The Power of Abortion Policy: Re-examining the effects of young women's access to reproductive control

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Abstract

I provide new evidence on the relative "powers" of contraception and abortion policy in effecting the dramatic social transformations of the 1960s and 1970s. Trends in sexual behavior suggest that young women's increased access to the birth control pill fueled the sexual revolution, but neither these trends nor difference-in-difference estimates support the view that this also led to substantial changes in family formation. Rather, the estimates robustly suggest that it was liberalized access to abortion that allowed large numbers of women to delay marriage and motherhood.

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1 Introduction

The oral contraceptive pill was introduced in 1960 at the inception of two decades of profound social and economic change characterized by sexual revolution, delayed marital formation, reduced fertility, and increased gender equality in the labor force. Journalists and commentators have credited these transitions to the pill, hailing it as "the most important scientific advance of the twentieth century" (*The Economist*, 1999) and"the pill that changed the world" (Asbell, 1995).

Empiricists seeking to evaluate such claims are stymied by non-random variation in the existence of pill *technology*, and instead must rely on variation in pill *policy*. In an influential paper, Goldin and Katz (2002) exploit variation in state policies governing young women's legal right to consent to contraceptive services. Using a difference-in-difference framework, they estimate that among college graduates, women who could legally consent to the pill before reaching age 18 married later and were more likely to pursue graduate education. Subsequent researchers adapted Goldin and Katz' identification strategy– as well, in many cases, as their "power of the pill" title– to produce additional evidence on the role of pill policy in causing reduced fertility (Bailey, 2006, 2009; Ananat and Hungerman, 2008), increased educational attainment (Hock, 2008), increased marital stability (Zuppann, 2012), improved long-run outcomes for children (Ananat and Hungerman, 2008), reduced crime (Pantano, 2007) and increased female labor supply, wages and occupational diversity (Bailey, 2006; Steingrimsdottir, 2010; Bailey et al., 2012).

These results appeal to conventional wisdom about the pill's role in the changes that swept the United States in the 1960s and 1970s, but but from a broader vantage they are perhaps more surprising. Fluctuations in fertility have occurred in the absence of technological innovation in contraception in other places and other times. The United States and France, for example, experienced reductions in fertility throughout the 19th century, beginning well before the introduction of vulcanized rubber condoms and diaphragms (Guinnane, 2011). In Japan the pill and other forms of hormonal contraception were not available until the late 1990s, but fertility and marriage nonetheless declined precipitously in the late 1940s, eventually reaching levels that were among the lowest in the world (Norgren, 2001; Retherford et al., 2001; Sato and Iwasawa, 2006). Examining the relationship between the pill and fertility in 21 developed countries from 1960 to 2002, Leridon (2006) concludes "within individual countries, there is unambiguously no systematic negative correlation between fertility and contraceptive pill use."

In this paper, I revisit the causes of demographic change in the United States in the 1960s and 1970s, focusing on age at first marriage and birth, family formation outcomes that are closely linked to human capital investments (Goldin and Katz, 2002; Bailey, 2006). The birth control pill's effects on family formation are theoretically ambiguous: The pill was a technological innovation in contraception, but with a failure rate of about 9 percent in the first year of typical use (Trussell, 2004), it still provides an imperfect means of preventing pregnancy. Trends in sexual behavior suggest that any reductions in unintended pregnancies among teens due to safer, pill-protected sex were offset by large increases in sexual activity. Difference-in-difference estimates also provide little evidence to support the view that pill policies had a substantial influence on age at first birth and marriage. Results in Goldin and Katz (2002) and Bailey (2006, 2009) that suggest otherwise are not robust to reasonable perturbations of the authors' research designs including addressing discrepancies in the legal codings, choosing alternative data sets, and/or adjusting sample selection procedures. Rather, the results robustly point to policies governing abortion, a second, less lauded but more certain means of preventing unwanted births, as the driving force behind delayed family formation in the 1970s.

In the analysis that follows, I distinguish between three possible environments governing access to both the pill and abortion: (1) the method was not legally available, (2) the method was legally available to adult women but young unmarried women could not provide legal consent, and (3) the method was legally available and young unmarried women could consent, providing them with "confidential access" without involving a parent. Using data from both 1979-1995 Current Population Survey (CPS) June Fertility supplements and the 1980 IPUMS U.S. Census 1 percent sample, I estimate difference-in-difference models designed to disentangle the effects of reproductive policies. The results suggest that policies governing access to the pill had little if any effect on the average probabilities of marrying and giving birth at a young age. In contrast, policy environments in which abortion was legal and readily accessible by young women are estimated to have caused a 34 percent reduction in first births, a 19 percent reduction in first marriages, and a 63 percent reduction in "shotgun marriages" prior to age 19.

2 Some suggestive trends

When the pill was introduced in 1960, women under the age of 21 could not readily access it in most states. In the ensuing years, states extended the right to consent to contraceptive services to young people as they lowered the age of majority, generally from 21 to 18, and enacted new medical consent laws defining circumstances under which legal minors could provide consent to medical care. These laws allowed young women rather than their parents to consent to contraception, a situation that I will term "confidential access" because it allowed them to receive contraceptive services without involving anyone else save their provider.[1] Most of the legal changes granting confidential access occurred between 1969 and 1974 when thirty-five states expanded the right to consent to the pill to young unmarried women (Myers, 2015).

[1] Bailey (2006) refers to this as "early legal access" (ELA). I do not adopt the same terminology because it suggests that absent ELA young women could not access the pill, when in fact they could legally do so if a parent provided consent.

As a first step in examining the potential impact of pill policy on fertility, I examine trends in sexual behavior for cohorts born between 1935 and 1960, who came of age before, during and after this rapid expansion in reproductive rights. Unfortunately, only two historical surveys directly asked young unmarried women about sexual behavior, and both are small cross sections.[2] To expand beyond the snapshots of sexual activity afforded by these studies, I summarize information by cohort of birth in Cycles 3-5 of the National Survey of Family Growth (NSFG), a nationally representative survey of women aged 15-44 that contains retrospective information about the age at and characteristics of first sexual intercourse (U.S. Dept. of Health and Human Services, 1982, 1988, 1995).[3]

Figure 1 summarizes age at first voluntary sexual intercourse by birth cohort. Age at first sexual intercourse changes little for women born in the forties, cohorts that generally could not consent to the pill until after marriage or the age twenty-one, by which time most of them report they had already had sex. The probability of having had sex as a teen rose dramatically between the 1950 and 1955 birth cohorts of women who came of age during the period of rapid pill diffusion between 1969 and 1974. These same cohorts of women are also become much more likely to have sought family planing services, as shown in Figure 2.

These trends support the view that the diffusion of the pill to young women may have contributed to the sexual revolution, but the effect on fertility is ambiguous. As a back-of-the-envelope calculation, begin with the assumption that between the 1950 and 1955 cohorts, all sexually active young women substituted from condoms, with a 15 percent failure rate, to the birth control pill, with a

[2]The surveys are the National Survey of Young Women (1971) and The National Survey of Adolescent Female Sexual Behavior (1976). The National Fertility Survey, conducted in 1965, 1970, and 1975, asked some retrospective questions about the use of contraception, but the samples are limited to married or ever-married women. See Goldin and Katz (2002) for more information.

[3]The lack of state identifiers in the public-use NSFG data precludes use of a difference-in-difference estimation framework. Even with this information, the sample size is much smaller than in the CPS, and estimates likely would be imprecise.

