The Paradox of the Pill

Andrew Beauchamp[*] Catherine Ruth Pakaluk
Boston College Ave Maria University

January 2, 2015

Abstract

Dramatic improvements in fertility control mark some of the most significant technological changes of the twentieth century, and so it seems paradoxical that the same period saw widespread and large increases in non-marital births. We link these two trends and examine the effect of increased access to the birth control Pill on non-marital childbearing and shot-gun marriage rates, along with other labor, education and fertility outcomes of women. Using multiple data sources and legal reforms which generated plausibly exogenous variation in access to the Pill for both married women and minors, we find that increased marital access on net increased non-marital childbearing rates, primarily through a dramatic decrease in marriage rates following a birth. We also confirm and add to the evidence on the contributions of the Pill to increased female education and birth spacing already found in the literature, but find convincing evidence that the Pill had heterogenous effects for women. Minorities and women from less educated families primarily saw increased non-marital childbearing, while whites and women from more highly educated families saw higher levels education and increased birth spacing. Finally we examine how the Pill altered spousal sorting and show that the introduction of the Pill improved the marital prospects of women from more highly educated households, and hurt them among those from less educated families. The results are consistent with the prior theoretical literature that the Pill created winners and losers both through re-sorting in the marriage market and by eliminating shot-gun marriage as a norm.

JEL Classification:
Keywords: Marriage, divorce, fertility, single motherhood, non-marital childbearing,

[*]Corresponding Author: E-Mail: beauchaa@bc.edu; Economics Department, 140 Commonwealth Ave, Chestnut Hill MA 02467.

1 Introduction

On April 7th, 1967, *Time* magazine opened its cover story on the Pill as follows.

> The "pill" is a miraculous tablet that contains as little as one thirty-thousandth of an ounce of chemical. It costs 11¢ to manufacture; a month's supply now sells for $2.00 retail. It is little more trouble to take on schedule than a daily vitamin. Yet in a mere six years it has changed and liberated the sex and family life of a large and still growing segment of the U.S. population: eventually, it promises to do the same for much of the world.[1]

On December 23rd, 1999, *The Economist* concurred, naming the Pill the "invention that defined the twentieth century."[2] These claims are not without reason. Examining variation in cohort-level access to the Pill, Goldin and Katz (2002) provide evidence that the technology shock did in fact facilitate the dramatic increases in education, career investment, age at first marriage, and age at first birth observed among American women after 1960. Following on this, work by Bailey (2006, 2010) exploits variation in state-level access to the Pill to argue that decreases in marital childbearing and increases in labor market participation are causally linked to Pill access.

However, the introduction of a new technology normally creates winners and losers. Existing models suggest that a contraceptive shock might generate gains for some agents, and losses for others. Akerlof et al. (1996) argue that rates of non marital childbearing might rise following improvements to birth control that erode a culture of shot-gun marriage. Similarly, Goldin and Katz (2002) contend that marriage prospects worsen among women with the lowest labor market prospects after a reordering of marriage markets following contraceptive improvements. These models and others (Greenwood and Gruner (2010), Burke and Pakaluk (2010)) advance the thesis that the Pill may have had not one but two distinct effects on American women.

In this paper we examine this hypothesis and ask whether the large increase in non-marital childbearing observed since 1960 was due in part to the advent and use of the oral contraceptive, paradoxically the very innovation that dramatically improved fertility control. Exploiting exogenous

[1] Contraception: Freedom from Fear, *Time*, April 7, 1967.

[2] The liberator, Millennium Issue: Oral contraceptives, *The Economist*, December 23, 1999.

variation in laws governing access to the Pill, we examine whether the Pill changed marriage and childbearing norms in such a way as to create two distinct groups: contraceptive winners and losers. We also test whether access to the Pill increased non-marital childbearing and lowered marriage rates while confirming much of the prior literature on how the Pill changed female education and marital fertility.

Understanding the trend in non-marital childbearing matters for a variety of important reasons. Children born outside of marriage are more likely to grow up in poverty, exhibit poor developmental outcomes, and experience unstable family arrangements (Ryan (2010), Wildsmith (2004)). At the same time, women and men who give birth to children outside of marriage are more likely to be poor and stay poor (McLanahan (2009)). Together these facts are related to the Chetty et al. (2014) finding that the single largest correlative predictor of economic mobility at the individual and community level is the fraction of children being raised by single mothers. They report that "mobility is significantly lower in areas with weaker family structures, as measured e.g. by the fraction of single parents. As with race, parents' marital status does not matter purely through its effects at the individual level. Children of married parents also have higher rates of upward mobility in communities with fewer single parents."

While the overall trends in non-marital fertility have been widely reported, rising from roughly 5 to 40 percent of all births since 1960 (Ventura and Bachrach (2000), Martin et al. (2013)), what is less well known is that the growth in non-marital births has been concentrated more heavily among poor and less-educated women. The Census Bureau reports that 68.9 percent of women in the very lowest income bracket, household income less than $10,000, experienced non-marital births, compared with 9.0 percent in the highest bracket, household income more than $200,000.[3] Between these endpoints, the percentages of unmarried births decreases monotonically with each income level. Correspondingly, in the same data 57.0 percent of births to women with a high school degree or less were non-marital, compared with only 8.8 percent among women with a 4-year college degree.[4] Any attempt to understand the trends in non-marital childbearing has to account for the strong SES gradient, which is at least somewhat counter-intuitive. Children bring direct costs and

[3] See Table 2 in Shattuck and Kreider (2013).
[4] *Ibid.*

also impose enormous opportunity costs in terms of foregone income and educational investment, both of which suggest larger incentives for cost-sharing through partnership among poor women. This observation has led some to wonder whether preferences for marriage have changed among low-income communities.

To evaluate this proposition, Edin and Kefalas (2005) conducted a 5-year study of single-mothers in Philadelphia and Camden. They report finding:

> surprisingly little evidence of the much-touted rejection of the institution of marriage among the poor. [...] Marriage was a dream that most still longed for, a luxury they hoped to indulge in someday when the time was right, but generally not something they saw happening in the near, or even foreseeable, future.[5]

Edin and Kefalas describe non marital childbearing as a complex phenomenon resulting from two competing realities for poor women: the intense desire for meaningful relationships, and the profound sense that marriage is unattainable. As the researchers put it,

> While the poor women we interviewed saw marriage as a luxury, something they aspired to but feared they might never achieve, they judged children to be a necessity, an absolutely essential part of a young woman's life, the chief source of identity and meaning.[6]

These findings do not support the thesis that desirability of marriage has declined among low-income women. Instead, they provide serious qualitative evidence for the theories advanced by both Akerlof et al. (1996) and Goldin and Katz (2002), in which a contraceptive shock deteriorates the marriage market prospects of certain groups of women.

Did access to the Pill cause increases in non-marital childbearing in the United States, together with decreases in marriage among women with the lowest labor market opportunities? The difficulties to answering this question are numerous since changes in contraceptive use occurred simultaneously with the evolution of social views on female education, labor force participation, childbearing and sexual norms. Our approach draws on two important articles aimed at understanding the

[5] Edin and Kefalas (2005) p. 6
[6] *Ibid.*

immediate and long-term consequences of policy changes related to the sexual revolution. The first is Bailey (2010), who presents evidence that pre-existing contraceptive sales bans across the United States provide plausibly exogenous variation in access to the birth control Pill in the early 1960's. These "Comstock" laws sometimes explicitly prohibited the sales of contraceptives, and sometimes did not, and these differences were codified by state legislatures in the late 19th century, providing a plausibly exogenous distribution of sales bans at the time the Pill was introduced. Additionally the early 1960's saw a number of states overturn their explicit sales bans, and in 1965 the U.S. Supreme Court's *Griswold* decision paved the way for legal access for married couples. The second paper we follow is Wolfers (2006) who outlines a strategy for measuring the policy impact of legal changes when there is a dynamic response of the outcome variable to the policy change, something we expect if the Pill altered sexual, childbearing and marital norms. While Wolfers (2006) focuses on the immediate and long-term effects of unilateral divorce on marriage, our analysis addresses how legalized access to contraceptives affected non-marital fertility, allowing differential effects of access across time. To further understand the context of changes in non-marital childbearing we also examine the effects of Pill access on marriage following birth, female schooling, birth spacing and partner selection.

