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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

The widening income gap between cities and suburbs throughout the twentieth century was an important cause of suburban growth. Many suburban towns have a wealthy electorate and a high tax base. I show that the marginal homeowner is willing to pay 3.7 percent more for an identical housing unit in a suburb whose median resident earned $10,000 more than the median city dweller. I compare neighboring houses that fall on opposite sides of city-suburban boundaries, thereby controlling for unobserved housing quality. The demand for wealthy co-residents is driven by lower property taxes, a smaller police budget and higher school quality.

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I. Introduction

In the United States, urban areas are anchored by poor central cities surrounded by affluent suburbs. This spatial polarization developed over the course of the twentieth century. Between 1940 and 2000, the income gap between city and suburban dwellers in the typical metropolitan area grew from three percent to 16 percent. At the same time, central cities lost population: the share of metropolitan residents who lived in the central city fell from 60 percent in 1940 to 32 percent in 2000.

The growing income gap between cities and suburbs was, in part, an outcome of suburbanization. Rich households faced lower costs of commuting by car and were attracted to the suburbs by the lower cost of housing on the periphery (Margo, 1992; Glaeser, Kahn and Rappoport, 2007). However, as this paper demonstrates, the city-suburban income gap was also a causal factor contributing to the growth of the suburbs. In particular, I document that the demand for suburban residence increases with the size of the income gap between the city and its suburbs. I focus on 1960 to 1980, a peak period of suburban growth. At this time, the marginal homeowner was willing to pay 3.7 percent more for an identical house located in a town whose residents' median income was $10,000 above that in the neighboring city (in $2000).

Because the presence of higher housing prices in wealthy towns could be driven by unobserved differences in housing quality and neighborhood characteristics, I limit my comparison to city and suburban housing units on adjacent blocks that fall on opposite sides of the jurisdiction boundary. The identifying assumption underlying this method is that housing and neighborhood quality evolve continuously over space while characteristics of the electorate and the tax base – and, by extension, the bundle of local public goods – change sharply at the border.

The observed decline in urban housing prices suggests that households were leaving the central city as the income gap with neighboring suburbs increased. I use estimates of the short-run elasticity of housing prices with respect to population to gauge the implied loss in urban population. By this metric, the widening income gap between cities and suburbs can explain 20 percent of urban population loss from 1940 to 2000.

My estimates suggest that the desire to live in a wealthy town stems from three main factors: lower property tax rates, lower expenditures on public safety and higher school quality. These motivations for suburban mobility exist above the value placed on wealthy neighbors and can explain the desire to move across jurisdiction lines, rather than simply across neighborhoods within the central city. Some studies have found that diverse jurisdictions provide fewer public goods (Cutler, Elmendorf and Zeckhauser, 1993; Alesina, Baqir, Easterly, 1998). In contrast, I find that housing prices are unaffected by a town's black population share after controlling for poverty or median income.

The existing literature on the causes of 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 technology- and political economy-based 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

[1] Exceptions include Cullen and Levitt (1998) which focuses on urban crime rates and Boustan (2010) which considers the role of racial diversity and white flight.

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 around 25 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; Kane, Staiger and Samms, 2003; Barrow and Rouse, 2004; Figlio and Lucas, 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 3.7 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 wealthy neighbors and co-residents.

The remainder of the paper is organized as follows. The next section introduces the estimation methods used to relate housing prices to differences in jurisdiction-level characteristics, including the median income and poverty rate of a town's residents. Section III describes the unique data set of Census blocks along jurisdiction borders. In section IV, I present the main relationship between jurisdiction level income and housing prices and test the maintained assumption of constant levels of housing quality across jurisdiction borders. Section V explores the local governmental channels that give rise to the willingness to pay for wealthy co-residents, including lower property tax rates, spending on various public goods and school quality. Section VI considers the implications of my estimates for urban population loss and the size of the suburban multiplier. Section VII concludes.

II. Using Housing Prices to Elicit the Demand for Wealthy Co-Residents

A. An Econometric Framework

The goal of this paper is to estimate the relationship between a jurisdiction's median income and average housing prices in the jurisdiction. The resulting estimate can be interpreted as the marginal homeowner's willingness to pay to live in a town with wealthy co-residents. The central empirical challenge is that housing units and neighborhoods in wealthy towns may also be of higher quality with larger lot sizes, 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. In particular, I narrow the analysis to Census blocks adjacent to city-suburban borders. The composition of a town's residents changes discretely at these borders. The necessary identifying assumption in a single cross-section is that neighborhood and housing quality evolve more continuously across borders. Later in the paper, I explore how the housing price gap across borders changes as the income difference between a city and its suburbs widens *over time*. This panel analysis is discussed in Section IV.

Are homeowners willing to pay more for the same housing unit if it is located in a town with wealthy co-residents? I pool data from 1960-1980 and estimate:

$$\ln(\text{price}_{ijbt}) = \alpha + \beta \ln(\text{median income})_{jt} + \Phi'(\text{block})_{it} + (B \cdot T) + \varepsilon_{ijbt} \quad (1)$$

where i indexes Census blocks, j political jurisdictions, b border areas, and t Census years. A border area consists of a pair of jurisdictions, one of which is a city and the other a suburb. I consider two dependent variables: the mean value of owner-occupied units or the mean rent for rental units at the block level. The key explanatory variable is either the median income or the

poverty rate of the jurisdiction (city or town) in which the block is located. Some specifications also add available block-level housing quality and neighborhood controls ($block_{it}$), including the share of the units that are in single family dwellings or are owner-occupied, the average number of rooms in housing units on the block, the number of residents per unit (density) and the share of household heads that are black.

