World War II Service and the GI Bill: New Evidence on Selection and Veterans' Outcomes from Linked Census Records

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Abstract

We examine new datasets of records linked between the 1940 and 1950 US censuses to characterize selection into military service during World War II and to analyze differences in veterans' post-war educational and labor market outcomes relative to nonveterans. Motivated by potentially disparate selection into and effects of service, we pay particular attention to groups distinguished by age, pre-war educational attainment, race, and nativity. We find that veterans were positively selected on pre-war educational attainment, but negatively or neutrally selected in terms of own or fathers' pre-war labor market characteristics. Younger veterans fared better in terms of education and labor market outcomes in 1950 than nonveterans who were observationally similar in 1940. Older veterans exhibited relative gains in education compared to observationally similar nonveterans, but not in labor market outcomes. Black veterans' relative gains in education were relatively large, but black veterans not in school were less likely to be employed than observationally similar nonveterans in 1950. All groups of veterans were more likely to be government employees after the war and were under-represented in self-employment.

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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 in World War II and, it was hoped, to ease the economy's reabsorption 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, stipend and tuition 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 served in World War II (WWII), and most veterans used one or more elements of the GI Bill in their post-war years.[1] Some veterans, politicians, pundits, and academic researchers have lauded the GI Bill as a transformative piece of legislation with strongly positive effects for veterans and for society more broadly (Clinton 1995; Obama 2014). It is "…one of the best-loved and most successful social programs ever sponsored by the American national government" (Skocpol 1997, p. 109). Yet it has also been argued that the GI Bill's effects were uneven, subject to discrimination, and benefited some groups more than others, perhaps exacerbating some dimensions of inequality while reducing others (Lawrence 2022). Various measurement issues, however, have clouded researchers' ability to discern whether and how much WWII service and the GI Bill influenced veterans' outcomes later in life, let alone whether such effects varied depending on one's background.

In this paper, we exploit new, large, representative datasets of linked census records, observing the same men in both 1940 and 1950. These datasets enable us to characterize, at unprecedented scale and detail, patterns of selection into military service based on men's pre-war characteristics.[2] We then analyze post-war educational and labor market outcomes for veterans and nonveterans, while taking account of differences in pre-war observables.[3] This provides novel views of the different trajectories of veterans' and nonveterans' lives over the 1940s, even among those from the same county, with the same level of pre-war education, and with similar socioeconomic backgrounds.

In our analyses, we are particularly interested in several dimensions of heterogeneity. First, we distinguish between older and younger veterans, corresponding to the 1910-1921 and 1922-1927 birth cohorts, respectively, and between those who had lower and higher levels of education before the war. These men may have been differentially selected into military service and differentially positioned to take advantage of the GI Bill's provisions afterwards. We also distinguish between white men whose fathers were born in the United States, those whose fathers were born in southern or eastern Europe, and those whose fathers were born in northern or western Europe. This is

[1] The veteran share is calculated using 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] The Selective Training and Service Act was signed in September of 1940. The 1940 census was taken in April.
[3] 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. See Mettler (2005, ch. 9) and Altschuler and Blumin (2009, ch. 5). 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).

motivated by the possibility that military service and the GI Bill spurred convergence between these groups, hastening the assimilation of immigrants' children decades after the 1920s' restrictions on immigration (Bruscino 2010).[4] Along these same lines, we provide new views of black men's selection into service and post-war outcomes, building on the important insights of Turner and Bound's (2003) study of racial differences in veterans' access to college education (see also Yamashita 2008).

Several measurement challenges are prominent in this setting. First and foremost, selection into military service was not random, which complicates the interpretation of differences between veterans and nonveterans in post-war datasets. In the absence of randomization or a natural experiment, we do not make strong causal claims in this paper. Nonetheless, our datasets of linked census records clarify patterns of selection into military service and enable much closer comparisons of veterans to nonveterans based on their pre-war observables than is possible with cross-sectional post-war data. (We discuss the pros and cons of instrumental-variable strategies that have been used with cross-sectional data later in the paper.) Second, even if WWII service were randomized, the effect of military service cannot be readily separated from the effect of the GI Bill. Military service was a pre-requisite for the GI Bill, and there was not random assignment of benefits within the population of WWII veterans.[5] Therefore, in this literature, attempts to measure "effects" are usually "net" in nature (e.g., Bound and Turner 2002), combining potentially differently signed effects from military service and post-service benefit programs. Our approach shares this feature. Third, the timing of the Korean War (1950-1953) complicates interpretations of census datasets from 1960 onward, which have been central to most prior studies, because men who did not serve in WWII were often drafted to serve in the Korean War. Our emphasis on the newly released 1950 census data avoids this issue because the census was taken before US involvement in Korea.[6] Of course, because this census was taken less than five years after the end of WWII, it is possible that some differences, such as in college completion, needed more time to unfold, and that others, such as in unemployment after demobilization, had already faded away.

We start by showing that aggregate military service rates were remarkably similar for white men whose fathers were born in the United States compared to those whose fathers were born in Europe. This finding is novel and important because it establishes that within the white population, despite the salience of ethnicity circa 1940 and differences in geographic distribution across groups,

[4] 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.

[5] In principle, it might be possible to exploit variation within the population of veterans to learn more. Detailed information on veterans' wartime experiences, linked from military records or post-war surveys of veterans, might differentiate the "military service" component of treatment. Such information is not available in the census records. Stanley (2003) uses a cutoff for benefit eligibility in 1955 to compare Korean War veterans to later veterans.

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

the burden of military service and later access to GI Bill benefits were evenly spread. The rate of military service for black men, however, was far lower for reasons we discuss below (see *inter alia* Lee 1966; Turner and Bound 2003; Guglielmo 2021). It is immediately apparent that black men, on average, were less likely to have been eligible for 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 occupational gains and migration (Maloney 1994; Margo 1995; Collins 2000, 2001; Aizer et al. 2020).

Next, we find evidence of positive selection into military service based on pre-war education variables; that is, veterans on average had higher levels of education in 1940 than nonveterans in the same birth cohort and ethnic or racial group. This pattern is more pronounced for men with US-born fathers (both white and black). We also measure selection into service based on labor market characteristics. For the older cohorts, who had entered the labor force by 1940, we observe individuals' own outcomes; for the younger cohorts, who were still in their parents' household, we observe the fathers' labor market outcomes. We find neutral or negative selection based on labor market observables. Lastly, men who worked (or, for the younger cohorts, whose fathers worked) in agriculture or manufacturing in 1940 were slightly less likely to serve than others, which may reflect deferments for certain kinds of workers within the Selective Service System. Overall, the linked datasets yield a more nuanced view of selection into military service than was previously possible. They show that the nature of selection into service was mixed—its degree and direction depended on the group and outcome variable. This finding is important in its own right, and it helps inform analyses of post-war differences in veterans' and nonveterans' outcomes. We also find that veteran-nonveteran differences in key 1940 variables are often sharply reduced by conditioning on other observables, suggesting that the information in richly detailed panel data may help reduce selection bias in the analysis of post-war outcomes.

When we examine post-war differences in outcomes between veterans and nonveterans in each group, we condition on pre-war socioeconomic observables, including county-of-residence and state-of-birth fixed effects, and, for the older cohorts, individuals' own educational and labor market outcomes in 1940. We emphasize that, although our pre-war observables are detailed, selection on *unobservables* cannot be ruled out in the absence of random assignment.[7] For educational outcomes in 1950, we find that veterans increased their level of educational attainment by more than nonveterans in every group (e.g., about 0.4 grades for younger white men and 0.2 for older white men, conditional on all observables). 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 across groups of white men, conditional on pre-war observables. Administrative reports indicate that younger men were more likely than older men to take advantage of the GI Bill's support for educational programs. In our data, this is reflected in larger differences in 1950 school attendance between veterans and nonveterans in the younger

[7] We do perform a bounding exercise following Oster (2019), finding that nearly all of our results are robust in sign to a 30-percent increase in explanatory power.

cohorts compared to the older cohorts, particularly for black men and particularly for those who had (or whose fathers had) 12 or more years of education in 1940. We find little difference between veterans and nonveterans in college completion, perhaps because it would have been difficult for veterans to start college after the war and graduate before the 1950 census. Overall, these findings are consistent with veterans benefiting slightly in terms of educational attainment from military service and the GI Bill (see also Olson 1974; Bound and Turner 2002; Stanley 2003). In our data circa 1950, this difference may have come through education and training below the college level, an aspect of military service and the GI Bill that merits more attention (Schiffman 1949; Goldstein 1951; Thomas 2017). The results are less consistent with the view that veterans from less advantaged socioeconomic groups benefited much less than others, at least on these observable dimensions.[8]

For 1950 labor market outcomes, we find no strong evidence that older veterans gained relative to nonveterans in terms of employment, income, or occupational status, conditional on 1940 observables (including 1940 labor market outcomes). This suggests that any benefits from the GI Bill were offset by lost years of civilian labor market experience. Among younger men, we find that veterans fared better than nonveterans in both earned income and occupational status, though these differences are rather small (fewer than 5 log points of earned income). In both the older and younger cohorts, black veterans were less likely to be employed than black nonveterans who were observationally similar in 1940. In addition, for all groups, we find positive veteran-nonveteran differences in the likelihood of government employment, though in different subsectors of government depending on race (e.g., police and firemen for white men vs. postal service for black men). On the other hand, for all groups, we find that veterans were less likely than their peers to be self-employed, which 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 meant to compensate veterans for the interruption of their educations and careers and to facilitate their transition into the civilian labor force; it was not designed to boost veterans far higher in the income distribution than they otherwise could have achieved.

This paper makes several contributions. First, beliefs about WWII and the GI Bill inform voters' and policymakers' thinking about the mid-century American economy and ways in which the government promoted growth, mobility, human capital accumulation, and middle-class wealth. President Clinton, for instance, asserted that "The GI bill helped to unleash a prosperity never before known" (1995).[9] Yet the war's legacies are likely to have been complicated and context dependent: different veterans may have benefited (or not) from different aspects of the GI Bill and at different times in their lives. Measurement in this setting is challenging, but new microdata sources and record-linkage techniques can help scholars clarify the story and, when needed, revise widely held beliefs that lack solid empirical grounding. In this paper, we use linked census records to anchor a

[8] We caution that the *quality* of schooling or training is not something the census variables on education will reflect.
[9] Clinton was speaking at an event marking the Franklin D. Roosevelt's legacy. See Skocpol (1997) for further context and discussion.

novel empirical vantage point from which, for the first time, we can consistently examine the experiences of several groups defined by age, race, and ethnicity with a combination of data from both before and after military service. Our findings thus enrich a somewhat small but impactful and growing economics literature on the effects of WWII service and the GI Bill.[10]

This 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 very much underway (Collins and Zimran 2019, 2023; Abramitzky et al. 2020, 2021b). It has been speculated that military service in WWII 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, there has been no quantitative analysis of the role of military service and the GI Bill in potentially improving outcomes for immigrants' children. The sheer size of the complete count census records makes it possible to do so, with separate analyses for men whose parents immigrated from northern or western Europe and those whose parents immigrated from southern or eastern Europe. Ultimately, we do not find evidence that WWII military service was associated with especially strong outcomes for children of European immigrants relative to other groups.

In a similar way, this paper contributes to the literature concerned with racial disparities in educational and economic outcomes. Rigid segregation within the military and in many areas of American life ensured that black men's experiences during WWII and in the post-war economy were materially different from those of white men.[11] 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; see also Onkst 1998). Recognition that the GI Bill, despite its "race-neutral" legislative language, may have exacerbated inequality in access to college education is a major qualification to the idea that it 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 research in this area is clearly merited. We contribute by studying other aspects of educational attainment, broadening the scope of analysis to a variety of labor market outcomes, and providing some of the first evidence on black veterans that builds from a panel dataset that spans the 1940s (Collins 2000).

2. Background

[10] See Bound and Turner (2002), Stanley (2003), Turner and Bound (2003), Fetter (2013), Thomas (2017), Althoff and Szerman (2022), Eden (2022), Abramitzky et al. (2024), Barrera et al. (2024), and Domnisoru (2024).
[11] There is a large literature in this area. See, *inter alia*, Bolté and Harris (1947), Atkins (1948), Dalfiume (1969); Onkst (1998); Collins (2000); Turner and Bound (2003); Katznelson (2005); Katznelson and Mettler (2008); Guglielmo (2021); and Eden (2022).

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

Figure 1 presents self-reported military service rates from the 1950 census by racial or ethnic group and birth cohort, as well as differences in these rates relative to white men with US-born fathers (*US-W*). Panel (a) shows that the peak rates for WWII military service are found in the early to mid-1920s birth cohorts (Ruggles et al. 2024a, b).[12] Among white men with southern or eastern European-born fathers (*SE*), northern or western European-born fathers (*NW*), 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.[13] In contrast, black men with US-born fathers (*US-B*) had service rates that were about 20 percentage points lower than white men. These patterns have important implications for the share of men in each group who experienced military service and were subsequently eligible for GI Bill benefits.[14]

These service rates reflect a combination of voluntary enlistment and conscription. In 1939, only 335,000 personnel were on active military duty, compared to a peak in 1945 of over 12 million (Gartner 2006a, p. 5-355). The Selective Training and Service Act of September 1940 laid the administrative groundwork for the mobilization of young men by requiring their registration with local boards and initiating the draft. In the wake of the Pearl Harbor attack on December 7, 1941, many volunteered for military service, but overall, approximately 61 percent of WWII servicemen were conscripted (Gartner 2006b, p. 5-363). 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).[15] But these standards varied over the course of the war, and some men who were initially rejected were reclassified and inducted later. Other men received occupational deferments, typically men already working in war industries or agriculture (US Selective Service System 1950, pp. 259-271, 289-293). In aggregate, circa February 1945, more registrants in the 18-37 age range had received deferments than had been rejected (US Selective Service System 1946, p. 290).

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 (Dalfiume 1969; Flynn 1984; Guglielmo 2021). The slow and reluctant opening of military service

[12] These post-war 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.
[13] These differences are even smaller when conditioning on 1940 characteristics.
[14] 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.
[15] 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). In spring 1943, illiteracy was removed as a disqualifying condition. Approximately 287,000 men were inducted and sent to special training units where 83 percent learned enough basic skills to serve (Goldstein 1951, p. 606).

opportunities for black men was a prominent political issue throughout the war. Black men were also rejected from service 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. After the war, discrimination on many fronts narrowed black veterans' scope for educational and economic advancement (Onkst 1998; Turner and Bound 2003; Katznelson and Mettler 2008). Nonetheless, Weaver (1945) and Schiffman (1949) argue that some black servicemen received valuable training, and black veterans' participation rate in GI Bill programs was comparable to that of white veterans (President's Commission on Veterans' Pensions 1956, p. 72).

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).[16] Approximately 16 million Americans served in the military during WWII. 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.[17] 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). Congress crafted the legislation to avoid extending the influence of federal agencies over state and local governments, thereby protecting entrenched segregation and discrimination.

The GI Bill created 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.[18] 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

[16] 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).
[17] Congress made revisions in 1945, mostly increasing generosity (Altschuler and Blumin 2009, p. 82).
[18] 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.

facilitate their search for employment.[19] 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 Veterans' 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. 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 rates 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).[20] This is useful to keep in mind because identification strategies that emphasize treatment effects for the youngest cohorts of WWII 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 times in the post-war 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." The 1979 Survey of Veterans helps to fill in the story (Department of Veterans Affairs 1979).[21] For instance, a higher share of black WWII veterans reported receiving VA support for education or training after the war than did white veterans (see also the President's Commission on Veterans' Pensions 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),

[19] Each month of service implied four weeks of unemployment benefits, up to 52 weeks and paying $20 per week.
[20] 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 (President's Commission on Veterans' 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).
[21] This is a small but valuable 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 0 in the tabulation.

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.[22]

2.2 Research in Economics

The literature on WWII service and the GI Bill is large and multidisciplinary, and therefore our view is shaped by a variety of sources.[23] In this section, we briefly describe some of the key research findings in the economics literature, and then we highlight how our paper adds novel perspectives by harnessing newly available data. The most relevant and recent economics literature tends to partition into a few different areas of study: educational outcomes (e.g., Bound and Turner 2002; Turner and Bound 2003; Stanley 2003; Thomas 2017), labor market outcomes (e.g., Angrist and Krueger 1994; Collins 2000; Thomas 2017; Barrera et al. 2024), and housing market outcomes, especially home ownership (Fetter 2013).

