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School Desegregation and Urban Change: Evidence from City Boundaries[†]

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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. Desegregation of public schools in central cities reduced the demand for urban residence, leading urban housing prices and rents to decline by 6 percent relative to neighboring suburbs. Aversion to integration was due both to changes in peer composition and to student reassignment to nonneighborhood schools. The associated reduction in the urban tax base imposed a fiscal externality on remaining urban residents. (*JEL* H75, I21, I28, J15, R23, R31)

The desegregation of public schools fundamentally changed the bundle of public goods available to many central city residents. Before desegregation, the typical white student in the North and West attended a local public school with predominately white peers. In the early 1970s, the Supreme Court ruled that these nonsouthern school districts could be obligated to redress 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

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[†] To comment on this article in the online discussion forum, or to view additional materials, visit the article page at http://dx.doi.org/10.1257/app.4.1.85.

[1] Guryan (2004) and Ashenfelter, Collins, and Yoon (2006) document that cohorts of black students who attended high school after the implementation of desegregation have lower dropout rates and higher earnings later in life. Weiner, Lutz, and Ludwig (2009) and Johnson (2011) 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.

for private schooling (Reber 2005; Baum-Snow and Lutz forthcoming). I show that falling demand for urban living resulted in a 6 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 to urban residents, both psychic and real.

This paper offers the first estimate of the effect of school desegregation on housing prices in a large sample of metropolitan areas.[2] Studying housing prices offers a precise metric of the marginal resident's willingness to pay to avoid school desegregation. By 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 introduction of racially integrated classrooms and 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).

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, litigation 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, contributing 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 a large portion of residential segregation takes place between cities and suburbs, the Court established stringent conditions for extending desegregation remedies across district lines in the 1974 *Miliken v. Bradley* decision (Orfield and Eaton 1996, xxii).[5] A district was considered segregated only if the racial composition of individual schools was out of balance with the district as a whole. By this definition, an all-white district would not be 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

[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 peer effects on existing students in school districts that accept minorities as part of the 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).

[4] Fifty 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).

[5] Under the *Miliken v. Bradley* decision, suburban districts could only be included in a desegregation plan if it could be shown either that the district itself engaged in a policy of segregation or that an inter-district plan was required to correct segregation due to state-level policy.

was required to desegregate in 1970, while the city was placed 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 over the 1970s. This comparison accounts for national trends that may have reduced the demand for urban residence over this decade, 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 prior decade (1960–1970).

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 the 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.[6] It is important to note that the willingness to pay to avoid school desegregation estimated at the border may not be generalizable to the city as a whole. In particular, residents on the city-suburban border are predominately white and may have higher incomes and stronger preferences for education than the typical city resident. In this case, the border design may provide an *overestimate* of the willingness to pay to avoid school desegregation present in the city as a whole.

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 II describes the original dataset combining census blocks along school district borders with information on the timing and content of desegregation plans. In Section III, I present the main effect of desegregation on housing prices and rents, while Section IV considers alternative specifications. Section V interprets the estimates in the context of the history of school desegregation. Section VI concludes.

I. 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, we would expect housing prices in urban districts that were required to desegregate to be lower than those in 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, the difference-in-differences specification compares

[6]This border discontinuity method was pioneered by Black (1999), who studied the willingness to pay for school quality across school attendance area boundaries. See also Kane, Staiger, and Samms (2003) and Figlio and Lucas (2004). Boustan (2007) compares housing prices across city-suburban boundaries to estimate the demand for living in a wealthy town.

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 subsample of metropolitan areas in which the central city was required to desegregate in the 1970s. Pooling data from 1970 and 1980, I estimate

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

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 ($B \times T$). This interaction allows a common neighborhood trend as the border area gentrifies or deteriorates over time.[7]

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 subsample, 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 underwent desegregation). I estimate

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

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.

