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ESCAPE FROM THE CITY? THE ROLE OF RACE, INCOME, AND LOCAL PUBLIC GOODS IN POST-WAR SUBURBANIZATION

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ABSTRACT

Suburbs allow for sorting across towns, increasing inequality in resources for education and other local public goods. This paper demonstrates that postwar suburbanization was, in part, a flight from the declining income and changing racial composition of city residents. I estimate the marginal willingness to pay for town-level demographics -- holding neighborhood composition constant -- by comparing prices for housing units on either side of city-suburban borders (1960-1980). A one standard deviation increase in residents' median income was associated with a 3.5 percent housing price increase. Homeowners value the fiscal subsidy associated with a higher tax base, and the fiscal isolation from social problems (for example, spending on police). In addition, white households avoided racially diverse jurisdictions, particularly those that experienced rioting or underwent school desegregation.

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I. Introduction

In the decades following World War II, Americans moved *en masse* to the suburban ring.[1] White households were more likely to move than black households and the rich were more likely to relocate than the poor.[2] Suburbanization led to a resorting of the population across towns that, while potentially efficient (Tiebout, 1957), generated inequities in resources for education and other public goods (Benabou, 1996).

Was this resorting an accidental consequence of the diffusion of the car and federal road building programs which encouraged households to move to the urban periphery (Baum-Snow, 2007)?[3] Or, was suburbanization in part a deliberate escape from the declining income and changing racial balance in the center city, a pattern that historians and sociologists often call "white flight" (Jackson, 1985; Sugrue, 1996; Meyer, 2000)?[4]

Distinguishing between these two possibilities is important. If household mobility is a function of the demographic composition of the central city, suburbanization can enter a vicious cycle. Baumol (1967) first noted this possibility in the late 1960s. The initial departure of the middle class, he argued, encouraged others to follow suit, leading to a downward spiral of population loss and urban decline. Was Baumol right?

Broadly speaking, changing demographics in the center city might encourage residents to relocate to the suburbs for three reasons: changes in the composition of peers in public schools,

[1] Between 1940 and 1980, the share of metropolitan area residents who lived outside the central city increased from 38.2 to 57.8 percent (Heim, 2000, p. 144).
[2] Only 9.8 percent of families earning under $13,500 ($2000) who lived in a central city in 1965 had moved to a suburb by 1970, compared to 17.7 percent of families earning over $110,000 (US Bureau of Census, 1970).
[3] In classic land use theory, the rich will be more likely to move to the suburbs if they had a comparative advantage in automobile commuting (Leroy and Sonstelie, 1983; Margo, 1992; Glaeser, Kahn and Rappaport, 2000).
[4] The black population share was 3.5 percent in the suburbs and 8.3 percent in central cities in 1940. By 1980, these shares had diverged further to 5.1 and 19.2 percent respectively. In 1940, the average male suburban resident earned 7.8 percent more than the average male in a central city (unadjusted for any differences in characteristics). By 1980, this gap had doubled to 16.3 percent. Author's calculations from the IPUMS samples (Ruggles and Sobek, 2003).

the local electorate, and the property tax base.[5] I study the demand for suburban political autonomy – which includes the power to administer schools, as well as to tax property and spend on local public goods – through an analysis of housing prices.[6] If the median homebuyer prefers living in a town with wealthy and/or predominately white co-residents, she will pay a premium for such a housing unit. To isolate the role of local policy, I compare neighboring housing units that fall on opposite sides of jurisdiction boundaries. Given the proximity of the houses in question, price difference are unlikely to reflect other benefits of suburban living. For instance, units are located at a similar distance from employment centers, are equally well served by new road construction, and are situated in the same general neighborhood.

This methodology applies the concept of a regression discontinuity to the spatial dimension (Black, 1999). The necessary identifying assumption in a single cross-section is that, while local policy changes discretely at jurisdiction borders, neighborhood and housing quality shift more continuously. However, abrupt differences in housing quality may emerge at borders due to local zoning regulations or the endogenous sorting of households. To control for any fixed disparities in the housing stock, I also examine the effect of relative *changes* in jurisdiction-level attributes on changes in the cross-border housing price gap.[7]

I focus on 1960 to 1980, a period of peak suburbanization. My sample consists of 82 borders representing 32 metropolitan areas, the full universe of (non-southern) borders for which

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[5] See Alesina, Baqir and Easterly (1998) and Cutler, Elmendorf and Zeckhauser (1993) on the relationship between demographic compostion and local public goods in US cities and counties.

[6] Much of the existing literature on white flight examines whether suburbanization levels are higher in areas with poorer central cities, a correlation that is difficult to interpret (see, for example, Bradford and Kelejian, 1973; Frey, 1979; and Adams, et al., 1996). The departure of the rich for any reason will have a direct, mechanical effect on the income levels and racial composition of remaining urban residents. Boustan (2006) uses changes in southern agriculture by state, coupled with persistent southern-state-to-northern-city migration patterns, to instrument for changes in urban racial composition.

[7] Kane, Staiger and Samms (2003) have questioned the comparability of neighborhood quality across political boundaries. Figlio and Lucas (2004) examine changes in housing prices as new information on school accountability is revealed.

block level data is available on both sides in these years.[8] Housing values and rents are drawn from block-level statistics reported by the Census of Housing. Blocks are coded by hand according to distance from the political border.

I find that the median homebuyer is willing to pay to live with richer co-residents in every decade. A one standard deviation increase in resident's median income is associated with a 2.0-3.5 percent increase in housing values in the cross-section and a 6.9 percent increase in the panel context. The demand for rich co-residents can be explained with a limited set of policy measures. Property tax rates are lower in rich towns which enjoy a higher tax base. This fiscal subsidy is capitalized into housing prices. Homeowners also like the fact that suburbs allocate less money per resident toward public safety and sanitation. However, rich and poor towns exhibit no difference in education spending per pupil; the shortfall in locally-raised revenue in poor towns is made up by state and federal transfers. School quality is unobserved during this period, but may play an additional role.

In contrast, a city's racial composition has no net effect on its housing prices relative to its suburban neighbors. Instead, black households sort into diverse towns. However, I find considerable heterogeneity in the willingness to pay for racial homogeneity, driven by events like riots and school desegregation. In 1970, the median homebuyer was keen to avoid cities with a large black population share if they had recently experienced a severe riot. Border areas were, on the whole, far from direct riot-related property damage and so this price may reflect concerns with a city's changing political balance. By 1980, many of the cities in the sample had been placed under court supervision to desegregate their schools. Households are willing to pay to avoid this process, particularly the active steps of busing and student re-assignment, which

[8] Ideally, I would observe housing prices along these borders in 1950 as well. Such data is only available for 20 of the sample borders.

reduce home values along sample borders by 8.4 percent. This finding reinforces evidence that desegregation reduced property values in Atlanta (Clotfelter, 1975) and led to falling white enrollment in urban districts nationwide (Reber, 2005; Lutz, 2005).

A large literature addresses the relationship between neighborhood racial composition and household mobility, with the most recent contribution being Card, Mas, and Rothstein (2007). While not the focus here, if residents also responded to neighborhood change, these estimates will be a lower bound of total "white flight." However, given the level of racial segregation by neighborhood *within* central cities, avoiding black neighbors did not require a suburban address.[9]

The rest of the paper is organized as follows. Section II discusses the conditions under which we would expect a price gap to emerge at the border and describes the research design. Section III outlines the panel sample of jurisdictional borders. In section IV, I test the maintained assumption of continuity in housing stock across municipal borders, and present the basic relationship between housing prices and town-level characteristics. Section V considers local policies that may account for the consistent demand for well-to-do residents, including spending priorities and property tax rates. Section VI examines two possible shocks to the cost of racial diversity – riots and desegregation. Section VII concludes.

II. Using Housing Prices to Analyze the Demand for Suburban Residence

Housing units embody a set of characteristics – attributes of the unit itself, of the neighborhood, and of the jurisdiction in which the unit is located – each of which commands a separate price (Kain and Quigley, 1975). In theory, one can isolate these prices and, by so doing,

[9] In 1960, after 20 years of African-American rural-to-urban migration, 55.8 percent of Census tracts in central cities were at least 99 percent white, declining from only 60.3 percent in 1940 (Cutler, Glaeser and Vigdor, 1999).

gain insight into the demand for a variety of non-market goods that are implicitly traded through the housing market. This technique follows from Rosen's (1974) seminal work on hedonic pricing. Recent examples include Black's (1999) analysis of the quality of elementary education and Davis's (2004) and Chay and Greenstone's (2005) examination of local environmental hazards.

