Dynamic Individuals, Static Neighborhoods: Migration and Earnings Changes in Poor Neighborhoods*

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Abstract

This paper studies migration and earnings dynamics in poor neighborhoods using new administrative data linking residential location and earnings. Out-migration rates are higher in poor neighborhoods than elsewhere, and the majority of people who leave a poor neighborhood move to a richer one. Residents of poor neighborhoods also see significant earnings mobility, with average growth rates similar to richer areas. Estimates based on idiosyncratic, firm-specific pay changes show that increases in earnings are linked to migration to better neighborhoods. This results in the earnings of the cohort who lived in a poor neighborhood at baseline growing more than twice as fast as the earnings of these neighborhoods' contemporaneous residents. Overall, our results highlight an underappreciated reason why poor neighborhoods stay poor: initial residents whose earnings grow tend to move away.

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1 Introduction

Concentrated neighborhood poverty is a prevalent and enduring feature of the United States. Policymakers and researchers have focused on concentrated poverty for over a century, driven by concerns about public health, quality of life, and educational and employment outcomes (Riis, 1890; President's Committee on Urban Problems, 1968; Jargowsky, 1997). Poor neighborhoods are also central to research on neighborhood effects (Kling et al., 2007), local economic opportunity (Chetty et al., 2026), and place-based policy (Busso et al., 2013).

Existing research points to markedly different explanations for why people live in poor neighborhoods. Work that focuses directly on concentrated poverty has emphasized the overlapping barriers that prevent residents from exiting these neighborhoods (Wilson, 1987; Sharkey, 2013).[1] Researchers have specifically illustrated that unstable income and poor credit, discrimination, and limited information about other neighborhoods all play important roles (e.g., Desmond, 2016; Christensen and Timmins, 2022; Bergman et al., 2024). On the other hand, common models of neighborhood choice imply that individuals may also choose to live in poor areas because they are more affordable (e.g., Epple and Sieg, 1999; Bayer et al., 2007; Bilal and Rossi-Hansberg, 2021). Each explanation surely applies in some cases, but their relative importance—which has significant implications for these neighborhoods and related policy—is not well understood.

In this paper, we provide a comprehensive analysis of migration and earnings changes in poor neighborhoods. We find that many residents have persistently low earnings and remain in concentrated poverty for years, consistent with work emphasizing immobility and structural barriers. However, there is also a large population who sees sizable earnings growth and moves to richer areas at a high rate, consistent with work emphasizing affordability as a key factor. The out-migration of this upwardly mobile group substantially depresses neighborhood earnings growth: if poor neighborhoods maintained their initial set of residents, their average earnings would grow

[1] In addition to describing barriers faced by the "truly disadvantaged" in poor, majority-Black, neighborhoods, Wilson (1987) discusses the effects of the out-migration of the Black middle class from these areas in the aftermath of the Civil Rights Movement. Sharkey (2013) documents the persistence of residence in poor neighborhoods across generations, particularly for Black families, and discusses potential explanations.

more than twice as rapidly. We conclude by illustrating how these individual dynamics make concentrated poverty persistent over time and discussing implications for policy.

The starting point for our study is a large panel that pairs detailed individual demographic characteristics with longitudinal information on earnings and employment. We link adult respondents to the American Community Survey (ACS) between 2005 and 2013 to newly available address history data from the Census Bureau and the Longitudinal Employer-Household Dynamics (LEHD) data to construct a panel of neighborhood locations and earnings. We define high-poverty neighborhoods as census tracts with a poverty rate over 30 percent, roughly the poorest decile of tracts.

Our first set of results are migration rates across different types of neighborhoods. Individual mobility is generally high, with 43 percent of all adults exiting their baseline census tract over the following ten years. People in high-poverty neighborhoods exit at a higher rate of 51 percent. These exits are not just to similarly poor areas: 78 percent relocate to tracts below our high-poverty threshold, including many tracts with much lower poverty rates.

In addition to simple transition rates, we examine longitudinal exposure to concentrated poverty. For each person living in a high-poverty tract in the baseline year, we calculate how many of the subsequent ten years they spend in a high-poverty tract. There is a bimodal distribution. About 54 percent live in concentrated poverty for all of the next ten years, while 31 percent spend five or fewer years in poor neighborhoods. We find stark heterogeneity in mobility out of high-poverty neighborhoods across some demographic subgroups. About 31 percent of individuals under age 35 spend all of the following ten years in similarly-poor neighborhoods, versus 71 percent of individuals over age 65. Renters also have markedly lower exposure than homeowners. By contrast, exposure differences across races are relatively small.

We next turn to economic mobility, beginning with new estimates on earnings changes among individuals residing in different types of neighborhoods. While prior literature has measured earnings dynamics among poor people, there is little evidence on whether earnings growth differs for those residing in poor *neighborhoods*. People with low earnings at a point in time generally have high earnings volatility and rapid subsequent growth (Bane and Ellwood, 1986; Abowd et al., 2018; Guvenen et al., 2021). However, this may not be the case in very poor neighborhoods if the neighborhood environment depresses earnings growth, perhaps through poor commuting access or job referral networks (e.g., Bayer et al., 2008). We find that people living in high-poverty neighborhoods at baseline have very similar future earnings growth rates to people in less poor areas, including a long right tail of large increases. Echoing the bimodal nature of our migration results, high-poverty neighborhoods feature both considerable upward earnings mobility among working residents and a relatively large share of people with consistently low attachment to the labor force.

The link between these earnings dynamics and migration is central to understanding both individuals' choices to live in poor neighborhoods and how individual dynamics pass through to neighborhood-level outcomes. We first show that there is a strong correlation between these variables. Among individuals beginning in a high-poverty tract, those whose real annual earnings increase by $20,000 over an eight-year period are 11 percentage points more likely to have exited concentrated poverty by the end of this period than those who experience no earnings growth, and they end up in tracts with $4,700 higher median income. Similarly, entry to high-poverty tracts is negatively correlated with earnings increases.

We then proceed to estimate the effect of persistent shocks to earnings on neighborhood choice. If affordability is an important constraint, the causal effect of a long-lasting earnings change should be large, while this may not be the case if other barriers are more important. We build on the design in Rose and Shem-Tov (2023) to examine the effects of idiosyncratic, firm-level pay shocks, which we infer using changes in coworker earnings following Koustas (2018) and Ganong et al. (2020). We find that a shock that increases annual household earnings four to eight years later by 1.27 log points on average leads to an increase in tract median income of 0.18 log points, implying an elasticity of 0.14. Individuals have a clear tendency to move towards higher-income neighborhoods after positive earnings shocks, though an elasticity below 1 implies that they do not perfectly sort to neighborhoods with residents whose earnings match their own. The implied elasticity is highest for renters, those with children, and those initially in poor tracts, suggesting that these groups are most likely to move in response to earnings increases. However, a back-of-the-envelope calculation suggests that, despite these large elasticities, most movement out of high-poverty neighborhoods is driven by factors other than earnings growth.

To conclude, we consider what these individual-level results tell us about poor neighborhoods. First, our estimates suggest that these neighborhoods stay poor not only because some persistently poor residents stay put, but also because those who do prosper tend to leave. We quantify this mechanism by comparing the average earnings growth of the cohort of people who *start* in a poor neighborhood (regardless of where they live in future years) to the set of people who *live* in poor neighborhoods in each year. The earnings of the baseline cohort increases by 25 percent over the subsequent ten years, similar to cohorts beginning in less poor neighborhoods. However, growth in the earnings of contemporaneous residents is only about 10 percent. This implies that selective migration meaningfully depresses average income growth in poor neighborhoods, illustrating the importance of the upwardly mobile group we identify. Second, we consider implications for neighborhood revitalization policy. High baseline migration rates blunt the neighborhood-level payoff of many interventions, a problem that may worsen if a program raises some residents' incomes. On the other hand, high earnings growth and selective out-migration may also represent levers for policy, as suggested by Jacobs (1961), who proposed "unslumming the slums, by creating conditions aimed at persuading residents to stay by choice over time."

Our primary contribution is to the literature on concentrated neighborhood poverty (Jargowsky, 1997).[2] Most directly related are a small set of papers studying migration in poor neighborhoods, primarily using the Panel Study of Income Dynamics (Gramlich et al., 1992; South and Crowder, 1997; Quillian, 2003).[3] We build on this work first by using large-scale data that allows for much more precise measurement of neighborhood transitions and how they vary across subgroups. In addition, we are the first to measure earnings growth in poor U.S. neighborhoods at scale and establish its connection with upwards migration. Last, we bring these advances together to show

[2]A long tradition of qualitative research has examined disadvantaged neighborhoods or "slums" (Whyte, 1943; Clark, 1965; Liebow, 1967; Suttles, 1968). Researchers have specifically studied kinship networks, neighborhood norms, and the public housing environment (Rainwater, 1970; Anderson, 1990; Venkatesh, 2000).

[3]Sampson and Sharkey (2008) and Coulton et al. (2012) produce similar estimates using a multi-wave surveys in particular cities. Other work has considered determinants of migration decisions in poor neighborhoods, particularly eviction and housing instability (Sharkey, 2012; Skobba and Goetz, 2013; Desmond, 2016; DeLuca et al., 2019).

that persistent concentrated poverty is driven not only by the migration barriers emphasized in prior work (Wilson, 1987; Sharkey, 2013) but also by the selective out-migration of upwardly mobile residents, a mechanism that points to a different set of policy levers.

Our paper also contributes to the broader literature that studies how households make location choices, often using structural models of migration in which individuals trade off different neighborhood characteristics (e.g., Bayer et al., 2007; Galiani et al., 2015; Bayer et al., 2016; Davis et al., 2021; Ferreira and Wong, 2023) or housing policy experiments that provide subsidies or other support (e.g., Kling et al., 2007; Chetty et al., 2016; Bergman et al., 2024). This literature generally does not focus on the relationship between migration and earnings changes, often because of a lack of data. We provide evidence on the importance of this relationship, along with empirical moments that can help discipline quantitative models that include earnings changes.

Finally, our analysis connecting individual-level dynamics to neighborhood-level outcomes adds to research on how neighborhood economic and demographic composition evolves over time (e.g., Card et al., 2008; Lee and Lin, 2018; Malone and Redfearn, 2018; Couture and Handbury, 2020). These papers generally study neighborhood-level outcomes and lack individual panel data. We show that, in the case of concentrated poverty, large earnings dynamics and frequent migration are essential components of the process that produces persistence in neighborhood poverty.

2 Conceptual Framework

We present a simple model that describes how residential mobility depends on changes in earnings and other factors and, in turn, helps to motivate and structure the empirical analysis that follows. The framework features barriers to mobility emphasized in prior work on poor neighborhoods along with heterogeneous earnings and affordability motivations.

Household i in demographic group g chooses a neighborhood j with time-invariant quality $q_j \in \mathbb{R}$ and rent r_j, where rent is increasing in quality. The household has exogenous earnings y_i that exceeds rent in all neighborhoods, and spends $c_{ij} = y_i - r_j$ on numeraire consumption.

Indirect utility from living in neighborhood j is

$$V_{ij} = \ln(y_i - r_j) + \big(\beta_g + \delta_g \ln y_i\big)\, q_j + a_{ij} + \varepsilon_{ij}. \tag{1}$$

This specification of preferences allows for both a baseline taste for neighborhood quality $\beta_g > 0$ as well as a term $\delta_g > 0$ that captures the complementarity between earnings and neighborhood quality, both of which may differ across groups (e.g., households with and without children). The term a_{ij} describes a non-earnings match component, which can reflect factors like family and social ties, discrimination, and moving costs. We refer to $a_i = (a_{i1}, \ldots, a_{iJ})$ as a household's match preferences, though it can also reflect constraints. The shock ε_{ij} is i.i.d. Type-I Extreme Value with group-specific scale σ_g. Match preferences enter choice probabilities as parameters, and we study how changes in them affect mobility. The shock ε_{ij}, by contrast, generates closed-form expressions for choice probabilities and is not an object of interest for comparative statics.

In this set-up, we can study how changes in earnings or match preferences shape residential mobility by analyzing comparative statics on expected neighborhood quality. For households with earnings y_i and match preferences a_i, expected quality is

$$\bar{q}_i \equiv \bar{q}(y_i, a_i) = \sum_j \Pr(j \mid y_i, a_i)\, q_j,$$

where $\Pr(j \mid y_i, a_i)$ is the probability of choosing neighborhood j given y_i and a_i. Because q_j is fixed, any change in $\bar{q}(y_i, a_i)$ reflects migration.

A household's expected neighborhood quality can evolve because of changes in earnings or match preferences. The total derivative of expected neighborhood quality is:

$$d\bar{q}_i = \underbrace{\frac{\partial \bar{q}_i}{\partial \ln y_i}\, d\ln y_i}_{\text{earnings}} + \underbrace{\frac{1}{\sigma_g}\, \mathrm{Cov}\big(q_j,\, da_{ij}\big)}_{\text{match preferences}}, \tag{2}$$

where the covariance is weighted by the household's neighborhood choice probabilities. The first term reflects moves induced by earnings growth. The second reflects moves induced by shifts in match preferences. All derivations are in Appendix A.

Decomposing the first term, a change in earnings shapes mobility through two channels:

$$\frac{\partial \bar{q}_i}{\partial \ln y_i} = \frac{1}{\sigma_g}\left[\underbrace{\delta_g \operatorname{Var}(q_j)}_{\text{complementarity channel}} + \underbrace{\operatorname{Cov}\left(q_j, \frac{y_i}{y_i - r_j}\right)}_{\text{budget-constraint channel}}\right] > 0. \tag{3}$$

Under the *complementarity channel*, higher earnings raises the weight $\delta_g \ln y_i$ on quality, making better neighborhoods more attractive. The effect is proportional to the variance of neighborhood quality among the household's likely choices, since there are greater potential gains when neighborhoods differ more. Under the *budget-constraint channel*, earnings increases make higher-quality neighborhoods, which have higher rents, more affordable. These more expensive neighborhoods are where the marginal utility of consumption is highest (under our assumption that utility is concave in consumption). This channel is stronger in cities where the quality-rent gradient is steeper. Both channels are positive, so earnings growth moves households to better neighborhoods.

Separating the drivers of migration into these channels illustrates why residential mobility might differ across groups, foreshadowing the heterogeneity we observe later. First, groups differ in how much their earnings grow ($d \ln y_i$): those with more earnings mobility upgrade more. Second, groups differ in the response of quality to earnings, $\partial \bar{q}_i / \partial \ln y_i$. This response is larger for lower-earnings groups—for whom $y_i/(y_i - r_j)$ varies more sharply across neighborhoods, leading to a stronger budget-constraint channel—and for groups with higher quality-earnings complementarity δ_g, such as parents. Third, changes in match preferences, $\frac{1}{\sigma_g}\operatorname{Cov}(q_j, da_{ij})$, might evolve differently as attachments and constraints change. Finally, these channels are muted for households who are more anchored to their current neighborhood. A high home-neighborhood match value a_{ij}—reflecting factors like moving costs and social ties—concentrates choice probabilities at the current location, compressing the variances and covariances above and dampening the response to earnings and preference shifts. The dispersion of idiosyncratic preferences, σ_g, also dampens migration responses.

3 Data and Sample Construction

3.1 Data Sources

We draw longitudinal address location information from the Census Bureau's Master Address File-Auxiliary Reference File (MAFARF). This data includes the geolocated housing unit of residence for the near-universe of adults in the U.S. in each year (our vintage includes 2000–2021), which is derived from federal tax, health, and housing records.[4] We use these data to identify each individual's (2010 vintage) census tract in each year, which we use as our definition of neighborhood throughout the paper. We validate the MAFARF using decennial census and American Community Survey (ACS) data, as discussed further below.

We use earnings data from the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) database. Our LEHD extract contains 25 states and at least the 2000 to 2021 time period for all states. The data includes quarterly employment and earnings for individuals whose employers participate in unemployment insurance, as well as employer information, such as industry NAICS code. The included states are representative of the country in terms of population, region, and demographic composition.[5] We adjust earnings (and all other dollar-valued variables used in the analysis) to 2019 dollars using the Consumer Price Index.

Individual and household demographics are mostly drawn from the ACS, which is an annual survey spanning roughly three percent of U.S. households. For each individual, we observe demographics including age, sex, education, and race/ethnicity. At the household level, we observe whether a household rents or owns their primary residence, their estimated house value or monthly rent, and total household income. In addition, the ACS contains self-reported measures of earnings,

[4] Graham et al. (2017) and Sullivan and Genadek (2024) discuss the administrative data sources used to construct the MAFARF and provide some basic validation.

[5] Our LEHD extract consists of Arizona, California, Colorado, Connecticut, Delaware, Indiana, Iowa, Maine, Maryland, Massachusetts, Nevada, New Jersey, New Mexico, North Dakota, Ohio, Oklahoma, Pennsylvania, South Carolina, South Dakota, Tennessee, Utah, Virginia, Washington, Wisconsin, and Wyoming.

wages, and hours worked that supplement the LEHD data. We also use the Department of Housing and Urban Development's Tenant Rental Assistance Certification System to identify whether households in the ACS are living in public housing or receiving voucher assistance.