[7] Some school districts contribute observations to two or more border areas in the sample. For example, the north side of Chicago adjacent to Evanston, Illinois is part of one border area, while the west side of Chicago next to Oak Park, Illinois 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, including those that received court orders to desegregate over the 1970s and those that did not, and estimates

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

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 ($CITY \times T$) controls for general trends that may have reduced the demand for city residence over the 1970s.[8] 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.

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 (particularly given that all school district borders in the sample coincide with a municipal border). In other words, because 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 urban districts that fell under court order over the 1970s were larger and had a higher black population share than other urban districts at the beginning of the decade.[9] However, treated districts were indistinguishable in terms of median income, poverty rate, and the share of the population with a college degree. Therefore, the most likely sources of bias are other events 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.

II. 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

[8] The other double interactions—($PLAN \times T$) and ($PLAN \times CITY$)—are subsumed by the border area-by-time and the side of the border fixed effects, respectively.

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

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 table compares the 13 cities that received a desegregation court order during the 1970s to the 36 cities that did not. Characteristics are measured in 1970.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

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. I restrict my attention to school districts with at least 10,000 residents for reasons of data availability. I also omit southern districts for two reasons. First, school desegregation activity in the South began in the 1960s, before the US Census Bureau began publishing block-level data on all but the largest suburban areas. 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. 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. Large, fragmented metropolitan areas with populous suburbs are slightly over-represented. Los Angeles-Orange County, California and New York City, New York-New Jersey, for example, together account for one-third of the sample but only contained a quarter of the nonsouthern metropolitan population in 1970.[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 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. One drawback

[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.

TABLE 2—NUMBER OF 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, Pennsylvania	2	
	Boston, Massachusetts	3	2
	Hartford, Connecticut	2	
	New York City, New York-New Jersey†	10	1
	Pittsburgh, Pennsylvania	2	2
	Providence, Rhode Island	1	
	Scranton, Pennsylvania	1	
	Springfield-Chicopee, Massachusetts	1	1
Midwest	Akron, Ohio	2	
	Canton, Ohio	1	
	Chicago, Illinois†	5	
	Cleveland, Ohio	2	2
	Dayton, Ohio	1	1
	Des Moines, Iowa	1	
	Detroit, Michigan	5	5
	Grand Rapids, Michigan	3	
	Indianapolis, Indiana	1	
	Kansas City, Kansas-Missouri	4	2
	Minneapolis-St. Paul, Minnesota	2	2
	Moline-Davenport, Illinois-Iowa	2	
	South Bend, Indiana	1	
	St. Louis, Missouri	1	
West	Denver, Colorado	2	
	Las Vegas, Nevada	1	
	Los Angeles, California†	17	8
	Phoenix, Arizona	1	
	San Bernard-Riverside, California	1	
	San Francisco, California†	3	
	San Jose, California	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, New Jersey; Jersey City, New Jersey; and Clifton, New Jersey (New York); Gary, Indiana (Chicago); Anaheim, California (Los Angeles); and Oakland, California (San Francisco).

of the census data 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 which is available for all blocks in the sample. 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).[12]

Table 3 presents summary statistics of these housing measures for blocks on the city or suburban side of the border sample compared to the city as a whole. Blocks on either side of the city-suburban border have typically "suburban" characteristics. 63 percent of units are owner-occupied and only 8 percent of residents are black in

[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.

TABLE 3—SUMMARY STATISTICS IN BORDER SAMPLE, 1970

	Border sample	City side, border sample	Suburban side, border sample	Whole city
Average value, owned units	$107,083	104,079	110,125	88,041
$N = 2087/1050/1037$	(40,725)	(37,580)	(43,487)	(24,783)
Average rent	$549.40	544.16	554.79	522.45
$N = 1513/767/746$	(166.60)	(156.34)	(176.48)	(94.68)
Average user cost	$8,546	8,314	8,782	7,093
$N = 2529/1276/1253$	(3,424)	(3,147)	(3,671)	(2,783)
Number units per block	45.046	46.371	43.721	—
	(53.388)	(59.178)	(46.875)	
Number rooms, owner-occupied units	5.786	5.763	5.809	5.690
	(0.861)	(0.829)	(0.891)	(0.510)
Share owner occupied	0.629	0.625	0.633	0.497
	(0.310)	(0.313)	(0.307)	(0.148)
Share black	0.079	0.080	0.079	0.144
	(0.221)	(0.222)	(0.219)	(0.138)
Share 0–4 years old	0.068	0.069	0.068	0.079
	(0.046)	(0.045)	(0.047)	(0.011)
Share 5–17 years old	0.213	0.211	0.215	0.230
	(0.101)	(0.102)	(0.101)	(0.032)

