WHO BENEFITED FROM WORLD WAR II SERVICE AND THE GI BILL?
NEW EVIDENCE ON HETEROGENEOUS EFFECTS FOR US VETERANS

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## ABSTRACT

We study the impacts of WWII service and access to GI Bill benefits on the educational and labor
market outcomes of individuals of various ethnic and racial groups. We address selection into
military service directly by linking veterans and nonveterans from 1950's census records to the
complete-count 1940 census. We find that veterans were positively selected on the basis of
education, and neutrally or negatively selected on the basis of their own or their fathers' labor
market characteristics. We show that selection can be dramatically reduced by using 1940
controls. Controlling for these characteristics, we find modest positive impacts in 1950 of WWII
service and the GI Bill on educational attainment of those with the least pre-war education, and
on the school attendance of those with the most pre-war education, with no effect evident for
college completion. These effects are relatively large for black men. We find mixed effects on
labor market outcomes: young veterans enjoyed slight gains in income and occupational status;
older veterans did not. We do not find systematic racial or ethnic differences in labor market
impacts. These findings are important given the continued salience of the GI Bill and its
potentially disparate outcomes in political discourse.

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# 1. Introduction

The Servicemen's Readjustment Act of 1944 established a multifaceted set of programs to compensate US veterans for their military service and, it was hoped, to ease the economy's absorption of millions of demobilized personnel at the war's conclusion. The Act, commonly referred to as the *GI Bill*, established veterans' eligibility for unemployment benefits, tuition and stipend support for education and vocational training, and loan guaranties for the purchase of housing or inputs for farming or businesses. Approximately 55 percent of men born in the United States between 1910 and 1927, and 73 percent of those born between 1922 and 1927, served in World War II (WWII), and most veterans used one or more elements of the GI Bill in their postwar years.[1] Veterans, popular commentators, and some academic researchers have lauded the GI Bill as a transformative piece of legislation with overwhelmingly positive effects for the veterans and for society more broadly. Yet it has also been argued that the GI Bill's effects were uneven, subject to discrimination, benefitted some groups more than others, and perhaps exacerbated some dimensions of American inequality while reducing others.

In this paper, we examine the effects of World War II service and the GI Bill on male veterans by exploiting a new dataset of linked census records that enables the observation of the same men in both 1940 and 1950.[2] In addition to baseline measures of the war's influence on the trajectory of young men's lives, we are particularly interested in three dimensions of heterogeneity. First, we provide new views of veterans who were the children of European immigrants, yielding insights about their mid-century upward mobility and the role of wartime service in such mobility. Second, we provide new views of black veterans' outcomes, building on the insights of Turner and Bound's (2003) study of black-white differences in college education (see also Yamashita 2008). Third, we distinguish between younger and older veterans and between those who had lower and higher levels of education before the war—these men were differently positioned to benefit (or not) from the GI Bill. In each case, we focus on educational and labor market outcomes. The digitized 1950 census of population microdata do not include information on home ownership, which has been the subject of prior research (Fetter 2013; Althoff and Szerman 2022).

Several measurement issues are prominent in the economics literature on World War II veterans and the GI Bill, and these influence our approach to the analysis and interpretation. First, selection into military service was not random, which complicates the interpretation of differences between veterans and nonveterans in postwar datasets. The new dataset of linked census records enables largescale direct comparisons of the *pre-war* characteristics of men who later served in World

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[1] The veteran share is calculated with the complete 1950 census, limited to US-born men. Data on benefits utilization are from the President's Commission on Veterans' Pensions, Staff Report No. IX, Part A (1956, p. 48).
[2] Approximately 350,000 women served in the US military during World War II. Their subsequent outcomes and wartime experiences are beyond this paper's scope, but merit closer study and the size of the 1950 census might be helpful in that regard. See Mettler (2005, ch. 9) and Altschuler and Blumin (2009, ch. 5).

War II and those who did not.[3] This clarifies and enables us to address patterns of selection into military service, the potential role of selection in driving differences in educational and labor market outcomes observed after the war, and the extent of differences in selection across ethnic, racial, and age groups. Second, the effect of military service cannot be easily separated from the effect of the GI Bill because the former was a pre-requisite for the latter, and there was not random assignment of benefits within the population of WWII veterans; moreover, standard datasets include little more than a WWII veteran-status dummy variable.[4] Therefore, measures of treatment effects in this setting are usually "net" in nature, combining potentially differently signed effects from military service and post-service benefit programs. Third, the timing of the Korean War (1950-1953) complicates the interpretation of the "World War II service effect" in census data from 1960 onward because men who did not serve in WWII were often drafted to serve in Korea. However, due to the small size of the original 1950 public use sample (only 1 percent of the full census that is now available), prior scholarship has focused on cross sections of census data from 1960 or later. Our emphasis on the newly released 1950 census data avoids this issue.[5] Of course, in 1950 we observe veterans when they were still young (ages 23 to 40). It is possible that some effects materialized later in the lifecycle and that others diminished over time. We discuss each of these issues in more detail later in the paper.[6]

Within the sample of linked records, we define four groups of US-born men for comparison: white men and black men whose fathers who were born in the US (abbreviated *US-W* and *B*, respectively), white men whose fathers were born in southern or eastern Europe (*SE*), and white men whose fathers were born in northern or western Europe (*NW*).[7] Within these groups, we also define cohorts of older and younger men, corresponding to the 1910-21 birth cohorts and 1922-27 birth cohorts, respectively. The older men were typically in the labor market in 1940, providing valuable pre-war indicators of their educational and economic status. The younger men were typically living with their parents in 1940, providing valuable intergenerational background information.

First, we show that military service rates were similar across groups of white men, but lower for black men. The black-white gap in rates of military service during WWII is well known (Turner and Bound 2003), but the comparative information for second-generation immigrants is novel.

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[3] The Selective Training and Service Act was signed into law in September of 1940, whereas the 1940 census was taken in April.

[4] In principle, it might be possible to exploit variation within the population of veterans to learn more. For instance, detailed information on veterans' wartime experiences, linked from military records or postwar surveys of veterans, could differentiate the "military service" component of treatment. Stanley (2003) uses a cutoff for benefit eligibility in 1955 to compare Korean War veterans to later veterans.

[5] The census was taken on April 1, 1950. US involvement in the Korean War began in June 1950.

[6] A fourth measurement issue is that the GI Bill may have had general equilibrium effects, which would complicate interpretations and require the development of a macro-level "no-GI Bill" counterfactual. This is beyond this paper's scope but merits consideration in future research.

[7] The 1920s immigration restrictions and 1930s Great Depression resulted in small numbers of immigrants of military service age circa 1940. But there were large numbers of immigrants' children, on whom we focus here. The linked census record samples are not large enough to yield precise results for Asian, Latin American, and Native American veterans.

Differences across groups in aggregate rates of military service are important to consider because military service determined eligibility for GI Bill benefits. It is immediately apparent that black men, on average, were less likely to have been eligible for WWII GI Bill benefits than white men; on the other hand, they were more likely to have experienced the benefits of a booming wartime labor market, which spurred migration and occupational gains (Maloney 1994; Margo 1995; Collins 2000, 2001; Aizer et al. 2020).

Next, we find some evidence of positive selection into military service based on pre-war education, and neutral or negative selection on the basis of an individual's own labor market observables or those of his father. We do not find systematic differences in the degree of selection across the various racial and ethnic groups that we study. That WWII veterans tended to be positively selected on certain dimensions is expected given what is known about the screening of candidates (US Selective Service 1950; Goldstein 1951). But this analysis provides a clearer view and more precise measurement of selection patterns than previously possible. It is important that the 1940 veteran-nonveteran gaps in key education and labor market variables, as well as the differences in these gaps across groups, can be sharply reduced by conditioning on other observables, pushing the difference to zero in some cases. While we acknowledge that selection on unobservables cannot be definitively ruled out, the findings suggest that richly detailed panel data may greatly reduce or even eliminate selection bias in the analysis of postwar outcomes. We harness this aspect of the panel data to calculate differences in 1950 outcomes for veterans and nonveterans that condition on individuals' pre-war characteristics.

For educational outcomes in 1950, we find that veterans had higher levels of educational attainment, measured by years of education, than nonveterans in every group even when conditioning on a detailed set of pre-war observables and location fixed effects. The relative advantage tended to be larger for black veterans, younger veterans, and veterans who had (or whose fathers had) less than 9 years of education in 1940. We find few differences of note between the *US-W*, *SE*, and *NW* groups in terms of the veteran-nonveteran gap in years of education, conditional on pre-war observables. Younger men were more likely than older men to take advantage of the GI Bill's support for educational programs. This is reflected in relatively large differences in 1950 school attendance between young veterans and nonveterans compared to older cohorts, particularly for black men, and particularly for those who had (or whose fathers had) 12 or more years of education in 1950. We find little impact on college completion, perhaps because it would be difficult to start college after the war and finish college before the 1950 census. Overall, these findings are consistent with veterans benefiting educationally from military service and the GI Bill, though this benefit may have come through education and training below the college level. The results are less consistent with the notion that veterans from less advantaged socioeconomic groups benefited less than others.

For 1950 labor market outcomes, conditional on 1940 observables, we find no strong evidence that older veterans fared substantially better than nonveterans in terms of employment, income, or occupational status, suggesting that lost years of civilian labor market experience offset benefits from the GI Bill. Among younger men, however, we find that veterans fared better than

nonveterans in both earned income and occupational status.  For all groups, we find positive veteran-nonveteran differences in the likelihood of government employment in 1950, although in different subsectors of government depending on race.  On the other hand, for all groups, we find that veterans were less likely than their peers to be self-employed, which again may reflect veterans' lost years of civilian labor market experience and missed opportunities for earning, saving, and entrepreneurship. Overall, these mixed results remind us that the GI Bill was not meant to give veterans a leg-up on other workers in the labor market; rather, it was meant to compensate veterans for the interruption of their educational and labor market experiences and to facilitate their transition into the civilian labor force.  For older veterans, these GI Bill benefits may have been just enough to keep them on par with their peers by most metrics, whereas for younger veterans, the combination of service and the GI Bill appears to have been more positive.

This paper contributes to several different economics literatures.  First, assessments of World War II military service and the GI Bill may speak to the potential role of government programs in bolstering economic mobility, human capital, civic engagement, and middle-class wealth.[8]  Indeed, the GI Bill is often cited as evidence that such programs can be successful and broadly beneficial. Widespread beliefs about military service and the GI Bill inform both policymakers' and voters' thinking about ways in which the federal government can intervene to promote a more equitable society.  As we suggest above and explore in greater detail below, the effects are likely to have been complicated and context dependent—different veterans may have benefited from different aspects of the program and at different times in their lives, if they benefited at all.  And in all cases, the measurement issues are challenging.  New microdata sources can help scholars clarify WWII's legacy and lessons.  In this paper's case, the recent release of the completely transcribed 1950 census records enables us to examine potentially heterogenous effects from a variety of perspectives several years after veterans returned to civilian life.  We hope and expect that much more research will follow from this valuable new resource.

The paper also adds to the economics literature on immigrants' economic assimilation (e.g., Borjas 1985; Card 2005).  Although the Age of Mass Migration had come to an end in the 1920s, the process of assimilating immigrants and their children was still underway (Collins and Zimran 2019, 2023; Abramitzky et al. 2020, 2021b).  It has been speculated that military service in World War II was an important catalyst for the acceptance of communities of immigrants from southern and eastern Europe into broader American society, breaking from previous discrimination based largely on religion (Gerstle 2001; Bruscino 2010).  To our knowledge, however, no quantitative analysis exists of the role of military service and the GI Bill in this process.  The sheer size of the complete count census records makes it possible to examine separately differential outcomes for men whose parents immigrated from northern or western Europe or from southern or eastern Europe in comparison to men whose parents were born in the United States.

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[8] A related literature centers on whether military service—or different experience within the military—has implications for later life outcomes (see MacLean and Elder 2007).

In a similar way, the paper contributes to literatures concerned with disparities in educational and economic outcomes across groups categorized by race. Rigid segregation within the military and in many areas of American life ensured that black men's experiences during World War II and in the postwar economy were materially different from those of white men.[9] Although the Veterans' Administration (VA) instructed its staff not to discriminate, the VA did not challenge segregation in the South where most black veterans lived. Black veterans often found that "VA administrators and local officials were indifferent or hostile to them" (Altschuler and Blumin 2009, p. 132). Recognition that the GI Bill, despite its "race-neutral" legislative language, may have exacerbated racial inequality in college education is a major qualification to the idea that the policy enhanced economic mobility (Turner and Bound 2003; Katznelson 2005; Katznelson and Mettler 2008; Lawrence 2022). Much of the quantitative evidence to date is centered on college education. More and broader quantitative research in this area is clearly merited. We contribute by studying other aspects of educational attainment and broadening the scope of analysis to a variety of labor market outcomes.

