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Does younger age at marriage affect divorce? Evidence from Johnson's Executive Order 11241

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Abstract

Before President Johnson's Executive Order 11241 in August 1965, married men received lower draft priority for military service. As the Vietnam War escalated in the summer of 1965, anecdotal evidence suggests draft-eligible men sought marriage to lower their likelihood of serving. This paper quantifies the effects of these Vietnam-era policies on marriage and finds that they significantly reduced the age at first marriage and altered the choice of spouse. However, younger marriages induced by the war were *less* likely to result in divorce 15 years later. Evidence also suggests that these younger marriages had little effect on long-term outcomes.

KEYWORDS

age at marriage, divorce, Vietnam War

JEL CLASSIFICATION

J12; J18; N32

1 | INTRODUCTION

Since 1960, US marriage and divorce patterns have changed dramatically. Historically, American men married at around age 25 and women at around age 22 (Figure 1). After falling during the baby boom (cohorts born from 1910 to 1935), age at first marriage has now risen to 26 for men and 24 for women—a historic high in the United States.[1]

This trend in marital delay corresponds to significant declines in divorce rates (Stevenson & Wolfers, 2007), leading many scholars to argue that the rising age of marriage is an important causal factor (Rotz, 2016). Economic models provide simple rationales for this claim. Looking longer for a mate may provide individuals with more opportunities to find a good match (Becker, 1991; Becker et al., 1977). Those who marry later may have larger social networks and a larger pool of potential mates (Glenn et al., 2010). A later age at marriage means individuals are more mature and self-aware, increasing the odds that they choose more suitable mates and that their marriages survive (Oppenheimer, 1988). To the extent that delaying marriage facilitates greater investments in both women's and men's human capital and careers (Bailey et al., 2012; Goldin & Katz, 2002; Hock, 2008), later marriage may reduce financial hardship and, thus, ameliorate an important source of marital stress (Dew et al., 2012).

Evidence on the causal link has been more limited. Although marriages formed at older ages are less likely to result in separation and divorce (Becker et al., 1977; Oppenheimer, 1988),[2] estimating the *causal* effect of marital delay on

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Abbreviations: CPS, Current Population Survey; NYT, The New York Times.
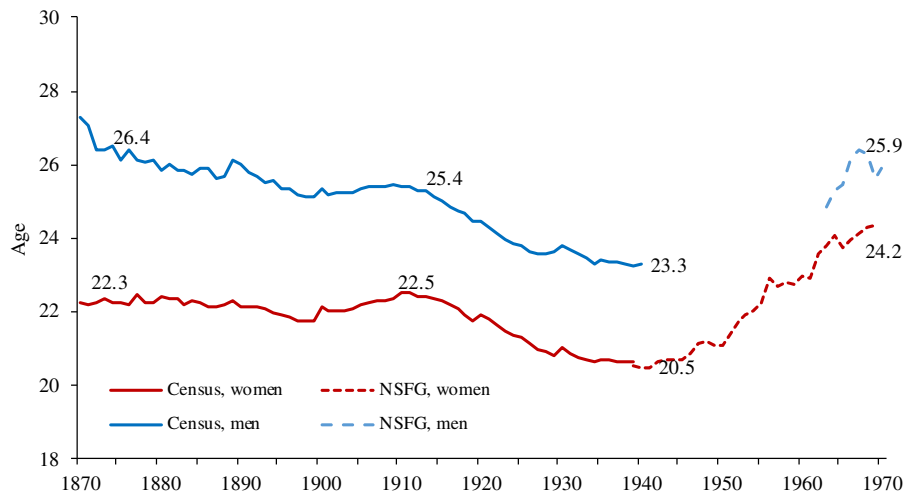
FIGURE 1   Mean age at first marriage, by birth cohort. *Notes:* The figure plots the mean age at first marriage (conditional on ever having married by age 39) against single year-of-birth cohort. NSFG and Current Population Survey (CPS) trends are based on 3-year cohort moving averages. *Sources:* Men and women are observed in the 1940–1980 decennial Census IPUMS samples (Ruggles et al., 2015); women are observed in 1979–1995 June CPS; 1982–2010 NSFG. Men are only sampled in the 2006–2010 NSFG (Smock et al., 2013)

marital stability is confounded by the correlation of marital timing with a host of observed (and likely unobserved) characteristics.[3] For this reason, empirical tests of the causal relationship between younger marriage and long-run outcomes have relied on natural experiments that exploit policies or environmental factors that induce some women to marry at younger ages. But these policies are rare in the United States, and most rely upon policy variation affecting marriages before the age of 17.[4] For instance, Dahl (2010) uses the increase in state age-of-consent laws (from 16 to 18) to show that early marriage is associated with a 28 percentage-point decrease in the probability of living in poverty later in life. Rotz (2016) uses these same marriage restrictions (in addition to two other empirical strategies) to show that delayed marriage reduced divorce rates. Similarly, Bharadwaj (2015) exploits a 1957 change in Mississippi marriage laws that increased the minimum legal age at marriage from 12 to 15 years for women (from 14 to 17 for men) to show that these laws increased girls' school enrollment and reduced subsequent birth rates.[5] Today, however, only 3.5% of women (who married by age 40) were married before the age of 17 (Bailey et al., 2014), potentially limiting the relevance of these findings for marital delay for individuals in their twenties. Twenty-year-olds may be more mature, and these marriages may be less likely to disrupt educational attainment.[6] The extent to which recent increases in age at marriage in the United States at *later* ages have affected marriage stability remains an open empirical question.

This paper studies this question using changes in the incentives to marry to obtain lower draft priority that occurred during the escalation of the Vietnam War. Widespread draft dodging through college enrollment, crime, and paternity is well documented (Bailey & Chyn, 2020a, 2020b; Card & Lemieux, 2001; Kutinova, 2009; Kuziemko, 2010); less well documented is that men also married early to receive lower priority draft status. As the war escalated in the summer of 1965, *The New York Times* (*NYT*) reported that officials had "been concerned by a noticeable increase in the number of marriages among 19-year-olds, some of which are apparently made to escape the draft" (Eder, 1965). In response, President Lyndon Johnson issued Executive Order 11241 late in the day on August 26, 1965, which abruptly eliminated the lower-priority status for married men but grandfathered in men married on or before that date. Responding to this powerful incentive, the *NYT* reported a deluge of "midnight marriages" that very night.

Using new microdata from marriage certificates and the 1980 Census, this paper quantifies the number of draft-induced marriages and the resulting shifts in marriage timing. The paper also documents the relationship of these earlier marriages with subsequent divorce. Our first research design using marriage certificates compares marriages (or the characteristics of those married) on August 26, 1965, to those in Augusts in other years—after accounting for day-of-the-week, month, and year fixed effects. Consistent with reports at the time, we find that a deluge of "midnight marriages" resulted from Johnson's Executive Order 11241. The number of daily marriages on August 26, 1965, was more than 190% higher than it would have been—an increase due almost entirely to marriages of draft-age men between the ages of 18 and 26, which increased by more than 350%. Men married on August 26, 1965, were 8 months younger, a change similar to around one-third of the *30-year* national increase in the age at first marriage among men (Figure 1). We also show that this policy-induced increase in marriage was not just a hastening among future would-be partners.

We find evidence consistent with changes in partners as well, as the average age difference for couples married on August 26, 1965, was 3 months smaller.

In the restricted, 20% long-form sample of the 1980 Census, we do not have access to exact day of marriage. For this reason, our second research design compares the durability of marriages formed in the third quarter of 1965 to those in the third quarter for two comparison periods: 1964–1965 and 1960–1970. Under the assumption that quarterly patterns in marriages would have been similar to comparison periods in the absence of draft-induced changes in the incentive to marry, this design recovers the reduced-form effects of younger marriages before August 26, 1965, on divorce. Consistent with draft-induced marriages being largely determined by idiosyncratic factors, we find that men induced to marry sooner were no more likely to attain a college degree than those in 1964, suggesting that idiosyncratic factors largely determined draft-induced marriages (i.e., happened to be in a relationship at the time with a woman who agreed before Executive Order 11241). Moreover, marrying to dodge the draft was an ineffective avoidance strategy, and we also find that men induced to marry sooner were no less likely to be Vietnam veterans.

