The Impacts of Neighborhoods on Intergenerational Mobility: Childhood Exposure Effects and County-Level Estimates*

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Abstract

We characterize the effects of neighborhoods on children's earnings and other outcomes in adulthood by studying more than five million families who move across counties in the U.S. Our analysis consists of two parts. In the first part, we present quasi-experimental evidence that neighborhoods affect intergenerational mobility through childhood exposure effects. In particular, the outcomes of children whose families move to a better neighborhood – as measured by the outcomes of children already living there – improve linearly in proportion to the time they spend growing up in that area. We distinguish the causal effects of neighborhoods from confounding factors by comparing the outcomes of siblings within families, studying moves triggered by displacement shocks, and exploiting sharp variation in predicted place effects across birth cohorts, genders, and quantiles. We also document analogous childhood exposure effects for college attendance, teenage birth rates, and marriage rates. In the second part of the paper, we identify the causal effect of growing up in every county in the U.S. by estimating a fixed effects model identified from families who move across counties with children of different ages. We use these estimates to decompose observed intergenerational mobility into a causal and sorting component in each county. For children growing up in families at the 25th percentile of the income distribution, each year of childhood exposure to a one standard deviation (SD) better county increases income in adulthood by 0.5%. Hence, growing up in a one SD better county from birth increases a child's income by approximately 10%. Low-income children are most likely to succeed in counties that have less concentrated poverty, less income inequality, better schools, a larger share of two-parent families, and lower crime rates. Boys' outcomes vary more across areas than girls, and boys have especially poor outcomes in highly-segregated areas. In urban areas, better areas have higher house prices, but our analysis uncovers significant variation in neighborhood quality even conditional on prices.

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I Introduction

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

In this paper, we present new quasi-experimental evidence on the effects of neighborhoods on intergenerational mobility and reconcile the conflicting findings of prior work. Our analysis, which uses data from de-identified tax records covering the U.S. population from 1996-2012, consists of two parts.

Part I: Quasi-Experimental Evidence of Childhood Exposure Effects. In the first part of this paper, we measure the degree to which the differences in intergenerational mobility across areas documented in observational studies are driven by causal effects of place. In previous work (Chetty, Hendren, Kline, and Saez 2014), we documented substantial variation across commuting zones in children's expected earnings (measured by their percentile rank in the national income distribution) conditional on their parents' income.[1] This geographic variation in intergenerational mobility could be driven by two very different sources. One possibility is that neighborhoods have causal effects on economic mobility: that is, moving a given child to a different neighborhood would change her life outcomes. Another possibility is that the observed geographic variation is due to systematic differences in the types of people living in each area, such as differences in demographic makeup or wealth.

We test these explanations and identify the causal effects of neighborhoods by studying more than five million families who move across counties and exploiting differences in their children's ages when they move. We first show that children whose parents move to a better neighborhood – i.e., a CZ or county where children of permanent residents (non-movers) at their income percentile have higher earnings in adulthood – earn more themselves.[2] Symmetrically, those who move to worse

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

[2] We measure children's incomes between the ages of 24 and 30; our results are not sensitive to varying the age at which child income is measured within this range.

neighborhoods have lower earnings as adults.[3] Importantly, the changes in earnings are proportional to the fraction of childhood spent in the new area. On average, spending an extra year in a CZ or county where the mean rank of children of permanent residents is 1 percentile higher increases a child's expected rank by approximately 0.03-0.04 percentiles. Stated differently, the outcomes of children who move converge to the outcomes of permanent residents of the destination area at a rate of approximately 3-4% per year of exposure.

Under the assumption that the timing of parents' moves is orthogonal to children's potential outcomes – an assumption that we revisit and validate below – this convergence pattern implies that neighborhoods have substantial *childhood exposure effects*. That is, every additional year of childhood spent in a better environment improves a child's long-term outcomes. The convergence is linear with respect to age: moving to a better area at age 8 instead of 9 is associated with the same improvement in earnings as moving to that area at age 15 instead of 16. The exposure effects persist until children are in their early twenties. Extrapolating over the duration of childhood, from age 0 to 20, the roughly 3.5% annual convergence rate implies that at least 50% and as much as 70% of the variance in observed intergenerational mobility across counties and commuting zones is due to the causal effects of place.[4] We find analogous childhood exposure effects for several other outcomes, including college attendance, teenage employment, teenage birth, and marriage.

The critical identification assumption underlying our approach is that children whose parents move to a better (or worse) area at a young age have comparable potential outcomes to children whose parents move when they are older. This orthogonality condition would be violated if, for instance, parents who move to a better area when their children are young are wealthier or invest more in their children. In addition, moving may itself be correlated with other factors – such as a higher-paying job or a change in marital status – that directly affect children in proportion to exposure time. We use three approaches to account for such selection and omitted variable biases: controlling for observable factors, isolating moves triggered by exogenous events, and implementing a set of sharp placebo (or overidentification) tests.

We control for factors that are fixed within the family (e.g., parent education) by including family fixed effects when estimating exposure effects, as in Plotnick and Hoffman (1996) and Aaronson

[3]Throughout the paper, we refer to areas where children have better outcomes in adulthood as "better" neighborhoods. We use this terminology without any normative connotation, as there are of course many other amenities of neighborhoods that may be relevant from a normative perspective.

[4]Formally, $0.035*20 = 70\%$ is a point estimate under the assumption that the causal effects and sorting components are uncorrelated. Without this assumption, the variance of predicted values, $(0.035 * 20)^2 = 0.49$, provides a lower bound.

(1998). This approach identifies exposure effects from comparisons between siblings, effectively asking whether the *difference* in outcomes between two siblings in a family that moves is proportional to the size of the age gap between them. We obtain an annual exposure effect of approximately 4% per year with family fixed effects, very similar to our baseline estimates. Controlling for parents' incomes and marital status in each year also has no effect on the estimates.

Of course, one may still be concerned that whatever unobserved change induced a family to move (e.g., a wealth shock) may also have had direct effects on their children's outcomes. To account for such unobserved factors, we next focus on a subset of moves where we have more information what caused the move. We identify moves that occur as part of large outflows from ZIP codes, which are typically caused by natural disasters or local plant closures. To remove the endogeneity of individual choice – for example, wealthier parents with young children sorting to better areas in response to the shock – we instrument for the 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. Once again, we obtain exposure effect estimates similar to the baseline in this subsample displaced by such exogenous shocks.

While the instrumental variables approach further validates the baseline exposure effect design in the small subset of areas that experience displacement shocks, our ultimate goal is to develop credible estimates of exposure effects for all areas in the U.S. We therefore turn to a third approach – implementing placebo (overidentification) tests that exploit heterogeneity in place effects across subgroups – which in our view is ultimately the most compelling method of assessing the validity of the design. We begin by analyzing heterogeneity in place effects across birth cohorts. Although there is considerable persistence in outcomes within CZs over time, some places improve and others decline. Exploiting this variation, we show that, in a multivariable regression, 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 not those of surrounding birth cohorts (conditional on their own birth cohort predictions). It would be unlikely that sorting or omitted variables would produce such a sharp cohort-specific pattern, especially because the cohort-specific effects are only observed ex-post after children grow up. Hence, this evidence of cohort-specific convergence supports the view that our neighborhood exposure effect estimates are not confounded by selection and omitted variable biases.

Next, we implement analogous placebo tests by exploiting variation in the *distribution* of outcomes, as opposed to focusing solely on mean outcomes. For instance, low-income children who spend their entire childhood in Boston or San Francisco have similar outcomes on average, but children in San Francisco are more likely to end up in the upper tail (top 10%) or lower tail (bottom 10%) of the income distribution. The causal exposure effects model predicts convergence not just at the mean but across the entire distribution; in contrast, it would be quite unlikely that omitted variables (such as changes in parent wealth) would happen to perfectly replicate the entire distribution of outcomes in each area. In practice, we find clear evidence of distributional convergence: controlling for mean outcomes, children's outcomes converge to predicted outcomes in the destination across the distribution in proportion to exposure time, again at a rate of approximately 3.5% per year.

Finally, we find analogous results when analyzing heterogeneity in outcomes across genders. Though place effects are highly correlated for boys and girls, there are some differences in predicted outcomes by gender across neighborhoods. For instance, highly-segregated areas tend to have lower mean outcomes for boys than girls. We find that when a family with both a daughter and a son moves to an area that is particularly good for boys, their son's outcomes improve in proportion to exposure time to the destination much more than their daughter's outcomes. Once again, if our findings were driven by sorting or omitted variables, one would not expect to find stark differences in impacts by gender unless families' unobservable investments in their children are differentially correlated with where they move.

Overall, these results suggest that neighborhoods matter for children's long-term outcomes and suggest that at least half of the variance in observed intergenerational mobility across areas is due to the causal effect of place. But, it does not directly tell us which areas produce the best outcomes. In the second part of this paper, we address this question by estimating the causal effect of each county and commuting zone (CZ) in the U.S. on children's earnings in adulthood.

Part II: County-Level Estimates of Causal Effects. We estimate each CZ and county's causal effect on children's incomes and characterize the properties of areas that produce good outcomes in four steps.

First, we estimate the fixed effect for each county (or CZ) using a regression model that is identified from families who move across areas with children of different ages. To understand how the model is identified, consider families in the New York area. If children who moved from Manhattan to Queens at younger ages earn more as adults, we can infer that Queens has positive childhood exposure effects relative to Manhattan under our central assumption that the timing of families' moves are orthogonal to their children's potential outcomes. Building on this logic, we use our sample of cross-county movers to regress children's earnings in adulthood on fixed effects for each county interacted with the fraction of childhood spent in that county. We estimate the county fixed effects separately by parent income level, permitting the effects of each area to vary with family income. We also include origin by destination fixed effects when estimating this model, so that each county's effect is identified purely from differences in the *age of the children* when families move across areas.

In the second step of our analysis, we estimate the variance components of a latent variable model of neighborhood effects, treating the fixed effects as the sum of a latent causal effect and noise due to sampling error. We estimate the signal variance of neighborhood effects by subtracting the portion of the variance in the fixed effects due to noise. For a child with parents at the 25th percentile of the national income distribution, we estimate that spending one additional year of childhood in a one SD better county (population weighted) increases household income at age 26 by 0.17 percentile points, which is approximately equivalent to an increase in mean earnings of 0.5%. Extrapolating over 20 years of childhood, this implies that growing up in a 1 SD better county from birth would increase a child's income in adulthood by approximately 10%.

Neighborhoods have similar effects in percentile rank or dollar terms for children of higher-income parents, but matter less in percentage terms because children in high-income families have higher mean earnings. For children with parents at the 75th percentile of the income distribution, the signal SD of annual exposure effects across counties is 0.16 percentiles, which is approximately 0.3% of mean earnings. Areas that produce better outcomes for children in low-income families are, on average, no worse for those from high-income families. This finding suggests that the success of the poor in certain areas of the U.S. does not necessarily come at the expense of the rich.

Our estimates imply that roughly two-thirds of the variation in intergenerational mobility across counties documented in (Chetty et al., 2014) for children in low-income (25th percentile) families is driven by causal effects. The remaining one third is driven by sorting, i.e. systematic differences in the characteristics of the people living in each county. The causal and sorting components are approximately uncorrelated with each other: there is no evidence that families with better unobservables systematically sort to better counties conditional on parent income in equilibrium.

The variance components of our model of neighborhood effects allow us to quantify the degree of signal vs. noise in each CZ and county's fixed effect estimate. In CZs and counties with large populations, such as Cook County in Chicago, the signal accounts for 75% of the variance in the fixed effect estimate. However, in smaller counties, more than half of the variance in the fixed effect estimates is due to noise. As a result, the raw fixed effects are not appropriate for forming forecasts of each county's causal effect for most counties.

In the third step of our analysis, we construct forecasts of each county's causal effect using a simple shrinkage estimator. We construct the best (minimum mean-squared-error) linear prediction of each county's causal effect by taking a weighted average of the fixed effect estimate based on the movers and a prediction based on permanent residents' outcomes. The permanent residents' mean outcomes have very little sampling error, but are imperfect predictors of a county's causal effect because they combine causal effects with sorting. Therefore, in large counties, where the degree of sampling error in the fixed effect estimates is small, the optimal predictor puts most of the weight on the fixed effect estimate based on the movers. In smaller counties, where the fixed effects estimates are very imprecise, the estimator puts more weight on the predicted outcome based on the permanent residents. The county-level predictions obtained from this procedure yield unbiased forecasts of the impacts of each county in the sense that moving a child to a county with a 1 percentile higher predicted effect will increase that child's earnings in adulthood by 1 percentile on average.

We use our county-level forecasts to identify the best and worst counties in the U.S. in terms of their causal effects on intergenerational mobility. Each additional year that a child spends growing up in Dupage County, IL – the highest-ranking county in terms of its causal effect on upward mobility among the 100 largest counties in the U.S. raises her household income in adulthood by 0.80%. This implies that growing up in Dupage County from birth – i.e., having about 20 years of exposure to that environment – would raise a child's earnings by 16% relative to the national average. In contrast, every extra year spent in the city of Baltimore – one of the lowest-ranking counties – reduces a child's earnings by 0.7% per year of exposure, generating a total earnings penalty of approximately 14% for children who grow up there from birth.[5]

Our estimates of causal effects at the county and commuting zone (CZ) level are highly correlated with the raw statistics on intergenerational mobility reported in (Chetty et al., 2014), but there are several significant differences. For example, children who grow up in New York City have above-average rates of upward mobility. However, the causal effect of growing up in New York City on upward mobility – as revealed by analyzing individuals who move into and out of New York – is negative relative to the national average. This negative effect of growing up in New York is masked

[5]These estimates are based on data for children born between 1980-86 and who grew up in the 1980's and 1990's. We find that neighborhoods' effects generally remain stable over time, but some cities have presumably gotten better in the 2000's, while others may have gotten worse.

when one simply studies the average outcomes of children who grow up there because families who live in New York tend to have unusually high rates of upward mobility. In particular, New York has a very large share of immigrants, and we find evidence consistent with immigrants having higher rates of upward mobility independent of where they live.

We find that neighborhoods matter more for boys than girls: the signal SD of county-level effects for boys is roughly 1.5-times that of girls in low-income (25th percentile) families. Moreover, the distribution of county-level forecasts is wider and has a thick lower-tail for boys, with some counties such as Baltimore and Wayne County in Detroit producing extremely negative outcomes for boys but less so for girls. Areas with high degrees of segregation and sprawl generate particularly negative outcomes for boys relative to girls. There are also significant gender differences related to marriage rates. For example, Northern California generates high levels of individual earnings for girls, but produces lower levels of household income because fewer children get married in their 20s.

What are the properties of areas that improve upward mobility? In the last step of our analysis, we characterize the properties of counties and CZs that produce good outcomes by correlating the estimated causal and sorting effects with observable characteristics. Within CZs, counties that produce better outcomes for children in low-income families tend to have five characteristics: lower rates of residential segregation by income and race, lower levels of income inequality, better schools, lower rates of violent crime, and a larger share of two-parent households. For high income families, we find positive correlations with school quality, social capital, and inequality. But, we find measures of segregation and poverty are not strongly correlated with the causal effects of counties on high-income families. However, they are strongly correlated with the sorting component for high-income families, implying that high-income families with good unobservables tend not to live in cities that generate worse outcomes for the poor (such as segregated areas).

Urban areas, particularly those with substantial concentrated poverty, typically generate much worse outcomes for children than suburbs and rural areas for both low- and high-income families. We also find that areas with a larger African-American population tend to have lower rates of upward mobility. These spatial differences amplify racial inequality across generations: we estimate that roughly one-fifth of the gap in earnings between blacks and whites can be attributed to the counties in which they grow up.

Finally, we evaluate how much more one has to pay in terms of housing costs to live in areas that generate good outcomes for children. Across CZs, we find a *negative* correlation with housing prices, as rural areas have low house prices and tend to produce better outcomes. However, across counties within CZs, counties that offer better prospects for children have higher house prices and rents. The correlation between rents and children's outcomes is particularly strong in cities that have high levels of segregation and sprawl, which may explain the persistence of poverty across generations in such cities.

Although rents are correlated with upward mobility in large cities, there are some bargains to be found. For example, in the New York metro area, Hudson County, New Jersey offers much higher levels of upward mobility than Queens or the Bronx even though median rents in that area are comparable to the New York boroughs over the period we study. If we divide neighborhood effects into the component that projects onto observable factors such as poverty and dropout rates and the residual "unobservable" component, only the observable component is capitalized in rents and house prices. Our findings show that there is substantial scope for households to move to areas within their CZ that produce better outcomes for children without paying higher rents, and our estimates provide guidance in identifying such areas empirically.

Our findings help reconcile the conflicting results in the prior literature on neighborhood effects, most notably the discrepancy between the findings from the Moving to Opportunity Experiment and observational studies documenting substantial variation in children's outcomes across areas even after controlling for observable differences in characteristics. Prior analyses of the MTO experiment have focused primarily on the effects of neighborhoods on adults and older youth (e.g. Kling et al. (2007)) and have not explicitly tested for exposure effects among children. In a companion paper (Chetty, Hendren, and Katz, 2015), we link the MTO data to tax records and show that the MTO data exhibit the same exposure time patterns as those we document here. In particular, we find large treatment effects for children who moved to better neighborhoods at young ages but not those who moved at older ages. More generally, our findings imply that much of the variation across neighborhoods documented in observational studies does in fact reflect causal effects of place, but that these effects arise through accumulated childhood exposure rather than impacts on adults.

The rest of the paper is organized as follows. In Section II, we present a stylized model of neighborhood effects to formalize our empirical objectives. Section III describes the data. Sections IV-VI present the analysis underlying the first part of the paper. Section IV presents baseline estimates of average neighborhood exposure effects on earnings by studying the effects of moving to areas where prior permanent residents are doing better (or worse). Section V presents a series of tests validating our baseline identification assumptions and Section VI presents estimates of exposure effects for other outcomes. Sections VII-X comprise the second part of our analysis.

Section VII presents the fixed effect estimates based on movers. Section VIII presents estimates of the variance components of the neighborhood effects model. Section IX presents our forecasts of each county and CZs causal effect based on the shrinkage estimator. In Section X, we correlate the estimated place effects with observables. Section XI concludes and discusses our findings in the context of prior work. Estimates of neighborhood effects and related covariates are available by commuting zone and county on the project website.

II Model and Empirical Objectives

We begin with a stylized model of neighborhood effects and location choice. We use this model to define the estimands of interest, derive estimating equations, and formalize the identification assumptions underlying our research design.

II.A Setup

Consider a discrete time model in which parents live for T periods. Children $i = 1, ..., I$ are born in year $t = 1$ and leave their parents' household and enter the labor market in year T_C. Let y_i denote a long-term outcome (e.g., earnings in adulthood) of child i. Let $f(i)$ denote the family to which child i is born; we allow multiple children per family to compare siblings' outcomes. Let $p(f(i))$ the percentile rank of child i's parents in the national income distribution, and $c(f(i), t)$ the neighborhood in which his family lives in year t. We treat parent income $p(i) = p(f(i))$ as exogenous and fixed over time.[6] Our model consists of a specification for the production function for children's outcomes y_i and the parents' choice of location $c(i, t)$ in each period.

Children's outcomes y_i are a function of neighborhood characteristics, family inputs, and disruption costs of moves during childhood. Let μ_{pc} denote the causal effect of growing up in neighborhood c for one's entire childhood (i.e., from periods 1 to T_C) for a child with parents at percentile p. Allowing neighborhoods to have heterogeneous effects across the parent income distribution turns out to be important empirically. Let θ_{it} denote the family inputs in year t, which we interpret as a combination of active investments by parents (e.g., via financial resources or time) and variation in latent ability (e.g., due to genetics). We model the child's outcome y_i as an additive function of the neighborhood and family inputs she receives over her childhood net of disruption costs of

[6] In our empirical analysis, we show this assumption does not affect our results by controlling for changes in income and measures of income separately by year.

moves:

$$y_i = \bar{\theta}_i + \sum_{t=1}^{T_C} [\lambda_t \mu_{p(i),c(f(i),t)} - \kappa_t I\{c(i,t) \neq c(i,t-1)\}], \tag{1}$$

where $\bar{\theta}_i = \sum_{t=1}^{T_C} \frac{1}{T_C} \theta_{it}$ is the mean level of parental inputs to child i and $I\{c(i,t) \neq c(i,t-1)\}$ denotes an indicator for having moved neighborhoods in year t. The weights λ_t allow for the possibility that some periods of a child's life may be more important than others for long-term development, where $\lambda_t > 1$ ($\lambda_t < 1$) indicates year t is relatively more (less) important than other years of childhood. Let $\Lambda_m = \sum_{t=1}^{m} \lambda_t$ denote the cumulative sum of growing up in a one-unit better area from birth to age m. We normalize $\Lambda_{T_c} = \sum_{t=1}^{T_C} \lambda_t = T_C$. Equation (1) imposes that the parent's location $c(i,t)$ after the child has left the house ($t > T_C$) has no causal effect on the child's outcome – an assumption we test below. The coefficients κ_t measure the disruption cost of moving neighborhoods at year (or age) t, with $\kappa_1 \equiv 0$.

The production function for y_i in (1) imposes two substantive restrictions that are relevant for our empirical analysis. First, it assumes that neighborhood effects are additive, i.e. there are no complementarities between neighborhood quality across years, and do not vary across individuals conditional on parent income p.[7] Second, it assumes that the disruption costs of moving κ_t do not vary across neighborhoods.[8] Equation (1) does allow for critical ages in which neighborhood outcomes may be more important (by varying λ_t), and in our baseline analysis we allow for these differences. However, for many outcomes, our empirical findings will suggest that a simpler linear exposure time specification with $\lambda_t = 1$ fits the data quite well:

$$y_i = \bar{\theta}_i + \sum_c m_{i,c} \mu_{pc} + \bar{\kappa}_i \tag{2}$$

where $\bar{\theta}_i$ is the mean of parental inputs, $\sum_c m_{i,c} \mu_{pc}$ is the sum of exposure effects[9] where $m_{i,c}$ is the number of years (of childhood, $t \leq T_C$) that child i spends in neighborhood c, and $\bar{\kappa}_i = \sum_{t=1}^{T_C} \frac{1}{T} \kappa_t I\{c(i,t) \neq c(i,t-1)\}$ is the net impact of moving disruptions.

While we do not attempt to estimate a utility function over parents' choice of neighborhoods and investments in children, it is useful for some of our empirical tests below[10] to conceptualize

[7] We defer the identification of complementarities and heterogeneity to future work. If the true production function features complementarities or heterogeneity, our reduced-form empirical estimates of μ_{pc} can be interpreted as the mean effect of spending an extra year in area c for the individuals who move to c from other areas.

[8] The key assumption for identification of μ_{pc} will be that κ_t cannot vary in a *differentially age-dependent* manner across neighborhoods; it is feasible to extend the model to allow for disruption cost that varies across neighborhoods but for which the age-gradient of κ_t does not vary across neighborhoods.

[9] Note that $\sum_{t=1}^{T_C} \lambda_t \mu_{p(i),c(f(i),t)} = \sum_c m_{i,c} \mu_{pc}$ if $\lambda_t = 1$ for all t.

[10] Specifically, in Section V.C we provide tests of our identification of neighborhood effects by exploiting restrictions on the parents' information set, Ω, in how neighborhoods can affect their children's outcomes in adulthood.

parents making decisions to maximize their expected utility. We imagine that parents of child i, $f(i)$, choose neighborhoods, $c(f(i), t)$, to maximize some lifetime utility function of children's outcomes, parent inputs, and other neighborhood- and time-specific factors:

$$\mathbb{E}\left[U_f(\overrightarrow{y_f}, \overrightarrow{\theta_f}, \overrightarrow{\chi_f})|\Omega\right] \tag{3}$$

where $\overrightarrow{y_f} = \{y_i|f(i) = f\}$ denotes the vector of outcomes for the children in family f, $\overrightarrow{\theta_f} = \{\theta_{it}|f(i) = f\}$ is the vector of family inputs, and $\overrightarrow{\chi_f} = (\chi_{f,c(f,1)}, ..., \chi_{f,c(f,t)})$ denotes other factors that vary across neighborhoods and time, such as local amenities, job opportunities and proximity to work, and local house prices. Parents choose a sequence of investment levels $(\theta_1, ..., \theta_{T_C})$ and neighborhoods $c(i, 1), ..., c(i, T)$ to maximize their expected utility U_f given their resource constraints and knowledge, Ω, about how their choices affect outcomes.

II.B Empirical Objectives

Objective #1. Our empirical analysis has two objectives, which we define here using (hypothetical) randomized experiments. Our first objective is directly motivated by the current debate in the literature on neighborhood effects. Prior work has documented robust differences in children's outcomes y_i across neighborhoods in observational data (e.g., Wilson 1987, Jencks and Mayer 1990, Massey 1993, Brooks-Gunn et al. 1993, Cutler et al. 1997, Leventhal and Brooks-Gunn 2000, Sampson et al. 2002). But experimental evidence to date finds little evidence that moving children to better neighborhoods – e.g., those with lower poverty rates – improves outcomes. Therefore, our first goal is to determine whether moving to an area in which other children do well has a causal effect on children's outcomes and to provide a lower bound on the fraction of the variation in observed economic outcomes reflects the causal effects of neighborhoods.

To formalize our first question, observe that the mean outcome of children who spend their entire childhood in area c is $\bar{y}_{pc} = T_C\mu_{pc} + \bar{\theta}_{pc}$, where $\bar{\theta}_{pc} = E[\frac{1}{T}\sum \theta_{it}|c(i,t) = c]$ is the mean level of investment in children by families who live in that area and $T_C\mu_{pc}$ is the cumulative effect of childhood exposure to area c. Mean parent investments $\bar{\theta}_{pc}$ vary across areas due to endogenous parent sorting and may be correlated with μ_{pc}.[11] We are interested in whether and by how much the mean outcomes across places reflect the causal impacts of those places. In other words, we seek to estimate $E[\mu_{pc}|\bar{y}_{pc}]$.

[11] For example, parents who place higher weight on their children's outcomes may choose to live in areas that are better for their child (higher μ_{pc}) and also invest more in their child directly (higher θ_{it}), leading to $Cov(\mu_{pc}, \bar{\theta}_{pc}) > 0$ in equilibrium. Conversely, parents may choose to invest in neighborhoods as a substitute for other investments, leading to $Cov(\mu_{pc}, \bar{\theta}_{pc}) < 0$.

One intuitive way to answer this question would be to randomly assign children to neighborhoods at a given age $m \in [1, T_C]$ and estimate the best linear predictor of children's outcomes y_i in the experimental sample using $\bar{y}_{pc}$:

$$y_i = \alpha + \beta_m \bar{y}_{pc} + \varepsilon_i \tag{4}$$

Given estimates $\overrightarrow{\beta} = \{\beta_m\}_{m=1}^{T_C}$, we define the exposure effect of moving to a better area at age m by $\beta_m - \beta_{m-1}$. Under the simple a linear exposure model in equation (2), the exposure effect is constant and given by $\beta_m - \beta_{m-1} = E\left[\mu_{pc}|\bar{y}_{pc}\right]$ for all m.[12]

Estimating exposure effects (i.e. the pattern of β_m across different ages, m) provides answers to several questions. First, finding a positive effect (at any age) allows us to reject the null that neighborhoods do not matter, a null of interest given experimental evidence to date. Second, the values of the exposure effects at different ages are informative about the ages at which neighborhood environments matter most for children's outcomes.[13] Finally, the magnitude of β_1 – the impact of assigning children to better neighborhood from birth – yields bounds on the variance of place effects, $\sigma^2_{\mu p} = Var(\mu_{pc})$:

$$\frac{T_c^2 \sigma^2_{\mu_p}}{\sigma^2_{\bar{y}_p}} \geq \beta_1^2 \tag{5}$$

Intuitively, the variance of predicted effects based on permanent resident outcomes $\bar{y}_{pc}$, $\beta_1^2 \sigma^2_{\bar{y}_p}$ is a lower bound for the total variance of place effects, $T_c^2 \sigma^2_{\mu p}$, of obtaining an entire childhood (T_c years) of exposure to the place effect.[14] Under an additional assumption of no covariance between the sorting and causal components (μ_p and θ_i), β_1 is exactly equal to the fraction of variance that is due to the causal effect, $\beta_1 = \frac{T_c^2 \sigma^2_{\mu p}}{\sigma^2_{\bar{y}_p}}$.[15]

Another key advantage of estimating $E\left[\mu_{pc}|\bar{y}_{pc}\right]$ is that it will facilitate a range of high powered placebo (overidentification) tests that utilize the information contained in the distribution of

[12] We assume that β does not vary across parent income percentiles p to simplify notation, but one could estimate (4) separately by p to identify a coefficient β_p for each p. In our empirical application, we show that β_p does not vary significantly across percentiles.

[13] More precisely, the pattern of $\{\beta_m\}$ identifies the ages at which moving to a better environment, as measured by the outcomes of prior residents, has the largest effects. Other measures of the quality of a child's environment could potentially generate different critical ages.

[14] To see this, we can use (1) to write the outcome of a child who is randomly assigned to a neighborhood c at age m as

$$y_i = (T_c - \Lambda_m)\mu_{pc} - \kappa_m + \nu_i, \tag{6}$$

where $E[\nu_i|c] = 0$ (the neighborhood effect before age m is subsumed in the error term ν_i because of random assignment). To see that that $\sigma^2_{\mu_p} \geq T_c^2 Var(\beta_1 \bar{y}_{pc}) = T_c^2 \beta_1^2 \sigma^2_{\bar{y}_p}$, note that $Var(\beta_1 \bar{y}_{pc}) = \beta_1^2 Var(\bar{y}_{pc}) = Cov(T_c \mu_{pc}, \bar{y}_{pc})/\sigma^2_{\bar{y}_{pc}} = \sigma^2_{\mu p} \frac{Cov(T_c \mu_{pc}, \bar{y}_{pc})}{\sigma^2_{\bar{y}_{pc}} \sigma^2_{\mu p}} = T_c^2 \sigma^2_{\mu_p} \rho^2_{\mu_{pc} \bar{y}_{pc}} \leq T_c^2 \sigma^2_{\mu_p}$ because the correlation coefficient $\rho_{\mu_{pc} \bar{y}_{pc}} \leq 1$.

[15] To see this, note that

$$\beta_1 = \frac{cov\left(T_c \mu_{pc}, \bar{y}_{pc}\right)}{var\left(\bar{y}_{pc}\right)} = \frac{T_c^2 \sigma^2_{\mu_{pc}}}{\sigma^2_{\bar{y}_{pc}}}$$

outcomes of permanent residents in an area to test for the presence of bias from sorting patterns (e.g. families of children with high θ_{pc} moving to places with high μ_{pc} when their kids are young). But, while $\overrightarrow{\beta}$ tells us about the *average* effects of exposure to neighborhoods where prior residents are doing better, $E[\mu_{pc}|\bar{y}_{pc}]$, estimating $\overrightarrow{\beta}$ itself is not adequate to identify the causal effects of growing up in each neighborhood c, $\{\mu_{pc}\}_{c=1}^{C}$.

Objective #2. Our second objective – which we take up in Part II (starting in Section VII) – is to directly estimate fixed effects for each place μ_{pc} to determine the causal impact of an additional year of exposure to each commuting zone and county in the U.S. The ideal experiment to estimate μ_{pc} would be to randomly assign children at each parent income level p to each neighborhood from birth. One could then identify each place's causal effect simply using mean observed outcomes in each area ($\mu_{pc} = \bar{y}_{pc}$), since random assignment guarantees $\bar{\theta}_{pc}$ does not vary across places (for all p).[16] In contrast to Objective #1, this does not require any information about the outcomes of permanent residents, $\bar{y}_{pc}$.

In Section VII, we construct unbiased estimates of μ_{pc}. We then decompose observed outcomes, $\bar{y}_{pc}$, into causal (μ_{pc}) and sorting ($\bar{\theta}_{pc}$) components, and estimate the variance of these components in Section VIII. This exercise breaks up the observed pattern of intergenerational mobility in the U.S. into a component due to the causal effects of places and a component due to systematic differences in the types of people living in different places who provide differential inputs to their children, θ_i. Next, in Section IX we combine our fixed effect estimates of μ_{pc} (identified solely from movers) with the estimate of $E[\mu_{pc}|\bar{y}_{pc}] = \beta\bar{y}_{pc}$ (identified using information in permanent resident outcomes) to form a forecast of each place's causal effect, μ_{pc}^{f}, that minimizes mean-square prediction error and delivers unbiased forecasts. Finally, in Section X we characterize the correlates of places with high values of μ_{pc} by regressing our estimates on observables, such as poverty rates and local school quality.

The remainder of the paper implements these empirical objectives using observational data on families who move across neighborhoods.

[16]In principle, one could go straight to identifying the causal effects of place $\{\mu_{pc}\}$ without identifying β. We do not take this approach for two reasons that we discuss further below: (1) we are able to estimate β under weaker orthogonality assumptions than μ_{pc} and (2) we obtain much more precise estimates of β than $\{\mu_{pc}\}$ by using data on prior residents' outcomes to collapse the problem into estimating one parameter rather than estimating thousands of place effects. Given that a key question in the literature is whether neighborhoods matter at all, we view credible estimation of β as a critical first step before turning to secondary questions about which neighborhoods are better or worse.

III Data, Geographic Definitions, and Summary Statistics

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 give us information on the earnings of those who do not file tax returns. Our analysis sample is essentially identical to that used to study intergenerational mobility in Chetty et al. (2014), and much of what follows in this section is taken directly from that paper.[17] Here, we briefly summarize the key variable and sample definitions. Note that in what follows, the year always refers to the tax year (i.e., the calendar year in which the income is earned).

III.A Sample Definitions

Our base dataset of children consists of all individuals who (1) have a valid Social Security Number or Individual Taxpayer Identification Number, (2) were born between 1980-1991[18], and (3) are U.S. citizens as of 2013. We impose the citizenship requirement to exclude individuals who are likely to have immigrated to the U.S. as adults, for whom we cannot measure parent income. We cannot directly restrict the sample to individuals born in the U.S. because the database only records current citizenship status.

We identify the parents of a child as the first tax filers (between 1996-2012) who claim the child as a child dependent and were between the ages of 15 and 40 when the child was born. If the child is first claimed by a single filer, the child is defined as having a single parent. For simplicity, we assign each child a parent (or parents) permanently using this algorithm, regardless of any subsequent changes in parents' marital status or dependent claiming.

If parents never file a tax return, we do not link them to their child. Although some low-income individuals do not file tax returns in a given year, almost all parents file a tax return at some point between 1996 and 2012 to obtain a tax refund on their withheld taxes and the Earned Income Tax Credit (Cilke 1998). We are therefore able to identify parents for approximately 95% of the children in the 1980-1991 birth cohorts. 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

[17] See Online Appendix A of Chetty et al. (2014) for a detailed description of how we construct the analysis sample starting from the raw population data. The records are complete as of the summer of 2013. This implies they include a complete set of information returns, but potentially exclude some amendments and late filings for 1040s in 2012. Restricting our baseline analysis to use data through 2011 yields very similar results.

[18] The teen labor outcomes in Figure XI include additional data from children born up to 1996.

limit our analysis to children born during or after 1980.

Our full analysis sample includes all children in the base dataset who (1) are born in the 1980-91 birth cohorts, (2) for whom we are able to identify parents, and (3) whose mean parent income between 1996-2000 is strictly positive (which excludes 1.2% of children).[19]

Geographic Definitions: We conceptualize neighborhood effects using a hierarchical model in which children's outcomes depend upon conditions in their immediate neighborhood (e.g., peers or resources in their city block), local community (e.g., the quality of schools in their county), and broader metro area (e.g., local labor market conditions). We characterize neighborhood effects first at the level of commuting zones (CZs) and then at the level of counties. CZs are aggregations of counties based on commuting patterns in the 1990 Census constructed by Tolbert and Sizer (1996). Since CZs are designed to span the area in which people live and work, they provide a natural starting point as the coarsest definition of "neighborhoods." CZs are similar to metropolitan statistical areas (MSA), but unlike MSAs, they cover the entire U.S., including rural areas. There are 741 CZs in the U.S.; on average, each CZ contains 4 counties and has a population of 380,000. Online Appendix Figure I provides an illustration of the Boston CZ.

Permanent Residents: 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. Two points should be kept in mind in interpreting our definition of permanent residents. First, our definition 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 her earnings in adulthood.[20] Second, 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 1993 birth cohort, we measure parents' location between 3 and 19. This creates measurement error in children's childhood environment that is larger in earlier birth cohorts. Fortunately, we find that our results do not vary significantly across birth cohorts, and in particular remain similar for the most recent birth cohorts. The reason such measurement error turns out to be modest empirically is that most families who stay in a given area for several years tend not to have moved in the past either. For example, among families who stayed in the same CZ c when their children were between ages 16-24, 81.5% of them lived in the same CZ when their children were age 8. Table I presents

[19]We limit the sample to parents with positive income because parents who file a tax return (as required to link them to a child) yet have zero income are unlikely to be representative of individuals with zero income and those with negative income typically have large capital losses, which are a proxy for having significant wealth.

[20]For example, in the 1980-82 birth cohorts, 38% of children live in a different CZ in 2012 relative to where their parents lived in 1996 (Chetty et al. 2014).

the summary statistics for the permanent residents of CZs sample. There are approximately 44 million children in our full sample, 22.9M of whom we observe at ages 24 and above.

Movers: We allocate those whose parents do not stay in the same CZ into our CZ movers sample. Table I illustrates there are 16.5M total movers across CZs in our full analysis sample. 7.8M of these children move just once during 1996-2012, 4.7M move twice, 2M move 3 times, and 2M move more than 3 times.

County. We also repeat our process of defining permanent residents and movers using the county-level definition of geography. Here, we have 19.9M permanent residents who we observe incomes at or above age 24. We also focus below on a sample of 1-time movers across counties. Of these who we can observe outcomes above age 24, 654K children move just once across CZs and 617.5K children move just once across counties within CZs.

III.B Variable Definitions and Summary Statistics

In this section, we define the key variables we use to measure intergenerational mobility. We measure all monetary variables in 2012 dollars, adjusting for inflation using the consumer price index (CPI-U).

Parent Income. Following Chetty et al. (2014), our primary measure of parent income is total pre-tax income at the household level, which we label *parent family income*. More precisely, in years where a parent files a tax return, we define family income as Adjusted Gross Income (as reported on the 1040 tax return) plus tax-exempt interest income and the non-taxable portion of Social Security and Disability benefits. In years where a parent does not file a tax return, we define family income as the sum of wage earnings (reported on form W-2), unemployment benefits (reported on form 1099-G), and gross social security and disability benefits (reported on form SSA-1099) for both parents.[21] In years where parents have no tax return and no information returns, family income is coded as zero.[22]

Our baseline income measure includes labor earnings and capital income as well as unemploy-

[21]The database does not record W-2's and other information returns prior to 1999, so non-filer's income is coded as 0 prior to 1999. Assigning non-filing parents 0 income has little impact on our estimates because only 2.9% of parents in our core sample do not file in each year prior to 1999 and most non-filers have very low W-2 income (Chetty et al. (2014)). For instance, in 2000, median W-2 income among non-filers was $29. Furthermore, defining parent income based on data from 1999-2003 (when W-2 data are available) yields virtually identical estimates (Chetty et al. (2014)). Note that we never observe self-employment income for non-filers and therefore code it as zero; given the strong incentives for individuals with children to file created by the EITC, most non-filers likely have very low levels of self-employment income as well.

[22]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.

ment insurance, social security, and disability benefits. It excludes non-taxable cash transfers such as TANF and SSI, in-kind benefits such as food stamps, all refundable tax credits such as the EITC, non-taxable pension contributions (e.g., to 401(k)'s), and any earned income not reported to the IRS. Income is always measured prior to the deduction of individual income taxes and employee-level payroll taxes.

In our baseline analysis, we average parents' family income over the five years from 1996 to 2000 to obtain a proxy for parent lifetime income that is less affected by transitory fluctuations (Solon 1992). We use the earliest years in our sample to best reflect the economic resources of parents while the children in our sample are growing up.[23] This approach implies that the age of the child when the parental income is measured will vary across cohorts. However, all of our analysis below will be done conditional on a child's cohort.

Parent Location. Following Chetty et al. (2014), children are assigned ZIP codes of residence based on their parents' ZIP code on the form 1040 in which the parent is matched to the child. In the few cases where a parent files a F1040 claiming the child but does not report a valid ZIP code, we search information returns (such as W-2 and 1099-G forms) for a valid ZIP code in that year. We map these ZIP codes to counties based on the 1999 Census crosswalk between ZIP codes and counties.[24] To account for zipcode changes over time, we match missing zipcodes to the 2011 zipcode to county crosswalk constructed by the department of housing and urban development. We then assign counties to commuting zones using the crosswalk provided by David Dorn.[25]

Child Income. We define child family income in exactly the same way as parent family income, however we measure it separately at different ages of the child (age 24-30) and we define household income based on current marital status rather than marital status at a fixed point in time. Because family income varies with marital status, we also report results using individual income measures for children, constructed in the same way as for parents. We define individual income as the sum of individual W-2 wage earnings, UI benefits, SSDI payments, and half of household self-employment

[23]Formally, we define mean family income as the mother's family income plus the father's family income in each year from 1996 to 2000 divided by 10 (or divided by 5 if we only identify a single parent). For parents who do not change marital status, this is simply mean family income over the 5 year period. For parents who are married initially and then divorce, this measure tracks the mean family incomes of the two divorced parents over time. For parents who are single initially and then get married, this measure tracks individual income prior to marriage and total family income (including the new spouse's income) after marriage. These household measures of income increase with marriage and naturally do not account for cohabitation; to ensure that these features do not generate bias, we assess the robustness of our results to using individual measures of income.

[24]We also assign geographic location based on the latitude and longitude of these zipcode centroids provided in this crosswalk.

[25]See download E6 on http://www.ddorn.net/data.htm, also available at http://www.equality-of-opportunity.org/data.

income (see Online Appendix A of Chetty et al. (2014) for more details)

College Attendance. We define college attendance as an indicator for having one or more 1098-T forms filed on one's behalf when the individual is aged 18-23. Title IV institutions – all colleges and universities as well as vocational schools and other post-secondary institutions eligible for federal student aid – are required to file 1098-T forms that report tuition payments or scholarships received for every student. Because the 1098-T forms are filed directly by colleges independent of whether an individual files a tax return, we have complete records on college attendance for all children. The 1098-T data are available from 1999-2012. Comparisons to other data sources indicate that 1098-T forms capture college enrollment quite accurately overall (Chetty et al. (2014), Appendix B).[26]

Teenage Birth. We define an indicator of teenage birth if the child is listed as a parent on a birth certificate between the ages of 13 and 19, using data on the birth certificates for the U.S. population.[27]

Teenage Employment. We construct an indicator of teen employment simply as an indicator of filing a form W-2 in the year in which the child is age a. We focus primarily on ages 16-18. Because these outcomes are measured earlier in a child's life, they allow us to extend the cohorts considered in this analysis to the 1996 cohort.

Summary Statistics. Table I reports summary statistics for the full sample of non-movers and movers. Mean parent family income is \$79,802 for CZ non-movers and \$71,422 for those that move 1-3x between 1996-2012 (in 2012 dollars). Children in our non-movers sample have a median family income of \$35,400 when they are approximately 30 years old and \$32,000 in the 1-3x movers sample. 69% of non-movers and 63.6% of 1-3x movers are enrolled in a college at some point between the ages of 18 and 23. 11% of women non-movers have a teenage birth and 13.7% of 1-3x women movers have a teenage birth.

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

[27]Birth certificate information comes from the DM-2 database maintained by the Social Security Administration. Comparing the data to population birth records from the CDC suggests that the 2008-2012 records appear to miss roughly 10% of births in the U.S. To verify the robustness of our results, we have replicated all of our analysis using dependent claiming to define teen birth; we define a woman as having a teen birth if she ever claims a dependent who was born while she was between the ages of 13 and 19. We obtain very similar results using this measure of teen birth. However, we do not use this definition as our primary measure since it only covers children who are claimed as dependents by their mothers (as opposed to, say, grandparents).

Part 1: Estimates of Childhood Exposure Effects

IV Baseline Estimates of Childhood Exposure Effects

In this section, we address our first empirical objective: assessing how much of the difference in observed outcomes across neighborhoods in the U.S. reflects causal effects of place. We begin by characterizing the heterogeneity in the earnings of children of permanent residents across commuting zones. We then turn to the sample of families that move across CZs to estimate the effects of childhood exposure to areas where permanent residents have better outcomes.

IV.A Geographical Variation in Outcomes of Permanent Residents

We begin by characterizing spatial variation in the outcomes of children who grew up in a single area for their entire childhood. Our analysis builds closely on Chetty et al. (2014), and much of this subsection is drawn from that study. The main difference is that here we focus on children whose families never move in order to characterize spatial variation for "permanent residents" rather than all children.

We first document the mean outcomes of children of permanent residents. To account for the fact that neighborhoods may have different effects across parent income levels and over time, we measure children's mean incomes conditional on parent income in each CZ, separately for each birth cohort. Chetty et al. (2014) show that measuring parent and children incomes using percentile *ranks* (rather than dollar levels or logs) has significant statistical advantages. Following their approach, we measure the percentile rank of the parents of child $p(i)$ based on their positions in the *national* distribution of parents who have children in child i's birth cohort. Similarly, we define children's percentile ranks y_i based on their positions in the national distribution of child incomes within their birth cohorts.

Figure 1 shows how we calculate mean outcomes for children born in 1985 to parents who are permanent residents of the Chicago CZ. This figure plots the mean child rank at age 26 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. This linearity of the rank-rank relationship is a very robust property across CZs (Chetty et al. (2014), Online Appendix Figure IV). Exploiting this linearity, we can parsimoniously summarize the rank-rank relationship for permanent residents of CZ c in birth cohort s by regressing child rank on parent rank:

$$y_i = \alpha_{cs} + \psi_{cs} p_i + \varepsilon_i. \tag{7}$$

We then define the expected rank of a child in birth cohort s whose parents have a national income rank of p and are permanent residents of CZ c as the fitted values from this regression:

$$\bar{y}_{pcs} = \alpha_{cs} + \psi_{cs} p. \tag{8}$$

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

Figure II presents a heat map of children's mean rank outcomes at age 26 given parents at the 25th percentile (Panel A) and 75th percentile (Panel B) of the national income distribution. Appendix Figure VI replicates these maps using age 30 outcomes. We construct these maps by dividing CZs into deciles based on their estimated value of $\bar{y}_{25,c,1985}$ and $\bar{y}_{75,c,1985}$. Lighter colors represent deciles with higher mean outcomes. As documented by Chetty et al. (2014), there is significant variation in children's mean outcomes across CZs, especially for children from low-income families. For example, the population-weighted standard deviation (SD) of $\bar{y}_{25,c,1985}$ across CZs is 3.6 percentiles, while the SD of $\bar{y}_{75,c,1985}$ is 2.8 percentiles. Places where low income children do well are not always the same as those where high-income children do well.[28] For instance, low-income children in California do particularly well, but high-income children do not. See Section V.C of Chetty et al. (2014) for a more detailed discussion of the key spatial patterns in these maps.

The spatial heterogeneity documented in Figure II is consistent with prior work documenting heterogeneity in children's outcomes based on where they grew up in observational data. The key question is whether these differences in outcomes are driven by the causal effects of place or differences in the people who live in each place. We turn to this issue in the next subsection.

IV.B Baseline Estimates of Exposure Effects

IV.B.1 Setup

We identify β_m – defined in equation (4) as the effect of moving at age m to a neighborhood where prior residents have one percentile better outcomes $\bar{y}_{pcs}$ – by studying the outcomes of children whose families move across neighborhoods with children of different ages. To align with the conceptual experiment, we focus on the sample of movers who have only 1 origin and 1 destination CZ and stay in the destination for at least 2 years (i.e. move prior to 2011 in our sample). This results in a sample of 6.9M movers, roughly 3.2M of which we observe at ages 24 and above. For the baseline analysis, we add two additional restrictions: we restrict attention to families that moved

[28] The correlation between $\bar{y}_{25,c,1985}$ and $\bar{y}_{75,c,1985}$ is 0.56.

more than 100 miles from their prior location and we restrict attention to CZ's with a population above 250,000 based on the 2000 Census. These restrictions exclude roughly half of the 1-time movers sample, rendering an analysis sample size of 1,553,021 for children with outcomes observed at age 24 and above, as shown in Table 1. We impose the distance restriction to remove cases where families move short distances but happen to cross our discrete CZ boundaries. We impose the 250K population restriction to ensure we have a very high quality measure of the outcomes of permanent residents. This larger population (and hence greater precision for permanent resident outcomes) is not essential for the baseline estimates, but for some of the tests that follow, the larger sample size enables very precise tests for selection effects.[29]

To simplify exposition, we begin by focusing on families who move across neighborhoods exactly once between 1996 and 2012. We then show that including families who move multiple times yields similar results. Let $m(i)$ denote the age at which child i moves neighborhoods in the one-time movers sample. Let $o(i)$ denote the child's origin neighborhood (where he lives until age $m-1$) and $d(i)$ denote the destination (where he lives from m to T_C). We identify β_m by comparing the mean outcomes of children whose families start in the same area o and move to different areas d at a given age m.

To begin, consider the following fixed-effects regression using the set of movers at a fixed age m:

$$y_i = \alpha_{qos} + b_m \Delta_{odps} + \eta_{1i}, \tag{9}$$

where α_{qos} denotes a fixed effect for each origin o by parent income decile q in birth cohort s and $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$ is the difference in predicted outcomes of permanent residents in the destination versus origin for the given parent income level p and birth cohort s.[30] Note that with origin-by-parent income fixed effects, this regression yields similar estimates if we replace Δ_{odps} with $\bar{y}_{pos}$. We use the Δ_{odps} notation here as it will remain the variable of interest later on when we also identify b_m using variation from the origin conditional on the destination.

Figure III presents a non-parametric analog of the regression in (9) for children who move at

[29] Appendix Table II shows that impact of removing these restrictions on the baseline results is fairly minor: we obtain an attenuation of the estimates by 10-20%. This attenuation is to be expected with measurement error in the permanent residents outcomes and coarseness of CZ boundaries without the distance restriction. Not imposing the population restriction does lead to significant attenuation in the family fixed effects specifications. This is to be expected because outcomes of permanent residents are estimated separately by cohort, which generates greater within-family measurement error than cross-family measurement error. By focusing on CZs with populations above 250K, we are able to abstract from issues associated with measurement error in permanent resident outcomes.

[30] We use parent income deciles rather than percentiles to define the fixed effects to simplify computation. In practice, we find that using finer bins to measure parent income groups has little effect on the results.

age $m = 13$. To construct this binned scatter plot, 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: $y_i^r = y_i - E[y_i|q, o, s, m]$ and $\Delta_{odps}^r = \Delta_{odps} - E[\Delta_{odps}|q, o, s, m]$. We then divide the Δ_{odps}^r residuals into twenty equal-size groups (vingtiles) and plot the mean value of y_i^r vs. the mean value of Δ_{odps}^r in each bin.[31]

Figure III shows that children who move to areas where children of permanent residents earn more as adults themselves have higher income ranks in adulthood. The estimated coefficient of $b_{13} = 0.629$ implies that a 1 percentile increase in $\bar{y}_{pds}$ is associated with a 0.629 percentile increase in y_i for the children who move at age 13. This regression coefficient combines the causal effect of moving to a better area (β_m) with a selection effect, namely that children whose families move to better areas may have better family environments as well. Formally, in Online Appendix A, we show that the coefficient b_m in this regression can be written as

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

where the selection effect

$$\delta_m = \frac{cov(\theta_i, \bar{y}_{pds}^r)}{var(\bar{y}_{pds}^r)}$$

measures the relationship between mean family inputs $\theta_i = \frac{1}{T_C} \sum \theta_{it}$ and mean destination quality $\bar{y}_{pds}$ for children who move at age m conditional on parent decile by origin by cohort fixed effects. In general, we expect the selection effect $\delta_m > 0$ based on our model because families that seek better neighborhoods for their children will also invest more in their children directly.

IV.B.2 Exposure Effects

To separate selection effects δ_m from the causal effect β_m, we compare children who move at different ages under the following identification assumption, which we evaluate in detail in subsequent sections.

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 *timing* of when families move to better or worse areas is orthogonal to mean inputs $\bar{\theta}_i$ and mean disruption costs, $\bar{\kappa}_i$. Under this assumption, we can obtain consistent estimates of the exposure effect at age m – i.e., the effect of spending year

[31] The regression coefficients and standard errors reported are estimated on the underlying microdata using OLS regressions.

m in a better area – using $b_m - b_{m+1} = \beta_m - \beta_{m+1}$. We can go further and estimate δ by studying the outcomes of children whose families move *after* their income is measured, e.g. in period $t \geq 26$ if income is measured at age 26. Because such moves cannot have a causal effect on children's outcomes at age 26, the coefficient $b_m = \delta$ for $m \geq 26$ under Assumption 1. Using the estimated selection effect, we can identify the causal effect of moving to a better area at age m as $\beta_m = b_m - \delta$.