Given the popular emphasis on the GI Bill's support for college enrollment and policy interest in higher-education subsidies, it is not surprising that economists have paid careful attention to veterans' college-level attainment. Bound and Turner (2002) and Turner and Bound (2003) begin their cross-sectional studies of WWII service effects by calculating differences in college attainment between veterans and nonveterans circa 1970 *within* birth cohorts, which they consider an upper-bound estimate in the presence of positive selection on educational background. The papers' primary emphasis, however, is on an empirical strategy that exploits variation in service rates *across* cohorts, which relies heavily on the decline in service rates for men born after 1927 and the assumption that cross-cohort trends in education would have been smooth otherwise.[24] On this basis, Bound and Turner (2002) conclude that the effect of WWII service and GI Bill eligibility was positive and sizable for white men—5 to 6 percentage points in college completion observed in 1970's census (Bound and Turner 2002, p. 807). This result is similar in magnitude to their within-cohort veteran-nonveteran differences when the sample is limited to those with at least 12 years of education (p. 798).[25] Turner and Bound (2003) pay particular attention to black-white differences and conclude 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 WWII service and the GI Bill across racial categories.

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[22] 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.

[23] For broad historical perspectives on the GI Bill see, *inter alia*, Ross (1969), Olson (1974), Mettler (2005), Frydl (2009), and Altschuler and Blumin (2009). Sociological perspectives include, *inter alia*, Nam (1964), Fredland and Little (1985), Sampson and Laub (1996), and Teachman and Tedrow (2004).

[24] Domnisoru (2024) uses this identification strategy to study veterans' underemployment.

[25] Lemieux and Card (2001) study Canadian veterans, who were eligible for various post-war 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).

Stanley (2003) also focuses on college-level education, but he relies primarily on evidence from the Occupational Changes in a Generation (OCG) datasets for men in 1962 and 1973. After a careful analysis of Korean War veterans' outcomes, the paper addresses WWII veterans' outcomes with a combination of insights from within-cohort comparisons (considered an upper-bound estimate of effects) and across-cohort comparisons (lower bound), assuming that the GI Bill had relatively small effects on college education for men from the early 1920s birth cohorts.[26] The results suggest a positive effect for WWII 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" in boosting Americans' college education (2003, p. 673).

Angrist and Krueger (1994) focus on the WWII veterans' earnings premium. In cross-sectional data for 1980, it is clear that WWII veterans earned more than similarly aged nonveterans, but it is unclear whether the gap was causally related to military service and the GI Bill. The paper instruments for veteran status with quarter of birth and assumes that labor market outcomes were uncorrelated with quarter of birth otherwise (c.f., Buckles and Hungerman 2013). On this basis, the authors conclude that the positive veteran earnings premium in 1980 was attributable to selection; in fact, their baseline 2SLS estimates for 1980 are negative (p. 83). Thomas (2017), on the other hand, employs a different identification strategy, one that is closer to Bound and Turner's approach, and finds positive effects on veterans' employment and being above the poverty line in 1970.[27]

Barrera et al. (2024) revisit the effect of WWII service on earnings with substantially richer data. Linking the 1940 full-count census to WWII enlistment records and administrative tax data in 1969, they document an earnings premium for veterans relative to nonveterans, conditional on 1940 observables. But when addressing selection with a regression discontinuity strategy that exploits the decline in service rates after the 1927 birth cohort (as in Fetter 2013, discussed below), they find evidence of a negative effect on income. Their large samples enable a distinction between veterans of various racial and ethnic groups (which overlap somewhat with those that we study); most relevant for comparison to our analysis is that they find suggestive evidence of a smaller income premium for black veterans as compared to white veterans.

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 regression discontinuity design centered on men who had just turned 18 or

[26] 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.

[27] We also want to highlight Teachman and Tedrow (2004), which analyzes veterans' positive income premium circa 1966, including attention to racial differences. This paper relies on the National Longitudinal Survey of Mature Men (N=3336, men born from 1906 to 1921). In addition to current labor market outcomes, the dataset includes retrospective information about men's military service and broad indicators of socioeconomic background. The dataset's information on vocational training is of particular interest.

were about to turn 18 at the war's end. He finds that access to home loan benefits under these GI Bills had an impact on homeownership in 1960 but not 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 documenting patterns of selection into WWII military service and in understanding how military service and engagement with the GI Bill might have varied across groups and depended upon their pre-war characteristics. Investigating this heterogeneity requires large datasets with detailed pre-war characteristics of men who would later serve in the military and those who would not. The complete-count censuses from 1940 and 1950 have become available to scholars since the literature described above was written (except for Barrera et al. 2024). This enables us to link large numbers of individuals over time to see their pre- and post-war situations in rich detail, opening a new vantage point for observing men's career trajectories over the 1940s.

The panel structure of our data is a key distinction relative to most of the work cited above, in which authors could not observe an individual's pre-war characteristics in post-war cross sections.[28] Rather, the literature's main empirical approach has centered on post-war cross sectional data, leveraging the substantial decline in WWII service between cohorts that were just old enough to serve in WWII and those that were too young.[29] This leads to a focus on younger cohorts of veterans and to local average treatment effects that hinge on the youngest men's outcomes. Because their wartime experiences were often brief and because their pre- and post-war situations were quite different from those of most veterans, measurements grounded in the youngest veterans' experiences might not generalize reliably to others.[30] For instance, we know from administrative 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 much more likely to have gained labor market experience, completed their formal education, or started families before entering the military. Older men were also more likely to have received occupational deferments. We offer analyses of both older and younger cohorts of veterans, broadening the literature's view of WWII servicemen and their subsequent outcomes.

[28] As reviewed above, Stanley (2003) is an exception because it relies on the OCG datasets, which include some retrospective information circa 1962 and 1972, but are small compared to the linked datasets we will employ. Barrera et al. (2024), like us, use the 1940 census to observe future veterans before they entered the service, though they do not focus on selection into service.

[29] Barrera et al. (2024) use this approach in combination with pre-war characteristics.

[30] Bound and Turner (2002), who pioneered this approach, are careful to point out that "…what identifies these models is the comparison of outcomes for men who were likely to begin service before and after V-J [Victory-over-Japan] day" (p. 802). Most veterans were born before 1922.

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).[31] We 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.[32]

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 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. Prior research, reviewed above, addressed this issue by moving to later censuses, 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 WWII had served in the Korean War and, therefore, were eligible for the Korean War GI Bill benefits.[33] Bound and Turner (2002), who rely on 1970's cross-sectional data, raise this issue explicitly: "the analysis is framed in terms of measuring the effects of WWII 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" (Bound and Turner 2002, pp. 791-792, emphasis ours). Our analysis, by focusing on the new 1950 census dataset, which is substantially larger than the original 1950 sample and may be linked to prior censuses, 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 highest rates of

[31] Collins and Zimran (2024) provide a replication package for the results in this paper.

[32] 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 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.

[33] 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.

WWII 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, and *their fathers'* labor market outcomes and educational attainment.[34] 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, in 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 of place of residence, and county of residence. In total, the linked datasets we analyze consist of 486,726 men from the 1922-1927 cohorts and 1,022,225 men from the 1910-1921 cohorts.

Table 1 reports the sample size for each dataset, divided into the four main ethnic or racial groups on which we focus. The goal is to provide a wide perspective on veterans from different backgrounds while maintaining a sufficiently large 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.[35] 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 *US-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.

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").[36] 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

[34] Men in the 1922-1927 cohorts with missing information on their father's status are dropped from the analysis.
[35] We attempted to analyze Native Americans and men of Latin American and Asian descent. Unfortunately, the linked dataset includes relatively small numbers in each group and yields noisy statistical results. We do think that careful and detailed analysis of these groups is a promising route for future research (see also Barrera et al. 2024).
[36] 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. We also experimented with dropping any observation with one year of education or less, as a report of one year was flagged by Ruggles et al. (2024a,b) as potentially problematic as well. This had no qualitative impact on our findings.

outcome variables; therefore, as long as there are no systematic differences in misreporting by veteran status, our estimates will not be affected.

We focus on two main categories of 1950 outcomes. The first comprises educational outcomes—years of education, school attendance, and college completion (16+ years of education). The second covers labor market outcomes—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 WWII might mean that individuals attending college had not yet graduated. Second, many men used educational benefits for purposes other than college, as shown in Figure 2(c) and revealed in the 1979 Survey of Veterans (Department of Veterans' Affairs 1979). Casting a wider net than only college education will help capture an underexplored aspect of veterans' experiences. Regarding the labor market variables, although 1950 is early in the lifecycle for many of the men in our sample, it is still of interest to learn whether veterans gained an advantage, or perhaps fell behind, relative to nonveterans in the labor market. The 1950 outcomes speak directly to the speed and nature of the economy's reconversion, its reabsorption 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 hiring preferences for veterans to lead veterans disproportionately into public sector jobs.[37] 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

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:

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

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; γ_{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 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 α_j vector, which measure the average differences in *x* between veterans and nonveterans in each ethnic and racial group. In the simplest regression, no

[37] Men who were still in the military in 1950 are excluded from this analysis.

controls are included apart from γ_{jc}^{1940}, yielding simple measures of *unconditional selection*. A positive value of α_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.[38] This provides a measure of *conditional selection*, based on close comparisons of veterans and observationally similar nonveterans. 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. For younger men, we can see whether 1940 school attendance rates differed, conditional on their fathers' observables, location, and so on. We are also interested in the differences in α_j estimates *across* groups. If one group's α_j is positive and greater than another's, then that group had relatively strong positive selection into military service, a useful piece of evidence in characterizing differences across groups in WWII service.

Measurement of the effects of wartime service and the GI Bill would be straightforward if men had been randomly selected for the military. But 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 WWII, 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 WWII, 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 clearcut than one might expect for such a widely studied event, as it requires detailed information on both veterans and nonveterans *before* the veterans' service. As discussed above, some men were rejected for health or educational deficiencies deemed to be incompatible with military service. On the other hand, some were granted occupational deferments or declined to enlist voluntarily though eligible to do so, particularly among the older cohorts. In the presence of these selection issues, measuring veterans' average gains or losses relative to a counterfactual in which they did not serve is challenging. Linked census records can help by clarifying the extent of selection on observables and providing direct and detailed controls for individuals' pre-war characteristics and outcomes. Yet, even after selection on these observable characteristics is addressed, bias from unobservables might linger. For instance, veterans might have been healthier than nonveterans in ways that are unobservable to us. For this and other reasons, veterans may have been predisposed to success in education and the labor market. These predispositions will, at least to some extent, be captured in the 1940 characteristics that we can observe, but some selection bias may remain even in the presence of controls. We revisit the proper

[38] 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.

of interpretation of our results as we present evidence on both pre-war selection and post-war outcomes.

Conceptually, it is important to be clear about what the "veteran treatment" entailed in this setting and to acknowledge that an event as massive as WWII 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 or other forms of education), 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 work experience and accumulation of wealth, but it also may have curtailed their investment in formal education since the opportunity cost of additional schooling was high. Given the GI Bill's 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 acknowledge because it is this counterfactual that policymakers must have contemplated and sought to avoid when creating the GI Bill. We also cannot assess a counterfactual in which WWII never happened at all. These considerations render our results less informative about the general effects of programs that subsidize education, training, or extended unemployment benefits. Nonetheless, there is value in better understanding WWII veterans' transition to civilian careers and their socioeconomic status relative to nonveterans in the postwar era. Their experiences have shaped perceptions of the basis for the US economy's prosperity and, thus, have influenced policy debates to the present.

Bearing these challenges in mind, in the next step of our empirical strategy, we aim to estimate how different veterans' outcomes were circa 1950 relative to nonveterans who were observationally similar before the war. We estimate equation (2) using outcome data from 1950 and detailed background information from 1940:

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

where y_{ijc}^{1950} is the outcome of interest for individual i of ethnic or racial group j born in cohort c; γ_{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. A particularly beneficial feature of equation (2) is that, for the older cohorts, whom we observe as adults in 1940, we can include the 1940 level of their outcome variable as a control. Since individual-level productivity, aptitude, and access to resources are likely to be reflected in pre-war educational and labor market outcomes, these are valuable controls and a key strength of linked datasets that span the war. The coefficients β_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' premium across groups. We use an Oster (2019)-type bounding exercise to assess the potential for selection on unobservables to bias β_j even after controlling for all 1940 variables.

As discussed above, our empirical approach, which uses detailed pre-war information to reduce selection bias in comparing veterans' and nonveterans' post-war outcomes, differs from that which exploits the reduction in the probability of service after the 1927 birth cohort for causal identification (i.e., a fuzzy regression discontinuity (FRD) or similar design). A simple consequence is that our results are based on within-birth cohort differences from many cohorts, whereas those from a FRD method are based on cross-cohort differences that rely for causal identification on only the youngest men. Our results may be subject to bias from selection on unobservables in ways that those from the FRD method are not, so long as the appropriate FRD identifying assumptions are met. Therefore, we do not push a strong causal interpretation of our "veterans' premia" coefficients. Yet even if they could be interpreted as causal effects (i.e., the conditional independence assumption held), we would not expect them to match estimates from the FRD method. First, this is because the FRD method estimates the local average treatment effect for men who served only because they were just barely old enough to do so. These men, who make up only a fraction of our youngest cohorts, would have had very different educational opportunities and military experiences than the bulk of WWII veterans. Second, we expect substantial differences from other estimates because we focus on 1950 outcomes rather than outcomes in later years. As Bound and Turner (2002) point out, analyzing later census years means that the control group in FRD applications is potentially contaminated by Korean War veterans. Moreover, there would have been many additional years for the effects of service and the GI Bill to materialize or erode (e.g., in 1970 the median WWII veteran would be about 50 years old).

4. Results

4.1 Selection into Military Service

Figures 3 and 4 present estimates of the coefficients α_j from equation (1) to gauge the degree of selection into military service on a variety of observable dimensions in 1940. 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 two 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 1940 characteristics, including indicators for birthplace and 1940 county of residence.[39] We dwell on these selection-into-

[39] 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, craft, farmer, operative, and unskilled occupations, indicators for farm status, urban status, and whether the home in which he lived was owned or rented, indicators for working in agriculture or in manufacturing, and population of city of residence or urban place of residence; when *x* is a labor market outcome, the list of controls excludes labor market variables (though including these controls does not qualitatively change our findings), 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.

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 better understanding the post-war 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 completed. When conditioning only on birthyear (blue circles), there is strong evidence of *unconditional* positive selection into military service. The coefficients are largest for black and white men with US-born fathers (*US-B* and *US-W* groups). For the younger cohorts (panel a), future veterans in the *US-B* and *US-W* groups were almost 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). In the *NW* and *SE* groups, the gap was closer to 5 percentage points. 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; again, differences in the *NW* and *SE* groups are smaller, closer to 0.5 percentage points.

Panels (c) and (d) show qualitatively similar unconditional selection patterns for highest grade completed. The veteran-nonveteran difference was largest for black men, with veterans having nearly 1.5 grades more education in 1940 in both the younger and older cohorts. For the *US-W* group, the difference was about 0.8 grades, and for the *NW* and *SE* groups the difference was about 0.4 to 0.5.

In all cases, adding controls for birthplace, county of residence and other 1940 characteristics substantially reduces the veteran-nonveteran gap in pre-war educational background (red squares). That is, conditional selection into WWII service in terms of 1940 education and school attendance was 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 highest grade, are sharply reduced as controls are added, though the *NW* and *SE* groups' veteran coefficients tend to be smaller than those for the *US-B* and *US-W* groups in all four panels. It is notable that, while the unconditional positive coefficient for the *US-B* group is much larger than for other groups, the difference is far less aberrant after controls are added.

Figure 4 focuses on 1940 labor market outcomes, and here the results are notably different than for education. Panels (a), (c), and (e) show veteran-nonveteran differences for the younger cohorts' *fathers'* outcomes, and panels (b), (d), and (f) show 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 (blue circles). There is no such evidence for fathers' employment status except for black veterans, whose fathers were less likely to be employed than nonveterans' fathers. For the older cohorts, the selection patterns are mixed. We find unconditional positive selection based on occupational income scores for all groups (panel (d)). For log annual

wage and salary income, we see positive unconditional selection for the *US-W* and *US-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.[40]

When adding controls, we again see that conditional selection patterns, as well as differences in selection patterns between groups, are diminished compared to 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. For younger men, including controls nearly eliminates differences in fathers' wage and salary income between veterans and nonveterans (panel (a)) and in the employment probability for black men's fathers (panel (e)). For instance, in the *US-W* group, the veteran coefficient for fathers' income (panel (a)) declines from over 20 log points to only 3 log points. For the older cohorts, when including control variables, we find that veterans in all groups were *negatively* selected in terms of log income (panel (b), red squares). Importantly, comparing the *US-W* and *US-B* groups, there is no statistically significant difference in the veteran coefficients for young cohorts' fathers or older cohorts' own income. Finally, in panel (f), all groups of future veterans were less likely to be employed than future nonveterans, but the coefficient for the *US-B* group no longer stands out with the inclusion of controls.

In sum, the new data enable us to see that future veterans and nonveterans were already different from one another in 1940, though different pictures emerge for younger versus older men and for education versus labor market variables.[41] This might reflect older men's wider scope for volunteering in the war's early years, whereas most of the younger men were inducted through the Selective Service System. More research and different data would be required to investigate this hypothesis. It would be tempting to sidestep the complex selection patterns revealed here by focusing solely on younger men or men with high levels of education, but this would come at the cost of omitting most veterans from the story. We find that conditioning on geographic variables and a subset of individual background variables reduces veteran-nonveteran differences in key 1940 outcomes. This suggests that the background information in linked datasets may help address selection on observables and narrow the scope for bias in subsequent analyses, while maintaining an empirical perspective that is wide enough to include the vast majority of WWII veterans.