Our most striking result is that younger marriages formed in late 1965—formed hastily and under duress—were *no more likely* to result in divorce 15 years later. Third-quarter marriages in 1965 were, in fact, *less likely* to result in divorce 15 years later. Men who married in the third quarter of 1965 appear similar in terms of long-term outcomes to men who married in the third quarter of 1964 or relative to men married in any third quarters of the 1960s. Similarly, women who married younger due to Executive Order 11241 appear similar in terms of family income and poverty rates in 1980. These women, however, appear to have fewer children and slightly higher college graduation rates.

Interpreting these results as the effect of early marriage requires that those induced to marry by the policy change and those in our comparison group are otherwise similar. That is, the policy change reshuffled partners and marital timing rather than changing who would marry. One violation of this requirement would be if a marriage may have helped men avoid serving in Vietnam. Because wartime stress and extended separations are very strong predictors of marital distress and divorce, hastily formed, younger marriages might be *more* likely to survive if they were less likely to experience these separations and stress. Another violation could occur if late-1965 grooms disproportionately desired avoiding military service. If these men pursued additional draft-dodging strategies such as enrolling in college, their actions may have independently reduced the likelihood of later divorce. Our tests of both hypotheses, however, show that men marrying in the third quarter of 1965 were neither less likely to serve in Vietnam nor more likely to enroll in college relative to third-quarter marriages in 1964. For our analysis to falsely reject the literature's best estimates of early marriage on the likelihood of divorce, *selection on unobserved characteristics* would need to be *1.7–5.3 times more important* than selection on observables using Oster's (2017) rule-of-thumb. In short, unobserved characteristics that are known *correlates* of education or military service (patriotism, risk aversion, etc.) are unlikely to drive our results.

We speculate that the difference between our findings and those in the existing literature result from heterogeneous effects of early marriage by age at first marriage. For example, studies of the effect of marital timing using age-of-consent laws capture changes in marriages at *very* early ages, well before most men and women had completed their education. Marrying to dodge the draft, however, tended to happen at later ages and appeared to have little adverse effect on the likelihood of marriage survival or women's outcomes.

2 | AN OVERVIEW OF DRAFT POLICY AND DRAFT DODGING DURING THE VIETNAM WAR

In the early 1960s, US men ages 18–26 were eligible for induction into the US military. The US Selective Service classified all registrants as available for civilian or military service, deferred, or ineligible for service.[7] For example, men who enrolled in college could receive a II-S deferment, and men who could prove a "bona fide" relationship with their children could receive a III-A deferment. Men would not be drafted so long as they maintained deferred status.

The US Selective Service also classified available individuals by priority status (see Appendix Table A1 for the priority ordering of classifications as of June 30, 1965). Although all men classified I-A were considered available for military service, local draft boards were instructed to prioritize within the I-A class beginning with (as highest priority) delinquents, volunteers, single men, and married men; the oldest in each category were to be selected first and, within each local draft board jurisdiction, each category was exhausted before any men from the next category were drafted.[8] In the summer of 1965, two important policy shifts significantly changed the likelihood a man would be drafted.

The first was the significant escalation in the scale of the Vietnam War. President Johnson supplemented the 23,000 "military advisers" already on the ground by 1965 with the first brigade of combat troops in March and rapidly

increased ground-force levels throughout 1965 (VanDeMark, 1991). Figure 2 shows that the escalation of the war resulted in sharp increases in inductions, from an average of around 10,000 per month before June 1965 to around 30,000 per month between June 1965 and June 1967. Increases in inductions increased the risk of being drafted for those with higher-priority classification, which affected many behaviors. Appendix Figure A1 shows that monthly marriage rates among 18- and 19-year-old men (panel A) increased sharply in the second half of 1965, and in August in particular. Survey evidence supports the claim that draft-avoidance was an important driver of these trends. One survey of 1586 men in Indiana, Michigan, and Washington, D.C. found that one out of every eight husbands reported marrying early to avoid the draft (Baskir & Strauss, 1978, p. 33).

The second major policy change occurred when President Johnson issued Executive Order 11241 at 5 PM Eastern Standard Time on August 26, 1965, eliminating lower draft priority for men married *after* midnight on the same day. Going forward, men married after August 26, 1965, received the same draft prioritization as single men among I-A classified men.

Urban legend and newspaper articles maintain that this Executive Order resulted in a deluge of midnight marriages. Even before the policy change, anticipation of the announcement may have exacerbated this midnight rush: according to a news report one week prior, Assistant Secretary of Defense for Manpower Norman S. Paul indicated that the policy was under evaluation due to increased draft calls (Eder, 1965). The day after the new policy went into effect, the *NYT* reported that hundreds flocked from California to Las Vegas and Carson City on August 26 to be married before the midnight deadline. In the two locations covered by *NYT* reporters, the Justice of the Peace conducted 171 marriage ceremonies—112 of them between 10 PM and midnight (United Press International, 1965). One newlywed was quoted: "Sure, I'm not ready to get married, but I'm not going to fight in some dirty jungle."

Although some secondary sources claim that lower priority for men married before August 27, 1965, was short-lived (Hanson, 2013; McCombs, 1991; Seelye, 2004),[9] Tatum and Tuchinsky's (1969) *Guide to the Draft*—the era's bible of draft dodging—noted that the official policy maintaining lower priority for men married before this date persisted. (We have also confirmed this in Selective Service rules.) As the war escalated, however, many local draft boards were forced to call even low-priority individuals. The National Advisory Commission on Selective Service (1967) reported that between 45 and 90% of boards surveyed in Alabama, Maryland, Minnesota, Tennessee, Utah, and Washington had drafted low-priority married men between January and May 1966. Out of the seven states in the survey, only Connecticut had not begun drafting men who married before August 26, 1965.

3 | QUANTIFYING THE EFFECTS OF EXECUTIVE ORDER 11241 ON MARRIAGE RATES, MARRIAGE TIMING, AND SPOUSE CHOICE

We first document the magnitude of the response to Executive Order 11241. We use roughly 2 million newly compiled individual marriage records from state archives and genealogical sites. The benefit of these microdata is that they
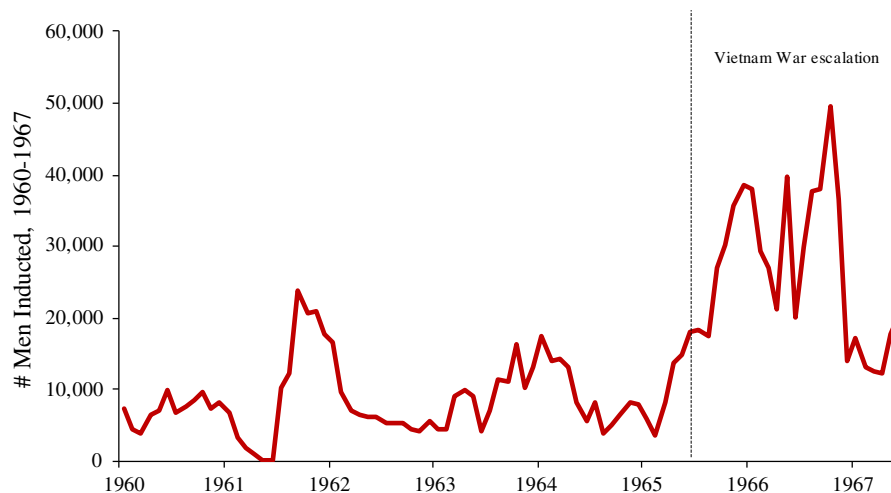

FIGURE 2 Monthly inductions, 1960–1967. *Sources:* Hand-entered monthly induction data from annual and semi-annual Selective Service reports (Selective Service System, 1960–1967). These figures exclude US territories

include the exact day of marriage and the ages of the bride and groom. A disadvantage is that they are not available for all states and they do not comprise the universe of marriages, even among included states.