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 border sample, city side and suburban, respectively. The number reported for user costs apply to the rest of the table as well. All dollar values are reported in 2000 dollars. Characteristics for the whole city (column 4) are taken from the City Databook, which reports median (rather than mean) housing values and rents. The number of rooms in the average owner-occupied unit is calculated from the 1970 IPUMS Form 1 State sample for owner-occupier households in central cities in one of the states contributing to the border sample.

the border sample, compared to an owner-occupancy rate of 50 percent and a black population share of 14 percent in the city as a whole. 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, both of which are higher than the city average.[13] 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 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. The site contains the full text of judicial decisions and enumerates each action that a district was required to undertake to counteract desegregation. In the main specification, I measure the presence

[13] Table 3 reports median (rather than mean) housing values and rents for the city as a whole using the available published census data. For comparison, I calculate mean housing values and rents for central cities in the states contributing to the border sample using the 1970 IPUMS Form 1 State sample (value = $87,100; rent = $466). The number of rooms in the average owner-occupied unit for the whole city is also taken from the IPUMS data.

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*).[14] 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 instead define measures of desegregation intensity, including the number of remedial actions required by the court order or the years elapsed since the case was decided. The median court order required two remedial steps. Steps include actions like redistricting school attendance areas, mandatory busing of students between schools, and the creation of magnet schools.

The treatment group contains 29 borders that divide a district facing a desegregation court order in the 1970s from a 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 drop borders from the control group 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

$$E_d = \left(\Sigma_{s=1,\ldots,n}[w_{sd} \times b_{sd}/t_{sd}]\right)/W_d, \tag{4}$$

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 the robustness section, 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

[14] I use the *State of Public School Integration* (2004) database because it covers both large central city and suburban districts. However, many empirical papers instead rely on Welch and Light's (1987) coding of desegregation plans. I cross-checked the dates of plan implementation across the two sources. While the sources often do not agree on the exact year of implementation, they result in identical indicators for the presence of a court-ordered desegregation plan over the 1970s except in two cases: Akron, Ohio and San Jose, California. The *State of Public School Integration* (2004) database reports that Akron (San Jose) did (did not) receive a desegregation plan in the 1970s, while Welch and Light (1987) report the opposite. I estimate the main specification using the alternate coding and find a very similar relationship between desegregation and housing prices (coeff. = −0.055; s.e. = 0.027; compare to Table 8, row 1).

Table 4—School Desegregation and White Exposure to Black Peers

Dependent variable = White exposure to black peers	Mean/SD: Placed under court order during 1970s	Mean/SD: Not placed 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 1970–1980 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

assume that students would have been assigned to their nearest public school (as the crow flies) within the relevant school district.[15] 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.

III. Results

A. *Desegregation and Exposure to Cross-Race Peers*

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 in metropolitan areas that did and did not fall under court order to desegregate over the 1970s. 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 this sample of court orders was 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 were not placed under court order during the 1970s. At the beginning 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 be placed 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 percentage points

[15] 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.

TABLE 5—SCHOOL DESEGREGATION AND RELATIVE CITY HOUSING PRICES AT THE DISTRICT BORDER, 1960–1980

Dependent variable = ln (housing value); coefficient on	Placed under court order during 1970s	Not placed under court 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 pooled regressions containing data for 1970 and 1980; coefficients are for 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 pooled 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 4,386 observations, 2,087 blocks from 1970 and 2,299 blocks from 1980. Row 4 contains census blocks adjacent to the 56 city-suburban borders with block-level data in 1960 (2,495 observations, 1,010 blocks from 1960 and 1,485 blocks from 1970).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

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 (column 1). This decline in the relative value of city housing likely reflects an aversion to school desegregation. This conclusion is bolstered by the fact that the premium for suburban housing remained steady across control borders from 1970 to 1980 (column 2).

