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

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Abstract

We estimate the causal effect of each county in the U.S. on children's incomes in adulthood. We first estimate a fixed effects model that is identified by analyzing families who move across counties with children of different ages. We then use these fixed effect estimates to (a) quantify how much places matter for intergenerational mobility, (b) construct forecasts 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%. There is substantial variation in counties' causal effects even within metro areas. Counties with less concentrated poverty, less income inequality, better schools, a larger share of two-parent families, and lower crime rates tend to produce better outcomes for children in poor families. Boys' outcomes vary more across areas than girls' outcomes, and boys have especially negative outcomes in highly segregated areas. Areas that generate better outcomes have higher house prices on average, but our approach uncovers many "opportunity bargains" – places that generate good outcomes but are not very expensive.

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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 2017), we showed that neighborhoods have significant childhood exposure effects on children's life outcomes. Although those results establish 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 children's outcomes.

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 incomes 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.[1]

We estimate counties' causal effects on children's ranks in the income distribution at age 26 using data from de-identified tax returns for all children born between 1980 and 1986.[2] We estimate each county's effect using a fixed effects regression model identified by analyzing families who move across counties, exploiting variation in children's ages when families move. To understand how the model is identified, consider families in the New York area. If children who move from Manhattan to Queens at younger ages earn more as adults, we can infer that growing up in Queens has a positive causal effect relative to growing up in Manhattan under the assumption that other 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 income ranks at age 26 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 parent income. We 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 move.

[1] To maximize statistical precision, we characterize neighborhood (or "place") effects at the county level. We recognize that counties are much larger than the typical geographic units used to define "neighborhoods." In the presence of heterogeneity across local areas within counties, the county-level effects we estimate can be interpreted as weighted averages of the local area effects. In future work, the methods we develop here could be applied to estimate place effects at smaller geographies, such as Census tracts.

[2] We measure incomes at age 26 because children's mean ranks in each area tend to stabilize by age 26. For example, the (population-weighted) correlation between mean income ranks at age 26 and age 32 across commuting zones is 0.93 for children growing up in low-income (25th percentile) families and 0.77 for children growing up in high-income (75th percentile) families.

The key assumption required to identify counties' causal 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 where permanent residents (non-movers) have better or worse outcomes on average is orthogonal to their potential outcomes. However, it is a stronger requirement than the condition required to identify average exposure effects in our 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 placebo tests by (a) estimating each area's fixed effect on teenage labor force participation rates at age 16 (a strong predictor of incomes in adulthood), using the subsample of families who move *after* age 16 and (b) estimating each area's effect on income at age 26 using 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. These placebo fixed effect estimates are uncorrelated with our baseline estimates, supporting the assumption that the time at which parents move to a given county is orthogonal to their children's potential outcomes.

We use the estimates of counties' causal effects for three purposes. First, we quantify how much neighborhoods matter for children's incomes. We model the estimated county effects as the sum of a latent causal effect and noise due to sampling error, and estimate the signal variance of the latent causal effects. 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, equivalent to an increase in mean income of approximately 0.5%. Extrapolating over 20 years of childhood, growing up in a 1 SD better county from birth would increase a child's household income in adulthood by approximately 10%.[3]

[3]We focus primarily on estimates of place effects on household income (including spousal income), but also report estimates using individual income below. The household income estimates are highly correlated with the individual income estimates for males, whose outcomes are typically used as a measure of economic opportunities in the literature on intergenerational mobility because the variance in earnings due to differences in labor force participation rates is smaller for men than women (e.g., Solon 1999).

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 incomes. 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 income. Importantly, areas that generate better outcomes for children in low-income families generate slightly better outcomes on average for children in high-income families as well. 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 forecasts 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 forecasts that minimize the mean-squared-error (MSE) of the predicted impact of growing up in a given neighborhood relative to the true impact. Although the raw county fixed effects provide unbiased estimates of counties' causal effects, they do not themselves provide good forecasts because many of the estimates have substantial noise, leading to high MSE. In highly populated counties, such as Cook County (the city of Chicago), nearly 75% of the variance in the fixed effect estimates is signal; however, in most counties, more than half of the variance in the fixed effect estimates is due to noise from sampling variation.

To obtain forecasts that have lower MSE, we use a shrinkage estimator that incorporates data on the permanent residents' (non-movers) outcomes in each area.[4] The permanent residents' mean outcomes have very little sampling error, but are imperfect forecasts of a county's causal effect because they combine causal effects with sorting. The best MSE-minimizing linear forecast 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, with greater weight on the fixed effect estimate when it is more precisely estimated (i.e., in large counties).

Among the 100 most populated counties in the country, DuPage County, IL is forecasted to generate the highest incomes for children growing up in low-income (25th percentile) families. Each additional year that a child in a low-income family spends in DuPage County, IL instead of the average county in the U.S. raises his or her household income in adulthood by 0.80%. Growing

[4] Our methodology contributes to a recent literature that builds on empirical Bayes methods dating to Robbins (1956) by using shrinkage estimators to reduce mean-squared-error (risk) when estimating a large number of parameters. For instance, Angrist et al. (2017) combine experimental and observational estimates to improve forecasts of school value-added. Our methodology differs from theirs because we have unbiased (quasi-experimental) estimates of causal effects for every area, whereas Angrist et al. have unbiased (experimental) estimates of causal effects for a subset of schools. Hull (2017) develops methods to forecast hospital quality, permitting nonlinear and heterogeneous causal effects. Abadie and Kasy (2017) show how machine learning methods can be used to reduce risk, using the fixed effect estimates constructed in this paper as an application.

up in DuPage County from birth – i.e., having about 20 years of exposure to that environment – would raise such a child's income by 16%. In contrast, growing up in Cook County – one of the lowest-ranking counties in the U.S. – from birth reduces a child's income by approximately 13%. Hence, moving from Cook County (the city of Chicago) to DuPage County (the western suburbs) at birth would increase a child's income by about 30% on average.[5]

We find that neighborhoods matter more for boys than girls: the signal standard deviation of county-level effects is roughly 60% 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 City and Wayne County (Detroit) produce especially negative outcomes for boys.[6]

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 Chetty and Hendren (2017) – but there are many significant differences. For example, children who grow up in low-income families in New York City have outcomes comparable to the national mean, but the causal effect of growing up in New York City – as revealed by analyzing individuals who move into and out of New York – is well below the national mean. One potential explanation for this pattern is that New York has a large share of immigrants, who tend to have high rates of upward mobility (Hilger 2016). More generally, this example illustrates the importance of estimating the causal effect of each area directly using movers (as we do in this paper) rather than predicting neighborhood effects purely from permanent residents' outcomes.

In the third part of this paper, we characterize the properties of CZs and counties that produce good outcomes for low-income children (i.e., generate high rates of upward mobility). Prior work has shown that in observational data, upward mobility is highly correlated with area characteristics such as residential segregation, income inequality, social capital, and school quality as well as demographic characteristics such as the fraction of children being raised by single mothers and racial shares (Wilson 1987; 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. For instance, is growing

[5]Interestingly, many families involved in the well-known Gautreaux housing desegregation project moved from Cook County to DuPage County. Our results support the view that much of the gains experienced by the children of the families who moved as part of Gautreaux (Rosenbaum 1995) was due to the causal effect of exposure to better neighborhoods.

[6]These gender differences are partly related to differences in rates of marriage. For example, the San Francisco area generates high individual incomes but relatively low household incomes for girls because growing up in San Francisco reduces the probability that a child gets married.

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 decompose the correlations documented in prior work into causal vs. sorting components by correlating each characteristic with both our causal effect estimates and permanent residents' outcomes, which combine both causal and selection effects. We find that many of the correlations between area-level characteristics and upward mobility are driven almost entirely by causal effects of place. For example, 80% of the association between segregation and upward mobility across CZs in observational data is driven by the causal effect of place; only 20% is due to sorting. Growing up in a CZ with a one standard deviation higher level of segregation from birth reduces the income of a child in a low-income (25th percentile) family by 5.2%.[7] Urban areas, particularly those with concentrated poverty, generate particularly negative outcomes for low-income children. These findings support the view that growing up in an urban "ghetto" reduces children's prospects for upward mobility (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 generate significantly worse outcomes for children in low-income families. Hence, the negative correlation between inequality and intergenerational 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 areas with different levels of inequality. Rather, putting a given child in an area 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% shares contrasts with the findings of Chetty et al. (2014), who find no correlation between top 1% shares (upper-tail inequality) and rates of upward mobility in observational data. Our analysis of movers reveals 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 and the causal effect of places of upward mobility in observational data.

We find strong correlations between areas' causal effects and output-based measures of school quality, such as test scores adjusted for parent income levels. We also find strong correlations between the causal effects and proxies for social capital, such as crime rates and Rupasingha and Goetz's (2008) summary index.

Selection plays a bigger role in explaining correlations between demographic characteristics and

[7] 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.

upward mobility in observational data. For example, the fraction of single mothers is the single strongest predictor of differences in upward mobility for permanent residents across areas. However, the fraction of single mothers – although still a significant predictor – is less highly correlated with CZs' causal effects on upward mobility than other factors such as segregation. This is because nearly half of the association between permanent residents' outcomes and the fraction of single mothers is due to selection. Similarly, areas with a larger African-American population have significantly lower rates of upward mobility in observational data. Roughly half of this association is also unrelated to causal effect of place, consistent with Rothbaum (2016). Nevertheless, the correlation between the causal effects of place and the African-American share remains substantial (-0.51 across CZs and -0.37 across counties within CZs). Place effects therefore amplify racial inequality: black children have worse economic outcomes because they grow up in worse neighborhoods.

Finally, we examine 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 produce better outcomes for children have slightly higher rents, especially in highly segregated cities. However, rents explain less than 5% of the variance in counties' causal effects for families at the 25th percentile. This result suggests that current "small area" fair market rent proposals in housing voucher programs – which condition voucher payments on local neighborhood rents – may not maximize vouchers' effects on upward mobility, as many areas that are more expensive do not produce better outcomes. Moreover, it shows that some areas are "opportunity bargains" – counties within a labor market that offer good outcomes for children without higher rents.[8] For example, in the New York metro area, Hudson County, NJ offers much higher levels of upward mobility than 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 area-level factors, such as poverty rates and school expenditures, and a residual "unobservable" component. We find that only the observable component is capitalized in rents, suggesting that the opportunity bargains may partly exist because families do not know which neighborhoods have the highest value-added. This result underscores the importance of measuring neighborhood quality directly using children's observed outcomes instead of using traditional proxies such as poverty rates.

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

[8] 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 is simply that housing costs themselves are not necessarily a barrier to moving to opportunity.

differences relative to the sample used in our first paper. In Section III, we formalize our empirical objectives using a statistical model. Section IV reports the baseline fixed effect estimates and evaluates the validity of the key identification assumptions. Section V quantifies the magnitude of place effects, Section VI presents the CZ- and county-level MSE-minimizing forecasts, and Section VII examines the characteristics of places that generate better outcomes. Section VIII presents the results on housing costs and opportunity bargains. Section IX concludes. Supplementary results and details on estimation methodology are provided in an online appendix. Estimates of CZs' and counties' causal effects are available on the Equality of Opportunity Project website.

II DATA AND SAMPLE DEFINITIONS

We use data from federal income tax records spanning 1996-2012. Our primary analysis sample is the same as the sample of movers in Chetty and Hendren (2017, Section II), with three exceptions.

First, we limit the sample to children in the 1980-86 birth cohorts because we measure children's incomes at age 26. Measuring children's incomes at age 26 strikes a balance between the competing goals of minimizing lifecycle bias by measuring income at a sufficiently old age and having an adequate number of birth cohorts to implement our research design. Among permanent residents (parents who stay in the same CZ from 1996-2012) at the 25th percentile of the income distribution, the population-weighted correlation between children's mean ranks at age 26 and age 32 across CZs is 0.93.[9] This suggests that measuring children's incomes at later ages would not affect our estimates of places' causal effects substantially.

Second, we focus on the subset of families who move across CZs or counties when their child is 23 or younger, motivated by our finding that childhood exposure effects persist until age 23. To simplify estimation, we focus on families who move exactly once during the sample period, dropping those who move multiple times. We divide the sample of one-time movers into two groups – those who move across CZs and those who move across counties within CZs – and analyze these two sets of movers separately.

Third, to maximize precision, we include all movers – not just those who move between large CZs – in our analysis sample. However, we only report estimates of causal effects for CZs with populations above 25,000 and counties with populations above 10,000 in the 2000 Census (excluding

[9] The key point here is that children's *average* ranks in each area stabilize by age 26. At the *individual* level, children's incomes stabilize later, around age 32 (Haider and Solon 2006, Figure 2a). Intuitively, children's ranks change rapidly in their late 20s, but these idiosyncratic individual-level changes average out at the area level, so that areas where children have high ranks at 26 tend to have high ranks at age 32 as well.

0.36% of the population).[10]

We measure children's and parents' incomes at the household level using data from 1040 forms (for those who file tax returns) and W-2 forms (for non-filers), which we label family (or household) income. We identify individuals' locations in each year using the ZIP code from which they filed their tax returns or to which their W-2 forms were mailed. We also measure a set of additional outcomes for children, such as individual income, college attendance, and marriage. All of these variables are defined in the same way as described in Section II of Chetty and Hendren (2017).

Table I presents summary statistics for children in our primary sample who move across CZs (Panel A) and counties within CZs (Panel B). There are 1,397,260 children whose parents move once across CZs and 931,138 children whose parents move across counties within CZs in our primary sample. The sample characteristics are generally very similar to those reported in Table I of Chetty and Hendren (2017), with a median family income of $24,253 at age 26 for children in the CZ movers sample and $24,993 in the county-within-CZ movers sample (in 2012 dollars).

III EMPIRICAL FRAMEWORK

In this section, we first define the estimands we seek to identify using a statistical model of neighborhood effects. We then describe the research design we use to identify these parameters and the key identification assumptions underlying our analysis. Finally, we discuss the empirical specification and estimation procedures we use to implement this research design.

III.A Statistical Model

We estimate place effects using a statistical model motivated by the childhood exposure effects documented in Chetty and Hendren (2017). Let y_i denote a child's income (or other outcome) in adulthood, measured at age T. We model y_i as a function of three factors: the neighborhoods in which the child grows up, disruption costs of moving across neighborhoods, and all other non-neighborhood inputs, such as family environment and genetics.

Let $c(i, a)$ denote the place in which child i lives at age $a = 1, ..., A$ of his childhood, where $A < T$. Let μ_c denote the causal effect of one additional year of exposure to place c on the child's

[10] In Chetty and Hendren (2017), we limited our primary sample to CZs with populations above 250,000 to minimize attenuation bias in exposure effect estimates resulting from noise in permanent residents' outcomes. This attenuation bias does not arise here because we identify causal effects purely from the sample of movers, without projecting their outcomes onto permanent residents. This is why we impose lower population restrictions here, providing estimates for a larger set of CZs. Because our goal is to provide estimates of place effects at the county (rather than CZ) level, we also do not impose any restrictions on the distance of moves we examine.

outcome y_i.[11] Given the linear childhood exposure effects documented in Chetty and Hendren (2017, Figure IV), we assume that the exposure effect μ_c is constant for ages $a \leq A$ and is zero thereafter. 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 genetic endowments, and the total amount of time-varying inputs, such as parental investments during childhood.

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

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

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).[12] Second, it assumes that place effects are additive and constant across ages, i.e., that there are no complementarities between neighborhood effects 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.[13] We believe these restrictions are reasonable approximations given the findings of our first paper, which show that childhood exposure effects are constant throughout childhood and symmetric when children move to areas with better or worse permanent resident outcomes on average (conditional on parent income). Nevertheless, we view estimating models that permit richer forms of heterogeneity in place effects as an important direction for future research.[14]

[11] In our empirical application, we permit place effects μ_{pc} to vary with parental income rank $p(i)$, but we suppress the parental income index in this section to simplify notation.

[12] This constant treatment effects assumption is a common simplification in the literature. For instance, in work on firm effects and teacher effects (Abowd et al. 1999, Chetty, Friedman, and Rockoff 2014a), analogous restrictions rule out worker-firm or teacher-student match effects. Our fixed effect estimates can be interpreted as mean place effects in the presence of heterogeneous treatment effects if such heterogeneity is orthogonal to individuals' transition rates across areas, i.e. as long as there is no "essential" heterogeneity. Understanding how the present estimates of μ_{pc} can be interpreted in the presence of essential heterogeneity and estimating models that permit richer forms of heterogeneity are important directions for further work.

[13] 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 of these extensions would affect our estimates. The key requirement for approach to identifying $\{\mu_c\}$ is that the disruption costs do not vary in an *age-dependent* manner across neighborhoods.

[14] We do not estimate such models here primarily because of a lack of adequate statistical power. As we will see below, obtaining precise estimates of 6,000 treatment effects (one per county, interacted with parent income) with our sample of approximately 3 million movers is itself challenging. Estimating higher-dimensional models may be feasible as additional years of tax data become available in the U.S. or using longer administrative panels in other countries.

Our objective in this paper is to identify $\vec{\mu} = \{\mu_c\}$, the causal exposure effect of spending a year of one's childhood in a given area (CZ or 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, Hendren, and Katz 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 Identifying Place Effects in Observational Data

Building upon the approach in Chetty and Hendren (2017), we identify $\vec{\mu}$ by exploiting variation in the *timing* of when children move across areas. To understand the intuition underlying our approach, consider a set of children who move from a given origin o (e.g., New York) to a given 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 the causal effect of growing up in Boston relative to New York ($\mu_d - \mu_o$) by comparing the outcomes of children who move at different ages; for instance, if those who move at younger ages have better outcomes, we learn that $\mu_d > \mu_o$.

Under the model in (1), we can combine information from all such pairwise comparisons to estimate each place's causal effect using the following fixed effects specification:

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

where α_{od} denotes an origin-by-destination fixed effect and $\vec{e}_i = \{e_{ic}\}$ is a vector whose entries denote the number of years of exposure that child i has to place c before age A. In a sample of children who move exactly once before age A, e_{ic} is given by

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

where m_i denotes the age of the child at the time of the move.

Equation (2) is a reduced form of the model in (1) for one-time movers, where $\alpha_{od} = \bar{\theta}_{od} - \kappa$ captures the sum of the disruption effect and the mean value of other inputs θ_i for children who move from o to d, while $\epsilon_i = \theta_i - \bar{\theta}_{od}$ captures idiosyncratic variation in other inputs. 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 to or from different areas.[15]

[15]The fixed effects $\vec{\mu}$ are identified up to the normalization that the average place effect is zero, $E[\mu_c] = 0$, because the matrix $\mathbf{E} = \{\vec{e}_i\}_i$ does not have full rank. Intuitively, using movers to identify place effects allows us to identify the effect of each place relative to the national average.

The identification assumption required to obtain consistent estimates of $\vec{\mu}$ when estimating (2) using OLS is the following 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:

$$Cov(e_{ic}, \epsilon_i) = 0 \;\forall c. \tag{3}$$

Assumption 2 requires that children with different exposure times to a set of places do not systematically differ in other inputs, $\epsilon_i = \theta_i - \bar{\theta}_{od}$, conditional on origin-by-destination fixed effects. This assumption is a stronger version of Assumption 1 in Chetty and Hendren (2017), which required that exposure to better places – as measured by the outcomes of permanent residents – is not correlated with ϵ_i *on average*. Assumption 2 extends that assumption to require that the amount of exposure to *every* place satisfies such an orthogonality condition. This stronger assumption allows us to go beyond establishing that neighborhoods have causal effects on average and characterize precisely which areas produce the best outcomes. We provide evidence supporting Assumption 2 in Section IV.B after presenting our baseline results.

III.C Empirical Implementation

In our empirical analysis, we generalize (2) to account for two features of the data that we omitted from the stylized model in Section III.A for simplicity.

First, we allow for the possibility that places may have different effects across parent income levels, as suggested by the maps of permanent residents' outcomes in Chetty and Hendren (2017, Figure II). To do so, we first measure the percentile rank of the parents of child i, $p(i)$, based on their positions in the national distribution of parental household income for child i's birth cohort. Chetty et al. (2014) show that within each area, children's expected income ranks are well approximated by a linear function of their parents' income rank $p(i)$. We therefore generalize (2) to allow place effects μ_c to vary linearly with parent income rank, $p\,(i)$. We denote the causal effect of place c at parent rank p by

$$\mu_{pc} = \mu_c^0 + \mu_c^1 p \tag{4}$$

where μ_c^0, the intercept, represents the causal effect of the place for children in the lowest-income families and μ_c^1, the slope, captures how the causal effect varies with parent rank. For symmetry, we also allow the origin-by-destination fixed effect α_{od} in (2) to vary linearly with parent rank p in order to capture potential heterogeneity in selection effects by parent income.

Second, because we measure children's incomes at a fixed age, we measure their incomes in different calendar years. In particular, incomes at age 26 are measured between 2006-2012 for children in our primary sample (the 1980-86 birth cohorts), a period with rapidly changing labor market conditions. We account for these fluctuations – allowing for differential shocks across areas and income groups – by including a control function

$$g_{od}(p,s) = \psi^0_{od}s + \psi^1_{od}s^2 + \psi^2_{od}sp + \psi^3_{od}s^2p \tag{5}$$

when estimating (2), where s denotes the child's birth cohort (or, equivalently, the year in which the child's income is measured). We show in Online Appendix A that alternative parameterizations of the $g(p,s)$ control function yield very similar results.

Incorporating these two extensions of (2), our baseline estimating equation is

$$y_i = \alpha_{od} + \alpha^P_{od}p + \vec{e}_i \cdot \vec{\mu}_p + g_{od}(p_i, s_i) + \varepsilon_i, \tag{6}$$

where α_{od} is an origin-by-destination fixed effect, $\alpha^P_{od}p$ is an origin-by-destination fixed effect interacted with parent rank, and $\vec{\mu}_p = \{\mu_{pc}\}$ denotes the vector of causal place effects parameterized as in (4).

Estimation: Hierarchical Structure. Our goal is to use (6) to identify the causal effects of places at two geographic levels: commuting zones – aggregations of counties that represent local labor markets – and counties. Directly estimating the 741 CZ-level and 3,138 county-level fixed effects along with the incidental parameters in (6) is not feasible because of computational constraints. We therefore estimate $\vec{\mu}_p$ using a hierarchical structure, separately estimating the causal effects of CZs and counties within CZs.

We begin by estimating CZ effects using our sample of cross-CZ movers. For computational tractability, we use a two-step estimator, described in detail in Online Appendix A. In the first step, we estimate (6) separately for each origin-destination (o,d) pair, which yields an estimate of the exposure effect for each origin relative to each destination, $\mu_{pod} = \mu_{pd} - \mu_{po}$, for each level of parental income, p.[16] We then consider a fixed parental income level (e.g. $p = 25$) and regress the pairwise effects $\{\mu_{pod}\}$ on a design matrix that consists of positive and negative indicators for each CZ to obtain an estimate of each CZ's fixed effect at percentile p (see Online Appendix A for the specification of this matrix). We weight each observation by the precision of the pairwise

[16]We restrict attention to the 11,216 $o-d$ pairs that have at least 25 observations, which account for 75% of moves across CZs in our sample. We have made the pairwise $o-d$ estimates publicly available in Online Data Table 5 to facilitate future research using alternative models of neighborhood effects (e.g., models that permit heterogeneous match effects).

estimate in this regression. Finally, we normalize these estimates to have a population-weighted mean of zero across CZs (using populations from the 2000 Census), so that the fixed effects can be interpreted as the causal effect of the CZ relative to the average CZ in the country. In our baseline specifications, we estimate the standard errors of $\hat{\mu}_{pc}$ using bootstrap resampling of the microdata (see Online Appendix B for details). For alternative specifications, we report analytical standard errors obtained from the regression in the second step of the estimation procedure – which are very similar to the bootstrapped estimates in the baseline case – to simplify computation.

Identifying CZ effects using this two-step approach requires that moves into and out of each CZ are balanced across counties. Intuitively, if all movers into a CZ moved to one particular county, one would effectively identify the sum of the CZ and that county's effect rather than the mean effect across all counties within the CZ. In practice, moves are generally balanced relative to county populations: the correlation between gross flows into and out of counties within each CZ and county populations recorded in the 2000 Census is 0.90.

Having identified the CZ-level effects, we estimate county effects within each CZ purely from moves across counties *within* CZs. Because there are only four counties on average within each CZ, we can directly estimate (6) separately for each CZ using movers across counties within that CZ.[17] We normalize these estimates to have a population-weighted mean of zero across counties within each CZ, so that the estimates can be interpreted as the causal effect of each county relative to the CZ mean. We obtain standard errors for these county-within-CZ fixed effects directly from the OLS regression in (6).

Finally, we construct county-level estimates by adding the CZ-level fixed effect estimate to the county-within-CZ fixed effect estimate. We use similar methods to estimate fixed effects for subgroups (e.g., for boys vs. girls) and other outcomes (e.g., rates of marriage).

IV FIXED EFFECT ESTIMATES

This section presents fixed effect estimates of CZ- and county-level causal effects. We first present baseline estimates using (6) and then discuss how we evaluate the key identification assumption (Assumption 2) underlying our design.

[17] Because the counties in each CZ are all in the same labor market, we do not permit the $\{\psi\}$ coefficients in the $g(p, s)$ cohort control function to vary with origin and destination when estimating the county-within-CZ models. This simplification reduces the number of parameters to be estimated significantly without affecting the results.

IV.A Baseline Estimates

As in our first paper, we measure children's incomes based on their percentile ranks in the income distribution. We define child i's percentile rank y_i based on his or her position in the *national* distribution of incomes relative to all others in his or her birth cohort. We focus on children growing up with parents at either the 25th or 75th percentiles of the parent income distribution ($p = 25$ or $p = 75$). Given the linearity of the relationship between children's expected ranks and parent ranks, these estimates correspond to the mean rank outcomes of children in below-median ($p < 50$) and above-median ($p \geq 50$) income families, and fully summarize the conditional distribution of children's outcomes given parents' incomes in each area.

We report place effects on both children's own (individual) incomes and their household incomes (including spousal income). However, we focus primarily on the household income results because they provide a better measure of how areas affect children's economic opportunities, independent of variation in labor force participation rates. Prior work on intergenerational mobility has focused on the individual earnings of males as a way to sidestep the challenges in measurement that arise from differences in female labor force participation rates. In our data, we find that males' individual income ranks at age 30 are very highly correlated with both male and female household income ranks for permanent residents across CZs (with population-weighted correlations above 0.9 for children with parents at $p = 25$), but are not highly correlated with female individual earnings ranks (correlation = 0.41). These correlations suggest that females' *household* incomes provide a better representation of the earnings levels that would prevail if labor force participation rates were held constant than females' individual incomes.[18]

Figure Ia plots the CZ fixed effect estimates on children's household incomes at age 26 given parents at $p = 25$, $\hat{\mu}_{25,c}$, vs. the outcomes of children of permanent residents in each CZ, $\bar{y}_{25,c}$. CZs with more than 2.5 million residents are labeled, with the dashed vertical bars showing 95% confidence intervals for these estimates. We first discuss the variation in the fixed effects plotted on the y-axis and then turn to the relationship between this variation and the permanent residents'

[18] One may be concerned that measuring household income at age 26, as we do in our baseline analysis to maximize precision, could yield biased estimates of areas' impacts on individuals' permanent household income because of differences in ages at first marriage across areas. We approach this issue empirically by evaluating what the best predictor of place effects on household income at age 32 is given information available by age 26. We regress permanent residents' mean household income ranks at age 32 given parents at $p = 25$ on three predictors: permanent residents' mean household income rank at age 26, mean individual income rank at age 26, and college attendance rates from ages 18-23. All three measures have predictive power, but the coefficient on household rank is significantly larger than the other variables. Moreover, the predicted values from this regression have a correlation of 0.97 with mean household income ranks at age 26 across CZs. Hence, mean household income ranks at age 26 provide an accurate representation of place effects on household incomes at older ages.

outcomes shown on the x-axis. As an example, we estimate that every year of exposure to Los Angeles decreases the expected income rank of a child growing up in a low-income family ($p = 25$) by 0.17 percentiles (s.e. = 0.04) relative to the average CZ in the U.S. In contrast, every year of exposure to Cleveland, OH increases a child's income rank by 0.12 percentiles (s.e. = 0.10) relative to the average CZ.

To interpret the magnitude of these effects, it is helpful to translate these percentile changes into dollar values.[19] To do so, we regress the mean income levels of children of permanent residents in each CZ, $\bar{y}^{\$}_{pc}$, on their mean income ranks, $\bar{y}_{pc}$, separately at each parent income percentile p, weighting by population. This regression yields a coefficient of \$818 for $p = 25$, implying that a 1 percentile increase in income translates to an additional \$818 at age 26 on average. The mean income of children with below-median income parents is \$26,091; therefore, a 1 percentile increase corresponds to approximately a $818/26,091 = 3.14\%$ increase in income. The point estimates in Figure Ia therefore imply that one year of exposure to Cleveland instead of the average CZ would raise a child's income by $0.12 \times 3.14\% = 0.38\%$, while an additional year of exposure to LA instead of the average CZ would reduce a child's income by $0.17 \times 3.14\% = 0.53\%$.

If we assume that these exposure effects are constant throughout childhood in each area, these estimates would imply that children who move to Cleveland at birth and stay there for 20 years would earn $20 \times 0.38\% = 7.5\%$ more than if they had grown up in the average CZ. Conversely, spending twenty years of childhood in LA instead of the average CZ would reduce a child's income by about 10.7% relative to the average CZ.[20]

Figure Ib presents analogous estimates for each county in the US, highlighting estimates for counties in the New York and Newark CZs that have populations above 500,000. For example, $\hat{\mu}_{25,c} = -0.23$ percentiles (s.e. = 0.10) in the Bronx, implying that growing up in the Bronx causes an income loss of approximately 0.72% per year of childhood exposure relative to the average county in the U.S. In contrast, $\hat{\mu}_{25,c} = 0.25$ percentiles (s.e. = 0.19) in Hudson County, NJ, equivalent to an income gain of 0.79% per year of exposure.

[19] A more direct method of estimating place effects on the level of income (in dollars) would be to estimate $\hat{\mu}_{pc}$ using income levels instead of ranks as the outcome. The estimates of $\sigma_{\mu_{pc}}$ obtained using income levels as the outcome are highly correlated with our baseline estimates using ranks (Online Appendix Table I, row 4), but have many more outliers because of outliers in income levels at the individual level. This is why we use rank outcomes, which yield more precise and stable results across specifications, for our primary analysis.

[20] Chetty and Hendren (2017, Figure IV) show that the effect of each additional year of exposure to a better area is roughly constant over the range of ages they are able to study (ages 9-23). To predict the causal impacts of growing up in an area from birth, we must assume that the exposure effects $\hat{\mu}_{25,c}$ remain constant even prior to age 9, a strong assumption that remains to be tested in future work. The estimates of the impact of growing up in a given area from birth reported here should therefore be interpreted as approximate values.

The dispersion in the estimates on the y-axes on Figures Ia and Ib suggests that there may be substantial variation in the causal effects of places μ_{pc}, although part of the observed dispersion is driven simply by sampling error in our estimates $\hat{\mu}_{pc}$. We quantify the magnitude of the variation in μ_{pc} after accounting for the variation in $\hat{\mu}_{pc}$ that is due to sampling error in Section V.

Comparison to Permanent Residents' Outcomes. It is instructive to compare the fixed effect estimates of μ_{pc} based on movers to the outcomes of children of permanent residents (non-movers) in each area. Under the model in equation (1), permanent residents' outcomes combine the causal effect of growing up in a given area with selection effects reflecting differences in family inputs across areas:

$$\bar{y}_{pc} = A\mu_{pc} + \bar{\theta}_{pc}, \tag{7}$$

where $A\mu_{pc}$ is the cumulative effect of childhood exposure to place c 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 obtained by children of permanent residents in location c.[21]

Figure I shows that the causal effect estimates $\hat{\mu}_{25,c}$ based on the sample of movers are highly correlated with permanent residents' outcomes $\bar{y}_{25,c}$, consistent with the findings in Chetty and Hendren (2017). At the CZ level, regressing $\hat{\mu}_{25,c}$ on $\bar{y}_{25,c}$ yields a slope of $\gamma_{25} = \frac{dE[\mu_{25,c}|\bar{y}_{25,c}]}{d\bar{y}_{25,c}} = 0.032$ (s.e. 0.003), illustrated by the best-fit line in Figure Ia. That is, a year of exposure to a CZ where permanent residents' outcomes are 1 percentile higher increases a given child's outcomes by 0.032 percentiles.[22] We find similar estimates at the county level and for children in high income families ($p = 75$) (Online Appendix Figure I).

Although $\bar{y}_{pc}$ is highly predictive of μ_{pc} on average, there are many differences between the causal effect estimates and permanent residents' outcomes in certain cases. For example, children of permanent residents in Cleveland have worse outcomes than those in Los Angeles. The causal effect estimates based on movers, however, imply that Cleveland produces *better* outcomes for a

[21] If neighborhood effects vary across areas *within* CZs or counties, as is likely to be the case, then differences in the geographical distribution of permanent residents relative to movers within an area c would also be incorporated into the selection term $\bar{\theta}_{pc}$.

[22] This estimate of $\gamma_{25} = 0.032$ differs from the estimate of $\gamma \simeq 0.04$ reported in our first paper because we impose less stringent population restrictions (requiring populations above 25,000 instead of 250,000), do not impose restrictions on the distance of moves, and focus on families at $p = 25$ rather than aggregating across all percentiles. In addition, we estimated γ in our first paper by directly projecting movers' outcomes onto the outcomes of permanent residents. The advantage of that approach relative to the analysis in Figure I is that it directly uses permanent residents' outcomes as "goal posts" for movers' expected outcomes in each place. This allows us to implement placebo tests exploiting heterogeneity across subgroups and increases statistical power, allowing us to estimate exposure effects non-parametrically by age and estimate specifications that control for family fixed effects or are identified from displacement shocks.

given child than Los Angeles. We present a more systematic comparison between causal effects and permanent residents' outcomes in Section VI.B below.

Alternative Specifications. In Online Appendix A, we assess the sensitivity of our estimates of $\hat{\mu}_{25,c}$ to several alternative specifications: (1) modeling heterogeneity in the impact of places across parental income levels using a quadratic function of parental income rank p instead of the linear specification used in (6); (2) controlling for fluctuations across cohorts using alternative parameterizations relative to the specification in (5); (3) measuring children's outcomes in levels instead of percentile ranks; and (4) using income measures that adjust for local costs of living. All of these specifications yield fixed effect estimates that are very highly correlated with our baseline estimates (Online Appendix Table I).

IV.B Validation of Research Design

The fixed effect estimates μ_{pc} obtained from (6) can only be interpreted as causal effects of areas under the identification assumption in (3), which requires that children's exposure to each area e_{ic} is orthogonal to other inputs θ_i, conditional on origin-by-destination fixed effects and parental income levels. In this section, we evaluate whether (3) holds using tests that build upon the methods in Section V of Chetty and Hendren (2017). We briefly summarize the results of these tests here; see Online Appendix C for details.

We organize our evaluation of (3) by partitioning θ_i into two components: a component $\bar{\theta}_i$ that reflects inputs that are *fixed* within families, such as parent genetics and education, and a residual component $\tilde{\theta}_i = \theta_i - \bar{\theta}_i$ that may vary over time within families, such as parents' jobs.

Fixed Factors. Fixed factors $\bar{\theta}_i$ can create selection bias in estimates of μ_{pc} if $\bar{\theta}_i$ is correlated with the age at which child i moves to a given area c. In Chetty and Hendren (2017), we showed that children who move to areas with better permanent resident outcomes $\bar{y}_{pc}$ at younger ages do not have significantly different levels of $\bar{\theta}_i$ using specifications with family fixed effects. Since $\bar{y}_{pc}$ is very highly correlated with the causal effects of place μ_{pc} (Figure I), this finding implies that any selection biases in our estimates $\hat{\mu}_{pc}$ must arise from heterogeneity in $\bar{\theta}_i$ that is *unrelated* to a place's causal effect μ_{pc}. Intuitively, the concern that remains is that the deviations of $\hat{\mu}_{pc}$ from the permanent resident predictions $\gamma_p(\bar{y}_{pc} - \bar{y}_p)$ in Figure I might reflect selection bias rather than causal effects.

We test for such selection biases using two placebo tests. First, we examine the incomes (at age 26) of children who are older than 23 when their parents move. These children provide a natural placebo group because they are less likely to move with their parents and because our first paper shows that neighborhoods no longer have exposure effects after age 23. Second, we implement a placebo test using teenage labor force participation at age 16. Teenage labor force participation (LFP) rates provide an informative pre-treatment indicator because they are positively correlated with differences in children's incomes in adulthood across CZs. Both of these placebo estimates of place effects are uncorrelated with our baseline estimates of μ_{pc} (Online Appendix Table I, rows 7-8), indicating that families who move to a given area at different times do not differ systematically in their children's potential outcomes.

Time-Varying Factors. The second potential source of bias in our estimates of μ_{pc} are time-varying factors $\tilde{\theta}_i$ that are correlated with families' decisions to move, such as parents' incomes. In Chetty and Hendren (2017), we showed that the changes in children's incomes when families move to areas with better permanent resident outcomes $\bar{y}_{pc}$ are not driven by time-varying confounds using a set of placebo tests exploiting heterogeneity across subgroups. As above, this result implies that any remaining bias must arise from time-varying factors that are uncorrelated with places' causal effects μ_{pc}.

To assess the potential bias from such factors, we control for changes in parental income and marital status when estimating (6). The estimates of $\hat{\mu}_{25,c}$ obtained with these controls are nearly identical to the baseline estimates, with correlations above 0.97 (Online Appendix Figure II). Hence, any violation of our key identification assumption would have to arise from time-varying unobservables that are uncorrelated with both permanent residents' outcomes $\bar{y}_{pc}$ (in the origin and destination) and with changes in income and marital status. We believe such violations of the identification condition are unlikely to be prevalent and therefore view our baseline fixed effects $\{\hat{\mu}_{pc}\}$ as providing unbiased estimates of place effects.

