Marriage and Emancipation in the Age of the Pill*

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Abstract

The contraceptive Pill was FDA approved in 1960. However, it would be another decade before young unmarried women had full access. In the meantime, marriage constituted a way to the Pill. The later 1960s/early 1970s also saw a convergence on 18 as the minimum age of marriage, many states lowering it from 21. Exploiting these law changes, we find that a lowered minimum age precipitated marriage, delayed marital fertility, and improved women's educational and occupational outcomes. Marriage easing credit constraints combined with the contraceptive properties of the Pill form the hypothesized pathway.

Keywords: Contraceptive Pill, Marriage, Education, Labor Market Outcomes, Divorce.
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1 Introduction

The invention of the contraceptive Pill and its 1960 Food and Drug Administration (FDA) approval mark a watershed in social history. As the first female-controlled, safe, and cheap contraceptive, the Pill afforded women the separation of sexual activity and fertility, previously the preserve of men.

Initially, Pill access was restricted to married women. In the late 1960s, early 1970s, the Pill was made available to "mature minors," i.e., unmarried young adults short of the legal age of majority. "Early Legal Access," to borrow the terminology of Bailey [2006], was effected through a combination of laws that lowered the age of consent for medical treatment in the late 1960s and early 1970s, as pointed out by Goldin and Katz [2002]'s seminal paper and the substantial literature that followed in its wake, e.g., Bailey [2006, 2009], Hock [2007], Pantano [2007], Guldi [2008], Ananat and Hungerman [2012], Bailey et al. [2012], Ananat and Hungerman [2012].

Goldin and Katz [2002] emphasized the role of *marriage delay* and focused on *college women*. Before the Pill, college women would marry promptly on graduation. Marriage secured a husband and allowed for the continuation of a relationship. However, marriage effectively ended further educational aspirations. Pill access, they argued, changed these dynamics. With the Pill, college women could stay unmarried without losing current or prospective relationship. Freed from the imperative to marry young lest missing the boat, college women could enter post-graduate programs.

But before there was early legal access there was marriage, and marriage emancipates. Once married, a woman attains majority and attendant rights, notably Pill access. Thus, the Pill was available to young women years before early legal access, albeit with a marriage-catch.

As stressed by Goldin and Katz [2002], marriage could negate the empowering properties of the the Pill. Although marriage bars had been all but abandoned already in the 1950s [Goldin, 1990], social norms and expectations rendered marriage and career an unlikely combination for many women. With

marriage came career interruptions, interruptions that can be particularly damaging if early and not carefully planned.

On the other hand, a key feature of the Pill is its contraceptive properties. These properties were well known at the time of the Pill's FDA approval in 1960 [Watkins, 1998] and evidence suggests that the Pill was thus employed, e.g., Bailey [2010]. Furthermore, while marriage at the time may have come with an expectation of withdrawal from the workforce (for women), that expectation was paired with one of spousal support. Thus, early marriage, especially if childless, could be conducive to further education and training. Although marriage may have reduced the need to invest in human capital, it never eliminated it. Education (and training) have long been recognized to provide women with insurance in case of widowhood, desertion or divorce; to assist in social advancement; and to be a vehicle for self realization.

Still, early marriage is possibly not the ideal form of education financing. Even when paired with the Pill, marriage can encumber a woman in a number of ways, e.g., by introducing spousal demand for children, co-location issues, or heightened expectations of "home-making." Furthermore, marriage concluded before the crystalizing of personal priorities may be at the expense of long term compatibility and marital stability. However, for women lacking parental or scholarship funding, the ability to marry early and tap into spousal support may have been useful.

This paper investigates the possibility that early marriage in combination with Pill access helped women build human capital and exploit changes to the legal minimum-age of marriage. In 1960, about two-thirds of states allowed 18-year old women to marry without parental consent and marriage at that age was not uncommon. The median age of first marriage for women was below 21 for most of the 1960s.[1] The remainder of the states converged on 18 as the minimum-age in the late 1960s and 1970s, thus providing a source of state-cohort variation in an era when marriage provided privileged access to the Pill.[2]

[1] U.S. Bureau of the Census, Current Population Reports (2000), "Estimated Age at First Marriage."

[2] The convergence in minimum-age marriage laws explored in this paper occurred after the

Our key explanatory variable is whether a woman could marry before age 21 without parental consent ($Mar21$), constructed from Blank et al. [2009]. We combine information on the minimum marriage age with data on women's educational, occupational, marital and fertility outcomes from the June Current Population Survey (CPS), years 1977-1995, and focus on women born in the period 1935-1959, age 36-44 years old.

The changes to the minimum marriage age (detailed in Table 1) were introduced around the same time as the "Early Legal Access" (ELA) laws. In order to safeguard against findings being confounded by ELA, we control for ELA by including a variable ELA indicating an unmarried woman's right to obtain the Pill before the age of 21, following Bailey [2006]. The inclusion of ELA also affords comparison with Goldin and Katz [2002]. The study period also saw the extension of abortion rights and throughout we control for such rights.

We start by asking if a lowered legal marriage age prompted earlier marriage, and the answer is yes. We estimate that a minimum age of marriage below 21 ($Mar21$) increased the probability of marriage by age 20 by 3.1 percentage points, or an 8 percent increase.

We then turn to educational attainment and we find that $Mar21$ raised the educational attainment of women, the some-college margin being the most sensitive. Our estimates suggests that $Mar21$ raised the probability of some college by 4.5 percentage points, or a 10 percent increase. We find no evidence of an effect on high-school graduation, as might be expected for an outcome that is typically determined by age 17. As for four-year college, results are positive but fail to be significant in the baseline specification.

Turning to labor market outcomes, we find a positive effect of $Mar21$. By middle age, $Mar21$ had raised the probability that a woman was in a professional or managerial position by 4 percentage point, a 14 percent increase.

The most direct evidence of Pill access through marriage, however, would be delayed marital fertility. An extended gap between first marriage and first child would also suggest a mechanism for how early marriage (combined with

U.S. Supreme Court's 1965 Griswold v. Connecticut decision, cf., Bailey [2010].

the Pill) could have strengthen women's résumés. We find that a minimum age of marriage below 21 extended the window between marriage and first birth, echoing the findings of Bailey [2010] (who used variation in married women's Pill access generated by the so called Comstock laws).

Early marriage could herald early divorce, and divorce in turn exerts an independent influence on women's labor force attachment [Johnson and Skinner, 1986], salient since the study period also saw the arrival of easier divorce. Indeed, we find $Mar21$ to be associated with a higher likelihood of divorce, and therefore revisit our previous findings controlling for the effects of divorce. While divorce shows an independent effect on educational and occupational outcomes, our results for $Mar21$ were strengthened. Thus, it appears, divorce neither confounded nor mediated our findings.

Throughout, our results for $Mar21$ are robust to the inclusion of ELA. Considering ELA in its own right, we confirm Goldin and Katz [2002]'s findings for college women: ELA raised the fraction of women Doctors or Lawyers; delayed marriage; and reduced divorce risk. For non-college women, however, $Mar21$ rather than ELA emerges as the more important right.

The remainder of the paper is organized as follows: Section 2 provides a brief background, including motivation for how early marriage might have helped women build human capital. Section 3 presents our data and results. Section 4 concludes.

2 Background

This section sketches a case for early marriage helping women build human capital and provide a brief discussion of the legal environment. We start, however, with a brief literature review.

2.1 Literature Review

Women's entry into the labor force in the last half-century is perhaps the marquee social change of the Post-WWII period. Its consequences range from the mundane to overarching questions of economic efficiency and growth, e.g., Hsieh et al. [2013]. What caused this change is a matter of substantial interest. No doubt medical and technological advances played important roles. Lower infant and juvenile mortality paved the way for fewer desired children and time-saving household appliances such as the refrigerator and the washing machine vastly reduced the need for household work, e.g., Greenwood and Guner [2009]. Still, for a while, the effect was largely limited to allowing women of successively poorer strata a life of relative leisure.

The role of contraceptives in the fertility decline seen over the 20th century may be surprisingly limited. As note by Becker [1991], the bulk of the decline happened before WWII, a time without major advancements in contraceptive technology. While lower fertility arguably freed up women's time and labor force participation increased, demands for gender equality was not high on the social agenda. The inclusion of "sex" in the 1964 Civil Rights Act was the result of an attempt to obstruct and ridicule the proposed legislation, not the result of feminist struggle.[3] It is not until the 1970s that "Women's Lib" moved into the mainstream.

The notion that contraceptives could be behind women's labor market gains remained relatively unexplored until Akerlof et al. [1996] pointed to reduced male transfers in the wake of female-controlled contraceptives. Such contraceptives shifted responsibility for children from men to women: instead of being the fault of men, children became the choice of women. Faced with this new reality – children on your own or no children – the possibility that women might retool for a sharper focus on gainful employment is not far behind.

Goldin and Katz [2002]'s seminal paper linked women's inroads into "high-powered" professions to the diffusion of the birth control Pill to unmarried young

[3] By segregationist Howard W. Smith (Virginia-Democrat).

women. The Pill lowered the cost of career investments of college women by allowing for fertility control and marriage delay, they argued. Using state and cohort variation in whether a women younger than 18 could obtain birth control services without parental consent (a combination of age of majority laws, mature minor doctrine and family planning statutes), they showed that college women with Pill access were less likely to be married by age 23 (using CPS data). Using the 1970, 1980 and 1990 Censuses, but exploring only cohort variation (state aggregated data) in access by age 21, the Pill was found responsible for the representation of women in professional occupations (particulary among Doctors and Lawyers) and improved marital matches (reduced divorce probability by age 30-49).