9 percent failure rate (Trussell, 2004).[4] Under this extreme assumption, the fraction of sexually active women experiencing an unintended pregnancy in the first year of contraceptive use would have declined by about 6 percentage points. Consider, however, the unconditional probability of pregnancy. The number of sexually active women was increasing markedly. Between the 1950 and 1955 birth cohorts, the fraction of women having sex prior to age 18 increased from 34 to 47 percent. Assuming that all of the women who became sexually active used the pill, overall pregnancy rates would have decreased by only 1 percentage point, one sixth the size of the reduction conditional on sexual activity.[5]

More moderate assumptions about pill take-up lead to predictions that the pill might even have led to an increase in the birth rate. There is a large gap between rates of sexual activity (Figure 1) and use of family planning services (Figure 2), suggesting that it is improbable that large fractions of women were substituting from condoms to the pill. In fact, the fraction of women using family planning services by age 18 increases by 12 percentage points between the 1950 and 1955 cohorts, roughly equal to the increase in sexual activity. In keeping with this observation, suppose that the increase in pill use is fully explained by women substituting from abstinence to the pill; in this case the pregnancy rate would have *increased* by 1 percentage point because the increase

[4] Both of these failure rates are "typical use" failure rates that account for the average experience and errors made by women using each form contraception in the first year following take-up. The estimated typical-use failure rates in Trussell (2004) are based on the experiences of women in 1995 National Survey of Family Growth (NSFG). Comparable estimates are not available for the 1970s because the first waves of the NSFG only surveyed married women, but it seems a reasonable assumption that they were similar. Schirm et al. (1982) estimate that the typical-use failure rate of the pill was 2.4 percent among *married* women in the 1972 and 1976 NSFG. Using the 1982 wave of the NSFG, which allowed for comparison between married and unmarried women, Grady et al. (1986) estimate that the typical use failure rate for unmarried women was 6 percent, twice as high as that for married women. Finally, all authors who have estimated typical use failure rates for the pill by demographic status have found that it is substantially higher than the perfect use failure rates, and particularly high among young unmarried women, the subjects of this analysis (Schirm et al., 1982; Grady et al., 1986; Fu et al., 1999; Trussell and Vaughn, 1999). Grady et al. (1986), using the 1982 NSFG, estimate that typical use failure rates among unmarried women aged 18 to 19 was 9.6 percent.

[5] The change in the unconditional probability is calculated as follows : $[Pr(pregnancy|sexuallyactive)]_{1955} * [Pr(sexuallyactive)]_{1955} - [Pr(pregnancy|sexuallyactive)]_{1950} * [Pr(sexuallyactive)]_{1950} = .09 * .47 - .15 * .34 = -0.01$.

in sexual activity more than offset the increase in contraceptive efficacy.[6]

These examples illustrate that it is not reasonable to expect pill diffusion to correspond to dramatic changes in fertility when it also is coincident with an increase in sexual activity. Trends in fertility support this conclusion: As shown in Figure 3, cohorts that experienced the most rapid changes in sexual behavior exhibited little change in fertility. This figure describes the cumulative probabilities of marrying and giving birth for cohorts of women born between 1935 and 1960 observed in the 1979-1995 CPS June Fertility Supplements (U.S. Dept. of Commerce, Burea of the Census 1979-1995).[7] There is a downward trend that begins with the 1940 cohort, which was 20 years old when the pill was introduced, but this is largely arrested between the 1950 to 1955 cohorts who experienced rapid diffusion of the pill. In fact, the probabilities of teen motherhood increase slightly over these cohorts; for example, the probability of becoming a mother before age 19 rises by 2 percentage points between the 1950 and 1955 birth cohorts.

Even if a pill-fueled sexual revolution did not affect fertility, it might have caused women to delay marriage by decreasing the cost of remaining single. However, trends in age at first marriage observed in Figure 3 are similar to those for births: The probabilities of marrying prior to various young ages decline over the 1940s cohorts, stall for early 1950s cohorts, and begin falling again over the 1955 to 1960 cohorts. The re-commencement of delayed family formation with the 1955 cohort could reflect a delayed impact of pill diffusion, but it also coincides with increased access to abortion. Between 1969 and 1972, five "repeal states" plus the District of Columbia legalized abortion under most

[6] The change in the unconditional probability is calculated as follows : $[Pr(pregnancy|sexuallyactive)]_{1955} * [Pr(sexuallyactive)]_{1955} - [Pr(pregnancy|sexuallyactive)]_{1950} * [Pr(sexuallyactive)]_{1950} = (.15 * .34 + .09 * .12) - .15 * .35 = 0.01$.

[7] I use the CPS June Fertility Supplement for every year between 1979 and 1995 in which it is available and contains information about both age at first marriage and birth. These years are 1979-1983, 1985-1988, 1990, 1992, and 1995. The sample used to create these summary statistics is limited to women aged 24 and older born from 1935 to 1960, yielding a total sample size of $303,274$.

circumstances, and the 1955 birth cohort was 17 to 18 years old in January of 1973 when the U.S. Supreme Court decision in *Roe v. Wade* had the effect of legalizing abortion nationwide.[8]

The effect of abortion legalization on fertility depends on its effect on sexual behavior and also on the degree to which legality impacted the cost of obtaining an abortion. Even when abortions were illegal, some physicians were willing to provide illegal abortions to trusted patients or to refer them to another physician, either locally or abroad, who would provide the service (see, e.g. Joffe, 1995).[9] However, some women did not have access to a physician or the resources to travel to obtain a legal abortion. Abortion surveillance figures are unavailable for illegal abortions, so to attempt to measure trends in abortion I use a question from Cycle 4 of the NSFG that solicited retrospective self-reports.[10] Figure 4 illustrates that the fraction of women reporting an abortion increased dramatically as the procedure became legal. Although it is likely that women were more likely to report legal than illegal abortions, the general trend is consistent with the hypothesis that the risks and costs of obtaining an illegal abortion were prohibitive for many young women.

Further support of this view is provided by Lahey (2014), who finds that the introduction of abortion restrictions in the 19th century increased birthrates

[8]The repeal states were Alaska (1970), California (1969), D.C. (1971), Hawaii (1970), New York (1970), and Washington (1970). Repeal took place in four of the states, Alaska, Hawaii, New York, and Washington, by action of the state legislatures. California reformed its abortion laws in 1967, but a court ruling in late 1969 had the practical effect of legalizing abortion. Abortions became available at several clinics in D.C. following a 1971 court decision regarding the interpretation of the District's pre-existing mother's health standard for the provision of abortion. Court rulings in 1972 in New Jersey and Vermont also invalidated anti-abortion laws in those states. Garrow (1998) and Myers (2015) provide evidence that these rulings had little practical effect. The New Jersey Attorney General threatened to (and did) prosecute any physician performing an abortion, and Vermont hospitals did not immediately change their abortion policies. I, like Joyce et al. (2010), do not view these two states as repeal states.

[9]Common international destinations to obtain abortions were Mexico, England, Japan, and Sweden (Joffe, 1995).

[10]Abortion surveillance was not conducted prior to 1969, when the CDC began collecting information on the incidence of legal abortion by state of occurrence. These data are incomplete, however, because of differences in state reporting requirements. The Guttmacher Institute began gathering more detailed and complete information in 1974 based on survey of abortion providers, but Guttmacher analysts caution against using their data to examine the effect of laws affecting young women because they are unable to reliably account for interstate travel (Dennis et al., 2009).

by 4 to 12 percent, and by Levine et al. (1999), who estimate that the repeal and invalidations of these laws in the seventies led to a 12 percent decline in teen birth rates, similar to fertility effects observed in Eastern Europe (Levine and Staiger, 2004) and Norway (Mølland, 2014). I will extend the analysis in Levine et al. to examine the effects of abortion policy on young women's family formation patterns measured by the probabilities of entering marriage and motherhood at a young age and also experiencing a "shotgun marriage." In addition to exploiting the variation in abortion repeal as in previous studies (see e.g., Levine et al., 1999; Donohue and Levitt, 2001; Goldin and Katz, 2002), I argue that there was substantial and plausibly random additional variation in access largely arising from variation in state laws granting pregnant minors the capacity to consent to medical care.