Using the National Vital Statistics to measure non-marital births and the National Survey of Families and Households, a rich source of demographic information, we find robust evidence that changes in marital access to the Pill substantially increased the probability of a birth occurring non-maritally, by 15 percentage points in the long-term. Using the National Fertility Surveys of 1965 and 1970, we show that states with early marital access saw dramatically higher pre-marital Pill use. We find the effects of the Pill on non-marital childbearing take years to develop, grow over time, and are concentrated among women aged 19 to 25. Women in this age group experienced a dramatic decrease in the probability of marriage following a birth in the years after marital access: 14 percentage points after 10 years of Pill-exposure. We also show the effects are primarily concentrated among women whose fathers did not graduate high school and among black females. Importantly, access for minors, who were mostly single women, lowered non-marital childbearing rates among these groups, countering some the increases arising from marital access; however we find little significant evidence that the probability of marriage following birth similarly rebounded. In terms of education, we estimate that Pill access for minors significantly raised the hazard of obtaining a bachelors degree (by 1.6 percentage points 10 years following minor's-access), and that education-effects were entirely concentrated among white women and those from families where the father had at least a high-school diploma. With respect to birth-spacing we find evidence that marital access significantly increased the space between first and second births (by nearly 10 months on average), however these increases in spacing did not exist for women whose first birth was non-marital, and were considerably smaller for women whose fathers had less than a high school diploma. Finally we exploit the reporting of partner earnings and wages in the NSFH and show that the introduction of minor access dramatically changed partner sorting as predicted by Goldin and Katz (2002). In the years following plausibly exogenous legal changes in Pill access, we show women from more highly educated families saw large increases in partner wages and earnings while those from less educated families saw nearly identical decreases in partners wages and earnings.

The rest of the paper is organized as follows. Section 2 discusses our data sources and the identification strategy built upon legal changes. Section 3 presents our empirical strategy exploiting changes in cohort exposure and the retrospective panel observed in the NSFH, while Section 4 presents the model estimates. Section 5 offers some final thoughts about how family policy might be designed in light of our findings.

2 Data and Legal Changes

The data we use are drawn from two main sources: administrative reporting through the National Vital Statistics System (NVSS) and retrospective survey data gathered in the National Survey of Families and Households (NSFH) Wave 1. Vital statistics data were consistently collected on the number of non-marital births occurring to women across United States and Territories from 1950 through 1990.[7] Wave 1 of the NSFH was fielded in 1987 and 1988 to a broad cross-section of households in the United States. The rich demographics make the survey data especially valuable

[7] Occasional years in the 1950's were not collected via NVSS, and so are missing for our sample as well. During the early part of our sample Alaska and Hawaii were US territories,

for understanding heterogeneity in the impacts of contraceptive access, at the cost of potentially being less representative than the administrative data. Ultimately the data sources complement one another.

The vital statistics data consist of non-marital childbearing rates measured at the state-level drawn from counts of the number of births occurring to unmarried women (at the time of birth) at the state-year level, coupled with population estimates for women of different ages. We use an unbalanced panel with potentially 36 years of data (1952, 1954-1956, and 1958 through 1990), and 50 states plus the District of Columbia.[8] Participation in NVSS reporting of non-marital births was gradual: in the 1950's only 35 states participated, with eventual adoption by all states in 1980.

The complete fertility and marriage histories in the NSFH allow us to examine patterns of non-marital childbearing retrospectively over the years during which contraceptive restrictions were progressively eased. These data contain observations of men and women from which we construct measures of non-marital childbearing, mainly whether a first birth occurred non-maritally. Additionally, the retrospective histories allow us to construct a panel data set, allowing us to deal with both cohort and calendar-year specific unobservables, which may be important in understanding the longer-term increase in non-marital childbearing. In addition to birth and marriage histories, the NSFH contains an enrollment and degree completion history, along with information on current partner earnings and wages which we examine below.

2.1 Legalized Contraceptive Access

Bailey (2010) and Bailey (2006) are two important studies that introduced plausibly exogenous changes in the availability of the contraceptive Pill for married and young women respectively. We draw from her classifications of state-specific policies governing sales bans and the timing of legal reforms. In Table 1 we report the dates of policy changes that allowed access to contraceptives among married couples (which reversed existing sales bans), and among minors, which occurred primarily when states changed their age-at-majority laws.[9] We have seven early repeal states split

[8] We only use vital statistics data through 1990 to keep the time-frames roughly similar across the two datasets.

[9] Other statues besides age-at-majority were also responsible for early legalized access for minors; see Bailey (2006) Section III.

between 1961 and 1963 that provide identification of the impact of access to marital contraceptives. Age-at-majority statutes were changed at various times throughout the 1960's and 1970's, and policy changes were largely driven by concerns over voting and military service.

The legal background of these policy reforms is recounted in detail in Bailey (2010) and Bailey (2006). Legal reforms granting marital access happened in states from across the U.S., and there was no evidence that an existing sales ban was correlated with demand for contraceptives. Of the 48 contiguous states, 47 enacted laws restricting "obscenity," but only 24 state statutes expressly prohibited sales.[10] For minor access legal changes were precipitated by the expansion of minors' rights, in particular demands that those who were forced to serve in the military should be allowed to vote. An indirect result of these changes at the state level was the ability of young women between 18 and 21 to make independent health care decisions, including use of the contraceptive pill.

To examine potential endogeneity of these policy changes we run a simple test of whether lagged non-marital childbearing can predict policy repeals for marital and minor access. Results are presented in Table 2. Coefficients reveal that for marital reforms there was no significant pre-trend in non-marital births that predicts enactment, and the magnitude of the coefficient is also small. However, for the repeal of age-at-majority laws there is a significant and meaningful correlation between non-marital birth rates and legal changes. But once we add state and year fixed effects the coefficient shrinks in magnitude by a factor of ten. This suggests that there were differences in pace of social changes across states that affected policy repeals, and so throughout we only exploit variation in access within states.

2.2 Did Marital Access Spillover?

We examine the legal history of the early contraceptive sales bans, looking for evidence on whether the statutes might have intentionally or unintentionally provided contraceptive access for the unmarried. *Griswold vs. Connecticut* grounded its overturning of Connecticut's sales ban on a "marital" right to privacy. However, evidence from state-level statutes and state-court rulings

[10]Territories under Federal jurisdiction, including Washington DC and Hawaii and Alaska in the 1950's allowed the sale of contraceptives, at least through physician exemptions, following a court decision in 1936.

suggests that in early repeal states unmarried access was not as explicitly addressed. In the case *Planned Parenthood vs. Maricopa County (1962)* the sales and advertising ban in place in Arizona was overturned by the Arizona Supreme Court. The case removed doctor-patient relationships, and referrals, from the definition of advertising, thus making contraceptives available legally. The key question for our concern is whether access was also facilitated among the unmarried. The court offered this on the topic:

> *In our estimation the statute could reasonably protect both the morals and the health of the community inasmuch as stimulation of sales of contraceptives might lead *243 to greater sexual activity among unmarried persons. Whether the statute was directed toward discouraging illicit sex experience among young people, for moral reasons, or because of the venereal disease problem, we consider the public interest served by the statute substantial compared with the private interest in free speech through advertising which is restricted by the statute.*

It appears the Supreme Court of Arizona foresaw at least the possibility that the statute under their decision (removing the strict interpretation of "advertising") would enable greater access to contraceptives among the unmarried.

In Figure 3 we examine first use of the contraceptive pill prior to first marriage using retrospective reporting from the National Fertility Surveys of 1965 and 1970. The data consist only of married female respondents, and we pool observations across samples. The survey collected retrospective Pill use and marital histories from which we can time the use of the pill prior to marriage. We examine whether the overall pill use was elevated during the time period when the seven reform states allowed for marital access. The left-hand panel of 3 reveals that compared to other states with a sales ban Pill use was dramatically higher during 1964 in early repeal states. The fractions reporting pre-marital use were 0.24 in repeal states and 0.09 in states with a sales ban. Thus there is evidence of elevated pre-marital use during the only full calendar year when all early repeal states allowed access, prior to 1965 when the U.S. supreme court ruled against the Connecticut ban on marital contraceptive sales. The right-hand panel reveals that there is a similar elevated level in the early repeal states relative to states with no restrictions via their Comstock statutes. This may reflect that the statutes repealed (or overturned) in the repeal states did in fact facilitate unmarried access as discussed by the Arizona Supreme Court. Finally, the Figure reveals just how common pill use was despite ostensible restrictions on unmarried access: in 1965, the first year under which the U.S. supreme court Pill allow unrestricted marital access, pre-marital use was already at 1 in 5 women. These points suggest that pre-marital sexual and childbearing norms were responsive to legal changes for married couples.

3 Empirical Strategy

To understand the impact of contraceptive access on non-marital childbearing we examine how legal changes affected both non-marital and marital childbearing rates, as well as the conditional probability of marrying following giving birth. The first data source draws from administrative vital statistics and so is run at the state-year level for the years 1952 through 1990. The first estimating equation takes the form:

$$
\begin{aligned}
NMBRate_{st} = \beta_0 &+ \sum_{k=1}^{K} \mathbb{1}\{\text{Married Access for } k \text{ periods}_{st}\}\beta_{1k} \\
&+ \sum_{k=1}^{K} \mathbb{1}\{\text{Minor Access for } k \text{ periods}_{st}\}\beta_{2k} \\
&+ \delta_s + \lambda_t + t * \eta_s + \varepsilon_{st}
\end{aligned}
\tag{1}
$$

where we measure the response of the non-marital birth-rate across K three-year periods following the overturning of a Comstock ban or the legal change in the age-at-majority (referred to as "Minor Access").[11] Here t refers to calendar time and we control for time effects, state fixed effects and state-specific linear trends.[12] In this framework we identify the impact of access on childbearing through non-linear deviations of non-marital childbearing from its state-specific trends. The non-marital childbearing rate is the ratio of non-marital births occurring to women in the population relative to the estimated population size.[13]

[11] Technically the over-turning of age-of-majority laws provided access for women under the age of majority.