Goldin and Katz [2002]'s paper spawned a literature on the social impact of the Pill. Bailey [2006] linked Pill access by age 21 to women's greater labor force attachment, focussing on the timing of the first birth.[4] That the Pill would reduce fertility is a priori plausible. Guldi [2008] found effects of Pill access on the age-specific birth rates of women 15 to 21 years old. Bailey [2010] showed that Pill access resulted in delayed marital fertility, exploiting the so called Comstock laws, laws which regulated the distribution of "obscene" material, including the Pill. Higher labor-force attachment translates into more work experience and Bailey et al. [2012] found women with Pill access to have experienced higher wage growth. Ragan [2013b] exploited variation in Pill access in Sweden stemming from local prevalence of pharmacies and found labor supply effects; Ragan [2013a] studied variation in Pill uptake, again in Sweden, arguing that it correlated with a retreat from marriage.

Hock [2007] found "late adolescent" access to the Pill (by age 18-19) to increase college enrolment of women (exploiting the October Supplement of the CPS, years 1968-1979). For men, the effects were lagged, consistent with the effect being through the partner (and further supporting a role of the Pill rather than other time and state varying factors).

[4]The estimated fertility effect of Pill on the probability of birth by age 22 was adjusted from 9 to 1.5 percentage points [Bailey, 2009, Table II].

For Pill effects on children, the posited mechanism turns on selection (cf. the literature on abortion access and child outcomes, e.g., Gruber et al. [1999]). Ananat and Hungerman [2012] found Pill access to result in mothers being more positively selected (short term effects were the opposite). Pantano [2007] looked at criminality among the cohorts whose mothers had had Pill access and found a negative effect. Madestam and Simeonova [2012] have found substantial positive effects on children's education and socioeconomic success using Swedish data.

Abortion rights changed around the same time as the extension of Pill access and most of the above studies have controlled for abortion access and found results to be robust to this inclusion. An exception is Joyce et al. [2011] which relied on an alternative measure of abortion access and regression specification.

In sum, the existing literature has emphasized *unmarried* women's Pill access. However, marriage emancipates and thus constituted a route to Pill access once FDA approved in 1960. While not for everybody, early marriage may have been an important access route for non-college women (a group excluded from Goldin and Katz [2002]'s analysis). Interestingly, Lang and Weinstein [2013] found that – before the Pill and abortion rights – teenage motherhood reduced education attainment and increased the likelihood of early marriage, but increased family income later in life. This effect was particularly pronounced for a middle group (least advantaged in terms of predicted schooling), suggesting a positive role for early marriage for this subset of women. Had the Pill been available, the effect might have been further strengthened, one may speculate.

Our paper is also related to a recent literature on the effects of marriage laws. Dahl [2010] focussed on early-teen marriage, ages 12-15, allowable in some states *subject to* parental (and individual) consent. Dahl found substantial negative consequences of such early marriage. Important differences with our work is his focus on: (i) substantially lower age of marriage (we focus on marriage before age 21); and (ii) earlier cohorts. Dahl studied women born 1920-1954, the bulk of whom would not have had Pill access in their early teens (nine out of the 35 cohorts reached 15 before the Pill's FDA approval in 1960). Bharadwaj [2009] looked at more stringent requirements enacted in 1957 in Mississippi.

He found a decline in marriage rates among 19-23 year old women. Lower marriage rates were accompanied by lower fertility and improved educational attainments, consistent with the mores of the pre-Pill era. Similarly, Buckles et al. [2011] exploited state repeals in blood test requirements in order to obtain a marriage licence. They found higher costs to marriage to reduce marriage rates. Their study period was 1980-2005, and perhaps indicative of the times, the reduction in marriage was accompanied by an increase in the out-of-wedlock fertility.

2.2 Marriage as a Facilitator of Women's Careers

How could early marriage further women's human capital accumulation? Typically, the opposite position is easier to argue. First, however, note that we are considering marriage around age 20 and for a period in which marriage gave Pill access and thus marriage without children was at least a theoretical possibility.

Second, marriage can come with spousal support. Gender roles prescribe a breadwinning role for the husband. Chivalry is one possibility. Additionally, the social tolerance of non-marital sexual relations or co-habitation was relatively low in the 1960s and early 1970s [Akerlof et al., 1996]. Another (and gender neutral) reason is economies of scale. A husband-wife team might be able to put one or both spouses through college. Furthermore, formal marriage may provide the legal backstopping to protect the spousal investment [Pollak, 1985, Borenstein and Courant, 1989].

Clearly, marriage was not the only way to finance post-secondary education. College minded women who could rely on the support of their parents might have preferred to do so. Even if childless, married life can interfere with higher education. Moreover, early marriage may be at the expense of long-term compatibility. However, for women lacking parental backing (or other forms of financing, e.g., stipends), those concerns may have been second order to the promise of spousal support.

To provide some evidence of the role of spousal support in education financ-

ing, we turn to Project Talent, a nationally representative sample of 9-12th graders in 1960, with follow-ups one, five and eleven years after the (expected) high school graduation year. Unfortunately, we do not have access to state identifiers and therefore cannot perform a regression analysis exploiting state variation in marriage laws. Instead, we provide descriptives on sources of education financing.

Using the 11-year follow-up and its question on post-secondary education financing, we find that while parents had provided the bulk of financing, spousal financing was non-trivial. Furthermore, parental financing was more important among women with a four-year college degree, suggestive of non-college women being credit constrained in their educational choices. As for spousal financing, it was more common among women with less than a college degree. Almost 10 percent of non-college women had received spousal financing, whereas this was true of only four percent of college women, Table 2, Panel A.

Higher reliance on parental financing and lower reliance on spousal financing among college women is also reflected in marriage patterns. Non-college women were more likely to have married by the 11-year follow up, Table 2, Panel B.

2.3 Legal Environment and the Coding of the Laws

In the 1960s and 1970s, the rights of young adults were expanded. The change was largely motivated "by the enhanced awareness, due in part by the Vietnam War, that young people had earned greater rights" [Goldin and Katz, 2002, page 764], a view epitomized by the Twenty-sixth Amendment to U.S. Constitution which lowered the voting age to 18 in 1971. At the state level, rights were expanded by giving "minors" (the definition of which changed throughout the period) the right to decide certain matters without parental consent, or by simply lowering the age of majority.

The expansion of rights extended to marriage and a number of states lowered the age at which a minor could marry without parental consent. By way of background, we note that the Christian tradition emphasizes individual con-

sent as the basis for marriage, e.g., Glendon [1996]. It is the consent that makes marriage a sacrament (a channel of divine grace), and marriage is open to everybody of a sound mind.[5] Thus, marriage itself implies the attainment of a certain level of mental capacity and maturity. In the context at hand, the most important implications is that marriage emancipates a minor giving him/her several rights, including the right to consent to medical treatment [DHEW, 1974]. At the time of the Pill's FDA approval in 1960, a majority of states allowed 18-year old women to marry without parental consent. By 1977, all but two states had lowered the minimum age for women to 18 [Blank et al., 2009].[6]

Our empirical analysis exploits state variation in these minimum age laws. Our key variable, whether a woman younger than 21 could marry without parental consent ($Mar21$), was constructed from the information on age of marriage in Blank et al. [2009]. Table 1 lists all minimum-marriage-age changes in the study period, highlighting those affecting 20-year olds.

We rely on Bailey [2006] for the coding of ELA (Pill access to unmarried women under 21); on Levine et al. [1996] for the timing of abortion rights; and on Gruber [2004] for the introduction of unilateral divorce. A summary of the expansion of these rights are in Figure 1.

The use of state law variation as a natural experiment rests on the assumption that law changes were unrelated to demands for teenage contraception or marriage. The exogeneity of these laws has been argued on both theoretical and empirical grounds. Goldin and Katz [2002] argued that the enactment of the laws granting Pill access to unmarried minors were likely unrelated to demand for contraception by this demographic group. Bailey [2006] investigated whether early ELA states differed on observable characteristics. Regressing state level characteristics on "time until liberalization" (the year ELA was enacted minus 1960), she found the coefficients on most characteristics to be statistically insignificant.

We replicate Bailey [2006]'s analysis by substituting $Mar21$ for ELA and by

[5] Save for a short list of impediments, notably relatedness or pre-existing marital bonds.
[6] The minimum marriage age is still 19 in Nebraska and Wyoming.

coding time to liberalization as zero for states with no change in $Mar21$.[7] Of the 21 outcomes considered, only the fraction of households with freezers and the fraction of men age 22-30 in the labor force were significant (10% and 5% levels respectively), see Table 3. Two significant outcomes out of 21 possible is consistent with a null hypothesis of no effect (type I error probability of 10%).

However, acceptance of the null hypothesis does not speak to unobserved differences in state characteristics. Following the literature [Goldin and Katz, 2002, Bailey, 2006, Ananat and Hungerman, 2012], we will control for gradually evolving characteristics using state trends.

3 Data and Results

Our main data set is constructed from the Marriage and Fertility Supplement of the June Current Population Survey (CPS). These data contain basic demographic and socioeconomic variable, as well as retrospective information on dates of marriage, child birth and divorce. Year and month of first marriage and first birth are consistently available for all women of childbearing age (18-44) for the years 1977, 1979-1983, 1985-1988, 1990, 1992 and 1995. For brevity, we will refer to these data as June CPS 1977-1995.

We restrict our sample to women born between 1935 and 1959 and 36 to 44 years old when surveyed. That is, we focus on women who turned 20 between 1955 and 1979; a period in which Pill access went from nil to universal and the access laws explored in this paper converged.

