The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects*

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Abstract

We show that the neighborhoods in which children grow up play a significant role in determining their earnings, college attendance rates, and fertility and marriage rates by studying more than 7 million families who move across commuting zones in the U.S. Exploiting variation in the age of children when families move, we find that neighborhoods have significant childhood exposure effects: the outcomes of children whose families move to a better neighborhood – as measured by the outcomes of children already living there – improve linearly in proportion to the time they spend growing up in that area, at a rate of approximately 4% per year of exposure. We distinguish the causal effects of neighborhoods from confounding factors by comparing the outcomes of siblings within families, studying moves triggered by displacement shocks, and exploiting sharp variation in predicted place effects across birth cohorts, genders, and quantiles to implement overidentification tests. The findings show that place affects intergenerational mobility primarily through childhood exposure, helping reconcile conflicting results in the prior literature.

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I Introduction

To what extent are children’s opportunities for economic mobility shaped by the neighborhoods in which they grow up? Despite extensive research, the answer to this question remains debated. Observational studies by sociologists have documented significant variation across neighborhoods in economic outcomes (e.g., Wilson 1987, Jencks and Mayer 1990, Massey 1993, Sampson et al. 2002, Sharkey and Faber 2014). However, experimental studies of families that move have traditionally found little evidence that neighborhoods affect economic outcomes (e.g., Katz et al. 2001, Oreopoulos 2003, Ludwig et al. 2013).

Using de-identified tax records covering the U.S. population, we present new quasi-experimental evidence on the effects of neighborhoods on intergenerational mobility that reconcile the conflicting findings of prior work and shed light on the mechanisms through which neighborhoods affect children’s outcomes. Our analysis consists of two papers. In this paper, we measure the degree to which the differences in intergenerational mobility across areas in observational data are driven by causal effects of place. In the second paper (Chetty and Hendren 2016), we build on the research design developed here to construct estimates of the causal effect of growing up in each county in the United States on children’s long-term outcomes and characterize the features of areas that produce good outcomes.

Our analysis is motivated by our previous work showing that children’s expected earnings conditional on their parents’ incomes vary substantially with the area (commuting zone or county) in which they grow up (Chetty, Hendren, Kline, and Saez 2014).\(^\text{1}\) This geographic variation in intergenerational mobility could be driven by two very different sources. One possibility is that neighborhoods have causal effects on economic mobility: that is, moving a given child to a different neighborhood would change his or her life outcomes. Another possibility is that the observed geographic variation is due to systematic differences in the types of people living in each area, such as differences in demographic makeup or wealth.

We assess the relative importance of these two explanations by asking whether children who move to areas with higher rates of upward income mobility among “permanent residents” have better outcomes themselves.\(^\text{2}\) Since moving is an endogenous choice, simple comparisons of the

\(^{1}\)We characterize neighborhood (or “place”) effects at two geographies: counties and commuting zones (CZs), which are aggregations of counties that are similar to metro areas but cover the entire U.S., including rural areas. Naturally, the variance of place effects across these broad geographies is a lower bound for the total variance of neighborhood effects, which would include additional local variation.

\(^{2}\)We define “permanent residents” as the parents who stay in the same commuting zone (or, in the county-level analysis, the same county) throughout the period we observe (1996-2012).
outcomes of children whose families move to different areas confound causal effects of place with selection effects (differences in unobservables). We address this identification problem by exploiting variation in the timing of children’s moves across areas. We compare the outcomes of children who moved to a better (or worse) area at different ages to identify the rate at which the outcomes of children who move converge to those of the permanent residents. The identification assumption underlying our research design is that the selection effects (children’s unobservables) associated with moving to a better vs. worse area do not vary with the age of the child when the family moves. This is a strong assumption, one that could plausibly be violated for several reasons. For instance, families who move to better areas when their children are young may be more educated or invest more in their children in other ways. We evaluate the validity of this identification assumption in detail and show that it holds in practice after presenting a set of baseline results.

In our baseline analysis, we focus on families with children born between 1980 and 1988 who moved once across commuting zones between 1997 and 2010, a sample that consists of 1.5 million movers. We find that on average, spending an extra year in a CZ or county where the mean income rank of children of permanent residents (for families at the same income level) is 1 percentile higher increases a child’s expected income rank by approximately 0.04 percentiles. That is, the outcomes of children who move converge to the outcomes of permanent residents of the destination area at a rate of approximately 4% per year of exposure. Symmetrically, moving to an area where permanent residents have worse outcomes reduces a child’s expected income by 4% per year. Children who move more than once – entering and leaving a given area within our sample – pick up gains that are proportional to the number of years in which they lived in that area.

These convergence patterns imply that neighborhoods have substantial childhood exposure effects: every additional year of childhood spent in a better environment improves a child’s long-term outcomes. Convergence is linear with respect to age: moving to a better area at age 8 instead of 9 is associated with the same improvement in earnings as moving to that area at age 15 instead of 16. The exposure effects persist until children are in their early twenties. Extrapolating over the duration of childhood, from age 0 to 20, the 4% annual convergence rate implies that children who move at birth to area with one unit better outcomes among permanent residents would pick up about 80% of that effect themselves. We find childhood exposure effects of a similar magnitude for several other outcomes, including rates of college attendance, teenage employment, teenage birth, 3

3Throughout the paper, we refer to areas where children have better outcomes in adulthood as “better” neighborhoods. We use this terminology without any normative connotation, as there are of course many other amenities of neighborhoods that may be relevant from a normative perspective.
and marriage. We also find similar exposure effects when families moves across counties.

As noted above, the identification assumption underlying the interpretation of the 4% convergence rate as a causal exposure effect is that the potential outcomes of children who move to better vs. worse areas do not vary with the age at which they move. We use three approaches to evaluate this assumption: controlling for observable factors, isolating plausibly exogenous moves triggered by aggregate displacement shocks, and implementing a set of outcome-based placebo tests. The first two approaches are familiar techniques in the treatment effects literature, while the third exploits the multi-dimensional nature of the treatments we study to implement overidentification tests.

To implement the first approach, we begin by controlling for factors that are fixed within the family (e.g., parent education) by including family fixed effects, as in Plotnick and Hoffman (1996) and Aaronson (1998). This approach identifies exposure effects from comparisons between siblings, effectively asking whether the difference in outcomes between two siblings in a family that moves is proportional to the size of the age gap between them. We estimate an annual exposure effect of approximately 4% per year with family fixed effects, very similar to our baseline estimates. The 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 that directly affects children in proportion to exposure time independent of neighborhoods. We cannot observe all such time-varying factors, but we do observe two particularly important characteristics of the family environment in each year: income and marital status. Controlling flexibly for changes in income and marital status interacted with the age of the child at the time of the move has no impact on the exposure effect estimates.

While the preceding results rule out confounds due to observable factors such as income, they do not address potential confounds due to unobservable factors. In particular, whatever event endogenously induced a family to move (e.g., a wealth shock) could also have had direct effects on their children’s outcomes. Our second approach addresses the problem of bias associated with endogenous choice by directly focusing on a subset of moves that are more likely to be driven by exogenous aggregate shocks. In particular, we identify moves that occur as part of large outflows from ZIP codes, typically caused by natural disasters or local plant closures. We replicate our baseline design within this subsample of displaced movers, comparing the outcomes of children who move to different destinations at different ages. We obtain similar exposure effect estimates for displaced households, mitigating concerns that our baseline estimates are biased by omitted
variables correlated with a household’s choice of when to move.\footnote{We also construct estimates that eliminate variation due to individuals’ choices of where to move by instrumenting for each household’s change in neighborhood quality using the average change in neighborhood quality of those who move out of the ZIP code during the years in our sample. These estimates are again very similar to our baseline results, further mitigating concerns about bias due to endogenous choice.}

Although the evidence from the first two approaches strongly supports the validity of the identification assumption, each of these approaches itself rests on assumptions – selection on observables and exogeneity of the displacement shocks – that could themselves potentially be violated. We therefore turn to a third approach – a set of placebo (overidentification) tests that exploit heterogeneity in place effects across subgroups – that in our view provides the most compelling method of assessing the validity of the research design. We begin by analyzing heterogeneity in place effects across birth cohorts. Although outcomes within CZs are highly persistent over time, some places improve and others decline. Exploiting this variation, we find using multivariable regressions that the outcomes of children who move to a new area converge to the outcomes of permanent residents of the destination in their own birth cohort but are unrelated to those of surrounding birth cohorts (conditional on their own birth cohort’s predictions). Such cohort-specific convergence is precisely what one would expect in the causal exposure effect model, but it would be unlikely to emerge from sorting or other omitted variables because the cohort-specific effects are only realized with a long time lag, after children grow up.

We implement analogous placebo tests by exploiting variation in the distribution of outcomes across areas. For instance, low-income children who spend their entire childhood in Boston and San Francisco have similar outcomes on average, but children in San Francisco are more likely to end up in the upper (top 10%) or lower tail (bottom 10%) of the income distribution. The causal exposure effects model predicts convergence not just at the mean but across the entire distribution; in contrast, it would be unlikely that omitted variables (such as changes in parent wealth) would happen to perfectly replicate the entire distribution of outcomes in each area in proportion to exposure time. In practice, we find quantile-specific distributional convergence: controlling for mean outcomes, children’s outcomes converge to predicted outcomes in the destination across the distribution in proportion to exposure time at a rate of approximately 4% per year.

Finally, we implement placebo tests exploiting heterogeneity in place effects across genders. Though place effects are highly correlated across genders, there are some places where boys do worse than girls (e.g., areas with highly concentrated poverty) and vice versa. When a family with both a daughter and a son moves to an area that is especially good for boys, their son’s outcomes
converge to those in the destination much more than their daughter’s outcomes. Once again, if our findings of neighborhood exposure effects were driven by sorting or omitted variables, one would not expect to find gender-specific convergence unless families are fully aware of the exact gender differences in outcomes across areas and sort to neighborhoods on these gender differences.

Putting together these results, we conclude that the baseline timing-of-move design yields consistent estimates of neighborhood exposure effects, of about 4% per year. An important caveat in interpreting this estimate is that it is a local average treatment effect estimated based on households who choose to move to certain areas. The mean exposure effect of moving a randomly selected household to a new area may differ, since households that choose to move to a given area may be more likely to benefit from that move than the average household in the population. The fact that exposure effects are similar within the subset of displaced households and are symmetric for moves to better and worse areas suggest that the endogeneity of choice does not have a substantial effect on the magnitude of exposure effects, but further work is needed to understand how exposure effects vary with households’ willingness to move.

Our findings yield three broad lessons. First, place matters for intergenerational mobility: the differences we see in outcomes across neighborhoods are largely due to the causal effect of places rather than differences in the characteristics of their residents. Second, place matters for intergenerational mobility largely through differences in childhood environment, rather than the differences in labor market conditions that have received attention in previous studies of place. Moving to a better area just before entering the labor market has little impact on individual’s outcomes, suggesting that place-conscious policies to promote upward mobility should focus primarily on improving the local childhood environment rather than conditions in adulthood. Third, we find that each year of childhood exposure matters roughly equally; there is no “critical age” after which the marginal returns to being in a better neighborhood fall sharply. This result is germane to recent policy discussions regarding early childhood interventions, as it suggests that improvements in neighborhood environments can be beneficial even in adolescence.

Our results help explain why previous experimental studies – most notably, the Moving to Opportunity (MTO) Experiment – failed to detect significant effects of moving to a better neighborhood on economic outcomes. Prior analyses of the MTO experiment focused primarily on the effects of neighborhoods on adults and older youth (e.g. Kling et al. (2007)), because of data limitations. In a followup paper (Chetty, Hendren, and Katz, 2016), we link the MTO data to tax records and show that the MTO data exhibit exposure effects similar to those identified here. In
particular, we find large improvements in earnings and other outcomes for children who moved to low-poverty neighborhoods at young ages but not those who moved at older ages.

More generally, our findings imply that much of the neighborhood-level variation in economic outcomes documented in observational studies does in fact reflect causal effects of place, but that these effects arise through accumulated childhood exposure rather than immediate impacts on adults. The idea that exposure time to better neighborhoods may matter has been noted since at least Wilson (1987) and Jencks and Mayer (1990), and has received growing attention in observational studies in sociology (Crowder and South (2011), Wodtke et al. (2011, 2012); Wodtke (2013), and Sampson 2012; Sharkey and Faber 2014). We contribute to this literature by presenting quasi-experimental estimates of exposure effects, which address the concerns about selection and omitted variable bias that arise in observational studies (e.g., Clampet-Lundquist and Massey (1993); Ludwig et al. (2008)). Although we find evidence of childhood exposure effects that are qualitatively consistent with the observational studies, we find no evidence of exposure effects in adulthood either in this study or our followup MTO study, contrary to the patterns observed in observational data (e.g., Clampet-Lundquist and Massey (1993)).

The paper is organized as follows. Section II describes the data. Section III presents our empirical framework, starting with a description of differences in intergenerational mobility across areas for permanent residents and then specifying our estimating equations. Section IV presents baseline estimates of neighborhood exposure effects on earnings and other life outcomes. Section V presents the three tests evaluating our identification assumption. Section VI discusses mechanisms, and Section VI concludes.

II Data

We use data from federal income tax records spanning 1996-2012. The data include both income tax returns (1040 forms) and third-party information returns (e.g., W-2 forms), which contain information on the earnings of those who do not file tax returns. Because our empirical analysis is designed to determine how much of the geographic variation in intergenerational mobility documented by Chetty et al. (2014) is due to causal effects of place, our analysis sample is essentially identical to the “extended sample” used in Chetty et al. (2014). Online Appendix A of Chetty et al. (2014) gives a detailed description of how we construct the analysis sample starting from the raw population data. Here, we briefly summarize the key variable and sample definitions, following
Section III of Chetty et al. (2014).\(^5\)

II.A Sample Definitions

Our base dataset of children consists of all individuals who (1) have a valid Social Security Number or Individual Taxpayer Identification Number, (2) were born between 1980-1988, and (3) are U.S. citizens as of 2013.\(^6\) We impose the citizenship requirement to exclude individuals who are likely to have immigrated to the U.S. as adults, for whom we cannot measure parent income. We cannot directly restrict the sample to individuals born in the U.S. because the database only records current citizenship status.

We identify the parents of a child as the first tax filers (between 1996-2012) who claim the child as a child dependent and were between the ages of 15 and 40 when the child was born. 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.

If parents never file a tax return, we do not link them to their child. Although some low-income individuals do not file tax returns in a given year, almost all parents file a tax return at some point between 1996 and 2012 to obtain a tax refund on their withheld taxes and the Earned Income Tax Credit (Cilke 1998). We are therefore able to identify parents for approximately 95% of the children in the 1980-1988 birth cohorts. The fraction of children linked to parents drops sharply prior to the 1980 birth cohort because our data begins in 1996 and many children begin to leave the household starting at age 17 (Chetty et al. (2014); Online Appendix Table I). This is why we limit our analysis to children born during or after 1980.

Our full analysis sample includes all children in the base dataset who are born in the 1980-88 birth cohorts, for whom we are able to identify parents, and whose mean parent income between 1996-2000 is strictly positive (which excludes 1.2% of children).\(^7\) We divide the full sample into two parts: permanent residents (or stayers) and movers. We define the permanent residents of each commuting zone (CZ) \(c\) as the subset of parents who reside in a single CZ \(c\) in all years of our

\(^5\)The tax records we use were drawn in the middle of 2013. They include a complete set of information returns (W-2’s) for 2012, but exclude a small number of amendments and late filings for 1040s. This slight incompleteness of the data is inconsequential, as using data through 2011 yields very similar results.