[12] Point estimates were nearly identical when including both state-specific linear and quadratic time trends.

[13] Both estimates are drawn from the NVSS surveys for each state and year; population numbers are generally interpolated Census estimates.

Following examining aggregate data we exploit both the cross-section and retrospective panel dimensions of the NSFH, when childbearing and marital histories were reported in 1987 and 1988, which covered the reform periods. The crucial piece of data also reported in the NSFH is the state of residence at age sixteen, which we use to link individuals with the policy environments they faced at the outset of their sexual, marital and childbearing decision making. Using the cross-section of data we examine the following model of whether an individual's first birth occurred outside marriage:

$$\begin{aligned}\mathbb{1}\{NM1stBirth_{isc} = 1\} = \alpha_0 &+ \sum_{k=1}^{K} \mathbb{1}\{\text{Married Access for } k \text{ periods}_{sc}\} \cdot \alpha_{1k} \\ &+ \sum_{k=1}^{K} \mathbb{1}\{\text{Minor Access for } k \text{ periods}_{sc}\} \cdot \alpha_{2k} \\ &+ \delta_s + \lambda_c + c * \eta_s + X_i^{'} \cdot \alpha_3 + \varepsilon_{ict}\end{aligned} \tag{2}$$

where i denotes individual, s denotes state, and c denotes the year in which the individual turned 16 (or their cohort). Thus this approach compares cohorts before and after the legal reforms within a state, while allowing for state-specific trends in non-marital childbearing across states. The definition of the policy variables in these models are based on the year in which a respondent turned sixteen (at which point we observe their policy environment). Thus the variable $\mathbb{1}\{\text{Married Access for 1 period}_{sc}\}$ equals one if the respondent was aged sixteen during the state's reform year or the following two calendar years.

We also create a panel stretching from 1941 through 1988 using the marital and birth histories. This approach allows for a more straightforward estimation of how contraceptive access affected entrance into marriage and childbearing differently by age, as well as allowing us to control for the year in which the conception occurred (calendar time effects in addition to the cohort effects controlled for in the cross-sectional regressions). The model for the panel data take the following form:

$$
\begin{aligned}
\mathbb{1}\{NMBirth_{isct} = 1\} = \gamma_0 &+ \sum_{k=1}^{K} \mathbb{1}\{\text{Married Access for } k \text{ periods}_{st}\} \cdot \gamma_{1k} \\
&+ \sum_{k=1}^{K} \mathbb{1}\{\text{Minor Access for } k \text{ periods}_{st}\} \cdot \gamma_{2k} \\
&+ \delta_s + \lambda_t + \theta_c + t * \eta_s + X_{ic}^{'}\gamma_3 \cdot + \varepsilon_{isct},
\end{aligned} \tag{3}
$$

where policy indicators are again defined using calendar time as in Equation (1). In both Equations (2) and (3) we control for background characteristics in X: maternal and paternal completed eduction and whether the respondent was non-white. Additionally in Equation (3) we include fixed effects for the respondents' age during the calendar year. We also estimate models of the form of Equation (3) where we examine the probability of a marital first birth, as well as marriage in the year or year following a birth to understand the causes of the impacts on non-marital fertility. Finally we also estimate versions of Equation (2) and (3) examining outcomes other than marriage and childbearing, including education attainment, birth-spacing and partner wages.

4 Results

The left-hand panel of Figure 1 presents the non-marital childbearing rate before and after repeal of Comstock sales bans for early repeal states along with rates for those states which had no ban in place.[14] Data come from the National Vital Statistics. The two bold, vertical lines indicate the dates of repeal of contraceptive bans. The period from 1960 to 1970 showed significant catch-up in terms of non-marital childbearing rates, driven mostly by steeper increases in states that formerly prohibited contraception. Indeed, the mean level of non-marital childbearing converged to identical levels by the early 1970's. One concern in the left-hand panel is evidence of a pre-trend among the repeal states. However, the same increase can be noted in the right-hand panel of Figure 1 once we condition out national trends, and there is less evidence of a pre-trend among repeal states. Both panels indicate that early repeal states began from significantly lower non-marital childbearing rates, and beginning in 1961, the year of the first sales ban-repeal, they experienced

[14]Repeal for these states is set 1957, when the Envoid Pill was released.

rapid increases in the decades following repeal. Given that there is some evidence of a pre-trend in the right-hand panel, we include indicators for being in a repeal state in the period immediately preceding repeal in our state-level regressions.

Table 3 presents estimates from the regression model outlined in Equation (1) based on the variation observed in Figure 1. We divide the dynamic response into three year windows, and all columns include state fixed effects, state-linear trends and year fixed effects, and standard errors are clustered at the state level.[15] The first column reports the non-marital birth rate for women between 15 and 44, while the second two columns split the results across age. Column (1) shows that marital access to contraceptives significantly increased non-marital birth rates in the decade following legalization. The results grow dramatically over time: at 3 to 5 years following repeal, non-marital birth rates were 10.7% higher, and 9 to 11 years after repeal they were 15.4% higher. Beyond 12 years the estimates are noisy but coefficients are large, at least suggesting that the effect at ten years persisted indefinitely. The coefficient on 9 to 11 years is large enough to account for 44% of the increase in non-marital birth rates between 1963 (the modal early repeal year) and 1973.[16] Estimating the impact of minor's-access we see negative signs but no significant overall effects. Splitting results by age produces an expected result: women at higher risk of birth (those below age 35) see bigger increases in non-marital births, but effects are again noisy. We also detect weakly significant decreases for these women in non-marital childbearing following access for minors. These signs are consistent with higher rates of Pill use (as Bailey (2006) showed) among these affected cohorts. Finally, the pre-repeal indicator is also insignificant in this regression, providing at least some evidence that policy repeal was not endogenous with respect to rising non-marital birth rates.

We turn to using the NSFH to better understand the patterns in the data, in particular the nature of heterogeneous responses, and as a check on our state-level regressions. Table 4 presents models of Equation (2), the probability that a first birth was non-marital in the cross-section of women interviewed in 1987 and 1988. The key variable observed is the state of residence at age 16, which we use to classify respondents as living in a state that allowed the sale of contraceptives

[15]Coefficient estimates looked very similar with smaller and larger windows (between 2-5) but our power weakens when the window is too small, given that we only observe seven early repeal states.

[16]Early repeal states had an average non-marital birth rate of 5.5 births per 1000 women in 1963 and 8.2 births per woman in 1973.

following repeal. Throughout we use the linear probability model, but here we test whether the mis-specification bias present leads to significantly different conclusions: column (1) presents the main linear specification, column (2) restricts the sample to those on whom we can estimate a binary choice model, and column (3) estimates a logit on the same indicator of a non-marital first birth. We see the same pattern as Table 3: effects are initially smaller but grow over time, and are similar in magnitude at 9 to 11 years and beyond 11 years. In these specifications we find significant evidence that the increased level of non-marital childbearing continued past 12 years following repeal. The linear probability model understates the effects relative to the logit, whose marginal effects are reported. Thus the estimates' magnitudes are very similar to those obtained from the state level data, with around a 15% long-term increase. Again we see little overall effect for minors' access.

Table 5 presents results form Equation (3), where we use the retrospective reporting of births and marriages to construct a panel. The three columns estimate the discrete-time hazard of having a non-marital birth (no longer conditioning on first births), for those age 18 and younger in the calendar year, those between 19 and 24, and those between 25 and 45. In addition to state and (calendar) year fixed effects, and state linear trends, these models also include fixed effects for respondent age in the calendar year and the calendar year in which they turned 16. Splitting results by age reveals important differences from the age-aggregated regressions in Tables 3 and 4, notably all of the increase in non-marital childbearing is concentrated among women aged 19 to 24. We also see that women exposed to the Pill due to changes in legal restrictions allowing minors' access to contraceptives saw significantly lower non-marital childbearing rates, of between 3 to 5 percent. We also note that the introduction of the Pill for minors did decrease non-marital birth rates among women aged 19 to 24 in the first six years of access.[17] These reforms followed chronologically after allowing for marital access, so these estimates are consistent with minors' access reversing of some of the increase in non-marital childbearing which followed improved marital access. Interestingly, these effects fade away fairly quickly. This finding may also reflect improved match quality immediately following the introduction of the Pill for minors.

[17]These estimates are consistent with Kearney and Levine (2009) who use more recent policy variation and find similar reductions.

