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War and Marriage: Assortative Mating and the World War II GI Bill

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Abstract World War II and its subsequent GI Bill have been widely credited with playing a transformative role in American society, but there have been few quantitative analyses of these historical events' broad social effects. We exploit between-cohort variation in the probability of military service to investigate how WWII and the GI Bill altered the structure of marriage, and find that it had important spillover effects beyond its direct effect on men's educational attainment. Our results suggest that the additional education received by returning veterans caused them to "sort" into wives with significantly higher levels of education. This suggests an important mechanism by which socioeconomic status may be passed on to the next generation.

Keywords Marital sorting · Education · WWII GI Bill

Introduction

World War II (WWII) and its subsequent Servicemen's Readjustment Act of 1944—informally known as the GI 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 more than 70 % of male enrollment in the

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immediate postwar years—and research has shown that these increases were related to the availability of postwar educational benefits combined with military service. Bound and Turner (2002), for example, documented that WWII and the subsequent GI Bill increased collegiate completion rates by approximately 40 %. The “legend” of the WWII GI Bill extends beyond its direct effects on education, however. For example, in his book *When Dreams Came True: The G.I. Bill and the Making of Modern America* (1996:8), Michael Bennett concluded 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, even how we eat and transport ourselves.” Similarly, Drucker (1993:3) stated 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 GI Bill’s broader social effects. This is somewhat surprising because the bill’s combined breadth and generosity surpass that of any other education policy in modern America. Furthermore, a burgeoning literature documents that in the modern context, exogenous shocks to education causally reduce crime, improve health, and increase human capital among individuals’ offspring (e.g., Currie and Moretti 2003; Lleras-Muney 2005; Lochner and Moretti 2004; Maurin and McNally 2008; Oreopoulos et al. 2006; Page 2007). Thus, it is plausible that the increase in education associated with WWII and the GI Bill had important spillover effects.

The aim of this article is to document how WWII and the resultant GI Bill affected the marital outcomes of returning veterans. In doing so, we hope to shed light on how these historical events affected a dimension of American society that is both interesting in its own right and has important implications for the intergenerational transmission of socioeconomic status.[1] Our analyses also provide insights into the mechanisms underlying assortative mating, which are not well understood. We use cross-cohort variation in military service rates to identify these effects, essentially exploiting the fact that sharp differences in conscription rates across individuals’ birth dates lead to different opportunities for men whom we would otherwise expect to be very similar.

We find evidence that WWII and the GI Bill had substantive effects on marital sorting. Cohorts who were eligible for GI benefits married women who had approximately 0.4 more years of education than cohorts who just missed the eligibility cutoff. Their wives were also discontinuously older. The most likely mechanism is that men’s marital opportunities were changed by the additional education that the GI Bill provided. Our estimates do not appear to be driven by other contemporaneous factors, such as GI housing benefits, combat-related differences in the sex ratio, changes in women’s educational opportunities, or changes in women’s human capital investments after marriage. Furthermore, when we use a similar estimation strategy to examine WWI cohorts, who did not receive educational benefits, we do not find evidence of discontinuous changes in either their own or their wives’ education levels. This suggests that our results are not driven by the effects of military service itself. Our findings add to the mounting evidence that individuals’ education investments have important spillover effects and that the well-documented associations between

[1] 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.

education and other measures of well-being are not simply an artifact of cross-sectional variation in innate characteristics.

Background

The GI 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 and had been honorably discharged. 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 by 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 GI Bill offered very generous financial provisions. It provided full tuition, books, and supplies for 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 one-half of the opportunity cost of not working; for a married veteran, it was equal to about 70 % of the opportunity cost (Bound and Turner 2002).

Bound and Turner (2002) and Stanley (2003) thoroughly investigated the effect of this legislation on men's schooling.[2] Bound and Turner, using between-cohort differences in military service generated by wartime changes in manpower requirements to identify the likelihood that an individual was benefit eligible, estimated that GI benefits increased white men's collegiate attainment by about 40 %. Stanley's estimates were based on comparisons of postsecondary education levels among cohorts of veterans who were less likely to avail themselves of the GI Bill because they had already completed their education with those who likely entered the military straight out of high school. This estimation strategy suggested that among veterans born between 1923 and 1926, the GI Bill increased postsecondary education levels by about 20 %.

These empirical strategies were motivated by concerns about selection into military service. Comparisons of educational attainment between veterans and nonveterans are likely to lead to overestimates of the legislation's effect because one of the primary reasons for deferment from WWII service was physical or mental disability. Among 19- to 25-year-old men deferred in 1945, for example, 56 % were deemed physically or mentally unfit (Bound and Turner 2002). Because individuals with low mental capacity probably had lower levels of education than average, veteran status alone is unlikely to identify the effects of the GI Bill.

Bound and Turner's identification strategy circumvented 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 and provides some intuition behind their estimation strategy. 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 Training and Service Act, which

[2] In a related study, Lemieux and Card (2001) estimated the effect of the Canadian GI Bill on education and earnings.

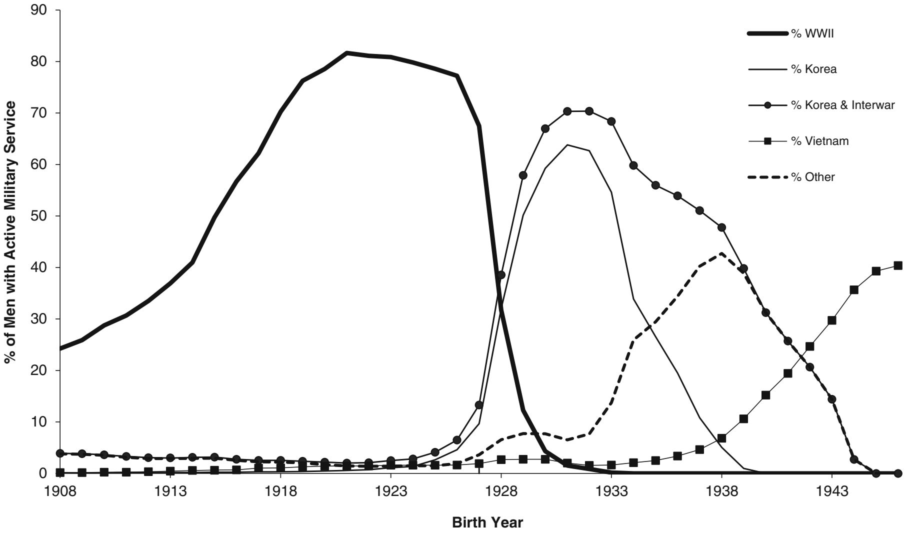

Fig. 1 Rates of military service by birth cohort. Calculations are based on white men born in the contiguous United States. *Source:* 1970 Integrated Public Use Microdata Series (IPUMS, three 1 % samples)

mandated registration of young men (ages 21–35) and ultimately required enlistment among those who were deemed eligible. After the United States entered WWII, registration was extended to men between the ages of 18 and 65, and men between the ages of 18 and 45 were subject to military service. As a result, cohorts born between 1920 and 1926, who would have been subject to the draft, experienced participation rates that were nearly constant, at roughly 80 %. Among those who turned 18 after V-J Day (cohorts born after the third quarter of 1927), service rates plummeted. Given that the draft produced a sharp correlation between benefit eligibility and an individual's birth date, but that 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 GI Bill.

