DISCUSSION PAPER SERIES

IZA DP No. 5468

Pill Power: The Prequel

Lena Edlund
Cecilia Machado

January 2011

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

Pill Power: The Prequel

Lena Edlund
Columbia University and Cornell University

Cecilia Machado
Getulio Vargas Foundation (EPGE-FGV)
and IZA

Discussion Paper No. 5468
January 2011

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

IZA Discussion Paper No. 5468
January 2011

ABSTRACT

Pill Power: The Prequel*

Goldin and Katz [2002], in an influential paper, argued that giving *unmarried* minors access to the contraceptive Pill was instrumental for women's professional advancement, because such access allowed marriage to be postponed. However, by 1960, married women could get the Pill and thence it is not clear why early marriage would interfere with the pursuit of professional interests. We explore the effects of this alternative, earlier, and common, route to the Pill. Using variation in state minimum-age marriage laws (EMA), we find that EMA precipitated marriage, delayed fertility within marriage, and improved the educational and occupational outcomes of women, especially non-college women. Thus, fertility control, marriage notwithstanding, emerges as a key enabler of women's educational and professional advancement.

JEL Classification: J13, J24

Keywords: contraceptive pill, marriage, education, occupation

Corresponding author:

Cecilia Machado
Fundação Getúlio Vargas
Praia de Botafogo 190, s.1102
Rio de Janeiro, RJ 22250-900
Brasil
E-mail: machadoc@gmail.com

* We have benefitted from discussions with Douglas Almond, Martha Bailey, Janet Currie, Rajeev Dehejia, Melanie Guldi, Mariesa Herrmann, Dan Hungerman, Ted Joyce, Robert Pollak and seminar participants at Clemson University, Yale University, IPEA-RJ, IBMEC-RJ, EESP-FGV and the 2011 AEA Annual Meeting.

1 Introduction

The invention of the contraceptive Pill and its FDA approval in 1960 marks a watershed in social history. The Pill was the first female controlled, safe, and cheap, contraceptive. It had the potential to greatly enhance women's control over fertility and allow for the separation of sexual activity from the demands of child bearing and rearing – until then the preserve of men. Arguably, the ensuing sexual revolution was driven by women's, not men's, greater willingness to engage in non-committal sex. In short, women became more promiscuous. The world did not fall apart. It was not the same either.

The extent and the mechanics of the role of the Pill in shaping post-1960s society is a matter of debate. A priori, if women were willing to be promiscuous, this could have far reaching consequences for gender relations [Trivers, 1972, Maynard Smith, 1977], including women's presence in the labor force.

The possibility that the Pill could be behind the marked gains women have made in the labor market in the last half century remained relatively unexplored until Akerlof et al. [1996]'s paper pointing to the possibility of female controlled contraceptives leading to reduced male transfers. Empirically, they linked abortion access to the decline in so called shot-gun marriages, until then the main form of payment for premarital sex, the authors argued. Faced with this new reality, children on your own, or no children, the possibility that women might orient themselves more seriously to the labor market is not far behind.

This proposition was explored by Goldin and Katz [2002] (henceforth GK) in their influential paper "The Power of the Pill", where they argued that

the Pill enabled women to pursue careers previously beyond reach because the marriage market dictated early marriage and early marriage precluded a career. The time demands of children – who in the pre-Pill days tended to follow on the heals of married life – is a candidate reason for the latter.[1] Pill access to *unmarried* women, they argued, allowed women to delay marriage without the cost of abstinence or a compromised pool of eligible young men.

The role of Pill access by *unmarried* minors, henceforth Early Legal Access (ELA), has since been explored for a number of outcomes: delayed fertility [Bailey, 2006, 2009, Guldi, 2008, Bailey, 2010]; women's educational outcome [Hock, 2007, Ananat and Hungerman, Forthcoming]; men's educational outcome [Hock, 2007]; and children's outcomes [Ananat and Hungerman, Forthcoming, Pantano, 2007].

In this paper, we explore a previously ignored but in no way obscure, route to the Pill: marriage.[2] Through marriage, a minor was considered emancipated for the purposes of contraceptives. At the time of the Pill's FDA approval, some two-thirds of states allowed 18-year old women to marry without parental consent, and early marriage was no rare occurrence. For most of the 1960s, the median age of first marriage for women was 20.[3] Before the Pill, early marriage may have precluded investments in human capital, presumably because of the imminent arrival of children. However, after 1960, married women had access to the Pill and its widely advertised contraceptive properties [Watkins, 1998]. Moreover, as emphasized by the

[1] In 1976, Nebraska became the first US state to recognize rape in marriage.

[2] The theoretical possibility of which has been noted by, *inter alia*, Chiappori and Oreffice [2008].

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

ELA literature, unmarried women were denied the same. Thus, it is at least a theoretical possibility that marriage served as a conduit to the Pill and that their combination allowed for the pursuit of professional goals. In addition to fertility control, marriage could have provided financial support, particularly important in a time of relatively limited student aid.

To investigate this hypothesis, we use the fact that states converged on a minimum-age of marriage of 18 in the 1960s and 1970s, thus providing a source of state-cohort variation in legal access through marriage. The typical change was from 21 to 18, and we consider the effect of "Early Marriage Access" (EMA), as defined by being able to marry without parental consent before age 21. We combine these law changes with data on women's marital, educational and occupational and fertility outcomes from the June CPS covering the period 1977-1995. We focus on women 36-44 years old when surveyed and born in the period 1935-1959.

We study the effects of EMA laws, along with ELA and abortion laws, all of which changed around the time the Pill became available. We look separately at the effect of these laws on college and non-college women. A reason for this is that the argument and analysis of GK focused on college women. There is, however, *a priori* little reason why Pill access would not extend to non-college women and, in that spirit, the subsequent literature has included non-college women as well. But in doing so, it has not distinguished the two groups, implicitly assuming that the effects documented by GK extend to non-college women as well. However, as we shall see, the separate analysis of college and non-college women reveals interesting heterogeneity.

While we confirm GK's findings of ELA effects for college educated women,

these effects do not extend to the some 3/4 of women in our sample short of a four-year college degree.

By contrast, for non-college women, we find that EMA both precipitated marriage and significantly improved their occupational outcomes. By age 22, EMA increased marriage by 3.7 percentage points, or a 5 percent increase. By mid-age, EMA had raised the probability of being in a professional or managerial position by 3.2 percentage points, or a 20 percent increase.

Since we condition our analysis on college graduation status, we also examine the effects of EMA on educational attainment. We find that EMA raised the probability of some college (e.g., an associate degree) by 4.7 percentage points, a 11 percent increase, but had no effect on college graduation. Thus, the found differences between college and non-college women do not appear to derive from EMA induced change in selection.

Early marriage may also spell early divorce, and divorce itself can impact educational and labor market outcomes. First, we consider divorce as an outcome and find that EMA does predict early divorce.

Second, to probe whether our education and occupation results are confounded, or mediated, by divorce, we pursue two strategies. First, we control for whether the woman resided in a state with unilateral divorce. Second, we include information on divorce history available for some CPS years. While divorce (measured either way) did have an independent effect on educational and occupational outcomes, our results for EMA were strengthened, which we interpret as evidence that the found EMA effects are neither confounded nor driven by divorce.

Tangential to our focus, but of some interest in its own right, we also

found unilateral divorce to precipitate marriage, confirming the adage that barriers to exit are barriers to entry (and suggested by Gruber [2004]).

Lastly, we present evidence that EMA extended the gap between marriage and first birth, strengthening the case for EMA leading to Pill use. Furthermore, the effect is more pronounced for non-college women, the group for whom EMA had a greater effect on marriage timing and educational and occupational outcomes.

