Blindly Discriminating: The GI Bill and Racial Inequality*

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Abstract

The World War II GI Bill was the largest education subsidy in US history and a cornerstone of the postwar US transition to a knowledge economy. Although the GI Bill's language was race-blind, its decentralized administration left implementation to local officials and segregated institutions, with sharply different consequences for Black and white veterans. This paper quantifies the GI Bill's impact on Black and white Americans' economic outcomes across two generations, using a regression discontinuity around WWII service eligibility cutoffs and a new data linkage from veterans in the 1940 and 1950 censuses to their sons' outcomes in the 2000s. The GI Bill widened racial inequality, doubling white veterans' college completion while steering Black veterans into often-fraudulent vocational programs with no earnings returns. The disparities propagated across generations, increasing the white-Black gap in sons' adult-neighborhood outcomes, including a 5-percentage-point (47 percent) widening of the racial college gap. Unequal returns to the same eligibility account for the intergenerational gap, with no contribution from prewar differences in socioeconomic status or geography.

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1. Introduction

The US college graduation rate grew by half between 1940 and 1960, and the World War II GI Bill ("Servicemen's Readjustment Act of 1944") is often credited with this transformation. The largest education subsidy in US history, it rapidly expanded the supply of college-educated workers and helped lay the foundation for the postwar knowledge economy (Acemoglu, 1998, 2002; Katz and Murphy, 1992; Goldin and Katz, 2008). The program's language was race-blind, but segregation, local administrative discretion, and discriminatory labor markets meant that the same eligibility yielded very different benefits by race. Because the GI Bill arrived just as the returns to college began their dramatic rise, racial disparities may have widened over generations. As Katznelson (2005) put it, there may have been "no greater instrument to widening an already huge racial gap in postwar America than the GI Bill."

This paper studies how the GI Bill affected Black and white Americans' economic outcomes across two generations. To identify the joint effects of WWII service eligibility and the GI Bill, we compare men just old enough to be drafted to those just too young—a regression discontinuity based on age-based eligibility cutoffs (Fetter, 2013; Collins and Zimran, 2025). We trace those effects to the next generation through a new linkage from fathers in the 1940 and 1950 census to their sons' adult address histories up until 2025. We find that effects of WWII eligibility are concentrated in college and vocational training, the GI Bill's two largest spending categories. Placebo designs around World War I (no GI Bill) and the Korean War (similar GI Bill) suggest that the GI Bill, not military service, drives the results.

Our first key finding is that being born early enough to serve in WWII and subsequently qualify for the GI Bill raised white men's college attainment but shifted Black men toward vocational training. Both Black and white men born just before the WWII eligibility cutoff were 9 and 17 percentage points more likely to become veterans, respectively. Our instrumental variable estimates imply that, for induced white veterans, four-year college completion rose by 9.5 percentage points, more than doubling the baseline of 9 percent. Induced Black veterans saw no comparable college gain. Instead, their vocational training completion rose by 36 percentage points. An array of qualitative evidence establishes that local VA counselors actively steered Black veterans away from college and into vocational programs, exploiting the GI Bill's decentralized administration (Humes, 2006; Turner and Bound, 2003; Herbold, 1994).

Our second key finding is that the GI Bill's education benefits yielded large returns for

white but not Black veterans. A Mincer-style regression of earnings on educational attainment (Mincer, 1974) shows positive earnings returns to college but near-zero returns to vocational training for Black men. Two forces drove the gap. First, the same minimal federal oversight that enabled steering allowed the vocational sector to expand from 100 institutions before the war to over 10,000 by 1950, with many schools emerging to capture GI Bill payments rather than deliver marketable skills (Olson, 1974; Frydl, 2009); Black veterans' vocational enrollment was concentrated in states with the most documented fraud. Second, discriminatory labor markets paid Black workers less for the same education even where credentials had value (Carruthers and Wanamaker, 2017). We find no effects on homeownership—another prominent but lower-expenditure target of the GI Bill—for either Black or white veterans, largely consistent with Fetter (2013).

Our third key finding is that the GI Bill widened racial inequality across generations. We construct a novel data linkage from fathers in the 1940 and 1950 censuses to their sons' adult address histories from 1990 to 2025. This linkage allows us to measure their adult-neighborhood socioeconomic status through race-specific block-group tabulations matched to each address. Sons of white induced veterans live as adults in neighborhoods with a 1.5-percentage-point higher college share, 1.6-percentage-point higher postgraduate share, and $3,347 higher median income (7 percent of the baseline) than sons of ineligible white fathers. Sons of induced Black veterans experience the opposite: their adult neighborhoods have a 10.4-percentage-point lower college share, with no corresponding gain in income. The educational steering of Black veterans thus carried large economic costs that extended to their children.

Our fourth key finding is that the GI Bill's racial gap cannot be explained by preexisting differences between Black and white veterans. Many Black men entered the war without the high school diploma needed for college, and were concentrated in the segregated South, where they were excluded from white colleges and HBCUs alone could not meet postwar demand. Under these conditions, the GI Bill's racial gap might have emerged even absent discrimination in its administration. But we find this alternative cannot account for the gap. Reweighting white veterans to match Black veterans on prewar education, income, and geography does little to shrink the disparity, and a formal decomposition attributes the entire gap to differential returns rather than prewar endowments. Within the South, Black veterans saw no college gains even in states with high HBCU density. The gap is also largest in areas with the most historical Klan presence, our proxy for entrenched prewar racial discrimination.

The intergenerational effects have a first-order impact on aggregate racial inequality.

Scaling our race-specific instrumental variable estimates by the cohort-specific share of fathers' veteran status implies that the GI Bill widened the white-Black gap college gap for sons by 47 percent (5 percentage points) and the corresponding income gap by 11 percent. For comparison, the exclusion of many Black workers from minimum wage coverage prior to 1966 widened the racial earnings gap by 20 percent (Derenoncourt and Montialoux, 2021).

Prior work documents direct effects of the GI Bill on veterans' college completion and homeownership (Bound and Turner, 2002; Stanley, 2003; Angrist, 1993; Collins and Zimran, 2025). Those estimates show that the GI Bill expanded college access for lower- and middle-income students only at non-elite colleges (Abramitzky et al., 2024), and highlight racial disparities in benefit utilization (Turner and Bound, 2003; Herbold, 1994; Eden, 2023; Katznelson and Mettler, 2008). We make three contributions. First, we identify the steering of Black veterans into vocational training—one of the program's largest spending categories and the one with the largest enrollment—as a key channel through which the GI Bill widened racial inequality. Second, we construct the first data linkage that follows WWII-era fathers to their sons' adult-neighorhood outcomes, allowing us to trace the GI Bill's racial effects across generations. Third, we show that this intergenerational gap reflects differential returns rather than pre-existing differences between Black and white veterans, indicating that race-blind subsidies channelled through unequal institutions can widen rather than close racial gaps.

Our results also contribute to a literature on the sources of slow Black-white economic convergence in the second half of the twentieth century. Existing work identifies a series of policies and events that narrowed racial gaps: improvements in Black school quality (Card and Krueger, 1992), school desegregation (Johnson, 2011), civil rights enforcement (Donohue and Heckman, 1991), the extension of the federal minimum wage (Derenoncourt and Montialoux, 2021), and the Great Migration out of the Jim Crow South (Derenoncourt, 2022). These forces produced substantial convergence between 1940 and 1970, but Black-white convergence stalled and partially reversed thereafter (Bayer and Charles, 2018; Chetty et al., 2020; Derenoncourt et al., 2024). We identify the GI Bill as a major policy that widened the racial gap, expanding white college attainment just before the college wage premium surged (Goldin and Katz, 2008; Juhn et al., 1993). With Black veterans largely excluded, racial gaps widened as the postwar economy restructured around college credentials, contributing to the post-1970s stagnation.

Our results also expand the growing evidence that *de jure* race-blind policies often generate racially unequal outcomes. Work on the American welfare state emphasizes the gap

between *de jure* universality and de facto equal access (Alston and Ferrie, 1999; Katznelson, 2005; Fishback et al., 2006), and recent evidence reveals that race-blind policies can narrow or widen racial gaps depending on how institutions translate eligibility into opportunity (Aaronson et al., 2021; Derenoncourt and Montialoux, 2021). We show that the same race-blindness can also widen gaps when implementation depends on discretionary local institutions, with effects that persist across generations and operate primarily through the steering of Black Americans into low-return programs.

2. Historical Background

World War II mobilized over 16 million Americans, and the GI Bill offered returning veterans generous education, training, and other benefits, fueling a postwar surge in college completion. The program was race-blind by statute, but qualitative and historical accounts document that Black veterans were steered away from college and into vocational programs. Appendix Table A1 provides a timeline of WWII and the GI Bill.

2.1. World War II and Military Service

WWII was the largest US military engagement of the 20th century. The country mobilized 16 million men, including more than three-quarters of those born in the 1920s. All men aged 18 to 64 were required to register for the draft, with those aged 18 to 44 liable for induction and younger men disproportionately inducted. The draft accounted for 70 percent of those who served, while the rest volunteered before voluntary enlistment was suspended in December 1942 (Stanley, 2003).

Black and white men enlisted and were drafted at similar rates, but the military's segregation policies and limited capacity for Black troops lowered Black induction rates.[1] Literacy requirements and the Army General Classification Test introduced in 1943 further constrained Black induction. Throughout the war, segregation remained official policy: white servicemen served across the full range of military occupations, while Black servicemen were assigned to service units that performed low-skill logistical work (Murray, 1971; Herbold, 1994). Black units served under white officers with few opportunities for advancement.

Men born too late to serve in WWII became eligible for the Korean War (1950–1953),

[1] In 1940, the US Army had only six Black units with just 4,450 soldiers (Turner and Bound, 2003).

during which 6.8 million Americans served. Unlike in WWII, college students were eligible for deferments, leading to sharp increases in postsecondary enrollment among draft-eligible men (Card and Lemieux, 2001). Korea was also the first conflict in which Black and white troops served in desegregated units (Indacochea, 2019), and returning veterans were eligible for similar benefits under the revised GI Bill.

2.2. The GI Bill

The Servicemen's Readjustment Act of 1944 ("GI Bill") gave returning veterans education, training, and homeownership benefits.

Education and Training Benefits. The GI Bill offered three distinct education and training tracks, each with its own payment structure and institutional setting. *College-level training* covered full tuition at any US college, including the most expensive private institutions, plus monthly stipends for room and board. *Vocational training* funded tuition and stipends at non-collegiate programs, including vocational and technical schools, business and secretarial schools, nursing schools, trade schools, and other schools below college level. *On-the-job* and *on-the-farm training* were structured instead as wage subsidies paid through a participating employer or supervising farm. The veteran had to secure an employer or farm operator willing to train him and have that sponsor certified as a training site by the state, after which the veteran drew both a reduced training wage from the sponsor and a subsistence allowance from the Veterans Administration (VA). Benefit duration scaled with length of service: veterans received one year of education plus one additional month for every month of active duty, up to a maximum of 48 months.

Vocational training and college each absorbed 38 percent of GI Bill education and training spending, while on-the-job and on-the-farm training together absorbed 24 percent (President's Commission on Veterans' Pensions, 1956; see Appendix Figure A1 for the full spending breakdown). Measured by enrollment, 29 percent of WWII beneficiaries attended college, 44 percent enrolled in vocational training, and the remaining 27 percent pursued on-the-job or on-the-farm training.[2] By the time the Bill expired in 1956, 7.8 million veterans had drawn on the education benefits (Stanley, 2003).

Before the war, less than five percent of adults had completed four or more years of college. Returns to college were lower than they would become after the war, as the

[2]Veterans Education Success, "1956 Education Law Ends G.I. Bill," archived at https://vetsedsuccess.org/wp-content/uploads/2018/09/1956-education-law-ends-gi-bill.pdf.

economy was concentrated in agriculture and manufacturing rather than skilled services, and most workers entered the labor force with a high school diploma or less. The GI Bill helped transform this landscape: by 1960, the share of adults with a college degree had grown by more than half. This supply expansion contributed to a long-run reshaping of the US economy around college-educated workers, with the college wage premium rising sharply from the 1980s (Katz and Murphy, 1992; Goldin and Katz, 2008).

Other Benefits. Beyond education, the GI Bill also expanded access to homeownership through federally backed loan guarantees. Rather than issuing loans directly, the government guaranteed a share of each loan—initially 50 percent of the loan amount, rising to 60 percent by 1950—at an interest rate initially capped at 4 percent (Chambers et al., 2014). More than 6.3 million veterans used the program, and over 60 percent of those who purchased homes could not have afforded a down payment otherwise (Fetter, 2013). The Bill also provided unemployment compensation of 20 US dollars per week for up to 52 weeks, job placement services, and health and disability benefits.

Racial Disparities. The GI Bill was race-blind in statute, but its decentralized administration allowed Jim Crow institutions to steer Black veterans away from the program's highest-return components. While the federal government set eligibility rules and benefit levels, implementation was delegated to local VA offices and state-level institutions. Segregationist politicians ensured that the law explicitly limited federal oversight, stipulating that "no department, agency, or officer of the United States, in carrying out the provisions of this part, shall exercise any supervision or control, whatsoever, over any State educational agency, or State apprenticeship agency, or any educational or training institution." By ceding authority to local actors, this design enabled discriminatory practices to persist without violating the bill's formal race neutrality.

Four features of the institutional environment shaped which veterans enrolled in colleges and the returns they received. First, colleges that could be attended by Black Americans were scarce as much of higher education was racially segregated (Turner and Bound, 2003). Second, politically appointed committees were given full discretion to decide whether schools were given permission to train veterans under the GI Bill. Few schools were allowed to teach Black veterans in the standard trades (Nasaw, 2025). Third, local VA counselors steered Black veterans away from college and into vocational programs. As one account notes, counselors "didn't merely discourage black veterans. They just said no. [...] No to job placement, except for the most menial positions. And no to

college" (Humes, 2006). Fourth, discriminatory labor markets reduced the returns to education even for veterans who obtained credentials. In 1946, 24 percent of job orders processed by US Employment Service offices were found to be explicitly racially discriminatory (Fair Employment Practice Committee, 1946; Katznelson, 2005), and substantial wage gaps persisted conditional on education (Carruthers and Wanamaker, 2017).

The result was that Black veterans were disproportionately allocated to vocational training rather than four-year colleges. The vocational training sector expanded from 100 institutions before 1944 to more than 10,000 by 1950, with many schools emerging primarily to capture GI Bill funds rather than to deliver marketable skills (Olson, 1974; Frydl, 2009). For Black veterans, this meant formal participation without access to the college pathways that drove white veterans' long-run gains, and the cash-equivalent value of their benefits was lower (Eden, 2023). As Frydl (2009) concludes, "Black veterans did not experience the same GI Bill as white veterans, and it is also a fact that this result did not stem from any direct discrimination in the Bill itself. It was a feature of its implementation—and an intended one."

Contemporary investigations confirmed widespread quality problems in the vocational sector that absorbed Black veterans. A 1949 audit found that 65 percent of vocational schools had payments questioned, leading to the recovery of $1.35 million (Teague Committee, 1952, pp. 49–50). Three years later, a House Select Committee identified hundreds of vocational schools as fraudulent, concluding that "the veterans' training program at the college level has enjoyed more harmony and success than any other phase of the program. [...] The greatest amount of waste, inefficiency, and fraud has occurred in [vocational] training" (Teague Committee, 1952, p. 221). Appendix Figure A2 reproduces a Committee photograph of one such school, clearly too small to accommodate the large number of veterans it was supposed to train.

Millions in Waste Charged In Schooling for Veterans

"Millions of dollars are being wasted by the Federal Government on the education and training of veterans who are attending vocational or trade schools of questionable value. Many so-called 'educators' have organized schools for the sole purpose of getting a slice of the $2,500,000,000 paid out annually by the Veterans Administration for the education of ex-servicemen. More than 5,600 of the 8,800 approved schools below college level which operate for profit have been established since the [GI Bill] went into effect in 1944."

The New York Times, front page, February 6, 1950.

3. Empirical Strategy and Data

Our design exploits a sharp birth-cohort discontinuity in WWII service eligibility, and therefore in GI Bill benefits: men born before the third quarter of 1927 were far more likely to serve than men born just after, who were too young to be drafted before the war ended. Comparing outcomes around this threshold yields causal estimates of the effects of military service and GI Bill eligibility, which we estimate separately by race for veterans and for the next generation. For veterans, we use 1970 and 1980 census cross sections. For their descendants, we construct a new data linkage: we identify fathers in the 1940 and 1950 censuses, match their sons to 1990–2025 address histories, and merge each address to 2000-era race-specific block-group tabulations.

3.1. Age-Based Eligibility Cutoff and First Stage

Our identification exploits the sharp drop in veteran status at birth quarter 1927.5, driven by the minimum age to serve in WWII: men born before this cutoff were more likely to serve and to receive GI Bill benefits than men born just after. One caveat is that men born just after the cutoff, though too young for WWII, were old enough for Korean War service (1950–1953), which carried its own GI Bill with similar education benefits. We therefore define veteran status as having served in either war, implying that estimates at the threshold understate the full effect of WWII service and GI Bill benefits. Appendix Figure A3 plots the share of veterans by birth year, separately for white and Black men, and shows a sharp drop at the cutoff for both races.

We estimate the first stage as a local linear regression on each side of the cutoff, separately by race:

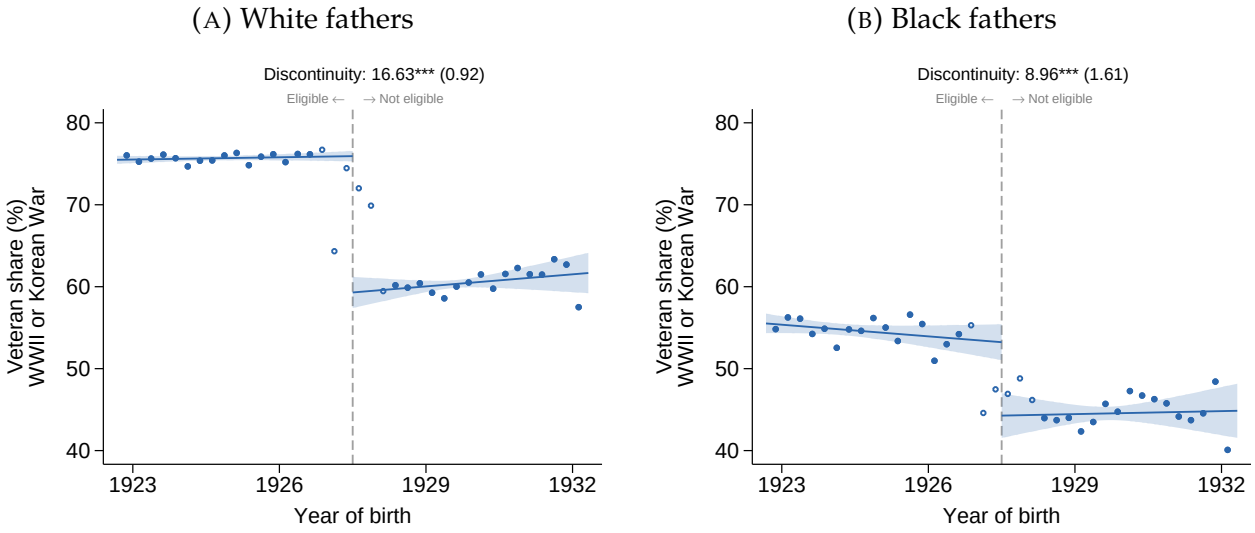
$$\text{Veteran}_i = \lambda\,\text{Eligible}_i + (\text{qob}_i - c)\Big(\rho_0 + \rho_1\,\text{Eligible}_i\Big) + \pi_{s(i)} + \eta_i, \tag{1}$$

where qob_i is man i's birth quarter on the calendar timeline (e.g., $\text{qob}_i = 1927.5$ corresponds to the third quarter of 1927); $\text{Eligible}_i = \mathbf{1}[\text{qob}_i \le c]$ with cutoff $c = 1927.5$; $\pi_{s(i)}$ is a within-year season-of-birth fixed effect ($s(i) \in \{1,2,3,4\}$) that absorbs seasonal patterns in births; and λ captures the discontinuous jump in veteran status. Our baseline specification uses a bandwidth of 4 years on each side of the cutoff and excludes cohorts within 3 quarters of the threshold ("donut" specification).[3]

[3] Appendix Figure A4 shows results are robust to alternative bandwidths and specifications.

Figure 1 illustrates the discontinuity separately by race. The first stage is 17 percentage points for white men and 9 percentage points for Black men, relative to right-of-cutoff (ineligible) veteran shares of 61 percent and 45 percent (Appendix Table A2). The magnitude is also similar across regions for either group.

FIGURE 1: First Stage: WWII Eligibility and Veteran Status, by Race



Notes: Panel (a) plots the regression discontinuity for white fathers; Panel (b) plots the regression discontinuity for Black fathers. Each point is the mean veteran share within a quarter-of-birth bin, using 1970 and 1980 IPUMS census samples (Ruggles et al., 2022). Solid lines are local linear fits on each side of the 1927.5 cutoff. Our baseline specification uses a 4-year bandwidth on each side of the cutoff with a 3-quarter donut around the cutoff. Omitted bins are shown as hollow circles. First stage estimates are reported above each graph, with standard errors clustered on quarter of birth in parentheses.

