The Baby Boom and Black Economic Progress*

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November 3, 2025

Abstract

The Black-White earnings gap closed into the 1970s, but then widened as the Baby Boomers entered the labor market. I use cross-cohort variation in fertility rates to estimate the effect of the Baby Boom, which was 50% larger for Blacks than Whites, on racial disparities. This increased cohort size caused an increase in the lifetime Black-White median earnings gap by lowering the average Black rank on the earnings distribution. Had Black cohort size increased at the same rate as Whites, the Black Baby Boom generation would have had 14.7% higher life-cycle earnings, and the Black-White lifetime earnings gap would have closed rather than widened. Using a state-level panel, I find that increased cohort sizes caused crowding-out in college markets and high-manufacturing, high-union labor markets, and increased congestion in traditionally low-education, low-earning, and Black occupations.

*I thank Timothy Bond, Kasey Buckles, William Evans, and seminar and brown bag participants at Cornell University and the University of Notre Dame for their thoughtful comments. I also thank Andrew McKenna for his fantastic assistance digitizing Vital Statistics data.

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1 Introduction

The Black-White median earnings gap in the United States closed substantially through the mid-20th Century and reversed course thereafter (Figure 1). Most previous research on this phenomenon has focused on demand-side explanations including deindustrialization and higher income inequality from rising returns to education.[1] What is surprising about the trend reversal is that it was shouldered by the Baby Boom generation, born between 1946 and 1964 (Figure 2). If we consider the Black-White median earnings gap within birth year instead of pooling across ages, we see that birth cohorts leading up through the 1940s continued to make substantial gains in closing the earnings gap through their lifetime; it is not until the Baby Boomers that we see a stark turn downward. This is especially striking given that these cohorts were the most educated Black cohorts of all time, and they were the first to enter the labor market after segregation ended.

However, they were also the largest, with about 75 million people born between 1946 and 1964. The Black population increased substantially more than the White population during this time, with fertility rates growing about 50% more (Figure 3). Overlaying the birth cohort earnings gap with fertility rates in year of birth presents a near perfect mirror image (Figure 4). It is possible that the substantial gains made through the eras of the Civil Rights Movement and the Great Migration encountered strain from these especially large cohorts, both White and Black.

In this paper, I assess the impact of the Baby Boom on the Black-White median earnings gap for those born during it. Using variation in fertility rates in both national and state-level approaches, I show that increased cohort size reduces earnings and employment for both Black and White men, but the marginal impacts for Black men are roughly twice as large. Every 100 additional cohort members per potential workforce leads to a 6.6 log point decrease in cohort earnings for prime working age White men (7.3 among earners), and a 15.8 log point decrease for Black men (12.5 among earners). The results are robust to considering alternative outcomes like mean earnings. Effects are larger at younger ages (25-39) compared to older ages (40-54). When estimating separately for the Northeast, Midwest, South, and West, I find that trends are consistent across regions, with somewhat smaller effects for Black men in the South.

With my main results, I estimate counterfactual life-cycle earnings if the Black cohort size had matched the White during the Baby Boom, or if both races had cohort sizes equal to their counterparts born in the 1930s. Results show that that cumulative life-cycle earnings would have been 14.7 to 19.9% higher for Black men and up to 4.9% higher for White

[1] See, e.g., Gould (2021); Batistich and Bond (2023); Bayer and Charles (2018).

men born between 1946 and 1964. The lifetime earnings gap widened between 1930s birth cohorts and Baby Boomer cohorts by 6.5%. Under the counterfactuals, this would have instead closed by 7.6 to 12.4%.

Cohort size may affect earnings through different mechanisms, some of which are race-neutral. One is congestion. Young workers are not necessarily substitutable with older workers within a firm, and firms may not perfectly adapt their production processes to accommodate shifting intensities of differently experienced workers. Young workers then compete with each other, driving down their own relative wages (Card and Lemieux, 2001a; Welch, 1979). This phenomenon has been studied in the context of the Baby Boom as well as other settings like immigration (Borjas, 2003). Another possible reason is that larger cohort sizes may lead to crowding out in inelastic markets. For example, institutions of higher education may not be able to perfectly absorb demand from larger cohorts (Bound and Turner, 2007). An example in the labor market could be unions, if barriers to union employment mean larger supply of non-unionized workers (Neumark and Wachter, 1995). In both the college and union examples, crowding out would cause increased supply in non-college and non-unionized markets, potentially reducing wages in those markets. A race-neutral cause for differential impacts by race is if different parts of the socioeconomic distribution are affected. We know that due to historical structural barriers, Blacks and Whites are concentrated in different parts of the overall earnings distribution, and it is possible that the cohort size increases did not have uniform impacts across that distribution.

Cohort effects may also be race-biased for a number of reasons. First, it is the case that the increase in cohort size for the Black population was about 50% larger during the Baby Boom. This may have led to increased congestion in markets where the Black population was especially concentrated, such as particular occupations or geographical areas. Second, discrimination within firms and other institutions may make them less likely to accommodate larger Black cohorts. For example, affirmative action policies within universities may consider the share of a minority group within their institution but do not necessarily reflect the racial makeup of the cohort or the population as whole. Third, given that the White cohort size increased substantially from the Baby Boom, it could be that given the baseline political and socioeconomic standing of the Black population, excess Whites may take jobs that a Black person may otherwise have taken.

Using my national-level approach, I explore the market channels underlying the resulting increase in the Black-White earnings gap. Taking direction from Bayer and Charles (2018), I consider that an increase in the level earnings gap can occur through a race-neutral widening of the earnings distribution, and/or through a race-biased movement along the earnings distribution that further separates where the Black and White medians lie. I explicitly test

the impact of cohort size on the spread of the earnings distribution and the rank of the median Black man on the White male earnings distribution to inform us on the role of each respective channel. In these tests, I find evidence that increased cohort size led to a widening of the earnings distribution, but not at the points where the Black and White medians were located. Instead, I see that an increase in cohort size leads to a meaningful lowering of the Black median ranking on the earnings distribution. That is, the impact of cohort size on the widening of the Black-White median earnings gap occurs in a race-biased manner.

I rule out compositional effects as a competing explanation. In this possibility, the increased fertility rates during the Baby Boom could have been concentrated in certain parts of the socioeconomic distribution, for example among especially low-income Black families and higher-income White families. In such a case, the ex ante earnings potential of the excess cohort members may be lower for the Black population (such as due to differences in home environment), resulting in a wider earnings gap. By estimating a correlation between cohort size and the cohort's median childhood household income percentile, I find no evidence of this explanation.

I also estimate the elasticities of substitution between Black and White workers and across age groups. I do this using a fully nested constant elasticity of substitution (CES) model in the style of Ottaviano and Peri (2012). Using 36 education-experience bins and 8 Census years, I find a substitution elasticity between Black and White workers around 20, similar to what Ottaviano and Peri (2012) find studying immigrants and natives in the U.S. from 1960 to 2006. I also find that Black and White workers are less substitutable at younger ages and among Northern educated workers. My estimate for Northern educated workers are comparable to what Boustan (2009) finds when studying Northern labor markets during the Great Migration. Using pooled race samples to estimate age elasticities, I find that age groups are imperfectly substitutable and that they have an especially low elasticity when restricting to 1960-1990, the years that the Baby Boomers were young labor market participants.

Using state-level variation, I confirm my main findings on employment and earnings and test specific mechanisms including college completion. I find that every 100 additional cohort members per potential workforce reduces Black male college degree completion by 1.7 percentage points, Black female college completion by 1.4 percentage points, and White male college degree completion by 1.6 percentage points, with a smaller and insignificant impact for White women. Incorporating a measure of college market tightness by state using historical college enrollment counts, I find that cohort size reduces college attendance and completion more when the college market is tighter. This is evidence in support of a college crowding-out story, where some in larger cohorts do not go to college when they

otherwise would have.

I also explore the role of unions and manufacturing. Using baseline rates of manufacturing by state, I estimate that impacts of cohort size on employment and earnings are larger in manufacturing-heavy states, and that these differential impacts are stronger for the Black population. We would not expect larger cohorts to have a causal impact on the secular decline in manufacturing taking place during this time. Instead, we might expect larger cohorts to experience stronger impacts in manufacturing-heavy states when confronted with constraints such as a strong union presence or an import competition shock. I do not find a large or significant impact of cohort size on the manufacturing share of employment, but I do find that states with higher historical unionization rates and higher exposure to Japanese import competition had larger effects of cohort size on employment, and that these effects channeled through large reductions in the manufacturing share of the population with smaller take-up of non-manufacturing employment.

Considering more broadly the impacts of cohort size on the type of employment the cohort takes up, I evaluate their share of employment in traditionally high- or low-earning, high- or low-education, and high- or low-Black-share occupations. I find that increased cohort size reduces the share of their employment in traditionally high-earning, high-education and White occupations. It also increases their share of employment in traditionally low-earning, low-education and Black occupations. Bringing together these findings with the results on college markets and manufacturing employment, I turn to earnings within occupations. I find no reductions to a cohort's wages in high-education, high-quality, and traditionally White occupations or in manufacturing, and substantial decreases in their wages in low-education, low-quality, and traditionally Black occupations and non-manufacturing industries.

The Baby Boomers were generally the children of the Great Migrants, so there was already an increased presence of Black workers in the North when the Baby Boomers entered the labor market. Boustan (2009) finds that Black wages in the North would have been around 7% higher in 1970 if not for the migrant influx, with no change to White wages. My results compare changes in earnings for a specific cohort relative to others in the labor market at the same time, and are thus separate and additional to impacts directly caused by the Great Migration. I find that impacts of cohort size are somewhat larger outside the South, and this may be because these markets were already more competitive from in-migration. Derenoncourt (2022) finds that Great Migration recipient commuting zones experience reductions in upward mobility for birth cohorts in the 1980s, the grandchildren of the Great Migrants. My analysis instead focuses on the children of the Great Migrants and their contemporaries, and looks at individual, within-cohort outcomes rather than broader commuting zone characteristics.

This paper contributes primarily to three major areas of the literature. First, it contributes to the literature on the impacts of cohort size on economic outcomes. A large body of literature has studied this in the context of the Baby Boom, with a particular focus on youth and early adult markets including college, marriage, employment and even crime (e.g., Card and Lemieux, 2001b; Welch, 1979; Bronson and Mazzocco, 2024; Jacobson, 2004). Much less attention has been paid to differential impacts by race. Previous work has been limited only to short term effects on young adults (Leveson, 1980; Wachter and Kim, 1982).

Second, this paper contributes to the literature on the causes of the widening of the Black-White earnings gap beginning in the mid-1970s. A wide body of evidence supports demand-side explanations for this trend reversal. Batistich and Bond (2023) show that increased import competition from Japan reduced employment and earnings primarily for Black men through an upskilling of the manufacturing sector. Other research has likewise pointed to industrial shifts during this time period playing a key role in the decline in Black outcomes via a decrease in labor demand (e.g., Acs and Danziger, 1993; Bound and Holzer, 1993, 2000). Gould (2021) shows that a decrease in demand for manufacturing labor played out especially hard for Black communities. Some research has looked into more specific mechanisms such as suburbanization of jobs (Kain, 1968; Zax and Kain, 1996; Mouw, 2000; Weinberg, 2004; Miller, 2023) and the dramatic rise in incarceration, especially for Black males (Neal and Rick, 2014; Evans et al., 2016; Western, 2002; Holzer et al., 2005). Another explanation comes from rising inequality more generally, particularly due to computerization and rising returns to skill (Bayer and Charles, 2018). In contrast, far fewer studies have investigated supply-side explanations, and these have focused on within-individual changes in the labor supply curve (Mead, 1986; Wilson, 1987). My paper proposes a simpler supply-side story: There was an increase in the supply of workers, and labor demand did not rise to meet it.

Finally, this paper contributes to the literature on the causes of rising earnings inequality in general (Katz and Murphy, 1992; Card and Lemieux, 2001a; Autor et al., 2008). Others have explored the role of the economy's changing age structure due to the Baby Boom in explaining aggregate earnings inequality (Almås et al., 2011). My paper assesses the impact of increased cohort size on the earnings distribution within the cohort, finding a causal widening between the top quartile and the median, but not the median and lower quartile.

The rest of the paper proceeds as follows. In Section 2, I describe my data sources and treatment and the construction of my analysis samples. In Section 3, I explain my empirical approach for the national and state-level analyses. In Section 4, I describe my results, including my headline national and regional level results, analysis of spread versus rank channels, substitution elasticity estimates, state-level results, and tests for crowding out and congestion mechanisms. In Section 5, I conclude.

2 Data

2.1 Data Sources

I use the full count United States Decennial Census data for the years 1910 through 1950, the 1960 5% Census sample, both 1970 1% state samples, the 1980, 1990, and 2000 5% samples, and the 2012 and 2022 5-year American Community Survey (ACS) samples, available through Integrated Public Use Microdata Series (IPUMS) (Ruggles et al., 2025, 2024). I use the 1950 1% sample for income variables for 1950 instead of the full count Census because according to IPUMS, the values for personal income in the full count version are often erroneous.[2] My instrument uses fertility counts by state and race from U.S. Vital Statistics annual reports dating back to 1917. Prior to 1933, not all continental states reported births. When conducting the state-level analysis described in Section 3.4, I fill in missing states in the early years with quasi-fertility rates calculated from the Census, which only account for about 1.5% of the sample's fertility rates. More details on the fertility data and my instrument are in Appendix A.1.1.

Throughout, because the Census is conducted early in the year, I define birth year in Census samples as Census year minus Census-reported age minus one, and then define age as Census year minus birth year. For the ACS samples, which are conducted throughout the year, I calculate birth year as sample year minus age for birth quarters 1 and 2, and sample year minus age minus one for birth quarters 3 and 4.

2.2 National and State-Level Analysis Samples

At the national level I analyze birth cohorts 1917 through 1970. Each birth year cohort is observed during prime working age years in three different Census years. I do not include birth cohorts past 1970 because we do not observe their full life cycle of earnings, and those born during those years are the descendants of Baby Boomers so are endogenously affected by the effects of the Baby Boom. My main analysis sample is U.S.-born men ages 25-54 identifying as Black or non-Hispanic White.[3] I also include women separately for analysis of educational attainment. Starting in the year 2000, multiple races are recorded in the Census and ACS samples. My main sample includes all White, non-Black, non-Hispanic individuals

[2]IPUMS is continually improving the quality of these datasets. See `https://usa.ipums.org/usa/full_count.shtml` for updated information on this preliminary release.

[3]In 1950, Hispanic is not available as variable so I just use all U.S.-born White men. According to a Census report, only about 2.28 million people living in California, New Mexico, Colorado, Texas, and California in 1950 had a Spanish surname, or about 1.5% of the U.S. population (U.S. Bureau of the Census. U.S. Census of Population: 1950. Vol. IV, Special Reports, Part 3, Chapter C, Persons of Spanish Surname. U.S. Government Printing Office, Washington, D.C., 1953.)

as White and all non-White Black individuals as Black. Results using the more restrictive single-race variables are reported in Appendix Section A.3.

At the state level I analyze birth cohorts 1926 through 1970. I do not use the 1950 Census since the 1% sample is too small a sample for state-level variation in my context. The cohorts born 1926-1935 are 25 to 34 years old in the 1960 Census, making them the earliest born cohorts for whom I can still see a full life cycle of three observations using the 1960, 1970, and 1980 Censuses. My unit of analysis is state by year by birth cohort within gender. I restrict my sample to states which have a population count of at least 300 in each year-birthyear-race-gender sample cell. This leaves me with 27 states.[4]

For earnings I take the sum of wage and salary income, business income, and farm income. Each of these is winsorized at the 1st and 99th percentile by year to address bottom and topcoding in a consistent way over time. I convert nominal dollars to 2024 U.S. Dollars using the Consumer Price Index available through the Federal Reserve Bank of Minneapolis. I calculate median earnings for all-working age men, men with earnings, and men working full-time full-year. Full-time full-year is defined as those working at least 50 weeks last year and who worked at least 35 hours last week in the 1950 to 1990 Censuses or who usually work at least 35 hours per week in the 2000 Census and later.

National level summary statistics are shown in the first three columns of Table 1. Key outcomes of interest include log median earnings for all prime working-age men, for earners and for full-time, full-year workers. I focus on medians because they are less sensitive to outliers or skewness in the distribution. Medians including all working-age males, using 0 as placeholders for those with no earnings, gives a more complete picture of economic standing. Particularly given the high and evolving non-employment and incarceration rates during the past century, which also vary by race, excluding non-earners biases the earnings outcome (Bayer and Charles, 2018; Vigdor, 2006; Neal and Johnson, 1996). To learn the impacts on the intensive margin of earnings, I also include earnings among earners and earnings among full-time, full-year workers. Results using mean log earnings are also shown in the Appendix.[5] We see that the level earnings gap is larger when considering all prime working age males than when considering earners or full-time full-year workers, and this is because there is also a racial gap in employment.

The states omitted due to insufficient data are relatively low in population, thus it is unsurprising that the state-level summary statistics shown in Columns (4) through (6) of Table 1 look similar to the national level. The state sample has slightly lower employment

[4]I show main results limiting the sample restricted to years where Vital Statistics fertility rates are available, and results using the full unbalanced panel in Appendix Table A.18.

[5]See Table A.15.

and labor force participation shares, and a wider gap for each. However, the confidence intervals between national and the state-level samples indicate they are not statistically different.

3 Empirical Strategy

3.1 National and State Level Approaches

To test the impact of cohort size on the earnings gap and other outcomes, I use two main approaches. First, I estimate a national-level regression model considering nationwide cohort sizes and outcomes. This approach has the advantage of a longer sample period and avoids any endogeneity arising from interstate migration in response to effects of cohort size. I use this approach as my main strategy for estimating overall impacts of cohort size on the Black-White earnings gap including my counterfactual estimates. I also use this approach to explore the market mechanisms underlying the effects. In particular, I test the impacts of cohort size on the spread of the earnings distribution and on Black rank along the earnings distribution. I also estimate substitution elasticities between Black and White workers and workers of different ages using national level mean earnings.

My second main approach uses a state-level panel with cross-state variation in cohort size for each birth year. This approach has a shorter window of analysis due to data constraints, but it is free from any endogeneity due to national level shocks that influence fertility rate in a given year and the birth cohort's subsequent labor market outcomes. I use this approach to confirm the national-level estimates. The state-level panel is also a useful environment for exploration of structural mechanisms, including crowding out in college markets and highly unionized and import competition-exposed labor markets. I also use the state-level panel to explore heterogeneity in the effects by geographic region. I describe both estimation strategies below.