Finally, we construct neighborhood characteristics by aggregating ACS responses to the tract level. We calculate poverty rates, median household incomes, and median home values using the 2005 to 2009 waves of the ACS, which coincides with the start of our sample period. We use time-invariant measures of neighborhood characteristics to ensure that these variables are not influenced by the migration that we study.

3.2 Sample Construction

We combine these data sources to create a large and representative panel of individuals containing information on demographics, residential location, and labor market outcomes. We then use that overarching panel to construct a migration sample, which we use for exercises that do not require earnings information, and an earnings sample.

The initial sampling frame consists of respondents to the 2005 to 2013 waves of the ACS. We restrict to individuals who, at the time they were sampled in the ACS, were at least 25 years old, not full-time students or in the armed forces, not the child of the household head, and not living in group quarters or a census tract in which more than 20 percent of the population is between the ages of 18 and 25. The last restriction, which affects about one percent of census tracts, is intended to remove neighborhoods with high amounts of college student housing from the sample.

We define the baseline year for each individual as the year that they were sampled in the ACS.[6] We then merge in longitudinal address information from the MAFARF for the three years prior to and ten years following the baseline.[7] For people living in a state included in our LEHD extract in the baseline year, we then add information on employment and earnings.

Throughout the paper, we group tracts into broad categories based on their poverty rate in the 2005–2009 ACS. The categories are 0 to 10 (roughly the bottom half of tract poverty rates), 10 to

[6] In the event that an individual was sampled twice, we use their first response.

[7] Graham et al. (2017) show that 94 percent of 2012 ACS respondents appear in the MAFARF.

20 (percentiles 50 to 75), 20 to 30 (percentiles 75 to 90), and 30 to 100 percent (the highest decile). For robustness, we also use population-weighted quintiles of median household income (defined nationally) to define neighborhood categories in some exercises.

Depending on the exercise, we use different subsamples of this overarching panel. Defining these samples separately highlights that we measure migration for a nationwide sample, while our analysis of earnings is limited to states in the LEHD extract and focuses on individuals well below common retirement ages. Appendix Table A.1 shows descriptive statistics for each sample.

Migration sample: For results on migration that do not incorporate any earnings or employment information, we use the matched ACS-MAFARF data for the full U.S. In order to include ten years of neighborhood locations following the baseline ACS survey, we include only respondents to the 2005 to 2011 ACS waves. The migration sample consists of 13.2 million unique individuals, including 670,000 who live in the high-poverty neighborhood category in the baseline year.

Earnings sample: For results on earnings dynamics or the relationship between earnings and migration, we use the matched ACS-MAFARF-LEHD data. We include the 2005 to 2013 ACS waves and use an eight-year follow-up window.[8] We also drop individuals who were over age 55 at the time of the ACS survey to limit the role of retirement. The resulting sample contains 3.6 million unique individuals, including 164,000 in high-poverty tracts at baseline. In some exercises, we define people with earnings below $4,000 in the LEHD as nonemployed.

3.3 High-Poverty Neighborhoods

We are especially interested in high-poverty neighborhoods, defined as census tracts with poverty rates over 30 percent.[9] This is approximately the poorest decile of tracts, and they have similar composition to the impoverished areas that were targeted by the federal Empowerment Zones

[8]We make this slight change from the selection criteria of the migration sample to reduce the share of observations that begin during the peak of the Great Recession.

[9]Prior literature has generally defined high-poverty neighborhoods as those where the poverty rate exceeds 30 (Wilson, 1987) or 40 percent (Jargowsky, 1997; Kneebone et al., 2011).

program in the 1990s. Concentrated poverty of this sort is highly persistent (Appendix Figure A.1). Only 8.7 percent of tracts that were above our high-poverty threshold in 1990 saw their poverty rate drop below 20 percent in 2010. While these neighborhoods are geographically widespread, they are disproportionately urban, with 92 percent of high-poverty residents living in an urban area. They contain substantial racial and economic diversity—illustrated below with some statistics from the 2005–2009 ACS and 2010 census—which underpins our examination of heterogeneity across many subgroups.

First, no single race or ethnic group dominates the set of poor neighborhoods: 29 percent of residents are non-Hispanic White, 29 percent are non-Hispanic Black, and 36 percent are Hispanic. However, these nationwide averages overstate the degree of racial diversity within individual high-poverty tracts. Ninety percent have a racial or ethnic majority, split roughly evenly between White, Black, and Hispanic majorities.

Similarly, not all residents of high-poverty neighborhoods are poor. Thirty-one percent make over $25,000 (approximately the national median of individual earnings). Labor force attachment is lower in poor neighborhoods: the prime-age (25–54) employment rate is 61 percent versus an average in less-poor neighborhoods of 78 percent. Differences in the housing market are consistent with these income patterns, with 58 percent of housing units in poor neighborhoods occupied by renters (versus an average of 31 percent in the remainder of the sample) and 8.2 percent of renter households using a housing choice voucher (versus an other-neighborhood average of 4.0 percent). The overall picture from these statistics is quite similar to the analysis of Jargowsky (1997) using the 1990 census, suggesting that the important patterns are stable over time.

4 Migration Across Neighborhoods

This section documents patterns of residential mobility across neighborhoods. We first show that migration rates are high, with residents of poor neighborhoods moving more frequently, usually to lower-poverty areas. We then study longitudinal exposure to high-poverty neighborhoods and find both a bimodal distribution and significant variation across demographic groups.

4.1 Migration Rates

Figure 1 plots the probability that an individual lives in their baseline tract in each of the ten subsequent years, separately for individuals beginning in different types of neighborhoods. Among all individuals, 43 percent of people live in a different tract at the end of the ten-year period. Panel A shows that rates of out-migration are elevated in higher-poverty neighborhoods: in tracts where the poverty rate exceeds 30 percent, over 10 percent of individuals move out of the tract each year and 51 percent live in a different neighborhood ten years later. Even in low poverty neighborhoods, over 40 percent of residents exit within 10 years. Panel B shows a similar pattern of higher migration rates out of poorer neighborhoods when classifying tracts based on their median income.[10] While these results are unique in describing migration patterns across the full spectrum of neighborhoods, they are consistent with recent estimates focused on specific groups of people and neighborhoods.[11]

One potential explanation for elevated migration rates out of poorer neighborhoods is that their residents simply belong to demographic groups who have higher migration rates regardless of where they live, such as younger individuals. To examine this, Table 1 reports regressions in which the dependent variable is an indicator for moving out of one's baseline tract over an eight-year period. Comparing the raw cross-neighborhood differences in mobility rates in column 1 to the within-CBSA, demographics-adjusted differences in column 3, resident composition does not appear to drive the baseline differences in mobility rates across neighborhoods.

The economic significance of these moves depends on the types of neighborhoods people move to. If individuals from poor neighborhoods simply move to other poor neighborhoods, there may be little improvement in the conditions or opportunities they face. We report the full matrix of tran-

[10] In Appendix Figure A.2, we replicate Figure 1 using tract locations drawn from the 2005–2009 ACS surveys and the 2010 decennial census rather than the MAFARF. Estimates are quite similar, consistent with the MAFARF capturing migration accurately.

[11] Brummet and Reed (2021) find that 52.3 percent of the residents of initially low-income, central city tracts in 2000 had moved by the time they were observed in 2010–2014. Staiger et al. (2024) study census block groups containing public housing buildings and find that just under 20 percent change block groups in the baseline year, increasing to 65 percent after ten years. Frenette et al. (2004) find a one-year exit rate of 13.8 percent in low-income census tracts in large Canadian cities.

sitions across neighborhood types over an eight-year period in Table 2. There are many transitions to other neighborhood types, especially among people originating in poorer neighborhoods. For example, 36.5 percent of people who begin in the highest poverty category transition to a tract with a poverty rate below 30 percent, implying that 78 percent of tract migrants move to lower-poverty neighborhoods (= 36.5/46.6). The small relative size of the highest-poverty category (roughly 10 percent of tracts and 5 percent of individuals in the sample) mechanically increases its out-migration rate relative to other categories, but in Panel B we similarly see that 17.8 percent of the bottom tract income quintile transitions to the third quintile or above.[12,13] Individuals also move from richer to poorer neighborhoods, although these types of moves are less common. This asymmetry is consistent with both individuals increasing neighborhood quality over the lifecycle and slow population growth in poor neighborhoods.

Several studies using the PSID also document high migration rates out of poor neighborhoods, but are limited by small sample sizes of hundreds or a few thousand individuals (Gramlich et al., 1992; Massey et al., 1994; South and Crowder, 1997).[14] The modern administrative data allows us to measure migration with less noise and consider more granular neighborhood categories. We also measure transitions directly over longer time horizons, whereas prior work observes only one-year transitions, which imply exaggerated long-run mobility.[15]

[12] In Appendix Figure A.3, we create a variant of Table 2 using both the MAFARF and tract locations drawn from the ACS and 2010 decennial census. The different data sources again yield very similar results.

[13] Table 2 implies that individuals starting in the bottom three quintiles of tract income are more likely to move to a different income quintile than individuals starting in the top quintile. Appendix Table A.3 shows that this difference survives controlling for CBSA fixed effects and individual demographics.

[14] Gramlich et al. (1992) report that 9.9 percent of poor residents of tracts with over 30 percent poverty move to a tract below that poverty threshold in a given year, while South and Crowder (1997) find an 8.0 percent annual exit rate using a 20 percent poverty cutoff. These are both close to our estimate of 8.8 percent (using a 30 percent cutoff). South and Crowder (1997) find that residents of poor tracts are more likely to relocate to another poor tract than to a non-poor one (12.5 versus 8.0 percent annually), whereas we find that the large majority of movers from the highest-poverty tracts reach lower-poverty neighborhoods. This difference could be driven by their lower tract poverty threshold, differences in sample period, or sampling error.

[15] In our data, the one-year out-migration rate from the highest-poverty category is 8.8 percent (Appendix Table A.2). Extrapolating this forward as a Markovian transition probability implies an eight-year out-migration rate of 52.1 percent ($= 1 - (1 - 0.088)^8$), which is much higher than our direct estimate of 36.5 percent (Table 2).

4.2 Longitudinal Exposure to Concentrated Poverty

These initial results indicate that many residents of high-poverty neighborhoods at a given point in time will exit over the subsequent decade. We quantify *longitudinal* exposure to concentrated poverty in Table 3, separately for a number of demographic groups. For this analysis, we take the set of people living in tracts with poverty rates over 30 percent in their baseline year and calculate the share of the following ten years that they spend in tracts above this threshold.[16] We freeze tract poverty at 2005–2009 levels and consider the role of neighborhood change separately.

The first row of Table 3 shows that the overall distribution of exposure to high-poverty neighborhoods is bimodal. Thirty-one percent of individuals living in a high-poverty tract at baseline spend no more than 5 out of the next 10 years in concentrated poverty, while 54 percent experience a full 10 years of exposure. The average individual has 7.2 years of exposure. Exits from concentrated poverty typically entail moves to significantly lower-poverty neighborhoods—the average person who has exited concentrated poverty by year 5 lives in a tract with poverty rate 24 percentage points lower than their baseline tract. These results are consistent with the existence of both a large mass of people who are "stuck in place" (Sharkey, 2013) and a significant group who is much more mobile. A similar aggregate pattern emerges in Quillian (2003), who studies concentrated poverty exposure using the PSID.[17] We next study differences across subgroups, shedding light on what factors predict short versus long spells.

The largest differences are across age groups: people aged 25–34 at baseline spend an average of 5.5 years in concentrated poverty, compared to 7.6 years for those aged 45–54 and 8.5 years for those over 65. This gradient is consistent with younger residents experiencing more change in their earnings and life circumstances, while older residents may have both less incentive to move

[16] We include only individuals who appear in the MAFARF in at least 8 follow-up years and we proportionally adjust exposure of those observed in fewer than 10 years.

[17] The most closely related result from Quillian (2003) is that, counting neighborhood change as an exit, 39.8 percent of individuals in a tract with over 20 percent poverty at time t are in a similarly high-poverty tract for all of the subsequent ten years. (We construct this estimate by pooling Quillian's estimates for White and Black individuals.) If we handle neighborhood change in the same way, the analogous estimate in our sample is 41.1 percent. In addition, Quillian (2003) finds very similar reductions in tract poverty among people exiting high-poverty areas (about 20 percentage points).

and higher moving costs. Surprisingly, exposure varies less by race, with Black (7.3 years) and Hispanic (7.5 years) individuals having only slightly higher exposure than White individuals (6.8 years). Households where children are present see 6.8 years of exposure, versus 7.5 for other households, including a particularly low 6.1 years among adults under 40 who live in a household with children. We cannot infer the causal effects of having children from these differences, but they suggest that most young children, who Chetty et al. (2016) find are most negatively affected by concentrated poverty, see less exposure.

Housing tenure, which shapes moving costs, also matters. Homeowners have the highest average exposure to concentrated poverty at 8.0 years, potentially reflecting high transaction costs and local attachments. Public housing residents are nearly as persistent at 7.9 years, consistent with their very limited ability to take their housing benefits elsewhere. Voucher holders, whose subsidies travel with them, show lower exposure at 6.7 years, and unsubsidized renters, who face the fewest structural barriers to moving, show the lowest at 6.1 years. Labor force participation also strongly predicts exposure: workers are 12 percentage points more likely to have five or fewer years of exposure. However, average exposure is nearly identical for the poor and non-poor, likely because poverty status is highly correlated with age and household composition.[18]

Finally, differences emerge across geographies. Exposure decreases slightly as CBSA size increases, potentially reflecting differences in the size and variance of the neighborhood choice set. Consistent with our model's prediction, the slope of the neighborhood quality-rent curve appears to be important. We stratify individuals who live in CBSAs according to the difference between the log of average rents in the CBSA's high-poverty tracts and its tracts with 20 to 30 percent poverty. In areas where this gap is over 0.20, individuals have an additional year of poverty exposure and are 10 percentage points less likely to exit within five years.

[18]These results focus on how demographics correlate with exit from poor neighborhoods, but studying entrants is also informative. Appendix Table A.4 shows that the people who enter high-poverty neighborhoods in a given year have similar demographic characteristics to the people who exit, while stayers are substantially older, more likely to own their home, and less likely to be employed. This is again consistent with the existence of a relatively young and mobile group that passes through these neighborhoods and looks markedly different from long-term residents. However, exiters also have somewhat higher earnings, income, and employment than entrants. This is consistent with individuals using poor neighborhoods to smooth consumption over low-income periods of the lifecycle or idiosyncratic shocks, as we explore further below.

For individuals who do *not* move, changes in neighborhood conditions can also affect long-run exposure to concentrated poverty. Column 6 shows the share of people who stayed in their baseline tract for ten years and saw the tract's poverty rate fall below 25 percent in the 2015–2019 ACS. These are "stayers" whose neighborhood poverty rate fell by at least 5 percentage points during the sample period. This group represents 12.7 percent of all individuals who start in a high-poverty neighborhood and nearly 25 percent ($= 0.127/0.538$) of all people with ten years of exposure. The average person in this group sees their tract poverty rate fall by 15 percentage points.[19]

5 Earnings Dynamics in Different Neighborhoods

We now turn from residential mobility to earnings mobility: Do residents of poor neighborhoods experience meaningful earnings growth? The combination of the LEHD and MAFARF data allows us to address this question directly, with the caveat that we only observe earnings from formal wage and salary employment. Unlike most of the earnings dynamics literature, we include all individuals in our analysis rather than imposing a minimum earnings threshold, since differences across neighborhoods in the share of low earners and non-employed individuals may be important.

We begin by describing labor force transitions over an eight-year period among people who lived in different types of neighborhoods in the baseline year. Table 4 shows that residents of high-poverty tracts are modestly more likely to be nonemployed in both periods (30.6 vs. 26.2 percent in the lowest poverty tracts), but the more notable pattern is that they are also more likely to transition between employment and nonemployment (in either direction).

The distributions of eight-year real earnings changes in Figure 2 reinforce this picture. The distributions are surprisingly similar overall, with comparable right tails and large shares experiencing modest gains. Notably, 13 percent of residents in the poorest neighborhoods experience real earnings growth over $20,000. This is likely driven in part by the high frequency of transitions

[19]These estimates may raise concerns that incorporating neighborhood change would significantly alter our exposure estimates. To investigate this issue, we calculate the average tract poverty rate experienced by individuals in the Table 3 sample using the 2005–2009 ACS poverty rates and the 2015–2019 rates. The figure is 31.9 percent with the earlier survey and 28.0 percent with the later, suggesting that neighborhood change slightly reduces exposure.

into employment in these areas. To distinguish between employed individuals with little earnings change and those who are consistently nonemployed, we omit the latter from the histogram and report their share of each neighborhood type in the corner of each panel.

