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

Leah Platt Boustan

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ABSTRACT

Cities are poorer than suburbs and this gap has grown over time. Comparing neighboring houses across city-suburban borders, I find that the marginal homeowner is willing to pay four percent more to live in a town whose median income is one standard deviation above the city's. Much of this suburban premium reflects fiscal isolation from social problems (especially spending on police). Growing urban poverty can explain around 25 percent of urban population loss from 1940-2000. Furthermore, the demand for wealthy co-residents leads to a vicious cycle of suburbanization, with implications for the rapid decline -- and recent revival -- of cities.

Leah Platt Boustan
Department of Economics
8283 Bunche Hall
UCLA
Los Angeles, CA 90095-1477
and NBER
lboustan@econ.ucla.edu

I. Introduction

The typical urban area in the United States is anchored by a poor central city surrounded by affluent suburbs. This spatial polarization has its roots in the mid-twentieth century. In 1940, suburbanites in the mean metropolitan area earned only three percent more than residents of the neighboring city, while by 2000 the income gap between city and suburban dwellers had increased to 16 percent. Because local governments are responsible for the provision of public goods, the subdivision of urban areas into rich and poor jurisdictions can reduce human capital acquisition and public safety with consequences for both equity and efficiency (Benabou, 1993, 1996).

An income gap between cities and suburbs first arose as rich households left the city, aided by the diffusion of the automobile and attracted by inexpensive land on the periphery (Margo, 1992; Glaeser, Kahn and Rappaport, 2007). The formation of wealthy communities outside the bounds of the central city may have provided a further impetus to suburbanize for households attracted to the increasingly differentiated suburban bundles of public goods (Tiebout, 1956). In this case, a growing income gap between cities and suburbs could be not only a consequence of suburbanization, but also an independent *cause*.

This paper documents that the demand for suburban residence was increasing in the size of the city-suburban income gap. This pattern could be driven by unobserved differences in housing quality and neighborhood characteristics between rich and poor towns. To control for these differences, I compare the price of city and suburban housing units on adjacent blocks that fall on opposite sides of a jurisdiction boundary. Focusing on 1960 to 1980, a peak period of suburban growth, I find that the marginal homeowner is willing to pay four percent more for an identical unit located in a town whose median income is one standard deviation above the neighboring city. The expanding income gap between cities and suburbs can explain between 20-30 percent of urban population loss from 1940 to 2000.

The observed demand for rich co-residents could be due to the fiscal subsidy provided by a larger suburban property tax base, the bundle of public goods selected by a wealthy electorate, or the composition of peers in public schools. While rich towns set lower tax rates, the capitalization of this tax benefit can explain only 20 percent of the overall price gap across borders. Another 30 percent can be attributed to fact that rich suburbs allocated fewer dollars per resident for public safety. By moving to the suburbs, homeowners could avoid the obligation to pay for policing "someone else's" neighborhood. School quality is unobserved during this period, but may have played an additional role.

The willingness to pay for rich co-residents may also have reflected a desire to avoid racial diversity (Boustan, 2007). A town's median income is highly correlated with its racial composition. There is some evidence that diverse jurisdictions provide fewer public goods to their residents (Cutler, Elmendorf and Zeckhauser, 1993; Alesina, Baqir, Easterly, 1998). This political channel might complement the tendency of white households to avoid diverse neighborhoods (Ellen, 1999; Card, Mas and Rothstein, 2007). I find no evidence of this racial channel at the town level. Housing prices were unaffected by a town's black population share after controlling for median income or poverty rates. An important caveat to this result arises with the desegregation of public schools in the 1970s. The marginal homeowner was willing to pay 5.3 percent more for an equivalent housing unit in order to avoid court-ordered busing.

The existing literature on suburbanization focuses on transportation improvements, including the automobile and new road building, which reduce the time cost of commuting to centrally-located employment (LeRoy and Sonstelie, 1983; Jackson, 1985; Mieszkowski and

Mills, 1993; Baum-Snow, 2007; Kopecky and Suen, 2007).[1] This paper demonstrates that suburbanization was also a response to the declining fortunes of central cities. However, the transportation-based and political economy explanations should not be considered mutually exclusive. Rather, the demand for wealthy co-residents can act as a suburban multiplier, augmenting the response to a given transportation shock. Because the rich are more likely to own a car, urban departures following new road construction will increase the income gap between a city and its suburbs. I show that the estimated demand for rich co-residents can increase the response to new highway construction by up to 50 percent. This feedback effect can help to explain the rapid decline of American downtown areas in the 1960s and 1970s (Baumol, 1967).

This paper also contributes to a growing literature using housing values to estimate household preferences for neighborhood and community attributes (Rosen, 1974; Black, 1999; Barrow and Rouse, 2004; Chay and Greenstone, 2005; Gibbons and Machin, 2005; Reback, 2005). Bayer, Ferreira and McMillan (2007) document that housing values increase by 5.6 percent for a $10,000 increase in the income of *immediate neighbors*. I find that, holding neighborhood composition constant, housing values increase by another 3.9 percent for a $10,000 increase in the median income of *residents in the same political jurisdiction*. Combining these two estimates provides a sense of the total willingness to pay for both the local and political benefits of moving from the median neighborhood in the central city to the median neighborhood in the adjoining suburb.

[1] An exception is Cullen and Levitt (1998) which studies the relationship between crime rates in the central city and suburbanization.

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

A. Predictions from Jurisdiction Choice Models

This analysis is motivated by models of jurisdiction choice, in which towns (cities and suburbs) offer distinct bundles of local public services and property tax rates (Tiebout, 1956). I briefly discuss the conditions under which a price gap is likely to emerge at jurisdiction borders in this framework.

One class of jurisdiction choice models assumes that the rich are more willing than the poor to trade off a dollar of private consumption for a dollar of public expenditure (see, for example, Ellickson, 1971; Westhoff, 1977; Fernandez and Rogerson, 1996).[2] As a result, the rich sort into towns that offer a high level of public goods at a high tax rate, while the poor select towns with low goods provision and low tax rates. In this basic set-up, we would not expect to find a price gap at the borders of rich and poor towns. Instead, as long as the housing supply is sufficiently elastic, supply will expand to meet the demand for housing units in either town.

According to this logic, we should not find a price gap at city-suburban borders during this period. The suburban housing supply was expanding dramatically after the Second World War, which would, in principle, allow the rich to self-select into the suburbs. However, most of the new suburban construction occurred on the metropolitan periphery. We can think of suburban housing as being subdivided into two distinct products: centrally-located and peripheral units. While both products offer access to suburban public goods, peripheral units require a longer commute. If the supply of inner-ring units is restricted, a price gap *could* emerge at the border of poor cities and their wealthy suburbs. In this case, the premium would reflect the price for access to the suburban bundle at no extra commuting cost.

[2] Epple and Romer (1991) show that these relative preferences hold when the function of local governments is to redistribute income, rather than to provide a public good.

It is important to note that, without making further assumptions about preferences by income, jurisdiction choice models do not automatically give rise to stratification. Instead, these models can generate a "poor chasing the rich" equilibrium, in which agents of all income levels prefer living in a town with a larger tax base (Buchanan and Goetz, 1972; Hamilton, 1976). In this case, the preference for rich co-residents reflects the fact that homeowners in a rich town will receive a cross-subsidy from owners of larger units, while owners of the same-sized house in a poor town will be cross-subsidizing smaller units in their jurisdiction. The fiscal subsidy available in rich jurisdictions will be capitalized into housing prices, generating a price gap at the border between rich and poor towns.

