How do neighborhoods and firms affect intergenerational mobility?

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ABSTRACT

We use data from the Longitudinal Employer Household Dynamics linked to the 2000 Census to study intergenerational earnings mobility in the United States. We augment the standard intergenerational transmission model relating children's log earnings to those of their parent with an additional term representing mean log parent earnings in the childhood neighborhood. The between-neighborhood intergenerational relationship is twice as strong as the within-neighborhood relationship, even after adjusting for measurement error in parents' earnings. Moreover, mean earnings of the parents in a neighborhood capture over 80% of the variation in unrestricted neighborhood effects that reflect differences in "absolute mobility". Next, we use an AKM framework to decompose parents', children's, and neighboring parents' earnings into person effects and establishment premiums. Children's person effects are mainly influenced by parents' and neighbors' person effects, whereas children's establishment premiums are mainly influenced by parents' and neighbors' establishment premiums. These patterns point to separate channels for human capital and access to jobs in the intergenerational transmission process. Finally, we explore the implications for the Black-white earnings gap. Neighborhoods explain 30% of the Black-white gap in children's earnings conditional on parents' earnings, operating largely through gaps in average person effects. Conditional on neighborhood average earnings, children from neighborhoods with higher Black shares achieve *higher* adult earnings.

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I. Introduction

A vast literature inspired by Galton (1886) shows that higher-earning parents have higher-earning children.[1] A related body of work shows that the neighborhoods where children grow up also matter (e.g., Wilson, 1987; Case and Katz, 1991; Sampson et al., 2002; Chetty and Hendren, 2018a,b; Chetty et al., 2026). The correlation between parents and children is widely attributed to a combination of correlated endowments and investment by higher-earning parents in the human capital of their children (e.g., Becker and Tomes, 1979). Neighborhoods may also raise children's earning capacity through local resources and peer effects in learning (e.g., Agrawal et al., 2019; Rothstein, 2019). But human capital is not the only plausible channel mediating the effects of parents and neighborhoods. They could also affect the probability of holding higher- or lower-paying jobs, conditional on human capital, through referral networks, proximity to high-wage industries or firms, or other mechanisms.

In this paper we present new evidence on the impacts of families and neighborhoods on children's wages, using data for families in the 2000 Census linked to earnings in the Longitudinal Employer-Household Dynamics (LEHD) database. We begin with a simple extension of the baseline Galton-style intergenerational correlation model that allows children's earnings, conditional on work, to depend on both their parents' wages and the average wages of other parents in their neighborhood. This greatly simplifies the models proposed by Chetty and associates (e.g., Chetty et al., 2014, 2026), which include unrestricted place effects, or even unrestricted intercepts and slopes for different places. Nevertheless, mean parental wages summarize all aspects of a neighborhood that are relevant to the magnitude of the parent-child intergenerational elasticity (IGE) in a Galton-style regression (Mundlak, 1978). Moreover, differences in average parental wages explain a surprisingly large

[1] See the reviews by Black and Devereux (2011), Deutscher and Mazumder (2023), and Mogstad and Torsvik (2023). Galton's (1886) paper studies the intergenerational correlation of heights, but Galton (1901) applied the same idea to the problem of social classes.

share of the variation captured by unrestricted neighborhood effects in a model for children's wages.[2]

We then extend this model by decomposing the wages of parents, neighbors, and children into person-specific components that are portable across jobs and pay premiums that vary across workplaces, as in the canonical "AKM" model (Abowd et al., 1999). We estimate the links between the constituent parts of children's, parents' and neighboring parents' wages and compare the impacts of the person-specific components of parent and neighboring parents' wages to the impacts of their respective pay premiums.[3] Finally, we use our extended IGE model to examine the sources of Black-white differences in children's wages, including parental and neighborhood wages or the AKM-based components of these wages.

We have four main findings. First, controlling for parental wages, we find that differences in neighboring parent wages have a large effect on children's outcomes. In a model for children's log wages (measured at ages 27-40) that includes the mean log wage of the highest-earning parent when the child was in their teens/early 20's and the average wage of all parents in the same Census tract, the effects of these two variables are similar in size and both equal to about 0.25.[4] For comparison, when we include just own-parent wages (as in a simpler IGE model) the coefficient is 0.30. The estimates are not much affected by instrumenting own-parent wages with average wages in a later interval, or including controls for single versus dual parent families or neighborhood characteristics such as the share of Black families. When we add information on mean parental wages in a wider neighborhood (made up of Census tracts around the index tract) we find that nearly all of the intergenerational effect comes from the parents in a child's own tract. We also find that the mean wages of the parents in a tract

[2] Mean earnings of full time/full year workers (which are similar to our wage measure) are highly correlated with neighborhood poverty rates, the target of mobility conditions in the Moving to Opportunity experiment (Chetty, Hendren, and Katz 2016).

[3] Shea (2000) uses PSID data to analyze the intergenerational effects of the components of parental earnings attributable to union status and industry, which are arguably correlated with the firm pay premiums earned by parents. He finds relatively small (but imprecisely estimated) effects. See also Mayer (1998).

[4] In this specification we instrument parents' wages with an average from a non-overlapping interval to remove bias from measurement error. In an OLS model, the neighborhood mean wage effect is larger and the own parent's wage effect is smaller.

explain about 85% of the variance of the tract fixed effects from a model with own-parent wages and unrestricted census tract effects.

Second, when we use an AKM model to decompose parents' and neighbors' wages, we find that the person effects have larger impacts on children's wages than the pay premiums, with coefficients of around 0.25 for both the own-parent and neighboring-parent person effects, versus 0.10 for the corresponding pay premiums.[5] We take this as evidence that the person effects of parents and neighbors are more strongly correlated with underlying family and neighborhood characteristics that affect children's wages than are the pay premium components. Nevertheless, the pay premiums of both parents and neighbors have highly significant effects on children's subsequent earnings.

Third, when we apply the AKM decomposition to children's earnings as well, we find that the person effects of parents and neighbors mainly affect children's person effects, while the pay premiums of parents and neighboring parents mainly affect children's pay premiums. In other words, the intergenerational linkages are roughly block-diagonal. Moreover, within both the "person effects block" and the "pay premiums block" the effects of the own-parent and neighboring parent wage components are very similar in magnitude, paralleling our findings with respect to overall wages. Looking further into the industry component of the pay premiums earned by parents and neighboring parents (Card et al., 2024a), however, we find an interesting exception to block-diagonality: when parents or neighbors work in high-paying industries, children end up in higher-paying industries but with lower person effects, perhaps reflecting a diversion from school in places where good jobs are readily available (as in Black et al., 2005).

Fourth, when we use our setup to examine racial differences in earnings, we find (like Chetty et al., 2020) that Black and white children have similar returns to higher parental earnings (i.e., similar measures of "relative mobility"), but that Black children have lower wages at every level of parental earnings (i.e., a lower rate of "absolute mobility"). Part of the

[5] To address measurement error in the estimated AKM components we instrument the estimated components for a parent from one 5-year interval with the estimated components from a later, non-overlapping interval.

explanation is neighborhoods. Holding constant parental earnings, the average earnings of the neighboring parents of a typical Black child are substantially lower than those of a typical White child.[6] This gap explains one-third of the racial gap in children's earnings that remains after controlling for own-parent earnings, suggesting that the differential sorting of Black and White families to neighborhoods remains an important driver of racial earnings differences even into the 21st century. When we use our AKM decompositions to examine the sources of the racial gaps, we find that virtually all of the racial wage gap among children is due to a gap in their person effects. Black and white children earn nearly the same average pay premiums, as do their parents and their neighboring parents, suggesting that the average differences across races in access to higher-pay jobs are small (as suggested in Card et al., 2024b).

Our findings make several contributions to the intergenerational transmission literature. First, from a measurement perspective, we re-evaluate key conclusions about the form of the IGE using individual wages rather than family incomes. Chetty et al. (2014), find that the rank-rank relationship between the family incomes of parents and children is closer to linearity than the log-log relationship. In contrast, we find that mean log wages of children are nearly linear in the mean log wages of their parents, while the rank-rank relationship exhibits more curvature. We therefore adopt simple log-log models that are easily interpretable and amenable to AKM decompositions.

Second, we contribute to the literature on place effects in children's outcomes by proposing a simple working model with only two key components: the mean wage of a child's own parent; and the average wage of neighboring parents. We show that average wages of neighboring parents can explain a very high fraction of variation of the neighborhood effects (i.e., of neighborhood "absolute" or "upward" mobility, as studied by Chetty et al., 2014, 2026) in a model that controls for own-parent wages, providing a tractable, albeit somewhat over-simplified, framework for future empirical and theoretical work.

[6] See Bayer and McMillan (2005) and Aliprantis et al. (2024) for similar findings.

Third, we use AKM-style decompositions to study the structure of intergenerational transmission in the U.S.[7] We find that the person effect components of parents and neighbors are the major source of intergenerational correlations in earnings – consistent with conventional views of how inherent skills and human capital investments are transmitted between generations. But the pay premiums of parents and neighbors also matter, pointing to potential roles for local access to jobs, network effects, and related channels. Among other implications, our results suggest that the earnings premiums earned by children are not simply a matter of luck, but can in part be traced back to the premiums earned by their parents and neighboring parents in the places they grew up.

Finally, we contribute to the analysis of racial differences among recent cohorts of US wage earners. Consistent with Chetty et al. (2020) we show that differences in parental wages explain nearly one-half of the raw differential in mean log wages between Blacks and Whites. In addition, we show that differences in the mean earnings of neighborhood parents explain about one-third of the remaining gap. Importantly, when we control for neighboring parent earnings we find that a higher share of Black families in a Census tract is associated with slightly higher earnings for the children (of both races) who grow up there. Our evidence therefore points to neighborhood stratification by wages, rather than by race, as a major source of the racial earnings gap for the current generation of young adults in the US.

The next section of the paper provides an overview of our data sources and the derivation of our main analysis sample. Then we present a discussion of our extended IGE model that includes own-parent and neighborhood average earnings, and the use of an AKM framework to decompose wages. The fourth section of the paper presents our main results. The final section concludes.

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[7] Dobbin and Zohar (2025) use an AKM framework to decompose children's earnings and estimate the impacts of parental earnings on the person effects and mean pay premiums of children in Israel. Forsberg et al. (2024) present a similar analysis for Sweden. Neither study decomposes parent earnings, or includes neighborhood effects. Engzell and Wilmers (2024) use Swedish data to decompose parent and child earnings and present correlations between these various components.

II. Data sources and analysis samples

A. Basic parent-child sample

Our analysis is based on two primary data sources: the full count data from the 2000 Census and the LEHD. We use the Census data to identify family relationships, race, and the family's location during the child's teenage years. The LEHD data provide information on the wage and salary earnings of parents and children, as well as the identities of their workplaces.

We begin with the universe of non-Hispanic Black and white children who were born between 1983 and 1990 and recorded in the 2000 Census as the child of the household head (including step-child or adopted child).[8] We take the household head and their spouse, if present, as the child's parents.[9] We then impose several restrictions to ensure the quality of the intergenerational and earnings linkages: We limit attention to children who were assigned a unique personal identification key (PIK) and can be matched to the LEHD's Individual Characteristics File (ICF). We also drop children whose birth month and year are not the same in the 2000 Census and the ICF; those who do not have at least one parent with a unique PIK who is matched to the ICF; and those whose Census households contained more than 15 people.

We refer to the resulting sample, which includes roughly 15.5 million child observations, as our "***basic parent-child sample***." The children in this sample are roughly 50% female; about 15% are Black. We note that multiple children from the same household (i.e., siblings or step-siblings) can be included in the sample. We also construct an analogue of this sample from the 2000 Census public use files; we impose the same restrictions apart from PIK availability and birthdate mismatches with the ICF.[10]

[8] The 2000 Census identifies "person 1" of the household as the one who owns, is buying, or is renting the housing unit. For simplicity we refer to this person as the "household head". The questionnaire identifies how all other people in the housing unit are related to person 1, creating some ambiguity in cases where a child and her parent(s) live with a grandparent (or other non-parent) who is person 1. Our procedures drop such subfamilies.

[9] Note that we only include married (opposite sex) parents. In this regard our approach is similar that of Chetty et al. (2014) who use tax records from the 1990s, which did not allow unmarried partners to file jointly.

[10] The public-use Census file has a somewhat larger Black share, 19% vs. 15%, perhaps indicating a lower rate of valid PIKs for Black than white children.

B. Measurement of wages

A series of influential studies by Chetty and associates (e.g., Chetty et al., 2014, 2020, 2026) analyze intergenerational correlations in *family incomes.* We depart from their approach by focusing on *wages* -- or rates of pay -- of children and parents. Conceptually, we believe that the wage is a better measure of an individual's *earnings capacity* – the object of interest in intergenerational models like those of Becker and Tomes (1979), Loury (1981), and Solon (1999) – than family income, which is affected by marital status and the income transfer system.[11] The wage for is also the earnings concept used in the extensive AKM-related literature. Because wages are not observed for non-workers, we limit attention to families in which the child and at least one parent work. Ideally, we could measure *hourly* wages for both full-time and marginally attached (part-time or episodic) workers, in both traditional jobs and self-employment. In practice, this is not possible; the LEHD only reports total quarterly earnings from wage and salary jobs. We therefore adopt a number of restrictions aimed at excluding quarters with zero earnings or earnings that indicate a part-time job, and use average quarterly earnings for the remaining quarters as our measure of wages.[12]

We measure children's wages in 2018-2023 Q1, excluding the COVID quarters of 2020 Q2-Q4. Our sample children were between ages 27 and 40 during this period. We measure parents' wages in 2003-2007, when most of our sample parents were in their 40s or early 50s.[13] As discussed below, we use parents' wages in a later window, 2010-2014, to form an instrument for 2003-2007 wages. Parents were mostly in their 50s in this window.[14] We drop

[11] The wage is also more closely related to occupational status – the main focus of research in sociology on intergenerational aspects of social stratification. Studies of occupational transmission typically exclude families with non-working parents or children, as do we.

[12] Our measure is very similar to the measure of earnings for full time/full year wage and salary workers that is widely used in the inequality literature (e.g., Katz and Murphy, 1992; Autor, Katz and Kearney, 2008).

[13] Children in our sample are between 13 and 24 in this window. Parents who had children at age 30 are therefore between 43 and 54.

[14] A few parents are in their 60s by this window – a parent who had a child at age 35 in 1983 would have a 17-year-old in 2000 and would be 62 in 2010. If this parent retired at 62, he or she would not have sufficient labor force attachment in 2010-2014 to be included in our sample. This affects a few percent of the older children in our cohorts of interest.

families from Massachusetts, since that state did not join the LEHD program until 2010.[15] We also drop families that did not report housing tenure status (i.e., rent vs. own) in the 2000 Census, as we use this variable in some later analyses.

Our primary measure of parents' and children's wages is the average across all quarters with earnings over $3,300 in the relevant window.[16] We present sensitivity analyses that include quarters with lower (positive) earnings in this average.

C. Basic LEHD sample

Our analyses focus on samples with substantial attachment to the formal labor market. Starting from our basic parent child sample, we eliminate parents and children who did not report earnings in at least 4 quarters of the relevant time window in the LEHD, and those who did not have at least one quarter with earnings of $3,300 or more. This excludes parents and children with limited wage and salary work histories, in particular those who only worked in self-employment. We refer to the resulting sample, which includes roughly 9.7 million child observations, as our "***basic LEHD sample.***" Using this sample we can estimate a variety of IGE and extended IGE models: these turn out to look broadly similar to models estimated on our more restrictive main analysis sample, described below.

Our samples include children and parents of both genders. Most of our models include a dummy indicating a female child. For children who lived with only one parent in 2000, we use that parent's earnings (most often their mother's) in our models. For children who lived with two parents we use the earnings of the parent with the highest average quarterly earnings in 2003-2007 (most often their father). In some models we control for the presence of a second

[15] Two additional states, Arizona and the District of Columbia, joined the LEHD in 2004 and 2005 respectively. We do not systematically exclude children who lived in these states, though if the parents remained there in 2003-2007 they may not have sufficient earnings to meet our sample restriction. Similarly, a few states do not have LEHD data for recent years: Alaska, Arkansas, Mississippi, Michigan, and North Carolina only have data through 2016, 2018, 2018, 2021, and 2022, respectively. We do not systematically exclude children from these states, though the limited data coverage makes it less likely that children who live there as adults will meet our requirement of sufficient quarterly earnings.

[16] This is approximately the quarterly earnings of a worker who worked 35 hours per week at the Federal minimum wage that has prevailed since 2009 ($7.25 per hour).

parent and for their employment status. In robustness analyses described below we have also estimated models using combinations of both parents' earnings for dual-parent families.