In the next four sections, we use the fixed effect estimates to (1) quantify the magnitude of place effects, (2) construct mean-squared-error-minimizing forecasts of the causal effect of growing up in each county, (3) characterize the properties of areas that produce higher levels of upward mobility, and (4) identify areas that produce good outcomes with low housing costs. The fixed effect estimates $\{\hat{\mu}_{pc}\}$ are provided in Online Data Tables 3 and 4, and hence all of the results that follow can be reproduced using publicly available data.

V MAGNITUDE OF PLACE EFFECTS

How much does the neighborhood in which a child grows up influence his or her outcomes in adulthood? In this section, we estimate the standard deviation of place effects ($\sigma_{\mu_{pc}}$) by decomposing the variation in the fixed effect estimates $\hat{\mu}_{pc}$ into the portion due to signal (differences in latent causal effects) vs. noise (sampling error).

V.A Methods

The raw standard deviation of place effect estimates $\sigma_{\hat{\mu}_{pc}}$ overstates the true (signal) standard deviation of place effects $\sigma_{\mu_{pc}}$ because part of the variation in the estimates $\hat{\mu}_{pc}$ is due to sampling error. To estimate $\sigma_{\mu_{pc}}$, we decompose the place effect estimates $\hat{\mu}_{pc}$ into the (latent) place effect μ_{pc} and sampling error η_{pc}:

$$\hat{\mu}_{pc} = \mu_{pc} + \eta_{pc}, \tag{8}$$

where η_{pc} is orthogonal to μ_{pc} ($E\left[\eta_{pc}|\mu_{pc}\right] = 0$). This decomposition implies that we can estimate $\sigma^2_{\mu_{pc}}$ by subtracting the variance induced by sampling error, $\sigma^2_{\eta_{pc}}$, from the variance in the observed estimates, $\sigma^2_{\hat{\mu}_{pc}}$:

$$\hat{\sigma}^2_{\mu_{pc}} = \sigma^2_{\hat{\mu}_{pc}} - \sigma^2_{\eta_{pc}}. \tag{9}$$

We estimate the noise variance $\sigma^2_{\eta_{pc}}$ as the average squared standard error,

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

where s_{pc} denotes the standard error of $\hat{\mu}_{pc}$, estimated using the methods discussed in Section III.C, and the expectation is taken across areas. We compute the standard error of the signal SD estimate $\hat{\sigma}_{\mu_{pc}}$ using an asymptotic approximation described in Online Appendix D. We use precision weights ($1/s^2_{pc}$) when estimating all of these parameters to maximize efficiency.[23]

V.B Results

Table II reports the standard deviation (SD) of the raw fixed effects $\sigma_{\hat{\mu}_{pc}}$, the noise component $\sigma_{\eta_{pc}}$, and the latent causal effects $\sigma_{\mu_{pc}}$. We report estimates at the CZ level, county level, and across counties within CZs for children whose parents are at $p = 25$ and $p = 75$. In Panel A, we report

[23] Precision weighting is efficient if the signal variance $\sigma^2_{\mu_{pc}}$ is constant (homoskedastic), but yields estimates that may vary with the sample in the presence of heteroskedasticity. The estimates reported below are very similar if we instead use population weights (which are sample-invariant), provided that we exclude estimates of $\hat{\mu}_{pc}$ with exceptionally high standard errors (e.g., above the 99th percentile of the distribution of s_{pc}).

estimates of the standard deviation of annual exposure effects on children's income ranks at age 26. Panel B rescales these estimates to present other metrics for the size of place effects.

Low-Income Families. We begin by discussing the magnitude of neighborhood effects for children who grow up in low-income families ($p = 25$). At the CZ level, the (precision-weighted) SD of the raw fixed effects at $p = 25$ is $\sigma_{\hat{\mu}_{25,c}} = 0.25$, as reported in the first row in Table II. A large fraction of this variation is due to noise: $\sigma_{\eta_{pc}} = \sqrt{E\left[s_{pc}^2\right]} = 0.21$. Subtracting the variance of the sampling error using (9) yields a signal SD across CZs of $\sigma_{\mu_{pc}} = 0.13$. That is, living in a one SD better CZ based on children's realized outcomes increases a given low-income child's expected rank by 0.13 per year of exposure.

Across counties at $p = 25$, we estimate $\sigma_{\hat{\mu}_{25,c}} = 0.43$, $\sigma_{\eta_{25,c}} = 0.40$, and $\sigma_{\mu_{pc}} = 0.17$. The county-level estimates exhibit more noise than the CZ-level estimates because sample sizes are smaller at the county level. The SD of causal effects across counties $\sigma_{\mu_{pc}}$ is larger than that across CZs, which is expected because CZs are aggregations of counties. The estimates imply that the SD of counties' causal effects within a given CZ is 0.10 on average, showing that there is nearly as much variation in children's outcomes across counties within CZs as there is across CZs.

To interpret the magnitude of these standard deviations, in Panel B, we rescale the annual exposure effects in three steps analogous to those used in Section IV.A. First, we multiply the annual exposure effect estimates by 20 to obtain a rough estimate of the causal effect of growing up in a given area from birth.[24] Second, we translate the percentile changes into dollar values by multiplying the estimates by \$818, given our estimate above that each additional income rank translates to an additional \$818 of income at age 26 on average for children with parents at $p = 25$. Third, we translate the estimates to percentage impacts on income by dividing by the mean level of income at age 26 for children with below-median income parents (\$26,091).

Using this rescaling, our estimates imply that for a child with parents at $p = 25$, growing up in a one SD better county from birth would increase his or her income at age 26 by 3.3 percentiles. This translates to an increase in income of \$2,700, which is a 10.4% increase in income (about 0.5% per year of childhood exposure). For comparison, a one SD increase in parent income ranks is associated with an 7.1 percentile increase in children's ranks at age 26 in our sample. The *causal* effects of neighborhoods are thus nearly half as large as the association between parent and child income. As another benchmark, Chetty, Friedman, and Rockoff (2014b) estimate that being assigned to a 1 SD

[24] As noted above, Chetty and Hendren (2017) show that exposure effects are approximately constant between ages 9-23, suggesting that the appropriate scaling factor to estimate impacts from birth is between 14 and 23.

better teacher (based on the teacher's test-score value-added) for a single school year raises income by 1.3%. Hence, growing up in 1 SD better county is roughly equivalent to having 8 consecutive years of a 1 SD higher value-added teacher. These comparisons show that neighborhood effects are an important determinant of children's outcomes, with an order of magnitude comparable to other potential interventions, such as changes in educational quality or family resources. However, like these other factors, the area in which a child grows up explains only a small portion of the total variance in children's outcomes: the SD of county effects (3.3 percentiles) is only 11.4% of the unconditional SD of children's ranks (which is 28.9 percentiles).[25]

The SD of the causal effects of places $\mu_{25,c}$ is smaller than the SD of permanent residents' outcomes $\bar{y}_{pc}$, implying that the variation in permanent residents' outcomes across areas is partly due to selection. At the CZ level, $\sigma_{\bar{y}_{25,c}} = 3.3$ percentiles, while the causal effect of growing up in a 1 SD better CZ from birth is 2.7 percentiles. The corresponding values at the county level are 4.2 and 3.3 percentiles. The correlation between $\bar{y}_{25,c}$ and $\mu_{25,c}$ is 0.80 across CZs and 0.58 across counties within CZs.[26] Hence, permanent residents' outcomes are quite informative about places' causal effects, but there are significant differences between $\mu_{25,c}$ and $\bar{y}_{25,c}$, especially at the county level. These results demonstrate that the differences between$\hat{\mu}_{25,c}$ and $\bar{y}_{25,c}$ in Figure I reflect not just sampling error, but differences between places' causal effects $\mu_{25,c}$ and permanent residents' outcomes that are driven by selection.

High-Income Families. Neighborhoods have similar effects in percentile rank and dollar terms for children of high-income ($p = 75$) parents, but matter less in percentage terms because children in high-income families have higher mean incomes. For children with parents at the 75th percentile of the income distribution, the signal SD of place effects at the county level is 3.1 percentiles, which translates to a dollar change of $2,600, or a 6.4% change in income. The place where a child grows up may matter less for children with higher-income parents because high-income families are able to insulate themselves from local conditions more effectively (e.g., by switching to private schools if public schools are weak).

For high-income families, the causal effects $\mu_{75,c}$ are highly correlated with permanent residents'

[25]In this sense, our estimates are consistent with the upper bound on neighborhood effects constructed by Solon, Page, and Duncan (2000) based on the correlation between neighbors' outcomes within an area. Solon et al. (2000, p390) estimate that a 1 SD increase in neighborhood (defined as a PSID sampling cluster) quality is associated with at most a 0.32 SD increase in years of education. Our estimates imply that a 1 SD increase in county quality causes a $3.3/28.9 = 0.11$ SD increase in children's ranks.

[26]We estimate these correlations as $Corr(\mu_{25,c}, \bar{y}_{25,c}) = Corr(\hat{\mu}_{25,c}, \bar{y}_{25,c})\frac{SD(\hat{\mu}_{25,c})}{SD(\mu_{25,c})}$, where the ratio of standard deviations is obtained from Table II and adjusts for the attenuation in $Corr(\hat{\mu}_{25,c}, \bar{y}_{25,c})$ due to sampling error in $\hat{\mu}_{25,c}$.

outcomes $\bar{y}_{75,c}$ across CZs (correlation = 0.91). However, across counties within CZs, the correlation between $\mu_{75,c}$ and $\bar{y}_{75,c}$ falls to 0.04, suggesting that the observational variation in children's outcomes across areas within a given CZ is driven primarily by sorting rather than causal effects for affluent families.

Are the places that generate good outcomes for the poor the same as those that generate good outcomes for the rich? Across CZs, the signal correlation between $\mu_{25,c}$ and $\mu_{75,c}$ is 0.72.[27] Across counties within CZs, the correlation is 0.08. In short, there is no evidence that places which generate better outcomes for the poor generate worse outcomes for the rich; if anything, at broad geographies, places that are better for the poor are better for the rich too.[28]

Heterogeneity by Gender. Estimating fixed effects $\hat{\mu}_{pc}$ separately for male and female children, we find that the place where a child grows up matters more for boys than girls, especially in low-income families (Online Appendix Table II). Growing up in a 1 SD better county from birth in a family at the 25th percentile increases boys' household income ranks by 5.5 percentiles (16.4%), compared with 3.5 percentiles (11.4%) for girls. The correlation between boys' and girls' place effects is 0.85 across counties, indicating that the places that are good for boys and generally good for girls as well. However, the variance of outcomes across areas is larger for boys, in particular because there are some areas with very negative outcomes for boys in poor families. We discuss these differences by gender in further detail in Section VI.

VI FORECASTS OF PLACE EFFECTS

Given that neighborhoods have substantial causal effects on children's outcomes, where should a family who wants to maximize their children's incomes live? In this section, we address this question by constructing forecasts of place effects that minimize the mean-squared error of the true impact of growing up in a given area relative to the predicted impact.

[27]When computing these correlations, we estimate $\mu_{25,c}$ using families with below-median income ($p < 50$) and $\mu_{75,c}$ using families with above-median income ($p > 50$) in order to obtain estimates from independent samples that are not spuriously correlated due to sampling error. We compute the signal correlation as $\rho = \frac{Cov(\mu_{25,c},\mu_{75,c})}{\sigma_{\mu_{25,c}}\sigma_{\mu_{75,c}}} = \frac{cov(\hat{\mu}_{25,c},\hat{\mu}_{75,c})}{\sigma_{\mu_{25,c}}\sigma_{\mu_{75,c}}}$. We use precision weights $(1/s^2_{\mu_{pc}})$ to estimate the signal SDs $\sigma_{\mu_{pc}}$ and weight by the inverse of the sum of the standard errors squared, $\frac{1}{s^2_{\mu_{25,c}}+s^2_{\mu_{75,c}}}$ when estimating $Cov\left(\hat{\mu}_{25,c}, \hat{\mu}_{75,c}\right)$.

[28]This cross-sectional correlation does not imply that policies that improve the outcomes of the poor will not affect the rich. Moreover, these correlations only show that better outcomes for the poor in a given CZ or county c are not associated with worse outcomes for the rich within the same CZ or county; they do not shed light on potential spillover effects on the outcomes of the rich in other areas.

VI.A Methods

The fixed effect estimates based on movers $\hat{\mu}_{pc}$ provide unbiased but imprecise estimates of place effects, as illustrated by the wide confidence intervals for some of the estimates in Figure I. To obtain more precise forecasts of places' causal effects, we combine $\hat{\mu}_{pc}$ with information on permanent residents' outcomes $\bar{y}_{pc}$. Permanent residents' outcomes are estimated with essentially no sampling error, but are biased predictors of μ_{pc} because they combine causal effects with sorting. By shrinking our estimates of $\hat{\mu}_{pc}$ towards predictions based on $\bar{y}_{pc}$, we substantially reduce prediction errors, decreasing the estimator's variance at the expense of introducing some bias.[29]

Formally, we construct forecasts μ^f_{pc} of each area's true causal effect μ_{pc} at a given level of parental income p that minimize the mean squared prediction error $\sum_c(\mu_{pc}-\mu^f_{pc})^2$.[30] For simplicity, we restrict attention to linear predictors:

$$\mu^f_{pc} = \alpha + \rho_{1p}(s_{pc})\hat{\mu}_{pc} + \rho_{2p}(s_{pc})\bar{y}_{pc}, \tag{10}$$

allowing the coefficients $\rho_{1p}(s_{pc})$ and $\rho_{2p}(s_{pc})$ to vary with the degree of sampling error s_{pc} in $\hat{\mu}_{pc}$.[31]

We make two additional simplifying assumptions in constructing forecasts using (10). First, we assume that $\bar{y}_{pc}$ is measured without error. Second, we model the true variance of place effects as homoskedastic, i.e. we assume $Var(\mu_{pc})$ does not vary with c. The first assumption is purely an expositional simplification; incorporating sampling error in $\bar{y}_{pc}$ yields forecasts that are correlated more than 0.99 with the baseline estimates. The second assumption is more substantive. We have found that permitting some forms of heteroskedasticity in μ_{pc} (e.g., by deciles of population size or sampling variance, s_{pc}) does not affect our estimates appreciably. Nevertheless, in future work, it would be useful to study whether more flexible models can yield further reductions in prediction errors using the estimates of $\hat{\mu}_{pc}$, s_{pc}, and $\bar{y}_{pc}$ that are publicly available in Online Data Tables 3 and 4.

[29] Including other predictors – such as racial demographics, poverty rates, or other observable neighborhood characteristics – in addition to permanent residents' outcomes yields very similar forecasts and does not reduce the MSE of the forecasts appreciably (Online Appendix D and Online Appendix Figure III).

[30] We use a quadratic loss function as an analytically tractable specification that penalizes large errors more heavily, based on the reasoning that large errors in predicting place's effects are likely to have much larger utility costs to families seeking to move than small errors. The fixed effect estimates reported in the online data tables could be used to construct forecasts that achieve other objectives, such as maximizing the likelihood of moving to a neighborhood with a high causal effect.

[31] In general, the MSE-minimizing forecast μ^f_{pc} would use the entire set of fixed effect estimates and their variance-covariance matrix $\{\hat{\mu}_{pc}, s_{pc}\}$. Intuitively, one would optimally incorporate information not only for place c but also other places c' whose effects may be correlated with the effect of place c to predict μ_{pc}. We focus on the model in (10) here for simplicity, but note that more general forecasting models could be estimated in future work using the fixed effect estimates reported in the online data tables.

Best Linear Predictors. For a given level of parental income p, the MSE-minimizing coefficients $\rho_{1p}(s_{pc})$ and $\rho_{2p}(s_{pc})$ in (10) are equivalent to those that would be obtained from a (hypothetical) ordinary least squares regression of μ_{pc} on $\hat{\mu}_{pc}$ and $\bar{y}_{pc}$, estimated with one observation per area (c) using the subset of areas whose fixed effect estimates have standard errors of a given level s_{pc}. We derive these coefficients using a partial regression approach in Online Appendix D. The resulting MSE-minimizing forecast is given by

$$\mu^f_{pc} = \frac{\chi^2_p}{\chi^2_p + s^2_{pc}}\hat{\mu}_{pc} + \frac{s^2_{pc}}{\chi^2_p + s^2_{pc}}\gamma_p(\bar{y}_{pc} - \bar{y}_p) \tag{11}$$

where $\bar{y}_p = E\left[\bar{y}_{pc}\right]$ is the mean of $\bar{y}_{pc}$ across areas, $\gamma_p = Cov(\hat{\mu}_{pc}, \bar{y}_{pc})/Var(\bar{y}_{pc})$ is the coefficient obtained from regressing $\hat{\mu}_{pc}$ on $\bar{y}_{pc}$, $\chi^2_p = Var\left(\mu_{pc} - \gamma_p(\bar{y}_{pc} - \bar{y}_p)\right) = \sigma^2_{\mu_{pc}}(1-\rho^2_{\mu_{pc},\bar{y}_{pc}})$ is the residual variance of place effects (across places c) after subtracting out the component explained by $\bar{y}_{pc}$, and s^2_{pc} is the noise variance (squared standard error) of $\hat{\mu}_{pc}$.

Equation (11) shows that the best linear prediction of each county's causal effect is a weighted average of $\hat{\mu}_{pc}$ and $\gamma_p(\bar{y}_{pc} - \bar{y}_p)$, where the weights depend on the degree of signal (measured by χ^2_p) vs. noise (measured by s^2_{pc}) in the fixed effect estimate. The weight on $\bar{y}_{pc}$ falls as the variance in the latent causal effects that cannot be captured by permanent residents' outcomes (χ^2_p) rises.[32] We estimate χ^2_p by subtracting the average sampling variance across places ($E\left[s^2_{pc}\right]$) from the variance of the residuals obtained from regressing $\hat{\mu}_{pc}$ on $\bar{y}_{pc}$:

$$\chi^2_p = Var\left(\hat{\mu}_{pc} - \gamma_p(\bar{y}_{pc} - \bar{y}_p)\right) - E\left[s^2_{pc}\right]. \tag{12}$$

We estimate χ^2_p, γ_p, and $\bar{y}_p$ weighting by the precision of the fixed effect estimates ($1/s^2_{pc}$) to maximize efficiency.

Graphical Representation of Optimal Forecasts. Figure II presents graphical intuition for the construction of these optimal forecasts for a subset of the CZs shown in Figure Ia. The circles plot the point estimates of each CZ's causal effect $\hat{\mu}_{25,c}$ at $p = 25$ vs. the permanent residents' mean ranks $\bar{y}_{25,c}$. The predicted values from a regression of $\hat{\mu}_{25,c}$ on $\bar{y}_{25,c}$ in the full sample of all CZs, $\gamma_{25}(\bar{y}_{25,c} - \bar{y}_{25})$, are shown by the solid line. The optimal forecasts (shown by the diamonds) are a weighted average of $\hat{\mu}_{25,c}$ and $\gamma_{25}(\bar{y}_{25,c} - \bar{y}_{25})$.

The dashed vertical lines on this figure show one-standard-error confidence intervals (s_{pc}) for $\hat{\mu}_{25,c}$. In CZs where the standard error s_{pc} is smaller, the optimal forecast is closer to the estimate

[32] For CZs with populations below 25,000 and counties with populations below 10,000 in the 2000 Census, we do not have an estimate of the causal effect $\hat{\mu}_{pc}$ given our sample restrictions. In these areas, we define the optimal forecast as $\gamma_p(\bar{y}_{pc} - \bar{y}_p)$, the forecast based purely on permanent residents' outcomes.

from movers $\hat{\mu}_{25,c}$ than the permanent resident prediction. For example, in Los Angeles, the optimal forecast $\mu^f_{25,c} = -0.13$ percentiles per year of exposure is obtained by placing 78% of the weight on $\hat{\mu}_{25,c}$ and 22% on $\gamma_{25}(\bar{y}_{25,c} - \bar{y}_{25})$. In smaller CZs, where the fixed effects estimates are less precise, the optimal forecast puts more weight on the predicted outcome based on the permanent residents. For example, in Providence, RI, the optimal forecast puts 70% of the weight on the permanent resident prediction.

Magnitude of Prediction Errors. The optimal forecasts differ from the true causal effect because of sampling error in $\hat{\mu}_{pc}$ and bias in permanent resident predictions, $\gamma_p(\bar{y}_{pc} - \bar{y}_p)$. The mean-squared error of the prediction in (11) for place c with standard error s_{pc} is

$$e^2_{pc} = E[\mu_{pc} - \mu^f_{pc}]^2 = \frac{1}{\frac{1}{\chi^2_p} + \frac{1}{s^2_{pc}}}.$$

If either the sampling error or sorting bias goes to 0, the root-mean-squared error (RMSE) e_{pc} converges to 0 because the optimal forecast puts weight purely on the measure that provides the most accurate prediction. At the other extreme, if the sampling error s_{pc} gets very large, e_{pc} is bounded above by χ_p, the error in the permanent resident prediction. As a result, one obtains forecasts that have much lower RMSE than forecasts based purely on $\hat{\mu}_{pc}$ (which would have RMSE $= s_{pc}$) especially in smaller CZs.[33]

In addition to having much lower MSE than the raw fixed effects $\hat{\mu}_{pc}$, an attractive feature of μ^f_{pc} is that it is forecast unbiased: moving a child to a county with a one percentile higher forecasted effect increases that child's income in adulthood by one percentile on average. In this sense, the forecasts μ^f_{pc} provide unbiased predictions of the expected impacts of moving to a different area on children's outcomes.

VI.B Forecasts for Commuting Zones

Figure III presents maps of the forecasted place effects μ^f_{pc} across CZs for children in below-median ($p = 25$) and above-median ($p = 75$) income families, with lighter colors depicting areas that produce better outcomes.[34] Table III lists the forecasts for the 50 most highly populated CZs

[33] From a Bayesian perspective, under the simplifying assumption that the $\hat{\mu}_{pc}$ estimates are drawn independently across places, $\mu^f_{pc} = E[\mu_{pc}|\hat{\mu}_{pc}, \bar{y}_{pc}, s_{pc}]$ is the posterior expectation of each place's causal effect given a Normal prior and likelihood function. The standard deviation of the posterior distribution is e_{pc} and the true parameter μ_{pc} lies within the credible interval $\mu^f_{pc} \pm 1.96 e_{pc}$ with 95% probability.

[34] The maps are colored by grouping CZs into (unweighted) deciles. The deciles are not symmetric around zero because μ^f_{pc} is normalized to have a *population-weighted* mean of zero and population density is negatively correlated with μ^f_{pc}.

(which accounted for 55.5% of the U.S. population in 2000), sorted in descending order based on $\mu^f_{25,c}$, the forecasted effect for low-income families.

Estimates for Low-Income Families. Among the 50 largest CZs, Salt Lake City, Utah has the most positive forecasted causal effect for children in below-median income families. We predict that every additional year spent growing up in Salt Lake City will increase a child's income by 0.17 percentiles (RMSE = 0.07) relative to an average CZ. Rescaling the estimates as described in Section V.B into dollar impacts, this estimate implies that growing up in Salt Lake City from birth (assuming 20 years of exposure) would increase children's incomes at age 26 by 10.4% relative to growing up in the average CZ. Conversely, at the bottom of the list, every additional year spent growing up in New Orleans is predicted to reduce a child's income by 0.21 percentiles (RMSE = 0.07) relative to an average CZ. This estimate implies that growing up in New Orleans from birth would reduce a child's income by 13.4% relative to the average CZ and 23.8% relative to Salt Lake City.

Figure III shows that many of the places that produce the highest incomes at $p = 25$ are in the rural Midwest, which generate income gains (from birth) exceeding 23.8% relative to the average area. Certain parts of the Northeast and West coast also generate very good outcomes, with gains above 10.6%. The Southeast produces some of the worst outcomes for children in low-income families, with income losses exceeding 9.4% relative to the average place. Parts of the industrial Midwest and other areas such as the large Native American reservations in South Dakota and Arizona generate very negative outcomes as well.

Causal Effects vs. Permanent Residents' Outcomes. The geographical patterns of forecasted causal effects $\mu^f_{25,c}$ in Figure III are broadly similar to the geographical patterns of permanent residents' outcomes $\bar{y}_{25,c}$ in observational data (Chetty and Hendren 2017, Figure II), but there are several notable differences. For instance, Los Angeles is above the national average in terms of its permanent residents' outcomes $\bar{y}_{25,c}$, but it is among the worst cities in terms of its causal effect on low-income children $\mu^f_{25,c}$ (Table III).

To quantify these differences more systematically, in the fourth column of Table III we show the forecast $\gamma_{25}(\bar{y}_{25,c} - \bar{y}_{25})$ that one would obtain if one were to use data only on permanent residents (rescaled into percentage impacts). In LA, $\mu^f_{25,c} = -8.1$, whereas the prediction based on permanent residents is +0.8%. Similarly, in New York, the causal impact is much more negative than one would predict based on permanent residents' outcomes. In Washington DC, the pattern is reversed: permanent residents' outcomes are close to the national mean, but the forecasted causal effect is +6.6%, among the highest for large CZs. These differences between the causal forecasts μ^f_{pc} and the permanent residents' outcomes $\bar{y}_{pc}$ can be interpreted as selection effects among permanent residents under our modeling assumptions, as shown in equation (7). We present a comprehensive analysis of the factors that drive the differences between μ^f_{pc} and $\bar{y}_{pc}$ in Section VII.

Overall, the (population-weighted) correlation between $\mu^f_{25,c}$ and $\bar{y}_{25,c}$ is 0.69 among the 50 largest CZs shown in Table III and 0.89 across all CZs. The correlation between $\mu^f_{25,c}$ and $\bar{y}_{25,c}$ is higher in smaller CZs because the MSE-minimizing forecast in (11) puts more weight on $\bar{y}_{25,c}$ when the causal effect is estimated with less precision. In short, Table III shows that the permanent residents' outcomes used in our first paper provide a very good starting point to predict places' causal effects, but combining that data with information on movers' outcomes as we do here yields much better predictions, especially in highly populated areas.

Estimates for High-Income Families. The right half of Table III and lower panel of Figure III show forecasts of place effects for children in above-median income ($p = 75$) families, $\mu^f_{75,c}$. As discussed in Section V, the variation in causal effects as a percentage of income is much smaller for families at $p = 75$ than $p = 25$. The predicted impact of moving from the worst CZ (Los Angeles) to the best CZ (Salt Lake City) is 13.7% for children in above-median income families, roughly half the corresponding range for children in below-median income families.

The geographical variation in causal effects at $p = 75$ is weakly positively correlated with the variation at $p = 25$. Rural areas produce better outcomes at $p = 75$, particularly in the Midwest. The Southeast tends to produce worse outcomes, although parts of the South such as Louisiana and Arkansas generate considerably better outcomes for the rich than for the poor, thereby amplifying inequality across generations. Perhaps most strikingly, much of the West coast and parts of the Northeast have the lowest values of $\mu^f_{75,c}$. This result turns out to be driven by measuring children's incomes at the household rather than individual level, as we discuss next.

Individual Income and Marriage Rates. In our baseline analysis, we measure children's outcomes at the household level, summing the incomes of spouses for married couples and using own income for single individuals. Online Appendix Figure IV and Appendix Tables III-IV replicate Figure III and Tables III-IV, measuring children's income at the individual level instead. The geographical patterns are broadly similar, with a (population-weighted) correlation between the household-income and individual-income estimates of 0.75 at $p = 25$ and 0.59 at $p = 75$. However, in certain areas – most notably in Coastal California and the Northeast at $p = 75$ – the patterns differ sharply. These areas generate relatively high levels of individual income even though they have among the lowest levels of household income. For example, growing up in the San Francisco CZ from birth in a $p = 75$ family is predicted to increase individual income at age 26 by 0.7% but reduce household income by 4.9% relative to the average CZ. Conversely, Salt Lake City has much more positive impacts on household income than individual income.

Places' causal effects on individual and household income differ largely because they have different causal effects on children's rates of marriage. As shown in Chetty and Hendren (2017, Figure VIIIb), places have linear childhood exposure effects on rates of marriage as well. We can therefore forecast each area's causal effect on marriage rates using the same approach as above, defining the outcome as an indicator for being married at age 26 instead of a child's income rank at age 26. The estimates are presented in Online Appendix Tables V and VI. Growing up in Salt Lake City from birth increases a given child's probability of being married by 10.8 percentage points (pp) at $p = 25$ and 15.8 pp at $p = 75$; the corresponding forecasts in San Francisco are -2.3 pp and -8.0 pp. More generally, the areas that produce the highest rates of marriage tend to produce higher levels of household income than individual income.

VI.C Forecasts for Counties

Table IV presents forecasts of causal effects (on household income) for the 100 most populous counties, focusing on those in the top and bottom 25 based on $\mu^f_{25,c}$. Analogous estimates for all counties in the U.S. are available in Online Data Table 2.[35]

Estimates for Low-Income Families. DuPage County, IL (the western suburbs of Chicago) produces the best outcomes for children from below-median income families among the 100 largest counties. Growing up from birth in DuPage County would increase a child's income by 16.0% relative to the average county. The counties that produce the best outcomes are dispersed across the country: they include Fairfax County in Virginia, Snohomish County in Washington, and Bergen County in New Jersey. At the bottom of the list, Mecklenburg County (the city of Charlotte in North Carolina) generates the most negative outcomes, reducing children's incomes by 14.5%. Baltimore City in Maryland, Hillsborough County in Florida, and Fresno County in California also produce large income losses for children in low-income families.

Counties' causal effects vary substantially even within metro areas. Figure IV illustrates this

[35]We construct these forecasts by applying (11) directly to the county fixed effect estimates $\hat{\mu}_{pc}$, defined as the sum of the CZ and county-within-CZ fixed effect estimates as discussed in Section III.C. An alternative approach – which permits different shrinkage factors at the county-within-CZ and CZ levels – is to forecast the effect of each county relative to the average county within each CZ and then add these county-within-CZ forecasts to the CZ-level forecasts. In practice, these two methods deliver very similar results.

variation by mapping $\mu^f_{25,c}$ for counties in the New York City and Boston Combined Statistical Areas (CSAs). The estimates imply that growing up in a low-income family in Manhattan from birth reduces children's incomes by 10.8% relative to the mean, whereas growing up across the Hudson river in Hudson County, NJ increases children's incomes by 4.2% relative to the mean. Hence, moving from Manhattan to Hudson County at birth would increase children's incomes by 15.0%. Likewise, moving at birth from the city of Boston (Suffolk County) across the Charles river to Middlesex County would increase children's incomes by 13.8%. These maps illustrate a pattern observed in many cities, which is that city centers tend to produce worse outcomes, particularly for children in low-income families, than suburbs.

One of the most striking examples of local area variation is the difference between DuPage County, the best county in the U.S., and Cook County (the city of Chicago), which is 8th worst county in the U.S. among the 100 largest counties. Moving from Chicago proper to the western suburbs of Chicago at birth would increase a child's household income by \$7,510 per year on average, a 28.8% increase. This comparison is of particular interest in light of a well-known 1976 U.S. Supreme Court ruling, *Hills v. Gautreaux*, which required that the Chicago Housing Authority (CHA) provide residents living in high-poverty housing projects in Cook County an opportunity to move to lower-poverty neighborhoods in the suburbs, many of which were in DuPage County. Observational studies comparing the outcomes of families who accepted offers to move to the suburbs to those who chose to remain in the city (e.g., Rosenbaum 1995) have found that children whose families moved to the suburbs had significantly better economic outcomes. The interpretation of these findings have been debated because the families who chose to move may have had better unobservables (θ_i in (1)). Our findings support the view that the gains observed for families who moved as part of Gautreaux reflect the causal impact of growing up in DuPage County instead of Cook County.

Estimates for High-Income Families. For children in above-median income ($p = 75$) families, the best county in the U.S. is Fairfax, VA, which produces an income gain of 11.0% and the worst county is Palm Beach, FL, which produces an income loss of 13.0%. Mirroring the CZ-level results, the degree of variation in outcomes across counties is generally smaller at $p = 75$ than $p = 25$, and there is a weak positive correlation between $\mu^f_{25,c}$ and $\mu^f_{75,c}$. For example, in Figure IV, the city of Boston produces worse outcomes even for children in rich families than Middlesex County; similarly, in New York, Manhattan produces worse outcomes than Hudson County.

Heterogeneity by Gender. The places that generate the best outcomes for boys generally generate the best outcomes for girls as well (Online Appendix Figures V and VI), but there is a thick lower tail of counties that produce particularly negative outcomes for boys in low-income families (Online Appendix Figure VII). For instance, growing up in Baltimore City from birth reduces household income by 27.9% for boys relative to the mean, but only 5.4% for girls (Online Appendix Tables VII-X). One explanation for why certain areas produce extremely low incomes for boys is that males in these areas are particularly likely to be incarcerated, and individuals who are incarcerated are included in our sample with zero or very low incomes.[36]

VII CHARACTERISTICS OF HIGH-OPPORTUNITY AREAS

What are the characteristics of places that produce high levels of upward mobility? Prior work has shown that in observational data, upward mobility is highly correlated with several factors: residential segregation, income inequality, the fraction of single mothers, social capital, school quality, and racial shares (Sampson et al. 2002; Chetty et al. 2014). These correlations could reflect two very different phenomena. One possibility is that they are predictive of places' causal effects: for instance, growing up in a more segregated area may cause worse economic outcomes for a given child (lower μ_{pc}) (Wilson 1987; Sampson 2012). Another possibility is that they capture sorting: the types of people who live in more segregated places may have different characteristics θ_i. Distinguishing between these two explanations is critical for understanding what types of areas *produce* the greatest economic opportunity, as opposed to simply attracting upwardly-mobile people.

In this section, we decompose the correlations documented in prior work into causal vs. sorting components by correlating local area characteristics with both our causal effect estimates $\hat{\mu}_{pc}$ based on movers and permanent residents' outcomes $\bar{y}_{pc}$, which combine both causal effects and selection effects. This analysis identifies the factors associated with the production of upward mobility; however, we caution that it does not show that these factors themselves have a direct causal effect on upward mobility.[37]

We organize our analysis into three parts. First, we analyze correlations between the incomes of children in low-income families ($\mu_{25,c}$) and area characteristics that are defined at the *group* level:

[36] Chetty et al. (2016) further explore heterogeneity in intergenerational mobility by gender and show that areas with concentrated poverty and crime have particularly negative impacts on the incomes and employment rates of boys growing up in low-income families.

[37] For instance, we show below that more segregated places produce worse outcomes for low-income children. This does not necessarily imply that policies which reduce segregation will increase upward mobility, because more segregated places may have other characteristics that lead to worse outcomes (such as weaker schools).

segregation, inequality, school quality, social capital. We then examine correlations between $\mu_{25,c}$ and demographic characteristics that are aggregates of variables defined at *individual* level: the fraction of single mothers, immigrant shares, and racial shares. We find that correlations between the group-level characteristics and upward mobility are driven almost entirely by causal effects of place, whereas a substantial portion of the correlations between the individual-level demographic aggregates and upward mobility is due to selection. Finally, we examine correlations between these factors and outcomes for children growing up in high-income families ($\mu_{75,c}$). In the interest of space, we focus on a subset of correlations that illustrate the key results, shown in Figures VI ($p = 25$) and VII ($p = 75$). A comprehensive set of correlations with 40 area-level characteristics is presented in Appendix Tables XI-XIV.

VII.A Group-Level Characteristics

Segregation. We measure racial segregation across Census tracts within each CZ using a Theil index H_c, which quantifies the extent to which the racial distribution in each Census tract differs from the overall racial distribution in the CZ (Chetty et al. 2014, equation 4).[38] We begin by examining the association between permanent residents' outcomes $\bar{y}_{25,c}$ and H_c, as in prior observational studies. The vertical tick mark in the first row of Figure Va shows that growing up in a CZ with 1 standard deviation (SD) higher segregation is associated with a 5.2% reduction in children's incomes for families at $p = 25$ ($\bar{y}_{25,c}$).[39] We estimate this 5.2% effect in three steps. First, we normalize H_c into standard deviation units by dividing the raw value of the index by its population-weighted standard deviation across CZs. Next, we regress $\bar{y}_{25,c}$ on the standardized value of H_c, weighting by CZ population. Finally, we multiply the regression coefficient by 3.14 to translate the percentile impact into percentage impacts on incomes, as in Section V.B.

To determine how much of this 5.2% effect is due to causal effects of place vs. sorting, we repeat the preceding exercise using the causal effect of growing up in an area from birth ($20 \times \hat{\mu}_{25,c}$) as the outcome.[40] We estimate that growing up in a 1 SD more segregated CZ from birth reduces a given child's income by 4.2%, depicted by the solid bar in Figure Va. Hence, $4.2/5.2 = 80.8\%$ of the association between segregation and permanent residents' outcomes reflects the causal effect

[38] The characteristics analyzed in this section are taken primarily from Chetty et al. (2014, Online Data Table 8); we provide detailed definitions and sources for all of the variables we use in Online Appendix Table XV of this paper.

[39] Standard errors for all of the estimates shown in Figures V and VI are given in Appendix Tables XI and XII. All of the estimates discussed below are statistically significant with $p < 0.05$ unless otherwise noted.