## 2. Background

### 2.1 World War II and the GI Bill: Context and Program Design

Figure 1 presents service rates by racial or ethnic group and birth cohort, as well as differences in these rates relative to the *US-W* group. Panel (a) shows that the peak rates for World War II military service are found in the early to mid-1920s birth cohorts (Ruggles et al. 2024a, b).[10] Among white men with European- or US-born fathers, service rates peaked at nearly 80 percent. Panel (b) shows that there were quite small differences across groups of white men with European- or US-born fathers.[11] In contrast, black men with US-born fathers had service rates that were about 20 percentage points lower than white men. These patterns had important implications for the share of men in each group who experienced military service and were subsequently eligible for GI Bill benefits.[12]

These service rates reflect a combination of voluntary enlistment and conscription. The Selective Training and Service Act of 1940 laid the administrative groundwork for the military mobilization of young men by requiring their registration with local draft boards. In the wake of the

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[9] There is a large literature in this area. See, inter alia, Bolté and Harris 1947; Dalfiume 1969; Onkst 1998; Collins 2000; Turner and Bound 2003; Katznelson 2005; Katznelson and Mettler 2008; Guglielmo 2021; and Eden 2022.

[10] These postwar rates of service pertain to those who survived the war and until 1950. More than 400,000 Americans did not survive. Their sacrifice is not registered in the kind of analyses we undertake below, but it is, of course, important to acknowledge in any consideration of World War II's effects on the population.

[11] These differences are even smaller when conditioning on 1940 characteristics.

[12] The documentation of the veteran status variables in the 1950 census provided by Ruggles et al. (2024a, b) indicate that there may have been issues of inconsistent and under-reporting. Nonetheless, the share of individuals who were World War II veterans in the same birth cohorts in the 5-percent sample of the 1960 samples is very similar, and nearly identical for the peak-service cohorts.

Pearl Harbor attack on December 7, 1941, many volunteered for military service, but approximately 61 percent of WWII servicemen were conscripted (National World War II Museum, undated). Many registrants were rejected for military service, most commonly for mental illness, low levels of education or literacy, "manifestly disqualifying defects," musculoskeletal issues, cardiovascular issues, or hernia (Goldstein 1951, pp. 595-596).[13] But these standards varied over the course of the war, and some men who were initially rejected were reclassified and inducted later. Others received occupational deferments, typically older men working in war industries or agriculture (US Selective Service System 1950, pp. 259-271, 289-293).

The US military was strictly segregated on black-nonblack racial lines throughout the war, with implications for how the draft was implemented, the branches in which black men were permitted to serve, and black men's assignment to units and activities within those branches. The slow and reluctant opening of military service opportunities for black men was a prominent political issue throughout the war (Dalfiume 1969; Flynn 1984; Guglielmo 2021). Black men were also rejected from service for medical and educational deficiencies at relatively high rates (Goldstein 1951; Murray 1971). In combination, these factors delayed and depressed black men's military participation and, conditional on serving, profoundly shaped their experiences. Nonetheless, Weaver (1945) and Schiffman (1949) argue that some black servicemen received valuable training. After the war, discrimination on many fronts narrowed black veterans' scope for educational and economic advancement (Turner and Bound 2003; Katznelson and Mettler 2008).

Long before the war ended, policymakers and lobbyists began proposing legislation to assist demobilized veterans' reintegration into the labor force, including the idea for limited support for higher education (Ross 1969; Mettler 2005; Frydl 2009).[14] Approximately 16 million Americans served in the military during World War II. Re-absorbing them into civilian life, without causing mass unemployment and civil unrest, was a priority. The American Legion, an organization formed by veterans of World War I, expanded upon earlier proposals for supporting veterans. The Legion marshalled legislative and public relations resources to advance the "GI Bill of Rights" in early 1944. President Franklin Roosevelt signed the bill into law on June 22, 1944, just two weeks after the D-Day invasion of Normandy.[15] The bill was not designed explicitly with the goal of increasing social mobility; rather, the Legion spoke in terms of what the nation owed veterans for their service and how the bill would enable them to resume productive civilian lives after the war's disruption (Altschuler and Blumin 2009, p. 54). The legislation was crafted to avoid extending the influence of federal agencies over state and local governments, thereby protecting entrenched segregation and discrimination. It was also designed to avoid the extension of comparable benefits to nonveterans.

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[13] This list refers to registrants through August 1, 1945. "Manifestly disqualifying defects" would include blindness, deafness, missing arms or legs, and "chronic or severe physical or mental disorders" (Goldstein 1951, p. 595).

[14] For instance, in a "fireside chat" in July 1943, President Roosevelt suggested that Congress should enact laws providing veterans with "mustering out" pay, educational assistance, unemployment insurance, and medical care and pensions for the disabled (Altschuler and Blumin 2009, p. 46).

[15] Congress made revisions in 1945, mostly increasing generosity (Altschuler and Blumin 2009, p. 82).

The GI Bill delineated several different types of benefits. Title II focused on education and training. Active-duty veterans who served at least 90 days and had not been dishonorably discharged were eligible for one year of tuition, fees, and stipend support with additional funding according to their length of service, up to four years in total. This funding could be used in any approved training program or educational institution, and it was generous enough to cover tuition at leading private universities. Title III focused on loan guaranties for veterans' purchase of homes, farms, and business property, covering up to 50 percent of the value of the loan.[16] Title IV required the US Employment Service to assign "veterans' employment representatives" to each state to facilitate the placement of veterans into civilian jobs. Title V defined "readjustment allowances" that provided income support to unemployed veterans for up to 52 weeks, depending on length of service, to facilitate their search for employment.[17] Receipt of one type of benefit did not exclude a veteran from receiving other benefits, and in practice many veterans availed themselves of more than one type of benefit.

The President's Commission on Veteran Pensions (1956) provides an aggregate perspective on veterans' engagement with the GI Bill programs. We reproduce charts from the Commission's report in Figure 2 because we do not have access to the administrative microdata that must have underpinned the report. In 1946, mustering-out pay and readjustment allowances dominated expenditures on veterans, but by 1947 and throughout the late 1940s and 1950, expenditures on education and training were the largest category (panel a). Readjustment allowances were the earliest and most widely used GI Bill benefit: 58 percent of WWII veterans received them (panel b). Schooling and training followed at a lower level and slightly later timing, peaking at around 51 percent of veterans. Home loan guaranties took off more gradually, reaching only 28 percent of WWII veterans by 1955. Within the education and training category, nearly 900,000 veterans were using college-level benefits in 1948, the peak year (panel c). But even more veterans pursued other kinds of training, a combination of "below college level" educational training, job training, and farm training. The Commission's report also shows that younger veterans, under age 25 at the time of discharge, were the primary beneficiaries of education and training programs and readjustment (unemployment) benefits (p. 82).[18] This is useful to keep in mind because identification strategies that emphasize treatment effects for the youngest cohorts of World War II veterans are centered on the group that most intensively utilized educational benefits; older veterans were less likely to pursue these opportunities.

A key point is that, as intended and designed, the GI Bill had several different components, which may have assisted veterans with different characteristics in different ways and at different

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[16] The maximum value for a loan was originally set at $2,000 but this was revised upward to $4,000 in 1945. See Fetter (2013) for detailed discussion and analysis of the VA's home loan guaranty program.

[17] Each month of service implied four weeks of unemployment benefits, up to 52 weeks and paying $20 per week.

[18] Sixty percent of those under 25 used education or training benefits by 1955, compared to 34 percent of those 30-34 and 25 percent of those 35 plus (US Presidents' Commission on Veteran Pensions 1956, p. 82). Differences in the use of loan guaranties were less pronounced. The median age for World War II veterans at time of discharge was 27.6 years (p. 104).

times in the postwar years. The prominence of the GI Bill's legacy in terms of college education is understandable, but it also overshadows a large swath of what the bill offered veterans and what they used it for, even within the category of "education and training." Again, the 1979 Survey of Veterans helps to fill in the story.[19] Overall, a higher share of black WWII veterans reported receiving VA support for education or training after the war than did white veterans. This relatively high rate of benefit usage is also remarked upon in the President's Commission report on veterans' benefits (1956, p. 72). Among WWII veterans who reported receiving VA support for post-service education or training, 44 percent of white men and 22 percent of black men pursued college-level education, implying that most men in both groups did something else with the resources. For black men, "other" schooling (42 percent), high school (11), and apprenticeships (10) were common; for white men, "other" schooling (21), on-the-job training (11), and farm training (8) were common. Among those who had ever bought a home (by 1979), 44 percent of white and 43 percent of black veterans used a VA home loan.[20]

*2.2 Research in Economics*

The literature on World War II service and the GI Bill is large and multidisciplinary, and therefore our view is shaped by a variety of sources and methodologies.[21] In this section, we briefly describe some of the key research findings in the economics literature, and then we highlight the ways in which our paper adds new perspectives by harnessing newly available data. The most relevant and recent economics literature tends to partition into a few different areas of study: veterans' educational outcomes (e.g., Bound and Turner 2002; Turner and Bound 2003; Stanley 2003; Thomas 2017), their labor market outcomes (e.g., Angrist and Krueger 1994; Collins 2000; Thomas 2017), and their housing market outcomes, especially home ownership (Fetter 2013).

Given the popular emphasis on the GI Bill's support for college education and longstanding policy interest in how public subsidies affect college enrollment, it is not surprising that this aspect of the GI Bill has received a great deal of attention in research.[22] After comparing within-cohort differences between veterans and nonveterans, arguably an upper bound on treatment effects, Bound

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[19] This is a valuable but small sample. Our tabulation of survey microdata includes 328 black veterans and 4468 white veterans of World War II. It builds on two survey questions: "After your last Armed Forces active duty service, did you attend a high school, college, vocational, technical, or business school; or take any correspondence, on-the-job, farm, or apprenticeship training?" and "Did you receive any of this schooling or training under the GI Bill or VA Rehabilitation Program…?" Those who answered "yes" to both questions are coded 1, and those who answered no to either question are coded as 0 in the tabulation.

[20] The survey question asked of those who had at some point owned a home was: "Have you ever had a VA home loan…?" Our tabulation used the survey weights. There were substantial differences in "ever bought a home" between black and white WWII veterans: 89 percent of white veterans had bought a home by the time of the survey compared to 66 percent of black veterans.

[21] For broad historical perspectives on the GI Bill see, *inter alia*, Olson (1974), Mettler (2005), Katznelson and Mettler (2008), Frydl (2009), and Altschuler and Blumin (2009). Sociological perspectives include Nam (1964) and Sampson and Laub (1996), and Teachman and Tedrow (2004).

[22] One theme in the historical literature that we do not attempt to address here is that institutions of higher education were transformed by the wave of veterans who enrolled.

and Turner (2002) and Turner and Bound (2003) shift their focus to differences in average outcomes across cohorts with different rates of military service. They conclude that the effect of World War II service and GI Bill eligibility on college attendance and completion was positive and sizable for white men (e.g., 5 to 6 percentage points in college completion observed in 1970's census). This result is similar in magnitude to within-cohort veteran-nonveteran differences when the sample is limited to those with at least 12 years of education (2002, p. 798). Turner and Bound (2003) pay particular attention to black-white differences, and concludes that positive effects accrued to white men throughout the US and to black men outside the South, but not to black men in the South. To date, this is the key economics paper on the differential effects of World War II service and the GI Bill across racial categories.

Stanley (2003), relying primarily on evidence from the Occupational Changes in a Generation datasets from 1962 and 1973, addresses the WWII GI Bill's impact with a combination of insights from within-cohort comparisons and across-cohort comparisons, the latter of which assumes that the GI Bill had small (if any) effects on college education for men from the earliest 1920s birth cohorts.[23] The findings suggest a positive effect for World War II veterans born in the mid 1920s, increasing college completion rates by between 4 and 7 percentage points (Table VIII, p. 697). These gains appear to have accrued mostly to those from above-median socioeconomic backgrounds. Stanley's overall assessment is that the GI Bill's effect was "evolutionary rather than revolutionary" (p. 673).

Angrist and Krueger (1994) focus on the WWII veterans' earnings premium. By 1980, it was clear that WWII veterans earned more than similarly aged nonveterans, but it was unclear whether the gap was causally related to military service and the GI Bill. They instrument for veteran status with quarter of birth and assume that labor market outcomes were uncorrelated with quarter of birth otherwise. On this basis, they conclude that the positive veteran earnings premium in 1980 was entirely attributable to selection; in fact, their baseline 2SLS estimates for 1980 are negative (p. 83).[24] Thomas (2017), on the other hand, employs a different identification strategy (closer to Bound and Turner's approach) and finds positive effects on employment and being above the poverty line in 1970's census data.