The estimating equation contains a separate border area dummy variable in each Census year (B · T). This vector captures unobserved neighborhood characteristics that are shared by houses on either side of a border at a point in time (for example, the presence of a nearby park, a bus line, or a commercial strip). Time-specific border area fixed effects also control for common aspects of the housing stock (for example, age and architectural style of the units). After control for border area fixed effects, the effect of any town-level characteristics – such as the median income of a town's residents – is identified by comparing the two jurisdictions within a border area. A positive β in equation 1 implies that houses located in wealthier towns command systematically higher prices than their cross-border neighbors.

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 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 is the distinction between jurisdictions and border areas. 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. Central cities often belong to two or more border areas, but suburbs tend to contribute to a single border area.

B. Predictions from Jurisdiction Choice Models

Various models of jurisdiction choice predict that housing in wealthy towns will be more expensive than comparable units in poor cities. The demand for wealthy co-residents can stem either from preferences over public goods or from the fiscal subsidy inherent in the property tax system. In this section, I briefly discuss the conditions under which a gap in housing prices is likely to emerge at the border of a rich town and a poor city.

One class of jurisdiction choice models finds that the rich sort into towns offering a high level of public goods at a high tax rate, while the poor select towns with low goods provision and low tax rates (see, for example, Tiebout, 1956; Ellickson, 1971; Westhoff, 1977; Epple and Romer, 1991; Fernandez and Rogerson, 1996).[2] In this framework, we would not expect to find a price gap at the borders of rich and poor towns as long as the housing supply was sufficiently elastic. Instead, housing supply would simply expand to meet the demand for housing units in either town.

The suburbs were expanding dramatically during this period. In principle, the elastic supply of housing in the suburbs would have allowed the rich to self-select into wealthy towns without increasing housing prices. However, most of the new suburban construction occurred in outlying areas. Peripheral housing may not be a perfect substitute for more proximate suburban

[2] A key assumption in these models is that the rich are more willing than the poor to trade off a dollar of private consumption for a dollar of public expenditure.

housing. While both housing types 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 city-suburban border. The price gap would reflect the willingness to pay to access the suburban bundle of public goods at no extra commuting cost.

An alternative class of jurisdiction choice models gives rise to a "poor chasing the rich" equilibrium, in which agents of all income levels prefer living in a town with wealthier co-residents (Buchanan and Goetz, 1972; Hamilton, 1976). Such an equilibrium emerges because, for a a housing unit of a given size, residents of rich towns receive a cross-subsidy through the property tax system from owners of larger housing units, while residents of poor towns need to cross-subsidize others. This fiscal subsidy 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 either dictate minimum lot sizes for new housing construction or prohibit the construction of multi-family dwellings. Given the potential for differences in zoning regulations across jurisdictions, it is particularly important to test that there are no sharp differences in unit size or multi-family use across sample borders.

III. Collecting Housing Prices Along Jurisdictional Borders

The data on housing values and rents in this study are taken from the Census of Housing. I use block-level Census maps to identify city-suburban borders with available block data on both sides. To be included in the analysis, borders cannot be entirely obstructed by a railroad, four-lane highway, body of water, or large tract of industrial land.[3] I exclude southern borders

[3] 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

because African-Americans, who constituted a large share of the poor in the South, did not have a secure right to vote there until 1965. Furthermore, jurisdictions on both sides of the border must have at least 10,000 residents to ensure the availability of town-level information on government expenditures and property tax rates.

In 1960, the Census Bureau only assigned blocks to central cities and a few large suburban areas. In this year, I identify 56 borders in 16 metropolitan areas that meet the sample criteria.[4] I use these borders to construct a balanced panel that can be followed from 1960-80. In 1970, the Census Bureau assigned blocks to smaller suburbs. The expanded sample for 1970 and 1980 contains 102 borders.[5]

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 over-represents large, fragmented cities with populous suburbs. Los Angeles-Orange County and New York City-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.

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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.

[4] To increase the size of the panel sample, I include 15 borders that divide two suburbs (e.g., Cambridge-Somerville, MA).

[5] 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.

Much of the analysis is limited to Census blocks that are adjacent to the city-suburban border. 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. I collect block information for eight block tiers in both directions.

The block-level dataset contains information on housing prices, housing quality measures, and distance from the jurisdiction border. Housing price variables include the mean value of owner-occupied units and the mean rent for rental units.[6] Due to confidentiality concerns, housing prices or rents are only published for blocks containing five or more owner-occupied or rental units. Housing quality 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). Block data must be entered by hand for 1960 but are available electronically in 1970 and 1980.[7]

Blocks are matched to the socio-economic characteristics of the jurisdiction in which they are located, including the median income, poverty rate and black population share. I also compile data on effective property tax rates and expenditures on education, public safety, and other categories. The data sources and definitions of the local policy measures are described in Appendix Table 1. Measures of school quality are unavailable during this period.

Appendix Table 2 presents means and standard deviations of block and jurisdiction level variables. The median family income in sample jurisdictions is $49,000 and the average

[6] 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.