The age restriction is imposed because some of the outcomes can be realized later in life – such as completed education or professional outcomes – and therefore the response will vary with the age of the respondent [Goldin and Katz, 2002, Bailey, 2006, Ananat and Hungerman, 2012]. Furthermore, for some outcomes (e.g., first birth within x years of marriage) it is natural to restrict the sample to ever-married women. This restriction is innocuous for women aged

[7] Our regressions are unweighted. Following the good practice of reporting both weighted and unweighted results [Solon et al., 2013], we note our results were not sensitive to the use of weights.

36-44 (92 percent of them had married), but less so for younger ages.

For further description of the data and variables see the Data Appendix.

The June CPS is appropriate for our analysis because it contains marriage, fertility and divorce information at a level of detail (month-year) unavailable in other public datasets. One limitation, however, is that only state of residence, not state of birth, is available in this data. We believe that the detailed information contained in the June CPS data outweigh this potential limitation.[8]

Our baseline model is of the form:

$$Y_{iscy} = \beta_M Mar21_{isc} + \beta_A A_{isc} + \gamma \times X_{iscy} + \alpha_s + \alpha_c + \alpha_y + \alpha_s \times y + \epsilon_{iscy}. \quad (1)$$

Y_{iscy} denotes the outcome of interest for individual i from cohort c living in state s by survey year y. $Mar21_{isc}$ is an indicator variable equal to 1 if woman i in state s and cohort c could marry without parental consent before the age of 21. The state minimum marriage ages were obtained from Blank et al. [2009]. We control for abortion access, A_{isc}, extended during the study period. For residents of Alaska, California, Hawaii, New York or Washington, access at age 20 starts with cohorts born after 1950 [Levine et al., 1996]. For the remainder, access is assumed for cohorts born after 1953 (and therefore younger then 20 at *Roe v. Wade*, 410 U.S. 113 (1973)). The model includes state and cohort fixed effects (α_s and α_c), year fixed effects (α_y), state-specific year trends ($\alpha_s \times y$) and race indicators (Black and other non-White) as controls in X_{iscy}, unless otherwise indicated. While changes in $Mar21$ were plausibly unrelated to pre-existing observable state characteristics, we include state-specific trends to proxy for unobserved and gradually evolving state differences (important because the CPS sample goes from 1977-1995).

We also consider specifications that control for ELA rights [Bailey, 2006], as well as age fixed effect (α_a) and state-cohort trends ($\alpha_s \times c$) instead of year

[8] State of residence was also used in Bailey [2006, 2010] and Ananat and Hungerman [2012]. To investigate whether state of birth or state of residence better predicts state of residence at age 20, we use the 1980 IPUMS and its information on migration. We find that for ages 20-44 year old women, state of residence does better than the state of birth.

fixed effects and state-specific year trends (since the year, cohort and age fixed effects are collinear). The inclusion of state-cohort trends, in turn, control for gradually evolving state characteristics of cohorts (important because cohorts range from 1935 to 1959). We use the June-CPS survey weights and cluster standard errors at the state level.

Minimum-marriage-age laws were not absolute barriers to Pill access. Unmarried minors could pretend to be engaged or could convince physicians they had irregular periods [Goldin and Katz, 2002]. Additionally, marriage laws could be bypassed by the misreporting of age or jurisdiction shopping (residents of state m marrying in state n) [Blank et al., 2009]. However, such slippage would work against finding effects of a lowered marriage age.

Our main econometric approach is to estimate a model of the form given by Equation 1 using a linear probability model (LPM) where the outcome is a binary variably indicating age at marriage, educational attainment, occupation, birth and divorce timing (relative to marriage).[9] However, for outcomes that we can date (e.g., age at marriage and birth and divorce timing), we also implement a discrete-time proportional hazard model, the complementary log-log hazard model, to estimate the likelihood of women i reporting an event by time t:

$$\text{cloglog}[\theta(t, \mathbf{Z}_{iscyt})] = \theta_0(t) + \beta \mathbf{Z}_{iscy}, \tag{2}$$

where t indexes the time at which the event in question occurred. The baseline hazard $\theta_0(t)$ is estimated non-parametrically.[10] Since we observe individuals once only, the vector $\mathbf{Z}_{iscyt} = \mathbf{Z}_{iscy}$ does not include time-varying characteristics, and corresponds to the same variables considered in equation (1). We account for serially correlated shocks within a women over time by clustering

[9] Results were robust to estimation by logit or probit.

[10] For the study of age at marriage, t corresponds to number of year elapsed since age 12 (age 12 is minimum age a marriage occurs in our data). For the study of birth timing, t corresponds to the number of years elapsed since marriage until the birth of the first child, and could be negative if the birth occurs before marriage. As for divorce timing, t corresponds to the number of years elapsed since marriage until divorce. Since time specific hazards cannot be estimates for time intervals where the event considered does not occur, we group those those time intervals accordingly to ensure identifiability.

standard errors at the individual level i.

3.1 Lower Minimum Marriage Age and Early Marriage

Before turning to our focal question – did early marriage in the age of the Pill help women build human capital – we ask whether lowered minimum marriage age ($Mar21$) resulted in women marrying younger.

Table 4 presents result from estimating Equation 1 for the probability of having married by age 20 using LPM (columns 1-4), and the probability of marrying (any age) using a proportional discrete-time duration model (column 5).

We find that $Mar21$ increases the probability of having married by age 20 by about 3.5 percentage points (or 8 percent). This finding is robust to the inclusion of age fixed effect and cohort-state trends (column 2); as well as the inclusion of ELA (column 3) and its interaction with $Mar21$ (column 4).

Since $Mar21$ and ELA can be seen as alternative routes to the Pill that have the opposite implications for marriage entry, they may be viewed as substitutes rather than complements. The statistically insignificant effect of the interaction between $Mar21$ and ELA is consistent with this interpretation.

However, this result is identified from only four states (that experienced one law change but not the other: Nebraska, Virginia, Pennsylvania and Florida) and therefore should be viewed with caution.

Turning to the probability of having married (at any age) employing a proportional hazard model model, we find a positive effect, albeit statistically insignificant at conventional levels (column 5).[11]

[11] Expanding the sample to all ages improves precision and renders the effect of $Mar21$ is statistically significant, not reported. For this outcome, removal of the age restriction does not introduce selection issues.

3.2 Educational and Professional Outcomes

We now turn to our main question: in the 1960s when the Pill was available to married women, did the ability to marry early improve women's educational and labor market outcomes? Table 5 shows results from estimating Equation 1, where the dependent variable indicates attainment of high-school, some-college, or four-year college, respectively.

We see that the probability of having graduated high school is unaffected by $Mar21$ (Panel A, column 1), regardless of whether age fixed effects, state-specific cohort trends or ELA controls are included (columns 2-4). The absence of an effect is perhaps not surprising given that high school can be obtained free of tuition and is generally completed before age 20.

By contrast, for post-secondary education we find a statistically significant effect of $Mar21$ (Panels B and C). The effect is stronger for the less selective some-college margin. The estimated effect on some-college is some four percentage points, or a 10% increase, and is robust to the inclusion of ELA (column 3) and the interaction term $Mar21 \times ELA$ (column 4). Again here the interaction of ELA and $Mar21$ is statistically insignificant, indicating substitution between the rights. For the four-year college margin, we find positive effects in the 10-15% range, but in the whole sample (columns 1-4) the coefficient on $Mar21$ is only significant when ELA and $Mar21 \times ELA$ are also included (column 4).

For outcomes that involves timing relative to marriage, we will limit our analysis to ever-married women. For comparability, columns 5 and 6 show results from restricting the sample to ever-married women. The effects for post-secondary education are stronger for this sample, statistically significant for four-year college in the specification with age fixed effects and state-specific cohort trends, column 6; a finding consistent with the effect of $Mar21$ working through the proposed marriage channel.

Next we turn to occupational outcomes and we are particularly interested in occupations indicative of labor force attachment and career building. While Goldin and Katz [2002] focused on High Professionals (professional occupations

excluding teachers and nurses) and Doctors/Lawyers, categories that presume four-year college, we widen the scope to include managerial positions and *all* professional occupations (as defined in the CPS, see the Data Appendix).

The somewhat less selective scope is partly motivated by our finding of positive effects on the some-college margin (Table 5). Still, we are considering selective occupations. Among women reporting an occupation, 11 percent were in a managerial occupation and 18 percent were in a professional occupation.

Table 6 reports results from estimating Equation 1 where the dependent outcome is an binary variable indicating successively more selective professional outcomes. Starting with managerial or professional occupations (Panel A), we see that $Mar21$ has a positive and significant effect (3-4 percentage points, or 10-14 %). This result is robust across specifications and is not muted by the inclusion of ELA. In fact, between $Mar21$ and ELA, $Mar21$ emerges as the more important right. The last two columns restricts the sample to ever married. Again, the results are strengthened, consistent with early marriage being the channel.

Turning to more selective occupations, High Professionals and Doctors/Lawyers, we see that $Mar21$ is still a relevant right (Panels B and C). For Doctors/Lawyers, the effect of $Mar21$ is rendered borderline significant once ELA is included in the specification (column 3), but regains significance once the interaction term $Mar21 \times ELA$ is included, column 4. As for ELA, our findings are consistent with Goldin and Katz [2002] (who found positive effects of ELA on the Doctors/Lawyers outcome).

The specification that includes the interaction between $Mar21$ and ELA (column 4) in principle allows for the estimation of separate effects of $Mar21$ (ELA) depending on whether alone or acting in tandem with the other right. The latter would be given by the sum of the coefficients $Mar21 + ELA \times Mar21$ ($ELA + ELA \times Mar21$). But as noted in the previous section, identification is obtained from few states, reducing statistical power.