[40] We multiply the annual exposure effect estimates by 20 to obtain impacts from birth for the reasons described in Section IV.A; using other plausible scaling factors (e.g., 15 or 23) yields qualitatively similar results.

of place. Under our modeling assumption that place effects μ_{pc} do not vary between movers and permanent residents, the remaining 19.2% of the association – depicted by the dashed line in Figure Va – reflects sorting, i.e. the association between $\bar{\theta}_{25,c} = \bar{y}_{25,c} - 20\hat{\mu}_{25,c}$ and H_c.[41]

This analysis shows that the majority of the association between segregation and upward mobility across CZs documented in prior work can be explained by the causal effect of place rather than sorting. The (population-weighted) correlation between the latent causal effect $\mu_{25,c}$ and H_c is -0.51 (shown on the right side of Figure Va), implying that segregation explains a significant portion of the variation in children's outcomes across CZs.[42] We find similar results using measures of income segregation. The correlation between a Theil index of income segregation and $\mu_{25,c}$ is -0.57 (Appendix Table XI).

We also find a strong negative correlation with measures of sprawl, which is strongly associated with segregation. The correlation between the fraction of people with a commute time less than 15 minutes and $\mu_{25,c}$ is 0.88. This is the single largest correlation we find across all 40 covariates we analyzed. Growing up in a CZ that is 1 SD lower in the distribution of sprawl (as measured by commute times) would increase a given child's income by more than 7% on average.

In Figure Vb, we replicate the analysis in Figure Va across counties within CZs. We obtain these county-within-CZ estimates by estimating regressions analogous to those described above at the county level, including CZ fixed effects and normalizing each covariate to have a SD of 1 across counties within CZs. Segregation remains a strong predictor of causal effects at the county level: the correlation between racial segregation and $\mu_{25,c}$ is -0.37 across counties within CZs. However, the sorting component is larger at the county level: more than two-thirds of the association between segregation and permanent residents' outcomes $\bar{y}_{25,c}$ is due to sorting. This finding is consistent with the intuition that families seeking better opportunities for their children are more likely to sort within rather than between labor markets.

In sum, our analysis strongly supports the hypothesis that growing up in a more segregated area – that is, in a neighborhood with concentrated poverty – is detrimental for disadvantaged youth. However, the mechanisms underlying our findings diverge from some of the theories posited in prior

[41] If the underlying causal effects μ_{pc} vary across movers and permanent residents – for example, because movers tend to live in different neighborhoods within CZs relative to permanent residents – then $20\hat{\mu}_{25,c}$ would not capture the causal effect of place for permanent residents. In this case, the "selection" term $\bar{y}_{25,c} - 20\hat{\mu}_{25,c}$ would also capture the difference between place effects for permanent residents and movers.

[42] We estimate this correlation as $Corr(\mu_{25,c}, H_c) = Corr(\hat{\mu}_{25,c}, H_c)\frac{SD(\hat{\mu}_{25,c})}{SD(\mu_{25,c})}$, where the ratio of standard deviations is obtained from Table II and adjusts for the attenuation in $Corr(\hat{\mu}_{25,c}, H_c)$ due to sampling error in the raw fixed effect estimates $\hat{\mu}_{25,c}$.

work. For instance, some studies have proposed that segregation is associated with worse outcomes for the poor because of spatial mismatch in access to jobs (Kain 1968; Kasarda 1989; Wilson 1996), an explanation that may appear particularly plausible given the strong correlation between upward mobility and commute times. However, the fact that our causal effect estimates $\hat{\mu}_{25,c}$ are identified from differences in *childhood* exposure is inconsistent with this theory. Our analysis shows that moving to a more sprawling, segregated city at an earlier age (e.g., 10 instead of 12) reduces a child's income in adulthood, demonstrating that these effects cannot be directly driven by a lack of access to jobs in adulthood. Moreover, we find a strong negative association between population density and $\mu_{25,c}$, showing that urban areas – which have more jobs – tend to be worse for upward mobility. Overall, these findings are more consistent with theories that emphasize peer effects or a lack of resources as an explanation for why growing up in a more segregated area reduces upward mobility.

Income Inequality. CZs and counties with greater income inequality produce significantly worse outcomes for children in low-income families. At the CZ level, the correlation between $\mu_{25,c}$ and the Gini coefficient is -0.76. Growing up from birth in an area with a 1 SD higher Gini coefficient reduces a given child's income by 6.4%. Interestingly, this effect is *larger* than the -4.2% effect observed for permanent residents. Under the assumption that the causal effect μ_{pc} is the same for movers and permanent residents, it follows that the sorting component $\bar{\theta}_{25,c}$ offsets the causal component $\mu_{25,c}$ in observational data. That is, residents of areas with high levels of income inequality tend to have better unobservables $\bar{\theta}_{25,c}$, leading prior observational studies (e.g., Chetty et al. 2014) to understate the association between inequality and upward mobility.

This offsetting pattern of selection and causal effects is particularly stark when we focus on upper-tail inequality, measured by the share of households in each CZ who are in the top 1% of the national income distribution. Growing up in an area with 1 SD greater upper-tail inequality reduces the incomes of children in low-income families by 4.1%, an estimate that is significant with $p < 0.01$ (Appendix Table XI). In contrast, the effect on $\bar{y}_{25,c}$ is only 1.0% and is not statistically significant. These findings are inconsistent with Chetty et al.'s (2014) hypothesis that "the factors that erode the middle class may hamper intergenerational mobility more than the factors that lead to income growth in the upper tail" based on their analysis of observational data. On the contrary, both upper-tail inequality and middle-class inequality are strongly negatively associated with causal effects on upward mobility; it is just that the effect of upper-tail inequality is masked by selection in observational data.

These results shed light on the determinants of the "Great Gatsby" curve, the widely noted negative correlation between inequality and intergenerational mobility (Krueger 2012; Corak 2013). The fact that inequality is negatively correlated with $\mu_{25,c}$ implies that the Great Gatsby curve is not driven by differences in genetics or other characteristics of populations in areas with different levels of inequality. Rather, placing a given child in an area with higher levels of inequality makes that child less likely to rise up in the income distribution, showing that areas with greater income inequality generate less upward mobility.[43]

Education. At both the CZ- and county-level, we find strong correlations between $\mu_{25,c}$ and output-based proxies for K-12 school quality, such as test scores and high school dropout rates controlling for parent income. We find weaker correlations with input-based measures of school quality, such as class size and expenditures per student. As with the other factors analyzed above, most of the association between proxies for school quality and permanent residents' outcomes $\bar{y}_{25,c}$ is due to the causal component $\mu_{25,c}$ rather than the sorting component $\bar{\theta}_{25,c}$.

Turning to higher education, we find that CZs with more colleges per capita tend to produce better outcomes $\mu_{25,c}$, with a correlation of 0.60. As with upper-tail inequality, this association is masked when one compares permanent residents' outcomes $\bar{y}_{25,c}$ across areas because of selection effects.

Social Capital. Growing up in a CZ with more social capital, as measured by the social capital index of Rupasingha and Goetz (2008), improves children's outcomes significantly (correlation = 0.70). This causal component accounts for virtually all of the correlation between social capital and permanent residents' outcomes observed in prior work (e.g., Chetty et al. 2014). We also find a significant negative association between $\mu_{25,c}$ and violent crime rates across CZs (Appendix Table XI). At the county-within-CZ level, we do not find a significant association between $\mu_{25,c}$ and the social capital index, but continue to find a significant relationship with violent crime rates.

Together, the factors discussed above explain 58% of the variance in $\mu_{25,c}$ across CZs and 24% of the variance across counties within CZs. These results imply that the places that produce good outcomes share a common set of traits, increasing the likelihood that their successes may be replicable in other areas.[44]

[43]Moreover, the gap in causal effects between children from low and high income families ($\mu_{75,c} - \mu_{25,c}$) is larger in areas with greater inequality, especially among smaller CZs. That is, areas with greater inequality also produce greater intergenerational persistence of income.

[44]The fact that much of the variance in places' causal effects can be explained by observables is noteworthy because efforts to explain causal effects in other settings based on ex-ante observables, such as teachers' value-added, have been much less successful (e.g., Chetty et al. 2014a).

VII.B Individual-Level Demographic Characteristics

We now turn to a set of characteristics that are aggregates of individual-level demographics.

Family Structure. In observational data, the strongest predictor of differences in rates of upward mobility across CZs is the fraction of single mothers (Chetty et al. 2014). A 1 SD increase in the fraction of single mothers is associated with an 7.6% reduction in the incomes of children of permanent residents at $p = 25$ ($\bar{y}_{25,c}$). However, when we examine areas' causal effects on upward mobility, we find that a 1 SD increase in the fraction of single mothers reduces a given child's income ($\mu_{25,c}$) by only 4.7%. Hence, 38% of the association between single parenthood rates and upward mobility in observational data is explained by selection. Across counties within CZs, selection accounts for nearly 70% of the association between the fraction of single mothers and $\bar{y}_{25,c}$.

Selection may play a larger role in explaining the correlation between the fraction of single mothers and $\bar{y}_{25,c}$ than factors such as school quality and social capital because the fraction of single mothers is simply an aggregation of a household-level demographic characteristic. Insofar as such characteristics have direct effects on children's outcomes, they must mechanically capture selection effects, i.e., differences in the types of families living in different areas. In contrast, school quality and the other area-level factors analyzed in the previous subsection do not have such a mechanical selection component.

Despite the importance of selection, the fraction of single mothers remains a strong predictor of the causal effect $\mu_{25,c}$, with a correlation of -0.57 across CZs and -0.38 across counties within CZs. However, it is no longer the strongest predictor of differences in upward mobility, as measures of segregation, inequality, and social capital are as or more highly correlated with $\mu_{25,c}$ than the fraction of single mothers.

Immigrant Shares. Another important demographic characteristic that is strongly associated with upward mobility is immigrant status. The children of certain immigrant groups, such as Asians, have higher rates of upward mobility than the children of natives, perhaps because immigrant parents tend to have lower observed incomes relative to their latent ability. Consistent with this intuition, we find a strong positive association between the fraction of immigrants in an area (measured using Census data) and the sorting component ($\bar{\theta}_{25,c} = \bar{y}_{25,c} - 20\hat{\mu}_{25,c}$). That is, permanent residents in areas with large immigrant populations do better than one would expect based on our estimates of the causal effects of those places, consistent with the results for Los Angeles and New York shown in Figure I.

Areas with larger immigrant shares also have more negative causal effects $\mu_{25,c}$ on average (correlation = -0.45), perhaps because they have other attributes (such as higher population density or greater inequality) that are negatively associated with $\mu_{25,c}$. Because the causal and selection effects work in opposite directions, immigrant shares are not significantly associated with permanent residents' outcomes $\bar{y}_{25,c}$, matching the observational findings of Chetty et al. (2014). These results echo the findings for single mothers in the sense that selection plays a key role in understanding the relationship between immigrant shares and upward mobility in observational data.

Racial Shares. The last demographic factor we consider is race. Areas with a larger share of black residents have much lower rates of upward mobility in observational data (Chetty et al. 2014). Across CZs, a 1 SD increase in the black share is associated with a 7.5% reduction in $\bar{y}_{25,c}$ and a 4.3% reduction in $\mu_{25,c}$. As with the fraction of single mothers, this implies that about half of the association between black shares and upward mobility across CZs in observational data is driven by factors unrelated to the causal effects of CZs. These could include other factors that cause lower rates of upward mobility for blacks than whites, such as racial discrimination in the labor market, or differences in the types of low-income white families who live in CZs with large black populations.[45]

Despite these points, the black share remains a strong predictor of the causal effect $\mu_{25,c}$, with a correlation of -0.51 across CZs and -0.32 across counties within CZs. An important implication of this result is that African American children grow up in areas that tend to produce worse economic outcomes. Under our maintained assumption that place effects are not heterogeneous by race or other characteristics, our estimates of $\mu_{25,c}$ imply that black children grow up in counties that produce 5.3% lower incomes than non-blacks on average. This suggests that residential segregation by race thus amplifies racial inequality across generations.

VII.C Predictors of Place Effects for High-Income Families

We turn now to the characteristics of areas that produce good outcomes for children in high-income families ($\mu_{75,c}$). Since $\mu_{25,c}$ and $\mu_{75,c}$ are highly positively correlated across CZs (Table II), one might expect the strongest correlates of $\mu_{25,c}$ to be highly correlated with $\mu_{75,c}$ as well. Indeed, we find that CZs with less residential segregation, higher quality education (as measured by test scores as well as class sizes), greater social capital, and less income inequality produce better outcomes

[45] Lacking data on race at the individual level, we cannot distinguish between these two explanations. Rothbaum (2016) uses SIPP-SSA linked data to show that upward mobility varies across racial groups within CZs and that controlling for race at the individual level reduces the degree of variation in intergenerational mobility across CZs.

for high-income children (Figure VIa). However, $\mu_{75,c}$ is not significantly correlated with black shares and single parent shares, perhaps because these demographic factors are more reflective of the characteristics of low-income populations in each area.

While $\mu_{25,c}$ and $\mu_{75,c}$ are positively correlated across CZs, $\mu_{25,c}$ and $\mu_{75,c}$ are essentially uncorrelated across counties within CZs. Correspondingly, the factors that strongly predict $\mu_{25,c}$ at the county level do not predict $\mu_{75,c}$. In general, the correlations between the causal effect $\mu_{75,c}$ and the factors we examine are quite small in magnitude and statistically insignificant at the county level (Figure VIb, Appendix Table XIV). There are, however, significant correlations between permanent residents' outcomes in high-income families $\bar{y}_{75,c}$ and county-level characteristics, which are driven primarily by selection effects. For example, a 1 SD increase in test scores (controlling for income) is associated with an 2.1% increase in permanent residents' incomes. However, the causal effect of growing up in a county with 1 SD higher test scores is only 0.15%, implying that 93% of the correlation observed for permanent residents is driven by selection. This finding suggests that for high-income families, places that have schools that are ostensibly of higher quality (as measured by test score performance) may not in fact produce better outcomes; they only appear to be better because they have a positively selected group of children.

Comparing the correlations at $p = 25$ and $p = 75$ shown in Figures V and VI, it is clear that the types of areas that produce better outcomes for the poor generally produce better – or at least no worse – outcomes for the rich. Most notably, there is no evidence that more residentially integrated areas are harmful for children in high-income families. Segregation is negatively correlated with $\mu_{75,c}$ across CZs and uncorrelated with $\mu_{75,c}$ across counties within CZs.

VIII HOUSING COSTS AND OPPORTUNITY BARGAINS

How much more does a family have to pay to live in an area that produces better outcomes for their children? In this section, we examine how opportunity for children is priced in the housing market.

VIII.A Methods

Letting r_{pc} denote the average rent paid by families at percentile p in area c, we characterize the relationship between r_{pc} and μ_{pc} in two ways. First, we measure how much it costs on average to live in a place that produces higher incomes for children by estimating the conditional expectation of rents given an area's causal effect, $E[r_{pc}|\mu_{pc}]$. Second, we measure the extent to which a family could find a place that produces better outcomes for their children without paying more rent by estimating the variance in rents explained by places' causal effects, $R^2 = Var(E[r_{pc}|\mu_{pc}])/Var(r_{pc})$.[46]

If areas' causal effects μ_{pc} were directly observed, these parameters could be estimated (under a linear approximation) using the following OLS regression:

$$r_{pc} = \alpha + \beta_p \mu_{pc} + \zeta_{pc} \tag{13}$$

Since μ_{pc} is not observed, we estimate the conditional expectation β_p by replacing μ_{pc} with $\mu^r_{pc} = \frac{\sigma^2_{\mu_{pc}}}{\sigma^2_{\mu_{pc}}+s^2_{pc}}\hat{\mu}_{pc}$, the MSE-minimizing forecast of each place's causal effect using data purely on movers.[47] This regression yields a unbiased estimate of β_p under the identification assumption in (3).[48] Intuitively, the shrinkage factor $\frac{\sigma^2_{\mu_{pc}}}{\sigma^2_{\mu_{pc}}+s^2_{pc}}$ adjusts for the fact that the raw causal effects $\hat{\mu}_{pc}$ are noisy estimates of μ_{pc}, leading to attenuation bias in β_p in a regression of r_{pc} on $\hat{\mu}_{pc}$.

Similarly, we estimate the variance in rents explained by μ_{pc} (R^2) using

$$R = Corr(r_{pc}, \mu_{pc}) = \frac{Cov(r_{pc}, \mu_{pc})}{SD(r_{pc})SD(\mu_{pc})} = \frac{Cov(r_{pc}, \hat{\mu}_{pc})}{SD(r_{pc})SD(\hat{\mu}_{pc})}\frac{SD(\hat{\mu}_{pc})}{SD(\mu_{pc})},$$

where $\frac{SD(\hat{\mu}_{pc})}{SD(\mu_{pc})}$ is computed using the total and signal standard deviations reported in Table II. Intuitively, the signal R^2 can be computed from the correlation between rents and $\hat{\mu}_{pc}$, again adjusting for attenuation due to noise in the causal effect estimates.

We scale μ^r_{pc} in terms of the percentage change in income per year of childhood exposure, as in Section V.B. We measure monthly rents using data from the 2000 Census, defining the rent in each CZ or county as the mean of the median rent in each Census tract, weighting by the number of families with children who have below-median income for $p = 25$ and above-median income for $p = 75$ (see Online Appendix Table XV). We weight all regressions and correlations by population in the 2000 Census.

[46]We use rents in our baseline specifications instead of house prices because most low-income families rent and because it is more straightforward to compare income gains for children to flow rental costs than house prices. We find qualitatively similar results using house prices, with a negative correlation between house prices and μ_{pc} across CZs and very small correlations across counties within CZs (Appendix Tables XI and XII).

[47]When estimating (13) at the CZ level, we construct the shrinkage factor $\frac{\sigma^2_{\mu_{pc}}}{\sigma^2_{\mu_{pc}}+s^2_{pc}}$ using the estimates of $\sigma_{\mu_{pc}}$ of 0.13 at $p = 25$ and 0.11 at $p = 75$ (Table II, columns (1) and (2)). When estimating (13) at the county-within-CZ level, we construct the shrinkage factor using the county-within-CZ signal SD of 0.10 at $p = 25$ and and 0.11 at $p = 75$ (Table II, columns (5) and (6)).

[48]Formally, $Cov(r_{pc}, \mu^r_{pc})/Var(\mu^r_{pc}) = \beta$ because $Cov(\mu_{pc}, \mu^r_{pc})/Var(\mu^r_{pc}) = 1$ and $Cov(\zeta_{pc}, \mu^r_{pc}) = 0$. Unlike in Section VI, we do not use data on permanent residents when constructing the forecasts here because rents may be correlated with the selection component of permanent resident outcomes $\bar{\theta}_{pc}$, leading to a biased estimate of β_p. Indeed, we find that the association between rents and $\bar{y}_{25,c}$ is larger than the association between rents and $\hat{\mu}_{25,c}$ (Appendix Table XI), implying that families with positive unobservables tend to select into high-priced areas.

VIII.B Relationship Between Rents and Children's Outcomes

Column (1) of Table V reports estimates of (13) at the CZ level at $p = 25$; results at $p = 75$ are qualitatively similar and are presented in Online Appendix Table XVI. CZs that produce 1% higher incomes for children have \$103 *lower* monthly rents on average. This is consistent with our finding that rural areas, such as the Great Plains, tend to produce better outcomes than urban areas. Of course, rural areas also tend to have fewer job opportunities and lower wage rates, so moving to such CZs may not be a plausible margin of choice for most families. Therefore, in the remainder of this section, we focus on variation across counties *within* a given CZ. Because CZs are constructed to approximate local labor markets (Tolbert and Sizer 1996), a household's location decision within a CZ aligns more closely with the conceptual exercise of determining the price of better outcomes for children while holding parents' job opportunities and wage rates fixed.

In column (2), we estimate the relationship between rents and children's outcomes across counties within a CZ by estimating (13) at the county level, including CZ fixed effects. On average, moving to a county that produces 1% higher income per year of exposure for children costs \$177 more in monthly rent. To interpret the magnitude of this coefficient, note that a 1% increase in income translates to approximately a \$4,900 increase in lifetime income for a child with parents at $p = 25$ in present value at age 10 (the middle of childhood) using a 3% discount rate (see Online Appendix D). Hence, a family with two children stands to gain approximately \$10,000 in the present value of future income by moving to a county that produces 1% better outcomes. This is four times larger than the $12 \times \$177 = \$2,124$ mean increase in annual rent associated with moving to a county that increases children's incomes by 1%.

The fraction of the variance in rents explained by $\mu_{25.c}$ (or, equivalently, the fraction of the variance in $\mu_{25,c}$ explained by rents) is $R^2 < 2\%$ across counties within a CZ. Even among the 100 most highly-populated CZs (population > 590,000), where housing supply is likely to be most constrained, $\beta_{25} = \$202$ and $R^2 = 5\%$ (column (3) of Table V).[49]

The relatively weak relationship between rents and children's outcomes suggests that policies that encourage low-income families to move to more expensive areas may not be sufficient to improve their children's outcomes. For example, current "small area" fair market rent proposals in housing voucher programs vary voucher payments with neighborhoods' rents rather than their impacts

[49] Among these large CZs, the rent gradient is steeper in more sprawling, residentially segregated areas. In CZs with above-median commute times, $\beta_{25} = \$255$, compared with $\beta_{25} = -\$66$ in CZs with below-median commute times. The greater price of access to high-opportunity neighborhoods could potentially explain why more segregated, sprawling cities tend to generate worse outcomes for children in low-income families.

on economic outcomes. Such policies may not maximize vouchers' effects on upward mobility, as many expensive areas do not have high levels of $\mu_{25,c}$. Indeed, our results imply that there are many "opportunity bargains" – counties within a labor market that offer good outcomes for children with relatively low rents.

Figure VII illustrates these findings by plotting our optimal forecasts $\mu^f_{25,c}$ – scaled as the percentage impact of growing up in a given county from birth relative to the national mean – vs. rents for counties in the New York City Combined Statistical Area. The substantial dispersion in this scatter plot around the mildly upward sloping best-fit line illustrates the key empirical results of this section: on average, moving to a better area does not cost much more, and there is considerable residual variation in children's outcomes that is orthogonal to rents. For example, Hudson County, NJ offers much better outcomes for low-income children than Manhattan despite having comparable rents. More generally, there are many counties – in the upper left of the figure – that offer "opportunity bargains" within the New York area.

The existence of these opportunity bargains may be encouraging for families seeking to improve their children's prospects for upward mobility as well as for policy makers looking for affordable areas that produce good outcomes as models to emulate. However, the existence of such areas demands further explanation from the perspective of economic models of spatial equilibrium, as these areas seemingly offer arbitrage opportunities that have been left unexploited. We turn to explaining these empirical results in the next subsection.

VIII.C Explaining Opportunity Bargains

Why are places' effects on children's future incomes not capitalized more fully into housing prices? We discuss three potential explanations for this result in this subsection.

First, the areas that appear to be opportunity bargains may have other disamenities that make them less desirable places to live. Our data suggest that such disamenities explain at least part of the residual variation in children's opportunities conditional on rents. The areas that generate the best outcomes for children in most cities are typically in the suburbs – which tend to have longer commutes and offer fewer urban amenities – as illustrated by the maps for New York and Boston in Figure IV. However, data from the Moving to Opportunity (MTO) experiment suggest that such disamenities do not fully explain the existence of opportunity bargains. MTO families who received housing vouchers to move to low-poverty Census tracts exhibited gains in both children's long-term outcomes *and* parents' subjective well-being and neighborhood satisfaction (Ludwig et al. 2012;

Chetty et al. 2016). These findings suggest that affordable neighborhoods that produce better outcomes for children in low-income families are not necessarily less desirable to parents because of other disamenities.

A second potential explanation for why children's opportunities are not fully priced in the housing market is a lack of information. Families may not know which areas produce the best outcomes for children, particularly because there is a long delay in observing these outcomes. To evaluate this hypothesis, we divide place effects $\mu_{25,c}$ into an "observable" component that is related to attributes families can observe, such as school quality and poverty rates, and an "unobservable" component unrelated to these factors. We estimate these components by regressing $\hat{\mu}_{25,c}$ (at the county level) on the fraction of African American residents, the Theil index of racial segregation, the Gini index, the fraction of single mothers, the social capital index, and expenditures on public schools per student.[50] We then define the observable component as the predicted value from this regression and the unobservable component as the residual, which we shrink by its signal-to-noise ratio as above to adjust for attenuation bias. 30% of the signal variance in $\mu_{25,c}$ is captured by the observable component; the remaining 70% is "unobservable" given these predictors.

In columns (4) and (5) of Table V, we replicate the specification in Column 3, regressing rents on the observed and unobserved components of place effects. We obtain an estimate of $\beta_{25} = \$430$ for the observed component. Hence, moving to a CZ that generates a 1% ($4,900) increase in lifetime income for a child along the "observed" dimension – for instance, an area with better-funded schools and more two-parent families – costs $5,160 in terms of annual rent on average. In contrast, there is no significant relationship between prices and the "unobservable" component (column (5)). These findings suggest that the causal effects $\mu_{25,c}$ may be under-priced partly because of a lack of information.[51]

A third explanation for the existence of opportunity bargains is failures in optimization due to cognitive constraints or behavioral biases. Recent ethnographic studies show that low-income families frequently move under time pressure, either because they have been evicted (Desmond

[50] We use these factors because they are highly predictive of children's outcomes, as shown in Figure V, and are in principle easily observed by families. However, including additional variables from the set in Figure V does not affect the results. We estimate the regression at the county level with CZ fixed effects, restricting the sample to counties in the 100 most highly populated CZs and weighting by the county population.

[51] One may be concerned that the unobservable component is highly transitory, in which case "opportunity bargains" could only be identified ex-post after children become adults rather than when families are deciding where to live. Empirically, this does not turn out to be the case. The unobservable component has a precision-weighted signal correlation of 0.42 with permanent residents' mean ranks at $p = 25$. Permanent residents' mean ranks in turn have a serial correlation exceeding 0.93 across cohorts, implying that one could reliably predict the unobservable component even while children are growing up based on observed outcomes for earlier cohorts.

2016) or because they are making a "reactive" move responding to a financial or health shock (DeLuca et al. 2013). In such circumstances, families often seek shelter as quickly as possible rather than weighing the benefits that may accrue to their children several years later if they choose a different neighborhood (DeLuca et al. 2013). Moreover, the decision of where to live has several features that may trigger well-established behavioral biases and induce suboptimal choice: delayed payoffs coupled with large initial up-front costs that compound present bias, a need to predict one's preferences in a very different environment that may induce projection bias, and complex planning with scarce mental bandwidth (Chetty 2015, Section IV.B).

We believe that each of these explanations – disamenities, a lack of information, and behavioral biases – is likely to play a role in explaining the existence of opportunity bargains. Understanding the relative importance of these theories is an important area for future research.

IX CONCLUSION

This paper has estimated the causal effect of each county in the U.S. on children's outcomes in adulthood. Overall, the findings provide support for place-focused approaches to improving economic opportunity, both by helping families move to opportunity and through place-based investments. The estimates show that there is substantial scope for households to move to areas within their labor market (CZ) that are opportunity bargains – places that produce better outcomes for children without paying higher rents. In addition, the areas that produce high levels of upward mobility share a common set of characteristics – such as less residential segregation and greater social capital – suggesting that their successes might be replicable in other areas.

There are two key areas for further research before one can apply these findings to make policy changes that improve children's outcomes. First, it would be useful to estimate places' causal effects at narrower geographies (e.g., Census tracts) and for specific subgroups (e.g., by race and ethnicity) using the methods developed here. Such estimates would provide more granular data for families seeking to move to opportunity within their cities and for policymakers seeking to make targeted investments in neighborhoods that currently produce lower levels of upward mobility.

Second, it would be useful to understand the mechanisms through which some places produce better outcomes than others by isolating exogenous variation in the predictors of upward mobility identified here. For example, studying changes in local policies that have reduced residential segregation could shed light on whether segregation directly harms children in low-income families. To facilitate further investigation of these mechanisms, we have made all of the county- and CZ-level estimates of causal effects constructed in this study available on the Equality of Opportunity Project website.[52]

[52]In addition to the estimates discussed in this paper, we also provide estimates for other outcomes and subgroups, such as college attendance rates and estimates for children in one vs. two-parent households. For all outcomes, we provide estimates of both the raw fixed effect estimates $\hat{\mu}_{pc}$ and optimal forecasts μ^f_{pc}, as the appropriate measure of place effects will vary across applications. As a rule of thumb, those seeking to use the causal effect as a dependent variable should use $\hat{\mu}_{pc}$, while those seeking to use the causal effect as an independent variable should use μ^f_{pc}.

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Online Appendix

A Estimation Methods for Place Fixed Effects

In this appendix, we describe the methods we use to estimate CZ fixed effects and control for changes in income and marital status in our baseline analysis. We then discuss estimates of fixed effects using alternative specifications.

Two-Step Estimator for CZ Fixed Effects. Equation (6) has more than one thousand parameters of interest – each CZ's fixed effect $\left\{\mu_c^0\right\}_c$ and the fixed effects interacted with parent income rank $\left\{\mu_c^1\right\}_c$ – as well as a large number of incidental parameters. Estimating this model using a single regression directly in the microdata is not feasible due to computational constraints. We therefore use a two-step approach to estimate (6), which we describe in this appendix.[53]

In the first step, we estimate exposure effects separately for each origin-destination pair. In each origin destination pair, we regress children's income ranks y_i on exposure time to the destination relative to the origin, $A - m_i$ using the following specification:

$$y_i = \alpha_{odps} + (A - m_i)\left(\mu_{od}^0 + \mu_{od}^1 p_i\right) + \epsilon_i, \tag{14}$$

where

$$\alpha_{odps} = \left(\alpha_{od}^0 + \alpha_{od}^P p + \psi_{od}^0 s + \psi_{od}^1 s^2 + \psi_{od}^2 sp + \psi_{od}^3 s^2 p\right) \tag{15}$$

In equation (14), $\mu_{od}^p = \mu_{od}^0 + \mu_{od}^1 p$ represents the causal impact of spending an additional year of one's childhood in destination d relative to origin o for children with parental income rank p. This step generates N_c^2 sets of estimates, where N_c denotes the number of CZs. To minimize the impact of outliers in small cells, we limit the sample to origin-destination cells with at least 25 children who move.

In the second step, we use the pairwise exposure effects μ_{od}^p to estimate the causal effect of each CZ c at percentile p, μ_{pc}. Under our modeling assumptions, the exposure effect for a given origin-destination pair μ_{od}^p is the difference between the destination's and origin's fixed effects at percentile p:

$$\mu_{od}^p = \mu_{p,d} - \mu_{p,o}.$$

To estimate μ_{pc} for a given parental income rank p (e.g., $p = 25$), we regress the N_c^2 pairwise estimates obtained from the first step (μ_{od}^p) on a matrix G that consists of positive and negative

[53] We thank Gary Chamberlain for suggesting this approach to estimating equation (6).

indicators for each CZ. To construct this matrix G, we enumerate all N_c^2 origin-destination pairs as rows, and each of the N_c CZs as columns. In each origin-destination row $o-d$, the column corresponding to the destination d is assigned a value of $+1$, the column corresponding to the origin o is assigned value of -1, and all other columns are assigned a value of 0. The resulting $N_c^2 \times N_c$ matrix G has the form:

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

We then estimate each place's effect μ_{pc} using the following OLS regression specification:

$$\vec{\mu}^p_{od} = G\vec{\mu}_p + \eta_{od}, \tag{16}$$

where $\vec{\mu}^p_{od}$ denotes a column vector of the pairwise effects μ^p_{od} and $\vec{\mu}_p = (\mu_{p1}, ..., \mu_{pN_c})$' is a column vector of the CZ effects at percentile p. We estimate (16) weighting by the precision ($\frac{1}{(s^p_{od})^2}$) of the μ^p_{od} estimates, based on the standard errors s^p_{od} obtained from equation (14) in the first step.

Controlling for Changes in Income and Marital Status. To control for other factors that may change when families move, such as parent income and marital status (X_i), we seek to estimate specifications of the form:

$$y_i = \alpha_{od} + \alpha^P_{od} p + \vec{e}_i \cdot \vec{\mu}_p + g_{od}(p_i, s_i) + \beta_0 X_i + \beta_1 X_i m_i + \varepsilon_i. \tag{17}$$

This specification generalizes the baseline specification in (6) by adding the terms $\beta_0 X + \beta_1 X m_i$, which control for X_i and its interaction with the age of the child at the time of the move, m_i. Again, estimating (17) directly is computationally infeasible. We therefore use a partial regression approach, following Chetty, Friedman, and Rockoff (2014a, Section I.B).

We first regress y_i on X_i and α_{odpsm} (origin by destination by parent income decile by child birth cohort by child age at move) fixed effects. The inclusion of the α_{odpsm} fixed effects yields estimates of β_0 and β_1 that are not biased by potential correlations between X_i and $\vec{e}_i$. Using the estimates $\hat{\beta}_0$ and $\hat{\beta}_1$, we construct residuals $\tilde{y}_i = y_i - \hat{\beta}_0 X_i - \hat{\beta}_1 X_i m_i$. We then estimate (6) with the residuals $\tilde{y}_i$ as the dependent variable to identify $\{\mu_{pc}\}$ using the two-step approach described above at the CZ level and a single regression in the microdata at the county-within-CZ level.

Alternative Specifications. We assess the sensitivity of our estimates of $\vec{\mu}_p$ to our baseline specification choices by estimating a range of alternative specifications. Appendix Table I reports the (population-weighted) correlation between the baseline estimates obtained from (6) and estimates using alternative specifications. The estimates of $\vec{\mu}_p$ using these alternative specifications are available in Online Data Tables 3 and 4.

In our baseline specification, we model heterogeneity in the impact of places across parental income levels using a linear function of parental income rank. To assess the importance of this functional form, we include a quadratic term in parent rank p when defining μ_{pc} in (4). The correlation between the fixed effect estimates obtained from this quadratic specification with our baseline estimates exceeds 0.88 at $p = 25$ and 0.78 at $p = 75$ at the CZ and county levels.

Next, we assess the sensitivity of the results to the functional form used to control for fluctuations across cohorts in (5). Using a linear function of s and p yields estimates that are correlated above 0.98 with the baseline quadratic specification. Similarly, including a cubic term in cohort (s) in (5) also does not affect the estimates appreciably.

In our baseline analysis, we use percentile ranks to measure children's incomes. Measuring children's outcomes in levels (2012 dollars) yields estimates that have correlations of between 0.78 and 0.92 with our rank-based estimates. The correlations are below 1 largely because of noise in the level estimates. As discussed in Section IV, the level estimates are much less precise than the rank estimates because they are sensitive to outliers in incomes.

Finally, our baseline estimates do not adjust for differences in price levels across areas. Following Chetty et al. (2014), we construct cost-of-living-adjusted income ranks for parents and children by dividing income in each year t by a local price index (based on the ACCRA survey and local house prices) for the CZ where the individual lived in year t. We then re-rank parents based on their mean real income from 1996-2000 and re-rank children based on their real incomes at age 26. Estimates of place effects based on real incomes are highly correlated ($\rho > 0.74$) with our baseline measures, indicating that the variation in the causal effects of places we document is largely unrelated to differences in cost of living.

B Standard Errors

In this appendix, we describe how we compute standard errors for the fixed effect estimates ($\hat{\mu}_{pc}$) and the signal standard deviations ($\sigma^2_{\hat{\mu}_{pc}}$) reported in the text.

Standard Errors for Fixed Effect Estimates. At the CZ level, we use bootstrap resampling to compute standard errors for our baseline estimates of $\hat{\mu}_{pc}$ because the two-step estimation procedure discussed in Section A does not yield analytical standard errors. We construct 100 samples (with replacement) from the microdata and repeat the two-step estimation procedure above, generating

100 estimates $\hat{\mu}_{pc}^{bs}$ for each CZ c. We define the standard error $s_{pc} = SD(\hat{\mu}_{pc}^{bs})$ as the standard deviation of these bootstrap estimates. Empirically, these bootstrapped standard errors are very similar to the analytical standard errors directly obtained from the second-stage regression in equation (16).[54] We provide both the bootstrapped and analytical standard errors for our baseline specification in Online Data Tables 3 and 4. For computational simplicity, we report the analytical standard errors obtained from estimating equation (16) for all other specifications.

At the county-within-CZ level, we obtain standard errors directly from the regression in equation (6) for each county within each CZ. Recall that we define each county's causal effect (relative to the average county in the country) as the sum of the county-within-CZ effect and the CZ's effect. To obtain standard errors for these county estimates, note that the CZ and county-within-CZ estimates are independent because the CZ effects are estimated from a sample of one-time movers across CZs, while the county-within-CZ effects are estimated from an sample of one-time movers within CZs. Therefore, we calculate the standard error of the estimate of each county's effect as $s_{p,cty}^2 = s_{p,CZ}^2 + s_{p,ctyincz}^2$, where $s_{p,ctyincz}^2$ 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 standard error for the CZ estimate.

Standard Errors for Signal Standard Deviations. We use a closed-form approximation to construct standard errors for our estimate of the signal standard deviation ($\sigma_{\hat{\mu}_{pc}}$) defined in equation (9). Fixing a parental income level p and suppressing the p subscript from equation (8) for simplicity, we write the fixed effect estimate of each place's causal effect as

$$\hat{\mu}_c = \mu_c + \eta_c,$$

where μ_c is drawn from a distribution $N\left(0, \sigma_{\mu_{pc}}^2\right)$. Using a Normal approximation to the distribution of sampling errors, $\eta_c \sim N\left(0, s_c^2\right)$, where s_c^2 is the standard error of the fixed effect estimate defined above.

As discussed in the text, we use the following plug-in estimator for $\sigma_{\mu_{pc}}^2$, the signal variance, at a given percentile p (again suppressing the p index to simplify notation):

$$\hat{\sigma}^2 = Var(\hat{\mu}_c) - E[s_c^2],$$

where we compute the variance and expectation across areas (c) using precision weights ($1/s_c^2$) to maximize efficiency. In this appendix, we describe how we estimate the sampling variance of this

[54] For example, the analytical standard errors from equation (16) have an unweighted correlation of 0.93 with the baseline standard errors (and a population-weighted correlation of 0.96). Using the analytical standard errors for our baseline CZ estimates, we would obtain a signal SD of CZs' causal effects at $p = 25$ on children's income ranks of 0.156 instead of our baseline estimate of 0.132 based on the bootstrapped standard errors reported in Table II.

estimate of the signal standard deviation $\hat{\sigma}$.