[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.

difference in median family income across a sample border is $10,000 (in $2000). There is substantial variation in local policy across borders. Crossing the typical border into the central city results in a 0.7 percentage point increase in property tax rates (measured as a share of the unit's market value) and a $500 increase in non-educational local government expenditures per capita.

The neighborhoods in the sample have attributes typically associated with suburban areas. In 1970, 76 percent of the units on the average block were detached, single family dwellings and 72 percent were owner-occupied. While the average block was 7.0 percent black, this value is a weighted average of 14 borders with a high black population share (39.6 percent) and 88 borders with a low black population share (0.7 percent). Later, I will show that the results are robust to excluding the 14 borders with a high black population share.

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

A. Testing for Differences in Observed Housing Attributes Across Borders

This paper asks whether the marginal resident is willing to pay more for an identical housing unit that is located in a town with a lower poverty rate or a higher median income. In order to find housing units of similar quality that are located in different jurisdictions, I narrow in on the border between cities and their suburbs. This section tests the identifying assumption that the housing units on either side of city-suburban borders are of equal quality. In particular, I investigate the relationship between town-level income characteristics and a series of available housing quality measures, including the share of units that are single family or owner occupied, the average number of rooms at the block level and the share of household heads that are black.

Table 2 presents regressions of these housing and neighborhood quality measures on town-level poverty rates in 1970 for the owner-occupied sub-sample.[8] The second column compares Census tracts adjacent to the jurisdiction border, while the third column narrows the comparison to the block-level. Census tracts contain eight block tiers, on average. For ease of interpretation, I report the implied effect of the mean difference in cross-border poverty rates (4.6 percentage points) rather than estimated coefficients. Census tracts located on the poorer side of jurisdiction borders have fewer single family units, lower owner occupancy and higher density. For example, a 4.6 point increase in the town poverty rate is associated with a 2.5 percentage point decline in the share of single family units at the tract level. In contrast, at the block level, this difference falls to 0.6 points and is no longer significant. This pattern holds for owner occupancy and population density as well. 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 4.6 point 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 is important to control for the average number of rooms on the block in the main analysis. Ideally, I would have access to other housing attributes, including total square footage, recent renovations, and so on, but this information is not available in the

[8] Housing stock differences for the full set of blocks are always smaller than those shown here. Results are qualitatively similar when using median income as the regressor of interest.
[9] 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.

Census of Housing. While the Census publishes a few other housing attributes at the tract level (for example, the age of the unit), these characteristics are not reported in the block data.

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 poor households will 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 4.6 point increase in the poverty rate 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 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 relationship between jurisdiction-level income and local housing prices. I start in Figure 2 with a simple graphical exercise relating the suburban housing price premium to the difference in poverty rates between city-suburban pairs. Each bubble represents a jurisdiction border in 1970, weighted by the underlying number of blocks along the border.[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

[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.

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 any outliers. The borders of the poorest central cities (for example, Newark, NJ) or wealthiest suburbs (Grosse Point, MI) have among the highest suburban premia (around 20 log points).[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] As before, I report implied effects for the mean cross-border difference in either poverty rates (4.6 percentage points) or median income ($10,000).

In the raw data, homeowners appear to be willing to pay 6.5 percent more for a housing unit in a suburb whose median income is $10,000 higher than the neighboring city (column 1). Controlling for block-level racial composition does not affect this result. Adding housing quality controls (particularly the average number of rooms) reduces the estimated willingness to pay for wealthy co-residents to 3.7 percent. The fourth column looks for changes in the willingness to pay for town-level attributes over time. The price premium associated with a $10,000 increase in median income rose from 3.4 percent in 1960 to 4.2 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

[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.

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) 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 3.7 percent increase in values for a $10,000 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.

C. Placebo Estimation

Thus far, I have found that housing prices respond to jurisdiction-level median income and poverty rates. Furthermore, observable differences in housing quality are not large enough to explain this pattern. However, there may be large *unobservable* differences in housing quality across city-suburban borders. Housing quality may improve steadily with distance from an impoverished urban core. The estimated housing price gap at the border could be picking up a small move along this quality gradient. If so, I should find similar coefficients in regressions that compare housing prices across "placebo" borders that are shifted one block into the poorer or the wealthier jurisdiction in each pair.

Table 4 presents results from two such placebo 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 times larger than any of the alternatives.

D. Panel Estimation

The placebo estimation discounts the possibility that the cross-border results can be explained by continuous improvements in housing quality with distance from the city center. However, this method does not address the prospect of a discontinuous jump in housing quality at jurisdiction borders. Housing quality could improve discretely at the border either due to differences in zoning regulations or due to population sorting on attributes that are correlated with the propensity to engage in home renovation and maintenance.

I exploit the panel nature of the dataset to control for any time-invariant differences in housing quality across borders. I pool data for 1960-1980 and estimate:

$$\ln(\text{price}_{ijbt}) = \beta \ln(\text{median income})_{jt} + \Phi' \text{block}_{it} + (B \cdot T) + (B \cdot J) + \varepsilon_{ijbt} \quad (2)$$

As before, the estimating equation includes separate border area fixed effects in each Census year to control for neighborhood characteristics shared by housing units on either side of the

border (B · T). In addition, Equation 2 adds a distinct fixed effect for each *side* of a border area, expressed as an interaction term between border areas and jurisdictions (B · J). This term absorbs any fixed difference in housing or neighborhood quality across borders – for example, due to a long-standing gap in school quality or a difference in local zoning ordinances between the two jurisdictions. β is now identified from differential *changes* in housing prices on either side of the border over time (B · J · T). Note that panel estimation cannot address the possibility of differential rates of neighborhood improvement or deterioration across borders over time.

