The Effects of Gentrification on the Well-Being and Opportunity of Original Resident Adults and Children*

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Abstract

Gentrification represents a striking reversal of decline in many US cities, yet it is controversial because of its perceived negative consequences for original neighborhood residents. In this paper, we use new longitudinal census microdata to provide the first causal evidence of how gentrification affects a broad set of outcomes for incumbent adults and children. Gentrification modestly increases out-migration, though movers are not made observably worse off and aggregate neighborhood change is driven primarily by changes to in-migration. At the same time, many original resident adults stay and benefit from declining poverty exposure and rising house values. Children benefit from increased exposure to neighborhood characteristics known to be correlated with economic opportunity, and some are more likely to attend and complete college. Our results suggest that accommodative policies, such as increasing housing supply in high-demand urban areas, could increase the opportunity benefits we find, reduce out-migration pressure, and promote long-term affordability.

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1 Introduction

Over the past two decades, high-income and college-educated individuals have increasingly chosen to live in central urban neighborhoods (Baum-Snow and Hartley 2017; Couture and Handbury 2017; Edlund et al. 2016; Su 2018). This gentrification process reverses decades of urban decline and could bring broad new benefits to cities through a growing tax base, increased socioeconomic integration, and improved amenities (Vigdor 2002; Diamond 2016). Moreover, a large neighborhood effects literature shows that exposure to higher-income neighborhoods has important benefits for low-income residents, such as improving the mental and physical health of adults and increasing the long-term educational attainment and earnings of children (Kling et al. 2007; Ludwig et al. 2012; Chetty et al. 2016; Chetty and Hendren 2018a,b; Chyn 2018). Gentrification thus has the potential to dramatically reshape the geography of opportunity in American cities.

However, gentrification has generated far more alarm than excitement. A key concern is that the highly visible changes occurring in gentrifying neighborhoods are driven by the direct displacement of original residents, making them worse off and preventing them from sharing in the aforementioned benefits. These concerns are central to current debates about the distributional consequences of urban change and about policies associated with those changes. More specifically, they have emerged as an obstacle to building more housing in high-cost cities and have helped fuel support for policies like rent control, both of which could have large, unintended welfare costs.[1] Thus, understanding how gentrification actually occurs and whether it harms or benefits original residents is of primary importance for urban policy. Yet despite its importance, there is little comprehensive evidence on this question. Largely because of data limitations, previous research has focused on particular outcomes, specific cities, or relied on purely descriptive approaches.

In this paper, we provide the first comprehensive, national, causal evidence of how gentrification affects original neighborhood resident adults and children. For adults, we estimate effects on a number of individual outcomes that together approximate well-being. For children, we estimate effects on individual exposure to neighborhood characteristics known to be positively correlated with economic opportunity and on educational and labor market outcomes. We focus on original residents of low-income, central city neighborhoods of the 100 largest metropolitan areas in the US and explore heterogeneity along a number of dimensions.

Three innovations are central to our approach. First, we construct a unique data set

[1] Ganong and Shoag (2017) and Hsieh and Moretti (2018) show that local housing supply restrictions have reduced regional convergence and national economic growth. Diamond et al. (2018) show that rent control in San Francisco benefits controlled residents at the expense of uncontrolled and future residents.

of longitudinal individual outcomes by linking individuals responding to both the Census 2000 and the American Community Survey 2010-2014. For each person, we observe at both points in time their neighborhood (census tract) of residence, detailed demographic and housing characteristics, and a variety of outcomes. The data allow us to identify original residents and to follow changes in their outcomes whether they move or stay.

Second, we develop a stylized neighborhood choice model to provide a comprehensive picture of how gentrification affects original resident well-being and to anchor our empirical approach. It shows that the overall effect on well-being is captured by its effect on two margins: the number of residents choosing to move instead of stay (out-migration or displacement) and changes in the observable outcomes of both movers and stayers. We capture the latter with changes to each original resident's income, rent paid or house value, commute distance, and neighborhood poverty rate. Out-migration matters even conditional on these changes because movers may experience unobserved costs of moving from the origin neighborhood.

Finally, we use three complementary methods to argue that our results are causal. We first estimate Ordinary Least Squares (OLS) models of the relationship between individual outcomes from 2000 and 2010-2014 and gentrification over the same period, controlling for a detailed set of individual, household, and neighborhood characteristics and pre-trends. To address potential bias from remaining omitted variables and spatial spillovers, we use coefficient stability methods from Altonji et al. (2005) and Oster (2017) and spatial first differences (SFD) methods from Druckenmiller and Hsiang (2018). These three methods use different assumptions and identifying variation yet yield quantitatively similar results, suggesting they provide plausible bounds for the causal effects of gentrification.[2]

Overall, we find that gentrification creates some important benefits for original resident adults and children and few observable harms. It reduces the average original resident adult's exposure to neighborhood poverty by 3 percentage points, with larger (7 percentage points) reductions for those endogenously choosing to stay and no changes for those endogenously choosing to move. Gentrification also increases the average original resident homeowner's house value, an important component of household wealth, with effects again stronger for stayers. Importantly, less-educated renters and less-educated homeowners each make up close to 25 percent of the population in gentrifiable neighborhoods, and 30 percent and 60 percent, respectively, stay even in gentrifying neighborhoods. Thus, the benefits experienced by these groups are quantitatively large. Gentrification increases

[2]The Oster method relaxes the OLS unconfoundedness assumption using data-driven rule-of-thumb values for the influence of remaining unobservables. SFD differences away observed and unobserved characteristics common to adjacent neighborhoods.

rents for more-educated renters but not for less-educated renters, suggesting the former may be more willing or able to pay for neighborhood changes associated with gentrification.[3] We find few effects on other observable components of adult well-being, including employment, income, and commute distance.

Given the importance of neighborhood quality for children's long-term outcomes (Chetty et al. 2016; Chetty and Hendren 2018a,b; Chyn 2018; Baum-Snow et al. 2019), we also study how gentrification affects original resident children. We find that on average, gentrification decreases their exposure to neighborhood poverty and increases their exposure to neighborhood education and employment levels, all of which have been shown to be correlated with greater economic opportunity (Chetty et al. 2018). We also find some evidence that gentrification increases the probability that children of less-educated homeowners attend and complete college, with these effects driven by those endogenously staying in the origin neighborhood.[4] Taken together, the results for children and adults show that many original residents are able to remain in gentrifying neighborhoods and share in any neighborhood improvements, answering a key unresolved distributional question.

At the same time, gentrification increases out-migration to any other neighborhood by 4 to 6 percentage points for less-educated renters and by slightly less for other groups. However, these effects are somewhat modest relative to baseline cross-neighborhood migration rates of 70 to 80 percent for renters and 40 percent for homeowners. Importantly, we find no evidence that movers from gentrifying neighborhoods, including the most disadvantaged residents, move to observably worse neighborhoods or experience negative changes to employment, income, or commuting distance. Our model shows that the key remaining channel through which gentrification may cause harm is through unobserved costs of leaving the origin neighborhood. These may be small given the high rates of baseline mobility we find and existing structural estimates of the value of community attachment.[5] We provide additional evidence that the highly visible changes associated with gentrification are driven almost entirely by changes to the quantity and composition

[3]This is consistent with recent findings on differences in preferences for urban consumption amenities by skill (Couture and Handbury 2017; Diamond 2016; Su 2018) or some degree of rental market segmentation.

[4]We find no effects on educational attainment or labor market outcomes for other children, though they may nevertheless benefit in non-economic ways from living in lower-poverty neighborhoods (Katz et al. 2001; Kling et al. 2007).

[5]Costs may be pecuniary (time and money spent finding and moving to a new location) or nonpecuniary (loss of proximity to friends, family, networks, or other neighborhood-specific human capital). Diamond et al. (2018) structurally estimate cross-neighborhood moving costs of $42,000 on average, which increase by $300 per year of living in the origin neighborhood. High baseline mobility suggests that gentrification may simply move up the date at which individuals decide to move, rather than causing them to make a move they would otherwise never make. Thus, $300 per year of residence may be closer to the unobserved cost than $42,000.

of in-migrants, not direct displacement.

Our results have important implications for how policymakers should respond to concerns about gentrification. Foremost, they should weigh the benefits of gentrification that accrue to original residents, including less-advantaged residents, against any harms. Moreover, neighborhoods are far more dynamic than typically assumed, with high baseline migration allowing them to change quickly without the wholesale direct displacement of original residents. Instead, neighborhood demographic changes are driven almost entirely by changes to those willing and able to move into gentrifying neighborhoods. Thus, preserving and expanding the affordability and accessibility of central urban neighborhoods should primarily take a forward-looking approach that seeks to accommodate increasing demand for these areas. A growing recent literature suggests that building more housing (whether market-rate or affordable) is a promising way of maintaining and expanding housing affordability (Mast 2019; Nathanson 2019; Favilukis et al. 2019). It would also maximize the integrative and opportunity benefits we find. These policies could be complemented with rental subsidies or other inclusionary policies carefully targeted to the relatively small population of the most disadvantaged original residents, for whom out-migration effects are highest. Additionally, targeting inclusionary policies to low-income families with children could encourage them to stay in neighborhoods improving around them, complementing existing programs like Moving to Opportunity (MTO) that seek to increase moves from low- to high-opportunity neighborhoods.

Our work builds on a broad existing literature studying the effects of gentrification across many disciplines. Ellen and O'Regan (2011a), Rosenthal and Ross (2015), and Vigdor (2002) provide thorough reviews of this literature. Most previous studies focus on displacement as the primary outcome of interest and, using descriptive approaches, find little evidence of more moving in gentrifying neighborhoods (Freeman 2005; McKinnish et al. 2010; Ellen and O'Regan 2011b; Ding et al. 2016; Dragan et al. 2019). Concurrent work by Aron-Dine and Bunten (2019) uses annual migration data and finds causal evidence that gentrification increases out-migration in the short term, similar to our findings of out-migration effects in the medium-to-long-term. We expand on these papers by taking a comprehensive approach toward understanding how gentrification causally affects well-being overall, not only displacement, and by exploring heterogeneity. In this sense, our paper is similar to Vigdor (2002) and Vigdor (2010), which provide the earliest applications of spatial concepts to understanding how gentrification might affect residents. They find no evidence of large negative effects and some evidence that neighborhood improvements increase welfare. We build on those papers by using longitudinal individual microdata on many outcomes and estimating causal effects. Finally, concurrent papers by

Couture et al. (2018) and Su (2018) use structural approaches to show that the increased residential sorting and amenity changes associated with gentrification have increased welfare inequality beyond what is implied by increases in the wage gap alone. By contrast, we focus on absolute effects for original residents, which are central to current policy debates and distributional concerns about who shares in the benefits of gentrification. Our results suggest that the important inequality effects they find exist alongside absolute benefits for original residents.

By studying how gentrification affects children, we also contribute to a large neighborhood effects literature that shows that moving families to low-poverty neighborhoods increases children's educational attainment and earnings (Chetty et al. 2016; Chetty and Hendren 2018a,b; Chyn 2018). We show that when neighborhoods gentrify, they improve along many dimensions known to be beneficial for children, and many original resident children (including the least advantaged) are able to stay and experience those improvements. Some are even more likely to attend and complete college. In complementary, concurrent research, Baum-Snow et al. (2019) find that improvements to neighborhood labor market opportunities similarly increase measures of neighborhood quality and improve children's test scores, labor market outcomes, and credit scores.[6] Our results and theirs suggest that housing policies designed to keep disadvantaged households in improving neighborhoods may achieve many of the same benefits as trying to move them to better neighborhoods.

The rest of this paper is organized as follows. Section 2 describes our data and sample characteristics. Section 3 describes a simple model of gentrification, location, and well-being. Section 4 discusses our regression model and identification strategies. Section 5 presents estimates of the effect of gentrification on original resident adults, and Section 6 presents estimates for original resident children. Section 7 concludes.

2 Data and Sample Characteristics

2.1 Longitudinal Census Microdata

We construct a national panel of individuals and their locations, characteristics, and outcomes over time using Census Bureau data and unique Protected Identification Keys

[6] While we focus on gentrifiable neighborhoods (initially low-income, central city neighborhoods of major metropolitan areas), they study all neighborhoods, including initially high-income and suburban neighborhoods, and their results are driven by suburban neighborhoods.

(PIKs).[7] We use PIKs to match individuals responding to both the Census 2000 long form and the 2010-2014 American Community Survey (ACS) 5-year estimates.[8] Approximately 10 percent of the Census 2000 long form sample matches, yielding around 3 million matched individuals. We observe in both years each individual's block of residence and block of work (if working), employment and income, homeownership status, rent paid or house value, and demographic characteristics. Key demographics include education, age, race/ethnicity, and household type. We define neighborhoods as census tracts and assign each individual in each period to a geographically consistent neighborhood of residence, neighborhood of work, and metropolitan area (Core-Based Statistical Area (CBSA)).[9] The resulting data set is unique to the gentrification literature and central to our paper. It allows us to identify original residents of neighborhoods, to follow their locations and other outcomes regardless of their choice to stay or leave, and to do so by many different individual characteristics. Our focus on changes from 2000 to 2010-2014 allows us to study medium-to-long-term effects.[10]

2.2 Adult Sample and Characteristics

We define original residents as all individuals living in initially low-income, central city neighborhoods of the 100 most populous metropolitan areas (CBSAs) in the year 2000. These are "gentrifiable." Low-income neighborhoods are census tracts with a median household income in the bottom half of the distribution across tracts within their CBSA. Central cities are the largest principal city in their CBSA.[11] We focus on these neigh-

[7] PIKs are assigned to individuals by the Census Bureau's Person Identification Validation System (PVS). The PVS uses probabilistic matching algorithms to match individuals in a given Census Bureau product to a reference file constructed from the Social Security Administration Numerical Identification File and other federal administrative data. Matching fields include social security numbers, full name, date of birth, and address (Alexander et al. 2015).

[8] We assess match quality by ensuring that certain individual characteristics change in expected ways or do not change in unexpected ways. For example, age should change 10 years from 2000 to 2010, plus or minus one due to the exact timing of the survey interview. We therefore drop individuals with unexpected changes in age and similar characteristics. They are a small share of our total matched sample.

[9] We observe each year 2000 observation's block of residence. We therefore construct a crosswalk from 2000 blocks to 2010 tracts using Census Bureau maps and geographic information system (GIS) software and use it to assign all year 2000 observations precisely to 2010 tracts.

[10] Most previous research on gentrification also studies decadal changes. The exceptions are Ding et al. (2016) and Aron-Dine and Bunten (2019), which use annual frequencies from the Federal Reserve Bank of New York (FRBNY) Consumer Credit Panel (CCP). Aron-Dine and Bunten (2019) find that the onset of gentrification increases subsequent out-migration by around 4 percentage points (hastening a move by 1.5 years). The estimate is similar to ours and suggests we may not be missing important short-term out-migration effects.

[11] All results are robust to different samples of metropolitan areas (10, 25, or 50 most populous), definitions of low-income (bottom quartile of the CBSA distribution), and definitions of central city (within some distance of the central business district).

borhoods because they are where gentrification trends have been strongest (Couture and Handbury 2017; Baum-Snow and Hartley 2017) and where gentrification concerns have been greatest. To focus on adults capable of making move decisions and for whom education levels are mostly fixed, we restrict the sample to individuals 25 or older in 2000, not enrolled in school, not living in group quarters, and not serving in the military. We focus on education level and tenure status as essential elements of heterogeneity and therefore stratify all results by four key types of individuals: less-educated renters, more-educated renters, less-educated homeowners, and more-educated homeowners.[12] Appendix B provides additional data details.

Table 1, Panel A, describes baseline changes in a number of original resident adult outcomes from 2000 to 2010-2014 that together approximate changes in well-being. Outmigration captures potential unobserved costs of leaving the origin neighborhood, is central to gentrification debates, and has been the focus of previous gentrification research. We measure it in three ways: move to any other neighborhood, move at least one mile away, and exit the metropolitan area. We measure changes in observable well-being using changes in self-reported rents for renters, self-reported house values for homeowners, neighborhood poverty rate, employment and income, and commute distance.[13] Among the patterns in Table 1, perhaps the most important is that migration for renters is high: 68 percent of less-educated renters and 79 percent of more-educated renters move to a different neighborhood over the course of a decade. This effectively places a limit on the potential for gentrification to cause displacement and makes it possible for neighborhoods to change quickly even without strong displacement effects.