Noting that $E[\hat{\mu}_c] = 0$, we can write

$$\begin{aligned}
\hat{\sigma}^2 &= \frac{1}{\sum_c \frac{1}{s_c^2}} \sum_c \frac{1}{s_c^2} (\hat{\mu}_c)^2 - \frac{1}{\sum_c \frac{1}{s_c^2}} \sum_c \frac{1}{s_c^2} (s_c^2) \\
&= \frac{1}{\sum_c \frac{1}{s_c^2}} \left[\sum_c \frac{1}{s_c^2} \left[\mu_c^2 + 2\mu_c \eta_c + \eta_c^2 - s_c^2 \right] \right]
\end{aligned}$$

Since the place effect, μ_c is uncorrelated with the sampling error η_c,

$$\lim_{N_c \to \infty} \sum_c \frac{\mu_c \eta_c}{s_c^2} = 0$$

Therefore, as the number of areas N_c grows large,

$$\hat{\sigma}^2 \simeq \frac{1}{\sum_c \frac{1}{s_c^2}} \left[\sum_c \frac{1}{s_c^2} (\mu_c^2 + \eta_c^2 - s_c^2) \right].$$

Since $E(\eta_c^2) = Var(\eta_c) = s_c^2$ by definition, it follows that $E(\hat{\sigma}^2) = \sum_c \frac{1}{s_c^2} (\mu_c)^2 / \sum_c \frac{1}{s_c^2}$. Hence,

$$\begin{aligned}
\hat{\sigma}^2 - E(\hat{\sigma}^2) &\simeq \frac{1}{\sum_c \frac{1}{s_c^2}} \left[\sum_c \frac{1}{s_c^2} (\eta_c^2 - s_c^2) \right] \\
&= \frac{1}{\sum_c \frac{1}{s_c^2}} \left[\sum_c \frac{1}{s_c^2} \left[\eta_c^2 \right] - N_c \right]
\end{aligned}$$

To obtain a simple analytical estimator for the standard error, we assume that the sampling errors η_c are uncorrelated across places (we discuss the empirical validity of this assumption at the end of this appendix). Under this assumption, $\sum_c \frac{1}{s_c^2} \left[\eta_c^2 \right]$ is a sum of N_c i.i.d. standard Normal variables, which has a Chi-squared distribution with N_c degrees of freedom. It follows that

$$\left(\sum_c \frac{1}{s_c^2} \right) \left[\hat{\sigma}^2 - E[\hat{\sigma}^2] \right] + N_c \sim \chi^2 (N_c)$$

As N_c grows large, $\chi^2 (N_c) \to N (N_c, 2N_c)$ by the central limit theorem, implying:

$$\left(\sum_c \frac{1}{s_c^2} \right) \left[\hat{\sigma}^2 - E[\hat{\sigma}^2] \right] \sim N (0, 2N_c).$$

Therefore we can write the sampling distribution of $\hat{\sigma}^2$ as

$$\hat{\sigma}^2 \sim N(E[\hat{\sigma}^2], \frac{2N_c}{(\sum_c \frac{1}{s_c^2})^2}),$$

i.e. the standard error of the signal variance $\hat{\sigma}^2$ is $\sqrt{2N_c} / \sum_c \frac{1}{s_c^2}$.

Finally, we derive the standard error for the estimated signal standard deviation $\hat{\sigma}$ instead of $\hat{\sigma}^2$. Applying the delta method with $f(x) = \sqrt{x}$, we have $f'\left(\hat{\sigma}^2\right) = \frac{1}{2\hat{\sigma}}$, implying

$$\hat{\sigma} \sim N(\sqrt{E[\hat{\sigma}^2]}, (\frac{1}{2\hat{\sigma}})^2 \frac{2N_c}{(\sum_c \frac{1}{s_c^2})^2}) = N(\sqrt{E[\hat{\sigma}^2]}, \frac{1}{2N_c(\frac{\hat{\sigma}}{N_c}\sum_c \frac{1}{s_c^2})^2}).$$

Thus, the standard deviation of the sampling distribution of $\hat{\sigma}$, i.e. the standard error of $\hat{\sigma}$, is

$$se\left(\hat{\sigma}\right) \approx \frac{1}{\sqrt{2N_c}\left(\frac{\hat{\sigma}}{N_c}\sum_c \frac{1}{s_c^2}\right)}. \tag{18}$$

Intuitively, the standard error falls with the square root of the number of places, $\frac{1}{\sqrt{N_c}}$ and the mean precision of the estimates relative to the point estimate $\frac{\hat{\sigma}}{N_c}\sum \frac{1}{s_c^2}$.

Equation (18) provides an approximate estimate of the standard error because it relies on (1) the approximation that $\sum_c \frac{\mu_c \eta_c}{s_c^2}$ converges to zero as N_c grows large, (2) the central limit theorems used to approximate the distribution of sampling errors η_c using Normal distributions and the Chi-squared distribution by a Normal distribution, and (3) the first-order approximation used to implement the delta method. Monte Carlo simulations show that approximations are quite accurate when we use parameter values corresponding to our point estimates with $N_c > 100$. For example, with $N_c = 741$ CZs and the signal and noise variances for $p = 25$ reported in Table II, we find that our approximations yield estimates of the standard error of $\hat{\sigma}$ that differ from the true standard error of $\hat{\sigma}$ by less than 5% of the point estimate $\hat{\sigma}$.

More importantly, the estimator for the standard error in (18) relies on the assumption that the estimation errors η_c are uncorrelated across places c. In practice, estimates of $\hat{\mu}_c$ using (6) have correlated sampling errors because a given mover's outcome enters the estimate of both the origin and destination area. To evaluate the bias created by this assumption, we compare the analytical estimates obtained from (18) to bootstrapped standard errors for the baseline specification in subsamples of the data (implementing the bootstrap in the full sample was computationally infeasible). The approximate standard errors are qualitatively similar to the true standard errors, and we therefore proceed to use (18) to compute standard errors for the signal SD estimates.

C Validation of Research Design

The fixed effect estimates μ_{pc} obtained from (6) can only be interpreted as causal effects of areas under the identification assumption in (3), which requires that children's exposure to each area e_{ic} is orthogonal to other inputs θ_i, conditional on origin-by-destination fixed effects and parental income levels. In this Appendix, we present a set of robustness checks and placebo tests to evaluate the validity of this key assumption.

As in Section V of our first paper (Chetty and Hendren 2017), we organize this evaluation by partitioning the unobserved determinant of children's outcomes θ_i into two components: a component $\bar{\theta}_i$ that reflects inputs that are *fixed* within families, such as parent genetics and education, and a residual component $\tilde{\theta}_i = \theta_i - \bar{\theta}_i$ that may vary over time within families, such as parents' jobs. Each of these components can create different forms of selection and omitted variable biases, and we therefore use different methods to assess their relevance. In each case, we first discuss the set of biases that are already ruled out by the tests conducted in Section V of Chetty and Hendren (2017) and then present tests to evaluate potential remaining biases.

Fixed Factors. Fixed factors $\bar{\theta}_i$ can create selection bias in estimates of μ_{pc} if $\bar{\theta}_i$ is correlated with the age at which child i moves to a given area c. The most plausible source of such sorting is selection on areas' treatment effects μ_{pc}. For example, families who move to areas that produce better outcomes (higher μ_{pc}) when their children are younger may also invest more in their children in other respects (higher $\bar{\theta}_i$), which would bias our exposure effect estimates. In our first paper, we showed that children who move to areas with better permanent resident outcomes $\bar{y}_{pc}$ at younger ages do not have significantly different levels of $\bar{\theta}_i$ using specifications with family fixed effects identified from sibling comparisons.[55] Since $\bar{y}_{pc}$ is very highly correlated with the causal effects of place μ_{pc} (as shown in Figure I), this finding indicates that direct selection on treatment effects is unlikely to be prevalent. Intuitively, if children with better potential outcomes (higher $\bar{\theta}_i$) systematically move to better areas (higher μ_{pc}) at younger ages, we would expect such families to sort to areas where permanent residents have better outcomes $\bar{y}_{pc}$. The fact that they do not do so suggests that families are not directly sorting on μ_{pc}.

The results in our first paper therefore imply that any selection biases in our estimates $\hat{\mu}_{pc}$ must arise from heterogeneity in $\bar{\theta}_i$ that is correlated with the timing of moves to a given area c for reasons *unrelated* to a place's causal effect μ_{pc}. Intuitively, the concern that remains is that the deviations of $\hat{\mu}_{pc}$ from the permanent resident predictions $\gamma_p(\bar{y}_{pc} - \bar{y}_p)$ in Figure I might reflect selection bias rather than causal effects. For example, one may be concerned that Los Angeles has a particularly negative estimate of $\hat{\mu}_{25,c}$ relative to its permanent residents' outcomes because

[55] Unfortunately, we have insufficient precision to estimate the fixed effects model in (6) with family fixed effects. Conceptually, there is insufficient variance in children's ages within families to identify the 6,000 parameters in (6). Regressing the point estimates obtained from such a specification on our baseline estimates yields a point estimate close to 1, but the confidence interval includes 0 as well.

families who move into LA at earlier points in their lifecycle happen to have worse unobservables $\bar{\theta}_i$, for reasons that are unrelated to LA's causal effect μ_{pc}.

We test for this form of selection bias using two placebo tests. First, we examine the incomes (at age 26) of children who are older than 23 when their parents move. These children provide a natural "placebo group" because they are less likely to move with their parents and because our first paper shows that neighborhoods no longer have exposure effects after age 23. If families who move at earlier times to a given area have different latent characteristics $\bar{\theta}_i$, they would be manifested in the outcomes of this placebo group and would generate fixed effect estimates similar to the actual causal effects μ_{pc}. If in contrast our identification assumption holds, we should find no correlation between these placebo effects and the true effects μ_{pc}.[56] We replicate our baseline specification on this group of older children – defining their "exposure" to each area as the number of years in which their parents lived in the new area – to estimate a set of placebo fixed effects $\{\hat{\mu}_{pc}^{23+}\}$. The correlation between these placebo fixed effects and μ_{pc} is 0.04 (s.e. $= 0.08$) at $p = 25$ across CZs and is similarly small and insignificant at $p = 75$ and at the county level (Appendix Table I, row 7).[57]

Second, we implement a placebo test using teenage labor force participation at age 16. Teenage labor force participation (LFP) rates provide an informative "pre-treatment" indicator because they are significantly positively correlated with differences in children's incomes in adulthood across CZs.[58] We estimate a set of fixed effects $\{\hat{\mu}_{pc}^{TL}\}$ by replicating our baseline specification using an indicator for working at age 16 (defined as receiving a W-2 form) as the outcome, restricting attention to families who move when their child was between ages 17 and 23.[59] Since moves after age 16 cannot affect outcomes at age 16, $\hat{\mu}_{pc}^{TL}$ are placebos: if families' unobservables $\bar{\theta}_i$ vary with the time at which they move, we would expect $\hat{\mu}_{pc}^{TL}$ to be correlated with our baseline estimates μ_{pc}, but if our identification assumption holds, we would expect to find no such correlation. Empirically, the correlation between $\hat{\mu}_{pc}^{TL}$ and μ_{pc} is small and statistically insignificant; for instance, the correlation

[56] If families were sorting directly on places' causal effects on children μ_{pc}, we would expect different selection patterns for parents with children below vs. above age 23, in which case this placebo test would be uninformative. However, that form of selection is already ruled out by the evidence in Chetty and Hendren (2017) discussed above.

[57] These correlations are adjusted for attenuation due to noise in the estimates of $\hat{\mu}_{pc}$ by estimating $Corr(\hat{\mu}_{pc}^{23+}, \mu_{pc}) = Corr(\hat{\mu}_{pc}^{23+}, \hat{\mu}_{pc}) \frac{SD(\hat{\mu}_{pc})}{SD(\mu_{pc})}$, where $\frac{SD(\hat{\mu}_{pc})}{SD(\mu_{pc})}$ is the ratio of the raw standard deviation of the fixed effect estimates to the signal standard deviation, computed as described in Section V below.

[58] The population-weighted correlation between LFP rates at age 16 and income ranks at age 26 across CZs for children with parents at $p = 25$ is 0.27. We use children in the 1983-86 birth cohorts when analyzing LFP at age 16 because W-2 information is available starting in 1999, so labor force participation at age 16 is observed only starting with the 1983 birth cohort.

[59] To eliminate spurious correlations due to correlated sampling errors, we randomly split the data into two halves to estimate $\hat{\mu}_{pc}^{TL}$ and $\hat{\mu}_{pc}$ on independent samples.

is 0.00 (s.e. $= 0.08$) at $p = 25$ across CZs (Appendix Table I, row 8). In sum, the two placebo tests uncover no evidence that families who move to a given area at an earlier time differ systematically in their children's potential outcomes.

Time-Varying Factors. The second potential source of bias is time-varying factors $\tilde{\theta}_i$ that are correlated with families' decisions to move. For example, families may move to a given area c when their income has gone up. Growing up in a higher-income family might itself improve children's outcomes in proportion to their age, which would create bias in estimates of μ_{pc} using our exposure time design.

In our first paper, we showed that the changes in children's incomes when families move to areas with better permanent resident outcomes $\bar{y}_{pc}$ are not driven by time-varying confounds using a set of placebo tests exploiting heterogeneity across subgroups. As above, these findings rule out what is perhaps the most plausible form of omitted variable bias – namely that families who move to areas where children do better (higher μ_{pc}) may have experienced a positive shock that directly affects their children's outcomes irrespective of where they live. Hence, any remaining bias must arise from time-varying factors that vary across areas in a manner that is uncorrelated with their causal effects on average.

To assess the potential bias from such time-varying factors, we focus on two observable variables that are among the most important determinants of children's outcomes: parents' income and marital status (Jencks and Mayer 1990). We control for the effects of changes in income around the move when estimating (6) by including controls for the change in the parent's income rank from the year before to the year after the move interacted with the child's age at move. Similarly, we control for the impact of changes in marital status by interacting indicators for each of the four possible changes in marital status of the mother in the year before vs. after the move (married to unmarried, unmarried to married, unmarried to unmarried, and married to married) with the child's age at move.[60]

Appendix Figure II presents a scatter plot of the estimates of $\hat{\mu}_{25,c}$ with these controls vs. our baseline estimates $\hat{\mu}_{25,c}$ across CZs (Panel A) and counties (Panel B). The estimates obtained with controls for changes in income and marital status are nearly identical to the baseline estimates, with correlations above 0.97. These results imply that the estimates of place effects are not biased by changes in income or family structure around the point of the move. While these findings do

[60]For computational reasons, we use a two-step residualization procedure to incorporate these controls; see Appendix A for details.

not definitively establish the validity of the identification condition in Assumption 2, they imply that any violation of that assumption would have to arise from time-varying unobservables that are uncorrelated with both permanent residents' outcomes $\bar{y}_{pc}$ (in the origin and destination) and changes in income and marital status. We believe such violations of the identification condition are unlikely to be prevalent and therefore interpret our baseline fixed effects $\{\hat{\mu}_{pc}\}$ as unbiased estimates of place effects.

D Derivation of Optimal Forecasts

This appendix derives the coefficients $\rho_{1p}(s_{pc})$ and $\rho_{2p}(s_{pc})$ in (10) for our baseline forecasts that use information on permanent residents' outcomes $\bar{y}_{pc}$ and then discusses alternative forecasts that incorporate additional predictors.

Derivation of Baseline Optimal Forecast. Fix a particular level of parental income p and consider a hypothetical OLS regression of μ_{pc} on $\hat{\mu}_{pc}$ and $\bar{y}_{pc}$:

$$\mu_{pc} = \alpha + \rho_{1p}(s_{pc})\hat{\mu}_{pc} + \rho_{2p}(s_{pc})\bar{y}_{pc} + \phi_{pc}, \tag{19}$$

estimated with one observation per area (c) using the subset of areas whose fixed effect estimates have standard errors of a given level s_{pc}. To obtain the regression coefficients $\rho_{1p}(s_{pc})$ and $\rho_{2p}(s_{pc})$, we first orthogonalize the right hand side variables in this regression by projecting $\hat{\mu}_{pc}$ onto $\tilde{y}_{pc} = \bar{y}_{pc} - \bar{y}_p$. Let $\gamma_p = Cov(\hat{\mu}_{pc}, \tilde{y}_{pc})/Var(\tilde{y}_{pc})$ denote the coefficient from regressing $\hat{\mu}_{pc}$ on $\bar{y}_{pc}$. With this orthogonalization, we can rewrite the regression specification in (19) as

$$\mu_{pc} = \tilde{\alpha} + \tilde{\rho}_{1p}(\hat{\mu}_{pc} - \gamma_p(\bar{y}_{pc} - \bar{y}_p)) + \tilde{\rho}_{2p}\tilde{y}_{pc} + \phi_{pc}. \tag{20}$$

Since $Cov(\hat{\mu}_{pc} - \gamma_p\tilde{y}_{pc}, \tilde{y}_{pc}) = 0$, the coefficients $\tilde{\rho}_{1p}$ and $\tilde{\rho}_{2p}$ are given by the standard univariate OLS regression formulas:

$$\tilde{\rho}_{1p} = \frac{Cov(\mu_{pc}, \hat{\mu}_{pc} - \gamma_p\tilde{y}_{pc})}{Var(\hat{\mu}_{pc} - \gamma_p\tilde{y}_{pc})} = \frac{Cov(\mu_{pc} - \gamma_p\tilde{y}_{pc}, \hat{\mu}_{pc} - \gamma_p\tilde{y}_{pc})}{Var(\hat{\mu}_{pc} - \gamma_p\tilde{y}_{pc})} = \frac{\chi_p^2}{\chi_p^2 + s_{pc}^2}$$

and

$$\tilde{\rho}_{2p} = \frac{Cov(\mu_{pc}, \tilde{y}_{pc})}{Var(\tilde{y}_{pc})} = \frac{Cov(\mu_{pc}, \bar{y}_{pc})}{Var(\bar{y}_{pc})} = \gamma_p.$$

The intercept $\tilde{\alpha} = 0$ because all the variables in (20) have mean zero. Hence, the best linear predictor is given by

$$\mu_{pc}^f = \gamma_p(\bar{y}_{pc} - \bar{y}_p) + \frac{\chi_p^2}{\chi_p^2 + s_{pc}^2}(\hat{\mu}_{pc} - \gamma_p(\bar{y}_{pc} - \bar{y}_p)).$$

Intuitively, the optimal forecast is given by the best prediction based on permanent residents' outcomes $\gamma_p(\bar{y}_{pc} - \bar{y}_p)$, adjusted based on the difference between that prediction and $\hat{\mu}_{pc}$ with a weight proportional to the signal to noise ratio in $\hat{\mu}_{pc}$. Re-arranging terms yields the expression in (11).

Forecasts Using Additional Predictors. Our baseline forecasts use only one variable – permanent residents' mean ranks $\bar{y}_{pc}$ – to reduce the prediction error in our raw fixed effect estimates $\hat{\mu}_{25,c}$. Can we further reduce the MSE of our forecasts using additional observable characteristics?

We showed in Section VII that $\hat{\mu}_{25,c}$ is highly correlated with several observable characteristics of areas, including the following: the fraction of black residents, the Theil index of racial segregation, the Gini index of income equality, the fraction of single mothers, the social capital index, and expenditures on public schools per student. We construct forecasts that incorporate these characteristics (X_{pc}) by first regressing $\hat{\mu}_{pc}$ on $\bar{y}_{pc}$ and X_{pc} to construct predicted values (μ^X_{pc}) and residuals (ε^X_{pc}). We then take a weighted average of μ^X_{pc} and $\hat{\mu}_{pc}$, with weights that are a function of the signal variance in ε^X_{pc} and s_{pc}, as in (11).

The correlation between our baseline forecasts $\mu^f_{25,c}$ and these augmented forecasts is 0.93 weighted by population and 0.94 unweighted. The estimates are very similar throughout the distribution, and shown by the scatter plot in Online Appendix Figure III. This is because the observable characteristics X_{pc} explain very little residual variation in the causal effects $\hat{\mu}_{pc}$ *conditional* on permanent residents' observed outcomes $\bar{y}_{pc}$. The simple approach of shrinking $\hat{\mu}_{pc}$ towards predictions based on $\bar{y}_{pc}$ thus provides nearly as accurate a forecast of μ_{pc} as models that incorporate additional factors.

E Present Value of Lifetime Income Gains

This appendix explains how we calculate the present discounted value (PDV) of a 1% increase in income used in Section VIII.B.

We begin by tabulating mean individual incomes by age for a random sample of the U.S. population in 2012 from ages 26-65 in the tax data. We apply a 0.5% real wage growth rate (the rate of real wage growth in the U.S. over the past decade) and a 3% annual discount rate (the current 30-year Treasury bond rate), to this profile to obtain an undiscounted sum of lifetime income for the average American of \$1.7 million and a PDV at age 10 of \$600,000. Children with parents at $p = 25$ have mean incomes that are 81% of the mean individual incomes in our sample of county movers at age 26. Assuming that their incomes follow a similar lifecycle trajectory, the

PDV of income for children with parents at $p = 25$ is \$486,000.

Finally, assuming that the 1% increase in income at age 26 from moving to a better area persists throughout life, this implies that a PDV gain of \$4,860. Evidence from the Moving to Opportunity experiment indicates that the impacts of moving to a better neighborhood on income grow in percentage terms with age (Chetty, Hendren, and Katz 2016), suggesting that this calculation likely understates the total income gains.

Note that this calculation approximates the impacts of moving to a better neighborhood on individual lifetime income based on our baseline estimates of neighborhoods' causal effects μ_{pc}, which measure income at the household level (in percentage terms). Because neighborhood effects on individual and household income are very similar (Appendix Table IIb), this approximation is unlikely to introduce significant error in our calculations.

TABLE I
Summary Statistics for Movers Analysis Samples

Variable	Mean (1)	Std. Dev. (2)	Median (3)	Sample Size (4)
A. Between CZ Movers				
Parent family income ($)	89,029	353,465	56,700	1,397,260
Child family income at 26 ($)	31,706	88,503	24,300	1,397,260
Child family income at 30 ($)	45,890	99,172	33,100	459,952
Child individual income at 26 ($)	23,731	79,083	19,900	1,397,260
Child married at 26 (%)	26.5%	44.1%	0.0%	1,270,634
B. County within CZ Movers				
Parent family income ($)	82,627	300,952	57,000	931,138
Child family income at 26 ($)	32,304	62,314	25,000	931,138
Child family income at 30 ($)	46,477	86,911	33,800	316,106
Child individual income at 26 ($)	24,260	49,620	20,700	931,138
Child married at 26 (%)	25.9%	43.8%	0.0%	842,547

Notes: This table presents summary statistics for the primary analysis samples used to estimate the causal effects of counties and CZs. The sample consists of individuals who (1) have a valid Social Security Number or Individual Taxpayer Identification Number, (2) were born between 1980-1986, (3) are U.S. citizens as of 2013, and (4) were claimed as a child dependent at some point between 1996-2012. Panel A includes the subset of children satisfying these restrictions whose families moved exactly once across commuting zones between 1996 and 2012 before they turned 23. Panel B includes children whose families moved across counties within a CZ exactly once between 1996 and 2012 before they turned 23. Parent family income is the average pre-tax household income from 1996-2000, measured as AGI for tax filers and using information returns for non-filers. Child family income is measured analogously at various ages, while child individual income is defined as the sum of individual W-2 wage earnings, UI benefits, SSDI payments, and half of household self-employment income. See Section II of Chetty and Hendren (2017) for additional details on sample and variable definitions. All dollar values are reported in 2012 dollars, deflated using the CPI-U.

TABLE II
Magnitudes of Place Effects

	Commuting Zones		Counties		Counties within CZs	
	Below Median p = 25 (1)	Above Median p = 75 (2)	Below Median p = 25 (3)	Above Median p = 75 (4)	Below Median p = 25 (5)	Above Median p = 75 (6)
A. Annual Exposure Effect Estimates						
SD of Raw Exposure Effects	0.248	0.243	0.434	0.435	0.357	0.361
SD of Noise Component	0.210	0.218	0.402	0.407	0.343	0.344
Signal SD of Exposure Effects	0.132	0.107	0.165	0.155	0.099	0.112
Standard Error of Signal SD	0.010	0.013	0.014	0.016	0.008	0.008
B. Signal SD of Causal Effects from Birth: 20 Years of Exposure						
Percentile Rank	2.65	2.14	3.31	3.09	1.98	2.23
Income in Dollars	2165	1796	2706	2596	1623	1875
Percentage Change in Income	8.3%	4.4%	10.4%	6.4%	6.2%	4.6%
C. Correlations with Other Outcomes						
Signal Correlation of Causal Effects and Permanent Resident Outcomes	0.799	0.909	0.698	0.492	0.577	0.044
Correlation between Causal Effects at p = 25 and p = 75	0.724		0.287		0.080	

Notes: This table quantifies the magnitude of place effects on children's household (i.e., family) incomes. The first pair of columns reports estimates of the variation in place effects across CZs, estimated using movers across CZs. The second pair reports estimates across counties, summing the fixed effect estimates obtained from the county-within-CZ movers sample and the CZ estimates; and the third pair reports the implied county-within-CZ estimates based on the difference between the estimates in the first two pairs of columns. In each case, we report estimates for families with below-median (p = 25) and above-median (p=75) income. In Panel A, the first row presents the raw standard deviation (SD) of the fixed effect estimates across areas, weighting by precision ($1/SE^2$), where SE is the estimated standard error of the fixed effect. The second row presents standard deviation of the sampling noise component (again weighted by precision, $1/SE^2$). The third row presents the estimated signal standard deviation, computed as the square root of the difference between the raw variance and the noise variance. The fourth row shows the standard error of the signal SD estimate, computed using the asymptotic approximation in Online Appendix B. Panel B quantifies the size of the effects of growing up in a 1 SD better area from birth (20 years of childhood exposure). The first row shows the impact on a child's percentile rank, multiplying the signal SD estimate in Panel A by 20. The next two rows rescale these percentile rank impacts into dollar increases and percentage increases in children's incomes using the methods described in Section V.B. In Panel C, the first row reports the correlation between the causal effect estimates and the permanent resident outcomes (weighted by the precision of the fixed effect estimates), multiplied by the ratio of the raw SD to the signal SD to adjust for noise. The second row presents the correlation between the 25th and 75th percentile fixed effect estimates, weighting by the inverse of the sum of the variances of these estimates. These estimates are constructed by splitting the sample at the median family income to obtain estimates of the p = 25 and p = 75 fixed effects from independent samples, adjusting for noise by rescaling the raw correlation using the ratio of the raw SDs to the signal SDs.

TABLE III
MSE-Minimizing Forecasts of Causal Effects for 50 Largest Commuting Zones

Rank (p = 25)	Commuting Zone	State	Below-Median Income Parents (p = 25): Impact on Rank: Forecast (1)	Below-Median Income Parents (p = 25): Impact on Rank: RMSE (2)	Below-Median Income Parents (p = 25): % Impact from Birth: Forecast (3)	Below-Median Income Parents (p = 25): % Impact from Birth: Perm. Res. (4)	Above-Median Income Parents (p = 75): Impact on Rank: Forecast (5)	Above-Median Income Parents (p = 75): Impact on Rank: RMSE (6)	Above-Median Income Parents (p = 75): % Impact from Birth: Forecast (7)	Above-Median Income Parents (p = 75): % Impact from Birth: Perm. Res. (8)
1	Salt Lake City	UT	0.17	0.07	10.4	13.2	0.11	0.04	4.4	4.8
2	Seattle	WA	0.14	0.06	8.8	4.3	-0.01	0.04	-0.4	-1.2
3	Washington DC	DC	0.10	0.05	6.6	1.4	0.06	0.03	2.5	0.2
4	Minneapolis	MN	0.10	0.07	6.4	6.0	0.08	0.04	3.2	3.2
5	Fort Worth	TX	0.06	0.06	3.6	0.8	0.05	0.04	2.0	1.5
6	San Diego	CA	0.06	0.05	3.5	3.7	-0.13	0.04	-5.4	-5.1
7	Boston	MA	0.06	0.06	3.5	4.3	0.03	0.04	1.4	1.3
8	Manchester	NH	0.05	0.07	3.2	5.9	0.02	0.04	1.0	1.0
9	San Jose	CA	0.05	0.06	3.0	4.8	-0.12	0.04	-4.9	-5.1
10	Las Vegas	NV	0.04	0.06	2.7	-0.1	-0.08	0.04	-3.2	-2.7
11	Denver	CO	0.04	0.07	2.6	2.6	-0.06	0.04	-2.5	-1.4
12	Portland	OR	0.04	0.07	2.4	0.7	-0.09	0.04	-3.8	-4.6
13	San Francisco	CA	0.03	0.06	1.8	2.3	-0.12	0.04	-4.9	-5.0
14	Pittsburgh	PA	0.01	0.06	0.8	3.0	0.10	0.04	4.3	4.0
15	Newark	NJ	0.01	0.05	0.7	1.1	0.06	0.03	2.4	1.5
16	Providence	RI	0.01	0.07	0.4	2.0	0.02	0.04	0.9	1.4
17	Sacramento	CA	0.01	0.06	0.4	1.3	-0.14	0.04	-5.9	-4.7
18	Phoenix	AZ	0.00	0.05	0.2	0.6	-0.02	0.04	-0.7	-1.7
19	Buffalo	NY	0.00	0.07	-0.2	-2.5	0.01	0.04	0.4	-0.2
20	Kansas City	MO	-0.01	0.07	-0.4	-2.3	0.02	0.04	0.8	1.1
21	Houston	TX	-0.03	0.05	-1.6	-0.3	0.01	0.04	0.3	-0.3
22	Miami	FL	-0.03	0.04	-1.6	-2.3	-0.20	0.04	-8.3	-7.8
23	Philadelphia	PA	-0.03	0.06	-1.8	-4.7	0.00	0.04	0.2	0.9
24	Grand Rapids	MI	-0.03	0.07	-2.0	-3.0	0.07	0.04	2.7	2.2
25	Dallas	TX	-0.04	0.06	-2.4	-3.1	-0.01	0.04	-0.4	-0.8
26	Cleveland	OH	-0.04	0.06	-2.7	-9.2	-0.03	0.04	-1.0	-1.7
27	Bridgeport	CT	-0.05	0.06	-2.9	0.7	0.03	0.04	1.2	1.5
28	Jacksonville	FL	-0.05	0.06	-3.0	-7.3	-0.07	0.04	-2.9	-2.7
29	Milwaukee	WI	-0.05	0.07	-3.0	-5.4	0.04	0.04	1.8	2.1
30	Dayton	OH	-0.06	0.07	-3.9	-6.4	0.02	0.04	0.6	0.4
31	Cincinnati	OH	-0.08	0.07	-5.2	-7.4	0.06	0.04	2.6	1.3
32	Columbus	OH	-0.09	0.07	-5.4	-7.1	0.01	0.04	0.3	-1.1
33	Nashville	TN	-0.09	0.07	-5.5	-6.4	-0.03	0.04	-1.1	-1.8
34	St. Louis	MO	-0.09	0.07	-5.6	-7.3	0.03	0.04	1.2	0.7
35	Austin	TX	-0.10	0.07	-6.1	-4.0	-0.10	0.04	-4.1	-3.4
36	Baltimore	MD	-0.10	0.07	-6.4	-7.5	0.07	0.04	2.8	2.1
37	San Antonio	TX	-0.11	0.06	-6.9	-3.5	-0.08	0.04	-3.2	-3.3
38	Tampa	FL	-0.11	0.05	-7.1	-4.5	-0.13	0.04	-5.3	-4.2
39	New York	NY	-0.12	0.04	-7.3	-0.9	-0.03	0.04	-1.3	-1.1
40	Indianapolis	IN	-0.12	0.07	-7.4	-8.5	-0.02	0.04	-0.8	0.1
41	Atlanta	GA	-0.12	0.04	-7.8	-12.5	-0.09	0.04	-3.9	-7.4
42	Los Angeles	CA	-0.13	0.04	-8.1	0.8	-0.23	0.03	-9.3	-7.4
43	Detroit	MI	-0.14	0.05	-8.5	-11.8	-0.13	0.04	-5.2	-6.7
44	Orlando	FL	-0.14	0.05	-8.5	-4.6	-0.14	0.04	-5.7	-3.9
45	Chicago	IL	-0.15	0.05	-9.7	-6.9	-0.03	0.03	-1.4	-1.3
46	Fresno	CA	-0.16	0.06	-10.3	-1.8	-0.12	0.04	-5.0	-4.3
47	Port St. Lucie	FL	-0.17	0.06	-10.9	-5.8	-0.20	0.04	-8.2	-6.2
48	Raleigh	NC	-0.19	0.06	-12.2	-9.6	-0.11	0.04	-4.7	-3.3
49	Charlotte	NC	-0.20	0.06	-12.8	-11.0	-0.08	0.04	-3.5	-2.2
50	New Orleans	LA	-0.21	0.06	-13.4	-7.8	-0.06	0.04	-2.5	-2.6

Notes: This table presents MSE-minimizing forecasts of causal effects on children's incomes in adulthood for the 50 most populous CZs. Columns (1)-(4) present estimates for children growing up in below-median income (p = 25) families. Column (1) reports estimates of the causal impact of spending an additional year of childhood in a given CZ relative to the (population-weighted) average CZ in the country on a child's household income rank at age 26. Column (2) reports the root mean squared error of this forecast. Column (3) rescales the estimates in Column (1) to show the percentage change in income from growing up from birth (i.e., 20 years of childhood exposure) in a given CZ. Column (4) reports the same statistic, but for a forecast that is constructed solely based on the outcomes of permanent residents, excluding the data on movers. Columns (5)-(8) report the analogous statistics for above-median income (p = 75) families. The table is sorted based on CZs' causal impacts at p = 25.

TABLE IV

MSE-Minimizing Forecasts of Causal Effects for 100 Largest Counties (Top and Bottom 25)

Rank (p = 25)	County	State	Below-Median Income Parents (p = 25): Impact on Rank: Forecast (1)	Below-Median Income Parents (p = 25): Impact on Rank: RMSE (2)	Below-Median Income Parents (p = 25): % Impact from Birth: Forecast (3)	Below-Median Income Parents (p = 25): % Impact from Birth: Perm. Res. (4)	Above-Median Income Parents (p = 75): Impact on Rank: Forecast (5)	Above-Median Income Parents (p = 75): Impact on Rank: RMSE (6)	Above-Median Income Parents (p = 75): % Impact from Birth: Forecast (7)	Above-Median Income Parents (p = 75): % Impact from Birth: Perm. Res. (8)
1	Dupage	IL	0.26	0.09	16.0	10.6	0.08	0.08	3.1	1.8
2	Fairfax	VA	0.24	0.10	15.0	13.6	0.26	0.10	11.0	2.5
3	Snohomish	WA	0.22	0.10	14.0	6.6	0.06	0.09	2.4	0.3
4	Bergen	NJ	0.22	0.10	13.8	11.5	0.15	0.10	6.3	2.4
5	Bucks	PA	0.20	0.10	12.4	8.1	-0.02	0.10	-0.9	2.1
6	Norfolk	MA	0.18	0.10	11.5	11.8	0.15	0.10	6.2	2.0
7	Montgomery	PA	0.16	0.10	9.7	7.3	0.07	0.09	3.0	1.8
8	Montgomery	MD	0.15	0.10	9.5	7.8	0.00	0.10	0.1	0.0
9	King	WA	0.15	0.08	9.3	3.2	0.08	0.08	3.2	-1.3
10	Middlesex	NJ	0.15	0.10	9.1	7.0	0.01	0.10	0.5	2.0
11	Contra Costa	CA	0.14	0.09	8.8	2.1	-0.07	0.09	-2.9	-2.7
12	Middlesex	MA	0.12	0.09	7.7	8.5	0.01	0.09	0.5	1.0
13	Macomb	MI	0.11	0.09	7.0	-1.0	0.03	0.09	1.1	-1.8
14	Salt Lake	UT	0.10	0.10	6.2	9.7	0.02	0.09	0.7	2.1
15	Ventura	CA	0.10	0.10	6.2	5.0	-0.05	0.09	-2.3	-3.1
16	San Mateo	CA	0.08	0.10	5.3	6.2	-0.04	0.10	-1.5	-1.7
17	Worcester	MA	0.08	0.11	4.7	2.6	0.13	0.11	5.4	1.6
18	Monmouth	NJ	0.07	0.10	4.7	2.4	0.07	0.10	3.0	1.0
19	Honolulu	HI	0.07	0.10	4.6	2.1	-0.13	0.11	-5.4	-3.8
20	Hudson	NJ	0.07	0.10	4.2	-0.4	0.16	0.11	6.7	-0.1
21	Kern	CA	0.06	0.09	3.9	5.1	-0.06	0.11	-2.5	-0.1
22	Clark	NV	0.06	0.07	3.7	-0.7	-0.05	0.09	-1.9	-2.2
23	San Diego	CA	0.06	0.06	3.7	2.6	-0.14	0.06	-5.6	-3.4
24	Providence	RI	0.05	0.10	3.0	-0.4	-0.04	0.11	-1.8	0.5
25	San Francisco	CA	0.05	0.10	2.8	4.4	-0.18	0.10	-7.6	-4.9
75	Jefferson	KY	-0.14	0.10	-8.6	-11.4	0.02	0.11	0.9	-2.4
76	Franklin	OH	-0.14	0.09	-8.6	-11.4	0.11	0.10	4.7	-2.4
77	San Bernardino	CA	-0.14	0.06	-8.8	0.2	-0.25	0.07	-10.1	-3.3
78	Davidson	TN	-0.14	0.10	-8.9	-13.9	-0.04	0.11	-1.5	-4.5
79	Pima	AZ	-0.14	0.08	-8.9	-4.7	-0.14	0.10	-5.7	-4.2
80	Montgomery	OH	-0.14	0.10	-8.9	-11.7	-0.02	0.12	-0.6	-1.9
81	Travis	TX	-0.15	0.09	-9.2	-7.8	-0.16	0.09	-6.6	-4.4
82	Essex	NJ	-0.15	0.10	-9.2	-8.4	0.07	0.10	3.0	-2.1
83	Bexar	TX	-0.15	0.09	-9.6	-4.8	-0.09	0.12	-3.8	-3.6
84	Milwaukee	WI	-0.16	0.10	-9.9	-12.0	-0.03	0.10	-1.1	-1.7
85	Riverside	CA	-0.16	0.07	-10.1	1.2	-0.25	0.08	-10.3	-3.4
86	Los Angeles	CA	-0.16	0.05	-10.3	-0.5	-0.25	0.05	-10.5	-6.0
87	Wake	NC	-0.17	0.10	-10.7	-6.7	-0.09	0.10	-3.9	-2.1
88	New York	NY	-0.17	0.08	-10.8	-5.3	-0.27	0.10	-11.4	-6.9
89	Fulton	GA	-0.17	0.08	-10.9	-15.8	0.02	0.08	1.0	-7.4
90	Bronx	NY	-0.17	0.08	-10.9	-7.2	-0.20	0.11	-8.3	-6.1
91	Wayne	MI	-0.18	0.08	-11.4	-14.3	-0.07	0.08	-3.0	-5.6
92	Orange	FL	-0.19	0.08	-12.1	-6.1	-0.09	0.09	-3.8	-3.6
93	Cook	IL	-0.20	0.06	-12.8	-9.7	-0.03	0.05	-1.2	-2.6
94	Palm Beach	FL	-0.21	0.08	-13.0	-5.6	-0.31	0.10	-13.0	-4.6
95	Marion	IN	-0.21	0.10	-13.1	-13.3	-0.10	0.09	-4.2	-2.8
96	Shelby	TN	-0.21	0.09	-13.1	-16.8	0.03	0.10	1.2	-4.9
97	Fresno	CA	-0.22	0.09	-13.5	-3.7	-0.05	0.11	-2.1	-3.5
98	Hillsborough	FL	-0.22	0.09	-13.8	-4.3	-0.19	0.10	-7.9	-2.4
99	Baltimore City	MD	-0.22	0.09	-14.0	-15.5	-0.02	0.10	-0.7	-6.4
100	Mecklenburg	NC	-0.23	0.10	-14.5	-13.4	-0.09	0.10	-3.7	-3.8

Notes: This table presents MSE-minimizing forecasts of counties' causal effects on children's incomes in adulthood. The table reports estimates for counties that are in the top or bottom 25 among the 100 most populous counties based on their impacts on below-median income families (p = 25). Columns (1)-(4) present estimates for children growing up in below-median income (p = 25) families. Column (1) reports estimates of the causal impact of spending an additional year of childhood in a given county relative to the (population-weighted) average county in the country on a child's household income rank at age 26. Column (2) reports the root mean squared error of this forecast. Column (3) rescales the estimates in Column (1) to show the percentage change in income from growing up from birth (i.e., 20 years of childhood exposure) in a given county. Column (4) reports the same statistic, but for a forecast that is constructed solely based on the outcomes of permanent residents, excluding the data on movers. Columns (5)-(8) report the analogous statistics for above-median income (p = 75) families. The table is sorted based on counties' causal impacts at p = 25.