This article exploits Bound and Turner's identification strategy to investigate the broader social impacts of the GI Bill. Although historians frequently credit the GI Bill with having created permanent changes in the structure of American society, most quantitative studies have been confined to analyses of its impact on education and earnings (Angrist and Krueger 1994; Bound and Turner 2002; Lemieux and Card 2001; Stanley 2003). There is reason to believe, however, that the GI Bill may have affected individuals' outcomes beyond 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 et al. 2006; Thomas et al. 1991), so a natural question is whether the additional education induced by wartime events had spillover effects onto other outcomes.[3] Only a few

[3]Recent studies have documented that the Vietnam War draft lottery had an impact on nonwage outcomes such as marital status, migration, and health. See, for example, Angrist and Chen (2011), Conley and Heerwig (2011), McCarthy (2012, 2013), and Malamud and Wozniak (2012). Similarly, Galiani et al. (2011) estimated the impact of military service on crime using the random assignment of men to military service in Argentina.

studies have empirically explored the relationship among WWII, the GI Bill, and nonlabor market outcomes;[4] and to our knowledge, no one has yet investigated the impact that these historic events may have had on marital opportunities and marital sorting in the United States.

There are several mechanisms by which WWII and the GI Bill might have affected veterans' probabilities of marriage and their ability to attract higher "quality" spouses than they might have otherwise. First, positive assortative mating on education is well documented (e.g., Cancian et al. 1993; Jepsen and Jepsen 2002; Juhn and Murphy 1997; Mare 1991; McCarthy 2013; Pencavel 1998), and as noted earlier, cohorts with high conscription rates obtained more schooling than those who just missed the cutoff. Veterans' presence in post-secondary institutions would have naturally brought them into contact with women attending those institutions (e.g., Nielsen and Svarer 2009). Education is also associated with higher earnings, occupations, and socioeconomic status. All these outcomes might affect the pool of available mates by changing affected cohorts' attractiveness to potential partners. If the additional education expands the set of possible matches to include women with higher levels of completed schooling, then the opportunity cost associated with choosing a partner with a low level of education increases (e.g., Fernandez et al. 2005). 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.

Second, military service might have an independent effect on marital outcomes. Elder et al. (1991:218) found that WWII generated "instant maturity" among those who served, which may have lead to different marital choices. The prestige associated with WWII service may have also increased veterans' marital prospects. Veterans may have also learned skills during the war that could be transferred to the labor market, thus increasing their earnings potential and making them more attractive marriage partners. Previous studies by Angrist and Krueger (1994) and Lemieux and Card (2001) found no evidence that WWII veterans earned more than nonveterans, but the possibility that skills learned during wartime service increased men's economic potential should nevertheless be kept in mind. On the other hand, physical and emotional disabilities resulting from combat may have reduced some veterans' marital prospects. Suggestive evidence on this front is provided in two qualitative studies by Pavalko and Elder (1990) and Dechter and Elder (2004), who found that among a small sample of high-ability men born prior to 1920, those who served during WWII had worse labor market outcomes and were more likely to divorce than those who did not. Wilmoth and London (2012) also provided an extensive consideration of the potential impacts and variation over the life course of military service more generally. We will explore these possible mechanisms in the upcoming section, Mediating Relationships and Further Interpretation.

[4] Bedard and Deschenes (2006) found 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 (2013) found evidence of a fading relationship between GI eligibility and homeownership, and Page (2007) showed that the children of affected cohorts had lower probabilities of repeating a grade.

Estimation Strategy

To begin, consider the following reduced-form equations:

$$HEd_{ic} = \phi_1 HCohort_{ic} + \phi_2 V_{ic} + \phi_3 \mathbf{X}_{ic} + \mu_{ic} \tag{1}$$

$$Married_{ic} = \beta_1 HCohort_{ic} + \beta_2 V_{ic} + \beta_3 \mathbf{X}_{ic} + \varepsilon_{ic} \tag{2}$$

$$WEd_{ic} = \varphi_1 HCohort_{ic} + \varphi_2 V_{ic} + \varphi_3 \mathbf{X}_{ic} + v_{ic}, \tag{3}$$

where *HEd* measures the educational attainment of individual *i* belonging to cohort *c*, *Married* is an indicator variable equal to 1 if individual *i* belonging to cohort *c* is married, and *WEd* is the educational attainment of individual *i*'s wife. *HCohort* is a linear variable measuring the cohort (by birth year and birth quarter) to which the man belongs, and controls for secular changes in educational attainment and marriage. **X** is a vector of controls. *V* is a dummy variable equal to 1 if individual *i* in cohort *c* is a WWII veteran and equal to 0 if he is not. The parameters ϕ_2, β_2, and φ_2 represent the effect of WWII participation on the three outcomes. Ordinary least squares (OLS) estimation of Eqs. (1)–(3) is unlikely to produce causal estimates of these parameters, however, because individuals' selection into military service was (and continues to be) nonrandom.

To overcome this selection problem, we take advantage of the fact that the end of the war generated abrupt differences in conscription rates between men born in the mid 1920s and late 1920s. Figure 1 and Table 1 make clear that although men born between 1923 and 1926 had participation rates approaching 80 %, the vast majority of men born after 1927 did not serve in WWII and would not have been eligible for GI benefits provided to WWII veterans. Cross-cohort variation in WWII participation rates generated by the sharp drop in manpower needs is independent of individual characteristics and can thus be used to circumvent selection bias and isolate the average effect of service, in the same spirit as Heckman and Robb (1985).

Focusing on this source of variation and collapsing Eqs. (1)–(3) to the cohort level, we obtain

$$\overline{HEd}_c = \phi_1 HCohort_c + \phi_2 \%WWII_c + \phi_3 \overline{X}_c + \mu_c \tag{1a}$$

$$\overline{Married}_c = \beta_1 HCohort_c + \beta_2 \%WWII_c + \beta_3 \overline{X}_c + \varepsilon_c \tag{2a}$$

$$\overline{WEd}_c = \varphi_1 HCohort_c + \varphi_2 \%WWII_c + \varphi_3 \overline{X}_c + v_c, \tag{3a}$$

where *%WWII* is the fraction of men in cohort *c* who served during WWII, and $\overline{X}$ are cohort average characteristics. Because we include linear trends and focus on cohorts born within narrow windows, we effectively identify the effect of veteran status using deviations from underlying trends in educational attainment and marriage. The most meaningful variation occurs between cohorts born between 1926 and 1928. Indeed, we obtain very similar results when we replace *%WWII* with a dummy variable indicating

Table 1 Summary statistics

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

Notes: 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 WWII, 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 WWII. *Highest Grade Completed, Wife* is based on the wives of the married men in our sample.

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

that the cohort was born after 1927.[5] However, because cohorts born close to 1927 (both before and after) effectively faced the same pool of potential partners, φ_2 captures the combined effect of any increase in wives' education levels that was experienced by the cohorts that were eligible for GI benefits *and* the resulting crowd-out experienced by the cohorts who just missed the cutoff. In other words, given a fixed distribution of education among potential partners, gains in wives' education for one group of men were likely accompanied by declines for others. This means that the difference in wives' average educational attainment between the treatment group and the control group is, in all likelihood, larger than the gain that the treatment group experienced relative to what it would have experienced in the absence of the war (or the partial equilibrium effect). This should be kept in mind when interpreting the estimates throughout the rest of this article.[6]

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 one-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; however, unlike men subject to the WWII draft, men who wanted to avoid serving in Korea could obtain educational deferments. As a result, estimates based on simple comparisons between cohorts who turned 18 on either side of V-J Day are likely to be compromised by the effects of the Korean War. Instead of estimating Eqs. (1a)–(3a), we therefore use Bound and Turner (2002) as a guide and estimate the following augmented equations:

$$\overline{HEd}_c = \phi_1 HCohort_c + \phi_2 \%WWII_c + \phi_3 \%Korea_c + \phi_4 \%Korea \times HCohort_c + \phi_5 \overline{X}_c + \mu_c \tag{1b}$$

$$\overline{Married}_c = \beta_1 HCohort_c + \beta_2 \%WWII_c + \beta_3 \%Korea_c + \beta_4 \%Korea \times HCohort_c + \beta_5 \overline{X}_c + \varepsilon_c \tag{2b}$$

$$\overline{WEd}_c = \varphi_1 HCohort_c + \varphi_2 \%WWII_c + \varphi_3 \%Korea_c + \varphi_4 \%Korea \times HCohort_c + \varphi_5 \overline{X}_c + \nu_c, \tag{3b}$$

[5] Seminar participants 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 (2003), who identified the impact of GI 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 et al. (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) found that estimates of the impact of GI benefits that used state-level mobilization rates as an instrument for eligibility were sensitive to the inclusion of state-level control variables.