Table 2 describes the individual and household characteristics of original resident adults in 2000. We include these as controls in our regression models. Most are correlated with education level and tenure status in the expected ways.[14] It is worth emphasizing that the sample is evenly distributed across the four types of individuals, not overwhelmingly disadvantaged as is often implicitly assumed. In fact, the largest group is less-educated homeowners, who a priori could benefit from increased neighborhood demand through rising house values, an important component of household wealth. The distribution of

[12] We stratify by education level and tenure status in 2000, the start of our study period. Less-educated residents are those with a high school degree or less, and more-educated residents are those with some college or more.

[13] For the employment and income outcomes only, we further restrict the sample to individuals less than age 55 in the second period (working age). This is standard and aids interpretation but does not affect our regression results.

[14] The sample counts are the rounded numbers of observations in our data set, while the means of each characteristic are weighted by census-provided person weights. The choice to weight or restrict to householders does not substantively alter any of the patterns described here or our regression results.

years spent living in the original residence also shows that a greater share of renters are recent in-migrants than is sometimes assumed.[15]

2.3 Children Sample and Characteristics

We similarly construct a sample of original resident children aged 15 and younger to study how gentrification affects them.[16] Instead of stratifying results by children's own education level, we stratify by household education level.[17] Table 1, Panel B, shows baseline changes in children's outcomes. While the adult outcomes attempt to capture changes in overall well-being, for children we focus on their individual educational and labor market outcomes, measured in 2010-2014, as well as changes in their exposure to neighborhood characteristics shown by Chetty et al. (2018) to be correlated with intergenerational economic mobility: neighborhood poverty rate, neighborhood share of individuals with a college degree or more, and number of employed individuals in the neighborhood.[18] We emphasize that we construct each child's change in exposure to these neighborhood characteristics by comparing the value for the neighborhood in which the child resides in 2010-2014 to the value for the neighborhood in which the child resides in 2000 (which is the origin neighborhood), regardless of whether it is the same neighborhood.[19] We do not include out-migration for children because results are similar to those for adults. Table 3 describes children's individual and household characteristics in 2000, which we use as controls in our regressions.

[15] Much of the concern about displacement is about longer-term residents. "Individual lived here 5 years ago" and "Household moved in" both show that around half of renters had lived in their 2000 residence for more than 5 years and only 22 percent for more than 10 years. We will find limited heterogeneity in the effect of gentrification on out-migration by these variables, suggesting they are useful for attempting to quantify the total number of longer-term residents affected by gentrification.

[16] Results are similar if we focus on samples of children 18 and younger or 12 and younger. We present results for children 15 and younger because they maximize our sample size (relative to only including children 12 and younger) and ensure that everyone has some minimum possible exposure to neighborhood changes before making college and employment decisions (relative to including children who are 16, 17, and 18).

[17] Less-educated households are those in which the highest education level obtained among all adults (18 or older) in the household in 2000 was a high school degree or less, and more-educated households are those in which at least one adult attended some college or more.

[18] We further restrict the samples for educational and labor market outcomes to children who are at least 16 years old in the second period we observe them. Results are not sensitive to this choice.

[19] Empirically, we will find that all of the gentrification-related changes in exposure to these characteristics are driven by changes occurring within the origin neighborhood (and thus experienced by stayers), not by changes driven by moving across neighborhoods. Having changes in neighborhood characteristics over time is therefore key. This is why we do not estimate effects on existing measures of intergenerational economic mobility, which only exist for a single point in time.

2.4 Defining Gentrification

Following the most recent research on the causes of gentrification, we conceptualize gentrification as an increase in college-educated individuals' demand for housing in initially low-income, central city neighborhoods (Baum-Snow and Hartley 2017; Couture and Handbury 2017). We measure gentrification specifically as the change from 2000 to 2010-2014 in the number of individuals aged 25+ with a bachelor's degree or more living in tract j in city c, divided by the total population aged 25+ living in tract j and city c in 2000:

$$gent_{jc} \equiv \frac{bachelors25_{jc,2010} - bachelors25_{jc,2000}}{total25_{jc,2000}} \,. \tag{1}$$

We fix the denominator at its 2000 level to avoid mechanically correlating gentrification with less-educated population decline. Neighborhoods experiencing large positive changes in $gent_{jc}$ are said to gentrify more than those experiencing smaller or negative changes. Across all gentrifiable neighborhoods in our sample, the mean of gentrification is 0.06. We also model gentrification using a binary variable equal to one if a neighborhood is in the top decile of $gent_{jc}$ across all neighborhoods in our sample and zero otherwise. This picks up important nonlinearities in the effects and is our preferred specification.[20] The mean level of gentrification within the top decile of neighborhoods is 0.37. While we prefer our gentrification measure to alternatives based on increases in aggregate neighborhood incomes, rents, or house values, our main takeaways are broadly similar when using these other measures.[21] Figures 1 and 2 and Table A1 describe patterns of gentrification using our binary definition and suggest that it is in fact picking up the neighborhoods and cities where people talk about gentrification occurring.[22]

Table 4 describes neighborhood characteristics in 2000 by gentrification status. The 10 percent of neighborhoods classified as gentrifying using our binary measure look quite different according to some measures yet very similar according to others. For exam-

[20] Results are robust to alternative nonlinear categorizations and are available upon request. We calculate percentiles using the distribution across all 10,000 neighborhoods in all 100 CBSAs in order to introduce an element of "absolute" gentrification into our definition. This allows, for example, a city like New York to have more than 10 percent of its neighborhoods defined as gentrifying. Results are similar when calculating gentrification percentiles within each CBSA.

[21] We dislike using these alternative measures for our study in part because they take as given many of the outcomes we are interested in studying: what happens to neighborhood incomes, rents, and house values when neighborhoods experience high-skill housing demand shocks.

[22] For example, the New York map in Figure 1 captures gentrification in north and central Brooklyn, the Lower East Side, and Harlem, among other places. Patterns in Figure 2 also match those discussed in popular media: areas north and east of the National Mall in Washington DC, areas north of downtown Portland OR, areas in downtown Seattle near Amazon, and areas south and east of downtown Atlanta near the BeltLine. The 10 most gentrifying central cities according to Table A1 are Washington DC, Portland OR, Seattle, Atlanta, Denver, Charleston, Austin, Boston, Raleigh, and Richmond.

ple, gentrifying neighborhoods started with higher education levels (21 percent college-educated vs. 13 percent), higher self-reported house values ($225,000 vs. $160,000), and lower minority shares (51 percent vs. 56 percent). Yet both types of neighborhoods had similar initial median household incomes ($41,000), median rents ($800), and share poverty (24 percent). These mixed differences suggest some neighborhoods may already have begun gentrifying before 2000, which is supported directly by the fact that gentrifying neighborhoods also experienced higher levels of gentrification over the previous decade. Gentrifying neighborhoods also had much lower initial populations (2,500 vs. 3,400), potentially allowing them to absorb new demand and helping explain our modest out-migration effects. Consistent with previous research on the causes of gentrification, gentrifying neighborhoods were also closer to the central business district, closer to other high-income neighborhoods, had a larger share of old housing (built before 1940), and were more likely to be near a coastline, providing additional support for the validity of our definition. We control for all of these characteristics, as well as changes from 1990 to 2000 for those that vary over time, in our regressions.

3 Model of Gentrification, Location, and Well-Being

The previous section shows that gentrifiable neighborhoods are quite dynamic (cross-neighborhood migration is high) and diverse (more- and less-educated homeowners each compose about one quarter of the population), suggesting the well-being and distributional effects of gentrification may not be clear-cut. In this section, we therefore develop a simple neighborhood choice model to highlight how gentrification affects original resident well-being through the various outcomes explored above and to anchor our empirical approach. Intuitively, it captures the idea that in any given neighborhood, over the course of a decade some original residents will choose to move and some will choose to stay. Gentrification affects the overall well-being of these original residents through its effect on two margins: the number of individuals choosing to move instead of stay (out-migration) and changes in the observable outcomes of both movers and stayers. The out-migration margin includes both the pecuniary costs (time and money spent finding and moving to a new location) and nonpecuniary costs (loss of proximity to friends and family, networks, or other neighborhood-specific human capital) of leaving the origin neighborhood. While we do not observe these, the total unobserved costs to original residents are increasing in the out-migration effect.

We begin with a standard model of neighborhood choice similar to those in Moretti (2011), Kline and Moretti (2014), and Busso et al. (2013). Individuals i choose a neighborhood j to live in at time t to maximize utility as a function of wages w, rents r, commuting costs κ, and neighborhood amenities a:

$$\begin{aligned} u_{ij}^t &= w_{ij}^t - r_{ij}^t - \kappa_{ij}^t + a_{ij}^t + \epsilon_{ij}^t \\ &= w_{ij}^t(H_j^t) - r_{ij}^t(H_j^t) - \kappa_{ij}^t(H_j^t) + a_{ij}^t(H_j^t) + \epsilon_{ij}^t \ . \end{aligned} \tag{2}$$

Gentrification can affect original resident utility because, based on existing results in the literature, each component of utility is a function of the number of high-skill individuals H in the neighborhood. Rents (or house values) are a function of high-skill individuals because housing supply is upward sloping. Wages are a function of high-skill individuals to capture the fact that increases in the number of such individuals could increase demand for local goods and services (Mian and Sufi 2014). These benefits could accrue in part to original neighborhood residents because of better information about new jobs, better commutes, or other reasons. Finally, neighborhood amenities may improve endogenously as a function of the number of high-skill individuals in a neighborhood (Diamond 2016; Su 2018). ϵ_{ij}^t is the fixed, idiosyncratic utility individuals derive from their origin neighborhood.

For all original residents of neighborhood j, their change in utility from 2000 to 2010-2014 can be written as the sum of changes among those endogenously choosing to stay in j and those endogenously choosing to leave for another neighborhood j':

$$\sum_{ij} \Delta u_{ij\cdot} = \sum_{ij} ((1 - Pr[move_{ij}])\Delta u_{ijj} + Pr[move_{ij}]\Delta u_{ijj'}) \ . \tag{3}$$

We will ignore the summations, so that the following discussion applies to the average original resident.

3.1 Effect of Gentrification

Differentiating equation 3 with respect to gentrification (ΔH_j) and rearranging reveals that the effect of gentrification on changes in original resident utility depends on three terms:[23]

$$\frac{\partial}{\partial \Delta H_j}\Delta u_{ij\cdot} = \underbrace{(1 - Pr[move_{ij}])\frac{\partial \Delta u_{ijj}}{\partial \Delta H_j}}_{\text{Always stayers}} + \underbrace{Pr[move_{ij}]\frac{\partial \Delta u_{ijj'}}{\partial \Delta H_j}}_{\text{Always movers}} + \underbrace{\frac{\partial Pr[move_{ij}]}{\partial \Delta H_j}(\Delta u_{ijj'} - \Delta u_{ijj})}_{\text{Induced movers}} \ . \tag{4}$$

[23] Appendix D describes these effects in additional detail.

Equation 4 makes clear why out-migration itself is not evidence of harm. It is not evidence of harm for those who out-migrate, since their observable outcomes may be unchanged and unobserved migration costs may be small. It also not evidence of harm for the average original resident, as even if if out-migrants are in fact made worse off, stayers might be made better off. Thus, determining whether gentrification actually harms or benefits original residents requires estimating its effects on both out-migration and other important observable outcomes, among both those who choose to move and those who choose to stay.

The first two terms of equation 4 are straightforward. The last term, the effect on induced movers, captures utility changes that accrue to individuals on the margin of moving.[24] These individuals are induced into moving from their original neighborhood by gentrification. We can estimate the first part of this margin, the effect of gentrification on the probability of moving, directly with our data. The second part, $(\Delta u_{ijj'} - \Delta u_{ijj})$, captures the change in utility among those moving from j to j' minus the change in utility among those staying in j. It includes an observed part (Δw, Δr, $\Delta \kappa$, and Δa) that we can estimate directly in our data and an unobserved part $(\epsilon_{ij'}^{2010} - \epsilon_{ij}^{2000})$ that we cannot.

This captures a key idea about moving. Moving affects residents' utility not only through observed changes in neighborhood characteristics but also in proportion to the potential loss of unobservable fixed, idiosyncratic benefits of living in the origin neighborhood j instead of the next-best neighborhood j'. These might include the benefits of living near friends and family and other forms of neighborhood capital or community attachment. If these are small or zero, then conditional on changes in observable utility we measure, evidence of out-migration may not be a concern. However, if they are sizable, then the unobserved harms from gentrification are increasing in the out-migration effect. Given the importance of displacement in gentrification debates, we do not make assumptions about the strength of these unobserved costs. More work is needed to better understand the pecuniary and nonpecuniary costs of moving across neighborhoods.

4 Empirical Approach

Given that gentrification is not randomly assigned, there are at least three major challenges to establishing a causal effect of gentrification in our cross-sectional setting: selection and omitted variables bias, spatial spillovers, and reverse causality. Omitted individual and neighborhood characteristics correlated with both gentrification and outcomes

[24]Gentrification could also reduce the probability of moving, so that "induced movers" would be more accurately described as "induced stayers."

create a selection problem and will bias our estimated gentrification effects.[25] [26] Spatial spillovers in how gentrification affects original residents could bias OLS estimates toward zero (when spillovers are from gentrifying to nongentrifying neighborhoods) or away from zero (when spillovers are from one gentrifying neighborhood to another), and Figures 1 and 2 suggest both could be present. Finally, reverse causality could arise if increasing out-migration from a neighborhood contributes to more college-educated in-migration to that neighborhood, perhaps through greater vacancy or falling rents. We address this concern by showing that our results are very similar when restricting the sample to individuals who lived in their origin neighborhood in 1995, five years before we start to measure gentrification.[27] We address omitted variables bias and spatial spillovers using the following three methods, which rely on different assumptions and identifying variation to establish a causal effect. They yield similar results, thus providing plausible bounds for the causal effects of gentrification.

4.1 OLS Regression Model

To determine the effect of gentrification on original resident outcomes, we first estimate the following OLS models:

$$\Delta Y_{ijc} = \beta_0 + \beta_1 gent_{jc} + \beta_2 X_{ijc} + \beta_3 W_{jc} + \beta_4 \Delta W_{jc,1990s} + \beta_5 gent_{jc,1990s} + \mu_c + \epsilon_{ijc} \ . \quad (5)$$

The dependent variable ΔY_{ijc} is one of our individual observable well-being or out-migration outcomes. We estimate models with binary outcomes as linear probability models. We estimate models using our binary definition of gentrification, as described in Section 2.4, and include some results using the continuous measure in Appendix A. X_{ijc} is a vector of detailed individual, household, and housing unit characteristics in 2000 described in Tables 2 and 3.[28] For models where the dependent variable is the change in self-reported rents or house values, employment status and income, commuting distance,

[25]This will create different directions of bias depending on the nature of selection and the particular outcome. For example, if individuals choose subsequently gentrifying neighborhoods because they anticipate changes they prefer or new job opportunities, this would bias effects on out-migration downward and bias effects on employment upward. If instead unobservably more mobile individuals select into subsequently gentrifying neighborhoods, this would bias effects on out-migration upward.

[26]Post-2000 neighborhood changes, such as rezonings or new transit, that are caused by gentrification should be considered part of the treatment effect of gentrification and are not problematic.

[27]These results are not included here but are available upon request. We remove sources of purely mechanical correlations when constructing our gentrification measure, as described before.

