# Gentrification and the Location and Well-Being of Original Neighborhood Residents*

Quentin Brummet† and Davin Reed‡

December 31, 2018

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**Abstract**

We use new longitudinal Census microdata to study how gentrification affects original neighborhood resident adults and children. Gentrification increases out-migration, though out-migrants do not experience worse changes in individual or neighborhood outcomes. At the same time, many residents remain and experience benefits in the form of declining exposure to neighborhood poverty and increasing house values. Rents increase for more-educated but not less-educated renters, and we find few effects on employment, income, or commute distance. Gentrification similarly increases children's exposure to proxies for neighborhood quality, but we find no effects on their educational or labor market outcomes.

*This research was conducted as a part of the Census Longitudinal Infrastructure Project (CLIP) while Brummet was an employee of the U.S. Census Bureau. Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau, the Federal Reserve Bank of Philadelphia, or the Federal Reserve System. All results have been reviewed to ensure that no confidential information is disclosed.

†Brummet: NORC at the University of Chicago. brummet-quentin@norc.org

‡Reed: Federal Reserve Bank of Philadelphia, Community Development and Regional Outreach Department. davin.reed@phil.frb.org. We particularly thank Sewin Chan, Ingrid Gould Ellen, and Katherine O'Regan for their feedback and support. Reed thanks the Horowitz Foundation for Social Policy and the Open Society Foundation for financial support while at New York University.

# 1 Introduction

Trends in preferences and labor demand have led college-educated and high-income individuals to increasingly locate in central urban neighborhoods (Baum-Snow and Hartley 2017; Couture and Handbury 2017; Edlund et al. 2016). How this gentrification process affects original neighborhood residents, and in particular less-educated and lower-income residents, has important distributional implications. Concern that gentrification might cause displacement or harm to original residents has led to policy proposals, such as restricting new housing supply or reviving rent control, that could have unintended adverse effects.[1] Gentrification might also bring neighborhood improvements, but it is unclear whether these would benefit any original residents. Understanding the different ways gentrification actually harms or benefits original residents is therefore of primary importance for housing policy.

In this paper we provide new evidence on how gentrification affects the location and well-being of original resident adults and children and how aggregate neighborhood change occurs. We do this by constructing a national, longitudinal Census microdata set that links individuals responding to both the Census 2000 and the American Community Survey 2010-2014.[2] For each individual, the data contain at both points in time their exact location of residence; exact location of work; detailed demographic and housing characteristics; and key outcomes such as employment, income, housing costs, commute distance, and neighborhood characteristics. We use these features to identify original residents, create measures of out-migration, and create changes in other individual outcomes.

We begin by presenting new descriptive facts about gentrification in our sample of original residents of initially low-income, central city neighborhoods of large metropolitan areas.[3] Two facts highlight the key dynamics at work. First, baseline migration rates are high: 75 percent of less-educated renters and 80 percent of more-educated renters move from their original neighborhood (census tract in 2000) to a different neighborhood over the course of a decade, regardless of gentrification. Second, most outcomes evolve very differently for those endogenously choosing to move than for those endogenously choosing to stay. A simple neighborhood choice model shows that the effect of gentrification on original resident well-being is captured by its effect on these two margins: the number of original residents choosing to move instead of stay (out-migration or displacement) and changes in the observable outcomes of both movers and stayers.

To estimate these effects, we use two approaches. We first estimate the relationship between changes in individual outcomes between 2000 and 2010-2014 and increases in neighborhood education levels (our definition of gentrification) over the same period. We control for a broad set of characteristics known to be correlated with gentrification or outcomes or both, including

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

[2] https://www.census.gov/programs-surveys/acs/

[3] This is the most common definition of gentrifiable neighborhoods used in previous gentrification research. Our results are similar for other definitions.

individual characteristics in 2000; neighborhood characteristics in 2000; changes in neighborhood characteristics (including gentrification) from 1990 to 2000; and metropolitan area fixed effects.

While these analyses include many controls, they assume that there are no remaining unobservable individual or neighborhood characteristics correlated with gentrification and our outcomes. We therefore use a recently developed estimator from Oster (2017), which builds on ideas from Altonji et al. (2005), to show that our results are robust to relaxing this unconfoundedness assumption. The method suggests that our key estimated effects are most likely lower bounds on the magnitudes of the true effects; that the estimated and true effects are likely quantitatively similar; and that the true effects are only zero under unlikely values for the sign and influence of remaining omitted variables. Taken together, our OLS results, which identify treatment effects assuming no omitted variables, and the "Oster estimates," which identify treatment effects using data-driven rule-of-thumb values for the influence of remaining omitted variables, provide plausible bounds for the causal effects of gentrification.[4]

Less-educated renters starting in neighborhoods that subsequently gentrify are 3 to 5 percentage points more likely to move to a different neighborhood by 2010-2014 compared to less-educated renters starting in non-gentrifying neighborhoods. These results are 50 to 100 percent larger among certain sub-samples of these residents, including those who in 2000 were also in poverty or in neighborhoods with very low initial education levels. Out-migration results are generally similar for more-educated renters and for more- and less-educated homeowners. Three additional findings qualify the out-migration results. First, those induced into out-migration by gentrification are not made observably worse off, though they may incur unobserved pecuniary or non-pecuniary moving costs. Second, most renters move even absent gentrification. Third, the out-migration effects only exist for gentrifiable neighborhoods in the top decile of gentrification.[5]

At the same time, many original residents (including less-educated renters and homeowners) remain even in gentrifying neighborhoods and experience observable benefits from gentrification through declines in exposure to neighborhood poverty and increases in house values for homeowners. As with out-migration, residents initially in poverty and those in neighborhoods with very low initial education levels experience larger effects. Movers are not made worse off in these dimensions.

Gentrification also increases rents for more-educated residents but not for less-educated residents, and the increases are driven by those endogenously choosing to stay.[6] This suggests that more-educated renters may be more willing to pay to stay and benefit from neighborhood changes associated with gentrification than less-educated renters, consistent with recent findings on differences in preferences for urban consumption amenities by skill (Couture and Handbury 2017; Su 2018; Diamond 2016).[7] We find few effects of gentrification on other observable out-

[4]Propensity score matching methods yield similar results.

[5]That is, only 10 percent of initially low-income, central city neighborhoods experience levels of gentrification that lead to increased out-migration.

[6]Both more- and less-educated renters who move from gentrifying neighborhoods experience no differential change in rents compared to movers from non-gentrifying neighborhoods.

[7]The muted effects for less-educated renter stayers could also be explained by sticky rents. We rule out the

comes for the average original resident, including employment, income, and commute distance.

Finally, to better understand how neighborhoods change, we estimate the effect of gentrification on *aggregate* neighborhood characteristics. We find that gentrification is associated with large increases in aggregate neighborhood rents, house values, incomes, and employment levels. Contrasted with our findings of moderate effects of gentrification on most of these outcomes and out-migration for original residents, this implies that aggregate neighborhood change is driven mostly by changes among in-migrants. Given that baseline migration rates are high in all neighborhoods, aggregate characteristics could change quickly over the course of a decade even with small changes to the number and composition of in-migrants.

Given the importance of neighborhood quality for children's long-run outcomes (Chetty et al. 2016; Chetty and Hendren 2016a,b), we also study how gentrification affects children aged 18 or younger and living in low-income, central city neighborhoods in 2000. As with adults, we find that on average gentrification decreases children's exposure to neighborhood poverty and increases exposure to other proxies of neighborhood quality such as aggregate education, employment, and income levels. However, we find no effects on their individual educational attainment, employment, or wages.[8] Gentrification increases out-migration among children and households with children, though as for all adults, moves are not to observably worse neighborhoods and most such households move even absent gentrification.

Overall, we find that gentrification does increase out-migration for some less-educated original residents but does not obviously harm them; benefits original residents who stay; and that aggregate neighborhood change is driven less by direct displacement than by changes among in-migrants.[9] The findings suggest that policies designed to accommodate gentrification rather than prevent it may be most beneficial for original and future residents. For example, constructing affordable or market rate housing in gentrifiable or already high-income central city neighborhoods could maximize the integrative benefits of gentrification while simultaneously dampening aggregate neighborhood price increases that could increase out-migration or limit future in-migration of the less advantaged.[10] Such accommodative policies also promote short- and long-run *regional* housing affordability.[11] Other policies like rent subsidies achieve some of these goals but not others.

role of subsidized housing by matching original residents to Department of Housing and Urban Development administrative data and estimating models excluding subsidized renters from the sample.

[8]These null effects could be explained by the fact that the declines in neighborhood quality experienced in our sample are much smaller than those in the Moving To Opportunity (MTO) experiment. They could also be explained by our inability to observe the actual dose of neighborhood quality received, which Chetty et al. (2016) show is important for estimating neighborhood effects.

[9]These conclusions are broadly similar when stratifying our sample of original residents by other proxies for sociodemographic disadvantage, such as income or minority status, instead of by education.

[10]Asquith, Mast, and Reed (2018) study how new housing construction affects nearby rents and migration patterns. Early results suggest that it decreases, relative to trend, nearby rents and in-migration from high-income neighborhoods. Mast (2018) shows that through filtering, even building market rate housing in high-income neighborhoods helps increase the supply of housing available in low-income neighborhoods.

[11]By contrast, preventative policies that aim to limit new construction or price increases in gentrifying neighborhoods may adversely affect future in-migration and regional affordability and may be difficult to effectively target.

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, not well-being, as the primary outcome of interest, adopt descriptive approaches, and find little evidence of higher mobility in gentrifying neighborhoods and some evidence of income gains (Freeman 2005; McKinnish et al. 2010; Ellen and O'Regan 2011b; Ding et al. 2016). By contrast, concurrent work by Aron-Dine and Bunten (2018) finds that gentrification does increase out-migration, particularly in the short term, similar to our findings of out-migration effects in the medium to long term.