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[40] We also examine an alternative outcome for the older cohorts—an indicator for being employed or in school (instead of excluding those in school from the analysis of the employment outcome. The selection patterns are very similar to those for employment.

[41] Online Appendix Figure A.1 presents results for selection into service on other dimensions. Panels (a) and (b) focus on employment in agriculture. Men who were employed (or whose fathers were employed) in agriculture were unconditionally far less likely to serve, but the degree of selection is substantially reduced by the inclusion of controls. Panels (c) and (d) focus on employment in manufacturing. For young men, there is no evidence of conditional selection into service on the basis of father's employment in manufacturing, but for older men, there is evidence that employment in manufacturing was conditionally negatively associated with service probability. Panels (e) and (f) focus on mother's or spouse's years of education, controlling for all 1940 characteristics (including own and father's education). There is strong evidence of unconditional positive selection into service on mother's or spouse's education, but this selection is nearly eliminated by the inclusion of controls.

4.2 Veterans' Premia in Education circa 1950

We turn now to estimates of veteran-nonveteran differences in 1950 outcomes, conditional on the full set of 1940 observables, including whenever possible the 1940 value of the dependent variable for each individual or his father (equation 2). For educational outcomes, we study years of education completed (highest grade), 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). It is important to note that these criteria bear differently on different groups. In Figure 5, panels (c) and (f) show 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 especially the *US-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 (panel (a)). Views that concentrate on high school graduates and their college attendance, therefore, omit a large share of veterans and may deliver a distorted view of WWII and the GI Bill.

In Figure 6, we find evidence of a positive veterans' premium for highest grade completed by 1950. First, for all groups and both younger and older cohorts, we find that on average veterans had completed more education than nonveterans by 1950, conditional on their 1940 observables, including their 1940 education. The confidence intervals indicate that several of these estimates are not statistically significant, but the bulk of the evidence points to differential positive gains in schooling for veterans.[42] Second, for the older cohorts (panel (b)), the largest veterans' premia were for men at the bottom of the 1940 education distribution (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 the older cohorts. For younger men 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 surveys of veterans cited above, including the evidence of relatively high take-up rates of educational benefits by black veterans. They are also consistent with less-educated men, including high school

[42] In Online Appendix Figure A.2, we present analogous results that do not divide individuals into 1940-based educational categories. In this case, all groups' veterans' premia are positive and statistically significant. The premium for the *US-B* group is the largest for both the younger and older cohorts.

dropouts, benefiting from educational opportunities in the service or incentives to take GED exams or pursue formal education after the war.[43] It is notable that, while using a different identification strategy, Thomas (2017) finds positive impacts on high school completion measured in 1970's census data. These patterns are also consistent, however, with veterans having latent academic talents compared to nonveterans. While positive selection into service based on school attendance and education (Figure 3) might indicate differences in academic talent, the results in Figure 6 control for 1940 educational attainment directly. In this sense, such selection is addressed but only insofar as it is captured by educational outcomes before the war and other controls. It is not known whether veterans were, on average, positively selected into military service based on latent academic skill after conditioning on actual educational attainment and other observables. Even if they were, it is not automatic that latent talents would have translated into higher levels of education after the war without GI Bill support.

The relatively positive finding for black men in Figure 6 merits deeper 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 that accrued elsewhere in the educational and age distributions. We are mindful of the historical record of black veterans' difficulties in accessing education and training opportunities (Onkst 1998; Katznelson and Mettler 2008), especially in the South, and we highlight this as an area where economic historians could make additional and valuable contributions.

Figure 7 focuses on college completion. In this case, it is important to note that individuals who used the GI Bill for college attendance might not have graduated by the time of the census in 1950. For the younger cohorts (panel (a)), the estimated veterans' premium for 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 circles), though only the *US-W* group's coefficient is both substantially positive and precisely estimated at around 1 percentage point. Overall, Figure 7 shows no evidence that military service led to substantial improvements in veterans' rates of college completion by 1950, nor does it suggest that veterans fell behind their nonveteran peers despite their time in the military.[44]

Figure 8, on the other hand, 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

[43] See Olson (1974, p. 35) for discussion of how colleges accommodated veterans: "…almost all colleges and universities modified their admission policies to admit by examination veterans who had not graduated from high school and to provide those with serious educational deficiencies the opportunity to complete high school on campus or at least finish their work through correspondence courses." See Quinn (2003) on the history of the GED.
[44] Online Appendix Figure A.3 shows similarly inconclusive evidence without dividing the sample according to 1940 education.

completed high school (blue circles) compared to those whose fathers had completed 8th grade or less (green diamonds), consistent with a socioeconomic gradient to post-war re-enrollment in school even with 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 and between 2 and 5 percentage points for black men.[45] There is suggestive evidence, particularly for groups other than *US-W*, that the veterans' premia in school attendance 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 school enrollment.[46]

To shed light on whether this result reflects college enrollment, Online Appendix Figure A.5 repeats the analysis, but divides the sample according to 1950 education instead of own or father's 1940 education. Clear, and in some cases substantial, veterans' premia in school enrollment among men with more than 12 years of education, particularly for the younger cohorts, suggest that college enrollment likely played a part in these results, though there are also clear, but smaller, veterans' premia in school enrollment probability for men with less completed education. 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 were not in school in 1940, suggesting that the war was unlikely to have interrupted their education; results are virtually unchanged when the sample is limited to these men (Online Appendix Figure A.6, 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.6, panel a).

In sum, we find evidence that is consistent with modest gains in veterans' educational outcomes in 1950 relative to nonveterans, except for college completion. These patterns were most pronounced in the form of additional years of education for those at the lower end of the 1940 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 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).

[45] Online Appendix Figure A.4 confirms these results without dividing the sample according to 1940 education.
[46] 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 (US Census Bureau n.d., pp. 1-477-1-478).

4.3 Post-war Veterans' Premia in Labor Markets

Figures 9 to 13 present results for veterans' premia in labor market outcomes in 1950. 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 all available controls from 1940.

Figure 9 focuses on employment.[47] For the younger cohorts, the three groups of white men (*US-W*, *NW*, and *SE*) exhibit veterans' premia of about 3 percentage points relative to a base employment rate of about 90 percent.[48] 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. What stands out, however, is that the employment premia for black veterans are statistically significant and negative. Given that racial discrimination in post-war labor markets continued to be pronounced, this pattern may be indicative of more difficult re-entry to the civilian labor market for black veterans.[49]

Figure 10 focuses on government employment, which is of particular interest given the potential for veterans to have received hiring preferences.[50] In this analysis, we exclude members of the armed forces. We find universally positive veterans' premia, typically in the range from 2 to 4 percentage points. For both sets of birth cohorts, the largest estimated conditional veterans' premium in government employment is for the *NW* group, followed by the *US-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 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.[51] Second-generation immigrants, on the other hand, particularly those in the *NW* group, were overrepresented in local public administration.

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[47] Online Appendix Figure A.7 shows that the results are qualitatively similar for an alternative outcome—an indicator for being employed or in school in 1950.

[48] This is similar to Thomas's (2017, p. 504) estimate for employment in a pooled sample of white and black men in 1970 using an identification strategy that is akin to that in Bound and Turner (2002).

[49] 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.

[50] For instance, the 1944 Veterans' Preference Act was passed soon after the GI Bill.

[51] See Boustan and Margo (2009) on black employment with the postal service.

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).[52] While the unconditional premia are universally positive (blue circles), 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 *US-B* group) to 27 (for the *SE* group), with the *US-W* and *NW* groups having slightly larger veterans' premia than the *SE* and *US-B* groups. For the older cohorts, the estimates are positive but small (<0.5) for the *US-W* and *US-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).[53] Although the unconditional estimates of veterans' premia are positive for all groups other than the older cohorts of *NW* and *SE* men (blue circles), the conditional estimates are much smaller (red squares). 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. They may be indicative of the older veterans' foregone civilian labor market experience in their 20s (though, again, we cannot rule out a role for selection on unobservables). For these men, the GI Bill and any hiring preferences for veterans were not sufficient to deliver an income advantage relative to nonveteran peers in the labor market—on average, they lost ground. For black men, the pattern of veterans' premia coefficients is similar to that of white men, but the confidence intervals for conditional differences often include zero.

Of all of our labor market results, those for income are most comparable with other published results. Both Angrist and Krueger (1994) and Barrera et al. (2024) find a positive veterans' income premium that is reversed when time-of-birth-based identification strategies are applied. Our conditional difference estimates for older cohorts are similar in that they are also negative. Our conditional difference estimates for younger cohorts, on the other hand, remain positive, albeit small for most groups. Positive selection on unobservables is a possible explanation for our results; however, we show below that Oster (2019) bounds for these estimates are still positive. Another potential interpretation is that LATE estimates that rely on the service rate discontinuity circa the 1927 birth cohort pertain to the very youngest veterans, a subset of the group we include in our younger cohorts and, therefore, not necessarily representative of their experiences. In the big picture, we do think it is fair to conclude, based on the empirical evidence to date, that WWII service and the GI Bill did *not* greatly improve the average earnings of veterans compared to nonveterans. In this

[52] 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.
[53] Individuals with zero earned income are excluded from the sample.

regard, we concur with an emerging view of WWII veterans' outcomes that is far less rosy than that espoused by some politicians and often held in popular opinion.

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 *US-B* and *SE* groups are less negative than those for the *US-W* group, at about -2 percentage points relative to bases of about 12-13 percent for the younger cohorts and 16-19 percent for the older cohorts. Online Appendix Figure A.8 defines two alternative self-employment variables. Panels (a) and (b) focus on the probability of being self-employed and a farmer, whereas panels (c) and (d) focus on the probability of being self-employed and not a farmer. The results for self-employment as a farmer are virtually identical to the self-employment results as a whole, implying that the results in Figure 13 are driven by farmers.

On the whole, our analysis of veterans' premia in labor market outcomes in 1950 presents a mixed picture. White veterans held some advantages in the likelihood of employment relative to observationally similar nonveterans, but black veterans did not. Veterans in all groups had higher rates of 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 lower rates of self-employment. The veterans' premia in labor markets 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.

4.4 Bounding Exercises in the Presence of Selection on Unobservables

As discussed above, even when controlling for all available 1940 characteristics in measuring the 1950 veterans' premia, we cannot rule out the possibility that differences between veterans and nonveterans were the product of unobservable differences, such as educational aptitude or health. We can make additional headway in understanding the potential role for remaining unobservables in driving our findings. Table 2 presents the results of an Oster (2019)-type bounding exercise, in which the impact of addressing selection on observables on the estimated veterans' premia is informative of the remaining potential for selection on unobservables to affect our estimates, under the assumption that selection on observables is more important than selection on unobservables. For each group, we estimate an adjusted version of equation (2). To make the estimation tractable, we control for 1940 state-of-residence fixed effects instead of 1940 county-of-residence fixed effects. For each outcome, the first row of results in Table 2 presents the estimated veterans' premium and the second row presents the bound on the estimated veterans' premium following Oster's (2019) recommended choice of $R_{max} = 1.3\,\tilde{R}$, where $\tilde{R}$ is the R-squared of the regression including all

controls. Educational outcomes are presented for the pooled sample (i.e., not divided according to the 1940 education of an individual or his father).

While these results are not dispositive, they can address concerns that our results are the product of selection into military service on unobservables. In almost all cases, the sign of the estimated coefficient is the same as the sign of the bound, meaning that, under the Oster (2019) assumptions, addressing additional unobserved selection into service would not reverse the direction of the estimated veterans' premium. For the younger cohorts (columns 1-4), there is no systematic evidence of such reversal. For the older cohorts (columns 5-8), there is evidence of a qualitatively important systematic reversal only for years of education.[54] For all three white groups, the Oster (2019) bounds cannot rule out a reversal of the sign of the veterans' premium, whereas for the *US-B* group we are able to rule out such a reversal.

Altogether, although they do not enable us to rule out the possibility that selection on unobservables is responsible for the veterans' premia that we document, these results do strengthen the case that such selection is probably not large enough to change our qualitative conclusions, with the exception of the educational attainment of the older cohorts of white men.

5. Conclusion

World War II was a watershed event in American economic history. 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 and the country's overall prosperity. Yet whether the men who served in WWII benefited from this experience and the GI Bill, and if so, the degree to which men of various ethnic, racial, and age groups benefited differently remains a subject of debate. Given the popular salience of the GI Bill's perceived impacts, it is important for scholars to work towards a clearer understanding of the post-war economy, veterans' transitions into civilian careers, and role of policy in supporting these transitions.

In this paper, we take advantage of newly available complete-count microdata from the 1950 census of population, which are linked to individual records in the 1940 census, to study patterns of selection into military service and to see how the lives of men who were observationally similar in 1940 diverged depending on whether they served in WWII. The essential strength of this approach is that we can directly observe millions of men before the United States entered WWII, and we can observe the same men after the war in civilian life. The picture that emerges from our analysis is that men were positively selected into service in terms of 1940 education, but that selection was mixed in terms of their own or their fathers' 1940 labor market outcomes. This view of selection includes both older and younger cohorts of men, and it distinguishes by race and ethnicity of second-generation immigrants. This breadth is novel to the literature, as is the complexity of the selection patterns that we uncover.

[54] We also find a reversal for college attendance, but because both our coefficients and the bounds are close to zero, this is not a qualitatively important reversal.

Relying on the rich pre-war information to sharpen within-cohort comparisons of veterans and nonveterans and reduce selection bias, we measure veterans' premia in various educational and labor market outcomes in 1950. Given that there is not random assignment to military service, we do not interpret our results as the causal effects of WWII service and the GI Bill. But we do think these close comparisons are informative and transparent, and they cover the full range of birth cohorts with significant military participation. Conditioning on 1940 characteristics, we find modest positive veterans' premia for educational outcomes in 1950 (other than college completion), slightly positive veterans' premia in labor market outcomes for the younger cohorts, and slightly negative veterans' premia in labor market outcomes for the older cohorts. For the most part, we find little evidence of systematic differences in veterans' premia across the four ethnic and racial groups that we study. Oster (2019) bounds suggest that there is limited scope for bias from unobservables, though as always in such frameworks, it cannot be ruled out.

At least one important aspect of our findings calls for additional research. In our data, black veterans with lower education levels before the war 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 for black men (Onkst 1998; 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 positive effects at the individual level smaller in aggregate than they would otherwise have been (i.e., if more black men had served). Even so, most black men born in the early to mid-1920s did serve, and the newly released 1950 census data will enable scholars to reassess the role of WWII in mid-century labor market disparities (Weaver 1945; Fredland and Little 1985; Maloney 1994; Margo 1995; Collins 2000, 2001; Teachman and Tedrow 2004; Ferrara 2022). At the same time, black veterans' relatively low rate of employment in 1950 demands more investigation of how transitions to the civilian labor market varied by race in a context where racial discrimination was common and wartime anti-discrimination measures had lapsed.

Our findings of mostly small veteran-nonveteran differences in outcomes circa 1950, with some notable differences according to age, align with the idea that the GI Bill compensated veterans for the war's interruption of their educations and careers. It was not designed to overhaul the American labor force or jumpstart intergenerational mobility. It may have provided a way back on track for many of the men involved, but for most, it was not a springboard to a new and previously unattainable set of career opportunities.[55] Of course, there is little doubt that some veterans' lives were in fact dramatically and favorably altered—it is not difficult to find testimonials to this—but there is also little doubt that many veterans lost critical years of work experience, endured the

[55] The case for large social impacts from the GI Bill might be stronger if writing from the perspective of how colleges (or attitudes about college) changed in response to the large influx of men after the war (e.g., Abramitzky et al. 2024), or from a macroeconomic perspective in which the GI Bill's provisions smoothed the re-entry of millions of men into the civilian labor force.

dislocation and damage of war, and found little or no benefit from the GI Bill upon their return. On average, US veterans fared well in the decades that followed the war, earning the moniker "the Greatest Generation." In retrospect, we see that other men in their cohorts fared just about as well. They all rode and propelled a long wave of widely shared economic growth (Goldin and Katz 2007).