Wheaton (1993) points out that zoning regulations can be used to prevent the poor from successfully chasing the rich. The most common zoning laws dictate minimum lot sizes for new housing construction or prohibit multi-family dwellings. Given this use of zoning, it is particularly important to test that there are no sharp differences in unit size or multi-family use across the jurisdiction borders.

B. An Econometric Framework

In jurisdiction choice models, a demand for wealthy co-residents can stem either from preferences over public goods or from the fiscal subsidy inherent in the property tax system. The central empirical challenge in identifying the willingness to pay for wealthy co-residents is the correlation between residents' income and the quality of the housing stock and surrounding neighborhood. Homes in wealthy towns are often characterized by larger lot size, newer construction, and so on.

To minimize the bias from unobserved aspects of housing and neighborhood quality, I compare housing units on opposite sides of jurisdiction borders (Black, 1999; Kane, Staiger and Samms, 2003; Figlio and Lucas, 2004). Both the tax base and the composition of the local electorate change discretely at these borders. The necessary identifying assumption in the cross-section is that neighborhood and housing quality shift continuously across borders. I also follow borders over time in order to assess whether an existing housing price gap widens as the income gap between a city and its suburbs grows. The panel analysis is discussed in Section IV.D.

In the cross-section, I test whether differences in housing values across city-suburban borders are associated with differences in town-level characteristics. I collect data on housing values for Census blocks adjacent to jurisdiction borders. The data collection process is described in more detail in the next section. The dependent variable is either the mean value of owner-occupied units or the mean rent for rental units at the block level (*price*).[3] The explanatory variables of interest are the median income or poverty rates of the jurisdictions in which these blocks are located. Pooling data from 1960-1980, I estimate:

$$\ln(\text{price}_{ibjt}) = \beta(\text{poverty rate})_j + \Phi'(\text{block})_{it} + \Psi'(Z_b \cdot Y_t) + \varepsilon_{ibjt} \quad (1)$$

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 a city and the other a suburb.

To clarify geographic terms further, Figure 1 presents a schematic illustration of two border areas in the Los Angeles metropolitan area. The first border area divides Los Angeles

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

from Santa Monica, CA, and the second divides Los Angeles from Torrance, CA. Each border is represented as a pair of neighborhoods or Census tracts (though, in reality, border areas often consist of more than one such pair). Nested within each neighborhood is a grid of blocks. Most of the analysis will concern only the first "tier" of blocks that are themselves adjacent to the jurisdiction boundary. Of particular note are the two distinct geographies into which blocks fit. On the one hand, all blocks in the city of Los Angeles are classified as part of the same jurisdiction ($j=1$), regardless of their physical location within the city. On the other hand, all blocks on the Los Angeles-Santa Monica border are classified as being part of the same border area ($b=1$), even though they fall into two jurisdictions.

The estimating equation contains a separate vector of border area dummy variables in each calendar year ($Z_b \cdot Y_t$). This vector captures unobserved characteristics that are shared by houses on both sides of the border at a point in time – for example, the presence of a nearby park, a bus line, or a commercial strip. Within a border area, the poverty rate can only vary across the border. A negative β suggests that houses located in the poorer of the two jurisdictions command systematically lower prices than their cross-border neighbors.

Some specifications also add a limited set of block-level characteristics (block_{it}). Block controls include the average number of rooms in housing units on the block, the share of units that are owner-occupied or in single family structures, the share of residents on the block who are black, and the number of residents per unit (density). Due to confidentiality concerns, housing prices or rents are only published for blocks containing five or more owner-occupied or rental units. Appendix Table 1 presents means and standard deviations of block and jurisdiction level variables.

III. Collecting Housing Prices Along Jurisdictional Borders

I use block-level Census maps to identify a sample of city-suburban borders and to classify blocks within these border areas according to their distance from the jurisdiction boundary. To be included in the analysis, borders must: (1) have available block data on both sides, (2) contain at least 10,000 residents in both jurisdictions, and (3) not be entirely obstructed by a railroad, four-lane highway, body of water, or large tract of industrial land.[4] The size restriction ensures the availability of town-level information on government expenditures and property tax rates. I also exclude southern borders because African-Americans, who constituted a large share of the poor, did not have a secure right to vote in the South until 1965.

In 1960, the Census Bureau assigned blocks only in central cities and a few large suburban areas. In this year, I identify 56 borders in 16 metropolitan areas that meet the sample criteria.[5] From these borders, I construct a balanced panel that can be followed from 1960-80. In 1970, the Census Bureau expanded block coverage to smaller suburbs, which allows for the inclusion of 46 additional city-suburban borders in 1970 and 1980.[6]

Table 1 lists the metropolitan areas that contribute borders to the sample. The first column demonstrates the geographic distribution of the 56 borders in the balanced panel. The sample is clearly not representative of the urban United States. Large, fragmented cities with

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[4] 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.

[5] To maintain the size of the panel sample, I do not restrict my attention to city-suburban borders in this year, but also include 15 borders that divide two suburbs (e.g., Cambridge-Somerville, MA).

[6] The number of borders in the sample is small relative to the total number of divisions in urban areas. I identified 925 jurisdiction borders in the 16 metropolitan areas that contribute to the panel sample, over 700 of which divided two suburbs. Of the 168 city-suburban borders in these metropolitan areas, 107 included a suburb with 10,000 or more residents and 78 were clear of any obvious obstruction. These 78 borders are included in the sample (56 in the panel sample and 22 from the expanded sample). The average central city in these metropolitan areas bordered on 10.5 suburbs.

populous suburbs are more likely to be included. Borders in the two metropolitan areas – Los Angeles-Orange County and New York-Northern New Jersey – account for nearly 50 percent of the sample. The expanded sample incorporates more geographic diversity, adding smaller college towns like Madison, WI and growing western cities like Las Vegas, NV (column 2). The number of borders in each metropolitan area that were excluded due to the presence of an obstruction are listed in column 3. The full sample consists of 102 borders from 31 metropolitan areas.

The unit of analysis is the Census block. Critical for the estimation is understanding how far each Census block falls from the jurisdiction boundary. Because Census blocks are not digitally mapped for this period, I code blocks by hand according to their distance from the border. I define blocks that are themselves adjacent to the boundary as being the first block "tier." The second block tier are blocks adjacent to the first, and so on. While the majority of the analysis consists only of the first tier of blocks, I collect distance information for up to eight tiers in both directions. I match the distance information with other characteristics of the block, including housing prices and quality measures. Block data are available electronically in 1970 and 1980, but must be entered by hand for 1960.[7]

IV. The City-Suburban Income Gap and the Demand for Suburban Residence

A. Testing for Differences in Observed Housing Attributes Across Borders

Drawing inferences from a comparison of housing prices depends on the assumption that the housing units and immediate neighborhoods under consideration are of equal quality. In this section, I show that the clear differences in demographics and housing quality between Census

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

tracts on either side of the border disappear once the sample is narrowed to neighboring Census blocks.