[6] If the treatment and control groups were exactly the same size, and were pulling from exactly the same pool of women, then a reasonable approximation of the partial equilibrium effect would be one-half of the estimated difference between the treatment and control groups. As more cohorts are added to the sample, however, the assumption that both groups are pulling wives from the same pool of women becomes increasingly tenuous, and more assumptions need to be made to estimate the magnitude of the partial equilibrium effect.

where *%Korea* is the fraction of men in the year and quarter-of-birth cell who identified themselves as Korean War veterans, and the interaction term between *%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 given that Korean War educational deferments were not introduced until 1951.[7]

The success of our identification strategy hinges on the assumption that in the absence of the war, cross-cohort variation in individual characteristics would not have followed the same discontinuous pattern as WWII participation rates. One might be concerned that the Great Depression threatens this assumption because some of our control cohorts were born during the Great Depression, while all of the treated cohorts were born when the economy was booming. Dehejia and Lleras-Muney (2004) documented that infants conceived during recessions are healthier than infants conceived when the economy is doing well; others have found that health at birth is a positive predictor of labor market success (e.g., Behrman and Rosenzweig 2004; Black et al. 2007; Oreopoulos et al. 2008; Royer 2009). If these effects play out in our estimates, however, they will cause us to underestimate the impact of WWII on assortative mating. Thomasson and Fishback (2014) found that the economic outcomes of most adults who were born during the Great Depression were not compromised.[8] Further, the high unemployment rates that characterized the Great Depression occurred during the early 1930s, several years after the birthdate discontinuity upon which our identification rests.

We further minimize the probability that the Great Depression, or other time-varying factors, contaminate our estimates by including a linear time trend and focusing on men born within a narrow time interval. Replacing the linear trend with year-of-birth and quarter-of-birth dummy variables yields very similar results. Our estimates are robust to confining the analyses to cohorts conceived before 1930. In analyses not shown, we also used data from the Panel Study of Income Dynamics (PSID) and the 1973 Occupational Change in a Generation Survey (OCG) to investigate the extent to which pre-service family background 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 small samples, which yield imprecise estimates.[9]

[7] For the sake of completeness, we also estimated equations in which we replace *%WWII* and *%Korea* with a variable that measures the fraction of the cohort who served in either war. For the reasons described earlier, this specification does not seem ideal. Nevertheless, it produces estimates that follow the same pattern as our main estimates. Like our main estimates, they are positive and often statistically different from zero, but they are generally smaller in magnitude than the estimates produced by Eqs. (1a)–(3a).

[8] The exception is for individuals born in very poor southern states. Our results are robust to the exclusion of these states.

[9] 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 because 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 GI Bill effects that are biased downward. Because the OCG data do not include quarter of birth, these analyses are based on, at most, 15 data points.

A related concern is that any sample that is used to study the impacts of the GI Bill will include only those men who survived the war. 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, because 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 Fig. 4 in the appendix. Despite 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 led to differences in male/female sex ratios, which may have had an independent effect on marital sorting. We explore this possibility later in the article, in the section on mediating relationships.

Our estimation strategy also assumes that the direct effects of the GI Bill were concentrated almost exclusively on men, and that female education levels did not respond in the same discontinuous way. Given that only about 3 % of women born during this period served in WWII,[10] this seems like a reasonable assumption, but we will explore it more directly in the section on mediating relationships. It may also be useful to keep in mind that among the cohorts included in our analyses, only about 9 % were married when they began their service.[11]

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. 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. We chose the 1970 census over the 1980 census because the latter shows notably higher levels of schooling among our cohorts, which likely results from factors unrelated to the GI Bill, such as differential mortality, overreporting 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.

[10] This figure comes from authors' calculations based on the 1980 census.

[11] This figure is from authors' calculations based on Army enlistment records available online through the National Archives Access to Archival Database (AAD) (http://aad.archives.gov/aad/). Estimates are not expected to differ for other branches of the Armed Forces.

We begin by focusing on men who were born between 1923 and 1929, and then add successive post-1929 cohorts, until we reach the cohort that was born in 1938. These cohorts would have been drafted at age 18[12] 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: previous studies have shown that the effects of WWII and the GI Bill were quite different across racial groups. Turner and Bound (2003) showed that WWII and the GI Bill had little effect on the collegiate outcomes of black veterans living in Southern states, probably because their educational choices were already very limited. As a result, the GI Bill may have exacerbated the education gap between Southern blacks and whites. We also exclude all men for whom information on race, sex, age, or veteran status was allocated.

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 or more years of school. We define a WWII veteran as anyone who served in WWII. In our main analyses, a Korean War veteran is defined as anyone who indicated that they served in the military but not during WWII. In our initial replication exercises, however, we follow Bound and Turner, and define a Korean War veteran as anyone who served in the Korean War.

Table 1 shows descriptive statistics for all men, regardless of marital status, in our sample. Our analyses are based on between 136,666 and 393,629 individuals, but because our identifying variation is at the birth-cohort level, the analyses aggregate our individual observations into cells defined by year and quarter of birth. Consistent with previous studies, we find that rates of WWII 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 1929 and 1935. Across all cohorts, completed schooling shows an upward trend, but there is no evidence of a trend in marriage probabilities.

Results

Effects of WWII and the GI Bill on Men's Educational Attainment

We begin by exactly replicating Bound and Turner's estimates of the relationship between men's 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 WWII 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 Bound and Turner discussed, 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

[12]Cohorts born in the early part of 1923 may have been 19 at the time they were drafted because the draft age was lowered from 21 to 18 in November 1942.

Table 2 Estimated effects of WWII and Korean War service on men's college attainment

	Birth Cohorts								
	1923–1928 (1)	1923–1929 (2)	1923–1930 (3)	1923–1931 (4)	1923–1932 (5)	1923–1938 (6)	1923–1932 (7)	1923–1938 (8)	1923–1938 (9)
Replication of Bound and Turner (2002)									
All males									
Years of completed college									
World War II service	0.36 (0.25)	0.30 (0.12)	0.34 (0.10)	0.40 (0.08)	0.42 (0.07)	0.39 (0.05)	0.23 (0.14)	0.35 (0.06)	0.28 (0.12)
Korean War service	0.37 (0.37)	0.28 (0.16)	0.34 (0.13)	0.42 (0.09)	0.43 (0.09)	0.39 (0.04)	0.15 (0.19)	0.34 (0.05)	0.22 (0.19)
Married males									
Years of completed college									
World War II service	0.21 (0.24)	0.17 (0.12)	0.20 (0.10)	0.34 (0.08)	0.37 (0.07)	0.36 (0.05)	0.15 (0.15)	0.32 (0.06)	0.30 (0.13)
Korean War service	0.18 (0.35)	0.13 (0.14)	0.16 (0.11)	0.35 (0.09)	0.37 (0.09)	0.37 (0.04)	0.07 (0.20)	0.32 (0.05)	0.27 (0.20)
Number of observations	24	28	32	36	40	64	40	64	64
Controlling for Korean War and Interwar Period Service									
All Males									
Years of completed college									
World War II service	0.51 (0.35)	0.33 (0.15)	0.38 (0.13)	0.46 (0.10)	0.50 (0.09)	0.70 (0.09)	0.22 (0.18)	0.70 (0.09)	0.44 (0.13)
Korean and interwar period service	0.54 (0.49)	0.31 (0.18)	0.37 (0.17)	0.47 (0.12)	0.50 (0.11)	0.83 (0.09)	0.13 (0.23)	0.83 (0.09)	0.52 (0.13)
Married Males									
Years of completed college									
World War II service	0.29 (0.30)	0.18 (0.12)	0.20 (0.12)	0.38 (0.10)	0.42 (0.09)	0.63 (0.09)	0.10 (0.17)	0.64 (0.09)	0.37 (0.13)
Korean and interwar period service	0.27 (0.41)	0.12 (0.14)	0.15 (0.14)	0.37 (0.12)	0.41 (0.11)	0.76 (0.09)	–0.01 (0.23)	0.77 (0.09)	0.45 (0.13)
Number of observations	24	28	32	36	40	64	40	64	64