Table 1 summarizes this new data resource.[10] Column 1 presents results for records from 1960 to 1970 for men ages 16–54. Column 2 limits the sample to men of draft-eligible ages 18–26. For comparison, column 3 presents estimates from the 1980 Census, including all lower 48 states. Although the Census is a much smaller sample, the characteristics of the records are extremely similar.[11] The quarter at marriage and the age difference between the bride and groom appear almost identical in both samples, which suggests that the marriage microdata are broadly representative of the population in those 16 states in our period of interest. Finally, the marriage microdata appear very similar to the population of all of the lower 48 states (compare columns 3 and 4). In the 16 states represented in the marriage microdata, the ages at marriage for men and women are negligibly different, as are the quarter of marriage, year of marriage, racial composition, and share living in poverty. Meanwhile, divorce rates are slightly lower and college graduation rates are slightly higher in all states compared to the 16 marriage microdata states. Finally, the characteristics of marriages in 1964 and 1965 (column 5) are also very similar to the marriage microdata (column 2).

Because we later examine the effects of early marriages on women (who were at very low risk of dying in Vietnam), and because this change affected many women who were outside the 18–26-year-old age range, columns 6 and 7 present the characteristics of these broader samples: women born in any of the lower 48 states who married at ages 16–34 between 1960 and 1970 (column 6) and the narrower group of women married between 1964 and 1965 (column 7). Both samples of women are slightly older than the brides of men in columns 2–5, foreshadowing the fact that some of these women married men outside the draft-eligible ages (and, therefore, were not captured in columns 2–5). Overall, these

TABLE 1 Summary statistics for marriage certificate microdata and 1980 Census samples

	(1) Marriage microdata	(2) Marriage microdata	(3) 1980 Census	(4) 1980 Census	(5) 1980 Census	(6) 1980 Census	(7) 1980 Census
Individual sample	Men 16–54	Men 18–26	Men 18–26	Men 18–26	Men 18–26	Women 16–34	Women 16–34
Marriage year sample	1960–1970	1960–1970	1960–1970	1960–1970	1964–1965	1960–1970	1964–1965
Total marriages	1,922,288	1,183,507	297,073	646,087	113,501	807,106	142,969
States	16	16	16	48	48	48	48
Age at marriage							
Man[a]	27.1	21.7	21.9	22.1	22.0	22.9	23.0
Woman[a]	24.5	20.2	20.0	20.1	20.1	20.8	20.8
Quarter of marriage	2.58	2.57	2.57	2.57	2.55	2.58	2.56
Year at marriage	1965.27	1965.3	1965.5	1965.5	1964.5	1965.4	1964.5
Share white			91.5	91.7	91.8	89.3	89.3
Share divorced or remarried[b]			28.3	27.2	27.9	31.2	32.1
Share college graduate[b]			27.3	29.2	27.5	19.2	18.2
Share living in poverty[b]			5.14	5.08	5.27	9.33	9.66
Family income (median)[b]			23,085	23,510	24,005	22,005	22,110
Children born[b]						2.3	2.4

Notes: The table numbers represent the mean of the variable (i.e., age at marriage, month at marriage, or year at marriage) or the share of the total records with a specific characteristic. All samples are restricted to marriages between 1960 and 1970 that have complete information on marriage date and the age of both the bride and the groom. Census results are from IPUMS data, which were nearly identical to summary results from the 20% restricted Census sample and included here in order to disclose as few restricted results as possible. The marriage microdata sample (columns 1 and 2) contains information from marriages in 16 states. Column 1 contains information on marriages between men ages 16–54 (inclusive) and women ages 14–54 (inclusive). Our main sample (column 2) excludes marriages to men over age 26 to focus on draft-age men. The 1980 Census samples in columns 3, 4, and 5 include men born in all of the lower 48 states whose age at first marriage is between 18 and 26, and the sample in columns 6 and 7 include women born in all of the lower 48 states whose age at first marriage is between 16 and 34. The 1980 Census samples in columns 5 and 7 additionally restrict the samples to individuals married in 1964 and 1965. All Census samples exclude individuals born and married in the same quarter because we cannot calculate age at marriage for these individuals.
[a]The age at marriage for men (women) in the microdata is the age on the marriage certificate. The age at marriage in the Census is computed for men and their wives at the time of the Census, or vice versa if the sample is of women.
[b]These characteristics are not contained in the marriage microdata; children born is not asked of men.

summary statistics highlight the similarities of these samples across a broad range of characteristics and suggest the representativeness of the marriage microdata for the purposes of this analysis.

3.1 | How large was the "midnight marriage rush" of August 26, 1965?

Using the marriage microdata, we first quantify the magnitude of the response to Executive Order 11241. Because August 26, 1965, was a Thursday and marriages are less likely to occur on weekdays, Figure 3 plots the number of marriages to 18–26-year-old men on Thursdays between 1960 and 1970. The volume of marriages on August 26, 1965, is a clear outlier relative to any other Thursday in the 1960s.

We formalize this test by aggregating the data into state-date cells (as noted in subscripts st) and running the following regression, first for all marriages to men ages 16–54 and then separately by age group:

$$\log(M_{st}) = \beta EO_t + f_{sm(t)} + g_{sy(t)} + h_{sd(t)} + \varepsilon_{st}. \tag{1}$$

The dependent variable is the log of the number of marriages (M) in state s occurring on date t, adding one to state-day cells with zero marriages.[12] The regressions account for potentially confounding factors by comparing the log number of marriages on August 26, 1965, to the average number in a given state-month ($f_{s, m(t)}$), state-year ($g_{s, y(t)}$), and state-day-of-the-week ($h_{s, d(t)}$), with $m = 1, 2, ..., 12$; $y = 1960, 1961, ..., 1970$ or 1964 and 1965; $d = 1, 2, ..., 7$ (for Monday, Tuesday, ..., Sunday).[13] The log-point divergence in the number of marriages taking place on August 26, 1965 from these averages is captured in the indicator variable on the date of Executive Order 11241 ($EO = 1(t = \text{August 26, 1965})$). All specifications report standard errors that account for potential autocorrelation within states across time (Arellano, 1987; Bertrand et al., 2004).

The regression results confirm the urban legend of the midnight marriage deluge. After accounting for a set of year, month, and day-of-the-week fixed effects and their interaction with state-level fixed effects, marriage rates across men of all ages (16–54) were an extraordinary 194% higher on August 26, 1965 (1.08 log points, Table 2A, column 1). This increase was concentrated among young men most susceptible to the draft. Marriages among men ages 18–26 were 356% higher (1.52 log points, column 3). But, the marriage rush spilled over to slightly younger and older men as well. Marriages among men ages 16–17 were 58% higher (0.46 log points, column 2), and among men ages 27–34, marriages were 25% higher (0.22 log points, column 4). These spillovers likely reflected measures by 16–17 and 27–34-year-olds (with extended age liability, see Appendix Table A1) to minimize their draft risk, but also—as Table 3 will show—the substantial increase in marriage market pressure from their prospective brides getting proposals from draft-age men. If anything, older men (ages 35–54) were less likely to marry (column 5), likely reflecting capacity constraints together
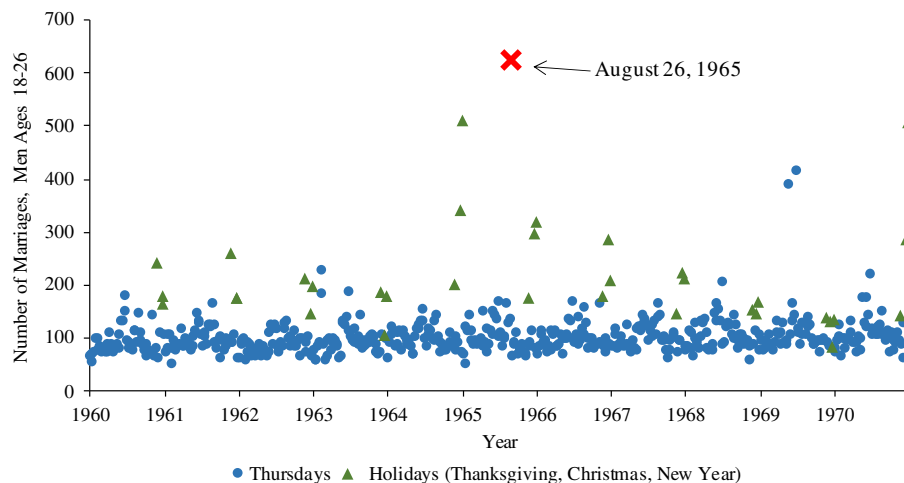

FIGURE 3 Daily number of marriages on Thursdays to men ages 18–26. *Notes:* The figure plots the number of marriages to men ages 18–26 on Thursdays (column 2 in Table 2). *Source:* 1960–1970 marriage microdata. See Table 1 notes for more information

with the overwhelming rise in demand for marriages on August 26 by younger men. Under the assumption that the 1964 and 1965 data provide a better counterfactual than using the data for all marriages over the 1960s, panel B re-estimates the same model on marriages in these 2 years. The estimates in all cases are statistically indistinguishable from the longer sample of years and qualitatively very similar.