We implement this strategy in Figure IV. In Panel A of Figure IV, the series in circles reports estimates of (9) for each age m between 11 and 30, measuring children's income at age 26. To increase precision, we pool all cohorts and estimate a single regression including separate interactions for Δ_{opds} for each age of move. Let M_i denote a vector that indicates the year in which child i's family moves; formally, M_i is a vector of length T with all elements equal to 0 except element $m(i)$, which is equal to 1. Similarly, let S_i denote a vector that indicates child i's birth cohort; it has all elements equal to 0 except element $s(i)$, and omits the most recent cohort for which data is available (1986 for outcomes measured at age 26). We run the regression:

$$y_i = \alpha_{qosm} + B' M_i \Delta_{odps} + \alpha' M_i + C' S_i \Delta_{odps} + \eta_{2i} \tag{10}$$

where $B' M_i \Delta_{odps} = \sum_m b_m \Delta_{odps}$ and $C' S_i = \sum_{s<\bar{s}} c_s \Delta_{odps}$. The estimates of $B = \{b_m\}$ decline linearly until approximately age 23, after which they level off and remain constant at a value of approximately 0.178. The linear decline is consistent with an exposure effect, i.e. that moving to a better neighborhood earlier in childhood yields larger improvements in long-term outcomes. The fact that $b_m > 0$ for $m > 26$ is direct evidence of a selection effect ($\delta > 0$).

The series in triangles in Figure IVa replicates the series in circles, measuring children's income ranks at age 24 instead of 26. This allows us to estimate b_m starting at age 9 and reveals that the linear exposure effect pattern continues back to age 9.[32] The insensitivity of our estimates to the age of outcome measurement may be surprising given that children's income ranks change rapidly in their mid 20's, with college graduates experiencing steeper wage growth as they enter the labor force (Haider and Solon (2006), Chetty et al. (2014)). However, our estimates of b_m are based on the extent to which the incomes of children who move correlate with the incomes of permanent residents in the destination measured at the *same age*. The incomes of permanent residents serve

[32] In Appendix Figure II, we replicate our baseline specification measuring income at ages 24, 26, 28, and 30. Measuring income at later ages restricts the age range over which we can study moves – for age 30 outcomes we can study moves starting at age 15. All four series display very similar patterns in the overlapping age ranges, showing that our estimates of b_m are not very sensitive to the age at which we measure children's incomes in adulthood. Moreover, all series pivot to a flat line above age 23, suggesting age 24 is the earliest age for which one can measure income outcomes from exposure effects. Section VI.A applies the baseline specification to other outcomes that can be measured at younger ages, including teen labor force participation, teen birth, and college attendance.

as goalposts that allow us to measure convergence in incomes at relatively early ages in adulthood, even before we observe children's permanent income.[33] We therefore measure income at age 24 in the remainder of this section in order to study moves at earlier ages.

When measuring income at age 24, we interpret the coefficients above age 23 as reflecting selection. The linearity of the relationship between b_m and the age at move m in Figure IVa below age 23 implies that the exposure effect $b_m - b_{m+1} = \beta_m - \beta_{m+1}$ is approximately constant with respect to age at move m. We estimate a slope of these points of -0.044 below age 24. That is, moving one year earlier to an area with 1 percentile better outcomes produces a 0.044 (s.e. = 0.0018) percentile improvement in earnings.

The estimated slope after age 23 is 0.001 (s.e. = 0.011). The fact that this slope is not significantly different from 0 is consistent with the assumption that selection effects $\delta_m = \delta$ do not vary with age. Extrapolating the line above age 23 to age 23 implies an estimate of $\delta = 0.125$. Moreover, the absence of any discrete jump in these coefficients around the year of income measurement suggests there is no discontinuous effect of arriving in an area that produces good outcomes just before age 24. It follows that under Assumption 1, the causal effect of moving at age m to an area with one percentile better outcomes and staying in the area until age 23 is $\beta_m = (23 - m) \times 0.044$.

The preceding analysis implicitly assumes that children move with their parents until age 23. In practice, not all children follow their parents, particularly after they complete high school at age 18. To account for this issue, note that the estimates of b_m in (10) can 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 families moved at age m. We can obtain treatment-on-the-treated (TOT) estimates for the children who actually move by inflating the ITT estimates by the fraction of children who move at each age m, ϕ_m: $\beta_m^{TOT} = (b_m - \delta)/\phi_m$. We measure ϕ_m as the fraction of children who are claimed as dependents, attend college, or work in the destination CZ in the years after the parental move.[34] Online Appendix Figure III plots the TOT estimates β_m^{TOT} and the ITT estimates $\beta_m = b_m - \delta$ for $m \leq 23$ using $\delta = 0.125$ as estimated above. We estimate a slope for

[33]For example, suppose a good neighborhood c sends many children to college and generates relatively low incomes at age 24. In this case, we will obtain a *higher* estimate of b_m if a child who moves to area c has a *low* level of income at age 24. We do not study income before age 24 because a large fraction of children are enrolled in college at earlier ages; instead, we directly study college attendance as an outcome below.

[34]More precisely, for children less than or equal to 18 at the time of the move, we define moving with one's parents as ever being claimed by parents filing from the destination CZ or ever having a W-2 or 1098-T (college attendance form) filed from the destination CZ. For children above age 18, we define moving as ever having a W-2 or 1098-T in the destination CZ.

β_m^{TOT} of 0.040, in contrast to the "ITT" slope of 0.044. The TOT and ITT estimates line up very closely, for two reasons. First, virtually all children move with their parents below age 18. Second, between ages 18 and 23, approximately 59% of children move with parents on average. Because the treatment effects β_m converge toward 0 as m approaches 23, inflating these values by ϕ_m has a second-order impact on the exposure effect gradient.

The TOT estimates show that the exposure impacts β_m decline between the ages of 18 and 23 for children who do move with their parents. When we measure income at age 24, we cannot determine whether the exposure effects stabilize after age 24 because moving after age 24 has no effect or because we measure income at that point. However, measuring income at later ages – e.g., age 26 as in Figure IVa or age 30 as in Appendix Figure II – reveals that the estimates of b_m are constant after age 23, suggesting that moving after that age has little causal effect on outcomes. In context of our model in Section II, this implies that $T_C = 23$, i.e. neighborhood environments affect children's long-term outcomes until they are in their early twenties.

Origin-Variation Experiment. Up to this point, the exposure coefficient is identified using variation in the quality of exposure to the destination, holding the origin fixed. An alternative source of variation is to consider two individuals who move to the same destination but differ in the quality exposure to their origin. To explore this, we can simply replace the α_{qosm} fixed effects in equation (10) with α_{qdsm} fixed effects that interact parent income decile, destination CZ, cohort, and age of the child at the time of the move. With this modification, b_m is identified from variation in the origins of the individuals, as opposed to the destinations. Online Appendix Figure IV presents the estimates of b_m. These estimates reveal a strikingly symmetric pattern relative to the estimates of b_m in Figure IVa: the later the family moved to the destination, the more the child's outcomes match the permanent residents in the origin, and this pattern levels off at around age 23.[35]

Parsimonious (Baseline) specification. The specification in equation (10) includes more than 200,000 fixed effects. This renders these specifications more difficult to implement in small samples and creates difficulty in introducing additional controls such as family fixed effects. As a more parsimonious alternative, we show here that one can drop these fixed effects and instead control for the outcomes of permanent residents in the origin and destination location. We replace the fixed effects, α_{qosm}, in equation (10) with indicators for the child's age at move, $\alpha' M_i$, interactions of the child's age at move with parental income, $B_p' M_i p$, cohort dummies, $\psi' S_i$, and interactions of cohort

[35]Indeed, the sum of the coefficients $b_m + b_m^o$ is close to 1 for each m.

dummies with the predicted rank outcomes in the origin, $C_o' S_i \bar{y}_{pos}$. As in equation (10), we include cohort interactions with Δ_{odps}, which is equivalent to simply controlling for cohort interactions with the predicted outcomes in the destination, $C' S_i \bar{y}_{pds}$.[36] We estimate the following regression specification:

$$y_i = B' M_i \Delta_{odps} + \alpha' M_i + B_p' M_i p + \psi' S_i + C' S_i \bar{y}_{pds} + C_o' S_i \bar{y}_{pos} + \eta_{3i}, \tag{11}$$

where B is a vector of coefficients, b_m, on the difference in predicted outcomes in the destination and origin location, α is a vector of age-at-move fixed effects, B_o is a vector of coefficients on the predicted outcome in the origin, B_p is a vector of coefficients on the parent rank, ψ is a vector of birth cohort fixed effects, and C and C_o are vectors of coefficients on predicted outcomes in the origin and destination interacted with birth cohort.

In this specification, the coefficients of interest are $B = \{b_m\}$, the impacts of moving at age m to an area where permanent residents have 1 percentile better outcomes relative to the origin. The $\alpha' M_i$ controls for difference in the child's age at the time of the move (e.g. disruption effects), and the $B_p' M_i p$ term controls for differences in children's outcomes by parent rank. The remaining terms control for the levels of the origin and destination predictions separately by birth cohorts. Allowing these controls to vary with birth cohort is potentially important because our ability to measure parent's locations during childhood varies across birth cohorts (since we only observe locations between 1996 and 2012, and children in our sample are born starting in 1980). As discussed in Section IV.A, this leads to greater measurement error in $\bar{y}_{pos}$ and $\bar{y}_{pds}$ for earlier birth cohorts, as we do not observe parent location in the early years of childhood for these cohorts.

Figure IVb plots the coefficients $\{b_m\}$ obtained from estimating (11). The coefficients are similar to those obtained from the more flexible specification used to construct Figure IVa. Regressing the b_m coefficients on m for $m \leq 23$, we obtain a slope of 0.038 (s.e. 0.02). We can also estimate this slope directly on the micro data. To do so, we further simplify the equation in (11) by parameterizing the coefficients $B' M_i \Delta_{odps}$ in Figure IVb using separate lines above and below age 23:

$$\begin{aligned} y_i = \alpha' M_i + \gamma I(m \leq 23)\Delta_{odps} + \beta I(m \leq 23)(23 - m)\Delta_{odps} + \gamma_{>23} I(m > 23)\Delta_{odps} \\ + \beta_{>23} I(m > 23)(23 - m)\Delta_{odps} + B_p' M_i p + \psi' S_i + C' S_i \bar{y}_{pds} + C_o' S_i \bar{y}_{pos} + \eta_{3i} \end{aligned} \tag{12}$$

Column (1) of Table 2 shows that the estimated exposure effect from this specification is $\beta = 0.040$

[36] As in equation (10), we omit the most recent birth cohort (1988 for income at age 24) interaction with Δ_{odps}.

(s.e. = 0.002), similar to the other estimates.[37] Intuitively, we are able to omit origin fixed effects because the origin prediction for permanent residents $\bar{y}_{pos}$ provides a good measure of the origin place effect μ_{pos}. Equations 11 and 12 form our baseline specifications for the remainder of the paper.

Columns (2)-(5) illustrate the robustness of the results to varying the sample and set of controls. Column (2) restricts the sample to $m \leq 23$; Column (3) restricts the sample to $m \leq 18$; Column (4) further restricts the sample in (3) to children who are claimed by in the destination CZ; Column (5) drops the cohort-varying controls for outcomes of prior residents, $C' S_i \bar{y}_{pds} + C'_o S_i \bar{y}_{pos}$, and replaces them with a single control for the outcome of those in the origin, $\bar{y}_{pos}$. In general, we find similar estimates across these specifications, with slightly attenuated coefficients in the specification without the cohort-varying controls for outcomes of prior residents.

The estimates of $B = \{b_m\}$ in equation (11) and (12) are identified from both the destination and origin of the movers. In contrast, to Figure IVa includes origin by age-at-move fixed effects so that only the destination variation identifies b_m. Using the parsimonious specification, we can introduce controls for the predicted outcomes of permanent residents in the origin interacted with the child's age at the time of the move, $B^o M_i \bar{y}_{pds}$. Column (6) presents the results from introducing these controls into equation 12. This yields an estimate of 0.041, similar to the baseline slope of 0.040.

We interpret the slope of β to reflect the effect of exposure time to neighborhoods while growing up. This contrasts with other ways in which neighborhoods could matter for children's adult outcomes, such as the quality of the local labor market. Indeed, the fact that 10 year olds look more like the destination outcomes than 20 year olds suggests it is not the result of a discrete - in or out - access to labor markets. To see this more clearly, Column (7) adds the child's CZ as a fixed effect (interacted with cohort) into the baseline specification. Of course, this specification controls for an endogenous outcome – as a result, the effect is attenuated to a slope of 0.03. But, it illustrates that the exposure pattern is better described as the result of exposure time to the area when growing up as opposed to a differential propensity to be in a particular labor market in adulthood.

Multiple Moves. The exposure time interpretation of our results is further supported by looking at the experience of multiple movers. The baseline specification in (12) provides a natural method

[37] The coefficient of 0.040 differs from the 0.038 slope in Figure IVb because of differential weighting across the age distribution when using the regression on the micro data.

for incorporating them into the analysis. Given a child with origin o, let d_j denote the jth destination location. We construct $\Delta^j_{odps} = \Delta_{od_jps} = \bar{y}_{pds} - \bar{y}_{pos}$ as the difference in the child's predicted outcome based on prior residents in destination j and the origin. We then multiply each Δ_j by the years of exposure below age 23 the child has in destination j.

Columns 8(a)-(c) of Table 2 presents estimates of the coefficients from a single regression that includes coefficients on the first, second, and third moves in the specification that generalizes equation (12) to incorporate exposure-time coefficients on each Δ_j for $j = 1, 2, 3$.[38] We generalize the controls by including $\sum_j C'_j S_i \bar{y}_{pds}$ instead of $C' S_i \bar{y}_{pds}$. And, we replace $\alpha' M_i$ with 3 terms for the number of years of under-23 exposure to the first, second, and third place. We replace $B'_p M_i p$ with 3 terms reflecting the interactions of the number of years of under-23 exposure to the first, second, and third destinations.

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

These results further support an exposure time interpretation over a theory of labor market access or age-specific effects. Children who leave before reaching adulthood still have outcomes correlated with their permanent resident counterparts in proportion to the time they spend growing up in the place. Along with the specification in Column (7) controlling for the child's location in adulthood, this again suggests that the effect is driven by where one grows up, as opposed to providing access to a particular labor market.

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

[38] As shown in Table 1, roughly 3% of the sample has more than 3 moves.

IV.B.3 Subgroup Heterogeneity

We also explore heterogeneity of effects across other sub-samples in Online Appendix Table III. Column 1 replicates the baseline analysis in equation (12). Columns 2 and 3 divide the sample into children whose parents have household income below or above the national median and replicate this baseline specification in these subsamples. We find significant exposure effects for both low- and high-income movers, with some evidence of larger effects for higher income populations. In Columns 4 and 5, we evaluate whether moves to areas with better or worse predicted outcomes relative to the origin neighborhood have different effects. Models of learning predict that moving to a better area will improve outcomes but moving to a worse area will not. In practice, we find little evidence of such an asymmetry: if anything, the point estimate of exposure effects for negative moves is larger. This result suggests that what matters for children's mean long-term outcomes is continuous exposure to a better environment. Finally, columns (6) and (7) report estimates separately by gender; here we find exposure effects of 0.041 for males and 0.042 for females. Overall, the exposure effect pattern is quite similar across subgroups.

IV.B.4 Summary

The key descriptive fact that emerges from the analysis above is that the outcomes of movers converge linearly to the outcomes of permanent residents of the destination area in proportion to time of exposure. Under Assumption 1 – i.e., that the types of families that move at different ages are comparable – this pattern implies that neighborhoods have causal exposure effects on children's long-term outcomes. The fact that the exposure impacts β_m are approximately linear implies that every additional year of exposure to a neighborhood where children have better outcomes – whether at age 9 or age 18 – has roughly the same benefit. This result implies that neighborhood environments have important effects well after early childhood. However, these conclusions rest on Assumption 1, which we now evaluate in detail.

V Quasi-Experimental Validation of Baseline Design

Assumption 1 could potentially be violated through differential sorting or omitted variables. In this section, we address these concerns using three methods. First, we control for observables and family fixed effects; second, we identify exposure effects using displacement shocks; third, we conduct outcome-based placebo tests, described in more detail in Section V.C.

V.A Sibling Comparisons and Controls for Observables

Our first approach to account for potential differences across children who move at different ages is to control for observable factors. It is useful to partition $\theta_i = \sum_{t=1}^{T_C} \theta_{it}$ into two components: a component $\bar{\theta}_i$ that reflects inputs that are fixed within families, such as parent genetics and education, and a residual component $\tilde{\theta}_i = \theta_i - \bar{\theta}_i$ that can vary over time within families, such as parents' jobs, marital status, or children's ability.

The most obvious potential violation of Assumption 1 is that families who invest more in their children (higher $\bar{\theta}_i$) move to better neighborhoods at earlier ages, which would bias our estimated exposure effect β upward. A natural method of controlling for differences in fixed family factors $\bar{\theta}_i$ is to include family fixed effects when estimating (11).[39] For example, consider a family that moves to a better area with two children, who are ages m_1 and m_2 at the time of the move. The exposure effect β is identified by the extent to which the *difference* in sibling's outcomes, $y_1 - y_2$, covaries with their age gap interacted with the quality of the destination CZ, $(m_1 - m_2)\bar{y}_{pds}$.[40] This sibling comparison nets out any variation due to fixed family inputs $\bar{\theta}_i$, as noted in prior work.

Table 3 and Figure Va present the results of adding family fixed effects to the baseline specification. Figure Va replicates Figure IVb with the addition off family fixed effects. We obtain a slope of 0.044, as shown in Column (4) of Table 3. This is similar to the baseline estimate of 0.04, replicated in Column (1). Column (5) adds controls for the age of the child at the time of the move interacted with the predicted outcomes of permanent residents in the origin. This yields a similar slope of 0.043, which is also similar to the analogous slope of 0.041 without family fixed effects, as shown in Column (2). Throughout, we obtain virtually the same pattern of exposure effects as in Figure IV.

As illustrated in Figure Va, the one parameter that does change is the level of the selection effect, δ. Once we include family fixed effects, the level of the selection effect (i.e., the level of

[39] The idea of using sibling comparisons to better isolate neighborhood effects dates was discussed in 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. More recently, Andersson et al. (2013) use a siblings design to estimate the impact of vouchers and public housing. Our analysis also relates to papers that seek to identify critical periods by studying immigrants (Basu (2010); van den Berg et al. (2014)). Our approach differs from Basu (2010), and van den Berg et al. (2014) in that we focus on how the difference in siblings' outcomes *covaries* with the outcomes of permanent residents in the destination neighborhood, whereas they effectively estimate the mean difference in siblings' outcomes as a function of the age gap.

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

b_m after age 24) becomes statistically insignificant.[41] This is precisely what one would expect if selection effects do not vary with children's ages, as in Assumption 1. The introduction of family fixed effects reduces the *level* of the b_m coefficients by accounting for selection, but does not affect the *slope* of the b_m coefficients.

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

Income and marital status are both strong predictors of children's outcomes in adulthood. Fortunately, we can directly control for these two time-varying factors in our data, as we observe parents' incomes and marital status in every year from 1996-2012. Figure Vb replicates Figure Va, controlling for changes parent income and parent marital status (in addition to family fixed effects). We construct the parental income rank by cohort by year, and use this to construct the difference in the parental income rank in the year after the move relative to the year before the move. We include this measure of income change and a full set of its interaction with $23-m$ and an indicator for $m > 23$. We also construct an indicator for the child's mother's marital status and construct four indicators for possible marital status changes (married $\rightarrow$ married, married $\rightarrow$ un-married, un-married $\rightarrow$ married, un-married $\rightarrow$ un-married). We then interact these four indicators with a full set of its interaction with $23 - m$ and an indicator for $m > 23$. Controlling for changes in parent income and marital status, in addition to family fixed effects, has little effect on the mean estimated exposure effect.[42]

Cohort Controls. The baseline specification includes separate controls for each cohort for the predicted outcome of permanent residents in the destination and origin. While the addition of these controls do not significantly alter the baseline specification, they do have some effects on the family fixed effect specification that are important to note. In particular, the level of the intercept is slightly declining in cohort, which is consistent with the origin being more accurately measured for later cohorts. Hence, comparisons between children born in the 1986 and 1988 cohort will naturally have a smaller slope in the absence of cohort-varying intercepts, because the intercept is generally

[41] The intercept, δ, is identified even with family fixed effects because $\bar{y}_{pds}$ varies across birth cohorts.

[42] Column (7) of Table 3 also shows a specification that includes a full set of parental income rank controls for each year (1996-2012) fully interacted with cohort dummies (in addition to family fixed effects). Here again, we obtain a similar exposure slope of 0.043 (s.e. 0.008).

higher for the 1986 cohorts than the 1988 cohorts. For this reason, we include cohort-varying intercepts in our baseline specification. However, to highlight the robustness, Column (3) drops these cohort controls in the baseline specification and Column (6) adds family fixed effects. With the introduction of family fixed effects, the estimated slope coefficient drops from 0.036 to 0.031, consistent with attenuation from the negative correlation between the intercept and the cohort and the increased reliance on cohort comparisons within as opposed to across families. Hence, our baseline analysis includes these cohort controls to prevent such bias.

Multiple moves. Column (9) of Table 3 presents results from the regression in Column (8) of Table 2 that incorporates all moves, in addition to 1-time movers. Here, we find a slope of 0.039 (s.e. 0.004), very similar to the analogous slope of 0.039 (s.e. 0.001) without family fixed effects.

Individual income. Our baseline specifications use the child's family income as the outcome of interest. Hence the baseline results incorporate cases where those with high earnings potentials choose to realize these potentials in the marriage market instead of the labor market. However, the results are robust to studying individual income. Column (10) of Table 3 illustrates that the exposure time slope when measuring individual income as the outcome is 0.036 with family fixed effects, similar to the baseline slope of 0.040 for individual income, shown in Column (10) of Table 2.

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

V.B Displacement Shocks

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

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

While many of the families who move in subsamples with large values of k_{zt} do so for exogenous reasons, their destination d is still the result of an endogenous choice that could lead to bias. For example, families who choose to move to better areas (higher $\bar{y}_{pds}$) when induced to move by an exogenous shock might also invest more in their children. To eliminate potential biases arising from endogenous choices of destinations, we isolate variation arising purely from the *average* change in neighborhood quality for individuals who are displaced. Let $E[\Delta_{odps}|q,z]$ denote the mean predicted outcome in the destinations to which individuals in origin zipcode z and parent decile q move. We instrument for the difference in predicted outcomes in each family's destination relative to origin (Δ_{odps}) with $E[\Delta_{odps}|q,z]$ and estimate (12) using 2SLS to obtain IV estimates of exposure effects, β^{IV}. Intuitively, β^{IV} is identified by asking whether displacement shocks that happen to occur in areas where more families to areas with better average outcomes for children generates larger improvements in outcomes for children

Figure VI presents the results of this analysis. To construct this figure, we take ZIP-year cells with above-average outflows ($k_{zt} > 1$) and divide them into (population-weighted) deciles based on the size of the shock k_{zt}. To ensure that large outflows are not simply driven by very small underlying populations, we exclude zipcode-by-year cells with less than 10 children leaving in the year.[43] The first point in Figure VI shows the estimate of β using all observations with $k_{zt} > 1$. The second point shows the estimate of β using all observations with k_{zt} at or above the 10th percentile. The remaining points are constructed in the same way, with the last point representing an estimate of β using data only from ZIP codes in the highest decile of outflow rates. The dotted

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

lines show a 95% confidence interval for the regression coefficients.

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

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

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

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

The outcome-based placebo tests exploit plausible assumptions about the preferences and information set of parents choosing to move to different locations. Recall that in equation (3), we assume parents choose locations to maximize their expected utility given their opportunities and their information set, Ω. Bias arises in our baseline estimates of equation (12) if parents that choose different levels of θ_i are choosing different levels of exposure to good places for their children.

Equation (12) implies that Δ_{odps} is a sufficient statistic for measuring the impacts of places on children's outcomes. We let $\Delta_{odps}^{placebo}$ denote a "placebo" prediction if the difference between the true prediction and the placebo prediction, $\Delta_{odps}^{placebo} - \Delta_{odps}$, is either not known to the individual at the time parents choose neighborhoods or not a factor that enters into the parental decision to move to the place. As a result, when parents select good (or bad) places, as measured by Δ_{odps}, they will on average select places that are good (or bad) as measured by $\Delta_{odps}^{placebo}$. Hence, adding $\Delta_{odps}^{placebo}$ to the regressions in equation (12) provides a test of omitted variable bias, providing a source of validation for the baseline design on the full sample of moves. We construct outcome-based placebos along three dimensions: birth cohorts, quantiles of the income distribution, and child gender.

Birth Cohorts. Place effects are generally quite stable across cohorts: the autocorrelation of $\bar{y}_{pcs}$ with $\bar{y}_{pc,s-1}$ is 0.95 at $p = 25$ and 0.92 at $p = 75$. Good places in one year are, on average, good places in the next year. However, outcomes in some areas (such as Oklahoma City) have improved over time, while others (such as Sacramento) have gotten worse.[44] Since the causal effect of an area c on a child i's outcomes depends on the properties of the area in the years the child lives there, permanent residents' outcomes $\bar{y}_{pc,s(i)}$ for a child's own birth cohort $s(i)$ should be much stronger predictors of exposure effects than $\bar{y}_{pcs}$ for other cohorts. In contrast, while parents may know that some areas are better than others for improving their children's outcomes, it is unlikely that they know whether a place is particularly good for their child's own cohort relative to nearby cohorts, as these outcomes are realized 10-15 years after the move.

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

$$Cov(\theta_i, m\Delta_{odp,s(i)}|X) > 0 \Rightarrow Cov(\theta_i, m\Delta_{odps'}|X, m\Delta_{odp,s(i)}) > 0 \tag{13}$$

where X corresponds to the additional control variables in equation (12). Under this assumption, mean outcomes for permanent residents in *other* birth cohorts ($s' \neq s(i)$) in the destination CZ can be used to test between selection and causal effects of neighborhoods. Let $t = s - s(i)$ index birth cohorts relative to a child's true cohort $s(i)$. We implement such placebo tests by estimating linear exposure effect models of the following form:

$$\begin{aligned} y_i = \alpha' M_i + \sum_{t=-4}^{4} \{ \tilde{\gamma}_t I(m \leq 23)\Delta_{odpt} + \tilde{\beta}_t I(m \leq 23)(23 - m)\Delta_{odpt} + \gamma_o I(m \leq 23)\Delta_{odpt} \\ + \beta_o I(m \leq 23)(23 - m)\Delta_{odpt} \} + \kappa_t \bar{y}_{pot} + B_p' M_i p + \psi' S_i + C' S_i \bar{y}_{pds} + C_o' S_i \bar{y}_{pos} + \eta_{3i} \end{aligned} \tag{14}$$

This equation replicates our baseline model in (12), except that we include not just the difference in the predicted outcomes based on permanent residents in the destination relative to the origin,

[44] In Oklahoma City, $\bar{y}_{pcs}$ at $p = 25$ went from 43.0 for the 1980 cohort to 46.3 for the 1986 cohort. Conversely, in Sacramento $\bar{y}_{pcs}$ at $p = 25$ went from 46.6 to 42.5. College attendance rates followed a similar pattern. Compared to the national average increase in college attendance for $p = 25$ of 5.6pp between the 1981 and 1988 cohorts, Oklahoma city increased 8.4pp (50.1% in the 1981 cohort to 58.5% in the 1988 cohort) and Sacramento increased 2.5pp (52.8% to 55.3%).

$\Delta_{odps(i)}$ for the child's own cohort, but also the predictions for the four preceding and subsequent cohorts Δ_{odpt}.

To illustrate the resulting patterns, the series in red triangles in Figure VII plots $\tilde{\beta}_t$ when we estimate (14) including only the predicted outcome for a single cohort $\bar{y}_{pdt}$. In other words, we exchange Δ_{odps} with Δ_{odpt} in the baseline regressions. Here, the estimates of $\tilde{\beta}_t$ are similar to our baseline estimate of $\beta = 0.040$ for the leads and lags, which is to be expected given the high degree of serial correlation in place effects.

The series in blue circles in Figure VII plots the coefficients, $\tilde{\gamma}_t$, in equation (14) for $t = -4, ..., 4$, Here, we find small coefficients on the placebo exposure effects ($\tilde{\gamma}_t$ for $t \neq 0$). Moreover, the exposure effect estimate for the correct (own cohort, $t = 0$) coefficient drops only slightly relative to the baseline estimate of $\beta = 0.040$ when we include predictions from surrounding cohorts.

Under (13), the results in Figure VII imply that our baseline estimates of β are unbiased, i.e. that Assumption 1 holds. Intuitively, the fact that children's outcomes do not correlate in an exposure-dependent manner with the predictions from other cohorts, conditional on the own-cohort prediction, implies that our estimates of β reflect causal neighborhood effects, which are cohort-specific, rather than omitted variables resulting from correlations of neighborhood choice and other parental inputs, which are *not* cohort-specific under (13). The logic of this test is analogous to an event study: provided that unobserved shocks θ_i do not happen to covary exactly with the destination place effect for the child's own cohort and not surrounding cohorts, the coefficient at $t = 0$ in Figure VII identifies the causal effect of exposure to a better area.

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

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

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

$$Cov(\theta_i, m\Delta^q_{odp,s(i)}|X^q) > 0 \Rightarrow Cov(\theta_i, m\Delta_{odps}|X^q, m\Delta^q_{odp,s(i)}) > 0 \tag{15}$$

where X^q are the control variables in equation (12) with the modification that $C'S_i\bar{y}_{pds} + C'_oS_i\bar{y}_{pos}$ is replaced with $C'S_iq_{pds} + C'_oS_iq_{pos}$. For example, it seems natural to assume that if parents are sorting to places where children are likely to end up in the top 10% of the income distribution then they're also sorting to places where, on average, children have higher incomes. Under the assumption in equation (15), the heterogeneity of exposure effects across the income distribution can be used to test between selection and causal effects. We implement these tests by focusing on outcomes in tails: reaching the top 10% of the income distribution or the bottom 10% of the income distribution.

We begin by constructing predictions of the probability of having an income above the 90th percentile or below the 10th percentile of the national income distribution at age 24 for children of permanent residents in each CZ c. We regress an indicator for being in the upper or lower 10% on parent ranks within each CZ using an equation analogous to (7) but that includes a quadratic term in parental income to account for nonlinearities at extreme quantiles identified in Chetty et al. (2014). We then calculate the predicted probability of being below the 10th percentile π^{10}_{pcs} and above the 90th percentile π^{90}_{pcs} using the fitted values from these regressions, as in (8).

Figure VIIIa presents a binned scatter plot of the probability a child is in the top 10%, y^{90}_i vs. the destination prediction π^{90}_{pds} and the mean rank prediction $\bar{y}_{pds}$ in the sample of children who move at or before age 13. The series in circles shows the non-parametric analog of a partial regression of a child's outcome on π^{90}_{pds}, controlling for the $\bar{y}_{pds}$ and the analogous predicted outcomes based on prior residents in the origin, π^{90}_{pos} and $\bar{y}_{pos}$. To construct this series, we regress both y^{90}_i

and π^{90}_{pds} on the mean predicted income rank, $\bar{y}_{pds}$, and the analogous origin controls, π^{90}_{pos} and $\bar{y}_{pos}$, bin the π^{90}_{pcs} residuals into 20 equal-sized bins, and plot the mean residuals of y^{90}_i vs. the mean residuals of π^{90}_{pcs} within each bin. The series in triangles is constructed analogously, except that we plot residuals of y^{90}_i vs. residuals of $\bar{y}_{pcs}$, the predicted mean rank.

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

Figure VIIIb replicates Figure VIIIa using non-employment (roughly the bottom 10%) as the outcome instead of reaching the top 10%. Once again, we find that children's probabilities of reaching the lower tail are strictly related to the predicted probability of reaching the lower tail based on permanent residents' outcomes rather than the predicted mean outcome. The fact that mean predicted outcomes of permanent residents $\bar{y}_{pcs}$ have no predictive power implies that other omitted factors, which are not quantile-specific under (15), do not drive our findings.

In Table IV, we estimate exposure effect models analogous to (12) using the distributional predictions instead of mean predictions. In Columns 1-3, the dependent variable is an indicator for having income in the top 10% of the income distribution. Column 1 replicates the baseline specification in equation (9), using $\Delta^{90}_{odps} = \pi^{90}_{pds} - \pi^{90}_{pos}$ instead of the mean prediction $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$.[45] We obtain an exposure effect estimate of $\beta = 0.043$ per year in this specification, similar to our baseline estimates. In Column 2, we use the mean prediction Δ_{odps} instead. Here, we obtain an estimate of 0.022, which is to be expected given the high degree of correlation in place effects across quantiles: places that push children into the top 10% also tend to improve mean outcomes. In Column 3, we include both the quantile prediction Δ^{90}_{odps} and the mean prediction Δ_{odps}, identifying the coefficients purely from differential variation across quantiles within CZs. Consistent with the findings in Figure VIII, we find that the coefficient on the quantile prediction remains unchanged at approximately 0.04, while the coefficient on the mean prediction is not significantly different from 0.

Columns 4-6 of Table IV replicate columns 1-3, using an indicator for being unemployed (defined

[45] Analogous to the baseline specification, we include cohort dummy interactions with π^{90}_{pds} and π^{90}_{pos}.

as an indicator for not having a W-2) as the dependent variable and using the prediction for being unemployed, Δ^{U}_{odps} instead of Δ^{90}_{odps} as the key independent variable. We find very similar patterns: children's probabilities of being in the lower tail of the income distribution are strongly predicted by the quantile-specific prediction rather than the mean prediction. In sum, we find evidence of distributional convergence: controlling for mean outcomes, children's outcomes converge to predicted outcomes in the destination across the distribution in proportion to exposure time, at a rate of approximately 4% per year.[46] Under the assumption in equation (15), these results imply that our exposure effect estimates are driven by causal effects of neighborhoods rather than other unobserved factors. Intuitively, it would be quite unlikely that omitted variables (such as changes in parent wealth) would happen to perfectly replicate the entire distribution of outcomes in each area.

Gender. Finally, we conduct an analogous set of placebo tests using heterogeneity in place effects by child gender. To implement these tests, we first construct gender-specific predictions of the mean outcomes of children of permanent residents. We estimate the relationship between child and parent ranks within each CZ using (7) separately for boys and girls. We then define $\bar{y}^{g}_{pcs}$ as the mean predicted outcome for permanent residents of CZ c in birth cohort s and gender $g \in \{m, f\}$, as in (8).

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

Figure IX presents a binned scatter plot of children's ranks vs. the difference in the destination and origin prediction, Δ^{g}_{odps}, for their own gender (circles) and the prediction Δ^{-g}_{odps} for the other gender (triangles) in the sample of children who move at or before age 13. Each series shows the non-parametric analog of a partial regression of a child's outcome on the prediction for a given gender, controlling for the other-gender prediction. To construct the series in circles, we regress both y_i and Δ^{g}_{odps} on Δ^{-g}_{odps} and origin by parent income decile by cohort by gender fixed effects.

[46] There is no reason that the rate of convergence should be identical across all quantiles of the income distribution because the prediction for permanent residents at each quantile 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.

We then bin the Δ^g_{odps} residuals into 20 equal-sized bins, and plot the mean residuals of y_i vs. the mean residuals of Δ^g_{odps} within each bin. The series in triangles is constructed analogously, except that we plot residuals of y_i vs. residuals of Δ^{-g}_{odps}, the prediction for the *other* gender. Figure IX shows that children who move before age 13 to areas where children of their own gender have better outcomes do much better themselves: a 1 percentile increase in the mean rank $\bar{y}^g_{pds}$ for $g = g(i)$ is associated with a 0.523 percentile increase in the movers' mean rank. In contrast, conditional on the own-gender prediction, variation in the prediction for the other gender is associated with only a 0.144 percentile increase in the movers' mean rank.

In Table V, we estimate exposure effect models analogous to (12) with separate predictions by gender. Column 1 replicates the baseline specification in (12), using the gender-specific prediction Δ^g_{odps} instead of the prediction that pools both genders. We continue to obtain an exposure effect estimate of $\beta = 0.038$ per year in this specification, consistent with our baseline results.[47] In Column 2, we use the prediction for the other gender Δ^{-g}_{odps} instead. Here, we obtain an estimate of 0.034, which is to be expected given the high degree of correlation in place effects across genders. In Column 3, we include predictions for both genders, identifying the coefficients purely from differential variation across genders within CZs. Consistent with the findings in Figure IX, we find that the coefficient on the own gender prediction is larger than the other-gender prediction.[48]

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

The differences between the own-gender and other-gender predictions support the view that the impacts of moving on children's outcomes reflects the causal effects of place rather than other omitted factors θ_i. In order for the patterns in Figure IX and Table V to be explained by other factors, families with higher inputs θ_i in child i would have to sort to areas where children of child

[47] In Online Appendix Table 2, we show that the exposure effect estimates are 0.039 and 0.04 for boys and girls using predicted outcomes that do not vary across genders.

[48] 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 (13).

i's gender do especially well. Such sorting may certainly be feasible to some extent; for instance, families who invest a lot in boys might seek to avoid highly segregated areas. However, such sorting would be much more difficult for families with children of two different genders, as it would require finding a neighborhood where the differences in outcomes of children of permanent residents across genders matches the *difference* in inputs θ_i across children within the family, in proportion to the age gap between the children. The fact that we find very similar results when we identify from sibling comparisons within families with a boy and a girl thus suggests that sorting is unlikely to be driving the heterogeneous impacts by gender.[49]

Together, these placebo tests show that our baseline design which simply compares families that move with children at different ages turns out to yield consistent estimates of exposure effects. We believe that selection and omitted variable effects do not confound the raw OLS estimates significantly for two reasons. First, the degree of age-dependent sorting across large geographies such as CZs and counties may be limited, as families seeking better schools or environments for their children at certain ages presumably move more locally. Second, children's outcomes conditional on parent income are not significantly correlated with mean parent incomes in an area (Chetty et al. (2014)). As a result, moving to a better area for children is actually not systematically associated with parents finding better jobs, mitigating what might be the most important confounding factor.

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

[49]The gender test is less definitive than the cohort and distributional convergence tests because gender-specific variation is easier to observe at the point of the move than cohort- or quantile-specific differences. However, the fact that the coefficients on the own- and other-gender predictions differ quite substantially suggest that gender-specific sorting to neighborhoods would have to be quite substantial to explain the findings.

VI Exposure Effect Estimates for Other Outcomes and Geographies

VI.A Other Outcomes

The analysis to this point illustrates the exposure effects of places on children's incomes. Here, we illustrate that this convergence occurs when measuring other outcomes. Figure X presents the baseline estimates for college attendance and marriage. For Panel A, we replicate the baseline specification in equation 11 replacing Δ_{odps} with $\Delta^{c}_{odps} = c_{pds} - c_{pos}$, where c_{pcs} is the fraction of children at parental income rank p who go to college. Here, we find a significant slope of 0.037 (0.003). While the graph is increasing as one considers moves at earlier ages, there is some evidence of a flattening slope below age 13. This suggests that, if anything, exposure to areas as a teenager are more important for college attendance than exposure in middle school years.

In Panel B, we replicate the baseline equation 11 replacing Δ_{odps} with $\Delta^{mar}_{odps} = mar_{pds} - mar_{pos}$, where mar_{pcs} is the fraction of children at parental income rank p who are married at age y. Panel B presents the results for both age 24 and age 26. We find a significant slope of 0.025 (0.02), which suggests places have causal effects on marriage in proportion to childhood exposure to the area.

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

Panel D in Figure XI considers teen birth, defined as being the parent listed on a birth certificate prior to age 20. We construct gender-specific predictions based on prior residents in each birth cohort and plot the estimated coefficients b_m from the baseline specification in Equation (11) replacing Δ_{odps} with $\Delta^{tb}_{odps} = r_{pds} - r_{pos}$, where r_{pcs} is the fraction of permanent residents in CZ c with parental income p in cohort s who have a child. We find significant exposure patterns for teen birth for both and girls. The pattern is linear below age 20 for males. For females, we find a linear exposure pattern prior to age 18, with some evidence of a sharp drop at age 18, consistent with exposure at ages 17-18 being a fairly critical time for teen birth outcomes for females.

In short, using the baseline design, we find evidence of exposure effects on college and teenage outcomes.

VI.B County-level Estimates

The analysis to this point focuses on moves across CZs, which is a quite broad notion of geography. This broader notion of geography affords us large samples of 1-time movers with which to create precise forecasts based on permanent residents. This allows us to conduct detailed robustness analysis and our outcome-based placebo tests. However, as shown in Figure II(c,d), there is considerable variation in outcomes across counties, in addition to CZ. Here, we apply our baseline design to a county-level analysis.

Table 6 replicates the baseline analysis at the finer county geography. We construct $\bar{y}_{pcs}$ using county-level permanent residents and we consider two samples of 1-time county movers.[50] First, we consider a sample of 1-time movers who move at least 100 miles between counties with populations above 250,000, analogous to the same sample restrictions we impose on the 1-time CZ movers. Column (1) shows we obtain a baseline slope of 0.035, slightly lower than our baseline slope of 0.040 at the CZ level. The smaller slope is consistent with a slightly larger degree of residential sorting at the county, as opposed to the CZ level – a finding we revisit in more detail in Section X. Column (2) adds family fixed effects to the baseline specification in Column (1) and obtains an exposure slope of 0.033 (0.011), not significantly different from the baseline slope of 0.035. This suggests the quasi-experimental design is not confounded by dynamic sorting patterns operating at the county level within the CZ.

Within CZ Moves. While our baseline analysis focused on moves above 100 miles, Columns (3)-(7) in Table 6 explore moves across counties within CZs. Column (3) replicates the baseline specification using moves across counties with populations at least 250,000, measuring outcomes of the children at age 24. Here, we obtain a slope of 0.022 (s.e. 0.003), significantly lower than the

[50] Appendix Table 1 provides summary statistics for these samples analogous to the CZ-samples.

estimate of 0.035 we obtain for longer distance moves. This drop is consistent with what one would expect if the child's environment was not completely altered as a result of these shorter moves.

Foreshadowing further analysis in Section VII, Column (4) measures the child's outcome (and predicted outcomes of permanent residents) at age 26 instead of age 24. Here, we obtain a similar but slightly higher slope of 0.032. Column (5) stacks the data across outcomes at age 24-32. Here, we obtain a more precisely estimated coefficient of 0.027 (s.e. 0.002). Column (6) adds family fixed effects to the specification in Column (5) and obtains a similar slope of 0.029 (s.e. 0.025). While our estimate remains stable, it is considerably more imprecise with the addition of family fixed effects across counties within CZs. Finally, Column (7) considers within-CZ moves across counties with populations of at least 10,000. Here, we obtain a similar but perhaps slightly attenuated coefficient of 0.024 relative to the 0.027 in column (5), consistent with the smaller samples used to estimate the predicted outcomes of permanent residents.

VI.C Summary of Part 1

On average, exposure to areas where permanent residents have better outcomes raises the expected outcomes of the children that move there. Across CZs and counties, the outcomes of movers converge to the outcomes of permanent residents at a rate of around 0.03 to 0.04 percent per year. Multiplying this by 20 years of exposure to form β_1 in equation (5), it implies $\beta_1 = 20{*}0.035 = 0.7$.[51] Hence, $\beta_1^2 = 0.49$. The lower bound in equation (5) therefore implies that at least 49% of the variation in outcomes across areas reflects the causal effects of these places. Under the additional assumption of no covariance between the sorting and causal effects (which we provide evidence for in Part 2), this result implies that 70% of the variance in intergenerational mobility across areas reflects the causal effects of place.

[51] Alternatively, one could assume 15 years of exposure (which corresponds more closely to our sample window), and hence $\beta_1 = 15 * 0.035 = 0.525$. This would imply a lower bound of $\beta_1^2 = 0.28$ and a point estimate of $\beta_1 = 52.5\%$ under the assumption of no covariance between the sorting and causal effects.

Part 2: Causal Estimates by CZ and County

VII Identification of Causal Effects Using Fixed Exposure Effects Design

While the analysis of Part 1 provides estimates of the variance of place effects, it does not provide estimates for each particular area. In general, the observed outcomes in any given area will partially reflect sorting of different types of residents (e.g. with different θ_i) and partially reflect causal effects μ_c. This section develops a fixed effects model to estimate the causal effect of each place, μ_c. We build on the exposure-time identification strategy but estimate these fixed effects without using information contained in the permanent resident outcomes. To do so, we estimate separate exposure effects for each place in the U.S., as opposed to a single average exposure effect.

Using the resulting fixed effect estimates, we then proceed in three steps in the remainder of part 2. First, in Section VIII, we use these resulting estimates to measure the variance components of the model outlined in Section II (i.e. the variance of μ_{pc} and $\bar{\theta}_{pc}$). Second, in Section IX, we provide forecasts of each place's causal effect and use this to generate list of the "best" and "worst" counties to grow up in the U.S. in terms of their impacts on a child's income. To develop these forecasts, we use a combination of the fixed effects (which contain sampling error) estimated in this section with the forecasts based on permanent residents (which contain little sampling variation but are biased because of sorting) in a manner that minimizes mean-square error. Finally, in Section X, we measure the characteristics of places that improve childrens' outcomes, μ_{pc}, and places in which the outcomes of permanent residents are confounded by the presence of sorting, $\bar{\theta}_{pc}$.

VII.A Identifying neighborhood effects, μ_{pc}

Returning to our structural equation (2), under linear exposure effects, we have for 1-time movers:

$$y_i = (T_c - m)(\mu_{pd} - \mu_{po}) + T_c\mu_{po} + \bar{\theta}_i + \kappa_0$$

where we assume for simplicity that $\bar{\kappa}_i = \kappa_0$ (alternatively, one can think of $\bar{\theta}_i$ as incorporating heterogeneous disruption effects). The key identification assumption is as follows.

Assumption 2. Conditional on origin and destination, the choice of when to move is independent of other inputs, $\bar{\theta}_i$, for all origin-destination pairs.

Assumption 2 is stronger than Assumption 1 because it requires that exposure time is not confounded with sorting for any particular origin-destination pair.[52] In contrast, Assumption 1 only requires that the exposure time is not confounded with sorting on average across areas where permanent residents are doing better (or worse) on average. To control for other origin-destination pair-specific effects, we write

$$\bar{\theta}_i = \alpha_{odps} + \eta_{4i}$$

where η_{4i} is independent of the exposure time to the origin and destination location and

$$\alpha_{odps} = \left(\alpha^0_{od} + \alpha^P_{od} p + \psi^0_{od} s + \psi^1_{od} s^2 + \psi^2_{od} sp + \psi^3_{od} s^2 p\right) 1\{d(i) = d; o(i) = o\} \tag{16}$$

captures variation in outcomes across parent income (p), cohort (s), origin (o), and destination (d). We parameterize separate controls for each origin-by-destination pair that vary linearly in parental income and include a quadratic term in cohort. These cohort controls ensure that the exposure-time coefficient is identified holding fixed the year of outcome measurements for the child. This motivates the empirical model

$$y_i = \underbrace{(T_c - m)}_{\text{Exposure}} \left[\underbrace{\left(\mu^0_d + \mu^P_d p\right) 1\{d(i) = d\}}_{\text{Dest. FE}} - \underbrace{\left(\mu^0_o + \mu^P_o p\right) 1\{o(i) = o\}}_{\text{Orig. FE.}} \right] + \alpha_{odps} + \eta_{4i} \tag{17}$$

The causal impact of an additional year of exposure to destination d relative to origin o for a child with parental income rank p is given by $\left(\mu^0_d + \mu^P_d p\right) 1\{d(i) = d\} - \left(\mu^0_o + \mu^P_o p\right) 1\{o(i) = o\}$. We assume these fixed effects of places are linear in parental income, consistent with the observation that movers outcomes are well-approximated by a weighted average of permanent residents' outcomes, and the outcomes of permanent residents are well-approximated using a linear function in parental income, as shown in Figure I.[53]

VII.B Outcomes

In Section IV, we focused primarily on the child's income rank at age 24, which led to similar regression coefficients on the permanent residents in each CZ. Intuitively, the permanent resident outcomes provided a "goalpost" for characterizing the impact of places at various ages of outcome measurement, so that movers on average picked up 3-4pp of the permanent resident outcomes per year of exposure. In estimating place fixed effects, we no longer use the permanent residents as

[52] In addition to the origin-destination pair fixed effects, we also re-estimate our model using moves above age 23 to construct placebo estimates of place effects. We show below that these placebo estimates are consistent with the identification assumption and suggests violations of this assumption are not generating bias in our estimates.

[53] We have also estimated the model with quadratic terms in parental income and find very similar results.

goalposts. As a result, we focus on the child's income rank at a slightly older age – age 26 – instead of age 24. To motivate this particular choice for the age of outcome measurement, Appendix Figure VII reports the correlation of permanent resident outcomes, $\bar{y}_{pc}$, at ages 20-32 with the permanent resident outcomes at age 32. While the correlation at age 24 is 0.83, this correlation across CZs reaches 0.93 at age 26.[54] As a result, we are confident that our measure of the impacts of places on childrens' income at age 26 is likely to be highly correlated with their impacts on incomes at older ages.

VII.C CZ Estimation

At the CZ level, estimation of the thousands of parameters in equation (17) is not directly feasible on the micro data due to computational constraints. We therefore estimate these fixed effects in two steps. First, for every origin-destination pair, we estimate a regression of child outcomes on exposure time to the destination, $T_c - m$,

$$y_i = (T_c - m)\left(\mu_{od}^0 + \mu_{od}^1 p\right) + \alpha_{odps} + \eta_{5i} \tag{18}$$

where $\mu_{od}^0 + \mu_{od}^1 p$ represents the impact of spending an additional year of childhood in destination d relative to origin o for the set of people moving from o to d with parental income rank p. We include the controls for parental income and cohort given by equation (16).

Given an estimate of $\mu_{od}^p = \mu_{od}^0 + \mu_{od}^1 p$ for each origin and destination, we regress

$$\mu_{od}^p = G\mu_{pc} + \eta_{6od} \tag{19}$$

where G is an $N_c^2 \times N_c$ matrix of the form

$$G = \begin{matrix} -1 & 0 & +1 \\ -1 & 0 & +1 \\ +1 & -1 & 0 \end{matrix}$$

To construct the G matrix, we enumerate all origin-destination pairs as rows, and all unique places as columns. For each origin-destination row, we code the column corresponding to the destination as +1, the column corresponding to the origin as -1, and all other columns as 0. This matrix collapses the N_c^2 pairwise exposure effects, μ_{od}^p, into a vector of N_c place fixed effects, $\vec{\mu}_p = (\mu_{p1}, ..., \mu_{pN_c})'$.[55]

[54] As shown in Chetty et al (2014), the child's income rank at age 30-32 does not appear to suffer significant lifecycle bias. Hence, our measure of place effects at age 26 are likely to be highly correlated with the measures of place effects on measures of lifetime income.

[55] We thank Gary Chamberlain for pointing out this useful design-matrix representation of the estimates in equation (17) in terms of origin-by-destination regressions.

We estimate $\vec{\mu}_p = \{\mu_{pc}\}$ using the regression in equation (19), weighting each origin-destination-pair observation by the precision of the estimated μ^p_{od} in the origin-destination cell. To reduce the impact of statistical noise in the estimation process, we restrict to origin-destination cells with at least 25 observations. We let $\hat{\mu}_{pc}$ denote the resulting estimates of μ_{pc}.

The G matrix has N_c columns, but its columns sum to zero; hence it only has rank $N_c - 1$. Intuitively, we can only identify the impact of exposure to places relative to one omitted place. We therefore normalize $\hat{\mu}_{pc}$ to have population-weighted mean zero weighting by population in the 2000 Census, so that μ_{pc} corresponds to the the impact of exposure to place c relative to where the average population lives. Because we utilize a two-step estimation process, we rely on a bootstrap method to compute the standard errors of $\hat{\mu}_{pc}$. We construct 100 samples with replacement (resampling by family) and measure the standard deviation of the estimated μ_{pc} in these bootstrap iterations.

We estimate $\hat{\mu}_{pc}$ using our baseline sample of 1-time movers who move at or below age $T_c = 23$.[56] This yields a sample of 1,869,560 for the child's income rank at age 26. Throughout, we drop estimates of μ_{pc} in CZs with populations less than 25,000 (but include these movers estimates for μ^p_{od} so that moves to and from these CZs still contribute to the estimated vector of CZ effects).[57]

Standard Errors. For our baseline results for below-median (p25) and above-median (p75) income families, we estimate a standard error for $\hat{\mu}_{pc}$ using a bootstrap procedure. We construct 100 samples (with replacement) and repeat our two-step estimation procedure, yielding se_{pc} as the estimated standard error across these bootstrap iterations. We have also verified that these standard errors would deliver very similar estimates if instead one simply used the analytical standard errors from the regression in equation (19). Formally, the bootstrap method imposes a clustering of the standard errors at the origin-by-destination level. In practice, however, both approaches deliver very similar standard error estimates. We provide both standard errors for the baseline specifications in Online Data Tables 3 and 4. For our other outcome and sample specifications, we use the analytic standard errors in equation (19) for simplicity.

Results. The full set of estimates are available in Online Data Table 3. Figure XII presents maps of estimates of the impact of exposure to each CZ (relative to an average CZ), $\hat{\mu}_{cp}$, on the child's income rank at age 26 for children with below-median income parents (p25) and above-

[56]Relative to the sample used in the baseline analysis in Table 4, we include movers in years 2011-2012 and include movers who moved less than 100 miles. Appendix Table II shows that our baseline results in Section IV are robust to these extensions. In particular, we include shorter distance moves because it increases the connectedness of the graph of moves across the U.S., thereby reducing estimation error for each fixed effect, μ_c.