We more formally examine differences in mean earnings growth across individuals in different neighborhoods in Table 5. We first regress eight-year individual earnings changes—in levels in Panel A and arc percent in Panel B—on indicator variables for individuals' baseline neighborhood poverty category.[20] Residents of high-poverty neighborhoods experience real earnings growth of almost $3,000 on average. Mean earnings increases in levels are higher in less-poor neighborhoods, but much of this gap can be explained by differences across individuals in race and ethnicity, age, and baseline education (column 3). The comparison of growth rates in Panel B is especially notable: the average arc percent change is quite similar across neighborhoods, and the modest remaining gap between the highest- and lowest-poverty neighborhoods narrows to 1.6 percent with individual controls. When we restrict to people with moderate labor force attachment, differences across neighborhood types vanish (Appendix Table A.5)—consistent with the residual gap in earnings growth rates being explained by changes in employment status, not differences in wage trajectories conditional on working.

Overall, our results suggest that poor neighborhoods contain a substantial number of upwardly-mobile individuals. This is consistent with prior work finding that poor neighborhoods do not significantly worsen adults' labor market outcomes (Kling et al., 2007). The earnings growth is important not only for characterizing who lives in poor neighborhoods generally, but also because it may be an important driver of the migration we studied in the previous section.

6 Earnings Changes and Migration

We next examine the relationship between earnings growth and residential mobility. We begin with descriptive evidence illustrating that individuals tend to move to higher-income neighbor-

[20] The arc percent change is the percent change using the average of the start and end points as the denominator. This allows us to calculate proportional earnings growth among people with zero earnings at the start or end point.

hoods when their earnings rise, especially when coming from poor neighborhoods. We then use a research design based on idiosyncratic, firm-specific changes in pay to better establish the connection between earnings growth and migration to richer neighborhoods. Motivated by the model in Section 2, we use these estimates to quantify the relative importance of earnings and non-earnings factors in driving migration. Overall, this evidence shows that earnings growth is a quantitatively important driver of individual migration across neighborhoods, though non-earnings factors are also important for some groups.

6.1 Descriptive Evidence

Figure 3 shows how neighborhood choices vary with long-term earnings changes. In Panel A, we bin individuals by their change in real annual earnings over an eight-year horizon along the x-axis and plot the average difference in median income between their ending and baseline census tracts on the y-axis. Because the difference in tract median income is equal to 0 for individuals who do not move, the average difference in each bin reflects both the frequency and direction of migration. We stratify individuals based on their baseline tract poverty rate, and we separate individuals who are nonemployed in both years from others.[21]

People with larger increases in earnings systematically move to higher-income tracts, and this relationship is strongest among those who begin in higher-poverty areas. Among individuals starting in the highest-poverty category, a $20,000–$30,000 increase in annual earnings is associated with a $4,700 larger gain in tract income than an earnings increase of $0–10,000. In contrast, among individuals beginning in the lowest-poverty neighborhoods, the same difference in earnings growth is associated with only a $600 gain in tract income. The y-axis intercepts vary significantly across neighborhoods as well. The average employed person with no earnings change who began in the highest poverty category improves their tract income by $11,000, while those in the lowest-poverty tracts see a slight decrease in tract income. This points to differences in individuals' trajectories that are unrelated to earnings, potentially driven by life-cycle factors or mean

[21] Results are similar when stratifying individuals by baseline tract income quintile (Appendix Figure A.4).

reversion. Individuals who are nonemployed in both years see neighborhood improvement very similar to employed individuals with little change in earnings.

Changes in individual earnings are also associated with exit and entry into high-poverty neighborhoods. Panel B of Figure 3 considers individuals originating in high-poverty neighborhoods and plots the probability that they move to a lower-poverty category. For individuals with no earnings growth, the probability of exit is about 43 percent.[22] This probability increases rapidly as earnings grow, with 54 percent of individuals with earnings growth of $20,000 to $30,000 exiting high-poverty neighborhoods.[23] As a comparison point, Panel C of Figure 3 focuses on individuals originating outside high-poverty neighborhoods and plots the probability that they live in one eight years later. People with lower earnings growth are more likely to enter a high-poverty neighborhood, especially those starting in a medium-poverty tract. This relationship is strongest for people with low baseline earnings (Appendix Figure A.5), consistent with their being closest to the margin of entry due to limited financial resources.

6.2 Quasi-Experimental Methodology

While suggestive, these correlations need not reflect a *causal* effect of earnings growth on residential mobility. They may instead capture correlated life-cycle trends in both earnings and neighborhood choice. To isolate a causal effect, we exploit idiosyncratic shocks to pay at specific employers as in Koustas (2018) and Ganong et al. (2020). This type of unanticipated earnings change is particularly interesting to study in this context, as forward-looking individuals might make migration choices based on expected earnings in the future. The extent to which positive earnings shocks lead to neighborhood upgrading informs the degree to which affordability is a binding constraint in neighborhood choice, and it also has implications for place-based policy.

Our research design seeks to compare observationally similar individuals who are employed at

[22] This is slightly higher than the 37 percent exit rate in Table 2 because this exercise uses the earnings sample.

[23] Some related work has studied the correlation between changes in individual poverty status and migration. Using Canadian administrative data, Frenette et al. (2004) find that less than half of migration into or out of concentrated poverty coincides with a large earnings change or change in household composition. Using the PSID, Massey et al. (1994) do not find that people who moved in a year are more likely to have entered or exited poverty in the same year.

observationally similar firms at baseline, but whose firms' payroll grows at different rates. Using idiosyncratic changes in pay at particular firms allows us to hold broader changes in neighborhood conditions constant. Following Rose and Shem-Tov (2023), we focus on individuals in our earnings sample who report working more than 1042 hours in the ACS, corresponding to approximately half-time employment.[24] We further restrict the sample to individuals for whom we can identify at least 25 coworkers in the LEHD data. Column 3 of Appendix Table A.1 presents summary statistics for this sample, which, by construction, contains individuals who are more likely to be consistently employed and have higher total income than individuals in the broader samples.

For each individual, we construct the "pay shock" as the average percent change in earnings among a holdout sample consisting of their coworkers who are observed in the LEHD but not sampled in the ACS between 2005 and 2013. We measure this earnings change between the baseline quarter (when the index individual was sampled in the ACS) and four quarters later.[25] Using year-over-year changes in earnings limits the impact of seasonality, and our restriction to individuals with at least 25 LEHD coworkers limits the noise in the pay shock variable.

To compare observably similar individuals at similar firms, we include both individual-level and firm-level controls in our primary specification, again following Rose and Shem-Tov (2023). The individual controls include: initial hourly wage (constructed using the ACS), 2005–2009 median income of the tract of residence in the ACS, age, sex, race, ethnicity, and education. Firm-level controls (computed using the holdout sample over the four quarters prior to the baseline) include: log firm employment, mean pay, median pay, average separation rate, average new worker accession rate, and average separation rate into non-employment.[26] Finally, we also include fixed effects that control for differential trends across industries and local labor markets. After the main results,

[24] To limit the role of outliers, we treat person-years in which an individual earned less than $4,000 or over $300,000 in the LEHD as missing. Similar sample restrictions are common in the literature (e.g. Abowd et al. 2018).

[25] To better measure changes in firm payroll, we restrict the set of coworkers to those who earn more than $2,600 at baseline (roughly half-time employment at minimum wage) and are employed at the same firm from the quarter before baseline until the fifth quarter after baseline. To limit the influence of outliers, we successively winsorize individual earnings, the percent change in individual earnings, and the pay shock at the 99th percentile.

[26] To allow for flexible functional forms, we use indicator variables for quintiles of control variables that are continuous (including hourly wage, 2005–2009 median income of the tract of residence in the ACS, age, log firm employment, mean pay, median pay, separation rate, new worker accession rate, and separations into non-employment).

we show robustness to specifications using a reduced set of controls and fixed effects.

The resulting regression specification for individual i sampled in year $a(i)$ is:

$$Y_{i,t} = \mu_i + \sum_{k \in \mathcal{E}} \beta_k \text{PayShock}_i \times \mathbb{1}[t = a(i) + k] + \theta_{t,q(i)} + \omega_{t,a(i),n(i),s(i)}$$
$$+ \lambda_{t,a(i),c(i)} + \sum_{k \in \mathcal{E}} X_i' \alpha_{a(i),k} \times \mathbb{1}[t = a(i) + k] + \epsilon_{i,t,k}, \quad (4)$$

where $Y_{i,t}$ is the outcome of interest in year t, $\mathcal{E}$ is the set of years used to estimate the event-study coefficients (-5 to 8, excluding -1), and $q(i)$ references the specific quarter-year the individual was sampled. We scale the PayShock_i variable so that coefficients can be interpreted as the effects of a 10 percent year-over-year increase in coworker earnings, which is slightly more than one standard deviation of the shock.[27] We control for common trends among individuals sampled in the same quarter-year using the fixed effects $\theta_{t,q(i)}$. The fixed effects $\omega_{t,a(i),n(i),s(i)}$ allow trends in outcomes to vary arbitrarily based on individuals' sampling year ($a(i)$), baseline 2-digit NAICS industry ($n(i)$), and baseline state of residence ($s(i)$). The fixed effects $\lambda_{t,a(i),c(i)}$ additionally absorb variation in changes over time by sampling year and baseline CBSA of residence ($c(i)$). X_i is the vector of individual and firm-level controls, which are interacted with relative time dummies and allowed to vary across ACS sample cohorts. Finally, we apply ACS sampling weights and cluster standard errors at the firm level.

6.3 Causal Effects of Earnings Changes on Migration

Panel A of Figure 4 shows the dynamic effects of coworker pay shocks on several labor market outcomes. A 10 percent increase in coworker earnings is associated with an immediate increase in individual annual earnings of about 2.5 percent.[28] There is little evidence of a change in earnings in previous years, consistent with limited anticipation of these changes. Although the shocks are defined using coworker earnings changes in a single year, the effect is persistent and reverts only

[27] The mean of the pay shock is 2.2 percent and the standard deviation is 9.1 percent.

[28] Our first-period estimate of 2.5 percent is smaller than Koustas (2018) (4.1 percent) and Ganong et al. (2020) (3.8 percent). This may occur because we measure annual earnings, while they use bimonthly or monthly earnings.

gradually, remaining elevated by 1 percent in year 8. *Cumulative* earnings effects thus grow over time. Because total household earnings may matter more for location decisions, we also plot the effect on this outcome, defined as the combined LEHD earnings of individuals and any partner they report in the ACS. The effect on household earnings is slightly smaller, reflecting the presence of multiple earners in many households.

Figure 4 also shows the effect of a pay shock on separations. We find that a positive shock increases the probability of remaining at one's initial firm, which, all else equal, might lower the incentive to migrate. Analogously, a negative shock should have the opposite effect.

Turning to the effects on migration, Panel B of Figure 4 shows that individuals with more positive pay shocks are more likely to move, with a cumulative effect size reaching 0.4 percentage points eight years after the initial shock. Our earlier results indicate that the baseline probability of moving over an eight-year period is roughly 40 percent, implying a 1 percent increase in the probability of moving in response to a persistent 1–2 percent increase in annual earnings. Interestingly, this increase is accounted for almost entirely by induced moves to higher-income tracts.

Figure 5 considers the relationship between the coworker pay shock and tract median household income and home value.[29] More positive shocks generate moves towards tracts with higher quality. In contrast to the effects on earnings, which fade over time, the effects on neighborhood quality *increase* over time, reaching roughly 0.25 percent at their peak. One potential explanation is that moving costs make long-run income expectations, rather than current income, the primary driver of relocation decisions. Mobility then rises as workers learn whether a shock is temporary or permanent. Relocation decisions may also be sensitive to accumulated earnings gains or losses, particularly for credit-constrained individuals buying homes. Overall, these results show that while individuals do not perfectly sort to a neighborhood that matches their new income after a shock, they do systematically move to better neighborhoods.[30]

[29] We use these measures of neighborhood quality because poverty rates are relatively flat in the upper third of the quality distribution, which blunts estimates based on samples that include all types of neighborhoods. Appendix Figure A.6 shows that positive pay shocks also lead to moves to tracts with a lower poverty rate.

[30] Our preferred regression specification includes many controls, but our results are also robust to alternative approaches. Appendix Figure A.7 replicates Figure 5 using only controls for industry, initial wage, initial tract income, and age and excluding state fixed effects, CBSA fixed effects, and firm-level controls and fixed effects. The results

To assess the magnitude of these effects, it is helpful to benchmark the long-term effect on neighborhood characteristics to the effect on household earnings. To facilitate such comparisons, we estimate a variant of equation (4) that pools periods into a pre-shock period, a short-run post period ($k \in [1, 3]$) and a long-run post period ($k \in [4, 8]$). We report estimates of this long-run post period coefficient in the first row of Table 6. Taking the ratio of the effect on log tract income (0.0018) to the effect on log own household income (0.0127) yields an elasticity of 0.140. Put differently, a coworker pay shock that increases long-run earnings by 10 percent also leads individuals to move to a neighborhood that has 1.4 percent higher median income. This elasticity is economically large. To put the magnitude in context, it implies that an eight-year log earnings increase equal to the population average of 0.08 should result in a move to a tract with 0.011 higher log median income, which is 35 percent of the actual average change of 0.032.[31]

6.4 Heterogeneity Across Subgroups

Table 6 describes heterogeneity across key subgroups in estimates of the impacts of coworker pay shocks on household earnings and tract income, along with the implied elasticity. It also includes the average earnings growth and average neighborhood improvement for each subgroup, which we use to empirically decompose migration towards better neighborhoods into factors related to earnings versus other factors.

Estimated elasticities vary widely, with the most striking contrasts appearing between renters and homeowners, and between individuals with and without children. The elasticity for renters is almost four times as large as the elasticity for owners (0.262 vs. 0.072), while the elasticity for individuals with children is more than twice that of childless individuals (0.204 vs. 0.092). We also

from the two specifications are nearly identical.

[31] In related work, Hilger (2016) estimates the impact of a layoff on zip code median house value using a sample of fathers observed in tax data. The implied elasticity of zip code median house value to earnings is 0.07 (reflecting a 1.2 percent decrease in zip code median house value and a 17.7 percent decrease in earnings five years after layoff). Our approach differs in the sample composition, use of a range of coworker pay shocks (not layoffs) that include intensive-margin earnings changes, and focus on tracts instead of zip codes. In contrast, studies of transitory windfalls like winning the lottery (Golosov et al., 2023) and temporary receipt of an unconditional cash transfer (Bartik et al., 2024) find positive effects on migration from baseline residence but no evidence of relocation to higher-income neighborhoods. This difference may occur because these shocks are not persistent.

find a substantially lower elasticity for White individuals than others. The elasticity differences are driven primarily by differences in the estimated effects of the shocks on neighborhood income, rather than the effects on household earnings.

The model in Section 2 provides three explanations for these cross-group differences. The complementarity channel predicts higher elasticities for groups whose valuation of neighborhood quality rises more steeply in income. This is a natural explanation for why the elasticity of individuals with children exceeds that of individuals without children. This channel also depends on the variance of quality across neighborhoods, which may explain why the elasticity is greater in larger metro areas. The budget-constraint channel predicts higher elasticities for lower-income groups, which could explain why the elasticity is larger for people in higher-poverty neighborhoods. Finally, we expect higher elasticities for groups with smaller idiosyncratic attachments or lower moving costs. This could explain why renters are more responsive than homeowners.

The results in Table 6 allow us to empirically decompose mobility towards better neighborhoods into a component explained by earnings increases—given by the product of average earnings growth and the elasticity of neighborhood quality to earnings—and a non-earnings component (with the caveat that this requires applying an elasticity identified from firm-driven earnings shocks to a broader set of earnings changes). The two components, shown in columns 7 and 8, both matter, with notable heterogeneity across groups. For renters, earnings growth explains 64 percent of the observed gain in neighborhood income, reflecting both a large elasticity and sizable earnings growth. By contrast, it explains only 18 percent among homeowners, who have a lower elasticity and experience smaller household earnings growth.

Finally, the results highlight several distinct explanations for disparities in mobility towards better neighborhoods across groups. In some cases, disparities can arise due to differences in responsiveness to income changes (i.e., elasticities), in other cases due to differences in average earnings growth, and in yet others due to differences in the non-earnings component. We find that younger individuals are more likely to move to better neighborhoods than older individuals despite similar elasticities—in this case, the larger earnings growth among the young drives the difference.

By contrast, we find that Hispanic individuals experience larger neighborhood improvements than White individuals despite nearly identical earnings growth because they have a much larger elasticity. There are also cases where mobility differs substantially across groups *despite* similar earnings growth and similar elasticities. In particular, individuals in neighborhoods with poverty rates of 10–20 percent are much more likely to move to better neighborhoods than individuals in the lowest poverty neighborhoods despite similar elasticities and earnings growth rates, suggesting some degree of convergence towards better neighborhoods that occurs irrespective of earnings growth.

