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THE IMPACTS OF NEIGHBORHOODS ON INTERGENERATIONAL MOBILITY I: CHILDHOOD EXPOSURE EFFECTS*

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We show that the neighborhoods in which children grow up shape their earnings, college attendance rates, and fertility and marriage patterns by studying more than 7 million families who move across commuting zones and counties in the United States. 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 amount of 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

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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 neighborhoods affect intergenerational mobility primarily through childhood exposure, helping reconcile conflicting results in the prior literature. *JEL Codes:* J62, C00, R00.

I. Introduction

To what extent are children's economic opportunities shaped by the neighborhoods in which they grow up? Despite extensive research, the answer to this question remains debated. Observational studies have documented significant variation across neighborhoods in economic outcomes (e.g., Wilson 1987; Jencks and Mayer 1990; Massey and Denton 1993; Sampson, Morenoff, and Gannon-Rowley 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, Kling, and Liebman 2001; Oreopoulos 2003; Ludwig et al. 2013).

Using deidentified 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 articles. In this article, 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 article (Chetty and Hendren 2018a), 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 incomes conditional on their parents' incomes vary substantially with the area (commuting zone or county) in which they grow up (Chetty et al. 2014).[1] This geographic

1. To maximize statistical precision, we characterize neighborhood (or "place") effects at two broad geographies: counties and commuting zones (CZs), which are aggregations of counties that are similar to metro areas but cover the entire United States, including rural areas. Counties are much larger than the typical

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 demographics 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.[2] Because moving is an endogenous choice, simple comparisons of the 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.[3] 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.[4] The identification assumption underlying our research design is that the selection effects (children's unobservables) associated with moving to a better versus 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 present evidence supporting the validity of this identification assumption after presenting a set of baseline results.

geographic units used to define "neighborhoods"; however, the variance of place effects across the broad geographies we study 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 CZ (or, in the county-level analysis, the same county) throughout the period we observe (1996–2012).

3. Several recent studies have used movers to identify causal effects of places on other outcomes using event-study designs, comparing individuals' outcomes before versus after they move (e.g., Chetty, Friedman, and Saez 2013; Finkelstein, Gentzkow, and Williams 2016). We use a different research design because we naturally do not have premove data on income in adulthood when studying the impact of moving during childhood.

4. Throughout the article, 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.

In our baseline analysis, we focus on families with children born between 1980 and 1988 who moved once across commuting zones (CZs) between 1997 and 2010. We find that on average, spending an additional year in a CZ where the mean income rank of children of permanent residents is 1 percentile higher (at a given level of parental income) increases a child's income rank in adulthood by approximately 0.04 percentiles. That is, the incomes of children who move converge to the incomes of permanent residents in the destination at a rate of 4% per year of childhood exposure. Symmetrically, moving to an area where permanent residents have worse incomes reduces a child's expected income by 4% per year. When analyzing children who move more than once during childhood, we find that children's incomes vary in proportion to the amount of time they spend in an area rather than the specific ages during which they live in that area, as would be the case in a model of "critical age effects" (e.g., Lynch and Smith 2005).

Together, these results 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. The outcomes of children who move converge linearly to the outcomes of permanent residents in the destination over the age range we are able to study in our data (ages 9 to 23). Hence, annual exposure effects are approximately constant: moving to a better area at age 9 instead of 10 is associated with the same increase in income as moving to that area at age 15 instead of 16. The exposure effects persist until children are in their early twenties. We find similar childhood exposure effects for several other outcomes, including rates of college attendance, marriage, and teenage birth. We also find similar exposure effects when families move across counties.

Our estimates imply that the majority of the observed variation in outcomes across areas is due to causal effects of place. The convergence rate of 4% per year of exposure between the ages of 9 and 23 implies that children who move at age 9 would pick up about $(23 - 9) \times 4\% = 56\%$ of the observed difference in permanent residents' outcomes between their origin and destination CZs. If we extrapolate to earlier ages by assuming that the rate of convergence remains at 4% even before age 9—a strong assumption that should be evaluated in future work—our estimates imply that children who move at birth to a better area and stay there for

20 years would pick up about 80% of the difference in permanent residents' outcomes between their origins and destinations.

As noted, 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 versus worse areas do not vary with the age at which they move. We use four approaches to evaluate this assumption: controlling for observable fixed family characteristics, controlling for time-varying observable characteristics, isolating plausibly exogenous moves triggered by aggregate displacement shocks, and implementing a set of outcome-based placebo tests. The first three approaches are familiar techniques in the treatment effects literature, whereas the fourth exploits the multi-dimensional nature of the treatments we study and the precision afforded by our large samples to implement overidentification tests of the exposure effect model.

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.[5] This approach identifies exposure effects from comparisons between siblings, by asking whether the difference in earnings outcomes between two siblings who move to a new area is proportional to their age difference interacted with permanent residents' outcomes in the destination. We estimate an annual exposure effect of approximately 4% per year with family fixed effects, very similar to our baseline estimate.

These sibling comparisons address confounds due to factors that are fixed within families, but they do not account for time-varying factors, such as a change in family environment at the time of the move that directly affects children in proportion to exposure time independent of neighborhoods. We cannot observe

5. The idea of using sibling comparisons to better isolate neighborhood effects dates to the seminal review by Jencks and Mayer (1990). Plotnick and Hoffman (1996) and Aaronson (1998) implement this idea using data on 742 sibling pairs from the Panel Study of Income Dynamics, but reach conflicting conclusions due to differences in sample and econometric specifications. Several studies also use sibling comparisons to identify critical periods that shape immigrants' long-term outcomes (Basu 2010; van den Berg et al. 2014). Our approach differs from these studies in that we focus on how the difference in siblings' outcomes covaries with the outcomes of permanent residents in the destination neighborhood, whereas the studies of immigrants estimate the mean difference in siblings' outcomes as a function of their age gap. This allows us to separate the role of neighborhood exposure from changes within the family that also generate exposure-dependent differences across siblings, such as changes in income or wealth when a family moves to a new country.

all such time-varying factors, but we do observe two particularly important characteristics of the family environment in each year: income and marital status. In our second approach, we show that 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.

The preceding results rule out confounds due to observable factors such as income, but 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 third approach addresses the problem of bias associated with endogenous choice by 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, often 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.[6]

Although the evidence from the first three approaches strongly supports the validity of the identification assumption, each approach itself rests on assumptions (selection on observables and exogeneity of the displacement shocks) that could potentially be violated. We therefore turn to a fourth approach: a set of placebo (overidentification) tests that exploit heterogeneity in permanent residents' outcomes across subgroups, which in our view provides the most compelling method of assessing the validity of the research design.

We begin by analyzing heterogeneity 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 the preceding and subsequent birth

6. We eliminate variation due to individuals' endogenous choices of where to move in these specifications 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.

cohorts (conditional on their own birth cohort's predictions). Such cohort-specific convergence is precisely what one would expect if places have causal effects on children's outcomes in proportion to exposure time, but 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 incomes on average in adulthood, but children in San Francisco are more likely to end up in the upper or lower tail of the income distribution (i.e., either in the top 10% or not employed). 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 convergence: controlling for mean incomes, children's incomes converge to predicted incomes in the destination across the distribution in proportion to exposure time, at a rate of about 4% a year.

Finally, we implement placebo tests exploiting heterogeneity in permanent residents' outcomes across genders. Although earnings outcomes are highly correlated across genders, there are some places where boys do worse than girls (e.g., areas with concentrated poverty) and vice versa. When a family with a daughter and a son moves to an area that is especially good for boys, their son does better than their daughter in proportion to the number of years they spend in the new area. Once again, if our findings of neighborhood exposure effects were driven by sorting or omitted variables, one would not expect to find such gender-specific convergence in incomes unless families are fully aware of the exact gender differences in incomes across areas and sort to neighborhoods based on these gender differences.

In sum, the four tests we implement imply that any omitted variable that generates bias in our exposure effect estimates must (i) operate within families in proportion to exposure time, (ii) be orthogonal to changes in parental income and marital status, (iii) persist in the presence of moves induced by displacement shocks, and (iv) precisely replicate permanent residents' outcomes by birth cohort, quantile, and gender in proportion to exposure time. We believe that plausible omitted variables are unlikely to have all of these properties and therefore conclude that our baseline estimate of 4% convergence in outcomes per year of childhood exposure to an area is unbiased.

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 largely because of 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, each year of childhood exposure matters roughly equally; there is no "critical age" after which the returns to living 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, Liebman, and Katz 2007), because data on the long-term outcomes of younger children were unavailable. In a companion paper (Chetty, Hendren, and Katz 2016), we link the MTO data to tax records and show that the MTO data exhibit childhood exposure effects consistent with those identified here. In particular, Chetty, Hendren, and Katz (2016) find substantial improvements in earnings and other outcomes for children whose families received experimental vouchers to move to low-poverty neighborhoods at young ages. In contrast, children who moved at older ages experienced no gains or slight losses.[7]

7. One important distinction between the two studies is that the analysis sample in the present quasi-experimental study consists entirely of families who moved across CZs, whereas the MTO experiment compares families who moved to lower poverty neighborhoods with families who did not move at all or moved within an area similar to where they lived before. As a result, the analysis here identifies the effects of moving to better versus worse areas conditional on moving to a different area, whereas the MTO analysis compares the effects of moving to a lower-poverty neighborhood versus staying in a higher-poverty area. The exposure

More generally, our findings imply that much of the neighborhood-level variation in economic outcomes documented in previous observational studies does in fact reflect causal effects of place, but that such 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, Harding, and Elwert 2011; Wodtke 2013; Sharkey and Faber 2014). We contribute to this literature by presenting quasi-experimental estimates of exposure effects, addressing the concerns about selection and omitted variable bias that arise in observational studies (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 in this study or our MTO study, contrary to the patterns observed in observational data (Clampet-Lundquist and Massey 2008).

Our findings are also consistent with recent studies that use other research designs—random assignment of refugees (Damm and Dustmann 2014), housing demolitions (Chyn forthcoming), and selection corrections using group characteristics (Altonji and Mansfield 2016)—to show that neighborhoods have causal effects on children's long-term outcomes. The present analysis complements these studies and Chetty, Hendren, and Katz's (2016) reanalysis of the MTO experiment in two ways. First, it sheds light on the mechanisms underlying neighborhood effects by delivering precise estimates of the magnitude and linear age pattern of childhood exposure effects. Second, it develops a scalable method to estimate neighborhood effects in all areas, even those where randomized or natural experiments are unavailable.[8]

effect estimates here net out any fixed disruption costs of moving to a different type of area, whereas such costs are not netted out in the MTO experiment. This distinction may explain why Chetty, Hendren, and Katz (2016) find slightly negative effects for children who move at older ages in the MTO data, whereas we estimate positive exposure effects of moving to a better area (conditional on moving) at all ages here.

8. Our estimates of neighborhood exposure effects are based on households who choose to move to certain areas. The effects 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. The fact that exposure effects are similar within the subset of displaced households and are symmetric for moves to better and worse areas suggests such heterogeneity in exposure effects is

This article 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 and then specifying our estimating equations. Section IV presents baseline estimates of neighborhood exposure effects and discusses the mechanisms through which neighborhoods affect children's incomes. Section V presents tests evaluating our identification assumption. Section VI presents estimates of exposure effects for other outcomes. Section VII concludes. Supplementary results and details on estimation methodology are provided in the Online Appendix.

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, we use an analysis sample that 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).

II.A. Sample Definitions

Our base data set of children consists of all individuals who (i) have a valid Social Security Number or Individual Taxpayer Identification Number, (ii) were born between 1980–1988, and (iii) are U.S. citizens as of 2013.[9] We impose the citizenship requirement to exclude individuals who are likely to have immigrated to the United States as adults, for whom we cannot measure parent income. We cannot directly restrict the sample to individuals born

limited, but further work is needed to understand how exposure effects vary with households' willingness to move.