TABLE V

Association Between Rents and Places' Causal Effects on Children's Incomes for Low-Income Families

	Dep. Var.: Mean Monthly Rent For Low-Income Families ($)				
	CZs	Counties within CZs			
		All Counties	Counties in 100 Largest CZs	Observable Component	Unobservable Component
	(1)	(2)	(3)	(4)	(5)
Causal Effect (1% Increase in Child's Income)	-102.9	176.8**	202.4**		
	(82.91)	(65.50)	(64.93)		
Observable Component				430.2***	
				(27.29)	
Unobservable Component					46.18
					(42.65)
Mean of Dependent Variable	797.02	811.49	884.67	876.94	876.94
Signal R-Squared	0.180	0.013	0.051	0.301	0.000
Number of Observations	595	2367	694	673	673

Notes : This table shows estimates from OLS regressions of average monthly rents on areas' causal effects for children in below-median income (p = 25) families. Standard errors are shown in parentheses. Rents are measured using data from the 2000 Census (in 2012 dollars), and are defined as the mean of median rents across Census tracts, weighting by the number of below-median income families with children. Causal effects are MSE-minimizing forecasts constructed purely using data on movers (excluding permanent residents) and scaled in terms of percentage changes in children's incomes in adulthood per year of childhood exposure to a given CZ or county. All regressions are weighted by population based on the 2000 Census. In column (1), we regress rents on causal effects across CZs; the coefficient implies that CZs that generate 1% higher earnings for a single year of exposure have $102.9 lower monthly rents on average. Columns (2)-(5) report estimates of regressions at the county level, including CZ fixed effects. Column (2) includes all CZs, while columns (3)-(5) restrict the sample to the 100 most highly populated CZs (with populations above 590,000). In column (4), the independent variable is the "observable" component of causal effects, the predicted values from a regression of the raw fixed effect estimates on the following area-level characteristics: the fraction of African American residents, the Theil index of racial segregation, the Gini index, the fraction of single parents, the social capital index, and expenditures on public schools per student. In column (5), the independent variable is the residual from the preceding regression, shrunk based on the signal-to-noise ratio as described in the text to account for sampling error. In each column, we also report the mean of the dependent variable (monthly rent for low-income families) and the signal R-squared, constructed as the square of the correlation between rents and the right-hand-side variable (after removing CZ fixed effects in columns (2)-(5)). The R-squared estimates in columns (1)-(3) and (5) are adjusted for noise using the signal to noise ratio as described in the text.

ONLINE APPENDIX TABLE I

Fixed Effect Estimates: Sensitivity to Alternative Specifications

	Below-Median Income (p = 25)		Above-Median Income (p = 75)	
	Correlation (1)	Standard Error (2)	Correlation (3)	Standard Error (4)
A. Commuting Zones				
1. Quadratic Income Specification	0.940	0.014	0.932	0.015
2. Linear Cohort Controls	0.989	0.006	0.988	0.006
3. Cubic Cohort Controls	0.989	0.006	0.987	0.007
4. Income Levels ($, not ranks)	0.921	0.016	0.888	0.019
5. COLI adjusted	0.748	0.027	0.797	0.025
6. Controls for Changes in Par. Inc. and Mar. Status	0.970	0.010	0.978	0.009
7. Placebo 1: Moves After Age 23	0.036	0.077	0.012	0.092
8. Placebo 2: Teen Labor Force Participation	0.000	0.081	-0.122	0.117
B. Counties				
1. Quadratic Income Specification	0.876	0.010	0.776	0.013
2. Linear Cohort Controls	0.992	0.003	0.992	0.003
3. Cubic Cohort Controls	0.994	0.002	0.992	0.003
4. Income Levels ($, not ranks)	0.872	0.010	0.784	0.013
5. COLI adjusted	0.809	0.012	0.852	0.011
6. Controls for Changes in Par. Inc. and Mar. Status	0.987	0.003	0.991	0.003
7. Placebo 1: Moves After Age 23	0.039	0.054	0.014	0.059
8. Placebo 2: Teen Labor Force Participation	0.110	0.088	0.047	0.073

Notes: This table reports correlations between estimates of places' causal effects using alternative specifications and the baseline specification in equation (6). Columns (1) and (2) show point estimates and their standard errors for children in below-median income families (p = 25), while columns (3) and (4) show estimates and their standard errors for children in above-median income families (p = 75). Panel A shows correlations across CZs, while Panel B shows correlations across counties. Correlations are weighted using $1/(se\_base^2+se\_alt^2)$, where se_base and se_alt denote the standard errors of the fixed effect estimates from the baseline and alternative specifications. The alternative specifications used in each of the eight rows are as follows: (1) including a quadratic term in parent income rank (p^2) in equation 4 in the main text when defining place effects; (2) excluding the quadratic cohort controls (s^2) in equation 5 ; (3) including cubic cohort controls (s^3) in equation 5; (4) measuring children's incomes in dollars rather than percentile ranks; (5) deflating both children's and parents' incomes by a local cost-of-living price index based on their residence when income is measured before defining their income ranks; (6) including controls for changes in parent income and marital status in equation (6); (7) restricting the sample to families who move after their child turns 23; and (8) defining the child's outcome as an indicator for working at age 16, restricting the sample to families who move after the child turns 16. In (8), we use data for the 1980-86 cohort to estimate baseline effects of children's ranks at age 26 and the 1983-86 cohorts to estimate effects on teenage labor force participation. We randomly split the overlapping 1983-86 cohorts into two halves to estimate the two sets of fixed effects on independent samples. Since the "placebo" correlations in (7) and (8) are estimated on samples that are non-overlapping by construction, we adjust these correlations for noise by inflating them by the ratio of the raw standard deviation of the baseline fixed effect estimates to the signal standard deviations reported in Table II.

ONLINE APPENDIX TABLE II
Magnitude of Place Effects: Heterogeneity by Gender

	Commuting Zones				Counties			
	Below Median p=25		Above Median p = 75		Below Median p = 25		Above Median p = 75	
Child's Gender:	Male (1)	Female (2)	Male (3)	Female (4)	Male (5)	Female (6)	Male (7)	Female (8)
A. Household Income								
Signal SD of Exposure Effects	0.213	0.160	0.104	0.127	0.275	0.174	0.275	0.202
Standard Error of Signal SD	0.014	0.017	0.030	0.024	0.019	0.029	0.019	0.025
Dollar Impact from Birth	3106	2889	1583	2424	4016	3148	4166	3866
Percentage Impact from Birth	12.7%	10.4%	4.1%	5.6%	16.4%	11.4%	10.9%	9.0%
Correlation with Baseline Specification	0.706	0.668	0.677	0.677	0.646	0.653	0.632	0.635
B. Individual Income								
Signal SD of Exposure Effects	0.231	0.129	0.112	0.200	0.284	0.128	0.252	0.294
Standard Error of Signal SD	0.014	0.020	0.029	0.015	0.020	0.037	0.022	0.017
Dollar Impact from Birth	2851	1678	1646	3247	3509	1673	3685	4782
Percentage Impact from Birth	13.3%	9.1%	5.1%	11.6%	16.4%	9.0%	11.4%	17.2%
Correlation with Baseline Specification	0.663	0.425	0.596	0.494	0.588	0.403	0.547	0.431

Notes: This table quantifies the size of place effects for sons and daughters using fixed effect models that are estimated separately by gender. Panel A shows estimates which use children's family income at age 26 as the outcome; Panel B uses children's individual income (excluding spousal income) as the outcome. The first four columns report estimates of variation across CZs, estimated using movers across CZs. The last four columns report estimates across counties, summing the fixed effect estimates obtained from the county-within-CZ movers sample and the CZ estimates. In each case, we report estimates for families with below-median (p = 25) and above-median (p = 75) income. In each panel, the first row presents the estimated signal standard deviation, computed as described in the notes to Table II. The second row shows the standard error of the signal SD estimate, computed using the analytical approximation in Online Appendix B. The third and fourth rows rescale these percentile rank impacts into dollar increases and percentage increases in children's incomes from growing up in a 1 SD better area from birth (20 years of exposure) using the method described in Section V.B. The fifth row reports correlations between estimates of places' causal effects and the baseline specification in equation (6), computed as described in the notes to Appendix Table I.

ONLINE APPENDIX TABLE III
Forecasts of Causal Effects for 50 Largest Commuting Zones on Individual Income

			Below-Median Income Parents (p = 25)				Above-Median Income Parents (p = 75)			
			Impact on Rank		% Impact from Birth		Impact on Rank		% Impact from Birth	
Rank (p = 25)	Commuting Zone	State	Forecast (1)	RMSE (2)	Forecast (3)	Perm. Res. (4)	Forecast (5)	RMSE (6)	Forecast (7)	Perm. Res. (8)
1	Minneapolis	MN	0.16	0.07	10.6	6.5	0.11	0.03	6.5	6.6
2	Boston	MA	0.15	0.06	10.0	9.5	0.20	0.03	11.3	11.5
3	Newark	NJ	0.15	0.05	9.9	7.7	0.23	0.03	13.1	12.1
4	Seattle	WA	0.14	0.06	9.2	2.8	-0.02	0.03	-1.3	-1.3
5	Washington DC	DC	0.14	0.06	9.0	5.4	0.14	0.03	8.0	6.0
6	Buffalo	NY	0.12	0.07	7.7	0.3	0.09	0.03	5.1	4.1
7	Philadelphia	PA	0.08	0.06	5.4	1.0	0.14	0.03	7.9	8.5
8	San Francisco	CA	0.07	0.06	4.6	6.1	0.01	0.03	0.7	0.5
9	Bridgeport	CT	0.06	0.06	3.7	6.0	0.18	0.03	10.5	10.5
10	Las Vegas	NV	0.05	0.06	3.2	0.5	-0.12	0.03	-6.7	-6.1
11	Cleveland	OH	0.05	0.07	3.2	-6.3	0.02	0.03	1.3	0.6
12	Providence	RI	0.05	0.08	3.1	5.8	0.16	0.03	9.1	9.6
13	San Jose	CA	0.04	0.07	2.8	7.2	-0.02	0.03	-1.2	-0.9
14	Manchester	NH	0.04	0.08	2.6	5.0	0.10	0.03	6.0	5.6
15	Pittsburgh	PA	0.04	0.07	2.5	2.8	0.15	0.03	8.4	8.2
16	Fort Worth	TX	0.04	0.06	2.4	-2.1	-0.04	0.03	-2.4	-2.4
17	Milwaukee	WI	0.03	0.07	1.9	-2.0	0.09	0.03	5.2	5.6
18	Portland	OR	0.02	0.07	1.1	-1.0	-0.12	0.03	-6.8	-7.5
19	New York	NY	0.02	0.04	1.1	4.4	0.13	0.03	7.7	7.2
20	Phoenix	AZ	0.01	0.05	0.7	0.7	-0.07	0.03	-4.0	-4.2
21	Sacramento	CA	0.00	0.06	-0.3	1.5	-0.11	0.03	-6.3	-5.0
22	San Diego	CA	-0.01	0.06	-0.5	4.2	-0.10	0.03	-5.5	-4.0
23	Salt Lake City	UT	-0.01	0.07	-0.6	1.5	-0.20	0.03	-11.5	-12.2
24	Miami	FL	-0.02	0.05	-1.0	0.0	-0.10	0.03	-6.0	-5.9
25	Denver	CO	-0.02	0.07	-1.0	0.2	-0.10	0.03	-5.9	-4.8
26	Kansas City	MO	-0.03	0.07	-2.2	-3.4	-0.01	0.03	-0.7	-0.8
27	Cincinnati	OH	-0.04	0.08	-2.4	-7.0	0.05	0.03	3.1	1.7
28	St. Louis	MO	-0.04	0.07	-2.5	-5.3	0.02	0.03	1.3	0.7
29	Jacksonville	FL	-0.04	0.07	-2.7	-7.0	-0.09	0.03	-5.3	-5.1
30	Grand Rapids	MI	-0.05	0.08	-3.2	-6.2	-0.06	0.03	-3.2	-3.7
31	Baltimore	MD	-0.06	0.07	-3.7	-2.3	0.12	0.03	6.9	7.1
32	Chicago	IL	-0.06	0.05	-3.9	-1.6	0.07	0.03	3.8	3.7
33	Dallas	TX	-0.06	0.06	-4.1	-2.4	-0.03	0.03	-1.8	-1.9
34	Indianapolis	IN	-0.06	0.08	-4.2	-7.4	-0.04	0.03	-2.4	-2.1
35	Dayton	OH	-0.07	0.08	-4.5	-7.8	-0.05	0.03	-2.7	-2.8
36	Columbus	OH	-0.08	0.07	-5.6	-7.1	-0.02	0.03	-1.1	-2.2
37	Houston	TX	-0.09	0.06	-5.7	-0.3	-0.01	0.03	-0.4	-1.4
38	Nashville	TN	-0.11	0.08	-7.2	-8.0	-0.12	0.03	-6.7	-6.9
39	Detroit	MI	-0.11	0.06	-7.4	-8.6	-0.11	0.03	-6.4	-7.3
40	Austin	TX	-0.11	0.07	-7.5	-4.1	-0.11	0.03	-6.1	-5.8
41	Tampa	FL	-0.12	0.05	-7.6	-4.0	-0.09	0.03	-5.4	-4.8
42	Charlotte	NC	-0.13	0.07	-8.5	-8.5	-0.09	0.03	-4.9	-4.0
43	Orlando	FL	-0.13	0.05	-8.5	-3.8	-0.12	0.03	-6.6	-5.1
44	San Antonio	TX	-0.14	0.07	-9.0	-3.3	-0.11	0.03	-6.3	-6.2
45	Los Angeles	CA	-0.14	0.04	-9.1	3.3	-0.18	0.03	-10.2	-6.1
46	Fresno	CA	-0.15	0.07	-10.0	-2.2	-0.16	0.03	-9.3	-8.5
47	Port St. Lucie	FL	-0.15	0.06	-10.0	-3.5	-0.13	0.03	-7.2	-5.3
48	Atlanta	GA	-0.16	0.04	-10.4	-11.3	-0.13	0.03	-7.3	-10.2
49	New Orleans	LA	-0.20	0.07	-13.0	-4.9	-0.06	0.03	-3.5	-4.1
50	Raleigh	NC	-0.20	0.07	-13.3	-7.3	-0.10	0.03	-5.8	-4.7

Notes: This table replicates Table III, measuring children's income at the individual rather than household level (i.e., excluding spousal income). See notes to Table III for details.

ONLINE APPENDIX TABLE IV

Forecasts of Causal Effects for 100 Largest Counties (Top and Bottom 25) on Individual Income

Rank (p = 25)	County	State	Below-Median Income Parents (p = 25)				Above-Median Income Parents (p = 75)			
			Impact on Rank		% Impact from Birth		Impact on Rank		% Impact from Birth	
			Forecast	RMSE	Forecast	Perm. Res.	Forecast	RMSE	Forecast	Perm. Res.
			(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1	Bergen	NJ	0.29	0.10	19.0	15.8	0.33	0.10	18.6	13.4
2	Norfolk	MA	0.27	0.10	18.0	16.0	0.37	0.10	21.1	13.1
3	Fairfax	VA	0.23	0.10	15.1	14.0	0.34	0.10	19.5	10.0
4	Dupage	IL	0.22	0.09	14.3	10.0	0.18	0.08	10.4	7.3
5	Middlesex	NJ	0.22	0.10	14.3	11.6	0.20	0.10	11.7	12.7
6	King	WA	0.22	0.08	14.2	3.9	0.06	0.08	3.4	0.0
7	Bucks	PA	0.20	0.10	13.2	9.1	0.08	0.10	4.8	9.5
8	Middlesex	MA	0.18	0.09	11.8	12.2	0.16	0.09	9.2	10.3
9	Hudson	NJ	0.17	0.10	11.1	5.5	0.28	0.11	16.1	9.4
10	Montgomery	MD	0.17	0.10	11.1	10.6	0.16	0.10	9.3	5.9
11	Worcester	MA	0.15	0.10	10.0	5.4	0.23	0.11	13.1	9.6
12	Monmouth	NJ	0.15	0.10	9.8	7.2	0.23	0.10	13.4	10.3
13	Snohomish	WA	0.15	0.10	9.8	4.4	0.02	0.10	1.2	0.1
14	Contra Costa	CA	0.14	0.09	9.3	3.9	-0.01	0.09	-0.3	0.0
15	Suffolk	NY	0.14	0.09	9.0	8.3	0.18	0.10	10.5	9.5
16	Montgomery	PA	0.13	0.09	8.5	8.5	0.18	0.09	10.5	9.1
17	San Mateo	CA	0.12	0.10	8.0	9.3	0.08	0.10	4.6	4.8
18	Ventura	CA	0.12	0.10	8.0	5.5	-0.02	0.09	-1.4	-2.8
19	Westchester	NY	0.11	0.10	7.4	7.1	0.34	0.11	19.6	9.4
20	San Francisco	CA	0.11	0.10	7.2	9.9	-0.01	0.11	-0.7	0.2
21	Providence	RI	0.09	0.10	5.6	3.6	0.07	0.11	4.2	8.0
22	Queens	NY	0.08	0.07	5.6	7.1	0.13	0.09	7.2	3.8
23	Hartford	CT	0.08	0.10	5.3	2.9	0.18	0.10	10.3	9.1
24	Nassau	NY	0.08	0.08	5.3	12.7	0.21	0.09	12.2	13.4
25	Macomb	MI	0.07	0.09	4.7	-0.4	0.03	0.09	1.6	-1.4
75	Cook	IL	-0.08	0.06	-5.4	-4.4	0.08	0.06	4.8	1.0
76	Milwaukee	WI	-0.08	0.09	-5.4	-7.2	0.03	0.09	2.0	0.4
77	Oklahoma	OK	-0.08	0.09	-5.5	-5.9	-0.01	0.11	-0.5	-5.0
78	Bernalillo	NM	-0.09	0.09	-5.8	-2.5	-0.19	0.11	-11.0	-8.7
79	Wayne	MI	-0.10	0.08	-6.7	-10.4	-0.06	0.08	-3.2	-7.6
80	Davidson	TN	-0.10	0.09	-6.8	-11.1	-0.14	0.10	-8.0	-8.3
81	Shelby	TN	-0.11	0.09	-7.0	-12.4	-0.10	0.11	-5.5	-9.4
82	Pima	AZ	-0.11	0.09	-7.4	-4.1	-0.19	0.10	-11.0	-8.0
83	Marion	IN	-0.11	0.09	-7.5	-9.7	-0.09	0.10	-5.4	-4.8
84	Orange	FL	-0.12	0.07	-7.9	-4.8	-0.12	0.09	-7.0	-5.9
85	Hillsborough	FL	-0.13	0.09	-8.4	-3.9	-0.18	0.10	-10.0	-3.8
86	Jefferson	KY	-0.14	0.10	-8.9	-8.8	0.01	0.11	0.6	-4.2
87	Montgomery	OH	-0.14	0.10	-9.0	-10.6	-0.08	0.12	-4.7	-4.3
88	Baltimore City	MD	-0.14	0.09	-9.2	-8.5	0.01	0.10	0.4	-7.1
89	Los Angeles	CA	-0.14	0.04	-9.5	2.5	-0.24	0.05	-13.8	-5.9
90	Mecklenburg	NC	-0.15	0.09	-9.7	-9.0	-0.11	0.10	-6.1	-4.5
91	Cobb	GA	-0.15	0.09	-10.0	-5.9	-0.02	0.10	-1.2	-4.9
92	Palm Beach	FL	-0.15	0.08	-10.1	-3.4	-0.29	0.10	-16.4	-5.0
93	Bexar	TX	-0.15	0.09	-10.2	-4.2	-0.13	0.12	-7.4	-7.3
94	Fresno	CA	-0.16	0.09	-10.8	-3.4	-0.11	0.11	-6.1	-7.9
95	Fulton	GA	-0.17	0.08	-11.1	-13.6	0.01	0.08	0.4	-10.2
96	Travis	TX	-0.17	0.09	-11.1	-5.9	-0.16	0.09	-9.0	-7.1
97	Jefferson	AL	-0.17	0.10	-11.4	-8.7	-0.08	0.11	-4.3	-6.2
98	San Bernardino	CA	-0.19	0.06	-12.2	-0.6	-0.26	0.07	-15.1	-6.5
99	Wake	NC	-0.19	0.10	-12.5	-5.0	-0.13	0.10	-7.3	-3.7
100	Riverside	CA	-0.21	0.07	-14.0	0.4	-0.30	0.08	-17.4	-6.8

Notes: This table replicates Table IV, measuring children's income at the individual rather than household level (i.e., excluding spousal income). See notes to Table IV for details.

ONLINE APPENDIX TABLE V

Forecasts of Causal Effects for 50 Largest Commuting Zones on Rates of Marriage at Age 26

Rank (p = 25)	Commuting Zone	State	Below-Median Income Parents (p = 25): Annual Impact (pp) Forecast (1)	Annual Impact (pp) RMSE (2)	Impact from Birth (pp) Forecast (3)	Impact from Birth (pp) Perm. Res. (4)	Above-Median Income Parents (p = 75): Annual Impact (pp) Forecast (5)	Annual Impact (pp) RMSE (6)	Impact from Birth (pp) Forecast (7)	Impact from Birth (pp) Perm. Res. (8)
1	Salt Lake City	UT	0.54	0.11	10.8	11.9	0.79	0.03	15.8	15.9
2	Portland	OR	0.20	0.10	4.1	2.1	0.02	0.03	0.5	0.3
3	Grand Rapids	MI	0.20	0.11	3.9	4.4	0.35	0.03	7.0	7.0
4	Fort Worth	TX	0.16	0.09	3.1	2.5	0.20	0.03	3.9	3.9
5	Sacramento	CA	0.13	0.09	2.6	0.3	-0.07	0.03	-1.3	-1.5
6	Dayton	OH	0.10	0.11	2.1	2.4	0.22	0.03	4.4	4.5
7	San Diego	CA	0.10	0.08	2.1	-0.1	-0.16	0.03	-3.2	-3.5
8	San Antonio	TX	0.09	0.10	1.9	1.2	0.05	0.03	1.1	1.1
9	Nashville	TN	0.08	0.11	1.5	1.5	0.21	0.03	4.2	4.0
10	Kansas City	MO	0.06	0.10	1.1	1.1	0.11	0.03	2.2	2.6
11	Seattle	WA	0.04	0.09	0.7	0.9	-0.05	0.03	-1.0	-1.2
12	Houston	TX	0.03	0.08	0.6	-0.6	-0.01	0.03	-0.3	0.1
13	Austin	TX	0.03	0.10	0.5	0.0	-0.01	0.03	-0.2	-0.3
14	Columbus	OH	0.02	0.10	0.5	0.6	0.08	0.03	1.6	1.4
15	Dallas	TX	0.02	0.08	0.5	-1.7	0.04	0.03	0.7	0.6
16	Fresno	CA	0.01	0.10	0.2	1.6	0.09	0.03	1.9	1.8
17	Phoenix	AZ	-0.02	0.08	-0.3	0.1	0.09	0.03	1.8	1.4
18	Las Vegas	NV	-0.02	0.08	-0.5	-1.2	0.07	0.03	1.5	1.2
19	Denver	CO	-0.04	0.10	-0.7	0.8	-0.05	0.03	-1.0	-0.9
20	Indianapolis	IN	-0.04	0.10	-0.7	-0.6	0.13	0.03	2.7	2.7
21	Jacksonville	FL	-0.05	0.10	-1.1	-1.7	0.05	0.03	1.0	1.1
22	Cincinnati	OH	-0.08	0.10	-1.5	0.0	0.07	0.03	1.4	1.5
23	Minneapolis	MN	-0.08	0.10	-1.5	-0.7	-0.05	0.03	-1.0	-0.8
24	Pittsburgh	PA	-0.09	0.10	-1.8	0.2	-0.09	0.03	-1.8	-1.6
25	Tampa	FL	-0.10	0.08	-2.0	-1.8	-0.08	0.03	-1.6	-1.5
26	San Jose	CA	-0.11	0.09	-2.2	-2.2	-0.35	0.03	-7.0	-7.0
27	San Francisco	CA	-0.11	0.08	-2.3	-4.7	-0.40	0.03	-8.0	-8.0
28	Manchester	NH	-0.13	0.10	-2.5	0.0	-0.21	0.03	-4.2	-4.2
29	Atlanta	GA	-0.13	0.07	-2.6	-5.6	-0.08	0.03	-1.7	-2.3
30	Los Angeles	CA	-0.13	0.05	-2.7	-3.0	-0.23	0.03	-4.5	-5.2
31	St. Louis	MO	-0.14	0.10	-2.7	-2.4	0.00	0.03	0.1	0.2
32	Orlando	FL	-0.14	0.08	-2.8	-2.2	-0.06	0.03	-1.1	-0.8
33	Detroit	MI	-0.15	0.09	-2.9	-3.9	-0.14	0.03	-2.8	-2.9
34	Buffalo	NY	-0.15	0.10	-3.0	-2.5	-0.19	0.03	-3.7	-3.5
35	Charlotte	NC	-0.16	0.09	-3.2	-2.5	0.05	0.03	1.1	1.3
36	Providence	RI	-0.17	0.10	-3.5	-3.7	-0.31	0.03	-6.2	-6.4
37	Milwaukee	WI	-0.18	0.10	-3.6	-4.1	-0.06	0.03	-1.3	-1.2
38	Washington DC	DC	-0.20	0.08	-4.1	-5.5	-0.30	0.03	-6.1	-6.0
39	Raleigh	NC	-0.21	0.10	-4.2	-2.5	-0.02	0.03	-0.5	-0.3
40	Baltimore	MD	-0.22	0.09	-4.4	-6.5	-0.19	0.03	-3.9	-3.8
41	Cleveland	OH	-0.24	0.10	-4.7	-3.5	-0.13	0.03	-2.6	-2.5
42	Port St. Lucie	FL	-0.25	0.09	-4.9	-4.7	-0.21	0.03	-4.2	-4.5
43	Philadelphia	PA	-0.26	0.08	-5.1	-6.7	-0.33	0.03	-6.5	-6.4
44	Boston	MA	-0.30	0.08	-6.1	-5.7	-0.42	0.03	-8.4	-8.7
45	Bridgeport	CT	-0.32	0.09	-6.3	-5.4	-0.38	0.03	-7.7	-7.5
46	New Orleans	LA	-0.32	0.10	-6.4	-4.7	-0.06	0.03	-1.3	-1.1
47	Miami	FL	-0.33	0.07	-6.5	-6.1	-0.34	0.03	-6.8	-6.7
48	Chicago	IL	-0.33	0.07	-6.6	-5.8	-0.27	0.03	-5.3	-5.5
49	Newark	NJ	-0.38	0.07	-7.6	-6.2	-0.45	0.03	-9.0	-8.7
50	New York	NY	-0.46	0.06	-9.2	-5.6	-0.48	0.03	-9.5	-9.0

Notes: This table replicates Table III, defining the outcome as an indicator for being married at age 26 instead of the child's income. All estimates are scaled in terms of percentage point (pp) impacts on rates of marriage. See notes to Table III for details.

ONLINE APPENDIX TABLE VI

Forecasts of Causal Effects for 100 Largest Counties (Top and Bottom 25) on Rates of Marriage at Age 26

Rank (p = 25)	County	State	Below-Median Income Parents (p = 25)				Above-Median Income Parents (p = 75)			
			Annual Impact (pp)		Impact from Birth (pp)		Annual Impact (pp)		Impact from Birth (pp)	
			Forecast (1)	RMSE (2)	Forecast (3)	Perm. Res. (4)	Forecast (5)	RMSE (6)	Forecast (7)	Perm. Res. (8)
1	Salt Lake	UT	0.43	0.13	8.6	9.9	0.48	0.17	9.6	12.9
2	El Paso	TX	0.19	0.12	3.7	3.2	-0.12	0.20	-2.4	-1.4
3	Macomb	MI	0.18	0.12	3.6	-0.8	-0.10	0.15	-2.1	-2.0
4	Kern	CA	0.16	0.14	3.2	3.2	0.17	0.24	3.5	3.5
5	Hidalgo	TX	0.13	0.13	2.7	2.0	-0.05	0.22	-0.9	-1.6
6	Snohomish	WA	0.12	0.13	2.4	1.2	0.11	0.16	2.2	-0.2
7	Tulsa	OK	0.11	0.13	2.3	3.1	0.29	0.19	5.7	4.9
8	Multnomah	OR	0.09	0.12	1.8	-1.1	-0.06	0.16	-1.3	-2.8
9	Kent	MI	0.08	0.13	1.6	1.4	0.34	0.19	6.9	4.7
10	Dupage	IL	0.07	0.12	1.4	-0.5	-0.20	0.12	-4.0	-3.8
11	Contra Costa	CA	0.07	0.12	1.4	-2.9	-0.19	0.14	-3.9	-6.1
12	Sacramento	CA	0.06	0.12	1.2	-1.2	0.04	0.15	0.7	-2.7
13	Pierce	WA	0.05	0.12	1.0	1.3	0.16	0.16	3.3	0.9
14	Harris	TX	0.02	0.09	0.5	-1.1	-0.13	0.10	-2.5	-0.6
15	Riverside	CA	0.02	0.09	0.4	0.8	0.01	0.12	0.2	-0.1
16	Bexar	TX	0.01	0.14	0.3	0.3	-0.03	0.24	-0.5	-0.5
17	Oakland	MI	0.01	0.11	0.2	-1.1	-0.03	0.13	-0.7	-3.5
18	Oklahoma	OK	0.01	0.12	0.1	1.9	0.15	0.19	3.0	4.4
19	San Diego	CA	0.00	0.14	0.0	0.0	-0.16	0.24	-3.2	-3.2
20	San Bernardino	CA	0.00	0.08	0.0	0.7	0.01	0.11	0.2	-0.1
21	Bernalillo	NM	0.00	0.13	-0.1	-0.9	0.05	0.19	1.0	-0.6
22	Maricopa	AZ	0.00	0.08	-0.1	0.2	0.23	0.09	4.5	1.2
23	Gwinnett	GA	0.00	0.12	-0.1	-0.6	0.29	0.15	5.9	-0.2
24	Tarrant	TX	-0.01	0.11	-0.2	-0.4	0.12	0.13	2.5	1.9
25	Cobb	GA	-0.01	0.12	-0.2	-0.5	-0.13	0.16	-2.5	-0.6
75	Milwaukee	WI	-0.31	0.13	-6.1	-6.5	-0.14	0.16	-2.8	-2.9
76	Norfolk	MA	-0.31	0.12	-6.1	-5.6	-0.34	0.14	-6.8	-8.8
77	Bergen	NJ	-0.32	0.12	-6.4	-5.7	-0.43	0.15	-8.7	-8.6
78	Mecklenburg	NC	-0.33	0.13	-6.7	-6.8	-0.23	0.17	-4.6	-3.4
79	Queens	NY	-0.34	0.08	-6.7	-5.4	-0.45	0.12	-9.0	-8.3
80	Hudson	NJ	-0.34	0.12	-6.8	-6.5	-0.35	0.17	-7.0	-8.5
81	Shelby	TN	-0.35	0.12	-7.1	-8.5	0.00	0.17	0.1	-1.6
82	Fulton	GA	-0.35	0.11	-7.1	-9.5	-0.03	0.13	-0.6	-7.4
83	Wayne	MI	-0.36	0.11	-7.2	-6.3	-0.21	0.12	-4.1	-3.8
84	Middlesex	MA	-0.36	0.11	-7.2	-4.5	-0.45	0.12	-9.0	-8.4
85	San Francisco	CA	-0.36	0.12	-7.3	-7.9	-0.58	0.16	-11.5	-11.5
86	Fairfield	CT	-0.38	0.13	-7.5	-6.2	-0.44	0.17	-8.8	-8.6
87	DeKalb	GA	-0.38	0.11	-7.6	-8.0	-0.27	0.15	-5.3	-7.6
88	Broward	FL	-0.39	0.10	-7.8	-6.2	-0.36	0.14	-7.2	-6.1
89	Cook	IL	-0.40	0.08	-7.9	-6.8	-0.26	0.08	-5.3	-6.6
90	Cuyahoga	OH	-0.41	0.12	-8.3	-6.4	-0.35	0.15	-6.9	-4.8
91	Philadelphia	PA	-0.42	0.10	-8.4	-9.2	-0.23	0.13	-4.5	-7.8
92	Prince Georges	MD	-0.42	0.12	-8.4	-8.0	-0.49	0.16	-9.8	-8.2
93	Bronx	NY	-0.43	0.09	-8.7	-8.2	-0.77	0.16	-15.4	-10.4
94	Suffolk	NY	-0.45	0.11	-9.0	-4.9	-0.53	0.14	-10.6	-7.4
95	New York	NY	-0.45	0.09	-9.0	-8.3	-0.75	0.14	-15.0	-11.4
96	Suffolk	MA	-0.46	0.12	-9.2	-8.7	-0.44	0.16	-8.8	-10.7
97	Baltimore City	MD	-0.46	0.11	-9.2	-9.8	-0.46	0.15	-9.2	-6.5
98	Essex	NJ	-0.46	0.12	-9.3	-9.4	-0.48	0.15	-9.7	-10.4
99	Columbia	DC	-0.47	0.12	-9.4	-10.4	-0.75	0.17	-14.9	-12.3
100	Nassau	NY	-0.52	0.10	-10.3	-6.7	-0.64	0.12	-12.8	-9.1

Notes: This table replicates Table IV, defining the outcome as an indicator for being married at age 26 instead of the child's income. All estimates are scaled in terms of percentage points (pp) impacts on rates of marriage. See notes to Table IV for details.