Figure 4 further disaggregates the effects of the August 26 announcement into individual ages (plotted on the x-axis).[14] The estimates show large increases on August 26 primarily among draft-age men (19–25), with estimates ranging from 85% (0.62 log points) for 25-year-olds to 688% (2.06 log points) for 19-year-olds. These estimates are consistently elevated for men younger than 24, who were less likely to be married and may have been facing the end of their college deferments. The higher spike for 19-year-olds likely reflects the fact that they were experiencing their first year of high draft priority. (18-year-olds were the last group to be called, after 26-year-olds.) To test for anticipation effects, we estimate a placebo specification that replaces the dummy variable for August 26 (EO_t) with a dummy variable for days including August 19–25—the entire preceding week. Consistent with the historical accounts, Figure 4 indicates little anticipation effect. The estimates for the week before August 26 are much smaller in magnitude and in no case individually statistically significant.

TABLE 2 The effect of Executive Order 11241 on men's daily and quarterly marriage rates

	(1) Ages 16–54	(2) Ages 16–17	(3) Ages 18–26	(4) Ages 27–34	(5) Ages 35–54
A. Log daily marriages, 1960–1970					
1(August 26, 1965)	1.080	0.456	1.518	0.224	−0.362
	[0.0342]	[0.115]	[0.0942]	[0.0420]	[0.0346]
Number of men	1,922,288	25,908	1,183,057	353,530	359,793
B. Log daily marriages, 1964–1965					
1(August 26, 1965)	1.059	0.472	1.509	0.215	−0.406
	[0.0342]	[0.0979]	[0.0885]	[0.0416]	[0.0338]
Number of men	352,246	4,902	216,147	63,587	67,610
C. Log quarterly marriages, 1960–1970					
Third quarter 1965	0.002	−0.0005	0.014	0.0039	−0.0308
	[0.005]	[0.0704]	[0.0065]	[0.0059]	[0.0127]
Number of men	1,922,288	25,908	1,183,057	353,530	359,793
D. Log quarterly marriages, 1964–1965					
Third quarter 1965	0.0356	−0.0277	0.062	0.0172	−0.0292
	[0.0037]	[0.1520]	[0.0046]	[0.0158]	[0.0095]
Number of men	352,246	4,902	216,147	63,587	67,610
E. Log quarterly first marriages (1980 Census, all states), 1960–1970					
Third quarter 1965	0.0264	−0.138	0.037	0.002	−0.082
	[0.015]	[0.124]	[0.016]	[0.046]	[0.099]
Number of men[a]	6,292,800	176,600	5,148,000	718,200	250,000
F. Log quarterly first marriages (1980 Census, all states), 1964–1965					
Third quarter 1965	0.0564	−0.31	0.077	−0.02	−0.135
	[0.0294]	[0.186]	[0.029]	[0.111]	[0.148]
Number of men[a]	1,106,700	31,100	902,100	126,800	46,700

Notes: Panels A and B use the specification described in Equation (1), and panels C through F use the specification described in Equation (2). Columns include marriages to men ages 16–54 (column 1) or the indicated age groups (columns 2–5) for the sample described in Table 1. All columns include state, year, month, day-of-week, state-by-year, state-by-month, and state-by-day-of-week fixed effects. Standard errors clustered at the state level are reported in the second row in brackets. See Table 1 notes for more information on samples and sources. Sample: panels A–D: marriage microdata, all marriages from 1960 to 1970 (panels A and C) or 1964 to 1965 (panels B and D), weighted by the number of marriages in the state-age cell; panels E and F: 1980 Census, restricted data, *first* marriages between 1960 and 1970 (panel E) or 1964 and 1965 (panel F).

[a]Restricted Census data disclosure requires the number of observations to be rounded to the nearest hundred.

TABLE 3 The effect of Executive Order 11241 on age at marriage and spouse choice, marriage microdata

	(1) Ages 18–26	(2)	(3)	(4)	(5) Ages 16–17	(6)	(7) Ages 27–34	(8)
A. Men's age at marriage								
Mean dependent variable	21.3		21.3		16.8	16.9	29.7	29.7
1(Married August 26, 1965)	−0.672	−0.672	−0.625	−0.626	0.0449	0.0173	0.125	0.193
	[0.018]	[0.017]	[0.026]	[0.026]	[<0.001]	[0.0061]	[<0.001]	[0.0103]
B. Women's age at marriage (for indicated sample of men in panel A)								
Mean dependent variable	19.4		19.4		17.0	16.8	24.7	24.8
1(Married August 26, 1965)	−0.437	−0.434	−0.390	−0.388	−0.421	−0.476	−1.547	−1.416
	[0.072]	[0.075]	[0.062]	[0.064]	[<0.001]	[0.0051]	[<0.001]	[0.0196]
C. Age difference within couple (for indicated sample of men in panel A)								
Mean dependent variable	1.85		1.88		−0.124	0.007	5.04	4.98
1(Married August 26, 1965)	−0.236	−0.238	−0.235	−0.238	0.466	0.493	1.672	1.609
	[0.066]	[0.068]	[0.067]	[0.071]	[<0.001]	[0.0108]	[<0.001]	[0.0295]
Sample years	1960–70	1960–70	1964-65	1964-65	1960-70	1964-65	1960-70	1964-65
Number of marriages	1,175,725	1,175,725	214,786	214,786	25,892	4,898	316,759	56,695
Covariates	S, Y, M, D	S, Y, M, D, S × Y, S × M, S × D	S, Y, M, D	S, Y, M, D, S × Y, S × M, S × D	S, Y, M, D, S × Y, S × M, S × D	S, Y, M, D	S, Y, M, D, S × Y, S × M, S × D	S, Y, M, D

Notes: Each panel uses the specification in Equation (1) to estimate the deviation in age of the groom (panel A), bride (panel B for women ages 14 to 34 married to the sample of grooms in panel A) or the age difference between the groom and the bride (panel C) from the model-based counterfactual. Columns 1–4 include marriages to men ages 18–26. Columns 5 and 6 include marriages to men ages 16–17, and columns 7 and 8 include marriages to men ages 27–34. All columns include state (S), year (Y), month (M), and day-of-week (D) fixed effects. In addition, we include state-by-year (S × Y), state-by-month (S × M), and state-by-day-of-week (S × D) fixed effects in columns 2, 4, 5, and 7. Standard errors clustered at the state level are reported in the second row in brackets. Regressions are weighted by the relevant number of marriages. Sample: Marriage microdata observations on *all* marriages from 1960 to 1970 in columns 1, 2, 5, and 7 and only marriages from 1964 to 1965 in columns 3, 4, 6, and 8. See Table 1 notes for more information on samples and sources.

One limitation of Census data, which we subsequently use to investigate the long-term effects of the midnight marriages, is that they only contain information on marriage quarter and age at *first* marriage—not information on the exact *date* of marriage. Although we find little evidence of immediate anticipation effects in Figure 4, Census specifications that aggregate by quarter could include other confounders (for instance, factors that we cannot account for with detailed fixed effects due to the aggregation of the data). To investigate this concern directly, we aggregate the microdata into quarterly marriage counts by state, and we replace month and day-of-the-week fixed effects in Equation (1) with state-by-quarter and state-by-year fixed effects as follows:

$$\log(M_{sqy}) = \tilde{\beta}EO_{qy} + f_{sq} + g_{sy} + \varepsilon_{sqy}. \tag{2}$$

The dependent variable is the log of the number of marriages (M) in state (s), occurring in each quarter (q) and year ($y = 1960, 1961, ..., 1970$ or 1964 and 1965), and alternative strategies to account for cells with zero marriages.[15] $\tilde{\beta}$ captures the log-point divergence from the model-based counterfactual in the number of marriages taking place in the quarter of Executive Order 11241, where EO_{qy} = 1(third quarter 1965). Even appropriately scaled, this specification may yield quantitatively different results from Equation (1) due to the failure to account for month and day-of-the week fixed effects and aggregation bias.