Is the income profile and racial composition of the town in which a housing unit is located one of the features for which households are willing to pay? Answering this question depends crucially on one's assumption about the elasticity of housing supply. Some models of local public goods provision predict that households will sort by income level due to common preferences over the tradeoff between private consumption and public goods (Ellickson, 1971; Epple and Romer, 1991; Fernandez and Rogerson, 1996). In this case, as long as the housing supply is sufficiently elastic, we would not expect a price gap to emerge at the border of rich and poor towns. Rather, wealthy residents will locate in the suburbs and the suburban housing supply will expand to meet growing demand.

If housing supply is less than perfectly elastic, some of the demand for the wealthy town will lead to higher housing prices rather than an expansion of the housing stock. But, is there any reason to believe that suburban housing supply was restricted in the post War period? At first glance, the answer appears to be "no." The suburbs were undergoing an unprecedented construction boom to accommodate growing demand. However, most of this new construction occurred on the suburban periphery. A house on the periphery is not equivalent to one just over the border with the city. In a city with a central business district, the first house will entail a much longer commute than the second. As long as the supply of houses on the inner ring are restricted, a price gap could emerge at the border.

Alternatively, models that incorporate property taxation recognize that, under some conditions, everyone prefers living in a rich town (Buchanan and Goetz, 1972; Hamilton, 1976). Wealthy jurisdictions have a large tax base, and can thus raise a given amount of revenue with a lower tax *rate*. Owners of a mid-sized house in a rich jurisdiction will receive a subsidy from their richer co-residents, while owners of the same house in a poor jurisdiction will be cross-subsidizing their poorer co-residents. The fiscal subsidy one would receive by moving to the richer town should be capitalized into housing prices.

A. An Econometric Framework

The price gap for housing units on either side of jurisdiction borders provides a useful window into the demand for characteristics of the local electorate. Housing values or rents may be a function of either the black population share or the median income of the jurisdiction in which the unit is located. For illustration, I consider here the black population share in a jurisdiction (%black). Starting with data from a single time period (1960, 1970 or 1980), I estimate:

$$\ln(\text{price}_{ijb}) = \alpha + \beta\ \%\text{black}_j + \Phi'\text{block}_i + \Theta'Z_b + \varepsilon_{ijb} \qquad (1)$$

where i and j index blocks and political jurisdictions, respectively, and b is a subscript common to both sides of a "border area." A border area consists of a pair of jurisdictions, one of which is usually a city and the other a suburb. Throughout the paper, I occasionally refer to jurisdictions as "towns." To clarify geographic terms further, Figure 1 presents a schematic illustration of two border areas in the Chicago metropolitan area. The first border area divides Chicago from Evanston, IL, and the second divides Chicago and Oak Park, IL. Depicted at each border is a

Census tract pair (in reality, border areas often consist of more than one such pair), and nested within each tract is a grid of blocks. All blocks in the city of Chicago are coded as being in the same jurisdiction ($j = 1$), whereas blocks in Evanston and Oak Park are located in distinct jurisdictions ($j = 2$; $j = 3$). Adjacent blocks from a jurisdiction pair are assigned to the same "border area." In the figure, the Chicago/Evanston border is coded as $b = 1$, and the Chicago/Oak Park border is $b = 2$.

The estimating equation contains a vector of border area dummy variables (Z_b), which take on a value of one for all blocks in a jurisdiction pair. Z_b captures unobserved characteristics that are shared by houses on both sides of the border – for example, the presence of a nearby park, a bus line, or a commercial strip. With the inclusion of Z_b, the remaining coefficients are estimated only from variation *within* border areas. Conceptually, this approach relates mean difference in housing prices across borders to differences in jurisdiction-level attributes. Standard errors are clustered at the jurisdiction level.

While the border dummies should pick up variation in neighborhood quality that is common to both sides of the border, some specifications also add a series of block-level characteristics ($block_i$). These include the average number of rooms in the block's housing units, the share of units that are owner-occupied or single family structures, and the share of residents on the block who are black.[10] Due to confidentiality concerns, published housing prices (rents) are available only for blocks containing five or more owner-occupied (rental) units; estimation is conducted on these sub-samples. Appendix Table 1 presents means and standard deviations of block and jurisdiction level variables in each year.

[10] Other measures include an indicator for the presence of group quarters (for example, college dormitories or retirement homes) and the density of block settlement, measured as the number of residents per unit. In addition to the share of residents who are black, the 1980 data also includes the share of residents who are Asian or Hispanic.

III. Collecting Housing Prices Along Jurisdictional Borders

This approach requires historical information on housing prices for small geographic units. For this, I rely on published block-level means of housing values/rents from the Census of Housing.[11] By inspecting urban area maps, I create two samples of jurisdiction borders, a balanced panel that contains all borders with available block-level data in 1960 and a full sample that adds borders for which data becomes available in 1970.[12] The panel sample contains 57 borders from 1960-80, while the full sample contains 82 borders in 1970 and 1980 alone. In order to have access to data on local policy, I restrict my attention to towns with at least 10,000 residents.

The 57 borders in the panel sample are taken from 16 metropolitan areas, four in the Northeast, eight in the Midwest and four in the West. Using a combination of Census block maps and historical US Geological Survey 1:24,000 maps, I first rule out seven borders that were obstructed by a railroad, four-lane highway, body of water, or large tract of industrial land.[13] Because southern cities had fewer large, long-established suburbs, only one southern border (Atlanta-East Point, GA) meets this criterion. I drop Atlanta from the sample because the

[11] In the Census, housing values and rents are based on 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.
[12] By 1960, the Census Bureau had divided every city with more than 50,000 residents, as well as a subset of their largest suburbs, into blocks. Ten years later, all suburbs were fully overlaid with Census blocks. Pittsburgh, the one exception, was fully blocked by 1960.
[13] Ruling out obstructed borders improves the plausibility of the identifying assumption. However, it also raises the question of endogenous border formation. Municipalities can erect bulwarks against unwanted populations by zoning for industrial use along their borders or constructing large roadways with limited ability for pedestrian crossing. Cicero, IL is (in)famous for its ethnic and racial exclusivity (Keating, 1988). It may be no coincidence, then, that the Chicago/Cicero border is obstructed by industrial land. As a result, the selection of borders into the sample will favor jurisdictions that are the *least* hostile to new arrivals, thus working against finding a housing price decline at the border.

mobility response to jurisdiction attributes, particularly black in-migration, likely differed by region.[14]

The first panel of Table 1 lists each metropolitan area in the balanced panel by region along with the number of borders that each contributes to the sample. Cities vary enormously in size, from the nation's largest (New York City, Chicago and Los Angeles) to small, regional cities like Moline-Davenport, IL-IN. New York City and Los Angeles alone account for 27 borders. Their over-representation is not due to their size alone.[15] Both the New York City and Los Angeles regions were highly fragmented and contained multiple central cities (for example, Newark, NJ; Anaheim, CA), thus increasing their probability of inclusion.[16] The sample is not representative of all suburban areas, but instead is tilted toward the largest suburbs and contains only those suburbs that share a border with the central city.

The full sample adds data from 16 additional metropolitan areas in 1970 and 1980. The central cities in these areas tend to be smaller than those in the panel sample. The full sample includes small regional cities like Hartford, CT; college towns like Madison, WI; and new sunbelt cities, like Las Vegas, NV and Phoenix, AZ.

IV. Results: Willingness to Pay for Town-Level Demographics

A. Testing for Differences in Observed Housing Attributes Across Borders

In order for this approach to be valid, it must be the case that the housing stock is of equal quality on both sides of sample borders. The Census of Housing collects a limited set of

[14] Contrary to the rest of the country, in southern cities there is no relationship between changes in urban black population share and white suburbanization (Boustan, 2006). White flight may have been muted in the South because of black disenfranchisement and the presence of racially-segregated school systems.
[15] New York City and Los Angeles contribute nearly 50 percent of the borders in the sample, while, in 1960, they contained only 20 percent of the population living in the top 25 cities.
[16] Indeed, in 1970, the Census Bureau subdivided the New York City SMSA into four parts (New York City, NY; Jersey City, NJ; Newark, NJ; and Clifton-Paterson-Passaic, NJ) and split the Los Angeles SMSA in two (Los Angeles-Long Beach and Anaheim-Santa Ana-Garden Grove).

information about the housing stock. I test this assumption by examining whether there are any observable differences in housing quality on opposite sides of boundaries. Results for the owner-occupied sub-sample are presented in Table 2.[17]

Somewhat surprisingly, a greater share of houses in racially diverse jurisdictions are single-family, owner-occupied units, though these differences are economically small and are not statistically significant.[18] Blocks in diverse jurisdictions also have fewer residents and fewer units per block (lower density). That the urban housing stock is no worse along these dimensions is *prima facie* evidence against the reach of zoning, which tends to restrict multi-family use and high-density development. However, units in diverse jurisdictions are slightly smaller than their cross-border counterparts. Estimates imply that the average unit in an all-black jurisdiction has 0.42 fewer rooms than in a neighboring all-white town. While there are statistically detectable quality differences across borders in 1970, these differences are diminished when considering the change from 1970 to 1980. This is particularly true for the least mutable characteristics like the share of units that are detached, single family structures.