To better understand why non-marital birth rates increased, we estimate the discrete-time hazard into marriage in the two years following a birth (that is we measure marriage in the year of birth and the year subsequent to birth to estimate this probability).[18] Akerlof, Yellen, and Katz (1996) argues the increase was due largely to the disappearance of shot-gun marriage, a point we can examine with these data. Estimates are presented in Table 6, where again only for women between 19 and 24 we observe a large decrease in the probability of marriage conditional on having a birth. The coefficient on marital access for 9 to 11 years after repeal is large enough to explain 86 percent of the 10-year observed drop on the probability of marriage following birth in early repeal states; the long-term effect, from 12 years and beyond, is large enough to explain 83 percent of the drop form 1963 to 1988.[19] Again the introduction of the Pill for younger (mostly single) women had a countervailing effect which was relatively short lived, consistent with initial improvements in match quality leading to increased marriage rates following a birth.

4.1 Heterogenous Effects of Access

The benefit of using the NSFH data is that we can examine important splits in the data to see which groups of women experienced changes in their childbearing and marriage experiences. We focus on white-minority differences, and examine results conditional on father's education. Table 7 presents three sets of regressions conditional on the father having less than a high-school diploma or not. The first two columns report the effects of contraceptive access on probability of having a non-marital birth for teens age 18 and younger. The regressions reveal nearly all of the decrease in non-marital childbearing among teens stemming from exposure to Pill-access as a teen is found among women whose father had more than a high school degree. Women from less educated families had more than double the non-marital birth rate and the overall birth rate, so these estimates are consistent with minors' access to the Pill having a bigger impact for those closer to the margin. The second two columns show the same regression but for women aged 19 to 24, and again all of the increase in the non-marital birthrate is concentrated among women from less educated families.

[18] Given uncertainty about the joint-timing of conceptions and marriage, this serves as a reasonable approximation to the shot-gun marriage rate in these data.

[19] The probability was 0.914 in 1963 and 0.75 in 1973 and 0.66 in 1988.

Finally the last column presents estimates for the probability of marriage conditional on birth in this year or last year for women age 19 to 24. Again all of the significant decreases in marriage are concentrated among the low-education group.

In Table 8 we examine the same set of regressions, teen and young adult non-marital birth rates, and young adult marriage rates conditional on birth, but examine results separately for blacks and whites. The results are very similar to when we split the sample by education, except the effects for blacks are even more pronounced. The black teen-non-marital birth rate fell significantly after two years of minors' access. For women between age 19 and 24, the results are even more pronounced than for women from lower education households. Again the magnitude on the long-term effect of access on marriage conditional on birth is large enough to explain nearly all of the observed decrease.[20]

4.2 Education, Family Planning and Partner Sorting

Having examined the evidence on non-marital childbearing and marriage following a birth, we turn to understanding how the Pill altered behaviors beyond marriage and childbearing. Table 9 shows evidence from a version of Equation (3) where the dependent variable is the probability of earning degree, conditional on not having earned it in the past and being below age 30. The strongest impacts show that women exposed to Pill access as minors increase educational attainment, primarily through higher rates of earning a bachelors degree, with some weak evidence that two year degree holding also increased. The coefficients are reasonably large in column (3): over the sample the fraction of women with a bachelors degree at age 30 was 0.21. In Table 10 we split the bachelors degree hazards by race and father's education. All of the significant effects are concentrated among whites and those whose fathers had at least a high-school diploma. This follows logically from the fact that very few minority women and women from less educated households were likely to obtain a bachelors degree, regardless of access to contraceptives: the fraction of 30-year old women holding a bachelors degree was 0.103 for blacks and 0.115 for women whose fathers dropped out of high school, compared to 0.244 and 0.366 for highly-educated families and

[20]The probability of marrying following a birth was 0.79 in 1960 and 0.11 in 1988.

whites respectively. We also see positive coefficients of meaningful size on graduate degrees, but the estimates are imprecise, consistent with Goldin and Katz (2002).

A major improvement of the contraceptive Pill over prior contraceptive methods was the ability to more precisely time childbearing. Table 11 presents results on how contraceptive access affected first and second birth spacing separately for different groups of women. Estimates are analogous to the cross-sectional regressions from Equation (2), where the dependent variable is now the distance in years between the first and second birth. In column (1), marital access facilitated longer gaps between births, especially in the long term (12 years and beyond) where the average spacing increased by roughly 10 months. Columns (2)-(4) show that the increase in spacing was concentrated primarily among women from more highly educated families, who in the long-term increase spacing by 1.5 years. For women whose first birth occurred outside of marriage there is no significant evidence of longer spacing, although estimates are imprecise, likely because conditioning on a second birth shrinks the sample. Splitting the sample by education shows some increases in birth spacing among women from less educated families early on, but estimates are noisy.

Finally we examine how contraceptive access affected partner sorting. As outlined in Goldin and Katz (2002), and consistent with our education estimates, the introduction of the Pill allowed some women to complete college who in the absence of the Pill would not have done so. This increased educational level (and labor-force participation as documented in Bailey (2006)) changed the marriage prospects of these women, but also the equilibrium in the marriage market. Namely these women now bring higher earning potential into their marriages. All else equal this should mean that these women see improvements in terms of partner quality. The corresponding result in a simplified model where everyone marries is that women who did not have improved education and labor market prospects must therefore see reductions in partner quality. We test this implication of the theory in Table 12, where we regress partner log-earnings and log-wages on the same vector of covariates: indicators for contraceptive access, state fixed effects and linear trends and time fixed effects. The NSFH gathered information on respondent and cohabiting partners' (including spouses') recent annual total earnings and wage earnings. We split the estimates based on father's education given the results in Table 9 which showed women in less educated families saw no significant increase in education following Pill access. The first two columns show the striking pattern for wages: women exposed to the contraceptive Pill as minors who came from more educated families saw large increases in their partners' wages. Women from less educated families saw a nearly corresponding reduction in their partners' wages. The second two columns show that the results for earnings are nearly identical, a point which is consistent with these effects working through male education levels.[21] Thus, at least splitting by father's education, there appear to be dramatically different effects of access for different groups of women, as Goldin and Katz (2002) pointed out theoretically.

5 Conclusion

In this paper we examine the effects of plausibly exogenous introductions of the birth control Pill on a number of life-cycle outcomes. The results reveal important complexities for how fertility control, partner sorting and marital norms operate to determine the overall level of non-marital childbearing we observe today.

Examining non-marital childbearing, and probability of marriage conditional on a birth revealed that improvements in fertility control appear to have undermined the norm of shot-gun marriages to such a degree that non-marital birth rates actually increased following the introduction of the Pill for married couples. More expansive access to minors moderated to some degree increased non-marital childbearing, but we find no evidence of a reversal in the probability of marriage conditional birth. Importantly the magnitude of the effects we estimate is large enough to account for the majority of the decrease in shot-gun marriage during the sample period. Furthermore we can see even these effects are not uniformly distributed in the population: minorities and women from less educated families experienced nearly all of the increase in non-marital births and decrease in marriage following a birth. At the same time more educated and white women seemed to have captured most of the benefits from the introduction of the Pill: they saw higher education levels, increased birth spacing, and improvements in partner earnings from the introduction of Pill. Thus the evidence here supports the notion that there were contraceptive winners and losers.

[21] The NSFH lacked detailed reporting on partner completed education levels.

In terms of relevance for current policy, our results suggest that although one may be able to further improve access to the Pill even today, the larger issue of whether women and children benefit from stable relationships, and therefore prefer them to single parenthood, would seem to require a different set of policies which encourage marriage following a non-marital birth. Given the enormous drop in marriage rates following a birth our results suggest that far from solving the problem of family instability, access to the contraceptive Pill may have contributed to the destabilization of American families in the bottom quartile.

Taken together then, these preliminary findings suggest that the Pill has had not one but two effects on American women. The first effect was to improve the educational and career outcomes of women in the middle- to upper-level socio-economic brackets. This correspondingly increased the opportunity cost of having children, leading to decreased marital fertility and smaller family sizes. The second effect was to weaken the marriage prospects of women in the lowest socio-economic brackets. Without facing the same opportunity costs of childbearing as their well-educated counterparts, the weakened marriage market led to an increase in non-marital childbearing in this group. Strikingly, these two effects appear to match the observed income/education split in non-marital childbearing described above, as well as the reduced family sizes of post-1960s America. If this story is correct, it would be right to say that the Pill represents a 20th century paradox, generating historic gains for some women, and discouraging losses for others.

References

Akerlof, G., J. Yellen, and M. Katz (1996): "An Analysis of Out-of-Wedlock Childbearing in the United States," *The Quarterly Journal of Economics*, 111, 277–317.

Bailey, M. J. (2006): "More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply," *The Quarterly Journal of Economics, MIT Press*, 121, 289–320.

——— (2010): ""Momma's Got the Pill": How Anthony Comstock and Griswold v. Connecticut Shaped US Childbearing," *American Economic Review*, 100, 98–129.

Burke, J. and C. R. Pakaluk (2010): "The Contraceptive Revolution," Ph.D. thesis, Harvard University.

Chetty, R., N. Hendren, P. Kline, and E. Saez (2014): "Where is the Land of Opportunity? The Geography of Intergenerational Mobility in the United States," *NBER Working Paper*.

