Wages and the Great War:

Evidence from the Largest Draft Lottery in History*

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Abstract

Do veterans earn less? During World War I, the U.S. organized what was hailed at the time as "the greatest human lottery in history": a random draft of 24 million men. Ultimately, 2.8 million Americans were selected to join the armed forces. We sample 10% of registrants of the 1917 lottery and match these men with the 1930 and 1940 U.S. Federal Censuses. Low lottery numbers significantly increased the likelihood of serving in World War I. Military service also had a positive causal effect on earnings and occupational outcomes: veterans joined professions with higher cognitive skill requirements and earned 8–13% more than their peers by 1930. The effect is the opposite of Angrist (1990)'s landmark Vietnam finding. We rationalize this contrast through complier analysis: WWI draftees at the margin of service were negatively selected, giving them more room to benefit from army experience. Veterans' children also gained: sons of WWI veterans were more likely to complete high school and college in both 1940 and 1950.

Keywords: veterans' income, lottery, IV, war participation, intergenerational mobility
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1 Introduction

Violent conflict is as old as mankind, and continues to affect the lives of millions of people (Blattman, 2022; Morris, 2014). In addition to civilian and military casualties and the destruction of property, armed conflict also shapes the lives of the men and women who fight. One research question in particular has attracted attention in economics: the effect of military service on occupational outcomes. Millions of Americans served in the armed forces during the last century. Following Angrist (1990)'s seminal work using the Vietnam era lottery as an instrument, numerous papers have confirmed and extended the key finding that exogenously induced military service tends to lower income and leads to worse economic outcomes. While the majority of empirical work has focused on the Vietnam era, similar negative effects have been documented for a range of countries and periods—veterans in general do unambiguously worse in the labor market according to the existing literature.

Like all instrumental variable strategies, the Vietnam lottery identifies the causal effect on compliers—young men who, at the time, would not otherwise have served in the military. As such, the estimated effect depends on the specific economic, social, and cultural context in which it occurs. Arguably, conditions for veterans returning from the battlefields of South-East Asia in the 1970s were special, and unfavorable: Vietnam was the first war the U.S. unambiguously lost. The war divided the nation, and numerous reports about atrocities by U.S. forces, such as the infamous My Lai massacre, undermined the public image of soldiers. Veterans returned at a time when the U.S. economy's great post-war boom was coming to an end, with unemployment rising and foreign competition increasing in many sectors. All these factors suggest that the external validity of Angrist's famous Vietnam lottery finding can be called into question.

In this paper, we present evidence of a causal, positive effect of military service on earnings and occupational outcomes. During WWI, as in other conflicts, physical fitness requirements may have induced positive selection for actual veterans; at the same time, the fact that some skilled laborers and managers were exempt from the draft to maintain economic production may have induced negative selection. We use randomized treatment arising from the U.S. WWI lottery to identify effects. The Selective Service Act of 1917 introduced a service obligation for all adult men. Men were selected by what was hailed at the time as "the greatest human lottery in history." Carried out four times, it registered a total of 24 million men; ultimately, 2.8 million men were inducted into the U.S. Army. We focus on the first draft lottery, which covered 10 million registrants, resulting in around 700,000 inductees.

We first show that lower order numbers, as assigned by the lottery, significantly increased the chances of military service: our first-stage F-statistic exceeds 500. We link over 350,000 of the 1917 registrants to their census records in 1930 and 1940, and

code the characteristics of their occupations using skill requirements from the Dictionary of Occupational Titles (DOT). Occupational outcomes are unambiguously positive for draftees: the higher the lottery-induced probability of serving in WWI, the more they sorted into demanding, better-remunerated, and prestigious occupations. Average occupational scores increased from the equivalent of window dressers to postmasters. With better occupations came higher cognitive, language, mathematics, and reasoning requirements. The effects are quantitatively large. The intelligence requirement of the typical job taken by our controls was equivalent to that of watchmen or roofers; those induced by the lottery to serve ended up, on average, in professions with intelligence requirements of detectives, typesetters, or ship captains. The average math requirement increased by the equivalent of the jump from huckster/peddler to pressman and engraver; average language ability increased by the equivalent of the distance from cooks to millwrights. Earnings were significantly higher as well: those induced by the lottery to serve earned 8–13% more than their peers by 1930, equivalent to the difference in income between the average tinsmith and the average machinist.

We also document significant intergenerational effects. Children of WWI veterans were more likely to attend school and complete higher levels of education. Looking at 1940 Census records, sons of veterans had achieved higher grades, were more likely to have completed high school, and more likely to have attended college. By 1950 the effects on college completion had grown further. Military service during WWI thus set families on a better educational trajectory. Because there was no G.I. Bill after WWI, any educational gains for veterans' children must flow through income and occupational improvements rather than direct schooling subsidies, making the lottery-identified effect a particularly clean measure of the intergenerational return to military service.

What can explain this positive effect, and the sharp contrast with the post-Vietnam experience? Both the Vietnam and the WWI lottery provide equally credible identification—yet the sign of the treatment effect is exactly the opposite. We argue this is highly context-dependent: WWI veterans returned to an economic, social and political environment that differed sharply from the one faced by Vietnam-era veterans. Once war had been declared on Germany, the nation was united in its determination to prevail. Demobilized soldiers were hailed as victors, having brought a long and painful conflict to a quick and victorious end. Popular films such as *Sergeant York* celebrated the exploits of the 'doughboys'. After a short, sharp recession in 1920–21, the U.S. economy entered a prolonged expansion that only came to an end in 1929–30. New production techniques, systematic R&D investment, and new management methods raised labor productivity. In contrast, after Vietnam, a generation of disillusioned veterans came back to a deeply polarized society struggling with stagflation.

We can rule out patriotism as the key mediating variable that explains the positive effect of military service post-WWI. More patriotic areas—those that saw more

volunteering—are not the ones with the highest returns to military service. If anything, effects are weaker there. Complier analysis clarifies the mechanism: men at the margin of service during WWI were negatively selected—shorter, stouter, and with lower occupational aptitude in 1917. This negative selection implies more room to benefit from military training and network formation than the always-takers who would have volunteered anyway. In high-volunteering districts, where marginal draftees were even more negatively selected, the gains from service were correspondingly larger.

1.1 Related Literature

Our results relate to the literature on the effects of war exposure on labor market outcomes. In his seminal contribution, Angrist (1990) showed that 10 years after fighting in the Vietnam war, white U.S. veterans earned up to 15 percent less than comparable men who did not fight. Several studies have confirmed and expanded these results: Small and Rosenbaum (2008), Angrist et al. (2011), and Angrist and Chen (2011) look at U.S. Vietnam veterans; Siminski (2013) at Australian veterans. The only lottery-based study of a different war is Angrist and Krueger (1994), who use the 1943–47 lotteries to show that WWII U.S. veterans were likely to earn less than comparable non-veterans.

Evidence on the effects of military service on education is mixed (Lemieux and Card, 2001; Bound and Turner, 2002; Angrist and Chen, 2011). WWI differed from Vietnam in a crucial respect: there was no G.I. Bill, so any educational gains for children must flow through income and occupation rather than direct schooling subsidies. Health consequences of military service are generally negative (Hearst et al., 1986; Bédard and Deschênes, 2006; Angrist et al., 2010; Costa and Kahn, 2010).

We are not the first to examine WWI service in the U.S. Tan (2020) looks at veterans' earnings during the interwar period and finds no significant effects. However, his identification strategy relies on quarter of birth rather than the lottery itself, which yields a substantially weaker first stage. Our lottery-based identification exploits the same source of variation as Angrist (1990) but in a very different setting.

On intergenerational transmission of shocks, Oreopoulos et al. (2008), Hilger (2016), and Nakamura et al. (2022) document large and persistent effects of parental shocks on children. We contribute clean lottery-based identification of the causal effect of a father's military service on children's educational outcomes. Unlike the post-WWII intergenerational literature, the absence of direct educational subsidies after WWI means that effects we identify operate entirely through the economic and occupational channels opened by military service, providing particularly clean evidence on intergenerational mobility.

2 Historical Background

2.1 Conscription in the U.S. during World War I

The United States declared war on Germany on the 6th of April, 1917. On that day, the U.S. army was 200,000–300,000 men strong, a fraction of the size of European armies (Ayres, 1919; Grotelueschen, 2007). Faced with the need to expand the army quickly, Congress passed the Selective Service Act, authorizing President Wilson to enlist one million men. While expanding the army through volunteers was a possibility, Wilson's administration opposed this alternative, fearing that it would weaken domestic production. Wilson was also wary of the experience of conscription during the Civil War, when the draft was considered unfair by many citizens, and wished to avoid repeating that divisive legacy. The administration also faced a political challenge: Wilson's rival, Theodore Roosevelt, was pressing for an all-volunteer force that he would personally lead. For all these reasons, men were to be selected through a random lottery. On the 5th of June 1917, every male resident 21 to 30 years old was required to register for military service. Two months after registration, a national lottery determined which men would join the army. Figure 1 gives an overview of the key dates.

Within a year of the lottery, the U.S. army sent more than half a million draftees to France. The lottery was held in Washington D.C. in the summer of 1917; the first conscripts started training in September 1917 and landed in France in the spring of 1918 (Crowder, 1918). Between April and September 1918, seventeen National Army divisions with 687,000 draftees arrived in France. Starting from June 1918, three additional lotteries extended the draft to all men 18 to 45 years old: these additional draftees brought the strength of the U.S. army to around 4.7 million men (DeBruyne and Leland, 2020), but almost none of them saw combat, because by November 1918 Germany had surrendered and WWI was over.

Draftees were selected in four stages. First, on the 5th of June 1917, every U.S. male resident between 21 and 30 years old had to register for military service at one of 4,322 local boards, receiving a lottery number unique within their board. Second, on the 20th of July 1917, a large lottery in Washington D.C. determined the order in which these draft numbers would be called. During the lottery, high U.S. officials and public clerks drew numbers from a fishbowl and announced them to the press (Figure 2). Third, local boards used the lottery order to call men and induct them into the army. Reasons for exemptions included dependents (e.g., children), employment in key sectors (e.g., weapon production) and physical unfitness (e.g., disability). Each board had to provide the army with a given number of soldiers, and called men until it reached this quota. The quotas were a function of board size minus the volunteers who joined the army before the lottery, equalizing the share of soldiers across the country. As a result,

where many men had volunteered already, fewer men were drafted through the lottery. Fourth, selected men were inducted into the army. Conscripts received a few months of training in one of the U.S. camps and then sailed to France, where they usually spent the first weeks in a quiet sector of the front before being rotated into combat.

Figure 3 maps volunteering rates by state. Volunteering was highest in the Mountain West and parts of New England, reaching 75% of inductees in some states. In the South and the large coastal states, volunteering rates were substantially lower. Because the quota system credited prior volunteers against each district's conscription burden, states with high volunteering rates drew fewer men through the lottery—leaving a more negatively selected pool of potential draftees at the margin.

2.2 The American Expeditionary Force

The high command of the AEF kept conscripts separated from volunteers and organized drafted men into a "National Army" of 18 divisions (Nishiura, 1989). Career officers from the Regular Army always retained command of these divisions. However, because the AEF was short of trained personnel at lower ranks, it selected lower-rank officers during the first weeks of training from among the best draftees. On April 13th, 1918, the first National Army division arrived in France; the other 17 divisions disembarked between April and September 1918 and fought until the end of the war. Despite their brief presence on the Western Front, these soldiers took part in the most important battles of 1918: the battles of Aisne-Marne in June–July, St. Mihiel in September, and Meuse-Argonne in September–October. During Germany's final offensives, they blunted the German army's momentum. The rapidly growing strength of the AEF was a key contributing factor to Allied victory.

Fighting WWI required different training, doctrine, and equipment from the conflicts the U.S. had faced before. When President Wilson declared war on Germany, the U.S. Army had just returned from the punitive expedition against Pancho Villa (March 1916–February 1917). This campaign had reinforced a doctrine developed within the U.S. Army after the Civil War, which discounted the importance of firepower and artillery, and placed emphasis on the courage and initiative of infantry. Infantrymen were expected to take positions through superior maneuvering and with almost no supporting fire. Moreover, the plan of each battle was often only sketched in broad strokes, the objective of every attack being to break through the defensive lines and penetrate deeply into enemy territory.

These ideas closely resembled those held by European commanders at the beginning of the conflict in August 1914, and caused massive casualties during the first years of the war. However, European generals had had three years to study the new battlefield and to learn the importance of artillery to protect advancing infantry. By contrast, the AEF

had to absorb all of this in just a few months. One of the reasons why American soldiers managed to adapt relatively quickly to trench warfare was the high degree of delegation on which the AEF relied—another of the tenets of U.S. warfare doctrine. After a few months of fighting, lower-rank officers at the company and battalion level learned to coordinate with artillery brigades and to determine which of the assigned objectives were feasible and which were not. This reliance on initiative below the brigade level explains both the high initial death rates experienced by U.S. divisions and the rapid improvement that followed as soldiers gained experience (Appendix Figure A.2).

The high death rate faced by American soldiers may have affected the composition of survivors if casualties were systematically drawn from particular groups. This argument is difficult to examine directly, as we do not have granular information on the characteristics of casualties. Since most casualties of WWI fell to artillery strikes, selectivity was probably limited overall. If anything, however, the composition of survivors is likely to be negatively selected relative to the original pool: officers were selected from the best draftees and were around 20 percent more likely to die in combat than the average soldier (see Appendix Figure A.3, taken from Ayres 1919). Because approximately 83 percent of U.S. officers were trained after the declaration of war and were themselves chosen from the strongest performers among the draftees, the way that WWI killed men likely imparts a modest downward bias on the quality of surviving veterans relative to all who served.

3 Data

The Selective Service Acts of 1917 and 1918 made every 18–45 year old man living in the United States liable for military service. Our population of interest consists of men born between the 5th of June 1886 and the 6th of June 1896—those aged 21 to 30 on registration day, and therefore subject to the first draft lottery. Other cohorts registered during the subsequent 1918 draft lotteries and had much lower probabilities of serving in the Army. Ten million men registered during the first draft, and 700,000 of them joined the Army after being selected in the July 1917 lottery.

We collect basic information on everyone who took part in the 1917 lottery from the National Archives and Records Administration (NARA). We use their name, date of birth, and state of birth to link their records to the U.S. Federal Censuses of 1930 and 1940. We then select a random sample of the men we match to both Censuses; for these individuals, we reconstruct the outcome of the draft lottery and create several labor market outcomes. We describe each of these steps in turn, leaving further details on variable construction to Appendix A and on the record linking procedure to Appendix B.

3.1 Sample

Our main database consists of a random sample of men who took part in the 1917 lottery. To construct this sample, we start from the full list of ten million men who took part in the 1917 lottery, digitized from their original registration cards (Figure 4, left). For every man, we observe basic information including first and last name as well as year and place of birth. We use this information and the method of Abramitzky et al. (2012) to link these men to their records in the 1930 and 1940 U.S. Federal Censuses, looking for individuals with similar names born in the same state and within a two-year window around birth year (see Figure 4, right, and Appendix B for details).

Of 10.2 million men in 1917, we find 2.2 million in the 1930 Census, 1.9 million in the 1940 Census, and 1.2 million in both. We focus on these men, whom we observe 13 and 23 years after the war: this ensures that we digitize information only for men for whom we have post-war data. We select a random sample of 512,829 men, stratifying by age and state of birth. As is typical in historical record linking, matched men are more likely to be white, U.S.-born, and to have uncommon names. The final analysis sample of 357,152 consists of observations with valid information for our main 1930 variables (veteran status and occupation).

Table 1 reports balance statistics comparing the full 1917 lottery population, the matched sample linked to both Censuses, and our analysis sample. Columns (4) and (5) report mean differences; columns (6) and (7) report the corresponding t-statistics. As is typical with historical record linking, matched men are more likely to be white, U.S.-born, and to have less common names. Critically, the matched and analysis samples are virtually identical across all observable characteristics, confirming that the stratified random draw introduced no additional selection beyond the initial linking step.

3.2 Main Variables and Sources

The three key variables for our analysis are the result of the 1917 lottery, veteran status, and labor market outcomes in 1930. To determine the outcome of the draft lottery, we obtain the image of each man's registration card (Appendix Figure A.1 shows an example). We digitize the draft number from these images: the numbers were recorded in red ink on the top-left corner of the cards, and were unique within each recruitment board. We then reconstruct the order in which draft numbers were drawn during the lottery held in Washington D.C. on 20 July 1917, using lists published in three newspapers the day after: the *New York Times*, the *Altoona Tribune*, and the *Los Angeles Evening Herald*. We cross-check the three sources to minimize errors (Appendix Figure A.4 shows pages from two of these newspapers as examples). The result of this procedure is a random number reflecting the order in which each registrant was called by his local board. Men with lower order numbers were called earlier and thus had a higher probability of being

inducted into the army; we use these numbers as our instrument for veteran status.

We observe veteran status in the 1930 U.S. Federal Census. In that year, the Census collected veteran information from every adult male, including the period of service. Around 4 million men were veterans in 1930, of whom 89.4 percent served in WWI. Our main treatment is a dummy for being a WWI veteran; we report results using a dummy for being a veteran of any war as a robustness check.

We use reported occupation to construct several labor market outcomes. We observe occupation in both 1917 (from the registration cards) and 1930 (from the Census). We develop novel crosswalks to link these occupations to the 4th edition of the Dictionary of Occupational Titles (U.S. Department of Labor, 1977), which assigns scores for intelligence, reasoning, language, math, and authority. We complement these with the IPUMS occupational score—a synthetic socioeconomic index—and income reconstructed at the occupation level from the Census Bureau's Statistical Abstracts of the U.S. for the 1930s. While occupation-level earnings do not capture variation at the individual level, they provide an important additional dimension to our occupational scoring. Taken together, these variables provide a comprehensive picture of our men before and after the war.

3.3 Data Description and Balance

Table 2 reports summary statistics. Panel A describes pre-war characteristics at registration in 1917. Of the 512,829 men we selected, we have complete data for 350,031: this constitutes our main sample. These men averaged 25.3 years of age, were mostly white (91%), of medium height and physical build, and half were married when the war broke out. In 1917 they were employed in occupations requiring relatively low cognitive skills: 1.9 out of 4 in intelligence, and 2.4, 3.1, and 2.5 in mathematical, reasoning, and language ability (out of 6). Less than 5% held supervisory or authority positions. The average occupational score was 21 (out of 100).

Panel B describes outcomes observed in the 1930 Census, some 13 years after the war. A little under a third of men (30.3%) are recorded as WWI veterans, and only one percent more are veterans of other wars. Thirteen years after the war, these men have moved slightly up the occupational ladder: their average occupational score is 26, and all cognitive scores have improved modestly. Appendix Figure A.7 gives an overview of 1930 occupational scores by state: the North-East, the area around the Great Lakes, and California/Arizona have relatively high scores, while the South and the Mid-West tend to host jobs with lower occupational scores.