[57]Note that movers to and from these small CZs will still contribute to the overall estimates of the fixed effects as they affect the fixed effect estimates for larger CZs.

median income parents (p75). The estimates suggest significant variation in exposure effects across CZs. For example, we find that areas like the South (e.g. Louisiana, Alabama, Mississippi, Georgia, and Virginia) and Mountain West (e.g. Nevada, Utah, Wyoming, and Montana) tend to produce lower outcomes; in contrast, the Midwest, Northeast, and Western South (e.g. Texas, Oklahoma, Kansas, and New Mexico) tend to have higher causal effects. However, the standard errors associated with these estimates are non-trivial. We discuss this issue further in Section VIII below.

VII.D County Estimation

We replicate our analysis of place effects at the county level. To do so, we estimate fixed effects in equation (17) directly for each county separately within each CZ. Then, given each county estimate within each CZ, we add the CZ-level effect. This provides nationwide county-level estimates.

In principle, one could have attempted to estimate county-level place effects directly. In practice, there are over 3,000 counties in the U.S., which leads to $3,000^2 = 9M$ possible origin-destination combinations that would enter the G matrix in equation (19). Such estimation is computationally infeasible and at finer geographies the G matrix becomes singular in finite samples. In contrast, by focusing on moves across counties within CZs, we can estimate the fixed effects in equation (17) directly without relying on a two-step estimator.[58]

We estimate county-level place effects for CZs with populations of at least 25,000 people on the sample of 1-time movers across counties within CZs who move at or below $T_C = 23$. This includes 1,323,455 movers. We report estimates of $\hat{\mu}_{pc}$ for counties with populations of at least 10,000 people. We impose the restriction (without loss of generality) that the coefficients, $\hat{\mu}_c^0$ and $\hat{\mu}_c^P$ have a population-weighted mean of zero within each CZ. This provides an estimate of $\hat{\mu}_{pc} = \hat{\mu}_c^0 + p\hat{\mu}_c^P$ for every county within each CZ.

To aggregate across CZs to national county-level estimates, we sum the CZ-level estimate and the county level estimate. This produces estimates for 2,379 counties nationwide, covering 98.2% of the US population.[59] Online Appendix Table 4 presents results for the full sample of county estimates.

[58] Due to computational constraints, we do not allow the cohort controls to vary at the origin-destination level in the county-level estimation. Formally, we assume α_{odpq} in equation (16) is given by:

$$\begin{aligned} \alpha_{odps} = & \left(\alpha_{od}^0 + \alpha_{od}^P p\right) 1\left\{d\left(i\right) = d; o\left(i\right) = o\right\} \\ & + \left(\psi_d^0 s + \psi_d^1 s^2 + \psi_d^2 sp + \psi_d^3 s^2 p\right) 1\left\{d\left(i\right) = d\right\} \\ & - \left(\psi_o^0 s + \psi_o^1 s^2 + \psi_o^2 sp + \psi_o^3 s^2 p\right) 1\left\{o\left(i\right) = o\right\} \end{aligned}$$

so that we include county-specific cohort controls that are quadratic in cohort and interacted with parental income.

[59] In cases where CZs are only one county, we simply use the CZ estimate.

VII.E Robustness

Appendix Table V reports the correlation of our baseline estimates with alternative specifications.[60] Panel A of Appendix Table V reports the results for the CZ-level estimates; Panel B reports the estimates for the county level estimates.

Income Controls. Our baseline specification controls solely for a single measure of parental income. If moves to a particular place are systematically associated with increases in parental income, one might worry that the increase in income is what's driving the improved child outcomes in proportion to exposure time, as opposed to the impact of the place. Here, we replicate the analysis, adding controls for income changes before versus after the move and their interactions with the child's age at the time of the move (analogous to the income controls added in Column (5) of Table 3). For each origin by destination in the CZ regressions in equation (18), we add terms for Δp and $\Delta p * m$, where $\Delta p = p_{post} - p_{pre}$. p_{pre} is the income rank of the parents in the year prior to the move and p_{post} is the income rank of the parents in the year after the move.[61] At the county level, we include terms for Δp and $\Delta p * m$ interacted with county dummies directly in equation (17).

Including these controls and their interactions with the age of the child at the time of the move, m, leads to very similar results. The estimates at the CZ level for below-median income families are correlated 0.946 with the baseline specification; this correlation is 0.942 for above-median income families. At the county level, the estimates are also very similar, with correlations of 0.974 at p25 and 0.973 at p75. In short, controlling for income changes interacted with the child's age at the time of the move leads to estimates that are very similar to the baseline specification.

Linearity. Equation (17) models the impact of places as a linear function of parental income. This is motivated by the strong linearity we observe in outcomes amongst permanent residents, but could potentially be violated when constructing the causal effects of places. Here, we relax the linearity assumption in two ways. First, we include quadratics in parental income. This specification generates very similar estimates that are highly correlated with our baseline estimates at both p25 and p75. At p25, we estimate a correlation of 0.94 at the CZ level and 0.876 at the county level; At p75 we estimate a correlation of 0.932 at the CZ level and 0.777 at the county level.

Second, we split the sample into below-median $p < 0.50$ and above-median $p > 0.50$ families

[60] As noted above, for the alternative specifications at the CZ level, we use the analytical standard errors derived from the OLS regression in equation (19).

[61] So, instead of α_{odpq} in equation (16), we include additional terms $\alpha^0_{od\Delta p}\Delta p + \alpha^1_{od\Delta p}\Delta p * m$.

and estimate the model separately on these two samples. Across CZs, the split-sample estimates have a correlation of 0.839 with the baseline estimates for below-median income families (p25) and 0.784 for above-median income families (p75). At the county level, the estimates are correlated at 0.841 for below-median income families and 0.659 for above-median income families. In short, consistent with the linearity in the outcomes of permanent residents shown in Figure I, the results are quite robust to relaxing the assumption of linearity in parental income.

Cost of Living. Our baseline estimates do not adjust for cost of living differences across areas. This is natural if one believes such differences largely reflect differences in amenities. But, it is also useful to illustrate the robustness of the results to adjusting both parent and child income ranks for cost of living differences across areas. To do so, we construct adjusted income ranks for both parents and children that divide income in year t by a cost of living index (based on ACCRA) corresponding to the location of the individual in that year.[62] We then re-compute the 5-year averages for parental income (1996-2000) and their associated national ranks, along with the national ranks for the child's income at age 26.

Across commuting zones, the cost of living-adjusted estimates are correlated 0.748 with the baseline specification for below-median income families and 0.797 for above-median income families. Across counties, cost of living adjustments lead to estimates that are correlated 0.808 for below-median income families and 0.852 for above-median income families. So while there are some differences, the broad spatial pattern is similar after adjusting for cost of living differences.

Overall, our baseline estimates are robust to controlling for changes in income, relaxing the linearity in parental income rank assumption, and adjusting for costs of living. All of these robustness specifications produce alternative estimates of place effects and are available at the CZ and county level in Online Data Table 3 (CZ) and Online Data Table 4 (County).

VIII Model and Estimation Variance Components

We begin our analysis of the estimates of $\hat{\mu}_{pc}$ by using them to analyze the variance of place effects at the CZ and county level. In particular, we use these estimates to quantify the variance components of the model, including the standard deviation of place effects across CZs and counties, and the correlation of the effects for children in below and above-median income families.

[62]See Chetty et al. (2014) for a detailed discussion of this cost of living adjustment. Loosely, we use a predicted value of the ACCRA index that allows us to expand the coverage of ACCRA to all CZs.

VIII.A Variance of Exposure Effects Across CZs

Table VII reports the standard deviation of place effects across CZs and counties. We arrive at these standard deviation estimates as follows. The raw standard deviation of $\hat{\mu}_{25,c}$ across CZs is 0.248, as reported in column (1). However, this variance of $\hat{\mu}_{pc}$ comes from two components: variation in the true place effects, μ_{pc}, and an orthogonal sampling error, ϵ_{pc},

$$\hat{\mu}_{pc} = \mu_{pc} + \epsilon_{pc}$$

Therefore, we can compute the variance of the true place effects, $\sigma^2_{\mu_{pc}}$, as

$$\sigma^2_{\mu_{pc}} = \sigma^2_{\hat{\mu}_{pc}} - \sigma^2_{\epsilon_{pc}} \tag{20}$$

where $\sigma^2_{\hat{\mu}_{pc}}$ is the variance of the estimated place effects and $\sigma^2_{\epsilon_{pc}}$ is the estimated variance of the statistical noise (because $\hat{\mu}_{pc}$ is an unbiased estimator, we have $E\left[\epsilon_{pc}|\mu_{pc}\right] = 0$ so that $cov\left(\epsilon_{pc}, \mu_{pc}\right) = 0$). We estimate the variance of the statistical noise as

$$\sigma^2_{\epsilon_{pc}} = E\left[se^2_{pc}\right]$$

where se_{pc} denotes the standard error of $\hat{\mu}_{pc}$ and the expectation is taken across CZs using precision weights $(1/se^2_{pc})$.

The second row of Table VII reports the standard deviation of the sampling error, $\sigma_{\epsilon_{pc}} = 0.210$, which implies a signal standard deviation of $\sigma_{\mu_{pc}} = 0.132$. A one standard deviation increase in $\mu_{25,c}$ across CZs corresponds to a 0.132 percentile increase in the child's rank per year of additional exposure to the CZ.

To put these units in perspective, we can scale these percentile changes to reflect the dollar-per-year increases in child earnings. To do so, we construct the mean income of permanent residents in each CZ for parents at each income percentile, $\bar{y}^{\$}_{pc}$. We then regress $\bar{y}^{\$}_{pc}$ on the mean rank outcomes, $\bar{y}_{pc}$ across CZs for each parent income rank, p. This yields a coefficient of \$818 for $p = 25$, suggesting that each additional income rank corresponds to an additional \$818 of earnings at age 26.[63] Therefore, a 1 standard deviation increase in $\mu_{25,c}$ for children in below-median income families corresponds to 0.132*818 = \$108 increase in mean earnings. Normalizing by the mean

[63] In principle one could have estimated place effects directly on mean income; indeed, replicating our baseline analysis using mean income as an outcome instead of mean income rank leads to estimates that are correlated at 0.92 at the CZ level (p25). However, the variance in mean incomes renders estimation quite difficult – indeed, as shown in Appendix Table 7, we cannot estimate a positive signal variance for the mean income specifications due to excess estimation error. Trimming outliers does allow us to estimate a signal variance; but the rank-rank specification has the advantage that we can estimate on the entire sample without trimming.

income of children at age 26 in below-median income families of \$26,091, the estimate suggests a 0.4% increase in mean earnings per year of exposure.[64]

For children in above-median income families, we estimate that the standard deviation of place effects is 0.107 percentiles. To put this in perspective, we can repeat the above scaling procedure for p75, which suggests each additional income rank corresponds to an additional \$840 of earnings at age 26. Normalizing the mean income of children from above-median income families of \$40,601, it suggests a 1 standard deviation increase in $\mu_{75,c}$ corresponds to a 0.22% increase in mean earnings per year of exposure.[65]

The variation in place effects is high for children in both above- and below-median income families. From a dollar-weighted perspective, the impacts are roughly similar for children in above- and below-median income families, reflecting the higher incomes earned by children from above-median income families offsetting the lower percentile improvement. But, in percentage terms, there is much more variation in forecasts for those in below-median families, reflecting their comparatively lower mean incomes.

Relationship between $\mu_{25,c}$ and $\mu_{75,c}$. Is there a tradeoff between areas that promote better outcomes for disadvantaged children and those in more affluent backgrounds? On the one hand, the world could be such that outcomes in a given area are a zero-sum process, so that better outcomes for children in affluent families come at the expense of outcomes for children in lower-income families. On the other hand, the process that generates higher outcomes in some CZs could be one that spans the parental income distribution – a rising tide that lifts all boats.

Across CZs, we find that areas that promote better outcomes for poor children are, on average, areas that promote better outcomes for more affluent children as well. Table VII reports the correlation between $\mu_{25,c}$ and $\mu_{75,c}$ of 0.724. Importantly, we estimate this correlation using two separate samples of above and below-median income families. We construct an estimate of $\mu_{25,c}$ on the subsample of children with $p < 0.5$ and we construct an estimate of $\mu_{75,c}$ on the subsample of children with $p > 0.5$. We then re-compute the signal standard deviations on these two samples

[64] An alternative methodology to arrive at income increases would have been to directly estimate the place effects on income as opposed to ranks. Appendix Table V, rows 10 and 11, report the correlation of the resulting estimates of μ_{pc} for income with our baseline rank estimates and illustrates they are very highly correlated. However, they contain considerably greater sampling uncertainty given the high variances in income outcomes. Indeed, we are unable to estimate a point estimate for the variance of place effects on income at the county level using this methodology. Trimming outliers restores the ability to estimate the place effect for incomes, but such trimming is arbitrary; therefore we focus on rank outcomes as our baseline methodology.

[65] Throughout the rest of the paper, we provide scalings for other outcomes and samples, such as gender-specific estimates on family and individual income. When scaling these rank measures to incomes and % increases, we reconstruct the scaling factors using the same methodology outlined here.

(0.134 and 0.107 respectively, as shown in Appendix Table 5) and compute the covariance between these two estimates. The ratio of the covariance to the product of the standard deviations yields our estimated signal correlation of 0.724.[66]

In short, across CZs there is wide variation in exposure effects. And, areas that promote better outcomes for affluent children also, on average, promote better outcomes for low-income children.

VIII.B Variance of Exposure Effects Across Counties

Across counties in the US, we estimate a standard deviation of $\mu_{25,c}$ of 0.165 and of $\mu_{75,c}$ of 0.155. Again scaling this to percentage changes in income, a 1 standard deviation higher value of $\mu_{25,c}$ corresponds to a $818*1.65 = $1,349 increase in earnings, or 0.5% of mean earnings. This suggests that there is roughly an equal amount of variation in place effects across CZs as across counties within CZs. To see this, note that the standard deviation of place effects for counties within CZs for children at p25 is 0.099, which is slightly below the estimates of 0.132. For above-median income families, we estimate a standard deviation of place effects of 0.107 across CZs and 0.112 across counties within CZs.

At the county-level, we again find that areas that produce better outcomes for children in below-median income families also produce better outcomes for children in above-median income families. Using the split-sample methodology discussed in Section VII.E (see footnote 66), we estimate a correlation between $\mu_{25,c}$ and $\mu_{75,c}$ of 0.287, implying a correlation across counties within CZs of 0.08. This is lower than the positive association we find across CZs. This suggests that there may be tradeoffs at the local level, consistent with the patterns of greater residential sorting across finer geographic units.

VIII.C Sorting versus Causal Effects

A one standard deviation increase in $\mu_{25,c}$ at the county level corresponds to roughly a 0.5% increase in earnings. Scaling this by 20 years of exposure implies that a 1 standard deviation increase in

[66] More precisely, we compute this correlation as

$$\rho = \frac{cov\left(\mu_{25,c}, \mu_{75,c}\right)}{\sigma_{\mu_{25,c}}\sigma_{\mu_{75,c}}} = \frac{cov\left(\hat{\mu}_{25,c}, \hat{\mu}_{75,c}\right)}{\sigma_{\mu_{25,c}}\sigma_{\mu_{75,c}}}$$

where we estimate $\hat{\mu}_{25,c}$ and $\hat{\mu}_{75,c}$ on separate $p < 0.5$ and $p > 0.5$ samples so that their estimation errors are not mechanically correlated. This yields $cov\left(\hat{\mu}_{25,c}, \hat{\mu}_{75,c}\right) = cov\left(\mu_{25,c}, \mu_{75,c}\right)$. We compute the signal SDs, $\sigma_{\mu_{25,c}}$, using these half-sample estimates, which are reported in Appendix Table 5. As in the calculation of the signal SD for the baseline specifications, we use precision weights to calculate these signal SDs, weighting observations by the square of their estimated standard errors. When measuring $cov\left(\hat{\mu}_{25,c}, \hat{\mu}_{75,c}\right)$, we measure the precision as the inverse of the sum of the two standard errors squared, $prec = \frac{1}{se\left(\mu_{25,c}\right)^2 + se\left(\mu_{75,c}\right)^2}$.

$\mu_{25,c}$ causes an increase in earnings of roughly 10%, or 20*0.165 = 3.308 percentiles for children who spend their entire childhood in a particular place. Following the model in Section II, we can use the causal effects of exposure to each place, combined with an estimate of the total relevant exposure time, T_C, to decompose the observed outcomes of permanent residents into sorting and causal components.

To do so, an estimate of T_C is required to aggregate the per-year measure of the exposure effect, μ_{pc}, to the impact of full exposure during childhood, $T_c\mu_{pc}$. We can then estimate the selection component of the permanent residents by taking the difference between the permanent resident outcomes, $\bar{y}_{pc}$, and the full childhood exposure effect, $T_c\mu_{pc}$.

$$\hat{\theta}_{pc} = \bar{y}_{pc} - T_c * \hat{\mu}_{pc}$$

Under the assumption – maintained in the model in Section II – that the causal effects of places are the same for movers and permanent residents, this provides a measure of the expected rank of the permanent residents in a place, c, in the counterfactual world in which they grew up in an average place.

Of course, our estimates of the mean selection effect in an area, $\bar{\theta}_{pc}$, will depend on our assumption about T_c. Our baseline results document a robust linear exposure pattern between the ages of 11 and 23 for incomes measured at age 26. This suggests a value of T_C between 12 and 23, but it does not necessarily suggest which estimate is most appropriate (or indeed whether the linearity of the model holds at earlier ages). For most of our analysis, we make a benchmark assumption of $T_C = 20$, but assess the robustness of this assumption to $T_c = 12$ and $T_c = 23$. It is important to note that our procedure for estimating the per-year exposure effects, μ_{pc}, does not require us to make an assumption about T_c; rather, this is only required for using the outcomes of permanent residents to estimate the mean selection component, $\bar{\theta}_{pc}$.

Appendix Table VI reports the estimated values of the outcomes of permanent residents, $\bar{y}_{pc}$, the causal component based on $T_C = 20$ years of exposure, $20 * \hat{\mu}_{pc}$, and the sorting component, $\hat{\theta}_{pc}$, for the 10 largest CZs in the US (Online Data Tables 3 and 4 allow one to construct these estimates for any CZ or county). In Los Angeles, children of below median income permanent residents have incomes at the 44.8 percentile of the national income distribution of 26 year olds on average. Los Angeles has an estimate of $\hat{\mu}_{25,c} = -0.17$. 20 years of exposure implies a causal effect of -3.41pp (s.e. 0.85) of growing up in LA relative to an average CZ. This suggests that children who happened to grow up in Los Angeles would, on average fall at the 48.2 percentile if they grew up in an average place as opposed to Los Angeles (as reported in Column (2)).

Conversely, we can consider Washington, DC. Children who grow up in below-median income households that are permanent residents in DC on average fall at the 45.1 percentile, roughly similar to Los Angeles. However, we estimate a causal effect per year of exposure of $\hat{\mu}_{25,c} = 0.16$, which suggests 20 years of exposure increases the child's income rank by 3.27pp (s.e. 1.34) relative to an average CZ. This suggests that the types of children who grew up in Washington, DC would on average fall at the 41.8 percentile (45.1 - 3.3) if they grew up in an average place as opposed to Washington, DC. So, although DC and LA have similar observed outcomes of permanent residents in below-median income families, $\bar{y}_{25,c}$, the exposure effect to DC, $\mu_{25,DC}$, is significantly higher than LA, $\mu_{25,LA}$.

A range of other patterns emerge for children in above-median income families. For example, permanent residents in New York have higher outcomes than those in LA (56.73 versus 52.69). However, we estimate a causal effect, $T_C\mu_{75,c}$, of 20 years of exposure of -5.47 in LA and -0.78 in NY. This suggests the observed difference between NY and LA permanent resident outcomes is largely accounted for by the difference in the effects these places have on children's outcomes, as opposed to differences in the types of children and families that live in these areas, $\bar{\theta}_{75,c}$.

Model Variance Components.

Panel B of Table VII reports the variance-covariance structure of the model parameters across CZs, $\bar{\theta}_{pc}$, $\bar{y}_{pc}$, and μ_{pc}. Across CZs, more of the variation is due to the causal effect of places as opposed to the sorting of different types of people to different areas. For children in below-median income families, we estimate a population-weighted standard deviation of CZ place effects for 20 years of exposure, $T_c\mu_c$, of $20 * 0.132 = 2.647$, as noted above. In contrast, We estimate a standard deviation of the sorting component , $\bar{\theta}_{25,c}$, of 1.960, and we estimate a correlation between the sorting and causal effect close to zero (-0.021).[67] Similarly, for above-median income families, we

[67]We obtain this estimate by regressing $T_c\hat{\mu}_{pc}$ on $\bar{y}_{pc}$, yielding $\beta_p = \frac{cov\left(T_c\hat{\mu}_{pc},\bar{y}_{pc}\right)}{var\left(\bar{y}_{pc}\right)}$. We then multiply by $var\left(\bar{y}_{pc}\right)$, yielding $\beta_p var\left(\bar{y}_{pc}\right) = cov\left(T_c\hat{\mu}_{pc}, \bar{y}_{pc}\right) = cov\left(T_c\mu_{pc}, \bar{y}_{pc}\right)$. Then, noting that $\bar{y}_{pc} = \bar{\theta}_{pc} + T_c\mu_{pc}$ we have

$$cov\left(T_c\mu_{pc}, \bar{\theta}_{pc}\right) = \beta_p var\left(\bar{y}_{pc}\right) - var\left(T_c\mu_{pc}\right)$$

Now, to obtain the variance of the sorting component, we have

$$var\left(\bar{\theta}_{pc}\right) = var\left(\bar{y}_{pc}\right) - var\left(T_c\mu_{pc}\right) - 2cov\left(T_c\mu_{pc}, \bar{\theta}_{pc}\right)$$

which provides an estimate of $var\left(\bar{\theta}_{pc}\right)$. Given this, we can construct the correlation between the sorting and causal components as

$$corr\left(T_c\mu_{pc}, \bar{\theta}_{pc}\right) = \frac{cov\left(T_c\mu_{pc}, \bar{\theta}_{pc}\right)}{var\left(T_c\mu_{pc}, \bar{\theta}_{pc}\right)}$$

find a standard deviation of $T_c\mu_{75,c}$ of 2.139, in contrast to a standard deviation of $\bar{\theta}_{pc}$ of 1.097.

Across counties within CZs, more of the variation in observed outcomes reflects the sorting of different types of people to different counties, $\bar{\theta}_p$, as opposed to the causal effect of those counties. Summing counties and CZs, we estimate a standard deviation of the causal effect of 20 years of exposure of 3.308 percentiles at p25 and 3.092 percentiles at p75. This is roughly the same order of magnitude as the standard deviation of the sorting component of 3.033 and 3.203 at p25 and p75, respectively. As a result, across counties within CZs, the sorting component SD is greater than the causal component. The county-within-CZ causal effect standard deviation is 1.984 at p25 and 2.233 at p75, which contrasts with a standard deviation of the sorting components of 2.315 and 3.009 at p25 and p75. Put differently, we find evidence that a larger fraction of the variation in outcomes of permanent residents across counties within CZs reflects residential sorting on unobservables, θ_i, as opposed to the causal effects, μ_{pc}.

Robustness to alternative choices of T_C. Appendix Table VII presents the model covariance structure for $T_C\mu_{pc}$ and $\bar{\theta}_{pc}$ under the alternative assumptions of $T_c = 12$ and $T_c = 23$. As expected, using $T_C = 12$ implies both (i) a higher standard deviation of the selection component and (ii) a higher covariance between the sorting and causal component. In general, if $T_C = 12$ we estimate a positive correlations between the sorting component and the causal effect, suggesting that those with higher θ_i tend to live in places with higher μ_{pc}. Conversely, if $T_C = 23$, the estimates of the sorting variance is lower and the correlation between the sorting and causal effects are generally negative, which would imply that those with higher θ_i tend to live in places with lower μ_{pc}. However, the general pattern remains of more variation in the sorting component than the causal component at the county within CZ level.

IX Combining Permanent Residents and Fixed Effects to Form Optimal Predictions

What are the places with the highest and lowest causal effects on children's outcomes? To this point, we have not focused heavily on the particular estimates of $\hat{\mu}_{pc}$. The fourth row of Table VII illustrates why: we find a signal to noise ratio, $\frac{\sigma^2_{\mu_{pc}}}{\sigma^2_{\epsilon_{pc}}}$, of 0.398 for $\hat{\mu}_{25,c}$ at the CZ level, illustrating that roughly 71% ($= \frac{1}{1+0.398}$) of the variation across CZs in the estimated place effects reflects sampling variation as opposed to the causal effect of the place. At the county-level, these signal to noise ratios are even smaller: we estimate a signal to noise ratio of 0.14-0.17 across counties across CZs, and 0.08-0.11 across counties within CZs. So, while we can use the estimates of $\hat{\mu}_{pc}$ to measure the variance of exposure effects and sorting components, we cannot use these estimates to form reliable predictions about exposure effects for every place.

For larger cities, like New York, we obtain fairly precise estimates (e.g. an estimate of -0.15 with s.e. of 0.04, as shown in Appendix Table VI), but in smaller CZs and counties our estimates are more imprecise. If one were to sort CZs based on their estimated $\hat{\mu}_{pc}$, the ordering of places from top to bottom would likely be driven by sampling error, as opposed to the true causal effect, μ_{pc}.

IX.A Optimal Forecasts

In the presence of sampling error, the goal of forecasting the "best" and "worst" places differs from the goal of finding unbiased estimates. We construct optimal forecasts by imagining the hypothetical experiment of randomly assigning a child to place c. We wish to construct an unbiased forecast of the causal exposure effect that place will have on her, μ_{pc}. Up to this point, we have two potential causal effects to assign to this child. The first is a projection based on the outcomes of permanent residents, $\beta\bar{y}_{pc}$: on average, each year of exposure generates a convergence to the permanent resident outcomes at a rate of 0.03-0.04. This estimate is precise ($\bar{y}_{pc}$ is effectively measured without sampling uncertainty given the large samples of permanent residents) but is biased because $\bar{y}_{pc}$ contains a sorting component, $\bar{\theta}_{pc}$.[68] Second, we have our estimated causal effect, $\hat{\mu}_{pc}$. This estimate is unbiased (under Assumptions 1 and 2) but contains non-trivial sampling uncertainty.

To construct optimal linear forecasts, we resolve the classic bias-variance tradeoff by conducting a hypothetical regression of the true causal effect on our two estimates:

$$\mu_{pc} = \rho_{1,pc}\bar{y}_{pc} + \rho_{2,pc}\hat{\mu}_{pc} + \eta^f$$

which yields an optimal forecast $\mu^f_{pc} = \hat{\rho}_0 + \hat{\rho}_1\bar{y}_{pc} + \hat{\rho}_2\hat{\mu}_{pc}$ that will minimize the mean-square error, $\sum_c \left(\mu^f_{pc} - \mu_{pc}\right)^2$, and form an unbiased forecast of the causal effect conditional on the forecast, $E\left[\mu_{pc}|\mu^f_{pc}\right] = \mu^f_{pc}$.

If we knew the causal effect of each place with certainty, μ_{pc}, we could run this regression and obtain the optimal forecast weights, $\hat{\rho}_j$. Absent knowledge of μ_{pc}, we proceed using the following methodology. Because $\hat{\mu}_{pc}$ is an unbiased estimate of μ_{pc}, we can form a prediction for μ_{pc} based on the permanent residents by regressing $\hat{\mu}_{pc}$ on $\bar{y}_{pc}$, yielding a coefficient β_p. For simplicity, we

[68]For simplicity, we imagine $\bar{y}_{pc}$ has been demeaned to have mean zero across places; alternatively, one can add a constant into the forecast.

assume β_p and $\bar{y}_{pc}$ are non-stochastic; because of the large samples, incorporating the sampling uncertainty of β_p and $\bar{y}_{pc}$ leads to very minimal changes in any of our estimates. We can then construct the residuals

$$\hat{\epsilon}_{pc} = \hat{\mu}_{pc} - \beta_p \bar{y}_{pc}$$

Let $\hat{s}_{pc}$ denote the standard error of $\hat{\mu}_{pc}$ estimated in equation (17). Because $\beta_p \bar{y}_{pc}$ is non-stochastic, $\hat{s}_{pc}$ is also the standard error of the residuals, $\hat{\epsilon}_{pc}$. Moreover, $E[\mu_{pc}|\bar{y}_{pc}] = \beta_{pc}\bar{y}$, so that it must be the case that $\rho_1 + \rho_2 = 1$. Hence, the problem of choosing the best linear forecast, μ^f_{pc}, reduces to the question of how much weight to place on the residuals, $\hat{\epsilon}_{pc}$. This will be given by the regression coefficient:

$$\rho_{2,pc} = \frac{cov\left(\hat{\epsilon}_{pc}, \mu^f_{pc} - \beta_p \bar{y}_{pc}\right)}{var\left(\hat{\epsilon}_{pc}\right)} = \frac{\chi_{pc}}{1 + \chi_{pc}}$$

where χ_{pc} is the signal-to-noise ratio of the residuals for place c. These are given by

$$\chi_{pc} = \frac{\sigma^2_{\epsilon_{pc}}}{\sigma^2_{\epsilon_{pc}} + \hat{s}^2_{pc}}$$

where $\sigma^2_{\epsilon_{pc}}$ is the estimated variance across places c of the true residuals (which is fixed across places, c) and $\hat{s}^2_{pc}$ is the estimated sampling variance of the residuals for each place c (which varies across places, c). We compute $\sigma^2_{\epsilon_{pc}}$ for each place c using the formula:

$$\sigma^2_{\epsilon_{pc}} = \sigma^2_{\mu_{pc}} - \sigma^2_{\beta \bar{y}_{pc}}$$

where $\sigma^2_{\mu_{pc}}$ is the estimated signal variance of the true place effects (see Panel A of Table VII) and $\sigma^2_{\beta \bar{y}_{pc}}$ is the variance of the predicted values based on permanent residents.[69] Hence, our optimal forecast is given by

$$\mu^f_{pc} = \beta_p \bar{y}_{pc} + \frac{\sigma^2_{\epsilon_{pc}}}{\sigma^2_{\epsilon_{pc}} + \hat{s}^2_{pc}} \left(\hat{\mu}_{pc} - \beta_p \bar{y}_{pc}\right) \tag{21}$$

The forecasts place more weight on the fixed effect estimates of a given place, c, if (a) there is more residual signal variance contained in these fixed effects across places, $\sigma^2_{\epsilon_{pc}}$ and (b) there is less sampling error in the fixed effect estimate of a given place, $\hat{s}^2_{pc}$. Note that the optimal weights vary across places according to the precision of the estimated fixed effects. If the fixed effects were estimated with perfect precision, $\hat{s}^2_{pc} = 0$ so that the optimal forecast would place a weight of 1 on the unbiased fixed effects estimates. In places where the fixed effects are estimated with greater sampling error, the optimal forecast places more weight on the predictions based on permanent residents – the MSE-minimizing forecast accepts some bias in order to reduce variance.

[69] Both the signal variance across places, $\sigma^2_{\mu_{pc}}$, and the variance of the predicted values, $\sigma^2_{\beta \bar{y}_{pc}}$, are estimated using precision weights, $\frac{1}{\hat{s}_{pc}}$.

IX.B Estimation

Appendix Table IV reports the estimates of β_p across specifications and parental income levels, along with the standard deviation of predicted values, $\sigma_{\beta\bar{y}_{pc}}$, and the standard deviation of the residuals, $\sigma^2_{\epsilon_{pc}}$. For CZs, we estimate a value for β of 0.032 at p25 and 0.038 at p75. For counties, we estimate β_p of 0.027 at p25 and 0.023 at p75. All estimates are roughly similar to our baseline exposure effect estimates.[70]

Consistent with the results in Section IV illustrating that the permanent resident outcomes are predictive of the causal effects, we estimate that the predictions $\beta_p\bar{y}_{pc}$ capture a significant portion of the underlying place effects. Across CZs, the predictions based on permanent residents have a standard deviation of 0.106 at p25 and 0.097 at p75, as compared to the total signal standard deviation of 0.132 and 0.107 reported in Table VII. Across counties, we estimate a standard deviation of the predictions based on permanent residents of 0.115 and 0.076 at p25 and p75, which correspond to analogous signal standard deviations of 0.165 and 0.155. But while the predictions based on permanent residents do capture a significant portion of the variation in causal effects, the residual standard deviations are also quite large. Across CZs, we find estimates of $\sigma_{\epsilon_{pc}}$ ranging from 0.08 at p25 and 0.045 at p75 across CZs. Across counties, we find estimates of 0.118 and 0.135 at p25 and p75. Hence, there is still considerable information in the estimated place effects, μ_{pc}, not captured in the forecasts based on permanent residents.

In large CZs, we estimate that the variation of $\hat{\mu}_{pc}$ accounts for roughly 75% of the variance – hence, the optimal forecasts will place considerable weight on the fixed effect estimates. In contrast, in smaller CZs, the raw fixed effect estimates become noisier, so that the optimal forecasts place considerably more weight on the permanent residents. Online Data Tables 3 and 4 contain all the underlying estimates that are required for replication of this forecasting methodology.[71]

IX.C Baseline Forecasts

Highest and Lowest CZs. Figure XIII plots the resulting values of $\mu^f_{p,c}$ for below-median (p25) and above-median (p75) income families. Table VIII lists the forecasts for the 50 largest CZs, sorted in descending order from highest to lowest values of $\mu^f_{25,c}$. We also report the root mean square

[70] In contrast to our baseline estimates in Section IV, the estimates here are not cohort-varying and the slope estimate does not contain cohort-varying intercepts. Hence, a more natural comparison is to column (5) of Table II, which has a coefficient of 0.036 (s.e. 0.002).

[71] While our forecasts are "optimal" conditional on finding a linear combination of the permanent resident forecast and the fixed effect, they are sub-optimal in that they do not use all of the available information in the joint distribution of the fixed effect estimates and permanent resident outcomes. For example, an interesting direction for future work would be to construct a forecast that incorporates the fixed effect estimates of neighboring counties.

error for each forecast, which provides a measure of how much, on average, one would expect these forecasts to be from the true place effect, μ_{pc}.[72]

Among the 50 largest CZs, we estimate that Salt Lake City, Utah has the highest causal effect on children in below-median income families. Every additional year spent growing up in Salt Lake City increases a child's earnings by 0.166 percentiles (rmse 0.066) relative to an average CZ. In dollar units[73], this corresponds to a \$136 increase in annual income per year of exposure, a roughly 0.52% increase; aggregating across 20 years of exposure, this is a 10% increase in the child's income for growing up in Salt Lake City as opposed to an average CZ.

Conversely, at the bottom of the list we estimate that every additional year spent growing up in New Orleans reduces a child's earnings by 0.214 percentiles (rmse 0.065) per year relative to an average CZ. This corresponds to a decrease of \$175 per year of exposure, or roughly 0.67%. Multiplying by 20 years of exposure, this implies that growing up in Salt Lake City as opposed to New Orleans would increase a child's income from a below-median income family by \$6,223, or roughly 24%.

As illustrated in Column (4), there is fairly wide variation across CZs in the forecasted impact of places on children's earnings. Relative to an average CZ, every year spent in New York lowers annual incomes at age 26 by roughly \$95.5 (0.366%); every year in Detroit lowers incomes by \$111 (0.425%); every year in Minneapolis increases incomes by \$84 (0.32%). For above-median income families, we estimate that Los Angeles produces the lowest outcomes. Every year spent growing up in Los Angeles reduces incomes for children in above-median income families by 0.226 percentiles, which corresponds to \$189, or roughly 0.466% reduction in incomes at age 26 per year of exposure during childhood.

Highest and Lowest Counties. Table IX presents estimates from the 100 largest counties, focusing on those in the top and bottom 25 based on the causal effect on family income rank for children in below-median income families, $\mu^f_{25,c}$. Figure XIV plots the forecasts for the New York City and Boston Combined Statistical Areas (CSAs). We find wide variation in place effects, even

[72]The RMSE provides a more appropriate measure of uncertainty than the standard error, which is considerably lower than the RMSE because the values are shrunk to the outcomes of permanent residents, which are statistically precise but contain the sorting component.

[73]Recall from above that we can scale these percentile changes to reflect the dollar-per-year increases in child earnings. We construct the mean income of permanent residents in each CZ for parents at each income percentile, $\bar{y}^{\$}_{pc}$. We then regress $\bar{y}^{\$}_{pc}$ on the mean rank outcomes, $\bar{y}_{pc}$ across CZs for each parent income rank, p. This yields a coefficient of \$818 for $p = 25$ and \$840 for $p = 75$, suggesting that each additional income rank corresponds to an additional \$818 of earnings at age 26 at $p = 25$ and \$840 at $p = 75$. Normalizing by the mean income of children at age 26 in below-median income families of \$26,091 at p25 and \$40,601 at p75 yields the percentage increase in child's earnings.

at close distances. For example, every additional year spent growing up in Hudson County, NJ increases incomes for children in below-median income families by 0.066pp (rmse 0.101), which corresponds to an increase of $54, or 0.208% of the mean child income for those in below-median income families. Conversely, every year spent growing up in the Bronx, NY reduces incomes by 0.174pp (rmse 0.076), which corresponds to an decrease of $142, or 0.544% of mean income. Combining these estimates, a child from a below-median income family that spends 20 years growing up in Hudson, NJ as opposed to the Bronx, NY will have incomes that are 15% ($3,920) higher.

At the top of the list, we find that Dupage county, IL (western suburbs of Chicago) has the highest causal effect on children from below-median income families. Every year spent growing up in Dupage increases a child's income by 0.255 percentiles (rmse 0.09), which corresponds to an increase of $209 or 0.80%. This contrasts with the nearby Cook county (Chicago) which lowers a child's earnings by 0.204 percentiles per year (rmse 0.06), corresponding to a reduction in incomes of $167, or 0.64%. Twenty years spent growing up in the western suburbs of Chicago as opposed to Chicago proper increases a child's income on average by $7,520, or roughly 28.8%.

At the bottom of the list of the 100 largest counties, we estimate that Mecklenburg County (Charlotte, NC) and Baltimore, MD have the lowest causal effect on the incomes of children in below-median income families. Every year spent growing up in Mecklenburg, NC reduces a child's income by 0.231 percentiles, which corresponds to $189 per year (0.72%) in earnings at age 26. This implies that twenty years of exposure to Dupage county, IL relative to Charlotte, NC would raise a child's income from a below-median income family by $7,948, or roughly a 30.5% increase in the earnings of a child from a below-median income family.

IX.D Estimates by Gender and Gender-Averaged Estimates

Estimates by Gender. In Section V.C we showed that the outcomes of permanent residents across genders are highly correlated (0.9 at p50), but they are not identical. Building on this, we construct measures of $\hat{\mu}_{pc}$ separately by child gender. Appendix Table V (rows 6 and 7) reports the correlation with the baseline specification and the signal standard deviation of the gender-specific estimates.

There is more variation in place effects, $\mu_{25,c}$, for boys in low-income households than for girls in low-income households. Across counties, we find a signal standard deviation of 0.277 for males and 0.172 for females. To illustrate the particular CZs and counties that have gender-specific effects, Tables X and XI present forecasts, $\mu^{f}_{25,c}$ separately by gender across CZs and counties. For brevity, we focus on the impacts on children in below-median income (p25) families; Online Data Tables 3 and 4 present the results for all CZs and counties using the linear model to construct measures at all parental income percentiles, p.

Table X presents the estimates for the 50 largest CZs for below-median income families for boys and girls separately. Online Appendix Figure X presents the national forecasts by CZ for males and females in below-median (p25) income families. For males in below-median income families, Minneapolis, MN has the highest effect of 0.155 percentiles per year of exposure, corresponding to a 0.5% increase in mean family income per year of exposure relative to the average CZ.[74] In contrast, the Detroit CZ has the lowest causal effect on family income for boys; every year a below-median income child spends growing up in Detroit lowers their incomes by 0.77%.

For females in below-median income families, New Orleans has the lowest causal effect on family income; every additional year spent in New Orleans lowers their incomes by -0.285 (s.e. 0.098) percentiles, a reduction of 0.932%. In contrast, we find that Salt Lake City, Utah has the highest causal effect on the family incomes of females. Every year spent growing up in Salt Lake City increases a female child's income from a below-median income family by 0.234 percentiles, or roughly 0.767%.

Table XI zooms in to the finer county-level geography. For males in below-median income families, Bergen County, NJ and Bucks County, PA have the highest causal effect on family income of males, increasing incomes at a rate of 0.831% and 0.841% per year of exposure. Conversely, Baltimore, MD has the lowest causal effects on male family income. Every additional year of exposure to Baltimore for males in low-income families lowers their income by 1.393%. Put differently, these suggest that 20 years of exposure to Bucks County, PA as opposed to Baltimore, MD for males in below-median income families would increase their income by 44.7%.

In contrast, we find slightly different patterns for girls. An additional year of exposure to Baltimore for women in below-median income families reduces their family income by -0.082 percentiles, or -0.27% per year. For Bergen County, NJ, and Bucks, PA we continue to find positive effects on females in below-median income families corresponding to a 0.56% and 0.46% increase in income per year of exposure.

[74] To obtain this translation from percentiles into dollars and percentage increase in dollars, we follow the same procedure as above for average income across genders. We construct the mean gender-specific income of permanent residents in each CZ for parents at each income percentile, $\bar{y}^{\$}_{pc}$. We then regress $\bar{y}^{\$}_{pc}$ on the mean rank outcomes, $\bar{y}_{pc}$ across CZs for each parent income rank, p, separately by gender. This yields the percentile-to-dollar translation. Normalizing by the gender-specific mean income of children at age 26 in below-median income families yields the percentage increase in child's earnings.

Overall, the patterns illustrate a wider variation in the role of place in determining boys as opposed to girls outcomes. To illustrate this, Appendix Figure VIII plots the cumulative distribution of forecast values, μ^{f}_{25c} across counties for males and females. As one would expect given the higher signal standard deviation, the distribution is more dispersed for males than for females. Moreover, the distribution is also slightly skewed for males: there is a thicker "left tail" of places that produce particularly poor outcomes for boys as opposed to girls. This suggests that there are pockets of places across the U.S., like Baltimore MD, Pima AZ, Wayne County (Detroit) MI, Fresno CA, Hillsborough FL, and New Haven CT, which seem to produce especially poor outcomes for boys. Twenty years of exposure to these counties lowers a child's income by more than 14% relative to an average county in the US.

Gender-averaged Estimates. Given the evidence of heterogeneity in effects across genders, we also present baseline rankings by CZ and county that allow for different models for girls and boys and then average the resulting estimates. Indeed, one could be worried that the pooled estimate does not recover the mean effect across gender due to subgroup heteroskedasticity or finite sample bias from differential fractions of males and females moving across areas. To that aim, Column (10) of Table XI reports the average of the two gender forecasts, which can be compared to the pooled specification estimate in Column (7).

In practice, these two estimates deliver nearly identical forecasts – their population-weighted correlation across counties is 0.97. Table XI is sorted in descending order according to the gender-averaged specification in Column (10).[75] We estimate that Dupage county increases a child's income by 0.756% per year of exposure; in contrast, we estimate that Baltimore, MD decreases a child's income by 0.864% per year. Twenty years of exposure to Dupage county versus Baltimore will increase a child's annual income (averaging across genders) by 32.4%.

IX.E Individual Income

Our baseline results focus on family income rank. Aggregating income across married spouses has the benefit of not penalizing joint household decision-making in which only one of the family members engages in primary employment. On the other hand, using a family income definition, as opposed to an individual income definition, means that the event of marriage can significantly increase one's measured income.

Therefore, a complementary outcome of interest is the individual's own income rank in the

[75]To construct the RMSE, we take the square root of the sum the square of the the two gender-specific forecasts.

national (cohort-specific) distribution of individual income. We replicate all of the analysis at both the CZ and county level, analogous to our baseline estimates for family income. Appendix Figure XI presents the national maps of the forecasts at the CZ level for individual income. Appendix Table VIII and IX present the estimates for the 50 largest CZs and top 25/bottom 25 of the 100 largest counties.

Broadly, the family income measures are similar to the baseline household income results.[76] However, there are some notably different patterns. Most saliently, cities have higher impacts on individual income than on family income, consistent with lower rates of marriage and an impact of places on age of marriage. For example, at the CZ level for children with below-median income parents, each additional year of exposure to New York decreases a child's family income by -0.117 percentiles (rmse 0.039) or -0.366%, but it increases individual income by 0.017 percentiles (rmse 0.039), or 0.054%.[77] Similarly, San Francisco increases a child's family income by 0.029 percentiles (rmse 0.060) or 0.09%, but it increases a child's individual income by 0.070 (rmse 0.062), or 0.23%.

Much of the difference is driven by the impact on females, and the patterns are broadly consistent with joint household decision-making combined with the evidence in Figure X that places have causal effects on marriage. For males, Minneapolis is not only the CZ with the highest impact on family income but also on individual income. For females, Philadelphia is the place with the highest impact on female individual income. Every additional year of exposure to Philadelphia increases a female's individual earnings by 0.203 percentiles (rmse 0.073), or 0.716%. However, New Orleans remains at the bottom of the list for female individual income: every additional year of exposure to New Orleans lowers a female's individual income by 0.468%.

Across income definitions and gender subgroups, male individual and household income along with female family income are all highly correlated with the baseline pooled family income specification. Our forecasts of individual and family income for males are correlated at 0.86 and 0.8 at the county level with the baseline family income specification at p25 pooling across genders. And, our forecasts for family income of females at p25 are correlated 0.92 with the baseline family income specification. But, our forecasts for female individual income at p25 are correlated only 0.38 with the baseline family income specification pooling across genders.

The importance of differential marriage rates across places in driving these patterns is illus-

[76]The raw estimates of $\mu_{25,c}$ are correlated at 0.8 with the baseline estimates at the CZ level and 0.77 at the county level, as shown in row 5 of Appendix Table V.

[77]We follow the procedures outlined above for translating percentiles to percentage increases in the child's individual income at age 26.

trated by a few additional examples. For example, exposure to the Salt Lake City CZ causes a 0.767% increase in family income per year of exposure, but a 0.123% *decrease* in individual income per year of exposure, consistent with a hypothesis that Salt Lake City has a causal exposure effect on marriage and increases the likelihood that females drop out of the labor force after marriage. In larger cities with lower marriage rates, we generally find a more muted but opposing pattern. Exposure to Boston, MA increases female household income by 0.039%, but increases female individual income by 0.369%. Exposure to Washington, DC increases female household income by 0.353% but increases female individual income by 0.522%.

Across counties, Bergen County, NJ has the highest place effects on individual earnings among the 100 largest counties for both males and females. Every year of exposure to Bergen County increases a male child's income from a below-median income family by 1.014% for males and 0.752% for females. Conversely, Baltimore, MD has the lowest effect for males: every additional year of exposure to Baltimore lowers a male's income by 0.487 percentiles, or 1.405%. Interestingly, although we find places like Baltimore and Charlotte produce generally lower outcomes for females, the county with the lowest impact on female individual income is San Bernardino County, CA. Every year of exposure to San Bernardino lowers a female's individual income by 0.119 percentiles (rmse 0.064), or roughly 0.42%.

Our analysis here only scratches the surface of the many potentially interesting underlying patterns. The results for the baseline family income and individual income, for the pooled and gender-specific samples are provided in Online Data Table 1 (CZ) and Online Data Table 2 (County).

X Characteristics of Good Neighborhoods and Positively-sorted Neighborhoods

What are the characteristics of good neighborhoods? Here, we relate the variation in the properties of neighborhoods to variation in our measure of neighborhood effects, μ_{pc}. We focus primarily on a set of characteristics that Chetty et al. (2014) explored as potential correlates of rates of observed intergenerational mobility. Chetty et al. (2014) found that observed patterns of upward mobility are correlated with measures of race, segregation, income inequality, K-12 school quality, social capital, and family structure; they also considered a range of other variables were less correlated with mobility, including measures of state and local taxes, college accessibility, local labor market conditions, and migration. In this section, we correlate these variables with the causal effects of

CZs and counties.[78]

In addition to characterizing the correlates of place effects, μ_{pc}, we also use our model for the observed outcomes of permanent residents, $\bar{y}_{pc} = \bar{\theta}_{pc} + T_c\mu_{pc}$, to decompose the observed pattern with permanent resident outcomes, $\bar{y}_{pc}$, into the portions driven by the causal component, $T_C\mu_{pc}$, and the sorting component, $\bar{\theta}_{pc}$ in each place. This asks whether the correlations in Chetty et al. (2014) are driven by correlations with the causal effects of places, μ_{pc}, or differences in the composition of types of people in each place, θ_i (or both).

Tables XII-XV and Figures XV and XVI report the results. For Tables XII-XV, Column (1) reports the standard deviation of the covariates.[79] Column (2) reports the correlation of the covariate with μ_{pc} (note this is also the correlation with $T_C\mu_{pc}$ for any T_C).[80] Column (3) reports the coefficient of a univariate regression of $\bar{y}_{pc}$ on the standardized covariate (each row corresponds to a separate regression). We standardize each covariate by subtracting its population weighted mean and dividing by its standard deviation, using population weights from the 2000 Census. Further, we weight the regressions using 2000 population.[81] Each coefficient is the average increase in the causal effect and sorting component corresponding to a 1 standard deviation increase in the covariate. We also report the standard errors for each estimate, which are clustered at the state level for the CZ regressions and CZ level for the county-within-CZ regressions to account for spatial autocorrelation.

Column (4) reports the coefficient of a univariate regression of $T_c\mu_{pc}$ on the standardized covariate under the assumption that $T_C = 20$. Column (5) reports the coefficient of a univariate regression of $\bar{\theta}_{pc} = \bar{y}_{pc} - T_C\mu_{pc}$ on the standardized covariate. Note that the coefficients in columns (4) and (5) sum to the coefficient in column (3), so that they provide a decomposition of the observed relationship between the covariate and outcomes of permanent residents into their causal and sorting components. Tables XII and XIII report results from CZ-level regressions for below-median (Table XII) and above-median (Table XIII) families. Tables XIV and XV report results from county-within-CZ regressions that include CZ fixed effects. We report estimates separately for $\mu_{25,c}$ (Table XIV) and $\mu_{75,c}$ (Table XV). Appendix Tables X-XIII replicate these tables using

[78]Relative to Section IX, we do not use the forecasted place effects for the correlations; rather, the measurement error in $\hat{\mu}_{pc}$ is not a problem for this section because it enters on the left-hand side of the regressions.

[79]Appendix Table XIV provides precise definitions and sources for each covariate used in the analysis.

[80]We estimate this correlation by regressing $\hat{\mu}_{pc}$ on the standardized covariate and then divide by the estimated signal standard deviation of μ_{pc}, shown in Table 7.

[81]This ensures that the coefficients have a population-level interpretation. However, as noted above, for estimation of all model parameters (e.g. the standard deviation of μ_{pc}, $\bar{\theta}_{pc}$) we precision-weight the observations to obtain efficient estimates of these parameters. The results are similar if instead we weighted these regression coefficients by precision instead of population.

gender-specific estimates for $\mu_{25,c}$.

For a selected set of covariates, Figures XV and XVI present a visual representation of the decomposition of the coefficients on the permanent residents into sorting and causal effects. The vertical black lines represent the coefficients on the permanent residents. The bars represent the coefficients on the causal component, $T_C\mu_{pc}$, and the dotted lines connecting the bars to the vertical black lines represent the coefficient on the sorting component. Figure XV presents the results for below-median income families and Figure XVI presents the results for above-median income families. Panel A provides the results at the CZ level and Panel B presents the results for regressions across counties within CZs.

The tables and figures present a wide range of covariates; for brevity, we focus our discussion on several themes that emerged in the exploration.

X.A Race

One of the salient findings in Chetty et al. (2014) is that areas with a higher fraction of African Americans have much lower observed rates of upward mobility. Column (2) of Table XII shows outcomes of permanent residents in below-median income families (p25) in CZs that have a one standard deviation higher fraction of black residents are -2.418pp (s.e. 0.229) lower – which corresponds to roughly 7.6% lower earnings. A natural question is whether this pattern is the result of different people living in different places (a sorting component) or the causal effect these places are having on children in these areas.

Figure XV, Panel A illustrates that this pattern is driven by a relationship with both the sorting and causal component. Roughly half of the spatial correlation with permanent resident outcomes is due to the sorting component; half due to the causal component. On average, 20 years of exposure to a CZ with a 1 standard deviation higher fraction black residents lowers a child's income rank by 1.361 (s.e. 0.339) percentiles for those in below-median income families. This coefficient is presented in the first bar in Figure XV. Scaling by the standard deviation of $\mu_{25,c}$, we find a correlation between the fraction of black residents and the causal effect of the CZ, $\mu_{25,c}$, of -0.514 (s.e. 0.128) reported in Column (2) of Table XII and the far right column of Figure XV. Conversely, the remainder 1.027 ($= 2.388 - 1.361$) is the coefficient on the sorting component. Those who grow up in below-median income families in a CZ with a 1 standard deviation higher fraction black residents have outcomes that would be 1.027pp lower than average regardless of where they grew up.

Across counties within CZs, we find a similar pattern shown in Panel B of Figure XV: there is a negative relationship between $\bar{y}_{pc}$ and the fraction of black residents, which is driven by a relationship with both the causal and sorting components. We find a coefficient of -2.253 (s.e. 0.174) on permanent residents, which decomposes into -0.632 (s.e. 0.201) for the causal component and -1.622 (s.e. 0.220) for the sorting component. And, it implies a correlation of -0.319 (s.e. 0.103) between the fraction of black residents and the causal effect of exposure to the county within the CZ.

For above-median income families, we also find strong negative correlations of outcomes of permanent residents with the fraction of black residents. However, here we find this is largely driven by the sorting component. As shown in Table XIII, those who grow up in above-median income families in a CZ (county-within-CZ) with a 1 standard deviation higher fraction black residents have outcomes that would be -0.501pp (-1.671pp) lower than average regardless of where they grew up. This suggests that the strong negative correlations of children's outcomes for above median income families with the fraction black residents is largely driven by a strong correlation with the sorting component across places.