Edin, K. and M. Kefalas (2005): *Promises I Can Keep: Why Poor Women Put Motherhood Before Marriage*, California: University of California Press.

Goldin, C. and L. F. Katz (2002): "The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions," *Journal of Political Economy*, 110, 730–770.

Greenwood, J. and N. Gruner (2010): "Social Change," *International Economic Review*, 51, 893–923.

Kearney, M. S. and P. B. Levine (2009): "Subsidized Contraception, Fertility, and Sexual Behavior," *The Review of Economics and Statistics*, 91, 137–151.

Martin, J. A., B. E. Hamilton, S. J. Ventura, M. J. K. Osterman, and T. J. Matthews (2013): "Births: Final Data for 2011," *National Vital Statistics Reports*, 62.

McLanahan, S. (2009): "Fragile Families and the Reproduction of Poverty," *Annals of the American Academy of Political and Social Science*, 621.

Ryan, R. M. (2010): "Marital Birth and Early Child Outcomes: The Moderating Influence of Marriage Propensity," *Child Development*, 83.

Shattuck, R. M. and R. M. Kreider (2013): "Social and Economic Characteristics of Currently Unmarried Women With a Recent Birth: 2011," *American Community Survey Reports, U.S. Census Bureau*, 21.

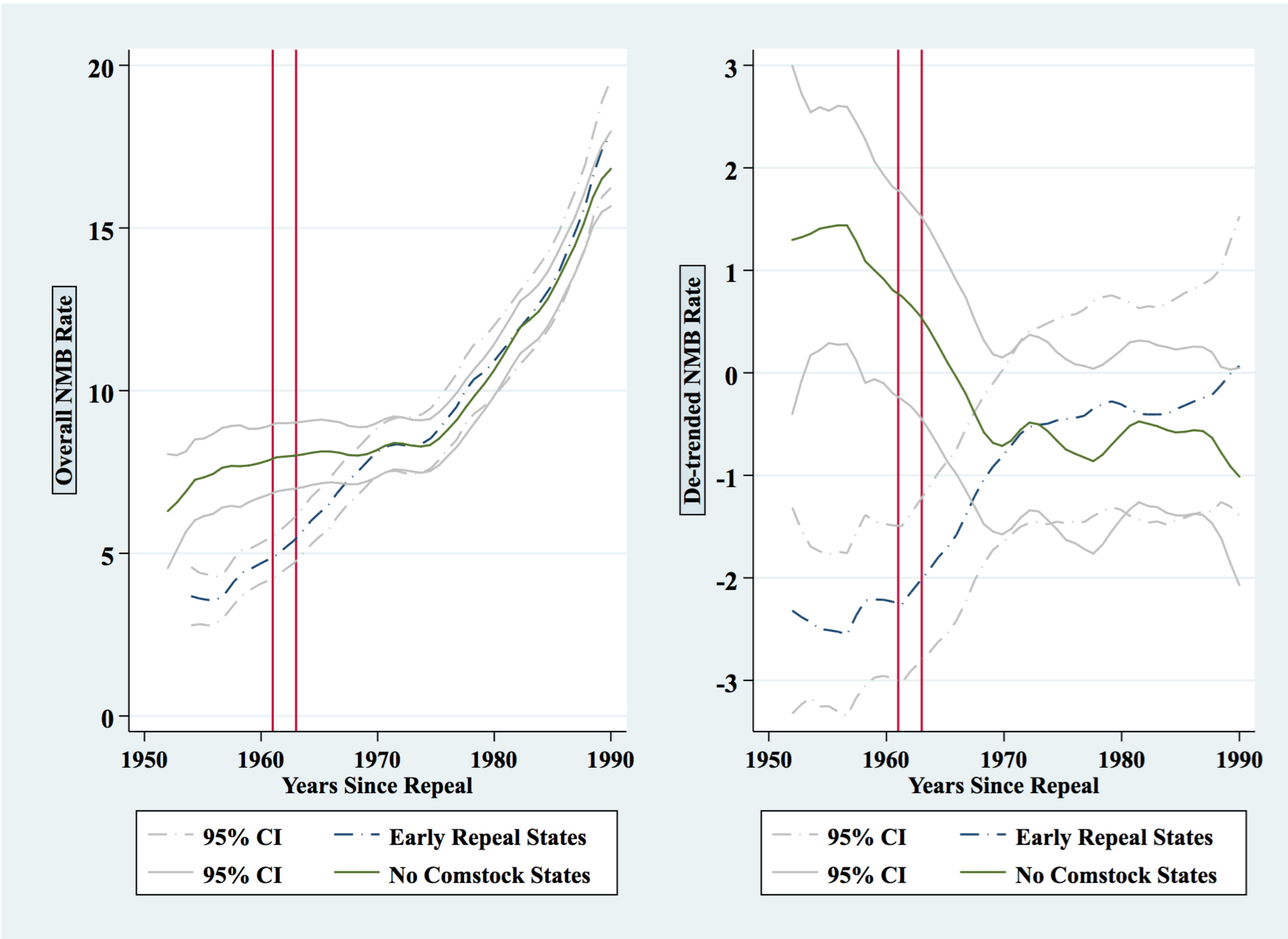
Ventura, S. J. and C. A. Bachrach (2000): "Nonmarital Childbearing in the United States, 1940-1999," *National Vital Statistics Reports*, 48.

Wildsmith, R. K. R. E. (2004): "Cohabitation and Children's Family Instability," *Journal of Marriage and Family*, 66.

Wolfers, J. (2006): "Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results," *American Economic Review*, 96, 1802–1820.