3.2 National Level Estimating Equation

The main estimating equation for outcome y for race r and cohort c in year t is

$$y_{rct} = \beta_0 + \beta_1 * \widehat{\text{Cohort Size}}_{rc} + \gamma_t + \lambda_a + \epsilon_{rct} \tag{1}$$

including Census year fixed effects γ_t and age fixed effects λ_a. Cohort size is defined as

$$\text{Cohort Size}_{rc} = 100 * \frac{\text{Cohort Total at Age 17}_{rc}}{\text{Total Potential Workforce Ages 18-64}_{rc}} \tag{2}$$

It is the number of own-race (or all-race) individuals in the relevant birth year measured at age 17, divided by the size of the own-race (or all-race) potential workforce (defined as adults ages 18-64) measured in the same year. For those turning 17 in intercensal years, I take a weighted average of the Census observations before and after their 17th birthday based on proximity to their 17th birthday. For example, for those who turn 17 in 1921, I use 0.9 times their birth cohort's count in 1920 plus 0.1 times their birth cohort's count in 1930. I similarly calculate the denominator for intercensal years as a weighted average. The cohort variable represents the cohort size for a given birth year relative to the size of the labor market they are about to enter. It also represents their cohort size relative to the mean size of the 47 preceding cohorts. Within the sampling frame, this variable for Whites ranges from 2.34 to 3.63 with a mean of 2.99, and for Blacks, it ranges from 2.78 to 4.73 with a mean of 3.63. The all-race cohort size ranges from 2.32 to 3.60 with a mean of 2.94.

3.3 Instrument

Because cohort size at age 17 may be influenced by economic factors that will affect labor market outcomes for that cohort in the subsequent years, I use as an instrument the fertility rate for the cohort during the year they were born. The instrument addresses potential endogeneity of the explanatory variable such as a smaller cohort size at age 17 due to outmigration in response to poor economic conditions (biasing results toward zero). It also addresses measurement error in the explanatory variable. The instrument for race r and birth year cohort c is

$$\text{Fertility Rate}_{rc} = 1000 * \frac{\text{Births}_{rc}}{\text{Women Ages 15-44}_{rc}} \tag{3}$$

where race r is Black, White, or All-Race depending on context. The numerator is birth counts from U.S. Vital Statistics reports, while the denominator comes from Census data. To calculate intercensal childbearing age female cohorts, I take a weighted average within state depending on proximity to each Census year, in the same way I do for cohort size. The strategy of using fertility rate in year of birth as an instrument is used elsewhere to study cohort size effects (e.g., Bronson and Mazzocco, 2024; Jacobson, 2004; Shimer, 2001).

3.4 State Level Approach

The main estimating equation for outcome y for cohort c in state s and year t is

$$y_{rcst} = \beta_0 + \beta_1 * \widehat{\text{Cohort Size}}_{rcs} + \gamma_t + \lambda_a + \eta_s + \epsilon_{rcst}. \tag{4}$$

This is similar to the national level approach but now with state level variation in cohort size and outcomes. I include year (γ_t), age (λ_a), and state (η_s) fixed effects. The coefficient of interest is β_1. Regressions are run separately for Black and White (male or female) groups and weighted by the population in each cell. As in the national approach, I instrument for cohort size using fertility rates, but here at the race by state level. As described in Section 2.2, the sample includes 27 states. Given 45 cohorts and 3 observed Census years for each cohort, this gives me 3,645 observations. In Appendix Figures A.11 and A.12 I show variation in cohort size and fertility rates across states. Because I only have 27 states in my sample, I use wild cluster bootstrapped standard errors clustered at the state level.[6]

4 Results

4.1 National Level Results

The results from a two-stage least squares (2SLS) estimation of Equation 1 on earnings and employment are shown in Table 2. Panel A shows results for Black men while Panel B shows the results for White men. In Column (1), we see that a one unit increase in cohort size (i.e., an increase of 100 cohort members relative to the potential workforce at age 17) leads to a 15.8 log point decrease in median earnings for Black men, and a 6.6 log point decrease for White men. We also see a decrease in earnings among earners in Column (2), and among full-time, full-year workers in Column (3). In each case the marginal impacts are roughly twice the size for the Black population, and the estimates for all are highly statistically significant, with p-values below 0.01 for each. In Column (5), we see that cohort size also affects employment, with a one unit increase in cohort size leading to a decrease in the employed share of the Black population by 3.1 percentage points and the Black labor force participation share by 1.8 percentage points. For Whites, we see a decrease by 0.4 percentage points and no discernible impact on labor force participation.

In sum, these results indicate that the marginal impacts of increased birth cohort size leads to substantial earnings and employment losses, and that the impacts are roughly twice as big for the Black population. The ordinary least squares specifications, for which results are shown in Appendix Table A.7, look very similar with slightly smaller coefficients and similar standard errors to the 2SLS specification.[7]

[6] I estimate these standard errors using the post-estimation command "boottest" in Stata. This command estimates a wild restricted efficient bootstrap for instrumental variable models.

[7] Given structural differences in labor market opportunities by race, I use the race-specific cohort measure throughout for analyzing race-specific outcomes. However, the results look similar when considering an all-race cohort size measure, shown in Appendix Table A.8. For the Black-White gap as the dependent variable, see Appendix Table A.9, and for a fully interacted stacked difference-in-differences model, see Appendix

Note that the Black mean cohort size is larger; it could be the case that nonlinearities in the impacts of cohort size are driving the difference in coefficients. In other words, it could be simply that larger cohorts experience larger impacts regardless of race and that the underlying functional form does not differ. When I estimate a fully flexible quartic specification, a Wald test cannot reject the null that the four coefficients of the quartic are the same; thus it is possible that the larger marginal impacts are due differences in the domain of cohort size by race.[8] Either way, realized marginal impacts of cohort size increases during this time period were bigger for the Black population and contributed to widening the earnings gap. A one unit increase in cohort size widens the Black-White median earnings gap by 7.7 or 9.2 log points when considering a race-neutral or race-specific cohort size, respectively.[9]

4.2 Migration and Regional Variation

The Baby Boom presented an "excess" of 28.9 million births between 1946 and 1964, 4.6 million of which were Black.[10] The second wave of the Great Migration occurred between 1940 and 1970, where four million Black migrants moved from the South to the North and West (Boustan, 2009). Other scholars have identified the impacts of the Great Migration on Northern labor markets including suppressed Black earnings and reduced mobility for the next generation (Boustan, 2009; Derenoncourt, 2022). Given the comparable magnitude and overlap in timing of these two major demographic shifts, I want to ensure that my findings are not being driven by migration.

During my analysis years, being in a larger birth cohort is correlated with the likelihood of being born and living in the North.[11] Therefore, one potential threat to my estimation strategy is that a regional composition effect is driving my results. To test for this directly, I estimate Equation 1 separately for each of the four Census divisions: Northeast, Midwest, South, and West. I show these results in Tables 3 and 4. We see that there are substantial impacts on a cohort's earnings and employment in response to cohort size across all four regions for both races, and all regions see an increase in the cohort Black-White earnings

Table A.10. Outcomes using alternative race definitions are shown in Appendix Table A.11. I also show outcomes for women in Appendix Table A.12 and outcomes by educational attainment in Appendix Table A.13.

[8] See Appendix Table A.14.

[9] See these results in Appendix Tables A.9 and A.10.

[10] I calculate excess births as the difference between actual births 1946-1964 and the births that would have occurred if the fertility rate had been at its minimum value between 1930 and 1939 but the number of women ages 15-44 was the same.

[11] In my data, 84 percent of the 1917 birth cohort measured at age 17 were still in the South, versus only 55 percent of the 1960 cohort.

gap. Still, impacts are somewhat smaller for Black men in the South than in the other regions, particularly for employment and log median earnings among all men.

Migration from the South to the other regions will cause the cohort size variable to be larger in the South than elsewhere, because outmigration will reduce the denominator. My fertility rate instrument will account for these differences, because the identified effect is only the impact of the increase in cohort size that can be predicted by fertility rate, and not any in- or outmigration inflating or deflating the cohort size denominator between birth and age 17. However, I further test for any possible confounding effects from migration using my state-level panel, with an alternative instrument that predicts cohort size based on state-level fertility rates in addition to predicted in-migration from the South using the approach introduced in Boustan (2009) and Derenoncourt (2022). The results, which can be found in Appendix A.1.2, look very similar in Northern states under this approach, suggesting that the different marginal effects by region are rooted in economic mechanisms rather than a measurement difference. I explore mechanisms further in Section 4.7.

4.3 Spread, Rank, and Supply Side Explanations

The analyses in Sections 4.1 and 4.2 imply there are distributional impacts of cohort size: either a movement along the earnings distribution for the median Black or White so that they are further apart, or a change in the dispersion of the distribution itself. We know that due to historical differences in education and labor market opportunities, Black and White medians occupy different places on the distribution. As shown in Bayer and Charles (2018) the median Black male generally falls around the 24th to 27th percentile on the White male earnings distribution, and it has remained in this ballpark for at least the past 80 years. It is possible that the widening of the earnings gap is due to cohort size causing a widening of the earnings distribution between the first quartile and the median, so that the level difference has increased. This would be a race-neutral effect. It is also possible that the increase in cohort size caused the median Black male to fall further down the earnings distribution so that it is further away from the White median. This would be a race-biased effect.

I explore these possibilities in Table 5. For this estimation I use the all-race cohort size measure since we are considering overall economic impacts. We first consider changes in the spread of the earnings distribution. In Column (1), we see that increased cohort size has potentially a positive but statistically insignificant impact on the level distance between the 25th and 75th percentile. Turning to Column (2), we see no impact of an increase in cohort size on the distance between the 25th and 50th percentile, which is where we would expect to see a widening if the racial earnings gap result was driven by a change in spread. The standard

error implies that the estimate is not very precise, but the coefficient is negative. Instead, we see that the widening of the earnings distribution is located in the upper percentiles, with a substantial widening between the 75th and 50th percentile in Column (3) and between the 90th and 50th percentile in Column (4). These results inform us that the increases in cohort size did lead to a widening of the earnings distribution between the top and the middle, but not between the Black and White medians.

Instead, in Column (5), we see a clear impact of cohort size on Black rank: A one unit increase in cohort size lowers the Black rank 1.9 percentiles from the White median, a sufficient magnitude to explain all of the earnings gap widening at the median.[12] There is also an impact for 90th percentile Black male, where in Column (6) we see that a one unit increase in cohort size leads to a 1.6 percentile decrease in his location on the White earnings distribution. In total, the results from Table 5 tell us that the increased cohort size widened the Black-White median earnings gap through a lowering of the Black rank on the earnings distribution rather than through a widening of the earnings distribution itself.[13]

Aside from the demand-side possibilities of spread and rank, a third competing explanation for this finding comes from composition on the supply side. It could be that the increase in cohort size was driven by fertility of women of particular socioeconomic status in a way that affected the ex ante opportunities for the cohort. One example is if the excess Baby Boom births were concentrated in especially low-income Black families and higher-income White families. In such a case, the larger Black cohort would contain more low-opportunity individuals who might be lower earning regardless of demand side effects. I explicitly test for this possibility in Table 6. In this test I estimate the correlation between fertility rate in the year the cohort was born and the median child's subsequent childhood household income.[14] I simply regress the median child's parental income on the fertility rate in the year they were

[12]Using the White male earnings distribution in the 1980 Census as a representative example: Starting from the sample mean of 22.744, a movement down 1.875 percentiles on the White male earnings distribution is a decrease of $\$3,160$ or 9.7 log points. A movement down 1.875 percentiles from the 25th percentile on this distribution is $\$3,350$ or 9.3 log points. Given that a one unit increase in the all-race cohort size leads to a 7.7 log point widening of the Black-White earnings gap as shown in Table A.9, the rank mechanism explains all of the widening of the gap.

[13]Bayer and Charles (2018) conduct a decomposition of the median earnings gap from 1940 to 2010, separating rank (race-biased) and spread (race-neutral) components. They find that the median Black rank approaches the White median rapidly in the 1960s and 1970s (from the 18th to 26th percentile), stalling in the 1980s and 1990s at the 26th percentile, with a slight closure in the 2000s (up to the 28th percentile). The Baby Boom generation was most present in the labor force during the 1990s and 2000s when this stalling happened; my results imply that the rank would have continued to improve during this time in the absence of the large cohort shock. Bayer and Charles (2018) find that the level median earnings gap widening beginning after 1980 was due entirely to a race-neutral spread of the distribution and not loss in Black rank. While the Baby Boom generation was a part of this distributional widening, my findings indicate that cohort size did not play a role in that component of the level earnings gap increase.

[14]Details on the construction of the median childhood household income percentile are in Appendix A.1.3.

born. Because children are captured at different ages in the Census (I use ages 8-17), I also include fixed effects for age at which the parental income was measured.

The results in Table 6 show that there is no correlation between fertility rate and where the median child falls in the household income distribution either overall or by race. In Column (4) I test the impact of the fertility rate on the distance between the Black and White median child on the household income distribution, and we likewise do not see an effect. This evidence does not support a "lumpy" cohort growth story. While this test is limited in sample size, with only 38 observations, I present the full childhood household earnings distribution for children from different birth cohorts in Appendix A.1.3, which show that the distribution is quite stable over time.[15]

4.4 Substitutability of Black and White (and Young and Old) Workers

The prior section indicates that the impacts of cohort size on labor market outcomes are race-biased, implying an imperfect substitutability of Black and White workers vis-à-vis firms and other institutions. We also know from the Economics literature that young and old workers are not perfect substitutes, and that this may cause larger cohorts to experience suppressed earnings relative to other age groups (Card and Lemieux, 2001a). To this discussion I add an estimation of elasticities between Black and White workers and across workers of different age groups in the fashion of Ottaviano and Peri (2012). Details on this estimation procedure can be found in Appendix A.1.4. I use six education bins (incorporating region of education), six five-year age bins, and eight Census years. I show a summary of the results in Figure 5.[16]

In panel a. we see that Black and White workers have an elasticity of about 20, which is similar to the result for native and foreign-born workers in Ottaviano and Peri (2012). We see that Black and White workers are less substitutable at younger ages. This could perhaps be due to (1) actual skills being more different in young adulthood and converging as one ages, (2) statistical discrimination lessening with learning over the life cycle, or (3) an increasingly selected earnings sample in later years as people start to drop out of the labor force. We also see that Black and White workers are somewhat less substitutable among higher educated groups and Northern educated groups. I estimate that the Black-White elasticity among the Northern educated is about 11.5, only slightly higher than what Boustan (2009) estimates for the Black-White elasticity in Northern labor markets during the second wave of the Great

[15] There is a slight movement upward in the household earnings distribution among Black children, reflective of economic progress made by parents of the Baby Boomers. This movement works in the opposite direction of what would be expected for a compositional explanation of the widening of the earnings gap.

[16] Complete results are shown in Appendix Tables A.4 through A.6.

Migration.

In panel b. I show the age elasticity results, which use average wages for the whole age-education bin (combining Black and White men). We see that age group elasticity is slightly higher Black-White elasticity, but less precisely estimated. Within education subgroups, those with lower education are relatively less substitutable, suggesting that perhaps returns to experience are higher for that group. We also see that age group elasticity is lower when we remove 2010 and 2020, and that it was especially low when we focus the estimation on 1960-1990, or the years that the Baby Boomer generation was young. Card and Lemieux (2001a) similarly estimate an age group elasticity between 4 and 6 when studying the Baby Boom impacts from 1970 to 1995.

On the one hand, it is reassuring to see these estimates reflect what others have found in the literature. We can confirm that Black and White workers are not perfectly substitutable, and find that this is especially true at young ages. We also see that age group elasticity was especially low during the years that the Baby Boomer generation was establishing their careers. On the other hand, we know that these elasticities rely on earnings from full-time workers where the Baby Boom also led to reductions in employment, meaning these elasticities only show a partial picture.

4.5 Counterfactual Lifetime Earnings

Based on the elasticity estimates in Section 4.4, we would expect the impacts of cohort size to be larger for both races at younger ages, and for the impacts on the Black-White earnings gap to be wider at younger ages as well. In Table 7, I estimate Equation 1 with an interaction for the younger half of the sample, ages 25 to 39. We see that indeed most of the effects are borne by the younger workers, and the earnings gaps are generally wider for this group.

With these results in mind, we turn to two counterfactual exercises. In the first, we consider a scenario in which the Black cohort size had equaled the White. In the second, we consider a more extreme case in which both the Black and White cohort sizes had remained the average size of their counterparts born in the 1930s. To conduct these counterfactuals, I simply estimate Equation 1 adding interactions of cohort size (and its instrument) with age and age squared to trace out the full earnings life-cycle. Using the predicted coefficients, I then use the alternative values for cohort size to calculate counterfactual earnings at each age.[17]

I show the results of these exercises in Figures 6 and 7. Starting with Figure 6, which is the milder counterfactual, we see that the counterfactual Black earnings would be roughly

[17]When pooling the cohorts born 1946-1964, we have two Census observations for each age, so I simply take the mean.

$5,000 per year higher in the earlier parts of the life cycle, with smaller but still substantial increases later in their career. Since there is no change to the White cohort size in this estimation, their counterfactual estimate matches the actual predicted estimate. In Figure 7, we see that Black annual earnings would have been over $10,000 higher in most of the earlier years and then narrowing to a little under $4,000 higher toward the end of the career cycle. For Whites, we see a substantial earnings increase in the counterfactual in earlier years, over $8,000 higher, and then a narrowing over time, approaching the predicted values around age 40.

Based on these counterfactual estimates, I calculate that cumulative lifetime earnings would have been 14.7% higher for Black Baby Boomers if their cohort had grown at the same pace as their White cohort peers, which is about $151,830 in 2024 USD, and the gap would have been 17.7% smaller. If instead the Baby Boom did not occur and both Black and White cohort sizes stayed at the average of the 1930s birth years, we would have seen a 19.9% increase ($205,101) in lifetime earnings for Black Baby Boomer men and a 4.9% increase ($91,566) for White male Baby Boomers. The earnings gap in this case would have been 13.3% smaller. To put these results into perspective, there was an increase in the gap in life-cycle earnings between the 1930s cohorts and the Baby Boomer cohorts of 6.5%. In these counterfactuals we would have instead seen this life-cycle earnings gap close by 8-12%.

4.6 State Panel Results

It is possible that factors increasing fertility systematically affect outcomes for those birth cohorts and not others in the labor market at the same time. For example, a national health shock may lead to more births, and having healthier parents may then improve one's labor market outcomes 25-54 years later. Such an example would *understate* the identified effects from the national approach. State-level variation will address any national policies and trends in health, education, and other domains that would lead to such endogeneity. The state-level panel is also a useful platform for exploring mechanisms, which I do in Section 4.7.

Using the state-level panel, I first reproduce the main results in Table 8. The results show confidence intervals based on state level Wild boostrap clustered standard errors, and the significance stars on the coefficients are also based on the boostrapped cluster.[18] Comparing this to the national level results in Table 2, we see that the marginal impacts for log median earnings in Column (1) are slightly larger for Black men and slightly smaller for White men. The impacts on earnings among earners and full-time, full-year workers are somewhat more

[18]Results showing traditional state-level clustered standard errors are in Appendix Table A.16.

muted than the national results. We still however see a trend that marginal impacts on earnings and employment are substantially larger for Black men. The difference between the two approaches is generally explained by the shorter pre-period in the state-level panel. When I restrict the national sample to match the birth years in the state-level panel, the results look very similar.[19]

The state-level panel includes a small number of observations where the instrument is quasi-fertility rates constructed from the Census. When I omit those observations (leaving the sample unbalanced), the results look quite similar. The state panel also only includes 27 states with sufficient population; when I include the rest of the states, the results also look similar, although some observations are lost for the Black outcomes due to insufficient data.[20]

4.7 Mechanisms

4.7.1 College Crowding Out

One possible mechanism at play is crowding out in higher education. Although colleges and universities would be aware of a larger cohort on the horizon, they may not be able to fully adapt their resources to accommodate the larger cohort, particularly if it is not a permanent change in cohort size.

