

Online Appendics for Where is the Land of Opportunity?
The Geography of Intergenerational Mobility in the United States

Raj Chetty, Harvard University and NBER
Nathaniel Hendren, Harvard University and NBER
Patrick Kline, UC-Berkeley and NBER
Emmanuel Saez, UC-Berkeley and NBER

June 2014

A. Data Construction

Core and Extended Samples. We begin with the universe of individuals in the Death Master (also known as the Data Master-1) file produced by the Social Security Administration. This file includes information on year of birth and gender for all persons in the United States with a Social Security Number or Individual Taxpayer Identification Number. We restrict this sample to all individuals who are current US citizens as of March 2013. The Data Master-1 file does not contain historical citizenship status, and thus we can only restrict to a sample who are currently US citizens as of the time at which we access the data. We further restrict to individuals who are alive through the end of 2012. The resulting dataset contains 47.8 million children across all cohorts 1980-1991 (Appendix Table I). We measure parent and child income, location, college attendance, and all other variables using data from the IRS Databank, a balanced panel covering all individuals in the United States who appear on any tax form between 1996-2012.

For each child, we define the parent(s) as the first person(s) who claim the child as a dependent on a 1040 tax form. If parents are married but filing separately, we assign the child both parents. To eliminate dependent claiming by siblings or grandparents, in the case of a potential match to married parents or single mothers, we require the mother to be age 15-40 at the birth of the child.¹ In the case of a match to a single father, we require the father to be age 15-40 at the birth of the child. If no such eligible match occurs in 1996, the first year of our data, we search subsequent years (through 2011) until a valid match is found.

Once we match a child to parent(s), we hold this definition of parents fixed regardless of subsequent dependent claims or changes in marital status. For example, a child matched to married parents in 1996 who divorce in 1997 will always be matched to the two original parents. Conversely, a child matched to a single parent in 1996 that marries in 1997 will be considered matched to a single parent, though spouse income will be included in our definition of parent income because we measure parent income at the family level in our baseline analysis.

To reduce the effects of outliers and measurement error in the upper tail of the income distribution, we use data from the IRS Statistics of Income (SOI) manually perfected cross-sectional files spanning 1996-2011 (see below for details on these files). If an individual's adjusted gross income exceeds \$10 million, we look for the individual in the SOI sample; if present, we use the SOI measure of adjusted gross income and wage income as reported on a F1040 return. If not, we replace the adjusted gross income with the total wages reported on the filed F1040 contained in the databank. Because the IRS Databank includes tax year 2012 whereas the SOI sample does not, we top code income at \$100 million for all individuals in 2012. These adjustments in the upper tail affect 0.017% of parents in our core sample (or, equivalently, 1.7% of parents in the top 1% of the income distribution).

For most of our analysis, we measure parent income at the household level. For certain robustness checks, we measure individual parent income as follows. For married parents, we define each parent's individual earnings as the sum of wage earnings from form W-2, unemployment benefits from form 1099-G, and Social Security and Disability benefits from form SSA-1099 for that individual. Individual earnings excludes capital and other non-labor income. To incorporate these sources of income, we add half of family non-labor income – defined as total family income minus total family earnings reported on form 1040 – to each parent's individual earnings. We divide non-labor

¹Children can be claimed as a dependent only if they are aged less than 19 at the end of the year (less than 24 if enrolled as a student) or are disabled. A dependent child is a biological child, step child, adopted child, foster child, brother or sister, or a descendant of one of these (for example, a grandchild or nephew). Children must be claimed by their custodial parent, i.e. the parent with whom they live for over half the year. Furthermore, the custodial parent must provide more than 50% of the support to the child. Hence, working children who support themselves for more than 50% cannot be claimed as dependents. See IRS Publication 501 for further details.

earnings equally between spouses because we cannot identify which spouse earns non-labor income from the 1040 tax return. For single parents, individual income is the same as family income.

Statistics of Income Sample. Starting in 1987, the IRS Statistics of Income cross sections – which are stratified random samples of tax returns – contain dependent information, allowing us to link children to parents. We use the 1987-2011 SOI cross-sections to construct a sample of children born in the 1971-1991 birth cohorts and correct for errors in the upper tail of the income distribution as described above. For each SOI cross-section from 1987 to 2007, we first identify all dependent children between the ages of 12 and 16 who are alive at age 30. We then pool all the SOI cross-sections that give us information for a given birth cohort. For example, the 1971 cohort is represented by children claimed at age 16 in 1987, while the 1991 cohort is comprised by children claimed at ages 12-16 in 2003-2007. Using the sampling weights for the SOI cross-sections (see Internal Revenue Service (2013) for details), each cohort-level dataset is representative of the population of children claimed on tax returns between the ages of 12 and 16 in that birth cohort.

Unlike in the population-based samples, we do not limit the SOI sample to children who are currently citizens because citizenship data are not fully populated for birth cohorts prior to 1980 and because we begin from a sample of children claimed by parents rather than the universe of children who currently appear in the population data (which includes later immigrants). In the years where the SOI and population-based samples overlap, we obtain very similar estimates in both samples. The citizenship restriction has a minor impact because the vast majority of children claimed as dependents between the ages of 12-16 are U.S. citizens as adults. We also do not impose any age restrictions on the parents in the SOI sample. In the population-based sample, some children are claimed by different adults across years and the age restriction is useful to discriminate between these potential parents. In the SOI sample, each child can only be linked to the parents who claim him in the cross-section file, so the age restriction would not play such a role. In practice, this restriction has little impact, as the age distribution of parents in the SOI sample is very similar to that in the core sample using the population data.

Children whose parents are sampled in multiple SOI cross-sections appear multiple times in the SOI sample. There are 228,295 unique children in the SOI sample and 523,700 total observations. The SOI sample grows from 4,383 unique children in 1971 to 21,231 unique children in 1991 because we have more cross-sections to link parents to children in more recent cohorts and because the size of the SOI cross-sections has increased over time (Appendix Table II). To be consistent with the core sample definition of parent income, we define parent income as the 5-year average of parent family income from 1996-2000 in the IRS Databank.

We provide additional information on the SOI sample in Chetty et al. (2014). Using sampling weights, we show that the SOI sample represents roughly 85% of children in each birth cohort (based on vital statistics counts) from 1971-1979, the cohorts we use to obtain estimates of intergenerational mobility after age 32 in Figure IIIa. We also show that summary statistics for the SOI sample are very similar to the core sample for the 1980-82 birth cohorts reported in Appendix Table III of this paper. Note that Chetty et al. (2014) compute parent income using the income of the parents in the single year of the parent-child match there, whereas we compute parent income as the five-year average over 1996-2000 here for consistency with results from the population data. Because we restrict to parents with positive income, this leads to a small difference in the SOI sample used across the two papers. For example, we have 4,383 children in the 1971 cohort, compared with 4,331 children in the sample used by Chetty et al. (2014).

Assignment of Children to Commuting Zones. Children are assigned ZIP codes of residence based on their parents' ZIP code on the form 1040 in which the parent is matched to the child. In the few cases where a parent files a F1040 claiming the child but does not report a valid ZIP code, we search information returns (such as W-2 and 1099-G forms) for a valid ZIP code in that year.

We map these ZIP codes to counties based on the 1999 Census [crosswalk](#) between ZIP codes and counties. We then aggregate counties into Commuting Zones using David Dorn’s county-to-CZ [crosswalk](#) (download file E6). The counties in the U.S. Census Bureau crosswalk and in Dorn’s crosswalk are not identical because they correspond to county definitions at different points in time; in particular the U.S. Census Bureau crosswalk includes changes between 1990 and 1999. We make manual adjustments for changes that affected 200 or more people. Using this procedure, we identify the CZ of 38,839 ZIP codes. To track individuals residing in ZIP codes that have been created since 1999, we add an additional 477 ZIP codes not valid in 1999 but appearing in the more up-to-date 2011 HUD-USPS [crosswalk](#). For example, in 2007, Manhattan’s ZIP code 10021 was split into three separate ZIP codes, resulting in the creation of new ZIP codes 10065 and 10075.

Of 9,864,965 children with non-missing ZIP codes in our core sample, 9,778,071 were assigned a childhood CZ using ZIP codes that existed in 1999; an additional 2,718 were assigned a CZ based on a ZIP code that existed in 2011 but not in 1999. For simplicity, we use the same crosswalk for all years of matching ZIP codes to CZs. We have verified that using year-specific crosswalks from ZIP codes to counties has a negligible effect on CZ assignments. All of the crosswalks we constructed are available on our project website.

Some of our specifications require tracking children’s locations into adulthood using the ZIP code where they live as adults when we measure their income (e.g., for cost-of-living adjustments). We define a child’s adult location using the latest non-missing ZIP code. We first search for a ZIP code in their 1040 form in 2012, followed by their information returns in 2012. We then repeat this procedure for 2011 if we do not find a zipcode in 2012. This yields 9,834,975 non-missing child ZIP codes in adulthood. Of these, we match 9,537,283 to a CZ from a ZIP code using the 1999 crosswalk (i.e. this ZIP code was in use in 1999) and an additional 198,317 using the later crosswalk because the ZIP code was created after 1999.

Construction of ZIP-Level Racial Shares. To construct Figure IXa and IXb, which condition on racial shares at the ZIP level, we need data on racial shares by ZIP code. The 2000 Census includes summary tables by ZIP code tabulation areas (ZCTAs) instead of ZIP code. ZCTAs are a U.S. Census Bureau geographical unit that in most cases correspond closely to ZIP codes, but sometimes do not. We use a [ZCTA to ZIP Crosswalk](#) from the John Snow Institute to assign each ZIP code a racial share based on Census 2000 ZCTA-level data from table P008.

CZ-Level Price Index. To measure real incomes, we first construct a CZ-level ACCRA price index using the [2010 ACCRA composite cost of living index](#) (table 728) for “urbanized areas” in 2010, which we crosswalk to CZs as follows. First, we use the 2012Q1-2013Q1 [correspondence](#) (downloaded on 11/21/2013) to assign 298 out of the 325 urbanized areas to MSAs. Each county in an MSA was assigned the same value of the index. We then merge counties to CZs and calculate an unweighted mean of the index among non-missing values within the CZ. Some CZs had no counties within an MSA and were therefore assigned a missing value of the ACCRA index.

To construct a price index that covers all CZs, we regress the CZ-level ACCRA index on a quadratic in log population density (from the 2000 Census), a quadratic in log median housing values, the latitude and longitude of the CZ centroid, and state fixed effects. Housing values are the population-weighted mean of tract median housing values for owner-occupied units in the 1990 Census short form. Latitude and longitude are the mean latitude and longitude across counties within each CZ, obtained from the Census 2000 [Gazetteer](#) county-level data. The predicted values from this regression constitute our final price index that covers all CZs.

B. Comparison to Survey Datasets

In Appendix Table IV, we compare selected moments of income distributions and other variables

in the tax data to data from two nationally representative surveys that have been used in prior work on the income distribution: the 2011-12 CPS and the 2011-12 ACS. We restrict the ACS and CPS samples to citizens in the same birth cohorts as our core sample (1980-82). To the extent possible, we define all income variables to match the concepts in the tax data.

To assess whether our method of linking children to parents based on dependent claiming creates selection bias, we compute statistics in the tax data both on the full sample of all children in the 1980-82 birth cohorts who are current U.S. citizens and the core sample of children linked to parents. Because most children are linked to parents, the differences between these two samples are small, though children who lack valid parent matches have slightly lower earnings on average.

Overall, the tax data are very similar to the CPS and ACS. The sum of the sampling weights in our survey-based samples provide estimates of the size of the target population being sampled. This population is very similar in the tax data and the two surveys. The mean and median earnings levels are very similar, as are the fractions with non-zero income. Perhaps more surprisingly, the interquartile range (P75-P25) of earnings is also similar across the three data sources. If survey data were reported with classical measurement error, we would expect the interquartile range to be larger in survey sources. However, survey reports of income exhibit “mean reverting” measurement error which has the effect of reducing variability (Bound and Krueger 1991, Bound, Brown and Mathiowetz 2001). Moreover, survey non-response tends to follow a U-shaped pattern (Kline and Santos 2013), with very high and low earning individuals being least likely to provide earnings responses, which can further reduce variability. The quantiles of family income also line up well across the three data sources, with the tax-based moments strongly resembling those from the ACS, perhaps because the ACS has a higher response rate for earnings than the CPS.

C. Comparison to Mitnik et al. (2014)

Mitnik et al. (2014) propose a new measure of the intergenerational elasticity that is more robust to the treatment of small incomes. In this appendix, we compare the traditional definition of the IGE (Solon 1999, Black and Devereux 2011) to the new measure proposed by Mitnik et al. We first show that the traditional IGE can be interpreted as the average elasticity of child income with respect to parent income in a model with heterogeneous elasticities, while Mitnik et al.’s new measure is a dollar-weighted (i.e., child-income-weighted) average of the same elasticity. We then compare estimates of the dollar-weighted IGE to estimates of the traditional IGE in our data and to the estimates of Mitnik et al.

Setup. Let Y_i denote the level of child income and X_i denote the level of parent income. Let $F_{Y|X=x}(y)$ denote the conditional distribution of Y given X , which we assume is differentiable with respect to x at all $(y, x) > 0$. Define the conditional quantile function (CQF) as the inverse of the CDF:

$$q(x, \tau) = F_{Y|X=x}^{-1}(\tau)$$

for $\tau \in [0, 1]$.² The CQF gives the quantiles of the conditional distribution of Y_i given X_i ; for example, $q(x, .5)$ is the median of Y_i when $X_i = x$.

We can use the CQF to represent Y_i as:

$$Y_i = q(X_i, U_i),$$

where $U_i|X_i \sim \text{Uniform}(0, 1)$. Hence, the conditional mean of child income given parent income

²At mass points, we define $q(x, \tau) \equiv \inf \{y : F_{Y|X=x}(y) \geq \tau\}$.

can be written as a function of the CQF:

$$E[Y_i|X_i = x] = E[q(X_i, U_i)|X_i = x] = \int_0^1 q(x, \tau) d\tau.$$

Define the elasticity of a given quantile of the child's income distribution with respect to parent income around a parent income level x as

$$\sigma(x, \tau) \equiv \frac{dq(x, \tau)}{dx} \frac{x}{q(x, \tau)} = \frac{q_x(x, \tau)x}{q(x, \tau)}.$$

In general, the elasticity will vary across quantiles τ .³ We now show that traditional estimates of the intergenerational elasticity (e.g., Solon 1992) and the new estimator proposed by Mitnik et al. (2014) can be interpreted as different averages of the elasticities $\sigma(x, \tau)$.

Traditional IGE. The intergenerational elasticity at a given parent income level x , which we denote by $IGE(x)$, is defined as the impact of an increase in log parent income (starting from x) on expected log child income:

$$\begin{aligned} IGE(x) &= \frac{dE[\log Y_i|X_i = x]}{d\log x} = \frac{d}{d\log x} \int_0^1 \log q(x, \tau) d\tau \\ &= \int_0^1 \frac{d}{d\log x} \log q(x, \tau) d\tau \\ &= \int_0^1 \sigma(x, \tau) d\tau \\ &= \bar{\sigma}(x) \end{aligned}$$

where $\bar{\sigma}(x) \equiv E[\sigma(X_i, U_i)|X_i = x]$. If we interpret the IGE as the average of $IGE(x)$ across levels of parent income x , we obtain

$$IGE = E[\bar{\sigma}(x)] = \int_{-\infty}^{\infty} \int_0^1 \sigma(x, \tau) d\tau dF_X(x),$$

where $F_X(\cdot)$ is the marginal distribution of X_i . Hence, the traditional IGE can be interpreted as the average elasticity of child income with respect to parent income across quantiles and parent income levels.

Mitnik et al. IGE. Mitnik et al. (2014) propose an alternative approach to estimating the IGE that switches the order of the log and the expectation relative to the traditional approach. They define the IGE as the impact of an increase in log parent income (starting from x) on the log of expected child income:

$$IGE_W(x) \equiv \frac{d\log E[Y_i|X_i = x]}{d\log x}.$$

³Naturally, this elasticity is only defined for quantiles where $q(x, \tau) > 0$; the standard empirical practice in the prior literature (e.g., Solon 1992) has been to exclude children with zero income for this reason.