Table 5 presents results from panel regressions of housing values on jurisdiction-level median income and poverty. The regression contains block-level demographics and housing quality controls and are best compared to estimates in the third column of Table 3. If the cross-sectional results merely reflected unobserved differences in housing quality, we would expect coefficients to be smaller in the panel setting. Instead, the willingness to pay for wealthy co-residents is, if anything, larger in the panel context. I estimate that housing values increase by 6.8 percent (compared to 3.7 percent in the cross-section) for a $10,000 increase in median income. There is little difference in the response to poverty rates between the two specifications. This finding casts doubt on the possibility that the cross-sectional results are simply picking up 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. The first row reproduces the coefficient from the baseline specification for the owner-occupied sample in 1970.[14] The analysis, which is

[14] The results of these robustness exercises are similar in other years and for the sample of rental units. The coefficient from the 1970 cross-sectional regression (0.023) is smaller than the coefficient on median income in

conducted at the block level, gives more weight to longer border areas. The coefficient is nearly unchanged in the second row of Table 6, which weights each border area equally. 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 with available value data, a figure that ranges between 5 and 297 (median = 21.1). In this case, the relationship between housing prices and town-level income increases slightly.

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 with a large black population in 1970.[15] 39.6 percent of household heads along these 14 border areas were black, compared to 0.7 percent in the remainder of the sample. The fifth row excludes any blocks with a black resident (21.1 percent of the sample). If town-level income is a proxy for neighborhood composition, we would expect smaller coefficients in these predominately white samples. In both cases, the coefficients are nearly unchanged.

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.

1970 from the pooled regression (0.031). In part, this difference may stem from the fact that the pooled regression constrains the effect of the block level controls to be the same in every decade.

[15] An area is considered to have a large black population if ten percent or more of the residents on either side are black. 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.

V. The Role of Public Goods in the Demand for Wealthy Co-Residents

Thus far, I have documented that the demand for suburban residence increases as the income gap between the city and the suburb grows. Unlike the desire to live in a wealthy neighborhood, which can be driven by local social interactions, the desire to live in a wealthy town relies on civic interactions through either the electoral process, the fiscal system or the public schools. This section considers a series of local policies that may account for the demand for wealthy co-residents.

The first panel of Table 7 examines the correlation between various local policy measures and a jurisdiction's median income. Wealthy towns set lower property tax rates than their cross-border neighbors. An additional $10,000 of town-level median income is associated with a 0.5 percentage point reduction in the effective tax rate (measured as a share of market value). Wealthy towns also spent less than poor central cities on non-educational functions, particularly on public safety. In contrast, sub-categories of educational expenditure (administrative, instructional) did not differ across jurisdiction boundaries, nor did spending on fire protection, park and road maintenance, or sanitation.[16]

The second panel of Table 7 tests whether homeowners are willing to pay for this set of local policies Home values fall by 3.3 percent for every point increase in the property tax rate. By this measure, a homeowner would break even after four years by purchasing a more expensive home in a jurisdiction with a lower tax rate. Home values also decline with additional spending on public safety. The mean cross-border difference in police expenditures is $66 per capita. A gap of this size is associated with a 2.0 percent decline in home values. These estimates may reflect the desire not to pay to police someone else's neighborhood. Residents likely faced

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

similar victimization rates across jurisdiction borders, but incurred very different obligations to pay for police services.

The third panel of Table 7 reports implied effects from the baseline regression of home values on jurisdiction-level median income both with and without the policy variables as additional correlates (equation 1). By comparing the implied effects across specifications, we can see that lower property tax rates explain part of the demand for wealthy suburbs. Including the property tax rate in the main specification reduces the coefficient on median income by 20 percent. Adding police expenditure to the main specification explains an additional one third of the suburban premium. When property tax rates and police expenditures are entered simultaneously, the implied effect of a standard deviation change in median income is cut in half (from a 2.8 percent decline in housing values to a 1.2 percent decline).

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) each provide estimates of willingness to pay for school quality of around 2.0 percent of housing value for a one standard deviation increase in elementary school test scores. Driscoll, Halcoussis, and Svorny (2003) estimate that a one standard deviation increase in district-level median income increases student test scores by between 16 and 40 percent of a standard deviation. At the high end of this range, differences in school quality across borders separated by a standard deviation in median income ($10,000) gives rise to an 0.85 percent housing price gap, accounting for nearly all of the unexplained price effect.

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 move across jurisdiction lines, rather than simply across neighborhoods within the central city.

VI. The Demand for Wealthy Co-Residents and the Suburban Multiplier

This section gauges the importance of the growing city-suburban income gap on urban population loss. To do so, I couple my estimate of the willingness to pay for wealthy co-residents with measures of the short-run elasticity of housing prices with respect to population. While a precise answer to this question may require a more complete, general equilibrium model of urban housing markets, these calculations are intended as a first step.