3.2. Direct Effects

We estimate reduced-form effects by regressing outcomes on the eligibility indicator, also separately by race:

$$Y_{it} = \alpha + \beta\,\text{Eligible}_i + (\text{qob}_i - c)\Big(\delta_0 + \delta_1\,\text{Eligible}_i\Big) + \pi_{s(i)} + \epsilon_{it}, \tag{2}$$

where Y_{it} is outcome Y for man i observed in census year t, and β is the reduced-form discontinuity at the cutoff. We pool the 1970 and 1980 cross sections, since the same eligibility cutoff identifies the discontinuity in both years, and estimates from each census year separately are similar. We report both reduced-form and instrumental variable estimates throughout. The reduced form gives the effect of being born on the eligible side of the cutoff for the average person in the cohort. The instrumental variable estimates scale

this by the race-specific first stage in veteran status to recover the per-induced-veteran (complier) effect.

The design requires that men born just above and just below the 1927.5 cutoff are otherwise comparable. Appendix Figures A5 and A6 present balance tests on predetermined 1940 covariates and the density of the running variable. Cohorts on either side of the cutoff look comparable on every observed 1940 characteristic—parental education, father's wage income, household homeownership and house value, Hispanic ethnicity, urban residence, foreign-born status, and number of siblings. Large sample sizes yield statistically significant differences on some margins, but all are economically negligible (below 0.05 standard deviations). The density of the running variable is also smooth through the cutoff (McCrary, 2008; Cattaneo et al., 2020).

The exclusion restriction requires that birth cohort affects outcomes only through WWII and GI Bill eligibility, not through other cohort-specific channels coinciding with the 1927.5 threshold. The prewar balance tests above rule out the possibility that cohorts on either side of the cutoff differ in predetermined family characteristics. A plausible alternative is wartime labor demand, since boys turning 16 or 17 between 1942 and 1945 faced unprecedented demand for factory labor. Section 4.2 shows that wartime labor demand explains the lower-tail dropout effects but not the postsecondary effects that drive our main results.

3.3. Intergenerational Effects

The birth cohort cutoff also identifies intergenerational effects: fathers' quasi-random eligibility allows us to estimate causal effects of WWII service and GI Bill benefits on their children. A standard intergenerational model links children's outcomes to their fathers' (Becker and Tomes, 1979; Solon, 1999):

$$Y_{child(i)} = \beta_0 + \beta^c Y_i + \varepsilon_{child(i)}, \tag{3}$$

where Y_i is the father's outcome, β^c governs intergenerational transmission, and $\varepsilon_{child(i)}$ reflects the child's endowment and other unobserved factors. Substituting fathers' veteran status as the source of variation in Y_i, this framework implies:

$$Y_{child(i)} = \alpha_c + \theta\,\text{Veteran}_i + u_{child(i)}, \tag{4}$$

where Veteran_i is instrumented by fathers' birth-cohort eligibility as in equation (1), and θ captures the total effect of fathers' veteran status—operating through parental education, household income, homeownership, and other channels—without requiring separate identification of each.

Because we do not observe sons' individual attainment, $Y_{child(i)}$ is a race-specific block-group tabulation (e.g., the male college share) matched to the son's adult address. The coefficient θ therefore captures the effect of fathers' veteran status on the socioeconomic status of sons' adult neighborhoods, not on their individual attainment.[4]

3.4. Data

Our estimates of the GI Bill's direct effects on veterans draw on 1960, 1970, and 1980 IPUMS census extracts (Ruggles et al., 2022), which provide 1–5 percent samples with harmonized measures of veteran status, education, income, housing, and migration. Most of this analysis pools 1970 and 1980, which include direct measures of veteran status and relevant outcomes for men who had completed their schooling by the census date. Vocational training is measured using the 1970 census, the only wave that records whether the respondent had ever completed a vocational training program outside of college, together with the main field of training.[5]

Our intergenerational analysis links fathers observed in the 1940 and 1950 full count censuses to adult sons observed in Infutor address histories (1990–2025). We restrict to sons because surname changes at marriage make it infeasible to link most women reliably by name between the 1950 census and later administrative records, a standard restriction in the literature. We follow standard record-linkage methods based on name, birth year, and sex (Abramitzky et al., 2014, 2021; Bailey et al., 2020). The 1940 census provides prewar father characteristics, and the 1950 census identifies household structure and children. Because we do not observe sons' individual education, income, or wealth, we merge each adult address to National Historical Geographic Information System (NHGIS) block-group tabulations of the race-specific distributions of education, income, and housing (Althoff and Reichardt, 2024; Schroeder et al., 2025). Throughout,

[4]Because the 1940 census records only year of birth, the intergenerational reduced form uses year (rather than quarter) of birth as the running variable, omits the donut, and includes father age and father age-squared controls to absorb paternal age effects.

[5]We use the variable `schlvoc`. The question instructs respondents to count completed programs such as high-school vocational tracks, apprenticeships, business, nursing, and trade schools, technical institutes, and Armed Forces schools, and to exclude on-the-job training, company schools, college courses beyond the second year, and correspondence courses. Appendix B reproduces the exact 1970 questionnaire wording.

intergenerational outcomes refer to these block-group tabulations for the son's race at his adult address—education shares, median income, median house value, and homeownership rate—rather than individual outcomes.

As is common with historical census linkage, match rates are imperfect and differ by race: 25 to 35 percent for white Americans and 11 to 17 percent for Black Americans. Crucially, match rates show no discontinuity at the 1927.5 threshold for either race, so differential selection around the cutoff does not drive the RD estimates (Appendix Figure B3). Appendix B provides details on the linkage procedure and representativeness checks.

Matched and unmatched Infutor records live in neighborhoods that look very similar on every observable we measure (Appendix Table B1). The matched sample is in neighborhoods that are only modestly better off: homeownership is 2 percentage points higher, median household income is $1,600 (3 percent) higher, and median home value is $3,100 (2 percent) higher. Education shares and demographic composition are essentially identical across the two samples. Our baseline intergenerational results are also robust to inverse-propensity reweighting of the linked sample to match either the underlying 1950 census population or the full 1940-to-1950 father-child universe.

4. Results on Direct Effects

Being born early enough to serve in WWII and qualify for the GI Bill generated sharply different outcomes by race. White veterans gained in four-year college completion, while Black veterans gained only in vocational training, with limited labor market returns. Effects on housing and migration were small or absent for both groups.

4.1. Divergent Educational Paths by Race

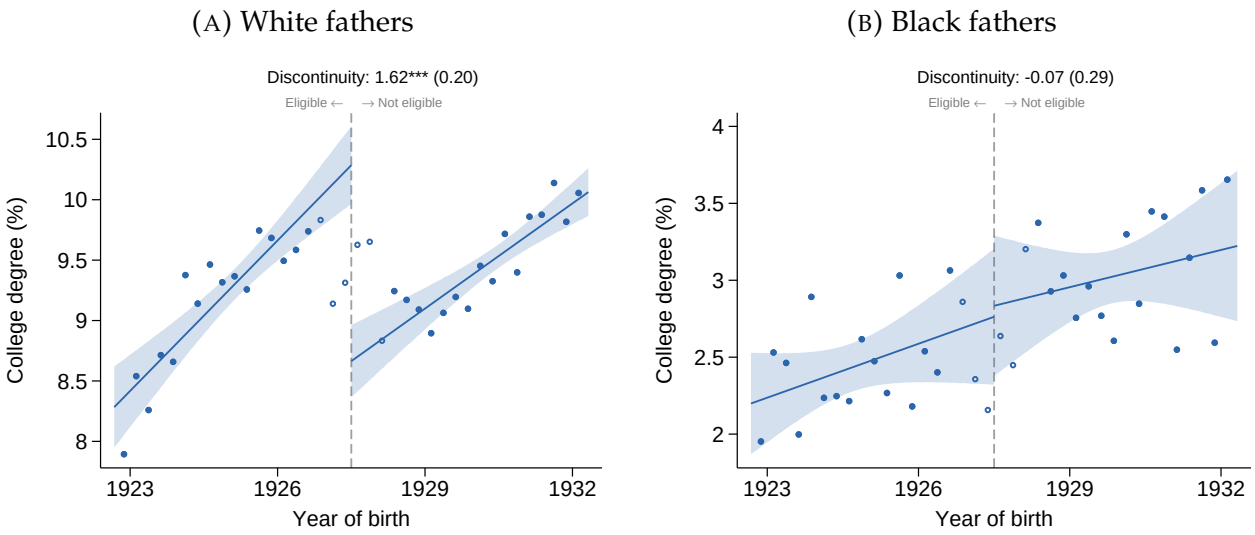
Our instrumental variable estimates show that military service and GI Bill eligibility shifted the educational distribution upward for white men and downward for Black men (Table 1, columns 3–4).

Eligibility raises white men's four-year college completion by 1.6 percentage points at the cutoff (Panel (a) of Figure 2 and Table 1, column 1). Scaling by the 16.7-percentage-point first stage in veteran status implies an effect of 9.5 percentage points for induced veteran compliers (column 3), more than doubling the 9 percent baseline among ineligible cohorts. This effect sits at the upper end of existing estimates: Bound and Turner

(2002) find 5 to 10 percentage points, Stanley (2003) reports 7 to 8 percentage points, and Collins and Zimran (2025) find smaller effects using 1950 census data, when many veterans had not yet completed their degrees. Our donut RD design reduces attenuation bias by excluding cohorts with weaker compliance near the cutoff.

Black veterans, by contrast, experienced no increase in college completion despite a strong first stage (Panel (b) of Figure 2 and Table 1, columns 2 and 4). Among several possible explanations—Black veterans' lack of interest, lower prewar educational attainment, limited HBCU capacity—the historical record points to active steering: VA counselors discouraged Black veterans from applying to college and directed them toward vocational programs (Onkst, 1998; Humes, 2006). As one 1947 report observed: "[Black] veterans want to develop skills, to get better jobs. The [Black] veteran has not only been unable to find better jobs, but he has had no place to go for advice on where to seek them" (Bolté and Harris, 1947, p. 9).

FIGURE 2: Reduced-Form Effects on College Degree by Race



Notes: Panel (a) plots the regression discontinuity for white men; Panel (b) plots the regression discontinuity for Black men. Each point is the mean share of men with a four-year college degree within a quarter-of-birth bin, using pooled 1970 and 1980 IPUMS census samples (Ruggles et al., 2022). Solid lines are local linear fits on each side of the 1927.5 WWII eligibility cutoff. Our baseline specification uses a 4-year bandwidth on each side of the cutoff with a 3-quarter donut around the cutoff. Omitted bins are shown as hollow circles. The sample includes men with nonmissing education data. Reduced-form estimates are reported above each graph, with standard errors clustered on quarter of birth in parentheses.

More broadly, we also document that white men's educational distribution became more unequal: less-than-high-school attainment rose by 15 percentage points, alongside the four-year college gains. We show in Section 4.2 that this lower-tail effect reflects young

men leaving school for war-production jobs, not the GI Bill itself. For Black veterans, the educational distribution shifted leftward: less-than-high-school attainment rose by 30 percentage points, some-college attainment fell by 12 percentage points, and total years of education declined, though imprecisely estimated. As we document below, the program's educational benefits did reach Black veterans, but almost entirely through vocational training.

TABLE 1: Direct Effects on Veterans' Educational Distribution

	Reduced form		IV	
	White	Black	White	Black
Less than HS	**2.57*****	**2.74*****	**15.41*****	**29.87*****
	(0.49)	(0.90)	(3.00)	(10.67)
HS degree	**-3.78*****	**-1.66****	**-22.69*****	**-18.16***
	(0.53)	(0.83)	(3.35)	(9.38)
Some college	**-0.54****	**-1.06*****	**-3.25****	**-11.56*****
	(0.23)	(0.34)	(1.36)	(4.08)
College degree	**1.58*****	-0.10	**9.45*****	-1.08
	(0.20)	(0.27)	(1.30)	(2.98)
Postgraduate	0.18	0.09	1.08	0.93
	(0.19)	(0.23)	(1.11)	(2.56)
Years of education	**0.06*****	**-0.11***	**0.37*****	-1.17
	(0.02)	(0.06)	(0.14)	(0.71)
Vocational training	-0.20	**3.26*****	-1.23	**35.59****
	(0.54)	(1.27)	(3.25)	(14.68)
N	1,197,947	121,253	1,197,947	121,253
N (vocational)	243,449	25,224	243,449	25,224

Notes: This table reports reduced-form and IV estimates for veterans' educational distribution at the 1927.5 WWII eligibility cutoff, by race, using the pooled 1970 and 1980 IPUMS census samples (Ruggles et al., 2022). IV estimates divide the reduced-form estimates by the race-specific first stage from Appendix Table A2. All outcomes are in percentage points except years of education. Outcome means are reported in Appendix Table A3. Vocational training is observed only in the 1970 census. The specification follows equation (2), with local linear fits on each side of the cutoff and within-year season-of-birth fixed effects. Our baseline specification uses a 4-year bandwidth on each side of the cutoff with a 3-quarter donut around the cutoff. Standard errors clustered on quarter of birth in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Vocational training, the largest single category of the GI Bill educational benefits, was the primary channel through which Black veterans used the program. Black men's vocational training completion rises discontinuously by 3.3 percentage points at the eligibility

cutoff, driven by trade and agricultural programs (Appendix Table A4). Scaled by the first stage, the IV estimate is 36 percentage points, more than doubling the control mean of 28 percent. The increase is concentrated in the South, where military service and GI Bill eligibility increased Southern Black vocational enrollment by 46 percentage points, roughly twice the effect estimated for other regions (Appendix A5). White veterans experienced no comparable shift into vocational programs.

Consistent with steering, Black veterans entered vocational training disproportionately in states and fields with the most documented fraud. Among the seven states a Congressional Committee identified as having at least three indicted or convicted vocational-school cases (Alabama, Arizona, Louisiana, New York, North Dakota, Pennsylvania, and Tennessee), being born early enough to serve in WWII raised Black men's vocational enrollment by 6.5 percentage points, whereas in other states the effect is essentially zero (Appendix Table A7). White men show no shift in either group of states, and the racial disparity is specific to vocational training. Other education margins differ little across high-fraud states and the rest of the country. Within vocational training, Black veterans were also concentrated in the fields contemporary investigators identified as the GI Bill's most problematic, agricultural and trade. These programs were often segregated, restricted to general or low-paying offerings, and frequently flagged for fraud and abuse (Onkst, 1998; Humes, 2006). The result was formal enrollment in training without access to marketable skills.

The steering of Black veterans toward vocational training was especially consequential because returns to credentials differed sharply by race (Table 2). Descriptive Mincer estimates using the 1970 Census indicate that vocational training is associated with 9 to 10 percent higher income for white men but no significant returns for Black men. Returns to year of education and to college are positive for both races (see Appendix Table A6).[6]

The zero return for Black men on vocational training reflects both the quality of the programs and the labor market they entered. A Congressional Committee concluded that "many of these schools are being operated by irresponsible persons who are not educators... [l]ittle or no instruction of value is being given to veterans enrolled" (Teague Committee, 1952, p. 199). Even where credentials carried value, "[W]hite veterans officials... directed [B]lack veterans away from highly paid, skilled work... toward menial, unskilled, low-wage positions" (Humes, 2006, p. 97). "Some of these schools, for instance. have been teaching veterans to be dancing instructors. This is no complaint against le-

[6]The small estimated white college return reflects controlling for linear years of schooling. The large Black college return reflect in part the screening role of credentials under labor market discrimination (Lang and Manove, 2011).

gitimate dancing instructors, but so many veterans have been induced, often by high-pressure methods, to squander their educational benefits on such instruction that if all of them were actually to become teachers there would be enough to make the entire population of the United States completely familiar with both the rumba and the samba within a year" (Ramey, 1948, p. 26).

TABLE 2: Returns to Vocational Training by Race and Region

	White		Black	
	South	Non-South	South	Non-South
	(1)	(2)	(3)	(4)
Vocational training	**0.10*****	**0.09*****	0.02	0.01
	(0.01)	(0.00)	(0.03)	(0.02)
Years of education	**0.12*****	**0.11*****	**0.09*****	**0.10*****
	(0.00)	(0.00)	(0.00)	(0.00)
Age, age^2 controls	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Observations	203,315	695,431	35,598	55,953
R-squared	0.139	0.118	0.057	0.057

Notes: Estimates from a Mincer-style log-income regression on the 1970 IPUMS census (Mincer, 1974; Ruggles et al., 2022), separately by race (white, Black) and region of residence (South, Non-South). The specification is $\log(\text{income}) = \beta_1 \text{ vocational} + \beta_2 \text{ years of education} + \gamma_1 \text{ age} + \gamma_2 \text{ age}^2 + \alpha_s + \varepsilon$, where *vocational* is an indicator for having completed a non-collegiate vocational program. Regressions are weighted by `perwt` and include state fixed effects; robust standard errors in parentheses. The sample is restricted to observations with non-missing `schlvoc`, recorded only on a sub-sample of the 1970 census long form. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

On-the-job training, another major GI Bill benefit, offered Black veterans no better path. Access required finding an employer willing to train them and securing approval for that employer as a certified training site. In the segregated South, few white employers accepted Black trainees, and those who did often exploited the arrangement. Field agents repeatedly reported that "white employers only hired and 'trained' [B]lack veterans so that they could use them as cheap labor" (Onkst, 1998, p. 524). Some even charged veterans for the privilege of working.

An additional possible mechanism for the GI Bill's unequal effect on education is that

HBCUs lacked the capacity to absorb Black veterans, since segregated Southern higher education barred Black students from white colleges. We test this by building a new geolocated database of HBCU openings and closings throughout US history. Within the South, we compare the GI Bill's effect on Black veterans across prewar locations with high versus low local HBCU access. The effect nearly identical across these locations (Appendix Table A7). HBCU capacity does not explain the racial gap in the GI Bill's college effect.

Other outcomes. Beyond education, we find divergent migration responses by race but muted effects on housing and labor market outcomes. White veterans were 7 percentage points less likely to move across state lines. Black veterans were 12 percentage points more likely to move to the South and 3 percentage points more likely to live on a farm (Appendix Table A10). Homeownership shows no discontinuity at the cutoff in 1970 or 1980 for either group (Appendix Table A9), consistent with Fetter (2013)'s finding that GI Bill loans accelerated rather than expanded ownership. Labor market effects are limited (Appendix Table A11): white veterans show slightly higher employment rates and occupational scores, while Black veterans do not benefit on either margin.

4.2. What Drives the Effects?

The effect of WWII and GI Bill eligibility could be driven by three key channels: the GI Bill, military service, and wartime labor demand for young men. Our evidence indicates that the GI Bill, not military service per se, drives the college effects, while wartime labor demand explains the simultaneous increase in less-than-high-school education.

To disentangle the effects of military service from the GI Bill itself, we apply the same RD design at the eligibility cutoffs for WWI (no GI Bill) and the Korean War (similar GI Bill). WWI eligibility produces IV estimates on four-year college completion that are statistically indistinguishable from zero,[7] while Korean War eligibility produces estimates of similar magnitude to WWII (Figure A7, Panel A). The pattern is robust to netting out cohort shocks common to both sexes through male-minus-female estimates (Panel B). Consistent with prior work finding small returns to wartime service alone (Angrist, 1998), the GI Bill, not service, drives the postsecondary effects.

Two further pieces of evidence point to the GI Bill. First, veterans' outcomes shift on the GI Bill's two largest spending categories—college and vocational training (Appendix Figure A1)—not on margins one would expect from generic service effects. Second, the

[7]There are also no effects on other educational margins; results available upon request.

racial gap is largest in the South, where institutional barriers to mainstream four-year colleges were most severe (Appendix Table A5). Service alone would produce uniform regional effects; the GI Bill's decentralized administration produces uneven ones.

Wartime labor demand drives the less-than-high-school increases. Young men near the cutoff faced elevated demand from war-production jobs, raising the opportunity cost of schooling. The rise in less-than-high-school attainment is concentrated among cohorts who turned 16-18 during WWII in counties in the top quartile of per-capita war-production contracts (Appendix Figure A8), and the pattern holds when we sort by state of birth: the white less-than-high-school effect is 2.9 percentage points in top-quartile war-production states versus 1.4 in bottom-quartile states (Appendix Table A7).[8] The effect on four-year college is essentially identical across both groups of states (1.7 versus 1.4 percentage points), confirming that wartime labor demand explains the less-than-high-school effects but not the college effects that drive our main analysis.

5. Results on Intergenerational Effects

The racial divergence in WWII service and GI Bill eligibility extended to the next generation. White sons of eligible fathers live in adult neighborhoods with higher college shares and incomes, while Black sons live neighborhoods with lower college shares. Because veteran fathers constituted a large share of families in the baby-boom cohort, these differences had first-order implications for postwar racial inequality.

