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# THE OPPORTUNITY ATLAS: MAPPING THE CHILDHOOD ROOTS OF SOCIAL MOBILITY

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The Opportunity Atlas: Mapping the Childhood Roots of Social Mobility
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## ABSTRACT

We construct a public atlas of mean outcomes in adulthood by childhood Census tract. Outcomes vary sharply across neighborhoods: for children whose parents earn $27,000, the standard deviation of mean household income in adulthood is $10,420 across tracts within counties. Only half the variation in outcomes is explained by traditional measures of neighborhood opportunity like poverty rates. Experimental and quasi-experimental estimates indicate that 60% of the variation in outcomes across neighborhoods is driven by causal effects. We demonstrate how our statistics can be applied to better target policies to improve low-opportunity areas and help families move to affordable high-opportunity areas.

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Atlas Interactive Map is available at https://www.opportunityatlas.org
Tract-Level Data is available at https://opportunityinsights.org/data/

# I Introduction

Recent experimental and quasi-experimental studies have established that the neighborhood in which a child grows up has substantial causal effects on his or her prospects of upward income mobility (Chetty et al. 2016; Chetty and Hendren 2018a; Chyn 2018; Deutscher 2018; Laliberté 2018; Nakamura et al. 2022). The emerging consensus that neighborhoods play a key role in shaping children's outcomes (Sharkey 2016, Chyn and Katz 2021) raises a natural question: which neighborhoods in the United States currently offer the best and worst opportunities for children? Prior research on place effects has not answered this question because it focuses either on a small subset of neighborhoods (e.g., the Moving to Opportunity Experiment) or on coarse geographies such as counties and commuting zones (e.g., Chetty et al. 2014).

In this paper, we construct a publicly available dataset – which we term the *Opportunity Atlas* – that provides the first comprehensive estimates of children's long-term outcomes across neighborhoods in the U.S. We report estimates for children who grew up in each Census tract, small geographic units that have a population of 4,250 people on average. Our statistics differ from traditional indicators of neighborhood conditions based on cross-sectional data – such as rates of poverty or crime – by tracing the roots of such outcomes back to the neighborhoods in which children *grew up* (rather than where they live currently).

We construct the tract-level statistics using an individual-level panel dataset from the U.S. Census Bureau that covers virtually the entire American population from 1989-2015. Following Chetty et al. (2020), we use de-identified data from the 2000 and 2010 decennial Censuses linked to data from federal income tax returns and the 2005-2015 American Community Surveys to obtain information on children's outcomes in adulthood and their parents' characteristics. We focus in our baseline analysis on children in the 1978-1983 birth cohorts who were born in the U.S. or are authorized immigrants who came to the U.S. in childhood. Our primary analysis sample consists of 20.5 million children, approximately 96.2% of the total number of children in the birth cohorts we study.

We construct tract-level estimates of children's incomes in adulthood and other outcomes such as incarceration rates and teenage birth rates by race, gender, and parents' household income level – the three dimensions on which we find children's outcomes vary the most. We assign children to locations in proportion to the amount of their childhood they spent growing up in each Census tract. In each tract-by-gender-by-race cell, we estimate the conditional expectation of children's

outcomes given their parents' household income using a univariate regression whose functional form is chosen based on estimates at the national level to capture potential non-linearities.

We provide the tract-level data for public use both in the form of downloadable datasets and in an interactive mapping tool that facilitates visualization of the spatial patterns. We illustrate how these data can be used to understand how neighborhoods shape children's outcomes and inform local policy using two applications: one that focuses on targeting policies based on observational variation in children's outcomes and another that studies moving to opportunity based on the causal effects of neighborhoods.

Our first application aims to inform the design of economic policies that target disadvantaged families. Many policies to increase opportunity – ranging from tax credits such as Opportunity Zones to educational interventions such as Head Start centers – are targeted based on proxies for opportunity such as poverty rates. We examine how the allocation of such resources would change if one were to instead target policies on the basis of the new tract-level outcome data constructed here. From the perspective of predicting children's outcomes, *observational* differences in outcomes across areas are of direct interest; it does not matter whether these outcomes arise from the causal effect of the neighborhood or from selection. We therefore present a descriptive characterization of how children's outcomes vary across neighborhoods, with the goal of informing those interested in targeting low-opportunity areas.

We find that children's outcomes in adulthood vary sharply across neighborhoods and subgroups, even conditional on parental income. For children with parents who earn $27,000 (the 25th percentile of the national household income distribution), the standard deviation (SD) of mean household income across tracts is approximately $12,850 in their mid-thirties (29% of mean income). Most of the tract-level variance is within counties; the SD of mean household income across tracts within counties is $10,420. The variation in outcomes across tracts remains similar even when controlling for a rich set of observable characteristics from the 2000 Census Long form, such as parental education, occupation, and marital status.

Children's outcomes often vary dramatically even across tracts that are a few miles apart. For example, 44% of Black men who grew up in the lowest-income families in Watts, a neighborhood in central Los Angeles, are incarcerated on a single day (April 1, 2010 – the day of the 2010 Census). By contrast, 6.2% of Black men who grow up in families with similar incomes in central Compton, 2.3 miles south of Watts, are incarcerated on a single day, a difference that is statistically significant even after certain adjustments for potential effects of multiple hypothesis testing and

selection on noise raised in the recent econometrics literature (Mogstad et al. 2020, Andrews et al. 2023). Outcomes also differ significantly across subgroups within neighborhoods. For instance, Hispanic men who grew up in Watts have an incarceration rate of 4.7% – an order of magnitude smaller than for Black men raised in the same tract.

Having characterized the variance of outcomes across areas, we next examine the covariance between our new measures and traditional proxies for neighborhood quality. We find no association between children's outcomes and rates of aggregate job growth. Job density is slightly *negatively* correlated with children's outcomes across neighborhoods within cities, challenging spatial mismatch theories (Kain 1968). In contrast, we find a positive correlation between the employment rates of adults who live in a tract and rates of upward mobility for children who grow up there (race-adjusted correlation = 0.3). What predicts upward mobility is not proximity to jobs, but growing up around people who have jobs. We find even stronger correlations between children's outcomes and other socioeconomic characteristics of adults in an area, such as mean incomes, the share of single-parent households, and measures of social capital. Along all of these dimensions, what matters are characteristics in one's own immediate neighborhood rather than nearby areas. Poverty rates beyond a 0.6 mile radius away from the house where a child grows up have essentially no predictive power for his or her outcomes conditional on poverty rates within that radius. Together, observable neighborhood characteristics explain half of the tract-level variance in children's outcomes, implying that our outcome-based estimates provide considerable new information that can help identify areas where opportunity is most lacking.

One challenge in using our estimates of social mobility to inform policy design is that they necessarily come with a lag, as one must wait until children grow up to observe their earnings. Fortunately, the predictive power of tract-level outcomes in forecasting outcomes for future birth cohorts decays by only about 10% over a decade in most CZs. Moreover, we show that if one's goal is to forecast outcomes for children born today, the optimal forecast places more than three times as much weight on estimates of upward mobility from children born 30 years ago (whose incomes are measured at age 30 today) as it does on present-day poverty rates. In short, even though mobility is not fixed over time, outcomes are sufficiently persistent in most places that the estimates we release remain informative for policy targeting. We also provide estimates of outcomes for the 1984-89 cohorts in addition to our baseline 1978-83 cohort estimates so that analysts can gauge where changes have been more substantial.

We illustrate how the data can be used by analyzing the Opportunity Zones program, whose goal

is to provide preferential tax treatment for investment in selected "low opportunity" neighborhoods. We show that if one were to select neighborhoods based on our estimates of upward mobility, the areas designated as Opportunity Zones would be substantially different from those currently selected. Even adjusting for selection on noise and imperfect persistence of mobility over time, the areas chosen based on the Opportunity Atlas measures exhibit substantially worse outcomes on average than those that were chosen based on traditional proxies of neighborhood disadvantage – demonstrating how using these new measures could significantly change the allocation of resources toward the lowest-opportunity areas.

Next, we turn to our second application, which is motivated by a question relevant both for a given family and for the design of affordable housing policies: "Where should a family seeking to improve their children's outcomes live?" For this application, it is critical to understand whether the observational variation documented above is driven by causal effects or selection. Does moving to an area with better observed outcomes improve a given child's outcomes?

To estimate the fraction of the variance in observed outcomes across tracts that is due to causal effects of place, we first compare our observational estimates to results from the Moving to Opportunity (MTO) experiment. The MTO experiment offered randomly selected families living in high-poverty housing projects housing vouchers to move to lower-poverty neighborhoods. Chetty et al. (2016) show that moving to a lower-poverty neighborhood led to large increases in earnings in adulthood for children who moved at young ages. We find a correlation of 0.5 between the earnings of children who were randomly assigned vouchers to move to different neighborhoods at young ages in the MTO data and the mean earnings of children who grow up in low-income families in those areas in our observational data. A \$1,000 increase in mean earnings (conditional on parental income) in the observational data is associated with a \$680 increase in earnings in the experimental data, suggesting that about 68% of the variance in the observational outcomes is due to causal effects of place.

To evaluate the extent to which the observational variation reflects causal effects of place more broadly, beyond the small number of neighborhoods included in the MTO experiment, we use the quasi-experimental research design developed by Chetty and Hendren (2018a). We study the outcomes of children whose families move across tracts, exploiting variation in the timing of moves between areas for identification. We find that children who move to areas with better observed outcomes earlier in their childhood have better outcomes themselves. Under the identifying assumption that unobservable determinants of children's outcomes in adulthood are uncorrelated with the age

at which they move to a different area – an assumption that we validate using sibling comparisons and a set of placebo tests, including a novel test based on pre-move birth outcomes – this result implies that neighborhoods have causal effects on children's outcomes. Growing up in a better neighborhood is beneficial throughout childhood, but where children live as adolescents (rather than at very early ages) is particularly influential in determining their later outcomes, consistent with Deutscher's (2018) recent findings in Australian data. The quasi-experimental estimates imply that about 57% of the observational variation across tracts in the national data is due to causal effects.

Our estimates imply that moving at birth from a neighborhood at the 25th percentile of the distribution of upward mobility within one's county to a neighborhood at the 75th percentile would increase the lifetime earnings of a child growing up in a low-income family by $387,000. Of course, the feasibility of such a move relies on being able to find affordable housing in high-opportunity neighborhoods. We show that the "price of opportunity" – the cost of moving to a neighborhood that produces $1 higher earnings for a child in present value is 21 cents. Moreover, in most cities it is feasible to find areas that deliver better outcomes for children with lower rents than the locations in which families currently live.

We apply these results to analyze implications for the design of housing voucher programs. Housing voucher recipients currently live in neighborhoods that offer much poorer prospects for upward mobility than the average neighborhood with comparable rents. The data constructed here could therefore be used to design voucher programs in ways that would generate much larger gain in children's outcomes. For example, if the families who received experimental vouchers in MTO had moved to equally affordable areas with the best observed outcomes in our data instead of the lowest poverty rates, their children's earnings would have increased by nearly twice as much as they did – underscoring the value of the outcome-based measures constructed here for policy design.

The paper is organized as follows. Section II describes the microdata we use. Section III describes the methods we use to construct tract-level estimates. Section IV characterizes variation in outcomes across tracts and discusses implications for policy targeting. Section V focuses on the causal effects of neighborhoods and implications for moving to opportunity. Section VI concludes. Supplementary results and methodological details are provided in an online appendix.

# II Data

The sample and variables we use are essentially identical to those used by Chetty et al. (2020). We therefore briefly summarize the sample and variables we use here, and refer readers to Section II and Appendix A of (Chetty et al., 2020), Online Appendix A, and Online Appendix B for further details.

We combine three sources of data housed at the Census Bureau: (1) the Census 2000 and 2010 short forms; (2) federal income tax returns in 1989, 1994, 1995, and 1998-2015; and (3) the Census 2000 long form and the 2005-2015 American Community Surveys (ACS). The Census short forms are designed to cover the entire population; the Census 2000 long form is a stratified random sample covering approximately one-sixth of households; and the American Community Survey is a stratified random sample covering approximately 2.5% of households in each year (U.S. Department of Commerce, Bureau of the Census 2000; U.S. Department of Commerce, Bureau of the Census 2003; U.S. Department of Commerce, Bureau of the Census 2014). These three sets of data are linked by Census Bureau staff using information such as Social Security Numbers (SSN), names, addresses, and dates of birth; all analysis in this paper is conducted using a dataset that is stripped of personally identifiable information.

## II.A Sample Definition

Our target sample frame consists of children who were born in the U.S. or are authorized immigrants who came to the U.S. in childhood and whose parents were also U.S. citizens or authorized immigrants. We construct this sample frame by identifying all children claimed as a child dependent on a 1040 tax form at some point between 1994-2015 by an adult who appears in the 2016 Numident file (a dataset that covers all SSN holders) and was between the ages of 15-50 at the time of the child's birth. In our primary analysis, we focus on children born between 1978-83, based on their record in the 2016 Numident; we also report estimates for children born between 1984-89 to investigate the stability of our estimates over time.

We define a child's "parent" as the person who first claims the child as a dependent (between 1994-2015). This person must be supporting the child, but may not necessarily be the child's biological parent. If the child is first claimed by a single filer, the child is defined as having a single parent. For simplicity, we assign each child a parent (or parents) permanently using this algorithm, regardless of any subsequent changes in parents' marital status or dependent claiming. Virtually all children in the 1978-83 birth cohorts are linked to parents through this procedure, because nearly

all children get claimed as dependents at some point in their childhood (Chetty et al. 2020, Online Appendix Table II). We limit our analysis to children born during or after 1978 because many children begin to leave the household starting at age 17 (Chetty et al. 2014, Appendix Table I), and the first year in which we have dependent claiming information is 1994.

Finally, we exclude the 3.8% of children for whom we have no address information during childhood (i.e., during or before the year in which they turn 23) because the addresses from which their parents filed their tax returns could not be mapped to a tract. The resulting primary analysis sample consists of 20.5 million children, which covers 96% of our target population. When reporting race-specific estimates, we exclude an additional 5% of children for whom race is missing because they could not be linked to the Census or ACS; however, these children are included in the estimates that pool all racial groups, because those estimates can be constructed purely using information from tax returns.

Chetty et al. (2020, Appendix B and Appendix Tables II-IV) show that this analysis sample provides an accurate representation of our target population by establishing that it has income distributions and demographic characteristics very similar to the ACS. They also show that pre-tax income measures in the tax data are closely aligned with those in survey data. For example, the median income in 2015 of children with non-missing race who appear in both our analysis sample and the 2015 ACS is $33,370 based on the tax data, compared with $34,000 based on the ACS data. Individuals recorded as having zero income in the tax records (because they do not file and have no W-2s) have a median income of only $5,000 in the ACS, showing that tax records do not miss substantial amounts of income for non-filers.

## II.B Variable Definitions

In this subsection, we briefly summarize the variables we use in our primary analysis; see Online Appendix A for detailed definitions of these variables. We measure all monetary variables in 2015 dollars, adjusting for inflation using the consumer price index (CPI-U).

*Parent Characteristics.* We define parent income as total pre-tax (household) income reported on IRS 1040 forms, averaging income over five years and coding non-filers as having zero income. Parental marital status is defined based on tax filing status in the year the child is first claimed as a dependent by parents and information on race and ethnicity is obtained from Census short form and ACS data. Parents are assigned locations based on the address from which they filed their tax returns or, for non-filers, to which information returns were sent.

*Children's Outcomes.* In our primary analysis, we measure children's individual and household incomes as their mean annual incomes in 2014 and 2015 as reported on IRS 1040 forms, when children in the 1978-83 birth cohorts are between the ages of 31 and 37. We use data from W-2 forms to impute income for non-filers and code those with no 1040 or W-2 income as having 0 income.[1] We also measure several non-monetary outcomes using tax and Census short form data: incarceration, defined defined as an indicator for being incarcerated on April 1, 2010 (the day of the 2010 Census); marriage, defined as filing a tax return jointly in 2015; having a teenage birth, defined (for women) as claiming a dependent who was born while she was between the ages of 13 and 19. We measure children's locations in adulthood based on the address from which they file tax returns in 2015 and, using this location, define outcomes such as an indicator for living in a low-poverty (below 10% poverty rate) neighborhood, remaining in one's Census tract or childhood CZ, and living with one's parents.

We supplement these measures of child outcomes for the full sample with an additional set of variables that can be measured on the subsample of children that can be linked to the Census Long Form or ACS. This includes an indicator for employment in the past year in the ACS, hours worked per week, hourly wage rates, and educational attainment. We also construct measures of child income restricted to those whose parents are native vs. immigrant mothers, defining such status from the 2000 long form or ACS.

### II.C Summary Statistics

Table I lists the variables included in the Opportunity Atlas and provides summary statistics for those variables using our primary analysis sample. Online Appendix Table I presents analogous statistics by race and ethnicity.

Pooling all races and ethnicities, the median household income of parents in our primary analysis sample is $56,730. Parental income and marital status vary sharply across racial and ethnic groups, as is well known from prior work. For example, median household income is $71,470 for white parents, $29,600 for Black parents, and $33,470 for Hispanic parents. 79.6% of white children are raised in two-parent families, compared with 32.5% of Black children and 57.2% of Hispanic children.

[1] We use information from W-2 forms for non-filing children but not parents because W-2 income data are only available since 2005 at the Census Bureau. Non-filing is uncommon among parents because they have substantial incentives to file taxes (even with low levels of income) in order to claim refundable tax credits such as the EITC. Chetty et al. (2014) show that only 2.9% of parents do not file in a given year and the median W-2 income among parents who were non-filers was $29.

The median household income among children in 2014-15 (between the ages of 31-37) is $42,360, while median individual earnings is $29,440. 1.5% of children are incarcerated on April, 2010 (between ages 27-32). 19.7% of women have a teenage birth. 69.5% earned some college credits. These outcomes again vary sharply across subgroups; for example, incarceration rates range from 0.2% for white women to 10.3% for Black men.

# III Tract-Level Estimates: Methodology

In this section, we describe how we construct our publicly available tract-level estimates.

Let $y_i$ denote an outcome for child $i$, such as his or her income in adulthood. In our primary analysis and the primary outcomes shown in the Opportunity Atlas, we measure both children's and parents' incomes using percentile *ranks* rather than dollar levels. Chetty et al. (2014) show that measuring income using ranks yields more robust estimates by reducing the influence of outliers and mitigating lifecycle bias because individuals' income ranks stabilize earlier in their lives than their income levels (Solon 1999, Haider and Solon, 2006, Grawe, 2006). We define child $i$'s percentile rank $y_i$ based on his position in the *national* distribution of incomes (measured between ages 31-37 in our baseline analysis) relative to all others in his birth cohort who are in our primary analysis sample.[2] Similarly, we measure the percentile rank of the parents of child $i$, $p(i)$, based on their position in the national distribution of parental income for child $i$'s birth cohort. We always hold the definition of these ranks fixed based on positions in the national aggregate income distribution, even when analyzing subgroups or local areas.

For certain analyses, such as calculations of the price of opportunity across neighborhoods or the monetary returns to moving to better areas, the estimand of interest is in dollars rather than percentile ranks. For those applications, we also construct estimates of mean household incomes (measured at ages 31-37) in dollars, top-coding incomes in the 99th percentile of the child income distribution, by parent income percentile. We also report estimates for various other outcomes, including quantile measures (e.g., the probability of reaching the top quintile or the top 1%) as well as non-monetary outcomes such as incarceration rates and educational attainment.[3] We use the same methods to estimate average outcomes for all of these measures, which we describe below.

[2]We include children with zero income, assigning them the mean rank of the individuals in that group. For example, if 10% of a birth cohort has zero income, all children with zero income would receive a percentile rank of 5.

[3]Due to privacy constraints, the publicly available dollar estimates we release are constructed by combining estimates for mean ranks and probabilities of reaching the top 20% and top 1% using a prediction model. This approach produces estimates very similar to those obtained by estimating mean incomes directly. See Online Appendix C and Online Appendix Tables II-III for details.

Our objective is to estimate children's expected outcomes given their parents' income percentile $p$, racial and ethnic group $r$, and gender $g$, conditional on growing up in Census tract $c$ from birth:

$$\bar{y}_{cprg} = E[y_i | c(i) = c, p(i) = p, r(i) = r, g(i) = g]. \quad (1)$$

We condition on parent income, race, and gender in (1) because these three variables have the most predictive power for children's outcomes and the incremental gains from including other observable characteristics – such as parental education, occupation, immigrant status, marital status – are much more modest (Online Appendix D and Online Appendix Table IV). We focus on characterizing how the neighborhoods in which children *grow up* affect their outcomes, which may differ from the neighborhoods in which they live as adults. We focus on childhood neighborhoods because of prior evidence that rates of intergenerational mobility depend on where children grow up rather than where they live as adults (Chetty et al. 2016; Chetty and Hendren 2018a).

There are two empirical challenges in estimating the conditional mean $\bar{y}_{cprg}$ in practice. First, there are insufficient observations to estimate $\bar{y}_{cprg}$ non-parametrically in each parent income percentile by race by gender by tract cell. Second, most children do not grow up in a single tract from birth, forcing us to account for movement across tracts when estimating mean outcomes. We construct an estimator that addresses each of these two challenges in turn.

*Parametric Estimator.* To address the first challenge, we estimate the conditional expectation of children's outcomes given their parents' household income using a univariate regression in each tract by gender by race cell. We choose the parametric form of the regression by examining the relationship between outcomes and parental income rank non-parametrically at the national level. To illustrate, Figure Ia plots the mean household income rank of children within each percentile bin of the parent income distribution for white parents, Black parents, and Hispanic parents, $E[y_i | p(i) = p, r(i) = r]$. Figure Ib replicates Figure Ia using incarceration rates as the outcome, restricting attention to male children. There are significant non-linearities in the conditional expectation functions, especially for incarceration.

To capture these non-linearities, we regress children's outcomes on a tract-invariant transformation of parental income rank $f_{rg}(p_i)$:

$$y_i = \alpha_{crg} + \beta_{crg} \times f_{rg}(p_i) + \varepsilon_i, \quad (2)$$

where $f_{rg}(p_i)$ is estimated using a lowess regression of $\bar{y}_{prg}$ on $p$ in each race by gender subgroup at the national level. Intuitively, we first fit a lowess regression to the non-parametric conditional

expectation functions plotted in Figure I to find a transformation of parental income rank $f_{rg}(p_i)$ that renders the relationship between $y_i$ and $f_{rg}(p_i)$ linear at the national level. We then run a linear regression of the outcome on transformed parental income in each tract-race-gender cell as in (2) and use the predicted values of this regression at each percentile $p$ as our estimate of $\bar{y}_{cprg}$.[4]

This estimation approach allows us to summarize the conditional expectation function in each tract using just two parameters, thereby yielding precise estimates of expected outcomes. The assumption underlying this estimator is that the shape of the conditional expectation of the outcome given parental income at the national level is preserved in each tract up to an affine transformation (within each race-gender subgroup). We evaluate this assumption in two ways. First, we add a quadratic term $(f(p_i))^2$ to (2) and examine whether the estimates change. Second, we estimate a local linear model in each tract. Online Appendix Table V reports the MSE of these models for household income ranks (Panel A) and incarceration (Panel B), estimating using a leave-one-out approach to obtain an out-of-sample estimate of MSE. The MSE of our estimator is similar to and often slightly lower than the more flexible alternatives across the parental income distribution as a result of gains in precision.[5] Furthermore, in large CZs and counties, we find that non-parametric estimates of conditional expectation functions are well approximated by an affine transformation of the national relationship. In sum, although the shape-preservation assumption underlying our estimator is strong, it appears to be a reasonable approximation that has significant benefits in terms of parsimony as well as precision in small cells.[6]

We obtain heteroskedasticity-robust standard errors for $\hat{y}_{cprg}$ directly from the regression in (2), treating $f_{rg}$ as known with certainty. Since robust standard errors can be biased downward in small cells (Chesher and Jewitt (1987)), we also construct an alternative set of standard errors using the leave-out methods of Kline et al. (2020). The KSS standard errors are very similar to the conventional robust standard errors: for tract-level estimates of children's mean household income ranks given parents at $p = 25$, the two standard errors are correlated 0.96 (see Online Appendix E

[4]In each tract-race-gender cell, we estimate $\alpha_{crg}$ and $\beta_{crg}$ in (2) by regressing the outcome $y_i$ on the predicted values from a lowess regression (with bandwidth 0.3) of $\bar{y}_{prg}$ on $p$ estimated by race and gender at the national level. For outcomes obtained from the ACS samples and for all outcomes for small racial subgroups (Asians, American Indians, Other), the lowess estimates at the national level are themselves noisy and are sometimes non-monotonic. In these cases, if the race-by-gender specific lowess fit exhibits any non-monotonicities, we use the functional form estimated from the lowess regression of the outcome on parent income percentile pooling all races and genders.

[5]The point estimates across these methods are also generally quite similar. For example, tract-level estimates of children's mean household income ranks given parents based on the linear and quadratic models are correlated 0.99 at $p = 25$ and 0.83 at $p = 1$.

[6]Parsimony is especially valuable given the privacy constraints discussed below: releasing local linear regression estimates publicly for each tract would require infusion of much greater noise than the two parameters we release in each tract.

and Online Appendix Figure I for details and further comparisons).

*Exposure Weighting.* If children spent their entire childhood in a single tract $c$, (2) would yield an unbiased estimate of $\bar{y}_{cprg}$ under the assumption described above. In practice, many children move across tracts in childhood.[7] To address this issue, first note that the CZ in which one grows up has causal effects on earnings and other outcomes in adulthood until approximately age 23 (Chetty and Hendren 2018a), a finding that we replicate at the tract level in Section V below. We therefore assign children to tracts in proportion to the amount of time they spend before age 23 in each tract over the years observed in our sample. For example, if a child spent half of his childhood in the tract 1 and half of his childhood in tract 2, he would effectively receive 50% weight in each of the two tracts $c = \{1, 2\}$ in the regression in (2).[8]

The estimates $\hat{y}_{cprg}$ obtained from this exposure-weighted regression differ from our target $\bar{y}_{cprg}$ – the mean outcomes of children who spend their *entire* childhood in tract $c$ – in two ways. First, they assign equal weight to all years of childhood, effectively assuming that each year of exposure between ages 0 and 23 contributes equally to children's long-term outcomes. In practice, neighborhoods appear to have slightly larger effects during adolescence than in the earliest years of childhood (see Figure IX). However, the differences in marginal effects by age are small enough that our baseline estimates based on equal weighting are very similar to estimates obtained using age-specific weights corresponding to the marginal age-specific treatment effects estimated below (e.g., correlation = 0.997 for mean household income rank given $p = 25$ across tracts).

Second, the estimates $\hat{y}_{cprg}$ are biased because the outcomes $y_i$ observed for children who live in other tracts incorporate the other tracts' causal effects, which may differ from $\bar{y}_{cprg}$. In general, we expect this bias to lead us to understate the true variance in observed outcomes across tracts relative to what one would observe if children grew up in a single tract for their entire childhood. Intuitively, children who spend part of their childhood in a tract with very positive observed effects will tend to spend the rest of their childhood in a worse tract on average because of mean reversion, pulling down $y_i$ (and hence $\hat{y}_{cprg}$) relative to $\bar{y}_{cprg}$. This bias also turns out to be small empirically, for two reasons. First, although children move, most children spend the majority of their childhood

[7] Chetty and Hendren (2018a) address this issue by restricting the sample to "permanent residents" – children who never move across CZs during their entire childhood. That approach yields imprecise estimates at the tract level because few children stay in a single tract for their entire childhood; for instance, among children born in 1991 (for whom we have a near-complete address history from birth to age 23), only 27% of children spend their entire childhood in a single tract.

[8] More precisely, we estimate the regression in (2) weighting by the total number of years the child is claimed as a dependent in tract $c$ before age 23. Since the tax records begin in 1989, the earliest age at which we observe children's locations in our primary analysis sample is age 6 (for the 1983 birth cohort).

in one tract. Children observed in a given tract spend 75% of their childhood in that tract (using the same weights as in our regression specification). Second, even when children move, they tend to move to an area very similar to the one in which they previously lived. Among children who move, the correlation between $\bar{y}_{cprg}$ and $\bar{y}_{c'prg}$ in the two tracts $c$ and $c'$ in which they spend the most time is 0.86 for mean household income rank given $p = 25$.[9] Given these parameters, if the variation across tracts were driven entirely by causal effects of place that are proportional to childhood exposure, the correlation between $\hat{y}_{cprg}$ and $\bar{y}_{cprg}$ would be

$$Corr(\hat{y}_{cprg}, \bar{y}_{cprg}) = Corr(0.75\bar{y}_{cprg} + 0.25\bar{y}_{c'prg}, \bar{y}_{cprg}) = 0.75 + 0.25 \times 0.86 = 0.96. \quad (3)$$

We conclude that the exposure-weighted regression estimates obtained from the parametric model in (2) are likely to provide accurate estimates of the conditional means of interest in (1).[10]

*Publicly Available Estimates.* After constructing the estimates described above for all tracts and subgroups, we take three final steps to construct the publicly available statistics.

First, we suppress estimates that are based on 20 or fewer children both to protect privacy and because such estimates typically have very large standard errors. Since most subgroups tend to be concentrated in specific tracts, this suppression leads us to omit relatively little data: the publicly available statistics that pool racial groups and genders cover 99.9% of the individuals in the microdata and the race-specific statistics cover 96% of individuals.

Second, to further reduce the risk of privacy loss, we add noise to the estimates we release, following the method developed in Chetty and Friedman (2019). We release estimates for each race-gender-tract cell at two parent income percentiles, typically $p = 25$ and $p = 75$.[11] We add independent, normally distributed noise to each of these point estimates and their standard errors. The standard deviation of the noise distribution is chosen based on the sensitivity of the estimates to a single individual's data, as in the literature on statistical disclosure limitation (Dwork 2006; Abowd and Schmutte 2015); see Online Appendix F for details. Because the SD of the noise we add is proportional to $1/n_{cprg}$, it is typically significantly smaller than the inherent noise in the

[9] To eliminate spurious correlations driven by having the same individual's data appear in both tracts, we estimate this correlation by first constructing two sets of tract-level estimates, randomly splitting our sample into two at the individual level, and then estimating the correlations *across* these two samples.

[10] The estimate assumes individuals move at most once for simplicity and, more importantly, it assumes that the observational variation across places is entirely due to causal childhood exposure effects. In the presence of selection effects (which may not be proportional to childhood exposure), the correlation would differ. Nevertheless, this calculation illustrates that the exposure weighted estimates are unlikely to differ substantially from the mean outcomes one would observe if children did not move across tracts.

[11] In tracts where fewer that 10% of parents are above the median ($p > 50$), we release estimates at $p = 1$ and $p = 50$; conversely, if fewer than 10% of parents are below the median, we release estimates at $p = 50$ and $p = 100$.

estimate from sampling error, which is proportional to $1/\sqrt{n_{cprg}}$.[12] The standard errors we report include both sampling error and the error from noise infusion to protect privacy.

The final tract-level estimates in the Opportunity Atlas are available through an interactive mapping tool and in downloadable flat files. A complete list of the variables along with a codebook are available in the Online Data Tables. For each of the outcome variables listed in Section II.B, we release means by parental income group, race, and gender as well as unconditional means (pooling all parental income levels) by race and gender. We report estimates at 5 parental income levels: lowest income ($p = 1$, incomes of approximately $\$1,000$), low income ($p = 25, \$27,000$), middle income ($p = 50, \$55,000$), high income ($p = 75, \$93,000$), and highest income ($p = 100, \$1,100,000$).

We also release estimates at the county and CZ levels, which are constructed using methods analogous to those described above. Because outcomes in the American Community Survey (such as college attendance) are available for a small sample of individuals, we only report estimates at the county and CZ levels for those outcomes.

# IV Observational Variation and Targeting

Many policies aimed at improving children's outcomes are targeted based on observable neighborhood characteristics as a proxy for opportunity. For example, the Tax Cuts and Jobs Act of 2017 provides preferential tax treatment for investment in Opportunity Zones – low-opportunity neighborhoods designated by states. Children who live in disadvantaged Census tracts are granted preferential status for admission to Chicago's selective public high schools. Head Start (early childhood education and care) centers are often placed in high-poverty neighborhoods to serve children with the most limited opportunities.

In this section, we investigate whether our new measures of children's outcomes by Census tract can provide useful additional information if one's goal is to target neighborhoods where children have the poorest prospects of climbing the income ladder. We divide our analysis into six parts. We begin with a comparison of outcomes in two Census tracts that illustrates the comparisons we envision users making with the Opportunity Atlas and the potential statistical challenges that can arise in such comparisons. Next, we characterize the amount of variation in

[12] Because the noise added is independent across subgroups, the estimates we report do not aggregate perfectly: for example, the estimates for men and women in a tract may not aggregate to the pooled estimate we report. The addition of noise can also result in values that fall outside the bounds of the original variables – for example negative incomes or employment rates above 100%. We report these values directly in the raw downloadable data so that researchers can compute unbiased aggregate moments, but bottom-code and top-code values at their natural bounds in the data visualization tool.

children's outcomes across Census tracts and subgroups to quantify the extent to which location is a useful predictor of child's later-life outcomes. Third, we characterize the correlations between children's outcomes and the observable characteristics of neighborhoods to provide a descriptive picture of the characteristics and size of high-upward-mobility areas. Fourth, we study how much new information our outcome-based measures provide beyond family-specific and neighborhood-level characteristics that are currently used to target place-based policies. Fifth, we analyze how our tract-level estimates change over time to assess whether our estimates of upward mobility (based on children born in the 1980s) provide useful information for targeting policies to children growing up today. Finally, we present applications that show how the use of our outcome-based measures would impact where resources are allocated relative to existing policies.

### IV.A An Illustrative Comparison: Watts vs. Compton

To illustrate the types of comparisons one can make using the Opportunity Atlas data, Figure IIa maps children's income ranks in adulthood in the Los Angeles (LA) metro area, by the tract in which they grew up. In this map and in most of the analysis that follows, we focus on children's mean household income ranks given parents at the 25th percentile of the national income distribution (roughly $27,000), which we term "upward mobility" for convenience.

Rates of upward mobility are much lower in the center of LA than in surrounding areas. Children growing up in the bottom decile of neighborhoods shown on this map in terms of upward mobility (shown in the darkest red colors) have predicted mean income ranks in adulthood below 35.0. Those growing up in the highest decile of neighborhoods (shown in the darkest blue colors) have predicted income ranks above 49.7.[13] Average household incomes for children growing up in families at the 25th percentile in the bottom decile of neighborhoods in the map are $33,748, as compared to $71,491 – more than twice as high – in the top decile of neighborhoods.

Part of the variation across neighborhoods in Figure IIa is due to differences in rates of upward mobility across racial and ethnic groups. Chetty et al. (2020) document that Black children – and Black men in particular – have much lower rates of upward mobility even if they grow up on exactly the same block. Since central Los Angeles has a much larger Black population than the suburbs, the lower rates of upward mobility in the center partly reflect the distinct challenges that

[13] An important caveat to keep in mind is that some of these estimates, particularly those in more affluent areas, are based on extrapolation. For example, there are relatively few low-income families living in coastal areas of Los Angeles such as Santa Monica; the estimates in such areas are obtained by extrapolating from the outcomes of higher-income children to forecast the outcomes of children with parents at the 25th percentile under the statistical model in Section III.

Black Americans face in climbing the income ladder rather than something specific about those neighborhoods.

Even within racial groups, however, there is considerable variation across neighborhoods. Panel B of Figure II plots upward mobility for Black male children. Black men's rates of upward mobility vary widely within central LA. Low-income Black men who grow up in the Nickerson Gardens Public Housing project in Watts reach the 17th percentile (s.e. 2.1) of the household income distribution as adults on average. On average, these men have household incomes of only $16,400 in 2015, when they are in their mid-thirties. These low income levels may not be surprising to those familiar with LA given the widely documented challenges that Watts has faced in terms of poverty and violence. What may be more surprising, however, is that in nearby Compton – just 2.3 miles south of Watts – the outcomes of Black men growing up in families with comparable incomes are much better. In Central Compton, Black men grow up to reach the 29th percentile (s.e. 2.1) and have average household incomes of $36,008.[14]

The disparities across areas are particularly stark for incarceration. Since incarceration rates vary non-linearly with parental income and are much higher among children growing up in the very poorest families (Figure Ib), we focus on incarceration rates among children growing up in the lowest-income families (bottom 1%). Figure IId shows that 44.1% (s.e. 10.9%) of Black males growing up in the poorest (bottom 1%) families in the Nickerson Gardens tract in Watts were incarcerated on April 1, 2010. In contrast, 6.2% (s.e. 5.0%) of Black men who grew up in the lowest-income families in central Compton were incarcerated on April 1, 2010.

The example above illustrates the types of comparisons that researchers and practitioners can make using the statistics constructed here. An important concern with such comparisons is that noise due to sampling error may make inferences about specific tracts unreliable. A recent econometrics literature has emerged analyzing what one can learn about relative rankings based on noisy estimates in small cells, using the Opportunity Atlas data as a leading example (e.g., Mogstad et al. 2020; Andrews et al. 2023; Gu and Koenker 2023; Kline 2023). Inference is challenging in this setting because of (a) the large number of comparisons necessary to develop complete rankings, which create multiple hypothesis testing concerns and (b) the noise inherent in tract-level estimates based on small samples, which can lead to a "winner's curse" phenomenon that leads the econometrician

[14]If we compare all eight tracts that comprise the Watts neighborhood (rather than just the tract that contains Nickerson Gardens) to all 24 tracts in the city of Compton (rather than just central Compton), we find qualitatively similar results: Black men raised in low-income families reach the 23rd percentile in Watts, but the 28th percentile in Compton.

to overstate the true difference in outcomes when selecting higher vs. lower upward mobility tracts based on observed outcomes.

A simple non-parametric approach to addressing both of these issues is to use an independent sample to re-evaluate comparisons identified to be of interest from the initial data. We identified the contrast between Watts and Compton using the Opportunity Atlas estimates for the 1978-83 birth cohorts originally released with this study. We can re-evaluate this comparison using subsequently released data from the 1984-89 birth cohorts. Because we bring a single hypothesis to the 1984-89 data, there is no need for a multiple comparison adjustments when comparing outcomes in the two tracts. There is also no need to adjust for selection on noise (the "winner's curse") because the noise in the 1984-89 sample is independent from that in the original 1978-83 sample. Using the 1984-89 data, we find that Black boys growing up with parents at $p = 25$ in the Nickerson Gardens' tract in Watts reach the 22th percentile at age 26, well below the mean income rank of 30 for comparable Black boys who grew up in Central Compton. We reject the hypothesis that outcomes in the two tracts are the same with $p = 0.003$, implying that the difference between the tracts reflects a true latent difference in Black boys' expected outcomes rather than sampling error.[15] More generally, this comparison illustrates how researchers can use the subsequent cohort of publicly available data to test hypotheses they have formulated with the baseline data.

The recent literature has also developed parametric procedures that can be applied to adjust comparisons when only a single sample of data is available. For example, Mogstad et al. (2020) develop a method of constructing confidence intervals for ranks that controls the family wise error rate to adjust for multiple hypothesis testing. Applying the Mogstad et al. estimator to rank all Census tracts in LA county based on upward mobility, we cannot reject the null hypothesis that Black boys raised in low-income families (or all children raised in low-income families) in the Nickerson Gardens tract in Watts and Central Compton have the same outcomes. This method is conservative because it assumes that the econometrician is comparing all tracts in LA county (whereas in practice we focused on Watts given its well-known history of poverty and violence) and because it controls the family wise error rate (i.e., it requires that the probability that one or more of the hundreds of thousands of pairwise comparisons is wrong is less than 5%). To see why these assumptions matter in practice, consider applying the same methodology to compare median incomes based on the 2010 Census in the Woodlawn and Hyde Park neighborhoods on the South

[15] This approach implicitly assumes that the latent parameter of interest is stable across the two sets of cohorts – an assumption that we believe is a good approximation in most cases over short horizons, because rates of upward mobility have very high rates of serial correlation across cohorts (see Section IV.E below).

Side of Chicago – well-known to be one of the poorest areas in the U.S. – to incomes elsewhere in Chicago (Cook county). Applying the Mogstad et al. approach, we cannot reject the hypothesis that any of the 23 tracts in Woodlawn and Hyde Park are in the top third of tracts in Chicago in terms of their median incomes; that is, we cannot be confident that the South side of Chicago is poorer than other parts of the city.

A commonly used, less conservative approach to adjusting for multiple comparisons is to control the false discovery rate (FDR) – which requires that the proportion of comparisons that we get wrong is less than 5% on average – rather than the family-wise error rate, which controls the probability of at least one false positive among all comparisons. Using the Benjamini-Hochberg procedure to control the FDR, a test for whether central Compton has different upward mobility than Nickerson Gardens in Watts has a p-value of 0.055 for Black boys and $p = 0.021$ pooling all children raised in low-income families. One may be able to further improve power using Bayesian methods or by combining estimates across tracts (Gu and Koenker 2023).

We conclude based on this illustrative analysis that one can make meaningful comparisons using the Opportunity Atlas data at least for certain applications. We leave the choice of appropriate inferential methods to downstream users, as the appropriate method depends on the decision maker's loss function and the ways in which the comparisons were formed.

### IV.B Variation in Upward Mobility Across Areas and Subgroups

Building on the illustrative example above, we now characterize the degree of variation in children's outcomes across Census tracts, adjusting for noise. Panel A of Table II shows statistics on the distribution of upward mobility across tracts, pooling all races (column 1) and separately for each race (columns 2-6). The first row of the table reports mean upward mobility across all tracts, weighting by the number of children from below-median income households in each tract. On average, children with parents at the 25th percentile reach the 40th percentile of the household income distribution, consistent with Figure Ia.

The second row of Table IIa reports the raw standard deviation (SD) of our estimates across tracts in the U.S. Pooling all racial groups, the SD of our estimates of upward mobility ($\hat{y}_{cprg}$) is 6.51 percentiles when measuring income in ranks or \$13,870 when measuring income in dollar levels. Part of the variation in $\hat{y}_{cprg}$ is due to noise – both from sampling error (as there are approximately 500 children per tract in our sample on average) and the random noise added to the estimates to protect privacy (see Section III). As a result, $SD(\hat{y}_{cprg})$ overstates the degree of variation in the

conditional expectation of children's outcomes ($\bar{y}_{cprg}$) across tracts, $SD(\bar{y}_{cprg})$.