3 Coding the reproductive policy environment

Like previous authors, I rely on variation in reproductive policies across states and over time to estimate the effect of the policy environment governing access to reproductive control. The relevant policies have been described by Goldin and Katz (2002), Bailey (2006), Guldi (2008), and Hock (2008), but there is a great deal of variation in the legal coding of young women's access to the pill in this literature. The coding of access at age 19, for example, differs for 35 of 51 states and the District of Columbia among these four sets of authors. (See Appendix A.) Only Guldi (2008) and Hock (2008) address minors' ability to consent to abortion, and there are substantial differences between these two studies as well: the authors differ on the year in which unmarried women under age 18 gained confidential access to abortion for 19 states.

I have conducted a new and extensive review of the policy environment based on based on sources that include annotated statutes, judicial rulings, attorney general opinions, and advisory articles in state medical journals and law reviews, as well as newspaper accounts of legal changes, clinic openings, hospital policies, and enforcement actions.[11] I describe the results of this new review in detail in Myers (2015), which includes an overview of reproductive policy in the sixties and seventies, a state-by-state review of the relevant regulations, and a detailed explanation of the criteria used for the coding of dates. Table 1 summarizes the results of this effort, documenting the years in which changes in the regulatory environment in each state first affirmed the rights of adult and minor women to consent to contraception and abortion.

Appendix A provides a detailed comparison and reconciliation of my suggested coding with that of Goldin and Katz (2002), Bailey (2006), Guldi (2008), Hock (2008), and Bailey et al. (2011). In comparing the year in which teenagers are coded as first gaining confidential access to the pill (Table A-1), the dates presented here prove similar to those reported by Hock (2008), whose coding differs from that in this paper for 10 out of 51 states. In contrast, my coding differs from that used in Guldi (2008) for 13 states, Bailey (2006) for 19 states and from Goldin and Katz (2002) for 27 states. Moreover, the magnitudes of the discrepancies tend to be large. The average difference (measured in years) between my coding and that of other authors ranges from 4 to 7 years.

Many of the discrepancies in the coding of access to the pill appear to reflect objective errors rather than different interpretations of an ambiguous legal environment. For instance, Goldin and Katz (2002) treat young women as able to consent to the pill since 1968 in California, appearing to have misinterpreted a law that allowed legally emancipated minors to consent to medical care as also granting the same right to unemancipated minors. Bailey and Guldi released a working paper that revises the legal coding in their original papers, but do not present new estimates based on the revised coding (Bailey et al., 2011). Their newer legal coding of access to the pill corresponds more closely with my own,

[11] When possible, the information is checked against that in other reviews that cover contraception or abortion during some portion of this period. The most comprehensive of these are DHEW (1978), which provides profiles of state regulatory environments in the mid-seventies, and Merz et al. (1995), which provides a history of the enforcement of parental involvement laws for abortion through 1994.

but still differs for 14 states.

Of these remaining discrepancies between my coding and Bailey and Guldi's, the most frequent explanation (applicable to 8 of the 14 states) is a disagreement on how to interpret age of majority laws that established a lower age for women than for men. Bailey and Guldi assert that when the age of majority is different for men and women, the law does not govern the ability to consent to medical care, but that when the age of majority is the same for men and women, it is governing. As discussed in greater detail in Myers (2015), I have not been able to find evidence that supports this interpretation. My review of the statutory language, judicial rulings, and secondary sources interpreting differential age of majority laws indicate that the differential ages of majority in these statutes applied to a variety of rights, likely including that to consent to medical care (Myers, 2015). While this disagreement in coding is subjective and remains unresolved, the affected states tend to be small in population. I will demonstrate that the results are robust to adopting Bailey and Guldi's interpretation of the statutes.

Table A-2 of Appendix A compares the dates in which I have coded women under age eighteen gaining access to confidential legal abortion services with the coding in Guldi (2008) and Hock (2008). My coding differs from Guldi's for 14 states and from Hock's for 13 states, but there is very little overlap in these two sets. Where my coding differs from Guldi's, I consider only four states (Florida, Hawaii, New Jersey, and North Carolina) to reflect different interpretations of an ambiguous legal environment. The remaining ten states for which our coding differs appears to reflect errors in Guldi's coding; for eight of these cases, my coding independently agrees with that of Hock. In contrast, where my coding differs from that of Hock, 12 of the 13 differences could be viewed as the result of differing interpretations of ambiguous legal environments. The most common difference in interpretation arises in the case of six states (Colorado, Florida, Indiana, Kentucky, Missouri, Nebraska) in which judicial rulings in 1974 and 1975 invalidated parental involvement laws. Whereas Guldi and I treat such rulings as granting *de facto* access for minors unless there was some other restrictive statute not addressed by the ruling, Hock requires explicit recognition of this right by the legislature. In Myers (2015) I note that the language of many of the court rulings striking down parental consent requirements strongly suggests that the ruling confirms the right of minors to consent. Paul et al. (1976) also treat the rulings as granting access in their review of the then current regulatory environment.

These ambiguities in the classification of historical policies suggest an important question: How salient were these historical reproductive policies to "on-the-ground" provision of reproductive services? Anecdotal evidence from historical sources including state medical journals, local and state newspapers, and descriptions of prosecutions in judicial cases (Myers, 2015) suggests that reproductive policies were observable to interested providers and patients. For instance, one U.S. District Court ruling describes the attempts of the plaintiff, a young unmarried women, to purchase contraception from three drugstores in Madison, Wisconsin. She was turned away by employees at all three because Wisconsin's Comstock law prohibited the provision of contraception to unmarried persons (*Baird v. Lynch*, 1974). However, some policy changes, particularly those arising from judicial rulings, appear to have been widely reported but the subject of varying interpretations. For instance, the Ohio Supreme Court adopted a mature minor standard for medical treatment in a 1956 ruling, but an advisory article in the Ohio State Medical Journal recommended caution in applying this standard to minors under the age of 18 (Myers, 2015). In the case of Florida, press accounts suggest that after a 1975 judicial ruling stuck down a parental consent requirement for abortion, some Florida hospitals began providing abortions to minors without parental consent while others did not (Myers, 2015).

In the absence of reliable historical panel data on the provision and utilization of contraception and abortion, it is impossible to test the first-order policy effects on contraception use and abortion rates. Rather, as in the previous literature, I am able only to estimate the effect of the policy environment on second-order outcomes of fertility and marriage. I also explore the sensitivity of the results to varying interpretations of the legal environment, including using previous authors' codings.

4 Empirical Strategy

In this paper I adopt a quasi-experimental research design to estimate the effects of the introduction of the the pill, the legalization of abortion, and laws permitting young women to access each confidentially. State-level variation in the timing of the removal of Comstock laws and the legalization of abortion combined with distinctions made in states' laws between young women's ability to consent to each resulted in substantial variation across states and over time that I exploit using a series of standard differences-in-differences linear probability specifications:[12]

$$I(Age\ at\ first\ occurrence_{iys} < k) = \beta_0 + pilllegal_{ysk}\beta_{pl} + consentpill_{ysk}\beta_{pc} + abortlegal_{ysk}\beta_{al} + consentabort_{ysk}\beta_{ac} + R_i\boldsymbol{\beta}_X + P_{st}\boldsymbol{\beta}_p + \upsilon_s + \upsilon_y + \upsilon_{sxy} + \epsilon_{iysk}.$$

The dependent variable indicates that an event has taken place for woman i born in year y residing in state s prior to the ages of $k = \{16, 17, ..., 21\}$. Three outcomes are considered: first marriage, first birth, and a first marriage that is followed in less than 8 months by a first birth, which is termed a "shotgun marriage."[13] Hence, in the case of marriage, for instance, a series of models are used to estimate the probabilities of having married prior to ages 16, 17,

[12] Appendix F reports alternative marginal effects estimated with a probit specification. They are very similar to those presented in the paper.