5.1. Children's Education

White sons of eligible fathers live as adults in neighborhoods where the male white college share is 0.2 percentage points higher than for white sons of ineligible fathers. For Black sons, the corresponding gap is -1.0 percentage points (Table 3, columns 1–2, and Appendix Figure A9). Scaled by the race-specific first stages in fathers' veteran status, the per-induced-veteran (IV) effects are 1.5 percentage points for white sons (a 9 percent difference relative to the 16.3 percent baseline) and -10.4 percentage points for Black sons—a sharp asymmetry in intergenerational effects.[9]

[8] Women in high-intensity counties show no comparable break, consistent with labor demand drawing young men out of school rather than schooling disruption in these areas.

[9] For white sons, the 1.5-percentage-point IV estimate corresponds to 15 percent of the 9.5-percentage-point gain in fathers' own college completion. Because sons' outcomes reflect the neighborhoods they sort into as adults rather than their individual attainment, this ratio is not directly comparable to standard

The intergenerational effects extend across the full educational distribution and mirror the divergent educational trajectories of their fathers. For white sons, the IV effect on less-than-high-school attainment in adult neighborhoods is -2.5 percentage points and on postgraduate attainment is 1.6 percentage points. For Black sons, the pattern reverses. Less-than-high-school attainment in their adult neighborhoods rises by 8.8 percentage points and some-college attainment by 7.6 percentage points. White veterans' college gains likely translated into better school access, stronger networks, and other channels that sorted their sons into higher-socioeconomic status adult neighborhoods. Black veterans' concentration in low-return vocational training produced no comparable shift (Turner and Bound, 2003; Chetty et al., 2020).

TABLE 3: Intergenerational Effects on the Educational Distribution of Sons' Adult Neighborhoods

	Reduced form		IV	
	White	Black	White	Black
Less than HS	**-0.41*** **	**0.81***	**-2.48*** **	**8.80****
	(0.06)	(0.38)	(0.37)	(4.33)
HS degree	-0.06	-0.37	-0.37	-4.00
	(0.05)	(0.55)	(0.32)	(6.05)
Some college	-0.03	0.69	-0.20	**7.55***
	(0.05)	(0.41)	(0.32)	(4.55)
College degree	**0.24*** **	**-0.95*** **	**1.46*** **	**-10.36*** **
	(0.06)	(0.26)	(0.39)	(3.19)
Postgraduate	**0.27*** **	-0.18	**1.59*** **	-1.99
	(0.06)	(0.28)	(0.37)	(3.06)
N	417,527	23,317	417,527	23,317

Notes: This table reports reduced-form and IV estimates for children's adult neighborhood education outcomes, by race, using the intergenerational sample described in Section 3. Outcomes are race-specific male block-group education shares (in percentage points) from 2000-era Census tabulations, matched to children via Verisk/Infutor address histories. IV estimates divide the reduced-form estimates by the race-specific first stage on father's veteran status from Appendix Table A2. Outcome means are reported in Appendix Table A12. Standard errors clustered on father's year of birth in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

father-son individual intergenarational transmission estimates, though it lies in a similar range (Black and Devereux, 2011).

5.2. Children's Income and Other Outcomes

Intergenerational effects on adult-neighborhood income mirror the educational pattern but at smaller magnitudes. The eligibility effect on the median income of white sons' adult neighborhoods is $558, with an IV estimate of $3,347, or 7 percent of the $47,122 baseline (Appendix Figure A12 and Table A14). For Black sons, the effect is $55, with an imprecise IV estimate of $596.

Housing follows a similar pattern (Appendix Table A15). The IV effect on white sons' adult-neighborhood median house value is $12,610. For Black sons, the effect is essentially zero. Adult-neighborhood homeownership rates show little response for either group, consistent with Fetter (2013), who finds that the GI Bill accelerated home purchases without expanding lifetime ownership. Taken together, GI Bill shifted white sons into higher-SES adult neighborhoods across education, income, and housing, with no comparable change for Black sons.

5.3. Role of Local Spillovers

The GI Bill's scale—16 million veterans, $22.5 billion in spending—raises the possibility that its effects spilled beyond direct participants. Veterans returning to a community might have raised local schooling norms, expanded the stock of skilled neighbors, or shifted labor-market opportunities. If such spillovers reached Black families, they would have narrowed the racial gap independent of own-family benefits.

To capture community-level spillovers, we augment the child-outcome specification with a residualized WWII veteran share in father i's 1940 county of residence:

$$Y_{child(i)} = \alpha_c + \theta\,\text{Veteran}_i + \delta\,V^*_{\ell(i)} + u_{child(i)}, \tag{5}$$

where δ captures the community-level effect beyond the father's own veteran status. We construct $V^*_{\ell(i)}$ from the 1940-1950 linked full-count census by residualizing county-level WWII draft intensity against individual- and county-level characteristics associated with deferment ($R^2 = 0.82$). The residual isolates idiosyncratic variation driven by differences in how more than 6,000 local draft boards implemented national policy. Appendix A.5 provides full details on the construction, balance on prewar characteristics, and qualitative corroboration from the World War II Rumor Project Collection (Office of War Information, 1942–1943).

Spillovers reached only white sons (Appendix Figure A16). Moving from the 25th to the 75th percentile of local veteran exposure raises white sons' educational attainment modestly, concentrated at the upper end of the distribution. For Black sons, spillover estimates show no consistent positive pattern. Local spillovers thus did not narrow the racial gap. The intergenerational divergence reflects family-level effects rather than community-level spillovers.

5.4. Aggregate Implications for Racial Inequality

Our estimated family-level intergenerational effects have first-order implications for aggregate racial inequality. Veterans made up a large share of the baby-boom cohort's fathers (Appendix Figure A17). To quantify the aggregate implications, we scale the race-specific intergenerational IV estimates by the share of each cohort with a veteran father. This exercise does not attribute the racial college gap to the GI Bill, but instead measures how much the program widened it. The calculation also assumes that the effects we estimate for compliers apply to all veteran fathers in each cohort.

We find that cohort-level exposure to veteran fathers widened the Black-white college gap for sons by 47 percent (5 percentage points) among baby boomers (Appendix Figure A18). On the lower-tail margin, the less-than-high-school gap narrowed by 15 percent. The median income gap widened by 11 percent. These magnitudes are large, comparable to other major policy-driven changes in racial inequality. The exclusion of many Black workers from minimum wage coverage prior to 1966 increased the racial earnings gap by 20 percent (Derenoncourt and Montialoux, 2021). For comparison, school desegregation raised Black earnings by 30 percent (Johnson, 2011), and improvements in school quality explain 15-20 percent of the Black-white earnings gap between 1960 and 1980 (Card and Krueger, 1992).

6. Decomposition and Mechanisms

Why did the unequal effects of the GI Bill on veterans persist across generations? Three components can affect the racial gap in intergenerational outcomes: differences in the prewar endowments families brought into the program, differences in the returns each family realized from the same eligibility, or differences in how outcomes are transmitted from fathers to sons.

This distinction has direct policy relevance. If endowments dominate, universal subsi-

dies alone are unlikely to close racial gaps without complementary investments in early-life conditions. If returns dominate, differences in how institutions allocate and translate the subsidy—through counseling, colleges, and labor markets—become central, rather than differences in starting conditions. We decompose the gap in intergenerational college effects into these components using standard methods (Oaxaca, 1973; Blinder, 1973; DiNardo et al., 1996).

6.1. The Role of Prewar Endowments

Equalizing prewar characteristics between white and Black fathers barely narrows the racial gap in sons' adult-neighborhood college share. If prewar endowments drove the gap, reweighting white fathers to match the Black distribution of prewar characteristics would compress it, but it does not (Appendix Figure A19). We construct DiNardo et al. (1996)-style reweighting weights based on cells defined by the father's birth cohort, 1940 homeownership status, 1940 state of residence, and quartiles of 1940 household-average income and education.[10] Reweighting leaves the gap close to its baseline magnitude. Observed prewar differences are important descriptively but account for only a small share of the racial gap in intergenerational effects.

6.2. Decomposing the Intergenerational Racial Gap

To formalize the reweighting result, we decompose the racial gap into endowment, return, and persistence components. Let θ^r denote the race-specific reduced-form intergenerational effect of fathers' eligibility:

$$\theta^r = \beta^r \int \tau^r(k, K)\, dF_r(k, K), \qquad r \in \{W, B\}, \tag{6}$$

where β^r is the intergenerational transmission coefficient, $\tau^r(k, K)$ is the race-specific mapping from fathers' veteran status into outcomes conditional on family capital k and community capital K, and $F_r(k, K)$ is the race-specific distribution of prewar capital. Suppressing (k, K) for notational clarity, the white–Black gap $\theta^W - \theta^B$ can be rewritten as:

[10] Household-average income and education are averaged over prime-age adults aged 18–54 in the father's 1940 household, since the fathers themselves were typically still children in 1940. We use coarse bins to preserve common support across cells. Weights are further multiplied by inverse-propensity weights correcting for differential linking rates between Black and white samples (Appendix B). A variant excluding state of residence yields nearly identical results (*Economic Reweighting* in Figure A19).

$$\underbrace{\beta^W\left(\int \tau^W\, dF_B - \int \tau^B\, dF_B\right)}_{\text{return}} + \underbrace{\beta^W\left(\int \tau^W\, dF_W - \int \tau^W\, dF_B\right)}_{\text{endowment}} + \underbrace{(\beta^W - \beta^B)\int \tau^B\, dF_B}_{\text{persistence}}. \quad (7)$$

The return component captures the importance of the same prewar characteristics generating different returns across race. The endowment component captures how much of the gap reflects differences in the distribution of prewar characteristics, evaluated at white returns. The persistence component captures differences in intergenerational transmission rates, evaluated at Black returns and endowments.

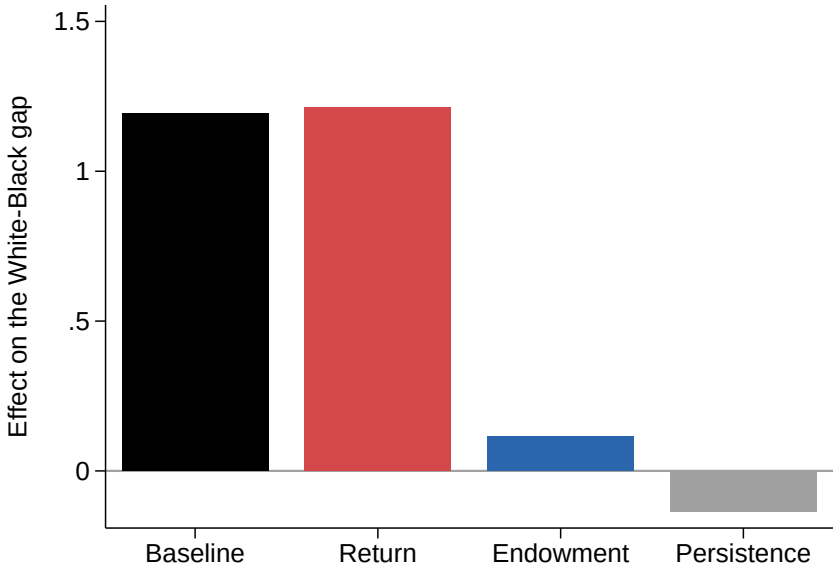
To operationalize the decomposition, we estimate the integrals from data and calibrate the transmission coefficients. The race-specific RD on sons' adult-neighborhood outcomes (Table 3) directly identifies θ^W and θ^B. The counterfactual part, $\beta^W \int \tau^W\, dF_B$, is estimated by re-running the RD for white sons with DiNardo et al. (1996) weights that match white fathers to the Black fathers distribution, as described in Section 6.1. We calibrate $\beta^W = 0.32$ and $\beta^B = 0.28$ from race-specific father-son income-rank elasticities in Chetty et al. (2020).[11] The decomposition is robust to the calibration choice: substituting alternative race-specific transmission estimates from the literature would scale the persistence component modestly but leaves the return component as the dominant term. Most estimates also suggest that that Black-white relative mobility is similar (Althoff et al., 2025), so the calibration is not central to our conclusions.

6.3. Returns Drive the Racial Gap

Unequal returns account for close to all of the racial gap in intergenerational effects: the return component is 1.21 percentage points, slightly larger than the total gap of 1.19 percentage points (Figure 3). Even if Black fathers had held the white prewar distribution of education, income, homeownership, and geography, the intergenerational effect would be essentially unchanged. By contrast, the endowment component explains only 10 percent of the gap (0.12 percentage points). The calibrated persistence component is small and slightly negative (-0.14 percentage points): if anything, Black-white differences in intergenerational transmission worked to narrow the gap rather than widen it.

[11] Race-specific father-son transmission rates are not directly identified in our neighborhood-based sample.

FIGURE 3: Decomposition of the Racial Gap in Intergenerational GI Bill Effects



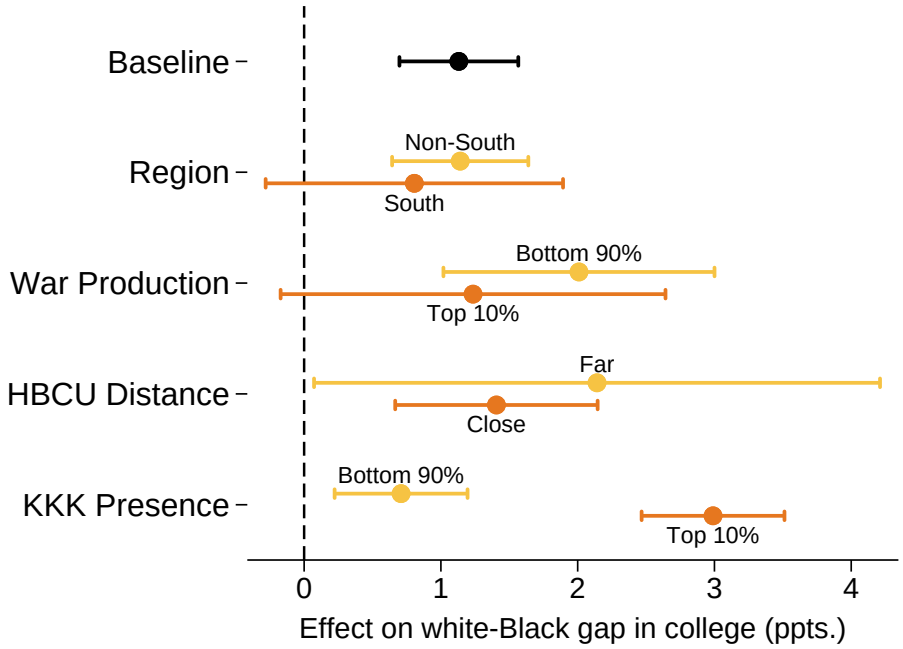
Notes: This figure decomposes the racial gap in the intergenerational effect of fathers' WWII service and GI Bill eligibility on the race-specific male four-year college share in sons' adult neighborhoods into return, endowment, and persistence components following equation (7). "Baseline" is the estimated white–Black gap in the race-specific reduced-form RD coefficients on sons' adult-neighborhood college share (from Table 3; 1.19 pp). "Endowment" is the change in the white RD coefficient when white fathers are reweighted to the Black distribution of birth cohort, 1940 homeownership, 1940 state of residence, and quartiles of 1940 household-average income and education (averaged over prime-age adults in the father's 1940 household), following DiNardo et al. (1996). "Persistence" is calibrated from race-specific father–son income-rank transmission rates reported in Chetty et al. (2020) ($\beta^W = 0.32$, $\beta^B = 0.28$), scaled by the Black father-level implied effect $\int \tau_B \, dF_B = \theta^B / \beta^B$ following equation (6). "Return" is the residual component, equal to the total gap minus the endowment and persistence components. The return and endowment bars are identified from our data; the persistence bar is a calibrated magnitude.

The pattern is consistent with the empirical evidence from Section 4: Black veterans sought college access but were steered into lower-return vocational programs, and the labor markets they entered offered limited returns to those credentials. Equal eligibility within discriminatory institutions produced unequal economic outcomes. The GI Bill is a leading example of a large-scale race-blind subsidy that, when implemented through segregated institutions and labor markets, generated unequal returns and persistent inequality across generations (Katznelson, 2005).

We also find that the intergenerational effects on the white-Black gap in sons' college completion are largest in areas with high historical levels of anti-Black violence, as measured via historical KKK presence in fathers' prewar counties of residence (Figure 4). The effects are also slightly larger in areas with less war production—i.e., those in which men

were more likely to serve in the military and the GI Bill was more likely to be taken up subsequently—and in places far from HBCUs—i.e., those in which college education was harder to access for both fathers and sons. Generally, the effects are similar across regions, comparing sons whose fathers grew up in the South versus non-South.

FIGURE 4: Heterogeneity of the Intergenerational White-Black Gap in the College Effect by Father's 1940 County Characteristics



Notes: Reduced-form estimates of the white-Black gap in the eligibility-cutoff effect on the race-specific male four-year-college share in sons' adult neighborhood, by father's 1940 county of residence. The gap and its 95% confidence interval come from a single stacked race-interacted regression, $y = \alpha + \gamma\,\mathbf{1}[\text{Black}] + (\text{running}_i \times \text{Eligible}_i) \times \mathbf{1}[\text{Black}] + \textit{age controls} + \varepsilon$, weighted by the IPW and clustered on father's year of birth, so the standard errors are correct for the difference. The plotted statistic is the white-minus-Black discontinuity. *Baseline* matches the headline RF white-Black gap implied by Table 3; the remaining specifications restrict the sample to the county subgroups defined in Table A13 (cuts: per-capita WWII production-contract spending, urban share, distance to nearest HBCU operational in 1944, klavern count). Horizontal bars show 95% confidence intervals.

7. CONCLUSION

In February 2025, Congress reintroduced the GI Bill Restoration Act, proposing to extend education and housing benefits to the descendants of Black WWII veterans denied effective access to the original program (Poulson, 2023). The bill is named after Sgt. Joseph Maddox, who served in WWII and was accepted to Harvard University but was denied

GI Bill tuition by his local VA office "to avoid setting a precedent" (Humes, 2006). Maddox eventually secured his benefits with the support of the NAACP, but the barriers he initially experienced were not exceptional: Black veterans faced discrimination at each stage of the GI Bill's administration, producing two fundamentally different experiences of the same statutory entitlement.

This paper quantifies the intergenerational consequences of that disparity. Among white men induced into WWII service and GI Bill eligibility by the age cutoff, four-year college completion roughly doubled, and these gains were transmitted across generations: their sons live in adult neighborhoods with higher college shares and higher median incomes. Black veterans, by contrast, were disproportionately steered toward low-return vocational programs, and their sons show no comparable shift in adult-neighborhood economic status. The Black–white gap in intergenerational effects reflects differences in returns, not pre-existing endowments. Decentralized administration, segregated institutions and discriminatory labor markets converted the same statutory eligibility into unequal outcomes across two generations.

Beyond its own historical episode, the GI Bill was central to the founding of the American knowledge economy. It dramatically expanded the supply of college-educated workers in the postwar period, before the surge in the college wage premium in the 1980s magnified the value of those credentials (Goldin and Katz, 2008; Katz and Murphy, 1992). Because Black families were largely excluded from this expansion, the resulting gaps in college attainment grew more costly as the economy restructured around college credentials. This contributed to the post-1970s stagnation in Black-white economic convergence (Bayer and Charles, 2018; Derenoncourt et al., 2024).

Several questions remain open. The Korean War GI Bill operated under desegregated military service and tighter oversight of vocational training. Comparing its intergenerational effects to those of the WWII GI Bill would help understand whether the racial disparities we document persisted or attenuated as institutions changed. While we find limited effects on homeownership, the GI Bill's role in shaping racial wealth gaps also deserves further study. More broadly, our results suggest that quantifying the intergenerational consequences of race-blind policies implemented through unequal institutions is an important agenda for understanding the persistence of racial inequality in the United States.

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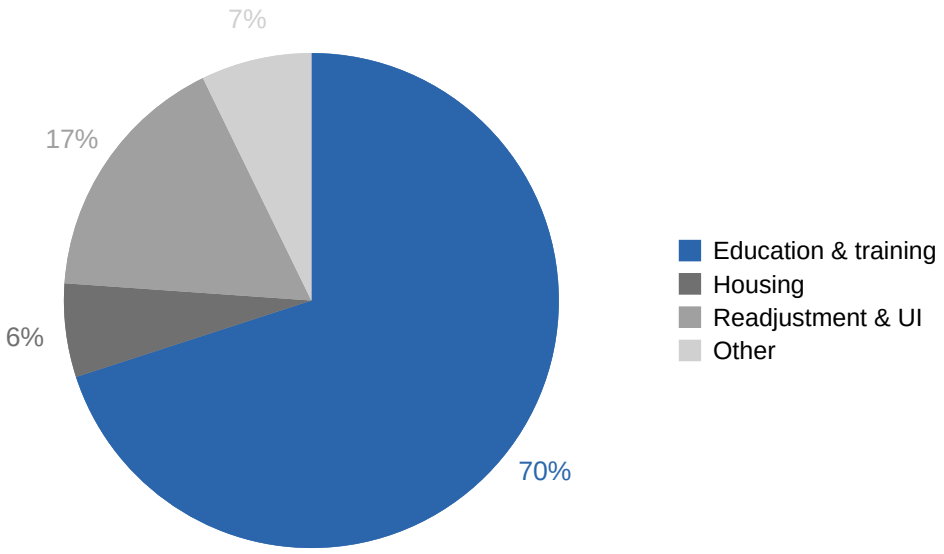
A. Appendix Figures and Tables

A.1. Historical Background

FIGURE A1: GI Bill Spending by Component, 1945–1955



Notes: This figure reports GI Bill spending by component (education and training, housing, readjustment and unemployment insurance, and other components) from 1945 to 1955, documenting the scale and composition of the federal investment amounting to a total of $22.5 billion (President's Commission on Veterans' Pensions, 1956; Collins and Zimran, 2025).