I estimate impacts on educational attainment for both men and women, using completed education measured between the ages of 30 and 39. In these estimates I include a Census division by year fixed effect to capture regional differences in educational attainment over time. We see in Table 9 that a one unit increase in cohort size leads to a 1.7 percentage point reduction in college degree completion for Black males, with an overall mean for this sample of 10.1%. For Black women, the effect is a 1.4 percentage point reduction. Turning to Whites, we see a one unit increase in cohort size leads to a 1.6 percentage point reduction in college degree completion for men, but no impact for women. In fact, for women we see an increase of any college attainment by 1.7 percentage points. This could potentially be a spillover effect of poorer earnings of the cohort's men.

The impact of cohort size on educational attainment is a combined effect of a number of forces, including responses to changing returns to education, and considerations about timing of labor market entry (e.g., Falaris and Peters, 1992; Stapleton and Young, 1988). To isolate the effect of crowding-out, I introduce a new variable representing a state's college market tightness. I collect data on 1940 college enrollment by state from the National Center

[19] See Appendix Table A.17 for these results.

[20] Results from both of these unbalanced panels are shown in Appendix Table A.18.

for Education Statistics (NCES), and then define college market tightness for cohort c and state s as:

$$\text{College Market Tightness}_{cs} = \frac{\text{Cohort Total at Age 17}_{cs}}{\text{Total College Enrollment}_{1940,s}}. \tag{5}$$

For ease of interpreting the variable, I use an indicator for being at or above the variable's mean. I then interact this term with cohort size to estimate the impacts of cohort size on educational attainment in a tight college market. I show these results in Table 10. We see in Panel A. that a one unit increase in cohort size in a tight college market leads to a 3.3 percentage point larger decrease in any college attainment and 2.2 percentage point larger decrease in college completion for Black men, with comparable results for Black women. For Whites, we likewise see that a one unit increase in cohort size in a tight college market leads to a 5.9 percentage point greater decrease in any college attainment and a 4.5 percentage point greater decrease in college degree completion for men, with respective estimates of -3.3 and -4.3 for women. Comparing to the uninteracted cohort size terms, we see that college market tightness plays a major role in the reduction in college attainment for larger cohorts. We also see some positive impacts of college market tightness on high school completion for Black men and women, with negative impacts for Whites. We do not expect the same type of resource constraints at the high school level, but perhaps these effects represent spillovers in education decisions based on changes in the resulting greater supply of noncollege workers in the cohort.

4.7.2 Manufacturing and Union Crowding Out

We saw substantial impacts of crowding out in the college market in Section 4.7.1. Another market that may exhibit crowding out is in manufacturing and unionized labor markets. Rates of employment in manufacturing and unionized industries were especially high in the mid-20th century, and Black Americans, after decades of discrimination preventing their access, were over-represented in manufacturing and unionized jobs by the early 1970s. We should not expect that larger cohort sizes would cause the secular decline in manufacturing in the U.S., but given structural constraints like strong unions or import competition, larger cohorts may experience crowding out in good manufacturing jobs.

I test the impact of crowding out in these industries here. In Table 11, I first test the impact of cohort size on the manufacturing share of employment by race. Despite the rapid decline in manufacturing employment during the years that the Baby Boomers were entering the labor market, we do not see that cohort size played a causal role in this decline.

I next interact cohort size with an indicator equal to one if the state had a high manufac-

turing share of employment in 1950. In Columns (2) through (4) the outcome variable is the employed share of the population by race. In Column (2), we see that a one unit increase in cohort size leads to a 1.2 percentage point bigger decrease in Black employment in high manufacturing states, with no impact on Whites. Testing more directly for constraints in manufacturing, in Column (3) I include an indicator for high union share of household in 1950 (using data from Farber et al. (2021)), and in Column (4) I include an indicator for high changes in Japanese imports per worker (using data from Batistich and Bond (2023)). Here we see that high union and high import-exposed states experienced stronger impacts of cohort size on employment for both Black and White workers, but the impacts are stronger for Black workers.

Moving to Table 12, we see in more detail the impacts of these constraints on employment. In Columns (1) through (3), I report the impacts on the manufacturing share of the population, and in Columns (4) through (6), I report non-manufacturing share of the population. These results show that high manufacturing, high union, and high import competition-exposed states had substantially larger impacts of cohort size on declines in manufacturing employment for both races. We see some movement into non-manufacturing employment but it does not perfectly offset the loss of manufacturing employment in most cases. From these results we can infer that the crowding out of manufacturing employment led to increased supply in non-manufacturing industries. In Section 4.7.3, we directly test for the impact of cohort size on crowding out of high-quality and traditionally White occupations and congestion in lower quality and traditionally Black occupations.

4.7.3 Congestion in Low-Earning, Low-Education and Traditionally Black Occupations

Sections 4.7.1 and 4.7.2 presented evidence on the impact of cohort size on crowding out access to college and good manufacturing jobs. We now turn to explore in more detail what types of occupations people who stay employed have as a result of increased cohort size. For this exercise, I aggregated Census occupations slightly to maintain a stable set of occupations over time, resulting in 174 occupations. Using the 1950 Census, I then calculate the mean earnings, educational attainment, and share Black by occupation type. I assign the highest 40 earning occupations as high-earning, and the bottom 40 as low-earning. I similarly assign high- and low-education and high- and low-Black occupations. I then calculate the share of employment by cohort (within race-sex cells) belonging to these categories. With these outcomes, I can then test the impact of cohort size on occupation type by estimating Equation 4 with these occupation shares as outcomes.

Starting with Table 13, we see that larger cohorts lead to reduced share of employment in

traditionally high earning, high education, and White occupations in Columns (1) through (3). This is true for both Black and White workers, although the impact on high earning occupations for White workers is relatively small and less significant. We see larger impacts on the decline in high-education and traditionally White jobs for White workers, who also have a substantially higher baseline mean. Turning to Columns (4) through (6), we see that there is an increase in the share of employment in traditionally low-earning, low-education, and Black occupations.

These results tell us that larger cohorts led to a relatively higher supply in low-earning, low-education and traditionally Black jobs. To confirm the impact of crowding-out from high quality employment and congestion in lower quality employment, we turn to earnings within occupations. I summarize the results for earnings in Figure 8.[21] We see that high-earning, high-education and traditionally White occupations did not experience cohort effects on earnings. In this sense, these occupations were largely sheltered for those who were able to obtain them. We also see that manufacturing employment had only a small and statistically insignificant decrease in earnings. The negative earnings impacts were instead felt in the low-earning, low-education, and traditionally Black occupations as well as in non-manufacturing industries.

5 Conclusion

In this paper I have shown that the Baby Boom led to a widening of the Black-White median earnings gap for those birth cohorts, who began to occupy the labor force in the 1970s while the aggregate racial earnings gap took a downward turn. My main contribution is highlighting a supply-side explanation for the change in aggregate trends. Using both a national level approach and state-level variation, I find a causal impact of cohort size on earnings and employment for both Black and White men, with substantially larger marginal impacts for Black men. Paired with larger increases in Black cohort size, this means that life cycle median earnings would have been 14.7-19.9% higher for Black men and up to 4.9% higher for White men, and the lifetime median earnings gap would have closed instead of widening by 6.5%.

Larger age groups were met with imperfectly increased demand, causing some to miss out on labor market opportunities they would otherwise have had. I find that the impacts of cohort size were race-biased, in that they lowered the rank of the median and 90th percentile Black male on the White male earnings distribution. Cohort size widened the earnings distribution between the 75th percentile and the median, but not between the median and

[21] See Appendix Table A.19 for the regression estimates corresponding to this figure.

the 25th percentile. An analysis of mechanisms point to an inelastic supply of college slots and union membership, increasing congestion in non-college and non-union labor markets.

The impacts of early labor market experience can persist through a lifetime.[22] Today, as the Baby Boomer generation heads into retirement age, symptoms of cohort size tension appear in markets like social security, housing, and long-term care.[23] In contrast, today's youth are among the smallest relative cohorts in history, with little difference between White and Black. As the Baby Boom generation exits the labor market, and the new cohorts enter, we see a potential uptick in the median earnings gap. It is possible that this rising generation will benefit from their smaller size.

[22]See Berger (1985); Kahn (2010); Oyer (2006, 2008); Oreopoulos et al. (2012).

[23]See, for example, Reznik et al. (2005); Culhane et al. (2013); Byrne et al. (2024); Henderson et al. (2023); Einav et al. (2025).

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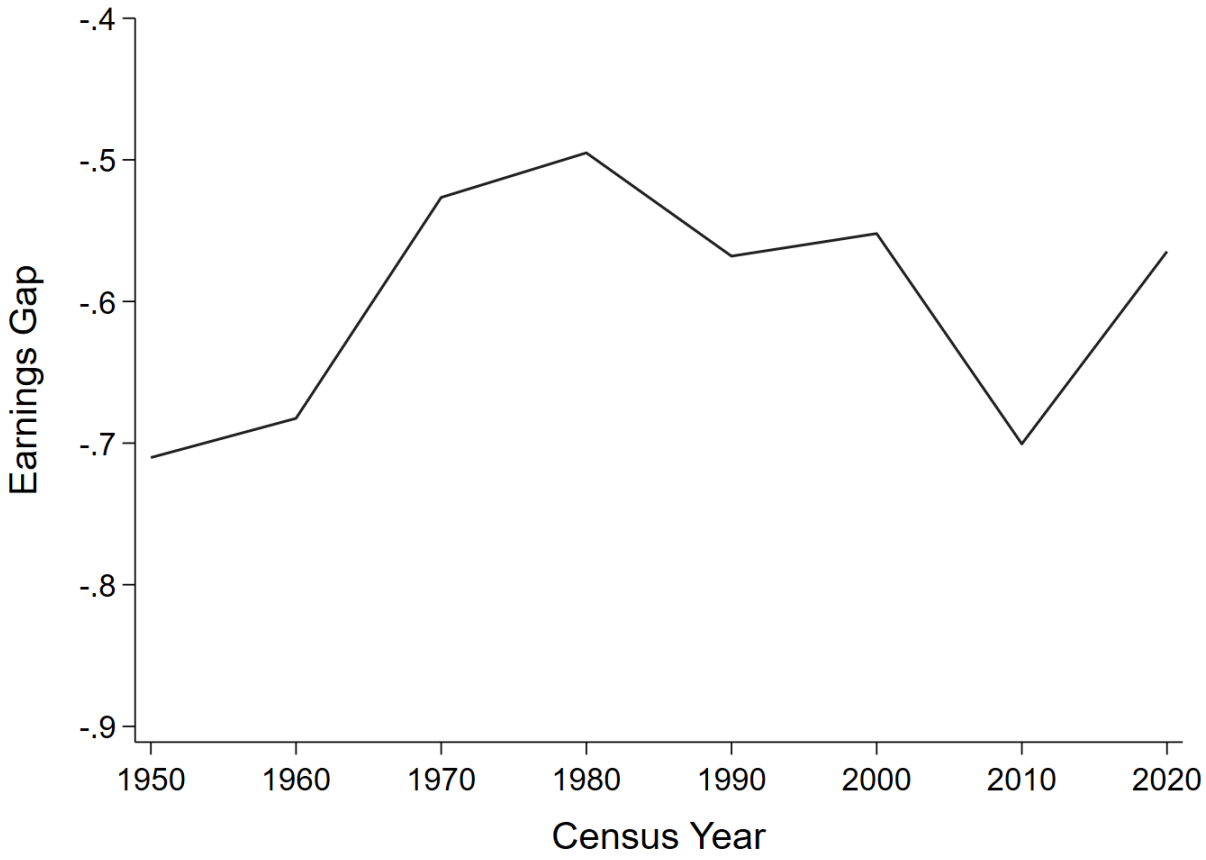
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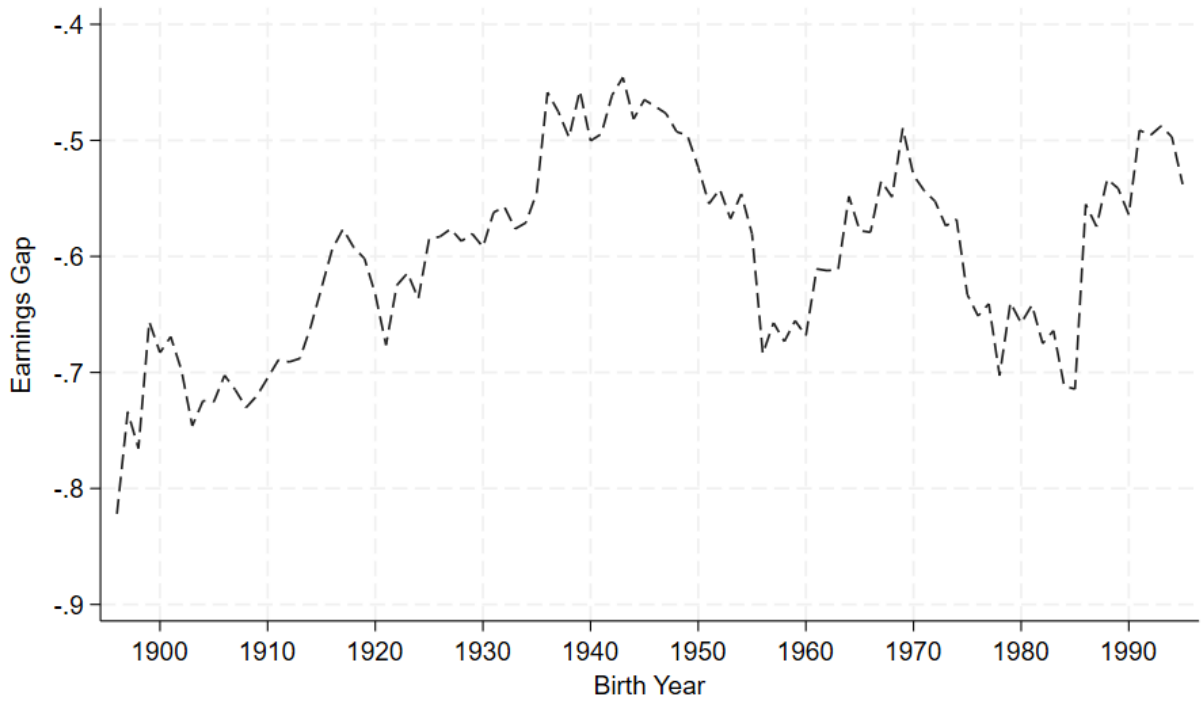
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Figure 1: Male Black-White Median Earnings Gap, Ages 25-54



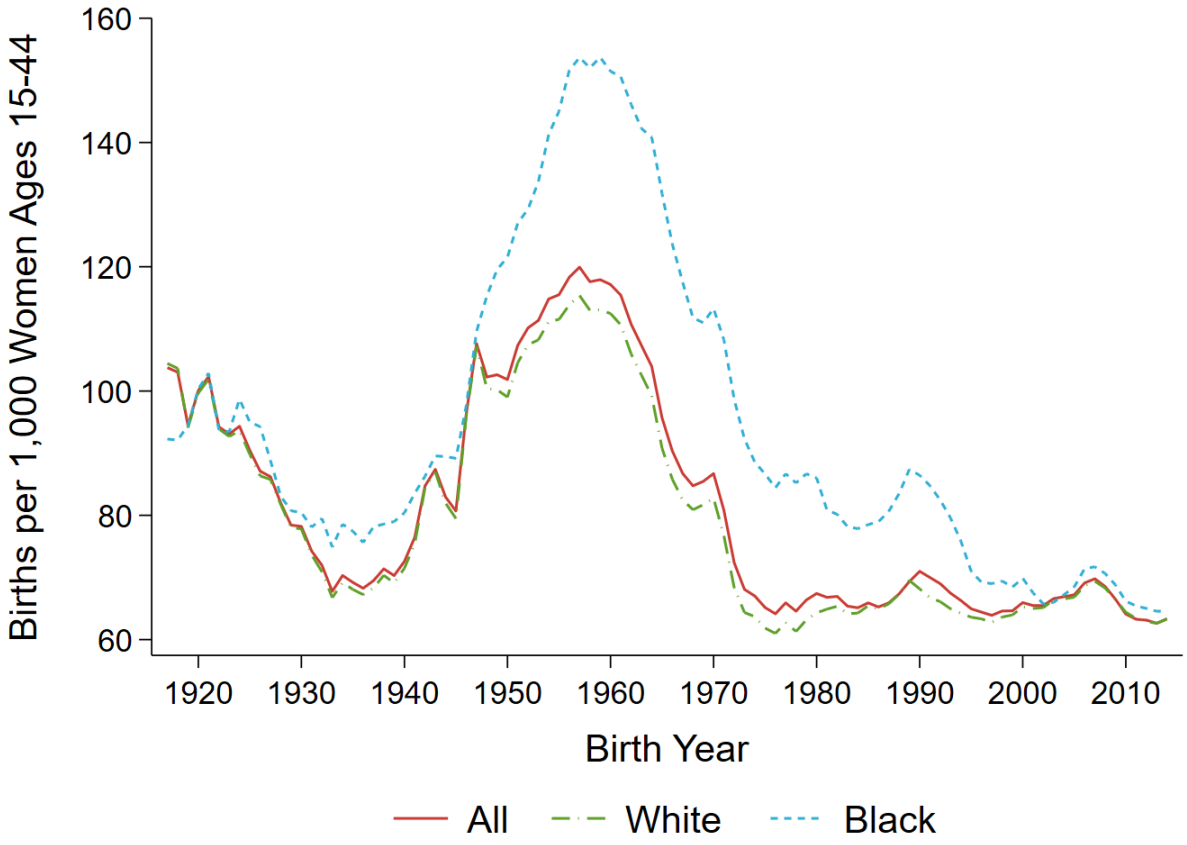
Notes: This figure shows the level log median earnings gap between all Black and White males ages 25 to 54 in a given Census year. Gaps are estimated using quantile regressions with indicators for five-year age bins. See Section 2.2 for more information on treatment of data.