ONLINE APPENDIX TABLE VII

Forecasts of Causal Effects for 50 Largest Commuting Zones on Household Income, by Child's Gender

			Below-Median Income Parents (p = 25)						Above-Median Income Parents (p = 75)					
			Sons			Daughters			Sons			Daughters		
			Impact on Rank		% Impact	Impact on Rank		% Impact	Impact on Rank		% Impact	Impact on Rank		% Impact
Rank (p = 25)	Commuting Zone	State	Forecast	RMSE	from Birth	Forecast	RMSE	from Birth	Forecast	RMSE	from Birth	Forecast	RMSE	from Birth
			(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1	Salt Lake City	UT	0.06	0.13	3.6	0.23	0.10	15.3	0.08	0.04	3.2	0.14	0.06	6.3
2	Seattle	WA	0.15	0.10	9.1	0.22	0.09	14.2	-0.02	0.04	-0.8	-0.03	0.06	-1.4
3	Washington DC	DC	0.08	0.10	4.7	0.11	0.08	7.1	0.00	0.04	0.0	0.08	0.05	3.6
4	Minneapolis	MN	0.16	0.13	9.2	0.15	0.10	10.1	0.05	0.04	2.1	0.11	0.06	5.0
5	Fort Worth	TX	0.10	0.11	5.8	0.02	0.09	1.4	0.00	0.04	0.1	0.11	0.06	4.9
6	San Diego	CA	0.02	0.10	1.1	0.09	0.08	5.7	-0.15	0.04	-6.1	-0.11	0.06	-4.7
7	Boston	MA	0.05	0.11	3.3	0.01	0.09	0.8	0.04	0.04	1.7	0.00	0.06	0.0
8	Manchester	NH	0.06	0.14	3.7	-0.01	0.11	-0.7	0.04	0.04	1.5	-0.01	0.06	-0.6
9	San Jose	CA	-0.13	0.11	-7.6	0.19	0.09	12.4	-0.13	0.04	-5.3	-0.12	0.06	-5.1
10	Las Vegas	NV	-0.03	0.09	-1.7	0.15	0.08	9.6	-0.08	0.04	-3.1	-0.07	0.06	-3.2
11	Denver	CO	0.04	0.12	2.1	0.01	0.10	0.5	-0.05	0.04	-2.1	-0.07	0.06	-3.0
12	Portland	OR	0.13	0.12	7.6	0.04	0.10	2.6	-0.09	0.04	-3.4	-0.14	0.06	-6.2
13	San Francisco	CA	0.00	0.10	-0.3	0.09	0.08	5.6	-0.12	0.04	-4.7	-0.15	0.06	-6.6
14	Pittsburgh	PA	0.00	0.13	-0.1	0.07	0.10	4.6	0.08	0.04	3.2	0.10	0.06	4.6
15	Newark	NJ	0.04	0.08	2.3	-0.05	0.07	-3.2	0.04	0.04	1.6	0.03	0.05	1.3
16	Providence	RI	0.00	0.13	-0.1	-0.01	0.10	-0.5	0.04	0.04	1.7	-0.01	0.06	-0.5
17	Sacramento	CA	-0.08	0.10	-4.5	0.07	0.08	4.6	-0.14	0.04	-5.4	-0.16	0.06	-7.2
18	Phoenix	AZ	-0.05	0.08	-3.2	0.08	0.07	5.0	-0.06	0.04	-2.2	-0.02	0.05	-0.8
19	Buffalo	NY	-0.01	0.12	-0.5	-0.01	0.10	-0.4	-0.01	0.04	-0.3	0.00	0.06	0.1
20	Kansas City	MO	-0.04	0.13	-2.5	-0.01	0.10	-0.8	-0.01	0.04	-0.3	0.05	0.06	2.1
21	Houston	TX	-0.09	0.09	-5.6	0.00	0.08	0.3	-0.02	0.04	-0.7	0.01	0.06	0.4
22	Miami	FL	-0.10	0.08	-6.1	0.01	0.07	0.9	-0.22	0.04	-8.7	-0.24	0.06	-10.5
23	Philadelphia	PA	-0.09	0.09	-5.2	0.02	0.08	1.6	0.01	0.04	0.3	-0.01	0.06	-0.6
24	Grand Rapids	MI	0.00	0.14	0.2	-0.05	0.11	-3.2	0.05	0.04	2.0	0.03	0.07	1.5
25	Dallas	TX	-0.15	0.09	-8.7	0.06	0.08	3.9	-0.07	0.04	-2.7	0.07	0.06	3.3
26	Cleveland	OH	0.10	0.12	5.7	-0.08	0.10	-5.1	-0.04	0.04	-1.4	-0.06	0.06	-2.7
27	Bridgeport	CT	-0.11	0.11	-6.8	-0.03	0.09	-2.1	0.02	0.04	0.7	0.01	0.06	0.4
28	Jacksonville	FL	0.03	0.12	1.9	-0.11	0.10	-7.5	-0.11	0.04	-4.5	-0.04	0.06	-1.9
29	Milwaukee	WI	-0.11	0.14	-6.8	-0.06	0.11	-3.9	0.03	0.04	1.3	0.05	0.06	2.1
30	Dayton	OH	-0.07	0.14	-4.3	-0.04	0.11	-2.9	0.00	0.04	-0.1	0.00	0.07	0.1
31	Cincinnati	OH	0.00	0.14	-0.1	-0.07	0.10	-4.7	0.01	0.04	0.6	0.08	0.06	3.4
32	Columbus	OH	0.06	0.13	3.6	-0.12	0.10	-7.7	-0.03	0.04	-1.1	0.01	0.06	0.6
33	Nashville	TN	-0.06	0.14	-3.4	-0.12	0.10	-7.7	-0.06	0.04	-2.4	-0.01	0.06	-0.4
34	St. Louis	MO	-0.06	0.13	-3.6	-0.10	0.10	-6.5	0.01	0.04	0.4	0.05	0.06	2.1
35	Austin	TX	-0.07	0.12	-4.3	-0.06	0.10	-4.2	-0.14	0.04	-5.5	-0.03	0.06	-1.4
36	Baltimore	MD	-0.24	0.11	-14.3	-0.02	0.09	-1.4	0.02	0.04	0.7	0.13	0.06	5.9
37	San Antonio	TX	-0.17	0.11	-10.0	-0.14	0.09	-9.2	-0.11	0.04	-4.5	-0.04	0.06	-1.8
38	Tampa	FL	-0.17	0.09	-10.0	-0.07	0.08	-4.4	-0.16	0.04	-6.4	-0.10	0.06	-4.6
39	New York	NY	-0.14	0.07	-8.2	-0.15	0.06	-9.9	-0.04	0.04	-1.4	-0.05	0.05	-2.3
40	Indianapolis	IN	-0.05	0.14	-3.1	-0.16	0.10	-10.4	-0.01	0.04	-0.3	-0.05	0.06	-2.2
41	Atlanta	GA	-0.13	0.07	-7.9	-0.13	0.07	-8.2	-0.15	0.04	-5.9	-0.09	0.05	-4.1
42	Los Angeles	CA	-0.21	0.06	-12.3	-0.09	0.05	-5.8	-0.20	0.03	-7.9	-0.26	0.05	-11.4
43	Detroit	MI	-0.26	0.10	-15.4	-0.04	0.09	-2.8	-0.14	0.04	-5.7	-0.17	0.06	-7.4
44	Orlando	FL	-0.23	0.09	-13.4	-0.14	0.08	-9.0	-0.14	0.04	-5.7	-0.13	0.06	-5.7
45	Chicago	IL	-0.23	0.08	-14.0	-0.12	0.07	-7.7	-0.04	0.04	-1.4	-0.03	0.05	-1.3
46	Fresno	CA	-0.24	0.11	-14.5	-0.11	0.09	-7.2	-0.13	0.04	-5.1	-0.11	0.06	-4.9
47	Port St. Lucie	FL	-0.26	0.11	-15.3	-0.06	0.09	-3.7	-0.19	0.04	-7.7	-0.22	0.06	-9.7
48	Raleigh	NC	-0.20	0.12	-11.8	-0.20	0.10	-13.3	-0.11	0.04	-4.5	-0.15	0.06	-6.8
49	Charlotte	NC	-0.19	0.11	-11.3	-0.27	0.09	-17.5	-0.09	0.04	-3.7	-0.05	0.06	-2.3
50	New Orleans	LA	-0.19	0.13	-11.1	-0.28	0.10	-18.6	-0.05	0.04	-2.1	-0.09	0.06	-4.1

Notes: This table replicates Table III, reporting separate estimates for sons and daughters (omitting the forecasts based purely on permanent resident outcomes). Commuting Zones ares sorted by their ranks in Table III (i.e., their impacts pooling genders). See notes to Table III for details.

ONLINE APPENDIX TABLE VIII

Forecasts of Causal Effects for 100 Largest Counties (Top and Bottom 25) on Household Income, by Child's Gender

			Below-Median Income Parents (p = 25)						Above-Median Income Parents (p = 75)					
			Sons			Daughters			Sons			Daughters		
			Impact on Rank		% Impact	Impact on Rank		% Impact	Impact on Rank		% Impact	Impact on Rank		% Impact
Rank (p = 25)	County	State	Forecast	RMSE	from Birth	Forecast	RMSE	from Birth	Forecast	RMSE	from Birth	Forecast	RMSE	from Birth
			(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1	Dupage	IL	0.20	0.16	12.2	0.28	0.11	18.2	0.14	0.12	5.7	0.05	0.11	2.1
2	Fairfax	VA	0.15	0.19	9.2	0.23	0.12	15.1	0.42	0.16	16.8	0.15	0.13	6.6
3	Snohomish	WA	0.23	0.18	13.9	0.22	0.12	14.6	0.08	0.16	3.2	0.03	0.13	1.2
4	Bergen	NJ	0.28	0.19	16.6	0.17	0.12	11.2	0.15	0.17	5.9	0.17	0.13	7.6
5	Bucks	PA	0.28	0.19	16.8	0.14	0.12	9.2	-0.07	0.16	-3.0	0.00	0.13	0.1
6	Norfolk	MA	0.21	0.19	12.4	0.14	0.12	8.9	0.18	0.16	7.2	0.11	0.13	4.9
7	Montgomery	PA	0.07	0.17	4.4	0.18	0.12	11.6	0.11	0.15	4.4	0.03	0.13	1.3
8	Montgomery	MD	0.13	0.18	7.5	0.21	0.12	13.6	-0.22	0.16	-8.6	0.16	0.13	6.9
9	King	WA	0.19	0.14	11.1	0.17	0.11	11.4	0.11	0.12	4.4	0.03	0.11	1.4
10	Middlesex	NJ	0.13	0.19	7.8	0.14	0.12	9.4	0.03	0.17	1.3	-0.01	0.14	-0.4
11	Contra Costa	CA	0.24	0.17	14.5	0.14	0.12	9.4	-0.08	0.15	-3.3	-0.07	0.12	-3.1
12	Middlesex	MA	0.13	0.16	7.6	0.08	0.11	5.2	0.11	0.14	4.2	-0.08	0.12	-3.8
13	Macomb	MI	0.04	0.16	2.5	0.14	0.11	8.9	0.04	0.15	1.4	-0.01	0.12	-0.5
14	Salt Lake	UT	-0.01	0.17	-0.9	0.16	0.12	10.2	-0.03	0.16	-1.4	0.09	0.13	4.0
15	Ventura	CA	0.18	0.18	10.9	0.05	0.12	3.5	-0.03	0.15	-1.2	-0.09	0.13	-4.1
16	San Mateo	CA	0.07	0.19	4.2	0.11	0.12	7.0	-0.13	0.17	-5.2	0.02	0.14	0.8
17	Worcester	MA	0.02	0.20	1.2	0.07	0.13	4.4	0.14	0.18	5.5	0.11	0.14	5.1
18	Monmouth	NJ	0.01	0.19	0.6	0.10	0.12	6.8	0.07	0.16	2.7	0.07	0.13	3.0
19	Honolulu	HI	0.01	0.19	0.5	0.05	0.12	3.4	-0.29	0.20	-11.5	-0.02	0.15	-1.1
20	Hudson	NJ	0.18	0.19	10.4	-0.02	0.12	-1.1	0.22	0.19	8.6	0.14	0.15	6.3
21	Kern	CA	0.10	0.15	6.0	0.02	0.11	1.1	-0.01	0.19	-0.4	-0.05	0.14	-2.2
22	Clark	NV	-0.02	0.10	-1.2	0.10	0.10	6.6	-0.13	0.12	-5.3	-0.05	0.16	-2.3
23	San Diego	CA	0.03	0.11	1.6	0.08	0.09	5.2	-0.23	0.11	-9.2	-0.06	0.10	-2.5
24	Providence	RI	0.11	0.19	6.5	0.01	0.13	0.8	0.05	0.19	1.9	-0.12	0.15	-5.2
25	San Francisco	CA	-0.09	0.18	-5.5	0.13	0.12	8.4	-0.15	0.18	-6.1	-0.21	0.14	-9.5
75	Jefferson	KY	0.00	0.20	-0.2	-0.11	0.13	-7.1	0.03	0.20	1.3	-0.04	0.15	-1.8
76	Franklin	OH	0.13	0.17	7.6	-0.20	0.12	-12.9	0.17	0.16	6.8	0.09	0.13	3.8
77	San Bernardino	CA	-0.20	0.10	-11.9	-0.09	0.08	-5.6	-0.14	0.11	-5.6	-0.33	0.10	-14.7
78	Davidson	TN	-0.10	0.18	-5.7	-0.15	0.12	-10.0	0.15	0.18	5.9	-0.08	0.14	-3.4
79	Pima	AZ	-0.39	0.16	-23.0	0.00	0.11	0.0	-0.22	0.17	-8.8	-0.09	0.14	-3.8
80	Montgomery	OH	-0.15	0.20	-9.0	-0.13	0.13	-8.7	-0.19	0.20	-7.4	0.00	0.15	0.1
81	Travis	TX	-0.17	0.16	-10.4	-0.07	0.11	-4.3	-0.34	0.15	-13.6	0.00	0.12	-0.1
82	Essex	NJ	-0.08	0.17	-4.8	-0.19	0.12	-12.7	0.09	0.16	3.5	0.06	0.13	2.7
83	Bexar	TX	-0.20	0.18	-12.0	-0.02	0.13	-1.6	-0.14	0.25	-5.6	-0.09	0.17	-4.0
84	Milwaukee	WI	-0.25	0.18	-14.8	-0.14	0.12	-9.4	-0.04	0.17	-1.4	-0.04	0.14	-1.9
85	Riverside	CA	-0.29	0.10	-17.0	-0.07	0.09	-4.7	-0.22	0.12	-8.7	-0.27	0.10	-12.0
86	Los Angeles	CA	-0.22	0.07	-13.0	-0.12	0.06	-8.0	-0.26	0.07	-10.5	-0.25	0.07	-11.3
87	Wake	NC	-0.23	0.19	-13.4	-0.14	0.12	-9.1	-0.10	0.17	-4.1	-0.14	0.14	-6.4
88	New York	NY	-0.12	0.13	-7.0	-0.23	0.10	-14.9	-0.19	0.17	-7.3	-0.39	0.13	-17.2
89	Fulton	GA	-0.20	0.13	-11.6	-0.18	0.10	-11.5	0.18	0.13	7.2	-0.07	0.11	-3.3
90	Bronx	NY	-0.26	0.13	-15.2	-0.14	0.10	-9.0	-0.16	0.18	-6.5	-0.25	0.14	-11.3
91	Wayne	MI	-0.29	0.13	-17.4	-0.11	0.10	-6.9	-0.05	0.12	-1.9	-0.13	0.11	-5.9
92	Orange	FL	-0.25	0.13	-14.6	-0.18	0.10	-12.0	-0.15	0.15	-6.1	-0.03	0.12	-1.6
93	Cook	IL	-0.23	0.09	-13.7	-0.20	0.08	-12.8	-0.04	0.08	-1.7	0.02	0.08	0.8
94	Palm Beach	FL	-0.28	0.15	-16.5	-0.08	0.11	-5.5	-0.31	0.16	-12.4	-0.38	0.13	-16.9
95	Marion	IN	-0.15	0.17	-8.8	-0.24	0.12	-15.5	-0.05	0.16	-1.8	-0.20	0.13	-9.0
96	Shelby	TN	-0.15	0.16	-9.0	-0.15	0.12	-10.1	0.19	0.18	7.7	-0.05	0.14	-2.2
97	Fresno	CA	-0.28	0.15	-16.8	-0.13	0.11	-8.5	-0.04	0.20	-1.7	-0.03	0.15	-1.2
98	Hillsborough	FL	-0.27	0.15	-16.3	-0.16	0.11	-10.2	-0.39	0.16	-15.4	-0.12	0.14	-5.5
99	Baltimore City	MD	-0.47	0.15	-27.9	-0.08	0.11	-5.4	-0.10	0.16	-3.9	0.04	0.13	2.0
100	Mecklenburg	NC	-0.22	0.17	-12.8	-0.22	0.12	-14.7	-0.16	0.17	-6.2	-0.01	0.14	-0.5

Notes: This table replicates Table IV, reporting separate estimates for sons and daughters (omitting the forecasts based purely on permanent resident outcomes). Counties are sorted by their ranks in Table IV (i.e., their impacts pooling genders). See notes to Table IV for details.

ONLINE APPENDIX TABLE IX
MSE-Minimizing Forecasts of Causal Effects for 50 Largest Commuting Zones on Individual Income, by Child's Gender

			Below-Median Income Parents (p = 25)						Above-Median Income Parents (p = 75)					
			Sons			Daughters			Sons			Daughters		
Rank	Commuting		Impact on Rank		% Impact	Impact on Rank		% Impact	Impact on Rank		% Impact	Impact on Rank		% Impact
(p = 25)	Zone	State	Forecast	RMSE	from Birth	Forecast	RMSE	from Birth	Forecast	RMSE	from Birth	Forecast	RMSE	from Birth
			(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1	Minneapolis	MN	0.19	0.14	10.7	0.17	0.09	12.0	0.06	0.05	2.9	0.16	0.11	9.2
2	Boston	MA	0.15	0.11	8.6	0.10	0.08	7.4	0.12	0.05	5.5	0.18	0.09	10.4
3	Newark	NJ	0.16	0.09	9.0	0.14	0.07	10.2	0.12	0.05	5.6	0.29	0.08	16.8
4	Seattle	WA	0.15	0.11	8.9	0.11	0.08	7.7	-0.01	0.05	-0.6	-0.12	0.09	-6.9
5	DC	DC	0.08	0.10	4.5	0.15	0.08	10.4	0.03	0.04	1.4	0.26	0.07	15.4
6	Buffalo	NY	0.16	0.13	9.5	0.03	0.09	1.9	0.04	0.05	1.6	0.25	0.11	14.5
7	Philadelphia	PA	-0.08	0.10	-4.4	0.20	0.07	14.3	0.06	0.05	2.6	0.15	0.08	8.7
8	San Francisco	CA	0.00	0.11	0.2	0.14	0.08	9.5	-0.06	0.05	-2.6	0.06	0.09	3.7
9	Bridgeport	CT	-0.06	0.12	-3.3	0.08	0.08	5.9	0.08	0.05	3.6	0.19	0.09	11.4
10	Las Vegas	NV	-0.06	0.10	-3.5	0.14	0.07	9.7	-0.08	0.05	-3.7	-0.20	0.10	-11.9
11	Cleveland	OH	0.18	0.13	10.4	0.03	0.09	1.9	-0.03	0.05	-1.4	0.11	0.11	6.5
12	Providence	RI	0.06	0.14	3.2	0.02	0.09	1.1	0.11	0.05	5.1	0.04	0.12	2.3
13	San Jose	CA	-0.08	0.12	-4.8	0.12	0.08	8.4	-0.09	0.05	-4.1	0.03	0.10	1.7
14	Manchester	NH	0.05	0.15	3.1	-0.02	0.09	-1.4	0.06	0.05	2.9	0.14	0.11	8.2
15	Pittsburgh	PA	0.07	0.14	3.9	0.01	0.09	0.9	0.10	0.05	4.5	0.10	0.11	5.9
16	Fort Worth	TX	0.10	0.12	6.0	-0.01	0.08	-0.9	-0.05	0.05	-2.0	0.06	0.10	3.6
17	Milwaukee	WI	-0.10	0.15	-6.0	0.02	0.09	1.1	0.06	0.05	2.6	0.10	0.12	5.8
18	Portland	OR	0.12	0.13	7.1	-0.05	0.09	-3.4	-0.09	0.05	-4.2	-0.13	0.11	-7.7
19	New York	NY	-0.04	0.07	-2.5	0.04	0.06	2.6	0.03	0.04	1.5	0.15	0.07	8.6
20	Phoenix	AZ	-0.03	0.09	-1.8	0.05	0.07	3.3	-0.06	0.05	-2.7	-0.12	0.08	-6.9
21	Sacramento	CA	-0.11	0.11	-6.3	0.08	0.08	5.3	-0.13	0.05	-5.8	-0.15	0.09	-8.5
22	San Diego	CA	-0.01	0.10	-0.7	-0.02	0.08	-1.4	-0.13	0.05	-5.8	-0.11	0.09	-6.5
23	Salt Lake City	UT	-0.03	0.14	-1.7	-0.03	0.09	-2.5	-0.06	0.05	-2.6	-0.19	0.11	-11.0
24	Miami	FL	-0.16	0.09	-9.4	0.07	0.07	5.2	-0.22	0.05	-9.9	-0.10	0.09	-5.8
25	Denver	CO	0.01	0.12	0.5	-0.01	0.09	-0.4	-0.11	0.05	-5.1	-0.16	0.09	-9.1
26	Kansas City	MO	-0.07	0.14	-4.1	-0.04	0.09	-2.6	-0.03	0.05	-1.5	0.02	0.11	0.9
27	Cincinnati	OH	-0.04	0.14	-2.4	0.01	0.09	0.7	0.02	0.05	0.7	0.15	0.11	8.7
28	St. Louis	MO	-0.07	0.14	-4.2	-0.02	0.09	-1.2	0.01	0.05	0.3	0.11	0.11	6.3
29	Jacksonville	FL	0.01	0.13	0.8	-0.11	0.09	-7.6	-0.14	0.05	-6.2	-0.06	0.11	-3.2
30	Grand Rapids	MI	0.09	0.16	5.2	-0.10	0.10	-6.7	0.00	0.05	0.0	-0.11	0.13	-6.5
31	Baltimore	MD	-0.26	0.12	-15.1	0.03	0.09	2.2	0.02	0.05	0.9	0.25	0.10	14.8
32	Chicago	IL	-0.19	0.09	-11.1	0.04	0.07	2.7	0.00	0.04	-0.1	0.18	0.07	10.7
33	Dallas	TX	-0.16	0.10	-9.5	0.04	0.07	3.2	-0.11	0.05	-5.2	0.18	0.08	10.5
34	Indianapolis	IN	-0.07	0.14	-4.0	-0.07	0.09	-4.9	-0.04	0.05	-1.8	-0.08	0.12	-4.5
35	Dayton	OH	-0.06	0.16	-3.7	-0.03	0.10	-1.9	-0.04	0.05	-1.6	-0.09	0.13	-5.4
36	Columbus	OH	0.06	0.14	3.2	-0.07	0.09	-5.0	-0.06	0.05	-2.5	0.03	0.12	1.6
37	Houston	TX	-0.07	0.10	-3.9	-0.06	0.07	-4.2	-0.01	0.05	-0.6	0.09	0.08	5.1
38	Nashville	TN	-0.10	0.15	-5.7	-0.06	0.09	-4.5	-0.13	0.05	-5.7	-0.02	0.12	-1.0
39	Detroit	MI	-0.20	0.11	-11.4	-0.01	0.08	-0.4	-0.11	0.05	-5.1	-0.12	0.09	-7.1
40	Austin	TX	-0.09	0.13	-5.2	-0.04	0.09	-3.0	-0.17	0.05	-7.6	0.04	0.11	2.6
41	Tampa	FL	-0.19	0.09	-11.3	-0.04	0.07	-2.7	-0.19	0.05	-8.5	-0.04	0.09	-2.4
42	Charlotte	NC	-0.19	0.12	-11.0	-0.06	0.08	-4.1	-0.13	0.05	-6.1	-0.06	0.10	-3.3
43	Orlando	FL	-0.27	0.09	-15.5	-0.04	0.07	-3.0	-0.15	0.05	-7.0	-0.15	0.10	-8.6
44	San Antonio	TX	-0.18	0.12	-10.3	-0.08	0.08	-6.0	-0.16	0.05	-7.1	0.03	0.11	1.6
45	Los Angeles	CA	-0.20	0.06	-11.5	-0.08	0.05	-5.8	-0.17	0.04	-7.8	-0.30	0.06	-17.4
46	Fresno	CA	-0.23	0.12	-13.4	-0.09	0.08	-6.2	-0.15	0.05	-6.7	-0.21	0.12	-12.1
47	Port St. Lucie	FL	-0.27	0.12	-15.7	-0.01	0.08	-0.7	-0.20	0.05	-9.2	-0.31	0.10	-17.9
48	Atlanta	GA	-0.23	0.08	-13.2	-0.10	0.06	-6.9	-0.17	0.05	-7.5	0.02	0.07	1.0
49	New Orleans	LA	-0.22	0.14	-12.9	-0.13	0.09	-9.4	-0.06	0.05	-2.6	0.04	0.12	2.2
50	Raleigh	NC	-0.24	0.13	-13.8	-0.09	0.09	-6.1	-0.15	0.05	-6.8	-0.17	0.10	-10.0

Notes: This table replicates Online Appendix Table VII, measuring income at the individual level. Commuting Zones ares sorted by their ranks in Online Appendix Table III (i.e., their impacts pooling genders). See notes to Table III for details.

ONLINE APPENDIX TABLE X

MSE-Minimizing Forecasts of Causal Effects for 100 Largest Counties (Top and Bottom 25) on Individual Income, by Child's Gender

			Below-Median Income Parents (p = 25)						Above-Median Income Parents (p = 75)					
			Sons			Daughters			Sons			Daughters		
Rank (p = 25)	County	State	Impact on Rank Forecast (1)	Impact on Rank RMSE (2)	% Impact from Birth (3)	Impact on Rank Forecast (4)	Impact on Rank RMSE (5)	% Impact from Birth (6)	Impact on Rank Forecast (7)	Impact on Rank RMSE (8)	% Impact from Birth (9)	Impact on Rank Forecast (10)	Impact on Rank RMSE (11)	% Impact from Birth (12)
1	Bergen	NJ	0.35	0.19	20.3	0.21	0.08	15.0	0.25	0.16	11.3	0.39	0.16	22.9
2	Norfolk	MA	0.31	0.19	17.8	0.19	0.08	13.4	0.31	0.16	14.1	0.39	0.16	22.5
3	Fairfax	VA	0.15	0.19	8.9	0.20	0.08	13.9	0.36	0.16	16.3	0.32	0.16	18.7
4	Dupage	IL	0.23	0.16	13.5	0.15	0.08	10.5	0.17	0.12	7.7	0.23	0.12	13.5
5	Middlesex	NJ	0.26	0.19	15.2	0.16	0.08	11.2	0.09	0.16	4.3	0.27	0.17	15.8
6	King	WA	0.21	0.14	11.8	0.13	0.07	9.4	0.05	0.12	2.1	-0.01	0.12	-0.8
7	Bucks	PA	0.25	0.19	14.5	0.11	0.08	8.1	-0.02	0.16	-0.8	0.11	0.16	6.4
8	Middlesex	MA	0.23	0.16	13.2	0.12	0.08	8.4	0.19	0.14	8.6	0.03	0.13	2.0
9	Hudson	NJ	0.28	0.19	16.1	0.10	0.08	7.3	0.17	0.18	7.7	0.45	0.19	25.9
10	Montgomery	MD	0.16	0.19	9.5	0.18	0.08	12.5	-0.10	0.16	-4.3	0.45	0.16	26.3
11	Worcester	MA	0.14	0.20	8.3	0.08	0.08	5.7	0.15	0.17	6.8	0.25	0.18	14.4
12	Monmouth	NJ	0.16	0.19	9.0	0.12	0.08	8.5	0.15	0.16	6.7	0.30	0.16	17.6
13	Snohomish	WA	0.18	0.18	10.7	0.04	0.08	2.9	0.02	0.15	0.8	-0.05	0.15	-3.2
14	Contra Costa	CA	0.22	0.17	12.5	0.09	0.08	6.7	-0.07	0.15	-3.2	0.07	0.15	3.9
15	Suffolk	NY	0.21	0.17	12.4	0.10	0.08	6.8	0.02	0.15	0.8	0.33	0.16	19.4
16	Montgomery	PA	-0.01	0.17	-0.3	0.13	0.08	9.1	0.11	0.15	4.9	0.21	0.15	12.4
17	San Mateo	CA	0.06	0.19	3.3	0.14	0.08	9.9	-0.08	0.17	-3.4	0.25	0.17	14.8
18	Ventura	CA	0.28	0.18	16.0	0.05	0.08	3.4	-0.04	0.15	-1.6	0.00	0.15	0.2
19	Westchester	NY	-0.08	0.19	-4.8	0.16	0.08	11.5	0.43	0.18	19.3	0.29	0.18	17.2
20	San Francisco	CA	-0.03	0.19	-1.9	0.16	0.08	11.4	-0.07	0.17	-3.3	0.08	0.17	4.6
21	Providence	RI	0.16	0.19	9.4	0.04	0.08	2.9	0.04	0.18	2.0	-0.04	0.19	-2.2
22	Queens	NY	0.00	0.11	0.3	0.11	0.07	8.0	-0.03	0.14	-1.5	0.23	0.14	13.6
23	Hartford	CT	0.08	0.19	4.7	0.07	0.08	4.8	0.12	0.16	5.4	0.15	0.16	8.8
24	Nassau	NY	0.10	0.15	6.0	0.11	0.08	7.8	-0.05	0.14	-2.4	0.42	0.14	24.2
25	Macomb	MI	0.16	0.16	9.2	0.01	0.08	1.0	0.12	0.14	5.5	-0.10	0.14	-5.6
75	Cook	IL	-0.19	0.10	-11.0	0.00	0.06	0.1	-0.03	0.08	-1.2	0.25	0.08	14.3
76	Milwaukee	WI	-0.26	0.18	-15.1	-0.02	0.08	-1.7	-0.02	0.16	-0.8	0.12	0.16	7.1
77	Oklahoma	OK	-0.07	0.17	-4.3	-0.05	0.08	-3.5	0.16	0.18	7.2	-0.01	0.18	-0.6
78	Bernalillo	NM	-0.28	0.18	-16.1	-0.02	0.08	-1.6	-0.25	0.18	-11.1	-0.26	0.18	-15.4
79	Wayne	MI	-0.23	0.14	-13.3	-0.02	0.07	-1.1	0.01	0.12	0.4	-0.11	0.12	-6.4
80	Davidson	TN	-0.12	0.18	-6.7	-0.05	0.08	-3.6	-0.02	0.17	-0.8	-0.08	0.17	-4.8
81	Shelby	TN	-0.13	0.16	-7.6	-0.03	0.08	-2.0	0.10	0.17	4.7	-0.10	0.18	-5.7
82	Pima	AZ	-0.37	0.16	-21.2	-0.01	0.08	-1.0	-0.24	0.16	-10.7	-0.06	0.17	-3.8
83	Marion	IN	-0.13	0.17	-7.6	-0.08	0.08	-6.0	-0.11	0.15	-5.0	-0.09	0.15	-5.3
84	Orange	FL	-0.34	0.13	-19.6	0.00	0.07	0.2	-0.18	0.14	-8.0	-0.04	0.14	-2.1
85	Hillsborough	FL	-0.21	0.15	-12.0	-0.03	0.08	-2.1	-0.35	0.16	-15.8	-0.12	0.17	-6.9
86	Jefferson	KY	-0.16	0.20	-9.0	-0.07	0.08	-5.0	0.03	0.19	1.4	0.08	0.20	4.9
87	Montgomery	OH	-0.18	0.20	-10.6	-0.08	0.08	-5.6	-0.21	0.19	-9.6	-0.08	0.19	-4.6
88	Baltimore City	MD	-0.49	0.16	-28.1	0.01	0.08	1.0	-0.15	0.16	-6.9	0.21	0.16	12.2
89	Los Angeles	CA	-0.20	0.07	-11.7	-0.05	0.05	-3.8	-0.23	0.07	-10.2	-0.27	0.07	-15.8
90	Mecklenburg	NC	-0.24	0.17	-14.0	-0.04	0.08	-2.6	-0.15	0.16	-6.6	-0.05	0.16	-2.7
91	Cobb	GA	-0.24	0.17	-14.0	-0.06	0.08	-4.5	-0.01	0.15	-0.5	0.06	0.15	3.6
92	Palm Beach	FL	-0.28	0.15	-16.2	-0.01	0.08	-0.5	-0.34	0.16	-15.4	-0.42	0.16	-24.3
93	Bexar	TX	-0.25	0.18	-14.7	-0.04	0.08	-3.0	-0.19	0.23	-8.5	0.00	0.24	-0.2
94	Fresno	CA	-0.24	0.16	-13.6	-0.08	0.08	-5.8	-0.07	0.18	-3.2	-0.09	0.19	-5.1
95	Fulton	GA	-0.29	0.13	-16.8	-0.08	0.07	-5.6	0.09	0.13	4.3	0.03	0.13	1.7
96	Travis	TX	-0.23	0.16	-13.1	-0.04	0.08	-2.9	-0.31	0.14	-13.8	0.05	0.14	3.1
97	Jefferson	AL	-0.34	0.19	-19.7	-0.10	0.08	-6.9	-0.06	0.19	-2.5	-0.02	0.19	-1.1
98	San Bernardino	CA	-0.22	0.10	-12.6	-0.12	0.06	-8.4	-0.15	0.11	-6.9	-0.42	0.11	-24.6
99	Wake	NC	-0.27	0.19	-15.8	-0.04	0.08	-3.0	-0.16	0.16	-7.3	-0.17	0.16	-9.7
100	Riverside	CA	-0.28	0.11	-16.0	-0.12	0.07	-8.2	-0.20	0.12	-8.9	-0.47	0.11	-27.4

Notes: This table replicates Online Appendix Table VIII, measuring income at the individual level. Counties are sorted by their ranks in Online Appendix Table IV (i.e., their impacts pooling genders). See notes to Table IV for details.

ONLINE APPENDIX TABLE XI

Associations Between Place Effects and Area-Level Characteristics Across Commuting Zones for Low-Income Families (p = 25)

		SD of Covariate (1)	Signal Correlation (2)		Regression Estimates: Perm. Res. (3)		Causal (4)		Sorting (5)	
		Std. Dev	Correlation	s.e.	%	s.e.	%	s.e.	%	s.e.
Segregation and Poverty	Fraction Black Residents	0.100	-0.51	0.13	-7.49	0.71	-4.27	1.06	-3.22	0.96
	Poverty Rate	0.041	-0.14	0.16	-1.74	0.93	-1.20	1.29	-0.55	1.28
	Racial Segregation Theil Index	0.106	-0.51	0.11	-5.16	0.78	-4.24	0.90	-0.92	0.98
	Income Segregation Theil Index	0.034	-0.57	0.14	-3.36	0.97	-4.76	1.14	1.40	1.19
	Segregation of Poverty (<p25)	0.029	-0.55	0.15	-3.82	0.88	-4.55	1.20	0.73	1.15
	Segregation of Affluence (>p75)	0.039	-0.58	0.13	-2.99	1.01	-4.81	1.08	1.82	1.20
	Share with Commute < 15 Mins	0.091	0.88	0.13	5.01	1.06	7.26	1.11	-2.25	1.02
	Log. Population Density	1.341	-0.65	0.12	-3.39	1.10	-5.37	0.99	1.98	0.87
Income Distribution	Household Income per Capita for Working-Age Adults	6920.4	-0.30	0.15	-0.58	0.88	-2.52	1.24	1.94	0.86
	Gini coefficient for Parent Income	0.082	-0.76	0.13	-4.20	1.56	-6.35	1.09	2.15	1.19
	Top 1% Income Share for Parents	0.050	-0.49	0.09	-0.97	0.91	-4.09	0.79	3.11	0.65
	Gini within Bottom 99%	0.054	-0.71	0.11	-5.50	1.20	-5.92	0.89	0.42	1.25
	Fraction Middle Class (Between National p25 and p75)	0.061	0.70	0.14	4.87	1.27	5.81	1.17	-0.94	1.23
Tax	Local Tax Rate	0.006	-0.13	0.14	-0.15	0.95	-1.04	1.14	0.90	0.96
	Local Tax Rate per Capita	0.327	-0.29	0.17	-0.30	1.08	-2.43	1.42	2.13	1.09
	Local Government Expenditures per Capita	673.6	-0.30	0.13	0.73	0.90	-2.49	1.09	3.22	1.27
	State EITC Exposure	3.712	0.15	0.15	2.52	0.92	1.25	1.28	1.27	0.81
	State Income Tax Progressivity	2.341	-0.08	0.16	1.89	0.64	-0.66	1.31	2.55	1.30
K-12 Education	School Expenditure per Student ($1000)	1.312	-0.02	0.15	0.79	0.89	-0.13	1.22	0.91	1.12
	Student/Teacher Ratio	2.665	-0.35	0.11	0.35	1.15	-2.87	0.89	3.22	1.21
	Test Score Percentile (Controlling for Parent Income)	7.157	0.51	0.10	2.27	2.05	4.22	0.84	-1.95	1.76
	High School Dropout Rate (Controlling for Parent Income)	0.016	-0.55	0.14	-4.92	1.04	-4.57	1.15	-0.35	0.92
College	Number of Colleges per Capita	0.006	0.60	0.13	1.42	0.90	4.99	1.07	-3.57	1.08
	Mean College Tuition	3311.9	-0.15	0.11	-0.31	0.86	-1.22	0.88	0.91	1.01
	College Graduation Rate (Controlling for Parent Income)	0.104	0.14	0.12	1.60	0.84	1.17	0.96	0.43	0.65
Local Labor Market	Labor Force Participation Rate	0.047	0.14	0.16	0.93	0.90	1.17	1.34	-0.24	1.06
	Fraction Working in Manufacturing	0.061	0.03	0.15	-0.64	0.94	0.23	1.22	-0.86	1.00
	Growth in Chinese Imports 1990-2000	0.979	-0.03	0.12	0.68	0.72	-0.27	0.97	0.95	0.67
	Teenage (14-16) Labor Force Participation Rate	0.100	0.55	0.14	3.90	1.47	4.60	1.15	-0.70	1.63
Migration	Migration Inflow Rate	0.011	-0.17	0.14	-0.02	0.88	-1.44	1.15	1.42	0.90
	Migration Outflow Rate	0.007	-0.12	0.13	0.81	0.88	-0.97	1.07	1.78	0.88
	Fraction of Foreign Born Residents	0.100	-0.45	0.10	0.73	0.89	-3.71	0.86	4.44	0.99
Social Capital	Social Capital Index (Rupasingha and Goetz 2008)	0.926	0.70	0.13	3.61	1.24	5.78	1.10	-2.17	1.29
	Fraction Religious	0.105	0.18	0.17	3.21	1.14	1.48	1.43	1.73	0.87
	Violent Crime Rate	0.001	-0.68	0.12	-2.91	1.86	-5.64	0.96	2.73	1.47
Family Structure	Fraction of Children with Single Mothers	0.036	-0.57	0.12	-7.55	1.08	-4.70	0.99	-2.85	1.20
	Fraction of Adults Divorced	0.015	0.04	0.16	-2.12	0.89	0.33	1.30	-2.45	0.86
	Fraction of Adults Married	0.033	0.52	0.14	4.31	1.14	4.33	1.17	-0.02	1.29
Prices	Mean House Prices for Low-Income (p = 25) families	97393.1	-0.38	0.14	0.58	0.97	-3.17	1.14	3.76	0.58
	Mean Monthly Rents for Low-Income (p = 25) families	178.538	-0.43	0.15	0.14	1.08	-3.54	1.24	3.68	0.92

Notes: This table shows associations between CZs causal effects on the incomes of children in low-income families (p = 25) and area-level characteristics. Online Appendix Table XV provides a definition and source for each of the area-level characteristics in this table. Column (1) reports the standard deviation of each covariate across CZs. Column (2) reports the signal correlation between each CZ's causal effect and the covariate, computed as the correlation between the raw fixed effect estimate for each CZ and the covariate multiplied by the ratio of the total SD to the signal SD of the causal effects from Table II. Columns (3) and (4) report coefficients from univariate OLS regressions of permanent resident outcomes and causal effects on each characteristic. The characteristics are normalized to have a (population-weighted) mean of zero and unit standard deviation across CZs in these regressions. Both permanent resident outcomes and the causal effects are rescaled so the coefficients can be interpreted as impacts in percentage units using the approach described in Section V.B. Column (3) shows the coefficient and standard error (s.e.) from the regressions of permanent resident outcomes on each covariate. Column (4) shows the coefficient and s.e. from regressions of the causal effect of growing up in an area from birth (20 years of exposure) on each covariate. The difference between these coefficients, shown in column (5), equals the coefficient from a regression of the difference between the permanent resident outcomes and causal effects on the covariate, which can be interpreted as the association between sorting and the covariate. Standard errors for all estimates are clustered at the state level to account for spatial autocorrelation. All statistics reported are computed using population weights, using CZ populations from the 2000 Census.