TABLE A1: Timeline of WWII Military Service, the GI Bill, and the Korean War

World War II

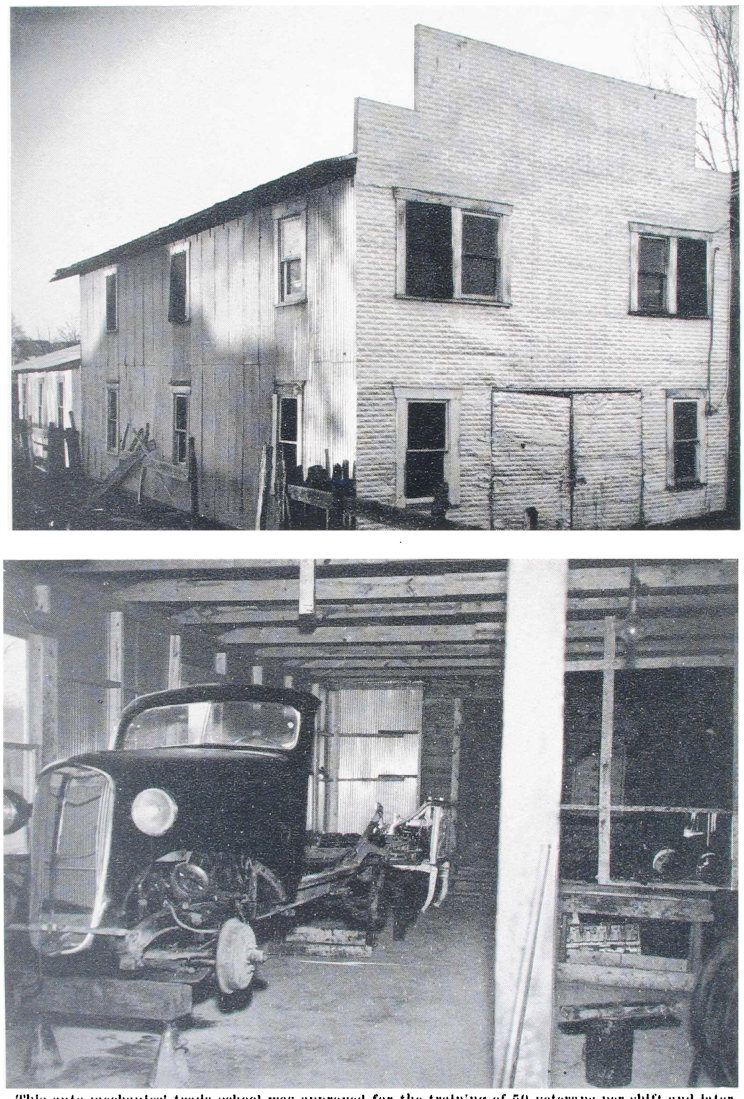
Sep. 1940	Selective Training and Service Act of 1940 signed into law
Oct. 1940	1st registration: all men aged 21–35 (Oct. **lottery** decided draft order)
Jul. 1941	2nd registration: men who turned 21 since the last registration (Jul. **lottery**)
Dec. 1941	**U.S. enters WWII after Pearl Harbor was bombed**
Feb. 1942	3rd registration: men aged 20–45 not previously registered (Mar. **lottery**)
Apr. 1942	4th registration: men aged 45–64 (never drafted into military service)
Jun. 1942	5th registration: men aged 18–20 (Nov. draft **young to old**, no lottery)
Dec. 1942	Ongoing registration: men registered on 18th birthday (draft **young to old**)
Jan. 1943	Men aged 38–45 no longer drafted; focus on younger men
Jun. 1943	Army General Classification Test introduced
Jun. 1944	**WWII GI Bill signed into law by FDR**
Sep. 1945	**End of WWII**
Jul. 1956	**WWII GI Bill education benefits expire (housing benefits expire Jul. 1957)**

Korean War

Jun. 1948	Registration of all men aged 18–26 for first U.S. peacetime draft
Jun. 1948	20,000 men aged 19–26 drafted between 1948 and Korean War (young to old)
Jun. 1950	Beginning of Korean War; sharp increase in number of men drafted
Jun. 1950	During the war 1.5 million men drafted, generally youngest to oldest
Jun. 1951	Age for military service lowered from 19 to $18\frac{1}{2}$
Jul. 1952	**Korean War GI Bill signed into law by Harry Truman**
Jul. 1953	End of Korean War
Jan. 1965	**GI Bill education benefits expire (no expiration date for housing benefits)**

Notes: This table summarizes the key dates in WWII and Korean War military service and GI Bill legislation. More details can be found in Section 2. Sources: Murray (1971); Olson (1974); Mettler (2005).

FIGURE A2: Photograph of an Approved and Later Disapproved Vocational School

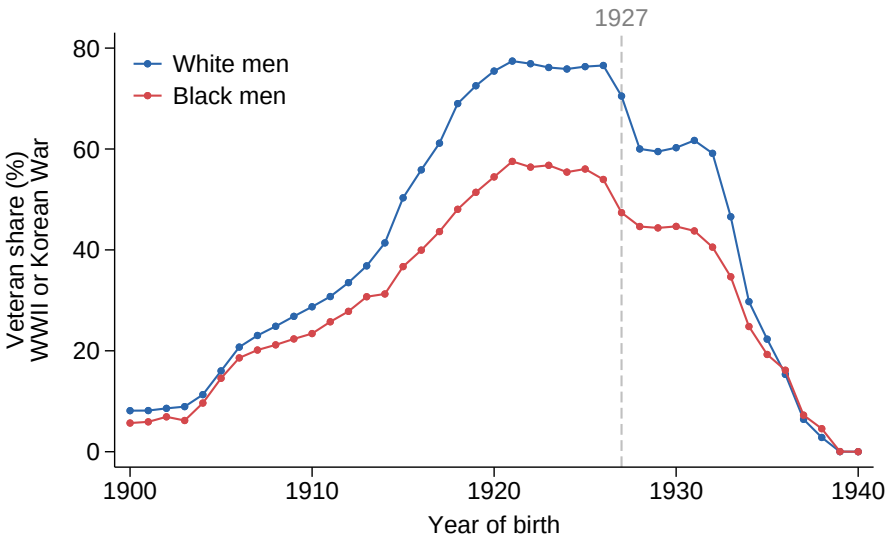


This auto mechanics' trade school was approved for the training of 50 veterans per shift and later disapproved. (Outside and inside views.)

Notes: Photograph reproduced from the 1952 Teague Committee Report (Teague Committee, 1952). The auto-mechanics' trade school shown was approved by the Veterans Administration for the training of 50 veterans per shift and later disapproved.

A.2. Empirical Strategy

FIGURE A3: Veteran Share by Year of Birth and Race



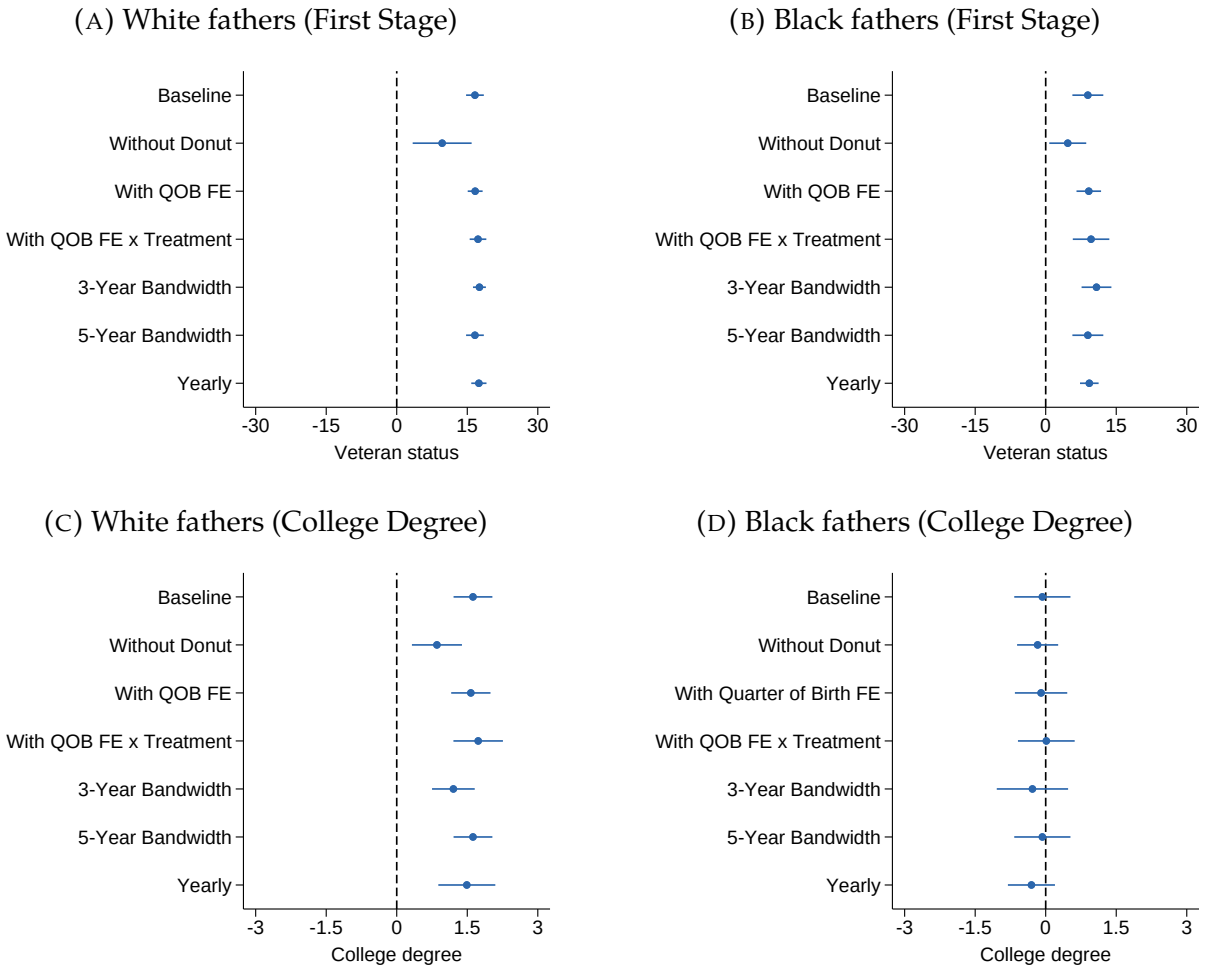
Notes: This figure plots the share of men reporting WWII- or Korean War-era veteran status by year of birth, separately for white and Black men, using pooled 1960, 1970, and 1980 IPUMS census samples (Ruggles et al., 2022). The dashed vertical line at 1927 marks the WWII service cutoff: men born before the third quarter of 1927 turned 18 while the war was ongoing and were far more likely to serve than those born just after, motivating our RD design.

TABLE A2: First Stage Estimates at the WWII Service Cutoff

	White			Black		
	All Regions	South	Non-South	All Regions	South	Non-South
Veteran Status	16.68***	16.74***	16.64***	9.16***	10.47***	8.16***
	(0.78)	(0.97)	(0.79)	(1.28)	(1.51)	(1.44)
Mean	60.64	59.18	61.27	44.60	38.84	50.15
N	950,466	281,034	661,169	95,624	46,859	48,463

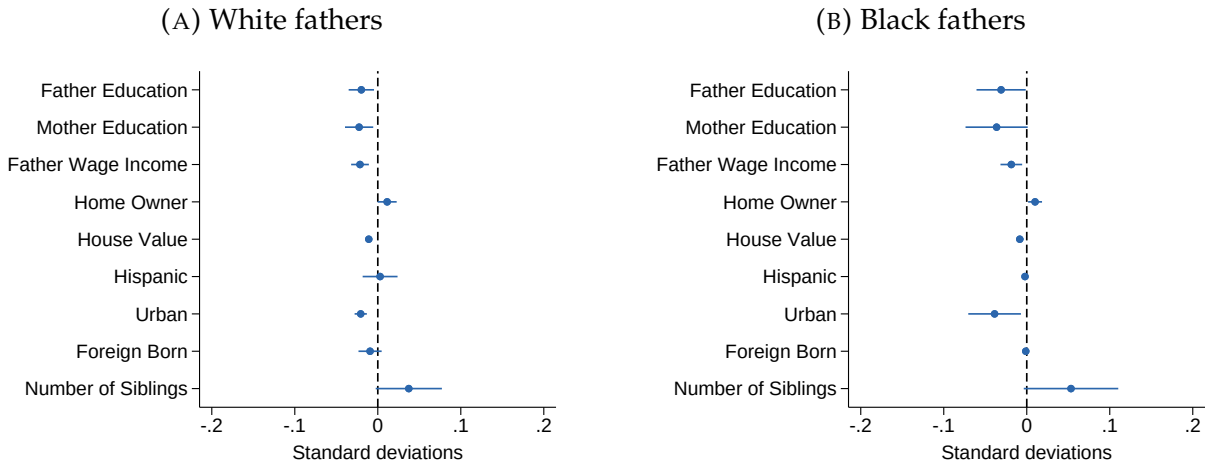
Notes: This table reports first-stage discontinuities in veteran status at the 1927.5 WWII eligibility cutoff, separately for pooled All Regions, the South, and the pooled Non-South. The sample includes men from pooled 1970 and 1980 censuses (Ruggles et al., 2022). "Mean" reports the right-of-cutoff (ineligible) mean veteran share within the estimation bandwidth, in percentage points. The specification follows equation (1), with local linear fits on each side of the cutoff and within-year season-of-birth fixed effects. Our baseline specification uses a 4-year bandwidth on each side with a 3-quarter donut around the threshold. Standard errors clustered on quarter of birth in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

FIGURE A4: Robustness Across Alternative Specifications



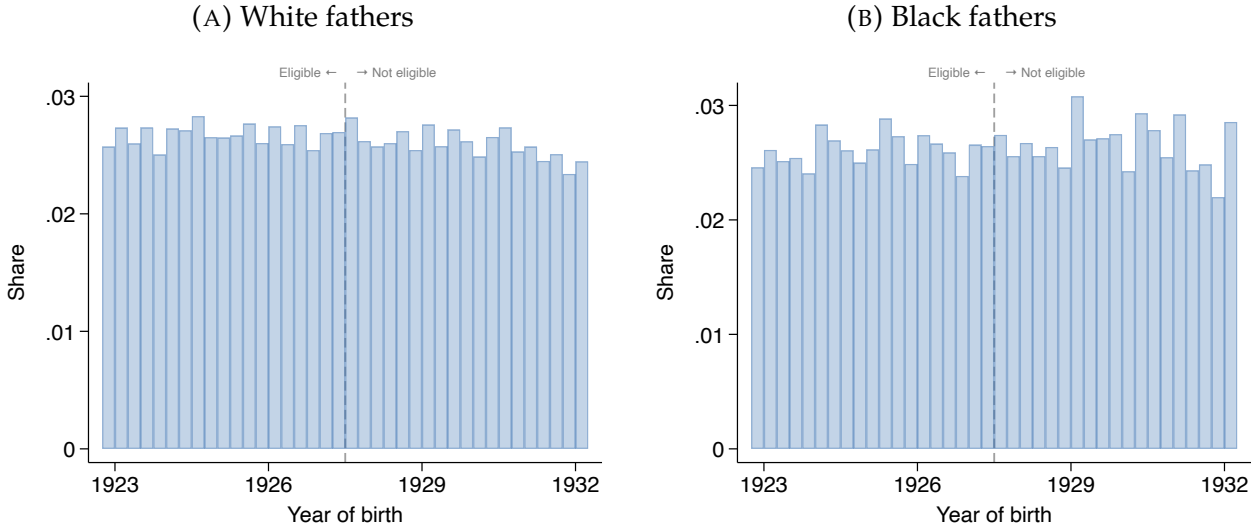
Notes: Panel (a) reports first-stage estimates for white fathers; Panel (b) reports first-stage estimates for Black fathers; Panel (c) reports reduced-form estimates for white fathers; Panel (d) reports reduced-form estimates for Black fathers. The outcome in Panels (a)–(b) is veteran status, while the outcome in Panels (c)–(d) is four-year college completion. Each panel reports estimates across seven specifications: (1) *Baseline* (4-year bandwidth, 3-quarter donut, local linear fits on each side of the cutoff, within-year season-of-birth fixed effects); (2) *Baseline without donut*; (3) *Baseline without quarter-of-birth fixed effects*; (4) *Baseline with quarter-of-birth fixed effects interacted with eligibility*; (5) *Baseline with 3-year bandwidth*; (6) *Baseline with 5-year bandwidth*; (7) *Baseline with yearly running variable*. The Baseline specification follows equation (1) (first stage) or equation (2) (reduced form). Horizontal bars show 95 percent confidence intervals. Standard errors are clustered on quarter of birth, except in the yearly specification, in which they are clustered on year of birth.

FIGURE A5: Balance Test on Prewar Covariates



Notes: This figure reports reduced form estimates of the effect of WWII eligibility on standardized prewar covariates for veterans by race. Each covariate is standardized to have mean zero and standard deviation one before estimation. Outcomes are all measured in the 1940 full count census. The specification follows equation (2), with year of birth yob_i replacing quarter of birth, as the 1940 census does not record quarter of birth. We use local linear fits on each side of the cutoff, a 4-year bandwidth, and drop the 1927 and 1928 birth-year cohorts adjacent to the 1927.5 cutoff (the annual analogue of the main-text 3-quarter donut). Horizontal bars show 95 percent confidence intervals. Standard errors are clustered on year of birth.

FIGURE A6: Density of Running Variable by Race



Notes: Panel (a) plots the density of the running variable for white fathers; Panel (b) plots the density for Black fathers. We use pooled 1970 and 1980 IPUMS census samples (Ruggles et al., 2022). The vertical dashed line marks the 1927.5 WWII eligibility cutoff. We find no evidence of manipulation at the cutoff.

A.3. Additional Results on Direct Effects

TABLE A3: Means for Veterans' Educational Outcomes

	White	Black
Educational Attainment (pooled 1970–1980, %)		
Less than High School	32.62	59.12
High School Graduate	32.46	23.42
Some College	14.85	10.56
4-Year College	9.45	3.05
Post-graduate	10.62	3.85
Years of Education	12.03	9.94
Vocational School	34.22	27.24
Vocational Training by Field (1970, %)		
Any Vocational Training	34.22	27.24
Business	4.43	2.62
Health	0.80	0.90
Trade	17.07	10.90
Science	4.33	1.39
Agriculture	0.90	0.56
Other	6.69	10.87

Notes: This table reports the untreated (ineligible) mean for veterans' educational outcomes, by race. Means are computed just to the right of the 1927.5 WWII eligibility cutoff on the corresponding estimation sample (4-year bandwidth, excluding the 3-quarter donut), using IPUMS census samples (Ruggles et al., 2022). Educational-attainment means use the pooled 1970-1980 sample; vocational-training-by-field means use the 1970 census, the only year in which vocational training information is recorded.