[28]Though not shown in Table 2, in the actual regressions, we include age as fixed effects and break out the minority indicator variable into a set of more detailed indicators.

and neighborhood poverty, we also control for the 2000 level of that variable.[29]

W_{jc} is a vector of neighborhood characteristics in 2000 and includes things found in previous research to be correlated with gentrification or migration or both. These include the education and income levels of the neighborhood, the mobility level in the neighborhood, other neighborhood demographic and housing characteristics (Lee and Lin 2018), distance to the nearest high-income neighborhood (top quartile of CBSA) (Guerrieri et al. 2013), distance to the central business district (Couture and Handbury 2017; Baum-Snow and Hartley 2017), and proximity to the coast (Lee and Lin 2018). Table 4 provides the complete list of these along with means by neighborhood gentrification status. $\Delta W_{jc,1990s}$ is a vector of changes in the same time-varying neighborhood characteristics from 1990 to 2000, and $gent_{jc,1990s}$ is gentrification in the neighborhood from 1990 to 2000. These help control for neighborhood pre-trends that could be correlated with gentrification. We always include CBSA fixed effects μ_c and cluster standard errors at the tract level.[30] OLS identifies a causal effect of gentrification with a standard unconfoundedness assumption: conditional on our controls, gentrification is as good as randomly assigned. While unlikely to hold exactly, Altonji and Mansfield (2018) show that controlling for observed group average characteristics using detailed demographic data can in some cases completely control for bias from individual sorting on unobservables.[31]

4.2 Oster Robustness

To assess the robustness of our results to remaining selection and omitted variables, we use an estimator recently developed by Oster (2017) that builds on ideas from Altonji et al. (2005) that are often referred to as "coefficient stability." The estimator uses changes in the gentrification coefficient and model R-squared without and with control variables to understand the potential influence of remaining unobservables under two assumptions. The "Oster estimates" are obtained as follows. First, we estimate a version of equation 5 with only gentrification and CBSA fixed effects to obtain a baseline gentrification coefficient and model R-squared. Second, we estimate the full version of equation 5 to obtain

[29] Controlling for baseline levels of our dependent variables (ΔY_{ijc}) in this way has no effect on our OLS point estimates but significantly improves model R^2 for some outcomes, particularly changes in house values, yielding more informative Oster estimates. While it is known that controlling for baseline levels in a change model can yield biased estimates of the baseline variable, unbiased estimates of those coefficients is not our goal.

[30] Including CBSA fixed effects precludes estimating effects of city-level increases in education levels that may affect original residents of all neighborhoods. When we estimate models where we replace the CBSA fixed effects with CBSA-level controls, a CBSA-level measure of gentrification, and its interaction with tract-level gentrification, we obtain insignificant coefficients for CBSA gentrification and the interaction term and coefficients for tract gentrification that are similar to those from equation 5.

[31] Given the quality of our controls, this may be particularly plausible in our setting.

a gentrification coefficient and model R-squared with full controls. The Oster estimator uses as inputs the change in gentrification coefficient, the change in model R-squared, an assumption about the maximum possible R-squared in a model with all remaining unobservables (R^{max}), and an assumption about the influence of remaining unobservables relative to the influence of full controls (δ). With these inputs, it provides a gentrification coefficient estimate that corrects for possible bias from remaining unobservables. We use Oster's rule-of-thumb values of $R^{max} = 1.3$ times the R-squared from our model with full controls and $\delta = 1$.[32] [33] The strength of this approach hinges on the quality of control variables available to the researcher. Given the large set of individual and household controls available in the census and the large set of neighborhood controls and pre-trends we assemble based on previous research, we believe this approach is particularly well suited to our setting.

4.3 Spatial First Differences

We also estimate spatial first differences (SFD) models as developed by Druckenmiller and Hsiang (2018), which leverage a different source of identifying variation and yield causal estimates for gentrification using a complementary and weaker set of assumptions than OLS and Oster. Intuitively, the model organizes all neighborhoods into a two-dimensional grid, with each neighborhood assigned a row and column index. Within each row, differences are taken across adjacent columns (neighborhoods). The estimating equation is a "spatially first differenced" version of equation 5:

$$\Delta(\Delta Y_{irc}) = \alpha_0 + \alpha_1 \Delta gent_{rc} + \alpha_2 \Delta X_{irc} + \Delta \upsilon_{irc} \ . \tag{6}$$

$\Delta(\Delta Y_{irc}) = \Delta Y_{irc} - \Delta Y_{irc-1}$ is a vector of differences in how individual outcomes change from 2000 to 2010-2014 between adjacent neighborhoods (columns c) within a row r. $\Delta gent_{rc} = gent_{rc} - gent_{rc-1}$ is a vector of differences in gentrification levels between adjacent neighborhoods, and $\Delta X_{irc} = X_{irc} - X_{irc-1}$ is an optional vector of differences in individual and neighborhood controls between adjacent neighborhoods.[34]

[32] Oster develops these rule-of-thumb values through a re-analysis of results from randomized experiments. These values allow 90% of the results from randomized experiments to remain significant. We implement the estimator using the Stata package psacalc, available from the Boston College Statistical Software Components (SSC) archive.

[33] An alternative way to assess robustness is to assume values for one of R^{max} or δ and to "tune" the other until the Oster estimate equals zero (or until the OLS confidence interval includes zero). Though not included here, this exercise reveals that our key out-migration and poverty results are only truly zero for unlikely values for the sign and influence of remaining unobservables. Results are available upon request.

[34] In practice, we first create means of individual outcomes and controls within each neighborhood, as

The estimator compares how outcomes evolve differently across the boundary of adjacent neighborhoods where one gentrifies (and the other does not) with how outcomes evolve differently across the boundary of adjacent neighborhoods where neither gentrifies or both gentrify. The identifying assumption is that unobservables are constant across adjacent neighborhood pairs. The assumption may be particularly plausible for individual unobservables: even if individuals select into general areas, whether they end up in one specific neighborhood as opposed to the adjacent neighborhood may be quasi-randomly determined by search timing, availability of vacancies, etc. Some version of this assumption is commonly used in spatial differencing approaches.[35] As described by Druckenmiller and Hsiang (2018), a priori SFD should work well when omitted variables are highly spatially correlated with the treatment of interest and observations are densely packed in space, both of which are likely true in our setting.

Importantly, SFD also address the problem of spatial spillovers. By estimating the effect of gentrification using comparisons of adjacent neighborhoods, one of which gentrified and one of which did not, SFD restricts the source of bias to the scenario in which spillovers are from gentrifying to nongentrifying neighborhoods (removing the scenario in which they are from one gentrifying neighborhood to another). It thus restricts the sign of bias toward zero.[36]

5 Effects of Gentrification on Adults

Table 5 shows OLS and Oster estimates of the effects of gentrification in our full sample of original resident adults. While effects in the full sample are most important for understanding the overall effect of gentrification, we discuss them alongside estimates from Table 6, which we obtain by first stratifying our sample by the endogenous choice to move or stay. They help us understand what may be driving the overall effects and whether movers specifically may be observably harmed. We discuss robustness to SFD in a later subsection.

described in Appendix D.

[35]Another way of thinking of identification in our setting is using the SFD equivalent of the standard difference-in-differences parallel trends assumption: absent gentrification, outcomes would have evolved similarly across neighborhood boundaries in adjacent pairs where one neighborhood gentrified and the other did not as in adjacent pairs where either both neighborhoods gentrified or neither did.

[36]While there may still be spillovers from other nearby gentrifying neighborhoods not in the specific pair, this should not bias our results. If both neighborhoods in the specific pair are near other gentrifying neighborhoods, the bias from spillovers cancels out. If only one of the neighborhoods in the specific pair is near other gentrifying neighborhoods, this is only problematic if nearness is systematically correlated with which neighborhood within the specific pair gentrified. Our results are robust to many different ways of constructing specific pairs, suggesting this is not the case.

5.1 Out-Migration

We first explore out-migration, the most controversial aspect of gentrification. According to our model, it is the channel through which gentrification could cause unobserved harm to original residents. Column 1 of Table 5 suggests that gentrification increases the probability that less-educated renters move to any other neighborhood by about 3 percentage points. The effect on moves to a neighborhood at least one mile away is higher, around 5 percentage points, perhaps reflecting spatial correlation in gentrification.[37] The Oster estimates are 1 to 2 percentage points higher than the OLS estimates.[38] This suggests that if anything, omitted variables may be biasing our OLS estimates downward (toward zero), so that they represent a lower bound on the true effect of gentrification on less-educated renter out-migration. It is also reassuring that the Oster estimates are similar in magnitude to the OLS estimates. Given the large number of individual and neighborhood controls we are able to include in our models, we believe that the OLS and Oster estimates provide plausible, informative bounds on the true effect. Table A2 uses the sample of less-educated renters to highlight patterns of OLS selection and the key empirical inputs into the Oster estimator.

Our interpretation of these results is that gentrification increases moves by less-educated renters to other neighborhoods by 4 to 6 percentage points. Recall from Table 1 that across all gentrifiable neighborhoods (regardless of gentrification status), 68 percent of less-educated renters move to any other neighborhood and 60 percent move to a neighborhood at least one mile away. At most, then, on average gentrification increases less-educated renter moves to other neighborhoods by around 10 percent (6 / 60). Move effects for more-educated renters are smaller, around 2 to 3 percentage points, as might be expected. An important caveat is that here, the Oster estimates are closer to zero than the OLS estimates, suggesting a slight upward bias from omitted variables. There are fewer expectations of how gentrification should affect moves by homeowners. It might increase moving if owners sell to cash in on appreciating house values or are unable to keep up with property tax payments on rising house values. It might also decrease moving if owners can afford rising property taxes and enjoy improvements in neighborhood quality or choose to hold on to the appreciating home as an asset. Empirically, we find that gentrification in fact increases moving by both less- and more-educated homeowners by around 3 percentage points, and these results are Oster-robust. The fact that out-

[37] In separate results, we find evidence for this idea, as gentrification slightly decreases the probability of moving to a neighborhood within one mile relative to not moving or moving to neighborhoods farther away.

[38] We do not include Oster estimate standard errors. These are obtainable via bootstrap, but in practice they are almost identical to the OLS standard errors.

migration effects are similar across homeowners, renters, and education levels, despite these groups likely having different abilities to remain in their neighborhoods, suggests to us that idiosyncratic preferences for origin neighborhoods may not be very strong on average. Gentrification also increases the probability that less-educated renters leave the CBSA entirely by around 4 percentage points, on a much lower baseline move rate of 15 percent. Interestingly, this effect looks to be zero for all other types of adults, suggesting that less-educated renters are differentially more likely to leave a housing and labor market entirely when their neighborhood gentrifies.[39]

Table 6, Panel A, provides additional evidence on how we should interpret the out-migration results. It shows that for all types of individuals, movers from gentrifying neighborhoods do not experience worse changes in observable outcomes than movers from nongentrifying neighborhoods. That is, they are not more likely to end up in a higher-poverty neighborhood, to become unemployed, or to commute farther than individuals moving from nongentrifying neighborhoods. This suggests that on average and over the course of a decade, gentrification does not appear to cause particularly constrained or otherwise suboptimal relocations. Though not shown here, the findings are the same for movers who exit the CBSA entirely.

5.2 Observable Well-Being

Neighborhood Poverty Neighborhood poverty is an important measure of neighborhood quality, and research has shown that the poverty rate of one's neighborhood can affect the physical and mental health of adults and the long-run educational attainment and earnings of children (Kling et al. 2007; Ludwig et al. 2012; Chetty et al. 2016). While it may be expected that an influx of college-educated individuals would lower a neighborhood's poverty rate mechanically, it is not guaranteed that it would reduce the poverty exposure of the average original resident.[40] Table 5 shows that gentrification does in fact decrease the average original resident's exposure to neighborhood poverty, by around 3.5 percentage points for less-educated renters and owners and slightly less for more-educated individuals. The Oster estimates are again only about 1 percentage point away from the OLS estimates, and they again suggest that the OLS estimate for less-educated renters is a lower bound. The baseline change in poverty exposure for less-educated renters over

[39]This result is consistent with the findings from Diamond et al. (2018) that the introduction of rent control in San Francisco decreased, by similar amounts, both the probability that renters left their origin neighborhood and the probability that they left the city entirely.

[40]For example, if all original residents were displaced, none would be exposed to the new lower poverty rate. Or if some did stay but others were displaced to higher-poverty neighborhoods, the overall effect could be to increase poverty exposure.

the decade was zero (Table 1), so gentrification appears to have led to an absolute decline in poverty exposure for this group. Table 6, Panel B shows that these overall effects are driven almost entirely by stayers: less-educated renters staying in gentrifying neighborhoods experience declines in exposure to poverty that are 7 percentage points larger than those staying in nongentrifying neighborhoods. Magnitudes are again similar across all types of individuals and very Oster-robust.

Rents Table 5 shows that somewhat surprisingly, gentrification has no effect on reported monthly rents paid by original resident less-educated renters. Rents increased on average for these individuals by $126 (Table 1), so gentrification simply did not increase rents paid by these individuals even further. Table 6 shows that the effect is also close to zero for less-educated renter stayers. By contrast, gentrification increases monthly rents paid by the average more-educated renter by around $50, with this effect driven by stayers ($90). The fact that we find large rent effects for more-educated renters, driven by stayers, but not for less-educated renters suggests that more-educated renters may have greater willingness to pay for neighborhood changes associated with gentrification or that there is some degree of rental market segmentation.[41] This is consistent with recent findings of differences in preferences for urban consumption amenities by skill and the increasing importance of these amenities in explaining the location choices of the college-educated (Couture and Handbury 2017; Diamond 2016; Su 2018). The small effects for less-educated renters could also be explained by sticky rents. Subsidized housing does not explain the result.[42] These results caution against using simple neighborhood median rents when studying gentrification, as is almost always done. Changes in median rents can miss important segmentation and heterogeneity, leading to incorrect conclusions about how the housing costs paid by different types of households are actually affected.

House Values Tables 5 and 6 also show that gentrification increases original resident house values and that these are driven by increases for stayers. Less-educated homeowners staying in their origin neighborhood experience increases in self-reported house values of around $15,000 on a baseline change of almost $40,000. Increases for more-educated homeowner stayers are slightly higher: $20,000 on a baseline of almost $60,000. While we find no effect here for movers (for whom we are simply comparing self-reported house

[41]If less-educated renters occupy lower-quality rental housing, that housing may be considered less of an option by college-educated in-migrants.

[42]We test the role of subsidized housing by matching our sample to Department of Housing and Urban Development (HUD) administrative data on rental assistance. Subsidized individuals are a small share of our less-educated renter sample, and dropping them does not substantially change the results.

values at two different times and locations), we show below that gentrification also causes large increases in aggregate neighborhood median house values. Thus, movers may be experiencing benefits from rising neighborhood house values not reflected in this table. While it is true that rising house values may also increase property taxes, which may be difficult to afford, we believe it is more likely to be a benefit given the importance of housing in household wealth, particularly as a share of wealth for less-educated or lower-income households. Though not shown here, we find little evidence that gentrification affects the probability that renters become homeowners or vice versa.

Employment, Income, and Commuting Finally, Tables 5 and 6 suggest that in general, gentrification has neither a positive nor a negative effect on original residents' employment, income, or commuting distance. The exception is more-educated homeowners, for whom gentrification increases their income by around $3,000 for the average original resident and by $5,000 for those endogenously choosing to stay (relative to similar individuals staying in nongentrifying neighborhoods). These results suggest that more-educated owners may benefit from an influx of more-educated individuals to their neighborhood, perhaps through new local job opportunities or networks.

5.3 Adult Robustness to Spatial First Differences

Table A3 shows SFD estimates of the effect of gentrification on adult outcomes. For each of our four key types of individuals, we show results for four specifications: without and with controls and for two different ways of constructing the neighborhood indices.[43] While the estimates are generally less precise than the OLS and Oster estimates, the pattern of results is very similar, suggesting that our overall conclusions are robust to some remaining sources of omitted variable bias and strengthening our causal arguments. Specifically, they show that gentrification increases out-migration, decreases exposure to neighborhood poverty, and has few effects on other individual adult outcomes. The biggest difference is that SFD shows no effect of gentrification on original resident house values, whereas OLS and Oster show that gentrification increases original resident house values.