9. 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.

in the United States 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 dependent and were between the ages of 15 and 40 when the child was born.[10] 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). As a result, approximately 94% of the children in the 1980–1988 birth cohorts are claimed as a dependent at some point between 1996 and 2012. The fraction of children linked to parents drops sharply prior to the 1980 birth cohort because our data begin in 1996 and many children begin to the 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 data set who are born in the 1980–1988 birth cohorts for whom we are able to identify parents and whose mean parent income between 1996 and 2000 is strictly positive.[11] We divide the full sample into two parts: permanent residents (or stayers) and movers. We define the permanent residents of each CZ c as the subset of parents who reside in a single CZ c in all years of our sample, 1996–2012. The movers sample consists of 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 in the 2000 census above 250,000 (excluding 19.6% of the observations) to ensure that we have adequately large samples to estimate permanent residents' outcomes (the key independent variables in our analysis) precisely. There

10. We impose the 15–40 age restriction to limit links to grandparents or other guardians who might claim a child as a dependent.

11. We limit the sample to parents with positive income (excluding 1.5% of children) because parents who file a tax return, as is 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.

are approximately 24.6 million children in the baseline analysis sample for whom we observe outcomes at age 24 or later, of whom 19.5 million are children of permanent residents.

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.

1. Parent Income. Our primary measure of parent income is total pretax income at the household level, which we label parent family (or household) 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 nontaxable portion of Social Security and Disability (SSDI) 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 SSDI benefits (reported on form SSA-1099) for both parents.[12] In years where parents have no tax return and no information returns, family income is coded as 0.[13] Income is measured prior to the deduction of income taxes and employee-level payroll taxes, and excludes nontaxable cash transfers and in-kind benefits.

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.[14] Because we measure parent income in a

12. The database does not record W-2s and other information returns prior to 1999, so a nonfiler's income is coded as 0 prior to 1999. Assigning nonfiling parents 0 income has little impact on our estimates because only 3.1% of parents in the full analysis sample do not file in each year prior to 1999 and most nonfilers have very low W-2 income (Chetty et al. 2014). For instance, in 2000, the median W-2 income among nonfilers in our baseline analysis sample was $0.

13. 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.

14. 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

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.

2. 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 W-2 forms for a payee ZIP code in that year. Nonfilers 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 and CZs using the crosswalks and methods described in Chetty et al. (2014, Online Appendix A).

Next we define the outcomes that we analyze for children.

3. Income. We define child family income in 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, unemployment insurance benefits, SSDI payments, and half of household self-employment income.

4. Employment. We define an indicator for whether the child is employed at a given age based on whether he or she has a W-2 form filed on his or her behalf at that age. We measure employment rates starting at age 16 to analyze teenage labor force participation.

5. 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 postsecondary institutions eligible for federal student

marital status, this is simply mean family income over the five-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.

aid—are required to file 1098-T forms that report tuition payments or scholarships received for every student. The 1098-T forms are available from 1999 to 2012 and are filed directly by colleges independent of whether an individual files a tax return. Comparisons to other data sources indicate that 1098-T forms capture more than 95% of college enrollment in the United States (Chetty et al. 2017).

6. *Teenage Birth.* For both males and females, we identify a birth as being listed as a parent on a child's application for a social security number, using data from the Social Security Administration's Kidlink (DM-2) database. We define a teenage birth as having a child between the ages of 13 and 19.[15]

7. *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 nonfilers as single because linked CPSIRS data show that the vast majority of nonfilers below the age of 62 are single (Cilke 1998).

8. *Summary Statistics.* Table I reports summary statistics for our analysis sample and various subgroups used in our CZ-level analysis; Online Appendix Table I presents analogous statistics for the sample used in our county-level analysis. The first panel reports statistics for permanent residents in our full analysis sample who live in CZs with more than 250,000 people. The second panel considers the 4.4 million children who moved between CZs with more than 250,000 people (excluding children whose parents moved more than three times between 1996 and 2012, who account for 3% of the observations). The third panel focuses on our primary analysis sample of one-time movers: children whose parents moved exactly once across CZs between 1996 and 2012, are observed in the destination CZ for at least two years, and moved at

15. The total count of births in the SSA DM-2 database closely matches vital statistics counts from the Centers for Disease Control and Prevention prior to 2008; however, the DM-2 database contains approximately 10% fewer births between 2008 and 2012. Using an alternative measure of teenage birth that does not suffer from this missing data problem—in which we define a person as having a teen birth if he or she ever claims a dependent who was born while he or 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.

TABLE I
SUMMARY STATISTICS FOR CZ PERMANENT RESIDENTS AND MOVERS

Variable	Mean (1)	Std. dev. (2)	Median (3)	Num. of obs. (4)
Panel A: Permanent residents: Families who do not move across CZs				
Parent family income	89,909	357,194	61,300	19,499,662
Child family income at 24	24,731	140,200	19,600	19,499,662
Child family income at 26	33,723	161,423	26,100	14,894,662
Child family income at 30	48,912	138,512	35,600	6,081,738
Child individual income at 24	20,331	139,697	17,200	19,499,662
Child married at 26	0.25	0.43	0.00	12,997,702
Child married at 30	0.39	0.49	0.00	6,081,738
Child attends college between 18–23	0.70	0.46	1.00	17,602,702
Child has teen birth (females only)	0.11	0.32	0.00	9,670,225
Child working at age 16	0.41	0.49	0.00	13,417,924
Panel B: Families who move 1–3 times across CZs				
Parent family income	90,468	376,413	53,500	4,374,418
Child family income at 24	23,489	57,852	18,100	4,374,418
Child family income at 26	31,658	99,394	23,800	3,276,406
Child family income at 30	46,368	107,380	32,500	1,305,997
Child individual income at 24	19,091	51,689	15,600	4,374,418
Child married at 26	0.25	0.43	0.00	2,867,598
Child married at 30	0.38	0.49	0.00	1,305,997
Child attends college between 18–23	0.66	0.47	1.00	3,965,610
Child has teen birth (females only)	0.13	0.33	0.00	2,169,207
Child working at age 16	0.40	0.49	0.00	3,068,421
Panel C: Primary analysis sample: families who move exactly once across CZs				
Parent family income	97,064	369,971	58,700	1,553,021
Child family income at 24	23,867	56,564	18,600	1,553,021
Child family income at 26	32,419	108,431	24,500	1,160,278
Child family income at 30	47,882	117,450	33,600	460,457
Child individual income at 24	19,462	48,452	16,000	1,553,021
Child married at 26	0.25	0.43	0.00	1,016,264
Child married at 30	0.38	0.49	0.00	460,457
Child attends college between 18–23	0.69	0.46	1.00	1,409,007
Child has teen birth (females only)	0.11	0.32	0.00	769,717
Child working at age 16	0.39	0.49	0.00	1,092,564

Notes. The table presents summary statistics for the samples used in our CZ-level analyses. The full analysis sample of children consists of all individuals in the tax data who (i) have a valid Social Security Number or Individual Taxpayer Identification Number, (ii) were born between 1980 and 1988, and (iii) are U.S. citizens as of 2013. We report summary statistics for three subsets of this sample. Panel A shows statistics for permanent residents—children whose parents do not move across CZs throughout our sample window (1996 to 2012)—who live in CZs with more than 250,000 people based on the 2000 census. Panel B shows statistics for families who moved once, twice, or three times across CZs with more than 250,000 people from 1996–2012. Panel C shows statistics for our primary analysis sample: children whose families moved exactly once across CZs with more than 250,000 people, are observed in the destination CZ for at least two years, and moved at least 100 miles (based on their ZIP codes). Parent family income is the average pretax household income from 1996 to 2000, measured as AGI plus tax-exempt interest income and the nontaxable portion of Social Security and Disability (SSDI) benefits for tax-filers, and using information returns for nonfilers. Child family income is measured analogously at various ages, while child individual income is defined as the sum of individual W-2 wage earnings, unemployment insurance benefits, SSDI payments, and half of household self-employment income. Marital status is defined based on the marital status listed on 1040 forms for tax filers; nonfilers are coded as single. College attendance is defined as having a 1098-T form filed on one's behalf at any point between the ages of 18 and 23. Teenage birth is defined (for women only) as having a child between the ages of 13 and 19, using data from the Social Security Administration's DM-2 database. We define an indicator for working at age 16 based on having a W-2 form filed on one's behalf at that age. All dollar values are reported in 2012 dollars, deflated using the CPI-U. See Section II for further details on variable and sample definitions.

least 100 miles.[16] There are 1.6 million children in this one-time movers sample.

Although our analysis does not require movers to be comparable to permanent residents, we find that movers and permanent residents have similar characteristics. Median parent family income is $61,300 for permanent residents, compared to $58,700 for one-time movers. Children of permanent residents have a median family income of $35,600 when they are 30 years old, compared with $33,600 for one-time 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. Eleven percent of daughters of permanent residents and one-time movers have a teenage birth.

III. Empirical Framework

In this section, we first present a descriptive characterization of intergenerational mobility for children who grow up in different areas in the United States. 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: CZ, counties, ZIP codes, and census tracts. In the main text of this article, we focus on variation across 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 United States; on average, each CZ contains four counties and has a population of 380,000. We replicate the results reported in the main text at the county level in Online Appendix C. We focus on variation across relatively broad geographic units to maximize statistical precision because some of our research designs require

16. We impose these restrictions to eliminate moves across CZ borders that do not reflect a true change of location. We measure the distance of moves as the distance between the centroids of the origin and destination ZIPs (obtained from www.boutell.com/zipcodes). We show the robustness of our results to using alternative cutoffs for minimum population size and move distances in Online Appendix A.

large sample sizes to discern fine variation in permanent residents' outcomes across subsamples.

We characterize the outcomes of children who spent their entire childhoods in a single CZ by focusing on children of "permanent residents" — parents who stay in the same CZ between 1996 and 2012.[17] 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 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 1980 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

17. Because 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 and 24, 81.5% of them lived in the same CZ when their children were age 8.

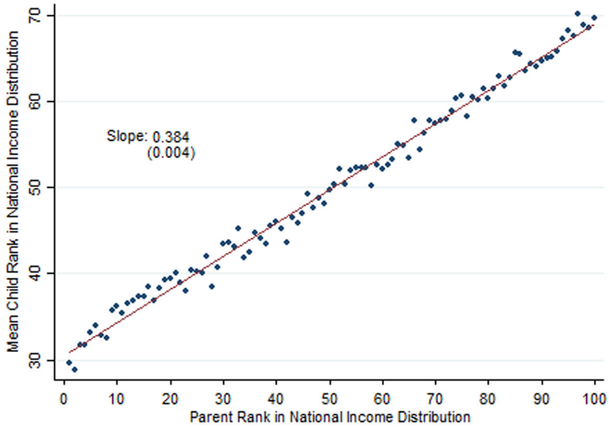

FIGURE I

Mean Child Income Rank versus Parent Income Rank for Children Raised in Chicago

This figure presents a binned scatter plot of the relationship between children's income ranks and parent income ranks for children raised in Chicago. The points on the figure plot the mean rank of children within each parental income percentile bin. The best-fit line is estimated using an OLS regression on the underlying micro data. The figure also reports the slope of the best-fit line (the rank-rank slope), along with the standard error of the estimate (in parentheses). The sample includes all children in the 1980 birth cohort in our analysis sample whose parents were permanent residents of the Chicago commuting zone during the sample period (1996–2012). Children's incomes are measured at the household (i.e., family) level at age 30; parents' incomes are defined as mean family income from 1996 to 2000. Children are assigned ranks based on their incomes relative to all other children in their birth cohort. Parents' are assigned ranks based on their incomes relative to other parents of children in the same birth cohort.