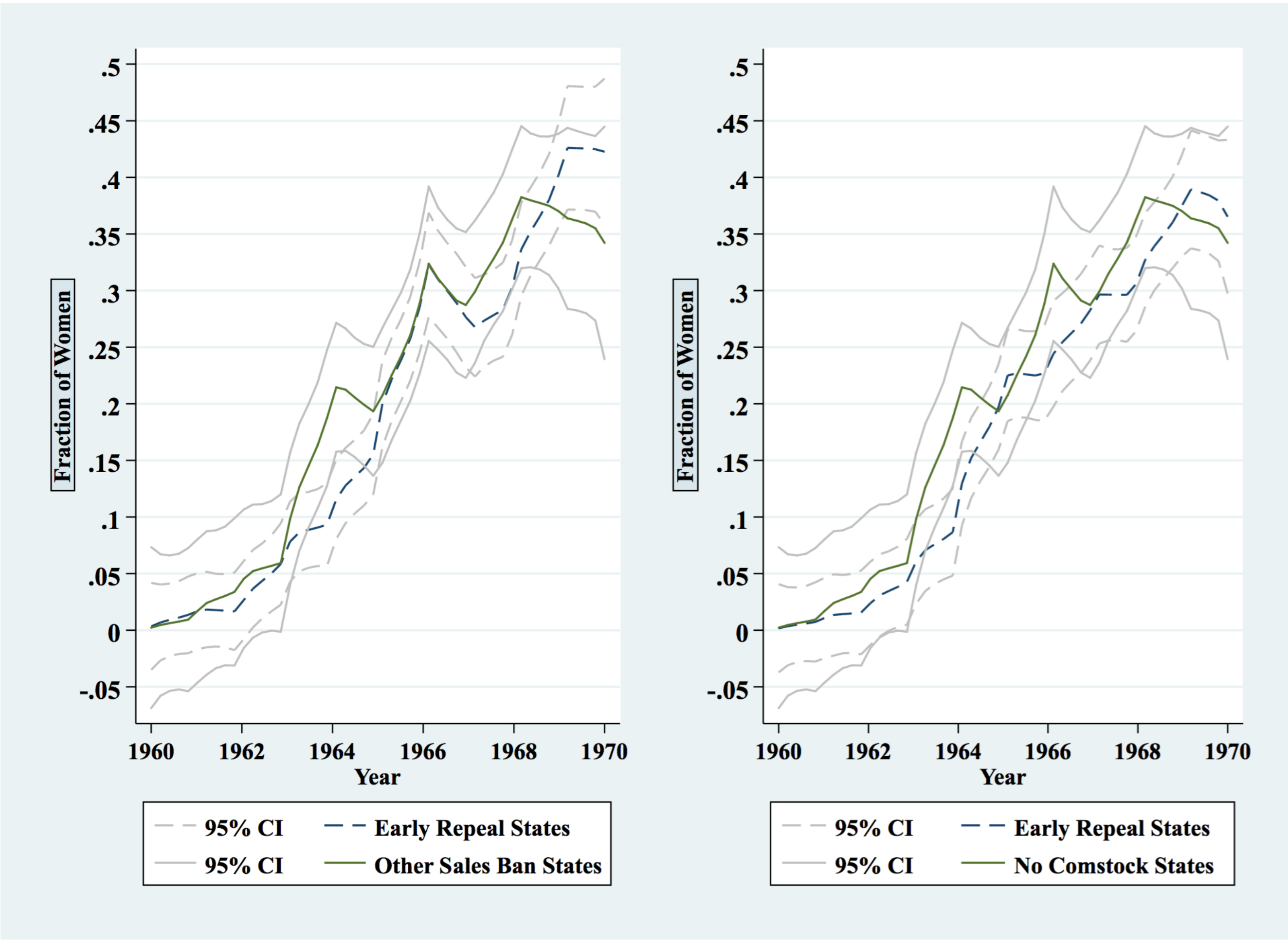
Tables and Figures

Figure 1: Non-marital Birth Rates, Early Repeal and No-Ban States



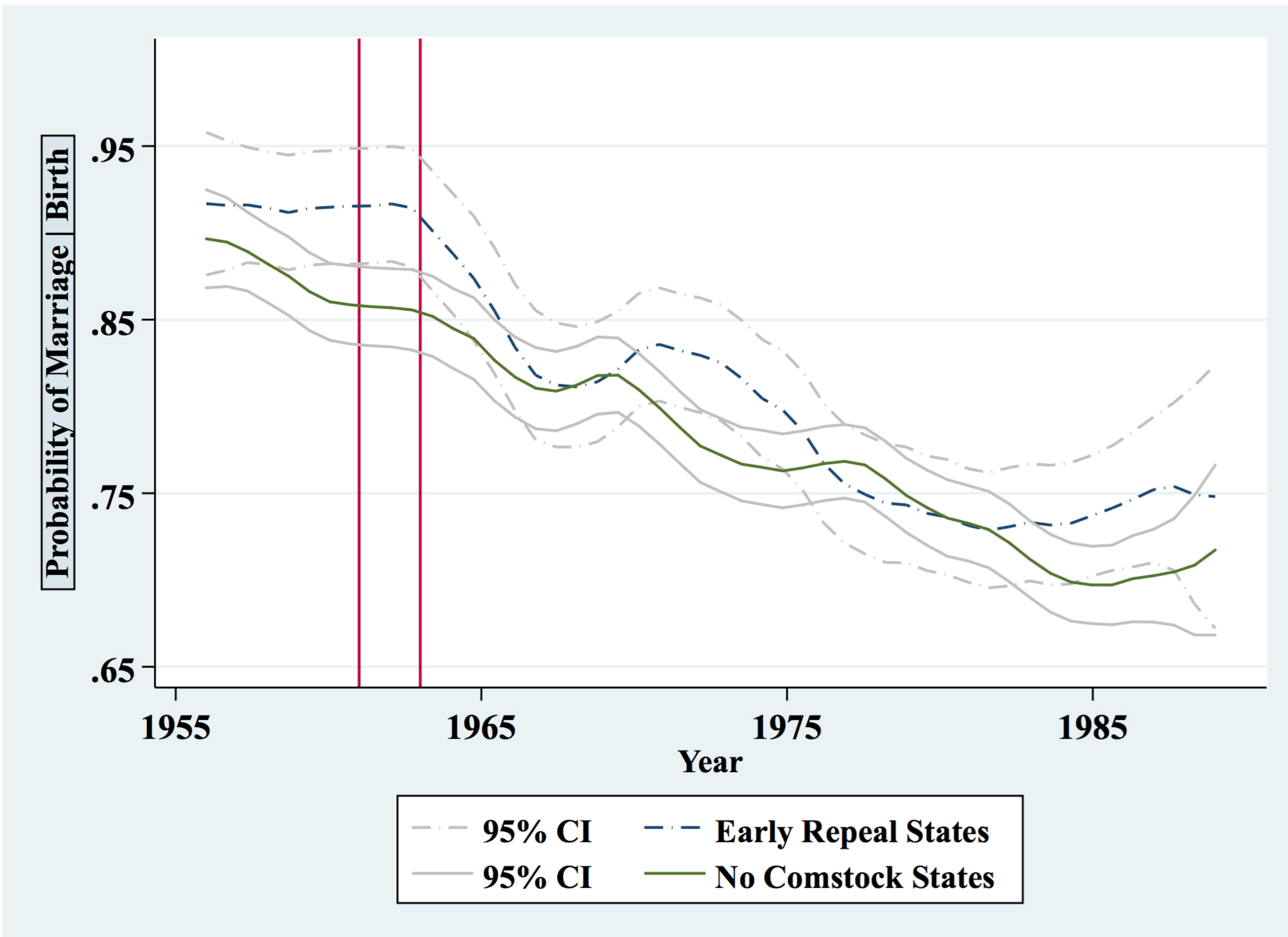
Note: The left-hand graph plots the average non-marital birth rates among states who repealed contraceptive sales bans prior to 1965 (early repeal states), and those who had no Comstock law explicitly mentioning "prevention of contraception." The right-hand graph shows the residual non-marital birth rates after removing national-year fixed effects. Early repeal dates of sales bans are indicated by the red lines.

Figure 2: Policy Spillovers: Premarital Pill Use in the NFS



Note: Data are drawn from the National Fertility Survey, 1965 and 1970. Lines plot the fraction of women reporting use of the contraceptive Pill prior to their first marriage.

Figure 3: Pill Access and Marriage Following Birth



Note: Data are drawn from the NSFH. Lines plot the fraction of women reporting marriage in the year or year following a birth, among women age 30 and under. The two lines show averages among states that repealed contraceptive sales bans prior to 1965 (early repeal states), and those that had no Comstock law explicitly mentioning "prevention of contraception."

Table 1: Contraceptive Access Across States and Time

State	Sales Ban in 1960	Sales Ban Repeal	Early Legal Access	State	Sales Ban in 1960	Sales Ban Repeal	Early Legal Access
Alabama	No	.	1971	Montana	Yes	.	1971
Alaska	No	.	1960	Nebraska	Yes	.	1972
Arizona	Yes	1963	1972	Nevada	Yes	1963	1973
Arkansas	Yes	.	1973	New Hampshire	No	.	1971
California	Yes	.	1972	New Jersey	Yes	1963	1973
Colorado	Yes	1961	1971	New Mexico	No	.	1971
Connecticut	Yes	.	1971	New York	Yes	.	1971
Delaware	Yes	.	1971	North Carolina	No	.	1971
District of Columbia	No		1971	North Dakota	No	.	1971
Florida	No	.	1973	Ohio	Yes	.	1960
Georgia	No	.	1968	Oklahoma	No	.	1972
Hawaii	No	.	1972	Oregon	Yes	.	1971
Idaho	Yes	.	1972	Pennsylvania	No	.	1970
Illinois	Yes	1961	1969	Rhode Island	No	.	1972
Indiana	Yes	1963	1973	South Carolina	No	.	1972
Iowa	Yes	.	1972	South Dakota	No	.	1972
Kansas	Yes	1963	1970	Tennessee	No	.	1971
Kentucky	No	.	1965	Texas	No	.	1973
Louisiana	No	.	1972	Utah	No	.	1975
Maine	No	.	1969	Vermont	No	.	1971
Maryland	No	.	1971	Virginia	No	.	1971
Massachusetts	Yes	.	1974	Washington	No	.	1968
Michigan	No	.	1972	West Virginia	No	.	1972
Minnesota	Yes	.	1972	Wisconsin	Yes	.	1972
Mississippi	Yes	.	1966	Wyoming	Yes	.	1969
Missouri	Yes	.	1973				

Note: Federal Comstock laws allowed the sales of contraceptives in the District of Columbia and Federally administered territories (AK and HI prior to 1959). All other reform information is drawn from Bailey (2010) and Bailey (2006) legal appendices.

Table 2: Policy Endogeneity: Predicting Contraceptive Reforms

	Marital	Minors	Marital	Minors
Lagged NMB rate	-0.0003	0.0051**	0.0003	0.0006
	(0.0008)	(0.0024)	(0.0048)	(0.0095)
State FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
N	269	524	269	524
R^2	0.0001	0.008	0.149	0.596

Note: Regressions predict early enactment of a Comstock repeal and changes in the age-at-majority using the one period lag of overall non-marital childbearing. Standard errors are in parentheses.

Table 3: State-Level Non-marital Birth Rates

Marital Access for:	(1) Overall	(2) Young Adults	(3) Older Adults
-3 to -1 years	0.162	0.212	0.100
	(0.222)	(0.281)	(0.075)
0 to 2 years	0.523*	0.737*	0.215*
	(0.284)	(0.400)	(0.117)
3 to 5 years	0.886**	1.217*	0.397*
	(0.423)	(0.645)	(0.213)
6 to 8 years	1.001*	1.355	0.536*
	(0.536)	(0.848)	(0.280)
9 to 11 years	1.272**	1.710*	0.638*
	(0.632)	(1.000)	(0.323)
≥ 12 years	1.210	1.663	0.590
	(0.731)	(1.148)	(0.363)
Minor Access for:			
≤ 2 years	-0.081	-0.177	0.017
	(0.203)	(0.272)	(0.056)
3 to 5 years	-0.345	-0.591	-0.011
	(0.296)	(0.389)	(0.095)
6 to 8 years	-0.473	-0.832	-0.021
	(0.365)	(0.510)	(0.142)
9 to 11 years	-0.931	-1.524*	-0.125
	(0.570)	(0.797)	(0.210)
≥ 12 years	-1.055	-1.780*	-0.174
	(0.720)	(1.021)	(0.275)
Observations	1519	1519	1519
Adjusted R^2	0.978	0.978	0.894
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State-linear Trends	Yes	Yes	Yes

Note: Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1% levels respectively. The non-marital birth rate is the number of non-marital births to 1,000 women in a given age-group. These data come from the National Vital Statistics System.