Overall, these results highlight the potential bias from inferring the causal effects of places solely from the outcomes of permanent residents. However, the evidence here validates the hypothesis that, on average, African Americans live in neighborhoods that cause lower outcomes for children in low-income families (Wilson (1987, 1996); Sampson (2008)). The average impact of exposure from birth, $20 * \mu_{25,c}$, in counties weighted by the fraction of black residents in the county is -1.38. In contrast, the average impact of exposure from birth, $20 * \mu_{25,c}$, in counties weighted by one minus the fraction of black residents in the county is 0.305. This suggests that, on average, African Americans live in counties that produce 1.69 percentile lower outcomes. Scaling this to percentage changes in incomes, it suggests the counties in which African Americans live cause incomes to be 5.3% lower relative to the counties in which non-African Americans live. Given the black-white earnings gap of 25% (Fryer (2010)), this suggests roughly 20% is explained solely by the differences in the counties in which these children grow up.

X.B Segregation, Concentrated Poverty, and Inequality

A large literature in the social sciences argues that neighborhoods with higher degrees of economic and racial segregation and areas of concentrated poverty and inequality are worse places for children to grow up. In this vein, Chetty et al. (2014) document a strong correlation between upward mobility and measures of segregation, inequality, and concentrated poverty. But, while one might wish to infer that these neighborhoods depress upward mobility, there are many reasons to expect that the types of individuals that live in these neighborhoods differ on their unobserved inputs provided to their children, θ_i.

The first two sets of rows in Tables XII-XV illustrate the regression results for measures of segregation, concentrated poverty, and inequality. Five themes emerge from this decomposition.

1. Poverty rates and Segregation across CZs. First, we find no significant correlation with exposure effects and poverty rates across CZs, as shown in the second row of Figure XV, Panel A. This suggests that, at the CZ level of geography, poverty rates are not a very useful proxy for the causal effect of the place on low-income children's outcomes. However, we do find a significant correlation across CZs with measures of segregation, inequality, and sprawl. As reported in Table XII, twenty years of exposure to a CZ with a 1 standard deviation higher fraction of people with commute times less than 15 minutes on average increases a child's income by 2.317 (s.e. 0.353) percentiles for children in below-median income families, corresponding to a more than 7% increase in income. This implies a correlation of 0.875 (s.e. 0.133) between commute times and the causal effects of CZs for below-median income families. Similarly, for the gini coefficient, we find a negative correlation of -0.765 (s.e. 0.131) with the exposure effects of CZs for below-median income families. Spending 20 additional years in a CZ with a one standard deviation higher gini coefficient on average lowers a child's income by -2.024 (s.e. 0.346) percentiles, which corresponds to a more than 6% reduction in income.

We also find evidence that highly segregated areas are especially bad for boys. Appendix Table X illustrates that CZs with a one standard deviation higher fraction of people with commute times shorter than 15 minutes cause an increase in males incomes of 3.364 (0.450) percentiles, which corresponds to $2,453 or a 10% increase in income at age 26. For females, the impact is more modest, with a coefficient of 1.940 (0.558) percentiles, corresponding to a 6.4% increase in incomes, as shown in Appendix Table XI. Importantly, these correlations with commute times are unlikely the direct effect of being closer to jobs. Recall we estimate these place effects using the exposure time methodology: the earlier a child gets to a place with a shorter commute time on average the higher his or her earnings will be. In this sense, it is likely some characteristic of places correlated with commute times that drives the underlying pattern. Indeed, we find similar patterns with other measures of segregation (e.g. Theil indices), as indicated in Appendix Tables X and XI. Overall, commuting zones with higher degrees of segregation and sprawl are areas that generally produce lower outcomes for children in low-income families, especially boys.

2. Urban areas and areas with more immigrants have low causal effects but are positively sorted. Second, we find that across CZs, areas with greater population density (i.e. cities) have both (a) lower causal effects but (b) positive sorting components for children in below-median income families. Regressing the causal component on the standardized log population density, we obtain a coefficient of -1.713 (s.e. 0.315) for children in below-median income families (correlation of -0.647). Yet, we find a positive coefficient of 0.633 (s.e. 0.278) for the sorting component, suggesting that the observed correlation with permanent residents over-states the true causal effects of large cities.

There are many reasons this positive sorting could occur. The results in Table XII do provide suggestive evidence consistent with the hypothesis that immigrants generate some of the positive sorting patterns. We find a positive coefficient of 1.417 (s.e. 0.315) when regressing the sorting component on the fraction foreign born, which is the largest coefficient we find in the data for the sorting component in Table XII (Column 5). This is consistent with the idea that (a) immigrants tend to live in urban areas and (b) children in poor immigrant families tend to have higher outcomes than children in native families with the same parental income level. As a result, the outcomes of permanent residents over-state the impacts these places have on intergenerational mobility. This is also consistent with, for example, New York having a relatively high rate of upward mobility (Chetty et al. (2014)), even though we estimate that it has some of the lowest causal effects on children from below-median income families.

3. Segregation and inequality do not positively correlate with the causal effects for above-median income families. Third, for above-median income families, we find no evidence that areas with more racial and economic segregation tend to produce better outcomes for children in affluent families. If anything, across CZs, areas with higher degrees of segregation and inequality have negative impacts on children's earnings from above-median income families. Figure XVI illustrates these patterns with fraction black residents, poverty share, and racial segregation – we generally find small causal effects. However, we do continue to find very strong negative correlations between the causal effects across CZs and measures of income inequality and other measures of segregation. The correlation of $\mu_{75,c}$ with income segregation is -0.557 (0.167) and with the gini coefficient is -0.694 (s.e. 0.227). This is related to the observation noted above (and in Table VII) that CZs that produce better outcomes for poor children also produce better outcomes for more affluent children.

4. Poverty rates are weakly correlated with $\mu_{25,c}$ across counties – measures of segregation and income inequality are stronger correlates. Fourth, across counties within CZs we find a correlation between $\mu_{25,c}$ and poverty rates of -0.232 (s.e. 0.108), suggesting this traditional metric for place quality is correlated with place effects at a more local level. However, we continue to find stronger correlations with other measures of county characteristics, including measures of economic and racial segregation and income inequality. Twenty years of exposure to a county within a CZ with a one-standard deviation higher gini coefficient lowers the child's income rank by -0.813 (s.e. 0.270) percentiles, which corresponds to a 2.5% reduction in income. Twenty years of exposure to a county within a CZ with a one standard deviation degree of economic segregation (Theil index) causes on average a reduction in the child's income rank of -0.837 (s.e. 0.200).

5. There is greater sorting across counties. Finally, across counties within CZs, we observe patterns consistent with higher degrees of residential sorting at finer geographies, as a higher fraction of the observed correlation appears to reflect variation in the sorting components. This is perhaps best illustrated by the dashed lines corresponding to the sorting component in Panel B of both Figure XV and XVI. For those in below-median income families, counties with a higher degrees of residential segregation and income inequality have lower outcomes for permanent residents; and indeed, the coefficients for the permanent residents are larger than what can be accounted for by 20 years of exposure, suggesting a portion of the observed relationship with permanent residents reflects a sorting pattern. For example, using the racial segregation Theil index, we find a negative coefficient of -0.735 (s.e. 0.190) for the causal effect, but a coefficient of -1.501 (s.e. 0.195) for the sorting component. This suggests that the observed correlation of outcomes of children in below-median income families with measures of segregation and concentrated poverty reflects both a sorting and causal component.

For those in above-median income families, we find larger evidence of sorting and less evidence of a correlation with the causal effect. The racial segregation theil index has a positive coefficient of 0.309 (s.e. 0.211) for the causal effect, but a negative coefficient of -1.642 (s.e. 0.223) for the sorting component. The observed negative relationship across counties within CZs for those in above-median income families with measures of segregation and concentrated poverty largely reflects a correlation with the sorting, not the causal, component.[82]

[82] In principle, the extent to which the variables are correlated with the sorting component depends on our assumption for T_c. However, in this instance, there is very minimal observed correlation between these variables and the causal effect; whereas there is an observed significant relationship with the outcomes of permanent residents. Hence,

In sum, the fact the negative correlation of place effects with these measures of segregation, inequality, and concentrated poverty is consistent with the idea that these conditions may play a causal role in limiting the economic outcomes of disadvantaged youth. However, our results add in several ways to this literature. First, in contrast to the pure spatial mismatch theory (Wilson (1987, 1996)), the exposure effects documented here operate when growing up, not during adulthood.[83] Second, we find strong evidence that CZs with more segregation and concentrated poverty have negative effects on kids from both rich and poor families – there does not appear to be a tradeoff whereby places with greater segregation improve outcomes for above-median income families. Third, at the finer geography of counties within CZs, counties with more segregation have negative effects on poor children; but a nontrivial portion of the observed negative correlation between observed outcomes of children in more affluent outcomes reflects a correlation with the sorting component, as opposed to a causal effect.

X.C Family Stability

Across CZs, there is a strong relationship between upward mobility and measures of family stability. Areas with lower fractions of single parents have much higher rates of upward mobility (Chetty et al. (2014)). This could reflect the causal effects of CZs with more single parents, but it could also reflect an impact of growing up in a single versus two-parent household or other family demographic effects.

In Table XII and Figure XV (Panel A), we present evidence that both effects are operating. For children in below-median income families, 20 years of exposure to CZs with a 1 standard deviation higher fraction of single parent households causes a child's income rank to be 1.5pp (s.e. 0.316) lower on average, or 4.7% reduction in incomes. This corresponds to a correlation of -0.567 (s.e. 0.119) between the fraction of single parents and the place effects, $\mu_{25,c}$. However, children living in areas with one standard deviation larger share of single parents on average will have outcomes that are 0.909pp lower than the average child regardless of where they live. Hence, slightly more than half of the observed relationship between family stability and upward mobility reflects the causal effects these areas are having on children's outcomes, as shown in Figure XV (Panel A).

Across counties within CZs, we find a similar pattern but find larger evidence of a correlation with sorting patterns. A one standard deviation higher fraction of single parents in the county

the conclusion that most of the relationship with the permanent residents is driven by a correlation with the sorting component is not overly dependent on our choice of T_c.

[83] This is consistent with the ideas expressed in Sampson (2008).

corresponds to -0.747 (s.e. 0.212) reduction in the child's income percentile but a -1.739 (s.e. 0.195) lower sorting component. So although both are significantly different from zero, a significant fraction of the relationship between the fraction of single parents and the outcomes of permanent residents reflects a sorting pattern.

For above median income families, we also observe a negative relationship between the fraction of single parent households and child outcomes of permanent residents across CZs and across counties within CZs, as shown in Figure XVI. Yet we find a minimal correlation between the fraction of single parents and the causal effect of the CZ on children in above-median income families. Hence, the observed lower outcomes in counties and CZs with a higher fraction of single parents for children in above-median income families is almost entirely driven by a correlation with the sorting component, not the causal effect.

X.D Social Capital

Social capital has been argued to play an important role in promoting upward mobility (Coleman (1988); Putnam (1995)), and measures of social capital are strongly positively correlated with the causal effects of place across CZs. Twenty years of exposure to CZs with a 1 standard deviation higher level of the social capital index of Rupasingha and Goetz (2008) cause an increase in incomes of 1.845 (s.e. 0.352) percentiles for children from below-median income backgrounds (Table XII) and 1.417 (s.e. 0.434) percentiles for above-median income backgrounds (Table XIII). In contrast, we find slightly negative coefficients for the sorting component. This suggests the observed correlation of intergenerational mobility with social capital across areas of the US largely reflects the differences in the causal effects of these places on childrens' outcomes from both high and low income backgrounds. Although this is only a correlation with the causal effects and does not establish a causal relationship between social capital and economic outcomes, it is consistent with the theory that social capital is a mechanism for promoting better outcomes for children across the parental income distribution.

We also find evidence that measures of social capital are more strongly correlated with the causal effects on low-income boys as opposed to girls outcomes. Twenty years of exposure to a CZ with a one standard deviation higher measure of the social capital index will increase a boys' income in adulthood by 2.609 (s.e. 0.447) percentiles, a 7.8% increase in income; for girls the increase is only 1.164 (s.e. 0.508) percentiles, or a 3.8% increase in income. Similarly, twenty years of exposure to CZs with a one standard deviation higher violent crime rate will cause, on average, a reduction in boys' incomes by -2.244 (0.366) percentiles, or 6.7%, but a reduction of girls' incomes by -1.322 (s.e. 0.580) percentiles, or 4.3%. CZs with more social capital and lower crime rates seem to have positive causal effects, especially on boys.[84]

X.E K-12 Education

Across CZs, outcomes of permanent residents are strongly correlated with measures of school quality. Tables XII-XIII and Figures XV-XVI illustrate that much of this correlation reflects the causal effects these places have on children. For children in below-median income families, moving to a CZ with a 1 standard deviation higher (income-residualized) test score percentile causes an increase in child's income percentile of 1.346pp (s.e. 0.269) for 20 years of exposure, corresponding to a 4.2% increase in incomes at age 26. Similarly, we find a coefficient of 1.473 (s.e. 0.438) for children in above median income families, corresponding to a 3.0% increase in income. CZs where children have higher test scores have higher causal effects on children's earnings in young adulthood.

Across CZs, we find no statistically significant positive correlations between measures of school quality and the sorting component, suggesting that much of the observed pattern reflects a correlation with the causal effects of these CZs. However, across counties within CZs, we do begin to find significant correlations with the sorting component, consistent with the existence of a greater degree of residential sorting at these finer geographies. Indeed, our estimates suggest much of the observed pattern of permanent residents reflects this sorting component across counties. We find coefficients from regressing the sorting component on residualized test scores of 1.055 (s.e. 0.316) at p25 and 0.958 (s.e. 0.334) at p75. However, we continue to find significant positive coefficients for below-median income families of 0.702 (s.e. 0.259) for the causal component, suggesting counties with higher quality schools have significantly positive impacts on below-median income children's outcomes.

Finally, we also find some evidence that areas with high quality measures of the K-12 education system have especially higher causal effects on low-income (p25) boys relative to girls. Across CZs, we estimate that places with a one standard deviation higher residualized test scores cause boys to

[84] At the county-within-CZ level, we do not find strong correlations with measures of social capital. However, we do find stronger negative correlations with other measures related to social capital including the violent crime rate. Across CZs, the violent crime rate has a strong negative correlation with both the causal and sorting components. Across counties within CZs, areas with a 1 standard deviation higher violent crime rate cause a reduction in children's incomes of -0.635 (s.e. 0.211) percentiles for those in below-median income families, corresponding to a correlation of -0.320 with the exposure effects. However, for above-median income families we find no significant correlation with the causal effects; rather, for both above and below-median income families we find strong negative correlations between the violent crime rate and the sorting component of the place.

earn 2.116 (0.402) percentiles more at age 26, or 6.3%. In contrast for girls the increase is 0.534 (s.e. 0.391) percentiles, or 1.7%. We find similar patterns for the dropout rate and student/teacher ratio; areas with higher measures of the quality of the K-12 education system have higher causal effects, especially on low-income boys.

X.F Other Covariates

We explored a wide range of covariates in our analysis, ranging from measures of the number and affordability of local colleges, structure of the local tax code and measures of tax expenditures, and measures of migration. Tables XII-XV report those correlations and coefficients. Appendix Tables X-XIII report the results for the gender-specific place effect estimates for below-median (p25) income families. We omit a detailed discussion of each of these covariates, as even this list of covariates is far from exhaustive. Online Data Tables 3 and 4 provide the raw data for future work exploring these patterns in more detail.

X.G Prices

Does it cost more to live in places that improve childrens' outcomes? In the last two rows of Tables XII-XV, we correlate our measures of place effects, μ_{pc}, with the median rent and median house price from the 2000 Census.[85] More expensive areas generally produce lower, not higher, outcomes. We find a strong negative correlation of -0.324 (s.e. 0.133) between $\hat{\mu}_{25,c}$ and house prices and -0.424 (s.e. 0.139) with rent.[86] The negative correlation with prices is perhaps not surprising, since rural areas have higher causal effects and are also less expensive. But, moving from an urban commuting zone to a rural commuting zone requires not only purchasing a new house – it generally requires obtaining a new job. Because the availability of jobs is another important factor in a location decision, it is potentially misleading to consider the negative correlation with rent and house prices as an indication that it is cheaper on net to move to a CZ with a higher causal effect.

We find more salient patterns when looking across counties within CZs. The location decision within a commuting zone aligns more closely with the conceptual experiment of holding fixed the set of job opportunities available to families when making location choices. Table XIV shows that across counties within CZs, house prices and rents are not positively correlated with $\mu_{25,c}$. But while we find zero correlation on average across counties within CZs, it turns out this masks several

[85] More specifically, we take the median prices in the county and average them across counties within the CZ.

[86] We find even stronger negative correlations for $\mu_{75,c}$ of -0.648 (s.e. 0.120) for house prices and -0.718 (s.e. 0.180), as shown in Table 10.

patterns in urban versus rural and segregated versus non-segregated CZs.

Figure XVII explores these patterns by quantifying how much, on average, it costs to move to a place with a 1-unit higher causal effect in various types of CZs across the U.S. To construct this measure, we seek hypothetical regression of prices on μ_{pc}. We obtain these coefficients by regressing prices on the forecast estimates, μ_{pc}^{f}, which remove the attenuation from the sampling uncertainty in $\hat{\mu}_{pc}$.[87] Figure XVII splits the sample into CZs with populations above and below 100K. Within the large CZs, we split them into those with above median segregation and below-median segregation, where segregation is defined as the fraction of people with commute times less than 15 minutes. In each sample, Figure XVII presents binned scatter plots of median rent on the forecasted place effects for children in below-median income families, $\mu_{25,c}^{f}$, conditional on CZ fixed effects, weighted by 2000 population.

Figure XVIIa illustrates that in large segregated CZs, moving to a county that is forecasted to increase a child's income rank by 0.1 percentiles per year (for children in below-median income families) incurs, on average, a \$52 increase in median monthly rent.[88] In contrast, in large non-segregated CZs, Figure XVIIb illustrates that we find no such pattern: counties that are forecast to increase a child's income rank by 0.1 percentiles per year have, on average, \$6 lower monthly rent, which is not statistically distinguishable from zero. In other words, there is a price-quality tradeoff across counties in large, highly-segregated CZs; but this tradeoff does not appear to emerge in large CZs with below-median levels of segregation.

In smaller CZs with populations below 100,000, we find that counties that produce better outcomes are actually cheaper. Moving to a county that is forecast to increase a child's income rank by 0.1 percentiles per year (for children in below-median income families) is associated with, on average, \$18 lower median monthly rents.[89] This negative correlation with prices across counties in rural CZs offsets the positive patterns we find in large segregated CZs, so that a pooled analysis does not reveal any underlying significant correlations with prices. In urban segregated CZs, rents are higher in areas that produce higher outcomes.

[87] Note that using the forecasts that incorporate permanent resident outcomes would introduce bias from the sorting component embodied in the permanent resident outcomes. Hence, we construct these forecasts by scaling $\hat{\mu}_{pc}$ by the signal-to-total-variance ratio, and do not use the permanent residents in the optimal forecast.

[88] We find very similar patterns for all of the results in this section if use the 25th percentile of the rent distribution in each county, as opposed to the median

[89] Although we do not have conclusive evidence on why this negative pattern exists, we have explored whether any correlates in Tables 9-12 can explain this pattern by having an inverse correlation with the county's effect on children and median rental prices. One such variable that follows this pattern is income inequality. In CZs with populations below 100,000, we find a strong negative correlation between the county place effects, $\mu_{25,c}$, and income inequality (e.g. as measured by the gini coefficient on incomes below the top 1%); but counties with higher income inequality generally have higher median rents amongst CZs with populations below 100,000.

Observables versus Unobservables. For families choosing to live in a particular location, it is perhaps difficult to know the place's impact on their child's outcomes later in life. As shown in Tables XII-XV, these place impacts are highly correlated with potentially observable measures of place quality, such as schools, social capital, segregation, and family structure. As a result, one can think of $\mu_{25,c}$ as having two components: an "observable" component that is projected onto observable covariates (excluding the permanent resident outcomes), such as school quality, social capital, etc, and an unobservable component that is the residual after projecting this forecast onto the observable covariates. It is natural to ask which of these two components is driving the positive correlation with housing prices in large CZs.

To explore this, we regress the county-level fixed effect estimates $\hat{\mu}_{pc}$ in CZs with populations greater than 100,000 on several standardized covariates in Tables XII-XV: the fraction of single parents, the fraction with travel time less than 15 minutes, the gini-99 coefficient (gini coefficient on incomes below the top 1%), the fraction below the poverty line, and a measure of school quality using an income-residualized measure of test scores. We include CZ fixed effects and restrict to CZs with populations above 100,000. We then define the observable component as the predicted value from this regression. We define the unobservable component as the residual from this regression, which we shrink by its signal-to-noise ratio so that it is an unbiased forecast of the residual for a particular place.[90]

Figure XVIIIa illustrates that the positive correlation with monthly rent is driven entirely by the observable component of $\mu^f_{25,c}$, despite using only a handful of variables to span the observable subspace.[91] Moving to a county within a CZ that produces a 0.1 percentile increase (i.e. a 0.3% increase in the child's earnings) per year exposure of based on its observable characteristics costs \$102.56 (s.e. \$8.35) per month, holding the unobservable component constant. In contrast, we find no significant relationship between prices and the unobservable component. Moving to a county within a CZ that will produce a 0.1 percentile increase per year of exposure based on its unobservable characteristics costs only \$21.68 per month (s.e. \$12.36), holding the observable

[90] Using these observables, we obtain a standard deviation of predicted values of 0.055 implying that roughly one-third of the signal variance is captured by our observable component.

[91] Figure XVIII provides a non-parametric representation of the (partial) regression coefficients obtained from regressing monthly rent on the observable and unobservable components, conditional on CZ fixed effects. For Figure XVIIIa, we regress the observable component of $\mu^f_{25,c}$ on CZ fixed effects and the unobservable component and bin the residuals into 20 equally sized vingtile bins. We also regress median monthly rent on the same CZ fixed effects and unobservable component of $\mu^f_{25,c}$. Figure XVIIIa then plots the average of this residual in the 20 vingtile bins. The slope then represents the partial regression of median monthly rents on the observable component of μ^f_{pc}, controlling for CZ fixed effects and the unobservable component of μ^f_{pc}. Figure XVIIIb repeats this process, interchanging the observable and unobservable components.

components constant.

Assuming that, all else equal, parents prefer to raise their children in places that have higher causal effects on income, the pattern is consistent with a couple of hypotheses: On the one hand, it could be the case that parents cannot uncover the unobservable component and it is therefore not incorporated into prices. Alternatively, it could be the case that parents do know about the unobservable component, but that places with positive unobservable components also have other worse amenities that prevent a higher price from being realized. A deeper analysis of the potential existence of such amenities is beyond the scope of this paper. But more generally, this finding suggests a potential direction for future work to better understand the objective function that parents are maximizing when choosing where to raise their children and the information set or heuristics they use to evaluate these decisions.

XI Conclusion

Where children grow up affects their outcomes in adulthood in proportion to the time they spend in the place. This result helps to reconcile a large observational literature documenting wide variation in outcomes across areas with an experimental literature that generally finds little effects of neighborhoods on economic outcomes. We show that 50-70% of the observational variation in children's outcomes cross CZs reflects the causal effect of growing up in those areas.

At first glance, our results might appear to be inconsistent with experimental evidence on the impacts of neighborhoods on economic outcomes. Most notably, the Moving to Opportunity (MTO) housing voucher experiment documents little in the way of economic impacts on adults and older youth (e.g. Kling et al. (2007)). However, if neighborhoods have causal effects in proportion to the exposure time to the neighborhood, then the subset of children that would benefit most from moving out of high poverty areas would be those who were youngest at the time of the experiment, precisely the subset of participants whose long-term outcomes have not, until recently, been available for analysis. In a follow-up paper (Chetty, Hendren, and Katz (2015)), we link the MTO data to tax data and show that the MTO data exhibit the same exposure time patterns as those we document here. Children whose families received an experimental housing voucher and moved to a low-poverty neighborhood at young ages (e.g., below age 13) earn 30% more in their mid 20's than the control group. Children who moved at older ages do not show such gains, consistent with exposure time being a key determinant of neighborhood effects.

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

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Online Appendix A. Fixed Effects Estimator of Exposure Effect.

In this appendix, we show that the fixed effects regression in (9) yields a coefficient $b_m = \beta_m + \delta_m$. The regression in (9) is equivalent to the univariate OLS regression.

$$y_i - \bar{y}_{pom} = b_m(\bar{y}_{pd} - \bar{y}_{pdom}) + \eta_i \tag{22}$$

where $\bar{y}_{pom} = E[y_i|p(i) = p, o(i) = o, m(i) = m]$ is the mean outcome for those who start in o and move elsewhere at age m and $\bar{y}_{pdom} = E[\bar{y}_{pd}||p(i) = p, o(i) = o, m(i) = m]$ is the mean outcome of the permanent residents in the destinations to which these individuals move.

Using the model in (1), the outcomes of children in the one-time movers sample can be written as

$$y_i = \Lambda_m \mu_{po} + (1 - \Lambda_m)\mu_{pd} + \theta_i - \kappa_m$$

where μ_{po} and μ_{pd} represent the causal effects of the origin and destination at percentile p, and $\theta_i = \frac{1}{T}\sum \theta_{it}$ is the mean level of investment by parents in child i over his childhood. It follows that

$$\bar{y}_{pom} = \Lambda_m \mu_{po} + (1 - \Lambda_m)\bar{\mu}_{pdom} + \bar{\theta}_{pom} - \kappa_m,$$

where $\bar{\theta}_{pom} = E[\theta_i|p(i) = p, o(i) = o, m(i) = m]$ and $\bar{\mu}_{pdom} = E[\mu_{pd}|p(i) = p, o(i) = o, m(i) = m]$ are the mean level of parental inputs and mean destination place effects in this sample. The deviation in child $i's$ outcome relative to other movers from his origin is

$$y_i - \bar{y}_{pom} = (1 - \Lambda_m)(\mu_{pd} - \bar{\mu}_{pdom}) + (\theta_i - \bar{\theta}_{pom})$$

Using the definition of β_m in (4) in a randomly assigned sample at age m, $E[\varepsilon_i|c] = 0$ and hence $E[y_i|c] = \alpha_m + \beta_m \bar{y}_{pc}$. In the same sample, equation (6) implies $E[y_i|c] = (1 - \Lambda_m)\mu_{pc} - \kappa_m$. It follows that

$$(1 - \Lambda_m)\mu_{pc} = \kappa_m + \alpha_m + \beta_m \bar{y}_{pc}$$

and hence

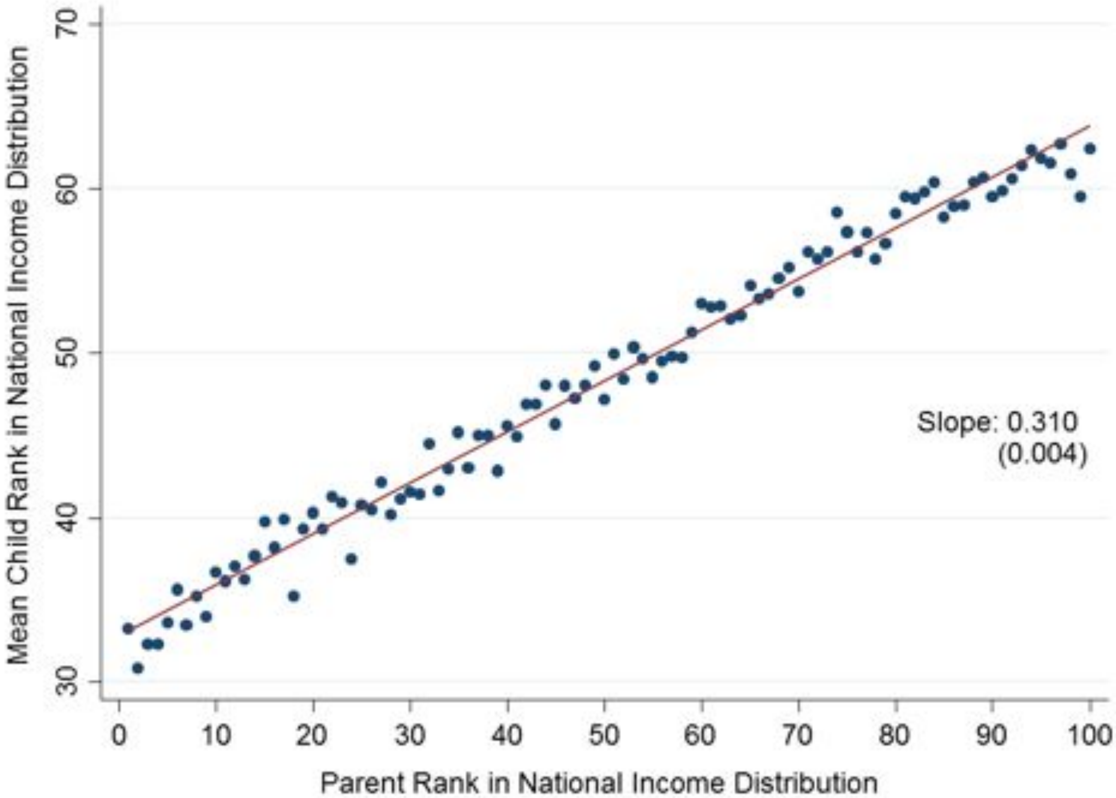
$$(1 - \Lambda_m)(\mu_{pd} - \bar{\mu}_{pdom}) = \beta_m(\bar{y}_{pd} - \bar{y}_{pdom}).$$

Therefore, the regression coefficient b_m in (22) is

$$\begin{aligned} b_m &= \frac{Cov((1 - \Lambda_m)(\mu_{pd} - \bar{\mu}_{pdom}) + \theta_i - \bar{\theta}_{pom}, \bar{y}_{pd} - \bar{y}_{pdom})}{Var(\bar{y}_{pd} - \bar{y}_{pdom})} \\ &= \beta_m + \frac{Cov(\theta_i - \bar{\theta}_{pom}, \bar{y}_{pd} - \bar{y}_{pdom})}{Var(\bar{y}_{pd} - \bar{y}_{pdom})} = \beta_m + \delta_m \end{aligned}$$

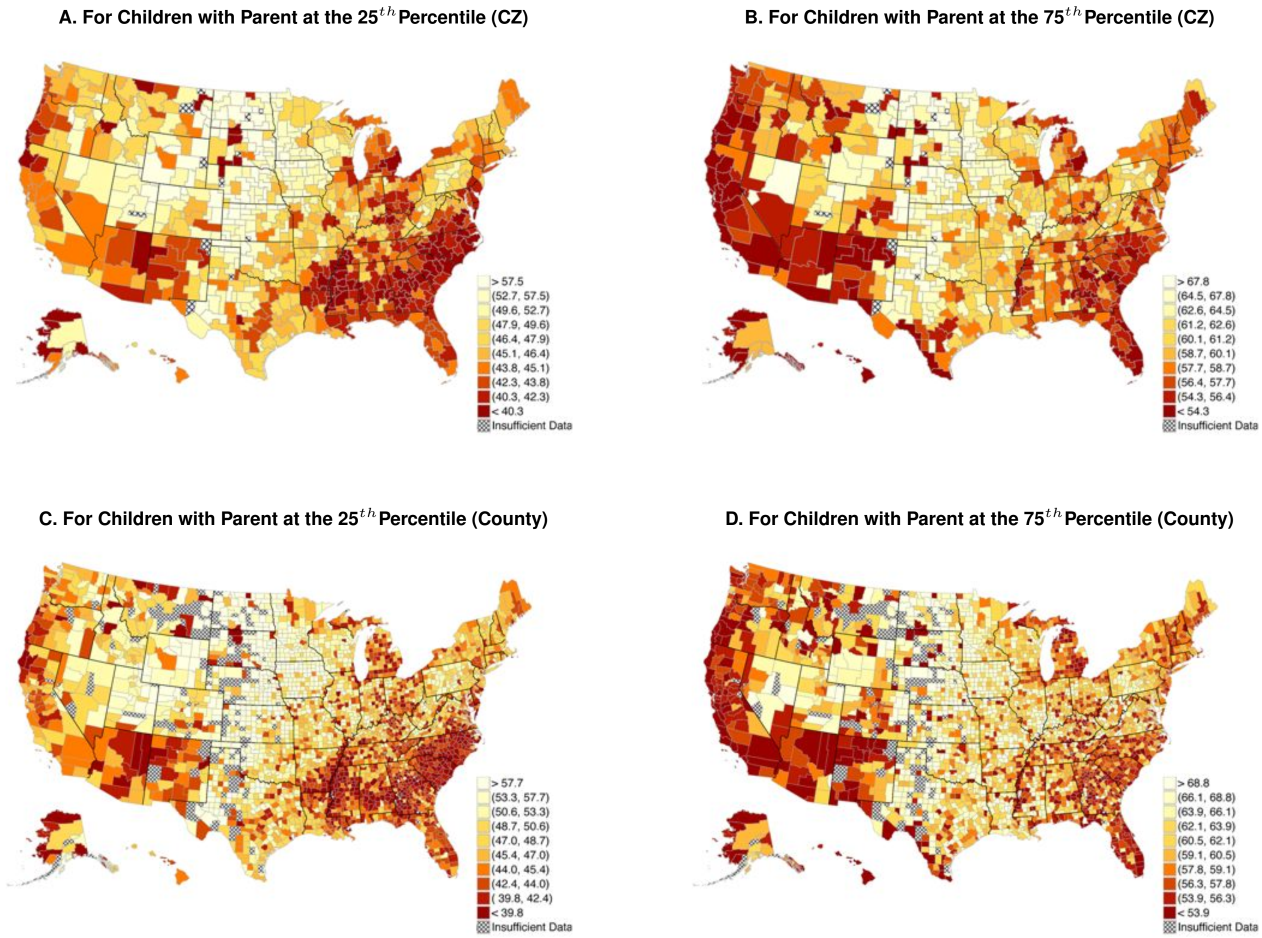
because $Cov(\bar{\theta}_{pom}, \bar{y}_{pd} - \bar{y}_{pdom}) = 0$.

FIGURE I: Mean Child Income Rank at Age 26 Vs. Parent Income Rank for Children Raised in Chicago



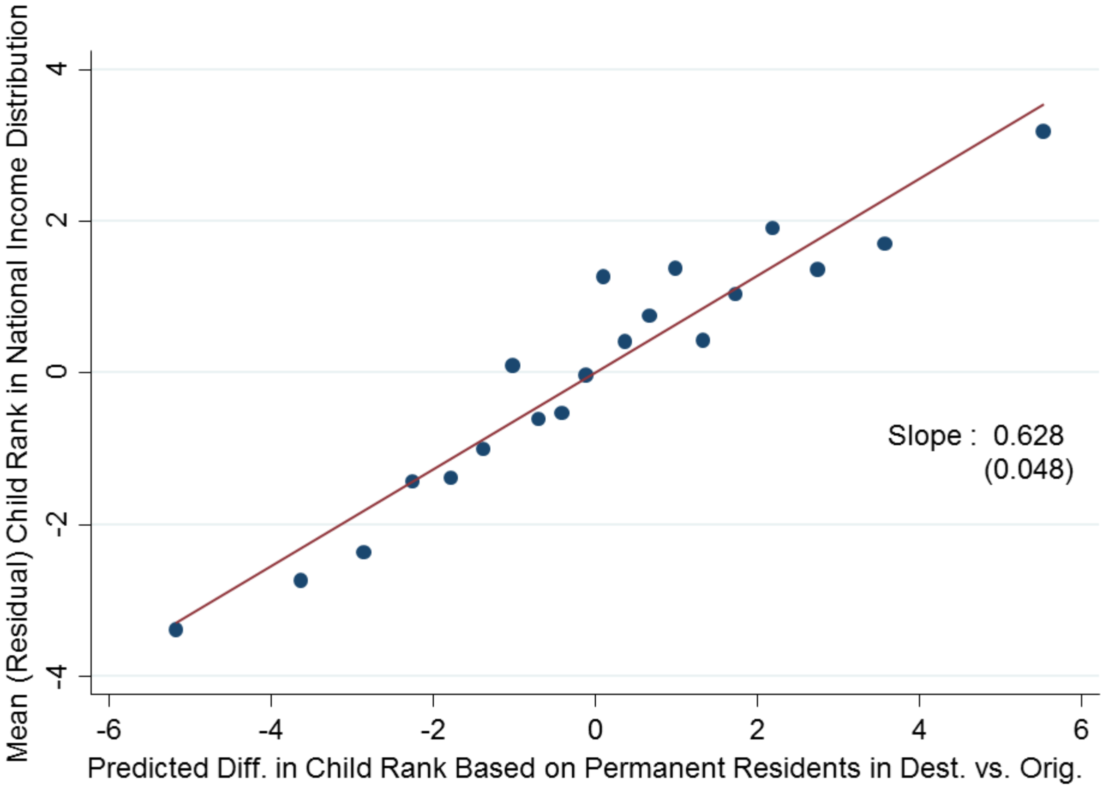
Notes: This figure presents a non-parametric binned scatter plots of the relationship between mean child income ranks and parent income ranks for all children raised in Chicago. Figure measures income of the children at age 26 using the 1985 cohort. Child income is family income at age 26, and parent income is mean family income from 1996-2000. We define a child's rank as her family income percentile rank relative to other children in her birth cohort and his parents' rank as their family income percentile rank relative to other parents of children in the core sample. The ranks are constructed for the full geographic sample, but the graph illustrates the relationship for the sub-sample of families who report living in Chicago for all years of our sample, 1996-2012. The figure then plots the mean child percentile rank at age 26 within each parental percentile rank bin. The slope and best-fit lines is estimated using an OLS regression on the micro data. Standard errors are reported in parentheses.

FIGURE II: Predicted Income Rank at Age 26 - Permanent Residents



Notes: These figures illustrate the geographic variation in child income rank outcomes at age 26 from the 1985 cohort amongst our sample of permanent residents across commuting zones (CZs) and counties in the U.S. Panel A reports the expected rank for children whose parental income is at the 25th percentile of the income distribution of parents, and Panel B reports the expected rank for children whose parental income is at the 75th percentile. Both figures use the baseline family income definitions for parents and children. The figure restricts to the subset of parents who stay in the commuting zone throughout our sample period (1996-2012) (but does not restrict based on the geographic location of the child at age 26). To construct this figure, we regress child income rank on a constant and parent income rank in each CZ, exploiting the linearity property shown in Figure I. Panel A then reports the predicted child rank outcome for parents at the 25th percentile of the family income distribution (~$30K per year). Panel B reports the predicted child rank outcome for parents at the 75th percentile of the family income distribution (~ $97K per year).

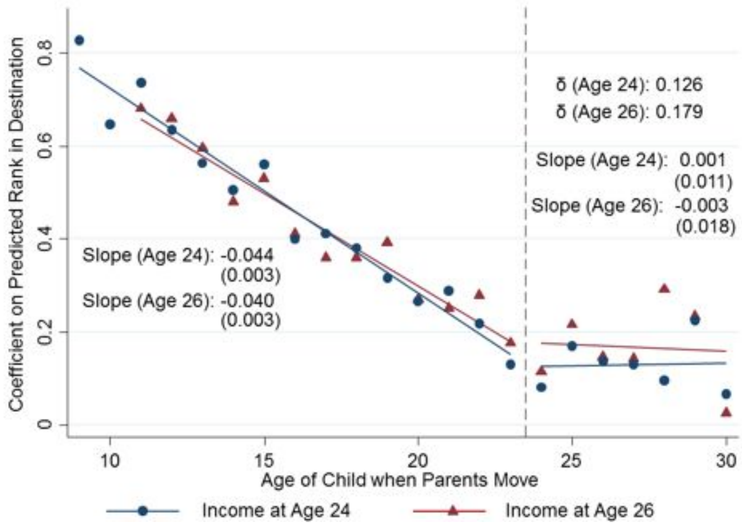
FIGURE III: Movers' Outcomes at Age 26 vs. Predicted Outcomes Based on Residents in Destination Moves at Age 13



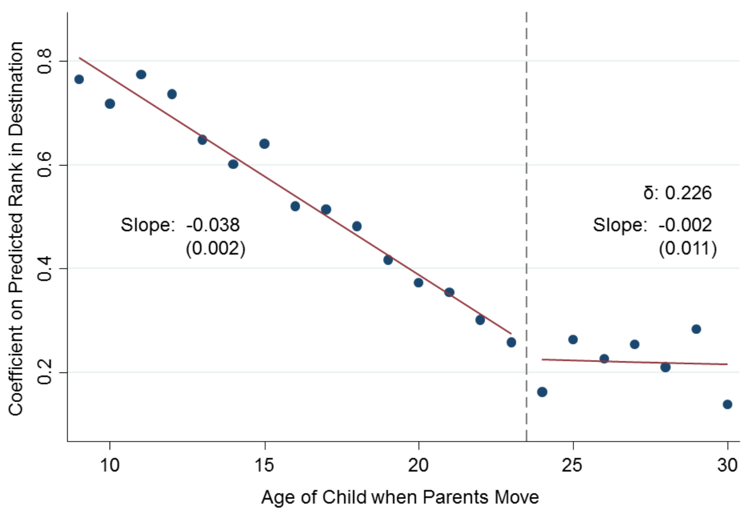
Notes: This figure presents a non-parametric illustration of the b_{13} coefficient in equation (6). The sample includes all children in 1-time moving households whose parents moved when the child was 13 years old. Child income is measured when the child is age 26. The figure is constructed by first partialing out the fixed effects (the interaction of (a) origin CZ, (b) the child's age at the parental move, (c) cohort, and (d) parental income deciles): we regress the difference in the destination versus origin prediction, Δ_{odps}, on the fixed effects and the child rank outcome on the fixed effects. The figure then plots the relationship between these residuals from each of these regression. We construct 20 equal sized bins of the residuals from the destination regression and, in each bin, plot mean of the residuals from the child rank regression.

FIGURE IV: Exposure Effect Estimates for Children's Income Rank in Adulthood

A. Income at Age 24 and 26



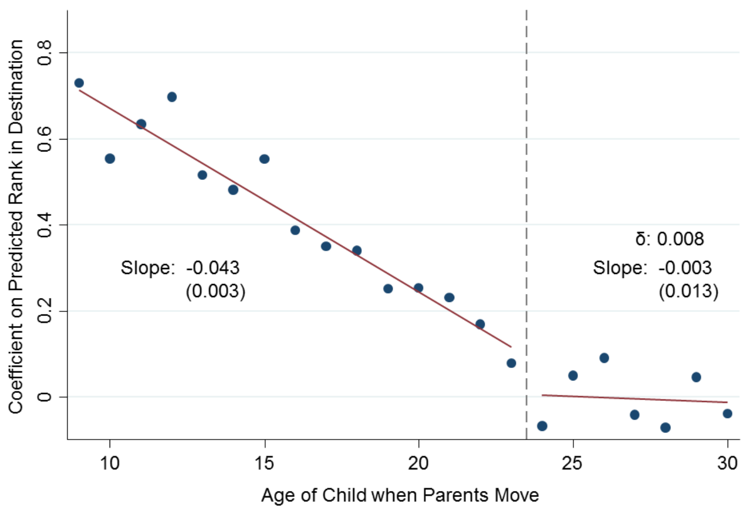
B. Income at Age 24



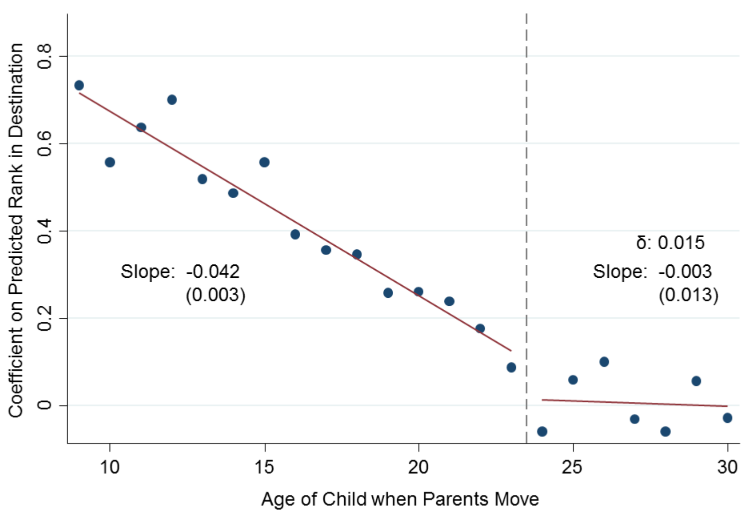
Notes: Panel A presents estimates of the coefficients, $B = (b_m)$, in equation (7) for various ages of the child of income measurement. The sample includes all children in 1-time moving households. Child income is measured when the child is age 24, and 26. We estimate these coefficients by regressing the child's family income rank on the difference in the predicted family income rank based on prior residents in the destination location relative to the origin location (computed using the linear regression illustrated in Figure I) interacted with each age of the child at the time of the move. We include the set of fixed effects for origin by parent income decile by cohort by the child's age at the time of the move (as in Figure III). Panel B presents estimates from the specification in equation (9). This specification drops the large set of fixed effects and instead includes (a) dummies for the child's age at the time of the move, (b) parental rank (within the child's cohort) interacted with child age dummies, and (c) cohort dummies and predicted outcomes in the destination and origin interacted with cohort dummies. Panels A and B report slopes and intercepts from a regression of the b_m coefficients on m separately for $m \leq 23$ and $m > 23$. We compute δ as the predicted value of the line at age 23 using the b_m estimates for $m > 23$.

FIGURE V: Exposure Effect Estimates for Children's Income Rank in Adulthood with Controls for Observables

A. Family Fixed Effects

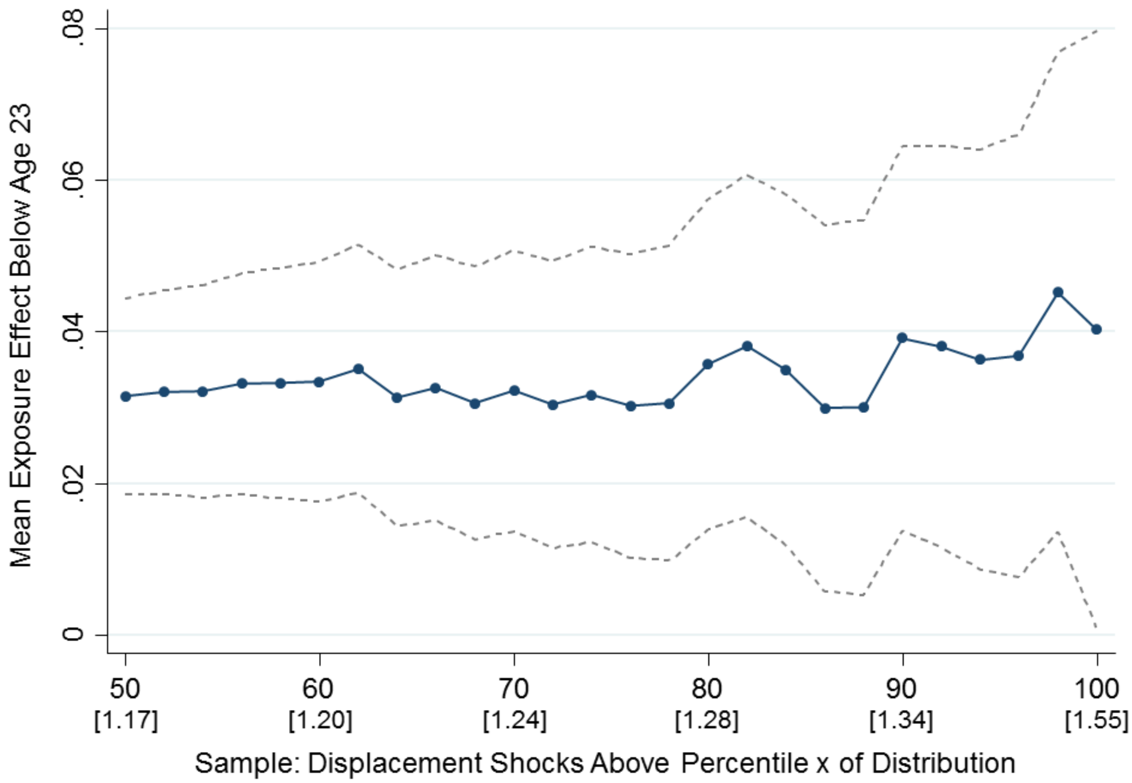


B. Family Fixed Effects and Time Varying Controls



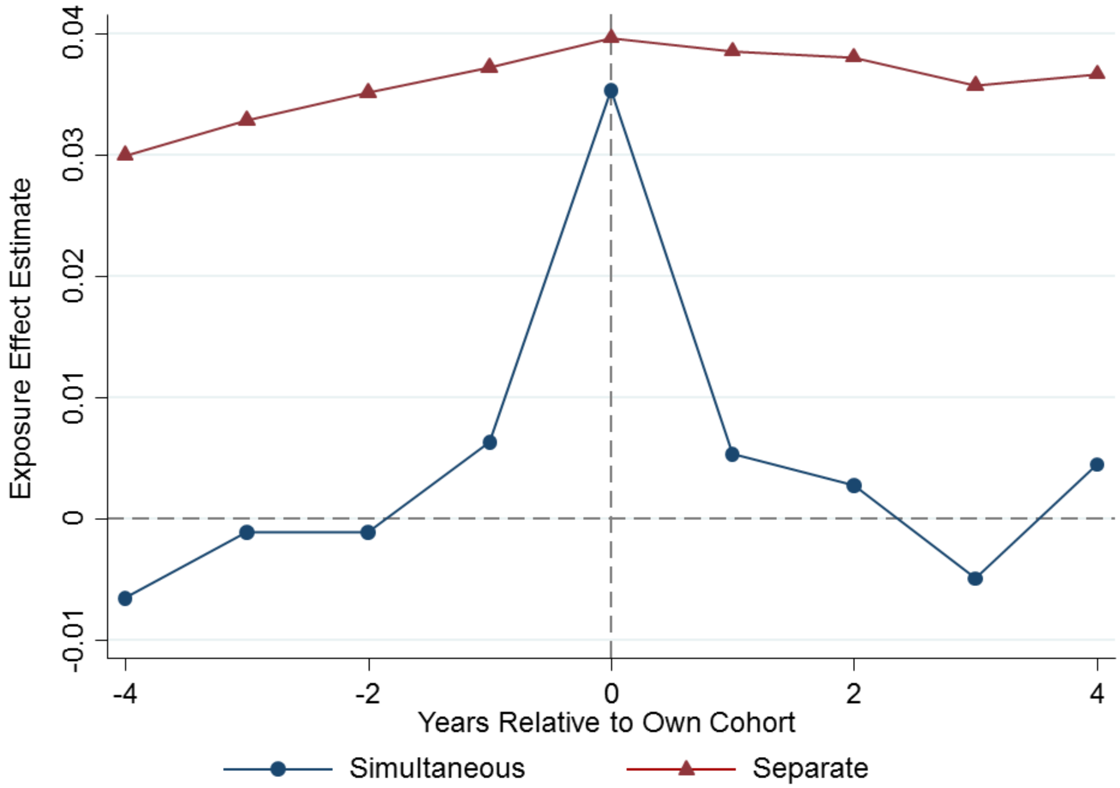
Notes: This figure presents estimates of the coefficients, $B = (b_m)$, in specifications that add family fixed effects (Panel A) and both family fixed effects and controls for changes in marital status and parental income (Panel B). Panel A presents estimates of b_m from the baseline specification in equation (9) with the addition of family fixed effects. Panel B adds family fixed effects along with a set of controls for income rank changes marital status changes around the time of the move. To do so, we construct parental income ranks by cohort by year of outcome measurement. We interact the differences in parental ranks in the year before versus after the move with a linear interaction with the child age at the time of the parental move (for ages below 24) and an interaction with an indicator for child age greater than 23 at the time of the parental move. We also construct a set of indicators for marital status changes. We define marital status indicators for the year before the move and the year after the move and construct indicators for being always married, getting divorced, or being never married (getting married is the omitted category). We include these variables and their linear interactions with the child age at the time of the parental move (for ages below 24) and an interaction wtih an indicator for child age greater than 23 at the time of the parental move. As in Figure IV, we report slopes and intercepts from a regression of the b_m coefficients on m separately for $m \leq 23$ and $m > 23$. We compute δ as the predicted value of the line at age 23 using the b_m estimates for $m > 23$.