To see how their estimand relates to the traditional IGE, observe that

$$\begin{aligned}
IGE_W(x) &= \frac{d}{d \log x} \log \int_0^1 q(x, \tau) d\tau \\
&= \frac{\int_0^1 q_x(x, \tau) x d\tau}{\int_0^1 q(x, \tau) d\tau} \\
&= \frac{\int_0^1 q(x, \tau) \sigma(x, \tau) d\tau}{\int_0^1 q(x, \tau) d\tau} \\
&= E[\omega(X_i, U_i) \sigma(X_i, U_i) | X_i = x]
\end{aligned}$$

where $\omega(X_i, U_i) \equiv \frac{q(X_i, U_i)}{E[q(X_i, U_i) | X_i]}$ is a set of quantile specific weights which sum to one for each value of X_i . Averaging $IGE_W(x)$ across levels of parent income x , Mitnik et al.'s statistic can be written as $IGE_W = E[IGE_W(x)]$. The parameter $IGE_W(x)$ is a weighted average of the elasticity $\sigma(x, \tau)$ across quantiles τ , with weights that are an increasing function of the child's income. Higher quantiles get larger weights, in proportion to their dollar value; the weights approach zero as the child's income approaches 0. In this sense, the IGE_W statistic defined by Mitnik et al. is a dollar-weighted mean of the IGE across quantiles.

The traditional IGE and the dollar-weighted IGE_W proposed by Mitnik et al. are two different parameters. The "correct" parameter depends on the question one seeks to answer. If one's goal is to estimate IGE_W , then the traditional IGE estimate will in general yield a biased estimate of IGE_W . Conversely, if one's target is to estimate the traditional IGE (e.g., for comparison to prior estimates in the literature), then IGE_W will in general be biased.

As Mitnik et al. note, one statistical benefit of the dollar-weighted IGE is that it is not sensitive small changes in incomes at the bottom of the distribution, such as recoding zero income as \$1. Intuitively, dollar-weighted elasticities are not sensitive to the impacts of parent income on children's income at the bottom of the distribution. In contrast, person-weighted estimates are very sensitive to changes in incomes at the bottom of the distribution, because such changes can affect elasticities at the lowest quantiles substantially. The traditional IGE is unstable because the elasticity of child income with respect to parent income is ill-defined at quantiles with zero income.

Empirical Estimates. In large samples, we can estimate $E[Y_i | X_i = x]$ non-parametrically as shown in Figure Ia. It is therefore straightforward to estimate IGE_W simply by regressing the non-parametric estimates of $\log E[Y_i | X_i = x]$ on $\log x$.⁴ The series in circles in Appendix Figure Ia plots $\log E[Y_i | X_i = x]$ vs. $\log x$ simply by taking logs of the data points shown in Figure Ia.⁵ As a reference, we also replicate the non-parametric plot of $E[\log Y_i | X_i = x]$ vs. $\log x$ from Figure Ib (excluding children with zero income) in the series in triangles.

The two series are very similar, implying that nonparametric estimates of $\frac{d \log E[Y_i | X_i = x]}{d \log x}$ and $\frac{d E[\log Y_i | X_i = x]}{d \log x}$ are very similar for most values of x . The dollar-weighted IGE estimate (including children with zero income) is $IGE_W = 0.335$, virtually identical to the traditional IGE estimate of $IGE = 0.344$ obtained when we exclude children with zero income. Between the 10th and 90 percentiles, the dollar-weighted IGE is 0.414, while the traditional IGE is 0.452.

In Appendix Figure Ib, we report dollar-weighted IGE estimates by the age of the child to assess the extent of lifecycle bias in the dollar-weighted IGE estimates. We find that the dollar-weighted IGE also stabilizes around age 30: the estimated IGE_W is 2.1% higher at age 32 than age 31 (0.343

⁴Mitnik et al. use a Poisson pseudo-maximum-likelihood (PPML) estimator to estimate IGE_W in survey data, which approximates $\log E[Y_i | X_i = x]$ in large samples.

⁵Unlike in Figure Ia, we include the top bin (the top 1% of parents) in this figure.

vs. 0.336).

Mitnik et al. (2014) obtain larger estimates of the dollar-weighted IGE (around 0.5) in their SOI sample. Although both studies use similar data from tax records, there are several small methodological differences between Mitnik et al.’s approach and our approach. A useful direction for future work would be to investigate which of these differences is responsible for the differences in the IGE estimates.

D. Comparison to Clark (2014)

Clark (2014) presents estimates of mobility across generations using surname averages of income, representation in elite professions, and other related outcomes. He obtains implied IGE estimates around 0.8, well above the estimates of intergenerational persistence obtained in our analysis and the prior literature (e.g., Solon 1999). In this appendix, we first show that in our data, estimates of mobility based on surname means are generally quite similar to our baseline individual-level estimates. We then present a simple hypothesis that may explain why Clark’s focus on rare surnames leads to a much higher estimated IGE.

Surname-Based Estimates. In Chetty et al. (2014, Appendix B), we construct estimates of intergenerational mobility across surnames as follows. We begin with all the children in our core sample and restrict attention to those whose surnames in 2012 are the same as their parents’ surnames.⁶ We then obtain a de-identified table of surname-level means of percentile ranks (using the baseline income definition) for both parents and children. Finally, we regress the surname-level mean ranks for children on surname-level mean ranks for parents (as suggested by Clark 2014, Appendix 2), weighting by the number of individuals with each surname, to obtain a surname-level rank-rank slope. We construct surname-level estimates of the log-log IGE analogously, computing surname level means of log income (excluding zeroes) for children and parents.

Appendix Table V reports the results of this analysis. Each row of the table shows the estimates for different subsets of names. The first row considers all names. Rows 2-4 restrict to rare surnames, i.e. names held by fewer than 25, 50, or 100 children. Rows 5-8 conversely limit the sample to common surnames, i.e. names held by more than 100, 1000, 10000, or 20000 people. In each row, we report the number of children in the sample (Column 1), the number of unique surnames in the sample (Column 2), surname-based estimates of the rank-rank slope (Column 3), individual-level estimates of the rank-rank slope (Column 4), surname-based estimates of the log-log IGE (Column 5), and individual-level estimates of the log-log IGE (Column 6).

Appendix Table V shows that surname-based averages of income generally do not imply much greater intergenerational persistence than individual-level regressions unless one uses specific subsets of names for the analysis. For example, when including all names (row 1), the individual-level rank-rank slope is 0.30, compared with a surname-level rank-rank slope of 0.39. If we restrict to the rarest names (shared by fewer than 25 people), the individual-level rank-rank slope is 0.27, compared with a surname-based rank-rank slope of 0.30. The IGE estimates at the individual level are also slightly smaller than those based on surname averages.

The only case in which the surname averages yield much larger implied IGEs and rank-rank slopes is in the last row of the table, where we restrict to the 7 most common names in the U.S. population. Here, the surname-based IGE is 0.81, compared with an individual-level IGE of 0.36. This implies that the rate of convergence in income across generations across these broad name groups – which likely capture broad differences in ethnicities or race – is much smaller than the

⁶As Clark (2014, Appendix 2) notes, surname-based analyses will yield attenuated estimates of the IGE if they include parents and children who do not actually have the same surname. Consistent with this hypothesis, we find smaller estimates of rank-rank correlations and IGEs when we use the full core sample, without limiting to children who have the same surname as their parents.

rate of convergence within the groups, a point we return to below.

Interpretation of Clark (2014) Estimates. Why does Clark obtain much larger estimates of intergenerational persistence than we find in Appendix Table V? There are many methodological differences between Clark’s analysis and our approach above. For example, Clark analyzes multiple generations and uses other proxies for status (such as professional occupation) rather than income. While a comprehensive analysis of the source of the difference is outside the scope of this study, we believe that one key difference is Clark’s focus on distinctive surnames. For instance, one comparison Clark (2014, page 60, Figure 3.10) gives is of the surname “Katz” vs. “Washington.” As he notes, Katz is a common Jewish surname, while Washington is a common black surname. The comparison of intergenerational convergence in income between these two names is thus analogous to using an indicator for race as an instrument in a traditional individual-level IGE regression.⁷

As is well known from prior work, using race to instrument for income yields much larger IGE estimates, presumably because race has direct effects on children’s income independent of its impact on parent income, as shown e.g., by Borjas (1992). If one uses an indicator for being black as an instrument, the IGE estimate is equivalent to the proportional reduction in the black-white income gap across generations. In 1980, blacks’ median earnings were 78.8% that of whites on average (Bureau of Labor Statistics 2011, Table 14, page 41). In 2010, blacks’ median earnings were 79.9% that of whites. Hence, the implied between-group IGE is $78.8/79.9 = 0.986$, consistent with Clark’s larger estimates. Importantly, even though there is very little convergence across racial groups during this time period, there is considerable social mobility within racial groups. This is why our estimates of the IGE based on individual-level data (or pooling all surnames) over the same period are much lower.

In sum, we believe that Clark’s approach effectively identifies a parameter analogous to the degree of convergence in income across generations between racial or ethnic groups rather than across individuals. This is an interesting parameter, but one that differs from standard studies of intergenerational mobility that seek to measure the extent to which an individual’s status is determined by his parents’ idiosyncratic circumstances.⁸ A useful direction for future research would be to investigate why the rate of income convergence across certain ethnic groups is small even though intergenerational mobility within these groups is much higher.

E. Comparison to Mazumder (2005)

Mazumder (2005) reports that IGEs estimated using even 5-year averages of parent earnings exhibit substantial attenuation bias because of long-lasting transitory shocks to income. This appendix provides further details on why we find much less attenuation bias than Mazumder.

Mazumder (2005, Table 4, row 1, page 246) obtains IGE estimates as high as 0.6 when using 15-year averages of parent income matched SIPP-SSA administrative data, 54% larger than his 4-year pooled estimate of 0.388. In contrast, we find little difference between IGEs based on five-year vs. fifteen-year averages of parent income both using our preferred rank-rank specification (Figure IIIb) and using a log-log IGE specification similar to that estimated by Mazumder. For example, we obtain a log-log IGE of 0.366 using a 15-year average of parent family income, 6% larger than the estimate using a 5 year average reported in row 1 of Table I.⁹

⁷Clark reports many other surname comparisons that do not map as directly to racial or ethnic groups. However, it is possible that similar group-level persistence effects are picked up by other unique surname contrasts as well.

⁸This interpretation differs from that put forth by Clark, who argues that individual-level estimates do not capture latent “status” as well as surname-based averages. Our analysis of surname means suggests that the differences in the results are driven by differences in the rate of income convergence within vs. between ethnic groups rather than a downward bias in measures of intergenerational persistence based on individual data.

⁹When computing long time averages, we measure earnings of parents at older ages than Mazumder (2005) because

We believe our results differ from Mazumder’s findings because we directly observe income for all individuals in our data, whereas Mazumder imputes parent income based on race and education for up to 60% of the observations in his sample to account for top-coding in social security records.¹⁰ These imputations are analogous to instrumenting for parent income using race and education, an approach known to yield higher estimates of the IGE, perhaps because parents’ education directly affects children’s earnings (Solon 1992). Because the SSA earnings limit is lower in the early years of his sample, Mazumder imputes income for a larger fraction of observations when he averages parent income over more years (Mazumder 2005, Figure 3). As a result, Mazumder’s estimates effectively converge toward IV estimates as he uses more years to calculate mean parent income, explaining why his estimates rise so sharply with the number of years used to measure parent income. Consistent with this explanation, when he drops imputed observations, his IGE estimates increase much less with the number of years used to measure parent income (Mazumder 2005, Table 6).

Mazumder also reports simulations of earnings processes showing that attenuation bias in the IGE should be substantial even when using five-year averages. However, he calibrates the parameters of the earnings process in his simulation based on estimates from survey data and his SIPP-SSA sample, both of which may have more noise than the population data we use here, which cover all workers and are not top-coded. If one replicates Mazumder’s simulations using a smaller variance share for transitory shocks, one obtains results similar to ours in Figure IIIb, with little attenuation bias in estimates based on five-year averages.

To be clear, Mazumder acknowledges the potential bias due to imputation, as he recommends in his conclusion that “future research should attempt to verify the results here using long-term measures of permanent earnings from other sources that do not require the kind of imputations that were necessary in this study.” We simply follow this recommendation.

F. Robustness of Spatial Patterns

In Appendix Table VII, we assess the robustness of the spatial patterns in mobility documented in Section V along four dimensions: (1) changes in sample definitions, (2) changes in income measures, (3) adjustments for differences in the cost-of-living and growth rates across areas, and (4) the use of alternative statistics to measure relative and upward mobility. The first number in each cell of this table reports the correlation across CZs of a baseline mobility measure (using child family income rank and parent family income rank in the core sample) with an alternative mobility measure described in each row. The second number in each cell reports the ratio of the standard deviation of the alternative measure to the baseline measure. Note that we do not report the ratio of standard deviations for statistics that are measured in different units relative to the corresponding baseline measure.

Column 1 reports the unweighted correlation (and SD ratio) across CZs between our baseline measure of absolute upward mobility ($\bar{r}_{25,c}$) and the corresponding alternative measure of $\bar{r}_{25,c}$. Column 2 replicates Column 1 for relative mobility (β_c). Columns 3 and 4 replicate Columns 1 and 2 weighting CZs by their population in the 2000 Census.

Sample Definitions. In the first section of Appendix Table VII, we assess the robustness of the structure of our data. However, Appendix Figure IIb shows that our estimates of mobility are not sensitive to varying the age in which parent income is measured over the range observed in our dataset. Hence, the differences between the findings of the two papers cannot be explained by differences in the age at which parent income is measured.

¹⁰There are also other differences in Mazumder’s specification, such as the imputation of income for children whose earnings are not covered by SSA. However, it is less obvious why these differences would produce sharp changes in the estimated IGE when using longer time averages of parent income.

spatial patterns to changes in the sample definition, as we did at the national level in Table I. Rows 1 and 2 restrict the sample to male and female children, respectively. Rows 3 and 4 consider the subsamples of married parents and single parents. The correlations of both absolute and relative mobility in these subsamples with the corresponding baseline measures is typically above 0.9.

In row 5, we replicate the baseline specifications using the 1983-85 birth cohorts (whose incomes are measured at age 27 on average in 2011-12). In row 6, we consider the 1986-88 birth cohorts instead. The intergenerational mobility estimates across CZs for these later birth cohorts are very highly correlated with the baseline estimates. This result has three implications. First, it demonstrates that the reliability of CZ-level estimates is quite high across cohorts; in particular, sampling error or cohort-specific shocks do not lead to much fluctuation in the CZ-level estimates. Second, because the later cohorts are linked to parents at earlier ages (as early as age 8), we conclude that the spatial patterns in intergenerational mobility are not sensitive to the precise age at which we link children to parents or measure their geographical location. Finally, because the earnings of later cohorts are measured at earlier ages, we conclude that one can detect the spatial differences in mobility even when measuring earnings quite early in children’s careers.

In row 7, we restrict the sample based on the age of parents at the birth of the child. We limit the sample to children whose mothers are between the ages of 24-28 and fathers are between 26-30 (a five year window around the median age of birth). The intergenerational mobility estimates in this subsample are very highly correlated with the baseline estimates, indicating that the cross-area differences in income mobility are not biased by differences in the age of child birth for low income individuals.

In row 8, we assess the extent to which the variation in intergenerational mobility comes from children who succeed and move out of the CZ as adults vs. children who stay within the CZ. To do so, we restrict the sample to the 62% of children who live in the same CZ in 2012 as where they grew up. Despite the fact that this sample is endogenously selected on an ex-post outcome, the mobility estimates remain very highly correlated with those in the full sample. Apparently, areas such as Salt Lake City that generate high levels of upward income mobility do so not just by sending successful children to other CZs as adults but also by helping children move up in the income distribution within the area.

In row 9, we restrict the sample to the 88% of children in the core sample who are not claimed as dependents by other individuals in subsequent years after they are linked to the parents we identify. We obtain very similar estimates for this “unique parent” subsample, indicating that the spatial pattern of our mobility estimates is not distorted by measurement error in linking children to their parents.

Income Definitions. In the second section of Appendix Table VII, we evaluate the sensitivity of the spatial patterns to alternative definitions of income. The definitions we consider match those in the robustness analysis in Table I; see Section IV.B for details on these definitions. In row 10 of Appendix Table VII, we define parent income as the income of the higher earner rather than total family income to evaluate potential biases from differences in parent marital status across areas. In row 11, we measure the child’s income using individual income instead of family income to assess the effects of differences in the child’s marital status. In row 12, we use the child’s individual earnings (excluding capital and other non-labor income). In row 13, we replicate the specification in row 11 for male children, using individual income for the child and family income for the parent. Row 14 replicates row 13, but defines parent income as the income of the higher earner instead. In row 15, we define parent income using data from 1999-2003 (when we have data from W-2’s) instead of 1996-2000. All of these definitions produce very similar spatial patterns in intergenerational mobility: correlations with the baseline measures exceed 0.9 in most cases.