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, $\bar{y}_{25,c,1980} = 40.1$ for children growing up at the 25th percentile of the national income distribution and $\bar{y}_{75,c,1980} = 59.3$ 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

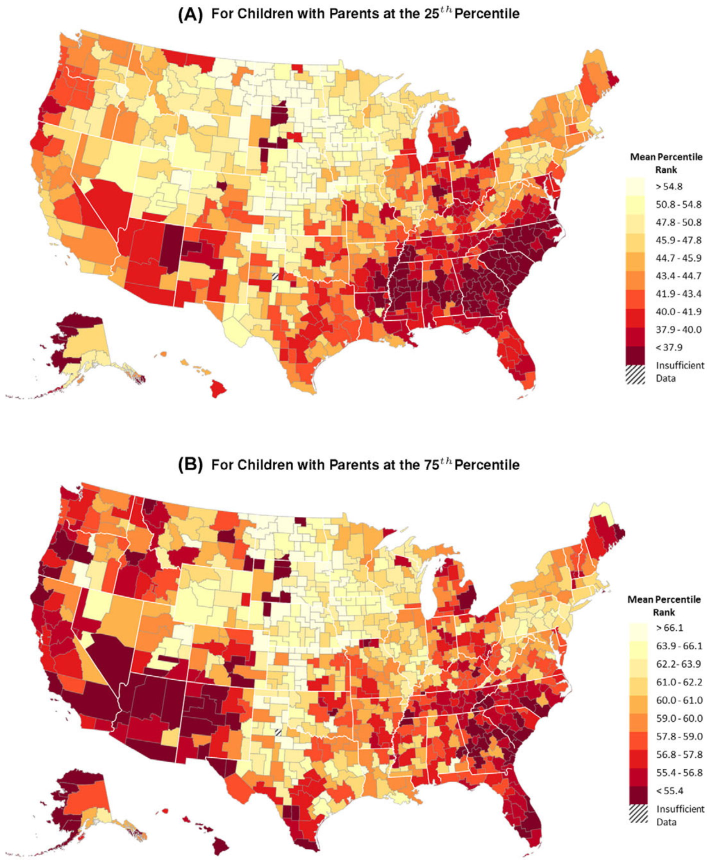

FIGURE II

Mean Income Ranks for Children of Permanent Residents

These maps plot children's mean percentile ranks at age 30 conditional on having parents at the 25th percentile (Panel A) and 75th percentile (Panel B). The maps are constructed by grouping CZs into 10 deciles and shading the areas so that lighter colors correspond to higher outcomes for children. Areas with fewer than 10 children, for which we have insufficient data to estimate outcomes, are shaded with the striped pattern. The sample includes all children in the 1980 birth cohort in our analysis sample whose parents are permanent residents (i.e., whose parents do not move across CZs between 1996 and 2012). To construct these estimates, we first regress children's family income ranks on a constant and their parents' family income ranks separately for each CZ and birth cohort. We then define the predicted income rank for children with parents at percentile p in CZ c in birth cohort s ($\bar{y}_{pcs}$) as the intercept + p times the slope of this regression. Panel A reports the predicted child rank for parents at $p = 25$, which corresponds to an annual household income of \$30,000. Similarly, Panel B reports the predicted child rank for parents at $p = 75$, which corresponds to an annual household income of \$97,000. See notes to Figure I for details on definitions of parent and child income ranks.

75th percentile (Panel B); analogous maps at the county level are presented in Online Appendix Figure I. We construct these maps by dividing CZs into deciles based on their estimated value of $\bar{y}_{25,c,s}$ and $\bar{y}_{75,c,s}$, with lighter colors representing deciles with higher mean incomes. As documented by Chetty et al. (2014), children's incomes vary substantially across CZs, especially for children from low-income families. Chetty et al. (2014, Section V.C) discuss 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 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 to estimate 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, based on the permanent residents' outcomes in CZ d ($\bar{y}_{pds}$), can be written as

$$y_i = \alpha_m + \beta_m \bar{y}_{pds} + \theta_i, \tag{3}$$

where the error term θ_i captures family inputs and other determinants of children's outcomes. Since random assignment guarantees that θ_i is orthogonal to $\bar{y}_{pds}$, estimating equation (3) using OLS yields a coefficient β_m that represents the mean impact of spending year m of one's childhood onward in an area where permanent residents have 1 percentile better outcomes. We define the exposure effect at age m as $\gamma_m = \beta_m - \beta_{m+1}$.[18] Note that if income y_i is measured at age T, $\beta_m = 0$ for $m > T$, as moving

18. For simplicity, we do not allow β_m to vary across parent income percentiles p in our baseline analysis, thereby estimating the average exposure effect across families with different incomes. We estimate equation (3) separately by parental income level in Online Appendix Table III.

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 prior experimental evidence. Second, $\{\gamma_m\}$ is 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 a 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 versus selection. If place effects are homogeneous across children within birth cohorts and parent income groups, $\beta_0 = 0$ would imply that all of the variation across areas is due to selection, and $\beta_0 = 1$ would imply that all of the variation reflects causal effects of place. More generally, the magnitude of β_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 β_0 sheds light on the causal effect of places on average, we caution that 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 based on permanent residents' outcomes ($\alpha + \beta_0 \bar{y}_{pds}$) because selection and causal effects will vary across areas. We build on the methodology developed in this article to estimate the causal effect of each CZ and county in the second article in this series (Chetty and Hendren 2018a).

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 θ_i in equation (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 equation (3) in an observational sample of families who move exactly once yields a regression coefficient

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

where $\delta_m = \frac{cov(\theta_i, \bar{y}_{pds})}{var(\bar{y}_{pds})}$ 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 a child's potential outcomes. Instead, it requires that when people move to better versus worse areas is orthogonal to a child'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 obtain consistent estimates of exposure effects $\gamma_m = \beta_m - \beta_{m+1} = b_m - b_{m+1}$ from equation (3) even in observational data because the selection effect δ cancels out when estimating the exposure effect. We can estimate the selection effect δ itself by examining the outcomes of children whose families move after their income is measured, for example, at age $a \geqslant 30$ 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 β_0, the causal effect of growing up from birth in an area with 1 percentile better outcomes.

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 $\{\gamma_m\}$. We begin by presenting a set of semiparametric estimates of $\{\gamma_m\}$ using specifications 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. Finally, we present a set of supplementary results that shed light on the mechanisms through which neighborhoods affect children's outcomes.

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 our 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 minimize sampling error in the estimates of permanent residents' outcomes $\bar{y}_{pds}$. We show that the findings are robust to alternative cutoffs for population size and move distance in Online Appendix A and present estimates that include families who move more than once in Online Appendix B.

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 twenties (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 data set.[19] Fortunately, measuring income at earlier ages (from 24 to 30) turns out not to affect the exposure effect estimates. The reason is that our estimates of b_m are identified by correlating the incomes of children who move with the incomes of permanent residents in the destination at the same age in adulthood. This property of our estimator allows us to measure the degree of convergence of movers' outcomes to permanent residents' outcomes at any age, irrespective of whether children's incomes at that age reflect their permanent incomes. For example, if a given area c sends many children to college and therefore generates relatively low incomes at age 24, we would obtain a higher estimate of b_m if a child who moves to area c has a lower level of income at age 24. We therefore measure income at age 24 in our baseline specifications to estimate exposure effects for the broadest range of ages.[20]

IV.A. Semiparametric Estimates

To begin, consider the set of children whose families moved when they were exactly m years old. We analyze how these children's incomes in adulthood are related to those of

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

20. We show below that we obtain similar estimates when measuring income at later ages (from 26 to 30) over the overlapping range of ages at which children move. We do not study income before age 24 because we find that exposure effects persist until age 23 when income is measured at any point between 24 and 30.

permanent residents in their destination CZ using the following linear regression:

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

where y_i denotes the child's income rank at age 24, α_{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 equation (3) that we would ideally estimate in experimental data.[21]

Figure III presents a nonparametric binned scatter plot corresponding to the regression in equation (4) for children who move at age $m = 13$. To construct the figure, we first demean both y_i and Δ_{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: $y_i^r = y_i - E[y_i|q, o, s]$ and $\Delta_{odps}^r = \Delta_{odps} - E[\Delta_{odps}|q, o, s]$. We then divide the Δ_{odps}^r residuals into 20 equal-size groups (ventiles) and plot the mean value of y_i^r versus the mean value of Δ_{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 Δ_{odps} is linear. The regression coefficient of $b_{13} = 0.615$, estimated in the microdata using equation (4), implies that a 1 percentile increase in $\bar{y}_{pds}$ is associated with a 0.615 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 specification:

$$(5) \qquad y_i = \alpha_{qosm} + \sum_{m=9}^{30} b_m I(m_i = m)\Delta_{odps} + \sum_{s=1980}^{1987} \kappa_s I(s_i = s)\Delta_{odps} + \varepsilon_{2i},$$

21. We use parent income deciles rather than percentiles to define the fixed effects α_{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 Δ_{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 equation (3), up to the approximation error from using parent deciles instead of exact percentiles.

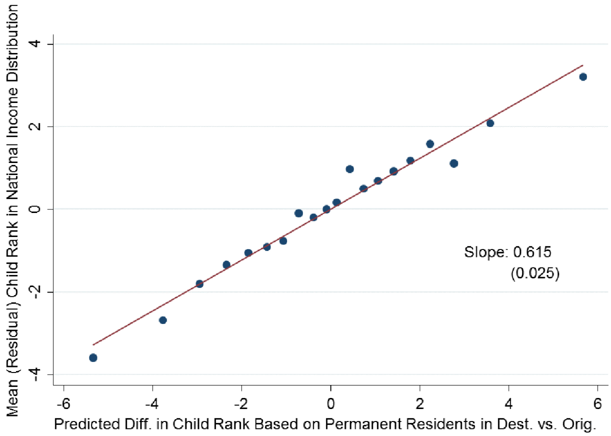

FIGURE III

Movers' Outcomes versus Predicted Outcomes Based on Permanent Residents in Destination

This figure presents a binned scatter plot depicting the relationship between the income ranks of children who moved to a different CZ at age 13 and the differences in the outcomes of permanent residents in the destination versus origin CZ. The sample includes all children in the 1980–1988 birth cohorts whose parents moved when the child was 13 years old and moved only once between 1996 and 2012. Children's family income ranks y_i are measured at age 24. Permanent residents' predicted ranks for each parent income percentile p, CZ c, and birth cohort s ($\bar{y}_{pcs}$) are constructed using the methodology described in the notes to Figure I. To construct the figure, we demean both y_i and $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$ 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: $y_i^r = y_i - E[y_i|q,o,s]$ and $\Delta^r_{odps} = \Delta_{odps} - E[\Delta_{odps}|q,o,s]$. We then divide the Δ^r_{odps} residuals into 20 equal-size groups (ventiles) and plot the mean value of y_i^r versus the mean value of Δ^r_{odps} in each bin. The slope of the best-fit line, which corresponds to b_{13} in equation (4), is estimated using an OLS regression on the underlying microdata, with standard errors in parentheses.

where α_{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 equation (4) by fully interacting the age at move m with the independent variables in equation (4). In addition, we permit the effects of Δ_{odps} to vary across birth cohorts (captured by the κ_s coefficients) because our ability to measure parents' 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 Δ_{odps} for earlier birth cohorts, which can confound our estimates of b_m because the distribution of ages at move is unbalanced across cohorts (see Online Appendix A for further details). By including cohort interactions, we identify $\{b_m\}$ from within-cohort variation in ages at move.[22]

Figure IV, Panel A plots estimates of b_m from equation (5). 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 income at 24. Families who move to better areas have children with better unobservable attributes. The degree of selection δ_m does not vary significantly with m above age 24: regressing b_m on m for $m \geqslant 24$ yields a statistically insignificant slope of 0.001 (std. err. = 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 δ_m for $m \geqslant 24$ is $\delta = 0.126$, that is, 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 horizontal line in Figure IV, Panel A and thereby identify causal exposure effects at earlier ages.

This leads to the second key pattern in Figure IV, Panel A, 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, that is, that moving to a better area earlier in childhood generates larger long-term gains.[23] The linearity of the relationship between b_m and the age at move m in Figure IV, Panel A below age 24 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 $\gamma = 0.044$ (std. err. = 0.003). That is, the outcomes of children who move converge to

22. To avoid collinearity, we omit the most recent cohort interaction with Δ_{odps} (the 1988 cohort when income is measured at age 24). We show below that these cohort interactions have little impact on the estimates obtained from equation (5), but play a larger role in specifications that include family fixed effects.

23. This declining pattern could also potentially be generated by critical age effects rather than effects that operate in proportion to exposure time. We present evidence in Section IV.C supporting the interpretation of these results as exposure effects.