D. AKM-based Restrictions

AKM proposed a simple additive model that decomposes the log of individual wages into a person effect, a pay premium for working at the current employer, and a vector of time varying characteristics (age and year effects). To implement this approach on our parent and child samples we estimate separate AKM models in each of three windows: 2003-2007, 2010-2014, and 2018-2023 (excluding 2020:Q2-4). In each time interval we use data for the full population of workers age 22-62 who appear in the LEHD and meet suitable labor force attachment restrictions. Our estimation samples exclude person-quarters in which a worker has more than one employer, or earns less than $3,800. We also exclude the first and last quarter of each job spell (to eliminate partial quarters). We further limit attention to individuals with at least 8 quarters of data in the relevant window meeting these restrictions. This eliminates marginally attached workers and those who change jobs very frequently. We use state employer id's (SEIN's) and employer sub-unit id's (SEINUNIT's) to identify establishments, and we normalize establishment pay premiums in each estimation window by setting the employment-weighted average of premiums in the restaurant industry (NAICS code 7225) to 0. Finally, as is standard in the AKM literature, we drop workers who are not included in the largest connected set of individuals and establishments.

After estimating the AKM models, we extract the estimated person and establishment effects and link them back to the parent and child samples described in the previous subsection. We drop children who are not included in the 2018-2023 AKM estimation sample, and primary parents who are not included in the estimation samples for both 2003-2007 and 2010-2014. We also drop families that lived in a Census tract in 2000 with fewer than 20 households remaining in the sample. This yields our "***main analysis sample***", which includes approximately 4.91 million children and their primary parent.

E. Sample description

Table 1 provides descriptive statistics for families in a public-use version of our basic sample (column 1) and in our main analysis sample (column 3).[17] As shown in column 1, 19% of the children meeting our basic sample criteria are Black, and 72% of them lived with two parents in 2000. In 93% of households in this sample at least one parent worked in 1999, and in 76% the highest earning parent earned at least $13,200 (=3300×4) and worked 40+ weeks. The highest-earning parent had average education of 13.7 years and average 1999 earnings of around $45,000.

As an aid to interpreting the restrictions in our main analysis sample, column 2 limits the public-use sample in column 1 to households in which the highest-earning parent worked at least 40 weeks and earned at least $13,200 in 1999. Most of these "higher labor force attachment" parents would meet our AKM inclusion restrictions for the 4 quarters of 1999. Children in this sample are less likely to be Black (14.9% versus 19.2% in column 1); are more likely to have two parents; and have better-educated and higher-earning parents ($55,000 for the highest-earning parent versus $44,500 in column 1).

Finally, column 3 presents characteristics of our main analysis sample, which incorporates data from the 2000 Census (shown in panel A) and the LEHD (shown in panel B). Looking first at the Census-based variables, we see that households included in this sample are a lot like those in column 2, suggesting that the labor force attachment restrictions imposed for our main analysis sample lead to the same kind of selection effects as the simpler restriction on 1999 labor market outcomes we imposed in column 2. An exception is that the Black share is lower in the linked Census-LEHD sample, perhaps reflecting the lower PIK rate for the Black population noted earlier.

Moving to the LEHD-based outcomes, we see that the geometric mean (annualized) wage of the highest-earning parent (averaged over all quarters included in our AKM estimation

[17] The sample in column 1 is derived from the 2000 Census public use microdata sample, using wherever possible the same procedures as we use for our basic sample. Note that our basic sample and analysis sample include families that did not complete the 2000 long form questionnaire, but assignment to the long form is random so we use the subset of long form completers.

samples) was $51,260 in 1999, $69,640 in 2003-2007, and $82,320 in 2010-2014. Adjusting for inflation, real wages of parents rose about 16% from 1999 to 2003-2007, and were roughly constant between 2003-2007 and 2010-2014.[18] By 2018-2022 the mean wages of the children in our main analysis sample were around $58,000 per year.[19]

We also show mean wages by child gender. Slightly less than one-half of the children in our main analysis sample are female, reflecting that fact that women are a little less likely to meet our labor force attachment criteria. The gender gap in mean log earnings (over quarters with earnings of at least $3,800) is 20.7%, some of which may be due to differences in hours of work between women and men.

To put our child sample in perspective, Appendix **Table 1** presents some descriptive statistics on samples of Black and white non-Hispanics who were born in the same years as our sample children, arrived in the US prior to 2000 if born abroad, and were interviewed in the 2018, 2019, 2021 or 2022 American Community Survey (ACS). We present data for all individuals meeting these criteria, and for those who worked 40 or more weeks in the previous year and earned at least $13,200 (i.e., the same criteria we used to select the subset of parents in column 2 of Table 1). In brief, we find that the "high attachment" subgroup has broadly similar gender/race composition, and about the same mean earnings (around $60,000 per year) as the children in our main analysis sample. In this subgroup the gender gap in annual log wages is about 18% and the Black-white gap is 30% -- very similar to the gaps we see for the children in the LEHD data.

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[18] In 1999 dollars, average earnings were $59,406 in 2003-7 and $59,732 in 2010-4. The 1999 earnings come from the Census long form survey, which asked about earnings in the previous year. The 2003-2007 and 2010-2014 earnings come from the LEHD and are averaged over all "full earning" quarters in our sample and then annualized. The exclusion of low-quarter earnings from our LEHD samples likely explains some of the apparent rise in earnings from 1999 to 2003-2007. Moreover, our sample is conditioned on substantial labor force attachment in 2003-2007 and 2010-2014 but not in 1999.

[19] This is about 25% lower, in inflation-adjusted terms, than the children's parents earned in 2003-2007. Some of this may be explained by the fact that the children are younger in 2019-2022 than their parents were in 2003-2007. Another factor is that we chose the highest earner for married parents but make no similar choice for the children. This is reflected in the gender composition: two-thirds of our primary parents are male, vs. 52% of our children.

Appendix Table 5 presents variance decompositions of earnings in 2003-7 and 2018-23 into person and establishment effect components for the parents and children from our main analysis sample.[20] In brief, these are very similar to the decompositions presented in Card et al. (2024a), based on LEHD data for 2010-2018, and to decompositions typically reported in the literature for other countries (see Kline, 2024). Person effects account for the majority of the variation in wages (74% of the variance in average wages of the children, 88% of the variance in average wages of parents, and 82% of the variance in mean parental wages in a Census tract); while the pay premiums account for a smaller share (14% of the variance of average wages of children, 11% for parents, and 6% for tract means). Pay premiums and worker effects are positively correlated, with their covariance accounting for 12% of the variance of children's wages, 5% of the variance of parent earnings, and 18% of the variance in tract mean parental earnings.[21]

III. Modeling Framework

In this section we lay out our extended intergenerational elasticity (IGE) model relating the wages of children to wages of their parents and the mean wages of other parents in the same neighborhood. We then expand the model using AKM decompositions of parent, neighboring parent, and child earnings.

We begin with a simple extension of a Galton-style IGE model that relates y_i, the mean log adult wage of a child from family i, to y_i^F, the mean log wage of the highest-earning parent in family i, and to the average log wage of the highest-earning parent for all families in the neighborhood $n(i)$ where the child was raised:

$$y_i \quad = \quad \beta_0 + \beta_1 y_i^F + \beta_2 \bar{y}_{n(i)}^F + \varepsilon_i, \tag{1}$$

[20] Results for 2010-4 are not reported but are similar to those reported for 2003-2007.

[21] Variance decompositions of AKM models are subject to "limited mobility bias" (Kline, Saggio, and Sølvsten, 2020; Bonhomme et al., 2023), which typically leads to understatement of the covariance term and overstatement of the variance terms (specifically that for pay premiums). These estimates are uncorrected. We expect that the bias in decompositions of tract means is much lower than that for individual earnings.

As discussed below, we follow the standard approach in the IGE literature and interpret β_1 and β_2 as reduced form coefficients that incorporate both the causal effects of own-parent and neighboring parent wages as well as the effects of family and neighborhood characteristics that are correlated with these variables. However, in our baseline models we also include commuting zone (CZ) dummies and indicators for the gender and race of the child. Without CZ effects, a model like (1) attributes regional differences in wages to neighborhood mean wages, leading to slightly larger estimates of β_2. Similarly, without a race dummy the estimates of β_1 and β_2 will be slightly larger, reflecting large racial gaps in child, parent, and neighboring parent wages. The gender control is less important, as child gender is not highly correlated with parental wages, but it could help to adjust for differential selection of male and female children into the labor force.

A. Relationship to simple IGE

The traditional "Galton style" IGE model omits neighboring parent wages:

$$y_i \quad = \quad \gamma_0 + \gamma_1 y_i^F + u_i. \tag{2}$$

The coefficient γ_1 in this model – often called "the" intergenerational elasticity – is related to the coefficients of (1) by:

$$\gamma_1 = \beta_1 + \beta_2 r_p, \tag{3}$$

where $r_p = V(\bar{y}_n^F)/V(y_i^F) \in [0,1]$ is the share of the variation in parent wages that is attributable to neighborhoods. This coefficient is also a convenient summary of the degree of sorting of families with similar wages across neighborhoods (see Kremer and Maskin, 1996): $r_p = 1$ indicates perfect sorting while $r_p = 0$ indicates no sorting.

Two implications of (3) are worth emphasizing. First, insofar as neighboring parents' wages matter to children's outcomes (i.e., $\beta_2 > 0$), the traditional IGE model (2) captures only a portion of the total effect $\beta_1 + \beta_2$ unless there is perfect sorting across neighborhoods. In contrast, an ecological regression of y_i on $\bar{y}_n^F$ will recover the sum $\beta_1 + \beta_2$ – see Appendix A. Second, the traditional IGE γ_1 may vary across places depending on the degree of sorting of families to neighborhoods.

B. Relationship to Chetty et al. (2014) measures of absolute and relative mobility

Chetty et al. (2014) estimate versions of equation (2) separately for commuting zones (indexed by *g)*, using data on the family incomes of parents and children:

$$y_i \quad = \quad \gamma_{0,g} + \gamma_{1,g} y_i^F + u_i'. \tag{4}$$

They refer to the intercept $\gamma_{0,g}$ as a measure of "absolute mobility" and the slope $\gamma_{1,g}$ as a measure of "relative mobility."[22] Chetty et al. (2026) estimate similar models for tracts rather than commuting zones, and refer to $\gamma_{0,g}$ as "upward mobility." Our extension of the IGE model can be seen as providing a simplified model of the variation in absolute/upward mobility. Specifically, consider the projection of $\gamma_{0,g}$ onto mean parental wages in location g:

$$\gamma_{0,g} \quad = \quad \kappa_0 + \kappa_1 \bar{y}_g^F + \nu_g \tag{5}$$

When (5) is substituted into (4) and relative mobility is constrained to be the same across units g, one obtains our basic two-parameter model (1). Moreover, ν_g (the residual in equation 5), is uncorrelated with $\bar{y}_g^F$, so the own-parent wage coefficient β_1 in equation (1) is invariant to whether the influence of neighborhoods is summarized by average parental earnings in the neighborhood, as in (1), or more flexibly with neighborhood fixed effects, as in (4).[23] We show below that average earnings of neighboring parents is very strongly correlated with absolute mobility as measured by the fixed effects in (4). In particular, the R-squared of equation (5), adjusted for sampling error in the estimated fixed effects, is around 85%. Moreover, other neighborhood characteristics add relatively little explanatory power to the model.

C. Interpreting the coefficients in the extended IGE model

To aid in the interpretation of the coefficients in (1), consider a specification (similar to the one laid out by Solon et al., 2000) that relates a child's wage in adulthood to their parent's

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[22] Absolute mobility depends on how y_i^F is normalized. Chetty et al. normalize both children's and parents' income ranks to have 25th percentiles of zero. For comparability with their results, we normalize log parent and child wages by subtracting the national 25th percentile from each.
[23] This follows from the logic of correlated random effects models: A group-level unobserved effect is uncorrelated with any individual-level covariate *x* conditional on the group mean of *x*. By the same logic, if additional neighborhood variables are included in (5) but their individual-level analogues are not included in (4), the coefficients on the neighborhood aggregates could reflect purely individual-level effects.

wage and the mean wages of parents in their neighborhood, to other characteristics x_i^F of their family, and to the average characteristics $\bar{x}_{n(i)}^F$ of other families in their neighborhood:

$$y_i \quad = \quad \tilde{\beta}_0 + \tilde{\beta}_1 y_i^F + \tilde{\beta}_2 \bar{y}_{n(i)}^F + \eta_1 x_i^F + \eta_2 \bar{x}_{n(i)}^F + \tilde{\varepsilon}_i. \tag{6}$$

If we assume that <u>all</u> systematic determinants of y_i are included in (6) and ignore for the moment the potential for measurement error in y_i^F, then it is innocuous to assume that the error term $\tilde{\varepsilon}_i$ is orthogonal to the included covariates – in other words, that the coefficients in (6) have a causal interpretation.

Standard regression logic implies that the coefficients in (1) are related to the coefficients in (6) by

$$\beta_1 = \tilde{\beta}_1 + \eta_1 \pi_1 \tag{7a}$$

$$\beta_2 = \tilde{\beta}_2 + \eta_1(\pi_2 - \pi_1) + \eta_2,. \tag{7b}$$

where π_1 is the vector of coefficients from a within-neighborhood regression of $x_i^F - \bar{x}_{n(i)}^F$ on $y_i^F - \bar{y}_{n(i)}^F$ and π_2 is a similar vector of coefficients from a between-neighborhood regression of $\bar{x}_{n(i)}^F$ on $\bar{y}_{n(i)}^F$. Any family-level factors (i.e., components of x_i^F) that contribute to children's wages will lead to omitted variable biases in both β_1 and β_2 relative to $\tilde{\beta}_1$ and $\tilde{\beta}_2$ (except in the special case where $\pi_1 = \pi_2$), while omitted neighborhood-level variables only affect β_2.[24] The literature suggests that parent characteristics like education, ambition, and attitudes toward economic success are strong determinants of a child's wage, even controlling for parental earnings capacity. Thus, we suspect that the "omitted variables" terms in (7a) and (7b) are relatively large.

<u>D. Measurement error in earnings</u>

We use parents' average quarterly earnings over a five year window to measure y_i^F. As pointed out by Mazumder (2005), however, even a five-year average may measure

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[24] This can be seen as an application of Gelbach's (2016) decomposition. The "within and between" attribution arises because the auxiliary regression of any variable on y_i^F and $\bar{y}_{n(i)}^F$ necessarily attributes the within-neighborhood component of the omitted variable to y_i^F and the between-neighborhood component to $\bar{y}_{n(i)}^F$. Thus, any variable that only varies at the neighborhood level loads solely on to $\bar{y}_{n(i)}^F$.

“permanent” wages with error. If the measurement error is classical, we would expect it to attenuate our estimate of β_1 and inflate our estimate of β_2 – making it appear that neighborhoods are more important than they are. We write out a simple model to show this formally in Appendix A.

To address this concern, we use an instrumental variables strategy. Specifically, we instrument y_i^F with parents’ log average earnings over a later five-year interval, separated by several years from the interval in which y_i^F is measured. We do not instrument for $\bar{y}_{n(i)}^F$, on the expectation that measurement errors average out at the neighborhood level. Supporting this, we have estimated models that use an average of neighboring parents’ log earnings in our second window as an instrumental variable for their log earnings in our main window (i.e., for $\bar{y}_{n(i)}^F$), and found that this has very little effect.[25]

Assuming that the measurement errors in our two separate measures of y_i^F are uncorrelated, 2SLS should provide consistent estimates of the coefficients β_1 and β_2 in (1). As we discuss in Appendix A, however, if the deviations of mean parent wages from their true “permanent” wages in the two intervals are positively correlated, then the 2SLS estimates will not fully correct the measurement error bias, leading to a downward asymptotic bias in the estimate of β_1 and an upward asymptotic bias in the estimate of β_2. Based on simulations of realistic processes for wages (discussed in the Appendix) we believe that such biases are likely to be small.