A decline in relative city housing prices is an indication that households are leaving central cities. In the short run, out-migration will reduce housing prices in the city and increase prices in the suburbs. The best estimate of the short-run elasticity of housing prices with respect to population implies that a one percent increase in population results in a one percent increase in housing prices and rents (Saiz, 2007).[17] At this parameter value, the estimated 3.7 percent decline in urban housing prices may be the result of an equivalent decline in city population. In the long run, housing supply can respond to changes in population. However, the durability of the housing stock ensures that urban population loss will not result in an immediate decline in urban housing supply (Glaeser and Gyuorko, 2005). Therefore, the long-run elasticity of prices with respect to population loss may still be large.

[17] Estimates of the housing price elasticity are typically based on the housing price response to migrant arrivals. Potepan (1994) focuses on internal migrants and Saiz (2003, 2007) considers international migrants. While migrants may increase housing demand – and, therefore, housing prices, they can also be attracted to an area by low housing prices. Saiz (2007) represents the most convincing attempt to address this reverse causality.

To put the implied decline in city population into context, I begin with the case of Detroit, MI. In 1940, the median resident in central city Detroit earned only three percent less than the median suburban resident. By 1970, this income gap had increased to 21 percent. By my estimates, Detroit's housing prices would have fallen by 3.3 percent in response to this wider city-suburban income gap, implying that the population of central city Detroit likely declined by 3.3 percent as well.[18] Detroit's population actually fell by 6.8 percent over this period (from 1.62 million to 1.51 million). This calculation suggests that 48 percent of the overall population loss was due to a decrease in relative city income levels.

Of course, Detroit may not be the typical case. I subdivide the metropolitan area sample into the 13 whose cities lost population from 1940 to 1970 and the 17 whose cities expanded over the period. By conducting the same exercise, I conclude that, on average, 30 percent of the population loss experienced in the declining cities can be attributed to city-suburban income divergence. Expanding cities would have grown 21 percent faster if not for their expanding city-suburban income gap. Extrapolating out of sample, these estimates imply that the growth in the city-suburban income gap can explain 20 percent of urban population loss over the second half of the twentieth century.

Income stratification can also intensify the response to other urban shocks. A major cause of suburbanization during this period was the construction of the interstate highway system (Baum-Snow, 2007). If households that left the city following new road construction were drawn from the top half of the urban income distribution, the resulting increase in the city-suburban income gap would have augmented the response to new infrastructure.

[18] The coefficient on the logarithm of median income in an estimation of equation 1 is 0.189 (s.e. = 0.035). The 18 percentage point increase in the city-suburban income gap in Detroit would lead to a 3.3 percent decline in housing prices (= 0.189 · 0.180).

Baum-Snow estimates that, in the average metropolitan area, the construction of one new highway through the urban center caused city population to fall by 4.5 percent in every decade from 1950 to 1990. How much additional mobility would have been generated by the resulting increase in the city-suburban income gap? To be concrete, I will return to the case of Detroit. For simplicity, I assume that all departing households earned above the city's median income and that all households resettled in the Detroit suburbs. These assumptions provide an upper bound on the power of the suburban multiplier.

1.8 million people lived in center city Detroit in 1950. According to Baum-Snow's estimates, the construction of a new highway would have resulted in the loss of 166,000 residents by 1970 (=1.8 million · 0.045 decline · 2 decades). In 1950, the median resident of both urban and suburban Detroit earned around $22,000. A loss of 166,000 residents from above the city's median income would place the new city median at the 41st percentile of the city's initial income distribution ($21,350), while the corresponding gain in suburban residents would have shifted the new suburban 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, the resulting increase in the city-suburban income gap would generate an independent demand for suburban residence. The new city-suburban income gap would cause relative city housing prices – and, by implication, population – to fall by 2.4 percent. In other words, the initial nine percent decline in population due to the construction of a new highway would have generated a further 2.4 percent population loss due to an increase in relative city poverty. As this example has shown, a suburban multiplier of this

nature can augment the response to a transportation innovation by around 25 percent, which would have a quantitatively important effect on the fate of cities.

VII. Conclusion

The diffusion of the automobile and new highway projects in the mid-twentieth century made it economically feasible for the first time for middle class households to settle in bedroom communities and commute into the central city for work. This paper demonstrates that the resulting decline in the income of the average urban household was an independent cause of suburbanization. In this period, the marginal homeowner was willing to pay four percent more for an otherwise equal housing unit located in a town whose median income was $10,000 above the neighboring city. Part of this premium reflects the fact that, by moving to the suburbs, households received a fiscal subsidy through the property tax system. Suburban residents also avoided the responsibility for addressing urban problems through local expenditures on public safety. Calculations that couple this willingness to pay estimate with estimates of the short-run elasticity of housing prices with respect to population suggest that the growth in the city-suburban income gap can explain 20 percent of urban population loss from 1940 to 2000.