In sum, while the previous literature has stressed Pill access to *unmarried* women and postponement of marriage, we point to marriage itself as an earlier route to the Pill, and argue that such access was both used and materially contributed to the educational and occupational upgrading of American women reaching adulthood in the 1960s and 1970s. Thus, our paper strengthens the case for the pivotal role of female fertility control, with or without marriage, for women's labor market outcomes.

The remainder of the paper is organized as follows. The next section provides a brief background. Section 2.2 discusses the laws. Section 3 presents our data and results. Section 4 concludes.

2 Background

This section provides a brief background (which can be skipped without loss of context by those already familiar with the literature).

2.1 Literature Review

Following Goldin and Katz [2002], a number of papers have studied the impact of the Pill on fertility [Bailey, 2006, 2009, Guldi, 2008, Bailey, 2010, Miller, 2010] (for a dissenting view, see Joyce et al. [2011]), women's empowerment [Hock, 2007, Ananat and Hungerman, Forthcoming, Miller, 2010], and child outcomes [Ananat and Hungerman, Forthcoming, Pantano, 2007]. Most of these papers have focussed on ELA and none has considered Pill access through marriage.

GK focussed on women with four-year college and argued that ELA greatly increased women's representation among medical doctors and lawyers, professions that requires high up-front investments in time consuming and otherwise demanding training. Empirically they showed that cohort variation in ELA predicted fraction doctors and lawyers among college graduate women in the 1970, 1980 and 1990 censuses (ages 30-49 years old, cohorts 1921-1960).

But there is no reason the Pill's effects would be limited to women with a four-year college degree (henceforth college), or why state variation in access laws would not be exploited, and subsequent papers have relaxed these restrictions, but has typically not separated out college and non-college women.

Bailey [2006] linked variation in ELA to women's greater labor force attachment. The paper claimed that ELA substantially delayed first birth, supporting a straightforward link from Pill access to labor force participation. While the strength of this additional piece of evidence was later found to be based on coding error (the estimated fertility effect of ELA was adjusted down from 9 percent to 1.5 percent [Bailey, 2009, Table II]), the notion

that the Pill would impact fertility is, however, quite plausible. In a later paper, Bailey [2010] showed that marital fertility was affected by Pill access, where the variation in Pill access derived from the so called Comstock laws.

Bailey [2006]'s findings of delayed but not reduced lifetime fertility and positive effects on labor force attachment were echoed in Miller [2010]'s study of the roll-out of family planning services in Colombia. He found contraceptive access to have had a limited impact on completed fertility, the main effect being on timing. Women with family planning access were also found to have better educational and labor market outcomes.

Hock [2007] used "late adolescent contraceptive consent", similar but not identical ELA laws as in GK and Bailey [2006], and found that women ages 21 and 22 were more likely to be currently enrolled in college, using the CPS, October supplement, years 1968-1979. He looked at BA completion using the 1990 and 2000 censuses, cohorts 1940-1959, and found that "late adolescent contraceptive consent" led to 0.7 percentage point (and statistically significant) higher BA completion rate for women. For men, he found no effect unless lagged, consistent with absence of a direct effect on men (or Pill access proxying for other time and state varying factors), any effect being through the partner market.

A few studies have looked at the possibility of Pill access to unmarried minors affecting child outcomes, and as with abortion access, e.g., Gruber et al. [1999], the argument hinges on selection effects. Ananat and Hungerman [Forthcoming] found that in the long term, mothers became more positively selected, thus improving child outcomes. In the short term, child birth became negatively selected. Their main data set was the 1970/80/90 censuses.

Pantano [2007] looked at criminality of the cohorts whose mothers had Pill access as minors, and found negative effects on crime.

Joyce et al. [2011] cast a critical eye on the "ELA-literature." First they note that the relationship between ELA and teen pill use is weak. In fact, ELA is only significant and of the expected sign using the coding of ELA employed by GK. That is, the ELA coding employed by Bailey [2006], Guldi [2008], Ananat and Hungerman [Forthcoming] respectively do not replicate GK's finding, using the same data set: the National Survey of Young Women 1971, thus calling into question the existence of a first stage for their studies. Second, they argued that Guldi [2008]'s finding that ELA reduced fertility (among 15-21 year old women) unravels once the effect of Pill access is allowed to vary by age. In other words, once the comparison is done within state, holding age constant, there is no discernible effect of Pill access. Third, they showed that the crude coding of abortion access resulting from assuming that abortion was unavailable to a woman until it was legalized in her state is incorrect and at the minimum understates the role of abortion for fertility. While abortion was legalized nationally in 1973, abortion was effectively available from 1970 for those willing to travel to a state that had legalized abortion, notably New York.

Our paper also adds to the recent literature that has focussed on the effect of marriage laws. Blank et al. [2009] (whose coding we use), argued that for marriage age, survey data may be more accurate than administrative data because of slippage in adherence resulting from jurisdiction shopping (residents of state X getting married in state Y) or lax enforcement. Two papers have used stringency (e.g., blood test, proof of age) in requirements

to look at effects of making the marriage procedure more or less costly. Perhaps unsurprisingly, more obstacles reduce marriage rates among the young. Whereas both papers found low socio-economic groups to be more affected, depending on the period studied, the incidental effects varied. Buckles et al. [Forthcoming], looked at state repeals of blood test requirements in the period 1980 to 2005 and found that such requirements reduced the number of marriages and increased the out-of-wedlock fertility rate among first time mothers. Bharadwaj [2009] looked at the opposite experiment, more stringent requirements in Mississippi enacted in 1960. Higher cost of marriage led to a decline in marriage rates among 19-23 year old women. Interestingly, lower marriage rates were also accompanied by a decline in fertility and a rise in educational attainments, perhaps symptomatic of the pre-ELA era when love and marriage supposedly went together.

2.2 State Laws

During the 1960's and 1970's many states enacted laws empowering young adults with respect to several rights. The legal changes were largely motivated "by the enhanced awareness, due in part by the Vietnam War, that young people had earned greater rights" GK (page 764). The 26th Amendment in 1971 lowered the voting age to 18. In its wake, many states extended additional rights to young adults by, for instance, lowering the age of majority, recognizing the mature minor doctrines allowing minors to consent to medical treatment, and creating family planning statutes allowing physicians to treat minors [Bailey, 2006].

The trend towards giving young adults more extensive rights also ex-

tended to marriage. Marriage emancipated a women with respect to 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 1975, all but three states had lowered the minimum marriage age for women (without parental consent) to 18 [Blank et al., 2009].

Thus, marriage constituted a route to Pill access for women younger than 21 years before the mature minor doctrine and family planning statutes paved the way for contraceptive access for unmarried minors. Or at least to the extent allowed by the so called "Comstock Laws," laws prohibiting the sales and distribution of obscenity material [Bailey, 2010]. These obstacles were removed with the 1965 landmark decision of *Griswold v. Connecticut*, 381 U.S. 479 (1965), whereby married couples' right to privacy was affirmed. Unmarried women's right to Pill use was affirmed in the 1972 *Eisenstadt v. Baird*, 405 U.S. 438 (1972), Supreme Court decision (based on the Equal Protection Clause of the Fourteenth Amendment).

3 Data and Results

Our main data set uses the Marriage and Fertility Supplement of the Current Population Survey (CPS) administered in the month June of selected years. Information on dates (month and year) of first marriage and first birth is consistently available for all females of childbearing age (18-44) for the years 1977, 1979, 1980, 1981, 1982, 1983, 1985, 1986, 1987, 1988, 1990, 1992 and 1995, which we will short hand as June CPS 1977-1995. We restrict the sam-

ple to women age 36-44, born between 1935 and 1959. 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. More information on data and variables are in the Appendix.