Despite these potential limitations to comparability, Table 2 suggests that the aggregated marriage microdata recover very similar estimates for 1960–1970 period (panel C), but that the escalation of the Vietnam War during the third quarter also increased demand for marriages in the weeks prior to Executive Order 11241. If the change in marriages was confined to August 26, 1965, then we would expect that the 356% increase (1.52 log points) should be scaled by 0.005, resulting in a 1.8% change quarterly.[16] In comparison, column 3 of panel C shows a slightly smaller 1.4% change, and panel D shows a larger 6.2% change in the aggregated models. This increase likely reflects the increased likelihood of marrying in July and August leading up to the policy change, net of reductions in marriages that would have happened later in the third quarter of 1965.

Although using 1964 and 1965 matters little for the daily estimates in panels A and B, this refinement increases the magnitude of the estimate by four times in the quarterly estimation strategy. This increase reflects that using 1964 provides a different counterfactual for 1965 (relative to other years in the decade, which may also be inflated due to the escalation of the war) and that the quarterly estimation strategy likely also picks up changes in marriage due to the increasing drafts and deployments prior to Johnson's Executive Order. Panels E and F show that the magnitudes are similar in the 1980 Census, which use all states. The upshot is that using Census information on quarterly marriages allows us to measure the *combination* of policy-induced changes in marriages leading up to and on August 26, 1965, as well as the downstream effects of the policy through the resulting tightening in the marriage market in the subsequent months.

In summary, this evidence implies that the additional August 26 marriages were not simply the result of already engaged couples moving up their weddings by a few months or weeks. The next section examines directly how the rush to get married on August 26 led to substantial changes in marriage timing.

3.2 | How did Executive Order 11241 change age at marriage and spouse choice?

Whether the earlier-aged marriages induced by the Vietnam War had long-run implications depends on how much they altered marriage timing as well as the choice of spouse, which we examine using the age at marriage and the age difference to one's spouse. Table 3 shows that Executive Order 11241 induced significantly *younger* marriages among both men and women. Using the dependent variable from Equation (1) with the age of the groom, Table 3A shows that the average age of 18–26-year-old men (draft-eligible ages) married on August 26 fell significantly.[17] Using the 1960–1970 sample, men married on August 26, 1965, were, on average, 8 months younger (-0.672×12, column 1) than predicted by the month-state-year counterfactuals. This estimate from the model including state, year, month, and day-of-the-week fixed effects (column 1) is, for all practical purposes, identical to the estimate from a considerably more demanding specification that includes state-by-year, state-by-month, and state-by-day-of-the-week fixed effects (column 2). Additionally, the estimates are robust to limiting to the sample years to 1964 and 1965 only (columns 3 and 4). The motivation for this narrower comparison sample is that the broader 1960–1970 sample may misstate the counterfactual age at marriage because age at first marriage was changing rapidly in the 1960s. Restricting to marriage years 1964–1965 has a very small effect on the estimates, changing the magnitude of the delay to 7.5 months (or -0.625×12,

column 4) rather than 8 months. But not all marriages on August 26, 1965, were induced by Executive Order 11241. Table 2 suggests that only 78% of the marriages to men ages 18–26 resulted from the policy change.[18] To recover the age changes among those who participated in the midnight marriage rush, we scale the coefficient to obtain a reduction of just over 10 months ($-0.672/0.78 \times 12$).

Table 3B repeats this analysis for the age of the bride (married to the sample of men indicated in panel A). If the earlier marriages among draft-aged men were simply taking place earlier with the same spouses (even if 7.5–8 months earlier), one would expect the changes in the ages of women married to the same sample of men to change by a similar magnitude. Estimates in columns 1–4, however, suggest otherwise. In contrast to the 7.5–8-month reduction in age at marriage for men, the average ages of women married on August 26, 1965, fell by only 4.6–5.2 months (-0.388 to -0.437×12, columns 2 and 4). Rescaling by the 78%, this implies a reduction of around 6.0–6.7 months for those participating in the midnight marriage rush. Consistent with large shifts in spousal matching, Table 3C shows that the change in the age difference between grooms and brides married on August 26 fell by a statistically significant 2.9 months (-0.236×12, column 1), or 3.6 months rescaled for the midnight marriage rush participants. Neither adding in a more demanding set of covariates (including state-by-year, state-by-month, and state-by-day-of-the-week fixed effects, column 2) nor limiting the sample years to 1964 and 1965 (columns 3 and 4) appreciably alters the magnitudes of these estimates.

This marriage rush among draft-age men also appears to have affected marriage age among older and younger men and their spouses. The average age for men ages 16–17 and 27–34 (Table 3A, columns 5–8) married on August 26 increased slightly, whereas the average age of women married to these men decreased (Table 3B, columns 5–8). The pattern of these changes is consistent with the marriage rush pressuring older and younger men to marry women in the same marriage pool. In contrast to the decreases in the age gap among draft-age men, changes in the marriages of older and younger grooms led to substantial *increases* in the age gap for non-draft-age men married on August 26. The age difference between 16- and 17-year-old men and their spouses married on August 26 grew by 6 months. For the oldest group of men, the age difference with their partners *increased* by an enormous 1.6 years, or 19 months. Because significantly fewer 16- and 17-year-olds were induced to marry by Executive Order 11241, these effects imply substantially larger changes in age among those 16–17-year-old men participating in the midnight marriage rush. In particular, Table 3 implies that only 36.6% of marriages to 16–17-year-old men and 20.0% of marriages to 27–34-year-old men on August 26 were midnight marriages. The average age of women married to 16–17-year-old men who married because of Executive Order 11241 fell by 13.8 months (or $-0.421/0.366$). Similarly, the average age of women married to 27–34-year-old men because of Executive Order 11241 fell by *7.8 years* (or $-1.55/0.20$). These results are nearly identical when we restrict to the 1964–1965 comparison, as shown in columns 6 and 8.

In short, the escalation of the Vietnam War and Executive Order 11241 had significant effects on marriage decisions of draft-aged men (as well as older men competing for the same spouses in the marriage market). The shift in average age at marriage that occurred among draft-aged men induced to marry on August 26 is approximately one-third the size ($-0.86/2.6$) of the *30-year*, national increase in the age at first marriage among men for the birth cohorts born between 1940 and 1970 shown in Figure 1. Executive Order 11241 had dramatic effects on spouse choice as well. The following sections use the 1980 Census to examine how earlier marriages to different partners affected marital stability and economic outcomes over the subsequent 15 years.

4 | MARITAL STABILITY AND LONG-RUN ECONOMIC OUTCOMES FOR DRAFT-INDUCED MARRIAGES

If, all else equal, younger marriages are more likely to fail, then one would expect marriages begun in the third quarter of 1965 to have been disproportionately likely to fail. Moreover, if younger age at marriage tended to make women economically worse off (limiting their education or work experience, for instance), then women marrying in the third quarter of 1965 should be disproportionately likely to experience worse economic outcomes. We test both of these hypotheses using the restricted, 20% long-form 1980 Census.

4.1 | Early marriage and marriage stability

The 1980 Census asks all men and women if they have ever been divorced or remarried. This allows us to construct a measure of whether each ever-married individual was ever divorced by 1980—almost 15 years after the midnight

marriage rush. We estimate the following specification, which differs only from Equation (2) in that we use individual-level controls:

$$Y_{isqy} = \theta EO_{qy} + f_{sq} + g_{sy} + X_i'\gamma + \varepsilon_{isqy}. \tag{3}$$

The dependent variable is our outcome variable of interest for an individual (i) born in state s and married in a particular quarter (q) and year ($y = 1960, 1961, ..., 1970$ or 1964 and 1965). We include state-marriage quarter and state-marriage year fixed effects, as well as a set of individual-level controls, which include state-birth year and quarter (with fixed effects for being born in 1920Q1, 1920Q2, ..., 1955Q4) and state-race fixed effects. Individual control variables do not affect the results but do improve precision. The divergence from the predicted outcomes for those who married in the third quarter of 1965 ($EO_{qy} = 1$(third quarter 1965)) is captured by θ.