While it is unlikely that a decade is long enough to witness an evolution in the housing stock, there is enough time between Censuses for substantial mobility and re-sorting of the population to occur. The second panel of Table 2 considers the only demographic measure available at the block level in all years: the share of units occupied by a black household head. Blocks in a diverse jurisdiction are more likely to have black residents, though this relationship diminishes as one approaches the border. Moving from a jurisdiction with no blacks to one that is 100 percent black (a comparison that is, of course, out of sample) increases the probability of having a black neighbor by 31 percentage points for residents who live within six blocks of the

[17] Housing stock differences for the full set of blocks are always smaller than those shown here.
[18] Note that the same patterns hold for poor jurisdictions, though I report only the racial composition results in the text.

border. This difference falls to 9 percentage points for residents who live within a block of border, but it remains statistically significant.

Is this difference driven by a few areas that are undergoing racial transition? I define 12 of the 82 borders in the sample as “transition areas.”[19] Only 66.6 percent of residents in transition areas are white, compared to 99.5 percent of residents along the other 70 borders. The relationship between jurisdiction-level and local demographics holds even in non-transition areas. Residents of non-transition areas would increase their probability of having a black neighbor by 2.9 percentage points by moving to the city side of the border (s.e. = 1.3). This relationship is not erased in a panel context; jurisdictions that become more racially diverse over the 1970s also attract more black residents.

If homeowners are willing to pay to avoid black neighbors, above and beyond black co-residents, endogenous sorting could bias estimates of town characteristics upward. I show below that the jurisdiction-level coefficients are robust to controlling for the block-level black share and to limiting the sample to the non-transition areas or to blocks with no black residents at all. In fact, residents on all-white blocks are willing to pay *more* than the median homeowner to live in a predominately white jurisdiction, while the opposite is true for blocks with at least one black resident. If the prevailing preference is for living in an own-race jurisdiction, we would expect blacks to outbid whites for houses on the diverse side of borders, which may explain the local sorting found here.

[19] Transition areas are defined as any border areas for which 10 percent or more of the residents are black. These include, for example, Compton-Long Beach, CA; Inglewood-Los Angeles, CA, and St. Louis-University City, MO.

B. Housing Prices and Jurisdiction-level Demographics

(i) Graphical Analysis

With these caveats in mind, I turn in this section to the analysis of housing prices. I begin in Figure 2 with a simple graphical exercise conducted here for 1970. For each border pair, I classify one jurisdiction as being more desirable than its neighbor due either to having a lower black population share (Panel A) or a higher median income (Panel B). The figure plots the mean housing values in log points by distance from the jurisdictional border; all values are *relative* to the first tier of blocks on "desirable" side. Housing prices are regression-adjusted for the full set of block-level characteristics listed in Table 2.

In Panel A, which classifies towns based on racial composition, housing prices fall by 3.4 percent when moving from the homogenous suburb into the diverse city. While there is some evidence of a price gradient as one moves further into the diverse town, which may reflect a slow decline in quality, the gap at the border is uniquely large and is the only pair comparison that is statistically different from zero. A discontinuity of this nature implies that the cross-border price gap cannot be fully explained by gradual improvements in unobserved housing quality through space. Panel B conducts the same exercise for median income. The picture looks similar; indeed, many of the same towns are likely to be coded as "desirable" under either rubric.

Rather than condensing jurisdiction differences into a single binary (diverse/not), Figure 3 uses all of the cross-border variation to explore the relationship between housing prices and jurisdiction characteristics. The horizontal axis indicates the difference between the neighboring towns in black population share or median income and the vertical axis portrays the associated gap in housing prices in log points. Long Beach and Compton, CA exhibit the largest cross-border gap in black population share (66 percentage points). Hartford and the suburb of West Hartford, CT share the widest gap in median income (54 percent). The biggest difference in housing prices occurs at the Cambridge-Somerville border; owner-occupied housing values increase by 126 percent as one leaves Somerville and enters Cambridge, MA.

The downward sloping regression-line through each scatter plot implies that the greater the demographic or socio-economic gap at the border, the greater the difference in cross-border housing values. By this estimate, doubling the median income of a neighboring town would be associated with a 17 percent increase in housing values. It is important to note that, in these graphs, each border area contributes one observation, despite the fact that the price gaps are calculated from very different numbers of housing units. The Cambridge-Somerville outlier, for instance, is based on only 28 owner-occupied housing units, while the median number of units along a border is 376. The regressions will therefore apply different weighting schemes to account for the variation in underlying information from which the border area means are calculated.

(ii) Cross-section Regressions: 1960-1980

Table 3 contains results from the cross-sectional specification, estimated separately in 1960, 1970 and 1980. The housing market is divided by tenure status, with either housing values or rents measuring the willingness to pay for jurisdiction-level characteristics. I begin by presenting results from the panel sample in each year. In 1970 and 1980, I also report regressions using the full sample.

The value regressions for owner-occupied housing are presented in the first two columns. I start in the first row of each panel by including only a single jurisdiction-level characteristic and a series of border area dummy variables. The coefficients imply that the median resident is willing to pay between 16-25 percent more for a house in a town that is 100 percent white compared to a town that is 100 percent black. Alternatively, doubling the median income of a town's residents leads to a 23-40 percent increase in housing prices. Both of these experiments are, of course, out of sample. The average border in 1970 divides two towns that have a 13 percentage point gap in black population share and 16 percent gap in median income. A standard deviation increase in black population share or decrease in median income would lead to a 1.3-3.5 percent drop in housing prices.

The willingness to pay for racially homogeneous co-residents is not robust to the inclusion of block characteristics. Adding a detailed racial and ethnic breakdown (share black, Asian, Hispanic) in 1980 cuts the coefficient on jurisdiction-level black population share in half (row 2). The full set of housing quality characteristics, which are added in the third row, further reduces the coefficient on share black, and the point estimate is no longer significant in 1960 and 1980.

Including block-level housing characteristics, particularly the mean number of rooms, also reduces the coefficients in the median income regressions but the relationship remains significant and large. The median household is willing to pay 12-21 percent more for an identical unit located in a town whose residents are twice as rich. Moving from the panel sample to the full sample does not qualitatively alter the conclusion (row 4).

Because race and income are correlated, both black population share and median income may be measuring the same underlying feature of a town's residents. When both are included, the coefficient on black population share falls to zero in 1970 and is positive in 1960 and 1980 (row 5). In contrast, the coefficient on median income remains unchanged or even increases by a small amount.[20] While there is not enough independent variation in the sample to deem a town's

[20] A similar pattern is observed when replacing median income with the share of residents above and below certain income thresholds in all years or the share of residents below the official poverty line in 1970 or 1980. (The concept of an absolute "poverty line," which takes into account income, family size, and the ages of family members, was had not been developed by 1960).

racial composition unimportant, it is clear that the relationship between housing values and black population share is less firm than that between housing values and residents' income.

The rent regressions present a similar pattern. Rents are higher in towns with a higher median income, though the coefficients are smaller than in the value regressions in 1970 and 1980 and are only marginally significant. As with values, the black population share of a jurisdiction appears to have no effect on rents after including local demographics and housing quality controls.

(iii) Cross-section Regressions: Robustness Checks

Table 4 addresses a number of concerns with the weighting and sample of blocks included in the analysis. The outcome of these robustness checks is similar in all years, and so I present only the 1970 owner-occupied sample here. The standard errors in Table 3 are clustered by jurisdiction, the level at which the demographic variables of interest are measured. To allow for a common error structure within metropolitan areas, many of which contain multiple border areas, the second row contains results clustered by metropolitan area. The standard errors increase between 20-40 percent, but the estimates are still significant.

Because the level of observation in the base specification is a block, longer border areas that contain more blocks will be given more weight. While there is little reason to believe that border length is correlated with jurisdiction attributes, the third row weights by the inverse of the number of blocks in the border area, thus weighting each border area equally. The coefficients are qualitatively unchanged. A larger concern is that the current specification treats the housing prices on each block as if they were calculated with equal precision despite the fact that each is based on a different number of underlying units. The third row weights each block by the number of owner-occupied housing units for which value information is available, a figure that ranges between 5 and 290 (median = 21.7). This specification will put more weight on blocks that are denser or on which a higher share of units are owner-occupied. Standard errors fall in this case.