Vigdor (2002) provides the earliest application of spatial concepts to understanding how gentrification might harm or benefit neighborhood residents. He explains why displacement may be a poor proxy for well-being and describes potential benefits in the form of labor market opportunities, neighborhood quality, and socioeconomic integration. In studies of gentrification during the 1980s and 1990s, he finds no evidence of large negative effects for low-income households and some evidence of neighborhood improvements that could increase welfare (Vigdor 2002, 2010). We build on this work through our emphasis on the distinction between out-migration and well-being and our findings that the neighborhood changes associated with gentrification carry benefits that accrue to original residents along with potential costs.

Concurrent papers by Couture et al. (2018) and Su (2018) use structural approaches to study how gentrification affects welfare inequality among all individuals, not only original residents. Both develop structural spatial equilibrium models of neighborhood choice, estimate them using cross-sections of individuals and neighborhoods over time, and find that gentrification has increased welfare inequality between high- and low-skill individuals beyond that implied by increases in the wage gap alone. Our reduced form findings that gentrification is associated with large changes in aggregate neighborhood demographic and housing characteristics is consistent with these results. Moreover, our findings for original residents suggest that the aggregate welfare inequality effects they estimate are likely driven less by disproportionate harm or out-migration among original residents and more by changes among in-migrants.

Our finding that gentrification increases original resident children's exposure to higher quality, higher opportunity neighborhoods is related to recent work on the effects of neighborhood quality on children's educational and labor market outcomes. These show that moving young children to lower-poverty neighborhoods increases college attendance and earnings and that the duration of exposure is important for these effects (Chetty et al. 2016; Chetty and Hendren 2016a,b). Baum-Snow et al. (2018) show that improving the neighborhood quality of children where they originally live (as opposed to moving them to better neighborhoods) similarly improves their test scores, labor market outcomes, and credit scores.[12]

The rest of this paper is organized as follows. Section 2 describes our data, and Section 3

[12] Their results are driven by suburban neighborhoods, not urban or low-income urban neighborhoods, which is consistent with our null findings in gentrifiable neighborhoods.

presents new empirical facts about original resident migration and changes in other outcomes over time. Section 4 describes a simple model of gentrification, location, and well-being. Section 5 describes our regression model and identification strategy. Section 6 presents estimates of the effect of gentrification on original resident adults, original resident children, and aggregate neighborhood characteristics. Section 7 concludes.

# 2 Data and Definitions

## 2.1 Longitudinal Census Microdata

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

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.[13] Approximately 10 percent of the Census 2000 long form sample matches, yielding around three million matched individuals. We observe in both years each individual's block of residence and block of work (if working); demographic characteristics; employment, wages, and income; homeownership status; and rent paid or house value. Key demographic variables 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 (CBSA).[14] Our measure of neighborhood quality is the neighborhood share of households in poverty, which we match to individuals by neighborhood of residence.[15]

Longitudinal individual data with geographic and demographic detail are central to our empirical and modeling approach. They allow 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 this by individual characteristics such as education level and housing tenure status. Being able to identify original residents and follow them over time is key because in the presence of individual selection, heterogeneous preferences, and moving costs, the effect of a spatial treatment on original residents does not necessarily equal the effect on contemporaneous residents that would

[13] 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. These dropped individuals are a small share of our total matched sample.

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

[15] In future drafts, we will add school quality, transit access, and retail options.

be estimated using cross sections of individuals or aggregate neighborhood characteristics (Bartik 2018). Answering the important policy question of whether gentrification actually harms *original* neighborhood residents therefore requires longitudinal individual data.

By studying the effects of gentrification from 2000 to 2010-2014, we are focused on medium- to long-term effects. Most previous research on gentrification has also focused on decadal changes, partly because these are important, partly by convention, and partly due to data limitations. The exceptions are Ding et al. (2016) and Aron-Dine and Bunten (2018). Both use the higher frequency migration data available in the Consumer Credit Panel (CCP) to follow gentrification and out-migration annually. While Ding et al. (2016) find no relationship between gentrification and mobility, Aron-Dine and Bunten (2018) develop an innovative approach and find that gentrification does increase out-migration, by around 4 percentage points in the short-term (on an annual migration rate of 14 percent). At longer time scales their migration rates in gentrifying and non-gentrifying neighborhoods look similar to ours, suggesting our medium-term effects may not be missing important short-term effects.

## 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 in the year 2000. 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. We focus on low-income, central city neighborhoods of major metropolitan areas 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.[16]

To focus on adults capable of making move decisions and for whom education level is mostly fixed, we first 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.[17] In our sample, education is relatively fixed: around 95 percent of individuals aged 25 or older in 2000 have no change in education level. We fix individuals' education at its 2000 level, but results are not sensitive to this choice.

We focus on education level as an essential element of heterogeneity and source of distributional effects. This is consistent with recent results from Baum-Snow and Hartley (2017) and Couture and Handbury (2017) showing that gentrification and urban revival are primarily characterized by the changing locations of college-educated and higher-income households. We define less-educated individuals as those completing a high school degree or less and more-educated individuals as those completing some college or more.[18] We hypothesize that education is an im-

[16]We exclude CBSAs in Hawaii and Alaska from the pool of all CBSAs from which we draw the 100 largest. Only the Honolulu CBSA is excluded as a result, and results do not differ when it is included.

[17]For our employment and earnings outcomes, we also run models with a sub-sample of individuals less than age 65 in the second period. Our results are qualitatively similar.

[18]When defining original residents, we combine those individuals completing a bachelor's degree or more with individuals completing an associates degree or some college in the more-educated group. This simplifies our results and yields better sample sizes in our sample of low-income, central city tracts. Results for both types are similar.

portant source of distributional effects because it is strongly correlated with income and earnings potential (and thus ability to afford housing); it allows residents to differently take advantage of labor market opportunities (as in a standard production function with two skill types); and because it may lead individuals to differently value neighborhood amenities. All could play important roles in determining the effects of gentrification on more- and less-educated original residents. Our results are qualitatively similar when stratifying the sample along socioeconomic dimensions other than education, such as initial income or race/ethnicity.

We include tenure status as a second essential element of heterogeneity. Increasing housing prices should have different implications for renters and homeowners, which in turn will have different implications for their decision to out-migrate. We therefore estimate all of our models fully stratifying our sample between less-educated renters, more-educated renters, less-educated homeowners, and more-educated homeowners. We stratify based on tenure status in 2000. Because tenure status is less fixed than education and more endogenous to gentrification, we include change in tenure status as an individual outcome.

### Individual Characteristics

An advantage of using Census data is the rich set of individual characteristics that it asks respondents about. We construct many of these characteristics for original residents based on their values in 2000. We include them as controls in our regression models and explore heterogeneity in gentrification's effects along these dimensions. We describe these in the following section.

### Out-Migration

Out-migration is central to gentrification debates and has been the focus of previous gentrification research. We measure out-migration in three complementary 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 they began in 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. We explore these outcomes in detail in the following section.

### Observable Well-Being Outcomes

Observable well-being outcomes capture the key observed components of a standard utility function. They include rents for renters, house values for homeowners, neighborhood poverty rate, employment and income, and commute distance. We measure changes in each of these for each individual original resident in our sample, regardless of whether they move or stay. To our knowledge, previous gentrification research has been unable to study the effects of gentrification on these longitudinal individual outcomes.

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 percent changes 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 proxy for neighborhood amenities 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.[19] Change in income is the percent change 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.[20] 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 age 60 in the second period we observe them.[21].

Change in commute distance is proxied by the percent change in the straight-line distance from tract of residence to tract of work. Because we use the midpoint method, individuals working in one period but not the other receive values of -2 or 2, and individuals working in neither period receive values of 0.[22]

We explore these observable well-being outcomes in detail in the following section.

### 2.3 Children Sample and Characteristics

We similarly construct a sample of original resident children aged 18 and younger to study how gentrification affects their well-being. Results are similar if we focus on samples of children 13 and younger or 5 and younger, so we present results for 18 and younger to maximize the sample size. Because the sample size is smaller than for adults, we only stratify the children sample by household tenure status in 2000 and not by individual or household characteristics.[23]

[19] We explore the effect on employment to determine if the potential labor market effects of gentrification work more through the intensive or extensive margin.

[20] We calculate percent changes using the midpoint method, so these would be included as changes of -2 and 2, respectively.

[21] Results are similar in our full sample of all individuals 25 or older, which is unsurprising given we find no effects in either sample

[22] Results are similar if we condition the sample only on those working in both periods, which is unsurprising given we find no effect on employment

[23] Cutting the sample in additional ways, such as by gender, minority status, or poverty status, yields noisy estimates and no obvious evidence of heterogeneity.

## 2.4 Aggregate Neighborhood Sample and Characteristics

In addition to looking at effects of gentrification on changes in original resident outcomes, we also look at effects on aggregate neighborhood characteristics. Comparing these two sets of results provides new insight into how aggregate neighborhood change actually occurs: through out-migration, effects on original residents, or through in-migration. McKinnish et al. (2010) previously explored how neighborhoods gentrified during the 1990s by constructing synthetic from non-longitudinal Census microdata.

To create aggregate neighborhood outcomes, we begin with the same microdata used to construct longitudinal outcomes for our original residents. Instead of first matching individuals across time (to form our adult and children samples), we simply collapse all individuals in the data. The results are similar to the tract-level aggregates publicly available from Census, but a key benefit is that we can create aggregates for more specific, custom definitions. For example, median rents for less-educated households.

## 2.5 Defining Gentrification

Following the most recent research on the causes of gentrification, we think of gentrification generally 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:

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

Neighborhoods experiencing large positive changes in $gent_{jc}$ are said to gentrify more than those experiencing smaller or negative changes. We hold population constant at its 2000 level to avoid attributing changes in the non-college-educated population mechanically to gentrification.[24] We are also able to exclude observations in our longitudinally matched (original resident) sample when constructing the gentrification measure, further reducing the potential for mechanical correlations between gentrification and changes in original resident outcomes.