E. Decompositions of parent and child wages

As highlighted in equations (7a) and (7b), the coefficients in our estimating model (1) are reduced form parameters that reflect a combination of *causal* effects and omitted parent and characteristics that are correlated with wages. One strategy to try to identify the causal

[25] A different source of measurement error in $\bar{y}_{n(i)}^F$ arises because families can move to different neighborhoods. Chetty et al. (2026) find that on average children spend about 75% of their childhood in the same Census tract, and that families who move tend to move to tracts with similar rates of upward mobility, which given our results below implies that they have similar mean parental wages. In light of these findings we suspect that the measurement error caused by mobility are small.

effect of earnings is to decompose y_i^F into a component that might be confounded by other characteristics and a component that is not (see Mayer, 1998). Shea (2000), for example, separates out the part of parental income that is attributable to union membership, industry, or job loss – factors that Shea describes as "luck" – and tests whether these components affect children's outcomes to the same extent as other components of parent income.[26]

We pursue a similar strategy, using AKM decompositions of own parent wages and neighborhood average wages. Following AKM, we assume that the log quarterly wages of worker *i* in quarter t, y_{it}, can be decomposed as:

$$y_{it} = \alpha_i + \psi_{j(i,t)} + X_{it}\beta + \xi_{it}, \tag{8}$$

where α_i is a person fixed effect, $\psi_{j(i,t)}$ is a fixed effect for the establishment at which the person works in quarter t, $j(i,t)$ is an index function giving the workplace of individual i in quarter t, $X_{it}\beta$ captures effects of time-varying observable covariates, such as experience and calendar time, and ξ_{it} is assumed to be orthogonal to the sequence of establishment effects, conditional on α_i.[27] In the AKM literature the person effect component of wages, α_i, is interpreted as an individual's earnings capacity at any job (attributable to such factors as education, ability, and ambition) while the pay premiums for specific workplaces, $\psi_{j(i,t)}$, are interpreted as some combination of efficiency wages (Piyapromdee, 2018), compensating wage differentials (Sorkin, 2018), rent sharing (Card et al., 2018), and cumulated success in moving up the job ladder (Manning, 2003). There are many reasons to expect that these two components of parent earnings will affect children's earnings capacity differently. Likewise, the average person effects and earnings premiums of neighboring parents might be expected to affect children through different channels and affect children's long run outcomes differently.

We estimate model (8) separately for the two intervals we use to measure parent wages (2003-2007 and 2010-2014) and for the 2018-2023 interval we use to study children's wages. In each case, we use data for *all individuals* in the interval who meet the sample restrictions discussed in Section II.E. We then assign the estimated person effects and average pay

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[26] In a similar vein, Bastian and Michelmore (2018) study the effects of expansions of the EITC on children's education outcomes. See also Barr et al. (2022)

[27] See Card, Heining, and Kline (2013) Card, Rothstein, and Yi (2024a, 2025) and Kline (2024) for discussion and evidence regarding the validity of this "exogenous mobility" assumption.

premiums to the parents and children in our main analysis samples, and average these for all the parents in a given Census tract.

With the AKM components in hand, we estimate variants of equation (1) that separate the person effect and pay premium components of own parent wages and average neighborhood wages:

$$y_i = \beta_0 + \delta_{1\alpha}\alpha_i^F + \delta_{1\psi}\bar{\psi}_i^F + \delta_{2\alpha}\bar{\alpha}_{n(i)}^F + \delta_{2\psi}\bar{\bar{\psi}}_{n(i)}^F + e_i' \tag{9}$$

where α_i^F is the estimated person effect for the main parent of child i and $\bar{\psi}_i^F$ is the mean of the estimated pay premiums received by that parent in 2003-2007 (averaged over all firms where the parent worked during this period). The neighborhood terms $\bar{\alpha}_{n(i)}^F$ and $\bar{\bar{\psi}}_{n(i)}^F$ are, respectively, the mean of the estimated person effects for all the parents in the neighborhood where the child was living at the time of the 2000 Census and the mean of the estimated pay premiums received by those parents (averaged over both the firms at which each parent works and over parents in the neighborhood). Importantly, since our models include both the person effect and pay premium components of wages, the coefficients $\delta_{1\psi}$ and $\delta_{2\psi}$ provide estimates of the effects of higher premiums, *holding constant the person effects* of a child's own parent and the parents in their neighborhood.[28]

As has been emphasized in the AKM literature (e.g., Kline 2024) the components α_i^F and $\bar{\psi}_i^F$ are estimated with error. Again, we adopt a 2SLS strategy, using an AKM model fit to the data from the second interval (2010-2014) to obtain estimates of α_i^F and $\bar{\psi}_i^F$ that we can use as instruments for the corresponding estimates from the 2003-2007 window. The estimation errors in the AKM components will be only weakly correlated across these windows if the error components in the wage generating process are not *too* persistent. Any remaining bias should be in the direction of understating the parent-level coefficients ($\delta_{1\alpha}$ and $\delta_{1\psi}$) and overstating their neighborhood analogues ($\delta_{2\alpha}$ and $\delta_{2\psi}$).

We can also decompose *children's* wages into person effects and pay premiums, estimating versions of (9) which take as dependent variables either α_i (the estimated person

[28] The person effect and pay premium components are positively correlated so a specification that excluded the person effect components would over-state the effects of the pay premium components relative to model (9).

effect for a given child) or $\bar{\psi}_i$ (the mean of the estimated pay premiums received by the child). Of particular interest is how the person effect and pay-premium components of parent wages, and neighboring parent wages, affect the different components of children's wages. To foreshadow our results, we find that parents and neighborhoods with higher person effects produce children with higher person effects, while parents and neighborhoods with higher pay premiums produce children with higher pay premiums. This pattern casts doubt on a pure luck interpretation of the pay premiums, and points instead to models in which access to higher-wage jobs is mediated in part by families and neighborhoods.

IV. Main Results

A. Basic IGE

We begin by exploring the basic relationship between parents' and children's wages, before moving on to models that incorporate neighborhoods.

Figure 1 presents a binscatter of child average log wages in 2018-22 against parent average log wages in 2003-7, estimated for our basic LEHD sample (i.e., the set of parents and children who are observed with at least one quarter of earnings over $3,800 in the LEHD) and for our main analysis sample (which requires at least 8 non-transitional quarters with earnings above that threshold for both parents and children, and also imposes a similar restriction for parents in the separate 2010-14 window).

Three features of the figures stand out. First, relative to the binscatter for the basic LEHD sample, the binscatter for our main analysis sample is shifted upward and to the right. This reflects the exclusion of parents and children with less than 8 "full earnings" quarters from the main analysis sample, which substantially truncates the lower tail of wages for both groups. Second, the binscatter relationships in both samples are quite close to linear. In an analysis not reported in the figure, we find more convexity in the lower tail when we construct parent wages using lower-earning quarters, supporting our presumption that many low-earning parents have self-employment income that is not recorded in the LEHD. Third, the slopes of the binscatters are quite similar for the two samples, suggesting that the more stringent restrictions needed to support our AKM analyses do not have dramatic impacts on the basic IGE.

In Panel A of **Figure 2** we present binscatters for the white and Black subsamples of our main analysis sample; these are both approximately linear and have similar slopes but different intercepts (as in Chetty et al., 2020). In Panel B of the figure we present binscatters constructed from earnings ranks rather than average log earnings. These are notably less linear than the log-log relationships, particularly for families with parents near the top of the earnings distribution.

Even our basic LEHD sample is limited to individuals with positive earnings in at least some quarters, thus excluding parents and children who are entirely out of the labor force. It is in principle possible to construct ranks that include parents and children with zero observed earnings. In our setting, however, these observations are disproportionately affected by non-classical measurement error: Workers who are self-employed throughout our window would have zero earnings in the LEHD, but this is a misleading estimate of their true earnings capacity. Indeed, we find that the share of observations with zero earnings in our full sample is much larger than in Census data or than the share with zero total income in Chetty et al. (2014)'s analyses.[29] Moreover, when we construct rank-rank relationships that include zero earners (not reported), they show substantial nonlinearity in the left tail. We thus focus on our main analysis sample, which we see as accurately reflecting the wages of parents and children who are strongly attached to the wage and salary labor market.

A regression fit to the data used to construct Figure 1 yields an IGE of 0.300 in the basic LEHD sample and 0.289 in our main analysis sample. As noted above, if the 5-year average of log earnings is a noisy measure of permanent earnings (Mazumder, 2005), this slope is attenuated relative to what would be obtained with a permanent earnings measure. Accordingly, we instrument for the highest-earning parent's mean log earnings in 2003-7 with the same parent's mean log earnings in 2010-14. The first stage coefficient for this model is

[29] Chetty et al. (2014) argue that nonlinearity in the tails of a log-log binscatter plot and near linearity of a rank-rank plot supports the use of ranks in their analysis. Their finding of nonlinearity in part reflects the presence of very low incomes in their sample. Their Figure I.B indicates that close to 10% of their parent sample has annual family incomes below our effective threshold of 4*3880=15,520, while they report that 6% of the children in their sample have zero incomes (Appendix Table 3).

0.842 (S.E. 0.003) , and the 2SLS estimate of the IGE is 0.319, suggesting that there is about a 10% attenuation in the IGE using a 5-year average of parent earnings.[30]

As noted by Moretti (2004) there are large differences in wages across U.S. cities (see Card et al., 2025a for evidence based on LEHD data). Since most children live close to where they grew up, this will inflate the measured IGE. Given our interest in neighborhoods, we prefer to focus on within-city differences. We thus augment the basic intergenerational regression with fixed effects for the commuting zone where the family lived at the time of the 2000 Census. The resulting estimates can thus be compared to IGE estimates constructed from observations in a single city or a smaller country. Column 1 of **Table 2** reports the 2SLS estimate of the slope of a simple IGE model with CZ fixed effects, which is 0.295. (The OLS estimate is 0.266, reported in column 1 of **Appendix Table 3**.)

B. Neighborhood-augmented IGE

Next, we turn to models that incorporate the role of neighborhoods. **Figure 3** shows a binscatter relating the mean log wage of children who grew up in a given Census tract to the mean log wage of parents in the tract. Column 2 of Table 2 presents the corresponding (person level) regression. The slope – which as noted in Section III is an estimate of the sum of the coefficients δ_1 and δ_2 in equation (7) – is 0.486, about 1.7× larger than the slope of the basic IGE in column 1. This is an initial indication that neighborhood wages have a substantial independent effect (i.e., that β_2 in equation (2) is large).

Figure 3 also shows the within-tract relationship between children's and parents' wages: this is slightly less linear (mainly in the extreme tails) and also has a much smaller slope (closer to 0.25). Column 3 of Table 2 presents the coefficients from a specification in which we include both the own-parent mean wage in 2003-7 (instrumented with the mean wage in 2010-14) and the average wage of all parents. In an OLS version of this model the coefficient on own-parent

[30] Mazumder (2005) estimates that the attenuation associated with use of a 5 year average of incomes is around 30%. However, his calibration is based on moments of annual earnings from survey data sources, which contain transitory measurement errors that are absent in the LEHD. Also, some of the transitory variation in annual earnings is due to fluctuations in weeks of work, most of which are eliminated by our wage measure.

wages is just the within tract regression coefficient – i.e., the slope of the "within neighborhood" binscatter in Figure 3. Perhaps surprisingly, even in an IV specification the estimated effect of own-parent wages is slightly smaller than the effect of parental average earnings, with both coefficients in the range of 0.25.

Recall from equations (8a) and (8b) that the own-parent and neighborhood average parent wage coefficients are $\delta_1 = \beta_1\lambda$, and $\delta_2 = \beta_2 + \beta_1(1-\lambda)$, where β_1 and β_2 are the effects of permanent wages and λ is a measure of the within-tract reliability of measured parental earnings. The fact that $\hat{\delta}_1 \approx \hat{\delta}_2$ points to one of two conclusions: either average parental wages are extremely noisy (even after instrumenting), or β_2 is relatively large. We suspect that there might be some attenuation in the IV estimate of δ_1, but not nearly enough to account for the magnitude of the estimate of δ_2, leading us to conclude that β_2 is large.

Column 5 of Table 2 replaces tract mean wages with Census tract fixed effects. These capture any neighborhood factor that is relevant for explaining the earnings of children who were raised there, including features that are uncorrelated with mean parental wages. Replacing mean parent wages with a full set of tract effects has no impact on the own parental wage coefficient, but it does raise the R-squared of the model, from 0.128 in column 3 to 0.134 in column 5. Given the number of Census tracts in the US (around 40,000) and the modest number of families per tract in our data set, however, some of this rise is spurious. In the next subsection we discuss a simple approach for taking account of this fact.

Columns 4 and 6 add several other family characteristics to the specifications in columns 3 and 5: Indicators for Black and male children, for single-parent families, and for families with two earners. These variables have very little effect on the own-parent wage coefficient and only a small effect on the neighborhood earnings coefficient, suggesting that average parental earnings are a good summary of the neighborhood features that matter for children's later earnings. Interestingly, both of the family structure measures have estimated coefficients that are the *opposite* of the expected signs: Children from single-parent families have higher wages than children from two-parent families, controlling for the wage of the highest-earning parent (and the average of this variable for other parents in the Census tract), while children from two-earner families have lower wages than do those from single-earner, two-parent families, again conditional on primary parent and neighborhood wages.

Finally, in columns 7 and 8 we return to models based on equation (7) but add a measure of the mean parental earnings of parents in the set of tracts surrounding the tract where the family lived in 2000.[31] These models probe whether the "neighborhood" that matters for a child's success is larger than their Census tract – as might be the case, for example, if school district resources are a major driver of children's wages, since school districts are typically larger than Census tracts. Interestingly, we find that the mean parental earnings in surrounding tracts have a very small effect once we control for mean earnings in the child's own tract. We take this as evidence that the tract is a good proxy for the actual neighborhood.

Appendix Tables 2, 3, and 4 present several alternative versions of the specifications in columns 1-4 of Table 2. Models without CZ effects (in Appendix Table 2) yield a slightly larger coefficient for the ecological model in column 2 (0.503 vs. 0.486) and attribute more of the total ecological effect to neighborhood average parent wages. The OLS models (Appendix Table 3) yield slightly smaller estimates of the effect of own-parent wages on children's wages. The effects of some of the other variables also differ slightly. Finally, Appendix Table 4 compares estimates of these specifications in three samples: our main sample; the more inclusive basic parent and child sample discussed above, which includes parents and children with just a few quarters of earnings in 2003-2007 and 2018-2023; and a subset of the basic parent and child sample that excludes parents without earnings in 2010-2014. Results are very similar across samples.

C. Determinants of Neighborhood quality

As noted in Section III, adding a control for the average wage of all parents in a neighborhood eliminates any bias caused by unobserved neighborhood factors in the estimation of the intergenerational transmission between parent's and children's wages. But this does not mean that other neighborhood characteristics don't matter. To evaluate the

[31] Specifically, we use a sample-size-weighted average of mean parental earnings in all tracts that share a border with the child's own tract.

effects of other characteristics that may be relevant, we take the estimated tract fixed effects from the model in column 6 of Table 2 (which controls for parent wages and 4 other family characteristics) and regress these omnibus measures of neighborhood “quality” on various characteristics of the Census tract. The results are presented in **Table 3**.

The model in column 1 uses just neighborhood mean wages as an explanatory variable. By construction, the coefficient here is (nearly) the same as the one in the second row of column 3 of Table 2,[32] but the R-squared of the model is of interest: Neighborhood mean wages explain 68% of omnibus neighborhood “quality.” Moreover, this R-squared is actually an under-estimate because the estimated tract effects contain sampling errors as well as true differences across neighborhoods. We estimate that the reliability of the estimated tract fixed effects is 0.80, so the implied share of variance of true tract effects, net of sampling error, that is explained is 0.68/0.80 = 84%.[33] This is remarkably high considering that parental wages only capture one dimension of neighborhood “quality” – though of course other characteristics may be correlated with mean parental wages.

Column 2 uses another plausible index of quality based on the mean education of adults in the tract in 2000 (derived from the 2000 long form Census). This variable explains only slightly less of the variance of child outcomes than mean parental wages. When we add both variables (column 3) the adjusted R-square of the model rises from 84% to 86%, and mean education remains highly significant even controlling for average parental wages. This suggests that there would be some gain to combining average parental wages and average education as an index of neighborhood quality. In the interests of simplicity, however, we proceed using mean parental wages as our primary measure of tract quality.

The model in column 4 explores two alternative tract-level characteristics: the share of children who are Black and the share who are boys. Our main interest is in the former: Each

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[32] The coefficients would be mechanically identical had we used OLS in Table 2, but recall that the specifications there uses 2SLS to eliminate bias from transitory measurement error in parental wages.