\(^6\)For selected outcomes that can be measured at earlier ages, such as teenage labor force participation rates, we extend the sample to include more recent birth cohorts, up to 1996.

\(^7\)We limit the sample to parents with positive income because parents who file a tax return (as required to link them to a child) yet have zero income are unlikely to be representative of individuals with zero income and those with negative income typically have large capital losses, which are a proxy for having significant wealth.
sample, 1996-2012. The movers sample consists of all individuals in the full sample who are not permanent residents.

In our baseline analysis, we focus on the subset of individuals who live in CZs with populations above 250,000 (based on the 2000 Census) to ensure that we have adequate precision to estimate place effects. We also restrict attention to movers who moved at least 100 miles to eliminate moves across CZ borders that do not reflect a true change of location. There are approximately 24.5 million children in the baseline sample, of whom 22.93 million are children of permanent resident and 1.55M million move at least 100 miles.

II.B Variable Definitions and Summary Statistics

In this section, we define the key variables we use in our analysis. We measure all monetary variables in 2012 dollars, adjusting for inflation using the headline consumer price index (CPI-U). We begin by defining the two key variables we measure for parents: income and location.

*Parent Income.* Our primary measure of parent income is total pre-tax income at the household level, which we label *parent family income*. In years where a parent files a tax return, we define family 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. In years where a parent does not file a tax return, we define family income as the sum of wage earnings (reported on form W-2), unemployment benefits (reported on form 1099-G), and gross social security and disability benefits (reported on form SSA-1099) for both parents. In years where parents have no tax return and no information returns, family income is coded as zero.

Our baseline income measure includes labor earnings and capital income as well as unemployment insurance, social security, and disability benefits. It excludes non-taxable cash transfers such as TANF and SSI, in-kind benefits such as food stamps, all refundable tax credits such as the EITC, non-taxable pension contributions (e.g., to 401(k)’s), and any earned income not reported

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8 We measure the distance of moves as the distance between the centroids of the origin and destination ZIPs. We show the robustness of our results to using alternative cutoffs for minimum population size and move distances in Appendix Table II.

9 In the county-level analysis reported in Online Appendix A, we define permanent residents as parents who stay in the same county in all years; in this analysis for our baseline specifications, there are 19.96 million permanent residents, 654.5K 1-time movers across CZs, and 617.5K 1-time movers across counties within CZs.

10 The database does not record W-2’s and other information returns prior to 1999, so non-filer’s income is coded as 0 prior to 1999. Assigning non-filing parents 0 income has little impact on our estimates because only 2.9% of parents in the full analysis sample do not file in each year prior to 1999 and most non-filers have very low W-2 income (Chetty et al. (2014)). For instance, in 2000, median W-2 income among non-filers was $29.

11 Importantly, these observations are true zeros rather than missing data. Because the database covers all tax records, we know that these individuals have 0 taxable income.
to the IRS. Income is always measured prior to the deduction of individual income taxes and employee-level payroll taxes.

In our baseline analysis, we average parents’ family income over the five years from 1996 to 2000 to obtain a proxy for parent lifetime income that is less affected by transitory fluctuations (Solon 1992). We use the earliest years in our sample to best reflect the economic resources of parents while the children in our sample are growing up.\textsuperscript{12} Because we measure parent income in a fixed set of years, the age of the child when parent income is measured varies across birth cohorts. We account for this variation by conditioning on the child’s birth cohort throughout our analysis.

\textit{Parent Location.} In each year, parents are assigned ZIP codes of residence based on the ZIP code from which they filed their tax return. If the parent does not file in a given year, we search information returns (such as W-2) for a ZIP code in that year. Non-filers with no information returns are assigned missing ZIP codes. For children whose parents were married when they were first claimed as dependents, we always track the mother’s location if marital status changes. We map parents’ ZIP codes to counties using a crosswalk that combines the union of a 1999 Census crosswalk and a 2011 Housing and Urban Development crosswalk (Census (1999); HUD (2011)).\textsuperscript{13} We then assign counties to commuting zones using the crosswalk constructed by David Dorn. See Online Appendix A of Chetty et al. (2014) for further details on the mapping of ZIP codes to CZs.

Next, we define the outcomes that we analyze for children.

\textit{Income.} We define child family income in exactly the same way as parent family income. We measure children’s annual incomes at ages ranging from 24-30 and define the child’s household based on his or her marital status at the point at which income is measured. For some robustness checks, we analyze individual income, defined as the sum of individual W-2 wage earnings, UI benefits, SSDI payments, and half of household self-employment income (see Online Appendix A of Chetty et al. (2014) for more details).

\textit{Employment.} We define an indicator for whether the child is employed at a given age based on whether he has a W-2 form filed on his behalf at that age. We measure employment rates starting

\textsuperscript{12}Formally, we define mean family income as the mother’s family income plus the father’s family income in each year from 1996 to 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 family income over the 5 year period. For parents who are married initially and then divorce, this measure tracks the mean family 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 family income (including the new spouse’s income) after marriage. These household measures of income increase with marriage and naturally do not account for cohabitation; to ensure that these features do not generate bias, we assess the robustness of our results to using individual measures of income.

\textsuperscript{13}The 1999 census crosswalk is no longer publicly posted at https://www.huduser.gov/portal/datasets/usps_crosswalk.html, but is available on our project website.
at age 16 to analyze teenage labor force participation.

**College Attendance.** We define college attendance as an indicator for having one or more 1098-T forms filed on one’s behalf when the individual is aged 18-23. Title IV institutions – all colleges and universities as well as vocational schools and other post-secondary institutions eligible for federal student aid – are required to file 1098-T forms that report tuition payments or scholarships received for every student. The 1098-T forms are filed directly by colleges independent of whether an individual files a tax return and are available from 1999-2012. Comparisons to other data sources indicate that 1098-T forms capture more than 95% of college enrollment in the U.S. (see Chetty et al. (2014), Appendix B).14

**Teenage Birth.** For women, we define an indicator for teenage birth if they are listed as a parent on a birth certificate when they are between the ages of 13 and 19, using data from the Social Security Administration’s DM-2 database.15

**Marriage.** We define an indicator for whether the child is married at a given age based on the marital status listed on 1040 forms for tax filers. We code non-filers as single because linked CPS-IRS data show that the vast majority of non-filers below the age of 62 are single (Cilke 1998).

**Summary Statistics.** Table I reports summary statistics for our analysis sample and various subgroups. In general, movers are slightly negatively selected on observables relative to permanent residents. For permanent residents, median parent family income is $52,800, as compared to $48,500 for our sample of one-time movers. Children of permanent residents have a median family income of $48,377 when they are 30 years old, compared with $47,882 for movers. Roughly 70% of children of permanent residents and one-time movers are enrolled in a college at some point between the ages of 18 and 23 and roughly 11% of daughters of permanent residents and one-time movers have a teenage birth.

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14Colleges are not required to file 1098-T forms for students whose qualified tuition and related expenses are waived or paid entirely with scholarships or grants. However, the forms are frequently available even for such cases because of automated reporting to the IRS by universities. Approximately 6% of 1098-T forms are missing from 2000-2003 because the database contains no 1098-T forms for some small colleges in these years (Chetty et al. (2014)). To verify that this does not affect our results, we confirm that our results are very similar when we exclude data from these years (not reported).

15The total count of births in the SSA DM-2 database closely matches vital statistics counts from the Center for Disease Control prior to 2008; however, the DM-2 database contains approximately 10% fewer births between 2008-2012. Using an alternative measure of teenage birth that does not suffer from this missing data problem – in which we define a woman as having a teen birth if she ever claims a dependent who was born while she was between the ages of 13 and 19 – yields very similar results (not reported). We do not use the dependent-claiming definition as our primary measure of teenage birth because it only covers children who are claimed as dependents by their mothers.
III  Empirical Framework

In this section, we first present a descriptive characterization of the earnings outcomes of children who grow up in different areas in the U.S. We then formally define our estimands of interest – childhood exposure effects – and describe the research design we use to identify these exposure effects in observational data.

III.A Geographical Variation in Outcomes of Permanent Residents

We conceptualize “neighborhood” effects as the sum of place effects at different geographies, ranging from broad to narrow: commuting zones, counties, ZIP codes, and blocks. In this paper, we focus on variation across commuting zones (CZs). CZs are aggregations of counties based on commuting patterns in the 1990 Census constructed by Tolbert and Sizer (1996). There are 741 CZs in the U.S.; on average, each CZ contains 4 counties and has a population of 380,000. We also replicate the results reported in the main text at the county level in Online Appendix . We focus on variation across relatively broad geographies to maximize statistical precision, as some of our research designs require large sample sizes to discern fine variation in place effects. Of course, the variation across CZs and counties we document is a lower bound for the total variance of neighborhood effects, which would include additional variation at narrower geographies.

We characterize children’s outcomes in each CZ using the same approach as in Chetty et al. (2014), except that we focus here on “permanent residents” – the subset of children whose families never move between 1996 and 2012 – to measure outcomes for children who spent their entire childhoods in a single area.16 Importantly, our definition of permanent residents conditions on parents’ locations, not children’s locations in adulthood. The CZ where a child grew up may differ from the CZ where he lives when we measure his earnings in adulthood.

Since places can have different effects across parent income levels and over time, we characterize children’s mean outcomes conditional on their parents’ income separately for each CZ \(c\) and birth cohort \(s\). Chetty et al. (2014) show that measuring incomes using percentile ranks (rather than dollar levels) has significant statistical advantages. Following their approach, we define child \(i\)’s

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16Because our data start in 1996, we cannot measure parents’ location over their children’s entire childhood. For the 1980 birth cohort, we measure parents’ location between the ages of 16 and 32; for the 1991 birth cohort, we measure parents’ location between 5 and 21. This creates measurement error in children’s childhood environment that is larger in earlier birth cohorts. Fortunately, we find that our results do not vary significantly across birth cohorts, and in particular remain similar for the most recent birth cohorts. The reason such measurement error turns out to be modest empirically is that most families who stay in a given area for several years tend not to have moved in the past either. For example, among families who stayed in the same CZ \(c\) when their children were between ages 16-24, 81.5% of them lived in the same CZ when their children were age 8.
percentile rank \( y_i \) based on his position in the national distribution of incomes relative to all others in his birth cohort. Similarly, we measure the percentile rank of the parents of child \( i \), \( p(i) \), based on their positions in the national distribution of parental income for child \( i \)’s birth cohort.

Let \( \bar{y}_{pcs} \) denote the mean rank of children with parents at percentile \( p \) of the income distribution in CZ \( c \) in birth cohort \( s \). Figure I illustrates how we estimate \( \bar{y}_{pcs} \) for children born in 1982 to parents who are permanent residents of the Chicago CZ. This figure plots the mean child rank at age 30 within each percentile bin of the parent income distribution, \( E[y_i|p(i) = p] \). The conditional expectation of a child’s rank given his parents’ rank is almost perfectly linear, a property that is robust across CZs (Chetty et al. (2014), Online Appendix Figure IV). Exploiting linearity, we parsimoniously summarize the relationship between children’s mean income ranks and their parents’ ranks by regressing children’s ranks on their parents’ ranks in each CZ \( c \) and birth cohort \( s \):

\[
y_i = \alpha_{cs} + \psi_{cs}p_i + \varepsilon_i. \tag{1}
\]

We then estimate \( \bar{y}_{pcs} \) using the fitted values from this regression:

\[
\bar{y}_{pcs} = \hat{\alpha}_{cs} + \hat{\psi}_{cs}p. \tag{2}
\]

For example, in Chicago, \( \bar{y}_{25,c,1985} = 40.8 \) for children growing up at the 25th percentile of the national income distribution and \( \bar{y}_{75,c,1985} = 56.1 \) for children growing up at the 75th percentile.

Figure II maps children’s mean income ranks at age 30 by CZ for children with parents at the 25th percentile (Panel A) and 75th percentile (Panel B). We construct these maps by dividing CZs into deciles based on their estimated value of \( \bar{y}_{25,c,s} \) and \( \bar{y}_{75,c,s} \); lighter colors represent deciles with higher mean outcomes. As documented by Chetty et al. (2014), children’s outcomes vary substantially across CZs, especially for children from low-income families. Chetty et al. (2014, Section V.C) summarize the spatial patterns in these maps in detail. Here, we focus on investigating whether the variation in these maps is driven by causal effects of place or heterogeneity in the types of people living in different places.

### III.B Definition of Exposure Effects

Our objective is to determine how much a given child’s potential outcomes would improve on average if he were to grow up in an area where the permanent residents’ outcomes are 1 percentile point higher. We answer this question by studying children who move across areas, focusing on identifying childhood exposure effects. We define the exposure effect at age \( m \) as the impact of spending year \( m \) of one’s childhood in an area where permanent residents’ outcomes are 1 percentile point higher.
Formally, consider a hypothetical experiment in which we randomly assign children to new neighborhoods \(d\) starting at age \(m\) for the rest of their childhood. The best linear predictor of children’s outcomes \(y_i\) in the experimental sample on the permanent residents’ outcomes in CZ \(d\) \((\bar{y}_{pds})\) can be written as

\[
y_i = \alpha + \beta_m \bar{y}_{pds} + \theta_i,
\]

where the error term \(\theta_i\) captures family inputs and other determinants of children’s outcomes. Since the random assignment guarantees that \(\theta_i\) is orthogonal to \(\bar{y}_{pds}\), estimating (3) using OLS yields a coefficient \(\beta_m\) that represents the mean impact of spending year \(m\) of one’s childhood onwards in an area where permanent residents have 1 percentile better outcomes. We define the exposure effect at age \(m\) as \(\gamma_m = \beta_m - \beta_{m+1}\).\(^{17}\) Note that if the earnings \(y_i\) is measured at age \(T\), \(\beta_m = 0\) for \(m > T\), as moving after the outcome is measured cannot have a causal effect on the outcome.

Estimating the exposure effects \(\gamma_m\) is of interest for several reasons. First, a positive effect (at any age) allows us to reject the null hypothesis that neighborhoods do not matter, a null of interest given experimental evidence to date. Second, \(\gamma_m\) informative about the ages at which neighborhood environments matter most for children’s outcomes. Third, the magnitude of \(\beta_0 = \sum_{t=0}^{T} \gamma_m\) – the impact of assigning children to better neighborhood from birth – provides an estimate of the degree to which the differences in children’s outcomes across areas are due to place effects vs. selection. If place effects are homogeneous within birth cohorts and parent income groups, \(\beta_0 = 0\) would imply that all of the variation across areas is due to selection, while \(\beta_0 = 1\) would imply that all of the variation would reflect causal effects of place. More generally, the magnitude of \(\beta_0\) tells us how much of the differences across areas in Figure II rub off on children who are randomly assigned to live there from birth.

Although identifying exposure effects sheds light on the importance of place effects on average, it does not identify the causal effect of any given area on a child’s potential outcomes. The causal effect of growing up in a given CZ \(c\) will generally differ from the mean predicted impact \(\beta_0 \bar{y}_{pds}\) based on permanent residents’ outcomes because the degree of selection and causal effects can vary across areas. We build on the methodology developed in this paper to estimate the causal effect of growing up in each CZ in the second paper in this series (Chetty and Hendren (2016)).