Adjustments for Cost-of-Living and Growth Rates. The third section of Appendix Table VII

considers a set of other factors that could bias comparisons of intergenerational mobility across areas. One natural concern is that our estimates of upward mobility may be affected by differences in prices across areas. To evaluate the importance of differences in cost of living, we construct a CZ-level price index using the American Chamber of Commerce Research Association (ACCRA) price index for urban areas combined with information on housing values, population density, and CZ location (see Appendix A for details). We then divide parents' income by the price index for the CZ where their child grew up and the child's income by the price index for the CZ where he lives as an adult (in 2012) to obtain real income measures.

Row 16 of Appendix Table VII shows that the measures of intergenerational mobility based on real incomes are very highly correlated with our baseline measures (see also Appendix Figure VIa). The reason that cost-of-living adjustments have little effect is that prices affect *both* the parent and the child. Intuitively, in high-priced areas such as New York City, adjusting for prices reduces the child's absolute rank in the national real income distribution. But adjusting for prices also lowers the real income rank of parents living in New York City. As a result, the degree of upward mobility – i.e., the difference between the child's rank and the parent's rank – is essentially unaffected by adjusting for local prices.

The preceding logic assumes that children always live in the same cities as their parents. In practice, some children move to areas with higher prices (e.g. from rural Iowa to New York City). Our measures of upward mobility are affected by the cost of living adjustment in such cases, but they are not sufficiently frequent to have a large impact on our estimates. The correlation between the cost of living in the child's CZ at age 30 and the parent's CZ is 0.77, and the correlation between a child's nominal percentile rank and the local price index is only 0.10. As a result, cost of living adjustments end up having a minor impact on the difference between child and parent income and thus have little effect on our mobility statistics.

Next, we assess the extent to which economic growth is responsible for the spatial variation in upward mobility. In row 17, we define parent income as mean family income in 2011-12, the same years in which we measure child income. Insofar as local economic growth raises the incomes of both parents and children, this measure nets out the effects of growth on mobility. Both the upward and relative mobility measures remain very highly correlated with the baseline measures, suggesting that differences in local economic growth drive relatively little of the spatial variation in mobility.

As an alternative approach to accounting for growth shocks, we regress our measures of mobility on the CZ-level growth rate from 2000-2010 and calculate residuals.¹¹ Row 18 of Appendix Table VII shows that the correlation of the growth-adjusted relative mobility measures with the baseline measures exceeds 0.9; the correlations for absolute mobility exceed 0.8. Note that these growth-adjusted measures over-control for exogenous growth shocks insofar as growth is partly a consequence of factors that generate upward income mobility in an area. Hence, the finding that even controlling for growth rates directly does not significantly change the spatial pattern of intergenerational mobility supports the view that most of the variation in mobility across areas is not due to exogenous growth shocks in the 2000's.¹²

¹¹We measure income in 2000 using the Census and in 2008 using the 5-year American Community Survey, averaged over 2006-2010. We calculate household income per working age adult as aggregate income in a CZ divided by the number of individuals aged 16-64 in that CZ. Annualized income growth is calculated as the annual growth rate implied by the change in income over the 8 year period; we use 8 years because 2008 is the midpoint of 2006-2010.

¹²The fact that college and teenage birth gradients are similar to income mobility gradients provides further evidence that growth shocks in the 2000s do not generate the differences in mobility across areas, as college and teenage birth are measured around 2000. These results also show that the spatial patterns are unlikely to be driven by differences in reporting of taxable income.

Alternative Statistics for Mobility. One potential concern with our approach is that using national ranks may misrepresent the degree of relative mobility within the local income distribution, which may better reflect a child’s opportunities. To address this concern, in row 19 of Appendix Table VII, we measure relative mobility using local ranks. We rank parents relative to other parents living in the same CZ and children relative to other children who grew up in the same CZ (no matter where they live as adults). We define relative mobility as the slope of the local rank-rank relationship.¹³ Relative mobility based on local ranks is very highly correlated with relative mobility based on national ranks. This is because local ranks are approximately a linear transformation of national ranks.

Finally, we consider two alternative measures of absolute upward mobility. In row 20, we measure absolute upward mobility based on the probability that the child rises from the bottom quintile of parent income to the top quintile of child income, as in Column 5 of Table III. In row 21, we measure absolute upward mobility as the probability that a child has family income above the poverty line conditional on having parents at the 25th percentile. To construct this statistic, we first regress an indicator for having family income above the federal poverty line in 2012 on parent rank in the national income distribution in each CZ.¹⁴ We then calculate the predicted fraction of children above the poverty line for parents at the 25th percentile based on the slope and intercept in each CZ. The spatial patterns in both of these measures – which are also shown in the maps in Appendix Figure VI – are very similar to those in our mean-rank based measure of upward mobility, with correlations across CZs above 0.9 in both cases.

G. Construction of CZ-Level Covariates

This appendix provides definitions and sources for the CZ-level covariates used in Section VO. [Online Data Table IX](#) contains detailed descriptions of each covariate and briefly describes the source of data for each variable. Here, we provide additional details on each data source along with links to original sources. As a reference, we provide Stata code on our website that constructs the final CZ-level covariates (data available in [Online Data Table VIII](#)) from the raw data downloaded from the links below.

Our source data are primarily at the ZIP code and county level. We map ZIP codes and counties to CZs using the procedure described in Appendix A. We compute CZ-level means of the ZIP- and county-level data using population-weighted averages, with population counts from the 2000 Census.

Racial Demographics. Racial shares are calculated from the [2000 census](#) short form (SF1) table P008. Note that all Census data can be obtained by searching for the relevant census table on the U.S. Census Bureau’s [American FactFinder](#). The black share is defined as the number of people in a CZ who are black alone divided by the CZ population; the white share is calculated similarly. For the Hispanic share, the numerator is the number of people of any race who are Hispanic. We also calculate a residual category where the numerator is the number of people that are neither black alone nor white alone nor Hispanic.

Segregation. We measure racial and income segregation using Theil indices as described in the text.¹⁵ We compute the racial segregation index using the census tract level data on racial

¹³We cannot study absolute mobility with local ranks because both child and parent ranks have a mean of 50 by definition: if one child moves up in the local distribution, another must move down.

¹⁴We define household size as the maximum household size in 2010-11, where household size is defined as 1 plus an indicator for being married plus the number of dependents claimed. The poverty line threshold is defined as \$11,170+(household size - 1)×\$3,960.

¹⁵As Iceland (2004) argues, the Theil index is an attractive measure conceptually because it captures segregation across multiple racial groups. However, we obtain similar results using alternative two-group measures of black-white

shares from table P008 from the 2000 Census.¹⁶ For segregation of affluence and poverty, we use the sample data from the [2000 census](#) long form (SF3) on the income distribution of households in 1999 by census tract contained in table P052. Our formulas for the three income segregation measures are taken directly from Reardon (2011). We compute $H(p_k)$ for each of the 16 income groups given in table P052. We then estimate $H(p_{25})$ and $H(p_{75})$ in each CZ using the 4th order polynomial version of the weighted linear regression in equation 12 on page 23 of Reardon (2011). The overall segregation of income index is Reardon’s rank-order index, which we compute from equation 13, where the δ vector is given in Appendix A4 of Reardon (2011).

To compute the commute time variable, we divide the number of workers that commute for less than 15 minutes by the total number of workers. The sample for both of these counts is restricted to workers that are at least 16 years old and do not work from home. Travel time data is from the [2000 census](#) table P031.

Income Distributions. We compute mean income per working age adult by dividing aggregate household income in a CZ by the total number of people aged 16-64 in that CZ. These data come from the [2000 census](#) table P054 and P008. The Gini coefficient, fraction middle class, and top 1% income share are computed using our sample of parents and the family income definitions used for the main analysis in this paper, but with family income top coded at \$100 million in all years.

K-12 Education. We use the National Center for Education Statistics’ [Common Core of Data](#) data for public schools for several of our K-12 covariates. School expenditures per student is taken from school-district data for the 1996-1997 fiscal year. We drop 8 CZs that are in the top 1% of the distribution of expenditures per student to reduce the influence of outliers. While we would ideally measure school spending in the 1980s and early 1990s, when the students in our core sample were in school, such data are not readily available for earlier years. However, the correlation in school spending per capita across states in 1982 and 1992 is 0.86, suggesting that using earlier data would not substantially affect our findings.

We use school-level data on student-teacher ratios for the 1996-1997 school year. We drop the top 0.1% of observations, which have student-teacher ratios that exceed 100. We also drop approximately 10% of schools whose student-teacher ratios are recorded as being 0.

High school dropout rates are obtained from school-district data for the 2000-2001 school year. We code the dropout rate as missing in CZs in which more than 25% of school districts have missing data on dropout rates. We construct an income-adjusted measure of dropout rates using residuals from a CZ-level regression of the dropout rate on mean parent family income (from 1996-2000) in the core sample.

We obtain a standardized measure of grade 3-8 test scores from the National Math Percentile and National Reading Percentile series in the [Global Report Card](#). We calculate the student-weighted mean of the math and reading rankings over 2004, 2005, and 2007 in each CZ to arrive at our measure of mean test scores. We then construct a measure of income-adjusted test scores using the residual from a CZ-level regression of mean test scores on mean parent family income (from 1996-2000) in the core sample.

segregation such as isolation indices or dissimilarity indices because alternative measures of segregation are highly correlated at the level of metro areas (Cutler, Glaeser and Vigdor 1999). The segregation patterns are sufficiently stark that one can directly see the differences in segregation between the least and most upwardly mobile cities using the color-coded dot [maps](#) produced by Cable (2013) using Census data. For instance, compare Atlanta – one of the most segregated cities and one of the lowest-mobility cities in our data – to Sacramento – one of the most integrated and highest-mobility cities.

¹⁶We also replicated the analysis using measures of segregation from the 1990 Census and find very similar results. For example, the correlation between upward mobility and the Theil racial segregation index measured using the 1990 Census is -0.357, compared with -0.361 when measured using the 2000 Census. The correlation between upward mobility and income segregation is -0.393 using both the 1990 and 2000 Census.

We construct enrollment-weighted means at the ZIP code level of all the school and school district level variables using the school and district ZIP codes provided in each of the data sources. We then take enrollment-weighted means across ZIP codes to construct CZ-level estimates using the ZIP to CZ crosswalk discussed in Appendix A.

Social Capital. For social capital, we use the 1990 county-level [social capital index](#) from Rupasingha and Goetz (2008). For religious affiliation, we use data on the self-reported number of religious adherents from the [Association of Religion Data Archives](#) at Pennsylvania State University. Data on crime rates are from the FBI’s Uniform Crime Reporting program. We downloaded county-level data from the [ICPSR](#) and use the number of arrests for serious (part 1 index) violent crimes divided by the total covered population.

Family Structure. We define the share of single mothers in each county as the number of households with female heads (and no husband present) with own children present divided by the total number of households with own children present. These data from the [2000 census](#) long form (SF3) in table P018. We calculate the fraction married and fraction divorced in each county using the number of people that are married or divorced (in the sample of people that are 15 years and older) using data from the [2000 census](#) in table P018.

Taxes and Government Expenditures. We estimate local tax rates using data on tax revenue by county from the U.S. Census Bureau’s 1992 Census of Government county-level summaries, which we downloaded from the [ICPSR](#). In particular, Part 2 of the ICPSR download contains the county-level summaries. We define the tax rate in each CZ as follows. First, we calculate county tax revenue divided by the county population estimate for each county in the CZ. We then take a population-weighted mean across these counties to obtain a CZ-level mean per-person taxes. Finally, we divide mean per-person taxes by the Census 1990 estimate of nominal income per household, which we downloaded from the [National Historical Geographic Information System \(NHGIS\)](#). We code the tax rate as missing for one CZ (Barrow, Alaska), which has a calculated tax rate of 90%.

We compute total government spending per capita in each county using Census data on [government expenditures](#) by aggregating all county-level total expenditure categories and dividing by the 1992 county population estimates. We then construct a CZ-level measure by taking population-weighted means of expenditures per capita in the counties in each CZ. We code local government expenditures as missing for two CZs (Barrow, Alaska and Kotzebue, Alaska), which have unusually high expenditures per capita that exceed 50% of per capita income.

We measure state income tax progressivity as the difference between 2008 state income tax rates for incomes above \$100,000 and incomes in the bottom tax bracket using data from the [Tax Foundation](#). We obtain data on State EITC rates by year from Hotz and Scholz (2003). We calculate mean EITC rate for the years 1980-2001, setting the rate to zero for state-year pairs where there was no state EITC. Note that Wisconsin’s state EITC rate depends on the number of children in a household; we use the rate for households with two children.

Higher Education. We use the Integrated Postsecondary Education Data System ([IPEDS](#)) to construct our three measures of college access and quality. We restrict the sample to Title IV institutions that have undergraduate students, and are degree offering. The number of colleges per capita in each CZ is the number of institutions in the 2000 IPEDS in each CZ divided by the CZ population. We define college tuition as the mean in-state tuition and fees for first-time, full-time undergraduates for the institutions in each CZ. We define the enrollment-weighted mean graduation rate based on the 150% of normal time college graduation rate from IPEDS 2009, the first year for which this data is available. We construct a measure of income-adjusted graduation rates using the residual from a CZ-level regression of graduation rates on mean parent income in the core sample.

Local Labor Market Conditions. The labor force participation rate is defined as the number of

people in the labor force by the total population in the sample of people that are at least 16 years old. These data are from the 2000 Census long form (SF3) in table P043. We compute the share of workers in manufacturing from the 2000 census in table P049; we divide the number of people working in manufacturing by the total number of workers.

The exposure to Chinese trade variable is the percentage change in imports per worker from China between 1990 and 2000. It is measured as the growth in imports allocated to a CZ, divided by the CZ work force in 1990 (with the growth rate defined as 10 times the annualized change). This variable was constructed by Autor et al. (2013) and provided to us by David Dorn.

The teenage labor force participation rate is defined in each CZ as the share of individuals who received one or more W-2's between the ages of 14 and 16. We calculate the teenage LFP rate using W-2 data for the 1985-1987 birth cohorts, the earliest cohorts for which we have W-2 data at age 14.

Migration Rates. For inflow and outflow migration data, we use the county-to-county migration data from the Internal Revenue Service's [Statistics of Income](#) for 2004-2005. Inflow migration is the number of people moving into a CZ from counties in other CZs divided by the total CZ population; outflow migration is calculated similarly. We compute the share of each CZ's population that is foreign born using sample data from the [2000 census](#) (table P021) on the number of foreign born inhabitants divided by total CZ population. In both cases, total CZ population is the sum of county populations from the 2000 Census (table P008) over counties in the CZ.

H. Correlates of Intergenerational Mobility: Other Factors

In this appendix, we first discuss correlations between absolute upward mobility and the four factors in Figure VIII that were not discussed in Section VI: local tax policies, higher education, labor market conditions, and migration. We then summarize the methodology used to estimate the correlations in Appendix Table VIII and the binned scatter plots in Appendix Figures X-XII.

Local Public Goods and Tax Policies. We assess correlations between local tax and expenditure policies and intergenerational mobility in the seventh panel of Figure VIII and Appendix Table VIII. We begin by correlating upward mobility with local tax rates. We measure the average local tax rate in each CZ as total tax revenue collected at the county or lower level in the CZ (based on the 1992 Census of Governments) divided by total household income in the CZ based on the 1990 Census.¹⁷ Note that 75% of local tax revenue comes from property taxes; hence, this measure largely captures variation in property tax rates. In the baseline unweighted specification pooling all CZs, the correlation between absolute upward mobility and the average local tax rates is 0.33. We find a robust positive correlation between tax rates and upward mobility across the specifications in Appendix Table VIII.

An alternative measure of local public good provision is local government expenditure. Tax revenue differs from local government expenditure because of inter-governmental transfers. We define local government expenditure as mean local govt. expenditure per capita at the county or lower level in the CZ (based on the 1992 Census of Governments). The correlation between government expenditure and upward mobility is also positive, but it is smaller than that between local tax rates and upward mobility. This could potentially be because local tax rates are used

¹⁷Government expenditures in the neighborhoods where low-income families live within the CZ (rather than average government expenditures) may be more relevant for upward mobility. To evaluate this possibility, we reconstructed each of the measures of public goods and school quality analyzed in this and the next subsection, weighting by the number of below-median income families living in each county or school district. The correlations between upward mobility and these measures of public goods for low-income individuals are very similar to those reported in Appendix Table VIII because expenditures in low-income areas are very highly correlated with mean expenditures at the CZ level.

primarily to finance schools, which may have a larger impact on upward mobility than expenditures funded by other sources of revenue.