[33] To estimate the reliability, we begin by computing estimated standard errors for the tract effects, treating the coefficients on the continuous regressors as known. The signal variance of the tract effects is the variance of the estimated coefficients minus the (weighted) mean of the squared standard errors of the tract effects.

percentage point increase in the neighborhood Black share is associated with a 0.14 log point reduction in neighborhood quality. (Recall that the neighborhood quality measure here comes from a specification that controls for the individual's own race.) However, in column 5 where we add back the neighborhood mean wage, we find that this more than fully captures the difference in neighborhood quality. Controlling for mean parent wages, a higher Black share leads to better outcomes for children who are raised in a tract. Interestingly, controlling for parental wages also reduces but does not fully eliminate the negative effect of a higher fraction of boys among children in the neighborhood.[34]

Column 6 adds two other characteristics that we might expect to be related to neighborhood quality (as indexed by the success of children who are raised there): the adult employment rate and the share of single parents. The employment rate has the opposite of the expected sign, but is small and statistically insignificant. Neighborhoods with higher single parent shares are worse, as expected (e.g., Wilson 1987). However, the model R2 in column 6 is only slightly higher than in column 1, indicating that the other demographic characteristics offer little explanatory power once parental wages are controlled.

Finally, column 7 adds controls for the share homeowners and the average value of owner-occupied homes in the tract. In many models (e.g., Bayer et al., 2007; Berry 1994) the latter should serve as a sufficient statistic for all amenities of the neighborhood that are observed and valued by homeowners, so if homeowners observe and value tract effects on children's wages then the inclusion of this control should drive out all other coefficients. This is not what we see. Although the home values coefficient is positive, it is small, and the homeowner share has the opposite of the expected sign. Moreover, adding these variables hardly increase the model's explanatory power, and the neighborhood mean wages coefficient is little changed from column 1.

[34] We measure the fraction male in our sample, which is restricted to children who are strongly attached to the labor market as adults. We have also estimated models that use the fraction male across all children in a neighborhood and find similar results. We note that the negative effect of a higher share of boys is consistent with Deza and Zhu's (2025) finding of negative peer effects of the fraction male at a school.

Overall, we conclude that neighborhood mean wages are close to a sufficient statistic for all other characteristics of a neighborhood, observed or unobserved, that influence the average wages of children who grow up in them – i.e., the rate of "absolute mobility" in the terminology of Chetty et al. (2014). This single variable explains approximately 84% of unrestricted neighborhood quality. Apart from average education, other variables that have often been proposed as measures of neighborhood advantage or disadvantage add surprisingly little to the explanatory power of mean parental wages. And even mean education has only a small added effect.

The estimate in column 1 of Table 3 indicates that the elasticity of child earnings with respect to neighborhood mean earnings, controlling for the family's own characteristics, is 0.22. This is surprisingly large – equal in magnitude to the within-neighborhood IGE, and over two-thirds of the overall IGE from Table 2, column 1. However, it is small relative to the effects of neighborhoods implied by many causal research designs. One comparison is to the Moving to Opportunity experiment, in which families living in public housing were offered housing vouchers that could be used only in lower-poverty neighborhoods. Chetty, Hendren, and Katz (2016) find that use of a voucher led to a 21 percentage point reduction in the average poverty rate of tracts in which recipients lived, and that it increased the earnings of the children exposed to the treatment by +31% (with an implied confidence interval of plus or minus 25 percentage points). Each percentage point reduction in neighborhood poverty is associated with a 0.01 change in neighborhood log income,[35] so the MTO estimate corresponds to an elasticity of nearly 1.5, more than six times our estimate, though the confidence interval extends down to about 0.3.

D. Decomposing wages into worker and firm components

[35] The MTO estimates are treatment-on-the-treated effects for children under age 13 at random assignment, instrumenting for use of a voucher with the voucher offer. See Chetty et al. (2016), Tables 2 and 3. In 2000 Census data, using all tracts in the five MTO states with poverty rates over 10%, the slope of log median earnings of full-time, full-year workers – a proxy for our mean neighborhood earnings of parents in our sample – with respect to the poverty rate has a slope of -1.01 (SE 0.02).

Next, we turn to our analysis of decompositions of parents' and children's earnings into person effects and establishment-specific pay premiums (or "firm effects", for simplicity). As discussed in Section III, these are estimated from separate AKM models fit to the full population of working age individuals in three windows – 2018-2023 for children, and 2003-2007 and 2010-2014 for parents. We average the estimated pay premiums at an individual's workplace in each quarter to get their average pay premium. We average the person effects and average pay premiums received by all parents in a tract to get the constituent components of the average parental wage in that neighborhood.

Table 4 presents models that decompose children's wage (panel A) and parents' and neighbors' wage (panel B) into their AKM components. We first discuss column 1, which (like the models in Table 2) takes children's log wages as the dependent variable. We present our basic two-parameter IGE specification in Panel A.[36] Panel B replaces the parent and neighborhood average wages in this model with the corresponding person and average firm effects (see equation 12 above). Recognizing potential mismeasurement of the person effect and average pay premium received by a parent, we instrument the own-parent wage components by the corresponding wage components derived for the same person using data from 2010-14. We assume, however, that the neighborhood average person effect and neighborhood average pay premium are measured accurately

Looking at the coefficients in the first column of Panel B, we see that all four of the parent and neighborhood wage coefficients are positive and significant: Children's wages are higher when their parents have higher person effects or pay premiums, or when their neighbors do. Just as we find in our basic 2-parameter IGE that the effects of own-parent wages and neighborhood mean wage are similar in magnitude, we see the same thing for the AKM components of wages: In each case, the neighborhood mean has a similar coefficient to the parents' own value. For parents and neighbors, the person effect coefficient is 2-2.5 times larger than the establishment premium coefficient. A natural interpretation is that unobserved family characteristics that affect children's wages are more strongly correlated with the person

[36] The specification is similar to Table 2, column 3, but in Table 4 we include in all specifications controls for the child's race and gender and for the Black share of the tract.

effect components of parental wages than with the pay premium components. In this interpretation, the pay premium coefficient is closer to the causal effect of parental economic resources, holding other family and neighborhood characteristics constant.

Next, we turn to the decomposition of children's earnings. We begin with the simpler specification in panel A. Columns 2 and 3 show that both the person effect and the establishment effect components of children's wages are strongly related to own=parent and neighboring parent wages, and in each case the estimated parent and neighborhood coefficients are approximately the same. About 85% of intergenerational transmission appears to operate through the child's person effect, and about 15% through the establishment effect. While the latter share may seem small, recall from our discussion of the AKM estimation results in Appendix Table 5 that firm effects only contribute about 15% of the variance in children's average wages – thus the relative magnitude of the coefficients in columns 2 and 3 makes sense.

As noted in Card, Rothstein, and Yi (2024a, 2025), it is potentially useful to break down the pay premiums received by children (or parents) into components attributable to place of work and industry. We follow that approach and decompose the pay premium at a given establishment into: (1) the mean pay premium for all establishments in the same CZ; (2) the mean pay premium for all establishments in the same industry (classified by 2-digit NAICS codes); and the deviation from the sum of these two components.[37] Columns 4-6 present models using the corresponding three components of children's earnings premiums as the dependent variable. We find that some of the intergenerational transmission operates through each component: Children from higher-earning families and neighborhoods tend to wind up in higher-wage CZs, higher-wage industries, and higher-wage firms within CZs and industries. The within-industry and CZ component accounts for approximately half of the effect in column 3, suggesting that higher parental and neighboring parent wages are associated with better access to high-wage firms even within CZs and industries.

[37] In Card et al. (2024a) we find that the CZ and industry components are approximately uncorrelated.

Staiger (2025; see also Kramarz and Skans, 2014) documents that many children start their first job at a firm where their parent worked. In cases where children are still working at a parent firm there is a mechanical link between their parents' wages and their own firm premiums. Only about 3% of the children in our sample, however, are observed by 2018-2023 working at a firm where their parents worked in 2003-2007.[38] The model in column 7 shows that this is slightly more common among children of higher-wage parents, though less common among children from neighborhoods where parents as a whole earn higher wages. When we estimate separate models for the pay premiums of children who never worked at the same firm as their parent, or did work at such a firm, we find that the overall pattern of intergenerational transmission shown in column 3 is driven by the former (much larger) group. This makes clear that direct nepotism of the type studied by Staiger (2025) cannot account for the broad pattern of intergenerational transmission that we see.

Panel B of Table 4 presents models where we break out parents' and neighbors' wages into their person and establishment effect components. In column 1, discussed above, the coefficients of the person effect components are roughly 2× the coefficients of the establishment effect components. When we separate out the person and firm effect components of children's wages in columns 2 and 3, we see a very interesting pattern underlying these differences. Specifically, the person effect components of parent and neighboring parent wages have relatively large effects on the person component of children's wages (on the order of 0.20), and only small effects on children's pay premiums (on the order of 0.03). Conversely, the establishment effect components of parent wages and average parent wages have relatively large effects on children's establishment effects (on the order of 0.10), and only small effects on children's person effects.

An interpretation of this "block diagonal" structure is that the person effect components of pay largely reflect human capital, and a child's human capital is mainly determined by the human capital of her parents and neighboring parents. In contrast, the pay premium components of wages are driven by access to higher-wage firms, and this access is mediated by

[38] A larger share of children presumably worked at their parent's firms earlier in their careers, but we purposely focus on children's wage in their 30s.

family- and neighborhood-based networks (as in, for example, Bayer et al., 2008). Consistent with this interpretation, the parent establishment effect coefficient is somewhat reduced when we exclude children who ever work at their parent's firm -- though even for these children there is a relatively large impact of their own parent's establishment effect, suggesting a mechanism other than direct nepotism. The models in columns 4 and 5 of Panel B show that the intergenerational transmission of establishment pay premiums has a very small geographic component, but there is some transmission of industry-wide pay premiums, and also a tendency for children to work at high-paying firms in their industry if the parents and neighboring parents did so.

To further explore the nature of the networks mediating the transmission of pay premiums, we conducted a simple decomposition exercise. Specifically, let D_i^p be an indicator for whether child *i* ever worked (in 2018-22) at a "firm" (more correctly an *establishment*) where their parent worked in 2003-7, and let D_i^n be an indicator if they ever worked at a firm where a neighboring parent in their 2000 Census tract worked. We can decompose the average pay premium received by a given child ($\bar{\psi}_i$) as:

$$\bar{\psi}_i = \underbrace{\bar{\psi}_i D_i^p}_{parent\ firm} + \underbrace{\bar{\psi}_i (1 - D_i^p) D_i^n}_{neighbor\ (not\ parent)\ firm} + \underbrace{\bar{\psi}_i (1 - D_i^p)(\bar{\psi}_i (1 - D_i^n)}_{neither\ parent\ or\ neighbor\ firm} \qquad (13)$$

i.e., as the sum of the firm effect if they worked for the same firm as their parent, plus the firm effect if they never worked at a parent's firm but worked at a neighboring parent's firm, plus the firm effect if they never worked at any firm that employed their parent or any neighbor. In **Appendix Table 6** we present models like those in columns 3-6 of Table 4, but breaking the child's firm effect into the three terms on the right hand side of (13). Notice that the sum of the intergenerational coefficients for each of these three terms equals the intergenerational coefficient for $\bar{\psi}_i$. Thus we can see how much of the overall transmission effect is attributed to each of the three channels identified in (13).

To set the stage for this analysis it is useful to recall that only 3% of children ever work at a parent's firm during our window, while about 8% work at a neighboring parent's firm and 89% do neither. As might be expected then, about 90% of the intergenerational transmission of parent wages to the child's firm effect (0.031/0.034 = 91%), and more than 100% of the transmission of neighboring parent's wage to a child's firm effect (0.042/0.032=131%) is attributable to effects for children who never work at either their parent's or a neighboring parent's firm. When we separate the AKM components of parent and neighboring parent wages (as in Panel B of Table 4), we see a similar pattern for the components of parent earnings but an interesting exception for neighboring parents. Specifically, we find that nearly all of the 0.078 coefficient for the effect of the average pay premium of neighboring parents on a child's pay premium (shown in column 3 of Table 4) is attributable to gains for children who end up working at a firm where at least one of the other parents in their Census tract worked. This points to a potentially important role for neighboring parents in helping children find jobs at their firms.

Finally, we fit models similar to those in Panel B of Table 4, but breaking out the firm effects of own parents and neighboring parents into an industry component and the deviation from the industry component. (We do not add a CZ component because our models include CZ effects, and therefore already deviate all explanatory variables from their CZ means). The results are presented in **Appendix Table 7.** When the dependent variable is the mean log wage of the child, we find that the industry components of the wage premiums earned by a child's own parents and their neighboring parents have zero effects – results that mirror the findings of Shea (2000). All of the effects on children's wages arise through the deviations of own-parent and neighboring parents' wage premiums from their industry means. When we break out children's wages into their AKM components, however, we find an offsetting pattern: the industry components of own-parent and neighboring parent firm effects have relatively large negative effects on the person effects of children (coefficients around -0.09), offset by similar sized positive effects on the firm effects of children. This is not the case for the deviations of the own-parent and neighboring parent wage premiums from their industry means, which have positive effects on both the person and firm effects of children.

If we interpret the person effects of children as mainly reflecting their human capital, then the negative effect of higher industry-specific wage premiums for parents and neighboring parents suggests that the local availability of jobs in high-paying industries depresses children's human capital accumulation. There is some evidence in the literature of such a "resource curse" for children who grow up where there are high-paying jobs in resource extraction (Black et al., 2005; Cascio and Narayan, 2022) or construction (Charles, Hurst, and Notowidigdo, 2018). Our findings suggest that there is no such effect when the wage premiums earned by parents or neighboring parents are specific to their firms, suggesting that the phenomenon may only arise when there are many high-premium employers in a specific local sector.

E. Racial differences in child earnings

Lastly, we explore racial differences in children's outcomes. As a point of departure **Table 5** presents Black-white wage gaps, both overall and within CZs, for parents and children. (Recall that these gaps are for workers with substantial labor force attachment). Focusing on the estimates with CZ controls, Black children in our sample have wages that are about 28% lower than white children, on average. The gap in their parents' wages is much larger, 48%. There is also a large 34% gap in neighborhood mean earnings, albeit smaller than that in parents' own earnings. Panel B of the table shows the corresponding gaps in the AKM-based components of children's, parents', and neighboring parents' wages.[39] An important finding for all three groups is that the racial gaps in wages are mainly due to gaps in the person effect components of wages. The gaps in establishment effects are much smaller, and for the children themselves there is no establishment effect gap at all.

Table 6 presents a series of models for children's log wages that include child race as a covariate. Column 1 reproduces the raw Black-white gap in children's log wages reported in Table 5. Just under half of this gap is explained by differences in parental wages (column 2). The remaining gap is 0.15 -- roughly the vertical distance between the bin-scatters for white and Black children shown in Figure 2. This falls by just over a third when we control for the mean wages of neighboring parents (column 3), implying a gap of around 10% -- similar to the estimated gaps in Table 2. In column 4 we break out own-parent and neighboring parent wages

[39] In **Appendix Table 8** we present comparisons of the AKM variance decompositions for Black and white children and parents, paralleling the overall variance decompositions in Appendix Table 2. Black children and parents have somewhat lower variances in their average wages and in their average wages across Census tracks and CZ's. However, the variance shares of the person and firm effects, and the correlations between these two sets of effects, are similar for Blacks and whites.

into their AKM-based components, as in the model in column 1, Panel B of Table 4.[40] This leads to a relatively small change in the estimated Black wage differential.

In column 5 of Table 6 we include interactions of both own-parents' wages and neighborhood mean wages with a Black indicator. The interaction with own-parent wages is very small (consistent with the nearly parallel lines in Figure 2) but there is larger negative interaction effect for neighborhood mean wages, suggesting that Black children benefit less from the presence of higher-wage neighboring parents than white children. We explore this phenomenon in **Appendix Table 9**, where we expand the IGE model slightly by including the average wage of neighboring parents <u>and</u> the average wage of neighboring parents of the same race as the child. This specification yields a coefficient of 0.165 for the effect of the average wage of neighboring parents on a child's average wages, and a coefficient of 0.071 for the effect of the average wage of neighboring parents of the same race.[41] For most white children the net effect is close to 0.23 (the typical estimate of the coefficient δ_2 in our simple two-factor IGE model), since on average about 95% of the parents in their neighborhoods are white. For Black children, however, the net effect is lower because the typical share of Black families in their neighborhoods is only about 60%.

Returning to Table 6, the model in column 6 extends the model in column 3 by replacing the mean wage of neighboring parents with a set of unrestricted tract fixed effects. This leads to a slightly larger estimate of the Black wage gap effect (-10.9% versus -9.6% in column 3), implying that all the other neighborhood level factors that impact Black children are slightly more favorable in the average neighborhoods of Black children than is indicated by the level of average parental wages. This is consistent with the positive coefficient for the neighborhood fraction Black in Table 3, column 5.

Table 7 presents a further analysis of the differences in IGE models for Black and white children. Here we estimate models separately by race, including models that break out

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[40] The two specifications differ somewhat: Table 4 includes controls for child gender and the neighborhood racial composition, while Table 6 does not.

