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CHANGING OPPORTUNITY: SOCIOLOGICAL MECHANISMS UNDERLYING GROWING CLASS GAPS AND SHRINKING RACE GAPS IN ECONOMIC MOBILITY

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ABSTRACT

We show that intergenerational mobility changed rapidly by race and class in recent decades and use these trends to study the causal mechanisms underlying changes in economic mobility. For white children in the U.S. born between 1978 and 1992, earnings increased for children from high-income families but decreased for children from low-income families, increasing earnings gaps by parental income ("class") by 30%. Earnings increased for Black children at all parental income levels, reducing white-Black earnings gaps for children from low-income families by 30%. Class gaps grew and race gaps shrank similarly for non-monetary outcomes such as educational attainment, standardized test scores, and mortality rates. Using a quasi-experimental design, we show that the divergent trends in economic mobility were caused by differential changes in childhood environments, as proxied by parental employment rates, within local communities defined by race, class, and childhood county. Outcomes improve across birth cohorts for children who grow up in communities with increasing parental employment rates, with larger effects for children who move to such communities at younger ages. Children's outcomes are most strongly related to the parental employment rates of peers they are more likely to interact with, such as those in their own birth cohort, suggesting that the relationship between children's outcomes and parental employment rates is mediated by social interaction. Our findings imply that community-level changes in one generation can propagate to the next generation and thereby generate rapid changes in economic mobility.

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I Introduction

Children's prospects for upward economic mobility vary substantially across areas and racial groups within the United States (Bhattacharya and Mazumder, 2011; Chetty et al., 2014*a*, 2020*b*). These present-day differences in economic mobility can be traced in part back to historical factors such as rates of slavery before 1860 (Berger, 2018), Jim Crow laws from 1870-1960 (Althoff and Reichardt, 2024), redlining in credit markets from 1930-1970 (Aaronson et al., 2021; Lane et al., 2022), and the migration of Black Americans from the South to the North between 1910-1970 (Derenoncourt, 2022). Given the long-lasting influence of such historical factors, can economic opportunity change in shorter time frames?

Although many studies have analyzed differences in mobility cross-sectionally, much less is known about how and why intergenerational mobility has changed over time, especially in recent decades.[1] Understanding the mechanisms that generate changes in economic mobility is essential for developing interventions to narrow disparities, since racial and socioeconomic disparities in economic outcomes are shaped by rates of intergenerational mobility (Becker and Tomes, 1979; Chetty et al., 2020*b*; Collins and Wanamaker, 2022; Davis and Mazumder, Forthcoming). For example, if Black children are less likely to climb the income ladder relative to their parents compared to white children, racial disparities in income will persist in the long run irrespective of current income levels.

We document sharp changes in economic mobility by race and class in recent decades and identify the causal mechanisms underlying these changes.[2] Our primary analysis uses de-identified data from federal income tax returns linked to information from decennial census data and the Numident database. These data cover 57 million children born between 1978 and 1992 with information on children's and parents' earnings, employment rates, marital status, mortality, and residential locations. We supplement these data using information on educational attainment, occupation, and other variables from the American Community Survey (ACS), as well as standardized (SAT and ACT) test scores.

We divide our analysis into four parts. In the first part of the paper, we analyze national trends in intergenerational income mobility. Between the 1978 and 1992 birth cohorts, household incomes in adulthood fell sharply for white children growing up in low-income families. At the same time, incomes increased for white children growing up in high-income families. These divergent trends resulted in growing white class gaps, with the intergenerational persistence of household income ranks for white children increasing by 28%. The gap in average household incomes for white children raised in low-income (25th percentile) versus high-income (75th percentile) families grew from \$17,720 in the 1978 birth cohort to \$20,950 in the

[1] Several studies document historical trends in economic mobility using surveys and Census data for the 1830-1980 birth cohorts (e.g., Collins and Wanamaker, 2022; Jácome, Kuziemko and Naidu, 2022; Ward, 2023; Davis and Mazumder, Forthcoming). We complement this work by studying more recent trends in economic mobility using administrative data that covers nearly the entire U.S. population for the 1978-1992 birth cohorts, permitting a finer-grained disaggregation of the changes in mobility and the corresponding mechanisms compared to prior work.

[2] We focus on five race and ethnicity groups—non-Hispanic white children, non-Hispanic Black children, Hispanic children, non-Hispanic Asian children, and non-Hispanic American Indian and Alaskan Native (AIAN) children—who together comprise 97.3% of the children with non-missing (self- or household-reported) race information in our sample. As has been noted in prior work, there is considerable heterogeneity in outcomes within these five groups, and our conclusions should not be interpreted as applying uniformly to all subgroups within each of these populations. For simplicity, we use "race" to refer to race and ethnicity, "white" to refer to non-Hispanic white individuals, "Black" to refer to non-Hispanic Black individuals, "American Indian" to refer to non-Hispanic American Indian and Alaskan Native individuals, and so on.

1992 birth cohort. In contrast, incomes in adulthood increased across all parental income levels for Black children. As a result of these trends, white-Black race gaps for low-income families shrank: the gap in average household incomes between white and Black children raised in low-income families fell by 28%, from \$20,810 for children born in 1978 to \$14,910 for children born in 1992. The class gap among Black families and the white-Black gap among high-income families remained essentially unchanged. Intergenerational mobility also changed much more modestly for Hispanic, Asian, and American Indian children during the period we study.[3]

The white-Black race gap among low-income families narrowed primarily because of changes in children's chances of escaping poverty rather than their chances of reaching the upper class. In the 1978 cohort, Black children from families in the bottom income quintile were 14.7 percentage points more likely to remain in the bottom quintile than their white counterparts. By the 1992 cohort, this gap shrank to 4.1 percentage points—a 72% reduction in the racial gap in the intergenerational persistence of poverty—a measure that has been the focus of recent policy discussions (National Academies of Sciences and Medicine, 2024). By contrast, there was relatively little change across cohorts in the likelihood that white or Black children from families in the bottom income quintile reached the top quintile of the income distribution.

We find similar patterns of growing white class gaps and shrinking white-Black race gaps for early-life outcomes, including educational attainment and SAT and ACT scores.[4] These results show that outcomes began to diverge even before children entered the labor market. Non-monetary outcomes, such as marriage, incarceration, and mortality rates also exhibit similar trends, indicating that the changes extended well beyond economic outcomes. For example, the white class gap in early adulthood mortality more than doubled between the 1978 and 1992 birth cohorts, while the white-Black race gap in early adulthood mortality decreased by 77%.

Outcomes deteriorated for low-income white families and improved for low-income Black families in nearly every part of the country, but the magnitudes of these changes varied across areas. Economic mobility fell the most for low-income white families in the Great Plains and the coasts, areas that had enjoyed relatively high rates of mobility in the 1978 birth cohort. By the 1992 cohort, these areas had levels of economic mobility comparable to the Southeast and industrial Midwest (e.g., Ohio and Michigan), which had low levels of mobility for all cohorts in our data. Economic mobility for low-income Black families increased sharply in the Southeast and the industrial Midwest, with modest changes on the coasts. Despite these divergent trends, low-income Black families still had significantly lower levels of economic mobility than low-income white families in virtually every county even in the 1992 birth cohort because the initial white-Black race gaps in mobility were so large for the 1978 birth cohort.

Trends differed even among cities with similar demographic characteristics and economic trajectories.

[3]We find similar patterns of growing white class gaps and shrinking white-Black race gaps across a broad range of economic outcomes, including employment rates and alternative measures of income such as individual earnings instead of household income. The trends are also insensitive to specification choices such as how we measure parents' and children's incomes. The trends are also similar by gender, although the magnitude of the change in the white-Black race gap for men is larger than for women, partly because the starting level of the white-Black race gap is much larger for men than women (Chetty et al., 2020*b*).

[4]Prior work has reached conflicting conclusions regarding changes in test score gaps by income (e.g., Reardon 2018 versus Hashim et al. 2023) because of the challenges in analyzing trends using survey data. We shed light on this debate using administrative data on test scores and income for 24.8 million students.

For example, Charlotte, NC and Atlanta, GA—two rapidly growing cities in the Southeast with similar demographics—both had very low rates of economic mobility for children born in 1978, particularly for low-income Black families. Economic mobility for Black families increased sharply in Charlotte, reaching the national average for low-income Black children in the 1992 birth cohort, but remained low in Atlanta.

In the second part of the paper, we use these differential trends in mobility across subgroups and areas to study the determinants of changes in economic mobility. We start by showing that changes in family characteristics, such as parental education, wealth, occupation, or marital status, explain only 7% of the growing white class gaps and 10% of the shrinking white-Black race gaps. We then show that the differential trends persist even when we control for childhood Census tract-by-cohort fixed effects, implying that white class gaps grew and white-Black race gaps shrank even among children who grew up in the same Census tract. The divergence in economic mobility must therefore be driven by factors that impact race and class groups differently *within* a given neighborhood.

One set of factors that could generate differential impacts across subgroups are changes in the social environments in which children grow up. The importance of social communities and ties has been widely discussed in observational sociological research, from early studies by Durkheim (1897) and Dubois (1898) to contemporary work by Wilson (1996), among many others. For example, Wilson (1996) argues, based on ethnographic studies of Black families in the South Side of Chicago, that the disappearance of work in a community leads to social disorganization, family dissolution, and a lack of role models that can affect downstream economic outcomes. Motivated by this hypothesis, we study the relationship between changes in children's outcomes and changes in the economic and social conditions of parents in their social *community*, which we define as other individuals who share the same race, class, and childhood county.[5]

We find that changes in children's outcomes—earnings, SAT/ACT scores, and educational attainment—are strongly positively correlated with changes in parental employment rates across cohorts in their community, even when controlling for the employment status of a child's own parents. For example, the outcomes of white children with low-income parents deteriorated much more sharply in areas where employment rates for low-income white parents fell more. The relationship between changes in children's outcomes and changes in parental employment rates is virtually identical across race and class groups. As a result, the growth in the white class gap and the reduction in the white-Black race gap can be explained almost entirely by the sharp fall in employment rates for low-income white parents relative to low-income Black and high-income white parents over the period we study.[6] We find similar relationships between changes in children's outcomes and changes in other community-level characteristics, such as parental marriage rates and mortality rates. In short, community-level changes in the parents' generation—which can be measured using a variety of parental outcomes—are highly correlated with their children's outcomes in adulthood.

The same community-level factors that explain changes in outcomes for white and Black children

[5] We define social communities based on race, class, and geography in light of evidence that individuals tend to interact most with people who live near them and who are from the same race and class groups (Blau, 1977; Chetty et al., 2022). In the last section of the paper, we analyze other determinants of social interaction.

[6] These differential trends in parental employment rates are consistent with prior work (discussed further at the end of this section) that has documented differential trends by race and class in adults' employment rates and examined the factors that drove these trends. We do not take a stance on the sources of changes in parental employment rates here and focus instead on the downstream consequences of these changes for children's outcomes.

can also explain the (smaller) changes we observe for other subgroups. The correlation between changes in children's outcomes and changes in parental employment rates across subgroups is 0.91. Hence, community-level changes—as proxied by parental employment rates or other outcomes in the parental generation—provide a unified explanation (in a predictive, not causal sense) for the divergence in outcomes by race and class.

One explanation for the correlation between changes in children's and parents' outcomes across communities is that changes in childhood environments (as proxied by parental outcomes) have a causal exposure effect on children's outcomes in adulthood. For example, higher parental employment rates may be associated with greater resources and positive social influences that shape children's behavior, ultimately improving their long-term outcomes in proportion to the number of years they spend in a community (e.g., Ananat et al., 2013, 2017).[7] Another explanation is that the correlation is driven by common shocks (e.g., to local labor demand) that affect both parents and children directly. A third possibility is that there is differential selection in the types of parents and children who live in declining versus improving areas.

The third part of the paper tests between these explanations by estimating the causal exposure effect of growing up in a community with higher parental employment rates on children's outcomes in adulthood. The ideal experiment to estimate this causal exposure effect would take a set of children born in different years in an origin community with stable parental employment rates across cohorts and randomly assign them at different ages to a community with increasing parental employment rates. If growing up in a community with higher parental employment rates has a causal exposure effect, outcomes would improve more across cohorts for children assigned at earlier ages to the community with increasing parental employment rates.

In the absence of such an experiment, we develop a quasi-experimental research design that compares children in different cohorts who move across counties at different ages. As in the hypothetical experiment described above, consider a set of children who are born in a county where parental employment rates do not change across cohorts and move to a county where parental employment rates are increasing. We estimate the causal exposure effect based on the difference in outcomes between children who make this move at younger versus older ages in earlier versus later birth cohorts. This research design permits selection effects across cohorts that may lead to differences in potential outcomes between children who move to a given county when parental employment rates are low versus high. However, it requires that these selection effects do not differ by the age at which children move—a "constant selection by age" identification assumption common in the prior literature on neighborhood effects (e.g., Chetty and Hendren, 2018*a*; Deutscher, 2020) that we evaluate in our context after presenting our baseline results.[8]

We find that children's outcomes improve across cohorts when they move to communities with increas-

[7] We focus on parental employment rates as a simple summary measure of the economic and social conditions in the parents' generation but caution that our analysis does not shed light on whether parental employment rates themselves are the key causal lever that affects children's outcomes. Rather, we test whether growing up in an area with changing parental employment rates—which is associated with changes in parental marriage rates and parental income, and presumably many other unobserved factors—has a causal exposure effect on children's outcomes in adulthood.

[8] Our research design is a dynamic generalization of the movers exposure effect design developed by Chetty and Hendren (2018*a*), with the key difference that it is identified from variation in childhood environments across cohorts *within* counties rather than between counties. Additionally, we relate changes in children's outcomes to an observable predictor—parental employment rates—rather than the outcomes of children of permanent residents, thereby providing an observable proxy for neighborhood quality rather than relying on ex-post outcomes.

ing parental employment rates, with larger effects for children who move at younger ages. Consider children who move at a young age (e.g., before age 8) to a community where parental employment increased between the 1978-1992 birth cohorts. Among these children, earnings rise systematically as we move from early to late birth cohorts. In contrast, among children who made the same moves at older ages (e.g., after age 13), there is little difference in earnings as we move from early to late birth cohorts. Under our identification assumption, these results imply that changes in parental employment rates across cohorts lead to an increase in children's earnings through a causal exposure effect.

The key potential threat to the validity of our identification assumption is that families with young children who move from one area (e.g., Atlanta) to another (e.g., Charlotte) when employment rates are higher in the destination may differ from families with older children who make the same move. For example, families who invest heavily in their children's human capital may seek better (higher parental employment) environments especially when their children are young. We test for such selection by comparing siblings' outcomes. When siblings move to a community with increasing parental employment rates, the younger sibling, who has more years of exposure to a high-parental-employment environment, earns significantly more than the older sibling. The differences in outcomes between siblings are proportional to the age gap between siblings. Estimates of childhood exposure effects from sibling comparisons are similar to those from our baseline approach, ruling out the possibility that our findings are driven by unobserved differences in fixed family characteristics and supporting the identification assumption underlying our research design.

The central innovation of our analysis relative to prior work analyzing the causal effects of neighborhoods is that it establishes that *changes* in environments—as captured by observable factors such as parental employment rates—have a causal exposure effect on children's long-term outcomes. Our findings demonstrate that one can potentially increase intergenerational mobility substantially through changes in childhood environments within a few years, even without changing slower-moving factors such as housing stocks or access to transportation.

In the fourth part of the paper, we explore the types of interventions that could generate such change by studying the mechanisms through which changes in childhood environments lead to changes in economic mobility. One class of mechanisms is related to social interaction: for example, connections to higher-income, employed adults may facilitate job referrals, shape children's aspirations, or influence their sense of identity through role-modeling or social mimicking mechanisms (e.g., Loury, 1977; Bourdieu, 1986; Akerlof and Kranton, 2000; Chetty et al., 2022; Newman and Skocpol, 2023). Another class of mechanisms revolves around economic resources: for example, higher-income, employed adults may have more resources to support schools and other programs that improve children's outcomes (e.g., Card and Krueger, 1992; Hoynes, Page and Stevens, 2011; Jackson and Mackevicius, 2024). We distinguish between these two mechanisms using variation in rates of interaction across different subgroups within a community, based on the logic that the social interaction mechanism would predict heterogeneity by the degree of interaction more than the economic resource mechanism.

We first exploit high-frequency variation in interactions across birth cohorts generated by the fact that children are much more likely to interact with peers in their own cohort than surrounding cohorts. We find that children's outcomes are strongly related to parental employment rates (and other correlated community-level characteristics) of peers in their *own* birth cohort. Parental employment rates in preceding or subsequent cohorts have much less predictive power for children's outcomes after we control for parental employment rates in their own cohort, consistent with recent work by Deutscher (2020) in Australia. Insofar as economic resources are unlikely to vary so sharply across adjacent cohorts, the cohort-specificity of the impacts points in favor of social interaction mechanisms.[9]

Next, we exploit variation arising from people's tendency to interact with others in their own race and class group. We find that the outcomes of white children growing up in low-income families are primarily driven by the employment rates of low-income white parents. Conditional on employment rates for low-income white parents, the employment rates of Black parents or high-income white parents are not strongly related to the outcomes of low-income white children. Similarly, for Black children growing up in low-income families, the employment rates of low-income Black parents are generally far more predictive of outcomes than the employment rates of low-income white parents.

Counties with greater interaction across racial lines are an exception to this pattern. When Black children constitute a small share of a county's population, they are more likely to interact with white peers (Blau, 1977; Currarini, Jackson and Pin, 2009; Cheng and Xie, 2013). In such counties, the outcomes of Black children are related to the employment rates of low-income *white* parents. The outcomes of Black children are also related to the employment rates of low-income white parents in counties with higher rates of interracial marriage, a proxy for cross-racial interaction, controlling for racial shares at the Census tract level. These findings provide further support for social interaction mechanisms, as captured, for example, by the Borjas (1992) model of "ethnic capital" in intergenerational mobility.[10]

Combining these results, we conclude that a parsimonious theory—that children's outcomes mimic those of the parents in their social communities—explains the divergent trends in economic mobility by race and class in the United States in recent decades.

Related Literature. This paper builds on four parts of a vast literature in economics and sociology studying intergenerational mobility and the drivers of racial and socioeconomic disparities. First, our work connects to studies examining trends in economic mobility by parental income or race in the United States. Overall rates of intergenerational mobility, pooling racial groups, have been fairly stable in recent decades (Chetty et al., 2014*b*). There has also been little change in the white-Black income gap in percentile ranks when pooling parental income groups (Bayer and Charles, 2018). We show that disaggregating the data by race *and* parental income—which was infeasible with the data used in prior work—reveals divergent trends at the intersection of race and class. These trends were not evident in past work because the improving outcomes among high-income white families were offset by the deteriorating outcomes among low-income white families, leaving the unconditional white-Black race gap relatively unchanged. Similarly, the improvement in children's outcomes for low-income Black families muted the change in the overall intergenerational correlation between parent and child income when pooling racial groups.

Second, our findings are consistent with a large body of work documenting similar differential trends

[9]These findings further support the view that changes in community environments have causal exposure effects on children's outcomes since correlated shocks across adults and children (e.g., labor demand shocks) are unlikely to have cohort-specific effects.

[10]While these results suggest that social interaction mediated the trends we study here, they do not imply that economic resources do not matter for economic opportunity more broadly.

by race and class in employment rates, incarceration rates, well-being, and health using data from repeated cross sections of adults (e.g., Stevenson and Wolfers, 2008; Sawhill, 2018; Binder and Bound, 2019; Case and Deaton, 2020; Schwandt et al., 2021). Prior studies argue that factors such as the decline of manufacturing, the rise of outsourcing, changes in labor supply, and the opioid epidemic reduced employment rates among less educated, lower wage individuals, while skill-biased technical change may have helped sustain employment at the top of the distribution (e.g., Autor, Katz and Kearney, 2006; Acemoglu and Autor, 2011; Autor, Dorn and Hanson, 2013; Binder and Bound, 2019; Case and Deaton, 2020). Employment rates among Black Americans have risen in recent decades relative to white Americans, potentially because of Civil Rights legislation, reductions in discriminatory practices, lower rates of incarceration, or differential labor supply responses to labor demand shocks (e.g., Bayer and Charles, 2018; Muller and Roehrkasse, 2022; Kahn, Oldenski and Park, 2023). We show how these changes in the parental generation—whatever their origin—have important downstream consequences for the next generation at the community level.

Third, our paper relates to ethnographic and observational research on the drivers of racial disparities, especially the literature initiated by Wilson (1986, 1987, 1996) and Massey and Denton (1998) on how the decline of economic activity, compounded by racial and economic segregation, can help explain the challenges faced by Black communities in urban areas. Our quasi-experimental evidence supports this mechanism and shows that the same forces affected low-income white Americans in recent decades.[11]

Finally, our work builds on the literature studying the causal effects of neighborhood environments on children's long-term outcomes (summarized by Chyn and Katz 2021). We contribute to this literature by showing how neighborhood effects change over time.[12] Our analysis shows that the key unit in which change occurs is not the neighborhood as a whole but rather communities delineated by race and class *within* neighborhoods, perhaps because social interactions tend to be stratified along these lines. Most importantly, our results show that the causal effects of communities on economic mobility can change substantially within a decade. Hence, differences in economic mobility by race and class are malleable in policy-relevant timeframes even though their roots lie partly in historical factors.

The paper is organized as follows. Section II describes our data. Section III characterizes national trends in intergenerational mobility. Section IV examines mechanisms for these trends, showing that changes in children's outcomes are correlated with changes in parental employment rates. Section V presents quasi-experimental evidence on the effects of changes in childhood environments on children's outcomes. Section VI presents evidence on social interaction versus economic resources as mediators of changes in mobility. Section VII concludes by discussing policy implications. Statistics on upward income mobility and other outcomes by race, gender, parental income group, birth cohort, and county can be downloaded from the Census Bureau or Opportunity Insights and visualized using the Opportunity Atlas.

[11] This unified explanation for differential trends by race is consistent with the sociological thesis of racial invariance – the idea that the ultimate causes of economic and social outcomes are the same for white and Black individuals. For example, Sampson and Wilson (1995) and Sampson, Wilson and Katz (2018) argue that violent crime stems from concentrated disadvantage in residential communities, with racial disparities in crime driven by the persistent structural disadvantages faced by Black communities.

[12] Recent studies have investigated how manufacturing shocks affect changes in economic mobility across cohorts (Ananat et al., 2017; McNeil, Luca and Lee, 2023; Tuhkuri, 2023; Seltzer, 2024). We contribute to this work by showing that changes in parental employment rates (and associated factors) affect children's outcomes through a childhood exposure effect mediated by social interaction and establishing that this mechanism explains divergent national trends in economic mobility by race and class.

II Data

We combine three sources of data housed at the Census Bureau: (1) the 2000 and 2010 Census short forms; (2) the 2000 Census long form and 2005-2019 ACS; and (3) federal income tax returns in 1979, 1984, 1989, 1994-1995, and 1998-2019. The Census short forms are designed to cover the entire population; the 2000 Census long form is a stratified random sample covering approximately one-sixth of households; and the ACS is a stratified random sample covering approximately 2.5% of households in each year (U.S. Department of Commerce, Bureau of the Census, 2000, 2003, 2014). These datasets are linked by unique person identifiers as described in Chetty et al. (2020*b*), who show that the linked dataset covers approximately 90% of the target sample that appears in the Census short form.

The remainder of this section describes our construction of the analysis sample, defines the variables of interest, and presents summary statistics. Our sample and variables build on those used by Chetty et al. (2020*b*) and much of this section is taken from Section III of that paper.

II.A Sample Definition

Our target sample for our primary analysis is all children in the 1978-1992 birth cohorts who satisfy the following conditions: (1) they were born in the U.S. or are authorized immigrants who came to the U.S. in childhood and (2) their parents are U.S. citizens or authorized immigrants. We limit our analysis to children born during or after 1978 because many children begin to leave the household at age 17 (Chetty et al., 2014*a*) and the first year in which we have dependent claiming information is 1994. We limit our analysis to children born during or before 1992 because we generally measure children's outcomes in adulthood at age 27 and the last year for which we have tax data is 2019. Finally, we limit our analysis to individuals who were born in the U.S. or who are authorized immigrants because coverage rates of tax data for unauthorized immigrants are difficult to determine.

To construct this sample, we first identify all children who were claimed as a child dependent on a 1040 tax form in the 1994-1995 or 1998-2019 data. We then limit the sample to children who were claimed by an adult who appears in the 2020 Numident file and who was between the ages of 15 and 50 at the time of the child's birth. We finally limit the sample to children who were born between 1978 and 1992, based on their record in the 2020 Numident. This sample definition excludes both unauthorized immigrants and child dependents claimed by unauthorized immigrants because unauthorized immigrants do not have SSNs and therefore do not appear in the Numident file.

We define a child's parent(s) as the first person(s) who claims the child as a dependent on a 1040 tax form in the 1994-1995 and 1998-2019 tax data. If parents are married but filing separately, we assign the child to both parents. The person(s) must be supporting the child to claim him or her as a dependent, but may not necessarily be the child's biological parent(s). The definition of a child's parent(s) is held fixed after the initial link, regardless of subsequent dependent claims or changes in marital status. We also exclude the approximately 3.7% of children whose parent's mean real or nominal income is zero or negative because individuals reporting zero or negative income typically have large capital losses, a proxy for significant wealth.[13]

Although we cannot link children to parents who never file a tax return, over 99.6% of children are claimed by an adult at some point in their childhood (Cilke, 1998; Chetty et al., 2020*b*). Chetty et al. (2020*b*) also show that the children in their sample have similar outcomes and demographic characteristics when compared to children in the same birth cohorts from representative surveys. The same pattern holds for the more recent birth cohorts that we study here.

II.B Variable Definitions

In this subsection, we briefly define the variables we use in our primary analysis. We measure all monetary variables in 2023 dollars, adjusting for inflation using the consumer price index (CPI-U).

<u>Variable Definitions for Parents</u>

Parental Income in the Child's Youth. We measure parental income each year using the total pre-tax income of the primary tax filer and their spouse (if applicable), which we label family or household income. We use the term household income for simplicity, but we do not include incomes from cohabiting partners or other household members aside from the primary tax filer's spouse. In years where a parent files a tax return, we define household income as the sum of Adjusted Gross Income, social security payments, and tax-exempt interest payments, as reported on their 1040 tax return. In years where a parent does not file a tax return, we define household income as W-2 income when available. Otherwise, we consider household income for non-filers to be zero. For our primary analysis, we define parental income in the child's youth as the mean household income over the five years in which the child is ages 13-17 (or the subset of those years for which we have tax data). We also consider alternative definitions of parental income in sensitivity analyses.

Marital Status in the Child's Youth. We measure parental marital status in the child's youth using the 1040 tax return in the first year in which a child is claimed. We consider the parents to be married (or, equivalently, that the household has two parents present) during childhood if there is both a primary and secondary filer in the first year in which the child is claimed.

Educational Attainment. We measure parental educational attainment using the 2000 Census long form and the ACS (prioritizing the long form when both are available). We define parental educational attainment as the highest level of education completed by the parent. High school completion is defined as receiving a high school diploma, GED, or equivalent credential. College completion is defined as completing a bachelor's degree or higher level of education. We prioritize the mother's education information if available and, if not, we use the father's education information.

Wealth. We measure parental wealth using the 2000 Census long form and the ACS (prioritizing the long form when both are available). We measure parental wealth using an indicator for home ownership, indicators for the monthly mortgage payment quintile, and indicators for the home value quintile. As above, we prioritize the mother's wealth information if available and, if not, we use the father's wealth information.

[13]For county- and commuting zone-level analyses that require information on childhood location, we further exclude approximately 3.4% of children for whom we do not observe parental address information.

Occupation. We measure parental occupation using the 2000 Census long form and the ACS (prioritizing the long form when both are available). We define parental occupation using the 1990 IPUMS definitions of occupation at the three-digit level. We prioritize the father's occupation information if available and, if not, we use the mother's occupation information, as fathers are more likely to be employed.

Employment Rates in the Child's Adulthood. We measure parental employment for each parent and year using an indicator for positive W-2 income. We consider parents who do not have a W-2 in a given year to be unemployed. In our baseline analysis, we define parental employment rates in the child's adulthood as the fraction of the child's parents who are employed when the child is age 27. For children claimed by a single parent when they are first linked to parents, this variable is an indicator equal to 1 if their parents are employed when the children are age 27. For children with married parents, the variable takes on values of 0, 0.5, or 1 depending upon whether 0, 1, or 2 of the claiming parents are employed when the children are 27 years old. We also measure parental employment at other ages in sensitivity analyses.

Marital Status in the Child's Adulthood. We measure parental marital status in the child's adulthood using the mother's 1040 tax return when the child is age 27.

Mortality Rates in the Child's Adulthood. We measure parental mortality using the Census Numident, which contains death records compiled by the Social Security Administration. We define parental mortality in the child's adulthood as the fraction of the child's parents who died when the child is ages 18-27. We measure mortality after children are 18 because we measure parental income when the child is ages 13-17 in our baseline analysis.

Location. We measure parental location each year using the address listed on their 1040 tax return. Addresses are geocoded and assigned to standard Census geographic units (e.g., block, tract, county) by Census staff. For non-filers, we use the address from information returns such as W-2s when available. We track the mother's location if the child is linked to two parents and parental marital status changes.

Variable Definitions for Children

Race and Ethnicity. We measure children's race and ethnicity using the information they or a household member report on the 2000 and 2010 Census short forms and the ACS. We prioritize the 2010 Census short form, then the 2000 Census short form, and finally the ACS. We use these data to construct five main race and ethnicity groups—non-Hispanic white, non-Hispanic Black, Hispanic, non-Hispanic Asian, and non-Hispanic American Indian and Alaskan Natives (AIAN)—who together comprise 97.3% of the children with non-missing race information in our sample.[14]

Income in Adulthood. In our primary analysis, we measure children's income at both the individual and household level using their pre-tax income at age 27, top coding incomes at $1 million. If a child files a tax return, we define household income as the sum of Adjusted Gross Income, social security payments, and tax-exempt interest payments, as reported on their 1040 tax return. We define individual income as

[14] Self- and household-identified race and ethnicity measures are fairly stable over time. For non-Hispanic white individuals who are in both the 2000 and 2010 Censuses, only 3% changed their response. For non-Hispanic Black individuals, only 6% changed their response (Liebler et al., 2017).

wage income reported on their W-2, in addition to self-employment and other non-wage income reported on their 1040 tax returns. We assign individuals who are married and filing jointly half of the self-employment and other non-wage income. For non-filers, we define both individual and household income as total wage earnings from W-2s, or as 0 if no W-2 is filed. We also consider alternative definitions of child income that measure income at later ages or average over multiple years in sensitivity analyses. Appendix Figure A.1 shows the mapping between dollars and percentiles for child household income at age 27, as well as a replication of our main results using this dollar-to-percentile mapping.

Employment Rates. We define children as employed at age 27 using an indicator for positive W-2 income.

Marital Status. We measure children's marital status using their filing status on 1040 tax returns at ages 27 and 32.

Mortality Rates. We measure children's mortality using the Census Numident. We measure mortality between ages 24 and 27. We measure mortality at these ages to ensure that we have data on child race and ethnicity—which we observe only in the 2000 and 2010 decennial censuses and the ACS—for all cohorts.

Incarceration Rates. We measure children's incarceration using the 2000 and 2010 Census short forms. We define individuals as incarcerated if on the day of the Census they live in a federal detention center, federal prison, state prison, local jail, residential correctional facility, military jail, or juvenile correctional facility. We measure incarceration at a fixed age to adjust for changes over the lifecycle, focusing on age 22 because our analysis begins with the 1978 birth cohort, who turned 22 in the year 2000. We compare incarceration rates in the 1978 birth cohort to the 1988 birth cohort, who turned 22 in the year 2010.

Educational Attainment. We measure children's educational attainment using the ACS. We measure the number of completed years of schooling by age 27. High school completion is defined as receiving a high school diploma, GED, equivalent credential, or higher level of education at or before age 27. College completion is defined as completing a bachelor's degree or higher level of education at or before age 27.

SAT/ACT Scores. We measure the fraction of children taking the SAT/ACT in high school and their mean SAT/ACT scores using aggregate statistics by race and parental income constructed in Chetty, Deming and Friedman (2023). Chetty, Deming and Friedman (2023) report statistics for students graduating from high school in 1998-2005, 2007, 2009, 2011, 2013, and 2015. These high school cohorts align approximately with the 1980-1997 birth cohorts. ACT scores are mapped into equivalent SAT scores using published concordance tables. SAT scores are prioritized when both SAT and ACT scores are available. Scores are converted to percentile ranks by ranking students relative to all other test takers in the U.S. in the same high school cohort.

II.C Summary Statistics

Table I reports summary statistics for our primary analysis sample. There are 34.9 million white children, 7.7 million Black children, 8.2 million Hispanic children, 1.9 million Asian children, and 0.5 million AIAN children in our primary analysis sample.

Panel A of Table I reports summary statistics for these children's parents. Parental income differs sharply across racial groups. The median parental income when the child is between the ages of 13 and 17 is $91,800 for white children, $38,250 for Black children, $44,600 for Hispanic children, $72,900 for Asian children, and $43,790 for AIAN children. These differences in household income are partly driven by differences in the rates of having two parents present in the household, with 80.0% of white children growing up in two-parent households compared to 29.1% of Black children and 54.0% of Hispanic children. Other parental characteristics similarly vary across the groups. Panel B of Table I reports summary statistics for the children in our sample. Consistent with prior work (e.g., Chetty et al., 2020*b*; Davis and Mazumder, Forthcoming), we observe large differences in outcomes for children by race.

Appendix Tables A.1-A.5 report analogous summary statistics for the 1978 and 1992 birth cohorts separately by race and class. These tables show that trends sharply diverge by race and class for children's economic and non-monetary outcomes in adulthood, as well as for parental employment rates by race and class. These divergent trends by race and class foreshadow the results below. We also find large decreases across birth cohorts in early adulthood marriage rates and modest increases in mortality for nearly every subgroup, showing that our disaggregated trends by race and class are consistent with the aggregate trends documented in recent work (e.g., Case and Deaton, 2015; Autor, Dorn and Hanson, 2019; Schwandt et al., 2021; Kearney, 2022).

III Trends in Economic Mobility by Race and Class

This section documents trends in children's outcomes by race and parental income ("class"). We first characterize trends in economic mobility and other non-monetary outcomes for white and Black children, the two racial groups exhibiting the largest changes over the period we study. We then examine how these trends differed across counties. Finally, we summarize changes in economic mobility for all racial and ethnic groups, including Hispanic, Asian, and American Indian children.

III.A Economic Mobility for White and Black Americans

Figure Ia plots the mean household income rank of children in adulthood versus the household income rank of their parents, separately for white and Black children in the 1978 and 1992 birth cohorts. We measure children's incomes at age 27 and parental income as the average income when the child is ages 13-17. Both children's and parents' incomes are measured in percentiles that rank children based on their incomes relative to all other children in the same birth cohort and rank parents based on their incomes relative to all other parents with children in the same birth cohort.

Figure Ia shows that the incomes of white children from high-income families increased from the 1978 to 1992 birth cohorts, while the incomes of white children from low-income families fell. Children growing up in families at the 25th percentile of the national income distribution reached, on average, the 48.4th percentile in the 1978 cohort, but only the 46.1st percentile in the 1992 cohort. Over the same period, white children growing up in families at the 75th percentile of the national income distribution saw their mean income rank rise from the 59.5th to the 60.2nd percentile. The relationship between white children's and parents' income ranks steepened across the entire income distribution, increasing the intergenerational correlation of income ranks from 0.23 in the 1978 birth cohort to 0.29 in the 1992 birth cohort.[15]

In contrast, the incomes of Black children increased across the parental income distribution, leading to an upward shift rather than a steepening of the relationship between children's and parents' income ranks. Black children born to parents at the 25th percentile of the national income distribution reached, on average, the 33.5th percentile in the 1978 cohort and the 35.1st percentile in the 1992 cohort. As a result, the white-Black race gap for children born to low-income families narrowed. The income ranks of Black children born to families at the 75th percentile of the national income distribution increased by 1.4 percentiles on average, similar to the change for white children born to families at the 75th percentile of the national income distribution. The white-Black race gap for children from high-income families thus remained essentially unchanged.

To summarize, between the 1978 and 1992 birth cohorts, the gap in incomes between white children raised in low- versus high-income families grew—a pattern we term *growing white class gaps*—while the gap in incomes between white versus Black children raised in low-income families fell—a pattern we term *shrinking white-Black race gaps*. We focus on the white-Black race gap for children from low-income families in what follows because over three-quarters of Black children are born to parents with below-median incomes (Table II).

Figure Ib examines the evolution of these race and class gaps across cohorts. The orange series plots the white class gap, defined as the difference in mean household income ranks in adulthood for white children born to families at the 25th versus 75th percentiles of the national income distribution. The green series plots the white-Black race gap, defined as the difference in mean household income ranks in adulthood for white versus Black children born to families at the 25th percentile of the national income distribution. The white class gap increased by 28% between the 1978 and 1992 birth cohorts, from 11.1 to 14.1 percentiles, while the white-Black race gap decreased by 27%, from 14.9 to 10.9 percentiles.[16]

The white-Black race gap shrank primarily because of changes in children's chances of rising out of poverty rather than their breaking into the upper class. In the 1978 birth cohort, Black children born to families in the bottom household income quintile were 14.7 percentage points more likely to remain in the bottom quintile than their white counterparts (Figure IIa). By the 1992 birth cohort, the white-Black race gap shrank to 4.1 percentage points—a 72% reduction. About half of this change was driven by a reduction in Black children's chances of remaining in the bottom quintile; the other half was driven by an increase in white children's chances of remaining in the bottom quintile. In contrast, both white and Black children's chances of reaching the top household income quintile, conditional on being born to families in the bottom quintile, changed much less. The white-Black race gap using this measure fell by only 1.9 percentage points (17%) between the 1978 and 1992 birth cohorts (Figure IIb).[17]

[15]The relationship between children's and parents' income ranks is flatter than in Chetty et al. (2014*a*) and Chetty et al. (2020*b*) because we measure children's income at age 27, compared to ages 29-30 in Chetty et al. (2014*a*) and ages 31-37 in Chetty et al. (2020*b*). The relationship between children's and parents' income ranks in our Census and tax data is similar to that reported by Chetty et al. (2014*a*) when we use children's income at ages 29-30 and focus on the same cohorts (Appendix Figure A.2). We also find similar trends in mobility by race and class when measuring household income at later ages (Appendix Figure A.3a).

[16]See Appendix Table A.6 for statistics on the levels of mean household income ranks by race, class, and birth cohort.

[17]For white children born to families in the top household income quintile, the chances of remaining in the top quintile increased

The changes in children's outcomes at the bottom of the income distribution are driven largely by changes in the fraction of children who were employed in early adulthood (Appendix Figure A.5). For example, in the 1978 birth cohort, Black children growing up in families at the 10th percentile of the national income distribution were 1.9 percentage points less likely to be employed at age 27 compared to their white counterparts. By the 1992 birth cohort, this employment gap had reversed, with Black children growing up in families at the 10th percentile of the national income distribution 4.3 percentage points more likely to be employed than their white counterparts.

We find similar patterns of growing white class gaps and shrinking white-Black race gaps when examining alternative measures of income such as individual earnings or household income measured in dollars rather than percentile ranks (Appendix Figure A.6). We also find similar trends in individual income ranks for men and women (Appendix Figure A.7). The magnitude of the change in the white-Black race gap for men is larger than for women, but largely because the starting level of the white-Black race gap in mobility is much larger for men than women (Chetty et al., 2020*b*).

Our findings are also insensitive to a variety of specification choices, such as how we measure children's and parents' incomes and how we construct our sample (Appendix Figure A.3). For example, we find that the white class gap increased by 23% and the white-Black race gap decreased by 17% when we measure household income for children at age 32 in the 1978 to 1987 birth cohorts, measured after the 2008 financial crisis and hence within the same business cycle. We also find similar trends when we (i) measure parental income using all available years in which the child is ages 0-18; (ii) measure parental income using the average across one year each in early, middle, and late childhood (thereby using the same number of years to measure parental income for all birth cohorts); (iii) measure parental income using only the mother's household income when the child is ages 13-17 (thereby ignoring the father's income if the two parents are not living together); (iv) match children to parents using only the first two years of available data when the child is ages 13-17 (thereby using the same number of years to match all birth cohorts); or (v) limit the sample to children whose parents were born in the U.S. (thereby eliminating any changes arising from differences in the share of immigrants across birth cohorts).

III.B Pre-Labor Market and Non-Monetary Outcomes

We find similar patterns of growing white class gaps and shrinking white-Black race gaps for a range of non-monetary outcomes, including mortality rates (Figure IIIa), marriage rates (Appendix Figure A.8) and incarceration rates (Appendix Figure A.9). For example, the white class gap in early adulthood (ages 24-27) mortality more than doubled between the 1978 and 1992 birth cohorts while the white-Black race gap decreased by 77%.[18] These results show that growing white class gaps and shrinking white-Black race gaps

while chances of falling to the bottom quintile did not change significantly. See Appendix Tables A.7-A.11 for quintile transition matrices for children in the 1978 and 1992 birth cohorts for all race groups. See Appendix Figure A.4 for the evolution of the white-Black race gap for children born to families in the bottom quintile of the national income distribution and the white class gap for white children born to families in the top versus bottom quintiles of the national income distribution.

[18] Our findings are consistent with recent work documenting an increase in mortality for white individuals at the bottom of the education distribution (e.g., Meara, Richards and Cutler, 2008; Cutler and Lleras-Muney, 2010; Cutler et al., 2011; Olshansky et al., 2012; Case and Deaton, 2020; Case, Deaton and Stone, 2020; Novosad, Rafkin and Asher, 2022) and growing white class gaps and shrinking white-Black gaps in arrest and incarceration rates across recent cohorts (e.g., Neil and Sampson, 2021; Muller and

reflect broad societal changes that affect children's life outcomes on many dimensions beyond their incomes.

The divergent trends by race and class emerge even before children enter adulthood, indicating that they are not driven simply by changes in the labor market. Figure IIIb plots trends in race and class gaps in educational attainment (years of completed education) using data from the ACS. The white class gap in educational attainment increased by 20% due to growing white class gaps in four-year college graduation rates (Appendix Figure A.10b). Meanwhile, the white-Black race gap in educational attainment disappeared by the 1992 birth cohort due to narrowing white-Black race gaps in high school graduation rates (Appendix Figure A.10a).

Figures IIIc and IIId show similar trends in the share of children taking the SAT/ACT at the end of high school (an indicator of intention to apply to a selective college) and in the mean SAT/ACT percentile ranks among those taking one of the tests. The white class gap in the share of children taking the SAT/ACT increased by 12.1 percentage points between the 1980 and 1991 birth cohorts, while the white-Black race gap decreased by 20.3 percentage points. Among children taking the SAT/ACT, we find an especially large increase in the white class gap in mean SAT/ACT scores, which grew by 62% between the 1980 and 1997 birth cohorts.[19] As with the other trends documented above, these patterns are driven by a combination of improving outcomes among low-income Black families and high-income white families coupled with deteriorating outcomes among low-income white families (Appendix Table A.12).

III.C Geographic Variation

We now examine how widespread the divergent trends by race and class were across different parts of the country. Figure IV shows the mean household income rank by county for white and Black children born to families at the 25th percentile of the national income distribution in the 1978 and 1992 birth cohorts. Following the methods developed in Chetty et al. (2020*a*), we estimate these mean ranks using a lowess-transformed regression of children's ranks on parents' ranks within each county-by-race-by-cohort cell, weighting by the proportion of their childhood (through age 18) that children spend in a given county (see Appendix A for details and Appendix Tables A.13-A.18 for the mean household income ranks in the 50 largest counties by population for different groups). We color the maps so that the purple colors represent areas with the lowest levels of upward mobility, while the blue-green colors represent areas with the highest levels of mobility. We use the same color scale across both sets of maps to facilitate visualization of changes in mobility across cohorts.

From the 1978 to 1992 birth cohorts, children's incomes in adulthood fell for low-income white families in nearly every part of the country. However, the magnitudes of these changes varied substantially across areas. Outcomes deteriorated the most for low-income white families in areas that were historically better for these families, such as the Great Plains and the coasts. The declines were more modest in areas that were historically worse for these families, such as Appalachia and the industrial Midwest (Appendix Figure A.11a). For example, in the 1978 cohort in the San Francisco Bay area and many parts of New England,

Roehrkasse, 2022).

[19]The changes in mean SAT/ACT scores are likely attenuated by the changing selection of test takers during our sample period. Students on the margin of taking the SAT/ACT tend to have lower scores than students who always take these tests (Dynarski, 1987; Dynarski and Gleason, 1993; Clark, Rothstein and Schanzenbach, 2009).

white children growing up in low-income families enjoyed fairly high rates of upward mobility. But by the 1992 cohort, white children growing up in low-income families in these areas had levels of economic mobility comparable to the Southeast and industrial Midwest (e.g., Ohio and Michigan), which had low levels of mobility throughout the period we study (Appendix Table A.13).

In stark contrast to the trends for low-income white families, outcomes improved for low-income Black families in most parts of the country, with the largest improvements in areas that were historically worse for families (Appendix Figure A.11b). For example, economic mobility increased sharply in the Southeast and the industrial Midwest between the 1978 and 1992 birth cohorts (Appendix Table A.14). Despite these gains, there are still vast and widespread white-Black race gaps in economic mobility for children born to low-income families even in the 1992 birth cohort, as underscored by the different range of the color scale used in Panel B versus panel A of Figure IV. Black children born in 1992 in counties with the highest levels of upward mobility for Black children still have poorer outcomes in adulthood on average than white children born in counties with the lowest levels of upward mobility for white children.

For high-income white families, economic mobility generally improved across most of the country, with the largest improvements in areas that were historically worse for these families, such as the Northeast and industrial Midwest (Appendix Figure A.11c, Appendix Figure A.12a, and Appendix Table A.15). High-income Black families also experienced improvements in outcomes in most areas (Appendix Figure A.11d, Appendix Figure A.12b, and Appendix Table A.16), although estimates for this subgroup are noisier due to the small sample sizes of high-income Black families in many counties.

The changes in outcomes across cohorts for white and Black children raised in low-income families have a strong positive correlation of 0.58 across areas (Appendix Figure A.13a). The areas in which outcomes of children born to low-income Black families improved the most tend to be areas in which outcomes of children born to low-income white families deteriorated the least. This positive correlation indicates that the gains for children born to low-income Black families did not come directly at the expense of their white counterparts in the same areas.

Although mobility generally improved more (in the case of Black families) or fell the least (in the case of low-income white families) in areas with the lowest levels of mobility in 1978, this pattern is not universal. For example, Charlotte (Mecklenburg County, NC) and Atlanta (Fulton County, GA) both had similarly low rates of economic mobility for children born in 1978, especially for children born to low-income Black families. Economic mobility increased sharply in Charlotte by 3.7 income ranks for children born to low-income Black families, with upward mobility reaching the national average for low-income Black families by the 1992 birth cohort. However, economic mobility remained low in Atlanta, where children born to low-income Black families experienced virtually no change in mobility between 1978 and 1992 (Appendix Table A.14).

As another example, Grand Rapids, MI and Milwaukee, WI—two Rust Belt cities—both had rates of economic mobility around the national average for children born to low-income white families in 1978. By 1992, however, mobility in Milwaukee fell sharply for children born to low-income white families, while mobility in Grand Rapids rose above the national average for these children. More generally, the most improved counties (in the case of Black families) and the most stable counties (in the case of low-income white families) include a diverse array of places across the country (Appendix Tables A.14, A.13).

In summary, the national trends we documented above occurred throughout the U.S., but the magnitude of the changes varied substantially across areas. Below, we use this variation to identify the drivers of changes in economic opportunity.

III.D Summary of Changes by Race and Ethnicity

We conclude this section by summarizing trends in economic mobility for all the race and class groups we study, including Hispanic, Asian, and American Indian (AIAN) children.

Table II reports the change in mean household income rank for children in the 1978 and 1992 cohorts born to low- and high-income families by race and ethnicity. The first two rows replicate the estimates for white and Black children shown in Figure I, while the remaining three rows show results for Hispanic, Asian, and AIAN children. Changes for the Hispanic, Asian, and AIAN children were generally much more modest than the changes for white and Black children. For children born to low-income families, for example, the mean household income ranks were unchanged for Asian children, and income ranks increased by only about 0.5 percentiles for Hispanic and AIAN children.[20]

In light of these results, we focus on identifying the sources of the divergent trends in mobility by race and class for white and Black Americans in the next section, and then test whether the mechanisms we identify can explain the full set of trends by race and class shown in Table II.

IV Mechanisms Underlying Changes in Mobility

In this section, we study the mechanisms underlying the changes in economic mobility documented above. We consider three classes of mechanisms: (i) changes in family-level factors such as education and wealth; (ii) changes in neighborhood-level factors that are common across subgroups such as schools and labor demand; and (iii) changes in community-level factors that differ across race and class groups within an area such as social influences.