TABLE 6—SCHOOL DESEGREGATION AND RELATIVE CITY HOUSING PRICES FOR THE DISTRICT AS A WHOLE

Dependent variable = ln(median housing value)	Placed under court order during 1970s	Not placed under court 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)
Δ 1960–1970	−0.027	−0.012	−0.015
	(0.022)	(0.013)	(0.024)

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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

The difference-in-differences estimator indicates that the suburban price premium increased by an additional 5.8 percentage points in metropolitan areas where the central city was required to desegregate over the 1970s (column 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 (1960–1970). 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.[16]

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.[17] Row 3 contains estimates of equations (1)–(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). Relative city prices declined by an additional 14.2 percentage points in cities subject to court-ordered desegregation over the 1970s, compared to

[16] Results are qualitatively similar when I restrict the sample to either the 56 borders with available block data in 1960.

[17] Recall that the US Census Bureau publishes median housing prices at the city level, while reporting mean housing prices at the block level.

TABLE 7—THE EFFECT OF DESEGREGATION ON OTHER HOUSING AND NEIGHBORHOOD OUTCOMES

	Court order in 1970s: Δ 1970–1980	No order in 1970s: Δ 1970–1980	Difference: order versus 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)
Share owner occupied	−0.014	0.002	−0.017
	(0.021)	(0.009)	(0.023)
ln (number units on block)	0.046	−0.010	0.055
	(0.055)	(0.030)	(0.064)
Mean 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 to 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

a much smaller 2.2 percent decline in cities that were not (column 2). Altogether, relative housing prices fell by 12 percentage points more over the 1970s in cities that were subject to court-ordered desegregation in that decade. As with the cross-border comparison, there is no downward trend in housing prices from 1960 to 1970 in cities that would later undergo 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 could be due to differential and unobserved trends in housing quality in the urban core relative to neighborhoods on the border. Alternatively, this gap could reflect different preferences between residents of border areas and households in other parts of the metropolitan area. If the difference in the estimates reflected different preferences, we would expect the estimate obtained at the border to be *larger* than the estimate covering the entire city because residents at the border have characteristics associated with aversion to desegregation (for example, they are more likely to be white and have higher incomes than the typical city resident). In contrast, I find that the estimate for the full district is larger than the estimate at the border, which is more consistent with the presence of omitted trends in housing quality in the full sample that are minimized in the close cross-border comparison.

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 where the 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. Desegregation had a slightly smaller effect on rents than on values, although, given the standard errors, I cannot reject that the two estimates are the same. Over the 1970s, relative city rents fell by 4.0 percentage points more along treated than along control borders. The effect of desegregation on owner-occupied and rental housing could differ for two reasons. First, renters tend to be younger, less well-off, and less likely to have children, characteristics that may lead renters 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.

Due to data restrictions, only a subset of sample blocks have available data on either housing values or rental rates. 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.[18] 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. Perhaps because desegregation reduced both housing values and rents, 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 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. The number of rooms in suburban housing units increased by 0.17 of a room relative to the neighboring city in areas under court order to desegregate, as compared to other cities. If all 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 resorting 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.

[18] 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.

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 share 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Despite the potential for resorting 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 declines in the presence of school-aged children over the 1970s, the magnitude of which was unrelated to the presence of a court order.[19]

IV. 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

[19] Although the available measures do not reveal evidence of sorting on observable characteristics, there could be sorting on unobservable characteristics, such as attitudes toward education, that are associated with the aversion to school desegregation. The lack of sorting on household composition is consistent with Baum-Snow and Lutz's (forthcoming) finding that, outside the South, urban residents were more likely to respond to desegregation by shifting to private schools rather than by leaving the city.