[41] Appendix Table 9 also includes specifications limited to the subsample of children in integrated neighborhoods, where characteristics of own-race and different-race neighbors can be measured separately. Results are broadly similar.

children's wages into their person and firm effect components, and models that break out parent and neighboring parent wages in the same way. All the models in this table also include a control for the fraction of Black children in the Census tract. Focusing first on the estimates in Panel A (which have own-parent and neighboring parent wages on the right hand side), we see in columns 1 and 4 that the effect of own-parent wages are similar for white and Black children, but the effect of neighboring parents' mean wages is smaller for Black children (as in the interacted model in column 5 of Table 6). Interestingly, the effect of a higher share of Black children in the tract is positive and of a very similar magnitude for both race groups. There is no indication that a higher fraction of Black children in a neighborhood leads to a problem for white or Black children.

Columns 2 and 5 present models in which the dependent variable is the person effect in children's wages for white and Black children, respectively, while columns 3 and 6 present parallel models for the mean firm effects of the two groups. As expected, the models for white children are very similar to the models for the pooled sample (in columns 2 and 3 of Table 4). For Black children the model for the firm effect component of wages is similar to the model for whites, but the model for the person effect component shows a lower effect of neighboring parent wages.

The models in Panel B include the AKM components of own-parent and neighboring parent wages. The own-parent wage components have very similar effects on child wages (and on the components of child wages) for both race groups. The biggest difference between races is in the transmission of the person effect component of neighboring parent wages, which has a larger effect on the person effects of white children (coefficient= 0.185) than Black children (coefficient =0.129). Interpreting the person effects as measures of human capital, it appears that Black children's human capital accumulation is less affected by the human capital of neighboring parents – perhaps because many of their neighbors are white and race stands in the way of potential human capital spillovers between families.

We have shown that differences in neighborhood quality account for about one-third of the Black-white gap in children's earnings conditional on parents' own earnings. This motivates a final analysis where we explore how differences in the degree of sorting between tracts in different CZ's lead to differences in the quality of the neighborhoods experienced by Black and white children. We begin in Panel A of **Table 8** by dividing CZs into three groups based on Christensen et al.'s (2021) measure of racial discrimination in the local housing market. This is the difference in call-back rates for inquiries about rental properties from potential renters with "white" versus "Black" names, and is designed to measure the ability for Black families to have fair access to the local housing market. In the first three columns, we present separate regressions of neighborhood quality (as measured by the tract fixed effects that we used as dependent variables in Table 3) on parental wages and race. While the parental wage-neighborhood quality relationship does not vary much across the groups of CZs, the Black coefficient is noticeably more negative in the more discriminatory CZs. This confirms evidence reported by Christensen et al. (2021) that disparate treatment by property owners and their agents limits Black families' access to high-quality neighborhoods.

In columns 5-7, we take as the dependent variable mean log wages of children raised in a tract, and again regress this on mean log parental wages and an indicator for Black race. Again, we see that the effect of own-parent wages on children's wages is fairly similar in the 3 groups of CZ's, but the racial gap in children's outcomes is larger in the more discriminatory CZs, consistent a gap in access to neighborhood quality.

Panel B explores an alternative way of classifying CZs, by the degree of economic segregation. In places where high- and low-wage families tend to live in the same neighborhoods, one would expect lower-wage and Black families to have better access to neighborhood quality than in places with greater stratification. We thus divide CZs into three groups based on CZ-specific estimates of r_p, the share of the variance of parent wages that is across neighborhoods (or, equivalently, the coefficient of a regression of neighborhood mean wages on family wages), which we interpret as a measure of economic segregation.

As expected, the models in columns 1-3 show that relationship between parental wages and neighborhood quality is stronger in the more segregated CZs.[42] The racial gap in

[42] This is mechanical: r_p equals the coefficient of a regression of $\bar{y}^F_{n(i)}$ on y^F_i, so given the results in Table 3 is very closely related to the coefficient of a regression of the tract fixed effect on y^F_i.

neighborhood quality is also larger, suggesting that in more economically segregated CZ's Black families tend to live in tracts with lower "quality" – as indexed by the tract fixed effect in child outcomes. In columns 5-7, where the dependent variable is the mean log wage of a child, we do not see a stronger reduced-form IGE in the more segregated cities (contrary to expectations based on equation 5, above). Nevertheless, we do see the racial gap in children's wages is larger in the cities with more income segregation.

V. Conclusions

We have presented new estimates of the intergenerational elasticity of wages from population administrative data for the United States. While there have been prior estimates of similar IGEs (e.g., Mazumder 2005; Chetty et al., 2014; Chetty and Hendren 2018a), our use of quarterly data on earnings, in a setting where we can isolate full-time workers, is unique.

We make several innovations relative to the long literature on intergenerational transmission. First, we highlight the important role of neighborhoods. We propose a simple single-variable summary of the aspects of neighborhoods that matter for intergenerational transmission: mean parental wages in the neighborhood. We show that the slope of the between-neighborhood relationship between parent and children's wages is more than two times larger than the slope of the within-neighborhood relationship, even in a 2SLS specification that eliminates bias due to measurement error in individual parent earnings.

Although in principle there could be multiple dimensions of a neighborhood that matter for the long run wage outcomes of children raised there, in practice we find that average wages of parents in the neighborhood explain over 80% of the variation in unrestricted neighborhood effects from a model that also controls for own-parent wages – i.e., differences in what Chetty et al. (2014) have called the rate of "absolute mobility". Moreover, many of the other characteristics that have traditionally been used to characterize neighborhood quality -- such as the single parent share, home prices, or the racial composition of the neighborhood -- have very small or opposite-signed relationships to child outcomes once average parental wages are controlled.

Second, we use an AKM decomposition to separate parents', and neighbors' wages into person effects and establishment premiums. We show that both the person effects and the establishment premiums earned by parents are transmitted to children, though the impact of the person effects is larger. This is consistent with the idea that omitted parental characteristics (such as human capital or motivation) are transmitted to children, creating a positive omitted variables bias in simple IGE estimates, and that these variables are more strongly correlated with the parent's person effect than with his or her workplace wage premiums. We see a similar pattern for neighboring parent wages: Neighbors' firm premiums are transmitted to children's earnings, though not as strongly as are neighbors' average person effects.

When we further decompose children's wages into person effects and wage premiums, we find that both components are related to the parent and neighbor AKM components, in approximately a block diagonal structure: Parent and neighboring parents' person effects are mainly transmitted to children's person effects, and parent and neighboring parents' firm effects are mainly transmitted to children's firm effects, but off-diagonal effects (e.g., of parent and neighbor person effects on children's firm effects) are relatively small.

The transmission of parents' firm effects is not driven by outcomes for children who work at the same firm as their parents, but the neighborhood firm effect component is largely captured by children working at firms where other parents in the neighborhood work. This strongly suggests that network-driven access to good jobs is a major source of children's advantage and disadvantage. There is also some evidence of a "resource trap": Children who grow up in neighborhoods where many of the adults work in high-wage industries (including extraction industries like mining) end up being more likely to work in high-wage industries but have lower person effects themselves, perhaps due to lower investment in human capital.

Finally, we explore the implications of our analyses for the Black-white gap in children's earnings. As in past work, we find that the IGE is similar for Black and white children, but that Black children have lower wages than do white children from families with similar parental wages. We find that approximately one-third of this gap is attributable to neighborhood quality, as captured by average neighborhood wages. If anything, a higher neighborhood share of Black families has positive effects on both Black and white children, once we control for neighborhood wages. Moreover, we find that Black-white gaps in neighborhood quality and in children's outcomes, conditional on parents' own earnings, are larger in commuting zones where housing markets are more discriminatory and where neighborhoods are more segregated, suggesting that access to good neighborhoods is still a problem in the U.S. today.

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Figure 1. Parent and child log earnings

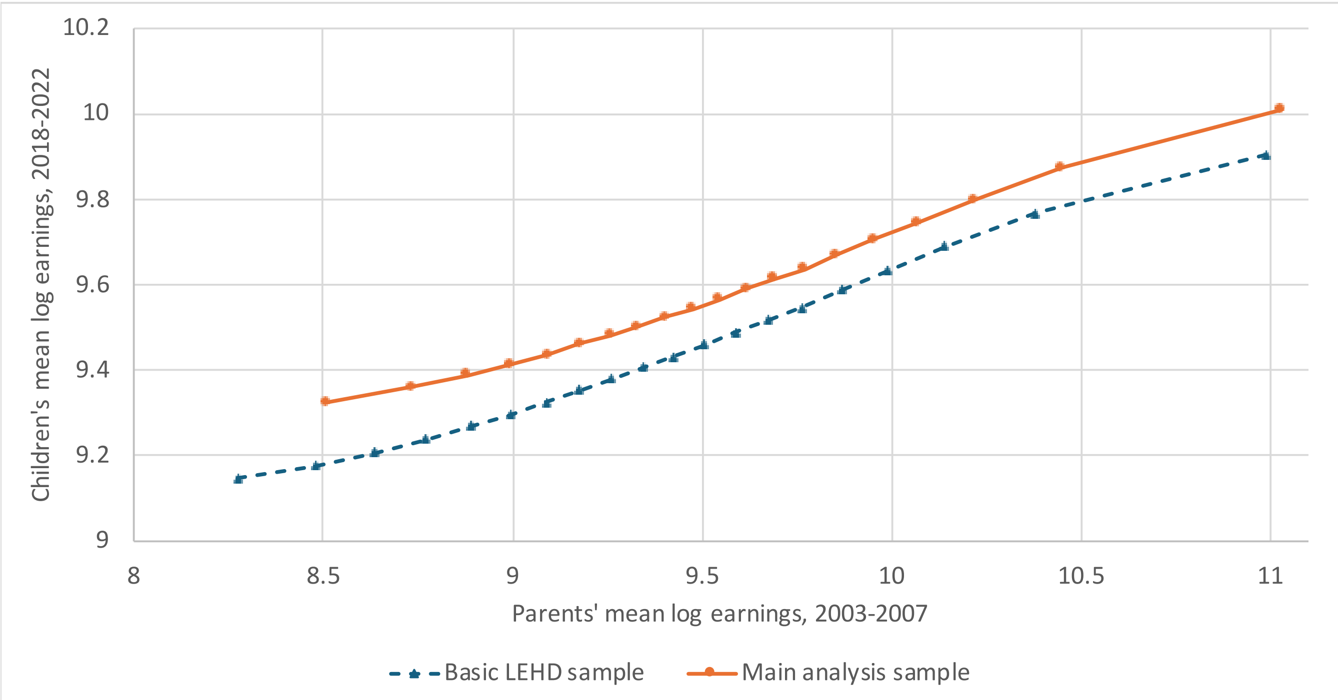
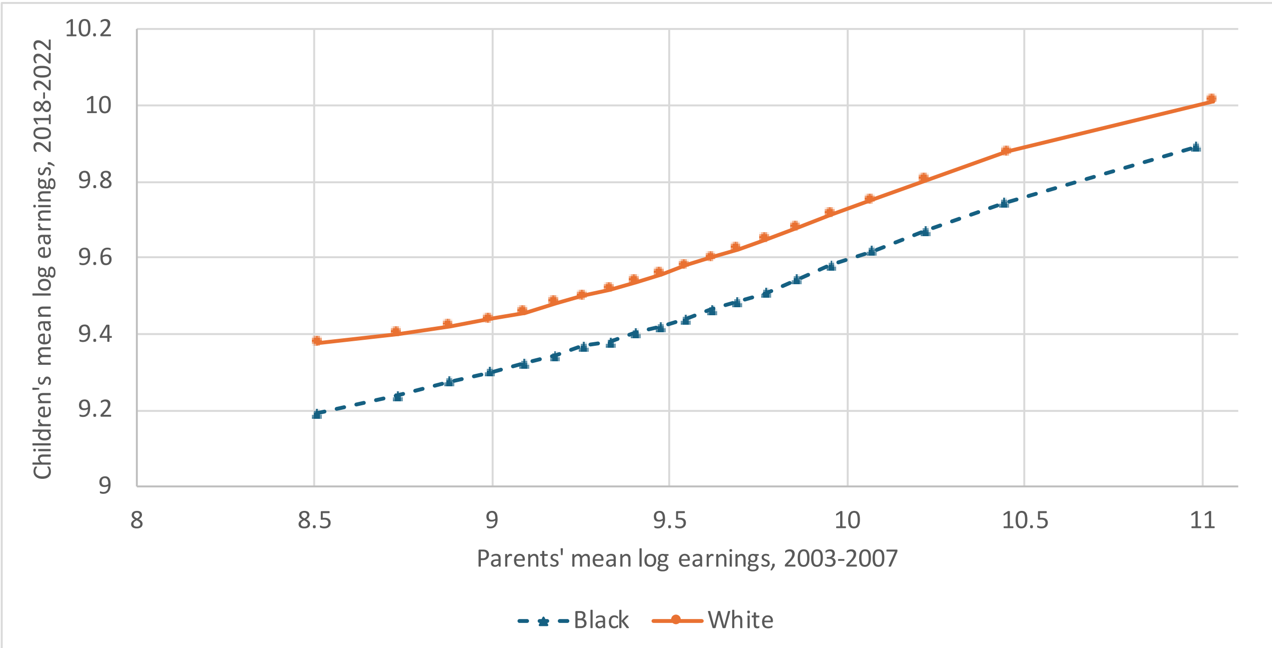