Table 2 presents a comparison of housing quality across jurisdiction borders for the owner-occupied sub-sample.[8] Each cell represents a separate regression of a housing quality measure on the jurisdiction-level poverty rate in 1970. We expect the housing stock on the poorer side of borders to be composed of fewer single family units that are also smaller and less likely to be owner-occupied. Indeed, I find that Census tracts located on the poorer side of jurisdiction borders contain fewer single family units. A one standard deviation increase in poverty reduces the share of single family units at the tract level by 2.5 percentage points. In contrast, at the block level, this difference falls to 0.6 points and is no longer significant. The same pattern holds for owner occupancy and density (residents per unit). Because zoning regulations often target multi-family dwellings and high-density development, these contrasts provide *prima facie* evidence against the possibility that zoning generates discontinuous changes in the housing stock across borders.[9]

The one observable difference in housing quality at the block level is unit size. A one standard deviation increase in the poverty rate is associated with 0.1 fewer rooms at the tract level and 0.07 fewer rooms at the block level. On average, an additional room increases the value of a housing unit by 20 percent, implying that even this small difference in unit size would generate a 1.4 percent price gap across borders. It will be important to control for the average number of rooms on the block in the main analysis. While the Census of Housing collects other housing attributes (for example, the age of the unit), they are not reported in the published block level data.

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[8] Housing stock differences for the full set of blocks are always smaller than those shown here.
[9] It is important to note that this result may be unique to inner-ring suburbs, where the housing stock at the border was already in place and thus may have been "grandfathered in" during the rise of zoning in the 1950s.

The only demographic measure available at the block level in 1970 is the share of units occupied by a black household head. Some jurisdiction choice models predict that the poor sort into towns with a higher poverty rate. Because race and income are correlated, we might expect to observe more black households on the poor side of jurisdiction borders. This pattern could confound the analysis if homeowners are willing to pay to avoid black neighbors. At the tract level, a one standard deviation increase in poverty increases the probability of having a black neighbor by 2.7 percentage points. However, at block level, this relationship shrinks to 0.4 percentage points and is no longer statistically significant. There is no discernable evidence of sorting at this close range.[10] I will show below that the results are also robust to restricting the sample to blocks or border areas with no black residents.

B. Housing Price Gaps Across Jurisdiction Borders

I turn in this section to the analysis of housing prices across borders. I start in Figure 2 with a simple graphical exercise. Each bubble represents a jurisdiction border in 1970, weighted by the underlying number of blocks.[11] The X-axis indicates the poverty rate gap between the city and its neighboring suburb. The Y-axis depicts the suburban housing premium at each border. The relationship exhibits a positive slope, with a larger poverty gap between a city and suburb associated with a larger suburban housing premium. The pattern does not appear to be driven by

[10] Bayer, Ferreira and McMillan (2007) find a three percentage point difference in black population share between block groups on opposite sides of school attendance boundaries separated by a one standard deviation difference in test scores. Block groups are closer in size to Census tracts than to blocks; the average block group contains 40 blocks. Thus, I view this result as similar to the observed differences in black population share at the tract level here and entirely consistent with a lack of sorting at the block level.

[11] The qualitative pattern is unchanged when weighting by the number of underlying housing units or when the borders are unweighted.

outliers. The largest suburban premia occurred at the borders of very poor central cities or very wealthy suburbs (for example, Newark and East Orange, NJ and Detroit and Grosse Point, MI).[12]

Table 3 contains results from a pooled cross-section regression using data from 1960, 1970 and 1980. The housing market is divided into owners and renters with price measured as either housing values or rents. I consider willingness to pay for two aspects of the income distribution of co-residents: median income and poverty rates.[13] Coefficients are normalized to reflect the response to a one standard deviation increase in the city-suburban difference in median income (19.8 percent) or poverty rates (4.6 points).

Homeowners were willing to pay 6.7 percent more for a housing unit in a suburb whose median income is one standard deviation higher than the neighboring city (column 1). Adding block-level racial composition does not affect this result. In contrast, adding housing quality controls (particularly the average number of rooms) reduces the estimated willingness to pay for wealthy co-residents to 3.9 percent. The fourth column looks for changes in the willingness to pay for town-level attributes over time. The price for a one standard deviation increase in median income increased from around 3.3 percent in 1960 and 1970 to 4.8 percent in 1980; this difference is significant at the 10 percent level.

Cities with a lower median income also tend to have a higher black population share. However, adding jurisdiction-level racial composition has no effect on the demand for wealthy co-residents (column 5). Considered alone, homeowners appear willing to pay to avoid racial diversity at the town level. Yet, when income and race are included together, the response to a one standard deviation increase in the black population share is actually *positive* (though small)

[12] Newark, NJ, which had an 18.4 percent poverty rate in 1970, is the poorest city in the sample. Grosse Point, MI is the wealthiest suburb (poverty rate = 1.1 percent).

[13] The concept of an absolute "poverty line," which takes into account family size and the ages of family members, was developed in the 1960s. Thus, the poverty rate regressions include only 1970 and 1980.

and not statistically significant (coeff. = 0.006; s.e. = 0.010). While there may not be enough independent variation in the sample to deem a town's racial composition unimportant, it is clear that the income results are robust to the inclusion of black population share.

The final column in Table 3 considers the effect of median income on rents. Rental prices are not as responsive to town-level median income – compare a 2.3 percent increase in rents to a 3.9 percent increase in values for a one standard deviation increase in median income. The weaker response may be due to the composition of the rental market; renters tend to be younger, less well-off, and less likely to have children. Unlike rents, housing prices might also incorporate expectations of future divergence between cities and their suburbs. Furthermore, the presence of rent control in some urban areas could limit the ability of the rental market to adjust through prices, leading to adjustments in quantities. Indeed, I find that a lower median income or higher poverty rate is associated with a higher vacancy rate among rental units but has no effect on the share of owner-occupied units that are vacant and for sale (not shown).

C. Placebo Estimation

Thus far, I have found that housing prices are higher in wealthy suburbs relative to their poorer central city neighbors. Furthermore, there are no observable differences in housing quality, with the exception of small differences in unit size, that could explain this pattern. Yet, there may be unobservable differences in housing quality that could explain this pattern. One possibility is that housing quality continuously improves with distance from an impoverished urban core. In this case, the estimated housing price gap at the border could be picking up a small move along this quality gradient. Alternatively, housing quality could improve discretely at the border due either to differences in zoning regulations or to the sorting of population into towns in a manner correlated with the propensity to engage in home renovation and maintenance. I will consider each of these possibilities in turn.

The first scenario holds that housing quality slowly improves with distance from the city center. In this case, there would be nothing unique about crossing the border; rather, the price estimate would simply reflect an improvement in housing quality along this gradient. If so, I should find similar coefficients in a regression that compares housing prices across a "placebo" border that is shifted one block into the poorer or the wealthier jurisdiction in each pair. Table 4 presents results from two alternative borders. For comparison, the first row reproduces the coefficients from the main specification (Table 3, column 3). The second row imagines shifting the border one block into the poor jurisdiction, thereby assigning the characteristics of the wealthier jurisdiction to the first block tier on the poor side. The third row conducts an analogous procedure by shifting the border into the rich jurisdiction. If the estimate at the actual border only reflected a move along a gradient of unobserved housing quality, we would expect negative coefficients of a similar magnitude for each of the placebo experiments. The true estimates stand out for being significantly different from zero and at least three larger than any of the alternatives.

D. Panel Estimation

While the placebo estimation rules out the possibility that the cross-border results can be explained by continuous improvements in housing quality, this method does not address the prospect of a discontinuous jump in housing quality at jurisdiction borders.