If an income gap between cities and suburbs 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 25 percent. A suburban "feedback loop" of this nature may help to explain the sharp declines in city fortunes at mid-century. Cities were losing both population and tax base throughout this period, leading in some cases to acute fiscal crisis. More speculatively, this suburban multiplier may be at work in the opposite direction today as some cities undergo a process of gentrification. 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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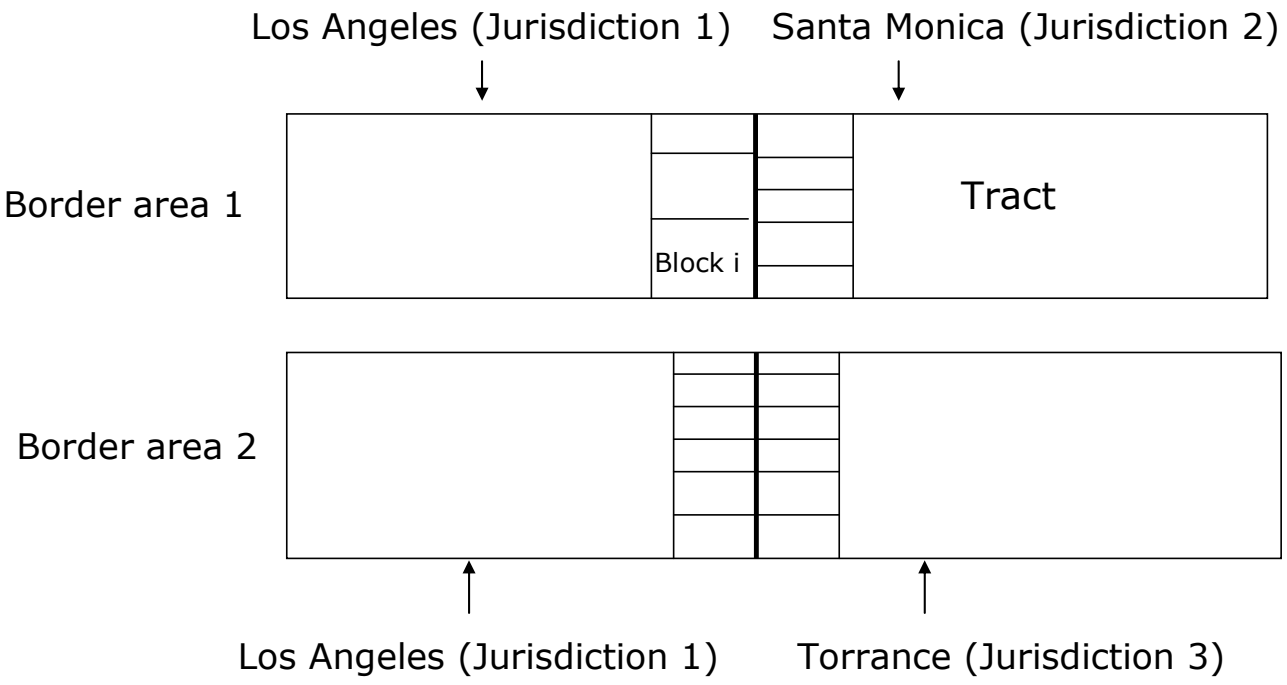
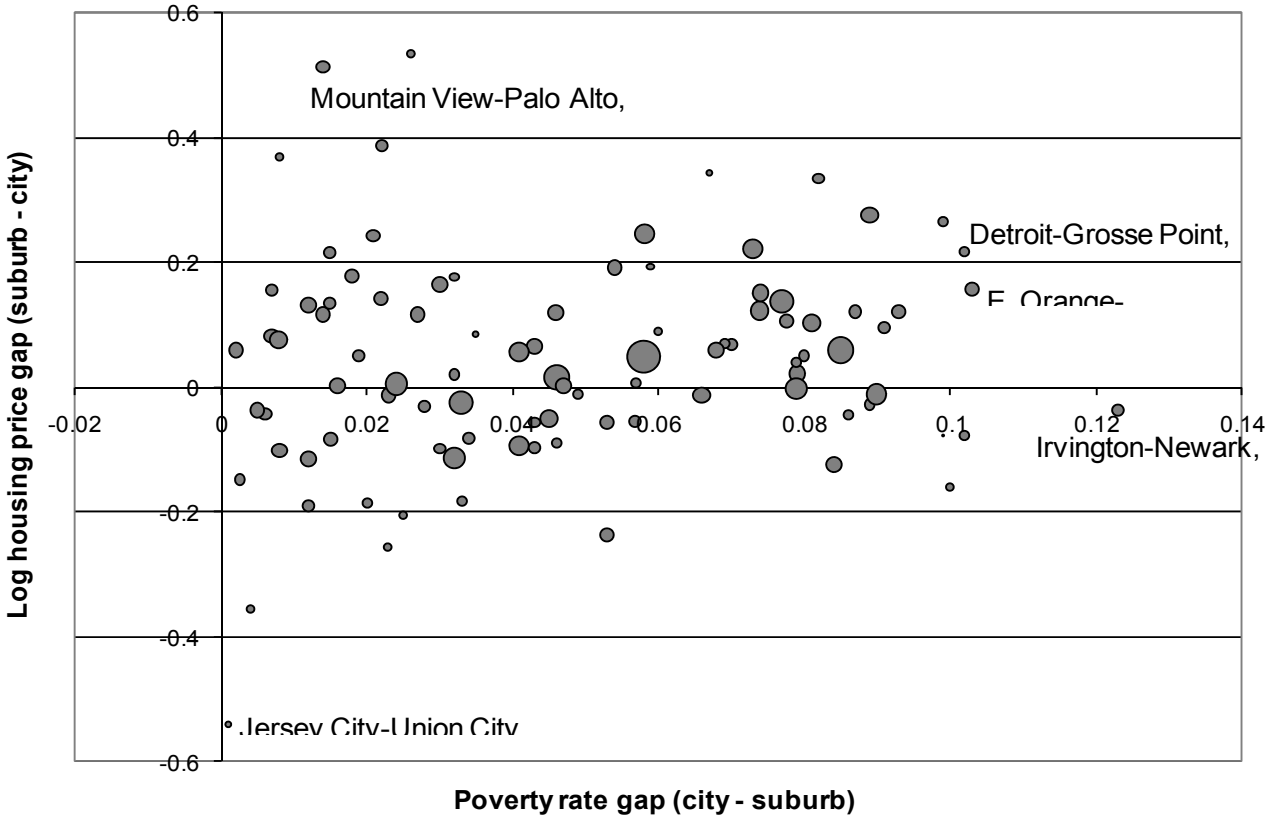
Figure 1: Schematic diagram of geographic terms