We also use occupation-level earnings reported in the 1930 Census as an indicator of labor market outcomes. Appendix Figure A.8 shows a binscatter of log income against the occupational score. While clearly highly correlated, a simple bivariate regression captures only 25% of the variation in log earnings, confirming that occupational score

and income add distinct information about labor market standing.

4 Empirical Analysis

We are interested in estimating the causal effect of fighting World War I on veterans' earnings and occupational outcomes. We first show the strength of our first stage from random assignment. Next, we examine the impact of exogenously induced military service on occupational outcomes, including earnings.

4.1 Data Exploration and OLS Estimates

We first analyze basic patterns in the data. As is commonly observed, veterans do better on a range of outcomes. Figure 8 gives an initial overview. Positions filled by veterans have relatively similar median occupational scores, but the mean and 75th percentile are higher. They work in occupations requiring more math, more reasoning, and higher intelligence. They also earn more by 1930, by 21% (an increase of $279 compared to an average of $1,356). We find similar patterns for both younger and older cohorts (Appendix Figure A.6). As Appendix Figure A.9 shows, the effect of war service appears particularly strong for people in the lower part of the income distribution, where the conditional quantile effect at the 20th to 40th percentile ranges from 20 to 40 percent.

These raw OLS patterns are likely biased upward by positive selection of volunteers (Angrist, 1990): men who chose to enlist before the lottery tend to be healthier, more motivated, and on average more capable, so simple comparisons overstate the causal effect. The IV estimates below are more conservative and use only lottery-induced variation in service.

4.2 Instrumenting War Participation

We instrument war participation with lottery order numbers. Sample characteristics pretreatment are balanced with respect to lottery numbers (Figure 6.[1] The probability of service as a function of order number O_i follows a simple theoretical prediction. Letting V denote the unconditional probability of volunteering, e the probability of exemption once called, and Q/e the registration-district quota normalized by the exemption rate, the probability of WWI service is:

$$\Pr[\text{WWI}_i = 1 \mid O_i] = \begin{cases} V + (1-e) \cdot \Pr\left[O_i \leq \frac{Q}{e}\right] & \text{if } O_i \leq \frac{Q}{e} \\ V & \text{otherwise.} \end{cases} \tag{1}$$

[1] There is also no evidence of non-random draws in the lottery – cf. Figure 7.

Since O is uniformly distributed, $\Pr[O_i \leq Q/e]$ decreases linearly up to the threshold Q/e. Figure 9 shows both the theoretical prediction and the estimated relationship: the data closely match the expected pattern.

The formal first-stage equation is:

$$\text{WWI}_i = \delta_0 + \delta_1 O_i + X_i^{1917\prime}\xi + e_i \tag{2}$$

where O_i is the lottery order number and X_i^{1917} is a vector of pre-war controls. Higher order numbers implied a lower probability of service.

Table 3 reports the formal first stage in detail. Our instrument is strong: we obtain F-statistics of 514 without controls, rising to 687 with full controls. Column (1) shows the baseline specification; columns (2)–(5) progressively add age fixed effects, marital status, and race controls. The key coefficient on order number is stable throughout at -0.006 per thousand ranks. Controlling for age barely changes the coefficient. Being married reduced the risk of service substantially, by almost half, because exemptions favored men with dependents. Adding controls for race leaves the coefficient unaffected, suggesting that the implementation of the 1917 lottery led to clean randomization. Appendix Figure A.10 confirms that the order number significantly predicts WWI participation both among unmarried and married men separately.

The first stage is not uniform across space. Figure 10 maps the point estimates from separate state-level first-stage regressions. The instrument has the highest predictive power in New Mexico, the Dakotas, and Washington; in Florida, Utah, and Delaware it is relatively weak. In 7 out of 49 states, the mean estimated effect is negative. The states with the lowest estimated coefficients generally had high prior volunteering rates, making it harder to pin down the effect of the lottery: once many men had already volunteered, the remaining pool of potential draftees was smaller, diluting the lottery's predictive power.

4.3 Reduced Form

The reduced form equation is:

$$Y_i^{1930} = \gamma_0 + \gamma_1 O_i + X_i^{1917\prime}\psi + r_i \tag{3}$$

In Figure 11, we explore the reduced form patterns graphically. Higher order numbers meant a lower probability of being drafted, all else equal. We see that the lines in terms of occupational outcomes slope downward for each of the eight indicators shown—lower risk of being conscripted was systematically associated with lower occupational scores, jobs that required less intelligence, less math, less reasoning and language ability, and lower pay. The downward-sloping relationship is visible in every panel, with remarkable

consistency across all cognitive skill dimensions. Appendix Figure A.11 shows the effect of the lottery number on earnings across different states: while there is considerable variation, in most states men who received smaller order numbers earned more in 1930.

Figure 12 examines how the reduced form pattern varies by birth cohort. It plots expected 1930 income as a function of birth year and lottery order number. The red and yellow shading (higher income) is concentrated in the bottom-left corner—younger men with low order numbers—and fades toward higher order numbers. The income gradient is steepest for the oldest cohorts (born 1885–1890) and more gradual for the youngest. This age gradient is consistent with older, more experienced men having been disproportionately selected into leadership roles, with larger career consequences.

Table 4 reports the formal reduced form estimates across all eight outcomes, including age fixed effects throughout. We find positive, significant effects throughout. The higher the order number, the lower the risk of military service—and the lower the skill requirements of the jobs that a man held by 1930, across all categories. Overall occupational scores were higher for those at greater risk of being drafted, and their income was correspondingly higher. When we combine the different occupational characteristics through principal component analysis, we again find a strong and significant effect (column 6). Similarly, occupational score and 1930 income are significantly lower for men with higher order numbers (columns 7–8).

4.4 Two-Stage Least Squares

We estimate the causal effect of war participation on occupational outcomes and earnings via 2SLS:

$$\widehat{\text{WWI}}_i = \hat{\delta}_0 + \hat{\delta}_1 O_i + X_i^{1917\prime}\hat{\xi} + w_i \quad \text{(first stage)} \tag{4}$$

$$Y_i^{1930} = \beta_0 + \beta_1 \widehat{\text{WWI}}_i + X_i^{1917\prime}\theta + v_i \quad \text{(second stage)} \tag{5}$$

Results are summarized in Table 5. Panel A reports baseline 2SLS estimates. We find strong and significant effects on all cognitive skill dimensions and occupational score. Coefficients are in the range of 0.12–0.14 for the veteran status dummy. Relative to sample dependent variable means, the largest effects (by order) are on PCA, authority, occupational scores, math, language, reasoning, intelligence, log income). The 2SLS estimates are 25–50% larger than the corresponding OLS estimates, consistent with negative selection of compliers: men at the margin of service were somewhat less skilled before the war, so the OLS estimate understates the causal gain from service. The fact that both OLS and 2SLS estimates are positive strongly suggests that positive selection of volunteers was not a dominant feature of the recruiting process—if volunteers were systematically the most able men, OLS would exceed 2SLS, which is the opposite of what we observe.

Panel B adds state fixed effects to absorb persistent geographic differences in labor markets and economic conditions. Coefficients are slightly smaller throughout but remain broadly consistent with Panel A, confirming that the result is not driven by state-level variation in volunteering rates or postwar economic trajectories. The principal component (column 6) and occupational score (column 7) remain strongly significant.

4.5 Coarsened Exact Matching

Establishing the effect of military service assumes that the treated and untreated are comparable. While this is a compelling assumption for the 2SLS strategy—where comparison is driven by random lottery variation—the OLS results may arguably be affected by differential selection into treatment. To compare like with like, we use coarsened exact matching (CEM; Iacus et al., 2012), forming comparison groups of highly similar individuals based on 1917 observables, with military service the only remaining difference. To ensure that comparison groups are sufficiently large, the estimator forms broader coarsened groups rather than requiring exact cell-by-cell matches.

In Table 6, we use exact matching on marital status, race, age class, nine geographic areas based on the Army's Service Commands, and quartiles for all 1917 occupational variables (intelligence, math, reasoning, language, authority, and occupational score). Panel A shows that the OLS coefficients on the CEM sample are essentially identical to the baseline, despite losing more than half of the observations because there is no match for some treated individuals. The fact that results are not affected when we force the sample to contain only treated and untreated individuals that are similar on observable pre-war characteristics strongly suggests that imbalances between veterans and non-veterans are not driving our findings.

For completeness, we also report reduced-form and 2SLS results estimated on CEM samples. For these exercises we define treatment as having an order number below 4,500—an almost certain indicator that a man would at least be called for Army examination. Panel B reports reduced form results and Panel C reports 2SLS estimates. As for OLS, results are unaffected by the sample restriction, and if anything are somewhat larger, consistent with the more negatively selected complier pool in the restricted sample.

Appendix Table A.2 reports an even more demanding specification in which we only keep treated and untreated individuals who share exactly the same age and state. This forces strictly local comparisons and yields larger coefficients throughout: the more comparable the samples of treated and untreated individuals, the bigger the estimated effects. This pattern is consistent with negative selection at the margin of service—the principal conclusion from the complier analysis below.

4.6 Feature Importance: Machine Learning Evidence

We can examine which variables modulated the effect of wartime service, on occupational standing using machine-learning. We estimate conditional treatment effects along the lines of Athey and Wager (2019). Effect size differs sharply as characteristics of individuals in 1917 vary. Figure 5 shows this variation. The largest treatment effects estimated by causal trees are plotted on the left; as we move right, the treatment effects gradually decline. What accounts for this heterogeneity? We can permute treatment effects for correlates through the Shapley procedure, determining the direction and magnitude of modifying effects. Figure A.5 presents the results. While many variables shift predictions in both the positive and negative domain, the single largest driver is occupational status pre-war, followed by age. We also see that marital status, physique, and general ability have substantial influence on predictions.

5 Mechanisms

5.1 Who Were the Compliers?

In the LATE framework, our 2SLS identifies the causal effect for compliers—men who served because they received a low order number but would not have served otherwise. Understanding who these men were helps to explain why the gains from military service were positive. Table 7 shows the structure of the sample by draft status and service status. Of 357,152 men, 108,004 served in WWI. Among 205,068 never called by their local board, 59,198 served regardless (always-takers); these are the volunteers who joined before or outside the lottery process. Among 152,084 called, 48,806 served (compliers plus always-takers), while 103,278 did not serve even after being called (never-takers). We cannot identify compliers directly, but comparing the characteristics of these groups reveals who was at the margin of service.

Figure 13 shows demographic characteristics of the four groups. Compliers are younger, single, and predominantly white—consistent with exemption rules that favored married men with dependents and workers in essential industries. Figure 14 shows physical and occupational characteristics: compliers are shorter, stouter, and scored somewhat lower on 1917 occupational aptitude measures. This negative selection at the margin of service matters: men who served because of the lottery had more room to gain from military training and network formation than the always-takers, who presumably would have found equivalent paths to success regardless. The contrast between compliers and always-takers illuminates the mechanism: it is precisely because the marginal draftee came from a lower starting point that military service could make such a difference.

5.2 Age Heterogeneity

Did all conscripts benefit equally from military service during WWI? The contour plot in Figure 12 already hints at a role for age: the income gradient with respect to order number appears steeper for older birth cohorts. The U.S. Army in 1917 was acutely short of officers, non-commissioned officers, and trained personnel at all levels. The Army quickly recognized that more experienced, older men could act as leaders, and accordingly selected lower-rank officers from among the best draftees during the first weeks of training. Men who were somewhat older in 1917 were therefore more likely to be assigned to leadership roles, to receive additional training, or to be posted to headquarters tasks—all routes that could open occupational windows of opportunity extending well beyond the war itself. Statistical results from regressions by age cohort are reported in Appendix Table A.1.

To test formally whether gains from service varied by age, we estimate:

$$Y_i^{1930} = \gamma_0 + \gamma_1 O_i + \gamma_2 O_i \times \text{Young}_i + X_i^{1917\prime}\psi + r_i \tag{6}$$

where $\text{Young}_i = 1$ if aged 21–25 in 1917. Table 8 reports the results. The interaction is generally small and insignificant: there is little systematic age heterogeneity in the returns to service in the aggregate, suggesting that the benefits accrued broadly across age groups. While the mechanism of leadership selection likely generated heterogeneous individual-level effects, these may average out across the many occupational roles that service opened up, or the oldest men—who were disproportionately selected as officers—may simply constitute too small a fraction of the sample for their larger gains to be detectable in the aggregate cross-cohort comparison.

5.3 Selection and Volunteering Rates

The quota system implies that high-volunteering districts needed fewer draftees to fill their quotas, leaving a more negatively selected pool of potential draftees at the margin. In districts where most men with the highest occupational aptitude had already volunteered, the men remaining for the lottery would on average have had lower pre-war skills and less to lose from interrupting their civilian careers—and correspondingly more to gain from army training, discipline, and the social networks formed during service. To test this prediction, we estimate:

$$Y_i^{1930} = \gamma_0 + \gamma_1 O_i + \gamma_2 O_i \times (V/S)_j + X_i^{1917\prime}\psi + r_i \tag{7}$$

where $(V/S)_j$ is the 1917 volunteering rate in registration district j.

Table 9 reports the results. Higher volunteering rates are associated with a significantly stronger positive effect of low order numbers on most outcomes. This is exactly

what the negative selection story predicts: in high-volunteering districts, the self-selected volunteers have already left the pool, so draftees at the margin are more negatively selected and gain more from service. Relative to sample means, the interaction is strongest for occupational scores, intelligence, math, language, and reasoning—precisely the occupational dimensions on which one would expect the greatest scope for human capital accumulation through the structure of military training and assignment.

6 Veterans' Children

Military service not only affected the veterans themselves but also shaped the outcomes of their children. We link children of our 1917 registrants to their fathers in the 1930 Census to construct an intergenerational sample, then follow these children to 1940 and 1950. The estimating equation is:

$$\text{Educ}_k^t = \pi_0 + \delta_1 \text{WWI}_{i(k)} + X_{i(k)}^{1917\prime}\chi + \upsilon_k \tag{8}$$

where k indexes children, $i(k)$ is the father of k, and $t \in \{1940, 1950\}$. We instrument $\text{WWI}_{i(k)}$ with the father's 1917 lottery order number and also report CEM estimates that compare children whose fathers had similar pre-war characteristics. Standard errors are clustered at the father level.

Because there was no G.I. Bill after World War I, the educational gains we find for veterans' children cannot reflect direct subsidies to schooling. They must instead flow through the improvements in income and occupational attainment documented in the preceding sections: men who served earned more and worked in more cognitively demanding jobs, and apparently invested part of those gains in their children's education. This makes the WWI setting a particularly clean context for isolating the intergenerational return to military service, unconfounded by the kind of direct educational support that complicates interpretation of the post-WWII literature (Lemieux and Card, 2001; Bound and Turner, 2002).

1940 outcomes. Table 10 reports CEM and 2SLS estimates for children's educational attainment in 1940. The CEM coefficients on veteran father status are 0.432 (s.e. 0.018) for highest grade completed, 0.051 (s.e. 0.004) for high school completion, and 0.029 (s.e. 0.002) for college attendance, all significant at the 1% level. The 2SLS estimates confirm these gains: high school completion rises by 0.248 (s.e. 0.123), and highest grade completed rises by 1.580 (s.e. 0.645). The Anderson-Rubin test rejects the null for both highest grade ($p = 0.013$) and high school completion ($p = 0.043$). Veterans' children were studying more by 1940.

1950 outcomes. Table 11 examines the same children in 1950, when most had completed their education. The most striking result is on college completion: the CEM coefficient is 0.118 (s.e. 0.008) and the 2SLS estimate is 0.390 (s.e. 0.181), significant at the 5% level ($p = 0.027$ by the Anderson-Rubin test). Veterans' children were meaningfully more likely to have completed college by 1950, suggesting that the economic gains from military service translated into sustained investment in the next generation's education. Military service during WWI thus set families on a better educational trajectory that continued to compound over the following decade: the positive 1940 effects on school attendance and high school completion fed directly into the college completion gains visible by 1950.

7 Robustness

Our results are robust to several alternative methods and approaches.

First, spatial autocorrelation across registration districts is unlikely to bias our standard errors downward. Our main source of variation consists of order numbers, which by construction have no spatial pattern: any given order number can appear in any registration district, since the national lottery was drawn without reference to local geography. We confirm the robustness of our results to spatial inference in Appendix Table A.3, where we report standard errors computed with the method of Conley (1999), which allows disturbances of places to be correlated up to a given spatial threshold. We experiment with several different thresholds and find that results remain significant throughout.

Second, outliers and influential observations do not affect our results. All our variables are bounded and have relatively little skew: WWI veteran status is a binary indicator, order number is distributed uniformly between 1 and 10,000, and occupational outcomes have few extreme values. Outliers and influential observations are therefore unlikely to play a major role in our results. Appendix Table A.4 confirms this intuition: it reports robust regression results that downweight influential observations (Li, 1985), showing that neither coefficients nor significance are affected by the downweighting.

Third, our results are not driven by specific geographic areas. Figure 10 showed that the strength of the first stage varies across U.S. states, which may suggest that our conclusions depend on a handful of individual states. Appendix Figures A.12–A.14 investigate the robustness of our results by re-estimating our models after dropping one state at a time. The figures plot beta coefficients for the OLS (Figure A.12), the reduced form (Figure A.13), and the two-stage least squares (Figure A.14); each panel reports results from regressions with a different dependent variable. Results are remarkably stable throughout, and no single state drives our findings.

Fourth, we use a treatment variable specifically constructed to capture exposure to WWI in the 1930 Census. We classify men as WWI veterans if two conditions apply:

first, they answer "yes" to Question 30 asking whether they are a veteran of U.S. military or naval forces; and second, they answer "WWI" to Question 31 asking which war or expedition. This double requirement minimizes false positives. However, errors in transcription or digitization may mean that we miss some WWI veterans whose answers were recorded in non-standard ways. Appendix Table A.5 reproduces the IV results with a more liberal measure of veteran status that classifies as veterans everyone who declared being a veteran, regardless of the war they specified. Results hold with this different measure of veteran status.

8 Conclusions

The "credibility revolution" in empirical economics has made important contributions to our understanding of questions ranging from the effects of immigration to the impact of global trade. As a demonstration of a technique's potential, few studies have been more influential than Angrist (1990)'s hallmark study of veteran status as a determinant of income, pioneering the use of instrumental variables. At the same time, criticisms of instrumental variable methods have often emphasized the limited external validity of the specific local average treatment effect identified, since both the treatment population and the economic context are particular to the setting (Deaton, 2010).

In this study, we re-visit the effect of veteran status on earnings using another draft lottery in the U.S.—the one conducted during World War I. While very similar in nature and motivation, and affecting millions of draftees, it occurred under very different circumstances: WWI was popular at home, there was no opposition, and returning veterans were not stigmatized as in the years after Vietnam. The war was victorious, and demobilized soldiers returned to a buoyant labor market.