Because gentrification may affect original residents in a non-linear way, we experimented with modeling gentrification flexibly using a set of indicator variables. These revealed that modeling gentrification using a binary indicator equal to one for neighborhoods in the top decile of the neighborhood distribution most parsiminously captured the non-linear effects in

[24]For example, if some neighborhoods lose less-educated population over the decade, this would contribute mechanically to them being measured as gentrifying by a definition based on the change in share college-educated. Avoiding these mechanical correlations is key given that most of the concern about gentrification is due to its potential displacement effects. In practice, our OLS models with full controls yield qualitatively similar results when using alternative definitions, such as the actual change in share college-educated or the percent change in the number of college-educated.

the data. We therefore show most of our results using a binary variable equal to one for these neighborhoods and zero otherwise.[25] Key results are qualitatively similar for the binary and continuous gentrification definitions.

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 by the 90th percentile cutoff. By contrast, calculating the distribution within each CBSA imposes that each CBSA have the same share of high gentrification neighborhoods. We include CBSA fixed effects in all regression models, so comparisons are always between neighborhoods within the same CBSA. Our results are similar when calculating gentrification percentiles within each CBSA.

# 3 New Facts about Gentrification and Neighborhood Change

Despite the amount of attention it receives, little empirical evidence exists about who lives in gentrifiable neighborhoods; how often individuals move across neighborhoods; or how many and what kinds of neighborhoods are affected by gentrification. Yet all have important implications for thinking about the distributional effects of gentrification and how policy might respond. We use the unique features of our data to provide new answers to these questions. They also motivate a simple neighborhood choice model of gentrification, which we use to help interpret our estimates of the effects of gentrification.

## 3.1 Who Lives in Gentrifiable Neighborhoods?

### Original Resident Characteristics

Table 1 describes the characteristics of original resident adults in 2000. Panel A describes the sample of all original residents, while Panels B and C stratify by move status. 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.[26]

Panel A shows that our sample of all 127,000 original residents (25 or older) of gentrifiable neighborhoods is reasonably evenly distributed across the four types of individuals. So gentrifiable neighborhoods, while skewing more towards the less-educated and renters than the population overall, is not overwhelmingly less-educated or renter. This matters for thinking about the different ways original residents might stand to gain or lose from gentrification.

Comparing Panels B and C provides insight into how original residents who subsequently moved differ from those who stayed over the decade. Far more renters move than stay. Within each type of individual (column), movers are on average younger and less likely to have children, consistent with general facts about migration.

[25] Results are robust to alternative non-linear categorizations and are available upon request.

[26] These are the same weights used in our regressions. Weighting does not substantially alter any of our findings.

"Lived here 5 years ago" shows that within columns, subsequent movers were more likely than stayers to have recently moved to the origin neighborhood consistent with them moving more often in general. Across columns, Panel A shows that before neighborhoods subsequently gentrified, 49 percent of less-educated renters and 34 percent of more-educated renters had lived in the origin for five years or more (since 1995). 67 percent of more-educated homeowners and 76 percent of homeowners had lived in the origin neighborhood for five years or more.

### Cross-Neighborhood Migration

Table 2 describes changes in original resident outcomes from 2000 to 2010-2014. Again, Panel A is all original residents, while Panels B and C stratify by move status.

Migration for renters is high: 70 to 80 percent of renters move to a different neighborhood over the course of a decade. This effectively places a limit on the potential for gentrification to cause out-migration, or displacement, for original resident renters over a decade. At the same time, high baseline migration makes it possible for neighborhoods to change quickly even with small changes to the composition of out-migrants and in-migrants. We explore this idea in detail below.

Consistent with more general migration facts, we find that migration rates are higher for renters than for homeowners and for more-educated individuals than for less-educated individuals. The actual differences in migration rates across our four types of individuals are quite high, suggesting the importance of being able to distinguish between them when estimating effects of gentrification. Patterns of cross-CBSA migration are similar across the four types of individuals, though the levels and differences are much smaller.

### Changes in Other Outcomes

The changes in other outcomes for the average original resident (both movers and stayers) in Panel A are helpful for interpreting our main regression results. The differences in how these outcomes evolve by move status (comparing Panels B and C) suggests an important, though non-causal, role of migration in original residents outcomes.

While original resident exposure to poverty was mostly unchanged for the average original resident (Panel A), movers substantially reduced their exposure to poverty while stayers, particularly owners, experienced increases in poverty exposure over the decade.

Rents and house values increased for all types of original residents of gentrifiable neighborhoods over the decade, though they increased more for movers than for stayers. Many movers may have moved to upgrade the quality (and cost) of their housing. Individuals may also be more inclined to stay if their rents do not increase.

Changes in employment and income were zero or declining across most types of original residents and similar by move status. The fact that these measures declined from 2000 to 2010-2014 may reflect the influences of the Great Recession and the aging of our sample over the study period. Commute distances decreased for most individuals, which makes sense given that we assign individuals not working a commute distance of zero.

## 3.2 What Neighborhoods Gentrify?

### Neighborhood Characteristics

Table 3 describes neighborhood characteristics in 2000 by gentrification status. It is worth emphasizing that gentrification significantly affects relatively few gentrifiable neighborhoods: only 10 percent based on our measure.[27] The 10 percent that did gentrify were generally quite different from those that did not, though not always in the way one might assume. Gentrifying neighborhoods started with higher education levels (20 percent college-educated vs. 13 percent), slightly lower minority shares (51 percent vs. 56 percent), and slightly higher rents and house values. Perhaps most importantly, they also had much lower initial populations (2,500 vs. 3,400) and slightly higher vacancy rates (10 percent vs. 8 percent). More vacant housing and vacant land could allow gentrifying neighborhoods to absorb new demand, helping explain our small estimated 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. Gentrifying neighborhoods also experienced much higher levels of gentrification over the previous decade: a 10 percentage point increase in share college-educated versus a 4 percentage point increase for neighborhoods that did not gentrify. This may help explain our small estimated out-migration effects and the fact that we find larger out-migration effects in neighborhoods with initially very low education levels: most individuals living in a neighborhood by 2000 had already selected into or out of that neighborhood based on neighborhood education level.

### Changes in Aggregate Neighborhood Characteristics

Finally, Table 4 describes average changes in aggregate neighborhood characteristics by gentrification status. We will test how gentrification affects these outcomes, and contrast them with the effect on original resident outcomes, in Section 6.

The table reveals many interesting trends. While poverty increased by around 6 percentage points in non-gentrifying neighborhoods, it declined by 3.5 percentage points in gentrifying neighborhoods. Employment and income also improved much more in gentrifying than in non-gentrifying neighborhoods.

Rent trends show striking differences by whether the neighborhood gentrified and also by type of renter. Among all households, median rents increased by 15 percent in non-gentrifying neighborhoods and by almost 35 percent in gentrifying neighborhoods. Median rents for *less-educated renters* increased by about the same amount in non-gentrifying neighborhoods (13

[27] We describe our gentrification measure in detail Section 2.5.

percent) but much less in gentrifying neighborhoods (only 16 percent). Instead, the large increase in median rents in gentrifying neighborhoods is paid by *more-educated* renters. The almost negligible increase in less-educated median rents in gentrifying neighborhoods could be explained by sticky rents or revealed preferences. Either way, this breakdown shows that there is important heterogeneity that is missed by simple overall median rent measures. We will return to this fact when interpreting our gentrification effects for original resident renters.

Median house value trends also show that gentrifying neighborhoods experience much larger increases in house values than non-gentrifying neighborhoods, and this difference is almost as stark for less-educated homeowners as for more-educated homeowners.

# 4 Model of Gentrification, Location, and Well-Being

We develop a simple neighborhood choice model to highlight how gentrification affects original resident well-being through the various outcomes explored above. Intuitively, it captures the idea that in any given neighborhood, over 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 the change in observable outcomes (that together approximate observable individual utility) 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 non-pecuniary 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 choose a neighborhood to live in order to maximize utility as a function of wages, rents, commuting costs, and neighborhood amenities:

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

Gentrification, as the in-migration of more-educated (high-skill) individuals, can affect original resident utility because, based on existing results in the literature, components of utility are a function of the number of high-skill individuals in the neighborhood. Rents (or house values) are a function of the number of high-skilled individuals because housing supply is upward sloping. Wages are a function of the number 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 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). Epsilon is the fixed, idiosyncratic utility individuals derive from their origin neighborhood and has a shape that governs how responsive individual migration will be to changes in the neighborhood (Moretti 2011; Kline and Moretti 2014).

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 $j'$:

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

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

### 4.1 Effect of Gentrification

Differentiating Equation 3 with respect to gentrification ($\Delta H_j$) and rearranging reveals that the effect of gentrification on changes in original resident utility depends on three terms:[28]

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

Appendix A describes these effects in additional detail. Intuitively, the idea is that gentrification affects the well-being of original residents through two margins: its effects on the outcomes of those whose decision to move or stay is affected by gentrification ("induced movers") and its effects on the outcomes of those who always move or always stay regardless of whether their neighborhood gentrifies.

Equation 4 makes clear why out-migration itself is not evidence of harm, either for those who out-migrate (because their observable outcomes may be unchanged and unobserved costs may be small) or for the average original resident (if some out-migrants are in fact made worse off but more stayers are made better off). Determining whether gentrification actually harms or benefits original residents thus 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, counts utility changes that accrue to individuals on the margin of moving.[29] These individuals are induced into moving from their original neighborhood by gentrification. We carefully consider each part of this margin.

[28] We take derivatives using the product rule because all parts of Equation 3 are implicit functions of $\Delta H_j$, as described in Equation 2.