Next, we evaluate whether areas that provide more transfers to low-income families through the tax system exhibit greater upward mobility. We use two state-level proxies for the progressivity of local tax policy. The first is the size of the state Earned Income Tax Credit. State EITC programs are the largest state-level cash transfer for low income earners. Because state EITC policies changed significantly over the period when children in our sample were growing up, we define a measure of mean exposure to the state EITC as the mean state EITC rate between 1981 and 2001, when the children in our sample were between the ages of 0 and 20.¹⁸ The mean state EITC rate is positively correlated with upward mobility, with a correlation of approximately 0.25 that is fairly robust across specifications. Our second proxy for the progressivity of the local tax code is 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 based on data from the Tax Foundation. There is a weak positive correlation between local tax progressivity and upward mobility across the various specifications in Appendix Table VIII, but the correlation is not statistically significant.

In summary, we find that areas that provide more local public goods and larger tax credits for low income families tend to have higher levels of upward mobility. However, factors such as segregation and inequality are much stronger and more robust predictors of the variation in intergenerational mobility than differences in local tax and expenditure policies.

Access to Higher Education. We construct three measures of local access to higher education using data from the Integrated Postsecondary Education Data System (IPEDS). The first measure is the number of Title IV, degree-granting colleges per capita in the CZ in 2000, which is similar to the distance-based instrument used by Card (1995). The second measure is the mean (enrollment-weighted) tuition sticker price for in-state, full-time undergraduates for colleges in the CZ, which reflects the affordability of local higher education. The third measure is the residual from an OLS regression of the mean (enrollment-weighted) graduation rate from colleges in the CZ on mean parent family income in the CZ, a rough proxy for the output of local higher education.

The correlations between all three of these measures – shown in the eighth panel of Figure VIII and Appendix Table VIII – are small and typically statistically insignificant. We also evaluated several additional measures of access to higher education, including the mean value of institutional grants to students enrolled in colleges in the CZ, the number of low-cost (below the national median) colleges per capita in the CZ, and the mean distance to the nearest low-cost college. We found no significant relationship between any of these measures and our measures of intergenerational mobility (not reported).

We conclude that very little of the spatial variation in intergenerational mobility is explained by differences in local access to higher education. Of course, this finding does not imply that college does not play a role in upward mobility. Indeed, areas with greater upward mobility tend to have high college attendance rates for children from low-income families (Appendix Figure VIIIa), suggesting that attending college is an important pathway for moving up in the income distribution. The point here is simply that the characteristics of local colleges are not a strong predictor of children’s success, perhaps because the marginal impact of improving local access to higher education on college attendance and later outcomes is small.

Labor Market Structure. Some analysts have suggested that the availability of certain types of jobs (e.g., manufacturing) may provide ladders for lower-skilled workers to move up in the income distribution (e.g., Wilson 1996). To explore this possibility, we measure various characteristics of

¹⁸We assign state-years without a state EITC a rate of 0 when computing this mean. See Appendix G for further details on the computation of state EITC rates.

the local labor market: (1) the overall employment rate in the local labor market in 2000, (2) the fraction of workers employed in the manufacturing industry, and (3) a measure of exposure to import competition based on the growth in Chinese imports per worker from Autor, Dorn and Hanson (2013). As shown in the ninth panel of Figure VIII and Appendix Table VIII, all of these characteristics are weakly correlated with the variation in upward mobility, with little evidence of a clear, robust relationship across specifications. We also find no significant correlation with other indicators such as the fraction of workers employed in management or professional occupations or industry establishment shares (not reported).

One labor market indicator that is strongly correlated with upward mobility is the teenage labor force participation rate. We measure the teenage labor force participation rate as the fraction of children who have a W-2 between the ages of 14-16 in the 1985-87 birth cohorts, the earliest cohorts for which W-2 data are available at age 14 in the tax data. The unweighted correlation between the teenage labor force participation rate and absolute upward mobility is 0.631. This could be because formal jobs help disadvantaged teenagers directly or because areas with good schools and other characteristics tend to have more teenagers who work. In either case, this finding mirrors the general pattern documented above: the strongest predictors of upward mobility are factors that affect children before they enter the labor force as adults.

Migration Rates. We evaluate whether there is a correlation between migration rates and intergenerational mobility using three measures: (1) the migration inflow rate, defined as the number of people who move into the CZ between 2004 and 2005 based on IRS Statistics of Income migration data divided by the CZ population in 2000 based on Census data, (2) the migration outflow rate, defined as the number of people who move out of the CZ between 2004 and 2005 divided by population in 2000, and (3) the fraction of foreign-born individuals living in the CZ based on the 2000 Census.

The correlations between all three of these measures – shown in the last panel of Figure VIII and Appendix Table VIII – are generally quite low and statistically insignificant. In the first two specifications, migration rates are negatively correlated with upward mobility, but in the population-weighted and urban-area specifications, there are no significant relationships.

Empirical Methodology: Appendix Table VIII. Appendix Table VIII reports each of the correlations corresponding to Figure VIII in Column 1. The remaining columns evaluate the robustness of these estimates to alternative specifications. In Column 2, we report estimates based on within-state variation by including state fixed effects in a regression specification analogous to that in Column 1 of Table IV. Column 3 replicates Column 1, weighting each CZ by its population as recorded in the 2000 Census.¹⁹ Column 4 restricts the sample to urban areas (CZs that intersect MSAs) and replicates Column 1. Column 5 replicates Column 1, controlling for the fraction of black individuals in the CZ and the local income growth rate from 2000-2010 (calculated as in Appendix G) using regression specifications of the form used in Table IV. Finally, in Column 6, we correlate each covariate with relative mobility β_c .

Empirical Methodology: Appendix Figures X-XII. Appendix Figures X-XII present binned scatter plots of absolute upward mobility ($\bar{r}_{25,c}$) in each CZ vs. various characteristics. Each figure is constructed using one observation for each CZ in which we have more than 250 parent-child pairs. To construct the binned scatter plots, we divide the variable plotted on the x-axis into 20 equally sized bins (vingtiles) and plot the mean value of the variable plotted on the y-axis (absolute upward mobility) vs. the mean value of the x variable within each bin. We also report the unweighted correlation between the x and y variables (estimated using the underlying CZ-level

¹⁹We normalize all variables by their weighted standard deviations in this and all other specifications that use weights, so that univariate regression coefficients can be interpreted as weighted correlations.

data), with standard errors clustered at the state level to correct for spatial correlation across CZs. To facilitate comparisons across figures that plot the relationship between upward mobility and different factors, we always use a fixed y scale ranging from 35 to 55, approximately the 5th to 95th percentile of the distribution of $\bar{r}_{25,c}$ across CZs.

I. Construction of Predicted Time Trends

This appendix describes the construction of Appendix Figure XIII. The series in circles is taken directly from Chetty et al. (2014, Figure 2). The solid circles show estimates of national rank-rank slopes by birth cohort using the SOI 0.1% sample. The open circles show forecasts of the rank-rank slope based on income measured at age 26 and the college attendance rates using the population data. The remaining series in the figure show predicted changes in relative mobility based on trends in the five factors that are most strongly correlated with the variation in intergenerational mobility across CZs (see Section VI). We choose proxies for the five factors that are strongly correlated with mobility in the cross section and are consistently measured over time.

We begin by describing how we construct the prediction based on changes in racial segregation, shown by the series in solid triangles. This series is constructed in four steps. (1) We regress the rank-rank slope on the Theil index of racial segregation, with one observation per CZ. This regression corresponds to the correlation reported in Row 2, Column 6 of Appendix Table VIII, except that we do not use normalized variables in this regression. (2) Using Census data from the [NHGIS](#), we compute the Theil index of racial segregation across census tracts in each CZ in 1980, 1990, and 2000, following the method described in Appendix G. We then compute the predicted value from the regression in step (1) using the population-weighted national mean of the racial segregation index in 1980, 1990, and 2000.²⁰ (3) We assign each birth cohort the predicted value when they were age 10, the mid-point of their childhood. For instance, the 1990 birth cohort is assigned the fitted value based on racial segregation in 2000. (4) We add a constant to the series to make the mean predicted values in 1970, 1980, and 1990 match the mean observed rank-rank slope (from Chetty et al. 2014) between 1971-1990. This final step normalizes the levels of the fitted values and allows us to focus on the predicted time trends.

The remaining series are constructed similarly; however, due to limitations in historical data availability, we cannot always use the same data source as the one used to estimate cross-sectional correlations. For the bottom 99% Gini coefficient, we follow Chetty et al. (2014) and use the U.S. Census Bureau’s time series ([Table F-4](#)) on the Gini coefficient for families, which we interpret as a measure of the bottom 99% Gini because of top-coding in survey data.²¹ For the religious share, we use a time series compiled by the [Association of Religion Data Archives](#) using data from the General Social Survey. We define the religious share as the share of people that attend a religious service at least once per month, the closest analog of the CZ-level definition of religious adherents that we use for the cross-sectional correlations. For the share of single mothers, we use data for the 1980, 1990, and 2000 Censuses obtained from the [NHGIS](#). For all three of these variables, we assign each birth cohort predicted values at age 10, as for racial segregation.

For the high school dropout rate, we use the same [CCD data](#) that we use for the CZ-level cross-sectional correlations. We assign each birth cohort the predicted value corresponding to the national high school dropout rate when they were 17. For simplicity, we do not residualize the HS dropout rate by income both in the cross-sectional regression and the prediction. For example, the 1997 high school dropout rate is assigned to the 1980 birth cohort. Analogous high school dropout

²⁰Tract-level data on racial shares are not available for all Census tracts in 1980; we compute the national mean using the tracts for which data are available.

²¹Chetty et al. (2014) note that there is a break in this series in 1993. We address this issue in the same manner as that paper by subtracting 0.021 from the Gini coefficient from 1993 onward.

rate data are unavailable in 1987, and hence we have no prediction for the 1970 birth cohort.

The predicted changes in the rank-rank slope from the 1970 to 1990 birth cohorts based on each of these factors are -0.024 (racial segregation), +0.026 (Gini), +0.003 (religious share), and +0.038 (fraction single mothers). The predicted change from the 1980 to 1990 birth cohort based on high school dropout rates is -0.010. Using a multivariable cross-sectional regression specification combining all five factors yields a predicted increase in the rank-rank slope from 1980 to 1990 of 0.010, or 0.001 per year. An analogous prediction for the change from 1970 to 1980 based on four factors (excluding the high school dropout rate) yields a predicted increase from 1970 to 1980 of approximately 0.014.

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ONLINE APPENDIX TABLE I
Sample Sizes vs. Vital Statistics Counts by Birth Cohort

				Base national dataset	Base CZ- level dataset
	Size of Birth Cohort (in '000s)	Percentage in DM1 database, US citizens, alive	and matched to a parent	with positive parent income in 1996-2000	and with valid parental geo information
	(1)	(2)	(3)	(4)	(5)
1977	3,327	95.9%	55.0%		
1978	3,333	97.0%	72.4%		
1979	3,494	97.6%	80.9%		
1980	3,612	99.2%	85.6%	85.2%	84.4%
1981	3,629	104.6%	91.6%	91.1%	90.3%
1982	3,681	105.5%	93.8%	93.2%	92.4%
1983	3,639	105.4%	95.4%	94.7%	93.8%
1984	3,669	105.1%	96.7%	95.8%	94.9%
1985	3,761	104.8%	97.5%	96.4%	95.4%
1986	3,757	104.7%	98.0%	96.6%	95.6%
1987	3,809	104.7%	98.4%	96.8%	95.8%
1988	3,910	104.5%	98.5%	96.8%	95.7%
1989	4,041	105.0%	98.5%	96.7%	95.6%
1990	4,158	104.7%	98.6%	96.7%	95.6%
1991	4,111	104.5%	98.5%	96.6%	95.5%
1980-1991	45,776	104.4%	96.0%	94.8%	93.8%

Notes: Column 1 reports the size of each birth cohort from 1987-1991, based on data from vital statistics obtained from the US Statistical Abstract 2012, Table 78. The remaining columns report the number of individuals in the population tax data as a percentage of the total number in the birth cohort, imposing the additional restrictions listed in the header of each column. Column 2 reports the number of individuals born in each cohort who are in the DM1 tax database, are current US citizens, and are alive in 2013. This column can differ from the birth cohort due to immigration and naturalization, emigration, and deaths before 2012. The percentage of citizens in the DM1 data rises in 1981 because citizenship status is missing for some individuals born before 1981. Column 3 further requires the individuals to be matched to parents (i.e., claimed as children dependents on individual income tax returns by a person aged 15-40 at the time of the birth of the child) in 1996 or after. Column 4, which requires in addition that parents have positive mean income between 1996-2000, is our key sample of interest for all national level statistics. Column 5 further requires valid geographical information (ZIP code) for parents. Column 5 is our key sample of interest for all local area statistics. The core sample includes the 1980-2 cohorts. The extended sample includes the 1980-91 cohorts.

ONLINE APPENDIX TABLE II
SOI Sample Counts by Birth Cohort

Cohort	Number of Observations	Number of Unique Children
	(1)	(2)
1971	4,383	4,383
1972	7,787	5,569
1973	10,831	6,154
1974	14,330	7,065
1975	17,736	8,207
1976	17,938	8,246
1977	18,459	8,156
1978	17,756	7,958
1979	18,375	7,614
1980	19,545	7,732
1981	19,916	8,155
1982	22,331	9,929
1983	24,599	10,927
1984	28,221	12,390
1985	31,711	13,476
1986	33,221	13,540
1987	35,382	14,234
1988	38,139	15,362
1989	42,450	18,162
1990	47,768	19,805
1991	52,821	21,231
Total	523,699	228,295

Notes: This table reports the sample size for the Statistics of Income stratified random sample by birth cohort. Column 1 reports the total number of observations per cohort. Column 2 reports the number of unique children per cohort. See Appendix A for details on the construction of the SOI sample.

ONLINE APPENDIX TABLE III
Summary Statistics for Core Sample: Children Born in 1980-82

Variable	Mean (1)	Std. Dev. (2)	Median (3)
<u>Parents:</u>			
Family Income (1996-2000 average)	87,219	353,430	60,129
Top Earner's Income (1999-2003 average)	68,854	830,487	48,134
Fraction Single Parents	30.6%	46.1%	
Fraction Female among Single Parents	72.0%	44.9%	
Father's Age at Child Birth	28.5	6.2	28
Mother's Age at Child Birth	26.1	5.2	26
Father's Age in 1996	43.5	6.3	43
Mother's Age in 1996	41.1	5.2	41
<u>Children:</u>			
Family Income (2011-12 average)	48,050	93,182	34,975
Fraction with Zero Family Income	6.1%	23.9%	
Individual Income	31,441	112,394	24,931
Individual Earnings	30,345	98,692	23,811
Fraction Female	50.0%	50.0%	
Fraction Single	44.3%	49.7%	
Attend College between 18-21	58.9%	49.2%	
Fraction of Females with Teen Birth	15.8%	36.5%	
Child's Age in 2011	30.0	0.8	30
Number of Children	9,867,736		

Notes: The table presents summary statistics for the core sample (1980-82 birth cohorts); see notes to Table I for further details on the definition of the core sample. Child income is mean income in 2011-12 (when the child is approximately 30 years old), while parent family income is mean income from 1996-2000. Family income is total pre-tax household income. Top earner's income is the income of the higher-earning parent from 1999-2003 (when W-2's are available). Parents' marital status is measured in the year the parent is matched to the child. Child's individual income is the sum of W-2 wage earnings, UI benefits, and SSDI benefits, and half of any remaining income reported on the 1040 form. Individual earnings includes W-2 wage earnings, UI benefits, SSDI income, and self-employment income. A child is defined as single if he/she does not file with a spouse in 2011 and 2012. College attendance is defined as ever attending college from age 18 to 21, where attending college is defined as presence of a 1098-T form. Teenage birth is defined (for females only) as having a child while being aged 19 or less. See Section III.B and Online Appendix A for additional details on sample and variable definitions. All dollar values are reported in 2012 dollars, deflated using the CPI-U.