[13] The estimates decrease in magnitude as the window defining "shotgun marriage" decreases. However, the conclusions presented in this paper are substantially the same regardless of whether a 6, 7 or 8 month window is used.

and so on.[14] On the right-hand-side, υ_s represents individual state fixed effects to control for time-invariant state characteristics, υ_y represents individual year-of-birth fixed effects to control for state-invariant temporal shocks, and υ_{sxy} is a set of state linear cohort trends that account for differences across states in fertility and marriage trends. The standard errors are corrected for correlation across individuals and over time in a given state by clustering at the state level as suggested by Bertrand et al. (2004).[15]

The variables of interest are *pill legal*, *consent pill*, *abort legal*, and *consent abort*. For a woman living in state s in year y, these measure the fraction of years between age 14 and age $k-1$ in which the pill was available to adult women, but minors could not consent (*pill legal*), the pill was available and minors could consent (*consent pill*), abortion was available but minors could not consent (*abort legal*), and abortion was available and minors could consent (*consent abort*). Hence, the model estimates the effects of exposure to three mutually exclusive and collectively exhaustive legal environments governing access to each form of reproductive control at a given age: one in which it is not legally available and two that distinguish between the ability of minors to consent to the legal service. In models where the outcome is giving birth, the lag between conception and observation of the outcome is accounted for by lagging the policy variables by a year.

Additional controls include R_i, a vector of dummies for the race and ethnicity of woman i, and P_{st}, a vector of variables measuring exposure to state sex and race discrimination laws and no-fault divorce laws.[16] The additional

[14] This estimation strategy can be thought of as a survival analysis approach that does not adopt parametric assumptions regarding the hazard function. The linear probability approach used here is flexible regarding the hazard function, allows for time-varying covariates, provides easily-interpretable marginal effects, and can be directly compared to the results of previous researchers who have estimated similar models for a single age k. The survival function is not restricted to be non-decreasing, although the estimated functions do satisfy this requirement. The results are similar if this restriction is imposed by limiting successive samples to women for whom the event had not occurred in previous periods.

[15] Standard errors calculated with a block bootstrap approach are similar to those reported here.

[16] Two variables measuring exposure to state anti-discrimination laws were created using dates of enactment provided in Neumark and Stock (2006). These measure years of exposure

policy controls also include exposure to state abortion reforms, which were enacted in 13 states prior to *Roe v. Wade* and permitted abortion under limited circumstances, including the preservation of the mother's life or health.[17]

I estimate these models using all years of the 1979-1995 CPS June Fertility supplements that contain information on age at first birth and marriage (in months). The sample is limited to cohorts of women born between 1935 and 1958 who are aged 22 and older at the time of observation. The oldest cohort was born in 1935 and was 25 years old when the pill was introduced and 38 when abortion was legalized nationwide. The youngest cohort was born in 1958 and was 2 years old when the pill was introduced and 15 when abortion was legalized nationwide. I choose to end with the 1958 cohort because this is the last cohort of women to reach majority before the 1976-1979 period during which the legal status of many state consent laws was unclear pending the resolutions of a series of U.S. Supreme Court cases beginning with *Planned Parenthood v. Danforth.* The restriction to women aged 22 and older ensures that the sample of women used for modeling outcomes prior to each age from 16 through 22 does not vary.

The common trends assumption

As always, difference-in-difference strategies rely on the common trends assumption, which in this case is that any differential change in outcome when states increase access to reproductive control is the result of that policy change. There are two reasons to question the common trends assumption with respect to pill policy. First, many of the laws granting young people access to the pill

to a state equal pay law (EPL) prior to the enactment of federal legislation in 1963 and years of exposure to a state Fair Employment Practices Act (FEPA) prohibiting racial discrimination in hiring, discharge and compensation. The effective dates of no-fault divorce laws were obtained from Vlosky and Monroe (2002). The no-fault divorce variable is years of exposure to a state policy permitting no-fault divorces.

[17]The reform states were Arkansas (1969), California (1967), C olorado (1967), Delaware (1969), Florida (1972), Georgia (1969), Kansas (1970), Maryland (1968), New Mexico (1969), North Carolina (1967), Oregon (1969), South Carolina (1970), and Virginia (1970). In addition, the District of Columbia had legalized abortion to preserve the life or health of the mother in 1901, and in 1944 the Massachusetts Supreme Court had interpreted that state's anti-abortion law to exempt abortions to preserve the woman's life or physical or mental health.

also granted them access to abortion. Previous authors (e.g., Goldin and Katz, 2002; Bailey, 2006) did not account for this, causing them to potentially conflate the powers of pill and abortion policy. Second, policies that specifically targeted minors' right to consent to family planning services may have been a product of increased demand for reproductive control. While I address the first issue by controlling for abortion policy, it is difficult to judge whether the second threatens the credibility of the estimated effects of pill policy in this and other papers. I demonstrate that the estimated null effects of the pill are robust to a variety of specifications of state-specific time trends as well as to falsification tests exploring pre-policy trends, but one may reasonably view difference-in-difference identification strategies based on variation in pill policy with skepticism. What is perhaps most convincing is that the results presented here based on policy variation within the United States are consistent with Leridon (2006) and Guinnane (2011), who describe the lack of systematic correlation between contraceptive technologies and demographic change across countries and over time.

The expansion of minors' access to abortion is a more credible "random shock" because much of the abortion policy variation for minors was an unintended consequence of pre-*Roe* laws permitting pregnant minors to consent to prenatal care. Consider the 45 non-repeal states where abortion became legal in January 1973 following *Roe v. Wade*. Nine of these states had previously passed "medical consent laws (MCL)" that permitted pregnant minors to consent to medical care related to their pregnancies. When abortion became legal, these laws also permitted pregnant minors to consent to abortion services, an effect that attorney general opinions and secondary sources from the period suggest was unanticipated and unintended (Myers, 2015). It is therefore plausible to consider minors in these 9 states as subject to a random "treatment' shock in which the legalization of abortion unexpectedly permitted them to consent to abortion services.

The effect of the policy change is readily apparent in Figure 5, which compares trends in outcomes for young women in these 9 states to those for women in the 25 states where no law permitted pregnant minors to consent to medical care, and where no policy change took place in the ensuing 2 years. The probabilities of becoming a mother and of marrying exhibited larger declines in the states where minors could consent to abortion at the time of *Roe*, and the magnitudes of these effects are very similar to those that I estimate in the paper. In the following years, these decreases began to reverse as some of the treated states passed laws closing the loopholes created by their policies regarding pregnant minors. And finally, when *Danforth* was decided 3 years later, permitting minors to consent to abortion (at least for a short time) in nearly all states, the probabilities fell sharply in the states where minors had not previously been granted the right to consent to abortion services.

Appendix B presents additional evidence that supports the "common trends" assumption in this context. The results that I will present are not sensitive to omitting the time trend or to specifying a quadratic, cubic or spline trend. Appendix B also presents estimates of the effect of the policy environment in the two years preceding the policy change. The results do not suggest that women were delaying family formation in advance of the reproductive policy changes.

5 Results

The probabilities of motherhood and marriage prior to age 19

Table 2 presents estimates of the effects of reproductive control on the probabilities of giving birth, marrying, or having a "shotgun marriage" prior to age 19. Model 1 includes controls only for pill policy, Model 2 add controls for abortion policy and Model 3 adds additional policy controls. For each specification, the top row reports adjusted predicted probabilities of each outcome setting the four reproductive policy variables of interest equal to zero.

For all three models and all three outcomes, the results do not provide evidence that pill policies had substantial effects. The estimated effects of pill policy are small, positive in the case of marriage and shotgun marriage, and lack statistical significance save for marginally significant estimates of modest effects of the removal of Comstock laws on age at first birth.[18] The results also provide no evidence that the ability to consent to the pill was relevant to these outcomes; the coefficients for *consentpill* and *pilllegal* are similar and magnitude and the small differences are not statistically significant.