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

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:

$$\begin{aligned} Pr[move_{ij}] &= Pr[u_{ij'}^{2010} > u_{ij}^{2010}] \\ &= Pr[(x_{ij'}^{2010} - x_{ij}^{2000}) - (\epsilon_{ij}^{2000} - \epsilon_{ij'}^{2010}) > (x_{ij}^{2010} - x_{ij}^{2000})] \end{aligned} \tag{5}$$

$x$ is a vector of the observable components of utility, $w$, $r$, $\kappa$, and $a$.[30]

It is worth emphasizing that while for movers we cannot observe the changes in utility they would have experienced had they stayed in neighborhood $j$, $(x_{ij}^{2010} - x_{ij}^{2000})$, 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 5 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. It includes an observed part ($\Delta w$, $\Delta r$, $\Delta \kappa$, and $\Delta a$) that we can estimate directly in our data and an unobserved part $(\epsilon_{ij'}^{2010} - \epsilon_{ij}^{2000})$ that we can't.

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 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. Given the importance of displacement in gentrification debates, we do not make assumptions about the strength of these costs. More work is needed to better understand the pecuniary and non-pecuniary costs of moving across neighborhoods.

# 5 Empirical Approach

In this section we describe our regression model, the key challenges to identifying causal effects of gentrification, and how we address those challenges.

[30] We have also used the fact that by assumptions about $\epsilon$, $\epsilon_{ij}^{2010} = \epsilon_{ij}^{2000}$. Additional details are in Appendix A.

### 5.1 Regression Model

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

$$\Delta Y_{ijc} = \beta_0 + \beta_1 gent_{jc} + \beta_2 X_{ijc} + \beta_3 W_{jc} + \beta_4 \Delta W_{jc,1990s} + \beta_5 gent_{jc,1990s} + \mu_c + \epsilon_{ijc} \tag{6}$$

We restrict our sample to individuals living in initially low-income, central city neighborhoods of the 100 largest CBSAs in 2000. The dependent variable $\Delta Y_{ijc}$ 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 separately defining gentrification using a continuous measure and using a binary measure, as described in Section 2.5.

Selection on observables, and robustness to selection, is key to our attempt to establish causality. We therefore leverage the large number of individual demographic and housing characteristics available in the Census, along with evidence from previous research on the causes of gentrification, to assemble a large number of controls that are thought to be correlated with gentrification or migration or both.

$X_{ijc}$ is a vector of individual characteristics in 2000 and includes whether the individual is the head of household; whether the individual is married; whether there are children in the household; age and age squared; gender; race/ethnicity; a binary variable indicating whether English is the primary language spoken at home; and a binary variable indicating whether the individual moved into the neighborhood within the past five years.[31] Table 1 describes these characteristics.[32]

$W_{jc}$ 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).[33] Table 3 provides the complete list of these along with means by neighborhood gentrification status.[34] $\mu_c$ is a vector of CBSA fixed effects. We cluster at the

[31] Initial individual employment, income, or other labor market outcomes are never included as controls. We are most interested in changes in these as outcomes. In a change model, one does not want to regress a change in a variable on an initial level of that variable. Therefore, to maintain consistency in our model specifications across all of our outcomes, we omit initial individual labor market characteristics as controls. Conditional on our individual characteristics, including or not including these does not change our results.

[32] In the actual regressions, we also include age squared and break out the minority indicator variable into a set of more detailed indicators.

[33] Results are similar when using other proxies for natural amenities created by Lee and Lin (2018), including their hedonic natural amenity index.

[34] In the actual regressions, we first take the inverse hypoerbolic sine of median rents, house values, and incomes; distances; age of housing stock; and population and population density.

tract level to allow the error terms of individuals in the same tract to be correlated with each other, which is required for correct inference given that our gentrification measures are at the tract level.

$\Delta W_{jc,1990s}$ is a vector of changes in the same neighborhood characteristics from 1990 to 2000, and $gent_{jc,1990s}$ is gentrification in the neighborhood from 1990 to 2000 (the lag of our variable of interest). These help control for neighborhood pre-trends that could be correlated with gentrification. Encouragingly, all of our main results are robust to including or excluding these pre-trends. A table of changes in neighborhood characteristics from 1990 to 2000 was not disclosed but is available upon request.

Equation 6 is a version of a change model. It uses cross-sectional variation in neighborhood gentrification during the 2000s and cross-sectional variation in changes in individual outcomes between 2000 and 2010-2014. We estimate the effect of gentrification by comparing how outcomes change differently for individuals in gentrifying and non-gentrifying neighborhoods when those individuals were observably similar in 2000; lived in neighborhoods that were observably similar in 2000 and on observably similar trajectories from 1990 to 2000; and lived in the same metropolitan area.

Importantly, this model does not answer the question of whether gentrification at the city level might similarly affect original residents of both gentrifying and non-gentrifying neighborhoods. For example, city-level increases in the number of college-educated individuals could lead to overall improvements in amenities or increases in out-migration among all original residents. By only comparing gentrifying and non-gentrifying neighborhoods within the same city, we miss these general effects. However, we believe that the benefits from comparing neighborhoods within the same city outweigh the costs of missing these general effects. When we do estimate regression models where we replace the metropolitan area fixed effects with metropolitan area-level controls (similar to our tract controls) and a metropolitan area-level measure of gentrification, we obtain insignificant estimates for the effect of metropolitan area-level gentrification and estimates of the effect of tract-level gentrification that are similar to the estimates from Equation 6.

The main threats to identification in Equation 6 are likely reverse causality and time-varying omitted variables that are correlated with both gentrification and outcomes, even conditional on our full set of controls. 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.[35] This would bias OLS toward finding more positive effects of gentrification on original resident out-migration. Omitted variable bias could arise from individual selection into neighborhoods based on unobservables correlated with gentrification and from unobserved neighborhood improvements that are correlated with gentrification. These could bias OLS toward finding smaller effects of gentrification on original resident out-migration.

[35] We limit the influence of mechanical reverse causality through the construction of our gentrification measures, as described before.

As described in the next section, we find empirical evidence that on net, the direction of OLS bias for most outcomes is towards zero. This could explain in part why most previous gentrification research has found no effect on mobility.

### 5.2 Oster Robustness

To assess the robustness of our results to selection and omitted variables, we use an estimator recently developed by Oster (2017) that builds on ideas from Altonji et al. (2005) that are loosely 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 6 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 6 to obtain a gentrification coefficient and model R-squared with full controls. The Oster estimator uses as inputs the change in gentrification coefficient; 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 ($\delta$). 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 $\delta = 1$.[36] [37]

In results not yet disclosed, we use these methods to show in detail that our key results are only truly zero when using implausible values for the sign and influence of remaining omitted variables. For example, the out-migration results could only be overturned if there are remaining omitted variables that 1) have the opposite correlation with gentrification and out-migration as the full set of controls (so that they move the estimate back toward zero), 2) are many times more influential than all of the controls included in our models combined. Given that the large set of controls we include are precisely those that previous research suggests are most correlated with gentrification, we believe the second point is particularly unlikely to hold.

## 6 Effects of Gentrification

Table 5 shows OLS and Oster estimates of the effect gentrification in our full sample of all original residents adults. OLS estimates are based on Equation 6 using our binary measure of gentrification and always include full controls and CBSA fixed effects. Standard errors are clustered at the tract level because we can have multiple observations within a tract, our level of treatment. Oster estimates are treatment effect estimates that adjust for the potential bias

[36] 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.

[37] We use the Stata implementation available as psacalc from the Boston College Statistical Software Components (SSC) archive.

from remaining omitted variables not included in our full control OLS models, as described in the previous section.

The effects on the full sample are most important for understanding the overall effect of gentrification on original resident well-being. However, we discuss these results alongside those in Table 6, which first stratifies the full sample by the endogenous choice to move, to better understand what may be driving the overall effects.

## 6.1 Out-Migration

We first explore out-migration, as it is the most controversial aspect of gentrification and, according to our model, 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 2.5 percentage points. Perhaps because of spatial correlation in gentrification or measurement error in moving, the effect on moves to a neighborhood at least one mile away is higher, around 4 percentage points. The Oster estimates are about one percentage point higher than the OLS estimates.[38] This suggests that if anything, omitted variables may be biasing our OLS estimates downward, toward finding no effect. Thus, the OLS estimates may 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 tract 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.

Our interpretation of these results is that gentrification increases less-educated moves to other neighborhoods by 3 to 5 percentage points. Recall from Table 2 that across all gentrifiable neighborhoods, 68 percent of less-educated renters moved to any other neighborhood and 60 percent moved to a neighborhood at least one mile away. At most, then, gentrification increases less-educated renter moves to other neighborhoods for the average original resident by around 8 percent (5 / 60).

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 non-gentrifying 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 non-gentrifying neighborhoods. This suggests that on average and over the course of a decade, gentrification does not result in particularly constrained or otherwise sub-optimal relocations.

Gentrification also increases less-educated renter moves to other CBSAs and non-metropolitan areas. The effect is similar in magnitude, around 4 percentage points, but on a much lower base-

[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. They are available upon request.

line move rate of 15 percent.

Move effects for more-educated renters are generally similar, though in both percentage point and percent terms they are smaller, as might be expected. An important caveat is that here the Oster estimates are closer to zero than the OLS estimates, suggesting an upward bias from omitted variables. While the Oster estimates are still positive and very similar to the OLS estimates for moves to other neighborhoods, giving us confidence there is some small move effect, they suggest that there is likely no effect on more-educated renter moves from the CBSA.

There is less expectation of how gentrification should affect moves by homeowners. It might increase out-migration if owners are unable to keep up with property tax payments on rising house values or if owners sell to realize a capital gain. It might also decrease out-migration 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. We find that gentrification in fact increases out-migration by both less- and more-educated homeowners by 3 to 4 percentage points. As for renters, results are more Oster-robust for less-educated than for more-educated homeowners. We find no effect on the probability that homeowners leave the CBSA.

## 6.2 Observable Well-Being

### Neighborhood Poverty

Neighborhood poverty is a proxy for many aspects of neighborhood quality, and research has shown that the poverty rate of one's neighborhood has particularly important effects on children's long-run employment and earnings. 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. 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.