By exploiting the panel nature of the dataset, we can control for time-invariant differences in housing quality across borders. For any fixed price gap across borders, we can observe how relative housing prices *evolve* as the city-suburban income gap narrows or widens over time. I pool data for 1960-1980 and estimate:

$$\ln(\text{price}_{ijbt}) = \beta(\text{poverty rate})_{jt} + \Phi'\text{block}_{it} + \Psi'(Z_b \cdot Y_t) + \Omega'(Z_b \cdot J_j) + \varepsilon_{ijbt} \qquad (2)$$

As before, equation 2 estimates common border area terms that vary by Census year ($Z_b \cdot Y_t$). The added interaction between border area and jurisdiction estimates a separate fixed effect for the neighborhoods on each side of the border ($Z_b \cdot J_j$). In other words, this interaction allows for a fixed difference in housing quality on across borders. β is now estimated from *differential changes* over time in cities relative to their suburban neighbors.

The effect of local zoning ordinances is likely to be absorbed by this procedure given the slow-changing nature of regulatory regimes. However, it is important to note that this method cannot control for differential rates of neighborhood deterioration across borders. The omitted interaction between border area, jurisdiction, and Census year, which would allow each side of the border to improve or deteriorate at its own pace, would fully saturate the model.

Table 5 presents results from panel regressions of housing values on jurisdiction-level median income and poverty. Estimates should be compared with the third column of Table 3, which includes block-level demographics and housing quality controls. If the results merely reflected unobserved differences in housing quality, we would expect the panel coefficients to be smaller than the cross-section. Instead, the willingness to pay for richer co-residents appears to be *larger* in the panel, particularly for median income (6.8 percent versus 3.9 percent for a one standard deviation increase in median income).There is little change in the response to poverty rates (3.4 percent versus 3.3 percent). This finding holds in both the balanced panel and in the full sample and is robust to adding racial composition at the jurisdiction level (not shown). This finding casts doubt on the possibility that the main result can be explained by a fixed difference in housing quality across borders.

E. Robustness Checks

Table 6 conducts a series of additional robustness checks for the relationship between housing values and town characteristics. While I present the 1970 owner-occupied sample here, results are similar both in other years and for the sample of rental units.

The main specification, which is conducted at the block level, gives more weight to longer border areas. In the second row of Table 6, which weights each border area equally, the coefficient is nearly unchanged. Another concern is that the average price information for each block is treated as if it were calculated with equal precision. 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 297 (median = 21.1). In this specification, the relationship between housing prices and town-level income increases slightly, but is qualitatively similar.

Town-level median income may be serving as a proxy for the racial composition of residents in the immediate neighborhood. I address this concern in two ways. The fourth row of Table 6 excludes the 14 border areas that had a large black population in 1970, defined as ten percent or more of the border area's residents on either side. 39.6 percent of household heads along these 14 border areas were black, compared to 0.7 percent in the remainder of the sample. Many of these areas became majority black by 1980.[14] The fifth row excludes any blocks that themselves had black residents (21.1 percent of the sample). If town-level income is a proxy for

[14]These transition areas include the well-known black enclaves of Compton-Long Beach, CA; Inglewood-Los Angeles, CA, Irvington-Newark, NJ; and St. Louis-University City, MO.

neighborhood composition, we would expect smaller coefficients in these predominately white samples. In both cases, the coefficients are unchanged.[15]

Together, Los Angeles and New York account for one third of the full sample. In the last rows of Table 6, I re-run the regressions while dropping first the Los Angeles and then the New York City borders. The results are not sensitive to this omission, nor are they sensitive to dropping both large metropolitan areas simultaneously (not shown).

V. The Demand for Rich Co-Residents and the Suburban Multiplier

The previous section documents that, in the 1960s and 1970s, the marginal resident was willing to pay four percent more for a housing unit located in a suburban town whose median resident earned $10,000 (in $2000) more than the median city resident. What can we learn from this price estimate about the effect of urban income divergence on post-war suburbanization?

Consider the case of Detroit, MI. While in 1940 the median resident in central city Detroit earned only 2 percent less than the median resident in the suburban ring, by 1970 the income gap had increased to 21 percent. As a result, Detroit's housing prices would have fallen by 4 percent relative to the suburbs. This price decline reflects lower demand for – and thus mobility away from – the center city. To assess *how much* mobility is implied by a price effect of this magnitude, we need an estimate of the short-run elasticity of housing prices with respect to population growth.[16] Estimates in the literature range from 0.65 to 0.9 (Potepan, 1994; Saiz,

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[15] The same result is found with a looser definition of racial transition (five percent black in 1970).

[16] In the long run, housing supply can respond to changes in population, and prices may return to previous levels. However, the durability of the housing stock implies that the housing supply response is asymmetric with respect to population gains versus population losses (Glaeser and Gyuorko, 2005). Given that population losses from central cities do not often result in destruction of the existing housing stock, the long-run elasticity of prices with respect to population may still be large and positive.

2003; Saiz, 2007).[17] At these parameter values, a 4 percent decline in urban housing prices would follow from a 2.6 to 3.6 percent decline in city population.

The population of central city Detroit *actually* fell by 6.8 percent over this period (from 1.62 million to 1.51 million). According to the more conservative estimate, 38 percent of this population loss was due to a widening city-suburban income gap. However, Detroit may not be the typical case. Of the other central cities in the sample, 17 gained population from 1940 to 1970 and 13 lost population. On average, 26 percent of the population loss experienced in the declining cities can be attributed to city-suburban income divergence. Expanding cities would have grown 18 percent faster if not for their expanding city-suburban income gap.

Not only does income stratification generate its own motivation for suburbanization, but it can also intensify the response to other urban changes. A major cause of suburbanization during this period was the construction of the interstate highway system. If households that left the city due to new roads earned more than the median resident, the resulting increase in the urban income gap would have augmented the response to new infrastructure. Baum-Snow (2007) estimates that, in the average metropolitan area, the construction of one new highway through the urban center caused city population to fall by 18 percent from 1950 to 1990. How much additional mobility would have been generated by a suburban multiplier of this kind? Returning to the case of Detroit, 1.8 million people lived in the central city in 1950. Assuming that population loss occurred uniformly through the period, Detroit would have lost 166,000 people by 1970 for each new highway built. For simplicity, I also assume that the earnings of all of the departing households would have been above the city's median income and all households would

[17] Population change can be due either to natural increase or to migration. Because natural increase does not exhibit much variation across places, estimates of this elasticity are based on the housing price response to migrant arrivals. Potepan (1994) focuses on internal migrants, while Saiz (2003, 2007) considers international migrants. Understanding how housing prices respond to population change is complicated by the fact that migrants can be attracted by low housing prices. Saiz (2007) represents the most convincing attempt to address this reverse causality.

have resettled in Detroit's suburban ring. These assumptions provide an upper bound on the power of the suburban multiplier. In 1950, the median income of urban and suburban Detroit were nearly identical (\$22,050 and \$22,750). A loss of 166,000 residents from a city of 1.8 million, all from above the median income, would have shifted the new median city income to the 41st percentile of the old income distribution (\$21,350). In a similar fashion, a gain of 166,000 residents to a suburban ring of 1.2 million would have shifted the new median income to the 64th percentile of original income distribution (\$24,850). As a result, the city-suburban income gap would have grown from three percent to 16 percent.

According to the willingness to pay estimates in Section IV, an income gap of this size would lead to a 6.5 percent decline in relative city housing prices. A conservative estimate of the elasticity of housing prices with respect to population suggests that a price decline of this magnitude was driven by a 4.2 percent loss in population. In other words, the initial nine percent population loss due to the construction of a new highway would have generated a further 4.2 percent loss in population for its role in expanding the city-suburban income gap. In this upper bound scenario, this feedback effect increased the suburbanization response to a transportation innovation by 50 percent. This example illustrates that a suburban multiplier can have a quantitatively important effect on the fate of cities.