IV.A Family-Level Factors

One natural hypothesis for the divergent trends in children's outcomes by race and class is that the family inputs that impact children's outcomes—e.g., parental education, wealth, occupation, and marital status—trended differently by race and class. For example, children raised in low-income families are increasingly likely to grow up with a single parent compared to children raised in high-income families (e.g., Lundberg, 2017; Kearney, 2022). If growing up with a single parent reduces a child's income in adulthood (Kearney, 2022), then trends in parental marital status could lead to growing class gaps in children's outcomes.

[20] An exception to this pattern are American Indian children born to high-income families. These children exhibit a significant increase in incomes but constitute only 0.3% of our sample, making it difficult to draw reliable inferences about trends, particularly across areas.

We study whether family-level factors explain the growing white class gap in intergenerational mobility by estimating OLS regressions of the form:

$$\begin{aligned} y_i = \alpha + \beta_1 HighIncome_i + \beta_2 \frac{s_i - 1978}{14} + \beta_3 HighIncome_i \cdot \frac{s_i - 1978}{14} \\ + \delta_1 X_i + \delta_2 HighIncome_i \cdot X_i + \sum_{j=1978}^{1992} (\delta_{3j} \mathbb{1}[s_i = j] \cdot X_i) + \varepsilon_i, \end{aligned} \tag{1}$$

where y_i is the child's household income rank, $HighIncome_i$ is an indicator equal to 1 if the child is born to a high-income family, s_i is the child's birth cohort, and X_i is a set of family characteristics such as parental education or marital status. We define low-income families as those between the 20th and 30th percentiles of the parental income distribution and high-income families as those between the 70th and 80th percentiles of the parental income distribution, dropping all other families from the regression. We allow for the relationship between family-level factors and children's outcomes to vary with parental income and time by permitting interactions between $HighIncome_i$ and X_i and including a set of indicators for s_i and X_i. We divide $s_i - 1978$ by 14 when including it linearly so that it ranges from 0 to 1 and the coefficient β_3 can be interpreted as the average change in the white class gap in household earnings between the 1978 and 1992 birth cohorts (a 14 year span), holding fixed family-level factors X_i.[21]

The orange bars in Figure V report estimates of β_3 from Equation (1). The first orange bar reports our baseline estimates with no controls, and shows that the white class gap increased by 3.37 percentiles between the 1978 and 1992 birth cohorts. The next four bars illustrate how controlling for family-level factors X_i, such as parental education, wealth, occupation, and marital status, affects the change in the white class gap between the 1978 and 1992 birth cohorts. The fifth bar reports β_3 controlling for all these family-level factors together.[22]

Changes in parental education, wealth, occupation, and marital status each explain very little of the changes in intergenerational mobility for white children in recent cohorts. Controlling for all these family-level factors together, we estimate that the white class gap grew by 3.13 percentiles between the 1978 and 1992 birth cohorts, 7% smaller than our baseline estimate with no controls. Hence, observable family characteristics explain a small share of the growing white class gap in intergenerational mobility.

We similarly study the role of family-level factors in explaining the shrinking white-Black race gap in

[21] Estimating Equation (1) using OLS provides a consistent estimate of the average change in the white class gap integrating over the distribution of X_i under the parametric assumptions made in Equation (1). Conditioning on X_i using more flexible methods such as non-parametric or propensity score reweighting yields very similar conclusions.

[22] Parental education, wealth, and occupation are observed only for children with at least one parent in the ACS or Census long form. For specifications that control for these variables, we first estimate the unconditional change in the white class gap β_3 in the ACS or Census long form subsample and the change in the white class gap after controlling for a given covariate. We then multiply the ratio of these two estimates (i.e., the fraction of the change in the white class gap that remains after controlling for X_i within the ACS or Census long form subsample) by the change in the raw white class gap in the full sample to generate the estimates reported in Figure V.

intergenerational mobility by estimating OLS regressions of the form:

$$\begin{aligned} y_i = \alpha + \beta_1 White_i + \beta_2 \frac{s_i - 1978}{14} + \beta_3 White_i \cdot \frac{s_i - 1978}{14} \\ + \delta_1 X_i + \delta_2 White_i \cdot X_i + \sum_{j=1978}^{1992} (\delta_{3j} \mathbb{1}[s_i = j] \cdot X_i) + \varepsilon_i, \end{aligned} \tag{2}$$

where $White_i$ is an indicator equal to 1 if the child is white and all other variables are defined as above. In this specification, we restrict the sample to white and Black families between the 20th and 30th percentiles of the parental income distribution. Here, the coefficient β_3 measures the change in the white-Black race gap in household earnings between the 1978 and 1992 birth cohorts, holding fixed family-level factors X_i.

The green bars in Figure V report estimates of β_3 from Equation (2). The first green bar reports our baseline estimate with no controls, and shows that the white-Black race gap decreased by 4.16 percentiles between the 1978 and 1992 birth cohorts. Controlling for all of the available family-level factors together yields an estimate of a 4.56 percentile decrease, showing that observable family characteristics do not explain the shrinking white-Black race gap in intergenerational mobility.

IV.B Neighborhood-Level Factors

Given the degree of residential segregation by race and class in the U.S., a second natural hypothesis for the differential trends in outcomes is that the economic shocks that impact children's outcomes differed across places in a way that correlates with race and class. For example, places with predominantly white populations may have experienced more negative economic shocks than places with predominantly Black populations, leading to differences in children's outcomes by race that emerge simply because of where children grow up.

To evaluate the importance of such neighborhood-level factors, we examine changes in economic mobility among children growing up in the same place. We use the same regression specifications as in Equations (1) and (2), but with X_i now representing fixed effects for the modal place (county or Census tract) in which child i lives during childhood (from birth until age 18). Because these specifications include interactions between place fixed effects and cohort indicators, they net out differential trends in outcomes across places. The coefficient β_3 can therefore be interpreted as average changes in class or race gaps among children growing up in the same area.[23] The final two pairs of bars in Figure V show that estimates controlling for either childhood county or childhood Census tract fixed effects are essentially identical to our baseline estimates. These neighborhood-level controls explain only 7% of the growing white class gap and only 4% of the shrinking white-Black race gap.[24] In short, trends in children's earnings diverge sharply by race

[23] Controlling for differences in location by reweighting the distribution of locations for one race and class group to match the other race and class group yields similar results.

[24] The estimates controlling for childhood county are obtained from the subsample of counties with at least one child in each parental income group (for the white class gap) or race group (for the white-Black race gap). The estimates controlling for childhood Census tract are obtained from the subsample of tracts with at least one child in each parental income group (for the white class gap) or race group (for the white-Black race gap). For both specifications, we compute the fraction of the gap that remains after controlling for the fixed effects in the relevant estimation sample as above. We then multiply this fraction by the unconditional gap in the full sample to generate the estimates reported in Figure V.

and class even among children who grow up in the same neighborhood and who share the same observable family characteristics.

IV.C Community-Level Factors

The preceding results indicate that the divergent trends in economic mobility must be driven by changes that affect race and class groups differently within neighborhoods. One set of factors that could generate such differential impacts are changes in the environments in which children grow up (Wilson, 1996; Chetty, Hendren and Katz, 2016; Chetty and Hendren, 2018*a*). Individuals tend to be highly segregated by race and class even within neighborhoods (Wimmer and Lewis, 2010; Chetty et al., 2022), creating scope for individuals in the same neighborhood to experience differential changes in financial and human capital (e.g., the classes children take or the places their parents work) and social capital (e.g., the people children interact with and are influenced by).

Motivated by this reasoning, we study the relationship between changes in children's outcomes and changes in their childhood *community*, defined as other families who share the same race, class, and childhood county. Numerous factors could have changed over time at the community level. We start by focusing on a factor emphasized in sociological research: adults' employment rates. For example, Wilson (1996) argues, based on ethnographic studies of Black families in the South Side of Chicago, that "Many of today's problems...crime, family dissolution, welfare, low levels of social organization, and so on...are fundamentally a consequence of the disappearance of work." Case and Deaton (2020) similarly argue that the disappearance of work in low-income white communities explains the rising morbidity and mortality rates among less-educated white individuals in recent decades.[25] Inspired by this prior work, we examine the association between changes in children's outcomes and changes in adults' employment rates across communities.

Baseline Results. We begin by estimating community-level changes in children's income ranks between the 1978 and 1992 birth cohorts. We estimate children's mean household income ranks at age 27 in each county-by-race-by-cohort cell using a lowess-transformed regression of children's ranks on parents' ranks, weighting children by the proportion of their childhood (up to age 18) that they spend in a given county, as in the maps in Section III.C (see Appendix A for details). We then regress the resulting estimates for the 25th and 75th percentiles of the parental income distribution on birth cohort (divided by 14) within each county-by-race cell. The coefficients from these linear regressions provide county-by-race-by-class estimates of the change in mean outcomes from the 1978-1992 cohorts.

Figure VI presents a binned scatterplot of changes in children's income ranks versus changes in the employment rates of same-race adults in their childhood counties, weighted by subgroup population counts. We include white and Black children with parents at the 25th and 75th income percentiles and absorb race-by-parental income percentile fixed effects, so that the plot can be interpreted as the average relationship between outcomes and parental employment across counties within each of the four race-by-class groups.

[25] Prior work suggests that changes in community-level employment rates could directly impact children's outcomes for several reasons, including changes in aspirations and attitudes towards work; changes in identity; and changes in mental health, substance abuse, and family dynamics (e.g., Wilensky, 1961; Darity and Goldsmith, 1996; Clark, Knabe and Rätzel, 2010; Luechinger, Meier and Stutzer, 2010; Brand, 2015). As we discuss later, changes in parental employment rates may also be correlated with other factors that affect children's outcomes in a community, such as parental marriage rates and parental income.

Changes in employment are measured at the county-by-race level as the difference in employment rates among adults aged 25-44 in the 2000 versus 1980 decennial Censuses. This corresponds to changes over the period when children in our focal birth cohorts were growing up: age 2 (in 1980) for children in the 1978 birth cohort and age 8 (in 2000) for the 1992 birth cohort.[26] Figure VI shows a clear positive relationship: children's incomes increased more across cohorts in counties where employment rates among adults of the same race during their childhood increased more.

Although this correlation is consistent with the hypothesis that community-level changes are associated with changes in children's outcomes, the county-by-race measures of employment used in Figure VI cannot explain the divergent trends across race and class groups because they do not vary across class. To analyze whether changes in adults' employment rates can explain the divergent trends, we must construct measures of parental employment rates at the county-by-race-by-class level. Because we define class based on parental income during childhood, we measure parental employment rates after children turn 18.[27] In our baseline analysis, we measure parental employment when the child is 27 years old because our information on parental employment (obtained from W-2 forms) starts in 2005, when children in the 1978 cohort are age 27. Although this approach measures parental employment after children have typically left home, we interpret these measures as proxies for childhood environments because they are likely to capture labor force attachment and other latent factors whose roots emerged while children were growing up.[28] Consistent with this interpretation, the relationship between children's outcomes and adults' employment rates during their childhood shown in Figure VI is very similar to the relationship between children's outcomes and parental employment rates when children are 27 years old (Appendix Figure A.15). We show below that our findings are not sensitive to the time point at which parental employment rates are measured because most of the changes in parental employment rates are driven by differences across cohorts that are stable over time—and therefore likely reflect latent factors that were present during childhood—rather than year-specific shocks.

Figure VIIa presents a binned scatterplot of changes in children's mean household income ranks versus changes in parental employment rates by county, separately for three subgroups: white and Black children with low-income (25th percentile) parents and white children with high-income (75th percentile) parents. We focus on white and Black children with low-income parents and white children with high-income parents because these subgroups drive the growing white class gaps and shrinking white-Black race gaps we focus on in this paper.[29] We estimate community-level changes in parental employment rates using the same smoothing method as for child income ranks above, but change the outcome variable to the share of parents who are employed (defined as described in Section II) when their child is 27 years old. In each subgroup,

[26] We multiply the changes in employment rates over the 20 year period by 14/20 to obtain an estimate of the trend across 14 cohorts, assuming linear trends.

[27] An alternative would be to define class using fixed measures such as parental education. However, there are significant differential trends in children's outcomes by parental income during childhood even conditional on parental education (Appendix Figure A.14). As a result, conditioning on parental income during childhood is essential to fully capture the divergent trends in intergenerational mobility documented above.

[28] For example, if the communities where parents stop working at earlier ages have broadly declining employment prospects or reduced investment in educational resources, children's human capital and aspirations may be affected even before the parents in their community ultimately stop working.

[29] We find a positive relationship between changes in parental employment rates and children's outcomes for Black children raised in high-income families as well, but estimates are noisier due to small sample sizes in many counties (Appendix Figure A.16).

counties are binned into twenty population-weighted bins based on the change in parental employment rate, so that each bin contains an equal number of children for that subgroup. The dots represent population-weighted averages for the counties in that bin, showing the mean change in children's ranks versus the mean change in parental employment rates for the counties in that bin.

For every race and class group, changes in children's outcomes across cohorts are strongly positively correlated with changes in parental employment rates in their community. For example, the outcomes of white children with low-income parents deteriorated much more sharply in areas where employment rates for low-income white parents fell more. For those growing up in communities where parental employment rates fell the most, mean household income ranks fell by 3.7 percentiles between the 1978 and 1992 cohorts. In contrast, for those growing up in communities where parental employment remained more stable, mean household income ranks did not change meaningfully. Black children raised in low-income families and white children raised in high-income families also fared worse if they grew up in communities with falling parental employment rates.

Importantly, the relationship between children's outcomes and parental employment rates is virtually identical across the three subgroups. Fitting a single regression line through the community-level data has an adjusted R^2 of 0.41; permitting different slopes and intercepts across subgroups increases the adjusted R^2 by only 0.01 relative to this baseline. This result implies that most of the changes in the white class gap and white-Black race gap can be explained by the sharp fall in employment rates for low-income white parents relative to low-income Black and high-income white parents during the period we study (Appendix Figure A.17).[30] Consistent with these results, including community-by-cohort parental employment rates as controls in Equations (1) and (2) fully explains the observed growth in the white class gap and explains 57% of the reduction in the white-Black race gap (Appendix Figure A.18).

The relationship between changes in children's outcomes and changes in community-level parental employment rates is not driven by children's own parents' employment status. When we replicate Figure VIIa on the subsample of children whose parents are employed when they are age 27, the relationship between changes in children's outcomes and changes in parental employment rates persists (Appendix Figure A.19e). Controlling for baseline levels of outcomes in the 1978 cohort—to account for the fact that the areas that exhibited the greatest improvements in outcomes tended to have the lowest levels at baseline (Appendix Figure A.11)—also does not change the relationship between changes in children's outcomes and changes in parental employment rates (Appendix Table A.19).

We also find very similar patterns for changes in children's educational attainment and end-of-high-school SAT/ACT scores (Figures VIIb, c) and changes in children's mortality rates in early adulthood (Appendix Figure A.20), showing that changes in community-level parental employment rates can explain the divergent trends in both monetary and non-monetary outcomes documented above. Since the educational outcomes are measured before children enter the labor market, the link between changes in children's out-

[30] These differential trends in employment rates are consistent with publicly available statistics. For example, national statistics exhibit a shrinking white-Black gap for the employment-population ratio over this period (Bureau of Labor Statistics, 2005-2019). Measures of adults' employment rates by race and education (an alternative proxy for class) constructed from publicly available cross-sectional datasets such as the ACS also show qualitatively similar changes in the white class and white-Black race gaps, although the magnitudes of the changes are attenuated relative to what we observe in our longitudinal data for reasons discussed in Appendix B).

comes and changes in parental employment rates cannot be a mechanical consequence of changes in labor market opportunities.

Other Measures of Parental Employment. We find a similar relationship between changes in children's outcomes and changes in parental employment rates when measuring parental employment at different points in time. For example, measuring parental employment in a fixed calendar year (e.g., 2012 or 2019) for all cohorts—and hence at different child ages—yields similar results (Appendix Figures A.19a, b). While we cannot measure parental employment rates at earlier ages for all cohorts (due to a lack of W-2 information in the early years of our sample), we can measure parental income at all ages (because 1040 tax forms are available in all years). We find similar relationships between changes in children's outcomes and changes in mean community-level parental income ranks measured when the child is age 27 or when the child is age 22 (Appendix Figures A.19c, d).

Our findings are not sensitive to the calendar year in which we measure parental employment because most of the variation in parental employment rates arises from differences across *cohorts* rather than across calendar years. In our baseline analysis, we measure parental employment rates when children are 27 years old, incorporating variation both across children's birth cohorts (1978-1992) and across the calendar years (2005-2019) for which employment is measured. When we construct measures based solely on cross-cohort variation (by estimating linear trends in parental employment across the 1978-1992 birth cohorts with calendar year fixed effects) and solely on cross-year variation (by estimating linear trends in parental employment across the 2005-2019 calendar years with cohort fixed effects), we find that the correlation between our baseline measure and the cross-cohort measure is stronger in all subgroups than the correlation with the cross-year measure (Appendix Figure A.21). These findings show that changes in parental employment rates largely capture work patterns for the parents of a given cohort rather than year-specific labor market shocks.

Other Measures of Community-Level Change. We find similar relationships between changes in children's outcomes and other measures of community-level change. Changes in children's outcomes are strongly positively correlated with changes in parental marriage rates (Figure VIId) and negatively correlated with changes in parental mortality rates (Appendix Figure A.19f).[31] As with parental employment rates, these correlations hold both across subgroups and within subgroups across area. In multivariable regressions estimated at the county-by-race-by-class level, changes in both parental employment rates and marriage rates are highly significant predictors of changes in children's outcomes in all subgroups (Appendix Table A.20).

These findings show that changes in parental employment rates are just one of many community-level factors that predict changes in children's outcomes. The broader point is that community-level changes in the parents' generation (which can be measured using a variety of parental outcomes) are strongly correlated with their children's outcomes in adulthood.

Explaining Trends in Economic Mobility Across Groups. The same community-level factors that explain changes in outcomes for white and Black children can also explain the (smaller) changes we observe for

[31] We analyze the relationship between children's outcomes and parental mortality rates at the commuting zone level rather than the county level to increase precision, as mortality in early adulthood is rare.

other subgroups. Figure VIII plots national-level changes in children's household income ranks versus changes in parental employment rates for all race and class groups.[32] The correlation between these variables is 0.91. The slope of the between-group relationship in Figure VIII is 0.37, nearly identical to the within-group slope in Figure VIIa. We find similarly strong correlations between national-level changes in children's outcomes and changes in parental marriage rates and parental mortality rates for all race and class groups (Appendix Figure A.22).

We conclude that community-level changes—as proxied by parental employment rates or other outcomes in the parental generation—provide a unified explanation (in a predictive, not causal sense) for the divergent trends in children's outcomes by race and class in recent decades. In the rest of the paper, we study the sources of this association, focusing on the correlation between children's outcomes and parental employment rates because we can measure parental employment with greater precision than other community-level factors across all subgroups and birth cohorts.

V Causal Effects of Changes in Childhood Environments

One explanation for the correlation between changes in parental employment rates and changes in children's outcomes in adulthood is that the changes in childhood environments associated with changes in parental employment rates have a causal exposure effect on children's outcomes.[33] A second explanation is that the correlation is driven by common shocks in adulthood (e.g., to local labor demand) that affect both parents and children directly. A third possibility is that there are compositional changes in the types of families who live in areas with declining versus improving employment prospects, leading to changes in children's observed outcomes through selection effects. In this section, we distinguish between these explanations by estimating the causal effect of changes in childhood environments on children's outcomes in adulthood.

V.A Empirical Framework

Statistical Model. We structure our empirical analysis using a statistical model of intergenerational mobility and neighborhood effects that generalizes the model in Chetty and Hendren (2018*b*) by allowing neighborhood effects to vary across birth cohorts and subgroups.

Let y_i denote a child i's income (or other outcome), measured in adulthood at age T. We model y_i as a function of three factors: the neighborhoods where the child grows up, labor demand shocks in the area where the child lives at age T, and all other non-neighborhood factors, such as family inputs.[34]

Let $c(i,a)$ denote the neighborhood in which child i lives at age $a = 1,...,A$ of her childhood, where $A < T$. Let μ_{cprs} denote the causal effect of one additional year of exposure to neighborhood c on y_i for children in parental income (class) p, race r, and birth cohort s. Based on evidence from prior work on

[32]We omit AIAN children with high-income parents given the small size of this subgroup.

[33]These changes in childhood environment could affect children through parents, or could independently affect both children and parents. For example, a drug epidemic could have both an exposure effect on children's outcomes and a direct effect on parental outcomes.

[34]Prior work also typically allows for age-specific disruption costs of moving between neighborhoods. We omit those costs here without loss of generality because all of our estimators compare individuals who move at the same age and thus any such disruption costs are netted out of our estimates.

neighborhood effects (Chetty and Hendren, 2018*a*; Deutscher, 2020; Chyn and Katz, 2021), we assume that the childhood exposure effect μ_{cprs} is constant for ages $a \leq A$ and zero thereafter.[35] Following recent evidence from Sprung-Keyser and Porter (2023), we also permit place and subgroup-specific labor demand shocks η_{cprs} that are independent of exposure and directly affect children's outcomes based on their location at age T. Finally, let θ_i denote the impact of other factors, such as family inputs. Combining these three components, children's outcomes are given by:

$$y_i = \sum_{a=1}^{A} \mu_{c(i,a)prs} + \eta_{c(i,T)prs} + \theta_i \tag{3}$$

Target Estimand. We are interested in identifying the extent to which the correlation between changes in parental employment rates and changes in children's outcomes documented in Section IV.C is driven by changes in causal childhood exposure effects (μ_{cprs}).

To define the target estimand formally, consider two groups of children born in different years $s = 0$ and $s = 1$ who live in community c throughout their lives. Let $\bar{y}_{cprs} = E[y_i|c(i,a) = c, p, r, s]$ denote the average outcome of children of parental income p, race r, and cohort s. Let $\Delta\bar{y}_{cpr} = \bar{y}_{cpr,s=1} - \bar{y}_{cpr,s=0}$ denote the change in average outcomes across cohorts and $\Delta\bar{e}_{cpr} = \bar{e}_{cpr,s=1} - \bar{e}_{cpr,s=0}$ denote the change in parental employment rates across cohorts. Using this notation, the OLS regression coefficient estimated in Figure VII can be written as:

$$\beta = \frac{Cov(\Delta\bar{y}_{cpr}, \Delta\bar{e}_{cpr})}{Var(\Delta\bar{e}_{cpr})}$$

Under the statistical model in Equation (3), this regression coefficient can be decomposed into three components:

$$\beta = \beta_\mu + \beta_\eta + \beta_\theta, \tag{4}$$

where

$$\beta_\mu = \frac{Cov(A\Delta\mu_{cpr}, \Delta\bar{e}_{cpr})}{Var(\Delta\bar{e}_{cpr})}, \beta_\eta = \frac{Cov(\Delta\eta_{cpr}, \Delta\bar{e}_{cpr})}{Var(\Delta\bar{e}_{cpr})}, \beta_\theta = \frac{Cov(\Delta\bar{\theta}_{cpr}, \Delta\bar{e}_{cpr})}{Var(\Delta\bar{e}_{cpr})}$$

Our goal is to identify β_μ, the causal effect of growing up from birth in a community with a 1 percentage point higher parental employment rate on children's outcomes, holding fixed labor demand. The key difference between our target parameter β_μ in Equation (4) and the parameters identified in the existing literature on neighborhood effects is that β_μ identifies the effect of *changes* in neighborhood effects over time within communities. In particular, β_μ measures how changes in neighborhoods' causal effects covary with changes in parental employment rates rather than how level differences in neighborhoods' causal effects covary with observable characteristics.[36]

Note that the change in exposure effects across cohorts in Equation (4) may arise both from a direct

[35] There is some evidence from recent work that neighborhood exposure effects may be larger during adolescence than earlier in childhood (Chetty et al., 2020*a*; Deutscher, 2020). Permitting exposure effects to vary smoothly with age—e.g., using a quadratic specification—does not affect our conclusions.

[36] To see why these covariances may differ, suppose μ_{cprs} consists of two components, a component that fluctuates across cohorts (e.g., peer characteristics) and a component that is fixed over the study period (e.g., physical infrastructure). These two components will have different associations with observable characteristics. Our goal is to identify the first component to explain the changes in economic mobility documented above.

effect of changes in parental employment rates (which could affect children's outcomes through changes in resources, job referrals, aspirations, etc.) as well as changes in other correlated factors (such as the quality of schools or other environmental conditions). As the definition of β_μ in Equation (4) makes clear, we do not seek to isolate the causal effect of changes in parental employment rates themselves; rather, we interpret parental employment rates as a proxy for a broader set of community-level factors that may influence children's outcomes.

Identification. To see how we can identify β_μ using data on children's outcomes, consider an experiment involving children born in an origin neighborhood o whose causal effect μ_{oprs} does not vary across cohorts, i.e., $\mu_{oprs} = \mu_{opr} \forall s$. We normalize $\mu_{opr} = 0$ for expositional simplicity.[37] Suppose we randomly assign children born in different cohorts $s = 0$ and $s = 1$ to a destination neighborhood d from age m onward. Under random assignment, average family inputs $E[\theta_i|s] = \bar{\theta}_{dpr}$ do not vary across cohorts s. Hence, the difference in average outcomes across cohorts for children of a given parental income p and race r who move to destination d at age m is:

$$\begin{aligned} \Delta\bar{y}_{dprm} &= E[y_i|c(i,a>m)=d,p,r,s=1] - E[y_i|c(i,a>m)=d,p,r,s=0] \\ &= (A-m)\Delta\mu_{dpr} + \Delta\eta_{dpr} \end{aligned} \tag{5}$$

Thus, the difference in average outcomes across cohorts reflects a combination of differences in average childhood exposure effects and differences in labor demand shocks across cohorts. To isolate the childhood exposure effect, consider how the cross-cohort change in outcomes differs between children who move to community d at birth ($m = 0$) versus the end of childhood ($m = A$):

$$\Delta\bar{y}_{dpr,m=0} - \Delta\bar{y}_{dpr,m=A} = A\Delta\mu_{dpr} \tag{6}$$

The labor demand shocks drop out of this comparison because children who move to a given destination d are exposed to the same labor demand shocks in adulthood regardless of the age at which they move.[38] It follows that

$$\frac{Cov(\Delta\bar{y}_{dpr,m=0} - \Delta\bar{y}_{dpr,m=A}, \Delta\bar{e}_{dpr})}{Var(\Delta\bar{e}_{dpr})} = \frac{Cov(A\Delta\mu_{dpr}, \Delta\bar{e}_{dpr})}{Var(\Delta\bar{e}_{dpr})} = \beta_\mu \tag{7}$$

Intuitively, under random assignment to neighborhoods, we can identify β_μ from the change in average outcomes across cohorts for children who move early versus late in childhood to an area that experienced a 1 percentage point increase in parental employment rates across cohorts. Figure IX illustrates this identification argument by plotting the average outcomes of children in cohorts $s = 0$ and $s = 1$ who are randomly assigned at birth ($m = 0$) versus at the end of their childhood ($m = A$) to a community where parental employment rates increase by $\Delta\bar{e}_{dpr} = 1$ percentage point across cohorts. The difference in mean outcomes across

[37] In our empirical analysis, we include origin-by-parent income percentile-by-race-by-cohort-by age at move fixed effects, which eliminates variation arising from differences in origin quality under the additive structural model in Equation (3).

[38] In this example, we assume that when children move to destination d as children, they remain in destination d in adulthood. This assumption eliminates any link between move age m and the labor market shock η. Sprung-Keyser and Porter (2023) note that in practice children who move to an area at a younger age are more likely to stay there as adults, potentially re-introducing η into Equation (6). We address this issue in our empirical analysis below by conditioning on locations in adulthood and other related approaches.

cohorts for those who move at age $m = A$ is driven by differences in labor demand η_{dpr} and differences in family inputs $\bar{\theta}_{dpr}$ across cohorts. The difference in mean outcomes across cohorts for those who move at birth $m = 0$ additionally includes the change in the childhood exposure effect μ_{dpr} across cohorts. Under random assignment, $\bar{\theta}_{dpr}$ does not vary with cohort or age at move, and thus the "difference-in-differences" across the four points in the figure identifies β_μ.

In observational data, estimating the covariance between changes in children's outcomes and changes in parental employment rates in Equation (7) yields a coefficient

$$b_\mu = \frac{Cov(\Delta \bar{y}_{dpr,m=0} - \Delta \bar{y}_{dpr,m=A}, \Delta \bar{e}_{dpr})}{Var(\Delta \bar{e}_{dpr})} = \beta_\mu + \frac{Cov(\Delta \bar{\theta}_{dpr,m=0} - \Delta \bar{\theta}_{dpr,m=A}, \Delta \bar{e}_{dpr})}{Var(\Delta \bar{e}_{dpr})} \tag{8}$$

where the additional selection term arises because family inputs θ_i may not be balanced across cohorts. To identify β_μ in observational data, we make the following identification assumption.

Assumption 1: Constant Selection by Age. The covariance between changes in unobserved family inputs θ_i and changes in parental employment rates $\bar{e}_{dpr}$ does not vary with the child's age of move m:

$$Cov(\Delta \bar{\theta}_{dprm}, \Delta \bar{e}_{dpr}) = \lambda \qquad \forall m = 0, ..., A \tag{9}$$

This assumption permits changes in the types of families who move to communities where parental employment rates are increasing. However, it requires that such selection effects do not vary with the child's age at move. In Figure IX, this assumption implies that $\Delta \bar{\theta}_{dpr,m=0} = \Delta \bar{\theta}_{dpr,m=A}$. Under this assumption, the difference-in-differences between the four points on this figure again identifies β_μ.

The "constant selection by age" assumption in Equation (9) underlies much of the quasi-experimental literature that uses movers designs to identify static neighborhood effects (Chetty and Hendren, 2018*a*). This assumption has been extensively validated in prior work by comparing quasi-experimental estimates based on this assumption to estimates based on random assignment or exogenous shocks as sources of variation and by implementing placebo tests that use pre-move outcomes such as birth weights (Chetty, Hendren and Katz, 2016; Chetty et al., 2020*a*, 2023; Chyn, Collinson and Sandler, 2023; Kawano et al., 2024). We therefore proceed under this assumption and then return to further assess its validity in our setting by controlling for observable family characteristics and comparing siblings' outcomes within families.

V.B Baseline Estimates of Changes in Exposure Effects

We estimate β_μ by analyzing the outcomes of children who move exactly once between counties during childhood (before age 18). We measure changes in parental employment rates $\Delta \bar{e}_{cpr}$ in each community (county) using the complement of this one-time movers sample (i.e., non-movers and children who moved more than once), weighting children in proportion to the share of their childhood spent in county c (see Appendix A for details).[39]

[39] To minimize noise in the estimates of changes in parental employment rates, we limit the analysis sample to origin and destination counties that contain more than 2,000 children from the relevant race and class group in all cohorts. See the notes to Figure X for additional details.

We structure our empirical analysis to construct a non-parametric empirical analog of Figure IX in a series of steps. First, we analyze the outcomes of children in the 1992 birth cohort who moved early in childhood (before age 8) to a new county. Figure Xa presents a binned scatterplot of household income ranks in adulthood for these young movers versus the change in parental employment rates from the 1978-1992 cohorts for children in the same race-by-class group in the destination county. We hold fixed origin quality by residualizing both household income ranks and parental employment rates on origin county-by-parental income percentile-by-race-by-age at move fixed effects. We also control for the group-specific parental employment rate in the 1978 cohort in the destination county to isolate variation arising from cross-cohort *changes* in destination counties. This approach effectively compares the outcomes of children moving from the same origin county to destinations with the same initial parental employment rate but different trends later on.

Figure Xa shows that children in the 1992 cohort who moved early in childhood to counties where parental employment rates increased significantly in their own race-by-class group over the prior 15 cohorts have significantly better outcomes in adulthood. The slope of this relationship (0.26) can be interpreted as the impact of a 1 percentage point increase in $\Delta\bar{e}_{dpr}$ on children's outcomes for the 1992 birth cohort $\bar{y}_{s=1992,m<8}$, corresponding to the point in the upper right corner of Figure IX, $\bar{y}_{dpr,s=1,m=0}$.[40]

Next, we estimate analogous slopes for children who made the same moves before age 8 in earlier birth cohorts, from 1978-1992. The green series in Figure Xc plots these slope estimates by birth cohort. The last point corresponds to the slope of 0.26 for the 1992 cohort shown in Figure Xa. There is a clear upward trend in the estimates over time: children born in early cohorts do not gain much from moving to areas where parental employment rates subsequently increase, whereas those in later cohorts do.

The upward trend in the green series in Figure Xc could be driven by any of the three channels discussed above: changes in childhood exposure effects across cohorts, changes in labor demand shocks across cohorts, or changes in the types of families moving to improving communities across cohorts. To isolate the childhood exposure channel, we replicate the preceding analysis focusing on children who move late in childhood (between ages 13-17), who are exposed to the destination community for much less of their childhood. Figure Xb replicates Figure Xa for children in the 1992 cohort who move to the same destination counties between ages 13-17 instead of before age 8. We obtain a much flatter slope of 0.04, indicating that the gains from moving to a community with increasing parental employment rates are much smaller for children who move at older ages.

The orange series in Figure Xc plots analogous slope estimates for late-childhood movers by birth cohort. This relationship is much flatter across cohorts than the corresponding series for young movers, implying that increases in parental employment rates translate to much smaller changes in children's outcomes if they arrive in the destination county late in childhood. Under the identification assumption in Equation (9), this result implies that changes in childhood environments drive much of the correlation between changes in parental employment rates and changes in children's outcomes documented in Figure VII. Intuitively, if the relationship between changes in parental employment rates and children's incomes were driven by com-

[40] In Figure IX, we focus on children who move at exactly age $m = 0$ to simplify exposition; in Figure X, we include all children who move below age 8 to increase power. We adjust for the difference in move ages when estimating β_μ below.

mon shocks such as labor demand, we would expect to see significant changes in outcomes across cohorts for late-childhood movers as well. The absence of such changes directly shows that changes in childhood environments are responsible for the changes we observe in children's outcomes.[41]

Figure Xc splits children into two coarse bins by their age at move: younger than 8 versus 13 and older. To obtain a more granular picture of how outcomes vary with age at move, Appendix Figure A.23 plots the impact of a 1 percentage point increase in $\Delta\bar{e}_{dpr}$ on children's outcomes by the age at which they move to the destination county. For children born in later cohorts (1987-1992) who are exposed to increasing parental employment rates in the destination, there is a steep downward-sloping relationship: moving at an earlier age to an area with increasing parental employment rates is highly beneficial. For those born in early cohorts (1978-1983), the relationship is much flatter, as expected because these cohorts do not experience the improvements in parental employment rates that occurred for subsequent cohorts. Both series are approximately linear, showing that the impacts of changes in childhood environments on children's outcomes are proportional to the number of years of exposure they have to that environment, with roughly constant dosage effects throughout childhood.

Quantification. We translate the estimates plotted in Figure Xc into a quantitative estimate of the childhood exposure effect β_μ using regression specifications of the form:

$$\begin{aligned} y_i = {} & \beta_\mu (\Delta\bar{e}_{dpr} \times \frac{s_i - 1978}{14} \times \frac{A - m_i}{A}) \\ & + \gamma_0 \Delta\bar{e}_{dpr} + \gamma_1 (\Delta\bar{e}_{dpr} \times \frac{s_i - 1978}{14}) + \gamma_2 (\Delta\bar{e}_{dpr} \times \frac{A - m_i}{A}) \\ & + \delta_{oprsm} + \bar{e}_{dpr,s=1978} \times \kappa_{sm} + \varepsilon_i \end{aligned} \tag{10}$$

We estimate this regression using the sample of children who move exactly once across counties during childhood (before age 18) from origin county o to destination county d at age m. We eliminate variation arising from differences in origin quality by controlling for origin county-by-parental income percentile-by-race-by-cohort-by-move age fixed effects δ_{oprsm}, as in Figure X. We also control for the baseline level of group-specific parental employment rates in the destination county interacted with cohort-by-move age fixed effects $\bar{e}_{dpr,s=1978} \times \kappa_{sm}$ to isolate variation in changes in parental employment rates across cohorts.

The key independent variable of interest in Equation (10) is $\Delta\bar{e}_{dpr}$, the change in parental employment rates in the destination county between the 1978 and 1992 cohorts. Motivated by the non-parametric estimates in Figure Xc and Appendix Figure A.23, we parameterize the model to allow the relationship between children's outcomes and $\Delta\bar{e}_{dpr}$ to vary linearly by cohort and move age. The four terms involving $\Delta\bar{e}_{dpr}$ correspond to the difference-in-differences estimator in Figure IX, exploiting continuous variation in $\Delta\bar{e}_{dpr}$ across areas to maximize precision rather than considering a single place where $\Delta\bar{e}_{dpr} = 1$. The parameter γ_0 measures the effect of $\Delta\bar{e}_{dpr}$ when $m_i = A$ in the 1978 birth cohort. The parameter γ_1 measures how the effect of $\Delta\bar{e}_{dpr}$ on outcomes varies across cohorts when $m_i = A$, while γ_2 measures how the effect of $\Delta\bar{e}_{dpr}$

[41] The comparison between early and late-childhood movers just within the 1992 cohort (Figure Xa versus Figure Xb) is insufficient to identify the causal effects of *changes* in childhood environments (proxied by parental employment rates) because those differences could simply arise from time-invariant characteristics of areas of the type identified in the prior literature on neighborhood effects. The fact that children's outcomes change *differentially* across cohorts by age at move establishes that changes in environments within a community have causal effects on children's outcomes.

on outcomes varies across move ages in the 1978 birth cohort. The key parameter of interest β_μ measures the interaction of these two effects, i.e., the differential impact of $\Delta\bar{e}_{dpr}$ on outcomes for young movers in the 1992 cohort relative to older movers and those who moved in earlier cohorts. Under Assumption 1 and certain regularity conditions, β_μ identifies the causal effect of spending one's whole childhood in a destination county where parental employment rates increased by 1 percentage point (see Appendix C.1 for a formal derivation).

Column 1 of Table III reports our baseline estimate of $\hat{\beta}_\mu = 0.339$, indicating that growing up from birth in a community with a 1 percentage point higher parental employment rate leads to an increase in children's mean household income ranks at age 27 by 0.339 ranks. For comparison, the OLS regression coefficient estimated in Figure VIIa is $\hat{\beta} = 0.38$. Hence, under Assumption 1, this estimate of β_μ implies that 90% of the cross-sectional relationship between changes in parental employment rates and changes in children's outcomes is driven by changes in causal childhood exposure effects.

V.C Sensitivity Analysis

Our baseline specification compares children who move to different destinations at different ages, exploiting variation in parental employment trends between destinations, holding fixed baseline levels of parental employment rates. A potential concern with this approach is that destinations with more positive parental employment trajectories may have higher levels of causal effects $\mu_{dpr,s=1978}$, even holding fixed $\bar{e}_{dpr,s=1978}$, due to other environmental factors unrelated to parental employment rates. In this case, children who move at younger ages to communities with higher $\Delta\bar{e}_{dpr}$ in later cohorts may do better simply because those communities had better opportunities to begin with, not because of the *change* in opportunity across cohorts.

To evaluate the importance of this concern, we present an alternative estimate of β_μ that compares the outcomes of children from different cohorts who move from the same origin to the *same* destination at the same age, thereby obtaining identification purely from cross-cohort variation in parental employment rates within communities. We estimate the following regression specification:

$$y_i = \beta_\mu \left(\frac{m_i \bar{e}_{oprs} + (A - m_i)\bar{e}_{dprs}}{A} \right) + \delta_{odprm} + \kappa_{prs} + \varepsilon_i \tag{11}$$

This specification isolates variation in parental employment rates that arises solely from changes across cohorts within places by including origin county-by-destination county-by-parental income percentile-by-race-by-move age fixed effects δ_{odprm}. We also include parental income percentile-by-race-by-cohort fixed effects κ_{prs} to eliminate national variation in trends in parental employment across subgroups. To maximize precision, we exploit variation in parental employment rates across cohorts in both the origin and destination, defining the key independent variable as an exposure-weighted average of cohort-specific origin and destination parental employment rates.

Under this specification, we obtain an estimate of $\hat{\beta}_\mu = 0.273$ (Column 2 of Table III), similar to and statistically indistinguishable from our baseline estimate in Column 1.[42] This finding suggests that level

[42]The drawback of this estimator relative to our baseline approach is that it assumes that labor demand shocks and selection effects are uncorrelated with parental employment, since it compares children who move at the same age (see Appendix C.2).

differences in neighborhood effects do not drive our baseline estimates, consistent with the evidence in Figure Xc showing that there is little difference in outcomes for children in the 1978 cohort who move at younger versus older ages to places where parental employment rates subsequently improve.

Column 3 of Table III shows that we obtain very similar estimates when we replicate the specification in Column 2 and restrict the sample to children whose own parents are employed. This result shows that children's outcomes are driven by changes in their broader community rather than changes in their own parents' employment rates, consistent with the observational analysis in Appendix Figure A.19e.

The additive model in Equation (3) underlying our analysis assumes that labor demand shocks η_{dpr} are independent of the age m at which a child moves to the destination d. As discussed above, Sprung-Keyser and Porter (2023) show that in practice, children who move at an earlier age in childhood are more likely to stay in the destination as adults. In principle, this differential probability of staying in the destination could generate the preceding results purely via labor demand shocks because children who move at earlier ages may effectively be more exposed to η_{dpr}.

To evaluate this concern, in Column 4 of Table III, we replicate the baseline specification in Column 1 controlling for adult county-by-parental income percentile-by-race-by-cohort fixed effects. This specification effectively compares two children who live in the same destination county as adults but who arrived at that destination at different ages. We continue to find a substantial childhood exposure effect in this specification ($\hat{\beta}_\mu = 0.445$), indicating that differential exposure to labor demand shocks is unlikely to drive our baseline results. This finding is consistent with the evidence in Figure III showing that differences in children's outcomes emerge before they enter the labor market, as measured by educational attainment and achievement.

V.D Evaluating the Constant Selection by Age Identification Assumption

The preceding estimates of β_μ all rely on the "constant selection by age" identification assumption in Equation (9). One may be concerned about the validity of this assumption because of selection effects. For instance, families who move to areas with higher parental employment rates when their children are young may be those who invest the most in their children (higher θ_i), violating the identification assumption and spuriously generating the patterns in Figure X.

In this subsection, we assess the validity of this identification assumption using two approaches: evaluating selection on observable family characteristics that predict children's outcomes and evaluating selection on unobservables by comparing siblings' outcomes within families.

Selection on Observables. We test for selection on observables by predicting children's household income ranks $\hat{y}_i$ using the same pre-move parental characteristics (education, occupation, wealth, and marital status) that we used to assess the importance of family-level factors in explaining divergent trends in Figure V. Figure Xd replicates Figure Xc using this predicted outcome instead of actual outcomes. We find essentially no relationship between changes in parental employment rates and predicted outcomes across cohorts and move

Fortunately, the orange series in Figure Xc suggests that labor demand shocks and selection correlated with parental employment drive very little of the change in children's outcomes across cohorts, explaining why we obtain similar estimates using this approach relative to our baseline approach.

ages. Column 5 of Table III reports the corresponding estimate of β_μ^p, replicating our baseline specification in Column 1 using the predicted outcome instead of the actual outcome. We obtain a placebo estimate of $\hat{\beta}_\mu^p = 0.020$, an order of magnitude smaller than the corresponding actual estimate of $\hat{\beta}_\mu = 0.339$ and statistically indistinguishable from 0. These findings show that differential changes in the parental characteristics that we observe in our data do not explain our baseline results.

Selection on Unobservables: Sibling Comparisons. Of course, there are still many unobservable factors that could vary with children's age at move to a higher-employment community and confound our estimates. We evaluate the importance of selection on unobservables by comparing the outcomes of siblings who move to areas with improving or declining parental employment rates.

For a given family f, let f_1 index the eldest sibling, f_2 index the youngest sibling, and $s(f_i)$ denote sibling i's birth cohort. Consider a family that moves from origin county o to destination county d when the eldest sibling is age $m(f_1)$ and youngest sibling is age $m(f_2)$, with $A \geq m(f_1) > m(f_2)$. Under the structural model in Equation (3), sibling f_i's outcome is

$$y_{f_i} = \sum_{a=1}^{A} \left[\mu_{c(f_i,a),s(f_i)}\right] + \eta_{d,s(f_i)} + \theta_f + \theta_{f_i} \tag{12}$$

where we decompose the idiosyncratic error $\theta_i = \theta_f + \theta_{f_i}$ into two components, one that varies across families (θ_f) and another that varies across siblings within families (θ_{f_i}). The difference in outcomes between the youngest and eldest siblings is:

$$\begin{aligned}\Delta y_f &= y_{f_2} - y_{f_1} \\ &= \left[m(f_2)\mu_{o,s(f_2)} + (A - m(f_2))\mu_{d,s(f_2)} + \eta_{d,s(f_2)} + \theta_{f_2}\right] \\ &- \left[m(f_1)\mu_{o,s(f_1)} + (A - m(f_1))\mu_{d,s(f_1)} + \eta_{d,s(f_1)} + \theta_{f_1}\right]\end{aligned} \tag{13}$$

We estimate the covariance between the difference between siblings' outcomes and changes in parental employment rates in the destination to which they move:

$$\beta_f = \frac{Cov(\Delta y_f, \Delta \bar{e}_{dpr})}{Var(\Delta \bar{e}_{dpr})}$$

To identify β_μ from the sibling comparison estimator β_f, we assume that selection occurs entirely at the family level, analogous to the identification assumption made in prior work that uses sibling designs to identify neighborhood effects (e.g., Chetty and Hendren 2018*a*).

Assumption 2: Selection Occurs at the Family Level. Differences in unobserved inputs between siblings $\Delta\theta_f = \theta_{f_2} - \theta_{f_1}$ are orthogonal to changes in parental employment rates:

$$Cov(\Delta\theta_f, \Delta \bar{e}_{dpr}) = 0 \tag{14}$$

Assumption 2 permits arbitrary selection across families — placing no restrictions on how θ_f covaries with μ_{cpr} — but requires that idiosyncratic variation across siblings within families θ_{f_i} is uncorrelated with the change in parental employment rates in the destination. Intuitively, Assumption 2 weakens Assumption 1 by permitting differences in the types of families who move to high-employment areas when their children are younger, but requires constant selection by age *within* families.[43]

Figure XIa shows how we identify β_f by presenting a binned scatterplot of the difference in child income ranks between siblings (Δy_f) against the change in parental employment rates in the destination county ($\Delta \bar{e}_{dpr}$) for siblings with an age gap of 4 or more years ($s(f_2) - s(f_1) \geq 4$). This figure uses the subsample of families who move once between counties and have at least two or more children who are below the age of 18 when they move. As in Figure Xa, we isolate variation arising from changes in parental employment rates in the destination county by controlling for the baseline level of group-specific parental employment rates in the destination county $\bar{e}_{dpr,s=1978}$ and the group-specific mean child household income rank in adulthood in the origin county $\bar{y}_{opr}$ interacted with move age fixed effects $\kappa_{m(f_2)}$.[44] We also include parental income percentile-by-race-by-cohort-by-move age-by-sibling age gap fixed effects to eliminate any variation arising purely from differential trends across groups at the national level.

Figure XIa shows that there is a strong and statistically significant positive relationship between the difference in siblings' outcomes and changes in parental employment rates across cohorts in the county to which they move. The younger sibling has better outcomes on average than the older sibling when the family moves to a community that is improving across cohorts. Figure XIb replicates this analysis for siblings with a less than 4 year age gap ($s(f_2) - s(f_1) < 4$). For this group, we see a significantly smaller slope, consistent with the fact that siblings closer in age have more similar exposure to the improving community.

Under Assumption 2, the relationship plotted in Figure XI must be driven by differences in the causal effects of communities, rejecting the hypothesis that the association between changes in parental employment rates and children's outcomes is driven entirely by selection effects with $p < 0.001$. We now go beyond testing this null and use this variation to obtain an estimate of our target parameter β_μ.

Estimating β_μ using Sibling Comparisons. If our goal were to identify static (time-invariant) causal effects of neighborhoods in a setting where labor demand does not fluctuate differentially by neighborhood across cohorts, as in Chetty and Hendren (2018*a*) and Chetty et al. (2020*a*), Assumption 2 would suffice to map β_f to β_μ. Using sibling comparisons to estimate *changes* in childhood exposure effects across cohorts requires addressing two new challenges.

First, as Equation (13) illustrates, siblings are exposed to different labor market shocks η_{dprs} because they are born in different years. The relationships plotted in Figure XI could therefore arise from differences in labor demand across cohorts rather than childhood exposure effects. We can bound the importance of fluctuations in labor demand across cohorts based on the finding in Figure Xc and Table III that the outcomes of children who move at the end of childhood to communities with increasing parental employment rates do

[43]This assumption would be violated if family-level inputs vary across children in proportion to their age at move. For instance, if parents' own incomes and employment improve when they move to an area with increasing parental employment rates and these greater family resources have larger impacts on the younger sibling, then Assumption 2 would fail and our sibling comparisons would yield upward-biased estimates of β_μ. In practice, we find that controlling for changes in own-family income and employment has little impact on our results (Column 3 of Table III), supporting the view that our findings are not driven by such time-varying confounds.