Under the statistical model in Section III, we can estimate the signal variance $Var(\bar{y}_{cprg})$ by subtracting the variance due to sampling error and the variance due to noise infusion from $Var(\hat{y}_{cprg})$, as both of these errors are orthogonal to our point estimates. The variance of the noise added to protect privacy is exogenously specified. We estimate the variance due to the sampling error as the mean squared standard error, using the conventional heteroskedasticity-robust standard errors from the regression specification in (2). In the pooled sample, the noise SD from these two sources is 1.97 percentiles, as shown in the third row of Table IIa. Hence, $(1.97/6.51)^2 = 9\%$ of the raw variance in our tract-level estimates is due to noise and 91% is due to signal, as shown in the fourth row of Table IIa. These variance component estimates could potentially be biased for the same reason that heteroskedasticity-robust standard errors can be downward biased as discussed in Section III; in practice, alternative unbiased variance component estimation methods recently developed by Kline et al. (2020) yield very similar estimates (Online Appendix E and Online Appendix Table VI). The fact that sampling error is relatively small compared to the signal variation in upward mobility across areas – as reflected by the high degree of reliability – is the core reason that comparisons between tracts such as that made in the preceding section are informative.

The reliability calculations reported in Table II rely on assumptions about the sampling process to calculate standard errors. As an alternative approach to assessing reliability that does not require any such assumptions, we again make use of data from the independent 1984-89 cohorts, as in the Los Angeles example above. The correlation between upward mobility $\hat{y}_{cprg}$ across tracts in the 1978-83 and 1984-89 cohorts (weighting by below-median-income population, as above) is 0.86. This is slightly below the reliability estimate of 0.91 reported in Table II, as expected because the cross-cohort correlation is below 1 both because of noise (i.e., imperfect reliability) and because of drift in the latent parameter across cohorts.[16] Restricting attention to variation across tracts within counties, the correlation in upward mobility estimates across the two sets of cohorts is 0.80 – again just slightly below the estimate of within-county reliability of 0.85 implied by the variance components reported in Table II. These cross-cohort correlations provide direct evidence that the tract-level estimates reported here contain substantial signal and validate our baseline estimates of variance components.

The signal SD of upward mobility across tracts is $SD(\bar{y}_{cprg}) = 6.2$ percentiles or \$12,850, as

[16] Furthermore, the estimates for the 1984-89 cohort use income measured at age 26, whereas our baseline estimates for the 1978-83 cohort use income measured between ages 31-37, further attenuating the correlation.

shown in the fifth and sixth rows of Table IIa. To benchmark this magnitude, note that a 1 SD increase in parental income rank is associated with approximately a 10 percentile increase in children's household income ranks. Growing up in a tract with 1 SD higher upward mobility is thus associated with an increase in income equivalent to the income gain from a $6.2/10 = 0.62$ SD increase in parental income.

We also find substantial variation across areas within racial groups, especially for whites, Asians, and American Indians, for whom the signal SD is around 5.7 percentiles. There is less variability for Black and Hispanic populations across areas, with an SD of approximately 3.5 percentiles. The reliability of the race-specific estimates is slightly lower (around 0.6-0.7), especially for the dollar estimates, because samples are smaller for racial subgroups.

Panel B of Table II replicates the preceding analysis for incarceration. Here, we again find substantial variation across tracts relative to the mean. This is especially the case for low-income Black men, for whom the signal SD in incarceration rates is 4.3 percentage points, consistent with the variation observed in Figure Ib.[17]

Panel C of Table II considers mean household income for children with high-income parents (parents at the 75th percentile). We find substantial variation across neighborhoods in the outcomes of children from relatively high-income families as well, but the degree of variation is smaller than at low parental income levels, especially as a percentage of income.

*Geographical Decomposition.* Next, we decompose the variation across tracts geographically, by estimating the fraction of the signal variance across tracts that is within counties, across counties within commuting zones, and between commuting zones.[18] We estimate these variance components as the adjusted R-squared from regressions of the tract-level estimates on CZ and county fixed effects, removing the variance due to noise. Figure III shows that 32% of the variation in upward income mobility across tracts, pooling all racial groups, is at the CZ level, while 13.5% is across counties within CZs. The remaining 54.5% of the variation in tract-level upward mobility is across tracts within counties. Studies of the geography of intergenerational mobility that focus on variation across counties or CZs (e.g., Chetty et al. 2014, Chetty et al. 2024) thus miss a substantial share of the variation in outcomes across places that one can observe when one "zooms in" to finer levels of geography.

[17] For subgroups that have small sample sizes and very low incarceration rates, such as Asian men, there is essentially no signal in the estimates – reliability is 0 – and we therefore omit these data from the interactive tool and analyses that follow.

[18] Commuting zones are aggregations of counties analogous to metropolitan statistical areas, but provide a complete partition of the entire United States.

One natural hypothesis for the variation across tracts within counties is that children in different parts of a county attend different schools, which attract different types of families and may differ in their value-added (Black 1999, Bayer et al. 2007). As a simple method of assessing the potential explanatory power of schools, we examine the fraction of variance that is across tracts within high school catchment areas vs. between high school catchment areas.[19] Figure III shows that 28% of the total variance in outcomes – and about half of the local tract-within-county variation – can be explained by school catchment area fixed effects.[20] Hence, although a significant share of the tract-level variation in outcomes could potentially be due to school effects, there is clearly substantial variation in outcomes even across neighborhoods among children who attend the same high school.[21]

*Heterogeneity in Outcomes Across Subgroups.* Neighborhoods that have better outcomes for one group are not always better for others. For example, Figure IIc shows that Black women generally have higher rates of upward mobility than Black men growing up in the same neighborhoods (shown in Figure IIb), but this is not always the case. In central Compton, Black men who grow up in low-income (25th percentile) families earn $36,008, while Black women who grow up in low-income families earn $33,882. We find analogous heterogeneity in outcomes by race and ethnicity. For example, 4.7% of Hispanic males in the lowest income (bottom 1%) families who grew up in Watts were incarcerated on April 1, 2010 – far less than the 44.1% rate for Black males. Table IIIa generalizes these examples by presenting correlations of upward mobility across racial and ethnic groups and by parent income level.[22] We estimate these correlations across tracts within CZs by

[19]We assign Census tracts to high school catchment areas in 2017 using data generously provided to us by Peter Bergman on the intersection of Census tracts with high school catchment boundaries in 2017, obtained from Maponics (2017); see Online Appendix B for details. We match 71,720 tracts to school catchment zones, covering roughly 97% of the population. Since school catchment areas do not perfectly nest Census tracts, we assign tracts to the school catchment zone that contains the largest share of their land area. Using information on exact school catchment boundaries in Mecklenburg County, NC we estimate that only 9.6% of the population gets misclassified into the wrong school catchment area using this approach because high school catchment boundaries follow tract boundaries fairly closely (see Online Appendix Figure II).

[20]Insofar as there is spatial autocorrelation in outcomes across tracts for reasons unrelated to schools, this estimate likely provides an upper bound on the portion of the variance in outcomes that can be attributed to schools, since any randomly drawn set of contiguous tracts would share a common variance component in the presence of spatial autocorrelation. However, in the other direction, our use of 2017 high school catchment boundaries may lead us to understate the role of schools because they do not reflect the boundaries faced by children in our sample, who attended school in the 1990s and early 2000s. In practice, tract boundaries appear to be reasonably stable over time: 87% of tract pairs that fell on different sides of school catchment boundaries in 2002 in Charlotte did so in 2017 as well. Moreover, when examining variation in outcomes for more recent birth cohorts up to the 1989 birth cohort, we find no evidence that schools explain a larger share of the variance for more recent cohorts.

[21]Part of this variation could still the causal effect of primary and middle schools, whose attendance boundaries may vary within high school catchment areas. We defer a more thorough analysis of the relative role of schools vs. neighborhoods to future work, as it requires research designs that permit identification of causal effects of both channels, as conducted by Laliberté (2018) in Canada.

[22]These correlations are signal correlations; we adjust for attenuation in the raw correlations due to sampling error

demeaning all variables by CZ and weighting by the number of low-income children in each tract. The correlations are all positive, but they are far from 1, showing that neighborhoods are not well described by a single factor model.

We find heterogeneity not just across groups but also across *outcomes* for a given group. Table IIIb shows correlations of mean outcomes across tracts for children with parents at the 25th percentile (using a split-sample approach to correct for correlated measurement error across outcomes). We control for race when estimating these correlations by estimating separate correlations for each of the five racial and ethnic groups listed in Table II and then taking a mean of these five correlations, weighting by each group's national population share in the 2000 Decennial Census. Once again, many of the correlations are well below 1, showing that the determinants of different outcomes differ significantly. For example, the correlation between teen birth rates for white women and upward mobility is -0.60. In neighborhoods with high teen birth rates, upward mobility is almost always low; however, when teenage birth rates are low, there is a wide spectrum of rates of upward mobility (Appendix Figure III). Hence, from a predictive perspective, low teen birth rates are a necessary but not sufficient condition for having high rates of upward income mobility.

### IV.C Characteristics of High-Upward Mobility Areas

Next, we examine the characteristics of places that have higher levels of upward mobility. Figure IV reports correlations between upward income mobility and various neighborhood characteristics.[23] We report race-controlled correlations (computing correlations separately for each race and taking a population-weighted average) in light of the significant heterogeneity in outcomes by race discussed above. To isolate variation across neighborhoods as opposed to the broad geographies studied in earlier work (e.g., Chetty et al. 2014), we include CZ fixed effects and study the correlations across tracts within CZs. The figure plots the magnitude of univariate correlations with various characteristics; green circles represent positive correlations, while red triangles show negative correlations. Details on the construction and definitions of the neighborhood characteristics used in Figure IV are given in Online Appendix B.

*Jobs.* We begin by analyzing the association between upward mobility and local proximity to jobs. Using data from the publicly available LEHD Origin-Destination Employment Statistics

and noise infusion by dividing the raw correlation by the product of the square root of the reliability estimates for the two subgroups.

[23] Correlations between the other outcomes analyzed in Table III and observable characteristics are qualitatively similar to those documented below. Correlations with mean income ranks for children with parents at the 75th percentile are also similar (Online Appendix Figure IV).

(LODES) dataset, we count the total number of jobs within 5 miles of the centroid of a tract. The first row of Figure IV shows that this traditional job proximity measure is slightly *negatively* associated with upward mobility, with a correlation of -0.174 (s.e. = 0.004). The number of "high-paying" (annual pre-tax wages above $40,000) jobs exhibits a similar pattern. We also find small correlations with the rate of job growth between 2004-2013, the period when children in our sample were entering the labor market. In short, there is little evidence of a positive association between local job supply and upward mobility, challenging traditional spatial mismatch theories of economic opportunity (Kain 1968).

One potential explanation for these results is that job availability does not matter at short distances, but matters at a labor-market level. To evaluate this hypothesis, Figure V presents a scatter plot of upward mobility vs. job growth for the fifty largest commuting zones, geographic areas that are widely used to approximate local labor markets. Even at the CZ level, there is no association between low-income children's earnings in adulthood and job growth rates. For example, Atlanta and Charlotte both experienced very high rates of job growth over the past two decades, yet they had among the lowest rates of upward mobility for children who grew up there. These cities achieve high rates of economic growth because they are magnets for talent – i.e., they attract high-skilled people to move in and fill high-paying jobs. By contrast, Minneapolis experienced much slower job growth (18%) but had higher rates of upward mobility for children who grew up there. We find similar results when focusing on upward mobility for whites alone, when looking at metropolitan areas instead of CZs, and when measuring job growth in earlier time periods (Online Appendix Figure V).

These findings show that job growth itself does not automatically translate into greater upward mobility for local residents. Policies targeted at aggregate job growth rates would therefore reach quite different areas from the places where upward mobility is lowest. More broadly, the factors that lead to stronger labor markets with high aggregate rates of job growth differ from the factors that promote human capital development and result in high levels of upward income mobility for local residents, as shown by Sprung-Keyser and Porter (2023).

In contrast to the lack of correlation with traditional measures of job availability, we find a strong positive correlation of 0.347 (s.e. 0.004) between the employment rates of the local residents in a neighborhood and the outcomes of children who grow up there. Evidently, what predicts upward mobility is not proximity to jobs, but growing up around people who have jobs. While we of course cannot conclude that this correlation is driven by a causal effect of peers or neighborhood

residents, this result echoes the seminal ethnographic work of Wilson (1996) highlighting the role of employment rates in shaping outcomes at the neighborhood level. Our analysis refines Wilson's conclusions by showing that what predicts upward mobility is not whether work disappears in one's neighborhood, but rather whether work disappears for the people living in one's neighborhood – echoing Case and Katz's (1991) finding that children's outcomes are correlated with the characteristics of the "company they keep."

*Traditional Proxies for Neighborhood Disadvantage.* We find similar correlations between children's outcomes and other socioeconomic characteristics that are commonly used as proxies for neighborhood disadvantage, such as the share of residents above the poverty line (correlation = 0.537). Areas with higher mean household incomes, a larger share of college graduates, and higher test scores also all tend to have higher levels of upward mobility (controlling for race) on average. These results show that traditional proxies for neighborhood disadvantage do in fact predict upward mobility, although they do not capture all of the variation.

*Family Structure and Social Capital.* Consistent with prior work on family structure and children's outcomes (e.g., Sampson 1987), we find a strong negative correlation of -0.587 (s.e. 0.003) between the fraction of single-parent households in a tract and upward mobility. Importantly, this correlation remains similar even conditional on the marital status of a child's own parents. In particular, children of single parents have higher rates of upward mobility if they grow up in a neighborhood with fewer single parent households (correlation = -0.52). This implies that the correlation is driven not by differences in outcomes between children raised by married vs. single parents, but rather by ecological (neighborhood-level) factors. We also find a strong race-controlled correlation of 0.56 (s.e. 0.004) between upward mobility and social capital, measured as the degree of interaction between low- and high-income people using data from Facebook (Chetty et al. 2022); because the social capital measures are available only at the ZIP code level, we estimate this correlation at the ZIP code rather than tract level.

*Size of Neighborhoods.* The predictors of upward mobility at the Census tract level are generally similar to those identified at the broader commuting zone and county levels in prior work (e.g., Chetty et al. 2014), except that upward mobility is more highly correlated with measures of income distributions at the Census tract level than at broader geographies. For example, the race-adjusted, population-weighted signal correlation of upward mobility and median household income is 0.35 at the CZ level, compared with 0.59 across tracts within CZs. One explanation for this result is that poverty rates affect children's outcomes at a highly local level.

To investigate whether that is the case directly, we regress upward mobility from white children in a given tract $c$ on poverty rates both in tract $c$ and its ten nearest neighbors (based on the distance between tract centroids).[24] Figure VIa plots the coefficients obtained from running this regression. Both upward mobility and poverty rates are standardized to have mean 0 and SD 1, so the coefficients that are plotted can be interpreted as partial correlations. The explanatory power of poverty rates decays very rapidly with distance. The coefficient on poverty rates in the child's own tract is -0.32 (s.e. 0.012); the coefficient on the next closest tract (which is on average 1 mile away) is -0.04 (s.e. 0.012). Summing the coefficients on the ten nearest neighbors, we can infer that a 1 SD increase in poverty rates in all ten of the nearest neighboring tracts (roughly a radius of about 3 miles) is associated with a 0.12 SD reduction in upward mobility for white children in a given tract, controlling for poverty rates in that tract. Hence, poverty rates in one's own tract matter 2.7 times more than those in surrounding tracts for upward mobility.

In Figure VIb , we replicate the analysis in Figure VIa at the Census *block* level rather than tract level to obtain a more precise picture of how the correlations decay with distance. To construct this figure, we regress the household income rank of children whose parents are between the 20th and 30th percentiles of the income distribution on block-level poverty rates for their own block and the 200 nearest blocks, binned into groups of 5. Since block-level poverty rates are not publicly available, we construct them using information from tax records as the share of families whose total income falls below the poverty line in 2010. We find more gradual and smooth decay across blocks than tracts, which is intuitive insofar as most plausible mechanisms that might underlie these correlations – peer effects, differences in resources, or selection – would be unlikely to operate purely at the own-block level. But the rate of decay is quite rapid with distance: the coefficients remain statistically significant only until about the 40th nearest block, which is about 0.6 miles away.[25]

In sum, neighborhood characteristics matter at a hyper-local level. This result is consistent with the findings of Damm and Dustmann (2014) and Billings et al. (2019), who also find that a

[24] We focus on the 50 largest CZs by population for this analysis to characterize the size of neighborhoods in large urban centers, but results are similar in the full sample. Additionally, we focus on white children here as a simple method of controlling for race; results are similar for Black children (Online Appendix Figure VI) and when we pool all racial groups. The decay rates documented below are also similar when we examine the other neighborhood characteristics in Figure IV instead of poverty rates.

[25] These granular differences are driven by a decay in neighborhoods causal effects. In particular, using the quasi-experimental movers design described in Section V.II, we show that moving to a higher poverty tract earlier in childhood significantly reduces a child's earnings. However, moving to an area where *surrounding* tracts have higher poverty rates (controlling for poverty rates in one's own tract) has essentially no impact on children's outcomes (Online Appendix Figure VII).

child's nearby neighborhood environment is most predictive of later life outcomes in other settings, consistent with local peer effect and role model mechanisms. Methodologically, these findings underscore the importance of being able to "zoom in" and analyze outcomes at the Census tract level as we do in the present study in order to understand neighborhood effects.

### IV.D Value of Outcome-Based Targeting Relative to Observables

Having characterized how children's outcomes vary across Census tracts, we now ask how much new information the outcome-based data constructed here contain relative to traditional measures used to target policies at the family and neighborhood level. Consider a policymaker who seeks to predict a given child's income in adulthood. How much value-added do tract-level mean outcomes have in forming such a prediction above and beyond observable family characteristics (race, parental income, wealth, etc.) as well as neighborhood characteristics that can be observed in cross-sectional data (poverty rates, demographics, etc.)?

*Family Characteristics.* We compare the explanatory power of tract-level outcomes to a rich set of family characteristics obtained from the 2000 Census long form, including parental education attainment, occupation, age, immigrant status, marital status, number of siblings, and house size (number of bedrooms). We regress children's household income ranks on four sets of variables: (1) parent income, (2) parent income interacted with Census tract fixed effects, (3) parent income and family characteristics, and (4) parent income interacted with Census tract fixed effects and family characteristics. We interact all covariates with race and gender fixed effects in all specifications.

Including tract fixed effects has an incremental R-squared of approximately 5% both without family characteristics (specification 1 to 2) and when we control for family characteristics (specification 3 to 4) (Online Appendix Table VII). Census tract fixed effects thus provide substantial explanatory power for outcomes even controlling for family characteristics. Moreover, the increase in R-squared from adding tract fixed effects is similar in magnitude to the gain from adding family characteristics (specification 1 to 3), indicating that knowing where a child grew up is as valuable as knowing a comprehensive set of family characteristics in terms of predicting their long-term outcomes. Indeed, if one were to choose one piece of additional information beyond parent income, race, and gender to best predict a child's outcomes, it would be the Census tracts in which a child grew up.

*Neighborhood Characteristics.* To analyze the informational content of our outcome-based measures relative to traditional proxies of neighborhood quality, we regress race-specific upward mobility

on the full set of Census tract characteristics analyzed in Figure IV above, weighting by the number of children with below-median-income parents of that race. Averaging across racial groups, we obtain a signal R-squared of 0.50. Half the variation in outcomes is captured in existing measures, while half is not. Looking directly at outcomes is thus quite valuable if one seeks to identify areas with the lowest level of upward mobility.

Furthermore, directly using outcomes alleviates the need to determine which predictors provide the most useful proxies for economic opportunity. The R-squared of 0.50 relies on using the optimal (linear) combination of existing proxies to forecast outcomes. Alternative combinations that form the basis for existing indices for economic opportunity (e.g., Acevedo-Garcia et al. 2014, Kind and Buckingham 2018, Opportunity-Nation 2017) have correlations with our outcome-based measures that range from 0 to 0.6, depending upon the exact set of variables and weights that are used.

The fact that we do not need to rely on a specific model relating neighborhood characteristics to outcomes is especially valuable because the relationship between neighborhood characteristics and children's outcomes itself varies across areas. To illustrate this point, consider the correlation between upward mobility and population density. On average, this correlation is small (around -0.23), as shown in Figure IV. However, this relationship is highly heterogeneous across the U.S. In the southeast, rural areas tend to have lower rates of upward mobility than urban areas (Online Appendix Figure VIII). In contrast, in the midwest and mountain west, rural areas tend to have significantly *higher* rates of upward mobility than urban areas. This heterogeneous relationship underscores the benefit of directly using data on outcomes. Instead of relying on observable characteristics of areas, one can use the information on actual experienced upward mobility in those areas to identify places with the poorest outcomes.

### IV.E Changes Over Time

Although the upward mobility measures are helpful in predicting children's outcomes historically, these measures come with a lag because one must wait until children grow up to observe their earnings. Are the estimates of upward mobility in the Opportunity Atlas – which are based on children who grew up in the 1980s and 1990s – useful for targeting policies to improve outcomes for children growing up in the same areas today?

To answer this question, we analyze how well one can predict expected upward mobility for birth cohort $t$ ($\bar{y}_{c,t}$) in tract $c$ using historical estimates of upward mobility for birth cohort $t-k$

($\hat{y}_{c,t-k}$).[26] Focusing on linear predictors

$$\bar{y}_{c,t} = \alpha + \beta_k \hat{y}_{c,t-k} + \varepsilon_{ct}, \tag{4}$$

the optimal weight placed on the historical estimate for tract $c$ is given by the standard formula for the regression coefficient:

$$\beta_k = \frac{Cov\left(\bar{y}_{c,t}, \hat{y}_{c,t-k}\right)}{Var\left(\hat{y}_{c,t-k}\right)}. \tag{5}$$

We analyze how the coefficients $\beta_k$ decay over time by estimating upward mobility $\hat{y}_{c,t-k}$ separately by single birth cohort (pooling racial groups) and running the regression in (4), weighting by tract-level counts as above. To identify as many lags as possible, we extend our primary analysis sample to children born in the 1978-89 birth cohorts and measure children's income ranks at age 26, the earliest at age at which we can obtain reliable estimates of permanent income ranks Chetty and Hendren (2018b).

Figure VIIa plots $\beta_k/\beta_1$ – the optimal weight with estimates that have a $k$ year lag relative to the optimal weight placed on an estimate that is one year old. There is very little decay in predictive power across cohorts. The optimal weight placed on an outcome observed 10 years ago is only 11% (s.e. = 0.9) smaller than the weight placed on an outcome observed in the previous year. When we focus on tracts that had the largest absolute changes in poverty rates between 1990 and 2000 (where poverty rates increased or decreased by at least 10%), the rate of persistence remains large at $\beta_{11}/\beta_1 = 87\%$ (as shown by the series in diamonds in Figure VIIa ).

The high degree of serial correlation is not driven purely by the persistence of broad regional differences (e.g., the Southeast vs. the Midwest). We estimate an average persistence of $\beta_{11}/\beta_1 = 90\%$ across tracts within CZs by estimating the specifications in (4) with CZ fixed effects. Moreover, when estimating persistence separately for each CZ, we find that 90% of the CZ-specific 11-year persistence rates are greater than 70% among the 100 most populous CZs. Put simply, the map of opportunity looks very similar at the beginning and end of our study period in most cities.

One reason that upward mobility is relatively stable across cohorts is that the structural factors that underlie differences in upward mobility are themselves stable over time. Figure VIIb illustrates this point by replicating Figure VIIa using poverty rates by tract from the Census and ACS instead of upward mobility. Poverty rates 23 years ago are 91% as predictive of current poverty rates in a neighborhood as poverty rates five years ago. Since upward mobility is strongly correlated

[26] We focus on measuring outcomes for children with parents at $p = 25$, pooling across race and gender groups throughout this section; to simplify notation, we suppress the group subscripts.

with poverty rates as shown in Section IV.C above, the relative stability of neighborhoods' income distributions over time contributes to persistence in their rates of upward mobility.

Of course, the new informational content of our outcome-based measures depends not on the portion of their predictive power that comes from the persistence of observables such as poverty rates, but rather on the portion that is orthogonal to such proxies. To quantify the incremental predictive power of our upward mobility measures – which are based on children's incomes at approximately age 30 – consider an analyst seeking to forecast upward mobility for children born in the current year $t$ using data on rates of upward mobility for child born in year $t-30$ and poverty rates in year $t$ ($\bar{x}_{c,t}$):

$$\bar{y}_{c,t} = \alpha + \beta_y \hat{y}_{c,t-30} + \beta_x \bar{x}_{c,t} + \omega_{ct}, \tag{6}$$

We cannot directly estimate (6) because we do not have data on upward mobility for 30 cohorts in our sample. However, we show in Online Appendix G that one can derive a feasible estimator for $\beta_y$ and $\beta_x$ under two assumptions: (i) that upward mobility has a stationary autocorrelation that decays exponentially with the lag $k$ between periods and (ii) that the correlation between upward mobility and poverty rates is stable across periods. Both of these assumptions hold within the 12 cohorts we observe in our sample (as shown in Figure VIIa and discussed further in Online Appendix G), and we assume that this remains the case over longer horizons.

Standardizing $\hat{y}_{c,t-30}$ and $\bar{x}_{c,t}$ to have mean 0 and standard deviation 1 to facilitate comparisons of units, we estimate that the optimal weights for predicting present-day upward mobility across tracts within counties are $\beta_y = 0.29$ and $\beta_x = -0.09$ (the weight on poverty rates is negative because higher poverty is associated with lower mobility). The optimal estimator places more than three times as much weight on historical estimates of upward mobility from the Opportunity Atlas as it does on current-day poverty rates. Intuitively, the residual variation in upward mobility conditional on poverty rates is sufficiently persistent over time that historical upward mobility estimates still provide significant information about the current generation's prospects for upward mobility even conditional on current information on neighborhood poverty rates.

Although the historical Opportunity Atlas data are informative for present-day targeting on average, any given area could still exhibit more significant changes in outcomes.[27] To provide some

[27] A recent paper (Chetty et al. (2024)) builds on the methods in the present study to document significant changes in upward mobility by race and class between the 1978 and 1992 cohorts at the county level. In Online Appendix G, we show that the changes documented by Chetty et al. (2024) are consistent with the relatively high degree of persistence we document here. In particular, the Chetty et al. (2024) data on upward mobility exhibit a correlation of 0.83 across 15 cohorts, which implies a standard deviation of changes within counties that is half as large as the standard deviation of levels of upward mobility across counties within a given cohort.

guidance on the degree of such changes, we provide public estimates of $\bar{y}_{c,t}$ (measuring income at age 26) by Census tract for the 1984-89 cohorts in addition to our baseline 1978-83 cohorts. By comparing the estimates for these two sets of cohorts, one can assess whether outcomes are changing rapidly for the target population of interest and thereby verify that targeting based on historical estimates is appropriate for the relevant application.

### IV.F Applications

We conclude our analysis by illustrating how using our new outcome-based measures would change the neighborhoods one targets in the policy applications discussed at the beginning of this section.

We begin by focusing on the Opportunity Zones (OZ) program, whose goal is to provide preferential tax treatment for investment in selected "low opportunity" neighborhoods. Online Appendix Figure IXa outlines the tracts that were designated as OZs in Los Angeles county.[28] Online Appendix Figure IXb shows the neighborhoods that would hypothetically be selected if one were to choose the same number of tracts, selecting those with the lowest levels of upward mobility.[29] The neighborhoods change quite substantially, with more neighborhoods in the center of the city selected by targeting the lowest-mobility tracts. Children who grew up in low-income (25th percentile) families in neighborhoods that are currently designated as OZs in Los Angeles county reach the 40th percentile on average; under the hypothetical designation, this figure would be the 35th percentile. The mean household income of children who grow up in low-income families in OZs is \$41,800; in the areas selected based on the Opportunity Atlas rank estimates, mean incomes are \$35,000. Hence, the upward mobility estimates could allow us to better identify the neighborhoods that offer the least opportunity for upward income mobility than existing policies that are based on observable characteristics such as poverty rates. However, there are two issues that connect to the preceding analyses and must be resolved before one can be confident in this conclusion.

First, as discussed in Section IV.A above, the estimated gains from targeting based on the Opportunity Atlas estimates may be overstated because of selection based on noise in the Atlas estimates – a problem termed a "winner's curse" by Andrews et al. (2023) and closely related to the

[28] The Tax Cuts and Jobs Act (TCJA) of 2017 defined the eligible pool for Opportunity Zone designation as those tracts with either (a) greater than 20% poverty rate or (b) tract-level median incomes less than 80% of metro-area median incomes (following the same definition as used in the New Markets Tax Credit). States were then given discretion to nominate up to 25% of their low-income Census tracts (as well as a small number of neighboring tracts) to be designated as Opportunity Zones, and the Treasury then certified these nominations.

[29] Adjusting for tract-level noise by shrinking towards the county mean does not significantly affect the tracts assigned to Opportunity Zones because the estimates are highly reliable. 94% of tracts assigned to these zones using the raw estimates are also assigned when using the shrunk estimates. The Spearman correlation between the shrunk and the unshrunk upward mobility estimates is 0.99.

prior literature on shrinkage estimators. We address this concern using three separate approaches, each of which relies on different assumptions. First, we use the 1984-89 cohorts to estimate the gains, which provides unbiased estimates of the target parameter if upward mobility is stable across cohorts because the noise in the 1984-89 estimates is independent of that in the baseline 1978-83 estimates. Second, we shrink the raw 1978-83 estimates of upward mobility in each Census tract to the mean level of upward mobility for the LA commuting zone, using the reliability of the cell-specific estimate as the shrinkage factor. As shown in Chetty and Hendren (2018b), assuming a Normal signal distribution, the mean difference in the shrunk estimates provides an unbiased estimate of the gains from targeting. Finally, we use a procedure recently developed by Andrews et al. (2023) that does not rely on distributional assumptions and instead adjusts the point estimates and confidence intervals to account for the bias and additional uncertainty induced from sorting the data and making inference about the tracts with the lowest mobility.

Online Appendix Figure X presents results from these three approaches. We find significant gains from targeting based on the Opportunity Atlas estimates even after adjusting for the winners curse in selection. In particular, the gains remain similar in magnitude when using data from the later cohorts or using the shrunk estimates in the baseline cohorts. Furthermore, we reject the null hypothesis that children's mean income ranks are the same when selecting tracts based on the Opportunity Atlas instead of the existing Opportunity Zone designation using the Andrews et al. (2023) adjusted "hybrid" 95% confidence intervals. Intuitively, these adjustments have relatively little impact in this application because the reliability of the estimates used for targeting exceeds 0.9; in other applications, such adjustments may have a greater impact and should be implemented accordingly.

The second concern is that our baseline estimates assume that the Opportunity Atlas estimates for children born in the 1980s would apply directly to those growing up in the same areas today. Extrapolating based on the 1% annual decay rate in forecasting upward mobility estimated in Figure VII above implies that the true gains from reclassifying neighborhoods for children growing up in LA today (who are born say in 2010) would be 30% smaller than suggested by our baseline estimates. As an alternative approach to accounting for change, we construct predictions by combining historical data on upward mobility with current poverty rates, as in equation (6). These predictions yield very similar classifications of OZs, with a high rate of agreement with the designations based on our baseline estimates; for example, 83% of the tracts classified as OZs with our baseline upward mobility statistics are classified as OZs based on the predictions that combine upward mobility and

poverty rates. These results suggest that, consistent with our results above, the historical mobility data remain informative for targeting lower-opportunity neighborhoods in the present day.

As another application, we consider how admissions to Chicago's selective public high schools, in which preference is granted to students from particularly disadvantaged neighborhoods, would change if one used upward-mobility targeting. Again, we see a significant shift even accounting for the two issues raised above, in particular with more tracts on the far South Side of the city granted a preference (Online Appendix Figures XI and X). These are areas that do not have particularly high poverty rates (and hence are not included under the current designation) yet have low observed rates of upward mobility .

Of course, these results do not imply that one *should* target different areas in these programs. Our point is simply that if a decision maker's goal were to target areas with limited opportunities for upward mobility, the new statistics constructed here would meaningfully change her allocation relative to what could be achieved with existing information.

# V Causal Effects and Neighborhood Choice

The neighborhood-level variation in outcomes documented above could be driven by two different sources. One possibility is that neighborhoods have causal effects on children's outcomes: that is, moving a given child to a different neighborhood would change his or her outcomes. Another possibility is that the variation is due to differences in the types of people living in each neighborhood. In this section, we analyze the extent to which our observational estimates of upward mobility reflect causal effects of place vs. selection. We then show how our estimates can be used to increase the impacts of policies that seek to help families move to opportunity, such as housing vouchers.[30]

To define the estimand of interest, consider a hypothetical experiment in which a new group of children are randomly assigned to grow up in different neighborhoods at birth. Let $y_i^E$ denote child $i$'s income rank in adulthood in the experimental sample and $\bar{y}_{cp}$ denote the mean income rank in adulthood for children raised from birth in tract $c$ at parental income rank $p$ in the observational data. Our goal is to identify the coefficient $\lambda$ in a regression of outcomes in the experimental sample

[30]The causal effect of a place reflects a bundle of treatment effects of a range of underlying mechanisms, potentially including schools, peers, environment, healthcare effects. Disentangling these mechanisms requires exogenous variation in those mechanisms. While such analysis is beyond the scope of the present paper, we hope our publicly available estimates can be used in future research to identify these mechanisms.

on the observational predictions:

$$y_i^E = \alpha + \lambda \bar{y}_{c(i),p(i)} + \eta_i. \tag{7}$$

Since children's potential outcomes are orthogonal to $\bar{y}_{c(i),p(i)}$ in the experimental sample, the parameter $\lambda$ represents the average causal effect of growing up in a neighborhood where observed ranks are 1 percentile higher. If $\lambda = 0$, then the observational variation in children's outcomes is entirely driven by selection effects; if $\lambda = 1$, it is entirely driven by causal effects. Our goal in this section is to estimate where we lie between these two poles.[31]

We estimate $\lambda$ using two research designs. First, we compare our observational estimates from the Opportunity Atlas with the experimental treatment effects for children who moved to different neighborhoods in the Moving to Opportunity (MTO) Experiment. Second, we use the quasi-experimental design of Chetty and Hendren (2018a) to estimate causal effects by comparing the outcomes of children who move across tracts at different ages. In the final subsection, we use our estimates to study how opportunity for children is priced in the housing market and discuss implications for the design of affordable housing policies.

## V.A Comparison to Estimates from the Moving to Opportunity Experiment

We begin by summarizing the design of the MTO experiment; see Sanbonmatsu et al. (2011) and Chetty et al. (2016) (hereafter CHK) for a more comprehensive description. The MTO experiment enrolled 4,604 low-income families living in high-poverty public housing projects in five U.S. cities – Baltimore, Boston, Chicago, Los Angeles, and New York – from 1994 to 1998. These families were randomized into three groups: 1) the experimental group, which received housing vouchers that subsidized private-market rents and could only be used in census tracts with 1990 poverty rates below 10%; 2) the Section 8 group, which received regular housing vouchers without any constraints; and 3) a control group, which received no assistance through MTO but retained the option to stay in public housing. Families in all three groups were required to contribute 30% of their annual household income toward rent and utilities. Families remained eligible for vouchers (or public housing) indefinitely as long their income was below 50 percent of the median income in their metro area.

[31] Under the additional assumption that the causal and selection components of $\bar{y}_{cp}$ are additive and uncorrelated, $\lambda$ can be interpreted as the fraction of the variance in $\bar{y}_{cp}$ that is due to the causal effects of place (see Online Appendix H). But this assumption is not necessary to interpret $\lambda$ as a forecast of the gain from moving to a neighborhood with higher $\bar{y}_{cp}$ or to test the null hypothesis that none of the observational variation is due to causal effects ($\lambda = 0$).

Using data from tax records, CHK show that children assigned at younger ages to the Experimental and Section 8 groups earned significantly more in adulthood than their peers in the control group. Their findings (CHK, Figure 1) are consistent with a dosage model in which children's outcomes improve in proportion to the number of years that they spend growing up in a higher-opportunity area. Here, we use Chetty et al.'s experimental estimates for children who were below age 13 at the point of random assignment to estimate $\lambda$.[32] We regress the MTO experimental estimates on the observational predictions:

$$\hat{y}_{ws}^{MTO} = \alpha_s + \lambda \hat{y}_{ws} + \varepsilon_{ws}. \tag{8}$$

In this specification, $\hat{y}_{ws}^{MTO}$ denotes mean individual earnings for children below age 13 at random assignment in site $s$ and treatment arm $w$ in the MTO experiment, while $\hat{y}_{ws}$ denotes the mean observed level of individual earnings from the Opportunity Atlas in the tracts where children in site $s$ and and treatment arm $w$ lived. We include site fixed effects $\alpha_s$ in (8) because random assignment occurred within sites.

To implement (8), we construct the estimates of $\hat{y}_{ws}^{MTO}$ from the intent-to-treat (ITT) estimates on individual earnings reported for each site by CHK (Appendix Table 7, Panel B). To adjust for the fact that not all families who were offered vouchers took them up and moved, we follow CHK and construct treatment-on-the-treated (TOT) estimates for the Section 8 and Experimental groups as the mean observed earnings for the control group in the relevant site plus the site-specific ITT estimate for each treatment arm divided by the voucher takeup rate in that arm (see Online Appendix I for details).

We construct the corresponding observational predictions $\hat{y}_{ws}$ using observational predictions of mean individual incomes for children with parents at the 10th percentile, roughly the median income level of MTO participants (Sanbonmatsu et al. 2011), following the methodology in Section III and Online Appendix I.[33] We take the neighborhoods reported as the most common locations for children in MTO from Appendix Table 1c of CHK and map these neighborhoods to Census tracts. We then calculate the average predicted individual income for each of the two treatment groups and the control group, weighting across these tracts by the number of children from below-median income families in each tract in the 2000 Census, to arrive at $\hat{y}_{ws}$.

[32] CHK report estimates separately by age at random assignment in Figure 1. The age-specific point estimates are consistent with a linear dosage effect, but are imprecise due to small sample sizes, which is why CHK pool children below age 13 in their primary analysis.

[33] We do not use race- and gender-specific predictions because CHK do not report site-specific treatment effects by race and gender, due to small sample sizes.

Figure VIII presents a scatter plot of $\hat{y}_{ws}^{MTO}$ vs. $\hat{y}_{ws}$. There are 15 points, representing each of the three treatment arms in the five sites. Solid circles represent the control group, while hollow triangles and solid diamonds represent the Section 8 voucher and Experimental voucher groups, respectively. To eliminate variation across sites, we demean both $\hat{y}_{ws}^{MTO}$ and $\hat{y}_{ws}$ within site and add back the values of $\hat{y}_{ws}^{MTO}$ and $\hat{y}_{ws}$ for the control group in Chicago to facilitate interpretation of the scale.

There is a clear positive relationship between the actual outcomes of children in the MTO experiment and the Opportunity Atlas observational predictions. The correlation coefficient is 0.50. The slope of the regression line is $\hat{\lambda} = 0.68$ (s.e. $= 0.33$): moving to an area where children in low-income (10th percentile) families earn \$1,000 more in the observational data increases children's earnings by \$680. This point estimate suggests that around 68% of the variation in observational estimates of upward mobility across rates is due to causal effects rather than sorting.

## V.B Quasi-Experimental Estimates of Causal Effects

The MTO-based estimate of $\lambda$ above uses data from voucher holders in a small set of neighborhoods in five selected cities, leading to wide confidence intervals and potential concerns about external validity. To estimate the causal share $\lambda$ more precisely in a broader set of neighborhoods, we turn to a second approach that builds on the quasi-experimental research design of Chetty and Hendren (2018a) and uses data from all tracts in the U.S. Chetty and Hendren study how the outcomes of children who move across CZs vary with the age at which they move to identify the causal effects of CZs; here, we use the same design to identify the causal effects of Census tracts, which could potentially be very different since selection patterns across tracts within CZs could differ from selection patterns across CZs. Since our approach closely follows Chetty and Hendren (2018a), we briefly summarize the estimation approach in what follows; further details regarding the sample specification, variable definitions, and estimating equations are in Online Appendix J.

We study the outcomes of children who move across tracts exactly once during our sample window. Let $i$ index children and $p_i$ denote their parental income ranks. In the sample of one-time movers, let $m_i$ denote the age at which child $i$ moves from origin tract $o$ to destination tract $d$. Let $\hat{y}_{cp}$ denote the exposure-weighted mean of $y_i$ for children who grew up in location $c$ with parental household income rank $p$, estimated using the approach in Section III except using only children who never move or who move more than one time (i.e., the complement to one-time movers). Let $\Delta_{odp} = \hat{y}_{dp} - \hat{y}_{op}$ denote the predicted difference in income ranks in the destination versus origin

tract for children.

We regress the income rank of children who move ($y_i$) on the measures of origin and destination quality and parental income rank, fully interacted with age-at-move fixed effects:

$$y_i = \sum_{m=2}^{28} I(m_i = m) \left[\alpha_m + \phi_m \hat{y}_{op} + \zeta_m p_i + b_m \Delta_{odp}\right] + \varepsilon_i \tag{9}$$

where $\alpha_m$ denotes an age-at-move-specific intercept and the parameters $\{\phi_m, \zeta_m\}$ are age-specific coefficients on the predicted income rank in the origin and on parental income rank, respectively. To adjust for attenuation bias due to measurement error in our estimates of $\hat{y}_{cp}$, we construct independent estimates $\hat{y}_{cp,1}$ and $\hat{y}_{cp,2}$ by randomly splitting families into two samples and then instrument for $\hat{y}_{op,1}$ with $\hat{y}_{op,2}$ and $\Delta_{odp,1}$ with $\Delta_{odp,2}$ when estimating (9).