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. A natural candidate 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. Reassuringly, adding this measure of riot activity has no effect on magnitude or precision of desegregation estimate.[20] I also find no main 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 epicenters of violence were far from suburban borders.

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. Adding a control for the local racial composition has no effect on the estimated effect of desegregation. However, controlling for the average number of rooms per unit reduces the estimated effect of desegregation on housing prices from 5.8 to 4.0 percent.[21] The gross effect of desegregation on housing prices (5.8 percent) combines the net effect of desegregation on housing prices for a given housing stock (4.0 percent) with the effect of desegregation on home maintenance. Comparing the gross and net effects implies that around 30 percent of the total relationship between desegregation and housing prices can be explained by differential changes in home renovation ($= [5.8 - 4.0]/5.8$). I view the differential incentives for home renovations across borders as an endogenous effect of the policy and treat the number of rooms in the average housing unit as an outcome variable in its own right accordingly in Table 7. However, some readers may prefer [to interpret] the net effect of desegregation on housing prices because it controls for an observed difference[s] in housing quality.

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 (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 each court order. Remedies include actions like rezoning school attendance areas, transferring students between schools, busing students between schools, or creating a magnet

[20] 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.

[21] The average number of rooms per unit is the only measure of housing quality provided at the block level in census data. It is possible that adding other housing quality indicators could reduce the net effect of desegregation on housing prices further.

TABLE 9—HETEROGENEOUS RESPONSE TO SCHOOL DESEGREGATION: INTERACTION WITH 1970 BLACK ENROLLMENT SHARE AT NEAREST HIGH SCHOOL

Dependent variable = ln(housing value)	No interaction	Local interaction (black share)	
		Nearest high school	District minus nearest high school
PLAN × *CITY* × 1980	−0.084***	−0.132***	−0.068**
	(0.027)	(0.025)	(0.031)
PLAN × *CITY* × 1980 × share black		0.336**	−0.529***
		(0.127)	(0.155)

Notes: Standard errors are reported in parentheses and clustered by school district. The sample includes 2,815 census blocks that are adjacent to a city-suburban district border, have fewer than 2 percent black residents in 1970 and 1980, and have available black enrollment share data in 1970 on both sides. 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

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 estimated 5.8 percent decline in housing values is reached around five years after the plan is first announced.

Households living in school attendance areas in which 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 $[(PLAN \times CITY \times T)]$ 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. I conduct this specification on the subsample of predominately white blocks (98 percent white or more).[22] The first

[22] The local black enrollment share is highly correlated with the residential black population share and so, without this sample restriction, I would be comparing the preferences of white households attending all-white high schools with a diverse set of households attending integrated high schools.

column re-estimates equation (3) for this subsample. The typical desegregation plan reduced housing prices by 8.4 percent.

The second column adds an interaction between the presence of a desegregation plan and the black enrollment share in the nearest high school in 1970. Desegregation reduces housing values by 13.2 percent in areas of the city that otherwise would have attended an all-white high school. As the initial black enrollment share of the local high school increases, the estimated effect of desegregation on housing values declines. According to these estimates, desegregation would have had no effect on housing values in areas that were otherwise assigned to a high school with a 40 percent black enrollment share in 1970.[23] Column 3 instead interacts the presence of a desegregation plan with the difference between the district-wide and local black enrollment shares. The larger the difference between the local high school and the district as a whole, the greater the local increase in black student enrollment necessary to be in compliance with the court order. Desegregation reduces housing values by 6.8 percent in areas of the city in which the local high school already matched the demographics of the district as a whole. Housing values fall by an additional 5.3 percent in areas in which the local high school's black enrollment share is 10 percentage points lower than the district as a whole.

