The Impacts of Neighborhoods on Intergenerational Mobility II: County-Level Estimates*

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December 2016

Abstract

We estimate the causal effect of each county in the U.S. on children's earnings and other outcomes in adulthood using a fixed effects model that is identified by analyzing families who move across counties with children of different ages. Using these estimates, we (a) quantify how much places matter for upward mobility, (b) construct predictions of the causal effect of growing up in each county that can be used to guide families seeking to move to opportunity, and (c) characterize which types of areas produce better outcomes. For children growing up in low-income families, 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%. There is substantial local area variation in children's outcomes: for example, growing up in the western suburbs of Chicago (DuPage county) would increase a given child's earnings by 30% relative to growing up in Cook county. Counties with less concentrated poverty, less income inequality, better schools, a larger share of two-parent families, and lower crime rates tend to produce greater upward mobility. Boys' outcomes vary more across areas than girls, and boys have especially poor outcomes in highly segregated areas. One-fifth of the black-white earnings gap can be explained by differences in the counties in which black and white children grow up. Areas that generate better outcomes tend to have higher house prices, but our approach uncovers many "opportunity bargains" – places that generate good outcomes but are not very expensive.

*An earlier version of this paper was circulated as the second part of the paper, "The Impacts of Neighborhoods on Intergenerational Mobility: Childhood Exposure Effects and County Level Estimates" (Chetty and Hendren (2015)). The opinions expressed in this paper are those of the authors alone and do not necessarily reflect the views of the Internal Revenue Service or the U.S. Treasury Department. This work is a component of a larger project examining the effects of tax expenditures on the budget deficit and economic activity. All results based on tax data in this paper are constructed using statistics originally reported in the SOI Working Paper "The Economic Impacts of Tax Expenditures: Evidence from Spatial Variation across the U.S.," approved under IRS contract TIRNO-12-P-00374 and presented at the Office of Tax Analysis on November 3, 2014. We thank David Autor, Gary Chamberlain, Max Kasy, Lawrence Katz, and numerous seminar participants for helpful comments and discussions. Sarah Abraham, Alex Bell, Augustin Bergeron, Michael Droste, Jamie Fogel, Nikolaus Hildebrand, Alex Olssen, Jordan Richmond, Benjamin Scuderi, and our other pre-doctoral fellows provided outstanding research assistance. This research was funded by the National Science Foundation, the Lab for Economic Applications and Policy at Harvard, Stanford University, and Laura and John Arnold Foundation.

I Introduction

How are children's economic opportunities shaped by the neighborhoods in which they grow up? In the first paper in this series (Chetty and Hendren 2016), we presented quasi-experimental evidence showing that neighborhoods have significant childhood exposure effects on children's life outcomes. Although those results show that place matters for intergenerational mobility, they do not tell us which areas produce the best outcomes, nor do they identify the characteristics of neighborhoods that generate good outcomes – two key inputs necessary for developing place-focused policies to improve upward mobility.

In this paper, we build on the exposure-time design developed in our first paper to estimate the causal effect of each county in the U.S. on children's earnings in adulthood. Formally, our first paper identified one treatment effect – the average impact of exposure to an area where children have better outcomes – while this paper pursues the more ambitious goal of identifying (approximately) 3,000 treatment effects, one for each county in the country. We use these estimates to (a) quantify the magnitude of place effects, (b) construct predictions of the causal effect of growing up in each county that can be used to guide families seeking to move to opportunity, and (c) study the characteristics of areas that produce high levels of upward mobility to shed light on the types of place-based policies that could improve upward mobility.

We estimate the causal effects of counties by analyzing families who move across counties using data from de-identified tax returns spanning 1996-2012, the same sample used in our first paper. We estimate each county's effect using a fixed effects regression model that is identified from variation in the ages of children when families move. 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 the assumption that other unobservable determinants of children's outcomes are unrelated to the age at which they move. 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 variation in the age of children when families make a given move rather than variation in where families choose to move.

The key assumption required to identify the county fixed effects using this research design is that children's potential outcomes are orthogonal to the age at which they move to a given county. This assumption is motivated by the evidence in our first paper showing that the age at which children move to an area with better outcomes (based on permanent residents) appears to be orthogonal to other determinants of their outcomes. However, it is a stronger requirement than the condition required to identify average exposure effects in the first paper because it imposes 3,000 orthogonality conditions – one for each county – rather than a single orthogonality condition that must hold on average. We assess the validity of this stronger identification assumption using two approaches. First, we show that controlling for parental income levels and marital status in the years before and after the move – which are strong predictors of children's outcomes – does not affect the estimates, supporting the view that our estimates are not confounded by selection on other determinants of children's outcomes. Second, we implement a placebo test by analyzing parents who move after their children turn 23, the point at which neighborhood exposure no longer appears to affect children's outcomes based on the evidence in our first paper. The county fixed estimates obtained from this placebo sample are uncorrelated with our baseline estimates, and we cannot reject the null hypothesis that there are no "place effects" using these placebo moves.

We use the estimated county effects for three purposes. First, we quantify how much neighborhoods matter for children's earnings using a variance decomposition. Treating the estimated county 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 due to noise from the total variance. For a child with parents at the 25th percentile of the national income distribution, we find 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. Importantly, we find that the areas that generate better outcomes for children in low-income families tend to generate slightly better outcomes for children in high-income families as well on average. This result suggests that the success of the poor does not have to come at the expense of the rich.

In the second part of the paper, we construct predictions of the causal effect of growing up in each county that can be used to guide families seeking to move to better areas. Formally, we construct predictions that minimize the mean squared-error (MSE) of the true impact of growing up in a given neighborhood relative to the predicted impact. Although the raw county fixed effects provide consistent estimates of county's causal effects, they do not themselves provide good predictions because many of the estimates have substantial noise, leading to high MSE. In counties with very large populations, such as Cook County in Chicago, 75% of the variance in the fixed effect estimate is signal and hence the fixed effect itself is quite informative. However, in most counties, which are significantly smaller, more than half of the variance in the fixed effect estimates is due to noise.

To obtain predictions that have lower MSE, we use a shrinkage estimator that brings in data on the permanent residents' (non-movers) outcomes in each area. 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. The best (MSE-minimizing) linear prediction of each county's causal effect is therefore a weighted average of the fixed effect estimate based on the movers and a prediction based on permanent residents' outcomes. The weights depend on the precision of the fixed effect estimate. In large counties, where the degree of sampling error in the fixed effect estimates is small, the optimal forecast 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 optimal forecast puts more weight on the predicted outcome based on the permanent residents. The county-level forecasts obtained from this procedure have substantially lower MSE than the raw fixed effects and 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.[1]

The county-level forecasts identify the best and worst areas in the U.S. in terms of their causal effects on upward 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 – instead of the average county raises his or her household income in adulthood by

[1]Conditional on permanent residents' outcomes, including other predictors – such as racial demographics, poverty rates, or other observable neighborhood characteristics – does not reduce the MSE of the forecasts appreciably. In this sense, the simple approach of taking a weighted average of the fixed effect based on movers and the permanent residents' outcomes provides an optimal forecast of neighborhood effects given currently available data.

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%. In contrast, every extra year spent in Cook County – one of the lowest-ranking counties in the U.S. – reduces a child's earnings by 0.64% per year of exposure, generating an earnings penalty of approximately 13% if one grows up there from birth.[2] Hence, moving from Cook County (the city of Chicago) to DuPage County (the Western suburbs) at birth would increase a child's earnings by 30% on average.[3]

Neighborhoods matter more for boys than girls: the signal standard deviation of county-level effects is roughly 1.5 times larger for boys than girls in low-income (25th percentile) families. The distribution has an especially thick lower-tail for boys, as counties with high concentrations of urban poverty such as Baltimore and Wayne County in Detroit produce extremely negative outcomes for boys but less so for girls. There are also significant gender differences related to marriage rates. For example, the San Francisco area generates high levels of individual earnings for girls, but produces lower levels of household income because growing up in San Francisco reduces the probability that a child gets married.

Our estimates of the causal effects of counties and commuting zones (CZs) are highly correlated with the observational statistics on intergenerational mobility reported in Chetty et al. (2014) – as expected given the findings in our first paper – but there are many significant differences. For example, children who grow up in New York City have above-average rates of upward mobility, but 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 below the national average. This is because families who live in New York tend to have high rates of upward mobility for other reasons unrelated to place. In particular, New York has a large share of immigrants, and immigrants tend to have high rates of upward mobility. More generally, this example illustrates the importance of estimating the causal effect of each area directly as we do in this paper rather than focusing exclusively on average neighborhood exposure effects as in our first paper.

Why do some areas produce much higher rates of upward income mobility than others? In the third part of the paper, we take a step toward answering this question by identifying the

[2]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.

[3]Interestingly, many families involved in the well-known Gautreaux housing desegregation project moved from Cook county to DuPage county. The Gatreaux project was not designed as a randomized controlled trial, but observational studies have shown that it appears to have led to large economic gains for these families and their children (Rosenbaum 1995). Our results support the view that much of the gain experienced by the children of the families who moved were due to the causal effect of exposure to better neighborhoods.

characteristics of areas that generate the best outcomes. A large body of research has shown that in observational data, children's outcomes are highly correlated with a variety of area-level factors including segregation, income inequality, school quality, social capital, the fraction of single-parent families, and racial demographics (Wilson 1987a, Sampson et al. 2002, Chetty et al. 2014). However, it is unclear whether these correlations are driven by the causal effects of place or selection effects (sorting). For instance, is growing up in a less segregated area beneficial for a given child or do families who choose to live in less segregated areas simply have better unobservable characteristics? We use our causal fixed effect estimates to decompose the correlations documented in prior work into causal vs. sorting components. We correlate each area characteristic with both our causal effect estimates (adjusting for noise using the signal to noise ratio) and permanent residents' outcomes, which combine both the causal effect and selection effects. We consider each of the characteristics analyzed by Chetty et al. (2014) in turn.

We find that most of the correlation between low-income children's outcomes and measures of racial and income segregation is driven by the causal component. For example, across CZs, the (population-weighted) correlation between a Theil index of racial segregation and the causal effect of an area on children's earnings for families at the 25th percentile is -0.51. This estimate implies that 80% of the association between segregation and children's outcomes for permanent residents in observational data is driven by the causal effect of place and only 20% is due to sorting.[4] Urban areas, particularly those with concentrated poverty and high rates of crime, generate much worse outcomes for low-income children than suburbs and rural areas. Together, these findings strongly support the view that growing up in an urban "ghetto" reduces children's opportunities for upward income mobility, consistent with prior work (e.g. Massey and Denton 1993, Cutler and Glaeser 1997).

Areas with greater income inequality – as measured by the Gini coefficient or top 1% income shares – also have significantly more negative causal effects on low-income children's earnings. These findings imply that the robust negative correlation between inequality and mobility documented in prior work – coined the "Great Gatsby curve" by Krueger (2012) – is not simply driven by differences in genetics or other characteristics of populations in different areas. Rather, putting a given child in an economy with higher levels of inequality makes that child less likely to rise up in the income distribution. The negative correlation between the causal effects and top 1%

[4]This result does not necessarily imply that reducing segregation in a given area will improve children's outcomes. Other factors associated with less segregation (e.g., better schools) could potentially be responsible for the gain a child obtains from moving to a less segregated area.

shares contrasts with the findings of Chetty et al. (2014), who find no correlation between top 1% shares and observed rates of upward mobility. We find that low-income families who live in areas with large top 1% shares (such as New York City) are positively selected, masking the negative association between top 1% shares (upper tail inequality) and the causal effect of places of upward mobility in observational data.

As with segregation and inequality, we find that the observational correlation of upward mobility with measures of school quality, such as student-teacher ratios and dropout rates, and proxies for social capital, such as crime rates and Rupasingha and Goetz's (2008) summary index, is driven primarily by a correlation with the causal effect of the place rather than sorting. That is, moving to a place with higher quality schools or more social capital improves a given child's outcomes.

Correlations with observed rates of upward mobility diverge more sharply from correlations with the causal effect for variables that are aggregations of individual characteristics and hence are more likely to capture selection. For example, the fraction of single parents is the single strongest predictor of differences in upward mobility for permanent residents across areas. However, the fraction of single parents – although still a significant predictor – is less highly correlated with CZs' causal effects than income segregation, the Gini coefficient, and the social capital index. This is because nearly half of the association between permanent residents' outcomes and the fraction of single parents is due to selection.

Similarly, areas with a larger African-American population tend to substantially have lower rates of upward mobility. Roughly half of this association is also driven by selection, consistent with Rothbaum (2016). Nevertheless, the correlation between the causal effects of place and the African-American share remains substantial (-0.51 across CZs, -0.32 across counties within CZs). This result implies that place effects amplify racial inequality: black children have worse economic outcomes partly because they grow up in worse neighborhoods. Our estimates imply that one-fifth of the black-white earnings gap can be explained by differences in the counties where blacks and whites grow up.[5]

Finally, we evaluate how much more one has to pay for housing to live in an area that generates better outcomes for one's children. Within CZs, counties that offer better prospects for children have slightly higher rents, especially in highly segregated cities. However, rents explain less than 10% of the variance in county's causal effects for families at the 25th percentile.[6] This result has

[5] We find qualitatively similar results when examining variation across CZs and across counties within CZs, but the correlations with causal effects are smaller and sorting components are larger at the county level.

[6] Between CZs, the correlation with house prices and rents is *negative*, as rural areas have low house prices and

two important policy implications. First, it suggests that encouraging low-income families move to more expensive areas – for instance by using "small area" fair market rents in housing voucher programs – may not significantly improve their outcomes. Distinguishing between causal effects and permanent residents outcomes is critical in uncovering this result. Counties with higher rents do have higher rates of upward mobility in observational data, but this is almost entirely due to positive selection of the types of families who live in more expensive areas.

Second, this finding suggests that many areas are "opportunity bargains," in the sense that they promote upward mobility without having higher housing prices.[7] For example, in the New York metro area, Hudson County, NJ offers much higher levels of upward mobility that Manhattan or Queens despite having comparable rents during the period we study. To understand the source of these opportunity bargains, we divide our causal county effects into the component that projects onto observable factors such as poverty rates and high school dropout rates and the residual "unobservable" component. We find that only the observable component is capitalized in rents and house prices, suggesting that the opportunity bargains may partly arise because families lack the information to identify which neighborhoods have the highest value-added. This result underscores the value of using an outcome-based approach – measuring neighborhood quality using data on long-term outcomes – relative to the traditional approach of using ex-ante characteristics such as poverty rates to define neighborhood quality (e.g., Jencks and Mayer 1990, Sampson et al. 2002).

Overall, the findings in this paper provide support for place-focused approaches to improving economic opportunity, both by helping families move to opportunity and through place-based investments. There is substantial scope for households to move to areas within their labor market (CZ) that produce better outcomes for children without paying higher rents, and our forecasting approach using data on movers' outcomes provides a practical method to identify such areas. In addition, we find that areas that produce high levels of upward mobility have a systematic set of characteristics, such as less residential segregation and greater social capital. Although these correlational results do not provide policy prescriptions for how to improve opportunity, the fact that high-mobility places share many common characteristics suggests that their successes might be replicable in areas that currently offer lower levels of opportunity.

The rest of the paper is organized as follows. In Section II, we briefly summarize the data,

tend to produce better outcomes. Since most families are likely to choose where to live within a labor market, the within-CZ correlation is more relevant from a policy perspective.

[7] Of course, the areas that are "opportunity bargains" in rents may come with other disamenities – such as longer commutes to work – that might make them less desirable. Our point here is simply that housing costs themselves are not necessarily a deterrent to moving to opportunity.

focusing on differences relative to the sample used in the first paper (Chetty and Hendren 2016). In Section III, we use a statistical model to formalize our empirical objectives. Section IV reports the baseline fixed effect estimates and assesses their robustness. Section V quantifies the magnitude of place effects, Section VI presents the CZ- and county-level forecasts, and Section VII studies the characteristics of places that generate better outcomes. Section VIII presents the results on house prices and opportunity bargains. Section IX concludes. Estimates of causal effects by county and CZ and related covariates are available on the Equality of Opportunity Project website.

II Data

Our data source parallels that of Chetty and Hendren (2016) and Chetty et al. (2014). We refer readers in particular to Chetty and Hendren (2016) for a detailed discussion of the data. Our primary outcome of interest is the child's income rank in their national cohort at age 26.[8] We also consider alternative outcome measures, including college attendance, individual income rank, and levels (as opposed to rank) of family and individual income.

Following Chetty and Hendren (2016), we construct estimates of place effects that vary with the parental family income rank, $p(i)$. We primarily use data from the 1980-88 cohorts, denoted by $s(i)$. As outcomes will be measured at fixed ages but in different years, it will also be important to control for the child's cohort. For each child and year, we assign a county and CZ location based on the Zip Code from which the family files an income tax return or to which their information returns (e.g. W-2s) are addressed.

We estimate the causal effect of each place using the sample of families who move exactly once when their child is at or below age 23. This restriction to 1-time movers is motivated by the finding in Chetty and Hendren (2016) that the patterns are similar when focusing on multiple movers. However, in contrast to the baseline analysis of Chetty and Hendren (2016), we do not impose additional sampling restrictions such as requiring moves to be further than 100 miles or that moves be to or from places with populations above 250,000. This is for two reasons. First, in contrast to Chetty and Hendren (2016), our primary specifications do not involve regressing child outcomes on forecasted outcomes based on permanent residents. As a result, we do not require the ability to construct precise measures of these permanent resident outcomes.[9] Second, including more places

[8] In Appendix Figure I, we document that the cross-CZ variation in outcomes of children who grow up in each CZ (the "permanent resident" sample in Chetty and Hendren (2016)) measured at age 26 is correlated at 0.93 with their outcomes measured at age 32, which suggests the spatial pattern of fixed effects for incomes at age 26 are likely to be informative about impacts at later ages as well.

[9] See Online Appendix B in Chetty and Hendren (2016) for a detailed discussion of the impact of measurement

and shorter distance moves increases the connectedness of the graph of moves across the U.S., thereby reducing estimation error for each fixed effect below. We report estimates for CZs with populations above 25,000 and counties with populations above 10,000 in the 2000 Census.

Table I presents the summary statistics of the CZ and county movers sample. We have a sample of 1,869,560 children whose parents move 1-time across CZs in our sample window and for whom we observe income at age 26. We have a sample of 1,323,455 children whose parents move across counties within CZs in our sample window and for whom we observe income at age 26. Mean child income at 26 is $31,559 in our CZ movers sample and 32,985 in our county movers sample in 2012 dollars, deflated using the CPI-U.

III Estimation Framework and Identification Assumptions

III.A Setup

We estimate place effects using an exposure-effect model that is motivated by the empirical patterns documented in Chetty and Hendren (2016). Let y_i denote the child's earnings (or other outcome) in adulthood, measured at age T. We model y_i as a function of three factors: the neighborhoods in which he grows up, disruption costs of moving across neighborhoods, and all other non-neighborhood inputs, such as family environment and genetics.

Child i is raised in place $c(i,a)$ for each age of childhood, $a = 1, ..., A$ where $A < T$. Let μ_c denote the causal effect of one additional year of exposure to place c on the child's outcome y_i. Motivated by the evidence of linear childhood exposure effects in Chetty and Hendren (2016), we assume that the exposure effect μ_c is constant for ages $a \leq A$ and is zero thereafter. In our empirical application, we permit place effects μ_{pc} to vary with parental income $p(i)$, but we suppress the parental income index in this section to simplify notation. Let κ denote the cost of moving from one neighborhood to another during childhood (e.g., due to a loss of connections to friends or other fixed costs of moving). Finally, let θ_i denote the impact of other factors, such as family inputs. The parameter θ_i captures both time-invariant inputs, such as fixed impacts of genetic endowments, and the total level of time-varying parental investments during childhood.

Combining the effects of neighborhoods, disruption effects of moving, and all other factors, the child's outcome y_i is

$$y_i = \sum_{a=1}^{A} \left[\mu_{c(i,a)} + \kappa 1\left\{c(i,a) \neq c(i,a-1)\right\}\right] + \theta_i \tag{1}$$

error in permanent resident outcomes on their analysis.

The production function for y_i in (1) imposes three substantive restrictions that are relevant for our empirical analysis. First, it assumes that neighborhood effects μ_c do not vary across children (conditional on parent income and other observables that we consider in our empirical application, such as gender). For instance, it assumes that neighborhood effects are the same for movers and non-movers. Second, it assumes that place effects are additive across ages, i.e. there are no complementarities between neighborhood quality across years. Third, it assumes that the disruption costs of moving κ do not vary across neighborhoods or the age of the child at the time of the move.[10]

The objective of this paper is to identify $\vec{\mu} = \{\mu_c\}$, the causal exposure effect of spending a year of childhood in each area (commuting zone and county) of the U.S. One way to identify $\vec{\mu}$ would be to randomly assign children of different ages to different places and compare their outcomes, as in the Moving to Opportunity experiment Chetty et al. (2016). Since conducting such an experiment in all areas of the country is infeasible, we develop methods of identifying place effects in observational data.

III.B Identification of Place Effects in Observational Data

Building upon the approach in Chetty and Hendren (2016), we identify $\vec{\mu}$ by exploiting variation in the *timing* of when children move across areas. For simplicity, we restrict attention to children who move exactly once during childhood. To understand the intuition underlying our approach, consider a set of children who move from the same origin o (e.g. New York) to the same destination d (e.g. Boston). Suppose that children who make this move at different ages have comparable other inputs, θ_i. Then one can infer that Boston has a higher causal effect than New York if the outcomes of children who move at younger ages are better than those who move at later ages.