FIGURE VI: Displacement Shocks IV Exposure Effects Estimates



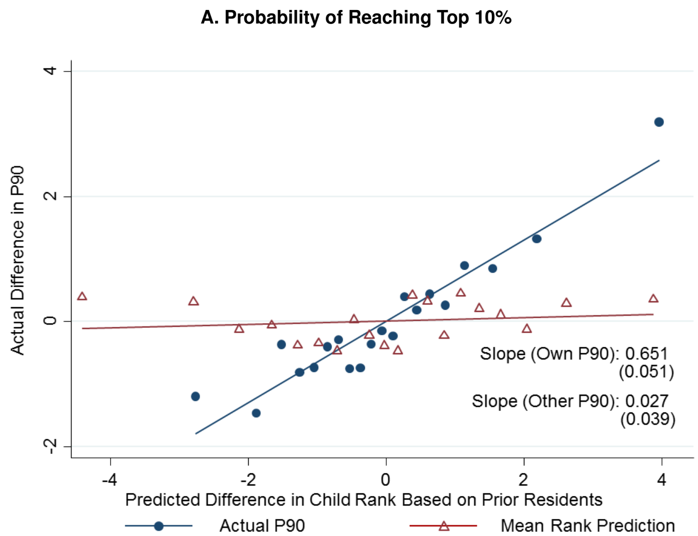
Notes: This figure presents estimates of the exposure time slope for a subsample of moves restricted to zipcode-by-year observations with large outflows, instrumenting for the change in predicted outcomes based on prior residents, Δ_{odps}, with the average change in predicted outcomes for the given origin. More specifically, for each zipcode in our sample of children in the 1980-1993 cohorts, we calculate the number whose parents leave the (5-digit) zipcode in each zipcode, z, in year t, m_{zt}. Then, we compute the average number of people who leave in a given year across our 1997-2012 sample window, $\bar{m}_z$. We then divide the outflow in a zipcode-year observation, m_{zt}, by the mean outflow for the county to construct our measure of the displacement shock, $d = \frac{m_{zt}}{\bar{m}_{zt}}$. The horizontal axis presents the results for varying quantile thresholds of d ranging from the median to the 95th percentile. The corresponding mean value of d for the sample is presented in brackets. For each zipcode, we compute the mean value of Δ_{odps} for each parental income decile (pooling across all years and all movers in the zipcode). Throughout, we restrict to zipcode-years with at least 10 observations. Then, for each sample threshold, the figure presents IV estimates of the exposure slope for values of d above the threshold.

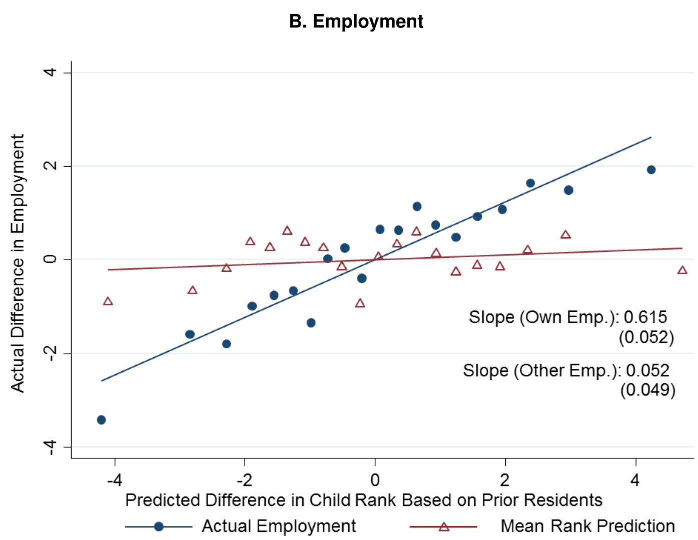
FIGURE VII: Exposure Effects Based on Cross-Cohort Variation, with Cohort-Varying Intercepts



Notes: This figure presents estimates of the exposure time slope using own and placebo cohort place predictions. The sample includes all children in 1-time moving households whose parents moved when the child was less than or equal to 23 years old. The series in red traingles plots estimates of 9 separate regressions using place predictions for child in cohort c as if s/he were in cohort $c+k$, where k ranges between -4 and 4. By construction, the estimate for $k=0$ corresponds to the baseline slope of 0.040, illustrated in Figure IV (Panel B). Regressions include the predicted outcomes based on prior residents in the origin and destination (for cohort $c+k$), and the interactions of the child's age at the time of the move with the predicted outcomes in the origin and destination based on prior residents (for cohort $c+k$). To be consistent with the baseline specifications, regressions also include dummy indicators for true cohort and its interaction with the predicted outcomes in the origin location. The blue series reports coefficients from a single regression that includes all variables in each of the regressions for $k=-4,...,4$ and plots the coefficient on the interaction of the child's age at the time of the move with the predicted outcome based on prior residents in the destination location in cohort $c+k$.

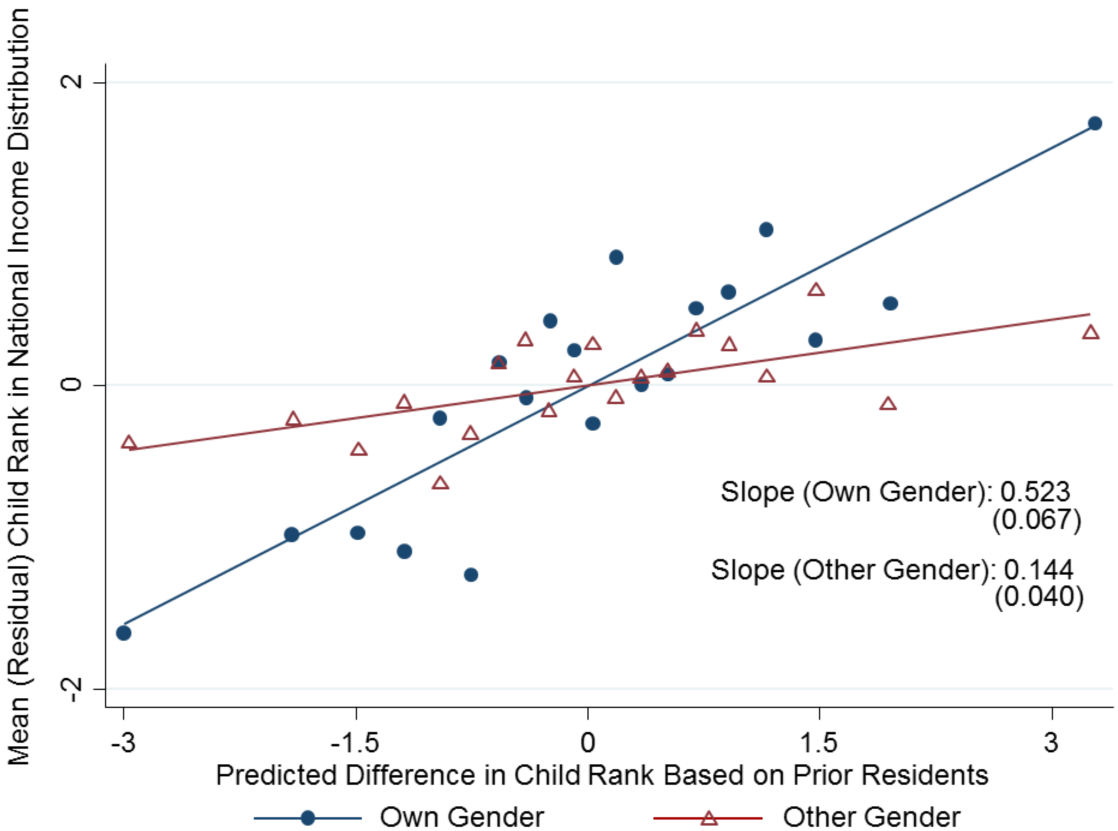
FIGURE VIII: Movers' Outcomes vs. Predicted Employment and Probability of Reaching top 10% in Destination





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

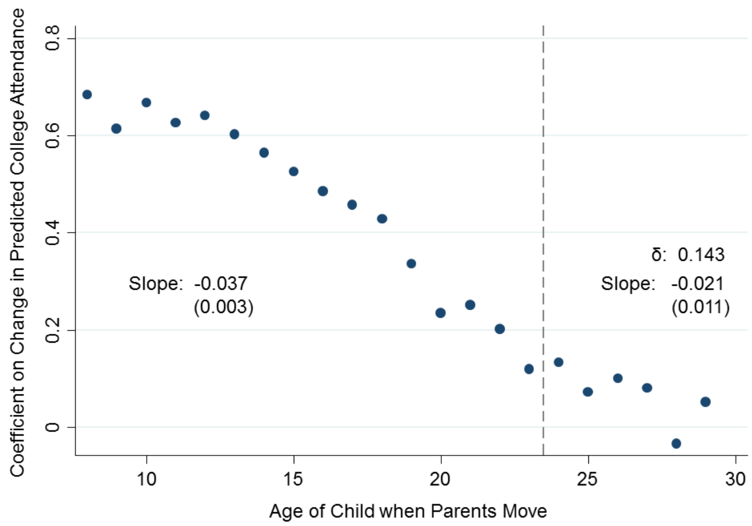
FIGURE IX: Movers' Outcomes vs. Gender-Specific Predicted Outcomes in Destination



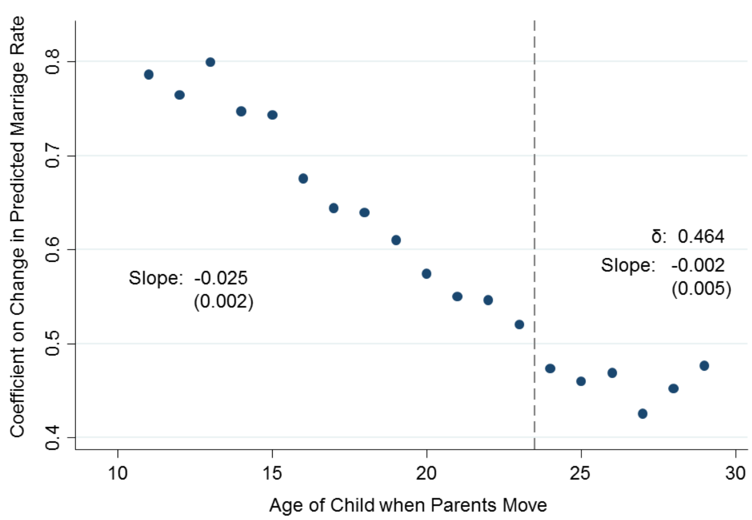
Notes: This figures presents binned scatter plots analogous to Figure III, but using gender-specific predicted outcomes based on prior residents. The blue series provides a non-parametric representation of the relationship between the child's own gender place prediction and the child's outcome; the red series provides a non-parametric representation of the relationship between the other (placebo) gender place predictions for the child's outcome, controlling for the own gender prediction. The sample includes all children in 1-time moving households whose parents moved when the child was less than or equal to 13 years old. Child income is measured when the child is age 26. For the blue circle series, we regress the own gender destination prediction for the child's outcome on the other gender destination prediction, other gender origin prediction, and own gender origin prediction. Similarly, we regress the child's income rank on the other gender destination prediction, other gender origin prediction, and own gender origin prediction. The figure then plots the relationship between these residuals from these regressions with sample means added to center the graphs. We construct 20 equal sized bins of the residuals from the destination regression and, in each bin, plot mean of the residuals from the child rank regression. For the red series, we repeat this process but using the placebo (other) gender predictions. We regress the other gender destination prediction for the child's outcome on the own gender destination prediction, other gender origin prediction, and own gender origin prediction. Similarly, we regress the child's income rank on the own gender destination prediction, other gender origin prediction, and own gender origin prediction. The red triangle series then plots the relationship between these residuals from these regressions with sample means added to center the graphs.

FIGURE X: Exposure Effect Estimates for College Attendence (18-23) and Marriage at Age 26

A. College Attendance (Age 18-23)

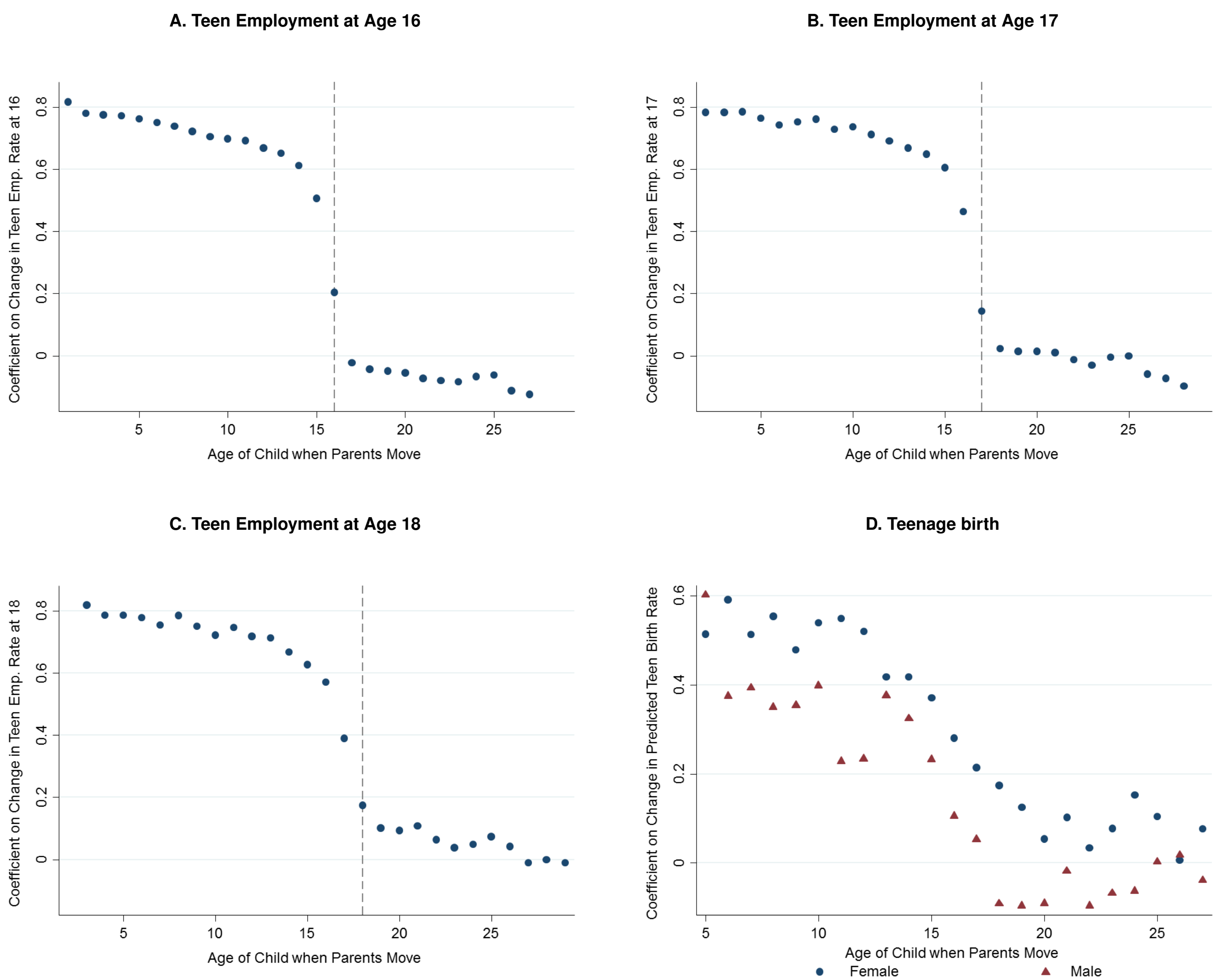


B. Marriage (Age 26)



Notes: This figure presents exposure effect estimates for college and marriage outcomes. In Panel A, we replicate the baseline specification (equation 9) replacing the child's outcomes with an indicator for college attendence at any age between 18-23. We construct separate analogous predicted outcomes based on the prior residents in each CZ for each outcome. We define college attendence as the existence of a 1098-T form (indicating college enrollment) when the child is 18-23 years old and restrict the sample to observations we observe for years 18-23. Because we observe college attendance in years 1999-2012, we obtain estimates for ages at move of 8-29. In Panel B, we replicate the baseline specification (equation 9) replacing the child's outcomes with an indicator for being married at age age 26 using the child's filing status at age 26.

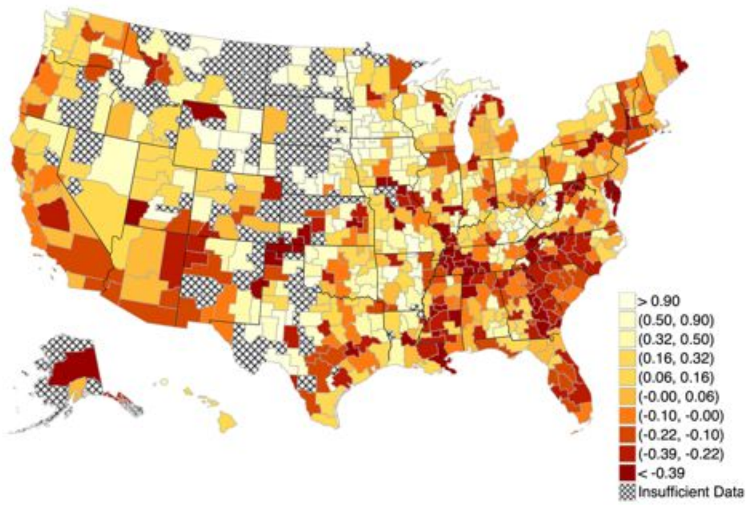
FIGURE XI: Exposure Effect Estimates for Teen Outcomes



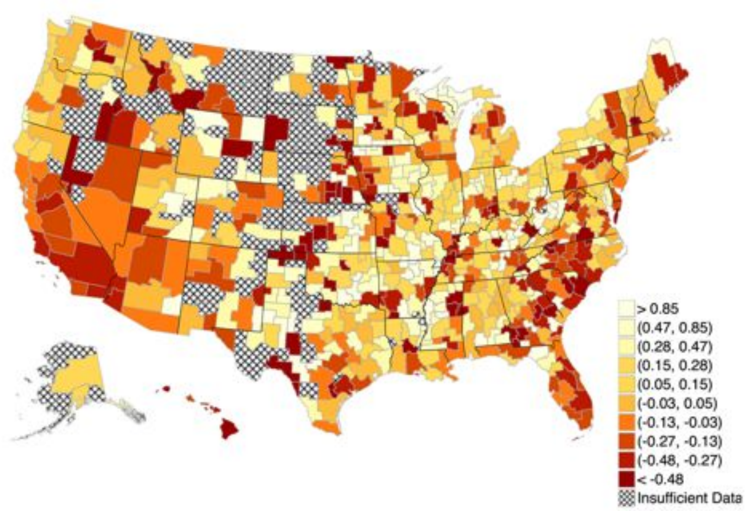
Notes: This figure presents exposure effect estimates for teen outcomes. Panels A-C replicate the baseline specification with origin prediction controls (Figure IV, Panel B), but replaces the child's outcomes with an indicator for working at age 16-18 (defined as the existence of a W-2 during the year in which the child turned age a). Panel D presents estimates from the baseline specification using teen birth as the outcome. We define teenage birth as having a birth in the calendar year prior to turning age 20 using birth certificate records from the social security administration's death master file (DM-2), and estimate the model separately for males and females.

FIGURE XII: The Geography of Exposure Effects on Income Across CZs

A. At 25th Percentile ($\mu_{25,c}$)



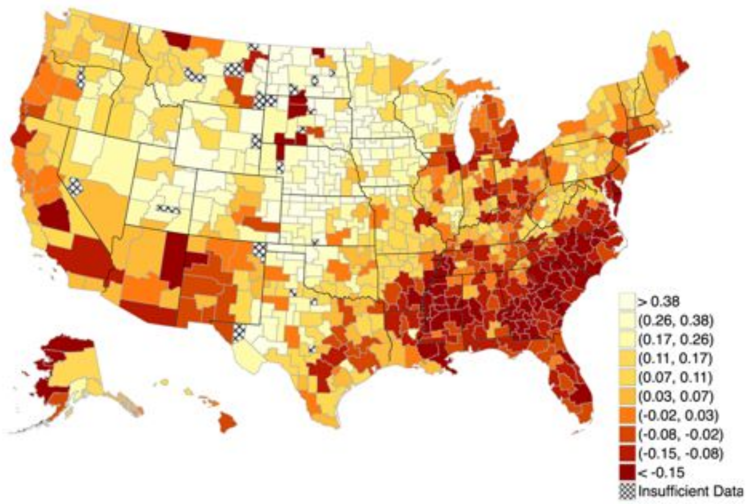
B. At 75th Percentile ($\mu_{75,c}$)



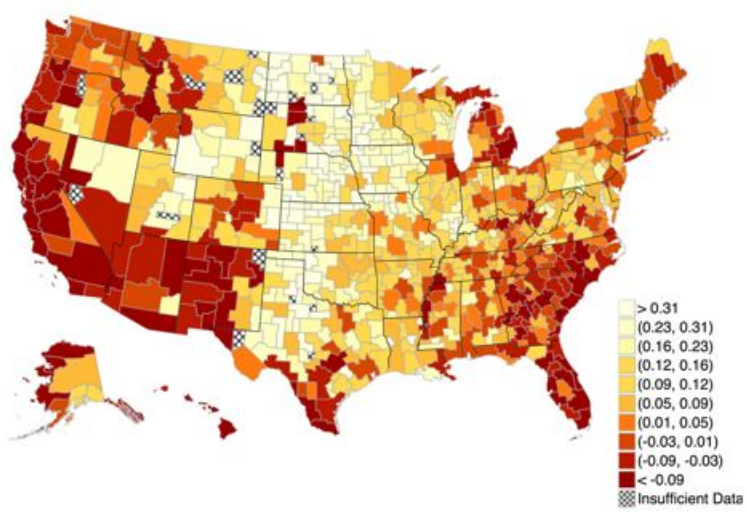
Notes: These figures present estimates of place effects, $\hat{\mu}_{pc}$ in for child income rank at age 26 by Commuting Zone, for children from families at the 25th percentile and 75th percentile of the parental income distribution. Section V discusses the estimation strategy and sample restrictions. The values represent the causal effect of spending 1 additional year growing up in a CZ (relative to a population-weighted average CZ).

FIGURE XIII: Predicted Estimates: National CZ

A. At 25th Percentile ($\mu_{25,c}$)

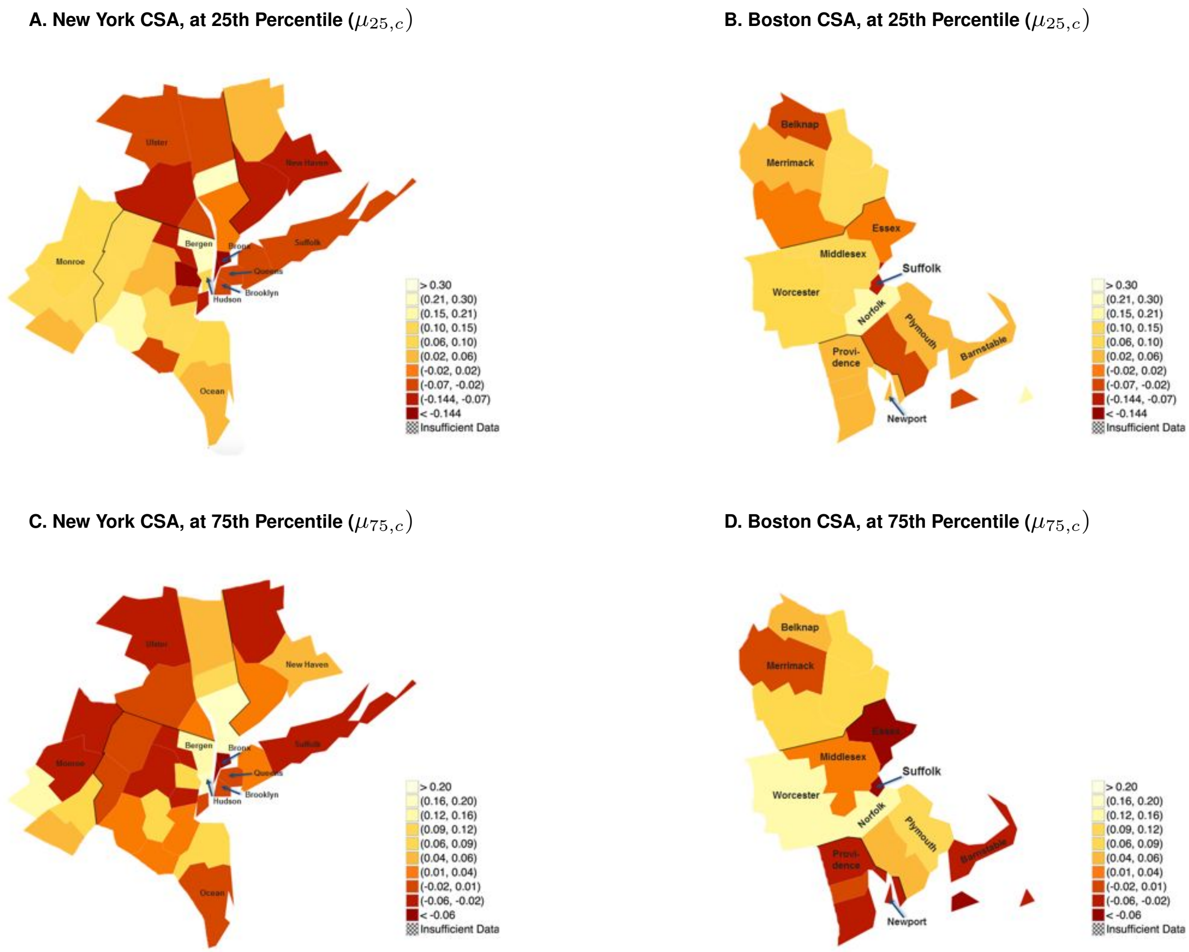


B. At 75th Percentile ($\mu_{75,c}$)



Notes: These figures present forecast estimates of each CZ's causal effects, μ^f_{pc}, for below-median ($p = 25$) and above-median ($p = 75$) income families. We compute these forecasts using the methodology discussed in Section IX.A and, in particular, using the formula in Equation 21. For small-population CZs for which we do not have fixed effect estimates, we display the permanent resident outcomes (which corresponds to the natural assumption that $\hat{s}_{pc} = \infty$ in Equation 21 in the case when we have no fixed effect estimate).

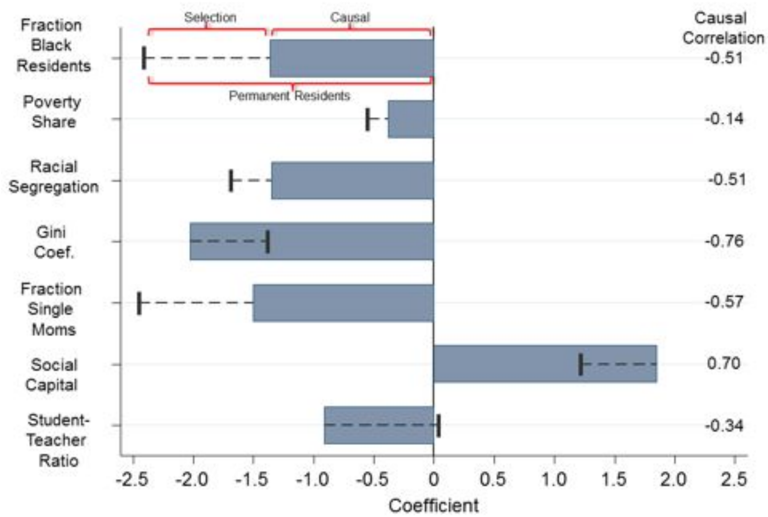
FIGURE XIV: Predicted Estimates for NY and Boston CSA by County



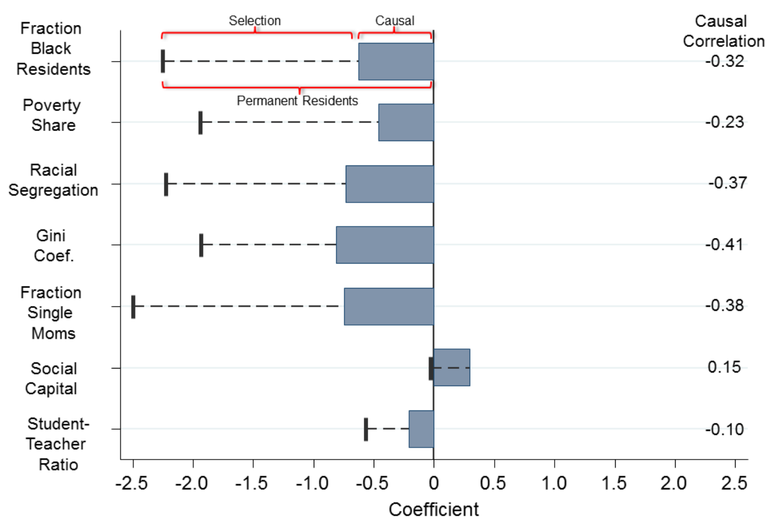
Notes: These figures present forecast estimates of the county-level causal effects, μ^f_{pc}, for below-median ($p = 25$) and above-median ($p = 75$) income families in the New York and Boston Combined Statistical Areas (CSAs). We compute these using the formula in Equation 21 using the county-level fixed effect estimates, $\hat{\mu}_{pc}$ (which are the sum of the CZ and county-within-CZ estimates, as discussed in Section XII.D), and the permanent resident forecasts, $\bar{y}_{pc}$, for each county.

FIGURE XV: Predictors of Exposure Effects For Children with Parents at 25^{th} Percentile

A. At the Commuting Zone Level



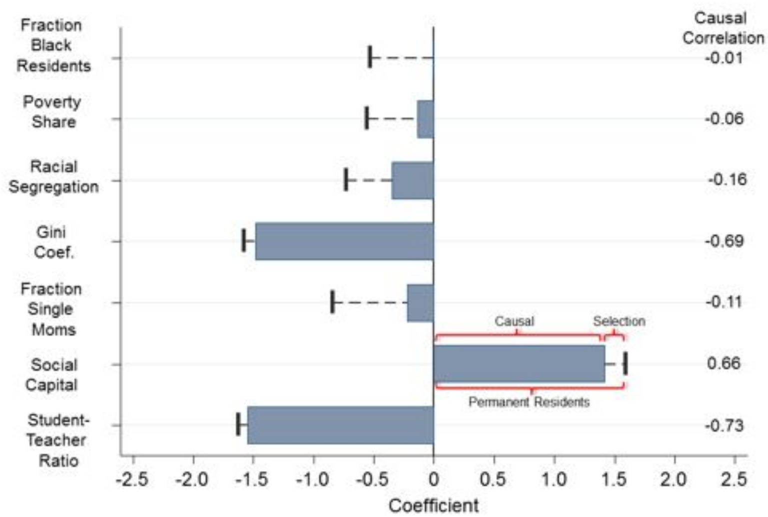
B. At the County Level; within CZs



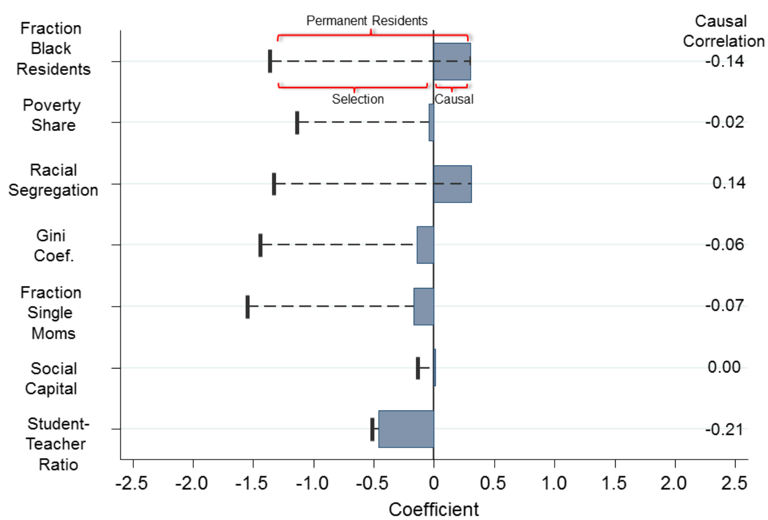
Notes: These figures show the coefficients of regressions of the model components for below-median income families ($p = 25$) on a set of covariates analyzed in Chetty et al. (2014) which are normalized to have mean zero and unit standard deviation. The vertical line represents the coefficient from a regression of the permanent resident outcomes, $\bar{y}_{25,c}$, on the covariate. The solid bar represents the coefficient from a regression of the causal component, $T_c\mu_{25,c}$, on the covariate, so that the difference between the bar and the vertical line (denoted by the dashed horizontal line) represents the regression coefficient from a regression of the sorting component, $\bar{y}_{25,c} - T_c\mu_{25,c}$, on the covariate. The column on the far left divides the regression coefficient by the standard deviation of $\mu_{25,c}$, providing the implied correlation between the covariate and the causal effects. We restrict the sample to CZs and counties for which we have both causal fixed effects and permanent resident outcome measurements. The covariate definitions are provided in Appendix Table X. Results for additional covariates provided in Tables XII-XV. Panel A presents the results at the CZ level. Panel B presents the results at the county within CZ level by conditioning on CZ fixed effects.

FIGURE XVI: Predictors of Exposure Effects For Children with Parents at 75^{th} Percentile

A. At the Commuting Zone Level



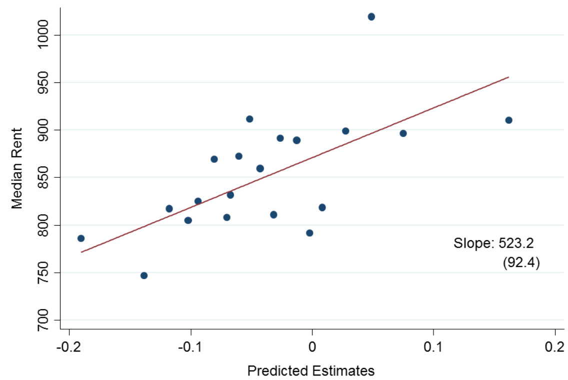
B. At the County Level; within CZs



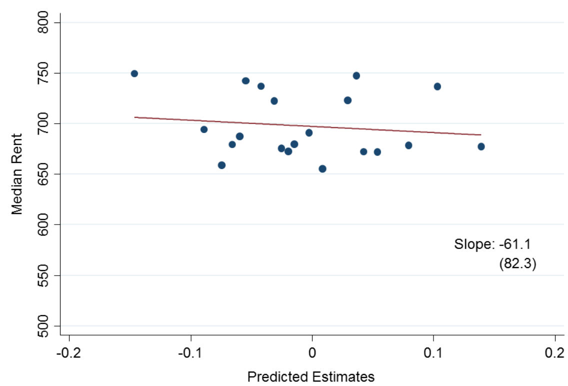
Notes: These figures show the coefficients of regressions of the model components for above-median income families ($p = 75$) on a set of covariates analyzed in Chetty et al. (2014) which are normalized to have mean zero and unit standard deviation. The vertical line represents the coefficient from a regression of the permanent resident outcomes, $\bar{y}_{75,c}$, on the covariate. The solid bar represents the coefficient from a regression of the causal component, $T_c\mu_{75,c}$, on the covariate, so that the difference between the bar and the vertical line (denoted by the dashed horizontal line) represents the regression coefficient from a regression of the sorting component, $\bar{y}_{75,c} - T_c\mu_{75,c}$, on the covariate. The column on the far left divides the regression coefficient by the standard deviation of $\mu_{75,c}$, providing the implied correlation between the covariate and the causal effects. We restrict the sample to CZs and counties for which we have both causal fixed effects and permanent resident outcome measurements. The covariate definitions are provided in Appendix Table X. Results for additional covariates provided in Tables XII-XV. Panel A presents the results at the CZ level. Panel B presents the results at the county within CZ level by conditioning on CZ fixed effects.

FIGURE XVII: Median Rent versus Exposure Effects

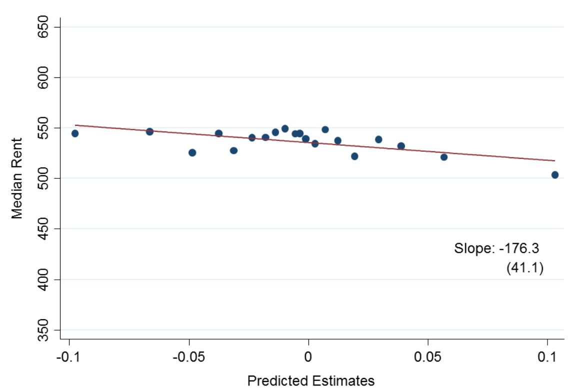
A. Above-Median Segregated CZs with Populations above 100,000



B. Below-Median Segregated CZs with Populations above 100,000



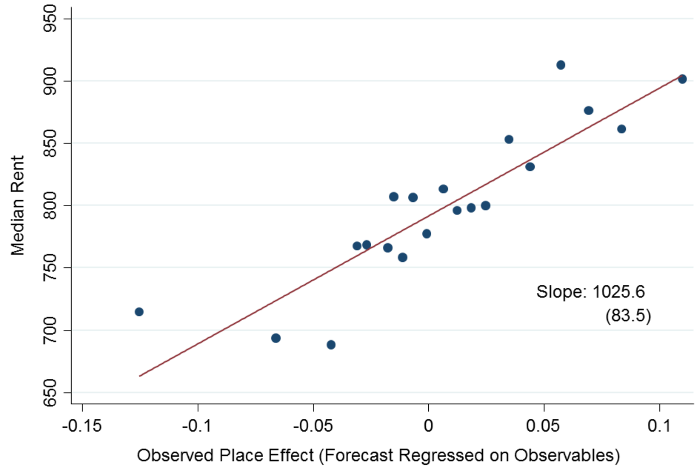
C. CZs with Populations below 100,000



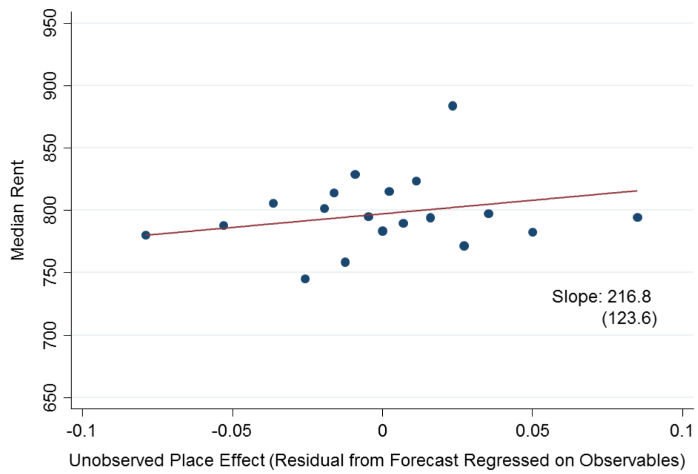
Notes: This figure presents binned scatterplots corresponding to a regression of median rent in the county (from the 2000 Census) on the predicted exposure effect for that county at $p = 25$, $\mu^f_{25,c}$. In contrast to the model in Section IX, we construct the forecasts $\mu^f_{25,c}$ using only the fixed effect estimates, $\hat{\mu}_{25,c}$ normalized by their signal-to-total variance ratio (we do not incorporate information from permanent residents, $\bar{y}_{pc}$, in order to avoid picking up correlations between prices and the sorting components). Panels A-C present binned scatter plots of the relationship between median rent in the county and the predicted exposure effect of the county, conditional on CZ fixed effects. We split counties into three groups: those in CZs with populations above and below 100,000 based on the 2000 Census. We then split the set of CZs with populations above 100,000 into two groups: those with above-median segregation/sprawl and below-median segregation/sprawl, where segregation/sprawl is defined by the fraction of people in the CZ that have commute times less than 15 minutes. Panel A reports the binned scatterplot for CZs with above-median segregation/sprawl and CZ populations above 100,000; Panel B reports the binned scatterplot for CZs with below-median segregation/sprawl and CZ populations above 100,000. Panel C reports the binned scatterplot for CZs with population below 100,000.

FIGURE XVIII: Median Rent versus Unobservable and Observable Exposure Effects

A. Median Rent versus Observable Component



B. Median Rent versus Unobservable Component



Notes: This figure presents binned scatter plots corresponding to a regression of median rent on the observable and unobservable components of the county-level forecasts, μ^f_{25c}, on the sample of CZs with populations above 100,000, conditional on CZ fixed effects. We construct the observable component by regressing $\hat{\mu}_{25,c}$ on five covariates that are standardized to have mean zero and unit variance: the fraction of children with single parents, the fraction with travel time less than 15 minutes, the gini coefficient restricted to the 0-99th percentiles of the income distribution (which equals the gini minus the fraction of income accruing to the top 1%), the fraction below the poverty line, and a residualized measure of test scores (see Appendix Table X for further variable details). We weight observations by the estimated precision of $\hat{\mu}_{25,c}$. We then define the "observable" component as the predicted values from this regression. For the unobservable component, we take the residual from this regression and multiply it by its estimated total variance divided by the signal variance of the residual. The total variance is given by the variance of the residuals, weighted by the estimated precision of $\hat{\mu}_{25,c}$. To construct the signal variance of the residual, we estimate the noise variance as the mean of the square of the standard errors, weighted by the estimated precision of $\hat{\mu}_{25,c}$. Given the observable and unobservable components, Panel A presents the binned scatterplot corresponding to the regression of median rent on the observable component, controlling for CZ fixed effects and the unobservable component. We regress median rent on the the unobservable component and CZ fixed effects and construct residuals. We then regress the observable component on the unobservable component and CZ fixed effects and construct residuals. We bin these residuals of the observable component into vengtiles and within each vingtile plot the average of the median rent residuals. Hence, the slope of the line corresponds to the partial regression coefficient of a regression of median rent on the observable component, controlling for the unobservable component and CZ fixed effects. For Panel B, we replace the observable and unobservable components in the process for Panel A, so that the slope of the graph corresponds to the partial regression coefficient on the unobservable component in a regression of median rent on the observable and unobservable components of the forecast.

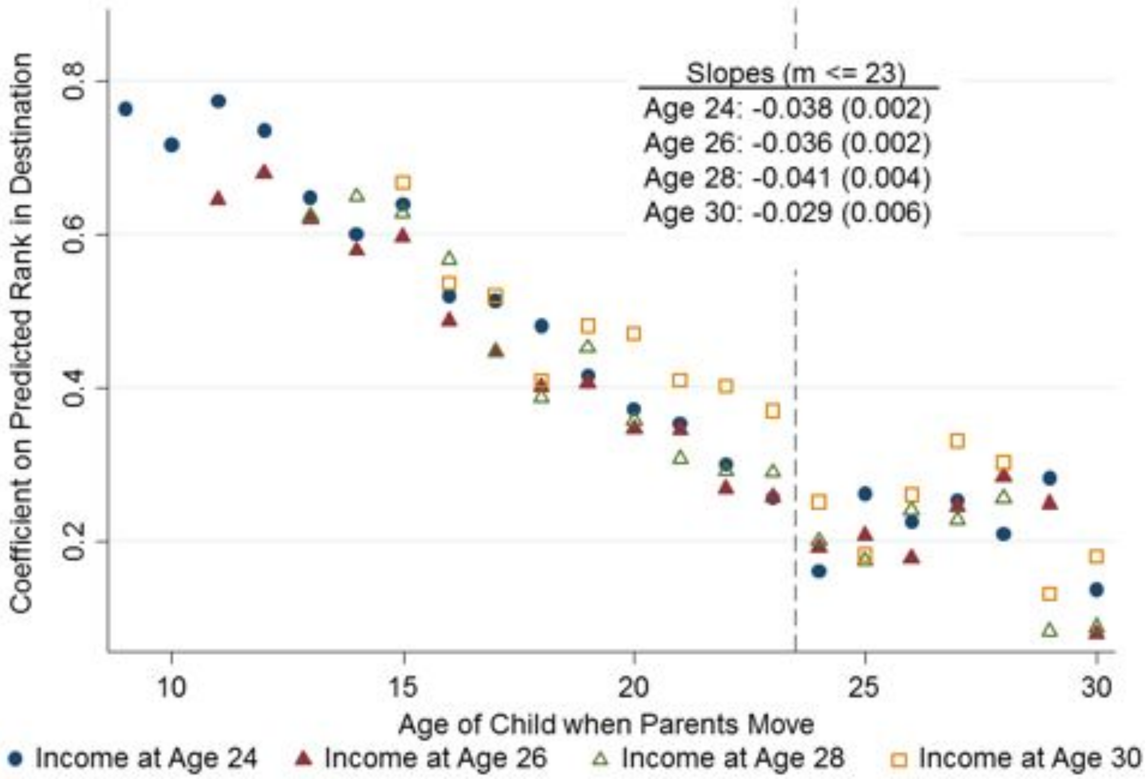
ONLINE APPENDIX FIGURE I

Map of Boston CZ

Notes: This figure presents a county map of the Boston commuting zone.

ONLINE APPENDIX FIGURE II

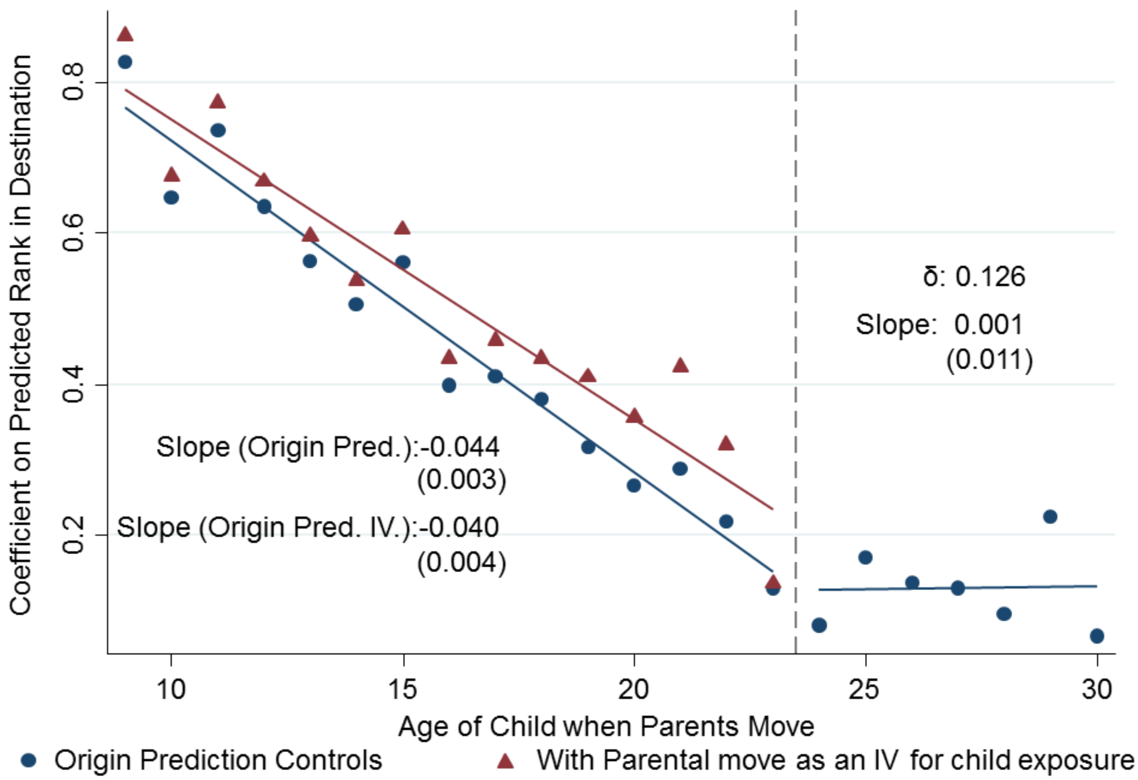
Exposure Effect Estimates at Age 24, 26, 28, and 30



Notes: This figure replicates our baseline specification in equation (8), shown in Figure IVb, using incomes measured at age 24, 26, 38, and 30. The figure presents estimates of b_m for the specification in equation (6) that includes origin by parent income decile by cohort by child age at move fixed effects. The figure reports the slopes from a regression of the b_m coefficients on m for $m \leq 23$, with standard errors in parentheses.

ONLINE APPENDIX FIGURE III

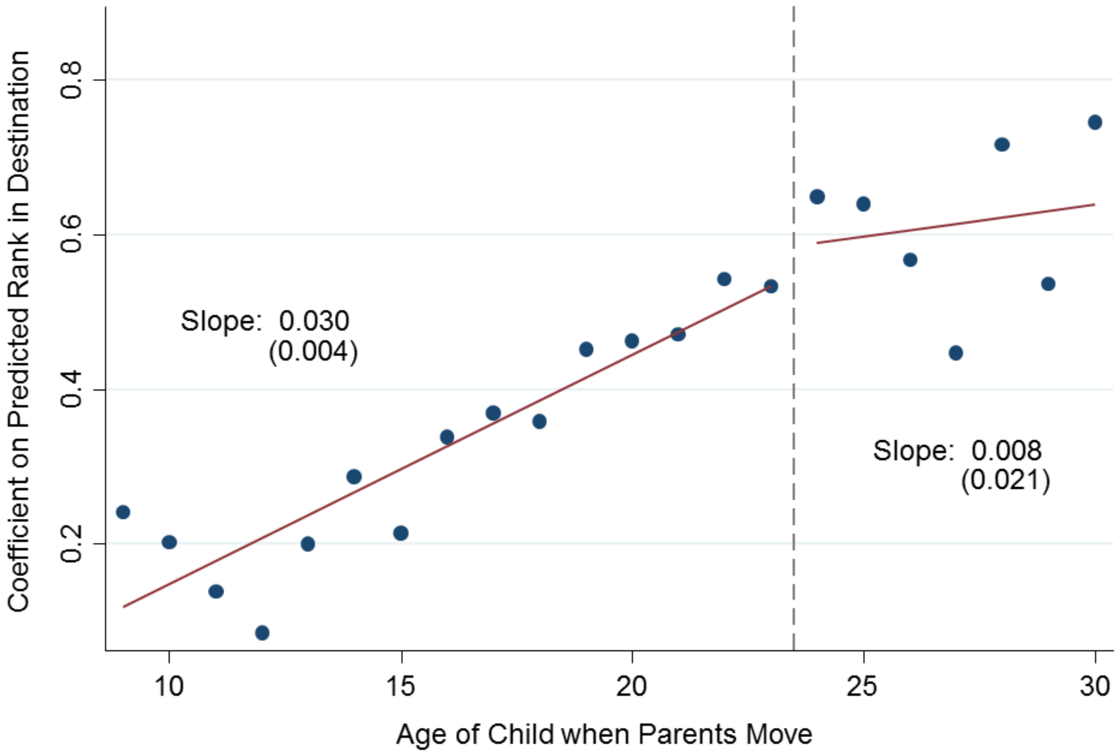
Exposure Effect Estimates using Parental Move as an Instrument for Child Exposure



Notes: This figure presents estimates of the coefficients b_m adjusted for the probability that the child follows the parent to the destination. Formally, we construct the fraction of children who follow their parents when the parents move when the child is m years old, ϕ_m, as the fraction of children who either (a) file a tax return in the destination, (b) have a form W-2 mailing address in the destination location, or (c) attend a college (based on 1098-T filings by institutions) in the destination location. The figure plots the series of $b_m^{IV} = \frac{b_m - \delta}{\phi_m} + \delta$, where $\delta = 0.125$ is the estimated selection effect shown in Figure IVa.

ONLINE APPENDIX FIGURE IV

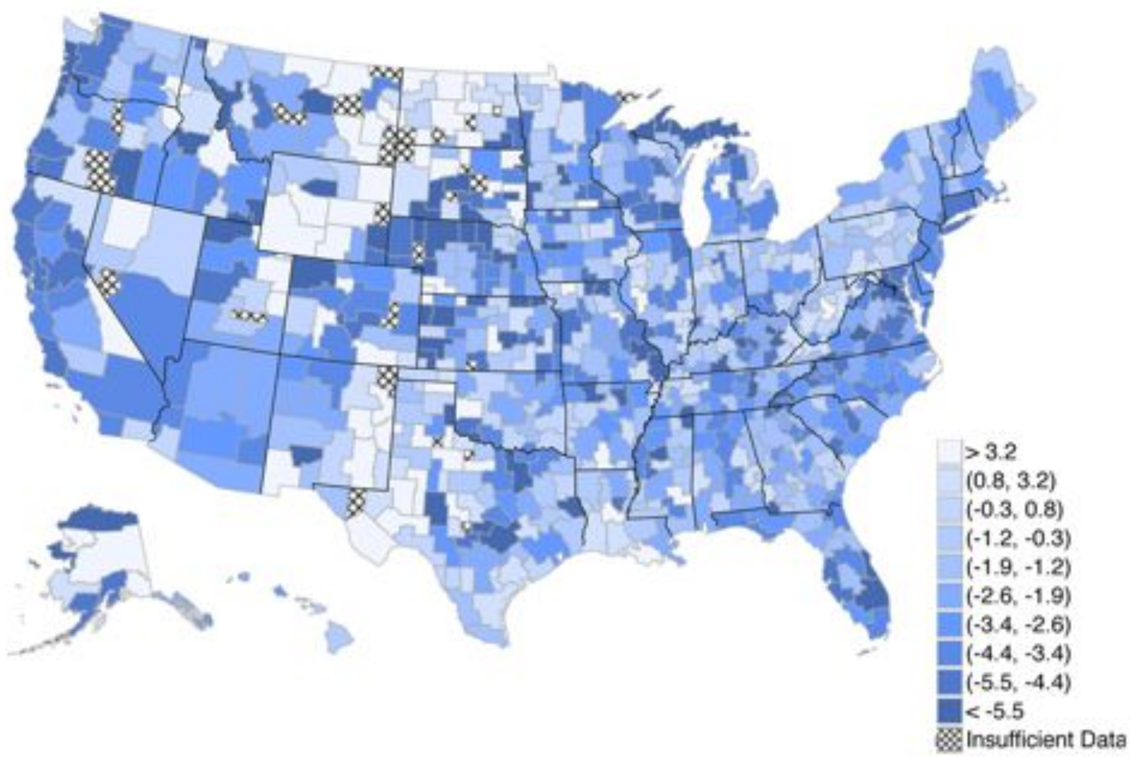
Exposure Effect Estimates using Origin Variation



Notes: This figure presents estimates of b_m^o from equation (8) separately for each age of the child at the time of the parental move, m (multipled by -1). Child income is measured at age 24. We use the same sample and specification as Figure IVa, but replace α_{qos} fixed effects with α_{qds} fixed effects and replace $\bar{y}_{pds}$ with $\bar{y}_{pos}$, so that the slope is identified from variation in the origin exposure. As in Figure IVa, the figure reports the estimated slopes from a regression on the dots on the figure.