Our results are the exact opposite of Angrist's. Those drafted during WWI ended up earning more—by up to 13%—and worked in more cognitively demanding jobs, requiring greater skills across a range of dimensions from language ability to mathematical aptitude. Importantly, randomly-assigned military service in WW I created intergenerational benefits: The sons of WWI veterans completed more schooling by 1940 and 1950 than the peers whose fathers didn't serve. This positive transmission across generations contrasts sharply with the negative consequences of Vietnam service documented elsewhere. Because there was no G.I. Bill after WWI, it cannot be attributed to direct educational subsidies.

Complier analysis explains the divergence: WWI draftees at the margin of service were negatively selected—shorter, stouter, lower-aptitude men. This gave them more room to benefit from military training and peer network formation. In high-volunteering districts, where marginal draftees were even more negatively selected, the gains were *larger*. This mechanism contrasts sharply with the Vietnam setting, where the draft pool

was differently constituted.

Our results establish that randomly assigned treatment alone is insufficient to determine the sign of effects. It depends crucially on the context in which the estimate was obtained (Deaton, 2010). In a different setting—a different war, a different economy, a different social reception for veterans, a different composition of marginal conscripts—the exact opposite pattern can emerge with equally credible identification. History, politics, and culture are all relevant determinants for the direction of treatment effects.

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Tables and Figures

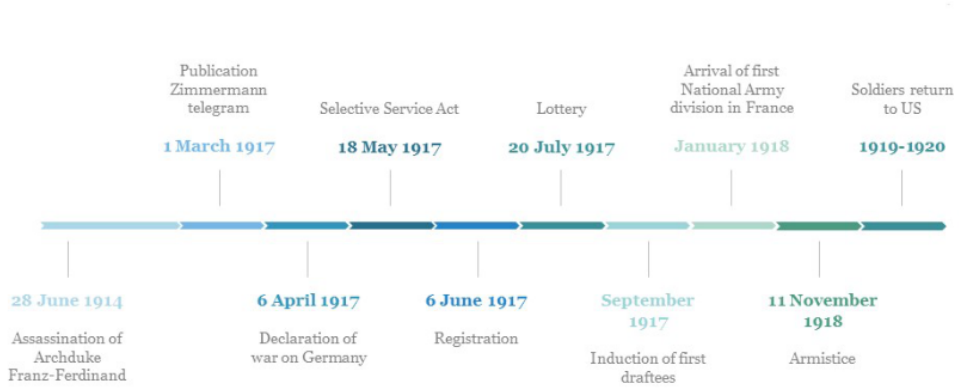


Figure 1: Key dates in U.S. entry into World War I. The declaration of war in April 1917, the Selective Service Act in May 1917, and the lottery in July 1917 rapidly transformed the small U.S. army into a force of millions.

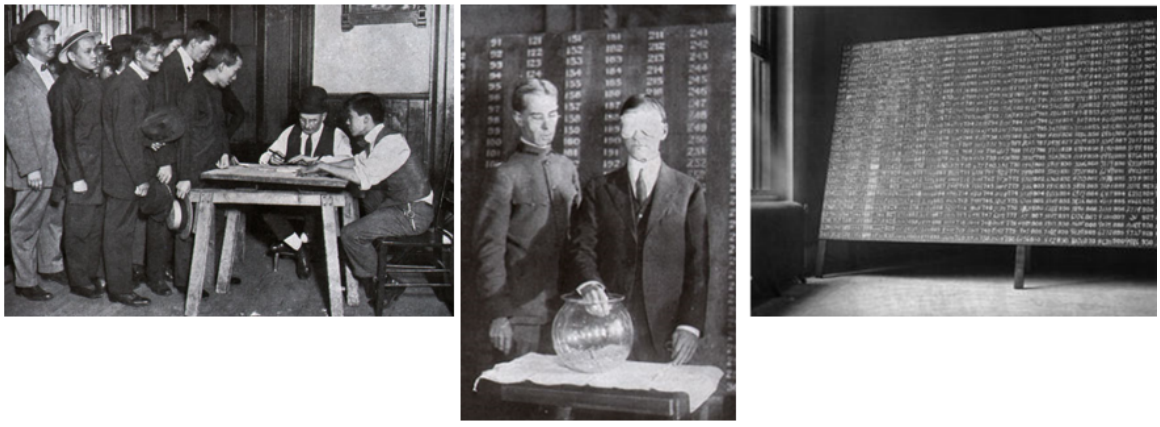


Figure 2: The 1917 draft lottery. Left: men receiving their lottery numbers. Center: U.S. Secretary of War Newton D. Baker draws the first number, blindfolded, in Washington D.C. on 20 July 1917. Right: the blackboard displaying the order in which the 10,000 lottery numbers were drawn.

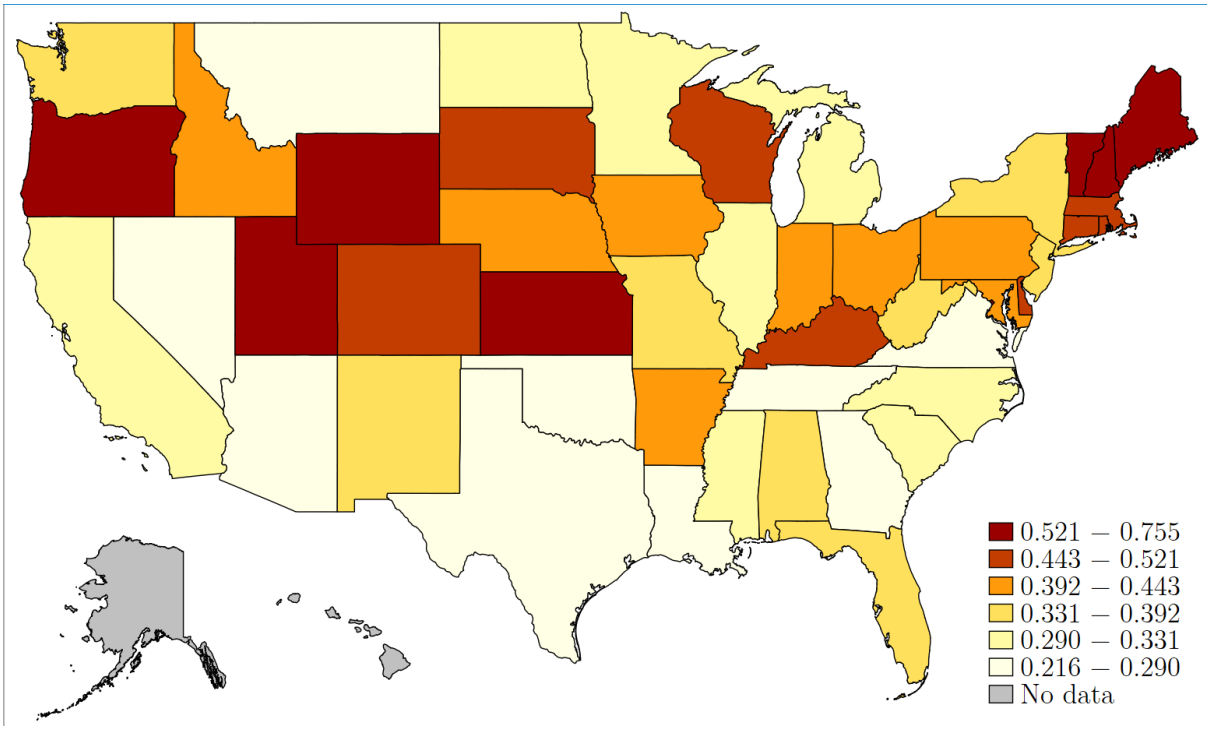


Figure 3: Volunteering rates by state. Share of men who volunteered for service before the 1917 lottery, relative to total eventual inductees. States in darker shades saw more volunteering; their quotas were correspondingly reduced.

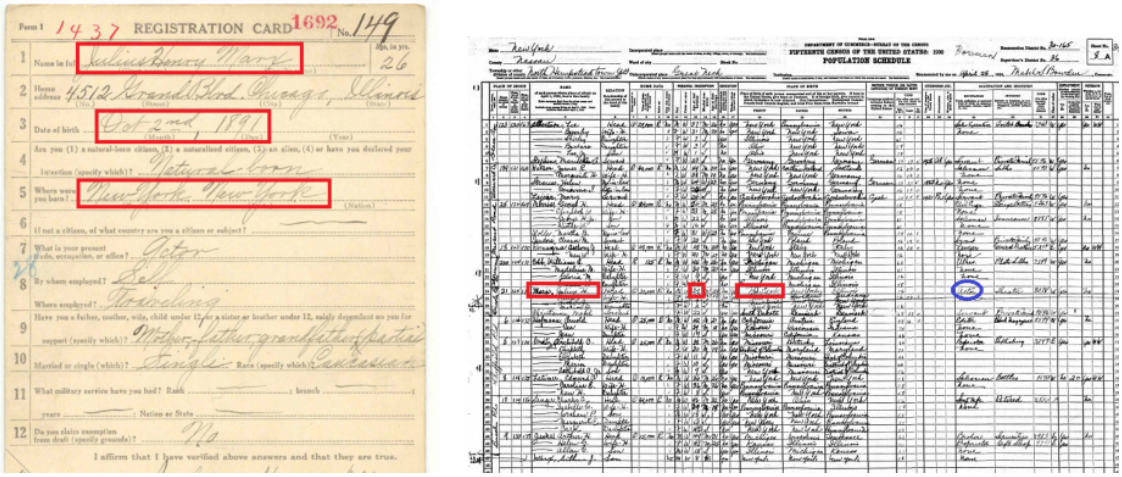


Figure 4: Record linking. *Left*: 1917 draft registration card; each registrant received a unique lottery number recorded in the top-left corner. *Right*: the same man's 1930 Federal Census record, with the matched entry highlighted in red. The linking method follows Abramitzky et al. (2012): similar names, same state of birth, ±2 year window around birth year.

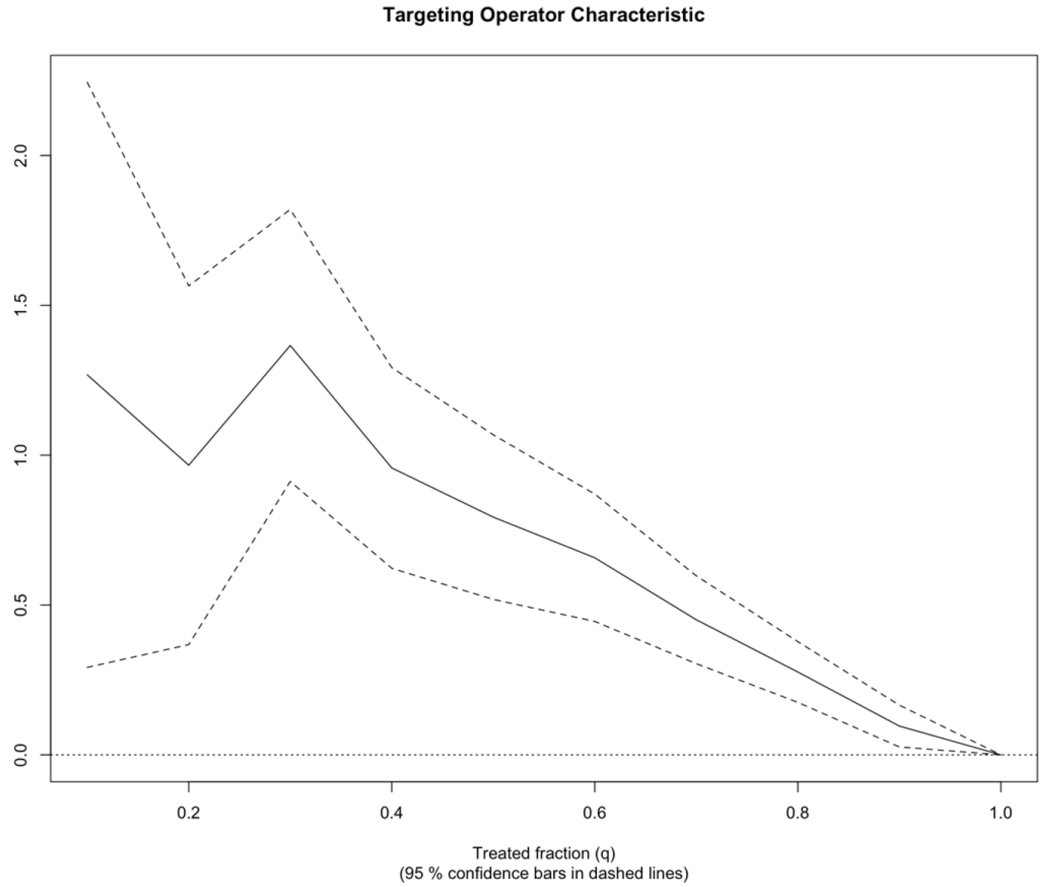


Figure 5: Targeting Operator Characteristic (TOC) curve for the effect of veteran status on 1930 occupational score. The figure reports the Targeting Operator Characteristic (TOC) curve derived from the Rank Average Treatment Effect (RATE) framework. The curve evaluates whether the causal forest successfully ranks individuals according to the magnitude of the predicted conditional average treatment effects (CATEs). The estimated effects refer to the impact of veteran status on 1930 occupational score. The causal forest uses the set of pre-war (1917) characteristics shown in Figure A.5 to predict treatment effect heterogeneity. Individuals are ordered by their predicted treatment effects, and the horizontal axis reports the fraction of the population targeted (q). The vertical axis reports the average treatment effect among the top q fraction of individuals with the highest predicted treatment effects. The solid line shows the estimated TOC curve, while dashed lines report 95% confidence intervals. A downward-sloping curve indicates that individuals predicted to benefit more from treatment indeed experience larger realized treatment effects, consistent with meaningful treatment effect heterogeneity captured by the causal forest.

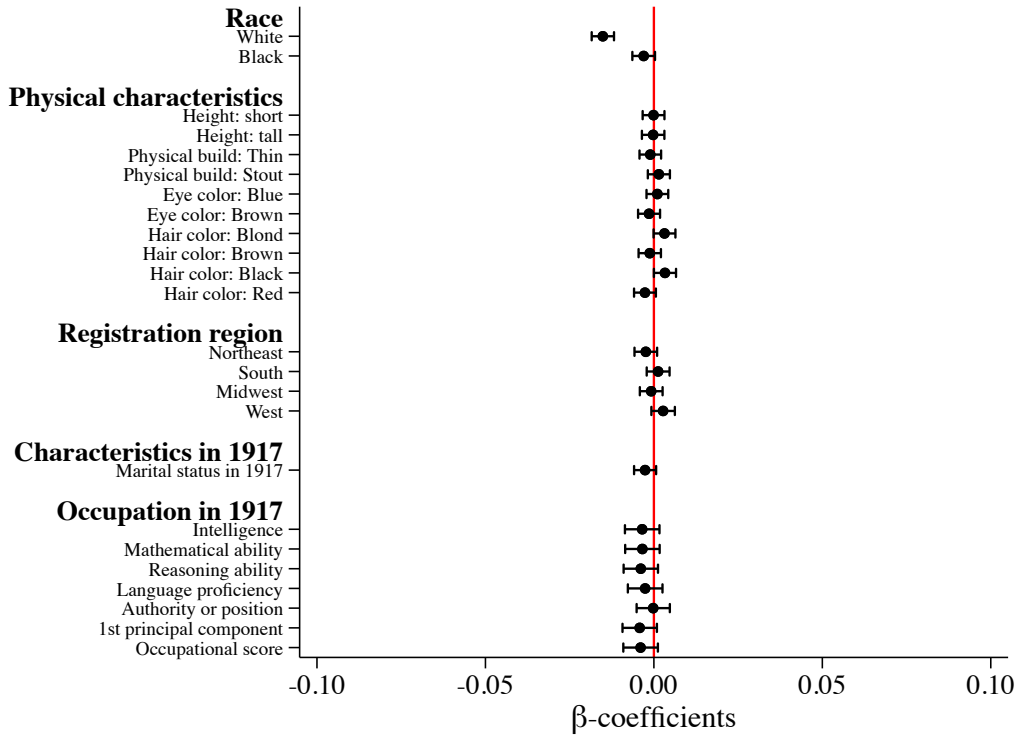


Figure 6: Balance of lottery assignment. Beta coefficients of the assigned order number in separate bivariate regressions, by pre-war characteristic. 95% confidence intervals with robust standard errors. No pre-war characteristic meaningfully predicts the lottery number.

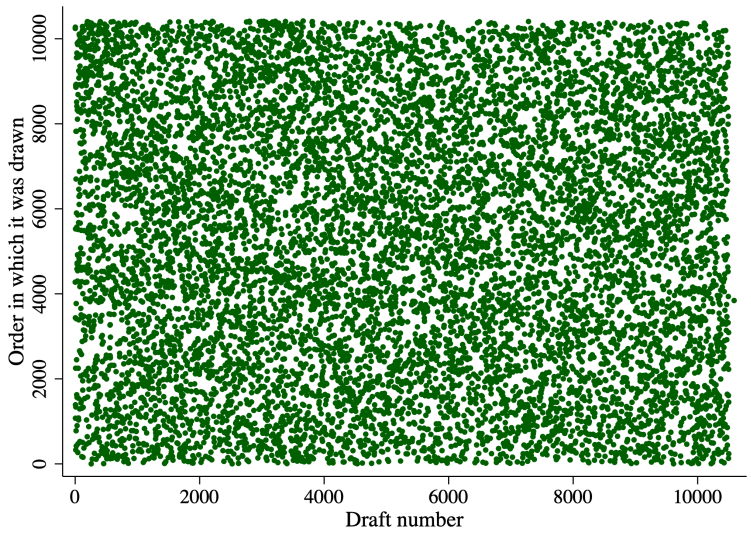


Figure 7: Randomness of the 1917 lottery. Horizontal axis: draft numbers as assigned to registrants. Vertical axis: order in which each number was drawn in Washington D.C. on 20 July 1917. No systematic pattern is visible.

