War and Marriage: Assortative Mating and the World War II G.I. Bill

Matthew Larsen  
Department of Economics  
UC Davis  
mflarsen@ucdavis.edu

T.J. McCarthy  
Department of Economics  
UC Davis  
tjmccarthy@ucdavis.edu

Jeremy Moulton  
Department of Public Policy  
UNC Chapel Hill  
moulton@email.unc.edu

Marianne E. Page  
Department of Economics  
UC Davis  
mepage@ucdavis.edu

Ankur Patel  
Department of Economics  
UC Davis  
ipatel@ucdavis.edu

September 2011

We would like to thank Rachana Bhatt, Daniel Fetter and Christine Schwartz for their helpful comments. We would also like to thank seminar participants in the All UC Labor Economics Workshop, American Education Finance and Policy Annual Meeting, University of Kentucky, University of Michigan Conference on the Long-Run Impacts of Early Life Events, and Society of Labor Economics annual meeting.
Abstract

We exploit between-cohort variation in the probability of military service to investigate how World War II and the G.I. Bill altered the structure of marriage, and find that they had important spillover effects beyond their direct effect on men’s educational attainment. Our analyses further motivate instruments to help identify the effect of men’s education on spousal “quality.” We find that the additional education received by returning veterans allowed them to “sort” into wives with comparably higher levels of education. This suggests an important mechanism by which socioeconomic status may be passed on to the next generation.
World War II and its subsequent G.I. Bill have been widely credited with playing a transformative role in American society. The end of the war created a surge of veterans on college campuses—veterans accounted for over 70% of male enrollment in the immediate post-war years—and research has shown that these increases were causally related to the availability of postwar educational benefits combined with military service. Bound and Turner (2002), for example, document that World War II and the G.I. Bill increased collegiate completion rates by close to 50%. The “legend” of the WWII G.I. Bill extends beyond its direct effects on education, however. For example, in his book *When Dreams Come True: The G.I. Bill and the Making of Modern America* (1996), Michael Bennett concludes that “Quite literally, the G.I. Bill changed the way we live, the way we house ourselves, the way we are educated, how we work and at what, and how we eat and transport ourselves.” Similarly, Drucker (1993) states that “Future historians may consider it the most important event of the 20th century…already it has changed the political, economic and moral landscape of the world.”

In spite of this rhetoric, there have been few quantitative analyses of the G.I. Bill’s broader social effects. This paper begins to fill this gap in the literature by using between-cohort variation in the probability of military service to investigate how these experiences affected the marital outcomes of returning veterans. In doing so, we also provide important insights into the mechanisms underlying assortative mating. A long literature documents that the education levels of husbands and wives are positively correlated, but the extent to which an additional year of education can change an individual’s marriage prospects is not well understood. This, no doubt, reflects the difficulty of finding plausible sources of identifying variation: education is likely correlated with a host of innate attributes that affect one’s attractiveness as a spouse, and addressing this omitted variables problem requires variation that moves education but is
exogenous to other individual characteristics—while having no direct influence on the potential spouse’s educational attainment.

Despite these challenges, knowing the answer to this question is important: a burgeoning literature suggests that the causal benefits associated with increased schooling extend well beyond its effect on wages. Previous researchers have documented that positive shocks to education are associated with reductions in criminal behavior, improved health, and higher levels of human capital among affected individuals’ offspring.\(^1\) This latter effect might be due to changes in parental behavior that result from additional schooling, or they might be due to changes in parents’ ability to secure a “better spouse.”\(^2\) Marital sorting has important implications for the intergenerational transmission of socio-economic status.

This paper has two objectives. First, we will investigate how the G.I. Bill and the experience of serving during WWII altered the structure of marriage. In doing so, we hope to shed light on how WWII affected an important dimension of American society. Second, we exploit between-cohort variation in the probability of military service and G.I. Bill benefit eligibility to motivate instruments that are used to identify the effect of men’s educational attainment on the probability of marrying and spousal “quality.” An advantage of this approach is that, relative to most existing studies that speak to the causal role of education in assortative mating, the genesis of our identifying variation is transparent. For example, unlike the within-family variation in schooling that is exploited in family fixed effects models, the variation induced by the World War II G.I. Bill is generated by a distinct policy that provided different educational opportunities to different individuals. Our identifying variation is generated by the “accident” of the timing of an individual’s date of birth, which leads to different opportunities for

---

1 e.g. Currie and Moretti, 2003; Lleras-Muney, 2005; Oreopolous, Page and Stevens, 2006; Lochner and Moretti, 2004; Maurin and McNally, 2008; Page, 2007.
2 See, for example, Mare and Maralani’s (2006) model of intergenerational mobility, in which the positive relationship between parental education and the education of one’s offspring is enhanced by the impact of education on marital sorting and mitigated by the impact of education on fertility.
men whom we would otherwise expect to be very similar. While our estimates will reflect the combined impact of military service and the G.I. Bill, they will be uncontaminated by differences in innate characteristics that are correlated with educational attainment, such as ability or motivation.

We find evidence that World War II and the G.I. Bill had important spillover effects beyond their effect on men’s educational attainment. The magnitude of these spillovers is comparable to previously documented effects of the G.I. Bill on men’s education. For example, our point estimates suggest that, relative to ineligible cohorts, cohorts of men who qualified for the G.I Bill had approximately 0.4 more years of education themselves, and also married women who had approximately 0.4 more years of education. One interpretation of these estimates is that each additional year of education received by returning veterans allowed them to “sort” into wives with comparably higher levels of education. Indeed, the implied instrumental variables estimates, though imprecise, are close to one. These findings add to the mounting evidence that the benefits of additional education extend well beyond education’s effect on earnings, and suggest an important mechanism through which socioeconomic status may be passed on to the next generation.

The remainder of the paper is organized as follows: Section I discusses World War II and the G.I. Bill, and describes the limited quantitative literature on how these events affected social outcomes. In Section II we review the literature on assortative mating. Sections III and IV outline our estimation strategy and data, respectively, and Section V discusses the results. Section VI describes the results from a series of robustness analyses. We provide concluding thoughts in Section VII.

I. World War II and the G.I. Bill

The G.I. Bill is widely regarded as one of the most significant education policies to have taken place in modern America. Signed into law on June 22, 1944, it provided unprecedented
educational aid to returning veterans who had served for at least 90 days or had been discharged early because of disabilities acquired during service. Anyone who had served on active duty between September 1940 and July 1947 was eligible for support, provided that he began his schooling before July 1951. The number of years of benefits for which a veteran qualified was determined according to the individual’s age and length of service, and ranged from one to four years. Most veterans were eligible for all four years of benefits.

The G.I. Bill made very generous financial provisions. It provided full tuition, books and supplies towards virtually any institution of higher education in the country, as well as a monthly stipend that varied by family size. Previous studies have estimated that for a single veteran this cash allowance was equal to about half the opportunity cost of not working, and for a married veteran it was equal to about 70% of the opportunity cost.  

The effect of this legislation on men’s schooling has been thoroughly investigated by Bound and Turner (2002) and by Stanley (2003). Bound and Turner estimate that G.I. benefits increased white men’s collegiate attainment by about 40%, using between-cohort differences in military service generated by wartime changes in manpower requirements to identify the likelihood that an individual was benefit eligible. Stanley’s estimates are based on comparisons of postsecondary education levels among cohorts of veterans who were less likely to avail themselves of the G.I. Bill because they had already completed their education to those who likely entered the military straight out of high school. This estimation strategy suggests that among veterans born between 1923 and 1926 the G.I. Bill increased postsecondary education levels by about 20%.

These empirical strategies are motivated by concerns about selection into military service. Comparisons of educational attainment between veterans and non-veterans are likely to

---

3 Bound and Turner (2002)
4 In a related study, Lemieux and Card (2001) estimate the effect of the Canadian G.I. Bill on education and earnings.
lead to overestimates of the legislation’s effect because one of the primary reasons for deferment from WWII service was physical or mental disability. Since individuals with low mental capacity probably had lower levels of education than average, veteran status alone is unlikely to identify the effects of the G.I. Bill.

Bound and Turner’s identification strategy gets around this problem by comparing outcomes for birth cohorts whose eligibility fell on either side of the sharp decline in manpower needs after 1945. Figure 1 documents the dramatic variation in WWII participation across cohorts. About 30% of men born in 1910 were enlisted and enlistment rates show a rapid increase among those born between 1914 and 1919. Military service was voluntary until 1940, when Congress passed the Selective Service Act, which mandated registration of young men and required enlistment among those who were deemed eligible. Thus, for cohorts born between 1920 and 1926, who would have been subject to the draft, the participation rate was nearly constant at a little over 80%. Among those who turned 18 after V-J day (cohorts born after the third quarter of 1927), service plummeted. Since the draft produces a sharp correlation between benefit eligibility and an individual’s birth date, but birth cohort is unlikely to be correlated with other innate characteristics, a comparison of education levels between pre-1927 and post-1927 cohorts provides clean estimates of the effect of military service and the G.I. Bill.

This paper exploits Bound and Turner’s identification strategy to investigate the G.I. Bill’s broader social impacts. While historians frequently credit the G.I. Bill with having created permanent changes in the structure of American society, most quantitative studies have been confined to analyses of its impact on earnings (Angrist and Krueger, 1994; Lemieux and Card, 2001) and education (Bound and Turner, 2002; Lemieux and Card, 2001; Stanley, 2003). There is reason to believe, however, that the G.I. Bill may have affected individuals’ outcomes beyond

---

5 Among 19-25 year old men deferred in 1945, for example, 56% were deemed physically or mentally unfit (Bound and Turner, 2002).
6 The figure is based on the three 1% samples in the 1970 Census. Appendix Figure 1 shows participation rates created using the 1960 and 1980 Censuses.
their labor market opportunities. In particular, evidence suggests that education may reduce crime (Lochner and Moretti, 2004), reduce mortality (Lleras-Muney, 2005), and improve some outcomes among individuals’ children (Currie and Moretti, 2003; Murnane, 1981; Oreopoulos, Page and Stevens, 2006; Thomas, Strauss and Henriques, 1991), so a natural question is whether the additional education induced by wartime events had spillover effects onto other outcomes. To our knowledge, only a few studies have empirically explored the relationship between World War II, the G.I. Bill, and non-labor market outcomes: Bedard and Deschenes (2006) find that cohorts with higher rates of WWII participation were more likely to die prematurely (excluding deaths attributed to combat) and that higher death rates among these cohorts are associated with higher rates of military-induced smoking. Yamashita (2008) and Fetter (2011) find evidence of a fading relationship between G.I. eligibility and homeownership, and Page (2007) shows that the children of affected cohorts had lower probabilities of repeating a grade. To our knowledge, no one has yet investigated the impact that these historic events may have had on marital opportunities and sorting in the United States.  