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

Figure 2: The relationship between differences in city-suburban poverty rates and the suburban housing price premiums, 1970



Notes: Each dot represents one of the 102 jurisdiction borders in the full sample. The size of the bubble indicates the number of underlying blocks along the border 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: The effect of jurisdiction-level poverty rates on housing quality and neighborhood demographics, 1970

Implied effects at the mean cross-border poverty rate gap (4.6 points)

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)
N		607	2561

Notes: Column 1 contains means and standard deviations of housing quality measures calculated at the tract-level. Columns 2 and 3 report implied effects from regressions of housing quality measures on jurisdiction-level poverty rates at either the tract or the block level. That is, coefficients and standard errors are normalized to reflect the implied effect of the mean city-suburban poverty rate gap (4.6 percentage points). Regressions also include a vector of border area dummy variables. Standard errors are clustered by border area. Block regressions are restricted to blocks with at least five owner-occupied units that are adjacent to the jurisdiction border. Data on the number of bedrooms and unit age are not available at the block level.

Table 3: The effect of jurisdiction-level income on housing prices

Implied effects at the mean cross-border income or poverty rate gap

	Dependent variables					
	ln(value of owner occupied units)					ln(rent)
RHS variables	(1)	(2)	(3)	(4)	(5)	
Panel A						
ln(median income)	0.065	0.063	0.037		0.044	0.023
	(0.010)	(0.010)	(0.007)		(0.010)	(0.009)
ln(med. income), 1960				0.034		
				(0.017)		
ln(med. income), 1970				0.031		
				(0.006)		
ln(med. income), 1980				0.042		
				(0.009)		
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	-0.045	-0.042	-0.030		-0.035	-0.014
	(0.010)	(0.010)	(0.008)		(0.011)	(0.008)
Share poverty, 1970				-0.024		
				(0.008)		
Share poverty, 1980				-0.032		
				(0.009)		
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: Cells contain implied effects from regressions of housing prices or rents on jurisdiction-level income measures (equation 1). That is, coefficients and standard errors are normalized to reflect the implied effect of the mean city-suburban income gap (19.8 percent) or poverty rate gap (4.6 percentage points). All regressions include border area dummy variables interacted with Census year. Reading across the columns, the regressions include additional block- or jurisdiction-level controls. Standard errors are clustered by border area.
The sample is restricted to blocks adjacent to the jurisdiction border. Median income regressions contain data from 1960 to 1980. Poverty rates are only available in 1970 and 1980. The value regressions contain blocks with at least five owner occupied units (N = 6884 in panel A and 4710 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: The effect of jurisdiction-level income on housing prices at placebo borders

Dependent variable = ln(value of owner occupied units)
Implied effects at the mean cross-border income or poverty rate gap

	ln(median income)	Share poverty
Actual border	0.037	-0.030
	(0.007)	(0.008)
Poor side: Tier 1 vs. Tier 2	0.010	-0.002
	(0.010)	(0.008)
Wealthy side: Tier 1 vs. Tier 2	-0.003	0.003
	(0.009)	(0.007)

Notes: Coefficients and standard errors are normalized to reflect the implied effect of the mean city-suburban income gap (19.8 percent) or poverty rate gap (4.6 percentage points) on housing prices. Specification details are described in the notes to Table 3. The first row reproduces the coefficients from the main specification (Table 3, column 3) at the actual city-suburban border. The second row considers a placebo border between the first and second block tier in the poor jurisdiction created by assigning the characteristics of the wealthier jurisdiction to the first tier. The third row considers a placebo border between the first and second block tier in the wealthy jurisdiction created by assigning the characteristics of the poorer jurisdiction to the first tier.

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

Dependent variable = ln(value of owner occupied units)
Implied effects at the mean cross-border income or poverty rate gap

	ln(median income)	Share poverty
Panel sample	0.074	-0.042
	(0.029)	(0.022)
N	4501	2968
Full sample	0.068	-0.034
	(0.027)	(0.019)
N	6869	5324

Notes: Coefficients and standard errors are normalized to reflect the implied effect of the mean city-suburban income gap (19.8 percent) or poverty rate gap (4.6 percentage points) on housing prices. The panel sample contains 56 city-suburban borders with available data from 1960 to 1980. The full sample adds 46 borders with data in 1970 and 1980. All regressions include border area dummy variables interacted with both Census year and jurisdiction and a full set of block-level housing quality controls (equation 2). Other specification details are described in the notes to Table 3.