We estimate a model of the form:

$$Y_{isc} = \beta_M MAR_{isc} + \beta_E ELA_{isc} + \beta_A A_{isc} + \alpha + \alpha_s + \alpha_c + \alpha_s \times c + \gamma \times X_{isc} + \epsilon_{isc} \quad (1)$$

Y_{isc}	1 if outcome is 1 for individual i, of cohort c, in state s.
MAR_{isc}	1 if i could marry before age 21.
ELA_{isc}	1 if i had Pill access before age 21.
A_{isc}	1 if i had Abortion access before age 21.
α_s, α_c	state and cohort fixed effects.
$\alpha_s \times c$	state specific cohort trend.
X_{isc}	race indicators (Black, other non-White).

All regressions use the June CPS survey weights and have standard errors clustered at the state level.

We define ELA and EMA as the right to obtain the Pill, or to marry, without parental consent before the age of 21, respectively. Those variables exhibit state by cohort variation, and the coefficients on them in equation (1) are identified by states and cohorts that experienced a law change. Table 1 describes the changes in the minimum marriage age (from Blank et al. [2009]) defined as the minimum age a women could marry without parental

consent[4]. The ELA coding is from Bailey [2006, table 1], and corresponds to the minimum age an unmarried women could have access to the Pill through age of majority laws, family planning statutes or mature minor doctrines.

Minimum age restrictions 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]. And many minors could bypass marriage laws by either misreporting their age, or by getting married in a state with a lower age requirement, without rendering their marriage invalid [Blank et al., 2009]. However, such slippage would work against finding effects of ELA and EMA.

Following the literature, we also control for abortion access. Access is assumed for all cohorts reaching age 20 after the *Row v. Wade* 1973 decision, and for cohorts reaching age 20 after 1970 in Alaska, California, Hawaii, New York or Washington [Levine et al., 1996].

Figure 1 depicts the evolution of the various rights for women younger than 21.

[4]The fixed effect strategy explores mostly changes of minimum marriage age from 21 to 18 (the two exceptions are Nebraska, with a 21 to 20 change, and West Virginia, with a 21 to 16 change). The few other laws changes in Table 1 not explored in our fixed effect strategy either include cases of changes that are subsequently reversed over a short period of time (Georgia in 1965 and 1972, Montana in 1971 and 1973, Iowa in 1972 and 1973, and Alaska 1974 and 1975) or law changes that always encompass the 18 (South Carolina in 1957 and Mississippi in 1958), the 19 (Wyoming in 1975) or the 20 (Hawaii in 1969 and Nebraska in 1972) marriage age. Sensitivity analysis to these other laws changes – such as re-defining EMA as the right to marry before ages 20 and 19, for example – rendered the results largely unchanged.

3.1 Marriage

We start by investigating whether allowing women to marry before age 21 induced women to marry earlier by estimating Equation 1 for the event that a woman was married by a specific age.

The typical change was a lowering of the minimum age from 21 to 18, but there are reasons to look outside this age window. As mentioned, we use the minimum age of marriage without parental consent, and most states allowed for earlier marriage in case parents also consented. Thus, the lowering of the minimum age from, 21 to 18 could have had an effect on the marriage age of those younger than 18 since the fact that a 17 year old would be only a year away from marrying anyway may have influenced parents' willingness to consent. Moreover, marriage among those younger 21 may have precipitated marriages among those older than 21.

Therefore, the outcomes we consider are whether married by age 18, 19,...,25. That is, Y_{isc} in Equation 1 is modified to read $Y(x)_{isc}, x \in \{18, ...25\}$.

Table 2 present the results. Panel A. presents results for all women, and we see that EMA is associated with a 3.6 percentage point, or 10%, higher probability of being married by age 20. ELA, in contrast, is associated with a reduction in the probability of being married, significant for ages 19, 22 and 23. Breaking the sample into college and non-college women reveals interesting heterogeneity: the precipitating effect of EMA is driven by non-college women (Panel B.), EMA raises the probability of being married by ages 20 through 25, and the effect size ranges from 2 to 4.6 percentage points, which corresponds to a 10% rise in the probability of being married by age 20. For this sub-sample, there is no discernible effect of ELA.

For college women (Panel C), EMA has a much more limited effect, only significant by age 20. By contrast, ELA is associated with a substantial reduction in the probability of being married by ages 22 and 23, 4.4 and 4.8 percentage points respectively, or some 10-15% (consistent with GK's findings using the 1980 Census).

Recall that the effect of EMA is identified off states that changed the consent requirements for a 20 year old in the period 1955-1979. Nine states began to allow women younger than 21 to marry in this period, and the changes took place after 1960 (Table 1), that is, after the Pill had gained FDA approval. Clearly, minimum age laws may have been binding for other reasons than Pill access, but Pill access is a candidate reason for our finding of precipitated marriage.

We also estimate the probability that a woman was ever married. Since the age of women at the time of the CPS ranges from 36 to 44, we also include a vector of age dummies. The results are in column 9. Neither EMA nor ELA has any effect on whether ever married. This finding is consistent with EMA and ELA changing the timing but not the eventual probability of marriage. About 90 percent of women in the sample are ever married. Going forward, we focus our analysis on them, restricting our sample to ever married women.

3.2 Educational and Professional Outcomes

Ultimately, we are interested in the role of the Pill for women's labor market advancements, and we now turn to the question whether early access laws affected women's educational and occupational outcomes by mid-age (ages

36-44). Since the outcome may depend on the age of the respondent, we augment Equation 1 to include a vector of age dummies.

We start with educational outcomes. In addition to being an important indicator of potential earnings, educational outcomes are also of interest because of the divide we have found between college and non-college women.

We estimate Equation 1 where the dependent variable is whether a woman (at the time of the survey) had completed high school or more, some college or more, or four-year college or more. The results are in Table 3. There is a statistically significant effect of EMA on the probability that a woman had some college or more. The effect is, however, confined to some college, since neither high school or more, nor four-year college or more, margins were affected. Thus, while EMA resulted in educational upgrading among non-college women, there appears to have been no effect on the college/non-college margin. ELA has no effect.

Next we turn to occupational outcomes. Following GK, we are particularly interested in occupations that indicate attachment to the labor force. In addition to the categories studied by GK – professional occupations, excluding teachers and nurses (High Professionals) and doctors/lawyers – we also look at women in managerial positions and women in all professional occupations (as defined by the CPS, see Data Appendix). This expansion is particularly relevant for women with only some college. While not as selective as doctors and lawyers (0.5 percent of women reporting an occupation), these other occupations are still selective. Among women reporting an occupation, only 11 percent were in a managerial occupation, and 18 percent were in a professional occupation. An important difference between managerial and

professional occupations is that the latter tends to require specific training, whereas managerial positions are more task defined. For instance, funeral director is a managerial occupation while nurse is a professional occupation.

Table 4 reports results from estimating Equation 1 for the probability that a woman reports her occupation to be one of the following: manager or professional, manager, professional, high professional, doctor/lawyer, as well as whether reporting an occupation at all (i.e., she is in the labor force). Panel A. reports the results for all women, and panels B. and C. report non-college and college women separately. For all women, we see that EMA raises the probability that a woman is in a professional or managerial occupation (columns 1, 2 and 4), and both EMA and ELA raise the probability of her being a doctor or lawyer (column 5). Again, looking separately at non-college and college women reveals interesting differences.