TABLE A4: Direct Effects on Veterans' Vocational Training: IV by Field

		White			Black		
		All Regions	South	Non-South	All Regions	South	Non-South
Any Vocational Training		-1.23	-5.38	-0.61	**35.59****	**46.03*****	21.81
		(3.25)	(3.95)	(3.97)	(14.68)	(13.55)	(25.09)
	Mean	34.22	31.57	35.43	27.24	22.30	31.69
Business		**-3.46*****	-1.82	**-4.31*****	-2.47	4.95	-10.73
		(0.82)	(2.01)	(0.96)	(4.15)	(4.54)	(9.23)
	Mean	4.43	4.66	4.37	2.62	1.92	3.27
Health		-0.28	-0.96	-0.21	0.86	-0.77	1.97
		(0.61)	(1.02)	(0.64)	(2.20)	(2.21)	(3.51)
	Mean	0.80	0.76	0.82	0.90	0.55	1.21
Trade		1.47	-1.86	2.29	**23.67****	**23.85****	24.38
		(2.22)	(3.44)	(2.47)	(10.90)	(11.97)	(20.54)
	Mean	17.07	14.96	17.98	10.90	9.39	12.21
Science		0.33	-2.35	1.35	1.84	0.64	3.89
		(1.20)	(2.02)	(1.52)	(3.39)	(3.99)	(6.18)
	Mean	4.33	3.62	4.65	1.39	0.83	1.89
Agriculture		**2.32*****	**2.97*****	**1.97*****	**5.23***	**7.92***	2.06
		(0.55)	(0.85)	(0.65)	(2.72)	(4.69)	(3.08)
	Mean	0.90	0.84	0.90	0.56	0.65	0.48
Other		-1.61	-1.34	-1.70	6.45	9.44	0.25
		(1.58)	(2.61)	(1.87)	(11.41)	(12.13)	(17.52)
	Mean	6.69	6.73	6.71	10.87	8.96	12.62
N		1,197,947	349,722	836,332	121,253	59,125	61,725

Notes: This table reports IV estimates for veterans' vocational training by field at the 1927.5 WWII eligibility cutoff, separately for pooled All Regions, the South, and the pooled Non-South (Northeast, Midwest, and West), using the 1970 IPUMS census (Ruggles et al., 2022), which is the only census that records vocational training. The IPUMS variable `schlvoc` records whether the respondent ever completed a vocational training program outside of college and, if so, the main field. Appendix B reproduces the full question wording. IV estimates divide the reduced-form estimates by the race-specific first stage from Table A2. "Mean" reports the untreated (ineligible) mean, computed just to the right of the cutoff within the estimation sample (i.e., within the bandwidth and excluding the donut hole), so IV estimates can be interpreted as effects above and beyond the untreated mean. Outcome means are reported in Appendix Table A3. The South and Non-South samples do not add up to All Regions because state of residence is missing for a small share of observations in the census, so those observations are included in All Regions but cannot be assigned to a region. The specification follows equation (2), with local linear fits on each side of the cutoff and within-year season-of-birth fixed effects. Our baseline specification uses a 4-year bandwidth on each side of the cutoff with a 3-quarter donut around the cutoff. Standard errors clustered on quarter of birth in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A5: Direct Effects on Veterans' Educational Distribution: IV by Region

	White			Black		
	All Regions	South	Non-South	All Regions	South	Non-South
Less than High School	**15.41*****	**7.57****	**19.09*****	**29.87*****	7.85	**51.71*****
	(3.00)	(3.06)	(3.44)	(10.67)	(11.19)	(17.02)
Mean	32.62	38.01	30.21	59.12	67.44	51.15
High School Graduate	**-22.69*****	**-13.17*****	**-27.13*****	**-18.16***	-3.36	**-34.40****
	(3.35)	(2.94)	(3.90)	(9.38)	(6.32)	(16.18)
Mean	32.46	28.19	34.25	23.42	18.24	28.36
Some College	**-3.25****	-3.14	**-3.20****	**-11.56*****	0.06	**-23.53*****
	(1.36)	(2.14)	(1.32)	(4.08)	(4.80)	(8.99)
Mean	14.85	14.61	15.00	10.56	7.97	13.03
4-Year College	**9.45*****	**8.95*****	**9.49*****	-1.08	-3.39	2.25
	(1.30)	(1.83)	(1.25)	(2.98)	(3.43)	(3.37)
Mean	9.45	9.67	9.40	3.05	2.82	3.28
Post-graduate	1.08	-0.20	1.74	0.93	-1.16	3.98
	(1.11)	(1.51)	(1.34)	(2.56)	(2.66)	(3.59)
Mean	10.62	9.52	11.14	3.85	3.54	4.17
Years of Education	**0.37*****	0.39	**0.34****	-1.17	0.13	**-2.28*****
	(0.14)	(0.25)	(0.16)	(0.71)	(0.90)	(0.82)
Mean	12.03	11.61	12.22	9.94	9.14	10.70
Vocational School	-1.23	-5.38	-0.61	**35.59****	**46.03*****	21.81
	(3.25)	(3.95)	(3.97)	(14.68)	(13.55)	(25.09)
Mean	34.22	31.57	35.43	27.24	22.30	31.69
N	1,197,947	349,722	836,332	121,253	59,125	61,725

Notes: This table reports IV estimates for veterans' educational outcomes at the 1927.5 WWII eligibility cutoff, separately for pooled All Regions, the South, and the pooled Non-South (Northeast, Midwest, and West), using the pooled 1970 and 1980 IPUMS census samples (Ruggles et al., 2022). IV estimates divide the reduced-form estimates by the race-specific first stage from Table A2. All outcomes are in percentage points except Years of Education. "Mean" reports the untreated (ineligible) mean, computed just to the right of the cutoff within the estimation sample (i.e., within the bandwidth and excluding the donut hole), so IV estimates can be interpreted as effects above and beyond the untreated mean. Outcome means are reported in Appendix Table A3. The South and Non-South samples do not add up to All Regions because state of residence is missing for a small share of observations in the census, so those observations are included in All Regions but cannot be assigned to a region. The specification follows equation (2), with local linear fits on each side of the cutoff and within-year season-of-birth fixed effects. Our baseline specification uses a 4-year bandwidth on each side of the cutoff with a 3-quarter donut around the cutoff. Standard errors clustered on quarter of birth in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A6: Returns to a Four-Year College Degree by Race and Region

	White		Black	
	South	Non-South	South	Non-South
	(1)	(2)	(3)	(4)
College degree	**-0.01***	**0.01*****	**0.12*****	**0.07****
	(0.01)	(0.00)	(0.04)	(0.03)
Years of education	**0.12*****	**0.11*****	**0.09*****	**0.10*****
	(0.00)	(0.00)	(0.00)	(0.00)
Age, age^2 controls	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Observations	405,677	1,389,850	70,801	111,853
R-squared	0.137	0.116	0.058	0.058

Notes: Companion to Table 2 in the main text. Estimates from a Mincer-style log-income regression on the 1970 IPUMS census (Mincer, 1974; Ruggles et al., 2022), separately by race (white, Black) and region of residence (South, Non-South). The specification is $\log(\text{income}) = \beta_1 \text{ college degree} + \beta_2 \text{ years of education} + \gamma_1 \text{ age} + \gamma_2 \text{ age}^2 + \alpha_s + \varepsilon$, where *college degree* is an indicator for four or more years of college and *years of education* is continuous. Regressions are weighted by `perwt` and include state fixed effects; standard errors in parentheses are clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A7: Heterogeneity of Direct Reduced-Form Effects on Veterans by State-of-Birth Characteristics

	Baseline		Low War Production		High War Production		Low-Urban		High-Urban		Low-HBCU South		High-HBCU South		Low-Jim Crow South		High-Jim Crow South		High-Fraud Voc. States	
	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black
Less than HS	2.45***	2.56***	1.39	-0.53	2.86***	5.74**	0.71	-0.71	3.36***	4.25***	0.93	2.58	1.55*	0.82	1.77**	3.52*	0.55	2.18	2.64***	3.03
	(0.55)	(0.92)	(0.91)	(1.37)	(0.57)	(2.52)	(0.84)	(1.09)	(0.58)	(1.59)	(0.87)	(1.78)	(0.82)	(1.47)	(0.82)	(1.93)	(0.84)	(1.75)	(0.68)	(2.18)
HS degree	-3.59***	-1.41	-2.92***	0.04	-4.45***	-3.42	-2.27***	0.12	-4.28***	-2.30	-1.72**	-1.33	-2.82***	-0.70	-2.57***	-2.38	-0.56	-1.38	-3.72***	-1.82
	(0.62)	(0.96)	(0.69)	(1.08)	(0.75)	(2.49)	(0.59)	(0.91)	(0.79)	(1.63)	(0.68)	(1.39)	(0.65)	(1.44)	(0.67)	(1.91)	(0.76)	(1.27)	(0.69)	(1.77)
Some college	-0.56**	-1.26***	-0.27	0.08	-0.34	-4.94***	-0.47	-0.32	-0.45*	-3.48***	-1.50***	-0.98**	-0.54	-0.40	-0.38	-1.81	-1.59***	-1.11***	-0.27	-1.43***
	(0.28)	(0.37)	(0.45)	(0.49)	(0.38)	(1.79)	(0.39)	(0.55)	(0.27)	(1.17)	(0.55)	(0.45)	(0.47)	(0.85)	(0.42)	(1.36)	(0.58)	(0.41)	(0.36)	(0.49)
College degree	1.37***	-0.07	1.43***	-0.30	1.69***	-0.10	1.63***	0.35	1.13***	-0.03	1.29***	0.08	1.57***	-0.35	1.26***	-0.89	0.94***	0.32	1.03***	0.14
	(0.21)	(0.26)	(0.32)	(0.47)	(0.35)	(0.58)	(0.25)	(0.39)	(0.35)	(0.47)	(0.27)	(0.42)	(0.30)	(0.45)	(0.46)	(0.77)	(0.34)	(0.36)	(0.27)	(0.71)
Postgraduate	0.32*	0.18	0.37	0.70	0.24	2.72***	0.40*	0.56	0.23	1.56**	1.01**	-0.35	0.24	0.63	-0.08	1.56	0.66*	-0.00	0.32	0.09
	(0.18)	(0.23)	(0.31)	(0.53)	(0.31)	(0.92)	(0.24)	(0.37)	(0.27)	(0.61)	(0.40)	(0.36)	(0.29)	(0.58)	(0.48)	(0.98)	(0.35)	(0.43)	(0.34)	(0.36)
Years of education	0.06**	-0.13**	0.11**	-0.02	0.07***	-0.14	0.13***	0.08	0.00	-0.06	0.09*	-0.22**	0.10	0.01	0.05	-0.18	0.06	-0.18*	0.03	-0.06
	(0.03)	(0.06)	(0.05)	(0.11)	(0.03)	(0.15)	(0.04)	(0.08)	(0.03)	(0.09)	(0.05)	(0.09)	(0.06)	(0.10)	(0.08)	(0.11)	(0.06)	(0.10)	(0.04)	(0.14)
Vocational training	0.13	3.19***	1.26	1.18	0.69	1.33	0.98	-0.13	1.27	6.04*	-1.41	1.65	-0.73	5.51***	-0.44	6.73	-0.92	0.81	0.08	6.53*
	(0.53)	(1.19)	(1.04)	(2.57)	(0.80)	(4.76)	(1.09)	(2.00)	(0.94)	(3.25)	(1.35)	(2.11)	(1.16)	(2.03)	(1.02)	(5.09)	(1.91)	(2.09)	(0.83)	(3.84)

Notes: This table shows heterogeneity in reduced-form RD coefficients on the WWII-eligibility cutoff at 1927.5, by race, across state-of-birth subsamples. *Baseline* re-estimates the headline reduced-form RD on the subsample of US-born respondents; coefficients differ slightly from the headline column of Table 1 because that table also includes the small share of foreign-born respondents. The remaining columns restrict to: bottom/top quartiles of state per-capita WWII production-contract spending; bottom/top quartiles of population-weighted urban share in 1940; within the South, the bottom and top halves of states by HBCUs operational in 1944 per 100,000 Black residents in 1940 (*Low/High-HBCU South*); and, within the South, the five Southern states with the lowest and the five with the highest values of a state-level factor-score index of Jim Crow severity (higher values indicate more severe Jim Crow regimes; *Low/High-Jim Crow South*; middle states are excluded from the contrast). The final column (*High-Fraud Voc. States*) restricts to the seven states with at least three indicted or convicted vocational-school cases listed in the 1952 Teague Committee report (Teague Committee, 1952): Alabama, Arizona, Louisiana, New York, North Dakota, Pennsylvania, and Tennessee. The South vs. Non-South decomposition is reported separately in Appendix Table A5. All specifications use the baseline 4-year bandwidth, 3-quarter donut, local linear fits with separate slopes on each side of the cutoff, within-year season-of-birth fixed effects, person weights, and standard errors clustered on the running variable. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A8: Means for Veterans' Housing, Migration, and Income Outcomes

	White	Black
Geography and Migration (pooled 1970–1980, %)		
Interstate Migration	38.13	48.29
Left South (South-born)	22.68	42.79
Entered South (non-South-born)	8.73	6.84
Urban	71.57	85.05
On Farm	3.99	1.04
Housing		
Homeownership, 1970 (%)	70.45	40.70
Homeownership, 1980 (%)	79.65	56.47
Value of Owned Home, 1970 (nominal)	21,649.93	12,523.38
Value of Owned Home, 1980 (nominal)	62,977.89	34,676.20
Income and Labor Market		
Total Income, 1970 (nominal)	10,508.08	5,775.40
Total Income, 1980 (nominal)	21,638.38	12,508.77
Business Income, 1970 (nominal)	1,122.11	268.20
Business Income, 1980 (nominal)	2,040.75	497.23
Farm Income, 1970 (nominal)	178.03	15.80
Farm Income, 1980 (nominal)	347.19	39.11
Employed, 1970 (%)	93.60	84.63
Employed, 1980 (%)	88.45	75.74
Labor Force Participation, 1970 (%)	95.75	88.30
Labor Force Participation, 1980 (%)	91.70	81.55
Occupation Score, 1970	29.36	22.68
Occupation Score, 1980	29.42	22.52

Notes: This table reports the untreated (ineligible) mean for veterans' housing, migration, and income outcomes, by race. Means are computed just to the right of the 1927.5 WWII eligibility cutoff on the corresponding estimation sample (within the 4-year bandwidth, excluding the 3-quarter donut), using the IPUMS census samples (Ruggles et al., 2022). See Appendix Table A10 for definitions of the migration outcomes. Housing, income, employment, labor-force participation, and occupation score means are shown separately for the 1970 and 1980 censuses. Dollar-valued outcomes are in nominal dollars.

TABLE A9: Direct Effects on Veterans' Housing: IV Estimates

		White		Black	
		1970	1980	1970	1980
Homeownership		-0.50	0.20	-14.33	-3.97
		(3.02)	(1.84)	(14.15)	(11.02)
	Mean	70.45	79.65	40.70	56.47
Value of Owned Home		-1,040.50	**-3,421.01****	-5,126.97	**-10,457.88***
		(851.21)	(1,708.95)	(3,151.89)	(6,311.89)
	Mean	21,649.93	62,977.89	12,523.38	34,676.20

Notes: This table reports IV estimates for veterans' housing outcomes at the 1927.5 WWII eligibility cutoff, separately by race and by census year (1970 and 1980), using the 1970 and 1980 IPUMS census samples (Ruggles et al., 2022). IV estimates divide the reduced-form estimates by the race-specific first stage from Table A2. Homeownership is reported in percentage points; home value is in nominal dollars. Outcome means are reported in Appendix Table A8. The specification follows equation (2), with local linear fits on each side of the cutoff and within-year season-of-birth fixed effects. Our baseline specification uses a 4-year bandwidth on each side of the cutoff with a 3-quarter donut around the cutoff. Standard errors clustered on quarter of birth in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A10: Direct Effects on Veterans' Migration: IV Estimates

		White	Black
Interstate Migration		**-7.15*****	14.25
		(2.42)	(9.65)
	Mean	38.13	48.29
Left South		-2.04	15.06
		(2.28)	(9.73)
	Mean	22.68	42.79
Entered South		**-4.44*****	**12.07***
		(1.51)	(6.46)
	Mean	8.73	6.84
Urban		-1.55	-8.00
		(2.68)	(9.34)
	Mean	71.57	85.05
On Farm		-0.38	**3.34****
		(0.76)	(1.31)
	Mean	3.99	1.04

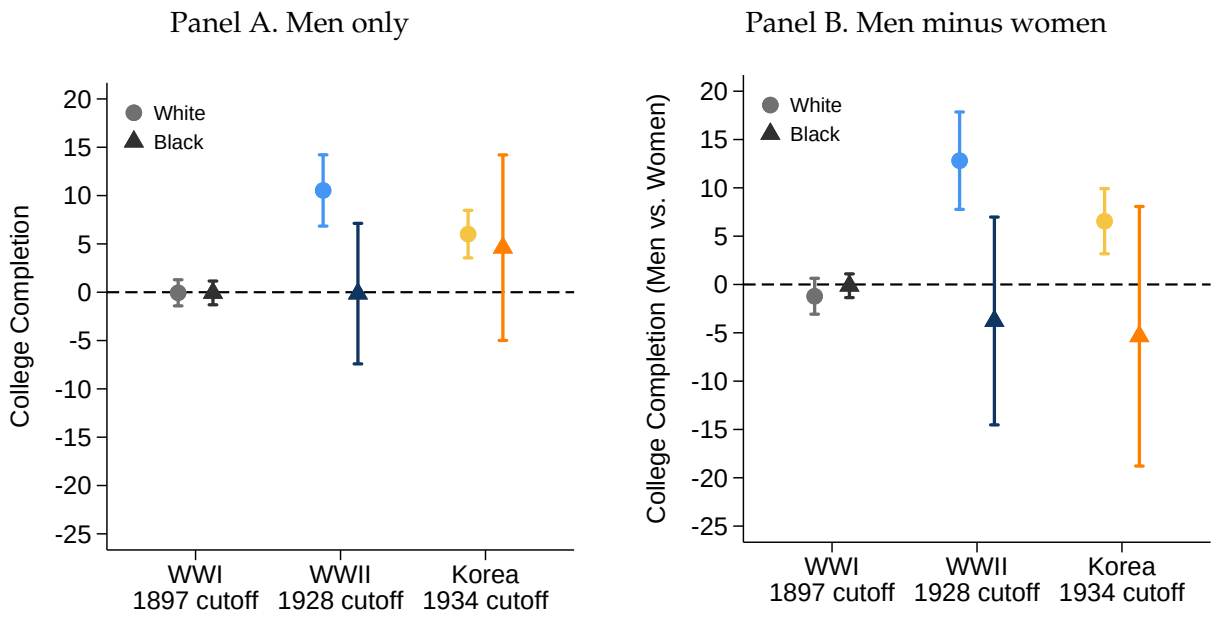
Notes: This table reports IV estimates for veterans' migration outcomes at the 1927.5 WWII eligibility cutoff, separately by race, using the pooled 1970 and 1980 IPUMS census samples (Ruggles et al., 2022). IV estimates divide the reduced-form estimates by the race-specific first stage from Table A2, except for Left South (estimated on South-born veterans) and Entered South (estimated on non-South-born veterans), where the first stage is re-estimated on the matching birthplace sample. All outcomes are in percentage points and measured at the census date. *Interstate Migration* indicates living in a different US state than the state of birth. *Left South* indicates living outside the South conditional on being born in the South, and *Entered South* indicates living in the South conditional on being born outside the South; the South comprises AL, AR, DE, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA, and WV. *Urban* is an indicator for living in an urban area and *On Farm* is an indicator for residing in a farm dwelling. Outcome means are reported in Appendix Table A8. The specification follows equation (2), with local linear fits on each side of the cutoff and within-year season-of-birth fixed effects. Our baseline specification uses a 4-year bandwidth on each side of the cutoff with a 3-quarter donut around the cutoff. Standard errors clustered on quarter of birth in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A11: Direct Effects on Veterans' Income and Labor Market Outcomes: IV Estimates

		White		Black	
		1970	1980	1970	1980
Total Income		-32.47	588.05	1,493.04	**-4,253.35***
		(317.34)	(503.18)	(1,324.39)	(2,417.16)
	Mean	10,508.08	21,638.38	5,775.40	12,508.77
Business Income		59.91	-20.51	-48.45	627.37
		(144.43)	(196.45)	(439.73)	(594.05)
	Mean	1,122.11	2,040.75	268.20	497.23
Farm Income		-39.71	-37.32	-58.14	-194.98
		(44.98)	(136.09)	(76.58)	(163.42)
	Mean	178.03	347.19	15.80	39.11
Employed		0.19	**6.01*****	**-13.55***	3.59
		(1.22)	(1.51)	(7.09)	(9.58)
	Mean	93.60	88.45	84.63	75.74
Labor Force Participation		0.04	**5.47*****	-5.36	9.74
		(0.84)	(1.49)	(7.36)	(11.33)
	Mean	95.75	91.70	88.30	81.55
Occupation Score		**1.23*****	**0.76****	-2.14	-4.22
		(0.44)	(0.35)	(2.08)	(2.63)
	Mean	29.36	29.42	22.68	22.52

Notes: This table reports IV estimates for veterans' income and labor-market outcomes at the 1927.5 WWII eligibility cutoff, separately by race and by census year (1970 and 1980), using the 1970 and 1980 IPUMS census samples (Ruggles et al., 2022). IV estimates divide the reduced-form estimates by the race-specific first stage from Table A2. Total personal income, business income, and farm income are in nominal dollars; employment status and labor-force participation are in percentage points; Occupation score is in score points. Outcome means are reported in Appendix Table A8. The specification follows equation (2), with local linear fits on each side of the cutoff and within-year season-of-birth fixed effects. Our baseline specification uses a 4-year bandwidth on each side of the cutoff with a 3-quarter donut around the cutoff. Standard errors clustered on quarter of birth in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

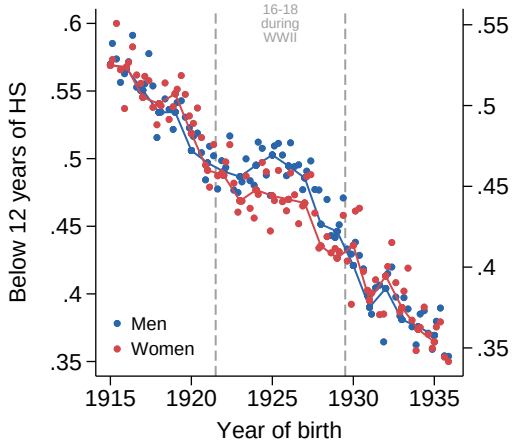
FIGURE A7: IV Estimates on Four-Year College for WWI, WWII, and Korean War Eligibility



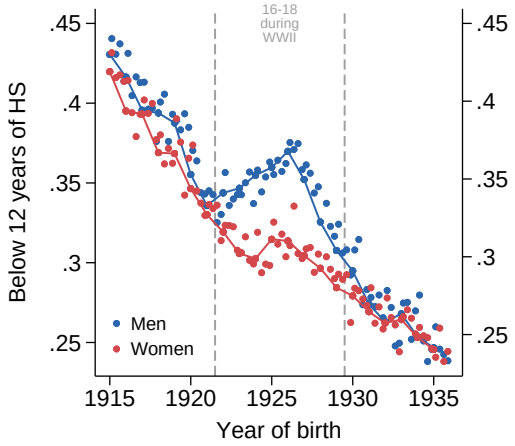
Notes: This figure shows IV estimates of the effect of military service on four-year college completion at three eligibility cutoffs: the May 1917 WWI draft upper-age limit (1897 birth year), the WWII Selective Service cutoff (1928), and the Korean War draft cutoff (1934). The IV scales the reduced-form discontinuity in college completion by the first-stage discontinuity in veteran status. WWI estimates use the 1940 full count for the reduced form and pooled 1950–1970 IPUMS for the first stage; WWII and Korean estimates use pooled 1970 and 1980 IPUMS (Ruggles et al., 2022). The WWII specification matches the headline 1927.5 design (4.823-year bandwidth, 3-quarter donut, season-of-birth fixed effects); the WWI and Korean specifications use a 5-year bandwidth (annual data) and 4-year bandwidth (quarterly data) respectively, with no donut where the cutoff falls between integer values. Panel A reports the men-only IV (men's reduced form divided by men's first stage); Panel B reports the gender-difference IV, in which the reduced form is the male-minus-female discontinuity in college completion and the first stage remains men-only (women's first stage is zero by assumption). Vertical bars show 95% confidence intervals. Standard errors are clustered on quarter of birth (WWII, Korea) or year of birth (WWI), with delta-method standard errors for the IV.