ONLINE APPENDIX TABLE IV
Comparison of Administrative Tax Data to CPS and ACS Survey Datasets

	Tax Data Full Sample (1)	Tax Data Core Sample (2)	2011-2012 CPS (3)	2011-2012 ACS (4)	Tax Data Full Sample (5)	Tax Data Core Sample (6)	2011-2012 CPS (7)	2011-2012 ACS (8)
<i>Income Distribution:</i>	<i>Earned Family Income</i>				<i>Total Family Income</i>			
% Zero	9.74%	7.32%	9.23%	12.64%	8.54%	6.11%	5.44%	8.00%
% Negative	0.00%	0.00%	0.00%	0.00%	0.33%	0.34%	0.04%	0.05%
Mean	44,278	46,805	54,313	42,382	45,406	48,050	56,438	44,845
Std. Deviation	104,528	109,667	58,556	47,879	90,594	93,182	59,145	50,072
P10	63	1,624	1,307	0	521	2,810	6,431	1,500
P25	12,724	14,984	18,843	12,000	12,842	14,919	20,414	14,000
P50	32,165	34,737	40,829	31,642	32,273	34,975	42,768	33,000
P75	62,095	65,148	75,000	57,000	62,992	66,169	76,554	60,000
P90	96,995	99,911	115,000	91,865	98,802	101,770	118,050	96,243
<i>Demographics:</i>								
% Married	42.43%	44.31%	49.32%	46.17%				
% Female	50.03%	49.97%	50.43%	49.98%				
% Live in South	36.83%	37.94%	38.33%	37.56%				
% College	54.62%	58.93%	66.20%	61.34%				
Observations	11,262,459	9,867,736	14,246	190,561	11,262,459	9,867,736	14,246	194,501
Sum of Samp. Weights	11,262,459	9,867,736	10,845,147	11,043,039	11,262,459	9,867,736	10,845,147	11,043,039

Notes: Columns (1) and (5) include all individuals in the Data Master-1 file from the SSA who were born in 1980-1982, are current U.S. citizens, and lived through 2012. In Columns (2) and (6), we impose the additional restriction that an individual was claimed as a dependent on a tax return in the years 1996-2012 by parents with positive income as described in the text. CPS sample consists of civilian, non-institutionalized citizens age 29-31 in the 2011 wave and 30-32 in 2012 waves of the Current Population Survey. ACS sample consists of civilian, non-institutionalized citizens born between 1980-1982 in the 2011 and 2012 American Community Surveys. Earned income refers to wages and salary plus social security and unemployment insurance plus positive self-employment income, except for the ACS measure, which does not include unemployment insurance. IRS wages and salary income is defined as the amount of all wages, tips, and other compensation before any payroll deductions (total of all amounts reported on all Forms W-2, Box 1). IRS unemployment compensation is defined as the amount of Unemployment Compensation and Railroad Retirement Board payments prior to tax withholding as reported on Form 1099-G, Box 1. IRS social security income is defined as total Social Security Administration benefits, as reported on Form SSA-1099 (as well as any Railroad Retirement Board benefits paid, as reported on Form RRB-1099, Box 3). IRS self-employment income is defined as the profit reported on Form 1040 Schedule C. In the CPS, self-employment income is business income; in the ACS, it is both farm and non-farm business income. In the tax data, total income is the sum of Adjusted Gross Income, social security, and tax exempt interest. Total income in CPS and ACS is all reported income including negative business and investment income. All dollar amounts are in 2012 dollars. Married refers to filing of joint return in 2011-2012 period for the tax data, and self-report of currently married in CPS/ACS samples. College means attended a degree granting institution between the ages of 18 and 21 in the tax data and self-report of more than high school attainment in CPS/ACS samples. South refers to filing a federal tax return in (for tax data) or being surveyed in (for ACS/CPS) one of the following states: DE, DC, FL, GA, MD, NC, SC, VA, WV, AL, KY, MS, TN, AR, LA, OK, TX. ACS and CPS moments computed using sampling weights (inverse probability of inclusion in sample). For the ACS and CPS, the sum of the sample weights is the average of the sum of the sample weights in 2011 and in 2012.

ONLINE APPENDIX TABLE V
Estimates of Intergenerational Mobility Using Surname Means vs. Individual Incomes

Name Freq. Restriction	Number of Children (1)	Number of Names (2)	Rank-Rank Slope		Log-Log IGE	
			Surname (3)	Individual (4)	Surname (5)	Individual (6)
1. No restriction	4,843,629	395,439	0.39	0.30	0.42	0.33
2. < 25	1,135,624	375,753	0.30	0.27	0.33	0.30
3. < 50	1,437,280	384,576	0.31	0.27	0.34	0.30
4. < 100	1,784,635	389,611	0.33	0.28	0.36	0.31
5. > 100	3,053,494	5,773	0.46	0.31	0.50	0.33
6. > 1,000	1,650,583	546	0.41	0.31	0.43	0.34
7. > 10,000	390,187	22	0.41	0.33	0.45	0.35
8. > 20,000	202,734	7	0.75	0.34	0.81	0.36

Notes: This table compares estimates of rank-rank slopes and log-log IGEs based on individual-level data to estimates based on surname means, as in Clark (2014). In this table, we restrict the core sample to children who have the same surname (in 2012) as their parents. The first row uses all children who satisfy this restriction. Rows 2-4 limit the sample to rare surnames: those that occur less than 25 times, 50 times, and 100 times in the sample. Conversely, rows 5-8 limit the sample to common surnames: those that occur more than 100, 1000, 10,000, and 20,000 times. Column 1 shows the number of children in each subsample (i.e., the number of observations used to estimate the rank-rank slope). Column 2 shows the number of distinct surnames in each sample. We estimate the individual-level rank-rank slopes and log-log IGE's (Columns 4 and 6) using OLS regressions on the microdata as in Table I. In Columns 3 and 5, we estimate the rank-rank slopes and log-log IGE's using OLS regressions on a dataset collapsed to surname-level means, weighting by the number of observations for each name. See Appendix D for further details.

ONLINE APPENDIX TABLE VI
National Quintile Transition Matrix: 1980-85 Cohorts

		Parent Quintile				
		1st	2nd	3rd	4th	5th
Child Quintile	1	33.1%	24.1%	17.7%	13.5%	11.7%
	2	27.7%	24.0%	19.6%	16.1%	12.6%
	3	18.7%	21.6%	21.9%	20.7%	17.0%
	4	12.7%	17.7%	21.8%	24.1%	23.7%
	5	7.8%	12.6%	18.9%	25.6%	35.1%

Notes. Each cell reports the percentage of children with family income in the quintile given by the row conditional on having parents with family income in the quintile given by the column for children in the 1980-85 birth cohorts. See notes to Table I for income and sample definitions. See Table II for an analogous transition matrix constructed using the 1980-82 birth cohorts.

ONLINE APPENDIX TABLE VII
Robustness of Spatial Variation in Intergenerational Mobility to Alternative Specifications

Change from Baseline Specification	Correlation with Baseline Mobility Estimates and Ratio of Std. Dev.			
	Upward mobility	Relative mobility	Upward mobility	Relative mobility
	Unweighted (1)	Unweighted (2)	Pop. Weighted (3)	Pop. Weighted (4)
<i>A. Alternative Samples</i>				
1. Male children	0.99, 1.07	0.94, 1.03	0.98, 1.07	0.98, 1.01
2. Female children	0.98, 0.96	0.95, 1.09	0.97, 0.98	0.98, 1.02
3. Children of married parents	0.97, 0.96	0.89, 0.92	0.91, 1.02	0.93, 0.89
4. Children of single parents	0.97, 0.97	0.61, 1.14	0.97, 0.96	0.83, 1.02
5. Birth cohorts 1983-85	0.97, 1.00	0.84, 1.08	0.96, 0.93	0.96, 1.05
6. Birth cohorts 1986-88	0.94, 0.95	0.73, 1.11	0.82, 0.91	0.88, 0.98
7. Parent age at child birth within 5 years of median	0.98, 1.06	0.90, 1.16	0.98, 1.05	0.96, 1.02
8. Children who stay within CZ	0.94, 1.02	0.87, 1.09	0.93, 1.12	0.95, 1.04
9. Children matched to unique parents	0.99, 0.93	0.98, 0.90	0.98, 0.93	0.99, 0.93
<i>B. Alternative Income Definitions</i>				
10. Top parent income	1.00, 1.05	0.97, 1.00	0.99, 1.04	0.99, 0.99
11. Individual child income	0.94, 0.74	0.89, 0.80	0.83, 0.97	0.95, 0.81
12. Individual child earnings	0.93, 0.72	0.86, 0.82	0.82, 0.93	0.93, 0.82
13. Individual child income (males only)	0.96, 1.03	0.90, 1.01	0.96, 1.14	0.95, 0.97
14. Indiv child income and top parent income (males only)	0.97, 1.07	0.87, 1.02	0.97, 1.15	0.94, 0.95
15. Parent Income 1999-2003	1.00, 0.99	0.98, 0.99	1.00, 0.98	1.00, 0.99
<i>C. Adjustments for Cost of Living and Growth Rates</i>				
16. Cost of living adjusted income	0.98, 1.06	0.99, 0.99	0.86, 1.01	0.99, 0.97
17. Parent income measured in 2011/12	0.97, 0.90	0.92, 0.89	0.94, 0.95	0.98, 0.93
18. Controlling for growth	0.83, 0.83	0.92, 0.92	0.81, 0.83	0.96, 0.97
<i>D. Alternative Measures of Mobility</i>				
19. Within-CZ ranks		0.95, 0.94		0.96, 0.98
20. Prob. Child in Q5 Parent in Q1	0.91		0.92	
21. Child income > poverty line	0.94		0.89	
<i>E. Alternative Child Outcomes</i>				
22. College Attendance (age 18-21)	0.71	0.68	0.53	0.72
23. College Quality (age 20)	0.71	0.51	0.55	0.65
24. Teenage Birth, females only	-0.61	-0.58	-0.64	-0.68

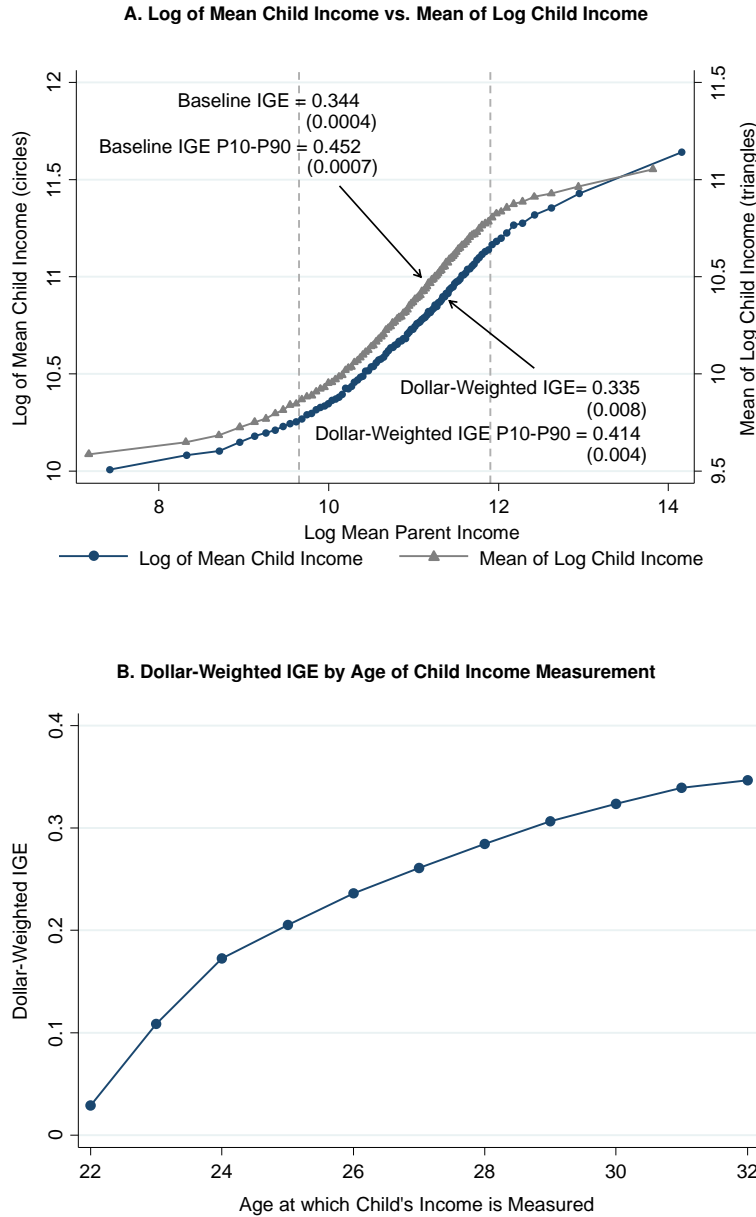
Notes: The first number in each cell of this table reports the correlation across CZs of a baseline mobility measure (using child family income rank and parent family income rank in the core sample) with an alternative mobility measure. The second number in each cell reports the ratio of the standard deviation of the alternative measure to the baseline measure. We do not report the ratio of standard deviations for statistics that are measured in different units relative to the corresponding baseline measure. The alternative mobility measures are defined either using a different sample (Panel A), a different income measure for parents or children (Panel B), adjusting for cost of living or local growth (Panel C), using alternative statistics for mobility (Panel D), or using earlier outcomes (Panel E). Column (1) reports the unweighted correlation (and SD ratio) between the alternative and baseline measure of absolute upward mobility, the expected rank of children whose parents are at the 25th national percentile in the core sample. Column (2) reports the unweighted correlation (and SD ratio) between the alternative and baseline measure of relative mobility, the slope of the rank-rank relationship in the core sample. Columns (3) and (4) repeat Columns (1) and (2), weighting the correlations and standard deviations by CZ population as recorded in the 2000 Census. With the exception of the transition probability in row 20, all absolute and relative mobility measures are constructed using OLS regressions of child outcomes on parent ranks as described in the text. Ranks are always defined in the full sample, prior to defining specific subsamples, except in row 19. See Appendix F for details on the definition of each measure.

ONLINE APPENDIX TABLE VIII
Correlates of Intergenerational Mobility Across Commuting Zones