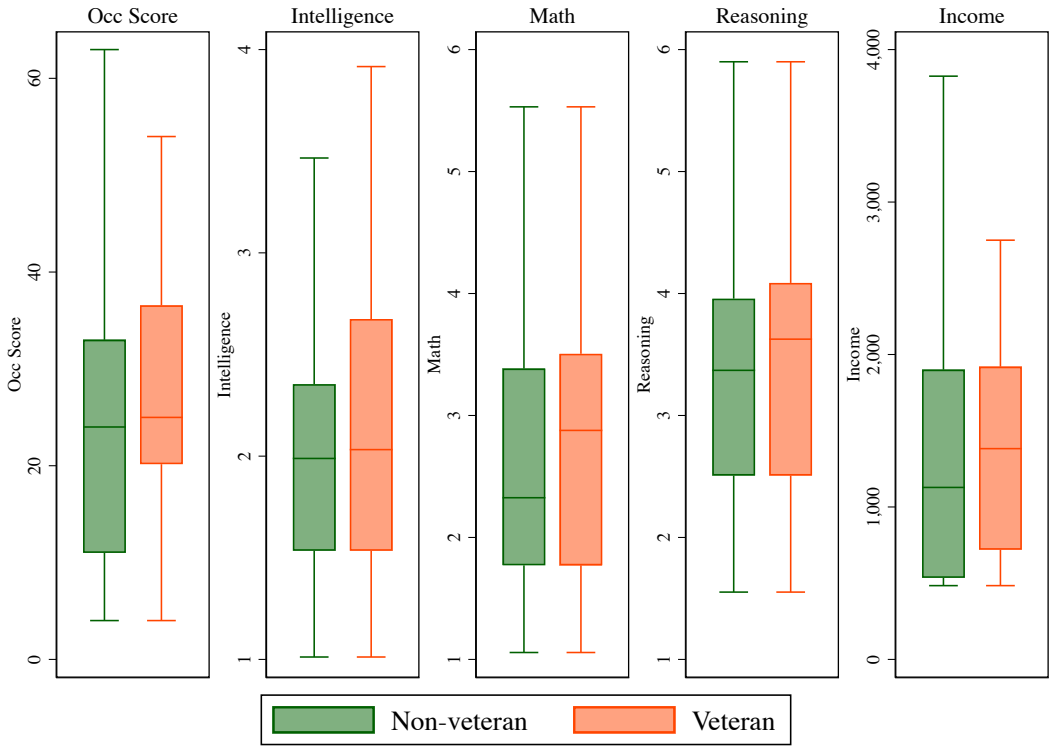


Figure 8: Occupational outcomes by veteran status. Boxplots of five outcome variables (1930 occupational score, intelligence, math, reasoning requirements, and 1930 income) comparing non-veterans (0) and veterans (1). Veterans occupy jobs with higher skill requirements and earn substantially more on average.

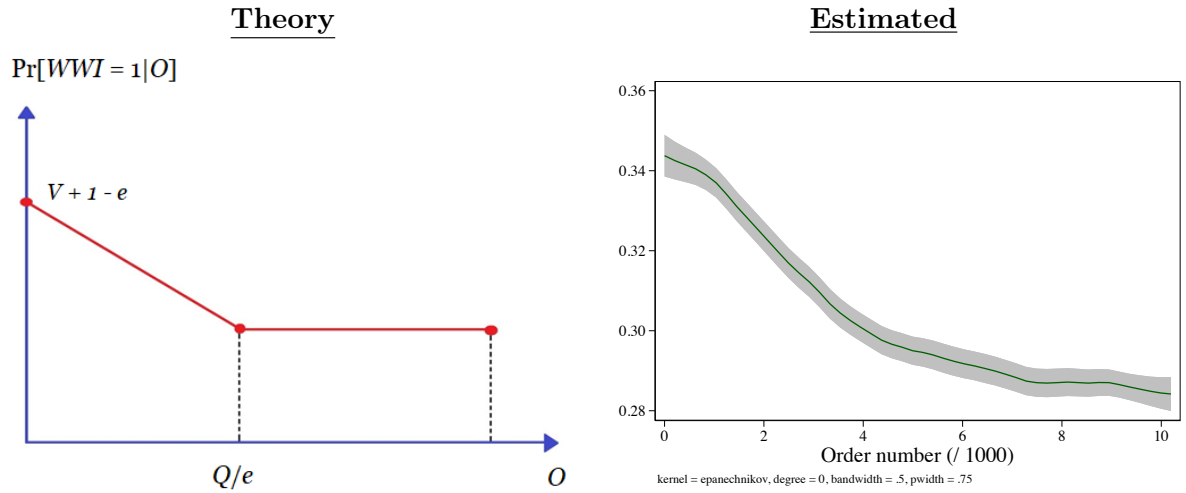


Figure 9: First stage: theory and estimated. *Left*: theoretical prediction for the probability of WWI service as a function of order number, given volunteering rate V, exemption probability e, and district quota Q/e. *Right*: estimated non-parametric relationship (local polynomial with 95% confidence band). The data closely match the theoretical prediction.

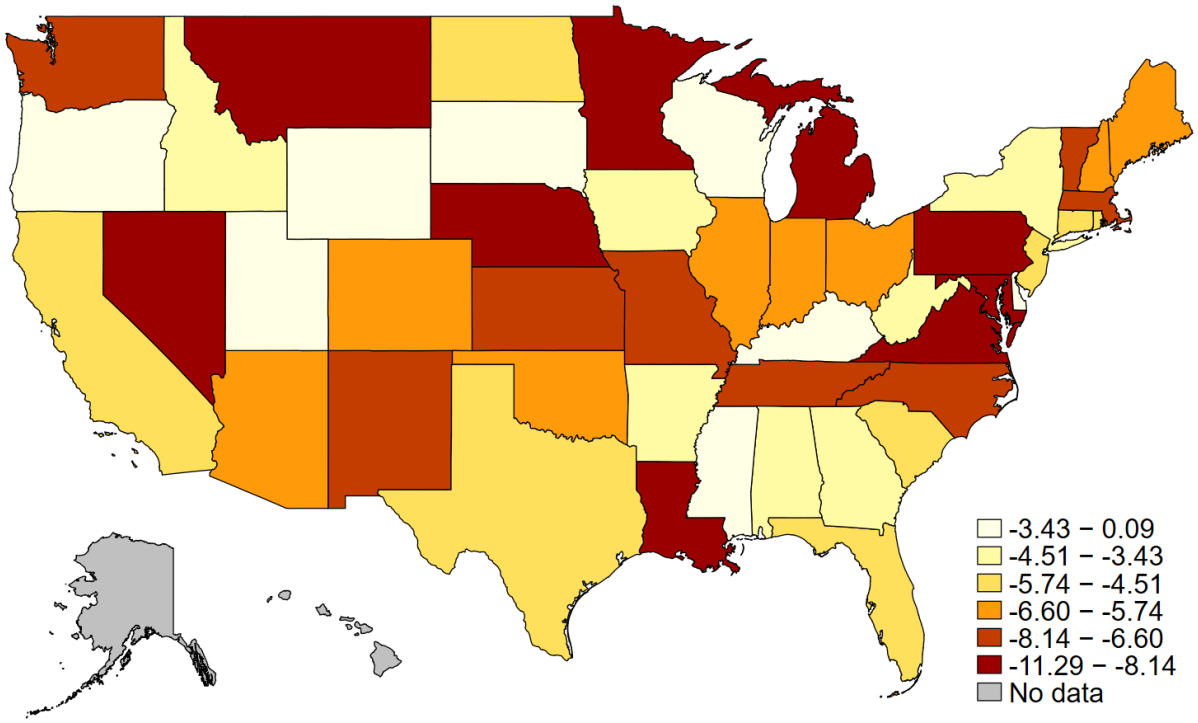


Figure 10: First stage across space. Point estimates of the first-stage regression (lottery order number → WWI veteran status) estimated separately for each of the 48 contiguous states. Darker shades indicate a stronger (more negative) relationship between order number and service. States with high prior volunteering rates tend to have weaker first stages.

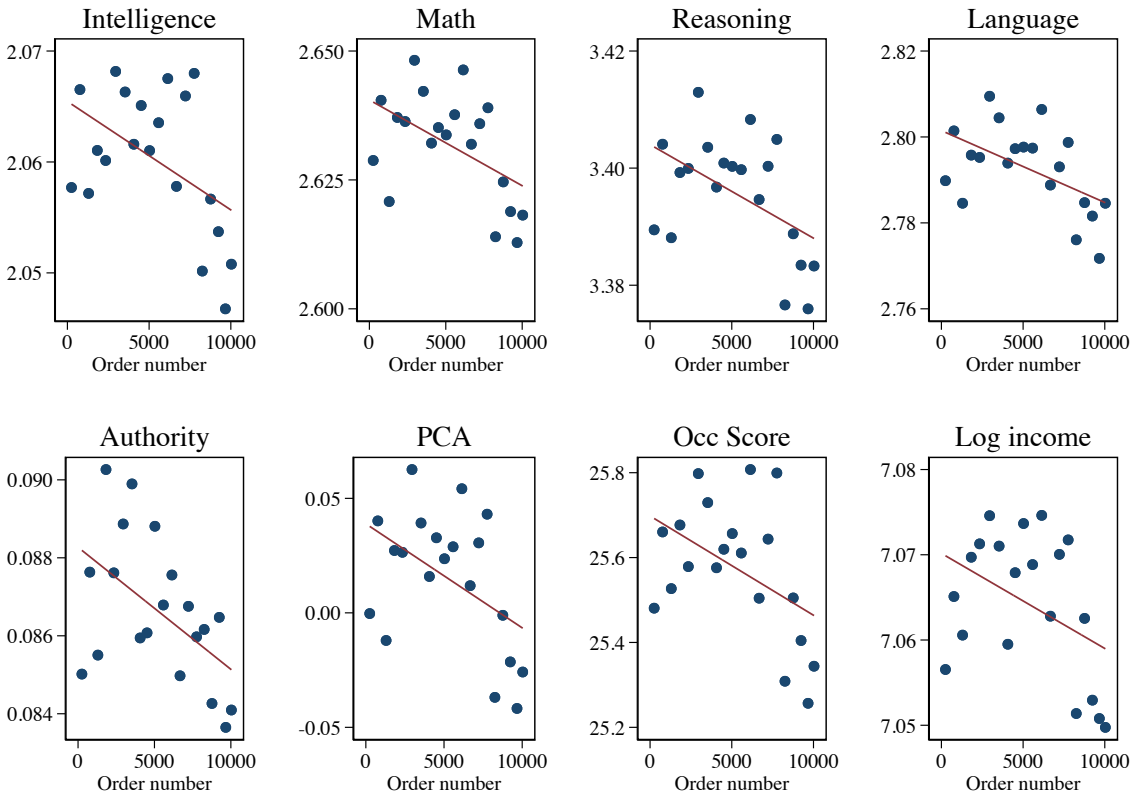


Figure 11: Lottery numbers and occupational outcomes in 1930. Each panel plots mean outcome (y-axis) against the lottery order number (x-axis) in 30 equal bins, with a fitted line. All eight outcomes—intelligence, mathematical ability, reasoning, language, authority, PCA index, occupational score, and log income—decline with higher order numbers, indicating worse outcomes for men less likely to be drafted.

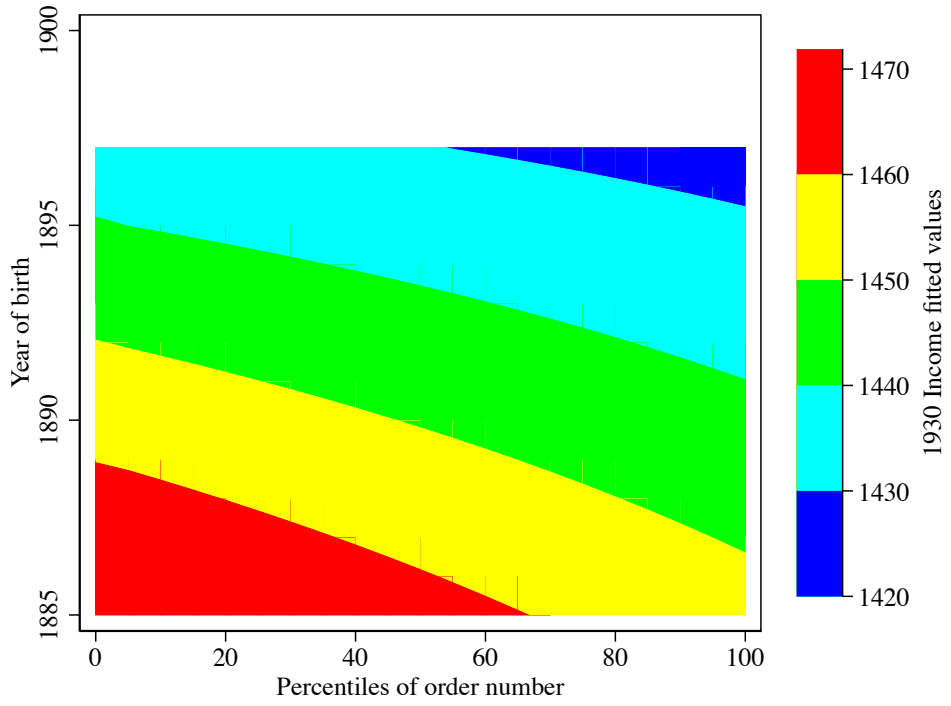


Figure 12: Expected 1930 income by birth cohort and lottery order number. Color indicates fitted income (from a flexible regression), ranging from low (blue) to high (red/yellow). The income gradient declines with order number and is steepest for the oldest cohorts, suggesting older men gained more from service.

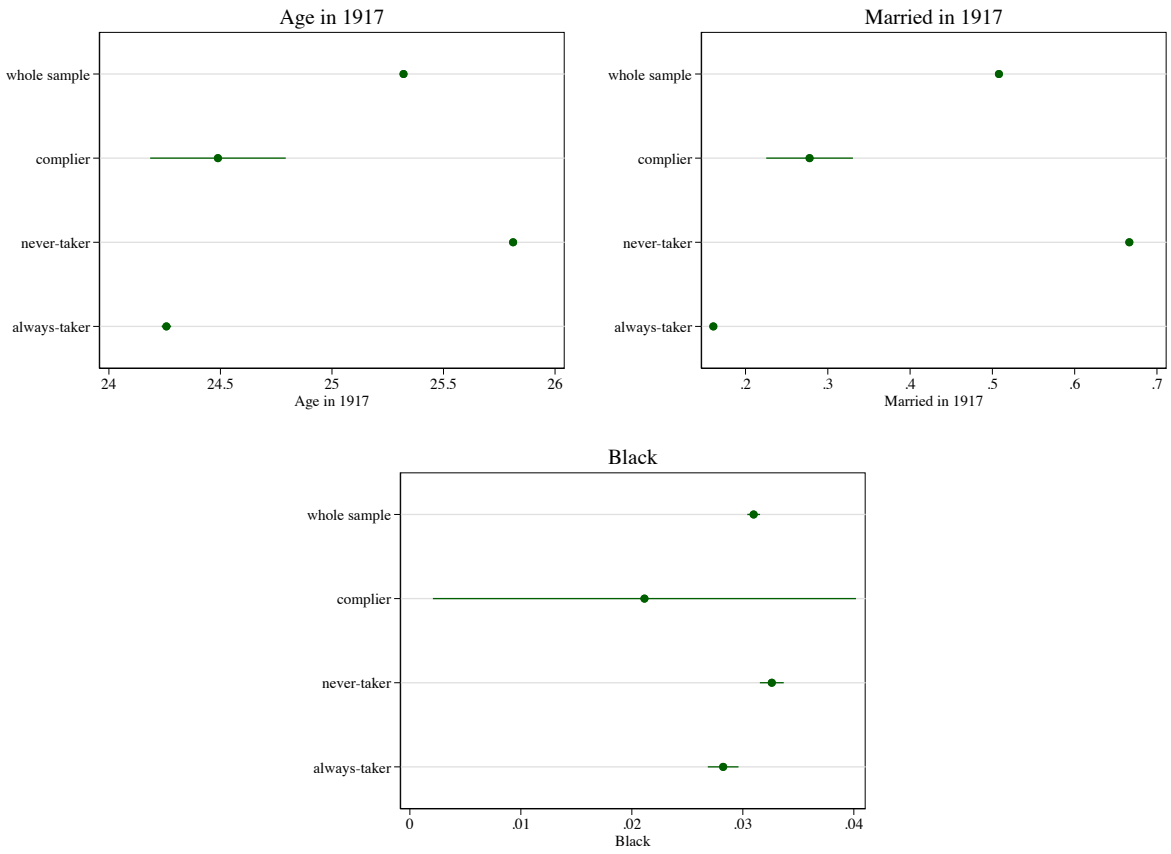


Figure 13: Complier characteristics: demographics. Mean age (1917), share married, and race, for the whole sample, compliers, never-takers, and always-takers. Compliers are younger, more likely to be single, and predominantly white.

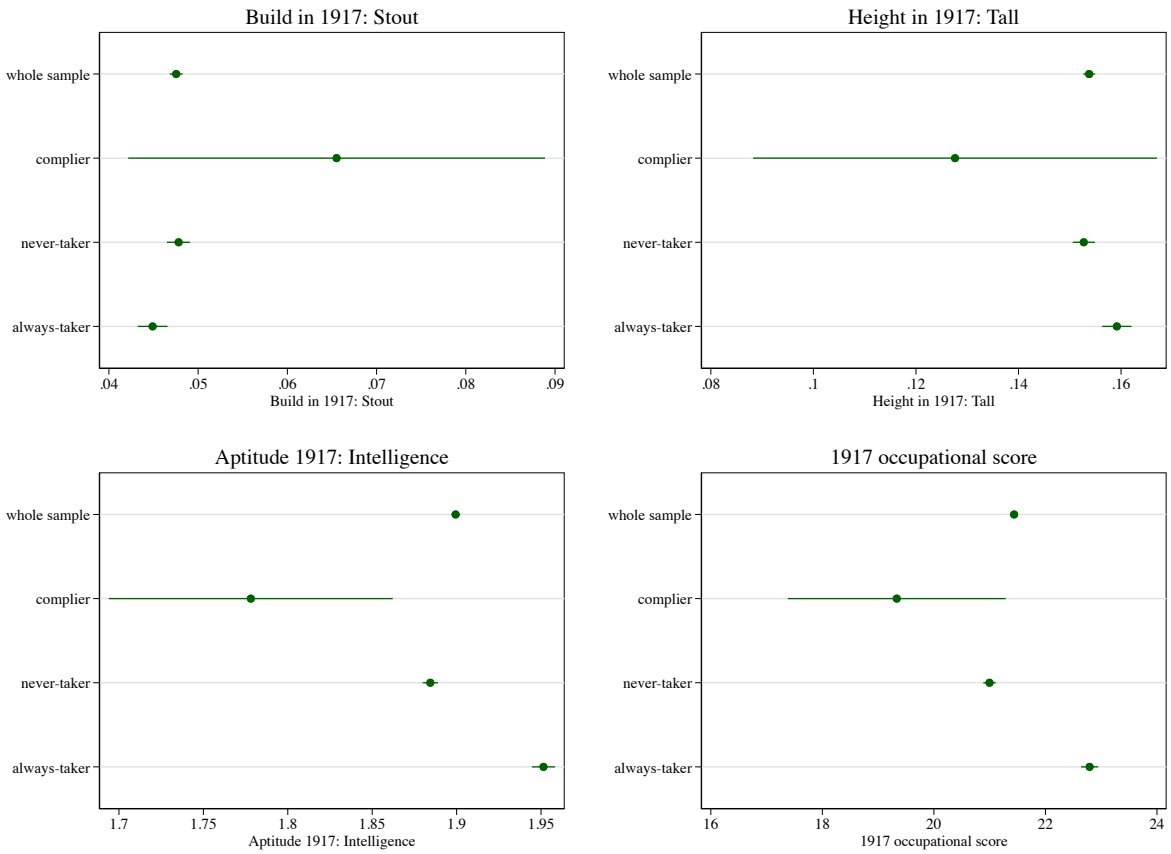


Figure 14: Complier characteristics: physical attributes and occupational aptitude. Build, height, intelligence score, and occupational score in 1917. Compliers are shorter, stouter, and had lower aptitude scores—consistent with negative selection at the margin of service.