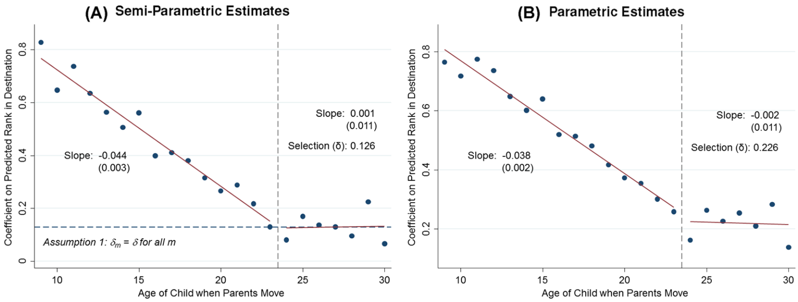

FIGURE IV

Childhood Exposure Effects on Income Ranks in Adulthood

Panel A plots estimates of the coefficients $\{b_m\}$ versus the child's age when the parents move (m) using the semiparametric specification in equation (5), measuring children's incomes at age 24. The sample includes all children in the primary analysis sample whose parents moved exactly once between 1996 and 2012. The $\{b_m\}$ coefficients can be interpreted as the effect of moving to an area where permanent resident outcomes are 1 percentile higher at age m. They are estimated by regressing the child's income rank in adulthood y_i on $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$, the difference between permanent residents' predicted ranks in the destination versus the origin, interacted with each age of the child at the time of the move m. We include origin CZ by parent income decile by birth cohort by age at move fixed effects when estimating this specification. Panel B plots estimates from the parametric specification in equation (6), measuring children's incomes at age 24. This specification replicates the specification used in Panel A, replacing the fixed effects with indicators for the child's age at the time of the move interacted with parent income rank and predicted outcomes for permanent residents in the origin interacted with birth cohort fixed effects. The dashed vertical lines separate the data into two groups: age at move $m \leqslant 23$ and $m > 23$. Best-fit lines are estimated using unweighted OLS regressions of the $\{b_m\}$ coefficients on m separately for $m \leqslant 23$ and $m > 23$. The slopes of these regression lines are reported along with standard errors (in parentheses) on the left side of each panel for $m \leqslant 23$ and on the right side for $m > 23$. The magnitudes of the slopes for $m \leqslant 23$ represent estimates of annual childhood exposure effects. The parameter δ is defined as the mean value of the b_m estimates for $m > 23$; this parameter represents a selection effect because moves after age 24 cannot affect income measured at age 24. In Panel A, the dashed horizontal line shows the value of the selection effect δ; the identification assumption underlying the analysis is that the selection effect δ does not vary with the child's age at move m.

the outcomes of permanent residents of the destination area at a rate of 4.4% per year of exposure until age 23.[24]

24. Figure IV, Panel A 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 II presents estimates of b_m identified from variation in origins by replacing the origin (α_{qosm}) fixed effects in equation (5) with destination (α_{qdsm}) fixed effects. The resulting estimates yield

Because some children do not move with their parents, the estimates of b_m in equation (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 children who move with their parents by inflating the ITT estimates by the fraction of children who moved with their parents at each age m.[25] In Online Appendix Figure III, 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 and 23. Because the treatment effects converge toward zero as the age at move approaches 23, inflating the coefficients by $\frac{1}{0.6}$ at later ages has little impact on exposure effect estimates.

IV.B. Parametric Estimates

Equation (5) includes more than 200,000 fixed effects (α_{qosm}), making it difficult to estimate in smaller samples and introduce additional controls such as family fixed effects. As a more tractable alternative, we estimate a model in which we control parametrically for the two key factors captured by the α_{qosm} fixed effects: (i) the quality of the origin location, which we model by interacting the predicted outcomes for permanent residents in the origin at parent income percentile p_i with birth cohort fixed effects, and (ii) 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

a qualitative pattern that is the mirror image of those in Figure IV, Panel A: 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 0.030 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.

25. We identify children who move with their parents based on whether they ever file a tax return, receive a W-2 form, or attend a college in the destination CZ.

leads to the following regression specification:

$$y_i = \sum_{s=1980}^{1988} I(s_i = s)\left(\alpha_s^1 + \alpha_s^2 \bar{y}_{pos}\right) + \sum_{m=9}^{30} I(m_i = m)\left(\zeta_m^1 + \zeta_m^2 p_i\right)$$

$$(6) \qquad + \sum_{m=9}^{30} b_m I(m_i = m)\Delta_{odps} + \sum_{s=1980}^{1987} \kappa_s^d I(s_i = s)\Delta_{odps} + \varepsilon_{3i}.$$

The first two terms of this specification control for origin quality and disruption effects. The third term represents the exposure effects of interest, and the fourth consists of cohort interactions with Δ_{odps} to control for differential measurement error across cohorts, as in equation (5).[26]

Figure IV, Panel B plots the coefficients $\{b_m\}$ obtained from estimating equation (6). The coefficients are very similar to those obtained from the more flexible specification used to construct Figure IV, Panel A. Regressing the b_m coefficients on m for $m \leqslant 23$, we obtain an average annual exposure effect estimate of $\gamma = 0.038$ (std. err. = 0.002). This 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 income or because we measure income at that point. In Online Appendix Figure IV, we replicate the analysis measuring income at ages 26, 28, and 30 in addition to age 24. All of these 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

26. In addition to having many fewer fixed effects, this specification uses variation in both the quality of the origin ($\bar{y}_{pos}$) and the destination ($\bar{y}_{pds}$) to identify $\{b_m\}$. In contrast, the semiparametric model in equation (5) is identified purely from variation in destinations because it includes origin fixed effects. Estimating a parametric model that identifies $\{b_m\}$ from variation in destinations by controlling for outcomes of permanent residents in the origin interacted with the age of the child at the time of the move ($\sum_{m=9}^{30} b_m I(m_i = m) y_{pos}$) yields very similar estimates.

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 income in subsequent years.

The kink at age 23 motivates the baseline regression specification that we use for the rest of our analysis. We parameterize both the exposure and selection effects shown in Figure IV linearly, replacing the nonparametric $\sum_{m=9}^{30} b_m I(m_i = m)\,\Delta_{odps}$ term in equation (6) with two separate lines above and below age 23:

$$\begin{aligned} y_i = {} & \sum_{s=1980}^{1988} I(s_i = s)\left(\alpha_s^1 + \alpha_s^2 \bar{y}_{pos}\right) + \sum_{m=9}^{30} I(m_i = m)\left(\zeta_m^1 + \zeta_m^2 p_i\right) \\ & + \sum_{s=1980}^{1987} \kappa_s^d I(s_i = s)\Delta_{odps} + I(m_i \leqslant 23)\left(b_0 + (23 - m_i)\gamma\right)\Delta_{odps} \\ & + I(m_i > 23)(\delta + (23 - m_i)\delta')\Delta_{odps} + \varepsilon_{3i}. \end{aligned} \tag{7}$$

Estimating this specification directly in the microdata yields an average annual exposure effect $\gamma = 0.040$ (std. err. $= 0.002$), as shown in column (1) of Table II.[27]

The estimates of γ are robust to alternative specifications and sample definitions. Columns (2) and (3) of Table II show that estimating γ using data only up to age 18 or 23—that is, excluding the data at older ages that identifies the selection effect in equation (7)—yields similar estimates of γ. Column (4) shows that excluding the cohort interactions, $\sum_{s=1980}^{1988} I(s_i = s)\alpha_s^2 \bar{y}_{pos}$ and $\sum_{s=1980}^{1987} \kappa_s^d I(s_i = s)\Delta_{odps}$, in equation (7) does not affect the estimate of γ significantly. Column (5) shows that we obtain an estimate of $\gamma = 0.041$ (std. err. $= 0.002$) when we measure movers'

27. This coefficient differs slightly from the coefficient of $\gamma = 0.038$ that we obtain when regressing the coefficients b_m on m in Figure IV, Panel B 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. The standard error in this and all subsequent specifications is also obtained from the regression in the microdata. To simplify computation, we report conventional (unclustered) standard errors. Clustering standard errors by family to account for correlated outcomes across siblings does not affect the standard errors appreciably. In addition, regressing the estimates of b_m on m in Figure IV, which is analogous to clustering the standard errors by the age at move, also yields a standard error of 0.002, showing that our inferences are not sensitive to the way in which standard errors are computed.

TABLE II
CHILDHOOD EXPOSURE EFFECT ESTIMATES

	Dependent variable: Child's income rank at age 24								
							With family fixed effects		
Specification:	Pooled	Age $\leqslant$ 23	Age < 18	No cohort controls	Individual income	Child CZ FE	Baseline	No cohort controls	Time-varying controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Exposure effect (γ)	0.040 (0.002)	0.040 (0.002)	0.037 (0.005)	0.036 (0.002)	0.041 (0.002)	0.031 (0.002)	0.044 (0.008)	0.031 (0.005)	0.043 (0.008)
Num. of obs.	1,553,021	1,287,773	687,323	1,553,021	1,553,021	1,473,218	1,553,021	1,553,021	1,553,021

Notes. This table reports estimates of annual childhood exposure effects on children's income ranks at age 24 (γ). The estimates can be interpreted as the impact of spending an additional year of childhood in a CZ where children of permanent residents have 1 percentile point higher income ranks at age 24. Standard errors are shown in parentheses. Each column reports estimates from a regression of a child's income rank at age 24 on the difference between permanent residents' predicted ranks in the destination versus the origin, interacted with the age of the child at the time of the move (m). We permit separate linear interactions for $m \leqslant 23$ and $m > 23$ and report the coefficient on the interaction for $m \leqslant 23$. Each regression also includes additional controls specified in equation (7). Permanent residents' predicted ranks are constructed using linear regressions of children's ranks on parents' ranks in each CZ and birth cohort, as shown in Figure I. Column (1) reports the estimate of γ from equation (7) using all children in the primary analysis sample of one-time movers, defined in the notes to Table I (Panel C). Columns (2) and (3) restrict the sample to those who move at or before age 23 or 18. In column (4), we exclude the cohort interactions with the predicted outcomes of permanent residents in the origin and destination location and instead include a single control for the predicted outcomes of permanent residents in the origin. Column (5) replicates column (1), using individual income ranks (rather than household income ranks) to measure both the child's outcome and the predicted outcomes of permanent residents in the origin and destination. Column (6) adds fixed effects for the child's CZ in 2012 to the specification in column (1); note that this specification has a slightly smaller number of observations because ZIP code information is missing for some children in 2012 (e.g., a child with no earnings or taxable income). Column (7) adds family fixed effects to the baseline specification in column (1). Column (8) adds family fixed effects to the specification in column (4) that does not include cohort-varying intercepts. Column (9) adds controls for changes in parental marital status and income rank in the year before versus after the move, along with their interactions with the age of the child at the time of the move and indicators for moving above and below age 23, to the specification in column (7).

income ranks y_i and permanent residents' income ranks $\bar{y}_{pcs}$ at the individual rather than household level.

We replicate the analysis in Table II at the county level in Online Appendix Table V. We obtain slightly smaller exposure effect estimates of $\gamma \simeq 0.035$ at the county level, indicating that selection effects account for a larger fraction of the variance in permanent residents' outcomes at smaller geographies. This is intuitive, as families are more likely to sort geographically (e.g., to better school districts) within rather than across labor markets.

IV.C. Mechanisms

In this subsection, we present a set of additional specifications that shed light on the mechanisms through which neighborhoods affect children's outcomes.

We begin by distinguishing the role of childhood environment from differences caused by variation in labor market conditions or local costs of living across areas. In column (6) of Table II, we add fixed effects for the CZ in which the child lives at age 24 (when income is measured) to the baseline model. This specification compares the outcomes of children who live in the same labor market in adulthood but grew up in different neighborhoods. We obtain an annual exposure effect of $\gamma = 0.031$ in this specification, indicating that the majority of the exposure effect in our baseline specification is driven by differences in exposure to a better childhood environment, holding fixed labor market conditions.[28] This conclusion is consistent with the fact that moving to an area where permanent residents have higher income just before entering the labor market (e.g., in one's early twenties) has little effect on income, as shown in Figure IV.