Figure 2. Parent and child earnings, by race

Panel A. Log-log scale



Panel B. Rank-rank scale

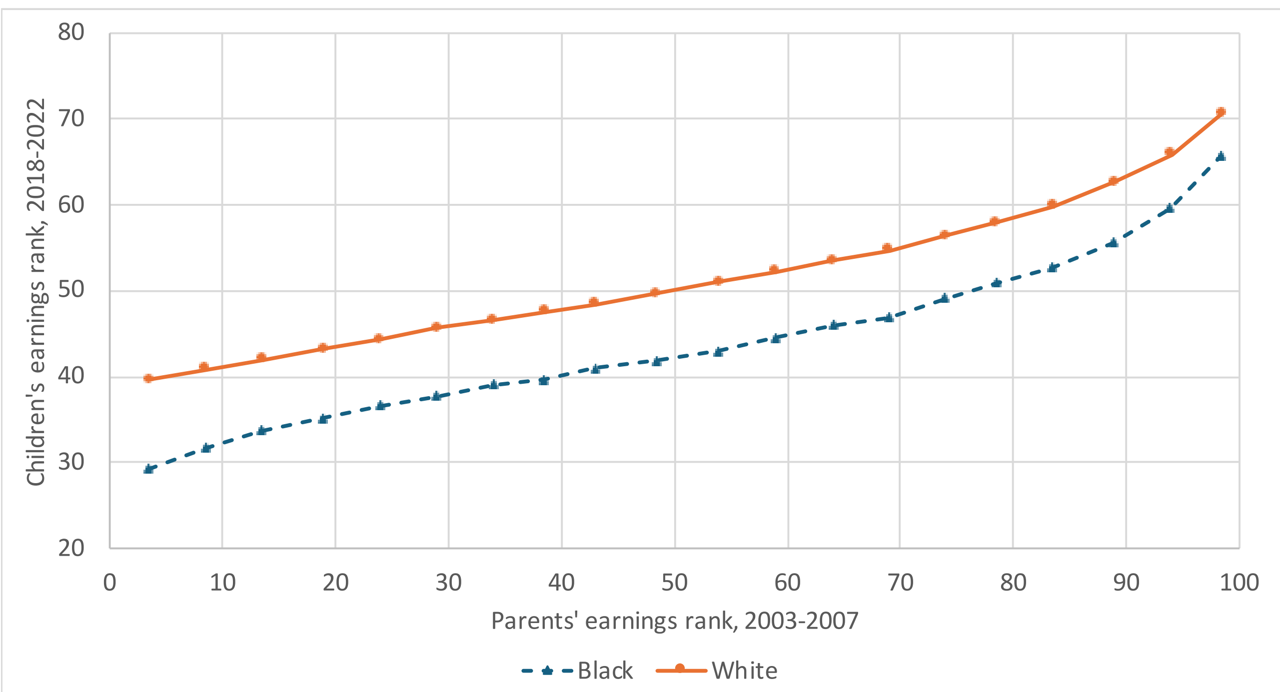


Figure 3. Within- and between-neighborhood income transmission

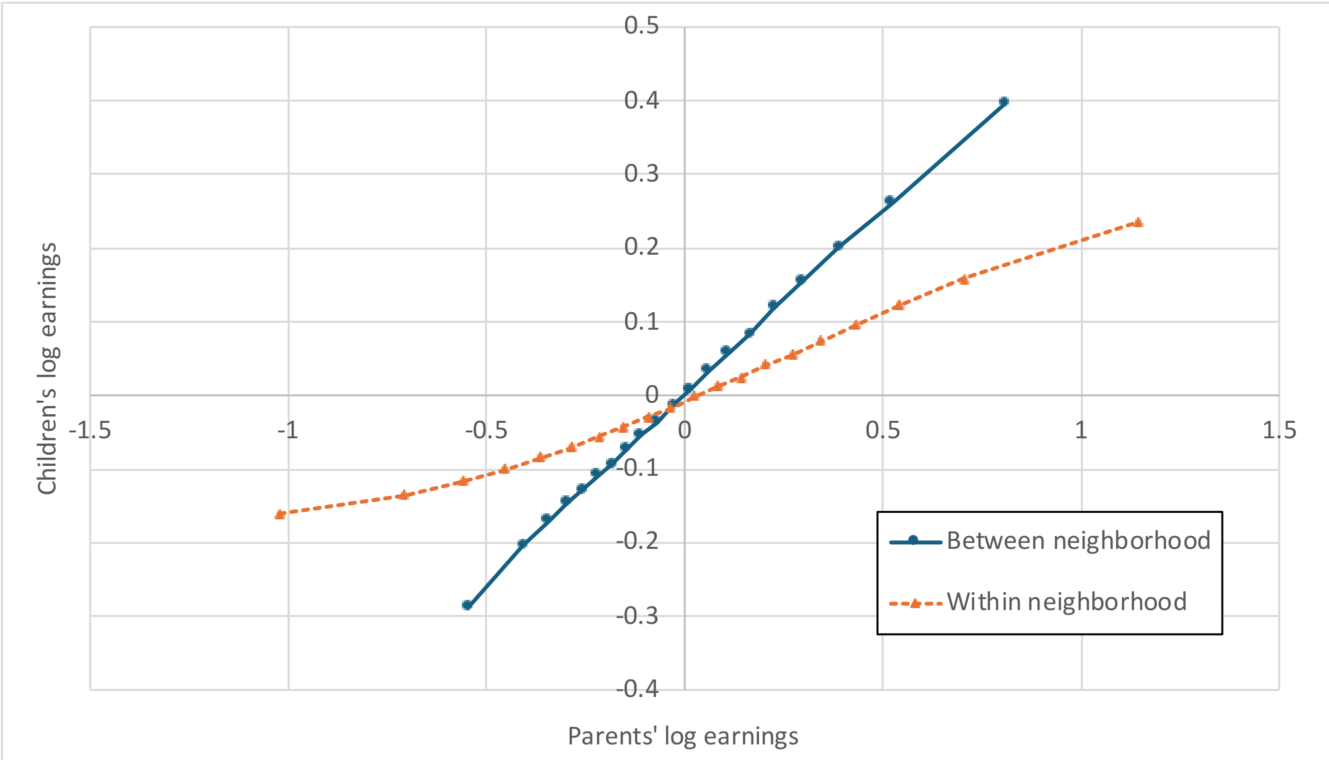


Table 1: Characteristics of potentially matched families in 2000 and earnings of parents and children in main sample

	Households in 2000 Census with child 9-17 (public use samples)		Households in main estimation sample (Census+LEHD)
	All households with child 9-17	Child 9-17, parent earned ≥$15,200 & worked ≥40 weeks	
	(1)	(2)	(3)
A. Characteristics of household in 2000:			
Married parents	0.720	0.797	0.802
Parent education	13.7	14.0	14.0
Parent worked last year	0.933	1.000	0.992
Parent weeks worked last year	46.0	51.4	50.2
Parent hours/week last year	42.2	46.3	45.8
Earnings of highest earning parent in 1999	44,482	55,011	51,260
Parent earned ≥$13,200, worked ≥40 weeks	0.778	1.000	--
Black (child)	0.192	0.149	0.106
Moved in last five years?	0.418	0.403	--
B. LEHD earnings			
Earnings of Main Parent in 2003-2007			
Mean log quarterly earnings			9.553
Exp(Mean log quarterly earnings) x 4			69,640
Earnings of Main Parent in 2010-2014			
Mean log quarterly earnings			9.693
Exp(Mean log quarterly earnings) x 4			82,320
Earnings of Child in 2018Q2-2022Q2 (excluding 2022Q2-Q4)			
Both Genders:			
Mean log quarterly earnings			9.583
Exp(Mean log quarterly earnings) x 4			58,064
Males only (52.6% of sample)			
Mean log quarterly earnings			9.681
Exp(Mean log quarterly earnings) x 4			64,042
Females only (47.4% of sample)			
Mean log quarterly earnings			9.474
Exp(Mean log quarterly earnings) x 4			52,067

Note: Sample in column 1 includes households with white and Black non-Hispanic children age 9-17 in 2000 Census public use sample who are children or step children of household head. Sample in column 2 limits attention to households where the highest-earning parent earned at least $15,200 in 1999 and worked 40+ weeks. Sample in column 3 is our main estimation sample. To be included the household must have at least one parent with a PIK who has earnings in the LEHD in the 2003-2007 and 2010-1014 periods that satisfy our sample restrictions in each 5-year interval. In addition the child must have a PIK and have earnings in the LEHD in the period 2018-2022 that satisfy our sample restrictions. For families with two parents with PIKS, the main parent whose earnings are reported from LEHD is the one with the highest average quarterly earnings in 2003-2007. See text for additional details.

Table 2: Models for determinants of mean log earnings of children

	Models with CZ effects				Models with Tract Effects		Models with controls for earnings in surrounding tracts & CZ effects	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean log parent earnings (2003-2007). ***Instrumented***	0.295		0.232	0.229	0.230	0.227	0.232	0.229
	(0.002)		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Tract average log parent earnings (2003-2007)		0.486	0.254	0.223			0.227	0.208
		(0.005)	(0.006)	(0.005)			(0.004)	(0.003)
Child is Black race				-0.085		-0.098		-0.083
				(0.005)		(0.003)		(0.005)
Child is male				0.202		0.203		0.202
				(0.003)		(0.003)		(0.003)
Single parent in 2000				0.016		0.014		0.016
				(0.001)		(0.001)		(0.001)
Both parents have earnings in 2003-2007				-0.028		-0.026		-0.028
				(0.002)		(0.002)		(0.002)
Avg. log earnings of parents in surrounding tracts							0.045	0.025
							(0.005)	(0.004)
Adjusted R-squared	0.112	0.096	0.128	0.163	0.134	0.169	0.127	0.163

Notes: Robust standard errors in parentheses. Dependent variable in all models is mean of the log of child earnings in 2018Q2 to 2022Q2 (excluding 2020 Q2-Q4), deviated from the 25th percentile of the distribution. Sample includes white and Black non-Hispanic children age 9-17 in 2000 Census who have a PIK and earnings in the LEHD in the period 2018-2022, and lived with a parent (or parents) in 2000 who have a PIK and have earnings in the LEHD in 2003-2007 and 2010-14. See text for additional details of sample. Sample size is approximately 4.91 million observations (4.89 million in columns 7-8); mean of dependent variable is 0.395. All models fit by two stage least squares, using mean log earnings of parent in 2010-14 as an instrumental variable for mean log earnings in 2003-7.

Table 3: Determinants of the tract effects in children's subsequent earnings

	Dep. variable: estimated tract effects from model in Table 2, column 6						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Tract average log parent earnings (2003-2007)	0.220		0.127		0.233	0.222	0.219
	(0.004)		(0.005)		(0.005)	(0.005)	(0.005)
Mean education in tract (from 2000 Census)		0.060	0.029				
		(0.001)	(0.001)				
Fraction of Black children in tract (from main analysis sample)				-0.142	0.031	0.049	0.052
				(0.008)	(0.010)	(0.011)	(0.010)
Fraction of male children in tract (from main analysis sample)				-0.098	-0.051	-0.057	-0.055
				(0.008)	(0.008)	(0.008)	(0.008)
Employment rate in tract in 2000 (from 2000 Census)						-0.016	-0.019
						(0.021)	(0.018)
Share single parents in tract in 2000 (from main analysis sample)						-0.061	-0.126
						(0.009)	(0.019)
Share parents in tract own their own home (from 2000 Census)							-0.090
							(0.018)
Mean log of home values in tract (from 2000 Census)							0.003
							(0.001)
Adjusted R-squared	0.676	0.669	0.693	0.456	0.679	0.681	0.682
R-squared corrected for sampling error in estimated tract effects	0.840	0.831	0.861	0.566	0.844	0.845	0.848

Notes: Robust standard errors in parentheses. Dependent variable in all models is the estimated tract effect assigned to a child from the first step model reported in column 6 of Table 2. Models are fit at the individual child level. See text for description of corrected R-squared reported in bottom row.

Table 4: Intergenerational transmission of AKM-based components of parent and child earnings

	Mean log child earnings	AKM decomp. of mean child earnings: Person effect	Mean firm effect	Decomposition of mean firm effect in child earnings: CZ effect	Industry effect	Firm effect net of CZ and industry	Indicator for ever work at parent firm	Mean firm effect for subset who: Never work at parent firm	Ever work at parent firm
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel a: using earnings of parent and neighboring parents									
Mean log parent earnings	0.225	0.190	0.034	0.008	0.009	0.017	0.010	0.033	0.084
(2003-2007). ***Instrumented***	(0.003)	(0.003)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.002)
Tract average log parent	0.234	0.199	0.032	0.014	0.002	0.016	-0.022	0.035	-0.088
earnings (2003-2007)	(0.005)	(0.004)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.004)
Adjusted R-squared	0.163	0.145	0.097	0.516	0.051	0.012	0.004	0.098	0.100
Panel b: using person and firm components of parent and neighboring parent earnings									
Person effect in parent earnings	0.249	0.220	0.024	0.008	0.005	0.011	0.010	0.026	0.000
(2003-2007). ***Instrumented***	(0.004)	(0.003)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.002)
Mean firm effect in parent earnings	0.104	0.005	0.106	0.009	0.036	0.061	-0.030	0.082	0.966
(2003-2007). ***Instrumented***	(0.005)	(0.004)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.012)
Average person effect in tract mean	0.215	0.184	0.032	0.015	0.002	0.015	-0.022	0.033	0.002
parental earnings (2003-2007)	(0.005)	(0.004)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.004)
Average firm effect in tract mean	0.111	0.029	0.078	-0.012	0.028	0.062	0.026	0.087	-0.301
parental earnings (2003-2007)	(0.018)	(0.013)	(0.008)	(0.004)	(0.003)	(0.006)	(0.003)	(0.008)	(0.021)
Adjusted R-squared	0.164	0.147	0.097	0.517	0.051	0.012	0.004	0.098	0.004

Notes: See note to Table 2. Robust standard errors in parentheses. Dependent variable is indicated in column heading, and is based on child earnings in 2018Q2 to 2022Q2 (excluding 2020Q2-202Q4). All models include CZ fixed effects, indicators for a Black and a male child, and the fraction Black in our main analysis sample in the tract. All models are estimated by two stage least squares. In panel a, mean log earnings of parent in 2010-14 is used as an instrumental variable for mean log earnings of parent in 2003-7. In panel b, person effect and mean firm effect in parental earnings in 2010-14 are used as instrumental variables for person effect and mean firm effect in 2003-7. Sample size for columns 1-7 is approximately 4.91 million observations; in columns 8 and 9 it is 4.78 million and 0.13 million, respectively.

Table 5. Black-white gaps in child and parent earnings and neighborhood characteristics

	Without controls		With CZ FE's
	Constant (white mean)	Black-white Gap	Black-white Gap
	(1)	(2)	(3)
Panel A. Basic characteristics			
Child mean log earnings (2019-22)	9.609	-0.254	-0.283
	(0.012)	(0.007)	(0.009)
Parent mean log earnings (2003-7)	9.597	-0.416	-0.485
	(0.019)	(0.010)	(0.011)
Neighborhood average parental log earnings (2003-7)	9.584	-0.287	-0.343
	(0.019)	(0.013)	(0.017)
Neighborhood fraction Black	0.051	0.522	0.470
	(0.003)	(0.022)	(0.026)
Panel B. Components of AKM decompositions			
Child person effect (2018-22)	9.395	-0.252	-0.281
	(0.008)	(0.005)	(0.007)
Child average firm effect (2018-22)	0.173	0.003	0.003
	(0.005)	(0.004)	(0.002)
Parent person effect (2003-7)	9.424	-0.428	-0.489
	(0.014)	(0.009)	(0.011)
Parent average firm effect (2003-7)	0.150	-0.006	-0.018
	(0.007)	(0.005)	(0.002)
Neighborhood average parental person effect (2003-7)	9.409	-0.291	-0.337
	(0.014)	(0.014)	(0.017)
Neighborhood average parental firm effect (2003-7)	0.150	-0.007	-0.018
	(0.007)	(0.005)	(0.002)

Notes: Each row reports coefficeints from two separate regressions of the variable indicated in the row heading on a Black race indicator, first without controls (columns 1-2) and then with CZ fixed effects (column 3). Robust standard errors in parentheses. Sample size is approximately 4.91 million observations.

Table 6. Models for Black-white differences in children's earnings

	Mean log earnings of children, 2018-2022					
	Models with CZ effects					Model with tract effects
	(1)	(2)	(3)	(4)	(5)	(6)
Child is Black race	-0.283	-0.149	-0.096	-0.087	-0.112	-0.109
	(0.009)	(0.007)	(0.005)	(0.005)	(0.005)	(0.003)
Mean log parent earnings		0.275	0.227		0.226	0.225
(2007-2007), ***instrumented***		(0.002)	(0.003)		(0.003)	(0.003)
Person effect in parent earnings				0.251		
(2003-2007). ***Instrumented***				(0.004)		
Mean firm effect in parent earnings				0.107		
(2003-2007). ***Instrumented***				(0.005)		
Interaction: Black child and mean					0.010	
log parent income. ***Instrumented***					(0.004)	
Tract average log parent earnings			0.223		0.231	
(2003-2007)			(0.005)		(0.005)	
Tract average person effect in				0.205		
parent earnings (2003-2007).				(0.004)		
Tract average firm effect in				0.117		
parent earnings (2003-2007).				(0.019)		
Interaction: Black child and tract					-0.072	
average log parent earnings					(0.005)	
Adjusted R2	0.059	0.118	0.130	0.131	0.130	0.136

Notes: Robust standard errors in parentheses. Dependent variable in all models is mean of the log of child earnings in 2018Q2 to 2022Q2 (excluding 2020 Q2-Q4), deviated from the 25th percentile of the distribution. See notes to Table 2. All models fit by two stage least squares. Models in columns 2, 3, 5, and 6 use mean log earnings of parent in 2010-14 as an instrumental variable for mean log earnings in 2003-7. In column 4, person effect and mean firm effect in parental earnings in 2010-14 are used as instrumental variables for person effect and mean firm effect in 2003-7. In column 5, mean log earnings of parent in 2010-14 interacted with dummy for Black race is used as an instrumental variable for the interaction of mean log earnings in 2003-7 and Black race.

Table 7: Racial differences in earnings transmission

	White children			Black children		
		AKM decomp. of mean child earnings:			AKM decomp. of mean child earnings:	
	Mean log child earnings	Person effect	Mean firm effect	Mean log child earnings	Person effect	Mean firm effect
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: using earnings of parent and neighboring parents						
Mean log parent earnings	0.226	0.190	0.034	0.234	0.197	0.035
(2003-2007). ***Instrumented***	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)
Tract average log parent	0.235	0.200	0.032	0.173	0.144	0.020
earnings (2003-2007)	(0.005)	(0.004)	(0.002)	(0.006)	(0.005)	(0.002)
Fraction of Black children in tract	0.023	0.010	0.013	0.023	0.007	0.011
	(0.008)	(0.007)	(0.002)	(0.005)	(0.005)	(0.002)
Adjusted R-squared	0.114	0.099	0.074	0.096	0.078	0.081
Panel B: using person and firm components of parent and neighboring parent earnings						
Person effect in parent earnings	0.249	0.219	0.024	0.266	0.232	0.025
(2003-2007). ***Instrumented***	(0.004)	(0.004)	(0.001)	(0.004)	(0.003)	(0.001)
Mean firm effect in parent earnings	0.109	0.007	0.108	0.097	0.017	0.095
(2003-2007). ***Instrumented***	(0.005)	(0.004)	(0.002)	(0.011)	(0.009)	(0.005)
Average person effect in tract mean	0.216	0.185	0.032	0.148	0.129	0.015
parental earnings (2003-2007)	(0.005)	(0.004)	(0.002)	(0.006)	(0.006)	(0.002)
Average firm effect in tract mean	0.117	0.034	0.080	0.179	0.098	0.075
parental earnings (2003-2007)	(0.021)	(0.013)	(0.009)	(0.037)	(0.029)	(0.012)
Fraction of Black children in tract	0.017	0.004	0.015	0.019	0.005	0.009
	(0.007)	(0.006)	(0.002)	(0.005)	(0.004)	(0.002)
Adjusted R-squared	0.116	0.101	0.074	0.097	0.079	0.082

Notes: Robust standard errors in parentheses. Dependent variable is indicated in column heading, and is based on child earnings in 2018Q2 to 2022Q2 (excluding 2020Q2-202Q4). All models include CZ fixed effects, an indicator for a male child, and the fraction Black in our sample in the tract. All models are estimated by two stage least squares. In panel a., mean log earnings of parent in 2010-14 is used as an instrumental variable for mean log earnings of parent in 2003-7. In panel b, person effect and mean firm effect in parental earnings in 2010-14 are used as instrumental variables for person effect and mean firm effect in 2003-7. Sample size is approximately 4.39 million observations in columns 1-3 and 0.52 million observations in columns 4-6.