5.4 Heterogeneity

We test for heterogeneity along a number of individual, neighborhood, and CBSA dimensions and generally do not find many differences. However, we do find substantive patterns of heterogeneity along two key dimensions. The first is individuals with low

[43] Appendix D describes in detail how we implement the SFD estimator.

ability to pay, which we separately measure as households in poverty, households with incomes below $15,000 per year, and households with high initial rent burdens. The second is neighborhoods in the early stages of the gentrification process, which we separately measure as neighborhoods with low initial education levels, very low initial incomes, and very low initial rents.[44] Table A4 shows effects of gentrification for less-educated renters using two measures of these dimensions.

The first four columns stratify by whether less-educated renters are also in poverty. Gentrification increases moves for those in poverty by 5 to 10 percentage points, while it only increases moves for those not in poverty by 2 to 4 percentage points, consistent with the former being less able to afford rent increases and being more likely to move instead. However, we also find stronger poverty reduction effects in this subsample. Though not shown here, we again find no evidence that movers move to worse neighborhoods or otherwise end up observably worse off than similar movers from nongentrifying neighborhoods.

The last four columns show that gentrification also has stronger effects among less-educated renters who started in neighborhoods with very low education levels (college share less than 5 percent). In these neighborhoods, gentrification increases moves among less-educated renters by 5 to 10 percentage points versus 3 to 6 percentage points for those in more-educated neighborhoods. This suggests that out-migration effects may be stronger in the earliest stages of gentrification. We again find stronger poverty reduction effects in this subsample and no evidence that movers end up in worse neighborhoods or with worse individual outcomes. We have not adjusted standard errors for multiple testing, so we avoid taking a strong stand on the statistical significance of these results. Nevertheless, they suggest that the overall out-migration effects we estimate for less-educated renters in Table 5 may mask some stronger effects for these two subsamples: individuals with very low incomes and neighborhoods in the early stages of the gentrification process. Each represents about one quarter of the less-educated renter population and one sixteenth of the total population in gentrifiable neighborhoods. Policies intending to help disadvantaged households remain in gentrifying neighborhoods could be targeted to these groups.

5.5 Gentrification and Aggregate Neighborhood Change

To better quantify how neighborhoods change, we also use our data to show how gentrification is associated with aggregate neighborhood demographic changes. Table A5 describes baseline changes from 2000 to 2010-2014, and Table A6 shows tract-level estimates of

[44] Specifically, in the bottom quartile of our sample of gentrifiable neighborhoods.

the effect of gentrification on these changes.[45] Both reveal similar patterns.[46] Table A6 shows that unsurprisingly, gentrification is associated with large decreases in aggregate neighborhood poverty rates and large increases in employment, income, rents, and house values. Most importantly, it also shows that while gentrification greatly increases the total neighborhood population, it has no effect on the change in the aggregate population of less-educated individuals. Table A5 shows that the less-educated population was declining across all gentrifiable neighborhoods in our sample, so gentrification does not accelerate this decline. The OLS, Oster, and SFD results are generally similar. Overall, given that we find no effect of gentrification on aggregate less-educated population, and contrasting the large aggregate effects we find with the smaller original resident effects, we infer that the aggregate neighborhood changes occurring in these neighborhoods because of gentrification are driven less by the direct displacement of original residents and more by changes to the quantity and composition of in-migrants. This process is sometimes referred to as "indirect displacement."

6 Effects of Gentrification on Children

Table 7 shows that the average child starting in a neighborhood that subsequently gentrifies ends up in a neighborhood that has lower poverty, more college-educated residents, and more employed residents. These have been shown by Chetty et al. (2018) to be correlated with neighborhoods that promote intergenerational economic mobility.[47] Thus, it appears that gentrification may increase children's exposure to high-opportunity neighborhoods.[48]

Table 8, Panel B, shows that these results are driven by those endogenously choosing to stay in the origin neighborhood. The decline in poverty exposure experienced by stayer children in less-educated households is around 6 percentage points, and the average poverty rate in all gentrifiable neighborhoods in 2000 was 24 percent (Table 4).[49]

[45] Regression models are identical to those in equation 5, except we exclude individual controls and no longer need to cluster at the tract level.

[46] Descriptive differences between gentrifying and nongentrifying neighborhoods in Table A5 are equivalent to coefficient estimates from a version of the OLS model used to generate Table A6 that omits controls.

[47] Their neighborohood employment measure is the share of individuals living in a neighborohod who are employed, while ours is a count (the numerator of the share). Though not included here, results are similar for the share. We can include the share in future drafts.

[48] We do not directly estimate effects of gentrification on the measure of intergenerational economic mobility from Chetty et al. (2018) because it does not vary over time. We view estimating effects on changes in exposure to known correlates of this opportunity measure as the next best option.

[49] By way of comparison, children below age 13 in the Moving to Opportunity (MTO) experiment studied by Chetty et al. (2016) began in neighborhoods with 41 percent poverty rates and experienced

It is not surprising that our measure of gentrification is associated with large increases in aggregate neighborhood education levels or declines in neighborhood poverty rates. What is new is our finding that many original resident children are able to remain in these neighborhoods and experience these changes. Housing subsidies targeted to gentrifying neighborhoods could further encourage families with children to stay in improving neighborhoods, complementing current approaches that focus on increasing moves from low- to high-opportunity neighborhoods. Importantly, Panel A of Table 8, also shows that, as with adults, children who move from gentrifying neighborhoods do not end up in observably worse neighborhoods or with worse other outcomes than children who move from nongentrifying neighborhoods.

Table 7 also provides some evidence that gentrification increases the probability that the average child in a less-educated homeowner household will attend and complete college. Table 8 shows that this effect is driven by stayers, consistent with the idea that greater exposure to improving neighborhood opportunity is driving the result. For example, increased exposure to college-educated adults could provide role models, information, or networks. The effects for less-educated owner stayers are around 11 percentage points, with baseline college attendance and completion rates of 48 percent and 9 percent, respectively (Table 1). The fact that the baseline probability of staying in the origin neighborhood is highest for less-educated homeowner households could explain why we find effects for children in these households and not those in others. Our inability to detect educational effects for other types of children or labor market effects for any children may in part reflect our inability to better measure the actual duration of exposure to neighborhoods (which Chetty et al. (2016) and Chetty and Hendren (2018a,b) find is important for detecting neighborhood effects), our more limited time horizon, and the fact that absolute reductions in poverty exposure, even among stayers, are lower than those experienced by mover households in the MTO experiment.

6.1 Children Robustness to Spatial First Differences

Table A7 shows SFD estimates of the effect of gentrification on children's outcomes. As with adults, for each of our four key types of individuals, we show results for four specifications: without and with controls and for two different ways of constructing the tract indices. Also as with adults, SFD estimates are generally less precise than the OLS estimates. Similarly to the children's OLS and Oster results, SFD shows that gentrification increases children's exposure to our three measures of neighborhood opportunity, though

declines in poverty exposure of 22 percentage points if taking up the experimental voucher and 12 percentage points if taking up the Section 8 voucher.

due to noisiness, the significance depends on how exactly the channel indices are created (column A vs. columns B).[50] SFD shows little effect of gentrification on children's individual outcomes. However, the results for the probability that the children of less-educated homeowners attend some college are similar in direction and magnitude to the OLS and Oster estimates, though imprecise. Finally, while SFD suggests negative effects on the probability that children in more-educated households complete college, this is offset by an increase in the probability that they are employed, suggesting gentrification (and perhaps associated opportunities) changes the relative value of working versus going to college.

7 Conclusion

Gentrification has increased substantially over the past two decades, reversing decades of urban decline. Yet the distributional consequences of gentrification are unclear and much debated. More specifically, concern that gentrification displaces or otherwise harms original neighborhood residents has featured prominently in the rise of urban NIMBYism and the return of rent control as a major policy option.[51] This paper constructs novel longitudinal census microdata to provide the first comprehensive, causal evidence of how gentrification actually harms and benefits original resident adults and children. Overall, we find that many original residents, including the most disadvantaged, are able to remain in gentrifying neighborhoods and share in any neighborhood improvements. Perhaps most importantly, low-income neighborhoods that gentrify appear to improve along a number of dimensions known to be correlated with opportunity, and many children are able to remain in these neighborhoods. This could provide new options for policies designed to increase children's exposure to high-opportunity neighborhoods, for example by targeting subsidies to help them stay in neighborhoods that are improving around them. While there is some evidence that gentrification increases out-migration, movers are not made observably worse off, and high baseline mobility means that almost all of neighborhood demographic change is explained by changes to in-migration, not direct displacement. Accommodating rising demand for central urban neighborhoods, such as through building more housing, could maximize the integrative benefits we find, minimize the out-migration effects we find, minimize gentrification pressures in nearby neighborhoods, and minimize

[50] Details are in Appendix D. Results for additional channel indices and channel heights show generally the same pattern and are available upon request.

[51] NIMBYism (Not in My Backyard) refers to organized political opposition that seeks to prevent the construction of new housing in a local area. It was traditionally used to refer to suburban homeowners but has recently been used to refer to urban renters. It has spawned a counter-movement advocating for more urban housing, YIMBYism.

aggregate rent increases that dampen future in-migration (Mast 2019; Nathanson 2019; Guerrieri et al. 2013).

Two important questions remain unresolved. First, the effects described above are average effects for our four key types of original residents. We find slightly larger out-migration effects for the most disadvantaged residents, though a caveat is that they represent a small share of our total sample and are also not made observably worse off. Targeted policy solutions could help these residents remain in improving neighborhoods while still promoting growth overall. Second, while we find that movers are not made observably worse off, they may still incur unobserved costs of moving, such as loss of proximity to friends and family, networks, or other neighborhood-specific human capital. To our knowledge, the only existing estimates of these unobserved cross-neighborhood costs suggest a total fixed moving cost of around \$42,000, which increases by a somewhat modest amount of around \$300 per year of living in a neighborhood (Diamond et al. 2018). Providing more and better estimates of the costs of moving across neighborhoods, building on the large existing literature estimating cross-state and cross-labor market moving costs, is an important area for future research.

More generally, the modest gentrification effects we find are partly explained by the fact that neighborhoods are far more dynamic than is typically assumed. Cross-neighborhood migration over the course of a decade is high (70 percent of less-educated renters and 80 percent of more-educated renters move to another neighborhood), allowing neighborhoods to change quickly primarily through changes to the composition of in-migrants, not the direct displacement of incumbents. Further exploration of the levels, dynamics, and causes of cross-neighborhood migration using longitudinal microdata, which has important implications for the distributional consequences of neighborhood change as well as the incidence and efficiency of place-based treatments (Busso et al. 2013), is an interesting area for future research.

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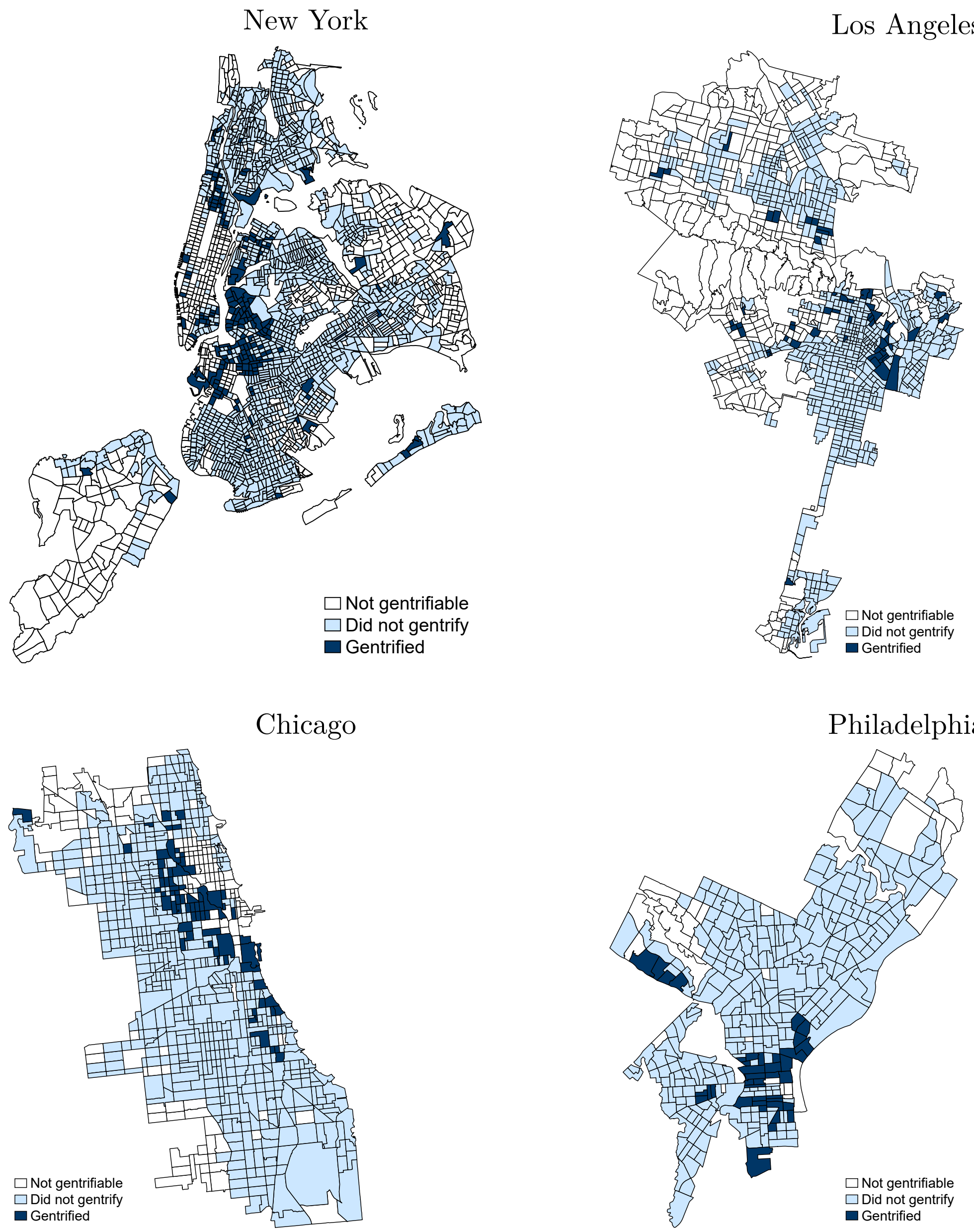
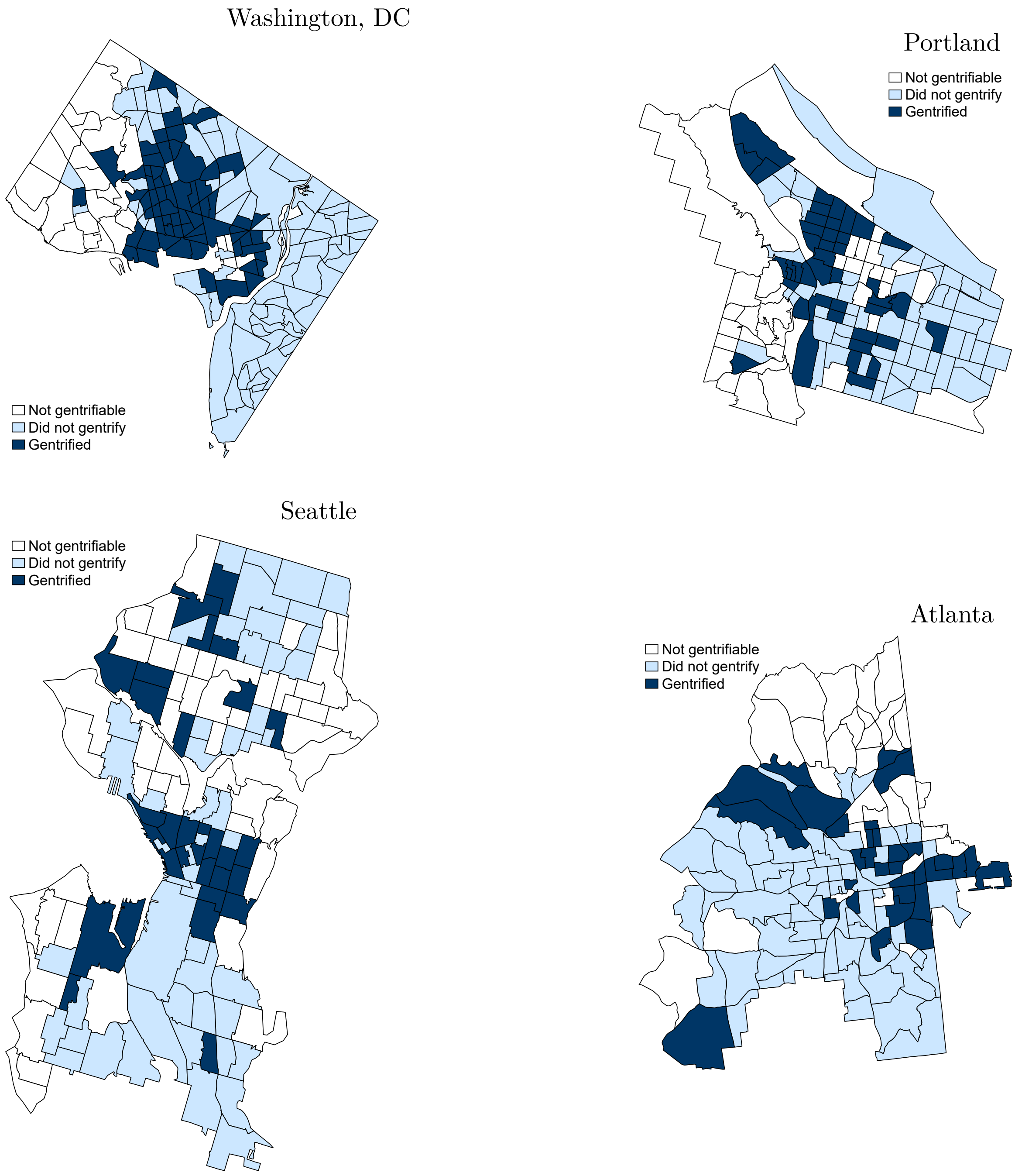
Figure 1: Gentrification in the Four Most Populous Metropolitan Areas