[44]We use parametric controls for origin quality instead of origin fixed effects to maximize precision in these binned scatterplots, but show below that more flexible origin controls yield similar results in parametric regression specifications.

not vary significantly across cohorts (i.e., $\gamma_1 \approx 0$ in Column 1 of Table III). As long as selection is weakly positive ($Cov(\Delta\bar{\theta}_{dprm}, \Delta\bar{e}_{dpr}) \geq 0 \; \forall m$) and changes in parental employment rates are positively correlated with labor demand for children in adulthood, this result implies that labor demand shocks do not covary with changes in parental employment rates: $Cov(\Delta\eta_{dpr}, \Delta\bar{e}_{dpr}) = 0$ (see Appendix D for details). Intuitively, the fact that children's outcomes do not vary across cohorts when they move at the end of childhood to an area with improving employment implies that labor demand for children in adulthood does not vary across cohorts unless there is substantial negative selection that offsets positive labor demand shocks.

The second complication in mapping β_f to β_μ is that differences in siblings' outcomes reflect a combination of differences in the levels of place effects in communities with larger increases in parental employment rates and changes in parental employment rates across cohorts within a given community.[45] To make progress, we must pin down the difference in levels of place effects. We do so by using the finding in Figure Xc and Table III that children's outcomes in the 1978 cohort do not vary with the age at which they move to a destination with increasing parental employment rates across cohorts (i.e., $\gamma_2 \approx 0$ in Column 1 of Table III) conditional on baseline parental employment rates $\bar{e}_{dpr,s=1978}$.[46] Under the assumption that children who move to better areas at younger ages are weakly positively selected — i.e., $Cov(\bar{\theta}_{dpr,m,s=1978} - \bar{\theta}_{dpr,m+1,s=1978}, \Delta\bar{e}_{dpr}) \geq 0 \; \forall m$ — this finding implies that the baseline level of destination place effects is uncorrelated with the change in destination parental employment rates, i.e., $Cov(\mu_{dpr,s=1978}, \Delta\bar{e}_{dpr}) = 0$ (see Appendix D).[47] Intuitively, the fact that children's outcomes in the 1978 cohort are unrelated to the age at which they move to a community with increasing parental employment implies that the quality of the destination was no better for the 1978 cohort in areas with improving employment unless young movers to better areas are negatively selected.

Under the weak positive selection assumptions described above, we show in Appendix D that the siblings estimator β_f can be expressed as β_μ rescaled by a constant:

$$\beta_f = \frac{\overline{g(f)}}{14} \cdot \frac{(A - \overline{m(f_2)}) + (\overline{s(f_1)} - 1978)}{A} \cdot \beta_\mu \tag{15}$$

where $\overline{g(f)} = \overline{s(f_2)} - \overline{s(f_1)}$ denotes the mean age gap between the siblings in the estimation sample. The effect of a 1 percentage point increase in parental employment rates from the 1978-1992 cohorts on the gap in siblings' outcomes is proportional to the effect of being exposed from birth to a community with 1 percentage point higher parent employment rates (β_μ) times the gap in ages between the two siblings ($\overline{g(f)}$). The other factors that enter the expression — the degree of childhood exposure to the destination ($A - \overline{m(f_2)}$) and the average cohort relative to the 1978 starting point ($\overline{s(f_1)} - 1978$) — capture the extent to which the destination had already improved across cohorts when the family moves.

[45] In Equation (13), these two terms appear as level differences between the origin and destination $(\mu_{d,s(f_i)} - \mu_{o,s(f_i)})$ and differences across cohorts in place effects within a place $(\mu_{d,s(f_2)} - \mu_{d,s(f_1)})$. In our empirical implementation, we compare families who start in the same origin and identify purely from differences across destinations, so the origin terms drop out and what matters is how place effects vary across destinations with different trends in parental employment rates.

[46] This result for movers is consistent with the fact that the average level of outcomes of children in the 1978 cohort $\bar{y}_{cpr,s=1978}$ is uncorrelated with changes in parental employment rates from the 1978-1992 cohorts $\Delta\bar{e}_{cpr}$ across communities, controlling for $\bar{e}_{cpr,s=1978}$ (Appendix Figure A.24).

[47] Point identification of β_μ also requires that changes in parental employment rates are positively correlated with baseline place effects; however, if this additional assumption does not hold, β_f provides a lower bound for β_μ.

Table IV reports estimates of β_f and β_μ using regression specifications of the form:

$$\Delta y_f = \beta_f \Delta \bar{e}_{dpr} + \delta_{p,r,s(f_2),m(f_2),g(f)} + \gamma \bar{e}_{dpr,s=1978} + \bar{y}_{opr} \times \kappa_{m(f_2)} + \varepsilon_f \tag{16}$$

where $\delta_{p,r,s(f_2),m(f_2),g(f)}$ are parental income percentile-by-race-by-cohort-by-move age-by-sibling age gap fixed effects. The controls in this regression specification are the same as those used in Figure XI.

Columns 1 and 2 of Table IV report estimates of β_f restricting the sample to families with a sibling age gap of 4 or more years (Column 1) and less than 4 years (Column 2), as in Figure XI. We obtain reduced-form slope estimates of $\hat{\beta}_f = 0.104$ and $\hat{\beta}_f = 0.044$ in these samples, respectively. When we rescale these coefficients using Equation (15), we obtain very similar estimates of β_μ across the two samples: $\hat{\beta}_\mu = 0.299$ for the large age gap sample and $\hat{\beta}_\mu = 0.319$ for the small age gap sample. The similarity of these estimates serves as an overidentification test of our model, showing that the magnitude of the difference in slopes plotted in Figure XIa and Figure XIb matches what one would predict based on differences in childhood exposure effects. The difference in reduced-form slopes between the two groups is driven by the fact that siblings' outcomes respond to changes in parental employment rates in direct proportion to the sibling age gap, as shown by Equation (15).

Column 3 of Table IV replicates the preceding specifications pooling all families. We obtain an estimate of $\hat{\beta}_f = 0.062$ in the full sample and a rescaled coefficient $\hat{\beta}_\mu = 0.301$. This estimate is very similar to and statistically indistinguishable from our baseline estimate of 0.339 in Column 1 of Table III.

Finally, in Column 4 of Table IV, we replace the semi-parametric control for origin quality $\bar{y}_{opr} \times \kappa_{m(f_2)}$ with origin county-by-parental income percentile-by-race-by-move age fixed effects $\kappa_{oprm(f_2)}$. The estimates are very similar to those in Column 3 (albeit less precise), showing that our results are robust to controlling for origin quality more flexibly.

In summary, sibling comparisons yield estimates of β_μ that are very similar to those obtained from our baseline analysis. These findings indicate that the degree of selection on unobservables is modest, supporting the "constant selection by age" assumption underlying our baseline estimates.

VI Social Interactions versus Economic Resources

Having established that the relationship between changes in children's outcomes in adulthood and community-level parental employment rates is largely driven by the causal effect of changes in childhood environments (μ_{cpr}), we now study how childhood environments changed in order to obtain further insight into the mechanisms underlying changes in opportunity.

One class of mechanisms through which having more employed adults in one's community could influence children's later outcomes is via *social interaction*. Interactions with adults who are in better economic positions could influence children's outcomes through many channels: e.g., direct job referrals, provision of information about career pathways, or more broadly, changes in aspirations through role-modeling or social mimicking mechanisms (e.g., Loury, 1977; Bourdieu, 1986; Borjas, 1992; Akerlof and Kranton, 2000; Chetty et al., 2022; Newman and Skocpol, 2023). An alternative class of mechanisms is via *economic resources*: a community with more employed, higher-income adults may have more resources to support programs that improve children's outcomes or more generally may exhibit positive changes in other factors that influence children, such as the quality of teachers or educational investment (e.g., Card and Krueger, 1992; Hoynes, Page and Stevens, 2011; Jackson and Mackevicius, 2024).

In this section, we distinguish between the social interaction and economic resource mechanisms using variation in the degree to which different types of children interact with each other, specifically exploiting variation in friendship rates across cohorts and subgroups. The logic underlying our analysis is that the social interaction mechanism predicts sharp heterogeneity in the impacts of parental employment rates on children's outcomes by degree of interaction, whereas the economic resource mechanism would not (under the assumption that resources are shared across social communities within an area).

VI.A Heterogeneity Across Cohorts

Children are much more likely to interact with peers in their own birth cohort than in adjacent cohorts. The orange series in Figure XII establishes this result using data on friendships from Facebook (see the notes to Figure XII and Chetty et al. (2022) for details). We examine friendship patterns for 1.6 million individuals in the 1993 birth cohort, the earliest cohort for which friendships made in school can be measured with precision given the timeline of Facebook adoption. The orange series plots the share of childhood Facebook friends who are in the child's own birth cohort, those born one year later, one year before, etc. The share of friendships decays rapidly with distance from one's own birth cohort; intuitively, children are more likely to meet and befriend children in their own grade in school.

Exploiting these sharp differences in rates of interaction across birth cohorts, we test between the social interaction and economic resource mechanisms by asking whether children's outcomes are more heavily influenced by the parental employment rates of peers in their own cohort versus surrounding cohorts. We do so by estimating an OLS regression of children's household income ranks (measured at child age 27) on the parental employment rate in the same county-by-race-by-parental income percentile group for the nine closest cohorts, controlling for county-by-race-by-parental income percentile and race-by-parental income percentile-by-cohort fixed effects:

$$\bar{y}_{cprs} = \sum_{t \in [-4,4]} \beta_t \bar{e}_{cpr,s+t} + \delta_{cpr} + \omega_{prs} + \varepsilon_{cprs} \tag{17}$$

The green series in Figure XII plots the coefficients on parental employment rate by cohort (β_t). Children's outcomes are strongly related to parental employment rates of other children in their own birth cohort. Furthermore, the decay of the coefficients on parental employment rates across adjacent cohorts closely mirrors the pattern of decay of social interaction across grades.[48]

Economic resources are unlikely to vary sharply across adjacent cohorts: having more employed parents in the grade before or after one's grade would presumably contribute similarly to greater resources in

[48]In Figure XII, we measure parental employment rates at age 27 as in our baseline analysis (and hence in different calendar years) for each cohort. We find similar results when we measure parental employment rates in a fixed calendar year for all cohorts (Appendix Figure A.25), consistent with the fact that most of the variation in parental employment rates is across cohorts rather than calendar years, as shown in Appendix Figure A.21.

the community. The sharp decay in the impacts of parental employment rates across cohorts thus points in favor of a social interaction mechanism.[49]

VI.B Heterogeneity Across Subgroups

Children are also much more likely to interact with peers of the same race and class.[50] We exploit variation in rates of interaction on these dimensions to test the social interaction mechanism.

We begin by analyzing how the outcomes of white children born to low-income (25th percentile) families relate to the employment rates of white versus Black parents in their own county. We regress the change in mean children's income ranks (measured at child age 27) from the 1978-1992 cohorts estimated as in Section II on changes in employment rates for low-income white and Black parents over the same cohorts:

$$\Delta\bar{y}_{cpr} = \beta_w \Delta\bar{e}_{c,p=25,r=\text{White}} + \beta_b \Delta\bar{e}_{c,p=25,r=\text{Black}} + \varepsilon_{cpr} \tag{18}$$

We weight this regression by the number of white children born to low-income families in county c and limit the sample to counties with at least 2,000 white and 2,000 Black children born to families with below-median parental income pooling all cohorts in our sample.

The first two bars in Figure XIIIa plot estimates of β_w and β_b from Equation (18). Changes in the parental employment rates of white children in a given county are positively associated with changes in white children's outcomes, with a coefficient of $\hat{\beta}_w = 0.29$, similar to that in Figure VIIa. In contrast, changes in the employment rates of low-income Black parents in the same county are unrelated to changes in white children's outcomes (conditional on the change for white parents). Furthermore, the outcomes of white children growing up in low-income families are much more strongly related to employment rates of low-income white parents than high-income white parents (Appendix Table A.21, Column 3), although the differences are less stark than those by race, consistent with prior evidence of greater homophily by race than by class.[51]

The right two bars in Figure XIIIa repeat this analysis for Black children, changing the outcome variable in Equation (18) to changes in mean household income ranks for Black children born to low-income families and weighting the regression by the number of Black children in low-income families. Here, we find the opposite pattern: Black children's outcomes are much more strongly associated with changes in Black parents' employment rates in the counties in which they grow up than with white parents' employment rates. The stronger influence of own-group parental employment rates on children's outcomes is consistent

[49]These cohort-specific patterns also provide further evidence that the relationship between children's outcomes and parents' employment rates is not driven by correlated labor demand shocks, since labor demand fluctuations are unlikely to covary at a high frequency between children and parents who have children in the same birth cohort.

[50]For example, using data from the National Longitudinal Study of Adolescent Health, Joyner and Kao (2000) show that only 10% of white children's high school friends are non-white, whereas 30% of their school peers are non-white. Homophily by class is weaker than by race but also significant: data on friendships from Facebook show that children whose parents have below-median socioeconomic status are 17% less likely to befriend children from above-median-SES families than they would if friendships were made uniformly by class (Chetty et al., 2022).

[51]The outcomes of white children growing up in high-income families are equally associated with employment rates of low- and high-income parents (Appendix Table A.21, Column 4). This may again be a consequence of weaker homophily by class than race or other correlated factors that influence outcomes for children in high-income families.

with the high degree of homophily by race in social interactions.

The differences in β_w and β_b by race far exceed the differences one would predict simply based on the average racial mix of counties in which white and Black people live. Amongst children born to low-income families in our sample, the average white child grows up in a county in which 33% of residents are low-income white individuals and 8.4% of residents are low-income Black individuals. Under the null hypothesis that mean parental employment rates matter and race-specific parental employment rates do not, we would expect a ratio of $\hat{\beta}_w/\hat{\beta}_b = 3.93$ for white children; in practice, we estimate a ratio of $\hat{\beta}_w/\hat{\beta}_b =$14.5. Similarly, for low-income Black children, we estimate $\hat{\beta}_w/\hat{\beta}_b =$3.86, whereas the low-income Black/white population ratio for the average Black child is 1.12.[52]

Although white parents' employment rates have a much weaker relationship with Black children's outcomes than Black parents' employment rates, they still have some predictive power. To probe the source of this relationship further and isolate the role of social interaction more precisely, we examine heterogeneity in this relationship across areas.

We first examine whether the influence of white parents' employment rates varies with the share of white individuals to whom Black children are exposed. Prior work shows that Black people are more likely to interact with white peers in communities where the white share is large (and the Black share is small), since there are fewer people in one's own group with whom to interact (Blau, 1977; Currarini, Jackson and Pin, 2009; Cheng and Xie, 2013). To test whether this difference in rates of interaction affects outcomes, we measure Black children's exposure to white children at the county level as the mean Census tract-level white share for Black children in each county. We then estimate Equation (18) separately for counties with below-median versus above-median white exposure using changes in the mean household income ranks of Black children born to low-income families as the outcome variable.

The left panel of Figure XIIIb plots estimates of β_w and β_b in counties with below-median versus above-median white exposure from these regressions. Consistent with prior evidence of heterogeneous social interaction based on white shares, changes in low-income white parents' employment rates are highly predictive of changes in low-income Black children's outcomes in counties where Black children are more exposed to white peers. In contrast, white parents' employment rates have little impact on Black children's outcomes in counties where Black children have below-median exposure to white children.

Although the preceding findings are consistent with social interaction mechanisms, they could also be generated by changes in resources if resources are not shared across racial groups within counties. For example, the resources available to Black children may be more strongly related to Black parents' employment rates than white parents' employment rates at the county level simply because Black children tend to attend schools with more Black peers on average. Such a mechanism could also potentially generate the heterogeneity by white shares at the Census tract level documented in Figure XIIIb.

To isolate the role of social interaction directly, we must examine variation not just in racial shares (exposure) but cross-race interaction conditional on exposure. We proxy for cross-race interaction at the county level using data on the share of Black children who have white spouses at age 30, constructed by Gold-

[52]This test relies upon the assumption that changes in parent employment rates are independent of demographic shares. We find very similar results when estimating a model that permits interactions between demographic shares and parental employment rates and testing the hypothesis that β_b/β_w equals the ratio of the demographic shares at the sample means.

man, Gracie and Porter (2024). We then augment Equation (18) by interacting changes in white and Black parents' employment rates with an indicator for having above-median rates of white-Black interracial marriage (δ_c). We continue to control for the change in both groups' parental employment rates interacted with an indicator for above-median white exposure (κ_c) to isolate heterogeneity by rates of interracial marriage holding fixed exposure, leading to the following regression specification:

$$
\begin{aligned}
\Delta\bar{y}_{cpr} = {} & \beta_w^0 \Delta\bar{e}_{c,p=25,r=\text{White}} \times \mathbb{1}[\delta_c = 0] + \beta_w^1 \Delta\bar{e}_{c,p=25,r=\text{White}} \times \mathbb{1}[\delta_c = 1] \\
& + \beta_b^0 \Delta\bar{e}_{c,p=25,r=\text{Black}} \times \mathbb{1}[\delta_c = 0] + \beta_b^1 \Delta\bar{e}_{c,p=25,r=\text{Black}} \times \mathbb{1}[\delta_c = 1] \\
& + \gamma_w \Delta\bar{e}_{c,p=25,r=\text{White}} \times (\kappa_c - 0.5) + \gamma_b \Delta\bar{e}_{c,p=25,r=\text{Black}} \times (\kappa_c - 0.5) \\
& + \delta_c + \kappa_c + \varepsilon_{cpr}
\end{aligned} \tag{19}
$$

The key coefficients of interest in this specification are $(\beta_w^0, \beta_w^1, \beta_b^0, \beta_b^1)$, which represent the effects of changes in white and Black parental employment rates in counties with below- versus above-median rates of interracial marriage. The right panel of Figure XIIIb plots estimates of these four coefficients with changes in the mean income ranks of Black children born to low-income families as the outcome variable. Holding fixed exposure to white people at the mean, changes in Black children's outcomes are strongly predicted by changes in white parents' employment rates in counties with high rates of interracial marriage ($\hat{\beta}_w^1 =$ 0.23), but are unrelated to white parents' employment rates in counties with low rates of interracial marriage ($\hat{\beta}_w^0 =$ -0.01). Although not conclusive because rates of interracial marriage are endogenous and could be correlated with other factors that shape the relationship between parental employment rates and children's outcomes, these findings support the view that the heterogeneous relationships between parental employment and children's outcomes by race documented above are mediated by social interaction rather than unequal allocation of resources.

In sum, changes in children's outcomes are predicted by the parental employment rates of peers in their own cohort with whom they interact most. These results suggest that the divergent trends in economic mobility by race and class in recent decades were driven at least in part by differential changes in the social environments in which children from different groups were raised.

VII Conclusion

This paper has shown that economic outcomes deteriorated sharply for white children from low-income families in recent birth cohorts in the United States, while improving for white children from high-income families and Black children from across the parental income distribution. These divergent trends in economic mobility were almost entirely driven by differential changes in the social environments in which children grew up by race and class. In particular, outcomes improve across cohorts for children who grow up in communities with increasing parental employment rates, with larger effects for children who moved to such communities at younger ages. Children's outcomes are more strongly related to the parental employment rates of peers they are more likely to interact with, suggesting that social interactions mediate changes in economic mobility.

The most important takeaway from our analysis is that changing opportunity is feasible in short time frames. Community-level changes in one generation propagate to the next generation and can thereby generate rapid changes in economic mobility. Our analysis has three specific implications for policies to increase economic mobility going forward.

First, existing workforce policies typically focus on supporting the current generation of working adults in areas or sectors with declining employment (e.g., job retraining or trade adjustment assistance programs). Our results suggest that it may be equally important to invest in supporting the next generation of *children* as well in declining communities. For example, when communities are hit by negative economic shocks such as plant closures, negative effects on the next generation could potentially be mitigated through targeted job training and mentorship programs for youth and young adults or investments in schools.

Second, most existing place-based efforts to improve economic mobility focus on neighborhoods as a whole. Our results show that *social communities*—defined by whom children interact with while they grow up—are a key unit at which change occurs. Importantly, social communities are shaped not just by where people live but by race and class within neighborhoods. One approach to increasing opportunity is therefore to increase connections between communities. For example, policies might focus on reducing racial and income segregation—e.g., by changing zoning restrictions, school district boundaries, or increasing the availability of affordable housing in high-opportunity areas—and fostering cross-race and cross-class interactions—e.g., forming groups designed to cut across existing lines of interaction. A complementary approach is to target childhood development programs specifically to social communities with low levels of economic mobility.

Finally, most existing policies to improve economic mobility provide financial capital (e.g., the Earned Income Tax Credit, Pell grants) or human capital (e.g., K-12 education). Our findings on social interactions as a key mediator of changes in opportunity suggest that investing in *social capital* may be equally important. Consistent with this result, recent randomized trials in a variety of domains—from housing vouchers to job training to higher education—show that interventions that combine the provision of financial or human capital with social support and connections (e.g., assistance in housing search, connections to employers, or support from college counselors) have the greatest impacts (Weiss et al., 2019; Katz et al., 2022; Bergman et al., 2024). Targeting such programs to communities with limited opportunity has the potential to improve economic opportunity and narrow racial and socioeconomic disparities significantly.

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A Construction of County-by-Race-by-Class-by-Cohort Estimates

This appendix describes how we construct county-by-race-by-class-by-cohort estimates of both children's outcomes in adulthood and parental employment rates in the child's adulthood, building on the methods developed by Chetty et al. (2020*a*).

Children's Outcomes. Our first objective is to estimate children's expected outcomes in adulthood $\bar{y}_{cprs}$, given their county of residence from birth c, parental income percentile p, racial and ethnic group r, and birth cohort s:

$$\bar{y}_{cprs} = E[y_i | c(i) = c, p(i) = p, r(i) = r, s(i) = s]. \tag{20}$$

We focus on characterizing how children's outcomes are affected by the neighborhood in which they grow up, which may differ from the neighborhoods in which they live as adults.

There are two empirical challenges when estimating $\bar{y}_{cprs}$ in practice. First, there are insufficient observations to estimate $\bar{y}_{cprs}$ non-parametrically for each county-by-race-by-parental-income-by-cohort cell. Second, most children do not spend their entire childhood in a single county, so we need to account for movement across counties when estimating mean outcomes.

To address these challenges, we first estimate the conditional expectation of children's outcomes in adulthood given their parental income percentile, using a univariate regression separately within each county-by-race-by-cohort cell. The relationship between children's outcomes in adulthood and parental income is based on the non-parametric relationship between children's outcomes in adulthood and parental income at the national level, which we capture using a lowess regression (with bandwidth 0.3) of $\bar{y}_{prs}$ on p separately within each race-by-cohort cell (e.g., Figure Ia). The predicted values from this lowess regression, denoted $f_{rs}(p_i)$, flexibly capture any non-linearity in the relationship between children's outcomes in adulthood and parental income at the national level.

After estimating $f_{rs}(p_i)$, we then estimate $\bar{y}_{cprs}$ using predicted values from univariate regressions of children's outcomes in adulthood on $f_{rs}(p_i)$ within each county-by-race-by-cohort cell:

$$y_i = \alpha_{crs} + \beta_{crs} \times f_{rs}(p_i) + \varepsilon_i. \tag{21}$$

To account for children moving across counties during childhood, we weight our estimates by the number of years that child i was claimed as a dependent in county c before age 18. We obtain similar estimates when we restrict the sample to children who spent their entire childhood in a single county. For example, we find that the correlation between our baseline estimates of mean children's household income ranks in adulthood and alternative estimates when using children who reside in the same county from birth is 0.93 for white children from low-income families, 0.93 for Black children from low-income families, and 0.96 for white children from high-income families.

We measure the county-by-race-by-parental-income-level *changes* in economic mobility using a linear regression of children's expected outcomes on birth cohort within each county-by-race-by-parental-income cell:

$$\bar{y}_{cprs} = \mu_{cpr} + \delta_{cpr} \times \frac{s}{1992 - 1978} + \nu_{cprs}. \tag{22}$$

We use δ_{cpr} as our baseline estimate of changes in economic mobility between the 1978 and 1992 birth cohorts for each county, race, and parental income percentile group.

Parental Employment Rates. Our second objective is to estimate parental employment rates in the child's adulthood at the county-by-race-by-parental-income-by-cohort level, which we calculate using an identical procedure. Our approach generates the same $\bar{y}_{cprs}$ for groups with the same $f_{rs}(p_i)$, thereby requiring that groups with the same $f_{rs}(p_i)$ at the national level have the same expected parental employment rate at the county level. One potential complication is that, unlike children's outcomes in adulthood, parental employment rates in the child's adulthood have a non-monotonic relationship with parental income during childhood. For example, Appendix Figure A.26 shows that for white children in the 1978 birth cohort, children at the 25th and 98th parental income percentiles have similar parental employment rates in adulthood at the national level. Our approach generates the same predicted parental employment rate at the 25th and 98th percentiles within each county for each race and birth cohort. We next discuss whether this is a reasonable approximation by testing whether the non-monotonic relationship at the national level holds at the county level.

We begin by comparing our approach to a more flexible method that directly estimates a lowess regression of parental employment rates in the child's adulthood on parental income percentiles in each county, race, and cohort cell. While this method requires us to estimate more parameters, it does not require the non-monotonic relationship at the national level to hold at the county level. In Appendix Table A.22, we compare the out-of-sample root mean square error (RMSE) of the two approaches. For each race group, we restrict our analysis to the 100 most populous counties to ensure sufficient observations when using the more flexible method. Our baseline approach performs at least as well, and typically better, than the more flexible method.

Next, we test whether the relationship between changes in children's outcomes in adulthood and changes in parental employment rates is sensitive to relaxing the assumption that the relationship at the national level is preserved, up to an affine transformation, at the county level. To do so, we generate our $\bar{y}_{cprs}$ predictions of parental employment rates by estimating $f_{rs}(p_i)$ and Equation (21) separately for children with above-median versus below-median parental income. In the context of the example discussed above, this approach allows for different predicted parental employment rates at the county level for white children at the 25th versus the 98th percentile, even if they have the same parental employment rate at the national level. In Appendix Figure A.27, we show that the relationship between changes in children's outcomes and changes in parental employment rates at the county level using this more flexible method is nearly identical to our baseline results in Figure VII.

B Comparison of Employment Trends in Tax Data and Publicly-Available ACS Data

This appendix compares the race- and class-specific parental employment trends constructed in this paper to the employment trends in the publicly-available ACS data.

Appendix Figure A.17 plots the baseline parental employment trends by race and class constructed in this paper using the tax data, as described in Section II. The white class gap in parental employment rates increased by 7.7 percentage points between the 1978 and 1992 birth cohorts while the white-Black race gap in parental employment rates for low-income families decreased by 6.4 percentage points over the same period.

We find qualitatively similar trends when using the publicly-available ACS data in Appendix Figure A.29, although the changes in the white class and white-Black race gaps in employment rates are attenuated relative to the changes in the tax data. We show in this appendix that this attenuation can be attributed to the importance of conditioning on class as measured by income during childhood, capturing parental employment for the relevant cohorts of children, and accounting for differences in mortality rates.

Defining Class by Parental Income: We first show the importance of conditioning on class as measured by income during childhood, as in our baseline results, rather than by parental education as is standard in most publicly-available datasets such as the ACS. To understand the importance of using parental income, we recreate our baseline parental employment trends splitting by both parental income during childhood and parental education. Appendix Figure A.28a plots the trends in parental employment rates for families where no parent has a four-year college degree and Appendix Figure A.28b shows results for families where at least one parent has a four-year college degree or more. Since we use the same measure of parental employment as in our baseline results in Appendix Figure A.17, the primary difference in Appendix Figure A.28 is the disaggregation of parental employment trends by parental income and parental education. Within these parental education subgroups, we still observe a growing white class gap and a shrinking white-Black race gap in parental employment rates. Among low-education families, the white class gap in parental employment rates increased by 8.1 percentage points between the 1978 and 1992 birth cohorts, while the white-Black race gap for low-income families decreased by 4.7 percentage points over the same period. Among high-education families, the white class gap increased by 10.8 percentage points, while the white-Black race gap for low-income families decreased by 2.4 percentage points. These results show that defining class by parental income during childhood reveals important trends in parental employment, beyond what we can capture when we define class by only parental education.

Sample of Parents: We next show the importance of measuring parental employment for the relevant cohorts of children, as in our baseline results. In most publicly-available datasets such as the ACS, the link between parents and children is not available after children leave the household. As a result, samples based on the publicly-available ACS data include all adults in a given age range, regardless of whether they have children in the relevant birth cohorts. To understand the importance of using our parent sample versus a generalized population of adults, we construct a new dataset based on all adults in the tax data who can be matched to the ACS.

Appendix Figure A.29a plots the trends in employment rates among adult women ages 48-57 (the approximate age range of parents in our relevant birth cohorts) in the tax data who can be matched to the ACS data. We focus on employment rates among adult women since the majority of Black children from low-income families reside with only their mothers. We define the employment rate as the fraction of adults working in a given year based on the tax data. To be consistent with our baseline results, we set the employment rate equal to zero if an individual is deceased, which we can observe in the tax data. To assess the importance of the sample choice in addition to our measure of class using parental income, we define the white class gap as the gap in employment rates among white adults with at least a four-year college degree versus those with less than a four-year college degree, and the white-Black race gap as the gap in employment rates among white versus Black adults with less than a four-year college degree. We use the ACS person weights throughout to account for the ACS sampling procedure. Appendix Figure A.29a shows that there is still a growing white class gap and a shrinking white-Black race gap in adult female employment rates using a general population of adult women and the education-based measure of class. However, the changes in the gaps are smaller compared to our baseline estimates, illustrating the importance of defining the sample of parents as those with children in the relevant birth cohorts and defining class using parental income during childhood versus parental education. Appendix Figure A.29c repeats the above exercise for men ages 48-57, with similar findings.

Accounting for Mortality: Finally, we show the additional importance of accounting for differences in mortality rates when calculating employment rates. Appendix Figure A.29b plots the trends in employment rates among adult women ages 48-57 constructed entirely using the publicly-available ACS data. We now define the employment rate as the fraction of adults working in a given year based on ACS data, a definition that excludes deceased individuals since the ACS only surveys living individuals. We continue using the education-based measure of class and ACS person weights. Appendix Figure A.29b shows that there is still a growing white class gap and a shrinking white-Black race gap in adult female employment rates in the publicly-available ACS data. However, the changes in the gaps are smaller than our baseline estimates in Appendix Figure A.17 and the estimates in Appendix Figure A.29a, highlighting the importance of conditioning on class as measured by income during childhood, capturing parental employment for the relevant cohorts of children, and accounting for differences in mortality rates. Appendix Figure A.29d repeats the above exercise for men ages 48-57, again with similar findings.

C Baseline Movers Estimator

In this appendix, we show formally how the regression specifications estimated in Table III identify the target estimand β_μ.

C.1 Proof that Equation (10) identifies β_μ

For reference, we reproduce Equation (10) below:

$$\begin{aligned} y_i = {} & b_\mu(\Delta\bar{e}_{dpr} \times \frac{s_i - 1978}{14} \times \frac{A - m_i}{A}) \\ & + \gamma_0 \Delta\bar{e}_{dpr} + \gamma_1(\Delta\bar{e}_{dpr} \times \frac{s_i - 1978}{14}) + \gamma_2(\Delta\bar{e}_{dpr} \times \frac{A - m_i}{A}) \\ & + \delta_{oprsm} + \bar{e}_{dpr,s=1978} \times \kappa_{sm} + \varepsilon_i \end{aligned}$$

Note that exposure to origin place effects $m\mu_{oprs}$ is fully absorbed by the δ_{oprsm} fixed effects under our additive structural model; we therefore omit the $m\mu_{oprs}$ terms in the derivations below.

We establish conditions under which b_μ identifies β_μ in two steps. First, fix any cohorts $s_2 > s_1$ and move ages $m_2 > m_1$. Conditional on the set $B_i = (s_i \in \{s_1, s_2\}, m_i \in \{m_1, m_2\})$, we can write b_μ using a difference-in-differences expression as in Section V.A:

$$
\begin{aligned}
b_\mu | B_i &= \frac{Cov((\bar{y}_{dpr,s_2,m_1} - \bar{y}_{dpr,s_1,m_1}) - (\bar{y}_{dpr,s_2,m_2} - \bar{y}_{dpr,s_1,m_2}), \Delta\bar{e}_{dpr} \times \frac{s_2-s_1}{14} \times \frac{m_2-m_1}{A})}{Var(\Delta\bar{e}_{dpr} \times \frac{s_2-s_1}{14} \times \frac{m_2-m_1}{A})} \\
&= \frac{Cov(\frac{(s_2-s_1)(m_2-m_1)}{14}\Delta\mu_{dpr} + (\bar{\theta}_{dpr,s_2,m_1} - \bar{\theta}_{dpr,s_1,m_1}) - (\bar{\theta}_{dpr,s_2,m_2} - \bar{\theta}_{dpr,s_1,m_2}), \Delta\bar{e}_{dpr})}{\frac{s_2-s_1}{14} \times \frac{m_2-m_1}{A} \times Var(\Delta\bar{e}_{dpr})} \\
&= \frac{Cov(A\Delta\mu_{dpr}, \Delta\bar{e}_{dpr})}{Var(\Delta\bar{e}_{dpr})} + \frac{Cov((\bar{\theta}_{dpr,s_2,m_1} - \bar{\theta}_{dpr,s_1,m_1}) - (\bar{\theta}_{dpr,s_2,m_2} - \bar{\theta}_{dpr,s_1,m_2}), \Delta\bar{e}_{dpr})}{\frac{s_2-s_1}{14} \times \frac{m_2-m_1}{A} \times Var(\Delta\bar{e}_{dpr})} \\
&= \beta_\mu
\end{aligned}
\tag{C.1}
$$

where the first equality holds because the interaction term in the regression coincides with the Difference-in-Differences (DiD) coefficient when there are two cohorts and move ages; the second equality holds by fixing B_i and assuming linear trends in destination place effects ($\mu_{dprs} = \mu_{dpr,s=1978} + \frac{s-1978}{14}\Delta\mu_{dpr}$); the third equality follows from linearity of expectations; and the final equality follows from applying Assumption 1 to any $\{s_1, s_2\}$ pair, which differences out the selection terms $\bar{\theta}_{dprsm}$.

Note that under linear trends and constant selection effects by move age, the additive structural model in Section V.A implies that there is no heterogeneity in treatment effects by B_i, i.e. $b_\mu | B_i = \beta_\mu \ \forall B_i$. In this sense, Equation (10) is overidentified; in principle, any pair of cohorts and move ages would suffice to identify β_μ.

Even under this homogeneity assumption, there is a benefit in terms of power to aggregating across multiple cohorts and move ages. Let $\tilde{X}_{dprsm}$ denote the residual from regressing $\Delta\bar{e}_{dpr} \times \frac{s_i-1978}{14} \times \frac{A-m_i}{A}$ on all other elements on the right hand side of Equation (10). Estimating Equation (10) pooling all cohorts and move ages identifies β_μ if the mean of $\tilde{X}_{dprsm}$ does not vary with B_i:

$$
\begin{aligned}
b_\mu &= \frac{Cov(\tilde{X}_{dprsm}, y_i)}{Var(\tilde{X}_{dprsm})} \\
&= E\big[\frac{(\tilde{X}_{dprsm} - E[\tilde{X}_{dprsm}])y_i}{Var(\tilde{X}_{dprsm})}\big] \\
&= E\big[\frac{(\tilde{X}_{dprsm} - E[\tilde{X}_{dprsm}|B_i])y_i}{Var(\tilde{X}_{dprsm})}\big] \\
&= E\Big[E\big[\frac{(\tilde{X}_{dprsm} - E[\tilde{X}_{dprsm}|B_i])y_i}{Var(\tilde{X}_{dprsm}|B_i)}|B_i\big]\frac{Var(\tilde{X}_{dprsm}|B_i)}{Var(\tilde{X}_{dprsm})}\Big] \\
&= E\big[b_\mu|B_i\frac{Var(\tilde{X}_{dprsm}|B_i)}{Var(\tilde{X}_{dprsm})}\big] \\
&= E[\beta_\mu\frac{Var(\tilde{X}_{dprsm}|B_i)}{Var(\tilde{X}_{dprsm})}] \\
&= \beta_\mu
\end{aligned}
\tag{C.2}
$$

where the first equality holds by the Frisch-Waugh-Lovell Theorem; the second by the definition of covariances and expectations; the third by assuming mean independence of $\tilde{X}_{dprsm}$ and B_i; the fourth by the Law of Iterated Expectations and multiplying by $\frac{Var(\tilde{X}_{dprsm}|B_i)}{Var(\tilde{X}_{dprsm}|B_i)} = 1$; the fifth by the aforementioned 2x2 differencing procedure; the sixth by Equation (C.1); and the final equality again by assuming mean independence of $\tilde{X}_{dprsm}$ and B_i.

In sum, identifying β_μ using the continuous linear regression specification in Equation (10) rather than a discrete 2 by 2 comparison across cohorts and move ages requires linear trends in destination place effects and the same distribution of residual variation across cohort and move-age pairs (a balancing condition).

C.2 Proof that Equation (11) identifies β_μ

For reference, we reproduce Equation (11) below:

$$y_i = b_\mu \left(\frac{m_i \bar{e}_{oprs} + (A - m_i)\bar{e}_{dprs}}{A} \right) + \delta_{odprm} + \kappa_{prs} + \varepsilon_i$$

For expositional simplicity, we abstract from the κ_{prs} fixed effects in the derivations below, which we include to net out variation at the national level by parental income percentile and race across cohorts.

To begin, consider a case with only two cohorts $s_2 > s_1$. With two cohorts, the δ_{odprm} fixed effects estimator is equivalent to the first differences estimator:

$$\begin{aligned} \bar{y}_{odpr,s_2,m} - \bar{y}_{odpr,s_1,m} = b_\mu \Big[\left(\frac{m\bar{e}_{opr,s_2} + (A-m)\bar{e}_{dpr,s_2}}{A} \right) - \left(\frac{m\bar{e}_{opr,s_1} + (A-m)\bar{e}_{dpr,s_1}}{A} \right) \Big] \\ + (\bar{\varepsilon}_{odpr,s_2,m} - \bar{\varepsilon}_{odpr,s_1,m}) \end{aligned} \tag{C.3}$$

Define the exposure-weighted variables $\mu_{cpr} = \frac{m\mu_{opr} + (A-m)\mu_{dpr}}{A}$ and $\bar{e}_{cpr} = \frac{m\bar{e}_{opr} + (A-m)\bar{e}_{dpr}}{A}$. Given the set $B_i = (s_i \in \{s_1, s_2\})$, the fixed effect estimator identifies

$$\begin{aligned} b_\mu | B_i &= \frac{Cov(\frac{s_2 - s_1}{14}\left[m\Delta\mu_{opr} + (A-m)\Delta\mu_{dpr} + \Delta\eta_{dpr} + \Delta\bar{\theta}_{odprm}\right], \frac{s_2-s_1}{14}\frac{m\Delta\bar{e}_{opr} + (A-m)\Delta\bar{e}_{dpr}}{A})}{Var(\frac{s_2-s_1}{14}\frac{m\Delta\bar{e}_{opr} + (A-m)\Delta\bar{e}_{dpr}}{A})} \\ &= \frac{Cov(A\Delta\mu_{cpr} + \Delta\eta_{dpr} + \Delta\bar{\theta}_{odprm}, \Delta\bar{e}_{cpr})}{Var(\Delta\bar{e}_{cpr})} \end{aligned} \tag{C.4}$$

where the first equality holds by assuming linear trends in$(\mu_{opr}, \mu_{dpr}, \bar{e}_{opr}, \bar{e}_{dpr}, \eta_{dpr}, \bar{\theta}_{oprsm}, \bar{\theta}_{dprsm})$.

Assumption 3. Labor demand shocks and selection effects do not covary with changes in parental employment rates across cohorts:

$$Cov(\Delta\eta_{dpr} + \Delta\bar{\theta}_{odprm}, \Delta\bar{e}_{cpr}) = 0 \tag{C.5}$$

Under this assumption, it follows immediately that $b_\mu | B_i$ identifies β_μ. Finally, we aggregate across multiple cohorts. Define $\tilde{X}_i$ as the residual from regressing $\frac{m_i\bar{e}_{oprs} + (A-m_i)\bar{e}_{dprs}}{A}$ on all other elements on the right hand side of Equation (11). Assuming mean independence of $\tilde{X}_i$ and B_i, the equivalence of b_μ and β_μ follows from the same proof as that above for Equation (C.2).

Empirical Evidence Supporting Assumption 3. The estimate of $\gamma_1 \approx 0$ in Column 1 of Table III (and the corresponding non-parametric evidence in Figure Xc) shows that the outcomes of children who move late in childhood to areas with increasing employment rates do not change across cohorts. This result provides empirical support for Assumption 3:

$$\begin{aligned} Cov(\Delta\bar{y}_{dpr,m=\text{late}}, \Delta\bar{e}_{dpr}) &= Cov(m\Delta\bar{\mu}_{opr} + (A-m)\Delta\mu_{dpr} + \Delta\eta_{dpr} + \Delta\bar{\theta}_{dpr,m=\text{late}}, \Delta\bar{e}_{dpr}) \\ &\approx Cov(\Delta\eta_{dpr} + \Delta\bar{\theta}_{dpr,m=\text{late}}, \Delta\bar{e}_{dpr}) \\ &= Cov(\Delta\eta_{dpr} + \Delta\bar{\theta}_{dprm}, \Delta\bar{e}_{dpr}) \\ &\approx 0 \end{aligned} \tag{C.6}$$

where the first equality holds by definition of the structural model in Section V.A; the second equality holds for the orange series of Figure Xc, which eliminates $m\Delta\bar{\mu}_{opr}$ by controlling for origin county-parental income percentile-race-move age fixed effects with separate regressions for each cohort and approximately eliminates $(A-m)\Delta\mu_{dpr}$ by setting $m \approx A$; the third equality holds by Assumption 1; and the final equality follows from the estimate of $\gamma_1 \approx 0$ in Column 1 of Table 3. This result suggests that selection effects and labor demand shocks do not covary with changes in parental employment rates across cohorts, supporting Assumption 3.

D Siblings Estimator in Movers Design

In this appendix, we show how the estimand β_f identified by the siblings specification in Section V.D maps to our target estimand β_μ. To simplify exposition, we hold $(o, m(f_1), m(f_2), s(f_1), s(f_2), p, r)$ fixed in the derivations that follow; we describe how we approximate this conditioning in our empirical implementation at the end of this appendix.

We begin from Equation (13), which is reproduced below.

$$\begin{aligned} \Delta y_f &= y_{f_2} - y_{f_1} \\ &= \left[m(f_2)\mu_{opr,s(f_2)} + (A-m(f_2))\mu_{dpr,s(f_2)} + \eta_{dpr,s(f_2)} + \theta_{f_2}\right] \\ &\quad - \left[m(f_1)\mu_{opr,s(f_1)} + (A-m(f_1))\mu_{dpr,s(f_1)} + \eta_{dpr,s(f_1)} + \theta_{f_1}\right] \end{aligned}$$

Note that under Assumption 2, the selection terms cancel out of the preceding expression and hence:

$$\begin{aligned} Cov(\Delta y_f, \Delta\bar{e}_{dpr}) &= Cov(m(f_2)\mu_{opr,s(f_2)} + (A-m(f_2))\mu_{dpr,s(f_2)} + \eta_{dpr,s(f_2)}, \Delta\bar{e}_{dpr}) \\ &\quad - Cov(m(f_1)\mu_{opr,s(f_1)} + (A-m(f_1))\mu_{dpr,s(f_1)} + \eta_{dpr,s(f_1)}, \Delta\bar{e}_{dpr}) \end{aligned} \tag{D.1}$$

Addressing Within-Family Differences in Labor Market Shocks. To address the differences in labor market shocks $\eta_{dpr,s(f_2)} - \eta_{dpr,s(f_1)}$ that enter the preceding expression, we make the following assumptions:

Assumption 4: Weakly Positive Selection Across Cohorts. The covariance between changes in unobserved inputs θ_i and changes in parental employment rates is weakly positive:

$$Cov(\Delta\bar{\theta}_{dprm}, \Delta\bar{e}_{dpr}) \geq 0 \qquad \forall m. \tag{D.2}$$

Assumption 5: Weakly Positively Correlated Labor Demand Shocks. The covariance between changes in labor demand shocks η_{dpr} and changes in parental employment rates is weakly positive:

$$Cov(\Delta\eta_{dpr}, \Delta\bar{e}_{dpr}) \geq 0 \tag{D.3}$$

Figure Xc implies $Cov(\Delta\eta_{dpr} + \Delta\bar{\theta}_{dprm}, \Delta\bar{e}_{dpr}) \approx 0$ in our data:

$$\begin{aligned} Cov(\Delta\bar{y}_{dpr,m=\text{late}}, \Delta\bar{e}_{dpr}) &= Cov(m\Delta\bar{\mu}_{opr} + (A-m)\Delta\mu_{dpr} + \Delta\eta_{dpr} + \Delta\bar{\theta}_{dpr,m=\text{late}}, \Delta\bar{e}_{dpr}) \\ &\approx Cov(\Delta\eta_{dpr} + \Delta\bar{\theta}_{dpr,m=\text{late}}, \Delta\bar{e}_{dpr}) \\ &\approx 0, \end{aligned} \tag{D.4}$$

where the first equality holds by definition of the structural model in Section V.A; the second equality holds in the orange series of Figure Xc, which eliminates $m\Delta\bar{\mu}_{opr}$ by controlling for origin county-by-parental income percentile-by-race-by-move age fixed effects with separate regressions for each cohort and approximately eliminates $(A-m)\Delta\mu_{dpr}$ by setting $m \approx A$; and the final equality follows from the flatness of the orange series of Figure Xc.

Under Assumptions 4 and 5, it follows that $Cov(\Delta\eta_{dpr}, \Delta\bar{e}_{dpr}) \approx 0$.[53] Assuming a linear trend in labor demand shocks, $Cov(\eta_{dpr,s_2} - \eta_{dpr,s_1}, \Delta\bar{e}_{dpr}) \approx 0$ for any cohort pair s_1, s_2. It then follows from Equation (D.1) that labor demand shocks do not enter the covariance between changes in parental employment rates and differences in siblings' outcomes:

$$\begin{aligned} Cov(\Delta y_f, \Delta\bar{e}_{dpr}) = Cov(&m(f_2)\mu_{opr,s(f_2)} + (A - m(f_2))\mu_{dpr,s(f_2)}, \Delta\bar{e}_{dpr}) \\ &- Cov(m(f_1)\mu_{opr,s(f_1)} + (A - m(f_1))\mu_{dpr,s(f_1)}, \Delta\bar{e}_{dpr}). \end{aligned} \tag{D.5}$$

Addressing Within-Family Differences in Exposure to Place Effects in Levels. To map this expression to our target estimand β_μ, define $\Delta\mu_{opr,f} = \mu_{opr,s(f_2)} - \mu_{opr,s(f_1)}$ as the within-family change in the origin place effect across cohorts and $\Delta\mu_{dpr,f} = \mu_{dpr,s(f_2)} - \mu_{dpr,s(f_1)}$ as the within-family change in the destination place effect across cohorts. We can then express Equation (D.5) as

$$\begin{aligned} Cov(\Delta y_f, \Delta\bar{e}_{dpr}) &= Cov(m(f_2)\Delta\mu_{opr,f}, \Delta\bar{e}_{dpr}) \\ &\quad + Cov((A - m(f_2))\Delta\mu_{dpr,f}, \Delta\bar{e}_{dpr}) \\ &\quad + Cov((m(f_2) - m(f_1))\mu_{opr,s(f_1)}, \Delta\bar{e}_{dpr}) \\ &\quad + Cov((m(f_1) - m(f_2))\mu_{dpr,s(f_1)}, \Delta\bar{e}_{dpr}) \end{aligned} \tag{D.6}$$

Equation (D.6) shows that the difference between sibling outcomes can be decomposed into an exposure-

[53] While Assumptions 4 and 5 are sufficient, we only need to assume that $Cov(\Delta\bar{\theta}_{dprm}, \Delta\bar{e}_{dpr})$ and $Cov(\Delta\eta_{dpr}, \Delta\bar{e}_{dpr})$ have the same sign for all m to establish this result.

weighted average of changes in origin place effects and changes in destination place effects, as well as a linear combination of origin and destination place effects in *levels* with weights that reflect the within-family difference in exposure to each place.