Fetter (2013) uses census data from 1960, 1970, and 1980 to study the effect of veterans' home loan guaranties on levels and timing of homeownership. He addresses selection into military service by developing a fuzzy regression discontinuity design centered on men who had just turned 18 or were about to turn 18 at the war's end (i.e., making cross-cohort comparisons). He finds that access to home loan benefits under these GI Bills had an impact on homeownership in 1960 but not

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[23] Stanley's paper focuses primarily on results from an identification strategy that estimates the GI Bill's effects on Korean War veterans relative to later veterans. The OCG data have some pre-war information on men and their fathers, a rare and valuable feature for data from this era. The OCG datasets are small compared to the linked census data we use here, have fewer pre-war variables, and rely on retrospection over a long period.

[24] Lemieux and Card (2001) study Canadian veterans, who were eligible for various postwar benefits but not subject to Korean War service. Their estimates of the effect on college attainment (p. 335) are qualitatively similar to those of Bound and Turner (2002)

later, indicating that the benefits accelerated home purchases for veterans, but did not, on average, induce individuals who would not otherwise have purchased a home to do so.

Our analyses are similar in spirit to those described above. However, we are particularly interested in understanding how the effects of military service and the GI Bill may have varied across groups and depended upon their pre-war characteristics. Investigating this heterogeneity requires large datasets. Fortunately, the complete count censuses from 1940 and 1950 have become available since the literature described above was written, which enables scholars to link large numbers of individuals over time to see their pre-war and postwar situations in rich detail. The linkage, in turn, opens a new pathway for studying the effects of military service. Prior to the release of the full-count 1950 census, it was not possible to observe an individual's pre- and postwar characteristics together with their military service status in large datasets. Instead, the main approach to address selection into service has exploited cross sectional data and the substantial decline in the probability of serving in World War II between cohorts that were just old enough to serve and those that were just too young, using quarter of birth.[25] This approach has led to a focus on younger cohorts, and local average treatment effects might hinge on the youngest men, whose wartime experiences were likely brief and whose pre- and postwar situations were quite different from those of most veterans. For instance, we know from administrative data reports that the extent and the nature of young veterans' uptake of GI Bill benefits was different from that of older cohorts, probably because older men were more likely to have gained labor market experience, completed their education, or started families before entering the military. By addressing selection into military service through the observation of detailed pre-service characteristics, we can analyze both the older and the younger cohorts and thereby broaden the literature's view of the correlates and effects of World War II service.

## 3. Data and Empirical Strategies

### 3.1 Data Sources, Record Linkage, and Outcomes of Interest

Our analysis is based on individual-level census records that are linked between the complete-count census datasets of 1940 and 1950 (Helgertz et al. 2024; Ruggles et al. 2024a, b). This combination of sources enables us to observe approximately 1.7 million men's educational and labor market outcomes after WWII, as well as detailed information about their own or their father's socioeconomic situation before the war. This paper's ability to compare veterans and nonveterans before and after the war in large samples is novel to the literature, providing a clearer view of selection into military service and a new basis for studying the impacts of military service on later outcomes.[26]

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[25] As reviewed above, Stanley (2003) is an exception because it relies on the OCG datasets, which include some retrospective information but are comparatively small.

[26] As the linkage crosswalks between 1940 and 1950 are provided and described by Helgertz et al. (2024), we do not delve into the details of the linkage here. As with all cases of linked data, we must address issues of selection into linkage and of false matches. To correct for selection into linkage on the basis of observable characteristics, we

Among the individuals with linked census records, our focus is on US-born men who were part of the 20-percent "sample-line" subset of the 1950 census. The 1950 census collected information on veteran status, education, parents' places of birth, and income, all of which are crucial to our analysis, only for 1-in-5 individuals. Until now, it was difficult to use the 1950 census microdata to study veterans because the original microdata sample was small (1 percent), leaving only a 0.2 percent sample with the sample-line information. Research to date has addressed this issue by moving to later censuses, especially the 1-percent public use sample of the 1960 census or the (combined) 3-percent sample of the 1970 census. But doing so entailed a tradeoff: the later census samples are larger than the original 1950 sample, but a key comparison group is contaminated. Specifically, by the time of the 1960 census, many nonveterans of World War II had served in the Korean War and, therefore, were eligible for the Korean War GI Bill benefits.[27] Bound and Turner, who rely on 1970's cross-sectional data, raise this issue explicitly: "the analysis is framed in terms of measuring the effects of World War II service relative to a control group, which is assumed to have had no military service and no GI benefits. *If researchers could rewind the clock or measure educational attainment at the start of 1950, this would certainly be true.* However, the hostilities in Korea may have had a marked effect on the presumed control group" (2002, pp. 791-792, emphasis ours). Our analysis, by focusing on the new 1950 census dataset, proposes to "rewind the clock" precisely as Bound and Turner suggest.

We analyze two separate linked datasets. The first covers the 1922-1927 birth cohorts, members of which would have been ages 13-18 in 1940. These cohorts had the greatest exposure to World War II service, and much of the prior economics literature has focused on them. In 1940, we observe their school attendance and educational attainment to that point (albeit potentially incomplete), and *their fathers'* labor market outcomes and educational attainment.[28] The second linked dataset covers the 1910-1921 birth cohorts, who would have been ages 19-30 in 1940. These men served at lower rates than younger men, but their service rates were high enough that, on aggregate, most WWII veterans were from these relatively understudied cohorts. In 1940, we observe *their own* educational and labor market outcomes—a particularly valuable set of individual-specific pre-war control variables. For all men, we observe whether their 1940 home was owned or rented, whether they resided on a farm, urban or rural status, population size of place of residence,

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create weights, as is standard in the literature; we match the observable characteristics of the linked sample to the distribution of observables in 1950. We test the sensitivity of the main results to potential false matches, which would likely cause us to understate selection and veterans' premia by introducing measurement error into the veteran status indicator, by tightening the linkage criteria to reduce the likelihood of false matches (Abramitzky et al. 2021a). In particular, we restrict the sample to men whose records match exactly on 1940 and 1950 race, state of birth, and age-implied birthyear. The results, shown in Online Appendix B, are qualitatively the same as the main results. This restriction entails eliminating 22.7-27.8 percent of white men in the young cohorts from the sample, 48.2 percent of black men in the young cohorts, 27.6-33.2 percent of white men in the old cohorts, and 56.2 percent of black men in the old cohorts.

[27] See Stanley (2003) for a detailed description of the Korean War GI Bill. In the 5-percent sample of the 1960 census, about 13 percent of men in the 1922-1927 birth cohorts who did not serve in World War II served in Korea.

[28] Men in the 1922-1927 birth cohorts with missing information on their father's status are dropped from the analysis.

and county of residence.  In total, the linked datasets we analyze consist of 570,907 men from the 1922-1927 cohorts and 1,127,520 men from the 1910-1921 cohorts.

Within each dataset, we divide individuals into four main ethnic or racial groups, presented with sample sizes in Table 1.  The goal is to provide a wide perspective on veterans from different backgrounds while maintaining sufficient sample size for statistical analysis.  We readily acknowledge that finer gradations and an even wider scope would be valuable, and we encourage scholars to dig deeper in this regard.[29]  The *US-W* group—white men with US-born fathers—is the largest, with approximately 375 thousand men for the younger cohorts and 754 thousand for the older cohorts.  The *SE* group—white men with fathers born in southern or eastern Europe—is the next largest, with about 65 and 161 thousand members, respectively.  For black men with US-born fathers (the *B* group), the sample includes about 31 thousand men in the younger cohorts and 60 thousand in the older cohorts.  Finally, there are about 15 thousand sons of northern and western European immigrants (the *NW* group) among the younger cohorts and 47 thousand among the older cohorts.  Our division of second-generation immigrants into the *SE* and *NW* groups reflects the two main waves of immigration during the Age of Mass Migration (Abramitzky and Boustan 2017).

Although the complete-count 1950 census data are an especially valuable resource for researchers, the preliminary version of the dataset that is currently available to researchers is not without limitations and shortcomings.  Ruggles et al. (2024a, b) describe issues in key income and military service variables, and our examination of the distribution of education variables indicates additional issues (e.g., for many sample-line individuals, the *higrade* variable is not available or recorded as "none").[30]  To reduce the potential influence of these issues on our results, we limit attention to men for whom income and education are recorded as being greater than zero and for whom the veteran status variable is not missing.  Many of these data-quality concerns pertain to our outcome variables; therefore, as long as there are no systematic differences in misreporting by veteran status, our estimates will not be affected.

The main 1950 outcomes on which we focus are years of education, school attendance, and college completion (16+ years of education), and then employment, earned income, occupational status, self-employment, and government employment.  Much prior research has focused on college education.  We supplement this with school attendance and years of education for two reasons.  First, 1950's temporal proximity to World War II might mean that individuals attending college have not yet graduated.  Second, many men used educational benefits for purposes other than college (as shown in Figure 2c and revealed in the 1979 Survey of Veterans).  Having a net wider than college education will help capture an underexplored aspect of WWII veterans' outcomes.  Regarding the labor market variables, although 1950 is early in the lifecycle for many of the men in our sample, it is

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[29] We attempted to analyze Native American men and men of Latin American and Asian descent.  Unfortunately, the linked dataset includes relatively small numbers in each group and yields noisy statistical results.  That said, we do think that careful and detailed analysis of these groups is a promising route for future research.

[30] We thank Brian Beach for bringing this to our attention. Between 5 and 6 percent of both white and black men between the ages of 20 and 40 have *higrade* listed as "none," which is implausible and inconsistent with both 1940 and 1960 data (and the original 1950 1-percent sample). See Collins and Margo (2006) for perspective.

still of interest to learn whether veterans gained an advantage relative to nonveterans in the labor market. The 1950 outcomes speak directly to the speed and nature of the economy's reconversion, its re-absorption of veterans, and their ability to make headway in the labor market. In addition to measuring employment rates and earned income, we examine self-employment and government-sector employment, which are less commonly studied outcomes. We examine self-employment because the GI Bill provided business and farm loans, and some training programs might have encouraged veterans' self-employment (e.g., apprenticeships and vocational programs). Our analysis of government employment is motivated by the potential for military training or preferences for veterans in hiring to lead veterans disproportionately into public sector jobs.[31] Summary statistics for the 1940 and 1950 variables that we observe are presented in Online Appendix Tables A.1 and A.2.

### 3.2 Empirical Strategies

If men had been randomly selected for military service, then measurement of the effects of military service and the GI Bill would be straightforward. In the presence of selection, whether due to self-selection of volunteers or the process of conscription, measurement is more difficult and prone to bias. Before World War II, men who chose to join the military tended to be negatively selected relative to their peers (Zimran 2019; Linn 2023); that is, their observable human capital tended to be low on average. During World War II, however, there are reasons to believe that this was not the case (Bound and Turner 2002), at least for those who were conscripted. Yet the picture is less clear than one might expect for such a widely studied event, as it requires detailed information on both nonveterans and veterans *before* the veterans' service. As discussed above, some men were rejected for educational or health deficiencies deemed to be incompatible with military service, though these standards varied over the course of the war. On the other hand, some were granted occupational deferrals or declined to enlist voluntarily though eligible to do so, particularly among older cohorts. The main empirical challenge of this paper is to measure veterans' average gains or losses in the presence of these selection issues. Linked census records can help by clarifying the extent of selection and, later in the paper, providing direct and detailed controls for individuals' pre-war characteristics.[32]

We begin by using data from 1940 to characterize differences between men who later became WWII veterans and those who did not. We estimate the equation:

$$x_{ijc}^{1940} = \alpha_j v_{ijc}^{1950} + \gamma_{jc}^{1940} + Z_{ijc}^{1940} \delta_j^{1940} + \varepsilon_{ijc}, \qquad (1)$$

where $x_{ijc}^{1940}$ is a pre-war educational or labor market characteristic of individual $i$ of ethnic or racial group $j$ born in cohort $c$, or of his father; $v_{ijc}^{1950}$ is an indicator for whether the individual was a veteran in the 1950 census; $\gamma_{jc}^{1940}$ are ethnic or racial group-by-birth cohort fixed effects; and $Z_{ijc}^{1940}$ is a set of controls from 1940 that varies depending on the outcome variable and whether we are

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[31] Men who were still in the military in 1950 are excluded from this analysis.
[32] Some of this approach is similar to that taken in Collins and Wanamaker (2014).

analyzing data for younger or older cohorts.  All coefficients are permitted to vary by racial or ethnic group.