While the results in Table 3 control for local demographics, the estimates may still be picking up the desire to live in an all-white *neighborhood*. I address this concern in two ways. The fourth row re-estimates using only the 70 borders deemed not to be in racial transition (defined above), while the fifth row includes only those blocks that have no black residents (79.4 percent of the sample). If the jurisdiction-level characteristics served as a proxy for neighborhood factors, we would expect the coefficients in these predominately white samples to fall. Instead, in both cases, the coefficients increase in absolute value. Interestingly, households on blocks with at least some black residents are willing to pay more to live in a jurisdiction with a higher black population share, though this relationship is not statistically significant. This result reinforces the need for care in making statements about *whose* preferences are being represented by these "willingness to pay" figures.

Together, borders in the Los Angeles and greater New York metropolitan areas account for one third of the full sample. In the last rows of Table 4, I re-run the regressions while dropping first the Los Angeles and then the New York borders. The results are not sensitive to this omission, nor are they sensitive to dropping both large metropolitan areas simultaneously (not shown).

(iv) Further Robustness: Panel Estimation, 1960-1980

The discontinuity of the housing price gap at the border rules out the possibility that this estimate reflects the slow evolution of housing quality with distance from the city center (Figure 2). However, either housing stock or neighborhood quality themselves could change abruptly at a political boundary. Towns have control over land use through zoning policy, which can include bans on multi-family units or large lot size requirements.[21] More generally, any local policy that raised property values in one municipality may have changed the incentives for home maintenance, renovation, and upkeep, eventually resulting in sharp changes in housing quality.

If zoning laws and the quality of public goods are relatively time invariant aspects of a jurisdiction, we can control for these – and any other – fixed differences in housing quality across borders by examining how price gaps *evolve* as disparities in jurisdiction-level characteristics narrow or widen over time. I pool data for the three decades and estimate:

$$\ln(\text{price}_{ijbt}) = \alpha + \beta\ \%\text{black}_{jt} + \Phi'\text{block}_{it} + \Pi'\text{Y}_t + \Psi'(\text{Z}_b \text{ x } \text{Y}_t) + \Omega'(\text{Z}_b \text{ x } \text{J}_j) + \varepsilon_{ijbt} \quad (2)$$

where Y_t indicates the Census year and J_j the political jurisdiction. Y_t adjusts for nationwide trends in the price of housing. The interaction term (Z_b x J_j) allows each side of the border to have its own fixed effect, while (Z_b x Y_t) allows any unobserved characteristics common to both sides of the border to change over time. These two interaction terms absorb the main jurisdiction and border effects (J_j and Z_b). β is estimated from changes in jurisdiction-level racial composition or median income over a decade relative to one's immediate neighbor.

Table 5 presents results from a series of panel regressions. In this context, the median resident remains willing to pay to live in a town with wealthier co-residents. As before, doubling the median income of a town's residents is associated with a 30-40 percent increase in housing value. This pattern is true both in the balanced panel and in the full sample (rows 1-2) and is robust to entering racial composition and median income together (row 3).

[21] Zoning rules that apply only to *new* construction should not differentially affect housing quality across the borders in this sample, most of which were already built up by the 1920s, when the first zoning laws were passed. Bans on multi-family use, on the other hand, apply both to new construction and to conversion of existing units.

In contrast, in the panel, the relationship between racial composition and housing value disappears altogether. While one might be tempted to conclude that the cross-sectional results are an artifact of unobserved differences in housing quality, the null result is in fact the average of two very different samples. As in the cross section, residents on blocks with no black residents are willing to pay to avoid living in a diverse jurisdiction, while residents on blocks with at least one black resident are willing to pay to join such jurisdictions.[22] The average black share on such blocks is 16 percent in 1960 and 35 percent in both 1970 and 1980, suggesting that the preferences of these residents might be far from those of the median homebuyer. The premium associated with these areas may reflect the scarcity of black, middle class neighborhoods (Bayer, Fang, and McMillan, 2005). Eventually, this process should lead to a resorting of the population, as blacks outbid whites and the neighborhood undergoes racial transition. This pattern helps to explain why blocks in diverse jurisdictions continue to attract more black residents over time.

V. The Role of Public Goods in the Willingness to Pay for Rich Co-Residents

The median homeowner is consistently willing to pay to live in a town with wealthier co-residents. In this section, I investigate a series of local policies that may account for this desire. The *Census of Governments* reports effective property tax rates by jurisdiction.[23] Effective rates adjust the nominal rate for the discrepancy between assessed and market values. To the best of my knowledge, proxy measures for the quality of public goods provision – for example, test scores for education quality – do not exist during this period. In their stead, I use the admittedly problematic measure of expenditures by category.[24] Higher spending cannot be interpreted as an indication of a higher quality or quantity of provision. Costs may vary with, say, the rate of

[22] The same pattern is found when splitting the sample into transition and non-transition areas (not shown).
[23] In some metropolitan areas, rates are only collected for central cities and the "balance of county," collapsing any potential variation between towns in the suburban ring.
[24] A full list of historical expenditure sources are presented in Appendix Table 2.

unionization or the degree of patronage and corruption. The production function may also differ by place. For example, school districts with ill-prepared students may need to hire more teachers to produce the same quality of education.

To explain the willingness to pay for richer co-residents, a given policy measure must be correlated with median income. In this sample, wealthier towns set lower property tax rates than their cross-border neighbors.[25] A one standard deviation increase in median income is associated with a lower annual property tax bill of $55 for the sample's average home value of $110,000 (in $2000). While rich and poor towns raise the same amount of revenue per resident, expenditure patterns differ by income. Rich towns spend more (locally-raised) revenue on education and less on non-educational purposes.[26] A series of state and federal transfers make up the difference in educational spending for poor towns. Rich and poor towns also spend equally on infrastructure (roads, parks or sewers). But, poor towns spend more per resident on sanitation and public safety. We can reasonably assume that, even with these higher spending levels, poor towns are not safer and cleaner than their rich neighbors. As a result, we cannot interpret the housing price estimates as "willingness to pay for crime reduction" (which, as we will soon see, would be negative!) but rather as a desire not to pay to police or clean someone else's neighborhood.

Table 6 adds each of these local policy measures in turn to the 1970 cross sectional regression of home values on median income.[27] Lower property tax rates in wealthy towns explain half of the willingness to pay for rich co-residents (column 2). Houses on the border are likely to be smaller than the median suburban unit and thus receive a fiscal subsidy from their wealthier co-residents. According to the coefficient estimate, the lower property tax rate

[25] The findings in this paragraph are based on a series of unreported regressions that are available from the author.
[26] A one standard deviation increase in median income is associated with $230 more local revenue spent per student and $130 less spent per resident on other goods and services.
[27] Because some jurisdictions are missing one or another policy measure, I re-estimate the basic regression for different samples in the odd columns.

associated with a one standard deviation increase in median income leads to a $387 housing price premium.[28] Given the $55 per year tax break, one would need to remain in the house for less than five years to break even in net present value terms (assuming a 5 percent interest rate).

Education spending per pupil does not vary by median income, and thus has no effect on willingness to pay for co-residents. I include measures of education spending in the fourth column to demonstrate that homeowners are willing to pay for instructional expenditure.[29] Column 6 adds the two expenditure categories that do vary with income: sanitation and police.[30] Homeowners appear to dislike spending on police, but this conclusion cannot be generalized beyond border area neighbors who presumably share a similar local victimization rate but pay very different amounts for police services. The higher spending on public safety and sanitation in poor towns explains around one third of the premium for rich co-residents. The last column includes measures of both property taxes and expenditure, which together wipe out the willingness to pay for rich co-residents altogether. The coefficient on median income falls by 75 percent from baseline, while the coefficients on the local policy measures, though less precisely estimated, remain unchanged.

VI. Heterogeneity in Willingness to Pay for a Predominately White Town

Consistent with a process of sorting by race, households on all-white blocks are willing to pay to live in a jurisdiction with a low black population share, while households on blocks with at least one black resident seem to prefer, all else equal, a town with a high black population

[28] A one dollar increase in the property tax rate (per $1000) reduces home values by 0.7 percent. A one standard deviation increase in median income is associated with a $0.503 lower rate. For the sample's average home value of $110,000, this translates into a premium of $387.30 (= $0.503 x 0.007 x $110,000).
[29] Increasing instructional expenditure per pupil by one standard deviation (0.65 in $1000) would lead to a 1.4 percent higher home value. For the average home value, this translates into a $1540 one-time increase in price in exchange for an increase in $650 in per pupil expenditure per year.
[30] A large "other" category, which accounts for around half of total non-educational expenditure, is also negatively correlated with income.

share. Two events during this period, race-related rioting in the 1960s and school desegregation in the early 1970s, might have increased the cost of living in a diverse jurisdiction. In some cases, riots increased the political sway of black voters (Sonenshein, 1993; Fording, 1997). Desegregation increased the probability that an urban child would be in school with an opposite-race peer. In addition, dismantling segregation often required some children to transfer away from their neighborhood schools, a fate that all parents, regardless of race, may seek to avoid. Can we find evidence that these two events mattered for location decisions in the data?