	Population	Matched	Sample	Diff (Pop-Matched)	Diff (Matched-Sample)	t-stat (Sample-Batch)	t-stat (Matched-Batch)
Birth year 1886	0.046	0.042	0.042	0.004	0.000	12.0	-0.009
Birth year 1887	0.083	0.076	0.076	0.006	0.000	15.0	-0.003
Birth year 1888	0.093	0.086	0.086	0.007	0.000	17.0	0.018
Birth year 1889	0.090	0.084	0.084	0.006	0.000	15.0	0.011
Birth year 1890	0.089	0.085	0.085	0.004	0.000	9.6	-0.004
Birth year 1891	0.087	0.085	0.085	0.002	0.000	4.6	0.009
Birth year 1892	0.096	0.095	0.095	0.001	0.000	3.0	0.009
Birth year 1893	0.092	0.093	0.093	-0.001	0.000	-1.0	-0.037
Birth year 1894	0.094	0.097	0.097	-0.003	0.000	-7.0	0.004
Birth year 1895	0.095	0.099	0.099	-0.004	0.000	-10.0	0.004
Birth year 1896	0.089	0.101	0.101	-0.012	0.000	-28.0	-0.008
White	0.783	0.850	0.850	-0.067	0.000	-114.0	0.022
Black	0.094	0.030	0.030	0.064	0.000	155.0	0.267
First Name Freq.	201,372	110,937	111,226	90,145	-289	212.0	-0.791
Last Name Freq.	9,836	4,580	4,546	5,289	33	211.0	1.781
Born Abroad	0.251	0.081	0.081	0.170	-0.001	277.0	-1.000
Born Midwest	0.264	0.423	0.422	-0.158	0.001	-248.0	1.230
Born Northeast	0.175	0.205	0.204	-0.029	0.000	-53.0	0.504
Born South	0.279	0.227	0.228	0.051	-0.001	79.0	-1.000
Born West	0.032	0.064	0.065	-0.033	0.000	-128.0	-0.457
Observations	9,675,734	721,718	512,829				

Table 1: Sample selection and balance. Mean characteristics for the full 1917 lottery population, the matched sample (linked to both 1930 and 1940 Censuses), and the analysis sample. Columns (4) and (5) show mean differences; columns (6) and (7) report t-statistics. As typical in historical linking, matched men are more likely to be white and U.S.-born; the matched and analysis samples are near-identical.

Panel A. 1917 characteristics	Min	Mean	St. dev.	Max	Obs.
Age	21.000	25.324	2.841	30.000	350031
White	0.000	0.907	0.290	1.000	350031
Black	0.000	0.031	0.173	1.000	350031
Race missing	0.000	0.059	0.236	1.000	350031
Height: short	0.000	0.048	0.214	1.000	350031
Height: tall	0.000	0.154	0.361	1.000	350031
Physical build: thin	0.000	0.133	0.340	1.000	350031
Physical build: stout	0.000	0.048	0.213	1.000	350031
Eye color: Blue	0.000	0.212	0.409	1.000	350031
Eye color: Brown	0.000	0.162	0.369	1.000	350031
Hair color: Blond	0.000	0.071	0.256	1.000	350031
Hair color: Brown	0.000	0.272	0.445	1.000	350031
Hair color: Black	0.000	0.087	0.282	1.000	350031
Hair color: Red	0.000	0.011	0.106	1.000	350031
Registration: Northeast	0.000	0.221	0.415	1.000	350031
Registration: South	0.000	0.228	0.420	1.000	350031
Registration: Midwest	0.000	0.440	0.496	1.000	350031
Registration: West	0.000	0.111	0.314	1.000	350031
Married in 1917	0.000	0.513	0.500	1.000	350031
Intelligence	1.012	1.900	0.504	3.916	147383
Mathematical ability	1.057	2.380	0.843	5.531	147383
Reasoning ability	1.552	3.155	0.773	5.900	147383
Language proficiency	1.085	2.548	0.794	5.873	147383
Authority or position	0.000	0.047	0.143	0.882	147383
1st principal component	-4.023	-0.002	2.508	10.208	147383
Occupational score	4.009	21.441	11.778	79.998	147383

Panel B. 1930 characteristics	Min	Mean	St. dev.	Max	Obs.
WWI veteran	0.000	0.303	0.460	1.000	350031
Veteran (any war)	0.000	0.313	0.464	1.000	350031
Intelligence	1.012	2.060	0.571	3.916	350031
Mathematical ability	1.057	2.632	0.914	5.531	350031
Reasoning ability	1.552	3.396	0.836	5.900	350031
Language proficiency	1.085	2.793	0.906	5.873	350031
Authority or position	0.000	0.087	0.190	0.882	350031
1st principal component	-4.371	0.015	2.488	8.687	350031
Occupational score	4.009	25.574	12.606	79.998	350031

Table 2: Summary statistics. Panel A: characteristics of registrants at the time of the 1917 lottery ($N = 350{,}031$). Panel B: outcomes observed in the 1930 Census. Cognitive skill scores are from the Dictionary of Occupational Titles (intelligence 1–4; math, reasoning, language 1–6; authority 0–1). Occupational score is the IPUMS index (0–100); log income is measured at the occupation level.

	WWI veteran (0/1)				
	(1)	(2)	(3)	(4)	(5)
Order number (/1000)	-0.006***	-0.006***	-0.006***	-0.006***	-0.006***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Married before 1917			-0.412***		-0.381***
			[0.002]		[0.002]
Black				-0.020***	-0.015***
				[0.005]	[0.004]
Other race				-0.103***	-0.122***
				[0.016]	[0.015]
Race missing				0.044***	-0.065***
				[0.003]	[0.004]
Age FEs	No	Yes	No	No	Yes
R^2	0.002	0.059	0.202	0.002	0.214
Mean dep. var.	2.632	2.632	2.632	2.632	2.632
F-test of excluded instrument	513.9	601.5	674.9	526.1	686.8
Observations	357152	357152	357152	357152	357152

Table 3: First stage. Dependent variable: WWI veteran status (0/1). Key regressor: lottery order number. Columns (1)–(5) add controls progressively: (1) order number only; (2) adds age fixed effects; (3) adds marital status; (4) adds race; (5) full specification. F-statistics in the bottom row confirm instrument strength. Standard errors clustered at registration district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Order number (/1000)	-0.001***	-0.002***	-0.002***	-0.002***	-0.000***	-0.004***	-0.023***	-0.001***
	[0.000]	[0.001]	[0.000]	[0.001]	[0.000]	[0.001]	[0.007]	[0.000]
Married before 1917	-0.042***	-0.055***	-0.066***	-0.066***	-0.010***	-0.199***	-1.125***	-0.059***
	[0.003]	[0.004]	[0.004]	[0.004]	[0.001]	[0.012]	[0.059]	[0.003]
Black	-0.311***	-0.516***	-0.498***	-0.509***	-0.061***	-1.455***	-5.339***	-0.330***
	[0.007]	[0.010]	[0.010]	[0.009]	[0.001]	[0.029]	[0.168]	[0.008]
Other race	-0.236***	-0.375***	-0.366***	-0.396***	-0.042***	-1.060***	-3.540***	-0.224***
	[0.019]	[0.031]	[0.031]	[0.031]	[0.004]	[0.088]	[0.422]	[0.021]
Race missing	-0.028***	-0.041***	-0.039***	-0.046***	-0.008***	-0.113***	-0.327***	-0.026***
	[0.005]	[0.008]	[0.007]	[0.008]	[0.002]	[0.021]	[0.107]	[0.005]
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.011	0.011	0.013	0.012	0.004	0.013	0.008	0.011
Mean dep. var.	2.060	2.632	3.396	2.793	0.087	0.015	25.574	7.064
Observations	350031	350031	350031	350031	350031	350031	350031	349145

Table 4: Reduced form. Dependent variables: eight occupational skill requirements and log income in 1930. Key regressor: lottery order number. A negative coefficient means that men drawn with lower numbers (higher conscription risk) ended up in better, higher-paying jobs. Age fixed effects throughout. Standard errors clustered at registration district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel A: Baseline

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WWI veteran	0.154***	0.263***	0.255***	0.264***	0.052***	0.722***	3.789***	0.180***
	[0.053]	[0.085]	[0.078]	[0.084]	[0.018]	[0.231]	[1.163]	[0.057]
Married before 1917	0.017	0.047	0.033	0.036	0.010	0.080	0.338	0.011
	[0.021]	[0.033]	[0.030]	[0.033]	[0.007]	[0.090]	[0.453]	[0.022]
Black	-0.308***	-0.513***	-0.494***	-0.505***	-0.060***	-1.444***	-5.282***	-0.328***
	[0.007]	[0.010]	[0.010]	[0.010]	[0.001]	[0.029]	[0.168]	[0.008]
Other race	-0.217***	-0.342***	-0.335***	-0.363***	-0.036***	-0.971***	-3.075***	-0.202***
	[0.021]	[0.034]	[0.034]	[0.034]	[0.005]	[0.096]	[0.465]	[0.023]
Race missing	-0.018***	-0.024**	-0.023**	-0.029***	-0.004**	-0.066**	-0.081	-0.014**
	[0.006]	[0.010]	[0.009]	[0.009]	[0.002]	[0.026]	[0.132]	[0.007]
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	2.060	2.632	3.396	2.793	0.087	0.015	25.574	7.064
First stage F-stat.	672.851	672.851	672.851	672.851	672.851	672.851	672.851	676.943
Anderson-Rubin test (p-value)	0.004	0.002	0.001	0.002	0.004	0.002	0.001	0.002
Observations	350031	350031	350031	350031	350031	350031	350031	349145

Panel B: With state fixed effects

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WWI veteran	0.106**	0.177**	0.170**	0.194**	0.045**	0.467**	2.392**	0.112**
	[0.052]	[0.082]	[0.075]	[0.082]	[0.018]	[0.223]	[1.122]	[0.055]
Married before 1917	0.004	0.022	0.009	0.017	0.008	0.008	-0.069	-0.009
	[0.020]	[0.032]	[0.029]	[0.032]	[0.007]	[0.086]	[0.435]	[0.021]
Black	-0.275***	-0.445***	-0.428***	-0.460***	-0.058***	-1.241***	-3.985***	-0.267***
	[0.007]	[0.010]	[0.010]	[0.010]	[0.001]	[0.029]	[0.152]	[0.007]
Other race	-0.212***	-0.326***	-0.318***	-0.360***	-0.036***	-0.910***	-2.713***	-0.181***
	[0.020]	[0.031]	[0.031]	[0.032]	[0.005]	[0.089]	[0.437]	[0.022]
Race missing	-0.020***	-0.028***	-0.028***	-0.032***	-0.005**	-0.081***	-0.169	-0.018***
	[0.006]	[0.009]	[0.008]	[0.009]	[0.002]	[0.025]	[0.124]	[0.006]
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	2.060	2.632	3.396	2.793	0.087	0.015	25.574	7.064
First stage F-stat.	680.244	680.244	680.244	680.244	680.244	680.244	680.244	684.204
Anderson-Rubin test (p-value)	0.043	0.033	0.024	0.019	0.010	0.037	0.034	0.043
Observations	350031	350031	350031	350031	350031	350031	350031	349145

Table 5: Two-stage least squares estimates. Dependent variables: eight occupational skill measures and log income in 1930. Endogenous variable: WWI veteran status, instrumented with the 1917 lottery order number. Panel A: baseline with age fixed effects. Panel B: adds state of registration fixed effects. Standard errors clustered at registration district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel A: OLS regressions

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WWI veteran	0.149***	0.214***	0.225***	0.227***	0.029***	0.659***	3.473***	0.181***
	[0.004]	[0.007]	[0.006]	[0.007]	[0.001]	[0.018]	[0.095]	[0.005]
Married before 1917	0.030***	0.047***	0.038***	0.050***	0.004***	0.112***	0.351***	0.018***
	[0.004]	[0.006]	[0.006]	[0.006]	[0.001]	[0.017]	[0.084]	[0.004]
Black	-0.334***	-0.550***	-0.541***	-0.542***	-0.064***	-1.566***	-5.835***	-0.363***
	[0.008]	[0.013]	[0.012]	[0.012]	[0.001]	[0.036]	[0.208]	[0.010]
Other race	-0.308***	-0.465***	-0.481***	-0.474***	-0.043***	-1.377***	-5.372***	-0.324***
	[0.034]	[0.056]	[0.052]	[0.055]	[0.010]	[0.153]	[0.791]	[0.037]
Race missing	-0.034***	-0.051***	-0.046***	-0.050***	-0.005	-0.136***	-0.269	-0.022**
	[0.010]	[0.016]	[0.014]	[0.015]	[0.003]	[0.043]	[0.213]	[0.011]
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.024	0.021	0.026	0.023	0.008	0.025	0.021	0.026
Mean dep. var.	2.029	2.590	3.348	2.747	0.083	-0.121	24.761	7.025
Observations	141713	141713	141713	141713	141713	141713	141713	141444

Panel B: Reduced form

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Order number (/1000)	-0.001***	-0.002***	-0.002**	-0.002**	-0.000	-0.005***	-0.031***	-0.001
	[0.000]	[0.001]	[0.001]	[0.001]	[0.000]	[0.002]	[0.011]	[0.001]
Married before 1917	-0.024***	-0.029***	-0.041***	-0.033***	-0.007***	-0.119***	-0.841***	-0.044***
	[0.003]	[0.006]	[0.005]	[0.005]	[0.001]	[0.015]	[0.077]	[0.004]
Black	-0.334***	-0.547***	-0.540***	-0.543***	-0.064***	-1.565***	-5.889***	-0.365***
	[0.008]	[0.013]	[0.012]	[0.012]	[0.001]	[0.036]	[0.204]	[0.010]
Other race	-0.264***	-0.438***	-0.439***	-0.441***	-0.038***	-1.263***	-4.474***	-0.294***
	[0.033]	[0.051]	[0.046]	[0.050]	[0.008]	[0.136]	[0.680]	[0.031]
Race missing	-0.037***	-0.055***	-0.051***	-0.056***	-0.005	-0.152***	-0.339	-0.027**
	[0.010]	[0.016]	[0.014]	[0.015]	[0.003]	[0.042]	[0.212]	[0.011]
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.012	0.012	0.014	0.013	0.004	0.014	0.008	0.012
Mean dep. var.	2.031	2.593	3.352	2.749	0.083	-0.109	24.838	7.029
Observations	145226	145226	145226	145226	145226	145226	145226	144942

Panel C: Two-stage least squares

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WWI veteran	0.224***	0.336***	0.301**	0.310**	0.039	0.905***	5.275***	0.133
	[0.080]	[0.130]	[0.117]	[0.127]	[0.027]	[0.349]	[1.790]	[0.088]
Married before 1917	0.063**	0.102**	0.076*	0.088*	0.008	0.233*	1.213*	0.008
	[0.031]	[0.051]	[0.046]	[0.050]	[0.010]	[0.137]	[0.701]	[0.034]
Black	-0.331***	-0.542***	-0.535***	-0.538***	-0.063***	-1.551***	-5.809***	-0.363***
	[0.008]	[0.013]	[0.013]	[0.012]	[0.002]	[0.036]	[0.207]	[0.010]
Other race	-0.237***	-0.397***	-0.403***	-0.404***	-0.034***	-1.155***	-3.841***	-0.278***
	[0.036]	[0.055]	[0.050]	[0.054]	[0.009]	[0.148]	[0.750]	[0.033]
Race missing	-0.033***	-0.048***	-0.045***	-0.050***	-0.004	-0.133***	-0.226	-0.024**
	[0.010]	[0.016]	[0.014]	[0.015]	[0.003]	[0.043]	[0.215]	[0.011]
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	2.031	2.593	3.352	2.749	0.083	-0.109	24.838	7.029
Observations	145226	145226	145226	145226	145226	145226	145226	144942

Table 6: Coarsened exact matching estimates. All panels use samples restricted to treated and control individuals matched exactly on: marital status, race, age class, Army Service Command region, and quartiles of 1917 occupational variables. Panel A: OLS, treatment = WWI veteran. Panel B: reduced form, treatment = order number. Panel C: 2SLS instrumented with order number. Standard errors clustered at registration district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Not called	Called	Total
Non-veteran	145870	103278	249148
Veteran	59198	48806	108004
Total	205068	152084	357152

Table 7: Sample composition by draft status and veteran status. Rows: veteran status. Columns: whether called for service. Never-takers did not serve even after being called (103,278); always-takers served without being called (59,198); compliers plus always-takers served when called (48,806).