The coefficients of interest are in the $\alpha_j$ vector, which measure the average differences in $x$ between veterans and nonveterans in each ethnic and racial group.  In the simplest regression, no controls are included apart from $\gamma_{jc}^{1940}$, yielding simple measures of *unconditional selection*.  A positive value of $\alpha_j$ indicates positive selection; that is, that future veterans had stronger socioeconomic outcomes than nonveterans *before* the war.  In subsequent specifications we add birthplace-by-group fixed effects, 1940 county-of-residence-by-group fixed effects, and $Z_{ijc}^{1940}$ controls, which include 1940 characteristics other than $x$ itself.[33]  This provides a measure of *conditional selection*.  For instance, for men in the labor force in 1940, we can see whether those who later entered military service were earning more than men who did not, conditional on educational attainment, location, and more.  Or, for younger men, we can see whether 1940 school attendance rates differed, conditional on their fathers' observables, location, and so on.  The conditional selection regressions demonstrate that tightening the comparisons of veterans to observationally similar nonveterans narrows the scope for selection bias.

We are also interested in the differences in $\alpha_j$ estimates *across* groups.  If one group's $\alpha_j$ is greater than another's, then that group had relatively strong positive selection into military service.  This is useful to know both when interpreting the nature of military service in WWII and how it cut across different subgroups of the population and when considering whether the degree of selection bias might differ substantially across groups in 1950-based analyses.

It is important to be clear about what the "veteran treatment" entailed in this setting and to acknowledge that an event as massive as World War II left no one unaffected.  For men who served in the military and survived, "treatment" entailed many things—lost civilian work experience, added military experience (which may have included occupational training), potential mental and physical injury from training or combat experience, GI Bill benefits upon discharge from the military, and any favorable post-military treatment apart from GI Bill (e.g., in hiring, promotion, or pay).  For similarly aged men who did not serve in the military, the wartime economy presented extraordinary labor market opportunities.  This may have enhanced their early-life workplace experience and accumulation of wealth, but it also may have curtailed their investment in formal education since the opportunity costs of additional schooling would have been high.  Given the program design and our data, a 1950 counterfactual in which veterans served in WWII but did not receive eligibility for the GI Bill is unattainable—this is important to recognize because it is this counterfactual that

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[33] For the selection regressions (equation 1), to avoid confounding the interpretation, we do not control for an individual's 1940 educational outcomes when the $x$ outcome is another 1940 educational outcome, or for an individual's labor market outcomes when the $x$ outcome is another labor market outcome. For instance, we do not control for 1940 occupation in regressions where 1940 income is the left-hand side variable. In regressions with 1950's data (described in equation 2), we include *all* 1940 observables, including the 1940 value of the 1950 outcome of interest, meaning that veteran-nonveteran comparisons are even tighter in those analyses.

policymakers must have contemplated before creating the GI Bill.[34] We also cannot assess a counterfactual in which WWII never happened at all. Instead, we aim to estimate how an average veteran might have fared circa 1950 if he had not served by relying on close comparisons with observationally similar nonveterans. Although this renders our results less informative about the general effects of programs that subsidize education, training, or extended unemployment benefits, there is value to understanding the process of postwar labor market adjustment and veterans' transition back into civilian lives, as their experiences shaped the US economy and have influenced policy debates to the present.

We estimate equation (2) using outcome data from 1950 and richly detailed background information from 1940:

$$y_{ijc}^{1950} = \beta_j v_{ijc}^{1950} + \gamma_{jc}^{1950} + Z_{ijc}^{1940} \delta_j^{1950} + u_{ijc}, \tag{2}$$

where $y_{ijc}^{1950}$ is the outcome of interest for individual $i$ of ethnic or racial group $j$ born in cohort $c$; $\gamma_{jc}^{1950}$ are ethnic or racial group-by-birth year fixed effects; and $Z_{ijc}^{1940}$ are controls from 1940, describing the individual, or for the younger cohorts, his father. Of particular importance is that, for the older cohorts, whom we observe as adults in 1940, we can include the 1940 level of the outcome variable as a control. Since individual-level productivity is likely to be reflected in their pre-war educational and labor market outcomes, these are valuable controls and a key strength of linked datasets that span the war. The coefficients $\beta_j$ represent the conditional within-birth cohort difference in the outcome between veterans and nonveterans of group $j$, which we will refer to as the "veterans' premium." As above, we are also interested in differences in the veterans' premia across groups.

A critical issue in interpreting our results is the extent to which estimates of $\beta_j$ can be interpreted as measuring the causal effect of military service and the GI Bill. For this to be true, one would have to be convinced that the average outcomes for veterans in 1950 would have been the same as those for nonveterans who were observationally similar in 1940, where "observationally similar" entails age, race and ethnicity, birthplace, location, education, and labor market observables—in some cases the 1940 value of the 1950 outcome itself. If some selection bias does remain in measures of $\beta_j$, it is possible that *differences* in $\beta_j$ across groups would still reveal differential effects of service if the residual selection bias were similar in the groups being compared. We address these issues of interpretation as we present evidence on both pre-war selection and postwar outcomes.

## 4. Results

### 4.1 Selection into Military Service

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[34] Their thinking was informed by the experience of World War I and the Great Depression.

Figures 3 and 4 present estimates of the coefficients $\alpha_j$ to gauge the degree of selection into military service. The coefficients can be interpreted as the difference in a given $x_{ijc}^{1940}$ variable between future veterans and nonveterans, with or without extensive control variables to tighten the comparison. Each panel includes three sets of estimates for each racial or ethnic group, corresponding to different specifications of equation (1). The first controls only for birthyear indicators; the second adds controls for birthplace and 1940 county-of-residence indicators; the last adds controls for various 1940 characteristics.[35] We dwell on these "selection into service" patterns because they are, to our knowledge, the first estimates that compare pre-war characteristics of WWII veterans and nonveterans based on largescale linked census datasets. The selection analysis is also a steppingstone to a better understanding of postwar differences in veterans and nonveterans outcomes and the role of military service and the GI Bill in driving those differences.

Figure 3 focuses on educational attainment in 1940, with panels (a) and (b) showing differences in school attendance and panels (c) and (d) showing differences in highest grade of schooling. When conditioning only on birthyear (blue dots), there is strong evidence of *unconditional* positive selection into military service. The coefficients are largest for black and white men with US-born fathers (*B* and *US-W* groups). For the younger cohorts (panel a), future veterans in the *B* and *US-W* groups were about 10 percentage points more likely to attend school than nonveterans, relative to a base rate of 62 percent for black men or 74 percent for white men (see summary statistics in Online Appendix Tables A.1 and A.2). For the older cohorts (panel b), the difference is about 1.5 to 2 percentage points relative to a base school attendance rate of 3.6 percent for black men and 6.9 percent for white men with US-born fathers. Panels (c) and (d) show qualitatively similar unconditional selection patterns. Black men's selection into service was clearly the most positive, with veterans having nearly 1.5 years more education than nonveterans. For the *US-W* group, the difference was about 0.8 years, and for the *NW* and *SE* groups the difference was about 0.4 to 0.5 years. These differences are large relative to the average 1940 levels of education of these groups, especially for black men.

*In all cases, adding controls for birthplace and county of residence (red diamonds) and other 1940 characteristics (green circles) greatly reduces the veteran-nonveteran gap in educational background*. That is, conditional selection is much smaller than unconditional selection, though still positive and statistically significant. It is also notable that differences across groups in the degree of selection, particularly in terms of years of education, are sharply reduced as controls are added, though the *NW* and *SE* groups' veteran coefficients tend to be smaller than those for the *B* and *US-W*

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[35] For the older cohorts, when *x* is an education variable, the control variables include log annual wage or salary income, an indicator for having no wage in 1940, occupational income score, an indicator for having an occupational income score of zero, indicators for white-collar and unskilled occupations, indicators farm status, urban status, and whether the home in which he lived was owned or rented; when *x* is a labor market outcome, the list of controls excludes labor market variables, but adds college completion, years of education, and school attendance. For the younger cohorts, the labor market characteristics that we control for are for the individual's father, and we also control for the individual's father's educational attainment.

groups in all four panels. That said, the coefficients estimates are precise enough to remain statistically significantly different from one another.

Figure 4 focuses on 1940 labor market outcomes. Panels (a), (c), and (e) show veteran-nonveteran differences for the younger cohorts' *fathers'* outcomes, and panels (b), (d), and (f) showing differences for the older cohorts' *own* outcomes. For the younger cohorts, there is evidence of unconditional positive selection based on fathers' income and occupational status, but not their employment status. For the older cohorts, the selection patterns are mixed. We find unconditional positive selection based on occupational income scores for all groups. For log annual wage and salary income, we see positive selection for the *US-W* and *B* groups, but *negative* selection for the *NW* and *SE* groups. And in all groups of older cohorts, we see *lower* pre-war employment rates for veterans than nonveterans, despite excluding from the sample those who reported attending school.

When adding controls, we again see that the conditional selection patterns are greatly diminished compared to the unconditional selection patterns. Veteran-nonveteran differences in 1940 occupational income scores are nearly eliminated (panels c and d), as are differences in coefficients across racial and ethnic groups. Including controls nearly eliminates differences in fathers' wage and salary income (panel a)—for the *US-W* group, the veteran coefficient declines from over 20 log points to only 3 log points. For all groups of the older cohorts, we find that veterans were *negatively* selected in terms of log income when including control variables. Importantly, for the *US-W* and *B* groups, there is no statistically significant difference in the veteran coefficients for young cohorts' fathers or older cohorts' own income. Finally, in panels (e) and (f), black veterans' unconditional negative selection in terms of employment status is nearly eliminated with the inclusion of controls.

In sum, after accounting for place and a subset of available personal (or fathers') characteristics in 1940, selection into military service appears slightly positive in terms of education and near zero, or sometimes negative, in terms of labor market outcomes. These results suggest that the bulk of unconditional selection into military service can be effectively addressed by conditioning on pre-war census characteristics. In addition, we show that including controls substantially reduces, and in some cases eliminates, differences across groups defined by race or ethnicity in the degree of selection. All of these findings are made possible by the newfound ability to link men from 1950, where we observe veteran status, to their pre-war census records. Our next set of analyses examines differences in veterans and nonveterans outcomes in 1950, deploying the full set of 1940 observables as control variables to assist in mitigating selection bias. This enables us to gauge the effects of military service and the GI Bill in a way that covers the vast majority of veterans, without focusing solely on the experiences of the youngest cohorts.

*4.2 Postwar Veterans' Premia in Education*

Next, we focus on estimates of the 1950 veterans' premia in educational outcomes. We study years of education, college completion, and school attendance. We offer separate estimates for the older cohorts according to their highest grade completed in 1940, and for the younger cohorts according to their fathers' highest grade completed in 1940. The motivation for this division is that

individuals may have been differentially positioned to take advantage of the GI Bill's subsidies for schooling depending on their educational background (e.g., if one's schooling ended before 9th grade, one might be far from the margin for college attendance). For empirical perspective, it is important to note that these criteria bear differently on different groups. Figure 5, panels (c) and (f), shows that high school completion (for the older cohorts) and dropout rates (for the younger cohorts) in 1940 were similar for the *US-W* and *NW* groups, conditional on 1940 county of residence, while the *SE* group and to a greater extent the *B* group had lower rates of high school completion and higher rates of dropout. It is also notable that for all groups, high school graduates were a relatively elite educational group. Even for the *US-W* group, only about half the men in the older cohorts had completed 12 years of schooling in 1940. Views that concentrate on high school graduates and their college attendance, therefore, omit a large share of veterans and may deliver a somewhat distorted view of the war and GI Bill's legacy.

In Figure 6, we find evidence of a positive effect of military service and the GI Bill on individuals' highest grade completed by 1950.[36] First, for all ethnic and racial groups, and for both the younger and older cohorts, we find that on average veterans had completed more education than nonveterans by 1950, conditional on their 1940 observables. The confidence intervals indicat that several of these estimates are not statistically significant, but the bulk of the evidence points to differential positive gains for veterans. Second, for the older cohorts (panel b), the largest veteran premia were for men at the bottom of the 1940 education distribution (plotted as green diamonds); moreover, all the coefficient estimates for black men exceed the corresponding coefficients for white groups, though some are imprecisely estimated. Third, for the younger cohorts (panel a), the point estimates are substantially larger than those for older men, tending to cluster around 0.5 versus 0.2 for older cohorts. Among those whose fathers had less than 9 years of education in 1940, the estimates are relatively precise and range from 0.3 to 0.5 for white groups and closer to 1.0 for black men.

These patterns are consistent with younger men being better positioned than older men to take advantage of GI Bill benefits for schooling, which accords with administrative records and VA surveys discussed above. They are also consistent with less-educated men, including high school dropouts, benefiting disproportionately from educational opportunities in the service or incentives to take GED exams or pursue formal education after the war. The relatively positive finding for black men merits closer examination in future research. It may seem contrary to the view that black men benefited less than white men in terms of college education (Turner and Bound 2003), but this could be a case in which focusing on college outcomes for younger cohorts obscures gains elsewhere in the educational and age distributions.