Let $\Delta\mu_{dpr}$ be the change in the destination place effect between 1978 and 1992. Our goal is to isolate $Cov(\Delta\mu_{dpr}, \Delta\bar{e}_{dpr})$ from Equation (D.6). We do so in three steps. First, maintaining the assumption of a linear trend in destination place effects and recalling the notation $g(f) = m(f_1) - m(f_2) > 0$,

$$\begin{aligned} \Delta\mu_{dpr,f} &= \frac{g(f)}{14}\Delta\mu_{dpr} \\ \mu_{dpr,s(f_1)} &= \mu_{dpr,s=1978} + \frac{s(f_1) - 1978}{14}\Delta\mu_{dpr} \end{aligned} \tag{D.7}$$

Substituting Equation (D.7) into Equation (D.6) yields:

$$\begin{aligned} Cov(\Delta y_f, \Delta\bar{e}_{dpr}) &= Cov(m(f_2)\Delta\mu_{opr,f}, \Delta\bar{e}_{dpr}) \\ &- Cov(g(f)\mu_{opr,s(f_1)}, \Delta\bar{e}_{dpr}) \\ &+ Cov(\frac{g(f)(A - m(f_2))}{14}\Delta\mu_{dpr}, \Delta\bar{e}_{dpr}) \\ &+ Cov(g(f)\mu_{dpr,s=1978} + \frac{g(f)(s(f_1) - 1978)}{14}\Delta\mu_{dpr}, \Delta\bar{e}_{dpr}) \end{aligned} \tag{D.8}$$

Second, since $(o, m(f_1), m(f_2), s(f_1), s(f_2), p, r)$ are held fixed, the first two terms of Equation (D.8) are eliminated and the exposure weights in the last two terms are constants, implying that:

$$\begin{aligned} Cov(\Delta y_f, \Delta\bar{e}_{dpr}) = &\frac{g(f)(A - m(f_2) + s(f_1) - 1978)}{14}Cov(\Delta\mu_{dpr}, \Delta\bar{e}_{dpr}) \\ &+ g(f)Cov(\mu_{dpr,s=1978}, \Delta\bar{e}_{dpr}) \end{aligned} \tag{D.9}$$

Finally, we show that the second term drops out of the preceding expression under the following assumptions:

Assumption 6: Weakly Positive Selection Across Move Ages. The covariance between the baseline level of unobserved inputs θ_i and changes in parental employment rates is weakly decreasing with move age:

$$Cov(\bar{\theta}_{dpr,m,s=1978} - \bar{\theta}_{dpr,m+1,s=1978}, \Delta\bar{e}_{dpr}) \geq 0 \qquad \forall m \tag{D.10}$$

Assumption 7: Weakly Positive Covariance Between Baseline Place Effects and Changes in Parental Employment. The covariance between the baseline level of destination place effects $\mu_{dpr,s=1978}$ and changes in parental employment rates is weakly positive:

$$Cov(\mu_{dpr,s=1978}, \Delta\bar{e}_{dpr}) \geq 0 \tag{D.11}$$

Recall that

$$Cov(\bar{y}_{dprm,s=1978}, \Delta\bar{e}_{dpr}) \approx 0 \qquad \forall m \tag{D.12}$$

conditional on baseline parent employment rates $\bar{e}_{dpr,s=1978}$, as shown in Figure Xc. Furthermore, for any move age m, given the origin county-by-parental income percentile-by-race-by-move age-by-cohort fixed effects (o,p,r,m,s) we include in Figure X,

$$\begin{aligned} Cov(\bar{y}_{dprm,s=1978},\Delta\bar{e}_{dpr}) = (A-m)&Cov(\mu_{dpr,s=1978},\Delta\bar{e}_{dpr}) \\ &+Cov(\eta_{dpr,s=1978},\Delta\bar{e}_{dpr})+Cov(\bar{\theta}_{dprm,s=1978},\Delta\bar{e}_{dpr}) \end{aligned} \tag{D.13}$$

It follows that

$$\begin{aligned} &Cov(\bar{y}_{dpr,m,s=1978},\Delta\bar{e}_{dpr})-Cov(\bar{y}_{dpr,m+1,s=1978},\Delta\bar{e}_{dpr}) = \\ &Cov(\mu_{dpr,s=1978},\Delta\bar{e}_{dpr})+Cov(\bar{\theta}_{dpr,m,s=1978}-\bar{\theta}_{dpr,m+1,s=1978},\Delta\bar{e}_{dpr}) \approx 0 \end{aligned} \tag{D.14}$$

Under Assumptions 6 and 7, Equations (D.12) and (D.13) imply:[54]

$$Cov(\mu_{dpr,s=1978},\Delta\bar{e}_{dpr}) = 0. \tag{D.15}$$

and the second term drops out of (D.9). It follows that the relationship between β_f and β_μ is given by

$$\begin{aligned} \beta_f &= \frac{Cov(\Delta y_f,\Delta\bar{e}_{dpr})}{Var(\Delta\bar{e}_{dpr})} \\ &= \frac{g(f)\left[(A-m(f_2))+(s(f_1)-1978)\right]}{14}\frac{Cov(\Delta\mu_{dpr},\Delta\bar{e}_{dpr})}{Var(\Delta\bar{e}_{dpr})} \\ &= \frac{g(f)}{14}\frac{(A-m(f_2))+(s(f_1)-1978)}{A}\beta_\mu \end{aligned} \tag{D.16}$$

β_f Provides a Lower Bound for β_μ Assuming Only Weak Positive Selection. If we make only the weak positive selection assumptions (Assumptions 4 and 6), then the siblings estimator provides a weak lower bound for the exposure effect β_μ. In particular, if we drop Assumption 5, then Equation (D.4) instead implies $Cov(\Delta\eta_{dpr},\Delta\bar{e}_{dpr}) \leq 0$, and β_f is a weakly conservative estimate of β_μ:

$$\begin{aligned} \beta_f &= \frac{g(f)}{14}\frac{(A-m(f_2))+(s(f_1)-1978)}{A}\beta_\mu + \frac{Cov(\Delta\eta_{dpr},\Delta\bar{e}_{dpr})}{Var(\Delta\bar{e}_{dpr})} \\ &\leq \frac{g(f)}{14}\frac{(A-m(f_2))+(s(f_1)-1978)}{A}\beta_\mu \end{aligned} \tag{D.17}$$

Similarly, if we drop Assumption 7, then Equation (D.14) instead implies $Cov(\mu_{dpr,s=1978},\Delta\bar{e}_{dpr}) \leq 0$, and β_f is again a weakly conservative estimate of β_μ:

$$\begin{aligned} \beta_f &= \frac{g(f)}{14}\frac{(A-m(f_2))+(s(f_1)-1978)}{A}\beta_\mu + \frac{g(f)Cov(\mu_{dpr,s=1978},\Delta\bar{e}_{dpr})}{Var(\Delta\bar{e}_{dpr})} \\ &\leq \frac{g(f)}{14}\frac{(A-m(f_2))+(s(f_1)-1978)}{A}\beta_\mu \end{aligned} \tag{D.18}$$

[54] While Assumptions 6 and 7 are sufficient, we only need to assume that $Cov(\bar{\theta}_{dpr,m,s=1978}-\bar{\theta}_{dpr,m+1,s=1978},\Delta\bar{e}_{dpr})$ and $Cov(\mu_{dpr,s=1978},\Delta\bar{e}_{dpr})$ have the same sign for all m to establish this result.

Empirical Implementation. The regression specification we use to estimate β_f (in Column 4 of Table IV is reproduced below[55]:

$$\Delta y_f = \beta_f \Delta \bar{e}_{dpr} + \delta_{p,r,s(f_2),m(f_2),g(f)} + \gamma \bar{e}_{dpr,s=1978} + \kappa_{oprm(f_2)} + \varepsilon_f \tag{D.19}$$

This specification differs from the estimator derived above because it does not condition on $(o, m(f_1), m(f_2), s(f_1), s(f_2), p, r)$. To increase precision, we instead control for parental income percentile-by-race-by-cohort-by-age at move-by-age gap fixed effects $\delta_{p,r,s(f_2),m(f_2),g(f)}$ and origin county-by-parental income percentile-by-race-by-age at move fixed effects, $\kappa_{oprm(f_2)}$. We condition on the age gap $g(f) = s(f_1) - s(f_2)$ coarsely by separately considering families with age gaps of less than 3 years or more than 3 years.

The $\kappa_{oprm(f_2)}$ fixed effects eliminate the first term in Equation (D.8) by holding $(o, m(f_2), p, r)$ fixed. The second term in Equation (D.8) is eliminated if the residual variation in $\mu_{opr,s}$ conditional on the $\delta_{p,r,s(f_2),m(f_2),g(f)}$ and $\kappa_{oprm(f_2)}$ fixed effects is orthogonal to changes in destination employment rates $\Delta \bar{e}_{dpr}$, which we view as a plausible approximation.

We substitute the sample means of $(g(f), m(f_2), s(f_1))$ into Equation (D.16), implicitly making an independence assumption that effectively requires that $(g(f), m(f_2), s(f_1))$ is balanced by destination. This assumption is supported by Columns 1 and 2 of Table IV, which show that the differences in $\hat{\beta}_f$ by $g(f)$ are consistent with our model, such that the rescaled estimates $\hat{\beta}_\mu$ are nearly identical.

[55]Columns 1-3 in Table IV replace the $\kappa_{oprm(f_2)}$ fixed effects with semi-parametric controls $\bar{y}_{opr} \times \kappa_{m(f_2)}$ and can be interpreted similarly under the assumption that the semi-parametric controls capture the variation absorbed by the non-parametric fixed effects.

TABLE I
Summary Statistics by Race

	White Children	Black Children	Hispanic Children	Asian Children	AIAN Children
A. Parental Characteristics	(1)	(2)	(3)	(4)	(5)
Median Household Income, Child Ages 13-17	$91,800	$38,250	$44,600	$72,900	$43,790
Mean Household Income Percentile, Child Ages 13-17	58.0	33.5	37.7	51.3	36.9
Two Parents Present	80.0%	29.1%	54.0%	80.7%	55.7%
Years of Education	14.8	14.1	12.2	14.0	13.8
High School Graduation	92.4%	83.4%	62.3%	79.5%	81.6%
Four-Year College Graduation	26.5%	15.5%	11.6%	37.5%	12.5%
Home Ownership	82.4%	56.4%	63.4%	75.0%	68.2%
B. Child Outcomes in Child Adulthood					
Median Household Income, Age 27	$43,400	$20,920	$32,310	$42,420	$22,090
Median Individual Income, Age 27	$33,180	$19,270	$26,970	$36,690	$16,910
Mean Household Income Percentile, Age 27	55.2	36.8	46.8	54.3	39.1
Bottom Quintile of Household Income, Age 27	15.7%	28.7%	20.3%	18.2%	32.6%
Top Quintile of Household Income, Age 27	25.5%	5.9%	13.9%	25.4%	10.7%
Employment, Age 27	84.4%	79.3%	81.2%	82.7%	74.4%
Marriage, Age 27	34.2%	9.2%	23.5%	19.5%	22.5%
Marriage, Age 32	50.7%	14.5%	34.1%	42.4%	29.0%
Mortality, Ages 24-27	0.35%	0.51%	0.31%	0.18%	0.75%
Incarceration, Age 22	0.71%	4.14%	1.17%	0.20%	2.02%
Years of Education, Age 27	15.2	14.2	14.2	16.0	13.8
High School Graduation, Age 27	94.4%	86.2%	85.8%	96.0%	84.2%
Four-Year College Graduation, Age 27	39.1%	19.8%	21.2%	58.1%	11.3%
C. Parental Outcomes in Child Adulthood					
Employment, Child Age 27	68.4%	67.4%	64.3%	61.9%	61.7%
Marriage, Child Age 27	67.3%	26.3%	49.1%	70.3%	42.2%
Mortality, Child Ages 18-27	3.44%	5.09%	3.06%	2.63%	5.55%
Number of Children (1,000s)	34,910	7,709	8,159	1,920	474

Notes: This table reports summary statistics for our primary analysis sample of children in the 1978-1992 birth cohorts who are claimed as child dependents by parents with positive household income between child ages 13-17. Panel A reports parental characteristics during the child's youth; Panel B reports child outcomes during the child's adulthood; and Panel C reports parental outcomes during the child's adulthood. See Section II for more details and variable definitions. All monetary values are reported in 2023 dollars. All estimates in this and all subsequent tables and figures have been rounded to four significant digits as part of the disclosure avoidance protocol. Counts are rounded in the following manner: numbers between 10,000 and 99,999 are rounded to the nearest 500; between 100,000 and 999,999 to the nearest 1,000; and above 1,000,000 to four significant digits. All statistics cleared under Census DRB release authorizations CBDRB-FY2023-CES005-025 and CBDRB-FY24-0359.

TABLE II

Mean Child Household Income Rank by Birth Cohort, Race, and Class

	Mean HH Income Rank at P=25				Mean HH Income Rank at P=75			
	1978 Cohort	1992 Cohort	Change	Share	1978 Cohort	1992 Cohort	Change	Share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
White Children	48.4	46.1	-2.3	22.8%	59.5	60.2	0.8	38.3%
Black Children	33.5	35.1	1.6	10.3%	43.9	45.3	1.4	3.2%
Hispanic Children	44.3	44.7	0.4	9.9%	53.1	53.6	0.4	4.3%
Asian Children	51.6	51.6	0.0	1.6%	57.4	58.1	0.7	1.7%
AIAN Children	35.2	35.7	0.5	0.6%	47.0	51.3	4.3	0.3%

Notes: This table reports the change in mean household income rank for children in the 1978 and 1992 birth cohorts. Columns 1-2 report mean household income ranks for children born to families at the 25th percentile of the national income distribution in the 1978 and 1992 cohort, respectively; Column 3 reports the change in mean household income rank for these children; Column 4 reports the share of all children who are in the indicated race group and born to families with below-median incomes; Columns 5-6 report mean household income ranks for children born to families at the 75th percentile of the national income distribution in the 1978 and 1992 cohort, respectively; Column 7 reports the change in mean household income rank for these children; and Column 8 reports the share of all children who are in the indicated race group and born to families with above-median incomes. We estimate mean household income ranks using fitted values from a lowess regression on parental income percentiles for each race and birth cohort. See Figure I for additional details on how we construct the estimates of mean household income ranks and Section II for details on the sample construction and variable definitions. All statistics cleared under Census DRB release authorizations CBDRB-FY2022-CES010-004 and CBDRB-FY2023-CES005-025.

TABLE III

Childhood Exposure Effect of Changes in Parental Employment Rates: Estimates Based on Movers

	Child Household Income Rank				Predicted Rank
	Semi-Parametric Estimates	Origin x Destination FE	Own Par. Employed	Adult County FE	Col 1, Pred. Outcomes
	(1)	(2)	(3)	(4)	(5)
△ Par. Emp. Destination x Scaled Cohort x Scaled Move Age (β_μ)	0.339				0.020
	(0.097)				(0.056)
△ Par. Emp. Destination (γ_0)	-0.016				-0.007
	(0.031)				(0.026)
△ Par. Emp. Destination x Scaled Cohort (γ_1)	0.059				0.001
	(0.070)				(0.037)
△ Par. Emp. Destination x Scaled Move Age (γ_2)	0.064				-0.033
	(0.058)				(0.040)
Exposure to Parental Employment (β_μ)		0.273	0.280	0.445	
		(0.080)	(0.098)	(0.052)	
Origin x Par. Inc. x Race x Cohort x Move Age FE	X			X	X
Dest. 1978 Par. Emp. x Cohort x Move Age FE	X				X
Origin x Dest. x Par. Inc. x Race x Move Age FE		X	X		
Par. Inc. x Race x Cohort FE		X	X		
Par. Inc. x Race x Cohort x Adult County FE				X	
Number of Children (1,000s)	5,213	3,090	1,449	3,973	857

Notes: This table reports OLS regression estimates of β_μ, the causal effect of growing up from birth in a community with a 1 percentage point higher parental employment rate on children's household income ranks in adulthood. The estimation sample includes children who moved across counties once during childhood. We limit the sample to children with origin and destination counties with more than 2,000 children in the same race and class (defined as above- or below-median parental household income) group, between the 1978 and 1992 cohorts. Column 1 reports estimates of β_μ from Equation (10). We regress children's household income ranks in adulthood on the change in race-by-parental income percentile-specific parental employment rates between the 1978 and 1992 birth cohorts in the destination county ($\Delta\bar{e}_{dpr}$). To construct the semi-parametric analogue of Figure X, we interact $\Delta\bar{e}_{dpr}$ with move age, cohort, and move age times cohort. We rescale move age and cohort as in Equation (10), so that they each range from 0 to 1 within our sample. We also control for origin county-by-race-by-parental income percentile-by-birth cohort-by-move age fixed effects and cohort-by-move age fixed effects interacted with the group-specific parental employment rate in the destination county for the 1978 birth cohort. In columns 2-4, we regress children's household income ranks on mean race-by-parental income percentile-by-cohort-specific parental employment rates across origin and destination counties, weighted by the number of years the child spent in each county during childhood. Column 2 reports estimates of β_μ from Equation (11). We control for race-by-parental income percentile-by-origin county-by-destination county-by-move age fixed effects and race-by-parental income percentile-by-birth cohort fixed effects. Column 3 repeats this specification, but restricts to children whose parents are employed when the child is age 27. Column 4 controls for origin county-by-parental income percentile-by-race-by-birth cohort-by-move age fixed effects and adulthood county-by-parental income percentile-by-race-by-birth cohort fixed effects. Adulthood county is where the child is living at age 27. Column 5 repeats the specification in Column 1 using predicted child household income rank based on invariant parental characteristics. See Section V.D for additional details on how we construct these predicted income ranks. All specifications calculate county-by-subgroup parental employment rates using non-movers and parents who move more than once following the procedure described in Appendix A. Standard errors, clustered by origin county, are reported in parentheses. See Section II for details on the variable definitions and Section V for details on the sample construction. All statistics cleared under Census DRB release authorizations CBDRB-FY2023-CES005-025 and CBDRB-FY24-0359.

TABLE IV

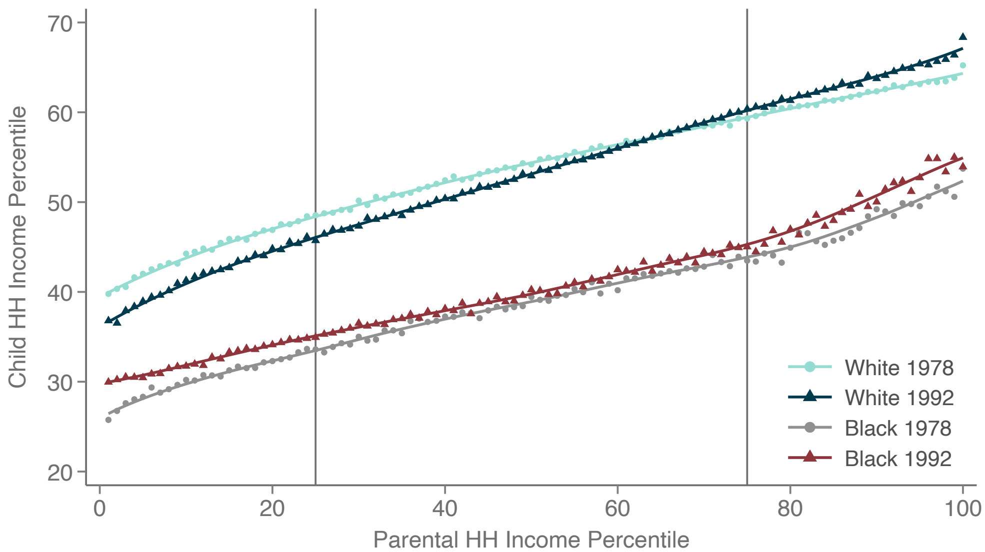
Child Exposure Effect of Changes in Parental Employment Rates: Sibling Comparisons

	Child Household Income Rank			
	Age Gap ≥ 4	Age Gap ≤ 3	All Age Gaps	Origin FE
	(1)	(2)	(3)	(4)
$\triangle$ Par. Emp. Destination (β_f)	0.104	0.044	0.062	0.054
	(0.021)	(0.013)	(0.011)	(0.013)
Implied Exposure Effect (β_μ)	0.299	0.319	0.301	0.260
	(0.060)	(0.092)	(0.055)	(0.062)
Par. Inc. x Race x Cohort x Sib. Age Diff. x Move Age FE	X	X	X	X
Dest. 1978 Par. Emp.	X	X	X	X
Origin Mean Child Inc. x Move Age FE	X	X	X	
Origin x Par. Inc. x Race x Move Age FE				X
Number of Children (1,000s)	563	1,153	1,716	1,654

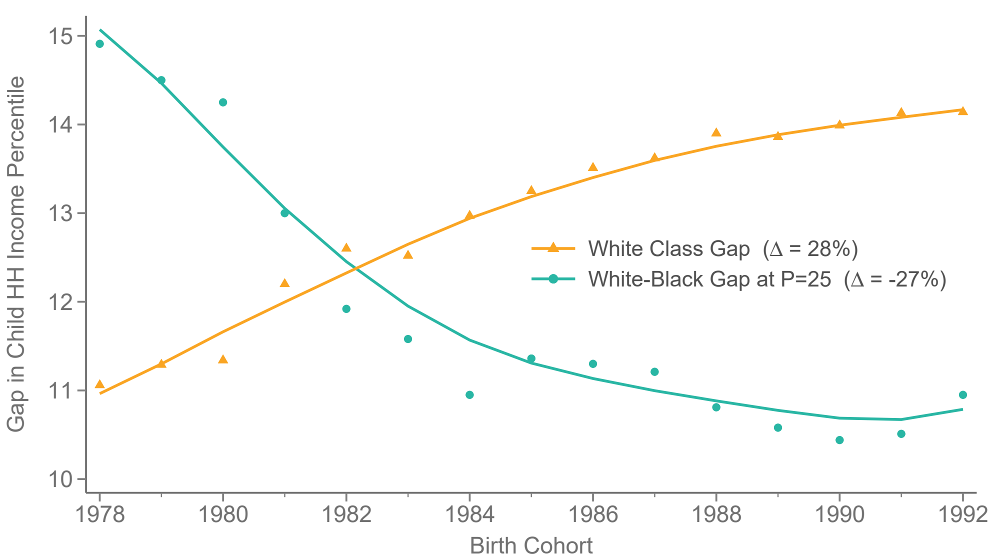
Notes: This table uses within-family variation between siblings to estimate the causal effect of growing up from birth in a community with 1 percentage point higher parental employment rates on children's household income ranks in adulthood. Columns 1-3 report OLS regression estimates of β_f from Equation (16). We regress the difference in children's household income ranks in adulthood between siblings (younger minus older) on the change in race-by-parental income percentile-specific parental employment rates between the 1978 and 1992 birth cohorts in the destination county ($\Delta\bar{e}_{dpr}$). We also report estimates of β_μ by rescaling β_f using Equation (15); see Appendix D for the derivation of the scaling factor. Column 1 restricts the sample to siblings at least four years apart in age. Column 2 restricts the sample to siblings three or fewer years apart in age. Column 3 uses all sibling pairs. Columns 1-3 control for race-by-parental income percentile-by-birth cohort-by-sibling age gap-by-move age fixed effects, the group-specific parental employment rate in the destination county for the 1978 birth cohort, and the group-specific mean child household income rank in the origin county across all birth cohorts interacted with move-age fixed effects. Column 4 replaces the group-specific mean child household income rank in the origin county x move-age fixed effects with origin county-by-race-by-parental income percentile-by-move-age fixed effects. In all specifications, we calculate parental employment rates using non-movers and parents who move more than once following the procedure described in Appendix A. We restrict the sample to the oldest and youngest siblings in each family who move from the same origin county to the same destination county in the same year and to siblings who moved across counties exactly once during childhood. We further limit the sample to origin and destination counties include more than 2,000 children in the same race and class (defined as above- or below-median parental household income) group between the 1978 and 1992 cohorts. Standard errors, clustered by origin county, are reported in parentheses. See Section II for details on the variable definitions and Section V for details on the sample construction. All statistics cleared under Census DRB release authorization CBDRB-FY24-0359.

FIGURE I
Intergenerational Mobility by Birth Cohort, Race, and Class

A. Child Household Income versus Parental Household Income for the 1978 and 1992 Birth Cohorts



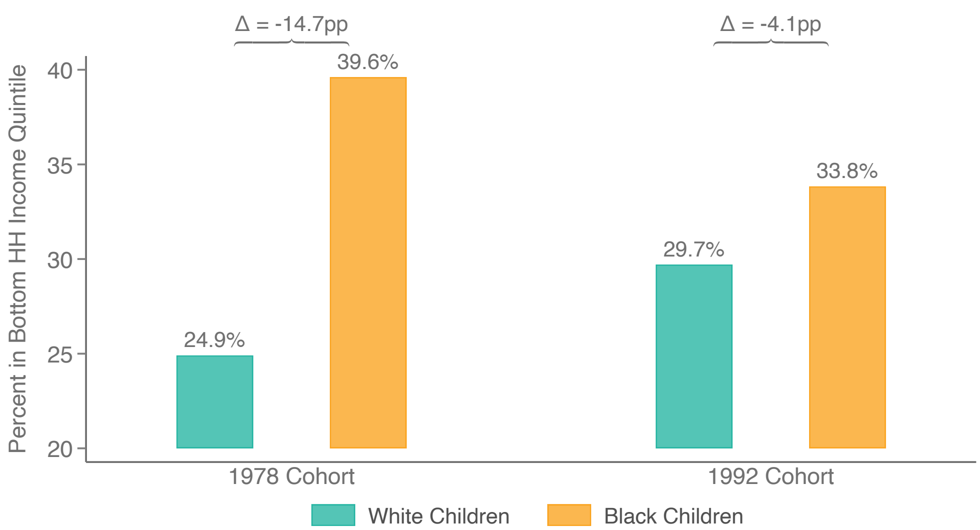
B. White-Black Race Gap at the 25th Percentile and Class Gap for White Children by Birth Cohort



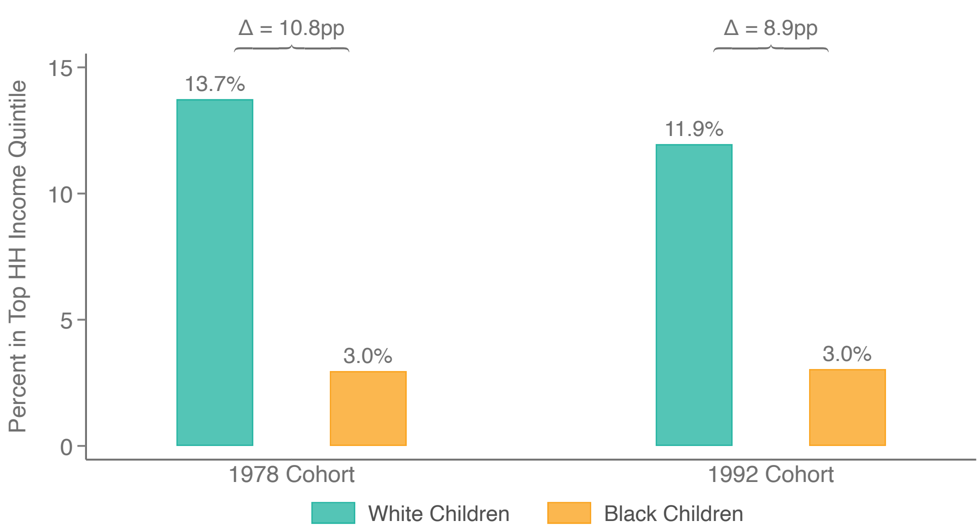
Notes: These figures plot changes in economic mobility for white and Black children between the 1978 and 1992 birth cohorts. Panel A plots mean household income ranks in adulthood for white and Black children in the 1978 and 1992 birth cohorts at each parental income percentile. We estimate the fitted lines for each race and birth cohort using a lowess regression on the binned series (with bandwidth 0.3). The vertical lines represent the 25th and 75th percentiles of the parental income distribution. Panel B plots the difference in mean household income ranks in adulthood for white versus Black children born to families at the 25th percentile of the national income distribution (the white-Black race gap) and the difference in mean household income ranks in adulthood for white children born to families at the 25th versus 75th percentiles of the national income distribution (the white class gap). We also report the percentage change in the white class and white-Black race gaps between the 1978 and 1992 birth cohorts. We estimate mean child household income ranks by race, cohort, and parent income percentile using fitted values from a lowess regression of children's ranks on parents' income ranks for each race and birth cohort. See Section II for details on the sample construction and variable definitions.

FIGURE II
Intergenerational Persistence of Poverty versus Upper Tail Success by Birth Cohort and Race

A. Probability of Remaining in the Bottom Household Income Quintile

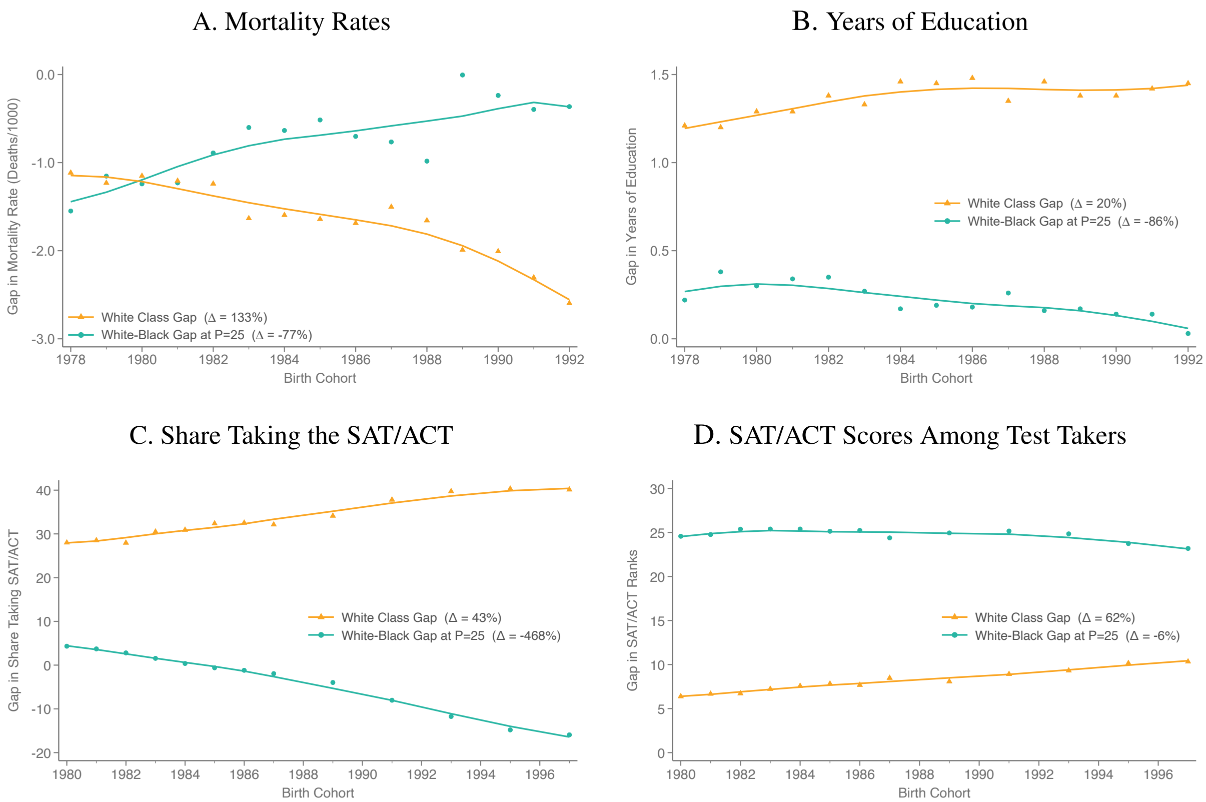


B. Probability of Reaching the Top Household Income Quintile



Notes: These figures plot changes in the intergenerational persistence of poverty versus upper tail success for white and Black children born to families in the bottom quintile of the national income distribution. Panel A plots the percent of children who remain in the bottom household income quintile, conditional on being born to families in the bottom quintile of the national income distribution. Panel B plots the percent of children who reach the top household income quintile, conditional on being born to families in the bottom quintile of the national income distribution. In each panel, we report the white-Black race gap for both the 1978 and 1992 birth cohorts. Numbers may not aggregate due to rounding. See Section II for details on the sample construction and variable definitions.

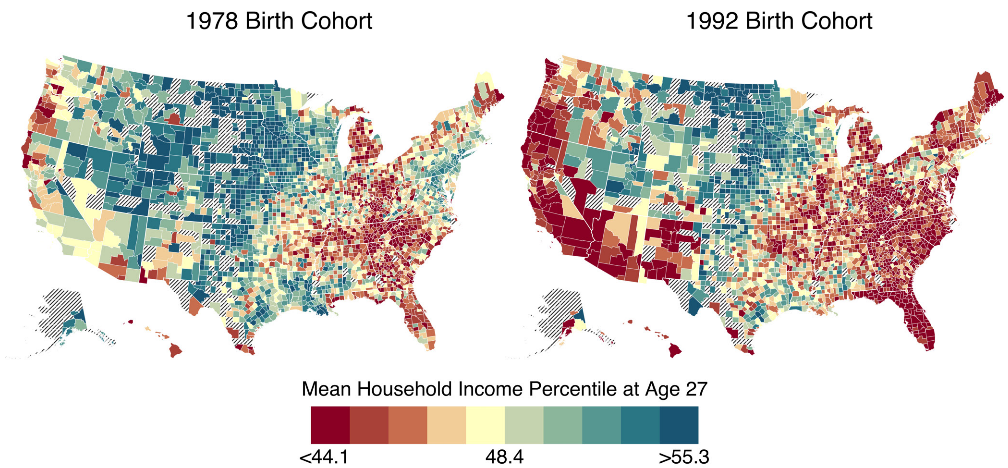
FIGURE III
White Class and White-Black Race Gaps in Pre-Labor Market and Non-Monetary Outcomes by Birth Cohort



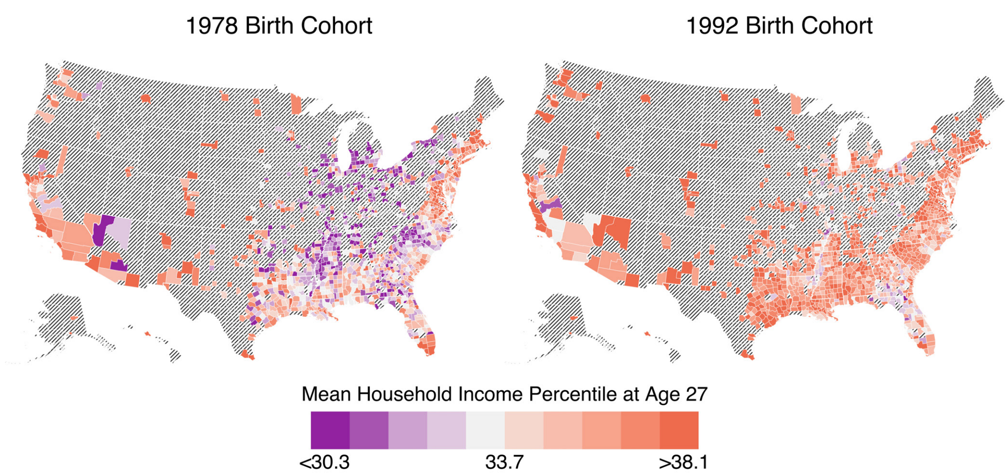
Notes: These figures plot the white class and white-Black race gaps for mortality, years of education, the share of children taking the SAT/ACT, and SAT/ACT scores among test takers. For each outcome, we report the percentage change in the white class and white-Black race gaps between the 1978 and 1992 birth cohorts. We estimate each outcome using fitted values from a lowess regression on parental income percentiles for each race and birth cohort. We take the difference in these fitted values to compute the white class and white-Black race gaps. See Figure I for additional details on how we construct the estimates and Section II for details on the sample construction and variable definitions.

FIGURE IV
The Changing Geography of Intergenerational Mobility for Low-Income Families

A. White Children at the 25th Percentile



B. Black Children at the 25th Percentile

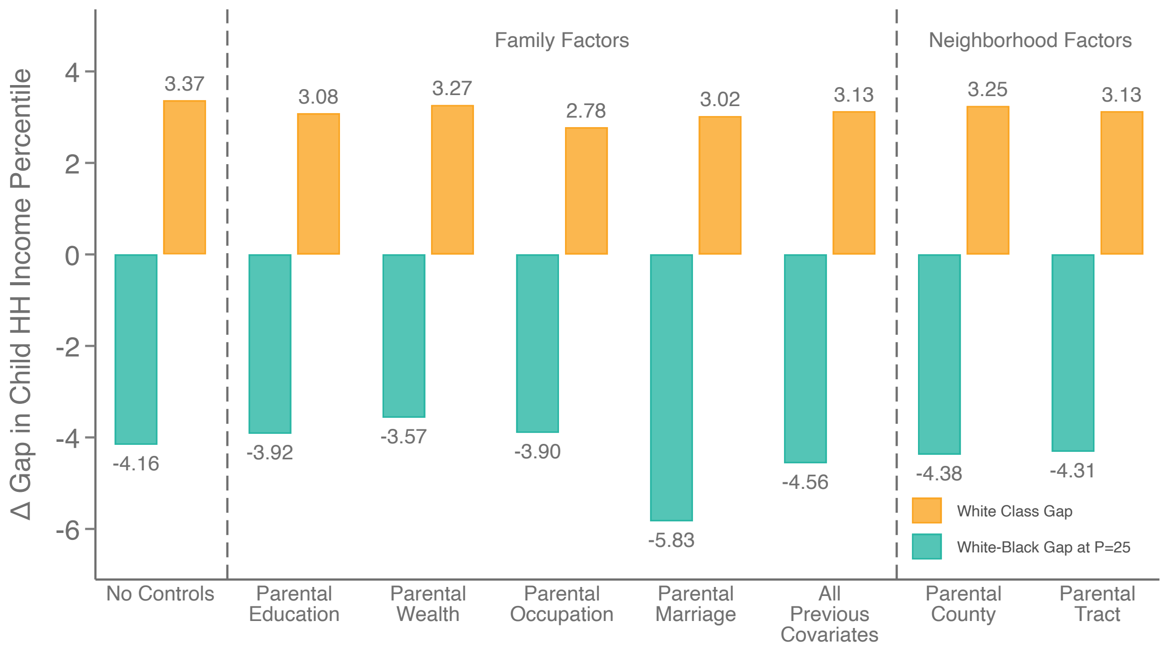


These maps must be printed in color to be interpretable.

Notes: These figures show maps of mean household income ranks in adulthood by county for white and Black children born to families at the 25th percentile of the national income distribution. Panel A restricts to counties with at least 250 white children born to families with below-median incomes in the 1978-1992 birth cohorts; Panel B restricts to counties with at least 250 Black children born to families with below-median incomes in the 1978-1992 birth cohorts. Counties shown in gray are areas with no estimates due to insufficient data in the relevant group. See Appendix A for details on how we construct county-by-race-by-class-by-cohort estimates of economic mobility and Section II for details on the sample construction and variable definitions.

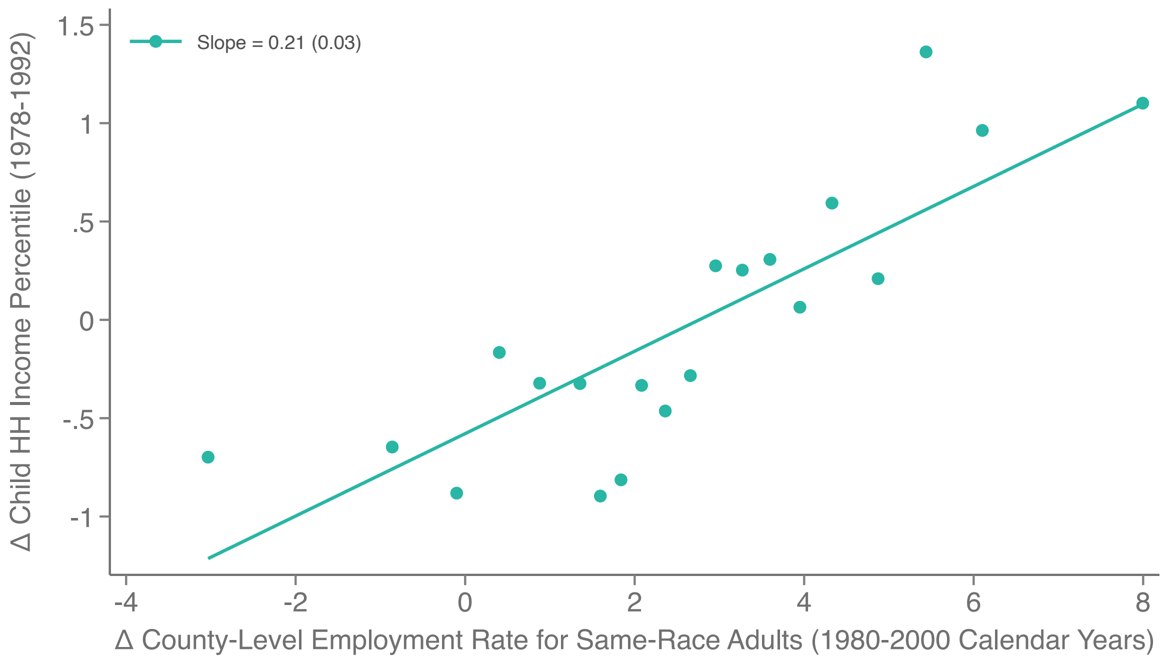
FIGURE V

Effects of Family- and Neighborhood-Level Factors on White Class and White-Black Race Gaps



Notes: This figure reports reports OLS regression estimates of the change in the white class and white-Black race gaps, controlling for family- and neighborhood-level factors. The first pair of bars reports the change in the white class and white-Black race gaps between the 1978 and 1992 birth cohorts with no controls, estimated by regressing children's household income ranks on a linear cohort control interacted with class (for the white class gap) or race (for the white-Black race gap). The next five pairs report estimates controlling for family-level factors interacted with class and cohort fixed effects (for the white class gap) or race and cohort fixed effects (for the white-Black race gap). The seventh pair reports estimates controlling for county fixed effects interacted with class and cohort (for the white class gap) or race and cohort (for the white-Black race gap). The final pair reports estimates controlling for Census tract fixed effects interacted with class and cohort (for the white class gap) or race and cohort (for the white-Black race gap). For the white class gap, we restrict the sample to white children born to families between the 20th and 30th percentiles of the parental income distribution or families between the 70th and 80th percentiles of the parental income distribution. For the white-Black race gap, we restrict the sample to white and Black children born to families between the 20th and 30th percentiles of the parental income distribution. Specifications with no controls and with controls for parental marriage use all available children. Specifications with controls for parental education, parental wealth, and parental occupation are restricted to families with at least one parent in the ACS or Census long form. Specifications with controls for geographic fixed effects are restricted to geographies with at least one child in each parental income group (for the white class gap) or race group (for the white-Black race gap). For all specifications, we first estimate the unconditional change in the white class and white-Black race gaps in the relevant subsample. We then estimate the conditional change in both gaps after accounting for the relevant set of controls. Finally, we multiply the ratio of the unconditional and conditional estimates in the relevant subsample by the unconditional change in the full sample to generate the estimates reported above. See Section II for details on the sample construction and variable definitions, and Section IV for details on the regression specifications.

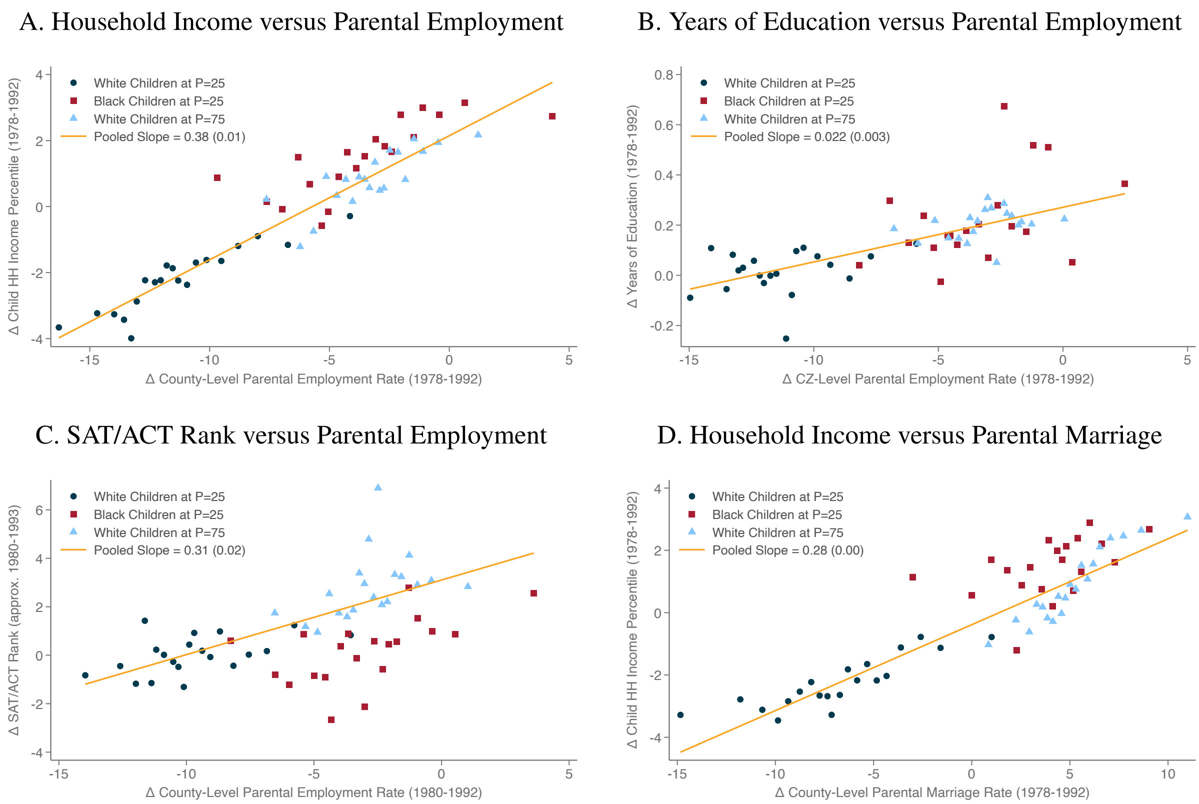
FIGURE VI
Changes in Children's Household Incomes in Adulthood versus Changes in Employment Rates for Same-Race Adults by County



Notes: This figure shows a binned scatterplot of changes in children's household income ranks in adulthood versus changes in the employment rate for same-race adults in their childhood counties, controlling for race-by-parental income percentile fixed effects. We control for these variables by residualizing both the x and y variables in the figure with respect to this control vector and presenting a binned scatter plot of the residuals to depict the variation identifying the partial regression coefficient of interest non-parametrically. We measure changes in children's household income ranks for white and Black children born to parents at the 25th and 75th percentiles of the national income distribution. We measure changes in the employment rate for same-race adults using the difference in employment rates among adults aged 25-44 in the 2000 versus 1980 decennial Censuses (pooling across class groups), corresponding to changes over the period in which children in our focal birth cohorts were growing up. We report the slope and standard error of the weighted best-fit line estimated in an OLS regression, where we weight by the number of children in each county-by-race-by-class cell. We restrict to counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct the county-by-race-by-class-by-cohort estimates for each outcome and Section II for details on the sample construction and variable definitions.

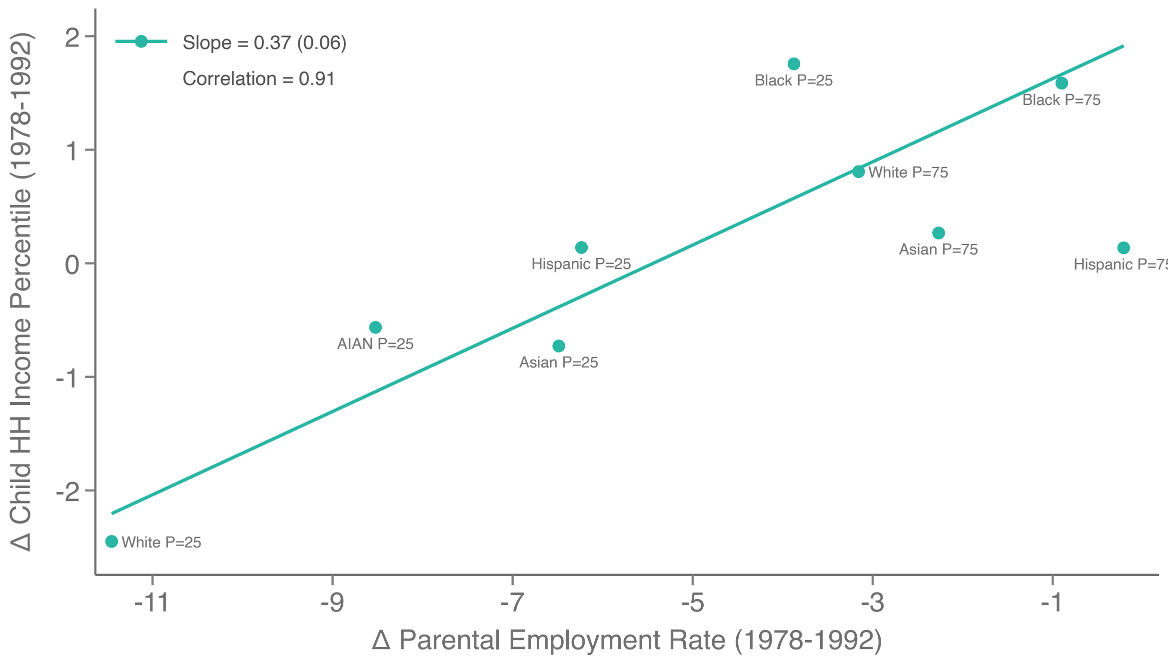
FIGURE VII

Changes in Children's Outcomes in Adulthood versus Changes in Parental Outcomes by Race, Class, and County



Notes: These figures show binned scatterplots of changes in children's outcomes in adulthood versus changes in parental outcomes for white and Black children born to families at the 25th percentile of the national income distribution and white children born to families at the 75th percentile of the national income distribution. Panel A plots changes in children's household income ranks in adulthood versus changes in parental employment rates; Panel B plots changes in children's educational attainment versus changes in parental employment rates; Panel C plots changes in end-of-high-school SAT/ACT scores; and Panel D plots changes in children's household income ranks in adulthood versus changes in parental marriage rates. We demean our measure of parental marriage rates so that it is comparable across birth cohorts at the national level, even though later cohorts are linked to their parents at a younger age. We also report the slope and standard error of the weighted best-fit line estimated in an OLS regression, where we weight by the number of children in each county-by-race-by-class cell (Panels A, C, D) or CZ-by-race-by-class cell (Panel B). We restrict to geographies with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct the county-by-race-by-class-by-cohort and CZ-by-race-by-class-by-cohort estimates for each outcome and Section II for details on the sample construction and variable definitions.