Furthermore, since the overall picture with respect to timing is that $Mar21$ was available through the 1960s, followed by ELA in the 1970s (see Figure 1),

one might argue that the sum of coefficients is mainly of relevance for ELA. By contrast, for cohorts reaching adulthood in the 1960s, the single coefficient on $Mar21$ is the relevant effect.

With these caveats in mind, we see that the sum of those coefficients are statistically zero in most cases. In other other words, $Mar21$ and ELA matter when alone, but the addition of the other right reduces the importance of the first one, perhaps unsurprising given our thesis that they offer alternative routes to Pill access.

In sum, the right that comes first matters the most, and since $Mar21$ preceded ELA in the vast majority of states, our results emphasize the importance of $Mar21$ and early marriage in the 1960s and 1970s for the education and occupation upgrading of women. We find evidence of a positive effect of the right to marry young, $Mar21$, on women's educational and professional outcomes. Between no Pill and Pill coupled with marriage, which is the effect captured by $Mar21$ and the case for most women in the 1960s, the latter helped women further their careers.

3.3 Fertility

Marital fertility allows us to further investigate the case for early marriage combined with the Pill improving educational and occupation outcomes. Clearly, early marriage quickly followed by the arrival of children may have done little to advance women's careers. However, curtesy of the Pill, fertility could have been delayed.

Table 7 reports results from estimating Equation 1 for the following outcomes: a first birth within 1 or 2 years of marriage, respectively, and any birth. Since the timing is relative to marriage entry, we restrict the sample to ever-married women.

We see evidence of $Mar21$ delaying fertility, statistically significant for births within the first two years of marriage, consistent with women marrying and using the Pill. Early marriage decreases the probability of a birth within one

year of marriage by 2.3-2.6 percentage points, an approximate 7% decrease. Similar magnitudes are found for the probability of a birth within two years of marriage.

Beyond two years, however, we found no fertility effects (not reported), consistent with early marriage leaving lifetime demand for children unaffected. The results for the duration model on fertility timing support this conclusion (Table 7, column 6). The coefficient on $Mar21$ estimates a multiplicative effect on the *entire* hazard function, and we see that the estimated effect on the hazard rate of a birth occurring is small and statistically insignificant.

In sum, early marriage extended the marriage-to-first-birth window, consistent with marriage facilitating human capital accumulation with the help of the Pill.

3.4 Divorce

Divorce is another outcome/channel of interest. Spouses married young may be less well matched and in an era of liberalized divorce, such marriages may have been less likely to last. Divorce in turn, actual or potential, likely has an independent influence on women's labor market attachment [Johnson and Skinner, 1986, Stevenson, 2007], raising the question of the role of divorce. Here, we seek to address two questions: did early marriage raise divorce rates; and were the labor market effects of $Mar21$ mediated or confounded by divorce?

We start by investigating the effects of $Mar21$ on divorce. For the years 1980, 1985, 1990, and 1995, the June CPS includes information on whether and when the first marriage terminated (not just current marital status). Thus, for these years we can study the longevity of the first marriage, arguably a measure of quality. Termination can be because of divorce, widowhood or separation, and we cannot distinguish among them. However, for the ages considered, widowhood is unusual leaving divorce or separation as the main reasons for marriage termination. For ease of exposition, we will refer to termination for any reason as "divorce." We use this "limited CPS sample" to look at whether

$Mar21$ relates to the probability of divorce and divorce timing.

A number of states introduced so called unilateral divorce in the 1970s and 1980s through a combination of laws that made divorce easier (primarily the introduction of no-fault grounds, the abolition of mutual consent requirement, and shortened separation requirements). Unilateral divorce made divorce easier, and may have had a similar effect on marriage – barriers to exit being barriers to entry. Therefore, we construct an indicator variable for whether, at 20, the woman had access to unilateral divorce using the coding of Gruber [2004, table 1].[12]

Table 8, shows the results from augmenting Equation 1 with our unilateral divorce variable. To provide a bridge to our earlier results, we start by showing that the estimated effect of $Mar21$ on marriage by age 20 is robust to the inclusion of a control for unilateral divorce access using either the June CPS 1977-1995 baseline sample, column (1), or the limited CPS sample, column (2).

Next, we turn to divorce as the outcome. In column (3) we see that $Mar21$ increased the likelihood of having divorced by 4.5 to 6 percentage points, borderline significant in the specification with year trends and year fixed effects (Panel A), but significant in the specification with cohort trends and age fixed effect (Panel B).[13] As for timing, we find that $Mar21$ increased the probability of having divorced within three years of the first marriage by 2.9-3.5 percentage points (column 4). The discrete-time hazard model points in the same direction. The hazard of divorce increases by $e^{.197}$=1.21 to $e^{.254}$=1.28, that is, women were 21-28% more likely to divorce if they had been able to marry at age 20, (column 5).

In view of these findings, we revisit the question of the role of divorce in shaping educational and professional outcomes and we find that our results are robust to the inclusion of controls for divorce. We considered two measures.

[12] We thus deviate slightly from the literature which has typically focussed on availability of unilateral divorce during the course of marriage on divorce risk.

[13] Wolfers [2006] cautioned against using state trends when estimating the effect of law changes on period divorce rates since there may be pent-up demand. That is not an issue here since we look at unilateral divorce access at the age of 20.

First, we control for whether the woman resided in a state that would have allowed her unilateral divorce at age 21 (as coded by Gruber [2004]). Second, we include information on actual divorce history (available for some CPS years). While divorce (measured either way) had an independent effect on educational and occupational outcomes, our results for $Mar21$ were strengthened by its inclusion. In the interest of space, the results are relegated to the Appendix, Tables A.1-A.4.

3.5 Results by college/non-college

Our paper has presented empirical support for the idea that early marriage combined with the Pill helped women build human capital. That women without a four-year college degree would on average be from poorer backgrounds and possibly more credit constrained in their education choices seems a reasonable conjecture (and in line with the findings on post-secondary education financing in Table 2). If so, early marriage could have been particularly important for non-college women.

Separating college and non-college women would also allow for comparison with Goldin and Katz [2002], whose study focused on college women.

However, the separate study of college and non-college women is potentially problematic since the compositions of the two groups may change due to the studied law change. Still, the evidence for selection on the college margin in the whole sample is weak, Table 5, columns (1)-(4). Furthermore, if $Mar21$ eased credit constraints and thus made four-year college more accessible, it is possible that both college and non-college women became more more negatively selected, arguably working against finding positive occupational outcomes in either group.

With these considerations in mind, we note that the effects of $Mar21$ is generally stronger for non-college women and this is particularly the case for birth timing and occupational outcomes save doctors and lawyers (professions that presuppose a four-year college degree), Appendix Tables A.5-A.10. These

results are consistent with the interpretation of early marriage, joint with Pill access and spousal support, helping the credit constrained invest in human capital.

4 Discussion

Although FDA approved in 1960, it would be another decade until young unmarried women had full Pill access [Goldin and Katz, 2002]. In the meantime, there was marriage. Marriage emancipates and a married woman could consent to medical treatment, including the Pill.

While the majority of states in 1960 allowed women to marry at age 18, not all did. In the late 1960s/1970s, the marriage age was lowered from 21 to 18 (or 19) in a number of states. In this paper, we have exploited the lowering of the minimum marriage age to investigate whether early marriage in the era of the Pill helped boost women's educational and professional outcomes.

This may seem a strange inquiry, marriage has generally not viewed as conducive to women 's career development. Social expectations of home making, career interruptions from child bearing and outright marriage bars have in the past conspired to make marriage and career hard to reconcile for women. In fact, Goldin and Katz [2002, page 766] emphasized the importance of Pill accesses to *unmarried* women and the ability to *delay* marriage, for "the power of the pill in affecting womens careers."

On the other hand, by 1960, at least two of these obstacles had lost some of their former relevance. One, by the 1950s, marriage bars were virtually abolished, save for flight attendants [Goldin, 1990]. Two, by 1960, the FDA approval of the Pill brought cheap and effective birth-control to the marital bedroom. Yet social norms and expectations would still remain. While those may have been more supportive of home-making than careers for the woman, their flip-side – male breadwinning – may have helped young married women build human capital, at least while childless.

Analyzing CPS data, we find that for the cohorts of women who turned 20

after the Pill's 1960 FDA approval, the ability to marry before age 21 resulted in earlier marriage, widened window between marriage and first birth, and improved educational and professional outcomes. We also document evidence that early marriage raised divorce rates, but improved occupational and professional outcomes resulted independent of any career boosting effects of divorce.

In sum, our paper has extended the existing literature by considering early marriage as a route to the Pill and showed evidence that such access also contribute to the educational and occupational upgrading of American women.