Figure 2: Male Black-White Median Earnings Gap by Birth Cohort



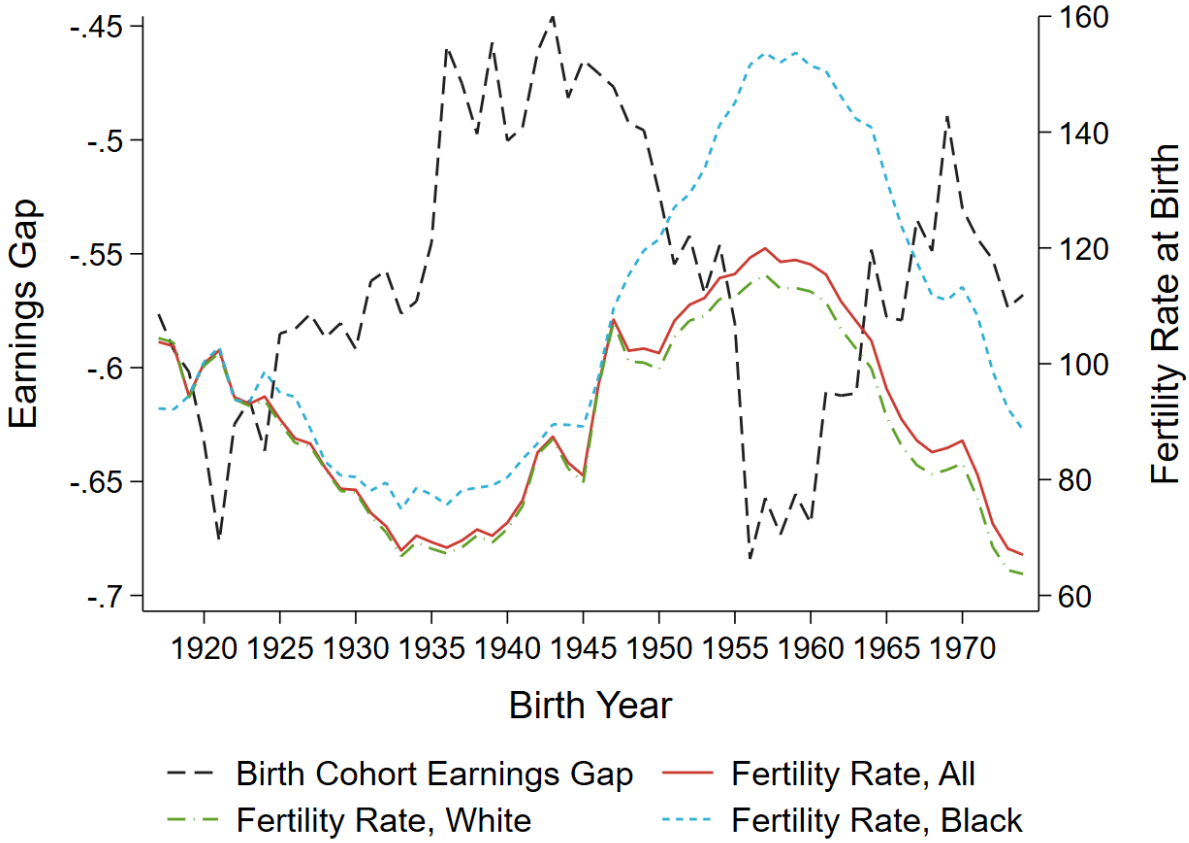
Notes: This figure shows the level log median earnings gap between all Black and White males within a given birth year. Gaps are estimated using quantile regressions at various ages between 25 and 54 depending on age in each Census year. For the figure, I then de-age the gaps by regressing them on a set of age dummies, and then I take a simple average by birth year. For 1920 and earlier, I take a three-year moving average. See Section 2.2 for more information on treatment of data.

Figure 3: U.S. Fertility Rates by Race, 1917-2014



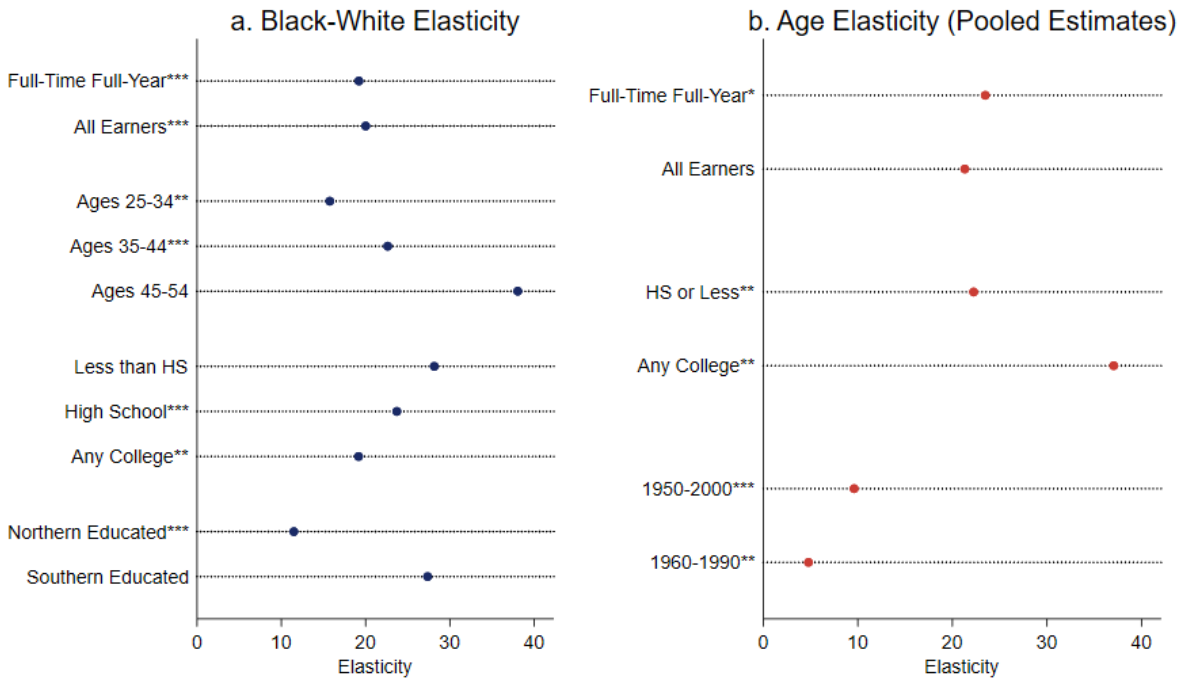
Notes: This figure shows fertility rates using data from the U.S. Vital Statistics, U.S. Census, and American Community Survey. See Section 2.2 for more information on treatment of data.

Figure 4: Fertility Rates and Cohort's Black-White Median Earnings Gap



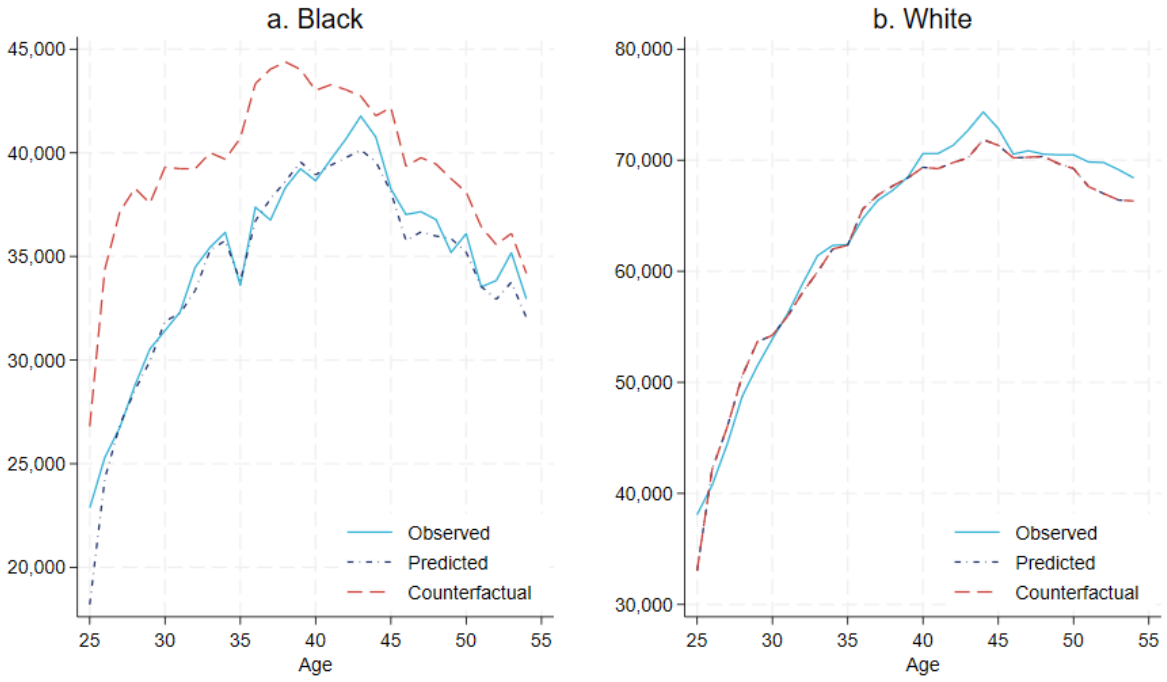
Notes: This figure shows fertility rates using data from the U.S. Vital Statistics, U.S. Census, and American Community Survey. It also shows the level log median earnings gap between all Black and White males within a given birth year. Gaps are estimated using quantile regressions at various ages between 25 and 54 depending on age in each Census year, and a simple average is taken. See Section 2.2 for more information on treatment of data.

Figure 5: Black-White and Age Group Elasticity Estimates



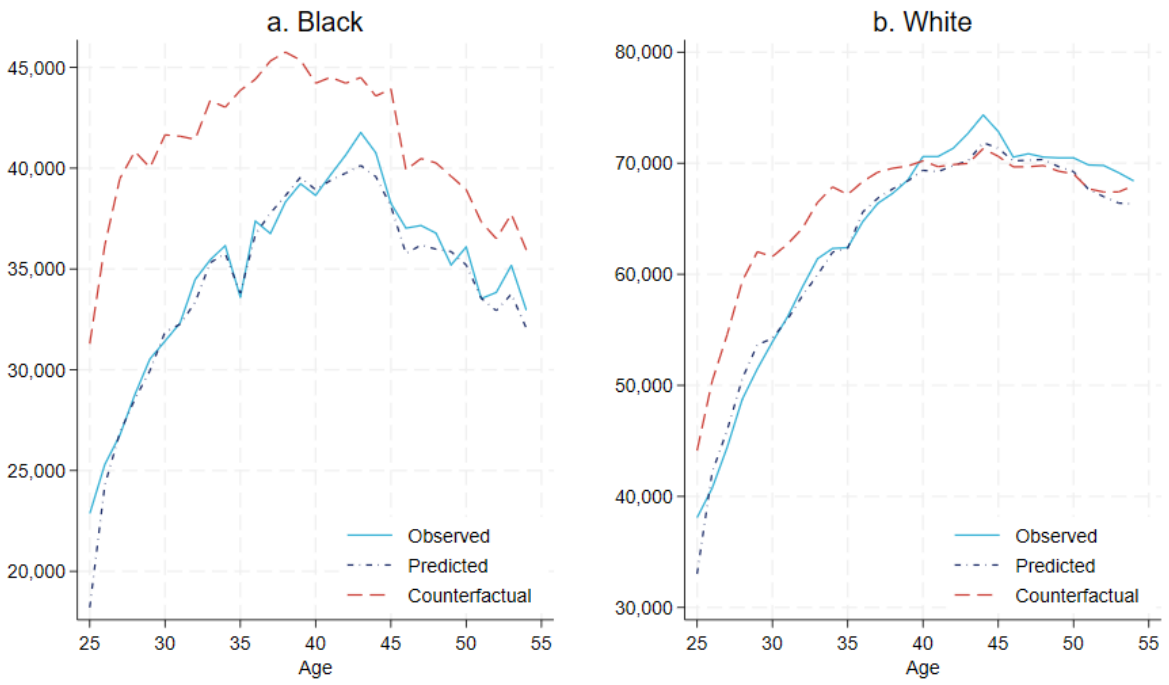
Notes: This figure shows implied elasticities from a nested CES model. Details on the procedure are in Appendix A.1.4. Results from the estimation procedure are shown in Tables A.4 through A.6. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

Figure 6: Counterfactual Male Life-Cycle Earnings For Birth Cohorts 1946-1964 if Black Cohort Size Matched White Cohort Size (2024 USD)



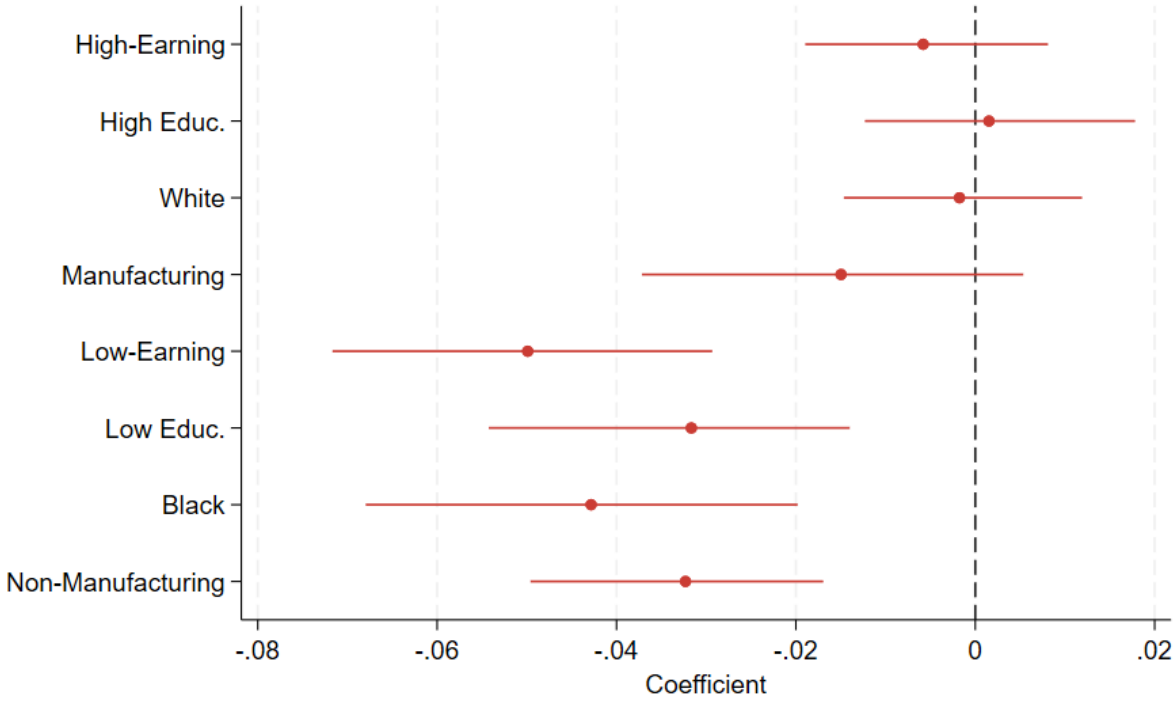
Notes: This figure shows the observed and predicted estimate of log median earnings by age for Black and White males born between ages 1946 and 1964, along with counterfactual estimates if the Black cohort size during those birth years had matched the White. The regression model follows Equation 1 where cohort size is interacted with age and age squared. More details on this exercise are in Section 4.5.

Figure 7: Counterfactual Male Life-Cycle Earnings For Birth Cohorts 1946-1964 if Black and White Cohort Sizes Had Stayed at Pre-Baby Boom Levels (2024 USD)



Notes: This figure shows the observed and predicted estimate of log median earnings by age for Black and White males born between ages 1946 and 1964, along with counterfactual estimates if the Black and White cohort sizes for these birth years had matched the mean race-specific cohort size from those born 1930-1939. The regression model follows Equation 1 where cohort size is interacted with age and age squared. More details on this exercise are in Section 4.5.

Figure 8: Impact of Cohort Size on Earnings By Occupation Type



Notes: This figure shows the results from 2SLS estimations of Equation 4. The estimation results are also shown in Appendix Table A.19. Regressions include state, age and year fixed effects. 95% confidence intervals indicated by the lines are based on a wild cluster bootstrap estimation of standard errors clustered at the state level.

Table 1: Summary Statistics (Men Ages 25-54)

	National Sample			State Sample		
	Black	White	Gap	Black	White	Gap
	(1)	(2)	(3)	(4)	(5)	(6)
Log Median Earned Income:						
All	10.456	11.032	-0.577	10.414	11.077	-0.628
	(0.260)	(0.235)	(0.090)	(0.442)	(0.218)	(0.391)
Earners	10.662	11.080	-0.418	10.688	11.131	-0.414
	(0.281)	(0.239)	(0.087)	(0.294)	(0.224)	(0.152)
Full-Time Full-Year	10.826	11.161	-0.335	10.861	11.217	-0.331
	(0.258)	(0.217)	(0.087)	(0.275)	(0.206)	(0.150)
Employed Share of Pop.	0.739	0.890	-0.151	0.704	0.883	-0.177
	(0.074)	(0.037)	(0.048)	(0.094)	(0.043)	(0.073)
LFP Share of Pop.	0.808	0.925	-0.117	0.780	0.920	-0.136
	(0.065)	(0.032)	(0.042)	(0.078)	(0.036)	(0.060)
Observations	162	162	162	3,645	3,645	3,645

Notes: This table shows summary statistics for the main outcomes for the national level sample in Columns (1) through (3) and the state level sample in Columns (4) through (6). The national sample includes 162 observations for each race, which represent 54 birth cohorts born 1917 through 1970 with three observations for each during their prime working age years 25 to 54. The state sample includes 3,645 observations for each race, which represent 45 birth cohorts born 1926 through 1970 from 27 states (including Washington, D.C.) with three observations for each during their prime working age years 25 to 54. Data come from the full-count Decennial Census and Census samples as well as the American Community Survey. More details on data samples and variable construction can be found in Section 2.