Table 4A presents the results. Younger marriages initiated in the third quarter of 1965 were less likely to result in divorce for men in the 1960–1970 and our 1964–1965 samples. In fact, men married in the third quarter of 1965 are 1.3 percentage points *less* likely to divorce (column 4), and the point estimate is statistically significant at less than the 1% level. Including a limited set of fixed effects (column 3) or estimating the more demanding specification (column 4) yields virtually identical results. Table 4B repeats this analysis for women, who were less likely to die or serve in the Vietnam War and, therefore, are less likely to be differentially represented in the 1980 Census. For women ages 16–34 at the time of marriage, the results appear 15–50% larger than for men. The third-quarter marriages were 1 percentage point *less likely* to result in divorce in the 1960–1970 sample and 1.5 percentage points less likely in the 1964–1965 sample (column 4). In all specifications, our 95% confidence interval can rule out that the substantially earlier marriages increased 15-year divorce rates.

Given the small share of marriages in the third quarter of 1965 induced by the Executive Order, a natural question is whether these estimates are significantly different from the local average treatment effects (i.e., treatment effects on the treated) for *very* young marriages in the literature. That is, is the causal effect of marrying earlier due to the Vietnam War (around ages 18–21) different than the causal effect of marrying under age 18? One of the best comparisons is between our Table 4 estimates and Rotz (2016). She finds that marrying before age 18 (relative to later ages) is associated with a 50% increase in divorce probability by couples' 10th anniversary and at least a 25% increase in divorce by couples' 20th anniversary (p. 986).[19] The data in Table 4 reject this hypothesis at the 5% level in every specification. Because our results are negative and statistically significant, scaling the coefficients and standard errors by the share of marriages on or prior to August 26, 1965 out of the entire quarter will widen our confidence intervals, but it will not affect the (negative) statistical significance of our estimates. Restricting our comparison to group to 1964 and 1965, however, results in point estimates that are even more negative and precise, with a 99% confidence interval of [−0.004, −0.023] for men and [−0.005, −0.025] for women.

The magnitudes of the local average treatment effects implied by Table 4 estimates are large but reasonable. Using the 1960–1970 marriage microdata, we find that the excess marriages from the beginning of third quarter of 1965 until the August 26 announcement comprise 11.0% of all marriages from that quarter.[20] A simple scaling implies that earlier marriages were 22% (dividing the scaled 6.4 percentage point reduction by the mean divorce rate of 29.3) less likely to divorce than expected in Table 4A column 2. Examining women instead in Table 4B column 2, we find that earlier marriages were 28% (dividing the scaled 9.7 percentage point reduction by the mean divorce rate of 34.5) less likely to divorce than expected.[21]

4.2 | Evaluating the role of selection into marriage in the 1980 Census

Could selection on observed or unobserved characteristics explain these results? On the one hand, the third-quarter 1965 marriages were disproportionately begun hastily, potentially under great duress, as young men sought to limit their exposure to the Vietnam War. This would amplify any negative effects of younger marriage. One would expect these marriages of convenience to be worse matches, and, therefore, less likely to survive for 15 years.

Other factors suggest these marriages could have fared better than others formed around the same period. First, beating the Executive Order 11241 deadline may have helped men avoid serving in Vietnam. Because wartime stress and extended separations are strong predictors of marital distress and divorce, midnight marriages might be *more* likely to survive if they were less likely to experience these separations and stress.[22]

TABLE 4 The effect of Executive Order 11241 on divorce and long-run outcomes, 1980 Census

	(1)	(2)	(3)	(4)
A. Mean ever divorced for men	0.293		0.298	
First married third quarter of 1965	−0.0066	−0.0071	−0.0133	−0.0133
	[0.0034]	[0.0033]	[0.0040]	[0.0042]
Ages in sample period	18–26	18–26	18–26	18-26
Number of men	5,148,000	5,148,000	902,000	902,000
B. Mean ever divorced for women	0.345		0.353	
First married third quarter of 1965	−0.0097	−0.0099	−0.0158	−0.0150
	[0.0033]	[0.0033]	[0.0041]	[0.0041]
C. Family income (median) for women	22,568		22,711	
First married third quarter of 1965	201.5867	207.3736	215.46	243.51
	[96.1661]	[96.6439]	[144.23]	[148.13]
D. Share living in poverty for women	0.105		0.110	
First married third quarter of 1965	−0.0037	−0.0036	−0.0039	−0.0045
	[0.0025]	[0.0025]	[0.0034]	[0.0034]
E. Share of women graduating college	0.161		0.150	
First married third quarter of 1965	0.0097	0.0097	0.0151	0.0160
	[0.0026]	[0.0027]	[0.0042]	[0.0042]
F. Number of children born	2.272		2.360	
First married third quarter of 1965	−0.005	−0.005	−0.029	−0.027
	[0.0079]	[0.0078]	[0.0113]	[0.0115]
Ages in sample period (panels B–F)	16–34	16–34	16–34	16–34
Number of women (panels B–F)	6,620,000	6,620,000	1,169,000	1,169,000
Sample years	1960–1970		1964–65	
Covariates	S, Y, Q, Race, BYQ	S, Y, Q, Race, BYQ, S × Y, S × Q, S × Race, S × BYQ	S, Y, Q, Race, BYQ	S, Y, Q, Race, BYQ, S × Y, S × Q, S × Race, S × BYQ

Notes: The specification is described in Equation (3). Standard errors clustered at the state level are reported in brackets. The Census sample necessarily excludes individuals born and married in the same quarter. See Table 1 notes for more information on samples and sources. Sample: 1980 Census, restricted data. The sample in columns 1 and 2 is restricted to men married at ages 18–26 from 1960 to 1970 (Table 1, column 4) and women married at ages 16–34 from 1960 to 1970 (Table 1, column 6). The sample in columns 3 and 4 is restricted to men married at ages 18–26 from 1964 to 1965 (Table 1, column 5) and women married at ages 16–34 from 1964 to 1965 (Table 1, column 7).

    Table 5 first investigates changes in the likelihood of having served in Vietnam as a function of marriage quarter. The specifications correspond closely to Tables 2 and 3, as they limit the Census sample to men who were first married between 1960 and 1970 (colums 1 and 2) or 1964 and 1965 (columns 3 and 4) and were ages 18–26 in the same period. Panel A shows that men who married in the third quarter of 1965 were not more successful in dodging the draft. Using the 1960–1970 period to construct counterfactuals, we find that men married in this period were *more* likely to be veterans when including only our basic set of fixed effects (column 1) and when including a more demanding set of covariates (column 2). Restricting our data to the 1964–1965 period yields a point estimate close to zero.[23] Consistent with histories of the period, local draft boards turned to low-priority men to fulfill their monthly draft calls as the war progressed.

    Second, men who married on August 26 may have been disproportionately selected on their desire to avoid military service. If these men pursued additional draft-dodging strategies, such as enrolling in college (Card & Lemieux, 2001), or if the increased likelihood of military service led to additional education through the G.I. Bill, these actions may have reduced the likelihood of later divorce. While we do see some evidence of this pattern in the 1960–1970 data, the results using the 1964–1965 sample yield a coefficient close to zero.
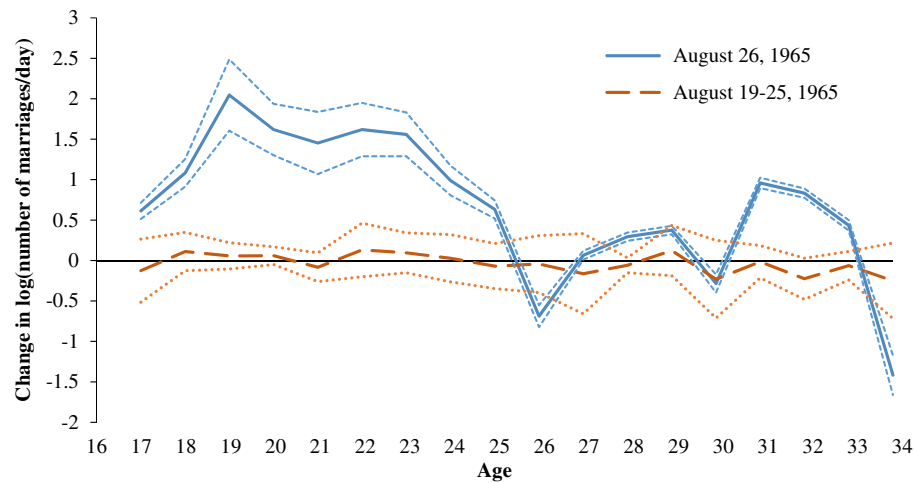
FIGURE 4 The impact of Executive Order 11241 on marriages by individual age. *Notes:* Marriage certificate microdata for marriages to men ages 14–34 who married women ages 14–54 in 1964–1965, with exact date of marriage (described in column 1 of Table 1). Log number of daily marriages estimated at the state-by-day level, including state-by-year, state-by-month, state-by-day of week, and state-by-age fixed effects. See Table 2 notes for more information. Dashed lines indicate 95% confidence intervals with robust standard errors