We combine information from all such pairwise comparisons in our data to estimate each place's effect using the following fixed effects specification:

$$y_i = \vec{e}_i \vec{\mu} + \alpha_{od} + \epsilon_i, \tag{2}$$

where $\vec{e}_i = \{e_{ic}\}$ is a vector of length N_c with entries denoting the number of years of exposure child i has to place c. Letting m_i denote the age of the child at the time of the move, e_{ic} is given

[10] The model can be extended to allow the disruption cost to vary with the neighborhood to which the child moves, or to allow the disruption cost to vary with the age of the child at the time of the move. Neither extension would affect the results. The key requirement for identification of μ_c is that the disruption cost does not vary in an *age-dependent* manner across neighborhoods.

by

$$e_{ic} = \begin{cases} A - m_i & if\ d(i) = c \\ m_i & if\ o(i) = c \\ 0 & o.w. \end{cases}$$

By including origin-by-destination (α_{od}) fixed effects in (2), we identify $\vec{\mu}$ purely from variation in the timing of moves (rather than comparing outcomes across families that moved from or to different areas).[11] The key identification assumption required to obtain consistent estimates of $\vec{\mu}$ by estimating (2) using OLS is a standard orthogonality condition.

Assumption 2. Conditional on α_{od}, exposure time to each place, $\vec{e}_i$, is orthogonal to other determinants of children's outcomes:

$$Cov(\epsilon_i, e_{ic}) = 0\ \forall c.$$

Assumption 2 requires that children with different exposure times to different places do not systematically differ in their other inputs, ϵ_i, conditional on an origin by destination fixed effect. This assumption is a stronger version of Assumption 1 in Chetty and Hendren (2016). Chetty and Hendren (2016) established that exposure to better places as measured by the outcomes of permanent residents is not correlated with ϵ_i. Assumption 2 extends this identification assumption to require that the amount of exposure to *every* place satisfies this orthogonality condition. We provide evidence supporting this stronger identification assumption in Section IV.D after presenting our baseline results.

IV Fixed Effect Estimates

IV.A Estimating Equation

We estimate place effects for each CZ and county in the U.S. using data that spans a range of cohorts and parental income backgrounds. As documented in Chetty and Hendren (2016) and Chetty et al. (2014), spatial patterns of mobility differ across the parental income distribution. To account for heterogeneity in the causal effects of places across the parental income distribution, we extend the model in Section III to allow place effects to vary linearly in a child's parental income, $p(i)$. We denote the causal effect of place c for child parental income p by

$$\mu_{pc} = \mu_c^0 + \mu_c^1 p$$

[11] The fixed effects $\vec{\mu}$ are only identified up to the normalization that the average place effect is zero, $E[\mu_c] = 0$, because $\vec{e}_i$ has rank $N_c - 1$. Intuitively, using movers to identify place effects allows us to identify the effect of each place relative to the national average.

where μ_c^0 and μ_c^1 are the intercept and slope of each place's causal effect.

We account for the fact that our data pools across cohorts 1980-1988 by adding controls to the fixed effects in equation (2) for a child's cohort, $s(i)$.[12] We let α_{odps} denote controls for a child's origin ($o(i) = o$), destination ($d(i) = d$), parental income ($p(i) = p$), and cohort, ($s(i) = s$). Equation (2) is then given by

$$y_i = (A - m_i) \sum_c (\mu_{pc} 1\{d(i) = c\} - \mu_{pc} 1\{o(i) = c\}) + \alpha_{odps} + \epsilon_i \tag{3}$$

We flexibly approximate α_{odps} using origin-by-destination fixed effects linearly interacted with parental income rank, along with origin and destination effects interacted linearly with income and a quadratic in the child's cohort. Appendix A provides the precise equation for α_{odps} in our county and CZ estimation.

IV.B Baseline Results

Directly estimating the set of 1,400+ fixed effects for each CZ (3,000+ for each county) in equation (3) is not feasible due to computational constraints. We therefore use a multi-step procedure to estimate these fixed effects. To begin, we estimate the fixed effects for each of the 741 CZs in two steps, outlined in detail in Appendix A. We first estimate equation (3) separately for each origin-destination pair, $\{o(i) = o, d(i) = d\}$. This yields an estimate of the exposure effect for each origin relative to each destination. We then regress these on a design matrix to recover each CZ's fixed effect. Finally, we normalize this CZ estimate to have mean zero across CZs by weighting by population in the 2000 Census so that each fixed effect corresponds to the causal effect of the CZ relative to an average CZ. This yields our estimated fixed effects in equation (3) for each CZ.

For the county-level estimates, we again cannot directly estimate the 3,000+ fixed effects in equation (3). However, within each CZ there are a smaller number of counties so that one can estimate the fixed effects directly using the sample of 1-time movers across counties within a CZ. We normalize these estimates to have population-weighted mean zero across counties within each CZ by weighting by population in the 2000 Census. This provides an estimate of the causal effect of each county relative to an average county within the CZ. Finally, to form our baseline county-level estimates of the fixed effects in equation (3), we add the CZ-level causal effect estimate to the county-within-CZ estimate. This yields an estimate of the county's causal effect relative to an average county in the U.S.

[12] Because we measure outcomes of the child at a fixed age, these controls also capture effects of local labor market fluctuations and other sources of potentially confounding variation across years.

Figure 1, Panel A, presents the fixed effect estimates for each CZ for children in below-median income families ($p = 25$), highlighting estimates for CZs with more than 2.5M residents. For example, we estimate that every year of exposure to Cleveland, OH increases a child's income rank by 0.12 percentiles (s.e. 0.10) relative to an average CZ. Conversely, every year of exposure to Los Angeles decreases a child's income rank by -0.17 percentiles (s.e. 0.043) relative to an average CZ. When combined, this suggests that each year of exposure to Cleveland instead of Los Angeles raises a child's income rank by 0.29 percentiles.

To interpret the percentile magnitudes, we scale these percentile changes to reflect the dollar-per-year increases in child earnings at age 26.[13] We estimate that one percentile in earnings corresponds to roughly $818 of earnings at age 26, which is 3.21% of mean earnings of $26,091 for children with below-median income parents. In this sense, one year of exposure to Cleveland instead of Los Angeles raises a child's income by roughly 0.9%.

Panel B of Figure 1 presents the estimates for each county in the US, highlighting estimates from the New York and Newark CZ with county populations above 500K. We estimate that every year of exposure to the Bronx, NY lowers one's income by 0.23 percentiles, $\hat{\mu}_{25,Bronx} = -0.23$ (s.e. 0.10), relative to an average county. Across the river in Hudson, NJ, we estimate each year of exposure increases a child's income by 0.25 (s.e. 0.19) percentiles, $\hat{\mu}_{25,Hudson} = 0.25$. Combining, each year of exposure to Hudson, NJ relative to the Bronx increases a child's income rank by 0.48, or roughly 1.5%.

IV.C Relation to Permanent Residents

The estimates of the causal effect of a place can differ from the observed outcomes of permanent residents in that place because of sorting of permanent residents to different places. In the language of the model in equation (1), permanent resident outcomes are given by

$$\bar{y}_{pc} = A\mu_{pc} + \bar{\theta}_{pc} \tag{4}$$

[13] To construct this number, we take 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. For $p = 75$, regressing incomes, $\bar{y}^{\$}_{75,c}$, on ranks, $\bar{y}_{75,c}$ yields a coefficient of $840.

An alternative methodology to arrive at income increases would have been to directly estimate the place effects on income as opposed to ranks. These estimates are provided in the online data tables. Appendix Table I rows 11 and 12, shows 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 for $p = 25$ 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.

where $A\mu_{pc}$ is the cumulative effect of childhood exposure and $\bar{\theta}_{pc} = E\left[\theta_i | p\left(i\right) = p, c\left(i,t\right) = c\ \forall t\right]$ is the average of the other inputs, θ_i, that is obtained by permanent residents of location c. Chetty and Hendren (2016) finds that moving to places where permanent residents do better improves children's outcomes in proportion to exposure time to the place. In the notation of the model, Chetty and Hendren (2016) establishes that:

$$\frac{dE_c\left[\mu_{pc}|\bar{y}_{pc}\right]}{d\bar{y}_{pc}} \equiv \gamma_p$$

where γ_p is roughly 0.035 to 0.04 for CZs for all p.

To translate this result into the present setting, the horizontal axis in Figure 1 shows how the estimates of causal effects relates to the observed outcomes of permanent residents. A regression of $\hat{\mu}_{25,c}$ on $\bar{y}_{25,c}$ yields a slope of $\gamma_{25} = 0.037$ (s.e. 0.003), illustrated as the solid line in Figure 1 , Panel A. On average, one year of exposure to a CZ with a 1 unit higher permanent resident outcome produces 0.037 units higher outcomes.

Although the correlation between $\bar{y}_{pc}$ and μ_{pc} is high, there is significant variation in μ_{pc} even conditional on the outcomes of permanent residents, $\bar{y}_{pc}$. The examples of Cleveland and Los Angeles illustrate this point. Outcomes of permanent residents in Cleveland are lower than those in Los Angeles. But, our causal estimates suggest that on average exposure to Cleveland produces higher outcomes than exposure to Los Angeles. In this sense, looking solely at the geographic patterns of intergenerational mobility of permanent residents, $\bar{y}_{pc}$, can provide a misleading picture of the causal impacts of these places. This is the fundamental motivation for the construction of the causal estimates, μ_{pc}, provided in this paper.

IV.D Validating the Identification Assumption

The estimates suggest places play a significant role in shaping children's earnings in young adulthood. But, as formalized in Assumption 2, interpreting these estimates as causal effects requires that there is not a correlation between the parent's choice of exposure, e_i, and other inputs, θ_i , after conditioning on a fixed effect of origin, destination, parent income, and cohort, α_{odps}.

Chetty and Hendren (2016) provides an in-depth validation of the exposure effect design for measuring the causal effect of an additional year of exposure to places in which permanent residents have higher outcomes. They show that other factors affecting the child's outcomes, θ_i, are not correlated with the average causal effect of place conditional on $\bar{y}_{pc}$. Using Figure 1 as an illustration of this result, Chetty and Hendren (2016) establish that the line reflecting $E\left[\mu_{pc}|\bar{y}_{pc}\right]$ reflects the average causal effect across places c that have a given level of $\bar{y}_{pc}$.

However, Assumption 2 is stronger. It requires exposure to each place, c, be orthogonal to θ_i after conditioning on α_{odps}. The key concern is that the deviation of the estimates, μ_{pc} from the line reflecting $E[\mu_{pc}|\bar{y}_{pc}]$ does not reflect the causal effect of place. A priori, it is difficult to tell a story where there is sorting to particular places, c, that does not generate systematic sorting to places in which permanent residents are doing better or worse. But, Figure 1 can illustrate graphically what must be the case if we have a violation of Assumption 2 but still satisfy the validity of Assumption 1 in Chetty and Hendren (2016). To illustrate, consider a below-median income (p25) family moving between Washington, DC and Los Angeles, CA. As shown in Figure 1, both CZs have similar outcomes for permanent residents at p25. But, we estimate that an additional year of exposure to DC improves outcomes relative to exposure to LA. One potential confounding story would be that the set of families that chose to move to DC versus LA when their kids are young versus old are also providing other inputs to their children (selection bias). Another story is that something happens to families who choose to move to DC versus LA (e.g. positive income shocks) and its those shocks that are affecting children in proportion to their exposure to the place. The key is that whatever these patterns are, they must not lead to systematic sorting to places in which permanent residents are doing better or worse. Such patterns are arguably unlikely, but here we present results from a series of additional controls and placebo specifications that provide support for the stronger identification assumption embodied in Assumption 2.

IV.D.1 Inclusion of Additional Controls

Our first strategy to assess the validity of our identification assumption is to include additional control variables in equation (3) that capture potentially confounding variables that are correlated with exposure to particular places and with other inputs, θ_i. For a given set of additional controls, X_i, we estimate regressions of the form

$$y_i = (A - m_i) \sum_c (\mu_{pc} 1\{d(i) = c\} - \mu_{pc} 1\{o(i) = c\}) + \alpha_{odps} + \beta_0 X_i + \beta_1 X_i m_i + \epsilon_i \qquad (5)$$

This specification is equal to the baseline specification in equation (3) with the addition of the term $\beta_0 X + \beta_1 X m_i$, which includes controls for X and its interaction with the age of the child at the time of the move, m_i.[14]

Additional Income Controls. Our baseline specification includes controls for a single measure of parental income, $p(i)$. If $p(i)$ is a noisy measure of parents' inputs in their children, θ_i

[14]We also explored including family fixed effects, as in Chetty and Hendren (2016). Unfortunately, this specification yields imprecise estimates. When regressing these estimates on our baseline estimates, we obtain estimates that are not statistically distinct from 1, but are extremely imprecise (e.g. confidence intervals of widths in excess of 4).

and parents with different inputs are sorting to different places in proportion to exposure time, then one would expect that controlling for additional measures of parental income would affect the causal exposure effect estimates. Moreover, one might also worry that the move itself might be correlated with an income shock that is correlated with the destination choice of the family and affects children in proportion to their exposure to the place.

To address these concerns, we add controls for the change in the parental income rank in the year before versus the year after the move. Let $p_{i,a}$ denote the parental income rank for child i when the child is age a. We define $X_i = p_{i,m(i)+1} - p_{i,m(i)-1}$, where $m(i)$ is the age of the child at the time of the move. To estimate equation (5), we begin by regressing y_i on X_i and a vector of fixed effects that include an interaction of (a) origin, (b) destination, (c) parent income decile, (d) child cohort, and (e) child age at move. As noted in Chetty et al. (2014), the inclusion of these fixed effects ensures that the coefficients, β_0 and β_1, are identified from variation that is orthogonal to the child's exposure to particular places. Given $\hat{\beta}_0$ and $\hat{\beta}_1$, we construct the residuals $\tilde{y}_i = y_i - \hat{\beta}_0 X_i - \hat{\beta}_1 X_i m_i$. Then, we send these residuals through equation (3) to estimate μ_{pc} using the same estimation methods as in our baseline estimates. We let $\hat{\mu}_{pc}^{inc}$ denote the resulting fixed effect estimates.

Figure II presents a scatterplot comparing $\hat{\mu}_{25,c}^{inc}$ to our baseline estimates $\hat{\mu}_{25,c}$ across CZs (Panel A) and counties (Panel B). For both geographies, the specifications with income controls are nearly identical. Regressing $\hat{\mu}_{25,c}^{inc}$ on $\hat{\mu}_{25,c}$ yields coefficients close to 1 (0.996 across CZs and 1.001 across counties), as shown in the figure and reported in row 3 of Appendix Table I. This suggests our baseline estimates are not meaningfully affected by the inclusion of these additional income controls.

Marital Status Controls. Exposure to places could also be correlated with marital status. Married versus single families may provide different inputs, θ_i, into their children. Moreover, moves to places may be correlated with changes in marital status; if so, it could be that the impact of marital status changes is affecting the children in proportion to exposure.

To assess this, we include a vector of controls, X_i, for marital status both before and after the move. We construct indicators for each of the four possible marital statuses of the mother in the year before and after the move (married to unmarried, unmarried to married, unmarried to unmarried, and married to married); and we include the interaction of these indicators with the child's age at move. We let $\hat{\mu}_{p,c}^{ms}$ denote the resulting fixed effect estimates.

Figure II presents a scatterplot comparing $\hat{\mu}_{25,c}^{ms}$ to our baseline estimates $\hat{\mu}_{25,c}$ across CZs

(Panel C) and counties (Panel D). As with the parental income control specifications, we find that our baseline results are not confounded from sorting correlated with changes in marital status. Regressing $\hat{\mu}^{ms}_{25,c}$ on $\hat{\mu}_{25,c}$ yields a coefficient of 0.984 across CZs and 0.995 across counties, as shown in the figure and reported in row 2 of Appendix Table I. These results suggest that our baseline estimates are very similar if we were to instead have estimated them using additional controls for parental income and marital status, and provide support for Assumption 2.

IV.D.2 Placebo Specification using Moves Above Age 23

To further validate our empirical design, we consider a placebo test using parental moves when the child is above age 23 and likely to be no longer living with their parents. Intuitively, one would not expect to find that places affect children when they are out of the house. But, one might be worried that our fixed effect estimates are picking up other shocks or trends that are correlated with the age of the child at the time they move from one place to another. To assess this, we exploit the set of moves when the children are above age 23 and presumably out of the house when their parents move. Using these moves, we replicate our baseline specification and let $\hat{\mu}^{23+}_{pc}$ denote the resulting fixed effect estimates.

Regressing the placebo specifications on the baseline estimates leads to coefficients that are very close to zero. Row 1 of Appendix Table I presents the results. Across CZs for $p = 25$, we estimate a coefficient of 0.041 (s.e. 0.086) and across counties we estimate a coefficient of 0.037 (s.e. 0.050). In none of our placebo specifications can we reject the null hypothesis that there is no correlation with our baseline estimates. This provides further support for the validity of Assumption 2.

IV.E Summary

Our results suggest that our place effect estimates provide an unbiased estimate of the causal effect of exposure to each county and CZ in the US on children's outcomes in adulthood. The next three sections use these estimates to quantify the magnitude of place effects, construct predictions of the causal effect of growing up in each county that can be used to forecast the impact of moving to a particular place on a child's earnings in adulthood, and study the characteristics of areas that produce higher levels of upward mobility.

V The Size of Place Effects

How much does exposure to places during childhood matter for shaping outcomes in adulthood? We summarize the importance of place effects using the standard deviation of μ_{pc}, which we denote by $\sigma_{\mu_{pc}}$.

V.A Method

We compute $\sigma_{\mu_{pc}}$ as follows. Across CZs, we estimate a raw standard deviation of place effects for children with below-median income parents to be 0.248, as reported in the first row in Table II. However, these estimates contain considerable sampling variation, as illustrated by the fairly wide confidence intervals shown in Figure 1. This means that variation in the estimates across places overstates the true variation in place effects, μ_{pc}, because it also includes variation from sampling error. To adjust for this, we let η_{pc} denote sampling error,

$$\hat{\mu}_{pc} = \mu_{pc} + \eta_{pc} \tag{6}$$

where η_{pc} is orthogonal to μ_{pc}, $E\left[\eta_{pc}|\mu_{pc}\right] = 0$. We compute the variance of true place effects, $\sigma^2_{\mu_{pc}}$, by subtracting the variance induced by sampling error, $\sigma^2_{\eta_{pc}}$, from the variance in the observed estimates, $\sigma^2_{\hat{\mu}_{pc}}$,

$$\sigma^2_{\mu_{pc}} = \sigma^2_{\hat{\mu}_{pc}} - \sigma^2_{\eta_{pc}} \tag{7}$$

We estimate the variance of the sampling error as the average squared standard error,

$$\sigma^2_{\eta_{pc}} = E\left[s^2_{pc}\right]$$

where s_{pc} denotes the standard error of $\hat{\mu}_{pc}$ and the expectation is taken across CZs using precision weights ($1/s^2_{pc}$). Appendix A.C provides details for the construction of our standard errors. The second row of Table II reports the estimated standard deviation, $\sqrt{E\left[s^2_{pc}\right]}$, of the sampling error component, η_{pc}.

V.B Baseline Results

For the CZ-level estimates for children in below-median income families, we estimate $\sigma_{\epsilon_{25,c}} = \sqrt{E\left[s^2_{25,c}\right]} = 0.210$. Subtracting the variance of the sampling error using equation (7), we find $\sigma_{\mu_{pc}} = 0.132$. 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.

Across counties, we estimate a larger raw standard deviation of our place effect estimates of 0.434 for children in below-median income families. However, our county-level estimates contain considerably more sampling error with a standard deviation of 0.402, as would be expected given their smaller sample sizes. Using equation (7), these imply a standard deviation of the place effects of 0.165 for children in below-median income families. As expected, this is larger than the estimates across CZs, and imply a standard deviation of the effect of county within each CZ of 0.099 for below-median income families. Scaling the county estimates to dollar units, we estimate that one year of exposure to a county that produces 1 standard deviation higher outcomes for children in below-median income families increases mean earnings at age 26 by $135. This represents a 0.5% increase in earnings relative to the mean earnings at age 26 of $26,091 for children with below-median income parents.

For children in above-median income families we estimate a standard deviation of place effects across CZs of 0.107 and across counties 0.155. This implies that a one standard deviation better county increases earnings by $130 per year of exposure, or 0.32% of their mean earnings at age 26 of $40,601.

To understand the importance of place for an entire childhood of exposure, we further scale by an assumption for the length of childhood, A. For a benchmark assumption of $A = 20$, twenty years of childhood exposure to a one standard deviation better county increases earnings by 3.308 percentiles, or roughly $2,700. For below-median income families, twenty years of childhood exposure to a one standard deviation better county increases a child's earnings by roughly 10%. For above-median income families, a one standard deviation better county increases earnings by 6.4%, or $2600.

Relationship between $\mu_{25,c}$ and $\mu_{75,c}$. Places that cause high outcomes for children in low-income families also tend to produce high outcomes for children in high-income families. Across counties, we estimate a correlation of $\mu_{25,c}$ and $\mu_{75,c}$ of 0.287.[15] Much of this correlation is driven

[15] To construct this statistic, 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 $p > 0.5$. This ensures that the $p = 25$ and $p = 75$ estimates are not mechanically correlated from sampling error. We then 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 $\hat{\mu}_{25,c}$ and $\hat{\mu}_{75,c}$ are estimated on separate $p < 0.5$ and $p > 0.5$ samples and we re-compute the signal standard deviations on these two samples (0.134 and 0.107 respectively). This yields $cov\left(\hat{\mu}_{25,c}, \hat{\mu}_{75,c}\right) = cov\left(\mu_{25,c}, \mu_{75,c}\right)$. As in 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, $prec = \frac{1}{s\left(\mu_{25,c}\right)^2 + s\left(\mu_{75,c}\right)^2}$.

by variation across CZs; we estimate a positive correlation of the CZ effects at $p = 25$ and $p = 75$ of 0.724. Across counties within CZs, we estimate a correlation 0.08. Although smaller, the correlation remains positive. In the cross-section of counties in the U.S., the success of the poor does not seem to come at the expense of the rich.