Table 8: Comparisons of sorting models for tract effect in child earnings between subsets of commuting zones

	Models for determinants of estimated tract effect for 3 subsets of CZ's			Difference, high-low CZs	Models for determinants of children's outcomes for 3 subsets of CZ's			Difference, high-low CZs
	Low	Medium	High		Low	Medium	High	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Classified by degree of racial discrimination in CZ								
Mean log parent earnings (2003-7)	0.061	0.065	0.052	-0.009	0.273	0.284	0.254	-0.020
Instrumented	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.007)	(0.008)
Child is Black race	-0.036	-0.029	-0.075	-0.039	-0.137	-0.125	-0.190	-0.053
	(0.009)	(0.004)	(0.018)	(0.020)	(0.012)	(0.005)	(0.023)	(0.026)
B. Classified by degree of income sorting to tracts in CZ								
Mean log parent earnings (2003-7)	0.030	0.054	0.061	0.031	0.275	0.276	0.273	-0.002
Instrumented	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)	(0.004)	(0.005)	(0.006)
Child is Black race	-0.015	-0.032	-0.047	-0.032	-0.110	-0.127	-0.151	-0.041
	(0.002)	(0.005)	(0.010)	(0.010)	(0.003)	(0.008)	(0.013)	(0.014)

Notes: Robust standard errors in parentheses. Each column and panel reports coefficients from a model of the effect of parental income and race on the estimated tract effect for the tract in which a family lives (columns 1-3) or on children's log earnings (columns 5-7), estimated separately for the subset of CZ's indicated by the column heading (low, medium or high) when CZ's are classified by the metric indicated in the panel heading. In Panel A, "low" discrimination CZs are those with a higher callback rates for African Americans relative to whites in Christensen et al.'s (2021, Figure SM3.2) audit study, and "high" discrimination CZs have lower relative callback rates. Models also include CZ effects and indicator for male child (not reported in table) and are estimated by two stage least squares. Tract effects in columns 1-3 are from the model in Table 2, column 8. See text for further discussion.

Appendix A. Measurement error in the neighborhood-augmented IGE

In this appendix, we demonstrate the claims made in the text about the impact of measurement error in family wages: That it leads to attenuation of the estimates of β_1 in equation (1) and γ_1 in equation (2), and to upward bias in the estimate of β_2 in (1).

We assume that (1) holds for underlying (permanent) wages, but that we observe a noisy measure, $\hat{y}_i^F$. We assume that we observe the neighborhood mean $\bar{y}_{n(i)}^F$ correctly, without error. This will be true if observed parent earnings differ from permanent earnings by a transitory error that averages to zero within neighborhoods.

Let $\hat{y}_i^F = y_i^F + u_i^F$, where u_i^F is measurement error that is orthogonal to both y_i^F and $\bar{y}_{n(i)}^F$ and independent of the error term in the child earnings equation (2). Let λ represent the within-neighborhood reliability of $\hat{y}_i^F$ as a measure of y_i^F:

$$\lambda \equiv \frac{V\left(y_i^F - \bar{y}_{n(i)}^F\right)}{V\left(\hat{y}_i^F - \bar{y}_{n(i)}^F\right)} = \frac{V\left(y_i^F - \bar{y}_{n(i)}^F\right)}{V\left(y_i^F - \bar{y}_{n(i)}^F\right) + V\left(u_i^F\right)} \in [0,1]. \qquad \text{(A1)}$$

The size of the coefficient λ depends on the error in y_i^F as a measure of permanent wages and on the degree to which families sort to neighborhoods on the basis of permanent wages. We suspect there is lot of signal in y_i^F for families with a main wage earner between 40 and 60 (which is the case for most of our families), so we believe λ is not too far below 1.

By traditional "shrinkage" logic,

$$\begin{aligned} E\left[y_i^F | \hat{y}_i^F, \bar{y}_{n(i)}^F\right] &= E\left[\bar{y}_{n(i)}^F + \left(y_i^F - \bar{y}_{n(i)}^F\right) | \hat{y}_i^F, \bar{y}_{n(i)}^F\right] \\ &= \bar{y}_{n(i)}^F + \lambda\left(\hat{y}_i^F - \bar{y}_{n(i)}^F\right) \\ &= (1-\lambda)\bar{y}_{n(i)}^F + \lambda \hat{y}_i^F. \end{aligned} \qquad \text{(A2)}$$

Incorporating (A2) into (2) yields an expression for child outcomes in terms of observed family and neighborhood wages:

$$y_i = \beta_0 + \delta_1 \hat{y}_i^F + \delta_2 \bar{y}_{n(i)}^F + e_i \qquad \text{(A3)}$$

where

$$\delta_1 = \beta_1 \lambda, \qquad \text{(A4a)}$$

$$\delta_2 = \beta_2 + \beta_1(1-\lambda). \qquad \text{(A4b)}$$

As shown in equations (A4a) and (A4b), measurement error in average parental earnings leads to a downward bias in δ_1 as an estimate of β_1 in equation (1) and an upward bias in δ_2 as an estimate of β_2. Interestingly, the <u>sum</u> of these coefficients, which is what would be estimated in a model that includes only $\bar{y}_n^F$, is $\delta_1 + \delta_2 = (\beta_1 + \beta_2)$, which is an unbiased estimate of the combined effects of parental and average neighborhood wages from (1).

We can also consider the implications of measurement error for the traditional IGE without neighborhood controls (i.e., for equation (2). Paralleling the discussion of equation (3), if $\bar{y}_n^F$ is omitted from (A3), the coefficient on $\hat{y}_i^F$ is $\delta_1 + \delta_2 r_y$, where $r_y = V(\bar{y}_i^F)/V(\hat{y}_i^F) \in [0,1]$ is the reliability of neighborhood average wages as a proxy for individual observed wages.[1] Thus, in the presence of measurement error in $\hat{y}_i^F$, the traditional IGE under-estimates the total effect $\beta_1 + \beta_2$.

On the other hand, if we estimate a purely ecological model, omitting y_i^F from (A3), the coefficient on $\bar{y}_{n(i)}^F$ identifies the total effect $\beta_1 + \beta_2$ without bias. This is because $\hat{y}_i^F$ equals $\bar{y}_{n(i)}^F$ plus orthogonal noise, so the omitted variables formula loads its entire effect onto the included variable.

As we discuss in the text, our preferred estimates use 2SLS, using an independent measure of $\hat{y}_i^F$ as an instrument for y_i^F in both (A3) and in a restricted version that excludes $\bar{y}_{n(i)}^F$. Assuming that measurement error in our two separate measures of y_i^F (drawn from two separate five-year periods, with a three-year gap in between) is independent, we should estimate the coefficients of (2) and (4) without bias. However, if the measurement error correction is not complete (e.g., if there is sufficient persistence in the shocks to transitory income that they are meaningfully correlated across our two windows) then we expect that the 2SLS estimates will not fully correct the biases discussed here.

Appendix B. Additional Results

Appendix Table 1 presents characteristics of individuals in the 2018-2022 ACS public-use samples who would have been between 7 and 14 years old in 2000. Samples exclude individuals who were born abroad and arrived in the U.S. after 2000. Column 1 presents statistics for all non-Hispanic children whose first reported race is either white or Black, and remaining columns separate this group by race (columns 2-3) and gender (columns 4-5). All means are weighted using ACS sample weights.

Appendix Table 2 presents basic IGE models, without the commuting zone fixed effects that are included in our main specifications. Columns 1-3 are estimated by OLS. Columns 4 and 5 repeat the models from columns 1 and 3, instrumenting for mean log parental earnings in 2003-2007 with the corresponding average from 2010-2014.

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[1] Under the assumption that $\bar{y}_i^F$ is measured without error, $r_y < r_p$ whenever $\lambda < 1$.

Appendix Table 3 presents OLS estimates of the specifications from Table 2. All specifications include CZ fixed effects; columns 5 and 6 further include tract fixed effects.

Appendix Table 4 present results that expand our main sample to include parents and children with less consistent labor force attachment. Results for our main sample are reported in columns 7-9. Columns 1-3 use our basic LEHD sample, described in the text – this requires that the parents have at least four quarters with positive earnings in 2003-2007, with at least one quarter with earnings of $3,800 or more, and that the child meets the same criteria in 2018-2023Q1. Within this sample, we average log earnings for the parent and for the child over all quarters with earnings above $3,300. Neighborhood mean log earnings in these columns are the average over everyone in the expanded sample. Columns 4-6 add an additional restriction that the parent meets the same employment and earnings requirements in 2010-2014 as well, and uses the same construction for the instrument. (In these columns, neighborhood averages are over all parents from the columns 1-3 sample, not excluding those without earnings in 2010-2014.)

Appendix Table 5 presents variance decompositions for parents' average log earnings in 2003-2007 and for children's average log earnings in 2018-2023Q1 (excluding Q2-4 of 2020) using the AKM specification described in the text. Columns 5-12 use the same specification, averaged to the tract or CZ level, to decompose tract or CZ mean log earnings.

Appendix Table 6 presents models aimed at exploring the sensitivity of the results to children who work at the same firm as their parent or as other parents in their Census tract. Column 1 repeats the specifications from Table 4, column 3, in which the dependent variable is the child's establishment effect (averaged over all establishments at which the child works). Label this variable $\bar{\psi}_i$. We then form two indicators: z_{1i} equals one if the child worked at any point in 2018-2023Q1 at an establishment where their parent worked at any point in 2003-2007, while z_{2i} equals 1 if $z_{1i} = 0$ *and* the child worked at a firm that any of their childhood neighbors' parents ever worked at in 2003-2007. This is relatively common – the mean of z_{1i} is 2.7% while the mean of z_{2i} is 7.9%. Columns 2, 3, and 4 repeat the specification from column 1 using as the dependent variables the products $\bar{\psi}_i z_{1i}$, $\bar{\psi}_i z_{2i}$, and $\bar{\psi}_i(1 - z_{1i})(1 - z_{2i})$, respectively. Because by construction z_{1i} and z_{2i} are mutually exclusive, the sum of these three variables is $\bar{\psi}_i$, so the coefficients in columns 2-4 decompose the coefficients in column 1 into portions operating through children who work at their parents' firms, children who work at neighbors' firms but not at their parents' firms, and others.

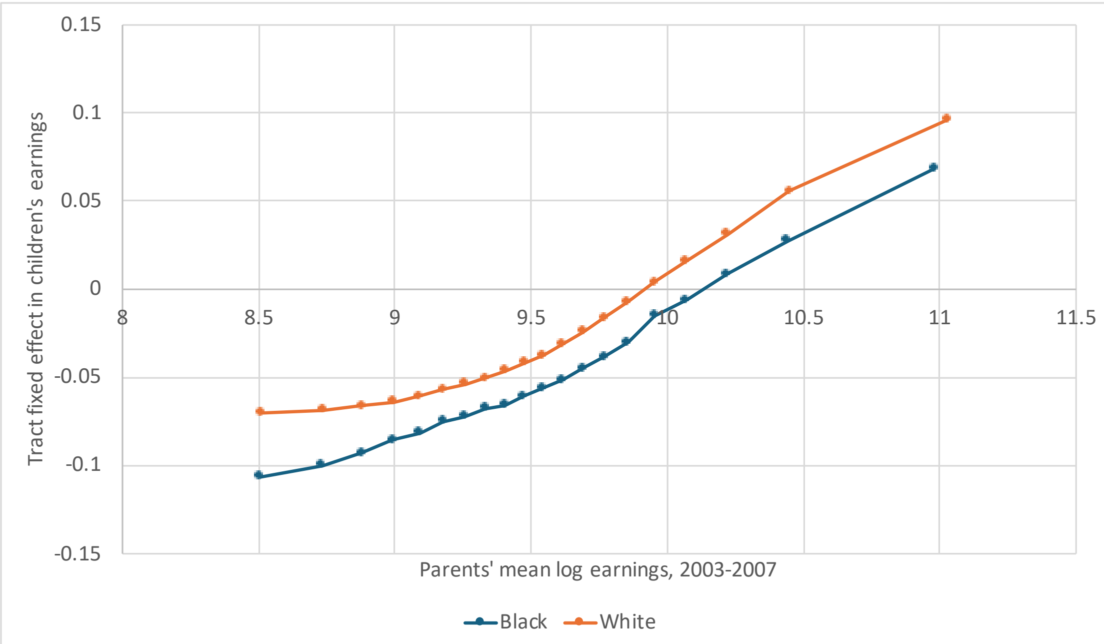
Appendix Table 7 presents variants of the specifications from Table 4, panel B. Here, separate parents' and neighbors' firm premiums into two components: Industry premiums, and the deviation of the firm premium from the industry premium.

Appendix Table 8 repeats the decompositions from Appendix Table 5, this time separating white and Black children and their parents.

Appendix Table 9 explores the possibility that neighborhood spillovers may be stronger among same-race neighbors than across racial groups. We begin with our basic specifications from Table 4, columns 1-3, but add to them the mean log earnings of same-race neighbors (in addition to the mean log earnings across all neighbors, which we retain in the specification). In Panel B, we use instead the mean person effects and firm effects of same-race neighbors. In these analyses, a positive coefficient on the same-race variables indicate that same-race neighbors have larger effects on children's earnings than other-race neighbors.

A concern in these specifications is that the same-race neighborhood average and overall neighborhood average are identical in segregated (single-race) neighborhoods, making the two variables strongly collinear. Columns 4-6 repeat the specifications from Panel A limiting to the subsample of neighborhoods that have non-zero shares of both white and Black children, while columns 7-9 further limit to neighborhoods with Black shares between 10% and 90%.

Appendix Figure 1. Neighborhood quality, by race



Appendix Table 1: Characteristics of Whites and Blacks Age 7-14 in 2000, Observed in 2018-22 ACS

	All (1)	White (2)	Black (3)	Male (4)	Female (5)
Overall Means					
Share female	0.497	0.492	0.515	0	1
Share Black	0.188	0	1	0.182	0.195
Share Immigrant	0.016	0.013	0.030	0.016	0.016
Years Education	14.2	14.4	13.4	13.9	14.5
Share Bachelor or Higher	0.410	0.446	0.255	0.365	0.455
Share Worked Past 12 mo.	0.857	0.869	0.806	0.886	0.828
Annual Hours Past 12 mo.	1,693	1,733	1,518	1,859	1,524
Annual Hours Past 12 mo. if worked	1,975	1,995	1,884	2,098	1,842
Share Full Time and Full Year	0.646	0.662	0.580	0.717	0.575
Wage and Salary Earnings in 2018$	42,853	45,869	29,853	49,979	35,628
Total Earnings in Past 12 mo. in 2018$	44,741	47,960	30,862	52,505	36,868
Total Income in Past 12 mo. in 2018 $	46,607	49,875	32,516	54,564	38,538
Share with wages ≥ $15,200 in 2018$	0.704	0.723	0.619	0.753	0.654
Means Among those with wages of least $15,200 in Past 12 mo.					
Share female	0.461	0.449	0.522	0.000	1.000
Share Black	0.166	0.000	1.000	0.147	0.188
Years Education	14.673	14.813	13.972	14.321	15.085
Share Bachelor or Higher	0.489	0.518	0.342	0.430	0.558
Annual Hours Past 12 mo.	2,148	2,159	2,092	2,232	2,049
Share Full Time and Full Year	0.857	0.859	0.848	0.890	0.819
Wage and Salary Earnings in 2018$	59,606	62,270	46,193	65,379	52,861
Total Earnings in Past 12 mo. in 2018$	60,234	62,969	46,460	66,211	53,249
Total Income in Past 12 mo. in 2018 $	61,579	64,408	47,334	67,727	54,395
Mean Log W/S Earnings in 2018$	10.788	10.830	10.578	10.869	10.694
Sample Size (overall)	1,017,263	882,593	134,670	516,411	500,852

Notes: Based on tabulations of American Community Survey (ACS) public use files for 2018, 2019, 2021 and 2022. Samples include only non-Hispanic individuals whose first reported race is either white or Black. Individuals born abroad who arrived in the U.S. after 2000 are excluded. Full time and full year workers are those who reported working 35 or more hours per week last year and 48 weeks or more. Means are weighted by ACS person weights.