In contrast, the estimated effects of abortion policies are substantial and robust. Consider, for instance, a woman who faced the same policy environment in all previous years of age from 14 through 17 (first birth outcome) or 18 (first marriage outcomes). Using the results from Model 3, a woman who lived in a state where abortion was legally available but where minors had not been granted confidential access ($abortion\ legal = 1$ and $consent\ abortion = 0$) is estimated to have been 3.2 percentage points less likely to give birth ($p < 0.001$), 2.3 percentage points less likely to marry ($p = 0.020$) and 1.9 percentage points less likely to have a "shotgun marriage" ($p = 0.004$) prior to age 19 than had abortion not been legal. If state law granted minors confidential access to abortion ($abortion\ legal = 0$ and $consent\ abortion = 1$), the estimated effects increase to a 5.7 percentage point decrease in the probability of giving birth ($p < 0.001$), a 4.8 percentage point reduction in the probability of marriage ($p = 0.001$) and a 4.0 percentage point reduction in the probability of a "shotgun marriage" ($p < 0.001$). Relative to the adjusted predictions, these are very large effects: liberalized abortion policy predicts a 34 percent decline in motherhood, a 20 percent decline in marriage, and a 63 percent decline in shotgun marriages prior to age 19.

As an additional means of gauging the magnitude of these estimates, I use

[18] This marginally significant result is not robust to alternative specifications of the time trend (Appendix B) or to choosing alternative age cut-offs for the outcome (Appendix C).

the regression coefficients to predict outcomes for the 1940 and 1958 cohort had abortion reforms and legalization not occurred. This exercise suggests that the liberalization of abortion policy explains about 80 percent of the decline in the probability of birth and 25 percent of the decline in the probability of marriage prior to age 19 observed between these birth cohorts.[19]

The estimated effects of 1960s abortion reforms that occurred prior to legalization are imprecise and lack statistical significance. The point estimates suggest a possible reduction in the probability of birth and shotgun marriage that is about one-third the magnitude of the effects of access to legal abortion under more broad circumstances.

Model 4 interacts minors' access to the pill with the abortion policy variables.[20] The coefficients on the interaction terms are imprecisely estimated and lack statistical significance for any of the examined outcomes. While this precludes reaching a conclusion regarding the degree to which the pill and abortion were complements or substitutes, the negative point estimates suggest that they are complements. One interpretation of this result is that women regarded abortion as back-up for the pill rather than as a substitute.

Turning to the remaining policy variables, the estimates do not suggest that the enactment of no-fault divorce laws or the establishment of state FEPAs prohibiting racial discrimination influenced the observed outcomes. Interestingly, the enactment of a state equal pay law is estimated to reduce the probability of motherhood by 1.4 percentage points ($p = 0.065$ in Model 3) and the probability of marriage by 2.5 percentage points ($p = 0.090$ in Model 3). If equal pay laws reduced discrimination against women in the labor market, then one

[19]These estimates are calculated by using the regression coefficients to predict the change in outcome based on the reproductive policy environments experienced by the relevant cohorts, and then dividing the result by the observed change in the outcome. Liberalized access to abortion, which encompasses reform, repeal and consent laws, is estimated to explain 0.794 (*s.e.* = 0.162) of the reduction in births and 0.252 (*s.e.* = 0.070) of the reduction in marriage between the 1940 and 1958 cohorts.

[20]Because all extant state Comstock laws were invalidated with *Griswold v. Connecticut* in 1965, four years before abortion became legal in the first repeal state, I do not interact *Pill legal* with the abortion policy variables.

might expect women to delay family formation in response. However, Neumark and Stock (2006) argue that these laws, which were enacted prior to the federal Equal Pay Act of 1963 and Title VII of the Civil Rights Act of 1964 and which did not include employment protections, effectively raised the relative price of female labor and reduced female employment, with no change in earnings conditional on remaining employed. It is somewhat perplexing that women may have delayed family formation in response, though perhaps this might reflect incorrect expectations regarding protection against discrimination in the labor market.

Cumulative probabilities, ages 16-22

Appendix C contains tables corresponding to Model 3 estimated over each cut-off from ages 16 to 22. These results are summarized by Figures 6-8, which graph the predicted cumulative probabilities of each outcome for different reproductive policy environments. The results do not provide evidence of a substantial effects of pill policies on fertility and marriage at any age between 16 and 22. Most of the coefficients are small in magnitude, small relative to the predicted probability of motherhood absent legal access to the pill or abortion, and small relative to the much larger estimated effects of access to abortion. The estimated effects of legalized abortion, in contrast, are large in magnitude and statistically significant across most ages.

The estimated effects of abortion policies on marriage prior to age k correspond to observably similar reductions in the probability of birth prior to age $k+1$, which suggests that increased access to abortion led to reductions in the probability of marriage at a young age primarily by allowing women who might once have married in response to an unplanned pregnancy to instead terminate that pregnancy. This conclusion is supported by the results from the models of "shotgun marriage," which are summarized by Figure 8. Access to legal abortion in a woman's state of residence is associated with large drops in the probability of shotgun marriage across the ages examined, and the absolute magnitudes of the declines are similar to the magnitudes of the reduction in the probability of marriage by the same year of age and the probability of having given birth by the end of the following year. The probability of having had a "shotgun marriage" prior to age 19, for instance, is estimated to decline by 4.0 percentage points ($p < 0.001$) if abortion is legally available and minors are able to consent to it. The corresponding declines in the overall probabilities of marriage by age 19 and birth by age 20 are 4.8 percentage points and 6.3 percentage points, respectively.

The results indicate that laws permitting minors to consent to abortion amplified the effects of legal abortion, suggesting that confidentiality mattered. This is consistent with Guldi (2008), who also finds that minors' ability to consent to abortion led to a reduction in birth rates. When estimating the effect of parental involvement laws enacted between 1985 and 1996, however, Levine (2003) finds a statistically insignificant 3 percent increase in the teen birth rate and concludes that parental consent requirements did not have a large effect on teen births. The laws in this later period may have been less binding than the legal environment in the seventies because judicial bypass was not available in any state in the early seventies while it was an option in all states enforcing parental involvement laws in the eighties and nineties.[21] Teens also may have been less willing to involve a parent in the seventies. A 1991 survey of teens seeking abortions in states without parental involvement laws found that 61 percent had told a parent about the abortion (Henshaw and Kost, 1992), whereas in a similar survey from 1979-1980, 55 percent had done so (Torres et al., 1980). In the 1979-1980 survey, respondents were also asked what they would have done if parental notification had been required by the clinic. Twenty-three percent reported that they would not have come to the clinic and, of these, 40 percent said that they would have had the baby (Torres et al., 1980).

[21] Little research has examined the frequency with which minors seek and are granted judicial bypass. Joyce et al. (2010) reports that 10 percent of all abortions among minors in Arkansas were obtained via judicial bypass in 2006 and 2007.

Though it is difficult to judge how well this self-report of hypothetical behavior might correspond to the actual response to parental consent requirements, or what the cumulative effect of parental involvement might be by the age of 19, it does suggest that it is plausible that confidentiality mattered to minors seeking abortion services in the seventies.

6 Alternative Legal Coding

Table 3 presents the results of a series of estimates of Model 3 based on different choices regarding the legal coding. The first column of the table repeats the estimates based on my preferred policy coding. The second column omits policy changes that were based on attorney general opinions or "mature minor doctrines." Mature minor doctrines, which were recognized both in judicial rulings and medical consent statutes, permitted a minor to consent to medical care if the physician judged her capable of understanding the nature and consequences of treatment. Attorney general opinions may not carry the legal weight of a statute or judicial ruling. If physicians did not confer access under these types of policy changes, then the estimated effects might be larger when they are excluded. The results, presented in Column 2, are similar for pill policy and slightly larger in magnitude for abortion policy.

The remaining columns adopt the subjective judgements made by previous researchers. The "Add FP policies" column follows Bailey (2006) in treating laws permitting state-funded family planning clinics to provide confidential services to minors as granting broad access. If these laws were conferring access to large numbers of women and if access to the pill affected fertility and marriage, the estimated effects of the pill might be larger in magnitude when they are included. However, the estimates change very little. The next column follows Hock (2008) in treating judicial rulings striking down parental consent provisions as not granting confidential access. If confidential access mattered and if these rulings were in fact not conferring it, then the estimates would increase in magnitude. In fact, they are slightly smaller, though the estimated effects of confidential access are still statistically significant. The final column uses the legal coding in Bailey et al. (2011) which, as described previously, differs primarily from my own in that they treat age-of-majority laws as not governing access in states where the age of majority was higher for men than for women. This disagreement in legal interpretation proves unimportant, consistent with the argument in this paper that policies governing young women's access to the pill did not have large effects on family formation.