For non-college women, there is a strong effect of EMA on probability of being in a managerial and professional occupation, some 3 percentage points, or a 20 percent increase (column 1), and on being in a high professional occupation, a 1.5 percentage point effect, or and increase of almost 60 percent (column 4). By contrast, there is no effect of ELA.

By contrast, for college women, we replicate GK's findings of ELA raising the proportion doctors and lawyers (our specification exploits cohort-state variation, not just cohort variation, and includes state specific cohort trends). For this sample, we find no EMA effects.

Finally, the early access laws have no effect on probability of reporting an occupation (column 6). In other words, these laws cannot explain the rise in female labor force participation seen in the study period.

3.3 Mediating Mechanisms

If early access through marriage leads to better occupational outcomes, especially for non-college women, we may ask what mechanisms may have been involved. Traditionally, marriage has led to a specialization away from market work for women. Of course, with the Pill, this relationship might have been upended or at least modified, since children are no longer a foregone conclusion. Before turning to fertility timing, we start by looking at divorce. If non-college women married earlier, their marriages may also have been less stable, which may prompt women to focus on the labor market, either in anticipation of, or following, divorce, e.g. Johnson and Skinner [1986].

3.3.1 Divorce

A number of states adopted unilateral divorce in the 1970s and 1980s, a factor that may both have made divorce and marriage easier (see e.g., Gruber [2004]). Therefore, it is of interest to consider divorce access, both as an explanatory variable of some independent interest and as a robustness check on our EMA results (e.g., states that introduced EMA may also have been early unilateral states, and unilateral divorce is widely believed to have discouraged traditional division of labor in the family, with concomitant implications for women's labor supply, e.g., Stevenson [2007]).

We make use of the fact that for 1980, 1985, 1990, and 1995, the June CPS includes information on termination date of the first marriage (not just current marital status) to look at whether early access relates to the probability of divorce. Termination can be because of divorce, widowhood or separation, but for the ages considered, widowhood is an unusual event,

and the category is dominated by divorce and separation. For brevity, we will refer to termination for any reason as "divorce."

Table 5 presents the results from estimating Equation 1 augmented with an indicator variable for whether the individual had unilateral divorce access in the year prior to the event in question, following Gruber [2004, table 1]. The events are: married by 22, ever divorced, had divorced by age 25, 30 and 35 respectively. For example, for marriage by 22, we use unilateral access by 21. When considering ever divorced, we include age dummies in the regression and define unilateral access as pertaining to the year prior to the survey. Except for the first outcome (columns 1 and 2), we condition on ever married. For comparison, column 1 presents results from estimating the probability of marriage before age 22 (which does not require information on divorce date) on our June CPS 1977-1995 baseline sample.

Interestingly, unilateral access raises the probability of having married before age 22 in both CPS samples (columns 1 and 2) and the effect is concentrated among non-college women (Panel B.). In the limited CPS sample (Panel B., column 2), the effect size is 4.2 percentage points, or a 7% increase. The EMA effect is, however, strengthened by this inclusion. For the baseline sample (Panel B., column 1), EMA is associated with a 4.5 percentage point increase in the probability of having married by 22 (or a 20% stronger effect than in the specification excluding unilateral divorce access, cf. Table 2, column 5).

For non-college women, EMA not only raises the probability of early marriage, but also of early divorce. For instance, EMA almost doubles the probability of having divorced by 30 (Panel B., column 5). While unilateral

divorce is strongly associated with first marriage ending in divorce, unilateral divorce does not predict divorce by any of the ages considered (25, 30, and 35).

Among college women (Panel C.), unilateral divorce access more than doubles the probability of being ever divorced, but there is also no effect on divorce (or marriage) at the specified ages.

Consistent with the notion that early marriage raise divorce risk, ELA is found to reduce divorce risk by age 30 (college women) and 35 (non-college women).

In sum, EMA raised probability of early marriage and divorce among non-college women. Could it be the case that the positive effects of EMA on educational and occupational outcomes among this group was driven by divorce rather than marriage (with Pill access)? To investigate this question, we re-estimate the education and occupational outcomes regressions including an indicator variable for unilateral divorce access.

Table 6 presents results for education for all women. Panel A. shows results when access to unilateral divorce (before age 21) is added to the regression and we see that our previous findings are robust to this inclusion, although the point estimate (and significance) of the effect of EMA on some college or more (column 2) is reduced to 3.6 percentage points from 4.7 percentage points. In the next panel, we make use of the limited CPS sample with marital history information and instead of using unilateral divorce law, we check if our EMA results are robust to the inclusion of indicator variables for first marriage ending in divorce. While this control is highly significant, the results for EMA in this limited sample are actually strengthened (Panel

C.). For instance, the effect of EMA on some college is now 6.7 percentage points, or a 14% increase from mean rate (column 2).

Turning to the occupational outcomes, Tables 7, we see that results are virtually unchanged by the inclusion of an indicator variable for unilateral divorce access (by age 21), with the exception that the effect of ELA on the probability of being a doctor or lawyer loses significance at conventional levels (Panel C, column 6).

As an additional robustness check, we next include an indicator variable for whether the first marriage ended in divorce, Table 8. Our previous results are now strengthened: EMA promotes the occupational outcomes of non-college women, and we find no effect of ELA.

We also note that divorce is a strong predictor of labor force participation (reporting an occupation), and the effect on the fraction women reporting a managerial occupation is positive but that on a professional occupation is negative. One interpretation is that on divorce, women who had not planned to, joined the labor force. If these women find it difficult to go back to school (e.g., because of age or financial constraints), their entry into the workforce reduces the fraction working women in professional occupations (which require specific training). However, lack of professional training may not preclude advancement to managerial positions, and the experience of divorce may direct women towards the labor market.

The effect of divorce on labor force participation is much stronger among college than non-college women. First marriage ending in divorce leads to a 7.8 percent increase in probability of reporting an occupation for non-college women, and a 12.1 percent increase among college women. A larger effect

on labor force participation among college women is consistent with labor supply of skilled women having increased disproportionately in the 1970s and 1980s [Juhn and Murphy, 1997], possibly as result of the increase in the returns to skills [Katz and Murphy, 1992, Juhn et al., 1993].

In sum, while divorce did have strong independent effects on marital outcomes and labor force participation, divorce does not appear to have mediated the effects of EMA on educational and occupational outcomes. Moreover, controlling for divorce renders the effect of ELA on probability of a woman being a doctor or lawyer insignificant.

3.3.2 Fertility

Finally, we revisit the question of effects on fertility of early access laws. As pointed out by Bailey [2006], lower and delayed fertility strengthens the argument that the Pill was indeed an important catalyst for the educational and occupational changes. In view of our finding that EMA improved both the educational and occupational profile of women, non-college women in particular, we are interested in whether there were also fertility effects and, if so, whether they were concentrated among non-college women.

Table 9 reports results from estimating Equation 1 where the outcomes are: a first birth within x, $x \in \{1, 2, 3, 4, 5\}$, years of marriage, and whether the woman has had a birth. A reason for our interested in the timing of first birth relative to first marriage is that the married-without-kids window may be particularly propitious for human capital accumulation. Marriage can provide financial support, and absent children, homemaking need not be time consuming. Again, we restrict the sample to ever married women.

In Panel A., we present results from the whole sample, college and non-college women combined. We see evidence of EMA delaying fertility, statistically significant for births within the first two years of marriage, consistent with women marrying and getting the Pill. While the sign of the coefficient on ELA tends to be negative, it is highly insignificant.[5]

Breaking down the results by non-college and college women, we see that the coefficient on EMA is consistently negative for non-college women and border-line significant for a first birth within two years of marriage (Panel B, column 2). By contrast, for college women (Panel C.), the coefficient on EMA is positive and far from significant. ELA has no effect.