TABLE 5 The effect of Executive Order 11241 on men's veteran status and educational attainment, 1980 Census

	(1)	(2)	(3)	(4)
A. Share of men who are Vietnam veterans in 1980				
Mean dependent variable	0.292		0.222	
First married third quarter of 1965	0.0084	0.0087	−0.0053	−0.0049
	[0.0033]	[0.0033]	[0.0059]	[0.0060]
Number of men	5,148,000	5,148,000	902,000	902,000
Ages	18–26	18–26	18–26	18–26
B. Share of men graduating from college by 1980				
Mean dependent variable	0.256		0.239	
First married third quarter of 1965	0.0094	0.0098	0.0046	0.0052
	[0.0028]	[0.0028]	[0.0036]	[0.0037]
Number of men	5,148,000	5,148,000	902,000	902,000
Ages	18–26	18–26	18–26	18–26
Sample years	1960–1970	1960–1970	1964–1965	1964–1965
Covariates	S, Y, Q, Race, BYQ	S, Y, Q, Race, BYQ, S × Y, S × Q, S × Race, S × BYQ	S, Y, Q, Race, BYQ	S, Y, Q, Race, BYQ, S × Y, S × Q, S × Race, S × BYQ

Notes: The specification is described in Equation (3). Standard errors clustered at the state level are reported in brackets. See Table 1 notes for more information on samples and sources. Sample: 1980 Census, restricted data. The sample is restricted to men married at ages 18–26 from 1960 to 1970 (Table 1, column 4) in columns 1 and 2. The sample in columns 3 and 4 is restricted to men married at ages 18–26 from 1964 to 1965 (Table 1, column 5). The Census sample necessarily excludes individuals born and married in the same quarter.

Although we cannot rule out harder-to-detect forms of selection, the evidence does not support the conclusion that men entering into marriages in the third quarter of 1965 were less likely to serve in Vietnam or dodge the draft by enrolling in college—characteristics that, if correlated with the likelihood of a midnight marriage, may have offset the potentially negative effects of marrying younger. The fact that we find little difference in college going and military service should also reassure readers that other unobserved characteristics that are *correlated*

TABLE 6   Summary of proportional analysis of selection on unobserved factors, 1980 Census

	Women				Men			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Importance of selection on unobserved vs. observed factors	5.29	5.30	1.73	1.73	5.25	5.26	1.75	1.74
Rotz's effect size: β	0.140	0.169	0.140	0.169	0.140	0.169	0.140	0.169
R_{max}	0.133		0.144		0.129		0.138	
Sample years	1960–1970		1964–1965		1960–1970		1964–1965	

Notes: The table reports an analysis of the importance of unobserved factors to achieve Rotz's (2016) estimates that marriage before age 18 leads to a 16.9 percentage point increase in the likelihood of divorce (even columns), or her lower 95% confidence interval estimate of 14.0 percentage points (odd columns). We estimate the maximum variation explained by observables and unobservables using Oster's (2017) rule-of-thumb. See Table 1 notes for more information on samples and sources. All specifications include the full set of observables from columns 2 and 4 of Table 4: state-by-year, birth-year-quarter, state-by-quarter, and state-by-race fixed effects, along with education fixed effects and continuous measures of household income and income as a share of the poverty line. Sample: 1980 Census, 9% public use sample (in order to minimize data disclosure requirements). 1960–1970 data includes 855,544 women and 800,649 men, and 1964–1965 data includes 151,806 women and 141,467 men. The sample in columns 1–4 is restricted to women who married at ages 16–34; the sample in columns 5–8 is restricted to men who married at ages 18–26.

with education or military service are unlikely to drive our results. For instance, although Census data limitations mean that we cannot test for differences in risk aversion, patriotism, or idealism, these characteristics are correlated with college going and military service. In short, if midnight marriage grooms were more likely to have *un*observed characteristics correlated with marital stability and other long-run outcomes, these *un*observed characteristics would need to be *uncorrelated* with education and veteran status and have *very* large, positive effects to lead to our results.

To evaluate the role of these unobserved characteristics in driving our results, Table 6 presents our results following Oster's (2017) method of proportional selection, which builds on Altonji et al. (2005). For our results to be consistent with Rotz's (2016) point estimate or her lower 95% confidence interval estimate of the effect of earlier marriage on younger teens (presented at the bottom of Table 6), the effect of selection on unobserved factors would have to be *1.7–5.3 times as important* as observed correlates of divorce, including education, income, race, birthplace, and birth cohort, using Oster's (2017) rule-of-thumb.[24] The same results hold for men as well. Taken together, there is little reason to conclude that—apart from occurring at younger ages and under potential duress—third quarter 1965 marriages should be any less likely to result in divorce.

4.3 | Early marriages and women's long-run outcomes

Even if the midnight marriages were no less durable than their counterparts, these marriages may have imposed substantial costs on the affected men and women. As other studies document, marrying at younger ages may disrupt or limit women's education or career investments and result in these families being more economically disadvantaged in the long run. Our final analysis examines how changes in the age at first marriage (and changes in spouse) affected women's longer-term outcomes. Again using the restricted, 20% long-form sample of the 1980 Census, Table 4 shows the relationship between third-quarter 1965 marriages and other outcomes considered in the literature: median family income (panel C), living in poverty (panel D), college graduation (panel E), and childbearing (panel F). Consistent with little selection among women married in the third quarter, these marriages did not raise family income or reduce poverty rates. The results for these outcomes are virtually identical across different specifications and covariates. However, women marrying in the third quarter of 1965 had higher rates of college completion. Contrary to the findings of previous work that women married at younger ages gain less education, panel E shows that women married in the third quarter of 1965 were 1.6 percentage points (10.6%) *more likely* to graduate from college (column 4), an estimate that is statistically significant at the 1% level. Women's increased likelihood of graduating from college likely reflects other factors, perhaps owing to shared resources within the marriage itself (Edlund & Machado, 2015). In accordance with their increased education, women who married in the third quarter of 1965 had fewer children (panel D). The magnitude of the estimate is, however, small, implying only that 1.1% of couples had one fewer child (column 4).

5 | CONCLUSIONS

For many years, scholars have speculated that the rising age at marriage in the United States is causally related to decreasing divorce rates and improvements in the economic status of women. A causal relationship is consistent with the fact that the average age at first marriage has risen by around 3 years for men and 4 years for women over the past 30 years, while divorce rates have fallen.

Yet evidence from the rush of midnight marriages during the Vietnam-draft era provides little evidence to support this claim. Although Executive Order 11241 catalyzed around 9500 marriages to draft-age men nationwide on August 26, 1965,[25] we find no evidence that these policy-induced earlier marriages were more likely to end in divorce or remarriage 15 years later. In fact, younger marriages in the third quarter of 1965 appear slightly *less* likely to result in divorce, a finding that is statistically significant and robust across men and women in multiple specifications.

New microdata show that marriages induced by Executive Order 11241 occurred at significantly younger ages for men and women, and they also formed with different partners. In contrast to a large literature documenting how earlier (mostly teen) marriages have deleterious effects on women's long-run outcomes, we find that women who married younger (but on average around age 19 and a half) as part of the marriage rush did not experience lower family incomes or higher poverty rates. In fact, these women appear slightly *better* off in terms of their college graduation rates. For men, earlier marriages have little measurable relationship to outcomes. Men who married to dodge the draft in 1965 were not any less likely to be called for service, nor were they more likely to graduate from college by 1980 (i.e., they do not appear more likely to have sought an education deferment). In order for our results to be consistent with effects in the literature for marriages earlier in women's teenage years, selection on unobservables using Oster's (2017) rule-of-thumb would have to be 1.7–5.3 times as important as selection on observables like education, income, race, birthplace, and birth cohort.