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Order number × 21-25 y.o.	0.000	-0.000	0.000	0.000	0.000	0.000	-0.004	-0.000
	[0.001]	[0.001]	[0.001]	[0.001]	[0.000]	[0.003]	[0.014]	[0.001]
Order number (/1000)	-0.001**	-0.001*	-0.002**	-0.002**	-0.000***	-0.004**	-0.021**	-0.001**
	[0.000]	[0.001]	[0.001]	[0.001]	[0.000]	[0.002]	[0.010]	[0.001]
21-25 y.o.	-0.050***	-0.070***	-0.063***	-0.079***	-0.002	-0.204***	-0.886***	-0.031***
	[0.006]	[0.010]	[0.009]	[0.010]	[0.002]	[0.027]	[0.135]	[0.007]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.011	0.011	0.013	0.012	0.004	0.013	0.008	0.011
Mean dep. var.	2.060	2.632	3.396	2.793	0.087	0.015	25.574	7.064
Observations	350031	350031	350031	350031	350031	350031	350031	349145

Table 8: Age heterogeneity. Reduced form interacting order number (/1000) with an indicator for being 21–25 years old in 1917 ("Young"). The interaction is generally small and insignificant. Standard errors clustered at registration district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Order number × volunteering rate	0.004**	0.005**	0.006**	0.005**	-0.000	0.016**	0.100***	0.005***
	[0.002]	[0.003]	[0.002]	[0.003]	[0.001]	[0.007]	[0.037]	[0.002]
Order number (/1000)	-0.002***	-0.003***	-0.003***	-0.003***	-0.000	-0.009***	-0.051***	-0.003***
	[0.001]	[0.001]	[0.001]	[0.001]	[0.000]	[0.003]	[0.016]	[0.001]
WWI volunteering rate	0.158***	0.256***	0.263***	0.232***	0.032***	0.784***	4.160***	0.218***
	[0.018]	[0.029]	[0.028]	[0.027]	[0.004]	[0.083]	[0.440]	[0.022]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.016	0.016	0.018	0.015	0.005	0.018	0.014	0.018
Mean dep. var.	1.985	2.509	3.272	2.683	0.073	-0.353	23.633	6.962
Observations	239161	239161	239161	239161	239161	239161	239161	238613

Table 9: Volunteering rate and the returns to service. Reduced form interacting order number (/1000) with the 1917 district-level volunteering rate. Greater prior volunteering is associated with larger gains for lottery-induced draftees, consistent with negative selection of marginal compliers. Standard errors clustered at registration district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	CEM			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
	Highest Grade	High School	College	Highest Grade	High School	College
WWI veteran father	0.432***	0.051***	0.029***	1.580**	0.248**	0.051
	[0.018]	[0.004]	[0.002]	[0.645]	[0.123]	[0.073]
Controls	No	No	No	Yes	Yes	Yes
CEM weights and strata FEs	Yes	Yes	Yes	No	No	No
R^2	0.422	0.262	0.185	.	.	.
Mean dep. var.	9.948	0.604	0.090	9.985	0.583	0.103
First stage F-stat.				112.371	112.371	112.371
Anderson-Rubin test (p-value)				0.013	0.043	0.485
Observations	82486	82486	82486	112752	112752	112752

Table 10: Education of veterans' children, 1940. Dependent variables: highest grade of school completed, high school completion, college attendance. Columns (1)–(3): Coarsened exact matching (CEM) estimates. Columns (4)–(6): 2SLS, instrumenting father's veteran status with his 1917 lottery order number. Control variables: age, father's age, race category, and father's race category. Standard errors clustered at father level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	CEM			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
	Highest Grade	High School	College	Highest Grade	High School	College
WWI veteran father	0.563***	-0.072***	0.118***	2.207	-0.093	0.390**
	[0.083]	[0.010]	[0.008]	[1.820]	[0.220]	[0.181]
Controls	No	No	No	Yes	Yes	Yes
CEM weights and strata FEs	Yes	Yes	Yes	No	No	No
R^2	0.208	0.208	0.213	.	.	.
Mean dep. var.	10.338	0.504	0.224	10.141	0.495	0.204
First stage F-stat.				35.353	35.353	35.353
Anderson-Rubin test (p-value)				0.222	0.672	0.027
Observations	14155	14155	14155	26133	26133	26133

Table 11: Education of veterans' children, 1950. Same specification as Table 10 using 1950 Census outcomes. The college completion advantage for veterans' children has strengthened by 1950.

A Variable Descriptions

This appendix provides detailed descriptions of each variable used in the analysis, including its source and construction.

Registrants. Number of men who registered for the draft lottery on 5 June 1917. The primary source is Crowder (1918), where registrant counts are recorded for each local registration board.

Volunteers. Number of men who voluntarily enlisted in the Army between the declaration of war (6 April 1917) and the date of the lottery (20 July 1917). The information is sourced from Crowder (1918), where it is recorded for each registration board as "registration credit." Boards were assigned credits based on how many volunteers had come forward before the lottery; credits were used to reduce each board's quota of men to be inducted through the lottery.

Region. Location where men registered during the draft lottery, divided into four broad Census regions: North-East, South, Mid-West, and West.

Service Command. Nine geographic organizational units assigned to separate Army administrative units. Used in the CEM analysis to form geographically homogeneous comparison groups.

Order Number. Constructed from NARA registration cards and the outcome of the lottery as reported by three newspapers on 21 July 1917 (*Altoona Tribune*, *Los Angeles Evening Herald*, *New York Times*). We first digitize the lottery number from the original draft card (Appendix Figure A.1). We then determine the order in which draft numbers were extracted during the lottery from the newspaper lists (Appendix Figure A.4). The order number therefore reflects the order in which each registrant was called by his local board.

Full Name. Extracted from the NARA registration cards filled out during lottery registration. We observe the same variable in the 1930 Federal Census and use it for record linking (see Appendix B).

State of Birth. Records the place of birth: either a U.S. state or a foreign country. Primary source: NARA registration cards. Also observed in the 1930 Federal Census and used for record linking (see Appendix B).

Age. Derived from NARA registration cards; records the registrant's age at the time of the draft lottery. Also observed in the 1930 Federal Census and used for record linking (see Appendix B).

Race. From NARA registration cards collected during lottery registration. Classifies men into: white, Black, American Indian, Chinese, Japanese, and Mexican or Latino. White and Black account for nearly all linked records.

Height. Categorizes registrants by height: short, medium, or tall. Recorded on NARA registration cards at the time of lottery registration.

Physical Build. Categorizes registrants by physical build: thin, medium, or stout. Recorded on NARA registration cards at the time of lottery registration.

Eye Color. Categorizes registrants by eye color: blue, brown, gray, or green (0.21% were green-eyed). Recorded on NARA registration cards at the time of lottery registration.

Hair Color. Categorizes registrants by hair color: blond, grey, white, brown, black, or red. Recorded on NARA registration cards.

Married in 1917. Binary variable indicating whether the registrant was married at the time of the 1917 lottery. Sourced from NARA registration cards.

WWI Veteran Status. Identifies whether the registrant was a WWI veteran, as recorded in the 1930 Federal Census. We require both an affirmative answer to the question on veteran status and the specific identification of "WWI" as the period of service.

1930 Occupation. The occupational field of the registrant in the year 1930. Sourced from the 1930 U.S. Federal Census.

Skill requirement: intelligence. Skill variables are constructed from the 1930 occupation by matching to U.S. Department of Labor (1977). Intelligence, measured as the "aptitude" required by a specific job, has a scale from 1 to 4, with higher values indicating greater required aptitude.

Skill requirement: math. Constructed from the 1930 occupation. Measured using GED scores on a scale from 1 to 6, with higher scores indicating greater required mathematical ability.

Skill requirement: reasoning. Constructed from the 1930 occupation. Measured using GED scores on a scale from 1 to 6, with higher scores indicating greater required reasoning ability.

Skill requirement: language. Constructed from the 1930 occupation. Measured using GED scores on a scale from 1 to 6, with higher scores indicating greater required language proficiency.

Skill requirement: authority. Constructed from the 1930 occupation. Measures the importance that influencing people has on the job (the "temperament" dimension), on a scale from 0 to 1. Higher values indicate greater supervisory or leadership responsibility.

Skill requirement: principal component. The first principal component of the five DOT skill measures (intelligence, math, reasoning, language, authority). Captures the common factor in occupational cognitive demands.

Occupational Score. Created by IPUMS, this socioeconomic index (ranging from 0 to 100) provides information on the socioeconomic standing of a particular occupation. Constructed from 1930 occupational data.

Log Income. Occupation-level income for 1930, derived from the Census Bureau's Statistical Abstracts of the U.S. for the 1930s. Varies at the occupation level, not at the individual level. While these earnings do not capture variation at the individual level, they provide an additional dimension to our occupational scoring.

B Record Linkage

We reconstruct occupational outcomes of men who took part in the 1917 draft lottery by matching them to their own records in the 1930 U.S. Population Census. We link records using the Abramitzky et al. (2012) algorithm, extended with the automated linking procedures of Abramitzky et al. (2021).

We focus on male individuals in the 1930 Census (as all lottery registrants are male). We extract from both sources the following variables: first and last name, year of birth, and place of birth (U.S. state or foreign country). We standardize names in both the registration cards and the Census by first converting common male nicknames into their full corresponding names (e.g., "Ben" → "Benjamin") and then applying the NYSIIS phonetic equivalent of each name, so that names spelled differently but pronounced similarly are treated as the same name (e.g., "Voth" and "Vogt"). These standardizations are an effective way to deal with errors in reporting, transcription, and digitization (Abramitzky et al., 2021).

We then search for exact matches of the standardized first and last names across the two databases, requiring that matched individuals be born in the same U.S. state or foreign country and within a five-year birth-year window. The result is a many-to-many set of potential matches, with many individuals left unmatched in both databases. To minimize false matches and reduce noise, we apply two conservative conditions to identify valid matches. First, we drop anyone who within either database has at least one other person carrying the same first and last name born within a five-year window;

such individuals are potential duplicates whose matches are ambiguous. Second, we drop anyone who has more than one potential match across databases. While these conditions reduce the size of the matched sample considerably, they allow us to concentrate on high-quality matches with a high probability of correctly linking the same individual across sources.

C Additional Figures and Tables

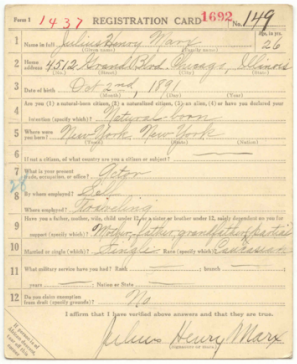


Figure A.1: Example of a 1917 draft registration card. The card records information for Julius Henry ("Groucho") Marx. The lottery number is recorded in red on the top-left corner ("1437"). We digitize lottery numbers and demographic information from a random sample of men who registered during the 1917 lottery.

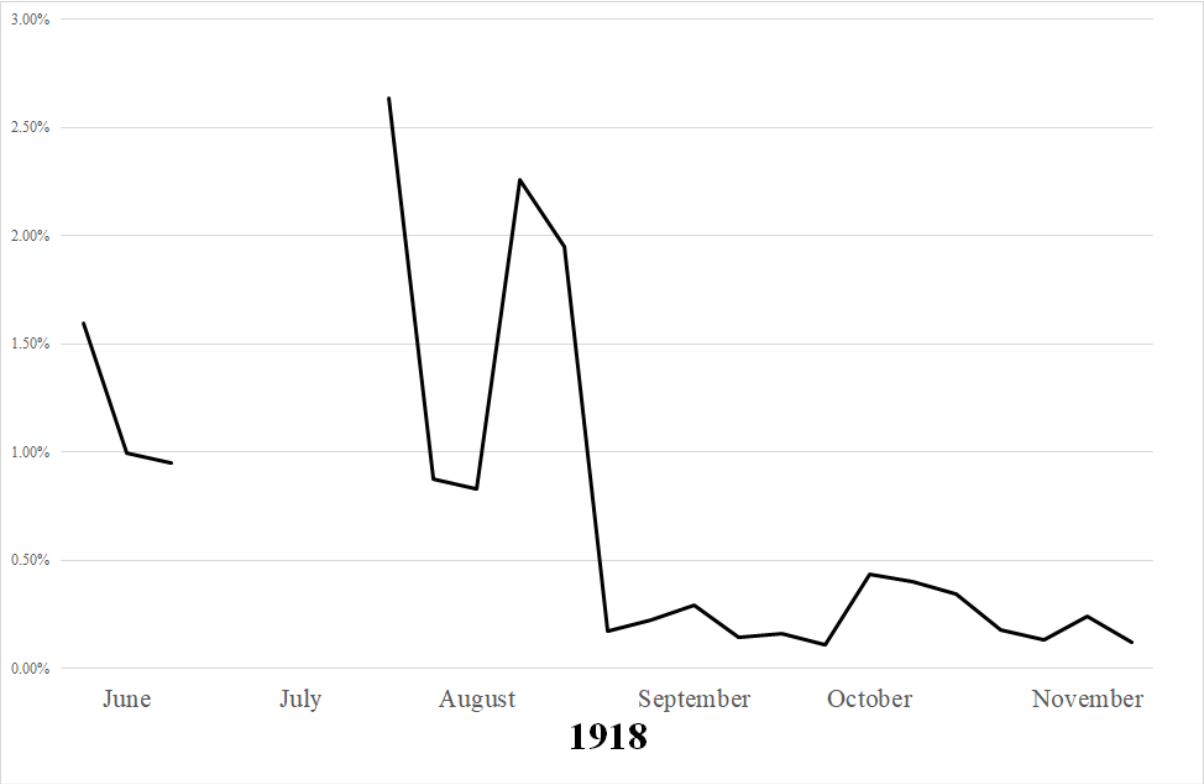


Figure A.2: Battle deaths as percent of men engaged, by month, 1918. Death rates peaked in July–August 1918 during the Aisne-Marne offensive and fell sharply by September as the AEF gained combat experience. Own calculations based on Ayres (1919).

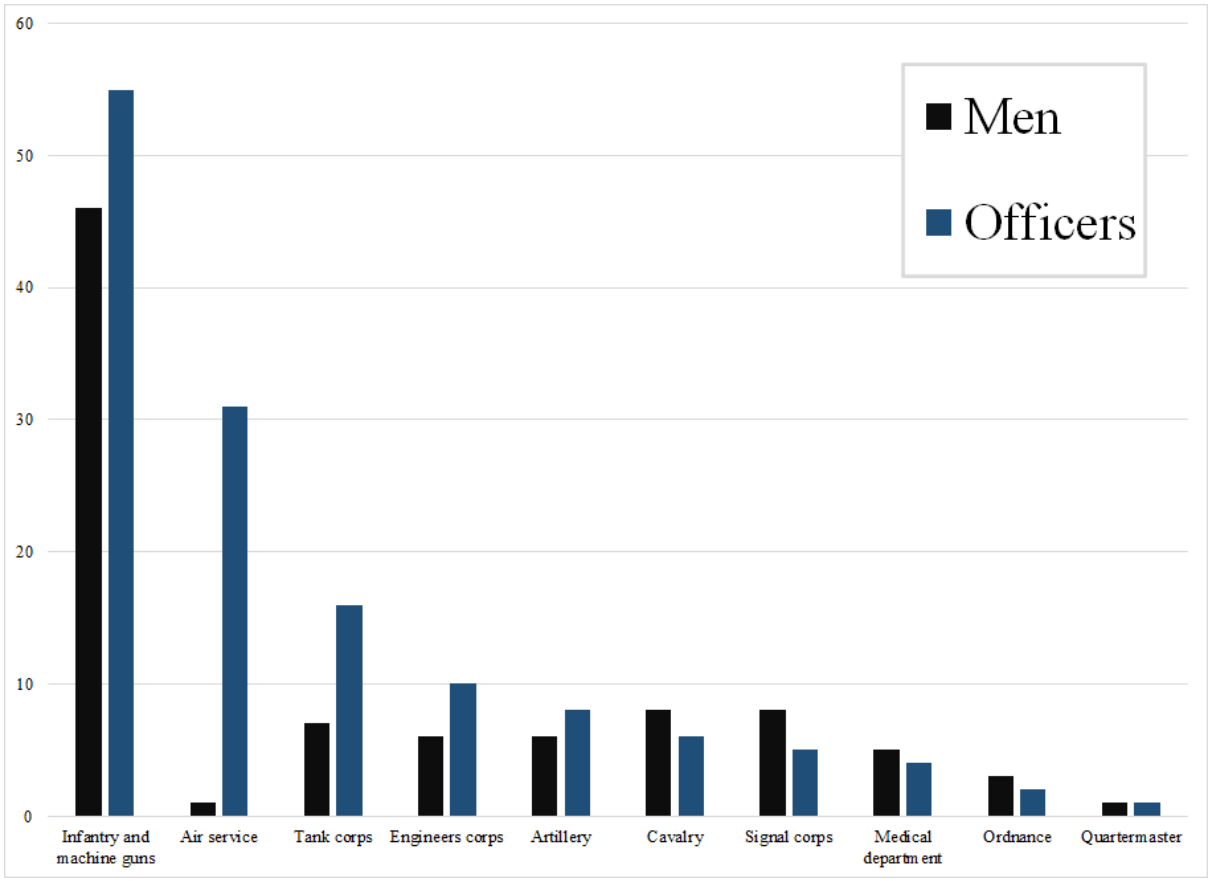


Figure A.3: Battle deaths per thousand men, by branch of service. Infantry officers, selected from the best draftees, faced approximately 20% higher fatality rates than the average enlisted man. Source: Ayres (1919).

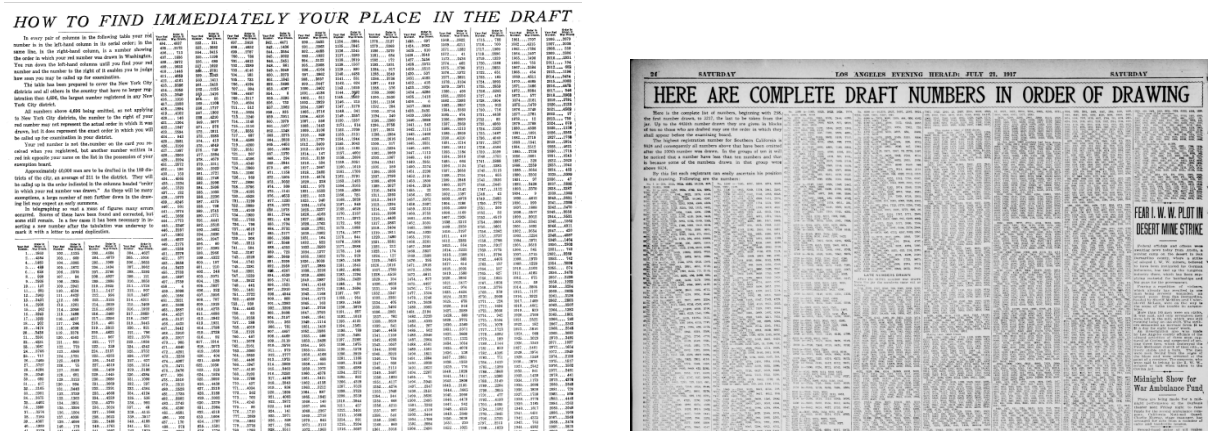


Figure A.4: Order of draft numbers as reported in the press. *Left*: page from the *New York Times*, 21 July 1917. *Right*: page from the *Los Angeles Evening Herald*, 21 July 1917. We digitize both lists and cross-check them to minimize transcription errors.