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Figures

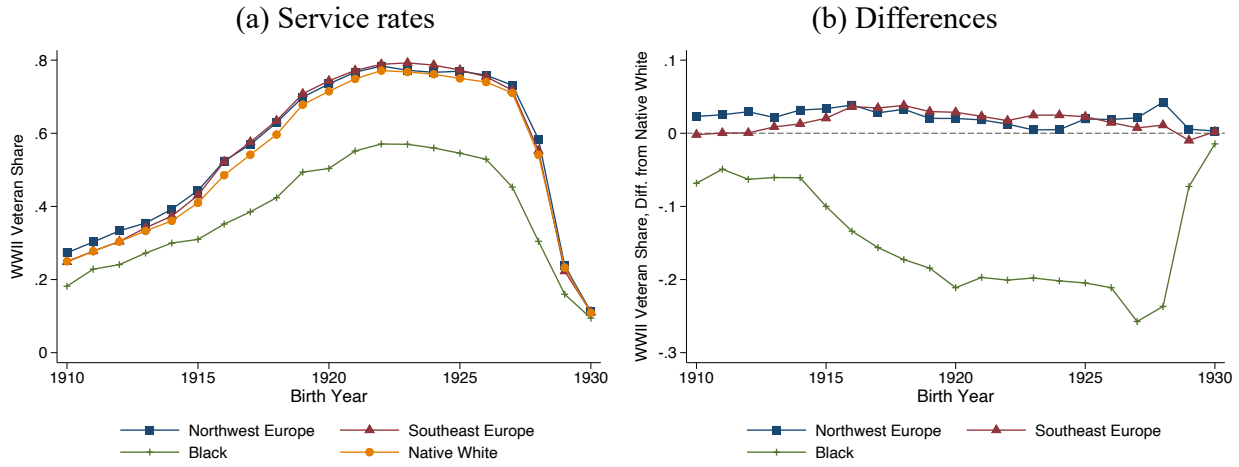
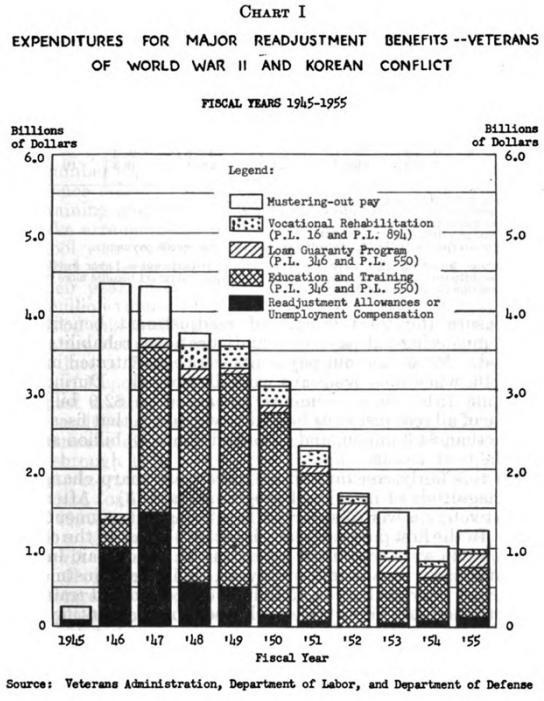


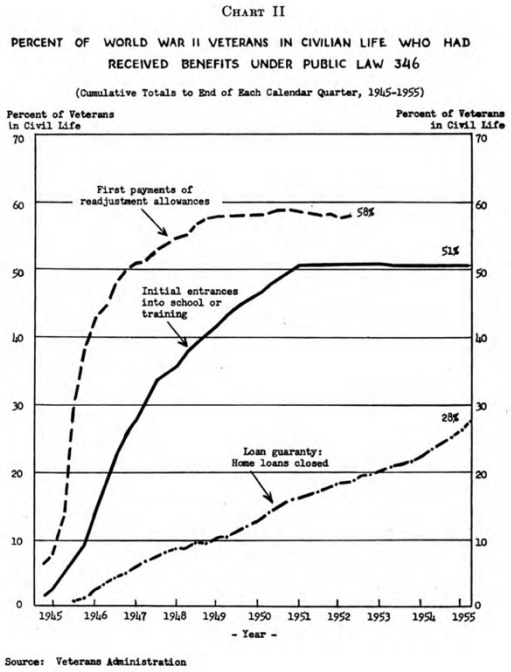
Figure 1: World War II service rates by race, ethnicity, and birth cohort

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

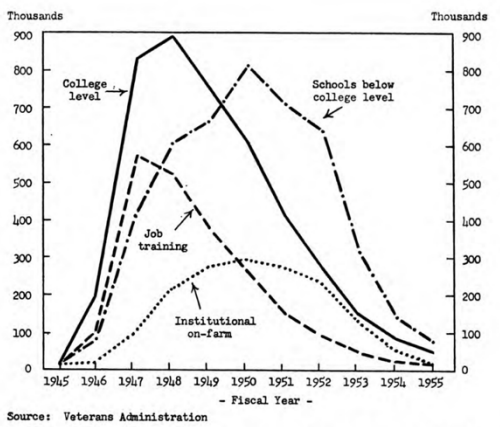


Figure 2: Administrative data on GI Bill implementation

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).

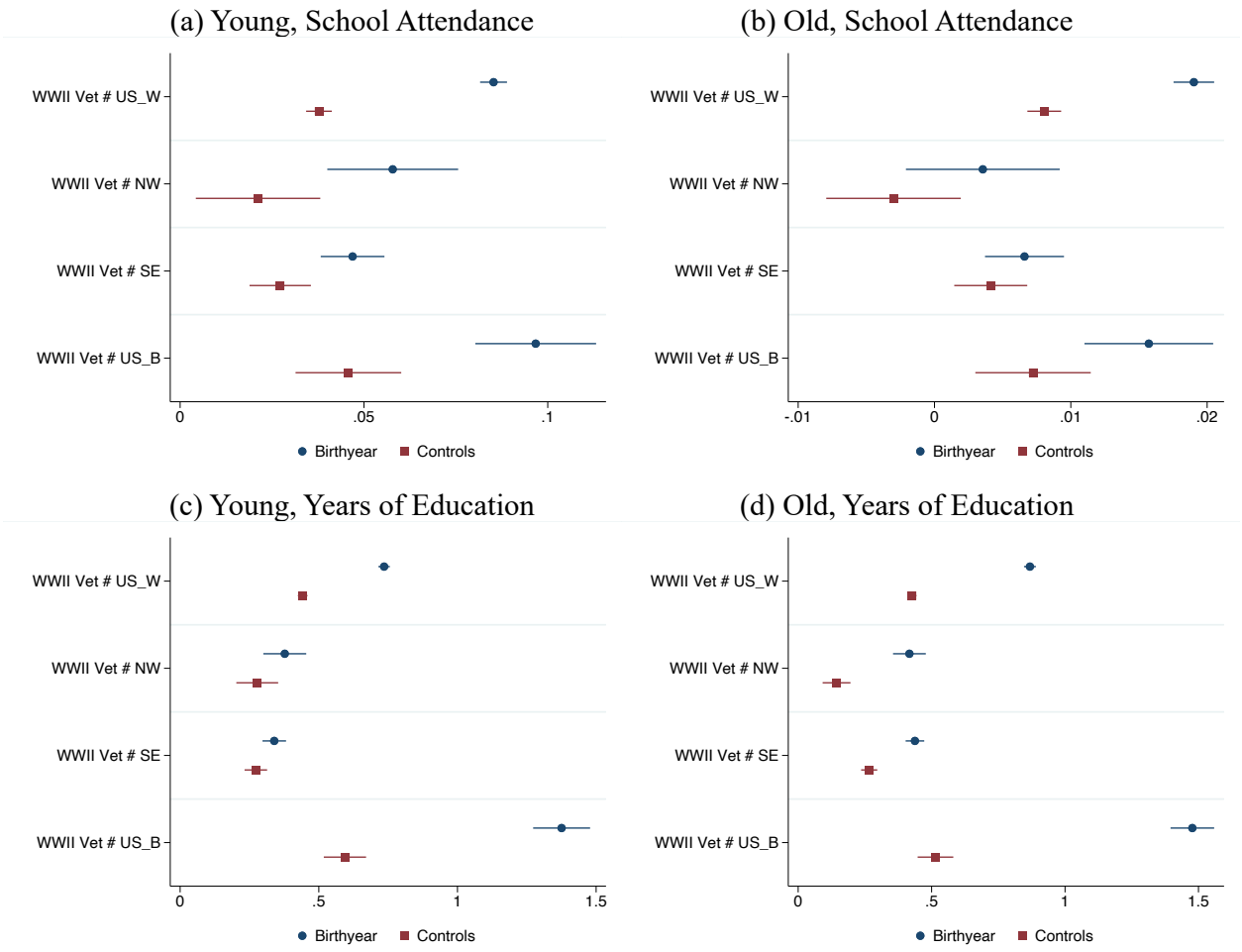
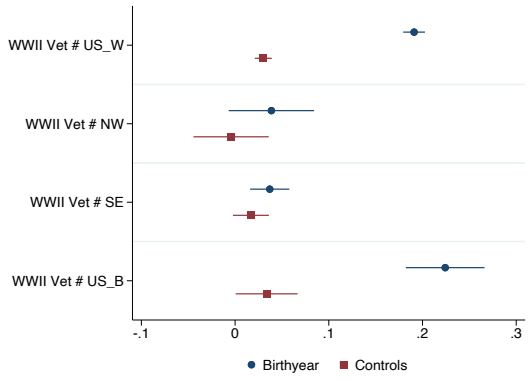


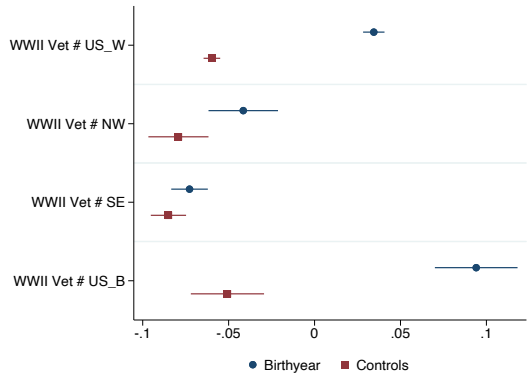
Figure 3: Selection into Service, Own Education

Notes: These figures present estimates of α_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 "controls," controls for group-by-birthplace and group-by-residence county fixed effects and 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.

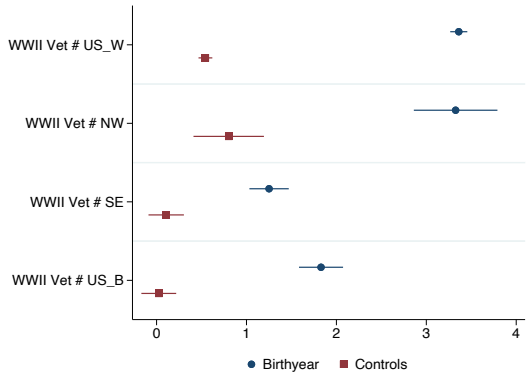
(a) Young, Father's log(Wage and Salary Income)



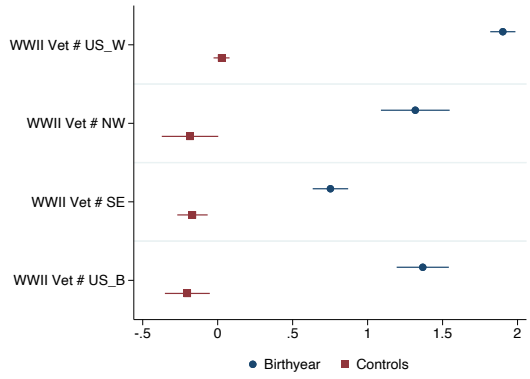
(b) Old, Own log(Wage and Salary Income)



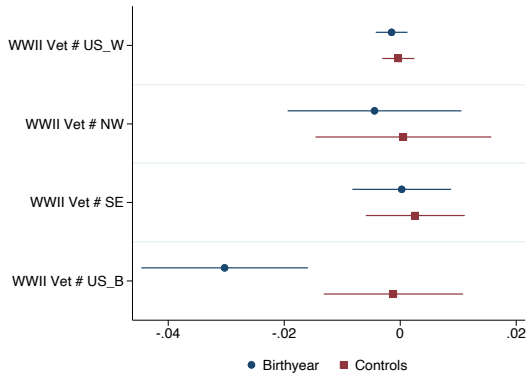
(c) Young, Father's Occupational Status



(d) Old, Own Occupational Status



(e) Young, Father's Employment



(f) Old, Own Employment

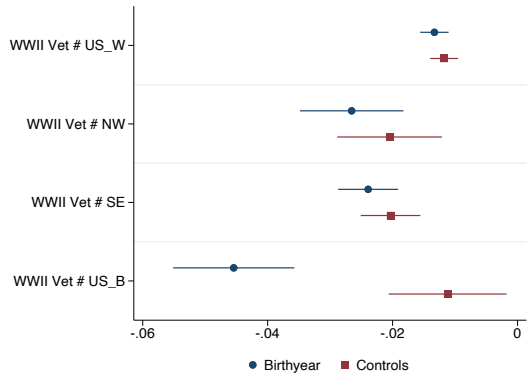
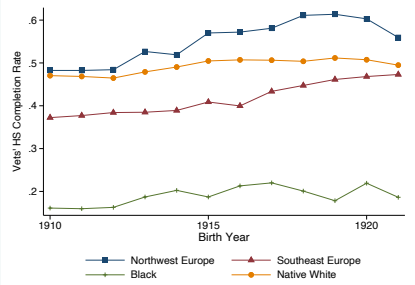


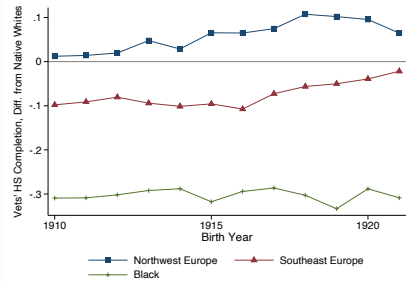
Figure 4: Selection into Service, Labor Market

Notes: These figures present estimates of α_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 "controls," controls for group-by-birthplace and group-by-residence county fixed effects and all other observables in 1940 other than those that might confound the interpretation of the outcomes (e.g., we do not control for other labor market 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).

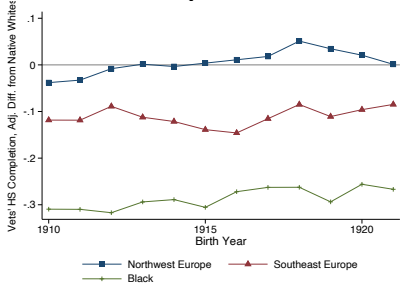
(a) HS completion, old



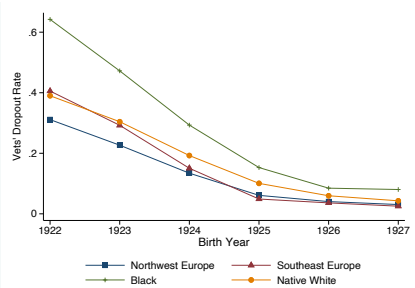
(b) Diffs. in HS completion, old



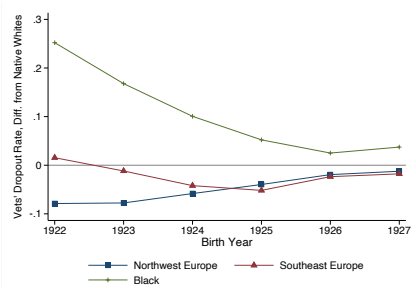
(c) Adj. diffs. in HS completion, old



(d) Dropout rates, young



(e) Diffs. in dropout rates, young



(f) Adj. diffs. in dropout rates, young

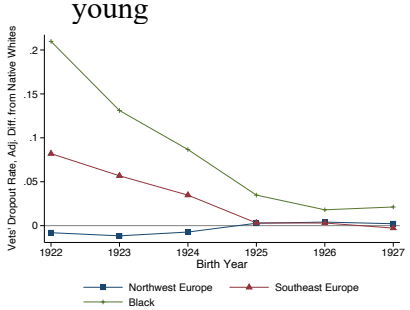


Figure 5: School completion and attendance rates in 1940

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).

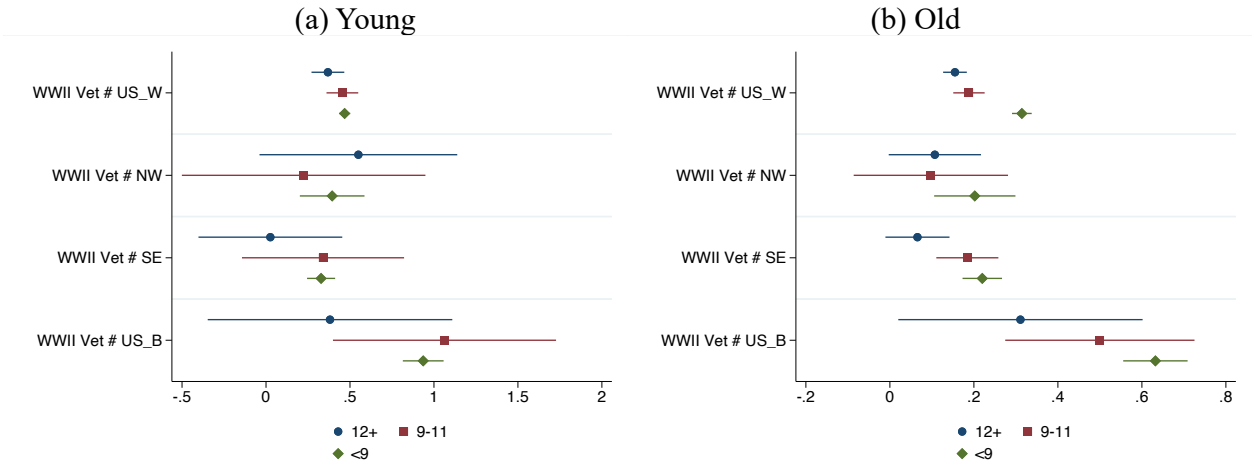


Figure 6: Veterans' Premia in 1950 Years of Education

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.