Notes: Population based on Core-Based Statistical Area (CBSA) in 2000. Gentrifiable tracts (light blue) are low-income census tracts of the largest central city in the CBSA. Gentrifying tracts (dark blue) are those in the top decile of our continuous gentrification measure. All numbers created using public use data in order to avoid disclosure issues. Source: Public use versions of the Census 2000 Long Form and 2010-2014 5-Year ACS Estimates.

Figure 2: Gentrification in the Four Most Gentrifying Central Cities



Notes: Most gentrifying central cities are defined as those with the highest shares of all gentrifiable neighborhoods that gentrified from 2000 to 2010-2014. Ordering is Washington, DC, Portland, Seattle, and Atlanta. Gentrifiable tracts (light blue) are low-income census tracts of the largest central city in the CBSA. Gentrifying tracts (dark blue) are those in the top decile of our continuous gentrification measure. Source: Public use versions of the Census 2000 Long Form and 2010-2014 5-Year ACS Estimates. All numbers created using public use data in order to avoid disclosure issues.

Table 1: Summary Changes in Original Resident Adult and Children Outcomes, 2000 to 2010-2014

Panel A: Adults

	Less-Educated Renters	More-Educated Renters	Less-Educated Owners	More-Educated Owners
Move (pp)	0.68	0.79	0.34	0.42
Move 1 mile (pp)	0.60	0.74	0.32	0.40
Exit CBSA (pp)	0.15	0.25	0.09	0.13
Change in poverty exposure (pp)	0.00	-0.02	0.03	0.02
Change in rent or house value ($)	126	171	38,490	63,340
Change in employment (pp)	0.04	-0.04	0.03	-0.03
Change in income ($)	1,160	8,481	-745	4,723
Change in commute (miles)	1.66	2.80	0.55	2.05
N	28,000	24,000	37,000	38,000

Panel B: Children

	Less-Educated Renters	More-Educated Renters	Less-Educated Owners	More-Educated Owners
Change in tract poverty pp)	-0.01	-0.02	0.02	0.02
Change in tract share college (pp)	0.05	0.05	0.05	0.07
Change in tract employment	96	63	67	-10
Some college or more	0.41	0.56	0.48	0.66
College degree or more	0.06	0.12	0.09	0.21
Employed	0.49	0.53	0.59	0.63
Income	9,199	10,580	11,640	13,610
N	14,500	11,000	7,500	13,500

Notes: Means of original resident outcomes by key individual types, 2000 to 2010-2014. Migration variables are means of binary indicator variables. Others are measured as changes with units in parentheses: percentage point (pp), dollars ($), thousands of dollars (1,000s $), and miles. Numbers of individuals rounded to the nearest 1,000. Source: Census 2000 Long Form and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table 2: Adult Characteristics, 2000

	Less-Educated Renters	More-Educated Renters	Less-Educated Owners	More-Educated Owners
Individual characteristics				
Householder	0.64	0.71	0.51	0.58
Age	44	40	51	47
Female	0.59	0.55	0.56	0.54
Minority	0.73	0.47	0.57	0.40
Not English language	0.40	0.25	0.30	0.18
Individual lived here 5 years ago	0.49	0.34	0.76	0.67
Household characteristics				
Married two-parent family	0.41	0.34	0.66	0.61
Other family	0.34	0.21	0.21	0.17
Nonfamily household	0.24	0.46	0.13	0.22
Children $<$ 18 present	0.51	0.32	0.42	0.37
Number of people in household	3.31	2.42	3.44	2.89
Household income	45,700	67,320	71,080	102,900
Household moved in				
$<=$ 1 year ago	0.24	0.31	0.06	0.09
2-5 years ago	0.36	0.41	0.18	0.23
6-10 years ago	0.17	0.14	0.15	0.18
$>$ 10 years ago	0.22	0.14	0.60	0.49
Building type				
Single family detached	0.20	0.15	0.70	0.70
Single family attached	0.09	0.06	0.16	0.14
Apartment	(By units)	(By units)	(Any)	(Any)
2	0.12	0.11	0.14	0.16
3-4	0.13	0.13	NA	NA
5-10	0.11	0.12	NA	NA
10-20	0.09	0.11	NA	NA
20-50	0.11	0.12	NA	NA
$>$ 50	0.16	0.19	NA	NA
Building year built				
1995 to 2000	0.02	0.03	0.02	0.03
1990 to 1995	0.03	0.03	0.02	0.02
1980 to 1989	0.09	0.11	0.05	0.07
1970 to 1979	0.15	0.16	0.09	0.09
1960 to 1969	0.16	0.15	0.14	0.13
1950 to 1959	0.16	0.15	0.22	0.19
1940 to 1949	0.14	0.11	0.15	0.12
Before 1940	0.25	0.26	0.31	0.34
Individual baseline outcomes				
Initial rent or house value	785	983	144,000	200,000
Initial employment	0.53	0.79	0.53	0.78
Initial income	18,220	37,010	21,360	44,300
Initial commute distance	3.48	6.46	3.40	6.36
N	28,000	24,000	37,000	38,000

Notes: These are the year 2000 individual and household characteristics included as controls in the regression models. Means for each variable by key individual types. Numbers of individuals rounded to the nearest 1,000. Source: Census 2000 Long Form. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table 3: Children Characteristics, 2000

	Less-Educated Renters	More-Educated Renters	Less-Educated Owners	More-Educated Owners
Individual characteristics				
Age	6.64	6.47	7.77	7.37
Female	0.50	0.49	0.49	0.50
Minority	0.86	0.76	0.73	0.56
Not English language	0.43	0.34	0.43	0.23
Individual lived here 5 years ago	0.44	0.43	0.63	0.68
Household characteristics				
Married two-parent family	0.41	0.51	0.69	0.75
Other household type	0.58	0.49	0.30	0.24
Number of people in household	4.73	4.45	5.17	4.76
Household income	33,590	53,600	59,120	92,180
Rent or house price	768	927	133,800	186,700
Household moved in				
<= 1 year ago	0.33	0.29	0.12	0.10
2-5 years ago	0.41	0.43	0.33	0.31
6-10 years ago	0.16	0.15	0.22	0.24
> 10 years ago	0.10	0.12	0.32	0.35
Building type				
Single family detached	0.21	0.22	0.69	0.74
Single family attached	0.11	0.10	0.18	0.14
Apartment	(By units)	(By units)	(Any)	(Any)
2	0.12	0.12	0.13	0.12
3-4	0.13	0.13	NA	NA
5-10	0.12	0.11	NA	NA
10-20	0.09	0.09	NA	NA
20-50	0.10	0.10	NA	NA
> 50	0.11	0.14	NA	NA
Building year built				
1995 to 2000	0.03	0.03	0.04	0.04
1990 to 1995	0.03	0.04	0.02	0.03
1980 to 1989	0.10	0.11	0.06	0.07
1970 to 1979	0.17	0.18	0.10	0.10
1960 to 1969	0.18	0.17	0.14	0.13
1950 to 1959	0.17	0.15	0.21	0.20
1940 to 1949	0.13	0.12	0.16	0.13
Before 1940	0.20	0.21	0.27	0.31
N	14,500	11,000	7,500	13,500

Notes: These are the year 2000 individual and household characteristics included in the regression models. Means for each variable by key individual types. Numbers of individuals rounded to the nearest 1,000. Source: Census 2000 Long Form. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table 4: Neighborhood Characteristics, 2000

By Binary Gentrification Status

	Not Gentrifying	Gentrifying
Share college	0.13	0.21
Median household income	40,440	41,040
Share employed	0.90	0.92
Share in poverty	0.24	0.24
Share minority	0.56	0.51
Share renters	0.58	0.69
Median rent	770	808
Median house value	159,600	225,000
Average age of housing	42	43
Share housing before 1940	0.26	0.37
Population	3,724	2,865
Population density	17,170	19,170
Within 500 meters of coast	0.06	0.13
Vacancy	0.08	0.10
Share lived here 5 years ago	0.48	0.44
Distance from CBD		
$<$ 1 mile	0.04	0.15
1-2 miles	0.11	0.20
2-5 miles	0.38	0.41
5-10 miles	0.35	0.19
$>$ 10 miles	0.11	0.05
Distance from high-income tract		
$<$ 1 mile	0.11	0.23
1-2 miles	0.29	0.37
2-3 miles	0.27	0.20
3-5 miles	0.26	0.16
$>$ 5 miles	0.07	0.03
Gentrification from 1990 to 2000	0.04	0.10
N	9,000	1,000

Notes: These are the year 2000 neighborhood characteristics included as controls in the regression models. Changes in these characteristics from 1990 to 2000 are also included in the regression models. Means for each variable by neighborhood level of gentrification. Number of neighborhoods rounded to the nearest 500. Source: Census 2000 Long Form and Lee and Lin (2018). These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table 5: Effect of Gentrification on Original Resident Adults

Among All Original Residents (Stayers and Movers)

	Less-Educated Renters		More-Educated Renters		Less-Educated Owners		More-Educated Owners	
	OLS	Oster	OLS	Oster	OLS	Oster	OLS	Oster
Move	0.0313***	0.043	0.0236***	0.0176	0.0252*	0.0282	0.0314***	0.0154
	(0.012)		(0.009)		(0.0152)		(0.0121)	
	0.183		0.211		0.117		0.148	
Move 1 mile	0.0479***	0.0662	0.0306***	0.0268	0.0292**	0.0316	0.0353***	0.0226
	(0.0128)		(0.00985)		(0.0149)		(0.0121)	
	0.182		0.208		0.115		0.143	
Exit CBSA	0.0400***	0.0456	0.0279**	0.0116	0.00306	0.00172	0.0114	-0.00429
	(0.0102)		(0.012)		(0.0092)		(0.0096)	
	0.0715		0.101		0.0468		0.058	
Tract poverty	-0.0328***	-0.0372	-0.0169***	-0.0118	-0.0351***	-0.0287	-0.0286***	-0.0177
	(0.00367)		(0.00267)		(0.00377)		(0.00317)	
	0.275		0.335		0.233		0.24	
Rent or house value	-11.23	-10.73	49.61**	45.63	16570***	12020	23830***	17990
	(15.48)		(22.01)		(6329)		(5870)	
	0.28		0.266		0.288		0.245	
Employment	-0.0082	-0.0103	-0.00362	0.0106	-0.0009	0.00251	-0.000416	0.00788
	(0.0173)		(0.0106)		(0.0224)		(0.0126)	
	0.441		0.391		0.437		0.372	
Income	-635.2	-929.1	-219.3	-1151	248.4	-332.7	3158**	2542
	(973.2)		(1187)		(1407)		(1596)	
	0.185		0.123		0.263		0.105	
Commute distance	-0.0271	-0.804	-2.315	-3.162	-0.576	-0.141	7.601	6.724
	(3.447)		(2.479)		(0.502)		(6.144)	
	0.216		0.336		0.647		0.334	
N	28,000		24,000		37,000		38,000	

Notes: Binary gentrification measure. All models include CBSA fixed effects and full controls: individual and household characteristics in 2000, tract characteristics in 2000, changes in tract characteristics from 1990 to 2000, and gentrification from 1990 to 2000. OLS standard errors in parentheses clustered at the tract level, followed by R-squared. Oster estimates described in Section 4.2. Numbers of individuals rounded to the nearest 1,000. Source: Census 1990 Long Form, Census 2000 Long Form, and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table 6: Effect of Gentrification on Original Resident Adults

By Endogenous Move Status

Panel A: Movers

	Less-Educated Renters		More-Educated Renters		Less-Educated Owners		More-Educated Owners	
	OLS	Oster	OLS	Oster	OLS	Oster	OLS	Oster
Tract poverty	-0.00930**	-0.0131	-0.00513*	0.000618	0.00917	0.0116	0.000873	0.00982
	(0.0045)		(0.00296)		(0.00559)		(0.0039)	
Rent or house value	-36.48*	-33.88	28.89	29.16	-8080	-3980	5294	4516
	(19.91)		(29.03)		(10650)		(9585)	
Employment	-0.0107	-0.0107	-0.00948	0.00139	-0.0119	0.00694	-0.00738	0.00166
	(0.0194)		(0.0111)		(0.0334)		(0.0163)	
Income	-1002	-1292	-566.9	-1679	-1219	-1832	1761	1270
	(1087)		(1278)		(2151)		(2100)	
Commute distance	0.467	-0.275	-2.504	-3.478	-1.623	-2.131	12.93	10.77
	(4.161)		(2.777)		(1.057)		(9.41)	
N	19,000		19,000		12,000		16,000	

Panel B: Stayers

	Less-Educated Renters		More-Educated Renters		Less-Educated Owners		More-Educated Owners	
	OLS	Oster	OLS	Oster	OLS	Oster	OLS	Oster
Tract poverty	-0.0686***	-0.07	-0.0535***	-0.0479	-0.0569***	-0.0438	-0.0491***	-0.0325
	(0.00544)		(0.0056)		(0.00461)		(0.00436)	
Rent or house value	17.07	12.6	91.36***	84.82	23880***	14090	35240***	25730
	(20.33)		(24.29)		(7885)		(6953)	
Employment	0.00563	-0.00669	0.0147	0.0457	0.00967	-0.00117	0.0246	0.0354
	(0.0374)		(0.0333)		(0.0313)		(0.0202)	
Income	1959	2156	-237.6	-285.1	1881	1401	5550**	5074
	(2027)		(2864)		(1899)		(2417)	
Commute distance	-2.687	-3.75	-0.293	1.076	0.3	1.333	-2.806**	-2.136
	(3.403)		(2.558)		(0.444)		(1.313)	
N	9,000		5,000		25,000		23,000	

Notes: Binary gentrification measure. We stratify the sample from Table 5 by endogenous move status and estimate the main regression models. All models include CBSA fixed effects and full controls: individual and household characteristics in 2000, tract characteristics in 2000, changes in tract characteristics from 1990 to 2000, and gentrification from 1990 to 2000. OLS standard errors in parentheses clustered at the tract level. Oster estimates described in Section 4.2. Numbers of individuals rounded to the nearest 1,000. Source: Census 1990 Long Form, Census 2000 Long Form, and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table 7: Effect of Gentrification on Original Resident Children