The key parameters of interest in (9) are the $b_m$ coefficients, which capture how children's outcomes vary with the age at which they move to an area with higher or lower predicted earnings in the observational data. Figure IX plots the coefficients $\{b_m\}$ for the specification in equation 9 using household income ranks at age 24 as the outcome.[34] Consistent with the results in Chetty and Hendren (2018a) at the CZ level, the coefficients $b_m$ decline steadily until age 23, after which they are flat at an average level of $\delta = 0.35$. Since moves after age 24 cannot affect income measured at age 24, this $\delta$ coefficient reflects selection: children whose parents move to areas with better observed outcomes tend be positively selected in terms of their potential earnings.[35]

Under our identification assumption that the selection effect does not vary with the age at which children move (illustrated by the dashed horizontal line in Figure IX), we can interpret the difference between $b_m$ and $\delta$ as the causal effect of moving to an area with one percentile higher predicted income ranks at age $m$. The declining pattern of the coefficients implies that neighborhoods have causal effects on children's outcomes in proportion to childhood exposure prior to age 23. The slope of this relationship is somewhat steeper between ages 13 and 23 than it is at earlier ages,

[34] We measure income at age 24 and expand the range of cohorts we analyze from 1978-1991 to maximize the range of ages at move that we are able to analyze. Measuring income at later ages, from 26 to 30, yields similar results over the age span for which we observe those incomes (Online Appendix Figure XII). Even though income ranks change substantially when children are in their twenties, the estimates of $\{b_m\}$ are insensitive to the age of income measurement because they measure the extent to which outcomes of movers converge to those of residents in the destination area at the *same* age. Intuitively, children who grow up in areas with high rates of upward mobility typically have higher income ranks at age 30 than at age 24 (e.g., while in graduate or professional schools). We find that movers' outcomes converge to those of children growing up in the destination and therefore exhibit the same income trajectories, with higher income ranks at 30 than 24, thus making $\{b_m\}$ invariant to $m$. This is why we are able to measure income at earlier ages in our movers exposure effect analysis than in our analysis of *levels* of upward mobility in Section III, where we measure income at later ages to capture permanent income.

[35] This selection term is higher than the corresponding estimate of $\delta = 0.22$ the CZ level (Chetty and Hendren 2018, Figure IV), suggesting that there is more unobservable selection across tracts than across CZs.

suggesting that where a child lives as an adolescent may be particularly influential in determining his earnings outcomes, consistent with the findings of Deutscher (2018) in Australian data.

Fitting a linear regression to the estimates below age 23, we estimate an average annual childhood exposure effect – the effect of growing up for an additional year in a tract with 1 percentile higher observed earnings – of $\gamma = 0.025$ (s.e. 0.002), where $\gamma$ is defined by parametrizing $b_m = b_0 + \gamma m$ using a linear specification in equation (9). Extrapolating linearly back to age 0, we obtain a predicted value of $b_0 = 0.92$, implying that the causal effect of moving at birth to an area with 1 percentile higher observed earnings is $\lambda = 0.92 - 0.35 = 0.57$. This estimate implies that 57% of the variation in the observational estimates is due to the causal effect of neighborhoods under our identification assumption. We find similar estimates of $\lambda$ for incarceration rates, teenage birth rates, and marriage rates (Online Appendix Table VIII).

We evaluate the validity of the key constant selection effects by age identification assumption using three approaches. First, we identify exposure effects from comparisons between siblings by including family fixed effects in our analysis (Online Appendix Table VIII). Effectively, we ask whether the *difference* in earnings outcomes between two siblings who move to a new area is proportional to their age difference interacted with observed outcomes in the destination. We find that the estimated exposure effect $\gamma$ remains very similar when identified from within-family comparisons.

These sibling comparisons address confounds due to factors that are fixed within families, but they do not account for *time-varying* factors, such as a change in family environment at the time of the move (e.g., a new job) that directly affects children in proportion to exposure time independent of neighborhoods. To evaluate whether such unobservables might bias our estimates, we turn to a second test of our identification assumption: outcome-based placebo tests. These tests are motivated by the finding in Section IV.B that neighborhoods are multi-dimensional: incomes, incarceration rates, teenage birth rates, and marriage rates are not perfectly correlated across tracts.

Using multivariable regressions, we find that moving to an area with higher incarceration rates, teenage birth rates, or marriage rates has little impact on children's incomes, conditional on observed incomes in the destination (Online Appendix Table IX). Similarly, for the other outcomes, the neighborhood quality measure based on predictions of the dependent variable is strongly significant with a coefficient of similar magnitude to those reported in Online Appendix Table VIII, but the coefficients on the other "placebo" predictions are small and small and typically statistically

insignificant. These results strongly support the view that the variation in children's outcomes across neighborhoods for movers is driven primarily by causal effects. Intuitively, it is unlikely that a correlated shock - such as an increase in wages when the family moves - would covary precisely with differences in neighborhood quality across all of these outcomes in proportion to exposure.

Third, we implement a balance test using pre-move birth outcomes (birth weight and gestational length), drawing on analysis originally reported in Chetty et al. (2023). Linking our movers sample to the universe of birth records in California, we estimate equation 9 with birth outcomes as the outcome variable (see Online Appendix K for details). We find no gradient in the relationship between birth outcomes and upward mobility by age at move (Online Appendix Figure XIII). Children who move to higher-upward-mobility areas at earlier ages have comparable birth weights and gestational lengths to those who move to the same areas at older ages. The fact that differences in children's outcomes emerge only after, not before, they move to higher-mobility Census tracts further supports the constant selection effects by age identification assumption.

Because our estimates are identified from the set of families who choose to move to a given area, one may be concerned that they reflect causal effects that apply only to the subset of families who chose to move to a neighborhood that is good for their children, rather than a broader population. In Online Appendix Table X, we show that estimates of $\gamma$ are similar across various subsamples – such as families that move to better vs. worse neighborhoods, smaller vs. larger moves in terms of quality of the origin vs. destination – suggesting that the observational Opportunity Atlas estimates predict the causal effects of neighborhoods for a wide variety of families in practice.

Averaging the estimates obtained from the baseline quasi-experimental estimator ($\lambda \simeq 57\%$) and the MTO analysis ($\lambda \simeq 68\%$), we conclude from that roughly $\lambda \simeq 62\%$ of the observational variation in outcomes across Census tracts reflects the causal effects of neighborhoods, and that this fraction is quite stable across subsamples. Combining this estimate of $\lambda$ with the signal SD of individual income across tracts within counties for children with parents at the 25th percentile ($5,343), we estimate that moving at birth from a neighborhood at the 25th percentile of the distribution of upward mobility within one's county to a neighborhood at the 75th percentile would increase the lifetime undiscounted earnings of a child growing up in a low-income family by $387,200 (or $101,550 in present value at birth).[36] This earnings gain would benefit not just the children who

[36]We arrive at these numbers by first tabulating mean individual earnings $w_a$ by age $a$in the publicly available 2015 ACS. We then apply a 1% wage growth, $g$, and mortality rate estimates at each age, $s_a$, from Chetty et al. (2016). From here, we obtain an undiscounted baseline sum of lifetime earnings for the average American of $B = \sum_{a=18}^{97} w_a \cdot s_a \cdot \frac{1}{(1-g)^a} \approx \$2.70$ million. The percent gain in earnings from such a move in our data is given by $\frac{\lambda \cdot \text{Signal SD of Kid Individual Income at P25} \cdot \text{NormalIQR}}{w_{34}} = 0.14$. Under the assumption that the gains in earnings from growing

move but taxpayers as well, through increased tax revenue and lower incarceration rates. These gains could be substantial; for example, assuming a tax rate of 20%, tax revenue would increase by $77,440 over the course of a child's lifetime.

### V.C Application: Housing Voucher Policies

The finding that a large fraction of the variation in the observational estimates of upward mobility is driven by causal effects of place suggests that economic mobility could be improved by helping families move to opportunity, e.g. using housing vouchers. The feasibility of this approach relies on being able to find affordable housing in high-opportunity areas. In this subsection, we first characterize the relationship between upward mobility and rents to understand the price families must pay to move to neighborhoods that produce better outcomes for their children and then analyze implications for the design of housing voucher policies.

We quantify the average price of upward mobility by regressing median annual rents on our estimates of upward mobility (measured in dollars of individual income in adulthood) across tracts within each CZ, weighting by number of children from below-median income families in the tract. The resulting regression coefficient can be interpreted as the average annual rental cost of a neighborhood with $1 higher future annual incomes for children with parents at the 25th percentile. We then inflate this regression coefficient by the reliability of our upward mobility estimate in that CZ (estimated by population decile) to adjust for noise and divide it by our estimate of $\lambda = 62\%$ from the previous subsection to obtain the annual rental cost of moving to a CZ that has a $1 higher causal effect on children's earnings in adulthood.

On average (with population weights) across all CZs, a $1,000 increase in future annual income at age 34 for children costs an additional $271 in annual rent for each year of the 23 years of their childhood. Using a 3% discount rate and summing up over childhood, this annual increase in rent translates to a $4,727 increase in rent over childhood. A $1,000 increase in income at age 34 translates in present value to additional lifetime income for the child of $22,723 under the same discount rate and assumptions made to calculate the earnings gains reported in Section V.B above.[37] Hence, the mean "price of opportunity" in present value terms is $4,727/$22,723 = 0.21.

up in a better neighborhood remains constant in percentage terms over the lifecycle, the increase in lifetime earnings is given by multiplying the percentage gain in earnings with $B$. To obtain the present value in birth, we apply a 3% annual discount rate to the baseline sum of lifetime earnings (yielding $B_{discounted}$ = $709,049) and repeat the calculation, so that $B_{discounted} \cdot 0.14 \approx 101,550$.

[37] The present value of the gains in the child's lifetime earnings is obtained using the same procedure and assumptions as the mover's analysis, with $\frac{1}{w_{34}}$ as the percentage gain in earnings. The rent cost is quantified as $\sum_{a=0}^{23} \frac{271}{(1+.03)^a} \approx 4727$.

That is, it costs about 21 cents to move to a neighborhood that generates a $1 increase in lifetime income for one's child (in present value terms) on average.

Furthermore, there is substantial dispersion in the relationship between upward mobility and rents within and across CZs, suggesting that many families may be able to find neighborhoods that offer better outcomes for their children even without paying higher rents. The standard deviation of the annual rental price of opportunity across CZs is 0.17. Figure Xa illustrates the within-CZ variation by plotting upward mobility vs. the median rent for two-bedroom apartments in 1990 (measured in 2015 dollars) for tracts in Chicago. Upward mobility is positively correlated with rent on average, but there is considerable residual variation in upward mobility conditional on rents. More broadly, across the United States, the within-CZ signal correlation between rent and upward mobility across tracts is 0.48. As a result, the residual SD of upward mobility controlling for median rent is $9,606 – showing that there is considerable scope to move to higher-upward-mobility areas even without paying higher rents.

*Determinants of the Price of Opportunity.* Why does the price of opportunity appear to be relatively low in equilibrium in many CZs? One potential explanation is that high-mobility, low-rent tracts have other disamenities, such as longer commute times, that deter families with children from moving there. An alternative explanation is that frictions in the housing market may prevent households – especially low-income households – from moving to high-upward-mobility neighborhoods. For instance, households may lack information, face discrimination, or may move under duress in a way that limits their available options (DeLuca et al. 2019; Christensen and Timmins 2022; Bergman et al. 2024). We find some support for the existence of informational frictions by splitting the variation in upward mobility into the component that is predicted by the observable neighborhood characteristics analyzed in Figure IV (such as poverty rates and test scores) and a residual ("unobservable") component. The observable component has a correlation of 0.516 with rent, while the unobservable component has a signal correlation of only 0.08 with rent (Figure Xb), suggesting that the components that are harder to observe are not priced in equilibrium.

While search and informational frictions may influence the price of opportunity, fundamentals such as land availability and regulations are nevertheless a strong predictor as well. Figure Xc presents a binned scatter plot of the price of opportunity against the Wharton Residential Land-Use Regulatory Index (Gyourko et al. 2008) across CZs. The price of opportunity is substantially higher in areas with tighter land use regulations: a 1 SD increase in the regulatory index is associated with a seven cent (23%) increase in the price of opportunity.

*Implications for Housing Voucher Policies.* Housing Choice Voucher holders live in neighborhood with lower rates of upward mobility than the average neighborhood with comparable rents in virtually every CZ in the United States (Online Appendix Figure XIV; see Online Appendix L for details). On average, children of voucher holders grow up in neighborhoods where individual incomes in adulthood are $32,254 for children who grow up in families at the 25th percentile, $2,148 lower than the average neighborhood with comparable rents.

These estimates suggest that it may be possible to design housing voucher policies to help families receiving vouchers move to affordable neighborhoods where their children would have better outcomes. Selecting such neighborhoods based on observed rates of upward mobility rather than traditional measures such as poverty rates is especially valuable. Among neighborhoods with comparable rents to those in which voucher holders currently live, children who grow up in low-income families in neighborhoods with poverty rates below 10% – the threshold used to define "high opportunity" neighborhoods in the MTO experiment – have average individual incomes in adulthood of $37,929, an $5,675 increase relative to the mean in voucher holders' current neighborhoods (see Online Appendix L for details). Using our prediction from Section V.A, we estimate that the gain in earnings in adulthood from moving to those neighborhoods at birth would be $3,859. If one were instead to select at equivalent number of neighborhoods that rank highest in terms of observed upward mobility, the earnings gain would be $7,157. Hence, one could achieve nearly twice as large a gain in earnings by helping families move to high opportunity areas as defined by observed upward mobility instead of poverty rates. Similarly, we find that selecting eligible neighborhoods based on the Opportunity Atlas estimates of upward mobility instead of poverty rates would have doubled the earnings gains of children who moved in the experimental voucher group in the MTO experiment (see Online Appendix L).

We caution that these partial equilibrium calculations do not take into account potential spillover effects onto other children. The impacts of moving to opportunity policies could be dampened in general equilibrium (Carrell et al., 2013). In particular, if differences in outcomes are largely driven by linear-in-means peer effects, then the reduction in mean income when many low-income families move to opportunity may reduce the destination areas' causal effects for all residents.[38] Our research designs are not powered to detect the causal effects of moves on peers' outcomes. However, observational evidence suggests that in mixed-income areas with more cross-class interac-

[38]Even in a linear-in-means peer effects model, it may be useful to help low-income families move to opportunity from a distributional perspective, since they may stand to gain the most given diminishing marginal utility.

tion (as measured in Facebook data), outcomes are better for children raised in low-income families but no worse for those raised in high-income families (Chetty et al. 2022). These findings are more consistent with a "levelling up" model where more integrated and connected communities benefit the poor without harming the rich. An important direction for further work is to evaluate the causal impacts of moving to opportunity policies taking general equilibrium effects into account.

Ultimately, the viability of policy changes that help families move to affordable high-upward-mobility areas depends upon whether families actually want to move to such neighborhoods. Families may have other reasons to prefer to stay in lower-opportunity areas (e.g., proximity to jobs or family) and moreover may be unable to find landlords willing to rent to them in higher-opportunity areas. In a companion paper (Bergman et al. 2024), we show using a randomized experiment that providing search assistance to voucher recipients substantially increases their chances of moving to and staying in high-upward-mobility areas. We conclude that voucher recipients currently tend to live in lower-opportunity areas primarily because of barriers in the search process and that using the Opportunity Atlas data to help families move to affordable high-opportunity areas could therefore potentially improve their children's outcomes significantly.[39]

# VI Conclusion

Cross-sectional statistics on neighborhood characteristics such as poverty rates and job growth have provided a foundation for economic policy and research on labor markets for several decades. In this paper, we constructed longitudinal statistics that measure children's outcomes in adulthood based on the Census tract in which they grew up, which can provide an analogous foundation for policies to improve social mobility and research on human capital development.

Using these new statistics, we show that neighborhoods have substantial causal effects on children's long-term outcomes at a granular level. Moving to a neighborhood that is just a few miles away can change children's average earnings by several thousand dollars a year and have significant effects on a spectrum of other outcomes ranging from incarceration to teenage birth rates. Much of this variation in children's outcomes is unrelated to traditional neighborhood-level proxies for economic success – such as rates of job growth – showing that the conditions that create greater upward mobility are not the same as those that lead to strong labor markets.

[39] If such policies were taken to scale, their impacts on children's outcomes would depend upon how neighborhood effects themselves changes in equilibrium. Understanding how neighborhood effects change with the composition of the neighborhood is an important question that warrants further work; see, for example, Derenoncourt (2022), who analyzes how the Great Migration changed neighborhood effects for Black families in northern states.

We view the Opportunity Atlas as an input for downstream research and policy applications. For researchers, the Opportunity Atlas data provide a new tool to study the determinants of economic opportunity. For example, recent studies have used the Opportunity Atlas data to analyze the effects of lead exposure, pollution, neighborhood redlining, and the Great Migration on children's long-term outcomes (Manduca and Sampson 2019; Colmer et al. 2019; Park and Quercia 2020; Aaronson et al. 2021; Derenoncourt 2022). Other studies use the Atlas statistics as inputs into models of residential sorting (Aliprantis et al. 2022; Davis et al. 2019) and to understand perceptions of inequality (Ludwig and Kraus 2019). The ongoing American Voices Project is interviewing families in neighborhoods with particularly low or high levels of upward mobility to uncover new mechanisms from a qualitative lens.

Policy makers can use the Opportunity Atlas data to design programs that improve economic opportunities for disadvantaged children. For example, the Creating Moves to Opportunity pilot program conducted by the Seattle and King County housing authorities helps housing voucher recipients move to higher-opportunity areas based on the Opportunity Atlas statistics (Bergman et al. 2024); a recent bill proposes to scale that approach nationally (Office of Todd Young 2019). Other proposals seek to expand affordable housing, change zoning restrictions, and invest in community redevelopment using the Opportunity Atlas statistics as an input (Freddie Mac 2022; Lake County Consortium 2020). The data also have applications outside place-focused policies. For example, the Atlas statistics have been used as a measure of neighborhood disadvantage in selective college admissions (Yale University 2023). These early applications illustrate how granular, outcome-based statistics that can be constructed using modern data have the potential to inform a broad range of policies to improve economic opportunity.

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## ONLINE APPENDICES

# The Opportunity Atlas: Mapping the Childhood Roots of Social Mobility

Raj Chetty, John N. Friedman, Nathaniel Hendren, Maggie R. Jones, Sonya R. Porter

## A Construction of Individual-Level Variables

In this appendix, we present comprehensive definitions of the variables we use in our primary analysis, expanding upon Section II.B.

Parental Characteristics.

*Income.* Our primary measure of parent income is total pre-tax income at the household level, which we label parent family or household income. In years where a parent files a tax return, we define household income as Adjusted Gross Income (as reported on the 1040 tax return) plus tax-exempt interest income and the non-taxable portion of Social Security and Disability benefits.[40] In years where a parent does not file a tax return, household income is coded as zero.[41] Following Chetty, Hendren, Kline and Saez (2014), we define our baseline parental income measure as the mean of parents' household income over five years: 1994, 1995, and 1998-2000, as tax records are unavailable in 1996 and 1997.[42] We exclude children whose mean parent income is zero or negative (1.0% of children) because parents who file tax returns (as is required to link them to a child) reporting negative or zero income typically have large capital losses, which are a proxy for having significant wealth.

*Marital Status.* We identify parents' marital status based on their tax filing status in the year the child is first claimed as a dependent by parents. We say that a child has a "father present" if one of the tax filers who claims the child as a dependent in that year is male and define "mother presence" analogously. Children claimed by two people in the year they are first claimed are defined as having two parents.

*Locations.* In each year, parents are assigned an address based on the address from which they filed their 1040 tax return. For non-filers, we use address information from information returns such as W-2s, which are available beginning in 2003.[43] Addresses are coded as missing in years when a parent does not file or does not have an information return. For children whose parents were married when they were first claimed as dependents, we prioritize the mother's location if marital status changes. Addresses are mapped to geographies such as Census tract or Census block

[40] We use the term "household" income for convenience, but we do not include incomes from cohabiting partners or other household members aside from the primary tax filer's spouse.

[41] Prior work (e.g., Chetty et al. 2014) has used information from W-2 forms to measure income for non-filers. We cannot follow that approach here since income data from W-2 forms are unavailable at the Census Bureau before 2005. However, this has little impact on results. Information from W-2s is more important when measuring the incomes of children in early adulthood, for whom we do have W-2 data at the Census Bureau.

[42] Formally, we define mean household income as the mother's individual income plus the father's individual income in each year of 1994-95 and 1998-2000 divided by 10 (or divided by 5 if we only identify a single parent). For parents who do not change marital status, this is simply mean household income over the 5 year period. For parents who are married initially and then divorce, this measure tracks the mean household incomes of the two divorced parents over time. For parents who are single initially and then get married, this measure tracks individual income prior to marriage and total household income (including the new spouse's income) after marriage.

[43] Address information from W-2s starts in 2003, but income amounts are not available until 2005.

using a geocoding algorithm developed by the Census Bureau (see Online Appendix A of Chetty et al. (2018) for details).

*Race.* We assign race and ethnicity to parents using the information they report on the 2010 Census short form. If the child's PIK does not appear in the 2010 Census microdata, we use the 2000 Census short form; if the child does not appear in the 2000 Census, we then use the ACS. We aggregate race and ethnicity categories into a Hispanic ethnicity category and a set of non-Hispanic races: White, Black, Asian, American Indian or Alaskan Native, and Other, following the Office of Management and Budget (1997). Individuals who report two or more races, Native Hawaiian or Pacific Islander, or Some Other Race are classified in the "Other" category.

Children's Outcomes Observed in Tax Records or Census Short Form.

We report statistics at the Census tract, county, and commuting zone (CZ) levels for the following outcomes, which we observe in the full sample using data from either tax records or the Census short form.

*Income.* We define children's annual household income in the same way as parents' income except in our treatment of non-filers. Since W-2 data are available for the years in which we measure children's incomes, we define income for a child who does not file a tax return as wage earnings reported on form W-2. We define children's individual incomes as their own W-2 wage earnings plus self-employment and other non-wage income, which we define as Adjusted Gross Income minus total wages reported on form 1040 divided by the number of tax filers (thereby splitting non-wage income equally for joint filers). In years in which children have no tax return and no information returns, both individual and household income are coded as zero. We measure children's individual and household incomes as their mean annual incomes in 2014 and 2015, when children are between the ages of 31 and 37. In addition to these baseline definitions, we also report mean incomes by age at ages 24, 26, and 29.

*Upper-Tail Income.* We define indicators for children reaching the top 20% and top 1% of the income distribution using the baseline definitions of individual and household income above.

*Employment.* We use two measures of employment, one based on the tax data and one based on the ACS (defined below). In the tax data, children are defined as working if they have positive W-2 income in 2015. In addition to this baseline definition, we also report employment rates in the tax data by age for ages 24, 26, 29 and 32.

*Marriage.* A child's marital status is measured based on whether he or she files a tax return jointly in 2015. In addition to this baseline definition, we report marriage rates by age for ages 24, 26, 29, 32.

*Incarceration.* Using data from the 2010 Census short form, we define an individual as incarcerated on the day of the Census (April 1, 2010) based on whether he or she lives in any of the following types of group quarters: federal detention center, federal prison, state prison, local jail, residential correctional facility, military jail, or juvenile correctional facility. This variable is defined only for children who appear in the 2010 Census.

*Teenage Birth.* We define a woman as having a teenage birth if she ever claims a dependent who was born while she was between the ages of 13 and 19. This measure is an imperfect proxy for having a teenage birth because it only covers children who are claimed as dependents by their mothers and because it may include others (e.g., siblings or cousins) claimed as dependents who are not the claimer's own children. Nevertheless, the aggregate level and spatial pattern of teenage births in our data are closely aligned with estimates based on the American Community Survey, with an (unweighted) correlation across states of 0.79.

*Spouse's Income.* For children who were married in 2015, we define spouse income analogously to the child's own individual income using an average of 2014 and 2015 individual income. To

capture spouses who are not within our primary analysis sample, we include all spouses born between 1973 and 1989.[44]

*Living in a Low-Poverty Neighborhood in Adulthood.* We measure children's locations in adulthood based on the address from which they file tax returns in 2015. For non-filers, we obtain address information from W-2 forms and other information returns. If no address information is available in 2015, we use the most recent year in which an address is available. Among children with a non-missing address in adulthood, we identify those living in a "low-poverty" neighborhood as those living in a tract with less than 10% of people living in poverty, as defined by the publicly available Census 2010 estimates.

*Staying in Childhood CZ or Tract.* Children are defined as staying in their childhood location (tract or CZ) if their most recent address matches any tract or CZ that they lived in during childhood (before age 23). This variable is defined among the set of children with non-missing addresses in adulthood.

*Staying with Parents.* Children are defined as staying with their parents if their 2015 address matches their parents' 2015 address. This variable is defined among the set of children with non-missing addresses in 2015 and whose parents have non-missing addresses in 2015.

*Income for those who Stay in Childhood CZ.* We measure income among the children who stay in their childhood CZ using the baseline definitions of household and individual income described above. These variables are defined among the set of children who stayed in their childhood CZ as adults.

*Fraction of Childhood Years Spent in Tract.* We calculate the fraction of childhood years that a child spends in a tract as the total number of years we observe the child in a given tract (based on their parents' tax returns) divided by the total number of years for which address data is available for the child's birth cohort before age 23. For example, for the 1983 cohort, address information before the child is 23 is available in 12 tax years (1989, 1994-1995, and 1998-2006). We construct the fraction of years in tract variable for the 1983 cohort by dividing the total number of years the child is observed in a given tract by 12.[45]

*Gender and Age.* Gender and age are obtained from the Numident file.

Children's Outcomes Observed in ACS or Census Long Form.

We report statistics at the county and CZ (but not tract) level for the following outcomes, which we observe for the subsample of individuals who appear in the Census 2000 long form or the 2005-2015 ACS.

*Employment.* In the ACS, children are defined as working if they report positive weeks worked in the past year. This and all other employment-related ACS measures described below are defined only among children who receive the ACS at age 30 or later.

*Hours Worked Per Week.* Annual hours of work are measured in the ACS as the product of hours worked per week and weeks worked per year. Individuals report weeks worked in bins; we use the midpoint of the bin to assign each individual a single value (e.g., those who choose "27 to 39 weeks" are assigned a value of 33). We convert the annual measures to average weekly hours worked by dividing annual hours worked by 51 (the midpoint of the top bin of weeks worked). Those not working in any week are coded as having zero hours of work.

[44] Since we cannot link children to parents prior to the 1978 birth cohort, we define spouse income percentile ranks relative to all individuals in the relevant birth cohort, not just those individuals linked to parents (see Section III).

[45] Because we observe locations only starting in 1989, location information in early childhood is missing for earlier birth cohorts for instance, for the 1978 birth cohort, we only observe locations starting at age 11. This censoring problem does not significantly affect our results, since the tract-level estimates remain similar when we use data for later cohorts (e.g., the 1989 cohort, for whom we observe location from birth).

*Hourly Wage.* Hourly wages are measured in the ACS by dividing reported annual wage and salary income by annual hours worked. The hourly wage is coded as missing for those with zero hours worked.

*Educational Attainment.* We measure children's educational attainment based on the highest level of education they report having completed in the ACS or the 2000 Census long form (prioritizing the ACS, since it is more recent). We say a student dropped out of high school if their educational attainment is "12th grade- no diploma" or less (hence, those with GEDs are not counted as having dropped out). We define some college attendance as having obtained "at least some college credit." We define community college completion as having an Associate's degree and 4-year college completion as having a Bachelor's degree. Graduate degree completion is defined as having a Master's, professional, or doctorate degree. All education variables are defined as having at least that level of education or higher. High school completion is defined among individuals who are at least 19 at the time they are surveyed. When measuring some college attendance, community college completion, and college completion we require that individuals are at least 24. We require that respondents are at least 30 years old at the time of the survey when measuring graduate degree completion.

*Public Assistance Receipt.* Children are coded as receiving public assistance in adulthood if they report positive public assistance income in the ACS. This variable is defined among individuals who receive the ACS at age 30 or older.

*Income for Children with Native or Immigrant Mothers.* We measure income among the children whose mothers are U.S. natives or immigrants using the baseline definitions of household and individual income described above. Children are defined as having a "native-born" mother if their mother was surveyed in the 2000 Census long form or the ACS and reported being born in the United States in either survey. Children are defined as having an immigrant mother if their mother received either survey and reported being born outside of the United States.

## B Construction of Neighborhood-Level Variables

This appendix provides definitions and sources for covariates used throughout the paper or shown in the Opportunity Atlas as neighborhood characteristics. Our source data are primarily at the tract level. We use 2010 Census tract definitions throughout. For covariates defined using 2000 tract boundaries, we use the 2010 Census Tract Relationship Files from the US Census Bureau to crosswalk 2000 tracts to 2010 tracts, weighting the 2000 tract-level covariates by the fraction of the 2000tract population that lives within the 2010 tract boundaries.

Tract-Level Characteristics:

*Jobs Within 5 Miles (2015).* The number of jobs within 5 miles of a tract is constructed using block-level information on the total number of jobs from the Workplace Area Characteristics (WAC) data files in the LEHD Origin-Destination Employment Statistics (LODES) provided by the Census Bureau. For each tract we compute the number of jobs within 5 miles as the total number of jobs in own and neighboring tracts whose centroids fall within a radius of 5 miles from the centroid of the tract.

*Number of High Paying Jobs Within 5 Miles (2015).* The number of high paying jobs within 5 miles of a tract is constructed using block-level information on the number of jobs with earnings greater than $3,333 per month from the Workplace Area Characteristics (WAC) data files in the LEHD Origin-Destination Employment Statistics (LODES) provided by the Census Bureau. For each tract we compute the number of high paying jobs within 5 miles as the number of high paying

jobs in own and neighboring tracts whose centroids fall within a radius of 5 miles from the centroid of the tract.

*Job Growth (2004-2013).* The measure of job growth at the tract level shown in the Opportunity Atlas is constructed using block-level information on the total number of jobs from 2004 to 2013 from the Workplace Area Characteristics (WAC) data files in the LEHD Origin-Destination Employment Statistics (LODES) provided by the Census Bureau. We compute job growth in each tract as the average annualized growth rate from 2004 to 2013.

*Job Density (2013).* The measure of job density at the tract level shown in the Opportunity Atlas is constructed combining block-level information on total number of jobs in 2013 from the Workplace Area Characteristics (WAC) data files in the LEHD Origin-Destination Employment Statistics (LODES) provided by the Census Bureau and tract-level information on land area in square miles from the 2010 Census Gazetteer Files. We compute job density as the number of jobs per square mile in each tract.

*Employment Rate (2000).* The rate of employment is constructed using tract-level data on labor market measures from tables NP043E and NP043C of the Census long form SF3a dataset obtained from the National Historical Geographic Information System (NHGIS) database. We construct the rate of employment in 2000 for each tract as the total employed population (the sum of employed females and employed males) divided by the total population 16 years and over.

*Poverty Rate (1990, 2000, 2006-2010, 2011-2015).* The poverty share variable is constructed as the share of individuals below the federal poverty line in each tract. For the measure in 1990 we use table NP117 from the 1990 Census form SFT3, for the measure in 2000 we use table NP087B from the 2000 Census long form SF3a, and for the measures for 2006-2010 and 2011-2015 we use table C17002 from the American Community Survey in relevant years, all obtained from the NHGIS database.

*Single Parent Share (1990, 2000, 2006-2010).* We define the share of single parents in each tract as the number of households with female head (and no husband present) or male head (and no wife present) with own children under 18 years old present divided by the total number of households with own children present. We use table NP19 of the 1990 Census form SFT3 for the measure in 1990, tables NP018E and NP018G of the 2000 Census SF1a form for the measure in 2000, and table B11003 of the 2006-2010 American Community Survey for the measure in 2010. All obtained from the NHGIS database.

*Racial Shares (2000, 2010).* Racial shares are calculated from the Census long form SF1a, tables NP008A and NP004E, taken from NHGIS database. All races (except Hispanic) exclude Hispanics and Latinos.

*Share Foreign Born* (2010). The share foreign born variable that is shown in the Opportunity Atlas is constructed as the number of foreign born residents in the 2010 Census divided by the sum of native and foreign born residents (long form SF3a, table NP021A) obtained from the NHGIS database.

*Share with Short Commute to Work and Mean Commute Time (2000, 2006-2010).* The share of workers with a short commute to work and mean commute time are constructed using tract-level data from table NP031B of the 2000 Decennial Census or tract-level data from table B08303 of the the 2006-2010 American Community Survey, both obtained from the NHGIS database. Fraction with a short to commute to work is computed by taking the share of people who commute less than 15 minutes to work over all workers 16 years and over who did not work at home. Mean commute time is constructed using the share of workers commuting to work in specific bins ($<$ 5 minutes, 5-9 minutes, 10-14 minutes, etc.), imputing the mean time commuted in a given bin (i.e. for 5-9 minutes, imputing mean commute time of 7 minutes), and then calculating a sum of imputed mean commute times within each bin weighted by the share commuting.

*Kid Counts (2000).* The counts of kids by race and gender used throughout the paper and shown in the Opportunity Atlas are constructed for kids under 18 using tract-level data from tables NP012F and NPCT012H of the 2000 Decennial Census using the NHGIS database.

*Census Return Rate (2010).* The Census return rate variable used in Figure IV and shown in the Opportunity Atlas is obtained from tract-level data from the Census 2016 Planning Database. It is calculated as the number of 2010 Census mail forms completed and returned over the number of valid occupied housing units where a Census form was expected to be delivered for mail return to Census.

*Mean Household Income (2000).* The measure of mean household income used in Figure IV is constructed using tract-level data from table NP052A of the 2000 Decennial Census found in the NHGIS database.

*Median Household Income (1990, 2012-2016).* The measure of median household income shown in the Opportunity Atlas is constructed using tract-level data from table NP80A of the 1990 Decennial Census and table B19013 of the American Community Survey (2012-2016) found in the NHGIS database.

*High School Graduate Wage Growth (2005-2014).* The measure of high school graduate wage growth is constructed using data from the 2005-2009 and 2010-2014 American Community Survey provided by NHGIS database. High school graduate wages at the tract level are computed by dividing the average high school graduate annual earnings by the product of overall average weekly hours worked and 52. High school graduate wage growth is then computed as the difference in logarithms between high school graduate wages in 2010-2014 and school graduate wages in 2005-2009.

*Share College Graduate (2000, 2006-2010).* The share college graduate variable shown in the Opportunity Atlas is constructed using tract-level data from table NP037C of the 2000 Census long form SF3a or tract-level data from table B15002 of the 2006-2010 American Community Survey (both obtained from the NHGIS database), and is calculated as the number of people aged 25 or older who have a bachelor's degree, master's degree, professional school degree, or doctorate degree, divided by the total number of people aged 25 or older in a tract.

*Population Density (2000, 2010).* The population density variable used in Figure VIIIc and shown in the Opportunity Atlas is calculated as the total tract-level population in the Census obtained from NHGIS database (long form SF1a, table H7V) divided by tract land area in square kilometers from the 2010 Census Gazetteer Files.

*Median Two-Bedroom Rent (2011-2015).* The median two-bedroom rent variable that is used in Figure X and shown in the Opportunity Atlas is constructed from tract-level ACS data (2011-2015) and is defined as the median gross rent for renter-occupied housing units with two bedrooms that pay cash rent (table AD79).

Characteristics at Other Levels of Geography:

*Job Growth (1990-2010, 2004-2013).* The measure of job growth at the CZ or MSA level that we use in Figure V and Online Appendix Figure V is constructed as the percentage change in employment between 1990 and 2010 in each CZ/MSA using county-level data from the Local Area Unemployment Statistics (LAUS) released by the Bureau of Labor Statistics. The measure of job growth at the county and CZ level that we use in the Opportunity Atlas is constructed as the average annualized growth rate in employment between 2004 and 2013 in each CZ using county level data from the Local Area Unemployment Statistics (LAUS) released by the Bureau of Labor Statistics.

*Opportunity Zones.* The list of tracts in Qualified Opportunity Zones shown in Online Appendix Figure IX was downloaded from the Community Development Financial Institutions Fund.

*Wharton Land Use Regulation Index (2008).* The Wharton Land Use Regulation Index is constructed using city-level data from Gyourko et al. (2008). The cities in the original dataset are crosswalked to 247 commuting zones (representing 87% of the US population).

*3rd Grade Math Score.* Data for 3rd grade test scores are downloaded from the Stanford Education Data Archive and measured at the district level. We create a crosswalk from districts to tracts by weighting by the proportion of land area that a given school district covers in a tract.

*High School Catchment Areas.* We match tracts to high school catchment areas across the U.S. using data on the intersection of census tracts with high school catchment areas in 2017 provided by Peter Bergman. These data come from Maponics (2017). Tracts are not perfectly nested within catchment areas; we create an approximate crosswalk by assigning tracts to the school catchment area that contains the majority of their land area. In a few cases where school catchment areas overlap (e.g. a whole tract belongs to two different school catchment areas) we assign the tract to the largest of the catchment areas that contain it. The results of our variance decomposition analysis are very similar if we alternatively assign these tracts to the smallest catchment area or simply don't use these tracts in the analysis. Shape-files of Mecklenburg County high school catchment areas in 2002 and 2017 come from the Charlotte-Mecklenburg Schools (CMS) education agency. The shape-files for the 2002 Mecklenburg County boundaries were generously provided by David Deming.

*Other Measures of Opportunity.* To compare our Opportunity Atlas measures to existing indices of economic opportunity, we obtain data for the Kirwan Child Opportunity Index at the metropolitan area level constructed by the Kirwan Institute and the Institute for Child, Youth and Family Policy (ICYFP) from diversitydatakids.org, and we obtain data for the Area Deprivation Index at the block level constructed by the University of Wisconsin School of Medicine and Public Health. Area Deprivation Index.

## C Estimating Children's Mean Incomes in Dollar Levels

In this appendix, we describe how we construct the estimates of mean incomes measured in dollars (rather than ranks) that we release publicly and use in Figures II and X. We begin by estimating equation (2) using child's household incomes (measured in real 2015 dollars) in adulthood as the outcome variable $y_i$. Denote these estimates as $\hat{y}_{cprg}^{INC}$ for children growing up in census tract $c$, with parental income percentile $p$, from racial and ethnic group $r$ and gender $g$ (we also construct analogous estimates that pool children of all racial and ethnic groups, genders, or both). To limit the influence of outliers, we replace all income values above the 99th percentile of the national cohort-specific income distribution with the average value of income above that threshold, and we replace all negative incomes with 0.

Privacy constraints prevent us from directly disclosing these statistics: in particular, unlike income ranks, incomes measured in dollars have a long right tail, and thus standard differentially private noise-infusion algorithms would require addition of substantial noise to release such statistics directly, considerably reducing their reliability. Therefore, instead of releasing these estimates directly, we predict these estimates using other adult outcomes for the same children that capture key moments of the income distribution and that we are able to release publicly: children's average household adult 1income rank ($\hat{y}_{cprg}^{RANK}$), the fraction of children with adult household income in the top 20% of the cohort-specific income distribution ($\hat{y}_{cprg}^{TOP20}$), and the fraction of children with adult household income in the top 1% of the cohort-specific income distribution ($\hat{y}_{cprg}^{TOP1}$) in each Census tract (see Section III and Online Appendix A for more details on these outcomes).

We then estimate the following regression at the Census tract level:

$$\hat{y}_{cpr}^{INC} = \zeta_{0r} + \zeta_r^{RANK}\hat{y}_{cpr}^{RANK} + \zeta_r^{TOP1}\hat{y}_{cpr}^{TOP1} + \zeta_r^{TOP20}\hat{y}_{cpr}^{TOP20} \quad (10)$$

for each racial and ethnic group $r$, as well as for estimates that pool children from all racial and ethnic groups. We run this regression using the pooled-gender estimates, and we "stack" observations from $p = 25$ and $p = 75$ to provide greater coverage of the potential cell-specific income distributions. We weight each observation for children at the 25th (75th) percentile of the parent income distribution using the number of children in the tract and racial and ethnic group below (above) median parental income.

Online Appendix Table II Panel A reports the coefficients and R-squared from these regressions. We generate predicted values of adult household income for all tract-level cells by combining the coefficients from Online Appendix Table IIa with the publicly released cell-specific values for household income rank, top 20%, and top 1%. To confirm that our predictions remain accurate for children's outcomes at particular parental income percentiles, and of specific genders (even though we did not estimate our model separately for those groups), Online Appendix Table IIIa presents correlations of the predicted values from our procedure and the actual cell-specific mean incomes. Our predictions are highly accurate; for instance, for Black females growing up in a given tract with parental income at the 25th percentile, our predictions are correlated 0.951 with the original dollar-level statistic across tracts.

We construct analogous dollar statistics at the county- and CZ-level by combining the same coefficients reported in Online Appendix Table IIa with the county- and CZ-level statistics on average household income ranks, top 20% shares, and top 1% shares. In practice, this approach produces predictions for average adult income levels that are more strongly correlated with the underlying original statistics across counties and CZs than does an alternative approach that re-estimates the prediction coefficients at the county- or CZ-level. Intuitively, the greater variation in income distributions at the tract level provides a better prediction of the average household income level even across the larger geographic areas.

We repeat the same procedure to estimate individual (rather than household) incomes. Online Appendix Table IIb presents the prediction coefficients for individual income, and Panels C and D of Online Appendix Table III report correlations between our predictions and the original statistics.

*Standard Errors.* We report approximate standard errors for our dollar estimates by rescaling the standard errors of our rank estimates, which we disclose publicly. We use equation 10 from above and our variance estimates for the three outcomes in our model, ignoring the covariance terms and treating our transformation as known:

$$SE(\hat{y}_{cprg}^{INC}) = \sqrt{\zeta_r^{RANK^2} \cdot Var(\hat{y}_{cprg}^{RANK}) + \zeta_r^{TOP1^2} \cdot Var(\hat{y}_{cprg}^{TOP1}) + \zeta_r^{TOP20^2} \cdot Var(\hat{y}_{cprg}^{TOP20})}$$

We verify in the internal data that the standard errors obtained from this linear approximation using publicly disclosed statistics are very similar to the actual standard errors on the dollar estimates. In addition, variance components estimated directly using the actual dollar statistics (both for the point estimates and standard errors) in the internal data are closely aligned with the estimates based on our approximations. For example, for children with parents at the 25th parent percentile, the approximations yield a signal SD of \$12,477 and a within-county signal SD of \$10,197, very similar to the true estimates of \$12,850 and \$10,420.