6.5 Estimates in High-Poverty Neighborhoods

Finally, we consider high-poverty neighborhoods, defined here using a 20 percent threshold due to the smaller coworker sample and the power limitations of our research design. The last row of Table 6 reports estimates for individuals living in these tracts at baseline, and Appendix Table A.6 breaks them out by housing tenure and the presence of children. Individuals in poor neighborhoods are especially responsive to earnings increases: the implied elasticity of 0.29 is the highest of any subgroup. Applied to their earnings growth of 0.079 (near the sample average), this yields a predicted 2.3 percent improvement in tract income, again among the highest of any subgroup. This suggests that affordability is indeed a binding constraint for many people in poor neighborhoods. Renters and those with children upgrade neighborhoods the most in response to positive earnings shocks, consistent with our analysis of longitudinal exposure in Table 3.

However, although this earnings component is large relative to other groups, it is only a small share of the average neighborhood improvement of 23 percent experienced by high-poverty residents. Most movement out of high-poverty neighborhoods appears to be driven by factors other than earnings growth. In the context of our model, these results highlight a significant role for changes in earnings and an even larger one for changes in match-specific preferences and constraints.

7 Implications for Poor Neighborhoods and Place-Based Policy

So far, we have focused on estimating individual-level changes in migration and earnings. We next illustrate how these individual dynamics come together to create a mechanism that keeps poor neighborhoods poor. We then discuss how knowledge of these dynamics can inform the design of place-based policies.

7.1 Individual Dynamics and the Persistence of Concentrated Poverty

In *The Death and Life of Great American Cities*, Jane Jacobs described a process through which individual migration and earnings dynamics can lead to neighborhood stagnation:

> "Once a slum has formed, the pattern of immigration that made it is apt to continue. . . Successful people, including those who achieve very modest gains indeed, keep moving out. . . they are quickly replaced by others who currently have little economic choice."

Our individual-level results have shown that each element of this mechanism exists, and we now illustrate the net effect on neighborhood-level outcomes. We compare the earnings growth among the *initial* cohort of residents in high-poverty neighborhoods to the earnings of the same neighborhoods' *contemporaneous* residents in subsequent years. The initial cohort's earnings growth reveals how much individual economic improvement a neighborhood could conceivably capture, and the wedge between this and contemporaneous resident earnings reveals the net effect of selective in- and out-migration.[32]

We first compute growth in mean earnings among the cohort of people who lived in each tract poverty category when surveyed in the 2005 to 2009 ACS waves, regardless of where they lived in later years (restricting to the 1965 to 1980 birth cohorts to minimize the roles of retirement and sample entry.) As shown in Figure 6, earnings growth is large over the subsequent ten years for every category, between 24 and 26 percent in real terms. This is consistent with our earn-

[32] This approach is conceptually similar to papers that separate the roles of in-migration and out-migration in producing gentrification (McKinnish et al., 2010; Ellen and O'Regan, 2011; Brummet and Reed, 2021).

ings dynamics results, again illustrating that individual earnings growth is similar in all types of neighborhoods.

We next compute the mean earnings in each year for the contemporaneous residents of each tract type (restricting to ACS respondents in the same birth cohorts). In low-poverty tracts, neighborhood average earnings grow at about the same rate as the earnings of individuals who began in those neighborhoods. However, this is not true in either the 20–30 or 30+ percent poverty neighborhoods, where contemporaneous earnings grow by only 11 percent over the ten-year period, even though earnings of their initial residents grow by about 25 percent. This wedge appears because of another individual-level result: people whose earnings increase tend to move to better neighborhoods.

To provide additional insight into the mobility patterns that generate this wedge, Appendix Figure A.8 breaks out the evolution of earnings for a different set of subgroups: "stayers" who lived in the neighborhood type at baseline and t years later, entrants who lived in the type at t but not at baseline, and "exiters" who lived in the type at baseline but not at t. This illustrates two negative selection dynamics in poor neighborhoods. First, individuals with high earnings growth exit the neighborhood and are replaced by individuals with lower earnings levels. Second, the stayer group has especially low earnings growth, which is perhaps not surprising given the high baseline exit rates from high-poverty neighborhoods. Together, these patterns significantly depress growth in average neighborhood earnings to less than half of the growth experienced by the baseline cohort. In contrast, the figure shows that low-poverty neighborhoods see the exact opposite dynamics: stayers are positively selected, and entrants have higher earnings than exiters.

This process helps explain why concentrated poverty tends to persist through many macroeconomic fluctuations and local economic shocks—those forces affect individual outcomes, but selective migration limits their pass-through to neighborhood-level outcomes.[33] This is a sharp contrast to a pure poverty trap story in which poor neighborhoods persist due to depressed earnings growth and low migration rates.

[33]If shocks were to simultaneously affect a large enough share of a neighborhood, they could also change neighborhood quality and alter the selective migration pattern itself.

7.2 Implications for Place-Based Policy

Our results have important implications for a broad set of policies that seek to connect residents of poor neighborhoods to better job opportunities. Examples include the provision of employment assistance to public-housing residents through the Jobs Plus program, community hiring provisions that aim to tie government procurement to the employment of residents of poor neighborhoods, and the large number of enterprise zone programs. There are two key reasons why these types of programs might not improve neighborhoods even if they help initial residents of target neighborhoods. First, the high baseline migration rates out of poor neighborhoods may lead much of the gains created by these programs to leak out of the targeted neighborhood. Second, this dynamic could be exacerbated if program-induced increases in individuals' economic standing leads them to move elsewhere. These factors were likely important in the Jobs Plus program, which produced significant income improvements for individuals but had little effect for the targeted public housing complexes as a whole (Miller et al., 2023).

Relatedly, our results suggest a path to neighborhood revitalization that sidesteps common concerns about gentrification. Jacobs (1961) argued that high-poverty neighborhoods can be improved without large-scale residential turnover by "creating conditions aimed at persuading residents to stay by choice over time." Our results suggest that earnings growth in poor neighborhoods is strong enough that increasing retention of people whose income increases could meaningfully boost average neighborhood income. Policies that improve neighborhood amenities may operate through this channel: current residents likely respond to amenity improvements more strongly than potential newcomers, since they can capture the gains without paying moving costs. While this alleviates some worries about involuntary displacement or cultural change, improving a neighborhood may still raise housing prices and create affordability challenges for lower-income residents.

8 Conclusion

This paper uses newly available administrative data linking survey responses, longitudinal address histories, and employer-employee earnings records to characterize the joint dynamics of migration and earnings in poor neighborhoods. Three facts stand out. First, residential mobility out of high-poverty neighborhoods is high, with half of residents leaving within a decade, and the large majority of movers relocating to lower-poverty areas. Second, earnings growth among residents of high-poverty neighborhoods closely resembles that of residents elsewhere, including a substantial right tail of large gains. Last, increases in earnings shift households toward higher-income neighborhoods, with an elasticity of neighborhood income to own earnings that is largest for renters, families with children, and residents of poor neighborhoods.

Why does this mobility matter? It explains a good deal about why concentrated poverty persists. A sizable upwardly mobile group breaks the link between changes in individual and neighborhood income, so a place can remain poor even as many people who pass through it do not. This has direct implications for policy. Programs that successfully raise the earnings of residents in targeted neighborhoods may show little neighborhood-level effect, because high baseline mobility—amplified by the very income gains the programs generate—causes benefits to leak out of the target area. Conversely, policies that increase retention of upwardly mobile residents, for instance by improving local amenities, could meaningfully raise average neighborhood income.

Our results also raise welfare questions that merit further study: if some households deliberately use inexpensive, high-poverty neighborhoods to smooth consumption or economize while young—as Kennan and Walker (2011) and Bilal and Rossi-Hansberg (2021) show at the regional level—eliminating such neighborhoods could impose meaningful costs. Quantifying these trade-offs in dynamic models with realistic earnings risk is a natural direction for future work. As longitudinal individual data become more widely available, researchers could also extend our analysis to other settings. Slums in developing countries would be a particularly interesting, albeit challenging, setting (Marx et al., 2013).

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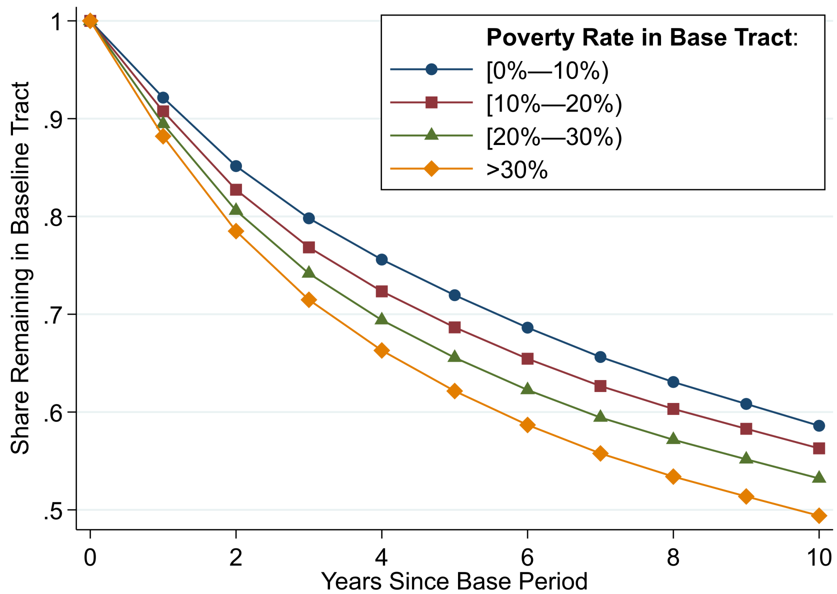
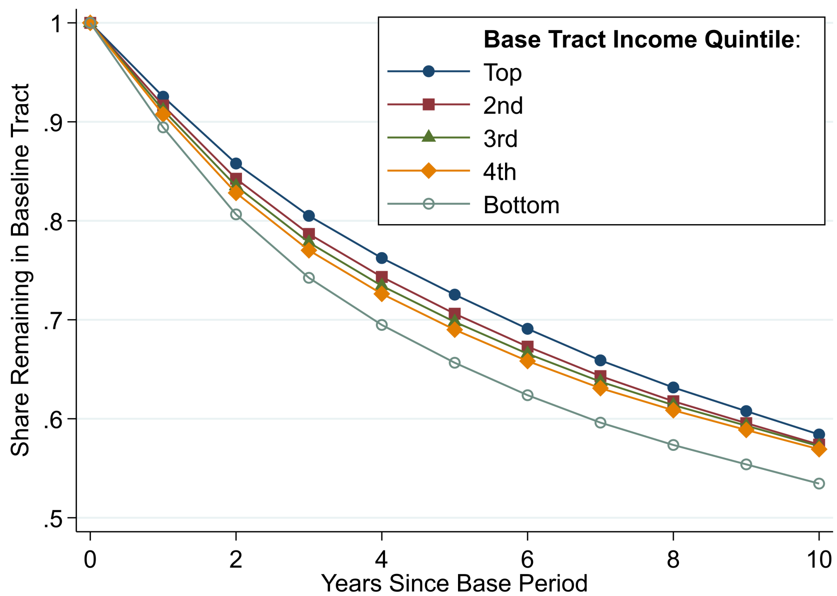
Figure 1: Share of Individuals Remaining in Baseline Tract Over 10 Years

(a) By Baseline Tract Poverty Rate Category



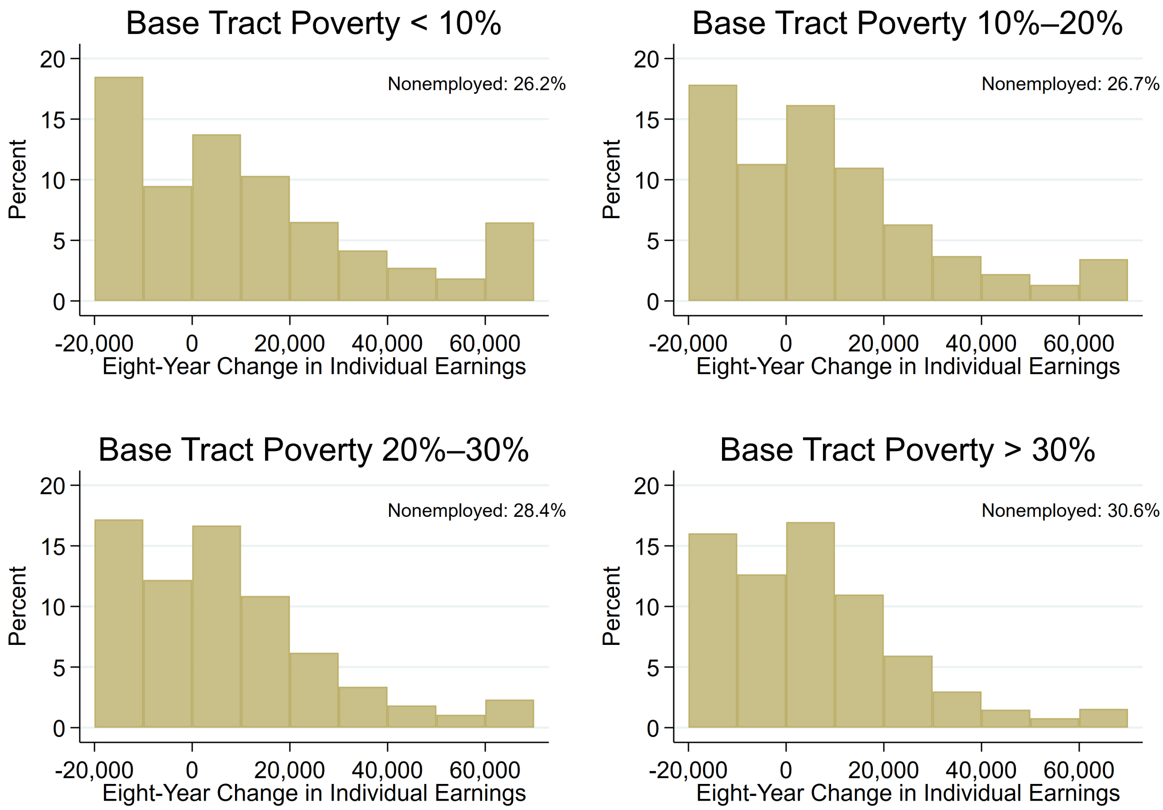
(b) By Baseline Tract Income Quintile



Notes: This figure shows the share of individuals who live in their baseline census tract for each of the 10 years after the baseline year (the year an individual is sampled in the ACS). Individuals are stratified based on the baseline tract's poverty rate (Panel A) or quintile of median household income (Panel B). This figure uses the migration sample, as described in Section 3.2.

Source: Authors' calculations using data from the ACS and MAFARF.

Figure 2: Distribution of Eight-Year Earnings Changes, By Baseline Tract Poverty Rate

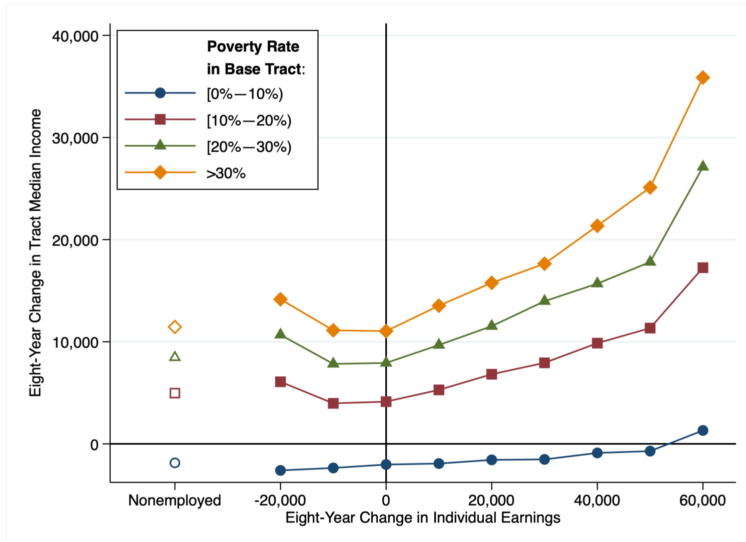


Notes: This figure shows the distribution of earnings changes over an eight-year period that starts when an individual is sampled in the ACS. Individuals are stratified based on the baseline tract's poverty rate. The left- and right-most bins are open-ended. If an individual has no earnings reported in the start or end year, earnings are treated as zero for that year. If earnings are missing in both the start and end year, individuals are binned separately in the "Nonemployed" category. The percent that is in the "Nonemployed" category is displayed in the top-right corner in each histogram. The figure uses the earnings sample, as described in Section 3.2.

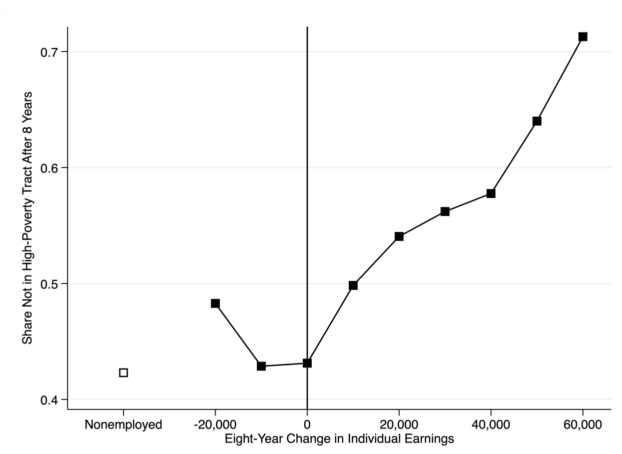
Source: Authors' calculations using data from the ACS, MAFARF, and LEHD.