II. Assortative Mating Literature

An extensive literature documents the existence of positive assortative mating across a number of characteristics, but economists tend to focus on the positive correlation between husbands’ and wives’ education and other labor market characteristics. Across all age groups, the correlation estimate hovers between 0.52 and 0.62. Beginning with Becker (1973,1974), a variety of theories have been developed to explain why husbands and wives have similar levels of

---

7 Brainerd (2006) and Kvasnicka and Bethmann (2009) investigate how sex ratio changes resulting from WWII affected marriage and fertility rates in Russia (Brainerd) and Bavaria (Kvasnicka and Bethmann). We investigate the impact of World War II induced changes in the sex ratio in Section IV.C.

8 In addition to well-documented correlations in husbands’ and wives’ human capital and labor market opportunities, positive assortative mating has been documented for religion (Johnson,1980), ethnic background (Pagnini and Morgan, 1990), and physical characteristics (Epstein and Guttman, 1984).

9 Mare, 1991; Cancian, Danzig and Gottschalk, 1993; Jepsen and Jepsen, 2002; Juhn and Murphy, 1997, Pencavel, 1998. Schwartz (2010) finds evidence that the association between husbands’ and wives’ earnings has been increasing over time.

10 Authors’ own calculations from the 1960, 1970 and 1980 Censuses.
human capital, but in spite of these well developed theories, very little is known about the causal impact of an additional year of education on individuals’ marriage prospects. To what extent can individuals affect their marital outcomes by investing in their own schooling?

There are several mechanisms by which schooling might be able to increase the probability of marriage and spousal quality. Education is thought to affect individuals’ earnings, occupations and socioeconomic status. All of these outcomes might in turn affect the pool of available mates by changing both the social circles that individuals inhabit and their own attractiveness to potential partners. An individual’s education may also change his or her spouse’s behavior. For example, if education increases a man’s earnings, then this might enable his wife to invest more in her own human capital.

Unfortunately, identifying the causal relationship is challenging, because the positive correlation between husbands’ and wives’ education levels may reflect assortative mating on other characteristics that are correlated with education. For example, more intelligent people may both invest in more education themselves and prefer more intelligent spouses. A few studies have used family fixed effects models\(^{11}\) to control for individuals’ innate characteristics, but a drawback of this approach is that it is unclear why education varies between siblings. Factors that lead to differences in siblings’ educational levels may also affect their marriage opportunities,\(^{12}\) leading to omitted variables bias. Lefgren and McIntyre (2006) identify the marital sorting

\(^{11}\) Examples of fixed effects studies include Behrman, Rosenzweig and Taubman (1994) and Behrman and Rosenzweig (2002) who compare the educational attainment of the spouses of identical twins who have themselves obtained different levels of schooling, and find that, on average, an individual who receives one additional year of education marries a spouse with 0.3 additional years of education. As mentioned above, a drawback of these identification strategies is that the reasons for the differences in twins’ education are unknown.

\(^{12}\) It is also well known that relative to OLS, fixed effects estimates are more prone to errors-in-variables bias (Griliches, 1979). A less widely appreciated problem is that if the “within” variation in unobserved characteristics gives rise to “within” differences in education levels then fixed effects models may actually exacerbate omitted variables problems (Bound and Solon, 1999; Griliches, 1979).
effects of women’s education using quarter-of-birth as an instrument. Quarter of birth affects only a small fraction of all women’s schooling decisions, however, and previous researchers have raised concerns about its exogeneity. Bound, Jaeger and Baker (1995), for example, cite several studies which suggest that quarter of birth is correlated with individual characteristics such as schizophrenia and autism (Sham et. al., 1992; Gillberg, 1990) which might in turn have an independent effect on individuals’ marriage opportunities. Lefgren and McIntyre also show that quarter of birth is correlated with family income during childhood.

In a similar vein, a number of researchers (e.g. Lleras-Muney, 2005; Lochner and Moretti, 2004; Oreopoulos, Page and Stevens, 2006) have used variation in compulsory schooling laws across states and over time to identify the effect of schooling on a variety of outcomes. Compulsory schooling laws cannot be used to isolate the impact of education on marital sorting, however, because changes in these laws affect the educational attainment of both individuals and their potential spouses. Variation in college openings and distance to college, which have also been used as instruments for education (e.g. Currie and Moretti, 2003; Card, 1995), suffer from the same problem.

In contrast, World War II and the G.I. Bill conveniently changed the educational opportunities of approximately 80% of males born between 1920 and 1926 without changing their family background or innate characteristics, and without changing their potential wives’ educational opportunities. Only about 3% of women born during this period served in World War II. The direct effects of the G.I. Bill were thus concentrated almost exclusively on men. In the next section we describe how the relationship between male cohorts’ G.I. benefit eligibility and their education can be exploited empirically to shed light on the mechanisms that contribute to assortative mating. As we describe our identification strategy, it may be useful to keep in mind

---

13 Lefgren and McIntyre investigate the relationship between women’s education and husbands’ income, but do not look at the relationship between women’s education and husbands’ education.
that among the cohorts included in our analyses, only about 9% were married at the time they began their service.¹⁴

III. Estimation Strategy

To begin with, consider the following reduced form equations

\[
H_{Ed_{ic}} = \phi_1 H_{Cohort_{ic}} + \phi_2 (Post1927)_{ic} + \phi_3 X_{ic} + \mu_{ic} \tag{1}
\]

\[
Married_{ic} = \beta_1 H_{Cohort_{ic}} + \beta_2 (Post1927)_{ic} + \beta_3 X_{ic} + \mu_{ic} \tag{2}
\]

\[
W_{Ed_{ic}} = \varphi_1 H_{Cohort_{ic}} + \varphi_2 (Post1927)_{ic} + \varphi_3 X_{ic} + \mu_{ic} \tag{3}
\]

where \(H_{Ed}\) measures the educational attainment of man \(i\) belonging to cohort \(c\), \(Married\) is an indicator variable that is equal to 1 if individual \(i\) belonging to cohort \(c\) is married and is equal to zero otherwise, and \(W_{Ed}\) is the educational attainment of individual \(i\)'s wife. \(H_{Cohort}\) is a linear variable measuring the cohort (by birth year and birth quarter) to which the man belongs, and \(X\) is a vector of individual controls. We do not include measures of the individual’s income or work experience since these may be affected by educational attainment. \(Post1927\) is a dummy variable that is equal to 0 for cohorts born before 1928 and 1 for cohorts born in or after 1928.

As Figure 1 and Table 1 make clear, the vast majority of men born after 1927 did not serve in WWII and would not have been eligible for G.I. benefits provided to WWII veterans. By including a linear trend, and focusing on cohorts born within narrow windows, it is reasonable to assume that the coefficient \(\beta_2\) identifies the change in men’s educational attainment that resulted from the abrupt decline in conscription rates among men born after 1927. We can similarly estimate the effects of military service and the G.I. Bill on men’s marital opportunities by estimating \(\beta_2\) and \(\varphi_2\) in equations (2) and (3). The analysis will focus only on white men since

¹⁴Authors’ calculations based on Army enlistment records available through The National Archives Access to Archival Database (AAD), online at http://aad.archives.gov/aad/. Estimates are not expected to differ for other branches of the Armed Forces.
previous studies have shown that the effects of the G.I. Bill were quite different across racial groups.\textsuperscript{15}

This research design would be easy to implement, but the Korean War draft, which affected many men born after 1927, makes it hard to interpret. More than a third of the 1928 cohort in our sample served in Korea, and the fraction increases among later cohorts. Like those who served during WWII, Korean War veterans were also eligible for educational benefits, but unlike men subject to the WWII draft, men who wanted to avoid serving in Korea could obtain educational deferments. This means that estimates based on simple comparisons between cohorts who turned 18 on either side of VJ day are likely to be compromised by the effects of the Korean War. Instead of estimating equations (1)-(3), we therefore estimate the following augmented equations

\begin{equation}
HE_{i\tau} = \alpha + \phi_1 H_{\text{Cohort}} + \phi_2 \%\text{WWII}_{i\tau} + \phi_3 \%\text{Korea}_{i\tau} + \phi_4 \%\text{Korea}_{i\tau} \times H_{\text{Cohort}} + \phi_5 X_{i\tau} + \eta_{i\tau} \tag{1a}
\end{equation}

\begin{equation}
Married_{i\tau} = \alpha + \beta_1 H_{\text{Cohort}} + \beta_2 \%\text{WWII}_{i\tau} + \beta_3 \%\text{Korea}_{i\tau} + \beta_4 \%\text{Korea}_{i\tau} \times H_{\text{Cohort}} + \beta_5 X_{i\tau} + \nu_{i\tau} \tag{2a}
\end{equation}

\begin{equation}
WEd_{i\tau} = \alpha + \phi_1 H_{\text{Cohort}} + \phi_2 \%\text{WWII}_{i\tau} + \phi_3 \%\text{Korea}_{i\tau} + \phi_4 \%\text{Korea}_{i\tau} \times H_{\text{Cohort}} + \phi_5 X_{i\tau} + \epsilon_{i\tau} \tag{3a}
\end{equation}

where \%\text{Korea} is the fraction of men in the individual’s year and quarter-of-birth cell who identified themselves as Korean War veterans and the interaction term between \%\text{Korea} and the linear trend allows for the possibility that the Korean conflict may have had a differential effect on later cohorts. This seems likely, as Korean War educational deferments were not introduced until 1951.

These specifications also replace the Post27 dummy with \%\text{WWII}—the fraction of men in the individual’s birth cohort who served during WWII and were thus eligible for G.I. benefits.

\textsuperscript{15} Turner and Bound (2003) show that it had little effect on the collegiate outcomes of black veterans living in Southern states, probably because their educational choices were already so limited. As a result, the G.I. Bill may have exacerbated the education gap between Southern blacks and whites.
This allows us to make use of the substantial variation in participation rates across quarter-of-
birth cohorts who turned 18 right around VJ day. The coefficients $\beta_2$, $\gamma_2$ and $\theta_2$ identify the 
average differences in outcomes between cohorts who were eligible for benefits and cohorts who 
were not eligible. Since our identifying variation is at the cohort level we collapse our individual 
level data into year and quarter of birth cells, and estimate equations (1a)-(3a) at the cell level. 