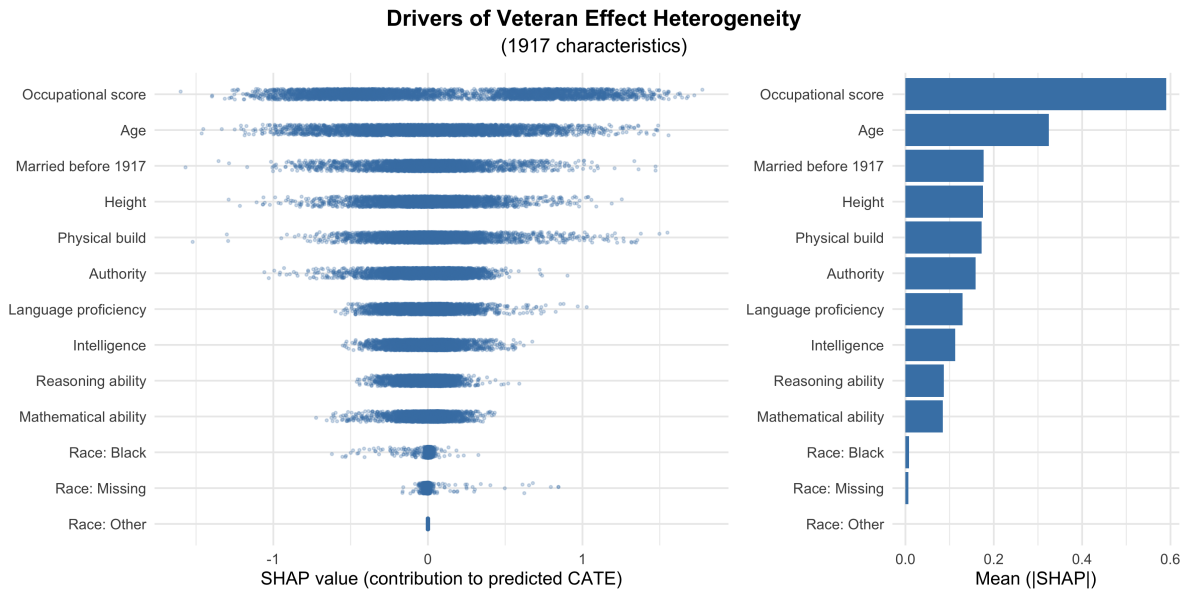


Figure A.5: Drivers of veteran effect heterogeneity. The figure reports SHAP (Shapley Additive Explanations) values summarizing the contribution of each pre-war characteristic to heterogeneity in the conditional average treatment effects (CATEs) estimated by the causal forest. The estimated contributions refer to the effect of veteran status on 1930 occupational score. The right panel ranks variables by their mean absolute SHAP value, indicating their relative importance in explaining variation in the predicted veteran effect. The left panel shows the distribution of SHAP values for each pre-war covariate across men, showing substantial heterogeneity. Positive SHAP values indicate that a covariate increases the predicted veteran effect on 1930 occupational score, while negative values indicate that it decreases it.

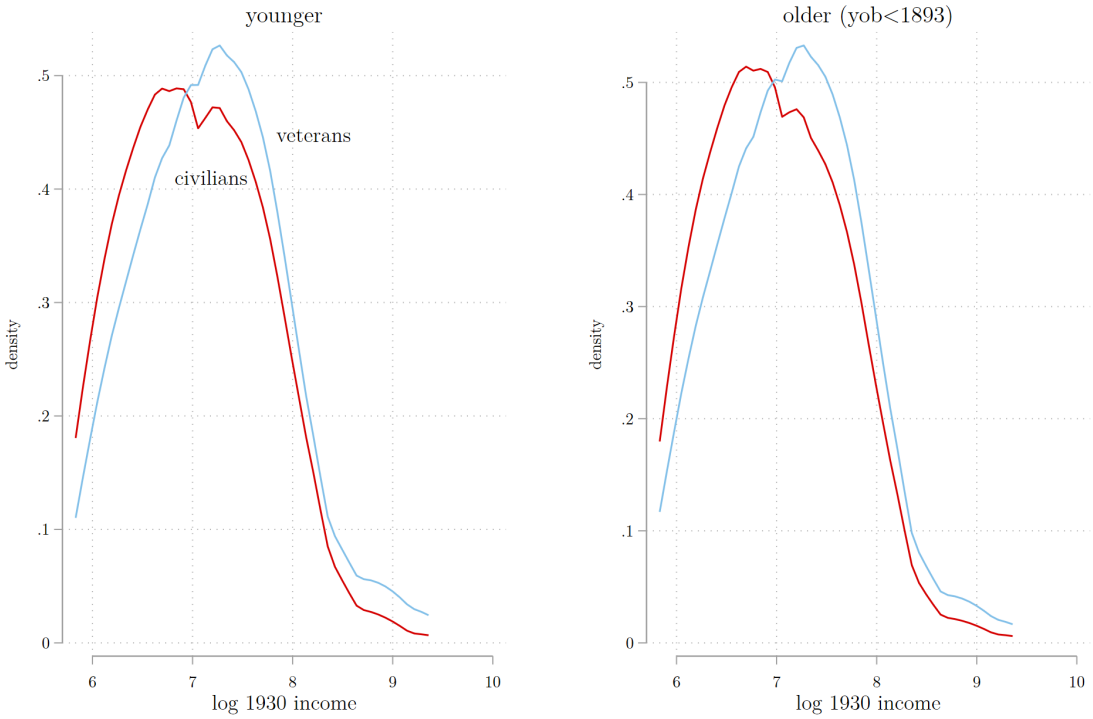


Figure A.6: Distribution of 1930 log earnings by age and veteran status. Kernel density estimates of log occupation-level income in 1930, separately for veterans (blue) and civilians (red), for younger (left) and older (right, born before 1893) cohorts. The veteran earnings advantage is visible in both age groups.

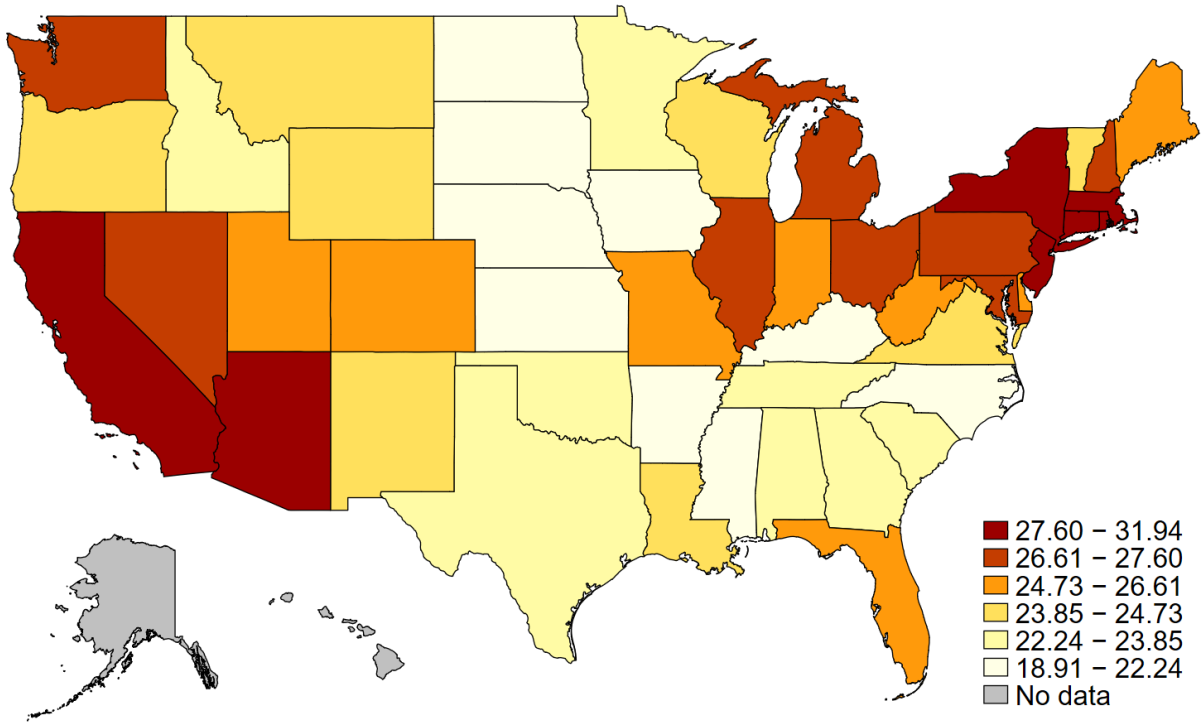


Figure A.7: Occupation scores in 1930, by state. Darker shading indicates higher average occupational scores. The North-East, the area around the Great Lakes, and California show relatively high scores; the South and the Mid-West tend to have lower occupational scores.

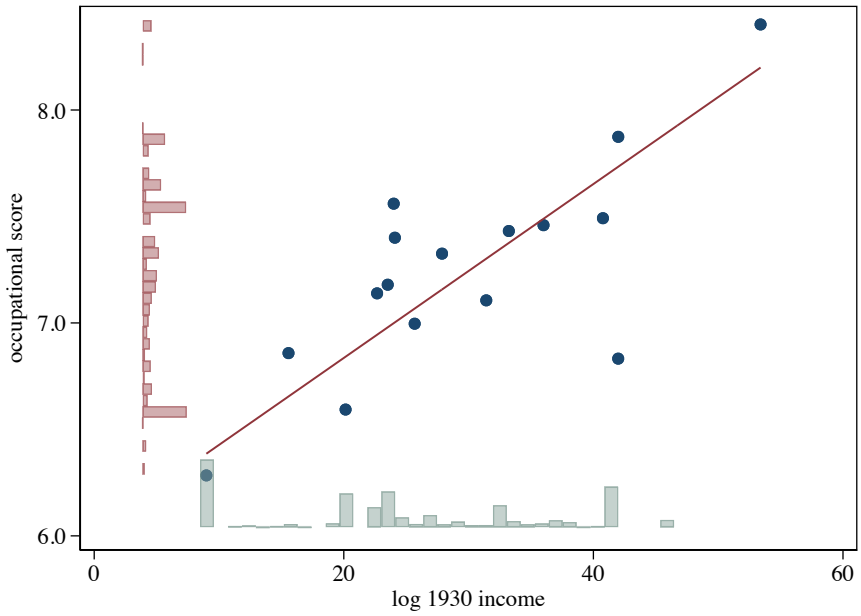


Figure A.8: Binscatter of occupational scores and log earnings, 1930. While the occupational score and log income are clearly correlated, a simple bivariate regression captures only 25% of the variation in log earnings, confirming that each measure adds distinct information about labor market standing.

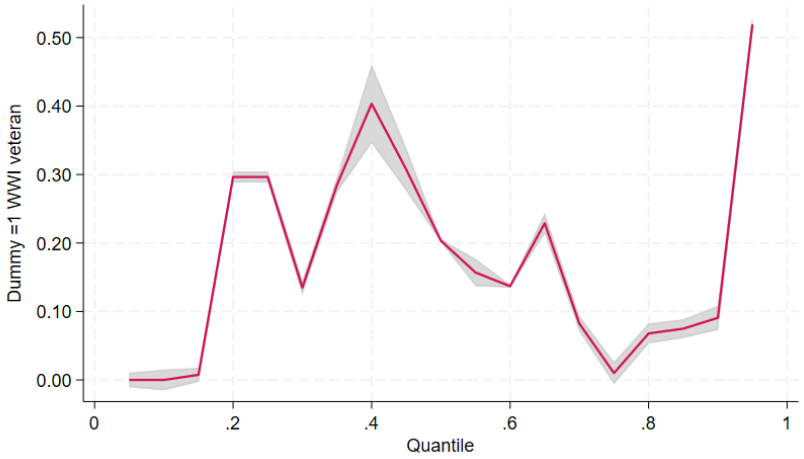


Figure A.9: Quantile regressions of 1930 income on veteran status. Conditional quantile effects from OLS quantile regression with full pre-war controls. The effect of veteran status is particularly pronounced at the 20th to 40th percentile of the income distribution.



Figure A.10: First stage by marital status. Non-parametric estimates of the relationship between order number and WWI veteran status, estimated separately for unmarried (*Left*) and married (*Right*) men. The order number significantly predicts service in both subsamples; married men have a lower baseline probability of service reflecting dependency exemptions.

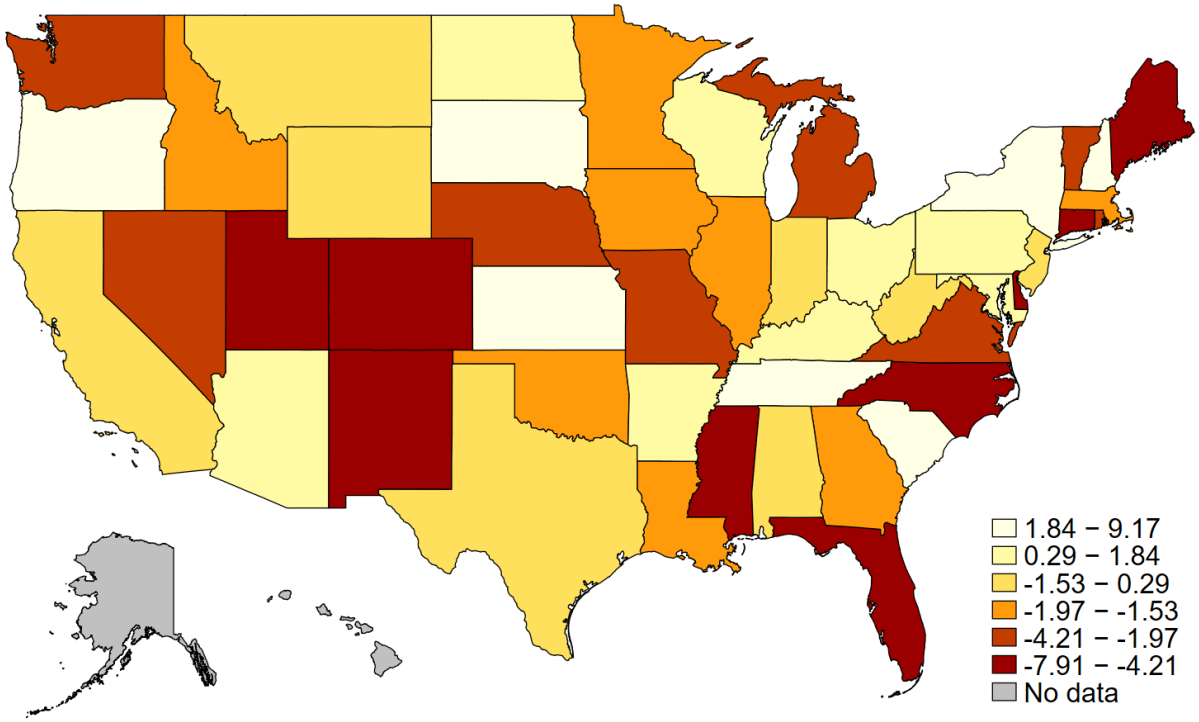


Figure A.11: Effect of order number on 1930 earnings, by state. Each state's shading reflects the estimated coefficient from a state-specific reduced form regression of log 1930 income on the lottery order number. While there is considerable variation, in most states men who received smaller order numbers earned more by 1930.

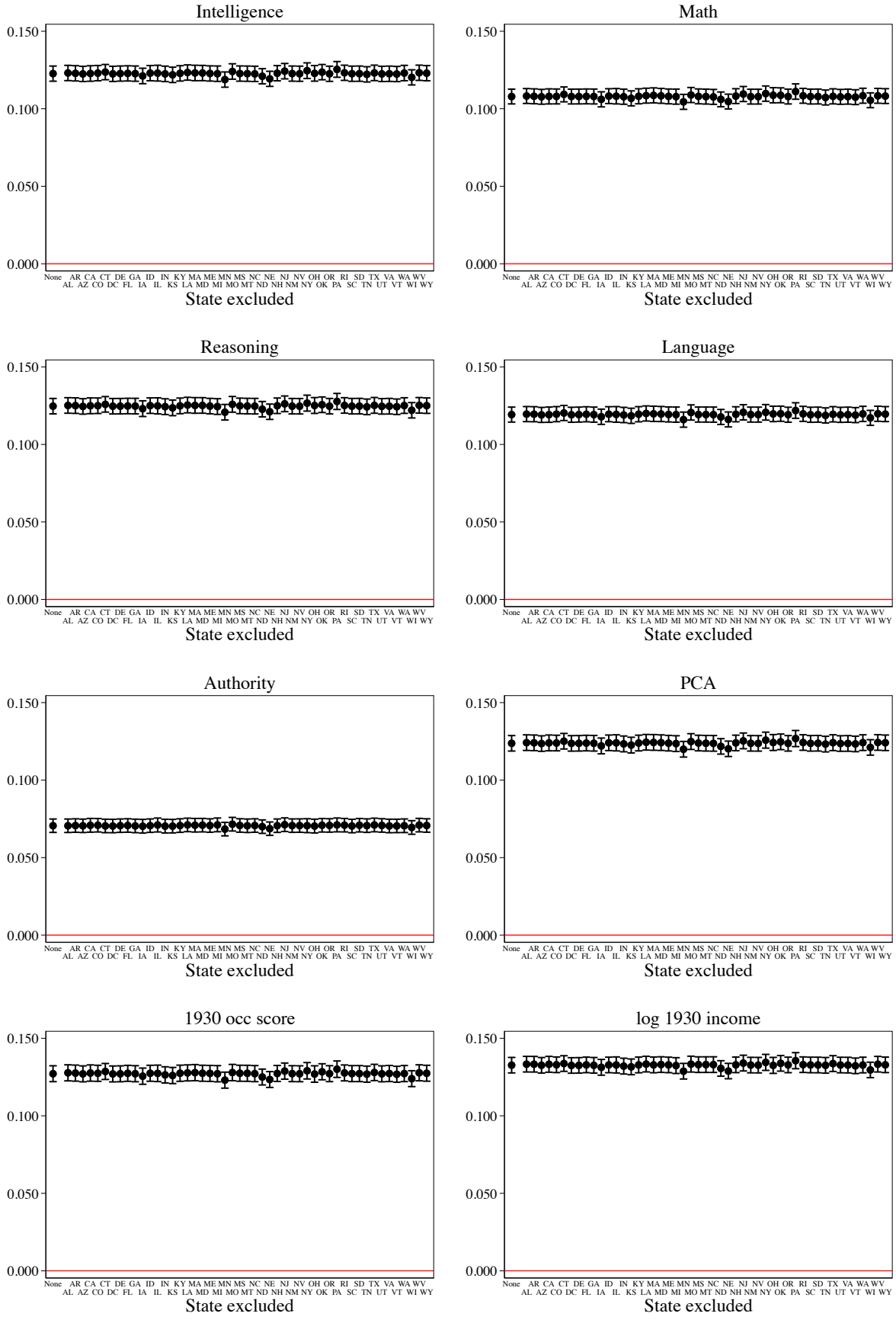


Figure A.12: Robustness to dropping one state at a time: OLS estimates. Each panel plots beta coefficients from OLS regressions of a dependent variable on WWI veteran status, estimated after dropping each state in turn. The leftmost coefficient in each panel reproduces the baseline result using all observations. Results are remarkably stable.