V. 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 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*

Desegregation plans reduced housing values and rents in urban school districts, thereby shrinking the residential tax base relative to neighboring suburbs. The average school district in the sample raised $2,100 per pupil in local property taxes in 1970 (in 2000 dollars). At the suburban border, the user cost of urban housing fell by 9.2 percent with the implementation of a desegregation plan. For this calculation, I assume that this estimate applies to all white neighborhoods in the central city. Under various assumptions about the effect of desegregation on housing values in black neighborhoods, my estimates suggest that desegregation would have reduced

[23] The implied effect on housing prices on a block assigned to a high school that was 40 percent black in 1970 is zero ($= -0.132 + [0.336 \times 0.4]$).

TABLE 10—DESEGREGATION AND SCHOOL REVENUES AND EXPENDITURES
COEFFICIENT ON *PLAN* × *CITY* × 1980

Dependent variables	Mean/SD	Coefficient
Federal revenue per pupil	402.24	360.80**
	(350.51)	(183.61)
State revenue per pupil	2,037.83	428.54
	(1,037.24)	(487.09)
Local tax revenue per pupil	2,134.55	48.96
	(1,409.58)	(514.28)
Instruction expenditure per pupil	3,010.18	160.25
	(669.22)	(174.28)

Notes: Standard errors are reported in parentheses and clustered by school district. Column 2 reports coefficients from district-level regressions of revenues or expenditures per pupil on the interaction between being a central city district in 1980 and being subject to a court-ordered desegregation plan over the 1970s (see equation 3). Revenue and expenditures data are taken from the Elementary and Secondary General Information System (ELSEGIS) for the 1969–1970 and 1979–1980 school years.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

the residential tax base by 6.2 to 7.7 percent.[24] If city districts had maintained a fixed property tax rate over this period, this housing price decline would have translated into a $130 to $162 reduction in (rate-neutral) revenue per pupil.[25]

The full effect of desegregation on available resources per pupil depends not only on the local tax base but also on potential changes in the local tax rate and in the level of state and federal transfers. Table 10 presents results from regressions that, following the form of equation (3), relate changes in revenues per pupil by funding source to the arrival of a desegregation plan over the 1970s. Despite their shrinking residential tax base, city districts that were placed under court order did not experience a decline in locally raised revenue per pupil over the 1970s, relative to their neighboring suburbs. This pattern suggests that localities responded to the loss of revenue by raising the property tax rate. As a result, the costs of desegregation would have been borne broadly by property owners and renters, rather than only by households with school-aged children.

Overall, districts placed under court order experienced an *increase* in school resources over the 1970s, primarily due to $360 in additional funding per pupil (in 2000 dollars) from the federal government. However, these extra federal resources may not have kept pace with new expenditures associated with the implementation of a desegregation plan, such as additional buses or higher teacher salaries. Furthermore, few, if any, of these additional dollars were allocated to instructional

[24] Eighty-four percent of census tracts in the median sample city were predominately white (defined here as less than two percent black). If housing values were unchanged in black neighborhoods following desegregation, the residential tax base would have declined by 7.7 percent ($= 0.16 \times 0.000 + 0.84 \times -0.092$). 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 6.2 percent ($= 0.16 \times 0.092 + 0.84 \times -0.092$). This calculation uses the user cost of housing estimates from Table 7, row 2.

[25] Note that 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.

spending. The point estimate relating desegregation to instructional expenditures is $160, but is not statistically different from zero. In other words, the majority of these funds were allocated to noninstructional purposes, including administrative and transportation costs. The fact that housing prices fell despite this infusion of federal funds implies that these transfers did not fully compensate urban residents for the costs of desegregation, either psychic or real.

B. *Cross-Race Peers and Neighborhood Schools*

Objections to school desegregation can be rooted in fears about cross-race classrooms and related worries about peer quality, but may also reflect concerns about the loss of neighborhood schools. In order to comply with desegregation orders, school districts could no longer place all students in the nearest school, but rather needed to assign some white students to distant schools in black neighborhoods and vice versa.