Table 2 (continued)

	Birth Cohorts								
	1923–1928	1923–1929	1923–1930	1923–1931	1923–1932	1923–1938	1923–1932	1923–1938	1923–1938
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Controls									
Linear trend	x	x	x	x	x	x	x	x	x
Korean (and interwar) service × Trend							x	x	x
Trend^2									x
Korean (and interwar) service × Trend^2									x

Notes: The sample is composed of white men. The variable *WWII service* is the fraction of all men in a given birth cohort who were veterans of WWII, regardless of their military service status in other periods. The variables *Korean War service* and *Korean and interwar period service* are the fraction of men who identified themselves as having participated in those conflicts but did not also serve in WWII. The time trend is defined as year of birth – 1929 + (quarter of birth / 4). We calculate Huber-White standard error estimates.

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

for GI 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.[13] Because the standard deviation in men's education is approximately three years, this represents a substantive difference in educational attainment.

Bound and Turner discussed the potentially contaminating effects of the Korean War, and noted that as younger cohorts are added to the analysis, these effects are less and less likely to be well captured by the *%Korea* variable. To address this concern, they added interactions between the percentage 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 nonlinear 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 of Table 2 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 with 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 GI Bill on men's schooling. 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, many men served in the military but do not identify themselves as veterans of 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 1955. Nevertheless, many of these men would have still qualified for educational benefits under the Korean War GI Bill because anyone who entered the military prior to February 1, 1955, and served for 90 days was eligible. When we more broadly control for the effects of the Korean War by including men who self-identified as serving either in Korea or at "any other time" (i.e., not during the specific war periods listed in the census survey), we find that the estimated effects of both WWII and Korean War service increase substantially (columns 6, 7, and 9).[14] We carry forward this definition of "probable" Korean War service throughout the rest of the article, but our findings are not affected by this decision in any substantial way.

[13]All our estimates exactly match Bound and Turner's except for our estimate based on the 1923–1932 cohorts: 0.42, versus Bound and Turner's published estimate of 0.30. The comparable estimate in the working paper version of their study (Bound and Turner 1999) is 0.42. Because the two sets of estimates are based on exactly the same specification, and all the estimates generated by the other samples match, we believe that the difference between the estimates for the 1923–1932 cohorts is likely due to a typographical error.

[14]As would be expected from Fig. 1, the estimates in columns 1–4 barely change.

The Relationship Among WWII, the GI Bill, and Assortative Mating

Given the clear association among WWII, the GI Bill, and men's education, it is natural to consider whether these historical events had spillover effects into other dimensions of family life. We begin to explore this possibility in Table 3, where we show estimated effects on marital status and wives' educational attainment using our preferred

Table 3 Reduced-form estimates of the effect of WWII service on men's college attainment, marital status, and wife's educational attainment

	Birth Cohorts			
	1923–1929	1923–1930	1923–1932	1923–1938
	(1)	(2)	(3)	(4)
All Men				
Years of completed college	0.33	0.38	0.50	0.44
	(0.15)	(0.13)	(0.09)	(0.13)
Probability married	0.03	0.00	0.04	0.00
	(0.05)	(0.04)	(0.03)	(0.03)
Probability separated or divorced	–0.02	0.00	–0.02	–0.01
	(0.01)	(0.02)	(0.01)	(0.01)
F statistic	5.3	8.2	29.4	11.7
Married Men				
Husband's years of completed college	0.18	0.20	0.42	0.37
	(0.12)	(0.12)	(0.09)	(0.13)
Wife's years of schooling	0.70	0.61	0.52	0.44
	(0.23)	(0.24)	(0.17)	(0.17)
Wife high school graduate	0.15	0.15	0.12	0.09
	(0.06)	(0.06)	(0.04)	(0.04)
Wife enrolled in college	0.05	0.05	0.06	0.06
	(0.03)	(0.03)	(0.03)	(0.03)
Wife college graduate	0.01	0.00	0.00	0.03
	(0.05)	(0.04)	(0.03)	(0.03)
F statistic	2.1	2.9	21.0	8.0
Number of Observations	28	32	40	64
Controls				
Linear trend	x	x	x	x
Korean and interwar period service × Trend				x
Trend^2				x
Korean and interwar period service × Trend^2				x

Notes: 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 WWII, regardless of their military service status in other periods. The variable *Korean and interwar period service* is the fraction of men who identified themselves as having participated in those conflicts but did not also serve in WWII. The time trend is defined as year of birth – 1929 + (quarter of birth / 4). We calculate Huber-White standard error estimates.

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

specifications. We find no evidence that the GI Bill had any effect on the probability of being married, separated, or divorced. These estimates are unsurprising given that cultural norms in the 1940s encouraged marriage, and there was no scarcity of "available" women. Among the cohorts used in our sample, the male/female ratio was around 0.98. In other words, the number of women exceeded the number of men. Given this, we would not expect to find that WWII cohorts crowded younger male cohorts out of marriage; rather, we would expect WWII service and the GI Bill to change the *type* of women that each group married. In fact, we find that WWII service improved men's 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. Our calculations from the census indicate that fewer than 9 % of white women born between 1923 and 1930 had bachelor's degrees. This suggests that the observed increases in spousal quality are not likely driven by immersion in the "marriage market" that college provided (see, e.g., Nielsen and Svarer 2009), and more likely occurred through other mechanisms.

These reduced-form estimates suggest that WWII and the GI Bill had substantive spillover effects beyond their effect on men's educational attainment. The estimated coefficients in column 4 of Table 3, for example, indicate that relative to men who just missed the cutoff, those who qualified for the GI Bill married women who had approximately 0.4 additional years of education. Because these two groups of men effectively faced the same marriage pool, this estimate potentially encompasses gains to the treatment group that came at the control group's expense. The estimate is therefore an upper bound estimate of what the GI Bill's partial equilibrium effect would have been if the control group's marital opportunities had remained constant.

Mediating Relationships and Further Interpretation

To clarify the nature of our estimated treatment effects, we next explore possible mechanisms. We first examine the role of direct channels other than the educational benefits provided by the GI Bill, which include the possible impacts of military service itself, GI housing benefits, and differing sex ratios across "treatment" and "control" cohorts. We also conduct more general falsification tests that are motivated by the fact that among women, GI benefit eligibility and take up was low. Finally, we consider whether our estimates reflect changes in sorting versus changes in human capital investments that took place *after* marriage by looking at cohort-level patterns in the age gap between husbands and wives.

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 GI benefits. The experience of serving in WWII may have had either positive or negative impacts on marital outcomes. One piece of evidence in this regard is that WWII veterans appear to have earned no more than nonveterans (Angrist and Krueger 1994; Lemieux and Card 2001). However, 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,[15] 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.