ONLINE APPENDIX TABLE XII

Associations Between Place Effects and Area-Level Characteristics Across Counties within CZs for Low-Income Families (p = 25)

		SD of Covariate (1)	Signal Correlation (2)		Regression Estimates: Perm. Res. (3)		Causal (4)		Sorting (5)	
		Std. Dev	Correlation	s.e.	%	s.e.	%	s.e.	%	s.e.
Segregation and Poverty	Fraction Black Residents	0.130	-0.32	0.10	-7.07	0.55	-1.98	0.64	-5.08	0.69
	Poverty Rate	0.055	-0.23	0.11	-6.12	0.72	-1.44	0.67	-4.67	0.63
	Racial Segregation Theil Index	0.118	-0.37	0.10	-7.01	0.46	-2.31	0.60	-4.70	0.61
	Income Segregation Theil Index	0.039	-0.42	0.10	-5.25	0.36	-2.62	0.63	-2.63	0.62
	Segregation of Poverty (<p25)	0.034	-0.46	0.10	-5.65	0.41	-2.88	0.64	-2.77	0.65
	Segregation of Affluence (>p75)	0.045	-0.36	0.11	-4.53	0.39	-2.22	0.66	-2.31	0.62
	Share with Commute < 15 Mins	0.100	0.02	0.12	0.73	0.62	0.12	0.73	0.62	0.98
	Log. Population Density	1.698	-0.27	0.11	-5.53	0.86	-1.67	0.69	-3.86	0.93
Income Distribution	Household Income per Capita for Working-Age Adults	9220.2	0.06	0.14	2.56	0.79	0.35	0.87	2.20	0.62
	Gini coefficient for Parent Income	0.113	-0.41	0.14	-6.05	1.31	-2.55	0.85	-3.50	0.86
	Top 1% Income Share for Parents	0.001	-0.23	0.10	-2.96	0.82	-1.41	0.59	-1.54	0.74
	Gini within Bottom 99%	0.112	-0.41	0.14	-6.06	1.31	-2.55	0.85	-3.51	0.86
	Fraction Middle Class (Between National p25 and p75)	0.074	0.13	0.13	2.14	0.83	0.80	0.83	1.34	0.72
Tax	Local Tax Rate	0.009	-0.21	0.12	-2.82	2.03	-1.32	0.77	-1.51	1.71
	Local Tax Rate per Capita	0.432	-0.15	0.11	-1.34	1.63	-0.91	0.66	-0.44	1.47
	Local Government Expenditures per Capita	1.0	-0.30	0.13	-3.26	1.80	-1.86	0.84	-1.40	1.37
	State EITC Exposure	3.747	-0.01	0.21	-0.27	0.19	-0.08	1.31	-0.19	1.23
	State Income Tax Progressivity	2.368	-0.19	0.27	-0.42	0.41	-1.20	1.68	0.78	1.80
K-12 Education	School Expenditure per Student ($1000)	1.478	-0.07	0.12	-1.13	1.29	-0.41	0.75	-0.73	1.23
	Student/Teacher Ratio	2.807	-0.10	0.11	-1.73	0.71	-0.65	0.67	-1.08	0.94
	Test Score Percentile (Controlling for Parent Income)	9.593	0.35	0.13	5.51	1.16	2.20	0.81	3.31	0.99
	High School Dropout Rate (Controlling for Parent Income)	0.023	-0.37	0.13	-5.63	0.69	-2.33	0.80	-3.30	0.95
College	Number of Colleges per Capita	0.011	-0.19	0.16	-3.10	0.42	-1.15	1.01	-1.95	0.97
	Mean College Tuition	4421.3	-0.02	0.14	-1.03	0.79	-0.10	0.86	-0.93	1.24
	College Graduation Rate (Controlling for Parent Income)	0.139	0.03	0.16	-1.71	0.63	0.22	0.97	-1.93	1.06
Local Labor Market	Labor Force Participation Rate	0.058	-0.10	0.12	2.97	0.75	-0.60	0.77	3.56	0.73
	Fraction Working in Manufacturing	0.070	0.24	0.13	3.06	0.46	1.52	0.80	1.54	0.80
	Teenage (14-16) Labor Force Participation Rate	0.108	0.09	0.12	3.25	0.66	0.54	0.77	2.71	0.79
Migration	Migration Inflow Rate	0.019	-0.04	0.09	3.20	0.72	-0.23	0.53	3.43	0.70
	Migration Outflow Rate	0.014	0.01	0.12	0.44	0.75	0.06	0.77	0.40	0.78
	Fraction of Foreign Born Residents	0.110	-0.03	0.12	-1.96	0.68	-0.18	0.77	-1.78	0.75
Social Capital	Social Capital Index (Rupasingha and Goetz 2008)	1.089	0.15	0.15	-0.15	0.72	0.92	0.92	-1.08	1.09
	Fraction Religious	0.127	0.07	0.14	-0.01	0.55	0.47	0.85	-0.48	0.89
	Violent Crime Rate	0.002	-0.32	0.11	-5.50	0.45	-1.99	0.66	-3.50	0.63
Family Structure	Fraction of Children with Single Mothers	0.069	-0.38	0.11	-7.80	0.81	-2.34	0.66	-5.45	0.61
	Fraction of Adults Divorced	0.017	-0.34	0.13	-5.28	0.52	-2.09	0.82	-3.20	0.81
	Fraction of Adults Married	0.062	0.33	0.09	7.46	0.42	2.07	0.58	5.39	0.64
Prices	Mean House Prices for Low-Income (p = 25) families	107811.0	-0.04	0.13	0.77	0.59	-0.27	0.80	1.04	0.81
	Mean Monthly Rents for Low-Income (p = 25) families	212.250	0.09	0.11	3.51	0.67	0.59	0.71	2.91	0.77

Notes: This table replicates Online Appendix Table XI at the county-within-CZ level. All standard deviations, correlations, and regressions are estimated at the county level after residualizing all variables with respect to CZ fixed effects, weighting by county population. Standard errors are clustered at the CZ level to account for spatial autocorrelation.

ONLINE APPENDIX TABLE XIII

Associations Between Place Effects and Area-Level Characteristics Across Commuting Zones for High-Income Families (p = 75)

		Standard Deviation of (1)	Signal Correlation (2)		Regression Estimates: Perm. Res. (3)		Causal (4)		Sorting (5)	
		Std. Dev	Correlation	s.e.	%	s.e.	%	s.e.	%	s.e.
Segregation and Poverty	Fraction Black Residents	0.100	-0.01	0.20	-1.06	0.71	-0.02	0.90	-1.04	0.54
	Poverty Rate	0.041	-0.06	0.21	-1.22	0.48	-0.28	0.92	-0.94	0.68
	Racial Segregation Theil Index	0.106	-0.16	0.10	-1.46	0.38	-0.72	0.45	-0.74	0.51
	Income Segregation Theil Index	0.034	-0.56	0.17	-2.81	0.49	-2.46	0.74	-0.35	0.51
	Segregation of Poverty (<p25)	0.029	-0.45	0.15	-2.55	0.43	-2.00	0.65	-0.55	0.50
	Segregation of Affluence (>p75)	0.039	-0.62	0.18	-2.98	0.53	-2.76	0.79	-0.22	0.53
	Share with Commute < 15 Mins	0.091	0.60	0.15	3.26	0.48	2.66	0.66	0.59	0.56
	Log. Population Density	1.341	-0.42	0.14	-2.02	0.54	-1.87	0.62	-0.15	0.46
Income Distribution	Household Income per Capita for Working-Age Adults	6920.4	-0.33	0.16	-1.22	0.41	-1.48	0.72	0.26	0.53
	Gini coefficient for Parent Income	0.082	-0.69	0.23	-3.22	0.98	-3.07	1.00	-0.15	0.63
	Top 1% Income Share for Parents	0.050	-0.51	0.17	-2.14	0.78	-2.27	0.76	0.13	0.45
	Gini within Bottom 99%	0.054	-0.59	0.18	-2.94	0.48	-2.59	0.77	-0.35	0.64
	Fraction Middle Class (Between National p25 and p75)	0.061	0.49	0.18	3.06	0.55	2.15	0.78	0.91	0.67
Tax	Local Tax Rate	0.006	-0.09	0.19	-0.58	0.50	-0.38	0.83	-0.20	0.55
	Local Tax Rate per Capita	0.327	-0.26	0.20	-1.18	0.49	-1.17	0.90	-0.01	0.61
	Local Government Expenditures per Capita	673.6	-0.69	0.25	-2.56	0.76	-3.07	1.09	0.52	0.60
	State EITC Exposure	3.712	0.16	0.13	1.41	0.50	0.71	0.59	0.69	0.38
	State Income Tax Progressivity	2.341	-0.42	0.33	-1.54	1.16	-1.84	1.46	0.30	0.46
K-12 Education	School Expenditure per Student ($1000)	1.312	0.03	0.19	0.31	0.67	0.14	0.83	0.17	0.46
	Student/Teacher Ratio	2.665	-0.73	0.19	-3.29	0.38	-3.21	0.84	-0.08	0.66
	Test Score Percentile (Controlling for Parent Income)	7.157	0.69	0.20	3.40	0.39	3.05	0.91	0.36	0.79
	High School Dropout Rate (Controlling for Parent Income)	0.016	-0.20	0.15	-1.77	0.56	-0.87	0.68	-0.90	0.60
College	Number of Colleges per Capita	0.006	0.46	0.22	2.35	0.58	2.02	0.97	0.33	0.74
	Mean College Tuition	3311.9	0.13	0.17	0.74	0.53	0.56	0.75	0.18	0.50
	College Graduation Rate (Controlling for Parent Income)	0.104	-0.03	0.15	0.56	0.57	-0.11	0.66	0.67	0.60
Local Labor Market	Labor Force Participation Rate	0.047	-0.04	0.21	0.57	0.55	-0.16	0.94	0.73	0.73
	Fraction Working in Manufacturing	0.061	0.36	0.17	1.47	0.48	1.57	0.76	-0.11	0.63
	Growth in Chinese Imports 1990-2000	0.979	0.01	0.14	0.55	0.34	0.05	0.61	0.50	0.50
	Teenage (14-16) Labor Force Participation Rate	0.100	0.48	0.25	3.04	0.63	2.10	1.12	0.94	0.72
Migration	Migration Inflow Rate	0.011	-0.53	0.15	-1.25	0.42	-2.34	0.65	1.09	0.56
	Migration Outflow Rate	0.007	-0.51	0.16	-1.92	0.45	-2.27	0.70	0.36	0.66
	Fraction of Foreign Born Residents	0.100	-0.86	0.18	-3.21	0.66	-3.79	0.80	0.59	0.48
Social Capital	Social Capital Index (Rupasingha and Goetz 2008)	0.926	0.66	0.20	3.26	0.51	2.93	0.90	0.33	0.71
	Fraction Religious	0.105	0.25	0.15	2.52	0.43	1.10	0.66	1.43	0.55
	Violent Crime Rate	0.001	-0.78	0.20	-2.74	0.60	-3.45	0.88	0.71	0.53
Family Structure	Fraction of Children with Single Mothers	0.036	-0.11	0.18	-1.67	0.69	-0.47	0.81	-1.20	0.51
	Fraction of Adults Divorced	0.015	0.11	0.19	-1.02	0.61	0.47	0.86	-1.49	0.60
	Fraction of Adults Married	0.033	0.48	0.18	2.85	0.64	2.12	0.80	0.73	0.55
Prices	Mean House Prices for High-Income (p = 75) families	66500.0	-0.67	0.12	-2.45	0.45	-2.96	0.52	0.51	0.44
	Mean Monthly Rents for High-Income (p = 75) families	264.368	-0.79	0.18	-3.20	0.52	-3.51	0.78	0.31	0.56

Notes: This table replicates Online Appendix Table XI for high-income families (p = 75); see notes to Online Appendix Table XI for details.

ONLINE APPENDIX TABLE XIV

Associations Between Place Effects and Area-Level Characteristics Across Counties within CZs for High-Income Families (p = 75)

		Standard Deviation of (1)	Signal Correlation (2)		Regression Estimates: Perm. Res. (3)		Causal (4)		Sorting (5)	
		Std. Dev	Correlation	s.e.	%	s.e.	%	s.e.	%	s.e.
Segregation and Poverty	Fraction Black Residents	0.130	0.14	0.14	-2.82	0.21	0.63	0.64	-3.46	0.71
	Poverty Rate	0.055	-0.02	0.16	-2.38	0.23	-0.09	0.76	-2.29	0.71
	Racial Segregation Theil Index	0.118	0.14	0.09	-2.75	0.25	0.64	0.44	-3.40	0.46
	Income Segregation Theil Index	0.039	-0.06	0.11	-2.58	0.16	-0.25	0.50	-2.32	0.49
	Segregation of Poverty (<p25)	0.034	-0.08	0.12	-2.64	0.17	-0.38	0.53	-2.26	0.53
	Segregation of Affluence (>p75)	0.045	-0.04	0.10	-2.34	0.18	-0.18	0.48	-2.16	0.47
	Share with Commute < 15 Mins	0.100	0.08	0.26	0.50	0.37	0.37	1.21	0.13	1.25
	Log. Population Density	1.698	-0.04	0.12	-3.01	0.33	-0.20	0.57	-2.81	0.55
Income Distribution	Household Income per Capita for Working-Age Adults	9220.2	-0.03	0.10	0.48	0.24	-0.12	0.45	0.59	0.48
	Gini coefficient for Parent Income	0.113	-0.06	0.13	-2.98	0.36	-0.30	0.62	-2.68	0.87
	Top 1% Income Share for Parents	0.001	-0.01	0.15	-1.94	0.36	-0.04	0.67	-1.90	0.92
	Gini within Bottom 99%	0.112	-0.06	0.13	-2.98	0.36	-0.30	0.62	-2.69	0.87
	Fraction Middle Class (Between National p25 and p75)	0.074	-0.14	0.14	1.35	0.30	-0.63	0.66	1.98	0.67
Tax	Local Tax Rate	0.009	-0.01	0.14	-1.16	1.06	-0.03	0.66	-1.13	1.28
	Local Tax Rate per Capita	0.432	-0.02	0.11	-0.75	1.02	-0.10	0.49	-0.65	1.14
	Local Government Expenditures per Capita	1.0	-0.18	0.10	-1.47	0.85	-0.85	0.48	-0.62	0.94
	State EITC Exposure	3.747	0.01	0.15	-0.11	0.07	0.07	0.70	-0.18	0.69
	State Income Tax Progressivity	2.368	-0.15	0.10	-0.26	0.23	-0.67	0.47	0.41	0.52
K-12 Education	School Expenditure per Student ($1000)	1.478	0.05	0.17	-0.55	0.86	0.23	0.77	-0.78	0.77
	Student/Teacher Ratio	2.807	-0.21	0.16	-1.04	0.55	-0.95	0.75	-0.09	0.67
	Test Score Percentile (Controlling for Parent Income)	9.593	0.03	0.12	2.12	0.25	0.15	0.55	1.98	0.69
	High School Dropout Rate (Controlling for Parent Income)	0.023	0.15	0.17	-2.22	0.26	0.68	0.80	-2.90	0.84
College	Number of Colleges per Capita	0.011	-0.43	0.43	-1.44	0.22	-2.00	2.00	0.56	2.00
	Mean College Tuition	4421.3	-0.15	0.14	-0.67	0.37	-0.71	0.63	0.04	0.73
	College Graduation Rate (Controlling for Parent Income)	0.139	-0.08	0.15	-1.00	0.25	-0.36	0.69	-0.65	0.66
Local Labor Market	Labor Force Participation Rate	0.058	-0.14	0.15	0.47	0.38	-0.63	0.70	1.10	0.75
	Fraction Working in Manufacturing	0.070	0.15	0.17	1.90	0.17	0.71	0.80	1.18	0.83
	Teenage (14-16) Labor Force Participation Rate	0.108	0.01	0.19	1.02	0.34	0.06	0.90	0.96	0.91
Migration	Migration Inflow Rate	0.019	-0.31	0.12	1.05	0.22	-1.41	0.57	2.46	0.52
	Migration Outflow Rate	0.014	-0.16	0.14	-0.54	0.44	-0.75	0.65	0.21	0.62
	Fraction of Foreign Born Residents	0.110	0.19	0.09	-1.52	0.26	0.89	0.41	-2.40	0.46
Social Capital	Social Capital Index (Rupasingha and Goetz 2008)	1.089	0.00	0.16	-0.31	0.36	0.01	0.74	-0.33	0.86
	Fraction Religious	0.127	-0.11	0.15	-0.01	0.32	-0.49	0.71	0.48	0.74
	Violent Crime Rate	0.002	0.06	0.15	-1.99	0.31	0.27	0.67	-2.26	0.66
Family Structure	Fraction of Children with Single Mothers	0.069	-0.07	0.14	-3.20	0.19	-0.34	0.63	-2.86	0.63
	Fraction of Adults Divorced	0.017	-0.12	0.16	-1.93	0.33	-0.57	0.74	-1.36	0.69
	Fraction of Adults Married	0.062	0.16	0.17	3.41	0.21	0.75	0.79	2.66	0.74
Prices	Mean House Prices for High-Income (p = 75) families	78006.5	0.02	0.14	0.73	0.41	0.11	0.65	0.62	0.51
	Mean Monthly Rents for High-Income (p = 75) families	283.212	-0.04	0.11	-0.26	0.46	-0.18	0.51	-0.08	0.55

Notes: This table replicates Online Appendix Table XII for high-income families (p = 75); see notes to Online Appendix Tables XI and XII for details.

ONLINE APPENDIX TABLE XV
Commuting Zone and County Characteristics: Definitions and Data Sources

	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

ONLINE APPENDIX TABLE XV, Continued
Commuting Zone and County Characteristics: Definitions and Data Sources

	Variable (1)	Definition (2)	Source (3)
Price	Monthly Rent for Below-Median Income Families	Median "Contract Rent" (monthly) for the universe of renter-occupied housing units paying cash rent at the tract level. Aggregated to the county/CZ level by taking the mean, weighting by the population with below-median incomes for families with children < 18 present (income bins 9 and below)	2000 Census SF3a (NHGIS SF3a, code: GBO) [Rent] 2000 Census SF3a (NHGIS SF3a, code: GYI) [for weights]
	Monthly Rent for Above-Median Income Families	Median "Contract Rent" (monthly) for the universe of renter-occupied housing units paying cash rent at the tract level. Aggregated to the county/CZ level by taking the mean, weighting by the population with above-median incomes for families with children < 18 present (income bins 10 and above)	2000 Census SF3a (NHGIS SF3a, code: GBO) [Rent] 2000 Census SF3a (NHGIS SF3a, code: GYI) [for weights]
	House Price for Below-Median Income Families	Median value of housing units at the tract level. Aggregated to the county/CZ level by taking the mean, weighting by the population with below-median incomes for families with children < 18 present (income bins 9 and below)	2000 Census SF3a (NHGIS SF3a, code: GB7) 2000 Census SF3a (NHGIS SF3a, code: GYI) [for weights]
	House Price for Above-Median Income Families	Median value of housing units at the tract level. Aggregated to the county/CZ level by taking the mean, weighting by the population with above-median incomes for families with children < 18 present (income bins 10 and above)	2000 Census SF3a (NHGIS SF3a, code: GB7) 2000 Census SF3a (NHGIS SF3a, code: GYI) [for weights]

Notes: This table provides a description of each variable used in Section X and reported in Appendix Tables XI to XIV 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.

ONLINE APPENDIX TABLE XVI

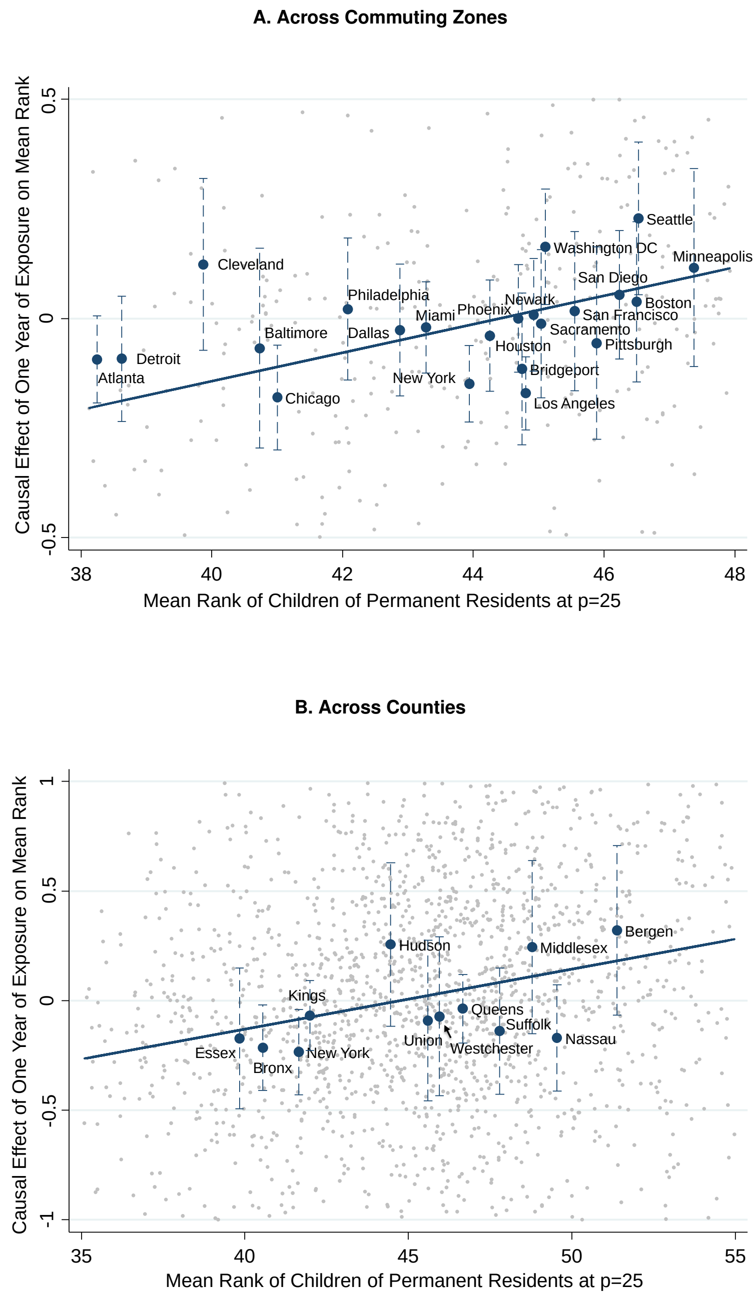
Association Between Rents and Causal Effects on Children's Incomes for High-Income Families

	Dep. Var.: Mean Monthly Rent For High-Income Families ($)				
	CZs	Counties within CZs			
		All Counties	Counties in 100 Largest CZs	Observable Component	Unobservable Component
	(1)	(2)	(3)	(4)	(5)
Causal Effect (1% Increase in Child's Income)	-574.0***	102.9	109.3		
	(158.2)	(101.7)	(104.2)		
Observable Component				409.1***	
				(64.25)	
Unobservable Component					35.99
					(67.47)
Mean of Dependent Variable	948.17	931.63	1038.02	1030.23	1030.23
Signal R-Squared	0.623	0.000	0.000	0.066	0.007
Number of Observations	595	2367	694	673	673

Notes : This table replicates Table V for high-income (p = 75) families; see notes to Table V for details.

FIGURE I

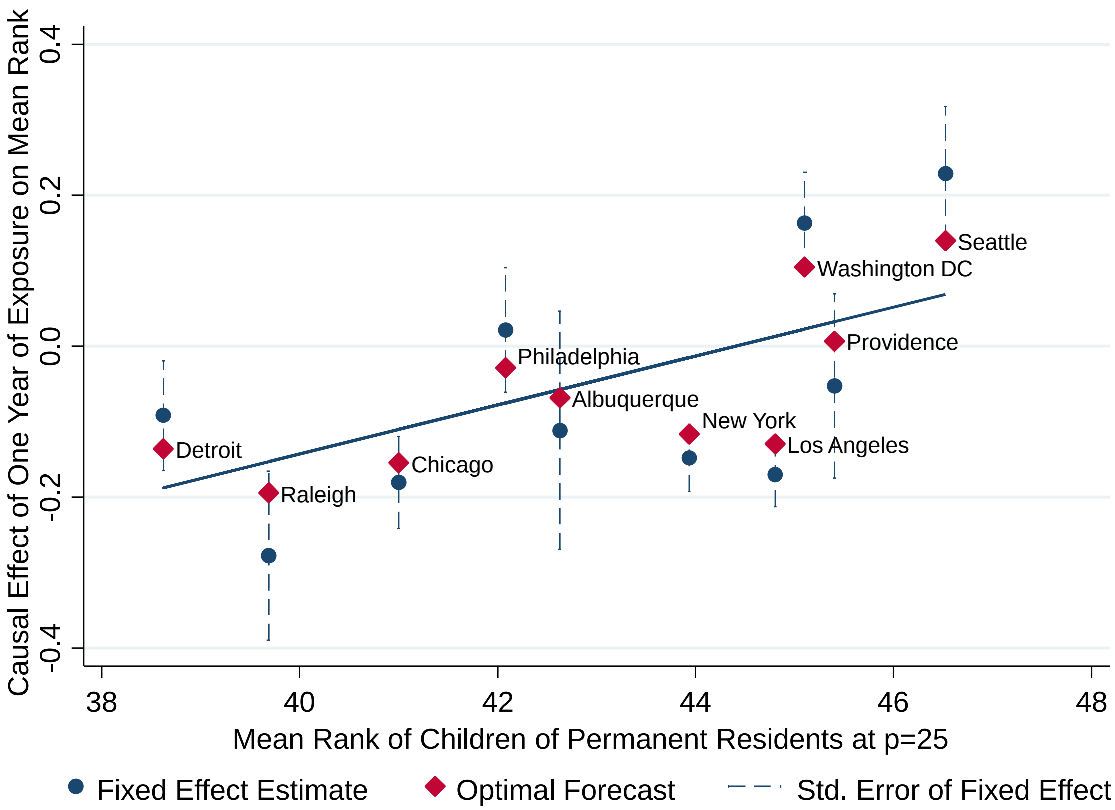
Causal Effect Estimates vs. Permanent Residents' Outcomes for Low-Income Families



Notes: Panel A plots causal effects of childhood exposure to each CZ, estimated using movers' outcomes, vs. permanent residents' outcomes. The vertical axis shows estimates of $\hat{\mu}_{25,c}$, the causal effect of an additional year of childhood exposure to a CZ (relative to the average CZ) on the mean percentile rank at age 26 for children in families at income percentile $p = 25$. The horizontal axis plots $\bar{y}_{25,c}$, the mean ranks of children of permanent residents (non-movers) at $p = 25$. CZs with populations above 2.5 million (based on the 2000 Census) are labeled. Dashed vertical bars show 95% confidence intervals for $\hat{\mu}_{25,c}$. The solid line shows the conditional expectation of $\hat{\mu}_{25,c}$ given $\bar{y}_{pc}$, estimated using an OLS regression pooling all CZs and weighting by the precision of $\hat{\mu}_{25,c}$. Panel B replicates Panel A at the county level. Counties within the New York and Newark CZs that have populations above 500,000 are labeled. The sample in both figures consists of all children in the 1980-86 birth cohorts who are U.S. citizens; see Section II for further details.

FIGURE II

Construction of Mean-Squared-Error Minimizing Forecasts
for Children in Low-Income Families

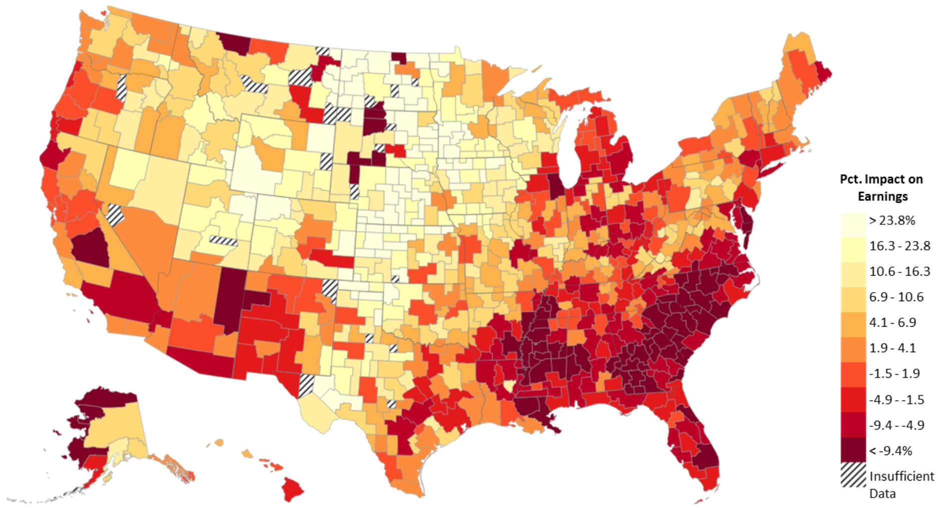


Notes: This figure illustrates the construction of the MSE-minimizing forecasts of causal effects, μ_{pc}^{f}, for a selected set of CZs. The forecasts shown are for children with parents at income percentile $p = 25$. The circles plot the raw fixed effect estimates $\hat{\mu}_{25,c}$ (per year of childhood exposure) vs. the mean ranks of children of permanent residents, as in Figure Ia. The dashed vertical lines around these points represent ± 1 standard error of $\hat{\mu}_{25,c}$. The solid regression line shows $E\left[\mu_{pc}|\bar{y}_{pc}\right]$, the predicted causal effect of each CZ given the outcomes of its permanent residents, which is estimated from a population-weighted OLS regression of $\hat{\mu}_{25,c}$ on $\bar{y}_{pc}$ using data for all CZs. The diamonds show the MSE-minimizing forecast, which is a weighted average of $\hat{\mu}_{25,c}$ (the circle) and $E\left[\mu_{pc}|\bar{y}_{pc}\right]$ (the prediction from the regression line), with greater weight placed on $\hat{\mu}_{25,c}$ when the standard error of $\hat{\mu}_{25,c}$ is smaller.

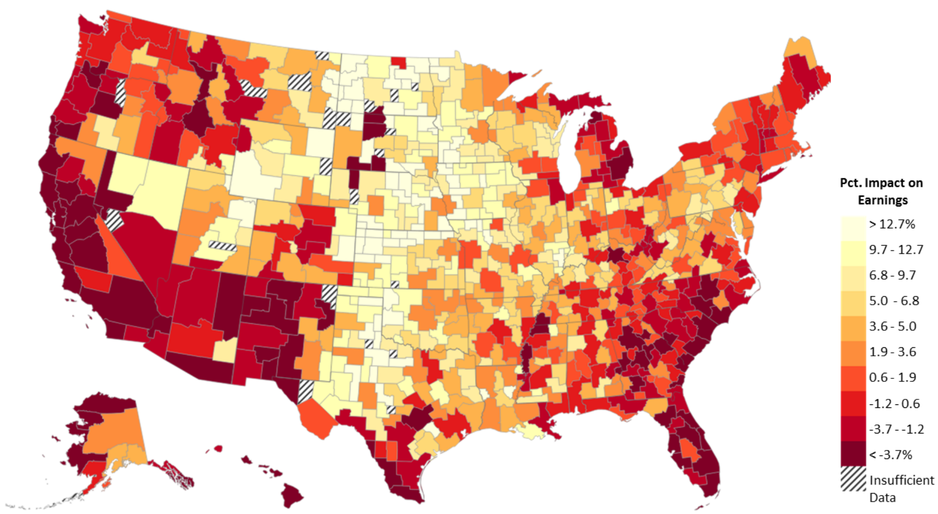
FIGURE III

Forecasts of Causal Effects on Children's Income by Commuting Zone

A. For Children with Parents at 25th Percentile ($\mu^f_{25,c}$)



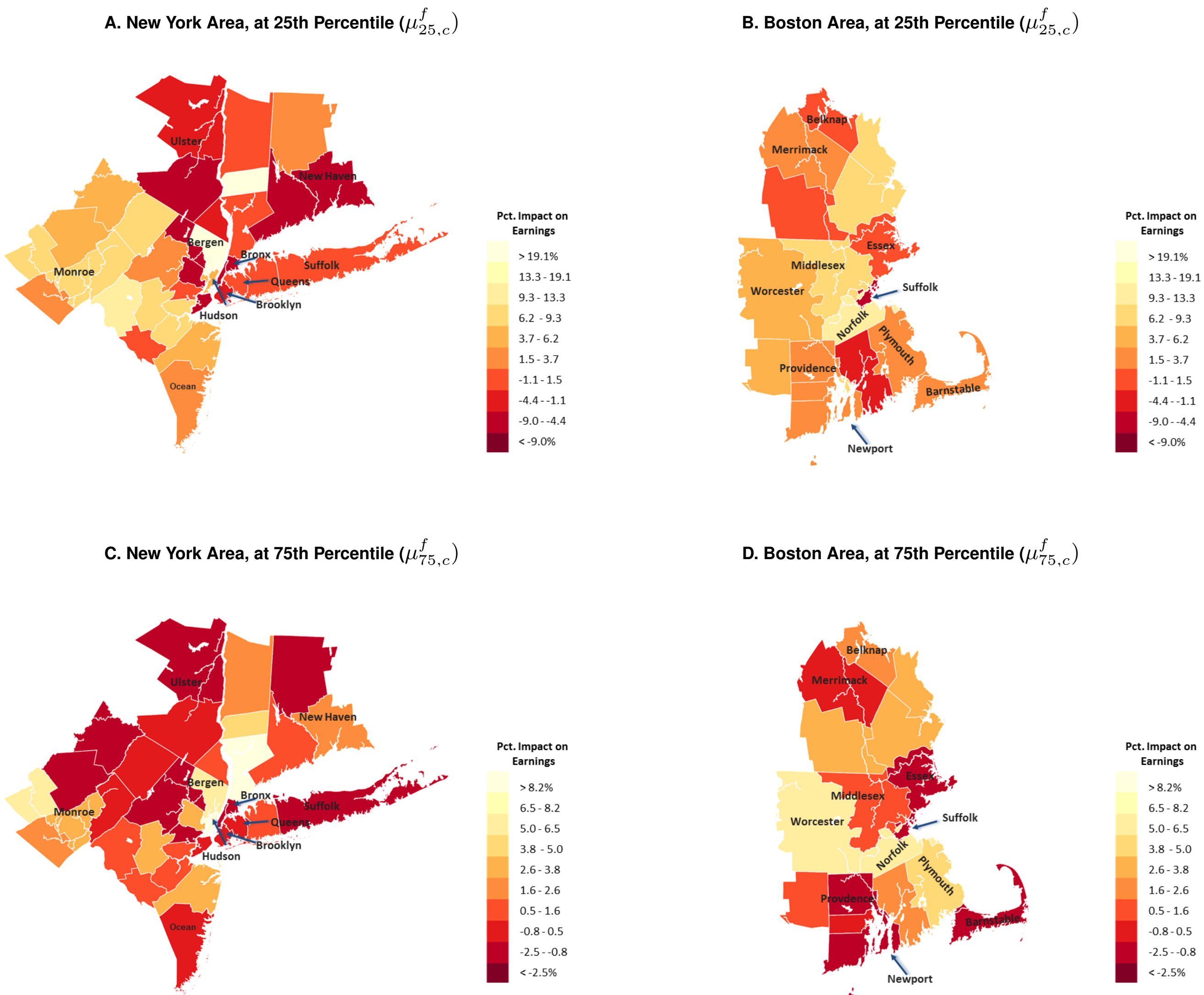
B. For Children with Parents at 75th Percentile ($\mu^f_{75,c}$)



Notes: These maps show MSE-minimizing forecasts of each commuting zone's causal effect, μ^f_{pc}, on children's household income at age 26. Panel A shows estimates for children in below-median income families ($p = 25$), while Panel B shows estimates for children in above-median income families ($p = 75$). These forecasts are constructed using the methodology described in the notes to Figure II. Estimates are scaled to show the percentage change in income from growing up from birth (i.e., 20 years of childhood exposure) in a given CZ relative to the (population-weighted) average CZ in the country. CZs are grouped into (unweighted) deciles based on their causal effects, with lighter colors depicting areas with more positive causal effects. For example, growing up in CZs in the highest decile raises children's incomes at $p = 25$ by more than 23.8% relative to the average CZ, while growing up in the CZs in the lowest decile reduces their incomes by 9.4% relative to the average CZ. CZs with fewer than 250 permanent residents, for which we do not report permanent resident outcomes and therefore do not have forecasts of causal effects, are shaded with the striped pattern.