Table 5 shows that gentrification does decrease original residents' exposure to neighborhood poverty, by around 3 percentage points. The result is similar across all types of original residents, though as before the results for less-educated individuals are more Oster-robust than for more-educated individuals. The average change for all less-educated renters over the decade was 0 (Table 2), so that gentrification may 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 percent larger than similar less-educated renters staying in non-gentrifying neighborhoods. Magnitudes are again similar across all types of individuals and very Oster-robust.

The average poverty rate in all gentrifiable neighborhoods in 2000 was 24 percent (Table 3), and the effect of gentrification on poverty exposure for less-educated renter stayers is around negative 7 percentage points. 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 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.

**Rent**

Table 5 shows that somewhat surprisingly, gentrification has no effect on reported rents paid by less-educated renter original residents. Rents increased for all less-educated renters by 12 percent (Table 2), so gentrification simply did not increase rents paid by these individuals even further. Table 6 shows that gentrification increased rents for less-educated renter stayers by 1 to 2 percentage points (though the estimates are imprecise) on a baseline increase for stayers of 10 percent (Table 2).

The small effects for less-educated renters are interesting. One explanation is that many less-educated renters are rent subsidized, but we rule this out by linking individuals 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. The small effects may be better explained by sticky rents or by out-migration and revealed preferences: any individuals experiencing rent increases could simply move to another, non-gentrifying neighborhood.

By contrast, gentrification increases rents paid by more-educated renters by around 3 percentage points, on a baseline increase of 13 percent. The effect for more-educated renter stayers is much higher, 7 percentage points, on a baseline of 10 percent. 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. 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).

**House Value**

Table 5 also shows that gentrification increases original resident house values and that these are driven by increases for stayers. However, the results are much less Oster-robust than other results. Recall that intuitively, the Oster method uses changes in the gentrification coefficient and R-squared when going from a model with no controls to a model with full controls to extrapolate out to the Oster estimate. Though not shown in detail here, the large change in the Oster estimates for house values is due to a small change in coefficient when going from no controls to full controls and a very small change in R-squared. That is, the lack of Oster-robustness is driven not by large changes in the gentrification coefficient when controls are added, but by very small increases in R-squared when controls are added.

The 6 percentage point increase in house value due to gentrification is on baseline increases across all gentrifiable neighborhoods of 7 percent for less-educated owners and 15 percent for more-educated owners. While it is true that rising house values may increase property tax payments that are difficult to afford, we believe it is more likely to be a benefit given the importance of the housing asset in household wealth.

Recall that the rent and house value effects are conditional on individuals having the same tenure status in both 2000 and 2010-2014. While not shown here, we find no effect of gentrification on the probability that more- or less-educated renters become homeowners or vice versa.

#### Employment and Income

Gentrification may lead to beneficial employment and income effects if it creates new, nearby work opportunities, or it may cause harm if it leads to moves that are disruptive or located far from employment opportunities. Table 5 suggests that gentrification is neither beneficial nor harmful for less-educated renters or homeowners.

However, gentrification does increase employment and income among more-educated owners, and as expected, the effects are driven by stayers. More-educated owners staying in gentrifying neighborhoods experience 3 percentage point increases in employment and 12 percentage point increases in income relative to those staying in non-gentrifying neighborhoods. Point estimates are similar, though noisier, for less-educated owners. Recall that employment and income are declining for all original residents in our sample, so these outcomes are simply declining less. Nevertheless, these results suggest that more-educated owners, and to a lesser extent less-educated owners, may benefit from an influx of more-educated individuals, perhaps through new local job opportunities or networks.

#### Commute Distance

Finally, we find little evidence that gentrification affects the commute distance of original residents. It does increase commute distance by around 10 percentage points for less-educated owners (on a baseline of -0.03 percent), but the fact that it is driven by stayers suggests it is explained more by the employment effects we find (individuals previously with no commute now have a positive commute distance) than by any migration effects that hold work location constant.

### 6.3 Heterogeneity

We test for heterogeneity along a number of individual, tract, and CBSA dimensions and do not find many substantial differences. Our sample of 10,000 gentrifiable neighborhoods is small enough that when we start adding many interactions or stratifications, the estimates get noisy and are indistinguishable from the main estimates we report.

However, we do find substantial heterogeneity along two key dimensions: individual poverty status and whether a neighborhood has a very low initial share college-educated. Table 7 shows effects of gentrification for less-educated renters. The first four columns stratify by whether these individuals are also in poverty, which is roughly equivalent in our sample to having a household income below 15,000 dollars. Gentrification increases moves for less-educated renters in poverty by 5 to 10 percentage points, while it only increases moves for those not in poverty by 2 to 3 percentage points. This is consistent with individuals with very low incomes being less able to afford any rent increases and being more likely to move instead.

The last four columns show that gentrification also has larger effects among less-educated renters who started in neighborhoods with very low education levels (college share $< 0.05$, which is about the bottom quartile of the tract distribution). In these neighborhoods, gentrification increases moves among less-educated renters by 6 to 11 percentage points versus 2 to 5 percentage points for those in more-educated neighborhoods. This suggests that by 2000, less-educated renters may have already selected into or out of their neighborhood based on their preference for education level. Individuals living in more-educated neighborhoods may have chosen them despite or because of their higher education levels (and perhaps anticipated increases), so subsequent gentrification would be less likely to induce them to leave. By contrast, those choosing less-educated neighborhoods may be less willing to stay in neighborhoods that subsequently gentrify.

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 certain sub-types of disadvantaged individuals. Of course, this also means that there are fewer individuals in these sub-types who are affected and that the effects are weaker for other sub-types of residents. The existence of heterogeneity implies that any prevention policies would need to be carefully directed even among less-educated renters in gentrifying neighborhoods and would be most effective when gentrification is detected early.

### 6.4 Gentrification and Aggregate Neighborhood Change

Until now we have focused on the effect of gentrification on original resident outcomes. In Table 8 we present estimates of the effect of gentrification on changes in aggregate neighborhood characteristics.[39] These capture effects not only on original residents, but also changes due to the way gentrification changes the quantities and characteristics of in-migrants. The estimates are less interpretable as causal, and as expected they are also less Oster-robust. Nevertheless, combined with our estimates for original residents they provide important new insight into how neighborhood change actually occurs.

Unsurprisingly, gentrification is associated with large decreases in aggregate neighborhood

[39]Regression models are identical to those in Equation 6 except we exclude individual controls and no longer need to cluster at the tract level.

poverty rates and large increases in employment, income, rents, and house values. Table 4 shows baseline changes in these characteristics across all gentrifiable neighborhoods to aid interpretation.

We further break out changes in aggregate (median) rents and house values by whether these are paid by more- or less-educated renters. Similar to the patterns for original residents, we find much smaller increases in median rents paid by less-educated renters than in median rents paid by more-educated renters. This again suggests that rents may be sticker for less-educated renters than for more-educated renters; that less-educated renter in-migrants are less willing to pay higher rents in gentrifying neighborhoods; or simply that there are fewer less-educated renter in-migrants to gentrifying neighborhoods. It may also suggest that the rental market is somewhat segmented, with rents for the stock used by less-educated renters increasing more slowly than the stock used by more-educated renters. The median house value estimates are again not Oster-robust due to the small improvements in R-squared we get from our control variables for these outcomes.

Contrasting these large associations between gentrification and aggregate neighborhood characteristics with the small effects we estimate for original residents, we infer indirectly that aggregate neighborhood change is driven less by the direct displacement of or effects on original residents and more by changes to the number and characteristics of in-migrants. This matters for thinking about the distributional effects of gentrification. It also cautions against using changes in aggregate neighborhood characteristics to infer anything about effects on original residents. It suggests that gentrification-related policy should consider not only protecting individuals originally there, but also maintaining affordability and accessibility for potential in-migrants.

### 6.5 Results for Children

Given the large effects on neighborhood quality we find for adults and the importance of neighborhood quality for children's outcomes, we also estimate the effects of gentrification on children's exposure to neighborhood quality and their educational and labor market outcomes. Table 9 shows these results for children who in 2000 lived in gentrifiable neighborhoods and were age 18 or younger. We stratify the sample only by tenure status because further stratifying by income, race/ethnicity, or gender does not yield meaningfully different results and yields noisier estimates.

As for adults, gentrification increases the probability that children move to a different neighborhood by 3 to 5 percentage points. However, it also decreases their exposure to neighborhood poverty, by 3 to 4 percentage point for renters and 5 to 6 percentage points for owners.[40] Though not shown here, these effects are driven by larger effects for stayers and no effects for movers. Gentrification also increases exposure to other proxies for neighborhood quality that could be particularly important for children, such as education levels, employment levels, and income lev-

[40]We find similar results for original resident adults who report the presence of children 18 or younger in the household.

els. Many of these will increase in *aggregate* mechanically with gentrification, but we emphasize that enough original resident children remain even in gentrifying neighborhoods to experience these changes.

However, we find no effect of gentrification on children's college attendance, employment, or income in 2010-2014. While the estimates for renters are suggestive of positive employment and income effects, they are very noisy and we avoid reading much into them. Stratifying by endogenous move choice yields even noisier estimates that we do not include here.

The fact that gentrification decreases children's exposure to neighborhood poverty, particularly for stayers, but yields no effect on their employment or income, appears at odds with recent findings that improving neighborhood quality improves children's labor market outcomes (Chetty et al. 2016; Baum-Snow et al. 2018). A few points could explain the difference. First, children in the MTO experiment began in much higher poverty neighborhoods (41 percent compared to our 24 percent) and experienced much larger reductions in poverty exposure (12 to 22 percentage points compared to our 3 to 7 percentage points (for stayers)). Second, Chetty et al. (2016) show that the dosage, or time spent in the lower poverty neighborhood, is crucial for their results. Because we only observe children at two points in time, we are unable to say precisely when the poverty reductions occurred or how long original residents were exposed to them. Finally, the findings in Baum-Snow et al. (2018) that local demand shocks benefit children is driven by suburban neighborhoods, which we exclude from our sample. Our null findings for initially low-income, central urban neighborhoods are in fact consistent with their null findings for central urban neighborhoods.