Figure 3: Tract Mobility and Earnings Growth Over Eight Years

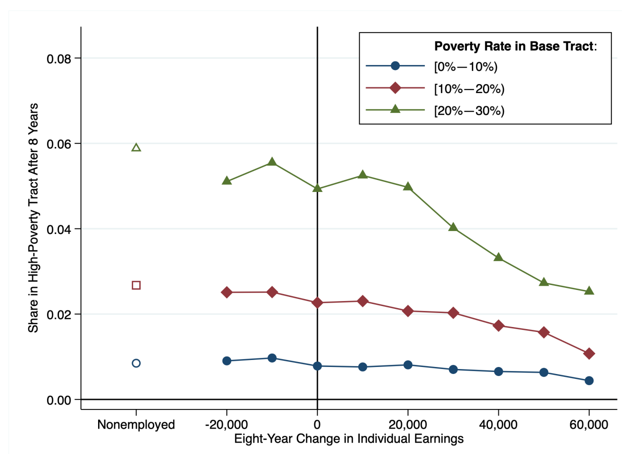
(a) Movement to Higher-Income Tracts, by Baseline Tract Poverty



(b) Exit From High-Poverty Tracts



(c) Entry Into High-Poverty Tracts

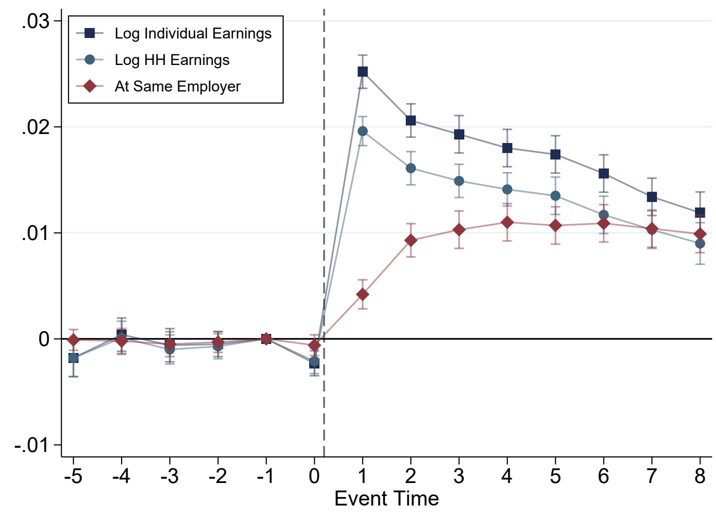


Notes: Figure presents binned scatter plots that show the average change in tract characteristics between each individual's baseline tract and the tract they live in eight years later for different levels of earnings changes over an eight-year period that starts when an individual is sampled in the ACS. If an individual has no earnings reported in the start or end year, earnings are treated as zero for that year. If earnings are missing in both the start and end year, individuals are binned separately in the "Nonemployed" category. The y-axis variable in Panel A is the average difference in tract median household income between the individual's baseline tract and the tract they live in eight years later in each earnings change bin, where individuals are further stratified based on the poverty rate of their baseline tract. Panel B shows the average probability of transitioning out of a high-poverty tract (baseline poverty rate over 30 percent) for those beginning in a high-poverty tract. Panel C shows the average probability of transitioning into a high-poverty tract for those who do not begin in a high-poverty tract, with individuals further stratified based on the poverty rate of their baseline tract. The left- and right-most bins are open-ended. The figure uses the earnings sample, as described in Section 3.2.

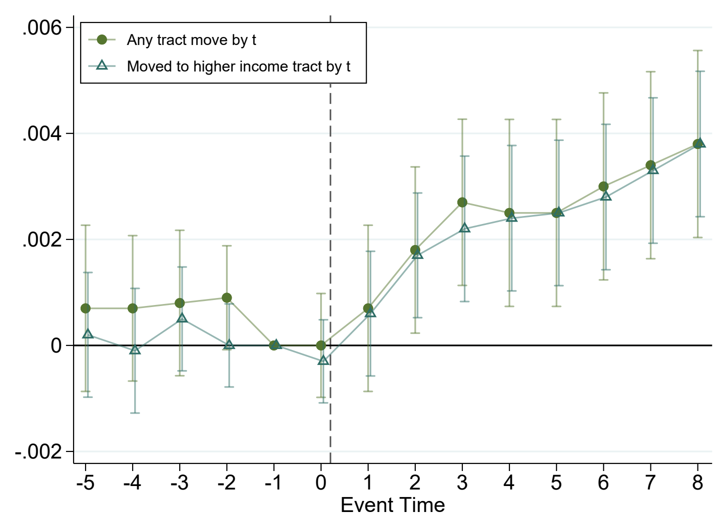
Source: Authors' calculations using data from the ACS, MAFARF, and LEHD.

Figure 4: Effects of a Coworker Earnings Shock on Labor Market and Migration Outcomes

(a) Effects on Labor Market Outcomes



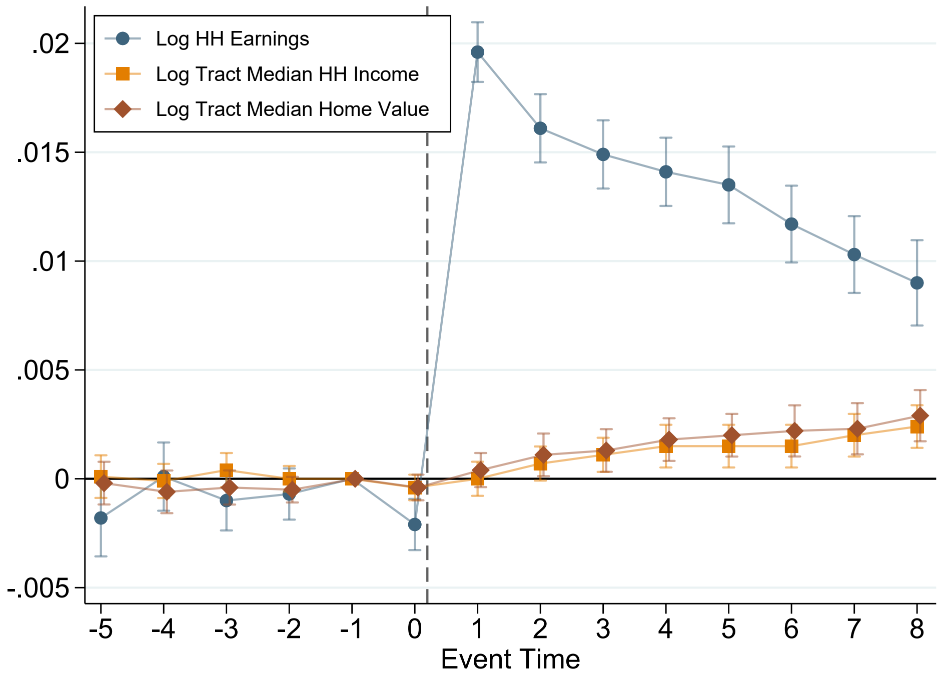
(b) Effects on Migration Outcomes



Notes: This figure displays OLS estimates of equation (4) with 95 percent confidence intervals. The figure uses a subset of the earnings sample consisting of individuals working at firms with at least 25 employees in the holdout sample, as described in Section 6.2. Effects are scaled so that coefficients can be interpreted as the effect of a 10 percent increase in coworker quarterly earnings over a four-quarter period that begins when an individual is sampled in the ACS. Panel A displays effects on the log of individual LEHD annual earnings, the log of household LEHD earnings (defined as the sum of one's own earnings and the earnings of any other individual who was a spouse or partner in the base year in the ACS), and an indicator for remaining at the baseline firm. Panel B displays effects on the cumulative probability of having moved from one's baseline tract and the cumulative probability of moving to a tract with a higher median household income than one's baseline tract. Standard errors are clustered at the firm level.

Source: Authors' calculations using data from the ACS, MAFARF, and LEHD.

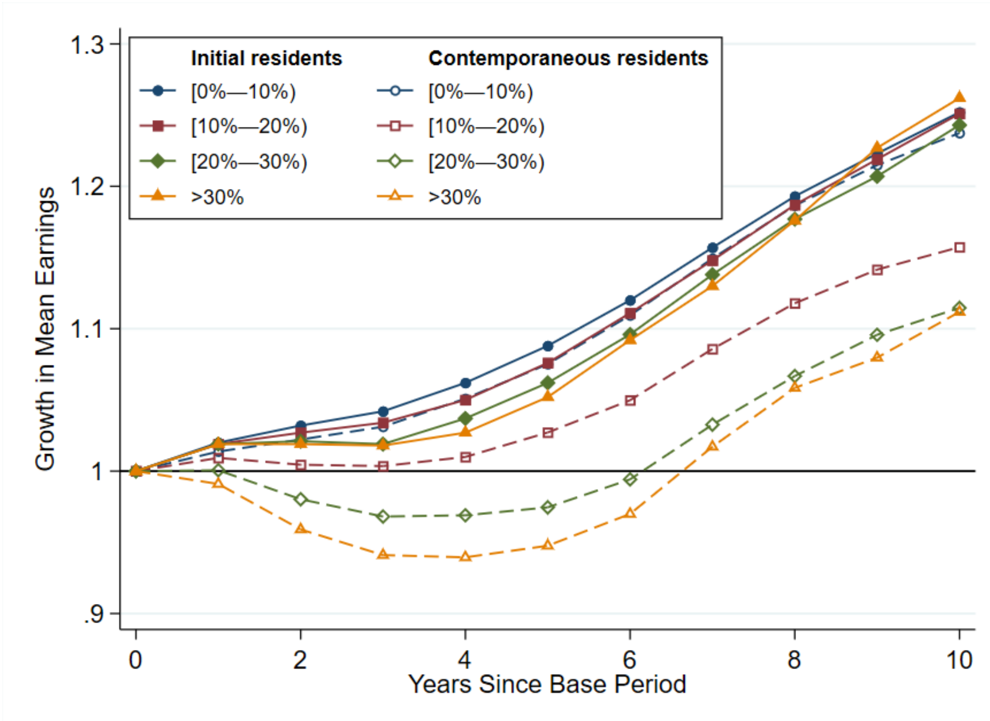
Figure 5: Effects of a Coworker Earnings Shock on Neighborhood Choice



Notes: This figure displays OLS estimates of equation (4) with 95 percent confidence intervals. The figure uses a subset of the earnings sample consisting of individuals working at firms with at least 25 employees in the holdout sample, as described in Section 6.2. Effects are scaled so that coefficients can be interpreted as the effect of a 10 percent increase in coworker quarterly earnings over a four-quarter period that begins when an individual is sampled in the ACS. The outcomes are the log of household LEHD earnings (reproduced from Panel A of Figure 4 for comparison), along with the log median household income and log median house value (calculated from 2005–2009) of the tract where individuals reside in each year. Standard errors are clustered at the firm level.

Source: Authors' calculations using data from the ACS, MAFARF, and LEHD.

Figure 6: Growth in Average Tract Earnings Versus Earnings Growth of Baseline Residents



Notes: This figure compares the evolution of average earnings in neighborhoods at different poverty levels among individuals who reside in these neighborhood types in each year ("contemporaneous residents") and among individuals who initially resided in each neighborhood type in the baseline year. Average earnings for initial residents appear in solid lines, and earnings for contemporaneous residents are in dashed lines. The exercise uses the earnings sample, as described in Section 3.2, and further restricts to the 1965 to 1980 birth cohorts (to minimize the role of retirement and sample entry). Mean earnings in each year are normalized by the value in the baseline year. Initial resident cohorts beginning in 2005 to 2009 are included, and the contemporaneous resident lines represent the average of the relevant comparison years for each of the initial cohorts (e.g., year 1 is the average of 2006 to 2010 residents).

Source: Authors' calculations using data from the ACS, MAFARF, and LEHD.

Table 1: Regressions of Migration Rate on Baseline Neighborhood Poverty

	(1)	(2)	(3)
Poverty rate 0–10	-0.097	-0.121	-0.087
	(0.002)	(0.002)	(0.002)
Poverty rate 10–20	-0.069	-0.070	-0.051
	(0.002)	(0.002)	(0.002)
Poverty rate 20–30	-0.038	-0.036	-0.026
	(0.003)	(0.002)	(0.002)
Intercept	0.466	0.480	0.454
Individuals	13,230,000	13,230,000	13,230,000
R-squared	0.003	0.019	0.105
CBSA and year FE		X	X
Individual controls			X

Notes: This table reports regressions in which the dependent variable is an indicator for whether the person moves away from their baseline tract over the eight-year period that starts when an individual is sampled in the ACS. The key explanatory variables of interest are indicators for the 2005–2009 poverty rate of the neighborhood where an individual is observed at the start of the eight-year period; the omitted indicator is for the poverty rate above 30 percent. Columns 2 and 3 include CBSA and year fixed effects. Column 3 also includes fixed effects for individual race, ethnicity, age, and baseline education. Standard errors, clustered at the level of the baseline tract, are reported in parentheses.

Source: Authors' calculations using data from the ACS and MAFARF.

Table 2: Eight-Year Transition Rates Across Neighborhood Types

Panel A: Neighborhood poverty rate

	Year $t+8$				
Year t	0–10	10–20	20–30	30+	Individuals
0–10	88.4	8.7	2.2	0.8	7,370,000
10–20	19.8	73.6	4.6	2.0	3,890,000
20–30	16.1	14.9	64.6	4.5	1,300,000
30+	12.7	13.9	9.8	63.5	672,000

Panel B: Quintile of tract median income

	Year $t+8$					
Year t	1	2	3	4	5	Individuals
1	71.8	10.4	8.2	6.1	3.5	2,070,000
2	7.8	70.3	9.2	7.7	5.0	2,610,000
3	5.1	7.5	70.6	9.5	7.3	2,780,000
4	3.2	5.3	7.8	72.5	11.2	2,820,000
5	1.8	3.1	5.2	9.1	80.9	2,950,000

Notes: This table reports the share of people who start in each type of neighborhood in year t (the year they are sampled in the ACS) that end up in each type of neighborhood in year $t+8$. We classify census tracts based on 2005–2009 poverty rates in Panel A and population-weighted quintiles of the 2005–2009 median household income distribution in Panel B. This exercise uses the migration sample, as described in Section 3.2. The last column reports the rounded number of individuals in the sample who start in each type of neighborhood.

Source: Authors' calculations using data from the ACS and MAFARF.

Table 3: Longitudinal Exposure to High-Poverty Neighborhoods

	(1)	(2)	(3)	(4)	(5)	(6)
		Share by years in high poverty tract			Mean years in high poverty tract	Share staying in improving tract
	Individuals	≤5	6–9	10		
Overall	530,000	0.309	0.153	0.538	7.179	0.127
Age 25–34	92,000	0.502	0.19	0.309	5.496	0.070
Age 35–44	100,000	0.364	0.165	0.471	6.704	0.117
Age 45–54	120,000	0.264	0.145	0.590	7.570	0.142
Age 55–64	110,000	0.207	0.123	0.670	8.087	0.163
Age 65+	100,000	0.156	0.133	0.711	8.474	0.153
Black	180,000	0.29	0.173	0.537	7.328	0.093
Hispanic	130,000	0.27	0.141	0.589	7.516	0.145
White	190,000	0.353	0.138	0.509	6.812	0.149
No kids	320,000	0.275	0.143	0.582	7.473	0.137
Has kids	210,000	0.352	0.166	0.482	6.806	0.114
Has kids, under 40	100,000	0.437	0.187	0.376	6.063	0.088
Owner-occupant	300,000	0.217	0.124	0.659	7.983	0.173
Private renter	180,000	0.430	0.182	0.387	6.122	0.081
Voucher renter	20,000	0.355	0.203	0.442	6.734	0.077
Public housing	22,000	0.224	0.177	0.600	7.876	0.055
In labor force	330,000	0.350	0.156	0.494	6.836	0.124
Not in labor force	200,000	0.229	0.148	0.623	7.849	0.132
Below poverty line	120,000	0.303	0.183	0.514	7.199	0.095
Not below poverty line	410,000	0.311	0.143	0.546	7.173	0.137
CBSA population						
Under 250k or rural	150,000	0.266	0.142	0.592	7.548	0.139
250k – 1M	120,000	0.302	0.155	0.544	7.233	0.100
Over 1M	270,000	0.333	0.158	0.509	6.967	0.133
CBSA rent gap						
0 – 0.1	150,000	0.342	0.166	0.492	6.892	0.113
0.1 – 0.2	160,000	0.348	0.159	0.492	6.832	0.126
>0.2	77,000	0.241	0.135	0.623	7.775	0.133

Notes: This table provides summary statistics among individuals in the migration sample who are observed living in a high poverty neighborhood (where the 2005–2009 poverty rate exceeds 30 percent) in the year they are sampled in the ACS. Column 1 reports the number of observations. Columns 2–4 report the share of individuals who spend less than or equal to 5, 6 to 9, or 10 of the subsequent 10 years in a high-poverty neighborhood. Column 5 reports the mean number of years (out of the following 10) that individuals spend in a high-poverty neighborhood. Column 6 reports the share of individuals who spend 10 years in a tract that initially has a poverty rate above 30 percent but falls below 25 percent in the 2015-2019 ACS. Individual characteristics are taken from ACS responses, except for public housing and voucher use, which are drawn from HUD data. The CBSA rent gap variable is equal to the log of the mean rent in high-poverty tracts minus the log of mean rent in tracts with poverty between 20 and 30 percent. We restrict to individuals who were observed in the MAFARF in at least 8 of the 10 years following their ACS year, proportionally adjusting exposure of those observed in fewer than 10 years.