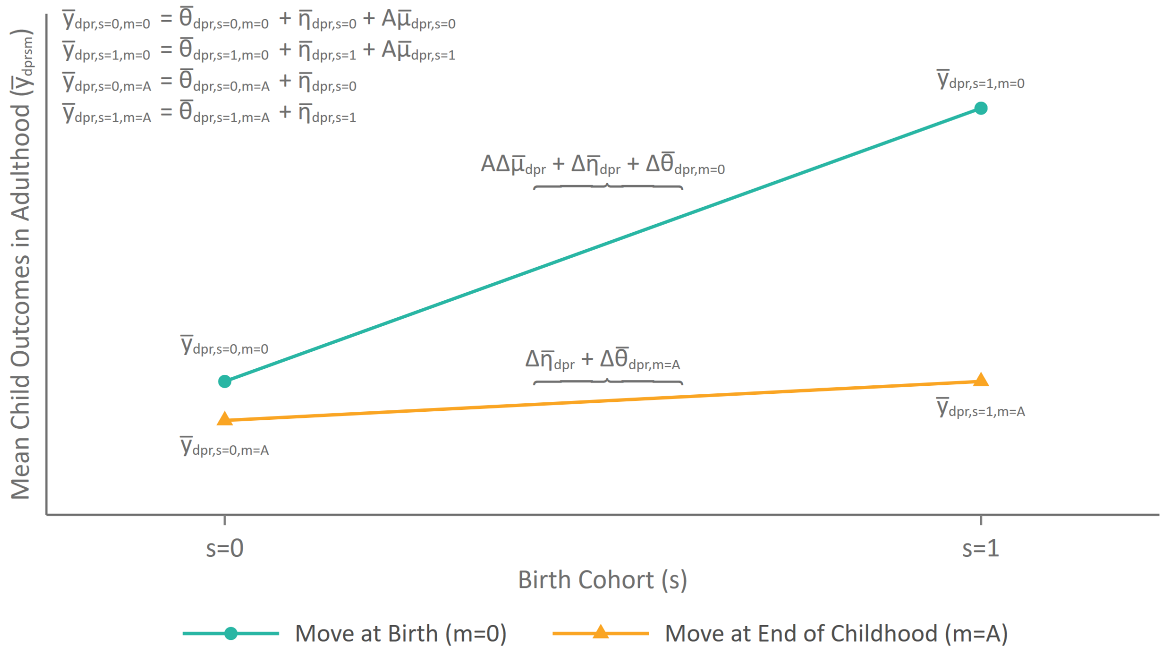
FIGURE VIII
Changes in Children's Household Income in Adulthood versus Changes in Parental Employment Rates by Race and Class



Notes: This figure plots national-level changes in children's household income ranks in adulthood versus changes in parental employment rate. We report the slope and standard error of the weighted best-fit line estimated in an OLS regression, where we weight by the number of children in each race-by-class cell (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. We also report the weighted correlation between both variables. See Section II for details on the sample construction and variable definitions and Appendix Table A.23 for the estimates for each point in the above scatterplot.

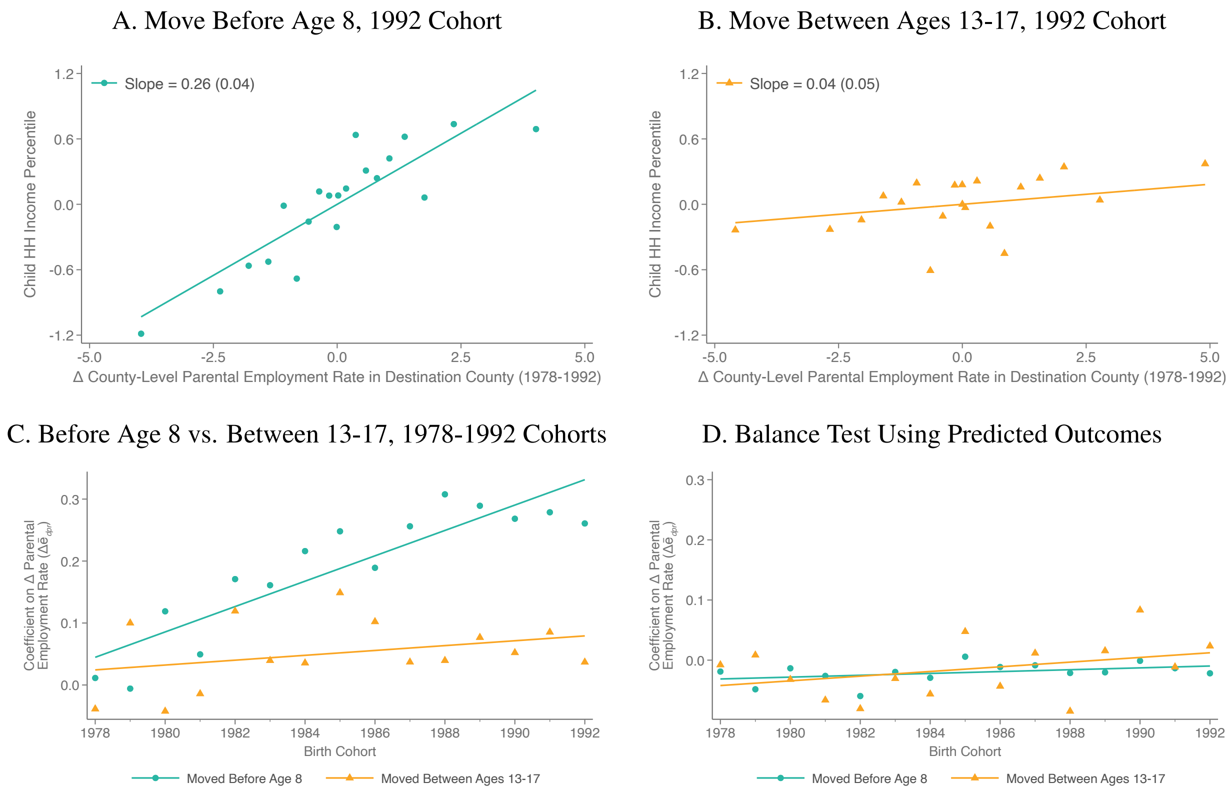
FIGURE IX

Identifying the Causal Exposure Effect of Changes in Childhood Environments



Notes: This figure illustrates how we identify the causal exposure effect of spending one's childhood in an area that experienced a 1 percentage point increase in parental employment rates across birth cohorts (β_μ). It plots the average outcomes ($\bar{y}_{dprsm}$) of children born in cohort s in an origin county o with parental income percentile p and race r who move to destination county d at age m. We compare mean outcomes for children in two cohorts ($s = 0, 1$) who move at birth ($m = 0$) or at the end of childhood ($m = A$). The difference in mean outcomes across birth cohorts for children who move at age $m = A$ ($\Delta\bar{y}_{dpr,m=A}$) reflects changes in labor market conditions in the destination county across cohorts ($\Delta\bar{\eta}_{dpr}$) and changes in selection ($\theta\bar{y}_{dpr,m=A}$). The difference across birth cohorts for children who move at age $m = 0$ ($\Delta\bar{y}_{dpr,m=0}$) additionally includes the change in the childhood exposure effect across cohorts ($A\Delta\bar{\mu}_{dpr}$). Under the identification assumption in Equation (9), changes in selection effects across cohorts do not vary with move age ($\Delta\bar{\theta}_{dpr,m=0} = \Delta\bar{\theta}_{dpr,m=A}$), so the difference in differences across the four points plotted in the figure ($\Delta\bar{y}_{dpr,m=0} - \Delta\bar{y}_{dpr,m=A}$) isolates $A\Delta\bar{\mu}_{dpr}$.

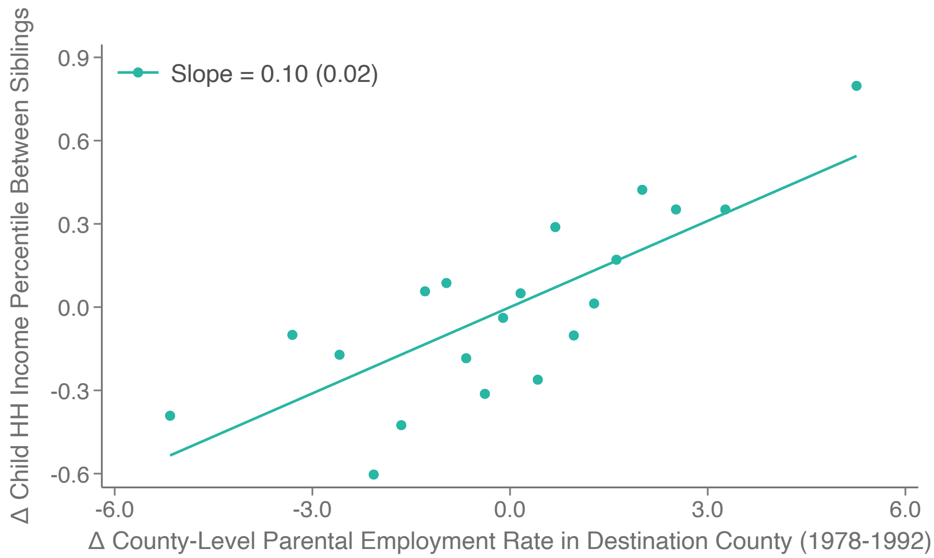
FIGURE X
Causal Exposure Effects of Changes in Childhood Environments by Move Age and Birth Cohort



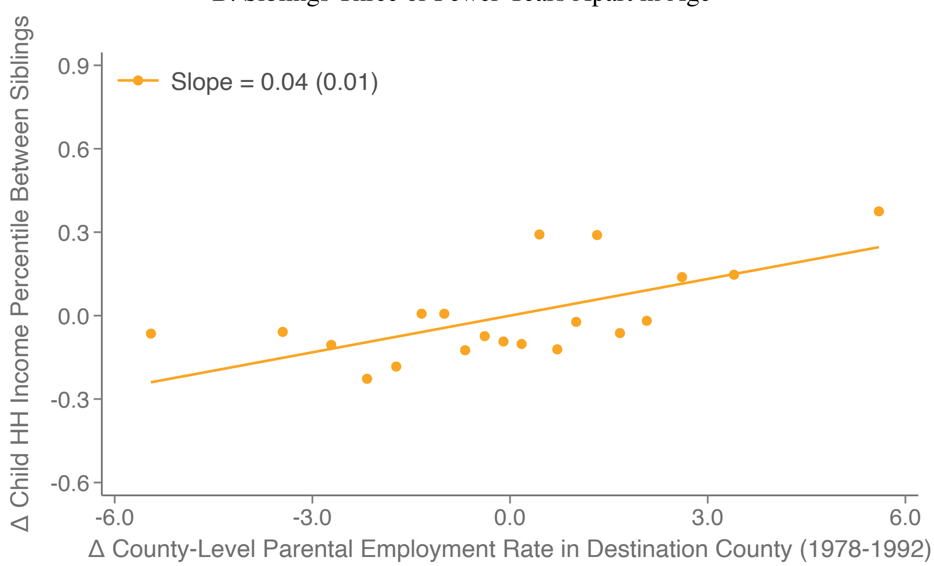
Notes: This figure analyzes the outcomes of children who moved exactly once across counties during childhood (before age 18). Each panel shows the relationship between changes in children's mean household income ranks in adulthood and changes in parental employment rates (for their race and class group) between the 1978 and 1992 birth cohorts in the county to which they moved ($\Delta\bar{e}_{dpr}$), by move age and birth cohort. Panels A and B consider children in the 1992 birth cohort and show binned scatterplots of children's household income ranks versus the change in parental employment rates (measured in percentage points) in the destination county, for children who move before age 8 and children who move between ages 13-17, respectively. These plots control for the group-specific parental employment rate in the destination county for the 1978 birth cohort and race-by-parental income percentile-by-origin county-by-move age fixed effects using the same method as in Figure VI. Panel C repeats the regressions shown in Panels A and B for each birth cohort between 1978 and 1992 and plots the coefficient on $\Delta\bar{e}_{dpr}$ separately for each birth cohort. Panel D plots the coefficient on $\Delta\bar{e}_{dpr}$ when the outcome is predicted children's household income rank in adulthood, based on baseline parental characteristics including parental education, parental wealth, parental occupation, and parental marital status in childhood. See Section V.D for additional details on how we construct these predicted income ranks. The change in parental employment rates is calculated by pooling non-movers and families who move more than once following the procedure described in Appendix A. We restrict the estimation sample for the figures to origin and destination counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. See Section II for details on the variable definitions and Section V for details on the sample construction.

FIGURE XI
Causal Exposure Effects of Changes in Childhood Environments: Within-Family Estimates

A. Siblings Four or More Years Apart in Age



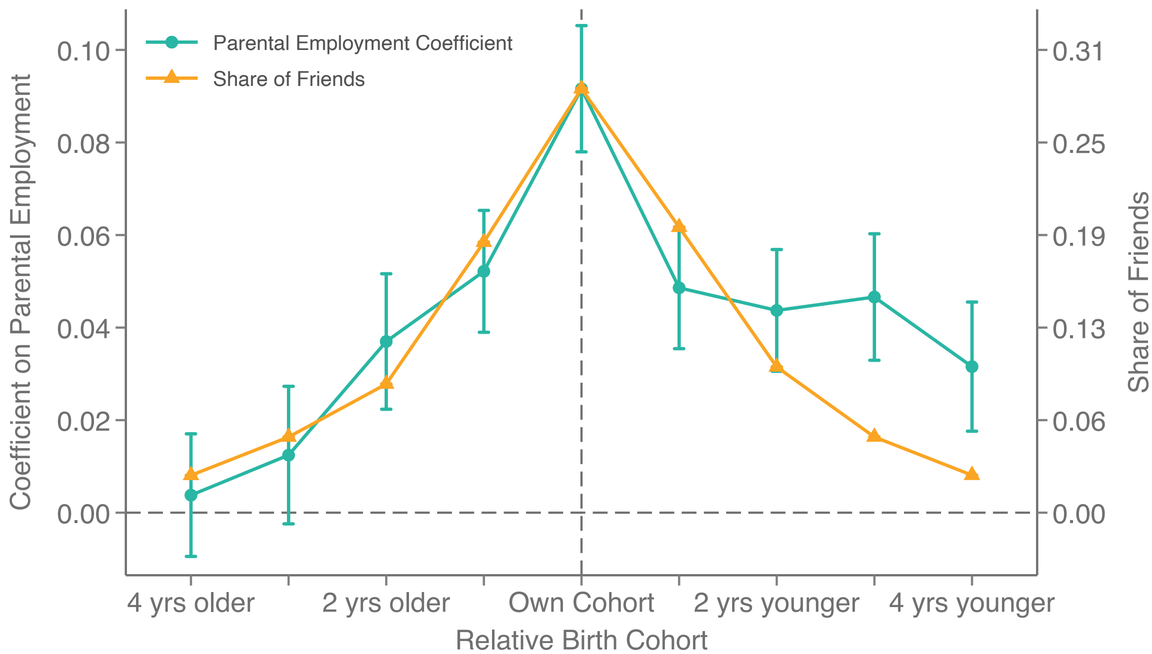
B. Siblings Three or Fewer Years Apart in Age



Notes: This figure analyzes the outcomes of siblings who moved across counties during their childhood. Each panel presents a binned scatterplot of the difference in household income ranks in adulthood between siblings (younger minus older) versus the change in group-specific parental employment rates (in percentage points) between the 1978 and 1992 birth cohorts in the destination county. Panel A considers siblings four or more years apart in age, while Panel B considers siblings three or fewer years apart in age. We control for race-by-parental income percentile-by-cohort-by-sibling age gap-by-move age fixed effects, the group-specific parental employment rate in the destination county for the 1978 birth cohort, and the group-specific mean children's household income rank in adulthood in the origin county across all birth cohorts interacted with move-age fixed effects using the same method as in Figure VI. The slopes reported in each panel are OLS regression estimates that correspond to β_f in Equation (16). We calculate the change in parental employment rates pooling non-movers and parents who move more than once following the procedure described in Appendix A. We restrict the estimation sample to the oldest and youngest siblings who move from the same origin county to the same destination county in the same year and to siblings who moved across counties once during childhood. We also restrict to origin and destination counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. See Section II for details on the variable definitions and Section V for details on the sample construction.

FIGURE XII

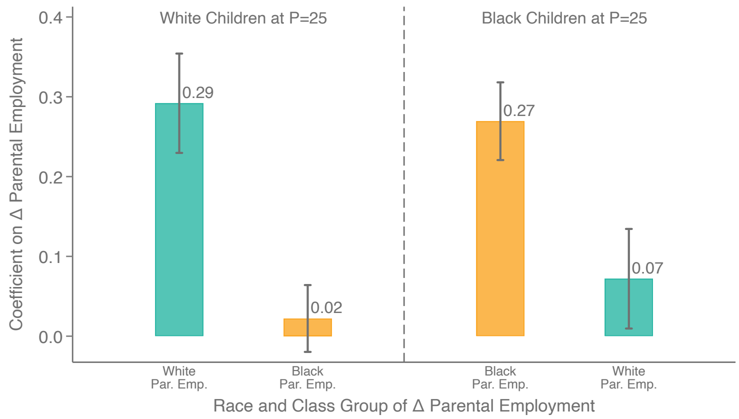
Children's Outcomes in Adulthood versus Parental Employment Rates by Relative Birth Cohort



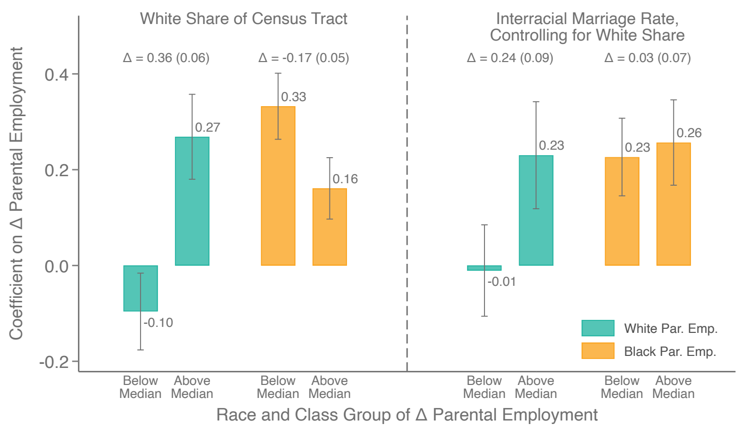
Notes: This figure shows how children's outcomes in adulthood correlate with parental employment rates in their own vs. adjacent birth cohorts. The green series in circles reports estimates from an OLS regression of children's household income ranks in adulthood on group-specific parental employment rates in one's own birth cohort and adjacent birth cohorts, controlling for county-by-race-by-parental income percentile fixed effects and cohort-by-race-by parental income percentile fixed effects, as in Equation (17). We weight by the number of children in each county-by-race-by-class cell. We restrict the sample to low-income white and Black children and high-income white children in the 1982-1988 birth cohorts and counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. The vertical bars denote 95% confidence intervals, with standard errors clustered at the county level. See Appendix A for details on how we construct the county-by-race-by-class-by-cohort estimates for each outcome and Section II for details on the sample construction and variable definitions. The orange series in triangles reports the share of childhood Facebook friends in one's own birth cohort and adjacent birth cohorts, where we define the friendship share as the number of friends one has in a given birth cohort divided by the total number of friends one has across all nine relative birth cohorts. We restrict the sample for this series to children in the 1993 birth cohort with at least ten friends across all nine relative birth cohorts. We also restrict to friendships made in childhood, where the user and the friend are in the same county, both the user and friend have been active on Facebook at least once in the past 30 days, and both the user and friend have not been flagged as fake accounts by Facebook's internal algorithms. We use methods from the differential privacy literature to add a small amount of noise to the Facebook statistics to protect privacy while maintaining a high level of statistical reliability.

FIGURE XIII
Changes in Children's Outcomes in Adulthood versus Changes in Parental Employment Rates by Degree of Social Interaction

A. Effects of Changes in Own- versus Other-Race Parental Employment Rates for Low-Income Children



B. Effects of Changes in Own- versus Other-Race Parental Employment Rates for Low-Income Black Children by Neighborhood Characteristics



Notes: This figure reports OLS regression estimates of the effect of county-level changes in parental employment rates on county-level changes in children's household income ranks in adulthood. Panel A reports estimates of the effect of changes in low-income white (green bars) and low-income Black (orange bars) parental employment rates on changes in outcomes for low-income white children and low-income Black children. We restrict the sample to white and Black children born to families at the 25th percentile of the national income distribution. Panel B reports estimates of the effect of changes in low-income white (green bars) and low-income Black (orange bars) parental employment rates on changes in outcomes for low-income Black children. Here, we restrict the sample to Black children born to families at the 25th percentile of the national income distribution. In Panel B, the bars on the left examine heterogeneity by below-median versus above-median exposure to white people, as proxied by the mean tract-level white share in a county. We also report the difference in estimates for counties with below-median versus above-median white exposure. The bars on the right examine heterogeneity by the degree of interaction with white people, as proxied by county-level interracial marriage rates controlling for white exposure using the specification in equation (19). We also report the difference in estimates for counties with below-median versus above-median interracial marriage rates. All specifications are weighted by the number of children in each race-by-class cell. We restrict to counties with more than 2,000 white and and 2,000 Black children born to families with below-median parental household income across the 1978 and 1992 birth cohorts. The vertical bars denote 95% confidence intervals. Numbers may not aggregate due to rounding. See Section VI.B for details on the sample construction, variable definitions, and regression specifications.

TABLE A.1
Summary Statistics by Birth Cohort and Class for White Children

	White Children at P=25			White Children at P=75		
	1978 Cohort	1992 Cohort	Change	1978 Cohort	1992 Cohort	Change
A. Parental Characteristics	(1)	(2)	(3)	(4)	(5)	(6)
Median HH Income, Child Ages 13-17	$37,800	$32,020	-$5,772	$122,100	$124,500	$2,430
Two Parents Present	57.0%	66.2%	9.3	93.9%	89.9%	-4.0
Years of Education	13.7	13.8	0.0	14.8	15.2	0.4
High School Graduation	85.4%	84.4%	-1.0	95.9%	96.9%	1.0
Four-Year College Graduation	11.3%	9.7%	-1.6	25.5%	31.6%	6.1
Home Ownership	75.2%	59.4%	-15.8	92.1%	87.9%	-4.2
B. Child Outcomes in Child Adulthood						
Median HH Income, Age 27	$39,510	$32,150	-$7,353	$56,000	$50,730	-$5,271
Median Indiv. Income, Age 27	$27,680	$26,150	-$1,530	$40,680	$41,540	$861
Mean HH Income Percentile, Age 27	48.6	46.2	-2.4	59.5	60.4	0.8
Bottom Quintile of HH Income, Age 27	19.4%	22.8%	3.4	10.8%	11.0%	0.3
Top Quintile of HH Income, Age 27	17.1%	15.4%	-1.7	29.1%	30.5%	1.5
Employment, Age 27	82.6%	80.3%	-2.4	89.1%	89.3%	0.1
Marriage, Age 27	40.4%	26.6%	-13.8	43.9%	31.0%	-12.9
Marriage, Age 32	49.1%	39.1%	-10.0	58.5%	52.1%	-6.3
Mortality, Ages 24-27	0.32%	0.61%	0.28	0.21%	0.34%	0.13
Incarceration, Age 22	1.16%	1.43%	0.26	0.36%	0.33%	-0.04
Years of Education, Age 27	14.3	14.2	-0.1	15.4	15.6	0.2
High School Graduation, Age 27	88.8%	91.6%	2.8	97.2%	97.5%	0.3
Four-Year College Graduation, Age 27	20.2%	19.4%	-0.8	41.6%	48.9%	7.3
C. Parental Outcomes in Child Adulthood						
Employment, Child Age 27	66.2%	55.8%	-10.4	78.4%	75.8%	-2.5
Marriage, Child Age 27	53.9%	36.5%	-17.4	84.3%	74.1%	-10.2
Mortality, Child Ages 18-27	4.20%	5.80%	1.60	2.44%	2.61%	0.18
Number of Children	155,000	158,000	3,000	243,000	307,000	64,000

Notes: This table reports summary statistics for white children in the 1978 and 1992 birth cohorts who are claimed as child dependents by parents with positive household income between child ages 13-17. Panel A reports parental characteristics during the child's youth; Panel B reports child outcomes during the child's adulthood; and Panel C reports parental outcomes during the child's adulthood. Columns 1-3 report statistics for children born to families between the 20th and 30th percentiles of the parental income distribution; Columns 4-6 report statistics for children born to families between the 70th and 80th percentiles of the parental income distribution. See Section II for more details and variable definitions. All monetary values are reported in 2023 dollars. All statistics cleared under Census DRB release authorizations CBDRB-FY24-0143 and CBDRB-FY24-0359.

TABLE A.2
Summary Statistics by Birth Cohort and Class for Black Children

	Black Children at P=25			Black Children at P=75		
	1978 Cohort	1992 Cohort	Change	1978 Cohort	1992 Cohort	Change
A. Parental Characteristics	(1)	(2)	(3)	(4)	(5)	(6)
Median HH Income, Child Ages 13-17	$37,260	$31,840	-$5,412	$121,700	$123,300	$1,543
Two Parents Present	22.0%	16.7%	-5.2	82.1%	53.1%	-29.1
Years of Education	13.6	13.8	0.2	14.9	15.4	0.5
High School Graduation	79.9%	83.2%	3.3	92.8%	95.0%	2.1
Four-Year College Graduation	9.2%	8.0%	-1.2	27.7%	33.5%	5.8
Home Ownership	59.6%	40.6%	-19.1	85.2%	75.3%	-9.9
B. Child Outcomes in Child Adulthood						
Median HH Income, Age 27	$21,690	$20,630	-$1,054	$33,930	$31,560	-$2,365
Median Indiv. Income, Age 27	$19,420	$21,030	$1,607	$30,460	$30,880	$424
Mean HH Income Percentile, Age 27	33.6	35.2	1.6	43.8	45.2	1.5
Bottom Quintile of HH Income, Age 27	33.3%	28.8%	-4.5	22.1%	20.5%	-1.6
Top Quintile of HH Income, Age 27	4.3%	3.9%	-0.3	10.7%	11.3%	0.6
Employment, Age 27	80.5%	83.5%	3.0	86.7%	88.2%	1.5
Marriage, Age 27	10.5%	6.6%	-3.9	16.4%	10.3%	-6.0
Marriage, Age 32	14.5%	11.1%	-3.4	24.0%	20.3%	-3.6
Mortality, Ages 24-27	0.48%	0.64%	0.16	0.36%	0.33%	-0.03
Incarceration, Age 22	4.53%	4.28%	-0.25	2.08%	1.74%	-0.34
Years of Education, Age 27	14.1	14.1	0.1	15.2	15.3	0.1
High School Graduation, Age 27	86.8%	90.4%	3.6	94.8%	94.6%	-0.2
Four-Year College Graduation, Age 27	16.9%	16.3%	-0.6	36.1%	40.0%	3.9
C. Parental Outcomes in Child Adulthood						
Employment, Child Age 27	74.9%	71.3%	-3.6	77.6%	76.5%	-1.1
Marriage, Child Age 27	20.6%	13.0%	-7.7	66.7%	54.5%	-12.2
Mortality, Child Ages 18-27	4.78%	4.97%	0.19	3.75%	3.28%	-0.47
Number of Children	61,500	96,500	35,000	18,500	30,000	11,500

Notes: This table reports summary statistics for Black children in the 1978 and 1992 birth cohorts who are claimed as child dependents by parents with positive household income between child ages 13-17. Panel A reports parental characteristics during the child's youth; Panel B reports child outcomes during the child's adulthood; and Panel C reports parental outcomes during the child's adulthood. Columns 1-3 report statistics for children born to families between the 20th and 30th percentiles of the parental income distribution; Columns 4-6 report statistics for children born to families between the 70th and 80th percentiles of the parental income distribution. See Section II for more details and variable definitions. All monetary values are reported in 2023 dollars. All statistics cleared under Census DRB release authorizations CBDRB-FY24-0143 and CBDRB-FY24-0359.

TABLE A.3
Summary Statistics by Birth Cohort and Class for Hispanic Children

	Hispanic Children at P=25			Hispanic Children at P=75		
	1978 Cohort	1992 Cohort	Change	1978 Cohort	1992 Cohort	Change
A. Parental Characteristics	(1)	(2)	(3)	(4)	(5)	(6)
Median HH Income, Child Ages 13-17	$37,240	$31,840	-$5,399	$121,400	$123,300	$1,851
Two Parents Present	49.3%	40.1%	-9.2	91.5%	71.4%	-20.1
Years of Education	10.7	11.3	0.7	14.0	14.2	0.2
High School Graduation	50.2%	53.1%	2.9	85.2%	84.5%	-0.8
Four-Year College Graduation	5.8%	5.6%	-0.3	18.2%	21.9%	3.7
Home Ownership	65.1%	46.8%	-18.2	86.5%	77.0%	-9.5
B. Child Outcomes in Child Adulthood						
Median HH Income, Age 27	$34,700	$31,600	-$3,098	$46,150	$41,100	-$5,052
Median Indiv. Income, Age 27	$27,500	$27,960	$463	$36,640	$36,210	-$424
Mean HH Income Percentile, Age 27	44.3	44.9	0.6	53.1	53.5	0.4
Bottom Quintile of HH Income, Age 27	22.0%	21.1%	-0.8	15.7%	14.7%	-0.9
Top Quintile of HH Income, Age 27	11.9%	11.5%	-0.4	21.1%	20.6%	-0.4
Employment, Age 27	81.4%	82.0%	0.6	87.1%	87.9%	0.9
Marriage, Age 27	29.2%	18.8%	-10.5	32.7%	21.1%	-11.6
Marriage, Age 32	36.0%	29.0%	-6.9	44.6%	37.6%	-7.0
Mortality, Ages 24-27	0.31%	0.38%	0.08	0.21%	0.32%	0.12
Incarceration, Age 22	1.53%	1.27%	-0.26	0.74%	0.43%	-0.30
Years of Education, Age 27	13.6	14.2	0.6	15.0	15.3	0.3
High School Graduation, Age 27	81.1%	88.1%	7.0	93.8%	96.8%	3.0
Four-Year College Graduation, Age 27	13.0%	19.7%	6.7	27.9%	39.8%	11.9
C. Parental Outcomes in Child Adulthood						
Employment, Child Age 27	67.5%	62.1%	-5.3	77.0%	77.1%	0.0
Marriage, Child Age 27	48.0%	31.8%	-16.1	79.4%	69.4%	-10.0
Mortality, Child Ages 18-27	3.06%	3.40%	0.35	2.25%	2.08%	-0.17
Number of Children	50,500	105,000	54,500	19,000	47,000	28,000

Notes: This table reports summary statistics for Hispanic children in the 1978 and 1992 birth cohorts who are claimed as child dependents by parents with positive household income between child ages 13-17. Panel A reports parental characteristics during the child's youth; Panel B reports child outcomes during the child's adulthood; and Panel C reports parental outcomes during the child's adulthood. Columns 1-3 report statistics for children born to families between the 20th and 30th percentiles of the parental income distribution; Columns 4-6 report statistics for children born to families between the 70th and 80th percentiles of the parental income distribution. See Section II for more details and variable definitions. All monetary values are reported in 2023 dollars. All statistics cleared under Census DRB release authorizations CBDRB-FY24-0143 and CBDRB-FY24-0359.

TABLE A.4
Summary Statistics by Birth Cohort and Class for Asian Children

	Asian Children at P=25			Asian Children at P=75		
	1978 Cohort	1992 Cohort	Change	1978 Cohort	1992 Cohort	Change
A. Parental Characteristics	(1)	(2)	(3)	(4)	(5)	(6)
Median HH Income, Child Ages 13-17	$37,450	$31,770	-$5,682	$122,000	$124,500	$2,494
Two Parents Present	71.1%	72.5%	1.4	92.6%	86.3%	-6.3
Years of Education	12.3	11.9	-0.3	14.9	15.3	0.4
High School Graduation	66.5%	64.8%	-1.7	89.6%	90.7%	1.1
Four-Year College Graduation	22.0%	18.9%	-3.0	43.1%	46.1%	3.0
Home Ownership	70.4%	54.3%	-16.1	90.1%	80.5%	-9.6
B. Child Outcomes in Child Adulthood						
Median HH Income, Age 27	$44,750	$39,380	-$5,374	$53,540	$49,470	-$4,075
Median Indiv. Income, Age 27	$35,760	$35,760	$0	$44,610	$44,690	$77
Mean HH Income Percentile, Age 27	51.6	51.7	0.1	57.2	58.3	1.1
Bottom Quintile of HH Income, Age 27	19.4%	19.6%	0.3	15.1%	15.8%	0.7
Top Quintile of HH Income, Age 27	21.6%	22.1%	0.5	27.9%	31.1%	3.2
Employment, Age 27	82.2%	82.7%	0.5	87.8%	87.3%	-0.5
Marriage, Age 27	27.0%	15.4%	-11.6	25.8%	14.6%	-11.1
Marriage, Age 32	45.6%	36.9%	-8.7	46.9%	39.6%	-7.3
Mortality, Ages 24-27	0.17%	0.22%	0.04	0.23%	0.21%	-0.02
Incarceration, Age 22	0.33%	0.20%	-0.14	0.15%	0.12%	-0.03
Years of Education, Age 27	15.7	15.8	0.2	16.3	16.5	0.2
High School Graduation, Age 27	96.3%	95.2%	-1.1	98.2%	98.0%	-0.2
Four-Year College Graduation, Age 27	49.1%	54.0%	4.9	60.9%	69.2%	8.3
C. Parental Outcomes in Child Adulthood						
Employment, Child Age 27	57.7%	52.1%	-5.6	73.7%	72.0%	-1.7
Marriage, Child Age 27	64.8%	52.9%	-12.0	84.1%	77.2%	-6.9
Mortality, Child Ages 18-27	3.50%	3.08%	-0.42	2.41%	2.12%	-0.28
Number of Children	8,700	14,500	5,800	7,900	15,500	7,600

Notes: This table reports summary statistics for Asian children in the 1978 and 1992 birth cohorts who are claimed as child dependents by parents with positive household income between child ages 13-17. Panel A reports parental characteristics during the child's youth; Panel B reports child outcomes during the child's adulthood; and Panel C reports parental outcomes during the child's adulthood. Columns 1-3 report statistics for children born to families between the 20th and 30th percentiles of the parental income distribution; Columns 4-6 report statistics for children born to families between the 70th and 80th percentiles of the parental income distribution. See Section II for more details and variable definitions. All monetary values are reported in 2023 dollars. All statistics cleared under Census DRB release authorizations CBDRB-FY24-0143 and CBDRB-FY24-0359.

TABLE A.5
Summary Statistics by Birth Cohort and Class for AIAN Children

	AIAN Children at P=25			AIAN Children at P=75		
	1978 Cohort	1992 Cohort	Change	1978 Cohort	1992 Cohort	Change
A. Parental Characteristics	(1)	(2)	(3)	(4)	(5)	(6)
Median HH Income, Child Ages 13-17	$37,380	$31,800	-$5,579	$121,200	$122,500	$1,337
Two Parents Present	45.4%	46.5%	1.1	92.7%	75.8%	-16.9
Years of Education	13.4	13.6	0.1	14.6	14.9	0.3
High School Graduation	79.6%	79.8%	0.3	92.3%	93.2%	0.9
Four-Year College Graduation	8.6%	6.3%	-2.3	25.4%	24.0%	-1.4
Home Ownership	70.7%	55.2%	-15.4	89.6%	82.0%	-7.7
B. Child Outcomes in Child Adulthood						
Median HH Income, Age 27	$21,460	$20,110	-$1,350	$36,190	$39,240	$3,047
Median Indiv. Income, Age 27	$15,360	$17,140	$1,774	$28,130	$32,850	$4,718
Mean HH Income Percentile, Age 27	35.2	35.9	0.6	46.2	51.6	5.5
Bottom Quintile of HH Income, Age 27	35.3%	36.4%	1.1	21.9%	18.6%	-3.3
Top Quintile of HH Income, Age 27	7.7%	7.7%	0.0	14.6%	21.4%	6.8
Employment, Age 27	75.7%	74.2%	-1.5	83.6%	83.3%	-0.3
Marriage, Age 27	25.9%	16.5%	-9.4	33.1%	25.3%	-7.8
Marriage, Age 32	28.6%	21.8%	-6.8	41.0%	38.2%	-2.7
Mortality, Ages 24-27	0.56%	1.31%	0.75	0.53%	0.37%	-0.16
Incarceration, Age 22	2.23%	2.24%	0.01	1.41%	0.89%	-0.52
Years of Education, Age 27	13.5	13.5	0.0	13.8	13.8	0.1
High School Graduation, Age 27	81.6%	90.0%	8.4	83.3%	88.0%	4.7
Four-Year College Graduation, Age 27	10.5%	5.0%	-5.5	16.7%	12.0%	-4.7
C. Parental Outcomes in Child Adulthood						
Employment, Child Age 27	67.9%	59.0%	-8.9	78.2%	72.1%	-6.1
Marriage, Child Age 27	38.5%	25.5%	-13.0	77.3%	66.1%	-11.2
Mortality, Child Ages 18-27	4.95%	6.70%	1.75	2.78%	3.65%	0.87
Number of Children	3,400	4,700	1,300	1,500	2,400	900

Notes: This table reports summary statistics for AIAN children in the 1978 and 1992 birth cohorts who are claimed as child dependents by parents with positive household income between child ages 13-17. Panel A reports parental characteristics during the child's youth; Panel B reports child outcomes during the child's adulthood; and Panel C reports parental outcomes during the child's adulthood. Columns 1-3 report statistics for children born to families between the 20th and 30th percentiles of the parental income distribution; Columns 4-6 report statistics for children born to families between the 70th and 80th percentiles of the parental income distribution. See Section II for more details and variable definitions. All monetary values are reported in 2023 dollars. All statistics cleared under Census DRB release authorizations CBDRB-FY24-0143 and CBDRB-FY24-0359.

TABLE A.6
Mean Child Household Income Rank by Birth Cohort, Race, and Class

	Mean HH Income Rank at P=25		Mean HH Income Rank at P=75	
	White Children	Black Children	White Children	Black Children
	(1)	(2)	(3)	(4)
1978 Cohort	48.4	33.5	59.5	43.9
1979 Cohort	48.1	33.6	59.4	44.1
1980 Cohort	48.0	33.7	59.3	44.1
1981 Cohort	47.3	34.3	59.5	45.2
1982 Cohort	46.9	35.0	59.5	45.7
1983 Cohort	46.8	35.2	59.3	45.8
1984 Cohort	46.4	35.5	59.4	45.8
1985 Cohort	46.3	35.0	59.6	45.9
1986 Cohort	46.2	34.9	59.7	45.6
1987 Cohort	46.2	35.0	59.8	45.6
1988 Cohort	46.0	35.1	59.9	45.8
1989 Cohort	46.0	35.4	59.9	45.9
1990 Cohort	46.0	35.5	60.0	46.1
1991 Cohort	45.9	35.4	60.1	45.7
1992 Cohort	46.1	35.1	60.2	45.3

Notes: This table reports mean household income ranks for white and Black children in the 1978-1992 birth cohorts who are claimed as child dependents by parents with positive household income between child ages 13-17. Columns 1-2 report mean household income ranks for children born to families at the 25th percentile of the national income distribution. Columns 3-4 report mean household income ranks for children born to families at the 75th percentile of the national income distribution. We estimate mean household income ranks using fitted values from a lowess regression on parental income percentiles for each race and birth cohort. See Figure I for additional details on how we construct the estimates of mean household income ranks and Section II for details on the sample construction and variable definitions. All statistics cleared under Census DRB release authorization CBDRB-FY2022-CES010-004.

TABLE A.7
Quintile Transition Matrix for White Children by Birth Cohort

	Parent Household Income Quintile				
	Q1	Q2	Q3	Q4	Q5
A. 1978 Birth Cohort	(1)	(2)	(3)	(4)	(5)
Child HH Income in Q1	24.9%	18.2%	14.0%	11.3%	10.3%
Child HH Income in Q2	24.2%	21.1%	18.5%	15.9%	13.5%
Child HH Income in Q3	19.6%	21.5%	21.5%	20.5%	17.5%
Child HH Income in Q4	17.6%	20.9%	23.3%	24.6%	24.5%
Child HH Income in Q5	13.7%	18.3%	22.7%	27.7%	34.2%
B. 1992 Birth Cohort					
Child HH Income in Q1	29.7%	21.2%	15.9%	11.8%	10.6%
Child HH Income in Q2	23.4%	21.2%	17.9%	14.6%	11.6%
Child HH Income in Q3	19.1%	21.4%	21.7%	20.2%	15.5%
Child HH Income in Q4	15.8%	19.6%	22.6%	24.6%	24.5%
Child HH Income in Q5	11.9%	16.7%	22.0%	28.8%	37.9%

Notes: This table reports summary statistics on intergenerational mobility for white children in the 1978 and 1992 birth cohorts who are claimed as child dependents by parents with positive household income between child ages 13-17. We report the probability that a child is in a given household income quintile conditional on being born to parents in a given household income quintile, where Q1 through Q5 refers to the first through fifth quintiles of the relevant income distribution. See Section II for more details and variable definitions. All statistics cleared under Census DRB release authorization CBDRB-FY2023-CES005-025.

TABLE A.8
Quintile Transition Matrix for Black Children by Birth Cohort

	Parent Household Income Quintile				
	Q1	Q2	Q3	Q4	Q5
A. 1978 Birth Cohort	(1)	(2)	(3)	(4)	(5)
Child HH Income in Q1	39.6%	32.2%	27.7%	23.1%	19.2%
Child HH Income in Q2	32.3%	30.5%	27.6%	25.5%	21.9%
Child HH Income in Q3	17.5%	21.6%	23.2%	24.6%	23.6%
Child HH Income in Q4	7.6%	10.8%	14.3%	16.8%	20.7%
Child HH Income in Q5	3.0%	4.9%	7.2%	10.0%	14.5%
B. 1992 Birth Cohort					
Child HH Income in Q1	33.8%	27.8%	24.8%	21.4%	18.5%
Child HH Income in Q2	35.6%	33.4%	29.6%	25.7%	20.5%
Child HH Income in Q3	18.4%	22.1%	23.3%	23.7%	21.3%
Child HH Income in Q4	9.1%	12.3%	15.7%	19.0%	22.5%
Child HH Income in Q5	3.0%	4.4%	6.5%	10.2%	17.2%

Notes: This table reports summary statistics on intergenerational mobility for Black children in the 1978 and 1992 birth cohorts who are claimed as child dependents by parents with positive household income between child ages 13-17. We report the probability that a child is in a given household income quintile conditional on being born to parents in a given household income quintile, where Q1 through Q5 refers to the first through fifth quintiles of the relevant income distribution. See Section II for more details and variable definitions. All statistics cleared under Census DRB release authorization CBDRB-FY2023-CES005-025.

TABLE A.9
Quintile Transition Matrix for Hispanic Children by Birth Cohort

	Parent Household Income Quintile				
	Q1	Q2	Q3	Q4	Q5
A. 1978 Birth Cohort	(1)	(2)	(3)	(4)	(5)
Child HH Income in Q1	25.9%	21.1%	18.4%	16.0%	14.5%
Child HH Income in Q2	26.5%	23.9%	21.1%	19.2%	16.7%
Child HH Income in Q3	22.4%	23.8%	23.5%	22.7%	20.3%
Child HH Income in Q4	15.8%	18.5%	20.8%	22.0%	22.9%
Child HH Income in Q5	9.4%	12.7%	16.2%	20.1%	25.6%
B. 1992 Birth Cohort					
Child HH Income in Q1	26.6%	20.4%	17.4%	15.2%	14.2%
Child HH Income in Q2	26.2%	23.2%	20.6%	18.8%	15.4%
Child HH Income in Q3	22.0%	24.6%	24.8%	23.3%	18.8%
Child HH Income in Q4	16.2%	19.7%	22.2%	23.4%	23.8%
Child HH Income in Q5	8.9%	12.1%	15.0%	19.2%	27.7%

Notes: This table reports summary statistics on intergenerational mobility for Hispanic children in the 1978 and 1992 birth cohorts who are claimed as child dependents by parents with positive household income between child ages 13-17. We report the probability that a child is in a given household income quintile conditional on being born to parents in a given household income quintile, where Q1 through Q5 refers to the first through fifth quintiles of the relevant income distribution. See Section II for more details and variable definitions. All statistics cleared under Census DRB release authorization CBDRB-FY2023-CES005-025.

TABLE A.10
Quintile Transition Matrix for Asian Children by Birth Cohort

	Parent Household Income Quintile				
	Q1	Q2	Q3	Q4	Q5
A. 1978 Birth Cohort	(1)	(2)	(3)	(4)	(5)
Child HH Income in Q1	21.6%	19.0%	16.4%	15.0%	15.0%
Child HH Income in Q2	20.7%	18.1%	16.6%	15.0%	13.4%
Child HH Income in Q3	18.9%	18.9%	20.0%	19.4%	16.7%
Child HH Income in Q4	19.9%	21.9%	22.9%	23.6%	24.1%
Child HH Income in Q5	19.0%	22.2%	24.1%	27.0%	30.7%
B. 1992 Birth Cohort					
Child HH Income in Q1	21.0%	19.1%	17.5%	16.1%	16.6%
Child HH Income in Q2	19.6%	17.9%	15.7%	14.2%	11.4%
Child HH Income in Q3	19.5%	18.9%	19.4%	17.7%	13.6%
Child HH Income in Q4	20.1%	21.8%	22.7%	22.6%	20.4%
Child HH Income in Q5	19.7%	22.4%	24.8%	29.4%	38.0%

Notes: This table reports summary statistics on intergenerational mobility for Asian children in the 1978 and 1992 birth cohorts who are claimed as child dependents by parents with positive household income between child ages 13-17. We report the probability that a child is in a given household income quintile conditional on being born to parents in a given household income quintile, where Q1 through Q5 refers to the first through fifth quintiles of the relevant income distribution. See Section II for more details and variable definitions. All statistics cleared under Census DRB release authorization CBDRB-FY2023-CES005-025.

TABLE A.11
Quintile Transition Matrix for AIAN Children by Birth Cohort

	Parent Household Income Quintile				
	Q1	Q2	Q3	Q4	Q5
A. 1978 Birth Cohort	(1)	(2)	(3)	(4)	(5)
Child HH Income in Q1	39.6%	33.2%	26.5%	22.3%	18.2%
Child HH Income in Q2	29.1%	27.2%	23.9%	23.5%	20.6%
Child HH Income in Q3	16.1%	19.5%	21.2%	21.3%	18.5%
Child HH Income in Q4	10.1%	12.0%	16.4%	18.4%	20.4%
Child HH Income in Q5	5.1%	8.1%	11.9%	14.4%	22.3%
B. 1992 Birth Cohort					
Child HH Income in Q1	43.6%	34.3%	26.7%	20.2%	16.4%
Child HH Income in Q2	25.4%	24.2%	23.6%	19.3%	15.8%
Child HH Income in Q3	16.3%	19.8%	20.7%	21.2%	18.0%
Child HH Income in Q4	9.7%	13.3%	16.5%	20.0%	21.3%
Child HH Income in Q5	5.0%	8.4%	12.6%	19.3%	28.5%

Notes: This table reports summary statistics on intergenerational mobility for AIAN children in the 1978 and 1992 birth cohorts who are claimed as child dependents by parents with positive household income between child ages 13-17. We report the probability that a child is in a given household income quintile conditional on being born to parents in a given household income quintile, where Q1 through Q5 refers to the first through fifth quintiles of the relevant income distribution. See Section II for more details and variable definitions. All statistics cleared under Census DRB release authorization CBDRB-FY2023-CES005-025.