Next, we examine heterogeneity in exposure effects across subsamples (Online Appendix Table III). Standard models of learning predict that moving to a better area will improve outcomes but moving to a worse area will not. In practice, the exposure effect for negative moves is larger than for positive moves: $\gamma = 0.030$ for moves to better CZs ($\Delta_{odps} > 0$), while $\gamma = 0.040$ for

28. This specification likely overadjusts for differences in labor market conditions and underestimates γ because the CZ in which the child resides as an adult is itself an endogenous outcome that is likely related to the quality of a child's environment. For example, one of the effects of growing up in a good area may be an increased probability of getting a high-paying job in another city.

moves to worse CZs ($\Delta_{odps} < 0$).[29] Spending part of one's childhood in a good neighborhood does not make a child immune to subsequent deterioration in his or her neighborhood environment. We also find slightly larger exposure effects for children from above-median income families relative to below-median income families ($\gamma = 0.047$ versus $\gamma = 0.031$).

Finally, we distinguish between two different mechanisms that could explain why moving to a better area at a younger age is more beneficial: exposure effects—the mechanism we have focused on above—and critical age effects. Critical age (or critical period) models predict that the impacts of moving to a different neighborhood vary with children's ages (e.g., Lynch and Smith 2005). For example, suppose that moving to a better neighborhood improves a child's network of friends with a probability that falls with the age at move and that once one makes new contacts, they last forever. In this model, neighborhood effects would decline with a child's age at move (as in Figure IV), but the duration of exposure to a better area would not matter for long-term outcomes. Alternatively, if better neighborhoods offer a positive treatment (such as better schooling) in each year of childhood, the key determinant of outcomes would be the total duration of exposure rather than the specific age at which a child moves. Distinguishing between these mechanisms can be important for policy: the critical age view calls for improving children's environments at certain key ages, while the exposure view calls for a sustained improvement in environment throughout childhood.

A critical age model cannot be distinguished from an exposure effect model in a sample of one-time movers because a child's age at move is perfectly collinear with his or her duration of exposure to the new area. However, this collinearity is broken when families move multiple times. Intuitively, one can distinguish between the critical age and exposure mechanisms by considering children who move to an area with better permanent residents' outcomes $\bar{y}_{pds}$ but then move back to the place where they started. In this case, the exposure model predicts that children will experience gains that are proportional to the number of years they spent in the

29. Moreover, roughly an equal fraction of families with children move to CZs with better versus worse outcomes; 48.7% move to CZs with $\Delta_{odps} > 0$. This contrasts with sorting models suggesting families with children would tend to sort to CZs that produce better outcomes.

destination CZ, whereas the critical age model predicts that the gain will depend only on the age at which the child first moves to the new area.

To implement this analysis, we first generalize the specification in equation (7) to include families who move more than once by replacing the Δ_{odps} terms with a duration-weighted measure of exposure to different areas over childhood (see Online Appendix B for details). This multiple-movers specification yields an annual exposure effect estimate of $\gamma = 0.035$ (std. err. $= 0.001$) (Online Appendix Table IV, column (2)). We test between the critical age and exposure mechanisms by controlling for the age of the child at the time of each move j interacted with the change in permanent residents' outcomes ($\Delta_{od(j)ps}$). This specification, which isolates variation in exposure that is orthogonal to the ages at which children move, yields an exposure effect estimate of $\gamma = 0.034$ (std. err. $= 0.006$) (Online Appendix Table IV, column (4)). The similarity between these estimates implies that what matters for children's incomes in adulthood is the total time spent in a given area (exposure) rather than the age at which one arrives in that area.[30]

IV.D. Summary

Under our key identification assumption (Assumption 1), the empirical results in this section yield three lessons. First, place matters: children who move at earlier ages to areas where prior residents have higher incomes earn more themselves as adults. Second, place matters in proportion to the duration of childhood exposure. Every year of exposure to a better area during childhood contributes to higher income in adulthood. Third, each year of childhood exposure matters roughly equally. The returns to growing up in a better neighborhood persist well beyond early childhood.

30. Critical age effects have been most widely documented in linguistic patterns and anthropometric measures (e.g., Singleton and Ryan 2004; Bleakley and Chin 2004; van den Berg et al. 2014). One potential explanation for why we do not find evidence of critical age effects here is that we focus on U.S. natives, for whom learning English is presumably less of an issue. Neighborhoods do have causal effects on more nuanced linguistic patterns, such as the use of vernacular English by African Americans (Rickford et al. 2015). Our findings are consistent with such cultural assimilation mechanisms insofar as they are exposure-dependent.

All of these conclusions rest on the assumption that selection effects do not vary with the child's age at move. We evaluate the validity of this assumption in the next section.

V. Validation of Baseline Design

We assess the validity of our key identifying assumption—that the potential outcomes of children who move to better versus worse areas do not vary with the age at which they move—using a series of tests that focus on different forms of selection and omitted variable bias. To organize the analysis, we partition the unobserved determinant of children's outcomes, represented by θ_i in equation (3), 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 may vary over time within families, such as parents' jobs.

We implement four tests for bias in this section. First, we address bias due to selection on fixed family factors $\bar{\theta}_i$ by comparing siblings' outcomes. Second, we control for changes in parents' income and marital status, two key time-varying factors $\tilde{\theta}_i$ that we observe in our data. Our remaining tests focus on unobservable time-varying factors, such as changes in wealth, that may have triggered a move to a better area. In our third set of tests, we isolate moves that occur due to displacement shocks that induce many families to move. Finally, we conduct a set of outcome-based placebo (overidentification) tests of the exposure effect model, exploiting heterogeneity in permanent residents' outcomes across subgroups to generate sharp testable predictions about how children's outcomes should change when they move to different areas. In our view, this last approach, although least conventional, provides the most compelling evidence that the identifying assumption holds and that neighborhoods have causal exposure effects on children's long-term outcomes.

V.A. Sibling Comparisons

If families with better unobservables (higher $\bar{\theta}_i$) move to better neighborhoods at earlier ages, Assumption 1 would be violated and our estimated exposure effect $\hat{\gamma}$ would be biased upward. We control for differences in such family-level factors $\bar{\theta}_i$ by including family fixed effects when estimating equation (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. When including family fixed effects, the exposure effect γ is identified by the extent to which the difference in siblings' outcomes, $y_1 - y_2$, covaries with the difference in their ages interacted with the change in permanent residents' outcomes, $(m_1 - m_2)\Delta_{odps}$.

Figure V, Panel A replicates Figure IV, Panel B, adding family fixed effects to equation (6). The linear decline in the estimated values of b_m until age 23 is very similar to that in the baseline specification. Children who move to a better area at younger ages have better outcomes than do their older siblings. Regressing the b_m coefficients on m for $m \leqslant 23$ yields an average annual exposure effect estimate of $\gamma = 0.043$ (std. err. $= 0.003$), very similar to our estimates above.

The selection effect (i.e., the level of b_m after age 24) falls from $\delta = 0.23$ in the baseline specification to $\delta = 0.01$ (not significantly different from 0) with family fixed effects.[31] Family fixed effects thus reduce the level of the b_m coefficients by accounting for differential selection in which types of families move to better versus worse areas, but do not affect the slope of the b_m coefficients. This is precisely what we should expect if selection effects in where families choose to move do not vary with children's ages when they move, as required by Assumption 1.

Table II, Column (7) shows that adding family fixed effects to the linear specification in equation (7) and estimating the model directly on the micro data yields an estimate of $\gamma = 0.044$. Other variants of this regression specification, analogous to those in columns (2)–(6) of Table II, all yield very similar estimates of γ, with one exception: excluding the cohort interactions with $\bar{y}_{pos}$ and Δ_{odps}, as in column (4), yields $\gamma = 0.031$ (Table II, column (8)). The reason that the estimate of γ falls in this specification is that we observe children's origin locations for fewer years in earlier birth cohorts, as discussed in Section IV.A. The missing data on origins increases the level of the selection effect δ in earlier cohorts (see Online Appendix A). Because we only observe moves at older ages for children in earlier cohorts, these differences across cohorts induce a positive correlation between δ_m and m, biasing our estimate of γ downward. This bias is magnified in the specifications with family fixed effects because they are identified purely by comparing the outcomes of children in different

31. δ is identified even with family fixed effects because Δ_{odps} varies across birth cohorts.

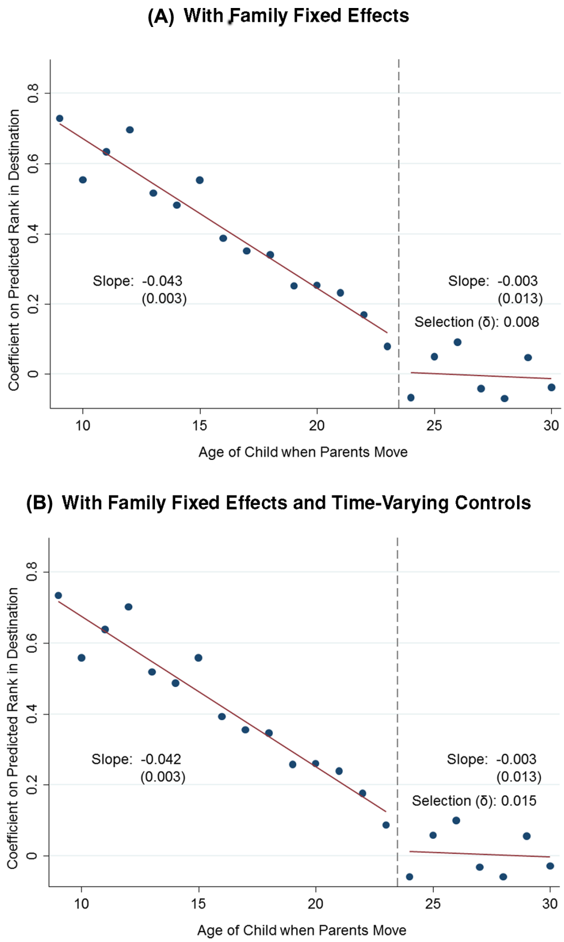

FIGURE V

Childhood Exposure Effects on Income Ranks with Additional Controls

This figure replicates Figure IV, Panel B using specifications analogous to equation (6) that include family fixed effects (Panel A) and family fixed effects and controls for changes in marital status and parental income around the time of the move (Panel B). To control for changes in parental income, we construct parental income ranks by child's birth cohort and calendar year. We interact the differences in parental ranks in the year before versus after the move with the child's age at the time of the move, along with interactions with indicators for moving before versus after age 23. To control for changes in marital status, we construct indicators for being always married, getting divorced, or being never married in the year before the move and the year after the move (getting married is the omitted category). We then interact these marital status indicators with the child's age at the time of the move along with interactions with indicators for moving above versus below age 23. See notes to Figure IV for additional details on the construction of the figure.

birth cohorts, whereas our baseline specifications also compare children in the same birth cohort whose parents move at different times. Including cohort interactions with Δ_{odps} eliminates this bias by permitting a separate selection term δ for each cohort.[32]

In sum, we continue to find childhood exposure effects of $\gamma \simeq 0.04$ when comparing siblings' outcomes, implying that our design is not confounded by differences in the types of families who move to better areas when their children are younger.

V.B. Controls for Time-Varying Observables

The research design in Figure V, Panel A accounts for bias due to fixed differences in family inputs $\bar{\theta}_i$, but it does not account for time-varying inputs $\tilde{\theta}_i$. 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 β upward even with family fixed effects.

Prior research has focused on changes in parents' income and marital status as two key factors that may induce moves and also directly affect children's outcomes in adulthood (e.g., Jencks and Mayer 1990). We can directly control for these two time-varying factors in our data, as we observe parents' incomes and marital status in each year from 1996 to 2012. We control for the effects of changes in income around the move when estimating equation (6) by including controls for the change in the parent's income rank from the year before to the year after the move interacted with indicators for the child's age at move. The interactions with age at move permit the effects of income changes to vary with the duration of childhood exposure to higher versus lower levels of parent income. Similarly, we control for the impact of changes in marital status by interacting indicators for each of the four possible changes in the mother's marital status in the year before versus after the move (married to unmarried, unmarried to married, unmarried to unmarried, and married to married) with indicators for the child's age at move.