7 Alternative Sub-Samples of the Data

Estimates of average effects for the population as a whole could mask substantial variation across states and socioeconomic and demographic groups. Table 4 provides additional results from estimating the empirical specification in Equation 1 (Model 3) for various sub-samples of the data.

Estimates by race

The separate estimates for non-Hispanic whites and blacks in Columns 2 and 3 of Table 4 suggest that the policy environment governing reproductive control had different effects for white and black women. For whites, the estimates suggest that the removal of Comstock laws limiting distribution of the pill may have had led women to delay motherhood. Women living in states where the pill was legally available were 1.3 percentage points less likely to have become mothers before age 19 ($p = 0.002$). However, the laws permitting women to consent to the pill, the focus of the "power of the pill" literature, have no additional effect. The difference between the coefficients on *consent pill* and *pilllegal* is small and not statistically significant (difference= 0.003, $p = 0.734$). For blacks, there is no evidence that the liberalized access to the pill decreased the probability of entering into motherhood or marriage prior to 19; in fact, though they are not statistically significant, the point estimates are positive.

This is potentially explained by the correlation between race and socioeconomic status. Akerlof et al. (1996) suggested that the the pill may have created a "technology shock" effect in which its existence generated pressure to engage in sex regardless of whether the technology actually was adopted, leading to an increase in pregnancies among young unmarried women. This could have been particularly true for low-income teens who likely had more limited access to prescription contraception.

The estimated effects of the legalization of abortion are larger in magnitude, statistically significant, and negative for both whites and blacks. White women who had access to abortion with parental consent are estimated to have been 2.3 percentage points less likely to give birth prior to age 19 ($p = 0.009$), while black women are estimated to have been 9.1 percentage points less likely to do so ($p = 0.001$). If the legal environment permitted them to obtain an abortion without parental consent, the magnitudes of the reductions increase to 4.6 percentage points for whites ($p < 0.001$)and 12.3 percentage points for blacks ($p = 0.001$). Although the absolute effects of abortion policy on the probability of birth are greater for blacks, the effects relative to adjusted predicted outcomes are similar. Relative to the predicted probability of birth in an environment in which neither the pill nor abortion was legally available, the estimated effects suggest that confidential access to legal abortion led to a 33 percent decline in the probability of birth for whites and a 36 percent decline in the probability of birth for blacks.

Estimates by educational status

The fourth and fifth columns separates the sample by college attainment.[22] This allows for a comparison of results with those of Goldin and Katz (2002), who are interested in the effects of the pill on college-educated women. There

[22]The CPS measured educational attainment by years of education completed until 1992, when it began measuring education by highest degree completed. I create comparable categories for the two measures using the scheme suggested by Jaeger (1997). Women in the pre-1992 sample are coded as completing college if they have completed 16 or more years of education.

is little evidence that legal access to the pill led to delayed family formation for women in either educational category. The estimates suggest strong negative effects of abortion policy for women with less than a college education and smaller effects for women with a college education. Educational attainment itself is a potential outcome, and changing compositions of these two groups may contribute to the observed effects.

Estimates omitting reform states

The estimated effects of the legalization of abortion rely on six "repeal states" that legalized first-trimester abortions under most circumstances prior to 1973. This approach groups the thirteen "reform states" that permitted abortion under limited circumstances prior to *Roe* together with the remaining non-reform states that prohibited abortion for any reason except to save the life of the mother.[23]

In fact, there was substantial variation in the number of legal abortions performed in reform states, and in some the abortion ratio (abortions per 1,000 live births) rivalled or exceeded those in repeal states which had legalized abortion under most circumstances. In 1971 the reported legal abortion ratio for four reform states was 18.1 in Arkansas and 16.6 in Georgia, but 277.1 in Kansas and 144.8 in Maryland. For comparison, the abortion ratio in two repeal states was 212.2 in Washington state and 344.36 in California (Smith and Bourne, 1973). Anecdotal and empirical evidence suggests that the higher abortion ratios in some reform states may reflect a greater willingness or ability among providers to use the mental health standard. In Maryland, mental health was the indication for 96 percent of legal abortions performed in the first six months of 1971 (Melton et al., 1972).[24] If it is indeed the case that abortions were much more

[23]The reform states are Arkansas (1969), California (1967), Colorado (1967), Delaware (1969), Florida (1972), Georgia (1969), Kansas (1970), Maryland (1968), New Mexico (1969), North Carolina (1967), Oregon (1969), South Carolina (1970), and Virginia (1970).

[24]See Myers (2015) for more evidence of substantial inter-state and inter-hospital variation in the provision of abortions under the mental health standard. Joffe (1995) also includes providers' first-hand accounts of variation in the application of mental health standards be-

widely available in some reform states than in non-reform states, then omitting reform states from the sample of control states should increase the magnitude of the estimated effects of the legalization of abortion. The penultimate column of Table 4 presents these results, which are consistent with this prediction. When reform states are omitted from the sample, the estimated effect of the legalization of abortion on the probability of giving birth prior to age 19 increases in magnitude from a 3.2 percentage point reduction ($p < 0.001$) to a 4.3 percentage point reduction ($p = 0.004$) if minors could not consent, and from a 5.7 percentage point reduction ($p < 0.001$) to a 6.7 percentage point reduction ($p < 0.001$) if minors could consent.

The possible role of interstate travel to access legal abortion

Another reason to expect estimates of the effects of the legalization of abortion that rely on the repeal/non-repeal state dichotomy to be conservative is that they fail to account for interstate travel to obtain abortions in repeal states from 1970 to 1972. Joyce et al. (2010) compile evidence from CDC abortion surveillance to show that 42 percent of legal abortions performed in 1971 were provided to out-of-state residents, with most occurring in California, New York, and Washington D.C..[25] Because many women already were traveling to obtain abortions in these repeal states, the legalization of abortion in their home states likely had less effect. Joyce et al. (2010) argue that interstate travel to repeal states may also have confounded estimates of the effects of confidential access to the pill. States that granted early legal access to the pill in the 1970-1972 period tended to be close to New York, and so previous authors may have been picking up access to abortion in that state rather than the effect of increased access to the pill. Joyce et al. (2010) demonstrate that once they control for distance to a legal abortion provider, estimates of the average effect of confidential access to the pill on birth rates decline. It is possible, however, that this masks a

tween hospitals and states.

[25] Their geographic isolation made Alaska and Hawaii less attractive destinations, while Washington state included a residency requirement in its repeal law.

larger effect of confidential access to the pill within the subset of states that were distant from legal abortion providers.

In the final column of Table 4, I restrict the sample to repeal states and states that are more than 500 miles from California, New York, and Washington D.C. as measured by the distance between the states' population centers (U.S. Census Bureau, 2010). The table refers to this group of states as "limited interstate travel states" because prior to the legalization of abortion, travel to another state to obtain a legal abortion was impossible (repeal states) or relatively costly (states that are distant from repeal states).[26] The coefficient on the measures of legal access to abortion in all of the models increase in magnitude when one restricts the sample in this way. Interestingly, the estimated effect of confidential access to the pill in reducing the probability of birth is now larger in magnitude and marginally statistically significant, albeit one-fourth the size of the estimated effect of confidential access to abortion. Taken together, these results suggest that travel to nearby states to access abortion tended to reduce the salience of own-state policies related to access to both the pill and abortion. The diffusion of greater access to the pill and abortion to young women in the early 1970s may have had much greater effects on fertility if women had not already been traveling across state lines to access abortion.