TABLE A.12
SAT/ACT Summary Statistics by Race, Class, and High School Cohort

	White Children			Black Children			Hispanic Children			Asian Children			All Children		
	Percentile	Count	Share	Percentile	Count	Share	Percentile	Count	Share	Percentile	Count	Share	Percentile	Count	Share
A. Low-Income	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
1998 Cohort	47.0	276	32.2%	22.4	92	27.8%	29.1	52	17.8%	49.7	30	62.0%	38.9	512	30.7%
1999 Cohort	46.9	279	32.2%	22.1	103	28.5%	28.9	57	17.3%	47.8	32	54.2%	38.0	535	30.4%
2000 Cohort	46.9	282	31.7%	21.5	108	28.9%	28.9	61	17.4%	47.2	33	50.9%	37.6	555	30.6%
2001 Cohort	46.6	283	32.2%	21.2	113	30.6%	28.0	64	18.1%	47.1	35	54.6%	37.0	554	30.9%
2002 Cohort	46.5	279	31.6%	21.2	119	31.2%	27.8	68	18.6%	47.8	36	55.9%	36.5	564	30.8%
2003 Cohort	46.4	281	31.3%	21.3	127	31.9%	28.1	76	19.7%	47.7	38	59.2%	36.2	592	31.3%
2004 Cohort	46.4	280	31.8%	21.2	132	32.9%	28.3	81	20.7%	48.3	39	60.3%	36.1	597	31.6%
2005 Cohort	45.2	268	30.5%	20.8	133	32.5%	28.1	82	20.3%	48.6	38	57.4%	35.2	600	31.4%
2007 Cohort	46.1	259	28.9%	21.2	148	32.9%	28.8	95	21.9%	50.1	41	59.3%	35.2	627	31.0%
2009 Cohort	46.3	256	29.5%	21.2	173	37.6%	28.3	121	25.1%	50.1	44	63.5%	34.4	656	31.8%
2011 Cohort	46.5	244	30.1%	21.7	182	41.8%	28.3	152	30.9%	50.9	46	68.7%	34.1	690	34.4%
2013 Cohort	45.7	232	29.8%	22.0	173	44.6%	28.7	177	36.3%	50.4	47	73.0%	34.0	700	36.3%
2015 Cohort	45.9	215	28.8%	22.7	172	44.7%	28.8	194	39.9%	49.9	48	81.0%	33.8	701	36.9%
B. High-Income															
1998 Cohort	53.4	806	60.2%	31.3	57	54.0%	42.3	44	37.6%	57.9	45	90.2%	51.6	1,070	64.1%
1999 Cohort	53.5	861	60.7%	31.6	61	55.1%	42.5	47	36.8%	57.8	48	88.0%	51.8	1,140	64.7%
2000 Cohort	53.6	878	59.7%	31.8	62	55.3%	42.1	50	36.6%	57.9	49	81.7%	51.8	1,182	65.0%
2001 Cohort	53.8	906	62.7%	31.7	64	57.8%	42.4	53	37.8%	59.4	54	87.2%	52.1	1,182	65.9%
2002 Cohort	54.1	923	62.5%	31.9	68	59.7%	42.3	56	38.1%	59.1	57	88.3%	52.1	1,215	66.2%
2003 Cohort	54.2	966	63.7%	32.3	72	61.9%	42.4	62	39.7%	59.9	61	92.0%	52.2	1,295	68.3%
2004 Cohort	54.1	964	64.3%	32.3	76	63.4%	42.5	67	40.7%	59.8	62	89.8%	52.2	1,295	68.5%
2005 Cohort	53.7	941	62.7%	32.9	77	62.4%	42.8	69	39.6%	60.1	63	88.2%	52.2	1,323	69.1%
2007 Cohort	54.2	977	63.0%	33.0	86	61.2%	42.7	84	41.0%	60.8	70	91.1%	52.4	1,399	69.1%
2009 Cohort	55.3	1,039	67.4%	33.8	100	66.6%	42.8	103	43.3%	61.5	77	95.2%	52.9	1,431	69.3%
2011 Cohort	55.8	1,024	69.8%	35.0	108	71.1%	43.0	127	51.6%	63.5	81	96.6%	53.3	1,462	73.0%
2013 Cohort	55.9	984	70.1%	36.0	105	75.4%	44.2	150	61.7%	64.0	82	94.6%	53.5	1,461	75.6%
2015 Cohort	56.2	945	68.9%	36.9	102	77.2%	43.9	163	65.6%	64.1	87	96.1%	53.7	1,438	75.5%

Notes: This table reports SAT/ACT summary statistics by race, class, and high school cohort. Panel A reports summary statistics for children born to families at the 25th percentile of the parental income distribution and Panel B reports summary statistics for children born to families at the 75th percentile of the parental income distribution. Columns 1-3 report the mean test score percentile ranks, the number of test takers (in 1,000s), and the estimated share of test takers for white children; Columns 4-6 report the same set of summary statistics for Black children; Columns 7-9 for Hispanic children; Columns 10-12 for Asian children; and Columns 13-15 for all children. We smooth test score percentiles by estimating the conditional expectation of test score percentiles given parental income using a univariate regression in each race-by-cohort cell. We calculate the share of children taking the SAT/ACT by dividing the number of test takers in a high school cohort by the total number of children in the relevant birth cohort, which we estimate as high school cohort minus 18. See Section II for details on how the test scores and score ranks are constructed. Statistics for AIAN children are not reported due to misalignment of racial group definitions between the datasets used to construct these measures. Statistics are constructed from data reported in Chetty, Deming and Friedman (2023).

TABLE A.13
Mean Child Household Income for Low-Income White Families by Birth Cohort and County

			Mean Household Income Rank at P=25		
Rank	County and State	Largest City	1978 Cohort	1992 Cohort	Change
1	Mecklenburg County, NC	Charlotte	45.4	45.4	0.0
2	Oakland County, MI	Troy	45.0	44.6	-0.4
3	Allegheny County, PA	Pittsburgh	48.1	47.6	-0.6
4	Travis County, TX	Austin	45.8	45.1	-0.7
5	Franklin County, OH	Columbus	43.0	42.2	-0.8
6	King County, WA	Seattle	49.2	48.3	-0.8
7	Fulton County, GA	Atlanta	46.5	45.5	-1.0
8	Hennepin County, MN	Minneapolis	50.0	49.0	-1.0
9	Wayne County, MI	Detroit	44.3	43.2	-1.1
10	Honolulu County, HI	Honolulu	46.7	44.9	-1.8
11	Salt Lake County, UT	Salt Lake City	51.6	49.6	-2.0
12	Dallas County, TX	Dallas	47.1	45.0	-2.0
13	New York County, NY	New York City	47.9	45.8	-2.1
14	Kings County, NY	Brooklyn	49.8	47.7	-2.1
15	Middlesex County, MA	Cambridge	51.5	49.4	-2.1
16	Cuyahoga County, OH	Cleveland	46.8	44.7	-2.1
17	Tarrant County, TX	Arlington	47.8	45.6	-2.3
18	Palm Beach County, FL	West Palm Beach	46.2	43.9	-2.3
19	Santa Clara County, CA	San Jose	48.9	46.6	-2.3
20	Wake County, NC	Raleigh	48.6	46.2	-2.4
21	Sacramento County, CA	Sacramento	47.2	44.7	-2.6
22	Collin County, TX	Plano	50.0	47.4	-2.6
23	Duval County, FL	Jacksonville	44.2	41.6	-2.6
24	Orange County, FL	Orlando	44.8	42.1	-2.7
25	Bexar County, TX	San Antonio	46.7	44.1	-2.7
26	Harris County, TX	Houston	48.7	46.0	-2.7
27	Cook County, IL	Chicago	50.9	47.9	-3.0
28	Miami-Dade County, FL	Miami	46.3	43.2	-3.1
29	St. Louis County, MO	Florissant	48.8	45.6	-3.2
30	Alameda County, CA	Oakland	48.6	45.3	-3.3
31	Broward County, FL	Fort Lauderdale	46.9	43.6	-3.3
32	Contra Costa County, CA	Concord	48.9	45.4	-3.5
33	Queens County, NY	Queens	52.0	48.5	-3.5
34	Pima County, AZ	Tucson	46.1	42.5	-3.6
35	Nassau County, NY	Hempstead	54.6	50.8	-3.8
36	Hillsborough County, FL	Tampa	45.1	41.2	-3.9
37	Suffolk County, NY	Brentwood	51.5	47.6	-3.9
38	San Diego County, CA	San Diego	49.1	44.7	-4.5
39	Maricopa County, AZ	Phoenix	48.9	44.4	-4.5
40	Riverside County, CA	Riverside	48.2	43.7	-4.5
41	Los Angeles County, CA	Los Angeles	48.4	43.7	-4.8
42	Montgomery County, MD	Germantown	52.2	47.2	-5.0
43	Clark County, NV	Las Vegas	47.6	42.5	-5.1
44	Bronx County, NY	Bronx	51.1	45.9	-5.1
45	Westchester County, NY	Yonkers	54.7	49.5	-5.2
46	Orange County, CA	Anaheim	49.2	43.9	-5.2
47	Fresno County, CA	Fresno	46.9	41.6	-5.3
48	San Bernardino County, CA	San Bernardino	48.6	43.2	-5.4
49	Fairfax County, VA	Centreville	53.2	47.0	-6.2
50	Philadelphia County, PA	Philadelphia	48.8	42.4	-6.4

Notes: This table reports the change in mean household income rank by county for white children born to families at the 25th percentile of the national income distribution. We report statistics for the 50 largest counties by population in the 2020 Census, sorted by the change in mean household income rank between the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct county-by-race-by-class-by-cohort estimates of economic mobility and Section II for details on the sample construction and variable definitions. All statistics cleared under Census DRB release authorization CBDRB-FY23-0375.

TABLE A.14
Mean Child Household Income for Low-Income Black Families by Birth Cohort and County

			Mean Household Income Rank at P=25		
Rank	County and State	Largest City	1978 Cohort	1992 Cohort	Change
1	Travis County, TX	Austin	31.1	37.7	6.7
2	Collin County, TX	Plano	34.6	40.6	6.1
3	Santa Clara County, CA	San Jose	36.4	41.3	4.9
4	King County, WA	Seattle	34.1	38.6	4.5
5	Salt Lake County, UT	Salt Lake City	36.8	40.5	3.7
6	Franklin County, OH	Columbus	31.8	35.5	3.7
7	Mecklenburg County, NC	Charlotte	31.8	35.5	3.7
8	Hennepin County, MN	Minneapolis	33.7	37.0	3.3
9	Oakland County, MI	Troy	33.1	36.3	3.2
10	Allegheny County, PA	Pittsburgh	31.3	34.5	3.2
11	Dallas County, TX	Dallas	33.5	36.7	3.2
12	Tarrant County, TX	Arlington	33.4	36.6	3.2
13	Cuyahoga County, OH	Cleveland	31.3	34.3	3.0
14	Wayne County, MI	Detroit	31.9	34.5	2.7
15	St. Louis County, MO	Florissant	33.1	35.7	2.7
16	Wake County, NC	Raleigh	33.6	36.0	2.4
17	Harris County, TX	Houston	34.4	36.6	2.2
18	Cook County, IL	Chicago	31.8	33.8	2.0
19	Alameda County, CA	Oakland	34.4	36.1	1.7
20	Queens County, NY	Queens	37.6	39.2	1.5
21	Suffolk County, NY	Brentwood	36.9	38.3	1.4
22	Bexar County, TX	San Antonio	35.2	36.6	1.4
23	Middlesex County, MA	Cambridge	43.0	44.3	1.3
24	Nassau County, NY	Hempstead	38.2	39.3	1.0
25	Westchester County, NY	Yonkers	37.5	38.5	1.0
26	Orange County, FL	Orlando	34.2	35.1	1.0
27	Pima County, AZ	Tucson	35.0	35.9	0.9
28	New York County, NY	New York City	34.8	35.0	0.2
29	Contra Costa County, CA	Concord	35.0	35.2	0.2
30	Duval County, FL	Jacksonville	34.1	34.2	0.1
31	Philadelphia County, PA	Philadelphia	34.5	34.3	-0.3
32	Bronx County, NY	Bronx	37.0	36.7	-0.3
33	Maricopa County, AZ	Phoenix	36.5	36.1	-0.4
34	San Bernardino County, CA	San Bernardino	35.5	35.0	-0.5
35	Fulton County, GA	Atlanta	33.6	33.1	-0.5
36	Kings County, NY	Brooklyn	38.0	37.5	-0.5
37	Honolulu County, HI	Honolulu	39.5	38.8	-0.7
38	Los Angeles County, CA	Los Angeles	34.8	33.6	-1.2
39	Palm Beach County, FL	West Palm Beach	36.7	35.4	-1.3
40	Hillsborough County, FL	Tampa	35.2	33.9	-1.3
41	Riverside County, CA	Riverside	36.3	34.9	-1.5
42	Sacramento County, CA	Sacramento	34.7	33.2	-1.5
43	Fresno County, CA	Fresno	33.1	31.3	-1.7
44	San Diego County, CA	San Diego	37.8	35.8	-2.0
45	Orange County, CA	Anaheim	38.9	36.9	-2.1
46	Broward County, FL	Fort Lauderdale	38.1	36.0	-2.1
47	Clark County, NV	Las Vegas	35.6	33.5	-2.1
48	Montgomery County, MD	Germantown	42.9	40.1	-2.8
49	Fairfax County, VA	Centreville	43.5	40.6	-2.9
50	Miami-Dade County, FL	Miami	38.1	34.8	-3.3

Notes: This table reports the change in mean household income rank by county for Black children born to families at the 25th percentile of the national income distribution. We report statistics for the 50 largest counties by population in the 2020 Census, sorted by the change in mean household income rank between the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct county-by-race-by-class-by-cohort estimates of economic mobility and Section II for details on the sample construction and variable definitions. All statistics cleared under Census DRB release authorization CBDRB-FY23-0375.

TABLE A.15
Mean Child Household Income for High-Income White Families by Birth Cohort and County

			Mean Household Income Rank at P=75		
Rank	County and State	Largest City	1978 Cohort	1992 Cohort	Change
1	Franklin County, OH	Columbus	57.1	59.9	2.8
2	Oakland County, MI	Troy	56.8	59.4	2.5
3	Allegheny County, PA	Pittsburgh	60.1	62.6	2.5
4	Cuyahoga County, OH	Cleveland	58.5	60.7	2.2
5	Fulton County, GA	Atlanta	58.2	60.3	2.2
6	King County, WA	Seattle	57.8	59.9	2.2
7	Hennepin County, MN	Minneapolis	60.2	62.3	2.1
8	Santa Clara County, CA	San Jose	56.6	58.7	2.1
9	Travis County, TX	Austin	56.5	58.6	2.1
10	Mecklenburg County, NC	Charlotte	58.0	60.0	2.0
11	Salt Lake County, UT	Salt Lake City	59.9	61.8	2.0
12	Wayne County, MI	Detroit	56.4	58.0	1.6
13	Middlesex County, MA	Cambridge	60.1	61.5	1.5
14	New York County, NY	New York City	56.3	57.5	1.2
15	Dallas County, TX	Dallas	59.0	60.2	1.2
16	Collin County, TX	Plano	60.5	61.6	1.1
17	Wake County, NC	Raleigh	59.0	60.0	1.0
18	St. Louis County, MO	Florissant	60.1	61.1	1.0
19	Tarrant County, TX	Arlington	59.5	60.3	0.9
20	Harris County, TX	Houston	60.1	60.9	0.8
21	Bexar County, TX	San Antonio	58.2	58.6	0.4
22	Contra Costa County, CA	Concord	57.9	58.2	0.3
23	Alameda County, CA	Oakland	57.2	56.8	-0.3
24	Orange County, FL	Orlando	56.1	55.7	-0.4
25	Duval County, FL	Jacksonville	56.8	56.3	-0.5
26	Honolulu County, HI	Honolulu	55.3	54.8	-0.6
27	Cook County, IL	Chicago	61.3	60.7	-0.6
28	Nassau County, NY	Hempstead	64.2	63.5	-0.7
29	Westchester County, NY	Yonkers	62.7	61.8	-0.9
30	Suffolk County, NY	Brentwood	61.4	60.4	-1.0
31	Palm Beach County, FL	West Palm Beach	56.6	55.5	-1.1
32	Maricopa County, AZ	Phoenix	58.5	57.4	-1.1
33	Pima County, AZ	Tucson	56.2	54.9	-1.2
34	Queens County, NY	Queens	61.0	59.6	-1.4
35	San Diego County, CA	San Diego	57.3	55.8	-1.5
36	Sacramento County, CA	Sacramento	56.8	55.2	-1.6
37	Montgomery County, MD	Germantown	60.9	59.3	-1.6
38	Kings County, NY	Brooklyn	60.0	58.2	-1.7
39	Broward County, FL	Fort Lauderdale	57.1	55.1	-1.9
40	Orange County, CA	Anaheim	57.8	55.8	-2.0
41	Fresno County, CA	Fresno	57.9	55.8	-2.1
42	Riverside County, CA	Riverside	57.0	54.8	-2.2
43	Los Angeles County, CA	Los Angeles	56.3	54.0	-2.3
44	Hillsborough County, FL	Tampa	57.5	55.2	-2.3
45	Clark County, NV	Las Vegas	56.9	54.2	-2.8
46	Miami-Dade County, FL	Miami	56.0	53.3	-2.8
47	Fairfax County, VA	Centreville	62.8	59.9	-3.0
48	San Bernardino County, CA	San Bernardino	58.1	55.1	-3.0
49	Philadelphia County, PA	Philadelphia	60.9	57.6	-3.3
50	Bronx County, NY	Bronx	62.0	57.2	-4.8

Notes: This table reports the change in mean household income rank by county for white children born to families at the 75th percentile of the national income distribution. We report statistics for the 50 largest counties by population in the 2020 Census, sorted by the change in mean household income rank between the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct county-by-race-by-class-by-cohort estimates of economic mobility and Section II for details on the sample construction and variable definitions. All statistics cleared under Census DRB release authorization CBDRB-FY23-0375.

TABLE A.16

Mean Child Household Income for High-Income Black Families by Birth Cohort and County

Rank	County and State	Largest City	Mean Household Income Rank at P=75		
			1978 Cohort	1992 Cohort	Change
1	Travis County, TX	Austin	43.0	48.7	5.7
2	King County, WA	Seattle	43.2	48.0	4.8
3	Allegheny County, PA	Pittsburgh	43.0	47.4	4.4
4	Oakland County, MI	Troy	41.9	46.1	4.2
5	Salt Lake County, UT	Salt Lake City	47.8	51.8	4.1
6	Wayne County, MI	Detroit	40.2	44.2	4.0
7	Cuyahoga County, OH	Cleveland	41.2	45.2	4.0
8	Santa Clara County, CA	San Jose	45.4	49.0	3.7
9	Franklin County, OH	Columbus	43.7	47.3	3.6
10	Hennepin County, MN	Minneapolis	44.7	48.2	3.5
11	Pima County, AZ	Tucson	44.2	47.0	2.8
12	St. Louis County, MO	Florissant	43.4	46.2	2.8
13	Mecklenburg County, NC	Charlotte	43.8	46.5	2.7
14	Cook County, IL	Chicago	41.6	43.9	2.3
15	Dallas County, TX	Dallas	45.1	47.2	2.1
16	Alameda County, CA	Oakland	43.0	45.0	2.0
17	Queens County, NY	Queens	45.9	47.8	2.0
18	Tarrant County, TX	Arlington	45.3	47.2	1.9
19	Nassau County, NY	Hempstead	47.0	48.9	1.9
20	Orange County, FL	Orlando	43.7	45.4	1.8
21	Middlesex County, MA	Cambridge	51.6	53.2	1.6
22	Harris County, TX	Houston	45.2	46.5	1.3
23	Suffolk County, NY	Brentwood	46.9	48.2	1.3
24	Wake County, NC	Raleigh	46.3	47.5	1.2
25	Duval County, FL	Jacksonville	44.5	45.4	0.9
26	Collin County, TX	Plano	49.0	49.8	0.8
27	Bexar County, TX	San Antonio	46.8	47.4	0.5
28	Fulton County, GA	Atlanta	43.0	43.5	0.5
29	New York County, NY	New York City	43.2	43.7	0.5
30	Honolulu County, HI	Honolulu	49.0	49.4	0.3
31	Hillsborough County, FL	Tampa	44.4	44.8	0.3
32	Palm Beach County, FL	West Palm Beach	44.0	44.3	0.3
33	Kings County, NY	Brooklyn	45.9	46.2	0.3
34	San Bernardino County, CA	San Bernardino	44.2	44.0	-0.2
35	Maricopa County, AZ	Phoenix	47.2	46.8	-0.3
36	Westchester County, NY	Yonkers	48.2	47.9	-0.4
37	Orange County, CA	Anaheim	47.0	46.2	-0.8
38	Contra Costa County, CA	Concord	45.0	44.2	-0.8
39	Los Angeles County, CA	Los Angeles	43.4	42.4	-1.0
40	Philadelphia County, PA	Philadelphia	45.2	44.2	-1.0
41	Bronx County, NY	Bronx	46.3	45.2	-1.0
42	Broward County, FL	Fort Lauderdale	46.0	44.6	-1.4
43	San Diego County, CA	San Diego	46.6	45.0	-1.6
44	Sacramento County, CA	Sacramento	44.7	43.1	-1.6
45	Riverside County, CA	Riverside	45.1	43.2	-1.9
46	Miami-Dade County, FL	Miami	45.1	42.9	-2.3
47	Montgomery County, MD	Germantown	51.2	48.7	-2.5
48	Fairfax County, VA	Centreville	53.3	50.5	-2.8
49	Fresno County, CA	Fresno	44.5	41.2	-3.3
50	Clark County, NV	Las Vegas	46.3	42.6	-3.7

Notes: This table reports the change in mean household income rank by county for Black children born to families at the 75th percentile of the national income distribution. We report statistics for the 50 largest counties by population in the 2020 Census, sorted by the change in mean household income rank between the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct county-by-race-by-class-by-cohort estimates of economic mobility and Section II for details on the sample construction and variable definitions. All statistics cleared under Census DRB release authorization CBDRB-FY23-0375.

TABLE A.17
Mean Child Household Income for All Low-Income Children by Birth Cohort and County

			Mean Household Income Rank at P=25		
Rank	County and State	Largest City	1978 Cohort	1992 Cohort	Change
1	Travis County, TX	Austin	41.3	44.0	2.7
2	Santa Clara County, CA	San Jose	47.4	48.9	1.5
3	Mecklenburg County, NC	Charlotte	38.4	39.8	1.4
4	Dallas County, TX	Dallas	42.3	43.3	1.0
5	Bexar County, TX	San Antonio	42.4	42.9	0.5
6	Honolulu County, HI	Honolulu	46.3	46.6	0.3
7	Harris County, TX	Houston	44.6	44.8	0.2
8	Queens County, NY	Queens	46.1	46.0	-0.1
9	King County, WA	Seattle	47.4	47.3	-0.2
10	Tarrant County, TX	Arlington	44.2	44.0	-0.2
11	Wayne County, MI	Detroit	37.8	37.6	-0.3
12	Bronx County, NY	Bronx	41.2	40.9	-0.3
13	Kings County, NY	Brooklyn	42.9	42.5	-0.5
14	Alameda County, CA	Oakland	45.9	45.4	-0.5
15	Franklin County, OH	Columbus	40.0	39.5	-0.5
16	Fulton County, GA	Atlanta	36.6	36.1	-0.5
17	New York County, NY	New York City	43.2	42.4	-0.8
18	Wake County, NC	Raleigh	42.3	41.2	-1.1
19	Collin County, TX	Plano	47.7	46.4	-1.3
20	Cuyahoga County, OH	Cleveland	40.3	38.9	-1.4
21	Cook County, IL	Chicago	42.1	40.6	-1.5
22	Middlesex County, MA	Cambridge	50.3	48.6	-1.7
23	Oakland County, MI	Troy	43.4	41.6	-1.8
24	Salt Lake County, UT	Salt Lake City	50.2	48.4	-1.8
25	Allegheny County, PA	Pittsburgh	44.4	42.6	-1.8
26	Orange County, FL	Orlando	41.9	40.0	-1.8
27	Pima County, AZ	Tucson	43.3	41.3	-1.9
28	Palm Beach County, FL	West Palm Beach	43.0	41.0	-2.0
29	Contra Costa County, CA	Concord	46.9	44.7	-2.3
30	Duval County, FL	Jacksonville	40.7	38.3	-2.3
31	Fresno County, CA	Fresno	43.8	41.0	-2.8
32	Hennepin County, MN	Minneapolis	46.1	43.2	-2.9
33	St. Louis County, MO	Florissant	42.8	39.9	-2.9
34	Suffolk County, NY	Brentwood	48.6	45.6	-3.0
35	Sacramento County, CA	Sacramento	45.5	42.5	-3.0
36	Philadelphia County, PA	Philadelphia	40.1	37.1	-3.0
37	Miami-Dade County, FL	Miami	45.2	42.2	-3.1
38	Nassau County, NY	Hempstead	49.9	46.8	-3.1
39	Hillsborough County, FL	Tampa	42.9	39.7	-3.2
40	Los Angeles County, CA	Los Angeles	46.3	43.1	-3.2
41	Maricopa County, AZ	Phoenix	46.3	43.0	-3.3
42	Broward County, FL	Fort Lauderdale	44.1	40.8	-3.3
43	Orange County, CA	Anaheim	48.7	45.3	-3.3
44	Westchester County, NY	Yonkers	47.8	44.4	-3.4
45	Riverside County, CA	Riverside	46.3	42.9	-3.4
46	San Bernardino County, CA	San Bernardino	46.2	42.7	-3.5
47	San Diego County, CA	San Diego	47.5	44.0	-3.5
48	Clark County, NV	Las Vegas	45.7	41.9	-3.8
49	Montgomery County, MD	Germantown	50.1	45.9	-4.2
50	Fairfax County, VA	Centreville	52.4	47.6	-4.8

Notes: This table reports the change in mean household income rank by county for all children (pooling racial groups) born to families at the 25th percentile of the national income distribution. We report statistics for the 50 largest counties by population in the 2020 Census, sorted by the change in mean household income rank between the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct county-by-race-by-class-by-cohort estimates of economic mobility and Section II for details on the sample construction and variable definitions. All statistics cleared under Census DRB release authorizations CBDRB-FY23-0375 and CBDRB-FY24-0143.

TABLE A.18
Mean Child Household Income for All High-Income Families by Birth Cohort and County

			Mean Household Income Rank at P=75		
Rank	County and State	Largest City	1978 Cohort	1992 Cohort	Change
1	Santa Clara County, CA	San Jose	55.5	58.1	2.6
2	Franklin County, OH	Columbus	56.0	58.5	2.4
3	Fulton County, GA	Atlanta	54.0	56.4	2.4
4	Allegheny County, PA	Pittsburgh	59.2	61.5	2.3
5	Travis County, TX	Austin	54.8	56.9	2.1
6	King County, WA	Seattle	57.0	59.2	2.1
7	Oakland County, MI	Troy	55.7	57.8	2.1
8	Cuyahoga County, OH	Cleveland	56.5	58.4	2.0
9	Salt Lake County, UT	Salt Lake City	59.4	61.1	1.7
10	Mecklenburg County, NC	Charlotte	56.1	57.5	1.4
11	Wayne County, MI	Detroit	53.0	54.4	1.4
12	Hennepin County, MN	Minneapolis	59.6	60.9	1.4
13	Middlesex County, MA	Cambridge	59.7	60.9	1.2
14	Alameda County, CA	Oakland	55.5	56.1	0.6
15	St. Louis County, MO	Florissant	58.6	59.2	0.6
16	Wake County, NC	Raleigh	57.7	58.2	0.5
17	Bexar County, TX	San Antonio	55.2	55.7	0.4
18	Collin County, TX	Plano	59.9	60.2	0.4
19	Honolulu County, HI	Honolulu	54.7	55.0	0.3
20	Harris County, TX	Houston	57.9	58.1	0.2
21	New York County, NY	New York City	52.9	53.0	0.1
22	Tarrant County, TX	Arlington	58.2	58.3	0.1
23	Dallas County, TX	Dallas	57.2	57.1	-0.1
24	Contra Costa County, CA	Concord	56.7	56.4	-0.3
25	Duval County, FL	Jacksonville	55.2	54.1	-1.2
26	Cook County, IL	Chicago	58.0	56.8	-1.2
27	Orange County, FL	Orlando	54.7	53.4	-1.3
28	Suffolk County, NY	Brentwood	60.3	58.9	-1.4
29	Queens County, NY	Queens	55.9	54.4	-1.5
30	Westchester County, NY	Yonkers	60.7	59.1	-1.5
31	Nassau County, NY	Hempstead	62.4	60.8	-1.6
32	Maricopa County, AZ	Phoenix	57.5	55.8	-1.7
33	Pima County, AZ	Tucson	55.1	53.3	-1.8
34	Kings County, NY	Brooklyn	54.2	52.3	-1.9
35	Palm Beach County, FL	West Palm Beach	55.6	53.7	-1.9
36	Sacramento County, CA	Sacramento	55.7	53.3	-2.4
37	San Diego County, CA	San Diego	56.2	53.9	-2.4
38	Montgomery County, MD	Germantown	60.0	57.4	-2.5
39	Orange County, CA	Anaheim	56.8	54.3	-2.5
40	Los Angeles County, CA	Los Angeles	54.1	51.4	-2.7
41	Hillsborough County, FL	Tampa	56.4	53.5	-2.9
42	Riverside County, CA	Riverside	55.6	52.5	-3.1
43	Broward County, FL	Fort Lauderdale	55.8	52.6	-3.2
44	San Bernardino County, CA	San Bernardino	55.8	52.4	-3.5
45	Fairfax County, VA	Centreville	62.1	58.6	-3.5
46	Clark County, NV	Las Vegas	56.1	52.4	-3.6
47	Bronx County, NY	Bronx	52.4	48.8	-3.6
48	Fresno County, CA	Fresno	56.1	52.4	-3.7
49	Miami-Dade County, FL	Miami	55.2	51.1	-4.0
50	Philadelphia County, PA	Philadelphia	57.6	52.8	-4.9

Notes: This table reports the change in mean household income rank by county for all children (pooling racial groups) born to families at the 75th percentile of the national income distribution. We report statistics for the 50 largest counties by population in the 2020 Census, sorted by the change in mean household income rank between the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct county-by-race-by-class-by-cohort estimates of economic mobility and Section II for details on the sample construction and variable definitions. All statistics cleared under Census DRB release authorizations CBDRB-FY23-0375 and CBDRB-FY24-0143.

TABLE A.19

County-Level Changes in Children's Household Income in Adulthood versus Changes in Parental Employment With Additional Controls

	△ Child Household Income Rank			
	(1)	(2)	(3)	(4)
△ Parental Emp. Rate	0.376	0.399	0.308	0.293
	(0.009)	(0.010)	(0.026)	(0.026)
Child Inc. in 1978		X		X
Par. Inc. x Race FE			X	X
Number of Counties	1,980	1,980	1,980	1,980

Notes: This table reports OLS regression estimates of changes in children's household income ranks in adulthood on changes in parental employment rates in the child's adulthood at the county-by-race-by-class level. Column 1 reports estimates of county-level changes in children's household income ranks in adulthood on county-level changes in parental employment rates, as in our baseline specification in Figure VIIa. Column 2 reports estimates that controls for children's household income rank in the 1978 birth cohort. Column 3 reports estimates that control for race-by-parental income percentile fixed effects. Column 4 reports estimates that control for both children's household income rank in the 1978 birth cohort and race-by-parental income percentile fixed effects. We calculate county-level changes in children's household income ranks in adulthood and parental employment rates using the smoothing procedure described in Appendix A. All specifications are weighted by the number of children in each county-by-race-by-class cell. We restrict to counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. Standard errors are reported in parentheses. See Section II for details on the sample construction and variable definitions. All statistics cleared under Census DRB release authorizations CBDRB-FY2023-CES005-025, CBDRB-FY23-0375, and CBDRB-FY24-0143.

TABLE A.20

County-Level Changes in Children's Outcomes in Adulthood versus Changes in Parental Outcomes

	△ Child HH Income Rank			△ Child Marriage Rate		
A. White Children at P=25	(1)	(2)	(3)	(4)	(5)	(6)
△ Parental Emp. Rate	0.321		0.287	0.126		0.096
	(0.017)		(0.017)	(0.019)		(0.019)
△ Parental Marriage Rate		0.198	0.164		0.155	0.143
		(0.014)	(0.013)		(0.014)	(0.014)
Number of Counties	1,800	1,800	1,800	1,800	1,800	1,800
B. Black Children at P=25						
△ Parental Emp. Rate	0.271		0.256	0.114		0.084
	(0.026)		(0.026)	(0.021)		(0.019)
△ Parental Marriage Rate		0.160	0.113		0.236	0.221
		(0.033)	(0.031)		(0.023)	(0.023)
Number of Counties	550	550	550	550	550	550
C. White Children at P=75						
△ Parental Emp. Rate	0.322		0.171	0.177		0.026
	(0.024)		(0.022)	(0.029)		(0.028)
△ Parental Marriage Rate		0.471	0.423		0.427	0.420
		(0.019)	(0.020)		(0.024)	(0.025)
Number of Counties	1,700	1,700	1,700	1,700	1,700	1,700

Notes: This table reports OLS regression estimates of changes in children's outcomes in adulthood on changes in parental employment rates and parental marriage rates in the child's adulthood at the county level, separately for each race-by-class group. Column 1 reports estimates of county-level changes in group-specific children's household income ranks in adulthood on county-level changes in parental employment rates in the same group. Column 2 reports estimates of county-level changes in group-specific children's household income ranks in adulthood on county-level changes in parental marriage rates in the same group. Column 3 reports estimates of county-level changes in group-specific children's household income ranks in adulthood on county-level changes in both parental employment rates and parental marriage rates in the same group. Columns 4-6 report the same set of estimates using county-level changes in group-specific child marriage rates as the outcome. We calculate county-level changes in group-specific outcomes using the smoothing procedure described in Appendix A. All specifications are weighted by the number of children in each race-by-class cell. We restrict to counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. Standard errors are reported in parentheses. See Section II for details on the sample construction and variable definitions. All statistics cleared under Census DRB release authorization CBDRB-FY24-0143.

TABLE A.21
County-Level Changes in Children's Household Income in Adulthood versus Changes in Same- and Different-Group Parental Employment Rates

	Δ Child Household Income Rank			
	White Children at P=25	White Children at P=25	Black Children at P=25	White Children at P=75
	(1)	(2)	(3)	(4)
△ White Parental Emp. Rate at P=25	0.292	0.223	0.072	0.266
	(0.032)	(0.017)	(0.032)	(0.019)
△ Black Parental Emp. Rate at P=25	0.022		0.269	
	(0.021)		(0.025)	
△ White Parental Emp. Rate at P=75		0.092		0.232
		(0.021)		(0.024)
Number of Counties	500	1,600	500	1,600

Notes: This table reports OLS regression estimates of changes in group-specific children's household income ranks in adulthood on changes in same- and different-group parental employment rates in the child's adulthood at the county level, separately for each race-by-class group. Column 1 reports estimates of county-level changes in children's household income ranks in adulthood for white children born to low-income families on county-level changes in employment rates among low-income white and low-income Black parents. Column 2 reports estimates of county-level changes in children's household income ranks in adulthood for white children born to low-income families on county-level changes in employment rates among low-income and high-income white parents. Column 3 reports estimates of county-level changes in children's household income ranks in adulthood for Black children born to low-income families on county-level changes in employment rates among low-income white and low-income Black parents. Column 4 reports estimates of county-level changes in children's household income ranks in adulthood for white children born to high-income families on county-level changes in employment rates among low-income and high-income white parents. We calculate county-level changes in group-specific children's household income ranks in adulthood and group-specific parental employment rates using the smoothing procedure described in Appendix A. All specifications are weighted by the number of children in each race-by-class cell. We restrict to counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income). Standard errors are reported in parentheses. See Section II for details on the sample construction and variable definitions. All statistics cleared under Census DRB release authorization CBDRB-FY2023-CES005-025.

TABLE A.22
Out-of-Sample Fit When Estimating Parental Employment Rates

	White Children at P=25	Black Children at P=25	White Children at P=75	Black Children at P=75
	(1)	(2)	(3)	(4)
$\frac{\text{RMSE of Baseline Estimates}}{\text{RMSE of Flexible Parameterization}}$	0.9969	1.0001	0.9998	0.9825

Notes: This table reports the out-of-sample fit of our baseline estimates of parental employment rates in a child's adulthood versus estimates using a more flexible parameterization approach. We assign each child to one of two random samples and use the other sample to evaluate predictive performance. In this holdout sample, we construct two different predictions of parental employment rates at the county-by-race-by-class-by-cohort-level. The first prediction is based on our baseline approach, described in Appendix A, and uses the relationship between parental employment rates and parental income for each race-by-class group at the national level to generate estimates at the county level. The second, more flexible approach directly estimates a lowess regression of parental employment rates in the child's adulthood on parental income percentiles in each county-by-race-by-cohort cell. We compute the root mean square error (RMSE) from OLS regressions, where we regress the share of parents employed in each county-by-race-by-class-by-cohort cell in the holdout sample on the predicted employment rate, separately for each approach. We then divide the RMSE calculated using our baseline approach by the RMSE calculated using the more flexible approach. A ratio smaller than one suggests that our baseline approach has greater predictive power than the more flexible approach. Columns 1-2 report the ratio for white and Black children, respectively, born to families at the 25th percentile of the parental income distribution. Columns 3-4 report the ratio for white and Black children, respectively, born to families at the 75th percentile of the parental income distribution. In all specifications, we restrict to the 100 most populous counties within each race group. All statistics cleared under Census DRB release authorization CBDRB-FY24-0143.

TABLE A.23

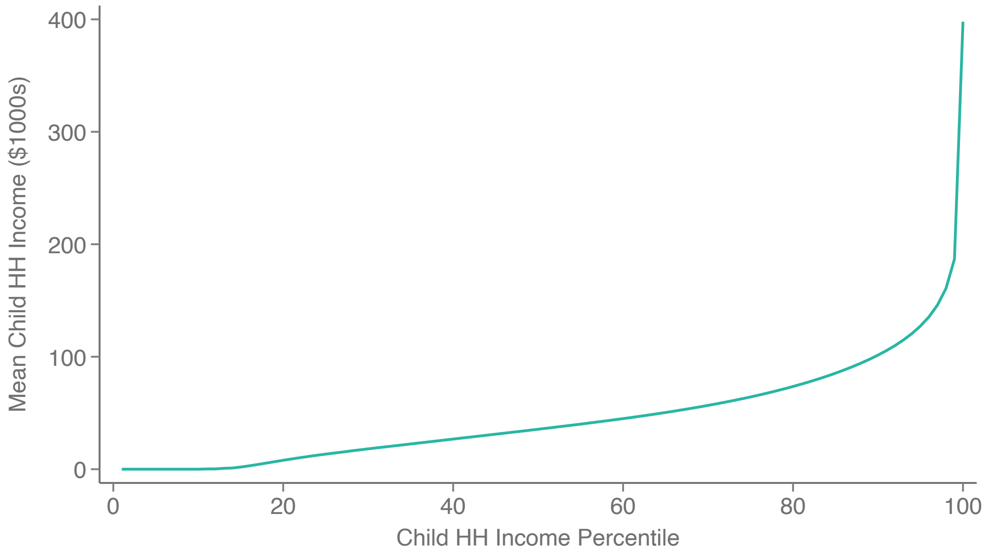
Child and Parental Outcomes by Birth Cohort, Race, and Class Based on a Linear Cohort Trend

	Outcomes at P=25			Outcomes at P=75		
	1978 Cohort	1992 Cohort	Change	1978 Cohort	1992 Cohort	Change
A. White Children	(1)	(2)	(3)	(4)	(5)	(6)
Household Income Percentile, Age 27	47.9	45.5	-2.4	59.3	60.1	0.8
Parental Employment, Child Age 27	64.9%	53.4%	-11.5	78.5%	75.4%	-3.2
Parental Mortality, Child Ages 18-27	4.15%	5.91%	1.8	2.41%	2.63%	0.2
Parental Marriage, Child Age 27	53.9%	33.7%	-20.2	84.3%	75.9%	-8.4
B. Black Children						
Household Income Percentile, Age 27	33.9	35.7	1.8	44.6	46.2	1.6
Parental Employment, Child Age 27	72.8%	68.9%	-3.9	76.7%	75.8%	-0.9
Parental Mortality, Child Ages 18-27	4.89%	4.87%	-0.0	3.87%	3.25%	-0.6
Parental Marriage, Child Age 27	21.0%	11.9%	-9.0	67.7%	57.9%	-9.9
C. Asian Children						
Household Income Percentile, Age 27	51.5	50.7	-0.7	56.9	57.1	0.3
Parental Employment, Child Age 27	56.7%	50.2%	-6.5	73.4%	71.1%	-2.3
Parental Mortality, Child Ages 18-27	3.19%	3.17%	-0.0	2.28%	2.14%	-0.1
Parental Marriage, Child Age 27	64.7%	51.9%	-12.9	83.9%	79.4%	-4.6
D. Hispanic Children						
Household Income Percentile, Age 27	44.7	44.8	0.1	53.2	53.4	0.1
Parental Employment, Child Age 27	66.8%	60.5%	-6.2	76.2%	76.0%	-0.2
Parental Mortality, Child Ages 18-27	3.01%	3.29%	0.3	2.29%	2.03%	-0.3
Parental Marriage, Child Age 27	49.0%	30.1%	-18.8	79.1%	71.5%	-7.6
E. AIAN Children						
Household Income Percentile, Age 27	35.9	35.4	-0.6	47.9	51.0	3.0
Parental Employment, Child Age 27	67.2%	58.7%	-8.5	76.1%	72.4%	-3.7
Parental Mortality, Child Ages 18-27	4.76%	6.52%	1.8	3.01%	3.59%	0.6
Parental Marriage, Child Age 27	39.7%	22.3%	-17.3	77.7%	67.9%	-9.8

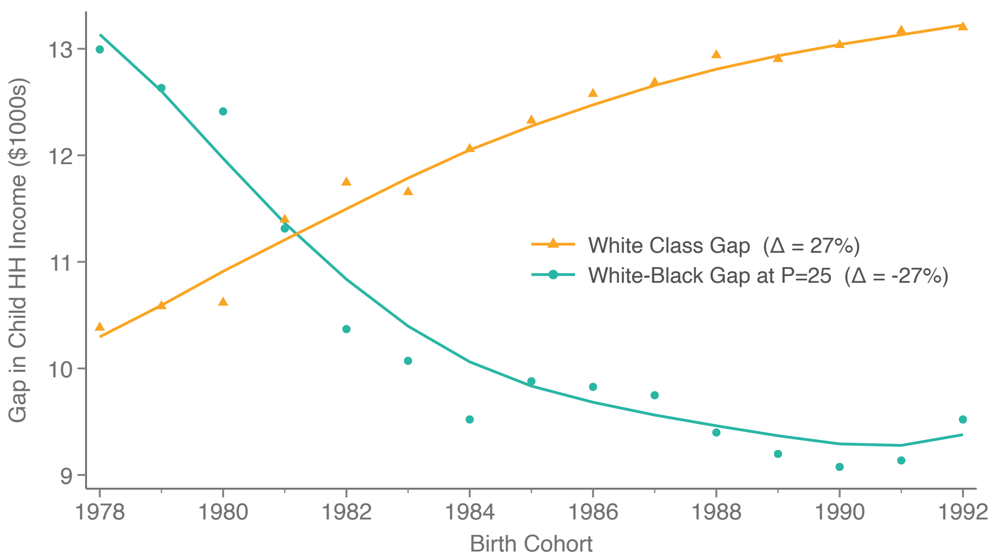
Notes: This table reports OLS regression estimates of the change in mean child and parental outcomes based on a linear cohort trend. Columns 1-2 report estimated outcomes for families at the 25th percentile of the national income distribution in the 1978 and 1992 cohort, respectively; Column 3 reports estimates of the change in outcomes for these families; Columns 4-5 report estimated outcomes for families at the 75th percentile of the national income distribution in the 1978 and 1992 cohort, respectively; and Column 6 reports estimates of the change in outcomes for these families. The estimates are obtained by regressing each outcome variable on birth cohort, separately for each race and class group. See Section II for details on the sample construction and variable definitions. All statistics cleared under Census DRB release authorizations CBDRBFY2022-CES010-004, CBDRB-FY2023-CES005-025, CBDRB-FY24-0143, CBDRB-FY24-0359.

FIGURE A.1
Mapping Between Dollars and Percentiles for Child Household Income

A. Mapping Between Dollars and Percentiles

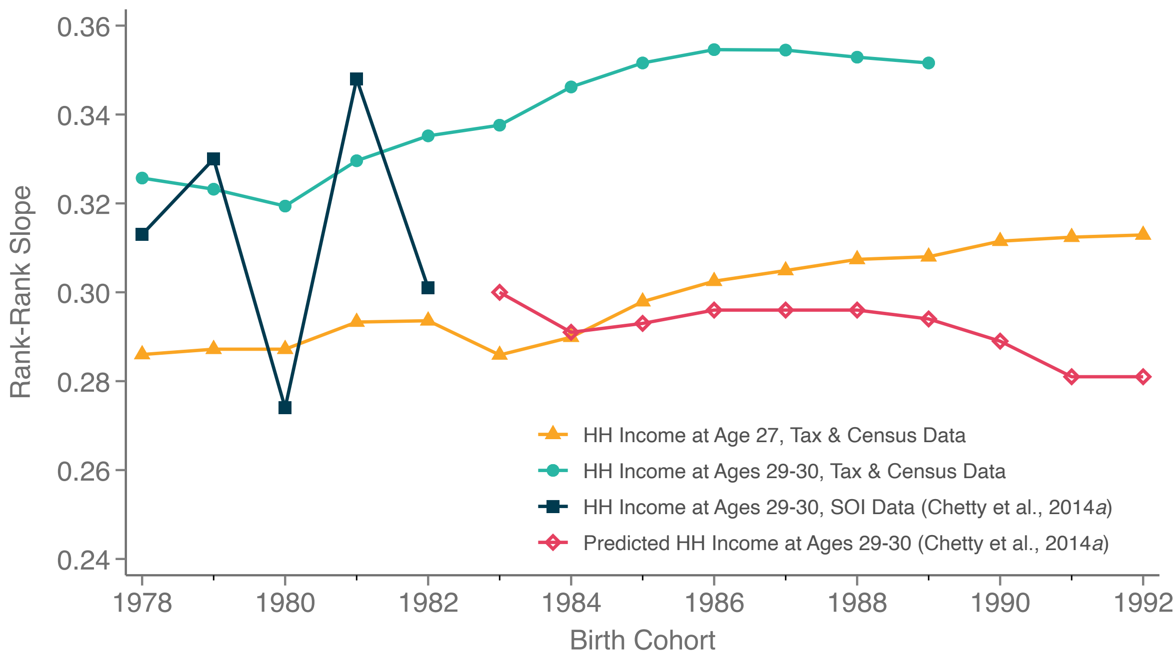


B. White Class and Black-White Race Gaps Using Percentile to Dollar Mapping



Notes: These figures plot the mapping between dollars and percentiles for child household income. Panel A plots the mean children's household income when the child is age 27 at each child income percentile for all children in the 1978-1992 birth cohorts. Panel B plots the white class and white-Black race gaps for mean children's household income using the dollar-to-percentile mapping from Panel A. We also report the percentage change in the white class and white-Black race gaps between the 1978 and 1992 birth cohorts. All monetary values are reported in 2023 dollars. See Section II for details on the sample construction and variable definitions.

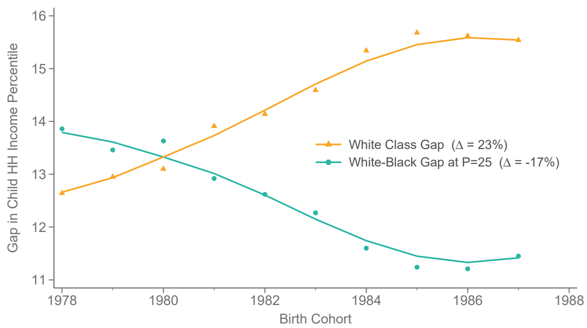
FIGURE A.2
Intergenerational Mobility Pooling all Children, by Birth Cohort: Comparison to Prior Estimates



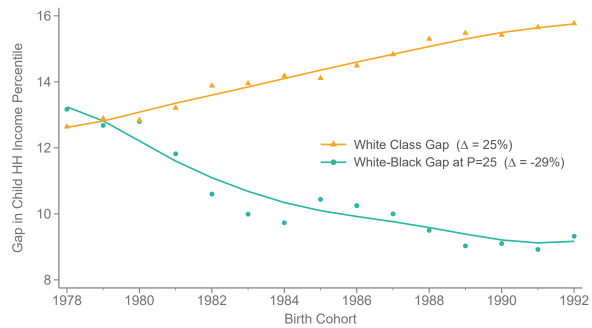
Notes: This figure plots OLS regrssion estimates of the relationship between mean children's household income ranks in adulthood and parental household income ranks for all children (pooling racial groups). Following Chetty et al. (2014*a*), children are ranked relative to other children in their birth cohort, and parents are ranked relative to all other parents in our primary analysis sample. The orange series in triangles uses our primary analysis sample and measures children's household income ranks at age 27. The green series in circles uses the same sample but measures children's household income ranks at ages 29-30. The navy series in squares follows Figure 2 of Chetty et al. (2014*a*) and measures children's household income ranks at ages 29-30 for children in the 1978-1982 birth cohorts found in the Statistics of Income (SOI) tax records. The pink series in diamonds forecasts the series in squares for the 1983-1992 birth cohorts using income at age 26 and college attendance and plots the predicted rank-rank slope for these children at ages 29-30. See Section II for additional details on the sample construction and variable definitions and Chetty et al. (2014*a*) for details on their approach.