FIGURE A8: War-Production Intensity and Less-than-High-School Attainment

(A) **Low** War-Production Counties



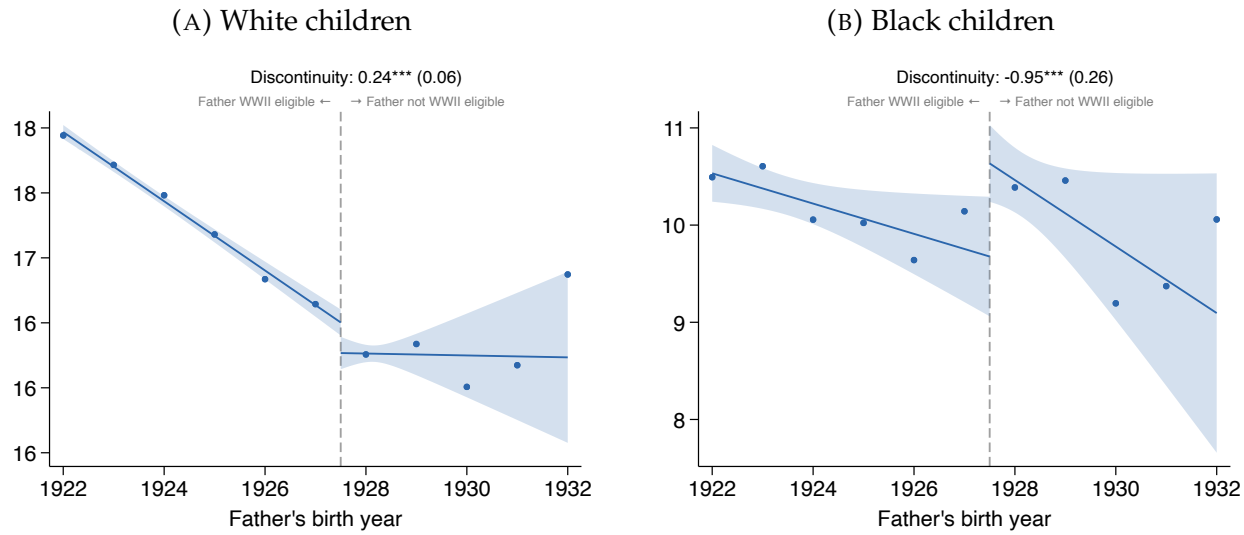
(B) **High** War-Production Counties



Notes: Each point is the mean share of white adults with less than twelve years of schooling, by year-of-birth quarter, measured from pooled 1970 and 1980 IPUMS extracts (Ruggles et al., 2022). Men (blue, left axis) and women (red, right axis) are plotted separately. Solid lines connect birth-year means, while vertical gray lines mark the cohorts who turned 16–18 during WWII (born 1921.5–1929.5). Panel (a) shows counties in the bottom quartile of per-capita WWII production contracts; Panel (b) shows counties in the top quartile. WWII production contract spending comes from the U.S. Department of Commerce, Bureau of the Census (2012) *County and City Data Book* consolidated file (ICPSR 7736). We sum the county-level supply-contract and facilities categories and divide by 1940 population. The sharp rise in less-than-high-school attainment in high-intensity counties is specific to men, consistent with young men leaving school for war-production jobs. See Section 3 for more discussion.

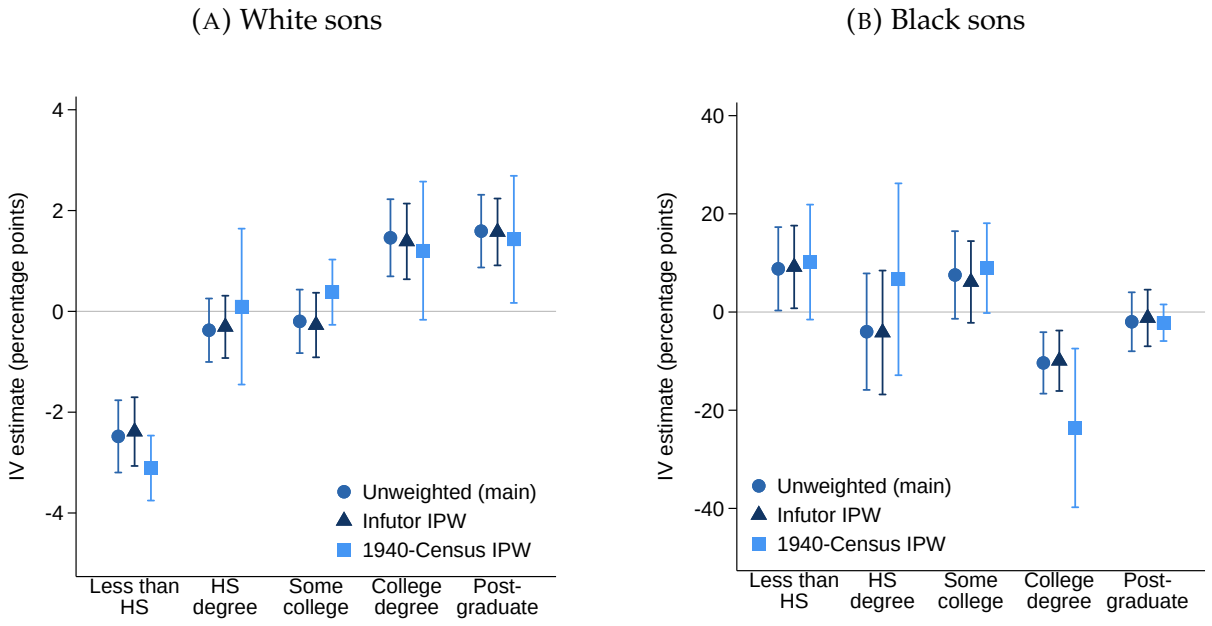
A.4. Additional Results on Intergenerational Effects

FIGURE A9: Intergenerational Reduced-Form Effects on Four-Year College Share in Sons' Adult Neighborhoods by Race



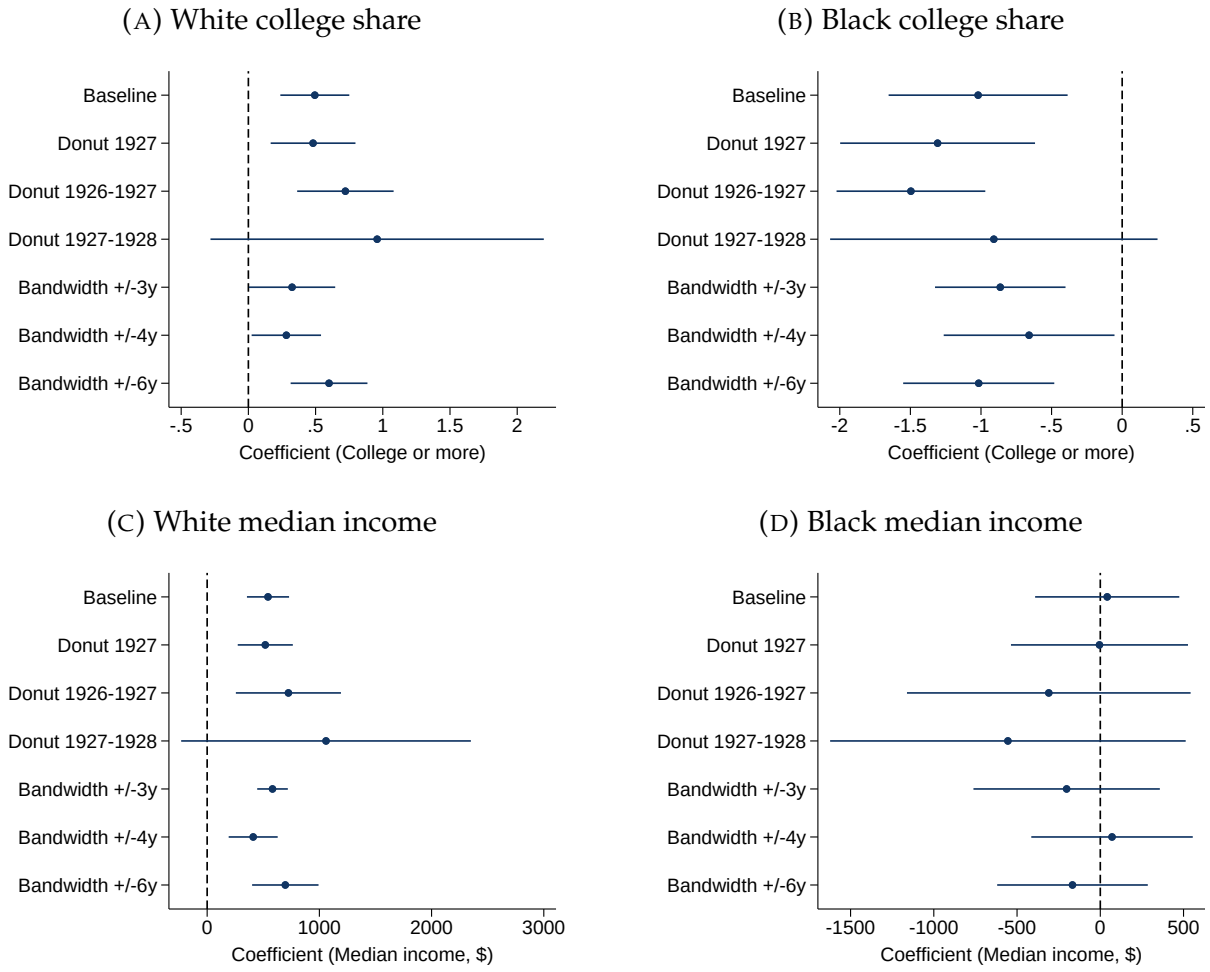
Notes: Panel (a) shows the RD for white children; Panel (b) for Black children. Each point is the race-specific male college share in the child's adult block group, averaged within a year-of-birth bin defined by the father's birth cohort. The sample is the intergenerational sample (Section 3), with adult neighborhoods identified from Verisk/Infutor address histories and merged to 2000-era Census block-group tabulations. Solid lines are local linear fits on each side of the 1927.5 cutoff. Reduced-form estimates appear above each panel with standard errors clustered on father's year of birth in parentheses. White control mean: 16.26; Black control mean: 10.20. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

FIGURE A10: Robustness of Intergenerational Effects to Inverse Propensity Weighting



Notes: This figure plots IV estimates for the son's adult block-group education shares (in percentage points), with 95 percent confidence intervals, comparing the baseline *Unweighted* specification to two inverse-propensity-weighted variants. *Infutor IPW* reweights by the inverse propensity of appearing in the matched sample, estimated on the son's adult ZIP-level neighborhood characteristics. *1940-Census IPW* reweights by the inverse 1940-to-1950 father-linkage propensity, estimated on 1940 father characteristics. Weights are winsorized at the 1st and 99th percentiles. Table 3 reports the baseline estimates.

FIGURE A11: Robustness of Intergenerational Effects Across Alternative Specifications



Notes: Reduced-form RD coefficients on two intergenerational outcomes, separately for white and Black children, across seven specifications. Panels (a)–(b) report the race-specific share of sons' adult-neighborhood block group with at least a four-year college degree (`blg_share_race_coll_plus`, the sum of the college degree and postgraduate rows of Table 3). Panels (c)–(d) report the race-specific block-group median income in nominal dollars (`blg_race_inc50_2000`). Specifications: (1) *Baseline* uses a linear running variable interacted with treatment, no donut, the father-birth-year window 1922–1932, IPW weights, linear and quadratic father age-at-birth controls, and standard errors clustered on father's year of birth; (2) *Donut 1927* drops the cohort straddling the 1927.5 cutoff; (3) *Donut 1926–1927* and (4) *Donut 1927–1928* drop two adjacent cohorts and widen the bandwidth by one year on each side (1921–1933) to preserve precision; (5), (6), and (7) vary the bandwidth to ± 3, ± 4, and ± 6 cohorts on each side of the cutoff. Horizontal bars show 95 percent confidence intervals.

TABLE A12: Means for Intergenerational Outcomes

	White	Black
Neighborhood Educational Shares (%)		
Less than High School	16.99	26.21
High School Degree	29.45	31.19
Some College	27.90	27.02
College Degree	16.26	10.20
Postgraduate	9.40	5.38
Neighborhood Income (nominal USD)		
Median Income	47,121.93	30,992.28
Median Family Income	54,391.29	33,089.04
Per Capita Income	23,018.91	14,287.12
Neighborhood Housing		
Homeownership (%)	73.58	51.70
Median House Value (nominal USD)	129,165.88	107,112.59

Notes: This table reports the untreated (ineligible) mean for each intergenerational outcome, by race. Means are computed on the intergenerational RD sample. Outcomes are race-specific block-group education shares, race-specific block-group income (median, median family, and per capita), race-specific block-group homeownership shares, and all-race block-group median house value, drawn from 2000-era Census tabulations and matched to sons via Verisk/Infutor address histories (see Section 3). Dollar-valued outcomes are in nominal dollars.

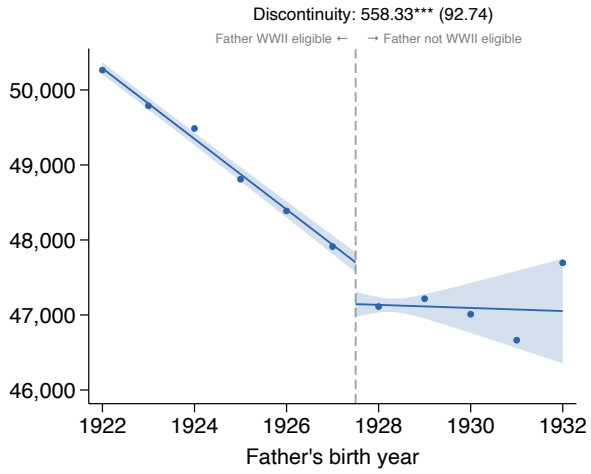
TABLE A13: Heterogeneity of Intergenerational Reduced-Form Effects by Father's 1940 County Characteristics

	Baseline		Bottom-90% War Production		Top-10% War Production		Low-Urban		High-Urban		Far from HBCU		Close to HBCU		Bottom-90% KKK Presence		Top-10% KKK Presence	
	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black
Less than HS	-0.40***	0.84**	-0.38***	0.65	-0.58***	1.83*	-0.78**	3.98***	-0.47***	1.94***	-0.47***	1.55	-0.14	0.61	-0.33***	0.08	-0.61***	4.36***
	(0.05)	(0.38)	(0.08)	(0.55)	(0.20)	(0.94)	(0.38)	(1.38)	(0.11)	(0.44)	(0.08)	(1.43)	(0.19)	(0.63)	(0.06)	(0.54)	(0.24)	(0.99)
HS degree	-0.05	-0.38	-0.04	-0.72	0.32	2.39*	0.62**	-3.43***	-0.00	0.90	-0.03	-0.87	0.41***	0.51	-0.10	-0.48	0.11	0.03
	(0.05)	(0.59)	(0.09)	(0.75)	(0.31)	(1.26)	(0.29)	(0.79)	(0.21)	(0.85)	(0.11)	(1.04)	(0.16)	(1.14)	(0.09)	(0.71)	(0.24)	(0.72)
Some college	-0.05	0.56	0.07*	1.61***	-0.06	-3.42*	0.55***	-1.52	-0.26***	0.58	-0.01	1.26	0.00	0.06	0.04	0.93**	-0.26**	-1.07**
	(0.05)	(0.38)	(0.04)	(0.40)	(0.13)	(1.75)	(0.20)	(1.67)	(0.08)	(0.88)	(0.09)	(1.45)	(0.18)	(0.35)	(0.05)	(0.38)	(0.13)	(0.50)
College degree	0.23***	-0.91***	0.22**	-1.79***	0.03	-1.18**	0.01	-0.10	0.33*	-3.36***	0.27**	-1.89*	-0.22***	-1.60***	0.23***	-0.49*	0.23	-2.77***
	(0.06)	(0.26)	(0.09)	(0.57)	(0.33)	(0.58)	(0.27)	(0.93)	(0.18)	(0.68)	(0.11)	(1.02)	(0.07)	(0.37)	(0.07)	(0.29)	(0.26)	(0.40)
Postgraduate	0.26***	-0.11	0.12**	0.24	0.29	0.37	-0.41***	1.07*	0.40**	-0.06	0.25*	-0.05	-0.05	0.42*	0.17***	-0.04	0.53***	-0.55
	(0.06)	(0.27)	(0.05)	(0.33)	(0.29)	(0.72)	(0.12)	(0.56)	(0.17)	(0.52)	(0.14)	(0.53)	(0.11)	(0.24)	(0.05)	(0.25)	(0.20)	(0.66)
Observations	414,797	23,199	192,694	9,796	104,000	2,466	36,029	2,556	184,141	4,900	235,197	4,682	61,550	7,580	298,434	19,516	116,363	3,683

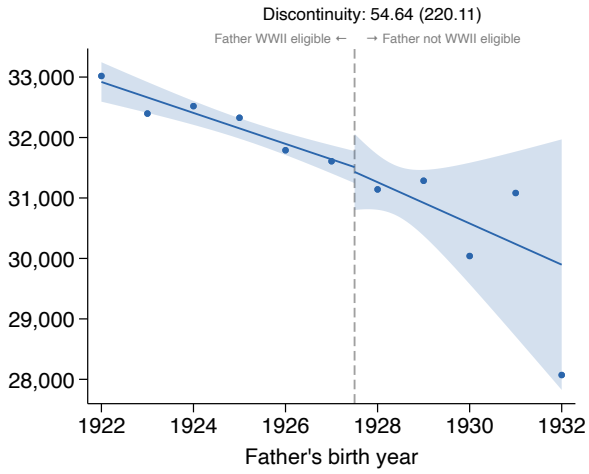
Notes: Reduced-form RD coefficients on the WWII-eligibility cutoff at 1927.5, separately by race, across nine specifications cut by the father's 1940 county of residence. Outcomes are race-specific male block-group education shares in sons' adult neighborhoods (in percentage points). *Baseline* reproduces the headline reduced-form column of Table 3. The remaining columns restrict to: counties in the bottom 90% of per-capita WWII production-contract spending in 1940 vs. counties in the top decile (*Bottom-90%/Top-10% War Production*, ICPSR 7736); bottom/top quartiles of population-weighted county urban share, 1940 IPUMS (*Low/High-Urban*); counties farther than the bottom-quartile distance threshold to the nearest HBCU operational in 1944 vs. those at or below it (*Far from / Close to HBCU*; cutoff $\approx$ 49 miles); and counties in the bottom 90% of klavern counts vs. counties in the top decile (*Bottom-90%/Top-10% KKK Presence*, VCU "Mapping the Klan" database). The asymmetric (top-decile vs. rest) WP and KKK cuts reflect the heavily right-skewed distributions (about 40 percent of counties have $\approx$\$0 WP per capita; about half have no recorded klavern); the distance-based HBCU cut avoids small-Black-population artifacts that arise with HBCU-density quartiles. All specifications follow the child-side baseline RD: linear running variable in father's year of birth, no donut, IPW weights, linear and quadratic father age-at-birth controls, and standard errors clustered on father's year of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE A12: Intergenerational Reduced-Form Effects on Median Income in Sons' Adult Neighborhoods by Race

(A) White children



(B) Black children



Notes: Panel (a) shows the RD for white children; Panel (b) for Black children. Each point is the block-group median income in the child's adult neighborhood, averaged within a quarter-of-birth bin defined by the father's birth cohort. Income is measured from 2000-era Census block-group tabulations merged to adult addresses via Verisk/Infutor. Solid lines are local linear fits on each side of the 1927.5 cutoff. Reduced-form estimates appear above each panel with standard errors clustered on father's year of birth in parentheses. white control mean: \$47,122; Black control mean: \$30,992. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A14: Intergenerational Effects on Median Income of Sons' Adult Neighborhoods

	Reduced Form		IV	
	White	Black	White	Black
Median Income	558.33***	54.64	3,347.47***	596.30
	(92.74)	(220.11)	(577.51)	(2,403.65)
Median Family Income	657.08***	217.47	3,939.58***	2,373.38
	(103.55)	(297.03)	(647.39)	(3,258.58)
Per Capita Income	322.32***	81.66	1,932.49***	891.23
	(90.61)	(251.52)	(550.64)	(2,747.76)

Notes: This table reports reduced-form and IV estimates for children's adult neighborhood income outcomes, by race, using the intergenerational sample described in Section 3. Income measures are race-specific block-group medians (in nominal dollars) from 2000-era Census tabulations, matched to children via Verisk/Infutor address histories. IV estimates divide the reduced-form estimates by the race-specific first stage on father's veteran status from Table A2. Outcome means are reported in Appendix Table A12. Standard errors clustered on father's year of birth in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A15: Intergenerational Effects on Housing in Sons' Adult Neighborhoods

	Reduced Form		IV	
	White	Black	White	Black
Homeownership	0.01	0.05	0.07	0.51
	(0.09)	(0.57)	(0.51)	(6.19)
Median House Value	**2,103.27****	954.03	**12,610.28*****	10,411.83
	(675.62)	(1,348.66)	(4,093.09)	(14,790.49)

Notes: This table reports reduced-form and IV estimates for children's adult neighborhood housing outcomes, by race, using the intergenerational sample described in Section 3. Outcomes are race-specific block-group homeownership shares (in percentage points) and all-race median house values (in nominal dollars) from 2000-era Census tabulations, matched to children via Verisk/Infutor address histories. IV estimates divide the reduced-form estimates by the race-specific first stage on father's veteran status from Table A2. Outcome means are reported in Appendix Table A12. Standard errors clustered on father's year of birth in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A.5. Role of Local Spillovers

We provide full details on the construction of the residualized community veteran share $V^*_{\ell(i)}$ used in the spillover equation 5, together with three pieces of validating evidence: balance on prewar characteristics, qualitative corroboration from contemporary accounts, and correlation between the residualized share and qualitative reports of board leniency.