8 Comparison with previous findings

The "power of the pill literature" is primarily based on two seminal papers, Goldin and Katz (2002) arguing the diffusion of the pill to young unmarried women led college-educated women to delay marriage and pursue advanced degrees, and Bailey (2006) arguing that the pill led to delayed fertility and

[26]The states in this sample are Alabama, Alaska, Arkansas, California, Colorado, the District of Columbia, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Louisiana, Minnesota, Mississippi, Missouri, Montana, Nebraska, New Mexico, New York, North Dakota, Oklahoma, Oregon, South Dakota, Tennessee, Texas, Utah, Washington, Wisconsin, and Wyoming. The 500 mile cutoff is admittedly arbitrary, but the findings are substantially the same using a 750 or 1,000 mile cut-off.

increased labor supply among young women. These papers have been widely cited in the literature,[27] and the authors' codings of the policy environment has been used by subsequent researchers including Zuppann (2012) using Goldin and Katz's coding, and Steingrimsdottir (2010) and Edlund and Machado (2015) using Bailey's coding. I have examined similar outcomes as Goldin and Katz (2002) and Bailey (2006)– the probabilities of marrying and giving birth prior to selected years of age– and now attempt to reconcile the differences in our findings.[28]

Replication of Goldin and Katz (2002)

When estimating the power of the pill in reducing the probability of marriage, Goldin and Katz (2002) focus on college-educated women while I have primarily focused on the effect for the population of women as a whole. In Table 5 I present the results of models for the same outcome as that in Goldin and Katz: the probability of marriage prior to age 23 for women who are aged 23 and older and have completed college at the time of observation. In the first panel I use Goldin and Katz's preferred sample and dataset: College educated women aged 23 and older observed in the 1980 Census 1 percent file (Ruggles et al., 2015).[29] In Column 1 of Panel 1, I use their data, specification and legal coding to replicate the result from Column 1 of Table 4 in their paper showing

[27]A Google Scholar search performed on the two papers on April 4, 2016 indicates that Goldin and Katz (2002) has been cited 848 times and Bailey (2006) has been cited 335 times. REPEC statistics obtained on the same date indicate that Goldin and Katz (2002) is in the top 1 percent of papers by number of citations weighted by simple impact factor.

[28]Guldi (2008) and Ananat and Hungerman (2008) also examine fertility effects, but do so using birth rates instead of the cumulative probability of having become a mother, so the results are not directly comparable to mine. Ananat and Hungerman (2008) do not control for confidential access to abortion, and estimate that births to women aged 14 to 20 declined by 21 percent in states where minors could consent to the pill. This is a very large effect that is nearly twice as large as the estimated effect of the legalization of abortion on teen birth rates in Levine (2003). Unlike Ananat et al., Guldi (2008) controls for confidential access to abortion as well as to the pill. Her findings indicate that confidential access to the pill led to an 8 percent decline in fertility for white women aged 15 to 21, though the effect disappears if she uses monthly instead of annual natality data. Joyce et al. (2010) demonstrate that when they stratify Guldi's sample into young (15-18) and older (19-21) age groups, so that identification is achieved by comparing the effects of access for more similarly-aged women, the estimated effects of confidential access to the pill also disappear.

[29]I thank Lawrence Katz and Claudia Goldin for graciously supplying me with their data and program files.

that college-educated women were 2.0 percentage points less likely to be married before age 23 ($p = 0.078$) if they were born in a state that, by the time they were 18 years old, had granted minors under age 18 capacity to consent to the pill. In the second column, I re-estimate the model using my revised legal coding, and the estimated effect of the pill disappears, becoming small and statistically insignificant ($coef = -0.0001$, $p = 0.994$). When I add the detailed legal controls used in my models, the results, presented in the third column of the first panel, again indicate a very large effect of access to abortion but little effect of the pill.

In the second panel of the table, I estimate the same three specifications presented in the first panel, but using the 1979-1995 CPS Fertility Supplement instead of 1980 Census IPUMs data. Interestingly, even using Goldin and Katz's specification and coding of the dates of legal changes (Column 4), the estimated effect of access to the pill is much smaller and statistically insignificant using the 1979-1995 CPS Fertility Supplements than the 1980 Census. This may because the wider time frame of the CPS offers a more balanced sample of birth cohorts. In Goldin and Katz' data, respondents are observed in 1980 only when the youngest cohort is only 23 years old. It may be that Goldin and Katz are picking up some effect of the pill in delaying marriage for women who not only completed college, but who did so "on time." The effect of reproductive control may have been relatively greater for this group.[30] In the final two columns of Table 5, I use my legal coding and specification and continue to fail to find evidence that access to the pill delayed marriage for college graduates.

Appendix D expands this exercise to all of Goldin and Katz's specifications of the probability of marriage, exploring the sensitivity of their estimates to using CPS Fertility Supplement data instead of Census data and/or to using my legal coding. These results all are consistent with those presented in Table 5

[30] In unreported models in which I restrict the CPS sample to 1980 only, I obtain a point estimates that are slightly larger in magnitude than those reported in the second panel of Table 5, though the sample size is small and the estimates imprecise.

and described above. The estimated effects of confidential access to the pill in delaying marriage for college graduates are not statistically significant and tend to be much smaller in magnitude when I use the CPS Fertility Supplement data. They also are sensitive to the choice of legal coding: using my legal coding, which corrects several objective errors, there is little evidence that access to the pill led to delayed marriage for college graduates regardless of which data are used.[31] In Appendix D I also discuss why Goldin and Katz' estimates of the effects of legalized abortion were attenuated; this is largely due to an error in their program files that led to the confusion of Alaska (a repeal state) with Alabama (not a repeal state).

Replication of Bailey (2006; 2009) and Bailey et al. (2013)

Bailey (2006) presented estimates that women who could consent to the pill before age 21 were 7 percentage points less likely to give birth before age 22. Bailey (2009) later released an addendum to this paper acknowledging errors in formatting the data and revising estimates of the effect of the pill on the probability of giving birth at a young age downward to at most a one to two percentage point decline. In subsequent work arguing for the salience of pill policy, Bailey et al. (2013) reproduce the estimates from the 2009 addendum, but mislabel the y-axis on the summary graph, with the effect that the estimates appear larger in magnitude. Bailey published a graph with a corrected axis on her web page in December 2015.

In Appendix E, I present the results of attempts to replicate both sets of corrected results, using data and program files provided by Martha Bailey. I describe and document the differences in our samples, and present a series of replication exercises exploring the sensitivity of the results to correcting errors in data cleaning, making alternative sample selection choices, using alternative legal coding, adding controls for the legalization of abortion, and using my

[31] Hock (2008) remarks in a footnote that he also does not find evidence of delayed marriage for college graduates using the 1968-1979 CPS Fertility Supplements.

preferred econometric specification. With the exception of one table, the specifications in Appendix E also all use indicator variables to measure access, as in the authors' original work.

Table 6 provides an illustrative example of this exercise. In the first panel, I estimate the the effect of pill policy on the probability of giving birth prior to age 19 using Bailey's sample criteria: women aged 36 and older born between 1935 and 1959 who had been married, given birth and did not have allocated values for age at first birth. In the first column, I use Bailey's preferred legal coding and obtain a similar estimate of the effect of confidential access to the pill: a 1.5 percentage point decline in the probability of birth ($p = 0.027$).[32] If I continue to use Bailey's sample and specification but my own legal coding (Column 2), the point estimate is reduced by two-thirds and is not statistically significant. In the final column, in which I use Bailey's sample and my legal coding and specification, the estimates continue to provide little evidence of a pill effect.