\(^{17}\)We assume that \(\beta_m\) does not vary across parent income percentiles \(p\) for simplicity, but one could estimate (3) separately by \(p\) to identify \(\beta_{mp}\) for each percentile \(p\). Empirically, we find that \(\beta_{mp}\) does not vary significantly across percentiles.
III.C Estimating Exposure Effects in Observational Data

We estimate exposure effects by studying families who move across CZs with children of different ages in observational data. In observational data, the error term $\theta_i$ in (3) will generally be correlated with $\bar{y}_{pds}$. For instance, parents who move to a good area may have latent ability or wealth that produces better child outcomes. Estimating (3) in an observational sample of families who move exactly once yields a regression coefficient

$$b_m = \beta_m + \delta_m,$$

where $\delta_m = \frac{\text{cov}(\theta_i, \bar{y}_{pds})}{\text{var}(\bar{y}_{pds})}$ is a standard selection effect that measures the extent to which parental inputs and other determinants of children’s outcomes for movers covary with permanent residents’ outcomes. Fortunately, the identification of exposure effects does not require that where people move is orthogonal to child’s potential outcomes. Instead, it requires that timing of moves to better areas is orthogonal to children’s potential outcomes, as formalized in the following assumption.

**Assumption 1.** Selection effects do not vary with the child’s age at move: $\delta_m = \delta$ for all $m$.

Assumption 1 allows for the possibility that the families who move to better areas may differ from those who move to worse areas, but requires that the extent of such selection does not vary with the age of the child when the parent moves. Under this assumption, we immediately obtain consistent estimates of exposure effects $\gamma_m = \beta_m - \beta_{m+1} = b_m - b_{m+1}$ because the selection effect $\delta$ cancels out when estimating the exposure effect. We can go further and estimate the selection effect $\delta$ itself by studying the outcomes of children whose families move after their income is measured, e.g. at age $a \geq 30$ if income is measured at age $T = 30$. Because moves at age $a > T$ cannot have a causal effect on children’s outcomes at age 30, $b_m = \delta$ for $m > T$ under Assumption 1. Using the estimated selection effect, we can identify the causal effect of moving to a better area at age $m$ as $\beta_m = b_m - b_{T+1}$ and thereby identify $\beta_0$, the total causal effect of growing up in area from birth.

Of course, Assumption 1 is a strong restriction that may not hold in practice. We therefore evaluate its validity in detail after presenting a set of baseline estimates in the next section.

IV Baseline Estimates of Childhood Exposure Effects

This section presents our baseline estimates of exposure effects. We begin with a set of semi-parametric estimates that condition on origin fixed effects and correspond most closely to the hypothetical experiment described in Section III.B. We then present estimates from parametric
models that show how movers’ outcomes can be parsimoniously modeled as a linear combination of the outcomes of permanent residents in origins and destination.

In our baseline analysis, we focus on children whose parents moved across CZs exactly once between 1996 and 2012 and are observed in the destination CZ for at least two years. We also restrict attention to families who moved at least 100 miles to exclude moves across CZ borders that do not reflect a true change of neighborhood and limit the sample to CZs with populations above 250,000 to mitigate measurement error in the estimates of permanent residents’ outcomes $y_{pds}$. We present estimates that include families who move more than once in Section 6 and show that the findings are robust to alternative cutoffs for population size and distance in Online Appendix Table II.

In prior work (Chetty et al. 2014), we found that the intergenerational correlation between parents’ and children’s incomes stabilizes when children turn 30, as college graduates experience steeper wage growth in their 20s (Haider and Solon 2006). Measuring income at age 30 limits us to estimating exposure effects only after age 15 given the time span of our dataset.18 Fortunately, measuring income at earlier ages (from 24-30) turns out not to affect the exposure effect estimates. The reason is that our estimates of $b_m$ correlate the incomes of children who move with the incomes of permanent residents in the destination measured at the same age. The incomes of permanent residents serve as goalposts that allow us to measure the degree of convergence in incomes at any age, even before we observe children’s permanent income. For example, if a given area $c$ sends many children to college and therefore generates relatively low incomes at age 24, we will obtain a higher estimate of $b_m$ if a child who moves to area $c$ has a low level of income at age 24. We therefore measure income at age 24 in our baseline specifications to estimate exposure effects for the broadest age range.19

IV.A Semi-Parametric Estimates

To begin, consider the set of children whose families moved when they were exactly $m$ years old. We analyze how these children’s earnings are related to those of the permanent residents in their destination CZ using the following linear regression:

$$y_i = \alpha_{qos} + b_m \Delta_{odps} + \varepsilon_{1i}, \quad (4)$$

18The most recent birth cohort for which we observe income at age 30 (in 2012) is the 1982 cohort; since our data begin in 1996, we cannot observe moves before age 15.

19We do not study income before age 24 because a large fraction of children are enrolled in college at earlier ages and because we find that exposure effects persist until age 23 when income is measured at any point between 24 and 30. We study college attendance as a separate outcome in Section VI.D.
where $y_i$ denotes the child’s household income rank at age 24, $\alpha_{qos}$ is a fixed effect for the origin CZ $o$ by parent income decile $q$ by birth cohort $s$ and $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$ is the difference in predicted income rank (at age 24) of permanent residents in the destination versus origin for the relevant parent income rank $p$ and birth cohort $s$. Equation (4) can be interpreted as an observational analog of the specification in (3) that we would ideally estimate in experimental data.\(^{20}\)

Figure III presents a non-parametric binned scatter plot corresponding to the regression in (4) for children who move at age $m = 13$. To construct the figure, we first demean both $y_i$ and $\Delta_{odps}$ within the parent decile ($q$) by origin ($o$) by birth cohort ($s$) cells in the sample of movers at age $m = 13$ to construct residuals: $y_i^r = y_i - E[y_i|q,o,s,m]$ and $\Delta_{odps}^r = \Delta_{odps} - E[\Delta_{odps}|q,o,s,m]$. We then divide the $\Delta_{odps}^r$ residuals into twenty equal-size groups (ventiles) and plot the mean value of $y_i^r$ vs. the mean value of $\Delta_{odps}^r$ in each bin.

Figure III shows that children who move to areas where children of permanent residents earn more at age 24 themselves earn more when they are 24. The relationship between $y_i$ and $\Delta_{odps}$ is linear. The regression coefficient of $b_{13} = 0.629$, estimated in the microdata using (4), implies that a 1 percentile increase in $\bar{y}_{pds}$ is associated with a 0.629 percentile increase in $y_i$ for the children who move at age 13.

Building on this approach, we estimate analogous regression coefficients $b_m$ for children whose parents move at each age $m$ from 9 to 30. We estimate $\{b_m\}$ using the following regression specification:

$$y_i = \alpha_{qosm} + \sum_{m=9}^{30} b_m I(m_i = m)\Delta_{odps} + \sum_{s=1980}^{1987} \kappa_s I(s_i = s)\Delta_{odps} + \varepsilon_{2i},$$

(5)

where $\alpha_{qosm}$ is an origin CZ by parent income decile by birth cohort by age at move fixed effect and $I(x_i = x)$ is an indicator function that is 1 when $x_i = x$ and 0 otherwise. This specification generalizes (4) by fully interacting the age at move $m$ with the independent variables in (4). In addition, we permit the effects of $\Delta_{odps}$ to vary across birth cohorts (captured by the $\kappa_s$ coefficients) because our ability to measure parent’s locations during childhood varies across birth cohorts. We observe children’s locations starting only at age 16 for the 1980 cohort, but starting at age 8 for the 1988 cohort. This leads to greater measurement error in $\Delta_{odps}$ for earlier birth cohorts, which

\(^{20}\)We use parent income deciles rather than percentiles to define the fixed effects $\alpha_{qos}$ to simplify computation; using finer bins to measure parent income groups has little effect on the estimates. Conditional on parent percentile, origin, and birth cohort, the variation in $\Delta_{odps}$ is entirely driven by variation in the destination outcomes ($\bar{y}_{pds}$). Hence, $b_m$ is identified from variation in $\bar{y}_{pds}$, as in (3), up to the approximation error from using parent deciles instead of exact percentiles.
could potentially confound our estimates of $b_m$ since the distribution of ages at move is unbalanced across cohorts. By including cohort interactions, we identify $\{b_m\}$ from within-cohort variation in ages at move.\footnote{To avoid collinearity, we omit the most recent birth cohort (1988 for income at age 24) interaction with $\Delta_{	ext{odps}}$. The inclusion of the cohort interactions has little impact on the estimates obtained from (5), as shown in Table II, Column (5), presumably because the fraction of the variance in $\Delta_{	ext{odps}}$ due to measurement error is small. The cohort interactions play a larger role in specifications that include family fixed effects, as the portion of the residual variance in $\Delta_{	ext{odps}}$ that is due to measurement error is larger in those specifications.}

Figure IVa plots estimates of $b_m$ from (4). The estimates exhibit two key patterns: selection effects after age 24 and exposure effects before age 24. First, the fact that $b_m > 0$ for $m > 24$ is direct evidence of selection effects ($\delta_m > 0$), as moves after age 24 cannot have a causal effect on earnings at 24. Families who move to better areas have children with better unobservable attributes. The degree of selection $\delta_m$ does not vary significantly with $m$ above age 24: regressing $b_m$ on $m$ for $m \geq 24$ yields a statistically insignificant slope of 0.001 (s.e. = 0.011). This result is consistent with Assumption 1, which requires that selection does not vary with the child’s age at move. The mean value of $\delta_m$ for $m \geq 24$ is $\delta = 0.126$, i.e. families who move to an area where permanent residents have 1 percentile better outcomes have 0.126 percentile better outcomes themselves purely due to selection effects. Assumption 1 allows us to extrapolate the selection effect of $\delta = 0.126$ back to earlier ages $m < 24$, as shown by the dashed line in Figure 1, and thereby identify causal exposure effects at earlier ages.

This leads to the second key pattern in Figure IVa, which is that the estimates of $b_m$ decline steadily with the age at move $m$ for $m < 24$. Under Assumption 1, this declining pattern constitutes evidence of an exposure effect, i.e. that moving to a better area earlier in childhood generates larger long-term gains. The linearity of the relationship between $b_m$ and the age at move $m$ in Figure IVa below age 23 implies that the exposure effect $\gamma_m = b_{m+1} - b_m$ is approximately constant with respect to age at move $m$. Regressing $\hat{b}_m$ on $m$ for $m < 24$, we estimate an average annual exposure effect of $\gamma = 0.044$ (s.e. = 0.0018). That is, the outcomes of children who move converge to the outcomes of permanent residents of the destination area at a rate of 4.4% per year of exposure until age 23.\footnote{Figure IVa is identified from variation in movers’ destinations holding their origin fixed. An alternative approach is to exploit variation in origins, holding destinations fixed. Online Appendix Figure III presents estimates of $b_m$ identified from variation in origins by replacing the origin ($\alpha_{qosm}$) fixed effects in (5) with destination ($\alpha_{qds}$) fixed effects. The resulting estimates yield a qualitative pattern that is the mirror image of those in Figure IVa: the later the family moves to the destination, the more the child’s outcomes match the permanent residents in the origin, up to age 23. The estimated exposure effect of 0.03 is smaller than the estimates above because we measure children’s origins with greater error than destinations, as our location data is left-censored. This is why we focus on variation in destinations in most of our specifications.}
Because some children do not move with their parents, the estimates of \( b_m \) in (5) should be interpreted as intent-to-treat (ITT) estimates, in the sense that they capture the causal effect of moving (plus the selection effect) for children whose parents moved at age \( m \). We can obtain treatment-on-the-treated (TOT) estimates for the children who move themselves by inflating the ITT estimates by the fraction of children who move at each age \( m \). In Online Appendix Figure IV, we show that the TOT estimate of the exposure effect is \( \gamma^{TOT} = 0.040 \). This estimate is very similar to our baseline estimate because virtually all children move with their parents below age 18 and roughly 60% of children move with their parents between ages 18-23. Because the treatment effects converge toward zero as the age at move approaches 23, inflating the coefficients by \( 1/0.6 \) at later ages has little impact on exposure effect estimates.

### IV.B Parametric Estimates

Equation (5) includes more than 200,000 fixed effects (\( \alpha_{qosm} \)), making it difficult to estimate in smaller samples and introduce additional controls such as family fixed effects. As a tractable alternative to controlling non-parametrically for parent income, origin, birth cohort, and age at move using fixed effects, we now estimate a more parsimonious model in which we control parametrically for two key factors captured by the \( \alpha_{qosm} \) fixed effects: (1) the quality of the origin location, which we model by interacting the predicted outcomes for permanent residents in the origin interacted with birth cohort fixed effects and (2) disruption costs of moving that may vary with the age at move and parent income, which we model using age at move fixed effects linearly interacted with parent income percentile \( p_i \). This leads to the following regression specification:

\[
y_i = \sum_{s=1980}^{1988} I(s_i = s)(\alpha_{s1}y_{pos}^{i1} + \alpha_{s2}y_{pos}^{i2} + \sum_{m=9}^{30} I(m_i = m)(\zeta_{m1}s_{m1} + \zeta_{m2}s_{m2}p_i) + \sum_{m=9}^{30} b_m I(m_i = m)\Delta_{odps} + \sum_{s=1980}^{1987} \kappa_{s}^{i3} I(s_i = s)\Delta_{odps} + \varepsilon_{3i}
\]

The first two terms of this specification control for origin quality and disruption effects. The third term represents the exposure effects of interest, and as in equation (5), the fourth consists of cohort interactions with \( \Delta_{odps} \) to control for differential measurement error across cohorts.

Figure IVb plots the coefficients \( \{b_m\} \) obtained from estimating (6). The coefficients are very similar to those obtained from the more flexible specification used to construct Figure IVa. Regressing the \( b_m \) coefficients on \( m \) for \( m \leq 23 \), we obtain an average annual exposure effect estimate of \( \gamma = 0.038 \) (s.e. 0.02). The exposure effect estimate is similar to that obtained from the fixed effects specification because controlling for the quality of the origin using the permanent residents’ outcomes is adequate to account for differences in origin quality. Put differently, movers’ outcomes
can be modeled as a weighted average of the outcomes of permanents residents in the origin and destination, with weights reflecting the amount of childhood spent in the two places.

When measuring income at age 24, we cannot determine whether \( b_m \) stabilizes after age 24 because moving after age 24 has no causal effect on earnings or because we measure income at that point. In Appendix Figure II, we replicate the analysis measuring income at ages 24, 26, 28, and 30. All four series display very similar patterns of exposure effects in the overlapping age ranges, showing that our estimates of \( b_m \) are insensitive to the age at which we measure children’s incomes in adulthood. In particular, all four series decline linearly at a rate of approximately \( \gamma = 0.04 \) until age 23 and are flat thereafter. These results imply that neighborhood exposure before age 23 is what matters for earnings in subsequent years.