V.C Alternative Specifications

We also construct estimates of place effects using alternative specifications. To begin, we consider our alternative control and placebo specifications. Using the estimates with additional income controls, $\hat{\mu}^{inc}_{25,c}$, we estimate a standard deviation of place effects of 0.155 across CZs and 0.181 across counties, neither of which are statistically distinct from our baseline estimates of the variance of place effects. For our specification with marital status controls, we estimate a standard deviation of place effects using $\hat{\mu}^{ms}_{25,c}$ of 0.159 across CZs and 0.186 across counties, neither of which are statistically distinct from our baseline estimates of the variance of place effects. In contrast, for the placebo specification using moves when children are above 23 years old, we cannot reject the null hypothesis that places have no effect at all on children's outcomes for three out of four specifications for $p = 25$ and $p = 75$ across counties and CZs.[16]

We also compute estimates of place effects for other outcomes including individual income and samples restricted to males or females. Broadly, we find similar variation across places when using individual instead of family income. For example, we estimate a standard deviation across CZs of 0.126 for $p = 25$ and 0.119 for $p = 75$. In contrast, we find significant differences in place effects across genders. Places have larger effects on males relative to females in below-median income families. Across CZs, we estimate a standard deviation of place effects on boys of 0.213 percentiles, as opposed to 0.16 for females. Turning to individual income for males, we find even larger differences for males versus females (0.231 versus 0.129). We return to a discussion of these gender differences in Sections VI and VII. The online data tables provide these estimates by gender for individual and family income, along with causal estimates of the impact of place on marriage.

We also construct place effect estimates for a range of other outcomes and specifications listed in Appendix Table I and discussed in more detail in Appendix B. These include adjustments for

[16] Across CZs for $p = 25$, we estimate a negative point estimate for the signal variance, which we report as a zero estimate for the standard deviation (the observed variation of the place effect across CZs is larger than the average square of the standard error). Across counties, our estimates are quite noisy and we estimate a standard deviation of place effects of 0.163. This is similar to our baseline estimates, but has a wide standard error and a p-value of 0.223 for the null hypothesis of no place effects. For $p = 75$, we again find a p-value for the null hypothesis of no place effects of 0.360 across CZs; but do obtain a p-value of 0.003 across counties.

cost of living, specifications that include quadratic terms in parental income, $p(i)$, in our estimating equation 3, and specifications using individual income as the outcome. Many of these specifications are discussed in more detail in the next two sections, and all alternative specifications are provided in the online data tables for each CZ and county.

VI Formation of Optimal Forecasts

The previous section illustrates that places matter for shaping children's outcomes in adulthood. But, they don't provide much guidance for a family seeking to improve their own child's outcomes; for this, it is important to know the likely impact of a particular place, not just the total variance of place effects. Here, the sampling variation in our causal estimates, $\hat{\mu}_{pc}$, makes them a potentially misleading guide: If families choose to move to the place with the highest estimate, $\hat{\mu}_{pc}$, they may end up in a place with greater sampling error, η_{pc}, as opposed to a better true place effect, μ_{pc}.

This section develops optimal forecasts for each CZ, μ^f_{pc}, that minimize mean-square difference between the true place effect and the forecasted place effect. The resulting forecasts are unbiased in the sense that on average places that are forecasted to have a 1 unit higher place effect on average have a 1 unit higher place effect.[17] We then use these forecasts to construct lists of places that are likely to most increase (or decrease) children's incomes.

We combine two pieces of information to form these forecasts. On the one hand, the causal effect estimates, $\hat{\mu}_{pc}$, are noisy but unbiased measures of place effects, μ_{pc}, as illustrated by the fairly wide confidence intervals in Figure I. On the other hand, the regression line in Figure I illustrates that the outcomes of permanent residents, $\bar{y}_{pc}$, provide information about the average causal effect of being exposed to a place in which permanent residents have 1-unit higher outcomes, γ_p. The resulting estimate, $\gamma_p \bar{y}_{pc}$ (where $\bar{y}_{pc}$ is demeaned across places, c), is precisely estimated and highly correlated with μ_{pc}. However, as noted in the previous section, permanent resident outcomes are not unbiased measures of causal effects because of sorting. In this sense, both the causal estimates, $\hat{\mu}_{pc}$, and the outcomes of permanent residents, $\bar{y}_{pc}$, contain information about the true causal effect, μ_{pc}, from two mutually exclusive samples of the population: movers and permanent residents. Given these pieces of information, where are the best places to grow up in the U.S.?

[17]This also generates correct utility rankings of places if families have a quadratic loss function over their child's outcomes.

VI.A Methods

We combine the causal effect estimates, $\hat{\mu}_{pc}$, with the estimates based on permanent residents, $\gamma_p \bar{y}_{pc}$, to form optimal linear forecasts, $\mu^f_{pc} = \rho_{1,pc}\hat{\mu}_{pc} + \rho_{2,pc}\left(\gamma_p \bar{y}_{pc}\right)$. We choose the coefficients to minimize the mean-squared forecast error, $\sum_c \left(\mu^f_{pc} - \mu_{pc}\right)^2$ given our knowledge of the sampling error of the causal effect, s_{pc}. This ensures that we incorporate our knowledge of the precision of each place effect estimate, $\hat{\mu}_{pc}$.

One can motivate the regression coefficients, $\rho_{1,pc}$ and $\rho_{2,pc}$ by imagining an experiment where we assign a random individual with parental income rank, p, to have one year of exposure to a place, c, relative to an average place. We can write the true causal effect using a hypothetical regression that is the sum of our linear forecast and the forecast error, ϕ_{pc},

$$\mu_{pc} = \rho_{1,pc}\hat{\mu}_{pc} + \rho_{2,pc}\left(\gamma_p \bar{y}_{pc}\right) + \phi_{pc} \tag{8}$$

We choose coefficients so that ϕ_{pc} is orthogonal to the forecast, $\mu^f_{pc} = \rho_{1,pc}\hat{\mu}_{pc} + \rho_{2,pc}\left(\gamma_p \bar{y}_{pc}\right)$ given s_{pc}.

For expositional simplicity, we make two approximations that turn out not to significantly affect our results. First, we assume $\gamma_p \bar{y}_{pc}$ is measured without error. In practice, incorporating sampling variation for $\gamma_p \bar{y}_{pc}$ does not significantly affect our results. Second, we model the variance of place effects as being uncorrelated with s_{pc}. Under this assumption, the optimal forecast is given by

$$\mu^f_{pc} = \frac{\chi^2_p}{\chi^2_p + s^2_{pc}}\hat{\mu}_{pc} + \frac{s^2_{pc}}{\chi^2_p + s^2_{pc}}\left(\gamma_p \bar{y}_{pc}\right) \tag{9}$$

where $\frac{\chi^2_p}{\chi^2_p + s^2_{pc}}$ is the weight placed on the causal effect estimate, $\hat{\mu}_{pc}$, and $\frac{s^2_p}{\chi^2_p + s^2_{pc}}$ is the weight placed on the forecast based on permanent residents, $\gamma_p \bar{y}_{pc}$. Here, χ^2_p is the variance of place effects after subtracting the component that is explained by $\bar{y}_{pc}$: $\chi^2_p = Var\left(\mu_{pc} - \gamma_p \bar{y}_{pc}\right)$.

The optimal forecast places a weight on the causal effect estimates that is inversely proportional to the standard error. If the standard error is zero, then $\mu^f_{pc} = \hat{\mu}_{pc}$ so that there is no value from incorporating information from the permanent residents. But, if the standard error is nonzero the optimal forecast places weight on the permanent resident forecasts. This weight is decreasing in the degree of bias that is embodied in the permanent resident forecast, which is captured by χ^2_p.

Figure III graphically illustrates the construction of these optimal forecasts using the subsample of CZs with populations above 5M for $p = 25$. Using the full sample and regressing $\hat{\mu}_{25,c}$ on the permanent resident outcomes, $\bar{y}_{25,c}$, yields a slope of $\gamma_{25} = 0.032$; taking the predicted values from this regression yields a predicted per-year exposure effect based on permanent resident outcomes of $0.032\left(\bar{y}_{25,c} - E\left[\bar{y}_{25,c}\right]\right)$, as illustrated by the solid line in Figure III (and in Figure I Panel A).[18]

To estimate χ_p^2, we proceed as follows. First, we let $\hat{\epsilon}_{pc} = \hat{\mu}_{pc} - \gamma_p \bar{y}_{pc}$ denote the residual of the regression of $\hat{\mu}_{pc}$ on $\bar{y}_{pc}$. We model the true $\epsilon_{pc} = \mu_{pc} - \gamma_p \bar{y}_{pc}$ as having constant variance across places. With this assumption, we can use the cross-sectional variation in $\hat{\epsilon}_{pc}$ to infer the true variance of ϵ_{pc} by subtracting the average sampling variance across places, $E_c\left[s_{pc}^2\right]$,

$$\chi_p^2 = var_c\left(\hat{\epsilon}_{pc}\right) - E_c\left[s_{pc}^2\right] \tag{10}$$

where $var_c\left(\hat{\epsilon}_{pc}\right)$ is the cross-sectional variance of $\hat{\epsilon}_{pc}$. Using equation (10), we estimate a signal standard deviation of the residual place effects, $\mu_{25,c} - \gamma_p \bar{y}_{pc}$, of $\chi_{25} = 0.07955$. This means that, on average, one expects that the true per-year exposure effect, $\mu_{25,c}$, lies about 0.07955 percentiles away from the prediction based on permanent residents, $\gamma_{25}\bar{y}_{25,c}$.

Now, consider the forecast for Los Angeles, as indicated by the red diamond at $\mu_{25,LA}^f = -0.130$ in Figure III. We estimate $\bar{y}_{25,LA} = 44.8$, which implies a prediction based on permanent residents of 0.0129. From the perspective of the permanent resident outcomes, LA has above-average outcomes. But, we estimate a causal fixed effect of $\mu_{25,LA} = -0.170$, with a standard error of $s_{25,LA} = 0.043$, which suggests the causal effect lies far below the prediction based on permanent residents. The sampling uncertainty in $\hat{\mu}_{25,LA}$ has a standard error of 0.043; the prediction based on permanent residents has a standard deviation of bias of $\chi_{25} = 0.07955$. So, our optimal weight that minimizes mean square error across CZs is then 0.777. We assign 77.7% weight to the estimated fixed effect and 22.3% to the prediction based on permanent residents. This yields $\mu_{25,LA}^f = -0.130$.

How "uncertain" is this forecast, $\mu_{25,c}^f$? The forecast differs from the true causal place effect for two reasons: (1) $\hat{\mu}_{pc}$ has sampling error and (2) $\gamma_p \bar{y}_{pc}$ is a biased measure of the place effect, μ_{pc}. Therefore, a natural measure of uncertainty in this forecast is its root mean square error, which sums these two sources of uncertainty. This is given by the formula:

$$rmse_{pc} = \sqrt{\frac{1}{\frac{1}{\chi_p^2} + \frac{1}{s_{pc}^2}}}$$

For example, if one had to rely entirely on the predictions based on permanent residents then one would obtain a RMSE of χ_p: by definition, the causal effect on average differs by χ_p from the permanent resident forecasts. Combining information on the permanent residents allows one to reduce this forecast error at a rate inversely proportional to the standard error of the causal effect

[18] Appendix Table II, Column (3) reports analogous coefficients for γ_{75} and estimates at the county as opposed to CZ level.

estimate, s_{pc}. For Los Angeles, we estimate $rmse_{25,LA} = \sqrt{\frac{1}{\frac{1}{0.07955^2}+\frac{1}{0.043^2}}} = 0.0375$. On average across CZs, we expect forecasts like the one for Los Angeles to differ from the true place effect by 0.0375 percentiles.

Repeating the exercise for $p = 75$, we find $\gamma_{75} = 0.038$ and a residual signal standard deviation of $\chi_{75} = 0.045$. For counties, we estimate γ_p of 0.027 at p25 and 0.023 at p75. Across counties, we find estimates of $\chi_{25} = 0.118$ and $\chi_{75} = 0.135$. The fact that these estimates are higher at the county level is consistent with the greater importance of sorting at finer geographies, so that the permanent resident outcomes do not provide as much information about the causal effects. This pushes the optimal forecast formula to place more weight on the causal estimates; but on the other hand, the causal estimates at the county level contain greater sampling variation, which for many counties will lead to lower weight on the causal estimates. Online Data Tables 1 and 2 present these forecasts by CZ and county, respectively, along with their RMSEs. We also provide estimates of causal forecasts for a range of outcomes beyond the baseline family income specification, which we discuss below.

The resulting forecasts have the property that places forecasted to have a 1-unit higher causal effect tend to actually have a 1-unit higher causal effect, $E\left[\mu_{pc}|\mu_{pc}^f\right] = \mu_{pc}^f$. This contrasts with our unbiased estimates of the causal effect, $\hat{\mu}_{pc}$, which are unbiased conditional on the true place effect, $E\left[\hat{\mu}_{pc}|\mu_{pc}\right] = \mu_{pc}$. However, because of sampling error, one would expect that places with a 1-unit higher estimate of $\hat{\mu}_{pc}$ will have less than a 1-unit higher true causal effect. In this sense, the resulting forecasts can be used to form lists of places with the highest and lowest causal effects on children's earnings outcomes.

VI.B Baseline Forecasts

VI.B.1 Commuting Zones

Figure IV maps the forecasts μ_{pc}^f across CZs in the U.S. for children in below-median (p25) and above-median (p75) income families. Table III lists the forecasts for the 50 largest CZs, sorted in descending order from highest to lowest values of $\mu_{25,c}^f$, along with their root mean square error.

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, 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%.

Overall, we find wide variation in the forecasts across CZs, for children in below-median and above-median income families. For above-median income families, we estimate that 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. In contrast, we continue to find Salt Lake City has the highest causal effect; it is forecasted to increase children's incomes by 0.218% per year of exposure.

VI.B.2 Counties

Zooming to the county-level estimates, Figure V plots forecasts for the New York City and Boston Combined Statistical Areas (CSAs). The results reveal wide variation in place effects, even at close distances. 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 a 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 be forecast to have a 15% ($3,920) higher income.

Best Counties. Table IV 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, $\mu^f_{25,c}$. At the top of the list, 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%.

Gautreaux De-Segregation Settlement. This comparison of DuPage to Cook County Chicago is perhaps particularly of interest in light of the 1976 U.S. Supreme Court ruling[19] that required that the Chicago Housing Authority (CHA) to provide residents living in housing projects in areas of concentrated poverty in Cook County with the opportunity to move to lower poverty neighborhoods in the suburbs, many of which were located in the western suburbs of DuPage County. An influential yet disputed line of research has studied the impact of Gautreaux on adult and child outcomes. By comparing families who accepted offers to move to the suburbs to those who chose to remain in the city, Rosenbaum (1995) finds that children whose families moved to the suburbs experience significantly better economic outcomes. For those who grew up in the suburbs, 54% attend college; this compares to only 20% who remained in the city; similarly, for those not in college, 75% are employed in the suburbs, as compared to 41% in the city. These large differences have previously been questioned because families had the choice of whether or not to move. Hence, one worries that the types of families that chose to move to the suburbs were also different in other characteristics, θ_i. While our results do not rule out the potential that the families that moved to the suburbs were positively selected (i.e. high θ_i), our results suggest that growing up in DuPage as opposed to Cook County should have significant economic impacts on children.

Worst Counties. In contrast to the positive impacts of growing up in DuPage County, we also find large negative forecasts at the bottom of the list of the 100 largest counties. Mecklenburg County (Charlotte, NC) and Baltimore, MD have the lowest forecasted causal effect for 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.

VI.C Robustness to Alternative Forecast Specifications

Our baseline forecasts incorporate two variables into equation (8): our causal effect estimates, $\hat{\mu}_{pc}$, and our prediction based on permanent resident outcomes, $\gamma_p \bar{y}_{pc}$. However, one could form precise predictions using variables beyond solely permanent resident outcomes, $\bar{y}_{pc}$. Since sorting of different types of families to different places renders the permanent resident outcomes biased

[19] See Hills vs. Gautreaux, 1976, No. 74-1047, available at http://caselaw.findlaw.com/us-supreme-court/425/284.html

measures of the causal effect, the forecast may be improved by including additional regressors that are correlated with sorting.

While in principle one could improve the forecasts by including additional variables beyond the permanent resident outcomes, in practice we find that including additional prediction variables does not meaningfully affect the results. Including additional controls, such as the fraction of black residents, fraction of foreign born residents, and other predictors of causal effects explored in Chetty et al. (2014) and in Section VII such as segregation, income inequality, and school quality do not generate forecasted place effects that meaningfully differ from our baseline results.

To illustrate this, Appendix Figure II presents a scatter plot comparing our baseline forecasts to alternative forecasts that include both permanent resident outcomes and the fraction of black residents in the CZ as predictors. We construct these alternative forecasts by first regressing $\hat{\mu}_{pc}$ on both $\bar{y}_{pc}$ and the fraction of African American residents in the CZ, $\hat{\mu}_{pc} = \gamma_p \bar{y}_{pc} + \gamma_p^{black} FractionBlack_c + \epsilon_{pc}$. We then let $\hat{\epsilon}_{pc} = \hat{\mu}_{pc} - \gamma_p \bar{y}_{pc} - \gamma_p^{black} FractionBlack_c$ and use this residual to construct the optimal weights on these predicted values versus the causal effects in equation (8). Including the fraction of African American residents does change the forecasts slightly. For example, Charlotte instead of New Orleans is the worst CZ. But, broadly the magnitudes and orderings of CZs is quite similar to our baseline forecasts. For example, Salt Lake City remains the highest ranked CZ and the correlation across all CZs between the two forecasts is 0.98 (both weighted and unweighted by 2000 population). Therefore, going forward we proceed with the simple model using only predictions from permanent residents. The online data tables contain the variables that can be used to explore whether other forecast specifications can improve the predictive content of the model.

VI.D Gender, Individual Income, and Marriage

Our baseline analysis focuses on a child's family income and estimates a common effect across genders. Here, we present forecasts for estimates constructed on gender-specific subsamples for both family and individual income, and the impact of place on marriage at age 26.

We generate the fixed effect estimates for family income for both males and females, $\hat{\mu}_{25,c,female}$ and $\hat{\mu}_{25,c,male}$ and combine them with their corresponding permanent resident outcomes to construct their associated optimal forecasts, $\mu^f_{25,c,female}$ and $\mu^f_{25,c,male}$, following the methods discussed above in Section VI.A. We repeat this procedure also using individual income for each gender, and for an indicator for being married at age 26. All estimates are provided in the online data tables for each county and CZ and for above ($p = 75$) and below ($p = 25$) median income families. Here, we discuss two broad patterns from these results: (1) places matter more for boys and (2) individual income outcomes for women are higher in places that have a negative causal effect on the likelihood of being married at age 26.

VI.D.1 Places Matter More for Boys

For family income outcomes, we find that the ordering of places for both boys and girls are similar to the gender-pooled estimates. For example, both Bergen County, NJ and Bucks County, PA produce high family incomes for both males and females in below-median income families. Appendix Table IV reports these forecasts. For boys, Bergen and Bucks county increase incomes at a rate of 0.831% and 0.841% per year of exposure. For girls, they increase incomes at a rate of 0.56% and 0.46% per year of exposure. In contrast, Baltimore, MD has the lowest causal effect on male family income. 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.[20]

While the broad ordering of places is similar across genders, our results suggest places matter more for boys than girls. To illustrate this, Appendix Figure III plots the cumulative distribution of forecast values, $\mu^f_{25,c,female}$ and $\mu^f_{25,c,male}$. Not only is the distribution more dispersed for males relative to females, but it is also more skewed: 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. that produce especially poor outcomes for boys, which include: Baltimore MD, Pima AZ, Wayne County (Detroit) MI, Fresno CA, Hillsborough FL, and New Haven CT. Twenty years of exposure to these counties lowers a child's income by more than 14% relative to an average county in the US. In Chetty et al. (2016), we further explore these patterns and document that exposure to areas with concentrated poverty tends to have greater negative effects on male children relative to female children's earnings and labor force participation in adulthood.

[20] 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 Appendix Tables III-VI 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. Appendix Table IV is sorted in descending order according to the gender-averaged specification in Column (10). 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%.

VI.D.2 Female incomes and Marriage Effects

Appendix Table V and VI present forecasts for individual income across CZs and counties for males and females. Broadly, we find patterns similar to our baseline family income patterns, especially for males. But, we find noticeable divergences for female individual income outcomes. For example, exposure to the Salt Lake City CZ causes a 0.767% increase in female family income per year of exposure, but a 0.123% *decrease* in female individual income per year of exposure. Conversely, the New York CZ causes a -0.49% decrease in female family income but a 0.13% increase in female individual income.