FIGURE A.3
White Class and Black-White Race Gaps by Birth Cohort: Alternative Samples and Specifications

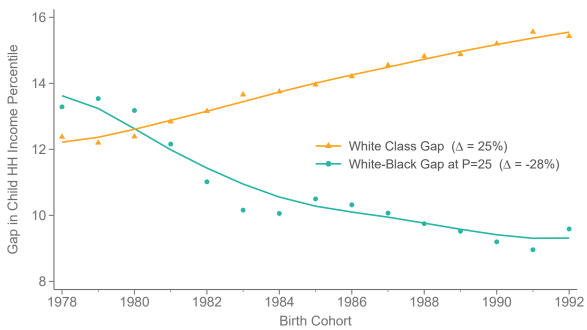
A. Child Household Income at Age 32



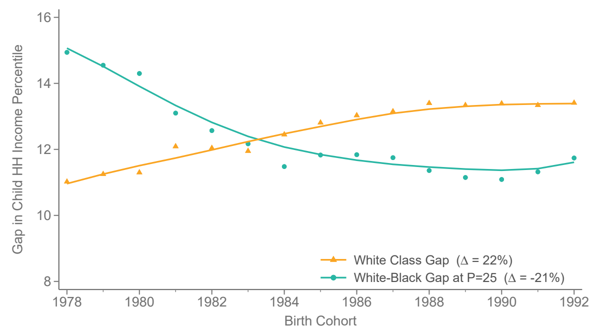
B. Parental Income at Child Ages 0-18



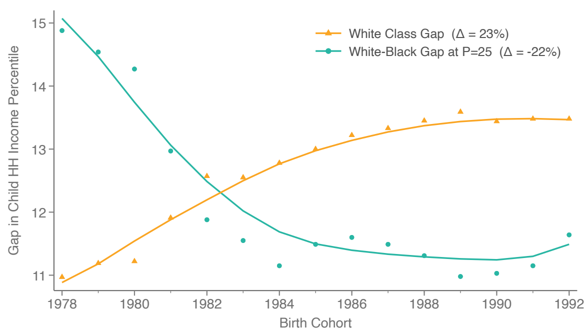
C. Parental Income at Child Ages 0-5, 6-12, 13-18



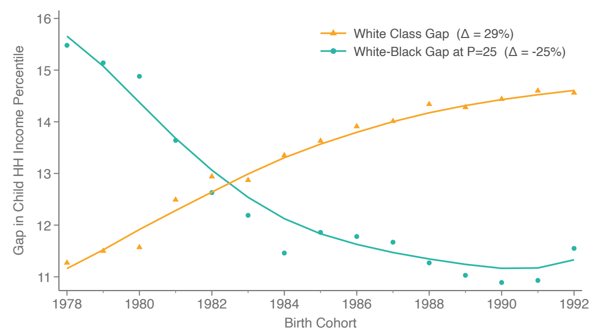
D. Mother's Household Income



E. Symmetric Parent-Child Linkage



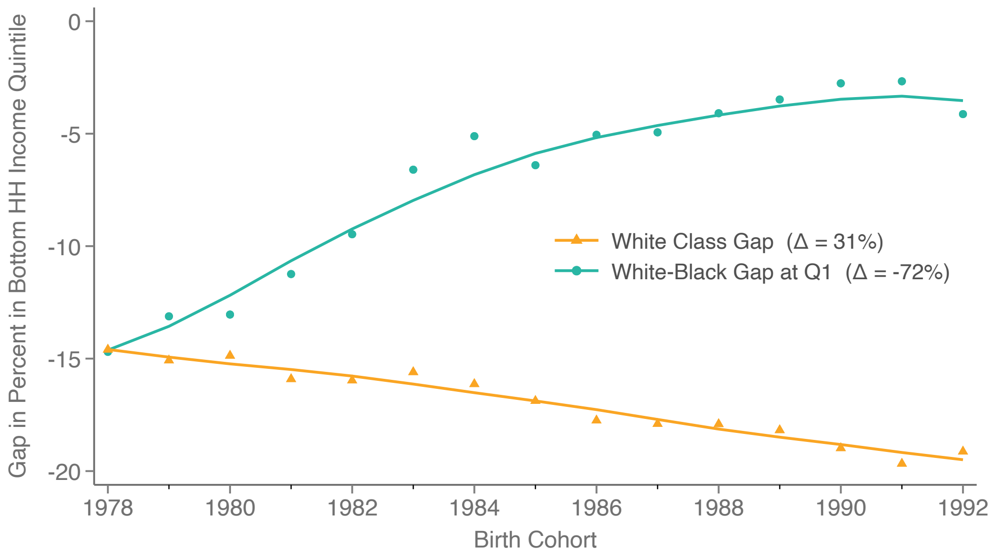
F. U.S.-Born Parents



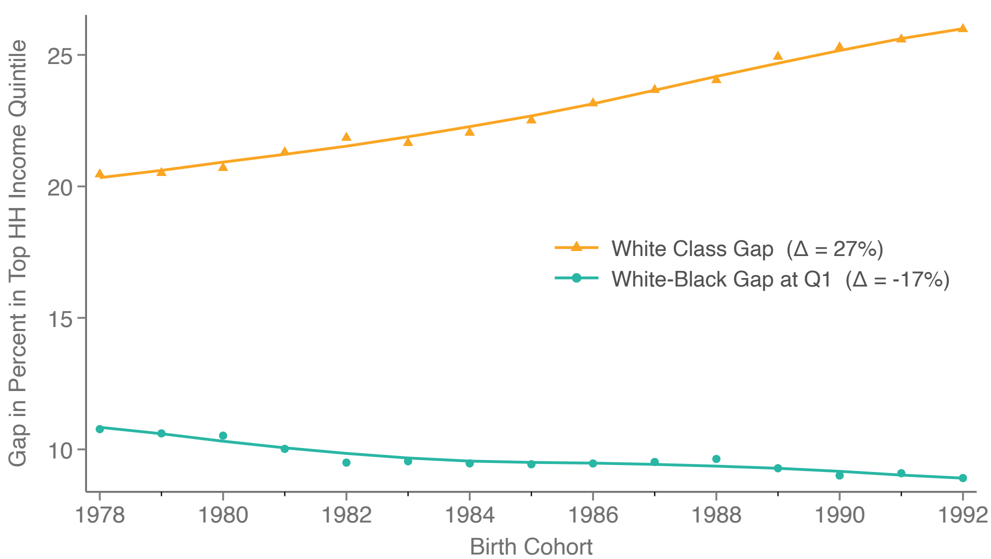
Notes: These figures plot the white class and white-Black race gaps for mean children's household income rank in adulthood using alternative definitions of child and parental household income and alternative samples. Panel A measures children's household income at age 32 for the 1978-1987 birth cohorts; Panel B measures parental income using all available years in which the child is ages 0-18; Panel C measures average parental income across one year each in early (ages 0-5), middle (ages 6-12), and late childhood (ages 13-18); Panel D measures parental income using only the mother's household income when the child is ages 13-17; Panel E matches children to parents using only the first two years of available data when the child is ages 13-17; and Panel F limits the sample to children whose parents were born in the U.S. We also report the percentage change in the white class and white-Black race gaps between the 1978 birth cohort and the last available birth cohort. We estimate mean household income ranks using fitted values from a lowess regression on parental income percentiles for each race and birth cohort. We take the difference in these fitted values to compute the white class and white-Black race gaps. See Figure I for additional details on how we construct the estimates of mean household income ranks and Section II for details on the sample construction and variable definitions.

FIGURE A.4
White Class and Black-White Race Gaps in Chances of Earning in the Bottom and Top Household Income Quintiles

A. Earning in the Bottom Household Income Quintile

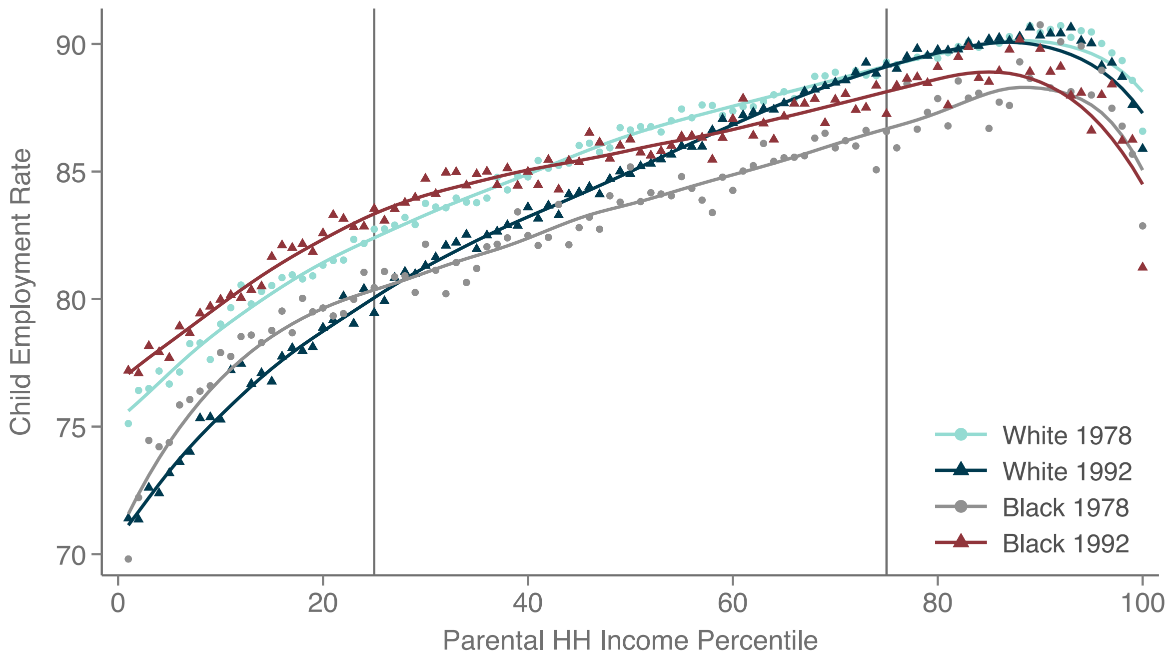


B. Earning in the Top Household Income Quintile



Notes: These figures plot the white class and white-Black race gaps for the chances of earning in the bottom and top household income quintiles. The white class gap is the difference between white children born to parents in the top versus bottom quintiles of the national income distribution. The white-Black race gap is the difference between white and Black children born to parents in the bottom quintile of the national income distribution. We also report the percentage change in the white class and white-Black race gaps between the 1978 and 1992 birth cohorts. We estimate each outcome using fitted values from a lowess regression on parental income quintiles for each race and birth cohort. We take the difference in these fitted values to compute the white class and white-Black race gaps. See Section II for details on the sample construction and variable definitions.

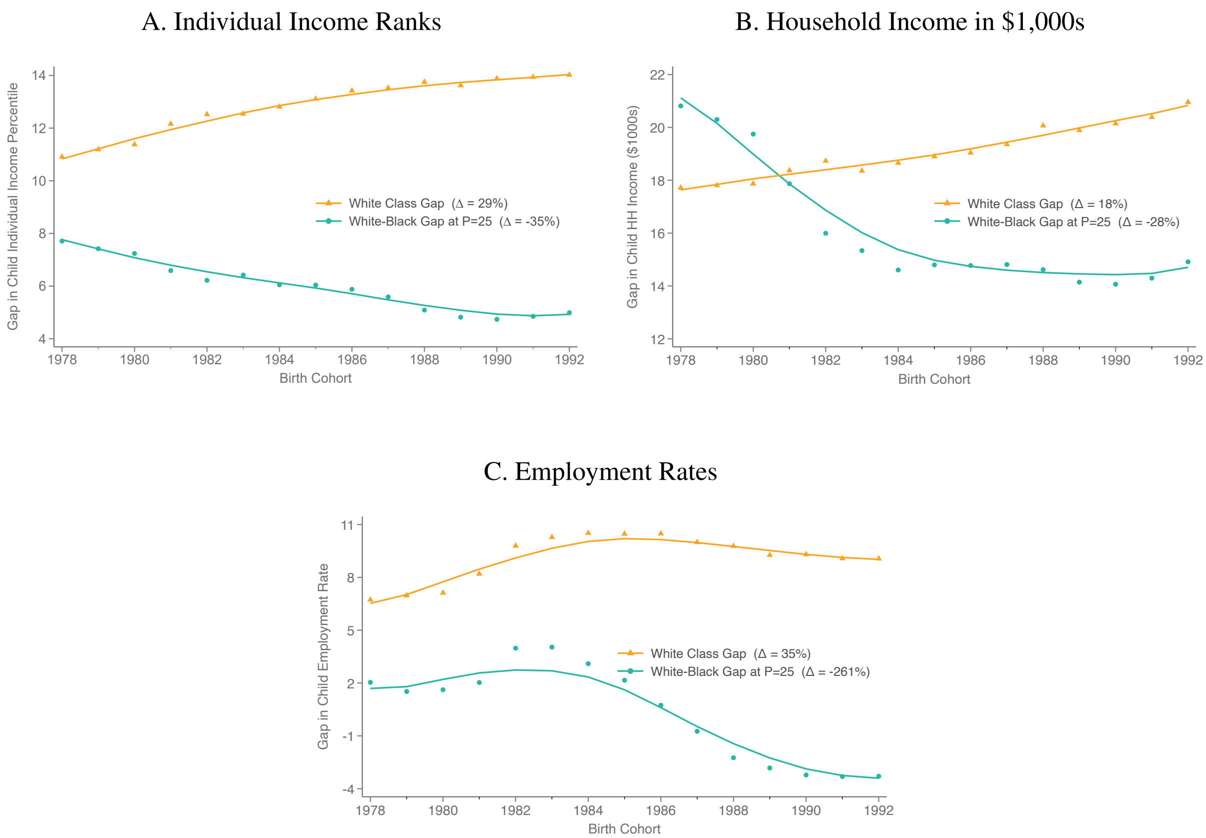
FIGURE A.5
Children's Employment Rates versus Parental Household Income for the 1978 and 1992 Birth Cohorts



Notes: This figure plots the mean employment rate in adulthood for white and Black children in the 1978 and 1992 birth cohorts at each parental income percentile. We estimate the fitted lines for each race and birth cohort using a lowess regression on the binned series (with bandwidth 0.3). See Section II for additional details on the sample construction and variable definitions.

FIGURE A.6

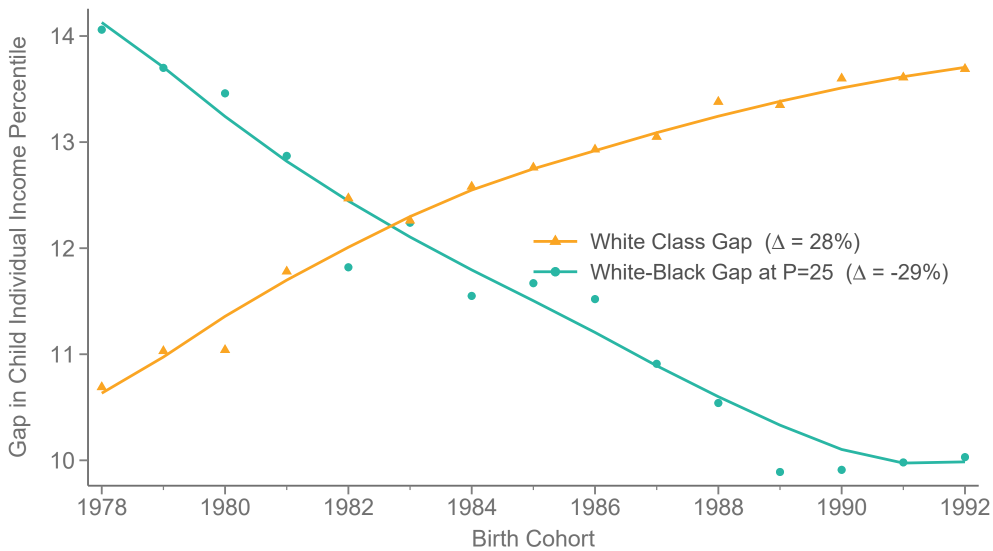
White Class and White-Black Race Gaps in Individual Income, Household Income in Dollars, and Employment



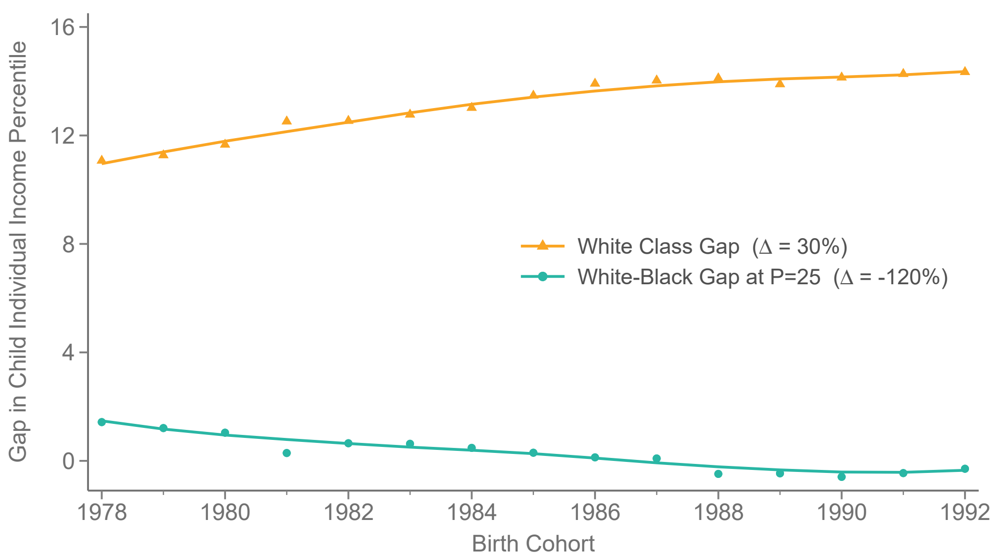
Notes: These figures plot the white class and white-Black race gaps for individual income ranks, household income in 2023 dollars (winsorized at $1 million), and employment rates in adulthood. We also report the percentage change in the white class and white-Black race gaps between the 1978 and 1992 birth cohorts. We estimate each outcome using fitted values from a lowess regression on parental income percentiles for each race and birth cohort. We take the difference in these fitted values to compute the white class and white-Black race gaps. See Figure I for additional details on how we construct the estimates and Section II for details on the sample construction and variable definitions.

FIGURE A.7
White Class and White-Black Race Gaps in Individual Income by Gender

A. Male Children



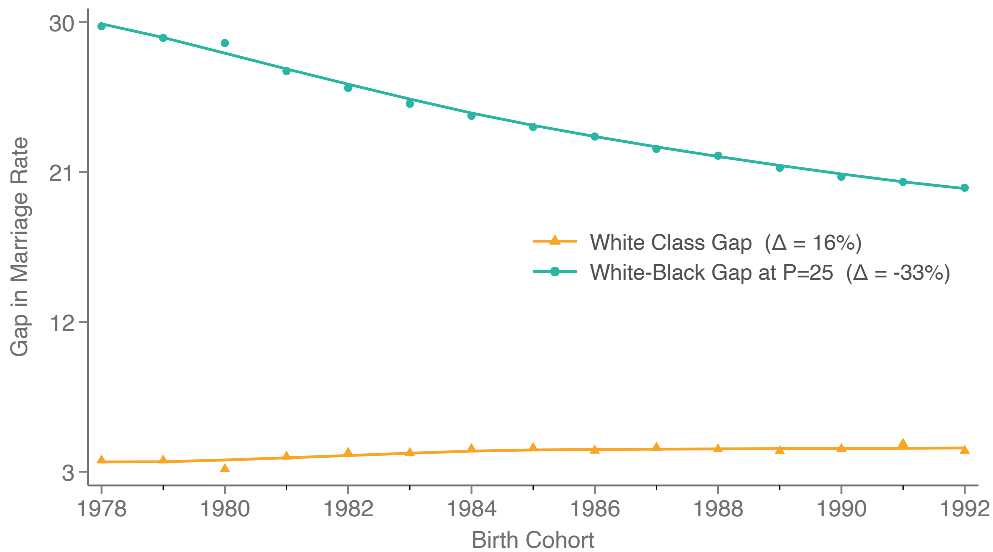
B. Female Children



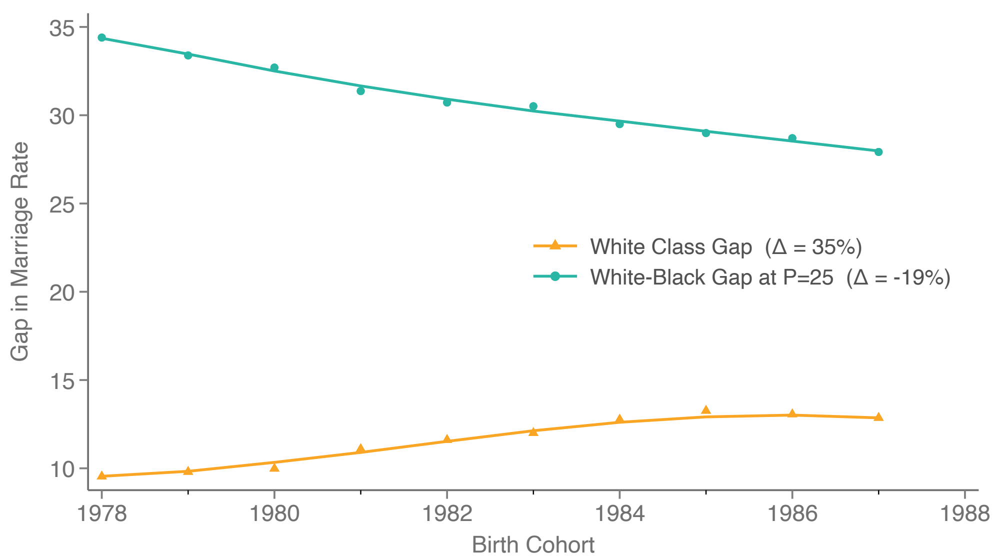
Notes: These figures plot the white class and white-Black race gaps for individual income ranks in adulthood by gender. We also report the percentage change in the white class and white-Black race gaps between the 1978 and 1992 birth cohorts. We estimate individual income ranks using fitted values from a lowess regression on parental income percentiles for each race, gender, and birth cohort. We take the difference in these fitted values to compute the white class and white-Black race gaps. See Figure I for additional details on how we construct the estimates and Section II for details on the sample construction and variable definitions.

FIGURE A.8
White Class and White-Black Race Gaps in Marriage Rates by Age

A. Marriage Rates at Age 27

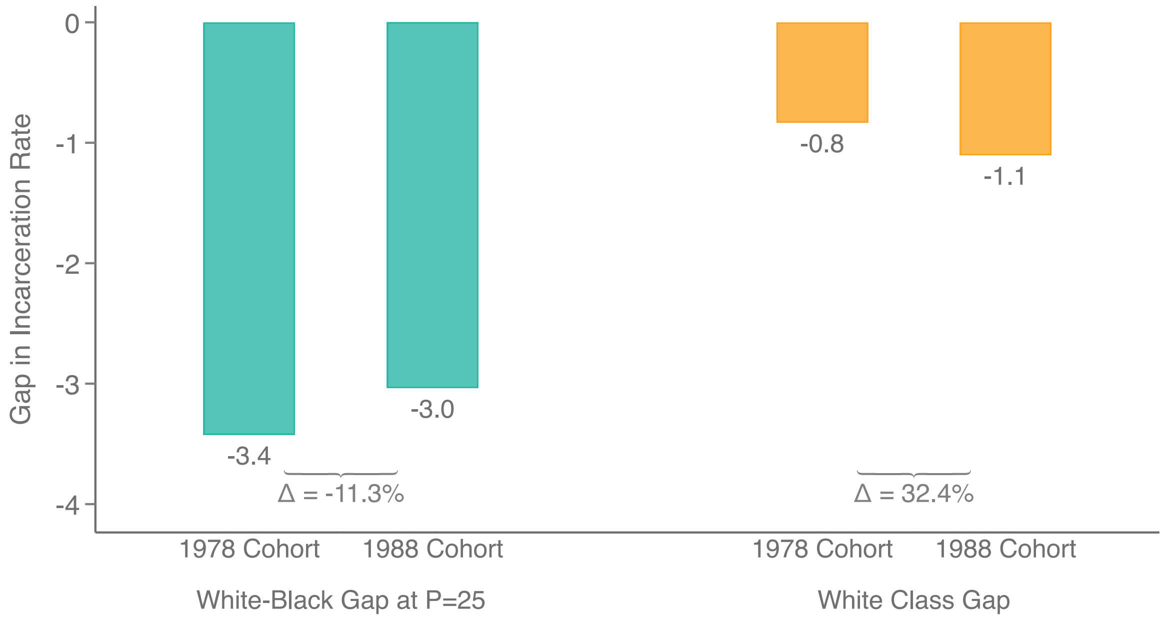


B. Marriage Rates at Age 32



Notes: These figures plot the white class and white-Black race gaps for marriage rates when the child is age 27 and age 32. We also report the percentage change in the white class and white-Black race gaps between the 1978 birth cohort and the last available birth cohort. We estimate marriage rates using fitted values from a lowess regression on parental income percentiles for each race and birth cohort. We take the difference in these fitted values to compute the white class and white-Black race gaps. See Figure I for additional details on how we construct the estimates and Section II for details on the sample construction and variable definitions.

FIGURE A.9
White Class and White-Black Race Gaps in Incarceration Rates

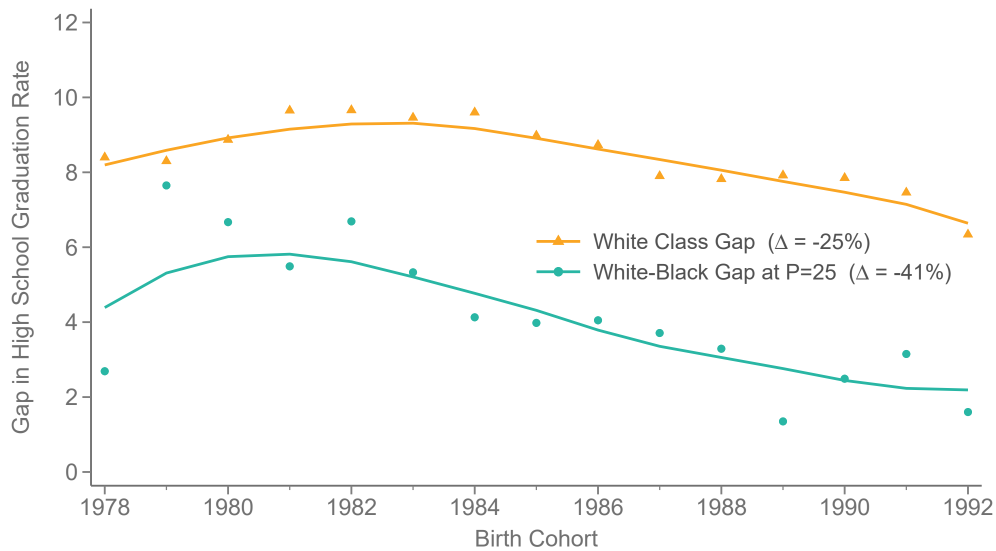


Notes: These figures plot the white class and white-Black race gaps for incarceration rates. Incarceration rates are measured when the child is age 22 using the 2000 and 2010 Census short forms. We also report the percentage change in the white class and white-Black race gaps between the 1978 and 1988 birth cohorts. We estimate each outcome using fitted values from a lowess regression on parental income percentiles for each race and birth cohort. We take the difference in these fitted values to compute the white class and white-Black race gaps. See Figure I for additional details on how we construct the estimates and Section II for details on the sample construction and variable definitions.

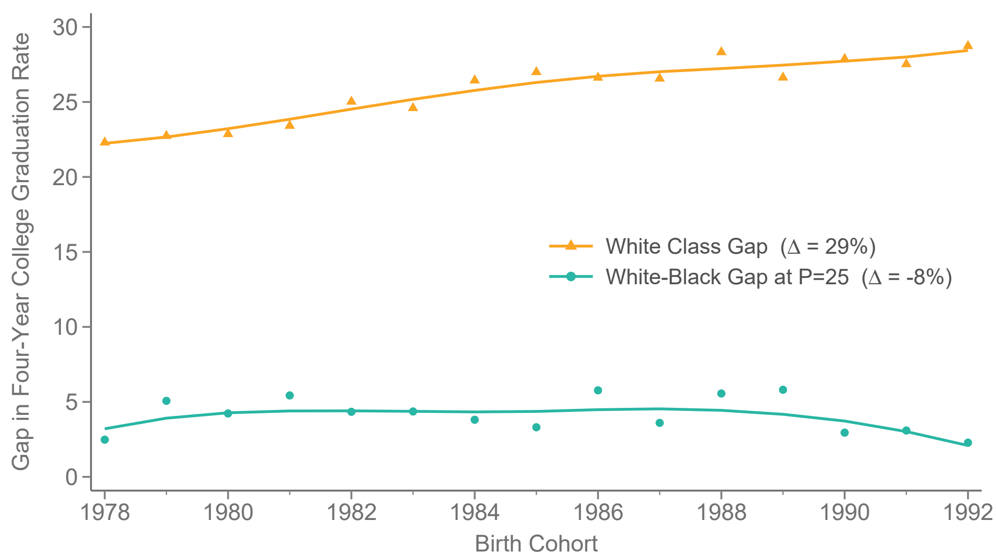
FIGURE A.10

White Class and White-Black Race Gaps in High School and Four-Year College Graduation Rates

A. High School Graduation Rates



B. Four-Year College Graduation Rates

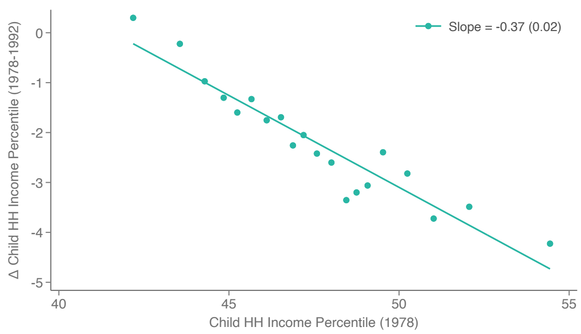


Notes: These figures plot the white class and white-Black race gaps for high school graduation and four-year college graduation rates. We also report the percentage change in the white class and white-Black race gaps between the 1978 and 1992 birth cohorts. We estimate each outcome using fitted values from a lowess regression on parental income percentiles for each race and birth cohort. We take the difference in these fitted values to compute the white class and white-Black race gaps. See Figure I for additional details on how we construct the estimates and Section II for details on the sample construction and variable definitions.

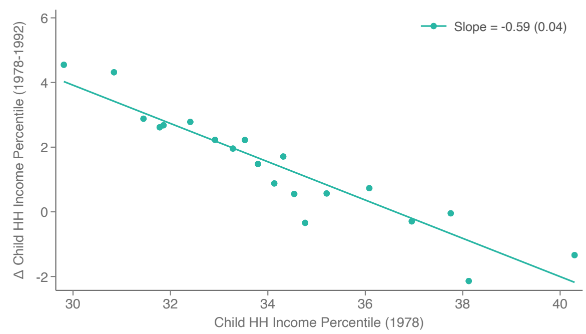
FIGURE A.11

Changes in Children's Household Income versus Children's Household Income in 1978

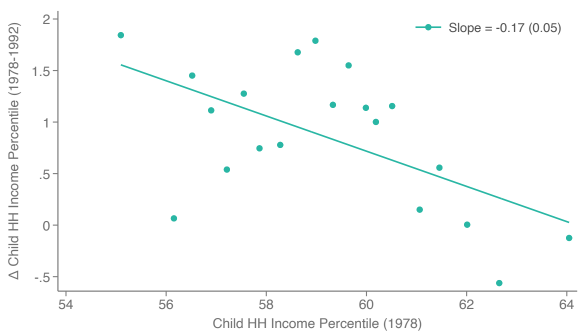
A. White Children at P=25



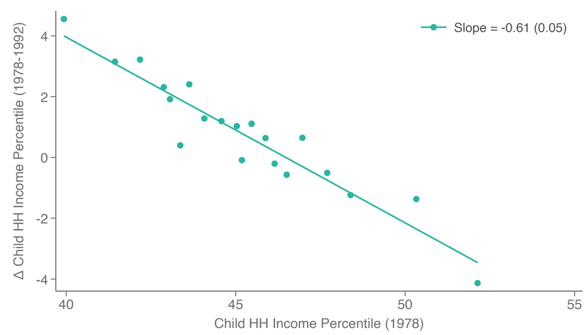
B. Black Children at P=25



C. White Children at P=75



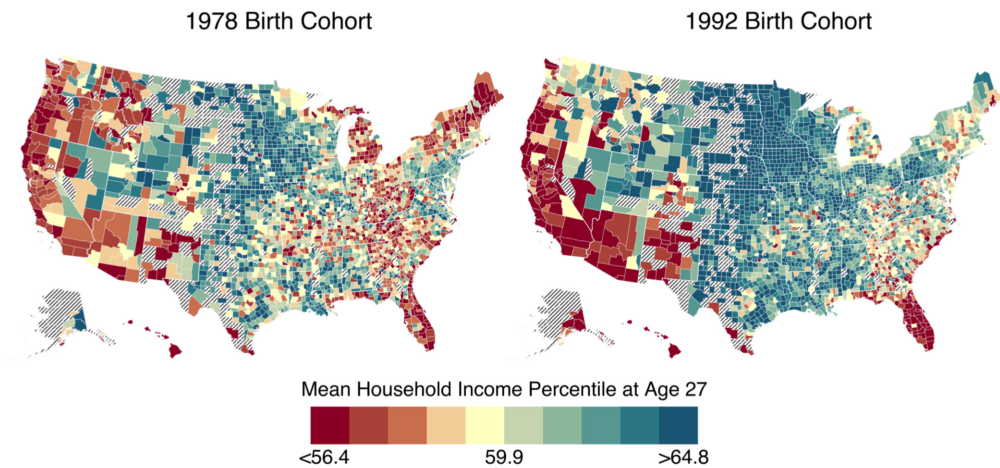
D. Black Children at P=75



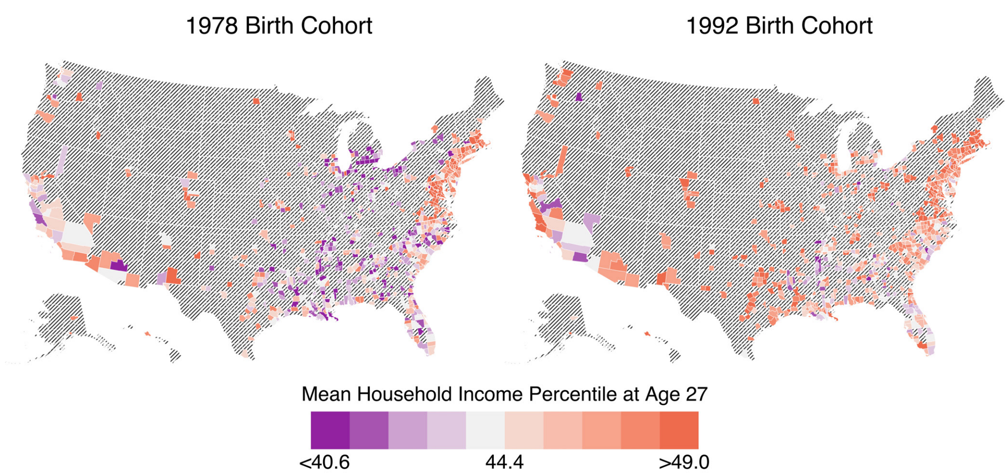
Notes: These figures show binned scatterplots of changes in children's household income rank in adulthood between the 1978 and 1992 birth cohorts versus the household income rank for children in the 1978 birth cohort. Panel A plots results for white children born to families at the 25th percentile of the national income distribution; Panel B for Black children born to families at the 25th percentile of the national income distribution; Panel C for white children born to families at the 75th percentile of the national income distribution; and Panel D for Black children born to families at the 75th percentile of the national income distribution. We also report the slope and standard error of the weighted best-fit line estimated in an OLS regression, where we weight by the number of children in each county-by-race-by-class cell. We restrict to counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct the county-by-race-by-class-by-cohort estimates of economic mobility and Section II for details on the sample construction and variable definitions.

FIGURE A.12
The Changing Geography of Intergenerational Mobility for High-Income Families

A. White Children at the 75th Percentile



B. Black Children at the 75th Percentile



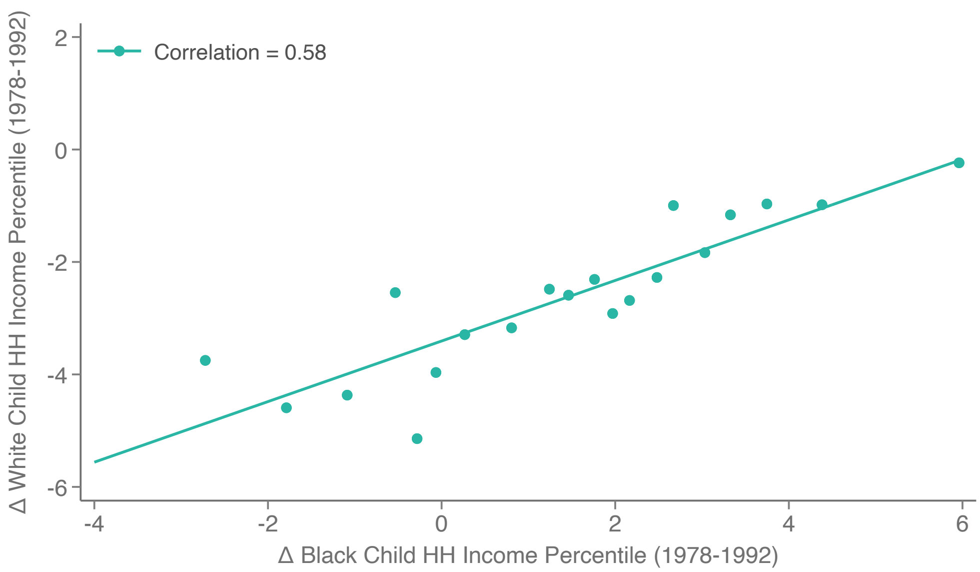
These maps must be printed in color to be interpretable.

Notes: These figures show maps of mean household income ranks in adulthood by county for white and Black children born to families at the 75th percentile of the national income distribution. Panel A restricts to counties with at least 250 white children born to families with above-median incomes in the 1978-1992 birth cohorts; Panel B restricts to counties with at least 250 Black children born to families with above-median incomes in the 1978-1992 birth cohorts. Counties shown in gray are areas with no estimates due to insufficient data in the relevant group. See Appendix A for details on how we construct county-by-race-by-class-by-cohort estimates of economic mobility and Section II for details on the sample construction and variable definitions.

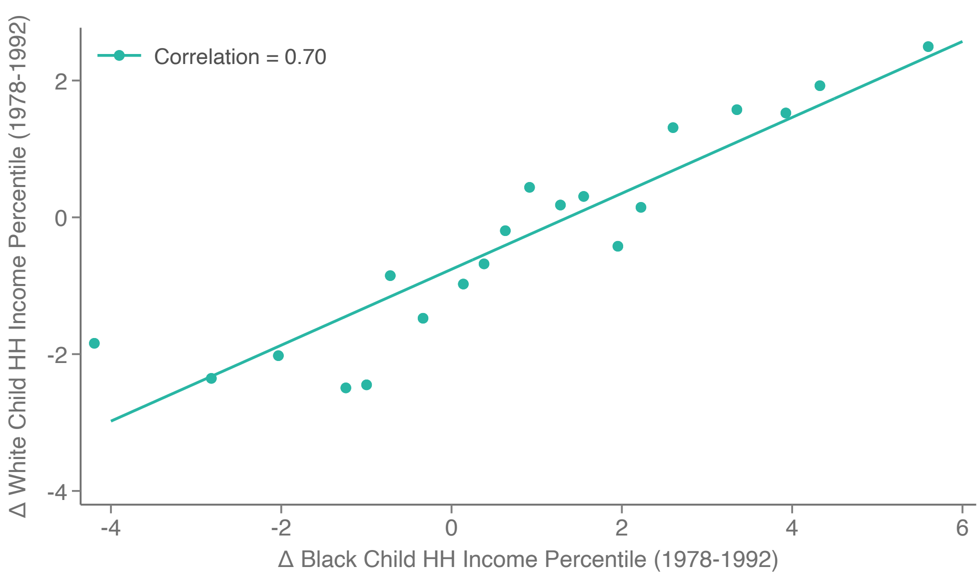
FIGURE A.13

Changes in Children's Household Income for White versus Black Children

A. White versus Black Children at P=25



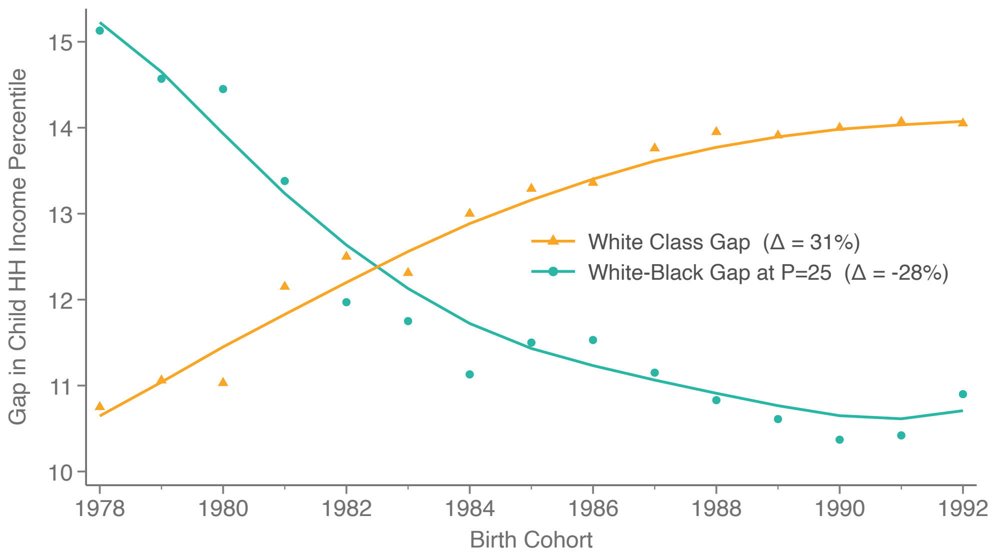
B. White versus Black Children at P=75



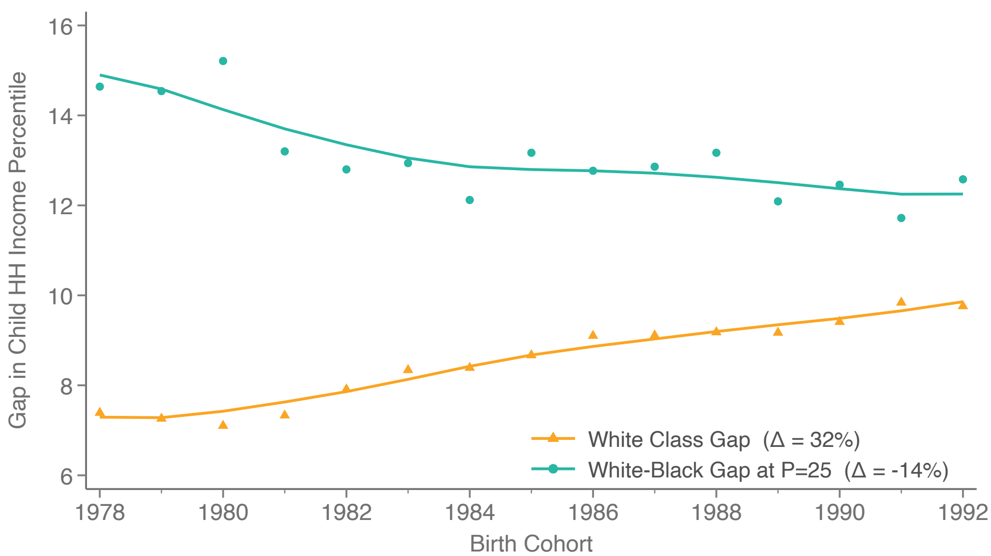
Notes: These figures show binned scatterplots of changes in the household income rank in adulthood for white versus Black children between the 1978 and 1992 birth cohorts. Panel A plots results for children born to families at the 25th percentile of the national income distribution; Panel B plots results for children born to families at the 75th percentile of the national income distribution. We also report the correlation between changes in the household income rank for white children and changes in the household income rank for Black children, where we weight by the number of Black children in each county-by-class cell. We restrict to counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct the county-by-race-by-class-by-cohort estimates of economic mobility and Section II for details on the sample construction and variable definitions.

FIGURE A.14
White Class and White-Black Race Gaps in Children's Household Income by Parental Education

A. Less Than a Four-Year College Degree

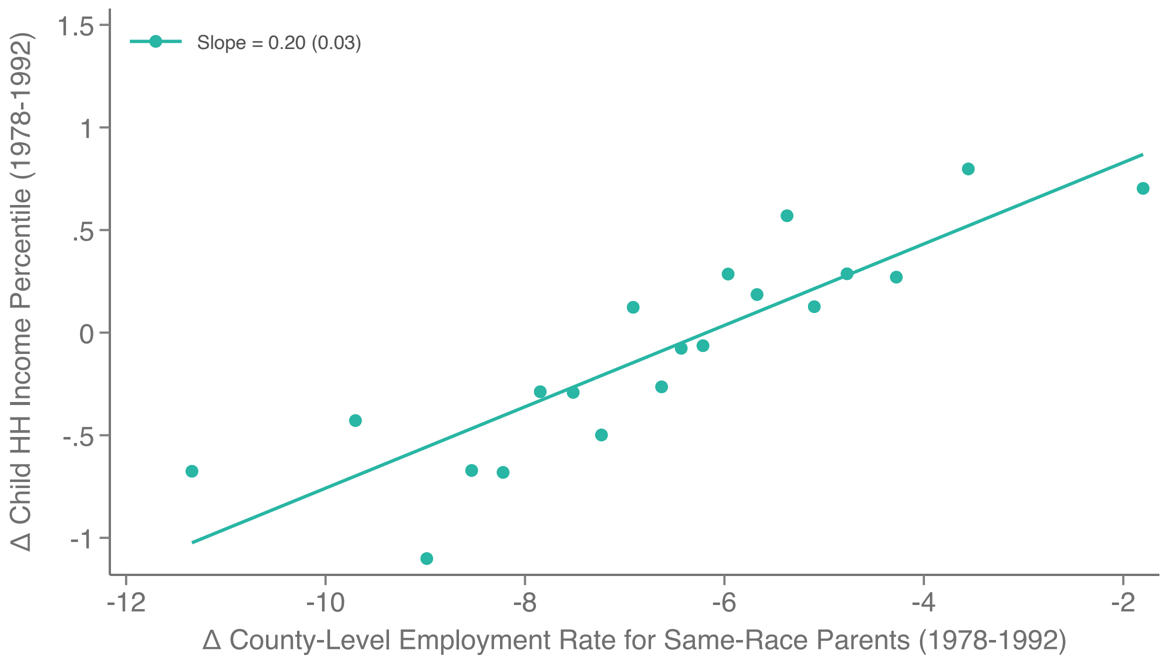


B. Four-Year College Degree or More



Notes: These figures plot the white class and white-Black race gaps for children's household income ranks in adulthood by parental education. Panel A plots results for families where no parent has a four-year college degree; Panel B plots results for families where at least one parent has a four-year college degree or more. We also report the percentage change in the white class and white-Black race gaps between the 1978 and 1992 birth cohorts. We estimate mean household income ranks using fitted values from a lowess regression on parental income percentiles for each race, birth cohort, and parental education group. We take the difference in these fitted values to compute the white class and white-Black race gaps. See Figure I for additional details on how we construct the estimates and Section II for details on the sample construction and variable definitions.