Table 2: Impact of Race-Specific Cohort Size on Male Earnings and Employment (2SLS)

Panel A. Black	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (Black)	-0.158***	-0.125***	-0.103***	-0.031***	-0.018***
	(0.016)	(0.017)	(0.014)	(0.003)	(0.002)
Mean Cohort Size	3.628	3.628	3.628	3.628	3.628
Mean of Dep. Var.	10.456	10.662	10.826	0.739	0.808
Observations	162	162	162	162	162
Panel B. White	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (White)	-0.066***	-0.073***	-0.039***	-0.004***	0.001
	(0.011)	(0.012)	(0.011)	(0.001)	(0.001)
Mean Cohort Size	2.994	2.994	2.994	2.994	2.994
Mean of Dep. Var.	11.032	11.080	11.161	0.890	0.925
Observations	162	162	162	162	162

Notes: This table shows the results from 2SLS estimations of Equation 1. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

Table 3: Impact of Race-Specific Cohort Size on Black Male Earnings and Employment (2SLS)

Panel A. Northeast	Log Median Earnings			Employed Share of Pop.	Labor Force Share
	All	Earners	FTFY		
	(1)	(2)	(3)	(4)	(5)
Cohort Size (Black)	-0.226***	-0.138***	-0.094***	-0.044***	-0.029***
	(0.021)	(0.015)	(0.010)	(0.005)	(0.005)
Mean of Dep. Var.	3.159	3.159	3.159	3.159	3.159
Observations	162	162	162	162	162
Panel B. Midwest	Log Median Earnings			Employed Share of Pop.	Labor Force Share
	All	Earners	FTFY		
	(1)	(2)	(3)	(4)	(5)
Cohort Size (Black)	-0.229***	-0.129***	-0.080***	-0.037***	-0.020***
	(0.026)	(0.021)	(0.016)	(0.003)	(0.003)
Mean of Dep. Var.	3.162	3.162	3.162	3.162	3.162
Observations	162	162	162	162	162
Panel C. South	Log Median Earnings			Employed Share of Pop.	Labor Force Share
	All	Earners	FTFY		
	(1)	(2)	(3)	(4)	(5)
Cohort Size (Black)	-0.107***	-0.104***	-0.070***	-0.023***	-0.012***
	(0.016)	(0.019)	(0.016)	(0.003)	(0.002)
Mean of Dep. Var.	3.895	3.895	3.895	3.895	3.895
Observations	162	162	162	162	162
Panel D. West	Log Median Earnings			Employed Share of Pop.	Labor Force Share
	All	Earners	FTFY		
	(1)	(2)	(3)	(4)	(5)
Cohort Size (Black)	-0.203***	-0.172***	-0.125***	-0.049***	-0.036***
	(0.035)	(0.036)	(0.028)	(0.009)	(0.007)
Mean of Dep. Var.	3.168	3.168	3.168	3.168	3.168
Observations	162	162	162	162	162

Notes: This table shows the results from 2SLS estimations of Equation 1. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

Table 4: Impact of Race-Specific Cohort Size on White Male Earnings and Employment (2SLS)

Panel A. Northeast	Log Median Earnings			Employed Share of Pop.	Labor Force Share
	All	Earners	FTFY		
	(1)	(2)	(3)	(4)	(5)
Cohort Size (White)	-0.077***	-0.079***	-0.050***	-0.003**	0.002**
	(0.009)	(0.009)	(0.008)	(0.001)	(0.001)
Mean of Dep. Var.	2.978	2.978	2.978	2.978	2.978
Observations	162	162	162	162	162
Panel B. Midwest	Log Median Earnings			Employed Share of Pop.	Labor Force Share
	All	Earners	FTFY		
	(1)	(2)	(3)	(4)	(5)
Cohort Size (White)	-0.071***	-0.070***	-0.049***	-0.007***	0.001
	(0.011)	(0.012)	(0.012)	(0.001)	(0.001)
Mean of Dep. Var.	3.004	3.004	3.004	3.004	3.004
Observations	162	162	162	162	162
Panel C. South	Log Median Earnings			Employed Share of Pop.	Labor Force Share
	All	Earners	FTFY		
	(1)	(2)	(3)	(4)	(5)
Cohort Size (White)	-0.063***	-0.056***	-0.055***	-0.004**	-0.001
	(0.016)	(0.015)	(0.014)	(0.002)	(0.001)
Mean of Dep. Var.	3.068	3.068	3.068	3.068	3.068
Observations	162	162	162	162	162
Panel D. West	Log Median Earnings			Employed Share of Pop.	Labor Force Share
	All	Earners	FTFY		
	(1)	(2)	(3)	(4)	(5)
Cohort Size (White)	-0.047***	-0.040***	-0.018	-0.006***	0.000
	(0.014)	(0.015)	(0.013)	(0.002)	(0.002)
Mean of Dep. Var.	2.871	2.871	2.871	2.871	2.871
Observations	162	162	162	162	162

Notes: This table shows the results from 2SLS estimations of Equation 1. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

Table 5: Impact of Cohort Size on the Median Earnings Gap: Spread vs. Rank

	Percentile Earnings Gaps, All Men (25-54)				Black Rank	
	75th-25th	50th-25th	75th-50th	90th-50th	50th	90th
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (All Races)	0.014	-0.001	0.015**	0.022***	-1.875***	-1.648***
	(0.015)	(0.013)	(0.006)	(0.005)	(0.320)	(0.538)
Mean of Dep. Var.	0.940	0.584	0.356	0.684	22.744	67.006
Mean Cohort Size	2.941	2.941	2.941	2.941	2.941	2.941
Observations	162	162	162	162	162	162

Notes: This table shows the results from 2SLS estimations of Equation 1. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

Table 6: Correlation Between Fertility Rate and Median Child's Parent Income Percentile

	Overall	Black	White	Black-White
	(1)	(2)	(3)	(4)
Fertility Rate, All	0.018	0.047	0.024	0.022
	(0.019)	(0.041)	(0.025)	(0.042)
Mean of Dep. Var.	56.132	26.342	60.895	-34.553
Mean Fertility Rate	94.725	94.725	94.725	94.725
Observations	38	38	38	38

Notes: This table shows the results from OLS estimations where the independent variable is fertility rate, with 38 birth year observations. Controls include indicators for age at which childhood household income was measured (8 to 17). In Column (1), the dependent variable is the median child's household income percentile on the household income distribution. In Columns (2) and (3), the dependent variables are the median Black and White child's household income percentile on the household income distribution. In Column (4), the dependent variable is the median Black minus the median White child's household parental income percentile. Household income distribution is defined by the earned income of household heads ages 18 to 64 in a given Census year, including wage, business and farm income. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

Table 7: Impact of Race-Specific Cohort Size on Male Earnings and Employment (2SLS), Interaction with Younger Age Group

Panel A. Black	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (Black)	-0.087***	-0.037**	-0.020*	-0.019***	-0.011***
	(0.020)	(0.016)	(0.012)	(0.003)	(0.003)
Cohort Size X Ages 25-39	-0.124***	-0.153***	-0.143***	-0.021***	-0.012***
	(0.024)	(0.020)	(0.018)	(0.003)	(0.003)
Mean of Dep. Var.	10.456	10.662	10.826	0.739	0.808
Observations	162	162	162	162	162
Panel B. White	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (White)	0.001	-0.011	0.016	-0.002	-0.000
	(0.013)	(0.014)	(0.013)	(0.001)	(0.001)
Cohort Size X Ages 25-39	-0.142***	-0.131***	-0.118***	-0.006**	0.003
	(0.018)	(0.016)	(0.017)	(0.003)	(0.003)
Mean of Dep. Var.	11.032	11.080	11.161	0.890	0.925
Observations	162	162	162	162	162

Notes: This table shows the results from 2SLS estimations of Equation 1. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

Table 8: Impact of Cohort Size on Male Earnings and Employment, State-Level TWFE (2SLS)

Panel A. Black	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (Black)	-0.178***	-0.075***	-0.052***	-0.027***	-0.019***
Wild Bootstrap CI	[-0.250, -0.109]	[-0.098, -0.051]	[-0.070, -0.035]	[-0.034, -0.021]	[-0.025, -0.013]
Mean of Dep. Var.	10.468	10.731	10.900	0.706	0.782
Mean Cohort Size	3.772	3.772	3.772	3.772	3.772
Observations	3,645	3,645	3,645	3,645	3,645
Panel B. White	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (White)	-0.035***	-0.024***	-0.009	-0.008***	-0.003**
Wild Bootstrap CI	[-0.053, -0.018]	[-0.042, -0.008]	[-0.027, 0.009]	[-0.011, -0.005]	[-0.006, -0.001]
Mean of Dep. Var.	11.073	11.126	11.213	0.884	0.921
Mean Cohort Size	2.940	2.940	2.940	2.940	2.940
Observations	3,645	3,645	3,645	3,645	3,645

Notes: This table shows the results from 2SLS estimations of Equation 4. Regressions include state, age and year fixed effects. Confidence intervals in brackets are based on a wild cluster bootstrap estimation of standard errors clustered at the state level, and the significance levels indicated by that procedure are represented by: *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

Table 9: Impact of Cohort Size on Educational Attainment (State-Level 2SLS)

	Men			Women		
Panel A. Black	High School	Any College	College Degree	High School	Any College	College Degree
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (Black)	0.008	0.000	-0.017***	0.019***	0.009	-0.014***
Wild Bootstrap CI	[-0.008, 0.023]	[-0.015, 0.014]	[-0.024, -0.011]	[0.009, 0.030]	[-0.004, 0.022]	[-0.021, -0.008]
Mean of Dep. Var.	0.712	0.338	0.101	0.740	0.388	0.122
Mean Cohort Size	3.758	3.758	3.758	3.749	3.749	3.749
Observations	1,215	1,215	1,215	1,215	1,215	1,215
	Men			Women		
Panel B. White	High School	Any College	College Degree	High School	Any College	College Degree
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (White)	-0.004	0.001	-0.016**	0.004	0.017**	-0.008
Wild Bootstrap CI	[-0.020, 0.009]	[-0.017, 0.017]	[-0.033, -0.000]	[-0.007, 0.014]	[0.001, 0.032]	[-0.026, 0.009]
Mean of Dep. Var.	0.840	0.515	0.267	0.851	0.480	0.221
Mean Cohort Size	2.939	2.939	2.939	2.939	2.939	2.939
Observations	1,215	1,215	1,215	1,215	1,215	1,215

Notes: This table shows the results from 2SLS estimations of Equation 4 for ages between 30 and 39. Regressions include state, age and year fixed effects, and year by Census division fixed effects. Confidence intervals in brackets are based on a wild cluster bootstrap estimation of standard errors clustered at the state level, and the significance levels indicated by that procedure are represented by: *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

Table 10: Impact of Cohort Size on Educational Attainment (State-Level 2SLS), Interaction with College Market Tightness

	Men			Women		
Panel A. Black	High School	Any College	College Degree	High School	Any College	College Degree
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size X	0.036**	-0.033**	-0.022***	0.023*	-0.022**	-0.020***
Tight Market	[-0.000, 0.070]	[-0.075, -0.007]	[-0.047, -0.005]	[-0.003, 0.055]	[-0.041, -0.007]	[-0.040, -0.007]
Cohort Size (Black)	0.003	0.003	-0.015***	0.016***	0.010**	-0.012***
	[-0.009, 0.016]	[-0.009, 0.014]	[-0.022, -0.009]	[0.006, 0.025]	[0.000, 0.021]	[-0.018, -0.007]
Tight College Market	0.004	0.018*	0.006	0.004	0.016**	0.006
	[-0.028, 0.043]	[-0.000, 0.043]	[-0.006, 0.024]	[-0.024, 0.035]	[0.002, 0.034]	[-0.002, 0.020]
Mean of Dep. Var.	0.712	0.338	0.101	0.740	0.388	0.122
Observations	1,215	1,215	1,215	1,215	1,215	1,215
	Men			Women		
Panel B. White	High School	Any College	College Degree	High School	Any College	College Degree
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size X	-0.023**	-0.059***	-0.045**	-0.014	-0.033**	-0.043**
Tight Market	[-0.046, -0.004]	[-0.129, -0.018]	[-0.091, -0.008]	[-0.033, 0.008]	[-0.082, -0.003]	[-0.090, -0.002]
Cohort Size (White)	-0.008	-0.004	-0.019**	0.001	0.015	-0.012
	[-0.025, 0.009]	[-0.028, 0.016]	[-0.035, -0.001]	[-0.008, 0.011]	[-0.005, 0.034]	[-0.032, 0.008]
Tight College Market	0.017	0.020	0.012	0.010	0.008	0.015
	[-0.003, 0.039]	[-0.008, 0.063]	[-0.008, 0.039]	[-0.007, 0.029]	[-0.011, 0.040]	[-0.004, 0.044]
Mean of Dep. Var.	0.840	0.515	0.267	0.851	0.480	0.221
Observations	1,215	1,215	1,215	1,215	1,215	1,215

Notes: This table shows the results from 2SLS estimations of Equation 4 for ages between 30 and 39, with additional covariates representing college market tightness and its interaction with cohort size. Regressions include state, age and year fixed effects, and year by Census division fixed effects. Confidence intervals in brackets are based on a wild cluster bootstrap estimation of standard errors clustered at the state level, and the significance levels indicated by that procedure are represented by: *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

Table 11: Impact of Cohort Size on Employment (State-Level, 2SLS), Interaction with High Manufacturing, Union, and Import Competition

Panel A. Black	Mfg. Shr. of Emp.	Employed Share of Population		
	(1)	(2)	(3)	(4)
Cohort Size (Black)	-0.006	-0.028***	-0.027***	-0.026***
	[-0.017, 0.005]	[-0.034, -0.021]	[-0.032, -0.021]	[-0.032, -0.021]
Cohort Size X		-0.012*		
High 1950 Manuf. Share		[-0.026, 0.001]		
Cohort Size X			-0.018**	
High 1950 Union Share			[-0.032, -0.004]	
Cohort Size X				-0.016***
High Δ Imports/Worker				[-0.029, -0.004]
Mean of Dep. Var.	0.235	0.706	0.706	0.706
Observations	3,645	3,645	3,510	3,645
Panel B. White	Mfg. Shr. of Emp.	Employed Share of Population		
	(1)	(2)	(3)	(4)
Cohort Size (White)	-0.007	-0.008***	-0.008***	-0.007***
	[-0.015, 0.002]	[-0.011, -0.005]	[-0.010, -0.005]	[-0.010, -0.005]
Cohort Size X		0.000		
High 1950 Manuf. Share		[-0.008, 0.007]		
Cohort Size X			-0.005***	
High 1950 Union Share			[-0.010, -0.001]	
Cohort Size X				-0.006**
High Δ Imports/Worker				[-0.010, -0.001]
Mean of Dep. Var.	0.248	0.884	0.884	0.884
Observations	3,645	3,645	3,510	3,645

Notes: This table shows the results from 2SLS estimations of Equation 4, with additional covariates representing the interactions of high manufacturing state, high union state, and high import exposure state with cohort size. Regressions include state, age and year fixed effects. Confidence intervals in brackets are based on a wild cluster bootstrap estimation of standard errors clustered at the state level, and the significance levels indicated by that procedure are represented by: *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

Table 12: Impact of Cohort Size on Manufacturing and Non-Manufacturing Employment (as Share of Population), Interactions with High Manufacturing, High Unions, and High Import Competition States

Panel A. Black	Manufacturing Share of Population			Non-Manufacturing Share of Population		
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (Black)	-0.010***	-0.007*	-0.007*	-0.017***	-0.019***	-0.020***
	[-0.019, -0.002]	[-0.017, 0.000]	[-0.014, 0.001]	[-0.026, -0.008]	[-0.028, -0.009]	[-0.028, -0.009]
Cohort Size X	-0.049***			0.037**		
High 1950 Manuf. Share	[-0.074, -0.025]			[0.009, 0.065]		
Cohort Size X		-0.050***			0.032**	
High 1950 Union Share		[-0.079, -0.024]			[0.002, 0.064]	
Cohort Size X			-0.043**			0.027
High Δ Imports/Worker			[-0.082, -0.011]			[-0.008, 0.067]
Mean of Dep. Var.	0.169	0.169	0.169	0.536	0.536	0.536
Observations	3,645	3,510	3,645	3,645	3,510	3,645
Panel B. White	Manufacturing Share of Population			Non-Manufacturing Share of Population		
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (White)	-0.007	-0.006	-0.007*	-0.001	-0.001	-0.001
	[-0.017, 0.005]	[-0.018, 0.003]	[-0.015, 0.001]	[-0.013, 0.009]	[-0.011, 0.010]	[-0.008, 0.008]
Cohort Size X	-0.034***			0.034***		
High 1950 Manuf. Share	[-0.054, -0.017]			[0.019, 0.050]		
Cohort Size X		-0.025*			0.020	
High 1950 Union Share		[-0.048, 0.007]			[-0.015, 0.044]	
Cohort Size X			-0.014			0.008
High Δ Imports/Worker			[-0.042, 0.017]			[-0.024, 0.037]
Mean of Dep. Var.	0.220	0.220	0.220	0.664	0.664	0.664
Observations	3,645	3,510	3,645	3,645	3,510	3,645

Notes: This table shows the results from 2SLS estimations of Equation 4, with additional covariates representing the interactions of high manufacturing state, high union state, and high import exposure state with cohort size. Regressions include state, age and year fixed effects. Confidence intervals in brackets are based on a wild cluster bootstrap estimation of standard errors clustered at the state level, and the significance levels indicated by that procedure are represented by: *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

Table 13: Impact of Cohort Size on Occupation Type Among Employed (State-Level, 2SLS)

Panel A. Black	High Earning	High Educ.	White	Low Earning	Low Educ.	Black
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (Black)	-0.012***	-0.009***	-0.004**	0.025***	0.009***	0.015**
Wild Bootstrap CI	[-0.015, -0.009]	[-0.012, -0.005]	[-0.007, -0.001]	[0.015, 0.035]	[0.003, 0.014]	[0.004, 0.027]
Mean of Dep. Var.	0.147	0.090	0.115	0.304	0.520	0.348
Observations	3,645	3,645	3,645	3,645	3,645	3,645
Panel B. White	High Earning	High Educ.	White	Low Earning	Low Educ.	Black
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (White)	-0.004	-0.014***	-0.007***	0.005**	0.021***	0.009***
Wild Bootstrap CI	[-0.009, 0.001]	[-0.020, -0.009]	[-0.010, -0.003]	[-0.000, 0.010]	[0.014, 0.026]	[0.004, 0.013]
Mean of Dep. Var.	0.321	0.153	0.234	0.142	0.322	0.168
Observations	3,645	3,645	3,645	3,645	3,645	3,645

Notes: This table shows the results from 2SLS estimations of Equation 4. Regressions include state, age and year fixed effects. Confidence intervals in brackets are based on a wild cluster bootstrap estimation of standard errors clustered at the state level, and the significance levels indicated by that procedure are represented by: *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

A Appendix

A.1 Supplemental Information on Data Set Construction and Empirical Strategies

A.1.1 Fertility Rates

To calculate fertility rates, I use U.S. Vital Statistics annual reports dating back to 1917. While the annual reports have fertility counts going back to 1915, they look quite noisy before 1917 so I decided not to include them. These reports include tables with counts of births by state and race for each year. Births are only reported by place of occurrence through 1938, then reported both by place of occurrence and place of residence for some years after, but are only reported by place of residence starting in 1945. I use place of residence starting in 1939. States start reporting to Vital Statistics in different years, but by 1933 all states except Alaska and Hawaii report. Alaska and Hawaii start reporting in 1959 and 1960, respectively. States reporting in a given year are referred to as registration areas.