Source: Authors' calculations using data from the ACS, MAFARF, and HUD.

Table 4: Labor Force Transition Frequency by Baseline Neighborhood Type

	(1) Nonemployed to employed	(2) Nonemployed to nonemployed	(3) Employed to nonemployed	(4) Employed to employed
Panel A: Tract poverty rate				
0–10	0.072	0.262	0.097	0.569
10–20	0.073	0.267	0.108	0.552
20–30	0.081	0.284	0.115	0.520
30+	0.092	0.306	0.120	0.482
Panel B: Quintile of tract income				
1	0.084	0.293	0.117	0.507
2	0.073	0.268	0.109	0.550
3	0.070	0.258	0.103	0.569
4	0.070	0.254	0.099	0.578
5	0.076	0.274	0.095	0.556

Notes: This table reports the share of individuals in different neighborhood types who made each type of labor force transition over the eight-years following their ACS response. We define nonemployment as LEHD earnings below $4,000. Rows in Panel A correspond to the poverty rate of the individual's baseline census tract, classified using 2005–2009 ACS data. Rows in Panel B correspond to population-weighted quintiles of 2005–2009 tract median household income.
Source: Authors' calculations using data from the ACS, MAFARF, and LEHD.

Table 5: Regressions of Earnings Growth on Baseline Neighborhood Poverty

	(1)	(2)	(3)
Panel A: Eight-year change in earnings, dollars			
Poverty rate 0–10	5093	4069	797.7
	(196.2)	(191.1)	(169.3)
Poverty rate 10–20	1617	1533	-566.8
	(155.2)	(168.2)	(175.3)
Poverty rate 20–30	545.2	574.6	-505.5
	(170.2)	(178.0)	(175.6)
Intercept	2709	3343	5957
	(124.3)	(144.2)	(177.3)
Individuals	3,573,000	3,573,000	3,573,000
R-squared	0.0001	0.0012	0.0028
CBSA and year FE		X	X
Individual controls			X
Panel B: Eight-year change in earnings, arc percent			
Poverty rate 0–10	0.0254	0.0211	0.0156
	(0.0037)	(0.0037)	(0.0038)
Poverty rate 10–20	-0.0040	-0.0042	-0.0093
	(0.0038)	(0.0038)	(0.0038)
Poverty rate 20–30	-0.0100	-0.0085	-0.0114
	(0.0043)	(0.0043)	(0.0043)
Intercept	-0.0023	0.0001	0.0050
	(0.0036)	(0.0036)	(0.0036)
Individuals	3,573,000	3,573,000	3,573,000
R-squared	0.0002	0.0051	0.0126
CBSA and year FE		X	X
Individual controls			X

Notes: This table reports regressions in which the dependent variable is the change in individual earnings over the eight-year period that starts when an individual is sampled in the ACS, measured in dollars (Panel A) or the arc percent (Panel B). For people with zero earnings in both periods, the arc percent is set to zero. The key explanatory variables of interest are indicators for the poverty rate of the neighborhood where an individual is observed at the start of the eight-year period; the omitted indicator is for the poverty rate above 30 percent. Columns 2 and 3 include CBSA and year fixed effects. Column 3 includes fixed effects for individual race, ethnicity, age, and baseline education. Standard errors, clustered at the level of the baseline tract, are reported in parentheses.

Source: Authors' calculations using data from the ACS, LEHD, and MAF.

Table 6: Heterogeneity in the Effects of a Coworker Earnings Shock

	(1) Effect on earnings (HH)	(2) SE	(3) Effect on tract income	(4) SE	(5) Elast.	(6) Δ earnings (HH)	(7) Δ tract inc. earnings component	(8) Δ tract inc. non-earnings component	(9) Δ tract inc. total
All	0.0127	0.0007	0.0018	0.0004	0.140	0.0800	0.0112	0.0207	0.0319
Renter	0.0150	0.0015	0.0039	0.0009	0.262	0.1300	0.0340	0.0189	0.0529
Owner	0.0117	0.0008	0.0008	0.0004	0.072	0.0640	0.0046	0.0206	0.0252
Has children	0.0114	0.0009	0.0023	0.0005	0.204	0.0990	0.0202	0.0093	0.0295
No children	0.0143	0.0011	0.0013	0.0007	0.092	0.0505	0.0046	0.0310	0.0356
Age 25–34	0.0148	0.0013	0.0022	0.0008	0.149	0.1873	0.0280	0.0362	0.0642
Age 36–50	0.0112	0.0008	0.0018	0.0004	0.161	0.0294	0.0047	0.0120	0.0167
White	0.0117	0.0008	0.0010	0.0005	0.088	0.0766	0.0068	0.0171	0.0239
Black	0.0106	0.0025	0.0023	0.0018	0.213	0.0537	0.0115	0.0461	0.0576
Hispanic	0.0141	0.0018	0.0025	0.0010	0.179	0.0822	0.0147	0.0265	0.0412
Asian	0.0152	0.0022	0.0031	0.0014	0.202	0.1304	0.0264	0.0360	0.0624
CBSA pop.									
$<$1M	0.0117	0.0013	0.0012	0.0007	0.105	0.0736	0.0077	0.0245	0.0322
$\geq$1M	0.0129	0.0008	0.0020	0.0005	0.155	0.0823	0.0128	0.0190	0.0318
Tract pov.									
0–10%	0.0123	0.0009	0.0016	0.0005	0.131	0.0803	0.0105	−0.030	−0.020
10–20%	0.0141	0.0014	0.0016	0.0009	0.111	0.0793	0.0088	0.0838	0.0925
20+%	0.0129	0.0020	0.0037	0.0014	0.289	0.0790	0.0229	0.2080	0.2309

Notes: This table reports heterogeneity in the effects of coworker earnings shocks on household earnings and neighborhood choice across demographic and neighborhood subgroups, together with a decomposition of mean neighborhood upgrading. Estimates are based on a variant of equation (4) that pools periods into a pre-shock period, a short-run post period ($k \in [1,3]$) and a long-run post period ($k \in [4,8]$); the table reports the $k \in [4,8]$ coefficient throughout. Column 1 reports the pooled estimate of the coworker pay shock on log household LEHD earnings, with standard errors clustered at the firm level in column 2. Column 3 reports the analogous estimate for log tract median household income, with standard errors in column 4. Column 5 reports the earnings-to-tract-income elasticity, computed as the ratio of column 3 to column 1. Column 6 reports the mean change in log household earnings over the eight-year period following ACS sampling. Columns 7–9 decompose mean neighborhood upgrading into earnings-driven and non-earnings components. Column 7 reports the portion of mean tract income growth explained by earnings growth, computed as the elasticity (column 5) times mean earnings growth (column 6). Column 8 reports the residual portion of mean tract income growth not explained by earnings growth. Column 9 reports the mean change in log tract median income, which equals the sum of columns 7 and 8. Race categories "White" and "Black" refer to non-Hispanic White and non-Hispanic Black individuals, respectively. All estimates use the coworker earnings shock sample described in Section 6.2. Shock estimates are scaled to reflect the effect of a 10 percent year-over-year increase in coworker earnings.
Source: Authors' calculations using data from the ACS, MAFARF, and LEHD.

Online Appendix

A Conceptual Framework: Derivations

This appendix derives the results in Section 2. Write systematic utility as

$$V_{ij}^s \equiv \ln(y_i - r_j) + \big(\beta_g + \delta_g \ln y_i\big)\, q_j + a_{ij}, \tag{A.1}$$

so that the probability household i chooses neighborhood j is

$$P_j \equiv \Pr(j \mid y_i, a_i) = \frac{\exp\big(V_{ij}^s/\sigma_g\big)}{\sum_k \exp\big(V_{ik}^s/\sigma_g\big)}. \tag{A.2}$$

Throughout, expectations, variances, and covariances are taken across neighborhoods using the probability choice weights from (A.2) unless noted otherwise. That is, for any neighborhood-varying quantities x_j and z_j,

$$\mathbb{E}[x_j] \equiv \sum_j P_j\, x_j, \qquad \operatorname{Cov}(x_j, z_j) \equiv \sum_j P_j\, x_j z_j - \mathbb{E}[x_j]\,\mathbb{E}[z_j], \qquad \operatorname{Var}(x_j) \equiv \operatorname{Cov}(x_j, x_j), \tag{A.3}$$

with the weights P_j given by (A.2).

A.1 A useful identity

For any scalar θ that enters systematic utility, the response of expected neighborhood quality is the covariance, under the choice probabilities, between quality and the marginal effect of θ on utility,

$$\frac{\partial \bar{q}_i}{\partial \theta} = \frac{1}{\sigma_g} \operatorname{Cov}\!\left(q_j,\, \frac{\partial V_{ij}^s}{\partial \theta}\right). \tag{A.4}$$

To see this, differentiate the logit probability (A.2),

$$\frac{\partial P_j}{\partial \theta} = \frac{P_j}{\sigma_g}\left(\frac{\partial V_j^s}{\partial \theta} - \sum_k P_k \frac{\partial V_k^s}{\partial \theta}\right), \tag{A.5}$$

and substitute into $\partial \bar{q}_i/\partial \theta = \sum_j q_j\, \partial P_j/\partial \theta$, using that q_j is fixed:

$$\frac{\partial \bar{q}_i}{\partial \theta} = \frac{1}{\sigma_g}\left[\mathbb{E}\Big[q_j \tfrac{\partial V_j^s}{\partial \theta}\Big] - \mathbb{E}[q_j]\,\mathbb{E}\Big[\tfrac{\partial V_j^s}{\partial \theta}\Big]\right] = \frac{1}{\sigma_g}\operatorname{Cov}\Big(q_j,\, \tfrac{\partial V_j^s}{\partial \theta}\Big). \tag{A.6}$$

We apply (A.4) repeatedly below.

A.2 Earnings mobility

Differentiating V_j^s in (A.1) with respect to $\ln y_i$ gives

$$\frac{\partial V_j^s}{\partial \ln y_i} = \frac{y_i}{y_i - r_j} + \delta_g q_j. \tag{A.7}$$

Substituting into the result (A.4) and using linearity of covariance,

$$\frac{\partial \bar{q}_i}{\partial \ln y_i} = \frac{1}{\sigma_g}\left[\delta_g \operatorname{Var}(q_j) + \operatorname{Cov}\left(q_j, \frac{y_i}{y_i - r_j}\right)\right]. \tag{A.8}$$

Since $\delta_g > 0$, the first term is positive. The second is also positive: $y_i/(y_i - r_j)$ is increasing in r_j, and r_j is increasing in q_j, so $y_i/(y_i - r_j)$ is increasing in q_j, and the covariance of q_j with any increasing function of q_j is positive whenever quality varies. Hence $\partial \bar{q}_i / \partial \ln y_i > 0$.

Higher income leads households to leave poor neighborhoods. Let ℓ index households' initial neighborhood. From (A.5) with $\theta = \ln y$, the effect of a change in income on living in one's initial neighborhood is:

$$\frac{\partial \Pr(\ell \mid \cdot)}{\partial \ln y} = \frac{P_\ell}{\sigma_g}\left(\frac{\partial V_\ell^s}{\partial \ln y} - \mathbb{E}\left[\frac{\partial V_j^s}{\partial \ln y}\right]\right), \tag{A.9}$$

which is negative when $\partial V_\ell^s / \partial \ln y = y/(y - r_\ell) + \delta_g q_\ell$ lies below its choice-weighted average—that is, when the initial neighborhood ℓ is a low-rent, low-quality neighborhood. Higher income then lowers the probability of residing in such a neighborhood, the comparative-statics counterpart of the selective migration discussed in the main text.

A.3 Non-earnings mobility

The result (A.4) gives $\partial \bar{q}_i / \partial a_{ik} = \frac{1}{\sigma_g} P_k(q_k - \bar{q}_i)$, and summing over a perturbation da_i,

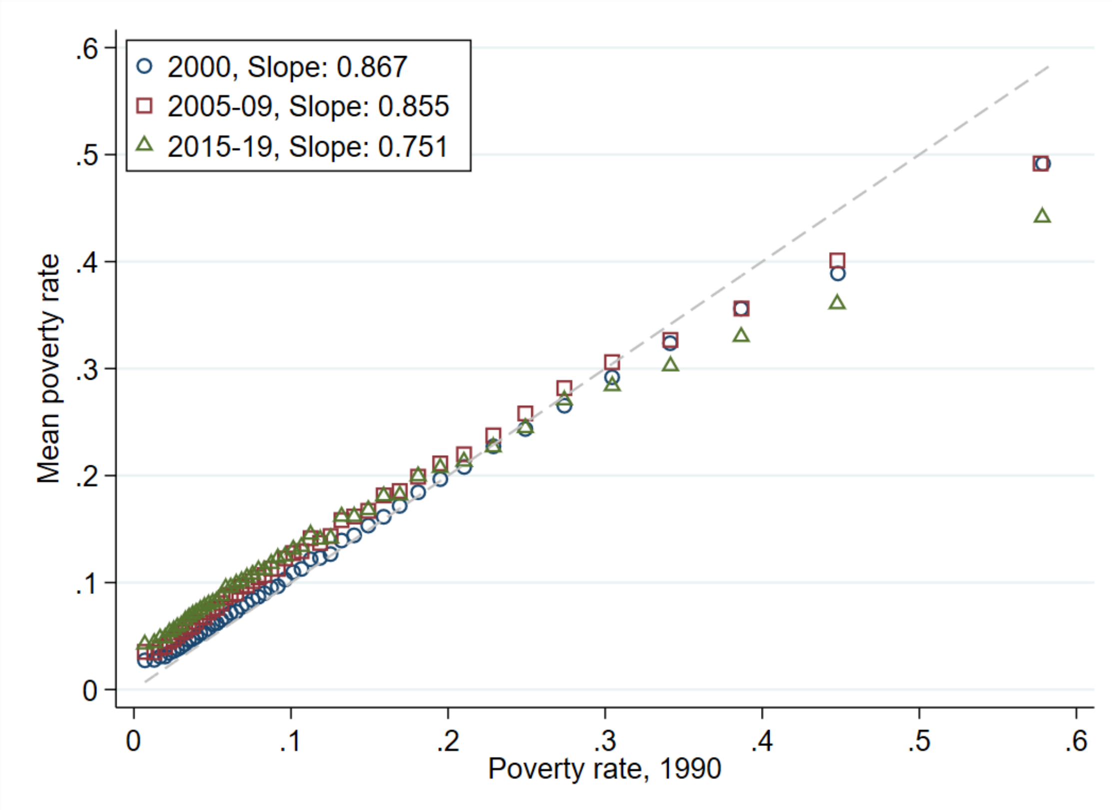
$$\sum_k \frac{\partial \bar{q}_i}{\partial a_{ik}} \, da_{ik} = \frac{1}{\sigma_g} \sum_k P_k(q_k - \bar{q}_i) \, da_{ik} = \frac{1}{\sigma_g} \operatorname{Cov}\big(q_j, da_{ij}\big). \tag{A.10}$$

Combining this with the earnings response (A.8) gives the total change in expected neighborhood quality,

$$d\bar{q}_i = \frac{\partial \bar{q}_i}{\partial \ln y_i} \, d \ln y_i + \frac{1}{\sigma_g} \operatorname{Cov}\big(q_j, da_{ij}\big), \tag{A.11}$$

which is equation (2) in the main text.

Figure A.1: Persistence of Neighborhood Poverty Rates Between 1990 and 2019

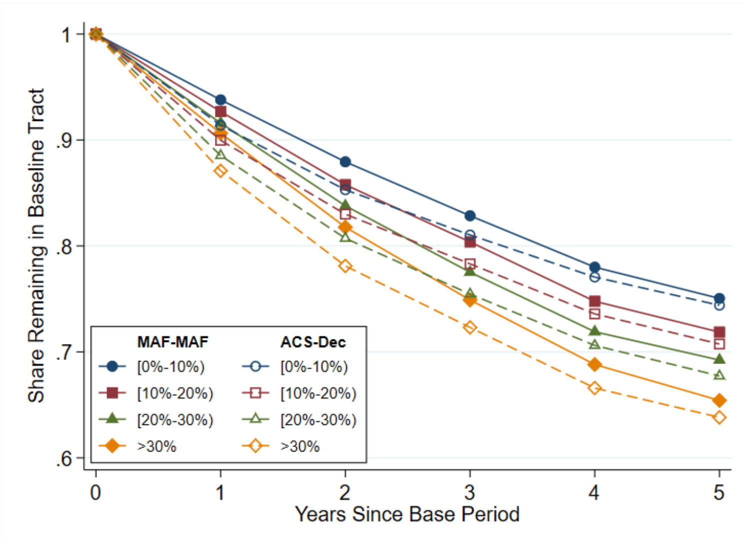


Notes: This figure shows average poverty rates in 2000, 2005–2009, and 2015–2019 against average poverty rates in 1990 for tracts grouped into 50 quantiles of the 1990 poverty rate distribution. The figure also shows a 45-degree line for reference.