These divergences in the impacts of New York and Salt Lake City on individual versus family income at age 26 are explained by the fact that New York and Salt Lake City have different causal effects on the likelihood of being married at age 26. Appendix Tables VII and VIII reports our forecasts for marriage at age 26 across CZs and counties. Every year of exposure to Salt Lake City is forecasted to increase the chances of being married by age 26 of 0.54pp relative to an average CZ. Conversely, every year of exposure to New York decreases the chance of being married at age 26 by -0.46pp. Twenty years growing up in Salt Lake City instead of New York will increase the odds of being married at age 26 by 20pp. In this sense, the divergent patterns for individual versus family income for females – especially in large cities which tend to have lower marriage rates – are consistent with exposure to many large cities causing a decrease in the likelihood of being married at age 26.

VII Characteristics of Good Neighborhoods

What are the characteristics of places that promote upward mobility? Chetty et al. (2014) documented several systematic correlations between upward mobility and a range of economic and social factors including segregation, income inequality, school quality, family structure, and social capital. These correlations with upward mobility could reflect one of two very different economic phenomena. On the one hand, places with greater segregation may (on average across the US) cause lower economic outcomes for low-income children. On the other hand, perhaps the types of people who live in places with greater segregation tend to be different in other characteristics (e.g. different other inputs, θ_i). Here, we distinguish between these two potential economic patterns by studying the characteristics of places that produce good outcomes for children (high μ_{pc}) and comparing this to the patterns for the observed outcomes of permanent residents, $\bar{y}_{pc}$. Importantly, we do not claim to uncover the causal factors of places that promote upward mobility; such an analysis would require exogenous variation in those factors to isolate their causal effect.

We study the correlation of place effects for a wide range of covariates largely taken from the analysis in Chetty et al. (2014), which are also defined in Appendix Table XV. Tables V and VI provide a comprehensive characterization of the correlation of $\mu_{25,c}$ with each covariate. Column (1) reports the standard deviation of the covariate, weighted by population in the 2000 Census. Column (2) reports the estimated correlation of the covariate with the place effect, $\mu_{25,c}$. To obtain this correlation, we regress our estimated place effect, $\hat{\mu}_{25,c}$ on the standardized covariate and then divide by the standard deviation of $\mu_{25,c}$, $\sigma_{\mu_{25,c}} = 0.132$, shown in Table II.[21] In Column (3), we report the coefficient from a regression of the permanent resident outcomes, $\bar{y}_{25,c}$, on the standardized covariate. In Column (4), we report the coefficient from a regression of $20\hat{\mu}_{25,c}$ on the standardized covariate. By scaling the per-year causal effect estimates by the additional assumption of $A = 20$ years of exposure during childhood, this characterizes the average impact on a child's income rank of growing up in a place that has a one standard deviation higher value of the covariate. We can then compare this magnitude to the coefficient from the regression using the outcomes of permanent residents, $\bar{y}_{25,c}$. In Column (5), we report the coefficient from a regression of $\bar{y}_{25,c} - 20\hat{\mu}_{25,c}$ on the standardized covariate. In the logic of the model, this characterizes the correlation of the covariate with the other inputs, θ_i, received by permanent residents of the area – in this sense it reflects a correlation with the sorting of children with different other inputs to different areas.[22]

Table V reports the results across CZs for children in below-median income families ($p = 25$); Table VI reports analogous results across counties within CZs. We obtain the county within CZ estimates by including CZ fixed effects in all of the regressions. Additionally, for a selected set of covariates, Figures VI and VII present a visual representation of the coefficients from the regressions using the permanent residents and causal effect estimates. The vertical black lines represent the coefficients on the permanent residents. The bars represent the coefficients on the causal component scaled by 20 years of exposure, $20\mu_{pc}$. In the logic of the model, the difference between the coefficient on $\bar{y}_{25,c}$ and $20\mu_{25,c}$ reflects the correlation of the mean value of θ_i in place c with the covariate. This is reflected in the dotted lines connecting the bar and vertical black line.

[21] Note here we use our estimated fixed effect, $\hat{\mu}_{pc}$, not the optimal forecast, μ^f_{pc}. Although $\hat{\mu}_{pc}$ is measured with sampling error, this left-hand side measurement error will not bias the estimated regression coefficient of $\hat{\mu}_{pc}$ on the standardized covariate.

[22] If there are heterogeneous effects of neighborhoods on movers and non-movers, then this residual sorting effect can also reflect the correlation with the neighborhood effects that are unique to permanent residents and not captured by our movers analysis.

In addition, Appendix Tables IX and X present the analogous patterns for children in above-median income families, and Appendix Tables XI-XIV present estimates separately by gender for children in below-median income families. Finally, Online Data Tables 3 and 4 provide the raw data (both the causal effects and covariates) that can be used for future work exploring these patterns. In the remainder of this section, we focus our attention on several more salient patterns that emerge from the results.

VII.A Impacts on Below-Median Income Children

Chetty et al. (2014) documents a strong correlation between the outcomes of children from below-median income families and a range of covariates, including racial composition, segregation and sprawl, income inequality, family structure, education, and social capital. In this subsection, we document that these characteristics also have a correlation with the causal effect of the covariate.

VII.A.1 Race and Outcomes for Children in Low-Income Families

We begin by studying the correlation of place effects with the racial composition of the county and CZ. 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 V 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.

On the one hand, this is potentially consistent with the large literature in economics and sociology that argues that blacks are systematically exposed to worse neighborhoods and that these impacts cause lower outcomes for children in these low-income families (Wilson (1987b, 1996); Sampson (2008)). On the other hand, it could be that the types of people who live in areas with more black residents have other characteristics, θ_i, that cause their incomes to be lower.[23]

Our results suggest CZs and counties with a higher fraction of black residents on average cause lower outcomes for low-income children. Across CZs, we estimate a correlation of $\mu_{25,c}$ and the fraction of black residents to be -0.514 (s.e. 0.128), as reported in Column (2) of Table V. Across

[23] These characteristics, θ_i , include any input beyond the county or CZ-level place effect. For example, this includes differential parental inputs, the impact of racism on black residents, or even local neighborhood effects within the CZ that disproportionately affect non-movers. The latter could drive a portion of this effect if (i) people are less likely to move in and out of the worst neighborhoods when moving across CZs or counties and (ii) places with more black residents have more of these neighborhoods. In this case, our approach continues to capture the causal effect of the CZ or county for the set of movers, but the correlation of the fraction of black residents with other inputs, θ_i, can include a component that captures differential local neighborhood inputs into permanent residents versus movers.

counties within CZs, we find another negative correlation of -0.319 (s.e. 0.103), as shown in Column (2) of Table VI. Scaling by $A = 20$, the estimates suggest growing up in a CZ with a one standard deviation higher fraction of black residents causes incomes to be lower by -1.361pp (s.e. 0.339). However, this is less than the -2.418 coefficient when regressing permanent resident outcomes on the fraction of black residents. This suggests that those who grow up in a CZ with a 1 standard deviation higher fraction of black residents on average have outcomes that would be 1.027pp lower than average regardless of where they grew up, as illustrated in Column (5) of Table V.

Across counties within CZs, we find a similar pattern shown in Table VI and also Panel B of Figure VI. Regressing $20\hat{\mu}_{25,c}$ on the (standardized) fraction of black residents across counties within CZs yields a coefficient of -0.632 (s.e. 0.201), implying 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. As with the estimates across CZs, this is larger than the coefficient for the causal effect.

The black-white earnings gap. The estimates suggest that on average African Americans grow up in counties that tend to produce worse economic outcomes. While our data do not permit estimation of causal effects separately by race, we can quantify the extent to which on average blacks grow up in counties that promote lower economic outcomes. To do so, we first note that if one takes the average impact of exposure from birth, $20 * \mu_{25,c}$, in counties and weights by the fraction of black residents in the county, this yields -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, black families live in counties that produce 1.69 percentile lower outcomes relative to non-blacks. 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% of the black-white earnings gap is explained solely by the differences in the counties in which these children grow up. Of course, future work can ideally separately estimate these causal effects by race, and conduct analysis at finer geographies than the county; in the meantime, our present analysis shows that African American children are systematically exposed to county environments that cause lower earnings in adulthood and broadly supports the hypothesis that across the U.S. African American children are exposed to worse neighborhoods.

VII.A.2 Segregation and Sprawl

A large literature in the social sciences argues that neighborhoods with higher degrees of economic and racial segregation and areas with greater urban sprawl are worse places for children to grow up (e.g. Wilson (1987b, 1996); Sampson et al. (2002)). In this vein, Chetty et al. (2014) document a strong correlation between upward mobility and measures of segregation and sprawl. Our results suggest that for children in low-income families, places with segregation and sprawl largely reflect a correlation with the causal effect of the place: Across CZs, we estimate a correlation between the Theil index of income segregation and the causal effect, $\mu_{25,c}$, of -0.574; we also estimate a correlation between the fraction of people with a commute time less than 15 minutes and the causal effect of 0.875, which is the largest correlation we find across all of our covariates. Across the U.S., places with greater segregation and sprawl tend to cause lower incomes for children in low-income families. 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.

Greater Impacts on Boys. We find evidence that highly segregated areas have more negative effects on boys than girls. Appendix Table XI 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 XIII.

In Chetty et al. (2016), we further document that places with greater segregation and concentrated poverty tend to cause lower labor force participation for boys relative to girls in addition to lower earnings, and can even explain a 'reversal' in which female labor force participation is higher than male labor force participation for children in low-income families from highly segregated areas.

Sorting. Not all of the spatial correlation with segregation and sprawl reflects a correlation with the causal effects. Across counties within CZs, the patterns suggest greater residential sorting at finer geographies. This is illustrated by the dashed lines corresponding to the sorting component in Panel B of both Figure VI and VII. 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.

In sum, the negative correlation of place effects with these measures of segregation and sprawl suggest these adverse environments 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 (1987b, 1996)), the exposure effects documented here operate when growing up, not during adulthood.[24] Indeed, the correlations with commute times are unlikely to be the direct effect of being closer to jobs – recall these are cumulative exposure effects: take two children who both move to the same place at age 20 but one child came from a place with shorter commutes – we'd expect that the place with shorter commutes will cause that child to have higher earnings than the other child. Second, while our analysis identifies the causal effects, we also find evidence of geographic sorting, especially at finer geographies.[25] This suggests that previous observational analyses identifying correlations between neighborhood characteristics and observed outcomes do not accurately identify the causal role of those neighborhoods on children's outcomes.

VII.A.3 Income Levels and Inequality

Chetty et al. (2016) documents that CZs with higher incomes do not generally have higher levels of upward mobility. Outcomes of permanent residents in CZs with a one standard deviation higher level of mean family income in 1996-2000 have incomes that are 0.217pp (s.e. 0.282) lower across CZs. However, as one moves to counties within CZs, we find a positive coefficient of 0.814 (s.e. 0.249). Despite this, we do not find a significant correlation with the causal effect of the place at the CZ or the county-within-CZ level. If anything, across CZs we find that places with higher incomes tend to cause lower outcomes for low-income children of -0.805pp (s.e. 0.397) and a coefficient of 0.112 (s.e. 0.278) across counties within CZs. This is consistent with the hypothesis that the observed positive correlation between mean parental income levels across counties within CZs largely reflects the sorting of families providing different other inputs, θ_i, into their children.

[24] This childhood exposure effect is consistent with the ideas expressed in Sampson (2008) for why the Moving to Opportunity experiment did not deliver positive gains.

[25] This is also supported by the patterns in Chetty and Hendren (2016): the outcomes of permanent residents in a destination to which a family moves when children are 27 is predictive of the child's earnings at age 26.

However, we find a much starker picture when looking at income inequality. CZs and counties with greater income inequality have much lower rates of upward mobility for permanent residents. For example, a 1 standard deviation higher Gini coefficient corresponds to incomes of permanent residents that are -1.387pp (s.e. 0.501) lower across CZs. We find a similar pattern when focusing on the causal effects. Twenty years of exposure to a CZ with a one standard deviation higher Gini coefficient on average will cause a reduction in incomes of -2.024pp (s.e. 0.347), or roughly a 6.3% reduction in income. Across the US, places with greater income inequality tend to cause lower incomes in adulthood.

VII.A.4 Family Structure

Chetty et al. (2014) documents that CZs with a lower fraction of single parents have much higher rates of upward mobility. Our results suggest that a significant portion of this pattern reflects a correlation with the causal effect of these places. Figure VI (Panel A) shows that twenty 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 a 4.7% reduction in income. This is smaller than the coefficient of -2.458 (0.345) when using the permanent resident outcomes, $\bar{y}_{25,c}$. This suggests that the majority of the correlation between the fraction of single parents and the outcomes of permanent residents reflects the causal effect of exposure to those places, as opposed to systematic differences in other inputs, θ_i, for children in places with higher fractions of single parents. In addition, the difference between the coefficient for permanent residents and twenty years of exposure to the CZ suggests that children living in areas with a 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.

We find similar patterns across counties within CZs, but with stronger evidence of sorting. Counties with a one standard deviation higher fraction of single parents have outcomes of permanent residents that are -2.500pp (0.257) lower. And, we estimate that twenty years of exposure to counties with a one standard deviation higher fraction of single parents cause a -0.747pp (s.e. 0.212) reduction in the child's income percentile. This suggests the majority of the correlation with the permanent resident outcomes across counties within CZs reflects sorting. But despite this sorting, the fraction of single parents continues to have a -0.377 correlation with the causal effect of place. Places with a higher fraction of single parents tend to cause lower incomes.

VII.A.5 Education

Across CZs and counties within CZs, outcomes of permanent residents are positively correlated with measures of school quality, such as student test scores and the high school dropout rate. For example, counties with a one standard deviation higher test score within the CZ have 1.750pp higher income ranks, or roughly 5.5% higher incomes. Our results suggest this pattern is, if anything, stronger when focusing on the causal effect of the CZ and county. We find a positive correlation between test scores and $\mu_{25,c}$ across CZ of 0.509 and 0.354 across counties within CZs. However, we do also find some evidence of residential sorting across counties within CZs: the 1.750 coefficient for permanent residents is higher than can be accounted for with 20 years of exposure and a coefficient of 0.702, suggesting that children residing in counties with higher test scores are also receiving other inputs, θ_i, beyond the impact of exposure to those counties.

VII.A.6 Social Capital

Social capital has been argued to play an important role in promoting upward mobility (Coleman (1988); Putnam (1995)), and Chetty et al. (2014) documents a strong correlation across CZs between measures of social capital and upward mobility. For example, CZs with a one standard deviation higher level of the social capital index of Rupasingha and Goetz (2008) have permanent resident incomes that are 1.216pp higher (3.8% of mean incomes). Our results suggest this largely reflects a correlation with the causal effect of exposure to these CZs. Twenty years of exposure to CZs with a one 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 (Figure VI, Panel A), which fully accounts for the observed correlation with permanent resident outcomes.

We also find suggestive evidence that measures of social capital are more strongly correlated with the causal effects on low-income boys as opposed to girls outcomes. Appendix Table XI shows that 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, as shown in Appendix Table XIII. 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.

VII.A.7 Immigrants and Positive Selection

In contrast to the above covariates, Chetty et al. (2014) does not document any significant correlation between upward mobility and the fraction of foreign born residents in the CZ. Across CZs, places with a one standard deviation higher fraction of foreign born residents have outcomes of permanent residents that are on average 0.196pp higher (s.e. 0.286). However, we find a strongly negative correlation with the causal effect of the place: twenty years of exposure to a place with a one standard deviation higher fraction of foreign born residents tends to cause incomes to be -1.184pp (s.e. 0.275) lower. Taking the difference between these estimates, places with a one standard deviation higher fraction of foreign born residents have outcomes of permanent residents that are 1.417pp (s.e. 0.315) higher than would have been expected based on the causal effect of childhood exposure to the CZ, $A\mu_{25,c}$. In this sense, the observed outcomes of permanent residents tend to over-state the causal impact of the CZ on outcomes of low-income children in places with a larger share of immigrants.

This pattern can also explain some specific divergences between places that look good from a permanent resident but not a causal effect perspective. For example, New York has a relatively high rate of upward mobility (Chetty et al. (2014)) and outcomes of permanent residents, $\bar{y}_{25,c}$. However, Figure I illustrates that New York has a fairly low causal effect on children from below-median income families. While there are many reasons this positive sorting could occur, it is consistent with the natural story that immigrants themselves are positively selected. For a given level of parental income, immigrant children may have higher earnings than their non-immigrant peers regardless of where they grow up. Hence, the permanent resident outcomes of children in places with a large share of immigrants will tend to over-state the causal effect of the place on a child's earnings.

VII.B Impacts on Above-Median Income Children

In addition to patterns for children in low-income families, we also find several interesting patterns for children in above-median income families. As noted in Section V, across CZs we find a positive correlation in the causal effect of a place for above- and below-median income children. This suggests that the causal effects for children with above-median income parents will share many of the same correlates. Indeed, Appendix Table IX shows that CZs with less residential segregation, higher quality K-12 education systems as measured by test scores and dropout rates, stronger measures of social capital, and less income inequality tend to have higher causal effects on children in above-median income families. However, the causal effects for these children do not share all of the same correlates as those for lower-income children. We find no correlation between the fraction of black residents and the fraction of single parents and the causal effect of the CZ on children in above-median income families.

Across counties within CZs, we find a smaller correlation of 0.08 between the causal effects for children in above- and below-median income families. Consistent with this, we also find that many of the strong correlations with the causal effects for low-income families are not present when focusing on children in higher-income families. For example, we do not find that counties with more segregation and poverty tend to have negative impacts on children from high-income families. On average, counties with a one standard deviation higher segregation of poverty cause incomes of children from above-median income families to be -0.181 (s.e. 0.258) lower (as shown in Appendix Table X), statistically not distinct from zero. This contrasts with the coefficient of -1.810 (s.e. 0.128) for children in below-median income families, roughly ten times as large. This is consistent with the idea that places with greater segregation tend to promote low outcomes primarily for those who live in the areas of concentrated poverty within the CZ and county. More generally, we do not find significant correlations between the causal effects for above-median income children, $\mu_{75,c}$, and measures of income inequality, social capital, family structure, or K-12 education as measured by test scores and dropout rates.

Although we do not find strong correlations with the causal effects, we do find significant evidence of sorting of above-median income families across counties within CZs that correlate with these variables. Permanent resident outcomes in counties within CZs that have higher income inequality, more segregation, more single parents, and lower quality K-12 education do have lower outcomes. For example, permanent resident outcomes in counties with a one standard deviation higher test score percentile have 1.021pp (s.e. 0.118) higher incomes. But, replacing the permanent resident outcomes with twenty years of exposure to the causal estimates, $20\hat{\mu}_{75,c}$, we find a coefficient of 0.070pp (s.e. 0.265). This suggests families that provide other inputs into their children's outcomes tend to locate in places with higher quality schools; but those places do not appear to systematically increase their children's incomes for children in above-median income families. In this sense, the most salient correlations of measures of disadvantage with the causal effects of counties within CZs occurs for low-income children.

VII.C Income Inequality and Relative Mobility: Distinguishing Mechanisms Underlying the "Great Gatsby" Curve

In addition to separately analyzing correlates of causal effects for above- and below-median income families, here we focus on the impact of place on the difference in outcomes for children at the top and bottom of the parental income distribution (i.e. relative mobility). A large literature has documented a positive correlation between relative mobility and income inequality, termed the "Gatsby Curve" (Corak (2013); Krueger (2012)). Countries with greater income inequality tend to have greater intergenerational income persistence. Chetty et al. (2014) documents this pattern similarly across CZs within the US. Figure VIII, Panel A, presents a binned scatterplot of the relationship between relative mobility of permanent residents in a CZ, $\bar{y}_{100,c} - \bar{y}_{0,c}$, against the Gini coefficient of the income distribution in that CZ. Areas with more income inequality tend to have lower rates of relative mobility: CZs with a 0.1 point higher Gini coefficient on average have a 3.8 percentile higher difference in outcomes for children in rich versus poor backgrounds, $\bar{y}_{100,c} - \bar{y}_{0,c}$.

This pattern could be driven by two distinct forces. On the one hand, places with greater income inequality may tend to cause greater intergenerational persistence. On the other hand, places with greater income inequality may have differences in their distribution of permanent residents that generates both within-generation income inequality and greater intergenerational persistence. For example, places with greater income inequality could have greater dispersion in genetic traits that cause differences in earnings potentials in both the parents and children's generations.

Our analysis provides new evidence that, across the U.S., areas with more income inequality tend to cause greater intergenerational persistence in income. Given two children – one from the top of the parental income distribution ($p = 100$) and one from the bottom ($p = 0$) – the causal effect of a year of exposure to place c on the difference in these children's outcomes is given by $\mu_{100,c} - \mu_{0,c}$. Figure VIII, Panel B, presents a binned scatterplot of the relationship between $\hat{\mu}_{100,c} - \hat{\mu}_{0,c}$ and the Gini coefficient in the CZ. Across the U.S., CZs with greater income inequality tend to cause greater intergenerational persistence in incomes. In an unweighted[26] regression across CZs, we estimate an aggregate slope of 3.325 which suggests that on average each additional year these children spend growing up in a CZ with a 0.1 point higher Gini coefficient causes a 0.3325 rank percentile difference in their incomes.

[26] As discussed below, we find a smaller relationship across large CZs, so that weighted regressions provide a much more muted relationship.