# 7 Conclusion

Gentrification has increased substantially over the past two decades, drawing much attention in the research community and contributing to policy proposals, such as rent control, that could have quantitatively large absolute and distributional effects. Understanding whether and how gentrification might actually harm or benefit original residents is therefore an important unresolved question for housing policy. To fill this gap, we construct novel longitudinal microdata that allow us to estimate the effect of gentrification on the overall well-being of original neighborhood resident adults and children.

Our results suggest that while gentrification does increase out-migration among original residents, most residents move even absent gentrification and out-migrants are not made observably worse off in terms of neighborhood quality, labor market outcomes, or commuting. This suggests that gentrification-related relocations may not be particularly disruptive, constrained, or otherwise sub-optimal in the medium- to long-term. However, out-migrants 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 costs suggest a total fixed moving cost of around \$42,000, which increases by a somewhat low 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 (and more specifically from an idiosyncratically preferred neighborhood), building on the large existing literature estimating cross-state and cross-labor market moving costs, is an important area for future research.

At the same time, many original residents, even less-educated renters, remain in gentrifying neighborhoods and experience large declines in exposure to neighborhood poverty. More- and less-educated homeowners also experience large increases in house values, an important part of household wealth. More-educated renters do end up paying more in rent, but less-educated renters do not, suggesting differences in willingness or ability to pay for neighborhood changes associated with gentrification.

Gentrification is associated with large increases in *aggregate* neighborhood characteristics. Contrasted with the small effects we find for original residents, we infer that gentrification is driven less by direct displacement or changes for original residents and more by changes in the number and types of in-migrants. We are exploring this in detail in future work.

Overall, our findings suggest that neighborhoods are dynamic and residents are highly mobile across neighborhoods. Thus, attempts to prevent gentrification, such as through limits on new construction, may not yield many benefits for original residents and, in time, would likely make neighborhood and regional affordability worse. Instead, policies designed to accommodate gentrification may be more beneficial. For example, increasing the supply of housing in gentrifiable and already gentrified central urban neighborhoods could maximize the integrative benefits we find, minimize the out-migration effects we find, and minimize aggregate rent increases that could dampen future in-migration. Concurrent work by Asquith, Mast, and Reed (2018) is directly testing the effect of new construction on nearby rents and migration patterns to understand, in part, the extent to which new supply may slow or accelerate gentrification (concern about induced demand is often used to delay or prevent new construction).

Alternative policies, such as providing rent subsidies to disadvantaged households, could also protect original residents from out-migration pressures. Our heterogeneity results suggest that to be most effective, these should be carefully targeted to the individuals (very low income) and neighborhoods (very low initial education levels) where out-migration effects are of greatest concern.

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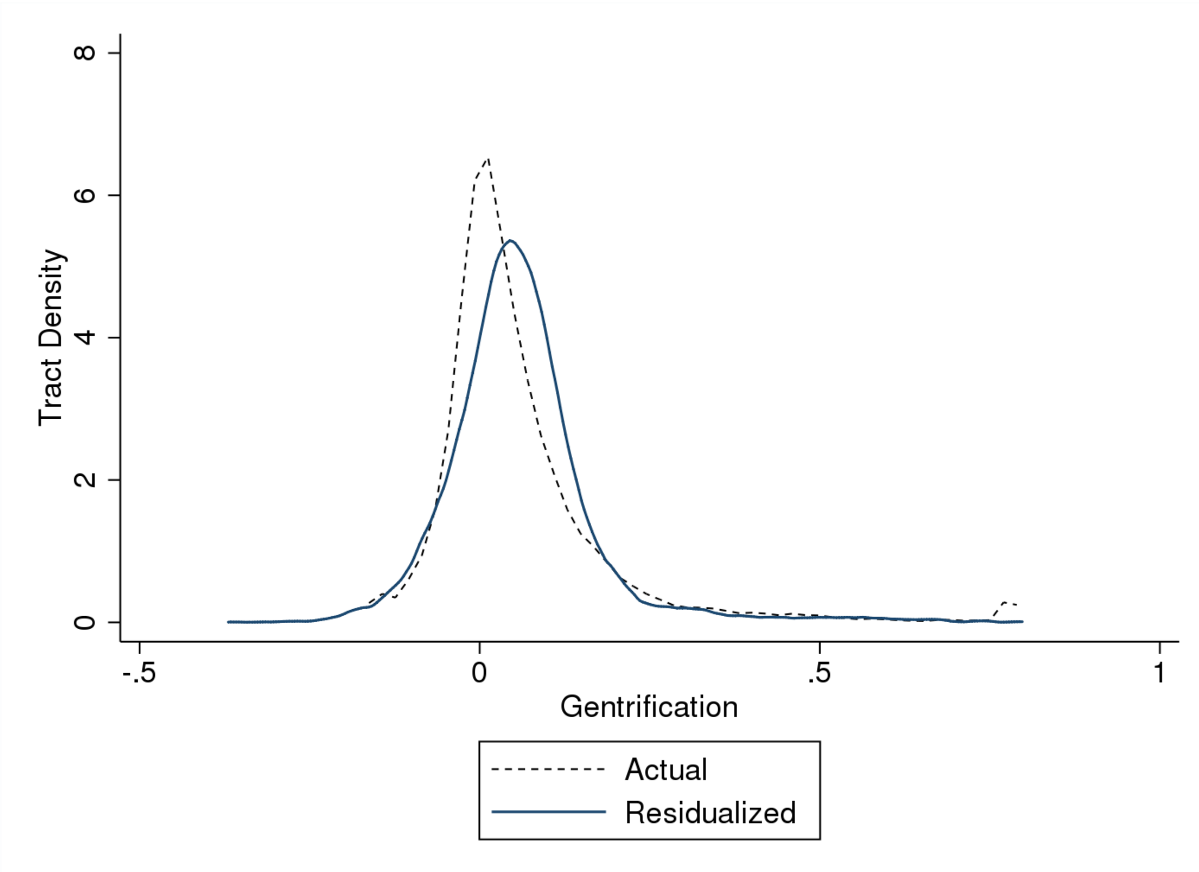
Figure 1: Gentrification Variation, 2000 to 2010-2014




Note: 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.

Table 1: Original Resident Adult Characteristics, 2000

Overall and By Move Status

**Panel A: All**

| | Less-Educated Renters | More-Educated Renters | Less-Educated Owners | More-Educated Owners |
|---|---|---|---|---|
| Householder | 0.639 | 0.713 | 0.507 | 0.583 |
| Age | 44.2 | 39.6 | 50.8 | 46.7 |
| Female | 0.590 | 0.546 | 0.561 | 0.543 |
| Minority | 0.729 | 0.467 | 0.568 | 0.396 |
| Not English language | 0.399 | 0.255 | 0.297 | 0.175 |
| Married family | 0.415 | 0.335 | 0.657 | 0.613 |
| Children present | 0.509 | 0.319 | 0.421 | 0.373 |
| Lived here 5 years ago | 0.486 | 0.337 | 0.761 | 0.674 |
| N | 28,000 | 24,000 | 37,000 | 38,000 |

**Panel B: Movers**

| | Less-Educated Renters | More-Educated Renters | Less-Educated Owners | More-Educated Owners |
|---|---|---|---|---|
| Householder | 0.622 | 0.706 | 0.469 | 0.561 |
| Age | 41.7 | 37.7 | 46.5 | 42.9 |
| Female | 0.578 | 0.538 | 0.539 | 0.528 |
| Minority | 0.721 | 0.447 | 0.517 | 0.340 |
| Not English language | 0.381 | 0.242 | 0.272 | 0.165 |
| Married family | 0.413 | 0.331 | 0.629 | 0.593 |
| Children present | 0.547 | 0.319 | 0.479 | 0.385 |
| Lived here 5 years ago | 0.402 | 0.262 | 0.662 | 0.563 |
| N | 19,000 | 19,000 | 12,000 | 16,000 |

**Panel C: Stayers**

| | Less-Educated Renters | More-Educated Renters | Less-Educated Owners | More-Educated Owners |
|---|---|---|---|---|
| Householder | 0.675 | 0.738 | 0.526 | 0.600 |
| Age | 49.7 | 46.6 | 53.0 | 49.4 |
| Female | 0.615 | 0.576 | 0.572 | 0.554 |
| Minority | 0.746 | 0.541 | 0.594 | 0.437 |
| Not English language | 0.438 | 0.303 | 0.310 | 0.183 |
| Married family | 0.419 | 0.351 | 0.672 | 0.628 |
| Children present | 0.425 | 0.320 | 0.390 | 0.364 |
| Lived here 5 years ago | 0.666 | 0.625 | 0.812 | 0.755 |
| N | 9,000 | 5,000 | 25,000 | 23,000 |

Note: These are the year 2000 individual 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.