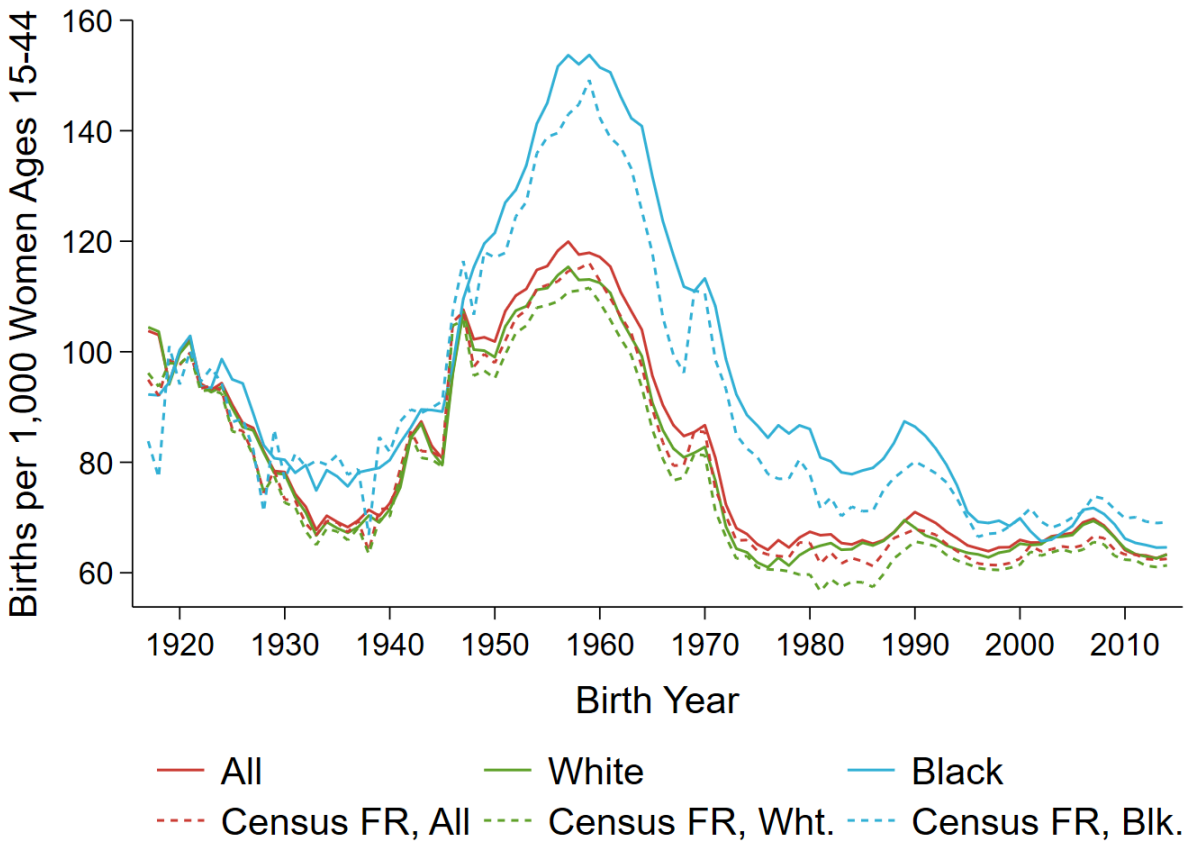
The national and state-level data were digitized by hand and then double-checked by comparing the sum of state totals to national totals. National totals only reflect registration areas. Births by race are reported for White, Black, and Other which sum to the total, and these categories are reported through 2014. (Starting in 2015, race categories switch to White Non-Hispanic, Black Non-Hispanic, Hispanic, and Other, but I do not need data for these years.) Data is reported by race of child until 1981, when it is reported by race of mother.

New Jersey birth counts are not available by race in 1962 and 1963, so I impute that White births are 84% of the total and Black births are 16% of the total in each year. From 1930 to 1934, the White category excludes Hispanic, so I use non-Hispanic White women 15-44 as the denominator in fertility rate for those years.

There is a concern about potential underreporting of births in the Vital Statistics data. To check the accuracy of my fertility measure, I calculated a supplemental quasi-fertility rate using Census data. To do this, I take U.S. born children between the ages of 1 and 10 in the Census and calculate their birth years as Census year minus age minus one. Net of childhood mortality, this gives me a close count to the number of children born in each year in the United States. This method has the advantage of using both numerator and denominator from Census data so that any underreporting in the numerator would be cancelled out by a corresponding equivalent underreporting in the denominator. A visual comparison of the two fertility measures in Figure A.1 reassures that there is not a dramatic underreporting of births in the Vital Statistics data. This alternative measure is also useful to fill in missing

fertility rates at the state level to preserve a balanced panel.

Figure A.1: U.S. Fertility Rates by Race, 1917-2014, Compared to Census



Notes: This figure shows fertility rates using data from the U.S. Vital Statistics, U.S. Census, and American Community Survey. It also shows quasi-fertility rates using Census data for both birth count and childbearing age women. See Section 2.2 for more information on treatment of data.

A.1.2 Migration and Results in the North

To further test the robustness of my results to impacts of the Great Migration, I incorporate an additional instrument based on the approach from Boustan (2009) and used in Derenoncourt (2022). I construct this instrument as follows. From the replication files of Boustan (2009), I use the county-level outmigration predictors for the South. The link to these data is here: `https://economics.princeton.edu/dl/Boustan/Chapter4.zip`. Following the LASSO prediction results from Derenoncourt (2022), I use the following predictor variables:

- Percent tenant farms
- Share of the labor force in agriculture
- WWII spending per capita
- Percent acreage in cotton
- Share of the labor force in agriculture × Tobacco growing state
- Indicator for mining state
- Indicator for mining state × Share of the labor force in mining

These are in the data as: perten, perag, warfac_pc, percot, peragtob, ot, and permit. I then calculate Black migration in Southern counties using the difference in the count of Black adults ages 16-44 in 1940 and 26-54 in 1950 from the full count Censuses, dividing by the 1940 16-44 Black population and multiplying by 100 to create a migration rate. I regress migration rate on the outmigration prediction variables, and then calculate predicted outmigration using the estimated coefficients. I calculate predicted inflow shares into non-Southern counties using the average of 1910-1920 and 1920-1930 county-to-county migration flows from links in the full count Censuses using Census Tree (`https://www.censustree.org/`).[24] These shares sum to one within a Southern county. Predicted inflow into a non-Southern county is then the sum of each 1910-1930 inflow share into the county times the predicted 1940-1950 outflow count of the corresponding Southern county. I then sum the predicted inflow counts across Northern counties to get totals by state, and convert it again to a rate by dividing by the state's total Black adult ages 16-64 population; these values are shown in Table A.1. I use the predicted migrant inflow and the fertility rate measure together to estimate a predicted cohort size and then use the predicted cohort size as my alternative instrument.

[24] Price et al. (2023a,b). See Price et al. (2021) and Buckles et al. (2023) for information on Census Tree linking methodology.

As a benchmark, I first show the results from estimating Equation 4 separately for Southern and Northern (i.e., non-Southern) states with my main instrument in Table A.2. Comparing Columns (1) through (3) to (4) through (6), we see that impacts of cohort size on earnings and employment are larger in the North for both Black and White workers, and the impact on Black-White gaps is also larger. This is consistent with my regional analysis in Tables 3 and 4 in the main text. The results are also consistent with Black and White workers being less substitutable in the North, as shown by the elasticity estimates in Section 4.4.

In Table A.3 I use the alternative instrument. Comparing Columns (1), (2) and (4) of this table to Columns (4), (5) and (6) of Panel A. of Table A.2, we see that the results are generally unchanged. This implies that the lower measurement in cohort size due to in-migration does not explain the differences in cohort size effects by region, and that the effects are driven by other economic means.

Table A.1: Predicted In-Migration in Northern States of Analysis Sample

	Predicted In-Migration
California	0.076
Connecticut	0.049
Illinois	0.110
Indiana	0.050
Massachusetts	0.028
Michigan	0.099
Missouri	0.094
New Jersey	0.041
New York	0.027
Ohio	0.099
Pennsylvania	0.048
Wisconsin	0.088

Table A.2: Impact of Cohort Size on Earning and Employment, By Region (State-Level, 2SLS)

	South			North/West		
	Earnings		Emp. Share	Earnings		Emp. Share
Panel A. Black	All	Earners	of Pop.	All	Earners	of Pop.
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (Black)	-0.088***	-0.049***	-0.021***	-0.240***	-0.070***	-0.036***
Wild Bootstrap CI	[-0.110, -0.064]	[-0.065, -0.028]	[-0.030, -0.012]	[-0.427, -0.112]	[-0.113, -0.031]	[-0.046, -0.027]
Mean of Dep. Var.	10.425	10.642	0.729	10.522	10.847	0.675
Mean Cohort Size	4.043	4.043	4.043	3.422	3.422	3.422
Observations	2,025	2,025	2,025	1,620	1,620	1,620
	Earnings		Emp. Share	Earnings		Emp. Share
Panel B. White	All	Earners	of Pop.	All	Earners	of Pop.
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (White)	-0.022	-0.012	-0.005**	-0.030***	-0.020***	-0.010***
Wild Bootstrap CI	[-0.059, 0.014]	[-0.047, 0.014]	[-0.008, -0.000]	[-0.044, -0.017]	[-0.032, -0.009]	[-0.012, -0.007]
Mean of Dep. Var.	10.990	11.055	0.881	11.126	11.172	0.885
Mean Cohort Size	2.976	2.976	2.976	2.917	2.917	2.917
Observations	2,025	2,025	2,025	1,620	1,620	1,620

Notes: This table shows the results from 2SLS estimations of Equation 4. Regressions include state, age and year fixed effects. Confidence intervals in brackets are based on a wild cluster bootstrap estimation of standard errors clustered at the state level, and the significance levels indicated by that procedure are represented by: *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

Table A.3: Impact of Race-Specific Cohort Size on Male Earnings and Employment (2SLS), Composite Instrument Incorporating Predicted In-Migration, North/West States

	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (Black)	-0.228***	-0.076***	-0.047***	-0.026**	-0.011*
Wild Bootstrap CI	[-0.382, -0.110]	[-0.110, -0.046]	[-0.083, -0.019]	[-0.040, -0.010]	[-0.023, 0.003]
Mean of Dep. Var.	10.468	10.731	10.900	0.706	0.782
Mean Cohort Size	3.772	3.772	3.772	3.772	3.772
Observations	1,620	1,620	1,620	1,620	1,620

Notes: *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

A.1.3 Childhood Household Income Percentile

An alternative explanation for growth in the earnings gap is that the Baby Boom was driven by particular income groups. We can test for this by estimating the correlation between fertility rate and income percentile. To do so, I calculate the median (overall/Black/White) child's parental income percentile by cohort, captured during childhood (ages 8-17).

I calculate earned income as I do for the main analysis, described in Section 2.2 in the main text. However, here, I only include those residing in households. I then calculate 100 percentiles among household heads ages 18 to 64, including non-earners. I then find the sum of children by birth year for ages 8 to 17 in each Census year that fall into each percentile bin, overall and by race. If there was any bunching at a particular percentile (which also means gaps at nearby lower percentiles), I reallocated spots to fill in the gaps evenly. Finding the median child overall and by race, I am now able to estimate the correlation between fertility rate at a child's birth and the income percentile of the median child during childhood.

My estimating equation for race r and birth year cohort c is

$$\text{Income Percentile}_{rc} = \beta_0 + \beta_1 * \text{Fertility Rate}_c + \lambda_a + \epsilon_{rc} \tag{6}$$

The regression includes fixed effects for age at which parental income was measured, λ_a. All-race fertility rate is the explanatory variable. Regressions are run separately for Black, White and overall. Given that I need to observe the cohort during childhood, I use birth years 1933-1970 (38 observations). The results of this estimation are in Table 6 in the main text, with a discussion in Section 4.3.

The regressions tell us there is no change in the location of the median child on the household income distribution in response to birth year fertility rate, which does not support the hypothesis that lumpy cohort growth was the driver behind the Black-White median earnings gap widening. Below in Figures A.2 through A.7 are probability distribution functions (PDFs) and cumulative distribution functions (CDFs) of the childhood household income percentile captured at age 12 for various cohorts over time. We see that these distributions are quite stable over time, with a slight tilt toward children living in higher-income families consistently across all years. When focused on Black children, we see that children in later years if anything move toward higher earning families, reflective of their movement upward on the earnings distribution during the 1940s through 1960s. We do not however see any concentration of Baby Boom children in lower percentile households, or White children in higher percentile households.

Figure A.2: Parent Income Percentile at Age 12 by Birth Year, All Races

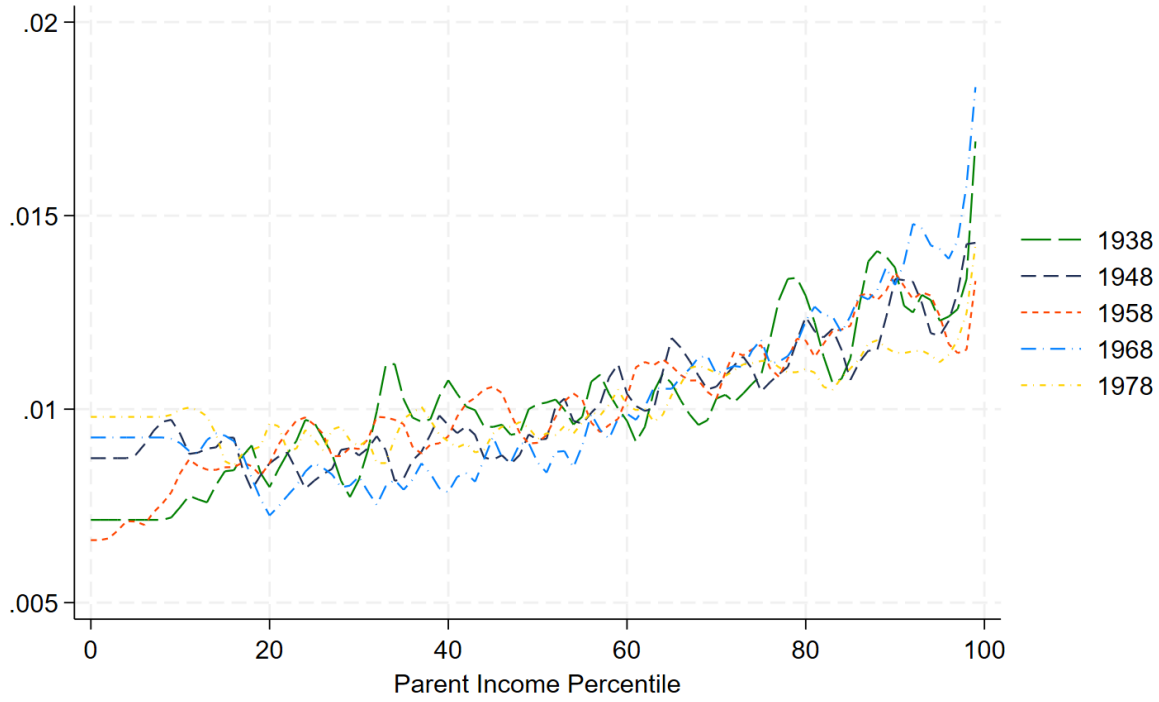


Figure A.3: Parent Income Percentile at Age 12 by Birth Year, Black

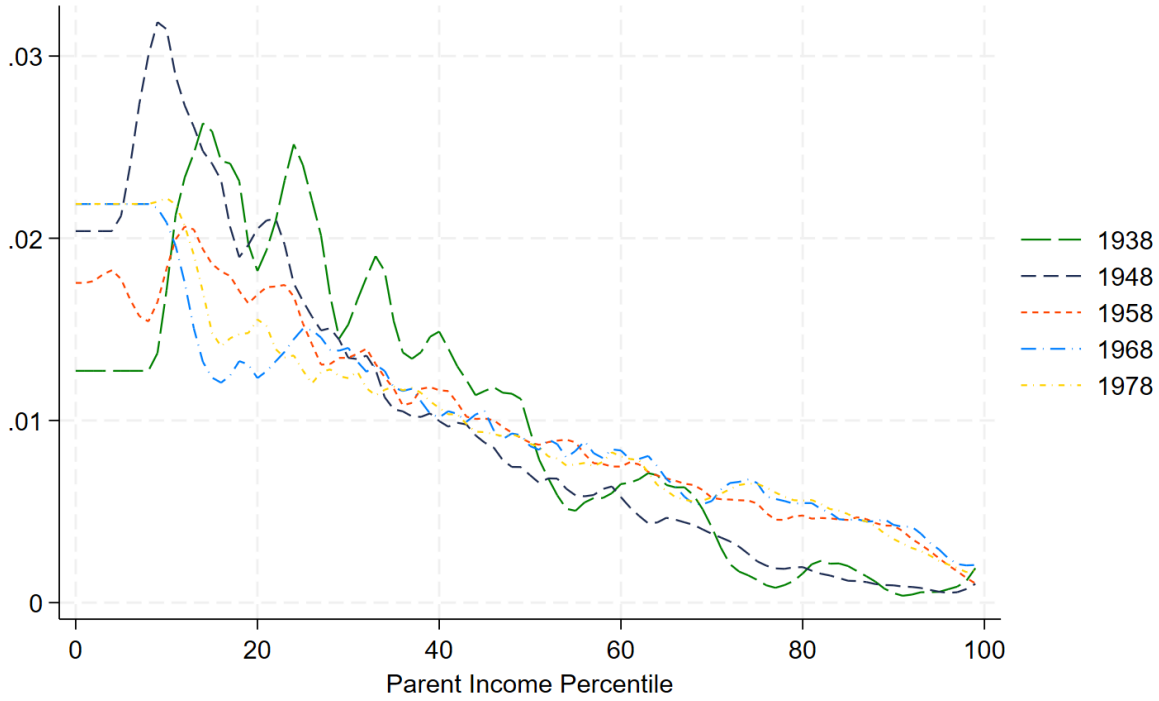


Figure A.4: Parent Income Percentile at Age 12 by Birth Year, White

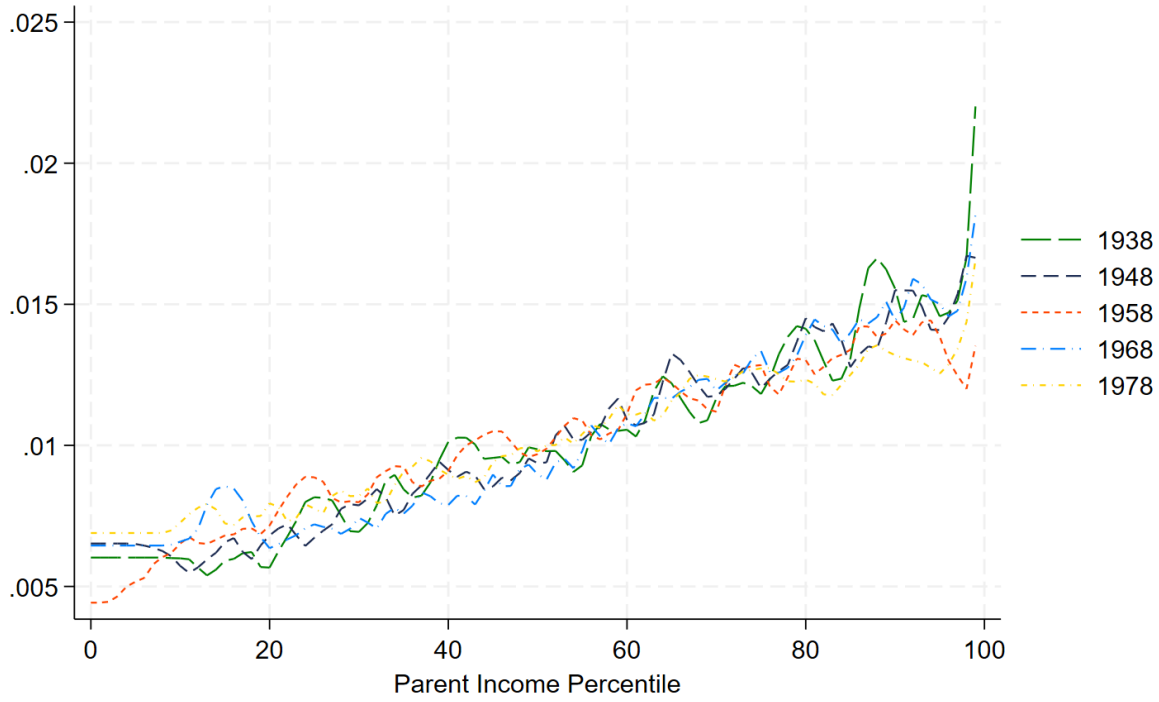


Figure A.5: Cumulative Parent Income Percentile at Age 12 by Birth Year, All Races