A. Riot Activity

Collins and Margo (2007) document that property values fell in cities that experienced riot activity in the 1960s. The border areas in my sample are, on the whole, far from black enclaves where the worst riot-related property damage occurred. However, riots may have affected the *political* cost of living in a diverse jurisdiction. The occurrence of a riot may have galvanized a black voting bloc. Indeed, many American cities elected their first black mayors in the aftermath of a riot. Carl Stokes of Cleveland and Richard Hatcher of Gary, Indiana, were elected in 1967. By the early 1970s, other major cities, including Detroit, Los Angeles, and Washington, D.C., followed suit.

I use Collins and Margo's index of riot severity to measure the presence of riot activity during the 1960s at the metropolitan area level. The index considers five components of riot damage – deaths, injuries, arrests, arsons and days of rioting – and calculates the share of all national riot damage that occurred in a given riot.[31] The index value for a metropolitan area is the sum over all local riot activity during the decade. Using this index, I define two indicators of high riot intensity: metropolitan areas that contained at least 10 (at least 5) percent of total riot

[31] Data on the location of 1960s riots and related damage was generously provided by Gregg L. Carter.

activity. I present results using the 10 percent indicator here; the results do not qualitatively change when using the less restrictive definition. While 11 of the metropolitan areas in the panel sample are deemed low riot areas by this measure, only three areas were completely riot-free (Moline, IL; San Jose, CA; St. Louis, MO). The seven high riot areas are Chicago, Cleveland, Detroit, Jersey City, Los Angeles, Newark and New York City.

I estimate a pooled cross-sectional regression in which each border area is allowed to have a decade-specific fixed effect.

$$\ln(\text{price}_{ibjt}) = \alpha + \Gamma_1(\%\text{black})_{jt} + \Gamma_2[I(\text{riot}) \cdot \%\text{black}]_{jt} + \gamma Y_t + \Theta' Z_b + \Psi'(Z_b \times Y_t) + \varepsilon_{ibjt} \qquad (3)$$

%black is a vector of black population share for each of the three decades and I(riot) is an indicator function equal to one if the border is in a metropolitan area that experienced a severe riot over the 1960s. The main effect of being in a high riot metropolitan area is absorbed by the border area dummies, and the indicator function is interacted with %black in every decade. I include interactions in 1960, before the riots occurred, to test whether the occurrence of a riot was symptomatic of other, longer-run differences between cities. The 1980 interaction indicates whether the effect of a riot (if any) persists over the intervening decade.

The estimation, which is presented in Table 7, is based on the panel sample. To focus on the preferences of white households, I exclude blocks with any black residents.[32] In this sample, there is no effect of black population share on housing prices in 1960 and a growing willingness to pay for white co-residents over time. By 1980, this group is willing to pay 30 percent more for a housing unit in an all-white relative to all-black town. The second column adds the riot

[32] I include only all-white blocks to conform with the above evidence that residents prefer to live in a majority own-race jurisdiction. If riots tipped the local political balance towards black candidates, we would only expect whites households to be averse to this outcome. Including all blocks in the regression leads to a qualitatively similar, though attenuated, pattern.

interactions to this specification. In 1970, two years after the most intense period of riots, the estimated demand for racially homogenous towns is driven entirely by metropolitan areas that experienced a severe event. By 1980, the difference between high and low riot intensity areas disappears. Perhaps the fears of follow-on violence or of large changes in the political equilibrium did not come to pass and the housing market reflects these revised assessments.[33] While the riot indicator may, in part, be picking up unmeasured differences across cities, there is a clear trend break in 1970. This finding accords with Collins and Margo's conclusion that, conditional on having a black population above a certain threshold, riots were essentially random events.

B. Court-Ordered Desegregation

The late 1960s was not only a period of violent unrest but was also a time of substantial uncertainty, as parents watched the courts for news on impending school desegregation. While southern schools were already dismantling their systems of *de jure* segregation, the courts had not yet revealed their stance toward *de facto* segregation in northern cities.[34] Courts solidified their position on northern districts in *Keyes v. School District No. 1, Denver* (1973). This decision ruled that school districts could be subject to remedial action even if segregation resulted from residential patterns rather than deliberate race-based school assignments.

Court-ordered desegregation may have increased the cost of living in a diverse jurisdiction for a number of reasons. First, desegregation plans often required reassigning

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[33] In contrast, Collins and Margo (2004) find that the value of black-owned property does not bounce back after a riot in the 1970s. However, the value of the *average* unit in their sample does experience some mean-reversion, which is consistent with these estimates for predominately white border areas.

[34] The school districts in my sample faced high rates of *de facto* segregation. The mean dissimilarity index at the elementary school level is 0.51 in 1970. Because high schools are larger and thus serve a broader set of neighborhoods, dissimilarity at the high school level is lower (0.31). These values are calculated from the Office for Civil Rights' school-level files, which were generously provided by Sarah Reber.

students from their neighborhood school to a school across town. Even parents who have no preference over the race of their child's classmates may dislike sending their children to a distant school (Bogart and Cromwell, 2002). Parents may have also cared about their race of their child's peers, either directly or due to the correlation between race and student preparedness (Hoxby and Weingarth, 2005).

I collect detailed information on the date of relevant court decisions, the findings in the case, and the required remedies, if any, from the *State of Public School Integration* website (Logan, 2004). I code any cases that occurred between 1965-1980. The presence of a court order is quantified in two ways: a continuous variable counting the number of remedial steps required, without regard to their intensity, and a set of dummy variable indicating the presence of either busing or a magnet school program in the court order. 34 borders in the panel sample contain at least one jurisdiction with a desegregation-related court case; 23 do not. Of the 34 borders with at least some court activity, ten of these experienced court supervision on both sides.

The desegregation measures are not time varying; rather, they provide summary information for the entire period. Thus, I interact both the black population share and the desegregation measures with decade fixed effects and estimate:

$$\ln(\text{price}_{ibjt}) = \alpha + \Gamma_1(\%\text{black})_{jt} + \Gamma_2[\text{Deseg. measure}]_{jt} + \gamma \text{Y}_t + \Theta'\text{Z}_b + \Psi'(\text{Z}_b \text{ x } \text{Y}_t) + \varepsilon_{ibjt} \quad (4)$$

Seven of the 21 cases under consideration occurred before 1970 and so we might expect a small price response in that year. By 1980, the housing market would have sufficient time to react to all court-orders in the sample. In contrast, we can reasonably treat the 1960 desegregation interaction as a "placebo" experiment examining the relationship between housing prices and desegregation plans that *would be implemented* in the future.

Table 8 contains the results from this specification. In 1980, the presence of a court-ordered desegregation plan was associated with lower housing prices. The average order required 2.3 remedial steps. For each step, housing prices fell by 1.6 percent (column 2). Student busing was the most active form of court intervention; this step led to an 8.4 percent decline in housing values (column 3). The magnitude of the price response accords with Bogart and Cromwell's estimate of a 9.9 percent decline in housing values following a student re-assignment and the loss of neighborhood schools in Shaker Heights, OH. The placebo regressions indicate that homes in cities that were to initiate busing programs were already worth around 2 percent less than their suburban neighbors in 1960; however, this gap grew four-fold after the implementation of a desegregation plan. The establishment of magnet schools, a program that is usually valued by city parents, also had a negative effect on prices, perhaps because this step was bundled with other requirements (column 4).

The first column of Table 8 contains the baseline effect of black population share on housing prices without the desegregation measures. As for the sub-sample in Table 7, the median homeowner's willingness to pay for white co-residents increased over time. The coefficient on black population share is only different from zero in 1980, at which point a housing unit is worth 16 percent more if it is located in an all-white relative to an all-black town. Comparing this column to the rest of the table, desegregation explains one third of the relationship between housing prices and jurisdiction-level racial composition in 1980 or two thirds of the *increase* in the willingness to pay for racial homogeneity in 1980 relative to the previous decade.

VI. Conclusion

Road building projects and the diffusion of the car made it economically feasible for many to settle in bedroom communities in the post-War period. Unlike cities, which are large, diverse political units, the suburbs offered an array of choices between distinct towns, each with a unique bundle of public goods. This paper demonstrates that the changing racial and socio-economic composition of the urban population was an independent cause of suburbanization. By moving to the suburbs, households paid for the fiscal subsidy they received through the property tax system from their better-off co-residents. Suburban residents also avoided the responsibility for addressing urban problems through local expenditures on public safety.