To glean a little more 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 WWI. Although these men received a monetary bonus for their service, educational benefits were not available to WWI veterans. Comparing the education and marital outcomes of cohorts near the WWI "break" may, therefore, provide some suggestive information about the likely influence of military service relative to educational benefits. In particular, although the experiences of servicemen differed in many important respects across the two wars, substantive differences in own or spouses' educational attainment between cohorts who served during WWI and those who narrowly missed the cutoff would call into question the plausibility of attributing our WWII estimates to GI benefits.[16]

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 we use this to estimate his year of birth. Following Fetter (2013), we look at men born between 1891 and 1902 for a change in outcomes across a participation cutoff for cohorts born between 1896 and 1897. Table 4 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. The coefficient estimates are small and noisy, thus providing no evidence that WWI participation affected any of these outcomes.[17] This helps strengthen our prior that the estimates in Tables 3 are not driven solely by cross-cohort variation in military experience.

[15]For example, Mettler (2005:10) stated, "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."

[16]Of course, there are many differences between the WWI and WWII eras, which might lead to different estimates. For example, U.S. troop involvement in WWI was more concentrated over time and not as broad as in WWII. The troops' warfront experiences also differed across the two wars, women played a larger role in WWII, and there were small increases in median education between the two periods. These factors might lead to different responses even without differences in GI benefits. Additionally, there is no guarantee that the true effects of education on marital outcomes were necessarily stable across this period. Although this exercise should therefore be considered cautiously, we believe that evidence of a discontinuity around the WWI cutoff would call into question the likelihood that GI benefits play a substantive role. The absence of such a discontinuity, while only suggestive, is nevertheless reassuring.

[17]We obtain the same qualitative result when we replace the *%WWI* variable with a dummy variable indicating that the cohort was born after 1896.

Table 4 Reduced-form estimates of the effect of WWI service on men's college education, marital status, and wife's educational attainment

	Birth Cohort: 1892–1901
All Men	
Years of completed college	
WWI service	0.00
	(0.03)
Years of education	
WWI service	0.22
	(0.15)
Probability married	
WWI service	0.04
	(0.02)
Married Men	
Husband's years of completed college	
WWI service	0.00
	(0.04)
Husband's years of education	
WWI service	0.10
	(0.18)
Wife's years of education	
WWI service	0.13
	(0.18)
Number of Observations	10
Controls	
Linear trend	x

Notes: 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 WWI 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 WWI. Each regression contains a time trend defined as year of birth. We calculate Huber-White standard error estimates.

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

The GI Bill and Homeownership

In addition to educational benefits, the GI Bill also guaranteed generous home and business loans that made it possible for approved lenders to provide no-down-payment mortgages to returning veterans. Between 1944 and 1952, the Veterans Administration (VA) guaranteed nearly 2.4 million home loans. Recent work by Yamashita (2008) and Fetter (2013) has suggested that these benefits had a significant impact on white veterans' rates of homeownership during the postwar period, although the advantage disappeared by 1980. This suggests that our assortative mating results might be driven by veterans' early access to housing rather than their higher education levels. 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. Specifically, we create a dummy variable 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 were homeowners.

The results of this exercise are shown in Table 5. Consistent with previous studies' evidence of fade-out effects, we find no evidence that GI benefit–eligible cohorts were more likely to own a home in 1970 than their ineligible counterparts. 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.[18] 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 estimates of the impact of WWII service on wives' schooling.[19] We have also estimated our regressions including homeownership rates calculated from the 1960 census because this is the census year for which both Yamishita and Fetter found evidence of homeownership differences across cohorts.[20] Including the 1960 control variable has no substantive impact on the estimated *%WWII* coefficients, either. Taken together, these results suggest that the estimates presented in Table 3 are not driven by the homeownership benefits that were associated with the GI Bill.

Cross-Cohort Differences in Sex Ratios

High rates of military service among our treatment cohorts also lead to lower male/female ratios. Of the roughly 16 million men who served in WWII, approximately 405,000 died.[21] Becker (1981) suggested that sex ratios 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. A few previous studies have investigated how changes in the sex ratio resulting from WWII affected marriage in Europe (Brainerd 2006; Kvasnicka and Bethmann 2013), but 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.[22]

Figure 2 plots the sex ratio by year and quarter of birth, showing a substantive difference in the ratio between the pre- and post-1927 cohorts. The figure is based on

[18]We obtain similar results when we use the other measures of wives' educational attainment that are included in Table 3. For the sake of brevity we do not include all of those measures in Table 5.

[19]Results are virtually identical if we restrict our definition of homeownership to include only heads of households.

[20]Unlike Yamishita and Fetter, we do not find evidence that GI 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 did not control for the effects of the Korean War at all. Fetter's analysis assumed 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.

[21]In contrast, Korean War participation rates were much lower (especially for our cohorts) and resulted in only 36,500 deaths.

[22]Bitler and Schmidt (2012) and Lafortune (2013) estimated the impact of sex ratios on assortative mating in the United States with respect to contexts other than WWII.

Table 5 Reduced-form estimates of the effect of WWII service on marital status and wife's educational attainment controlling for homeownership

	Birth Cohorts			
	1923–1929	1923–1930	1923–1932	1923–1938
	(1)	(2)	(3)	(4)
All Men				
Years of completed college				
WWII service	0.33	0.41	0.50	0.44
	(0.14)	(0.15)	(0.11)	(0.13)
Own home, 1970	1.00	0.70	0.03	–0.02
	(0.93)	(0.87)	(0.57)	(0.52)
Probability married				
WWII service	0.03	0.01	0.04	0.00
	(0.05)	(0.04)	(0.04)	(0.03)
Own home, 1970	0.05	0.05	0.13	0.22
	(0.14)	(0.14)	(0.12)	(0.10)
Probability separated or divorced				
WWII service	–0.02	0.00	–0.02	0.00
	(0.01)	(0.02)	(0.01)	(0.01)
Own home, 1970	0.10	0.08	0.04	–0.06
	(0.08)	(0.08)	(0.07)	(0.06)
F statistic	5.7	7.7	20.6	11.6
Married Men				
Husband's years of completed college				
WWII service	0.18	0.22	0.42	0.37
	(0.12)	(0.13)	(0.11)	(0.13)
Own home, 1970	0.84	0.64	0.01	–0.05
	(0.82)	(0.80)	(0.63)	(0.62)
Wife's years of schooling				
WWII service	0.70	0.66	0.65	0.44
	(0.20)	(0.21)	(0.17)	(0.16)
Own home, 1970	1.76	2.14	1.82	1.85
	(1.27)	(1.17)	(0.82)	(0.93)
F statistic	2.3	3.0	14.6	8.0
Number of Observations	28	32	40	64
Controls				
Linear trend	x	x	x	x
Korean and interwar period service × Trend				x
Trend^2				x
Korean and interwar period service × Trend^2				x

Notes: 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 WWII, regardless of their military service status in other periods. The variable *Korean and interwar period service* is the fraction of men who identified themselves as having participated in those conflicts but did not also serve in WWII. 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). We calculate Huber-White standard error estimates.

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

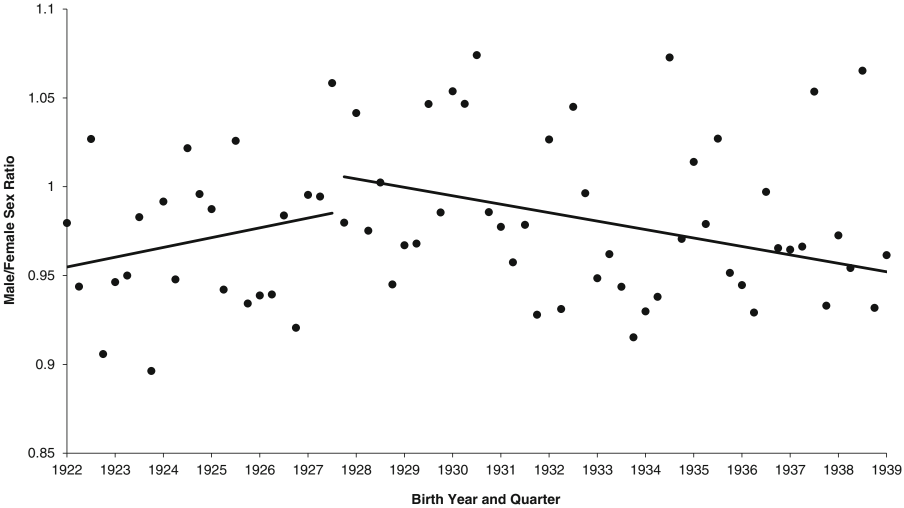

Fig. 2 Sex ratio (male/female) by birth cohort. Estimates are based on white men and white women born in the contiguous United States. We define the sex ratio as the number of men born in a year-quarter divided by the average number of women born in the previous and subsequent 8 quarters. *Source:* 1960 Integrated Public Use Microdata Series (IPUMS, 1 % sample)

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 made their marital decisions. Because 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 tried several alternative measures and obtained very similar, or smaller, results.[23]

Figure 2 shows that relative to cohorts born after 1927, cohorts born in the pre-1927 period experienced a male/female ratio that was 2.5 % lower. 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 6, 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.