## D Controlling for Richer Family Characteristics

This appendix presents further details on how we control for other family characteristics beyond race and parent income in two analyses conducted in Sections III and IV.D and reported in Online Appendix Table IV and VII. The first analysis investigates the key dimensions along which children's outcomes differ beyond parent income at a national level, which guides our choice of the dimensions on which to allow place effects to vary. The second analysis investigates how much additional information geographic location adds above and beyond family characteristics. For both analyses, we obtain data on additional family characteristics from the 2000 Census long form, focusing on the (random) subsample of children and parents who appear in the long form.

*Dimensions of Variation.* To answer the first question, we focus on the following vector of discrete family characteristics $X$: house size (number of bedrooms), race, gender, number of siblings, and both the mother and father's education (measured as no school, less than high school, college no degree, associate's degree, bachelor's degree, and professional degree), occupation (grouping 2000 Census Occupation codes into their Occupation Categories), marital status, and citizenship. We cycle over members $x \in X$ and regress an outcome (e.g., incarceration) on 100 bins of parent income interacted with $x$. We select the covariate $x^*$ with the highest adjusted R-squared and remove it from the list $X$. Then, we cycle over members of $x \in X$ again, regressing incarceration on parent income interacted with $x^*$ and $x$. We select the covariate with the highest adjusted R-squared and repeat the exercise until we obtain the three covariates $x_1, x_2, x_3$ that resulted in the highest incremental R-squared in this sequential exercise. We list these covariates in Online Appendix Table IV, along with the R-squareds from regressions of the outcome on parent income, parent income interacted with $x_1$, parent income interacted with $x_1$ and $x_2$, and parent income interacted with $x_1, x_2, x_3$. Panel A reports results with individual income rank as the outcome; Panel B with household income rank; and Panel C with incarceration.

*Explanatory Power of Geography.* To answer the second question, we expand the set of family characteristics $X$ above to additionally include several continuous variables: mother and father's individual income ranks, their ages, and their household incomes. We also increase the granularity of our education and occupation measures, measuring education as the sixteen categories in the 2000 Census and occupation at the three digit level. If any variable is missing, we code it as 0, and including an indicator for the variable being missing.

We then estimate four OLS regressions in a dataset with one observation per child and childhood tract (e.g., a child who lived in two different tracts between ages 018 would have two rows in the dataset), weighting by the fraction of childhood spent in each tract. First, we regress household income rank in adulthood on parent income in childhood, transformed by the tract-invariant transformation of parental income rank $f_{rg}(p)$. Second, we regress household income rank on parent income interacted with tract fixed effects. Third, we regress household income rank on parent income and the vector of family characteristics $X$. Fourth, we regress household income rank on parent income and the vector of covariates, interacting parent income with tract fixed effects.

Online Appendix Table VII reports the adjusted R-squareds from these four regressions by subgroup. The first row is for all observations pooled, and the four specifications are fully saturated with race and gender fixed effects. The second row onwards report results for various race and gender subgroups.

## E Alternative Standard Error Estimates

In this appendix, we compare our baseline heteroskedasticity-robust standard errors, which could be downward-biased in small cells, to an alternative estimator proposed by Kline, Saggio, and Solvsten Kline et al. (2020) that provides unbiased standard errors.

*Construction of Baseline Standard Errors.* We estimate our baseline heteroskedasticity-robust standard errors (HC1) by estimating the following regression in each Census tract and subgroup:

$$y_i = \alpha_{crg} + \beta_{crg} \times f_{rg}(p_i) + \varepsilon_i,$$

We then compute SEs at the desired parent percentiles using the following formula:

$$var(\hat{y}_{crgp}) = var(\alpha_{crg}) + var(\beta_{crg}) \times f_{rg}(p)^2 + cov(\alpha_{crg}, \beta_{crg}) \times f_{rg}(p)^2$$

*Construction of KSS Standard Errors.* To implement the KSS estimator, we construct the variance of $\hat{\beta}$ (coefficients on equation 1) using the sandwich estimator:

$$\hat{V}[\hat{\beta}] = (X^\top W X)^{-1} X^\top W \hat{\Omega} W X (X^\top W X)^{-1}$$

where $X$ is the design matrix for each regression, $W$ is the exposure weighting matrix, and $\hat{\Omega}$ is the individual-level variance matrix. What distinguishes the KSS standard errors is $\hat{\Omega}$. In the KSS case, the variances are defined as:

$$\hat{\Omega}_{ii} = \frac{y_i \hat{\epsilon}_i}{1 - P_{ii}}$$

where $P_{ii}$ is the leverage of observation $i$, the corresponding diagonal element of the hat matrix. This differs from the HC1 variance matrix, which is simply:

$$\hat{\Omega}_{HC1,ii} = \hat{\epsilon}_i^2$$

When sample sizes are small, each observation will exhibit considerable leverage over its own residual which leads to a downward bias in the individual-level variance term of HC1 standard errors. The KSS formula corrects this bias.

*Comparison of KSS Standard Errors to Conventional Robust Standard Errors.* Online Appendix Figure Ia presents a binned scatterplot of the KSS sampling variances vs. the robust variances for mean tract-level household income ranks for children with parents at the 25th income percentile. The correlation between the two variances is 0.96, with a mean absolute difference in variance of 0.000046, or 12%.

To assess the degree of bias of the conventional robust standard errors in small cells, we plot the average KSS and robust sampling variance as a function of the number of low-income children in each tract in Online Appendix Figure Ib. In the smallest cells, there is a larger divergence between the two standard error estimates, as expected, but even in cells with counts below 500, the mean absolute difference in the sampling variance estimates is 0.000099, or 13.9%.

Finally, we replicate the variance decompositions in Table II using robust standard errors without differential privacy noise added in Online Appendix Table VIa and using KSS standard errors in Online Appendix Table VIb. In particular, we use Equation (5) of KSS to estimate within-country variances across tracts. The two methods yield very similar estimates of signal variances and other variance components.

We conclude that the KSS standard errors are quite similar to the conventional robust standard errors in our application.

# F Noise Infusion for Privacy Protection

Due to privacy concerns, we are not able to directly release the baseline estimates described in Section III and Online Appendix E without adding noise. In this appendix, we describe the noise-infusion procedure we use to protect privacy and release statistics at the tract-, county-, and CZ-level.

Our method, which follows the method developed in Chetty and Friedman (2019) and is inspired by techniques developed in the differential privacy literature, adds independent, normally distributed noise to each estimate $\hat{y}_{cprg}$. The standard deviation of the noise distribution is chosen based on the sensitivity of the estimates to a single individual's data. We calculate the sensitivity of the estimates by calculating the maximum change in the estimate $\hat{y}_{cprg}$ that can result from adding a single observation to the relevant tract-race-gender cell. Because all of the outcome variables we process using this approach are bounded, this value is well-defined. We then compute the maximum sensitivity of the estimates across all tracts within a given state; label this value as $\delta^y_{rgs}$ for outcome variable $y$, state $s$, racial and ethnic group $r$,and gender $g$. We then add noise with standard deviation of $\frac{\delta^y_{rgs}}{n_{crg}\varepsilon}$, that is proportional to this maximum state-level sensitivity and inversely proportional to the number of observations $n_{crg}$ in the relevant subgroup and a parameter $\varepsilon$ that controls the degree of disclosure risk, which we set at $\varepsilon = 8$. We report the SD of the noise added to each tract-level estimate in the publicly available data.

We also add noise to the standard errors of these estimates following an analogous procedure, but the resulting standard errors remain only an estimate of the sampling error inherent in the underlying cell-level estimate $\hat{y}_{cprg}$ and importantly excluding the error resulting from the adding noise from the procedure described in the previous paragraph. The total sampling variance of the each released estimate combines the square of the released "raw" standard error plus the square of the standard deviation of added DP noise ($\frac{\delta^y_{rgs}}{n_{crg}\varepsilon}$). We report the square-root of this total noise variance in our published datasets.

In each cell, we release statistics (infused with noise) from Census at two parental income percentiles (typically $p = 25, 75$), but we report statistics for five parental income percentiles for most cells in the public data ($p = 1, 25, 50, 75, 100$). Using the public estimates for these two points, we linearly extrapolate to produce estimates at the other values of parent income percentile. It is more complex to produce estimates of sampling variances at the other parent income percentiles, because the sampling variance for a linear combination of estimates depends not only on the sampling variance for each estimate but also on the covariance in sampling variance. For instance, the sampling variance for an estimate at $p = 50$ is $SV_{crg,p=50} = \frac{1}{2}\left(SV_{crg,p=25} + SV_{crg,p=75} + Cov^{SV}_{crg,p=25,75}\right)$. We estimate the relevant covariances using a parametric model.[46] Specifically, using non-noise-infused estimates of sampling variance inside Census, we estimate a model in which we predict the sampling variance for the p25 and p75 estimates within a tract using the saturated interaction of indicators for quintiles of the fraction of children below the median parental income in a given cell, deciles of the estimate $\hat{y}_{cprg}$, and the function $1/\sqrt{n_{cprg}}$ (where $n$ is the number of below-median children in the cell for estimates at $p = 25$ and number of above-median children in the cell for estimates at $p = 75$). We then release the coefficient estimates from this model and construct predictions of

[46] Directly using noise-infused estimates of the cell-specific covariance term sometimes produces negative-definite variance-covariance matrices or otherwise unstable estimates.

the covariance term for each cell combining the model parameters with cell-specific covariates. We then use the formula above (or an equivalent formula for $p = 0, 100$) to calculate the raw sampling variance at the other percentiles $p = 0, 50, 100$. Finally, we further add a term reflecting the sampling variance arising from the infused noise, which (due to the independence of the noise infused into the two released estimates) is simply a linear extrapolation of the variances of the noise infused at $p = 25$ and $p = 75$ using the same weights as for the linear extrapolation of the estimates.

For a small share (6%) of cells, we cannot release estimates at $p = 25$ ($p = 75$) due to a very small number of children from low (high) income families. In cases where fewer than 10% of children in the tract have parental incomes below (above) median, we instance release (noise-infused) estimates and standard errors for estimates from $p = 0$ and $p = 50$ ($p = 50$ and $p = 100$). We then follow the procedure described above to produce standard errors for those percentiles and then extrapolate the estimates (linearly) and the standard errors (using the non-linear approach described in the previous paragraph) to other percentiles. The procedure for extrapolating sampling variances only differs in that we do not estimate a different prediction function using released sampling variances from these alternative percentiles; rather, we use the same function as estimated above from $p = 25, 75$.

## G Predicting Contemporary Mobility Using Historical Data

In this appendix, we assess the predictive power of historical estimates of upward mobility from the Opportunity Atlas for forecasting upward mobility for children growing up today relative to traditional proxies for opportunity (e.g., poverty rates) that are available contemporaneously while children are growing up. We focus specifically on predicting the outcomes of children born today using Opportunity Atlas data, which is based on children who are approximately 30 years old at the point at which we measured their incomes in adulthood. That is, we seek to forecast upward mobility for the current generation based on estimates for children born $k = 30$ cohorts earlier.

*Derivation of Estimator.* Formally, our goal is to predict expected incomes in adulthood ($\bar{y}_{ct}$) for the current generation $t$ of children growing up in Census tract $c$ using estimates of mean incomes in adulthood for an earlier cohort of children ($\hat{y}_{c,t-k}$) and contemporary neighborhood poverty rates ($\bar{x}_{ct}$). We seek to determine how much weight an optimal (MSE-minimizing) linear forecast would place on $\hat{y}_{c,t-k}$ vs. $\bar{x}_{ct}$, focusing on within-county variation across tracts since that is the most relevant variation for local applications, such as the Opportunity Zone application discussed in Section IV.F.

We focus on solving this prediction problem using our estimates pooling children of all races and genders for children born to parents at the 25th percentile of the parent-income distribution. To simplify exposition, we drop subscripts for these characteristics. We seek to estimate the parameters in the regression:

$$\bar{y}_{ct} = \theta_0 + \beta_y \hat{y}_{c,t-k} + \beta_x \bar{x}_{ct} + \omega_{ct} \tag{11}$$

for a specific cohort $t$. We demean $\hat{y}_{c,t-k}$ and $\bar{x}_{ct}$ within each county in order to isolate within-county variation. To facilitate interpretation of the coefficients $\beta_y$ and $\beta_x$ as relative weights, we divide $\hat{y}_{c,t-k}$ and $\bar{x}_{ct}$ by their respective standard deviations, so that both variables have variance 1.

If we could observe $\bar{y}_{ct}$ directly in our data, we could directly estimate (11) using OLS. In practice, because we only observe 11 birth cohorts, we cannot observe both of these variables when $k = 30$. In this appendix, we show how one can nevertheless identify $\beta_y$ and $\beta_x$ by estimating the VCV matrix of $\{\bar{y}_{ct}, \hat{y}_{c,t-k}, \bar{x}_{ct}\}$ in available data under relatively weak stationarity assumptions.

Let $\tilde{y}_{c,t-k}$ denote the residual from a linear regression of $\hat{y}_{c,t-k}$ on $\bar{x}_{ct}$:

$$\tilde{y}_{c,t-k} = \hat{y}_{c,t-k} - \text{Cov}\left(\hat{y}_{c,t-k}, \bar{x}_{ct}\right) \bar{x}_{ct},$$

By the Frisch-Waugh-Lovell theorem, $\beta_y$ is the coefficient from the regression of $\bar{y}_{ct}$ on $\tilde{y}_{c,t-k}$ :

$$\beta_y = \frac{\text{Cov}\left(\bar{y}_{ct}, \hat{y}_{c,t-k} - \text{Cov}\left(\hat{y}_{c,t-k}, \bar{x}_{ct}\right) \bar{x}_{ct}\right)}{\text{Var}\left(\hat{y}_{c,t-k} - \text{Cov}\left(\hat{y}_{c,t-k}, \bar{x}_{ct}\right) \bar{x}_{ct}\right)} = \frac{\text{Cov}\left(\bar{y}_{ct}, \hat{y}_{c,t-k}\right) - \text{Cov}\left(\hat{y}_{c,t-k}, \bar{x}_{ct}\right) \text{Cov}\left(\bar{y}_{ct}, \bar{x}_{ct}\right)}{1 - \text{Cov}\left(\hat{y}_{c,t-k}, \bar{x}_{ct}\right)^2}. \tag{12}$$

In this equation, all covariances and variances are conditional on $t$, so that $\beta_y$ is a statistic that is a function of the variation across tracts $c$. An analogous derivation for $\beta_x$ yields:

$$b = \frac{\text{Cov}\left(\bar{y}_{ct}, \bar{x}_{ct}\right) - \text{Cov}\left(\bar{x}_{ct}, \hat{y}_{c,t-k}\right) \text{Cov}\left(\bar{y}_{ct}, \hat{y}_{c,t-k}\right)}{1 - \text{Cov}\left(\bar{x}_{ct}, \hat{y}_{c,t-k}\right)^2}. \tag{13}$$

We can directly estimate $\text{Cov}\left(\bar{x}_{ct}, \hat{y}_{c,t-k}\right)$. We cannot directly observe the terms $\text{Cov}\left(\bar{y}_{ct}, \bar{x}_{ct}\right)$ or $\text{Cov}\left(\bar{y}_{ct}, \hat{y}_{c,t-k}\right)$. To make progress, we make two assumptions relating to the stability of the joint distribution of $y$ and $x$ over time.

First, we assume that the outcome $y$ has a stationary autocorrelation structure that decays exponentially with the lag $k$ between periods:

$$\text{Cov}\left(\hat{y}_{ct}, \hat{y}_{c,t-k}\right) = \text{Cov}\left(\hat{y}_{ct}, \hat{y}_{c,t-1}\right)^k$$

for all $t$ and $k$. The evidence from Figure VII is consistent with this parametric approximation to the autocorrelation structure of $y_{ct}$ across the 12 cohorts that we can observe in our data, and here we assume that this proportional decay process continues over a longer period.[47]

Second, we assume that the contemporary correlation between $y$ and $x$ is stable over time:

$$\text{Cov}\left(\bar{y}_{ct}, \bar{x}_{ct}\right) = \text{Cov}\left(\hat{y}_{c,t-k}, \bar{x}_{c,t-k}\right)$$

for all $t$ and $k$. To evaluate this assumption, in our empirical implementation below, we estimate this covariance term in two independent samples, once using the 1978-83 birth cohorts and once using the 1984-89 birth cohorts, and find very similar estimates.

Under these two assumptions, we can calculate the key coefficients from the prediction equation entirely using moments that we can estimate in our data, in particular the serial correlation in upward mobility across cohorts and correlation between poverty rates and upward mobility in earlier cohorts.

*Empirical Implementation.* We use the formulas in (12) and (13) to predict upward mobility for children born in 2010 ($\bar{y}_{ct}$) using poverty rates based on the 2010 Census ($\bar{x}_{ct}$) and the baseline Opportunity Atlas mean income rank estimates for children with parents at the 25th percentile in the 1978-83 cohorts ($\hat{y}_{c,t-k}$). Weighting tract-level observations by population below median income and demeaning all variables within county to obtain within-county covariances, we estimate $\text{Cov}\left(\bar{x}_{ct}, \hat{y}_{c,t-k}\right) = -0.59$ and $\text{Cov}\left(\bar{x}_{c,t-k}, \hat{y}_{c,t-k}\right) = -0.57$ (where $\bar{x}_{c,t-k}$ represents poverty rates based on the 1980 Census). Based on the autocorrelation estimate of 0.99 reported in Figure VII, we estimate $\text{Cov}\left(\bar{y}_{ct}, \hat{y}_{c,t-k}\right) = (0.99)^{30} = 0.74$.

[47] While exponential decay is consistent with the available data, other parametric forms also provide a good approximation. We find very similar results with other parametrizations; for instance, if we assume that the autocorrelation decays linearly across cohorts instead of exponentially, we obtain estimates of $\beta_y = .26$ and $\beta_x = -.11$ (as compared with $\beta_y = 0.29$ and $\beta_x = -0.09$ assuming exponential decay).

Plugging these values into (12) and (13), we obtain estimates of $\beta_y = 0.29$ and $\beta_x = -.09$. The optimal estimator thus places substantial weight on historical opportunity measures – more than 3 times as large as the weight placed on standardized poverty rates – when forecasting long-term outcomes for children in the 2010 cohort, even conditioning on contemporaneous poverty rates.

Replicating these calculations to forecast outcomes for the 2010 birth cohort using outcomes for children born around 1990 yields similar results. In particular, we use the Opportunity Atlas data on mean income ranks at age 26 for the children with parents at the 25th percentile in the 1984-1989 cohorts as $\hat{y}_{c,t-k}$, poverty rates from the 1990 Census as $\bar{x}_{c,t-k}$, and poverty rates from the 2010 Census as $\bar{x}_{ct}$. We re-estimate the covariances between poverty and mobility, finding $\text{Cov}\left(\bar{x}_{ct}, \hat{y}_{c,t-k}\right) = -0.57$ and $\text{Cov}\left(\bar{x}_{c,t-k}, \hat{y}_{c,t-k}\right) = -0.57$. Using our formula, we obtain estimates of $\beta_y = .38$ and $\beta_x = -.08$. We conclude that the historical Opportunity Atlas data has substantial relevance for targeting current policies, even conditional on contemporary data on poverty rates.

*Reconciliation with Data on Changes in Opportunity.* Although historical data on upward mobility are highly predictive of outcomes for the current generation, this does not imply that rates of upward mobility are completely fixed over time. In a recent paper, Chetty et al. (2024) focus on how upward mobility changed at the county level between the 1978 and 1992 cohorts (measuring income ranks in adulthood at age 27). To understand how the changes they identify are consistent with our results here on the stability of the Opportunity Atlas estimates across cohorts, observe that

$$\text{Var}\left(\hat{y}_{ct} - \hat{y}_{c,t-k}\right) = \text{Var}\left(\hat{y}_{c,t}\right) + \text{Var}\left(\hat{y}_{c,t-k}\right) - 2\text{Cov}\left(\hat{y}_{c,t}, \hat{y}_{c,t-k}\right).$$

The Chetty et al. (2024) data imply that $\text{Corr}\left(\hat{y}_{c,1992}, \hat{y}_{c,1978}\right) = 0.83$ across counties for children with parents at the 25th percentile, pooling all racial groups. This is very similar to what one would expect based on the one-year autocorrelation of 0.99 shown in Figure VII, since $0.99^{15} = 0.86$.[48] Chetty et al. (2024) also report that $SD\left(\hat{y}_{c,1978}\right) = 4.0$ ranks and $SD\left(\hat{y}_{c,1992}\right) = 3.5$ ranks. Together, these parameters imply that $SD\left(\hat{y}_{c,1992} - \hat{y}_{c,1978}\right) = 2.2$ ranks. Hence, the standard deviation of changes in opportunity over the 15 cohorts studied by Chetty et al. (2024) across counties is 55% as large as the standard deviation in levels of upward mobility in the 1978 cohort across counties. These calculations show that even though opportunity is highly persistent over time – with a correlation of 0.83 across 15 cohorts – and is therefore a very useful predictor for targeting current policies as shown above, there are still meaningful changes in rates of upward mobility within areas.

## H Estimating the Causal Share of Observational Variation Across Tracts

In this appendix, we show how one can interpret our estimates of $\lambda$ in Section V as estimates of the share of the observational variation in outcomes across Census tracts due to causal effects of place.

Suppose that child $i$'s outcome $y_i = \mu_{c(i),p(i)} + \theta_i$, where $\mu_{cp}$ denotes the causal effect of growing up in tract $c$ given parents at percentile $p$ and $\theta_i$ denotes a selection term that reflects family inputs or other factors unrelated to where a child grows up. Define $\bar{y}_{cp} = E[\theta_i|c,p]$ as the mean income in adulthood in the observational sample; similarly define $\bar{\theta}_{cp} = E[\theta_i|c,p]$ as the mean selection effect

[48] This estimate is based on persistence at the tract level within counties; in practice, we find similar rates of persistence over time across and within counties.

in the observational sample, which can vary across tracts, so that $\bar{y}_{cp} = \mu_{cp} + \bar{\theta}_{cp}$. We also observe incomes in adulthood from the experimental sample, which we denote by $y^E_{cp}$. By construction, the selection term does not vary across tracts in the experimental sample, and so $y^E_{cp} = \mu_{cp} + \theta_E$ (where $\theta_E$ is a constant selection term reflecting selection into the experimental sample).

Consider regressing the tract-level outcomes from the experimental sample ($\tilde{y}_{cp}$) on the tract-level outcomes from the observational sample ($\bar{y}_{cp}$), as in equation 7. The regression coefficient is

$$\lambda = \frac{Cov(y^E_{cp}, \bar{y}_{cp})}{Var(\bar{y}_{cp})} = \frac{Var(\mu_{cp}) + Cov(\bar{\theta}_{cp}, \mu_{cp})}{Var(\bar{y}_{cp})}.$$

If $Cov(\bar{\theta}_{cp}, \mu_{cp}) = 0$ in the observational sample – i.e., if the selection and causal components are uncorrelated across tracts – then the regression coefficient simplifies to $\lambda = Var(\mu_{cp})/Var(\bar{y}_{cp})$, the fraction of the variation in $\bar{y}_{cp}$ that is due to causal effects. If instead $\bar{\theta}_{cp}$ is correlated with $\mu_{cp}$, the regression coefficient $\lambda$ cannot be interpreted as the variance share of causal effects. However, even in this case we can still test the null hypothesis that there are no causal effects by estimating $\lambda$. Under the null hypothesis where $\mu_{cp} = 0$, adult outcomes$\tilde{y}_{cp}$ would not vary across tracts and $Cov(y^E_{cp}, \bar{y}_{cp}) = 0$. As a result $\lambda > 0 \Leftrightarrow Var(\mu_{cp}) > 0$; rejecting the null hypothesis that $\lambda = 0$ also rejects the null hypothesis that there are no causal effects.

# I Comparison to Moving to Opportunity Estimates

This appendix describes the construction of the site-by-treatment-arm estimates that we use to compare the observational predictions of the Opportunity Atlas to the causal estimates from the Moving to Opportunity experiment.

To construct $\hat{y}^{MTO}_{ws}$ (the left-hand-side variable in equation 8), we begin with previously published estimates of the intent-to-treat (ITT) effects of assignment to the Section 8 and Experimental treatment arms (relative to the Control arm) from each of the five sites for children who were younger than 13 at the time of the experiment (Chetty et al. (2016), Online Appendix Table 7b, Panel A). We then transform these ITT estimates in treatment-on-the-treated estimates, dividing by the site- and treatment-arm-specific voucher take-up rate, and then add back the mean observed earnings in the site-specific control group (Chetty et al. (2016), Online Appendix Table 7b, Panel A) to arrive at our estimates of $\hat{y}^{MTO}_{ws}$.

To construct $\hat{y}_{ws}$ (the right-hand-side variable in equation 8), we seek to estimate the tract-specific pooled-race pooled-gender average individual earnings (measured in dollars). We measure incomes for children at age 26 (the average age at which children's earnings were measured in the MTO sample) with parents at the 10th percentile of the income distribution (approximately the average income percentile of parents in the MTO sample). Due to privacy limitations, we are unable to directly release these dollar estimates; instead, we predict the mean incomes from our rank estimates using a procedure analogous to the one described in Online Appendix C.[49] We use the predictions from this model for children at $p = 25$ and $p = 75$ to linearly extrapolate to construct estimates of individual incomes at $p = 10$. Finally, to create estimates to match each

[49] We were unable to release tract-level estimates of the probability of reaching the top 20% and top 1% of the individual income distribution at age 26 because of limitations in our total privacy budget. We therefore estimate the model from Online Appendix Table II using only mean income ranks to predict individual income. These predictions turn out to be quite accurate for the sub-population relevant for the MTO exercise; in particular, the true pooled-race pooled-gender tract-specific estimates of individual income for children from $p = 25$ are correlated 0.94 with our predictions from mean income ranks. Intuitively, the upper tail of the tract-specific income distribution are less important for the rank-to-dollar prediction from children in very low-income, high poverty areas.

site and treatment arm from MTO, we identify neighborhoods involved in the MTO experiment by mapping the neighborhood names listed in Online Appendix Table 1c of Chetty et al. (2016) to Census tracts. We then take a population-weighted mean of the tract-specific individual income estimates across the relevant Census tracts in each site and treatment arm. For the control group, we use these estimates directly. Because children younger than 13 at the time of the MTO experiment were 8 years old on average, they were exposed to their original (pre-MTO) neighborhoods for 8/23 years and their new (post-MTO) neighborhoods for 15/23 years on average. Therefore, for the Section 8 and Experimental groups, we take a weighted average of the estimates based on the control group locations and the treatment group locations, with weights of 8/23 and 15/23, respectively.

The estimate of $\lambda$ that we obtain from regressing $\hat{y}_{ws}^{MTO}$ on $\hat{y}_{ws}$ should be interpreted as a rough approximation for at least two reasons. First, we use data on the most common initial neighborhoods to which MTO participants moved (from Appendix Table Ic of CHK) to estimate $\hat{y}_{ws}$ rather than the exact locations where MTO children grew up throughout their childhood. Second, our estimates of $\hat{y}_{ws}$ themselves contain estimation error, $\hat{\lambda}_{MTO}$ slightly understates the fraction of the variance in the conditional expectation $\bar{y}_{cp}$ due to the causal effects of place. Given the reliability estimate of about 0.9 in Table II, the second source of bias leads us to understate $\lambda$ by about 10%. Quantifying the magnitude and sign of potential bias from the first issue is more challenging, but we expect that this source of error is likely to be smaller than the uncertainty in $\hat{\lambda}_{MTO}$ due to sampling error.

## J Quasi-Experimental Estimates of Causal Effects

In this appendix, we provide further details on the sample construction, variable definitions, empirical specifications, and results underlying the quasi-experimental movers analysis of neighborhood effects in Section V.B.

*Sample Construction and Variable Definitions.* Our core sample and data construction are the same as that described in Section III, but we expand the sample in two directions that increase our ability to observe moves at younger ages. First, we extend our analysis to include the 1978-1991 cohorts. Second, we focus on income ranks measured at age 24, as in Chetty and Hendren (2018a), in order to be able to measure income for the most recent (1991) cohort. We also present results (for a smaller set of ages) for income ranks measured at later ages, up to age 30.[50]

Using the location of each child's parents in each year in our sample, we form a sample of one-time movers. These are defined as children whose parents move across tracts exactly once when the children are age 28 or below.[51] We define the year of the move as the tax year in which the parents report living in a different tract relative to the previous year. In cases where we do not observe sequential years of location information (e.g. we do not observe 1990-93 and 1996-97), we assign the year of move as the midpoint between the two nearest years in which different addresses are reported (e.g. if we see a new location in 1994 relative to 1989, we assign the year of move to be 1992.5). In cases where this leads to a non-integer year of move, we randomly select the nearest

[50] We also analyze impacts on other outcomes: marriage at age 30, incarceration in 2010, and teenage birth. Each of these variables are defined for a subset of the available cohorts. Marriage at age 30 cannot be observed past the 1985 cohort since our data ends in 2015. Because individual income is only well defined starting in 2005, our age 24 individual income measure is missing for cohorts 1978-1980. Finally, we require incarceration to be measured after age 23, and therefore omit cohorts 1987 and later for that outcome.

[51] When constructing the sample, we observe location up to age 30. But, as discussed below, we follow Chetty and Hendren (2018a) and require that we observe the parents in the destination for at least two years. Therefore, the oldest age of move for the children is 28.

year for the move (1992 or 1993 in the case above). We then define the child's age at the time of the move as the year of the move minus the child's cohort.

Following Chetty and Hendren (2018a), we make three additional sample restrictions. First, we restrict to moves between origins and destinations that have at least 20 observations used to calculate $\bar{y}_{op}$ and $\bar{y}_{dp}$. As shown in Online Appendix A of Chetty and Hendren (2018a) imposing such sample restrictions limits the impact of attenuation bias from sampling error in the $\bar{y}_{cp}$ estimates. Second, we require that we are able to observe the parents for at least two years after the move in order to enter the sample (i.e., we only consider moves through 2013, since location is observed until 2015). Third, we require families to move at least 25 miles to isolate "real" moves; moves less than 25 miles suffer from more severe measurement error in children's actual locations, as we discuss in detail below.

We use the sample of children whose parents are *not* one-time movers, i.e. those observed in exactly one or 3+ tracts, to estimate children's predicted outcomes $\bar{y}_{cp}$ in tract $c$ using the regression specification in (2). We do not include one-time movers to ensure that a child's own outcome does not enter our definition of neighborhood quality.

Online Appendix Table XI presents summary statistics for the one-time movers sample and the complementary sample used to estimate children's predicted outcomes.

*Empirical Specifications.* In the one-time movers sample, consider the outcomes of child $i$ with parental income rank $p_i$ who moved at age $m_i$ from origin tract, $o$, to destination tract, $d$. We estimate childhood exposure effects on a given outcome $y_i$ using a specification analogous to that developed in Chetty and Hendren (2018a). Let $\Delta_{odp} = \bar{y}_{dp} - \bar{y}_{op}$ denote the difference in the income rank of exposure-weighted residents in the destination versus origin for children with parental income rank $p$.

We estimate three types of regression specifications: a semi-parametric specification (used in Figure IX), a parametric specification (used in Online Appendix Table VIII, Column 1), and a parsimonious specification (used in Online Appendix Table VIII, Column 2). The semi-parametric specification is given in equation (9) in the main text. In the parametric specification, we parameterize the age-specific effects plotted in Figure IX using a two-piece linear spline, permitting different slopes above and below age 23:

$$\begin{aligned} y_i &= \sum_{m=2}^{28} I(m_i = m)\left[\alpha_m + \phi_m \bar{y}_{op} + \zeta_m p_i\right] + I(m_i \leq 23)(\gamma' + \gamma m_i)\Delta_{odp} \\ &\quad + I(m_i > 23)(\rho' + \rho m_i)\Delta_{odp} + \varepsilon_i \end{aligned} \tag{14}$$

Here, the coefficient of interest is $\gamma$, the annual childhood exposure effect, which is the average effect of moving to a tract with 1 percentile higher observed income ranks one year earlier, at or before age 23. The coefficient $\rho$ measures the corresponding slope for moves after age 23.

The parsimonious specification is:

$$\begin{aligned} y_i &= \sum_{m=2}^{28} I(m_i = m)\left[\alpha_m + \zeta_m p_i\right] + \phi'_m \bar{y}_{op} + I(m_i \leq 23)(\gamma' + \gamma m_i)\Delta_{odp} \\ &\quad + I(m_i > 23)(\rho' + \rho m_i)\Delta_{odp} + \varepsilon_i, \end{aligned}$$

which drops the interaction between age-at-move and origin tract predicted outcomes from the parametric specification.

In all of these specifications, we account for measurement error in $\bar{y}_{op}$ and $\Delta_{odp}$ using a split-sample instrumental variables approach. We randomly split those in the complementary sample

into two groups of families (thereby requiring that siblings are included in the same group to ensure independence of the samples), and instrument for $\bar{y}_{op}$ and $\Delta_{odp}$ measured in one group using the same variables as measured in the second group.

*Baseline Estimates.* Column 1 of Online Appendix Table VIII presents OLS regression estimates of $\gamma$ and $\rho$ using (14). Consistent with the non-parametric estimate in Figure IX , we obtain an estimate of $\gamma = 0.027$, implying that $23 \times 0.027 = 62\%$ of the variation in observational estimates can be attributed to the causal effect of neighborhoods. Column 2 presents estimates from a more parsimonious specification that eliminates the interactions between age-of-move dummies and origin place quality $\bar{y}_{op}$, effectively constraining the coefficient on $\bar{y}_{op}$ to be constant across ages. The coefficients are again very similar. Columns 3 and 4 replicate Column 1, replacing household income with an indicator for being married at age 30 (Column 3) and for being incarcerated on April 1, 2010 (Column 4).[52] We obtain similar estimates of childhood exposure effects – with a convergence rate of approximately 2.5% per year of exposure to the mean outcomes observed in the destination – for these outcomes as well.

*Validation of Identification Assumption.* The preceding analysis rests on the assumption that the potential outcomes of children who move to better or worse neighborhoods do not systematically vary with age of move. We evaluate the validity of this assumption using two approaches. First, in Column 5 of Online Appendix Table VIII, we add family fixed effects to the specification in Column 1. This approach identifies exposure effects from comparisons between siblings. The coefficients in Column 5 are very similar to those in Column 1.

These sibling comparisons show that confounds due to factors that are fixed within families are not a significant source of bias in our estimates, but they do not account for *time-varying* factors, such as a change in family environment at the time of the move (e.g., a new job) that directly affects children in proportion to exposure time independent of neighborhoods. To evaluate whether such unobservables might bias our estimates, we implement a set of outcome-based placebo tests in Online Appendix Table IX that exploit the heterogeneity in place effects across outcomes documented in Section IV.B.

We start from the parsimonious specification in Column 2 of Online Appendix Table VIII and include not only $\bar{y}_{op}$ and $\Delta_{odp}$ as regressors, but also analogous tract-level predictions of marriage rates at age 30, as well as incarceration rates in 2010 (for men) and teenage birth rates (for women), each interacted with age.[53] In Column 1, we use children's income ranks at age 24 as the dependent variable, as in Online Appendix Table VIII Column 2. For both men (in Panel A) and women (in Panel B), the coefficient measuring the exposure effects to neighborhoods based on the income predictions remains similar to that in our baseline specification. However, moving to an area with higher incarceration rates (for men), teenage birth rates (for women), or marriage rates has no significant impact on children's incomes, conditional on observed incomes in the destination. Column 2 repeats this exercise with marriage as the dependent variable; Columns 3 and 4 do so with incarceration and teenage birth (respectively). In each case, the neighborhood quality measure based on predictions of the dependent variable is strongly significant with a coefficient of similar magnitude to those reported in Appendix Table VIII, but the coefficients on the other "placebo" predictions are typically statistically insignificant.

The magnitudes of the placebo and actual coefficients in Panel A of Online Appendix Table IX cannot be directly compared because each of the outcomes are measured in different units (fractions for marriage, ranks for income, etc.). To make units comparable and quantify the potential amount

[52]Note that we replace not only the left-hand side variable but also the neighborhood-specific predictions with these alternative outcome measures.

[53]We use the more parsimonious specification to eliminate the multiple sets of interactions, which reduce power; results are qualitatively similar though noisier with using specification in Column 1 of Appendix Table VIII.

of bias arising from any failures of these placebo tests, Panel B of Online Appendix Table IX rescales the coefficients in Panel A by the coefficient obtained by regressing the dependent variable outcome on the placebo outcome at the individual level. For example, for the placebo test of income rank on tract-level marriage (Column 1, Row 2), income rank is regressed on a marriage indicator along with the other controls from our parsimonious movers regression specification, including all variables except for age at move interacted with move quality. We then multiply the original coefficient by this regression coefficient. This rescaling quantifies the degree of bias in the own-outcome coefficient that would emerge through a failure of the placebo. The resulting rescaled placebo coefficients in Panel B are all an order of magnitude smaller than the actual (non-placebo), indicating that the scope for bias through channels associated with the placebo outcomes is very small.

Since it is unlikely that a correlated shock - such as an increase in wages when the family moves - would covary precisely with differences in neighborhood quality across all of these outcomes, these tests indicate that the variation in children's outcomes across neighborhoods for movers is driven by causal exposure effects.[54]

*Heterogeneity Analysis.* One concern with our baseline estimates is that they are identified from the set of families who choose to move to a given area, and hence may capture causal effects that apply only to the particular families who chose to move to a neighborhood that is good for their children, rather than a broader population. For example, our estimates of $\lambda$ might not have external validity to other families who may be induced to move by other factors, such as changes in housing policies.

In the presence of such selection on heterogeneous treatment effects, we would expect moves to better areas ($\Delta_{odp} > 0$) to produce larger exposure effects in absolute value than moves to worse areas ($\Delta_{odp} < 0$). Intuitively, in this scenario, children moving to better neighborhoods would gain even more than we predict based on average earnings, while those moving to worse neighborhoods would suffer less relative to the observational predictions. We test whether this is the case in Column 2 of Online Appendix Table X by replicating the specification in Column 1, allowing for separate exposure effects for moves to better places (where $\Delta_{odp} > 0$) and moves to worse places. We find completely symmetric effects of moves to better vs. worse places, suggesting that there is relatively little heterogeneity in treatment effects, at least based on observed patterns of selection.

Column 3 replicates Column 1 for the subset of families who make large moves, defined as moves either from the top decile of neighborhoods in terms of $\hat{y}_{c,p=25}$ to the bottom decile or vice versa. Once again, the estimates are very similar, suggesting that roughly 60% of the differences in observed outcomes reflect causal effects even in the tails of the distribution.

Columns 4 and 5 of Table Online Appendix Table X test whether the component of children's incomes that is predictable by median rents and the observable characteristics analyzed in Figure IV have the same causal content as the "unobservable" component of the variation in observed outcomes. In Column 4, we use the predicted values of $\hat{y}_{op}$ and $\hat{y}_{dp}$ based on observables to construct the key right-hand-side regressors in (14); in Column 5, we conversely use the residuals from the regressions on observables. We find that the estimated exposure effects are nearly identical between these two components, allaying the concern that the "unobserved" portion of the variation in children's outcomes across areas predominantly reflects selection.[55]

[54] Formally, this test relies on the assumption that if unobservables $\theta_i$ are correlated with exposure to neighborhood quality as measured by a specific outcome variable $y$, they must also be correlated with neighborhood quality as measured by another outcome variable $y'$ (conditional on control variables). See Chetty and Hendren (2018a) for further details.

[55] We also find little heterogeneity in the exposure effects by the racial composition of the neighborhood to which children move. For example, Black children who move to neighborhoods with a higher value of $\hat{y}_{dp}$ at younger ages experience similar gains irrespective of whether the Black share in the destination neighborhood is small or large.

*Granularity of Causal Effects.* We use the movers design to revisit the analysis on the geographic scale of neighborhoods in Section IV.C, examining whether neighborhoods' *causal* effects also operate at a fine geographic scale. Online Appendix Figure VII plots estimates from a regression that replaces $\hat{y}_{op}$ and $\hat{y}_{dp}$ in the parsimonious specification used in Column 2 of Appendix Table VIII with poverty rates in the origin and destination tracts along with symmetric interactions between age at move and poverty rates in the ten tracts that are closest to the actual origin and destination tracts, respectively. We plot the eleven coefficients on the interactions between the destination-origin difference in poverty rates and age at move (for moves below age 23). These coefficients can be interpreted as the causal childhood exposure effect of moving to a tract (or near a tract) that has 1 SD higher poverty rates. Moving to a higher poverty tract earlier in childhood significantly reduces a child's earnings. However, moving to an area where *surrounding* tracts have higher poverty rates (controlling for poverty rates in one's own tract) has essentially no impact on children's outcomes. This figure replicates the correlational finding in Figure IV , showing that neighborhood characteristics are predictive not just of sorting patterns at a hyperlocal level but of causal effects.[56]

*Distance Restriction to Mitigate Measurement Error in Locations.* Throughout our analysis, we include only moves to destination tracts that are at least 25 miles away from the origin tract (based on distance between tract centroids) to minimize measurement error in locations that is induced by having a censored sample. For most cohorts in our sample, we do not observe location at early ages; for example, for children in the 1978 birth cohort, we cannot see location prior to age 11 (in 1989). As a result, some of the children whom we classify as "one-time" movers in the period we observe are not in fact one-time movers; they have actually moved at earlier ages.

If these earlier moves were uncorrelated in terms of neighborhood quality with later moves, they would not bias our estimates. However, many families actually move back to a location where they previously lived, particularly if they move a very short distance. To establish this result, we focus on the 1986-1991 birth cohorts and truncate the sample to use data only on location from age 11 onwards. Among those children classified as one-time movers in this truncated sample, we then examine locations before age 11 (which we can see for these more recent cohorts). Around 50% of both short-distance (less than 25 mile) and long-distance (more than 25 mile) movers have already moved at least once before age 11. But short-distance movers are disproportionately likely to move to tracts $d$ which are very close to their pre-age-11 tract $o'$. Tract $o'$ lies within 5 miles of tract $d$ for 43% of short-distance movers, but only for 11% of long-distance movers.