Figure V, Panel B replicates Panel A, controlling for all of these variables in addition to family fixed effects. Controlling for changes in parent income and marital status has little effect on

32. The attenuation bias in γ is further amplified in CZs with smaller populations, where Δ_{odps} is measured with greater error (see Online Appendix A and Appendix Table VI).

the estimates of $\{b_m\}$. The estimates of $\gamma = 0.042$ and $\delta = 0.015$ are virtually identical to those when we do not control for these time-varying factors. Table II, column (9) confirms that including these controls in a linear regression estimated on the micro data yields similar estimates.

These results show that changes in income and family structure are not a significant source of bias in our design. However, other unobserved factors could still be correlated with moving to a better or worse area in a manner that generates omitted variable bias. The fundamental identification problem is that any unobserved shock that induces child *i*'s family to move to a different area could be correlated with parental inputs θ_i. These changes in parental inputs could potentially increase the child's income 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 both move to a better neighborhood and 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 factors.

V.C. Displacement Shocks

One approach to accounting for unobservable shocks is to identify moves where we have some information about the shock that precipitated the move. Suppose we identify 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. Such displacement shocks can induce differential changes in neighborhood quality as measured by permanent residents' outcomes (Δ_{odps}). For instance, Hurricane Katrina displaced families from New Orleans (an area with relatively poor outcomes compared with surrounding areas), leading to an increase in average neighborhood quality for displaced families ($\Delta_{odps} > 0$). In contrast, Hurricane Rita hit Houston, an area with relatively good outcomes, and may have reduced neighborhood quality ($\Delta_{odps} < 0$). If these displacement shocks do not have direct exposure effects on children that are correlated with Δ_{odps}—for example, the direct effects of the disruption induced by hurricanes does not covary with neighborhood quality changes—then Assumption 1 is satisfied and we obtain unbiased estimates of the exposure effect γ by focusing on displaced families. Conceptually, by isolating a subset of moves caused by known exogenous shocks, we can more credibly ensure that changes in children's outcomes are not driven by unobservable factors.[33]

To operationalize this approach, we first 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 sample of one-time movers and $\bar{K}_z$ denote mean outflows between 1996 and 2012. We define the shock to outflows in year t in ZIP z as $k_{zt} = \frac{K_{zt}}{\bar{K}_z}$.[34]

Though many of the families who move in subsamples with large values of k_{zt} do so for exogenous reasons, their destination d is still 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 reduce potential biases arising from the endogenous choice of destinations, we isolate variation arising from the average change in neighborhood quality for individuals who are displaced. Let $E[\Delta_{odps}|q, z]$ denote the difference in the mean predicted outcome in the destination CZs relative to the origin CZ for individuals in origin ZIP code z and parent income decile q (averaging over all years in the sample, not just the year of the shock). We instrument for the difference in predicted outcomes in each family's destination relative to origin (Δ_{odps}) with $E[\Delta_{odps}|q, z]$ and estimate the linear specification in equation (7) using 2SLS to identify the exposure effect, γ_{IV}.[35]

Figure VI presents the results of this analysis. To construct this figure, we take ZIP-year cells with above-median outflows

33. This research design is related to Sacerdote's (2012) analysis of the effects of Hurricanes Katrina and Rita on student test score achievement. Although we use similar variation, we do not focus on the direct effects of the displacement itself, but on how children's long-term outcomes vary in relation to the outcomes of permanent residents in the destination to which they were displaced.

34. Searches of historical newspaper records for cases with the highest outflow rates k_{zt} reveal that they are frequently associated with events such as natural disasters or local plant closures. Unfortunately, there is insufficient power to estimate exposure effects purely from the events identified in newspapers.

35. This approach does not fully eliminate the scope for selection bias, as biases from the endogenous choice of destinations could persist if there is unobserved heterogeneity across areas experiencing displacement shocks. However, it reduces the scope for selection bias by focusing on moves induced by aggregate displacement shocks and eliminating variation in Δ_{odps} due to individual choice, which is more likely to be correlated with unobservables θ_i than the area-level variation in $E[\Delta_{odps}|q, z]$. By testing if the estimate of γ remains stable when we use an estimator that reduces the scope for selection, we can gauge whether our baseline estimate of γ is biased.

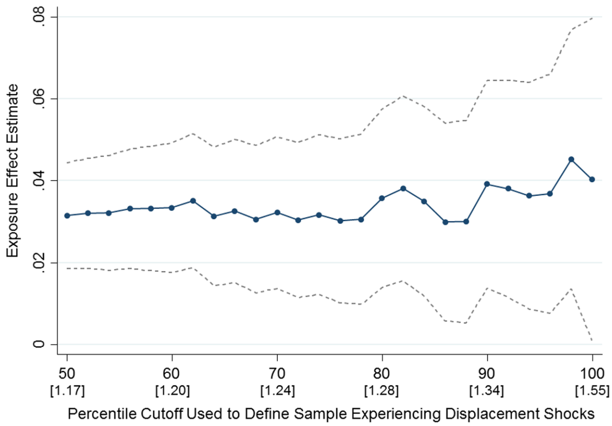

FIGURE VI

Exposure Effect Estimates Using Displacement Shocks

This figure presents estimates of annual childhood exposure effects (γ) for the subset of areas that experience displacement shocks, defined as ZIP code by year cells that have large outflows in the number of residents. We measure outflows by defining K_{zt} as the number of families who leave ZIP code z in year t in our one-time movers sample and $\bar{K}_z$ as mean outflows between 1996 and 2012. We define the shock to outflows in year t in ZIP z as $k_{zt} = \frac{K_{zt}}{\bar{K}_z}$. We then take ZIP-year cells with above-median outflows ($k_{zt} > 1.17$) and divide them into 25 population-weighted bins based on the size of the shock k_{zt}. For each subset of observations with values of k_{zt} above the percentile threshold listed on the x-axis, we estimate γ using equation (7), instrumenting for the change in predicted outcomes based on permanent residents Δ_{odps} with the average change in predicted outcomes for movers from the origin ZIP, $E[\Delta_{odps}|q, z]$. We define $E[\Delta_{odps}|q, z]$ as the mean value of Δ_{odps} for each parental income decile q, pooling across all years and all movers out of ZIP code z. The figure plots the resulting estimates of γ versus the percentile threshold cutoff for the sample. The dashed lines show 95% confidence intervals for the estimates. The mean value of the outflow shock k_{zt} used in each subsample is shown in brackets below the percentile thresholds.

($k_{zt} > 1.17$) and divide them into population-weighted bins based on the size of the shock k_{zt}.[36] The first point in Figure VI shows the 2SLS estimate of the annual exposure effect γ_{IV} using all observations with k_{zt} greater than its median value (1.17). The second point shows the estimate of γ_{IV} using all observations with k_{zt} at or above the 52nd percentile. The remaining points are constructed in the same way, increasing the threshold by two percentiles at each point, with the last point representing an estimate of γ_{IV}

36. To ensure that large outflows are not driven by areas with small populations, we exclude ZIP-year cells with fewer than 10 children leaving in that year.

using data only from ZIP codes in the highest two percentiles of outflow rates. The dotted lines show a 95% confidence interval for the regression coefficients.

If the baseline estimates were driven entirely by selection, γ_{IV} would fall to 0 as we limit the sample to individuals who are more likely to have been induced to move because of an exogenous displacement shock. But the coefficients remain quite stable at $\gamma_{IV} \simeq 0.04$ even when we restrict to moves that occurred as part of large displacements. That is, when we focus on families who move to a better area for what are likely to be exogenous reasons, we continue to find that children who are younger at the time of the move earn more as adults.

These findings support the view that our baseline estimates of exposure effects capture the causal effects of neighborhoods rather than other unobserved factors that change when families move. Moreover, they indicate that the treatment effects of moving to a different area are similar for families who choose to move for idiosyncratic reasons and families who are exogenously displaced. This result suggests that the exposure effects identified by our baseline design can be generalized to a broader set of families beyond those who choose to make a particular move.

V.D. Outcome-Based Placebo Tests

As a final approach to test for bias due to unobservable factors, we implement placebo tests that exploit the heterogeneity in permanent residents' outcomes across subgroups. We exploit variation along three dimensions: birth cohorts, quantiles of the income distribution, and child gender. The causal exposure effect model predicts precise convergence of a child's outcome to permanent residents' outcomes for his or her own subgroup. In contrast, we argue below that omitted variable and selection models would not generate such subgroup-specific convergence under plausible assumptions about parents' information sets and preferences. The heterogeneity in permanent residents' outcomes thus gives us a rich set of overidentifying restrictions to test whether neighborhoods have causal effects.[37] We consider each of the three dimensions of heterogeneity in turn.

1. Birth Cohorts. Although permanent residents' outcomes are generally very stable over time, outcomes in some areas (such as

37. In addition to being useful for identification, these results are also of direct interest in understanding the heterogeneity of place effects across subgroups.

Oklahoma City, OK) have improved over time, while others (such as Sacramento, CA) have gotten worse.[38] Such changes could occur, for instance, because of changes in the quality of local schools or other area-level characteristics that affect children's outcomes. We exploit this heterogeneity across birth cohorts to test for confounds in our baseline research design.

Under the causal exposure effect model, when a child's family moves to destination d, the difference in permanent residents' outcomes $\Delta_{odp,\, s(i)}$ for that child's own birth cohort $s(i)$ should predict his or her outcomes more strongly than the difference in outcomes Δ_{odps} for other cohorts $s \neq s(i)$. Intuitively, what matters for a child's outcome is a neighborhood's quality for his own cohort, not the neighborhood's quality for younger or older cohorts. In contrast, it is unlikely that other unobservables θ_i will vary sharply across birth cohorts s in association with Δ_{odps} because the fluctuations across birth cohorts are realized only in adulthood and thus cannot be directly observed at the time of the move.[39] Therefore, by testing whether exposure effects are predicted by a child's own versus surrounding cohorts, we can assess the importance of bias due to unobservables.

We implement this analysis by estimating the baseline specification in equation (7), replacing the change in permanent residents' outcomes for the child's own cohort, $\Delta_{odp,\, s(i)}$, with analogous predictions for adjacent birth cohorts $s(i) + t$, $\Delta_{odp,\, s(i)+t}$ (see Online Appendix D for details). The series in red triangles in Figure VII (color version online) plots the exposure effect estimates ($\tilde{\gamma}_t$) obtained from these regressions, with t ranging from -4 to 4. The estimates of $\tilde{\gamma}_t$ are similar to our baseline estimate of $\gamma = 0.040$ for the leads and lags, consistent with the high degree of serial correlation in permanent residents' outcomes. The series in blue circles plots analogous coefficients $\tilde{\gamma}_t$ when all the cohort-specific predictions from the four years before to the four years after the child's own cohort are included simultaneously. In this specification, the coefficients on the placebo exposure effects ($\tilde{\gamma}_t$ for $t \neq 0$) are all very close to 0— and not statistically

38. The autocorrelation of $\bar{y}_{pcs}$ with $\bar{y}_{pc,s-1}$ across children's birth cohorts is 0.95 at the 25th percentile of the parent income distribution.

39. For instance, a family that moves with a 10-year-old child will not observe $\bar{y}_{pds}$ for another 14 years (if income is measured at age 24).