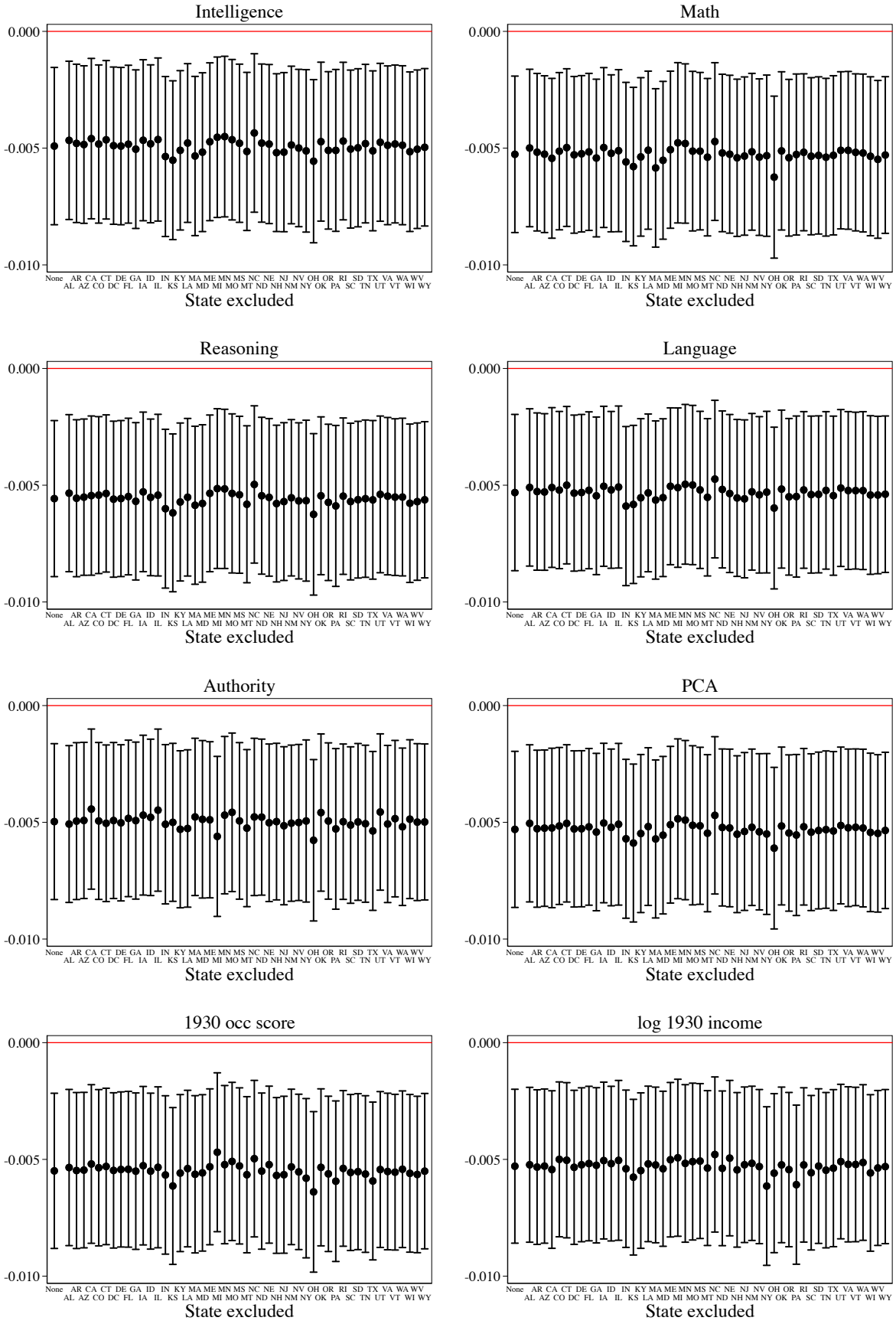


Figure A.13: Robustness to dropping one state at a time: reduced form. Each panel plots beta coefficients from reduced form regressions of a dependent variable on the order number, estimated after dropping each state in turn. The leftmost coefficient reproduces the baseline. Results are remarkably stable.

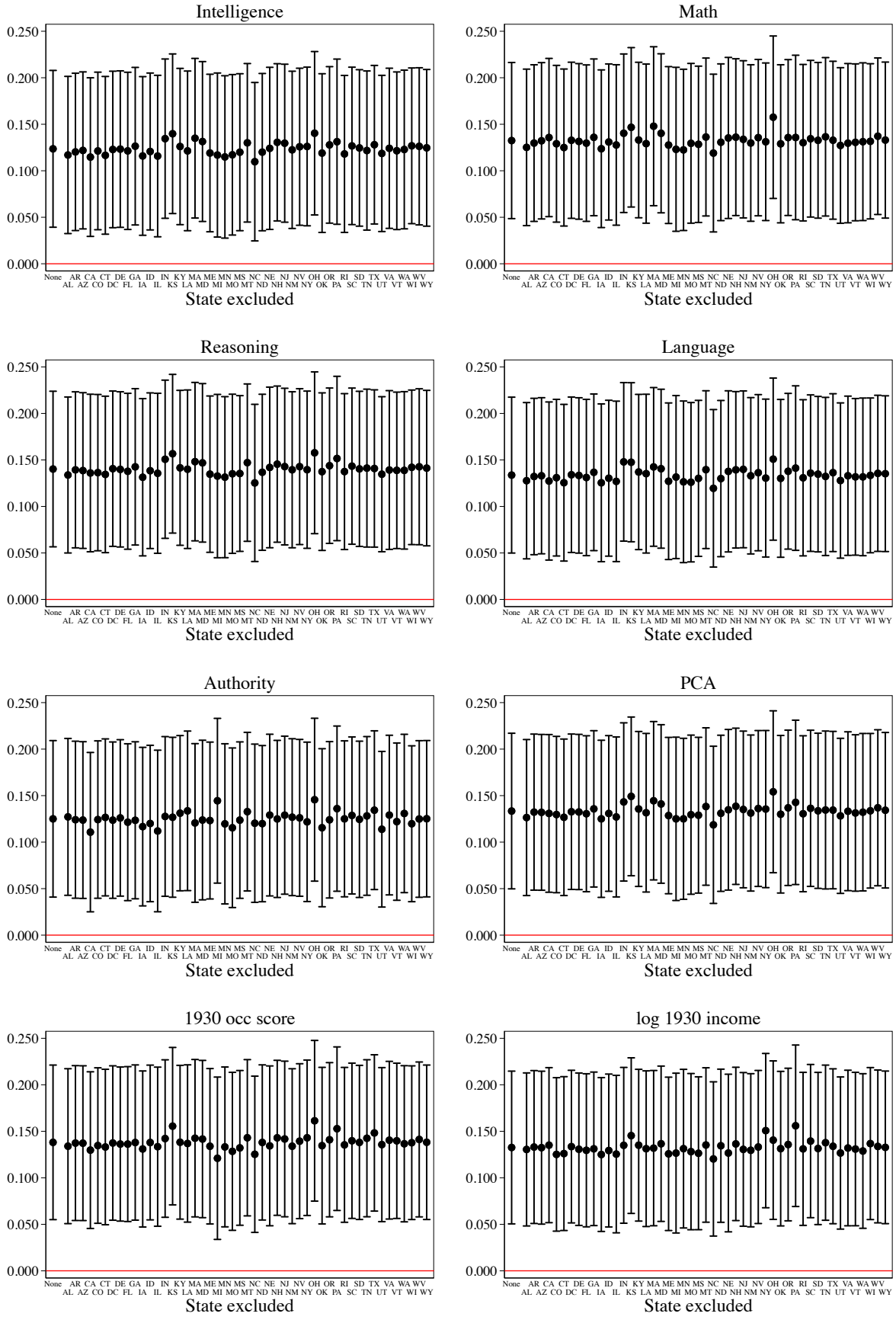


Figure A.14: Robustness to dropping one state at a time: 2SLS estimates. Each panel plots beta coefficients from 2SLS regressions of a dependent variable on instrumented veteran status, estimated after dropping each state in turn. The leftmost coefficient reproduces the baseline. Results are remarkably stable.

	1930 occ score			log 1930 income		
	(1)	(2)	(3)	(4)	(5)	(6)
	21-24	25-27	28-30	21-24	25-27	28-30
Order number (/1000)	0.003	-0.053**	-0.032*	0.000	-0.002	-0.002*
	[0.014]	[0.022]	[0.018]	[0.001]	[0.001]	[0.001]
Black	-4.503***	-4.079***	-4.254***	-0.297***	-0.285***	-0.279***
	[0.248]	[0.377]	[0.310]	[0.012]	[0.018]	[0.016]
Other race	-3.870***	-2.862***	-2.424***	-0.254***	-0.193***	-0.151***
	[0.741]	[1.022]	[0.752]	[0.039]	[0.052]	[0.039]
Race missing	-0.274*	-0.726*	-0.226	-0.027***	-0.043**	-0.003
	[0.149]	[0.416]	[0.352]	[0.007]	[0.021]	[0.017]
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.005	0.003	0.003	0.007	0.006	0.006
Mean dep. var.	25.998	26.208	26.110	7.089	7.097	7.092
Observations	77966	37690	54906	77751	37557	54719

Table A.1: Reduced form by age cohort. Coefficients of the effect of the lottery order number on 1930 occupational scores (columns 1–3) and log 1930 earnings (columns 4–6), estimated separately by age tercile (21–24, 25–27, 28–30). Older draftees show somewhat steeper gradients, consistent with the contour plot in Figure 12, though the formal age interaction in Table 8 is not significant. Standard errors clustered at registration district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel A: OLS regressions

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WWI veteran	0.153***	0.228***	0.236***	0.231***	0.028***	0.691***	3.660***	0.191***
	[0.004]	[0.007]	[0.007]	[0.007]	[0.001]	[0.019]	[0.099]	[0.005]
Married before 1917	0.015***	0.024***	0.011*	0.031***	0.003**	0.036*	-0.168*	-0.008*
	[0.004]	[0.007]	[0.006]	[0.006]	[0.001]	[0.018]	[0.092]	[0.005]
Black	-0.345***	-0.561***	-0.552***	-0.552***	-0.065***	-1.597***	-6.067***	-0.368***
	[0.009]	[0.015]	[0.014]	[0.014]	[0.002]	[0.040]	[0.231]	[0.011]
Other race	-0.234***	-0.369***	-0.385***	-0.380***	-0.030	-1.081***	-5.169***	-0.221**
	[0.073]	[0.124]	[0.112]	[0.116]	[0.024]	[0.334]	[1.591]	[0.088]
Race missing	-0.029***	-0.038**	-0.035**	-0.039**	-0.005	-0.101**	-0.091	-0.013
	[0.010]	[0.017]	[0.016]	[0.016]	[0.003]	[0.047]	[0.235]	[0.012]
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.024	0.022	0.027	0.023	0.007	0.026	0.025	0.029
Mean dep. var.	2.002	2.549	3.301	2.708	0.080	-0.256	23.984	6.983
Observations	112704	112704	112704	112704	112704	112704	112704	112507

Panel B: Reduced form

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Order number (/1000)	-0.002***	-0.003***	-0.003***	-0.003***	-0.000	-0.008***	-0.044***	-0.002***
	[0.001]	[0.001]	[0.001]	[0.001]	[0.000]	[0.002]	[0.011]	[0.001]
Married before 1917	-0.023***	-0.029***	-0.040***	-0.030***	-0.007***	-0.116***	-0.879***	-0.046***
	[0.004]	[0.006]	[0.006]	[0.006]	[0.001]	[0.017]	[0.083]	[0.004]
Black	-0.369***	-0.603***	-0.594***	-0.586***	-0.066***	-1.720***	-6.786***	-0.402***
	[0.008]	[0.013]	[0.013]	[0.012]	[0.001]	[0.036]	[0.216]	[0.010]
Other race	-0.245***	-0.396***	-0.408***	-0.418***	-0.041***	-1.170***	-4.950***	-0.273***
	[0.053]	[0.088]	[0.078]	[0.084]	[0.013]	[0.235]	[1.118]	[0.054]
Race missing	-0.030***	-0.043***	-0.041***	-0.046***	-0.005	-0.116***	-0.073	-0.015
	[0.010]	[0.017]	[0.015]	[0.016]	[0.003]	[0.045]	[0.227]	[0.011]
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.012	0.012	0.014	0.012	0.003	0.014	0.009	0.012
Mean dep. var.	2.010	2.564	3.318	2.719	0.080	-0.209	24.252	6.998
Observations	127267	127267	127267	127267	127267	127267	127267	127038

Panel C: Two-stage least squares

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WWI veteran	0.311***	0.441***	0.442***	0.426***	0.042	1.277***	6.907***	0.250***
	[0.079]	[0.129]	[0.116]	[0.123]	[0.026]	[0.345]	[1.781]	[0.087]
Married before 1917	0.098***	0.142***	0.132***	0.136***	0.010	0.380***	1.804***	0.051
	[0.031]	[0.050]	[0.045]	[0.048]	[0.010]	[0.135]	[0.696]	[0.034]
Black	-0.365***	-0.597***	-0.589***	-0.581***	-0.065***	-1.703***	-6.696***	-0.398***
	[0.008]	[0.013]	[0.013]	[0.013]	[0.002]	[0.037]	[0.219]	[0.010]
Other race	-0.211***	-0.348***	-0.359***	-0.371***	-0.036***	-1.030***	-4.194***	-0.246***
	[0.057]	[0.094]	[0.084]	[0.089]	[0.013]	[0.251]	[1.218]	[0.057]
Race missing	-0.023**	-0.032*	-0.030*	-0.035**	-0.004	-0.085*	0.097	-0.009
	[0.010]	[0.017]	[0.015]	[0.016]	[0.003]	[0.046]	[0.232]	[0.012]
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	2.010	2.564	3.318	2.719	0.080	-0.209	24.252	6.998
Observations	127267	127267	127267	127267	127267	127267	127267	127038

Table A.2: Coarsened exact matching with stricter conditions. All panels use samples restricted to treated and control men who share the exact same age and state of birth. Panel A: OLS regressions. Panel B: reduced form. Panel C: 2SLS. Relative to Table 6, the more demanding matching requirements yield larger coefficients, consistent with negative selection at the margin of service. Standard errors clustered at registration district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
WWI veteran	0.151	0.259	0.251	0.260	0.051	0.711	3.749	0.179
Cluster: registration board	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***
Conley s.e. (p-value): 10 Km cutoff	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***
Conley s.e. (p-value): 25 Km cutoff	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***
Conley s.e. (p-value): 50 Km cutoff	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***

Table A.3: Robustness to spatial inference. 2SLS coefficients for all main outcomes. Standard errors allowing for spatial correlation are computed following Conley (1999), for distance cutoffs: 10, 25, and 50 km. P-values in brackets. Results remain significant across all thresholds examined. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel A: OLS (robust regression)

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WWI veteran	0.148***	0.230***	0.229***	0.224***	0.001***	0.663***	3.131***	0.181***
	[0.002]	[0.004]	[0.004]	[0.004]	[0.000]	[0.010]	[0.052]	[0.003]
Married before 1917	0.025***	0.039***	0.032***	0.046***	-0.000***	0.086***	0.408***	0.018***
	[0.002]	[0.004]	[0.003]	[0.003]	[0.000]	[0.010]	[0.049]	[0.003]
Black	-0.315***	-0.537***	-0.519***	-0.511***	-0.000***	-1.493***	-5.389***	-0.338***
	[0.006]	[0.010]	[0.008]	[0.009]	[0.000]	[0.025]	[0.123]	[0.006]
Other race	-0.228***	-0.379***	-0.360***	-0.383***	-0.000	-1.031***	-3.158***	-0.213***
	[0.018]	[0.030]	[0.026]	[0.027]	[0.000]	[0.077]	[0.387]	[0.020]
Race missing	-0.019***	-0.031***	-0.026***	-0.033***	0.000	-0.078***	-0.123	-0.015***
	[0.005]	[0.008]	[0.007]	[0.007]	[0.000]	[0.020]	[0.102]	[0.005]
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.022	0.020	0.024	0.022	0.007	0.024	0.017	0.023
Mean dep. var.	2.060	2.632	3.396	2.793	0.087	0.015	25.574	7.064
Observations	350031	350031	350031	350031	350031	350031	350031	349145

Panel B: Reduced form (robust regression)

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Order number (/1000)	-0.001***	-0.002***	-0.002***	-0.002***	-0.000	-0.005***	-0.027***	-0.001***
	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.001]	[0.007]	[0.000]
Married before 1917	-0.033***	-0.049***	-0.056***	-0.044***	-0.001***	-0.170***	-0.803***	-0.053***
	[0.002]	[0.004]	[0.003]	[0.003]	[0.000]	[0.009]	[0.045]	[0.002]
Black	-0.316***	-0.542***	-0.525***	-0.514***	-0.000***	-1.504***	-5.431***	-0.340***
	[0.006]	[0.010]	[0.008]	[0.009]	[0.000]	[0.025]	[0.123]	[0.006]
Other race	-0.243***	-0.406***	-0.387***	-0.403***	-0.000	-1.107***	-3.515***	-0.233***
	[0.018]	[0.030]	[0.027]	[0.028]	[0.000]	[0.078]	[0.389]	[0.020]
Race missing	-0.028***	-0.046***	-0.041***	-0.046***	-0.000	-0.120***	-0.321***	-0.026***
	[0.005]	[0.008]	[0.007]	[0.007]	[0.000]	[0.020]	[0.102]	[0.005]
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.011	0.011	0.013	0.011	0.003	0.013	0.007	0.010
Mean dep. var.	2.060	2.632	3.396	2.793	0.087	0.015	25.574	7.064
Observations	350031	350031	350031	350031	350031	350031	350031	349145

Table A.4: Robustness to outliers and influential observations. Coefficients from regressions where influential observations are downweighted following Li (1985). Panel A: OLS regressions. Panel B: reduced form. Neither coefficients nor significance are materially affected by the downweighting. Standard errors clustered at registration district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Intelligence	Math	Reasoning	Language	Authority	PCA	Occ Score	log income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Veteran (any war)	0.150***	0.258***	0.250***	0.258***	0.051***	0.707***	3.710***	0.177***
	[0.052]	[0.083]	[0.076]	[0.083]	[0.017]	[0.226]	[1.139]	[0.056]
Married before 1917	0.017	0.047	0.033	0.036	0.010	0.080	0.336	0.011
	[0.021]	[0.033]	[0.030]	[0.033]	[0.007]	[0.090]	[0.452]	[0.022]
Black	-0.309***	-0.513***	-0.495***	-0.506***	-0.060***	-1.446***	-5.290***	-0.328***
	[0.007]	[0.010]	[0.010]	[0.010]	[0.001]	[0.029]	[0.168]	[0.008]
Other race	-0.217***	-0.342***	-0.335***	-0.363***	-0.036***	-0.970***	-3.069***	-0.201***
	[0.021]	[0.034]	[0.034]	[0.034]	[0.005]	[0.096]	[0.465]	[0.023]
Race missing	-0.018***	-0.024**	-0.023**	-0.029***	-0.004**	-0.066**	-0.081	-0.014**
	[0.006]	[0.010]	[0.009]	[0.009]	[0.002]	[0.026]	[0.132]	[0.007]
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	2.060	2.632	3.396	2.793	0.087	0.015	25.574	7.064
First stage F-stat.	702.843	702.843	702.843	702.843	702.843	702.843	702.843	705.739
Anderson-Rubin test (p-value)	0.004	0.002	0.001	0.002	0.004	0.002	0.001	0.002
Observations	350031	350031	350031	350031	350031	350031	350031	349145

Table A.5: Robustness to definition of veteran status. 2SLS coefficients where the endogenous variable is a dummy equal to one if the man reported being a veteran of any war, regardless of the specific war or expedition declared. The instrument is the 1917 lottery order number. Results are consistent with the baseline specification. Standard errors clustered at registration district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.