Bound and Turner have previously documented that cohort-level variation in WWII 
participation is correlated with men’s schooling. The success of the identification strategy also 
 hinges on the assumption that innate family background characteristics are the same across 
cohorts. This concern is minimized by including a linear trend and focusing on men born within 
a narrow time interval.\(^\text{16}\) We have also used data from the Panel Study of Income Dynamics 
(PSID) and the 1973 Occupational Change in a Generation Survey (OCG) to look at the extent to 
which pre-service characteristics varied across these cohorts. In no case could we reject the null 
hypothesis that these characteristics were the same across cohorts, although this is partly due to 
the fact that the samples are small and yield imprecise estimates.\(^\text{17}\)

Under certain assumptions, we can use the estimates produced by equations (1a)-(3a) to 
recover an estimate of the effect of men’s schooling on the probability of marrying, and spousal 
quality. Specifically, the implied instrumental variables estimates of the return to a year of 
college will be $\frac{\delta_2}{2}$ and $\frac{\theta_2}{2}$. In addition to our assumption that innate family background

\(^{16}\) Replacing the linear trend with year-of-birth dummies and quarter-of-birth dummies yields very similar 
results.

\(^{17}\) Family background variables include: father’s education (PSID), and whether the individual lived with 
both parents at age 16, his father’s occupation at age 16, and his parents’ educational attainment (OCG). 
The OCG data also include retrospective reports on parents’ income when the individual was age 16. The 
parental income data are reported in bins. It is unclear whether respondents are reporting nominal or real 
dollars. This makes it difficult to interpret statistical analyses using this variable, since different cohorts 
turned 16 in different years. In a few specifications, we find that the fraction of individuals coming from 
high income families is larger among the younger cohorts in our sample, which would be consistent with 
estimates of G.I. Bill effects that are biased downward. Since the OCG data do not include quarter of birth, 
these analyses are based on, at most, 15 data points.
characteristics are similar across cohorts, however, this estimation strategy also relies on the assumption that military service did not have an independent effect on marital opportunities, beyond its effect on education. One piece of evidence in this regard is that WWII veterans appear to have earned no more than non-veterans (Angrist and Krueger, 1994; Lemieux and Card, 2001), but earnings are only one measure of success, and in principal one can imagine the bias going in either direction. The general public viewed returning veterans as heroes,\(^{18}\) which may have positively influenced their social interactions and made them more attractive marriage partners. At the same time, the stress resulting from combat may have left permanent scars on other veterans’ abilities to make social connections and provide for their families. These possibilities suggest that the instrumental variables estimates should be interpreted cautiously. Nevertheless, given the dearth of causal evidence on assortative mating mechanisms, the variation in men’s educational attainment induced by the WWII G.I. Bill may provide the most transparent identification strategy to date.\(^{19}\)

## IV. Data

Our analyses are based on the three 1\% samples of the 1970 Integrated Public Use Microdata Series (IPUMS), which includes both individual and household level data from the 1970 decennial census. Each of these files provides a 1/100 sample of individuals in the United States.

\(^{18}\) E.g. Mettler (2005), p. 10 “Importantly, their deservingness for the generous benefits was considered to be beyond question, given that through their military service they had put themselves in harm’s way for the sake of the nation.”

\(^{19}\) Seminar participants and interested readers have proposed two alternative identification strategies that we feel are less compelling than cohort level variation in benefit eligibility: one suggestion has been to follow the approach used by Stanley, who identifies the impact of G.I. benefits using variation in take-up rates across eligible cohorts. The drawback to this approach is that we do not have a solid understanding of why take-up rates varied. Whatever underlies the variation might also have affected marital sorting. The second suggestion is to use cross-state variation in mobilization rates, similar to Acemoglu, Autor and Lyle (2004). However, that study also documents correlations between state mobilization rates and other state characteristics, and those characteristics may be correlated with marital outcomes. In previous work, Page (2007) has found that estimates of the impact of G.I. benefits that used state level mobilization rates as an instrument for eligibility were sensitive to the inclusion of state level control variables.
States. By aggregating, we are able to create a 3% sample of all men living in the United States in 1970. We chose the 1970 Census over the 1960 Census because of its larger sample size and to allow sufficient time for the youngest cohorts to make their education and marital decisions.\textsuperscript{20}

We chose the 1970 Census over the 1980 Census because the 1980 Census shows notably higher levels of schooling among our cohorts, which likely results from factors unrelated to the G.I. Bill such as differential mortality, over-reporting of educational attainment that increases with age, and later enrollment in college (Bound and Turner, 2002). Results using the 1980 Census are qualitatively similar but are often (as expected) smaller in magnitude.

We focus on men who were born between 1923 and 1929-39. These cohorts are close in age and should thus have had similar life experiences prior to the war. In addition, the 1923-1927 cohorts faced similar probabilities of being drafted. We limit the sample to white men who were born in the United States, and we exclude all men for whom information on race, sex, age, or veteran status (men only) was allocated. We also exclude individuals who were not born in the United States.

The 1970 Census reports individuals’ completed years of schooling. We use this information to create a continuous measure of husbands’ years of college education (1-4 years) based on whether they completed 13, 14, 15 or 16+ years of school. We define a WWII veteran as anyone who served in World War II. A Korean War veteran is defined as anyone who served in the Korean War (the same definition used by Bound and Turner), or, in some specifications, indicated that they served in the military but not during WWII.\textsuperscript{21}

Table 1 shows descriptive statistics for all men, regardless of marital status, in our sample. Our analyses are based on between 136,666 and 442,917 individuals, but since our identifying variation is at the birth cohort level, the analyses aggregate our individual

\textsuperscript{20} The 1960 PUMS is a 1% sample.
\textsuperscript{21} The relevance of this alternative specification will be discussed in the next section.
observations into cells defined by year and quarter of birth. Consistent with previous studies, we find that rates of military service are around 80% among the oldest cohorts, and that participation quickly falls to nearly zero for cohorts born after 1928. In contrast, Korean War service is common among men born between 1928 and 1935. Across all cohorts, completed schooling shows an upward trend, but there is no evidence of a trend in marriage probabilities.

Because we focus on a narrow set of cohorts, we assume that cross-cohort variation in average pre-treatment characteristics is negligible. Our sample includes only those men who survived the war, however, so a potential issue is that cross-cohort variation in the probability of experiencing combat and risk of death may induce cross-cohort variation in unobserved characteristics. Suppose, for example, that more “able” veterans were less likely to be on the front lines. Then, since later cohorts of veterans were also less likely to engage in combat, the oldest cohorts in our sample would be positively selected. Our OCG and PSID analyses provide no evidence that family background characteristics vary across cohorts, but we investigate the possibility of cross-cohort variation in unobserved characteristics further by estimating the rate of return to education for each cohort. If older cohorts are more “able” than younger cohorts, then their rate of return should be higher. The results of this exercise are shown in Figure 2. While there is a clear downward trend in the estimated rate of return among cohorts born during the first half of the century, estimates for the cohorts born immediately before and after 1927 do not differ significantly from this trend. A related issue is that cross-cohort differences in the probability of combat are likely to have lead to differences in male/female sex ratios, which may have had an independent effect on marital sorting. Section VI explores this possibility more thoroughly.

V. Results

V.A. Effects of WWII and the G.I. Bill on Men’s Educational Attainment
We begin by exactly replicating Bound and Turner’s estimates of the relationship between WWII participation and educational attainment, and then extend their empirical framework to look at other outcomes. Table 2 provides between-birth-cohort estimates of the effect of World War II and Korean War service on men’s collegiate attainment. The estimates presented in the first six columns are differentiated by the number of post-treatment cohorts that are included in the sample. As discussed by Bound and Turner, the benefit of analyzing fewer cohorts is that the resulting estimates are unlikely to be biased by the presence of other cross-cohort differences, but the cost is that the identifying variation misses the youngest cohorts who are least likely to be eligible for G.I. benefits. Across the different samples, a 100% increase in the probability of serving is associated with an increase of between 0.3 and 0.4 years of education. The standard deviation in men’s education is approximately 3 years, so this represents a substantive difference in educational attainment.

Bound and Turner discuss the potentially contaminating effects of the Korean War, and note that as younger cohorts are added to the analysis these effects are less and less likely to be well captured by the %Korea variable. In order to address this concern, they add interactions between the percent of the cohort that participated in the Korean War and a linear trend. When cohorts born during the second half of the 1930s are included, they also add a quadratic trend and an interaction between the quadratic trend and the fraction of the cohort who served in Korea. This allows the effects of service in Korea to vary across birth cohorts in a non-linear way, which is a plausible assumption given that Korean War educational deferments were not introduced until 1951.

We replicate this part of their analysis in Columns 7-9 and show that when we include these controls the estimated coefficients on %WWII fall slightly. The estimate in column 7 is most affected because compared to columns 8 and 9, the analysis includes fewer post-treatment cohorts, which makes it harder to simultaneously identify the effects of the war from the linear
trend. The standard error estimate also increases. The estimate in column 8 is quite similar to that in column 6, but here the linear trend and its interaction with \%Korea may not sufficiently control for the part of the cross-cohort variation in educational attainment that is generated by Korea. Because column 9 includes a more complete set of Korean War controls, we believe (like Bound and Turner) that these estimates, along with the estimates presented in the first few columns of Table 2, represent the cleanest estimates of the combined impact of WWII service and the G.I. Bill on men’s schooling. Column 10 is based on the same specification but boosts precision by adding two more years of data.