Dep. Var.:		Absolute Upward Mobility								Relative Mobility			
		Baseline		State FEs		Pop. Weighted		Urban Areas Only				Controls	
		(1)		(2)		(3)		(4)		(5)		(6)	
Race	Fraction Black Residents	-0.580	(0.066)	-0.353	(0.048)	-0.616	(0.074)	-0.673	(0.063)			0.631	(0.048)
Segregation	Racial Segregation Theil Index	-0.361	(0.045)	-0.274	(0.027)	-0.311	(0.092)	-0.360	(0.068)	-0.273	(0.046)	0.406	(0.048)
	Income Segregation Theil Index	-0.393	(0.065)	-0.260	(0.036)	-0.169	(0.105)	-0.184	(0.068)	-0.267	(0.054)	0.183	(0.063)
	Segregation of Poverty (<p25)	-0.407	(0.066)	-0.261	(0.038)	-0.216	(0.098)	-0.210	(0.066)	-0.274	(0.054)	0.218	(0.059)
	Segregation of Affluence (>p75)	-0.369	(0.064)	-0.250	(0.035)	-0.142	(0.106)	-0.155	(0.070)	-0.250	(0.052)	0.146	(0.063)
	Share with Commute < 15 Mins	0.605	(0.126)	0.342	(0.092)	0.335	(0.115)	0.548	(0.080)	0.415	(0.131)	-0.447	(0.074)
Income Distribution	Household Income per Capita for Working-Age Adults	0.050	(0.071)	-0.013	(0.075)	0.046	(0.092)	0.043	(0.076)	0.064	(0.080)	-0.145	(0.081)
	Gini coefficient for Parent Income	-0.578	(0.093)	-0.281	(0.050)	-0.236	(0.162)	-0.537	(0.120)	-0.362	(0.086)	0.346	(0.089)
	Top 1% Income Share for Parents	-0.190	(0.072)	-0.065	(0.031)	0.059	(0.094)	-0.144	(0.069)	-0.072	(0.065)	0.019	(0.063)
	Gini Bottom 99%	-0.647	(0.092)	-0.433	(0.063)	-0.416	(0.123)	-0.616	(0.114)	-0.470	(0.104)	0.473	(0.090)
	Fraction Middle Class (Between National p25 and p75)	0.679	(0.111)	0.500	(0.102)	0.293	(0.129)	0.551	(0.126)	0.458	(0.145)	-0.451	(0.109)
K-12 Education	School Expenditure per Student	0.246	(0.095)	0.026	(0.099)	0.219	(0.088)	0.236	(0.092)	0.053	(0.082)	-0.279	(0.092)
	Teacher-Student Ratio	-0.328	(0.100)	-0.213	(0.128)	0.062	(0.139)	0.024	(0.104)	-0.249	(0.088)	0.009	(0.108)
	Test Score Percentile (Controlling for Parent Income)	0.588	(0.087)	0.466	(0.074)	0.176	(0.220)	0.413	(0.147)	0.393	(0.093)	-0.317	(0.122)
	High School Dropout Rate (Controlling for Parent Income)	-0.574	(0.089)	-0.413	(0.060)	-0.433	(0.100)	-0.441	(0.108)	-0.440	(0.086)	0.328	(0.099)
Social Capital	Social Capital Index (Rupasingha and Goetz 2008)	0.641	(0.091)	0.349	(0.092)	0.299	(0.131)	0.517	(0.116)	0.478	(0.097)	-0.327	(0.085)
	Fraction Religious	0.521	(0.085)	0.357	(0.061)	0.410	(0.096)	0.417	(0.096)	0.484	(0.065)	-0.101	(0.090)
	Violent Crime Rate	-0.380	(0.146)	-0.163	(0.058)	-0.149	(0.166)	-0.367	(0.145)	-0.244	(0.062)	0.217	(0.140)
Family Structure	Fraction of Children with Single Mothers	-0.764	(0.074)	-0.571	(0.085)	-0.613	(0.129)	-0.719	(0.063)	-0.611	(0.066)	0.641	(0.046)
	Fraction of Adults Divorced	-0.486	(0.100)	-0.333	(0.085)	-0.389	(0.074)	-0.346	(0.103)	-0.569	(0.086)	0.158	(0.088)
	Fraction of Adults Married	0.571	(0.062)	0.417	(0.063)	0.221	(0.127)	0.377	(0.069)	0.365	(0.089)	-0.370	(0.078)
Tax	Local Tax Rate	0.325	(0.070)	0.135	(0.073)	0.155	(0.092)	0.182	(0.073)	0.207	(0.071)	-0.328	(0.061)
	Local Government Expenditures per Capita	0.186	(0.083)	0.074	(0.028)	0.192	(0.087)	0.085	(0.079)	0.107	(0.083)	-0.301	(0.080)
	State EITC Exposure	0.245	(0.064)			0.279	(0.076)	0.355	(0.073)	0.163	(0.073)	-0.144	(0.047)
	State Income Tax Progressivity	0.207	(0.146)			0.265	(0.070)	0.198	(0.098)	0.155	(0.133)	-0.150	(0.106)
College	Number of Colleges per Capita	0.200	(0.114)	-0.015	(0.118)	0.108	(0.088)	-0.045	(0.076)	0.060	(0.142)	-0.125	(0.052)
	Mean College Tuition	-0.018	(0.067)	-0.044	(0.039)	0.058	(0.096)	-0.015	(0.087)	-0.029	(0.066)	0.109	(0.064)
	College Graduation Rate (Controlling for Parent Income)	0.155	(0.062)	0.141	(0.052)	0.107	(0.089)	0.120	(0.095)	0.173	(0.073)	-0.025	(0.057)
Local Labor Market	Labor Force Participation Rate	0.212	(0.086)	-0.045	(0.052)	0.022	(0.090)	0.267	(0.113)	0.146	(0.073)	-0.237	(0.082)
	Fraction Working in Manufacturing	-0.261	(0.091)	0.007	(0.079)	-0.158	(0.090)	-0.128	(0.096)	0.002	(0.085)	0.393	(0.070)
	Growth in Chinese Imports 1990-2000 (Autor and Dorn 2013)	-0.175	(0.078)	0.006	(0.023)	0.001	(0.070)	0.008	(0.102)	-0.107	(0.048)	0.171	(0.083)
	Teenage (14-16) Labor Force Participation Rate	0.631	(0.087)	0.358	(0.098)	0.299	(0.153)	0.540	(0.109)	0.388	(0.090)	-0.516	(0.084)
Migration	Migration Inflow Rate	-0.258	(0.074)	-0.186	(0.049)	-0.146	(0.076)	-0.040	(0.078)	-0.285	(0.069)	-0.084	(0.067)
	Migration Outflow Rate	-0.163	(0.070)	-0.162	(0.048)	0.062	(0.094)	0.013	(0.076)	-0.145	(0.071)	-0.150	(0.070)
	Fraction of Foreign Born Residents	-0.027	(0.064)	-0.014	(0.039)	0.237	(0.083)	0.092	(0.064)	-0.004	(0.051)	-0.247	(0.055)

Notes: Each cell reports estimates from OLS regressions of a measure of mobility on the variable listed in each row, normalizing both the dependent and independent variables to have mean 0 and standard deviation 1 in the estimation sample, so that univariate regression coefficients equal correlation coefficients. Standard errors, reported in parentheses, are clustered at the state level. The dependent variable in Columns 1-5 is our baseline measure of absolute upward mobility, the expected rank of children whose parents are at the 25th national percentile. The dependent variable in Column 6 is relative mobility, the rank-rank slope in each CZ. All mobility estimates are constructed using the core sample (1980-82 cohorts) and baseline family income measures. Column 1 reports estimates from univariate unweighted regressions (raw correlation coefficients). Column 2 adds state fixed effects. Column 3 weights by Census 2000 population (and normalizes variables by weighted standard deviations). In Column 4, we restrict to CZs that intersect a Metropolitan Statistical Area. In Column 5 we control for the black share and income growth between 2000 and 2006-2010 as measured in Census data. The typical sample in Column 4 consists of 325 CZs that intersect MSAs. In the other columns the typical sample consists of the 709 CZs with at least 250 children in the core sample; however, some rows have fewer observations due to missing values for the independent variable. See Section VI, Online Data Table IX, and Appendix G for definitions of each of the correlates analyzed in this table. See Online Data Table VIII for the CZ-level data on each covariate.

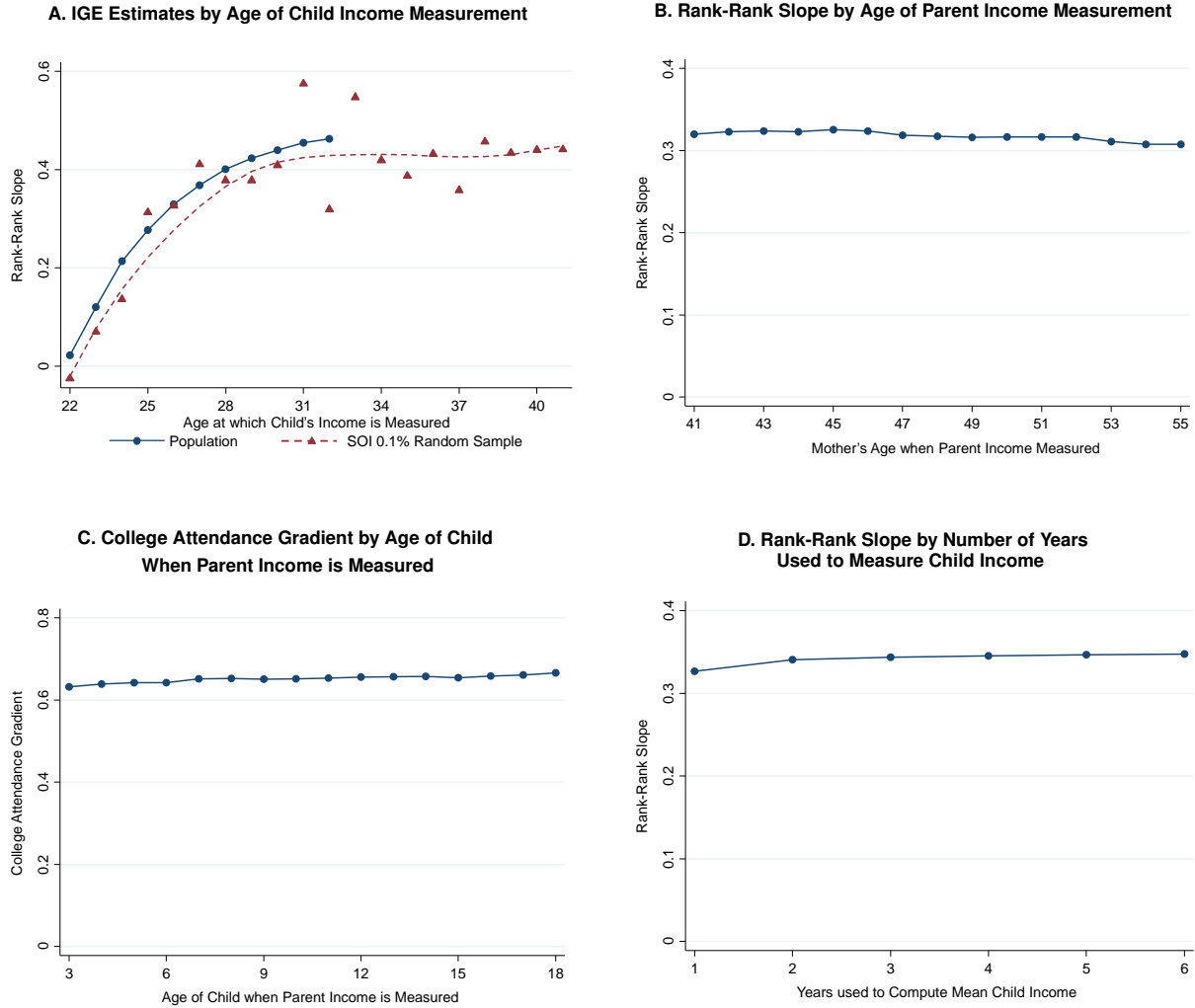
ONLINE APPENDIX FIGURE I Dollar-Weighted vs. Traditional IGE Estimates



Notes: This figure compares dollar-weighted (Mitnik et al. 2014) and traditional IGE estimates. Panel A is based on the core sample (1980-82 birth cohorts) and baseline family income definitions for parents and children. The series in circles (left axis) plots log of mean child income against log of mean parent income. The series is constructed by taking the logs of the points in Figure Ia; however, here we do not omit the top income bin. The slope coefficients, which correspond to the dollar-weighted IGE defined in Appendix C, and standard errors are estimated by OLS on the binned data. The series in triangles (right axis) reports the mean of log child income vs. the mean of log parent income (reproducing the series in Figure Ib). The slope coefficients and standard errors for the traditional IGE are estimated on the microdata. The dashed lines in Panel A show the 10th and 90th percentiles of the parent income distribution. Panel B shows how the dollar-weighted IGE varies with the age at which child income is measured. We estimate the dollar-weighted IGE by grouping parents into 100 bins based on their income rank and regressing the log of mean child income on the log of mean parent income across the 100 bins. The figure plots the slope from this regression vs. the age at which child income is measured. We measure child income in 2011-12 and analyze how the IGE varies across birth cohorts, as in Figure IIIa; see notes to that figure for further details. The first point corresponds to the children in the 1990 birth cohort, who are 21-22 when their incomes are measured in 2011-12 (denoted by age 22 on the figure). The last point corresponds to the 1980 cohort, who are 31-32 (denoted by 32) when their incomes are measured.

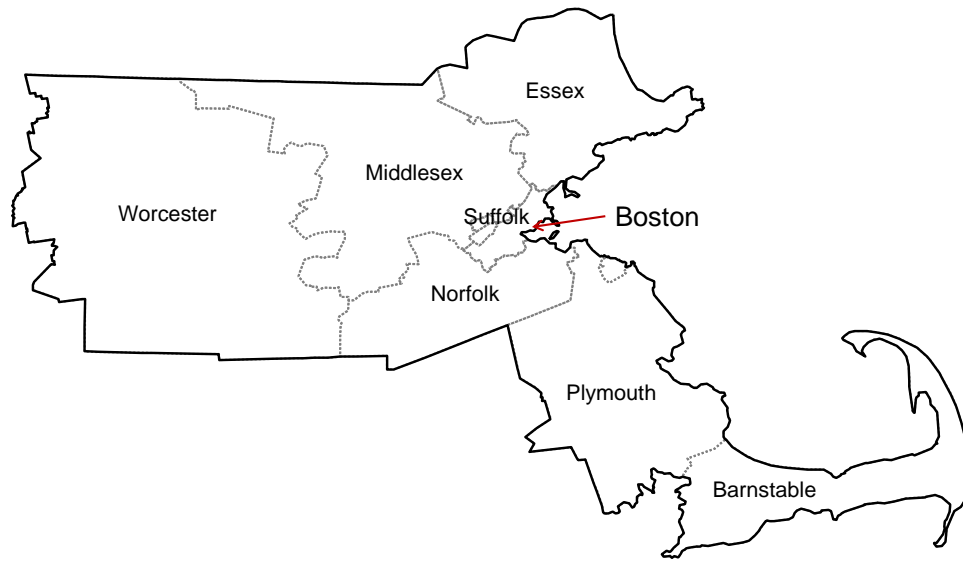
ONLINE APPENDIX FIGURE II

Additional Evidence on Robustness of Intergenerational Mobility Estimates



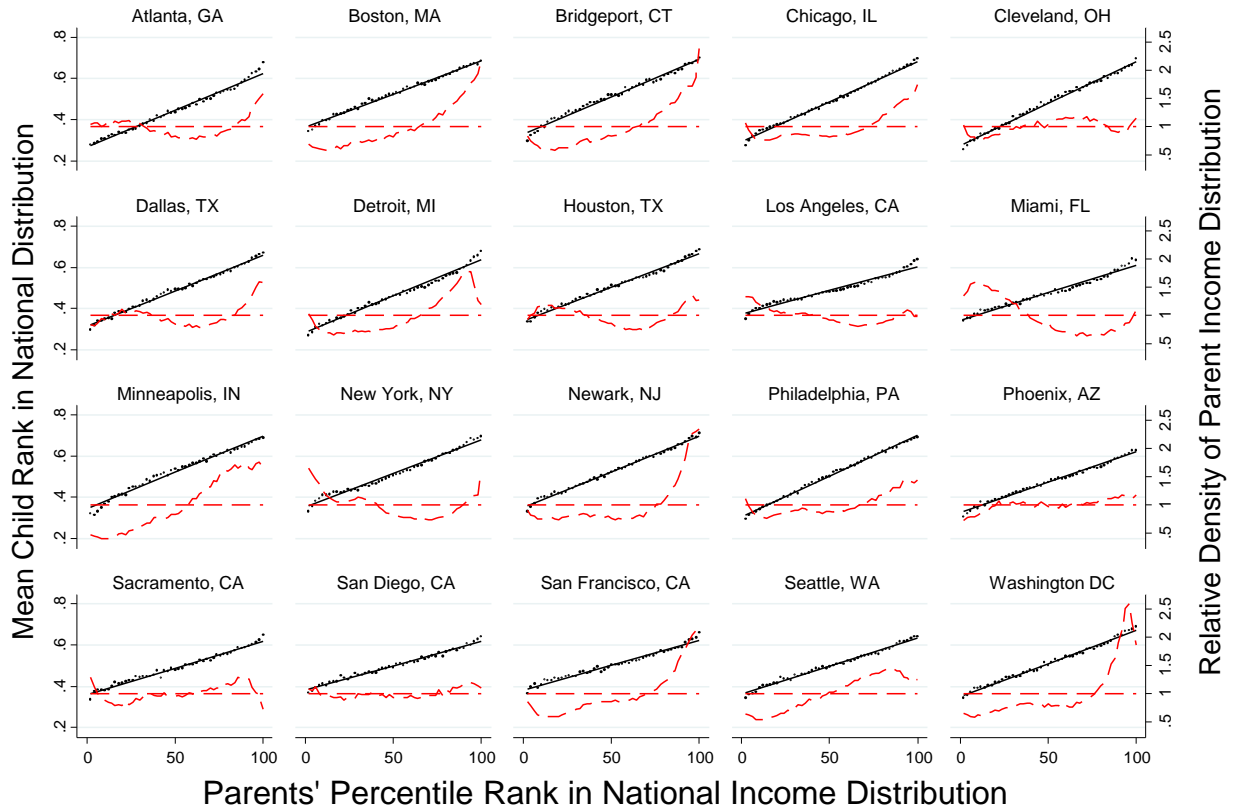
Notes: This figure evaluates the robustness of intergenerational mobility measures to lifecycle and attenuation bias. Panel A evaluates the robustness of the IGE to changes in the age at which child income is measured. Panel B evaluates the robustness of the rank-rank slope to changes in the age at which parent income is measured. Panel C evaluates the robustness of the college attendance gradient to the age of the child when parent income is measured. Panel D evaluates the robustness of the rank-rank slope to the number of years used to measure the child's income. In Panel A, we estimate the log-log IGE (excluding children with zero income), varying the age at which child income is measured. We restrict the sample to parents with income between the 10th and 90th percentile when estimating the IGE, as shown in Figure 1b. We measure child income in 2011-12 and analyze how the IGE varies across birth cohorts, as in Figure 3IIa; see notes to that figure for further details. In Panel B, each point shows the slope coefficient from an OLS regression of child income rank on parent income rank (as in Figure 1Ia), using the core sample and varying the age at which parent income rank is measured. The first point measures parent income in 1996, when the mean age of mothers is 41. The last point measures income in 2010, when parents are 55. Panel C reproduces Appendix Figure 2b from Chetty et al. (2014). In this figure, each point shows the slope coefficient from an OLS regression of an indicator for the child attending college at age 19 on parent income rank (similar to Figure 4Va), varying the year in which parent income rank is measured from 1996 to 2011. In this series, we use data from the 1993 birth cohort, which allows us to analyze parent income starting when children are 3 years old in 1996. We list the age of the child on the x axis to evaluate whether the gradient differs when children are young (although parent age is of course also rising in lockstep). In Panel D, each point shows the slope coefficient from the same rank-rank regression as in Panel B using the core sample, but here we always use a five-year (1996-2000) mean to measure parent income and vary the number of years used to compute mean child income. The point for one year measures child income in 2012 only. The point for two years uses mean child income in 2011-12. We continue adding data for prior years; the 6th point uses mean income in years 2007-2012.