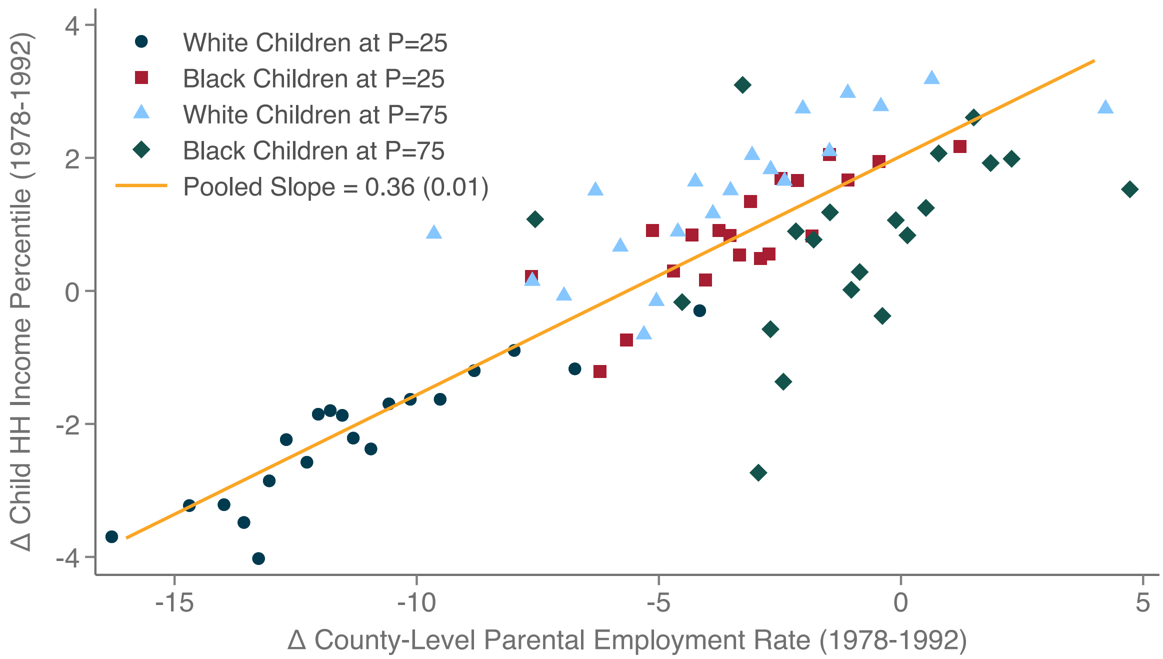
FIGURE A.15
Changes in Children's Household Income in Adulthood versus Changes in Employment Rates for Same-Race Parents by County



Notes: This figure shows a binned scatterplot of changes in children's household income ranks in adulthood versus changes in the employment rate for same-race parents measured at child age 27, controlling for race-by-parental income percentile fixed effects using the same method as in Figure VI. We measure changes in children's household income ranks for white and Black children born to parents at the 25th and 75th percentiles of the national income distribution. We measure changes in the employment rate for same-race parents using the difference in employment rates between the 1978 and 1992 birth cohorts (pooling across class groups) when the child is age 27. We report the slope and standard error of the weighted best-fit line estimated in an OLS regression, where we weight by the number of children in each county-by-race-by-class cell. We restrict to counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct the county-by-race-by-class-by-cohort estimates for each outcome and Section II for details on the sample construction and variable definitions.

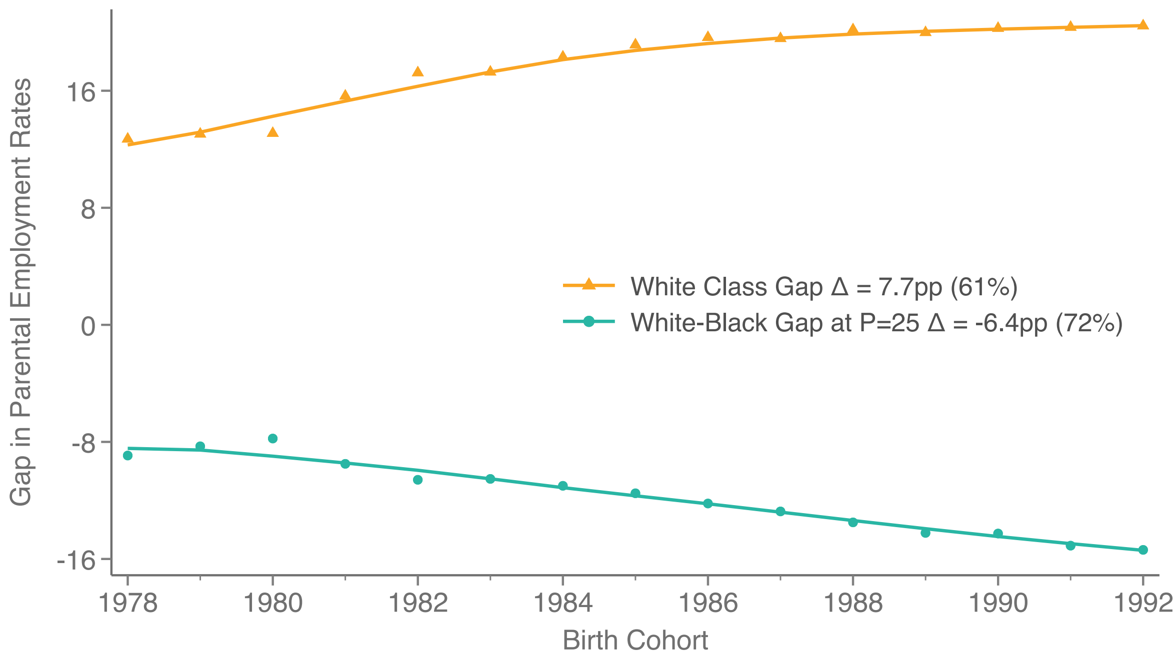
FIGURE A.16

Changes in Children's Household Income in Adulthood versus Changes in Parental Employment Including High-Income Black Families



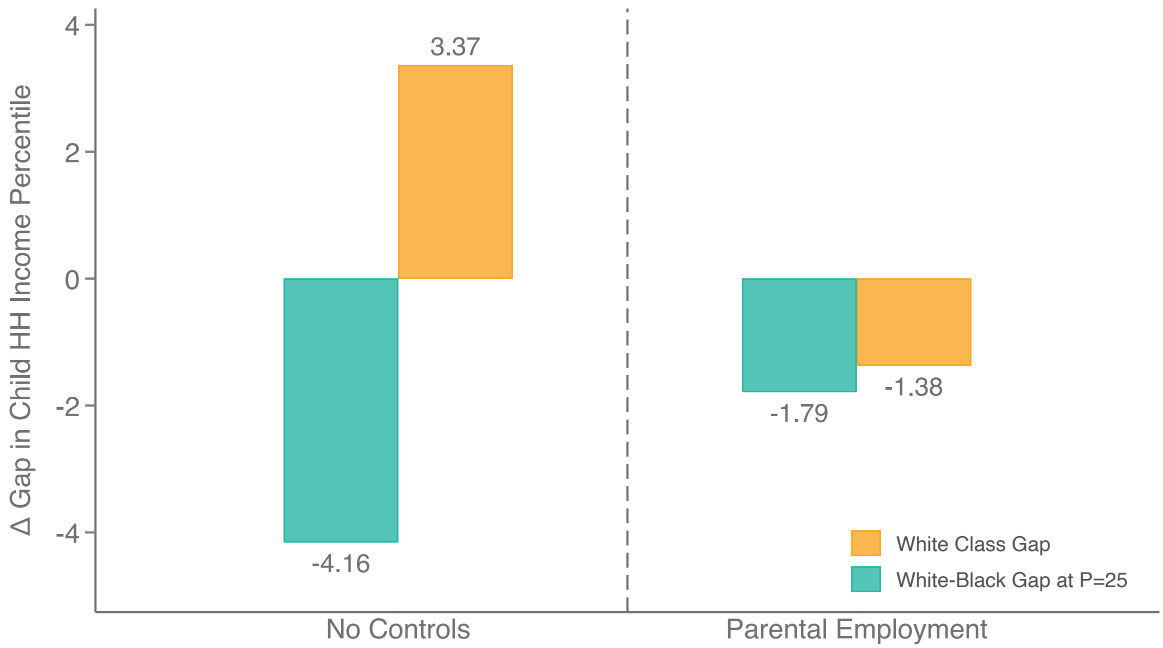
Notes: This figure shows a binned scatterplot of changes in children's household income rank in adulthood versus changes in parental employment, extending Figure VIIa to include Black children born to families at the 75th percentile of the national income distribution. We also report the slope and standard error of the weighted best-fit line estimated in an OLS regression, where we weight by the number of children in each county-by-race-by-class cell. We restrict to counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct the county-by-race-by-class-by-cohort estimates and Section II for details on the sample construction and variable definitions.

FIGURE A.17
White Class and White-Black Race Gaps in Parental Employment Rates



Notes: This figure plots the white class and white-Black race gaps for parental employment rates. We also report the percentage point and percent changes in the white class and white-Black race gaps between the 1978 and 1992 birth cohorts. We estimate parental employment rates using fitted values from a lowess regression on parental income percentiles for each race and birth cohort. We take the difference in these fitted values to compute the white class and white-Black race gaps. See Figure I for additional details on how we construct the estimates and Section II for details on the sample construction and variable definitions.

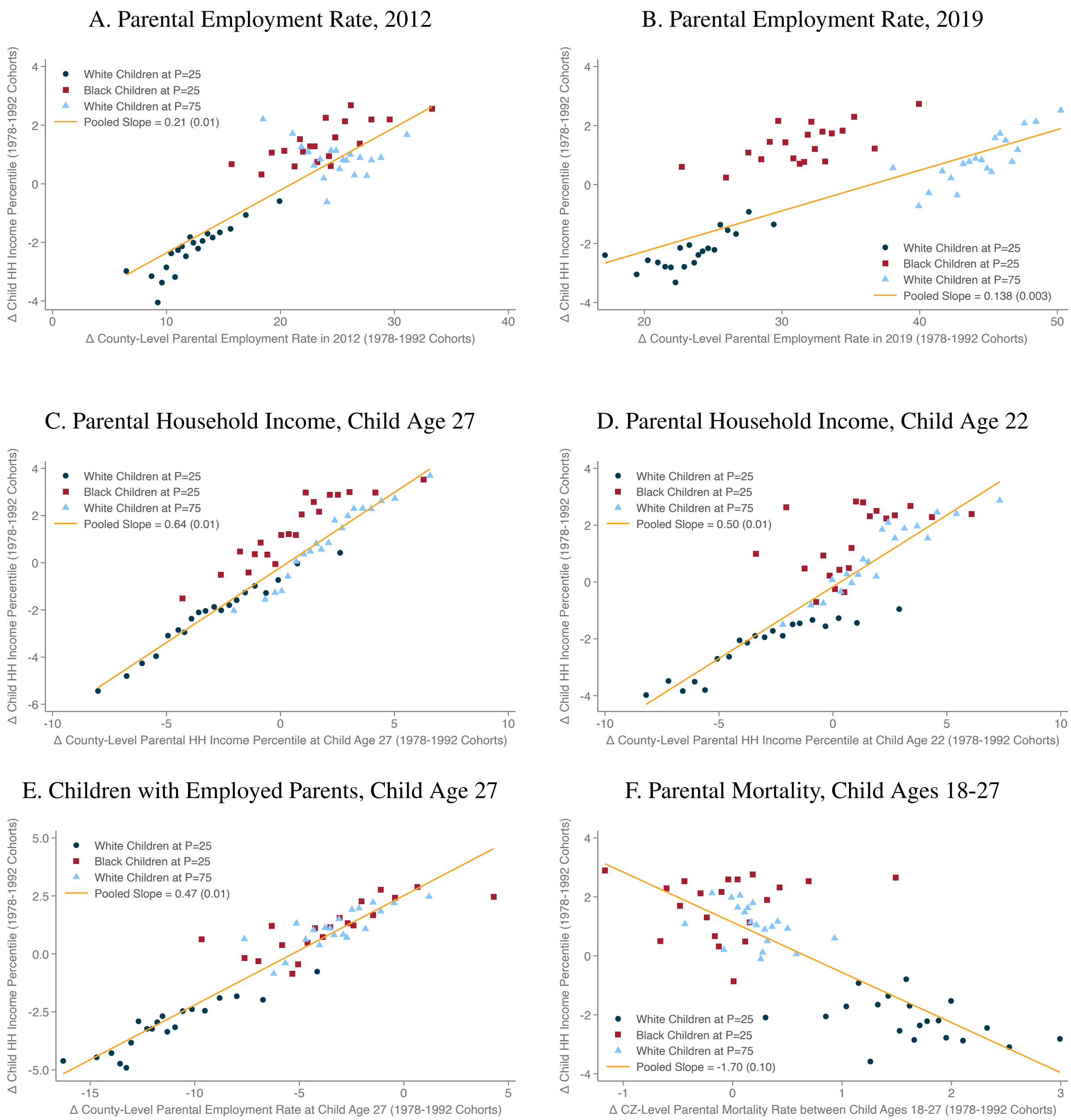
FIGURE A.18
Effect of Community-Level Parental Employment Rates on White Class and White-Black Race Gaps



Notes: This figure reports OLS regression estimates of the change in the white class and white-Black race gaps controlling for community-level parental employment rates. The first pair of bars replicates the results from Figure V showing the change in the white class and white-Black race gaps between the 1978 and 1992 birth cohorts with no controls, estimated by regressing children's household income ranks on a linear cohort control interacted with class (for the white class gap) or race (for the white-Black race gap). The second pair of bars reports estimates controlling for race-by-class-by-cohort specific county-level parental employment rates interacted with class and cohort fixed effects (for the white class gap) or race and cohort fixed effects (for the white-Black race gap). For the white class gap, we restrict the sample to white children born to families between the 20th and 30th percentiles of the parental income distribution or families between the 70th and 80th percentiles of the parental income distribution. For the white-Black race gap, we restrict the sample to white and Black children born to families between the 20th and 30th percentiles of the parental income distribution. Specifications with no controls use all available children. Specifications with controls for parental employment restrict to children for whom location information is available. For all specifications, we first estimate the unconditional change in the white class and white-Black race gaps in the relevant subsample. We then estimate the conditional change in both gaps after accounting for the relevant set of controls. Finally, we multiply the ratio of the unconditional and conditional estimates in the relevant subsample by the unconditional change in the full sample to generate the estimates reported above. See Section II for details on the sample construction and variable definitions, and Section IV for details on the regression specifications.

FIGURE A.19

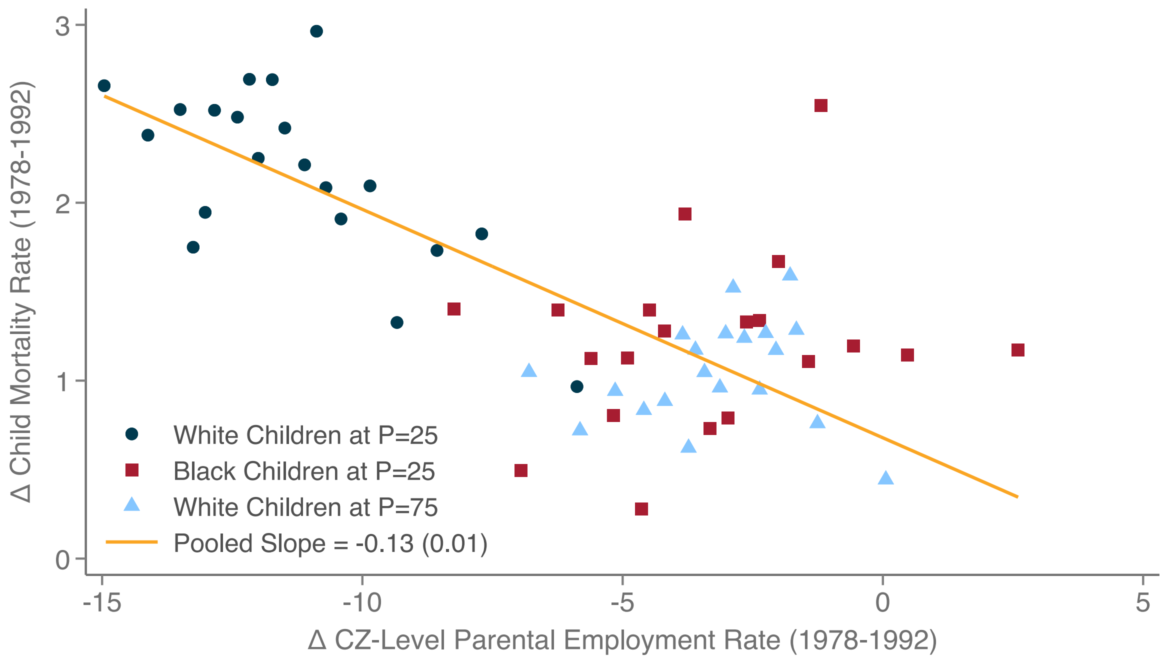
Changes in Children's Household Income in Adulthood versus Changes in Parental Outcomes: Alternative Samples and Specifications



Notes: These figures show binned scatterplots of changes in children's household income ranks in adulthood versus changes in parental outcomes using alternative samples and specifications. Panel A measures parental outcomes using employment rates in 2012; Panel B measures parental outcomes using employment rates in 2019; Panel C measures parental outcomes using household income ranks when the child is age 27; Panel D measures parental outcomes using household income ranks when the child is age 22; Panel E measures parental outcomes using employment rates when the child is age 27 and restricts to children whose own parents are employed; and Panel F measures parental outcomes using mortality rates when the child is between ages 18-27. We also report the slope and standard error of the weighted best-fit line estimated in an OLS regression, where we weight by the number of children in each county-by-race-by-class cell (Panels A-E) or CZ-by-race-by-class cell (Panel F). We restrict to geographies with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct the county-by-race-by-class-by-cohort and CZ-by-race-by-class-by-cohort estimates for each outcome and Section II for details on the sample construction and variable definitions.

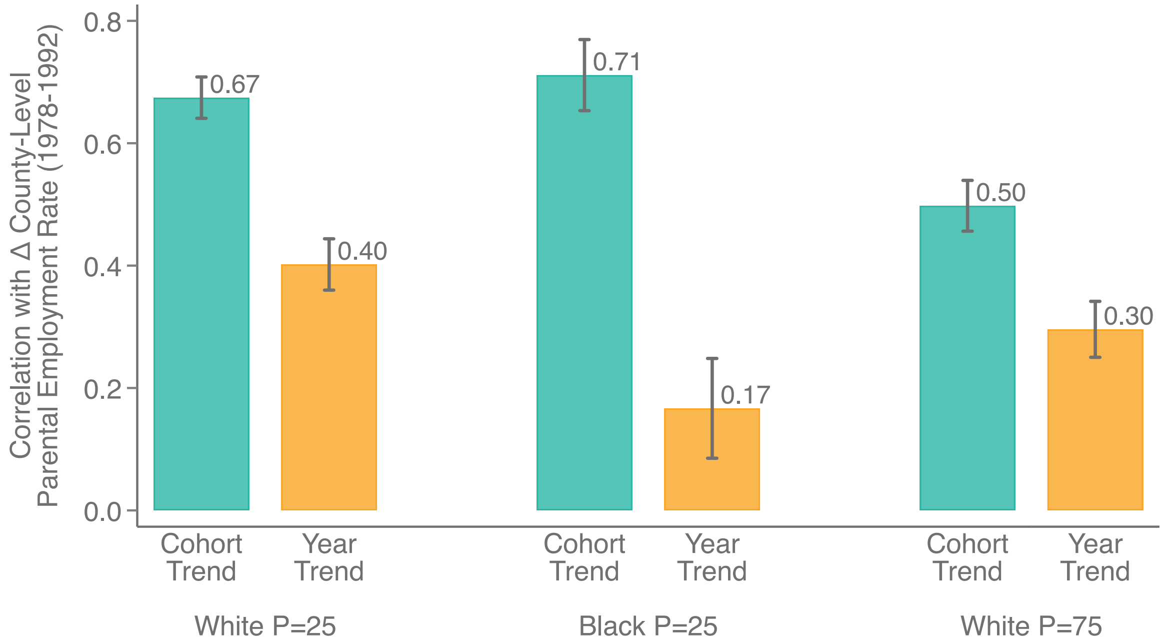
FIGURE A.20

Changes in Children's Mortality Rates in Early-Adulthood versus Changes in Parental Employment



Notes: This figure shows a binned scatterplot of changes in children's mortality rates at ages 24-27 (deaths/1000) versus changes in parental employment rates. We also report the slope and standard error of the weighted best-fit line estimated in an OLS regression, where we weight by the number of children in each CZ-by-race-by-class cell. We restrict to CZs with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct the CZ-by-race-by-class-by-cohort estimates for each outcome and Section II for details on the sample construction and variable definitions.

FIGURE A.21
Cohort versus Year Variation in Parental Employment Rates

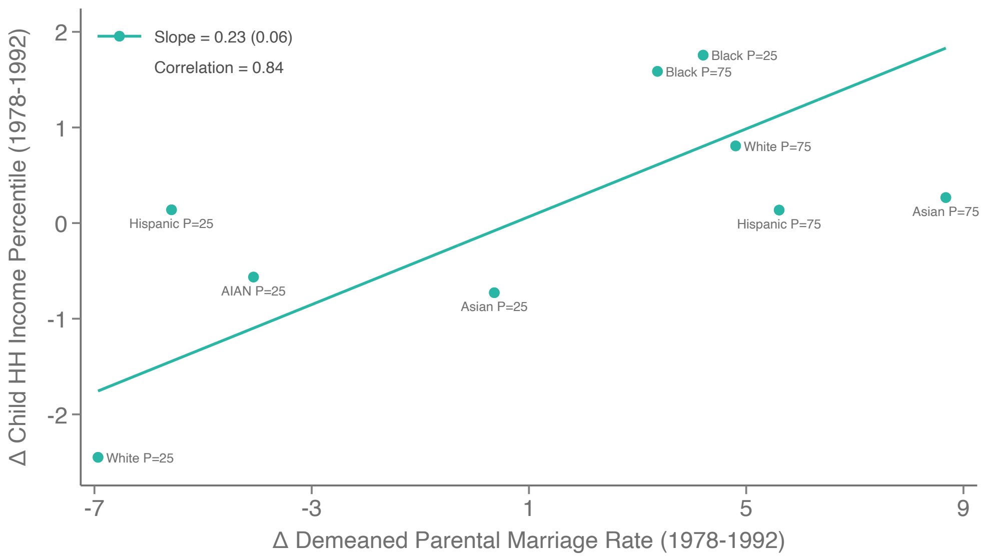


Notes: This figure plots the county-level correlation between our baseline estimates of the change in parental employment rates and alternative measures that isolate variation across cohorts or years. We estimate cohort and year trends separately for each race and class group. To construct the cohort trend in each county, we estimate an OLS regression of parental employment rates on a linear cohort control and year fixed effects. We estimate the cohort trend based on the difference in predicted values between the 1978 and 1992 birth cohorts. We then correlate these county-level cohort trends with our baseline estimates of the change in parental employment rates, where we weight by the number of children in each county-by-race-by-class cell. The year trend repeats this exercise, but instead regresses parental employment rates on a linear year control and cohort fixed effects. We estimate the year trend based on the difference in predicted values between calendar years 2005 and 2019. We restrict to counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. We also restrict to years in which children in the relevant birth cohort are at least 18 years old. The vertical bars denote 95% confidence intervals. See Appendix A for details on how we construct county-by-race-by-class-by-cohort estimates for each outcome and Section II for details on the sample construction and variable definitions.

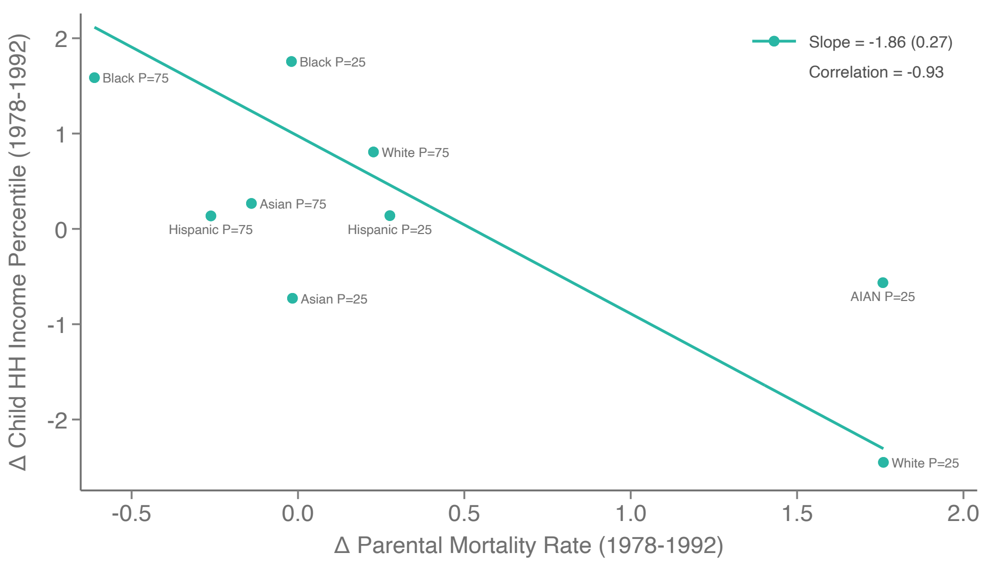
FIGURE A.22

Changes in Children's Household Income in Adulthood versus Changes Parental Marriage and Mortality Rates by Race and Class

A. Parental Marriage

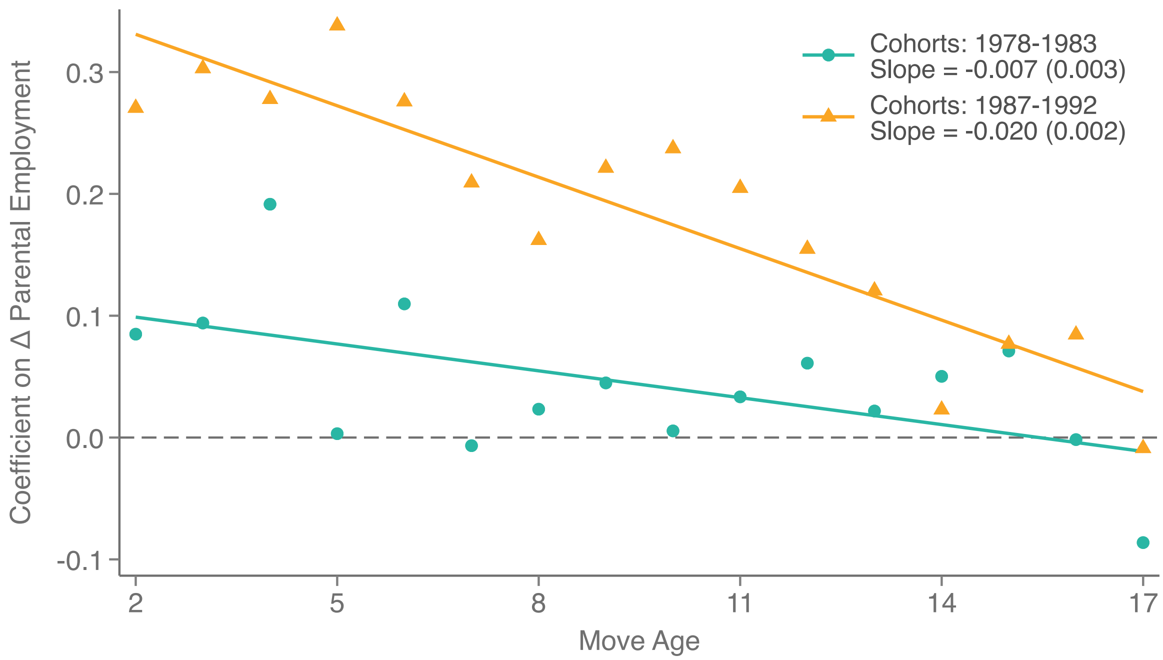


B. Parental Mortality



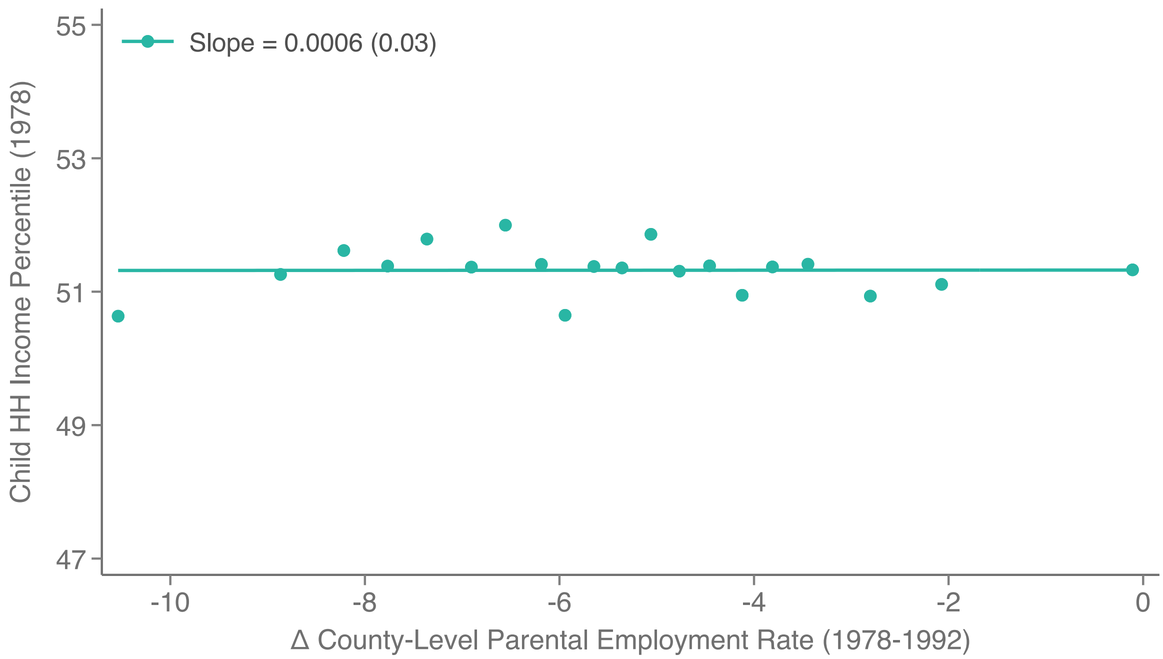
Notes: These figures plot national-level changes in children's household income ranks in adulthood versus changes in parental marriage and mortality rates. We report the slope and standard error of the weighted best-fit line estimated in an OLS regression, where we weight by the number of children in each race-by-class cell (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. We also report the weighted correlation between national-level changes in children's household income ranks and national-level changes in both parental outcomes. See Section II for details on the sample construction and variable definitions and Appendix Table A.23 for the estimates for each point in the above scatterplot.

FIGURE A.23
Effect of Changes in Parental Employment Rates in Destination County by Move Age and Cohort



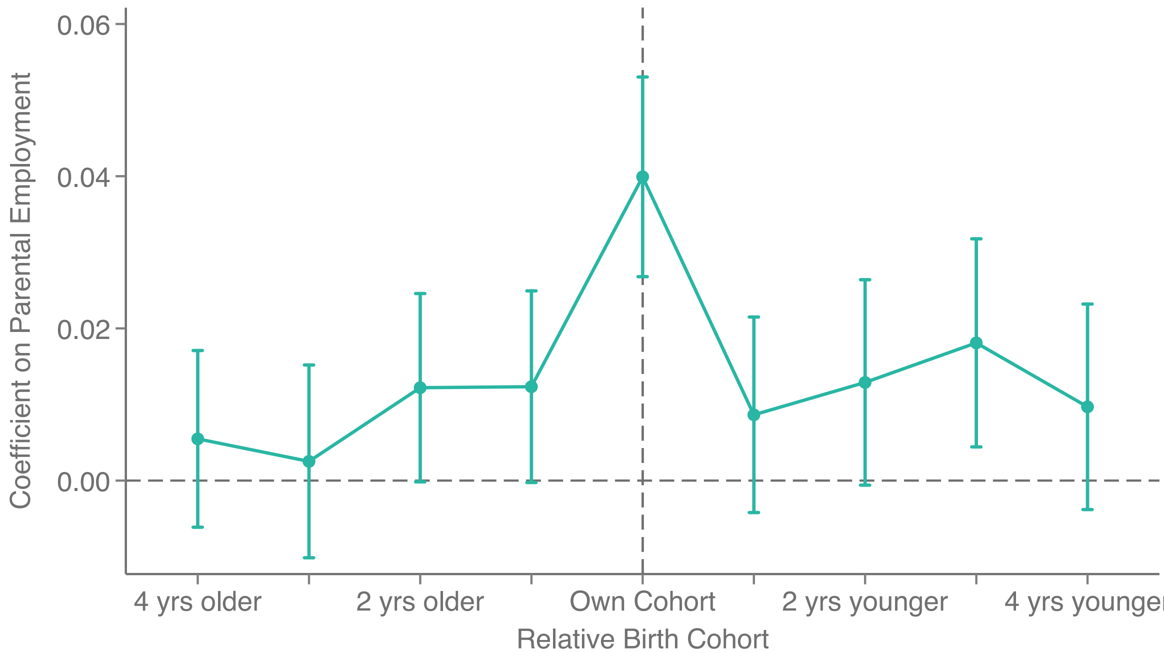
Notes: This figure reports OLS regression estimates of the effect of changes in race-by-parental income percentile parental employment rates in the destination county ($\Delta\bar{e}_{dpr}$) on children's household income ranks in adulthood at each move age, separately for children in early (1978-1983) and late (1987-1992) birth cohorts. We control for the group-specific parental employment rate in the destination county for the 1978 birth cohort, interacted with move age indicators, and origin county-by-parental income percentile-by-race-by-birth cohort-by-move age fixed effects. The change in parental employment rates is calculated using non-movers and parents who move more than once following the procedure described in Appendix A. We restrict the sample to children who moved across counties once during childhood. We also restrict to origin and destination counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. See Section II for details on the variable definitions and Section V for details on the sample construction.

FIGURE A.24
Children's Household Income for the 1978 Birth Cohort versus Changes in Parental Employment Rates



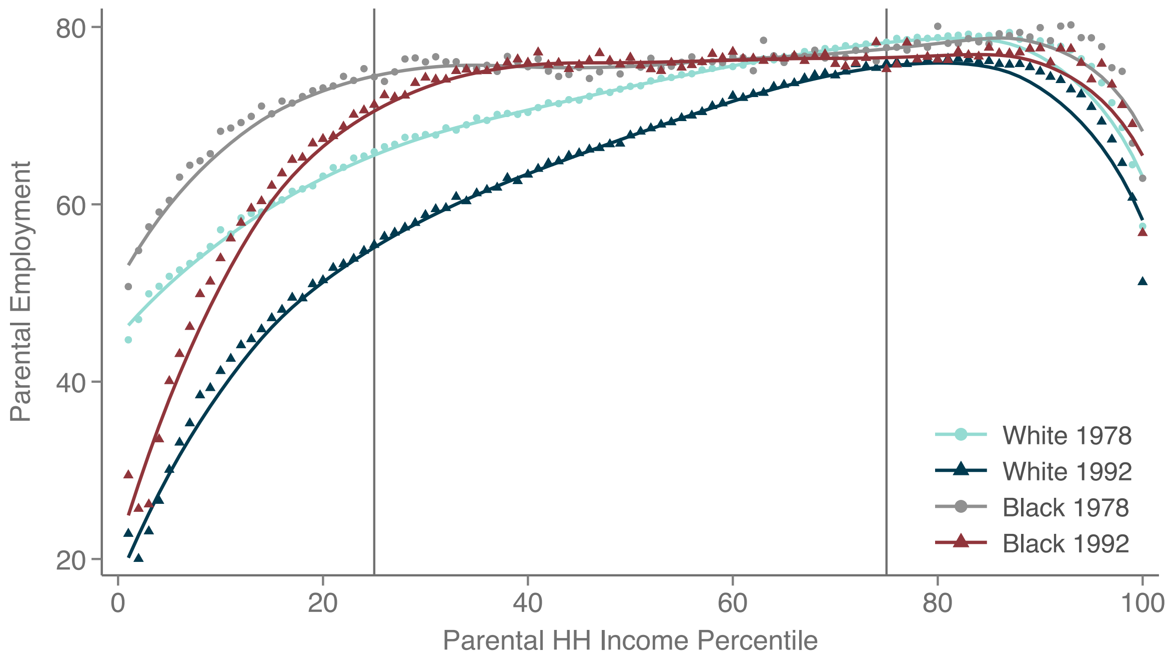
Notes: This figure shows a binned scatterplot of county-level estimates for children's household income rank in the 1978 birth cohort versus county-level changes in parental employment rates. We control for county-level parental employment rates in the 1978 birth cohort and race-by-parental income percentile fixed effects using the same method as in Figure VI. Children's household income rank and parental employment are defined at the race-parental income percentile-cohort level. We report the slope and standard error (clustered at the county level) of the weighted best-fit line estimated in an OLS regression, where we weight by the number of children in each county-by-race-by-class cell. We restrict to counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct county-by-race-by-class-by-cohort estimates for each outcome and Section II for details on the sample construction and variable definitions.

FIGURE A.25
Children's Household Income in Adulthood versus Parental Employment Rates in 2019 by Relative Birth Cohort



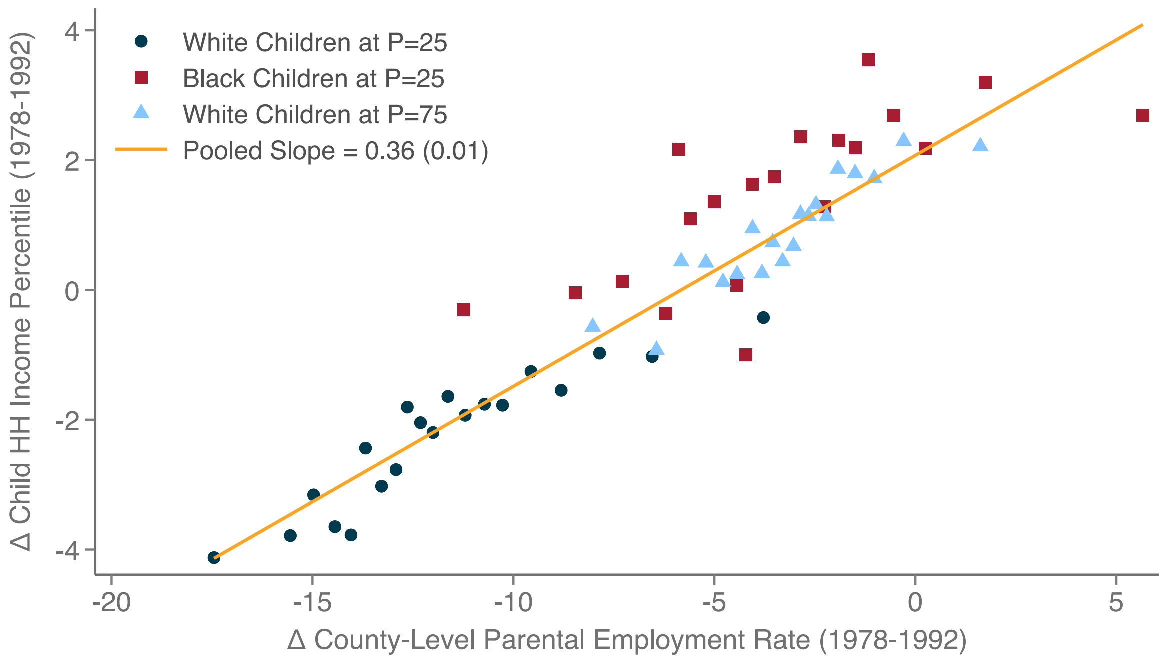
Notes: This figure reports estimates from an OLS regression of children's household income ranks in adulthood on parental employment rates in 2019 in one's own birth cohort and adjacent birth cohorts. We control for county-by-race-by-parental income percentile fixed effects and cohort-by-race-by parental income percentile fixed effects. We restrict the sample to low-income white and Black children and high-income white children in the 1982-1988 birth cohorts. We also restrict to counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. The vertical bars denote 95% confidence intervals. Standard errors are clustered at the county level. See Appendix A for details on how we construct the county-by-race-by-class-by-cohort estimates for each outcome and Section II for details on the sample construction and variable definitions.

FIGURE A.26
Parental Employment Rates versus Parental Household Income for the 1978 versus 1992 Birth Cohorts



Notes: This figure plots the mean parental employment rate for white and Black families in the 1978 and 1992 birth cohorts at each parental income percentile. We estimate the fitted lines for each race and birth cohort using a lowess regression on the binned series (with bandwidth 0.3). See Section II for details on the sample construction and variable definitions.

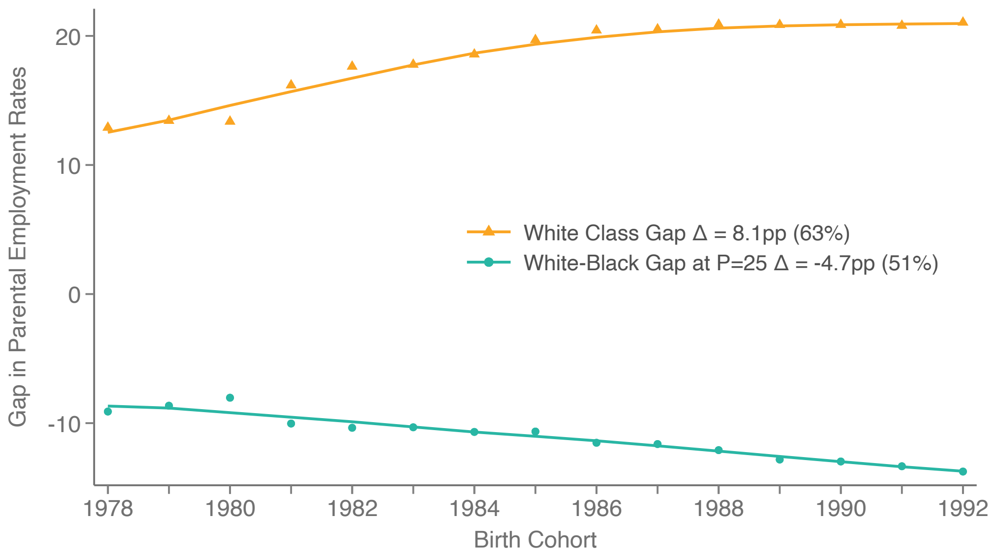
FIGURE A.27
Changes in Children's Household Income in Adulthood versus Changes in Parental Employment: Alternative Smoothing Approach



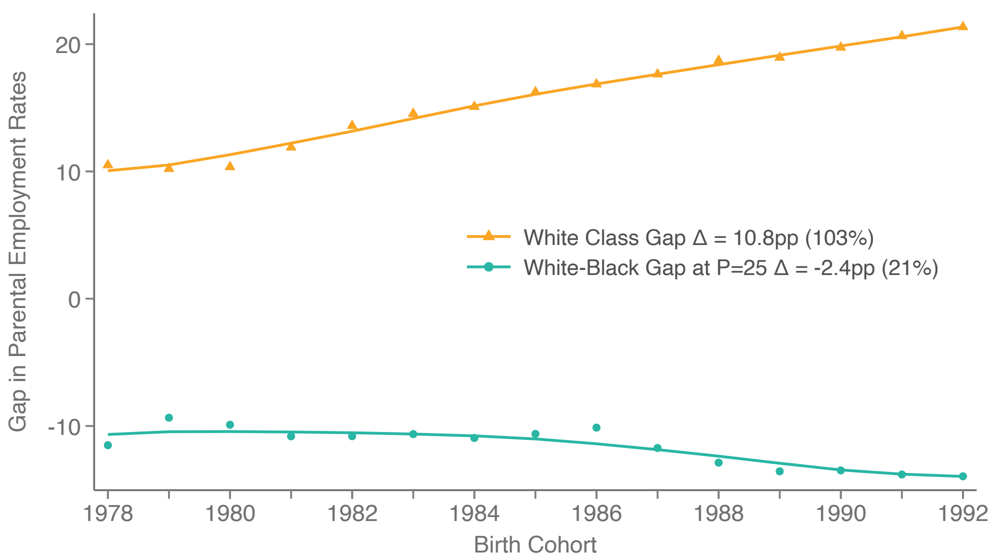
Notes: This figure shows a binned scatterplot of changes in children's household income rank in adulthood versus changes in parental employment rates using the alternative smoothing approach described in Appendix A. We also report the slope and standard error of the weighted best-fit line estimated in an OLS regression, where we weight by the number of children in each county-by-race-by-class cell. We restrict to counties with more than 2,000 children in the relevant race and parental income group (split by above- or below-median household income) across the 1978 and 1992 birth cohorts. See Appendix A for details on how we construct the county-by-race-by-class-by-cohort estimates for each variable and Section II for details on the sample construction and variable definitions.

FIGURE A.28
White Class and White-Black Race Gaps in Parental Employment Rates by Parental Education

A. Less Than a Four-Year College Degree



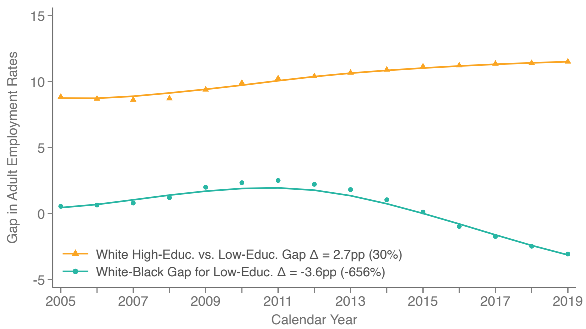
B. Four-Year College Degree or More



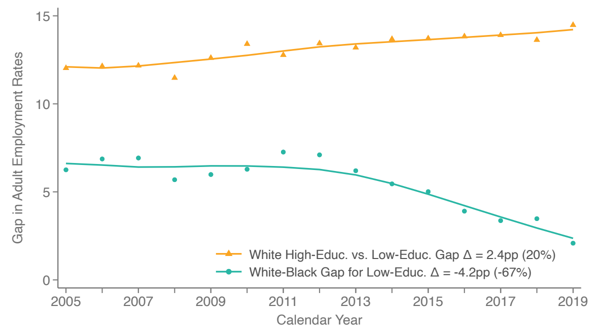
Notes: These figures plot the white class and white-Black race gaps for parental employment rates by parental education. Panel A plots results for families where no parent has a four-year degree; Panel B plots results for families where at least one parent has a four-year college degree or more. We also report the percentage point and percent changes in the white class and white-Black race gaps between the 1978 and 1992 birth cohorts. We estimate parental employment rates using fitted values from a lowess regression on parental income percentiles for each race, birth cohort, and parental education group. We take the difference in these fitted values to compute the white class and white-Black race gaps. See Figure I for additional details on how we construct the estimates and Section II for details on the sample construction and variable definitions.

FIGURE A.29
Gaps in Employment Rates by Race and Education: Tax Data versus ACS Data

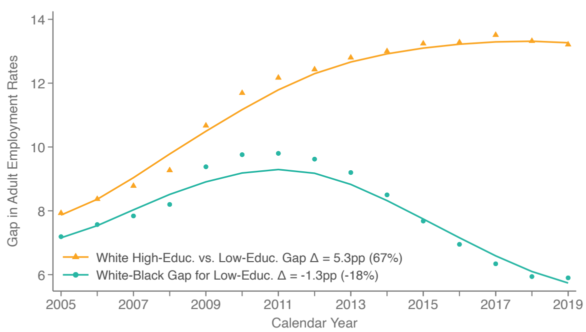
A. Female Adults, Tax Data



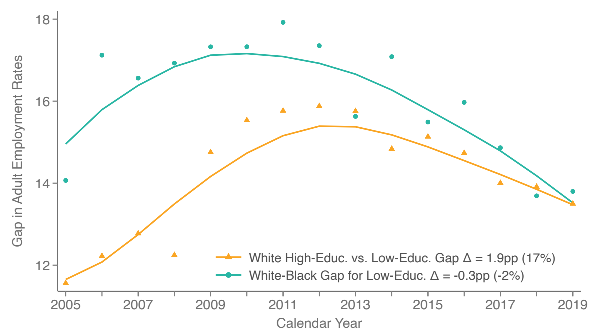
B. Female Adults, ACS Data



C. Male Adults, Tax Data



D. Male Adults, ACS Data



Notes: These figures plot the white class and white-Black race gaps for employment rates among adults ages 48-57 between calendar years 2005 and 2019 when defining class using parental education instead of our baseline definition using parental income. We define the white class gap as the gap in employment rates among white adults with at least a four-year college degree versus those with less than a four-year college degree. We define the white-Black race gap as the gap in employment rates among white versus Black adults with less than a four-year college degree. Panels A and C plot results for women and men, respectively, in the tax data who can be matched to the ACS data. Here, we define employment rate as the fraction of adults ages 48-57 working in a given year based on the tax data. Panels B and D plot results for women and men, respectively, who appear only in the ACS data. We alternatively define employment rate as the fraction of adults ages 48-57 working in a given year based on the ACS data. We also report the percentage point and percent changes in the white class and white-Black race gaps between calendar years 2005 and 2019. See Appendix B for additional details on how we construct the estimates and details on the sample construction and variable definitions.