This pattern of returns to one's origins induces a correlation between $\bar{y}_{dp}$ and $\varepsilon_i$ in (9) because individuals who tend to move to higher-upward mobility areas also tend to have lived in higher-upward-mobility areas at earlier ages (before we observe their locations). This leads to a systematic downward bias in our estimates of exposure effects, one that is amplified for short-distance moves.

To obtain further insight into what drives these patterns of return migration at short distances, we examine heterogeneity by parental marital status. One might expect that the measurement error problem described above would be particularly pronounced for children with single parents, for whom short-distance "moves" as recorded in the tax data may simply reflect children being claimed in different years by separated parents or custodians living in different nearby neighborhoods. This phenomenon is less likely when one's parents live far apart or for children of married parents. Consistent with this hypothesis, we find that children of single parents are indeed more likely to return to a location near where they lived earlier in their childhood than children of married parents.

To test whether these differences are manifested in our exposure effect estimates, in Online

[56] We find a similar pattern when using children's mean observed outcomes or other covariates as regressors instead of poverty rates: in all cases, what matters are observed outcomes and characteristics in one's own tract, not nearby tracts.

Appendix Table XII, we replicate the specification in Column 1 of Online Appendix Table VIII, splitting children by parental marital status: single in the initial year where the child is claimed (Columns 1 and 4); married in the initial year of claiming but later divorced (Columns 2 and 5); or married in all years of the sample (Columns 3 and 6). For children of married parents, the key coefficient on the interaction between move quality and age-at-move (below age 23) is -0.022 for short-distance movers, which is relatively close to the coefficient of -0.030 for long-distance movers. In contrast, for children of single parents, the key coefficient of interest falls in magnitude from -0.024 for long-distance moves to -0.006 for short-distance moves. These findings confirm that the greater measurement error in locations for children of single parents who make short-distance moves leads to further attenuation of that coefficient. Based on this evidence, we conclude that short-distance moves are too likely to be returns to prior locations in our censored sample, and therefore restrict our analysis to those who move more than 25 miles.

## K Birth Outcomes Placebo Test for Movers Design

As discussed above, the key identification assumption underlying the movers research design is that selection effects in neighborhood choice – the extent to which different types of people move to higher vs. lower quality neighborhoods – do not vary with the age of their children at the point they move. This appendix – which is inspired by the work of Eshaghnia (2023) and reproduces results originally reported in Chetty et al. (2023) – presents evidence from a balance test based on birth outcomes (birthweight and length of gestation). Because birth outcomes are predetermined relative to childhood neighborhoods, measured accurately in administrative data for large populations, and correlated with many long-term outcomes of interest (Black et al. 2007, Royer 2009), they provide ideal pre-move outcomes on which to test for balance. In particular, we ask: are children who move at earlier ages to high-mobility Census tracts less likely to be born premature or with low birth weight – which would violate the assumptions of the design – or are their birthweights and lengths of gestation comparable to those who make similar moves at later ages?

Building on work by Kennedy-Moulton et al. (2022), who link data on the universe of birth records in California to tax records housed at the U.S. Census Bureau, we analyze birth outcomes for children born in California between 1978-1999. We construct our analysis sample and define variables exactly as above, except that we (1) expand the range of birth cohorts we study from 1978-1999 to maximize precision, (2) use data through the 2017 tax year, and (3) subset the sample to those who appear in the California birth record data (and hence were born in California). We focus on children who moved across Census tracts exactly once after age 1 (over the age range we observe them), a sample that consists of 487,000 children.

We first replicate the findings reported in Appendix H above in the subsample of children born in California. We begin by measuring average upward mobility rates by neighborhood based on the average household income percentile ranks at age 30 for children with parents at a given parental income level in the full national sample, excluding one-time movers, as above. We then analyze how children's incomes at age 30 vary with the age at which they move to higher- vs. lower-mobility Census tracts for the subsample of children for whom we observe income at age 30 (the 1978-87 birth cohorts). Online Appendix Figure XV presents this result by replicating Figure IX in the California subsample, plotting the coefficient on the difference in neighborhood upward mobility rates between the destination and origin Census tracts interacted with age at move. See the notes to Figure IX for details on the construction of this figure. Consistent with the baseline movers estimates reported in Appendix H, we find that children who move at earlier ages to higher-upward-mobility neighborhoods have higher income ranks themselves in adulthood.

Next, we repeat this analysis using birth outcomes as placebos. Panel A of Online Appendix Figure XIII replicates Online Appendix Figure XV , but replaces the dependent variable with the child's birthweight rank. In stark contrast with the pattern observed for income, there is no gradient in the relationship between birthweight and neighborhood upward mobility rates by age at move: the relationship is flat across the range and the estimated slope is not significantly different from 0. Since the relationship between children's incomes in adulthood and birthweights is highly non-linear – with birthweights below the 20th percentile associated with particularly low incomes in adulthood (Online Appendix Figure XVI) – we next replicate the placebo test using an indicator for birthweight below the 20th percentile (6 pounds 4.7 ounces). Again, we find no gradient by age at move (Panel B). Finally, in Panel C, we use an indicator for premature birth (before 259 days, or three weeks before term) as the outcome and again find no gradient by age at move. Hence, all of these pre-move outcome tests support the constant selection by age identification assumption that underlies the movers exposure design.[57]

To gauge the power of these placebo tests, note that the estimate of the slope in Online Appendix Figure XIII is sufficiently precise to rule out the hypothesis that the coefficient of birthweight rank on neighborhood upward mobility declines by more than 0.1% by age at move. In contrast, Online Appendix Figure XV implies a slope of -1.7% on average over the ages where we observe income at age 30, an order of magnitude larger. Since the correlation between birthweight and income ranks is bounded above by 1, we can rule out the hypothesis that differential selection on factors captured by birthweight ranks drives more than 5% of the exposure effect on income.

We conclude that pre-move birth outcomes are balanced by age at move to tracts with different levels of economic mobility. Differences in children's outcomes emerge after, not before, they move to higher-mobility Census tracts, providing further support for the identification assumption underlying the movers design.

## L Moving to Opportunity Based on Opportunity Atlas Estimates

In this appendix, we estimate the potential earnings gains from defining "high opportunity" neighborhoods using the Opportunity Atlas estimates constructed here instead of the poverty rate measures used to define high-opportunity areas, as in the Moving to Opportunity (MTO) experiment. We conduct this analysis both in the MTO sample and for the broader sample of all housing voucher recipients.

*MTO Sample.* We begin by identifying affordable high-opportunity neighborhoods that could have been targeted in the MTO experiment. To do so, we first count the number of tracts that would have been available to experimental voucher holders in the MTO experiment based on the official requirement that the poverty rate should be less than 10% in the 1990 census, which we denote by $N_c$. Then, we identify the $N_c$ highest ranking tracts in terms of upward mobility (measured in terms of individual income at age 26, as in our main MTO analysis) that have both lower median rents than the areas to which MTO voucher holders moved and shorter commute times using public transport to the tracts where control group residents lived (see the notes to Online Appendix Figure XVII for details).[58] Online Appendix Figure XIX illustrates some of the areas

[57] Online Appendix Figure XIII shows that the association between neighborhood upward mobility and children's birth outcomes (and, by extension, their potential earnings outcomes) do not vary with age at move, but it does *not* establish that those who move at younger vs. older ages have comparable birth outcomes or potential earnings outcomes. The latter assumption is not required for the movers exposure design.

[58] We obtain similar results if we further restrict attention to opportunity bargain areas with high rates of racial diversity (Online Appendix Figure XVIII).

identified as opportunity bargains in Chicago, which include Uptown (North of the Loop) and Alsip/Marrionette and Evergreen (Southwest of downtown).

After identifying the set of affordable high-opportunity tracts in each city using this procedure, we compute average individual earnings in adulthood across the relevant Census tracts using our observational estimates, conditional on having parents at the 10th percentile of the income distribution, using the same method as in Online Appendix I. Online Appendix Figure XVII uses these estimates to predict the earnings children in the MTO experiment would have had if they had moved to these opportunity bargain tracts. It replicates Figure VIII , but adds five points (in open circles) that show the observational estimates of income on the $x$-axis and predicted values for (hypothetical) movers in the MTO sample corresponding to those estimates. These predictions are linear extrapolations using the regression line estimated using the actual MTO experimental estimates, which is shown by the solid line in the figure.

On average, across the five MTO sites, we predict that MTO children's individual earnings would have been \$2,245 higher had they moved to opportunity bargain areas instead of the areas to which experimental voucher holders moved. For comparison, the mean earnings gain (relative to the control group) that was actually realized by children in the experimental voucher group who moved to low-poverty neighborhoods was \$1,893 – implying that one could have achieved more than double the earnings gains by defining "high opportunity" areas using the new tract-level data on upward mobility.

*All Housing Voucher Recipients.* To extend our analysis to Housing Voucher recipients in general, we obtain publicly available data on the count of of voucher holders by Census tract, which we denote by $n_{c,v}$, from the Housing and Urban Development agency in 2015 (Housing and Urban Development, Office of Policy Development and Research, 2015). We calculate the number of non-voucher-holders $n_{c,nv}$ in tract $c$ by subtracting $n_{c,v}$ from 2010 Census population estimates. We calculate the average individual income of children with parents at the 25th percentile in tracts where voucher holders live by taking a weighted average of our tract level estimates of mean individual incomes (in dollars, constructed as in Appendix E) with parents at $p = 25$, weighting by $n_{c,v}$. Similarly, we calculate the mean individual income of children with parents at the 25th percentile in tracts where non-voucher holders live using a weighted average of the same tract-level statistics, weighting by $n_{c,nv}$. We obtain estimates of \$32,254 for voucher holders and \$35,590 for non-voucher-holders. Online Appendix Figure XIVa plots the distribution of mean individual incomes conditional on having parents at the 25th percentile for the voucher vs. non-voucher-holders, weighting in the same manner. The distribution for voucher holders is shifted leftward, showing that voucher holders tend to live in areas where children grow up to have lower incomes, controlling for their own parents' incomes

Next, to compute the same difference conditional on the price of housing, we use data on median rents in 2015 from the ACS (see Appendix B). We then reweight the distribution of rents for non-voucher-holders to match rents in the neighborhoods where voucher holders live.[59] The average individual income in adulthood for children with parents at $p = 25$ among non-voucher holders in neighborhoods with comparable rents is \$34,402, lower than the raw mean for non-voucher-holders but still significantly above the mean income of \$32,254 in voucher holders' neighborhoods. Voucher holders live in lower-mobility areas than non-voucher-holders with comparable income even

[59] We implement this by estimating the density of median rents, once weighting by $n_{c,v}$ and once by $n_{c,nv}$, and then taking the ratio of these two densities as weights for the non-voucher holder distribution. Let $f_v, f_{nv}$ represent kernel densities of the distribution of rents, weighting by $n_{c,v}, n_{c,nv}$ respectively. We use weights of $\frac{f_v(r)}{f_{nv}(r)}$ for tracts with rent $r$ and calculate the average individual income in adulthood with these weights for non-voucher holders.

conditional on rents. In Online Appendix Figure XIVb, we replicate this analysis within each CZ and plot the difference in outcomes between voucher-holders and re-weighted non-voucher holders across CZs. In virtually all CZs, voucher holders live in systematically worse areas, even conditional on rent.

Finally, we analyze the potential gain in children's incomes if voucher holders' were to move to low-poverty (below 10% poverty rate) Census tracts vs. high-upward-mobility Census tracts with comparable rents to those where they currently live, as in the MTO analysis described above. Let $N_c$ now denote the total number of tracts with poverty rates below 10% in 2015. We compute the average individual income in adulthood for children with parents at $p = 25$ in these $N_c$ low-poverty tracts, reweighting tracts so that their distribution of rents matches the distribution of rents in the Census tracts where voucher holders currently live.The resulting mean is \$37,929 – \$5,675 higher than the mean in the tracts where voucher holders currently live. Applying our estimate that $\lambda = 68\%$ from our MTO analysis of the observational difference in children's outcomes among voucher holders is driven by causal effects of place, we conclude that voucher holders' children could gain \$3,859 in annual income at age 35 if they were to move to low-poverty neighborhoods. Replicating this exercise selecting the $N_c$ tracts with the highest levels of upward mobility (individual income for children with parents at $p = 25$) instead of low poverty rates yields an estimated income gain of \$7,157. Hence, the gain from moving to higher-opportunity areas as defined by the Opportunity Atlas measures is 85% larger than the gain from moving to high-opportunity as defined based on having low poverty rates.

Table I
Summary Statistics for Primary Analysis Sample

| | Pooled (1) | Male (2) | Female (3) |
|---|---|---|---|
| *A. Parental Characteristics* | | | |
| Median Parent Household Income ($) | 56,730 | 56,890 | 56,560 |
| Mean Parent Household Income Percentile Rank | 50.5 | 50.6 | 50.5 |
| Father Present in Household? | 78.9% | 79.7% | 78.2% |
| Mother Present in Household? | 89.7% | 89.2% | 90.3% |
| Both Parents Present in Household? | 68.7% | 68.9% | 68.5% |
| *B. Children's Income and Employment Outcomes in 2014-15* | | | |
| Median Household Income ($) | 42,360 | 41,250 | 43,590 |
| Mean Household Income Percentile Rank | 50.2 | 48.9 | 51.6 |
| Median Individual Income ($) | 29,440 | 35,120 | 24,390 |
| Mean Individual Income Percentile Rank | 50.2 | 53.9 | 46.4 |
| Employed (Individual Income > 0)? | 76.5% | 77.8% | 75.1% |
| *C. Outcomes in Adulthood Observed for Full Population* | | | |
| Married in 2015? | 45.1% | 42.6% | 47.8% |
| Incarcerated on April 1, 2010? | 1.5% | 2.7% | 0.3% |
| Had a Child as a Teenager? | | | 19.7% |
| Mean Spouse Individual Income Percentile Rank | 62.4 | 53.5 | 71.2 |
| Living in Low Poverty Tract in 2015? | 47.9% | 47.4% | 48.5% |
| Living in Childhood CZ in 2015? | 66.0% | 66.4% | 65.5% |
| Living in Childhood Tract in 2015? | 20.5% | 22.7% | 18.3% |
| Living with Parents in 2015? | 15.0% | 16.7% | 13.3% |
| *D. Outcomes in Adulthood Observed for ACS Subsample* | | | |
| Employed? | 84.8% | 88.6% | 81.0% |
| Hours Worked Per Week | 31.9 | 35.7 | 28.1 |
| Median Hourly Wage Rate ($) | 18.2 | 19.3 | 17.2 |
| Graduated from High School? | 86.2% | 83.7% | 88.6% |
| Earned Some College Credits? | 69.5% | 63.8% | 75.1% |
| Graduated with 2-Year College Degree? | 46.3% | 40.5% | 51.9% |
| Graduated with 4-Year College Degree? | 36.4% | 31.6% | 41.1% |
| Has Post-Graduate Degree? | 13.3% | 10.6% | 16.0% |
| Receives Public Assistance? | 2.3% | 1.4% | 3.2% |
| Mean Household Income Rank \| Child of U.S. Native Parents | 53.2 | 52.0 | 54.4 |
| Mean Individual Income Rank \| Child of U.S. Native Parents | 52.0 | 56.4 | 47.4 |
| Mean Household Income Rank \| Child of Immigrant Parents | 53.0 | 51.1 | 55.1 |
| Mean Individual Income Rank \| Child of Immigrant Parents | 54.1 | 56.4 | 51.7 |
| Pct. of Observations Included in Opportunity Atlas Public Data | 99.98% | 99.97% | 99.97% |
| Number of Obs in Full Sample | 20,500,000 | 10,400,000 | 10,000,000 |
| Number of Obs in ACS Subsample | 3,979,000 | 1,979,000 | 2,000,000 |

*Notes:* This table presents summary statistics for children in our primary analysis sample: children born in the 1978-1983 birth cohorts who are claimed as child dependents in tax records at some point between 1994-2015 and who have at least one non-missing address before age 23. Panel A presents summary statistics for parents of the children in our analysis sample; Panel B presents statistics on children's incomes from the tax data; Panel C presents statistics on other outcomes in adulthood observed in the Census or tax data for the full sample; and Panel D presents statistics for children who received the ACS at some point between 2005-2015. Employment and wage statistics in Panel D are based on the subset of children who receive the ACS at or after age 30. See Section II and Online Appendix A for more details and definitions of variables. All values in this and all subsequent tables and figures have been rounded to four significant digits as part of the disclosure avoidance protocol. Counts are rounded in the following manner: numbers between 10,000 and 99,999 are rounded to the nearest 500; between 100,000 and 9,999,999 to the nearest 1,000 and above 10,000,000 to the nearest 10,000. Sources for this and all subsequent tables and figures: authors calculations based on Census 2000 and 2010, tax returns, and American Community Surveys 2005-2015. See Online Appendix Table I for analogous summary statistics by race and ethnicity.

Table II

Variance Decomposition for Tract-Level Estimates of Upward Mobility

| | All Races (1) | White (2) | Black (3) | Hispanic (4) | Asian (5) | American Indian and Alaska Native (6) |
|---|---|---|---|---|---|---|
| *A. Household Income Rank for Children of Parents at the 25th Percentile* | | | | | | |
| Mean | 40.46 | 45.06 | 32.11 | 42.79 | 57.11 | 31.39 |
| Total SD | 6.51 | 6.14 | 4.14 | 4.63 | 7.95 | 7.24 |
| Noise SD | 1.97 | 2.88 | 2.30 | 2.83 | 5.01 | 3.56 |
| Reliability | 0.91 | 0.78 | 0.69 | 0.63 | 0.60 | 0.76 |
| Signal SD | 6.20 | 5.42 | 3.44 | 3.66 | 6.17 | 6.31 |
| Signal SD ($) | $12,850 | $12,650 | $4,974 | $6,718 | $23,590 | $9,107 |
| Within County Signal SD | 4.66 | 3.55 | 2.49 | 2.52 | 4.38 | 2.78 |
| Within County Signal SD ($) | $10,420 | $8,680 | $3,583 | $5,046 | $16,770 | $4,420 |
| *B. Share Incarcerated for Sons of Parents at the 25th Percentile* | | | | | | |
| Mean | 4.80 | 3.00 | 11.25 | 3.34 | 0.54 | 6.00 |
| Total SD | 4.19 | 3.53 | 6.23 | 3.53 | 4.61 | 6.11 |
| Noise SD | 2.66 | 3.02 | 4.52 | 2.82 | 4.42 | 4.61 |
| Reliability | 0.60 | 0.27 | 0.47 | 0.36 | 0.08 | 0.43 |
| Signal SD | 3.23 | 1.83 | 4.28 | 2.12 | 1.28 | 4.01 |
| Within County Signal SD | 2.44 | 1.44 | 2.69 | 1.40 | 0.76 | 0.92 |
| *C. Household Income Rank for Children of Parents at the 75th Percentile* | | | | | | |
| Mean | 58.31 | 60.55 | 43.69 | 53.91 | 65.15 | 45.80 |
| Total SD | 5.65 | 4.67 | 6.73 | 6.89 | 8.34 | 11.81 |
| Noise SD | 2.07 | 2.23 | 4.97 | 5.40 | 5.84 | 7.32 |
| Reliability | 0.87 | 0.77 | 0.45 | 0.39 | 0.51 | 0.62 |
| Signal SD | 5.25 | 4.10 | 4.54 | 4.28 | 5.95 | 9.27 |
| Signal SD ($) | $16,580 | $15,610 | $8,735 | $11,990 | $31,060 | $18,150 |
| Within County Signal SD | 4.20 | 2.82 | 3.64 | 3.56 | 4.73 | 3.86 |
| Within County Signal SD ($) | $13,510 | $11,030 | $7,372 | $10,250 | $22,820 | $9,782 |

*Notes:* This table reports estimates of variance components of children's outcomes in adulthood by Census tract conditional on parent income at the 25th and 75th percentiles. Panel A and C analyze the mean household income rank for children with parent incomes at the 25th and 75th percentiles, respectively; Panel B analyzes incarceration rates (defined as being incarcerated on April 1, 2010 based on the 2010 Census) for boys with parents at the 25th percentile. The first row in each panel shows the mean of the outcome in the primary analysis sample. The total SD is simply the national standard deviation of the conditional means of the relevant outcomes across tracts, weighted by the number of children in each tract with parent incomes below the median for the 25th percentile calculations and above the median for the 75th percentile calculations. The noise SD is the square root of the average squared standard error of the tract-level estimates; the signal SD is the square root of the difference between the total variance and noise variance. Reliability is the ratio of signal variance to total variance. We report estimates of signal standard deviations in both percentile ranks and real 2015 dollars. Column 1 reports statistics pooling all children; Columns 2 through 6 report the same statistics for children from a specific racial or ethnic subgroup.

Table III
Correlations Between Tract-Level Estimates of Children's Outcomes Within CZs

*A. Mean Household Income Ranks: Correlation Across Racial Groups and Parental Income Levels*

| | *Parents at 25th Percentile* | | | | | |
|---|---|---|---|---|---|---|
| | White | Black | Hispanic | Asian | American Indian & Alaska Natives | Parents at 75th Pctile, Same Race |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| White | 1 | 0.521 | 0.526 | 0.467 | 0.431 | 0.604 |
| Black | | 1 | 0.471 | 0.311 | 0.261 | 0.452 |
| Hispanic | | | 1 | 0.324 | 0.395 | 0.352 |
| Asian | | | | 1 | 0.132 | 0.463 |
| American Indian & Alaska Natives | | | | | 1 | 0.356 |

*B. Race-Controlled Correlations Across Outcomes for Children with Parents at 25th Percentile*

| | Household Income Rank | Individual Income Rank | Employment Rate | Incarceration Rate | Teenage Birth Rate |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Household Income Rank | 1 | 0.964 | 0.446 | -0.767 | -0.870 |
| Individual Income Rank | | 1 | 0.559 | -0.742 | -0.844 |
| Employment Rate | | | 1 | -0.334 | -0.312 |
| Incarceration Rate | | | | 1 | 0.774 |
| Teenage Birth Rate | | | | | 1 |

*Notes:* This table presents correlations between tract-level estimates of various child outcomes conditional on parent income at the 25th percentile (Columns 1- 5) or the 75th percentile (Column 6 in Panel A). Columns 1-5 of Panel A present correlations between mean household income ranks by tract conditional on having parents at the 25th percentile of the national income distribution across different racial groups. These correlations are estimated using variation across tracts within CZs and are adjusted for attenuation due to sampling error and noise infusion by inflating the raw correlations by the square root of the product of the reliabilities of the two outcome variables. Column 6 reports correlations between mean household income for children of a given race with parents at the 25th and 75th percentile across tracts. Panel B presents correlations between five different tract-level mean outcomes: household income rank, individual income rank, fraction employed, fraction incarcerated on April 1, 2010 (see Figure I for more details), and teenage birth rate (defined for women only as an indicator for claiming a child born when the child is between 13 and 19 years old). To eliminate correlations due to correlated measurement error, the correlations in Panel B are estimated by splitting families into two random samples, estimating correlations across the two samples, adjusting for noise by dividing the raw correlation by the product of the square root of the reliabilities of the two outcome variables, and then averaging between the two estimates obtained from the two different split samples. These correlations control for race and CZ fixed effects, following the methods described in the notes to Figure V.

Online Appendix Table Ia
Summary Statistics for Primary Analysis Sample, by Race and Ethnicity

| | White | | | Black | | | Hispanic | | |
|---|---|---|---|---|---|---|---|---|---|
| | Pooled (1) | Male (2) | Female (3) | Pooled (4) | Male (5) | Female (6) | Pooled (7) | Male (8) | Female (9) |
| *A. Parental Characteristics* | | | | | | | | | |
| Median Parent Household Income ($) | 71,470 | 71,510 | 71,430 | 29,600 | 29,910 | 29,300 | 33,470 | 33,400 | 33,540 |
| Mean Parent Household Income Percentile Rank | 58.4 | 58.4 | 58.3 | 33.1 | 33.4 | 32.8 | 36.5 | 36.4 | 36.6 |
| Father Present in Household? | 86.2% | 86.8% | 85.5% | 49.6% | 50.7% | 48.4% | 73.7% | 74.8% | 72.7% |
| Mother Present in Household? | 93.4% | 92.9% | 93.9% | 83.0% | 82.3% | 83.6% | 83.5% | 82.5% | 84.4% |
| Both Parents Present in Household? | 79.6% | 79.7% | 79.5% | 32.5% | 33.0% | 32.0% | 57.2% | 57.3% | 57.2% |
| *B. Children's Income and Employment Outcomes in 2014-15* | | | | | | | | | |
| Median Household Income ($) | 53,920 | 52,120 | 55,970 | 20,740 | 17,780 | 22,820 | 35,250 | 35,310 | 35,190 |
| Mean Household Income Percentile Rank | 55.8 | 54.7 | 57.0 | 34.9 | 32.7 | 37.0 | 45.7 | 44.7 | 46.8 |
| Median Individual Income ($) | 33,760 | 40,830 | 26,730 | 19,630 | 18,270 | 20,510 | 27,220 | 32,280 | 23,060 |
| Mean Individual Income Percentile Rank | 53.4 | 58.6 | 48.0 | 42.1 | 40.9 | 43.3 | 48.2 | 51.7 | 44.6 |
| Employed (Individual Income > 0)? | 78.6% | 81.7% | 75.4% | 76.2% | 71.0% | 81.2% | 76.9% | 77.8% | 76.1% |
| *C. Outcomes in Adulthood Observed for Full Population* | | | | | | | | | |
| Married in 2015? | 54.7% | 51.5% | 58.1% | 16.3% | 16.9% | 15.8% | 37.3% | 35.0% | 39.7% |
| Incarcerated on April 1, 2010? | 0.9% | 1.5% | 0.2% | 5.1% | 10.3% | 0.6% | 1.5% | 2.9% | 0.2% |
| Had a Child as a Teenager? | | | 13.5% | | | 41.3% | | | 29.3% |
| Mean Spouse Individual Income Percentile Rank | 63.2 | 54.2 | 72.2 | 57.4 | 52.8 | 62.6 | 58.2 | 48.5 | 67.6 |
| Living in Low Poverty Tract in 2015? | 54.6% | 53.9% | 55.2% | 27.7% | 27.4% | 27.9% | 36.3% | 35.6% | 37.1% |
| Living in in Childhood CZ in 2015? | 63.2% | 63.9% | 62.5% | 71.7% | 71.1% | 72.3% | 75.0% | 75.1% | 74.9% |
| Living in Childhood Tract in 2015? | 19.2% | 21.1% | 17.1% | 22.6% | 25.5% | 19.8% | 24.2% | 26.4% | 22.0% |
| Living with Parents in 2015? | 11.6% | 13.1% | 10.1% | 21.0% | 23.9% | 18.6% | 23.0% | 24.8% | 21.2% |
| *D. Outcomes in Adulthood Observed for ACS Subsample* | | | | | | | | | |
| Employed? | 86.6% | 91.5% | 81.6% | 75.1% | 70.1% | 79.6% | 81.5% | 85.5% | 77.8% |
| Hours Worked Per Week | 32.98 | 37.67 | 28.34 | 26.01 | 24.88 | 27.05 | 29.72 | 33.02 | 26.60 |
| Median Hourly Wage Rate ($) | 18.89 | 19.76 | 17.71 | 14.71 | 14.72 | 14.57 | 16.19 | 16.90 | 15.69 |
| Graduated from High School? | 88.7% | 86.7% | 90.8% | 78.0% | 73.0% | 82.6% | 76.9% | 73.4% | 80.3% |
| Earned Some College Credits? | 72.7% | 67.4% | 78.0% | 56.7% | 47.2% | 65.5% | 56.4% | 50.3% | 62.1% |
| Graduated with 2-Year College Degree? | 50.4% | 44.5% | 56.2% | 29.0% | 21.6% | 35.7% | 30.2% | 25.3% | 34.9% |
| Graduated with 4-Year College Degree? | 40.1% | 35.0% | 45.0% | 21.0% | 15.4% | 26.0% | 21.3% | 17.4% | 25.0% |
| Has Post-Graduate Degree? | 14.6% | 11.7% | 17.4% | 8.0% | 4.9% | 10.8% | 7.0% | 5.2% | 8.7% |
| Receives Public Assistance? | 1.9% | 1.3% | 2.5% | 4.6% | 2.0% | 6.9% | 3.1% | 1.5% | 4.5% |
| Mean Household Income Rank \| Child of U.S. Native Parents | 57.0 | 56.0 | 58.1 | 35.5 | 33.5 | 37.5 | 47.0 | 46.0 | 47.9 |
| Mean Individual Income Rank \| Child of U.S. Native Parents | 54.2 | 59.6 | 48.6 | 42.9 | 41.9 | 43.8 | 48.5 | 52.5 | 44.8 |
| Mean Household Income Rank \| Child of U.S. Immigrant Parents | 58.5 | 56.8 | 60.4 | 44.9 | 42.2 | 47.5 | 48.3 | 47.0 | 49.6 |
| Mean Individual Income Rank \| Child of Immigrant Parents | 56.8 | 60.5 | 52.9 | 51.1 | 49.5 | 52.5 | 50.8 | 53.9 | 47.6 |
| Pct. Of Observations Included in Opportunity Atlas Public Data | 99.93% | 99.81% | 99.80% | 97.87% | 95.41% | 95.79% | 96.51% | 92.53% | 92.36% |
| Number of Obs in Full Sample | 13,000,000 | 6,639,000 | 6,360,000 | 2,640,000 | 1,294,000 | 1,346,000 | 2,517,000 | 1,262,000 | 1,255,000 |
| Number of Obs in ACS Subsample | 2,855,000 | 1,429,000 | 1,426,000 | 433,000 | 207,000 | 226,000 | 443,000 | 220,000 | 224,000 |

*Notes:* This table replicates Table I, presenting summary statistics by race and gender for children in our primary analysis sample. All racial groups except Hispanics exclude individuals of Hispanic ethnicity.

Online Appendix Table Ib
Summary Statistics for Primary Analysis Sample, by Race and Ethnicity, cont.

| | Asian | | | American Indian and Alaska Native | | |
|---|---|---|---|---|---|---|
| | Pooled | Male | Female | Pooled | Male | Female |
| | (10) | (11) | (12) | (13) | (14) | (15) |
| *A. Parental Characteristics* | | | | | | |
| Median Parent Household Income ($) | 53,350 | 52,680 | 54,040 | 36,710 | 36,820 | 36,610 |
| Mean Parent Household Income Percentile Rank | 49.4 | 49.1 | 49.8 | 38.2 | 38.3 | 38.1 |
| Father Present in Household? | 88.5% | 88.6% | 88.3% | 71.1% | 72.2% | 70.1% |
| Mother Present in Household? | 92.1% | 91.8% | 92.5% | 88.5% | 87.8% | 89.1% |
| Both Parents Present in Household? | 80.6% | 80.4% | 80.8% | 59.6% | 60.0% | 59.2% |
| *B. Children's Income and Employment Outcomes in 2014-15* | | | | | | |
| Median Household Income ($) | 63,850 | 56,660 | 72,050 | 23,490 | 22,320 | 24,550 |
| Mean Household Income Percentile Rank | 60.7 | 57.6 | 64.0 | 37.8 | 36.7 | 38.8 |
| Median Individual Income ($) | 43,790 | 45,640 | 41,860 | 17,440 | 20,370 | 15,270 |
| Mean Individual Income Percentile Rank | 60.4 | 61.6 | 59.1 | 40.4 | 43.0 | 37.7 |
| Employed (Individual Income > 0)? | 79.6% | 80.5% | 78.6% | 70.3% | 70.5% | 70.0% |
| *C. Outcomes in Adulthood Observed for Full Population* | | | | | | |
| Married in 2015? | 50.0% | 45.4% | 54.7% | 32.3% | 30.2% | 34.3% |
| Incarcerated on April 1, 2010? | 0.3% | 0.5% | 0.0% | 2.9% | 5.0% | 0.8% |
| Had a Child as a Teenager? | | | 6.8% | | | 31.4% |
| Mean Spouse Individual Income Percentile Rank | 67.4 | 58.8 | 75.4 | 55.6 | 47.4 | 63.8 |
| Living in Low Poverty Tract in 2015? | 60.7% | 59.3% | 62.1% | 29.2% | 29.2% | 29.2% |
| Living in Childhood CZ in 2015? | 66.5% | 69.2% | 63.8% | 65.7% | 66.3% | 65.2% |
| Living in Childhood Tract in 2015? | 22.4% | 26.3% | 18.4% | 27.1% | 29.0% | 25.2% |
| Living with Parents in 2015? | 27.1% | 31.8% | 22.4% | 20.3% | 22.7% | 18.0% |
| *D. Outcomes in Adulthood Observed for ACS Subsample* | | | | | | |
| Employed? | 88.2% | 90.5% | 85.9% | 73.6% | 77.0% | 70.2% |
| Hours Worked Per Week | 34.14 | *36.31* | 31.95 | 24.99 | 27.15 | 22.84 |
| Median Hourly Wage Rate ($) | 23.96 | 23.54 | 24.43 | 13.96 | 14.71 | 13.26 |
| Graduated from High School? | 91.4% | 90.1% | 92.8% | 77.3% | 74.5% | 80.1% |
| Earned Some College Credits? | 84.6% | 81.6% | 87.7% | 51.2% | 44.2% | 58.2% |
| Graduated with 2-Year College Degree? | 67.3% | 62.4% | 72.3% | 22.8% | 18.4% | 27.2% |
| Graduated with 4-Year College Degree? | 58.9% | 53.9% | 64.1% | 14.6% | 12.0% | 17.3% |
| Has Post-Graduate Degree? | 23.4% | 19.9% | 26.9% | 4.2% | 3.2% | 5.2% |
| Receives Public Assistance? | 1.1% | 1.0% | 1.3% | 4.8% | 2.6% | 7.0% |
| Mean Household Income Rank \| Child of U.S. Native Parents | 58.2 | 55.8 | 60.4 | 38.5 | 37.5 | 39.5 |
| Mean Individual Income Rank \| Child of U.S. Native Parents | 57.5 | 60.5 | 54.8 | 41.0 | 43.6 | 38.3 |
| Mean Household Income Rank \| Child of Immigrant Parents | 63.3 | 60.1 | 66.7 | 42.2 | 40.9 | 43.6 |
| Mean Individual Income Rank \| Child of Immigrant Parents | 63.1 | 63.9 | 62.2 | 44.6 | 46.4 | 42.5 |
| Pct. of Observations Included in Opportunity Atlas Public Data | 84.89% | 72.90% | 70.69% | 54.43% | 44.26% | 45.91% |
| Number of Obs in Full Sample | 673,000 | 344,000 | 330,000 | 134,000 | 68,000 | 66,000 |
| Number of Obs in ACS Subsample | 128,000 | 65,000 | 63,000 | 31,000 | 15,500 | 15,500 |

*Notes*: This table replicates Table I, presenting summary statistics by race and gender for children in our primary analysis sample. All racial groups except Hispanics exclude individuals of Hispanic ethnicity.

Online Appendix Table II
Predicting Outcomes in Dollars Based on Outcomes in Ranks

| | All Races (1) | White (2) | Black (3) | Hispanic (4) | Asian (5) | American Indian and Alaskan Native (6) | Other (7) |
|---|---|---|---|---|---|---|---|
| *A. Household Income in Dollars* | | | | | | | |
| Mean Household Income Rank | 85,510 | 78,900 | 111,000 | 114,800 | 121,700 | 113,800 | 112,900 |
| | (311.8) | (386.6) | (291.7) | (468.8) | (1,725.0) | (1,405.0) | (961.2) |
| Prob. of Being in the Top Percentile | 676,900 | 660,900 | 462,400 | 484,000 | 531,600 | 518,600 | 509,600 |
| | (1,220.0) | (1,220.0) | (1,754.0) | (1,551.0) | (2,141.0) | (6,621.0) | (2,566.0) |
| Prob. of Being Above the 20th Percentile | 77,780 | 79,370 | 46,840 | 47,870 | 72,960 | 39,750 | 51,900 |
| | (316.6) | (341.4) | (352.2) | (382.8) | (1,068.0) | (1,490.0) | (757.2) |
| Constant | -3,068 | 559 | -10,960 | -12,640 | -17,130 | -11,300 | -10,900 |
| | (102.6) | (143.6) | (89.4) | (180.0) | (769.1) | (451.3) | (384.1) |
| *B. Individual Income in Dollars* | | | | | | | |
| Mean Individual Income Rank | 60,740 | 59,120 | 79,540 | 79,370 | 74,840 | 76,980 | 77,280 |
| | (199.6) | (223.9) | (243.0) | (320.3) | (990.2) | (1,161.0) | (630.6) |
| Prob. of Being in the Top Percentile | 396,200 | 387,400 | 279,900 | 273,000 | 335,400 | 288,000 | 319,400 |
| | (720.4) | (727.1) | (1,049.0) | (963.2) | (1,299.0) | (4,291.0) | (1,543.0) |
| Prob. of Being Above the 20th Percentile | 46,820 | 47,270 | 27,920 | 28,750 | 44,020 | 27,000 | 31,060 |
| | (181.6) | (193.4) | (218.6) | (236.0) | (589.4) | (1,079.0) | (457.1) |
| Constant | -3,681 | -2,692 | -9,828 | -9,747 | -8,572 | -8,620 | -8,371 |
| | (70.3) | (83.8) | (86.0) | (128.9) | (448.0) | (399.8) | (258.7) |

*Notes:* Each column of this table reports estimates from a separate OLS regression of average incomes measured in dollars on three rank-based outcomes: (1) mean ranks, (2) the probability of being in the top income percentile, and (3) the probability of being above the 20th income percentile. Each regression is estimated in a dataset with one observation per tract and parent income level (25th or 75th percentile), pooling genders. The regressions are weighted by the number of children in each tract with parent incomes below (above) median income for the 25th (75th) percentile observations. Panel A presents results for household income; Panel B presents results for individual income. See Appendix C for further details.

Online Appendix Table III
Correlation between Actual Mean Incomes and Predicted Mean Incomes Across Tracts

| | All Races (1) | White (2) | Black (3) | Hispanic (4) | Asian (5) | American Indian and Alaskan Native (6) | Other (7) |
|---|---|---|---|---|---|---|---|
| *A. Household Income, Children of Parents at the 25th Percentile* | | | | | | | |
| All Genders | 0.9785 | 0.9576 | 0.9560 | 0.9430 | 0.9400 | 0.9515 | 0.9229 |
| Male | 0.9623 | 0.9326 | 0.9465 | 0.9363 | 0.9363 | 0.9647 | 0.9053 |
| Female | 0.9691 | 0.9455 | 0.9510 | 0.9374 | 0.9333 | 0.9361 | 0.9175 |
| *B. Household Income, Children of Parents at the 75th Percentile* | | | | | | | |
| All Genders | 0.9663 | 0.9554 | 0.9079 | 0.8977 | 0.9210 | 0.9377 | 0.8965 |
| Male | 0.9472 | 0.9325 | 0.9024 | 0.8873 | 0.9184 | 0.9258 | 0.8935 |
| Female | 0.9532 | 0.9403 | 0.8982 | 0.8841 | 0.9074 | 0.9447 | 0.8968 |
| C. Individual Income, Children of Parents at the 25th Percentile | | | | | | | |
| All Genders | 0.9748 | 0.9585 | 0.9568 | 0.9387 | 0.9416 | 0.9493 | 0.9278 |
| Male | 0.9627 | 0.9395 | 0.9477 | 0.9296 | 0.9369 | 0.9497 | 0.9075 |
| Female | 0.9673 | 0.9496 | 0.9569 | 0.9464 | 0.9327 | 0.9407 | 0.9322 |
| D. Individual Income, Children of Parents at the 75th Percentile | | | | | | | |
| All Genders | 0.9638 | 0.9582 | 0.9084 | 0.8889 | 0.9184 | 0.9063 | 0.9038 |
| Male | 0.9474 | 0.9405 | 0.9037 | 0.8623 | 0.9087 | 0.9044 | 0.8951 |
| Female | 0.9532 | 0.9473 | 0.9055 | 0.9028 | 0.9010 | 0.9156 | 0.9238 |

*Notes:* This table reports the correlation between actual mean incomes in adulthood and predictions of those incomes using publicly available rank-based outcomes, constructed using the regression models in Online Appendix Table II. The predicted values are correlated with the income estimates within each race, gender, and percentile cell, weighting 25th (75th) percentile outcomes by the number of kids below (above) the median income. Panel A reports results for the 25th percentile and Panel B reports results for the 75th percentile, both using household income measures. Panel C and Panel D reports results for individual income at the 25th and 75th percentiles respectively. Column 1 reports estimates pooling all racial and ethnic groups, while Columns 2-7 report race-specific correlations.

Online Appendix Table IV
Variation in Children's Outcomes Explained by Family Characteristics

| | Adjusted R-Squared |
|---|---|
| *A. Individual Income Rank* | |
| Parent Income Only | 0.0675 |
| Parent Income, Gender | 0.0913 |
| Parent Income, Gender, Race | 0.1026 |
| Parent Income, Gender, Race, Mother's Education | 0.1122 |
| *B. Household Income Rank* | |
| Parent Income Only | 0.1083 |
| Parent Income, Race | 0.1337 |
| Parent Income, Race, Mother's Education | 0.1427 |
| Parent Income, Race, Mother's Education, Father's Citizenship | 0.1492 |
| *C. Incarceration* | |
| Parent Income Only | 0.0075 |
| Parent Income, Gender | 0.0213 |
| Parent Income, Gender, Race | 0.0386 |
| Parent Income, Gender, Race, Mother's Occupation | 0.0427 |

*Notes:* This table reports R-squared values from regressing children's outcomes in adulthood on parent income percentiles (100 bins) interacted with other family characteristics. We start from a potential set of childhood characteristics that consists of: house size (number of bedrooms), number of siblings, and Mother and Father's educational attainment, occupation, citizenship status, and marital status. These characteristics are obtained by merging our primary sample with the 2000 Census Long Form. In the first row of each panel, we report the adjusted R-squared from a regression of the outcome on parent income percentiles only. In the second row, we interact parent income bins with each covariate and choose the variable with the highest adjusted R-squared. We report that variable and the adjusted R-squared from the corresponding specification. That variable is removed from the list of covariates, and the exercise is repeated until three covariates are obtained. In Panel A, the outcome is the child's individual income rank in adulthood; in Panel B, it is the child's household income rank; and in Panel C, it is an indicator for being incarcerated. See Online Appendix D for further details.