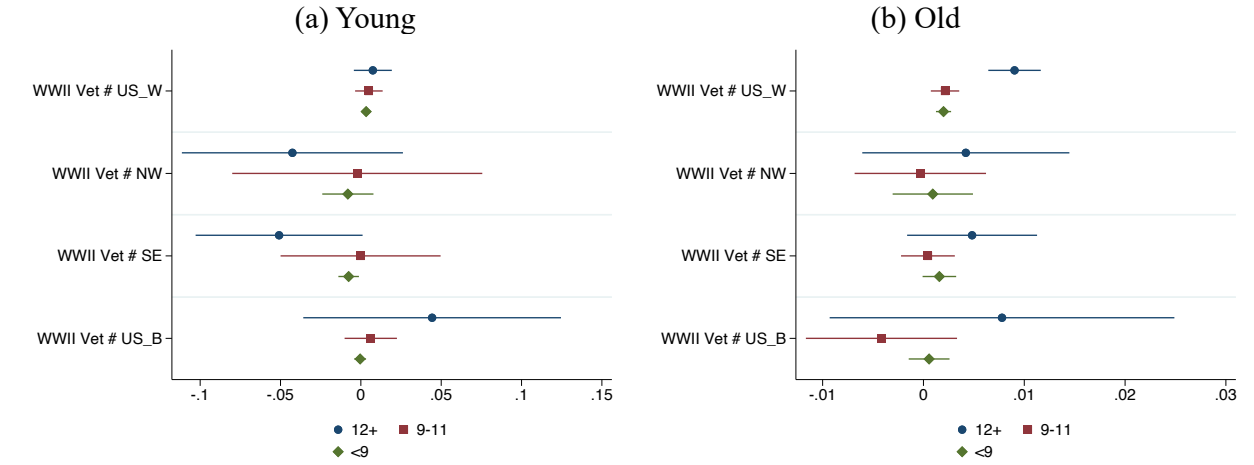


Figure 7: Veterans' Premia in 1950 College Completion

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.

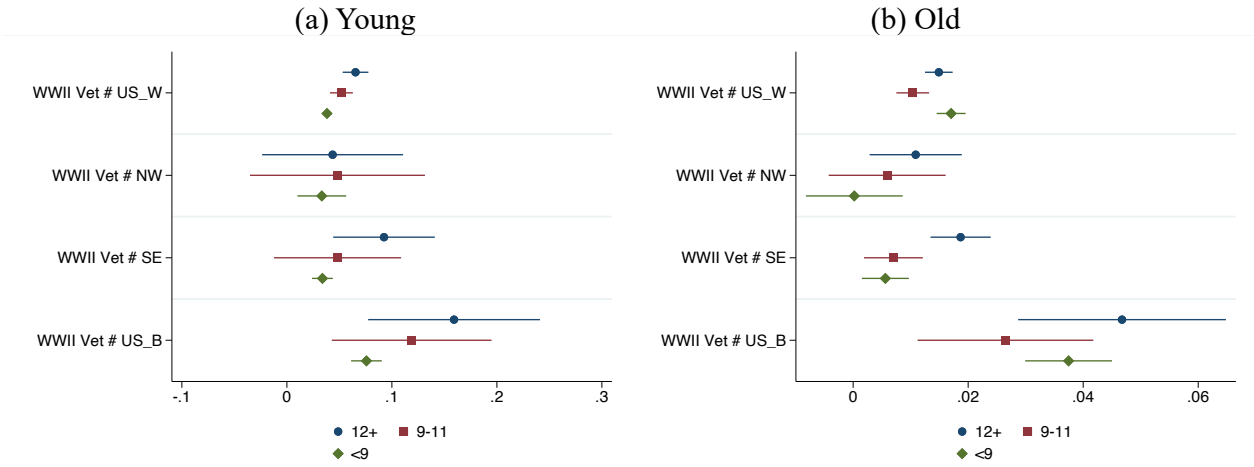


Figure 8: Veterans' Premia in 1950 School Attendance

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.

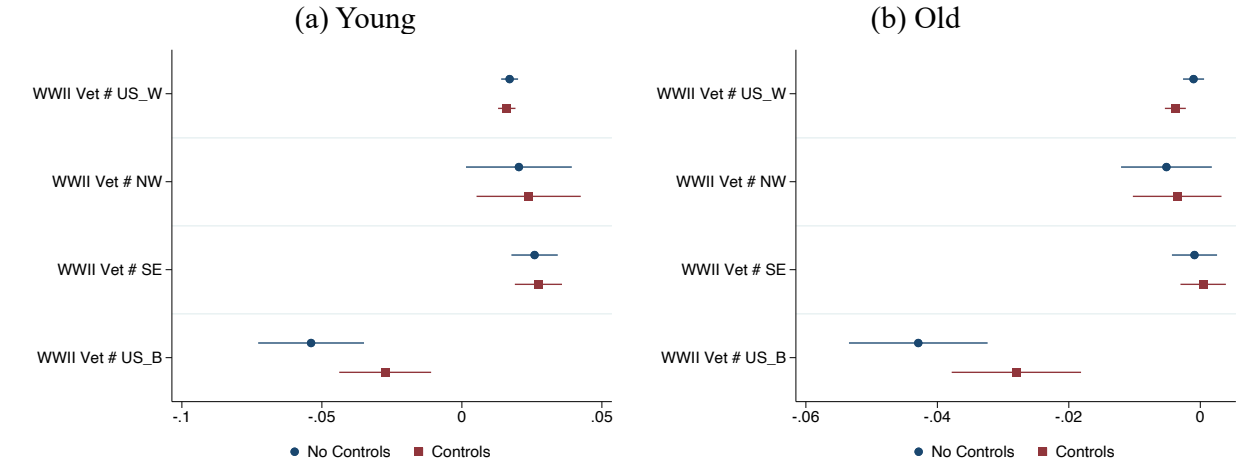


Figure 9: Veterans' Premia in 1950 Employment

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.

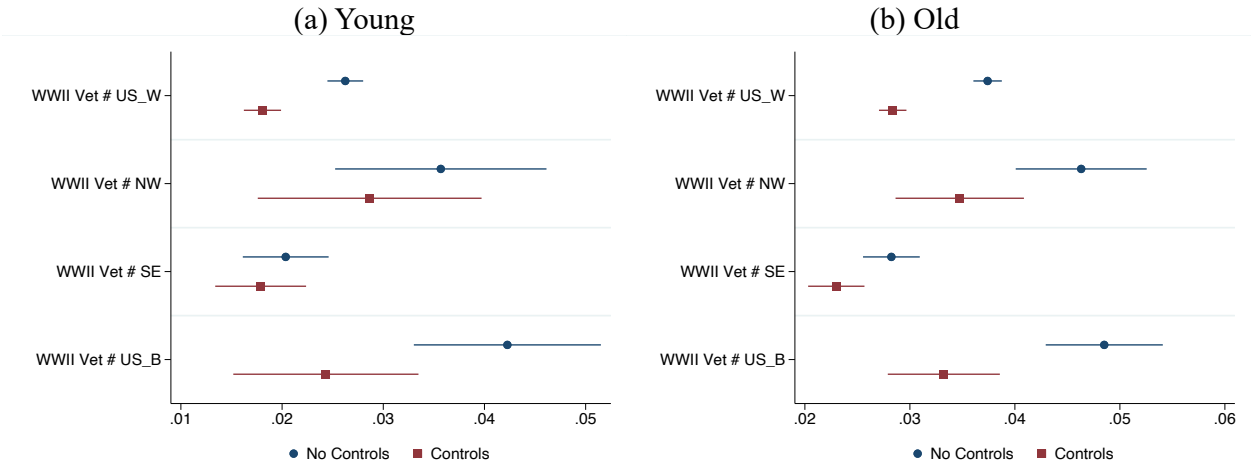


Figure 10: Veterans' Premia in 1950 Government Employment

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.

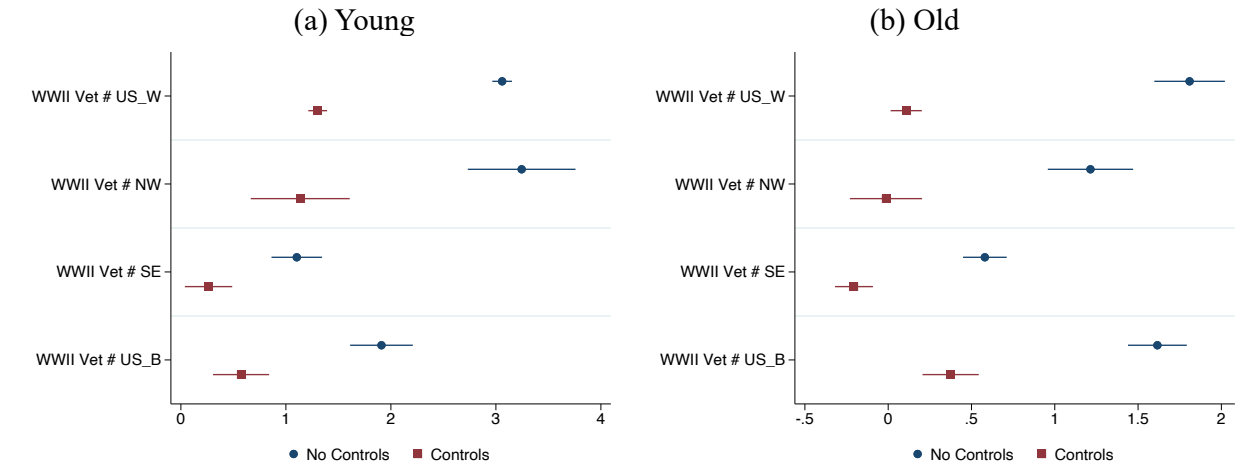


Figure 11: Veterans' Premia in 1950 Occupational Status

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.

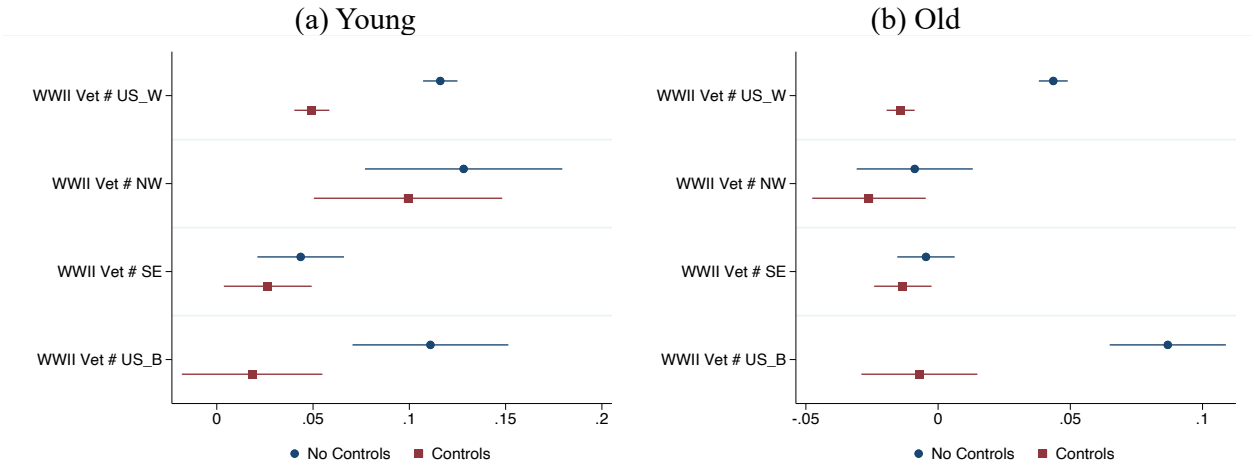


Figure 12: Veterans' Premia in 1950 log(Earned Income)

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.

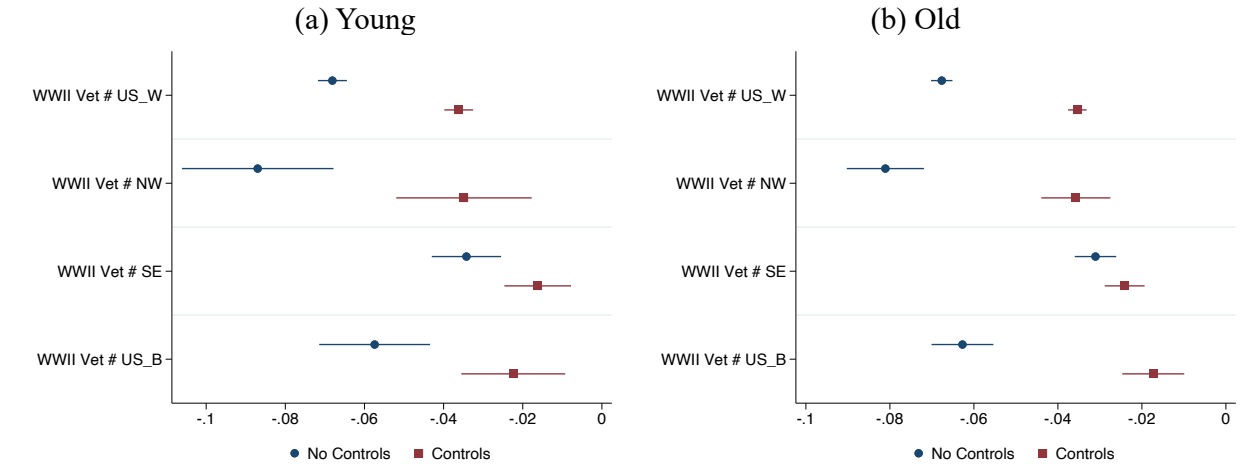


Figure 13: Veterans' Premia in 1950 Self Employment

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
US-B	Black sons of men born in the United States	30,752	60,027
	Total	486,726	1,022,225

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.

Table 2: Oster (2019) bounds

	Young				Old			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variable	US-W	NW	SE	US-B	US-W	NW	SE	US-B
Years of Education	0.478	0.451	0.323	0.899	0.215	0.154	0.152	0.579
	0.204	0.293	0.220	0.467	−0.081	−0.025	−0.034	0.174
College	0.003	−0.009	−0.011	−0.002	0.005	0.002	0.001	0.000
	−0.008	−0.018	−0.016	−0.005	−0.007	−0.005	−0.004	−0.003
School	0.045	0.042	0.038	0.080	0.015	0.009	0.011	0.037
	0.040	0.034	0.037	0.075	0.007	0.003	0.004	0.031
Employment	0.016	0.027	0.028	−0.033	−0.004	−0.003	0.000	−0.027
	0.015	0.028	0.027	−0.026	−0.004	−0.001	0.003	−0.019
Government Employment	0.018	0.023	0.018	0.024	0.029	0.036	0.023	0.033
	0.014	0.019	0.017	0.017	0.027	0.034	0.022	0.026
Occupational Status	1.337	1.303	0.278	0.641	0.134	−0.040	−0.199	0.395
	0.697	0.597	−0.018	0.151	−0.369	−0.367	−0.332	−0.012
log(Earned Income)	0.053	0.101	0.028	0.018	−0.012	−0.026	−0.012	−0.005
	0.028	0.087	0.019	−0.017	−0.026	−0.025	−0.009	−0.033
Self Employment	−0.037	−0.037	−0.020	−0.026	−0.037	−0.037	−0.025	−0.019
	−0.027	−0.021	−0.016	−0.014	−0.023	−0.019	−0.017	−0.001

Notes: For each variable, the first row presents the estimated veterans' premium for that group and outcome when controlling for all 1940 observables. The second row presents the bound on the estimated veterans' premium following Oster's (2019) recommended choice of $R_{max} = 1.3\,\tilde{R}$.