Figure 7 focuses on college completion. In this case, it is important to note that individuals who used the GI Bill benefits for college attendance might not yet have completed college by the time of the census in 1950. For the younger cohorts (panel a), the estimated veterans' premium for

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[36] Note that all these analyses condition on all 1940 controls, including the 1940 level of educational attainment.

the *US-W* group is less than 1 percentage point and relatively precisely estimated. For other groups, the coefficients are often imprecise and sometimes negative. For older cohorts (panel b), we see larger point estimates for those who already had 12 years of schooling by 1940 (blue dots), though only the *US-W* group's coefficient is both substantially positive and precisely estimated at around 1 percentage point. Overall, the evidence in Figure 7 is inconclusive about whether military service led to substantial differences across groups in rates college completion by 1950.

On the other hand, Figure 8 shows substantial veteran-nonveteran differences in school attendance in 1950, particularly for the younger cohorts (panel a) where estimates for the white groups range from about 5 to 10 percentage points, and estimates for black men range from 8 to 16 percentage points. In general, the point estimates are larger for young men whose fathers had completed high school (blue dots) compared to those whose fathers had completed 8th grade or less (green diamonds), consistent with a socioeconomic gradient to postwar re-enrollment in school even given GI Bill subsidies. For the older cohorts, the veteran-nonveteran enrollment differences are much smaller (panel b), less than 2 percentage points for white men. For older black men, however, the veteran-nonveteran gaps are larger, between 2 and 5 percentage points. There is suggestive evidence, particularly for groups other than *US-W*, that the veterans' premia were larger for those who had completed more formal schooling before the war. Thus, although we find little evidence of substantial veteran-nonveteran differences in college completion by 1950 (Figure 7), the school attendance results in Figure 8 are consistent with higher levels of college or sub-college enrollment.[37] To shed light on whether this result reflects college enrollment, Online Appendix Figure A.1 repeats the analysis, but divides the sample according to 1950 education instead of own or father's 1940 education. Clear, and in many cases substantial, veterans' premia in school enrollment among men with more than 12 years of education indicate that college enrollment likely played a part in these results, though there are also clear, though smaller, impacts on enrollment for men with less completed education. Interestingly, these school attendance results do not appear to simply reflect resumption of postponed education among the older cohorts, as nearly 95 percent of the sample of men in the older cohorts was not in school in 1940, suggesting that the war was less likely to have interrupted their education, and results are virtually unchanged when the sample is limited to these men (Online Appendix Figure A.2, panel b). For the younger cohorts, however, eliminating the roughly 75 percent of men who were still in school in 1940 from the sample reduces the veterans' premia for nearly all groups, and in some cases drives them to zero (Online Appendix Figure A.2, panel a).

In sum, we find evidence that is consistent with modest positive effects of WWII service and the GI Bill on veterans' educational outcomes in 1950, except for college completion. These effects were most pronounced in the form of additional years of education for those at the lower end of the

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[37] In 1950's census, the school attendance question pertained to the previous two months. Any schooling that was part of the "regular school system" and advanced someone toward a degree was supposed to be counted, but correspondence courses, on-the-job training, and some vocational training were not, depending on whether such training occurred through a "regular school system." See 1950 enumerator's reference manual (p. 1-477-478).

initial-education distribution and for school attendance for those at the higher end. It is notable that in both respects the estimated veterans' premia are relatively large for black men, despite no strong evidence of differential conditional selection relative to the *US-W* group (Figures 2 and 3), though we cannot definitively rule out that part of the effect is driven by differential selection into service that remains even after including our rich set of controls. We do not find systematic differences between the premia of the various white groups (*US-W*, *NW*, and *SE*), though in some instances, we find that the *SE* and *NW* groups' veteran premia were smaller (e.g., the older cohorts' school attendance and the years-of-education premia for those with least initial education).

*4.3 Postwar Veterans' Premia in Labor Markets*

      Figures 9 to 13 present results for veterans' premia in labor market outcomes in 1950. In all of these analyses, we exclude from the sample individuals who were in school in 1950 so as not to conflate uptake of educational benefits with poor labor market outcomes. Each panel of each figure presents two sets of estimates—one that conditions only on group-by-birthyear fixed effects and the other that conditions additionally for group-by-birthplace fixed effects, group-by-1940 county fixed effects, and available controls from 1940.

      Figure 9 focuses on employment. For the younger cohorts, the three groups of white men (*US-W*, *NW*, and *SE*) exhibit veterans' premia of about 3 percentage points of employment relative to a base employment rate of about 90 percent. For the older cohorts, the estimated premia for each group of white men are just above or just below zero, depending on whether controls are included. There are no economically meaningful differences across the white groups, though the difference between the *US-W* and *SE* group for the younger cohorts is statistically significant. What stands out, however, is that the employment premia for black veterans, both younger and older, are substantially smaller than those for white veterans and statistically significant and negative. Given that racial discrimination in postwar labor markets continued to be pronounced, this pattern may be indicative of more difficult re-entry to the civilian labor market for black veterans.[38]

      Figure 10 focuses on government employment, which is of particular interest given the potential for veterans to have received hiring preferences.[39] In this analysis, we exclude members of the armed forces. We find universally positive veterans' premia. For both sets of birth cohorts, the largest estimated conditional veterans' premium in government employment is for the *NW* group, followed by the *B*, then the *US-W*, and finally the *SE* group (red squares). In general, the differences across groups are not statistically significant, except that the premium for the *SE* group among the older cohorts is significantly smaller than that for the *US-W* group. Examining the composition of government employment across veterans of the different groups yields interesting insights as to the potential mechanisms underlying the effect. Online Appendix Tables A.3 and A.4 present the

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[38] These results are qualitatively robust to controlling for group-by-1950 county of residence fixed effects, meaning that they are not the product of veterans of different races entering different labor markets. For both black and white men, the divisions of non-employed men between unemployed and out of the labor force are qualitatively the same.
[39] For instance, the 1944 Veterans' Preference Act was passed soon after the GI Bill.

distribution of the IPUMS *ind1950* codes for veterans in government employment for each group. Most striking is black veterans' substantial overrepresentation in the postal service and underrepresentation in state and local public administration, the latter of which consists primarily of police and firemen.[40] Second-generation immigrants, on the other hand, were overrepresented in local public administration.

Figure 11 focuses on the veterans' premia in the occupational income score (excluding from the sample individuals with zero occupational income score), providing a summary statistic for occupational status in which each unit corresponds to a $100 difference in median occupational income (1950 dollars).[41] While the unconditional premia are universally positive (blue dots), the inclusion of controls reduces the estimated premia (red squares). The conditional estimates are positive and statistically significant for all the younger cohorts, but always below 1.5 relative to a base of about 20 (for the *B* group) to 27 (for the *SE* group), with the *US-W* and *NW* groups having slightly larger veterans' premia than the *SE* and *B* groups. For the older cohorts, the estimates are positive but small (<0.5) for the *US-W* and *B* groups, and zero or negative for the *NW* and *SE* groups, relative to bases of 21-29.

Figure 12 presents veterans' premia in earned income (wage and salary income plus business and farm income). Though the unconditional estimates of veterans' premia are positive for all groups, other than the *NW* and *SE* groups among the older cohorts, the conditional estimates are much smaller. Across groups of white men in the younger cohorts (panel a), we find a statistically significant and positive conditional veterans' premia of about 3 to 10 log points, depending on the group. But for the older cohorts of white men, we find *negative* and statistically significant veterans' premia; these relative disadvantages are small (under 3 log points) but still remarkable and likely indicative of the older veterans' foregone civilian labor market experience in their 20s. For these men, the GI Bill and any hiring preferences for veterans were not sufficient to fully offset the negative consequences of having served—they lost ground relative to their peers in terms of income. For black men, the pattern of coefficients is similar to that of white men, but the confidence intervals often include zero.

Finally, Figure 13 focuses on the probability of self-employment. Given the GI Bill's provision of business and farm loans and vocational training, it is possible that veterans would have been better equipped to enter self-employment. At the same time, a prolonged absence from the labor force due to their service might have hindered veterans' ability to accumulate savings and to launch businesses. Consistent with the latter, our estimated veterans' premia in self-employment are universally negative, even when controls are included in the regressions. Interestingly, however, the veterans' premia for the *B* and *SE* groups are less negative than those for the *US-W* group, at about -2

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[40] See Boustan and Margo (2009) on black employment with the postal service.
[41] We caution against interpretation of the occupation score as if it were individual-level or group-specific income per se. Black men, for instance, earned less than white men within occupational categories. We present it here as a simple and widely used gauge of occupational status. The next analysis looks at individual-level income directly.

percentage points relative to bases of about 12-13 percent for the younger cohorts and 16-19 percent for the older cohorts.

On the whole, our analysis of veterans' premia in labor market outcomes in 1950 presents a mixed picture. White veterans held some advantages in likelihood of employment relative to observationally similar nonveterans, but black veterans did not. Veterans in all groups had advantages in government employment, though with differences across race in terms of what kind of government job was held. Although younger veterans fared better than nonveterans in occupational status and income, older veterans did not. All groups of veterans had a disadvantage in the likelihood of self-employment. These premia differed across race and ethnic groups and varied by the outcome studied, but not in a way that clearly suggests that some groups of veterans systematically gained or lost more than others in the labor market, at least not by 1950.

## 5. Conclusion

World War II was a catalyst in driving convergence in economic and social status between the native-born white population on the one hand and the communities of racial minorities and second-generation immigrants on the other. Although the transformative effect of the war and the surrounding economic and social upheaval came from many different sources, one aspect of the legislative response to the war—the GI Bill—has been frequently cited as a crucial factor in veterans' economic success. Yet the degree to which men of different ethnic, racial, and age groups were able to benefit from service in World War II and the GI Bill—whether due to differences in service rates, discrimination in the military and civilian labor markets, or differences in incentives or barriers to access benefits—remains a subject of debate to the present.

In this paper, we take advantage of newly available microdata from the US census of 1950 which are linked to individual records from the 1940 census to study selection into military service and the effects of military service and the GI Bill on veterans' educational and labor market outcomes. The picture that emerges from our analysis is that men were modestly positively selected into service in terms of education, conditional on other 1940 characteristics, but they exhibited mixed selection into service in terms of their own or their fathers' 1940 labor market outcomes. It is clear that having pre-war data on veterans and nonveterans helps to sharpen within-cohort comparisons and reduce selection bias in analyses of outcomes in 1950. Conditioning on all 1940 characteristics, we find modest positive effects of military service and the GI Bill on educational outcomes other than college completion in 1950, with slightly positive labor market effects for the younger cohorts and slightly negative labor market effects for the older cohorts. We find little evidence of systematic differences in the veterans' premia across the four ethnic and racial groups that we study.

One important exception calls for additional research. Black veterans with the low education levels before the war notably improved their "highest grade" relative to observationally similar nonveterans by 1950, and black veterans had relatively high rates of school attendance relative to

nonveterans in 1950. These findings are perhaps surprising considering the ways that discrimination, segregation, and racism compromised the potential benefits of military service and the GI Bill (Turner and Bound 2003; Katznelson and Mettler 2008). It is important to bear in mind that black men's lower rate of military service would render any aggregate positive effects smaller than they would otherwise have been (i.e., if individual treatment effects were positive and similar across groups, lower services rates would imply smaller aggregate effects). Even so, most black men born in the early to mid-1920s did serve, and the newly released 1950 census data will allow scholars to deeply reassess the role of World War II in spurring mid-century economic gains.

We interpret our findings as indicative of the differential effect that the war had on those who did not serve in the military but were affected by the war in many other ways, and those who served in the military, survived, and had access to GI Bill benefits thereafter. Our analyses cannot determine whether the effects that we estimate were the product of military service, access to GI Bill benefits, differential treatment in postwar labor markets, or some combination of all these factors. This renders our results less informative about the general effects of programs that subsidize education, training, or extended unemployment benefits. Nonetheless, given the importance of World War II and its veterans in US economic history and the continued salience of the perceived impacts of the GI Bill, it is important to work towards a better understanding of the process of postwar labor market adjustment, veterans' transition into civilian careers, and role of policy in supporting these transitions. In this historical context, the GI Bill was meant to compensate veterans for their foregone opportunities, not to lavish them with advantageous perquisites. This recognition, together with other findings of modest or transitory effects of World War II service and the GI Bill (Angrist and Krueger 1994; Bound and Turner 2002; Stanley 2003; Fetter 2013), make our findings less surprising despite the outsized reputation that the GI Bill has developed in the public perception.

