.023)	(0.006)	(0.012)

Notes: The sample includes the 48 districts that constitute the "city" side of the 92 city-suburban border areas. Many city districts border on multiple suburban areas. The regressions compare the 26 cities that received a desegregation court-order to the 22 cities that did not in 1970.

Table 3: School desegregation and white exposure to black peers

Dependent variable = white exposure to black peers

RHS variable	(1)
=1 if desegregated	0.109
	(0.027)
Mean exposure, 1970	0.091
Standard deviation exposure, 1970	(0.122)
N	292

Notes: The sample includes 73 school district pairs for which there is data on exposure of white students to black peers in 1970 and 1980. The regression relates white exposure to black peers to the presence of a court-ordered desegregation plan. The regression also includes the black population share and the logarithm of distance population as well as vectors of border area-by-decade and side of the border (district-by-border area) fixed effects. Standard errors are clustered by district and reported in parentheses.

Table 4: The effect of school desegregation on neighborhood characteristics

Coefficient on =1 if under desegregation court-order

Dependent variables	Mean/Standard deviation, 1970	Coefficient, Full sample	Coefficient, < 2% black
ln(# housing units)	44.453	0.037	-0.038
	(50.216)	(0.041)	(0.042)
Share owner occupied	0.724	0.005	0.008
	(0.256)	(0.013)	(0.016)
Av. # rooms, owner occupied	5.783	-0.129	-0.116
	(0.859)	(0.078)	(0.092)
Share black	0.056	0.018	0.0002
	(0.184)	(0.015)	(0.0003)
N		4671	3553

Notes: The sample includes Census blocks that are adjacent to one of 92 city-suburban school district borders in the data set and that have at least five owner-occupied units in 1970 and 1980. The first column contains means and standard deviations of the block-level characteristics from the Censuses of Housing and Population in 1970. The second and third columns present coefficients from a regression of block characteristics on the presence of a desegregation plan in the relevant school district. The regression also includes the black population share in the district and logarithm of district population as well as vectors of border area-by-decade and side of the border (district-by-border area) fixed effects. Standard errors are clustered by district and reported in parentheses.

Table 5: The effect of school desegregation on housing prices and rents

Coefficient on =1 if under desegregation court-order

Dependent variables	Mean/Standard deviation, 1970	Coefficient, Full sample	Coefficient, < 2% black
ln(value), owner occupied	78,908	-0.051	-0.064
N = 4671; 3553	(58,345)	(0.023)	(0.025)
ln(rent), rental units	290.56	-0.073	-0.030
N = 3156; 2121	(300.03)	(0.021)	(0.023)
ln(user cost), all units	6,508.63	-0.096	-0.096
N = 5334; 3954	(2,276.51)	(0.026)	(0.027)

Notes: The sample includes Census blocks adjacent to 92 city-suburban school district borders in 1970 and 1980. Standard errors are reported in parentheses and are clustered by school district. Data on housing values (rents) are only available for blocks containing at least five owner-occupied (rental) units. The number of observations underlying each regression is reported below the dependent variable for the full sample and blocks with less than two percent black population share.

The first column contains means and standard deviations of the block-level characteristics from the Censuses of Housing in 1970. The second and third columns present coefficients from regressions of the block characteristics on the presence of a desegregation plan in the relevant school district. The regression also includes the black population share in the district and logarithm of district population as well as vectors of border area-by-decade and side of the border (district-by-border area) fixed effects. The housing value regressions also control for the average number of rooms in owner-occupied units on the block.

Table 6: Alternate specifications, Desegregation and housing values

Dependent variable = ln(value) for owner-occupied units

	Coefficient, Full sample	Coefficient, < 2% black
(1) Number of steps in court-order	-0.017	-0.022
	(0.003)	(0.003)
(2) Weight order by years since passed	-0.013	-0.013
	(0.004)	(0.003)
(3) Only borders with desegregation	-0.051	-0.057
	(0.022)	(0.026)
(4) Interaction with local school		
=1 if desegregated	-0.070	-0.113
	(0.026)	(0.021)
=1 if deseg · (black enroll share, 1970)	0.051	0.248
	(0.113)	(0.125)

Notes: The sample includes Census blocks that have at least five owner-occupied units and are adjacent to one of the 92 city-suburban school district borders in 1970 and 1980. Standard errors are reported in parentheses and are clustered by school district. The third row contains only those blocks adjacent to one of the 36 city-suburban borders in which the city district fell under court-order to desegregate in the 1970s.
All regression control for the average number of rooms in owner-occupied units on the block as well as the black population share in the district and logarithm of district population. Regressions also include vectors of border area-by-decade and side of the border (district-by-border area) fixed effects.