Chetty et al. (2014) also identify heterogeneous patterns for large and small CZs. The correlation between income inequality and observed intergenerational persistence is strongest in the smaller CZs. We find similar patterns between income inequality and the causal effects. Panels C and D of Figure VIII split the aggregate pattern in Panel B into CZs with above and below 2.5M residents. For CZs with populations below 2.5M residents, we find a strong pattern with a slope of 3.528 (s.e. 1.321). In contrast, in large CZs with populations above 2.5M, we do not find a strong pattern between income inequality and the causal effect on income persistence, with a slope of 0.37 (s.e. 0.825). In this sense, the correlations of income inequality with both observed relative mobility ($\bar{y}_{100,c} - \bar{y}_{0,c}$) and the causal impact on relative mobility ($\mu_{100,c} - \mu_{0,c}$) are strongest in smaller CZs.

How much of the Gatsby Curve can be explained by the causal effect of place? Multiplying the aggregate pattern of 0.3325 by 20 years of exposure. corresponds to a 6.6pp difference in their percentile ranks. This is larger than the observed slope for permanent resident outcomes, $\bar{y}_{100,c} - \bar{y}_{0,c}$, illustrated in Panel A. This suggests that the entirety of the Great Gatsby curve that is observed across CZs in the U.S. can be explained by the causal effect of childhood exposure to those CZs on intergenerational persistence. In this sense, it provides suggestive evidence that intergenerational persistence in other factors in θ_i (e.g. genetics) play a minor role in generating the correlation between income inequality and intergenerational persistence in the U.S.

Finally, we reiterate that our results do not imply that changing a particular characteristic of a place will cause a difference in the causal effect of that place; we do not have exogenous variation in the income inequality in a place. Rather, the "Gatsby Curve" in the U.S. reflects a causal relationship in the following sense: on average, across the U.S. places with greater income inequality cause greater intergenerational income persistence.

VIII Prices and Opportunity Bargains

Does it cost more to live in places that improve children's outcomes? In the last two rows of Tables V and VI, we correlate our measures of place effects, μ_{pc}, with the median rent and median house price from the 2000 Census.[27] 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.[28] The negative correlation with prices is perhaps not surprising: rural

[27]For the CZ analysis in Table V, we take the median prices in the county and average them across counties within the CZ.

[28]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) for rent, as shown in Appendix Table IX.

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 moving into 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. To more closely approximate a residential choice decision within a local labor market, we study the patterns of prices across counties within CZs. Because commuting zones are defined to approximate local labor markets, 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. To quantify how much, on average, it costs to move to a place that causes a 1-percentile increase in a child's earnings from a below-median income family, we regress median rent in the county on CZ fixed effects and $\frac{1}{\gamma_c}\hat{\mu}_{25,c}$, where $\gamma_c = \frac{\sigma^2_{\mu_{25,c}}}{\sigma^2_{\mu_{25,c}}+s^2_{25,c}}$ is the signal-to-total variance ratio of the fixed effect estimate.[29] Dividing by γ_c removes attenuation bias from sampling error in $\hat{\mu}_{25,c}$.

The results suggest that in large, segregated CZs, better counties are more expensive. Figure IX (Panel A) plots the patterns across counties within CZs with populations above 100,000 and above median fraction of the population with commute times above 15 minutes. In these CZs, moving to a county that is expected to increase a child's income rank by 0.1 percentiles per year of exposure costs, on average, \$52 more in median monthly rent.[30] In contrast, in large CZs with less sprawl/segregation, we find no significant correlation: 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 (Figure IX, Panel B). 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.

Opportunity Bargains. Even within these counties in highly segregated CZs, we find evidence of "opportunity bargains" – places that have the same rent but with higher causal effects on children's outcomes. Regressing the shrunk causal effect on median rent and CZ fixed effects yields a root mean square error of 0.08. This suggests that for a given price, individuals in an average county could expect to find another county that increases their child's incomes by 0.08 percentiles

[29]The numerator, $\sigma^2_{\mu_{25,c}}$, is the signal variance, which we estimate as $\sigma^2_{\mu_{25,c}} = Var\left(\hat{\mu}_{25,c}\right) - E\left[s^2_{25,c}\right]$ where $s_{25,c}$ is the standard error of the estimate for place c.

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

per year of exposure yet has the same rental price. Moving to a one-standard deviation better county that has the same median rent will on average increase a child's earnings by 5%.

In smaller CZs with populations below 100,000, we find that counties that produce better outcomes are actually cheaper (Figure IX, Panel C). 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.[31]

Observables versus Unobservables. Why isn't it always more expensive to move to a place that causes higher outcomes for children? Of course, one potential explanation is that the areas that appear to be opportunity bargains are those with other dis-amenities, such as longer commutes or different job options, or other factors like lower quality restaurants. But a distinct potential explanation for the existence of opportunity bargains is that families do not know the causal impact that a particular place has on their children's outcomes later in life.

To explore whether individuals know the causal effect, we think of $\mu_{25,c}$ as having two components with different degrees of 'observability': (1) an "observable" component that is projected onto covariates that are observable at the time the children are being raised, such as school quality, social capital, etc, and (2) a residual "unobservable" component that is the residual after projecting this forecast onto the observable covariates. If individuals have knowledge about a place's causal effect on their children's outcomes and sort to those locations based on this information, one would expect a positive correlation between housing costs and both the observable component and the unobservable component.

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 that are predictive of the causal effect: 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

[31] Although we do not have conclusive evidence on why this negative pattern exists, we have explored whether any correlates in Table XV 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.

of the residual for a particular place.[32]

Figure X (Panel A) illustrates that the positive correlation with monthly rent in large CZs is driven entirely by the observable component of the place effect, despite using only a handful of observable variables to define the observable component.[33] 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 of exposure 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 (Panel B). 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 components constant. This suggests parents may not have precise information about the causal effect of these places on their children's outcomes. A potential direction for future work is to better understand the knowledge and constraints that are faced by families when choosing where to raise their children.

IX Conclusion

This paper has estimated the causal effect of childhood exposure to each county in the U.S. on children's outcomes in adulthood by analyzing the outcomes of children whose families move across areas. We find that growing up in a one standard deviation better county from birth increases a low-income child's adult earnings by 10%. We use our estimates to construct predictions of the causal effect of growing up in each county that can be used to guide families seeking to move to better areas. These estimates allow us identify areas that are "opportunity bargains" – places that produce high levels of mobility without high housing costs. We also show that areas that produce high levels of upward mobility share a systematic set of characteristics, such as less residential segregation and greater social capital.

Our findings provide support for place-focused approaches to improving opportunity, but there are two important directions for further work that are necessary before one can apply these findings

[32]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.

[33]Figure X 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. In Panel A, 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 $\mu^f_{25,c}$. Panel A 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}. Panel B repeats this process, interchanging the observable and unobservable components.

to improve policy. First, our current estimates are at broad geographic levels: commuting zones and counties. It would be very useful to use the methods developed here to identify the importance of place effects at finer levels of geography, such as ZIP codes and census tracts. Such finer geographic variation could provide new insights into the pathways through which place affects children's outcomes; for example, one could examine how the causal effects vary across school district boundaries. Furthermore, such analysis could be fruitful for providing further guidance to policymakers and families seeking to improve their children's opportunities, in particular for identifying opportunity bargains.

Second, further work is needed to explore what makes some places produce better outcomes than others. Our correlational analysis characterizes the properties of places that tend to have high causal effects, but identifying the causal mechanisms through which places can improve upward mobility requires exogenous variation in these factors. For example, studying changes in policies targeted at reducing residential segregation would help us understand the extent to which segregation causes worse outcomes for children in low-income families.

To facilitate further investigation of these causal mechanisms, we have made all of the county- and CZ-level estimates of causal and sorting effects available on the Equality of Opportunity Project website. In addition to the estimates of earnings outcomes we focus on in this study, we also provide estimates for other outcomes and subgroups not explored in detail here, such as college attendance, rates of marriage, and estimates for children in one vs. two-parent households. We hope these data will allow researchers to develop a more precise understanding of the production function for economic opportunity.

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Online Appendix

A Estimation Details

A.A CZ Estimation 2-Step Implementation

At the CZ level, estimation of the more than a thousand parameters, $\left\{\mu_c^0, \mu_c^1\right\}_c$, in equation (3) is not directly feasible on the micro data due to computational constraints. We therefore estimate these fixed effects in two steps.

In the first step, we estimate the exposure effect separately for each origin-destination pair. We regress y_i on exposure time to the destination relative to the origin, $A - m_i$, for each origin destination pair,

$$y_i = (A - m_i)\left(\mu_{od}^0 + \mu_{od}^1 p_i\right) + \kappa + \alpha_{odps} + \epsilon_i \tag{11}$$

where $\mu_{od}^p = \mu_{od}^0 + \mu_{od}^1 p$ represents the impact of spending an additional year of childhood in destination d relative to origin o and α_{odps} is given by

$$\alpha_{odps} = \left(\alpha_{od}^0 + \alpha_{od}^P p + \psi_{od}^0 s + \psi_{od}^1 s^2 + \psi_{od}^2 sp + \psi_{od}^3 s^2 p\right) \tag{12}$$

Formally, by estimating these effects separately by origin-destination pair, we allow for flexible cohort controls that vary by origin-destination cell.

In the second step, for a given p we take the dataset of these N_{CZ}^2 estimates, $\{\mu_{od}^p\}_{od}$, and regress them on indicators for each CZ,

$$\mu_{od}^p = G\mu_{pc} + \eta_{od} \tag{13}$$

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

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

This matrix collapses the N_c^2 pairwise exposure effects, μ_{od}^p, into a vector of N_c place fixed effects, $\vec{\mu}_p = (\mu_{p1}, ..., \mu_{pN_c})$'. 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.[34]

We estimate $\vec{\mu}_p = \{\mu_{pc}\}$ using the regression in equation (13), weighting each origin-destination-pair observation by the estimated precision of the μ_{od}^p estimates in the regression in equation (11). To reduce the impact of statistical noise in this two-step estimation process, we restrict to origin-destination cells with at least 25 observations.[35]

A.B County Estimation

As described in the text, we estimate the county level effects using moves across counties within CZs. Within each CZ, we regress y_i as:

$$y_i = (A - m_i)\sum_c \left(\mu_{pc} 1\left\{d(i) = c\right\} - \mu_{pc} 1\left\{o(i) = c\right\}\right) + \alpha_{odps} + \epsilon_i$$

where c indexes the counties in the CZ, $\mu_{pc} = \mu_c^0 + \mu_c^1 p$ is a linear specification in parental income rank, and α_{odps} is approximated by:

$$\alpha_{odps} \quad = \quad \left(\alpha_{od}^0 + \alpha_{od}^P p\right) 1\left\{d(i) = d; o(i) = o\right\} + \psi^0 s + \psi^1 s^2 + \psi^2 sp + \psi^3 s^2 p$$

A.C Standard Errors

To account for the 2-step estimation procedure discussed above, we estimate this standard error for each CZ, c, using a bootstrap procedure. We construct 100 samples (with replacement) and repeat our two-step estimation procedure, yielding se_{pc} as the standard deviation of 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

[34] We thank Gary Chamberlain for suggesting this design matrix approach for estimating equation (3).

[35] Weighting by precision in equation (13) does not solve issues of statistical noise because we must use the estimated precision as opposed to true precision.

the regression in equation (13). 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 (13) for simplicity. To estimate the standard errors for each county's estimate, we obtain the standard error from the regression in equation (3) for each county within each CZ. We form an estimate of the standard deviation of each county's effect as $s^2_{p,cty} = s^2_{p,CZ} + s^2_{p,ctyincz}$, where $s^2_{p,ctyincz}$ is the square of the standard error for the county-within-CZ estimate for county cty and $s_{p,CZ}$ is the square of the bootstrapped standard error for the CZ estimates. Note that these standard errors are additive because our 1-time movers samples across CZs and across counties within CZs are mutually exclusive.

B Robustness

We assess the robustness of our estimates to the specification assumptions made in Section III. Appendix Table I reports the coefficient from a regression of the alternative specification on our baseline estimate, along with the standard deviation of the place effects under the alternative specification. Panel A reports the results for the CZ-level estimates; Panel B reports the estimates for the county level estimates.

Linearity. Equation (3) 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 for μ_{pc}. This specification generates very similar estimates that are highly correlated with our baseline estimates at both p25 and p75. Regressing these alternative specifications on our baseline specification for $p = 25$, we estimate a coefficient of 0.974 at the CZ level and 0.991 at the county level; For $p = 75$, we estimate a coefficient of 0.994 at the CZ level and 0.969 at the county level. In short, consistent with the linearity in the outcomes of permanent residents documented in Chetty et al. (2014), 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 follow Chetty et al. (2014) by constructing adjusted income ranks for both parents and children that divide income in year t by a cost of living index[36] corresponding to the location of the individual in that year. 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.

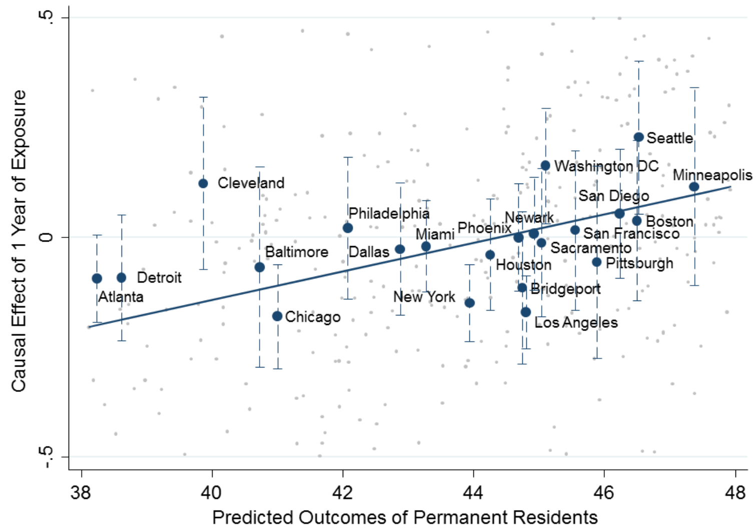
Across commuting zones, the cost of living-adjusted estimates lead to a coefficient of 0.952 when regressing the alternative estimates on the baseline estimates for $p = 25$; we obtain a coefficient of 0.951 across counties.

Overall, our baseline estimates are robust to 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 in the online data tables.

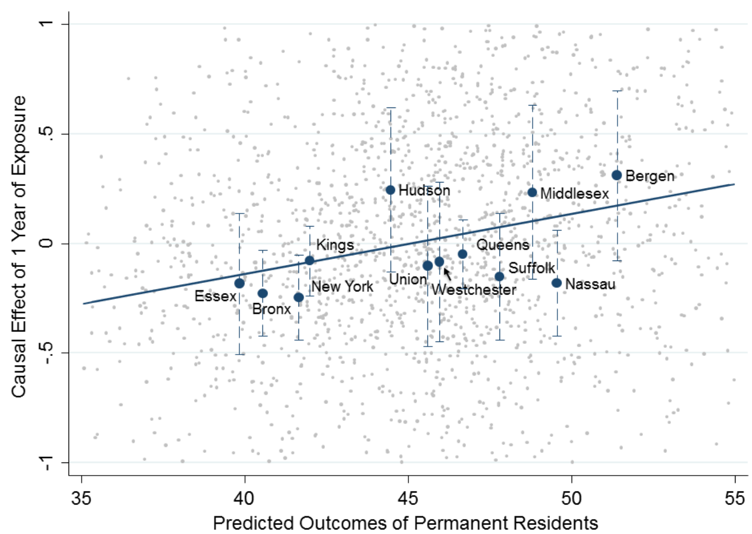
[36]See Chetty et al. (2014) for a detailed discussion of this cost of living adjustment. Loosely, we use a predicted value of the ACCRA index that allows us to expand the coverage of ACCRA to all CZs.

FIGURE I: Causal Effect of 1 Year of Exposure Versus Permanent Resident Outcomes

A. $\mu_{25,c}$ for CZs with Populations above 2.5M

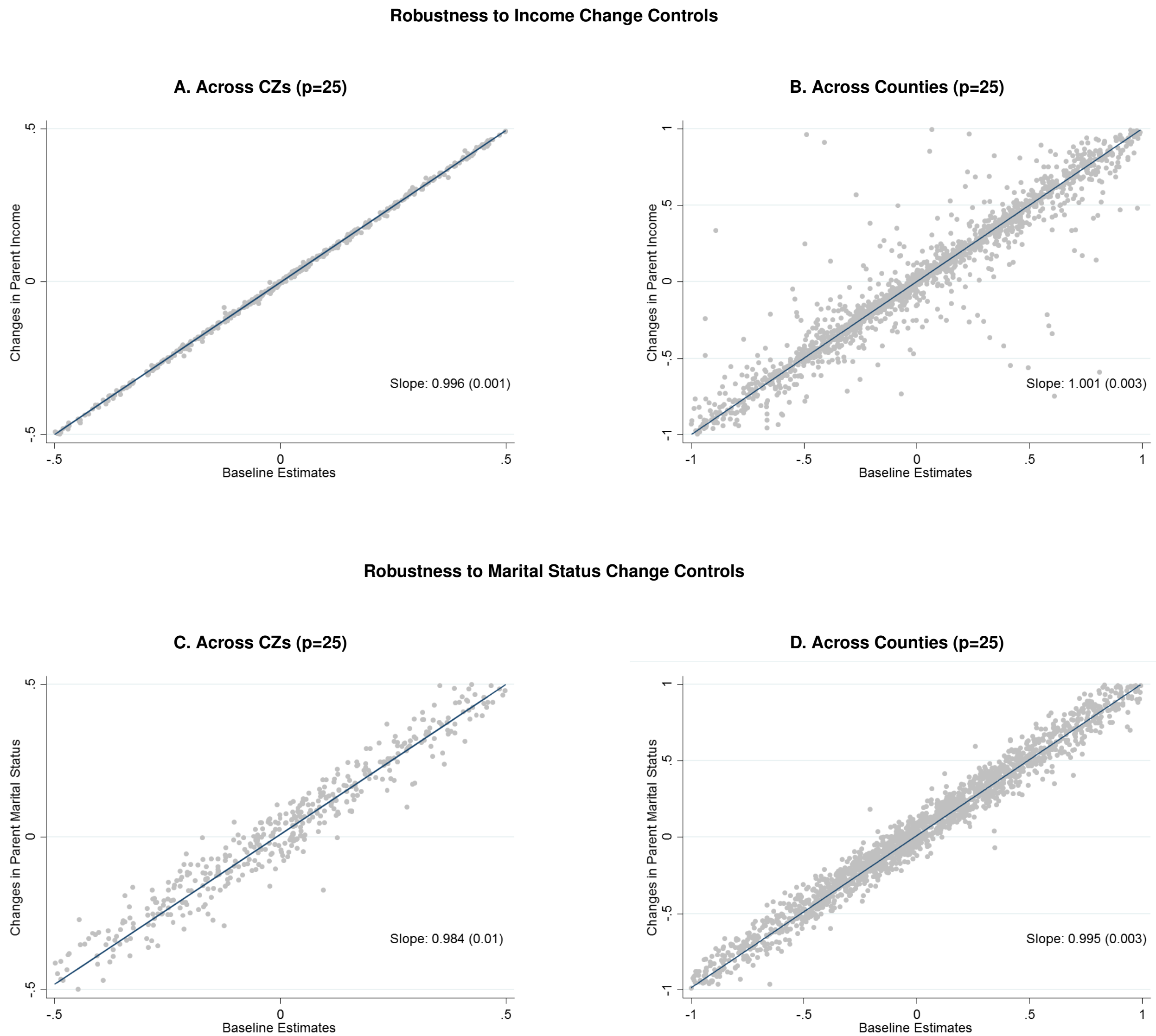


B. $\mu_{25,c}$ for Counties within New York and Newark CZs with populations above 500K



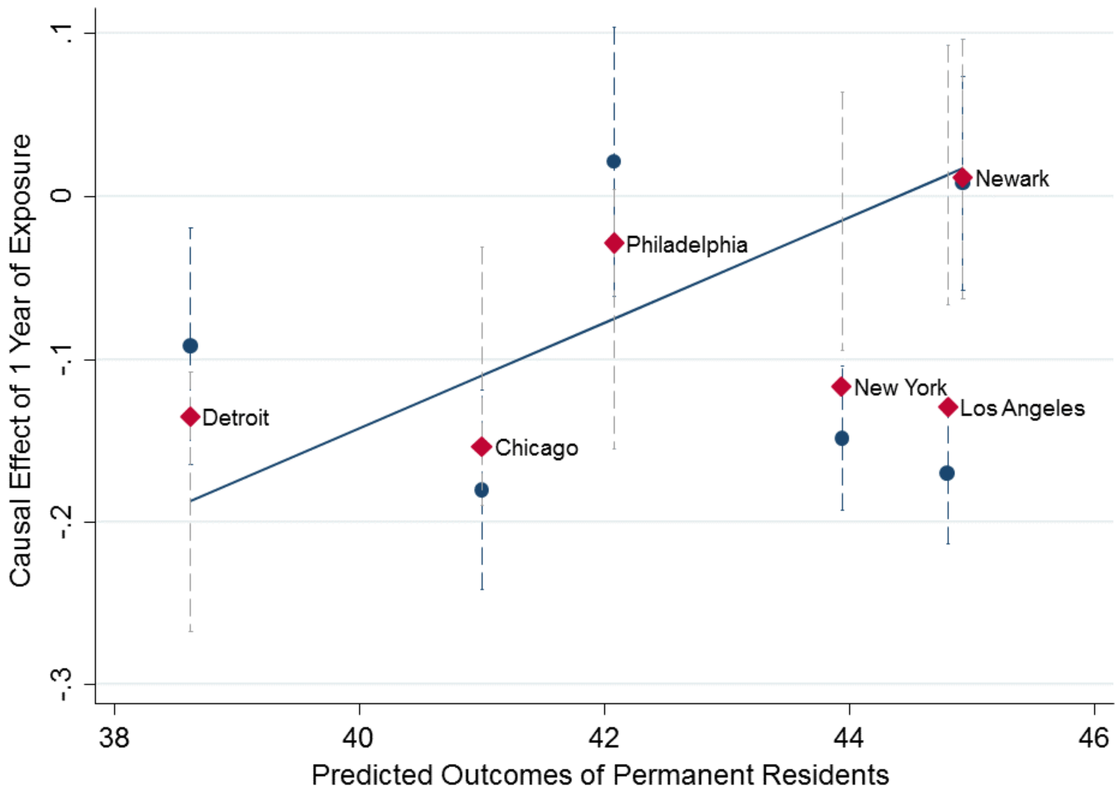
Notes: These figures present estimates of place effects, $\hat{\mu}_{pc}$ for child income rank at age 26 from families at the 25th percentile. For each place, the vertical axis presents the causal effect, μ_{pc}, and the horizontal axis presents the outcomes of permanent residents, $\bar{y}_{pc}$. Panel A presents estimates across CZs. CZs with populations above 2.5M are labeled and highlighted in blue, and vertical bars reflect their 5/95% confidence intervals. Panel B presents estimates across counties; Counties within the New York and Newark CZs that have populations above 500K are labeled and highlighted in blue, and vertical bars reflect their 5/95% confidence intervals. The solid line presents the predicted values from a regression of μ_{pc} on $\bar{y}_{pc}$ (using all places, c).