The estimates in the bottom panel of Table 2 are based on the same identification strategies but control for Korean War service a little differently. Figure 1 suggests that among the youngest cohorts in our sample, there are many men who served in the military but do not identify themselves as veterans of either WWII, or the Korean or Vietnam wars. Men born in 1935, for example, are nearly equally likely to identify themselves as Korean War veterans or as having engaged in “other” military service (not WWII or Vietnam). It is likely that many of these men did not classify themselves as Korean War veterans because their primary period of service was after January of 1955. Nevertheless, many of these men would have still qualified for educational benefits under the Korean War G.I. Bill since anyone who entered the military prior to Feb 1, 1955 and served for ninety days was eligible. When we more broadly control for the effects of the Korean War by including men who identify themselves as serving either in Korea or at “any other time”\(^22\) we find that the estimated effects of both WWII and “Korean War” service increase substantially (columns 9 and 10).\(^23\) Since the standard error estimates that accompany the estimated effects of the Korean War also shrink substantially, we carry forward this definition

\(^{22}\) i.e. Not during the specific war periods listed in the Census survey.
\(^{23}\) As would be expected from Figure 1, the estimates in columns 1-4 barely change.
of “probable” Korean War service throughout the rest of the paper. Our findings are not affected by this decision in any substantial way.\textsuperscript{24}

\textit{V.B. The Relationship Between the G.I. Bill and Assortative Mating}

Given the clear association between WWII, the G.I. Bill and men’s education, it is natural to consider whether these historical events had spillover effects onto other dimensions of family life. We begin to explore this possibility in Table 3, where we show estimated reduced form effects on marital status and wives’ educational attainment using our preferred specifications.\textsuperscript{25} We find no evidence that the G.I. Bill had any effect on the probability of being married, separated, or divorced, but there is evidence that among married men, it improved their ability to attract higher “quality” spouses. Cohorts with high WWII participation rates married women with more years of schooling, higher probabilities of having graduated from high school, and higher probabilities of having enrolled in college. The lack of a relationship between wives’ bachelor’s degree status and husbands’ WWII participation may be due to the fact that only small numbers of women graduated from college during this period.\textsuperscript{26}

These reduced form estimates suggest that WWII and the G.I. Bill had important spillover effects beyond their effect on men’s educational attainment. The magnitude of these spillovers, while not precisely estimated, are comparable, and sometimes even greater than, the direct estimate of the G.I. Bill on men’s education. For example, the estimated coefficients in column 4 indicate that the G.I. Bill increased men’s education by about 0.4 years, and that, relative to ineligible men, those who qualified for the G.I. Bill were simultaneously able to marry women who themselves had approximately 0.4 additional years of education. One interpretation

\textsuperscript{24} Results available from the authors upon request.
\textsuperscript{25} Results based on other specifications are available on request.
\textsuperscript{26} Our calculations from the census indicate that fewer than 9\% of white women born between 1923 and 1930 had bachelor’s degrees.
of these results is that each additional year of education received by returning veterans also allowed them to “sort” into wives with comparably higher levels of education.

If we accept that WWII and the G.I. Bill had a substantive effect on men’s college attainment, but no effect on their probability of marrying, then we might be able to use cohort-level G.I. benefit eligibility to explore the causal relationship between education and spousal “quality.” Using the estimates presented in columns 3 through 5, the implied IV estimate of the impact of men’s education on wives’ educational attainment is close to one.

To put these estimates in context, Table 4 presents OLS estimates of the relationship between men’s education, the probability of being married, and wives’ education. Like other studies, the OLS estimates document a strong correlation between husbands’ and wives’ education: men with an additional year of college marry women with approximately 1 more year of schooling, although there is no evidence of a significant correlation between a man’s level of education and his probability of finding a marital partner. What is striking is how similar the OLS and implied IV estimates are: this suggests that the correlation in spouse’s education that we observe in a cross-section is not driven merely by sorting on other innate characteristics that happen to be correlated with schooling. An important implication is that marital sorting may be manipulated by policies.

Table 5 shows the two-stage least squares estimates, using as an instrument the fraction of the cohort that served during WWII. This exercise produces the same estimates as the implied IV estimates in Table 3, with accompanying standard error estimates. We see again that men who received an additional year of collegiate education because of the benefits associated with WWII married women with approximately the same amount (or more) of additional schooling.

---

27 The OLS estimates are based on regressions that include all of the variables in equations (2a) and (3a) except the %WWII variable.
Importantly, since women during this time period were less likely to graduate from college, most of the sorting is on the high school or “some college” margin.

VI. Robustness Checks and Further Interpretation

In this section we explore our main results further, and provide evidence that our results are not driven by other aspects of WWII or the G.I. Bill. We consider the possible impacts of military service itself, differing sex ratios across “treatment” and “control” cohorts, and veterans’ housing benefits. We also conduct more general falsification tests that are motivated by the fact that, among women, G.I. benefit eligibility and take up was low.

VI.A. Distinguishing between the Effects of Military Service and Education Benefits

As described earlier, the estimates in Tables 2 and 3 represent the combined effect of military service and G.I. benefits. War service may have had either positive or negative impacts on marital outcomes. In order to glean some insight into how the impact of military service contributes to our estimates, we look at variation in education and spousal quality among cohorts of men who came of age around the time of the First World War. Although these men received a generous monetary bonus for their service, educational benefits were not available to World War I veterans. Comparing the education and marital outcomes of cohorts near the World War I “break” may, therefore, provide some information about the likely influence of military service relative to educational benefits. In particular, differences between cohorts who served during WWI and those who narrowly missed the cutoff can be roughly thought of as an upper bound estimate of the impact of service.

We explore this phenomenon using data from the 1930 and 1940 Censuses. Information on WWI service comes from the 1930 Census, and information on educational attainment is taken from the 1940 Census. These Census files do not record year and quarter of birth; rather, age is reported in years. Thus, we assume that each survey respondent’s birthday falls after the census
was taken in April, and use this to estimate his year of birth. Following Fetter (2011), we look at men born between 1891 and 1902, and look for a change in outcomes across a participation cutoff for cohorts born between 1896 and 1897. Table 6 shows the estimated coefficient on a variable that controls for the fraction of each cohort that participated in WWI, for a series of regressions with different dependent variables (men’s educational attainment, marital status and wives’ educational attainment). Each regression equation also includes a linear trend. There is no evidence that World War I participation affected any of these outcomes, which suggests that the direct effects of service during World War I were negligible. This strengthens our hope that the estimates in Tables 3 and 5 are driven by cross-cohort variation in education benefits.

VI.B. The GI Bill and Homeownership

In addition to educational benefits, the WWII G.I. Bill also guaranteed generous home and business loans which made it possible for approved lenders to provide no-down payment mortgages to returning veterans. Between 1944 and 1952, the Veterans Administration guaranteed nearly 2.4 million home loans. Recent work by Yamashita (2008) and Fetter (2011) suggests that these benefits had a significant impact on white veterans’ rates of homeownership during the post-war period, although the advantage disappeared by 1980. This suggests that our assortative mating results might be driven by veterans’ access to housing rather than their higher education levels. In order to investigate this possibility we create a measure of cohort-level homeownership rates from the Census and include this variable as an additional control variable.29

28 We obtain the same qualitative result when we replace the %WWI variable with a dummy variable indicating that the cohort was born after 1896.
29 Specifically, we create a dummy variable that is equal to 1 if the individual reports that his living quarters are owned or bought by himself or someone in his household, and 0 if the individual reports that his living quarters are rented or occupied without payment of cash rent. We then use this variable to calculate the fraction of each cohort who owned their own home. We have also used the 1960 Census to create a comparable variable.
The results of this exercise are shown in Table 7. Consistent with previous studies’ evidence of fade-out effects, we find no evidence that G.I. benefit eligible cohorts were more likely to own a home in 1970 than their ineligible counterparts. However, in some specifications, owning a home is positively correlated with the probability of being married, and it is always positively associated with wife’s years of education. This suggests that the improvements in veterans’ access to housing may have affected their ability to attract higher quality wives. However, inclusion of the housing variable has virtually no impact on our reduced form estimates of the impact of WWII service on wives’ schooling. We have also estimated our regressions including homeownership rates calculated from the 1960 Census since this is the Census year for which both Yamishita and Fetter find evidence of homeownership differences across cohorts. Including the 1960 control variable has no substantive impact on the estimated WWII coefficients either. Taken together, these results suggest that the findings presented in Tables 3-5 are not driven by the homeownership benefits that were associated with the GI Bill.

VI.C. Cross-cohort Differences in Sex Ratios

High rates of military service among our treatment cohorts also lead to lower male/female ratios. About 16 million men served in World War II, and of these, approximately 405,000 died. Figure 3 plots the sex ratio by year and quarter of birth, and shows a substantive

---

30 We obtain similar results when we use the other measures of wives’ educational attainment that are included in Tables 3-5. For the sake of brevity, we do not include all of those measures in Table 7.
31 Results are virtually identical if we restrict our definition of home ownership to include only heads of households.
32 Unlike Yamishita and Fetter, we do not find evidence that G.I. benefit eligible cohorts were more likely to own a home in 1960 than their ineligible counterparts. The discrepancy appears to emanate from differences in the way the Korean War is incorporated into the different analyses. Yamishita does not control for the effects of the Korean War at all. Fetter’s analysis assumes that the impact of participating in WWII and participating in Korea would be the same for a given cohort. Our specification provides more flexibility on this front.
33 In contrast, Korean War participation rates were much lower (especially for our cohorts) and resulted in only 36,500 deaths.
difference in the ratio between the pre and post 1927 cohorts. The figure is based on the 1960 Census because differences in the sex ratio are much smaller by 1970. It is also closer to the time period during which we expect most of these cohorts married. Since men often marry women whose age is within a few years of their own age, our measure of the sex ratio divides the number of men in each quarter and year of birth by the average number of women in quarter and year of birth cohorts falling within two years of the male cohort. We have tried several alternative measures and obtain very similar results.

Figure 3 shows that relative to cohorts born after 1927, cohorts born in the pre-1927 period experienced a male/female ratio that was 2.5 percent lower. Becker (1981) suggests that this difference could have strong implications for assortative mating: in particular, a decrease in the number of men implies that men should be able to mate with higher quality women than would otherwise be possible. Several recent studies have found that changes in sex ratios impact marital and fertility outcomes. In order to test whether this phenomenon is driving our estimates we include the sex ratio as an additional control variable in our main regression. The results of this exercise are presented in Table 8, where we see that including this variable has essentially no impact on the estimated relationship between WWII service and the probability of marriage or wife’s education.

VI.D. Effects of WWII and the GI Bill on Women

Table 9 shows reduced form estimates of the effect of WWII service on women’s education and their husbands’ educational attainment. Only about 3% of women born between 1923 and 1938 served during WWII, service was voluntary across cohorts, and take-up rates among G.I. benefit-eligible women were lower than among men (Mettler, 2005). Sharp differences in educational attainment between women born before and after 1927 might thus lead

---

34 Figure available by request.
to concerns that our estimates are picking up the effects of differences in something other than military service and G.I. benefit eligibility. We use two approaches to investigate this possibility: first, we match our measure of male WWII participation by year and quarter of birth to cohorts of females that are defined by the same year and quarter of birth. We then re-estimate equations 1-3 and the analogous 2SLS equations. Cross-cohort variation in male participation rates should not have an independent effect on women, and as expected, we find no evidence that discontinuous declines in male military service are associated with discontinuous cross-cohort changes in female schooling levels or schooling of husbands. Similarly, we find no evidence that male participation in WWII “works” as an instrument for female education.