Table 4: 1987-1988 Cross Section, Non-marital First Birth

	(1)	(2)	(3)
Marital Access for:	LPM	LPM	Logit
$\leq$ 2 years	0.064**	0.067**	0.095*
	(0.030)	(0.030)	(0.054)
3 to 5 years	0.050**	0.053**	0.078*
	(0.021)	(0.022)	(0.040)
6 to 8 years	0.078**	0.080**	0.125**
	(0.030)	(0.031)	(0.058)
9 to 11 years	0.100***	0.107***	0.171***
	(0.030)	(0.030)	(0.060)
$\geq$ 12 years	0.118***	0.123***	0.179**
	(0.036)	(0.038)	(0.084)
Minor Access for:			
$\leq$ 2 years	0.020	0.021	0.014
	(0.028)	(0.028)	(0.022)
3 to 5 years	-0.020	-0.022	-0.014
	(0.036)	(0.036)	(0.023)
6 to 8 years	-0.042	-0.044	-0.023
	(0.048)	(0.047)	(0.028)
9 to 11 years	-0.050	-0.050	-0.026
	(0.060)	(0.059)	(0.033)
$\geq$ 12 years	-0.034	-0.034	-0.016
	(0.060)	(0.061)	(0.039)
Observations	7200	7017	7017
Adjusted R^2	0.192	0.193	
Pseudo R^2			0.235
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State-linear Trends	Yes	Yes	Yes

Note: Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1% levels respectively. Column (2) conditions on being in the logistic regression sample. Data are drawn from the cross-section of women in the NSFH.

Table 5: P(Non-marital Birth)

Marital Access for:	(1) Teens	(2) Young Adults	(3) Adults
$\leq$ 2 years	-0.011	-0.008	-0.001
	(0.008)	(0.008)	(0.003)
3 to 5 years	-0.011	0.017*	-0.001
	(0.010)	(0.010)	(0.003)
6 to 8 years	-0.019	0.024**	-0.001
	(0.012)	(0.010)	(0.004)
9 to 11 years	-0.022	0.027**	0.000
	(0.013)	(0.011)	(0.004)
$\geq$ 12 years	-0.017	0.040***	-0.002
	(0.015)	(0.013)	(0.005)
Minor Access for:			
$\leq$ 2 years	-0.008	-0.021***	-0.002
	(0.007)	(0.006)	(0.002)
3 to 5 years	-0.020**	-0.017**	-0.005*
	(0.009)	(0.008)	(0.002)
6 to 8 years	-0.031***	-0.010	-0.000
	(0.011)	(0.009)	(0.003)
9 to 11 years	-0.036**	-0.016	0.001
	(0.015)	(0.011)	(0.004)
$\geq$ 12 years	-0.052***	-0.011	0.003
	(0.020)	(0.013)	(0.004)
Observations	23194	34002	70233
Adjusted R^2	0.033	0.029	0.014
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State-linear Trends	Yes	Yes	Yes

Note: Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1% levels respectively. Coefficients report the change in the discrete-time (annual) hazard into non-marital birth for women aged 15 to 45. Teens are age $\leq$ 18, Young Adults are 19 to 24, and Adults are 25 to 45. All models include cohort and (pregnancy) age fixed effects. These data come from the NSFH.

Table 6: P(Marriage | Birth)

Marital Access for:	(1) Teens	(2) Young Adults	(3) Adults
$\leq$ 2 years	-0.152	-0.002	0.013
	(0.104)	(0.032)	(0.026)
3 to 5 years	-0.030	-0.099**	0.011
	(0.112)	(0.047)	(0.035)
6 to 8 years	0.023	-0.128**	-0.009
	(0.122)	(0.050)	(0.043)
9 to 11 years	0.155	-0.142**	0.007
	(0.142)	(0.059)	(0.051)
$\geq$ 12 years	0.093	-0.219***	0.011
	(0.159)	(0.068)	(0.061)
Minor Access for:			
$\leq$ 2 years	-0.025	0.087***	0.026
	(0.057)	(0.026)	(0.020)
3 to 5 years	0.063	0.071*	0.074***
	(0.084)	(0.037)	(0.024)
6 to 8 years	0.105	0.042	0.025
	(0.096)	(0.045)	(0.031)
9 to 11 years	0.075	0.045	0.006
	(0.120)	(0.055)	(0.038)
$\geq$ 12 years	0.042	0.054	-0.002
	(0.149)	(0.066)	(0.044)
Observations	2125	9301	9747
Adjusted R^2	0.247	0.217	0.153
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State-linear Trends	Yes	Yes	Yes

Note: Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1% levels respectively. Coefficients report the change in the discrete-time (annual) hazard into marriage conditional on birth in the present or prior year among women aged 15 to 45. Teens are age $\leq$ 18, Young Adults are 19 to 24, and Adults are 25 to 45. All models include cohort and (marriage) age fixed effects. These data come from the NSFH.

Table 7: Heterogeneity in Non-marital Childbearing Across SES

	P(Non-marital Birth)				P(Marriage | Birth)	
	Teens		Young Adults		Young Adults	
Marital Access for:	Low SES	High SES	Low SES	High SES	Low SES	High SES
$\leq$ 2 years	-0.021	-0.004	-0.006	-0.010	-0.017	-0.018
	(0.013)	(0.009)	(0.010)	(0.010)	(0.039)	(0.057)
3 to 5 years	-0.011	-0.012	0.027*	0.002	-0.160***	0.008
	(0.016)	(0.012)	(0.014)	(0.012)	(0.062)	(0.071)
6 to 8 years	-0.020	-0.017	0.048***	-0.009	-0.200***	-0.009
	(0.019)	(0.014)	(0.016)	(0.012)	(0.063)	(0.080)
9 to 11 years	-0.025	-0.019	0.047***	-0.000	-0.202***	-0.079
	(0.020)	(0.016)	(0.016)	(0.015)	(0.074)	(0.100)
$\geq$ 12 years	-0.011	-0.025	0.068***	0.006	-0.278***	-0.169
	(0.023)	(0.018)	(0.019)	(0.018)	(0.084)	(0.119)
Minor Access for:						
$\leq$ 2 years	-0.011	-0.005	-0.032***	-0.010	0.114***	0.032
	(0.012)	(0.008)	(0.009)	(0.008)	(0.032)	(0.042)
3 to 5 years	-0.029*	-0.011	-0.030**	-0.003	0.099**	0.014
	(0.016)	(0.010)	(0.012)	(0.010)	(0.047)	(0.059)
6 to 8 years	-0.053***	-0.011	-0.018	-0.001	0.068	-0.047
	(0.019)	(0.013)	(0.014)	(0.012)	(0.055)	(0.076)
9 to 11 years	-0.045*	-0.026	-0.025	-0.006	0.059	-0.018
	(0.025)	(0.017)	(0.017)	(0.015)	(0.069)	(0.090)
$\geq$ 12 years	-0.078**	-0.028	-0.018	-0.000	0.072	-0.000
	(0.032)	(0.024)	(0.022)	(0.016)	(0.085)	(0.108)
Observations	12800	10394	18895	15107	6179	3122
Adjusted R^2	0.032	0.022	0.029	0.022	0.231	0.232
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-linear Trends	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1% levels respectively. Coefficients report the change in the discrete-time (annual) hazard into marriage conditional on birth in the present or prior year among women aged 15 to 24. Teens are age $\leq$ 18, Young Adults are 19 to 24. All models include cohort and (marriage or pregnancy) age fixed effects. SES is measured with Low as (a woman's) father's education less than a high school diploma; High as 12 or more years of completed schooling. These data come from the NSFH.