VI. The Role of Public Goods in the Demand for Rich Co-Residents

I have shown that the demand for suburban residence increases with the city-suburban income gap and that the preference for living in a wealthy town can generate a suburban multiplier. While this finding does not depend on understanding *why* households prefer rich co-residents, investigating these channels can be of independent interest. Unlike the desire to live in a wealthy neighborhood, which may be due to local social interactions, the desire to live in a wealthy town is driven by civic interactions through public schools, the electoral process, or the fiscal system. In this section, I consider a series of local policies that may account for the demand for wealthy co-residents.

I compile town-level data on effective property tax rates and expenditures on education, public safety, and other categories. Systematic measures of school quality are unavailable during this period.[18] To explain the demand for rich co-residents, a policy measure must be correlated with jurisdiction-level median income. I test this condition in the first panel of Table 7. Wealthy towns set lower property tax rates than their cross-border neighbors (column 1). A one standard deviation increase in median income is associated with a 0.5 percentage point reduction in the tax rate. Wealthy towns also spent less than poor central cities on non-educational functions, particularly on public safety (columns 4-5). In contrast, total expenditure per pupil, as well as sub-categories of educational expenditure (administrative, instructional), did not differ across jurisdiction boundaries (columns 2-3), nor did spending on fire protection, park and road maintenance, or sanitation (not shown).[19]

For a local policy to explain the demand for rich co-residents, the policy *itself* must be something for which residents are willing to pay. Not surprisingly, homeowners would pay more to live in a jurisdiction that levied lower property tax rates (Panel B).[20] Adding the property tax rate to the main specification reduces the coefficient on median income by 20 percent (Panel C).

[18] The data sources and definitions of the local policy measures are described in Appendix Table 2.

[19] While school districts in rich towns spend more locally-raised revenue on education, state and federal transfers to poor districts make up this difference.

[20] Home values fall by 3.3 percent for every point increase in the property tax rate. A one standard deviation increase in median income reduces the property tax rate by 0.5 points. By this measure, a homeowner would break even after four years by purchasing a more expensive home in the jurisdiction with the lower tax rate (assuming a five percent discount rate).

While lower property tax rates explain part of the demand for wealthy suburbs, the majority cannot be attributed to this fiscal subsidy.

Turning to public expenditures, I find that homeowners particularly dislike spending on police (Panel B). Residents likely faced similar victimization rates across jurisdiction borders, but incurred very different obligations to pay for police services. These estimates reflect the desire not to pay to police someone else's neighborhood. Adding police expenditure or total non-educational expenditure to the main specification explains an additional one third of the suburban premium (Panel C).

Adding both property tax rates and non-educational expenditures simultaneously reduces the coefficient on median income by over 50 percent (column 6). The remaining demand for wealthy co-residents could reflect aspects of school quality that are not being measured by per-pupil spending. Black (1999) and Bayer, Ferreira and McMillan (2007) provide comparable estimates of willingness to pay for school quality, as measured by elementary school test scores (2.1 and 1.8 percent of housing value, respectively, for a one standard deviation increase in test scores). A one standard deviation increase in district-level median income increases student test scores by between 16 and 40 percent of a standard deviation, depending on the covariance between income and other socio-economic measures (Driscoll, Halcoussis, and Svorny, 2003). At the high end of this range, variation in school quality across borders separated by one standard deviation in median income would generate an 0.85 percent difference in housing values, accounting for nearly all of the remaining unexplained price gap (1.2 percent; Table 7, column 7).

Taken together, these results suggest that the desire to live in a wealthy town stems from three main factors: lower property tax rates set by jurisdictions with a higher tax base; lower expenditures on public safety in towns that are fiscally independent from the urban core; and higher school quality, despite equal expenditures per pupil, in wealthier districts. These motivations for suburban mobility exist above the value placed on wealthy neighbors and can explain the desire to sort across jurisdiction lines, rather than simply across neighborhoods within the central city.

VII. Willingness to Pay to Avoid School Desegregation

In Section IV, I show that, in the 1960s and 1970s, homeowners were unwilling to pay for a unit located in a predominately white town. However, the advent of school desegregation plans may have changed the costs associated with town-level racial composition. By the late 1960s, southern school districts were already in the process of dismantling systems of *de jure* segregation (Cascio, et al., 2007). However, it was not until the 1973 *Keyes* decision that the Supreme Court ruled against the *de facto* segregation of northern schools. According to this ruling, school districts could be forced to desegregate even if existing segregation was the outcome of residential patterns rather than deliberate district policy.

Following the *Keyes* decision, a number of urban districts in the border sample received court-orders to desegregate their schools. Prior to this episode, school assignments were based on residential location. Given high levels of neighborhood segregation, white children often had predominately white classmates, even in cities with a large black community.[21] Desegregation increased the probability that children shared classrooms with opposite-race peers (Reber, 2005). The prospect of mixed-race classrooms raised concerns for some parents, either directly or

[21] The school districts in the panel sample were highly segregated in 1970. The mean dissimilarity index at the elementary school level is 0.51. 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.

because of the correlation between race and levels of student preparation (Hoxby and Weingarth, 2005). In addition, some desegregation remedies reassigned children to schools across town, a further cost for the many parents who prefer neighborhood schools (Bogart and Cromwell, 2002).

Reber (2005) and Lutz (2005) show that white enrollments declined in urban districts following desegregation. These losses were due to some combination of suburban mobility and substitution toward private schools (Baum-Snow and Lutz, 2008). If desegregation reduced the demand for living in diverse central cities, I expect to find a housing price gap emerge at borders dividing court-ordered urban districts from their suburban neighbors (Clotfelter, 1975).

I collect detailed information on court decisions from the *State of Public School Integration* website (Logan, 2004). The presence of a court order is quantified in two ways: a continuous variable counting any remedial steps in the order, without regard to their intensity, and a dummy variable indicating that the court order required busing and student reassignment. 65 borders contain at least one district with a desegregation-related court case.

I interact the desegregation variables with decade fixed effects and estimate:

$$\ln(\text{price}_{ijbt}) = \Gamma_1[\text{Deseg}_j \cdot \text{Y}_t] + \beta(\%\ \text{black})_{jt} + \Phi'(\text{block})_{it} + \Psi'(\text{Z}_b \cdot \text{Y}_t) + \varepsilon_{ibjt} \qquad (3)$$

Desegregation plans were phased in over the 1970s. If the interactions are negative and significant in 1960 and 1970, before desegregation occurred, we can infer that there was a pre-existing housing price gap along borders that eventually received a desegregation order. Comparing the 1980 interaction with either the 1960 or the 1970 interactions reveals the price response to desegregation.[22] The specification also controls for the town-level black population share because the probability that a district faced litigation increases with the number of black students who could serve as plaintiffs.

The first column of Table 8 reports coefficients for the continuous measure of desegregation. Housing prices in treated districts were 1.6 percent lower than their cross-border neighbors for every remedial step included in the desegregation order. The average order required 2.2 remedial steps, resulting in a 3.5 percent reduction in housing values. The willingness to pay to avoid busing was higher than the price attached to the average desegregation plan. In 1980, housing prices in districts required to bus their students were 7.9 percent lower than their cross-border neighbors.[23] The pre-period coefficients indicate that housing units in cities that would eventually initiate busing programs were already worth 1-2 percent less than their suburban neighbors in 1960 and 1970. This initial price gap grew by 6.1 percentage points from 1970 to 1980. The second panel of Table 8 controls for town-level median income as well. Income differences absorb all of the pre-period differences between neighboring districts with and without desegregation. In this specification, busing reduces housing values by 5.3 percent.