We would also like to explore the impact of female military service on women’s schooling and marital sorting. As noted above, because rates of WWII participation and G.I. benefit take-up among women were low, we would expect such an analysis to produce negligible estimates. Unfortunately, the 1970 Census does not contain information on female military service, so we instead create a measure of female participation using data from the 1980 Census and merge this variable onto our dataset from 1970.\(^{36}\) The results of this exercise are shown in the bottom panel of Table 9. Again, we find no evidence of variation in women’s (or their husbands’) educational attainment across cohorts with differential access to G.I. benefits.

\textit{VI.E. Marital Sorting vs. Alternative Mechanisms?}

The evidence presented thus far supports the hypothesis that the G.I. Bill affected both husbands’ and wives’ educational attainment. One interpretation of our results is that the change in men’s schooling levels that resulted from their access to educational benefits allowed them to gain access to a “higher quality” pool of potential mates. An alternative

\(^{36}\) As we noted in Section IV, there are a number of reasons that we prefer to base our analyses on the 1970 Census, rather than on the 1980 Census. However, WWII participation rates calculated from the 1980 Census are likely to be a reasonable proxy for WWII participation rates among the same cohorts in 1970. When we calculate participation rates for men using the 1970 and 1980 Census they are very similar.
interpretation is that WWII veterans married the same women that they would have married in the absence of the war, but that because of their higher education levels, their wives were subsequently able to increase their own schooling.

Given that only 9% of men were married at the time they entered the service, we think that the latter mechanism is unlikely to be driving our estimates. We cannot definitively rule this possibility out, but Figures 4A and 4B provide some evidence of changes in marital sorting by age that are consistent with the differences that we see in wives’ educational attainment. Figure 4A plots the average husband-wife age gap for male cohorts born between 1910 and 1940. The figure shows a distinct increase in the magnitude of the gap right around the 1927 cutoff, suggesting that men who just missed eligibility for WWII benefits married women who were discontinuously younger than their eligible counterparts. If the G.I. Bill did not induce a change in marital sorting, then we would expect the average age gap to remain more or less constant. Figure 4B, which plots the standard deviation of the age gap, also provides evidence of a shift in marital sorting right around the cutoff. Here, we see an abrupt increase in the standard deviation of the age gap for cohorts born between 1924 and 1930. Taken together, these figures suggest that cohorts who were able to take advantage of G.I. benefits may have “poached” more educated women from adjacent cohorts who would have otherwise married them. This suggests that our results are more likely to be driven by changes in marital sorting than by changes in wives’ educational investments.

VII. Conclusion

Previous studies have shown that the World War II G.I. Bill had substantial effects on men’s educational attainment. This paper investigates whether these historical events had spillover effects into marital sorting, and exploits the relationship between the G.I. Bill and education to provide a potentially causal estimate of the relationship between male schooling and
spousal quality. We find that the G.I. Bill, combined with military service, changed the sorting of husband/wife pairs. In addition to achieving higher levels of education themselves, cohorts of men who served in WWII married women whose schooling was also higher by approximately the same amount. Specifically, cohorts who were eligible for G.I. benefits obtained about 0.4 additional years of education and also married women with approximately 0.4 additional years of education. We know that this result is not driven by an independent effect of the G.I. Bill on female schooling for two reasons: first, only about 3% of age-eligible women participated in WWII. Second, our identifying variation is based on differences in eligibility across cohorts of males who did not typically marry women belonging to the same cohort.

Although the instrumental variables estimates of the impact of husbands’ education on wives’ schooling are imprecise, their magnitudes suggest that investments in human capital yield substantial marriage market returns. The point estimates are approximately equal to one, and statistically different from zero, enabling one to rule out the possibility that assortative mating on unobservable innate characteristics is driving the observed correlation between spouses’ education. Our results strongly imply that at least some of the assortative mating we observe in society can be causally attributed to environmental influences, and may be altered by individuals’ investment decisions.
References


Thomas, Duncan, John Strauss, and Maria-Helena Henriques, “How Does Mother’s Education Affect Child Height?” *Journal of Human Resources*, 1991, 26(2), 183-211.


Figure 1: Rates of Military Service by Birth Cohort

Source: 1970 Integrated Public Use Microdata Series (IPUMS, three 1% samples)
Note: Calculations are based on white men born in the contiguous United States.
Figure 2: Estimated Returns to Education

Source: 1970 Integrated Public Use Microdata Series (IPUMS, three 1% samples)
Note: Calculations are based on white men born in the contiguous United States. Estimates are derived from a regression of the log wage on educational attainment, birth year-quarter fixed effects and the interaction between birth year and educational attainment.
Figure 3: Sex Ratio (Male/Female) by Birth Cohort (1960 Census)

Source: 1970 Integrated Public Use Microdata Series (IPUMS, three 1% samples)
Note: Estimates are based on white men and white women born in the contiguous United States. We define the sex ratio as the number of men born in a year-quarter divided by the average number of women born in the previous and subsequent eight quarters.
Figure 4A: Average Age Gap between Husband and Wife by Husband's Birth Cohort

Source: 1970 Integrated Public Use Microdata Series (IPUMS, three 1% samples).
Note: Estimates are based on married white men born in the contiguous United States. The Age Gap is calculated by averaging the difference between husbands' and wives' ages within each year and quarter of birth cell. We detrend this variable and plot the residual.

Figure 4B: Standard Deviation of Age Gap between Husband and Wife by Husband's Birth Cohort

Source: 1970 Integrated Public Use Microdata Series (IPUMS, three 1% samples).
Note: Estimates are based on married white men born in the contiguous United States. The variable Age Gap is calculated by taking the differences between husbands' and wives' ages within each year and quarter of birth cell. We take the standard deviation of this variable, detrend it and plot the residual.
Table 1: Summary Statistics

<table>
<thead>
<tr>
<th>Birth Year</th>
<th>Observations</th>
<th>% WWII</th>
<th>% Korea</th>
<th>% Korea and Interwar</th>
<th>Highest Grade Completed</th>
<th>% Completed College</th>
<th>Years of College</th>
<th>% Married</th>
<th>Highest Grade Completed, Wife</th>
</tr>
</thead>
<tbody>
<tr>
<td>1920</td>
<td>25,190</td>
<td>79%</td>
<td>0%</td>
<td>1%</td>
<td>11.4</td>
<td>14%</td>
<td>0.77</td>
<td>88%</td>
<td>11.4</td>
</tr>
<tr>
<td>1921</td>
<td>26,618</td>
<td>82%</td>
<td>1%</td>
<td>1%</td>
<td>11.5</td>
<td>15%</td>
<td>0.80</td>
<td>88%</td>
<td>11.5</td>
</tr>
<tr>
<td>1922</td>
<td>25,681</td>
<td>81%</td>
<td>1%</td>
<td>1%</td>
<td>11.5</td>
<td>15%</td>
<td>0.82</td>
<td>88%</td>
<td>11.5</td>
</tr>
<tr>
<td>1923</td>
<td>25,701</td>
<td>81%</td>
<td>1%</td>
<td>2%</td>
<td>11.6</td>
<td>16%</td>
<td>0.85</td>
<td>88%</td>
<td>11.6</td>
</tr>
<tr>
<td>1924</td>
<td>26,937</td>
<td>80%</td>
<td>1%</td>
<td>2%</td>
<td>11.6</td>
<td>17%</td>
<td>0.89</td>
<td>88%</td>
<td>11.6</td>
</tr>
<tr>
<td>1925</td>
<td>25,837</td>
<td>79%</td>
<td>3%</td>
<td>4%</td>
<td>11.7</td>
<td>18%</td>
<td>0.90</td>
<td>88%</td>
<td>11.6</td>
</tr>
<tr>
<td>1926</td>
<td>25,510</td>
<td>77%</td>
<td>5%</td>
<td>6%</td>
<td>11.7</td>
<td>18%</td>
<td>0.95</td>
<td>88%</td>
<td>11.6</td>
</tr>
<tr>
<td>1927</td>
<td>26,465</td>
<td>68%</td>
<td>10%</td>
<td>12%</td>
<td>11.7</td>
<td>18%</td>
<td>0.95</td>
<td>88%</td>
<td>11.7</td>
</tr>
<tr>
<td>1928</td>
<td>25,488</td>
<td>32%</td>
<td>32%</td>
<td>37%</td>
<td>11.7</td>
<td>18%</td>
<td>0.94</td>
<td>87%</td>
<td>11.7</td>
</tr>
<tr>
<td>1929</td>
<td>24,893</td>
<td>12%</td>
<td>50%</td>
<td>56%</td>
<td>11.9</td>
<td>19%</td>
<td>0.97</td>
<td>88%</td>
<td>11.8</td>
</tr>
<tr>
<td>1930</td>
<td>24,845</td>
<td>4%</td>
<td>59%</td>
<td>66%</td>
<td>12.0</td>
<td>19%</td>
<td>0.99</td>
<td>88%</td>
<td>11.8</td>
</tr>
<tr>
<td>1931</td>
<td>23,798</td>
<td>2%</td>
<td>64%</td>
<td>70%</td>
<td>12.1</td>
<td>21%</td>
<td>1.05</td>
<td>88%</td>
<td>11.9</td>
</tr>
<tr>
<td>1932</td>
<td>24,209</td>
<td>1%</td>
<td>63%</td>
<td>70%</td>
<td>12.2</td>
<td>21%</td>
<td>1.08</td>
<td>88%</td>
<td>11.9</td>
</tr>
<tr>
<td>1933</td>
<td>22,657</td>
<td>0%</td>
<td>55%</td>
<td>68%</td>
<td>12.2</td>
<td>21%</td>
<td>1.08</td>
<td>88%</td>
<td>11.9</td>
</tr>
<tr>
<td>1934</td>
<td>23,690</td>
<td>0%</td>
<td>34%</td>
<td>60%</td>
<td>12.2</td>
<td>20%</td>
<td>1.05</td>
<td>87%</td>
<td>11.9</td>
</tr>
<tr>
<td>1935</td>
<td>23,371</td>
<td>0%</td>
<td>27%</td>
<td>57%</td>
<td>12.2</td>
<td>20%</td>
<td>1.03</td>
<td>88%</td>
<td>12.0</td>
</tr>
<tr>
<td>1936</td>
<td>22,816</td>
<td>0%</td>
<td>19%</td>
<td>55%</td>
<td>12.3</td>
<td>20%</td>
<td>1.04</td>
<td>87%</td>
<td>12.0</td>
</tr>
<tr>
<td>1937</td>
<td>23,297</td>
<td>0%</td>
<td>11%</td>
<td>54%</td>
<td>12.3</td>
<td>19%</td>
<td>1.02</td>
<td>87%</td>
<td>12.0</td>
</tr>
<tr>
<td>1938</td>
<td>24,115</td>
<td>0%</td>
<td>5%</td>
<td>53%</td>
<td>12.4</td>
<td>20%</td>
<td>1.06</td>
<td>86%</td>
<td>12.1</td>
</tr>
<tr>
<td>1939</td>
<td>23,607</td>
<td>0%</td>
<td>1%</td>
<td>49%</td>
<td>12.4</td>
<td>20%</td>
<td>1.06</td>
<td>85%</td>
<td>12.1</td>
</tr>
<tr>
<td>1940</td>
<td>24,637</td>
<td>0%</td>
<td>0%</td>
<td>45%</td>
<td>12.5</td>
<td>21%</td>
<td>1.12</td>
<td>83%</td>
<td>12.2</td>
</tr>
</tbody>
</table>