Construction. We construct $V^*_{\ell(i)}$ from the 1940-1950 linked full-count census, restricted to men aged 12–50 in 1940 with non-missing 1950 veteran status. The residualization proceeds in two steps. First, we estimate county fixed effects on 1950 veteran status in a regression that partials out individual-level 1940 covariates: family demographics, race, occupation, industry, schooling, ownership, marital status, rural and foreign-born status, migration, and Hispanic ethnicity. Second, we residualize the recovered county fixed effects against 1940 county-level characteristics, such as Black population share, female labor force participation, agricultural employment share, essential-job shares, and per-capita WWII supply spending (from the U.S. Department of Commerce, Bureau of the Census, 2012, *County and City Data Book*, ICPSR 7736)).

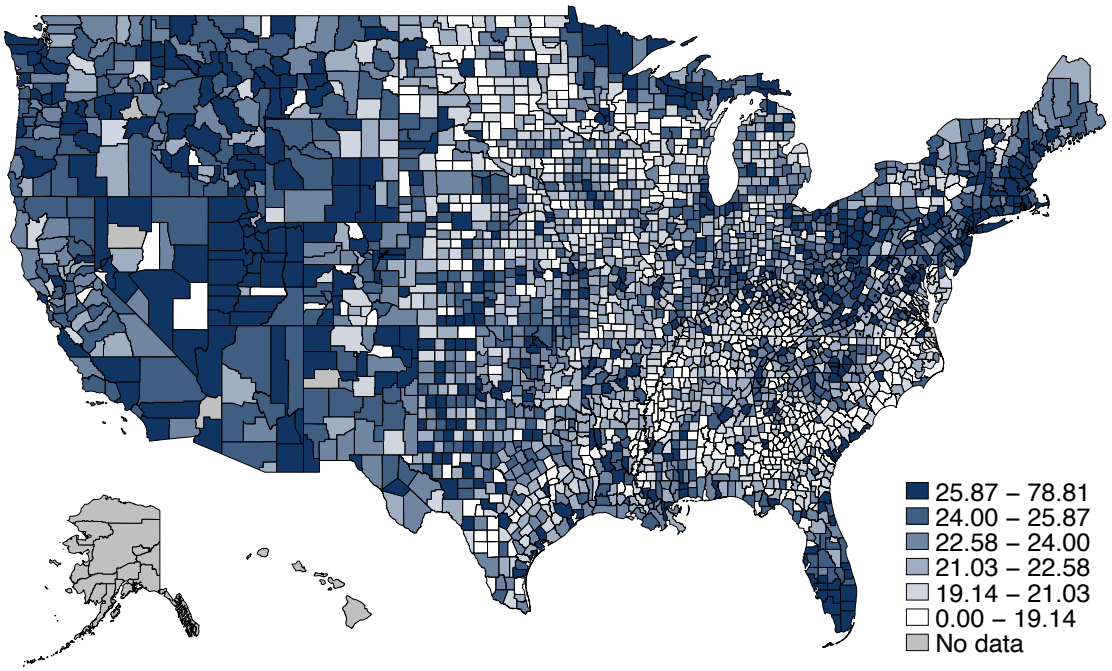
The first step absorbs individual-level determinants of military service by the draft deferment system.[12] The second stage removes county-level characteristics that predict aggregate veteran shares through demographic composition or wartime industrial structure. Together, both steps remove most observable determinants of local veteran concentration ($R^2 = 0.82$), leaving variation driven by differences in how more than 6,000 local draft boards implemented national drafting policy.

Appendix Figure A13 plots raw and residualized veteran shares across counties. While raw shares exhibit clear regional patterns reflecting demographics, labor markets, and wartime industrial structure, residualized variation is more dispersed, consistent with quasi-random differences in draft board implementation.

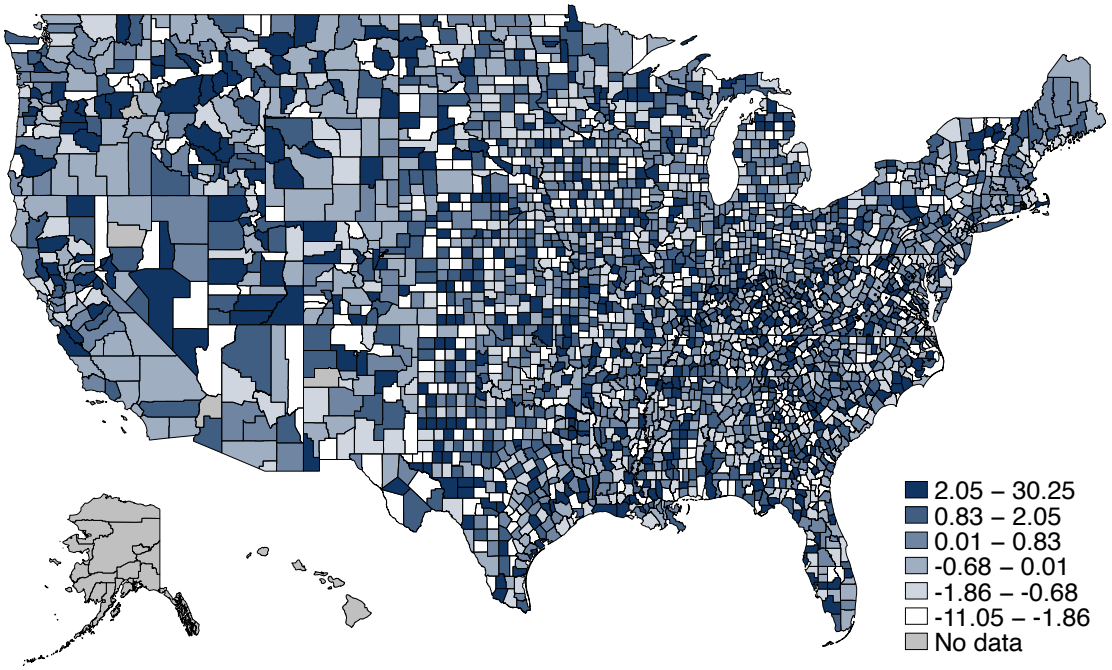
[12]Eligible older men could receive occupational or dependency-based draft deferments. Class 2 covered essential civilian roles (including defense and agriculture), while Class 3 deferred married men and fathers. Class 4 included those exempt from service, such as individuals with prior service, public officials, clergy, conscientious objectors, and those deemed unfit. A temporary Class H, introduced in 1943, exempted men aged 38-45, though they could still volunteer (US Selective Service System, 2022).

FIGURE A13: County-Level Draft Intensity

(A) WWII Veteran Share



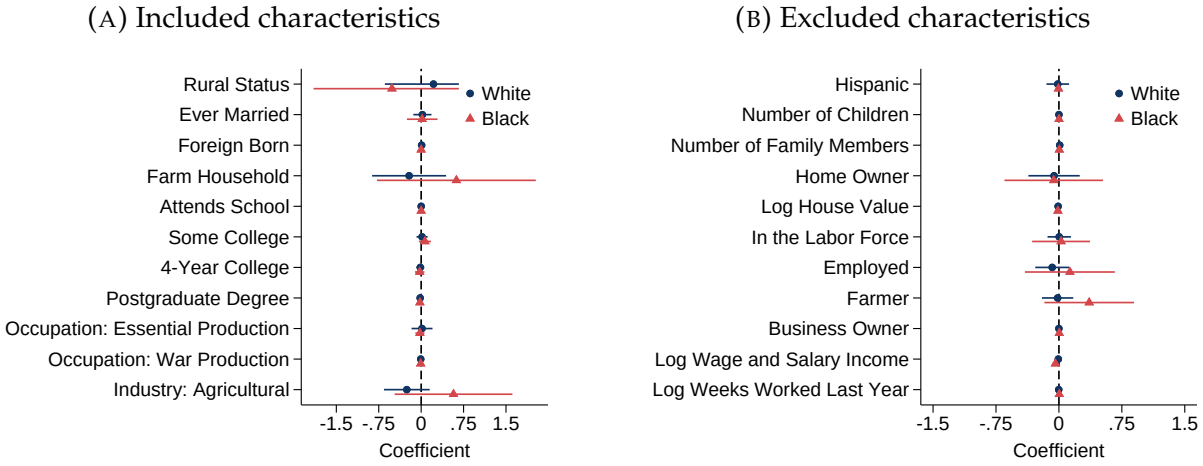
(B) Residualized WWII Veteran Share



Notes: Maps are at the level of child *i*'s father's 1940 county of residence. Panel (a) shows the share of men reporting WWII veteran status; Panel (b) shows the residualized veteran share, after partialling out individual-level 1940 covariates, county-level characteristics, and commuting-zone fixed effects. More details about the procedure can be found in Section 5.3). The raw share in Panel (a) exhibits clear regional patterns reflecting demographics and labor-market conditions; Panel (b) shows substantial residual variation across counties after these systematic components are removed, consistent with idiosyncratic differences in local draft-board implementation. Both measures use the 1940–1950 linked full-count census restricted to men aged 12–50 in 1940 with non-missing 1950 veteran status.

Balance on Prewar Characteristics. The identifying assumption is that $V^*_{\ell(i)}$ is uncorrelated with unobserved determinants of children's outcomes, conditional on father characteristics. We assess this by testing whether $V^*_{\ell(i)}$ is correlated with prewar characteristics included and excluded in the residualization procedure. Appendix Figure A14 reports the coefficients from regressions of $V^*_{\ell(i)}$ on this broad set of prewar covariates, indicating that the residual variation does not capture observable local differences.

FIGURE A14: Validation of $V^*_{\ell(i)}$: Balance on Pre War Characteristics



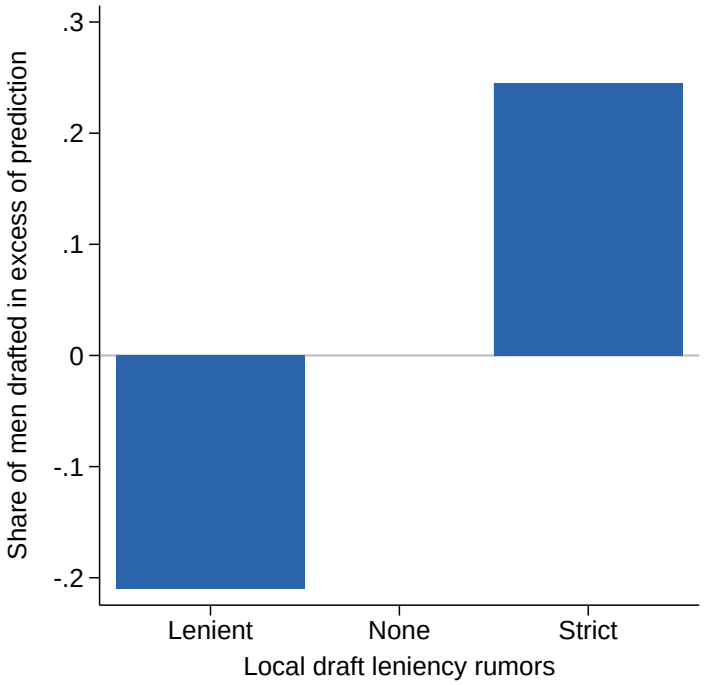
Notes: This figure reports estimated coefficients from regressions of 1940 prewar characteristics on the residualized community veteran share $V^*_{\ell(i)}$, separately by race and by whether the characteristic was included in or excluded from the residualization procedure (Section 5.3). Panel (a) reports characteristics included in the residualization (urban status, ever married, foreign-born, farm household, school enrollment, education attainment, war and essential occupations, industry composition); Panel (b) reports characteristics excluded from the residualization (Hispanic ethnicity, number of children, family size, homeownership, log house value, labor-force participation, employment, farmer, business owner, log wage income, log weeks worked). Horizontal bars show 95 percent confidence intervals. Standard errors are clustered at the county level.

Qualitative Corroboration. Contemporary accounts corroborate the interpretation that $V^*_{\ell(i)}$ captures idiosyncratic draft-board variation. The World War II Rumor Project Collection, compiled by the Office of War Information in 1942–1943, documents widespread concerns about favoritism and uneven enforcement across local draft boards (Office of War Information, 1942–1943). Reports describe cases where individuals avoided service due to local connections ("He wasn't called because his father's on the draft board" in St. Louis, Missouri) and differential treatment across groups ("Selective Service Boards are showing favoritism to many single men while they are drafting married men" in Raleigh, North Carolina). These accounts indicate that the implementation of national drafting policy varied substantially across local boards in ways not captured by demo-

graphic or economic fundamentals.

Consistent with these accounts, areas described in the rumors as having lenient boards exhibit lower-than-predicted veteran shares in our residualized measure $V^*_{\ell(i)}$, while areas described as strict exhibit higher shares (Appendix Figure A15).

FIGURE A15: Draft Board Leniency Rumors and Residualized Veteran Shares



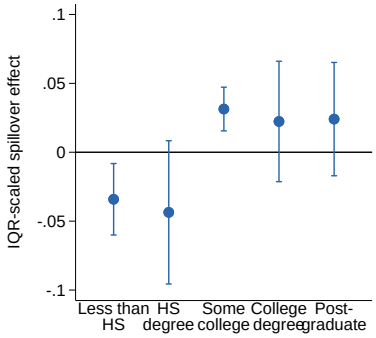
Notes: This figure corroborates the residualized community veteran share with qualitative evidence from the World War II Rumor Project Collection (Office of War Information, 1942–1943). We code each rumor transcript as reflecting a lenient or strict local draft board and geocode the rumors to counties. The figure plots the residualized veteran share by coded leniency, using the partialling-out regression described in Section 5.3. Counties with rumors of lenient draft boards have lower-than-predicted veteran shares; counties with rumors of strict boards have higher-than-predicted shares.

Spillover Estimates. Appendix Figure A16 reports estimates of δ from Equation 5 for white and Black sons separately, across the educational distribution. For white sons, moving from the 25th to the 75th percentile of local veteran exposure is associated with small improvements in educational attainment, concentrated at the upper end of the distribution (some-college, college, and postgraduate margins). For Black sons, spillover estimates show no consistent positive pattern, with point estimates often near zero or slightly negative across education margins. Across both groups, the spillover effects are economically small relative to the family-level effects reported in the main text, supporting the

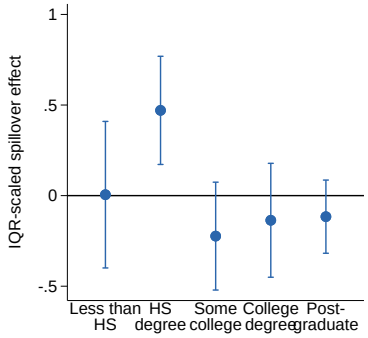
conclusion that the racial divergence in intergenerational outcomes operates primarily through the family channel.

FIGURE A16: Local Spillovers:
IQR-Scaled Effects on Sons' Adult-Neighborhood Education

(A) White children



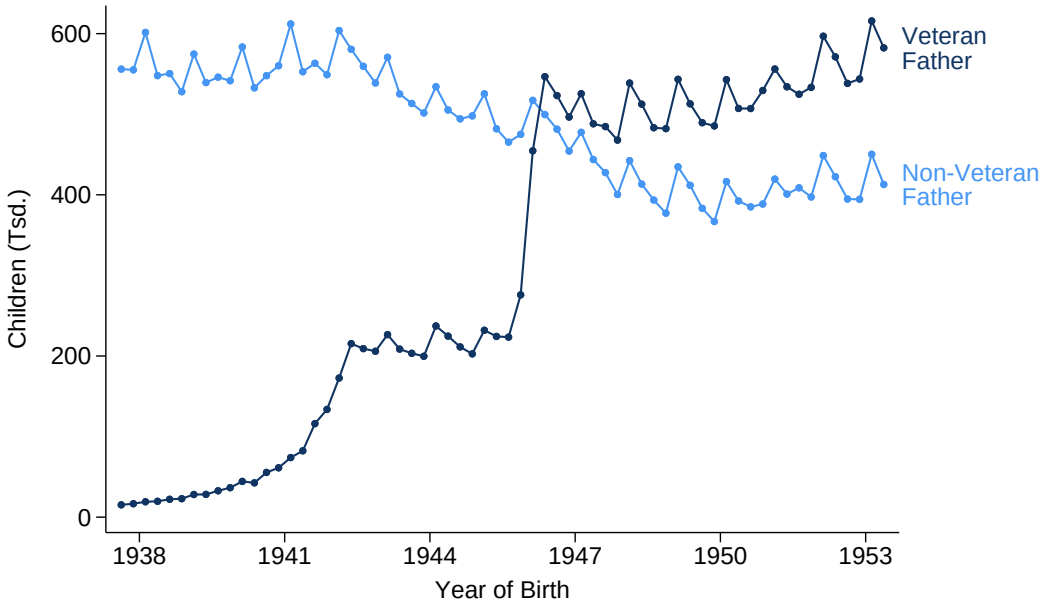
(B) Black children



Notes: This figure reports estimates of local spillover effects scaled by the interquartile range (IQR) of residualized county veteran share. Estimates for white and Black sons are shown in Panels (a) and (b), respectively. Each point estimate shows the change in sons' adult-neighborhood outcome associated with moving from the 25th to the 75th percentile of local veteran exposure, holding father veteran status constant. Outcomes are race-specific block-group education shares. Regressions control for a linear spline in birth cohort around the 1927.5 eligibility cutoff and father age at birth, and standard errors are clustered by father's year of birth.