Online Appendix for

World War II Service and the GI Bill
New Evidence on Selection and Veterans' Outcomes from Linked Census Records

William J. Collins and Ariell Zimran
Vanderbilt University and NBER

December 2024

A. Additional Tables and Figures

Table A.1: Summary statistics for the 1922-1927 cohorts

Variable	(1) US-W	(2) NW	(3) SE	(4) US-B
Panel A: Service				
Veteran	0.748	0.770	0.773	0.540
	(0.434)	(0.421)	(0.419)	(0.498)
Panel B: 1940 Education				
School	0.740	0.784	0.754	0.623
	(0.438)	(0.412)	(0.430)	(0.485)
Years of Education	8.271	8.900	8.795	6.055
	(2.359)	(2.020)	(2.082)	(2.827)
Panel C: 1940 Father's Labor Market				
log(Wage Income)	6.961	7.183	6.883	6.221
	(0.853)	(0.709)	(0.694)	(0.806)
Occscore	24.637	25.550	25.613	17.639
	(10.629)	(9.429)	(9.006)	(6.557)
Panel D: 1950 Education				
College Completion	0.078	0.092	0.076	0.015
	(0.268)	(0.289)	(0.264)	(0.120)
Years of Education	10.380	11.112	10.582	7.520
	(3.615)	(3.409)	(3.471)	(3.625)
School	0.205	0.230	0.207	0.189
	(0.403)	(0.421)	(0.405)	(0.392)
Panel E: 1950 Labor Market				
log(Earned Income)	7.550	7.639	7.618	7.203
	(0.836)	(0.807)	(0.814)	(0.884)
Occscore	25.456	26.396	27.017	20.013
	(9.360)	(9.132)	(8.651)	(7.180)
Employment	0.913	0.906	0.892	0.820
	(0.281)	(0.292)	(0.311)	(0.384)
Government Employment	0.056	0.062	0.042	0.052
	(0.230)	(0.240)	(0.202)	(0.222)
Self Employment	0.143	0.120	0.117	0.127
	(0.350)	(0.325)	(0.322)	(0.333)

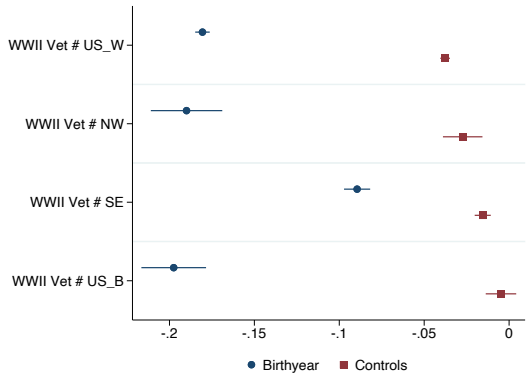
Notes: Sample limited to native-born men in the 1922-1927 birth cohorts who could be linked between the 1940 and 1950 censuses. The sample is reweighted to correct for selection into linkage. Labor force variables exclude men in school in 1950. Individuals with no wage are excluded from the income variables, and those with no occupational income score are excluded from the occupational status variables.

Table A.2: Summary statistics for the 1910-1921 cohorts

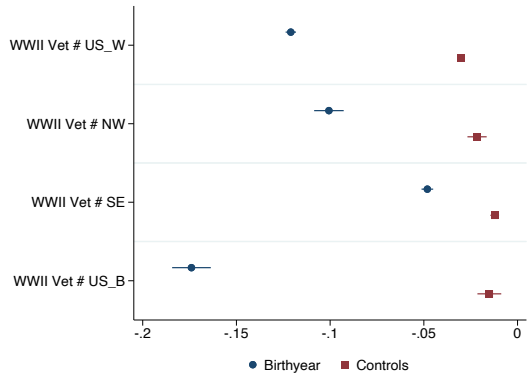
Variable	(1) US-W	(2) NW	(3) SE	(4) US-B
Panel A: Service				
Veteran	0.481	0.482	0.496	0.349
	(0.500)	(0.500)	(0.500)	(0.477)
Panel B: 1940 Education				
School	0.069	0.065	0.058	0.036
	(0.254)	(0.247)	(0.234)	(0.186)
Years of Education	10.167	10.847	10.216	6.777
	(3.051)	(2.564)	(2.713)	(3.501)
Panel C: 1940 Labor Market				
log(Wage Income)	6.460	6.693	6.622	5.891
	(0.882)	(0.816)	(0.808)	(0.840)
Occscore	22.355	23.915	24.615	17.077
	(9.502)	(9.167)	(8.894)	(7.060)
Panel D: 1950 Education				
College Completion	0.074	0.087	0.070	0.019
	(0.262)	(0.282)	(0.256)	(0.136)
Years of Education	9.878	10.523	9.878	6.774
	(3.632)	(3.435)	(3.447)	(3.618)
School	0.059	0.051	0.053	0.064
	(0.236)	(0.220)	(0.223)	(0.244)
Panel E: 1950 Labor Market				
log(Earned Income)	7.707	7.803	7.791	7.298
	(0.864)	(0.841)	(0.826)	(0.871)
Occscore	27.036	28.083	28.629	20.788
	(10.328)	(9.839)	(9.611)	(7.208)
Employment	0.939	0.937	0.934	0.869
	(0.239)	(0.243)	(0.248)	(0.337)
Government Employment	0.058	0.068	0.049	0.044
	(0.234)	(0.251)	(0.216)	(0.205)
Self Employment	0.217	0.188	0.196	0.163
	(0.412)	(0.391)	(0.397)	(0.369)

Notes: Sample limited to native-born men in the 1910-1921 birth cohorts who could be linked between the 1940 and 1950 censuses. The sample is reweighted to correct for selection into linkage. Labor force variables exclude men in school in 1950. Individuals with no wage are excluded from the income variables, and those with no occupational income score are excluded from the occupational status variables.

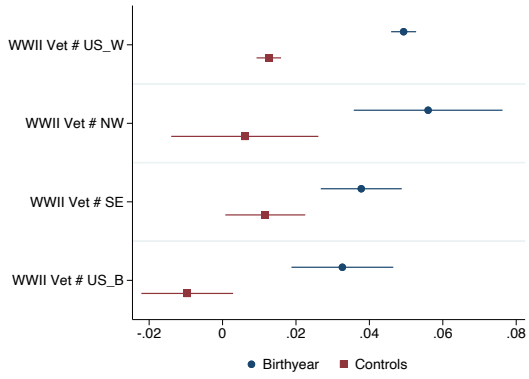
(a) Young, Father's Employment in Agriculture



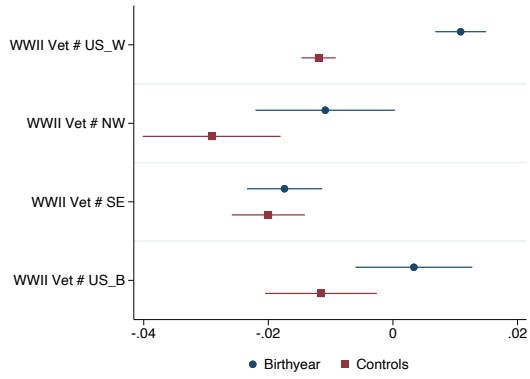
(b) Old, Own Employment in Agriculture



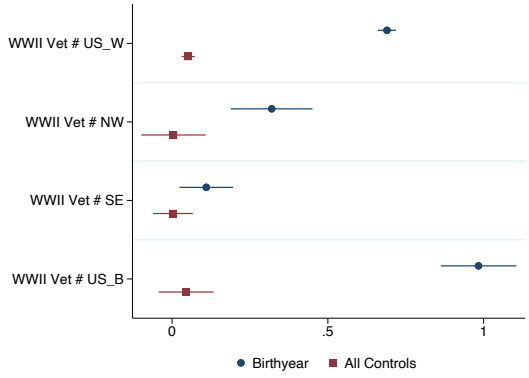
(c) Young, Father's Employment in Manufacturing



(d) Old, Own Employment in Manufacturing



(e) Young, Mother's Education



(f) Old, Spouse's Education

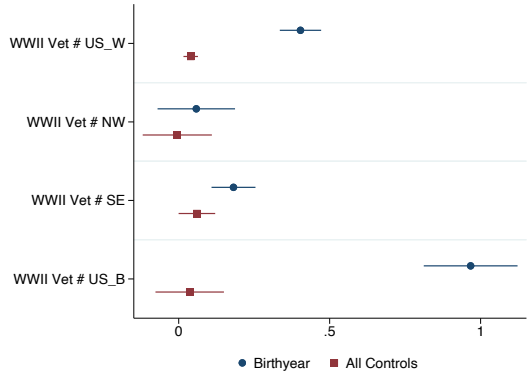


Figure A.1: Selection into Service, Additional Characteristics

Notes: These figures present estimates of α_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 (or mothers in panel e). The first set of estimates, labeled "birthyear," control only for group-by-birthyear indicators. The second set, labeled "controls," controls for group-by-birthplace and group-by-residence county fixed effects and all other observables in 1940 other than those that might confound the interpretation of the outcomes. Bars indicate 95-percent confidence intervals. Groups are indicated on the *y*-axis using the same group abbreviations introduced in text.

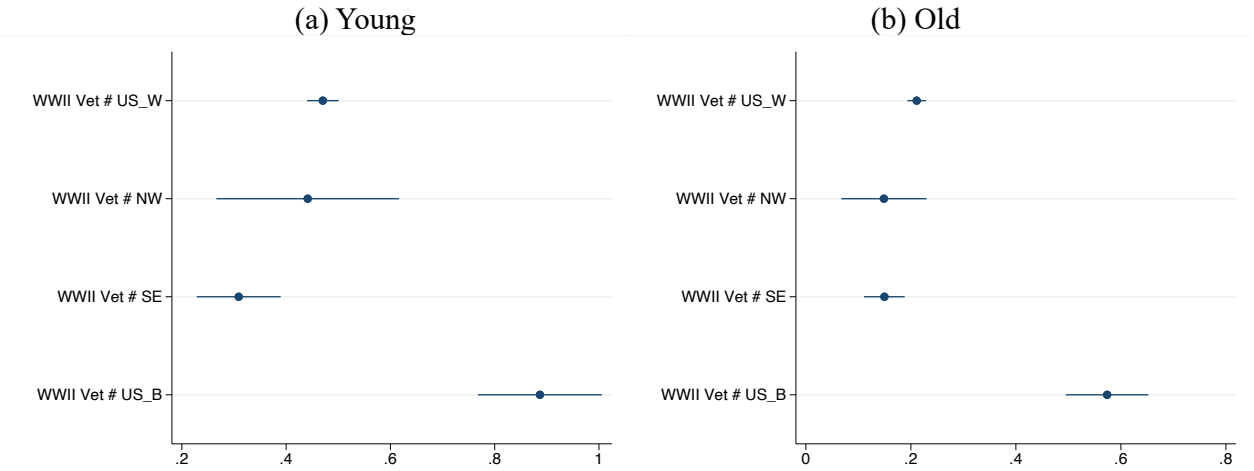


Figure A.2: Veterans' Premia in 1950 Years of Education

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.

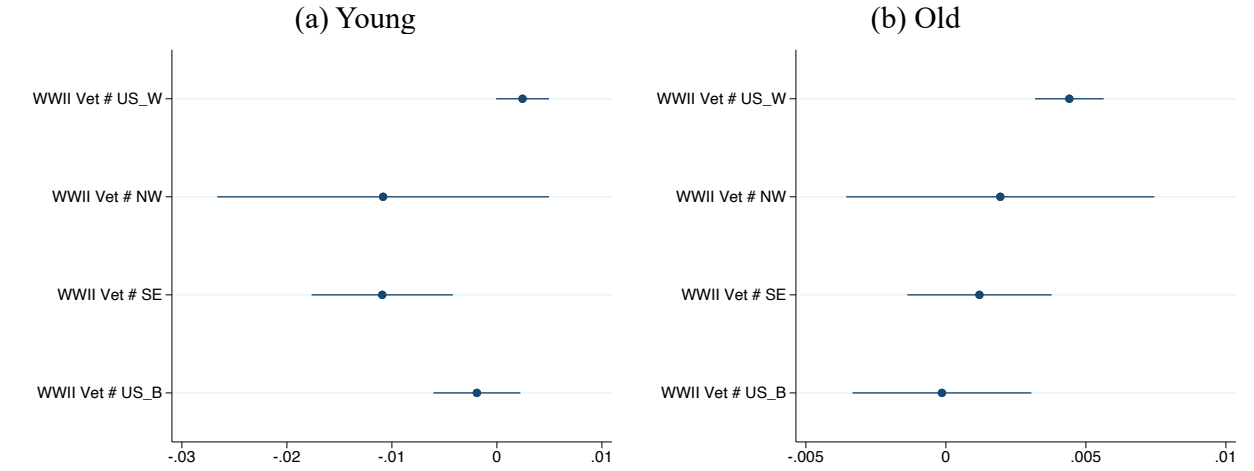


Figure A.3: Veterans' Premia in 1950 College Completion

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.

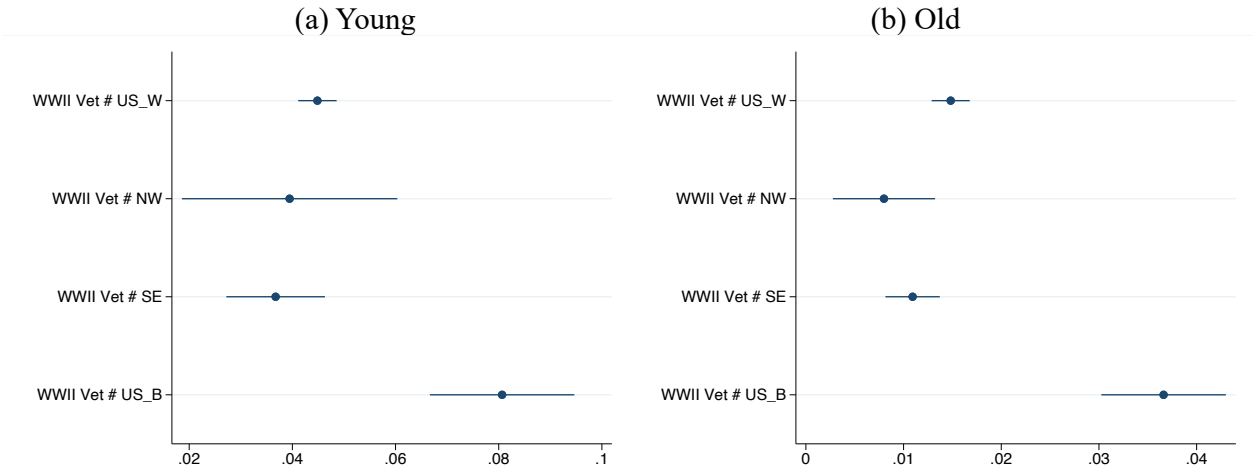


Figure A.4: Veterans' Premia in 1950 School Attendance

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.

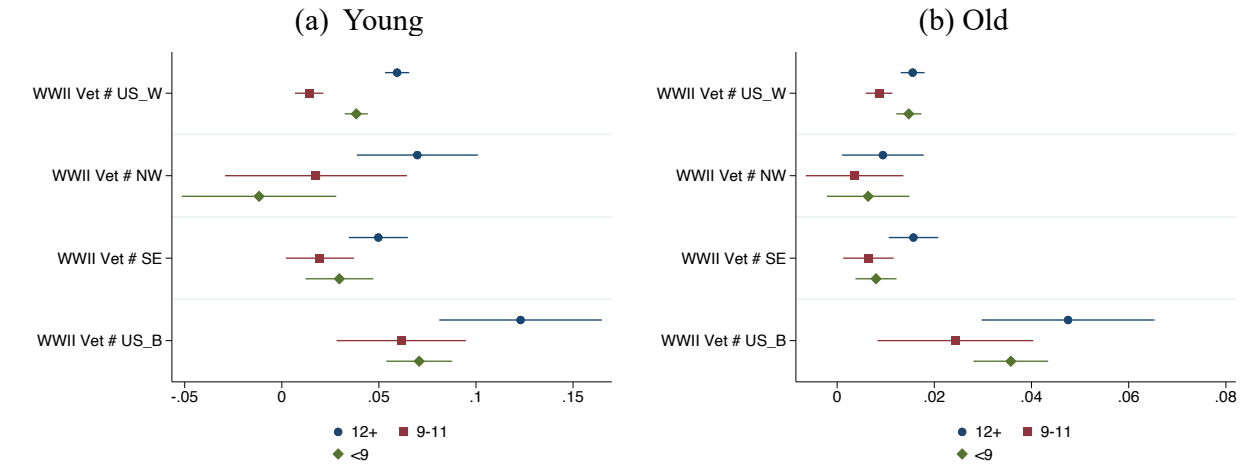


Figure A.5: Additional School Enrollment Results by Own 1950 Education

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 individuals' 1950 education.