Online Appendix Table V
Mean Squared Error of Alternative Prediction Models

| | | Transformed Linear (1) | Transformed Quadratic (2) | Local Linear (BW = 10) (3) | Local Linear (BW = 25) (4) |
|---|---|---|---|---|---|
| A. Household Income | | | | | |
| Parent Income Percentile | 1 | 0.0389 | 0.0385 | 0.0414 | 0.0397 |
| | 25 | 0.0638 | 0.0639 | 0.064 | 0.0639 |
| | 50 | 0.0695 | 0.0697 | 0.0696 | 0.0696 |
| | 75 | 0.0698 | 0.0699 | 0.0699 | 0.0698 |
| | 100 | 0.0806 | 0.0807 | 0.0807 | 0.0808 |
| B. Incarceration | | | | | |
| Parent Income Percentile | 1 | 0.1131 | 0.1209 | 0.112 | 0.112 |
| | 25 | 0.0424 | 0.0427 | 0.0424 | 0.0424 |
| | 50 | 0.0215 | 0.0216 | 0.0215 | 0.0215 |
| | 75 | 0.0097 | 0.0097 | 0.0097 | 0.0097 |
| | 100 | 0.0017 | 0.0017 | 0.0017 | 0.0017 |

*Notes:* This table reports the mean squared error (MSE) of alternative prediction models (shown in the four columns) at five percentiles of the parent income distribution for our baseline household income measure in Panel A and incarceration for men in Panel B. We compute the MSE using a leave-one-out procedure. For each child i in an estimation cell (tract by race by gender), we compute a prediction for their outcome in adulthood by fitting our model on all other observations within the cell. The prediction error is the difference between i's actual outcome in adulthood and this leave-one-out prediction. Then, pooling over all children, we calculate the MSE within each parent income percentile. The figure presents the MSE for our baseline linear transformation model (described in Section III), a quadratic version of that model, and a local-linear model using a uniform kernel with two different bandwidths: 10 ranks and 25 ranks.

Online Appendix Table VIa
Variance Decomposition for Tract-Level Estimates of Upward Mobility Without DP Noise

| | All Races (1) | White (2) | Black (3) | Hispanic (4) | Asian (5) | American Indian and Alaska Native (6) |
|---|---|---|---|---|---|---|
| *A. Household Income Rank for Children of Parents at the 25th Percentile* | | | | | | |
| Mean | 40.26 | 44.71 | 32.23 | 43.01 | 57.33 | 32.80 |
| Total SD | 6.42 | 6.00 | 4.13 | 4.70 | 7.67 | 7.42 |
| Noise SD | 1.89 | 2.81 | 2.33 | 2.92 | 5.08 | 4.01 |
| Reliability | 0.91 | 0.78 | 0.68 | 0.62 | 0.56 | 0.71 |
| Signal SD | 6.14 | 5.30 | 3.41 | 3.69 | 5.74 | 6.24 |
| Within County Signal SD | 4.83 | 4.05 | 2.49 | 2.66 | 4.18 | 3.42 |
| *B. Share Incarcerated for Sons of Parents at the 25th Percentile* | | | | | | |
| Mean | 4.81 | 3.14 | 11.32 | 3.27 | 0.56 | 5.95 |
| Total SD | 3.86 | 3.00 | 5.96 | 3.09 | 1.84 | 5.87 |
| Noise SD | 2.26 | 2.54 | 4.44 | 2.47 | 1.66 | 4.53 |
| Reliability | 0.66 | 0.28 | 0.45 | 0.36 | 0.18 | 0.40 |
| Signal SD | 3.13 | 1.60 | 3.98 | 1.85 | 0.78 | 3.72 |
| Within County Signal SD | 2.38 | 1.25 | 2.40 | 1.03 | | 1.98 |
| *C. Household Income Rank for Children of Parents at the 75th Percentile* | | | | | | |
| Mean | 58.06 | 60.25 | 43.80 | 54.16 | 64.76 | 48.52 |
| Total SD | 5.57 | 4.60 | 6.49 | 6.63 | 8.16 | 12.29 |
| Noise SD | 2.01 | 2.23 | 4.72 | 5.15 | 5.82 | 7.74 |
| Reliability | 0.87 | 0.76 | 0.47 | 0.40 | 0.49 | 0.60 |
| Signal SD | 5.20 | 4.03 | 4.45 | 4.18 | 5.71 | 9.55 |
| Within County Signal SD | 4.30 | 3.02 | 3.66 | 3.52 | 4.51 | 5.17 |

*Notes:* This table replicates Table II using tract outcome estimates without added differential privacy noise. In this table, tract-level robust standard errors are constructed using the conventional HC1 sandwich estimator.

Online Appendix Table VIb
Variance Decomposition for Tract-Level Estimates of Upward Mobility with KSS Standard Errors

| | All Races (1) | White (2) | Black (3) | Hispanic (4) | Asian (5) | American Indian and Alaska Native (6) |
|---|---|---|---|---|---|---|
| *A. Household Income Rank for Children of Parents at the 25th Percentile* | | | | | | |
| Mean | 40.26 | 44.71 | 32.23 | 43.01 | 57.33 | 32.80 |
| Total SD | 6.42 | 6.00 | 4.13 | 4.70 | 7.67 | 7.42 |
| Noise SD | 1.97 | 2.92 | 2.45 | 3.09 | 5.35 | 4.33 |
| Reliability | 0.91 | 0.76 | 0.65 | 0.57 | 0.51 | 0.66 |
| Signal SD | 6.11 | 5.24 | 3.32 | 3.55 | 5.49 | 6.02 |
| Within County Signal SD (KSS) | 4.79 | 3.98 | 2.38 | 2.49 | 3.89 | 3.22 |
| *B. Share Incarcerated for Sons of Parents at the 25th Percentile* | | | | | | |
| Mean | 4.81 | 3.14 | 11.32 | 3.27 | 0.56 | 5.95 |
| Total SD | 3.86 | 3.00 | 5.96 | 3.09 | 1.84 | 5.87 |
| Noise SD | 2.30 | 2.62 | 4.57 | 2.56 | 1.78 | 4.71 |
| Reliability | 0.65 | 0.24 | 0.41 | 0.31 | 0.07 | 0.36 |
| Signal SD | 3.10 | 1.46 | 3.83 | 1.73 | 0.48 | 3.50 |
| Within County Signal SD (KSS) | 2.35 | 1.09 | 2.21 | 0.86 | | 2.39 |
| *C. Household Income Rank for Children of Parents at the 75th Percentile* | | | | | | |
| Mean | 58.06 | 60.25 | 43.80 | 54.16 | 64.76 | 48.52 |
| Total SD | 5.57 | 4.60 | 6.49 | 6.63 | 8.16 | 12.29 |
| Noise SD | 2.07 | 2.32 | 5.18 | 5.58 | 6.24 | 8.53 |
| Reliability | *0.86* | 0.75 | 0.36 | 0.29 | 0.41 | 0.52 |
| Signal SD | 5.17 | 3.98 | 3.91 | 3.58 | 5.25 | 8.85 |
| Within County Signal SD (KSS) | 4.25 | 2.96 | 3.06 | 2.84 | 3.98 | 4.45 |

*Notes:* This table replicates Online Appendix Table VIb with tract-level robust standard errors constructed using the unbiased formula from Remark 1 in Kline et al. (2020). The within county signal standard deviation calculations use the leave-one out variance decomposition method from Example 1 of Kline et al. (2020).

Online Appendix Table VII
Variation in Children's Outcomes Explained by Tract vs. Family Characteristics

| | Parent income (1) | Parent Income x Tract (2) | Parent Income and Covariates (3) | Parent Income x Tract and Covariates (4) |
|---|---|---|---|---|
| Pooled | 0.1359 | 0.1872 | 0.1607 | 0.2030 |
| White Males | 0.0820 | 0.1321 | 0.1033 | 0.1460 |
| White Females | 0.0922 | 0.1432 | 0.1217 | 0.1625 |
| Black Males | 0.0663 | 0.1659 | 0.0911 | 0.1834 |
| Black Females | 0.0821 | 0.1884 | 0.1196 | 0.2101 |
| Asian Males | 0.0458 | 0.1630 | 0.0828 | 0.1873 |
| Asian Females | 0.0357 | 0.1644 | 0.0827 | 0.1895 |
| Hispanic Males | 0.0466 | 0.1357 | 0.0639 | 0.1477 |
| Hispanic Females | 0.0573 | 0.1550 | 0.0821 | 0.1711 |

*Notes:* This table reports the adjusted R-squared values from regressions of children's household income rank in adulthood on parent income rank (Column 1), parent income rank interacted with tract fixed effects (Column 2), covariates and parent income rank (Column 3), and covariates and parent income rank interacted with tract (Column 4). The covariates are the following family-level characteristics: number of siblings, number of bedrooms, mother and father's marital status, educational attainment, age, citizenship status, occupation, and each parent's individual income rank. Most of these variables are obtained by linking the primary sample to the 2000 Census Long Form. The first row is fully saturated with race and gender fixed effects. The remaining rows report results within various race and gender cells. See Online Appendix D for further details.

Online Appendix Table VIII
Quasi-Experimental Estimates of Tract-Level Causal Exposure Effects Using Movers

| | Income Rank at 24 (1) | Income Rank, Parsimonious (2) | Married at 30 (3) | Incarcerated | Income Rank, Family FEs (5) |
|---|---|---|---|---|---|
| Age <= 23 | -0.027 | -0.026 | -0.027 | -0.025 | -0.021 |
| | (0.001) | (0.001) | (0.001) | (0.005) | (0.002) |
| Age > 23 | -0.008 | -0.004 | 0.003 | 0.010 | -0.004 |
| | (0.009) | (0.008) | (0.009) | (0.033) | (0.009) |
| Num. of Obs. | 2,814,000 | 2,814,000 | 1,614,000 | 1,484,000 | 2,814,000 |

*Notes:* This table reports regression estimates of annual childhood tract level exposure effects on children's household income ranks at age 24 (Columns 1, 2 and 5), marriage (Column 3), and incarceration (Column 4). Standard errors are shown in parentheses. Columns 1, 2, and 5 each report estimates from a split-sample IV regression of a child's household income rank at age 24 on the difference between parent income-specific predicted income ranks in the destination vs. the origin, interacted with the age of the child at the time of the move (m). Column 1 reports estimates from equation (9) using all children of one-time movers in the primary analysis sample. The predicted income ranks are estimated on a sample excluding one-time movers. We permit separate linear interactions for age m≤23 and m>23. The estimates can be interpreted as the impact of delaying by one year moving to a tract which has a 1 percentile point higher predicted income rank, essentially a linear fit to the coefficients in Figure IX above and below age 23. Column 2 estimates exposure effects using a more parsimonious specification that omits the interaction terms between age and predicted ranks in the origin tract that were included in column 1. Columns 3 and 4 replicate column 1 using marriage rates at 30 and incarceration rates in 2010 respectively (rather than household income ranks) to measure both the child's outcome (dependent variable) and the predictions (independent variables). Column 5 adds family fixed effects to the specification in column 1; here we identify exposure effects from families of one-time movers with two or more children of different ages at the time of move. See Online Appendix J for further details on sample and variable definitions and the exact specification used to estimate these coefficients.

Online Appendix Table IX
Quasi-Experimental Estimates of Tract-Level Exposure Effects: Outcome-Based Placebo Tests

| | Income Rank at 24 (1) | Married at 30 (2) | Incarceration (3) | Teen Birth (4) |
|---|---|---|---|---|
| *A. Male Children* | | | | |
| Mean Income Rank at 24 | -0.024 | -0.005 | 0.001 | |
| | (0.002) | (0.006) | (0.002) | |
| Frac. Married at 30 | 0.000 | -0.022 | 0.000 | |
| | (0.001) | (0.003) | (0.001) | |
| Incarceration Rate | -0.001 | -0.009 | -0.032 | |
| | (0.007) | (0.016) | (0.005) | |
| Num. of Obs. | 1,132,000 | 824,000 | 734,000 | |
| *B. Female Children* | | | | |
| Mean Income Rank at 24 | -0.032 | 0.002 | | -0.003 |
| | (0.003) | (0.007) | | (0.003) |
| Frac. Married at 30 | -0.003 | -0.029 | | 0.004 |
| | (0.001) | (0.002) | | (0.001) |
| Teen Birth | -0.005 | -0.010 | | -0.026 |
| | (0.002) | (0.004) | | (0.002) |
| Num. of Obs. | 1,068,000 | 776,000 | | 1,347,000 |
| *C. Male Children, Rescaled* | | | | |
| Mean Income Rank at 24 | -0.024 | -0.003 | 0.000 | |
| | (0.002) | (0.003) | (0.000) | |
| Frac. Married at 30 | 0.000 | -0.022 | 0.000 | |
| | (0.000) | (0.003) | (0.000) | |
| Incarceration Rate | 0.000 | 0.003 | -0.032 | |
| | (0.002) | (0.005) | (0.005) | |
| *D. Female Children, Rescaled* | | | | |
| Mean Income Rank at 24 | -0.032 | 0.001 | | 0.000 |
| | (0.003) | (0.004) | | (0.000) |
| Frac. Married at 30 | 0.000 | -0.029 | | 0.000 |
| | (0.000) | (0.002) | | (0.000) |
| Teen Birth | 0.000 | 0.000 | | -0.026 |
| | (0.000) | (0.000) | | (0.002) |

*Notes:* This table reports regression estimates of annual childhood exposure effects when simultaneously including tract-level predictions for multiple outcomes in the regression specification, separately for males (Panel A) and for females (Panel B), and for different outcomes. The underlying specification is analogous to the parsimonious specification in Column 2 of Online Appendix Table VIII (see those table notes and Online Appendix J for more detail). Here, we include as explanatory variables not only tract-level predictions for income ranks at age 24, but also for marriage rates at age 30, incarceration rates on April 1, 2010 (for men), and teenage birth rates (for women). The coefficients reported in this table are for the predictions interacted with the (age<=23) indicator. The estimates in each column can be interpreted as the impact on a given individual outcome of moving to a tract which has a 1 percentile or 1 percentage point higher predicted value of each of the regressors one year later prior to age 23. Column 1 uses child income rank at age 24 as the dependent variable, while columns 2, 3, and 4 use marriage at 30, incarceration, and teenage birth as the outcome variables, respectively. Standard errors are shown in parentheses. To facilitate interpretation of magnitudes given the difference in units across regressors, Panels C and D rescale each coefficient reported in Panels A and B by the coefficient obtained by regressing the dependent variable outcome on the placebo outcome at the individual level. For example, for the placebo test of income rank on tract-level marriage (Column 1, Row 2), income rank is regressed on a marriage indicator along with all the other controls from our parsimonious movers regression specification except for age at move interacted with move quality. We then multiply the original coefficient in Panel A by this regression coefficient. We treat the scaling coefficient as known, and rescale the standard errors accordingly.

Online Appendix Table X
Quasi-Experimental Estimates of Tract-Level Exposure Effects: Heterogeneity Analysis

| | Baseline | Good vs. Bad Moves | Large Moves | Observed Component of Upward Mobility | Unobserved Component of Upward Mobility |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Age <= 23 | -0.027 | | -0.046 | -0.020 | -0.025 |
| | (0.001) | | (0.017) | (0.001) | (0.003) |
| Age <= 23, Pos. Moves | | -0.031 | | | |
| | | (0.002) | | | |
| Age <= 23, Neg. Moves | | -0.027 | | | |
| | | (0.002) | | | |
| Observations | 2,814,000 | 2,814,000 | 22,500 | 2,692,000 | 2,692,000 |

*Notes:* This table reports regression estimates of annual childhood exposure effects on children's household income ranks at age 24 for different subgroups of one-time movers. Standard errors are shown in parentheses. Column 1 replicates Column 1 from Online Appendix Table VIII as a reference. Column 2 reports exposure effects separately for one-time movers who move to tracts with higher (pos. moves) vs. lower (neg. moves) predicted income ranks using a specification that allows the effects to vary for these two groups. Column 3 restricts to the subgroup of one-time movers who move either from the top to bottom or bottom to top decile of the within-CZ rankings of upward mobility estimates. In Column 4, we replace mean observed income ranks on the right hand side of the regression with ranks predicted based on the following neighborhood characteristics: the total number of jobs within 5 miles (measured in 2015), the total number of high paying jobs within 5 miles (measured in 2015), local unemployment rates (measured in 2000), local poverty rates (measured in 2000), grade 3 math scores (measured in 2013), the fraction attending college locally (the fraction of people 25 and older in the tract who have a college degree or higher), the fraction completing high school locally (the fraction of people 25 and older in the tract who have less than a high school diploma), the median two-bedroom rent in the tract (in 1990), the share of area residents who are owner-occupiers (in 2010), the local share of single-parent families (in 2000), and area population density (in 2000). In Column 5, we instead use the residuals from the regression on observables (the "unobservable" component of incomes) as the regressor. All specifications use split-sample instrumental variables, as in Online Appendix Table VIII. See notes to Online Appendix Table VIII and Online Appendix J for more details on these specifications.

Online Appendix Table XI
Summary Statistics for Movers Analysis Sample

| | | One-time Movers | Non 1-time Movers (0 & 2+ Movers) |
|---|---|---|---|
| Parent Family Income Rank | Mean | 56.9 | 48.6 |
| | Std. Dev. | 29.2 | 28.7 |
| | Num. of Obs. | 3,100,000 | 42,000,000 |
| Child Individual Income Rank at 24 | Mean | 51.5 | 49.5 |
| | Std. Dev. | 29.1 | 28.8 |
| | Num. of Obs. | 2,400,000 | 34,000,000 |
| Child Household Income Rank at 24 | | 51.8 | 49.6 |
| | Std. Dev. | 29.1 | 28.8 |
| | Num. of Obs. | 3,100,000 | 42,000,000 |
| Child Incarcerated in 2010 | Mean | 0.9% | 1.4% |
| | Std. Dev. | 9.4% | 11.8% |
| | Num. of Obs. | 2,500,000 | 33,000,000 |
| Child Married at 30 | Mean | 42.6% | 38.1% |
| | Std. Dev. | 49.4% | 48.6% |
| | Num. of Obs. | 1,800,000 | 22,000,000 |

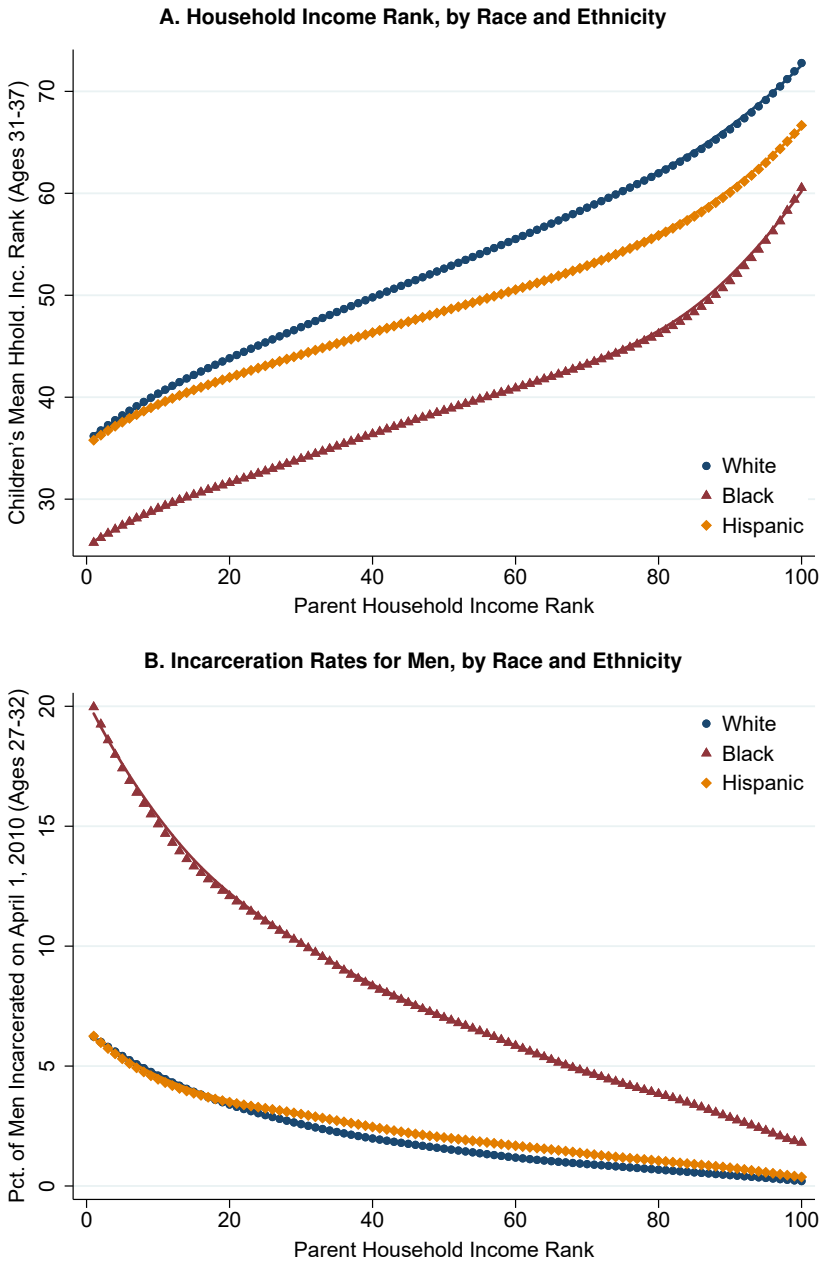
*Notes*: This table presents summary statistics for the samples used in Online Appendix Table VIII, our quasi-experimental analyses of causal exposure effects based on families who move across tracts. The movers analysis sample extends the core sample described in Section II by including additional cohorts up until 1991 in order to observe moves at younger ages. Column 1 reports summary statistics for children whose parents moved across tracts exactly once between 1989-2015 when they were age 28 or below and who moved at least 25 miles (based on their tract centroids). Column 2 reports summary statistics for children whose parents do not move across tracts throughout our sample window or whose parents move more than once across tracts, the sample used to estimate the key regressors in equation (9). Parent household income is the average pre-tax household income from 1994-2000, measured as AGI plus tax-exempt interest income and the non-taxable portion of Social Security and Disability benefits. Child individual income is defined as the sum of individual W-2 wage earnings and half of household self-employment income. Incarceration is based on the individual's group home status in the 2010 US population census. Marital status is defined based on the marital status listed on 1040 forms for tax filers in the 2015 tax year; non-filers are coded as single. See Section II and Appendix A for further details on sample and variable definitions.

Online Appendix Table XII
Quasi-Experimental Exposure Effect Estimates by Distance of Move and Parents' Marital Status

| | Moves <= 25 miles | | | Moves > 25 miles | | |
|---|---|---|---|---|---|---|
| | Single Parent (1) | Mixed Two Parent (2) | Stable Two Parent (3) | Single Parent (4) | Mixed Two Parent (5) | Stable Two Parent (6) |
| Age <= 23 | -0.006 | -0.017 | -0.022 | -0.024 | -0.028 | -0.030 |
| | (0.002) | (0.001) | (0.002) | (0.002) | (0.002) | (0.002) |
| Observations | 2,636,000 | 3,438,000 | 2,329,000 | 704,000 | 1,212,000 | 898,000 |

*Notes*: This table reports estimates of annual childhood tract-level exposure effects on children's household income ranks at age 24 for different subgroups of one-time movers using the specification in Column 1 of Online Appendix Table VIII. Columns 1-3 show exposure effects for moves between tracts that are less than 25 miles apart; columns 4-6 show estimates for moves between tracts that are more than 25 miles apart. Columns 1 and 4 restrict to children who were claimed by a single parent in the year they were linked to parents (following the procedure described in Section II). Columns 2 and 5 restrict to children claimed by two (married) parents who did not remain married in all years of our sample. Columns 3 and 6 restrict children claimed by two parents who remained married throughout our sample. See notes to Online Appendix Table VIII for details on these specifications.

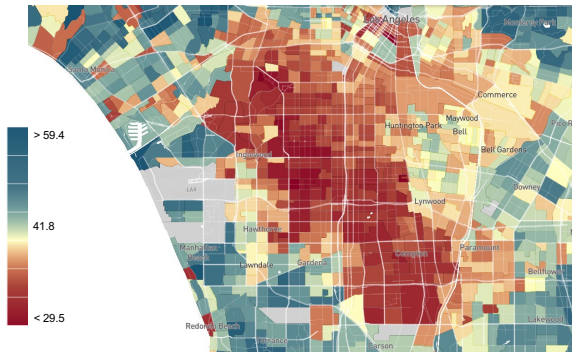
FIGURE I: Children's Outcomes vs. Parental Income Rank



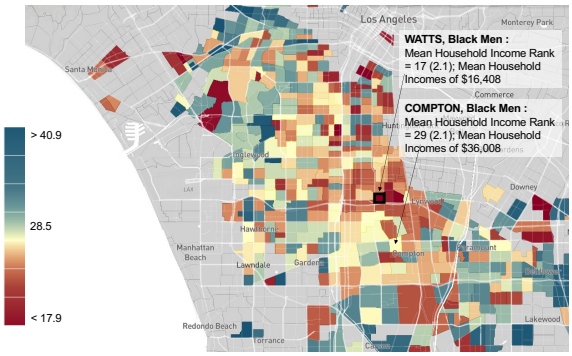

*Notes:* This figure plots the relationship between children's outcomes in adulthood and the income of their parents for non-Hispanic Black children, non-Hispanic white children, and Hispanic children in our primary analysis sample (1978-83 birth cohorts). Panel A plots children's mean household income ranks in adulthood vs. their parents' income percentile. In each series, each point represents the mean income rank of children with parents in a single income percentile. Child income is the mean of 2014-2015 household income (when the child is between 31-37 years old), while parent income is mean household income from 1994-1995 and 1998-2000. Children are assigned percentile ranks relative to all other children in their birth cohort, while parents are ranked relative to all parents with children in the same birth cohort. Panel B replicates Panel A, replacing the outcome with an indicator for being incarcerated on April 1, 2010, as recorded on the 2010 Decennial Census Short Form, and focusing solely on male children. Incarceration is defined living in a federal detention center, federal prison, state prison, local jail, residential correctional facility, military jail, or juvenile correctional facility. For each series, we plot curves showing the lowess fit (with a bandwidth of 0.3) that we use as our estimate of the conditional expectation function $f_{rg}(p)$ discussed in Section III.

FIGURE II: Children's Outcomes in Adulthood, by Census Tract in Los Angeles

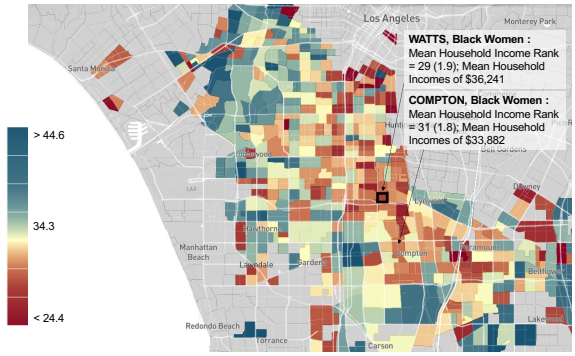
**A. All Children: Household Income Given Parents at 25th Percentile**



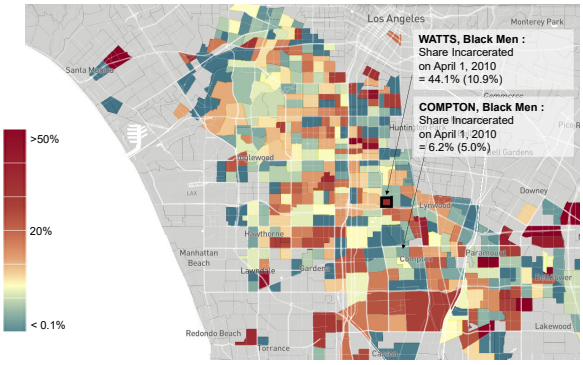

**B. Black Men: Household Income Given Parents at 25th Percentile**




**C. Black Women: Household Income Given Parents at 25th Percentile**



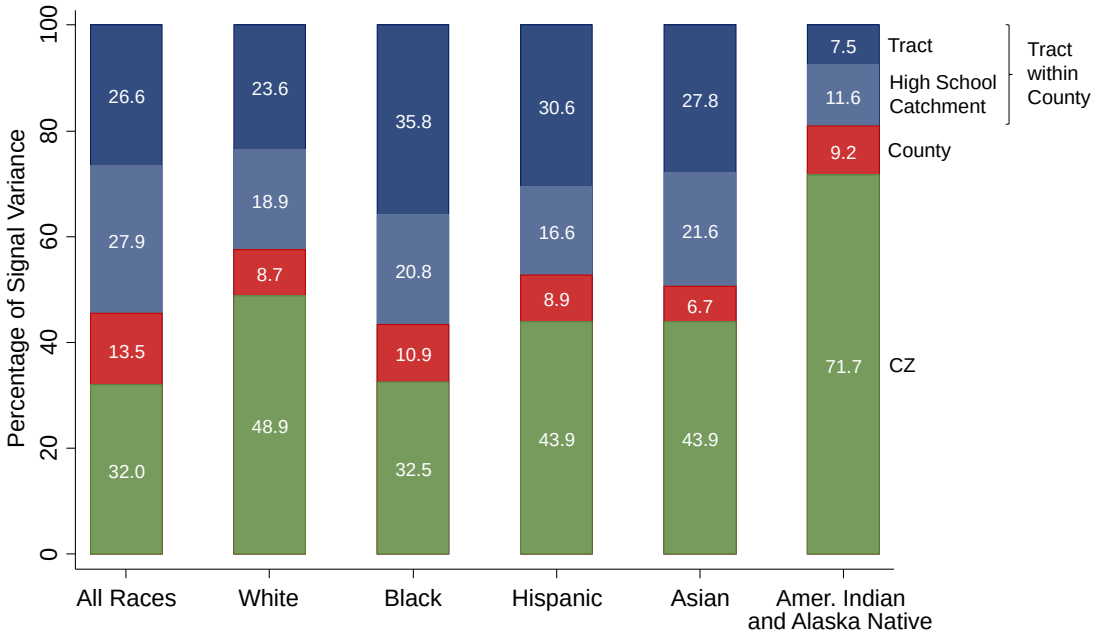

**D. Black Men: Incarceration Rates Given Parents at 1st Percentile**




*These maps must be printed in color to be interpretable.*

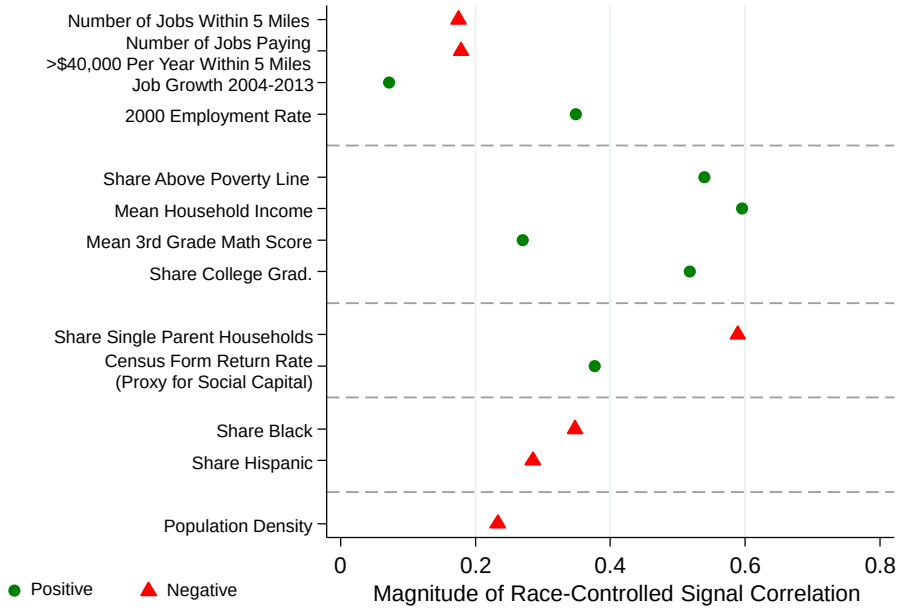
*Notes:* These maps display mean outcomes in adulthood of children who grew up in the Los Angeles metro area, by the tract in which they grew up. Panel A plots our estimates of mean household income ranks for children whose parents were at the 25th percentile of the national household income distribution (an income of approximately $27,000) using our primary analysis sample (1978-83 birth cohorts), which we hereafter refer to as "upward mobility." Upward mobility is estimated separately in each tract using linear regressions of children's income ranks on a tract-invariant transformation of parent income rank $f_{rg}(p)$ that is estimated at the national race-by-gender level using a lowess fit, as shown in Figure I. We weight each child by the number of years they lived in each tract up to and including the age of 23 when estimating these regressions. Finally, we add independent Gaussian noise to the resulting tract-level estimates to protect privacy; the standard deviation of this noise is typically less than one-tenth of the standard error due to sampling variation. Panels B and C replicate Panel A, limiting the sample to non-Hispanic Black male and female children, respectively. Panel D replicates Panel B for Black men with parents at the 1st percentile, using an indicator for being incarcerated on April 1, 2010 as the outcome. In each panel, we report point estimates and standard errors (in parentheses) for selected tracts. The standard errors reported include the noise added to protect privacy. The dollar values in Panels B and C are constructed as described in Appendix C. Tracts shown in gray are areas with no estimate due to insufficient data (fewer than 20 observations in the race-by-gender cell). See notes to Figure I for definitions of income and incarceration.

FIGURE III: Geographic Decomposition of Variance in Upward Mobility



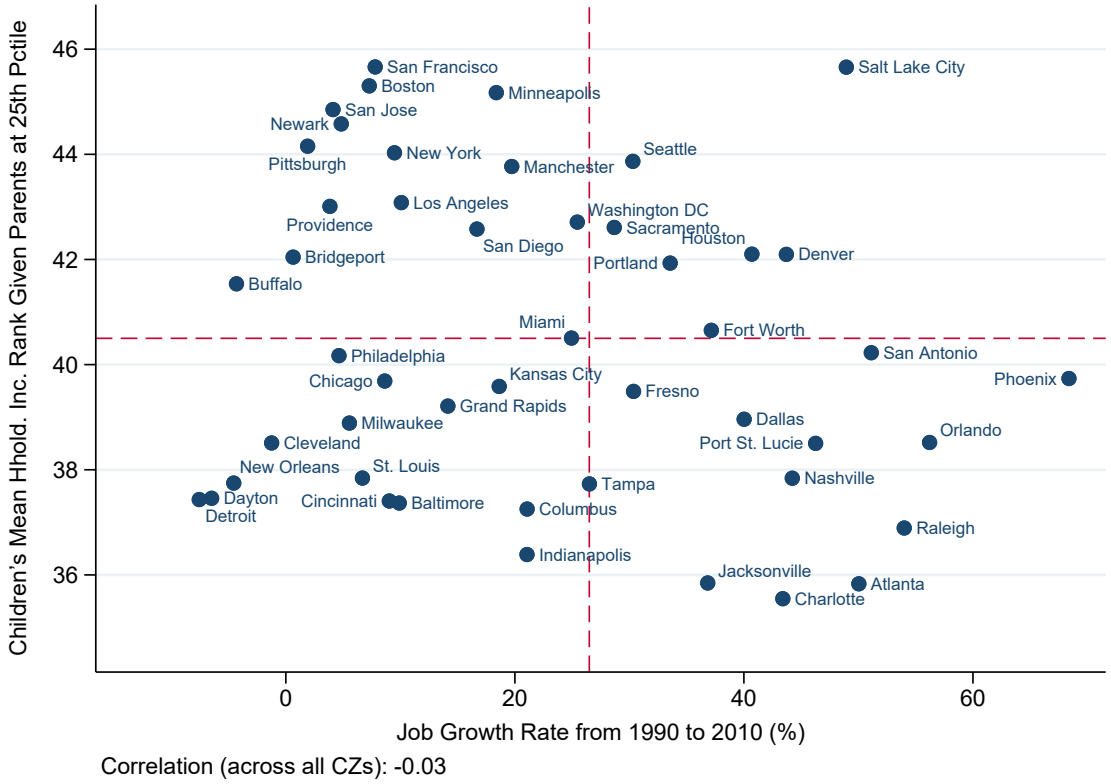

*Notes:* This figure presents a geographical variance decomposition of the tract-level estimates of upward mobility (children's mean household income ranks given parents at the 25th percentile), which are constructed as described in the notes to Figure II. We estimate the share of variance explained by each level of geography as the adjusted R-squared in a regression of the tract-level estimates on fixed effects for different levels of nested geographies, weighted by the number of children in each tract whose parents earn less than the national median income. We correct for sampling and noise-infusion error by rescaling the adjusted R-squared by the reliability ratio – the ratio of the signal variance to total variance of the tract-level estimates reported in Table II. We plot the share of signal variance explained by CZ fixed effects, county fixed effects, high school catchment area fixed effects, and the residual (attributed to tract-within-school catchment area). Tracts are not perfectly nested within catchment areas; we create an approximate crosswalk by assigning tracts to the school catchment area that contains the majority of their land area, as discussed in Online Appendix A.

FIGURE IV: Tract-Level Correlations Between Neighborhood Characteristics and Upward Mobility



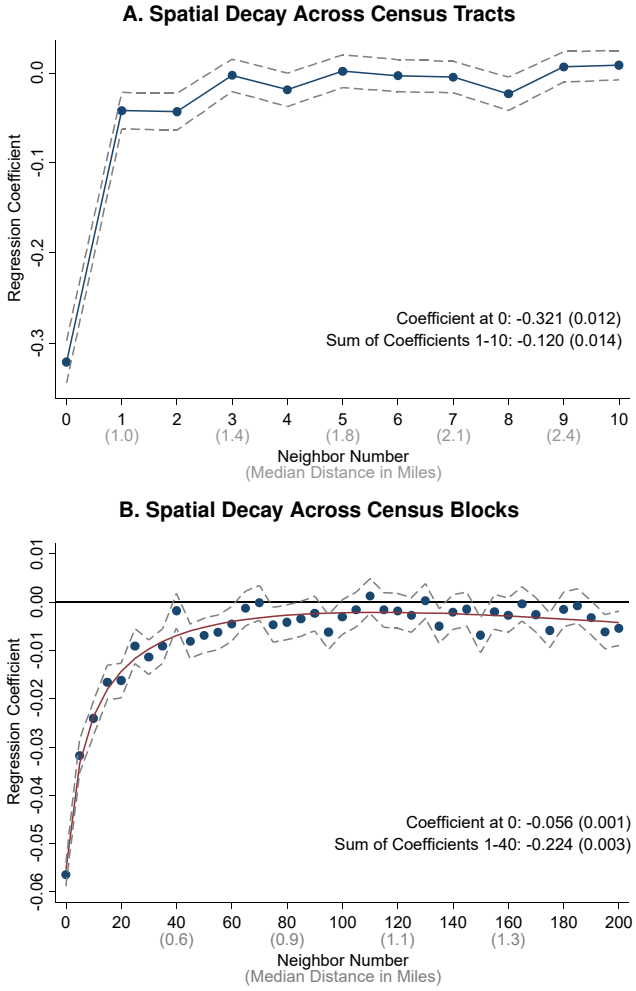

*Notes:* This figure plots univariate, race-controlled correlations between various tract-level characteristics and our estimates of upward mobility in each tract (children's mean household income ranks given parents at the 25th percentile, constructed as described in the notes to Figure II). The correlations are weighted by the number of children in each tract whose parents earn less than the national median income and are estimated using tract-within-CZ variation by demeaning all variables by CZ prior to estimating the correlations. We control for race when estimating each correlation coefficient by first estimating five separate correlations for each racial group (non-Hispanic Asian, non-Hispanic Black, non-Hispanic white, American Indian, and Hispanic populations) and then taking a mean of the five correlations, weighting each of the five groups by its national population share in the 2000 Decennial Census. We estimate signal correlations that adjust for attenuation due to sampling error and noise infusion in our upward mobility estimates by dividing the raw correlations by the square root of the reliability ratio. The reliability ratio is defined as one minus the ratio of the noise variance (estimated as the mean standard error squared) to the total within-CZ variance of the upward mobility estimates. Red triangles denote negative correlations, while green circles denote positive correlations. See Online Appendix B for definitions of each of the characteristics.