ONLINE APPENDIX FIGURE V

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

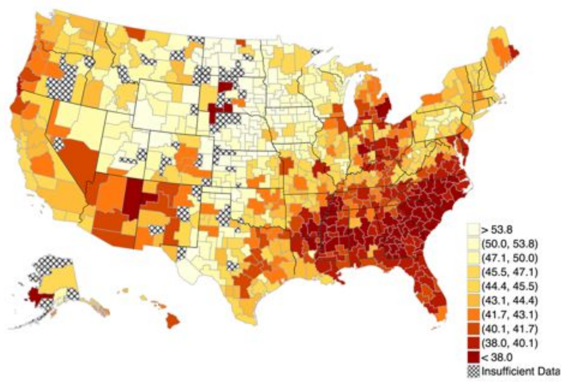


Notes: This figure presents estimates of the difference in male versus famale outcomes of permanent residents, $\bar{y}^{m}_{pcs} - \bar{y}^{f}_{pcs}$ by CZ, c, for income at age 24. To estimate $\bar{y}^{m}_{pcs}$ and $\bar{y}^{f}_{pcs}$, we estimate linear regressions of child rank on parent income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

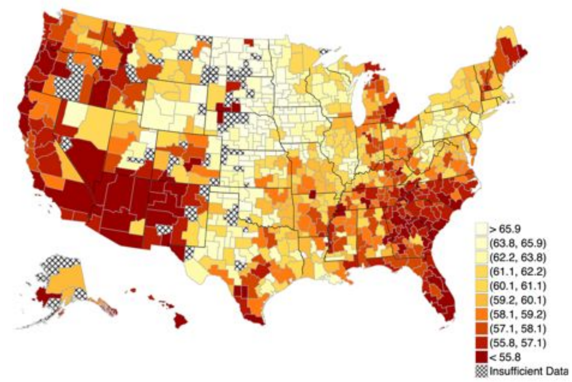
ONLINE APPENDIX FIGURE VI

Predicted Income Rank at Age 30 - Permanent Residents

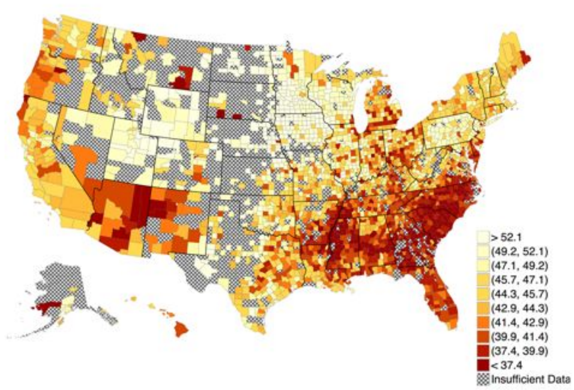
A. For Children with Parent at the 25^{th} Percentile (CZ)



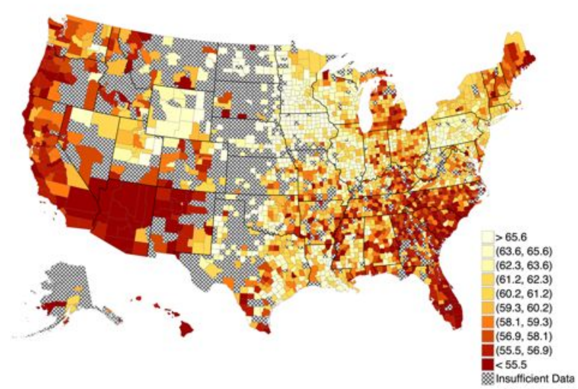
B. For Children with Parent at the 75^{th} Percentile (CZ)



C. For Children with Parent at the 25^{th} Percentile (County)

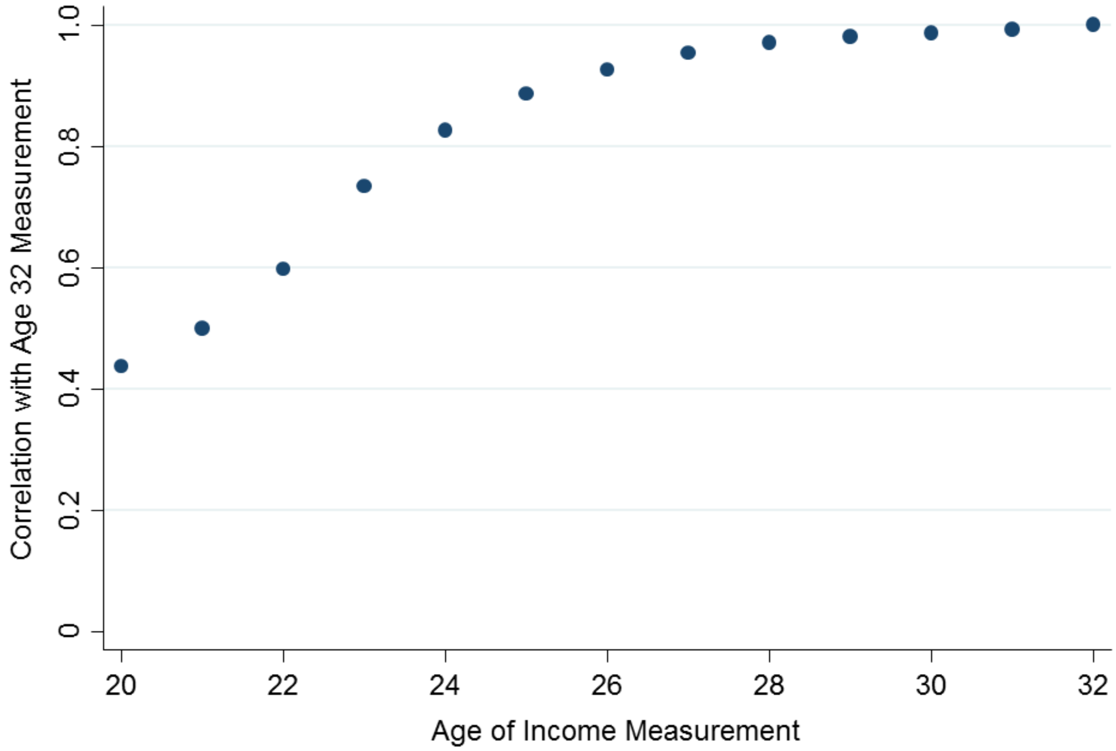


D. For Children with Parent at the 75^{th} Percentile (County)



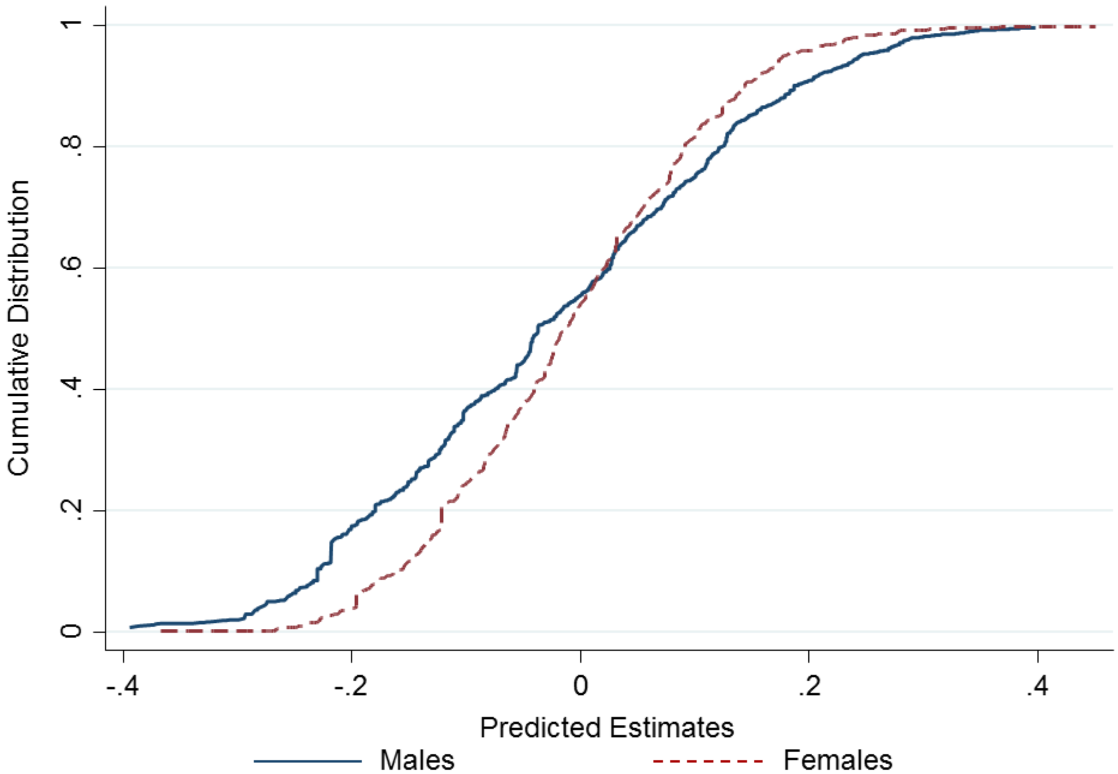
Notes: These figures present the estimated $\bar{y}_{pcs}$ by CZ and County for $p = 25$ and $p = 75$.

ONLINE APPENDIX FIGURE VII: Correlations of place effects by age (p25)



Notes: This figure presents the estimated correlation between $\bar{y}_{pc}$ across CZs when measured at age 32 with measurements at earlier ages (20-32). Correlations are weighted by CZ population in the 2000 Census. The vertical axis presents the estimated correlation; the horizontal axis corresponds to the varying age of income measurement.

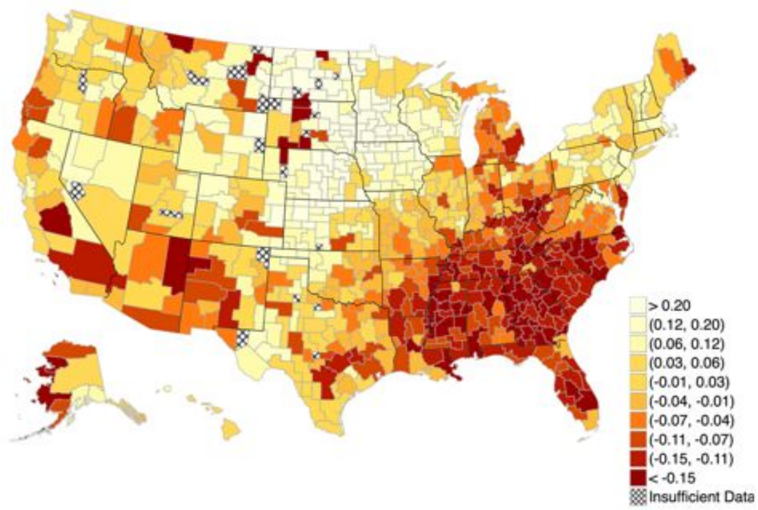
ONLINE APPENDIX FIGURE VIII: Distribution of Predicted Values by Gender



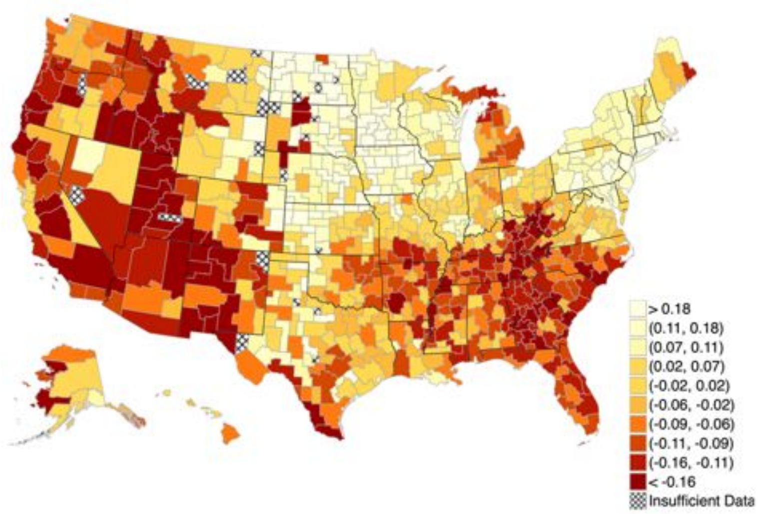
Notes: This figure presents the cumulative distribution of the gender-specific forecasts of county exposure effects for family income for children in below-median (p25) income families, $\mu^f_{25,c}$. The solid (blue) line presents the cumulative distribution for male forecasts. The dashed (red) line presents the cumulative distribution of the female forecasts.

ONLINE APPENDIX FIGURE IX: Predicted Estimates: National CZ - Using Individual Incomes

A. At 25th Percentile ($\mu_{25,c}$)



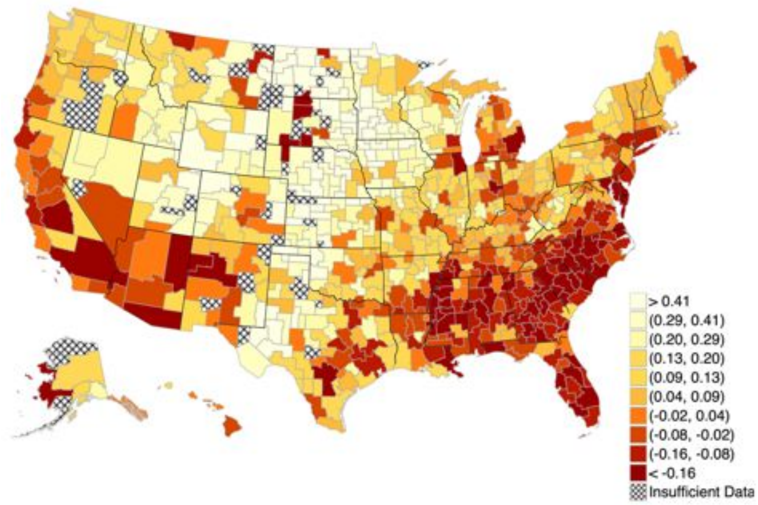
B. At 75th Percentile ($\mu_{75,c}$)



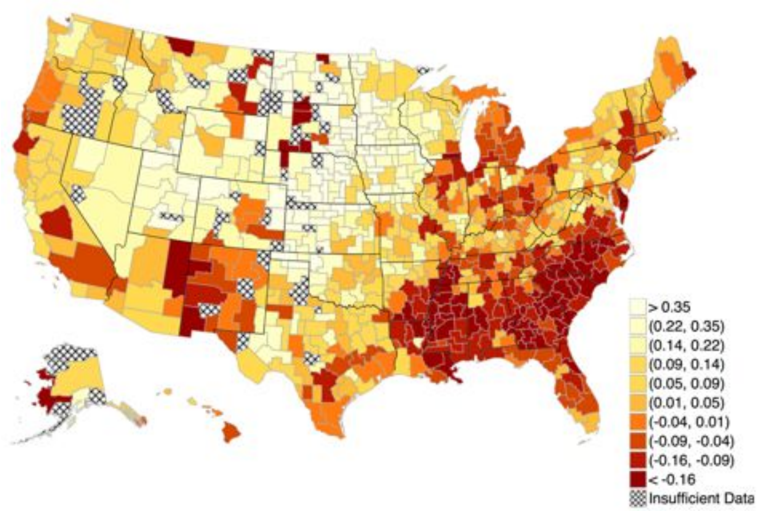
Notes: These figures present forecast estimates of each CZ's causal effects on individual income (as opposed to family income, shown in Figure XIII), μ^f_{pc}, for below-median ($p = 25$) and above-median ($p = 75$) income families. We estimate the fixed effects, $\hat{\mu}_{pc}$, and permanent resident outcomes, $\bar{y}_{pc}$, using the child's individual income at age 26. We then compute these forecasts using the methodology discussed in Section IX.A and, in particular, using the formula in Equation 21. For small-population CZs for which we do not have fixed effect estimates, we display the permanent resident outcomes (which corresponds to the natural assumption that $\hat{s}_{pc} = \infty$ in Equation 21 in the case when we have no fixed effect estimate).

ONLINE APPENDIX FIGURE X: Predicted Estimates: National CZ - Male and Female

A. Male ($\mu_{25,c}$)



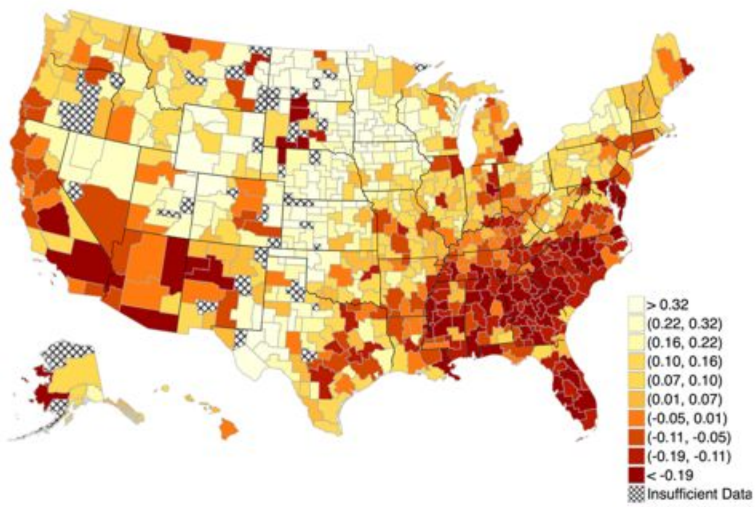
B. Female ($\mu_{25,c}$)



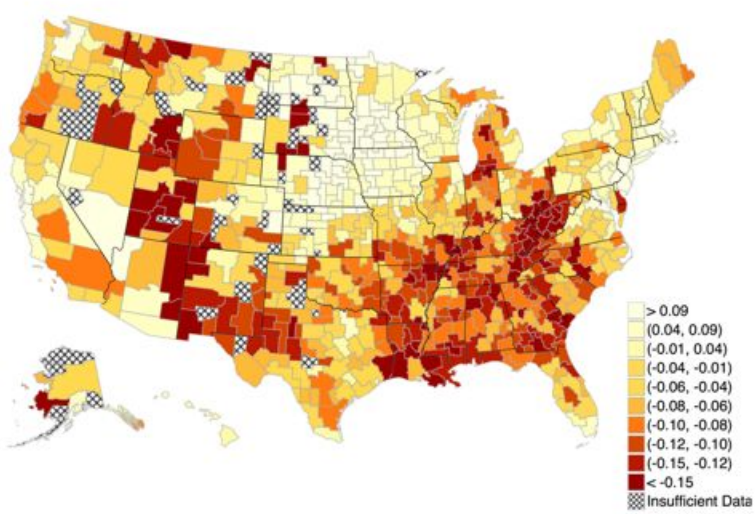
Notes: These figures present forecast estimates of each CZ's causal effects on family income for children in below-median ($p = 25$) families on separate samples of male (Panel A) and female (Panel B) children. We estimate the fixed effects, $\hat{\mu}_{pc}$, and permanent resident outcomes, $\bar{y}_{pc}$, using the child's family income at age 26 on separate gender samples. We then compute these forecasts using the methodology discussed in Section IX.A and, in particular, using the formula in Equation 21. For small-population CZs for which we do not have fixed effect estimates, we display the permanent resident outcomes (which corresponds to the natural assumption that $\hat{s}_{pc} = \infty$ in Equation 21 in the case when we have no fixed effect estimate).

ONLINE APPENDIX FIGURE XI: Predicted Estimates: National CZ - Male and Female - Using Individual Incomes

A. Male ($\mu_{25,c}$)



B. Female ($\mu_{25,c}$)



Notes: These figures present forecast estimates of each CZ's causal effects on individual income for children in below-median ($p = 25$) families on separate samples of male (Panel A) and female (Panel B) children. We estimate the fixed effects, $\hat{\mu}_{pc}$, and permanent resident outcomes, $\bar{y}_{pc}$, using the child's individual income at age 26 on separate gender samples. We then compute these forecasts using the methodology discussed in Section IX.A and, in particular, using the formula in Equation 21. For small-population CZs for which we do not have fixed effect estimates, we display the permanent resident outcomes (which corresponds to the natural assumption that $\hat{s}_{pc} = \infty$ in Equation 21 in the case when we have no fixed effect estimate).

TABLE I
Summary Statistics for CZ Permanent Residents and Movers

Variable	Mean (1)	Std. Dev. (2)	Median (3)	Sample Size (4)
Non-Movers				
Parent Income	79,802	310,537	52,800	44,175,313
Child family income at 24	24,853	130,276	19,700	22,933,771
Child family income at 26	33,706	149,981	26,200	17,592,224
Child family income at 30	48,377	129,801	35,400	7,239,831
Child individual earnings at 24	20,484	193,368	17,000	23,046,067
College attendence (18-23)	0.69	0.46	1.00	23,526,466
College quality (18-23)	31,306	13,138	30,900	23,526,466
Teen Birth (13-19)	0.11	0.31	0.00	16,829,532
Teen employment at age 16	0.28	0.45	0.00	43,950,854
Number of movers				
1 time	7,784,976			
2 times	4,725,843			
3 times	2,010,537			
4+ times	2,043,889			
Total	16,565,245			
1 time -3 times Movers				
Parent Income	71,422	285,880	44,100	14,521,356
Child family income at 24	23,484	62,130	18,200	6,810,190
Child family income at 26	31,249	90,855	23,700	5,127,832
Child family income at 30	44,812	133,057	32,200	2,059,365
Child individual earnings at 24	18,804	54,408	15,200	6,810,190
College attendence (18-23)	0.636	0.481	1.000	7,067,553
College quality (18-23)	29,386	12,537	28,700	7,067,553
Teen Birth (13-19)	0.137	0.344	0.000	5,225,131
Teen employment at age 16	0.268	0.443	0.000	14,521,356
One-time Movers				
Parent Income	85,271	316,143	48,500	3,418,710
Child family income at 24	23,867	56,564	18,700	1,553,021
Child family income at 26	32,419	108,431	24,300	1,160,278
Child family income at 30	47,882	117,450	33,200	460,457
Child individual earnings at 24	19,781	48,784	16,200	1,553,021
College attendence (18-23)	0.695	0.460	1.000	1,622,145
College quality (18-23)	31,332	13,430	30,600	1,622,145
Teen Birth (13-19)	0.109	0.311	0.000	1,212,352
Teen employment at age 16	0.257	0.437	0.000	3,418,710

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

TABLE II
Exposure Effect Estimates

Specification:	Baseline Spec.			Claimed Sample	No Cohort Controls	Origin Controls (Destination)	Child CZ Fixed Effects	Pooled moves				Individual Income
	Pooled	Age ≤ 23	Age ≤ 18					1st Destination	2nd Destination	3rd Destination	Constrained	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8a)	(8b)	(8c)	(9)	(10)
Exposure Slope	0.040	0.041	0.041	0.031	0.036	0.041	0.031	0.040	0.037	0.031	0.039	0.040
	(0.002)	(0.002)	(0.006)	(0.005)	(0.002)	(0.002)	(0.002)	(0.001)	(0.004)	(0.006)	(0.001)	(0.002)
Controls												
Cohort-Varying Intercept	X	X	X	X		X		X	X	X	X	X
Child age (m) x y_{ops} Interactions						X						
Child Income Definition	Family	Family	Family	Family	Family	Family	Family	Family	Family	Family	Family	Individual
Num of Obs.	1,553,021	1,287,773	687,323	604,602	1,553,021	1,553,021	1,473,218	4,374,418	4,374,418	4,374,418	4,374,418	1,553,021

Notes: Table II reports the coefficients on the child's age at the time of the parental move interacted with the difference in the predicted outcomes based on prior residents in the destination relative to the origin. Coefficients are multipled by -1 to correspond to exposure to destination. We allow separate lines allowed for child age <= 23 and child age > 23 at the time of the parental move. Column (1) reports the coefficient β in equation (9). Column (2) restricts the sample to those below age 23 at the time of the move. Column (3) restricts the sample to those below age 18. Column (4) further restricts to the sample of children who are claimed as a dependent on a 1040 in the destination CZ in the years subsequent to the move. Column (5) drops the cohort interactions with the predicted outcomes of permanent residents in the origin and destination location and instead includes one control for the predicted outcomes of those in the origin location. Column (6) adds controls for the child's age at move interacted with the predicted outcomes of those in the origin location to the baseline specification in column (1) and equation (9). Column (7) adds the child's CZ in adulthood (2012) as a fixed effect. Column (8a-c) present estimates for the exposure effect of the 1st, 2nd, and 3rd move using the sample of 1-3-time movers, as opposed to the 1-time movers sample. Column (9) presents the estimates of the exposure effect restricting the coefficient to be the same across each move. Column (10) presents the baseline specification (equation 9) using individual income for both the outcome and predicted outcomes in the origin and destination

TABLE III

Exposure Effect Estimates: Family Fixed Effects and Time-Varying Controls for Income and Marital Status

	Baseline Spec.			Family FE						
Specification:	Baseline	Origin Controls	No Cohort Controls	Baseline	Origin Controls	No Cohort Controls	Inc Controls	Inc/Mar. Controls	Multiple Moves	Individual Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exposure Slope	0.040	0.041	0.036	0.044	0.043	0.031	0.043	0.043	0.039	0.036
	(0.002)	(0.002)	(0.002)	(0.008)	(0.009)	(0.005)	(0.008)	(0.008)	(0.004)	(0.005)
Controls										
Cohort-Varying Intercept	X	X		X	X		X	X	X	X
Child age (m) x y_{ops} Interactions		X			X			X		
Family FE				X	X	X	X	X	X	X
Income and Marital Status Changes							X	X		
Child Income Definition	Family	Family	Family	Family	Family	Family	Family	Family	Family	Individual
Num of Obs.	1,553,021	1,553,021	1,553,021	1,553,021	1,553,021	1,553,021	1,553,021	1,553,021	4,374,418	1,553,021

Notes: This table presents estimates of the exposure effect estimated with the inclusion of family fixed effects and controls for changes in parental income and marital status around the time of the move. Columns (1) and (2) replicate the baseline specification in Table 2 for which β is identified using the pooled variation (Column 1) and the destination variation (Column 2), as outlined in equation (9). Column (3) presents the baseline estimates in equation (9) without the inclusion of cohort-specific controls (i.e. no cohort dummies or interactions of these dummies with the predicted outcomes based on prior residents in the origin or destination CZ). Column (4) adds family fixed effects to the specification in equation (9). Column (5) adds family fixed effects to the specification in equation (9) that also includes interactions of the child's age at the time of the parental move and the predicted outcomes based on the prior residents in the origin CZ. Column (5) takes the baseline specification in column (1) and adds family fixed effects and controls separately for each age of the child, fully interacted with cohort dummies (1980-1988). Column (6) adds family fixed effects to this specification in column (3) that does not include cohort-specific controls. Column (7) add family fixed effects and year- and cohort-specific controls for parental income for each age of the child and cohort over the range of our data (1996-2012). Column (8) takes the baseline specification in column (1) and adds both family fixed effects and controls for changes in marital status and income around the time of the parental move, along with their interaction with under-23 exposure time the child has in the destination CZ. We construct the parental income rank by cohort by year, and use this to construct the difference in the parental income rank in the year after the move relative to the year before the move. We include this measure of income change and a full set of its interaction with 23-m and an indicator for m>23. We also construct an indicator for the child's mother's marital status by year and construct 4 indicators for possible marital status changes (married -> married, married -> un-married, un-married -> married, un-married -> un-married). We then interact these four indicators with a full set of its interaction with 23-m and an indicator for m>23. Column (9) adds family fixed effects to the specification incorporating all movers (not just 1x movers) in Column (8) of Table 2. Finally, Column (10) illustrates the robustness of the family fixed effects results to individual income as the outcome, as opposed to family income. This column presents the exposure slope in the specification in column (10) of Table 2 with the addition of family fixed effects.

TABLE IV
Distributional Convergence

	Child Rank in top 10%			Child Employed		
	(1)	(2)	(3)	(4)	(5)	(6)
Distributional Prediction	0.043		0.040	0.046		0.045
	(0.002)		(0.003)	(0.003)		(0.004)
Mean Rank Prediction (Placebo)		0.022	0.004		0.021	0.000
		(0.002)	(0.003)		(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: Table presents estimates of the exposure time relationships for the outcome of being in the top 10% of the cohort-specific income distribution at age 24 and being employed. We define employment as an indicator for filing a W-2 at some point during the year in which the child is age 24. Analogous to these outcomes, we construct predicted outcomes using permenent residents each CZ. Column (1) presents the estimated exposure time slope using top 10% indicator as the dependent variable and predicted outcomes based on permanent residents in the origin and destination CZ. Column (2) continues to use the indicator of being in the top 10% as the dependent variable, but uses the mean rank predictions from the baseline regressions as the origin and destination predictions. Column (3) combines all variables in specifications (1) and (2). Column (4) presents the estimated exposure time slope using an indicator of being employed as the dependent variable and predicted outcomes based on permanent residents in the origin and destination CZ. Column (5) retains the employment indicator as the dependent variable but replaces the predicted outcomes in the origin and destination with the mean rank predictions from the baseline regressions. Column (6) combines all variables in specifications (4) and (5).

TABLE V
Gender Placebos

	No Family Fixed Effects			Family Fixed Effects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Own Gender Prediction	0.038 (0.002)		0.031 (0.003)	0.031 (0.006)		0.027 (0.006)	0.0308 (0.007)
Other Gender Prediction (Placebo)		0.034 (0.002)	0.009 (0.003)		0.017 (0.006)	0.017 (0.006)	0.0116 (0.007)
Family Fixed Effects				X	X	X	X
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	490964

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

Table VI
County Exposure Effect Estimates

Specification:	Baseline Spec.		Within CZ Moves				
	Baseline	Family FE	Age 24	Age 26	Age ≥ 24	Family FE	Small CZs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exposure Slope	0.035	0.033	0.022	0.032	0.027	0.029	0.024
	(0.003)	(0.011)	(0.003)	(0.004)	(0.003)	(0.025)	(0.002)
Num of Obs.	654,491	654,491	617,502	457,140	2,900,311	2,900,311	7,311,431

Notes: Table II reports exposure effect coefficients in equation (9), analogous to those presented in Tables II and III, using county-level predictions for the sample of 1-time county movers. Column (1) presents the baseline specification analogous to Column (1) of Table 2, replacing CZ-level predictions with county-level predictions based on prior residents. We restrict the sample to moves of at least 100 miles and require the county-level population to be at least 250,000 in the origin and destination county. Column (2) adds family fixed effects to the specification in Column (1). Columns (3)-(7) drop the distance restriction and consider the set of within-CZ county moves (between counties with populations of at least 250,000). Column (3) replicates the baseline specification. Column (4) replicates the baseline specification using income at age 26 as the outcome, analogous to the outcomes considered in Section V. Column (5) presents the pooled estimate that stacks all outcomes for ages 24 and above (multiple observations per person). Column (6) adds family-by-age of outcome fixed effects to the specification in Column (5). Column (7) expands the sample in Column (5) to include moves between all CZs with populations above 10,000).

Table VII
Model Variance Components: Causal and Selection Effects

	Commuting Zones		Counties		County within CZ	
	Below Median (p25)	Above Median (p75)	Below Median (p25)	Above Median (p75)	Below Median (p25)	Above Median (p75)
Model Component	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Exposure Effect Estimates						
Signal vs. Noise (per year of exposure)						
Raw (per year) Exposure Effect (SD)	0.248	0.243	0.434	0.435	0.357	0.361
Noise (SD)	0.210	0.218	0.402	0.407	0.343	0.344
Signal of Exposure Effects (SD)	0.132	0.107	0.165	0.155	0.099	0.112
Signal to Noise Ratio	0.398	0.241	0.170	0.144	0.084	0.106
Correlation between p25 and p75 Exposure Effects	0.724		0.287		0.080	
Panel B: Model Variance Components						
Sorting vs. Causal Components (T_C=20 yrs)						
Causal Effect (SD of Signal)	2.647	2.139	3.308	3.092	1.984	2.233
Permanent Residents (SD)	3.259	2.585	4.203	3.257	2.653	1.982
Sorting Component (SD)	1.960	1.097	3.033	3.203	2.315	3.009
Correlation between Sorting and Causal Effect	-0.021	0.193	-0.123	-0.465	-0.246	-0.753

Notes: This table presents the estimated variance components of the fixed effects model in equation (16). Panel A presents the estimates of the raw variance of the estimates. The first row presents the raw standard deviation across CZs, weighting by precision (1/SE, where SE is the estimated standard error of the estimate). The second row presents the estimated standard deviation of the sampling noise (again weighted by precision, 1/SE). The third row presents the estimated signal standard deviation, computed using the formula Signal_Variance = Total Variance - Noise Variance. The fourth row presents the signal to noise ratio (=Signal Variance / Noise Variance). The last row of panel A presents the correlation between the 25th and 75th percentile estimates. To construct this correlation, we compute the covariance using a split sample of above-median and below-median samples to estimate the p75 and p25 estimates, respectively, to avoid mechanical correlations, and then divide by the standard deviations of the p25 and p75 place effects (estimated on these split samples) to arrive at an estimate of the correlation. Panel B presents the model variance components. The first row presents the standard deviation of the causal effects (=20*signal of exposure effects). The second row presents the standard deviation of the permanent resident outcomes (precision weighted). The third row presents the standard deviation of the sorting component (precision weighted). See the text for details on computing this standard deviation. The fourth row presents the estimated correlation between the sorting and causal effect across CZs. The columns present the estimates on various samples. Columns (1)-(2) present the estimates for below-median and above-median income families across Commuting Zones; Columns (3)-(4) present the estimates across counties. Columns (5)-(6) present the implied estimates for counties within CZs. For example, we compute the standard deviations using the identity: var(county_within_cz) = var(county) - var(cz).

Table VIII
Predicted Place Effects for 50 Largest CZs

Commuting Zone	State	Below-Median Income Parents (p25): Family Income Rank: Prediction (1)	RMSE (2)	Scaling: $ Increase (3)	% Increase (4)	Above-Median Income Parents (p75): Family Income Rank: Prediction (5)	RMSE (6)	Scaling: $ Increase (7)	% Increase (8)	Row Number
Salt Lake City	UT	0.166	0.066	135.9	0.521	0.105	0.041	88.4	0.218	(1)
Seattle	WA	0.140	0.059	114.3	0.438	-0.009	0.038	-7.3	-0.018	(2)
Washington DC	DC	0.105	0.051	85.8	0.329	0.062	0.034	51.7	0.127	(3)
Minneapolis	MN	0.103	0.065	84.1	0.322	0.077	0.041	65.0	0.160	(4)
Fort Worth	TX	0.057	0.061	46.6	0.178	0.049	0.039	41.3	0.102	(5)
San Diego	CA	0.056	0.054	46.1	0.177	-0.131	0.038	-110.0	-0.271	(6)
Boston	MA	0.055	0.061	45.3	0.174	0.033	0.040	27.7	0.068	(7)
Manchester	NH	0.051	0.070	41.8	0.160	0.025	0.041	20.7	0.051	(8)
San Jose	CA	0.048	0.065	39.1	0.150	-0.118	0.039	-99.2	-0.244	(9)
Las Vegas	NV	0.043	0.057	35.0	0.134	-0.078	0.039	-65.6	-0.162	(10)
Denver	CO	0.042	0.065	34.0	0.130	-0.060	0.038	-50.5	-0.124	(11)
Portland	OR	0.038	0.067	31.0	0.119	-0.091	0.041	-76.4	-0.188	(12)
San Francisco	CA	0.029	0.060	23.4	0.090	-0.119	0.037	-99.6	-0.245	(13)
Pittsburgh	PA	0.013	0.065	10.8	0.041	0.104	0.041	87.6	0.216	(14)
Newark	NJ	0.012	0.051	9.5	0.036	0.057	0.034	48.2	0.119	(15)
Providence	RI	0.007	0.067	5.7	0.022	0.022	0.042	18.4	0.045	(16)
Sacramento	CA	0.006	0.058	4.6	0.018	-0.144	0.038	-120.6	-0.297	(17)
Phoenix	AZ	0.004	0.049	3.1	0.012	-0.018	0.038	-15.1	-0.037	(18)
Buffalo	NY	-0.003	0.067	-2.2	-0.009	0.010	0.041	8.6	0.021	(19)
Kansas City	MO	-0.007	0.067	-5.4	-0.021	0.020	0.042	16.7	0.041	(20)
Houston	TX	-0.025	0.050	-20.7	-0.079	0.006	0.036	5.3	0.013	(21)
Miami	FL	-0.026	0.044	-20.9	-0.080	-0.201	0.039	-169.0	-0.416	(22)
Philadelphia	PA	-0.029	0.057	-23.5	-0.090	0.005	0.037	3.9	0.010	(23)
Grand Rapids	MI	-0.031	0.070	-25.7	-0.098	0.066	0.043	55.6	0.137	(24)
Dallas	TX	-0.038	0.055	-30.8	-0.118	-0.009	0.036	-7.8	-0.019	(25)
Cleveland	OH	-0.042	0.062	-34.7	-0.133	-0.025	0.041	-21.1	-0.052	(26)
Bridgeport	CT	-0.045	0.059	-37.2	-0.143	0.028	0.038	23.6	0.058	(27)
Jacksonville	FL	-0.048	0.061	-39.0	-0.149	-0.071	0.042	-59.6	-0.147	(28)
Milwaukee	WI	-0.048	0.067	-39.3	-0.150	0.044	0.042	37.1	0.091	(29)
Dayton	OH	-0.062	0.071	-51.1	-0.196	0.015	0.043	12.9	0.032	(30)
Cincinnati	OH	-0.082	0.069	-67.3	-0.258	0.063	0.041	53.1	0.131	(31)
Columbus	OH	-0.086	0.068	-70.7	-0.271	0.006	0.042	5.3	0.013	(32)
Nashville	TN	-0.087	0.070	-71.4	-0.274	-0.027	0.042	-22.6	-0.056	(33)
St. Louis	MO	-0.090	0.067	-73.7	-0.282	0.029	0.041	24.6	0.061	(34)
Austin	TX	-0.097	0.066	-79.6	-0.305	-0.098	0.040	-82.6	-0.203	(35)
Baltimore	MD	-0.103	0.066	-84.1	-0.322	0.067	0.039	56.4	0.139	(36)
San Antonio	TX	-0.110	0.063	-90.1	-0.345	-0.078	0.040	-65.2	-0.160	(37)
Tampa	FL	-0.114	0.048	-92.8	-0.356	-0.128	0.040	-107.8	-0.265	(38)
New York	NY	-0.117	0.039	-95.5	-0.366	-0.032	0.035	-26.7	-0.066	(39)
Indianapolis	IN	-0.118	0.070	-96.9	-0.371	-0.019	0.041	-16.3	-0.040	(40)
Atlanta	GA	-0.124	0.043	-101.3	-0.388	-0.094	0.036	-78.7	-0.194	(41)
Los Angeles	CA	-0.130	0.038	-105.9	-0.406	-0.226	0.032	-189.4	-0.466	(42)
Detroit	MI	-0.136	0.054	-111.0	-0.425	-0.125	0.039	-105.3	-0.259	(43)
Orlando	FL	-0.136	0.054	-111.3	-0.427	-0.137	0.040	-115.1	-0.284	(44)
Chicago	IL	-0.154	0.048	-126.2	-0.484	-0.035	0.033	-29.1	-0.072	(45)
Fresno	CA	-0.164	0.062	-134.3	-0.515	-0.120	0.042	-100.6	-0.248	(46)
Port St. Lucie	FL	-0.174	0.057	-142.6	-0.547	-0.198	0.040	-166.7	-0.410	(47)
Raleigh	NC	-0.195	0.065	-159.3	-0.610	-0.114	0.041	-96.0	-0.236	(48)
Charlotte	NC	-0.205	0.061	-167.6	-0.642	-0.084	0.040	-70.7	-0.174	(49)
New Orleans	LA	-0.214	0.065	-175.3	-0.672	-0.060	0.042	-50.1	-0.123	(50)

Notes: Table presents per-year exposure predictions for the 50 largest CZs using the estimation strategy discussed in Section VIII. Column (1) reports the predictions for the child's family income rank at age 26. Column (2) reports the root mean square error for this prediction, computed as the square root of 1/(1/v_r + 1/v)) where v_r is the residual signal variance and v is the squared standard error of the fixed effect estimate. Column (3) scales the numbers to dollars by multiplying the estimates in column (1) by 818, the coefficient obtained by regressing the permanent resident outcomes at p25 for child family income at age 26 on the analogous outcomes for child rank at age 26. Column (4) divides the income impacts in column (3) by the mean income of children from below-median (p25) income families of $26,090. Columns (5)-(8) report the analogous statistics for above-median income families. Column (5) reports the prediction for the child's family income rank at age 26; column (6) reports the root mean square error. Column (7) scales the numbers in Column (1) by 2.068, the coefficient obtained by regressing the permanent resident outcomes at p25 for child family income at age 26 on the analogous outcomes for child rank at age 26. Column (8) divides the income impacts on column (5) by the mean income of children from above-median (p75) income families of 40,601.

Table IX
Predicted Place Effects for 100 Largest Counties (Top and Bottom 25)

		Below-Median Income Parents (p25)				Above-Median Income Parents (p75)				
		Family Income Rank		Scaling		Family Income Rank		Scaling		Row Number
		Prediction	RMSE	$ Increase	% Increase	Prediction	RMSE	$ Increase	% Increase	
County	State	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Dupage	IL	0.255	0.090	208.8	0.800	0.076	0.077	63.8	0.157	(1)
Fairfax	VA	0.239	0.100	195.5	0.749	0.265	0.096	222.5	0.548	(2)
Snohomish	WA	0.224	0.099	182.9	0.701	0.058	0.094	48.9	0.120	(3)
Bergen	NJ	0.220	0.102	179.7	0.689	0.152	0.099	127.7	0.315	(4)
Bucks	PA	0.198	0.101	161.6	0.620	-0.023	0.098	-19.3	-0.047	(5)
Norfolk	MA	0.183	0.101	149.6	0.573	0.151	0.099	126.5	0.312	(6)
Montgomery	PA	0.155	0.096	127.0	0.487	0.072	0.092	60.5	0.149	(7)
Montgomery	MD	0.151	0.099	123.5	0.473	0.003	0.098	2.2	0.005	(8)
King	WA	0.149	0.084	121.8	0.467	0.077	0.076	64.8	0.160	(9)
Middlesex	NJ	0.146	0.102	119.1	0.456	0.013	0.101	11.2	0.027	(10)
Contra Costa	CA	0.141	0.095	115.2	0.442	-0.069	0.091	-58.3	-0.144	(11)
Middlesex	MA	0.123	0.091	100.6	0.386	0.013	0.089	11.0	0.027	(12)
Macomb	MI	0.111	0.088	91.1	0.349	0.028	0.091	23.1	0.057	(13)
Salt Lake	UT	0.099	0.095	80.7	0.309	0.016	0.093	13.8	0.034	(14)
Ventura	CA	0.099	0.100	80.6	0.309	-0.055	0.093	-46.0	-0.113	(15)
San Mateo	CA	0.085	0.102	69.2	0.265	-0.035	0.102	-29.7	-0.073	(16)
Worcester	MA	0.075	0.107	61.4	0.235	0.130	0.107	109.3	0.269	(17)
Monmouth	NJ	0.075	0.103	61.2	0.235	0.073	0.096	61.7	0.152	(18)
Honolulu	HI	0.073	0.100	59.9	0.230	-0.130	0.113	-109.2	-0.269	(19)
Hudson	NJ	0.066	0.101	54.4	0.208	0.161	0.110	135.5	0.334	(20)
Kern	CA	0.062	0.086	50.4	0.193	-0.059	0.110	-49.9	-0.123	(21)
Clark	NV	0.059	0.074	48.3	0.185	-0.046	0.087	-38.9	-0.096	(22)
San Diego	CA	0.058	0.063	47.8	0.183	-0.136	0.064	-114.4	-0.282	(23)
Providence	RI	0.048	0.101	39.2	0.150	-0.043	0.108	-35.8	-0.088	(24)
San Francisco	CA	0.045	0.100	37.1	0.142	-0.183	0.104	-154.0	-0.379	(25)
Jefferson	KY	-0.137	0.105	-112.3	-0.431	0.022	0.111	18.5	0.046	(75)
Franklin	OH	-0.137	0.092	-112.4	-0.431	0.114	0.096	95.9	0.236	(76)
San Bernardino	CA	-0.140	0.062	-114.5	-0.439	-0.245	0.073	-205.9	-0.507	(77)
Davidson	TN	-0.141	0.098	-115.6	-0.443	-0.036	0.105	-29.8	-0.073	(78)
Pima	AZ	-0.142	0.083	-116.5	-0.446	-0.139	0.099	-116.7	-0.287	(79)
Montgomery	OH	-0.142	0.104	-116.5	-0.447	-0.016	0.116	-13.2	-0.032	(80)
Travis	TX	-0.147	0.089	-120.2	-0.461	-0.159	0.087	-133.6	-0.329	(81)
Essex	NJ	-0.147	0.096	-120.5	-0.462	0.074	0.098	61.8	0.152	(82)
Bexar	TX	-0.152	0.090	-124.7	-0.478	-0.092	0.122	-77.4	-0.191	(83)
Milwaukee	WI	-0.158	0.096	-129.4	-0.496	-0.027	0.097	-22.4	-0.055	(84)
Riverside	CA	-0.161	0.067	-131.6	-0.505	-0.248	0.075	-208.3	-0.513	(85)
Los Angeles	CA	-0.164	0.045	-134.1	-0.514	-0.254	0.049	-212.9	-0.524	(86)
Wake	NC	-0.171	0.101	-139.8	-0.536	-0.094	0.102	-79.1	-0.195	(87)
New York	NY	-0.173	0.076	-141.5	-0.542	-0.275	0.100	-230.7	-0.568	(88)
Fulton	GA	-0.173	0.077	-141.6	-0.543	0.024	0.083	19.9	0.049	(89)
Bronx	NY	-0.174	0.076	-142.0	-0.544	-0.201	0.107	-169.1	-0.416	(90)
Wayne	MI	-0.182	0.077	-148.6	-0.570	-0.073	0.079	-61.5	-0.152	(91)
Orange	FL	-0.193	0.077	-157.9	-0.605	-0.093	0.092	-77.9	-0.192	(92)
Cook	IL	-0.204	0.060	-166.9	-0.640	-0.030	0.051	-24.9	-0.061	(93)
Palm Beach	FL	-0.208	0.084	-169.8	-0.651	-0.314	0.097	-263.9	-0.650	(94)
Marion	IN	-0.209	0.097	-170.8	-0.655	-0.102	0.091	-85.4	-0.210	(95)
Shelby	TN	-0.210	0.093	-171.5	-0.657	0.030	0.103	25.2	0.062	(96)
Fresno	CA	-0.215	0.089	-176.1	-0.675	-0.051	0.110	-42.4	-0.105	(97)
Hillsborough	FL	-0.220	0.088	-180.3	-0.691	-0.192	0.102	-161.4	-0.397	(98)
Baltimore City	MD	-0.223	0.092	-182.4	-0.699	-0.017	0.097	-14.6	-0.036	(99)
Mecklenburg	NC	-0.231	0.095	-188.6	-0.723	-0.090	0.100	-75.5	-0.186	(100)

Notes: Table presents per-year exposure predictions for the top 25 and bottom 25 largest counties using the estimation strategy discussed in Section VIII, sorted by the impact on family income rank for children in below-median (p25) income families. Column (1) reports the predictions for the child's family income rank at age 26. Column (2) reports the root mean square error for this prediction, computed as the square root of 1/(1/v_r + 1/v)) where v_r is the residual signal variance and v is the squared standard error of the fixed effect estimate. Column (3) scales the numbers to dollars by multiplying by the estimates in column (1) by 3.13, the coefficient obtained by regressing the permanent resident outcomes at p25 for child family income at age 26 on the analogous outcomes for child rank at age 26. Column (4) divides the income impacts in column (3) by the mean income of children from below-median (p25) income families of $26,090. Columns (5)-(8) report the analogous statistics for above-median income families. Column (5) reports the prediction for the child's family income rank at age 26; column (6) reports the root mean square error. Column (7) scales the numbers in Column (1) by 2.068, the coefficient obtained by regressing the permanent resident outcomes at p25 for child family income at age 26 on the analogous outcomes for child rank at age 26. Column (8) divides the income impacts on column (5) by the mean income of children from above-median (p75) income families of 40,601.

Table X
Predicted Place Effects for 50 Largest CZs for Below-Median Income Parents (p25)

Commuting Zone	State	Male Family Income: Prediction (1)	RMSE (2)	% Increase (3)	Female Family Income: Prediction (4)	RMSE (5)	% Increase (6)	Pooled Spec: Prediction (7)	RMSE (8)	% Increase (9)	Average: Prediction (10)	RMSE (11)	% Increase (12)	Row Number
Seattle	WA	0.154	0.101	0.457	0.217	0.087	0.711	0.140	0.059	0.438	0.185	0.067	0.581	(1)
Minneapolis	MN	0.155	0.130	0.461	0.154	0.101	0.503	0.103	0.065	0.322	0.154	0.082	0.484	(2)
Salt Lake City	UT	0.060	0.131	0.178	0.234	0.105	0.767	0.166	0.066	0.521	0.147	0.084	0.461	(3)
Washington DC	DC	0.078	0.097	0.233	0.108	0.081	0.353	0.105	0.051	0.329	0.093	0.063	0.292	(4)
Portland	OR	0.127	0.124	0.379	0.040	0.100	0.131	0.038	0.067	0.119	0.084	0.079	0.262	(5)
Fort Worth	TX	0.097	0.109	0.290	0.021	0.090	0.069	0.057	0.061	0.178	0.059	0.071	0.186	(6)
Las Vegas	NV	-0.029	0.091	-0.087	0.147	0.078	0.482	0.043	0.057	0.134	0.059	0.060	0.185	(7)
San Diego	CA	0.019	0.098	0.056	0.087	0.084	0.286	0.056	0.054	0.177	0.053	0.064	0.167	(8)
San Francisco	CA	-0.005	0.101	-0.014	0.086	0.085	0.281	0.029	0.060	0.090	0.041	0.066	0.127	(9)
Pittsburgh	PA	-0.002	0.132	-0.005	0.070	0.102	0.230	0.013	0.065	0.041	0.034	0.084	0.107	(10)
Boston	MA	0.055	0.106	0.163	0.012	0.089	0.039	0.055	0.061	0.174	0.033	0.069	0.105	(11)
San Jose	CA	-0.127	0.114	-0.378	0.189	0.093	0.618	0.048	0.065	0.150	0.031	0.073	0.096	(12)
Manchester	NH	0.063	0.137	0.187	-0.011	0.106	-0.036	0.051	0.070	0.160	0.026	0.086	0.081	(13)
Denver	CO	0.035	0.116	0.104	0.008	0.095	0.026	0.042	0.065	0.130	0.021	0.075	0.067	(14)
Phoenix	AZ	-0.054	0.084	-0.161	0.076	0.075	0.250	0.004	0.049	0.012	0.011	0.056	0.035	(15)
Cleveland	OH	0.096	0.121	0.284	-0.078	0.099	-0.256	-0.042	0.062	-0.133	0.009	0.078	0.027	(16)
Sacramento	CA	-0.076	0.100	-0.227	0.069	0.085	0.228	0.006	0.058	0.018	-0.003	0.066	-0.011	(17)
Providence	RI	-0.001	0.131	-0.004	-0.007	0.103	-0.023	0.007	0.067	0.022	-0.004	0.083	-0.013	(18)
Newark	NJ	0.039	0.084	0.116	-0.048	0.072	-0.158	0.012	0.051	0.036	-0.004	0.056	-0.014	(19)
Buffalo	NY	-0.008	0.124	-0.024	-0.007	0.099	-0.022	-0.003	0.067	-0.009	-0.007	0.079	-0.023	(20)
Grand Rapids	MI	0.003	0.144	0.009	-0.049	0.109	-0.161	-0.031	0.070	-0.098	-0.023	0.090	-0.072	(21)
Kansas City	MO	-0.042	0.135	-0.125	-0.013	0.104	-0.041	-0.007	0.067	-0.021	-0.027	0.085	-0.086	(22)
Columbus	OH	0.060	0.132	0.178	-0.118	0.102	-0.387	-0.086	0.068	-0.271	-0.029	0.084	-0.092	(23)
Philadelphia	PA	-0.088	0.090	-0.260	0.024	0.078	0.080	-0.029	0.057	-0.090	-0.032	0.060	-0.099	(24)
Cincinnati	OH	-0.002	0.135	-0.007	-0.071	0.104	-0.234	-0.082	0.069	-0.258	-0.037	0.085	-0.116	(25)
Jacksonville	FL	0.032	0.118	0.094	-0.114	0.095	-0.374	-0.048	0.061	-0.149	-0.041	0.076	-0.129	(26)
Dallas	TX	-0.146	0.095	-0.434	0.060	0.079	0.197	-0.038	0.055	-0.118	-0.043	0.062	-0.135	(27)
Miami	FL	-0.103	0.083	-0.306	0.014	0.073	0.046	-0.026	0.044	-0.080	-0.044	0.055	-0.139	(28)
Houston	TX	-0.094	0.090	-0.279	0.005	0.076	0.016	-0.025	0.050	-0.079	-0.045	0.059	-0.140	(29)
Dayton	OH	-0.073	0.145	-0.217	-0.045	0.109	-0.146	-0.062	0.071	-0.196	-0.059	0.091	-0.184	(30)
Austin	TX	-0.073	0.125	-0.217	-0.064	0.100	-0.210	-0.097	0.066	-0.305	-0.069	0.080	-0.215	(31)
Bridgeport	CT	-0.114	0.109	-0.339	-0.032	0.090	-0.106	-0.045	0.059	-0.143	-0.073	0.071	-0.230	(32)
St. Louis	MO	-0.061	0.132	-0.182	-0.100	0.102	-0.327	-0.090	0.067	-0.282	-0.080	0.083	-0.252	(33)
Milwaukee	WI	-0.114	0.135	-0.339	-0.059	0.105	-0.194	-0.048	0.067	-0.150	-0.087	0.086	-0.272	(34)
Nashville	TN	-0.057	0.139	-0.170	-0.118	0.105	-0.386	-0.087	0.070	-0.274	-0.087	0.087	-0.274	(35)
Indianapolis	IN	-0.052	0.135	-0.154	-0.159	0.104	-0.522	-0.118	0.070	-0.371	-0.106	0.085	-0.331	(36)
Tampa	FL	-0.169	0.089	-0.501	-0.067	0.077	-0.218	-0.114	0.048	-0.356	-0.118	0.059	-0.369	(37)
Atlanta	GA	-0.132	0.075	-0.393	-0.125	0.065	-0.410	-0.124	0.043	-0.388	-0.129	0.050	-0.404	(38)
Baltimore	MD	-0.240	0.114	-0.714	-0.022	0.094	-0.071	-0.103	0.066	-0.322	-0.131	0.074	-0.410	(39)
New York	NY	-0.137	0.065	-0.409	-0.151	0.059	-0.493	-0.117	0.039	-0.366	-0.144	0.044	-0.452	(40)
Los Angeles	CA	-0.206	0.057	-0.613	-0.089	0.052	-0.291	-0.130	0.038	-0.406	-0.147	0.039	-0.462	(41)
Detroit	MI	-0.259	0.103	-0.771	-0.043	0.086	-0.141	-0.136	0.054	-0.425	-0.151	0.067	-0.474	(42)
San Antonio	TX	-0.168	0.115	-0.500	-0.141	0.093	-0.461	-0.110	0.063	-0.345	-0.154	0.074	-0.484	(43)
Port St. Lucie	FL	-0.258	0.109	-0.766	-0.057	0.089	-0.187	-0.174	0.057	-0.547	-0.157	0.070	-0.493	(44)
Chicago	IL	-0.235	0.081	-0.698	-0.118	0.070	-0.386	-0.154	0.048	-0.484	-0.176	0.053	-0.553	(45)
Fresno	CA	-0.245	0.113	-0.727	-0.109	0.094	-0.358	-0.164	0.062	-0.515	-0.177	0.073	-0.555	(46)
Orlando	FL	-0.225	0.088	-0.670	-0.138	0.078	-0.451	-0.136	0.054	-0.427	-0.182	0.059	-0.570	(47)
Raleigh	NC	-0.198	0.120	-0.588	-0.204	0.096	-0.666	-0.195	0.065	-0.610	-0.201	0.077	-0.629	(48)
Charlotte	NC	-0.191	0.114	-0.567	-0.267	0.092	-0.875	-0.205	0.061	-0.642	-0.229	0.073	-0.718	(49)
New Orleans	LA	-0.187	0.127	-0.557	-0.285	0.098	-0.932	-0.214	0.065	-0.672	-0.236	0.080	-0.740	(50)

Notes: Table presents per-year exposure predictions by gender for the 50 largest CZs. Estimates are for children in below-median (p25) income families. Column (1) reports the predictions for the child's family income rank at age 26. Column (2) reports the root mean square error for this prediction, computed as the square root of 1/(1/v_r + 1/v)) where v_r is the residual signal variance and v is the squared standard error of the fixed effect estimate. Column (3) scales the numbers to the percentage dollar increase by multiplying the estimates in column (1) by the regression coefficient from regressing the permanent resident outcomes at p25 for child family income at age 26 on the analogous outcomes for child rank at age 26 divided by the mean income of children from below-median (p25) income families. Columns (4)-(6) repeat the analysis on the sample of female children. Columns (7)-(9) report the baseline (pooled gender) forecasts. Columns (10) reports the average of the two gender-specific forecasts. Column (11) reports the rmse of this forecast, constructed as the square root of the sum of the squared male and female rmse divided by two. Column (12) scales this to the percentage increase in incomes using the same scaling factors as in Column (9). The rows are sorted in decending order according to the gender-average specification.