Alongside the literature, these findings suggest that there may be considerable heterogeneity in the effects of early marriage on marriage stability and women's outcomes. In our sample, men and women induced to marry by the policy are in their late teens and early twenties whereas past literature finds the relationship between age at marriage and divorce is strongest for individuals who marry at much younger ages (Lehrer, 2008). Relative to the younger teenage brides and grooms of Dahl (2010) and Bharadwaj (2015), the men and women who married in August 1965 would have had longer to search for a suitable mate, invest in their own human capital, and mature as individuals. Moreover, while an unexpected pregnancy may drive some early teenage marriages, straining relationships from the start, the late-1965 marriages were not motivated by this difficult financial and emotional circumstance.[26] Although marriage postponement in recent decades has been cited as a main driver in the fall in divorce rates, our results indicate that other factors may be more consequential for men and women in their late teens and early twenties who are driving marriage delays today.

ENDNOTES

[1] For these same cohorts, the share of those ever married remains close to its historic mean of 90% (Bailey et al., 2014; Manning et al., 2014).

[2] Of course, delaying too long implies fewer potential partners, which could affect the likelihood of marrying or reduce the quality of one's match. Indeed, Becker et al. (1977) document a u-shaped relationship between age at marriage and divorce probability, with a positive relationship emerging after age 30. Other studies do not find increased divorce probabilities for those married at older ages, although the negative relationship between age and probability of divorce is strongest for individuals married in their teens and early twenties (Bumpass et al., 1991; Bumpass & Sweet, 1972; Glenn et al., 2010; Heaton, 1991; Lampard, 2013; Lehrer & Chen, 2011; Lehrer & Chen, 2013; Teachman, 1983, 2002).

[3] For instance, education, health, and socioeconomic status are correlated with later marriage (Hahn et al., 2018; Kirdar et al., 2012; Oppenheimer et al., 1997) and also with greater marital stability (Lefgren & McIntyre, 2006). See Lyngstad and Jalovaara (2010) for a review of predictors of marital stability.

[4] There is more work in developing countries. For instance, Field and Ambrus (2008) use age of menarche as an instrument for early marriage in rural Bangladesh and find that 1 year of delayed marriage leads to an additional 0.22 years of schooling for girls.

[5] Other studies in the United States focus on births and outcomes besides marriage stability. For instance, Buckles et al. (2011) use state-level variation in blood test requirements for obtaining marriage licenses and find that these deter marriage and increase the incidence of non-marital births. Blank et al. (2009) find that many individuals circumvent marriage restrictions by misreporting their ages and traveling to other states. Studies of divorce laws are also closely related and consider the impact of divorce laws on various outcomes like fertility, education, marriage quality, and incidence of domestic violence (Rasul, 2006; Stevenson, 2007; Stevenson & Wolfers, 2006; Stevenson & Wolfers, 2007).

[6] See Frimmel et al. (2014) for a study on a reduction in the average age at marriage for women in their mid-twenties in Austria, which also found no impact on marital stability.

[7] The I-A-O classification designated men who had received status as conscientious objectors, and they could only be required to report for civilian service. The I-A classification designated men who were available for military service. Men with the I-A-O classification were ranked in the same priority groups as the I-A classification men.

[8] Annual Report of the Director of Selective Service (1966: p. 17) notes that the first four categories included men ages 19–25 only. There were two additional priority categories within the I-A and I-A-O classification after married men: men age 26, youngest first, and men between age 18 and 6 months and age 19, oldest first (Appendix Table A1).

[9] Reports of the timing of this change vary. Katharine Seeyle of the *NYT* writes, "On Oct. 6, 1965, the Selective Service lifted its ban against drafting married men who had no children." Phil McCombs of the *Washington Post* writes "...toward the end of the year – on Oct. 26, 1965 – the Selective Service lifted the ban against drafting childless married men."

[10] Census results in Table 1 are from the 1980 IPUMS data (Ruggles et al., 2015). In order to minimize the disclosure burden, we did not disclose these nearly identical results for the restricted 20% Census sample. By necessity, we exclude from our Census samples those individuals born and married in the same quarter because we cannot calculate their year of marriage. For example, an individual married at age 25 in the third quarter of the year who was born in the third quarter of 1940 could have married in 1965 if he was born in July or in 1966 if he was born in September.

[11] The very slight age differences between the two samples may be due to the fact that age was misreported on marriage certificates (Blank et al., 2009), the fact that we cannot identify and exclude second marriages from the microdata (Census data include only first marriages), or our inability to compute date of marriage in the Census for individuals born and married in the same quarter.

[12] Coverage of state-dates is not balanced from 1960 to 1970. Because coverage is low for some states, 60% of all state-date cells are zero, as are 80% of all state-date-age-group cells. For this reason, we weight our regressions based on the total number of marriages recorded in our microdata. Appendix Table A2 includes a version of Table 2 using Poisson regressions, and they are nearly identical.

[13] Appendix Table A3 shows that estimates in Table 2 are robust to omitting state interactions, which is the same specification used in the Appendix Table A1 Poisson regressions.

[14] For brevity, we present only the results for the sample of marriages in 1964 and 1965. Appendix Figure A2 shows that using all marriages from 1960 to 1970 yields nearly identical results, and that our results are robust to the inclusion of additional controls.

[15] Thirty-five percent of quarter-state-age-group cells have zero marriages in the marriage microdata, and there are no zero cells in the 1980 Census. Appendix Table A2 shows that these results are also robust to using Poisson regressions. The results are also robust to using the 1960–1965 period.

[16] We obtain a scaling factor of 0.005 by noting that Thursday marriages comprise 5.4% of marriages, August marriages make up 37.5% of third-quarter marriages, and assuming an equal distribution of marriage by weeks. A similar exercise using the typical Thursday in a third week of August rather than treating all weeks equally yields a scaling factor of 0.004.

[17] Unlike Equation (1) specification, we necessarily exclude state-date cells in which no marriages took place.

[18] To see this, note that a 356% increase in marriages means that the "additional" number of marriages equals 3.56 times the "counterfactual" number of marriages, and that the share of 1965Q3 marriages that are induced by EO1965 is additional/(counterfactual + additional).

[19] Results based on a probit specification of the likelihood of divorce by the chosen anniversary. Instrumental variable results using state age-of-marriage laws to instrument for age at marriage yield nearly identical estimates at the 10th anniversary and even larger effects at the 20th anniversary.

[20] We use nationally aggregated daily marriage microdata to estimate total daily marriages to men ages 18-26, excluding 1965, using year, month, day, and day-of-week fixed effects. We calculate residuals for all marriages to men ages 18–26, and we sum residuals from July 1 until August 26, 1965 to find an excess 3562 marriages, which are 11.0% of all marriages in the third quarter.

[21] We repeat the previous exercise with marriages to all women ages 16–34, estimating an excess 4435 marriages, which are 10.2% of all marriages in that period.

[22] Hanson (2013), however, finds that marital stability was no different during periods when Vietnam War marriage deferments were and were not in effect.

[23] If we scale by the share of excess marriages prior to the executive order, we cannot rule out modest changes in the likelihood of being a veteran.

24 Rotz (2016, p. 979) estimates that marriage before age 18 is associated with a 16.9 percentage point increase in the likelihood of divorce within 15 years (95% CI: 0.140, 0.198). To minimize data reporting requirements, we use public use data files for this analysis. These results are slightly more conservative than the restricted data results because the point estimates are closer to zero in the public-use data set.

25 This is based on multiplying our point estimate for men ages 18–26 by the predicted 253,200 marriages in the third quarter of 1965 to men ages 18–26, based on the 1980 Census. We calculate a minimum of 5000 midnight marriages across the country using a more conservative estimation strategy that accounts for variation in the number of marriages across days of the week and weeks of the year.

26 For the 1946 cohort of women, 33% of those who married at ages 16–19 were pregnant at the time of marriage, compared with one-sixth and one-thirteenth for those who married between ages 20 and 21 and 22 and 23, respectively (Kiernan, 1986).

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of this article.