The specification in (6) is one of many potential parametric specifications one could use to estimate exposure effects. In Table II, we show that variants of this specification that account for the two factors described above – controls for origin quality and disruption effects – all yield very similar estimates of \( \gamma \). In Column 1, we parameterize the exposure and selection effects in Figure IV linearly, replacing the non-parametric \( \sum_{m=9}^{30} b_m I(m_i = m) \Delta_{odps} \) term in (6) with two separate lines above and below ages 23:

\[
y_i = \sum_{s=1980}^{1988} I(s_i = s)(\alpha_s^1 + \alpha_s^2 \bar{y}_{pos}) + \sum_{m=9}^{30} I(m_i = m)(\zeta_m^1 + \zeta_m^2 \bar{p}_i) + \sum_{s=1980}^{1987} \kappa_s^d I(s_i = s) \Delta_{odps} (7)
+ I(m_i \leq 23)(b_0 + (23 - m)\gamma) \Delta_{odps} + I(m_i > 23)(\delta + (23 - m)\delta') \Delta_{odps} + \varepsilon_{3i},
\]

Estimating this specification directly in the microdata, we obtain an average annual exposure effect \( \gamma = 0.040 \) (s.e. 0.002).\(^{23}\)

The remaining columns of Table II estimate variants of (7). Columns 2 and 3 show that estimating \( \gamma \) using data only up to age 18 or 23 (i.e., excluding data at older ages) yields similar estimates of \( \gamma \). Restricting the sample to children claimed in the destination CZ (to ensure that the children moved with the parents) also yields similar estimates (Column 4). Column 5 shows that dropping the cohort interactions, \( \sum_{s=1980}^{1988} I(s_i = s)\alpha_s^2 \bar{y}_{pos} \) and \( \sum_{s=1980}^{1988} \kappa_s^d I(s_i = s) \Delta_{odps} \), in (7) has little effect on the results. Column 6 shows that identifying \( \gamma \) purely from variation in the quality of the destination – by controlling for the outcomes of permanent residents in the origin interacted with the age of the child at the time of the move, \( \sum_{m=9}^{30} b_m I(m_i = m) \bar{y}_{pos} \) – yields a very similar

\(^{23}\)This coefficient differs slightly from the coefficient of \( \gamma = 0.038 \) that we obtain when regressing the coefficients \( b_m \) on \( m \) in Figure IVb because estimating the regression in the microdata puts different weights on each age (as we have more data at older ages), while estimating the regression using the \( b_m \) coefficients puts equal weight on all ages.
estimate of $\gamma$. Finally, Column 7 shows we obtain similar estimates when using a child’s individual income rank as the outcome, $y_i$, as opposed to their family income rank.

In sum, the results in this section yields three important lessons. First, place matters: children who move at earlier ages to areas where prior residents have higher earnings earn more themselves as adults. Second, place matters via *childhood* exposure. There is no evidence that moving to an area where children earn more as adults just before entering the labor market – as one might expect if the variation across areas was caused by differences in labor market conditions or the mix of jobs – has a discontinuous benefit. Instead, every year of exposure to the better area during childhood contributes to higher earnings in adulthood. Third, each year of childhood exposure matters roughly equally. The returns to growing up in a better neighborhood remain substantial well beyond early childhood. All of these results are predicated on our assumption that selection effects do not vary with the child’s age at move. We turn to evaluating this critical assumption in the next section.

V Validation of Baseline Design

Assumption 1 could potentially be violated through differential sorting or omitted variables. In this section, we address these concerns using three methods. First, we control for observables and family fixed effects. Second, we identify exposure effects using displacement shocks. Third, we conduct a set of outcome-based placebo (overidentification) tests of the exposure effect model.

V.A Sibling Comparisons and Controls for Observables

Our first approach to account for potential differences across children who move at different ages is to control for observable factors. It is useful to partition $\theta_i$ into two components: a component $\bar{\theta}_i$ that reflects inputs that are fixed within families, such as parent genetics and education, and a residual component $\tilde{\theta}_i = \theta_i - \bar{\theta}_i$ that can vary over time within families, such as parents’ jobs, marital status, or children’s ability.

*Robustness to Sorting on Fixed Family Characteristics.* The most obvious potential violation of Assumption 1 is that families who invest more in their children (higher $\bar{\theta}_i$) move to better neighborhoods at earlier ages, which would bias our estimated exposure effect $\beta$ upward. A natural method of controlling for differences in fixed family factors $\bar{\theta}_i$ is to include family fixed effects when estimating (6). For example, consider a family that moves to a better area with two children,
who are ages $m_1$ and $m_2$ at the time of the move. The exposure effect $\beta$ is identified by the extent to which the difference in sibling’s outcomes, $y_1 - y_2$, covaries with their age gap interacted with the quality of the destination CZ, $(m_1 - m_2)\bar{y}_{pds}$. This sibling comparison nets out any variation due to fixed family inputs $\bar{\theta}_i$, as noted in prior work.

Robustness to Sorting on Time-Varying Family Characteristics. Table III and Figure Va present the results of adding family fixed effects to the baseline specification. Figure Va replicates Figure IVb with the addition of family fixed effects to equation (6). Table III, Column (4) adds family fixed effects to the linear specification in equation (7). We obtain a slope of 0.044, as shown in Column (4) of Table III. This is similar to the baseline estimate of 0.040, replicated in Column (1). Column (5) adds controls for the age of the child at the time of the move interacted with the predicted outcomes of permanent residents in the origin. This yields a similar slope of 0.043, which is also similar to the analogous slope of 0.041 without family fixed effects, as shown in Column (2). Throughout, we obtain virtually the same pattern of exposure effects as in Figure IV.

As illustrated in Figure Va, the one parameter that does change is the level of the selection effect, $\delta$. Once we include family fixed effects, the level of the selection effect (i.e., the level of $b_m$ after age 24) becomes statistically insignificant. This is precisely what one would expect if selection effects do not vary with children’s ages, as in Assumption 1. The introduction of family fixed effects reduces the level of the $b_m$ coefficients by accounting for selection, but does not affect the slope of the $b_m$ coefficients.

The research design in Figure Va accounts for bias due to fixed differences in family inputs $\bar{\theta}_i$, but it does not account for time-varying inputs $\bar{\theta}_i$. For example, moves to better areas may be triggered by events such as job promotions that directly affect children’s outcomes in proportion to their time of exposure to the destination. Such shocks could bias our estimate of $\beta$ upward even with family fixed effects.

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25To the extent to which siblings are in different cohorts, the exposure effect is also formally identified from variations the outcomes of permanent residents in differing cohorts. We explore these variations in more detail in Section V.C.

26Similarly, we obtain a slope of 0.043 when regressing the $b_m$ estimates on $m$, as shown in Figure Va.

27The intercept, $\delta$, is identified even with family fixed effects because $\bar{y}_{pds}$ varies across birth cohorts.
Income and marital status are both strong predictors of children’s outcomes in adulthood. Fortunately, we can directly control for these two time-varying factors in our data, as we observe parents’ incomes and marital status in every year from 1996-2012. Figure Vb replicates Figure Va, controlling for changes parent income and parent marital status (in addition to family fixed effects). We construct the parental income rank by cohort by year, and use this to construct the difference in the parental income rank in the year after the move relative to the year before the move. We include this measure of income change and a full set of its interaction with $23 - m$ and an indicator for $m > 23$. We also construct an indicator for the child’s mother’s marital status and construct four indicators for possible marital status changes (married → married, married → un-married, un-married → married, un-married → un-married). We then interact these four indicators with a full set of its interaction with $23 - m$ and an indicator for $m > 23$. Controlling for changes in parent income and marital status, in addition to family fixed effects, has little effect on the mean estimated exposure effect.\footnote{Column (7) of Table III also shows a specification that includes a full set of parental income rank controls for each year (1996-2012) fully interacted with cohort dummies (in addition to family fixed effects). Here again, we obtain a similar exposure slope of 0.043 (s.e. 0.008).}

\textit{Cohort Controls.} The baseline specification includes separate controls for each cohort for the predicted outcome of permanent residents in the destination and origin. While the addition of these controls do not significantly alter the baseline specification, they do have some effects on the family fixed effect specification that are important to note. In particular, the level of the intercept is slightly declining in cohort, which is consistent with the origin being more accurately measured for later cohorts. Hence, comparisons between children born in the 1986 and 1988 cohort will naturally have a smaller slope in the absence of cohort-varying intercepts, because the intercept is generally higher for the 1986 cohorts than the 1988 cohorts. For this reason, we include cohort-varying intercepts in our baseline specification. However, to highlight the robustness, Column (3) drops these cohort controls in the baseline specification and Column (6) adds family fixed effects. With the introduction of family fixed effects, the estimated slope coefficient drops from 0.036 to 0.031, consistent with attenuation from the negative correlation between the intercept and the cohort and the increased reliance on cohort comparisons within as opposed to across families. Hence, our baseline analysis includes these cohort controls to prevent such bias.

While changes in income and family structure are not a significant source of bias, other unobserved factors could still be correlated with moving to a better area. The fundamental identification problem is that any unobserved shocks that induce child $i$’s family to move to a better area could
be positively correlated with parental inputs $\theta_i$. These increased parental inputs could potentially increase the child’s earnings $y_i$ in proportion to the time spent in the new area even in the absence of neighborhood effects. For example, a wealth shock might lead a family to increase investments in the child in the years after the shock, which could improve $y_i$ in proportion to exposure time independent of neighborhood effects. In the next two subsections, we address concerns about bias due to such unobserved, time-varying factors using two different approaches.

V.B Displacement Shocks

Our first approach to accounting for unobservable shocks is to identify a subset of moves where we have some information about the shock that precipitated the move. To motivate our approach, suppose we identify a subset of families who were forced to move from an origin $o$ to a nearby destination $d$ because of an exogenous shock such as a natural disaster. We know that these families did not choose to move to a different neighborhood because of an unobservable shock. Hence, it is plausible that the level of parental inputs $\theta_i$ does not covary systematically with the quality of the destination $\bar{y}_{pds}$ differentially by child age, i.e. that Assumption 1 holds in such a subsample.

To operationalize this approach, we identify displacement shocks based on population outflows at the ZIP code level. Let $K_{zt}$ denote the number of families who leave ZIP code $z$ in year $t$ in our full sample and $\bar{K}_z$ mean outflows between 1996 and 2012. We define the shock to outflows in year $t$ in ZIP $z$ as $k_{zt} = K_{zt}/\bar{K}_z$. High outflow rates $k_{zt}$ are frequently driven by events such as natural disasters or local plant closures.

While many of the families who move in subsamples with large values of $k_{zt}$ do so for exogenous reasons, their destination $d$ is still the result of an endogenous choice that could lead to bias. For example, families who choose to move to better areas (higher $\bar{y}_{pds}$) when induced to move by an exogenous shock might also invest more in their children. To eliminate potential biases arising from endogenous choices of destinations, we isolate variation arising purely from the average change in neighborhood quality for individuals who are displaced. Let $E[\Delta_{odps}|q,z]$ denote the mean predicted outcome in the destinations to which individuals in origin zipcode $z$ and parent decile $q$ move. We instrument for the difference in predicted outcomes in each family’s destination relative to origin ($\Delta_{odps}$) with $E[\Delta_{odps}|q,z]$ and estimate (7) using 2SLS to obtain IV estimates of exposure effects, $\beta^{IV}$. Intuitively, $\beta^{IV}$ is identified by asking whether displacement shocks that happen to occur in areas where more families to areas with better average outcomes for children generates larger
improvements in outcomes for children

Figure VI presents the results of this analysis. To construct this figure, we take ZIP-year cells with above-average outflows \((k_{zt} > 1)\) and divide them into (population-weighted) deciles based on the size of the shock \(k_{zt}\). To ensure that large outflows are not simply driven by very small underlying populations, we exclude zipcode-by-year cells with less than 10 children leaving in the year.\(^{29}\) The first point in Figure VI shows the estimate of \(\beta\) using all observations with \(k_{zt} > 1\). The second point shows the estimate of \(\beta\) using all observations with \(k_{zt}\) at or above the 10th percentile. The remaining points are constructed in the same way, with the last point representing an estimate of \(\beta\) using data only from ZIP codes in the highest decile of outflow rates. The dotted lines show a 95% confidence interval for the regression coefficients.

If our baseline estimates were driven entirely by selection, one would expect the estimates of \(\beta\) to fall toward 0 as we restrict the sample to individuals who are more likely to have been induced to move because of an exogenous shock. But the coefficients remain quite stable: even in the top decile, where outflow rates are on average 34% higher than the annual mean for the ZIP code, \(\beta = 0.38\) (s.e. 0.13).

In sum, when we focus on families who move to a better (higher \(\bar{y}_{pds}\)) area for what are likely to be exogenous reasons, we find clear evidence that children who are younger at the time of the move earn more as adults. These findings indicate that our estimates of exposure effects capture the causal effects of neighborhoods rather than other unobserved factors that change when families move.

V.C Outcome-based Placebo (Over-Identification) Tests

While the preceding results are re-assuring about the validity of the baseline design, a priori some of the tests conducted so far are not “sharp” tests of the presence of selection bias. For example, in the family fixed effects design, one could imagine risk-averse households compensate a younger sibling with greater investment, \(\theta_i\), in the event they move to a worse place. In the displacement shocks analysis, one could imagine differential impacts of place for those who move in response to an exogenous shock as opposed to those whose moves occur in equilibrium. So, while we are re-assured that these tests suggest our analysis is not confounded by selection or omitted variable bias, we present a set of additional tests that can potentially be applied even in settings where the

\(^{29}\)The mean sample size within a parent decile-by-zipcode-by-year cell is 42 (median is 25). To ensure the results are not driven by a bias towards OLS due to the many instruments problem, we have replicated the analysis restricting to cells with at least 50 children and obtained similar results that are statistically indistinguishable from the results presented in Figure VI.
previous methods may not deliver consistent results.

The outcome-based placebo tests exploit plausible assumptions about the preferences and information set of parents choosing to move to different locations. Bias arises in our baseline estimates of equation (7) if parents that choose different levels of $\theta_i$ are choosing different levels of exposure to good places for their children.

Equation (7) implies that $\Delta_{odps}$ is a sufficient statistic for measuring the impacts of places on children’s outcomes. We let $\Delta^{placebo}_{odps}$ denote a “placebo” prediction if the difference between the true prediction and the placebo prediction, $\Delta^{placebo}_{odps} - \Delta_{odps}$, is either not known to the individual at the time parents choose neighborhoods or not a factor that enters into the parental decision to move to the place. As a result, when parents select good (or bad) places, as measured by $\Delta_{odps}$, they will on average select places that are good (or bad) as measured by $\Delta^{placebo}_{odps}$. Hence, adding $\Delta^{placebo}_{odps}$ to the regressions in equation (7) provides a test of omitted variable bias, providing a source of validation for the baseline design on the full sample of moves. We construct outcome-based placebos along three dimensions: birth cohorts, quantiles of the income distribution, and child gender.

**Birth Cohorts.** Place effects are generally quite stable across cohorts: the autocorrelation of $\bar{y}_{pcs}$ with $\bar{y}_{pc,s-1}$ is 0.95 at $p = 25$ and 0.92 at $p = 75$. Good places in one year are, on average, good places in the next year. However, outcomes in some areas (such as Oklahoma City) have improved over time, while others (such as Sacramento) have gotten worse. Since the causal effect of an area $c$ on a child $i$’s outcomes depends on the properties of the area in the years the child lives there, permanent residents’ outcomes $\bar{y}_{pc,s(i)}$ for a child’s own birth cohort $s(i)$ should be much stronger predictors of exposure effects than $\bar{y}_{pcs}$ for other cohorts. In contrast, while parents may know that some areas are better than others for improving their children’s outcomes, it is unlikely that they know whether a place is particularly good for their child’s own cohort relative to nearby cohorts, as these outcomes are realized 10-15 years after the move.