Table XI
Predicted Place Effects for 100 Largest Counties (Top and Bottom 25 based on Family Income Rank)

		Male Family Income			Female Family Income			Pooled Spec			Average			
		Prediction	RMSE	% Increase	Prediction	RMSE	% Increase	Prediction	RMSE	% Increase	Prediction	RMSE	% Increase	Row Number
County	State	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	
Dupage	IL	0.205	0.157	0.608	0.278	0.112	0.909	0.255	0.090	0.800	0.241	0.096	0.756	(1)
Snohomish	WA	0.234	0.178	0.696	0.224	0.122	0.732	0.224	0.099	0.701	0.229	0.108	0.718	(2)
Bergen	NJ	0.279	0.190	0.831	0.171	0.124	0.560	0.220	0.102	0.689	0.225	0.113	0.706	(3)
Bucks	PA	0.283	0.186	0.841	0.141	0.123	0.461	0.198	0.101	0.620	0.212	0.112	0.664	(4)
Contra Costa	CA	0.243	0.167	0.724	0.144	0.116	0.471	0.141	0.095	0.442	0.194	0.102	0.607	(5)
Fairfax	VA	0.155	0.189	0.461	0.231	0.124	0.755	0.239	0.100	0.749	0.193	0.113	0.604	(6)
King	WA	0.187	0.139	0.557	0.174	0.106	0.570	0.149	0.084	0.467	0.181	0.087	0.566	(7)
Norfolk	MA	0.209	0.186	0.622	0.135	0.123	0.443	0.183	0.101	0.573	0.172	0.112	0.540	(8)
Montgomery	MD	0.126	0.185	0.376	0.208	0.122	0.682	0.151	0.099	0.473	0.167	0.111	0.525	(9)
Middlesex	NJ	0.131	0.193	0.391	0.143	0.124	0.469	0.146	0.102	0.456	0.137	0.115	0.430	(10)
Montgomery	PA	0.074	0.168	0.220	0.177	0.118	0.579	0.155	0.096	0.487	0.125	0.103	0.393	(11)
Ventura	CA	0.183	0.181	0.545	0.053	0.123	0.174	0.099	0.100	0.309	0.118	0.109	0.371	(12)
Middlesex	MA	0.128	0.159	0.381	0.079	0.114	0.260	0.123	0.091	0.386	0.104	0.098	0.325	(13)
Macomb	MI	0.042	0.157	0.126	0.136	0.113	0.447	0.111	0.088	0.349	0.089	0.097	0.280	(14)
San Mateo	CA	0.071	0.190	0.211	0.106	0.124	0.348	0.085	0.102	0.265	0.089	0.113	0.278	(15)
Hudson	NJ	0.175	0.188	0.521	-0.017	0.122	-0.057	0.066	0.101	0.208	0.079	0.112	0.247	(16)
Salt Lake	UT	-0.015	0.174	-0.044	0.156	0.122	0.511	0.099	0.095	0.309	0.071	0.106	0.221	(17)
Pierce	WA	0.092	0.170	0.273	0.030	0.119	0.099	0.033	0.096	0.104	0.061	0.104	0.191	(18)
Providence	RI	0.110	0.190	0.326	0.012	0.125	0.039	0.048	0.101	0.150	0.061	0.114	0.190	(19)
Kern	CA	0.101	0.149	0.300	0.017	0.110	0.054	0.062	0.086	0.193	0.059	0.093	0.184	(20)
Monmouth	NJ	0.010	0.192	0.031	0.103	0.125	0.338	0.075	0.103	0.235	0.057	0.114	0.178	(21)
San Diego	CA	0.027	0.106	0.082	0.079	0.088	0.258	0.058	0.063	0.183	0.053	0.069	0.166	(22)
Worcester	MA	0.020	0.203	0.059	0.068	0.129	0.221	0.075	0.107	0.235	0.044	0.120	0.137	(23)
Hennepin	MN	0.081	0.172	0.242	0.004	0.119	0.014	-0.024	0.094	-0.076	0.043	0.105	0.134	(24)
Hartford	CT	0.084	0.192	0.249	-0.001	0.125	-0.004	0.027	0.102	0.084	0.041	0.114	0.129	(25)
Davidson	TN	-0.095	0.182	-0.284	-0.153	0.121	-0.501	-0.141	0.098	-0.443	-0.124	0.109	-0.390	(75)
Fairfield	CT	-0.227	0.198	-0.675	-0.038	0.127	-0.125	-0.101	0.104	-0.318	-0.133	0.118	-0.416	(76)
New Haven	CT	-0.252	0.182	-0.748	-0.015	0.122	-0.051	-0.085	0.099	-0.267	-0.133	0.110	-0.418	(77)
Essex	NJ	-0.081	0.174	-0.241	-0.195	0.118	-0.637	-0.147	0.096	-0.462	-0.138	0.105	-0.432	(78)
Montgomery	OH	-0.152	0.196	-0.451	-0.133	0.127	-0.437	-0.142	0.104	-0.447	-0.143	0.117	-0.447	(79)
San Bernardino	CA	-0.200	0.096	-0.596	-0.085	0.082	-0.280	-0.140	0.062	-0.439	-0.143	0.063	-0.448	(80)
Monroe	NY	-0.234	0.215	-0.695	-0.057	0.132	-0.186	-0.108	0.110	-0.338	-0.145	0.126	-0.455	(81)
Shelby	TN	-0.151	0.162	-0.448	-0.154	0.116	-0.505	-0.210	0.093	-0.657	-0.152	0.099	-0.478	(82)
Jefferson	AL	-0.182	0.191	-0.540	-0.142	0.125	-0.463	-0.102	0.102	-0.320	-0.162	0.114	-0.507	(83)
Los Angeles	CA	-0.218	0.067	-0.648	-0.122	0.060	-0.398	-0.164	0.045	-0.514	-0.170	0.045	-0.532	(84)
New York	NY	-0.118	0.127	-0.351	-0.228	0.098	-0.747	-0.173	0.076	-0.542	-0.173	0.080	-0.543	(85)
Riverside	CA	-0.285	0.105	-0.849	-0.071	0.087	-0.234	-0.161	0.067	-0.505	-0.178	0.068	-0.559	(86)
Palm Beach	FL	-0.277	0.146	-0.824	-0.084	0.112	-0.275	-0.208	0.084	-0.651	-0.181	0.092	-0.566	(87)
Wake	NC	-0.225	0.190	-0.670	-0.139	0.123	-0.455	-0.171	0.101	-0.536	-0.182	0.113	-0.571	(88)
Fulton	GA	-0.196	0.130	-0.581	-0.176	0.101	-0.576	-0.173	0.077	-0.543	-0.186	0.082	-0.582	(89)
Marion	IN	-0.148	0.172	-0.439	-0.237	0.118	-0.775	-0.209	0.097	-0.655	-0.192	0.105	-0.603	(90)
Pima	AZ	-0.387	0.157	-1.151	-0.001	0.114	-0.002	-0.142	0.083	-0.446	-0.194	0.097	-0.608	(91)
Bronx	NY	-0.256	0.127	-0.760	-0.137	0.098	-0.448	-0.174	0.076	-0.544	-0.196	0.080	-0.615	(92)
Milwaukee	WI	-0.249	0.180	-0.740	-0.144	0.122	-0.471	-0.158	0.096	-0.496	-0.196	0.109	-0.616	(93)
Wayne	MI	-0.293	0.135	-0.872	-0.106	0.104	-0.347	-0.182	0.077	-0.570	-0.200	0.085	-0.626	(94)
Fresno	CA	-0.282	0.155	-0.840	-0.130	0.113	-0.427	-0.215	0.089	-0.675	-0.206	0.096	-0.647	(95)
Cook	IL	-0.230	0.095	-0.683	-0.196	0.079	-0.641	-0.204	0.060	-0.640	-0.213	0.062	-0.667	(96)
Orange	FL	-0.246	0.126	-0.731	-0.184	0.099	-0.601	-0.193	0.077	-0.605	-0.215	0.080	-0.673	(97)
Hillsborough	FL	-0.274	0.151	-0.815	-0.155	0.113	-0.509	-0.220	0.088	-0.691	-0.215	0.095	-0.673	(98)
Mecklenburg	NC	-0.215	0.173	-0.640	-0.225	0.119	-0.737	-0.231	0.095	-0.723	-0.220	0.105	-0.690	(99)
Baltimore City	MD	-0.469	0.155	-1.393	-0.082	0.112	-0.270	-0.223	0.092	-0.699	-0.275	0.096	-0.864	(100)

Notes: Table presents per-year exposure predictions by gender for the top 25 and bottom 25 of the 100 largest counties. Estimates are for children in below-median (p25) income families. Column (1) reports the predictions for the child's family income rank at age 26. Column (2) reports the root mean square error for this prediction, computed as the square root of 1/(1/v_r + 1/v)) where v_r is the residual signal variance and v is the squared standard error of the fixed effect estimate. Column (3) scales the numbers to the percentage dollar increase by multiplying the estimates in column (1) by the regression coefficient from regressing the permanent resident outcomes at p25 for child family income at age 26 on the analogous outcomes for child rank at age 26 divided by the mean income of children from below-median (p25) income families. Columns (4)-(6) repeat the analysis on the sample of female children. Columns (7)-(9) report the baseline (pooled gender) forecasts. Columns (10) reports the average of the two gender-specific forecasts. Column (11) reports the rmse of this forecast, constructed as the square root of the sum of the squared male and female rmse divided by two. Column (12) scales this to the percentage increase in incomes using the same scaling factors as in Column (9). The rows are sorted in decending order according to the gender-average specification.

TABLE XII

Regressions of Place Effects Across Commuting Zones on Selected Covariates (Below-Median Income Parents (p25))

		Standard Deviation of Covariate	Exposure Effect Correlation		Regression Decomposition on Model Components					
					Permanent Residents		Causal (20 years)		Sorting	
		(1)	(2)		(3)		(4)		(5)	
		Std. Dev	Correlation	s.e.	Coeff	(s.e.)	Coeff	(s.e.)	Coeff	(s.e.)
Segregation and Poverty	Fraction Black Residents	0.100	-0.514	(0.128)	-2.418	(0.229)	-1.361	(0.339)	-1.027	(0.306)
	Poverty Rate	0.041	-0.144	(0.156)	-0.551	(0.296)	-0.381	(0.412)	-0.174	(0.408)
	Racial Segregation Theil Index	0.107	-0.510	(0.109)	-1.693	(0.249)	-1.351	(0.288)	-0.294	(0.312)
	Income Segregation Theil Index	0.034	-0.574	(0.137)	-1.141	(0.307)	-1.518	(0.364)	0.448	(0.378)
	Segregation of Poverty (<p25)	0.030	-0.549	(0.145)	-1.287	(0.280)	-1.452	(0.384)	0.233	(0.366)
	Segregation of Affluence (>p75)	0.039	-0.580	(0.130)	-1.027	(0.320)	-1.534	(0.345)	0.579	(0.384)
	Share with Commute < 15 Mins	0.095	0.875	(0.133)	1.624	(0.322)	2.317	(0.353)	-0.718	(0.325)
	Log. Population Density	1.376	-0.647	(0.119)	-1.143	(0.345)	-1.713	(0.315)	0.633	(0.278)
Income Distribution	Household Income per Capita for Working-Age Adults	6,945	-0.304	(0.150)	-0.217	(0.282)	-0.805	(0.397)	0.618	(0.275)
	Gini coefficient for Parent Income	0.083	-0.765	(0.131)	-1.387	(0.501)	-2.024	(0.346)	0.686	(0.381)
	Top 1% Income Share for Parents	5.032	-0.493	(0.095)	-0.347	(0.289)	-1.304	(0.251)	0.994	(0.206)
	Gini Bottom 99%	0.054	-0.713	(0.107)	-1.795	(0.384)	-1.888	(0.284)	0.135	(0.398)
	Fraction Middle Class (Between National p25 and p75)	0.061	0.700	(0.141)	1.615	(0.404)	1.853	(0.374)	-0.299	(0.393)
Tax	Local Tax Rate	0.006	-0.126	(0.138)	0.002	(0.301)	-0.332	(0.365)	0.286	(0.306)
	Local Tax Rate per Capita	0.381	-0.292	(0.172)	-0.078	(0.255)	-0.774	(0.454)	0.678	(0.348)
	Local Government Expenditures per Capita	680.7	-0.300	(0.131)	0.235	(0.278)	-0.794	(0.346)	1.026	(0.405)
	State EITC Exposure	3.708	0.151	(0.154)	0.799	(0.296)	0.400	(0.407)	0.404	(0.258)
	State Income Tax Progressivity	2.336	-0.080	(0.158)	0.592	(0.205)	-0.212	(0.419)	0.814	(0.415)
K-12 Education	School Expenditure per Student	1.312	-0.015	(0.147)	0.254	(0.286)	-0.041	(0.388)	0.291	(0.358)
	Student/Teacher Ratio	2.681	-0.346	(0.108)	0.038	(0.386)	-0.915	(0.285)	1.028	(0.385)
	Test Score Percentile (Controlling for Parent Income)	7.204	0.509	(0.102)	0.787	(0.662)	1.346	(0.269)	-0.623	(0.562)
	High School Dropout Rate (Controlling for Parent Income)	0.016	-0.551	(0.138)	-1.628	(0.329)	-1.458	(0.366)	-0.112	(0.294)
College	Number of Colleges per Capita	0.007	0.647	(0.136)	0.547	(0.250)	1.713	(0.359)	-1.127	(0.351)
	Mean College Tuition	3,315	-0.147	(0.106)	-0.113	(0.275)	-0.389	(0.280)	0.290	(0.324)
	College Graduation Rate (Controlling for Parent Income)	0.104	0.141	(0.116)	0.519	(0.267)	0.373	(0.307)	0.139	(0.209)
Local Labor Market	Labor Force Participation Rate	0.047	0.141	(0.162)	0.278	(0.286)	0.373	(0.428)	-0.076	(0.338)
	Fraction Working in Manufacturing	0.062	0.028	(0.147)	-0.239	(0.301)	0.073	(0.390)	-0.276	(0.320)
	Growth in Chinese Imports 1990-2000 (Autor and Dorn 2013)	0.979	-0.032	(0.117)	0.176	(0.231)	-0.086	(0.309)	0.301	(0.213)
	Teenage (14-16) Labor Force Participation Rate	0.101	0.554	(0.138)	1.293	(0.467)	1.466	(0.365)	-0.223	(0.520)
Migration	Migration Inflow Rate	0.011	-0.174	(0.139)	-0.054	(0.278)	-0.459	(0.368)	0.452	(0.286)
	Migration Outflow Rate	0.007	-0.117	(0.129)	0.208	(0.284)	-0.311	(0.342)	0.569	(0.280)
	Fraction of Foreign Born Residents	0.100	-0.447	(0.104)	0.196	(0.286)	-1.184	(0.275)	1.417	(0.315)
Social Capital	Social Capital Index (Rupasingha and Goetz 2008)	0.936	0.697	(0.133)	1.216	(0.392)	1.845	(0.352)	-0.692	(0.411)
	Fraction Religious	0.107	0.178	(0.172)	1.062	(0.361)	0.471	(0.456)	0.551	(0.278)
	Violent Crime Rate	0.001	-0.679	(0.115)	-0.959	(0.584)	-1.798	(0.305)	0.871	(0.467)
Family Structure	Fraction of Children with Single Mothers	0.036	-0.567	(0.119)	-2.458	(0.345)	-1.500	(0.316)	-0.909	(0.382)
	Fraction of Adults Divorced	0.015	0.040	(0.156)	-0.710	(0.287)	0.106	(0.414)	-0.781	(0.273)
	Fraction of Adults Married	0.034	0.522	(0.141)	1.449	(0.365)	1.382	(0.373)	-0.007	(0.410)
Prices	Median House Prices	82,926	-0.324	(0.133)	0.286	(0.270)	-0.858	(0.351)	1.194	(0.202)
	Median Monthly Rent	206.8	-0.424	(0.139)	-0.006	(0.335)	-1.123	(0.368)	1.186	(0.276)

Notes: This table presents estimates of regressions of the place effects for children in below-median income families (p25) at the CZ level on normalized covariates. Appendix Table XIV provides a definition and source for each of these variables. Each covariate is standardized to have mean 0 and standard deviation 1 using population weights by CZ from the 2000 Census. Column (1) reports the standard deviation of the covariate prior to this normalization. Column (2) reports the correlation between the place exposure effect and the covariate. We compute this as the regression coefficient of the place exposure effect estimate on the covariate; we then divide this coefficient (and its standard error) by the estimated signal standard deviation (reported in Table VII) to arrive at the correlation and its standard error. Column (3) reports the coefficient of a regression of the permanent resident outcomes on the normalized covariate (and its standard error). Columns (4)-(5) decompose this regression coefficient into the regression of the place exposure effect (multiplying by 20 years of exposure) on the normalized covariate (Column (4)) and the sorting component (=permenant resident outcomes - 20*place exposure effect) on the normalized covariate. All regressions include population weights using 2000 Census populations. Standard errors presented in parentheses are clustered at the state level to account for spatial autocorrelation.

TABLE XIII

Regressions of Place Effects Across Commuting Zones on Selected Covariates (Above-Median Income Parents (p75))

		Standard Deviation of Covariate (1)	Exposure Effect Correlation (2)		Regression Decomposition on Model Components: Permanent Residents (3)		Causal (20 years) (4)		Sorting 5)	
		Std. Dev	Correlation	s.e.	Coeff	(s.e.)	Coeff	(s.e.)	Coeff	(s.e.)
Segregation and Poverty	Fraction Black Residents	0.100	-0.005	(0.203)	-0.539	(0.343)	-0.011	(0.434)	-0.501	(0.262)
	Poverty Rate	0.041	-0.063	(0.209)	-0.563	(0.227)	-0.134	(0.446)	-0.455	(0.331)
	Racial Segregation Theil Index	0.107	-0.163	(0.102)	-0.737	(0.185)	-0.348	(0.219)	-0.358	(0.244)
	Income Segregation Theil Index	0.034	-0.557	(0.167)	-1.395	(0.236)	-1.190	(0.357)	-0.170	(0.249)
	Segregation of Poverty (<p25)	0.030	-0.453	(0.148)	-1.271	(0.206)	-0.969	(0.317)	-0.265	(0.243)
	Segregation of Affluence (>p75)	0.039	-0.623	(0.179)	-1.472	(0.250)	-1.332	(0.383)	-0.107	(0.255)
	Share with Commute < 15 Mins	0.095	0.602	(0.150)	1.555	(0.222)	1.288	(0.321)	0.287	(0.270)
	Log. Population Density	1.376	-0.423	(0.140)	-1.012	(0.261)	-0.905	(0.299)	-0.073	(0.221)
Income Distribution	Household Income per Capita for Working-Age Adults	6,945	-0.334	(0.162)	-0.619	(0.196)	-0.714	(0.346)	0.123	(0.258)
	Gini coefficient for Parent Income	0.083	-0.694	(0.227)	-1.586	(0.473)	-1.483	(0.485)	-0.074	(0.302)
	Top 1% Income Share for Parents	5.032	-0.514	(0.172)	-1.055	(0.372)	-1.099	(0.369)	0.062	(0.218)
	Gini Bottom 99%	0.054	-0.585	(0.175)	-1.444	(0.230)	-1.252	(0.374)	-0.170	(0.311)
	Fraction Middle Class (Between National p25 and p75)	0.061	0.487	(0.177)	1.512	(0.264)	1.041	(0.379)	0.440	(0.322)
Tax	Local Tax Rate	0.006	-0.086	(0.188)	-0.244	(0.237)	-0.185	(0.402)	-0.096	(0.265)
	Local Tax Rate per Capita	0.381	-0.264	(0.203)	-0.432	(0.193)	-0.564	(0.435)	-0.007	(0.297)
	Local Government Expenditures per Capita	680.7	-0.695	(0.247)	-1.209	(0.368)	-1.486	(0.529)	0.250	(0.290)
	State EITC Exposure	3.708	0.161	(0.132)	0.674	(0.245)	0.345	(0.283)	0.336	(0.184)
	State Income Tax Progressivity	2.336	-0.416	(0.329)	-0.749	(0.563)	-0.890	(0.704)	0.144	(0.224)
K-12 Education	School Expenditure per Student	1.312	0.031	(0.188)	0.148	(0.325)	0.067	(0.401)	0.082	(0.222)
	Student/Teacher Ratio	2.681	-0.726	(0.191)	-1.628	(0.177)	-1.553	(0.408)	-0.040	(0.320)
	Test Score Percentile (Controlling for Parent Income)	7.204	0.689	(0.205)	1.669	(0.186)	1.473	(0.438)	0.173	(0.383)
	High School Dropout Rate (Controlling for Parent Income)	0.016	-0.196	(0.153)	-0.902	(0.273)	-0.420	(0.327)	-0.437	(0.291)
College	Number of Colleges per Capita	0.007	0.518	(0.220)	1.086	(0.254)	1.109	(0.470)	0.161	(0.354)
	Mean College Tuition	3,315	0.127	(0.169)	0.342	(0.255)	0.272	(0.362)	0.085	(0.242)
	College Graduation Rate (Controlling for Parent Income)	0.104	-0.025	(0.149)	0.276	(0.276)	-0.054	(0.318)	0.325	(0.288)
Local Labor Market	Labor Force Participation Rate	0.047	-0.037	(0.213)	0.246	(0.265)	-0.079	(0.457)	0.353	(0.351)
	Fraction Working in Manufacturing	0.062	0.356	(0.173)	0.674	(0.232)	0.761	(0.369)	-0.052	(0.306)
	Growth in Chinese Imports 1990-2000 (Autor and Dorn 2013)	0.979	0.011	(0.139)	0.240	(0.168)	0.023	(0.297)	0.241	(0.242)
	Teenage (14-16) Labor Force Participation Rate	0.101	0.476	(0.253)	1.482	(0.305)	1.017	(0.542)	0.452	(0.349)
Migration	Migration Inflow Rate	0.011	-0.529	(0.148)	-0.638	(0.202)	-1.131	(0.317)	0.525	(0.273)
	Migration Outflow Rate	0.007	-0.514	(0.159)	-0.957	(0.214)	-1.100	(0.340)	0.173	(0.321)
	Fraction of Foreign Born Residents	0.100	-0.858	(0.182)	-1.572	(0.316)	-1.835	(0.388)	0.283	(0.232)
Social Capital	Social Capital Index (Rupasingha and Goetz 2008)	0.936	0.663	(0.203)	1.590	(0.244)	1.417	(0.434)	0.157	(0.342)
	Fraction Religious	0.107	0.248	(0.148)	1.252	(0.207)	0.531	(0.318)	0.689	(0.266)
	Violent Crime Rate	0.001	-0.780	(0.199)	-1.334	(0.287)	-1.669	(0.425)	0.343	(0.255)
Family Structure	Fraction of Children with Single Mothers	0.036	-0.105	(0.184)	-0.851	(0.331)	-0.225	(0.393)	-0.581	(0.248)
	Fraction of Adults Divorced	0.015	0.105	(0.195)	-0.529	(0.291)	0.226	(0.417)	-0.720	(0.292)
	Fraction of Adults Married	0.034	0.480	(0.181)	1.419	(0.304)	1.027	(0.388)	0.351	(0.264)
Prices	Median House Prices	82,926	-0.648	(0.120)	-1.224	(0.204)	-1.387	(0.256)	0.193	(0.198)
	Median Monthly Rent	206.8	-0.718	(0.180)	-1.367	(0.282)	-1.536	(0.385)	0.207	(0.260)

Notes: This table presents estimates of regressions of the place effects for children in above-median income families (p75) at the CZ level on normalized covariates. Appendix Table XIV provides a definition and source for each of these variables. Each covariate is standardized to have mean 0 and standard deviation 1 using population weights by CZ from the 2000 Census. Column (1) reports the standard deviation of the covariate prior to this normalization. Column (2) reports the correlation between the place exposure effect and the covariate. We compute this as the regression coefficient of the place exposure effect estimate on the covariate; we then divide this coefficient (and its standard error) by the estimated signal standard deviation (reported in Table VII) to arrive at the correlation and its standard error. Column (3) reports the coefficient of a regression of the permanent resident outcomes on the normalized covariate (and its standard error). Columns (4)-(5) decompose this regression coefficient into the regression of the place exposure effect (multiplying by 20 years of exposure) on the normalized covariate (Column (4)) and the sorting component (=permenant resident outcomes - 20*place exposure effect) on the normalized covariate. All regressions include population weights using 2000 Census populations. Standard errors presented in parentheses are clustered at the state level to account for spatial autocorrelation.

TABLE XIV

Regressions of Place Effects Across Counties within Commuting Zones on Selected Covariates (Below-Median Income Parents (p25))

		Standard Deviation of Covariate	Exposure Effect Correlation		Regression Decomposition on Model Components					
					Permanent Residents		Causal (20 years)		Sorting	
		(1)	(2)		(3)		(4)		5)	
		Std. Dev	Correlation	s.e.	Coeff	(s.e.)	Coeff	(s.e.)	Coeff	(s.e.)
Segregation and Poverty	Fraction Black Residents	0.130	-0.319	(0.103)	-2.253	(0.174)	-0.632	(0.205)	-1.622	(0.220)
	Poverty Rate	0.056	-0.232	(0.108)	-1.940	(0.224)	-0.461	(0.214)	-1.491	(0.200)
	Racial Segregation Theil Index	0.119	-0.371	(0.096)	-2.231	(0.145)	-0.735	(0.190)	-1.501	(0.195)
	Income Segregation Theil Index	0.039	-0.422	(0.101)	-1.686	(0.113)	-0.837	(0.200)	-0.838	(0.197)
	Segregation of Poverty (<p25)	0.034	-0.463	(0.103)	-1.810	(0.128)	-0.919	(0.204)	-0.884	(0.206)
	Segregation of Affluence (>p75)	0.046	-0.357	(0.107)	-1.460	(0.123)	-0.708	(0.212)	-0.737	(0.199)
	Share with Commute < 15 Mins	0.104	0.019	(0.117)	0.198	(0.188)	0.037	(0.233)	0.196	(0.313)
	Log. Population Density	1.752	-0.269	(0.112)	-1.764	(0.267)	-0.533	(0.221)	-1.230	(0.297)
Income Distribution	Household Income per Capita for Working-Age Adults	9,236	0.056	(0.140)	0.814	(0.249)	0.112	(0.278)	0.702	(0.199)
	Gini coefficient for Parent Income	0.113	-0.410	(0.136)	-1.933	(0.413)	-0.813	(0.270)	-1.117	(0.274)
	Top 1% Income Share for Parents	0.064	-0.227	(0.095)	-0.943	(0.256)	-0.451	(0.188)	-0.492	(0.234)
	Gini Bottom 99%	0.112	-0.410	(0.136)	-1.936	(0.412)	-0.814	(0.270)	-1.119	(0.273)
	Fraction Middle Class (Between National p25 and p75)	0.075	0.129	(0.134)	0.711	(0.260)	0.255	(0.265)	0.428	(0.231)
Tax	Local Tax Rate	0.010	-0.212	(0.124)	-0.853	(0.609)	-0.421	(0.246)	-0.480	(0.545)
	Local Tax Rate per Capita	0.475	-0.146	(0.107)	-0.412	(0.502)	-0.290	(0.212)	-0.140	(0.468)
	Local Government Expenditures per Capita	1.062	-0.299	(0.135)	-1.013	(0.545)	-0.593	(0.267)	-0.447	(0.438)
	State EITC Exposure	3.745	-0.013	(0.211)	-0.084	(0.061)	-0.026	(0.419)	-0.061	(0.392)
	State Income Tax Progressivity	2.358	-0.192	(0.270)	-0.132	(0.128)	-0.381	(0.535)	0.249	(0.574)
K-12 Education	School Expenditure per Student	1.505	-0.066	(0.121)	-0.274	(0.339)	-0.130	(0.240)	-0.233	(0.393)
	Student/Teacher Ratio	2.837	-0.104	(0.107)	-0.572	(0.210)	-0.207	(0.212)	-0.344	(0.300)
	Test Score Percentile (Controlling for Parent Income)	9.630	0.354	(0.130)	1.750	(0.360)	0.702	(0.259)	1.055	(0.316)
	High School Dropout Rate (Controlling for Parent Income)	0.024	-0.375	(0.129)	-1.777	(0.214)	-0.743	(0.256)	-1.054	(0.303)
College	Number of Colleges per Capita	0.012	-0.039	(0.177)	-0.415	(0.183)	-0.078	(0.352)	-0.426	(0.342)
	Mean College Tuition	4,421	-0.017	(0.138)	-0.330	(0.255)	-0.033	(0.274)	-0.297	(0.397)
	College Graduation Rate (Controlling for Parent Income)	0.139	0.035	(0.156)	-0.543	(0.203)	0.069	(0.309)	-0.615	(0.338)
Local Labor Market	Labor Force Participation Rate	0.058	-0.096	(0.124)	0.897	(0.234)	-0.190	(0.245)	1.136	(0.234)
	Fraction Working in Manufacturing	0.070	0.244	(0.129)	0.941	(0.143)	0.485	(0.257)	0.490	(0.255)
	Teenage (14-16) Labor Force Participation Rate	0.109	0.087	(0.124)	1.026	(0.205)	0.172	(0.245)	0.864	(0.253)
Migration	Migration Inflow Rate	0.019	-0.036	(0.085)	0.996	(0.225)	-0.072	(0.169)	1.095	(0.225)
	Migration Outflow Rate	0.014	0.009	(0.124)	0.119	(0.235)	0.018	(0.246)	0.126	(0.249)
	Fraction of Foreign Born Residents	0.109	-0.029	(0.124)	-0.633	(0.217)	-0.058	(0.246)	-0.568	(0.239)
Social Capital	Social Capital Index (Rupasingha and Goetz 2008)	1.102	0.148	(0.148)	-0.033	(0.221)	0.293	(0.293)	-0.344	(0.348)
	Fraction Religious	0.129	0.075	(0.137)	0.025	(0.168)	0.149	(0.271)	-0.152	(0.284)
	Violent Crime Rate	0.002	-0.320	(0.106)	-1.742	(0.141)	-0.635	(0.211)	-1.118	(0.200)
Family Structure	Fraction of Children with Single Mothers	0.070	-0.377	(0.107)	-2.500	(0.257)	-0.747	(0.212)	-1.739	(0.195)
	Fraction of Adults Divorced	0.017	-0.336	(0.132)	-1.670	(0.161)	-0.667	(0.261)	-1.019	(0.259)
	Fraction of Adults Married	0.063	0.333	(0.094)	2.390	(0.131)	0.661	(0.186)	1.719	(0.203)
Prices	Median House Price	124,006	-0.058	(0.068)	0.158	(0.406)	-0.115	(0.134)	0.278	(0.379)
	Median Monthly Rent	219.3	0.078	(0.125)	0.737	(0.227)	0.154	(0.248)	0.623	(0.254)

Notes: This table presents estimates of regressions of the place effects for children in below-median income families (p25) at the county level on normalized covariates, conditional on a set of CZ fixed effects. Appendix Table XIV provides a definition and source for each of these variables. Each covariate is standardized to have mean 0 and standard deviation 1 using population weights by CZ from the 2000 Census. Column (1) reports the standard deviation of the covariate prior to this normalization. Column (2) reports the correlation between the place exposure effect and the covariate conditional on CZ fixed effects. We compute this as the regression coefficient of the place exposure effect estimate on the covariate conditional on CZ fixed effects; we then divide this coefficient (and its standard error) by the estimated signal standard deviation (reported in Table VII, column (5)) to arrive at the correlation and its standard error. Column (3) reports the coefficient of a regression of the permanent resident outcomes on the normalized covariate (and its standard error), conditional on CZ fixed effects. Columns (4)-(5) decompose this regression coefficient into the regression of the place exposure effect (multiplying by 20 years of exposure) on the normalized covariate (Column (4)) and the sorting component (=permenant resident outcomes - 20*place exposure effect) on the normalized covariate. All regressions include population weights using 2000 Census populations. Standard errors presented in parentheses are clustered at the CZ level to account for spatial autocorrelation.

TABLE XV

Regressions of Place Effects Across Counties within Commuting Zones on Selected Covariates (Above-Median Income Parents (p75))

		Standard Deviation of Covariate	Exposure Effect Correlation		Regression Decomposition on Model Components					
					Permanent Residents		Causal (20 years)		Sorting	
		(1)	(2)		(3)		(4)		5)	
		Std. Dev	Correlation	s.e.	Coeff	(s.e.)	Coeff	(s.e.)	Coeff	(s.e.)
Segregation and Poverty	Fraction Black Residents	0.130	0.137	(0.138)	-1.363	(0.102)	0.305	(0.309)	-1.671	(0.342)
	Poverty Rate	0.056	-0.020	(0.164)	-1.138	(0.111)	-0.044	(0.366)	-1.108	(0.342)
	Racial Segregation Theil Index	0.119	0.138	(0.095)	-1.329	(0.120)	0.309	(0.211)	-1.642	(0.223)
	Income Segregation Theil Index	0.039	-0.055	(0.108)	-1.255	(0.075)	-0.123	(0.241)	-1.123	(0.235)
	Segregation of Poverty (<p25)	0.034	-0.081	(0.116)	-1.283	(0.082)	-0.181	(0.258)	-1.094	(0.255)
	Segregation of Affluence (>p75)	0.046	-0.039	(0.104)	-1.144	(0.087)	-0.087	(0.232)	-1.045	(0.225)
	Share with Commute < 15 Mins	0.104	0.079	(0.262)	0.208	(0.170)	0.177	(0.586)	0.064	(0.604)
	Log. Population Density	1.752	-0.043	(0.122)	-1.453	(0.154)	-0.096	(0.273)	-1.358	(0.264)
Income Distribution	Household Income per Capita for Working-Age Adults	9,236	-0.025	(0.098)	0.227	(0.114)	-0.057	(0.219)	0.287	(0.231)
	Gini coefficient for Parent Income	0.113	-0.064	(0.134)	-1.443	(0.174)	-0.144	(0.299)	-1.298	(0.422)
	Top 1% Income Share for Parents	0.064	-0.010	(0.145)	-0.936	(0.170)	-0.022	(0.324)	-0.918	(0.443)
	Gini Bottom 99%	0.112	-0.065	(0.134)	-1.444	(0.173)	-0.144	(0.298)	-1.299	(0.421)
	Fraction Middle Class (Between National p25 and p75)	0.075	-0.136	(0.143)	0.661	(0.144)	-0.304	(0.318)	0.956	(0.325)
Tax	Local Tax Rate	0.010	-0.008	(0.143)	-0.534	(0.486)	-0.017	(0.319)	-0.546	(0.620)
	Local Tax Rate per Capita	0.475	-0.022	(0.105)	-0.352	(0.479)	-0.049	(0.235)	-0.313	(0.550)
	Local Government Expenditures per Capita	1.062	-0.184	(0.103)	-0.689	(0.393)	-0.411	(0.230)	-0.298	(0.454)
	State EITC Exposure	3.745	0.014	(0.152)	-0.053	(0.035)	0.032	(0.340)	-0.087	(0.335)
	State Income Tax Progressivity	2.358	-0.145	(0.101)	-0.123	(0.107)	-0.324	(0.225)	0.198	(0.253)
K-12 Education	School Expenditure per Student	1.505	0.051	(0.166)	-0.198	(0.345)	0.113	(0.370)	-0.378	(0.371)
	Student/Teacher Ratio	2.837	-0.206	(0.163)	-0.513	(0.247)	-0.460	(0.365)	-0.043	(0.322)
	Test Score Percentile (Controlling for Parent Income)	9.630	0.031	(0.119)	1.021	(0.118)	0.070	(0.265)	0.958	(0.334)
	High School Dropout Rate (Controlling for Parent Income)	0.024	0.148	(0.174)	-1.064	(0.121)	0.330	(0.388)	-1.403	(0.404)
College	Number of Colleges per Capita	0.012	-0.148	(0.188)	-0.166	(0.153)	-0.329	(0.421)	0.116	(0.440)
	Mean College Tuition	4,421	-0.154	(0.137)	-0.324	(0.181)	-0.343	(0.306)	0.021	(0.351)
	College Graduation Rate (Controlling for Parent Income)	0.139	-0.077	(0.148)	-0.485	(0.122)	-0.173	(0.331)	-0.313	(0.320)
Local Labor Market	Labor Force Participation Rate	0.058	-0.136	(0.152)	0.195	(0.183)	-0.303	(0.340)	0.530	(0.361)
	Fraction Working in Manufacturing	0.070	0.155	(0.174)	0.890	(0.079)	0.345	(0.388)	0.573	(0.402)
	Teenage (14-16) Labor Force Participation Rate	0.109	0.013	(0.194)	0.486	(0.160)	0.028	(0.434)	0.466	(0.442)
Migration	Migration Inflow Rate	0.019	-0.305	(0.122)	0.486	(0.110)	-0.682	(0.274)	1.189	(0.252)
	Migration Outflow Rate	0.014	-0.163	(0.142)	-0.275	(0.211)	-0.365	(0.316)	0.104	(0.301)
	Fraction of Foreign Born Residents	0.109	0.192	(0.089)	-0.739	(0.125)	0.428	(0.198)	-1.162	(0.223)
Social Capital	Social Capital Index (Rupasingha and Goetz 2008)	1.102	0.003	(0.159)	-0.136	(0.166)	0.007	(0.356)	-0.157	(0.415)
	Fraction Religious	0.129	-0.105	(0.153)	0.013	(0.149)	-0.235	(0.342)	0.231	(0.357)
	Violent Crime Rate	0.002	0.059	(0.146)	-0.954	(0.147)	0.132	(0.326)	-1.092	(0.319)
Family Structure	Fraction of Children with Single Mothers	0.070	-0.074	(0.137)	-1.556	(0.093)	-0.165	(0.307)	-1.384	(0.304)
	Fraction of Adults Divorced	0.017	-0.123	(0.160)	-0.929	(0.153)	-0.274	(0.356)	-0.660	(0.333)
	Fraction of Adults Married	0.063	0.162	(0.172)	1.652	(0.099)	0.361	(0.384)	1.285	(0.360)
Prices	Median House Price	124,006	-0.228	(0.050)	-0.264	(0.117)	-0.508	(0.111)	0.251	(0.114)
	Median Monthly Rent	219.3	-0.045	(0.117)	0.033	(0.239)	-0.101	(0.262)	0.162	(0.265)

Notes: This table presents estimates of regressions of the place effects for children in above-median income families (p75) at the county level on normalized covariates, conditional on a set of CZ fixed effects. Appendix Table XVI provides a definition and source for each of these variables. Each covariate is standardized to have mean 0 and standard deviation 1 using population weights by CZ from the 2000 Census. Column (1) reports the standard deviation of the covariate prior to this normalization. Column (2) reports the correlation between the place exposure effect and the covariate conditional on CZ fixed effects. We compute this as the regression coefficient of the place exposure effect estimate on the covariate conditional on CZ fixed effects; we then divide this coefficient (and its standard error) by the estimated signal standard deviation (reported in Table VII, column (5)) to arrive at the correlation and its standard error. Column (3) reports the coefficient of a regression of the permanent resident outcomes on the normalized covariate (and its standard error), conditional on CZ fixed effects. Columns (4)-(5) decompose this regression coefficient into the regression of the place exposure effect (multiplying by 20 years of exposure) on the normalized covariate (Column (4)) and the sorting component (=permenant resident outcomes - 20*place exposure effect) on the normalized covariate. All regressions include population weights using 2000 Census populations. Standard errors presented in parentheses are clustered at the CZ level to account for spatial autocorrelation.

Appendix Table 1
Summary Statistics for Permanent Residents and Movers

Variable	Mean (1)	Std. Dev. (2)	Median (3)	Sample Size (4)
Panel A: County Permanent Residents and Movers				
Non-Movers				
Parent Income	81,932	320,026	54,800	37,689,238
Child family income at 24	25,066	136,016	19,900	19,956,828
Child family income at 26	34,091	157,537	26,600	15,364,222
Child family income at 30	48,941	133,264	36,200	6,355,414
Child individual earnings at 24	20,686	202,833	17,300	20,069,124
College attendence (18-23)	0.703	0.457	1.000	20,418,691
College quality (18-23)	31,608	13,207	31,400	20,418,691
Teen Birth (13-19)	0.107	0.309	0.000	14,503,588
Teen employment at age 16	0.276	0.447	0.000	37,464,779
One-time Movers Across CZ Sample				
Parent Income	94,738	400,685	55,100	1,498,319
Child family income at 24	23,815	72,306	18,200	654,491
Child family income at 26	32,532	139,563	24,300	483,407
Child family income at 30	48,834	110,619	33,500	188,801
Child individual earnings at 24	20,247	61,185	16,000	654,491
College attendence (18-23)	0.717	0.451	1.000	690,207
College quality (18-23)	32,171	14,001	31,900	690,207
Teen Birth (13-19)	0.103	0.304	0.000	524,194
Teen employment at age 16	0.233	0.423	0.000	1,498,319
One-time Movers Within CZ Sample				
Parent Income	84,850	356,758	48,900	1,425,096
Child family income at 24	24,006	68,559	18,300	617,502
Child family income at 26	32,993	75,520	24,500	457,140
Child family income at 30	49,974	108,248	33,500	179,856
Child individual earnings at 24	20,844	56,639	16,500	617,502
College attendence (18-23)	0.719	0.450	1.000	650,045
College quality (18-23)	32,883	14,086	33,200	650,045
Teen Birth (13-19)	0.095	0.293	0.000	496,122
Teen employment at age 16	0.245	0.430	0.000	1,425,096
Panel B: CZ and County Samples for Fixed Effects Estimation in Section VII				
CZ Movers Sample				
Parent Income	74,390	293,213	45,200	6,791,026
Child family income at 24	23,613	49,457	18,500	2,692,104
Child family income at 26	31,559	83,716	24,400	1,869,560
Child family income at 30	45,225	91,195	33,300	616,947
Child individual earnings at 24	18,787	42,333	15,600	2,692,104
College attendence (18-23)	0.625	0.484	1.000	4,026,000
College quality (18-23)	29,005	12,284	27,700	4,026,000
Teen Birth (13-19)	0.121	0.326	0.000	2,321,994
Teen employment at age 16	0.279	0.449	0.000	6,791,026

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County Movers Sample				
Parent Income	76,285	276,185	51,500	3,772,532
Child family income at 24	24,569	54,583	19,500	1,756,981
Child family income at 26	32,985	70,944	25,700	1,323,455
Child family income at 30	47,500	104,900	34,700	532,388
Child individual earnings at 24	19,832	45,082	16,800	1,756,981
College attendence (18-23)	0.637	0.481	1.000	2,316,963
College quality (18-23)	29,691	12,521	29,200	2,316,963
Teen Birth (13-19)	0.115	0.319	0.000	1,356,990
Teen employment at age 16	0.274	0.446	0.000	3,772,532

Notes: The table presents summary statistics for county movers sample discussed in Section VI (Panel A) and the sample used for the fixed effect estimation in Section VII (Panel B).

Appendix Table II
Population and Distance Restrictions

	Baseline	No Distance			100 Miles (Baseline)			200 Miles		
		Pop > 50K	Pop > 250K	Pop > 500K	Pop > 50K	Pop > 250K	Pop > 500K	Pop > 50K	Pop > 250K	Pop > 500K
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exposure Slope	0.040	0.032	0.035	0.037	0.036	0.039	0.040	0.037	0.039	0.041
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Num of Obs.	1,553,021	3,066,854	2,199,834	1,607,626	2,126,859	1,609,330	1,210,164	1,719,687	1,345,125	1,036,668

Notes: This table presents estimates of the baseline specification in equation (9) varying the sample restriction. Column (1) presents the baseline sample restricting to populations in the origin and destination CZ of greater than 250,000 people based on the 2000 Census and requiring a distance of move > 100 miles between zipcode centroids. Columns (2)-(10) vary these distance assumptions and population restrictions.

Appendix Table III
Heterogeneity in Exposure Effects

		Parental Income		Moves		Child Gender	
	Baseline	Above Median Income	Below Median Income	Positive Moves	Negative Moves	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exposure Slope	0.040	0.047	0.031	0.030	0.040	0.041	0.042
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)
Num of Obs.	1,553,021	803,189	749,832	783,936	769,085	783,181	769,717

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

Appendix Table IV
Prediction Regressions

	CZ		County	
	Below Median Income	Above Median Income	Below Median Income	Above Median Income
	(1)	(2)	(3)	(4)
Prediction Regression				
Permanent Residents Regression Coeff.	0.032	0.038	0.027	0.023
(s.e.)	(0.003)	(0.004)	(0.002)	(0.003)
SD of predicted values	0.106	0.097	0.115	0.076
SD of residual values	0.224	0.222	0.419	0.429
Noise SD of residuals	0.210	0.218	0.402	0.407
Signal SD of residuals	0.080	0.045	0.118	0.135
Num of Obs.	595	595	2,370	2,370

Notes: This table presents the coefficients from the regression of the fixed effects on permanent resident outcomes. The first row presents this regression coefficient (regression is precision-weighted). The lower four rows present the standard deviation of the predicted values, the standard deviation of the residual values, and the estimated signal and noise standard deviation (computed under the simplifying assumption of no uncertainty in the permanent resident outcomes).

Appendix Table V
Correlates of Alternative Measures of Place Effects

		Variable	Below Median Income Parents (p=25th percentile)		Above Median Income Parents (p=75th percentile)	
			Correlation with Baseline (1)	Signal SD (2)	Correlation with Baseline (3)	Signal SD (4)
Panel A: CZ Correlations						
		Baseline	1.000	0.132	1.000	0.107
	1.	Income Change Controls	0.946	0.151	0.942	0.111
Robustness	2.	Quadratic Income	0.940	0.144	0.932	0.134
	3.	Split Sample (Above/Below Median)	0.839	0.134	0.784	0.107
	4.	COLI adjusted	0.748	0.230	0.797	0.206
	5.	Individual Income	0.800	0.126	0.767	0.119
	6.	Males	0.706	0.213	0.677	0.104
	7.	Females	0.668	0.160	0.677	0.127
	8.	Males (Individual Income)	0.663	0.231	0.596	0.112
	9.	Females (Individual Income)	0.425	0.129	0.494	0.200
	10.	Individual income ($, not ranks)	0.746	102.1	0.659	125.4
	11.	Family Income ($, not ranks)	0.921	132.7	0.888	143.6
Panel B: County Correlations						
		Baseline	1.000	0.165	1.000	0.155
	1.	Income Change Controls	0.974	0.177	0.973	0.193
Robustness	2.	Quadratic Income	0.876	0.180	0.777	0.162
	3.	Split Sample (Above/Below Median)	0.841	0.208	0.659	0.195
	4.	COLI adjusted	0.808	0.253	0.852	0.235
	5.	Individual Income	0.771	0.144	0.754	0.175
	6.	Males	0.645	0.277	0.631	0.274
	7.	Females	0.656	0.172	0.639	0.196
	8.	Males (Individual Income)	0.586	0.277	0.547	0.247
	9.	Females (Individual Income)	0.404	0.110	0.433	0.287
	10.	Individual income ($, not ranks)	0.696		0.611	280.6
	11.	Family Income ($, not ranks)	0.872		0.785	363.8

Notes: Table presents the correlation of exposure effects under alternative specifications with the baseline (child income rank at age 26) estimates. Column (1) reports the correlation with the baseline estimates for below-median income families. We weight the observations by inverse of the sum of the variances of the two specifications. Column (2) reports the estimated signal standard deviation for the alternative specification. Columns (3) and (4) repeat columns (1) and (2) on the sample of above-median income families (p75).

Appendix Table VI

Decomposition of College Attendance Outcomes into Causal and Sorting Components for 10 Largest CZs

Commuting Zone	State	Below Median Income Parents				Above Median Income Parents			
		Permanent Residents	Decomposition			Permanent Residents	Decomposition		
			Sorting	Causal	(s.e.)		Sorting	Causal	(s.e.)
		(1)	(2)	(3)	4)	(5)	(6)	(7)	(8)
Los Angeles	CA	44.80	48.21	-3.41	(0.85)	52.69	58.16	-5.47	(0.91)
New York	NY	43.94	46.91	-2.97	(0.89)	56.73	57.51	-0.78	(1.14)
Chicago	IL	41.00	44.61	-3.60	(1.22)	56.65	57.45	-0.80	(1.01)
Newark	NJ	44.92	44.77	0.16	(1.31)	58.45	56.71	1.74	(1.09)
Philadelphia	PA	42.08	41.65	0.43	(1.65)	58.02	58.62	-0.60	(1.29)
Detroit	MI	38.62	40.46	-1.84	(1.46)	53.14	53.39	-0.25	(1.55)
Boston	MA	46.50	45.74	0.76	(1.87)	58.32	57.59	0.73	(1.79)
San Francisco	CA	45.55	45.21	0.34	(1.85)	54.26	56.55	-2.29	(1.34)
Washington DC	DC	45.10	41.84	3.27	(1.34)	57.58	54.80	2.78	(1.04)
Houston	TX	44.25	45.04	-0.78	(1.30)	57.24	56.58	0.66	(1.22)

Notes: Table presents estimates of the sorting and causal effects of several CZs. Column (1) presents the permanent resident average child income rank at age 26 for those with below-median parent income rank (p25). Column (3) presents the estimated causal component, which equals 20*u, where u is the estimated causal effect of an additional year of exposure (evaluated at p=25). Column (2) presents the sorting component, which equals column (1) - column (3). Column (4) presents the standard error of the estimated causal effect, which equals 20 times the standard error of the estimated per-year causal effect. Columns (5)-(8) repeat columns (1)-(4) evaluating the estimates for children in above-median income families (p75).