Table 2: Changes in Original Resident Adult Outcomes, 2000 to 2010-2014

Overall and By Move Status

**Panel A: All**

| | Less-Educated Renters | More-Educated Renters | Less-Educated Owners | More-Educated Owners |
|---|---|---|---|---|
| Move | 0.683 | 0.792 | 0.338 | 0.420 |
| Move 1 mile | 0.604 | 0.736 | 0.317 | 0.398 |
| Exit CBSA | 0.151 | 0.247 | 0.088 | 0.128 |
| Tract poverty (pp) | -0.004 | -0.018 | 0.034 | 0.017 |
| Rent or house value (pct) | 0.117 | 0.134 | 0.069 | 0.148 |
| Employment (pp) | 0.013 | -0.054 | -0.009 | -0.072 |
| Income (pct) | -0.142 | -0.099 | -0.206 | -0.152 |
| Commute distance (pct) | -0.008 | -0.064 | -0.026 | -0.146 |
| N | 28,000 | 24,000 | 37,000 | 38,000 |

**Panel B: Movers**

| | Less-Educated Renters | More-Educated Renters | Less-Educated Owners | More-Educated Owners |
|---|---|---|---|---|
| Tract poverty (pp) | -0.019 | -0.030 | -0.017 | -0.032 |
| Rent or house value (pct) | 0.130 | 0.154 | 0.183 | 0.301 |
| Employment (pp) | 0.009 | -0.056 | -0.009 | -0.068 |
| Income (pct) | -0.152 | -0.088 | -0.199 | -0.116 |
| Commute distance (pct) | -0.001 | -0.056 | 0.017 | -0.091 |
| N | 19,000 | 19,000 | 12,000 | 16,000 |

**Panel C: Stayers**

| | Less-Educated Renters | More-Educated Renters | Less-Educated Owners | More-Educated Owners |
|---|---|---|---|---|
| Tract poverty (pp) | 0.027 | 0.029 | 0.060 | 0.053 |
| Rent or house value (pct) | 0.097 | 0.095 | 0.029 | 0.057 |
| Employment (pp) | 0.026 | -0.042 | -0.008 | -0.077 |
| Income (pct) | -0.113 | -0.159 | -0.212 | -0.188 |
| Commute distance (pct) | -0.029 | -0.110 | -0.058 | -0.203 |
| N | 9,000 | 5,000 | 25,000 | 23,000 |

Note: 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) and percent (pct). Numbers of individuals rounded to the nearest 1,000. Source: Census 2000 Long Form and 2010-2014 5-Year ACS Estimates.

Table 3: Neighborhood Characteristics, 2000

By Binary Gentrification Status (Top Decile of Continuous Measure)

| | Not Gentrifying | Gentrifying |
|---|---|---|
| Share college | 0.129 | 0.210 |
| Share employed | 0.900 | 0.915 |
| Share in poverty | 0.238 | 0.236 |
| Share minority | 0.560 | 0.509 |
| Share renters | 0.582 | 0.687 |
| Median rent | 740 | 767 |
| Median house value | 118,184 | 161,135 |
| Median household income | 38,177 | 38,177 |
| Miles to high-income tract | 2.2 | 1.7 |
| Miles to CBD | 4.3 | 2.6 |
| Average age of housing | 40.4 | 40.2 |
| Share housing before 1940 | 0.264 | 0.369 |
| Population | 3,405 | 2,530 |
| Population density | 9,433 | 9,127 |
| Within 500 meters of coast | 0.056 | 0.130 |
| Vacancy | 0.082 | 0.102 |
| Share lived here 5 years ago | 0.475 | 0.437 |
| Gentrification, 1990 to 2000 | 0.038 | 0.098 |
| N | 9,000 | 1,000 |

Note: These are the year 2000 neighborhood characteristics 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).

Table 4: Changes in Aggregate Neighborhood Outcomes, 2000 to 2010-2014

By Binary Gentrification Status (Top Decile of Continuous Measure)

| | Not Gentrifying | Gentrifying |
|---|---|---|
| Tract poverty (pp) | 0.057 | -0.035 |
| Share employed (pp) | 0.011 | 0.098 |
| Median household income (pct) | -0.545 | 0.480 |
| Median rent (pct) | | |
| All | 0.154 | 0.347 |
| Less-educated | 0.133 | 0.156 |
| More-educated | 0.146 | 0.343 |
| Median house value (pct) | | |
| All | 0.122 | 0.432 |
| Less-educated | 0.089 | 0.314 |
| More-educated | 0.110 | 0.405 |
| N | 9,000 | 1,000 |

Note: Means of changes in aggregate neighborhood outcomes, 2000 to 2010-2014. Variables are measured as changes with units in parentheses: percentage point (pp) and percent (pct). Numbers of neighborhoods rounded to the nearest 500. Source: Census 2000 Long Form and 2010-2014 5-Year ACS Estimates.

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 | 0.0263** | 0.0355 | 0.0283*** | 0.0253 | 0.0320** | 0.0372 | 0.0408*** | 0.0286 |
| | (0.0121) | | (0.0094) | | (0.0154) | | (0.0122) | |
| Move 1 mile | 0.0413*** | 0.0555 | 0.0323*** | 0.0301 | 0.0351** | 0.0394 | 0.0432*** | 0.0338 |
| | (0.0127) | | (0.0101) | | (0.0150) | | (0.0122) | |
| Exit CBSA | 0.0366*** | 0.0404 | 0.0247** | 0.00573 | 0.00555 | 0.00476 | 0.0142 | -0.00135 |
| | (0.0102) | | (0.0119) | | (0.0093) | | (0.0097) | |
| Tract poverty | -0.0320*** | -0.036 | -0.0190*** | -0.0144 | -0.0338*** | -0.0266 | -0.0293*** | -0.0187 |
| | (0.0037) | | (0.0027) | | (0.0038) | | (0.0031) | |
| Rent or house value | -0.00737 | -0.015 | 0.0314* | 0.0277 | 0.0664*** | -0.00543 | 0.0569*** | 0.0338 |
| | (0.0200) | | (0.0189) | | (0.0248) | | (0.0172) | |
| Employment | 0.00465 | 0.0144 | -0.0101 | 0.00307 | 0.0161 | 0.018 | 0.0133 | 0.0196 |
| | (0.0205) | | (0.0130) | | (0.0255) | | (0.0130) | |
| Income | -0.0253 | -0.0178 | -0.00673 | -0.00156 | 0.0248 | 0.00603 | 0.0620* | 0.0705 |
| | (0.0420) | | (0.0300) | | (0.0507) | | (0.0327) | |
| Commute distance | 0.0464 | 0.0593 | 0.0256 | 0.0333 | 0.113* | 0.109 | 0.0175 | 0.0187 |
| | (0.0442) | | (0.0391) | | (0.0590) | | (0.0402) | |
| N | 28,000 | | 24,000 | | 37,000 | | 38,000 | |

Note: Binary gentrification measure. All models include CBSA fixed effects and full controls: individual 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 5.2. 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.

Table 6: Effect of Gentrification 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 | -0.00874* | -0.0124 | -0.00722** | -0.00209 | 0.0125** | 0.0161 | -0.000144 | 0.00836 |
| | (0.00451) | | (0.00298) | | (0.00582) | | (0.00394) | |
| Rent or house value | -0.0362 | -0.0485 | 0.0118 | 0.00882 | -0.0279 | 0.00626 | -0.00680 | 0.0121 |
| | (0.0276) | | (0.0255) | | (0.0477) | | (0.0278) | |
| Employment | -0.000823 | 0.00826 | -0.0127 | -0.00120 | -0.0337 | -0.0204 | 0.00314 | 0.0135 |
| | (0.0236) | | (0.0137) | | (0.0391) | | (0.0168) | |
| Income | -0.0420 | -0.0379 | -0.00938 | -0.00545 | -0.0859 | -0.100 | 0.0260 | 0.0407 |
| | (0.0490) | | (0.0329) | | (0.0842) | | (0.0444) | |
| Commute distance | 0.0369 | 0.0441 | 0.0209 | 0.0233 | 0.0665 | 0.0574 | 0.0172 | 0.00646 |
| | (0.0511) | | (0.0432) | | (0.0903) | | (0.0558) | |
| 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 | -0.0678*** | -0.0680 | -0.0564*** | -0.0517 | -0.0554*** | -0.0404 | -0.0486*** | -0.0307 |
| | (0.00534) | | (0.00556) | | (0.00466) | | (0.00431) | |
| Rent or house value | 0.0223 | 0.0122 | 0.0688*** | 0.0650 | 0.0890*** | -0.254 | 0.0969*** | -0.132 |
| | (0.0263) | | (0.0242) | | (0.0293) | | (0.0212) | |
| Employment | 0.0202 | 0.0375 | -0.0143 | 0.00830 | 0.0497 | 0.0449 | 0.0342* | 0.0397 |
| | (0.0408) | | (0.0353) | | (0.0341) | | (0.0194) | |
| Income | 0.0159 | 0.0417 | -0.0184 | 0.00152 | 0.0922 | 0.0659 | 0.120** | 0.131 |
| | (0.0827) | | (0.0775) | | (0.0662) | | (0.0481) | |
| Commute distance | 0.0599 | 0.105 | 0.0246 | 0.0635 | 0.112 | 0.109 | 0.0127 | 0.0330 |
| | (0.0873) | | (0.0920) | | (0.0779) | | (0.0581) | |
| N | 9,000 | | 5,000 | | 25,000 | | 23,000 | |

Note: Binary gentrification measure. We stratify the sample from Table 5 by endogenous move status and estimate the main regression models. Stayer results are relative to those staying in non-gentrifying neighborhoods. Mover results are relative to those moving from non-gentrifying neighborhoods. All models include CBSA fixed effects and full controls: individual 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 5.2. 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.