FIGURE V: Upward Mobility vs. Job Growth in the 50 Largest CZs



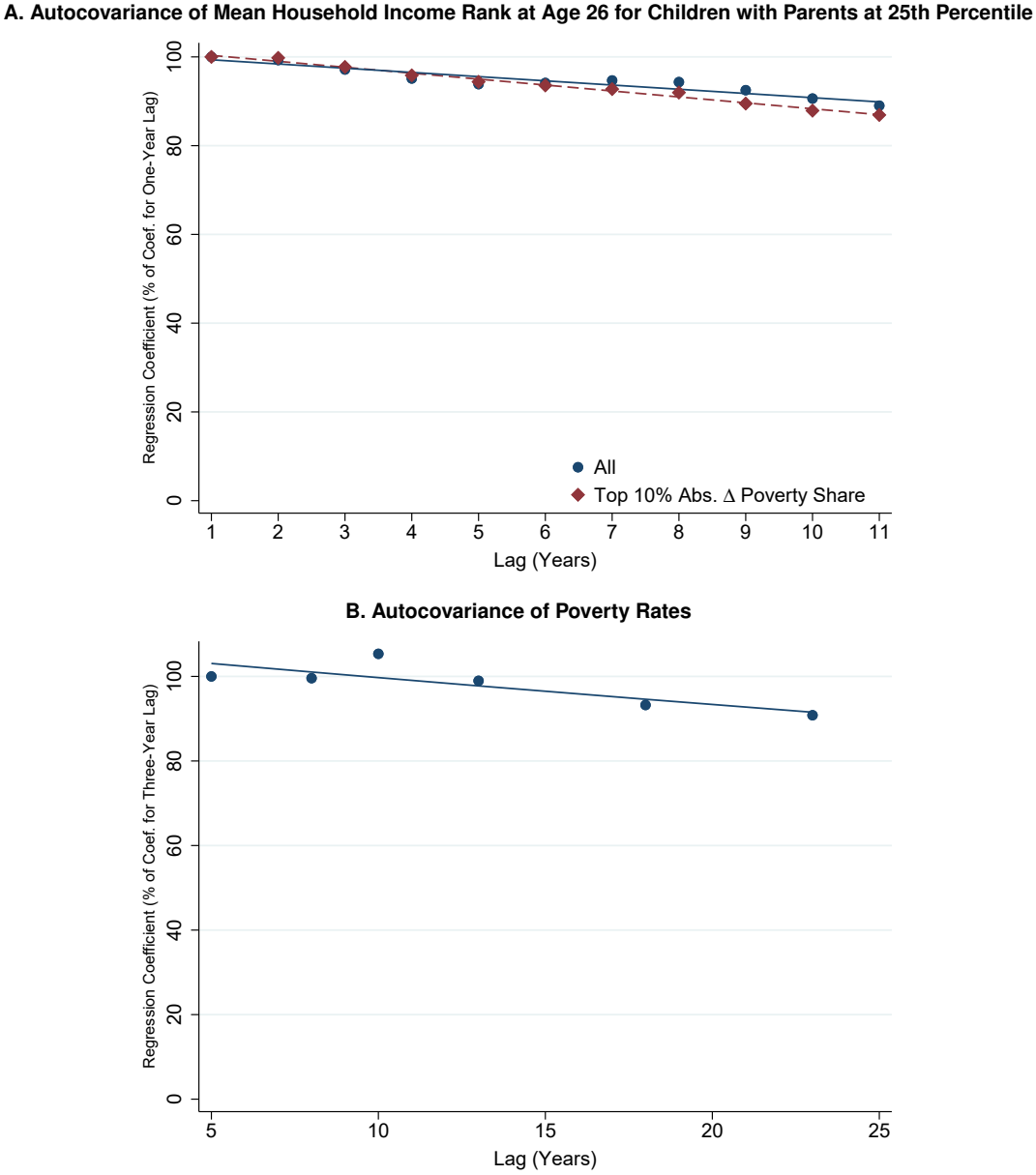

*Notes:* This figure presents a scatter plot of upward mobility in each CZ vs. the rate of job growth between 1990 and 2010 in the 50 largest CZs based on their populations in 2000. Upward mobility is constructed as described in the notes to Figure II. Job growth rates are defined as the percentage change in employment in each CZ using data from the Local Area Unemployment Statistics from the Bureau of Labor Statistics. We omit Las Vegas and Austin from the figure for scaling purposes as they have exceptionally high growth rates; the x and y coordinates for these CZs are: Las Vegas (107.7, 38.9) and Austin (87.9, 40.3). We also report the signal correlation across all CZs as a reference (weighted by the number of children in each CZ with household income below the national median). We estimate this signal correlation that adjusts for attenuation due to sampling error and noise infusion in our upward mobility estimates by dividing the raw correlation by the square root of the reliability ratio, which is one minus the ratio of the noise variance (estimated as the mean standard error squared) to the total variance of the upward mobility estimates.

FIGURE VI: Spatial Decay of Correlation Between Upward Mobility and Poverty Rates



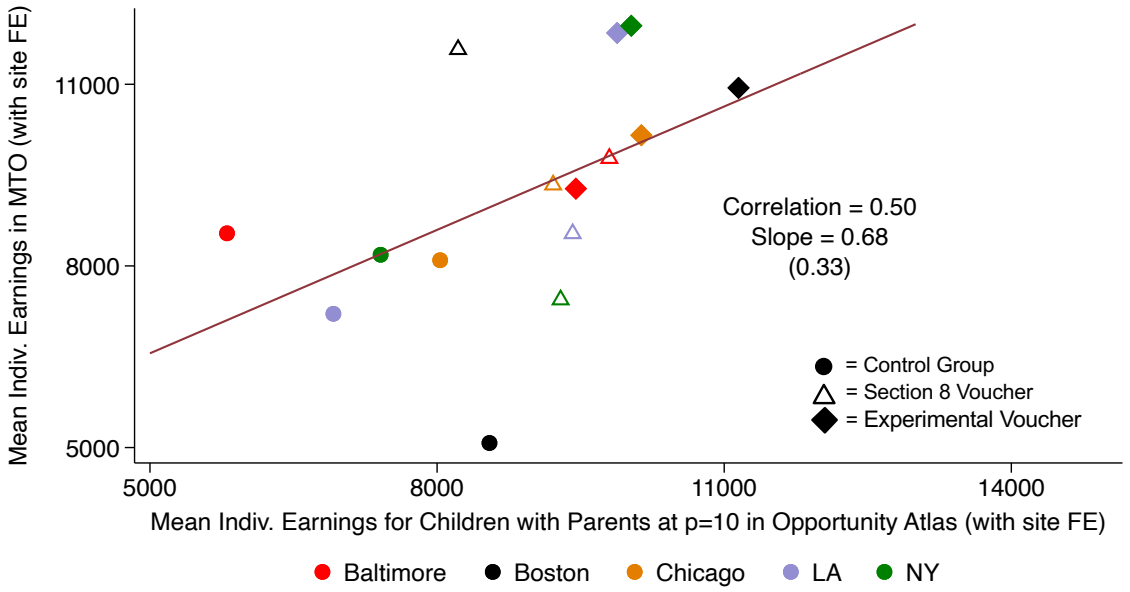

*Notes:* This figure plots the spatial decay of the relationship between upward mobility for whites and poverty rates in the top 50 commuting zones by population at two different levels of geography: Census tracts (Panel A) and Census blocks (Panel B). Upward mobility refers to children's mean household income ranks given parents at the 25th percentile, constructed as described in the notes to Figure II. Tract-level poverty rates are obtained from the publicly available 2000 Decennial Census. Block-level poverty rates are estimated using tax records as the share of families whose total income (wages, social security income, dividends, interest income, and schedule E gains or losses) falls below the poverty line in 2010. To construct Panel A, we first standardize both the upward mobility and poverty rate measures, weighting by the number of children whose parents earn less than the national median. We then regress upward mobility on poverty rates in the same tract and the ten nearest neighbors (defined by the minimum cardinal distance between centroids) and plot the coefficients. To construct Panel B, we regress the household income rank of white children whose parents are between the 20th and 30th percentiles of the income distribution on block-level poverty rates for their own block and the 200 nearest blocks, binned into groups of 5. 95% confidence intervals for the estimates are shown by the dashed lines. In the regressions for both panels, we also include indicator variables for having neighbors in a given distance bin, as some of the neighboring tracts are non-residential areas with no households. We report the median distance between the own-tract (or block) and neighboring tracts (or blocks) in each of the bins as a reference. We replicate this figure for Black families in Online Appendix Figure VI.

FIGURE VII: Stability of Tract-Level Outcomes and Characteristics Over Time



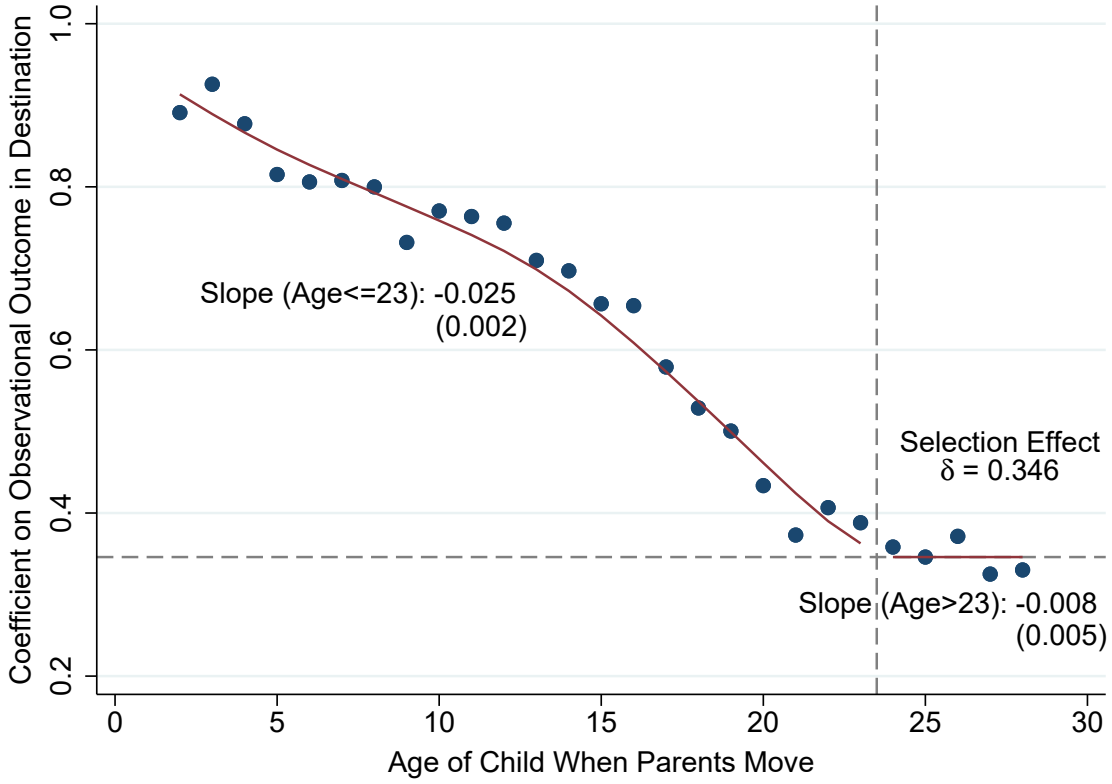

*Notes:* This figure examines the serial correlation of upward mobility (Panel A) and poverty rates (Panel B) within tracts over time. Panel A shows the rate of decay in one's ability to forecast future cohorts' outcomes using historical data. It plots the coefficients from regressions of tract-level estimates of upward mobility for a given cohort $t$ (constructed as described in the notes to Figure II) on estimates of upward mobility from a different birth cohort $t \pm x$, varying $x$ from 1 to 11. The series in blue circles plots the coefficients when including all tracts in the sample. The series in red diamonds plots coefficients for tracts in the top or bottom decile of changes in poverty rates between 1990 and 2000, corresponding to absolute changes in poverty rates of more than 10% (when calculating deciles, tracts are weighted by the number of children with parent incomes below the median). We normalize the estimates by the coefficient of the regression with the one year lag/lead so that the estimates that are plotted can be interpreted as the percentage decay in the forecast coefficient. We extend our primary analysis sample to children born in the 1978-89 birth cohorts and measure children's incomes at age 26 in this figure in order to estimate as many lags as possible. To maximize precision, we use all available cohorts to estimate each covariance; for instance, the covariance at a lag of 1 is estimated using 11 pairs of cohorts. Panel B plots the autocovariance of tract-level poverty rates using publicly available data from the 1990 and 2000 Decennial Census and ACS data collected between 2006 and 2010 and between 2011 and 2015, which we pool to obtain an estimate for 2008 and 2013, respectively. This figure is constructed in the same way as Panel A, estimating the relationship between poverty rates at lags and leads of 5, 8, 10, 13, 18, and 23 years. See Online Appendix B for definition of poverty rates.

FIGURE VIII: Experimental Estimates of Earnings from Moving to Opportunity Experiment vs. Observational Estimates from Opportunity Atlas



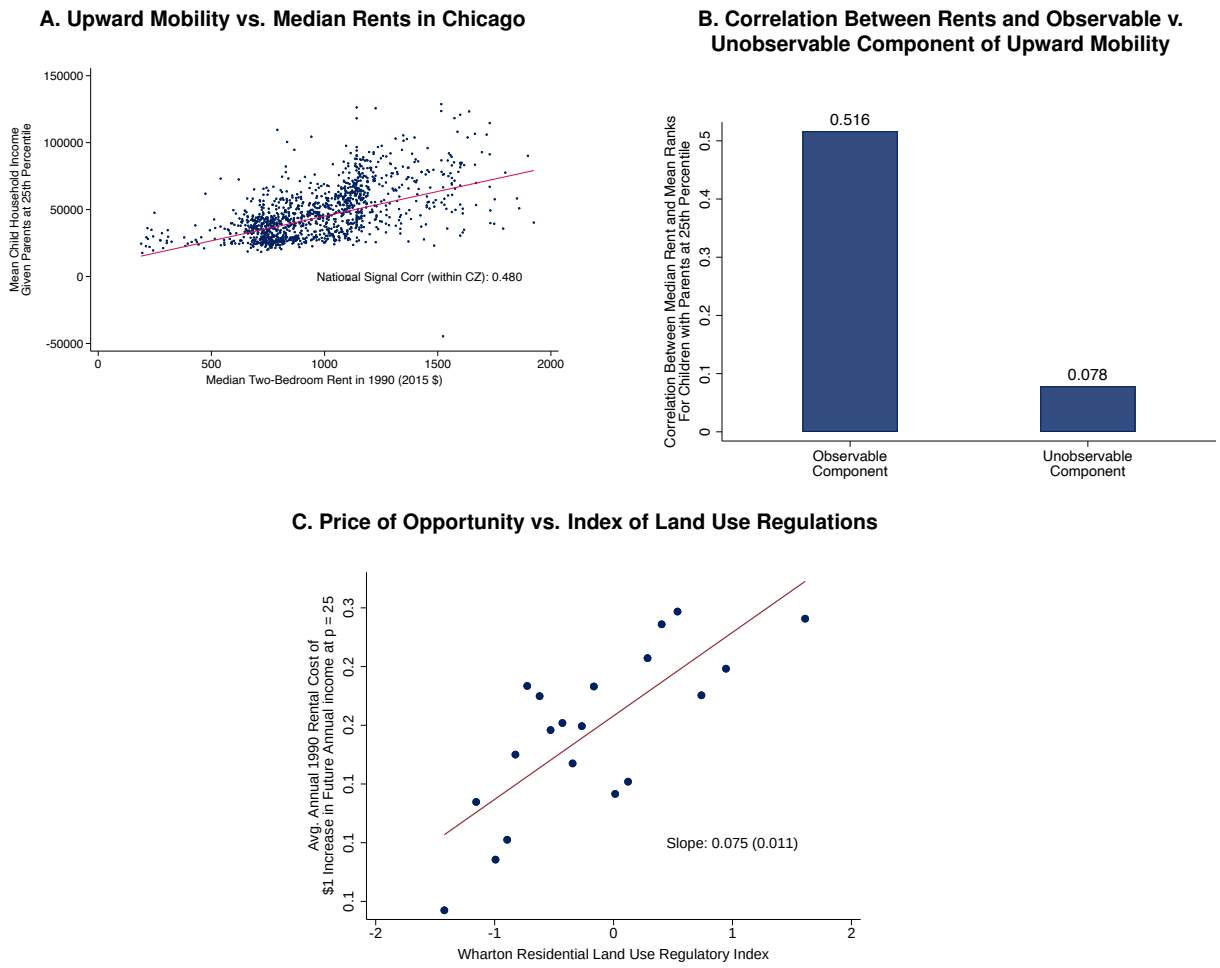

*Notes:* This figure plots estimates of children's earnings in adulthood from the Moving to Opportunity (MTO) experiment vs. children's mean observed earnings in adulthood in the Opportunity Atlas. The y-axis plots fifteen MTO estimates of earnings outcomes for children who were younger than 13 at the time of the experiment, for each of the five cities (sites) where MTO was conducted and for each of the three treatment arms (Control group, Section 8 Voucher group, and Experimental Voucher group). To construct the values, we start from the ITT estimates reported in Chetty, Hendren, and Katz (2016, Online Appendix Table 7b, Panel A). We then construct implied treatment-on-the-treated (TOT) values for the Section 8 and Experimental groups as the mean observed earnings for the control group in the relevant site plus the site-specific ITT estimate for each treatment arm divided by the voucher takeup rate in that arm. To eliminate non-experimental variation across sites, we demean each set of estimates within site, and then add the mean income value observed for those in the MTO control group in Chicago (thereby normalizing estimates to observed earnings levels in Chicago). The x-axis plots observational estimates from the Opportunity Atlas of children's mean earnings in adulthood conditional on having low-income parents for the neighborhoods corresponding to those where children in each of the MTO groups grew up. To construct these estimates, we first identify these neighborhoods by mapping the neighborhood names listed in Online Appendix Table 1c of Chetty, Hendren, and Katz (2016) to Census tracts. We then take a population-weighted mean of children's predicted individual incomes at age 26 in adulthood (the average age at which children's earnings were measured in the MTO sample) across the relevant Census tracts, conditional on having parents at the 10th percentile of the income distribution (approximately the average income of parents in the MTO sample). We obtain these income estimates using a procedure detailed in Appendix I. For the Section 8 and Experimental groups, we assign 15/23 weight to the Atlas estimates in those groups and 8/23 weight to the corresponding control group estimates, to adjust for the average age which a child moved in the MTO sample. As with the MTO estimates, we demean children's incomes within site and add back the estimate for the mean over the set of tracts we use for the control group in Chicago. The best-fit line and slope estimates are based on an unweighted regression of the MTO estimates on the Opportunity Atlas estimates. The figure reports both the regression coefficient (with standard error in parentheses) and the corresponding correlation coefficient.

FIGURE IX: Childhood Exposure Effects: Quasi-Experimental Estimates Using Movers



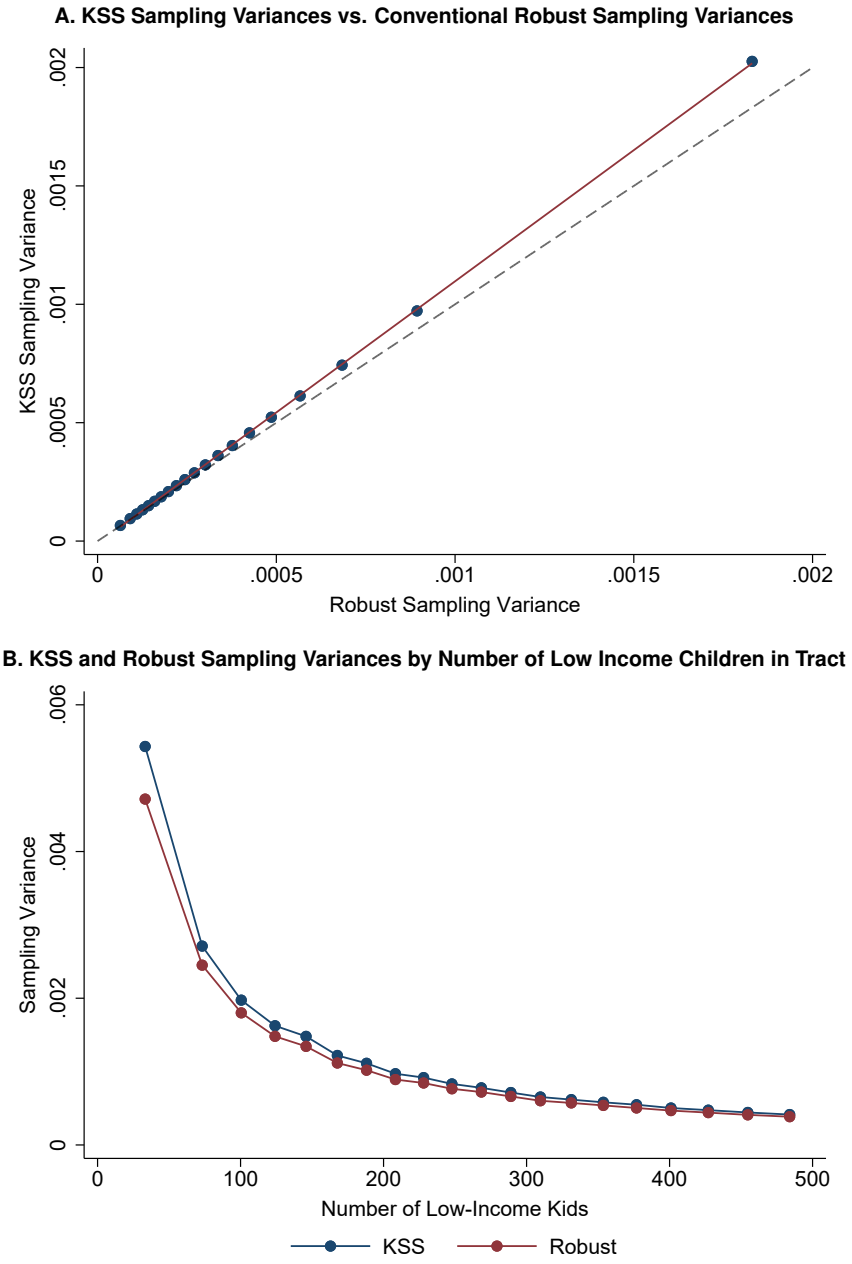

*Notes:* This figure plots the effect of moving to a tract where children have one percentile point higher household income ranks in adulthood, by the age at which children move. To construct the figure, we first estimate mean observed outcomes in each tract following the methodology described in Figure II, except that we (1) pool data from the 1978-91 birth cohorts and measure income at age 24 and (2) exclude all children who move exactly once when they were age 28 or below between 1989-2015. We extend our primary sample to the 1978-91 birth cohorts for this analysis in order to observe moves at earlier ages and exclude one-time movers to avoid having the same observations on the left- and right-hand side of the regression specifications we use in what follows. We then take the set of children who move exactly once between two tracts that are at least 25 miles apart, and regress their household income ranks at age 24 on the difference in the observational predictions between their destination and origin tracts (at the relevant parental income percentile) interacted with indicators for their ages at move as well as the other controls specified in equation (8). The figure plots the resulting regression coefficients ($b_m$) vs. children's ages at move ($m$), along with a lowess fit to these points below age 23. We also report linear slopes and standard errors using unweighted OLS regressions of $b_m$ on $m$, separately for moves at or below age 23 and above age 23. The parameter $\delta$ – defined as the mean value of the age-of-move-specific coefficients for moves older than age 23 – represents a selection effect because moves after age 24 cannot affect income measured at age 24. The dashed horizontal line shows the value of the selection effect $\delta$; the identification assumption underlying the analysis is that the selection effect $\delta$ does not vary with the child's age at move $m$. Under this assumption, the magnitude of the slope for moves below age 23 represents an estimate of the average annual causal childhood exposure effect.

# FIGURE X: The Price of Opportunity



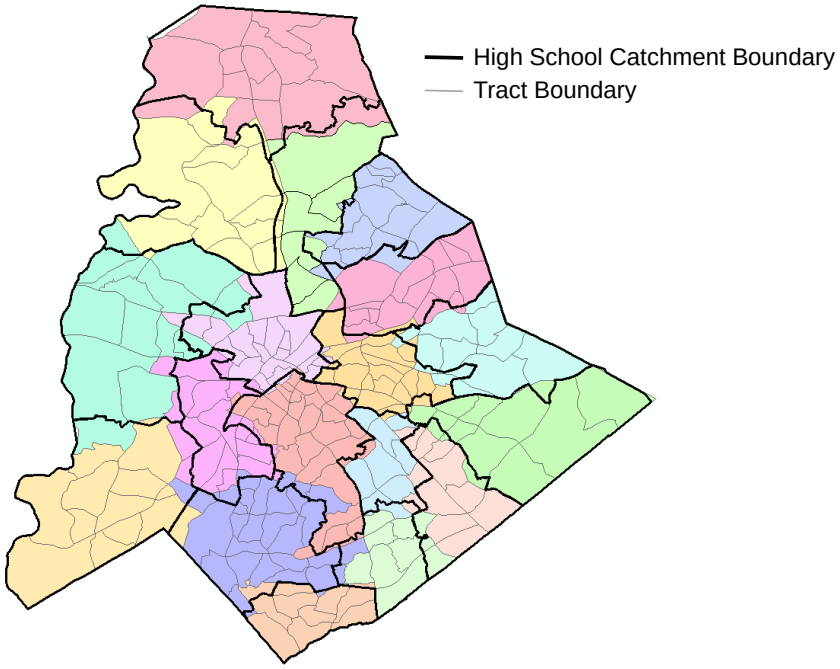

*Notes:* This figure assesses the relationship between our tract-level estimates of upward mobility (constructed as described in the notes to Figure II) and tract-level median rents (measured in the 1990 American Community Survey as the median rent in a tract for a two-bedroom apartment and inflated to 2015 dollars). Panel A presents a scatter plot of upward mobility (measured in dollars) vs. median rent, by tract in the Chicago CZ. We also report the signal correlation between upward mobility and rent within CZs nationally. This and all subsequent correlations and standard deviations that follow are weighted by the number of children with below-median income parents. This signal correlation is estimated by first demeaning both variables within CZs and then adjusting for attenuation due to sampling error and noise infusion in our upward mobility estimates by dividing the raw correlations by the square root of the reliability ratio, which is the ratio of the noise variance to the total within-CZ variance of the upward mobility estimates. Panel B reports tract-level within-CZ correlations between median rents and the observable and unobservable components of our upward mobility estimates. We define the observable component as the predicted value from a national regression of upward mobility on the set of tract-level characteristics used in Figure IV. We define the unobservable component as the residuals from the same regression. We adjust for noise in the unobservable component by reporting a signal correlation. Panel C presents a binned scatter plot of the CZ-specific price of opportunity vs. the Wharton Residential Land Use Regulation Index (WRLURI). To calculate the CZ-specific price of opportunity, we first regress median annual rents on individual income upward mobility (estimated as described in Appendix C) across tracts within a CZ, weighting as specified above. We then inflate this regression coefficient by the reliability of our upward mobility estimate in that CZ (estimated by that CZ's population decile) to adjust for noise. This coefficient can be interpreted as the average annual rental cost of a $1 increase in future annual income for children with parents at the 25th percentile. The WRLURI is obtained from Gyourko, Saiz, and Summers (2007, Wharton Land Regulation Data File) and is available for 247 CZs; we limit our sample to these CZs in Panel C. The slope and standard error from a regression of the price of opportunity on the WRLURI are reported on the plot.

ONLINE APPENDIX FIGURE I: KSS vs. Robust Sampling Variances



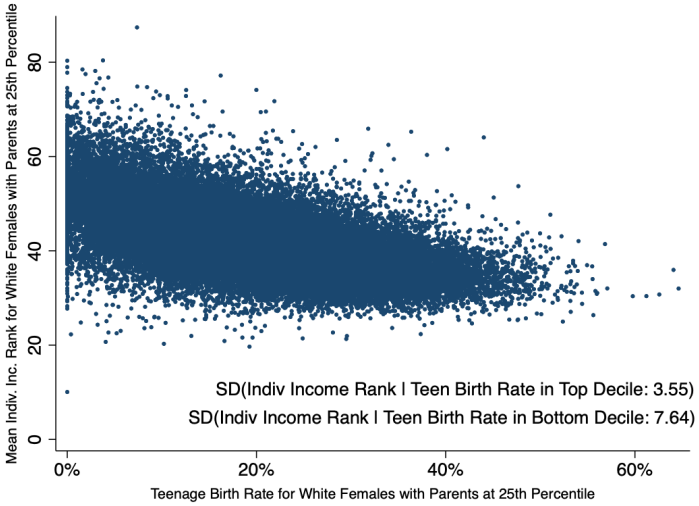

*Notes:* This figure compares the size of Kline, Saggio, and Solvsten (2020) (KSS) and robust (HC1) sampling variances. Panel A is a binned scatter plot of the KSS sampling variances vs. the HC1 robust sampling variances for household income ranks of kids with parents at the 25th income percentile, weighted by the number of children with below-median income parents. . Details on the construction of both sets of sampling variances can be found in Appendix E. We also plot the 45-degree line as a reference. Panel B is a binned scatter plot showing the KSS and HC1 sampling variances for household income ranks of kids with parents at the 25th income percentile as a function of the number of low-income kids in each tract, restricted to tracts with fewer than 500 low-income children.

ONLINE APPENDIX FIGURE II: School Catchment Zone Boundaries in Mecklenburg County, NC



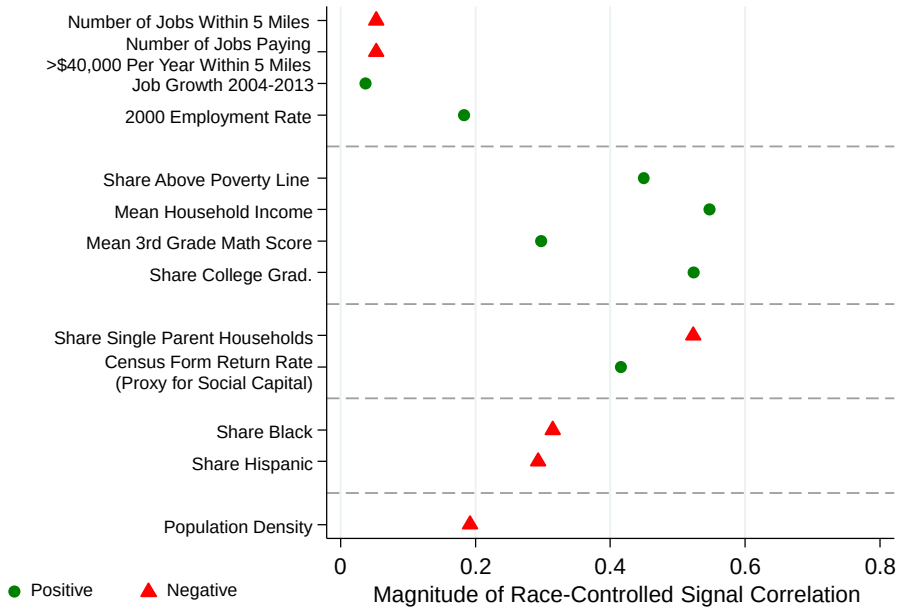

*Notes:* This figure presents a map of exact high school catchment areas (bold lines) in Mecklenburg County, North Carolina overlaid on tract boundaries (thin lines).

ONLINE APPENDIX FIGURE III: Upward Mobility vs. Teenage Birth Rates for White Women



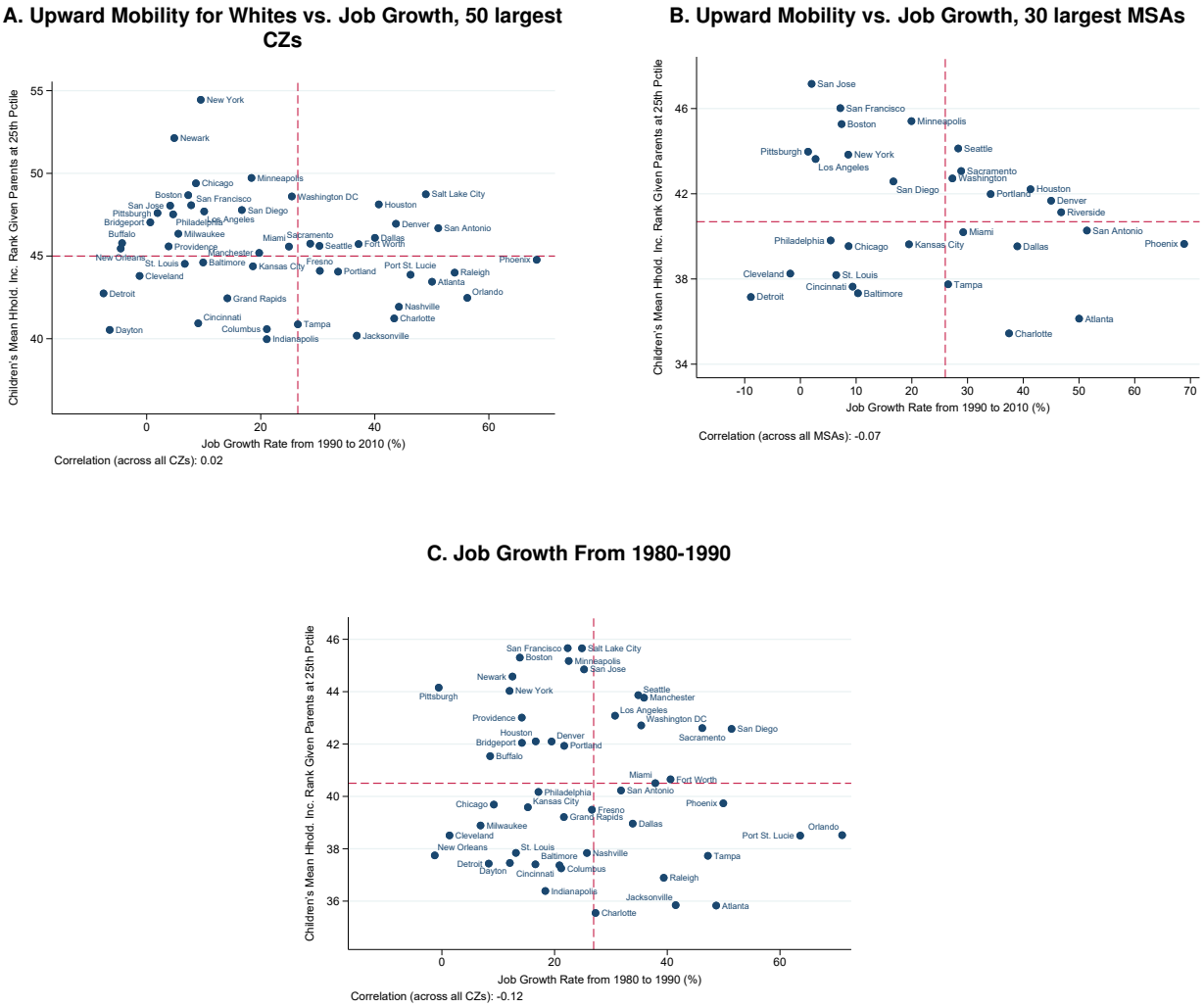

*Notes:* This figure presents a scatter plot of mean individual income ranks vs. the teenage birth rate for white women with parents at the 25th percentile, by Census tract. Mean individual ranks are estimated as described in the notes to Figure II. Teenage birth is an indicator for ever claiming a dependent on a tax return who was born while the claimer was between ages 13 and 19. We limit the sample to tracts in which there are at least 100 observations for white women and bottom-code tracts with negative teenage birth rates to zero (negative values arise due to the addition of noise to the estimates). The standard deviations of mean income ranks reported conditional on having teenage birth rates in the bottom or top decile of the distribution are weighted by the number of white women in each tract whose parents earn less than the national median. We omit one tract in Canton, Michigan for scaling purposes; the x and y coordinates for this tract are (90%, 65.35).

ONLINE APPENDIX FIGURE IV: Tract-Level Correlations Between Neighborhood Characteristics and Children's Outcomes Given Parents at the 75th Percentile



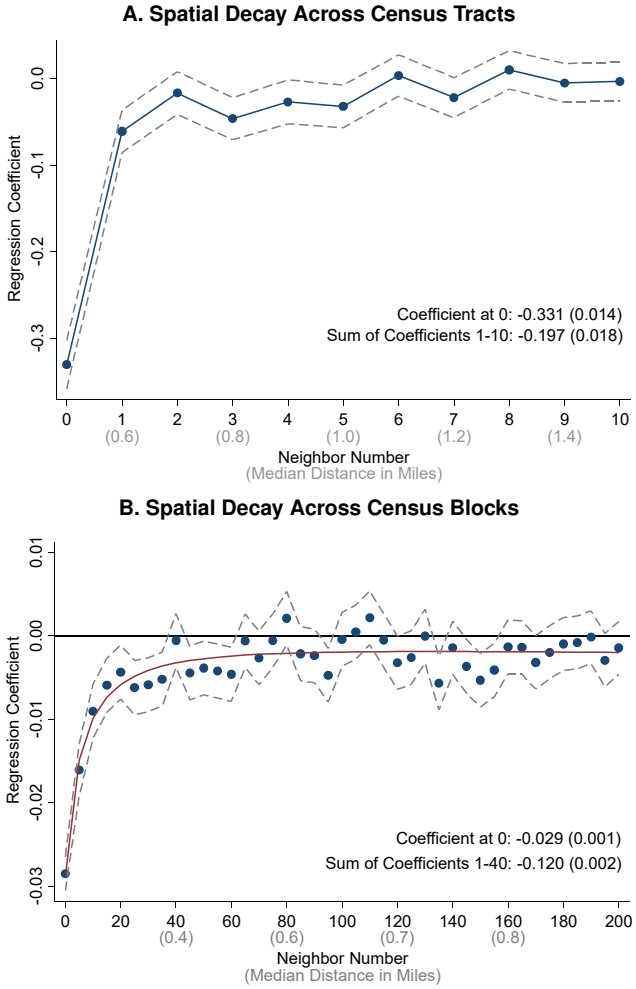

*Notes:* This figure replicates Figure IV using children's mean household income ranks given parents at the 75th percentile, instead of the 25th percentile. See notes to Figure IV for details.

ONLINE APPENDIX FIGURE V: Upward Mobility vs. Job Growth



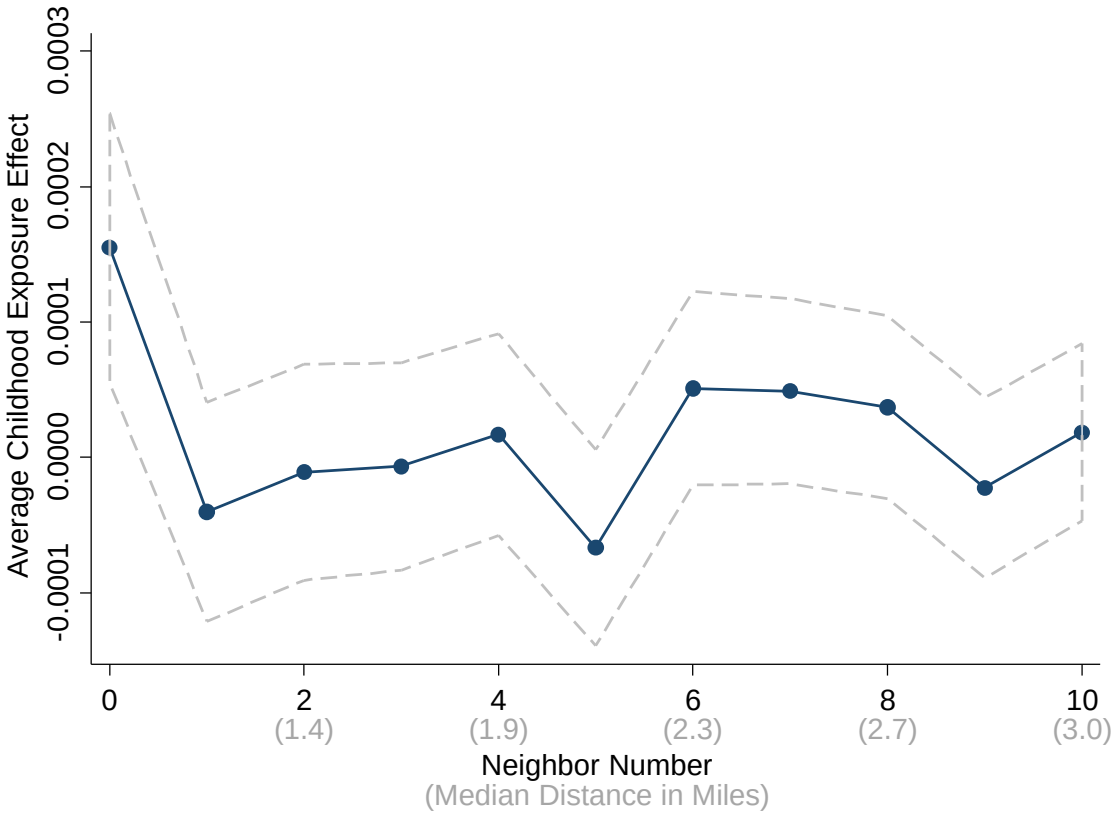

*Notes:* Panel A replicates Figure V using upward mobility for whites on the y-axis. Panel B replicates Figure V for the 30 largest metropolitan statistical areas instead of the 50 largest commuting zones. Panel C replicates Figure V using employment growth from 1980 to 1990. See notes to Figure V for details.

ONLINE APPENDIX FIGURE VI: Spatial Decay of Correlation Between Upward Mobility for Black Children and Poverty Rates




*Notes:* This figure replicates Figure VI for Black children by replacing the dependent variable in the regressions with upward mobility for Black children instead of white children. See notes to Figure VI for details.

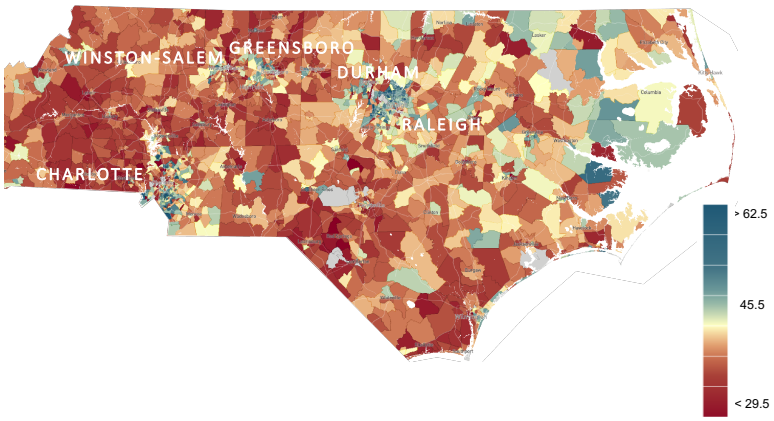
ONLINE APPENDIX FIGURE VII: Predictive Power of Poverty Rates in Actual Destination vs. Neighboring Tracts



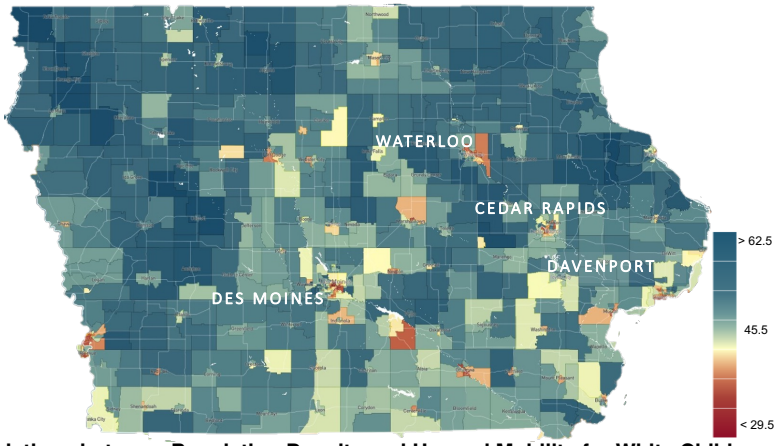

*Notes:* This figure plots coefficients from a regression that identifies childhood exposure effects using a specification analogous to that in Column 2 of Appendix Table IV, which is estimated on the sample of one-time movers who moved at least 25 miles. In the specification in Appendix Table IV, we regress children's household income ranks at age 24 on the difference in the observational predictions between their destination and origin tracts linearly interacted with their age at move (below age 23) and other controls specified in equation (9). Here, we replace the observational predictions on the right hand side with the poverty rates in the origin and destination tracts. We also include symmetric interactions between age at move and poverty rates in the ten tracts that are closest to the actual origin and destination tracts, respectively. We plot the eleven coefficients on the interactions between the destination-origin difference in poverty rates and age at move (for moves below age 23). These coefficients can be interpreted as the causal childhood exposure effect of moving to a tract that is $x$ neighbors away from a tract that has 1 SD higher poverty rates. Dashed lines show 95% confidence intervals for the point estimates. We also report the median distance between the own-tract and neighboring tracts in each of the bins as a reference.