Among All Original Residents (Stayers and Movers)

	Less-Educated Renters		More-Educated Renters		Less-Educated Owners		More-Educated Owners	
	OLS	Oster	OLS	Oster	OLS	Oster	OLS	Oster
Tract poverty	-0.0245***	-0.0293	-0.00762	-0.00862	-0.0241***	-0.0213	-0.0355***	-0.0287
	(0.0064)		(0.00675)		(0.00877)		(0.00597)	
	0.301		0.297		0.268		0.214	
Tract share college	0.0408***	0.0528	0.0356***	0.0471	0.0648***	0.0721	0.0714***	0.0711
	(0.00655)		(0.00776)		(0.011)		(0.00744)	
	0.203		0.254		0.135		0.139	
Tract employment	194.1***	157.6	75.12	22.5	255.4***	222.6	142.5***	98.64
	(50.94)		(47.26)		(66.62)		(49.85)	
	0.277		0.268		0.233		0.228	
Some college or more	-0.0116	-0.0297	0.0045	0.00664	0.0578	0.0635	0.00221	-0.0073
	(0.0261)		(0.0288)		(0.0383)		(0.0263)	
	0.11		0.142		0.132		0.133	
College degree or more	-0.0135	-0.0231	-0.0191	-0.0269	0.0499**	0.0406	-0.0343	-0.0503
	(0.0141)		(0.02)		(0.025)		(0.0226)	
	0.115		0.169		0.168		0.215	
Employment	-0.000181	-0.0000162	0.0395	0.0483	0.0276	0.0215	0.0172	0.0179
	(0.0273)		(0.0296)		(0.0382)		(0.026)	
	0.107		0.104		0.125		0.113	
Income	-892.9	-999.2	1442	1276	-446.1	-956.5	-245.1	-623.4
	(777.3)		(1107)		(1427)		(1151)	
	0.157		0.171		0.201		0.207	
N	14,500		11,000		7,500		13,500	

Notes: Binary gentrification measure. All models include CBSA fixed effects and full controls: individual and household characteristics in 2000, tract characteristics in 2000, changes in tract characteristics from 1990 to 2000, and gentrification from 1990 to 2000. OLS standard errors in parentheses clustered at the tract level, followed by R-squared. Oster estimates described in Section 4.2. Numbers of individuals rounded to the nearest 1,000. Source: Census 1990 Long Form, Census 2000 Long Form, and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table 8: Effect of Gentrification on Original Resident Children

By Endogenous Move Status

Panel A: Movers

	Less-Educated Renters		More-Educated Renters		Less-Educated Owners		More-Educated Owners	
	OLS	Oster	OLS	Oster	OLS	Oster	OLS	Oster
Tract poverty	-0.00934	-0.014	0.00744	0.0074	0.0164	0.0142	-0.0146*	-0.00795
	(0.00755)		(0.00771)		(0.0122)		(0.00857)	
Tract share college	0.0150**	0.0318	0.00478	0.0212	-0.0109	0.00645	0.0126	0.0182
	(0.00637)		(0.0083)		(0.0113)		(0.00988)	
Tract employment	100.6*	50.25	22.75	-27.36	11.83	-63.58	-140.7**	-210.1
	(59.14)		(56.28)		(104.3)		(69.36)	
Some college or more	-0.00542	-0.0233	-0.00137	-0.00491	-0.0104	-0.00789	-0.00485	-0.0108
	(0.029)		(0.0329)		(0.0525)		(0.0369)	
College degree or more	-0.00826	-0.0194	-0.0155	-0.0265	0.000773	-0.011	-0.0263	-0.0482
	(0.0164)		(0.023)		(0.0307)		(0.0296)	
Employment	-0.0164	-0.0201	0.0227	0.021	0.0416	0.0479	-0.014	-0.0195
	(0.0322)		(0.0338)		(0.0519)		(0.0363)	
Income	-1217	-1413	1288	859.9	-555	-1505	-664.4	-1328
	(916)		(1269)		(2112)		(1594)	

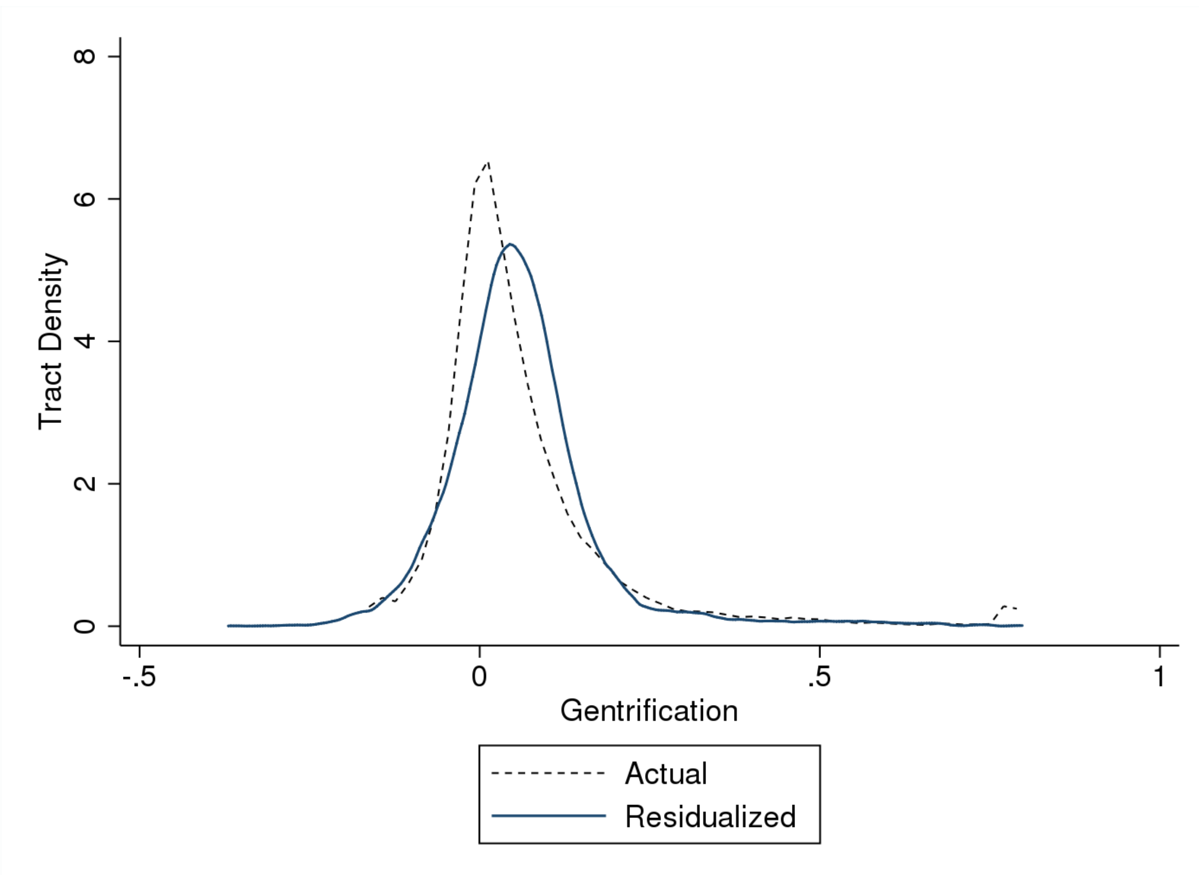
Panel B: Stayers

	Less-Educated Renters		More-Educated Renters		Less-Educated Owners		More-Educated Owners	
	OLS	Oster	OLS	Oster	OLS	Oster	OLS	Oster
Tract poverty	-0.0642***	-0.0592	-0.0612***	-0.0607	-0.0695***	-0.0481	-0.0553***	-0.0424
	(0.00866)		(0.00939)		(0.00945)		(0.00715)	
Tract share college	0.131***	0.0845	0.143***	0.0961	0.150***	0.0954	0.142***	0.0993
	(0.00794)		(0.0086)		(0.00833)		(0.00742)	
Tract employment	521.0***	616.6	376.2***	432.2	440.2***	468.1	474.5***	464.4
	(60)		(44.07)		(61.37)		(48.47)	
Some college or more	-0.0348	-0.054	0.0304	0.0715	0.121**	0.138	0.00215	-0.0144
	(0.0552)		(0.0593)		(0.0553)		(0.0385)	
College degree or more	-0.0283	-0.0343	-0.0246	-0.0132	0.110***	0.104	-0.0399	-0.0498
	(0.0272)		(0.0418)		(0.0403)		(0.0341)	
Employment	0.0137	0.00689	0.141**	0.229	0.0047	-0.0104	0.0528	0.0606
	(0.052)		(0.0665)		(0.0546)		(0.0367)	
Income	-562.2	-870	1690	1889	-145.7	-345.2	-445	-715.6
	(1585)		(2201)		(1611)		(1520)	

Notes: Binary gentrification measure. We stratify the sample from Table 7 by endogenous move status and estimate the main regression models. All models include CBSA fixed effects and full controls: individual and household characteristics in 2000, tract characteristics in 2000, changes in tract characteristics from 1990 to 2000, and gentrification from 1990 to 2000. OLS standard errors in parentheses clustered at the tract level. Oster estimates described in Section 4.2. Numbers of individuals not included to avoid disclosure risk. Source: Census 1990 Long Form, Census 2000 Long Form, and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Appendix A: Additional Results

Figure A1: Gentrification Variation, 2000 to 2010-2014



Notes: Kernel densities of gentrification. Across all tracts (dotted gray line), the mean is 0.06. The mean within the top decile of all tracts (our binary gentrification measure) is 0.37. Dotted gray line is winsorized at the 1st and 99th percentiles. Blue line is residualized with neighborhood controls and CBSA fixed effects. The sample consists of the 10,000 low-income, central city tracts of the 100 largest metropolitan areas. Source: Census 2000 Long Form and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table A1: Gentrification in Selected Central Cities

Panel A: 10 Most Populous CBSAs

Central City	Population Rank	Gentrifiable Tracts	Gentrifying Tracts	Percent Gentrifying	Gentrification Rank
New York, NY	1	1,513	185	12.2	21
Los Angeles, CA	2	666	54	8.1	36
Chicago, IL	3	649	69	10.6	28
Philadelphia, PA	4	342	39	11.4	25
Dallas, TX	5	205	12	5.9	48
Miami, FL	6	81	11	13.6	17
Washington, DC	7	151	66	43.7	1
Houston, TX	8	309	31	10.0	30
Detroit, MI	9	276	2	0.7	78
Boston, MA	10	137	31	22.6	8

Panel B: 10 Most Gentrifying Central Cities

Central City	Population Rank	Gentrifiable Tracts	Gentrifying Tracts	Percent Gentrifying	Gentrification Rank
Washington, DC	7	151	66	43.7	1
Portland, OR	25	104	43	41.3	2
Seattle, WA	15	82	30	36.6	3
Atlanta, GA	11	97	32	33.0	4
Denver, CO	22	112	30	26.8	5
Charleston, SC	84	20	5	25.0	6
Austin, TX	40	107	25	23.4	7
Boston, MA	10	137	31	22.6	8
Raleigh, NC	58	47	10	21.3	9
Richmond, VA	46	55	10	18.2	10

Notes: Population based on Core-Based Statistical Area (CBSA) in 2000. Most gentrifying central cities are defined as those with the highest shares of all gentrifiable neighborhoods that gentrified from 2000 to 2010-2014. Gentrifiable tracts are low-income census tracts of the largest central city in the CBSA. Gentrifying tracts are those in the top decile of our continuous gentrification measure. Source: Public use versions of the Census 2000 Long Form and 2010-2014 5-Year ACS Estimates. All numbers created using public use data in order to avoid disclosure issues.

Table A2: Selection Details for Adult Effects

Among Less-Educated Renter Original Residents

	No Controls	Individual Controls	Tract Controls	Full Controls
Move	0.0108	0.0109	0.0358***	0.0313***
	(0.0125)	(0.0115)	(0.0127)	(0.012)
	0.0594	0.173	0.0864	0.183
Move 1 mile	0.0220*	0.0209*	0.0531***	0.0479***
	(0.0129)	(0.0122)	(0.0133)	(0.0128)
	0.0801	0.17	0.106	0.182
Exit CBSA	0.0316***	0.0290***	0.0417***	0.0400***
	(0.00992)	(0.00959)	(0.0103)	(0.0102)
	0.0304	0.0657	0.042	0.0715
Tract poverty	-0.0239***	-0.0232***	-0.0317***	-0.0328***
	(0.00456)	(0.00449)	(0.00374)	(0.00367)
	0.067	0.0818	0.25	0.275
Rent or house value	-10.54	-11.05	-19.11	-11.23
	(16.89)	(15.13)	(17.94)	(15.48)
	0.025	0.258	0.0354	0.28
Employment	-0.00073	-0.0015	0.0113	-0.0082
	(0.0233)	(0.0168)	(0.0239)	(0.0173)
	0.0235	0.436	0.0292	0.441
Income	81.2	-140.8	-204	-635.2
	(1023)	(918.7)	(1078)	(973.2)
	0.0151	0.18	0.0198	0.185
Commute distance	2.041	0.717	0.125	-0.0271
	(2.53)	(2.587)	(3.336)	(3.447)
	0.00677	0.213	0.012	0.216
N	28,000	28,000	28,000	28,000

Notes: Binary gentrification measure. OLS estimates of effect of gentrification on original resident adults using four different sets of controls: none, individual (and household) only, tract (and tract lags) only, and full. All models include CBSA fixed effects. Standard errors in parentheses, followed by R-squared. Shows how the gentrification OLS estimate changes with four different sets of controls: none, individual (and household) only, tract (and tract lags) only, and full controls. Results in the last column, Full Controls, correspond to the OLS estimates in Table 5. Comparing results across columns provides some insight into the extent of selection when going from no controls to full controls and how selection is driven by unobservables that are correlated with individual vs. neighborhood characteristics. The coefficients with no controls and full controls and the R-squareds with full controls are the key empirical inputs into the Oster estimator described in Section 4.2. Source: Census 1990 Long Form, Census 2000 Long Form, and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table A3: Spatial First Differences Estimates of the Effect of Gentrification on Original Resident Adults