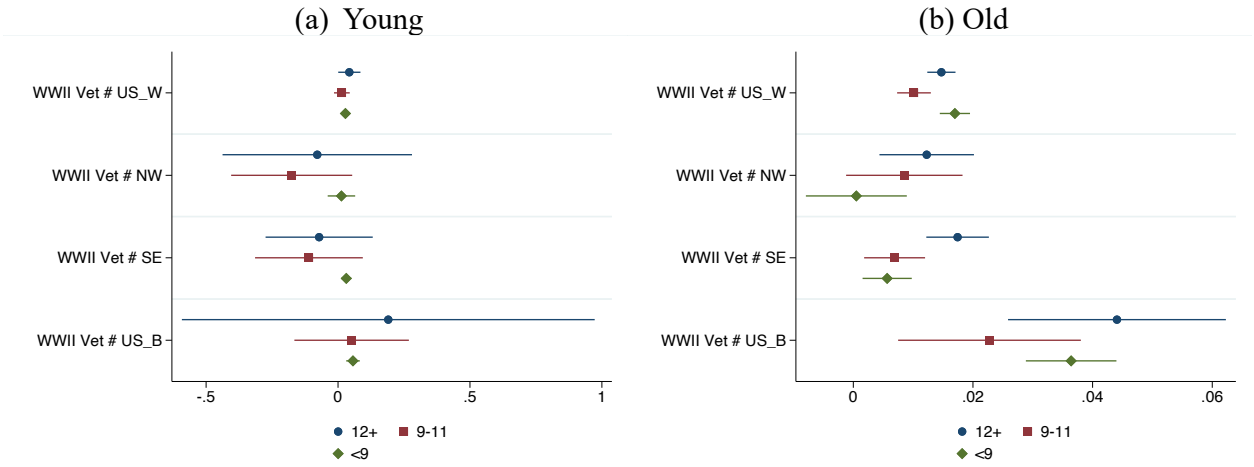


Figure A.6: Additional School Enrollment Results for Men Not in School in 1940

Notes: Sample restricted to men who were not in school in 1940. 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.

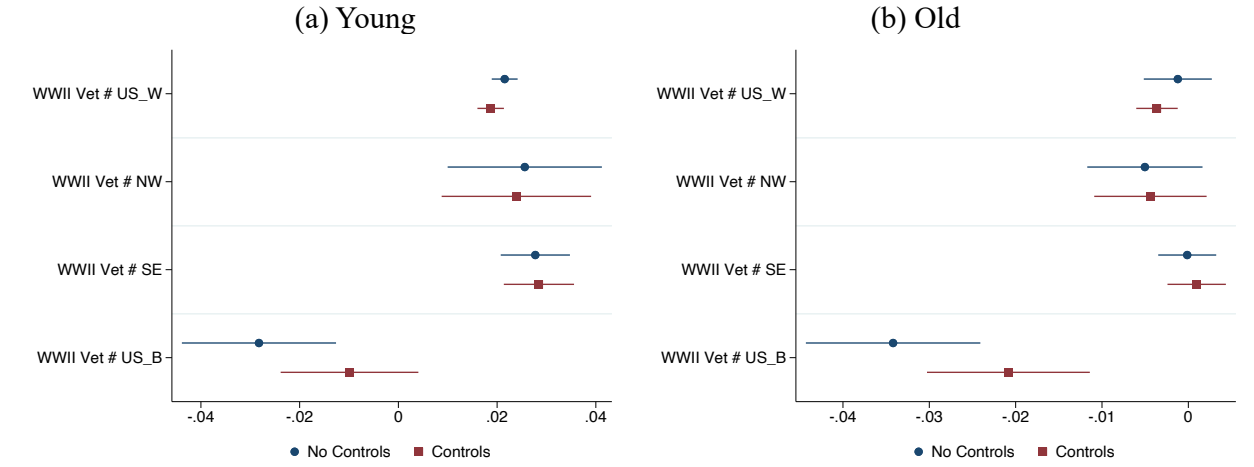


Figure A.7: Veterans' Premia in 1950 Employment or School Attendance

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. 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 A.3: Industry distribution of veterans in government employment, older cohorts

Industry	US-W	NW	SE	US-B	Total
Postal Service	22.52	25.66	30.25	40.31	25.28
Federal Public Administration	43.78	30.06	34.84	44.32	41.72
State Public Administration	8.93	7.65	7.13	2.07	8.04
Local Public Administration	24.39	36.45	27.54	12.87	24.61
Public Administration	0.38	0.17	0.24	0.43	0.35

Notes: This table presents the distribution for each ethnic or racial group across specific occupations of veterans from the older cohorts in government employment (excluding members of the armed forces) in 1950. Each column sums to 100%.

Table A.4: Industry distribution of veterans in government employment, younger cohorts

Industry	US-W	NW	SE	US-B	Total
Postal Service	20.60	27.37	30.41	37.46	23.47
Federal Public Administration	42.78	29.18	35.49	48.71	42.03
State Public Administration	8.86	7.66	6.78	2.59	8.01
Local Public Administration	27.38	35.41	26.99	10.81	26.11
Public Administration	0.38	0.38	0.34	0.43	0.38

Notes: This table presents the distribution for each ethnic or racial group across specific occupations of veterans from the older cohorts in government employment (excluding members of the armed forces) in 1950. Each column sums to 100%.

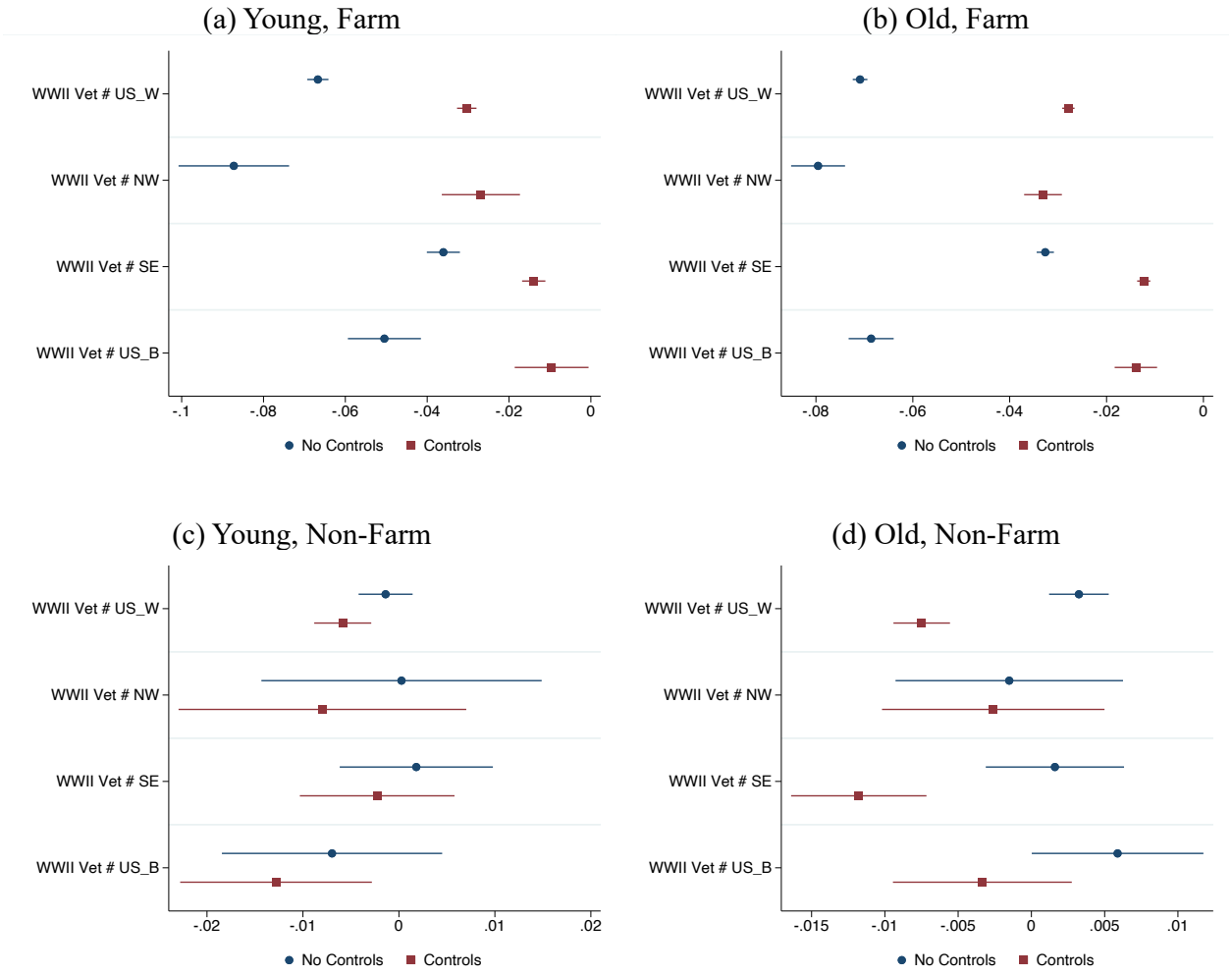


Figure A.8: Veterans' Premia in 1950 Self Employment, By Type

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.

B. Results with Strict Links

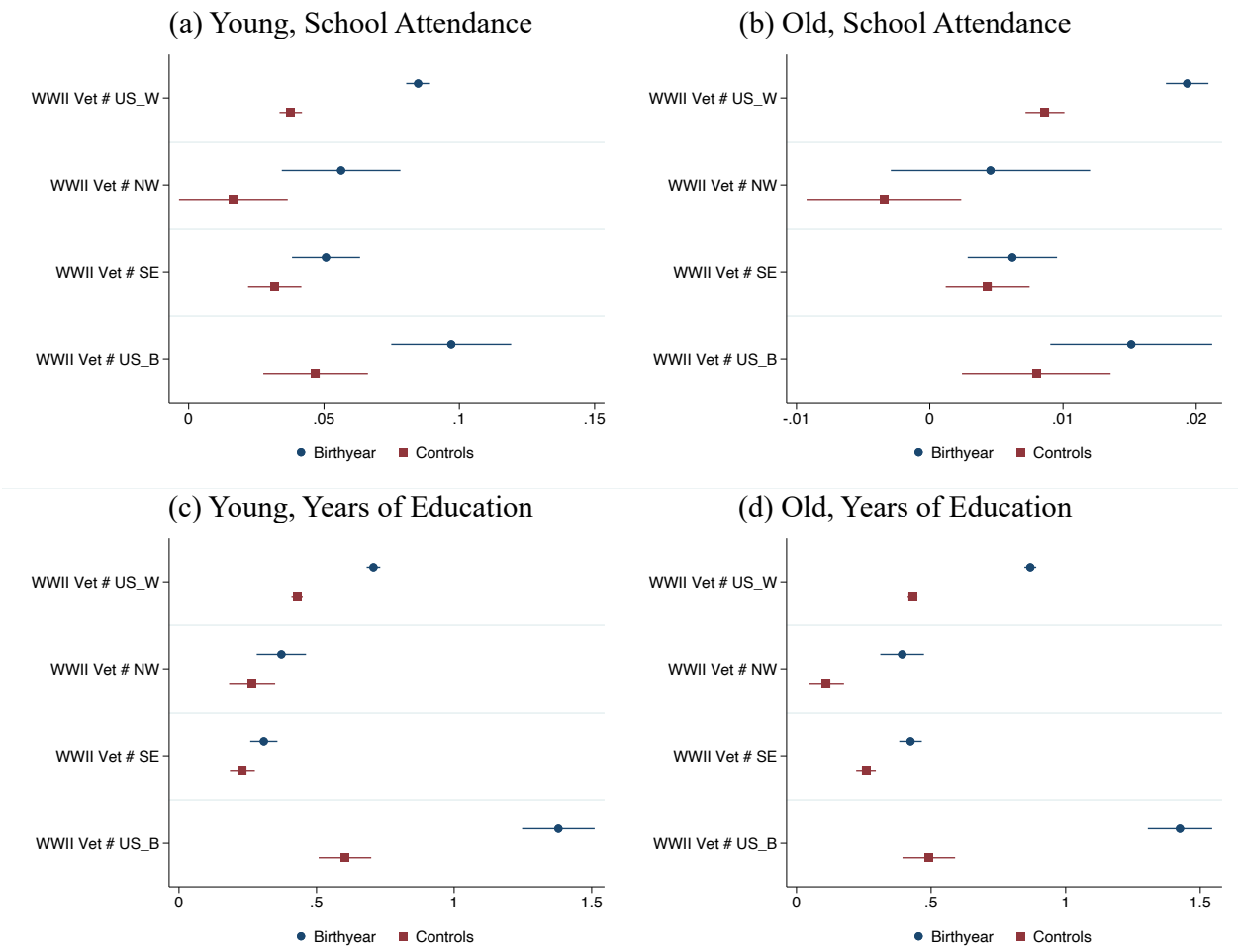
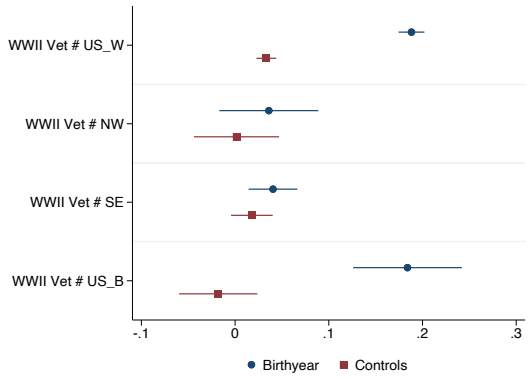


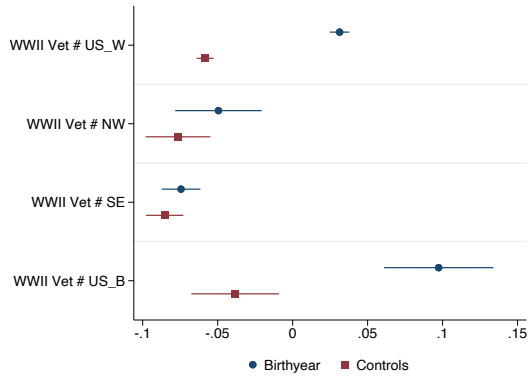
Figure B.1: Selection into Service, Own Education

Notes: These figures present estimates of α_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 "controls," controls for group-by-birthplace and group-by-residence county fixed effects and 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.

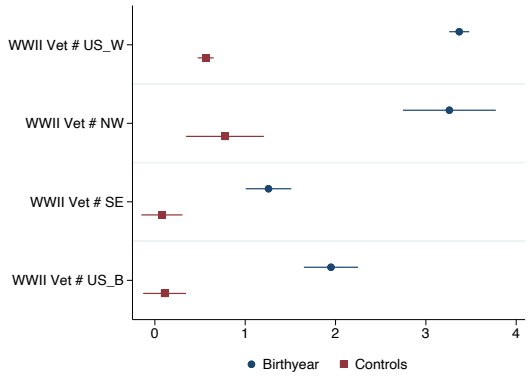
(a) Young, Father's log(Wage and Salary Income)



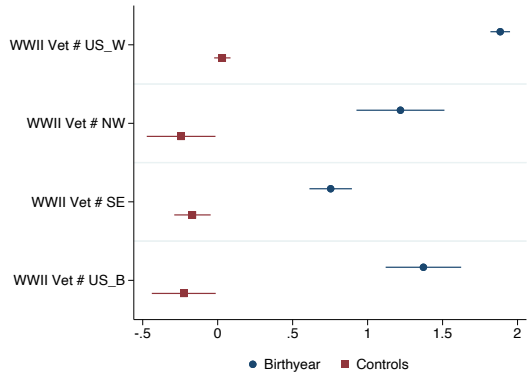
(b) Old, Own log(Wage and Salary Income)



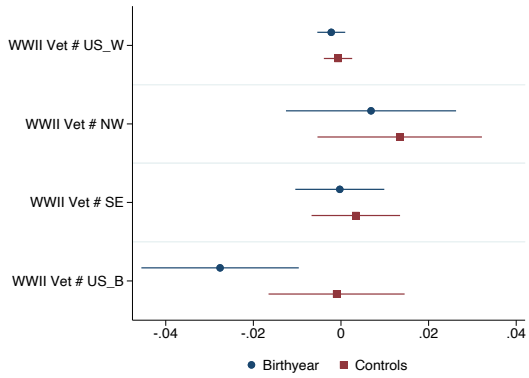
(c) Young, Father's Occupational Status



(d) Old, Own Occupational Status



(e) Young, Father's Employment



(f) Old, Own Employment

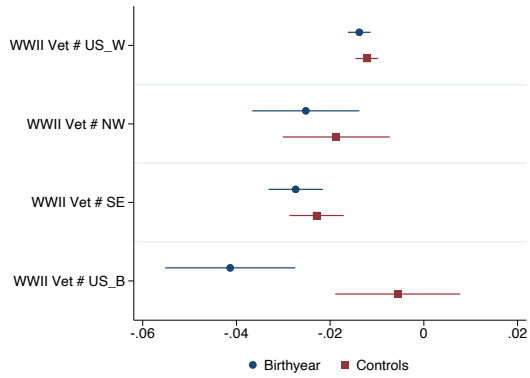


Figure B.2: Selection into Service, Labor Market

Notes: These figures present estimates of α_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 "controls," controls for group-by-birthplace and group-by-residence county fixed effects and all other observables in 1940 other than those that might confound the interpretation of the outcomes (e.g., we do not control for other labor market 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).