Kane, Riegg, and Staiger (2006) estimate the willingness to pay for a school with a lower black enrollment share by comparing housing prices across attendance area boundaries in Charlotte-Mecklenberg, North Carolina, while controlling for distance to school. According to their estimate, housing prices would decline by 3.8 percent following the 14.5 point increase in black enrollment associated with the typical desegregation plan.[26] By this measure, two-thirds of the estimated housing price response to school desegregation at the city border can be attributed to concerns about mixed-race classrooms and the associated change in peer quality ($= 3.8/5.8$). The remainder of the estimated price response is likely due to concerns about school reassignment. Bogart and Cromwell (2000) find that assignment to a nonneighborhood 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 reassignment ($= [5.8 - 3.8]/7.5$), 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.[27] These views—whether those of angry segregationists who gathered to block the desegregation of Central High in Little Rock, Arkansas or those of crusading integrationists who marched in Selma, Alabama—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

[26] Kane, Riegg, and Staiger (2006) 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 3) would lead to a 3.8 percent decline in housing prices.

[27] A nonexhaustive list of the vast historical literature on responses to desegregation includes Carter 1995; Lassiter and Lewis 1998; Webb 2005; Sokol 2006, and Crespino 2009.

federal funding for K–12 education nationwide. In order to be eligible for funding, school districts had to integrate their schools (at least nominally). 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 4 percentage points. Cascio et al. (2010) estimate that the typical southern district was willing to engage in this amount of desegregation for $1,000 per pupil per year of federal funding (in 2000 dollars).

To compare my results with Cascio et al. (2010), I convert percentage changes in housing prices into dollars per pupil. By my estimate, the 4 percentage point increase in black enrollment share required by Title I is associated with a 2.6 percent decline in the user cost of housing, or a $222 reduction in annual user costs for the average housing unit ($= \$8{,}546 \times 0.026$).[28] Converting this value into dollars per child yields an annual payment of $399 per child, equivalent to 40 percent of the estimated federal payments required to induce the typical southern school district to begin the desegregation process.[29] By this metric, the median southern voter was 2.5 times as resistant to school desegregation than was the marginal northern resident. Although this gap is not as large as we might have predicted based on the case study evidence alone, it still reveals substantial differences in attitudes towards integration across region.

Of course, this regional comparison depends on the strong assumption that the preferences of the median voter are similar to those of the marginal resident whose behavior determines the equilibrium housing price. This assumption could fail for two reasons. First, preferences of the *marginal* resident may differ from those of the *median* resident. Bayer, Ferreira, and McMillan (2007) argue that this divergence is most likely when the good in question is in short supply (for example, houses with a view of the Golden Gate bridge); however, housing units free from the obligation to desegregate were abundant in the rapidly expanding suburbs. Secondly, the median *voter* need not reflect the preferences of the median *resident* if individuals who feel passionately about school segregation are most likely to participate in school board elections. This turnout effect may overstate the aversion to desegregation in the South.[30]

VI. 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. However, the 1974

[28] The typical plan in my sample increased black enrollment share by 14.5 percent (Table 4, row 3) and reduced user costs by 9.2 percent (Table 7, row 2). 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 relevant metric for this calculation because it combines the preferences of homeowners and renters.

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

[30] Note also that the Cascio et al. (2010) paper identifies the effect of federal funding on desegregation by comparing districts with different characteristics. The marginal district in their analysis will be a richer district in a less generous state (such as Louisiana) whose residents may have been more opposed to integration than the southern average.

Miliken v. Bradley decision established strict limits on the use of inter-district plans, effectively excluding the suburbs from most court-ordered desegregation initiatives.

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 nonneighborhood 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 in cities under court order fell by 6 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. (2010), for instance, estimate that the typical southern district would have engaged in a token amount of desegregation for a payment of $1,000 per pupil (in 2000 dollars). By this measure, northern residents appear to be 2.5 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:

- 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 online resources. In some cases, road names had changed from 1970 to 2000 and could be edited by hand; in others, schools appear to have closed in the intervening three decades.

- 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.

- 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 case, I assign the average racial composition of the two closest high schools to that area.

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