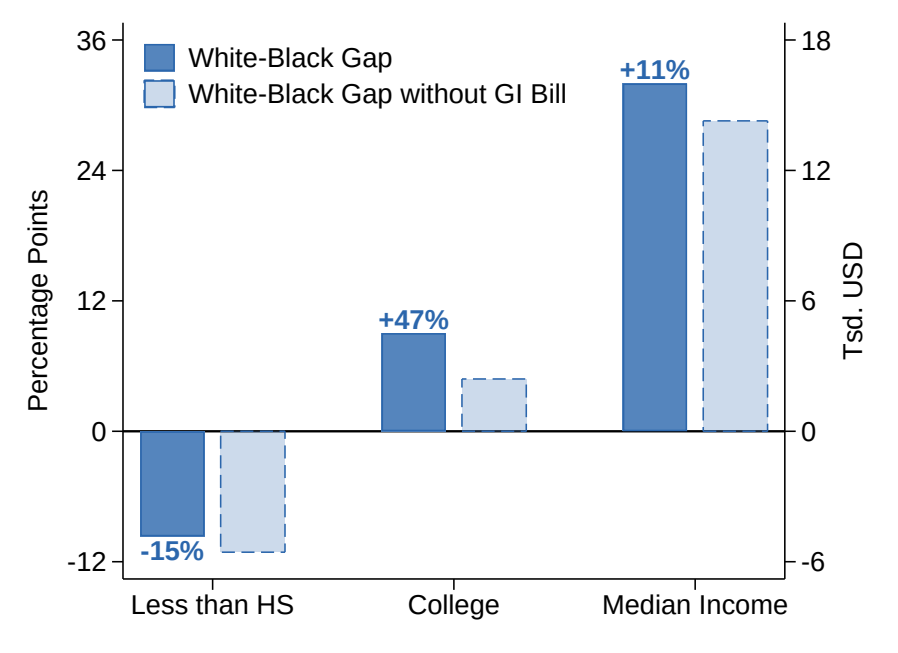
A.6. Aggregate Implications for Racial Inequality

FIGURE A17: Baby-Boom Children by Father's Veteran Status



Notes: This figure plots the number of US-born children, in thousands, by year of birth and father's WWII veteran status. The prevalence of veteran fathers among baby-boom cohorts motivates our analysis in Section 5.4.

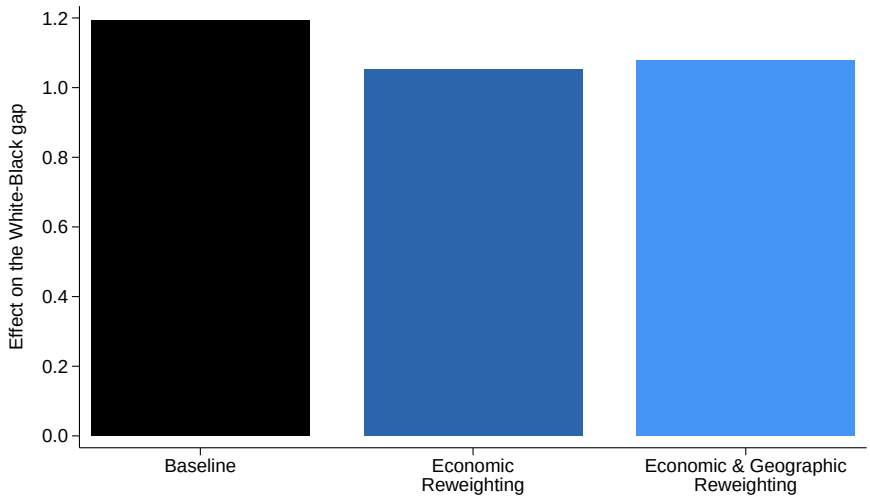
FIGURE A18: Counterfactual Racial Gaps Among Baby Boomers



Notes: This figure scales the family-level intergenerational effects from Table 3 into implied aggregate effects on racial gaps among baby boomers. The counterfactual subtracts the race-specific intergenerational effects (weighted by the share of each cohort with veteran fathers) from observed boomer outcomes. We then recompute the white-Black gap under this counterfactual and compare to the observed gap. The implied widening of the college gap is 47 percent of the observed gap; the less-than-HS gap narrows by 15 percent, and the income gap widens by 11 percent. The underlying cohort exposure to veteran fathers are plotted in Figure A17.

A.7. Decomposition and Mechanisms

FIGURE A19: Prewar Endowments Explain Little of the Racial Gap in Intergenerational Reduced-Form Four-Year College Effects



Notes: This figure shows the racial gap in the intergenerational reduced-form effects on the male college share in sons' adult neighborhoods, before and after reweighting white fathers to match Black fathers on 1940 prewar characteristics. *Economic Reweighting* matches white fathers to Black fathers on cells defined by father's birth cohort, 1940 homeownership status, and quartiles of 1940 household-average income and education (averaged over prime-age adults aged 18–54 in the father's 1940 household, since the fathers themselves were typically still children in 1940); *Economic & Geographic Reweighting* additionally matches on father's state of residence in 1940. The small change after reweighting indicates that observable prewar differences explain little of the intergenerational racial gap. See Section 6 for details on the reweighting procedure.

A.8. Additional Validation Exercises

Robustness Checks. The main results are robust to alternative bandwidth choices and donut-hole specifications that exclude cohorts immediately adjacent to the cutoff. Appendix Figure A4 shows that the first-stage and direct reduced-form effects on white and Black veterans' college completion are stable across these specifications, and Appendix Figure A11 shows the same stability for the intergenerational effect on sons' adult-neighborhood college share.

Military Service vs. GI Bill. The three twentieth-century wars provide a natural comparison: WWI carried no GI Bill, while WWII (1944) and the Korean War (1952) each came with similar education subsidies. We apply the same RD design at each war's draft

upper-age cutoff—1886 for WWI (May 1917 limit), 1928 for WWII, and 1934 for the Korean War—separately by race. Any cohort discontinuity in WWI veterans' outcomes must come from service itself or from other cohort-level channels (Depression-era schooling disruption, the 1918 influenza pandemic, child-labor laws), since WWI veterans received no education subsidy. The Korean War, in contrast, had a similar GI Bill. To net out cohort shocks common to both sexes, we also estimate each RD with the reduced form expressed as the male-minus-female discontinuity, leveraging that women report minimal veteran service in each war.

Figure A7 reports IV estimates on four-year college completion. Panel A (men only) shows WWI eligibility producing estimates statistically indistinguishable from zero, while WWII and Korean War eligibility produce estimates of similar magnitude. Panel B (male-minus-female) shows similar effects after netting out cohort shocks. The finding that WWI did not raise college completion—or affect other education margins (results available upon request—is consistent with prior work finding small or negative returns to wartime service alone (Angrist, 1998). The Korean War's effect matches that of WWII, which we would expect if the GI Bill is the operative channel.

B. Data Appendix

The two parts of the paper rely on distinct but complementary data sources. We estimate direct effects on veterans from postwar census cross sections, and intergenerational effects from a combination of historical census linkages and modern address histories that follow descendants into adulthood.

B.1. Veteran and Intergenerational Data

We estimate direct effects on veterans using the 1960, 1970, and 1980 IPUMS samples (1–5%) (Ruggles et al., 2022), restricted to white and Black male cohorts within the RD bandwidth of the 1927.5 eligibility cutoff. These files provide both the first-stage measure of veteran status and the veteran-generation outcomes used throughout the paper, including education, income, housing, and migration.

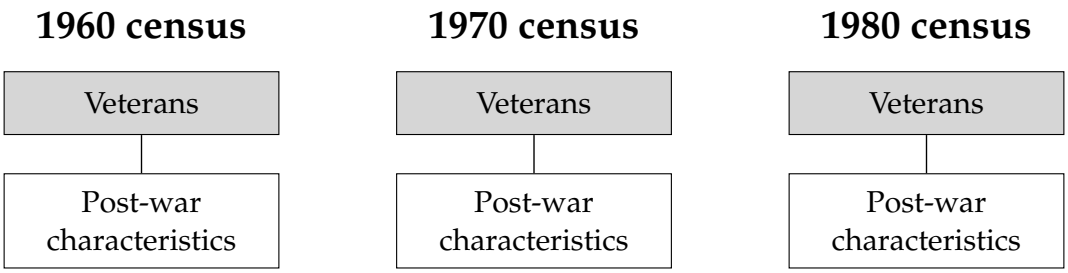
We estimate intergenerational effects using a data linkage that combines three layers: (i) children observed in the 1950 census; (ii) their fathers linked back to 1940 for prewar characteristics; and (iii) adult outcomes for those children inferred from Verisk-Infutor address histories observed between 1990 and 2025, geocoded and merged to NHGIS tab-

ulations (Schroeder et al., 2025) at the ZIP, block-group, and county levels. This structure allows us to follow the descendants of the veteran cohorts into adulthood while keeping the intergenerational sample aligned with the same birth-cohort discontinuity that identifies the direct effects.

B.2. Veteran Sample

The veteran sample is constructed from men observed directly in postwar census cross sections using harmonized IPUMS variables. The resulting data allow us to study how WWII and GI Bill eligibility translated into later-life outcomes for veterans before turning to children. The first-stage measure is an indicator for WWII or Korean War veteran status, built from `vetwwii` and `vetkorea` in the cleaned IPUMS files. Education outcomes are built from `educd`, vocational training from `schlvoc`, and income, housing, and migration outcomes follow the definitions in Appendix Section B.4. Appendix Figure B1 illustrates the data structure for the veteran sample.

FIGURE B1: Data for Direct Effects on Veterans



Notes: This figure illustrates the data structure for the veteran-generation analysis. Each column represents a separate 1–5% IPUMS census cross section (Ruggles et al., 2022). Veterans are identified by self-reported veteran status; post-war outcomes include education, vocational training, income, housing, and migration.

B.3. Intergenerational Sample

B.3.1 Father-Child Linkage

The intergenerational sample is constructed in four steps: linking fathers across the 1940 and 1950 censuses, identifying children in the 1950 household structure, matching those children to present-day address histories, and constructing adult outcomes from them.

Step 1: Linking fathers from 1940 to 1950. We combine two 1940–1950 census crosswalks: the Census Linking Project (CLP) crosswalk (Abramitzky et al., 2021) and the Machine Linking Project (MLP) crosswalk (Bailey et al., 2020). Each crosswalk provides matched `histid` pairs linking individual records across the two censuses. We merge the two and drop any `histid` where the CLP and MLP disagree on the matched record. The remaining concordant links forms the father-linkage sample. Because the crosswalks are constructed primarily for men and our focus is on male veterans, we restrict the linked sample to male records. This step supplies the father's 1950 census record alongside prewar 1940 characteristics: education, income, state of residence, urban status, and household-level SES measures constructed from prime-age adults aged 18–54 in the father's 1940 household.

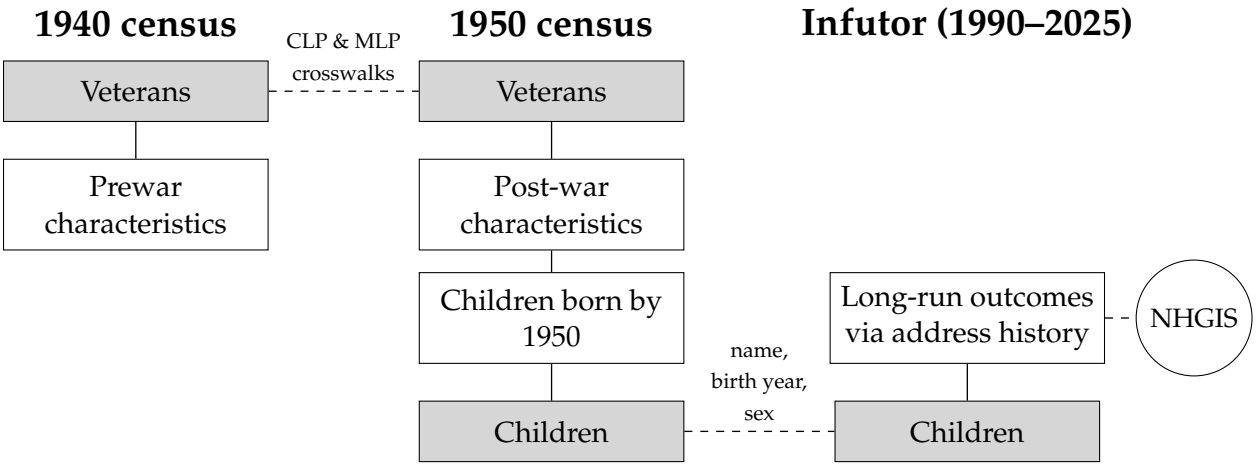
Step 2: Identifying children in the 1950 census. The 1950 census records household structure, allowing us to identify children who co-reside with their fathers using the IPUMS `poploc` pointer within household identifiers. We focus on children of fathers born around 1927, the cutoff of our RD design. Within the $\pm$5-year RD bandwidth, these fathers are 18–28 years old in 1950, so children born by 1950 are predominantly young and still living at home. We retain the child's `histid`, birth year, race, birthplace, cleaned names, the father's 1950 identifier, and the child's 1950 county.

Step 3: Matching children to Infutor. We match children identified in the 1950 census to Verisk/Infutor address-history records on name, birth year, and sex. We restrict the extract to male children before matching, because most women changed their surname at marriage between 1950 and the Infutor observation window, which collapses match rates and induces selection on marital history. Restricting to sons is also standard the historical-linkage literature (Abramitzky et al., 2014, 2021). The matching pipeline cleans names and processes records in surname-initial batches. It matches on sex and cleaned names with a narrow birth-year window when date of birth is observed, and falls back to name-only passes for records without date of birth. We retain only unique matches and exclude the weakest (those with no middle name and no date of birth).

Step 4: Constructing outcomes from address histories. In Verisk/Infutor records, each person has a complete address history with first and last dates at each address. Each address comes with geocoded latitude and longitude. We spatially join each address point to 2000-vintage U.S. Census block-group polygons, producing a year-2000 GEOID

for every address in the history. This avoids using ZIP codes, whose boundaries change over time. We then construct day-weighted summaries across each person's addresses, weighting by length of residence. We merge the resulting GEOIDs to NHGIS tract and block-ground tabulations from the 2000 decennial census (Schroeder et al., 2025), yielding the race-specific educational, income, and housing measures used in the intergenerational analysis.[13] Appendix Figure B2 illustrates this linkage structure.

FIGURE B2: Data for Intergenerational Effects: Overview of Novel Data Linkage



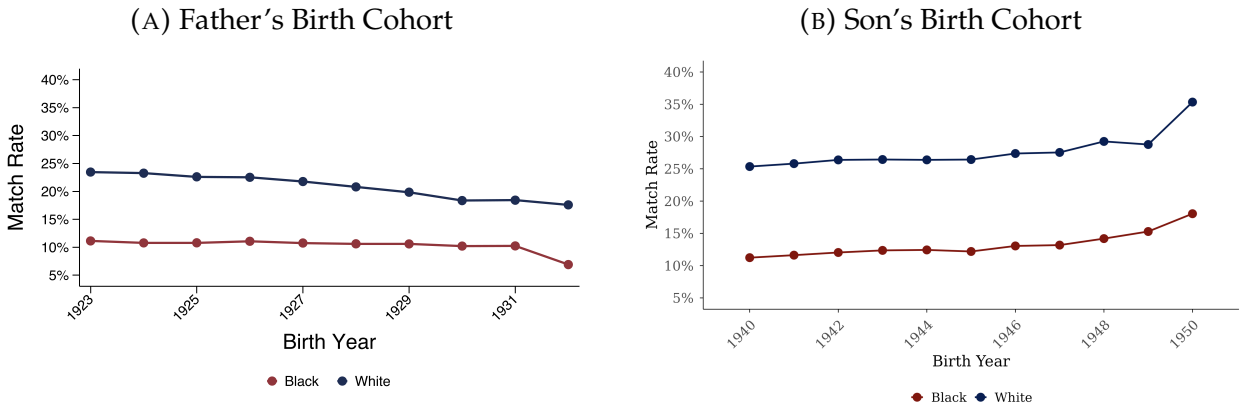
Notes: This figure illustrates the intergenerational data linkage. The 1940 census provides prewar father characteristics; the 1950 census identifies household structure and children; and Infutor/Verisk address histories (1990–2025) provide adult addresses matched to neighborhood-level outcomes from NHGIS Census tract and block-group tabulations (Schroeder et al., 2025). Dashed lines indicate record linkages across datasets. Shaded boxes indicate the units tracked across sources.

B.3.2 Linkage Quality and Representativeness

Figure B3 reports match rates by race and birth cohort. Linking rates are higher for white than for Black fathers and sons, reflecting the broader racial gap in historical record linkage. Most importantly for our research design, there is no visible discontinuity in linkage around the 1927.5 draft-eligibility cutoff that identifies the first stage.

[13] ZIP-level measures, used in Appendix Table B1, are constructed analogously: each observed ZIP5 is merged to a pre-tabulated 2000 Census ZIP5 file, with day weights aggregating across the ZIPs a person inhabited.

FIGURE B3: Census-Infutor Match Rates by Race and Birth Cohort



Notes: This figure plots the share of children observed in the 1950 census that are successfully linked to Infutor records, by race. Panel (a) groups by fathers' birth cohorts; Panel (b) groups by sons' birth cohorts.

To assess representativeness on observables, we construct two inverse-propensity weights. The first (*Infutor IPW*) reweights matched children toward a random sample of unmatched Verisk/Infutor records using birth year and adult ZIP-level observables (education, ownership, income, and house values). The second (*1940-Census IPW*) reweights the linked sample toward the underlying 1940 father-child census frame using prewar household-level variables (wage income, education, urban residence, and home ownership) and father birth year. Both weights are winsorized at the 1st and 99th percentiles before estimation.

Appendix Table B1 compares matched sons to the unmatched Verisk/Infutor draw on nine ZIP5-level covariates and son's birth year. The matched sample differs only modestly: homeownership is 2 percentage points higher, median household income $1,600 higher, and median home value $3,100 higher. The remaining variables are essentially identical across the two samples. Appendix Figure A10 further confirms that the two inverse-propensity reweighting options leave the baseline estimates essentially unchanged.

TABLE B1: Summary Statistics: Matched vs. Unmatched Verisk/Infutor Records

	Matched (1)	Unmatched (2)
Mean birth year	1,944.4	1,944.1
Share with less than high school	0.18	0.20
Share with high school degree	0.28	0.28
Share with college degree	0.17	0.16
Share with postgraduate degree	0.10	0.10
Homeownership rate	0.69	0.67
Median household income	46,314	44,733
White per-capita income	24,440	23,788
Median home value	134,547	131,410
White male median age at death	73.3	73.1
N	4,785,694	712,135

Notes: This table compares sample means for the matched sub-sample (sons successfully linked to Verisk/Infutor) and a random sub-sample of unmatched Verisk/Infutor records, pooled across races and restricted to sons born by 1949. ZIP5-level covariates are drawn from year-2000 NHGIS tabulations; dollar-valued variables are winsorized at the 1st and 99th percentiles. Covariates for matched records are day-weighted across each person's address history; covariates for unmatched records are measured at each person's modal ZIP5, because the unmatched extract does not include per-address day weights.

B.3.3 Sample Selection and Interpretation

The intergenerational analysis is defined on a selected descendant sample rather than on all children of relevant male cohorts. A child enters the intergenerational sample only if he is born and co-resident with his father in the 1950 census, survives the 1940–1950 father linkage, and appears in the Verisk/Infutor extract. Children born after 1950 are excluded by construction. These restrictions matter for interpretation because GI Bill eligibility could affect fertility timing, co-residence in 1950, and whether descendants appear in Verisk/Infutor records. The intergenerational estimates should therefore be read as effects on the observed descendants, not on the underlying population.

B.4. Outcome Construction

Outcomes for the veteran analysis are built from harmonized IPUMS census variables. Education comes from `educd`: we construct years of schooling and attainment indicators for less than high school, high school, some college, four-year college, and postgraduate schooling. Binary outcomes are expressed in percentage points and defined for ages 24–75.

Vocational schooling is measured using `schlvoc`, a categorical variable available only in the 1970 census (Form 1, universe persons aged 14 and older). It records whether the respondent ever completed a vocational training program and, if so, the main field of training. The coded categories are: (1) no vocational training; (2) business or office work; (3) nursing or health fields; (4) trades and crafts; (5) engineering, drafting, or science technician; (6) agriculture or home economics; (7) other vocational field, and (8) field not reported. The original wording reads:

> 27a. Has this person ever completed a vocational training program? For example, in high school; as apprentice; in school of business, nursing or trades; technical institute; or Armed Forces schools. (☐ Yes ☐ No – Skip to 28)

The accompanying enumerator instruction specifies the scope: "Count only programs that he finished. Do not count courses which are not part of an organized program of study. Do not count training he got on-the-job, in company schools, in college after the second year, or by correspondence."

We use `schlvoc` to construct an indicator for any completed vocational training and separate indicators for each reported field (business, health, trades, engineering/science, agriculture, and other). Because the variable captures only completed programs, the estimates in the paper should be read as effects on the probability of having ever completed a formal vocational program outside of college, rather than as effects on any vocational training, completed or not.

Income, housing, and migration outcomes for veterans follow standard definitions. Invalid codes for total income (`inctot`) and house value (`valueh`) are recoded to missing. Homeownership is an indicator for owner-occupied housing; our preferred measure restricts ownership to household heads or spouses, and we interact it with house value for a combined ownership-and-value measure. Migration outcomes compare birthplace to current state of residence, yielding indicators for interstate migration, leaving the South, and moving to the South.

For the intergenerational sample, the outcome variables are long-run neighborhood-based measures attached to linked adult addresses rather than direct survey outcomes. We merge ZIP- and block-group-level covariates from NHGIS and related sources, then form day-weighted averages across the observed address history.

Education outcomes are race-specific male block-group attainment shares from the 2000 Census, for less than high school, high school, some college, college, and post-graduate. Income and housing outcomes are the corresponding race-specific block-group median income, median family income, per-capita income, and owner-occupancy share, together with all-race median house value. All share outcomes are reported in percentage points, and IV estimates scale the reduced forms by the pooled 1970–1980 first-stage estimate on WWII or Korean War veteran status.