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**Figures**

(a) Service rates
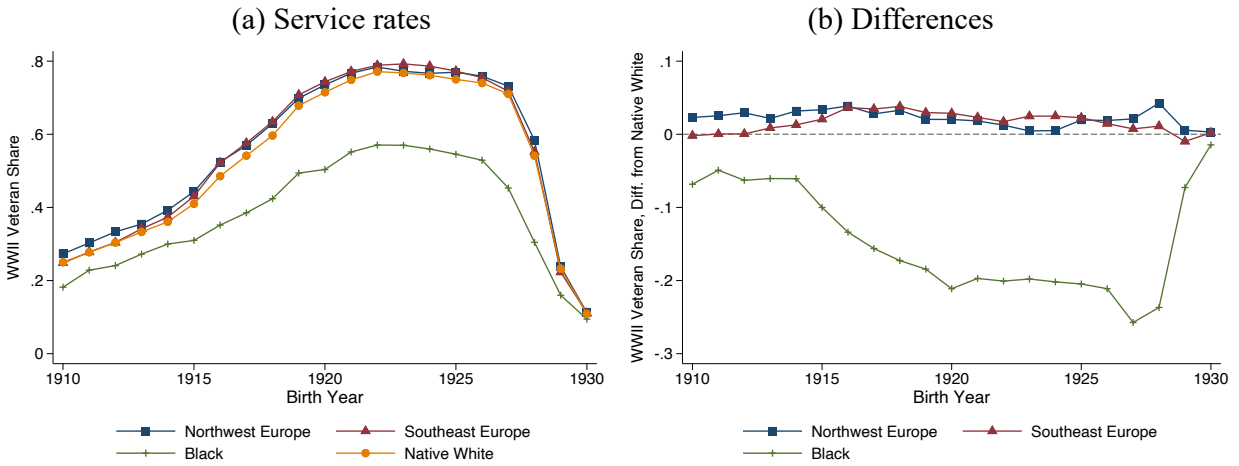


(b) Differences

Figure 1: World War II service rates by race, ethnicity, and birth cohort

*Notes:* Data are from the complete-count 1950 census. Panel (a) presents service rates for each race or ethnicity group by birth year. Panel (b) presents differences in service rates relative to the native white (*US-W*) group.

(a) Subsets of expenditures, by year
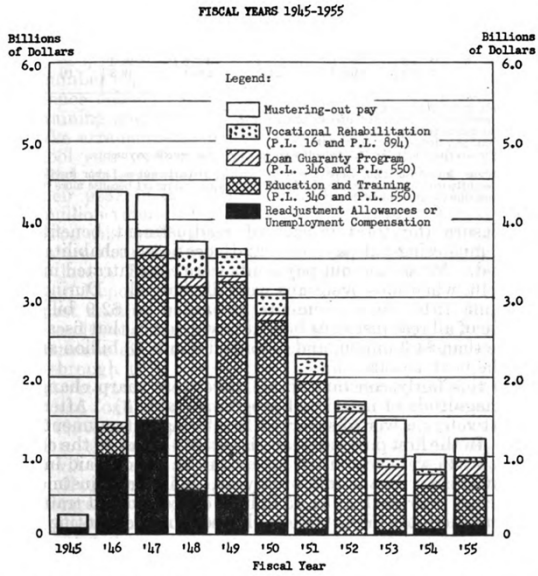


(b) Rates of benefit utilization
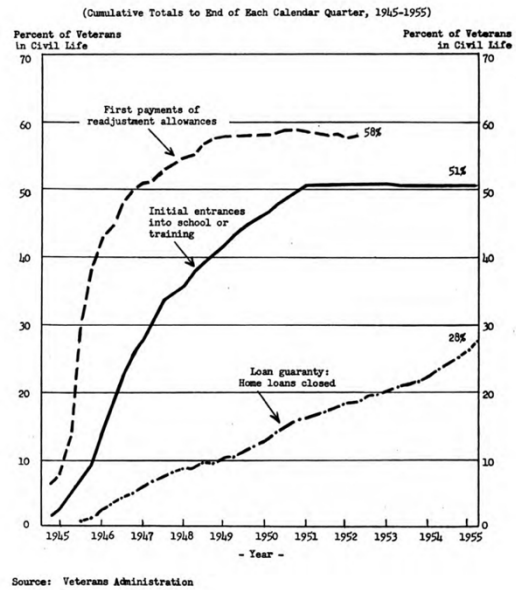


(c) Numbers of veterans in schooling or training
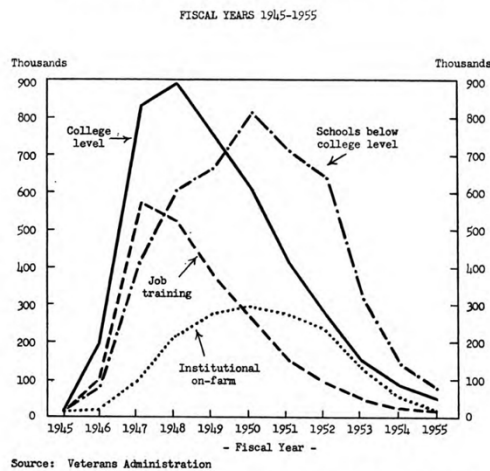


Figure 2: Administrative data on GI Bill implementation

*Notes:* Public Law 346 is the Servicemen's Readjustment Act of 1944 (the WWII GI Bill). Public Law 550 is the Veterans' Readjustment Assistance Act of 1952, which established similar benefits for Korean War veterans. Panel (a) includes Korean War veterans (after 1950).

*Source:* Charts are taken directly from US President's Commission on Veterans Benefits (1956).

(a) Young, School Attendance

(b) Old, School Attendance

(c) Young, Years of Education

(d) Old, Years of Education
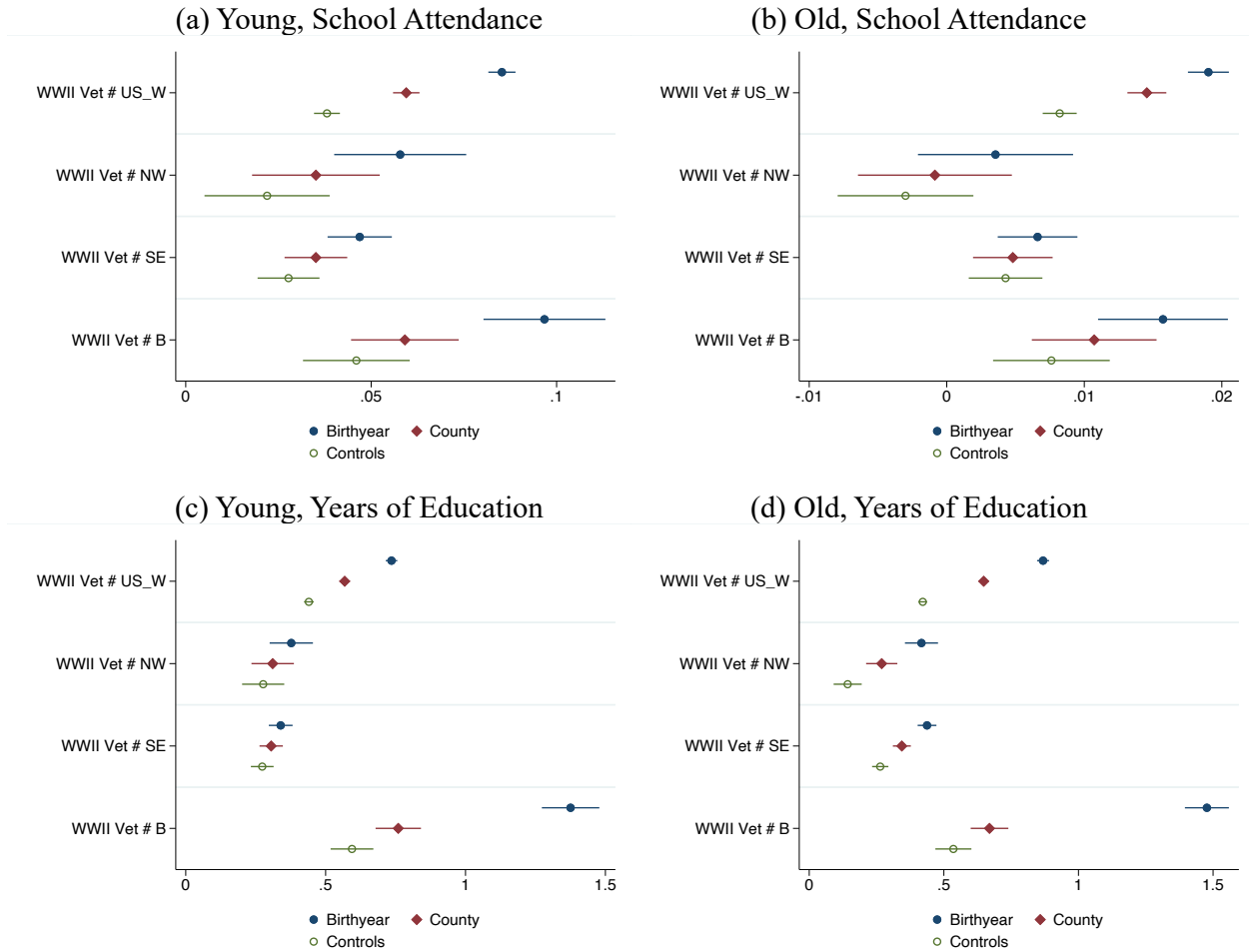
Figure 3: Selection into Service, Own Education

*Notes:* These figures present estimates of $\alpha_j$ from estimation of equation (1) for selection into military service based on 1940 data. The first set of estimates, labeled "birthyear," control only for group-by-birthyear indicators. The second set, labeled "county," controls additionally for group-by-birthplace and group-by-residence county fixed effects. The last set, labeled "controls," controls additionally for all other observables in 1940 other than those that might confound the interpretation of the outcomes (e.g., we do not control for other educational variables). Bars indicate 95-percent confidence intervals. Groups are indicated on the *y*-axis using the same group abbreviations introduced in text.

(a) Young, Dad's log(Wage and Salary Income)
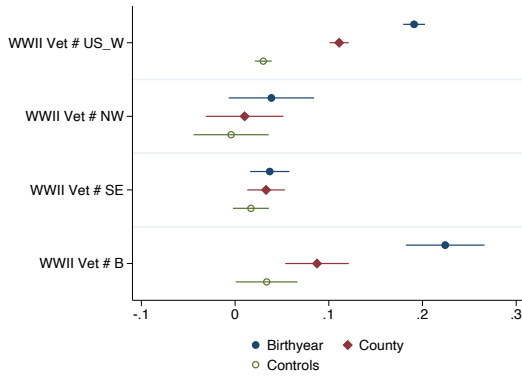
(b) Old, Own log(Wage and Salary Income)
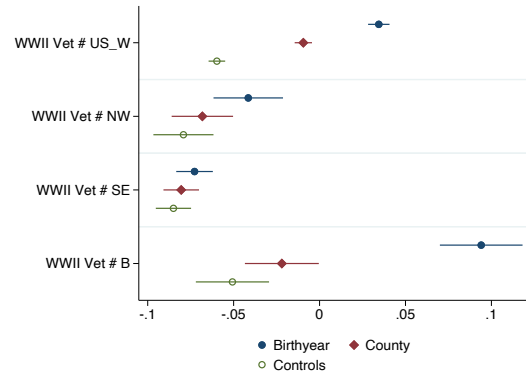
(c) Young, Dad's Occscore
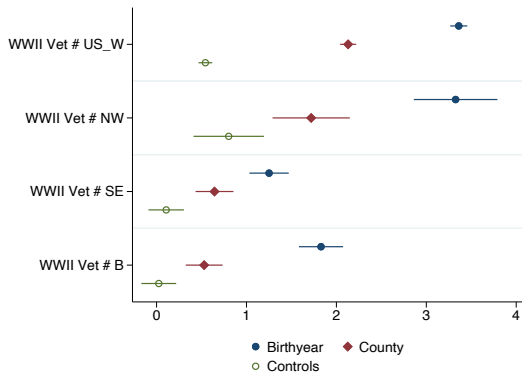
(d) Old, Own Occscore
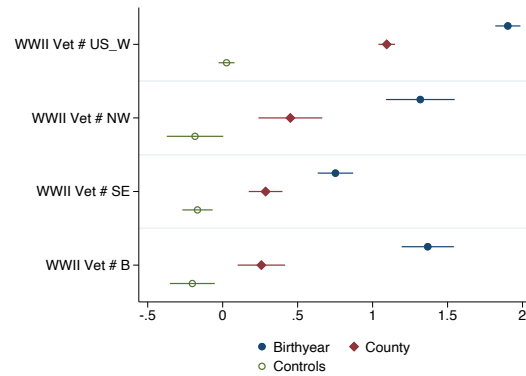
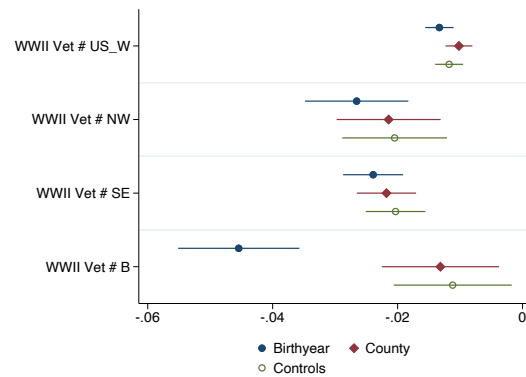(e) Young, Dad's Employment
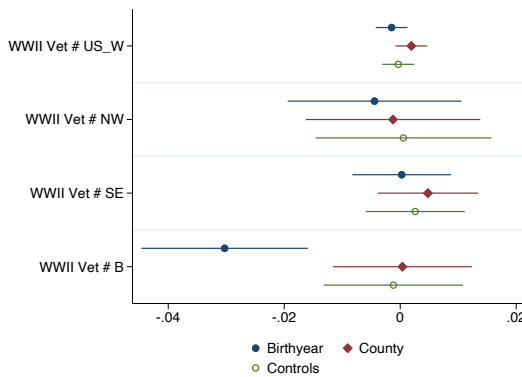
(f) Old, Own Employment