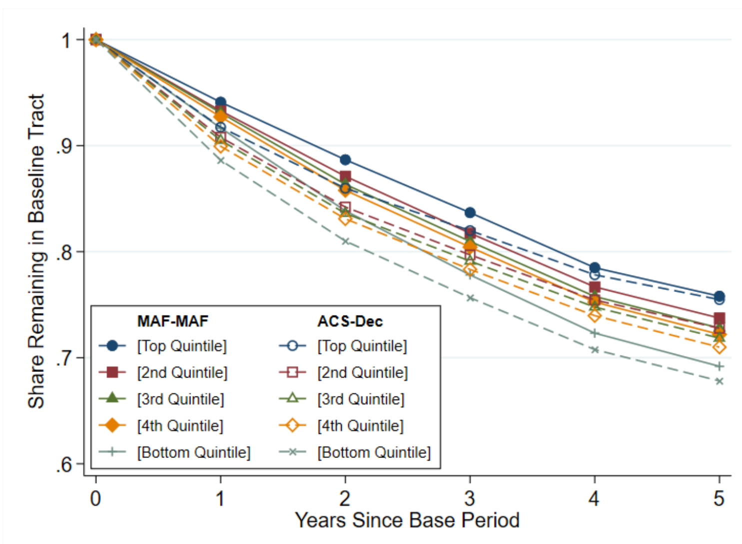
Source: Authors' calculations using publicly available Census and ACS data.

Figure A.2: Share of Individuals Remaining in Baseline Tract (MAFARF Validation)

(a) By Baseline Tract Poverty Rate Category



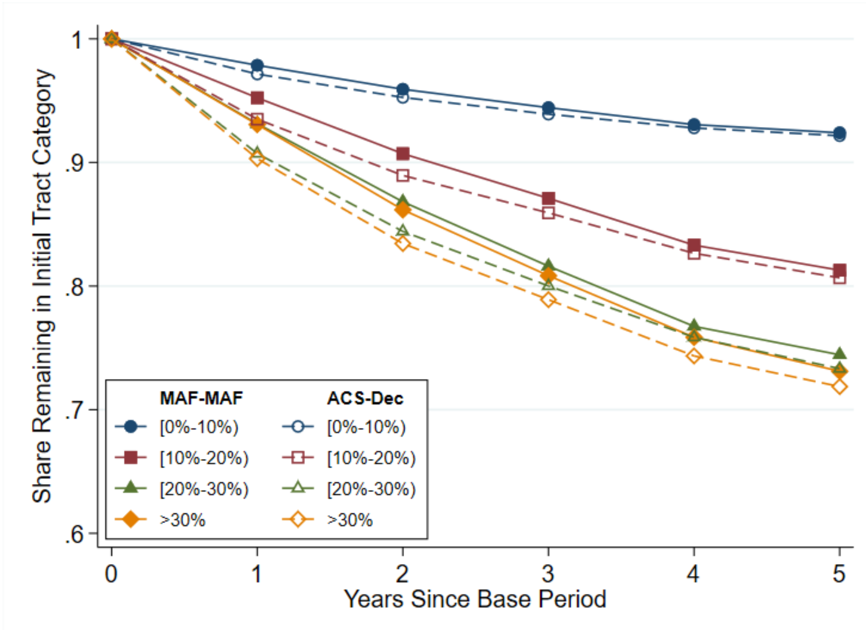
(b) By Baseline Tract Income Quintile



Notes: This figure recreates Figure 1 comparing tract locations drawn from the MAFARF to those drawn from 2005–2009 ACS surveys at baseline and the 2010 decennial census at endline. This figure shows the share of individuals who live in their baseline census tract for 1 to 5 years after the baseline year (the year an individual is sampled in the ACS). Individuals are stratified based on the baseline tract's poverty rate (Panel A) or quintile of median household income (Panel B). The solid lines correspond to the census tract measured by the MAFARF at both the baseline and endline (2010). The dashed lines correspond to the census tract being measured by the ACS at baseline and the 2010 decennial census at endline. Unlike Figure 1, each dot represents a different cohort of individuals; i.e., 1 year since baseline consists of those who responded to the ACS in 2009, 2 years since baseline consists of those who responded to the ACS in 2008, and so on.

Source: Authors' calculations using data from the ACS, MAFARF, and the 2010 decennial census.

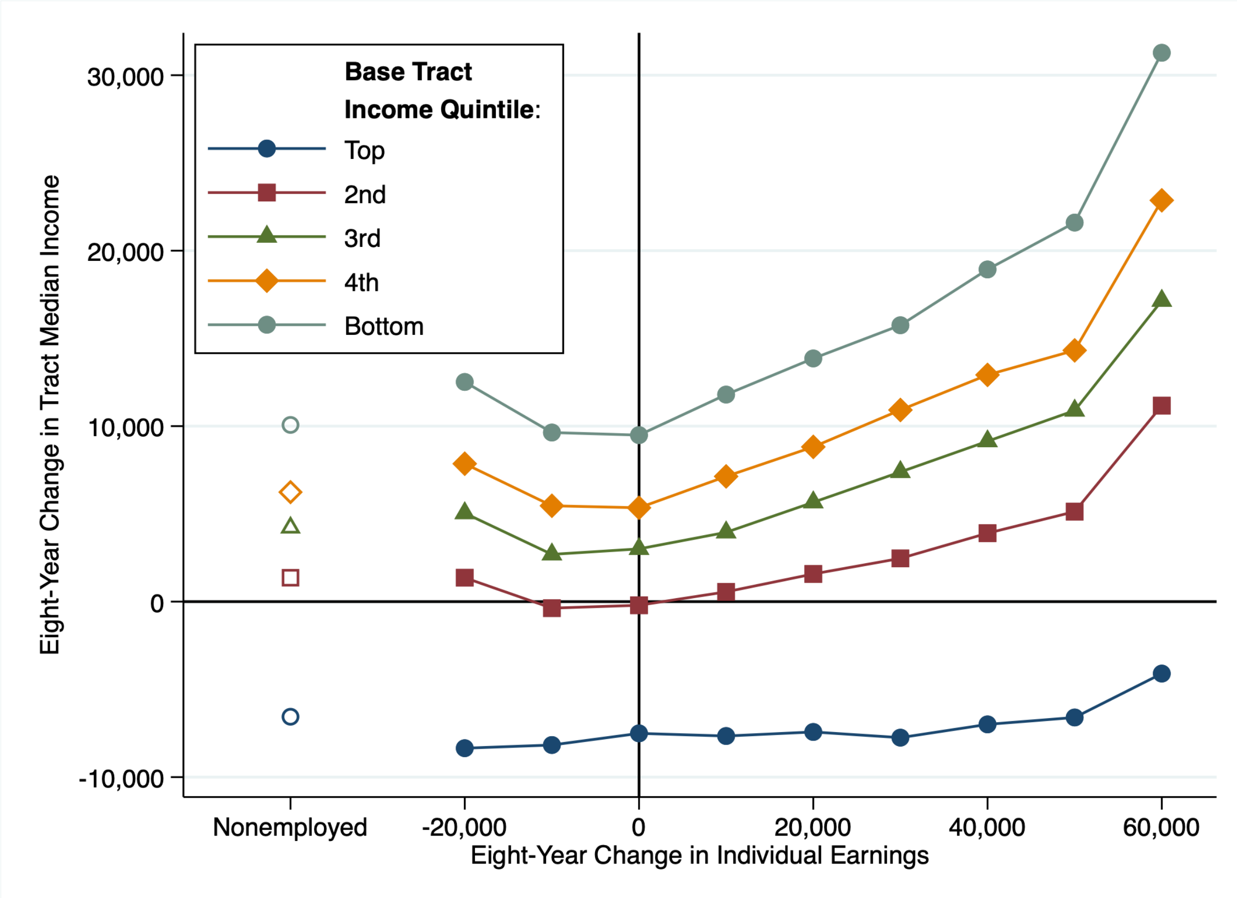
Figure A.3: Share of Individuals Remaining in Baseline Tract Category (MAFARF Validation)



Notes: This figure validates the diagonal of Panel A of Tables 2 and A.2, comparing the share of individuals remaining in their baseline tract poverty rate category as measured by the MAFARF to that measured using the 2005–2009 ACS at baseline and the 2010 decennial census at endline. This figure shows the share of individuals who remain in the same tract poverty rate category for 1 to 5 years after the baseline year (the year an individual is sampled in the ACS). Individuals are stratified by the baseline tract's poverty rate category, as defined in Table 2. The solid lines correspond to the tract poverty category being measured from the MAFARF at both baseline and endline (2010). The dashed lines correspond to the tract poverty category being measured by the ACS at baseline and the 2010 decennial census at endline. As in Figure A.2, each dot represents a different cohort of individuals; i.e., 1 year since baseline consists of those who responded to the ACS in 2009, 2 years since baseline consists of those who responded to the ACS in 2008, and so on.

Source: Authors' calculations using data from the ACS, MAFARF, and the 2010 decennial census.

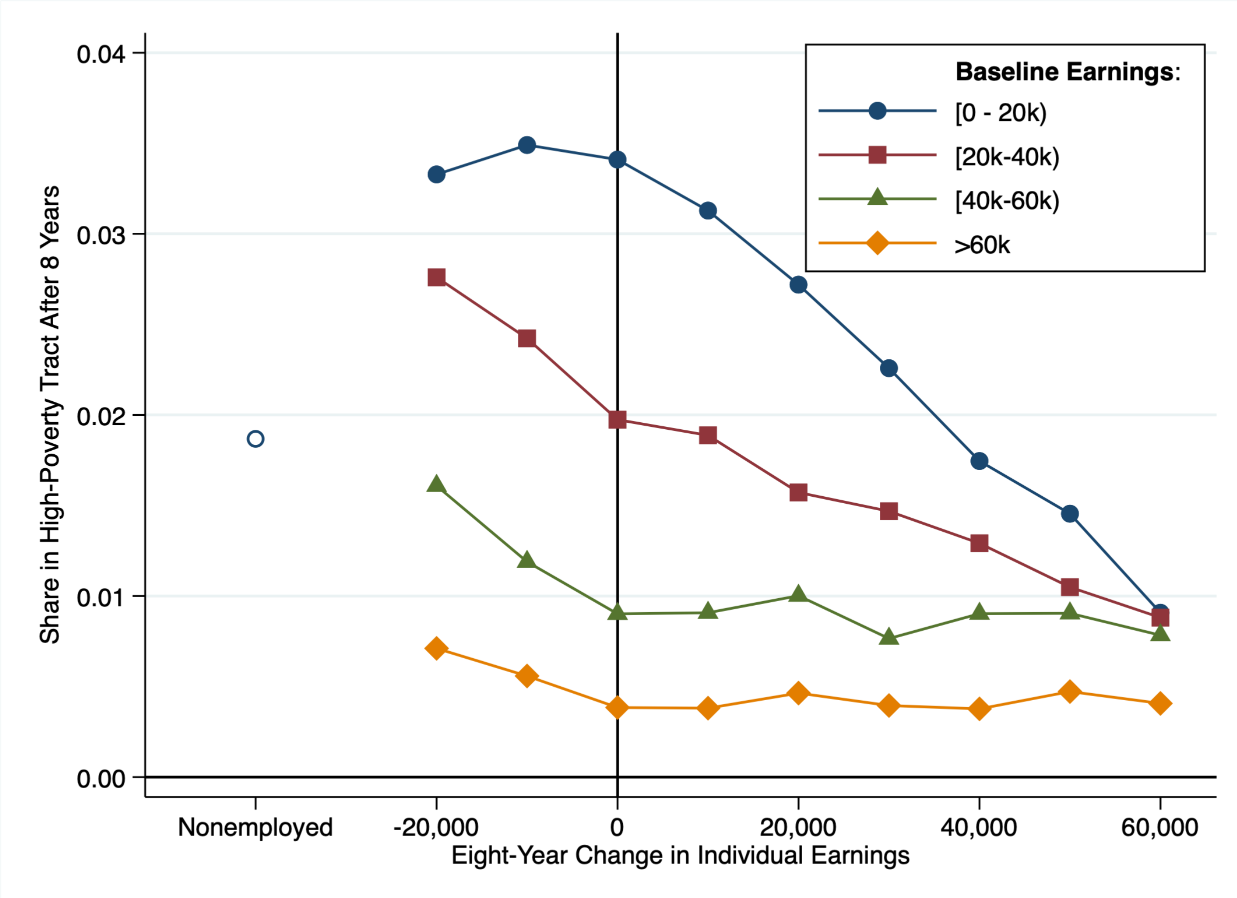
Figure A.4: Tract Mobility and Earnings Mobility, By Initial Neighborhood Income Quintile



Notes: Figure replicates Panel A of Figure 3 stratifying individuals by quintiles of the median household income of their baseline tract instead of by the poverty rate. See note to Figure 3 for further details.

Source: Authors' calculations using data from the ACS, MAFARF, and LEHD.

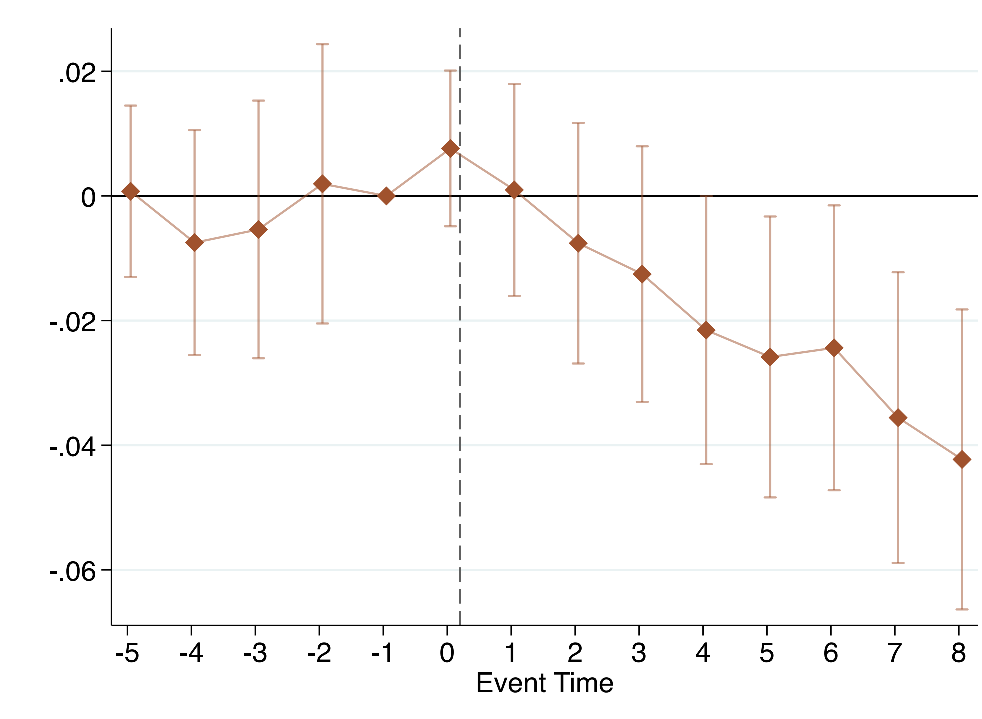
Figure A.5: Entry Into High Poverty Neighborhoods, By Baseline Individual Earnings



Notes: Figure replicates Panel C of Figure 3 stratifying individuals initially living outside of high-poverty neighborhoods (those with baseline poverty rates below 30 percent) by their own individual baseline earnings instead of by their initial neighborhood characteristics. See note to Figure 3 for further details.

Source: Authors' calculations using data from the ACS, MAFARF, and LEHD.