4 Discussion

In an influential paper, Goldin and Katz [2002] (GK), emphasized the role of unmarried women's access to the contraceptive pill – through so called Early Legal Access (ELA) – for enabling women to enter high-powered professions. However, the majority of young women had "early access" years before ELA: they could marry. And unless marriage annihilates the benefits of fertility control offered by the Pill, access through marriage could also have contributed to the educational and professional upgrading of American women who reached adulthood after the Pill's FDA approval in 1960.

In this paper, we have explored this previously ignored route to Pill access and presented evidence that such access before age 21 – "Early Marriage Access" (EMA) – precipitated marriage, especially among women with less

[5]Since we found ELA to postpone marriage, Table 2, and our birth event is relative to marriage age, our findings are consistent with Bailey [2006, 2009].

than a four-year college degree (some three-quarters of women) *and* lead to better educational and professional outcomes among these women. Whereas we also find that early marriage predicts early divorce, and divorce can have an independent effect on women's labor market outcomes, divorce does not appear to be behind the educational and occupational gains associated with EMA.

Early marriage has generally not been considered conducive to human capital investments, as stressed by GK, alone a reason to believe the found positive effects of EMA are Pill related. Delayed fertility provides additional evidence. We find EMA to be associated with a postponement of marital fertility, and the effect is more pronounce among non-college women, the group which also saw a positive effect of EMA on educational and occupational outcomes.

Since most of the literature has focussed on access to unmarried women younger than 21, Early Legal Access (ELA), all our regressions have also controlled for ELA. With respect to Pill access, ELA substitutes for marriage, and we confirm previous findings of ELA delaying marriage (and resulting in more stable marriages), but having limited impact on educational and occupational outcomes save doctors and lawyers. However, for this one group, the effect disappears once controls for divorce are included (unilateral divorce law or individual marital history); whereas the effects of EMA are robust to their inclusion.

In sum, our paper has extended the existing literature by considering a hitherto empirically ignored access route to the Pill open to the majority of 18-year old women already in 1960 – early marriage – and showed evidence

that such access contribute to the educational and occupational upgrading of American women, marriage notwithstanding.

References

George Akerlof, Janet Yellen, and Michael Katz. An analysis of out-of-wedlock childbearing in the United States. *Quarterly Journal of Economics*, 111(2):277–317, May 1996.

Elizabeth Oltmans Ananat and Daniel M. Hungerman. The power of the pill for the next generation. *Review od Economics and Statistics*, Forthcoming.

Martha Bailey. More Power to the Pill, Quarterly Journal of Economics, February 2006: Erratum and Addendum. August 25 2009. URL `http://www-personal.umich.edu/~baileymj/Erratum_draft_8_22_09.pdf`.

Martha J. Bailey. More power to the pill: The impact of contraceptive freedom on womens life cycle labor supply. *The Quarterly Journal of Economics*, 121(1):289–320, February 2006.

Martha J. Bailey. Momma's Got the Pill: How Anthony Comstock and Griswold v. Connecticut Shaped U.S. Childbearing. *American Economic Review*, 100(1):98–129, March 2010.

Prashant Bharadwaj. Impact of changes in marriage law: Implications for fertility and school enrollment. University of California, San Diego, 2009.

Rebecca M. Blank, Kerwin Kofi Charles, and James M. Sallee. A cautionary tale about the use of administrative data: Evidence from age of marriage laws. *American Economic Journal: Applied Economics*, 1, 2009.

Kasey S. Buckles, Melanie Guldi, and Joseph Price. Changing the price of

marriage: Evidence from blood test requirements. *The Journal of Human Resources*, Forthcoming.

Pierre-Andre Chiappori and Sonia Oreffice. Birth control and female empowerment: An equilibrium analysis. *Journal of Political Economy*, 116 (1):113–130, February 2008.

DHEW. Family planning, contraception, and voluntary sterilization: an analysis of laws and policies in the United States, each State and jurisdiction (as of September 1971): a report of the National Center for Family Planning Services, Health Services and Mental Health Administration. DHEW publication no. 74-16001, Department of Health and Economic Welfare Center for Family Planning Program Development (Planned Parenthood - World Population), Rockville, MD, 1974.

Claudia Goldin and Lawrence F. Katz. The power of the pill: Oral contraceptives and women's career and marriage decisions. *Journal of Political Economy*, 110(4):730–770, August 2002.

Jonathan Gruber. Is making divorce easier bad for children? The long-run implications of unilateral divorce. *Journal of Labor Economics*, 4(22):799–833, 2004.

Jonathan Gruber, Phillip Levine, and Douglas Staiger. Abortion legalization and child living circumstances: Who is the "marginal child"? *The Quarterly Journal of Economics*, 114(1):263–291, February 1999.

Melanie Guldi. Fertility effects of abortion and birth control pill access for minors. *Demography*, 45(4):817–827, November 2008.

Heinrich Hock. The pill and the college attainment of American women and men. Florida State University, October 2007.

William R. Johnson and Jonathan Skinner. Labor supply and marital separation. *American Economic Review*, 76(3):455–469, June 1986.

Ted Joyce, Ruoding Tang, and Yuxiu Zhang. Changes in birth rates of young women following access to the pill and abortion in the early 1970s. CUNY, January 2011.

Chinhui Juhn and Kevin M. Murphy. Wage inequality and family labor supply. *Journal of Labor Economics*, 15(1):72–97, January 1997.

Chinhui Juhn, Kevin M. Murphy, and Brooks Pierce. Wage inequality and the rise in returns to skill. *Journal of Political Economy*, 101(3):410–442, June 1993.

Lawrence F. Katz and Kevin M. Murphy. Changes in the relative wages, 1963-87: Supply and demand factors. *Quarterly Journal of Economics*, 107(1):35–78, February 1992.

Phillip B. Levine, Douglas Staiger, Thomas J. Kane, and David J. Zimmerman. Roe v. Wade and American fertility. Working Paper 5615, National Bureau of Economic Research, June 1996. URL `http://www.nber.org/papers/w5615`.

John Maynard Smith. Parental investment: A prospective analysis. *Animal Behaviour*, 25:1–9, 1977.

Grant Miller. Contraception as development? New evidence from family planning in Colombia. *The Economic Journal*, 120(545):709–736, June 2010.

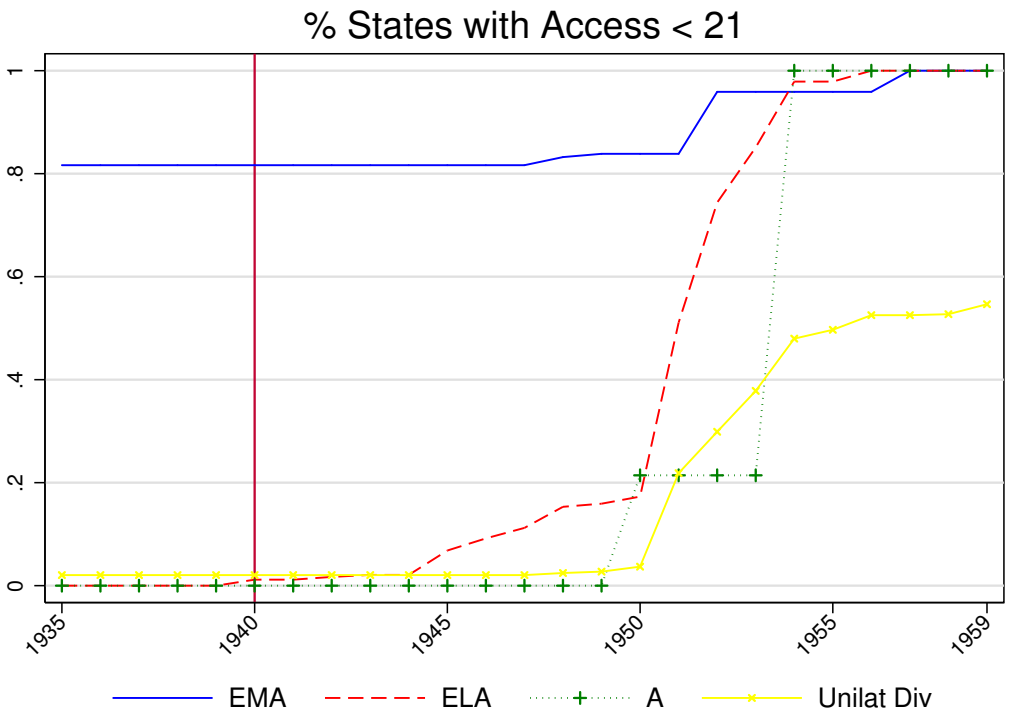
Juan Pantano. Unwanted fertility, contraceptive technology and crime: Exploiting a natural experiment in access to the pill. University of California - Los Angeles, PWP-CCPR-2007-028, December 2007.