ONLINE APPENDIX FIGURE VIII: Heterogeneity in Relationship between Upward Mobility and Population Density

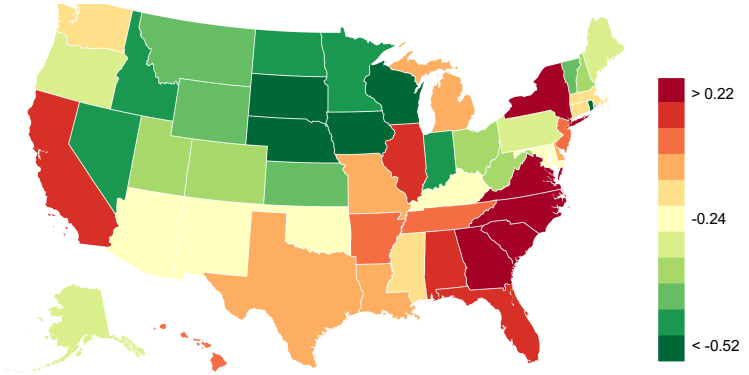
**A. Mean Household Income Rank of White Children with Parents at 25th Percentile, North Carolina**




**B. Mean Household Income Rank of White Children with Parents at 25th Percentile, Iowa**




**C. Correlations between Population Density and Upward Mobility for White Children, by State**

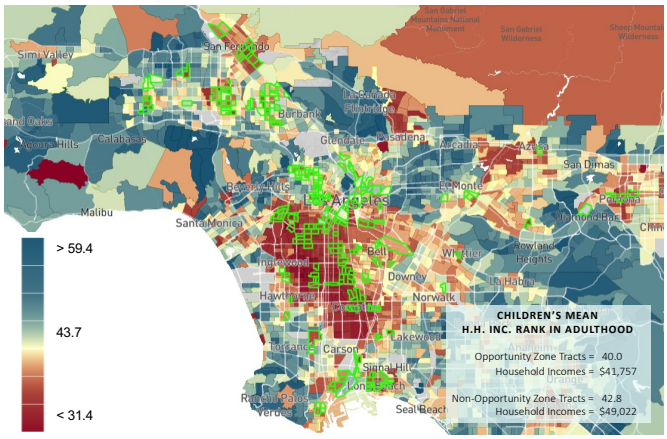



*These maps must be printed in color to be interpretable*

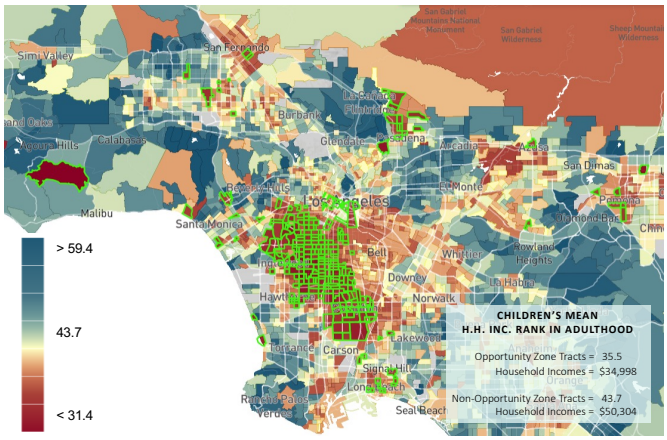
*Notes:* This figure analyzes the relationship between upward mobility (constructed as described in the notes to Figure II) and population density across the U.S. Panels A and B replicate Figure IIa, for white children in North Carolina (Panel A) and Iowa (Panel B). Panel C shows the signal correlation between upward mobility for white children and population density (measured using the 2000 Decennial Census) within each state, weighted by the number of children in each tract whose parents earn less than the national median. We estimate signal correlations that adjust for attenuation due to sampling error and noise infusion in our upward mobility estimates by dividing the raw correlations by the square root of the reliability ratio, which is one minus the ratio of the noise variance (estimated as the mean standard error squared) to the total variance of the upward mobility estimates.

# ONLINE APPENDIX FIGURE IX: Targeting Opportunity Zones in Los Angeles

**A. Actual Tracts Designated as Opportunity Zones**



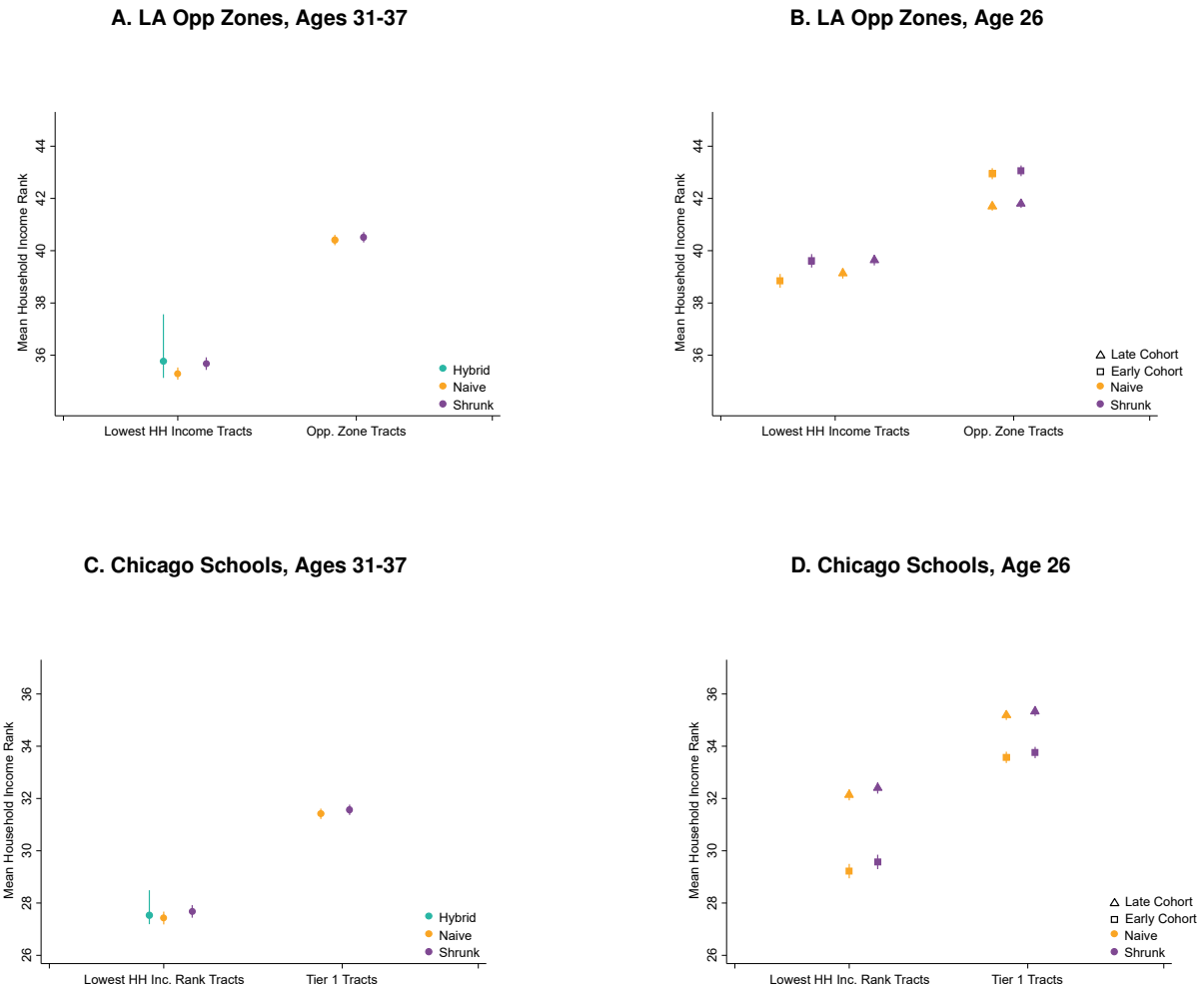

**B. Hypothetical Opportunity Zones using Upward Mobility Estimates**




*These maps must be printed in color to be interpretable.*

*Notes:* These maps replicate Figure IIa, plotting children's mean household income ranks given parents at the 25th percentile in Los Angeles. In Panel A, we outline in green borders the tracts that have been designated as Opportunity Zones in Los Angeles. Opportunity Zones are a federal incentive included in the Tax Cuts and Jobs Act to spur investment and improve economic opportunity in low-opportunity neighborhoods. State governments designated qualified areas to receive a host of tax benefits based on poverty and income. In Panel B, we consider a hypothetical alternative targeting strategy, designating the same number of zones in Panel A, but choosing the tracts with the lowest rates of upward mobility in Los Angeles county. In each case, we also report the mean household income rank in adulthood of children with parents at the 25th percentile for areas designated as Opportunity Zones vs. those that are not. Dollar values are obtained by taking mean household income outcomes, as defined in Appendix C, within each set of tracts.

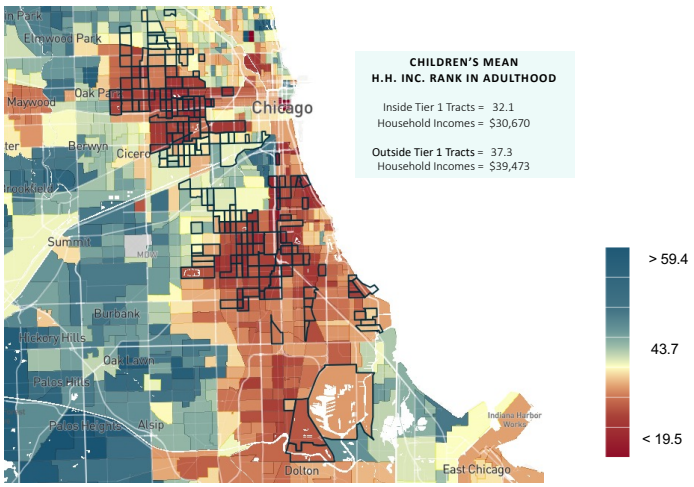
ONLINE APPENDIX FIGURE X: Gains from Targeting Using Alternative Methods



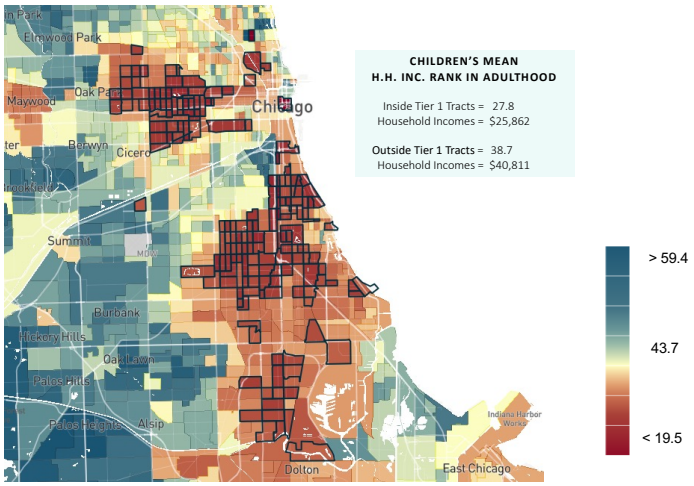

*Notes:* This figure plots mean household income ranks in adulthood of children with parents at the 25th percentile along with 95% confidence intervals. Panels A and C measure incomes between ages 31 and 37 for children born in 1978-1983, as in our baseline analysis. Panels B and D measure incomes at age 26 for children born in 1978-1983 ("Early Cohort") and children born in 1984-1989 ("Late Cohort"). Panels A and B compare mean ranks for children raised in the 269 designated Opportunity Zone census tracts in Los Angeles ("Opp. Zone Tracts") with mean ranks for children raised in the 269 census tracts with lowest upward mobility using Atlas estimates ("Lowest HH Inc. Rank Tracts"). Panels C and D compare mean ranks for children raised in the 181 designated Tier 1 census tracts among Chicago schools with mean ranks for children raised in the 181 census tracts with lowest upward mobility using Atlas estimates. We report estimates of mean ranks and confidence intervals constructed using three methods: *Naive*: conventional means and asymptotic confidence intervals. *Shrunk*: conventional means with asymptotic confidence intervals using tract estimates that have been shrunk towards the county mean. *Hybrid*: the hybrid "winner's curse" estimators and confidence intervals proposed by Andrews, Kitagawa, and McCloskey (2020). All estimates reported in this figure are unweighted means over the relevant tracts.

ONLINE APPENDIX FIGURE XI: Targeting Selective High School Admissions in Chicago

**A. Actual Tracts Granted Tier 1 Preferential Admission Status**



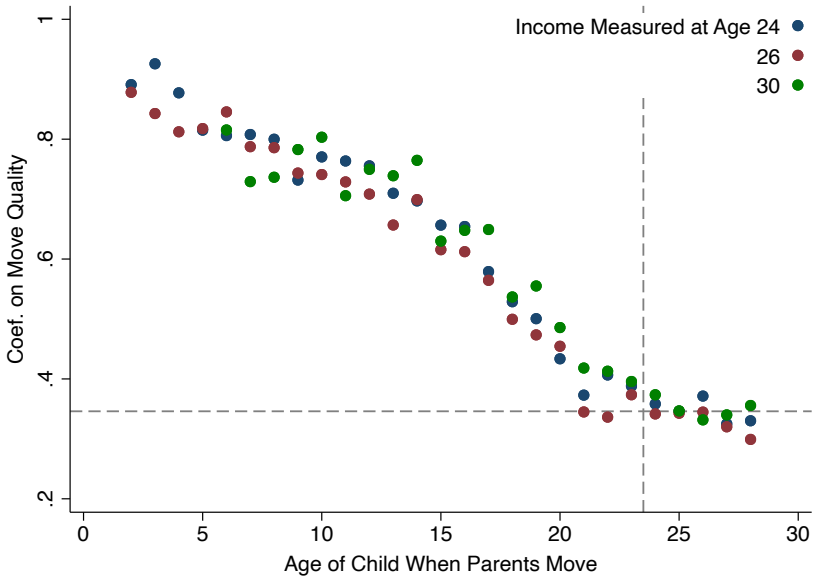

**B. Hypothetical Tier 1 Tracts using Upward Mobility Estimates**




*These maps must be printed in color to be interpretable.*

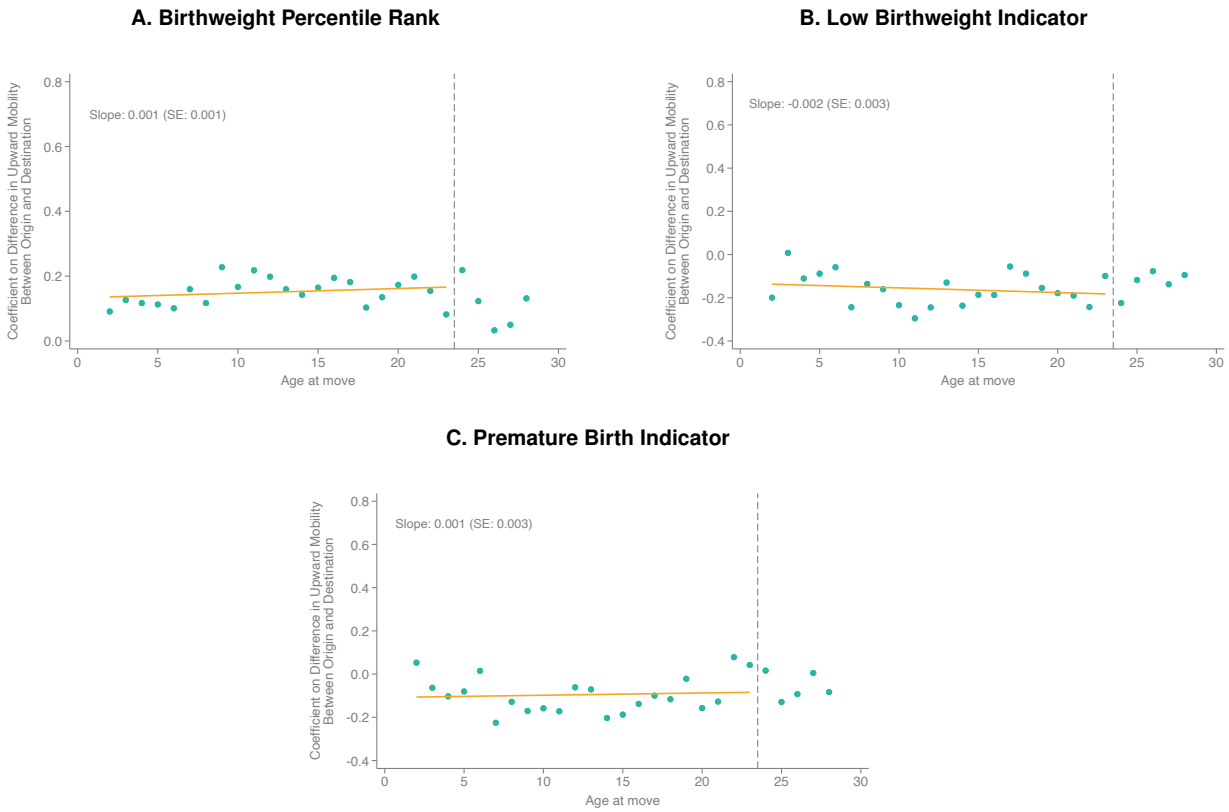
*Notes:* These maps replicate Figure IIa, plotting children's mean household income ranks given parents at the 25th percentile in Chicago. In Panel A, we outline in black borders the tracts that have been designated as Chicago Exam School Tier 1 tracts. The Chicago Public School tier-based admission system was created to give students from underserved areas greater access to selective schools. Chicago tracts are placed in one of four tracts, where Tier 1 tracts are the most underserved. In Panel B, we consider a hypothetical alternative targeting strategy, designating the same number of zones in Panel A, but choosing the tracts with the lowest rates of upward mobility in Cook County. In each case, we also report the mean household income rank in adulthood of children with parents at the 25th percentile for areas designated as Tier 1 tracts vs. those that are not. Dollar values are obtained by taking mean household income outcomes, as defined in Appendix C, within each set of tracts.

ONLINE APPENDIX FIGURE XII: Movers Estimates Using Outcomes at Different Ages



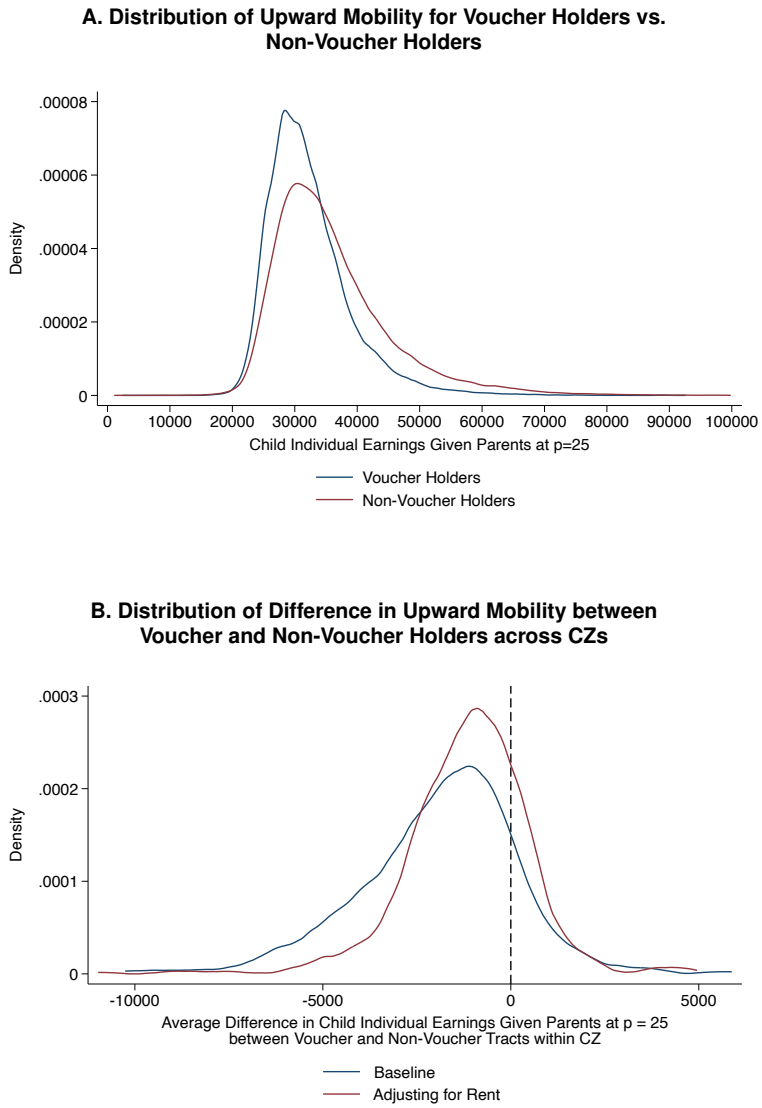

*Notes:* This figure replicates our movers analysis from Figure IX. We use the same specification but report estimates in which we measure earnings (for both the left and right-hand side variables) using age 26 or age 30 income rank as well as age 24.

ONLINE APPENDIX FIGURE XIII: Birth Outcomes Placebo Tests for Movers Design



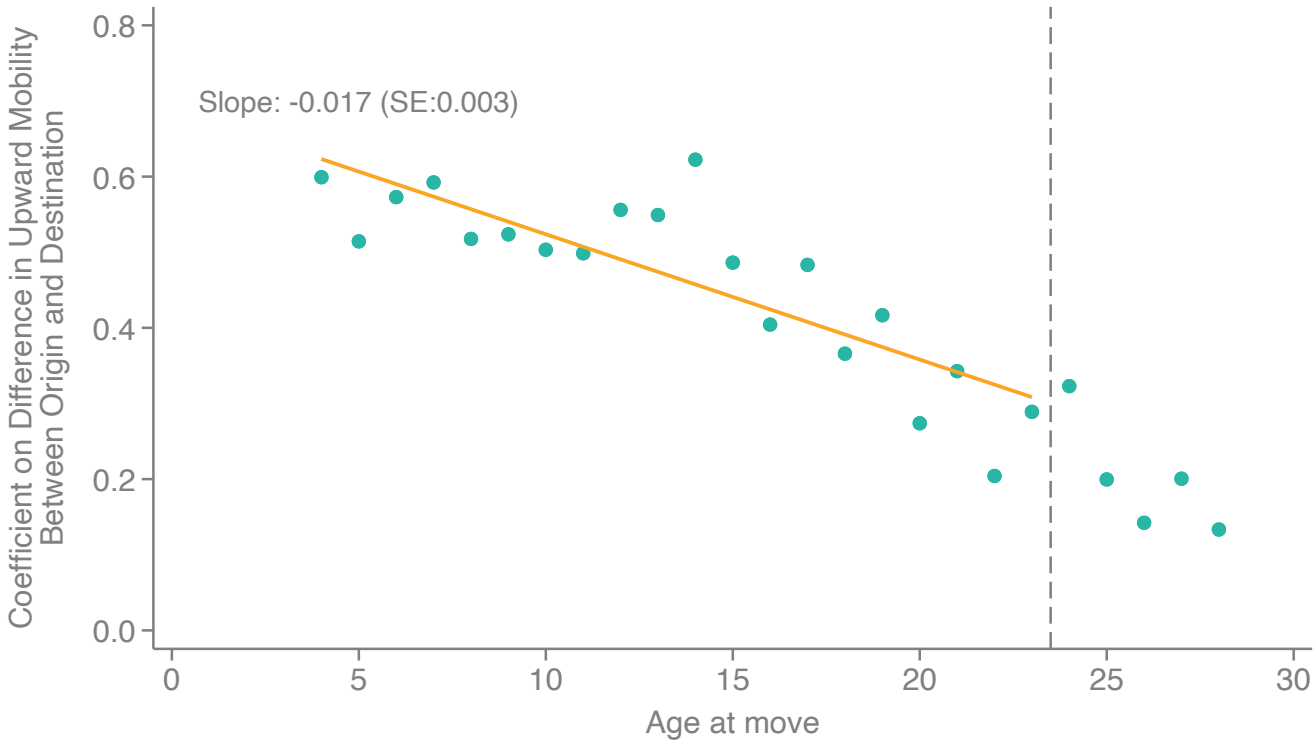

*Notes:* This figure replicates our movers specification with different outcome variables on the left hand side of the regression. In Panel A, the outcome is within-birth-cohort birthweight percentile rank. In Panel B, it is an indicator for whether a child is low birthweight, defined by having birthweight below the 20th percentile of the within-cohort birthweight distribution. In Panel C, the outcome is an indicator for whether the child was born preterm (gestation length below 259 days, 3 weeks less than full term).

ONLINE APPENDIX FIGURE XIV: Upward Mobility in Neighborhoods Where Voucher Recipients vs. Non-Recipients Live



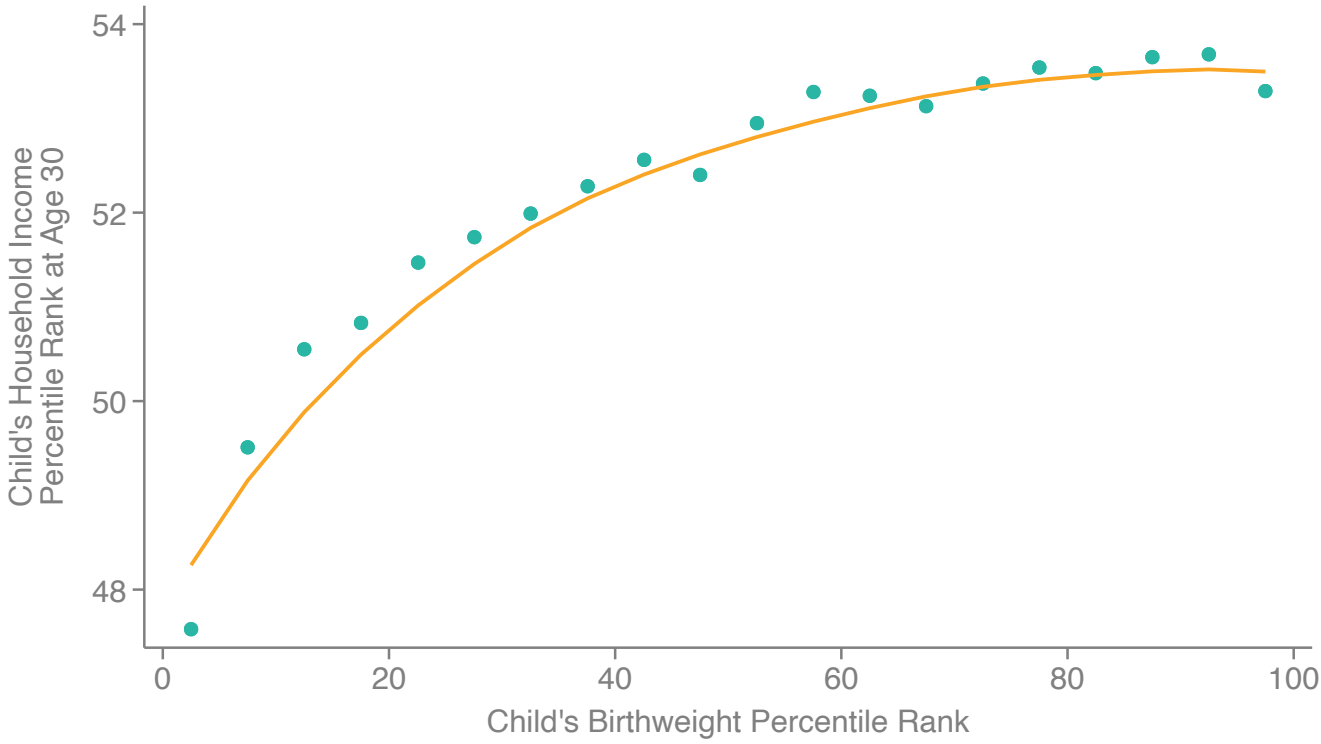

*Notes:* Panel A plots the distribution of children's individual earnings given parents at the 25th income percentile across tracts, weighted by the number of voucher holders in the tract (blue series) or the number of non voucher holders (red series). Panel B plots the distribution of the average difference in earnings between tracts where voucher holders grow up versus average neighborhoods across CZs, first raw and then controlling for rent. Within each CZ, we estimate the average upward mobility over tracts, weighting by the number of voucher holding residents in 2015, according to publicly available HUD data. We then compute average upward mobility weighted by the number of non-voucher holders in each tract, which we obtain by subtracting the number of voucher holders from 2010 Census population data. The blue series (Baseline) plots the distribution of the difference between these two means across CZs. To construct the series that controls for rents, we estimate the density of rents across tracts, once weighting by the number of voucher holding residents and once weighting by the number of non-voucher holding residents, taking the ratio of these two densities as weights. We then compute average mobility for non-voucher holders weighting by this ratio. Finally, we calculate the difference between upward mobility for voucher-holders and the rent-adjusted estimates for non-voucher holders, and again plot the distribution of these differences across CZs.

ONLINE APPENDIX FIGURE XV: Impact of Moving to a Higher-Upward-Mobility Neighborhood on Income at Age 30 in California



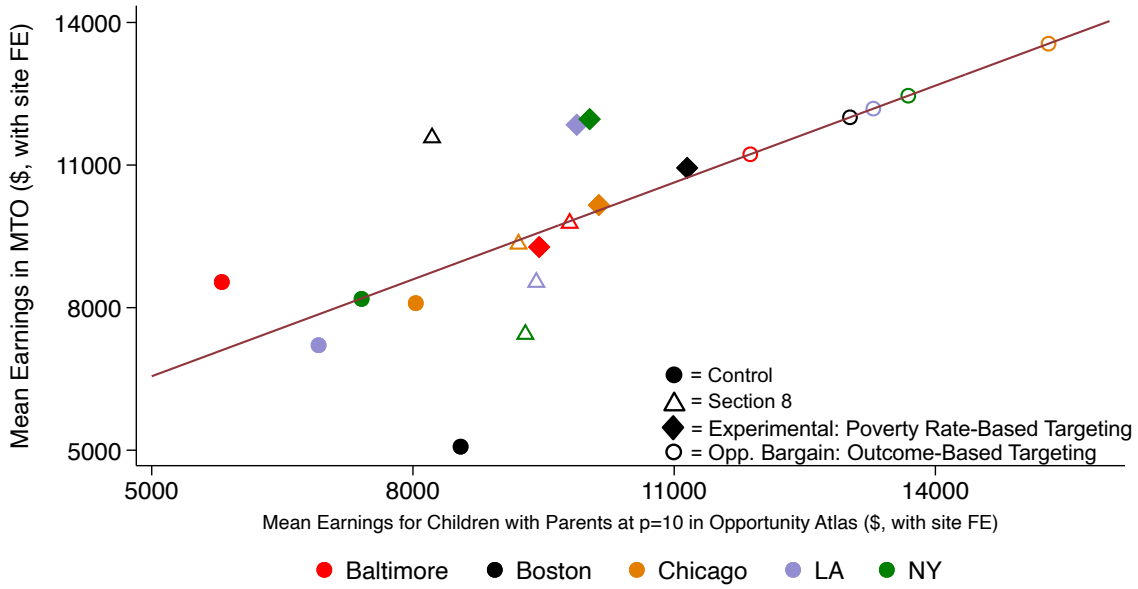

*Notes*: This figure plots the effect of moving to a Census tract where children have one percentile point higher income ranks at the age of 30, by the age at which children move. To construct the figure, we first estimate mean observed outcomes in each tract following the methodology of Section III, except that here we pool across 1978-1987 birth cohorts to measure income at age 30. We then implement the same specification as in Figure IX, taking the set of one-time movers born in California (for whom we have birth outcome data) who move between tracts that are more than 25 miles apart and regressing their household income ranks at age 30 on the difference in observational measures of upward mobility (predicted income rank at age 30 for non-one-time movers) between their destination and origin tracts interacted with age at move, as well as controls for parent income and origin upward mobility interacted with age at move. We then plot the resulting regression coefficients on the difference in upward mobility by age at move, along with a linear fit to these points below age 23. We report unweighted OLS linear regression slopes and standard errors of the coefficients on the age at move for available ages up to age 23. See notes to Figure IX for further details.

ONLINE APPENDIX FIGURE XVI: Household Income Rank at Age 30 vs Birthweight Rank



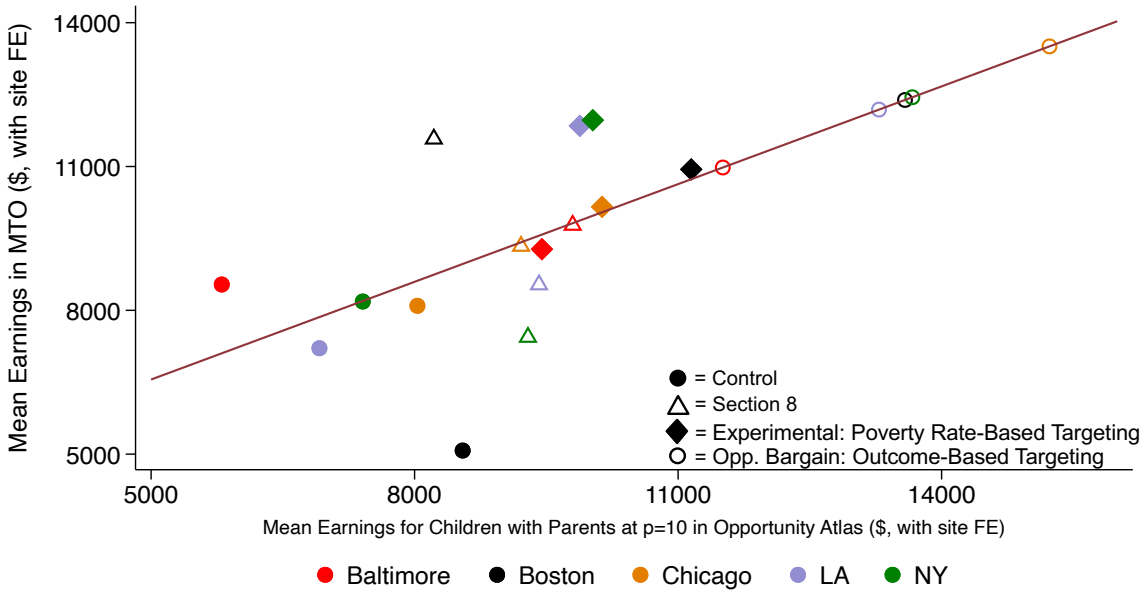

*Notes*: This figure is a binned scatter plot of children's household income percentile rank at age 30 against their birthweight percentile rank. A lowess fit is shown on the points of the binned scatter plot.

ONLINE APPENDIX FIGURE XVII: Predicted Impacts of Moving to "Opportunity Bargain" Areas in MTO Cities



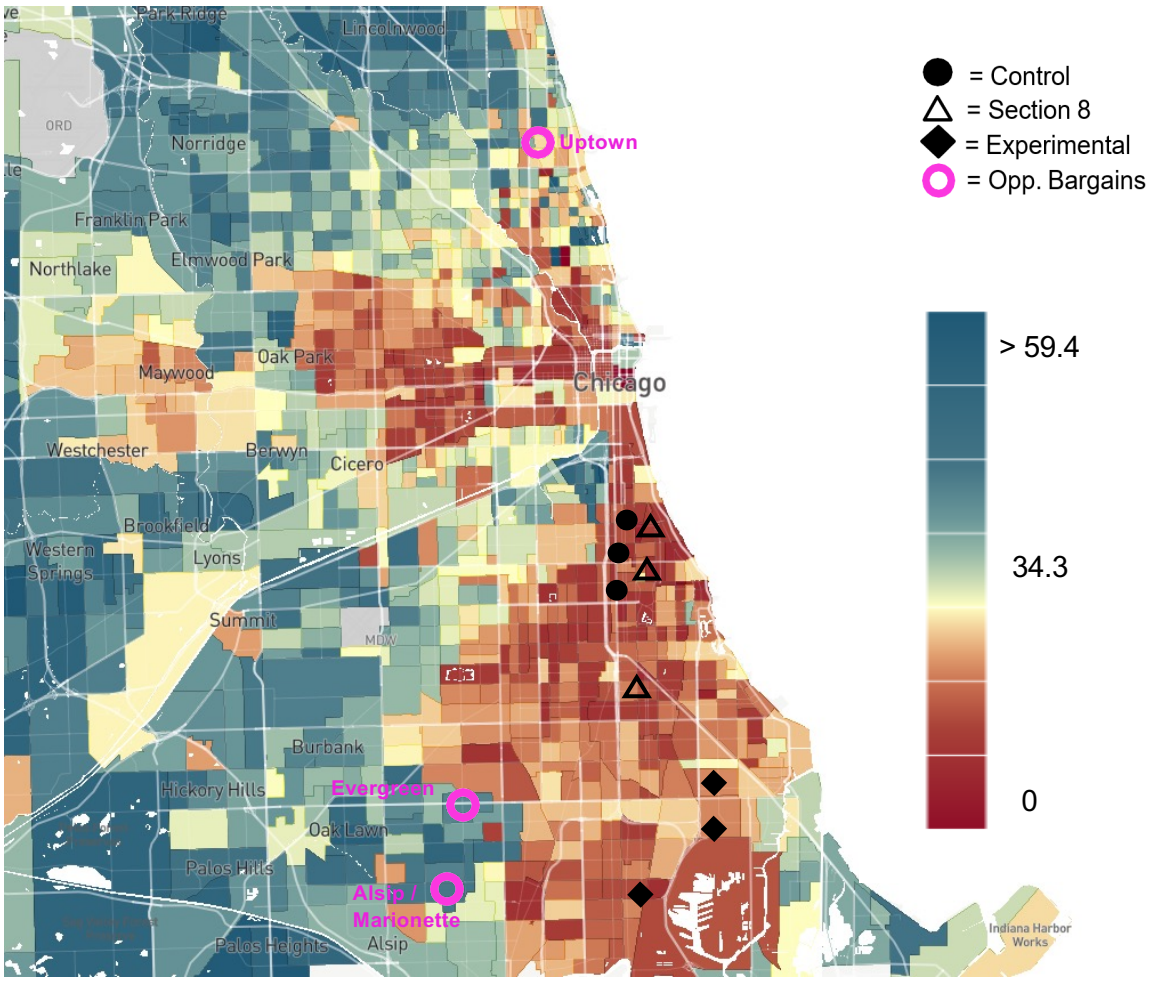

*Notes:* This figure replicates Figure VIII, adding five additional points (open circles) that show the predicted outcomes of children who grow up in "opportunity bargain" tracts in each of the five MTO cities. We define opportunity bargain areas in two steps. First, we count the number of tracts that would have been available to experimental voucher holders in the MTO experiment based on the official requirement that the poverty rate should be less than 10% in the 1990 census, which we denote by $N_c$. In the second step, we rank tracts within each city in descending order based on their mean observed individual income at age 26 conditional on having parents at the 10th percentile in the Opportunity Atlas data. The steps we take to construct this outcome are detailed in Appendix I. We then take the $N_c$ highest ranking tracts from the second step that satisfy the following two criteria: (1) median rent in 2000 (based on publicly available 2000 Decennial Census data) is less than or equal to the 90th percentile of the distribution of rents across the tracts where experimental or Section 8 voucher recipients moved in the same city and (2) the commute time using public transportation (as of May 29, 2018 at 8:00 AM, obtained from Google Maps) from the MTO control group tracts is less or equal to the 90th percentile of the distribution of commute times from the control locations to the tracts where experimental or Section 8 voucher recipients moved in the same city. Tracts where control group members and voucher recipients lived are identified by mapping the neighborhood names listed in Online Appendix Table 1c of Chetty, Hendren, and Katz (2016) to Census tracts. Once we have identified the set of opportunity bargain tracts in each city, we compute a population-weighted mean of children's predicted individual income at age 26 in adulthood across the relevant Census tracts.

ONLINE APPENDIX FIGURE XVIII: Predicted Impacts of Moving to "Opportunity Bargain" Areas with High Minority Shares in MTO Cities




*Notes:* This figure replicates Appendix Figure XVII, except adding one additional criterion that an "opportunity bargain" must satisfy: the fraction of residents in a tract who do not self-identify as non-Hispanic white alone must be at least 20%, as measured in the 2000 Decennial Census. See notes to Appendix Figure XVII for details.

ONLINE APPENDIX FIGURE XIX: Most Common Neighborhoods for MTO Participants vs. Opportunity Bargain Tracts in Chicago




*This map must be printed in color to be interpretable.*

*Notes:* This figure maps tracts in Chicago, plotting children's mean household income ranks given parents at the 1st percentile. We mark the most common neighborhoods where families in each of the three treatment arms of MTO lived on the map, using the list in Online Appendix Table 1c of Chetty, Hendren, and Katz (2016). We also mark selected "opportunity bargain" neighborhoods in Chicago, which are identified as described in the notes to Appendix Figure XVII.