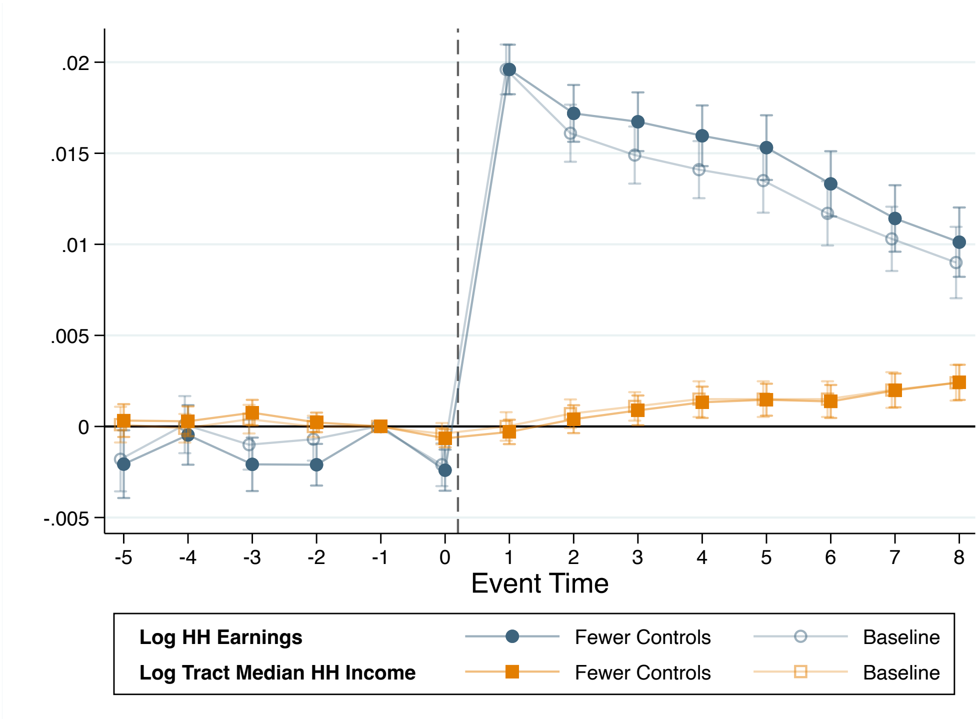
Figure A.6: Effect of a Coworker Earnings Shock on Neighborhood Poverty Rate



Notes: This figure displays OLS estimates of equation (4) with 95 percent confidence intervals. The figure uses a subset of the earnings sample consisting of individuals working at firms with at least 25 employees in the holdout sample, as described in Section 6.2. Effects are scaled so that coefficients can be interpreted as the effect of a 10 percent increase in coworker quarterly earnings over a four-quarter period that begins when an individual is sampled in the ACS. The outcome is the poverty rate measured in percentage points (0–100 scale) in the tract where individuals reside in each year. Standard errors are clustered at the firm level.

Source: Authors' calculations using data from the ACS, MAFARF, and LEHD.

Figure A.7: Effect of a Coworker Earnings Shock: Fewer Controls



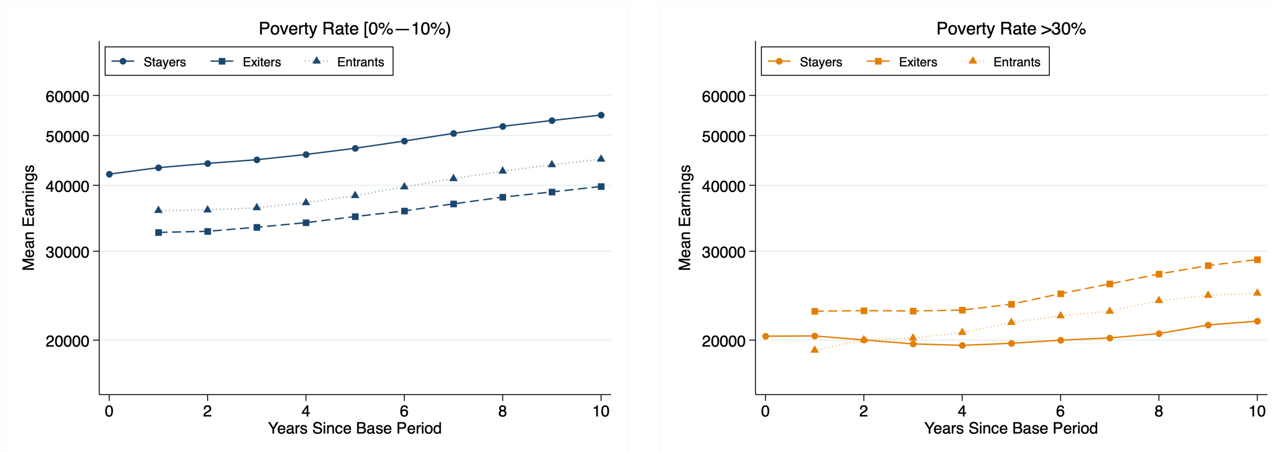
Notes: This figure replicates the event-study estimates of the effect of coworker earning shocks on log household earnings and log tract median household income in Figure 5, both under the original specification (lighter shades, hollow markers) and under a specification with fewer controls (darker shades, filled markers). The "Fewer Controls" specification limits controls to only industry, initial wage, initial tract income, and age. In contrast to the baseline specification, the "Fewer Controls" specification excludes state and CBSA fixed effects as well as firm-level controls. The exact regression for the "Fewer Controls" specification is

$$Y_{i,t} = \mu_i + \sum_{k \in \mathcal{E}} \beta_k \text{PayShock}_i \times \mathbb{1}[t = a(i) + k] + \theta_{t,a(i)} + \omega_{t,a(i),n(i)} + \sum_{k \in \mathcal{E}} X_i' \alpha_{a(i),k} \times \mathbb{1}[t = a(i) + k] + \epsilon_{i,t,k},$$

where $a(i)$ refers to the year that individual i first appears in the ACS. We control for common trends among individuals sampled in the same ACS year over time using the fixed effects $\theta_{t,a(i)}$, which capture the interaction between the ACS year of observation ($a(i)$) and the outcome year (t). The fixed effects $\omega_{t,a(i),n(i)}$ allow trends in outcomes to vary arbitrarily based on individuals' baseline 2-digit NAICS industry codes ($n(i)$) and ACS year of observation ($a(i)$). X_i is a limited vector of individual controls, which are interacted with relative time dummies and allowed to vary across cohorts of individuals sampled in the ACS in different years $a(i)$. These controls are indicator variables for quintiles of initial hourly wage, initial tract income, and age.

Source: Authors' calculations using data from the ACS, MAFARF, and LEHD.

Figure A.8: Earnings Growth Among Stayers, Exiters, and Entrants, by Tract Poverty Level



Notes: This figure shows the mean earnings in each year for stayers, entrants, and exiters from different types of tracts. These categories are determined relative to individuals' neighborhood type in the baseline year. For example, entrants to high-poverty neighborhoods in year six are defined as people who lived in this neighborhood type in year six but not year zero. As in Figure 6, the exercise uses the earnings sample and further restricts to the 1965 to 1980 birth cohorts (to minimize the role of retirement and sample entry). The y-axis is on a log scale.

Source: Authors' calculations using data from the ACS, MAFARF, and LEHD.

Table A.1: Summary Statistics

	(1) Migration sample	(2) Earnings sample	(3) Coworker earnings shock sample
Demographics			
Age	51.41	38.90	38.41
Female	0.525	0.516	0.481
White	0.796	0.757	0.726
Black	0.102	0.088	0.110
Asian	0.047	0.072	0.082
Hispanic	0.111	0.145	0.148
Education			
Less than high school	0.131	0.093	0.061
High school	0.288	0.260	0.221
Some college	0.282	0.296	0.293
College or more	0.299	0.351	0.424
Household			
Child under 18 present	0.374	0.622	0.585
Renter	0.248	0.312	0.314
Individual in poverty	0.088	0.083	0.023
Labor force status			
Employed	0.636	0.827	0.976
Unemployed	0.037	0.045	0.013
Not in labor force	0.327	0.128	0.012
Income (2019 dollars)			
Individual wage earnings	$37,940	$50,520	$65,730
Individual total income	$51,080	$56,180	$67,470
Household total income	$97,360	$108,700	$114,800
Baseline tract poverty rate			
0–10%	0.549	0.600	0.646
10–20%	0.288	0.263	0.231
20–30%	0.106	0.091	0.081
30+%	0.057	0.046	0.041

Notes: This table reports means of individual and household characteristics across the three samples used in the paper: the migration sample, the earnings sample, and the coworker earnings shock sample. The construction of these samples is described in Sections 3.2 and 6.2. All variables correspond to the baseline year (when people are sampled in the ACS). Income variables are drawn from the ACS and deflated to 2019 dollars using the CPI. Tract poverty rates are calculated from the 2005–2009 ACS.
Source: Authors' calculations using data from the ACS and MAFARF.

Table A.2: One-Year Transition Rates Across Neighborhood Types

Panel A: Neighborhood poverty rate

	Year $t+1$			
Year t	0–10	10–20	20–30	30+
0–10	97.2	2	0.5	0.2
10–20	4.2	94	1.2	0.6
20–30	3.5	3.7	91.5	1.3
30+	2.7	3.4	2.7	91.2

Panel B: Quintile of neighborhood median income

	Year $t+1$				
Year t	1	2	3	4	5
1	93.5	2.6	1.9	1.3	0.7
2	2.1	93.1	2.1	1.7	1
3	1.3	1.8	93.3	2.1	1.4
4	0.8	1.2	1.8	94	2.2
5	0.4	0.7	1.1	1.9	96

Notes: Table reports the share of people who start in each type of neighborhood in year t (the year they are sampled in the ACS) that end up in each type of neighborhood in year $t+1$. We classify census tracts based on 2005–2009 poverty rates in Panel A and population-weighted quintiles of the 2005–2009 median household income distribution in Panel B. This exercise uses the migration sample, as described in Section 3.2.

Source: Authors' calculations using data from the ACS and MAFARF.

Table A.3: Regression of Migration across Income Quintiles on Baseline Neighborhood Income

	(1)	(2)	(3)
Quintile 2	0.015	0.011	0.013
	(0.002)	(0.001)	(0.001)
Quintile 3	0.012	-0.011	-0.007
	(0.001)	(0.001)	(0.001)
Quintile 4	-0.007	-0.048	-0.041
	(0.001)	(0.001)	(0.001)
Quintile 5	-0.091	-0.143	-0.129
	(0.001)	(0.001)	(0.001)
Intercept	0.282	0.309	0.303
	(0.001)	(0.001)	(0.001)
Individuals	13,230,000	13,230,000	13,230,000
R-squared	0.009	0.024	0.071
CBSA and year FE		X	X
Individual controls			X

Notes: This table reports regressions in which the dependent variable is an indicator for whether the person moves over the eight-year period that starts when an individual is sampled in the ACS to a neighborhood in a different population-weighted quintile of 2005–2009 median household income. The key explanatory variables of interest are indicators for the population-weighted quintile of 2005–2009 median household income of the neighborhood where an individual is observed at the start of the eight-year period; the omitted indicator is for the first quintile. Columns 2 and 3 include CBSA and year fixed effects. Column 3 also includes fixed effects for individual race, ethnicity, age, and baseline education. Standard errors, clustered at the level of the baseline tract, are reported in parentheses.

Source: Authors' calculations using data from the ACS, LEHD, and MAFARF.

Table A.4: Characteristics of Entrants, Exiters, and Stayers in High-Poverty Neighborhoods

	(1) Entrants	(2) Exiters	(3) Stayers	(4) Rest of sample
Mean age	42.76	42.69	51.66	51.85
Share under 30	0.197	0.194	0.081	0.066
Share white	0.519	0.492	0.464	0.818
Share with no college	0.578	0.574	0.652	0.402
Share married family	0.359	0.403	0.439	0.657
Share with child under 18	0.468	0.503	0.399	0.372
Share renter	0.725	0.679	0.478	0.227
Share not in labor force	0.299	0.283	0.404	0.320
Share unemployed	0.098	0.084	0.061	0.035
Share in poverty	0.300	0.260	0.264	0.075
Mean individual earnings ($)	23,150	24,880	19,920	39,420
Mean household income ($)	53,450	57,990	52,090	100,800
Share in high-poverty in $t + 10$	0.371	0.156	0.647	0.015

Notes: This table reports the mean characteristics of four groups, defined by whether they lived in a high-poverty neighborhood in each of two consecutive years. The baseline year is the year an individual was sampled in the ACS, and the follow-up period is the next year. Entrants did not live in a high-poverty tract (defined as a rate over 30 percent) in the baseline year but did in the following year. Exiters made the opposite move, while Stayers lived in a high-poverty neighborhood in both years. Column 4 consists of all remaining individuals in the migration sample. All characteristics are drawn from ACS responses, except the share in a high-poverty tract 10 years after the ACS year, which is taken from the MAFARF.

Source: Authors' calculations using data from the ACS and MAFARF.

Table A.5: Regressions of Earnings Growth on Baseline Neighborhood Poverty (Higher Labor Force Attachment Sample)

	(1)	(2)	(3)
Panel A: Eight-year change in earnings, dollars			
Poverty rate 0–10	4143	3425	1147
	(136.3)	(132.3)	(129.5)
Poverty rate 10–20	2203	2015	473
	(144.2)	(138.8)	(130.4)
Poverty rate 20–30	851.5	694.1	-68.83
	(160.9)	(154.0)	(142.3)
Intercept	5726	6234	8110
	(125.1)	(123.5)	(121.8)
Individuals	1,674,000	1,674,000	1,674,000
R-squared	0.0015	0.0185	0.059
CBSA and year FE		X	X
Individual controls			X
Panel B: Eight-year change in earnings, arc percent			
Poverty rate 0–10	-0.0025	-0.0061	-0.006
	(0.0028)	(0.0028)	(0.0029)
Poverty rate 10–20	-0.0014	-0.0034	-0.0059
	(0.0030)	(0.0029)	(0.0029)
Poverty rate 20–30	-0.0038	-0.0048	-0.0065
	(0.0034)	(0.0033)	(0.0032)
Intercept	0.1040	0.1069	0.1076
	(0.0028)	(0.0028)	(0.0028)
Individuals	1,674,000	1,674,000	1,674,000
R-squared	0	0.0135	0.0401
CBSA and year FE		X	X
Individual controls			X

Notes: This table reports results from estimating the regressions in Table 5 on a restricted sample of individuals who report working more than 1042 hours in the ACS (corresponding to approximately half-time employment). See note to Table 5 for further details.

Source: Authors' calculations using data from the ACS, LEHD, and MAF.

Table A.6: Effects of Coworker Earnings Shocks Among Individuals in High-Poverty Tracts

	(1) Effect on earnings (HH)	(2) SE	(3) Effect on tract outcome	(4) SE	(5) Δ earnings (HH)	(6) Δ tract out. earnings component	(7) Δ tract out. non-earnings component	(8) Δ tract outcome total
Panel A: Tract outcome: Log tract median income								
Pov. rate 20+%	0.0129	0.0020	0.0037	0.0014	0.0790	0.0229	0.2080	0.2309
Renter	0.0167	0.0032	0.0062	0.0023	0.1205	0.0445	0.2367	0.2812
Owner-occupant	0.0085	0.0025	0.0011	0.0018	0.0471	0.0059	0.1864	0.1923
Has children	0.0104	0.0026	0.0045	0.0018	0.0954	0.0415	0.1645	0.2060
No children	0.0158	0.0032	0.0021	0.0024	0.0558	0.0074	0.2588	0.2662
Panel B: Tract outcome: Tract poverty rate								
Pov. rate 20+%	0.0129	0.0020	−0.0916	0.0448	0.0790	−0.5593	−7.2607	−7.82
Renter	0.0167	0.0032	−0.1482	0.0713	0.1205	−1.0700	−8.6030	−9.67
Owner-occupant	0.0085	0.0025	−0.0368	0.0565	0.0471	−0.2030	−6.1940	−6.40
Has children	0.0104	0.0026	−0.1202	0.0584	0.0954	−1.1053	−6.1227	−7.23
No children	0.0158	0.0032	−0.0329	0.0707	0.0558	−0.1163	−8.5437	−8.66

Notes: This table reports heterogeneity in the effects of coworker earnings shocks on household earnings and neighborhood choice among individuals residing in tracts with a poverty rate above 20 percent. Estimates are based on a variant of equation (4) that pools periods into a pre-shock period, a short-run post period ($k \in [1, 3]$), and a long-run post period ($k \in [4, 8]$); the table reports the $k \in [4, 8]$ coefficient throughout. Column 1 reports the pooled estimate of the coworker pay shock on log household LEHD earnings, with standard errors clustered at the firm level in column 2. Column 3 reports the analogous estimate for the neighborhood outcome, with standard errors in column 4; in Panel A the outcome is log tract median household income, and in Panel B it is the tract poverty rate in percentage points (0–100 scale). Column 5 reports the mean change in log household earnings over the eight-year period following ACS sampling. Columns 6–8 decompose mean neighborhood change into earnings-driven and non-earnings components. Column 6 reports the portion of mean neighborhood change explained by earnings growth, computed as the ratio of column 3 to column 1 times mean earnings growth (column 5). Column 7 reports the residual portion not explained by earnings growth. Column 8 reports the mean change in the neighborhood outcome, which equals the sum of columns 6 and 7. All estimates use the coworker earnings shock sample described in Section 6.2. Shock estimates are scaled to reflect the effect of a 10 percent year-over-year increase in coworker earnings.
Source: Authors' calculations using data from the ACS, MAFARF, and LEHD.