Formally, we assume that if unobservables $\theta_i$ are correlated with the current cohort place effect, they are also correlated with the place effects of neighboring cohorts:

$$\text{Cov}(\theta_i, m_{\Delta_{odp,s(i)}} | X) > 0 \Rightarrow \text{Cov}(\theta_i, m_{\Delta_{odps}} | X, m_{\Delta_{odp,s(i)}}) > 0$$

(8)

where $X$ corresponds to the additional control variables in equation (7). Under this assumption,

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30In Oklahoma City, $\bar{y}_{pcs}$ at $p = 25$ went from 43.0 for the 1980 cohort to 46.3 for the 1986 cohort. Conversely, in Sacramento $\bar{y}_{pcs}$ at $p = 25$ went from 46.6 to 42.5. College attendance rates followed a similar pattern. Compared to the national average increase in college attendance for $p = 25$ of 5.6pp between the 1981 and 1988 cohorts, Oklahoma city increased 8.4pp (50.1% in the 1981 cohort to 58.5% in the 1988 cohort) and Sacramento increased 2.5pp (52.8% to 55.3%).
mean outcomes for permanent residents in other birth cohorts ($s' \neq s(i)$) in the destination CZ can be used to test between selection and causal effects of neighborhoods. Let $t = s - s(i)$ index birth cohorts relative to a child’s true cohort $s(i)$. We implement such placebo tests by estimating linear exposure effect models of the following form:

$$y_i = \sum_{s=1980}^{1988} I(s_i = s)(\alpha_s^1 + \alpha_s^2 y_{pos}) + \sum_{s=1980}^{1987} \kappa_s^d I(s_i = s)\Delta_{odps} + \sum_{m=9}^{30} I(m_i = m)(\zeta_m^1 + \zeta_m^2 p_i) \quad (9)$$

This equation replicates our baseline model in (7), except that we include not just the difference in the predicted outcomes based on permanent residents in the destination relative to the origin, $\Delta_{odps(i)}$ for the child’s own cohort, but also the predictions for the four preceding and subsequent cohorts $\Delta_{odpt}$.

To illustrate the resulting patterns, the series in red triangles in Figure VII plots $\tilde{\gamma}_t$ when we estimate (9) including only the predicted outcome for a single cohort (i.e. omitting $t' \neq t$ in the second line of equation (9). In other words, we exchange $\Delta_{odps}$ with $\Delta_{odpt}$ in the baseline regressions. Here, the estimates of $\tilde{\gamma}_t$ are similar to our baseline estimate of $\gamma = 0.040$ for the leads and lags, which is to be expected given the high degree of serial correlation in place effects.

The series in blue circles in Figure VII plots the coefficients, $\tilde{\gamma}_t$, in equation (9) for $t = -4, ..., 4$. Here, we find small coefficients on the placebo exposure effects ($\tilde{\gamma}_t$ for $t \neq 0$). Moreover, the exposure effect estimate for the correct (own cohort, $t = 0$) coefficient drops only slightly relative to the baseline estimate of $\gamma = 0.040$ when we include predictions from surrounding cohorts.

Under (8), the results in Figure VII imply that our baseline estimates of $\beta$ are unbiased, i.e. that Assumption 1 holds. Intuitively, the fact that children’s outcomes do not correlate in an exposure-dependent manner with the predictions from other cohorts, conditional on the own-cohort prediction, implies that our estimates of $\gamma$ reflect causal neighborhood effects, which are cohort-specific, rather than omitted variables resulting from correlations of neighborhood choice and other parental inputs, which are not cohort-specific under (8). The logic of this test is analogous to an event study: provided that unobserved shocks $\theta_i$ do not happen to covary exactly with the destination place effect for the child’s own cohort and not surrounding cohorts, the coefficient at $t = 0$ in Figure VII identifies the causal effect of exposure to a better area.

Quantiles: Distributional Convergence. Places differ not only in children’s mean outcomes, but also in the distribution of children’s outcomes. For example, consider children who grow up in
Boston and San Francisco in families at the 25th percentile of the national income distribution. In both of these CZs, children’s mean percentile rank at age 24 is $\bar{y}_{25,c,1980} = 46$. However, children in San Francisco are more likely to end up in the upper or lower tail of the income distribution. The probability of reaching the top 10% is 7.3% in San Francisco vs. 5.9% in Boston; the corresponding probabilities for the bottom 10% are 15.5% and 11.7%.

If neighborhoods have causal exposure effects, we would expect convergence in mover’s outcomes not just at the mean but across the entire distribution in proportion to exposure time. It is less likely that omitted factors such as wealth shocks would perfectly replicate the distribution of outcomes of permanent residents in each CZ, for reasons analogous to those above. Indeed, families are unlikely to be able to forecast their child’s eventual quantile in the income distribution, making it difficult to sort precisely on quantile-specific neighborhood effects. Second, even with such knowledge, there is no strong reason to expect unobserved shocks such as changes in wealth to have differential and potentially non-monotonic effects across quantiles, in precise proportion to the outcomes in the destination.

To formalize this test, let $q_{pcs}$ denote the $q$th quantile of the income distribution of children’s of permanent residents in area $c$, and let $\Delta^q_{odps} = q_{pds} - q_{pos}$. If individuals do not know the precise quantile at which their children will fall in the income distribution 10-15 years after making their neighborhood choices, then it is natural to assume the following: if unobservables $\theta_i$ are correlated positively with outcomes at a given quantile $q$, they are also correlated with mean outcomes conditional on the quantile outcome:

$$\text{Cov}(\theta_i, m\Delta^q_{odp,s(i)}|X^q) > 0 \Rightarrow \text{Cov}(\theta_i, m\Delta_{odps}|X^q, m\Delta^q_{odp,s(i)}) > 0 \quad (11)$$

where $X^q$ are the control variables in equation (7) modified to control for the corresponding permanent resident outcomes $q$ instead of $y_i$.

For example, it seems natural to assume that if parents are sorting to places where children are likely to end up in the top 10% of the income distribution then they’re also sorting to places where, on average, children have higher incomes. Under the assumption in equation (11), the heterogeneity of exposure effects across the income distribution

\[ \sum_{s=1980}^{1988} I(s_i = s)(\alpha_s^1 + \alpha_s^2 q_{pos}) + \sum_{s=1980}^{1987} \kappa^d_s I(s_i = s)\Delta_{odps} \]

with

\[ \sum_{s=1980}^{1988} I(s_i = s)(\alpha_s^1 + \alpha_s^2 q_{pos}) + \sum_{s=1980}^{1987} \kappa^d_s I(s_i = s)(q_{pds} - q_{pos}) \]
can be used to test between selection and causal effects. We implement these tests by focusing on outcomes in tails: reaching the top 10% of the income distribution or the bottom 10% of the income distribution.

We begin by constructing predictions of the probability of having an income above the 90th percentile or below the 10th percentile of the national income distribution at age 24 for children of permanent residents in each CZ \( c \). We regress an indicator for being in the upper or lower 10% on parent ranks within each CZ using an equation analogous to (1) but that includes a quadratic term in parental income to account for nonlinearities at extreme quantiles identified in Chetty et al. (2014). We then calculate the predicted probability of being below the 10th percentile \( \pi_{10}^{pcs} \) and above the 90th percentile \( \pi_{90}^{pcs} \) using the fitted values from these regressions, as in (2).

Figure VIIIa presents a binned scatter plot of the probability a child is in the top 10\%, \( y_{i}^{90} \) vs. the destination prediction \( \pi_{pds}^{90} \) and the mean rank prediction \( \bar{y}_{pds} \) in the sample of children who move at or before age 13. The series in circles shows the non-parametric analog of a partial regression of a child’s outcome on \( \pi_{pds}^{90} \), controlling for the \( \bar{y}_{pds} \) and the analogous predicted outcomes based on prior residents in the origin, \( \pi_{pos}^{90} \) and \( \bar{y}_{pos} \). To construct this series, we regress both \( y_{i}^{90} \) and \( \pi_{pds}^{90} \) on the mean predicted income rank, \( \bar{y}_{pds} \), and the analogous origin controls, \( \pi_{pos}^{90} \) and \( \bar{y}_{pos} \), bin the \( \pi_{pcs}^{90} \) residuals into 20 equal-sized bins, and plot the mean residuals of \( y_{i}^{90} \) vs. the mean residuals of \( \pi_{pcs}^{90} \) within each bin. The series in triangles is constructed analogously, except that we plot residuals of \( y_{i}^{90} \) vs. residuals of \( \bar{y}_{pcs} \), the predicted mean rank.

Figure VIIIa shows that children who move before age 13 to areas where children are more likely to be in the top 10\% are much more likely to reach the upper tail themselves: a 1 percentile increase in \( \pi_{pcs}^{90} \) is associated with an 0.651 percentile increase in the movers’ probability of reaching the top 10\%, controlling for the mean rank outcomes of permanent residents in the origin and destination CZ along with the top 10\% prediction in the origin CZ. In contrast, conditional on the probability of reaching the top 10\%, variation in the mean predicted outcome has no impact at all on a child’s probability of reaching the top 10\% (slope of 0.030).

Figure VIIIb replicates Figure VIIIa using non-employment (roughly the bottom 10\%) as the outcome instead of reaching the top 10\%. Once again, we find that children’s probabilities of reaching the lower tail are strictly related to the predicted probability of reaching the lower tail based on permanent residents’ outcomes rather than the predicted mean outcome. The fact that mean predicted outcomes of permanent residents \( \bar{y}_{pcs} \) have no predictive power implies that other omitted factors, which are not quantile-specific under (11), do not drive our findings.
In Table IV, we estimate exposure effect models analogous to (7) using the distributional predictions instead of mean predictions. In Columns (1)-(3), the dependent variable is an indicator for having income in the top 10% of the income distribution. Column 1 replicates the baseline specification in equation (9), using \( \Delta_{90}^{odps} = \pi_{90}^{pos} - \pi_{90}^{pds} \) instead of the mean prediction \( \Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos} \).\(^{32}\) We obtain an exposure effect estimate of \( \gamma = 0.043 \) per year in this specification, similar to our baseline estimates. In Column 2, we use the mean prediction \( \Delta_{odps} \) instead. Here, we obtain an estimate of 0.022, which is to be expected given the high degree of correlation in place effects across quantiles: places that push children into the top 10% also tend to improve mean outcomes. In Column 3, we include both the quantile prediction \( \Delta_{90}^{odps} \) and the mean prediction \( \Delta_{odps} \), identifying the coefficients purely from differential variation across quantiles within CZs. Consistent with the findings in Figure VIII, we find that the coefficient on the quantile prediction remains unchanged at approximately 0.04, while the coefficient on the mean prediction is not significantly different from 0.

Columns 4-6 of Table IV replicate Columns 1-3, using an indicator for being unemployed (defined as an indicator for not having a W-2) as the dependent variable and using the prediction for being unemployed, \( \Delta_{odps}^{U} \) instead of \( \Delta_{odps}^{90} \) as the key independent variable. We find very similar patterns: children’s probabilities of being in the lower tail of the income distribution are strongly predicted by the quantile-specific prediction rather than the mean prediction. In sum, we find evidence of distributional convergence: controlling for mean outcomes, children’s outcomes converge to predicted outcomes in the destination across the distribution in proportion to exposure time, at a rate of approximately 4% per year.\(^{33}\) Under the assumption in equation (11), these results imply that our exposure effect estimates are driven by causal effects of neighborhoods rather than other unobserved factors. Intuitively, it would be quite unlikely that omitted variables (such as changes in parent wealth) would happen to perfectly replicate the entire distribution of outcomes in each area.

**Gender.** Finally, we conduct an analogous set of placebo tests using heterogeneity in place effects by child gender. To implement these tests, we first construct gender-specific predictions of the mean outcomes of children of permanent residents. We estimate the relationship between child and parent ranks within each CZ using (1) separately for boys and girls. We then define \( \bar{y}_{pcs}^{g} \) as the

\(^{32}\) Analogous to the baseline specification, we include cohort dummy interactions with \( \pi_{90}^{pds} \) and \( \pi_{90}^{pos} \).

\(^{33}\) There is no reason that the rate of convergence should be identical across all quantiles of the income distribution because the prediction for permanent residents at each quantile \( \pi_{pc}^{90} \) could reflect a different combination of causal effects and sorting. The key test is whether the prediction for the relevant quantile has more predictive power than predictions at the mean or other quantiles.
mean predicted outcome for permanent residents of CZ c in birth cohort s and gender \( g \in \{m, f\} \), as in (2).

Places that are better for boys and generally better for girls as well: the (population-weighted) correlation of \( \bar{y}_{pcs}^m \) and \( \bar{y}_{pcs}^f \) across CZs is 0.9 at \( p = 0.50 \). However, there is some variation. Online Appendix Figure V presents a heat map of \( \bar{y}_{pcs}^m - \bar{y}_{pcs}^f \) that highlights where differences in outcomes are largest across genders. For example, the difference in outcomes between males versus females is high in Syracuse and Albany, NY (i.e. comparatively good for males versus females), and low in Milwaukee, WI (i.e. comparatively good for females relative to males).

Figure IX presents a binned scatter plot of children’s ranks vs. the difference in the destination and origin prediction, \( \Delta_{odps}^g \), for their own gender (circles) and the prediction \( \Delta_{odps}^{-g} \) for the other gender (triangles) in the sample of children who move at or before age 13. Each series shows the non-parametric analog of a partial regression of a child’s outcome on the prediction for a given gender, controlling for the other-gender prediction. To construct the series in circles, we regress both \( y_i \) and \( \Delta_{odps}^g \) on \( \Delta_{odps}^{-g} \) and origin by parent income decile by cohort by gender fixed effects. We then bin the \( \Delta_{odps}^g \) residuals into 20 equal-sized bins, and plot the mean residuals of \( y_i \) vs. the mean residuals of \( \Delta_{odps}^g \) within each bin. The series in triangles is constructed analogously, except that we plot residuals of \( y_i \) vs. residuals of \( \Delta_{odps}^{-g} \), the prediction for the other gender. Figure IX shows that children who move before age 13 to areas where children of their own gender have better outcomes do much better themselves: a 1 percentile increase in the mean rank \( \bar{y}_{pcs}^g \) for \( g = g(i) \) is associated with a 0.523 percentile increase in the movers’ mean rank. In contrast, conditional on the own-gender prediction, variation in the prediction for the other gender is associated with only a 0.144 percentile increase in the movers’ mean rank.

In Table V, we estimate exposure effect models analogous to (7) with separate predictions by gender. Column 1 replicates the baseline specification in (7), using the gender-specific prediction \( \Delta_{odps}^g \) instead of the prediction that pools both genders. We continue to obtain an exposure effect estimate of \( \gamma = 0.038 \) per year in this specification, consistent with our baseline results.\(^{34}\) In Column 2, we use the prediction for the other gender \( \Delta_{odps}^{-g} \) instead. Here, we obtain an estimate of 0.034, which is to be expected given the high degree of correlation in place effects across genders. In Column 3, we include predictions for both genders, identifying the coefficients purely from differential variation across genders within CZs. Consistent with the findings in Figure IX, we find

\(^{34}\)In Online Appendix Table II, we show that the exposure effect estimates are 0.039 and 0.04 for boys and girls using predicted outcomes that do not vary across genders.
that the coefficient on the own gender prediction is larger than the other-gender prediction.\footnote{It is not surprising that the other gender prediction remains positive, as the prediction for the other gender may be informative about a place's effect for children of a given gender due to measurement error. In general, finding a 0 effect on the “placebo” prediction is sufficient but not necessary to conclude that there is no sorting under an assumption analogous to \eqref{eq:sort}.}

In principle, it could be the case that parents know that a given place is better for one particular gender relative to the other. Therefore, it is also illustrative to combine this test with family fixed effects. Columns 4-6 of Table V replicate Columns 1-3, including family fixed effects so that the estimates are identified purely from sibling comparisons. Column 7 replicates Column 6, restricting the sample to families that have at least one boy and one girl. The own-gender prediction remains a much stronger predictor of children’s outcomes than the other-gender prediction when we compare siblings’ outcomes within families.