School desegregation changed the relationship between a city's racial composition and the demand for suburban residence. Before the advent of desegregation, households were attracted to the suburbs by the fiscal advantages of living in a town with rich co-residents. Conditional on the racial composition of one's own neighborhood, the fact that suburban residents were also predominately white was not of independent interest. School desegregation

[22] Baum-Snow and Lutz (2008) show that most of the urban households prompted to move following school desegregation lived in the outer rings of the central city nearest to the suburbs. By comparing housing prices across jurisdiction boundaries, this procedure may overstate mean willingness to pay to avoid desegregation.
[23] Bogart and Cromwell's (2002) estimate a 9.9 percent decline in housing values following a student re-assignment and the loss of neighborhood schools in Shaker Heights, OH.

added a new cost to living in a racially diverse jurisdiction, resulting in further mobility to the suburbs (as indexed by relative housing prices).

VIII. Conclusion

Road building projects and the diffusion of the car made it economically feasible for the first time 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. The marginal homeowner was willing to pay around four percent more for an otherwise equal unit located in a suburb whose median income was one standard deviation higher than the neighboring city. Part of this premium reflects the fact that, 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. The implementation of school desegregation plans in the 1970s provided another reason to leave racially diverse central cities. Homeowners were willing to pay an additional 5.3 percent more for a housing unit to avoid desegregation plans that included student reassignment and busing.

If a larger income gap itself generates suburbanization, cities can enter a vicious cycle of population loss and urban decline. I show that the estimated demand for rich co-residents can augment the response to a given transportation shock by up to 50 percent. A feedback effect of this nature can help to explain the sharp declines in city fortunes at mid-century. Cities were bleeding both population and tax base in the 1960s, leading in some cases to acute fiscal crisis.

Federal and state governments stepped in to aid ailing cities – most notably to prevent New York City from defaulting in 1974. More speculatively, we might expect that this multiplier could work in the opposite direction as well. It remains to be seen whether rising incomes in some downtown areas, spurred by educated young workers and wealthy empty-nesters, could form the basis of an urban revival.

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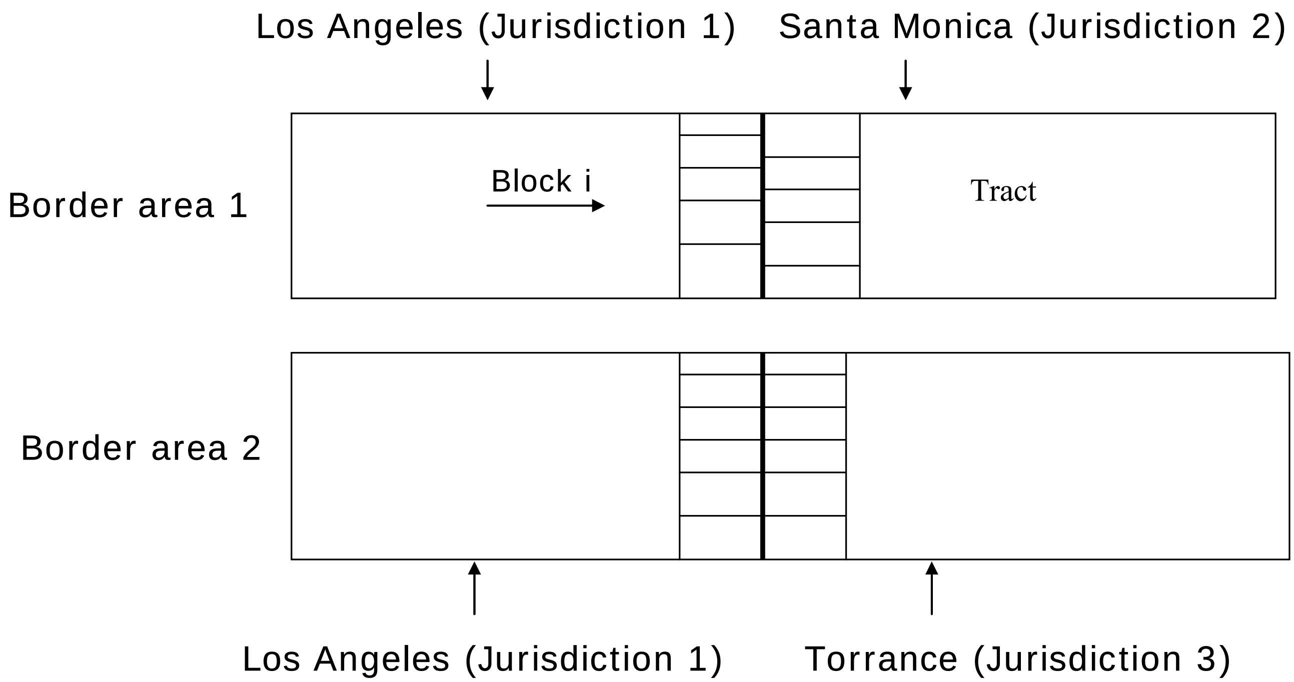
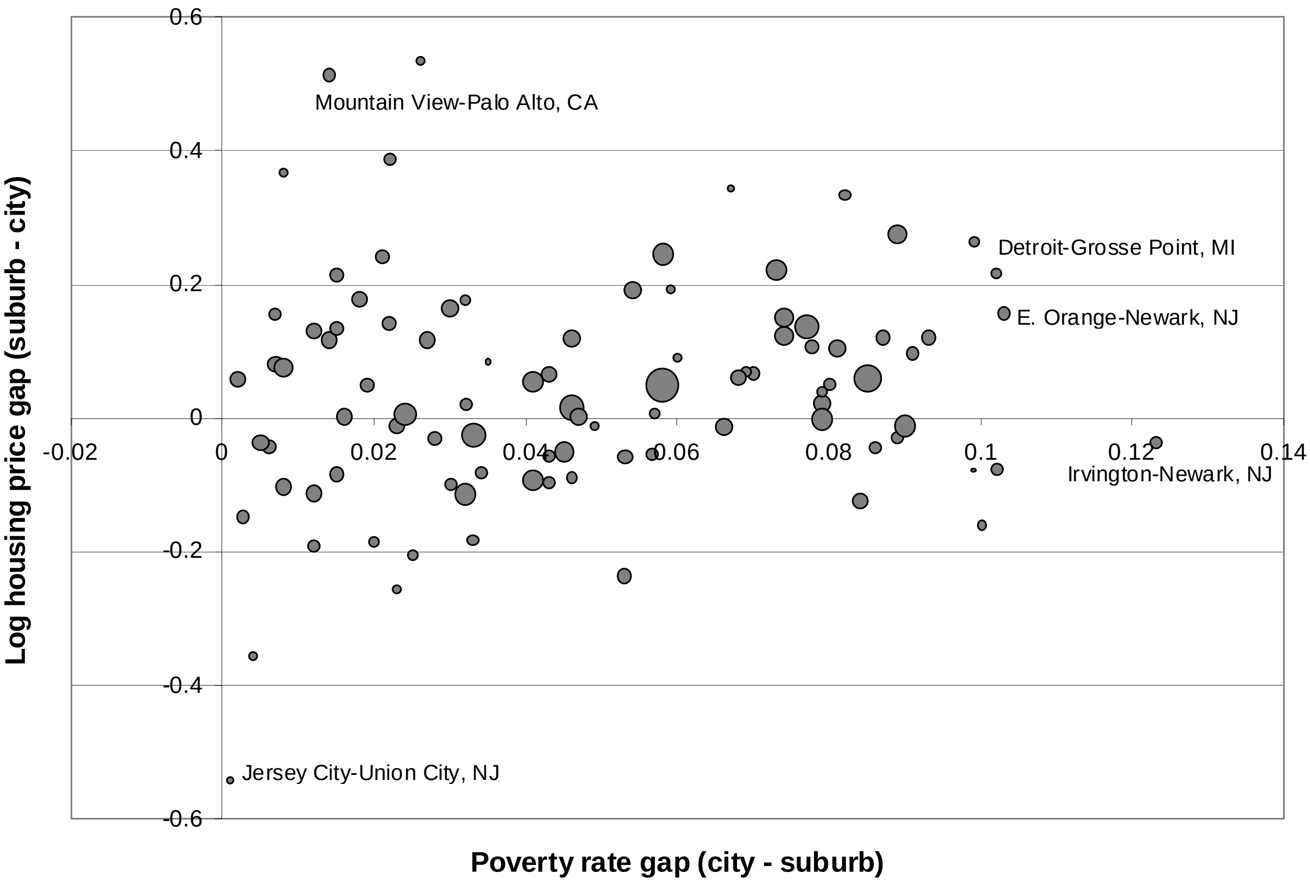
Figure 1: Schematic diagram of geographic terms