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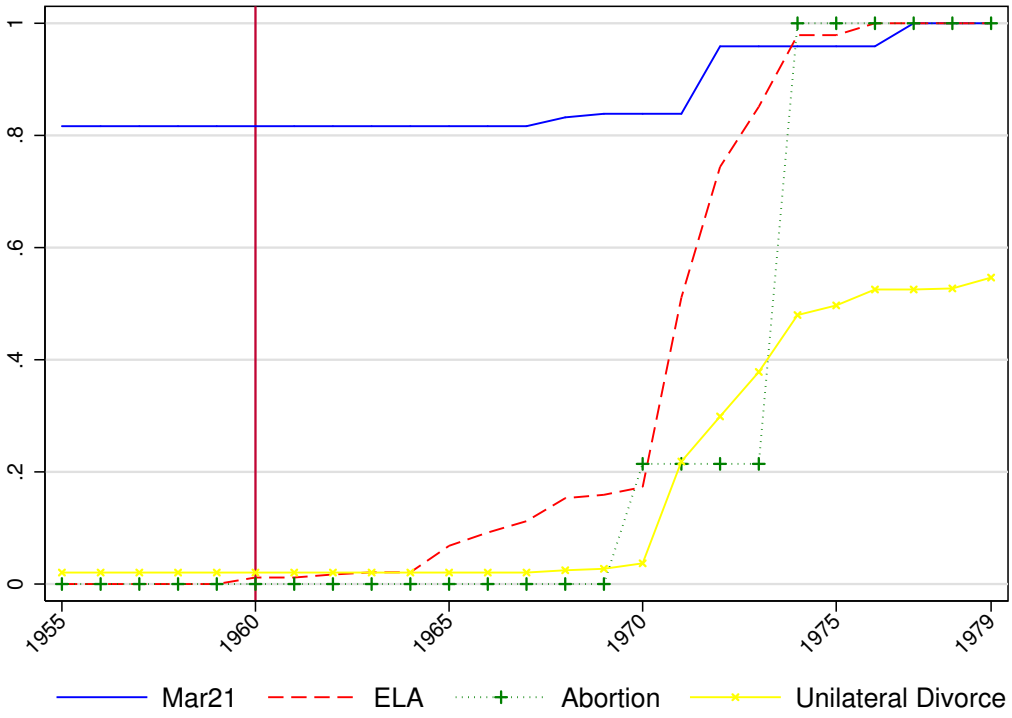
Figure 1: Timing of Laws
% States with Rights, by Year



Notes: The graph shows the share of states with early marriage access ($Mar21$), early legal access (ELA), abortion and unilateral divorce rights. $Mar21$ corresponds to the right to marry before age 21 from Blank et al. [2009], see Table 1. ELA corresponds to Pill access to unmarried women before age 21 from Bailey [2006]. Abortion rights are coded as in Levine et al. [1996] and unilateral divorce access is from Gruber [2004]. Each state is weighted by its female population ages 36-44 in the 1980 Census.

Table 1: Changes in the Minimum Age of Marriage

		Age	
State	Year	from	to
South Carolina	1957	14	18
Mississippi	1958	18	15
Georgia	1965	18	19
Kentucky*	1968	21	18
Hawaii	1969	20	18
Nebraska*	1969	21	20
Montana	1971	18	19
Iowa	1972	18	19
Virginia*	1972	21	18
Connecticut*	1972	21	18
Rhode Island*	1972	21	18
Georgia	1972	19	18
Pennsylvania*	1972	21	18
Louisiana*	1972	21	18
Nebraska	1972	20	19
West Virginia*	1972	21	16
Iowa	1973	19	18
Montana	1973	19	18
Alaska	1974	18	19
Alaska	1975	19	18
Wyoming	1975	18	19
Florida*	1977	21	18

Notes: The table show all changes in female minimum age of marriage (without parental consent) between 1955 and 1979 from Blank et al. [2009]. Events that involved a lowering of the minimum marriage age below 21 are marked by *.

Table 2: Post-Secondary Education Financing

	(1)	(2)	(3)
	Sample:		
	College or More	Less than College	Difference
Panel A. Fraction with Post Secondary Education Financing by:			
Parents	0.777	0.501	-0.276***
			(0.009)
Spouse	0.041	0.098	0.057***
			(0.005)
Spouse, if Ever Married	0.053	0.112	0.060***
			(0.006)
Panel B. Marital Status:			
Ever Married	0.780	0.856	0.076***
			(0.007)
Currently Married	0.731	0.766	0.035***
			(0.008)
N	5,940	6,365	-

Notes: Data are from Project Talent, access provided by the American Institutes for Research. The sample is restricted to women attending high school grades 11 and 12 in 1960 and who had attended some college by the 11-year follow-up survey. The table presents averages using survey weights (weights "c", as provided in the dataset) for women with a college degree or more and with less than a college degree, and the difference in means between the groups (standard errors in parenthesis).
*** significant at 0.01.

Table 3: 1960 State Level Predictors of Years Until Marriage Liberalization

Dependent Variable: Years until Minimum Marriage Age Below 21					
Independent Variable:					
Fraction of Population:			Fraction Women in Ages:		
On Farm	Black	Foreign Born	15-21	22-30	31-45
-10.516	2.819	5.275	-72.580	-39.050	31.376
(7.392)	(5.444)	(19.427)	(81.933)	(51.463)	(43.685)
	Women Born 1920-1929:		Fraction of Population:		
Education (Women)	Age 1st Marriage	Children Born	Poor	Catholic	Vietnam Casuality
-1.495	0.661	-1.163	2.831	2.975	-573.637
(1.243)	(0.828)	(1.979)	(5.809)	(2.957)	(956.103)
Fraction of Households with:					
Radio	Washer	Dryer	Freezer	≥1 car	≥2 cars
-20.540	-0.121	-4.617	-12.453*	-8.835	-12.848
(14.911)	(7.416)	(5.378)	(6.424)	(7.588)	(8.410)
Men Ages 22-30			Women Ages 22-30		
In LF[a]	Unemployed	Wages	In LF[a]	Unemployed	Wages
-47.436**	11.222	-0.000	-17.974	-149.884	-0.002
(19.330)	(66.066)	(0.001)	(14.967)	(124.807)	(0.003)

Notes: Data are from 1960 IPUMS. Data on church membership and Vietnam casualties are from Bailey [2006], curtesy of the author. Each cell corresponds to the point estimate from regressing the dependent variable on the respective state characteristic. The dependent variable is the number of years (since 1960) until a woman younger than 21 could marry, see table 1. It is zero for states that allowed women younger that 21 to marry already in 1960. Standard errors are in parenthesis. Regressions are unweighted. There are 51 observations in each regression, except for the fraction Catholic for which data only covered the 48 contiguous U.S. states.

[a] In LF – in labor force.

* significant at 0.10; ** significant at 0.05.

Table 4: Effects of Early Access on Marriage

	(1)	(2)	(3)	(4)	(5)
	LPM:				Duration Model:
	Married by Age 20 (mean: 0.375)				Marriage
$Mar21$	0.035**	0.031*	0.036**	0.033**	0.054
	(0.013)	(0.016)	(0.015)	(0.015)	(0.036)
ELA			-0.013	-0.024	
			(0.010)	(0.025)	
$Mar21 \times ELA$				0.012	
				(0.026)	
N	96,011	96,011	96,011	96,011	1,111,673
Cohort Fixed Effect	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓
Year Fixed Effect	✓				✓
State-Specific Year Trend	✓				✓
Age Fixed Effect		✓	✓	✓	
State-Specific Cohort Trend		✓	✓	✓	

Notes: Data are from the June CPS 1977, 79-83, 85-88, 90, 92, 95, and restricted to women of ages 36-44 and cohorts 1935-59. Columns (1) to (4) display the results of a linear probability model (LPM) for equation (1) with standard errors clustered at the state level. The outcome is whether married by age 20. Column (5) displays results for the discrete duration model in equation (2) with standard errors clustered at the individual level. Time is in years since age 12 until first marriage. $Mar21$ corresponds to the right to marry before age 21 from Blank et al. [2009]. ELA corresponds to Pill access to unmarried women before age 21 from Bailey [2006]. All regressions control for abortion access before age 21 (as in Levine et al. [1996]) and race (White/non-White) and use survey weights. Standard errors are in parenthesis.
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

Table 5: Early Access and Educational Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.			≥ High School Graduate (mean: 0.843)			
$Mar21$	0.009	0.011	0.009	0.013	0.012	0.012
	(0.016)	(0.014)	(0.015)	(0.013)	(0.014)	(0.013)
ELA			0.005	0.023*		
			(0.005)	(0.011)		
$Mar21 \times ELA$				-0.019		
				(0.012)		
Panel B.			≥ Some College (mean: 0.443)			
$Mar21$	0.030**	0.037**	0.039**	0.045***	0.038**	0.045**
	(0.014)	(0.016)	(0.016)	(0.015)	(0.015)	(0.017)
ELA			-0.006	0.020		
			(0.010)	(0.028)		
$Mar21 \times ELA$				-0.028		
				(0.029)		
Panel C.			≥ Four Year College (mean: 0.215)			
$Mar21$	0.017	0.026	0.023	0.029*	0.026	0.033*
	(0.018)	(0.020)	(0.021)	(0.016)	(0.017)	(0.018)
ELA			0.008	0.032		
			(0.008)	(0.027)		
$Mar21 \times ELA$				-0.026		
				(0.028)		
N	96,011	96,011	96,011	96,011	88,260	88,260
Cohort Fixed Effect	✓	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓	✓
Year Fixed Effect	✓				✓	
State-Specific Year Trend	✓				✓	
Age Fixed Effect		✓	✓	✓		✓
State-Specific Cohort Trend		✓	✓	✓		✓
Ever-Married Restriction					✓	✓

Notes: Data are from the June CPS 1977, 79-83, 85-88, 90, 92, 95, and restricted to women of ages 36-44 and cohorts 1935-59. The table reports results of a linear probability model for equation (1) with standard errors clustered at the state level. The outcomes are indicators for education attainment: high school or more, some college or more, four year college or more. $Mar21$ corresponds to the right to marry before age 21 from Blank et al. [2009]. ELA corresponds to Pill access to unmarried women before age 21 from Bailey [2006]. All regressions control for abortion access before age 21 (as in Levine et al. [1996]) and race (White/non-White) and use survey weights. Standard errors are in parenthesis.
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