The differences between the own-gender and other-gender predictions support the view that the impacts of moving on children’s outcomes reflects the causal effects of place rather than other omitted factors $\theta_i$. In order for the patterns in Figure IX and Table V to be explained by other factors, families with higher inputs $\theta_i$ in child $i$ would have to sort to areas where children of child $i$’s gender do especially well. Such sorting may certainly be feasible to some extent; for instance, families who invest a lot in boys might seek to avoid highly segregated areas. However, such sorting would be much more difficult for families with children of two different genders, as it would require finding a neighborhood where the differences in outcomes of children of permanent residents across genders matches the difference in inputs $\theta_i$ across children within the family, in proportion to the age gap between the children. The fact that we find very similar results when we identify from sibling comparisons within families with a boy and a girl thus suggests that sorting is unlikely to be driving the heterogeneous impacts by gender.\footnote{The gender test is less definitive than the cohort and distributional convergence tests because gender-specific variation is easier to observe at the point of the move than cohort- or quantile-specific differences. However, the fact that the coefficients on the own- and other-gender predictions differ quite substantially suggest that gender-specific sorting to neighborhoods would have to be quite substantial to explain the findings.}

Together, these placebo tests show that our baseline design which simply compares families that move with children at different ages turns out to yield consistent estimates of exposure effects. We believe that selection and omitted variable effects do not confound the raw OLS estimates significantly for two reasons. First, the degree of age-dependent sorting across large geographies such as CZs and counties may be limited, as families seeking better schools or environments for their children at certain ages presumably move more locally. Second, children’s outcomes conditional on parent income are not significantly correlated with mean parent incomes in an area (Chetty et al.)
As a result, moving to a better area for children is actually not systematically associated with parents finding better jobs, mitigating what might be the most important confounding factor.

**Summary.** The results in this section show that any omitted variable correlated with the other factors affecting children’s outcomes, \( \theta_i \), that generates bias in our exposure effect estimates must: (1) operate within the family in proportion to exposure time (family fixed effects); (2) be orthogonal to changes in parental income and marital status (controls for observables); (3) be correlated with the onset of large outflow shocks, such as Hurricane Katrina, in a way that is correlated the mean outcomes of where people go from the displaced areas (displacement shocks regressions); and (4) replicate the permanent residents’ outcomes by birth cohort, quantile, and gender in proportion to exposure time and conditional on placebo measures of these outcomes (outcome-based placebo tests). We believe that most plausible omitted variables are unlikely to have all of these properties and therefore conclude that places have causal effects on children in proportion to the amount of time they spend growing up in the area.

**VI  Mechanisms and Supplementary Evidence**

**VI.A  Labor Market Access versus Exposure Effects**

We interpret the slope, \( \gamma \), to reflect the effect of exposure time to neighborhoods while growing up. This contrasts with other ways in which neighborhoods could matter for children’s adult outcomes, such as the quality of the local labor market. Two key aspects of our results are supportive of this interpretation. First, our results suggest moving to a place at age 23 has no effect, despite increasing the odds that a child resides in the destination labor market as an adult. Second, our results suggest that if one takes two children who both reside in a given CZ, but one spent a longer time growing up in a better place, then that child will have higher incomes. To see this, Column (1) of Table VI adds fixed effects for the CZ of the child at the time their income, \( y_i \), is measured to our baseline specification in Column (1) of Table II. This yields a coefficient to 0.031 (s.e. 0.002). Of course, the CZ in which the child resides as an adult is an endogenous outcome; but the fact that the exposure pattern remains affirms the idea that childhood exposure is driving the results.

**VI.B  Multiple Movers: Critical Age Effects vs. Exposure Effects**

The exposure time interpretation of our results is further supported by looking at the experience of multiple movers. The baseline specification in (7) provides a natural method for incorporating them into the analysis. Given a child with origin \( o \), let \( d_j \) denote the \( j \)th destination location. We
construct $\Delta_{odps}^j = \Delta_{od, ps} = \bar{y}_{pds} - \bar{y}_{pos}$ as the difference in the child’s predicted outcome based on prior residents in destination $j$ and the origin. We then multiply each $\Delta_j$ by the years of exposure below age 23 the child has in destination $j$.

Columns (1)-(3) of Table VI presents estimates of the coefficients from a single regression that includes coefficients on the first, second, and third moves in the specification that generalizes equation (7) to incorporate exposure-time coefficients on each $\Delta_j$ for $j = 1, 2, 3$. We generalize the controls by including separate interactions for the outcomes of permanent residents in each of the 3 destinations, and we include controls for the number of years of under-23 exposure to the first, second, and third place, along with their interaction with parental income rank.

Overall, we find very similar estimates using multiple movers to the 0.040 baseline estimate in column (1). We estimate a slope of 0.040 on the first destination, 0.037 for the second destination, and 0.031 on the third destination. Constraining the coefficients to be equal yields a coefficient of 0.039, again very similar to the baseline estimated slope of 0.040.

These results further support an exposure time interpretation over a theory of labor market access or age-specific effects. Children who leave before reaching adulthood still have outcomes correlated with their permanent resident counterparts in proportion to the time they spend growing up in the place. Along with the specification in Column (1) controlling for the child’s location in adulthood, this again suggests that the effect is driven by where one grows up, as opposed to providing access to a particular labor market.

The multiple moves specification also suggests the pattern is not driven by heterogeneous critical age effects. In the simple 1-time movers specification, the destination could be more important for a 10 year old moving than a 15 year old moving for two reasons: (a) places matter in proportion to exposure time or (b) there is something about moving at age 10 to a good destination as opposed to age 15. Put differently, it could just be that experiences at age 10 are more important for determining earnings than experiences at age 15. However, the fact that we obtain similar results when pooling the analysis in the exposure time model suggests that living in a destination from age 10-12 has roughly the same impact as living there from age 13-15. This suggests places matter because of exposure time, not because of age-specific effects that are more important at younger ages. Every year spent in a better neighborhood tends to improve the child’s outcomes in adulthood.

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37 As shown in Table I, roughly 3% of the sample has more than 3 moves.
VI.C Subgroup Heterogeneity

We also explore heterogeneity of effects across other sub-samples in Online Appendix Table III. Broadly, we find similar convergence of movers’ outcomes to permanent residents’ outcomes when looking at moves to better versus worse places, children with above- and below-median income parents, and children of each gender.

For example, in Columns 4 and 5 we evaluate whether moves to areas with better or worse predicted outcomes relative to the origin neighborhood have different effects. Models of learning predict that moving to a better area will improve outcomes but moving to a worse area will not. In practice, we find little evidence of such an asymmetry: if anything, the point estimate of exposure effects for negative moves is larger. This result suggests that what matters for children’s mean long-term outcomes is continuous exposure to a better environment.

Of course, our estimates are identified using the equilibrium set of moves. This raises potential questions about the external validity of our results beyond this equilibrium set of moves. Here, the stability of the coefficients across subgroups is suggestive of stable and common effects of places on children’s outcomes.

VI.D Exposure Effect Estimates for Other Outcomes

The analysis to this point illustrates the exposure effects of places on children’s incomes. Here, we illustrate that this convergence occurs when measuring other outcomes. Figure X presents the baseline estimates for college attendance and marriage. For Panel A, we replicate the baseline specification in equation 6 replacing $\Delta_{odps}$ with $\Delta_{odps}^c = c_{pds} - c_{pos}$, where $c_{pcs}$ is the fraction of children at parental income rank $p$ who go to college. Here, we find a significant slope of 0.037 (0.003). While the graph is increasing as one considers moves at earlier ages, there is some evidence of a flattening slope below age 13. This suggests that, if anything, exposure to areas as a teenager are more important for college attendance than exposure in middle school years.

In Panel B, we replicate the baseline equation 6 replacing $\Delta_{odps}$ with $\Delta_{odps}^{mar} = mar_{pds} - mar_{pos}$, where $mar_{pcs}$ is the fraction of children at parental income rank $p$ who are married at age $y$. Panel B presents the results for both age 24 and age 26. We find a significant slope of 0.025 (0.02), which suggests places have causal effects on marriage in proportion to childhood exposure to the area.

Figure XI explores events that occur earlier in a child’s life, exploring the role of place in affecting outcomes during the teenage years. Panels (a)-(c) consider an indicator for teen employment at ages 16-18 (based on the existence of a form W-2). Here, we find fairly discontinuous pattern:
children that move at age 14 or 15 to a destination where more 16 year olds work are much more likely to work when age 16 than children that move at age 17. In contrast, children whose parents move when their kids are older than 16 years old are not more likely to work. This suggests places have causal effects on the likelihood that children work in formal employment at young ages. The effects are sharp and not proportional to exposure time. Yet at the same time they potentially provide insights into the nature of the exposure effect of childhood. The discontinuous pattern is consistent with a model that the “exposure effect” for earnings is the aggregation of the effects from a set of discrete experiences during childhood, such as having a summer job. The fact that the intercept reaches approximately 0.8 at young ages suggests that roughly 80% of the variation in teenage labor force participation rates permanent residents across commuting zones reflects the causal effects those places.

Panel D in Figure XI considers teen birth, defined as being the parent listed on a birth certificate prior to age 20. We construct gender-specific predictions based on prior residents in each birth cohort and plot the estimated coefficients $b_m$ from the baseline specification in Equation (6) replacing $\Delta_{odps}$ with $\Delta^{tb}_{odps} = r_{pds} - r_{pos}$, where $r_{pcs}$ is the fraction of permanent residents in CZ $c$ with parental income $p$ in cohort $s$ who have a child. We find significant exposure patterns for teen birth for both and girls. The pattern is linear below age 20 for males. For females, we find a linear exposure pattern prior to age 18, with some evidence of a sharp drop at age 18, consistent with exposure at ages 17-18 being a fairly critical time for teen birth outcomes for females.

VII Conclusion

Where children grow up affects their outcomes in adulthood in proportion to the time they spend in the place. The idea that exposure time to neighborhoods plays an important role has been recognized since at least Wilson (1987) and Jencks and Mayer (1990). Our results highlight that it is exposure during childhood that appears to matter most, up to the early twenties – and that at least 50% of the variation in intergenerational mobility across the U.S. reflects the causal effects of childhood exposure.

The importance of accounting for differences in exposure during childhood when analyzing neighborhood effects has received growing attention in the sociology literature (Sharkey and Faber 2014, Crowder and South (2011), and Wodtke et al. (2011, 2012); Wodtke (2013)). And, this exposure time perspective helps to reconcile a large observational literature documenting wide variation in outcomes across areas with an experimental literature that generally finds little effects
of neighborhoods on economic outcomes. At first glance, our results might appear to be inconsistent with experimental evidence on the impacts of neighborhoods on economic outcomes. Most notably, the Moving to Opportunity (MTO) housing voucher experiment documents little in the way of economic impacts on adults and older youth (e.g. Kling et al. (2007)). However, if neighborhoods have causal effects in proportion to the exposure time to the neighborhood, then the subset of children that would benefit most from moving out of high poverty areas would be those who were youngest at the time of the experiment, precisely the subset of participants whose long-term outcomes have not, until recently, been available for analysis.

In a follow-up paper (Chetty, Hendren, and Katz (2015)), we link the MTO data to tax data and show that the MTO data exhibit the same exposure time patterns as those we document here. Children whose families received an experimental housing voucher and moved to a low-poverty neighborhood at young ages (e.g., below age 13) earn 30% more in their mid 20’s than the control group. Children who moved at older ages do not show such gains, consistent with exposure time being a key determinant of neighborhood effects.

Relative to MTO, the advantage of the present paper is its ability to estimate neighborhood effects on a national scale. In Part 2, we use the exposure effects design to estimate the causal effect of spending an additional year growing up in each county in the U.S. We characterize the properties of areas with positive causal effects, but importantly our correlational analysis does not provide direct evidence on the factors that cause places to produce better outcomes for children. To facilitate further investigation of these issues, we have made all of the county- and CZ-level estimates of causal and sorting effects available on the project website. We provide the estimates by gender for individual and family income discussed above and also provide estimates for other outcomes and subgroups not explored in detail here, such as college attendance and marriage and estimate for children in single vs. two-parent households. We hope these data facilitate future work exploring the mechanisms through which neighborhoods have causal effects on intergenerational mobility.
References


Online Appendix A. County-Level Estimates

Appendix Table III replicates our baseline specifications using county-level moves instead of CZ moves. Broadly, the patterns at the county level reflect those at the CZ level, but with slightly attenuated values for γ, consistent with the presence of greater residential sorting at the county level. Appendix Figure VI presents estimates of the permanent resident outcomes across counties for \( p = 25 \) and \( p = 75 \).

Appendix Table IV presents the summary statistics of our county-level movers samples. We construct \( y_{pcs} \) using county-level permanent residents and we consider two samples of 1-time county movers. First, we consider a sample of 1-time movers who move at least 100 miles between counties with populations above 250,000, analogous to the same sample restrictions we impose on the 1-time CZ movers. Column (1) shows we obtain a baseline slope of 0.035, slightly lower than our baseline slope of 0.040 at the CZ level. The smaller slope is consistent with a slightly larger degree of residential sorting at the county, as opposed to the CZ level. Column (2) adds family fixed effects to the baseline specification in Column (1) and obtains an exposure slope of 0.033 (0.011), not significantly different from the baseline slope of 0.035. This suggests the quasi-experimental design is not confounded by dynamic sorting patterns operating at the county level within the CZ.

Within CZ Moves. While our baseline analysis for CZ moves and for the county moves in Columns (1)-(2) focused on moves above 100 miles, Columns (3)-(7) in Table VI explore moves across counties within CZs including moves less than 100 miles. Column (3) replicates the baseline specification using moves across counties with populations at least 250,000, measuring outcomes of the children at age 24. Here, we obtain a slope of 0.022 (s.e. 0.003), significantly lower than the estimate of 0.035 we obtain for longer distance moves. This drop is consistent with what one would expect if the child’s environment was not completely altered as a result of these shorter moves.