Above and beyond income differences, white households were willing to pay to leave jurisdictions with large black population share, while black households were willing to pay to enter. There is some evidence that blacks outbid whites for such units, leading to a greater concentration of black residents on the diverse side of borders. The demand for residence in a predominately white town is highest in 1980, due, in large part, to efforts at school desegregation. The average court-ordered remedy is associated with a 1.6 percent decline in housing values. Busing, the most onerous step, reduced housing values by 8.4 percent. In contrast, in 1970, the demand for an all-white town exists only in metropolitan areas that have recently undergone a severe riot.

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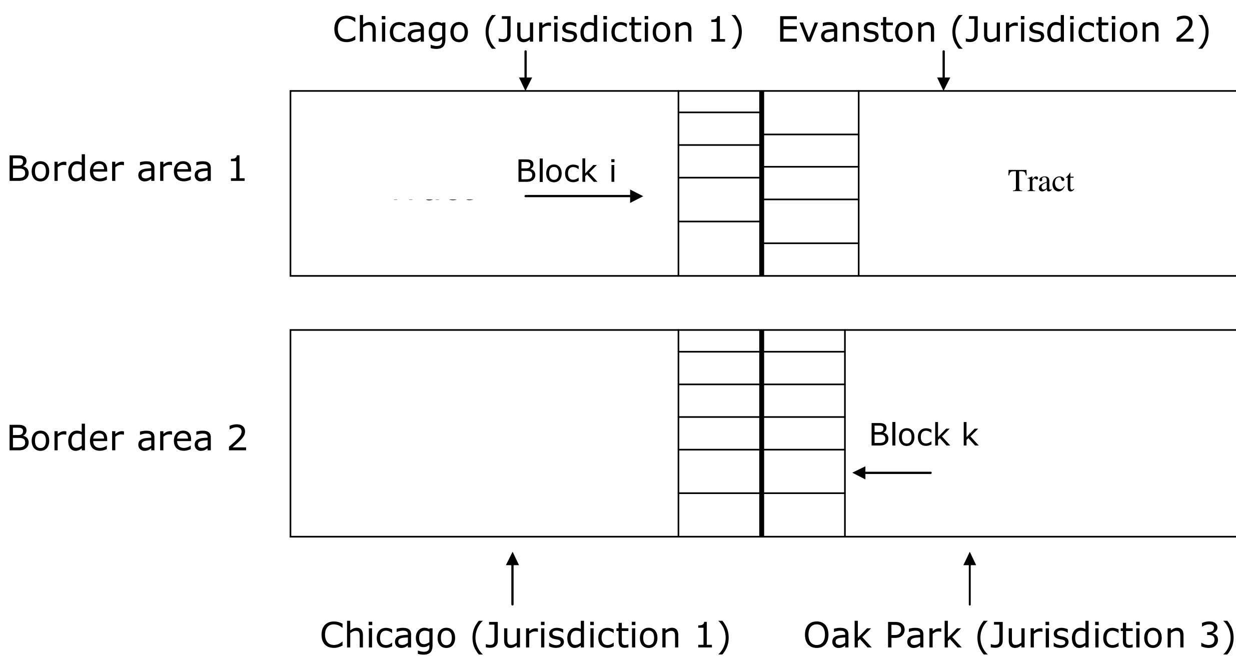
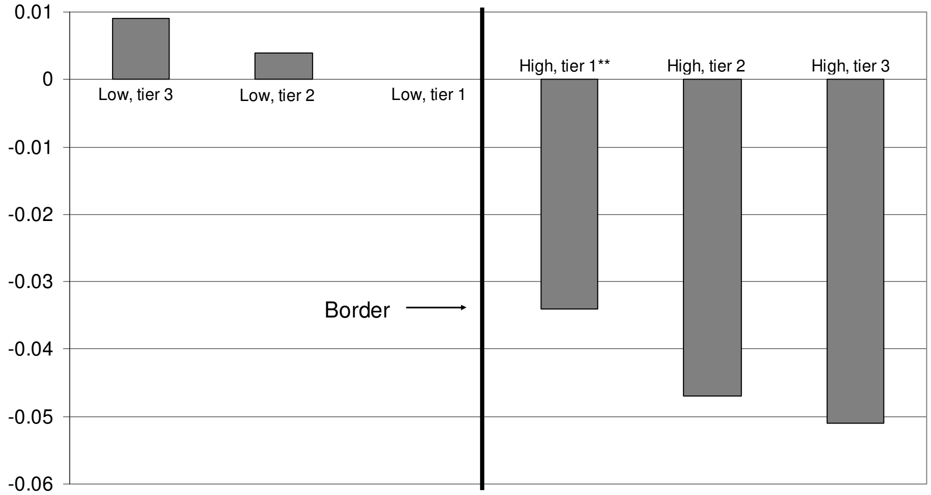
Figure 1: Schematic diagram of geographic terms



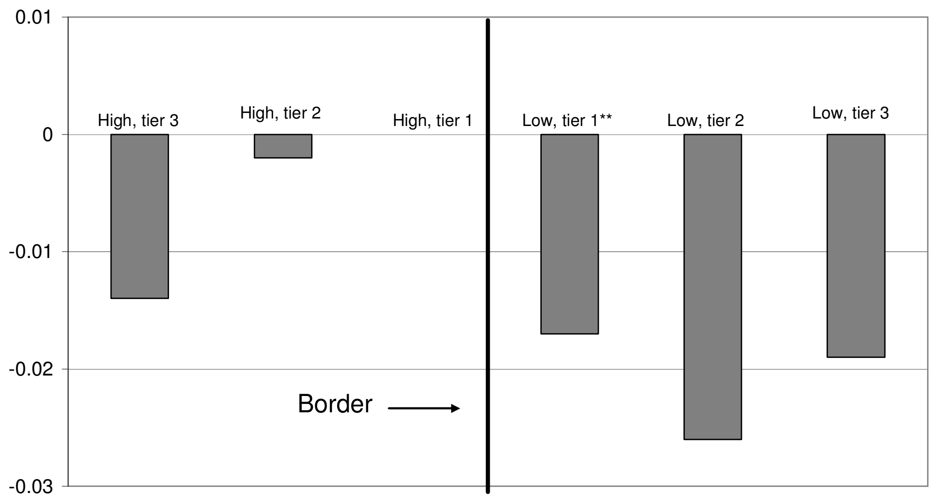
- Block i is in border area 1 (b = 1) and jurisdiction 1 (j = 1)
- Block k is in border area 2 (b = 2) and jurisdiction 3 (j = 3)

Figure 2: Mean housing prices by distance from the jurisdiction border, 1970

A: Jurisdictions classified as high/low black population share;
Prices relative to first blocks on 'low' side



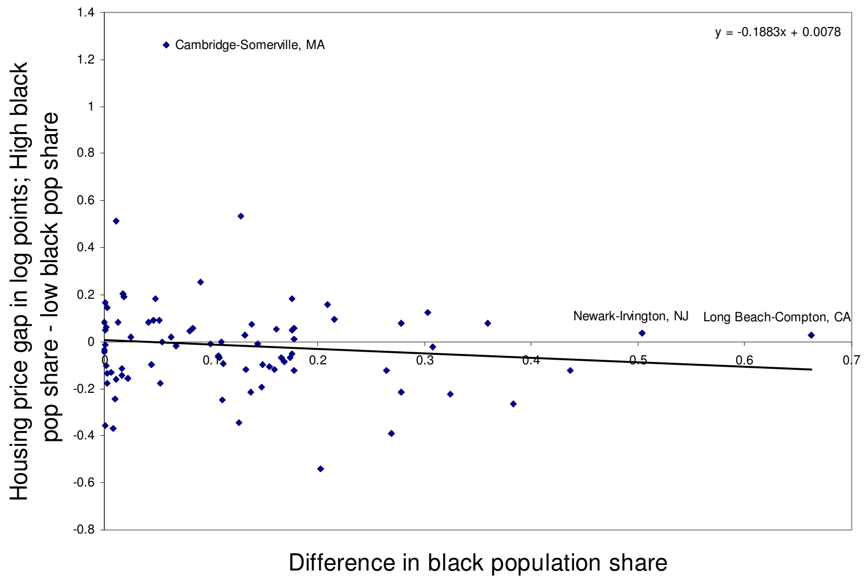
B: Jurisdictions classified as high/low median income;
Prices relative to first blocks on 'high' side