Figure 4: Selection into Service, Labor Market

*Notes:* These figures present estimates of $\alpha_j$ from estimation of equation (1) for selection into military service based on 1940 data. Outcomes for the older cohorts are for the linked individuals themselves, while those for the younger cohorts are for their fathers. The first set of estimates, labeled "birthyear," control only for group-by-birthyear indicators. The second set, labeled "county," controls additionally for group-by-birthplace and group-by-residence county fixed effects. The last set, labeled "controls," controls additionally for all other observables in 1940 other than those that might confound the interpretation of the outcomes (e.g., we do not control for other educational variables). Bars indicate 95-percent confidence intervals. Groups are indicated on the *y*-axis using the same group abbreviations introduced in text. Individuals with no income are excluded in panels (a) and (b).

| (a) HS completion, old | (b) Diffs. in HS completion, old | (c) Adj. diffs. in HS completion, old |
| --- | --- | --- |



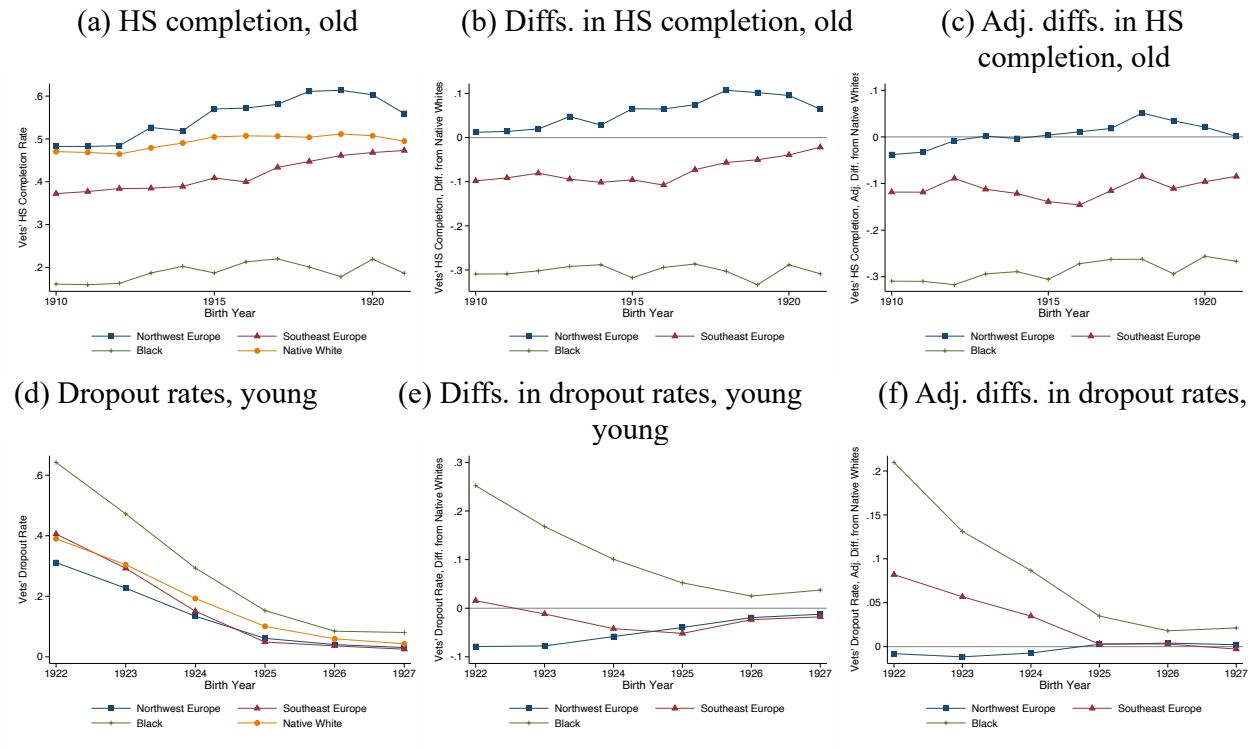| (d) Dropout rates, young | (e) Diffs. in dropout rates, young | (f) Adj. diffs. in dropout rates, young |
| --- | --- | --- |

Figure 5: School completion and attendance rates in 1940

*Notes:* Sample in all figures limited to future veterans. Panels (a)-(c) focus on 1940 high school completion rates of the older cohorts. Panel (a) presents raw rates. Panel (b) presents differences between groups. Panel (c) presents differences between groups after controlling for 1940 county of residence. Panels (d)-(f) focus on dropout rates of the younger cohorts in 1940, defined as not having completed high school and not being in school, with rates, differences, and adjusted differences analogous to panels (a)-(c).
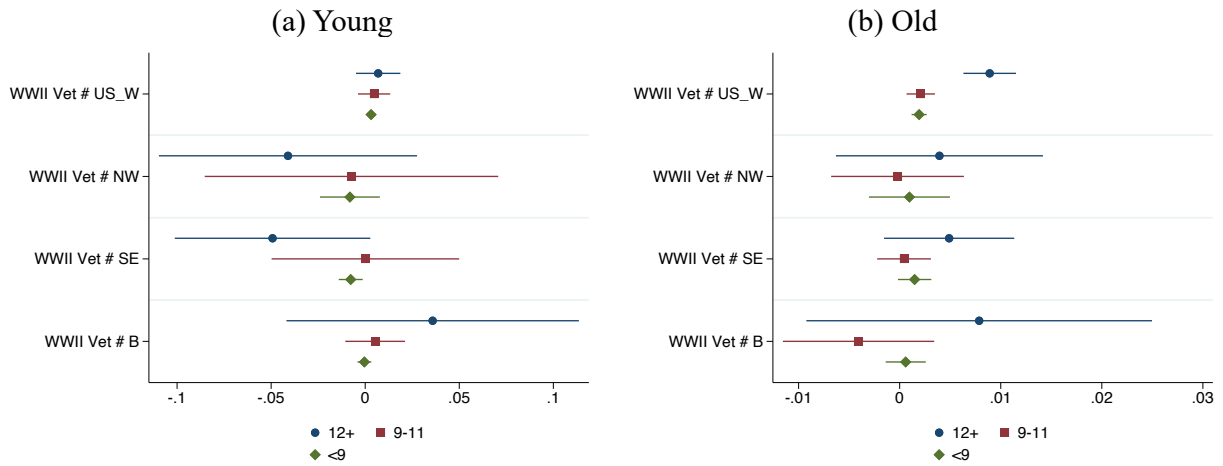
## (a) Young | (b) Old
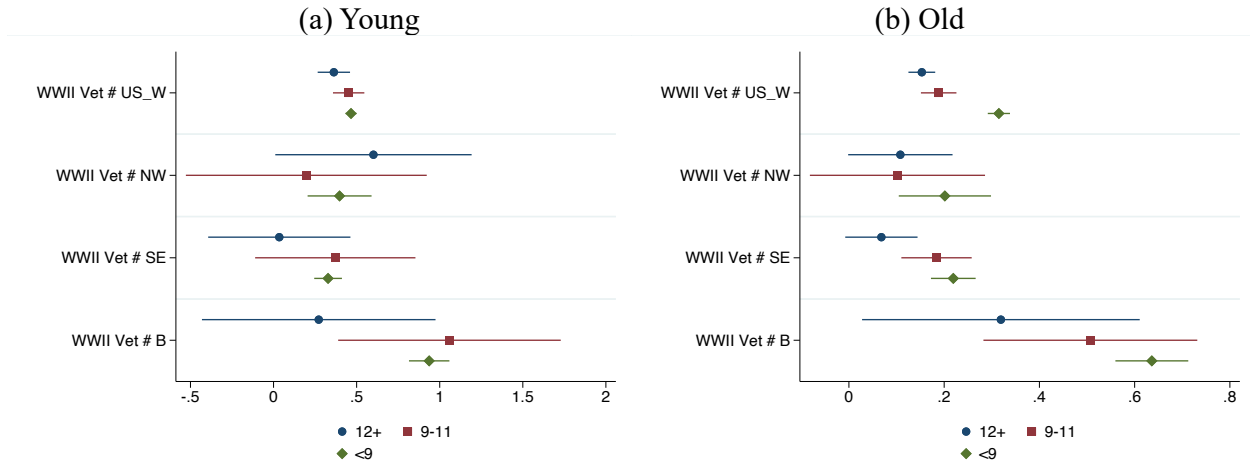


Figure 6: Veterans' Premia in 1950 Years of Education

*Notes:* These are coefficients from estimating equation (2) using the 1950 data. Panel (a) covers the 1922-1927 birth cohorts while panel (b) focuses on the 1910-1921 birth cohorts. All regressions control for all available controls of an individual (and his father, in the younger cohorts) from 1940. Each racial or ethnic group is divided according to 1940 education—own education for the older cohorts and father's education for the younger cohorts.
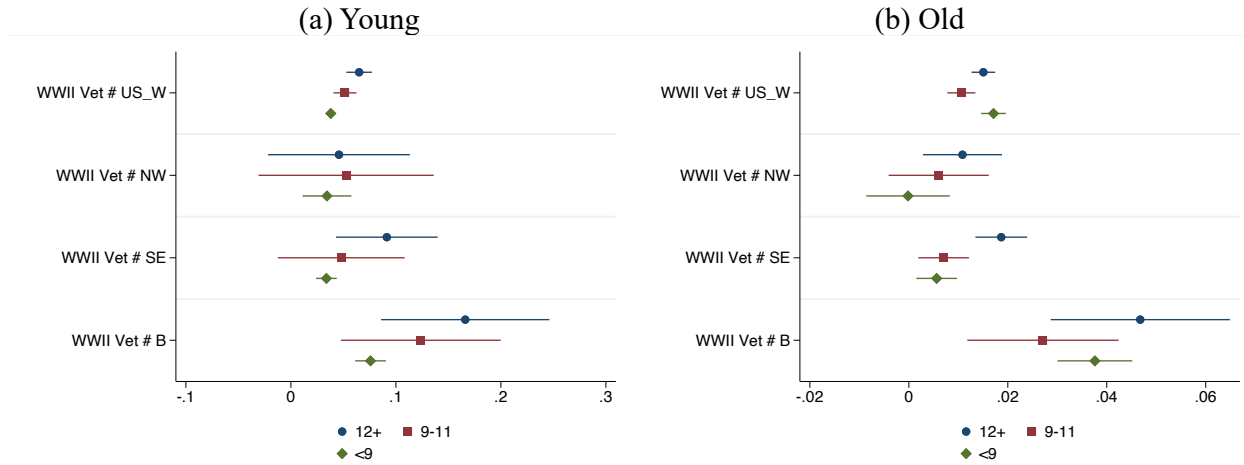
## (a) Young | (b) Old



Figure 7: Veterans' Premia in 1950 College Completion

*Notes:* These are coefficients from estimating equation (2) using the 1950 data. Panel (a) covers the 1922-1927 birth cohorts while panel (b) focuses on the 1910-1921 birth cohorts. All regressions control for all available controls of an individual (and his father, in the younger cohorts) from 1940. Each racial or ethnic group is divided according to 1940 education—own education for the older cohorts and father's education for the younger cohorts.
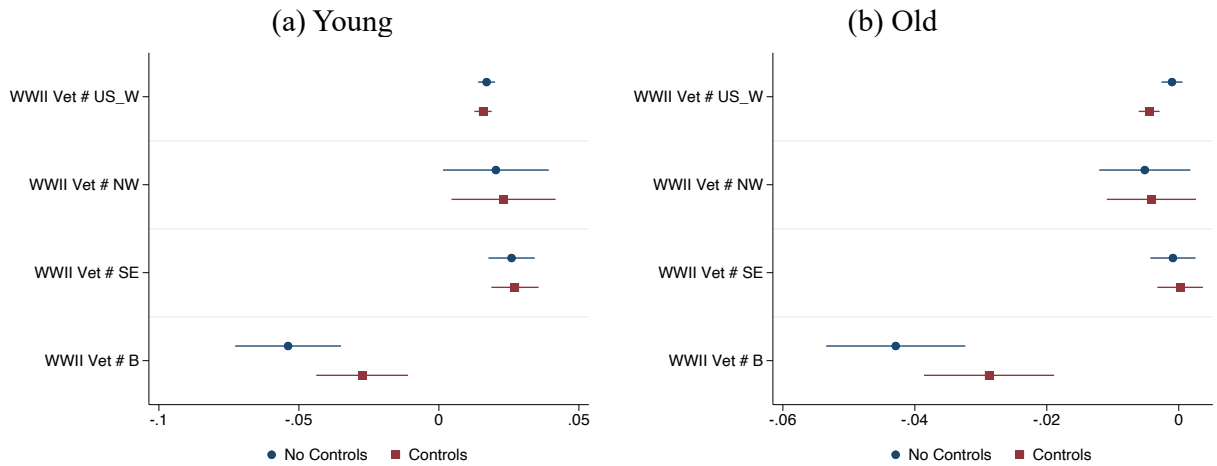
Figure 8: Veterans' Premia in 1950 School Attendance

*Notes:* These are coefficients from estimating equation (2) using the 1950 data. Panel (a) covers the 1922-1927 birth cohorts while panel (b) focuses on the 1910-1921 birth cohorts. All regressions control for all available controls of an individual (and his father, in the younger cohorts) from 1940. Each racial or ethnic group is divided according to 1940 education—own education for the older cohorts and father's education for the younger cohorts.