Betsey Stevenson. The impact of divorce laws on marriage-specific capital. *Journal of Labor Economics*, 25(1):75–94, 2007.

Robert L. Trivers. Parental investment and sexual selection. In Bernard Campbell, editor, *Sexual Selection and the Descent of Man*, pages 136–179. Aldine de Gruyter, Chicago, 1972.

Elizabeth Siegel Watkins. *On the Pill: A Social History of Oral Contraceptives 1950-1970*. Johns Hopkins University Press, Baltimore, 1998.

Figure 1: Timing of Laws



Souce: Blank et al. [2009], Bailey [2006], Gruber [2004], Levine et al. [1996].
The graph displays proportion of States with marriage, Pill, abortion and unilateral divorce access before age 21 by cohort (x-axis).
States are weighted by female population ages 36-44 in the 1980 Census.

Table 1: Changes in Female Minimum Age of Marriage without Parental Consent

State	Year	Age	Change*
South Carolina	1957	18	4
Mississippi	1958	15	-3
Georgia	1965	19	1
Kentucky	1968	18	-3
Hawaii	1969	18	-2
Nebraska	1969	20	-1
Montana	1971	19	1
Iowa	1972	19	1
Virginia	1972	18	-3
Connecticut	1972	18	-3
Rhode Island	1972	18	-3
Georgia	1972	18	-1
Pennsylvania	1972	18	-3
Louisiana	1972	18	-3
Nebraska	1972	19	-1
West Virginia	1972	16	-5
Iowa	1973	18	-1
Montana	1973	18	-1
Alaska	1974	19	1
Alaska	1975	18	-1
Wyoming	1975	19	1
Florida	1977	18	-3

Source: Blank et al. [2009].

*: eg, Kentucky reduced the minimum age from 21 to 18 in 1968.

Table 2: Effects of Early Access (Before 21) on Marriage, by Education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Married by Age:								
	18	19	20	21	22	23	24	25	Ever Married[a]
Panel A.	All Women								
EMA	-0.001	0.021	0.036**	0.024	0.025	0.018	0.021	0.014	-0.002
	(0.009)	(0.013)	(0.015)	(0.022)	(0.019)	(0.017)	(0.013)	(0.012)	(0.010)
ELA	-0.009	-0.016*	-0.014	-0.005	-0.016*	-0.012*	-0.011	-0.003	-0.001
	(0.007)	(0.008)	(0.010)	(0.009)	(0.009)	(0.006)	(0.007)	(0.007)	(0.006)
Mean of Y	0.138	0.257	0.375	0.486	0.583	0.662	0.719	0.760	0.914
Panel B.	Less than Four-Year College								
EMA	0.003	0.032	0.046***	0.034**	0.037***	0.036***	0.033***	0.020*	-0.011
	(0.012)	(0.020)	(0.014)	(0.015)	(0.011)	(0.012)	(0.010)	(0.010)	(0.012)
ELA	-0.008	-0.014	-0.008	0.005	-0.004	0.001	-0.003	0.003	0.002
	(0.010)	(0.010)	(0.011)	(0.010)	(0.011)	(0.008)	(0.008)	(0.007)	(0.006)
Mean of Y	0.169	0.312	0.449	0.568	0.659	0.723	0.768	0.801	0.923
Panel C.	Four-Year College or More								
EMA	-0.002	0.010	0.033**	0.029	0.021	-0.007	0.007	0.007	0.029
	(0.012)	(0.014)	(0.016)	(0.033)	(0.044)	(0.041)	(0.028)	(0.027)	(0.020)
ELA	-0.002	-0.008	-0.017	-0.023	-0.044**	-0.048**	-0.032	-0.022	-0.015
	(0.008)	(0.009)	(0.013)	(0.015)	(0.018)	(0.019)	(0.020)	(0.020)	(0.015)
Mean of Y	0.027	0.056	0.106	0.189	0.307	0.439	0.538	0.611	0.881

Data are June CPS 1977, 79-83, 85-88, 90, 92, 95, ages 36-44, cohorts 1935-1959.

All regressions include cohort and state fixed effects, state trends, indicator variables for race (Black, Other non-White) and an indicator for whether individual had abortion access before age 21.

All regressions use survey weights and have standard errors clustered at the state level.

[a] – This regression includes age dummies.

Table 3: Education, Ever Married Women

	(1)	(2)	(3)
		(N=88,260)	
	$\geq$ High School Graduate	$\geq$ Some College	$\geq$ Four-Year College
EMA	0.011	0.047***	0.032
	(0.013)	(0.017)	(0.019)
ELA	0.004	-0.005	0.004
	(0.006)	(0.010)	(0.009)
Mean of Y	0.846	0.438	0.207

Data are June CPS 1977, 79-83, 85-88, 90, 92, 95, ages 36-44, cohorts 1935-1959.
All regressions include cohort and state fixed effects, state trends, indicator variables for race (Black,Other non-White) and an indicator for whether individual had abortion access before age 21.
All regressions use survey weights and have standard errors clustered at the state level.
All regressions include age dummies.

Table 4: Occupation, By Education, Ever Married Women

	(1) Man+Prof	(2) Man	(3) Prof	(4) HiProf	(5) DrLwr	(6) Occ
Panel A.			All Women			
EMA	0.036**	0.023*	0.012	0.011**	0.006*	0.009
	(0.016)	(0.012)	(0.017)	(0.005)	(0.003)	(0.012)
ELA	0.012	0.006	0.006	0.004	0.003**	-0.003
	(0.011)	(0.009)	(0.008)	(0.006)	(0.001)	(0.008)
N	64401	64401	64401	64401	64401	88260
Mean of Y	0.283	0.107	0.176	0.062	0.005	0.731
Panel B.			Less than Four-Year College			
EMA	0.032*	0.025	0.007	0.015**	n.a.	-0.001
	(0.019)	(0.017)	(0.011)	(0.006)		(0.013)
ELA	0.008	0.008	-0.000	0.002	n.a.	-0.006
	(0.010)	(0.010)	(0.006)	(0.005)		(0.011)
N	50153	50153	50153	50153	n.a.	70068
Mean of Y	0.161	0.093	0.068	0.026	n.a.	0.717
Panel C.			Four-Year College or More			
EMA	-0.029	0.008	-0.037	-0.029	0.022	0.027
	(0.038)	(0.021)	(0.043)	(0.025)	(0.016)	(0.023)
ELA	0.006	-0.006	0.012	0.006	0.010*	0.001
	(0.020)	(0.015)	(0.019)	(0.021)	(0.006)	(0.018)
N	14248	14248	14248	14248	14248	18192
Mean of Y	0.710	0.156	0.553	0.184	0.021	0.787

Data are June CPS 1977, 79-83, 85-88, 90, 92, 95, ages 36-44, cohorts 1935-1959.