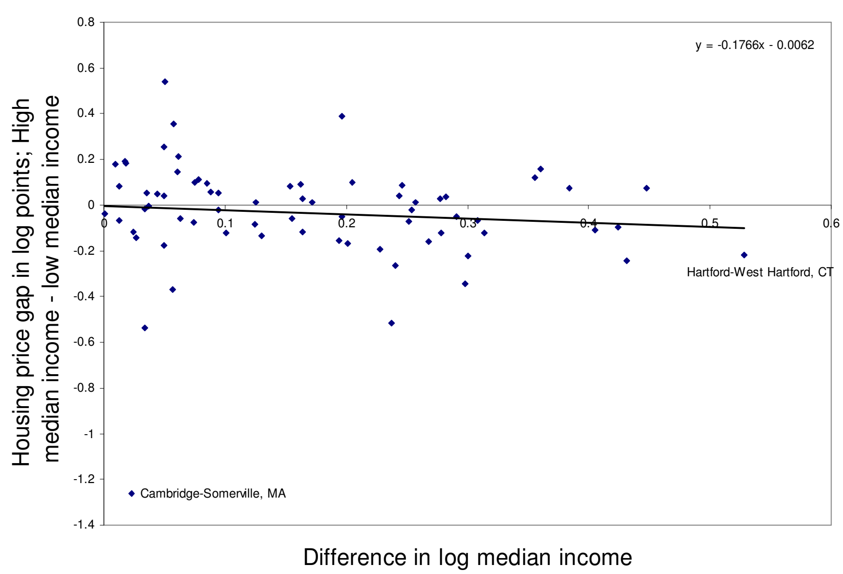
Notes: Each bar represents a coefficient from a regression of the logarithm of housing value on a series of indicator variables for distance from the border. Distance from the border is measured in "block tiers," with the first tier including all blocks adjacent to the border, and so on. Each jurisdiction in the pair is classified as having either a high or low black population share (or median income) relative to its neighbor. The first block tier in the low black share/high median income jurisdiction is the omitted category. A tier whose housing prices are significantly different at the 5 percent level from its immediate neighbor to the left is starred. The regression also includes the block-level controls listed in the notes to Table 3. The sample only includes borders for which each jurisdiction has at least three block tiers with available data on either side.

Figure 3: The relationship between housing prices and jurisdiction-level characteristics, 1970

A. Black population share



B. Median income



Notes: Each dot represents one of the 82 borders in the full sample. The x-axis indicates the difference in black population share or log median income between the two jurisdictions. The y-axis indicates the difference in the mean value of owner-occupied housing.

Table 1: Jurisdiction borders with available block-level data by metropolitan area, 1960-80

Region	Metropolitan area	Number of Borders
I. Panel sample (1960-80)		
Northeast	Boston	2
	New York†	10
	Pittsburgh	3
	Providence	3
Midwest	Chicago†	6
	Cleveland	2
	Dayton	1
	Detroit	1
	Kansas City, KS-MO	2
	Minneapolis/St.Paul	1
	Moline-Davenport, IL-IA	1
	St. Louis	1
West	Denver	1
	Los Angeles†	17
	San Francisco†	2
	San Jose	4
		Subtotal: 57
II. Full sample (1970-80)		
Northeast	Allentown-Bethlehem	2
	Hartford	2
	Scranton	1
	Springfield-Chicopee	2
Midwest	Akron	4
	Canton	3
	Des Moines	1
	Grand Rapids	1
	Indianapolis	1
	Madison	1
	South Bend	2
West	Las Vegas	1
	Oxnard-Thousand Oaks	1
	Phoenix	1
	Portland	1
	San Bernardino-Riverside	1
		Subtotal: 25
		Total: 82

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: The relationship between housing quality and jurisdiction-level characteristics, 1970-80

Dependent variable	Share black 1970	Share black 1970-80	ln(median income) 1970	ln(median income) 1970-80
A: Housing quality				
Share single family	0.082	0.007	-0.034	-0.147
	(0.064)	(0.040)	(0.065)	(0.125)
Share owner occupied	0.097	0.059	-0.039	-0.021
	(0.042)	(0.037)	(0.036)	(0.072)
Share no plumbing	0.005	-0.002	-0.003	-0.007
	(0.005)	(0.004)	(0.005)	(0.011)
=1 if any group quart	-0.062	---	0.077	---
	(0.036)		(0.045)	
Mean # rooms, own	-0.418	-0.291	0.455	0.533
	(0.185)	(0.219)	(0.160)	(0.376)
Number of residents	-90.020	-13.100	53.532	-25.898
	(52.186)	(22.562)	(30.536)	(59.958)
Number of units	-56.878	-3.296	35.969	-9.394
	(34.609)	(9.216)	(18.231)	(23.104)
Residents/unit	-0.015	-0.016	-0.087	-0.398
	(0.134)	(0.076)	(0.168)	(0.177)
B: Demographics				
Share black, 6 tiers	0.314	0.522	-0.107	-0.652
N1=4944	(0.063)	(0.123)	(0.056)	(0.224)
Share black, 3 tiers	0.199	0.372	-0.049	-0.579
N1=3446	(0.042)	(0.114)	(0.036)	(0.201)
Share black, 1 tier	0.091	0.237	-0.011	-0.427
N = 1905	(0.025)	(0.075)	(0.021)	(0.142)

Notes: Each cell represents the coefficient from a separate regression, the dependent variable of which is listed in the first column. Regressions include a vector of border area dummy variables. Standard errors, which are reported in parentheses, are clustered by jurisdiction. In Panel A, the sample is restricted to blocks adjacent to the jurisdiction border with at least 5 owner occupied units; this is the sample used for the housing value regressions in Tables 3-7. The 1970 cross-section regressions in columns 1 and 3 contain 1905 block-level observations along 82 border areas. The panel regressions contain 3872 block-level observations. Panel B compares the racial composition of residents living six blocks, three blocks, and one block from the border.

Table 3: The relationship between housing prices and jurisdiction-level characteristics

Dependent variable:	ln(mean value)		ln(mean rent)	
RHS variable:	Share black	ln(med inc)	Share black	ln(med inc)
1960				
Alone	-0.167	0.337	-0.118	0.212
N1 = 1533 N2= 1147	(0.097)	(0.083)	(0.090)	(0.071)
Add share black, block	-0.136	0.332	-0.105	0.213
	(0.100)	(0.084)	(0.090)	(0.071)
Add housing controls	-0.090	0.157	-0.066	0.136
	(0.060)	(0.062)	(0.098)	(0.071)
Together	0.171	0.248	0.158	0.223
	(0.106)	(0.100)	(0.129)	(0.086)
1970				
Alone	-0.169	0.227	-0.056	0.104
N1 = 1433; N2 = 1250	(0.074)	(0.062)	(0.066)	(0.045)
Add share black, block	-0.141	0.218	-0.013	0.092
	(0.077)	(0.062)	(0.071)	(0.047)
Add housing controls	-0.100	0.115	0.025	0.051
	(0.045)	(0.034)	(0.071)	(0.052)
Full sample	-0.102	0.115	0.018	0.068
N1=1905; N2 = 1466	(0.042)	(0.028)	(0.071)	(0.049)
Together	0.000	0.115	0.169	0.175
	(0.063)	(0.046)	(0.076)	(0.051)
1980				
Alone	-0.251	0.408	-0.115	0.195
N1 = 1502; N2 = 1160	(0.103)	(0.063)	(0.071)	(0.043)
Add share black, block	-0.128	0.308	-0.022	0.125
	(0.091)	(0.068)	(0.069)	(0.046)
Add housing controls	-0.077	0.211	0.007	0.078
	(0.072)	(0.053)	(0.067)	(0.040)
Full sample	-0.135	0.200	-0.003	0.067
N1 = 1979; N2 = 1385	(0.067)	(0.037)	(0.059)	(0.049)
Together	0.085	0.240	0.106	0.129
	(0.076)	(0.076)	(0.059)	(0.043)

(Notes on next page.)

Notes: Standard errors are reported in parentheses and clustered by jurisdiction. The sample is restricted to blocks adjacent to the jurisdiction border. The number of blocks underlying the value and rents regressions are listed in the left-hand column as N1 and N2, respectively. The 1960 results are based on the 57 borders in the panel sample, as are the 1970 and 1980 results in rows 1-3. Rows 4-5 in those years are based on the full sample of 82 borders. All regressions include a vector of border area dummy variables. Housing quality controls in rows 3-5 include: the share of housing units that are in single-family units, are owner-occupied, or lack some indoor plumbing; the average number of rooms by tenure status; the number of residents per unit (density); and an indicator for the presence of group quarters.