Table 6: Early Access Occupational Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.	Managerial and Professional Occupations (mean: 0.290)					
$Mar21$	0.030*	0.035**	0.030**	0.037**	0.037**	0.040**
	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.015)
ELA			0.016	0.046		
			(0.010)	(0.030)		
$Mar21 \times ELA$				-0.033		
				(0.030)		
Panel B.	High Professional Occupations (mean: 0.065)					
$Mar21$	0.012**	0.015***	0.012***	0.016**	0.011*	0.012***
	(0.005)	(0.004)	(0.004)	(0.006)	(0.006)	(0.004)
ELA			0.006	0.021		
			(0.005)	(0.015)		
$Mar21 \times ELA$				-0.016		
				(0.016)		
Panel C.	Doctors or Lawyers (mean: 0.006)					
$Mar21$	0.007**	0.006*	0.005	0.005*	0.007**	0.007**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
ELA			0.003**	0.004		
			(0.001)	(0.005)		
$Mar21 \times ELA$				-0.001		
				(0.005)		
N	70,759	70,759	70,759	70,759	64,401	64,401
Cohort Fixed Effect	✓	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓	✓
Year Fixed Effect	✓				✓	
State-Specific Year Trend	✓				✓	
Age Fixed Effect		✓	✓	✓		✓
State-Specific Cohort Trend		✓	✓	✓		✓
Ever-Married Restriction					✓	✓

Notes: Data are from the June CPS 1977, 79-83, 85-88, 90, 92, 95, and restricted to women of ages 36-44 and cohorts 1935-59. The table reports results of a linear probability model for equation (1) with standard errors clustered at the state level. The outcomes are indicators for three occupation groups: managerial and professional occupations, professional occupations excluding teachers and nurses (high professional occupations) and Doctors and Lawyers. $Mar21$ corresponds to the right to marry before age 21 from Blank et al. [2009]. ELA corresponds to Pill access to unmarried women before age 21 from Bailey [2006]. All regressions control for abortion access before age 21 (as in Levine et al. [1996]) and race (White/non-White) and use survey weights. Standard errors are in parenthesis.
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

Table 7: Early Access and Fertility

	(1)	(2)	(3)	(4)	(5)	(6)
	1st birth within[a] x years of 1st marriage:				Birth>0	Duration Model:
	$x=1$	$x=2$	$x=1$	$x=2$		Birth Timing
$Mar21$	-0.023*	-0.028*	-0.026**	-0.034**	0.009	-0.015
	(0.012)	(0.016)	(0.012)	(0.016)	(0.013)	(0.038)
N	88,260	88,260	88,260	88,260	88,260	2,623,608
Mean of y	0.329	0.558	0.329	0.558	0.900	-
Cohort Fixed Effect	✓	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓	✓
Year Fixed Effect	✓	✓			✓	✓
State-Specific Year Trend	✓	✓			✓	✓
Age Fixed Effect			✓	✓		
State-Specific Cohort Trend			✓	✓		

Notes: Data are from the June CPS 1977, 79-83, 85-88, 90, 92, 95, and restricted to ever married women of ages 36-44 and cohorts 1935-59. Columns (1) to (5) display the results of a linear probability model (LPM) for equation (1) with standard errors clustered at the state level. The outcomes are whether the first birth occurs within 1 year of first marriage, within 2 years of first marriage and whether ever giving birth. Births can happen either before of after marriage. Column (6) displays results for the discrete duration model in Equation (2) with standard errors clustered at the individual level. Time is in years since first marriage until first birth. $Mar21$ corresponds to the right to marry before age 21 from Blank et al. [2009]. All regressions control for abortion access before age 21 (as in Levine et al. [1996]) and race (White/non-White) and use survey weights. Standard errors are in parenthesis.

* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

Table 8: Early Access and Divorce

	(1)	(2)	(3)	(4)	(5)
	Sample:				
	Baseline (1977-95)	Limited CPS (1980,85,90,95)			
	Married by 20		Ever Divorced	Divorce Timing: ≤3 years	Divorce Timing: Duration Model
Panel A.					
$Mar21$	0.034**	0.033*	0.045	0.029**	0.197**
	(0.013)	(0.019)	(0.029)	(0.013)	(0.092)
Unilateral	-0.004	0.013	0.005	0.007	0.015
	(0.011)	(0.012)	(0.011)	(0.008)	(0.062)
Year Fixed Effect	✓	✓	✓	✓	✓
State-Specific Year Trend	✓	✓	✓	✓	✓
Panel B.					
$Mar21$	0.032*	0.034	0.060*	0.035**	0.254**
	(0.016)	(0.026)	(0.034)	(0.015)	(0.101)
Unilateral	0.004	0.018	0.008	0.005	0.025
	(0.011)	(0.013)	(0.013)	(0.009)	(0.068)
Age Fixed Effect	✓	✓	✓	✓	✓
State-Specific Cohort Trend	✓	✓	✓	✓	✓
Panels A and B.					
N	96,011	30,268	27,435	27,435	419,508
Mean of y	0.375	0.346	0.349	0.058	-
Cohort Fixed Effect	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓
Ever-Married Restriction			✓	✓	✓

Notes: Data are from the June CPS 1977, 79-83, 85-88, 90, 92, 95, and restricted to women of ages 36-44 and cohorts 1935-59. The Limited CPS sample is drawn from years 1980, 1985, 1990 and 1995 and imposes the same cohort and age restrictions as for the main sample. Columns (1) to (4) display the results of a linear probability model (LPM) for equation (1) with standard errors clustered at the state level. The outcomes are indicators for whether married by age 20, whether ever divorced and whether divorced within three years of marriage. Column (5) displays results for the discrete duration model in Equation (2) with standard errors clustered at the individual level. Time is in years since first marriage until "divorce" (divorce, widowhood or separations). $Mar21$ corresponds to the right to marry before age 21 from Blank et al. [2009]. Unilateral indicates whether unilateral divorce laws were in place before age 21 (from Gruber [2004]). All regressions control for abortion access before age 21 (as in Levine et al. [1996]) and race (White/non-White) and use survey weights. Standard errors are in parenthesis.
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

APPENDIX

I Data Appendix: Marriage and Fertility Supplement of the June Current Population Survey

I.1 Sample Restrictions

The Marriage and Fertility Supplement of the Current Population Survey (CPS) is administered in the month of June of selected years. Although the questionnaire and interview universe vary by survey year, information on dates (month and year) of first marriage and first birth is consistently available for all women of childbearing age (18-44) for 1977, 1979, 1980, 1981, 1982, 1983, 1985, 1986, 1987, 1988, 1990, 1992 and 1995.

Dates on first marriage are available for all women ever married, and the cap on age 44 is imposed in years 1986, 1987, 1988 and 1992. Dates on first marriage are also available in years 1984 and 1994, but the date of first birth is not, we do not include those years. While the codebook for years 1986, 1987 and 1988 describes the fertility information to be available only for ever-married women, the actual facsimile of the questionnaire does not restrict this question to ever-married women, and in fact, date of first birth exist also for never-married women. We did not find any clarification in the documentation for the occurrence of this information, but we verified the response was equally likely for never and ever-married women and allocation flags did not indicate imputed values for never-married women. Therefore, we considered those data entries to be valid. The cap on age 44 for fertility information is present in the years 1986, 1987, 1988 and 1992.

We use the data retrospectively and restrict the sample to women ages 36-44 from cohorts 1935-1959. The age threshold is imposed in order to obtain a reasonably complete picture of educational attainment, occupational outcomes and fertility. The cohort restriction means that we are looking at women who turned 20 between 1955 and 1979, a period in which Pill access went from nil

to universal, and for which the access laws (marriage, Pill, unilateral divorce) explored in this paper converged.

Qualifiers for the Marriage and Fertility Supplement questionnaire are gender, age and marital status. We drop observations with imputed values for those variables. We also drop the observations for which dates of first marriage and first birth were imputed.

Information on date of divorce (of first marriage) is available only for the years 1980, 1985, 1990 and 1995. When looking at divorce as an outcome or using it as a control, our sample is restricted to those years.

I.2 Variables

State Identifiers. The June CPS has information on state of residence only. We use that state information to assign the relevant state laws on marriage, Pill and Abortion access, and Unilateral Divorce.

Cohort Cohorts are defined by year of birth.

Age at First Marriage We generate age at first marriage by combining dates (month and year) of birth and first marriage.

Education We focus on the following three classifications:

High School Graduate or more High-school graduate or or more.

Some College or more 1 year of college, or more.

College Graduate or more Four-year college or more.

In surveys years 1977 to 1990, education is reported as the highest grade attended, where the categories are: 1-8 years of elementary school, 1-4 years of high school 1-6+ years of college. We combine this variable with information on whether the highest grade attended was completed or not.

For years 1992 and 1995, education is reported in terms of attainment. High school graduates include those with diploma, GED or equivalent.

For college, the categories are: (a) some college but no degree, (b) Associate Degree in college/vocational program, (c) Associate Degree in college academic program, (d) Bachelor's Degree, (e) Master's Degree, (f) Professional School Degree, (g) Doctorate Degree. We classify (a) through (g) as Some College or more; and (d) through (g) as College Graduate or more.

Occupation The June CPS uses the Census Classification of Occupations of 1970 until 1982, and then the Census Classification of 1980. We focus on managerial and professional occupations.[14] We further look into increasingly selective subgroups of the professional occupation: "High Professional Occupations" and "Doctors or Lawyers."

Managerial Occupations Managers and Administrators, except Farm (codes 200-245). The Managerial Occupations include executive, administrative and managerial occupations as chief executives, financial managers, public administrators, personnel and purchasing managers, among the many other management related occupations.

1977-1982: codes 200-245.

1983-1995: codes 0-42.