where i = block; b = border area; j = jurisdiction

Figure 2: The suburban housing premium and the city-suburban poverty rate gap, 1970



Notes: Each bubble represents one of the 102 jurisdiction borders in the full sample weighted by the underlying number of blocks with five or more owner-occupied units. Denver-Lakewood, CO contributes the most blocks to the analysis (464) and Cleveland-Cleveland Heights the fewest (3).

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

Region	Metropolitan area	Number of borders Panel sample	Full sample	Excluded
Northeast	Allentown-Bethlehem, PA		2	
	Boston, MA	2	1	4
	Hartford, CT		3	2
	New York, NY-NJ†	10		3
	Pittsburgh, PA	3		
	Providence, RI	3	1	
	Scranton, PA		1	
	Springfield-Chicopee, MA		1	1
Midwest	Akron, OH		2	2
	Canton, OH		1	
	Chicago, IL†	5	2	6
	Cleveland, OH	2		
	Dayton, OH	1		
	Des Moines, IA		2	
	Detroit, MI	1	6	
	Grand Rapids, MI		4	
	Indianapolis, IN		1	3
	Kansas City, KS-MO	2	2	3
	Madison, WI		1	
	Minneapolis/St. Paul, MN	1	1	3
	Moline-Davenport, IL-IA	1	1	
	South Bend, IN		1	
	St. Louis, MO	1		4
West	Denver, CO	1	2	
	Las Vegas, NV		1	
	Los Angeles, CA†	17	5	7
	Phoenix, AZ		1	1
	Portland, OR		2	1
	San Bernard.-Riverside, CA		1	3
	San Francisco, CA†	2	1	
	San Jose, CA	4		
	TOTAL:	**56**	**46**	**44**

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: Comparing housing quality and neighborhood demographics across jurisdiction borders at the Census tract and block level, 1970

		Effect of a one standard deviation increase in the cross-border poverty rate gap	
Dependent variable	Mean/SD	Tract	Block
Share single family	0.592	-0.025	0.006
	(0.284)	(0.007)	(0.013)
Share owner occupied	0.591	-0.024	-0.001
	(0.236)	(0.006)	(0.015)
Residents/unit	3.080	0.027	0.016
	(0.840)	(0.015)	(0.029)
Rooms/unit	5.047	-0.101	-0.069
	(0.784)	(0.025)	(0.032)
Share >=3 bedrooms	0.444	-0.017	---
	(0.215)	(0.006)	
Share built after 1960	0.205	-0.011	---
	(0.203)	(0.005)	
Share black	0.108	0.027	0.004
	(0.249)	(0.010)	(0.006)

Notes: The means and standard deviations in Column 2 are calculated at the tract-level. The cells in columns 3 and 4 contain coefficients from separate regressions of a housing quality measure on jurisdiction-level poverty rates at either the tract or the block level. Standard errors are clustered by border area. Both coefficients and standard errors are normalized to reflect the response to a one standard deviation increase in the city-suburban poverty rate gap. Regressions also include a vector of border area dummy variables. Tract regressions contain 607 observations. Block regressions are restricted to blocks with at least five owner-occupied units and contain 2561 observations. Data on the number of bedrooms and unit age are not available at the block level.

Table 3: Willingness to pay for wealthier co-residents at the jurisdiction level
Pooled cross section for 1960-80

Effect of a one standard deviation increase in jurisdiction-level measure

	ln(value of owner occupied units)					ln(rent)
	(1)	(2)	(3)	(4)	(5)	
Panel A						
ln(median income)	0.067	0.065	0.039		0.045	0.023
	(0.011)	(0.012)	(0.008)		(0.012)	(0.010)
ln(med. income), 1960				0.033		
				(0.017)		
ln(med. income), 1970				0.031		
				(0.006)		
ln(med. income), 1980				0.048		
				(0.011)		
Share black on block	N	Y	Y	Y	Y	Y
Housing controls	N	N	Y	Y	Y	Y
Share black in town	N	N	N	N	Y	N
Panel B						
Share poverty	-0.049	-0.045	-0.033		-0.037	-0.010
	(0.011)	(0.011)	(0.008)		(0.011)	(0.006)
Share poverty, 1970				-0.024		
				(0.008)		
Share poverty, 1980				-0.038		
				(0.010)		
Share black on block	N	Y	Y	Y	Y	Y
Housing controls	N	N	Y	Y	Y	Y
Share black in town	N	N	N	N	Y	N

Notes: Each column represents a separate regression, the dependent variable of which is the logarithm of either housing values or rents. Regressions include border area dummy variables interacted with calendar year. Standard errors are clustered by border area. Reported coefficients and standard errors are normalized to reflect the response to a one standard deviation increase in the city-suburban income or poverty rate gap.
The sample is restricted to blocks adjacent to the jurisdiction border. The value regressions contain blocks with at least five owner occupied units (N = 6063 in panel A and 4518 in panel B). The rent regressions contain blocks with at least five rental units (N= 4028 and 3003).
The housing quality controls include: the shares of housing units that are in single-family structures or are owner-occupied; the average number of rooms by tenure status; and the number of residents per unit (density).

Table 4: Placebo estimation for pooled cross-section, 1960-80

Dependent variable = ln(value of owner occupied units)
Effect of a one standard deviation increase in jurisdiction-level measure

	ln(median income)	Share poverty
Actual border	0.039	-0.033
	(0.008)	(0.008)
Poor side: Tier 1 vs. Tier 2	0.011	0.000
	(0.013)	(0.009)
Wealthy side: Tier 1 vs. Tier 2	-0.003	0.002
	(0.011)	(0.011)

Notes: Standard errors are clustered by border area and reported in parentheses. Regressions include blocks adjacent to the jurisdiction border. Specification details are described in the notes to Table 3.

Table 5: Panel estimation: Do changes in the city-suburban income gap lead to changes in the suburban price premium?