Figure 9: Veterans' Premia in 1950 Employment

*Notes:* These are coefficients from estimating equation (2) using the 1950 data. Panel (a) covers the 1922-1927 birth cohorts while panel (b) focuses on the 1910-1921 birth cohorts. There are two specifications presented in each panel. The first, with results labeled "No Controls," controls only for birthyear-by-group fixed effects. The second, with results labeled "Controls," controls additionally for birthplace-by-group fixed effects, 1940 county-by-group fixed effects, and all available 1940 controls.
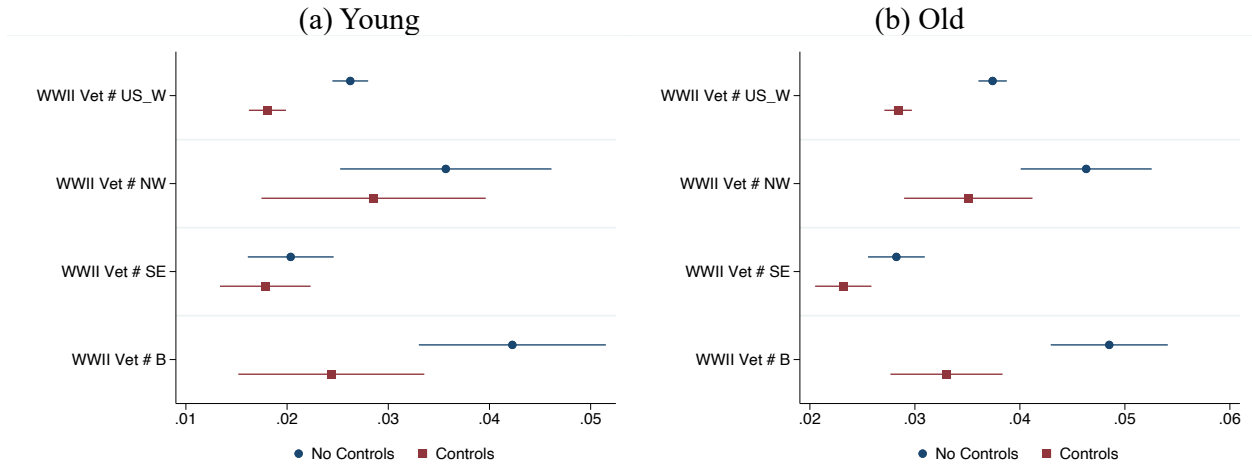
34

Figure 10: Veterans' Premia in 1950 Government Employment

*Notes:* These are coefficients from estimating equation (2) using the 1950 data. Panel (a) covers the 1922-1927 birth cohorts while panel (b) focuses on the 1910-1921 birth cohorts. There are two specifications presented in each panel. The first, with results labeled "No Controls," controls only for birthyear-by-group fixed effects. The second, with results labeled "Controls," controls additionally for birthplace-by-group fixed effects, 1940 county-by-group fixed effects, and all available 1940 controls.
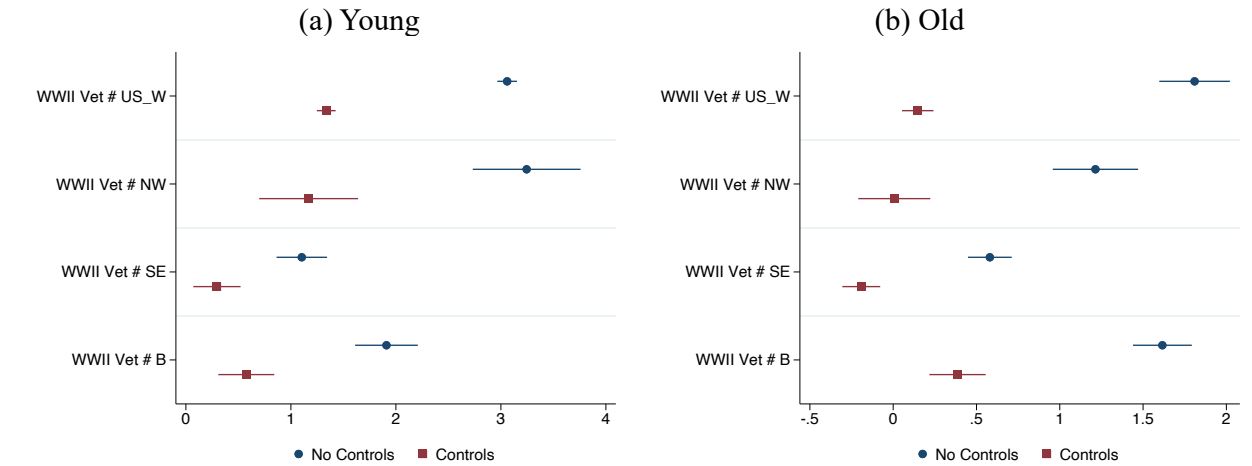


Figure 11: Veterans' Premia in 1950 Occupational Status

*Notes:* These are coefficients from estimating equation (2) using the 1950 data. Occupational status is measured using the occupational income score. Observations with zero occupational income score are excluded. Panel (a) covers the 1922-1927 birth cohorts while panel (b) focuses on the 1910-1921 birth cohorts. There are two specifications presented in each panel. The first, with results labeled "No Controls," controls only for birthyear-by-group fixed effects. The second, with results labeled "Controls," controls additionally for birthplace-by-group fixed effects, 1940 county-by-group fixed effects, and all available 1940 controls.
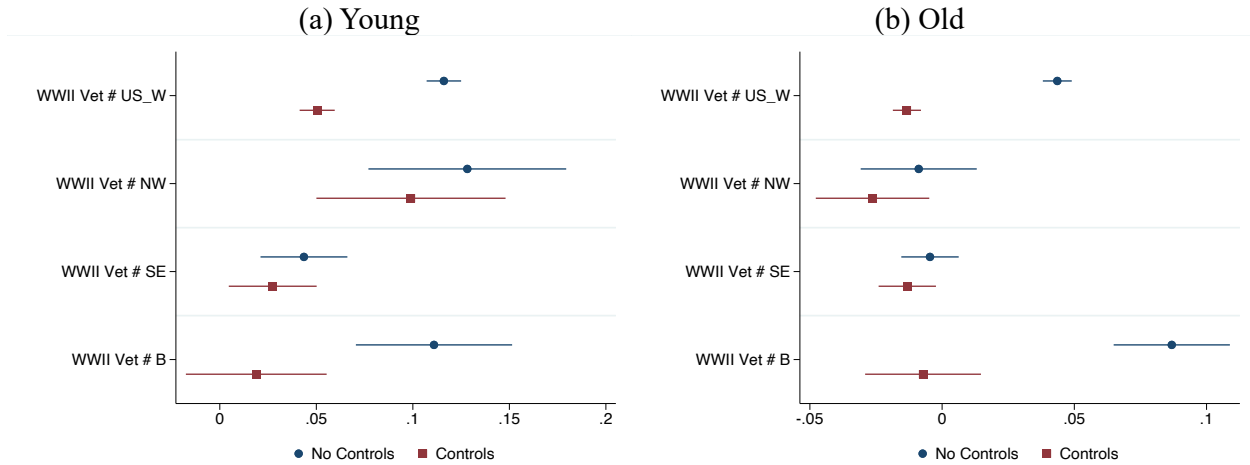
35

Figure 12: Veterans' Premia in 1950 log(Earned Income)

*Notes:* These are coefficients from estimating equation (2) using the 1950 data. Earned income is defined as the sum of wages and salaries and business and farm income. Observations with zero income are excluded. Panel (a) covers the 1922-1927 birth cohorts while panel (b) focuses on the 1910-1921 birth cohorts. There are two specifications presented in each panel. The first, with results labeled "No Controls," controls only for birthyear-by-group fixed effects. The second, with results labeled "Controls," controls additionally for birthplace-by-group fixed effects, 1940 county-by-group fixed effects, and all available 1940 controls.
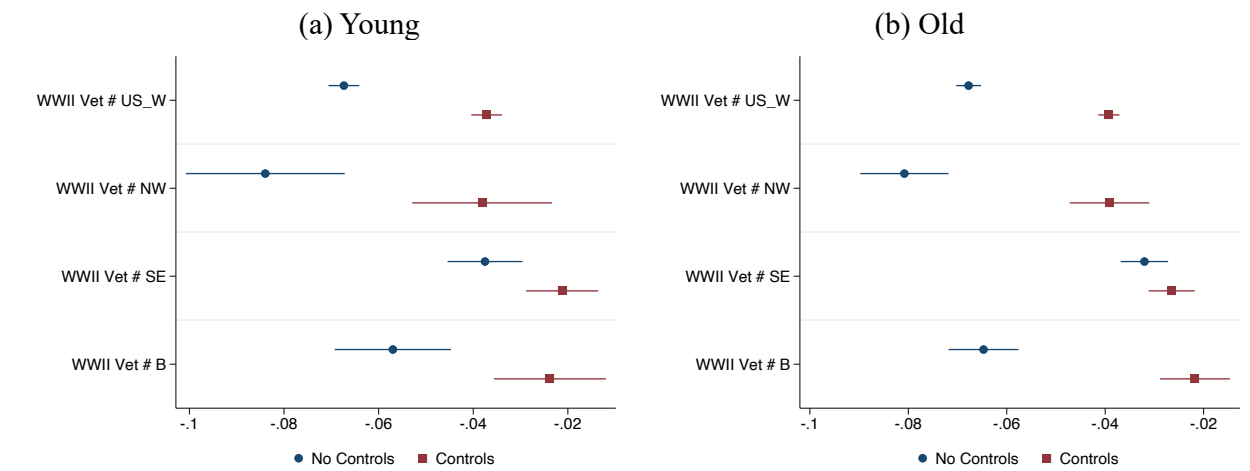


Figure 13: Veterans' Premia in 1950 Self Employment

*Notes:* These are coefficients from estimating equation (2) using the 1950 data. Panel (a) covers the 1922-1927 birth cohorts while panel (b) focuses on the 1910-1921 birth cohorts. There are two specifications presented in each panel. The first, with results labeled "No Controls," controls only for birthyear-by-group fixed effects. The second, with results labeled "Controls," controls additionally for birthplace-by-group fixed effects, 1940 county-by-group fixed effects, and all available 1940 controls.

Table 1: Group abbreviations and sizes

| Abbreviation | Group description | Group sample sizes | |
|---|---|---|---|
| | | 1922-1927 | 1910-1921 |
| *US-W* | White sons of men born in the United States | 375,389 | 754,262 |
| *NW* | White sons of men born in northern and western Europe | 15,107 | 46,596 |
| *SE* | White sons of men born in southern and eastern Europe | 65,478 | 161,340 |
| *B* | Black sons of men born in the United States | 30,752 | 60,027 |
| | Total | 490,343 | 1,026,210 |

*Notes:* This table summarizes the abbreviations that we use to refer to each ethnic or racial group in the sample. All individuals in our dataset are men born in the United States; divisions are based on race and the birthplace of an individual's father. Sample limited to individuals who could be linked between the 1940 complete count and the 1950 20-percent sample line, and who had a non-missing veteran status in 1950.