Table 7: Heterogeneity of Gentrification Effects

Among Less-Educated Renters Only

| | Individual in Poverty | | Not in Poverty | | Origin Low Education | | Not Low Education | |
|---|---|---|---|---|---|---|---|---|
| | OLS | Oster | OLS | Oster | OLS | Oster | OLS | Oster |
| Move | 0.0466** | 0.0575 | 0.0172 | 0.0257 | 0.0804*** | 0.116 | 0.0169 | 0.0252 |
| | (0.0215) | | (0.0147) | | (0.0300) | | (0.0132) | |
| Move 1 mile | 0.0776*** | 0.104 | 0.0262* | 0.0359 | 0.0574* | 0.098 | 0.0366*** | 0.053 |
| | (0.0221) | | (0.0155) | | (0.0293) | | (0.0140) | |
| Exit CBSA | 0.0380** | 0.041 | 0.0356*** | 0.0394 | -0.000764 | 0.0161 | 0.0445*** | 0.0492 |
| | (0.0172) | | (0.0125) | | (0.0184) | | (0.0117) | |
| Tract poverty | -0.0393*** | -0.0453 | -0.0282*** | -0.0313 | -0.0536*** | -0.053 | -0.0287*** | -0.0269 |
| | (0.0067) | | (0.0040) | | (0.0097) | | (0.0038) | |
| Rent or house value | 0.0159 | 0.0146 | -0.0157 | -0.0267 | 0.0747 | 0.0637 | -0.0245 | -0.0411 |
| | (0.0371) | | (0.0215) | | (0.0515) | | (0.0210) | |
| Employment | 0.0826** | 0.106 | -0.0258 | -0.0173 | 0.0522 | 0.0626 | -0.00903 | 0.0000812 |
| | (0.0360) | | (0.0233) | | (0.0492) | | (0.0230) | |
| Income | 0.054 | 0.0704 | -0.0562 | -0.052 | 0.0967 | 0.111 | -0.0523 | -0.0421 |
| | (0.0786) | | (0.0457) | | (0.1070) | | (0.0463) | |
| Commute distance | 0.104 | 0.167 | 0.0274 | 0.0307 | 0.0405 | 0.0524 | 0.0302 | 0.0345 |
| | (0.0696) | | (0.0554) | | (0.0913) | | (0.0503) | |

Note: 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 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 5.2. Numbers of less-educated renters by sub-type not disclosed. Source: Census 1990 Long Form, Census 2000 Long Form, and 2010-2014 5-Year ACS Estimates.

Table 8: Effect of Gentrification on Aggregate Neighborhood Characteristics

| | OLS | Oster |
|---|---|---|
| Tract poverty | -0.0651*** | -0.0475 |
| | (0.00321) | |
| Share employed | 0.0720*** | 0.0588 |
| | (0.00290) | |
| Median household income | 0.660*** | 0.294 |
| | (0.0319) | |
| Median rent | | |
| All | 0.149*** | 0.122 |
| | (0.00664) | |
| Less-educated | 0.0250** | 0.0383 |
| | (0.0102) | |
| More-educated | 0.152*** | 0.114 |
| | (0.00792) | |
| Median house value | | |
| All | 0.173*** | 0.0661 |
| | (0.0120) | |
| Less-educated | 0.102*** | -0.0233 |
| | (0.0199) | |
| More-educated | 0.151*** | -0.0654 |
| | (0.0137) | |
| N | 10,000 | |

Note: Tract-level. Binary gentrification measure. 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 5.2. Numbers of neighborhoods rounded to the nearest 500. Source: Census 1990 Long Form, Census 2000 Long Form, and 2010-2014 5-Year ACS Estimates.

Table 9: Effect of Gentrification on Original Resident Children

Among All Original Residents (Stayers and Movers)

| | Renters | | Owners | |
|---|---|---|---|---|
| | OLS | Oster | OLS | Oster |
| Move | -0.00546 | 0.00351 | 0.0461** | 0.0582 |
| | (0.0146) | | (0.0205) | |
| Move 1 mile | 0.0277* | 0.0464 | 0.0496** | 0.0656 |
| | (0.0155) | | (0.0201) | |
| Exit CBSA | 0.00788 | 0.00814 | 0.0153 | 0.0155 |
| | (0.0119) | | (0.0131) | |
| Tract poverty | -0.0140*** | -0.0172 | -0.0285*** | -0.0236 |
| | (0.0047) | | (0.0047) | |
| Some college or more | 0.0122 | 0.00632 | 0.0247 | 0.0179 |
| | (0.0145) | | (0.0171) | |
| College or more | -0.0108 | -0.0172 | -0.0144 | -0.027 |
| | (0.0096) | | (0.0143) | |
| Employed in 2010 | 0.0189 | 0.0191 | 0.00719 | 0.00123 |
| | (0.0150) | | (0.0179) | |
| Income in 2010 (asinh) | 0.125 | 0.128 | -0.00355 | -0.081 |
| | (0.1520) | | (0.1680) | |
| N | 29,000 | | 25,000 | |

Note: Binary gentrification measure. All models include CBSA fixed effects and full controls: individual 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 5.2. 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.

# Appendix A

## A.1 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:

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

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 this parameter. This parameter can also include fixed costs of moving that are constant across all neighborhoods, for example the cost of hiring movers or searching for a new residence.

## A.2 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 $j'$:

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

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

## A.3 Effect of Gentrification

Differentiating Equation A.2 with respect to gentrification ($\Delta H_j$) and rearranging reveals that the effect of gentrification on changes in original resident utility depends on three margins:[41]

[41] We take derivatives using the product rule because all parts of Equation A.2 are implicit functions of $\Delta H_j$.

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

#### A.3.1 Effect on Always Stayers

The first term of Equation A.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 A.1, we can write the second part (still suppressing all terms' dependence on $\Delta H_j$), as:

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

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 0 on average, and therefore $\frac{\partial}{\partial \Delta H_j}\Delta \epsilon_{ijj}$ also equals 0.[42]

#### A.3.2 Effect on Always Movers

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

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

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.

We cannot observe $\epsilon$ 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 $\epsilon_{ij}$ that make the origin neighborhood $j$ preferable to the next best alternative, $j'$. We therefore assume that $\frac{\partial}{\partial \Delta H_j}\Delta \epsilon_{ijj'} = 0$.

#### A.3.3 Effect on Induced Movers

Finally, the third term of Equation A.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.

[42] By assumption that epsilon 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 2011, Vigdor 2010).

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:

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

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

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 A.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 $\Delta w$, $\Delta r$, $\Delta \kappa$, and $\Delta a$) directly in our data.

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

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

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

Equation A.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.

The magnitude of $\epsilon_{ij'}^{2010} - \epsilon_{ij}^{2000}$ is therefore important for understanding how gentrification affects original resident utility. If on average $\epsilon_{ij'}^{2010} - \epsilon_{ij}^{2000}$ is small or 0, then our well-being calculations using only observable changes in utility will be close to correct.

If instead $\epsilon_{ij'}^{2010} - \epsilon_{ij}^{2000}$ is big on average, then we would be missing an important way in which gentrification affects original resident well-being.

Moretti and Kline and Moretti assume that $\epsilon$ is distributed X, with some shape parameter Z.

The shape parameter determines whether epsilon has a high variation or a small variation. This in turn governs the migration response to a change in the origin, such as gentrification. It would also govern the extent to which $\epsilon_{ij'}^{2010} - \epsilon_{ij}^{2000}$ is large or small: wider variation in $\epsilon$ would yield larger losses of idiosyncratic uitlity among marginal movers. However, it is worth noting that in their setup, Kline and Moretti (2014) assume for simplicity that regardless of the distribution of $\epsilon$, the marginal movers are indifferent between locations so that we can completely ignore $\epsilon_{ij'}^{2010} - \epsilon_{ij}^{2000}$. Given the importance of displacement in gentrification debates, we do not make this assumption and instead consider the well-being effects under different assumptions about $\epsilon_{ij'}^{2010} - \epsilon_{ij}^{2000}$.

Table A.1: 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 | OLS | OLS | OLS |
| Move | 0.0994*** | 0.0773*** | 0.141*** | 0.158*** |
| | (0.0312) | (0.0235) | (0.0412) | (0.0317) |
| Move 1 mile | 0.114*** | 0.0694*** | 0.146*** | 0.156*** |
| | (0.0329) | (0.0265) | (0.0398) | (0.0319) |
| Exit CBSA | 0.0861*** | 0.0819*** | 0.0750*** | 0.0865*** |
| | (0.0259) | (0.0301) | (0.0244) | (0.0244) |
| Tract poverty | -0.110*** | -0.0519*** | -0.159*** | -0.116*** |
| | (0.0100) | (0.0074) | (0.0116) | (0.0085) |
| Rent or house value | 0.0339 | 0.000168 | 0.293*** | 0.157*** |
| | (0.0498) | (0.0475) | (0.0667) | (0.0459) |
| Switch tenure | 0.00122 | -0.0303 | 0.0650** | 0.0418* |
| | (0.0337) | (0.0319) | (0.0326) | (0.0218) |
| Employment | 0.0121 | -0.00291 | -0.0433 | 0.0108 |
| | (0.0546) | (0.0337) | (0.0677) | (0.0351) |
| Income | -0.0013 | -0.0612 | -0.032 | 0.00701 |
| | (0.1060) | (0.0795) | (0.1350) | (0.0845) |
| Commute distance | 0.0494 | 0.0963 | 0.0953 | 0.0842 |
| | (0.1170) | (0.1000) | (0.1510) | (0.1050) |
| N | 28,000 | 24,000 | 37,000 | 38,000 |

Note: Continuous gentrification measure. All models include CBSA fixed effects and full controls: individual characteristics in 2000, tract characteristics in 2000, changes in tract characteristics from 1990 to 2000, and gentrification from 1990 to 2000. 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.

Table A.2: Effect of Continuous Gentrification Measure on Original Resident Children

Among All Original Residents (Stayers and Out-Migrants)

| | Renters | Owners |
|---|---|---|
| | OLS | OLS |
| Move | 0.00546 | 0.141*** |
| | (0.0337) | (0.0504) |
| Move 1 mile | 0.0902** | 0.162*** |
| | (0.0373) | (0.0500) |
| Exit CBSA | 0.0465 | 0.0962*** |
| | (0.0327) | (0.0335) |
| Tract poverty | -0.0685*** | -0.102*** |
| | (0.0123) | (0.0130) |
| Some college or more | 0.0286 | 0.0153 |
| | (0.0385) | (0.0443) |
| College or more | -0.019 | -0.0584* |
| | (0.0250) | (0.0342) |
| Employed in 2010 | -0.0327 | 0.0582 |
| | (0.0384) | (0.0458) |
| Income in 2010 (asinh) | -0.309 | 0.761* |
| | (0.3930) | (0.4290) |
| N | 29,000 | 25,000 |

Note: Continuous gentrification measure. All models include CBSA fixed effects and full controls: individual characteristics in 2000, tract characteristics in 2000, changes in tract characteristics from 1990 to 2000, and gentrification from 1990 to 2000. 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.