Table 4: Checking the robustness of the relationship between housing values and jurisdiction level characteristics, 1970

Dependent variable = ln(housing values)

	Share black	Ln(med inc)
1. Baseline	-0.102	0.115
	(0.042)	(0.028)
2. Cluster by metro area	-0.102	0.115
	(0.058)	(0.033)
3. Weight by inverse of # blocks	-0.112	0.101
	(0.051)	(0.027)
4. Weight by # houses	-0.136	0.157
	(0.033)	(0.024)
5. Non-transition borders	-0.174	0.129
N = 1458	(0.063)	(0.035)
6. Blocks with no black residents	-0.173	0.141
N = 1514	(0.063)	(0.038)
7. Blocks with at least one black resident	0.097	0.072
N = 391	(0.085)	(0.057)
8. Without Los Angeles	-0.131	0.125
N = 1416	(0.046)	(0.032)
9. Without Greater New York area	-0.114	0.103
N = 1692	(0.046)	(0.032)

Notes: Standard errors are reported in parentheses. With the exception of row 1, they are clustered by jurisdiction. Regressions only include blocks adjacent to the jurisdiction border. The full sample of border areas is used, resulting in 1905 blocks underlying the regressions in the first three rows. Sample sizes associated with the various restrictions in rows 4-7 are reported. All regressions contain a vector of border area dummy variables. The set of included housing quality controls is reported in the notes to Table 3.

Table 5: The relationship between changes in housing prices and changes in jurisdiction-level characteristics over time

Dependent variable = ln(housing values)

	Share black	ln(median income)
Entered alone		
Panel sample	-0.015	0.374
N = 4446	(0.065)	(0.105)
Full sample	-0.029	0.391
N = 5395	(0.065)	(0.093)
Entered together		
Full sample	0.128	0.479
	(0.046)	(0.092)
No black residents	-0.304	0.271
N = 3821	(0.154)	(0.131)
At least one black resident	0.356	0.693
N = 1574	(0.081)	(0.249)

Notes: Standard errors are reported in parentheses and clustered by jurisdiction. All regressions include a set of main effects for jurisdictions, Census years, and border areas, as well as interactions between border areas and both jurisdiction and Census year. The sample is restricted to blocks adjacent to the jurisdiction border. Regressions include the set of block-level controls that are available in all three decades: share of block residents who are black, density on the block, the average number of rooms in owner-occupied units, and the share of units that are owner occupied.

Table 6: Can variation in property tax rates and public expenditure explain the demand for rich co-residents, 1970?

Dependent variable = ln(housing values)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ln(med income)	0.095	0.055	0.104	0.096	0.109	0.069	0.024
	(0.035)	(0.043)	(0.027)	(0.029)	(0.022)	(0.032)	(0.061)
(per $1000)							
Property tax rate, median		-0.007					-0.007
		(0.003)					(0.004)
(in $1000)							
$ per pupil, instruction				0.023			
				(0.013)			
$ per pupil, administration				-0.222			
				(0.113)			
$ on police, per resident						-0.269	-0.243
						(0.141)	(0.193)
$ sanitation, per resident						0.214	0.240
						(0.230)	(0.292)
N (blocks)	1175	1175	1679	1679	1858	1858	1175
N (borders)	49	49	73	73	80	80	49

Notes: Standard errors are reported in parentheses and clustered by jurisdiction. The sample is restricted to blocks adjacent to the jurisdictional border. All regressions include the full set of block-level controls, which are listed in the notes to Table 3. Notes on and sources for the public goods measures are in Appendix Table 2. The following borders are missing information on public expenditure (rows 6-7): Oxnard-Port Huerte, CA; Pittsburgh-Swissvale, PA. The following additional borders are missing information on educational expenditure: Canton-North Canton, OH; Grand Rapids-East Grand Rapids and Grand Rapids-Walker, MI; Kearney-North Arlington, NJ; Pittsburgh-McKeesrock, Pittsburgh-Stowe and Pittsburgh-Wilkinsburg, PA; and Scranton-Dunmore, PA.

Table 7: Does riot activity increase the aversion to living in a diverse jurisdiction?

Dependent variable = ln(housing values)

	(1)	(2)
Share black 1960	-0.062	0.081
	(0.063)	(0.127)
Share black 1970	-0.198	0.095
	(0.056)	(0.104)
Share black 1980	-0.319	-0.309
	(0.095)	(0.113)
Share black · I(riot) 1960		-0.185
		(0.131)
Share black · I(riot) 1970		-0.388
		(0.107)
Share black · I(riot) 1980		-0.045
		(0.180)

Notes: Standard errors are reported in parentheses and clustered by jurisdiction. The sample is restricted to blocks in the panel sample with no black residents that are adjacent to the jurisdictional border (N = 3134). Regressions include the four block-level controls that are available in all decades; see the notes to Table 5. The riot indicator is defined in the text.

Table 8: Does court-ordered desegregation increase the aversion to living in a diverse jurisdiction?

Dependent variable = ln(housing values)

		Desegregation measures		
		Number of steps in court-order	=1 if includes busing	=1 if includes magnet
Share black 1960	-0.018	-0.016	0.031	-0.046
	(0.072)	(0.084)	(0.074)	(0.067)
Share black 1970	-0.073	-0.042	-0.053	-0.079
	(0.054)	(0.050)	(0.050)	(0.053)
Share black 1980	-0.162	-0.106	-0.138	-0.152
	(0.079)	(0.070)	(0.064)	(0.079)
Deseg measure 1960		-0.000	-0.020	0.058
		(0.005)	(0.017)	(0.035)
Deseg measure 1970		-0.005	-0.016	0.018
		(0.003)	(0.019)	(0.035)
Deseg measure 1980		-0.016	-0.084	-0.052
		(0.004)	(0.025)	(0.028)

Notes: Standard errors are reported in parentheses and clustered by jurisdiction. The sample is restricted to blocks in the panel sample that are adjacent to the jurisdictional border (N =4357). The desegregation variables are based on court-orders that were handed down between 1965-1980. Coding is based on the *State of Public School Integration* website at Brown University. Regressions include the four block-level controls that are available in all decades; see the notes to Table 5.

Appendix Table 1: Summary Statistics of Jurisdiction- and Block-level Variables in the Panel Sample, Across Borders and Over Time

	1970		1960-70/1970-80
Mean (S.D.)	All jurisdictions	Difference across borders	Difference across borders
Panel 1: Jurisdiction level			
Share black	0.109 (0.146)	0.132 (0.142)	0.036/0.024 (0.063)/(0.092)
Median family income, $ 2000	$49,117 ($8,696)	$8,088 ($6,254)	$2018/$2406 ($2497)/($2456)
Property tax rate, median single family, per $1000	11.676 (3.774)	1.106 (1.347)	
In $1,000 ($2000):			
Instruction $ per pupil	3.001 (0.652)	0.512 (0.473)	
Administration $ per pupil	0.133 (0.055)	0.044 (0.046)	
Non-educ $ per capita	0.511 (0.297)	0.269 (0.298)	
$ on sanitation, per capita	0.033 (0.019)	0.017 (0.016)	
$ on police, per capita	0.097 (0.041)	0.047 (0.035)	

(table continued…)

Appendix Table 1, continued			
	1960	1970	1980
Panel 2: **Block level**			
Average value, owned	$101,077	$110,103	$179,063
	(54,347)	(41,638)	(96,838)
Average contract rent	$457.14	$524.11	$596.27
	(144.41)	(175.86)	(196.87)
Share single family	---	0.613	0.653
		(0.349)	(0.348)
Share owner occupied	0.595	0.588	0.605
	(0.322)	(0.309)	(0.313)
Mean # rooms, owned	5.757	5.685	5.435
	(0.991)	(1.060)	(0.846)
Mean # rooms, rented	4.142	4.047	---
	(0.788)	(1.046)	
Mean # rooms, all units	---	---	5.111
			(1.126)
Share "unsound"	0.142	---	---
	(0.272)		
Share lacking plumbing	---	0.015	0.011
		(0.053)	(0.032)
Residents/unit	3.063	2.983	2.774
	(1.116)	(0.979)	(0.315)
=1 if group quarters	0.046	0.027	---
	(0.210)	(0.162)	
Share black	0.038	0.087	0.161
	(0.145)	(0.225)	(0.314)

Appendix Table 2: Sources for Jurisdiction-level Public Goods Data

Variable	Source
Current (non-educational) expenditure[1] - on roads, parks, sanitation, sewers, police fire, other	*Census of Governments*, 1967
Educational expenditure, per pupil[1] - instructional - administrative	*Elementary and Secondary General Information System* (ELSEGIS), 1968-69
Effective property tax rates: [2] - nominal rate - assessment-to-market ratio	*Census of Governments, 1972*

Notes:
1: Educational spending per pupil is collected both from independent school districts and municipal school systems. Non-educational expenditures are measured at the municipal level only. In some states, counties provide some public services. Most jurisdiction pairs in the sample fall within the same county, and thus county spending will not produce cross-border variation.
2: The *Census of Government* estimates assessment-to-market ratios by jurisdiction from a sample of recent home sales. Ratios are often reported only for the central city and for the "balance of the metropolitan area."