	Less-Educated Renters		More-Educated Renters		Less-Educated Owners		More-Educated Owners	
	A	B	A	B	A	B	A	B
Move	0.00991	0.0313**	0.0225	0.0352*	0.0345	0.0194	0.0287	-0.00945
	(0.0168)	(0.0153)	(0.0158)	(0.0189)	(0.0285)	(0.0286)	(0.0238)	(0.0219)
	[0.57]	[0.077]	[0.19]	[0.12]	[0.26]	[0.51]	[0.22]	[0.66]
	0.0301	0.0281	0.0301	0.0287	0.0307	0.0424	0.0317	0.0345
Move 1 mile	0.00844	0.0320**	0.025	0.0278	0.0455*	0.0111	0.032	-0.0039
	(0.0196)	(0.0155)	(0.0193)	(0.0235)	(0.0264)	(0.0257)	(0.023)	(0.0209)
	[0.66]	[0.098]	[0.25]	[0.3]	[0.094]	[0.67]	[0.17]	[0.88]
	0.0315	0.0265	0.0346	0.034	0.0307	0.0398	0.033	0.0361
Exit CBSA	0.0419***	0.0615***	0.012	0.0161	0.00736	-0.0000977	0.0174	-0.015
	(0.0146)	(0.0142)	(0.0188)	(0.0172)	(0.0117)	(0.0118)	(0.0153)	(0.0139)
	[0.02]	[0.017]	[0.54]	[0.38]	[0.54]	[0.99]	[0.26]	[0.27]
	0.0316	0.0349	0.0405	0.0453	0.0395	0.0431	0.0327	0.036
Tract poverty	-0.0256***	-0.0286***	-0.00952**	-0.0119***	-0.0268***	-0.0304***	-0.0256***	-0.0303***
	(0.00529)	(0.00652)	(0.0041)	(0.00403)	(0.00627)	(0.00805)	(0.0052)	(0.00502)
	[0.001]	[0]	[0.042]	[0.005]	[0]	[0.002]	[0]	[0]
	0.0421	0.0361	0.0487	0.0462	0.0355	0.0389	0.0516	0.0463
Rent or house value	-40.94	-38.7	47.73	32.46	4586	3061	-17210	-15450
	(50.16)	(38.05)	(38.23)	(46.24)	(8403)	(9414)	(8897)	(9122)
	[0.85]	[0.75]	[0.23]	[0.5]	[0.59]	[0.75]	[0.075]	[0.14]
	0.0523	0.0545	0.0796	0.0816	0.0346	0.0335	0.041	0.0362
Employment	0.0174	0.0654***	0.00169	0.0126	-0.0344	-0.0323	0.0664***	0.0284
	(0.0296)	(0.0213)	(0.0108)	(0.0249)	(0.053)	(0.056)	(0.0253)	(0.0219)
	[0.7]	[0.067]	[0.87]	[0.63]	[0.57]	[0.59]	[0.025]	[0.21]
	0.0516	0.0527	0.0555	0.0617	0.0579	0.0669	0.0516	0.0436
Income	-2556	2461	-748.6	2349	-1059	804.2	2576	2708
	(1575)	(2005)	(1806)	(2638)	(2034)	(2273)	(2420)	(2719)
	[0.078]	[0.59]	[0.69]	[0.48]	[0.67]	[0.73]	[0.32]	[0.36]
	0.0536	0.0473	0.0507	0.063	0.066	0.0751	0.0561	0.0538
Commute distance	-13.84**	1.57	-0.758	-2.496	-2.268	-0.0607	1.812	5.128
	(5.282)	(1.264)	(4.577)	(4.485)	(2.593)	(1.198)	(1.937)	(4.108)
	[0.2]	[0.2]	[0.87]	[0.6]	[0.53]	[0.97]	[0.34]	[0.23]
	0.0609	0.055	0.058	0.0308	0.0549	0.053	0.086	0.069

Notes: Binary gentrification measure. Tract-level sample of adults constructed as described in Appendix D. All models include CBSA fixed effects and full controls: individual and household characteristics in 2000, tract characteristics in 2000, changes in tract characteristics from 1990 to 2000, and gentrification from 1990 to 2000. Controls are differenced as in equation 6. Parentheses show standard errors clustered at the CBSA level, with asterisks showing corresponding p-values: * 0.1, ** 0.05, *** 0.01. Brackets show p-values from Wild bootstrap blocked at the CBSA level. R-squared shown last. Sample counts similar to overall tract count and not included to avoid disclosure risk. Source: Census 1990 Long Form, Census 2000 Long Form, and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table A4: Heterogeneity of Adult Gentrification Effects

Among Less-Educated Renter Adults Only

	Individual in Poverty		Not in Poverty		Origin Low Education		Not Low Education	
	OLS	Oster	OLS	Oster	OLS	Oster	OLS	Oster
Move	0.0512**	0.0642	0.0203	0.0303	0.0686**	0.097	0.0239*	0.0358
	(0.0208)		(0.0146)		(0.029)		(0.0131)	
Move 1 mile	0.0822***	0.109	0.0309**	0.044	0.0526*	0.0884	0.0460***	0.0679
	(0.0219)		(0.0156)		(0.031)		(0.014)	
Exit CBSA	0.0378**	0.041	0.0410***	0.0474	-0.00447	0.00827	0.0500***	0.0579
	(0.0175)		(0.0126)		(0.0187)		(0.0116)	
Tract poverty	-0.0383***	-0.0438	-0.0294***	-0.0331	-0.0512***	-0.0497	-0.0289***	-0.0275
	(0.00681)		(0.00403)		(0.00979)		(0.00382)	
Rent or house value	-4.992	-7.808	-20.49	-20.83	29.31	27.37	-15.55	-16.14
	(27.43)		(18.71)		(32.32)		(17.83)	
Employment	0.007	-0.02	-0.0205	-0.0172	0.0582	0.0558	-0.0169	-0.0162
	(0.0342)		(0.0195)		(0.0387)		(0.0198)	
Income	1457	1474	-1234	-1683	2010	1894	-1136	-1424
	(1643)		(1188)		(1946)		(1117)	
Commute distance	0.363	0.519	-0.282	-1.561	-0.521	-1.013	-0.326	-1.012
	(0.395)		(4.734)		(1.157)		(3.614)	

Notes: Binary gentrification measure. Effects of gentrification for less-educated renters, further stratified by two different characteristics. The first two columns stratify less-educated renters by individual poverty status. The last two columns stratify less-educated renters by whether their origin neighborhood has a very low initial education level (< .05) or not. All models include CBSA fixed effects and full controls: individual and household characteristics in 2000, tract characteristics in 2000, changes in tract characteristics from 1990 to 2000, and gentrification from 1990 to 2000. Oster estimates described in Section 4.2. Standard errors clustered at tract level included in parentheses. Numbers of individuals not included to avoid disclosure risk. Source: Census 1990 Long Form, Census 2000 Long Form, and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table A5: Summary Changes in Aggregate Neighborhood Outcomes, 2000 to 2010-2014

	Not Gentrifying	Gentrifying
Tract poverty (pp)	0.06	-0.04
Employment (pp)	0.01	0.10
Income	-3,321	7,014
Rent		
All	135	352
Less-educated	116	144
More-educated	140	389
House value		
All	42,510	125,800
Less-educated	39,150	95,130
More-educated	41,750	121,800
Population		
All	-304	610
Less-educated	-381	-293
N	9,000	1,000

Notes: Means of changes in aggregate neighborhood outcomes, 2000 to 2010-2014. Variables are measured as changes with units in parentheses: percentage point (pp) and dollars ($). Percentage points rounded to the nearest thousandth and dollars to the nearest one. Numbers of neighborhoods rounded to the nearest 500. Source: Census 2000 Long Form and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table A6: Effect of Gentrification on Aggregate Neighborhood Characteristics

	OLS	Oster	SFD A	SFD B
Tract poverty	-0.0659***	-0.0511	-0.0551***	-0.0572***
	(0.00322)		(0.00383)	(0.00407)
Share employed	0.0718***	0.06	0.0552***	0.0575***
	(0.00294)		(0.00521)	(0.00482)
Income	7548***	5109	5833***	5991***
	(302.1)		(542.3)	(570.5)
Rent				
All	164.2***	106.8	134.1***	133.5***
	(7.768)		(11.32)	(8.106)
Less-educated	23.88***	51.24	41.63***	35.00***
	(8.632)		(9.876)	(12.47)
More-educated	189.8***	116.8	138.5***	138.0***
	(9.26)		(15.78)	(9.93)
House value				
All	50280***	24730	24810***	25430***
	(3784)		(3769)	(3094)
Less-educated	25340***	-4800	5629	-1831
	(5940)		(5359)	(7132)
More-educated	45910***	-451.5	27950***	22930***
	(4334)		(5109)	(3771)
Population				
All	718.5***	605	606.0***	600.9***
	(42.32)		(65.97)	(69.64)
Less-educated	-21.95	-67.35	24.54	-0.602
	(26)		(30.44)	(28.14)

Notes: Tract-level. Binary gentrification measure. Tract-level models. SFD sample constructed as described in Appendix D. All models include CBSA fixed effects and full tract controls: tract characteristics in 2000, changes in tract characteristics from 1990 to 2000, and gentrification from 1990 to 2000. Oster estimates described in Section 4.2. Standard errors in parentheses, followed by R-squared. Bootstrapped p-values for SFD not included because they give the same inference as OLS. Numbers of neighborhoods rounded to the nearest 500. Source: Census 1990 Long Form, Census 2000 Long Form, and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table A7: Spatial First Differences Estimates of the Effect of Gentrification on Original Resident Children

Among All Original Residents (Stayers and Movers)

	Less-Educated Renters		More-Educated Renters		Less-Educated Owners		More-Educated Owners	
	A	B	A	B	A	B	A	B
Tract poverty	-0.000916	-0.0282***	-0.0078	-0.00388	-0.00355	-0.00432	-0.017	-0.0312***
	(0.011)	(0.0107)	(0.0115)	(0.0109)	(0.023)	(0.0246)	(0.0129)	(0.0103)
	[0.94]	[0.025]	[0.53]	[0.74]	[0.89]	[0.87]	[0.18]	[0.025]
	0.0779	0.0845	0.077	0.0782	0.124	0.127	0.0548	0.0465
Tract share college	0.0305***	0.0398***	0.0345***	0.0474***	0.0309	0.0447**	0.0322***	0.0291**
	(0.0077)	(0.00953)	(0.0126)	(0.0151)	(0.0189)	(0.0202)	(0.0115)	(0.0115)
	[0.011]	[0.003]	[0.067]	[0.075]	[0.13]	[0.059]	]	[0.063]
	0.0808	0.0908	0.0778	0.0762	0.157	0.144	0.0851	0.0684
Tract employment	154.9**	37.86	-78.34	-13.93	396.4**	254.6*	110.3	16.7
	(73.95)	(84.49)	(100.7)	(90.03)	(171.1)	(139.4)	(95.55)	(79.1)
	[0.097]	[0.68]	[0.48]	[0.87]	[0.032]	[0.11]	[0.28]	[0.85]
	0.068	0.0627	0.0785	0.0852	0.134	0.121	0.0639	0.0615
Some college or more	-0.0242	0.0224	0.028	0.0606**	0.145	0.0943	-0.0969	-0.0597
	(0.0482)	(0.0382)	(0.0631)	(0.03)	(0.108)	(0.102)	(0.0595)	(0.06)
	[0.63]	[0.6]	[0.69]	[0.17]	[0.18]	[0.33]	[0.18]	[0.43]
	0.0895	0.0933	0.132	0.144	0.151	0.143	0.107	0.0961
College degree or more	-0.00426	-0.00482	-0.0606**	-0.0229	0.0242	0.0252	-0.0849**	-0.0739*
	(0.0315)	(0.018)	(0.0291)	(0.0368)	(0.0728)	(0.0897)	(0.0327)	(0.0387)
	[0.9]	[0.78]	[0.23]	[0.65]	[0.75]	[0.8]	[0.019]	[0.058]
	0.121	0.0915	0.122	0.114	0.147	0.164	0.099	0.0996
Employment	0.0138	-0.0167	0.0773	0.166***	-0.0936	-0.0483	0.0364	0.116***
	(0.0808)	(0.0651)	(0.0546)	(0.0418)	(0.139)	(0.12)	(0.0578)	(0.0436)
	[0.88]	[0.82]	[0.42]	[0.06]	[0.5]	[0.71]	[0.54]	[0.022]
	0.106	0.111	0.148	0.131	0.142	0.132	0.0833	0.0869
Income	-397	170.9	-1109	5066***	-3932	942.8	-528.4	392.6
	(1237)	(2105)	(1518)	(939.7)	(4200)	(3070)	(1550)	(1203)
	[0.75]	[0.95]	[0.63]	[0.012]	[0.36]	[0.74]	[0.74]	[0.72]
	0.0817	0.106	0.125	0.0913	0.131	0.141	0.0883	0.0629

Notes: Binary gentrification measure. Tract-level sample of children constructed as described in Appendix D. All models include CBSA fixed effects and full controls: individual and household characteristics in 2000, tract characteristics in 2000, changes in tract characteristics from 1990 to 2000, and gentrification from 1990 to 2000. Controls are differenced as in equation 6. Parentheses show standard errors clustered at the CBSA level, with asterisks showing corresponding p-values: * 0.1, ** 0.05, *** 0.01. Brackets show p-values from Wild bootstrap blocked at the CBSA level. R-squared shown last. Sample counts similar to overall tract count and not included to avoid disclosure risk. Source: Census 1990 Long Form, Census 2000 Long Form, and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table A8: Effect of Continuous Gentrification Measure on Original Resident Adults

Among All Original Residents (Stayers and Movers)

	Less-Educated Renters		More-Educated Renters		Less-Educated Owners		More-Educated Owners	
	OLS	Oster	OLS	Oster	OLS	Oster	OLS	Oster
Move	0.119***	0.19	0.0615***	0.0603	0.111***	0.131	0.125***	0.0898
	(0.0311)		(0.0224)		(0.0406)		(0.0314)	
	0.183		0.211		0.117		0.148	
Move 1 mile	0.138***	0.238	0.0636**	0.0705	0.122***	0.141	0.127***	0.0982
	(0.033)		(0.0257)		(0.0394)		(0.0316)	
	0.182		0.208		0.115		0.143	
Exit CBSA	0.0993***	0.131	0.0847***	0.0518	0.0633***	0.0677	0.0735***	0.0388
	(0.0259)		(0.0302)		(0.0239)		(0.024)	
	0.0714		0.101		0.0471		0.0583	
Tract poverty	-0.112***	-0.125	-0.0438***	-0.0308	-0.158***	-0.157	-0.110***	-0.0897
	(0.00992)		(0.00724)		(0.0114)		(0.00855)	
	0.278		0.335		0.24		0.244	
Rent or house value	9.648	8.897	44.3	36.16	74320***	67560	78710***	72430
	(39.12)		(50.84)		(15230)		(16320)	
	0.28		0.265		0.288		0.245	
Employment	-0.0197	-0.016	0.0199	0.0628	-0.0431	-0.0236	0.005	0.0334
	(0.0468)		(0.0277)		(0.0586)		(0.0334)	
	0.441		0.391		0.437		0.372	
Income	-647.2	-747.7	-13.53	-1947	2308	1240	7006*	5490
	(2699)		(3034)		(3289)		(3950)	
	0.185		0.123		0.263		0.105	
Commute distance	-3.545	-6.1	-5.913	-8.697	-0.627	0.369	11.1	8.975
	(9.121)		(4.357)		(1.227)		(7.758)	
	0.216		0.336		0.647		0.333	
N	28,000		24,000		37,000		38,000	

Notes: Continuous gentrification measure. All models include CBSA fixed effects and full controls: individual and household characteristics in 2000, tract characteristics in 2000, changes in tract characteristics from 1990 to 2000, and gentrification from 1990 to 2000. OLS standard errors in parentheses clustered at the tract level, followed by R-squared. Oster estimates described in Section 4.2. Numbers of individuals rounded to the nearest 1,000. Source: Census 1990 Long Form, Census 2000 Long Form, and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Table A9: Effect of Continuous Gentrification Measure on Original Resident Children

Among All Original Residents (Stayers and Movers)

	Less-Educated Renters		More-Educated Renters		Less-Educated Owners		More-Educated Owners	
	OLS	Oster	OLS	Oster	OLS	Oster	OLS	Oster
Tract poverty	-0.103***	-0.12	-0.0438**	-0.0436	-0.0738***	-0.0793	-0.117***	-0.103
	(0.017)		(0.0177)		(0.0267)		(0.0151)	
	0.303		0.297		0.269		0.217	
Tract share college	0.140***	0.165	0.107***	0.123	0.212***	0.231	0.259***	0.248
	(0.0168)		(0.0202)		(0.0323)		(0.0215)	
	0.208		0.255		0.146		0.155	
Tract employment	693.9***	559.6	320.9**	102.1	740.7***	548.2	761.4***	569.1
	(123.8)		(152.7)		(182.3)		(117.7)	
	0.278		0.268		0.234		0.231	
Some college or more	-0.0182	-0.0511	-0.0112	-0.00308	0.0155	0.0507	0.0581	0.0525
	(0.0693)		(0.0792)		(0.103)		(0.0633)	
	0.11		0.142		0.132		0.133	
College degree or more	-0.00175	-0.0199	-0.0391	-0.0544	0.0657	0.0456	-0.0766	-0.0925
	(0.036)		(0.0495)		(0.0635)		(0.0541)	
	0.115		0.169		0.167		0.215	
Employment	-0.0569	-0.0775	-0.0212	-0.00499	0.0377	0.00297	0.0945	0.0969
	(0.0725)		(0.0746)		(0.0962)		(0.0711)	
	0.107		0.104		0.125		0.113	
Income	-1990	-2674	680.9	201.4	-523	-2323	3651	2844
	(2046)		(2770)		(3487)		(3625)	
	0.157		0.171		0.201		0.208	
N	14,500		11,000		7,500		13,500	

Notes: Continuous gentrification measure. All models include CBSA fixed effects and full controls: individual and household characteristics in 2000, tract characteristics in 2000, changes in tract characteristics from 1990 to 2000, and gentrification from 1990 to 2000. OLS standard errors in parentheses clustered at the tract level, followed by R-squared. Oster estimates described in Section 4.2. Numbers of individuals rounded to the nearest 1,000. Source: Census 1990 Long Form, Census 2000 Long Form, and 2010-2014 5-Year ACS Estimates. These results were disclosed by the US Census Bureau's Disclosure Review Board, authorization number CBDRB-FY19-397.