Effects of WWII and the GI Bill on Women's Education

Another possibility is that our results reflect changes in women's own schooling levels that were induced by the war. It is unlikely that female military service or female responses to own GI benefits are driving our results because only about 3 % of women born between 1923 and 1938 served during WWII, but the absence of potential partners during the war years may have made investments in education more attractive. Such investments will threaten the interpretation of our estimates if women's educational attainment follows a discontinuous pattern similar to men's. Furthermore, using the 1960 census, Jaworski (2014) found that women coming of age during WWII had

[23]For example, we calculated the sex ratio using only men and women who belong to the same birth cohort.

Table 6 Reduced-form estimates of the effect of WWII service on marital status and wife's educational attainment controlling for the male/female sex ratio

	Birth Cohorts			
	1923–1929	1923–1930	1923–1932	1923–1938
	(1)	(2)	(3)	(4)
All Men				
Years of completed college				
WWII service	0.35	0.38	0.49	0.45
	(0.15)	(0.14)	(0.10)	(0.13)
Sex ratio	0.05	0.03	–0.02	–0.02
	(0.11)	(0.10)	(0.09)	(0.07)
Probability married				
WWII service	0.05	0.00	0.04	0.00
	(0.04)	(0.03)	(0.03)	(0.03)
Sex ratio	0.06	0.05	0.02	0.00
	(0.01)	(0.01)	(0.02)	(0.02)
Probability separated or divorced				
WWII service	–0.03	0.00	–0.02	–0.01
	(0.01)	(0.02)	(0.01)	(0.01)
Sex ratio	–0.02	–0.02	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
F statistic	5.5	7.6	26.4	11.7
Married Men				
Husband's years of completed college				
WWII service	0.19	0.20	0.41	0.37
	(0.12)	(0.12)	(0.09)	(0.13)
Sex ratio	0.04	0.03	–0.05	–0.03
	(0.10)	(0.10)	(0.09)	(0.08)
Wife's years of schooling:				
WWII service	0.69	0.63	0.50	0.46
	(0.23)	(0.22)	(0.18)	(0.17)
Sex ratio	–0.06	–0.18	–0.07	–0.08
	(0.19)	(0.17)	(0.14)	(0.10)
F statistic	2.4	2.8	19.2	8.4
Number of Observations	28	32	40	64
Controls				
Linear Trend	x	x	x	x
Korean and interwar period service × Trend				x
Trend^2				x
Korean and interwar period service × Trend^2				x

Notes: 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 WWII, regardless of their military service status in other periods. The variable *Korean and interwar period service* is the fraction of men who identified themselves as having participated in those conflicts but did not also serve in WWII. 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). We calculate Huber-White standard error estimates.

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

lower (not higher) levels of education, and also that by 1970, the differences across cohorts were no longer distinguishable from zero.

Nevertheless, we look for discontinuities in female schooling levels by matching our measure of male WWII participation by year and quarter of birth to cohorts of women born in the same quarter and year, and then estimating versions of Eqs. (1b)–(3b) in which the dependent variables are replaced with measures of women's educational attainment. The top panel of Table 7 displays the results of this exercise, revealing little evidence that male participation rates predict education levels among women in the

Table 7 Reduced-form estimates of the effect of male WWII service on women's education

	Birth Cohorts			
	1923–1929	1923–1930	1923–1932	1923–1938
	(1)	(2)	(3)	(4)
Male WWII Participation Matched to Female Cohorts (1970 census)				
All women				
Years of schooling	0.35	0.29	0.19	0.23
	(0.62)	(0.38)	(0.29)	(0.29)
High school graduate	0.02	0.08	0.12	–0.01
	(0.05)	(0.04)	(0.03)	(0.03)
Enrolled in college	–0.01	0.04	0.02	0.05
	(0.04)	(0.04)	(0.03)	(0.04)
College graduate	–0.03	–0.01	–0.06	0.03
	(0.07)	(0.04)	(0.03)	(0.03)
Male WWII Participation Matched to Female Cohorts Two Years Younger (1970 census)				
All women				
Years of schooling	0.98	1.65	0.01	0.15
	(1.04)	(0.76)	(0.37)	(0.28)
High school graduate	0.19	0.15	0.01	0.00
	(0.17)	(0.12)	(0.04)	(0.04)
Enrolled in college	0.04	0.06	–0.08	–0.02
	(0.15)	(0.11)	(0.04)	(0.05)
College graduate	0.03	0.08	–0.06	–0.02
	(0.08)	(0.06)	(0.03)	(0.02)
Number of Observations	28	32	40	64
Controls				
Linear trend	x	x	x	x
Korean and interwar period service × Trend				x
$Trend^2$				x
Korean and interwar period service × $Trend^2$				x

Notes: 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 WWII, regardless of their military service status in other periods. The variable *Korean and interwar period service* is the fraction of men who identified themselves as having participated in those conflicts but did not also serve in WWII. The time trend is defined as year of birth – 1929 + (quarter of birth / 4). We calculate Huber-White standard error estimates.

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

same cohort. Very few of the estimates are statistically different from zero, and the estimates that are significant are not consistently positive. The lower panel of Table 7 presents the results from the same exercise, only matching women to men who are two years older, to better approximate the "typical" age gap between husbands and wives. As in the top panel, most of the estimates are indistinguishable from zero. We conclude that cohorts of women for whom there were many absent men did not respond by increasing their own education levels.

Marital Sorting Versus Post-Marriage Investments

Our analyses suggest that the cohort effects we estimate are not likely to be driven by "competing events." We find no evidence that the impact of military service, changes in marital opportunities that resulted from cohort differences in sex ratios, or housing benefits provided through the GI Bill are associated with the patterns in our data. One interpretation of our results, therefore, 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 interpretation that is consistent with the evidence is that WWII veterans married the same women that they would have married in the absence of the war, but that because of the husbands' 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 panels a and b of Fig. 3 provide some evidence of changes in marital sorting by age that mimic the differences that we see in wives' educational attainment. If the GI Bill did not induce a change in marital sorting, then we would expect the average age gap between husbands and wives to remain more or less constant. Panel a plots the standard deviation of the age gap between husbands and their wives. The standard deviation captures the degree of heterogeneity in age sorting within cohorts, and we see that consistent with a change in age sorting, the standard deviation increases substantially for cohorts born between 1924 and 1930. This tells us that affected cohorts were matching with women from a wider range of birth years than was the norm for men who came of age both earlier and later.