Dependent variable = ln(housing values)

	ln(median income)	Share poverty
Panel sample	0.074	-0.042
N = 4441	(0.029)	(0.022)
Full sample	0.068	-0.034
N = 6063	(0.027)	(0.019)

Notes: Standard errors are reported in parentheses and clustered by border area. All regressions include border area dummy variables interacted with both Census year and jurisdiction. The sample is restricted to blocks adjacent to the jurisdiction border. Regressions include block level controls that are described in the notes to Table 3.

Table 6: Robustness: Housing values and jurisdiction-level median income, 1970

Dependent variable = ln(housing values)
Effect of a one standard deviation increase in jurisdiction-level measure

	ln(median income)
1. Baseline	0.023
N = 2573	(0.005)
2. Weight by inverse of # blocks	0.021
	(0.006)
3. Weight by # houses	0.032
	(0.005)
4. Non-transition borders	0.023
N = 2045	(0.006)
5. Blocks with no black residents	0.025
N = 2045	(0.007)
6. Blocks with at least one black resident	0.017
N = 528	(0.013)
7. Without Los Angeles	0.025
N = 1780	(0.006)
8. Without Greater New York area	0.021
N = 2360	(0.006)

Notes: Standard errors are clustered by border area and reported in parentheses. Regressions only include blocks adjacent to the jurisdiction border. Sample sizes associated with the various restrictions in rows 4-8 are reported. Specification details are described in the notes to Table 3.

Table 7: Does variation in local policy explain the demand for rich co-residents?

Panels A and C: Effect of a one standard deviation increase in ln(median income)

	Tax bill as % of value	$ per pupil, administration	$ per pupil, instruction	$ per capita (non-education)	Police $ per capita	Tax bill & police $
A. DV = Policy measure						
ln(median income)	-0.532	-0.009	-0.033	-0.270	-0.042	---
	(0.073)	(0.015)	(0.114)	(0.088)	(0.007)	
Observations	127	194	194	194	194	
Borders	65	97	97	97	97	
B. DV = ln(housing values)						
Policy	-0.033	-0.303	0.035	-0.039	-0.337	---
	(0.010)	(0.183)	(0.015)	(0.013)	(0.096)	
Observations	1815	2424	2424	2424	2424	
Borders	65	97	97	97	97	
C. DV = ln(housing values)						
ln(median income)	0.026	0.024	---	0.024	0.024	0.028
W/o policy measure	(0.009)	(0.006)		(0.006)	(0.006)	(0.008)
ln(median income)	0.020	0.022	---	0.018	0.016	0.012
With policy measure	(0.016)	(0.009)		(0.007)	(0.008)	(0.012)
Observations	1815	2424	2424	2424	2424	1631
Borders	65	97	97	97	97	61

Notes: Standard errors are reported in parentheses and clustered by metropolitan area. Notes on and sources for the public goods measures are in Appendix Table 2. In Panel A, the unit of observation is the jurisdiction by border area. Regressions include a vector of border area dummy variables and the policy variable listed in the first row. For panel B, the unit of observation is the Census block. The sample is restricted to blocks adjacent to the jurisdictional border. These regressions include a vector of border area dummy variables, the block-level controls listed in the notes to Table 3, and the town-level policy variable listed in the first row. The regressions underlying panel C have the same structure. However, in the first row of panel C, the policy variable is excluded and instead the only town-level variable is residents' median income.

Table 8: Willingness to pay to avoid school desegregation
Pooled cross section for 1960-80

	Desegregation measure	
	Number of steps in court-order	=1 if includes busing
Panel A: Control for % black		
1960	0.004	-0.010
	(0.005)	(0.018)
1970	-0.004	-0.018
	(0.002)	(0.011)
1980	-0.015	-0.079
	(0.004)	(0.023)
Panel B: Additional controls for median income		
1960	0.011	0.002
	(0.004)	(0.017)
1970	-0.001	-0.007
	(0.002)	(0.010)
1980	-0.009	-0.053
	(0.003)	(0.021)

Notes: Standard errors are reported in parentheses and clustered by border area. The sample is restricted to blocks that are adjacent to the jurisdictional border. The desegregation variables are based on court-orders reported on the *State of Public School Integration* website at Brown University. Regressions include block-level housing quality controls described in the notes to Table 3.

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

	1970		1970-80
Mean (S.D.)	All jurisdictions	Difference across borders	Change in cross-border difference over time
Panel 1: Jurisdiction level Demographics			
Share black	0.086 (0.142)	0.151 (0.145)	0.055 (0.068)
Median family income, $ 2000	$49,980 ($10,227)	$9,926 ($8,918)	$2,880 ($2,181)
Poverty rate	0.067 (0.036)	0.046 (0.031)	0.026 (0.025)
Local policy			
Property tax rate, % of sale price (J = 105; B = 65)	2.535 (1.115)	0.723 (0.482)	
In $1,000 ($2000):			
Instruction $ per pupil (J = 138, B = 97)	3.001 (0.652)	0.512 (0.473)	
Admin. $ per pupil (J = 138, B = 97)	0.133 (0.055)	0.044 (0.046)	
Non-educ $ per capita (J = 138, B = 97)	0.736 (0.424)	0.493 (0.431)	
$ on police, per capita (J = 138, B = 97)	0.114 (0.053)	0.066 (0.045)	
$ on sewer, per capita (J = 115 , B = 74)	0.028 (0.044)	0.040 (0.050)	

(table continued…)

Appendix Table 1, continued			
	1960	1970	1980
Panel 2: Block level			
Value sample			
Average value, owned	$104,183	$107,784	$157,805
	(48,508)	(41,892)	(91,666)
Mean # rooms, owned	5.795	5.765	5.434
	(0.981)	(0.856)	(1.035)
Share single family	---	0.757	0.731
		(0.296)	(0.311)
Share owner occupied	0.701	0.716	0.673
	(0.279)	(0.261)	(0.274)
Residents/unit	3.176	3.134	2.857
	(1.116)	(0.855)	(0.916)
Share black	0.031	0.070	0.151
	(0.125)	(0.207)	(0.301)
Rental sample			
Average contract rent	$468.72	$551.92	$580.84
	(157.99)	(172.45)	(188.08)
Mean # rooms, rented	4.143	4.165	---
	(0.785)	(0.778)	
Mean # rooms, all units	---	---	5.111
			(1.126)

Notes: J reports the number of jurisdictions for which each local policy variables is available. B reports the resulting number of border areas for which there is policy information on both sides.

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

Variable	Source
Current (non-educational) expenditure[1] - on fire, parks, police, roads, sanitation, sewers, other	*Census of Governments*, 1972
Educational expenditure, per pupil[1] - instructional - administrative	*Elementary and Secondary General Information System* (ELSEGIS), 1968-69
Effective property tax rates[2]	*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 are responsible for providing 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 effective property rates from samples of recent home sales. A unit's effective property tax rate is the property tax bill as a share of the unit's market value. These rates are reported for the 25th, 50th and 75th percentile of the market value distribution. I assign units on the poor (wealthy) side of borders the effective rate for homes at the 75th (25th) percentile of the value distribution in their jurisdiction. That is, I assume that the houses on the border are larger than the typical city unit and smaller than the typical suburban unit. Exact data on property tax rates are available for 38 jurisdiction pairs. In 27 additional pairs, I assign the suburb the property tax rate reported for the "balance of the metropolitan area" (that is, for all home sales in the suburban ring). In the remaining 37 cases, there is no information on the property tax rate levied on the suburban side of the border.