Appendix Table VII

Model Variance Components: Robustness to Alernative T_C Assumptions

	Commuting Zones		Counties		County within CZ	
	Below Median (p25)	Above Median (p75)	Below Median (p25)	Above Median (p75)	Below Median (p25)	Above Median (p75)
Model Component	(1)	(2)	(3)	(4)	(5)	(6)
Baseline (T_C=20)						
Sorting vs. Causal Components (T_C=20 yrs)						
Causal Effect (SD of Signal)	2.647	2.139	3.308	3.092	1.984	2.233
Permanent Residents (SD)	3.259	2.585	4.203	3.257	2.653	1.982
Sorting Component (SD)	1.960	1.097	3.033	3.203	2.315	3.009
Correlation between Sorting and Causal Effect	-0.021	0.193	-0.123	-0.465	-0.246	-0.753
Full 23 years (T_C=23)						
Sorting vs. Causal Components (T_C=23 yrs)						
Causal Effect (SD of Signal)	3.044	2.459	3.804	3.556	2.281	2.568
Permanent Residents (SD)	3.259	2.585	4.203	3.257	2.653	1.982
Sorting Component (SD)	2.008	1.082	3.133	3.443	2.406	3.269
Correlation between Sorting and Causal Effect	-0.219	-0.101	-0.278	-0.567	-0.360	-0.795
Observed Exposure (T_C=12)						
Sorting vs. Causal Components (T_C=12 yrs)						
Causal Effect (SD of Signal)	1.588	1.283	1.985	1.855	1.190	1.340
Permanent Residents (SD)	3.259	2.585	4.203	3.257	2.653	1.982
Sorting Component (SD)	2.207	1.515	3.156	2.847	2.256	2.410
Correlation between Sorting and Causal Effect	0.461	0.704	0.301	-0.089	0.100	-0.569

Notes: Table presents estimated model variance components in Panel B of Table VII for alternative assumptions of the number of years of exposure corresponding to "full" exposure. The first set of results presents the baseline (20 years). See notes to Table VII for details on the calculation of these statistics. The second set of results report the estimates under the assumption that permanent residents obtain 23 years of exposure. The lower set of estimates assume individuals obtain 12 years of exposure, which is the number of years we observe in our data for outcomes at age 26.

Appendix Table VIII
Predicted Place Effects for 50 Largest CZs for Below-Median Income Parents (p25) Individual Income

Commuting Zone	State	Male Individual Income			Female Individual Income			Pooled Spec			Average			Row Number
		Prediction	RMSE	% Increase	Prediction	RMSE	% Increase	Prediction	RMSE	% Increase	Prediction	RMSE	% Increase	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	
Minneapolis	MN	0.186	0.139	0.537	0.170	0.091	0.600	0.161	0.070	0.530	0.178	0.166	0.586	(1)
Newark	NJ	0.156	0.090	0.450	0.144	0.068	0.508	0.151	0.052	0.497	0.150	0.113	0.494	(2)
Seattle	WA	0.154	0.107	0.446	0.110	0.080	0.387	0.140	0.064	0.462	0.132	0.133	0.435	(3)
Boston	MA	0.148	0.113	0.428	0.105	0.082	0.369	0.151	0.062	0.499	0.127	0.140	0.416	(4)
Washington DC	DC	0.078	0.102	0.225	0.148	0.076	0.522	0.136	0.058	0.448	0.113	0.127	0.372	(5)
Cleveland	OH	0.179	0.129	0.518	0.027	0.088	0.095	0.048	0.072	0.158	0.103	0.156	0.339	(6)
Buffalo	NY	0.164	0.133	0.473	0.027	0.088	0.097	0.118	0.072	0.387	0.096	0.159	0.315	(7)
San Francisco	CA	0.003	0.108	0.008	0.135	0.078	0.477	0.070	0.062	0.230	0.069	0.133	0.228	(8)
Philadelphia	PA	-0.077	0.096	-0.222	0.203	0.073	0.716	0.081	0.060	0.268	0.063	0.120	0.208	(9)
Fort Worth	TX	0.104	0.116	0.301	-0.012	0.081	-0.043	0.036	0.061	0.120	0.046	0.142	0.152	(10)
Pittsburgh	PA	0.067	0.142	0.194	0.012	0.091	0.043	0.037	0.073	0.123	0.040	0.168	0.131	(11)
Las Vegas	NV	-0.060	0.096	-0.173	0.137	0.072	0.485	0.049	0.058	0.160	0.039	0.120	0.127	(12)
Portland	OR	0.122	0.133	0.353	-0.049	0.088	-0.171	0.017	0.074	0.056	0.037	0.159	0.122	(13)
Providence	RI	0.056	0.141	0.162	0.015	0.091	0.054	0.048	0.075	0.157	0.036	0.168	0.118	(14)
San Jose	CA	-0.083	0.122	-0.239	0.119	0.084	0.419	0.043	0.068	0.142	0.018	0.148	0.059	(15)
Manchester	NH	0.054	0.148	0.157	-0.020	0.093	-0.071	0.039	0.078	0.129	0.017	0.175	0.056	(16)
Bridgeport	CT	-0.057	0.117	-0.165	0.084	0.082	0.297	0.056	0.063	0.183	0.014	0.143	0.045	(17)
Phoenix	AZ	-0.031	0.088	-0.090	0.047	0.069	0.167	0.010	0.053	0.033	0.008	0.112	0.027	(18)
Denver	CO	0.009	0.124	0.026	-0.006	0.086	-0.020	-0.016	0.066	-0.051	0.002	0.151	0.005	(19)
New York	NY	-0.043	0.069	-0.123	0.037	0.056	0.132	0.017	0.039	0.054	-0.003	0.089	-0.009	(20)
Grand Rapids	MI	0.090	0.156	0.259	-0.095	0.095	-0.335	-0.048	0.080	-0.159	-0.003	0.183	-0.009	(21)
Columbus	OH	0.055	0.142	0.159	-0.072	0.090	-0.252	-0.085	0.072	-0.279	-0.008	0.168	-0.027	(22)
San Diego	CA	-0.011	0.104	-0.033	-0.019	0.077	-0.068	-0.007	0.057	-0.024	-0.015	0.129	-0.050	(23)
Cincinnati	OH	-0.042	0.144	-0.120	0.009	0.091	0.033	-0.037	0.076	-0.122	-0.016	0.171	-0.053	(24)
Sacramento	CA	-0.110	0.107	-0.316	0.075	0.078	0.266	-0.005	0.057	-0.015	-0.017	0.132	-0.056	(25)
Salt Lake City	UT	-0.029	0.141	-0.085	-0.035	0.093	-0.123	-0.010	0.075	-0.032	-0.032	0.168	-0.106	(26)
Milwaukee	WI	-0.103	0.146	-0.298	0.015	0.093	0.054	0.028	0.073	0.094	-0.044	0.173	-0.145	(27)
Miami	FL	-0.164	0.088	-0.472	0.074	0.068	0.262	-0.015	0.055	-0.049	-0.045	0.112	-0.147	(28)
St. Louis	MO	-0.073	0.141	-0.211	-0.017	0.090	-0.059	-0.037	0.073	-0.123	-0.045	0.167	-0.148	(29)
Dayton	OH	-0.064	0.156	-0.184	-0.027	0.095	-0.096	-0.069	0.078	-0.227	-0.046	0.183	-0.150	(30)
Jacksonville	FL	0.013	0.126	0.039	-0.108	0.085	-0.380	-0.042	0.069	-0.137	-0.047	0.152	-0.155	(31)
Kansas City	MO	-0.072	0.144	-0.207	-0.038	0.092	-0.132	-0.034	0.075	-0.111	-0.055	0.170	-0.180	(32)
Dallas	TX	-0.165	0.100	-0.475	0.045	0.074	0.158	-0.062	0.056	-0.204	-0.060	0.125	-0.197	(33)
Houston	TX	-0.067	0.096	-0.195	-0.059	0.071	-0.209	-0.087	0.056	-0.286	-0.063	0.119	-0.209	(34)
Austin	TX	-0.091	0.133	-0.262	-0.043	0.089	-0.151	-0.114	0.074	-0.376	-0.067	0.160	-0.220	(35)
Indianapolis	IN	-0.069	0.145	-0.200	-0.070	0.092	-0.247	-0.064	0.075	-0.212	-0.070	0.171	-0.229	(36)
Chicago	IL	-0.193	0.085	-0.557	0.038	0.066	0.134	-0.059	0.053	-0.195	-0.077	0.107	-0.255	(37)
Nashville	TN	-0.098	0.148	-0.283	-0.064	0.092	-0.225	-0.109	0.076	-0.360	-0.081	0.174	-0.266	(38)
Detroit	MI	-0.198	0.109	-0.570	-0.006	0.078	-0.021	-0.113	0.061	-0.371	-0.102	0.135	-0.335	(39)
Baltimore	MD	-0.262	0.122	-0.757	0.031	0.085	0.109	-0.056	0.069	-0.184	-0.116	0.149	-0.380	(40)
Tampa	FL	-0.195	0.094	-0.563	-0.039	0.071	-0.137	-0.115	0.054	-0.380	-0.117	0.118	-0.385	(41)
Charlotte	NC	-0.191	0.121	-0.550	-0.058	0.083	-0.206	-0.129	0.069	-0.424	-0.124	0.147	-0.410	(42)
San Antonio	TX	-0.178	0.123	-0.513	-0.085	0.084	-0.298	-0.136	0.070	-0.448	-0.131	0.149	-0.432	(43)
Los Angeles	CA	-0.199	0.060	-0.573	-0.082	0.050	-0.289	-0.138	0.037	-0.454	-0.140	0.078	-0.462	(44)
Port St. Lucie	FL	-0.272	0.116	-0.786	-0.010	0.081	-0.037	-0.152	0.063	-0.502	-0.141	0.141	-0.465	(45)
Orlando	FL	-0.269	0.093	-0.775	-0.042	0.072	-0.149	-0.129	0.054	-0.424	-0.155	0.117	-0.512	(46)
Fresno	CA	-0.232	0.121	-0.670	-0.088	0.084	-0.309	-0.152	0.070	-0.501	-0.160	0.148	-0.526	(47)
Raleigh	NC	-0.239	0.128	-0.690	-0.086	0.086	-0.304	-0.202	0.067	-0.666	-0.163	0.154	-0.535	(48)
Atlanta	GA	-0.229	0.079	-0.660	-0.098	0.062	-0.344	-0.158	0.044	-0.520	-0.163	0.100	-0.537	(49)
New Orleans	LA	-0.223	0.137	-0.643	-0.133	0.088	-0.468	-0.197	0.070	-0.649	-0.178	0.163	-0.585	(50)

Notes: This table presents per-year exposure effect predictions on individual income by gender for the 50 largest CZs. Estimates are for children in below-median (p25) income families. Column (1) reports the predictions for the child's individual income rank at age 26. Column (2) reports the root mean square error for this prediction, computed as the square root of 1/(1/v_r + 1/v)) where v_r is the residual signal variance and v is the squared standard error of the fixed effect estimate. Column (3) scales the numbers to the percentage dollar increase by multiplying the estimates in column (1) by the regression coefficient from regressing the permanent resident outcomes at p25 for child individual income at age 26 on the analogous outcomes for child rank at age 26 divided by the mean individual income of children from below-median (p25) income families. Columns (4)-(6) repeat the analysis on the sample of female children. Columns (7)-(9) report the pooled gender forecasts. Columns (10) reports the average of the two gender-specific forecasts. Column (11) reports the rmse of this forecast, constructed as the square root of the sum of the squared male and female rmse divided by two. Column (12) scales this to the percentage increase in incomes using the same scaling factors as in Column (9). The rows are sorted in decending order according to the gender-average specification.

Appendix Table IX

Predicted Place Effects for 100 Largest Counties (Top and Bottom 25 based on Individual Income Rank)

		Male Individual Income			Female Individual Income			Pooled Spec			Average			Row Number
		Prediction	RMSE	% Increase	Prediction	RMSE	% Increase	Prediction	RMSE	% Increase	Prediction	RMSE	% Increase	
County	State	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	
Bergen	NJ	0.351	0.192	1.014	0.213	0.080	0.752	0.288	0.099	0.949	0.282	0.208	0.930	(1)
Norfolk	MA	0.308	0.188	0.889	0.190	0.080	0.671	0.274	0.098	0.902	0.249	0.204	0.820	(2)
Middlesex	NJ	0.263	0.194	0.760	0.159	0.080	0.560	0.216	0.100	0.713	0.211	0.210	0.695	(3)
Dupage	IL	0.234	0.159	0.676	0.149	0.076	0.524	0.217	0.089	0.714	0.191	0.177	0.630	(4)
Hudson	NJ	0.279	0.190	0.806	0.103	0.080	0.363	0.169	0.098	0.556	0.191	0.206	0.629	(5)
Bucks	PA	0.251	0.188	0.726	0.115	0.080	0.404	0.200	0.099	0.658	0.183	0.204	0.602	(6)
Fairfax	VA	0.153	0.190	0.443	0.197	0.080	0.694	0.229	0.099	0.754	0.175	0.206	0.576	(7)
Middlesex	MA	0.228	0.162	0.659	0.119	0.077	0.421	0.179	0.089	0.588	0.174	0.179	0.573	(8)
Montgomery	MD	0.164	0.186	0.475	0.177	0.079	0.624	0.168	0.097	0.554	0.171	0.202	0.562	(9)
King	WA	0.205	0.141	0.592	0.134	0.074	0.471	0.215	0.082	0.708	0.169	0.159	0.557	(10)
Ventura	CA	0.278	0.184	0.802	0.048	0.080	0.170	0.122	0.098	0.401	0.163	0.200	0.537	(11)
Contra Costa	CA	0.217	0.170	0.627	0.095	0.078	0.334	0.142	0.092	0.467	0.156	0.187	0.514	(12)
Suffolk	NY	0.214	0.168	0.618	0.096	0.078	0.338	0.136	0.091	0.449	0.155	0.185	0.511	(13)
Monmouth	NJ	0.156	0.193	0.449	0.121	0.080	0.427	0.149	0.100	0.492	0.138	0.209	0.455	(14)
Snohomish	WA	0.185	0.179	0.533	0.041	0.079	0.144	0.149	0.096	0.489	0.113	0.196	0.371	(15)
Worcester	MA	0.143	0.204	0.413	0.080	0.081	0.283	0.152	0.103	0.499	0.112	0.220	0.367	(16)
Erie	NY	0.214	0.210	0.616	0.004	0.082	0.014	0.069	0.105	0.226	0.109	0.225	0.358	(17)
Nassau	NY	0.103	0.151	0.298	0.110	0.076	0.389	0.081	0.085	0.266	0.107	0.169	0.351	(18)
Prince Georges	MD	0.126	0.173	0.363	0.079	0.078	0.279	0.043	0.093	0.143	0.102	0.190	0.337	(19)
Providence	RI	0.163	0.191	0.470	0.041	0.080	0.144	0.085	0.099	0.280	0.102	0.208	0.335	(20)
San Mateo	CA	0.057	0.192	0.166	0.140	0.080	0.494	0.122	0.099	0.402	0.099	0.208	0.325	(21)
Macomb	MI	0.160	0.159	0.462	0.014	0.076	0.051	0.071	0.088	0.235	0.087	0.177	0.287	(22)
Hartford	CT	0.081	0.193	0.234	0.068	0.080	0.241	0.081	0.100	0.267	0.075	0.209	0.246	(23)
Suffolk	MA	0.116	0.175	0.334	0.019	0.078	0.066	0.006	0.093	0.020	0.067	0.192	0.221	(24)
San Francisco	CA	-0.032	0.186	-0.093	0.162	0.079	0.572	0.109	0.098	0.359	0.065	0.202	0.214	(25)
Bronx	NY	-0.192	0.132	-0.556	0.025	0.072	0.090	-0.058	0.076	-0.191	-0.084	0.150	-0.275	(75)
Tulsa	OK	-0.121	0.188	-0.348	-0.057	0.079	-0.200	-0.052	0.097	-0.171	-0.089	0.204	-0.292	(76)
Cook	IL	-0.191	0.098	-0.551	0.001	0.063	0.003	-0.081	0.061	-0.268	-0.095	0.116	-0.313	(77)
Gwinnett	GA	-0.221	0.166	-0.637	0.022	0.077	0.078	-0.047	0.090	-0.155	-0.099	0.183	-0.326	(78)
Marion	IN	-0.132	0.173	-0.380	-0.085	0.078	-0.300	-0.113	0.091	-0.373	-0.108	0.189	-0.357	(79)
Jefferson	KY	-0.157	0.196	-0.452	-0.071	0.081	-0.251	-0.136	0.099	-0.446	-0.114	0.212	-0.375	(80)
Hillsborough	FL	-0.208	0.152	-0.601	-0.030	0.076	-0.105	-0.128	0.086	-0.421	-0.119	0.170	-0.392	(81)
Wayne	MI	-0.231	0.138	-0.667	-0.016	0.073	-0.057	-0.102	0.078	-0.335	-0.124	0.156	-0.407	(82)
Los Angeles	CA	-0.203	0.070	-0.585	-0.054	0.052	-0.192	-0.144	0.044	-0.474	-0.129	0.087	-0.423	(83)
Montgomery	OH	-0.183	0.195	-0.528	-0.080	0.080	-0.281	-0.137	0.099	-0.451	-0.131	0.211	-0.432	(84)
Travis	TX	-0.226	0.159	-0.653	-0.041	0.076	-0.144	-0.169	0.089	-0.556	-0.134	0.176	-0.440	(85)
Mecklenburg	NC	-0.243	0.173	-0.701	-0.037	0.078	-0.130	-0.147	0.094	-0.484	-0.140	0.190	-0.460	(86)
Milwaukee	WI	-0.262	0.180	-0.756	-0.025	0.079	-0.087	-0.081	0.093	-0.268	-0.143	0.197	-0.472	(87)
Palm Beach	FL	-0.280	0.150	-0.809	-0.006	0.076	-0.023	-0.153	0.084	-0.505	-0.143	0.168	-0.472	(88)
Bexar	TX	-0.255	0.180	-0.735	-0.042	0.080	-0.149	-0.155	0.088	-0.509	-0.148	0.197	-0.489	(89)
Bernalillo	NM	-0.280	0.178	-0.807	-0.023	0.079	-0.080	-0.089	0.089	-0.292	-0.151	0.195	-0.497	(90)
Cobb	GA	-0.243	0.175	-0.702	-0.064	0.078	-0.227	-0.152	0.094	-0.500	-0.154	0.192	-0.506	(91)
Wake	NC	-0.274	0.189	-0.790	-0.043	0.079	-0.151	-0.190	0.097	-0.627	-0.158	0.205	-0.521	(92)
Fresno	CA	-0.235	0.158	-0.679	-0.082	0.076	-0.289	-0.165	0.089	-0.542	-0.159	0.175	-0.522	(93)
Orange	FL	-0.339	0.128	-0.979	0.003	0.071	0.012	-0.120	0.074	-0.395	-0.168	0.147	-0.553	(94)
San Bernardino	CA	-0.218	0.099	-0.629	-0.119	0.064	-0.420	-0.186	0.062	-0.612	-0.168	0.118	-0.555	(95)
Fulton	GA	-0.291	0.134	-0.840	-0.079	0.072	-0.280	-0.168	0.077	-0.553	-0.185	0.152	-0.610	(96)
Pima	AZ	-0.367	0.159	-1.059	-0.014	0.077	-0.048	-0.112	0.085	-0.369	-0.190	0.177	-0.626	(97)
Riverside	CA	-0.277	0.109	-0.798	-0.116	0.067	-0.408	-0.213	0.066	-0.701	-0.196	0.128	-0.646	(98)
Jefferson	AL	-0.341	0.190	-0.985	-0.098	0.080	-0.344	-0.173	0.098	-0.570	-0.219	0.206	-0.722	(99)
Baltimore City	MD	-0.487	0.157	-1.405	0.014	0.076	0.048	-0.140	0.088	-0.460	-0.237	0.175	-0.779	(100)

Notes: This table presents per-year exposure effect predictions on individual income by gender for the top 25 and bottom 25 amongst the 100 largest counties. Estimates are for children in below-median (p25) income families. Column (1) reports the predictions for the child's individual income rank at age 26. Column (2) reports the root mean square error for this prediction, computed as the square root of 1/(1/v_r + 1/v)) where v_r is the residual signal variance and v is the squared standard error of the fixed effect estimate. Column (3) scales the numbers to the percentage dollar increase by multiplying the estimates in column (1) by the regression coefficient from regressing the permanent resident outcomes at p25 for child individual income at age 26 on the analogous outcomes for child rank at age 26 divided by the mean individual income of children from below-median (p25) income families. Columns (4)-(6) repeat the analysis on the sample of female children. Columns (7)-(9) report the pooled gender forecasts. Columns (10) reports the average of the two gender-specific forecasts. Column (11) reports the rmse of this forecast, constructed as the square root of the sum of the squared male and female rmse divided by two. Column (12) scales this to the percentage increase in incomes using the same scaling factors as in Column (9). The rows are sorted in decending order according to the gender-average specification.

Appendix Table X

Regressions of Place Effects For Males Across Commuting Zones on Selected Covariates (Below-Median Income Parents (p25))

		Standard Deviation of Covariate	Exposure Effect Correlation		Regression Decomposition on Model Components					
					Permanent Residents		Causal (20 years)		Sorting	
		(1)	(2)		(3)		(4)		(5)	
		Std. Dev	Correlation	s.e.	Coeff	(s.e.)	Coeff	(s.e.)	Coeff	(s.e.)
Segregation and Poverty	Fraction Black Residents	0.100	-0.351	(0.122)	-2.683	(0.260)	-1.494	(0.519)	-1.153	(0.446)
	Poverty Rate	0.041	-0.018	(0.137)	-0.351	(0.325)	-0.076	(0.583)	-0.290	(0.597)
	Racial Segregation Theil Index	0.107	-0.479	(0.100)	-2.049	(0.243)	-2.041	(0.427)	0.045	(0.391)
	Income Segregation Theil Index	0.034	-0.574	(0.119)	-1.665	(0.316)	-2.444	(0.506)	0.853	(0.481)
	Segregation of Poverty (<p25)	0.030	-0.539	(0.124)	-1.789	(0.287)	-2.295	(0.526)	0.578	(0.474)
	Segregation of Affluence (>p75)	0.039	-0.587	(0.115)	-1.548	(0.334)	-2.501	(0.491)	1.029	(0.487)
	Share with Commute < 15 Mins	0.094	0.790	(0.106)	2.187	(0.302)	3.364	(0.450)	-1.190	(0.394)
	Log. Population Density	1.370	-0.569	(0.119)	-1.675	(0.357)	-2.423	(0.505)	0.810	(0.372)
Income Distribution	Household Income per Capita for Working-Age Adults	6,943	-0.358	(0.127)	-0.755	(0.309)	-1.526	(0.543)	0.811	(0.424)
	Gini coefficient for Parent Income	0.083	-0.636	(0.130)	-1.798	(0.501)	-2.710	(0.555)	0.965	(0.482)
	Top 1% Income Share for Parents	5.029	-0.478	(0.113)	-0.845	(0.322)	-2.035	(0.483)	1.226	(0.389)
	Gini Bottom 99%	0.054	-0.531	(0.085)	-1.962	(0.400)	-2.260	(0.363)	0.346	(0.492)
	Fraction Middle Class (Between National p25 and p75)	0.061	0.606	(0.119)	2.074	(0.403)	2.581	(0.505)	-0.569	(0.539)
Tax	Local Tax Rate	0.006	-0.105	(0.137)	-0.267	(0.334)	-0.446	(0.584)	0.116	(0.483)
	Local Tax Rate per Capita	0.328	-0.304	(0.150)	-0.621	(0.374)	-1.293	(0.637)	0.663	(0.511)
	Local Government Expenditures per Capita	680.2	-0.265	(0.141)	-0.179	(0.320)	-1.127	(0.601)	0.938	(0.674)
	State EITC Exposure	3.709	0.198	(0.168)	0.751	(0.325)	0.842	(0.715)	-0.083	(0.504)
	State Income Tax Progressivity	2.337	-0.110	(0.148)	0.452	(0.223)	-0.469	(0.629)	0.935	(0.599)
K-12 Education	School Expenditure per Student	1.312	-0.050	(0.106)	0.043	(0.296)	-0.213	(0.450)	0.252	(0.447)
	Student/Teacher Ratio	2.678	-0.348	(0.090)	-0.183	(0.398)	-1.481	(0.384)	1.384	(0.461)
	Test Score Percentile (Controlling for Parent Income)	7.197	0.497	(0.094)	1.005	(0.663)	2.116	(0.402)	-1.178	(0.619)
	High School Dropout Rate (Controlling for Parent Income)	0.016	-0.421	(0.113)	-1.718	(0.363)	-1.791	(0.481)	0.134	(0.438)
College	Number of Colleges per Capita	0.007	0.647	(0.127)	0.877	(0.257)	2.754	(0.539)	-1.820	(0.542)
	Mean College Tuition	3,315	-0.079	(0.094)	-0.268	(0.308)	-0.335	(0.401)	0.097	(0.445)
	College Graduation Rate (Controlling for Parent Income)	0.104	0.222	(0.103)	0.696	(0.312)	0.947	(0.438)	-0.258	(0.389)
Local Labor Market	Labor Force Participation Rate	0.047	0.100	(0.149)	0.072	(0.343)	0.426	(0.633)	-0.328	(0.491)
	Fraction Working in Manufacturing	0.062	0.118	(0.128)	-0.022	(0.347)	0.503	(0.546)	-0.480	(0.398)
	Growth in Chinese Imports 1990-2000 (Autor and Dorn 2013)	0.979	0.058	(0.092)	0.266	(0.256)	0.249	(0.390)	0.060	(0.319)
	Teenage (14-16) Labor Force Participation Rate	0.101	0.429	(0.123)	1.388	(0.462)	1.826	(0.522)	-0.496	(0.659)
Migration	Migration Inflow Rate	0.011	-0.214	(0.108)	-0.134	(0.308)	-0.912	(0.459)	0.832	(0.373)
	Migration Outflow Rate	0.007	-0.177	(0.107)	-0.092	(0.298)	-0.753	(0.457)	0.712	(0.448)
	Fraction of Foreign Born Residents	0.100	-0.457	(0.093)	-0.238	(0.330)	-1.946	(0.398)	1.745	(0.470)
Social Capital	Social Capital Index (Rupasingha and Goetz 2008)	0.934	0.613	(0.105)	1.412	(0.400)	2.609	(0.447)	-1.260	(0.552)
	Fraction Religious	0.107	0.142	(0.145)	1.163	(0.394)	0.603	(0.616)	0.512	(0.431)
	Violent Crime Rate	0.001	-0.527	(0.086)	-1.168	(0.598)	-2.244	(0.366)	1.120	(0.597)
Family Structure	Fraction of Children with Single Mothers	0.036	-0.323	(0.117)	-2.584	(0.341)	-1.374	(0.499)	-1.156	(0.483)
	Fraction of Adults Divorced	0.015	0.104	(0.128)	-0.596	(0.341)	0.441	(0.546)	-0.994	(0.429)
	Fraction of Adults Married	0.033	0.379	(0.120)	1.825	(0.359)	1.613	(0.509)	0.136	(0.484)
Prices	Median House Prices	82,847	-0.354	(0.100)	-0.175	(0.331)	-1.507	(0.426)	1.389	(0.316)
	Median Monthly Rent	206.7	-0.466	(0.113)	-0.568	(0.373)	-1.982	(0.482)	1.490	(0.431)

Notes: This table replicates Table XII in the text using Place Effects and Permanent Residents characteristics For Males Only

Appendix Table XI
Regressions of Place Effects for Males Across Counties within Commuting Zones on Selected Covariates (Below-Median Income Parents (p25))

		Standard Deviation of Covariate (1)	Exposure Effect Correlation (2)		Regression Decomposition on Model Components: Permanent Residents (3)		Causal (20 years) (4)		Sorting 5)	
		Std. Dev	Correlation	s.e.	Coeff	(s.e.)	Coeff	(s.e.)	Coeff	(s.e.)
Segregation and Poverty	Fraction Black Residents	0.130	0.059	(0.206)	-2.394	(0.202)	0.211	(0.734)	-2.608	(0.742)
	Poverty Rate	0.055	0.016	(0.192)	-1.983	(0.271)	0.056	(0.684)	-2.048	(0.778)
	Racial Segregation Theil Index	0.118	-0.232	(0.083)	-2.442	(0.163)	-0.824	(0.294)	-1.622	(0.310)
	Income Segregation Theil Index	0.039	-0.381	(0.129)	-1.919	(0.129)	-1.355	(0.460)	-0.568	(0.469)
	Segregation of Poverty (<p25)	0.034	-0.401	(0.130)	-2.028	(0.142)	-1.427	(0.464)	-0.607	(0.479)
	Segregation of Affluence (>p75)	0.045	-0.342	(0.127)	-1.686	(0.138)	-1.217	(0.451)	-0.469	(0.449)
	Share with Commute < 15 Mins	0.102	-0.197	(0.264)	0.301	(0.201)	-0.700	(0.940)	1.014	(0.941)
	Log. Population Density	1.718	-0.291	(0.128)	-2.039	(0.296)	-1.037	(0.456)	-1.011	(0.504)
Income Distribution	Household Income per Capita for Working-Age Adults	9,222	-0.127	(0.164)	0.729	(0.252)	-0.453	(0.585)	1.181	(0.615)
	Gini coefficient for Parent Income	0.113	-0.226	(0.108)	-2.102	(0.494)	-0.804	(0.384)	-1.294	(0.424)
	Top 1% Income Share for Parents	0.064	-0.076	(0.098)	-1.093	(0.326)	-0.270	(0.350)	-0.819	(0.402)
	Gini Bottom 99%	0.112	-0.227	(0.108)	-2.105	(0.493)	-0.806	(0.384)	-1.295	(0.424)
	Fraction Middle Class (Between National p25 and p75)	0.074	0.060	(0.154)	0.872	(0.275)	0.213	(0.548)	0.647	(0.563)
Tax	Local Tax Rate	0.009	-0.241	(0.294)	-0.975	(0.687)	-0.858	(1.046)	-0.131	(0.841)
	Local Tax Rate per Capita	0.432	-0.204	(0.281)	-0.530	(0.586)	-0.728	(0.999)	0.190	(0.760)
	Local Government Expenditures per Capita	1.019	-0.212	(0.211)	-1.106	(0.611)	-0.755	(0.751)	-0.368	(0.587)
	State EITC Exposure	3.750	-0.061	(0.121)	-0.067	(0.059)	-0.218	(0.432)	0.142	(0.440)
	State Income Tax Progressivity	2.365	-0.073	(0.167)	-0.101	(0.138)	-0.260	(0.594)	0.159	(0.649)
K-12 Education	School Expenditure per Student	1.483	0.129	(0.195)	-0.472	(0.424)	0.459	(0.695)	-0.951	(0.777)
	Student/Teacher Ratio	2.816	-0.587	(0.481)	-0.549	(0.226)	-2.089	(1.712)	1.554	(1.760)
	Test Score Percentile (Controlling for Parent Income)	9.610	0.141	(0.128)	1.879	(0.417)	0.503	(0.456)	1.381	(0.537)
	High School Dropout Rate (Controlling for Parent Income)	0.024	-0.412	(0.289)	-1.920	(0.248)	-1.465	(1.028)	-0.465	(1.052)
College	Number of Colleges per Capita	0.011	0.258	(0.157)	-0.444	(0.199)	0.917	(0.557)	-1.419	(0.531)
	Mean College Tuition	4,421	0.030	(0.122)	-0.365	(0.267)	0.106	(0.434)	-0.473	(0.488)
	College Graduation Rate (Controlling for Parent Income)	0.139	0.197	(0.171)	-0.617	(0.206)	0.701	(0.608)	-1.318	(0.597)
Local Labor Market	Labor Force Participation Rate	0.058	-0.327	(0.297)	0.882	(0.261)	-1.164	(1.056)	2.076	(1.039)
	Fraction Working in Manufacturing	0.070	0.492	(0.280)	1.119	(0.163)	1.752	(0.998)	-0.622	(1.010)
	Teenage (14-16) Labor Force Participation Rate	0.108	-0.345	(0.460)	1.020	(0.228)	-1.229	(1.636)	2.250	(1.629)
Migration	Migration Inflow Rate	0.019	-0.144	(0.120)	1.104	(0.250)	-0.512	(0.427)	1.627	(0.487)
	Migration Outflow Rate	0.014	-0.129	(0.137)	0.071	(0.244)	-0.458	(0.488)	0.541	(0.515)
	Fraction of Foreign Born Residents	0.109	-0.018	(0.074)	-0.776	(0.246)	-0.064	(0.263)	-0.709	(0.369)
Social Capital	Social Capital Index (Rupasingha and Goetz 2008)	1.096	0.045	(0.126)	-0.146	(0.246)	0.159	(0.448)	-0.321	(0.478)
	Fraction Religious	0.128	0.176	(0.159)	-0.031	(0.185)	0.627	(0.565)	-0.673	(0.563)
	Violent Crime Rate	0.002	-0.176	(0.085)	-1.804	(0.170)	-0.626	(0.302)	-1.181	(0.339)
Family Structure	Fraction of Children with Single Mothers	0.069	-0.222	(0.130)	-2.613	(0.316)	-0.789	(0.464)	-1.820	(0.541)
	Fraction of Adults Divorced	0.017	-0.227	(0.428)	-1.777	(0.174)	-0.806	(1.524)	-0.977	(1.523)
	Fraction of Adults Married	0.062	0.073	(0.138)	2.551	(0.168)	0.260	(0.491)	2.290	(0.530)
Prices	Median House Price	124,001	-0.067	(0.087)	0.160	(0.378)	-0.239	(0.310)	0.401	(0.612)
	Median Monthly Rent	217.8	-0.160	(0.213)	0.608	(0.246)	-0.568	(0.759)	1.194	(0.724)

Notes: This table replicates Table XIV in the text using Place Effects and Permanent Residents characteristics For Males Only

Appendix Table XII

Regressions of Place Effects for Females Across Commuting Zones on Selected Covariates (Below-Median Income Parents (p25))

		Standard Deviation of Covariate	Exposure Effect Correlation		Regression Decomposition on Model Components					
					Permanent Residents		Causal (20 years)		Sorting	
		(1)	(2)		(3)		(4)		(5)	
		Std. Dev	Correlation	s.e.	Coeff	(s.e.)	Coeff	(s.e.)	Coeff	(s.e.)
Segregation and Poverty	Fraction Black Residents	0.100	-0.430	(0.125)	-2.163	(0.242)	-1.371	(0.398)	-0.763	(0.428)
	Poverty Rate	0.041	-0.137	(0.113)	-0.756	(0.289)	-0.436	(0.361)	-0.324	(0.437)
	Racial Segregation Theil Index	0.107	-0.348	(0.147)	-1.344	(0.262)	-1.110	(0.468)	-0.185	(0.519)
	Income Segregation Theil Index	0.034	-0.312	(0.150)	-0.620	(0.304)	-0.995	(0.480)	0.448	(0.528)
	Segregation of Poverty (<p25)	0.030	-0.321	(0.157)	-0.787	(0.281)	-1.023	(0.500)	0.306	(0.525)
	Segregation of Affluence (>p75)	0.039	-0.297	(0.142)	-0.506	(0.312)	-0.949	(0.455)	0.515	(0.515)
	Share with Commute < 15 Mins	0.094	0.608	(0.175)	1.081	(0.350)	1.940	(0.558)	-0.914	(0.577)
	Log. Population Density	1.368	-0.503	(0.129)	-0.619	(0.340)	-1.603	(0.413)	1.062	(0.484)
Income Distribution	Household Income per Capita for Working-Age Adults	6,943	-0.135	(0.133)	0.310	(0.266)	-0.429	(0.424)	0.774	(0.380)
	Gini coefficient for Parent Income	0.083	-0.540	(0.132)	-0.984	(0.502)	-1.722	(0.420)	0.788	(0.545)
	Top 1% Income Share for Parents	5.028	-0.302	(0.114)	0.139	(0.266)	-0.963	(0.364)	1.148	(0.347)
	Gini Bottom 99%	0.054	-0.545	(0.150)	-1.634	(0.388)	-1.739	(0.477)	0.148	(0.559)
	Fraction Middle Class (Between National p25 and p75)	0.061	0.485	(0.165)	1.171	(0.416)	1.548	(0.527)	-0.448	(0.580)
Tax	Local Tax Rate	0.006	-0.107	(0.137)	0.236	(0.286)	-0.342	(0.437)	0.544	(0.436)
	Local Tax Rate per Capita	0.328	-0.174	(0.172)	0.412	(0.321)	-0.557	(0.549)	0.966	(0.519)
	Local Government Expenditures per Capita	676.9	-0.044	(0.134)	0.635	(0.251)	-0.140	(0.427)	0.778	(0.441)
	State EITC Exposure	3.709	-0.039	(0.178)	0.845	(0.279)	-0.124	(0.566)	0.972	(0.484)
	State Income Tax Progressivity	2.337	0.067	(0.101)	0.729	(0.210)	0.214	(0.323)	0.527	(0.343)
K-12 Education	School Expenditure per Student	1.312	-0.060	(0.176)	0.449	(0.293)	-0.191	(0.562)	0.639	(0.503)
	Student/Teacher Ratio	2.678	-0.004	(0.128)	0.276	(0.373)	-0.013	(0.410)	0.358	(0.510)
	Test Score Percentile (Controlling for Parent Income)	7.196	0.167	(0.123)	0.568	(0.666)	0.534	(0.391)	-0.032	(0.591)
	High School Dropout Rate (Controlling for Parent Income)	0.016	-0.372	(0.156)	-1.543	(0.330)	-1.187	(0.499)	-0.300	(0.452)
College	Number of Colleges per Capita	0.007	0.399	(0.144)	0.241	(0.267)	1.272	(0.460)	-1.066	(0.413)
	Mean College Tuition	3,315	-0.307	(0.111)	0.031	(0.267)	-0.978	(0.356)	1.029	(0.387)
	College Graduation Rate (Controlling for Parent Income)	0.104	-0.058	(0.101)	0.354	(0.235)	-0.184	(0.322)	0.523	(0.317)
Local Labor Market	Labor Force Participation Rate	0.047	0.095	(0.114)	0.488	(0.259)	0.304	(0.362)	0.198	(0.366)
	Fraction Working in Manufacturing	0.062	-0.018	(0.159)	-0.424	(0.274)	-0.059	(0.508)	-0.343	(0.560)
	Growth in Chinese Imports 1990-2000 (Autor and Dorn 2013)	0.979	-0.118	(0.129)	0.106	(0.218)	-0.375	(0.411)	0.515	(0.359)
	Teenage (14-16) Labor Force Participation Rate	0.101	0.296	(0.170)	1.196	(0.483)	0.945	(0.542)	0.201	(0.623)
Migration	Migration Inflow Rate	0.011	-0.004	(0.140)	0.046	(0.258)	-0.014	(0.447)	0.101	(0.390)
	Migration Outflow Rate	0.007	0.052	(0.142)	0.514	(0.281)	0.166	(0.452)	0.392	(0.360)
	Fraction of Foreign Born Residents	0.100	-0.198	(0.123)	0.616	(0.254)	-0.633	(0.393)	1.281	(0.382)
Social Capital	Social Capital Index (Rupasingha and Goetz 2008)	0.934	0.365	(0.159)	1.013	(0.398)	1.164	(0.508)	-0.211	(0.531)
	Fraction Religious	0.107	0.056	(0.183)	0.949	(0.351)	0.179	(0.583)	0.737	(0.441)
	Violent Crime Rate	0.001	-0.414	(0.182)	-0.764	(0.603)	-1.322	(0.580)	0.596	(0.538)
Family Structure	Fraction of Children with Single Mothers	0.036	-0.501	(0.129)	-2.329	(0.371)	-1.598	(0.412)	-0.682	(0.552)
	Fraction of Adults Divorced	0.015	0.089	(0.160)	-0.803	(0.259)	0.282	(0.509)	-1.052	(0.427)
	Fraction of Adults Married	0.033	0.430	(0.152)	1.080	(0.378)	1.373	(0.484)	-0.364	(0.555)
Prices	Median House Prices	82,845	-0.139	(0.171)	0.739	(0.226)	-0.445	(0.546)	1.234	(0.452)
	Median Monthly Rent	206.7	-0.200	(0.165)	0.550	(0.304)	-0.639	(0.525)	1.258	(0.406)

Notes: This table replicates Table XII in the text using Place Effects and Permanent Residents characteristics for Females Only

Appendix Table XIII

Regressions of Place Effects for Females Across Counties within Commuting Zones on Selected Covariates (Below-Median Income Parents (p25))

		Standard Deviation of Covariate	Exposure Effect Correlation		Regression Decomposition on Model Components					
					Permanent Residents		Causal (20 years)		Sorting	
		(1)	(2)		(3)		(4)		5)	
		Std. Dev	Correlation	s.e.	Coeff	(s.e.)	Coeff	(s.e.)	Coeff	(s.e.)
Segregation and Poverty	Fraction Black Residents	0.130	-0.371	(0.255)	-2.131	(0.159)	-0.486	(0.334)	-1.646	(0.347)
	Poverty Rate	0.055	0.139	(0.478)	-1.940	(0.199)	0.183	(0.626)	-2.129	(0.600)
	Racial Segregation Theil Index	0.118	-0.452	(0.281)	-2.049	(0.140)	-0.593	(0.368)	-1.459	(0.363)
	Income Segregation Theil Index	0.039	-0.488	(0.237)	-1.451	(0.113)	-0.640	(0.311)	-0.806	(0.298)
	Segregation of Poverty (<p25)	0.034	-0.540	(0.244)	-1.596	(0.129)	-0.708	(0.320)	-0.885	(0.311)
	Segregation of Affluence (>p75)	0.045	-0.420	(0.241)	-1.224	(0.122)	-0.551	(0.316)	-0.668	(0.302)
	Share with Commute < 15 Mins	0.102	0.335	(0.530)	0.174	(0.199)	0.439	(0.694)	-0.253	(0.756)
	Log. Population Density	1.715	-0.453	(0.306)	-1.520	(0.269)	-0.593	(0.401)	-0.927	(0.433)
Income Distribution	Household Income per Capita for Working-Age Adults	9,222	-0.222	(0.348)	0.921	(0.263)	-0.291	(0.456)	1.210	(0.394)
	Gini coefficient for Parent Income	0.113	-0.775	(0.325)	-1.783	(0.358)	-1.016	(0.426)	-0.766	(0.346)
	Top 1% Income Share for Parents	0.064	-0.693	(0.455)	-0.812	(0.214)	-0.909	(0.596)	0.100	(0.576)
	Gini Bottom 99%	0.112	-0.775	(0.324)	-1.786	(0.357)	-1.015	(0.424)	-0.770	(0.345)
	Fraction Middle Class (Between National p25 and p75)	0.075	-0.086	(0.348)	0.516	(0.263)	-0.112	(0.456)	0.614	(0.438)
Tax	Local Tax Rate	0.009	0.234	(0.795)	-0.808	(0.619)	0.306	(1.042)	-1.132	(1.425)
	Local Tax Rate per Capita	0.432	0.005	(0.499)	-0.316	(0.463)	0.007	(0.654)	-0.331	(0.989)
	Local Government Expenditures per Capita	1.016	-0.502	(0.223)	-0.954	(0.532)	-0.657	(0.292)	-0.312	(0.628)
	State EITC Exposure	3.752	0.449	(0.381)	-0.105	(0.065)	0.588	(0.500)	-0.703	(0.460)
	State Income Tax Progressivity	2.365	-0.039	(0.599)	-0.164	(0.125)	-0.051	(0.785)	-0.114	(0.834)
K-12 Education	School Expenditure per Student	1.483	1.135	(1.332)	-0.240	(0.405)	1.488	(1.745)	-1.749	(1.872)
	Student/Teacher Ratio	2.816	-0.317	(0.371)	-0.575	(0.231)	-0.416	(0.486)	-0.143	(0.583)
	Test Score Percentile (Controlling for Parent Income)	9.612	0.346	(0.490)	1.654	(0.335)	0.454	(0.642)	1.202	(0.688)
	High School Dropout Rate (Controlling for Parent Income)	0.024	-0.449	(0.348)	-1.682	(0.205)	-0.589	(0.456)	-1.105	(0.458)
College	Number of Colleges per Capita	0.011	0.201	(0.863)	-0.491	(0.197)	0.264	(1.130)	-0.810	(1.128)
	Mean College Tuition	4,421	-0.079	(0.296)	-0.305	(0.249)	-0.103	(0.387)	-0.206	(0.520)
	College Graduation Rate (Controlling for Parent Income)	0.139	-0.374	(0.359)	-0.480	(0.207)	-0.491	(0.471)	0.007	(0.511)
Local Labor Market	Labor Force Participation Rate	0.058	-0.116	(0.502)	1.019	(0.225)	-0.152	(0.658)	1.189	(0.656)
	Fraction Working in Manufacturing	0.070	0.272	(0.370)	0.818	(0.140)	0.356	(0.484)	0.473	(0.476)
	Teenage (14-16) Labor Force Participation Rate	0.108	-0.065	(0.497)	1.075	(0.201)	-0.085	(0.651)	1.160	(0.629)
Migration	Migration Inflow Rate	0.019	-0.227	(0.277)	0.938	(0.217)	-0.297	(0.363)	1.247	(0.371)
	Migration Outflow Rate	0.014	-0.070	(0.349)	0.203	(0.247)	-0.092	(0.457)	0.309	(0.453)
	Fraction of Foreign Born Residents	0.109	-0.081	(0.371)	-0.500	(0.202)	-0.107	(0.486)	-0.390	(0.456)
Social Capital	Social Capital Index (Rupasingha and Goetz 2008)	1.096	0.370	(0.604)	0.072	(0.219)	0.485	(0.791)	-0.426	(0.815)
	Fraction Religious	0.128	-0.230	(0.380)	0.050	(0.179)	-0.302	(0.497)	0.334	(0.518)
	Violent Crime Rate	0.002	-0.681	(0.320)	-1.713	(0.132)	-0.892	(0.419)	-0.822	(0.387)
Family Structure	Fraction of Children with Single Mothers	0.069	-0.429	(0.268)	-2.392	(0.212)	-0.562	(0.352)	-1.826	(0.265)
	Fraction of Adults Divorced	0.017	-0.763	(0.404)	-1.617	(0.169)	-0.999	(0.529)	-0.629	(0.542)
	Fraction of Adults Married	0.062	0.304	(0.299)	2.245	(0.113)	0.398	(0.392)	1.845	(0.387)
Prices	Median House Price	124,012	-0.182	(0.139)	0.173	(0.461)	-0.239	(0.182)	0.413	(0.437)
	Median Monthly Rent	217.7	0.058	(0.340)	0.940	(0.219)	0.076	(0.445)	0.877	(0.466)

Notes: This table replicates Table XIV in the text using Place Effects and Permanent Residents characteristics For Females Only

Appendix Table XIV
Commuting Zone and County Characteristics: Definitions and Data Sources

Notes: This table provides a description of each variable used in Section X and reported in Tables 12 to 15 and Figures XV and XVI. For variables obtained at the county level, we construct population-weighted means at the CZ level. See Appendix D of Chetty et al. (2014) for further details on data sources and construction of the variables.

	Variable (1)	Definition (2)	Source (3)
Segregation and Poverty	Fraction Black	Number of individuals who are black alone divided by total population	2000 Census SF1 100% Data Table P008
	Poverty Rate	Fraction of population below the poverty rate	2000 Census SF3 Sample Data Table P087
	Racial Segregation	Multi-group Theil Index calculated at the census-tract level over four groups: White alone, Black alone, Hispanic, and Other	2000 Census SF1 100% Data Table P008
	Income Segregation	Rank-Order index estimated at the census-tract level using equation (13) in Reardon (2011); the δ vector is given in Appendix A4 of Reardon's paper. $H(p_k)$ is computed for each of the income brackets given in the 2000 census. See Appendix D for further details.	2000 Census SF3 Sample Data Table P052
	Segregation of Poverty (<p25)	H(p25) estimated following Reardon (2011); we compute H(p) for 16 income groups defined by the 2000 census. We estimate H(p25) using a fourth-order polynomial of the weighted linear regression in equation (12) of Reardon (2011).	2000 Census SF3 Sample Data Table P052
	Segregation of Affluence (>p75)	Same definition as segregation of poverty, but using p75 instead of p25	2000 Census SF3 Sample Data Table P052
	Fraction with Commute < 15 Mins	Number of workers that commute less than 15 minutes to work divided by total number of workers. Sample restricts to workers that are 16 or older and not working at home.	2000 Census SF3 Sample Data Table P031
	Logarithm of Population Density	Logarithm of the Population Density where the Population Density is defined as the Population divided by the Land Area in square miles.	2000 Census Gazetteer Files
Income Inequality	Household Income per Capita	Aggregate household income in the 2000 census divided by the number of people aged 16-64	2000 Census SF3 Sample Data Table P054
	Gini	Gini coefficient computed using parents of children in the core sample, with income topcoded at $100 million in 2012 dollars	Tax Records, Core Sample of Chetty et al. (2014)
	Top 1% Income Share	The fraction of income within a CZ going to the top 1% defined within the CZ, computed using parents of children in the core sample	Tax Records, Core Sample of Chetty et al. (2014)
	Gini Bottom 99%	Gini coefficient minus top 1% income share	Tax Records, Core Sample of Chetty et al. (2014)
	Fraction Middle Class (between p25 and p75)	Fraction of parents (in the core sample) whose income falls between the 25th and 75th percentile of the national parent income distribution	Tax Records, Core Sample of Chetty et al. (2014)
Tax	Local Tax Rate	Total tax revenue per capita divided by mean household income per capita for working age adults (in 1990)	1992 Census of Government county-level summaries
	Local Tax Rate Per Capita	Total tax revenue per capita	1992 Census of Government county-level summaries
	Local Govt Expenditures Per Capita	Total local government expenditures per capita	1992 Census of Government county-level summaries
	Tax Progressivity	The difference between the top state income tax rate and the state income tax rate for individuals with taxable income of $20,000 in 2008	2008 state income tax rates from the Tax Foundation
	State EITC Exposure	The mean state EITC top-up rate between 1980-2001, with the rate coded as zero for states with no state EITC	Hotz and Scholz (2003)
K-12 Education	School Expenditure per Student	Average expenditures per student in public schools	NCES CCD 1996-1997 Financial Survey
	Student Teacher Ratio	Average student-teacher ratio in public schools	NCES CCD 1996-1997 Universe Survey
	Test Score Percentile (Income adjusted)	Residual from a regression of mean math and English standardized test scores on household income per capita in 2000	George Bush Global Report Card
	High School Dropout Rate (Income adjusted)	Residual from a regression of high school dropout rates on household income per capita in 2000. Coded as missing for CZs in which dropout rates are missing for more than 25% of school districts.	NCES CCD 2000-2001
College	Number of Colleges per Capita	Number of Title IV, degree offering insitutions per capita	IPEDS 2000
	College Tuition	Mean in-state tuition and fees for first-time, full-time undergraduates	IPEDS 2000
	College Graduation Rate (Income Adjusted)	Residual from a regression of graduation rate (the share of undergraduate students that complete their degree in 150% of normal time) on household income per capita in 2000	IPEDS 2009
Local Labor Market	Labor Force Participation	Share of people at least 16 years old that are in the labor force	2000 Census SF3 Sample Data Table P043
	Share Working in Manufacturing	Share of employed persons 16 and older working in manufacturing.	2000 Census SF3 Sample Data Table P049
	Growth in Chinese Imports	Percentage growth in imports from China per worker between 1990 and 2000,	Autor, Dorn, and Hanson (2013)
	Teenage (14-16) Labor Force Participation	Fraction of children in birth cohorts 1985-1987 who received a W2 (i.e. had positive wage earnings) in any of the tax years when they were age 14-16	Tax Records, Extended Sample
Migration	Migration Inflow Rate	Migration into the CZ from other CZs (divided by CZ population from 2000 Census)	IRS Statistics of Income 2004-2005
	Migration Outflow Rate	Migration out of the CZ from other CZs (divided by CZ population from 2000 Census)	IRS Statistics of Income 2004-2005
	Fraction Foreign Born	Share of CZ residents born outside the United States	2000 Census SF3 Sample Data Table P021
Social Capital	Social Capital Index	Standardized index combining measures of voter turnout rates, the fraction of people who return their census forms, and measures of participation in community organizations	Rupasingha and Goetz (2008)
	Fraction Religious	Share of religious adherents	Association of Religion Data Archives
	Violent Crime Rate	Number of arrests for serious violent crimes per capita	Uniform Crime Reports
Family Structure	Fraction of Children with Single Mothers	Number of single female households with children divided by total number of households with children	2000 Census SF3 Sample Data Table P015
	Fraction of Adults Divorced	Fraction of people 15 or older who are divorced	2000 Census SF3 Sample Data Table P018
	Fraction of Adults Married	Fraction of people 15 or older who are married and not separated	2000 Census SF3 Sample Data Table P018
Prices	Median Monthly Rent	Median "Contract Rent" (monthly) for the universe of renter-occupied housing units paying cash rent	2000 Census SF3a (NHGIS SF3a, code: GBG)
	Median House Price	Median value of housing units at the county level (population weighted to CZ level for CZ covariate).	2000 Census SF3a (NHGIS SF3a, code: GB7)