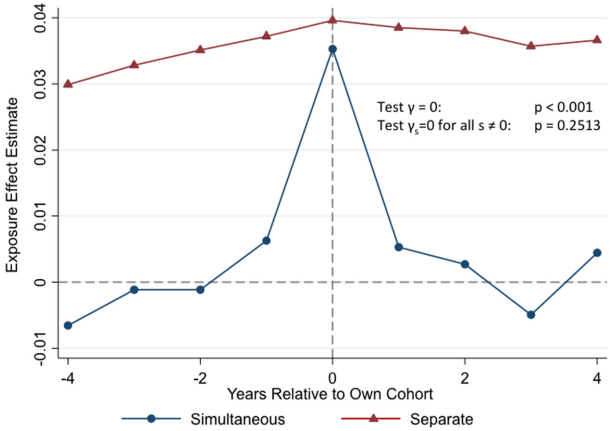

FIGURE VII

Exposure Effect Estimates Based on Cross-Cohort Variation

This figure presents estimates of the annual childhood exposure effect on children's income ranks in adulthood using permanent resident predictions for the child's own birth cohort and surrounding "placebo" birth cohorts. The series in triangles plots estimates of the exposure effect γ_t from nine separate regressions analogous to that in equation (7), using permanent resident predictions from cohort $s + t$ (where t ranges between -4 and 4) as the key independent variables and the outcomes of children in birth cohort s as the dependent variable. By construction, the exposure effect estimate for $t = 0$ (highlighted by the dashed vertical line) corresponds to the baseline estimate of $\gamma = 0.040$ in column (1) of Table II. The series in circles plots estimates from a single multivariable regression that simultaneously includes all nine permanent resident predictions $t = -4, ..., 4$ and plots the coefficient on the interaction of the child's age at the time of the move m with $\Delta_{odp,\, s+t}$, the difference between permanent residents' predicted ranks in the destination versus the origin in cohort $s + t$. The figure also reports p-values from two hypothesis tests: the hypothesis that γ (the estimate using the actual cohort, $t = 0$) equals 0 in the simultaneous specification and the hypothesis that all other coefficients γ_{s+t} excluding the own-cohort coefficient are equal to 0. See Online Appendix D for further details on the regression specifications.

significant.[40] However, the exposure effect estimate for the child's own cohort remains at approximately $\gamma = 0.04$ even when we control for the surrounding cohorts' predictions and is significantly different from the estimates of $\tilde{\gamma}_t$ for $t \neq 0$ ($p < .001$).

The evidence in Figure VII strongly supports the view that the change in children's outcomes is driven by causal effects of exposure to a different place. Intuitively, it is unlikely that a correlated

40. A test of the joint hypothesis that all $\tilde{\gamma}_t = 0$ for all $t \neq 0$ yields a p-value of .251.

shock (such as a change in wealth when the family moves) would covary precisely with cohort-level differences in place effects, as manifested in the outcomes of children of permanent residents. Formally, this test relies on the assumption that if unobservables θ_i are correlated with exposure to a given cohort $s(i)$'s place effect (proxied for by permanent residents' outcomes), they must also be correlated with exposure to the place effects of adjacent cohorts t:

$$(8) \quad Cov(\theta_i, m\Delta_{odp,s(i)}|X) > 0 \Rightarrow Cov(\theta_i, m\Delta_{odpt}|X, m\Delta_{odp,s(i)}) > 0,$$

where X represents the vector of fixed effects and other controls in equation (7). Under this assumption, the findings in Figure VII imply that our estimates of γ reflect causal neighborhood effects (which are cohort-specific) rather than omitted variables, which are not cohort-specific under equation (8).

2. Quantiles: Distributional Convergence. Places differ not only in children's mean outcomes, but also in the distribution of children's outcomes. For example, children who grow up in low-income families in Boston and San Francisco have comparable mean ranks, but children in San Francisco are more likely to end up in the tails of the income distribution than those in Boston. If neighborhoods have causal exposure effects, we would expect convergence in movers' outcomes not just at the mean but across the entire distribution in proportion to exposure time. In contrast, it is less plausible that omitted variables such as wealth shocks would perfectly replicate the distribution of outcomes of permanent residents in each CZ.[41] Therefore, testing for quantile-specific convergence can distinguish the causal exposure effect model from omitted variable explanations.

To implement these tests, we begin by constructing predictions of the probability of having an income in the upper or lower tail of the national income distribution at age 24 for children of permanent residents in each CZ c. In each CZ, we regress an indicator for a child being in the top 10% of the distribution or an indicator for not being employed on parent income rank p using

41. 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. Even with such knowledge, there is no ex ante reason to expect unobserved shocks such as changes in wealth to have differential and potentially nonmonotonic effects across quantiles, in precise proportion to the outcomes in the destination.

TABLE III

CHILDHOOD EXPOSURE EFFECT ESTIMATES: DISTRIBUTIONAL CONVERGENCE

Dep. var:	Upper-tail: child in top decile			Lower-tail: child not employed		
	(1)	(2)	(3)	(4)	(5)	(6)
Distributional prediction	0.043 (0.002)		0.040 (0.003)	0.041 (0.003)		0.043 (0.003)
Mean rank prediction (placebo)		0.024 (0.002)	0.003 (0.003)		0.018 (0.002)	−0.002 (0.003)
Num. of obs.	1,553,021	1,553,021	1,553,021	1,553,021	1,553,021	1,553,021

Notes. This table reports estimates of annual childhood exposure effects (γ) for upper-tail and lower-tail outcomes: being in the top 10% of the cohort-specific income distribution at age 24 or not being employed. Standard errors are shown in parentheses. Column (1) reports estimates from a regression of an indicator for being in the top 10% on the difference between permanent residents' predicted probabilities of being in the upper-tail in the destination versus the origin, interacted with the age of the child at the time of the move (m). We permit separate linear interactions for $m \leqslant 23$ and $m > 23$ and report the coefficient on the interaction for $m \leqslant 23$. The regression also includes additional controls analogous to those in equation (7); see Online Appendix D for details. Column (2) continues to use the indicator for being in the top 10% as the dependent variable, but uses the difference between permanent residents' predicted mean ranks in the destination versus the origin instead of their predicted upper-tail probabilities on the right side of the regression. Column (3) includes both the upper-tail (distributional) prediction as well as the mean rank prediction in the same regression. Columns (4)–(6) replicate columns (1)–(3) using an indicator for being unemployed at age 24 as the outcome and the difference between the permanent residents' predicted probabilities of being unemployed in the destination versus the origin as the key independent variable. Employment is defined as an indicator for having a W-2 filed on one's behalf at age 24. In all columns, the sample consists of all children in the primary analysis sample of one-time movers, defined in the notes to Table I (Panel C).

an equation analogous to equation (1), including a quadratic term in parental income rank p to account for the nonlinearities in tail outcomes identified in Chetty et al. (2014). We then calculate the predicted probability of being nonemployed π^{U}_{pcs} and being above the 90th percentile π^{90}_{pcs} using the fitted values from these regressions, as in equation (2).

In Table III, we estimate exposure effect models analogous to equation (7) using these 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 (7), using $\Delta^{90}_{odps} = \pi^{90}_{pds} - \pi^{90}_{pos}$ instead of the mean prediction $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$ as the key independent variable (see Online Appendix D for the exact regression specifications). We obtain an exposure effect estimate of $\gamma = 0.043$ a year in this specification. Column (2) uses the change in the predicted mean rank, Δ_{odps}, instead. Here, we obtain a statistically significant estimate of 0.024, as expected given the high degree of correlation in permanent residents' outcomes across quantiles: places where more children reach the top 10% also tend to have higher mean incomes. In column (3), we include both the quantile prediction Δ^{90}_{odps} and the mean prediction Δ_{odps}, identifying the coefficients purely from differential variation across quantiles within CZs. The coefficient on the quantile prediction remains unchanged at approximately $\gamma = 0.04$, while the coefficient on the mean prediction is not significantly different from 0.

Table III, columns (4)–(6) replicate columns (1)–(3), using an indicator for nonemployment as the dependent variable and the prediction for nonemployment Δ^{U}_{odps} instead of Δ^{90}_{odps} as the key independent variable. As in the upper tail, children's probabilities of being in the lower tail of the income distribution are fully determined by the quantile-specific prediction rather than the mean prediction. In column (6), the coefficient on the nonemployment prediction Δ^{U}_{odps} is $\gamma = 0.043$, and the placebo coefficient on the mean rank prediction is -0.002.

In short, we find evidence of distributional convergence: controlling for mean incomes, the distribution of children's incomes converges to the distribution of incomes in the destination in proportion to exposure time, at a rate of approximately 4% a year.[42] Because omitted variables such as wealth shocks would be unlikely to generate such distributional convergence, this finding again supports the view that the convergence in movers' outcomes is driven by causal effects of place. Formally, assume that if unobservables θ_i are correlated positively with exposure to place effects on upper (or lower) tail outcomes π^{q}_{pcs}, they must also be correlated with exposure to the place effects on mean incomes (proxied for by permanent residents' outcomes):

$$(9) \quad Cov\left(\theta_i, m\Delta^{q}_{odps}|X^q\right) > 0 \Rightarrow Cov\left(\theta_i, m\Delta_{odps}|X^q, m\Delta^{q}_{odps}\right) > 0.$$

Under this assumption, the findings in Table III imply that our estimates of γ reflect causal place effects (which are quantile-specific) rather than omitted variables, which are not quantile-specific under equation (9).

3. Gender. Finally, we conduct an analogous set of placebo tests exploiting heterogeneity in permanent residents' outcomes by child gender. We begin by constructing gender-specific

42. The rate of convergence need not be identical across all quantiles of the income distribution because the prediction for permanent residents at each quantile π^{90}_{pcs} 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.

TABLE IV
CHILDHOOD EXPOSURE EFFECT ESTIMATES: GENDER-SPECIFIC CONVERGENCE

	Dependent variable: child's income rank at age 24						
	No family fixed effects			With family fixed effects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Own gender prediction	0.038		0.030	0.031		0.027	0.030
	(0.002)		(0.003)	(0.006)		(0.006)	(0.007)
Other gender prediction (placebo)		0.031	0.010		0.016	0.017	0.009
		(0.002)	(0.003)		(0.005)	(0.005)	(0.007)
Sample		Full sample			Full sample		2-gender HH
Num. of obs.	1,552,898	1,552,898	1,552,898	1,552,898	1,552,898	1,552,898	490,964

Notes. This table reports estimates of annual childhood exposure effects (γ) using gender-specific permanent resident predictions. Standard errors are shown in parentheses. In all columns, the dependent variable is the child's family income rank at age 24. In columns (1)–(6), the sample consists of all children in the primary analysis sample of one-time movers, defined in the notes to Table I (Panel C). Column (1) replicates column (1) of Table II, replacing the predicted outcomes based on all permanent residents in the origin and destination with predictions based on the outcomes of children who have the same gender as the child who moves. Column (2) replicates column (1), replacing the own-gender predicted outcomes with the predicted outcomes of the opposite gender. Column (3) combines the variables in columns (1) and (2), including both the own-gender and other-gender (placebo) predictions. Columns (4)–(6) replicate (1)–(3) including family fixed effects. Column (7) replicates column (6), restricting the sample of movers to families with at least one child of each gender. Each regression also includes additional controls analogous to those in equation (7); see Online Appendix D for details.

predictions of the mean household income ranks of children of permanent residents by estimating equation (1) separately for male and female children, which we denote by $\bar{y}^m_{pcs}$ and $\bar{y}^f_{pcs}$. Places that are better for boys are generally better for girls as well: the (population-weighted) correlation of $\bar{y}^m_{pcs}$ and $\bar{y}^f_{pcs}$ across CZs is 0.93 at the median ($p = 50$).[43] We exploit the residual variation across genders to conduct placebo tests analogous to those above, based on the premise that unobservable shocks are unlikely to have gender-specific effects.

In Table IV, we estimate exposure effect models analogous to equation (7) with separate predictions by gender. Column (1) replicates equation (7) using the gender-specific prediction Δ^g_{odps} instead of the prediction that pools both genders. We obtain an exposure effect estimate of $\gamma = 0.038$ per year in this specification. In column (2), we use the prediction for the other gender Δ^{-g}_{odps} instead. Here, we obtain an estimate of 0.031, as expected given the high degree of correlation in outcomes across genders.

43. Online Appendix Figure V presents choropleth maps of $\bar{y}^m_{pcs} - \bar{y}^f_{pcs}$ at $p = 25$ and $p = 75$. For low-income families ($p = 25$), outcomes for boys are relatively worse than those for girls in areas with higher crime rates, a larger fraction of single parents, and greater inequality (Chetty et al. 2016).