Column (4) measures the child’s outcome (and predicted outcomes of permanent residents) at age 26 instead of age 24. Here, we obtain a similar but slightly higher slope of 0.032. Column (5) stacks the data across outcomes at age 24-32. Here, we obtain a more precisely estimated coefficient of 0.027 (s.e. 0.002). Column (6) adds family fixed effects to the specification in Column (5) and obtains a similar slope of 0.029 (s.e. 0.025). While our estimate remains stable, it is considerably more imprecise with the addition of family fixed effects across counties within CZs. Finally, Column (7) considers within-CZ moves across counties with populations of at least 10,000 as opposed to 250,000. Here, we obtain a similar but slightly attenuated coefficient of 0.024 relative to the 0.027 in column (5). This is consistent consistent with attenuation bias from using relatively imprecise
estimates of the permanent resident outcomes in smaller places.
FIGURE I: Mean Child Income Rank at Age 30 Vs. Parent Income Rank for Children Raised in Chicago

Notes: This figure presents a non-parametric binned scatter plots of the relationship between mean child income ranks and parent income ranks for all children raised in Chicago. Figure measures income of the children at age 30 using the 1982 cohort. Child income is family income at age 30, and parent income is mean family income from 1996-2000. We define a child’s rank as her family income percentile rank relative to other children in her birth cohort and his parents’ rank as their family income percentile rank relative to other parents of children in the core sample. The ranks are constructed for the full geographic sample, but the graph illustrates the relationship for the sub-sample of families who report living in Chicago for all years of our sample, 1996-2012. The figure then plots the mean child percentile rank at age 30 within each parental percentile rank bin. The slope and best-fit lines is estimated using an OLS regression on the micro data. Standard errors are reported in parentheses.
FIGURE II: Predicted Income Rank at Age 30 - Permanent Residents

A. For Children with Parent at the 25th Percentile

B. For Children with Parent at the 75th Percentile

Notes: These figures illustrate the geographic variation in child income rank outcomes at age 30 from the 1980-82 cohorts amongst our sample of permanent residents across commuting zones (CZs) in the U.S. Panel A reports the expected rank for children whose parental income is at the 25th percentile of the income distribution of parents, and Panel B reports the expected rank for children whose parental income is at the 75th percentile. Both figures use the baseline family income definitions for parents and children. The figure restricts to the subset of parents who stay in the commuting zone throughout our sample period (1996-2012) (but does not restrict based on the geographic location of the child at age 30). To construct this figure, within each cohort we regress child income rank on a constant and parent income rank in each CZ, exploiting the linearity property shown in Figure I. Panel A then reports the predicted child rank outcome for parents at the 25th percentile of the family income distribution (~ $30K per year), pooling across cohorts 1980-82. Similarly, Panel B reports the predicted child rank outcome for parents at the 75th percentile of the family income distribution (~ $97K per year).
FIGURE III: Movers’ Outcomes at Age 24 vs. Predicted Outcomes Based on Residents in Destination Moves at Age 13

Notes: This figure presents a non-parametric illustration of the $b_{13}$ coefficient in equation (4). The sample includes all children in 1-time moving households whose parents moved when the child was 13 years old. Child income is measured when the child is age 24. The figure is constructed by first partialing out the fixed effects (the interaction of (a) origin CZ, (b) the child’s age at the parental move, (c) cohort, and (d) parental income deciles): we regress the difference in the destination versus origin prediction, $\Delta_{\text{odps}}$, on the fixed effects and the child rank outcome on the fixed effects. The figure then plots the relationship between these residuals from each of these regression. We construct 20 equal sized bins of the residuals from the destination regression and, in each bin, plot mean of the residuals from the child rank regression.
FIGURE IV: Exposure Effect Estimates for Children’s Income Rank in Adulthood

Notes: Panel A presents estimates of the coefficients, \{b_m\}, from the semi-parametric specification in equation (5) for various ages of the child of income measurement. The sample includes all children in 1-time moving households. Child income is measured when the child is age 24, and 26. We estimate these coefficients by regressing the child’s family income rank on the difference in the predicted family income rank based on prior residents in the destination location relative to the origin location (computed using the linear regression illustrated in Figure I) interacted with each age of the child at the time of the move. We include the set of fixed effects for origin by parent income decile by cohort by the child’s age at the time of the move (as in Figure III). Panel B presents estimates from the specification in equation (6). This specification drops the large set of fixed effects and instead includes (a) dummies for the child’s age at the time of the move, (b) parental rank (within the child’s cohort) interacted with child age dummies, and (c) cohort dummies and predicted outcomes in the destination and origin interacted with cohort dummies. Panels A and B report slopes and intercepts from a regression of the b_m coefficients on m separately for m ≤ 23 and m > 23. We compute \( \delta \) as the predicted value of the line at age 23 using the b_m estimates for m > 23.
FIGURE V: Exposure Effect Estimates for Children’s Income Rank in Adulthood with Controls for Observables

A. Family Fixed Effects

B. Family Fixed Effects and Time Varying Controls

Notes: This figure presents estimates of the coefficients, \{b_m\}, in specifications that add family fixed effects (Panel A) and both family fixed effects and controls for changes in marital status and parental income (Panel B). Panel A presents estimates of \(b_m\) from the baseline specification in equation (6) with the addition of family fixed effects. Panel B adds family fixed effects along with a set of controls for income rank changes marital status changes around the time of the move. To do so, we construct parental income ranks by cohort by year of outcome measurement. We interact the differences in parental ranks in the year before versus after the move with a linear interaction with the child age at the time of the parental move (for ages below 24) and an interaction with an indicator for child age greater than 23 at the time of the parental move. We also construct a set of indicators for marital status changes. We define marital status indicators for the year before the move and the year after the move and construct indicators for being always married, getting divorced, or being never married (getting married is the omitted category). We include these variables and their linear interactions with the child age at the time of the parental move (for ages below 24) and an interaction with an indicator for child age greater than 23 at the time of the parental move. As in Figure IV, we report slopes and intercepts from a regression of the \(b_m\) coefficients on \(m\) separately for \(m \leq 23\) and \(m > 23\). We compute \(\delta\) as the predicted value of the line at age 23 using the \(b_m\) estimates for \(m > 23\).
Notes: This figure presents estimates of the exposure time slope for a subsample of moves restricted to zipcode-by-year observations with large outflows, instrumenting for the change in predicted outcomes based on prior residents, $\Delta_{odps}$, with the average change in predicted outcomes for the given origin. More specifically, for each zipcode in our sample of children in the 1980-1993 cohorts, we calculate the number whose parents leave the (5-digit) zipcode in each zipcode, $m_{zt}$. Then, we compute the average number of people who leave in a given year across our 1997-2012 sample window, $\bar{m}_{z}$. We then divide the outflow in a zipcode-year observation, $m_{zt}$, by the mean outflow for the county to construct our measure of the displacement shock, $d = \frac{m_{zt}}{\bar{m}_{z}}$. The horizontal axis presents the results for varying quantile thresholds of $d$ ranging from the median to the 95th percentile. The corresponding mean value of $d$ for the sample is presented in brackets. For each zipcode, we compute the mean value of $\Delta_{odps}$ for each parental income decile (pooling across all years and all movers in the zipcode). Throughout, we restrict to zipcode-years with at least 10 observations. Then, for each sample with values of $d$ above the threshold, we estimate $\gamma$ in equation (7). We plot the estimate of $\gamma$ as a function of the threshold.
Notes: This figure presents estimates of the exposure time slope using own and placebo cohort place predictions. The sample includes all children in 1-time moving households whose parents moved when the child was less than or equal to 23 years old. The series in red triangles plots estimates of 9 separate regressions using place predictions for child in cohort $c$ as if s/he were in cohort $c + k$, where $k$ ranges between -4 and 4. By construction, the estimate for $k = 0$ corresponds to the baseline slope of 0.040, illustrated in Figure IV (Panel B). Regressions include the predicted outcomes based on prior residents in the origin and destination (for cohort $c + k$), and the interactions of the child’s age at the time of the move with the predicted outcomes in the origin and destination based on prior residents (for cohort $c + k$). To be consistent with the baseline specifications, regressions also include dummy indicators for true cohort and its interaction with the predicted outcomes in the origin location. The blue series reports coefficients from a single regression that includes all variables in each of the regressions for $k = -4, ..., 4$ and plots the coefficient on the interaction of the child’s age at the time of the move with the predicted outcome based on prior residents in the destination location in cohort $c + k$. 
FIGURE VIII: Movers’ Outcomes vs. Predicted Employment and Probability of Reaching top 10% in Destination

A. Probability of Reaching Top 10%

B. Employment

Notes: This figure presents binned scatter plots analogous to Figure III, but with the outcome being employed at age 24 and the event that the child reaches the top 10% of the income distribution at age 24 (Panel A) and the event that the child is employed (Panel B), controlling for the mean rank predictions. In Panel A, we construct the event that the child is in the top 10% of the national (cohort-specific) income distribution. Using permanent parental residents in each CZ, we compute the fraction of children in the top 10% of the national cohort-specific income distribution. The blue series presents a non-parametric representation of the relationship between the event the child is in the top 10% and the predicted chance that the child is in the top 10% based on the prior residents in the destination CZ, controlling for the predicted chance the child is in the top 10% based on prior residents in the origin CZ and placebo controls for the predicted mean child rank in the origin and destination locations. Analogous to the binned scatter plots above, we partial out these controls, bin the residuals for the regression of the destination location into 20 equal bins, and plot the mean residual of the child outcome in each bin. For the red series, we instead plot the placebo relationship between the child being in the top 10% and the predicted mean rank of the child in the destination, controlling for the mean rank predictions in the origin and the top 10% predictions in both the origin and destination. In Panel B, we define employed is defined as filing a w2 at some point during the age of 24. We then repeat this process replacing the event the child is in the top 10% with the event that the child is employed. The blue series presents a non-parametric representation of the relationship between the event the child is employed and the predicted mean rank of the child in the destination, controlling for the mean rank predictions in the origin and the top 10% predictions in both the origin and destination. Analogous to the binned scatter plots above, we partial out these controls, bin the residuals for the regression of the destination location into 20 equal bins, and plot the mean residual of the child outcome in each bin. For the red series, we instead plot the placebo relationship between the child being employed and the predicted mean rank of the child in the destination, controlling for the mean rank predictions in the origin and the employment predictions in both the origin and destination.
FIGURE IX: Movers’ Outcomes vs. Gender-Specific Predicted Outcomes in Destination

Notes: This figure presents binned scatter plots analogous to Figure III, but using gender-specific predicted outcomes based on prior residents. The blue series provides a non-parametric representation of the relationship between the child’s own gender place prediction and the child’s outcome; the red series provides a non-parametric representation of the relationship between the other (placebo) gender place predictions for the child’s outcome, controlling for the own gender prediction. The sample includes all children in 1-time moving households whose parents moved when the child was less than or equal to 13 years old. Child income is measured when the child is age 26. For the blue circle series, we regress the own gender destination prediction for the child’s outcome on the other gender destination prediction, other gender origin prediction, and own gender origin prediction. Similarly, we regress the child’s income rank on the other gender destination prediction, other gender origin prediction, and own gender origin prediction. The figure then plots the relationship between these residuals from these regressions with sample means added to center the graphs. We construct 20 equal sized bins of the residuals from the destination regression and, in each bin, plot mean of the residuals from the child rank regression. For the red series, we repeat this process but using the placebo (other) gender predictions. We regress the other gender destination prediction for the child’s outcome on the own gender destination prediction, other gender origin prediction, and own gender origin prediction. Similarly, we regress the child’s income rank on the own gender destination prediction, other gender origin prediction, and own gender origin prediction. The red triangle series then plots the relationship between these residuals from these regressions with sample means added to center the graphs.
Notes: This figure presents exposure effect estimates for college and marriage outcomes. In Panel A, we replicate the baseline specification in equation (6) replacing the child’s outcomes with an indicator for college attendance at any age between 18-23. We construct separate analogous predicted outcomes based on the prior residents in each CZ for each outcome. We define college attendance as the existence of a 1098-T form (indicating college enrollment) when the child is 18-23 years old and restrict the sample to observations we observe for years 18-23. Because we observe college attendance in years 1999-2012, we obtain estimates for ages at move of 8-29. In Panel B, we replicate the baseline specification in equation (6) replacing the child’s outcomes with an indicator for being married at age age 26 using the child’s filing status at age 26.
FIGURE XI: Exposure Effect Estimates for Teen Outcomes

A. Teen Employment at Age 16

B. Teen Employment at Age 17

C. Teen Employment at Age 18

D. Teenage birth

Notes: This figure presents exposure effect estimates for teen outcomes. Panels A-C replicate the baseline specification with origin prediction controls (Figure IV, Panel B), but replaces the child’s outcomes with an indicator for working at age 16-18 (defined as the existence of a W-2 during the year in which the child turned age a). Panel D presents estimates from the baseline specification using teen birth as the outcome. We define teenage birth as having a birth in the calendar year prior to turning age 20 using birth certificate records from the social security administration’s death master file (DM-2), and estimate the model separately for males and females.
Notes: This figure presents a county map of the Boston commuting zone.
Notes: This figure replicates our baseline specification in equation (6), shown in Figure IVb, using incomes measured at age 24, 26, 28, and 30. The figure reports the slopes from a regression of the $b_m$ coefficients on $m$ for $m \leq 23$, with standard errors in parentheses.
Notes: This figure presents estimates of $b_m$ in equation (5) that replaces the $\alpha_{qosm}$ fixed effects in equation (5) with $\alpha_{qdsm}$ fixed effects that control for the destination, $d$, instead of the origin, $o$, so that the slope is identified from variation in the origin exposure. As in Figure IVa, the figure reports the estimated slopes from a regression on the dots on the figure.
Notes: This figure presents estimates of the coefficients $b_m$ adjusted for the probability that the child follows the parent to the destination. Formally, we construct the fraction of children who follow their parents when the parents move when the child is $m$ years old, $\phi_m$, as the fraction of children who either (a) file a tax return in the destination, (b) have a form W-2 mailing address in the destination location, or (c) attend a college (based on 1098-T filings by institutions) in the destination location. The figure plots the series of $b_{m}^{IV} = \frac{b_m - \delta}{\phi_m} + \delta$, where $\delta = 0.125$ is the estimated selection effect shown in Figure IVa.
ONLINE APPENDIX FIGURE V

Map of Difference in Gender Outcomes, $\bar{y}_{pcs}^m - \bar{y}_{pcs}^f$, Evaluated at the 25th Percentile of Parental Income.

Notes: This figure presents estimates of the difference in male versus female outcomes of permanent residents, $\bar{y}_{pcs}^m - \bar{y}_{pcs}^f$ by CZ, c, for income at age 24. To estimate $\bar{y}_{pcs}^m$ and $\bar{y}_{pcs}^f$, we estimate linear regressions of child rank on parent income rank for each CZ on separate male and female samples, pooling cohorts 1980-1988 cohorts.
County-Level Predicted Income Rank at Age 30 - Permanent Residents

A. For Children with Parent at the 25th Percentile

B. For Children with Parent at the 75th Percentile

Notes: These figures present the estimated $\bar{y}_{pcs}$ by CZ and County for $p = 25$ and $p = 75$. 
<table>
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<tr>
<th>Variable</th>
<th>Mean (1)</th>
<th>Std. Dev. (2)</th>
<th>Median (3)</th>
<th>Sample Size (4)</th>
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<td>2 times</td>
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<td>3 times</td>
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<td>4+ times</td>
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<td>Total</td>
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<td><strong>1 time -3 times Movers</strong></td>
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</tbody>
</table>

Notes: The table presents summary statistics for the samples used in the CZ-level analyses. We split the summary statistics into the permanent residents ("non-movers") whose parents do not move across CZs throughout our sample window (1996-2012) and movers. Section III provides details on variable and sample definitions.
### TABLE II
Exposure Effect Estimates

<table>
<thead>
<tr>
<th>Specification</th>
<th>Baseline Spec.</th>
<th>Controls</th>
<th>Origin Controls (Destination)</th>
<th>Individual Income</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pooled</td>
<td>Age ≤ 23</td>
<td>Age ≤ 18</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td></td>
</tr>
<tr>
<td>Exposure Slope</td>
<td>0.040</td>
<td>0.041</td>
<td>0.041</td>
<td>0.041</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.006)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cohort-Varying Intercept</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Child age (m) x y_{ops} Interactions</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Child Income Definition</td>
<td>Family</td>
<td>Family</td>
<td>Family</td>
<td>Family</td>
</tr>
<tr>
<td>Num of Obs.</td>
<td>1,553,021</td>
<td>1,287,773</td>
<td>687,323</td>
<td>604,602</td>
</tr>
<tr>
<td></td>
<td>1,553,021</td>
<td>1,553,021</td>
<td>1,553,021</td>
<td>1,553,021</td>
</tr>
</tbody>
</table>