Professional Occupations Professional, Technical and Kindred Workers. The Professional Occupations include engineers, doctors, mathematicians, natural scientists, social scientist, lawyers, judges, and teachers, among others.

1977-1982: codes 0-200, except 80-85 and 163-173 (technicians not included in professional occupations from 1983 and onwards).

1983-1995: codes 43-199.

High Professional Occupations Professional occupations excluding pri-

[14] Among the following groups: managerial and professional specialty occupations; farming, forestry, and fishing occupations; experienced unemployed not classified by occupations technical, sales, and administrative support occupations; precision production, craft, and repair occupations; service occupations; and operators, fabricators, and laborers.

mary and secondary teachers and health assessment and treating occupations.

1977-1982: Excludes codes 64 and 74-76 (registered nurses, dietitians, pharmacists and therapists), and 141-145 (primary and secondary teachers).

1983-1995: Excludes codes 95-106 and 155-162.

Professional Occupations excluding teachers and nurses.

Doctors or Lawyers Physicians, dentists, veterinarians, lawyers and judges.

1977-1982: codes 30-31, 62, 65, and 74.

1983-1995: codes 84-86 and 178-179.

Birth Timing We generate timing of first birth relative to first marriage using dates (month and year) of first marriage and first birth.

Divorce Dates on first marriage termination are reported in two variables: date the first marriage ended for marriages ending in divorce or widowhood, and date the respondent stopped living with spouse for marriages ending in divorce or separation. In order to maximize data availability on marriage terminations we consider first the date a marriage ends, and when missing, consider the date the respondent stopped living with spouse. For brevity, we refer to these terminations as divorce.

To generate divorce (widowhood or separation) within a particular time window we combine information on date of first marriage termination and date of birth.

Race Controls We use three race categories: "white", "negro/black" and "other" – the race categories until 1988. Starting in 1990, "American Indian, Aleut, Eskimo" and "Asian or Pacific Islander" can also be specified. We code those two new options under "other."

II Replication of Education and Occupation results controlling for unilateral divorce and marital history

Table A.1: Effects of Early Access on Education – Controlling for Unilateral Divorce Access

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.			≥ High School Graduate (mean: 0.843)			
$Mar21$	0.009	0.012	0.010	0.014	0.012	0.013
	(0.016)	(0.014)	(0.015)	(0.013)	(0.014)	(0.013)
ELA			0.005	0.022*		
			(0.005)	(0.011)		
$Mar21 \times ELA$				-0.019		
				(0.012)		
Unilateral	-0.001	0.004	0.003	0.003	-0.001	0.003
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Panel B.			≥ Some College (mean: 0.443)			
$Mar21$	0.023	0.029*	0.031*	0.037**	0.029*	0.034*
	(0.015)	(0.016)	(0.016)	(0.014)	(0.015)	(0.017)
ELA			-0.005	0.021		
			(0.009)	(0.026)		
$Mar21 \times ELA$				-0.028		
				(0.027)		
Unilateral	-0.030***	-0.033**	-0.033**	-0.033**	-0.037***	-0.042***
	(0.011)	(0.014)	(0.014)	(0.014)	(0.011)	(0.014)
Panel C.			≥ Four Year College (mean:0.215)			
$Mar21$	0.017	0.026	0.023	0.029*	0.023	0.031
	(0.018)	(0.020)	(0.021)	(0.016)	(0.017)	(0.019)
ELA			0.008	0.032		
			(0.008)	(0.026)		
$Mar21 \times ELA$				-0.026		
				(0.028)		
Unilateral	-0.002	-0.001	-0.001	-0.001	-0.010	-0.010
	(0.010)	(0.010)	(0.010)	(0.010)	(0.013)	(0.013)
N	96011	96011	96011	96011	88260	88260
Cohort Fixed Effect	✓	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓	✓
Year Fixed Effect	✓				✓	
State-Specific Year Trend	✓				✓	
Age Fixed Effect		✓	✓	✓		✓
State-Specific Cohort Trend		✓	✓	✓		✓
Ever-Married Restriction					✓	✓

Notes: See notes to Table 5. Unilateral indicates whether unilateral divorce laws were in place before age 21 (from Gruber [2004]).
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

Table A.2: Effects of Early Access on Education – Controlling for Marital History

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.			≥ High School Graduate (mean: 0.865)			
$Mar21$	0.048**	0.050***	0.047***	0.059***	0.051**	0.051***
	(0.021)	(0.017)	(0.017)	(0.019)	(0.020)	(0.016)
ELA			0.010	0.044**		
			(0.007)	(0.019)		
$Mar21 \times ELA$				-0.038*		
				(0.023)		
Ever Divorced	-0.043***	-0.043***	-0.043***	-0.043***	-0.043***	-0.043***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Never Married	-0.040***	-0.040***	-0.040***	-0.040***		
	(0.008)	(0.008)	(0.008)	(0.008)		
Panel B.			≥ Some College (mean:0.483)			
$Mar21$	0.067**	0.065**	0.064**	0.083***	0.070**	0.069**
	(0.029)	(0.030)	(0.029)	(0.026)	(0.030)	(0.030)
ELA			0.004	0.059*		
			(0.012)	(0.031)		
$Mar21 \times ELA$				-0.062*		
				(0.035)		
Ever Divorced	-0.045***	-0.045***	-0.045***	-0.045***	-0.047***	-0.047***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Never Married	0.028***	0.028***	0.028***	0.028***		
	(0.008)	(0.007)	(0.007)	(0.007)		
Panel C.			≥ Four Year College (mean: 0.237)			
$Mar21$	0.014	0.011	0.004	0.016	0.015	0.011
	(0.033)	(0.030)	(0.029)	(0.026)	(0.035)	(0.032)
ELA			0.020*	0.054**		
			(0.012)	(0.027)		
$Mar21 \times ELA$				-0.038		
				(0.031)		
Ever Divorced	-0.081***	-0.082***	-0.082***	-0.082***	-0.084***	-0.084***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Never Married	0.047***	0.047***	0.047***	0.047***		
	(0.011)	(0.011)	(0.011)	(0.011)		
N	30,268	30,268	30,268	30,268	27,435	27,435
Cohort Fixed Effect	✓	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓	✓
Year Fixed Effect	✓				✓	
State-Specific Year Trend	✓				✓	
Age Fixed Effect		✓	✓	✓		✓
State-Specific Cohort Trend		✓	✓	✓		✓
Ever-Married Restriction					✓	✓

Notes: Sample is the Limited CPS sample drawn from years 1980, 1985, 1990 and 1995. Further information in notes to Table 5. Ever divorce and never married are binary indicators for those categories.
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

Table A.3: Effects of Early Access on Occupation – Controlling for Unilateral Divorce Access

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.	Managerial and Professional Occupations (mean: 0.290)					
$Mar21$	0.027*	0.035**	0.029*	0.037**	0.033**	0.038**
	(0.016)	(0.015)	(0.015)	(0.016)	(0.016)	(0.015)
ELA			0.016	0.046		
			(0.010)	(0.030)		
$Mar21 \times ELA$				-0.033		
				(0.030)		
Unilateral	-0.010	-0.000	-0.001	-0.001	-0.018	-0.008
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)
Panel B.	High Professional Occupations (mean: 0.065)					
$Mar21$	0.011**	0.015***	0.013***	0.016**	0.010*	0.012***
	(0.005)	(0.004)	(0.005)	(0.007)	(0.006)	(0.004)
ELA			0.006	0.021		
			(0.005)	(0.015)		
$Mar21 \times ELA$				-0.016		
				(0.015)		
Unilateral	-0.003	0.002	0.002	0.002	-0.005	0.000
	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)
Panel C.	Doctors or Lawyers (mean: 0.006)					
$Mar21$	0.008**	0.008**	0.007*	0.007*	0.009**	0.009***
	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
ELA			0.003*	0.004		
			(0.002)	(0.004)		
$Mar21 \times ELA$				-0.001		
				(0.005)		
Unilateral	0.007***	0.006**	0.006**	0.006**	0.007***	0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
N	70,759	70,759	70,759	70,759	64,401	64,401
Cohort Fixed Effect	✓	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓	✓
Year Fixed Effect	✓				✓	
State-Specific Year Trend	✓				✓	
Age Fixed Effect		✓	✓	✓		✓
State-Specific Cohort Trend		✓	✓	✓		✓
Ever-Married Restriction					✓	✓

Notes: See notes to Table 6. Unilateral indicates whether unilateral divorce laws were in place before age 21 (from Gruber [2004]).
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

Table A.4: Effects of Early Access on Occupation – Controlling for Marital History