Similarly, Panel b of Fig. 3 plots the average husband–wife age gap for male cohorts born between 1910 and 1940. This figure shows a distinct increase in the magnitude of the gap right around the 1927 cutoff, and documents that men who just missed eligibility for WWII benefits married women who were discontinuously younger than the women who paired up with their eligible counterparts. Table 8 in the appendix provides more detail, including the full distribution of wives' birth years for each male cohort in our sample. Note that although panels a and b both provide evidence of changes in marital sorting by age, the pattern in panel b is the opposite of what one would expect if pre-1927 cohorts simply "poached" women from male cohorts who just missed the cutoff. The changes in sorting induced by the war were clearly more complex. Nevertheless, the figures make clear that cohorts who were able to take advantage of GI benefits married different women than they would have in the absence of these historical events. Panel b also suggests that because the treatment group married relatively older women than they would have otherwise, the extent to which

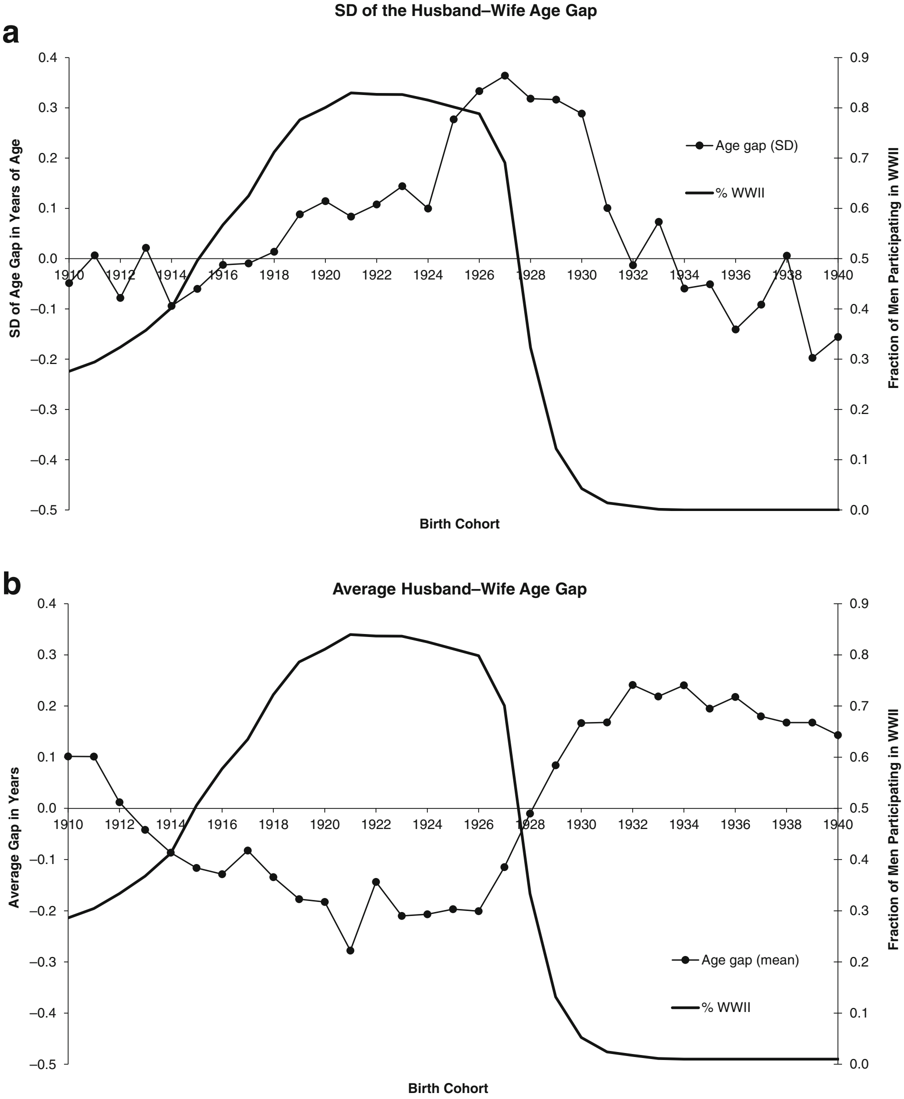

Fig. 3 Standard deviation (SD) of the husband–wife age gap (panel **a**) and average husband–wife age gap (panel **b**), by husband's birth cohort. Estimates are based on married white men born in the contiguous United States. In panel **a**, the age gap is calculated by taking the differences between husbands' and wives' ages within each year-of-birth and quarter-of-birth cell; we take the standard deviation of this variable, detrend it, and plot the residual. In panel **b**, the *age gap* is calculated by averaging the difference between husbands' and wives' ages within each year-of-birth and quarter-of-birth cell. We detrend this variable and plot the residual. *Source:* 1970 Integrated Public Use Microdata Series (IPUMS, three 1 % samples)

the treatment group's behavior reduced spousal quality among the control group may have been quite limited. The estimated coefficients in Table 3, therefore, may come quite close to capturing a partial equilibrium effect. Both figures clearly imply that our main results are more likely to be driven by changes in marital sorting than by changes in wives' educational investments that took place after marriage.

Conclusion

A number of previous studies have documented that WWII and the GI Bill had substantial effects on men's educational attainment, but the degree to which these historical events affected other social outcomes is not well understood. In this article, we exploit quasi-random variation created by the abrupt decline in WWII manpower requirements to investigate whether the impact of WWII and the GI Bill spilled over onto marital sorting. We find substantive evidence that it did: cohorts of men who were eligible for GI benefits married women who had approximately 0.4 more years of education than cohorts who just missed the eligibility cutoff. Their wives were also discontinuously older.

Although our estimation strategy does not allow us to separately identify the relative impacts of military service from the GI Bill, the most likely mechanism is that men's marital opportunities were changed by the additional education that they received. Our analyses indicate that the estimates are not likely driven by combat-related changes in the sex ratio or by housing benefits that were provided through the GI Bill. We find no evidence that they are driven by an independent effect of the GI Bill on female schooling levels. In addition, analyses of cohorts who came of age during WWI do not produce similar results—providing no evidence of discontinuities in education levels for WWI cohorts or their wives—and if our estimates of the effects of WWII and the GI Bill operated through the effects of military service, then previous generations of servicemen might exhibit similar patterns. Finally, the fact that affected cohorts of men married women who were both more educated *and relatively older* than men who just missed qualifying for GI benefits minimizes the possibility that our estimates are driven by changes in wives' schooling that took place after they were married. In short, other potential mechanisms do not appear to explain the observed patterns, leaving the changes in men's educational attainment induced by the GI Bill as the most likely explanation.

Our estimates are of substantial magnitude, but one should be careful about using them to make specific policy recommendations. One reason for this is that our analyses neither isolate the partial equilibrium effect of giving a random man access to college nor identify the general equilibrium effect of increasing everyone's access. In addition, women's education levels and labor market opportunities have changed markedly in the past 70 years, which hinders our ability to extrapolate our findings to the present day.

Nevertheless, our results underscore that individual investments in human capital may yield substantive marriage market returns, and they strongly imply that at least some of the assortative mating that we observe in society can be manipulated by policy. This would be nearly impossible to document using education policy levers in the United States today given that nearly all recent policies that increase access to education affect both men and women. Our ability to examine the impact of a policy regime that increased educational attainment for a defined group of men while holding the distribution of education among their potential spouses constant, allows us to document that education affects marital opportunities and that positive assortative mating is not merely a reflection of sorting on innate characteristics that are correlated with education. This adds to the mounting evidence that education has causal effects on individual well-being that go well beyond wage effects. The full range of these impacts must be understood to accurately weigh the costs and benefits of education policies.

Our study also highlights an often overlooked path by which policies that aim to improve particular adults' economic outcomes may ultimately influence intergenerational mobility: such policies are often thought to be associated with improvements in the next generation's well-being because they increase the resources that are available to targeted adults' children. Our findings suggest that they may also influence children indirectly by altering the composition of parents within the household. In the case of WWII and the GI Bill, the additional education and improved economic outcomes experienced by affected cohorts may have directly affected their offspring's socioeconomic success, but there may have also been an indirect effect operating through changes in the types of women that WWII veterans married. Maternal education is independently a strong predictor of children's outcomes. We look forward to investigating these mechanisms further in future research.