ONLINE APPENDIX FIGURE III
Boston Commuting Zone



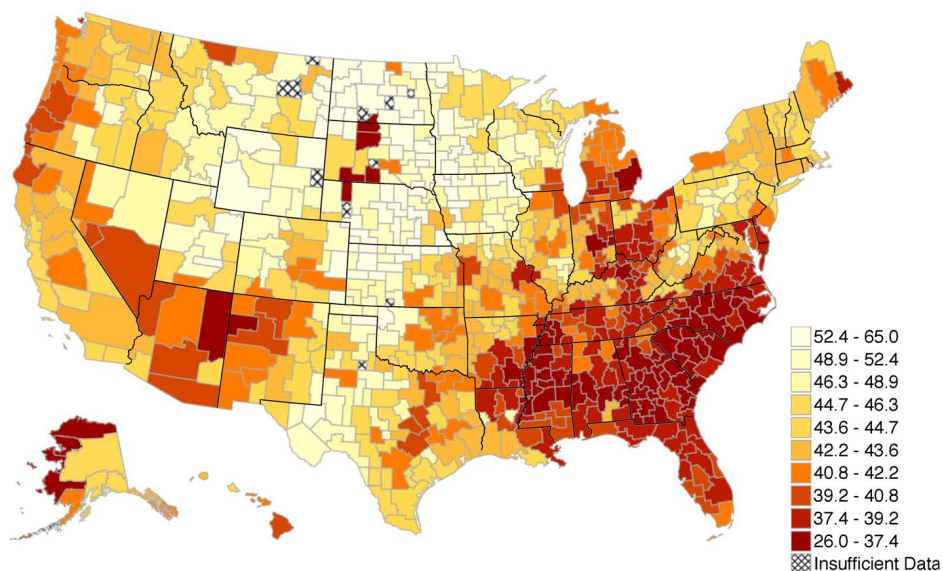
Notes: This figure shows a map of the counties that comprise the Boston Commuting Zone. The city of Boston is shown by the arrow.

ONLINE APPENDIX FIGURE IV Rank-Rank Relationships and Income Distributions in the 20 Largest CZs



Notes: These figures present non-parametric binned scatter plots (shown by the points and solid line, left y-axis) of the relationship between child and parent income ranks in the twenty largest CZs based on population in the 2000 Census. All figures are based on the core sample (1980-82 birth cohorts) and baseline family income definitions for parents and children. Children are assigned to commuting zones based on the location of their parents. Parent and child percentile ranks are always defined at the national level, not the CZ level. To construct each rank-rank series, we group parents into 50 equally sized (two percentile point) bins and plot the mean child percentile rank vs. the mean parent percentile rank within each bin. The dashed curve (right y-axis) in each panel depicts the income distribution in the CZ relative to the national distribution. This curve plots the share of parents with income in each bin in the CZ divided by the share in the same bin in the national income distribution. By construction, this curve averages to one in each CZ, shown by the horizontal dashed line in each panel.

ONLINE APPENDIX FIGURE V
Estimates of Absolute Upward Mobility Pooling 1980-82 and 1980-85 Cohorts

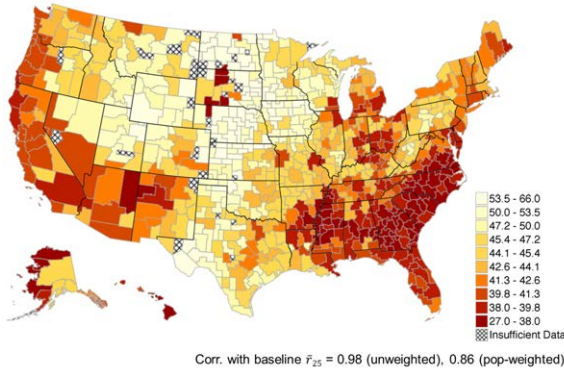


Notes: The figure presents the map of absolute upward mobility by CZ shown on the project homepage (www.equality-of-opportunity.org). For the 709 CZs that have at least 250 children in the 1980-82 cohorts, we compute absolute upward mobility exactly as in Figure VIa. For an additional 22 CZs that have fewer than 250 children in the 1980-82 cohorts but at least 250 children in the 1980-85 cohorts, we report estimates of absolute upward mobility using the 1980-85 birth cohorts. We estimate absolute upward mobility using exactly the same procedure as described in the notes to Figure VIa. The map is constructed by grouping CZs into ten deciles based on the hybrid absolute mobility measure and shading the areas so that lighter colors correspond to higher absolute mobility. Areas with fewer than 250 children in the 1980-85 cohorts are shaded with the cross-hatch pattern. The CZ-level statistics underlying this map are reported in Online Data Table V.

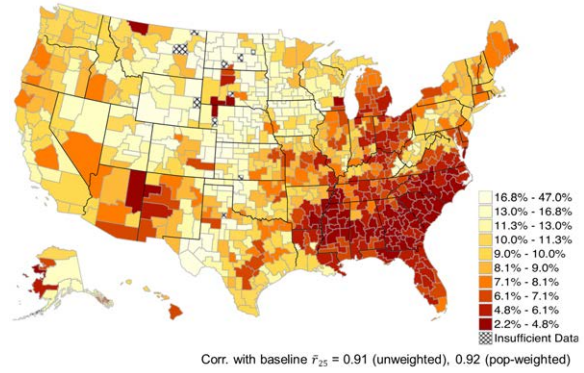
ONLINE APPENDIX FIGURE VI

Alternative Measures of Upward Mobility

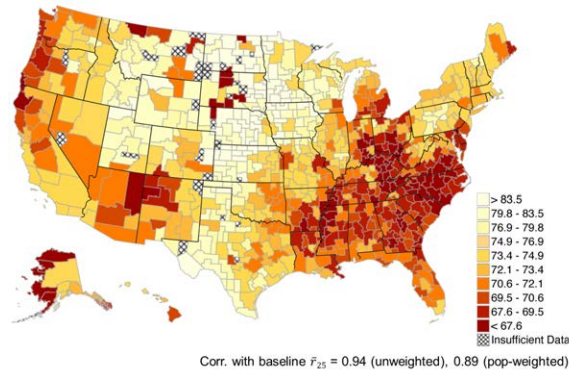
A. Absolute Upward Mobility Adjusted for Local Cost-of-Living



B. Probability of Reaching Top Quintile from Bottom Quintile



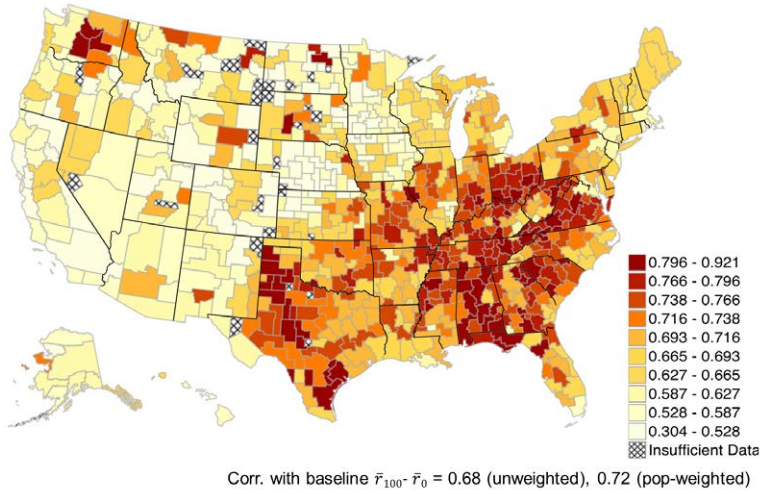
C. Fraction of Children Above Poverty Line Given Parents at 25th Percentile



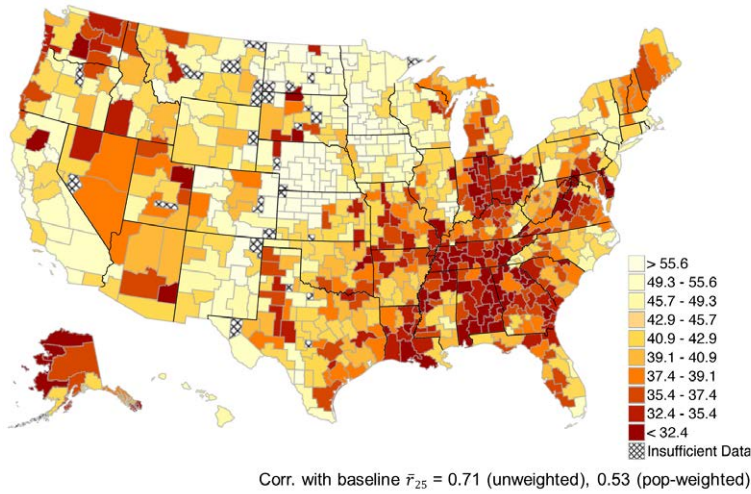
Notes: These figures present heat maps for alternative measures of upward income mobility. Children are assigned to commuting zones based on the location of their parents (when the child was claimed as a dependent), irrespective of where they live as adults. All panels use baseline family income definitions for parents. Panels A and C use the core sample (1980-82 birth cohorts) and panel B uses the 1980-85 birth cohorts. Panel A replicates Figure VIa, adjusting for differences in cost-of-living across areas. To construct this figure, we first deflate parent income by a cost-of-living index (COLI) for the parent's CZ when he/she claims the child as a dependent and child income by a COLI for the child's CZ in 2012. We then compute parent and child ranks using the resulting real income measures and replicate the procedure in Figure VIa exactly. The COLI is constructed using data from the ACCRA price index combined with information on housing values and other variables as described in Appendix A. Panel B presents a heat map of the probability that a child reaches the top quintile of the national family income distribution for children conditional on having parents in the bottom quintile of the family income distribution for parents. These probabilities are taken directly from Online Data Table VI. Panel C shows the fitted values at parent rank 25 from a regression of an indicator for child family income being above the poverty line on parent income rank (see Appendix F for details). The maps are constructed by grouping CZs into ten deciles and shading the areas so that lighter colors correspond to higher mobility. Areas with fewer than 250 children in the core sample (or the 1980-85 cohorts for Panel B), for which we have inadequate data to estimate mobility, are shaded with the cross-hatch pattern. We report the unweighted and population-weighted correlation coefficient across CZs between these mobility measures and the baseline measure in Figure VIa. The CZ-level statistics underlying Panels A and C are reported in Online Data Table V.

ONLINE APPENDIX FIGURE VII The Geography of College Attendance by Parent Income Gradients

A. Slope of College Attendance-Parent Rank Gradients by CZ



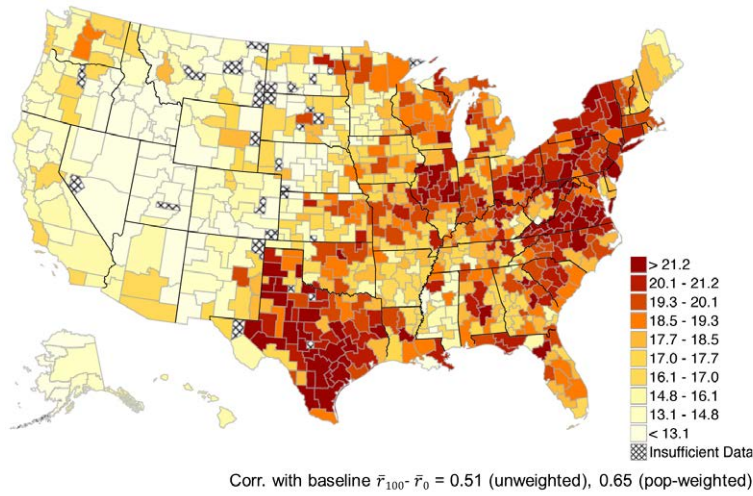
B. College Attendance Rates for Children with Parents at the 25th Percentile by CZ



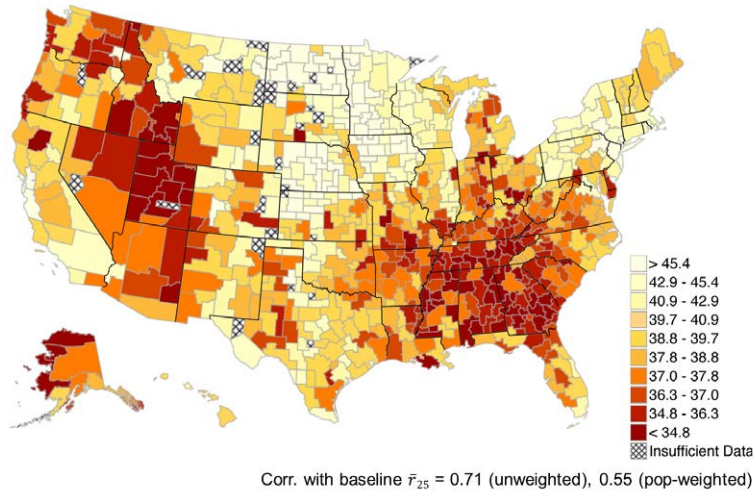
Notes: To construct these figures, we regress an indicator for college attendance on parent income rank (in the national distribution) for each CZ separately. College attendance is defined by the presence of a 1098-T form filed by a college on behalf of the student. We use the core sample (1980-82 birth cohorts) and baseline family income definitions for parents. Children are assigned to commuting zones based on the location of their parents (when the child was claimed as a dependent), irrespective of where they live as adults. In Panel A, we map the slope coefficients on the college attendance indicator from the CZ-level regressions. Panel B maps the fitted values from the regressions at parent rank 25. The maps are constructed by grouping CZs into ten deciles and shading the areas so that lighter colors correspond to higher mobility (smaller slopes in Panel A and higher fitted values in Panel B). Areas with fewer than 250 children in the core sample, for which we have inadequate data to estimate mobility, are shaded with the cross-hatch pattern. We report the unweighted and population-weighted correlation coefficients across CZs between these mobility measures and the baseline measures in Figure VI. The CZ-level statistics underlying these figures are reported in Online Data Table V.

ONLINE APPENDIX FIGURE VIII The Geography of College Quality by Parent Income Gradients

A. College Quality Gradient (P75-P25) by CZ



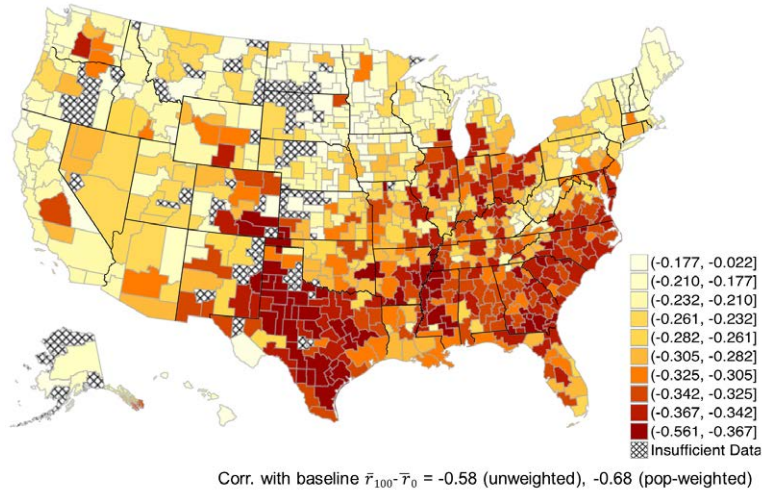
B. Mean College Quality Rank for Children with Parents at the 25th Percentile by CZ



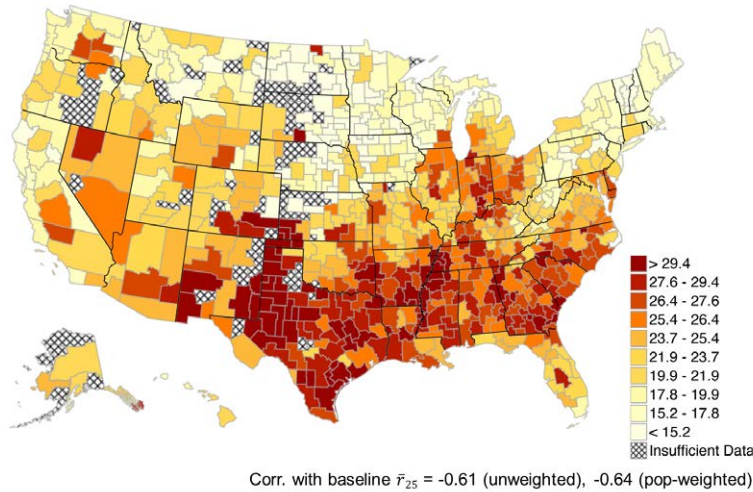
Notes: To construct these figures, we regress college quality rank on a quadratic in parent income rank (in the national distribution) for each CZ separately. College quality rank is defined as the percentile rank of the college that the child attends at age 20 based on the mean earnings at age 31 of children who attended the same college (children who do not attend college are included in a separate “no college” group); see Section III.B for further details. We use the core sample (1980-82 birth cohorts) and baseline family income definitions for parents. Children are assigned to commuting zones based on the location of their parents (when the child was claimed as a dependent), irrespective of where they live as adults. In Panel A, we map the college quality income gradient, defined as the difference between the fitted values at parent rank 75 and parent rank 25 from the CZ-level regressions. Panel B maps the fitted values of college quality rank at parent rank 25 from these regressions. The maps are constructed by grouping CZs into ten deciles and shading the areas so that lighter colors correspond to higher mobility (smaller gradients in Panel A and higher fitted values in Panel B). Areas with fewer than 250 children in the core sample, for which we have inadequate data to estimate mobility, are shaded with the cross-hatch pattern. We report the unweighted and population-weighted correlation coefficients across CZs between these mobility measures and the baseline measures in Figure VI. The CZ-level statistics underlying these figures are reported in Online Data Table V.