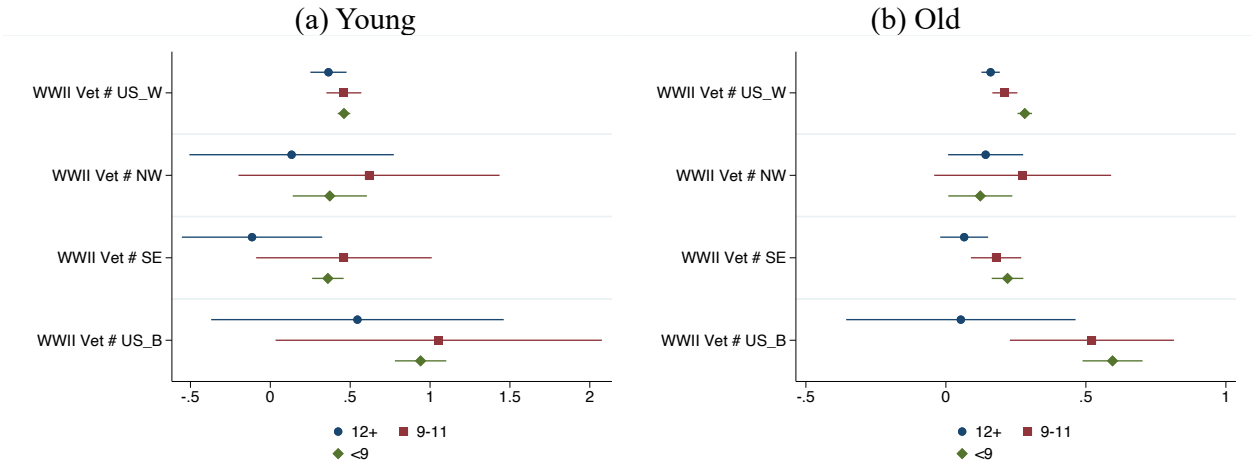


Figure B.3: Veterans' Premia in 1950 Years of Education

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.

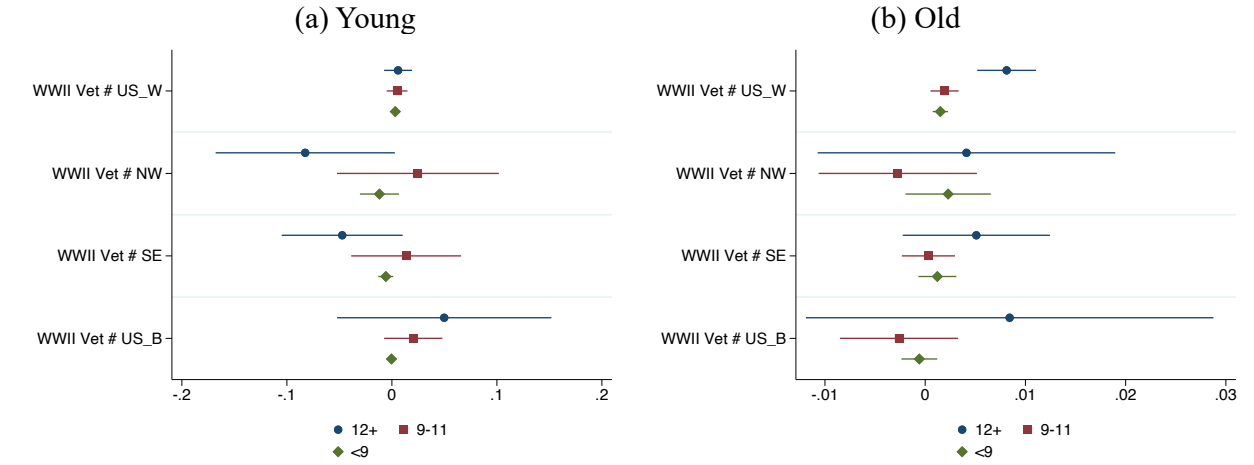


Figure B.4: Veterans' Premia in 1950 College Completion

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.

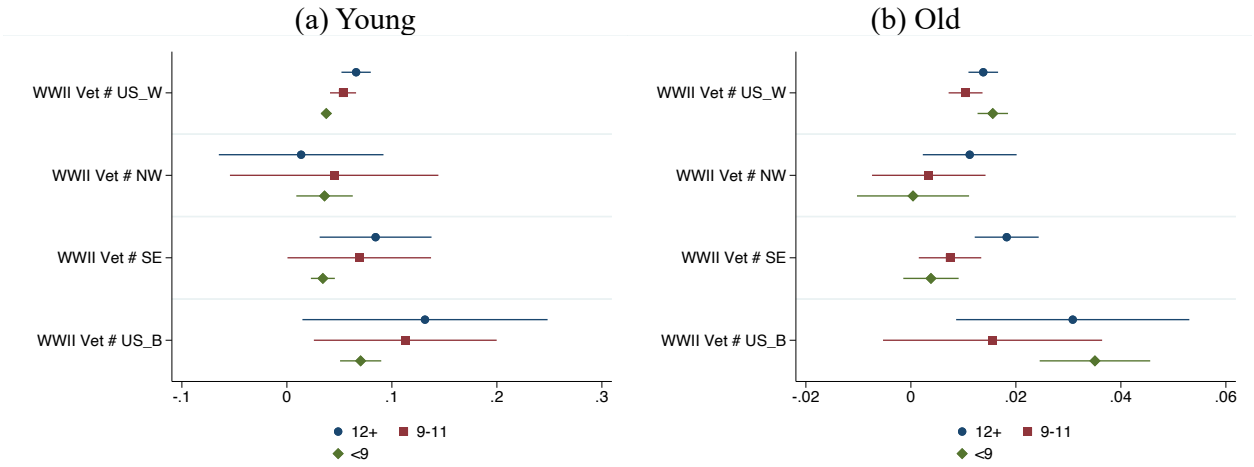


Figure B.5: Veterans' Premia in 1950 School Attendance

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.

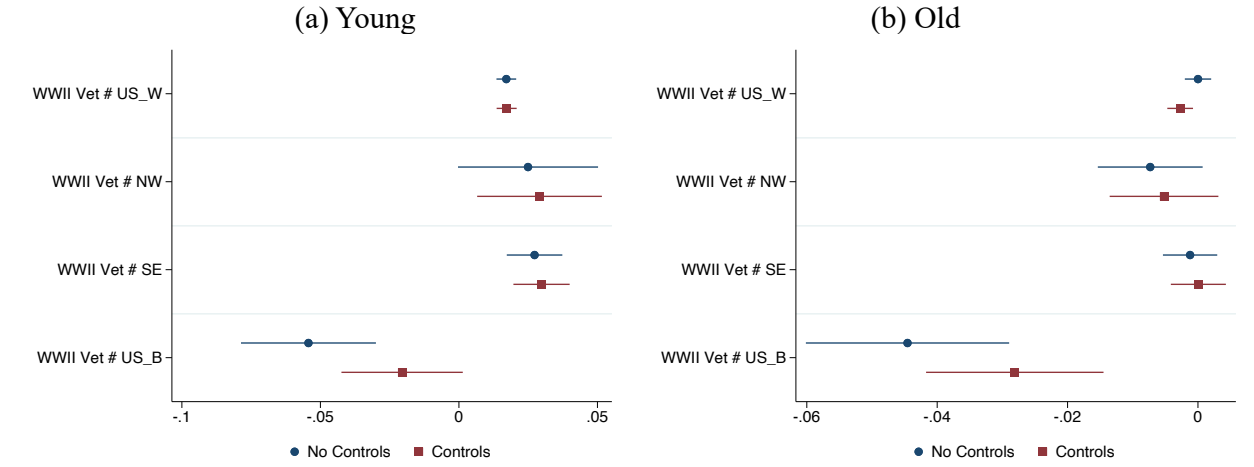


Figure B.6: Veterans' Premia in 1950 Employment

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.

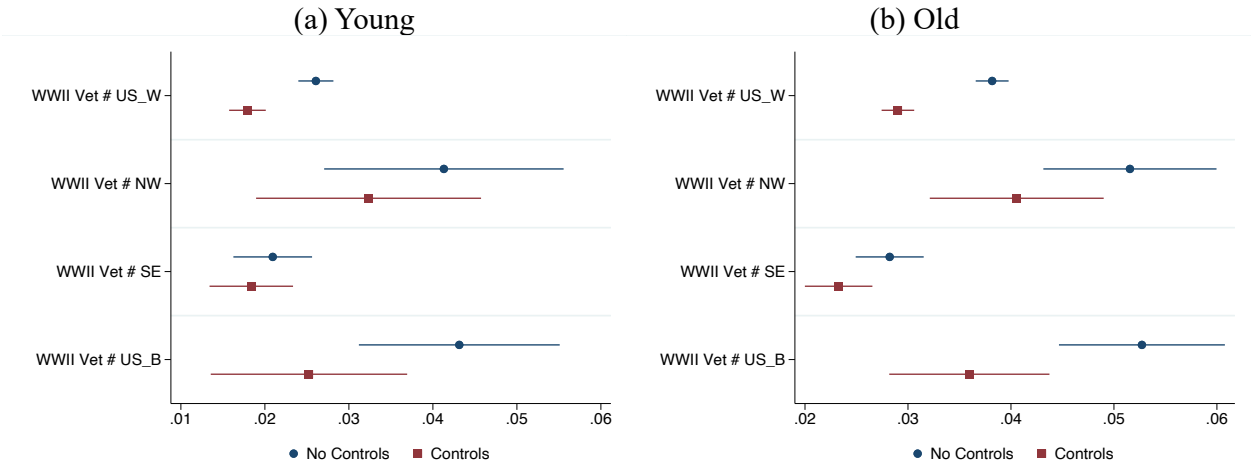


Figure B.7: Veterans' Premia in 1950 Government Employment

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.

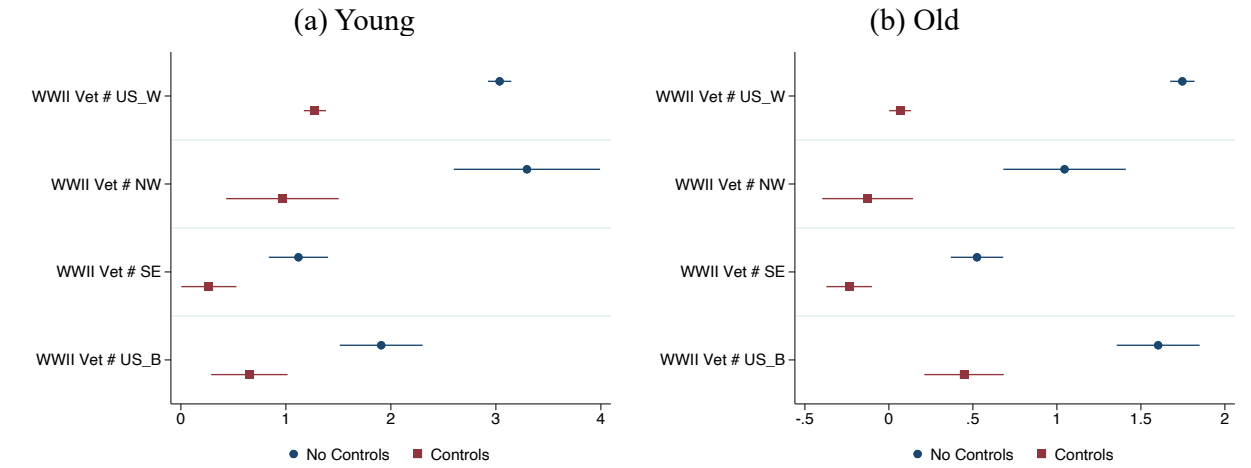


Figure B.8: Veterans' Premia in 1950 Occupational Status

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.

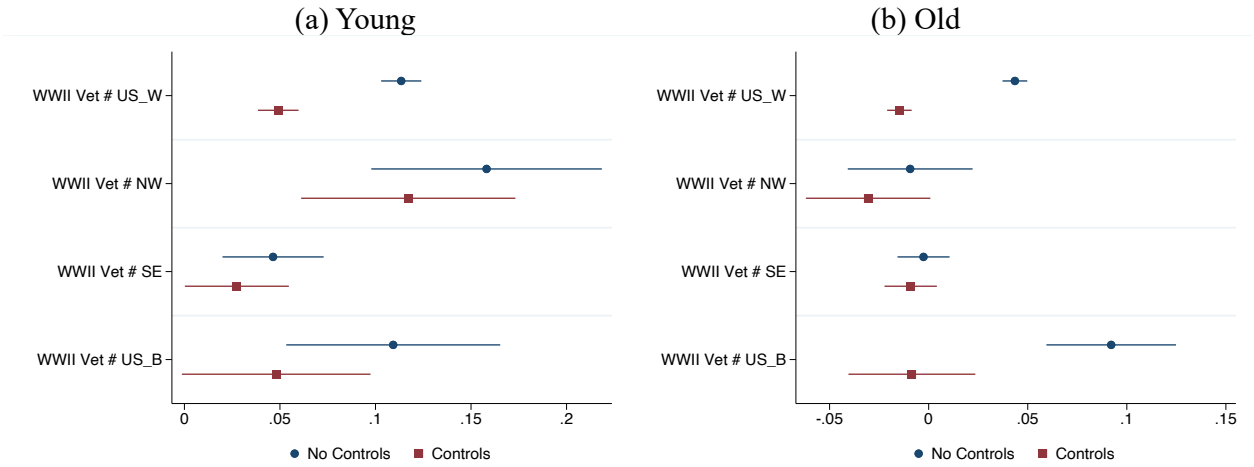


Figure B.9: Veterans' Premia in 1950 log(Earned Income)

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.

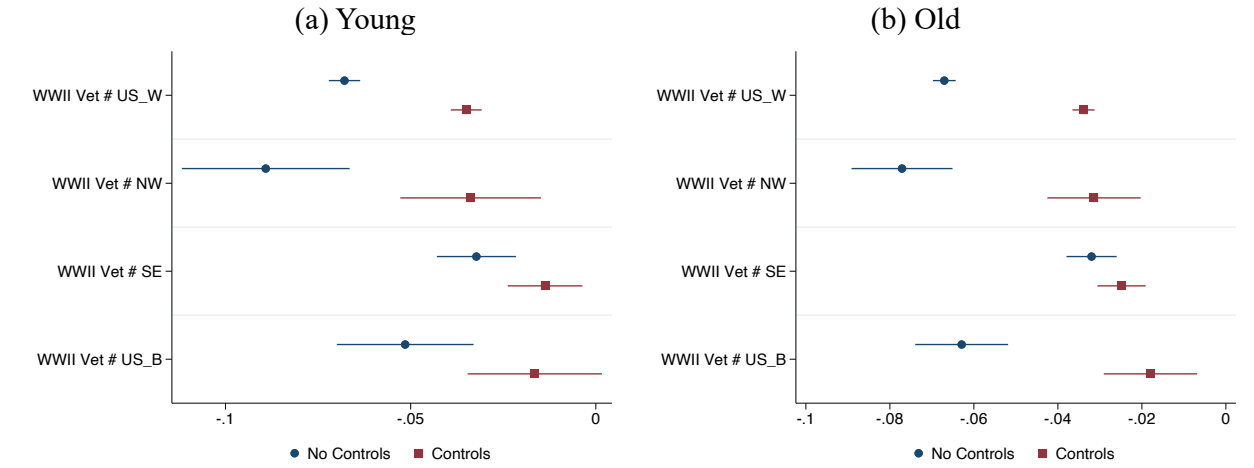


Figure B.10: Veterans' Premia in 1950 Self Employment

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 B.1: Oster (2019) bounds

	Young				Old			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variable	US-W	NW	SE	US-B	US-W	NW	SE	US-B
Years of Education	0.482	0.458	0.343	0.912	0.215	0.223	0.152	0.553
	0.210	0.306	0.240	0.468	−0.088	0.057	−0.035	0.167
College	0.003	−0.012	−0.008	−0.001	0.004	0.002	0.001	−0.000
	−0.007	−0.022	−0.013	−0.004	−0.007	−0.005	−0.004	−0.003
School	0.045	0.042	0.038	0.079	0.013	0.009	0.011	0.034
	0.039	0.034	0.037	0.073	0.005	0.003	0.004	0.028
Employment	0.017	0.028	0.030	−0.028	−0.003	−0.005	−0.000	−0.027
	0.016	0.029	0.029	−0.019	−0.003	−0.004	0.003	−0.019
Government Employment	0.018	0.025	0.019	0.025	0.030	0.042	0.023	0.036
	0.014	0.020	0.018	0.018	0.028	0.040	0.022	0.029
Occupational Status	1.310	1.254	0.277	0.550	0.066	−0.142	−0.237	0.410
	0.674	0.517	−0.023	0.036	−0.399	−0.482	−0.363	0.013
log(Earned Income)	0.053	0.121	0.029	0.023	−0.013	−0.032	−0.009	−0.001
	0.029	0.104	0.020	−0.008	−0.026	−0.033	−0.004	−0.030
Self Employment	−0.035	−0.039	−0.017	−0.021	−0.036	−0.034	−0.026	−0.019
	−0.025	−0.021	−0.012	−0.010	−0.021	−0.016	−0.018	−0.001

Notes: For each variable, the first row presents the estimated veterans' premium for that group and outcome when controlling for all 1940 observables. The second row presents the bound on the estimated veterans' premium following Oster's (2019) recommended choice of $R_{max} = 1.3\,\tilde{R}$.