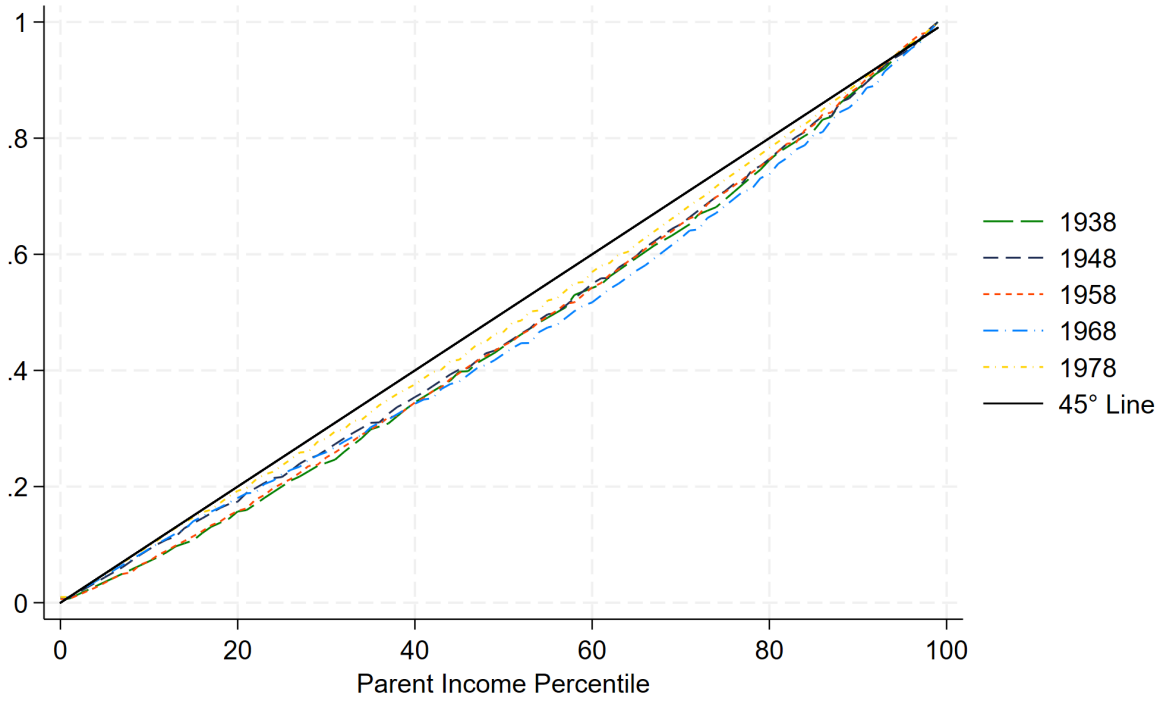


Figure A.6: Cumulative Parent Income Percentile at Age 12 by Birth Year, Black

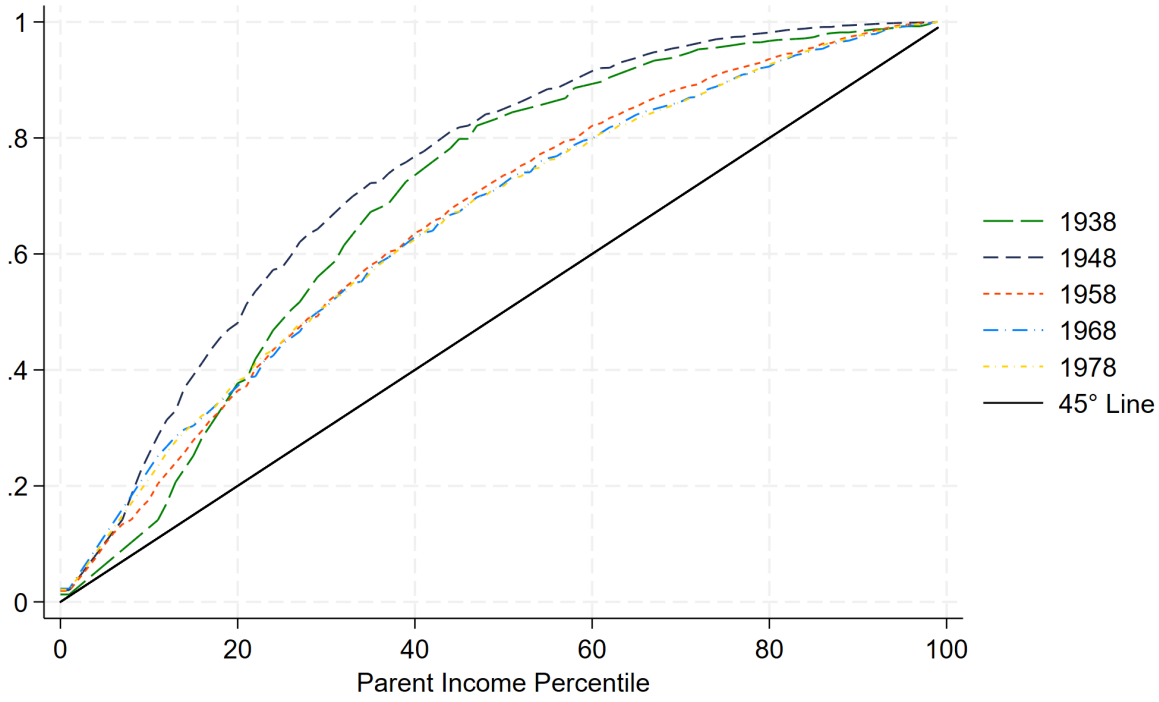
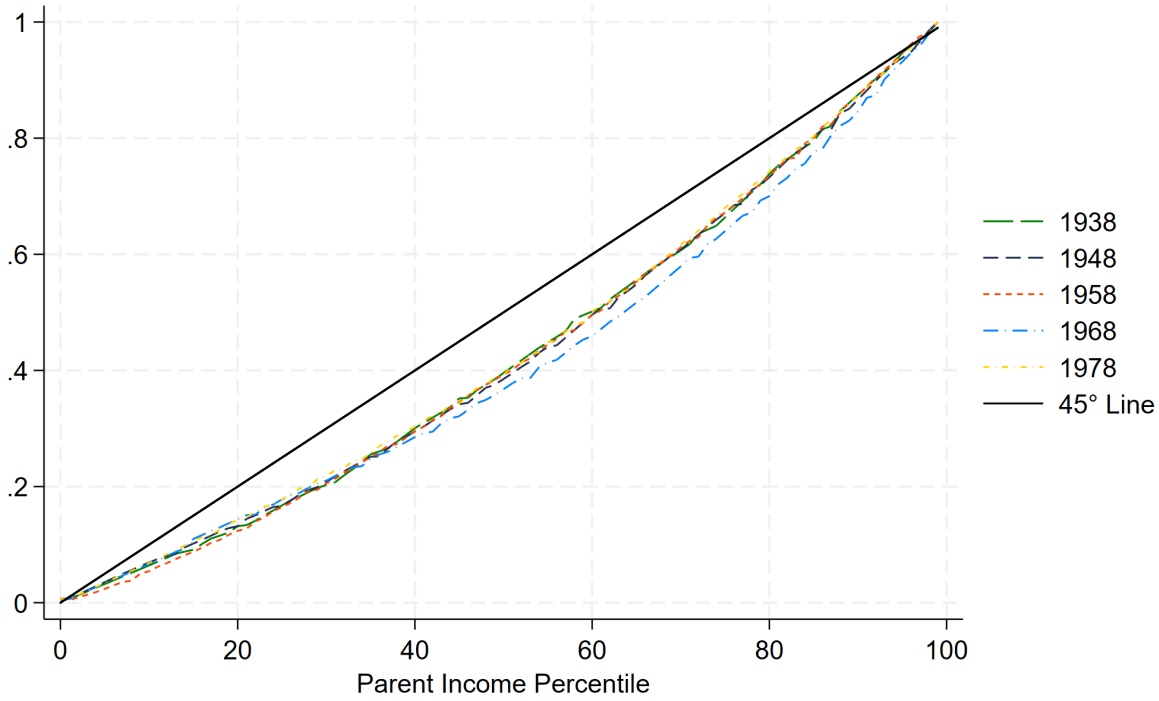


Figure A.7: Cumulative Parent Income Percentile at Age 12 by Birth Year, White



A.1.4 Nested Constant Elasticity of Substitution (CES) Elasticity Estimates

I estimate substitution elasticities of Black and White workers generally following models A and B from Ottaviano and Peri (2012). The top nest is education level. Given substantial differences in education in the South and the North during the study period, I consider six different education bins incorporating region of education. These are less than high school, high school, and any college attainment in the South, and the same in the North.

The second nest is age bins. For this I use five year age bins: ages 25-29, 30-34, 35-39, 40-44, 45-49 and 50-54. The lowest nest is then race: Black and White. With 8 Census years (1950 through 2020), I have 288 total observations.

I first estimate the elasticity of Black and White workers with this equation:

$$\ln \frac{\omega_{Bkt}}{\omega_{Wkt}} = \phi_k + \gamma_t + \beta_1 * \ln \frac{L_{Bkt}}{L_{Wkt}} + \epsilon_{kt} \tag{7}$$

where ω_{Bkt} (ω_{Wkt}) is the annual earnings of Black (White) male full-time, full-year workers in age-education bin k in year t. $L_{Bkt}(L_{Wkt})$ is the Black (White) male population count in bin k and year t.

I then pool Black and White workers together to estimate the age elasticity as follows:

$$\ln \omega_{kt} = \phi_k + \gamma_t + \beta_2 * \ln L_{kt} + \epsilon_{kt} \tag{8}$$

where ω_{kt} is the annual earnings of Black and White male full-time, full-year workers in age-education bin k in year t. L_{kt} is that of the Black and White male population pooled together. In Equation (8), an age group's labor supply is instrumented by the average of the group's birth year fertility rates.

The elasticity of substitution σ_N for N={race, age} is then $\frac{-1}{\beta_1}$ and $\frac{-1}{\beta_2}$, respectively. I show the results from these estimations in Tables A.4 through A.6 below. The implied elasticities from these estimations are shown in Figure 5 in the main text.

Table A.4: Black/White Elasticity Estimates, Overall and By Age Group

	Overall		By Age		
	FTFY	Weekly Wages	25-34	35-44	45-54
	(1)	(2)	(3)	(4)	(5)
Log Relative Supply	-0.052***	-0.050***	-0.063**	-0.044***	-0.026
	(0.011)	(0.009)	(0.024)	(0.012)	(0.016)
Implied Elasticity	19.20	20.01	15.75	22.62	38.05
Observations	288	288	96	96	96

Notes: *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

Table A.5: Black/White Elasticity Estimates, By Education

	Education Subgroups			Region	
	Less Than HS	HS	College	Northern	Southern
	(1)	(2)	(3)	(4)	(5)
Log Relative Supply	-0.036	-0.042***	-0.052**	-0.087***	-0.037
	(0.021)	(0.014)	(0.019)	(0.028)	(0.027)
Implied Elasticity	28.15	23.70	19.16	11.48	27.36
Observations	96	96	96	144	144

Notes: *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

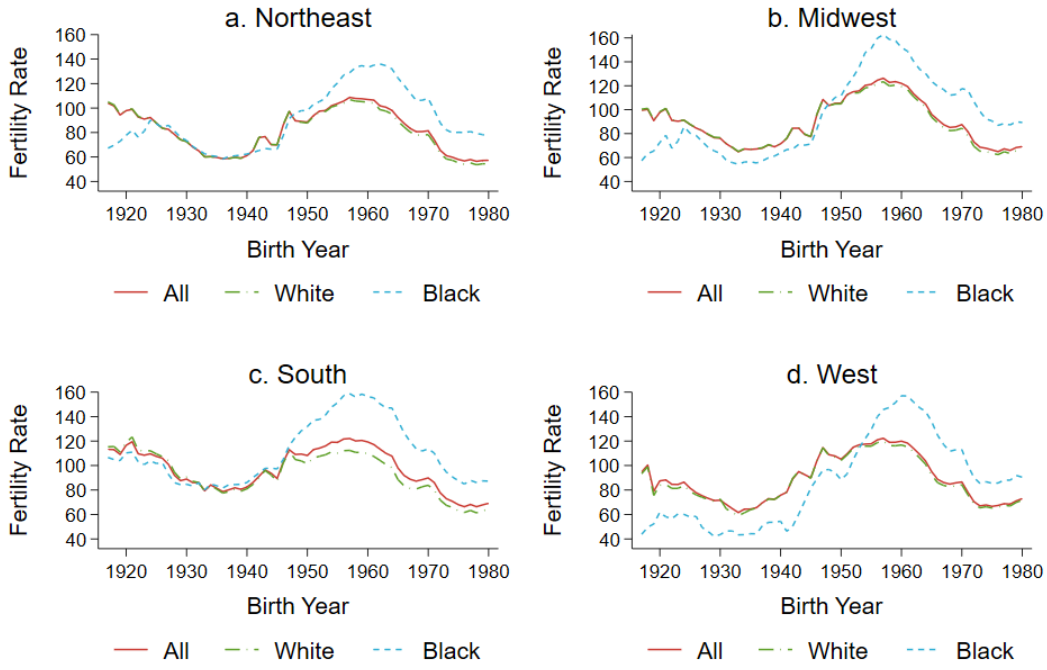
Table A.6: Age Elasticity, Pooled Estimates

	Overall		Education Subgroups		Year Subgroups	
	FTFY	Weekly Wages	HS or Less	Any College	1950-2000	1960-1990
	(1)	(2)	(3)	(4)	(5)	(6)
Log Supply	-0.043*	-0.047	-0.041**	-0.027**	-0.104***	-0.209**
	(0.023)	(0.030)	(0.019)	(0.011)	(0.030)	(0.098)
Implied Elasticity	23.47	21.31	24.15	37.06	9.61	4.79
Observations	288	288	192	96	216	144

Notes: *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

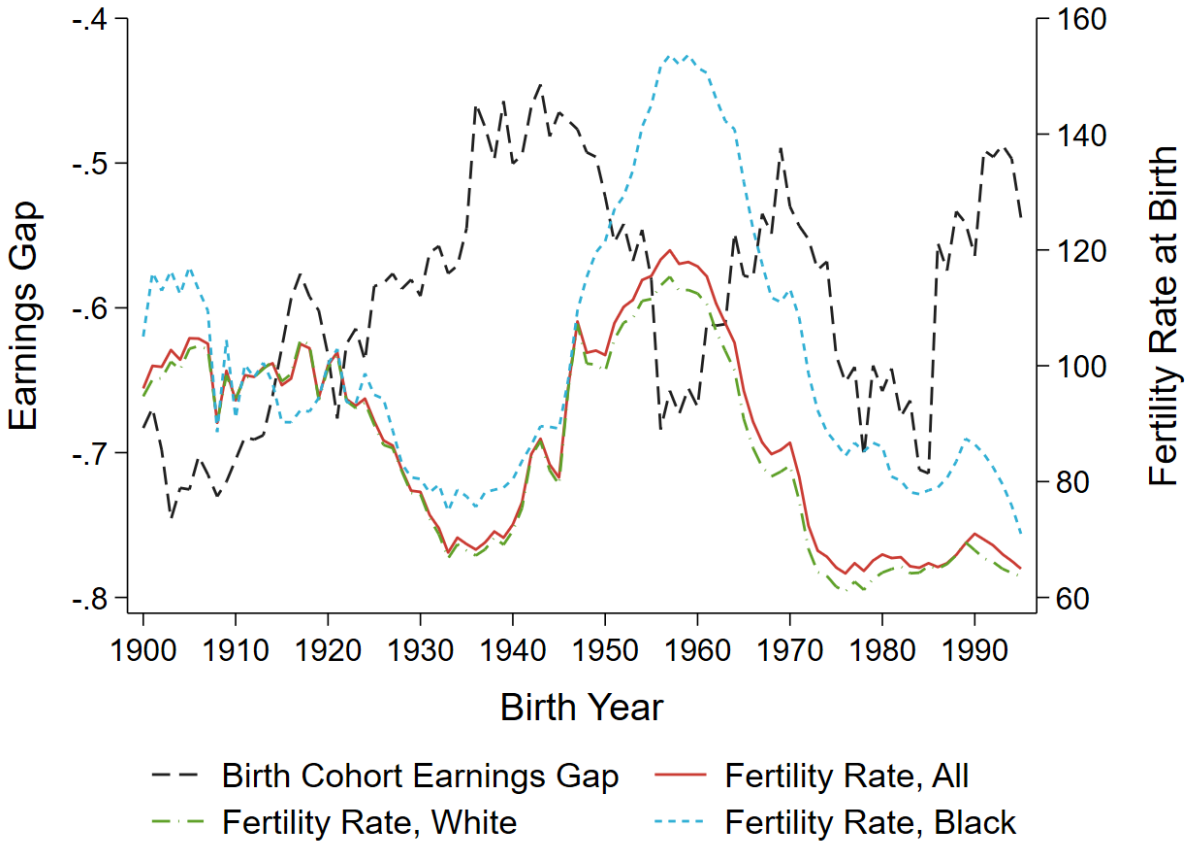
A.2 Supplemental Figures

Figure A.8: U.S. Fertility Rates by Race and Region



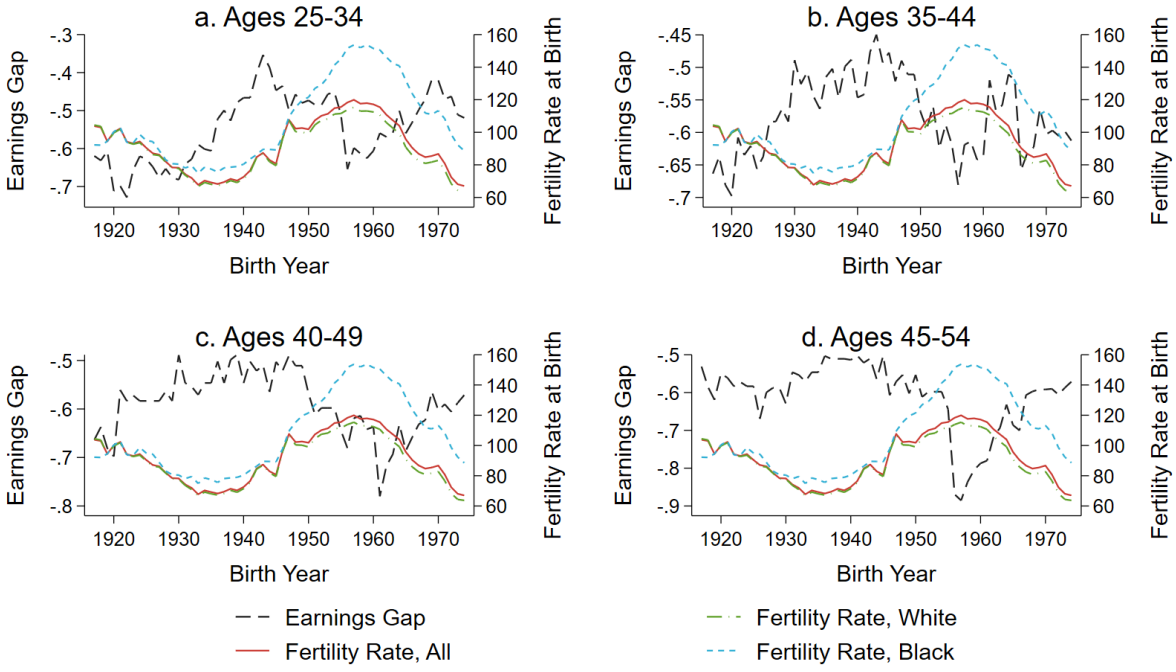
Notes: This figure shows fertility rates using data from the U.S. Vital Statistics, U.S. Census, and American Community Survey. See Section 2.2 for more information on treatment of data.