FIGURE II: Robustness of Causal Effects



Notes: These figures present scatterplots of the relationship between our baseline causal effect estimates and the estimates from two alternative specifications. The first specification includes controls for changes in family income around the time of the move and their interactions with the child's age at the time of the move. Panel A presents the scatterplot for the CZ-level estimates at $p = 25$; Panel B presents the scatterplot for the County-level estimates at $p = 25$. The second specification includes controls for marital status and its change around the time of the move along with their interactions with the child's age at the time of the move. Panel C presents the scatterplot for the CZ-level estimates at $p = 25$; Panel D presents the scatterplot for the County-level estimates at $p = 25$ with marital status controls. The horizontal axis represents the baseline specification and the vertical axis represents the estimate under the alternative specification. The figures also report the coefficient from a precision-weighted regression of the alternative estimates on the baseline estimates. Appendix Table I also reports these coefficients, along with the analogous coefficients for above-median income families ($p = 75$).

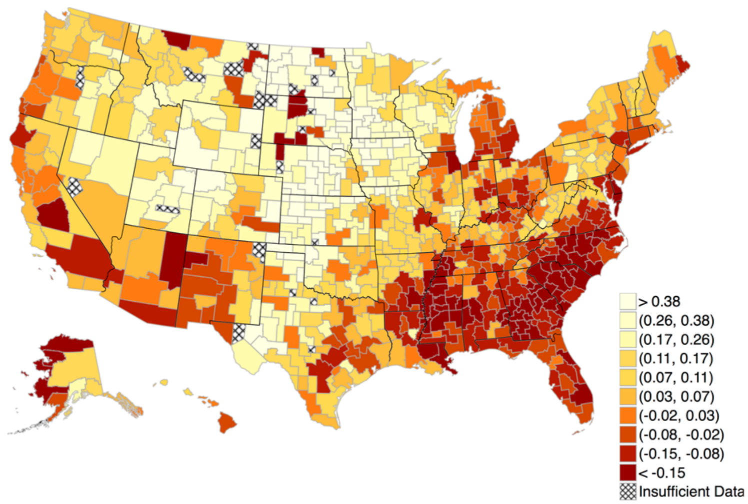
FIGURE III: Construction of Optimal Forecasts



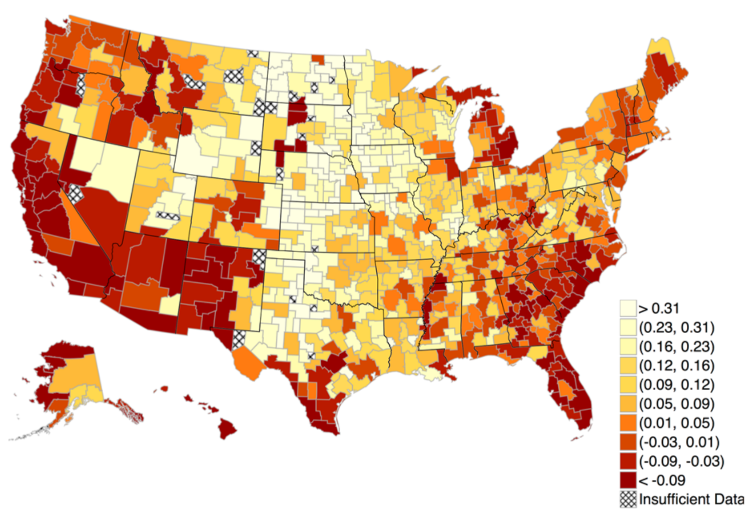
Notes: This figure illustrates the construction of our optimal forecasts, μ^f_{pc}, across CZs for children with below-median income parents ($p = 25$). The blue circles present the causal effect estimates for a select set of CZs with populations above 5M. The horizontal axis reflects the outcomes of permanent residents in the CZ; the solid line reflects the predicted causal effect given the outcomes of permanent residents, $E[\mu_{pc}|\bar{y}_{pc}] = \gamma_p\bar{y}_{pc}$. The dashed vertical bars around the causal estimates reflect 1 standard error; the dashed bars around the solid prediction line, $\gamma_p\bar{y}_{pc}$, reflect the estimated standard deviation of $\mu_{pc} - \gamma_p\bar{y}_{pc}$. The red triangle presents the optimal forecast, which places more weight on the causal effect estimate when the standard error of the estimate is low and more weight on the prediction based on permanent residents when the standard deviation of $\mu_{pc} - \gamma_p\bar{y}_{pc}$ is low.

FIGURE IV: Forecasts by CZ

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

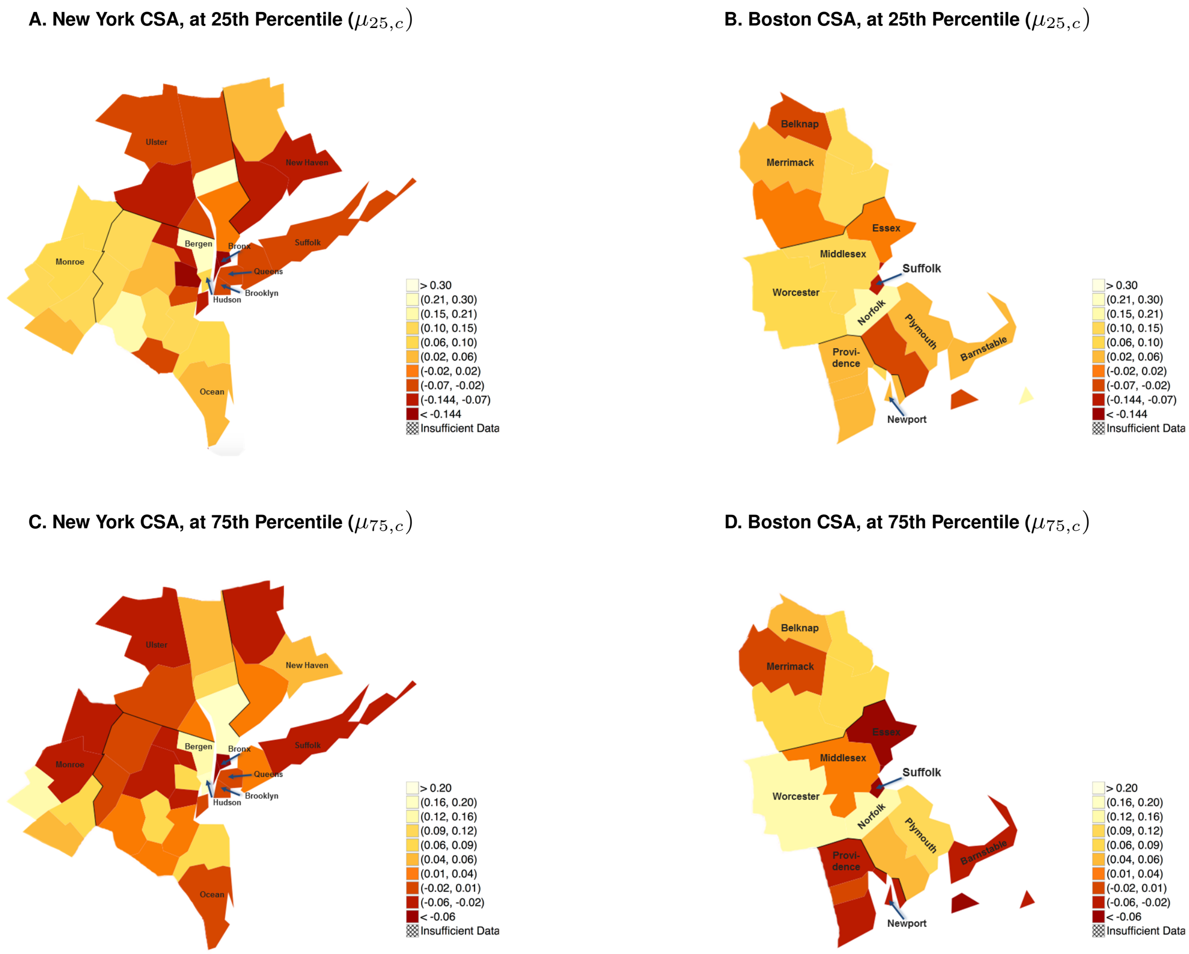


B. At 75th Percentile ($\mu_{75,c}$)



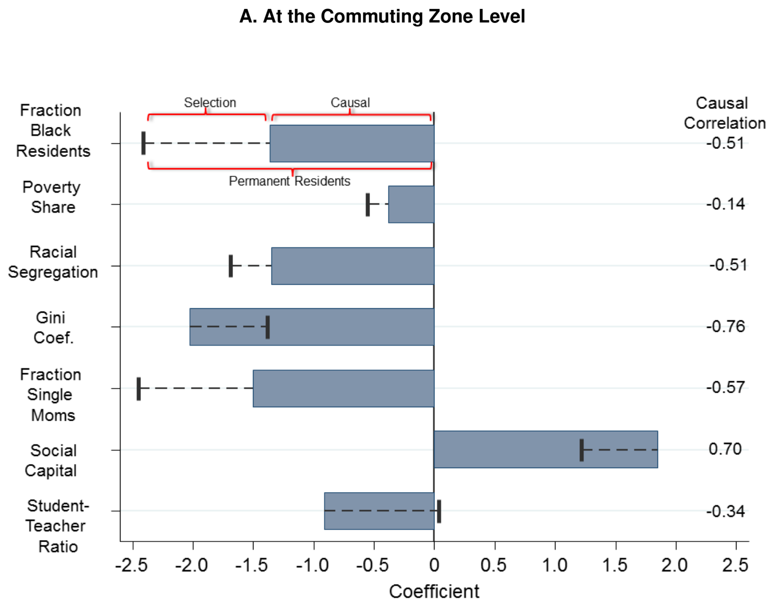
Notes: These figures present the forecasts of each CZ's causal effects, μ^f_{pc}, for below-median ($p = 25$) and above-median ($p = 75$) income families. We compute these forecasts using the methodology discussed in Section VI.

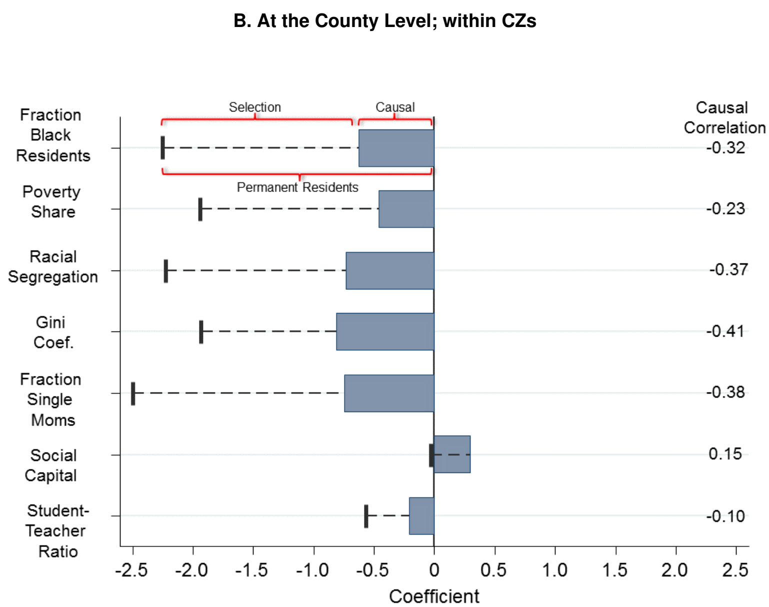
FIGURE V: Forecasts by County for NY and Boston CSA



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 forecasts using the methodology discussed in Section VI.

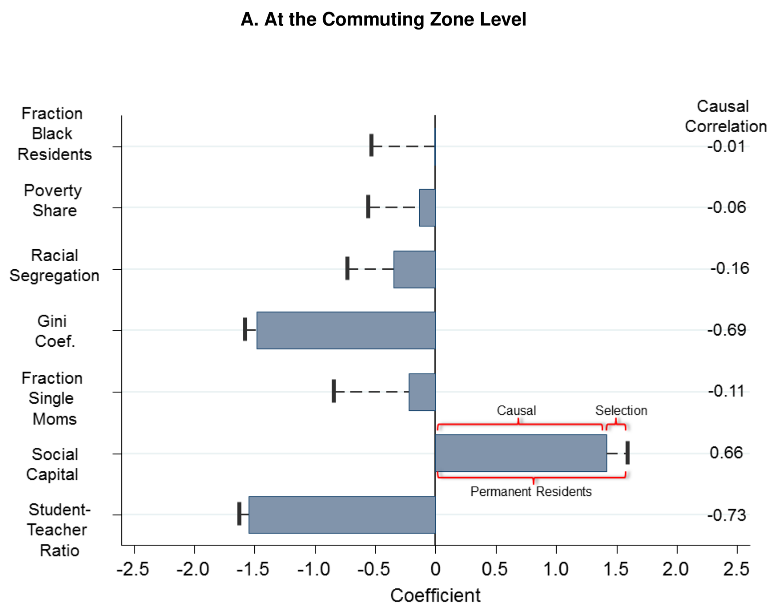
FIGURE VI: Predictors of Exposure Effects For Children with Parents at 25^{th} Percentile

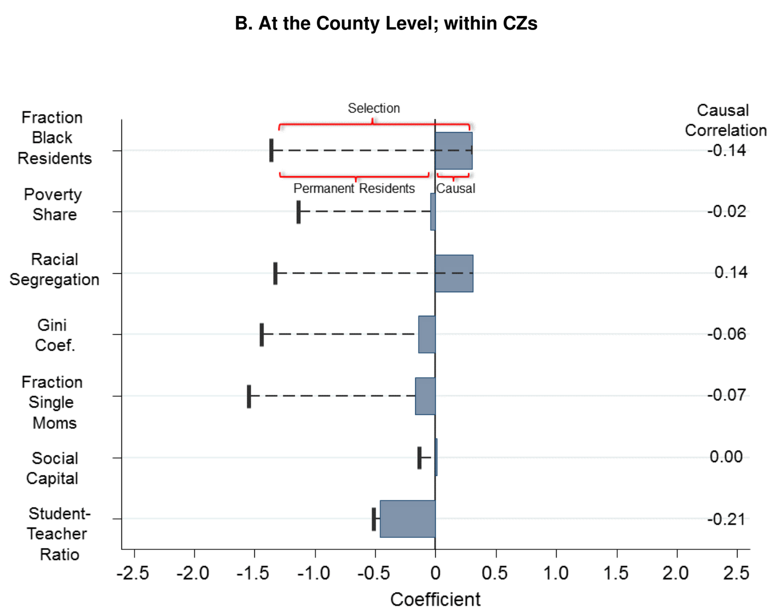




Notes: These figures show the coefficients of regressions of the model components for below-median income families ($p = 25$) on a set of covariates analyzed in Chetty et al. (2014) which are normalized to have mean zero and unit standard deviation. The vertical line represents the coefficient from a regression of the permanent resident outcomes, $\bar{y}_{25,c}$, on the standardized covariate. The solid bar represents the coefficient from a regression of the causal effect of twenty years of exposure, $20\mu_{25,c}$, on the covariate. Therefore, the difference between the bar and the vertical line (denoted by the dashed horizontal line) represents the regression coefficient from a regression of $\bar{y}_{25,c} - 20\mu_{25,c}$, on the covariate. The column on the far right divides the regression coefficient by the estimated standard deviation of $\mu_{25,c}$ (from Table II), 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 XV, and results for additional covariates are provided in Tables V and VI. 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.

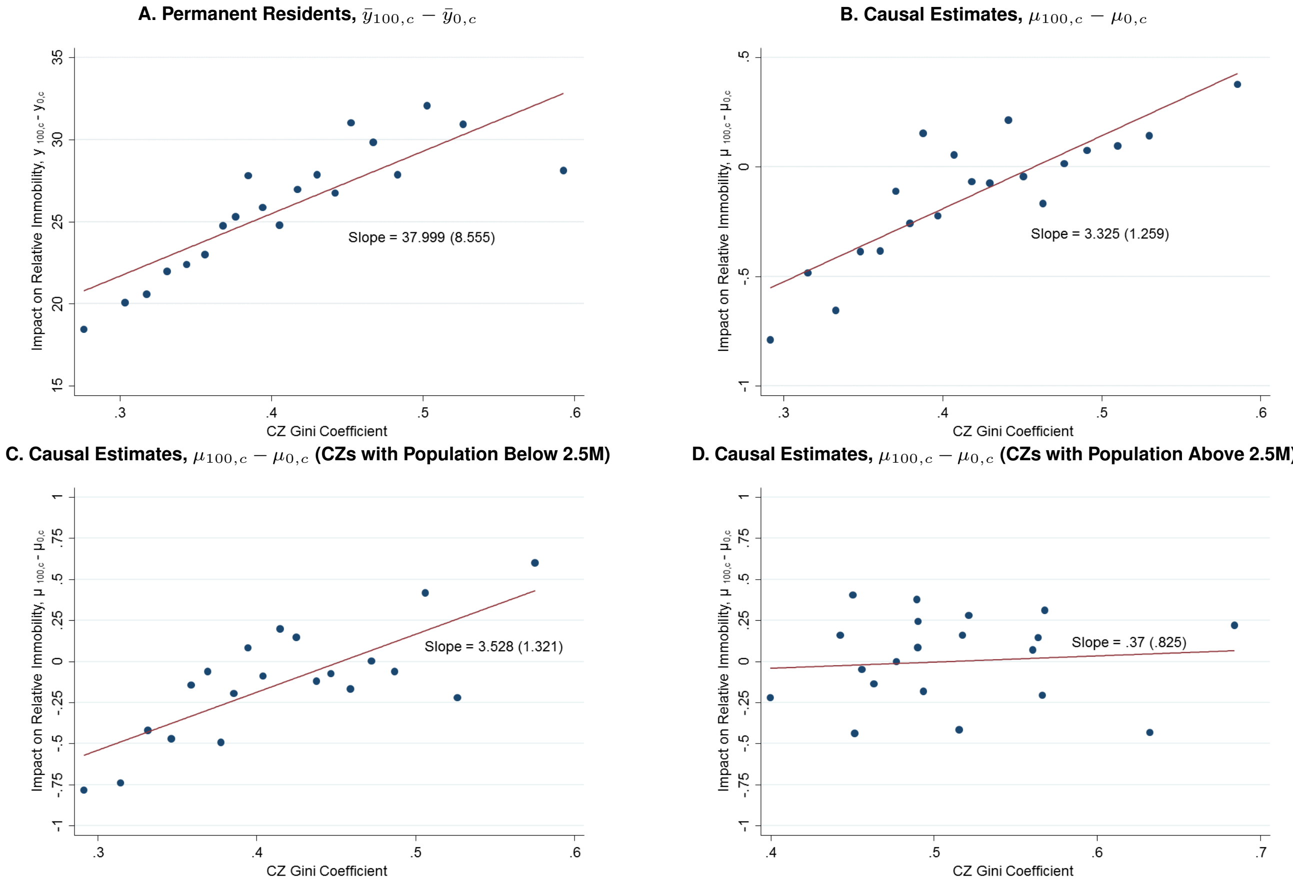
FIGURE VII: Predictors of Exposure Effects For Children with Parents at 75^{th} Percentile





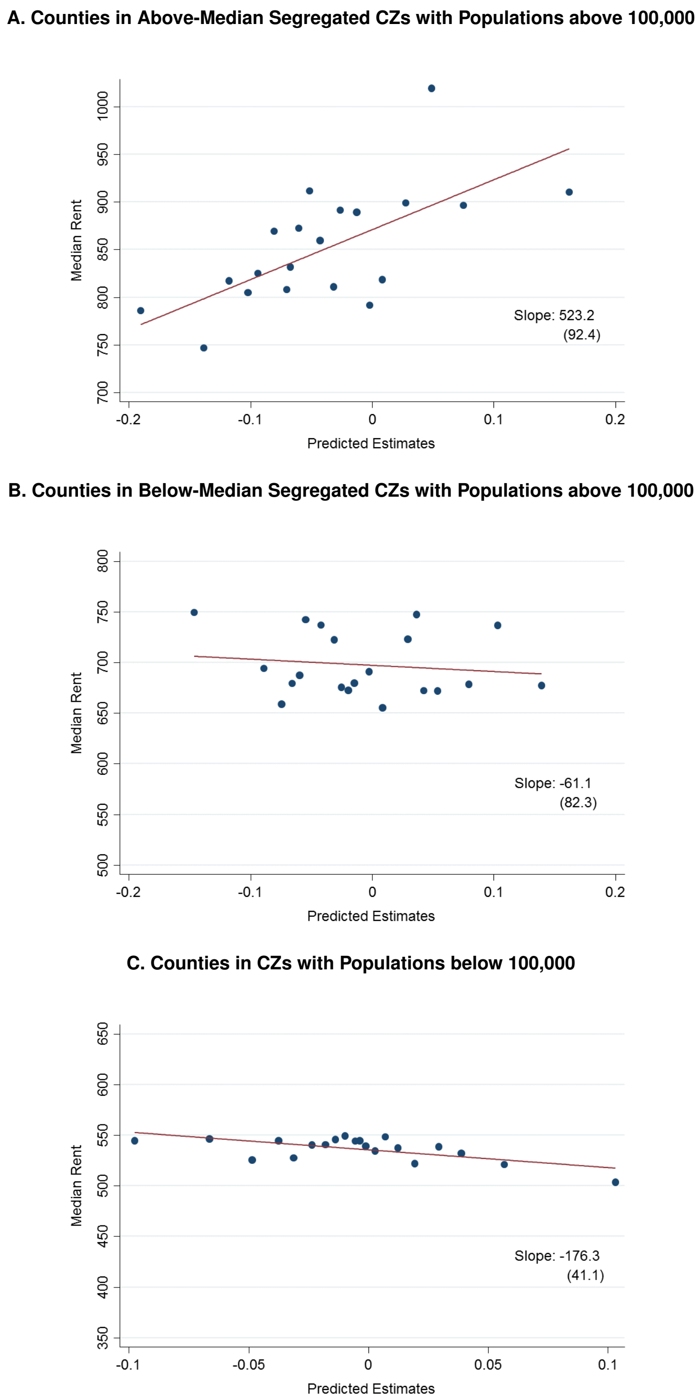
Notes: These figures repeat the panels in Figure VI using children in above-median families ($p = 75$) instead of below-median income families ($p = 25$).