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.	Managerial and Professional Occupations (mean:0.313)					
$Mar21$	0.058**	0.053	0.049	0.058*	0.049*	0.043
	(0.027)	(0.032)	(0.031)	(0.031)	(0.027)	(0.034)
ELA			0.012	0.037**		
			(0.016)	(0.017)		
$Mar21 \times ELA$				-0.028		
				(0.022)		
Ever Divorced	-0.040***	-0.040***	-0.040***	-0.041***	-0.042***	-0.042***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Never Married	0.033**	0.032**	0.032**	0.032**		
	(0.015)	(0.015)	(0.015)	(0.015)		
Panel B.	High Professional Occupations (mean: 0.072)					
$Mar21$	0.024***	0.020**	0.021**	0.030**	0.017*	0.011
	(0.008)	(0.010)	(0.010)	(0.013)	(0.009)	(0.011)
ELA			-0.003	0.020		
			(0.010)	(0.014)		
$Mar21 \times ELA$				-0.026*		
				(0.015)		
Ever Divorced	-0.008	-0.008	-0.008	-0.008	-0.009	-0.009
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Never Married	0.030***	0.029***	0.029***	0.029***		
	(0.005)	(0.005)	(0.005)	(0.005)		
Panel C.	Doctors and Lawyers (mean: 0.008)					
$Mar21$	0.006	0.006	0.006	0.007	0.004	0.006
	(0.005)	(0.007)	(0.007)	(0.006)	(0.005)	(0.006)
ELA			0.002	0.006		
			(0.003)	(0.006)		
$Mar21 \times ELA$				-0.005		
				(0.007)		
Ever Divorced	-0.003**	-0.003**	-0.003**	-0.003**	-0.003**	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Never Married	0.003	0.003	0.003	0.003		
	(0.002)	(0.002)	(0.002)	(0.002)		
N	22,760	22,760	22,760	22,760	20,411	20,411
Cohort Fixed Effect	✓	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓	✓
Year Fixed Effect	✓				✓	
State Specific Year Trend	✓				✓	
Age Fixed Effect		✓	✓	✓		✓
State Specific Cohort Trend		✓	✓	✓		✓
Ever Married Restriction					✓	✓

Notes: Sample is the Limited CPS sample drawn from years 1980, 1985, 1990 and 1995. Further information in notes to Table 6. Ever divorce and never married are binary indicators for those categories.
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

III Results by Education: Less than College/College Graduate or More

Table A.5: Effects of Early Access on Marriage by Education

	(1)	(2)	(3)	(4)	(5)
	LPM: Married by Age 20				Duration Model: Marriage
Panel A. Less than College	(mean: 0.449)				
$Mar21$	0.044***	0.043***	0.046***	0.047***	0.068
	(0.011)	(0.014)	(0.014)	(0.016)	(0.042)
ELA			-0.008	-0.003	
			(0.011)	(0.030)	
$Mar21 \times ELA$				-0.006	
				(0.031)	
N	75,447	75,447	75,447	75,447	816,708
Panel B. College or More	(mean: 0.106)				
$Mar21$	0.030*	0.028	0.033**	0.024	0.086
	(0.017)	(0.017)	(0.015)	(0.017)	(0.073)
ELA			-0.017	-0.050***	
			(0.013)	(0.014)	
$Mar21 \times ELA$				0.036**	
				(0.016)	
N	20,564	20,564	20,564	20,564	27,4401
Cohort Fixed Effect	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓
Year Fixed Effect	✓				✓
State-Specific Year Trend	✓				✓
Age Fixed Effect		✓	✓	✓	
State-Specific Cohort Trend		✓	✓	✓	

Notes: See notes to Table 4.
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

Table A.6: Effects of Early Access on Occupation: Less than College

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.	Managerial and Professional Occupations (mean: 0.161)					
$Mar21$	0.035	0.032	0.030	0.029	0.039*	0.035*
	(0.021)	(0.019)	(0.019)	(0.021)	(0.021)	(0.019)
ELA			0.006	0.001		
			(0.009)	(0.019)		
$Mar21 \times ELA$				0.005		
				(0.019)		
Panel B.	High Professional Occupations (mean: 0.027)					
$Mar21$	0.008	0.010	0.009	0.007	0.014**	0.016***
	(0.007)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)
ELA			0.003	-0.005		
			(0.005)	(0.013)		
$Mar21 \times ELA$				0.009		
				(0.013)		
N	54,256	54,256	54,256	54,256	50,153	50,153
Cohort Fixed Effect	✓	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓	✓
Year Fixed Effect	✓				✓	
State-Specific Year Trend	✓				✓	
Age Fixed Effect		✓	✓	✓		✓
State-Specific Cohort Trend		✓	✓	✓		✓
Ever-Married Restriction					✓	✓

Notes: See notes to Table 6.
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

Table A.7: Effects of Early Access on Occupation: College or More

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.	Managerial and Professional Occupations (mean: 0.710)					
$Mar21$	-0.025	-0.015	-0.020	-0.019	-0.036	-0.027
	(0.024)	(0.034)	(0.035)	(0.038)	(0.027)	(0.037)
ELA			0.019	0.022		
			(0.019)	(0.045)		
$Mar21 \times ELA$				-0.004		
				(0.049)		
Panel B.	High Professional Occupations (mean: 0.191)					
$Mar21$	0.005	0.005	0.003	0.015	-0.017	-0.027
	(0.015)	(0.025)	(0.026)	(0.027)	(0.015)	(0.025)
ELA			0.009	0.049		
			(0.019)	(0.043)		
$Mar21 \times ELA$				-0.044		
				(0.048)		
Panel C.	Doctors or Lawyers (mean: 0.022)					
$Mar21$	0.025**	0.022	0.019	0.021*	0.024*	0.025
	(0.011)	(0.014)	(0.015)	(0.012)	(0.013)	(0.015)
ELA			0.010	0.014		
			(0.007)	(0.019)		
$Mar21 \times ELA$				-0.004		
				(0.020)		
N	16,503	16,503	16,503	16,503	14,248	14,248
Cohort Fixed Effect	✓	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓	✓
Year Fixed Effect	✓				✓	
State-Specific Year Trend	✓				✓	
Age Fixed Effect		✓	✓	✓		✓
State-Specific Cohort Trend		✓	✓	✓		✓
Ever-Married Restriction					✓	✓

Notes: See notes to Table 6.
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

Table A.8: Effects of Early Access on Fertility, by Education

	(1)	(2)	(3)	(4)	(5)	(6)
	1st birth within[a] x years of 1st marriage:				Birth>0	Duration Model:
	$x = 1$	$x = 2$	$x = 1$	$x = 2$		Birth Timing
Panel A. Less than College						
$Mar21$	-0.029*	-0.039*	-0.026	-0.039*	0.007	-0.024
	(0.016)	(0.021)	(0.017)	(0.021)	(0.012)	(0.044)
N	70,068	70,068	70,068	70,068	70,068	2,052,690
Mean of y	0.374	0.618	0.374	0.618	0.918	-
Panel B. College or More						
$Mar21$	0.016	0.030	-0.001	0.014	0.018	0.028
	(0.025)	(0.027)	(0.028)	(0.030)	(0.023)	(0.075)
N	18,192	18,192	18,192	18,192	18,192	497,915
Mean of y	0.160	0.331	0.160	0.331	0.829	-
Cohort Fixed Effect	✓	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓	✓
Year Fixed Effect	✓	✓			✓	✓
State-Specific Year Trend	✓	✓			✓	✓
Age Fixed Effect			✓	✓		
State-Specific Cohort Trend			✓	✓		

Notes: See notes to Table 7.
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

Table A.9: Effects of Early Access on Divorce: Less than College

	(1)	(2)	(3)	(4)	(5)
	Sample:				
	Baseline (1977-95)	Limited CPS (1980,85,90,95)			
	Married by 20		Ever Divorced	Divorce Timing: ≤3 years	Divorce Timing: Duration Model
Panel A.					
$Mar21$	0.044***	0.048**	0.073**	0.025*	0.249**
	(0.012)	(0.019)	(0.031)	(0.013)	(0.102)
Unilateral	0.002	0.019	-0.003	0.007	-0.015
	(0.012)	(0.015)	(0.016)	(0.009)	(0.069)
Year Fixed Effect	✓	✓	✓	✓	✓
State-Specific Year Trend	✓	✓	✓	✓	✓
Panel B.					
$Mar21$	0.045***	0.045*	0.083**	0.028	0.289***
	(0.015)	(0.023)	(0.034)	(0.018)	(0.111)
Unilateral	0.007	0.020	-0.002	0.004	-0.015
	(0.013)	(0.019)	(0.019)	(0.010)	(0.075)
Age Fixed Effect	✓	✓	✓	✓	✓
State-Specific Cohort Trend	✓	✓	✓	✓	✓
Panels A and B.					
N	75,447	23,097	21,143	21,143	331,438
Mean of y	0.449	0.422	0.374	0.062	-
Cohort Fixed Effect	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓
Ever-Married Restriction			✓	✓	✓

Notes: See notes to Table 8.
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.

Table A.10: Effects of Early Access on Divorce: College or More

	(1)	(2)	(3)	(4)	(5)
	Sample:				
	Baseline (1977-95)	Limited CPS (1980,85,90,95)			
	Married by 20		Ever Divorced	Divorce Timing: ≤3 years	Divorce Timing: Duration Model
Panel A.					
$Mar21$	0.024	0.002	-0.033	0.033	-0.002
	(0.016)	(0.025)	(0.044)	(0.024)	(0.212)
Unilateral	-0.025*	-0.021	0.028	0.003	0.136
	(0.014)	(0.018)	(0.026)	(0.015)	(0.139)
Year Fixed Effect	✓	✓	✓	✓	✓
State-Specific Year Trend	✓	✓	✓	✓	✓
Panel B.					
$Mar21$	0.026	0.006	-0.007	0.048**	0.111
	(0.016)	(0.032)	(0.061)	(0.022)	(0.246)
Unilateral	-0.010	-0.011	0.035	0.001	0.167
	(0.012)	(0.017)	(0.029	(0.019)	(0.155)
Age Fixed Effect	✓	✓	✓	✓	✓
State-Specific Cohort Trend	✓	✓	✓	✓	✓
Panels A and B.					
N	20,564	7,171	6,292	6,292	88,070
Mean of y	0.106	0.099	0.266	0.045	-
Cohort Fixed Effect	✓	✓	✓	✓	✓
State Fixed Effect	✓	✓	✓	✓	✓
Ever-Married Restriction			✓	✓	✓

Notes: See notes to Table 8.
* significant at 0.1; ** significant at 0.05; *** significant at 0.01.