Figure A.9: Fertility Rates and Cohort's Black-White Median Earnings Gap



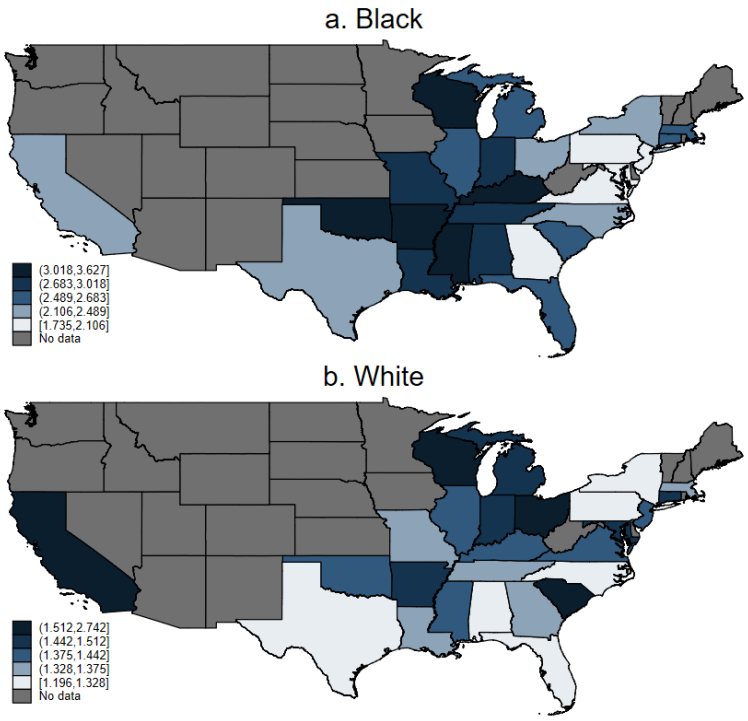
Notes: This figure shows fertility rates using data from the U.S. Vital Statistics, U.S. Census, and American Community Survey. It also shows the level log median earnings gap between all Black and White males within a given birth year. Gaps are estimated using quantile regressions at various ages between 25 and 54 depending on age in each Census year, and a simple average is taken. See Section 2.2 for more information on treatment of data.

Figure A.10: Male Black-White Median Earnings Gap, by Age Group



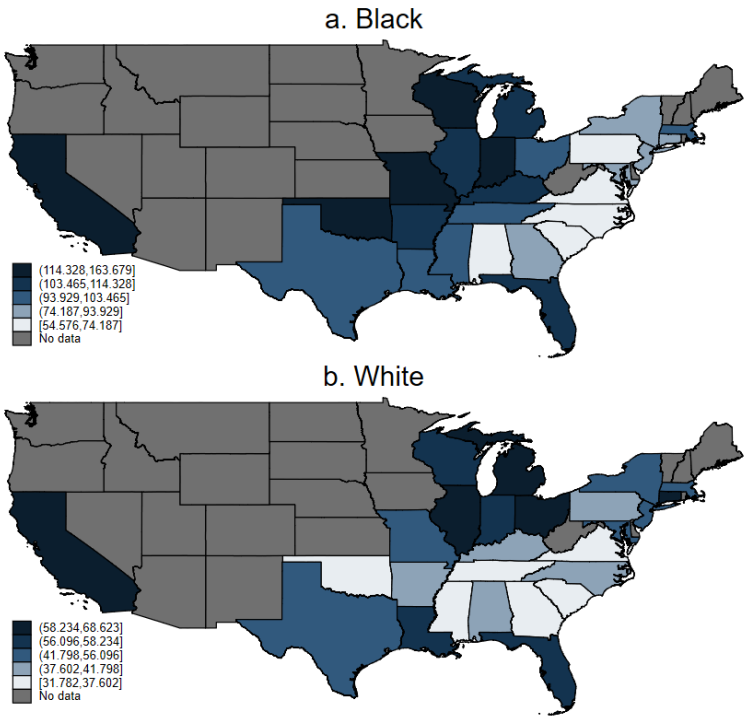
Notes: This figure shows fertility rates using data from the U.S. Vital Statistics, U.S. Census, and American Community Survey. It also shows the level log median earnings gap between all Black and White males within a given birth year. Gaps are estimated using quantile regressions at various ages between 25 and 34, 35 and 44, 40 and 49, and 45 and 54 depending on age in each Census year. See Section 2.2 for more information on treatment of data.

Figure A.11: Change in Cohort Size by State (Identifying Variation)



Notes: This figure shows the maximum minus the minimum cohort size by race and state for the analysis sample years.

Figure A.12: Change in Fertility Rate by State (Instrument)



Notes: This figure shows the maximum minus the minimum fertility rate by race and state for the analysis sample years.

A.3 Supplemental Tables

Table A.7: Impact of Cohort Size on Male Earnings (OLS)

Panel A. Black	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (Black)	-0.115***	-0.083***	-0.066***	-0.025***	-0.013***
	(0.015)	(0.014)	(0.011)	(0.002)	(0.002)
Mean of Dep. Var.	10.456	10.662	10.826	0.739	0.808
Observations	162	162	162	162	162
Panel B. White	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (White)	-0.044***	-0.051***	-0.020*	-0.003***	0.002*
	(0.011)	(0.012)	(0.012)	(0.001)	(0.001)
Mean of Dep. Var.	11.032	11.080	11.161	0.890	0.925
Observations	162	162	162	162	162

Notes: This table shows the results from OLS estimations of Equation 1, without employing the instrumental variable. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

Table A.8: Impact of Cohort Size on Male Earnings and Employment (2SLS), All-Race Cohort Size

Panel A. Black	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (All Races)	-0.149***	-0.122***	-0.091***	-0.032***	-0.018***
	(0.019)	(0.018)	(0.015)	(0.003)	(0.003)
Mean of Dep. Var.	2.941	2.941	2.941	2.941	2.941
Observations	162	162	162	162	162
Panel B. White	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (All Races)	-0.072***	-0.079***	-0.044***	-0.005***	0.001
	(0.012)	(0.013)	(0.012)	(0.001)	(0.001)
Mean of Dep. Var.	2.941	2.941	2.941	2.941	2.941
Observations	162	162	162	162	162

Notes: This table shows the results from 2SLS estimations of Equation 1. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$. Cohort Size Mean: 2.941, Range: [2.320, 3.596]

Table A.9: Impact of Cohort Size on Male Earnings Gaps (2SLS)

	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (All Races)	-0.077***	-0.042***	-0.047***	-0.028***	-0.019***
	(0.017)	(0.010)	(0.009)	(0.003)	(0.002)
Mean of Dep. Var.	-0.577	-0.418	-0.335	-0.151	-0.117
Observations	162	162	162	162	162

Notes: This table shows the results from 2SLS estimations of Equation 1. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$. Cohort Size Mean: 2.941, Range: [2.320, 3.596]

Table A.10: Impact of Cohort Size on Male Earnings, Fully Interacted Stacked Differences (2SLS)

	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size	-0.066***	-0.073***	-0.039***	-0.004**	0.001
	(0.014)	(0.014)	(0.012)	(0.002)	(0.002)
Cohort Size x Black	-0.092***	-0.052***	-0.063***	-0.027***	-0.019***
	(0.020)	(0.019)	(0.017)	(0.003)	(0.002)
Mean of Dep. Var.	10.744	10.871	10.994	0.814	0.867
Mean White Cohort Size	2.994	2.994	2.994	2.994	2.994
Mean Black Cohort Size	3.628	3.628	3.628	3.628	3.628
Observations	324	324	324	324	324

Notes: This table shows the results from 2SLS estimations of Equation 1 but as a fully stacked model with separate observations for Black and White in each cohort, and an interaction with a Black indicator for all variables. Regressions include age and year fixed effects (and their interaction with the Black indicator). Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

Table A.11: Impact of Cohort Size on Male Earnings and Employment (2SLS), Alternative Race Measure

	Log Median Earnings			Employed Share	Labor Force
Panel A. Black	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (All Races)	-0.148***	-0.121***	-0.091***	-0.032***	-0.017***
	(0.020)	(0.017)	(0.016)	(0.003)	(0.003)
Mean of Dep. Var.	10.454	10.661	10.826	0.739	0.808
Observations	162	162	162	162	162
	Log Median Earnings			Employed Share	Labor Force
Panel B. White	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (All Races)	-0.072***	-0.080***	-0.044***	-0.005***	0.001
	(0.012)	(0.012)	(0.011)	(0.001)	(0.001)
Mean of Dep. Var.	11.033	11.080	11.161	0.890	0.925
Observations	162	162	162	162	162

Notes: This table shows the results from 2SLS estimations of Equation 1. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$

Table A.12: Impact of Race-Specific Cohort Size on Female Outcomes (2SLS)

Panel A. Black	Log Median Earnings		Employed Share	Poverty	Share Ever Married	
	Earners	FTFY	of Pop.	Share	Married Only	Plus Cohabit.
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (Black)	-0.054***	-0.052***	-0.006	0.015***	-0.058***	-0.055***
	(0.019)	(0.014)	(0.005)	(0.004)	(0.013)	(0.011)
Mean Cohort Size	3.628	3.628	3.628	3.628	3.628	3.628
Mean of Dep. Var.	10.213	10.526	0.606	0.274	0.786	0.798
Observations	162	162	162	153	162	162
Panel B. White	Log Median Earnings		Employed Share	Poverty	Share Ever Married	
	Earners	FTFY	of Pop.	Share	Married Only	Plus Cohabit.
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (White)	0.073***	0.016***	0.040***	0.003**	-0.026***	-0.028***
	(0.010)	(0.006)	(0.004)	(0.001)	(0.007)	(0.006)
Mean Cohort Size	2.994	2.994	2.994	2.994	2.994	2.994
Mean of Dep. Var.	10.392	10.710	0.592	0.076	0.903	0.911
Observations	162	162	162	153	162	162

Notes: This table shows the results from 2SLS estimations of Equation 1. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

Table A.13: Impact of Race-Specific Cohort Size on Male Log Median Earnings (Among Earners) and Employed Share of Population, By Educational Attainment (2SLS)

Panel A. Black	Less than High School		High School		Any College	
	Earnings	Emp.	Earnings	Emp.	Earnings	Emp.
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (Black)	-0.138***	-0.054***	-0.112***	-0.020***	-0.107***	-0.020***
	(0.020)	(0.008)	(0.014)	(0.004)	(0.014)	(0.004)
Mean Cohort Size	3.628	3.628	3.628	3.628	3.628	3.628
Mean of Dep. Var.	10.339	0.598	10.673	0.827	10.927	0.827
Observations	162	162	162	162	162	162
Panel B. White	Less than High School		High School		Any College	
	Earnings	Emp.	Earnings	Emp.	Earnings	Emp.
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (White)	-0.064***	-0.021***	-0.050***	0.002	-0.031***	0.002
	(0.010)	(0.003)	(0.009)	(0.003)	(0.010)	(0.003)
Mean Cohort Size	2.994	2.994	2.994	2.994	2.994	2.994
Mean of Dep. Var.	10.734	0.756	10.989	0.923	11.233	0.923
Observations	162	162	162	162	162	162

Notes: This table shows the results from 2SLS estimations of Equation 1. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

Table A.14: Impact of Cohort Size on Male Earnings: Quartic (2SLS)

	All Men		All Male Earners	
	Black	White	Black	White
	(1)	(2)	(3)	(4)
Cohort Size	-55.008	199.576	-15.004	200.444
	(59.593)	(141.497)	(53.833)	(151.632)
Cohort Sizeˆ2	22.659	-99.759	6.456	-100.154
	(23.872)	(71.248)	(21.565)	(76.351)
Cohort Sizeˆ3	-4.102	22.001	-1.228	22.076
	(4.218)	(15.841)	(3.810)	(16.976)
Cohort Sizeˆ4	0.275	-1.807	0.086	-1.812
	(0.277)	(1.312)	(0.251)	(1.407)
Wald P-Value		0.448		0.665
Observations	162	162	162	162

Notes: This table shows the results from 2SLS estimations of Equation 1 with additional covariates of cohort size to the second, third, and fourth powers. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. Column (2) shows the p-value from a Wald test for whether the four cohort size coefficients are jointly zero between Columns (1) and (2), and likewise in Column (4) for the estimates from Columns (3) and (4). *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

Table A.15: Impact of Race-Specific Cohort Size on Other Male Outcomes (2SLS)

	Mean Log Earnings		Poverty	Incarcerated	Share Ever Married	
Panel A. Black	Earners	FTFY	Share	Share	Married Only	Plus Cohabit.
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (Black)	-0.136***	-0.081***	0.012***	0.010***	-0.070***	-0.068***
	(0.018)	(0.013)	(0.002)	(0.003)	(0.012)	(0.009)
Mean Cohort Size	3.628	3.628	3.628	3.628	3.628	3.628
Mean of Dep. Var.	10.452	10.755	0.185	0.053	0.760	0.778
Observations	162	162	153	162	162	162
	Mean Log Earnings		Poverty	Incarcerated	Share Ever Married	
Panel B. White	Earners	FTFY	Share	Share	Married Only	Plus Cohabit.
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (White)	-0.055***	-0.038***	0.003***	0.000	-0.031***	-0.034***
	(0.011)	(0.010)	(0.001)	(0.000)	(0.009)	(0.007)
Mean Cohort Size	2.994	2.994	2.994	2.994	2.994	2.994
Mean of Dep. Var.	10.939	11.110	0.058	0.010	0.852	0.862
Observations	162	162	153	162	162	162

Notes: This table shows the results from 2SLS estimations of Equation 1. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

Table A.16: Impact of Cohort Size on Male Earnings and Employment, State-Level TWFE (2SLS)

Panel A. Black	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (Black)	-0.178***	-0.075***	-0.052***	-0.027***	-0.019***
	(0.034)	(0.011)	(0.008)	(0.003)	(0.003)
Mean of Dep. Var.	10.468	10.731	10.900	0.706	0.782
Mean Cohort Size	3.772	3.772	3.772	3.772	3.772
Observations	3,645	3,645	3,645	3,645	3,645
Panel B. White	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (White)	-0.035***	-0.024***	-0.009	-0.008***	-0.003***
	(0.008)	(0.008)	(0.008)	(0.001)	(0.001)
Mean of Dep. Var.	11.073	11.126	11.213	0.884	0.921
Mean Cohort Size	2.940	2.940	2.940	2.940	2.940
Observations	3,645	3,645	3,645	3,645	3,645

Notes: This table shows the results from 2SLS estimations of Equation 4. Regressions include state, age and year fixed effects. Instead of wild cluster bootstrapped standard errors as used throughout the state panel analysis, this table shows traditional standard errors clustered at the state level. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

Table A.17: Impact of Race-Specific Cohort Size on Male Earnings and Employment (2SLS), Restricted to 1926-1970 Birth Years

Panel A. Black	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (All Races)	-0.140***	-0.074***	-0.058***	-0.031***	-0.019***
	(0.024)	(0.018)	(0.016)	(0.004)	(0.003)
Mean of Dep. Var.	2.914	2.914	2.914	2.914	2.914
Observations	135	135	135	135	135
Panel B. White	Log Median Earnings			Employed Share	Labor Force
	All	Earners	FTFY	of Pop.	Share
	(1)	(2)	(3)	(4)	(5)
Cohort Size (All Races)	-0.031***	-0.034***	-0.002	-0.006***	-0.002*
	(0.011)	(0.011)	(0.010)	(0.001)	(0.001)
Mean of Dep. Var.	2.914	2.914	2.914	2.914	2.914
Observations	135	135	135	135	135

Notes: This table shows the results from 2SLS estimations of Equation 1. Regressions include age and year fixed effects. Standard errors clustered at the level of birth year in parentheses. *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

Table A.18: Impact of Cohort Size on Male Earnings and Employment (2SLS), Alternative Sample Selections

	Vital Statistics Years Only			Including All States and Birth Years		
	Log Median Earnings			Log Median Earnings		
Panel A. Black	All	Earners	FTFY	All	Earners	FTFY
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (Black)	-0.175***	-0.072***	-0.050***	-0.170***	-0.077***	-0.054***
Wild Bootstrap CI	[-0.248, -0.105]	[-0.097, -0.047]	[-0.070, -0.031]	[-0.239, -0.108]	[-0.099, -0.055]	[-0.070, -0.036]
Mean of Dep. Var.	10.468	10.731	10.900	10.465	10.732	10.902
Mean Cohort Size	3.772	3.772	3.772	3.781	3.781	3.781
Observations	3,591	3,591	3,591	6,128	6,074	5,875
	Log Median Earnings			Log Median Earnings		
Panel B. White	All	Earners	FTFY	All	Earners	FTFY
	(1)	(2)	(3)	(4)	(5)	(6)
Cohort Size (White)	-0.033***	-0.022**	-0.008	-0.029***	-0.020***	-0.006
Wild Bootstrap CI	[-0.052, -0.015]	[-0.040, -0.006]	[-0.026, 0.009]	[-0.044, -0.017]	[-0.034, -0.008]	[-0.021, 0.007]
Mean of Dep. Var.	11.073	11.126	11.213	11.062	11.115	11.202
Mean Cohort Size	2.940	2.940	2.940	2.974	2.974	2.974
Observations	3,591	3,591	3,591	6,615	6,615	6,615

Notes: This table shows the results from 2SLS estimations of Equation 4. Regressions include state, age and year fixed effects. Confidence intervals in brackets are based on a wild cluster bootstrap estimation of standard errors clustered at the state level, and the significance levels indicated by that procedure are represented by: *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.

Table A.19: Impact of Cohort Size on Earnings by Occupation Type (State-Level, 2SLS), Men Ages 30-49

	High Earning	High Education	White	Manufacturing	Low Earning	Low Education	Black	Non-Manufacturing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cohort Size (All Races)	-0.006	0.002	-0.002	-0.015	-0.050***	-0.032***	-0.043***	-0.032***
Wild Bootstrap CI	[-0.019, 0.008]	[-0.012, 0.018]	[-0.015, 0.012]	[-0.037, 0.005]	[-0.072, -0.029]	[-0.054, -0.014]	[-0.068, -0.020]	[-0.050, -0.017]
Mean of Dep. Var.	11.408	11.390	11.329	11.137	10.766	10.920	10.795	11.137
Observations	2,430	2,430	2,430	2,430	2,430	2,430	2,430	2,430

Notes: This table shows the results from 2SLS estimations of Equation 4. Regressions include state, age and year fixed effects. Confidence intervals in brackets are based on a wild cluster bootstrap estimation of standard errors clustered at the state level, and the significance levels indicated by that procedure are represented by: *= $p < 0.1$, **= $p < 0.05$, ***= $p < 0.01$.