Source: 1970 Integrated Public Use Microdata Series (IPUMS, three 1% samples)

Note: The sample is composed of white men. The variable %WWII is the fraction of all men in a given birth cohort who were veterans of World War II, regardless of their military service status in other periods. The variables %Korea and %Korea and Interwar are the fraction of men who identified themselves as having participated in those conflicts but did not also serve in World War II. Wives' Highest Grade Completed is based on the wives of the married men in our sample.
### Table 2: Estimated Effects of WWII and Korean War Service on Men's College Attainment

*Replication of Bound and Turner (2002)*

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>All Males</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Years of Completed College:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>World War II Service</td>
<td>0.36</td>
<td>0.30</td>
<td>0.34</td>
<td>0.40</td>
<td>0.42</td>
<td>0.39</td>
<td>0.23</td>
<td>0.35</td>
<td>0.28</td>
<td>0.24</td>
</tr>
<tr>
<td>(0.25)</td>
<td>(0.12)</td>
<td>(0.10)</td>
<td>(0.08)</td>
<td>(0.07)</td>
<td>(0.05)</td>
<td>(0.14)</td>
<td>(0.06)</td>
<td>(0.12)</td>
<td>(0.11)</td>
<td></td>
</tr>
<tr>
<td>Korean War Service</td>
<td>0.37</td>
<td>0.28</td>
<td>0.34</td>
<td>0.42</td>
<td>0.43</td>
<td>0.39</td>
<td>0.15</td>
<td>0.34</td>
<td>0.22</td>
<td>0.15</td>
</tr>
<tr>
<td>(0.37)</td>
<td>(0.16)</td>
<td>(0.13)</td>
<td>(0.09)</td>
<td>(0.09)</td>
<td>(0.04)</td>
<td>(0.19)</td>
<td>(0.05)</td>
<td>(0.19)</td>
<td>(0.16)</td>
<td></td>
</tr>
<tr>
<td><strong>Married Males</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Years of Completed College:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>World War II Service</td>
<td>0.21</td>
<td>0.17</td>
<td>0.20</td>
<td>0.34</td>
<td>0.37</td>
<td>0.36</td>
<td>0.15</td>
<td>0.32</td>
<td>0.30</td>
<td>0.23</td>
</tr>
<tr>
<td>(0.24)</td>
<td>(0.12)</td>
<td>(0.10)</td>
<td>(0.08)</td>
<td>(0.07)</td>
<td>(0.05)</td>
<td>(0.15)</td>
<td>(0.06)</td>
<td>(0.13)</td>
<td>(0.12)</td>
<td></td>
</tr>
<tr>
<td>Korean War Service</td>
<td>0.18</td>
<td>0.13</td>
<td>0.16</td>
<td>0.35</td>
<td>0.37</td>
<td>0.37</td>
<td>0.07</td>
<td>0.32</td>
<td>0.27</td>
<td>0.16</td>
</tr>
<tr>
<td>(0.35)</td>
<td>(0.14)</td>
<td>(0.11)</td>
<td>(0.09)</td>
<td>(0.09)</td>
<td>(0.04)</td>
<td>(0.20)</td>
<td>(0.05)</td>
<td>(0.20)</td>
<td>(0.18)</td>
<td></td>
</tr>
</tbody>
</table>

**Controlling for both the Korean War, and Interwar Period Service**

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>All Males</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Years of Completed College:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>World War II Service</td>
<td>0.51</td>
<td>0.33</td>
<td>0.38</td>
<td>0.46</td>
<td>0.50</td>
<td>0.70</td>
<td>0.22</td>
<td>0.70</td>
<td>0.44</td>
<td>0.67</td>
</tr>
<tr>
<td>(0.35)</td>
<td>(0.15)</td>
<td>(0.13)</td>
<td>(0.10)</td>
<td>(0.09)</td>
<td>(0.09)</td>
<td>(0.18)</td>
<td>(0.09)</td>
<td>(0.13)</td>
<td>(0.12)</td>
<td></td>
</tr>
<tr>
<td>Korean &amp; Interwar Period Service</td>
<td>0.54</td>
<td>0.31</td>
<td>0.37</td>
<td>0.47</td>
<td>0.50</td>
<td>0.83</td>
<td>0.13</td>
<td>0.83</td>
<td>0.52</td>
<td>0.70</td>
</tr>
<tr>
<td>(0.49)</td>
<td>(0.18)</td>
<td>(0.17)</td>
<td>(0.12)</td>
<td>(0.11)</td>
<td>(0.09)</td>
<td>(0.23)</td>
<td>(0.09)</td>
<td>(0.13)</td>
<td>(0.12)</td>
<td></td>
</tr>
<tr>
<td><strong>Married Males</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Years of Completed College:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>World War II Service</td>
<td>0.29</td>
<td>0.18</td>
<td>0.20</td>
<td>0.38</td>
<td>0.42</td>
<td>0.63</td>
<td>0.10</td>
<td>0.64</td>
<td>0.37</td>
<td>0.59</td>
</tr>
<tr>
<td>(0.30)</td>
<td>(0.12)</td>
<td>(0.12)</td>
<td>(0.10)</td>
<td>(0.09)</td>
<td>(0.09)</td>
<td>(0.17)</td>
<td>(0.09)</td>
<td>(0.13)</td>
<td>(0.12)</td>
<td></td>
</tr>
<tr>
<td>Korean &amp; Interwar Period Service</td>
<td>0.27</td>
<td>0.12</td>
<td>0.15</td>
<td>0.37</td>
<td>0.41</td>
<td>0.76</td>
<td>-0.01</td>
<td>0.77</td>
<td>0.45</td>
<td>0.61</td>
</tr>
<tr>
<td>(0.41)</td>
<td>(0.14)</td>
<td>(0.14)</td>
<td>(0.12)</td>
<td>(0.11)</td>
<td>(0.09)</td>
<td>(0.23)</td>
<td>(0.09)</td>
<td>(0.13)</td>
<td>(0.12)</td>
<td></td>
</tr>
</tbody>
</table>

**Controls:**
- Linear Trend
- Korean & Interwar Service x Trend
- Trend
- Korean & Interwar Service x Trend²

Source: 1970 Integrated Public Use Microdata Series (IPUMS, three 1% samples)

Note: The sample is composed of white men. The variable *World War II Service* is the fraction of all men in a given birth cohort who were veterans of World War II, regardless of their military service status in other periods. The variables *Korean War Service* and *Korean & Interwar Period Service* are the fraction of men who identified themselves as having participated in those conflicts but did not also serve in World War II. The time trend is defined as year of birth - 1929 + (quarter of birth/4). Standard error estimates are corrected for heteroskedasticity and shown in parentheses.
Table 3: Reduced Form Estimates of the Effect of WWII Service on Men's College Attainment, Marital Status and Wife's Educational Attainment

<table>
<thead>
<tr>
<th>Birth Cohorts</th>
<th>1923-29</th>
<th>1923-30</th>
<th>1923-32</th>
<th>1923-38</th>
<th>1922-39</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) All Men:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Years of Completed College</td>
<td>0.33</td>
<td>0.38</td>
<td>0.50</td>
<td>0.44</td>
<td>0.67</td>
</tr>
<tr>
<td></td>
<td>(0.15)</td>
<td>(0.13)</td>
<td>(0.09)</td>
<td>(0.13)</td>
<td>(0.12)</td>
</tr>
<tr>
<td>Probability Married</td>
<td>0.03</td>
<td>0.00</td>
<td>0.04</td>
<td>0.00</td>
<td>-0.01</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.04)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Probability Separated or Divorced</td>
<td>-0.02</td>
<td>0.00</td>
<td>-0.02</td>
<td>-0.01</td>
<td>-0.01</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.02)</td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>F-Statistic</td>
<td>5.3</td>
<td>8.2</td>
<td>29.4</td>
<td>11.7</td>
<td>32.0</td>
</tr>
<tr>
<td>Married Men:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Husband's Years of Completed College</td>
<td>0.18</td>
<td>0.20</td>
<td>0.42</td>
<td>0.37</td>
<td>0.59</td>
</tr>
<tr>
<td></td>
<td>(0.12)</td>
<td>(0.12)</td>
<td>(0.09)</td>
<td>(0.13)</td>
<td>(0.12)</td>
</tr>
<tr>
<td>Wife's Years of Schooling</td>
<td>0.70</td>
<td>0.61</td>
<td>0.52</td>
<td>0.44</td>
<td>0.51</td>
</tr>
<tr>
<td></td>
<td>(0.23)</td>
<td>(0.24)</td>
<td>(0.17)</td>
<td>(0.17)</td>
<td>(0.14)</td>
</tr>
<tr>
<td>Wife High School Graduate</td>
<td>0.15</td>
<td>0.15</td>
<td>0.12</td>
<td>0.09</td>
<td>0.08</td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td>(0.06)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Wife Enrolled in College</td>
<td>0.05</td>
<td>0.05</td>
<td>0.06</td>
<td>0.06</td>
<td>0.08</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Wife College Graduate</td>
<td>0.01</td>
<td>0.00</td>
<td>0.00</td>
<td>0.03</td>
<td>0.04</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.04)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>F-Statistic</td>
<td>2.1</td>
<td>2.9</td>
<td>21.0</td>
<td>8.0</td>
<td>23.8</td>
</tr>
<tr>
<td>Controls:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Linear Trend</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Korean &amp; Interwar Period Service x Trend</td>
<td>x</td>
<td>x</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Trend²</td>
<td>x</td>
<td>x</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Korean &amp; Interwar Period Service x Trend²</td>
<td>x</td>
<td>x</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Source: 1970 Integrated Public Use Microdata Series (IPUMS, three 1% samples)