Table 8: Heterogeneity in Non-marital Childbearing Across Races

	P(Non-marital Birth)				P(Marriage | Birth)	
	Teens		Young Adults		Young Adults	
Marital Access for :	Black	White	Black	White	Black	White
$\leq$ 2 years	-0.017	-0.008	0.012	-0.014**	-0.145	0.034
	(0.033)	(0.008)	(0.028)	(0.007)	(0.109)	(0.030)
3 to 5 years	0.028	-0.016	0.093***	-0.004	-0.456***	-0.007
	(0.045)	(0.010)	(0.033)	(0.010)	(0.140)	(0.046)
6 to 8 years	0.009	-0.013	0.166***	-0.004	-0.646***	-0.003
	(0.053)	(0.012)	(0.043)	(0.009)	(0.144)	(0.048)
9 to 11 years	0.039	-0.027*	0.130***	0.000	-0.559***	-0.034
	(0.056)	(0.014)	(0.043)	(0.010)	(0.168)	(0.060)
$\geq$ 12 years	0.002	-0.001	0.170***	0.007	-0.713***	-0.091
	(0.069)	(0.018)	(0.050)	(0.012)	(0.197)	(0.073)
Minor Access for:						
$\leq$ 2 years	-0.025	0.001	-0.052***	-0.008	0.187***	0.029
	(0.024)	(0.006)	(0.019)	(0.006)	(0.062)	(0.027)
3 to 5 years	-0.069**	0.002	-0.042	0.001	0.139	-0.009
	(0.030)	(0.009)	(0.026)	(0.008)	(0.087)	(0.038)
6 to 8 years	-0.101***	-0.002	-0.029	0.006	0.082	-0.033
	(0.034)	(0.011)	(0.029)	(0.010)	(0.099)	(0.050)
9 to 11 years	-0.088**	-0.008	-0.024	0.001	0.096	-0.039
	(0.044)	(0.015)	(0.035)	(0.012)	(0.109)	(0.061)
$\geq$ 12 years	-0.142**	-0.016	0.005	-0.002	0.147	-0.040
	(0.060)	(0.020)	(0.041)	(0.013)	(0.124)	(0.075)
Observations	4831	16785	7096	24635	2307	6266
Adjusted R^2	0.019	0.020	0.028	0.008	0.270	0.101
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-linear Trends	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1% levels respectively. Coefficients report the change in the discrete-time (annual) hazard into marriage conditional on birth in the present or prior year among women aged 15 to 24. Teens are age $\leq$ 18, Young Adults are 19 to 24. All models include cohort and (marriage or pregnancy) age fixed effects. These data come from the NSFH.

Table 9: Effects of Pill Access on Degree Holding

	Degree:			
Marital Access for:	High School	2-year	Bachelor's	Graduate
$\leq$ 2 years	0.010	0.001	0.000	-0.017
	(0.014)	(0.003)	(0.007)	(0.025)
3 to 5 years	-0.010	-0.001	-0.003	0.002
	(0.015)	(0.004)	(0.007)	(0.024)
6 to 8 years	0.012	0.001	-0.008	0.002
	(0.017)	(0.005)	(0.008)	(0.026)
9 to 11 years	-0.010	0.003	-0.010	-0.032
	(0.018)	(0.005)	(0.009)	(0.028)
$\geq$ 12 years	0.007	0.000	-0.011	-0.025
	(0.021)	(0.006)	(0.010)	(0.034)
Minor Access for:				
$\leq$ 2 years	0.010	0.000	0.010**	0.013
	(0.010)	(0.003)	(0.005)	(0.018)
3 to 5 years	0.009	0.004	0.005	0.012
	(0.012)	(0.004)	(0.006)	(0.021)
6 to 8 years	0.010	0.003	0.006	0.013
	(0.014)	(0.004)	(0.006)	(0.024)
9 to 11 years	0.002	0.003	0.016**	0.006
	(0.016)	(0.005)	(0.007)	(0.027)
$\geq$ 12 years	0.011	0.010*	0.018**	0.005
	(0.020)	(0.006)	(0.008)	(0.029)
Observations	36117	46110	42520	7419
Adjusted R^2	0.166	0.008	0.034	0.013
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-linear Trends	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1% levels respectively. Coefficients report the change in the discrete-time (annual) hazard into degree holding, among women less than 30. All models include cohort and (degree) age fixed effects. These data come from the NSFH.

Table 10: Heterogeneity in Bachelor's Degree Holding

	P(Bachelor's Degree)			
Marital Access for:	White	Non-white	Low SES	High SES
$\leq$ 2 years	-0.000	-0.005	-0.010	0.004
	(0.008)	(0.010)	(0.006)	(0.015)
3 to 5 years	-0.004	-0.007	-0.005	-0.008
	(0.008)	(0.012)	(0.008)	(0.014)
6 to 8 years	-0.008	-0.011	-0.005	-0.020
	(0.009)	(0.014)	(0.009)	(0.015)
9 to 11 years	-0.007	-0.025*	-0.009	-0.013
	(0.010)	(0.014)	(0.010)	(0.017)
$\geq$ 12 years	-0.008	-0.022	-0.007	-0.015
	(0.012)	(0.017)	(0.012)	(0.020)
Minor Access for:				
$\leq$ 2 years	0.013**	-0.001	0.001	0.013
	(0.006)	(0.008)	(0.005)	(0.010)
3 to 5 years	0.008	-0.004	-0.005	0.012
	(0.007)	(0.009)	(0.005)	(0.011)
6 to 8 years	0.009	-0.002	0.000	0.013
	(0.008)	(0.011)	(0.007)	(0.012)
9 to 11 years	0.020**	0.002	-0.002	0.029**
	(0.009)	(0.012)	(0.008)	(0.014)
$\geq$ 12 years	0.025**	-0.003	0.003	0.032**
	(0.010)	(0.013)	(0.009)	(0.015)
Observations	32593	9927	16821	20442
Adjusted R^2	0.038	0.021	0.011	0.044
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-linear Trends	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1% levels respectively. Coefficients report the change in the discrete-time (annual) hazard into Bachelor's degree holding, among women less than 30. All models include cohort and (degree) age fixed effects. Low SES and High SES are measured with (a woman's) father's education less than 12 years and greater than 12 years respectively. These data come from the NSFH.

Table 11: Heterogeneity in 1st-to-2nd Birth Spacing

Marital Access for:	Overall	Non-marital 1st Birth	High SES	Low SES
$\leq$ 2 years	0.158	-0.762	0.304	0.128
	(0.245)	(1.417)	(0.381)	(0.507)
3 to 5 years	0.477*	-0.494	0.855**	0.822*
	(0.269)	(1.440)	(0.393)	(0.458)
6 to 8 years	0.354	-0.423	0.796	0.573
	(0.359)	(1.820)	(0.519)	(0.711)
9 to 11 years	0.331	-0.399	0.745	0.841
	(0.357)	(2.157)	(0.525)	(0.697)
$\geq$ 12 years	0.821**	0.309	1.411**	0.702
	(0.348)	(2.424)	(0.590)	(0.651)
Minor Access for:				
$\leq$ 2 years	0.407*	0.251	0.645*	0.407
	(0.237)	(0.437)	(0.363)	(0.500)
3 to 5 years	0.359	0.355	0.591	0.825
	(0.239)	(0.616)	(0.394)	(0.545)
6 to 8 years	0.110	0.259	0.330	0.047
	(0.321)	(0.801)	(0.443)	(0.558)
9 to 11 years	-0.103	-0.172	-0.305	-0.004
	(0.358)	(0.821)	(0.486)	(0.690)
$\geq$ 12 years	0.168	-0.202	0.443	-0.331
	(0.465)	(1.665)	(0.801)	(0.753)
Observations	4309	829	1414	1908
Adjusted R^2	0.038	0.142	0.114	0.053
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-linear Trends	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1% levels respectively. Coefficients report the change in months between a woman's first and second birth (conditional on having at least two live births). Low SES and High SES are measured with (a woman's) father's education less than 12 years and greater than 12 years respectively. These data come from the NSFH.

Table 12: Effects of Access on Spousal Characteristics

	log(Husband Wage)		log(Husband Earnings)	
Marital Access for:	High SES	Low SES	High SES	Low SES
$\leq$ 2 years	0.098	0.267	0.010	0.028
	(0.173)	(0.160)	(0.168)	(0.218)
3 to 5 years	-0.087	0.457**	-0.155	0.442**
	(0.217)	(0.180)	(0.215)	(0.167)
6 to 8 years	-0.047	0.381	-0.075	0.368
	(0.193)	(0.302)	(0.188)	(0.265)
9 to 11 years	0.120	0.354	0.040	0.412*
	(0.242)	(0.234)	(0.239)	(0.243)
$\geq$ 12 years	0.044	0.288	-0.059	0.344
	(0.307)	(0.263)	(0.284)	(0.249)
Minor Access for:				
$\leq$ 2 years	0.239**	0.020	0.235***	-0.042
	(0.097)	(0.155)	(0.077)	(0.147)
3 to 5 years	0.277**	-0.389*	0.284**	-0.434**
	(0.120)	(0.214)	(0.127)	(0.205)
6 to 8 years	0.319*	-0.581*	0.401*	-0.590*
	(0.179)	(0.322)	(0.201)	(0.310)
9 to 11 years	0.571***	-0.729**	0.594**	-0.681**
	(0.207)	(0.326)	(0.228)	(0.302)
$\geq$ 12 years	0.864***	-0.750	0.764***	-0.773*
	(0.248)	(0.493)	(0.278)	(0.444)
Observations	1405	1199	1464	1266
Adjusted R^2	0.164	0.116	0.149	0.132
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-linear Trends	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1% levels respectively. Coefficients report the change in the log of 1986 wages and total earnings of the respondent's husband (or male co-resident partner). Low SES and High SES are measured with (a woman's) father's education less than 12 years and greater than 12 years respectively. These data come from the NSFH.