FIGURE IV

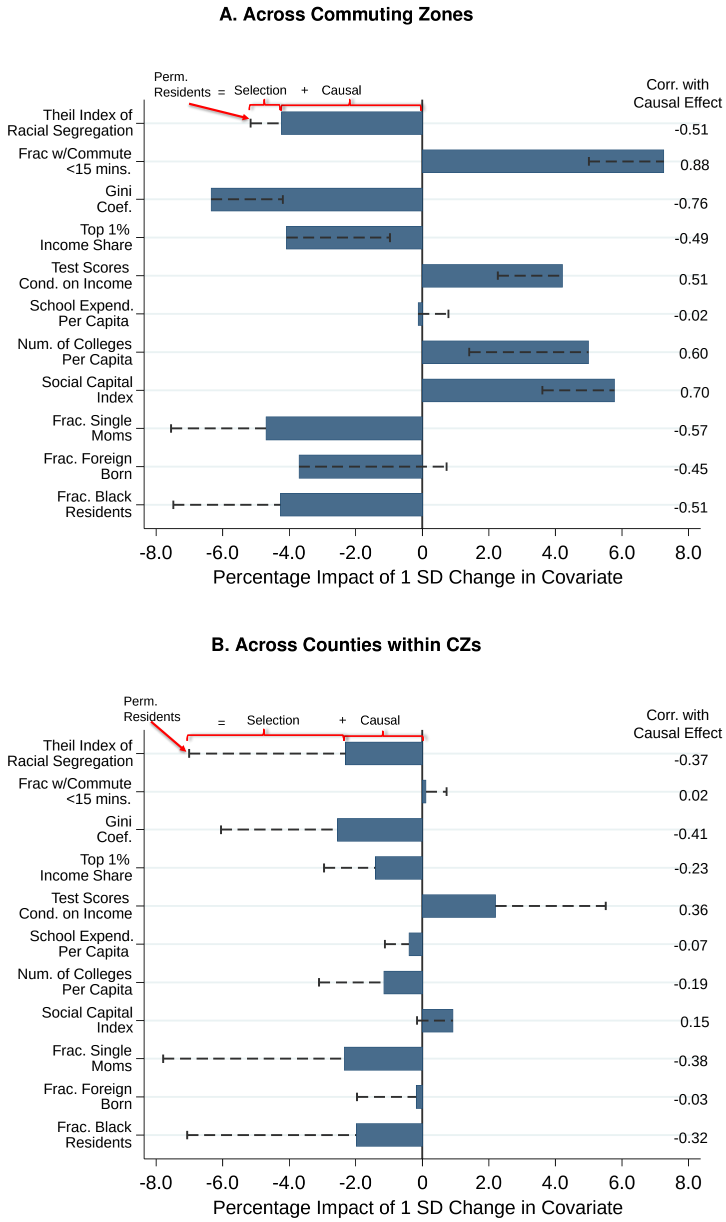
Forecasts of Causal Effects by County in the New York and Boston Areas



Notes: These maps show MSE-minimizing forecasts of each county's causal effect, μ^f_{pc}, on children's household income at age 26 for counties in the New York and Boston Combined Statistical Areas. Panels A and B show estimates for children in below-median income families ($p = 25$), while Panels C and D show estimates for children in above-median income families ($p = 75$). These county-level forecasts are constructed using an approach analogous to that described in the notes to Figure II. Estimates are scaled to show the percentage change in income from growing up from birth (i.e., 20 years of childhood exposure) in a given county relative to the (population-weighted) average county in the country. Counties are grouped into deciles at the national level based on their causal effects, with lighter colors depicting areas with more positive causal effects.

FIGURE V

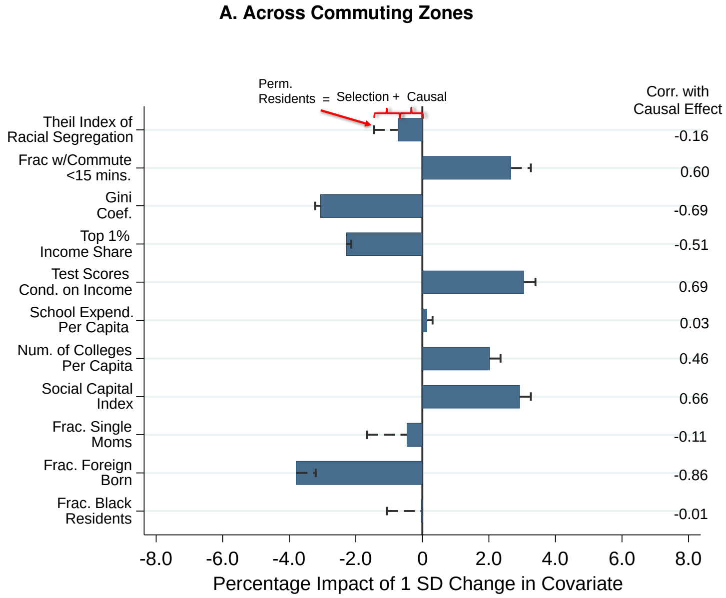
Predictors of Place Effects For Children with Parents at 25^{th} Percentile

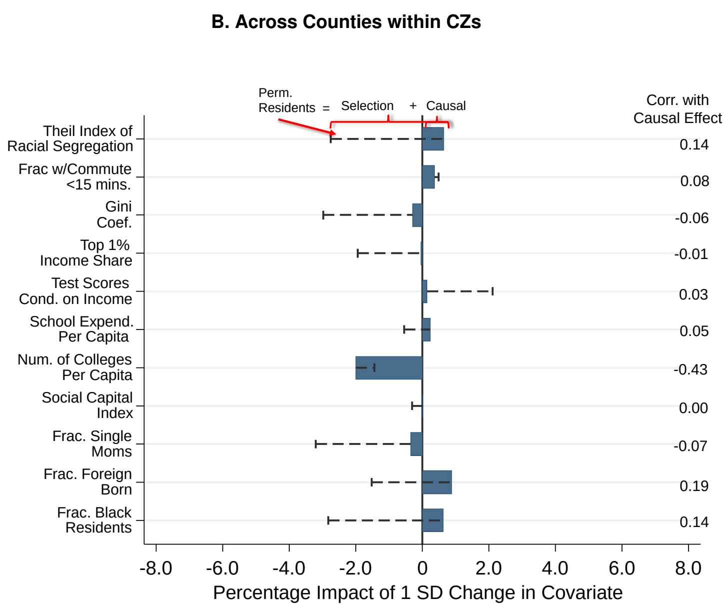


Notes: Panel A plots coefficients from univariate OLS regressions of permanent resident outcomes $\bar{y}_{25,c}$ and causal effects $\hat{\mu}_{25,c}$ for below-median income families ($p = 25$) on various CZ-level characteristics (x_c), weighting by population. The characteristics are normalized to have a (population-weighted) mean zero and unit standard deviation across CZs. Both $\bar{y}_{25,c}$ and $\hat{\mu}_{25,c}$ are rescaled so the coefficients can be interpreted as impacts in percentage units using the approach described in Section V.B. The vertical tick marks plot coefficients from regressions of $\bar{y}_{25,c}$ on x_c. The solid bars plot coefficients from regressions of the causal effect of growing up in an area from birth (20 years of exposure), $20\hat{\mu}_{25,c}$, on x_c. The difference between the tick mark and the bar (depicted by the dashed horizontal line) therefore represents the coefficient from a regression of $\bar{y}_{25,c} - 20\hat{\mu}_{25,c}$ on the covariate x_c, which can be interpreted as the association between selection effects and the covariate. The numbers on the right report the correlations between $\mu_{25,c}$ and x_c, which are obtained by dividing the coefficient from regressing $20\hat{\mu}_{25,c}$ on x_c by 20 times the standard deviation of $\mu_{25,c}$ (from Table II). Panel B presents analogous estimates at the county-within-CZ level, standardizing the covariates to have (population-weighted) mean zero and unit standard deviation across counties within CZs, and estimating regressions at the county level controlling for CZ fixed effects. Point estimates and standard errors for the characteristics shown in this figure are provided in Online Appendix Tables XI and XII, along with results for additional characteristics. Definitions of the covariates are provided in Online Appendix Table XV.

FIGURE VI

Predictors of Place Effects For Children with Parents at 75^{th} Percentile

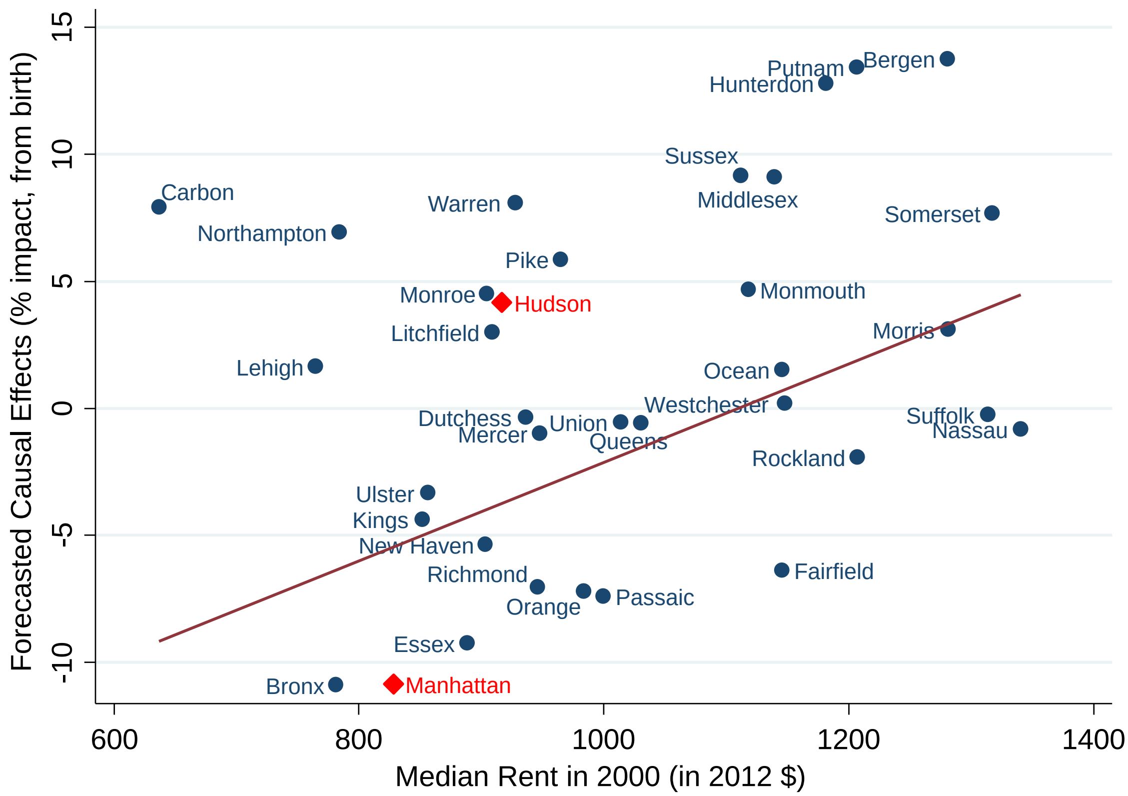




Notes: This figure replicates Figure V using children in above-median income families ($p = 75$) instead of below-median income families ($p = 25$); see notes to Figure V for details.

FIGURE VII

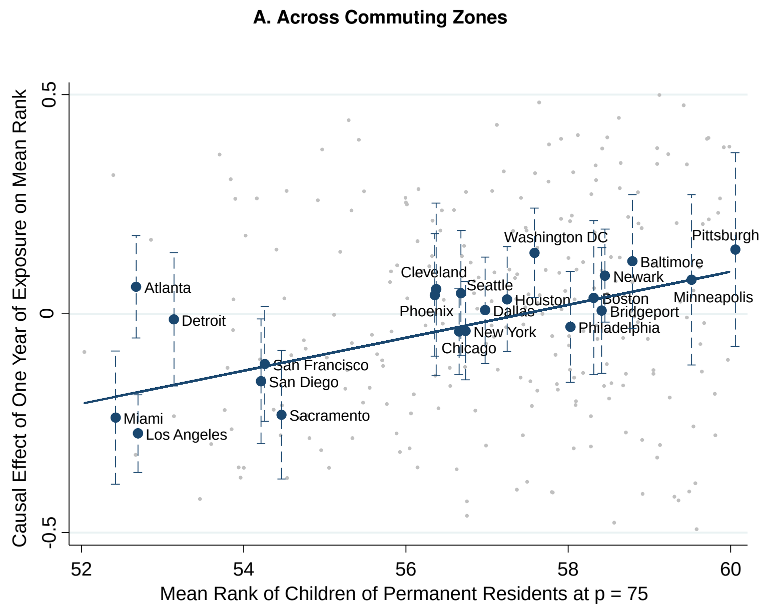
Opportunity Bargains in the New York Area

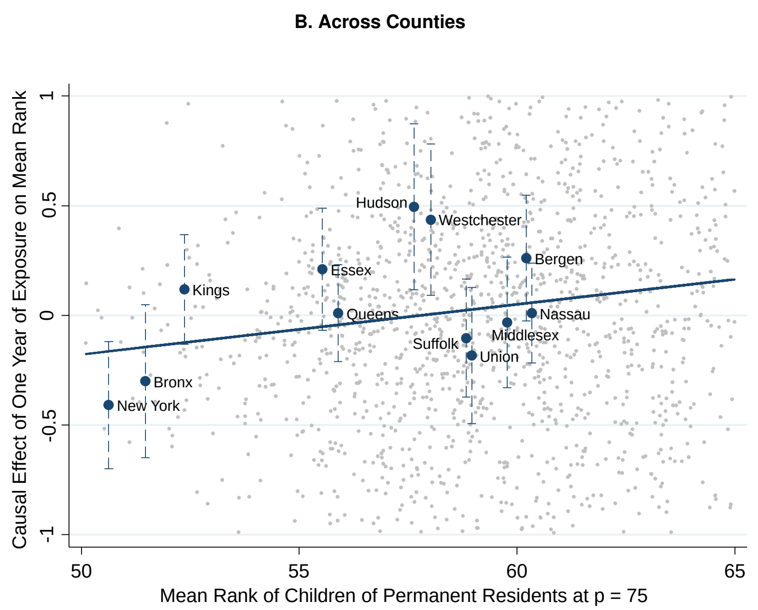


Notes: This figure plots the MSE-minimizing forecasts of causal effects, $\mu^f_{25,c}$, for counties in the New York Combined Statistical Area (shown in Figure IVa) vs. average monthly rents for low-income families. The forecasted causal effects $\mu^f_{25,c}$ are scaled to show the percentage change in income from growing up from birth (i.e., 20 years of childhood exposure) in a given county relative to the average county in the country. See notes to Figure IVa for further details on construction of $\mu^f_{25,c}$. Monthly rents are measured using data from the 2000 Census (in 2012 dollars), and are defined as the mean of median rents across Census tracts, weighting by the number of families with children who have below-median income. The solid line shows the best-fit line obtained from regressing $\mu^f_{25,c}$ on rents, weighting by county population. Points above the line are "opportunity bargains" – counties that produce particularly good outcomes for children relative to other counties with comparable rents.

ONLINE APPENDIX FIGURE I

Causal Effect Estimates vs. Permanent Residents' Outcomes for High-Income Families



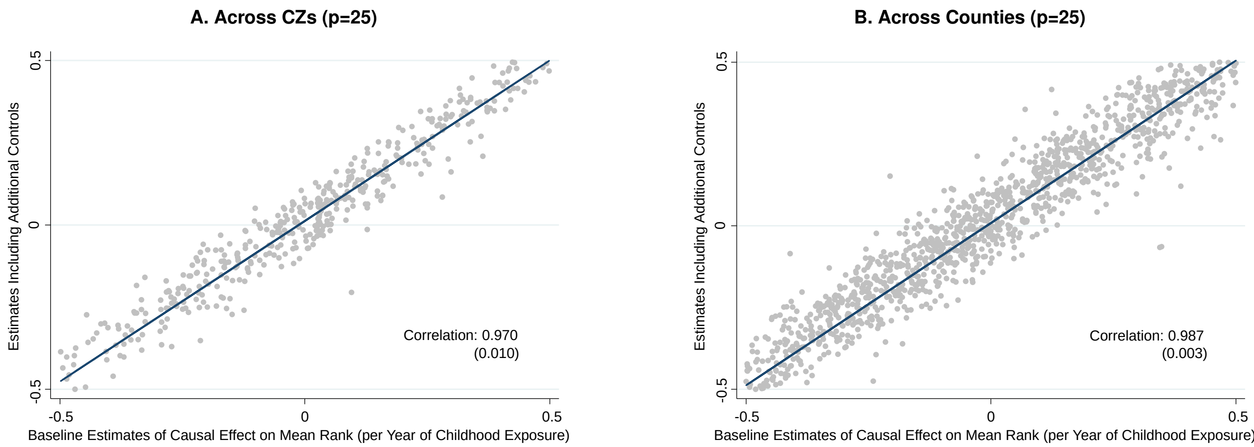


Notes: This figures replicates Figure I using children in above-median income families ($p = 75$) instead of below-median income families ($p = 25$); see notes to Figure I for details.

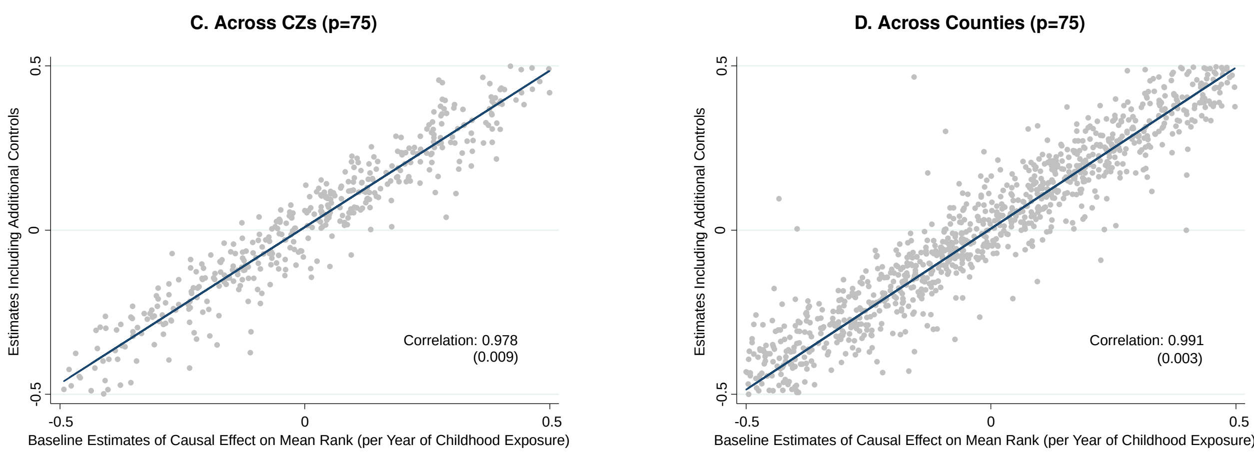
ONLINE APPENDIX FIGURE II

Sensitivity of Causal Effect Estimates to Controls

A. Across Commuting Zones



B. Across Counties

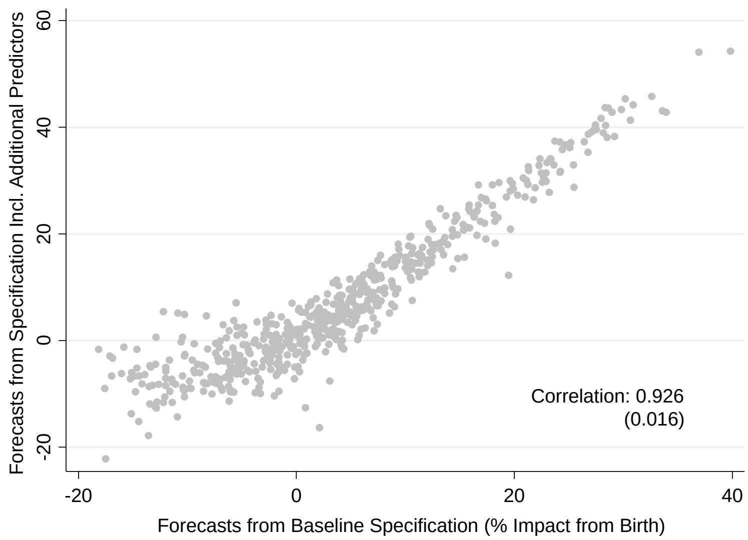


Notes: These figures present scatterplots of the relationship between our baseline causal effect estimates $\hat{\mu}_{25,c}$ and estimates using specifications that include controls for changes in parent income and marital status around the time of the move interacted with the child's age at move (see Section IV.B for details). Panel A plots CZ-level estimates for children with below-median income parents ($p = 25$). Panel B replicates Panel A at the county level. Panels C and D replicate Panels A and B for above-median income families ($p = 75$). In each figure, the horizontal axis shows estimates from the baseline specification and the vertical axis shows estimates using the specifications with additional controls. The figures also report the precision-weighted correlation between the two sets of estimates, with standard errors in parentheses.

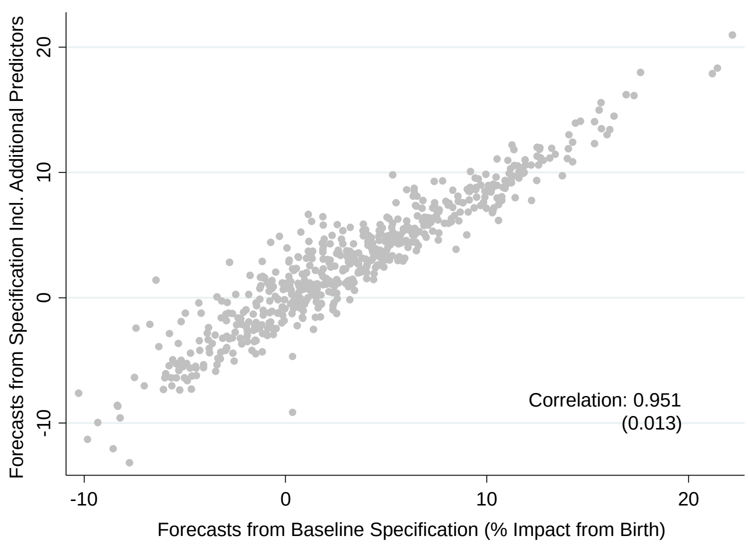
ONLINE APPENDIX FIGURE III

Forecasts using Additional Covariates vs. Baseline Forecasts

A. Low-Income Families ($p = 25$)



B. High-Income Families ($p = 75$)

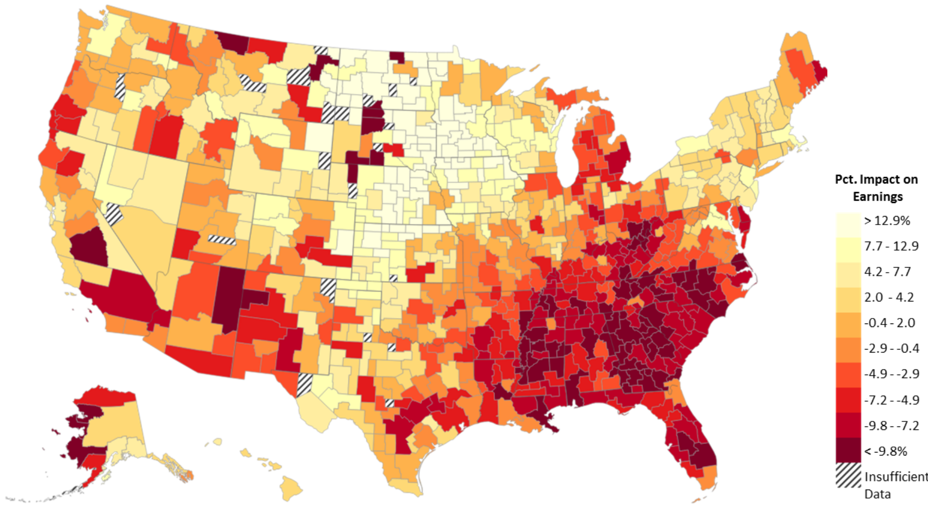


Notes: This figure presents a scatter plot of alternative forecasts of CZs' causal effects on children's income ranks vs. our baseline forecasts μ_{pc}^{f} (shown in Figure III). The baseline forecasts only use data on movers' and permanent residents' outcomes. The alternative forecasts also use the following additional covariates: the fraction of African American residents, the Theil index of racial segregation, the Gini index, the fraction of single parents, the social capital index, and expenditures on public schools per student. These characteristics are incorporated using the methodology described in Appendix C. Panel A shows estimates for children in below-median income families ($p = 25$), while Panel B shows estimates for children in above-median income families ($p = 75$). Each panel also reports the (population-weighted) correlation between the baseline and alternative forecasts, with standard errors in parentheses. See notes to Figure III for additional details on construction of these forecasts.

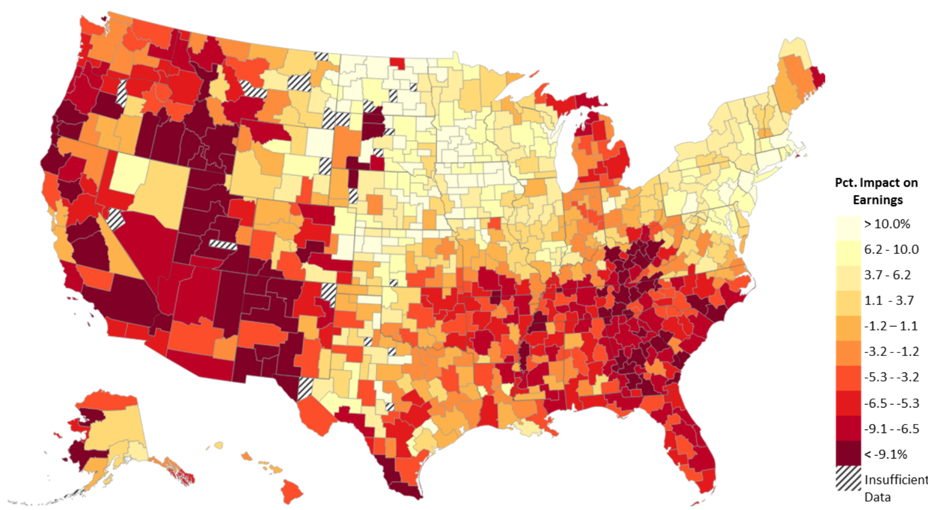
ONLINE APPENDIX FIGURE IV

Forecasts of CZs' Causal Effects on Individual Income

A. Low-Income Families ($\mu^{f}_{25,c}$)



B. High-Income Families ($\mu^{f}_{75,c}$)

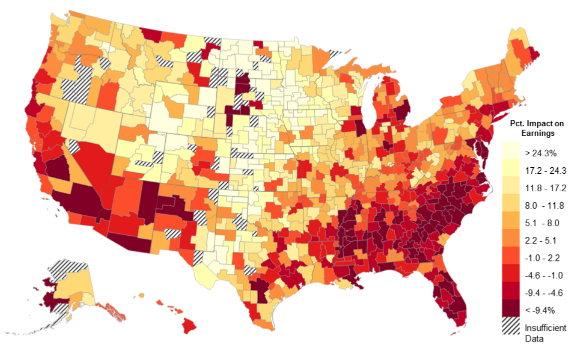


Notes: This figure replicates Figure III, measuring children's income at the individual rather than family level (i.e., excluding spousal income). See notes to Figure III for details.

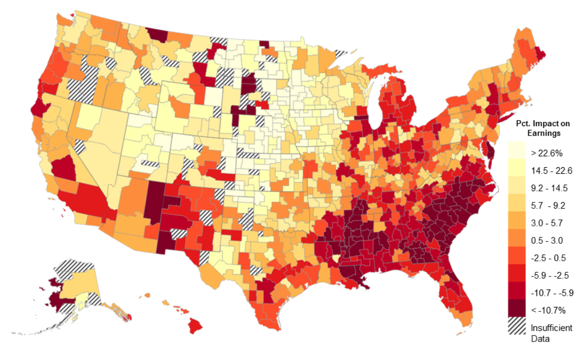
ONLINE APPENDIX FIGURE V

Forecasts of CZs' Causal Effects on Household Income by Child's Gender

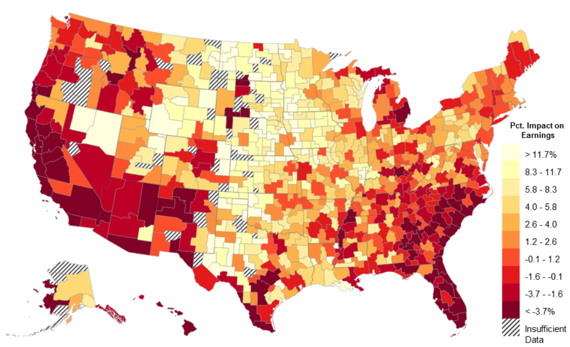
A. Sons in Low-Income Families ($\mu^f_{25,c}$)



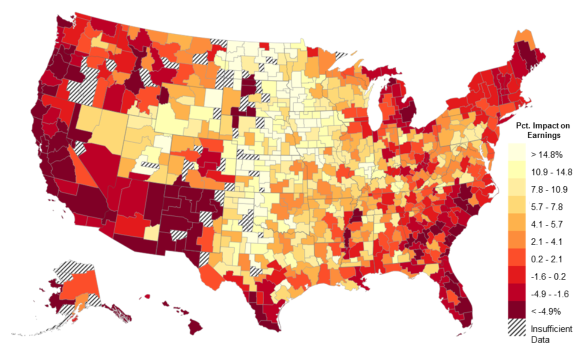
B. Daughters in Low-Income Families ($\mu^f_{25,c}$)



C. Sons in High-Income Families ($\mu^f_{75,c}$)



D. Daughters in High-Income Families ($\mu^f_{75,c}$)

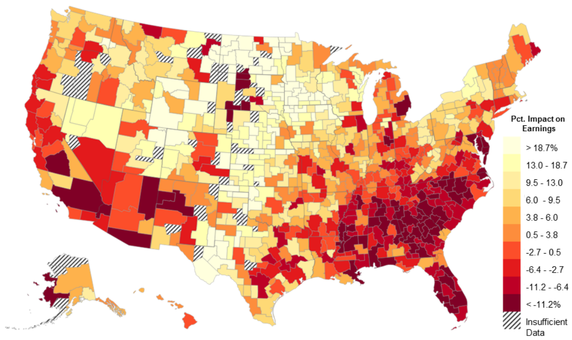


Notes: This figure replicates Figure III, measuring income at the household (i.e., family) level and reporting separate estimates for sons and daughters. See notes to Figure III for details.

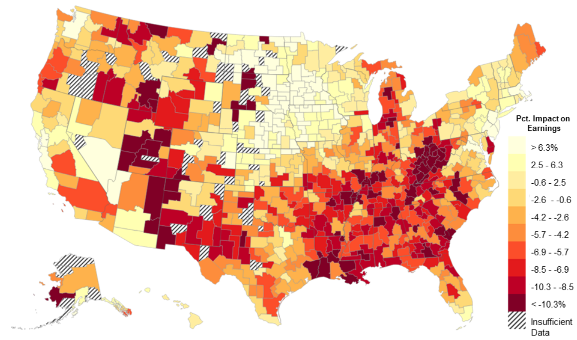
ONLINE APPENDIX FIGURE VI

Forecasts of CZs' Causal Effects on Individual Income by Child's Gender

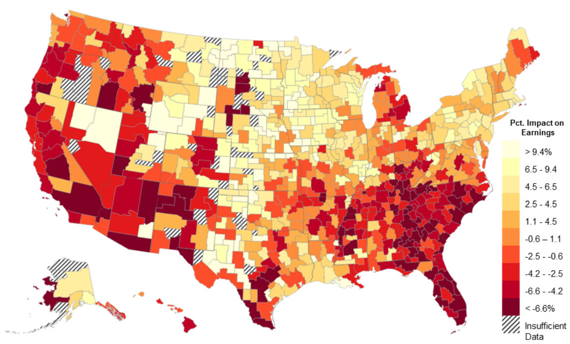
A. Sons in Low-Income Families ($p = 25$)



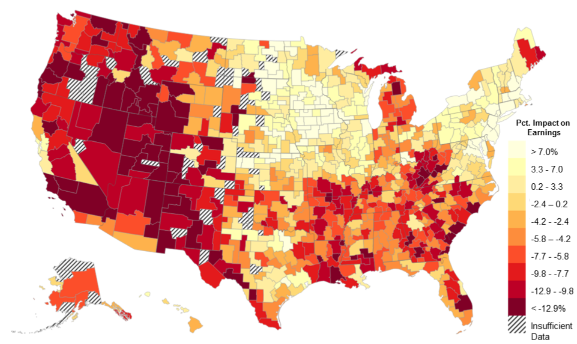
B. Daughters in Low-Income Families ($p = 25$)



C. Sons in High-Income Families ($p = 75$)



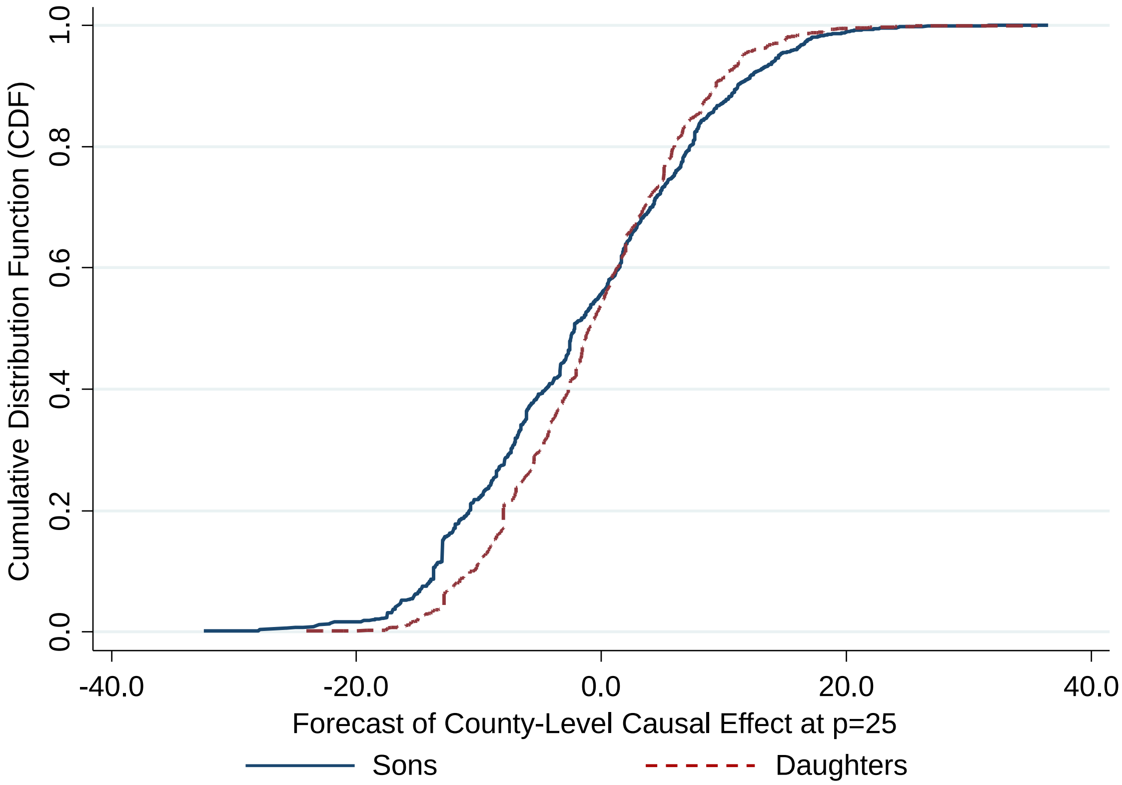
C. Daughters in High-Income Families ($p = 75$)



Notes: This figure replicates Appendix Figure V, measuring income at the individual level and reporting separate estimates for sons and daughters. See notes to Figure III for details.

ONLINE APPENDIX FIGURE VII

Distribution of Counties' Causal Effects by Gender



Notes: This figure presents the cumulative distribution function (CDF) of our county-level forecasts of causal effects $\mu^f_{25,c}$ for sons (solid line) and daughters (dashed line) in low-income families ($p = 25$). The causal effects $\mu^f_{25,c}$ are scaled to show the percentage change in income from growing up from birth (i.e., 20 years of childhood exposure) in a given county relative to the average county in the country. See notes to Figure IV for details on construction of these forecasts.