FIGURE VIII: Income Inequality and the Great Gatsby Curve



Notes: This Figure illustrates the relationship between income inequality in a CZ and relative mobility (the difference in outcomes of children in high- and low-income families). Panel A presents a binned scatterplot of the relationship between the difference in outcomes of permanent residents, $\bar{y}_{100,c} - \bar{y}_{0,c}$, and the gini coefficient in the CZ. Panel B presents a binned scatterplot of the relationship between the difference in causal effects for children in high- and low-income families, $\hat{\mu}_{100,c} - \hat{\mu}_{0,c}$, and the gini coefficient in the CZ. Panel C (Panel D) repeats the analysis in Panel B restricting the sample to CZs with below (above) 2.5M residents based on the 2000 Census.

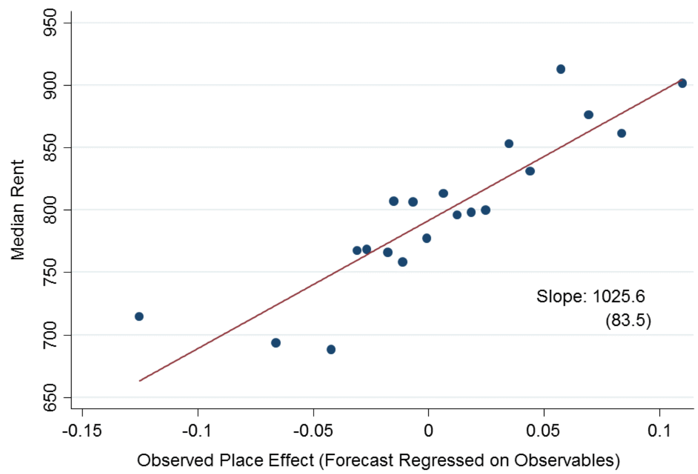
FIGURE IX: Median Rent versus Exposure Effects



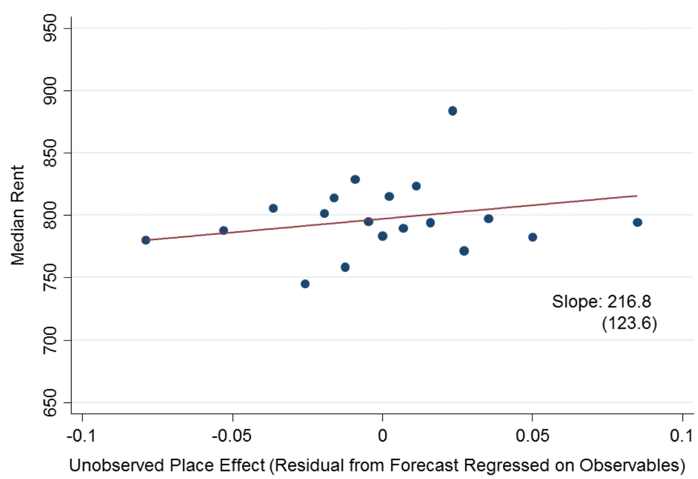
Notes: This figure presents binned scatterplots corresponding to a regression of median rent in the county (from the 2000 Census) on the predicted exposure effect for that county at $p = 25$, $\mu^f_{25,c}$. In contrast to the model in Section VI, we construct the forecasts $\mu^f_{25,c}$ using only 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 X: Median Rent versus Unobservable and Observable Exposure Effects

A. Median Rent versus Observable Component

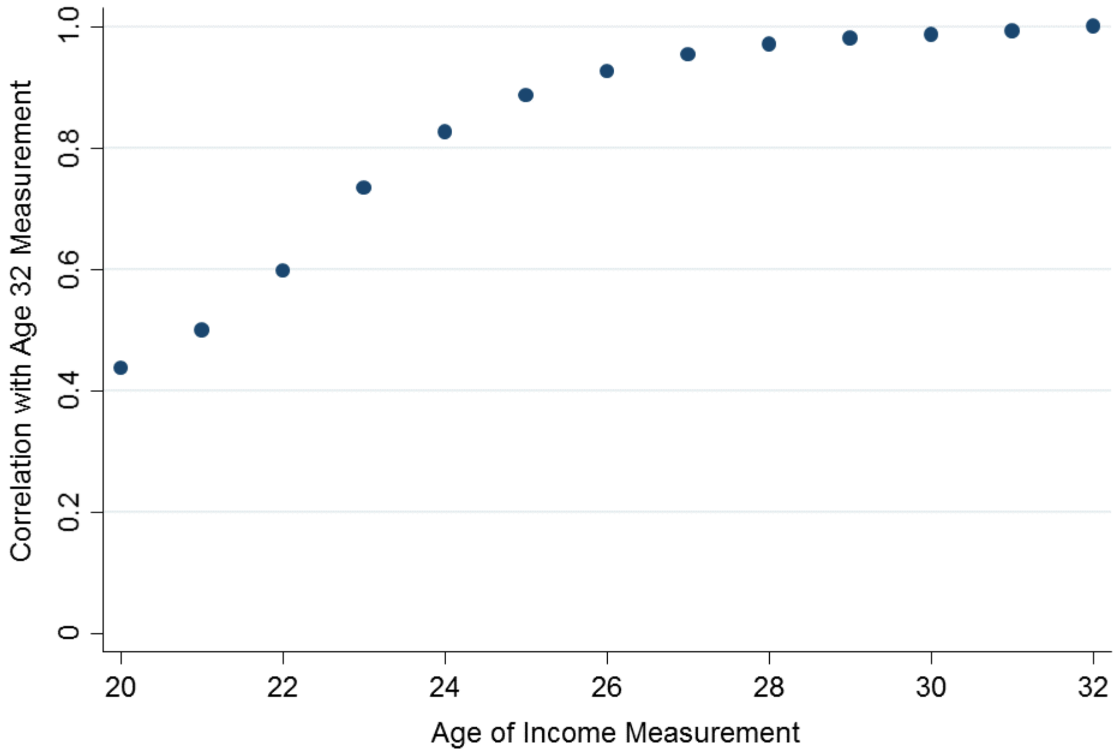


B. Median Rent versus Unobservable Component



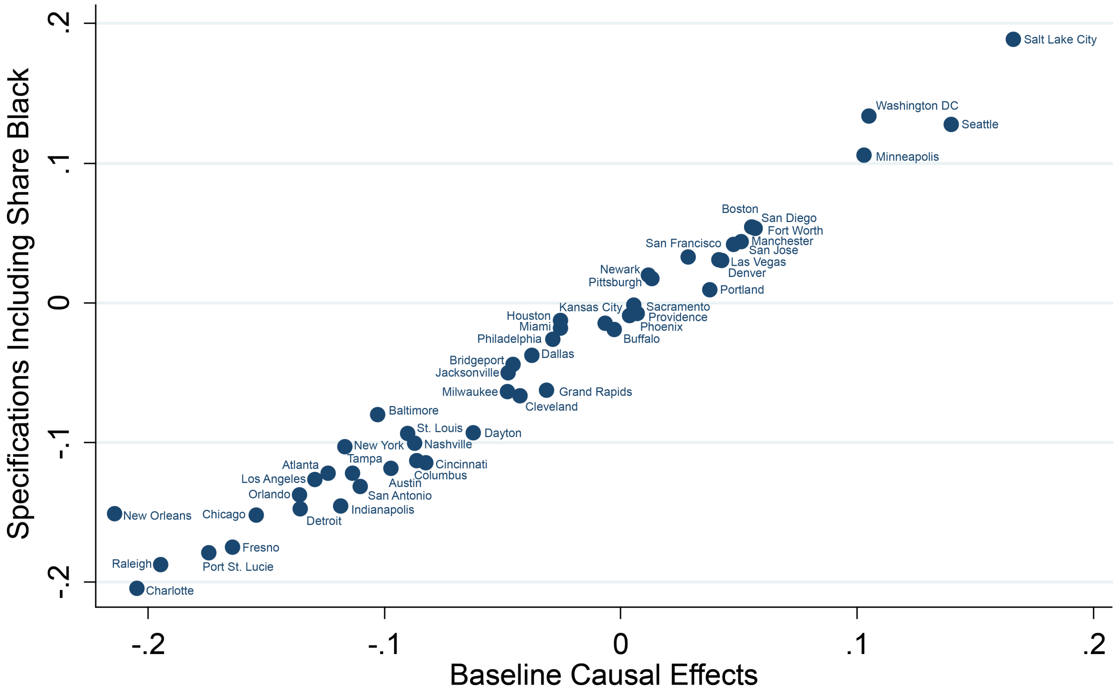
Notes: This figure presents binned scatter plots corresponding to a regression of median rent on the observable and unobservable components of the county-level forecasts, μ^f_{25c}, on the sample of CZs with populations above 100,000, conditional on CZ fixed effects. We construct the observable component 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 XV 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 vingtiles 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: Correlations of place effects by age (p25)



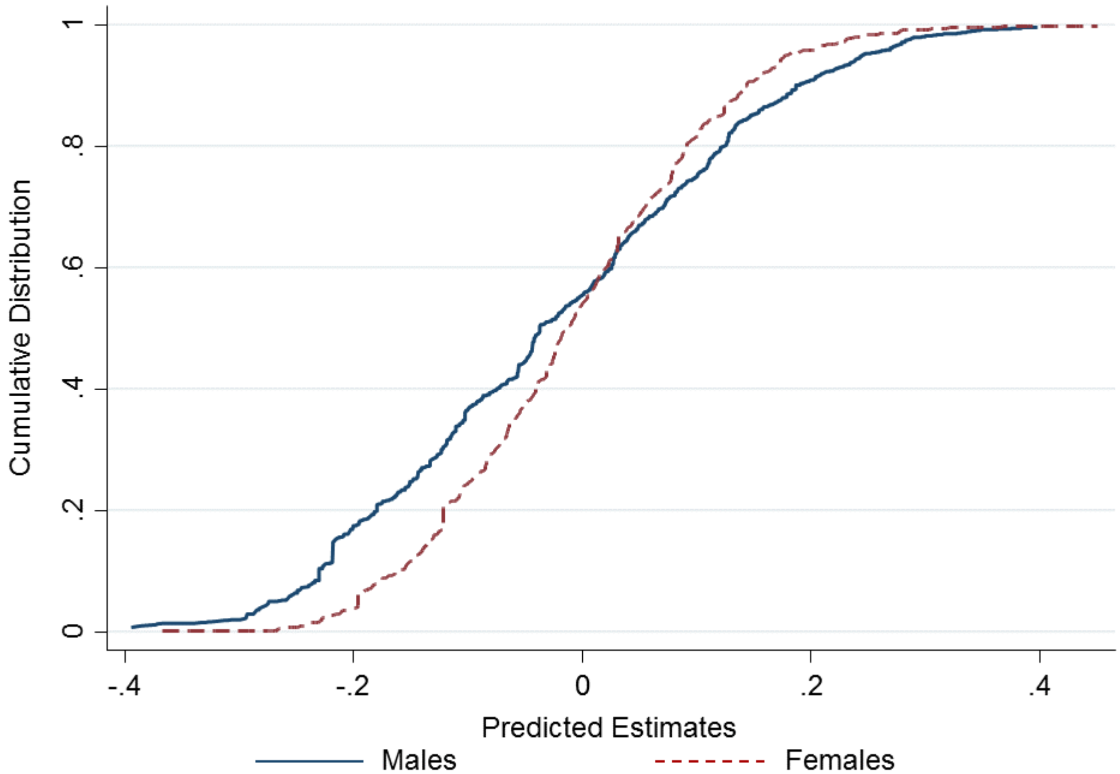
Notes: This figure presents the estimated correlation between $\bar{y}_{pc}$ across CZs when measured at age 32 with measurements at earlier ages (20-32). Correlations are weighted by CZ population in the 2000 Census. The vertical axis presents the estimated correlation; the horizontal axis corresponds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

ONLINE APPENDIX FIGURE II: Forecasts using Both Permanent Residents and Fraction African American Residents



Notes: This figure presents a scatter plot for the 50 largest CZs comparing our baseline forecasts which use the causal effect estimates and permanent resident estimates with alternative forecasts that also include the fraction of African American residents in the prediction in equation (8). We regress $\hat{\mu}_{25,c} = \gamma_p \bar{y}_{25,c} + \gamma_{25}^{black} FractionBlack_c$ and replace $\gamma_p \bar{y}_{25,c}$ with $\gamma_p \bar{y}_{25,c} + \gamma_{25}^{black} FractionBlack_c$. We then use the weights in equation (9) but construct the residual variance after subtracting both the permanent resident and FractionBlack components, $\chi_{25}^2 = Var\left(\mu_{25,c} - \gamma_{25}\bar{y}_{25,c} - \gamma_{25}^{black} FractionBlack_c\right)$. The horizontal axis presents the baseline forecast; the vertical axis presents the alternative forecast.

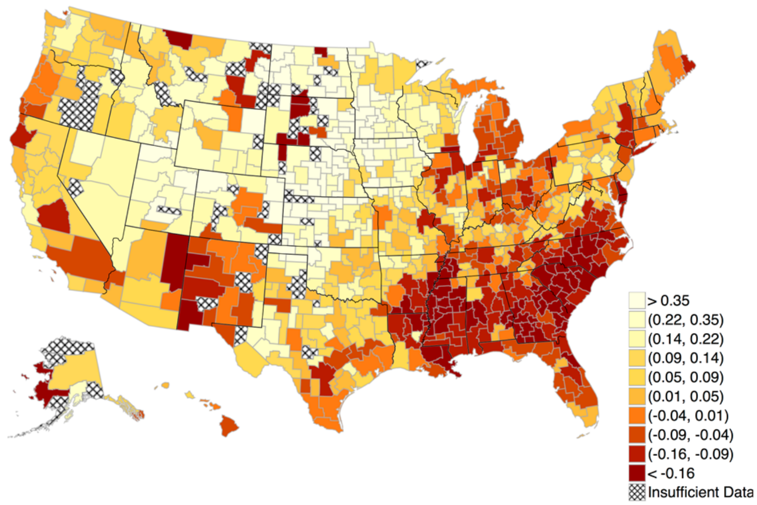
ONLINE APPENDIX FIGURE III: Distribution of Predicted Values by Gender



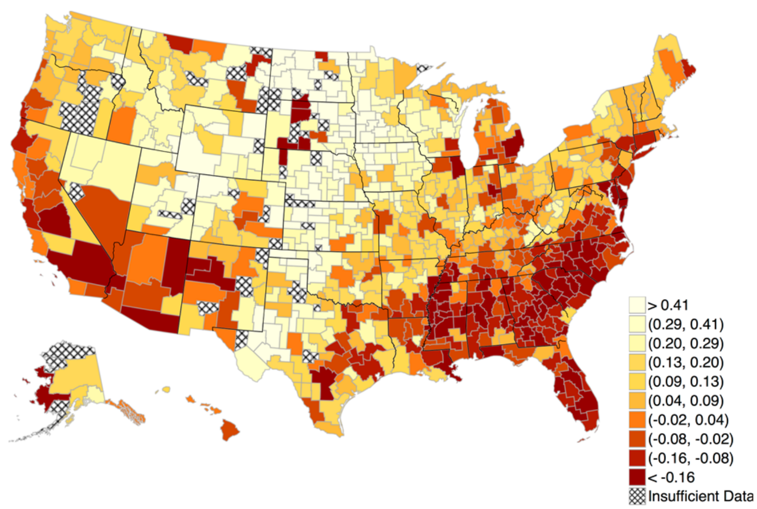
Notes: This figure presents the cumulative distribution of the gender-specific forecasts of county exposure effects for family income for children in below-median (p25) income families, $\mu^{f}_{25,c}$. The solid (blue) line presents the cumulative distribution for male forecasts. The dashed (red) line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

ONLINE APPENDIX FIGURE IV: Forecasts by CZ: Male and Female

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



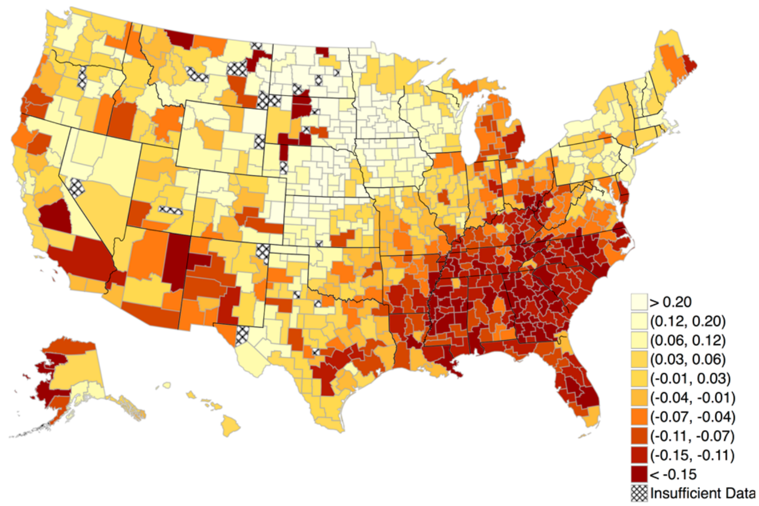
B. Female ($\mu_{25,c}$)



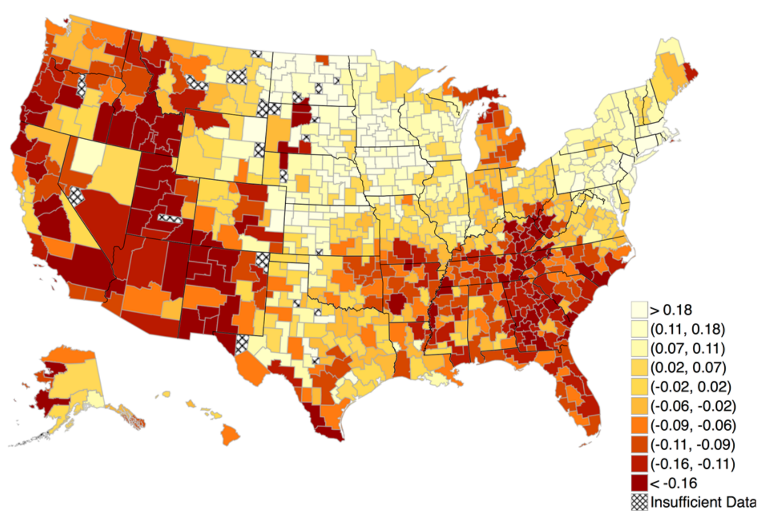
Notes: These figures present forecast estimates of each CZ's causal effects on family income for children in below-median ($p = 25$) families on separate samples of male (Panel A) and female (Panel B) children. We compute these forecasts using the methodology discussed in Section VI but do so separately conditional on child gender.