Notes: Table II reports the coefficients on the child's age at the time of the parental move interacted with the difference in the predicted outcomes based on prior residents in the destination relative to the origin. Coefficients are multiplied by -1 to correspond to exposure to destination. We allow separate lines allowed for child age <= 23 and child age > 23 at the time of the parental move. Column (1) reports the coefficient γ in equation (6). Column (2) restricts the sample to those below age 23 at the time of the move. Column (3) restricts the sample to those below age 18. Column (4) further restricts to the sample of children who are claimed as a dependent on a 1040 in the destination CZ in the years subsequent to the move. Column (5) drops the cohort interactions with the predicted outcomes of permanent residents in the origin and destination location and instead includes one control for the predicted outcomes of those in the origin location. Column (6) adds controls for the child's age at move interacted with the predicted outcomes of those in the origin location to the baseline specification in column (1) and equation (9). Column (7) presents the baseline specification (equation 6) using individual income for both the outcome and predicted outcomes in the origin and destination.
### TABLE III
Exposure Effect Estimates: Family Fixed Effects and Time-Varying Controls for Income and Marital Status

<table>
<thead>
<tr>
<th>Specification:</th>
<th>Baseline Spec.</th>
<th>Family FE</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Baseline</td>
<td>Origin Controls</td>
</tr>
<tr>
<td>Exposure Slope</td>
<td>0.040 (0.002)</td>
<td>0.041 (0.002)</td>
</tr>
<tr>
<td>Controls</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cohort-Varying Intercept</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Child age (m) x y_{acts} Interactions</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Family FE</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Income and Marital Status Changes</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Child Income Definition</td>
<td>Family</td>
<td>Family</td>
</tr>
<tr>
<td>Num of Obs.</td>
<td>1,553,021</td>
<td>1,553,021</td>
</tr>
</tbody>
</table>

**Notes:** This table presents estimates of the exposure effect estimated with the inclusion of family fixed effects and controls for changes in parental income and marital status around the time of the move. Columns (1) and (2) replicate the baseline specification in Table II. Column (3) presents the baseline estimates in equation (6) without the inclusion of cohort-specific controls (i.e. no cohort dummies or interactions of these dummies with the predicted outcomes based on prior residents in the origin or destination CZ). Column (4) adds family fixed effects to the specification in equation (6). Column (5) adds family fixed effects to the specification in equation (6) that also includes interactions of the child's age at the time of the parental move and the predicted outcomes based on the prior residents in the origin CZ. Column (5) takes the baseline specification in column (1) and adds family fixed effects and controls separately for each age of the child, fully interacted with cohort dummies (1980-1988). Column (6) adds family fixed effects to this specification in column (3) that does not include cohort-specific controls. Column (7) adds family fixed effects and year- and cohort-specific controls for parental income for each age of the child and cohort over the range of our data (1996-2012). Column (8) takes the baseline specification in column (1) and adds both family fixed effects and controls for changes in marital status and income around the time of the parental move, along with their interaction with under-23 exposure time the child has in the destination CZ. We construct the parental income rank by cohort by year, and use this to construct the difference in the parental income rank in the year after the move relative to the year before the move. We include this measure of income change and a full set of its interaction with 23-m and an indicator for m>23. We also construct an indicator for the child's mother's marital status by year and construct 4 indicators for possible marital status changes (married -> married, married -> un-married, un-married -> married, un-married -> un-married). We then interact these four indicators with a full set of its interaction with 23-m and an indicator for m>23. Finally, Column (9) illustrates the robustness of the family fixed effects results to individual income as the outcome, as opposed to family income.
### TABLE IV
Distributional Convergence

<table>
<thead>
<tr>
<th>Child Rank in top 10%</th>
<th>Child Employed</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Distributional Prediction</td>
<td>0.043</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
</tr>
<tr>
<td>Mean Rank Prediction</td>
<td>0.022</td>
</tr>
<tr>
<td>(Placebo)</td>
<td>(0.002)</td>
</tr>
</tbody>
</table>

Num. of Obs. 1,553,021 1,553,021 1,553,021 1,553,021 1,553,021 1,553,021

*Notes:* Table presents estimates of the exposure time relationships for the outcome of being in the top 10% of the cohort-specific income distribution at age 24 and being employed. We define employment as an indicator for filing a W-2 at some point during the year in which the child is age 24. Analogous to these outcomes, we construct predicted outcomes using permanent residents each CZ. Column (1) presents the estimated exposure time slope using top 10% indicator as the dependent variable and predicted outcomes based on permanent residents in the origin and destination CZ. Column (2) continues to use the indicator of being in the top 10% as the dependent variable, but uses the mean rank predictions from the baseline regressions as the origin and destination predictions. Column (3) combines all variables in specifications (1) and (2). Column (4) presents the estimated exposure time slope using an indicator of being employed as the dependent variable and predicted outcomes based on permanent residents in the origin and destination CZ. Column (5) retains the employment indicator as the dependent variable but replaces the predicted outcomes in the origin and destination with the mean rank predictions from the baseline regressions. Column (6) combines all variables in specifications (4) and (5).
### TABLE V
Gender Placebos

<table>
<thead>
<tr>
<th>Sample</th>
<th>No Family Fixed Effects</th>
<th>Family Fixed Effects</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Own Gender</td>
<td>0.038</td>
<td>0.031</td>
</tr>
<tr>
<td>Prediction</td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Other Gender</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Prediction (Placebo)</td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Family Fixed Effects</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Num. of Obs.</td>
<td>1,552,898</td>
<td>1,552,898</td>
</tr>
</tbody>
</table>

Notes: Table presents estimates of the exposure time relationships using gender-specific predictions based on prior residents. The outcome is child rank when the child is 24 years old. Column (1) presents estimates for the baseline specification replacing the predicted outcomes based on prior residents in the origin and destination with gender-specific predictions. Column (2) replaces own-gender predicted outcomes with predicted outcomes in the origin and destination based on the other gender. Column (3) combines all variables in the specification in (1) and (2). Columns (4)-(6) repeat the specifications in (1)-(3) with the addition of family fixed effects. Column (7) repeats the specification in (6) but restricts to households with at least two children and at least one of each gender.
TABLE VI
Mechanisms

<table>
<thead>
<tr>
<th>Specification:</th>
<th>Baseline Specification (Pooled)</th>
<th>Child CZ Fixed Effects</th>
<th>Multiple moves</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>1st Destination</td>
</tr>
<tr>
<td>Exposure Slope</td>
<td>0.040 (0.002)</td>
<td>0.031 (0.002)</td>
<td>0.040 (0.001)</td>
</tr>
<tr>
<td>Controls</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cohort-Varying Intercept</td>
<td>X</td>
<td></td>
<td>X</td>
</tr>
<tr>
<td>Child age (m) x y_{ops} Interactions</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Child Income Definition</td>
<td>Family</td>
<td>Family</td>
<td>Family</td>
</tr>
<tr>
<td>Num of Obs.</td>
<td>1,553,021</td>
<td>1,473,218</td>
<td>4,374,418</td>
</tr>
</tbody>
</table>

Notes: Table VI reports the coefficients on the child's age at the time of the parental move interacted with the difference in the predicted outcomes based on prior residents in the destination relative to the origin. Coefficients are multiplied by -1 to correspond to exposure to destination. We allow separate lines allowed for child age <= 23 and child age > 23 at the time of the parental move. Column (1) reports the coefficient $\beta$ in equation (9) in our baseline specification. Column (2) adds the child's CZ in adulthood (2012) as a fixed effect. Columns (3)-(5) present estimates for the exposure effect of the 1st, 2nd, and 3rd move using the sample of 1-3-time movers, as opposed to the 1-time movers sample. Column (6) presents the estimates of the exposure effect restricting the coefficient to be the same across each move.
### Appendix Table I

**Population and Distance Restrictions**

<table>
<thead>
<tr>
<th>Baseline</th>
<th>No Distance</th>
<th>100 Miles (Baseline)</th>
<th>200 Miles</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pop &gt; 50K</td>
<td>Pop &gt; 250K</td>
<td>Pop &gt; 50K</td>
</tr>
<tr>
<td>Exposure Slope</td>
<td>0.040 (0.002)</td>
<td>0.032 (0.001)</td>
<td>0.035 (0.002)</td>
</tr>
<tr>
<td></td>
<td>0.037 (0.002)</td>
<td>0.037 (0.001)</td>
<td>0.039 (0.002)</td>
</tr>
<tr>
<td></td>
<td>0.040 (0.002)</td>
<td>0.039 (0.002)</td>
<td>0.040 (0.002)</td>
</tr>
<tr>
<td></td>
<td>0.037 (0.002)</td>
<td>0.039 (0.002)</td>
<td>0.041 (0.002)</td>
</tr>
<tr>
<td>Num of Obs.</td>
<td>1,553,021</td>
<td>3,066,854</td>
<td>1,607,626</td>
</tr>
<tr>
<td></td>
<td>2,199,834</td>
<td>1,609,330</td>
<td>1,210,164</td>
</tr>
<tr>
<td></td>
<td>1,719,687</td>
<td>1,345,125</td>
<td>1,036,668</td>
</tr>
</tbody>
</table>

**Notes:** This table presents estimates of the baseline specification in equation (6) varying the sample restriction. Column (1) presents the baseline sample restricting to populations in the origin and destination CZ of greater than 250,000 people based on the 2000 Census and requiring a distance of move > 100 miles between zipcode centroids. Columns (2)-(10) vary these distance assumptions and population restrictions.
### Appendix Table II
**Heterogeneity in Exposure Effects**

<table>
<thead>
<tr>
<th>Baseline</th>
<th>Parental Income</th>
<th>Moves</th>
<th>Child Gender</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Above Median Income</td>
<td>Below Median Income</td>
<td>Positive Moves</td>
</tr>
<tr>
<td>Exposure Slope</td>
<td>0.040 (0.002)</td>
<td>0.031 (0.003)</td>
<td>0.030 (0.004)</td>
</tr>
<tr>
<td>Num of Obs.</td>
<td>1,553,021</td>
<td>803,189</td>
<td>749,832</td>
</tr>
</tbody>
</table>

**Notes:** This table presents estimates of the heterogeneity in the baseline exposure time estimates (Column (1) of Table II) for various subsamples. Column (1) reports the baseline coefficient. Column (2) (Column (3)) restricts to moves by parents with above (below) median income (median defined as parent rank = 0.5; note there are more observations of 1x movers with parent rank > 0.5, reflecting the fact that the likelihood of moving is increasing in parental income). Column (4) (Column (5)) restricts to moves in which the predicted outcomes based on prior residents in the destination are higher (lower) than in the origin. Columns (6) and (7) restrict the sample to male and female children, respectively.
## Appendix Table III
County Exposure Effect Estimates

<table>
<thead>
<tr>
<th>Specification:</th>
<th>Baseline Spec.</th>
<th>Within CZ Moves</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Baseline</td>
<td>Family FE</td>
</tr>
<tr>
<td>Exposure Slope</td>
<td>0.035</td>
<td>0.033</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>Num of Obs.</td>
<td>654,491</td>
<td>654,491</td>
</tr>
</tbody>
</table>

Notes: Table II reports exposure effect coefficients in equation (6), analogous to those presented in Tables II and III, using county-level predictions for the sample of 1-time county movers. Column (1) presents the baseline specification analogous to Column (1) of Table II, replacing CZ-level predictions with county-level predictions based on prior residents. We restrict the sample to moves of at least 100 miles and require the county-level population to be at least 250,000 in the origin and destination county. Column (2) adds family fixed effects to the specification in Column (1). Columns (3)-(7) drop the distance restriction and consider the set of within-CZ county moves (between counties with populations of at least 250,000). Column (3) replicates the baseline specification. Column (4) replicates the baseline specification using income at age 26 as the outcome, analogous to the outcomes considered in Section V. Column (5) presents the pooled estimate that stacks all outcomes for ages 24 and above (multiple observations per person). Column (6) adds family-by-age of outcome fixed effects to the specification in Column (5). Column (7) expands the sample in Column (5) to include moves between all CZs with populations above 10,000).
Appendix Table IV
Summary Statistics for County Permanent Residents and Movers

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean (1)</th>
<th>Std. Dev. (2)</th>
<th>Median (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Non-Movers</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Parent Income</td>
<td>81,932</td>
<td>320,026</td>
<td>54,800</td>
</tr>
<tr>
<td>Child family income at 24</td>
<td>25,066</td>
<td>136,016</td>
<td>19,900</td>
</tr>
<tr>
<td>Child family income at 26</td>
<td>34,091</td>
<td>157,537</td>
<td>26,600</td>
</tr>
<tr>
<td>Child family income at 30</td>
<td>48,941</td>
<td>133,264</td>
<td>36,200</td>
</tr>
<tr>
<td>Child individual earnings at 24</td>
<td>20,686</td>
<td>202,833</td>
<td>17,300</td>
</tr>
<tr>
<td>College attendance (18-23)</td>
<td>0.703</td>
<td>0.457</td>
<td>1.000</td>
</tr>
<tr>
<td>College quality (18-23)</td>
<td>31,608</td>
<td>13,207</td>
<td>31,400</td>
</tr>
<tr>
<td>Teen Birth (13-19)</td>
<td>0.107</td>
<td>0.309</td>
<td>0.000</td>
</tr>
<tr>
<td>Teen employment at age 16</td>
<td>0.276</td>
<td>0.447</td>
<td>0.000</td>
</tr>
<tr>
<td><strong>One-time Movers Across CZ Sample</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Parent Income</td>
<td>94,738</td>
<td>400,685</td>
<td>55,100</td>
</tr>
<tr>
<td>Child family income at 24</td>
<td>23,815</td>
<td>72,306</td>
<td>18,200</td>
</tr>
<tr>
<td>Child family income at 26</td>
<td>32,532</td>
<td>139,563</td>
<td>24,300</td>
</tr>
<tr>
<td>Child family income at 30</td>
<td>48,834</td>
<td>110,619</td>
<td>33,500</td>
</tr>
<tr>
<td>Child individual earnings at 24</td>
<td>20,247</td>
<td>61,185</td>
<td>16,000</td>
</tr>
<tr>
<td>College attendance (18-23)</td>
<td>0.717</td>
<td>0.451</td>
<td>1.000</td>
</tr>
<tr>
<td>College quality (18-23)</td>
<td>32,171</td>
<td>14,001</td>
<td>31,900</td>
</tr>
<tr>
<td>Teen Birth (13-19)</td>
<td>0.103</td>
<td>0.304</td>
<td>0.000</td>
</tr>
<tr>
<td>Teen employment at age 16</td>
<td>0.233</td>
<td>0.423</td>
<td>0.000</td>
</tr>
<tr>
<td><strong>One-time Movers Within CZ Sample</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Parent Income</td>
<td>84,850</td>
<td>356,758</td>
<td>48,900</td>
</tr>
<tr>
<td>Child family income at 24</td>
<td>24,006</td>
<td>68,559</td>
<td>18,300</td>
</tr>
<tr>
<td>Child family income at 26</td>
<td>32,993</td>
<td>75,520</td>
<td>24,500</td>
</tr>
<tr>
<td>Child family income at 30</td>
<td>49,974</td>
<td>108,248</td>
<td>33,500</td>
</tr>
<tr>
<td>Child individual earnings at 24</td>
<td>20,844</td>
<td>56,639</td>
<td>16,500</td>
</tr>
<tr>
<td>College attendance (18-23)</td>
<td>0.719</td>
<td>0.450</td>
<td>1.000</td>
</tr>
<tr>
<td>College quality (18-23)</td>
<td>32,883</td>
<td>14,086</td>
<td>33,200</td>
</tr>
<tr>
<td>Teen Birth (13-19)</td>
<td>0.095</td>
<td>0.293</td>
<td>0.000</td>
</tr>
<tr>
<td>Teen employment at age 16</td>
<td>0.245</td>
<td>0.430</td>
<td>0.000</td>
</tr>
</tbody>
</table>

Notes: The table presents summary statistics for county movers sample discussed in Online Appendix A.