All regressions include cohort and state fixed effects, state trends, indicator variables for race (Black, Other non-White) and an indicator for whether individual had abortion access before age 21.

All regressions use survey weights and have standard errors clustered at the state level.

All regressions include age dummies.

Table 5: Unilateral Divorce Access and Marital Outcomes, by Education

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Limited				
	(1977-95)	(1980,85,90,95)				
	Married by 22		Ever Divorced[a,b,c]	Divorced by[b,c]:		
				25	30	35
Panel A.	All Women					
EMA	0.032	0.058**	0.063*	0.033**	0.081***	0.078***
	(0.021)	(0.028)	(0.034)	(0.016)	(0.023)	(0.025)
ELA	-0.017*	-0.021	-0.017	-0.019*	-0.027**	-0.028**
	(0.009)	(0.014)	(0.012)	(0.011)	(0.013)	(0.014)
`unilateral`	0.026*	0.039***	0.167***	-0.002	0.018	-0.011
	(0.013)	(0.014)	(0.015)	(0.009)	(0.018)	(0.024)
N	96011	30268	27435	27435	27435	27435
Mean of Y	0.583	0.546	0.349	0.094	0.185	0.270
Panel B.	Less than Four-Year College					
EMA	0.045***	0.067***	0.087**	0.030	0.089***	0.097***
	(0.013)	(0.021)	(0.034)	(0.023)	(0.029)	(0.032)
ELA	-0.005	-0.004	-0.010	-0.019	-0.018	-0.031*
	(0.011)	(0.015)	(0.016)	(0.014)	(0.017)	(0.017)
`unilateral`	0.030*	0.042***	0.109***	-0.001	0.015	-0.022
	(0.016)	(0.016)	(0.012)	(0.013)	(0.019)	(0.025)
N	75447	23097	21143	21143	21143	21143
Mean of Y	0.659	0.627	0.374	0.109	0.205	0.292
Panel C.	Four-Year College or More					
EMA	0.023	0.024	-0.009	0.031*	0.042	0.017
	(0.046)	(0.055)	(0.059)	(0.017)	(0.029)	(0.043)
ELA	-0.044**	-0.040*	-0.026*	-0.010	-0.043**	-0.011
	(0.018)	(0.022)	(0.015)	(0.014)	(0.019)	(0.022)
`unilateral`	0.011	0.009	0.354***	0.006	0.035	0.034
	(0.018)	(0.030)	(0.077)	(0.021)	(0.031)	(0.046)
N	20564	7171	6292	6292	6292	6292
Mean of Y	0.307	0.284	0.266	0.044	0.119	0.195

Column 1 data are June CPS 1977, 79-83, 85-88, 90, 92, 95, ages 36-44, cohorts 1935-1959.

Column 2-6 data are June CPS 1980, 1985, 1990, 1995, ages 36-44, cohorts 1935-1959.

All regressions include cohort and state fixed effects, state trends, indicator variables for race (Black, Other non-White) and an indicator for whether individual had abortion access before age 21.

All regressions use survey weights and have standard errors clustered at the state level.

[a] – This regression include age dummies.

[b] – Sample restricted to ever married women.

[c] – Divorced refers to a 1st marriage termination either due to divorce, widowhood or separation.

`unilateral` indicates exposure to unilateral divorce law the year prior to outcome event.

Table 6: Education, Unilateral Divorce and Marital History Controls, Ever Married Women

	(1) ≥ High School	(2) ≥ Some College	(3) ≥ Four-Year College
Panel A.	With Unilateral Divorce[a] (N=88,260)		
EMA	0.011	0.036**	0.029
	(0.013)	(0.018)	(0.020)
ELA	0.004	-0.003	0.005
	(0.006)	(0.010)	(0.009)
`unilateral`	0.003	-0.042***	-0.011
	(0.007)	(0.014)	(0.013)
Mean of Y	0.846	0.438	0.207
Panel B.	With Marital Status History[b] (N=27,435)		
EMA	0.049***	0.067**	0.006
	(0.017)	(0.030)	(0.032)
ELA	0.006	0.004	0.016
	(0.008)	(0.013)	(0.013)
`divorced`	-0.043***	-0.047***	-0.084***
	(0.010)	(0.011)	(0.007)
Mean of Y	0.868	0.478	0.230

[a] – Baseline Sample: June CPS 1977, 79-83, 85-88, 90, 92, 95, ages 36-44, cohorts 1935-1959.

[b] – Limited Sample: June CPS 1980, 1985, 1990, 1995, ages 36-44, cohorts 1936-1959.

All regressions include cohort and state fixed effects, state trends, indicator variables for race (Black,Other non-White) and an indicator for whether individual had abortion access before age 21.

All regressions use survey weights and have standard errors clustered at the state level.

All regressions include age dummies.

`unilateral` indicates whether individual had unilateral divorce access before age 21.

`divorced` refers to a 1st marriage termination either due to divorce, widowhood or separation.

Table 7: Occupation, with Unilateral Divorce Control, By Education, Ever Married Women

	(1) Man+Prof	(2) Man	(3) Prof	(4) HiProf	(5) DrLwr	(6) Occ
Panel A.			All Women			
EMA	0.033**	0.022*	0.012	0.011**	0.008**	0.008
	(0.016)	(0.012)	(0.017)	(0.005)	(0.004)	(0.013)
ELA	0.013	0.006	0.007	0.004	0.003**	-0.003
	(0.011)	(0.009)	(0.008)	(0.006)	(0.001)	(0.008)
`unilateral`	-0.009	-0.006	-0.003	-0.000	0.007***	-0.003
	(0.014)	(0.009)	(0.010)	(0.006)	(0.002)	(0.008)
N	64401	64401	64401	64401	64401	88260
Mean of Y	0.283	0.107	0.176	0.062	0.005	0.731
Panel B.			Less Than Four-Year College			
EMA	0.031*	0.025	0.006	0.015**	n.a.	-0.000
	(0.018)	(0.016)	(0.011)	(0.007)		(0.013)
ELA	0.008	0.008	-0.000	0.002	n.a.	-0.006
	(0.010)	(0.010)	(0.006)	(0.005)		(0.011)
`unilateral`	-0.002	-0.001	-0.001	-0.001	n.a.	0.003
	(0.012)	(0.012)	(0.007)	(0.005)		(0.011)
N	50153	50153	50153	50153	n.a.	70068
Mean of Y	0.161	0.093	0.068	0.026	n.a.	0.717
Panel C.			Four-Year College or More			
EMA	-0.030	0.006	-0.035	-0.027	0.027	0.022
	(0.037)	(0.021)	(0.043)	(0.026)	(0.017)	(0.024)
ELA	0.006	-0.006	0.012	0.006	0.010	0.001
	(0.020)	(0.014)	(0.019)	(0.021)	(0.006)	(0.018)
`unilateral`	-0.003	-0.011	0.009	0.008	0.020**	-0.022
	(0.025)	(0.015)	(0.024)	(0.023)	(0.008)	(0.021)
N	14248	14248	14248	14248	14248	18192
Mean of Y	0.710	0.156	0.553	0.184	0.021	0.787

Data are June CPS 1977, 79-83, 85-88, 90, 92, 95, ages 36-44, cohorts 1935-1959.