Table 6: Robustness: The effect of jurisdiction-level median income on housing prices, 1970

Dependent variable = ln(value of owner occupied units)
Implied effects at the mean cross-border income gap

	ln(median income)
1. Baseline	0.023
N = 2561	(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: Coefficients and standard errors are normalized to reflect the implied effect of the mean city-suburban income gap (19.8 percent) on housing prices. Sample sizes associated with the various restrictions in rows 4-8 are reported. Other specification details are described in the notes to Table 3.

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

Panels A and C: Implied effects at the mean cross-border income gap

PANEL A	Dependent variable = Local policy measure				
Dep. variable →	Property	Spending ($1000 per cap.)		Spending ($1000 per pupil)	
RHS variable ↓	tax rate	Total	Police	Instructional	Administrative
ln(med income)	-0.532	-0.270	-0.042	-0.033	-0.009
	(0.073)	(0.088)	(0.007)	(0.114)	(0.015)
Observations	127	194	194	194	194
Borders	65	97	97	97	97
PANEL B	Dependent variable = ln(value of owner-occupied units)				
RHS variable →	Property	Spending ($1000 per cap.)		Spending ($1000 per pupil)	
Dep. variable ↓	tax rate	Total	Police	Instructional	Administrative
ln(housing value)	-0.033	-0.039	-0.337	0.035	-0.303
	(0.010)	(0.013)	(0.096)	(0.015)	(0.183)
Observations	1815	2424	2424	2424	2424
Borders	65	97	97	97	97
PANEL C	Dependent variable = ln(value of owner-occupied units)				
Policy variable →	Property	Spending ($1000 per capita)		Spending ($1000 per pupil)	
RHS variable ↓	tax rate	Total	Police	Instructional	Administrative
1. Without policy variable on RHS					
ln(med income)	0.026	0.024	0.024	0.024	0.024
	(0.009)	(0.006)	(0.006)	(0.006)	(0.006)
2. With policy variable on RHS					
ln(med income)	0.020	0.018	0.016	0.022	0.024
	(0.016)	(0.007)	(0.008)	(0.009)	(0.009)
Observations	1815	2424	2424	2424	2424
Borders	65	97	97	97	97

Notes: Panel A reports implied effects from regressions of local policy measures on jurisdiction-level median income. Regressions also contain a vector of border area dummy variables. Panel B reports coefficients from regressions of housing prices on local policy measures. Regressions include border area dummy variables and the block-level controls listed in the notes to Table 3. Panel C reports implied effects from regressions of housing prices on jurisdiction median income. In the first row, median income is the only jurisdiction-level regressor. The second row adds the local policy variable. Sources for the local policy measures are reported in Appendix Table 1. Standard errors are reported in parentheses and clustered by border area.

Appendix Table 1: Sources for jurisdiction-level public goods data

Variable	Source
Current (non-educational) expenditure[1] - Fire, parks, police, roads, sanitation, sewers, other	*Census of Governments*, 1972
Educational expenditure, per pupil[2] - Instructional - Administrative	*Elementary and Secondary General Information System* (ELSEGIS), 1968-69
Effective property tax rates[3]	*Census of Governments, 1972*

Notes:
1: Non-educational expenditures are measured at the municipal level. In some states, counties are responsible for providing public services. Most jurisdiction pairs in the sample belong to the same county.
2: Educational spending per pupil is collected both from independent school districts and municipal school systems.
3: The *Census of Government* estimates effective property rates at the town level from samples of recent home sales. The effective property tax rate of a housing unit is the ratio of the property tax bill to the transaction price. These rates are reported for the 25^{th}, 50^{th} and 75^{th} percentile of the market value distribution. I assign units on the poor (wealthy) side of borders the effective rate for homes at the 75^{th} (25^{th}) 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 city-suburban borders. For 27 additional borders, 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.

Appendix Table 2: Summary statistics, Jurisdiction and block level variables

	1970		1970-80
Mean (S.D.)	All jurisdictions	Difference across borders	Change in cross-border difference over time
Jurisdiction level variables			
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)
Share black	0.086 (0.142)	0.151 (0.145)	0.055 (0.068)
Property tax rate, % of sale price	2.535 (1.115)	0.723 (0.482)	
In $1,000 ($2000):			
Instruction $ per pupil	3.001 (0.652)	0.512 (0.473)	
Administrative $ per pupil	0.133 (0.055)	0.044 (0.046)	
Non-education $ per capita	0.736 (0.424)	0.493 (0.431)	
Police $ per capita	0.114 (0.053)	0.066 (0.045)	

(Table continued…)

Appendix Table 2, continued

	1960	1970	1980
Block level variables			
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 on block	0.031	0.070	0.151
	(0.125)	(0.207)	(0.301)
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: Sources for local policy variables reported in Appendix Table 1. Demographic and socio-economic variables are available for 102 city-suburban borders, of which expenditure variables are available for 97 and property tax rates for 65. All block-level variables reported for the sub-sample of blocks with at least five owner-occupied units, with the exception of average contract rent and mean number of rooms in rented units. These rental variables are reported for the sub-sample of blocks with at least five rental units.