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Appendix

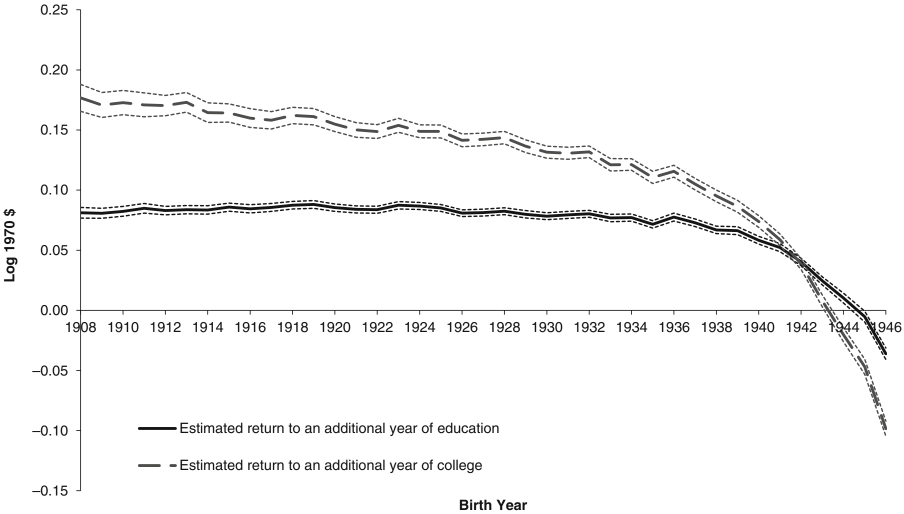

Fig. 4 Estimated returns to education. Calculations are based on white men born in the contiguous United States. Estimates are derived from a regression of the log wage on educational attainment, birth year-quarter fixed effects, and the interaction between birth year and educational attainment. *Source:* 1970 Integrated Public Use Microdata Series (IPUMS, three 1% samples)

Table 8 Distribution of married men across wife's birth year, by husband's birth year

	Husband's Birth Cohorts																
	1923	1924	1925	1926	1927	1928	1929	1930	1931	1932	1933	1934	1935	1936	1937	1938	1939
Wife's Birth Cohort																	
1916	0.005	0.004	0.002	0.002	0.001	0.001	0.001	0.001	0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
1917	0.008	0.005	0.003	0.004	0.002	0.002	0.002	0.001	0.001	0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.000
1918	0.009	0.007	0.006	0.004	0.003	0.003	0.002	0.001	0.001	0.001	0.001	0.000	0.000	0.000	0.000	0.000	0.000
1919	0.015	0.009	0.007	0.005	0.004	0.003	0.002	0.002	0.002	0.001	0.001	0.001	0.001	0.000	0.000	0.000	0.000
1920	0.022	0.015	0.012	0.008	0.006	0.004	0.003	0.003	0.001	0.001	0.001	0.001	0.000	0.000	0.000	0.000	0.000
1921	0.035	0.024	0.015	0.011	0.008	0.005	0.004	0.002	0.003	0.001	0.001	0.001	0.001	0.001	0.000	0.000	0.000
1922	0.061	0.036	0.023	0.015	0.010	0.007	0.006	0.004	0.003	0.003	0.002	0.001	0.001	0.001	0.000	0.000	0.000
1923	0.102	0.059	0.035	0.024	0.013	0.009	0.006	0.004	0.003	0.003	0.002	0.001	0.001	0.000	0.000	0.000	0.000
1924	0.122	0.105	0.059	0.034	0.022	0.014	0.009	0.007	0.005	0.003	0.002	0.002	0.001	0.001	0.001	0.001	0.001
1925	0.123	0.116	0.106	0.060	0.034	0.019	0.013	0.009	0.006	0.004	0.003	0.002	0.002	0.001	0.001	0.001	0.000
1926	0.111	0.124	0.121	0.105	0.061	0.034	0.020	0.011	0.007	0.006	0.004	0.003	0.002	0.002	0.001	0.000	0.001
1927	0.096	0.118	0.128	0.123	0.104	0.059	0.030	0.019	0.012	0.008	0.005	0.004	0.003	0.002	0.001	0.001	0.001
1928	0.072	0.095	0.113	0.125	0.120	0.101	0.056	0.027	0.018	0.010	0.007	0.005	0.003	0.002	0.002	0.001	0.001
1929	0.052	0.073	0.093	0.115	0.129	0.123	0.100	0.053	0.030	0.017	0.010	0.007	0.006	0.004	0.002	0.002	0.001
1930	0.038	0.052	0.072	0.095	0.117	0.132	0.126	0.104	0.054	0.029	0.018	0.011	0.008	0.006	0.004	0.003	0.002
1931	0.025	0.036	0.049	0.071	0.091	0.111	0.129	0.127	0.105	0.053	0.026	0.015	0.010	0.006	0.004	0.003	0.003
1932	0.020	0.028	0.037	0.050	0.074	0.097	0.112	0.135	0.129	0.108	0.052	0.026	0.015	0.011	0.006	0.006	0.003
1933	0.014	0.019	0.024	0.035	0.049	0.071	0.092	0.112	0.130	0.126	0.109	0.052	0.026	0.015	0.010	0.007	0.005
1934	0.011	0.014	0.018	0.026	0.036	0.054	0.074	0.099	0.120	0.135	0.134	0.108	0.053	0.026	0.013	0.009	0.007
1935	0.008	0.010	0.016	0.019	0.026	0.039	0.058	0.076	0.100	0.123	0.141	0.134	0.109	0.049	0.025	0.012	0.008
1936	0.006	0.007	0.011	0.014	0.018	0.027	0.040	0.055	0.074	0.098	0.123	0.145	0.141	0.112	0.050	0.023	0.013

Table 8 (continued)

	Husband's Birth Cohorts																
	1923	1924	1925	1926	1927	1928	1929	1930	1931	1932	1933	1934	1935	1936	1937	1938	1939
1937	0.005	0.007	0.007	0.010	0.015	0.018	0.030	0.040	0.054	0.074	0.097	0.126	0.142	0.140	0.114	0.051	0.022
1938	0.005	0.004	0.007	0.009	0.012	0.016	0.021	0.027	0.041	0.059	0.079	0.105	0.132	0.156	0.149	0.114	0.050
1939	0.003	0.005	0.005	0.007	0.010	0.011	0.016	0.020	0.029	0.042	0.056	0.077	0.102	0.130	0.155	0.149	0.112
1940	0.003	0.003	0.004	0.005	0.006	0.009	0.011	0.015	0.019	0.027	0.037	0.053	0.077	0.105	0.136	0.157	0.148
1941	0.002	0.003	0.003	0.004	0.005	0.007	0.009	0.012	0.015	0.018	0.026	0.035	0.052	0.074	0.103	0.140	0.171
1942	0.001	0.003	0.002	0.003	0.004	0.006	0.007	0.009	0.010	0.016	0.019	0.030	0.040	0.054	0.079	0.115	0.154
1943	0.002	0.002	0.002	0.003	0.003	0.004	0.005	0.006	0.008	0.010	0.015	0.018	0.026	0.037	0.053	0.078	0.114
1944	0.001	0.001	0.002	0.002	0.002	0.003	0.004	0.005	0.004	0.007	0.010	0.012	0.015	0.023	0.031	0.048	0.069
1945	0.001	0.001	0.001	0.001	0.002	0.001	0.003	0.003	0.004	0.004	0.006	0.008	0.011	0.014	0.020	0.029	0.040
1946	0.001	0.001	0.001	0.001	0.001	0.002	0.002	0.003	0.003	0.004	0.004	0.005	0.007	0.011	0.013	0.019	0.029

Note: The sample is composed of white married men.

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

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