Appendix Table 2: OLS and IV models for determinants of mean log earnings of children, without CZ effects

	OLS			IV	
	(1)	(2)	(3)	(4)	(5)
Mean log parent earnings (2003-2007).	0.289		0.198	0.319	0.236
	(0.004)		(0.003)	(0.004)	(0.003)
Tract average log parent earnings (2003-2007)		0.503	0.305		0.267
		(0.010)	(0.012)		(0.011)
Constant	0.276	0.396	0.314	0.263	0.298
	(0.006)	(0.005)	(0.005)	(0.006)	(0.005)
Adjusted R-squared	0.097	0.089	0.120	0.097	0.120

Note: see notes to Table 2. Models in columns 1-3 are estimated by OLS. Models in columns 4-5 are estimated by two stage least squares, using mean log parental earnings in 2010-2014 as an instrument for mean log parental earnings in 2003-2007.

Appendix Table 3: OLS models for determinants of mean log earnings of children

	Models with CZ effects				Models with Tract Effects		Models with controls for earnings in surrounding tracts & CZ effects	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean log parent earnings	0.266		0.198	0.193	0.198	0.192	0.198	0.193
(2003-2007)	(0.002)		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Tract average log parent		0.486	0.288	0.255			0.261	0.240
earnings (2003-2007)		(0.005)	(0.006)	(0.005)			(0.004)	(0.003)
Child is Black race				-0.087		-0.105		-0.086
				(0.005)		(0.003)		(0.005)
Child is male				0.202		0.203		0.202
				(0.003)		(0.003)		(0.003)
Single parent in 2000				0.003		0.001		0.002
				(0.001)		(0.001)		(0.001)
Both parents have earnings in				-0.025		-0.022		-0.025
2003-2007				(0.002)		(0.001)		(0.002)
Average log earnings of parents							0.045	0.025
in surrounding tracts							(0.005)	(0.004)
Adjusted R-squared	0.112	0.096	0.128	0.164	0.134	0.170	0.128	0.164

Notes: Specifications are the same as in Table 2 but are estimated by OLS rather than 2SLS.

Appendix Table 4: Sensitivity to exclusion of children and parents with low labor force attachment

	Basic LEHD sample (N=9.7m)			Subgroup with parental earnings in 2010-2014 (N=8.1m)			Main sample (N=4.9m)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. OLS									
Mean log parent earnings	0.278	0.201	0.190	0.280	0.204	0.192	0.266	0.198	0.193
(2003-2007).	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)
Tract average log parent		0.336	0.279		0.328	0.272		0.288	0.255
earnings (2003-2007)		(0.007)	(0.005)		(0.007)	(0.005)		(0.006)	(0.005)
Controls for race, gender, single parent, dual earner?	N	N	Y	N	N	Y	N	N	Y
Adjusted R-squared	0.117	0.138	0.172	0.115	0.135	0.169	0.112	0.128	0.164
Panel B. 2SLS									
Mean log parent earnings				0.311	0.239	0.230	0.295	0.232	0.229
(2003-2007). ***Instrumented***				(0.003)	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)
Tract average log parent					0.292	0.240		0.254	0.223
earnings (2003-2007)					(0.007)	(0.005)		(0.006)	(0.005)
Controls for race, gender, single parent, dual earner?				N	N	Y	N	N	Y
Adjusted R-squared				0.083	0.104	0.140	0.112	0.128	0.163

Notes: Robust standard errors in parentheses. Dependent variable in all models is mean of the log of child earnings in 2018Q2 to 2022Q2 (excluding 2020 Q2-Q4), deviated from the 25th percentile of the distribution. See text for explanation of main sample and basic parent and child sample, which includes many parents and children with few quarters of earnings. Columns 4-6 restrict the basic parent and child sample to exclude observations with no parental earnings observations in 2010-2014 (used to form our instrument).

Appendix Table 5: Comparison of Variance Decompositions of Two-Way Fixed Effects Models: Parents vs. Children

	Individual Level				Tract Level				CZ Level			
	Parents (2003-7)		Children (2018-23)		Parents (2003-7)		Children (2018-23)		Parents (2003-7)		Children (2018-23)	
	Std Dev or Correl.	Var. Share	Std Dev or Correl.	Var. Share	Std Dev or Correl.	Var. Share	Std Dev or Correl.	Var. Share	Std Dev or Correl.	Var. Share	Std Dev or Correl.	Var. Share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Wage/mean wage	0.579	1.000	0.536	1.000	0.318	1.000	0.183	1.000	0.172	1.000	0.109	1.000
Variance components												
Person effects	0.545	0.885	0.461	0.740	0.288	0.820	0.148	0.653	0.119	0.481	0.067	0.383
Firm effects	0.188	0.105	0.199	0.137	0.079	0.062	0.060	0.107	0.069	0.164	0.052	0.232
Covariate index	0.046	0.006	0.046	0.007	0.013	0.002	0.009	0.002	0.006	0.001	0.007	0.004
Residual	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Covariance components												
Person/firm	0.078	0.048	0.181	0.115	0.399	0.179	0.464	0.246	0.731	0.410	0.657	0.392
Person/covariates	-0.277	-0.041	0.000	0.000	-0.789	-0.056	-0.089	-0.007	-0.839	-0.037	-0.129	-0.011
Firm/covariates	-0.043	-0.002	-0.003	0.000	-0.339	-0.007	-0.029	-0.001	-0.713	-0.019	-0.014	-0.001

Notes: Table shows variance decompositions based on equation (8). Columns 1-4 pertain to the variance of (mean) individual earnings. Columns 5-8 pertain to the variance of mean tract earnings. Columns 9-12 pertain to the variance of mean earnings by CZ. Children's data for 2018-2023 exclude 2020 Q2-4 and include only Q1 of 2023. Entries in odd-numbered columns for "variance components" are estimated standard deviations of earnings component indicated in row heading; entries in odd-numbered columns for "covariance components" are the estimated correlations of the indicated variance components. Entries in even-numbered columns are variance shares explained by variance or covariance components.

Appendix Table 6: Distinguishing nepotism from networks: a further decomposition of the effect of parent and neighboring parent earnings on children's firm effects

	Mean child firm effect	Mean child firm effect × indicators for:		
		Ever work at parent's firm	Ever work at neighbor's firms but never parent's	Never work at parent's or neighbor's firms
	(1)	(2)	(3)	(4)
Share of children in category	100%	2.7%	7.9%	89.5%
Panel a: using earnings of parent and neighboring parents				
Mean log parent earnings	0.034	0.004	-0.001	0.031
(2003-2007). ***Instrumented***	(0.001)	(0.000)	(0.000)	(0.001)
Tract average log parent	0.032	-0.006	-0.004	0.042
earnings (2003-2007)	(0.002)	(0.000)	(0.000)	(0.002)
Adjusted R-squared	0.097	0.004	0.015	0.095
Panel b: using person and firm components of parent and neighboring parent earnings				
Person effect in parent earnings	0.024	0.001	-0.004	0.027
(2003-2007). ***Instrumented***	(0.001)	(0.000)	(0.000)	(0.001)
Mean firm effect in parent earnings	0.106	0.021	0.015	0.070
(2003-2007). ***Instrumented***	(0.002)	(0.001)	(0.001)	(0.003)
Average person effect in tract mean	0.032	-0.004	-0.008	0.045
parental earnings (2003-2007)	(0.002)	(0.000)	(0.001)	(0.002)
Average firm effect in tract mean	0.078	-0.003	0.073	0.007
parental earnings (2003-2007)	(0.008)	(0.001)	(0.005)	(0.009)
Adjusted R-squared	0.097	0.003	0.017	0.095

Notes: see note to Table 4. Dependent variable in column 1 is mean firm effect in children's earnings: models are the same as in column 3 of Table 4. Dependent variables in columns 2-4 are mean firm effect in children's earnings interacted with indicators for: (a) children who worked at least once at a firm their parent worked in 2003-2007 (column 2); (b) children who never worked at a firm their parent but worked, but worked at least once at a firm where one of the neighboring parents worked in 2003-2007 (column 3); (c) children who never worked at a firm that their parents or their neighboring parents worked (column 4).

Appendix Table 7: Distinguishing transmission of parent and neighbor industry versus firm effects

	Mean log child earnings	Decomp. of mean child earnings:		Decomposition of mean firm effect in child earnings:		
		Person effect	Mean firm effect	CZ effect	Industry effect	Firm effect net of CZ, industry
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Own parent earnings components:</u>						
Person effect in parent earnings	0.249	0.220	0.024	0.008	0.005	0.011
(2003-2007). ***Instrumented***	(0.004)	(0.003)	(0.001)	(0.000)	(0.000)	(0.000)
Mean industry effect in parent earnings	0.007	-0.079	0.093	-0.013	0.101	0.005
(2003-2007). ***Instrumented***	(0.008)	(0.006)	(0.002)	(0.001)	(0.001)	(0.002)
Mean parental firm effect relative to	0.152	0.046	0.112	0.021	0.004	0.087
industry mean. ***Instrumented***	(0.006)	(0.005)	(0.003)	(0.001)	(0.001)	(0.002)
<u>Neighboring parent earnings components:</u>						
Average person effect in tract mean	0.213	0.182	0.032	0.014	0.004	0.014
parental earnings (2003-2007)	(0.005)	(0.004)	(0.002)	(0.001)	(0.001)	(0.001)
Average industry effect in tract mean	0.002	-0.095	0.085	-0.124	0.158	0.052
parental earnings (2003-2007)	(0.066)	(0.047)	(0.022)	(0.008)	(0.008)	(0.014)
Mean tract firm effect relative to average	0.137	0.063	0.074	0.024	-0.009	0.059
industry effect in mean tract earnings	(0.028)	(0.018)	(0.012)	(0.005)	(0.003)	(0.009)
Adjusted R-squared	0.165	0.148	0.097	0.518	0.056	0.011

Notes: See notes to Table 4 and text for discussion of industry effect components in own parent and neighboring parent earnings. Person effect of parent earnings in 2010-14 is used as an instrumental variable for person effect of parent earnings in 2003-7. Mean industry effect in parental earnings in 2010-14 is used as an instrumental variable for mean industry effect in parental earnings in 2003-7. Deviation of mean firm effect from mean industry effect in parental earnings in 2010-14 is used as an instrumental variable for deviation of mean firm effect from mean industry effect in parental earnings in 2003-7.

Appendix Table 8: Comparison of Variance Decompositions of Two-Way Fixed Effects Models: By Race

	Individual Level				Tract Level				CZ Level			
	Parents (2003-7)		23)		Parents (2003-7)		23)		Parents (2003-7)		23)	
	Std Dev or Correl.	Var. Share	Std Dev or Correl.	Var. Share	or Correl.	Var. Share	Std Dev or Correl.	Var. Share	Std Dev or Correl.	Var. Share	Std Dev or Correl.	Var. Share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: White Children & Their Parents												
Wage/mean wage	0.577	100%	0.537	100%	0.309	100%	0.176	100%	0.190	100%	0.115	100%
Variance components												
Person effects	0.542	88%	0.460	73%	0.277	80%	0.137	61%	0.139	53%	0.076	43%
Firm effects	0.190	11%	0.201	14%	0.080	7%	0.061	12%	0.070	14%	0.053	21%
Covariate index	0.046	1%	0.046	1%	0.013	0%	0.009	0%	0.006	0%	0.007	0%
Residual	0.000	0%	0.000	0%	0.000	0%	0.000	0%	0.000	0%	0.000	0%
Covariance components												
Person/firm	0.074	5%	0.185	12%	0.422	19%	0.509	28%	0.715	39%	0.606	37%
Person/covariates	-0.263	-4%	0.002	0%	-0.772	-6%	-0.104	-1%	-0.854	-4%	-0.148	-1%
Firm/covariates	-0.041	0%	-0.003	0%	-0.343	-1%	-0.027	0%	-0.721	-2%	-0.009	0%
Panel B: Black Children & Their Parents												
Wage/mean wage	0.470	100%	0.465	100%	0.268	100%	0.182	100%	0.130	100%	0.089	100%
Variance components												
Person effects	0.425	82%	0.401	75%	0.231	74%	0.147	65%	0.077	36%	0.052	33%
Firm effects	0.168	13%	0.170	13%	0.087	11%	0.068	14%	0.069	28%	0.048	29%
Covariate index	0.045	1%	0.051	1%	0.015	0%	0.014	1%	0.004	0%	0.007	1%
Residual	0.000	0%	0.000	0%	0.000	0%	0.000	0%	0.000	0%	0.000	0%
Covariance components												
Person/firm	0.151	10%	0.178	11%	0.363	20%	0.332	20%	0.633	40%	0.609	38%
Person/covariates	-0.291	-5%	-0.024	0%	-0.471	-5%	-0.007	0%	-0.510	-2%	-0.067	-1%
Firm/covariates	-0.065	0%	-0.007	0%	-0.190	-1%	-0.025	0%	-0.462	-2%	-0.071	-1%

Notes: See note to Appendix Table 5.

Appendix Table 9: Distinguishing racial groups within neighborhoods

	Main sample			Minimally integrated neighborhoods			Integrated neighborhoods		
	Mean log child earnings	AKM decomp. of mean child earnings:		Mean log child earnings	AKM decomp. of mean child earnings:		Mean log child earnings	AKM decomp. of mean child earnings:	
		Person effect	Mean firm effect		Person effect	Mean firm effect		Person effect	Mean firm effect
	(1)	(2)	(3)	(2)			(2)		
Panel A: using earnings of parent and neighboring parents									
Mean log parent earnings	0.224	0.189	0.034	0.222	0.187	0.034	0.228	0.192	0.034
(2003-2007). ***Instrumented***	(0.003)	(0.003)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Tract average log parent	0.165	0.134	0.032	0.169	0.138	0.031	0.102	0.077	0.024
earnings (2003-2007)	(0.009)	(0.007)	(0.003)	(0.009)	(0.007)	(0.003)	(0.011)	(0.009)	(0.003)
Tract average log parent earnings	0.071	0.066	0.001	0.073	0.069	0.000	0.115	0.109	0.002
among same-race neighbors	(0.007)	(0.006)	(0.002)	(0.007)	(0.006)	(0.002)	(0.009)	(0.008)	(0.003)
Adjusted R-squared	0.163	0.145	0.097	0.163	0.149	0.096	0.143	0.136	0.093
Panel B: using person and firm components of parent and neighboring parent earnings									
Person effect in parent earnings	0.249	0.219	0.024						
(2003-7). ***Instrumented***	(0.004)	(0.003)	(0.001)						
Mean firm effect in parent earnings	0.104	0.005	0.106						
(2003-7). ***Instrumented***	(0.005)	(0.004)	(0.002)						
Average person effect in tract mean	0.153	0.126	0.030						
parental earnings (2003-7)	(0.009)	(0.007)	(0.003)						
Avg person effect in tract mean parental	0.063	0.059	0.003						
earnings among same-race	(0.007)	(0.006)	(0.003)						
neighbors (2003-7)									
Average firm effect in tract mean	0.102	0.008	0.093						
parental earnings (2003-7)	(0.022)	(0.016)	(0.009)						
Avg firm effect in tract mean parental	0.010	0.022	-0.016						
earnings among same-race	(0.015)	(0.013)	(0.007)						
neighbors (2003-7)									
Adjusted R-squared	0.165	0.147	0.097						

Notes: Robust standard errors in parentheses. Dependent variable is indicated in column heading, and is based on child earnings in 2018Q2 to 2022Q2 (excluding 2020Q2-202Q4). All models include CZ fixed effects, indicators for a Black and a male child, and the fraction Black in our sample in the tract. All models are estimated by two stage least squares. In panel a., mean log income of parent in 2010-14 is used as an instrumental variable for mean log income of parent in 2003-7. In panel b, person effect and mean firm effect in parental earnings in 2010-14 are used as instrumental variables for person effect and mean firm effect in 2003-7. Sample size for models in columns 1-3 is approximately 4.91 million observations.