All regressions include cohort and state fixed effects, state trends, indicator variables for race (Black,Other non-White) and an indicator for whether individual had abortion access before age 21.

All regressions use survey weights and have standard errors clustered at the state level.

All regressions include age dummies.

`unilateral` indicates whether individual had unilateral divorce access before age 21.

Table 8: Occupation, with Divorced, Separated, Widow Control, By Education, Ever Married Women

	(1) Man+Prof	(2) Man	(3) Prof	(4) HiProf	(5) DrLwr	(6) Occ
Panel A.			All Women			
EMA	0.040	0.034*	0.006	0.013	0.006	-0.007
	(0.033)	(0.020)	(0.020)	(0.011)	(0.006)	(0.020)
ELA	0.011	-0.004	0.015	-0.007	0.002	0.007
	(0.017)	(0.011)	(0.014)	(0.010)	(0.002)	(0.018)
`divorced`	-0.042***	0.021***	-0.063***	-0.009	-0.003**	0.080***
	(0.009)	(0.006)	(0.009)	(0.005)	(0.001)	(0.007)
N	20411	20411	20411	20411	20411	27435
Mean of Y	0.308	0.118	0.190	0.068	0.007	0.747
Panel B.			Less Than Four-Year College			
EMA	0.069**	0.041*	0.028**	0.019*	n.a.	-0.005
	(0.030)	(0.023)	(0.012)	(0.010)		(0.023)
ELA	-0.013	-0.004	-0.009	-0.005	n.a.	-0.009
	(0.014)	(0.012)	(0.011)	(0.008)		(0.022)
`divorced`	0.007	0.016**	-0.009	0.002	n.a.	0.078***
	(0.007)	(0.006)	(0.005)	(0.004)		(0.008)
N	15395	15395	15395	15395	n.a.	21143
Mean of Y	0.175	0.102	0.073	0.029	n.a.	0.731
Panel C.			Four-Year College or More			
EMA	-0.020	0.015	-0.035	-0.001	0.019	-0.016
	(0.055)	(0.022)	(0.049)	(0.032)	(0.033)	(0.051)
ELA	0.020	-0.014	0.034	-0.020	0.003	0.036
	(0.022)	(0.022)	(0.029)	(0.025)	(0.009)	(0.027)
`divorced`	-0.017	0.060***	-0.077***	0.014	-0.002	0.121***
	(0.020)	(0.015)	(0.024)	(0.017)	(0.005)	(0.012)
N	5016	5016	5016	5016	5016	6292
Mean of Y	0.714	0.167	0.547	0.185	0.028	0.800

Data are June CPS 1980, 1985, 1990, 1995, ages 36-44, cohorts 1936-1959.

All regressions include cohort and state fixed effects, state trends, indicator variables for race (Black,Other non-White) and an indicator for whether individual had abortion access before age 21.

All regressions use survey weights and have standard errors clustered at the state level.

All regressions include age dummies.

`divorced` refers to a 1st marriage termination either due to divorce, widowhood or separation.

Table 9: Fertility, Ever Married Women

	(1)	(2)	(3)	(4)	(5)	(6)
	First Birth within[a] x Years of First Marriage:					
$x =$	One	Two	Three	Four	Five	A Birth
Panel A.	All Women (N=88,260)					
EMA	-0.024*	-0.032*	-0.017	-0.013	-0.004	0.007
	(0.013)	(0.017)	(0.016)	(0.018)	(0.022)	(0.014)
ELA	-0.007	-0.008	0.002	-0.001	-0.008	-0.004
	(0.008)	(0.011)	(0.009)	(0.008)	(0.008)	(0.006)
Mean of Y	0.329	0.558	0.676	0.746	0.792	0.900
Panel B.	Less Than Four-Year College (N=70,068)					
EMA	-0.024	-0.036	-0.015	-0.017	-0.005	0.008
	(0.018)	(0.024)	(0.017)	(0.015)	(0.017)	(0.013)
ELA	-0.006	-0.009	-0.000	0.001	-0.005	-0.003
	(0.009)	(0.011)	(0.009)	(0.008)	(0.009)	(0.006)
Mean of Y	0.374	0.618	0.731	0.792	0.830	0.918
Panel C.	Four-Year College or More (N=18,192)					
EMA	0.001	0.015	0.006	0.016	0.018	0.003
	(0.028)	(0.030)	(0.026)	(0.038)	(0.044)	(0.025)
ELA	-0.005	0.001	0.013	-0.006	-0.019	-0.006
	(0.012)	(0.017)	(0.016)	(0.017)	(0.016)	(0.014)
N	18192	18192	18192	18192	18192	18192
R^2	0.044	0.066	0.066	0.060	0.049	0.023
Mean of Y	0.160	0.331	0.466	0.570	0.647	0.829

Data are June CPS 1977, 79-83, 85-88, 90, 92, 95, ages 36-44, cohorts 1935-1959.
All regressions include cohort and state fixed effects, state trends, indicator variables for race (Black,Other non-White) and an indicator for whether individual had abortion access before age 21.
All regressions use survey weights and have standard errors clustered at the state level.
All regressions include age dummies.
[a] – Including premarital births. However, this inclusion does not change results.

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

A.1 Sample Delimitation

The Marriage and Fertility Supplement of the Current Population Survey (CPS) is administered in the month 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 females of childbearing age (18-44) for 1977, 1979, 1980, 1981, 1982, 1983, 1985, 1986, 1987, 1988, 1990, 1992 and 1995.[6] We use the data retrospectively and restrict the sample to women ages 36-44 from cohorts 1935-1959. The age range guarantees that we use the latest information or the final outcome available for those women, such as educational attainment, occupational outcomes and fertility. The cohort restriction means 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

[6]Dates on first marriage are available for all females 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 since the date of first birth are not asked by then, we disregard those years in our sample. While the codebook of data in 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, data do display date on first birth for all women. We did not find any clarification in the documentation for this pattern, 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. Thus, we considered those data entries as valid. The cap on age 44 for fertility information is again present in years 1986, 1987, 1988 and 1992.

gender, age and marital status. We drop the 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 four years.

A.2 Variables

State Identifiers. The June CPS has information on state of residence only, and we use that 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 groups:

High School Graduate or more 4 years of completed high school, or more.

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

College Graduate or more 4 years of completed college studies, or more.

In surveys year 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 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.[7] We further look into increasingly selective subgroups of the professional occupation: "high professional occupations" and doctors or lawyers.

Man Managerial Occupations.

Prof Professional Occupations.

Man+Prof Managerial and professional occupations.

HiProf Professional Occupations excluding teachers and nurses.

DrLwr Doctors or Lawyers.

[7]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.

For 1977 to 1982. "Man" are those classified as 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. "Prof" are those classified as Professional, Technical and Kindred Workers (codes 0-200). We exclude codes 80-85 and 163-173, who are technicians not included in professional occupations from 1983 and onwards. The Professional Occupations include engineers, doctors, mathematicians, natural scientists, social scientist, lawyers, judges, and teachers, among others. "HiProf" excludes primary and secondary teachers (codes 141-145) and and health assessment and treating occupations (registered nurses, dietitians, pharmacists and therapists, codes 64 and 74-76). "DrLwr" are physicians, dentists, veterinarians, lawyers and judges (codes 30-31, 62, 65, and 74).

For 1983 to 1995. The occupational groups are defined as above, but the codes vary. "Man" corresponds to codes 0-42. "Prof" corresponds to codes 43-199. "HiProf" excludes codes 155-162 95-106 from the "Prof" category. "DrLwr" are 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.

First Marriage Ended 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.

First Marriage Ended before Age x To generate age at divorce (widowhood or separation) of first marriage, 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."