Appendix B: Data Details

Adult Sample We measure out-migration in three ways. The simplest, "Move," is a binary indicator equal to 1 if we observe an individual in a different census tract in 2010-2014 than in 2000. "Move 1 mile" indicates whether an individual moved to a different census tract that is also at least one mile away, and "Exit CBSA" indicates whether an individual moved to a different CBSA.

Housing price outcomes include self-reported gross rents for renters and self-reported house values for homeowners. Changes in rents and house values are created as differences from 2000 to 2010-2014, measured in 2012 dollars. Differences are conditional on individuals being renters or owners in both periods, respectively. We test whether gentrification has an effect on tenure status and find no effect.

We measure adult neighborhood quality using the neighborhood poverty rate. We create this measure longitudinally by assigning to each individual in each of 2000 and 2010-2014 the poverty rate of that neighborhood in that year. We then calculate the difference between them. Declining exposure to poverty helps measure greater socioeconomic integration, which could benefit residents directly and indirectly through improvements to public goods like safety and school quality.

Change in employment takes value 0 if there was no change in employment, -1 if individuals changed from employed to unemployed, and 1 if individuals changed from not employed to employed from 2000 to 2010-2014. We measure change in income as the difference in income from wage sources from 2000 to 2010-2014. It includes both individuals switching from positive income in 2000 to zero income in 2010-2014 and individuals switching from zero income to positive income.[52] Change in commute distance is measured as the difference in the straight-line distance in miles from tract of residence to tract of work from 2000 to 2010-2014. Individuals not working receive a commute distance of zero.

Children Sample We construct three measures children's exposure to neighborhood quality, which have been shown to be correlated with intergenerational mobility (opportunity) (Chetty et al. 2018). Exposure to neighborhood poverty is constructed the same as for adults. Exposure to college share is the same, except we replace the poverty rate with the ratio of the total number of individuals in a neighborhood with a college degree or more to the total number of individuals in that neighborhood. For exposure to neighborhood employment, we replace the poverty rate with the total number of employed individuals in a neighborhood. As with adults, we measure each individual's change in exposure to these neighborhood characteristics by taking the difference between the 2010-2014 value in the 2010-2014 neighborhood of residence and the 2000 value in the 2000 neighborhood of residence. The change in poverty and change in college share are percentage point

[52]Because we do not require individuals to be working in both periods, our average changes in these values from 2000 to 2010-2014 will be lower than expected if individuals are more likely to exit the labor market as they age. We restrict the income and employment samples to individuals who were also less than or equal to age 54 in the second period we observe them, though results are similar in our full sample of all individuals 25 or older.

changes, and the employment change is a count. All children 15 or younger receive values for these variables regardless of their age in the first or second period we observe them.

We include four individual measures of educational and labor market outcomes, observed in 2010-2014. Some college or more is an indicator equal to 1 if an individual had completed attended or completed some college or more by 2010-2014 and 0 otherwise. College or more is a subset of some college and equals 1 only if the individual completed a bachelor's degree or more by 2010-2014. Employed is an indicator equal to 1 if the individual was employed in 2010-2014 and 0 otherwise (whether or not they were actively looking for work). Income is the income in dollars if working and 0 otherwise. Children younger than 16 in the second period we observe them (2010-2014) do not receive values for these four educational and labor market variables, effectively excluding them from the summary statistics and regression samples for these variables.[53]

[53] They are still included in samples used to create summary statistics and regression results for the neighborhood quality outcomes. Educational and labor market results are similar if we do not impose these restrictions.

Appendix C: Model Details

In this section, we develop a simple neighborhood choice model that highlights exactly how gentrification affects original resident well-being through the various outcomes explored above. It does so through its effect on two margins: the number of individuals choosing to move instead of stay in the origin neighborhood (out-migration) and the observable outcomes (that together approximate observable individual utility) of both movers and stayers.

We begin with a standard model of neighborhood choice similar to those in Moretti (2011), Kline and Moretti (2014), and Busso et al. (2013). Individuals choose a neighborhood to live in order to maximize utility as a function of wages, rents, commuting costs, and neighborhood amenities:

$$\begin{aligned} u_{ij}^t &= w_{ij}^t - r_{ij}^t - \kappa_{ij}^t + a_{ij}^t + \epsilon_{ij}^t \\ &= w_{ij}^t(H_j^t) - r_{ij}^t(H_j^t) - \kappa_{ij}^t(H_j^t) + a_{ij}^t(H_j^t) + \epsilon_{ij}^t \ . \end{aligned} \tag{C.1}$$

Wages are expressed as a function of the number of high-skill individuals in a neighborhood to capture the fact that increases in the number of such individuals could increase demand for local goods and services (citations). These benefits could be expected to accrue in part to residents of those neighborhoods for various reasons (better information about new jobs, better commutes, etc.). Rents are a function of number of high-skilled individuals because increased high-skill demand for a neighborhood will put pressure on neighborhood rents if housing supply is upward sloping. Finally, we allow amenities to improve endogenously as a function of the number of high-skill individuals in a neighborhood following work by Diamond (2016) and Su (2018).

Epsilon is the fixed, idiosyncratic utility individuals derive from their origin neighborhood. This will have some shape, which governs how responsive individual migration will be to changes in their neighborhood. Moretti (2011) and Kline and Moretti (2014) discuss the distribution and importance of this parameter. This parameter can also include fixed costs of moving that are constant across all neighborhoods, such as the cost of hiring movers or searching for a new residence.

Changes in Utility Over Time

For all original residents of neighborhood j, their change in utility from 2000 to 2010 can be written as the sum of changes among those endogenously choosing to stay in j and those endogenously choosing to leave for another neighborhood j':

$$\sum_{ij} \Delta u_{ij\cdot} = \sum_{ij} ((1 - Pr[move_{ij}])\Delta u_{ijj} + Pr[move_{ij}]\Delta u_{ijj'}) \ . \tag{C.2}$$

We will ignore the summations for convenience, so that the following results hold for the average original resident.

Effect of Gentrification

Differentiating equation C.2 with respect to gentrification (ΔH_j) and rearranging reveals that the effect of gentrification on changes in original resident utility depends on three margins:[54]

$$\frac{\partial}{\partial \Delta H_j}\Delta u_{ij\cdot} = \underbrace{(1 - Pr[move_{ij}])\frac{\partial \Delta u_{ijj}}{\partial \Delta H_j}}_{\text{Always stayers}} + \underbrace{Pr[move_{ij}]\frac{\partial \Delta u_{ijj'}}{\partial \Delta H_j}}_{\text{Always movers}} + \underbrace{\frac{\partial Pr[move_{ij}]}{\partial \Delta H_j}(\Delta u_{ijj'} - \Delta u_{ijj})}_{\text{Induced movers}} \ . \tag{C.3}$$

Effect on Always Stayers

The first term of equation C.3 counts utility changes accruing to "always stayers." The first part, $1 - Pr[move_{ij}]$, is simply the *ex ante* probability of staying. Using equation C.1, we can write the second part (still suppressing all terms' dependence on ΔH_j), as:

$$\frac{\partial \Delta u_{ijj}}{\partial \Delta H_j} = \frac{\partial}{\partial \Delta H_j}(\Delta w_{ijj} + \Delta r_{ijj} + \Delta \kappa_{ijj} + \Delta a_{ijj} + \Delta \epsilon_{ijj}) \ . \tag{C.4}$$

To be precise about these changes *for stayers*, we write:

$$\Delta x_{ijj} \equiv x_{ij}^{2010} - x_{ij}^{2000} \ .$$

The term $\Delta \epsilon_{ijj}$ equals zero on average, and therefore $\frac{\partial}{\partial \Delta H_j}\Delta \epsilon_{ijj}$ also equals zero.[55]

Effect on Always Movers

The second term of equation C.3 counts utility changes accruing to "always movers." The first part, $Pr[move_{ij}]$, is simply the *ex ante* probability of moving. Using equation C.1, we can write the second part (still suppressing all terms' dependence on ΔH_j), as:

$$\frac{\partial \Delta u_{ijj'}}{\partial \Delta H_j} = \frac{\partial}{\partial \Delta H_j}(\Delta w_{ijj'} + \Delta r_{ijj'} + \Delta \kappa_{ijj'} + \Delta a_{ijj'} + \Delta \epsilon_{ijj'}) \ . \tag{C.5}$$

To be precise about these changes *for movers*, we write:

$$\Delta x_{ijj'} \equiv x_{ij'}^{2010} - x_{ij}^{2000} \ .$$

We observe $\Delta w_{ijj'}$, $\Delta r_{ijj'}$, $\Delta \kappa_{ijj'}$, and $\Delta a_{ijj'}$ in our data and can therefore estimate how each is affected by gentrification in the origin neighborhood.

[54] We take derivatives using the product rule because all parts of equation C.2 are implicit functions of ΔH_j.

[55] By the assumption that the epsilons are random draws, even if gentrification makes the neighborhood worse for some original residents, it will make it better for others. We can also say that empirical evidence that gentrification increases residents' perception of neighborhood quality makes negative changes in epsilon unlikely (Ellen and O'Regan 2011b; Vigdor 2010).

We cannot observe ϵ and therefore cannot estimate $\frac{\partial}{\partial \Delta H_j}\Delta\epsilon_{ijj'}$. However, by assumption, gentrification in the origin neighborhood should be uncorrelated with the fixed, idiosyncratic characteristics ϵ_{ij} that make the origin neighborhood j preferable to the next best alternative, j'. We therefore assume that $\frac{\partial}{\partial \Delta H_j}\Delta\epsilon_{ijj'} = 0$.

Effect on Induced Movers

Finally, the third term of equation C.3 counts utility changes that accrue to individuals on the margin of moving. These individuals are induced into moving from their original neighborhood by gentrification. We carefully consider each parts of this margin.

To understand how gentrification affects the utility of induced movers, we first consider when individuals endogenously choose to move in general. Individuals move if the *incurred*, observed change in utility minus the *incurred*, unobserved costs of moving from the origin neighborhood (both loss of idiosyncratic preference and other fixed costs of moving) exceed the *avoided*, unobserved change in utility they would have experienced had they stayed:

$$\begin{aligned} Pr[move_{ij}] &= Pr[u_{ij'}^{2010} > u_{ij}^{2010}] \\ &= Pr[u_{ij'}^{2010} - u_{ij}^{2000} > u_{ij}^{2010} - u_{ij}^{2000}] \\ &= Pr[(x_{ij'}^{2010} - x_{ij}^{2000}) - (\epsilon_{ij}^{2000} - \epsilon_{ij'}^{2010}) > (x_{ij}^{2010} - x_{ij}^{2000}) - (\epsilon_{ij}^{2000} - \epsilon_{ij}^{2010})] \\ &= Pr[(x_{ij'}^{2010} - x_{ij}^{2000}) - (\epsilon_{ij}^{2000} - \epsilon_{ij'}^{2010}) > (x_{ij}^{2010} - x_{ij}^{2000})] \ . \end{aligned} \tag{C.6}$$

x is a vector of the observable components of utility, w, r, κ, and a. In the last line, we have used the fact that by assumptions about ϵ, $\epsilon_{ij}^{2000} - \epsilon_{ij}^{2010} = 0$.

It is worth emphasizing that while for movers we cannot observe the changes in utility they would have experienced had they stayed, $(x_{ij}^{2010} - x_{ij}^{2000})$, these changes are irrelevant for the purposes of estimating the effect of gentrification on their utility. These counterfactual changes simply affect the probability of moving, which in turn can affect overall utility changes through the second part of the induced movers term, described in detail below. But these counterfactual changes themselves are avoided and so do not affect utility directly.

While equation C.6 is helpful for understanding when individuals move in response to gentrification, we can simply estimate the effect of gentrification on the probability of moving, $\frac{\partial Pr[move_{ij}]}{\partial \Delta H_j}$, directly with our data.

The second part of the induced movers margin, $(\Delta u_{ijj'} - \Delta u_{ijj})$ says that the overall effect of gentrification on the utility of induced movers is increasing in the difference in the change in utility among movers minus the change in utility among stayers.

We can estimate the observed parts of $(\Delta u_{ijj'} - \Delta u_{ijj})$ (each of Δw, Δr, $\Delta\kappa$, and Δa) directly in our data.

The unobserved part of $(\Delta u_{ijj'} - \Delta u_{ijj})$ is:

$$
\begin{aligned}
\Delta\epsilon_{ijj'} - \Delta\epsilon_{ijj} &\equiv (\epsilon_{ij'}^{2010} - \epsilon_{ij}^{2000}) - (\epsilon_{ij}^{2010} - \epsilon_{ij}^{2000}) \\
&= \epsilon_{ij'}^{2010} - \epsilon_{ij}^{2010} \\
&= \epsilon_{ij'}^{2010} - \epsilon_{ij}^{2000}\ .
\end{aligned} \tag{C.7}
$$

We can write the last line because by assumption the fixed, idiosyncratic preferences for neighborhoods do not change over time.

Equation C.7 makes precise a key idea about moving. Moving affects residents' utility not only through observed changes in neighborhood characteristics, but also in proportion to the potential loss of unobservable fixed, idiosyncratic benefits of living in the origin neighborhood instead of the next best neighborhood. These might include the benefits of living near friends and family and other forms of neighborhood capital or community attachment.

Appendix D: Spatial First Differences Details

We implement the spatial first differences (SFD) in the following way, which closely follows the approach described by Druckenmiller and Hsiang (2018). To create channel (row i) and index (column j) values for each neighborhood, we begin with our full sample of 10,000 initially low-income, central city tracts. Within each central city, we identify the northernmost and southernmost points among these tracts. We then partition the central city into a number of horizontal channels of equal height in order to span this north-south distance. The height of each channel is some multiple of the diameter of the median tract in that city.[56] Then, starting with the northernmost channel, we identify all tracts that intersect with that channel. Moving from west to east, we assign each intersecting tract a sequential index value based on the x coordinate (degree longitude) of its centroid. We proceed to the next-northernmost channel, identify the intersecting tracts that do not yet have an index value, and assign them an index value in the same way. We continue through all channels until all tracts in that central city have a unique channel and index value. We complete this process simultaneously for all 100 central cities and ensure that all 10,000 tracts in our sample have a unique central city-channel-index ID.

To estimate equation 6, we start with one of our samples: adults, children, or tracts. For the first two, we collapse individuals to the tract level, the unit of spatial analysis. We create tract-level means of the individual-level outcome variables and individual- and household-level control variables.[57] We then merge the tract-level sample with the channel and index values created before and estimate equation 6 as described in the main text.

As suggested by Druckenmiller and Hsiang (2018), we show that our results are robust to the following different ways of constructing the channel and index values: using different channel heights, creating index values from east to west instead of from west to east, creating channels from south to north instead of from north to south, and including or not including control variables. The last two robustness checks are included in the draft, while the others are available upon request.

[56] Our reported results use 100% of the diameter, which is similar to the choice of Druckenmiller and Hsiang (2018) to use the average diameter of a county in the US in their maize yield example. Our results are robust to using other multiples such as 150% or 200%.

[57] We tested an alternative approach that allows us to estimate SFD models at the individual level when we have multiple observations per tract. Specifically, we create an additional index for individuals within each tract and then "stack" the standard cross-sections of neighborhoods by this index. It yields similar results, but we believe our favored approach of collapsing individual observations to the tract level is more transparent and more similar to our OLS and Oster settings.