ONLINE APPENDIX FIGURE IX The Geography of Teenage Birth by Parent Income Gradients

A. Slope of Teenage Birth-Parent Rank Gradients by CZ



B. Teenage Birth Rates for Children with Parents at the 25th Percentile by CZ

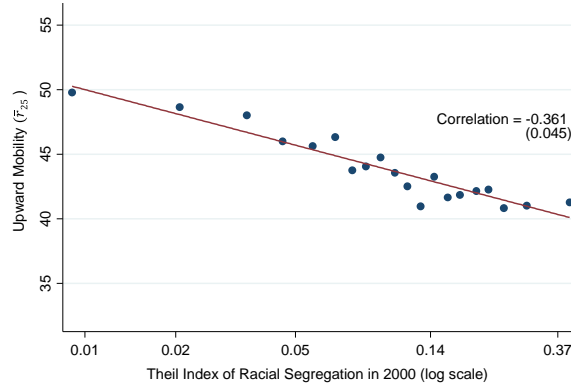


Notes: To construct these figures, we regress an indicator for teenage birth on parent income rank (in the national distribution) for each CZ separately. Teenage birth is defined as ever claiming a dependent child who was born while the mother was aged 13-19. We use female children in the core sample (1980-82 birth cohorts) and baseline family income definitions for parents. Children are assigned to commuting zones based on the location of their parents (when the child was claimed as a dependent), irrespective of where they live as adults. In Panel A, we map the slope coefficient on the teenage birth indicator from the CZ-level regressions. Panel B maps the fitted values from these regressions at parent income rank 25. The maps are constructed by grouping CZs into ten deciles and shading the areas so that lighter colors correspond to smaller slopes (in magnitudes) in Panel A and smaller fitted values in Panel B. Areas with fewer than 250 female children in the core sample, for which we have inadequate data to estimate mobility measures, are shaded with the cross-hatch pattern. We report the unweighted and population-weighted correlation coefficients across CZs between these mobility measures and the baseline measures in Figure VI. The CZ-level statistics underlying these figures are reported in Online Data Table V.

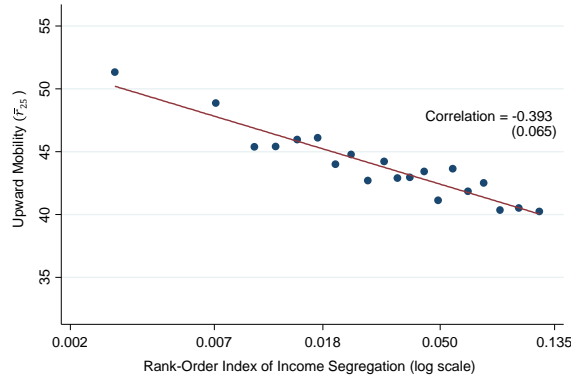
ONLINE APPENDIX FIGURE X

Segregation and Upward Mobility

A. Upward Mobility vs. Theil Index of Racial Segregation in CZ

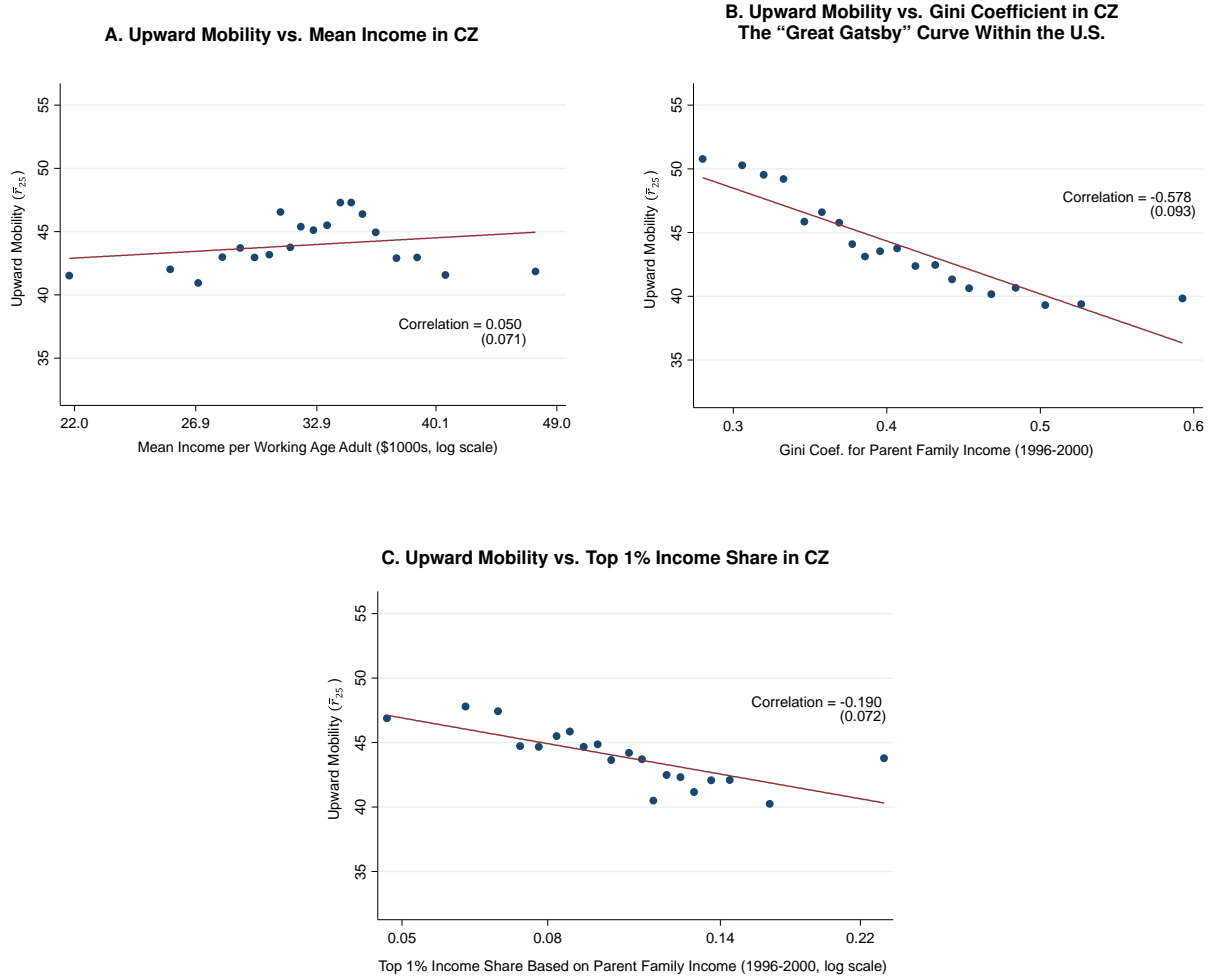


B. Upward Mobility vs. Rank-Order Index of Income Segregation in CZ



Notes: Panel A presents a binned scatter plot of absolute upward mobility (\bar{r}_{25}) vs. a multi-group Theil index of racial segregation (based on census tract level data from the 2000 Census). To construct this figure, we group CZs into twenty equally sized bins (vingtiles) based on their segregation index. We then plot the mean level of absolute upward mobility vs. the mean segregation index within each of the twenty bins (using a log scale on the x axis). Panel B presents an analogous binned scatter plot of absolute upward mobility vs. the rank-order index of income segregation from Reardon (2011). See text for details on the construction of these segregation indices. Note that these binned scatter plots provide a non-parametric representation of the conditional expectation function, but they do not show the variance in the underlying data across CZs. The correlations between the variables are estimated using the underlying CZ-level data, with standard errors (reported in parentheses) clustered by state. The correlations are estimated in levels (not logs) for consistency with Appendix Table VII.

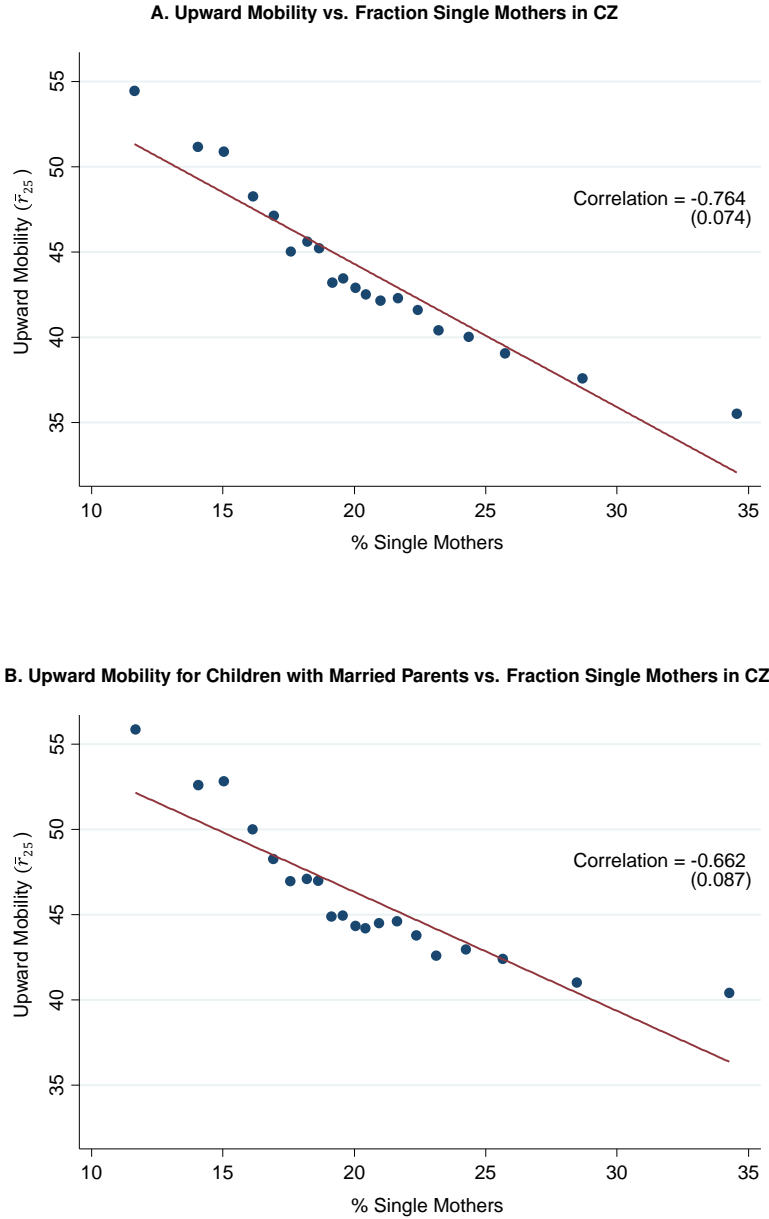
ONLINE APPENDIX FIGURE XI Local Income Distributions and Upward Mobility



Notes: Panel A presents a binned scatter plot of absolute upward mobility (\bar{r}_{25}) vs. mean income per working age adult in the CZ (based on data from the 2000 Census). To construct this figure, we group CZs into twenty equally sized bins (vingtiles) based on mean income levels. We then plot the mean level of absolute upward mobility vs. the mean income level within each of the twenty bins (using a log scale on the x axis). Panel B presents an analogous binned scatter plot of absolute upward mobility vs. the Gini coefficient in the CZ, computed based on the core sample and mean parent income for 1996-2000. Panel C presents a binned scatter plot of absolute upward mobility vs. the fraction of income in the CZ accruing to parents in the top 1% of the local distribution (using a log scale on the x axis), again using the core sample and parents' average income for 1996-2000. Note that these binned scatter plots provide a non-parametric representation of the conditional expectation function, but they do not show the variance in the underlying data across CZs. The correlations between the variables are estimated using the underlying CZ-level data, with standard errors (reported in parentheses) clustered by state. The correlations are estimated in levels (not logs) for consistency with Appendix Table VII.

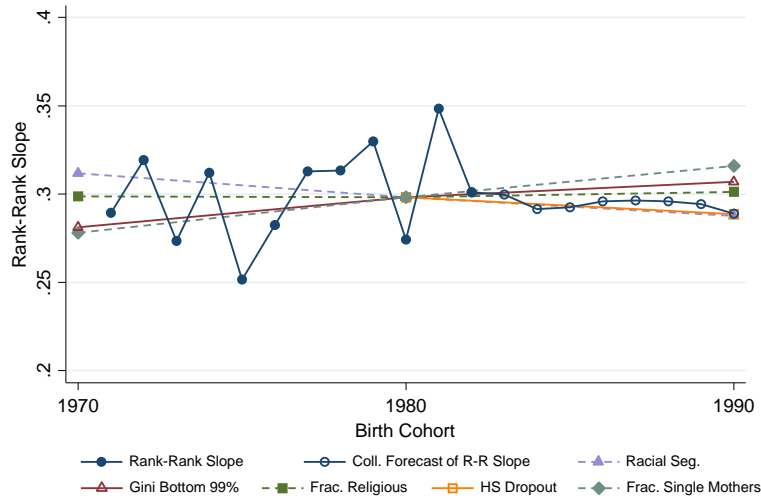
ONLINE APPENDIX FIGURE XII

Single-Parent Families and Upward Mobility



Notes: Panel A presents a binned scatter plot of absolute upward mobility (\bar{r}_{25}) vs. the fraction of children being raised by single mothers in the CZ (based on data from the 2000 Census). To construct this figure, we group CZs into twenty equally sized bins (vingtiles) based on the fraction of single parents. We then plot the mean level of absolute upward mobility vs. the mean fraction of single parents within each of the twenty bins. Panel B replicates Panel A, restricting the sample used to estimate upward mobility in each CZ to children whose own parents are married in the year they first claim the child as a dependent. Note that these binned scatter plots provide a non-parametric representation of the conditional expectation function, but they do not show the variance in the underlying data across CZs. The correlations between the variables are estimated using the underlying CZ-level data, with standard errors (reported in parentheses) clustered by state.

ONLINE APPENDIX FIGURE XIII
Predicted vs. Actual Time Trends in Relative Mobility



Notes: This figure compares actual trends in rank-rank slopes at the national level, estimated in Chetty et al. (2014), with projected changes based on trends in the five factors most strongly correlated with differences in mobility across CZs in the cross-section. The series in circles is from Chetty et al. (2014, Figure 2). The solid circles show estimates of rank-rank slopes by birth cohort using the SOI 0.1% sample. The open circles show forecasts of the rank-rank slope based on income measured at age 26 and the college attendance rates using the population data. The other series show projections of trends, each based on a different factor: (1) Theil index of racial segregation, (2) high school dropout rate, (3) Gini coefficient, (4) violent crime arrest rate, and (5) fraction of single parents. We construct these projections based on unweighted univariate CZ-level regressions of relative mobility on each factor separately. We normalize the projections (by adding a constant) so that their values match the mean observed rank-rank slope (i.e., the mean value of the series in circles) from 1971-1990. See Appendix I for further details.