In column (3), we include predictions for both genders, identifying the coefficients purely from differential variation across genders within CZs. In this specification, the coefficient on the own gender prediction is $\gamma = 0.03$, three times larger than the other-gender prediction, which is close to 0.[44]

One may be concerned that families sort to different areas based on their child's gender, which—unlike the quantile and cohort-specific variation used above—is known at the time of the move. To address this concern, Table IV, columns (4)–(6) replicate columns (1)–(3) including family fixed effects. The own-gender prediction remains a stronger predictor of children's outcomes than the other-gender prediction even when we compare siblings' outcomes within families. Column (7) shows that this remains the case when we restrict the sample to families that have at least one boy and one girl, for whom differential sorting by gender is infeasible.

The gender-specific convergence documented in Table IV supports the causal exposure effects model under an assumption analogous to equation (8), namely, that the unobservable θ_i does not vary differentially across children of different genders within a family. This assumption requires that families who move to areas that are particularly good for boys do not systematically invest more in their sons relative to their daughters, a restriction that would hold if, for instance, families do not have different preferences over their sons' and daughters' outcomes. Under this assumption, the gender-specific convergence in proportion to exposure time must reflect causal place effects.

V.E. Summary

The results in this section show that various refinements of our baseline design—such as including family fixed effects or exploiting cohort- or gender-specific variation—all yield annual exposure effect estimates of $\gamma \simeq 0.04$. These findings imply that any omitted variable θ_i that generates bias in our estimate of the exposure effect γ must: (i) operate within families in proportion to exposure time (family fixed effects); (ii) be orthogonal to changes

44. 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 equation (8).

in parental income and marital status (controls for observables); (iii) persist in the presence of moves induced by displacement shocks (displacement shock analysis); and (iv) precisely replicate permanent residents' outcomes by birth cohort, quantile, and gender in proportion to exposure time (outcome-based placebo tests). We believe that plausible omitted variables are unlikely to have all of these properties and therefore conclude that our estimate of $\gamma \simeq 0.04$ is an unbiased estimate of the annual childhood exposure effect.

The convergence rate of 4% per year of exposure between the ages 9 to 23 implies that children who move at age 9 would pick up about $(23 - 9) \times 4\% = 56\%$ of the observed difference in permanent residents' outcomes between their origin and destination CZs. If we assume that the rate of convergence remains at 4% even before age 9, our estimates would imply that children who move at birth to a better area and stay there for 20 years would pick up about 80% of the difference in permanent residents' outcomes between their origins and destinations.

An auxiliary implication of the results in this section is that the simple baseline design of comparing families who move with children of different ages is not confounded by selection and omitted variable biases. Although there is clear evidence of selection in terms of where families move—as shown by the estimate of $\delta > 0$ in Figure IV — we find no evidence of differential selection based on when families move to a better versus worse area (at least after their children are nine years old).[45] This finding implies that research designs exploiting variation in the timing of moves can be used to identify the causal effects of neighborhoods in observational data, providing a scalable tool for identifying neighborhood effects even in the absence of randomized experiments.

VI. Other Outcomes

In this section, we estimate neighborhood effects for several other outcomes beyond income: college attendance, marriage, teenage birth, and teenage employment. This analysis provides

45. Such differential selection might be small because the outcomes of children of permanent residents $\bar{y}_{pcs}$ are not highly correlated with mean parent incomes across areas (Chetty et al. 2014). As a result, moving to a better area for children (higher $\bar{y}_{pcs}$) is not systematically associated with parents finding higher-paying jobs, mitigating what might be the most important confounding factor for our design.

further evidence on the types of outcomes that are shaped by neighborhoods and illustrates how neighborhoods affect behavior before children enter the labor market.

Figure VIII replicates Figure IV, Panel B using college attendance and marriage as the outcomes. In Panel A, we replicate the specification in equation (6), replacing Δ_{odps} with $\Delta^{C}_{odps} = C_{pds} - C_{pos}$, where C_{pcs} is the fraction of children who attend college at any point between ages 18 and 23 (among children of permanent residents in CZ c in birth cohort s with parental income rank p). In Panel B, we replace Δ_{odps} with $\Delta^{M}_{odps} = M_{pds} - M_{pos}$, where M_{pcs} is the fraction of children who are married at age 26.

We find evidence of childhood exposure effects until age 23 for both of these outcomes. Moving to an area with higher college attendance rates at a younger age increases a child's probability of attending college. Likewise, moving at a younger age to an area where permanent residents are more likely to be married increases a child's probability of being married. Using parametric models analogous to equation (7), the estimated annual exposure effect for college attendance is comparable to our estimates for income ($\gamma = 0.037$) and is slightly smaller for marriage ($\gamma = 0.025$).

In Figure VIII, Panels C and D, we analyze outcomes measured while children are teenagers. Panel C considers teen birth, defined (for both men and women) as having a child between the ages of 13–19. We construct gender-specific predictions of teenage birth rates and plot estimates from the baseline specification in equation (6), replacing Δ_{odps} with $\Delta^{z}_{odpsg} = z_{pdsg} - z_{posg}$, where z_{pcsg} is the fraction of children of permanent residents with parental income p in CZ c, cohort s, and gender g who have a teenage birth. For both boys and girls, there are clear childhood exposure effects: moving at an earlier age to an area with a higher teen birth rate increases a child's probability of having a teenage birth. The gradient is especially steep between ages 13 and 18, suggesting that a child's neighborhood environment during adolescence may play a particularly important role in determining teen birth outcomes.

In Figure VIII, Panel D, we analyze neighborhood effects on teenage employment rates, measured at age 16. The key independent variable (corresponding to Δ_{odps}) in this figure is the difference in age 16 employment rates of children of permanent residents in the destination versus the origin CZ. We find a discontinuous effect of moving just before age 16 on the probability of working at 16, rather than a continuous exposure effect. Children who move at age 15 to a CZ where more 16-year-olds work

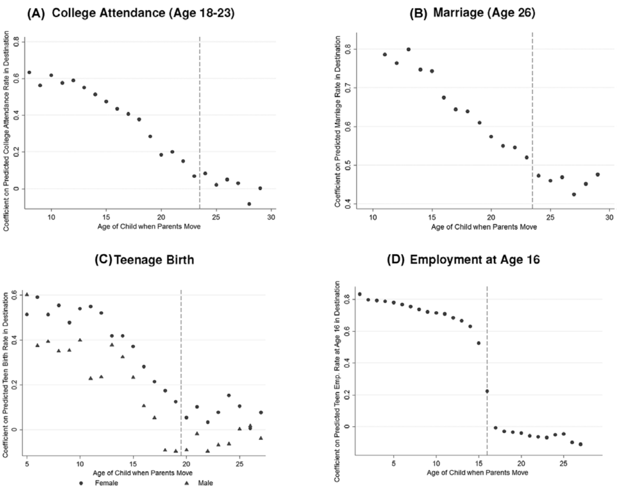

FIGURE VIII

Exposure Effects on College Attendance, Marriage, Teen Birth, and Teen Employment

This figure plots exposure effects for college, marriage, and the outcomes of teenagers using an approach analogous to that in Figure IV, Panel B. In Panel A, we replicate the specification in equation (6), using an indicator for college attendance at any age between 18 and 23 as the dependent variable instead of the child's income rank and replacing the key independent variable $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$ with the difference between permanent residents' college attendance rates in the destination versus the origin. The coefficients that are plotted can therefore be interpreted as the effect of moving to an area where permanent residents' college attendance rates are 1 percentage point higher at age m. We require that the child be observed between ages 18 and 23 to define college attendance; because we observe college attendance in years 1999–2012, we obtain estimates for children who move between the ages of 8 and 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 26 and replacing $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$ with the difference between permanent residents' marriage rates in the destination versus the origin. Panel C replicates the parametric specification in equation (6), using teenage birth as the dependent variable and replacing the key independent variable $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$ with the difference between permanent residents' teen birth rates in the destination versus the origin. We define teenage birth as having a child between the ages of 13 and 19, using data from the Social Security Administration's DM-2 database, and estimate separate specifications for males and females who have a child. Panel D replicates the parametric specification in equation (6), using an indicator for working at age 16 (based on having a W-2) as the dependent variable and replacing the key independent variable $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$ with the difference between permanent residents' teen employment rates in the destination versus the origin at the corresponding age. The coefficients that are plotted can therefore be interpreted as the effect of moving at age m to an area where permanent residents' teen employment rates are 1 percentage point higher at age 16. Age 16 is shown by the vertical dashed line; because moves after the age at which employment is measured cannot have a causal effect, the coefficients to the right of the dashed lines reflect a selection effect. See notes to Figure IV for further details on the construction of this figure.

are much more likely to work at age 16 than children who make the same move at age 17. Making the same move at earlier ages (before age 16) further increases the probability of working at age 16, but the exposure effect is small relative to the jump at age 16 itself.[46] Analogous jumps are observed at ages 17 and 18 when we measure employment at ages 17 and 18 (Online Appendix Figure VI). These jumps suggest that neighborhood effects may be partly driven by distinct experiences at different points of childhood, such as summer jobs that are available in a given area at certain ages. Such age-specific impacts may aggregate to produce the linear childhood exposure effects that shape outcomes in adulthood.

Although the mean income of individuals in an area is correlated with other outcomes such as college attendance and teenage birth rates, there is substantial independent variation in each of these outcomes. For example, permanent residents' mean income ranks at age 30 have a (population-weighted) correlation of 0.46 with college attendance rates for children with parents at $p = 25$ (Online Appendix Table VII). Hence, the finding that movers' outcomes converge to those of permanent residents on all of these dimensions constitutes further evidence that neighborhoods have causal effects, as it would be unlikely that unobserved confounds would generate such convergence on a spectrum of different outcomes.[47] Moreover, the fact that neighborhoods have causal effects on a wide variety of outcomes beyond earnings further suggests that the mechanism through which neighborhoods shape children's outcomes is not driven by labor market conditions but rather a set of environmental factors that shape behaviors throughout childhood.

VII. Conclusion

This article has shown that children's opportunities for economic mobility are shaped by the neighborhoods in which they grow up. Neighborhoods affect children's long-term outcomes

46. The magnitudes of the $\{b_m\}$ coefficients in Panel D are approximately 0.8 at young ages and 0 after age 16. Under our identifying assumption of constant selection effects by age, this implies that children who move at birth pick up 80% of the differences in teenage employment rates across CZs observed for permanent residents.

47. This logic is analogous to the tests for distributional convergence in Section V.D; here, we effectively test for convergence in the joint distribution of income and various other outcomes.

through childhood exposure effects: every extra year a child spends growing up in an area where permanent residents' incomes are higher increases his or her income. Movers' outcomes converge to those of permanent residents in the destination to which they move at a rate of approximately 4% per year of childhood exposure (up to age 23). This estimate implies that much of the variation in intergenerational mobility observed across CZs and counties is driven by causal effects of place rather than differences in the types of people living in those places. For instance, children who move at age 9 (the earliest age we observe in our data) would pick up 56% of the observed difference in permanent residents' outcomes between their origin and destination CZs. If the rate of convergence remains at 4% even before age 9—a strong assumption that should be evaluated in future work—our estimates imply that children who move at birth to a better area and stay there for 20 years would pick up about 80% of the difference in permanent residents' outcomes between their origins and destinations.

These results motivate place-focused approaches to improving economic mobility, such as making investments to improve outcomes in areas that currently have low levels of mobility or helping families move to higher opportunity areas. Identifying specific policy solutions—that is, the investments needed to improve mobility and the areas to which families should be encouraged to move—requires identifying the causal effect of each neighborhood and understanding what makes some areas produce better outcomes than others. The analysis in the present article shows that differences in permanent residents' outcomes are predictive of neighborhoods' causal effects on average. However, it does not provide estimates of the causal effect of each area on children's outcomes, as the outcomes of permanent residents in any given area will reflect a different mix of selection and causal effects. We construct estimates of the causal effect of growing up in each CZ and county in the United States and characterize the properties of areas that produce good outcomes in the next article in this series.

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Harvard University and National Bureau of Economic Research

Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics*. Code used to generate tables and figures in this article can be found in Chetty and Hendren (2018b), in the Harvard Dataverse, doi:10.7910/DVN/CEMFTJ.

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