ONLINE APPENDIX FIGURE V: Forecasts by CZ: Individual Incomes

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



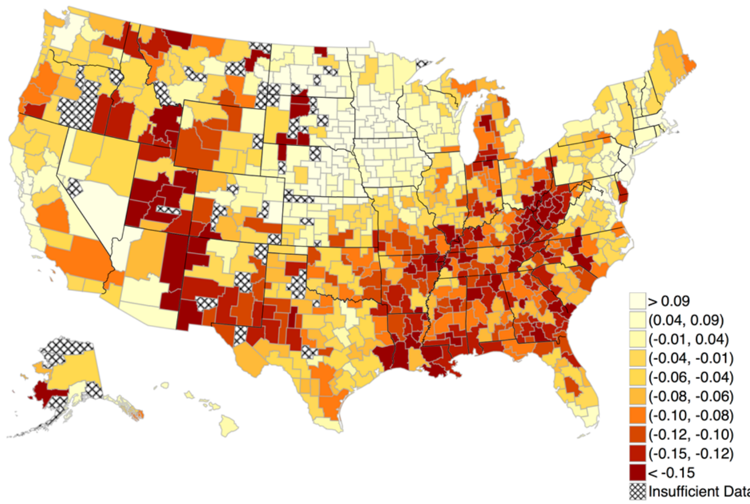
B. At 75th Percentile ($\mu_{75,c}$)



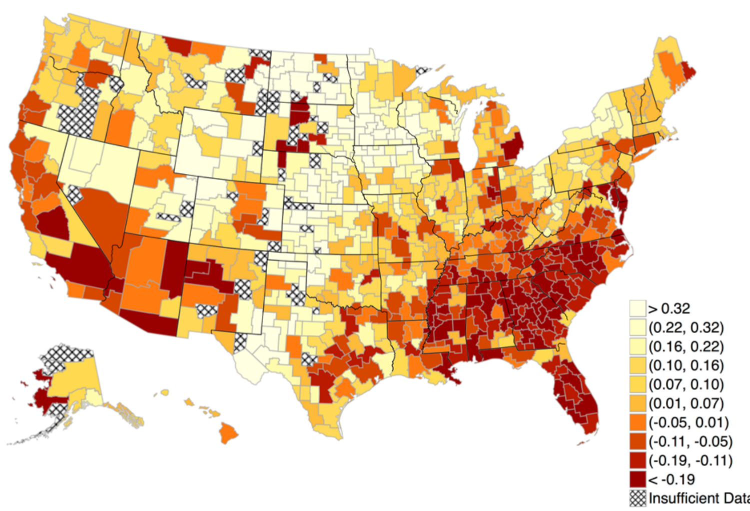
Notes: These figures present forecast estimates of each CZ's causal effects on individual income (as opposed to family income, shown in Figure IV), μ^f_{pc}, for below-median ($p = 25$) and above-median ($p = 75$) income families. We compute these forecasts using the methodology discussed in Section VI but replacing a child's family income with individual income.

ONLINE APPENDIX FIGURE VI: Forecasts by CZ: Male and Female 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 compute these forecasts using the methodology discussed in Section VI but replacing a child's family income with individual income, and separately estimating the models conditional on child gender.

Table I
Summary Statistics for Movers

Variable	Mean (1)	Std. Dev. (2)	Median (3)	Sample Size (4)
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
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

Notes: This table presents summary statistics for the commuting zone and county movers samples used for the fixed effect estimation.

Table II
Quantification of Place 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: Quantification						
Causal effect of Childhood Exposure (SD)						
A=20	2.647	2.139	3.308	3.092	1.984	2.233
A=12	1.588	1.283	1.985	1.855	1.190	1.340
A=23	3.044	2.459	3.804	3.556	2.281	2.568
Impact of 20 Years of Exposure to 1SD Higher Place						
Dollars ($)	2165	1796	2706	2596	1623	1875
% Increase in Earnings	8.3%	4.4%	10.4%	6.4%	6.2%	4.6%

Notes: This table quantifies the size of place effects using the estimated variances of the fixed effects model. 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 ($1/SE^2$, 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, $1/SE^2$). 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 quantifies the size of the effects for various assumptions for the length of childhood exposure, A. The first three rows present the standard deviation of the causal effects multiplied by 20, 12, and 23 years of exposure. The final two rows scale the estimates for A=20 into dollar increases in child earnings and % increases in child earnings. To construct this scaling factor, we regress mean income of permanent residents in each CZ for parents at each income percentile on the mean rank outcomes of children at that parent income percentile. 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. For p=75, this yields a coefficient of $840. The final row scales this estimate by the mean earnings of children at the parent income percentile of 26,091 for p=25 and $40,601 for p=75. 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 III
Forecasted 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: This table presents per-year exposure forecasts for the 50 largest CZs using the estimation strategy discussed in the text. Column (1) reports the forecast for the child's family income rank at age 26. Column (2) reports the root mean square error for this forecast 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 p75 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 IV
Forecasted 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: This table presents per-year exposure forecasts for the top 25 and bottom 25 largest counties using the estimation strategy discussed in the text, sorted by the impact on family income rank for children in below-median (p25) income families. Column (1) reports the forecasts for the child's family income rank at age 26. Column (2) reports the root mean square error for this forecast, 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 p75 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 V

Regressions of Place Effects Across 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)		Residual/Sorting (5)	
		Std. Dev	Correlation	s.e.	Coeff	(s.e.)	Coeff	(s.e.)	Coeff	(s.e.)
Segregation and Poverty	Fraction Black Residents	0.100	-0.514	(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 XV 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 Appendix Table I) 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 VI

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)		Residual/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 XV 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 Appendix Table I) 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 I
Alternative Specifications for Fixed Effects

Variable	Below Median Income Parents (p=25th percentile)					Above Median Income Parents (p=75th percentile)				
	Regression On Baseline		Signal SD			Regression On Baseline		Signal SD		
	Coeff (1)	s.e. (2)	Estimate (3)	s.e. (4)	p-value (5)	Coeff (6)	s.e. (7)	Estimate (8)	s.e. (9)	p-value (10)
Panel A: CZ Correlations										
Baseline	1.000	0.000	0.132	0.019	0.000	1.000	0.000	0.107	0.025	0.009
1. Placebo (>23) moves	0.041	0.086	0.000	0.054	0.691	0.008	0.083	0.091	0.056	0.360
2. Marital Status Controls	0.984	0.010	0.159	0.017	0.000	0.963	0.008	0.119	0.024	0.002
3. Parent Income Controls	0.996	0.001	0.155	0.017	0.000	0.996	0.001	0.128	0.020	0.001
4. Quadratic Income	0.974	0.015	0.144	0.020	0.000	0.994	0.016	0.134	0.024	0.000
5. COLI adjusted	0.952	0.035	0.230	0.014	0.000	0.979	0.028	0.206	0.016	0.000
6. Males	1.109	0.046	0.213	0.028	0.000	0.995	0.044	0.104	0.041	0.154
7. Females	0.977	0.045	0.160	0.035	0.008	0.999	0.045	0.127	0.040	0.069
8. Individual Income	0.776	0.024	0.126	0.020	0.000	0.757	0.026	0.119	0.025	0.001
9. Males (Individual Income)	1.086	0.051	0.231	0.029	0.000	0.875	0.049	0.112	0.043	0.137
10. Females (Individual Income)	0.561	0.049	0.129	0.037	0.032	0.752	0.056	0.200	0.033	0.001
11. Family Income ($, not ranks)	803.065	13.926	132.677	15.955	0.000	974.707	20.710	143.620	27.160	0.001
12. Individual income ($, not ranks)	475.767	17.444	102.143	11.866	0.000	539.083	25.259	125.426	15.982	0.000
Panel B: County Correlations										
Baseline	1.000	0.000	0.178	0.026	0.000	1.000	0.000	0.169	0.026	0.000
1. Placebo (>23) moves	0.037	0.050	0.163	0.081	0.282	0.020	0.050	0.318	0.075	0.007
2. Marital Status Controls	0.995	0.003	0.186	0.026	0.000	0.986	0.003	0.161	0.028	0.005
3. Parent Income Controls	1.001	0.003	0.181	0.024	0.000	0.987	0.004	0.160	0.029	0.001
4. Quadratic Income	0.991	0.011	0.183	0.028	0.000	0.969	0.017	0.160	0.034	0.003
5. COLI adjusted	0.951	0.014	0.256	0.020	0.000	0.941	0.011	0.235	0.022	0.000
6. Males	1.042	0.026	0.275	0.036	0.000	1.036	0.026	0.275	0.035	0.000
7. Females	0.981	0.024	0.174	0.050	0.049	1.013	0.025	0.202	0.047	0.013
8. Individual Income	0.744	0.013	0.160	0.027	0.002	0.736	0.014	0.185	0.025	0.000
9. Males (Individual Income)	0.961	0.027	0.284	0.037	0.001	0.885	0.029	0.252	0.042	0.001
10. Females (Individual Income)	0.557	0.026	0.128	0.048	0.182	0.671	0.030	0.294	0.032	0.000
11. Family Income ($, not ranks)	819.408	9.480	0.000	11.287	0.941	967.914	15.808	365.241	180.694	0.000
12. Individual income ($, not ranks)	464.679	9.878	0.000	14.803	0.848	540.948	14.516	283.256	139.961	0.000

Notes: This table presents results from alternative fixed effect specifications. Columns (1) and (2) report the coefficient and standard error from a regression of the alternative estimate on the baseline estimate. Column (3) presents the estimated signal SD of the alternative estimate, along with its bootstrapped standard error (Column (4)) and p-value for a test that it differs from zero (Column (5)). Columns (6)-(10) repeat the analysis in Columns (1)-(5) for children in above-median income families, p=75, instead of p=25. Row 1 considers a placebo specification using moves when children are above age 23. Rows 2 and 3 report results when including additional marital status controls and parental income controls discussed in the text. Row 4 adds a quadratic term in parental income to our baseline linear specification. Row 5 adjusts parent and child cost of living using the methods in Chetty et al. (2014). Row 6 (Row 7) considers family income for males (females). Row 8 considers individual income, and rows 9-10 split this by gender. Finally, rows 11 and 12 consider family and individual income in levels, as opposed to ranks. All signal SD standard errors are bootstrapped using 1000 iterations.

Appendix Table II
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.

Appendix Table III
Forecasted Place Effects for 50 Largest CZs for Below-Median Income Parents (p25)

Commuting Zone	State	Male Family Income			Female Family Income			Pooled Spec			Average			Row Number
		Prediction (1)	RMSE (2)	% Increase (3)	Prediction (4)	RMSE (5)	% Increase (6)	Prediction (7)	RMSE (8)	% Increase (9)	Prediction (10)	RMSE (11)	% Increase (12)	
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: This table presents per-year exposure forecasts by gender for the 50 largest CZs. Estimates are for children in below-median (p25) income families. Column (1) reports the forecasts 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.

Appendix Table IV

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

		Male Family Income			Female Family Income			Pooled Spec			Average			Row Number
County	State	Prediction (1)	RMSE (2)	% Increase (3)	Prediction (4)	RMSE (5)	% Increase (6)	Prediction (7)	RMSE (8)	% Increase (9)	Prediction (10)	RMSE (11)	% Increase (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: This table presents per-year exposure forecasts 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 forecasts 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.

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

		Male Individual Income			Female Individual Income			Pooled Spec			Average			
Commuting Zone	State	Prediction (1)	RMSE (2)	% Increase (3)	Prediction (4)	RMSE (5)	% Increase (6)	Prediction (7)	RMSE (8)	% Increase (9)	Prediction (10)	RMSE (11)	% Increase (12)	Row Number
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 forecasts on individual income by gender for the 50 largest CZs. Estimates are for children in below-median (p25) income families. Column (1) reports the forecasts 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 VI
Forecasted 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 forecasts 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 forecasts 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 VII
Forecasted Place Effects on Marriage at Age 26 for 50 Largest CZs

Commuting Zone	State	Below-Median Parent Income (p25) Prediction (1)	Below-Median Parent Income (p25) RMSE (2)	Above-Median Parent Income (p75) Prediction (3)	Above-Median Parent Income (p75) RMSE (4)	Row Number
Salt Lake City	UT	0.541	0.106	0.788	0.034	(1)
Portland	OR	0.203	0.100	0.024	0.034	(2)
Grand Rapids	MI	0.196	0.109	0.352	0.034	(3)
Fort Worth	TX	0.157	0.092	0.195	0.034	(4)
Sacramento	CA	0.129	0.085	-0.067	0.033	(5)
Dayton	OH	0.104	0.109	0.222	0.034	(6)
San Diego	CA	0.104	0.084	-0.158	0.033	(7)
San Antonio	TX	0.094	0.095	0.053	0.034	(8)
Nashville	TN	0.076	0.105	0.211	0.034	(9)
Kansas City	MO	0.056	0.104	0.110	0.034	(10)
Seattle	WA	0.036	0.088	-0.049	0.033	(11)
Houston	TX	0.028	0.078	-0.015	0.033	(12)
Austin	TX	0.025	0.100	-0.010	0.034	(13)
Columbus	OH	0.023	0.102	0.081	0.034	(14)
Dallas	TX	0.023	0.082	0.037	0.033	(15)
Fresno	CA	0.009	0.095	0.093	0.034	(16)
Phoenix	AZ	-0.016	0.077	0.092	0.033	(17)
Las Vegas	NV	-0.023	0.080	0.073	0.034	(18)
Denver	CO	-0.036	0.096	-0.051	0.033	(19)
Indianapolis	IN	-0.037	0.103	0.133	0.034	(20)
Jacksonville	FL	-0.054	0.095	0.048	0.034	(21)
Cincinnati	OH	-0.075	0.104	0.068	0.034	(22)
Minneapolis	MN	-0.077	0.099	-0.051	0.034	(23)
Pittsburgh	PA	-0.091	0.102	-0.090	0.034	(24)
Tampa	FL	-0.098	0.078	-0.081	0.033	(25)
San Jose	CA	-0.110	0.092	-0.348	0.033	(26)
San Francisco	CA	-0.114	0.082	-0.399	0.033	(27)
Manchester	NH	-0.125	0.103	-0.212	0.034	(28)
Atlanta	GA	-0.132	0.065	-0.084	0.032	(29)
Los Angeles	CA	-0.135	0.053	-0.226	0.031	(30)
St. Louis	MO	-0.136	0.102	0.004	0.034	(31)
Orlando	FL	-0.142	0.079	-0.056	0.034	(32)
Detroit	MI	-0.146	0.085	-0.139	0.033	(33)
Buffalo	NY	-0.152	0.097	-0.186	0.034	(34)
Charlotte	NC	-0.161	0.092	0.053	0.034	(35)
Providence	RI	-0.175	0.100	-0.311	0.034	(36)
Milwaukee	WI	-0.181	0.102	-0.063	0.034	(37)
Washington DC	DC	-0.203	0.080	-0.305	0.032	(38)
Raleigh	NC	-0.208	0.095	-0.023	0.034	(39)
Baltimore	MD	-0.222	0.091	-0.193	0.033	(40)
Cleveland	OH	-0.236	0.097	-0.129	0.034	(41)
Port St. Lucie	FL	-0.245	0.087	-0.210	0.033	(42)
Philadelphia	PA	-0.257	0.075	-0.326	0.032	(43)
Boston	MA	-0.305	0.085	-0.419	0.033	(44)
Bridgeport	CT	-0.316	0.086	-0.384	0.033	(45)
New Orleans	LA	-0.321	0.095	-0.064	0.034	(46)
Miami	FL	-0.326	0.071	-0.339	0.033	(47)
Chicago	IL	-0.330	0.069	-0.267	0.032	(48)
Newark	NJ	-0.378	0.069	-0.451	0.032	(49)
New York	NY	-0.462	0.055	-0.475	0.031	(50)

Notes: This table presents per-year exposure forecasts for marriage for the 50 largest CZs. Column (1) reports the exposure effect on the probability the child is married at age 26 for children in below-median income families (p25). The units are multipled by 100 to reflect probabilities, so that the coefficient of 0.541 implies that every year of exposure to Salt Lake City increases the chance of being married by 0.541pp; 20 years of exposure to Salt Lake City increases this chance by 10.8pp. 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. Columns (3)-(4) repeat the analysis for children in above-median income families (p75).

Appendix Table VIII
Forecasted Place Effects on Marriage at Age 26 for 100 Largest Counties

County	State	Below-Median Parent Income (p25) Prediction (1)	Below-Median Parent Income (p25) RMSE (2)	Above-Median Parent Income (p75) Prediction (3)	Above-Median Parent Income (p75) RMSE (4)	Row Number
Salt Lake	UT	0.434	0.127	0.483	0.167	(1)
El Paso	TX	0.188	0.121	-0.117	0.203	(2)
Macomb	MI	0.176	0.116	-0.103	0.147	(3)
Kern	CA	0.175	0.115	0.217	0.181	(4)
San Diego	CA	0.135	0.091	0.018	0.105	(5)
Hidalgo	TX	0.135	0.129	-0.045	0.220	(6)
Snohomish	WA	0.122	0.126	0.109	0.156	(7)
Tulsa	OK	0.118	0.129	0.287	0.193	(8)
Multnomah	OR	0.088	0.123	-0.063	0.159	(9)
Kent	MI	0.080	0.131	0.344	0.189	(10)
Dupage	IL	0.071	0.116	-0.200	0.122	(11)
Contra Costa	CA	0.068	0.116	-0.194	0.135	(12)
Sacramento	CA	0.061	0.116	0.036	0.152	(13)
Pierce	WA	0.055	0.123	0.163	0.155	(14)
Harris	TX	0.023	0.087	-0.125	0.096	(15)
Riverside	CA	0.020	0.090	0.008	0.116	(16)
Bexar	TX	0.018	0.141	-0.023	0.236	(17)
Oakland	MI	0.012	0.112	-0.034	0.128	(18)
Oklahoma	OK	0.009	0.124	0.151	0.187	(19)
San Bernardino	CA	0.000	0.085	0.010	0.112	(20)
Bernalillo	NM	-0.002	0.124	0.052	0.185	(21)
Maricopa	AZ	-0.003	0.083	0.226	0.092	(22)
Gwinnett	GA	-0.003	0.120	0.291	0.152	(23)
Tarrant	TX	-0.007	0.106	0.125	0.128	(24)
Cobb	GA	-0.010	0.124	-0.124	0.157	(25)
Milwaukee	WI	-0.304	0.125	-0.141	0.161	(75)
Norfolk	MA	-0.304	0.122	-0.342	0.143	(76)
Bergen	NJ	-0.320	0.123	-0.433	0.149	(77)
Mecklenburg	NC	-0.332	0.124	-0.229	0.168	(78)
Queens	NY	-0.334	0.082	-0.452	0.121	(79)
Hudson	NJ	-0.340	0.122	-0.350	0.171	(80)
Shelby	TN	-0.355	0.115	0.003	0.169	(81)
Fulton	GA	-0.356	0.106	-0.033	0.133	(82)
Wayne	MI	-0.356	0.106	-0.205	0.122	(83)
Middlesex	MA	-0.358	0.112	-0.448	0.122	(84)
San Francisco	CA	-0.362	0.122	-0.577	0.160	(85)
Fairfield	CT	-0.372	0.128	-0.439	0.167	(86)
DeKalb	GA	-0.381	0.109	-0.265	0.145	(87)
Broward	FL	-0.388	0.096	-0.357	0.141	(88)
Cook	IL	-0.396	0.081	-0.263	0.080	(89)
Cuyahoga	OH	-0.410	0.118	-0.345	0.150	(90)
Prince Georges	MD	-0.418	0.119	-0.488	0.157	(91)
Philadelphia	PA	-0.419	0.103	-0.227	0.126	(92)
Bronx	NY	-0.432	0.094	-0.769	0.160	(93)
Suffolk	NY	-0.446	0.112	-0.529	0.137	(94)
New York	NY	-0.449	0.093	-0.749	0.144	(95)
Suffolk	MA	-0.457	0.117	-0.438	0.156	(96)
Baltimore City	MD	-0.461	0.113	-0.459	0.153	(97)
Essex	NJ	-0.463	0.117	-0.483	0.145	(98)
Washington DC	DC	-0.471	0.122	-0.745	0.172	(99)
Nassau	NY	-0.512	0.104	-0.638	0.123	(100)

Notes: This table presents per-year exposure forecasts for marriage for the top 25 and bottom 25 amongst the 100 largest counties. Column (1) reports the exposure effect on the probability the child is married at age 26 for children in below-median income families (p25). The units are multipled by 100 to reflect probabilities as in Appendix Table VII. 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. Columns (3)-(4) repeat the analysis for children in above-median income families (p75).

Appendix Table IX

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)		Residual/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 XV 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 Appendix Table I) 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.

Appendix Table X

Regressions of Place Effects Across Counties within 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)		Residual/Sorting (5)	
		Std. Dev	Correlation	s.e.	Coeff	(s.e.)	Coeff	(s.e.)	Coeff	(s.e.)
Segregation and Poverty	Fraction Black Residents	0.130	0.137	(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 XV 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 Appendix Table I) 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 XI

Regressions of Place Effects For Males Across 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)		Residual/Sorting (5)	
		Std. Dev	Correlation	s.e.	Coeff	(s.e.)	Coeff	(s.e.)	Coeff	(s.e.)
Segregation and Poverty	Fraction Black Residents	0.100	-0.351	(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 V in the text using Place Effects and Permanent Residents characteristics For Males Only

Appendix Table XII

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)		Residual/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 VI in the text using Place Effects and Permanent Residents characteristics For Males Only

Appendix Table XIII

Regressions of Place Effects for Females Across 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)		Residual/Sorting (5)	
		Std. Dev	Correlation	s.e.	Coeff	(s.e.)	Coeff	(s.e.)	Coeff	(s.e.)
Segregation and Poverty	Fraction Black Residents	0.100	-0.430	(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 V in the text using Place Effects and Permanent Residents characteristics for Females Only

Appendix Table XIV

Regressions of Place Effects for Females 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)		Residual/Sorting (5)	
		Std. Dev	Correlation	s.e.	Coeff	(s.e.)	Coeff	(s.e.)	Coeff	(s.e.)
Segregation and Poverty	Fraction Black Residents	0.130	-0.371	(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 VI in the text using Place Effects and Permanent Residents characteristics For Females Only

Appendix Table XV
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 Outlflow 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)