Note: The sample is composed of white men. The variable *World War II Service* is the fraction of all men in a given birth cohort who were veterans of World War II, regardless of their military service status in other periods. The variables *Korean War Service* and *Korean & Interwar Period Service* are the fraction of men who identified themselves as having participated in those conflicts but did not also serve in World War II. The time trend is defined as year of birth - 1929 + (quarter of birth/4). Standard error estimates are corrected for heteroskedasticity and shown in parentheses.
Table 4: OLS Estimates of the Effect of College Education on Marital Status and Wife’s Educational Attainment

<table>
<thead>
<tr>
<th>Birth Cohorts</th>
<th>1923-29</th>
<th>1923-30</th>
<th>1923-32</th>
<th>1923-38</th>
<th>1922-39</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td></td>
</tr>
</tbody>
</table>

**All Men:**

- Probability Married
  - 0.02
  - 0.01
  - 0.04
  - 0.01
  - -0.01
  - (0.04)
  - (0.03)
  - (0.02)
  - (0.02)
  - (0.02)

- Probability Separated or Divorced
  - -0.02
  - 0.01
  - -0.02
  - -0.01
  - -0.01
  - (0.02)
  - (0.02)
  - (0.02)
  - (0.01)
  - (0.01)

**Married Men:**

- Wife's Years of Schooling
  - 1.22
  - 1.32
  - 0.87
  - 0.78
  - 0.77
  - (0.21)
  - (0.20)
  - (0.17)
  - (0.15)
  - (0.13)

- Wife High School Graduate
  - 0.23
  - 0.29
  - 0.17
  - 0.13
  - 0.12
  - (0.05)
  - (0.06)
  - (0.05)
  - (0.04)
  - (0.03)

- Wife Enrolled in College
  - 0.17
  - 0.15
  - 0.14
  - 0.15
  - 0.14
  - (0.03)
  - (0.03)
  - (0.03)
  - (0.03)
  - (0.02)

- Wife College Graduate
  - 0.12
  - 0.11
  - 0.08
  - 0.09
  - 0.09
  - (0.04)
  - (0.03)
  - (0.02)
  - (0.02)
  - (0.02)

**Controls:**

<table>
<thead>
<tr>
<th>Linear Trend</th>
<th>x</th>
<th>x</th>
<th>x</th>
<th>x</th>
<th>x</th>
</tr>
</thead>
<tbody>
<tr>
<td>Korean &amp; Interwar Period Service x Trend</td>
<td>x</td>
<td>x</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Trend^2</td>
<td>x</td>
<td>x</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Korean &amp; Interwar Period Service x Trend^2</td>
<td>x</td>
<td>x</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Source: 1970 Integrated Public Use Microdata Series (IPUMS, three 1% samples)

Note: The sample is composed of white men. The variable College Education is measured in years. The variables Korean War Service and Korean & Interwar Period Service are the fraction of men who identified themselves as having participated in those conflicts but did not also serve in World War II. The time trend is defined as year of birth - 1929 + (quarter of birth/4). Standard error estimates are corrected for heteroskedasticity and shown in parentheses.
### Table 5: Two-Stage Least Squares Estimates of the Effect of College Education on Marital Status and Wife’s Educational Attainment

<table>
<thead>
<tr>
<th>Birth Cohorts</th>
<th>1923-29</th>
<th>1923-30</th>
<th>1923-32</th>
<th>1923-38</th>
<th>1922-39</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
</tbody>
</table>

#### All Men

- **Probability Married**: 0.09, 0.01, 0.07, 0.01, -0.02
  - (0.14), (0.09), (0.07), (0.06), (0.03)
- **Probability Separated or Divorced**: -0.07, -0.01, -0.04, -0.01, -0.02
  - (0.04), (0.04), (0.02), (0.03), (0.01)

- **First Stage F-Statistic**: 5.3, 8.2, 29.4, 11.7, 32.0

#### Married Men

- **Wife’s Years of Schooling**: 3.93, 3.00, 1.24, 1.21, 0.86
  - (1.87), (1.31), (0.33), (0.43), (0.21)
- **Wife High School Graduate**: 0.85, 0.74, 0.28, 0.26, 0.14
  - (0.53), (0.35), (0.09), (0.11), (0.05)
- **Wife Enrolled in College**: 0.25, 0.22, 0.13, 0.15, 0.14
  - (0.15), (0.15), (0.06), (0.07), (0.04)
- **Wife College Graduate**: 0.05, 0.01, 0.01, 0.08, 0.07
  - (0.23), (0.17), (0.07), (0.06), (0.03)

- **First Stage F-Statistic**: 2.1, 2.9, 21.0, 8.0, 23.8

#### Controls:

- Linear Trend: x x x x x
- Korean & Interwar Period Service x Trend: x x
- Trend$^2$: x x
- Korean & Interwar Period Service x Trend$^2$: x x

**Source:** 1970 Integrated Public Use Microdata Series (IPUMS, three 1% samples)

**Note:** The sample is composed of white men. The variable *World War II Service* is the fraction of all men in a given birth cohort who were veterans of World War II, regardless of their military service status in other periods. This variable is used as an instrument for men’s years of college. The variables *Korean War Service* and *Korean & Interwar Period Service* are the fraction of men who identified themselves as having participated in those conflicts but did not also serve in World War II. The time trend is defined as a year of birth - 1929 + (quarter of birth/4). Standard error estimates are corrected for heteroskedasticity and shown in parentheses.
Table 6: Reduced Form Estimates of the Effect of WWI Service on Men's College Education Marital Status and Wife's Educational Attainment

| Birth Cohorts | 1892-1901 |

**All Men:**

- **Years of Completed College:**
  - World War I Service: 0.00 (0.03)

- **Years of Education:**
  - World War I Service: 0.22 (0.15)

- **Probability Married:**
  - World War I Service: 0.04 (0.02)

**Married Men:**

- **Husband's Years of Completed College:**
  - World War I Service: 0.00 (0.04)

- **Husband's Years of Education:**
  - World War I Service: 0.10 (0.18)

- **Wife's Years of Education:**
  - World War I Service: 0.13 (0.18)

**Controls:**

| Linear Trend | X |

Source: 1930 and 1940 Integrated Public Use Microdata Series (IPUMS, 1% samples)

Note: Estimates are based on birth-year cell level averages for white men born between 1892 and 1901. Birth-year averages for education come from the 1940 Census. Birth-year averages for World War I service come from the 1930 Census. *World War I Service* is defined as the fraction of all men in a given birth cohort who were veterans of World War I. Each regression contains a time trend defined as year of birth. Standard error estimates are corrected for heteroskedasticity and shown in parentheses.
Table 7: Reduced Form Estimates of the Effect of WWII Service on Marital Status and Wife’s Educational Attainment Controlling for Home Ownership

<table>
<thead>
<tr>
<th></th>
<th>Birth Cohorts</th>
<th>1923-29</th>
<th>1923-30</th>
<th>1923-32</th>
<th>1923-38</th>
<th>1922-39</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>All Men:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Years of Completed College:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>World War II Service</td>
<td></td>
<td>0.33</td>
<td>0.41</td>
<td>0.50</td>
<td>0.44</td>
<td>0.66</td>
</tr>
<tr>
<td>(0.14)</td>
<td></td>
<td>(0.15)</td>
<td>(0.11)</td>
<td>(0.13)</td>
<td>(0.12)</td>
<td></td>
</tr>
<tr>
<td>Own Home, 1970</td>
<td></td>
<td>1.00</td>
<td>0.70</td>
<td>0.03</td>
<td>-0.02</td>
<td>-0.41</td>
</tr>
<tr>
<td>(0.93)</td>
<td></td>
<td>(0.87)</td>
<td>(0.57)</td>
<td>(0.52)</td>
<td>(0.47)</td>
<td></td>
</tr>
<tr>
<td>Probability Married:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>World War II Service</td>
<td></td>
<td>0.03</td>
<td>0.01</td>
<td>0.04</td>
<td>0.00</td>
<td>-0.01</td>
</tr>
<tr>
<td>(0.05)</td>
<td></td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.03)</td>
<td>(0.02)</td>
<td></td>
</tr>
<tr>
<td>Own Home, 1970</td>
<td></td>
<td>0.05</td>
<td>0.05</td>
<td>0.13</td>
<td>0.22</td>
<td>0.25</td>
</tr>
<tr>
<td>(0.14)</td>
<td></td>
<td>(0.14)</td>
<td>(0.12)</td>
<td>(0.10)</td>
<td>(0.09)</td>
<td></td>
</tr>
<tr>
<td>Probability Separated or Divorced:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>World War II Service</td>
<td></td>
<td>-0.02</td>
<td>0.00</td>
<td>-0.02</td>
<td>0.00</td>
<td>-0.01</td>
</tr>
<tr>
<td>(0.01)</td>
<td></td>
<td>(0.02)</td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.01)</td>
<td></td>
</tr>
<tr>
<td>Own Home, 1970</td>
<td></td>
<td>0.10</td>
<td>0.08</td>
<td>0.04</td>
<td>-0.06</td>
<td>-0.05</td>
</tr>
<tr>
<td>(0.08)</td>
<td></td>
<td>(0.08)</td>
<td>(0.07)</td>
<td>(0.06)</td>
<td>(0.05)</td>
<td></td>
</tr>
<tr>
<td><strong>F-Statistic</strong></td>
<td></td>
<td>5.7</td>
<td>7.7</td>
<td>20.6</td>
<td>11.6</td>
<td>30.2</td>
</tr>
</tbody>
</table>

**Married Men:**

Husband's Years of Completed College:

<table>
<thead>
<tr>
<th></th>
<th>Birth Cohorts</th>
<th>1923-29</th>
<th>1923-30</th>
<th>1923-32</th>
<th>1923-38</th>
<th>1922-39</th>
</tr>
</thead>
<tbody>
<tr>
<td>World War II Service</td>
<td></td>
<td>0.18</td>
<td>0.22</td>
<td>0.42</td>
<td>0.37</td>
<td>0.57</td>
</tr>
<tr>
<td>(0.12)</td>
<td></td>
<td>(0.13)</td>
<td>(0.11)</td>
<td>(0.13)</td>
<td>(0.13)</td>
<td></td>
</tr>
<tr>
<td>Own Home, 1970</td>
<td></td>
<td>0.84</td>
<td>0.64</td>
<td>0.01</td>
<td>-0.05</td>
<td>-0.47</td>
</tr>
<tr>
<td>(0.82)</td>
<td></td>
<td>(0.80)</td>
<td>(0.63)</td>
<td>(0.62)</td>
<td>(0.61)</td>
<td></td>
</tr>
</tbody>
</table>

Wife's Years of Schooling:

<table>
<thead>
<tr>
<th></th>
<th>Birth Cohorts</th>
<th>1923-29</th>
<th>1923-30</th>
<th>1923-32</th>
<th>1923-38</th>
<th>1922-39</th>
</tr>
</thead>
<tbody>
<tr>
<td>World War II Service</td>
<td></td>
<td>0.70</td>
<td>0.66</td>
<td>0.65</td>
<td>0.44</td>
<td>0.55</td>
</tr>
<tr>
<td>(0.20)</td>
<td></td>
<td>(0.21)</td>
<td>(0.17)</td>
<td>(0.16)</td>
<td>(0.14)</td>
<td></td>
</tr>
<tr>
<td>Own Home, 1970</td>
<td></td>
<td>1.76</td>
<td>2.14</td>
<td>1.82</td>
<td>1.85</td>
<td>1.33</td>
</tr>
<tr>
<td>(1.27)</td>
<td></td>
<td>(1.17)</td>
<td>(0.82)</td>
<td>(0.93)</td>
<td>(0.90)</td>
<td></td>
</tr>
</tbody>
</table>

**F-Statistic** | 2.3 | 3.0 | 14.6 | 8.0 | 20.8 |

**Controls:**

<table>
<thead>
<tr>
<th></th>
<th>Birth Cohorts</th>
<th>1923-29</th>
<th>1923-30</th>
<th>1923-32</th>
<th>1923-38</th>
<th>1922-39</th>
</tr>
</thead>
<tbody>
<tr>
<td>Linear Trend</td>
<td></td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Korean &amp; Interwar Period Service x Trend</td>
<td></td>
<td>x</td>
<td>x</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Trend²</td>
<td></td>
<td>x</td>
<td>x</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Korean &amp; Interwar Period Service x Trend²</td>
<td></td>
<td>x</td>
<td>x</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Source: 1970 Integrated Public Use Microdata Series (IPUMS, three 1% samples)

Note: The sample is composed of white men. The variable *World War II Service* is the fraction of all men in a given birth cohort who were veterans of World War II, regardless of their military service status in other periods. The variables *Korean War Service* and *Korean & Interwar Period Service* are the fraction of men who identified themselves as having participated in those conflicts but did not also serve in World War II. The variable *Own Home* is the share of the cohort living in a home owned by himself or someone in the household. The time trend is defined as year of birth - 1929 + (quarter of birth/4). Standard error estimates are corrected for heteroskedasticity and shown in parentheses.
Table 8: Reduced Form Estimates of the Effect of WWII Service on Marital Status and Wife's Educational Attainment Controlling for the Male/Female Sex Ratio

<table>
<thead>
<tr>
<th>Birth Cohorts</th>
<th>1923-29</th>
<th>1923-30</th>
<th>1923-32</th>
<th>1923-38</th>
<th>1922-39</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(2)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(3)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(4)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(5)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**All Men:**

Years of Completed College:
- **World War II Service**
  - 0.35 (0.15)
  - 0.38 (0.14)
  - 0.49 (0.10)
  - 0.45 (0.13)
  - 0.65 (0.12)
- **Sex Ratio**
  - 0.05 (0.11)
  - 0.03 (0.10)
  - -0.02 (0.09)
  - -0.02 (0.07)
  - -0.07 (0.06)

Probability Married:
- **World War II Service**
  - 0.05 (0.04)
  - 0.00 (0.03)
  - 0.04 (0.03)
  - 0.00 (0.03)
  - -0.01 (0.02)
- **Sex Ratio**
  - 0.06 (0.01)
  - 0.05 (0.01)
  - 0.02 (0.02)
  - 0.00 (0.02)
  - 0.00 (0.01)

Probability Separated or Divorced:
- **World War II Service**
  - -0.03 (0.01)
  - 0.00 (0.02)
  - -0.02 (0.01)
  - -0.01 (0.01)
  - -0.01 (0.01)
- **Sex Ratio**
  - -0.02 (0.01)
  - -0.02 (0.01)
  - 0.00 (0.01)
  - 0.00 (0.01)
  - 0.00 (0.01)

**F-Statistic**
- 5.5
- 7.6
- 26.4
- 11.7
- 30.0

**Married Men:**

Husband's Years of Completed College:
- **World War II Service**
  - 0.19 (0.12)
  - 0.20 (0.12)
  - 0.41 (0.09)
  - 0.37 (0.13)
  - 0.57 (0.12)
- **Sex Ratio**
  - 0.04 (0.10)
  - 0.03 (0.10)
  - -0.05 (0.09)
  - -0.03 (0.09)
  - -0.08 (0.07)

Wife's Years of Schooling:
- **World War II Service**
  - 0.69 (0.23)
  - 0.63 (0.22)
  - 0.50 (0.18)
  - 0.46 (0.17)
  - 0.49 (0.14)
- **Sex Ratio**
  - -0.06 (0.19)
  - -0.18 (0.17)
  - -0.07 (0.14)
  - -0.08 (0.10)
  - -0.10 (0.09)

**F-Statistic**
- 2.4
- 2.8
- 19.2
- 8.4
- 22.7

**Controls:**
- Linear Trend
  - x
  - x
  - x
  - x
  - x
- Korean & Interwar Period Service x Trend
  - x
  - x
- Trend²
  - x
  - x
- Korean & Interwar Period Service x Trend²
  - x
  - x

Source: 1970 Integrated Public Use Microdata Series (IPUMS, three 1% samples)

Note: The sample is composed of white men. The variable *World War II Service* is the fraction of all men in a given birth cohort who were veterans of World War II, regardless of their military service status in other periods. The variables *Korean War Service* and *Korean & Interwar Period Service* are the fraction of men who identified themselves as having participated in those conflicts but did not also serve in World War II. We define the *Sex Ratio* as the number of men in a birth cohort divided by the average number of women born in the previous and following eight quarters. The time trend is defined as year of birth - 1929 + (quarter of birth/4). Standard error estimates are corrected for heteroskedasticity and shown in parentheses.
### Table 9: Reduced Form Estimates of the Effect of WWII Service on Women's Education and Husbands' Educational Attainment

<table>
<thead>
<tr>
<th>Birth Cohorts</th>
<th>1923-29</th>
<th>1923-30</th>
<th>1923-32</th>
<th>1923-38</th>
<th>1922-39</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
</tbody>
</table>

**Male WWII participation matched to female cohorts (1970 Census)**

**All Women:**
- Years of Completed College: -0.09, -0.03, -0.19, 0.23, 0.13
  - (0.27), (0.18), (0.15), (0.12), (0.09)
- Probability Married: 0.02, 0.03, 0.00, -0.02, -0.04
  - (0.03), (0.03), (0.02), (0.03), (0.02)
- Probability Separated or Divorced: -0.03, -0.04, -0.03, 0.01, 0.02
  - (0.02), (0.02), (0.02), (0.02), (0.01)
- F-Statistic: 0.1, 0.0, 1.8, 3.5, 2.2

**Married Women:**
- Wife's Years of Completed College: 0.01, 0.06, -0.14, 0.22, 0.14
  - (0.22), (0.17), (0.14), (0.13), (0.10)
- Husband's Years of Schooling: 0.15, 0.50, 0.32, 0.11, 0.16
  - (0.40), (0.38), (0.28), (0.32), (0.25)
- F-Statistic: 0.0, 0.2, 1.0, 2.9, 2.1

**Female WWII participation from 1980 Census matched to female cohorts in 1970**

**All Women:**
- Years of Completed College: -0.24, -0.14, -0.70, 2.23, 0.21
  - (0.63), (0.47), (0.41), (1.28), (0.75)
- Probability Married: 0.11, 0.15, 0.05, 0.62, 0.33
  - (0.11), (0.09), (0.09), (0.23), (0.17)
- Probability Separated or Divorced: -0.15, -0.20, -0.15, -0.42, -0.11
  - (0.10), (0.09), (0.08), (0.17), (0.16)
- F-Statistic: 0.2, 0.1, 2.9, 3.0, 0.1

**Married Women:**
- Wife's Years of Completed College: -0.34, -0.17, -0.91, 2.36, -0.06
  - (0.64), (0.50), (0.52), (1.16), (0.71)
- Husband's Years of Schooling: -1.24, 0.13, -0.43, 0.06, 2.01
  - (1.99), (2.04), (1.65), (3.57), (2.50)
- F-Statistic: 0.3, 0.1, 3.1, 4.1, 0.0

**Controls:**
- Linear Trend: x, x, x, x, x
- Korean & Interwar Period Service x Trend: x, x
- Trend²: x, x
- Korean & Interwar Period Service x Trend²: x, x

Sources: 1970 and 1980 IPUMS (Three 1% samples (1970); 5% sample (1980)).

Note: The sample is composed of white women. The variable *World War II Service* is the fraction of all men in a given birth cohort who were veterans of World War II, regardless of their military service status in other periods. The variables *Korean War Service* and *Korean & Interwar Period Service* are the fraction of men who identified themselves as having participated in those conflicts but did not also serve in World War II. The time trend is defined as year of birth - 1929 + (quarter of birth/4). Standard error estimates are corrected for heteroskedasticity and shown in parentheses.
Appendix Figure 1: World War II Participation, 1960 1970 1980 Census

Note: Figure is based on data from the 1% 1960, 3% 1970, and 5% 1980 IPUMS samples. Sample based on white men born in the contiguous United States between 1908 and 1946.