The sample selection criterion that explains most of the difference between my and Bailey's preferred samples is the age of the respondent: I include all women age 22 and older, whereas Bailey includes only women age 36 and older (who also have married and given birth). The result of the higher age threshold is that Bailey's sample is heavily unbalanced across cohorts and only includes 4,778 women born after 1954, the group that was subject to the most dramatic changes in the policy environment. My preferred sample includes 74,711 women in these cohorts.[33] In the second panel of Table 6, I re-estimate the models from the first panel using women aged 22 and older. These results do not provide evidence of a substantial pill-policy effect regardless of whose legal coding is

[32]This result corresponds to Table IV, Model 1, Age 19 in Bailey (2009). It is very similar to the published estimate, but not identical for two reasons. First, I have corrected minor errors the authors made when preparing the data for analysis. Second, even with the errors intact, the replication materials provided to me do not produce the published results. These differences also are minor.

[33]Appendix Table E-2 presents the cohort distributions by sample.

used.[34]

In summary, the estimated effects of pill policy on marriage (Goldin and Katz, 2002) and fertility (Bailey, 2006, 2009; Bailey et al., 2013) are substantially weakened when using any of a series of alternative research design choices.

9 Was pill policy relevant in the pre-Roe era?

In October 2016, after this paper was accepted for publication, Martha Bailey wrote a letter to editor James Heckman arguing that the results and my interpretation obfuscate evidence on the power of the pill that is consistent with the conclusions in her work. I have added this additional section to the paper to present Bailey's case and my response.

Bailey points out that her earlier work observes the most substantial policy effects for women born prior to 1950 who came of age after the invention of the pill and before the legalization of abortion (Bailey, 2009; Bailey et al., 2013). In Appendix E, I replicate these results, demonstrate that they are sensitive to reasonable alternative specification, and also point out that they suggest policy effects in advance of the actual policy change. (More on this latter point below.) In her letter, Bailey suggests a simple additional test: Estimate the fertility results in Table 2 of this paper after limiting the sample to the 1935 to 1949 birth cohorts. I present the results in Panel A of Table 7.[35] The results (Column 1) do not provide evidence that the removal of Comstock laws had a substantial effect, but do suggest that the ability to consent to the pill prior to age 18 led to a 3.0 percentage point reduction ($p = 0.025$) in the probability of motherhood before age 19. Bailey argues that this should be treated as evidence that pill policy was relevant. I argue that this research design lacks both internal and external validity.

[34]Appendix E reports results in which each of Bailey's sample restrictions are relaxed in turn. The restriction to women observed after age 35 explains most of the differences between our estimates.

[35]The variables *Abort legal* and *Consent abortion* are not included because none of the women in these birth cohorts were exposed to changes in these policies.

With respect to internal validity, the estimated effect of confidential access is based on policy variation in only two states: Mississippi and Ohio. No other states granted women in these birth cohorts the legal right to consent to the pill prior to age 18.[36] In both states, the policy change occurred in 1965 at the height of the Civil Rights Movement that likely was changing both the costs and payoffs of human capital investments for young black women. To explore whether the effects of pill policy changes in these states might be conflated with those of the Civil Rights movement, Table 7 includes results estimated separately by race (Columns 2 and 3) and alternately allowing Mississippi and Ohio to individually identify the effect (Panels B and C). The aggregate effects are driven in large part by fertility declines among young black women, particularly in Mississippi where the coefficient on *consent pill* might suggest that confidential access caused a 24.3 percentage point reduction ($p < 0.001$) in the probability of motherhood while *increasing* the corresponding probability for white teens by 3.1 percentage points ($p = 0.011$). I conduct a test granting Alabama– a neighboring state also rocked by the Civil Rights movement– a placebo pill policy (Panel D). These results are similar to those based on policy variation in Mississippi despite the fact that no change in pill policy actually occurred.

Setting aside the question of internal validity, in no other states than Mississippi and Ohio did minors under age 18 gain the right to consent to the pill between 1960 and 1967. If one wishes to explain what happened in the 1960s, the answer cannot be the diffusion of confidential pill access to younger teens. The policies that *did* potentially affect these cohorts were the repeal and/or invalidation of Comstock laws between 1960 and 1965 and variation in age-of-majority statutes affecting women aged 18 to 20.[37] The specifications in Table 7

[36]This assertion is true regardless of whether one uses the legal coding in Myers (2015) or Bailey et al. (2011). In both states, the relevant change arose because mature minor doctrines permitted minors to consent to medical care after *Griswold v. Connecticut* invalidated these states' Comstock laws.

[37]Using the legal coding in Myers (2015), by 1968 eighteen-year-old women could consent to the pill in 10 states, in all cases because of age-of-majority laws. If one uses the legal coding in Bailey et al. (2011) instead, then 18 year-olds gained confidential access in only five states:

do include controls for Comstock laws (*pilllegal*), and the results do not provide evidence that their removal caused a substantial reduction in births prior to age 19. Perhaps, though, pill policies in the sixties had an effect at slightly older ages. Table 8 expands Bailey's suggested test, presenting the results of models that estimate the effects of these policies on births to prior to ages 20 through 22, again using only the 1935-1949 birth cohorts. The results are highly imprecise, but if one interprets non-statistically significant point estimates, they suggest that there was little effect of the removal of Comstock laws on the probability of motherhood prior to ages 20 or 21, and that confidential access to the pill *increased* the probability of motherhood . The imprecise estimates in the final model suggest that the removal of Comstock laws decreased the probability of motherhood prior to age 22 by 1.7 percentage points ($p = 0.483$), but that there was no additional effect of confidential access (difference$= 0.003$, $p = 0.887$).

Bailey also asks that I address a second point made in Bailey (2009); Bailey et al. (2013): Pill policies could have dynamic effects. If one sees no change in the probability of motherhood at age 20, for instance, this could be because women who delayed motherhood at younger ages gave birth by age 20. While this is a theoretical possibility, the empirical evidence in Figures 6-8 and Appendix C does not provide evidence of substantial pill policy effects at any age, suggesting neither static nor dynamic effects. Focusing on the pre-*Roe* cohorts, the results in Table 8 also do not suggest dynamic effects of the sort proposed by Bailey. If anything, the point estimates suggest that confidential access at ages 18 and 19 advanced motherhood, an observation that also holds if Mississippi and Ohio are excluded (Column 4) so that births prior to age 20 is the first point at which one could expect to observe a policy effect.

I further address the question of dynamic effects in Appendix E. In addition to being sensitive to reasonable alternative specifications, the results in Bailey

Kentucky and Ohio in 1965, Mississippi in 1966, and at publicly-funded clinics in Georgia and Washington in 1968. This raises questions of external validity: pill policy cannot explain wide-scale demographic change if pill policy itself was not widely varying.

(2009) and Bailey et al. (2013) suggest effects before the actual policy change occurred. Across multiple models– including those that eliminate Mississippi and Ohio from the sample (Table E-7) and use indicators to distinguish between policy changes occurring at age 18, 19 or 20 (Table E-8)– confidential access to the pill between ages 18 and 20 is associated with declining motherhood at ages 18 and younger. These coefficients are substantially attenuated if one include state linear time trends (Table E-10). This suggests contaminating effects rather than dynamic ones, an issue that I discuss in greater detail in the appendix.

As a whole, the evidence to support the belief that pill policies influenced family formation is at best weak. In this paper and the associated appendices, I have presented the results of nearly 800 specifications that explore the sensitivity of the results to using alternative legal coding, samples, data sets, and choices of whether to model reproductive policies using exposure or indicator variables. The estimated effects of pill policies on young women's family formation tend to be small, imprecise, and of varying signs. Perhaps these policies had small or heterogeneous effects, but the results do not clear the empirical bar for concluding that legal access to the pill was a game-changer in delaying motherhood and marriage.

This need not implies that pill policies were impotent. Older married women likely had different costs and benefits of abstinence, consistent with findings in Bailey (2010, 2013) that legal access to the pill allowed married women to avoid or delay second and higher-order births. Public funding of family planning clinics may also have reduced childbearing among poor women even if it had little effect on overall fertility (Bailey, 2012, 2013). However, these possible effects are outside of the scope of this paper and not directly relevant to large aggregate changes in family formation patterns.

10 Conclusion

Goldin and Katz (2002) claim the case of Japan–where fertility and marriage declined precipitously with the legalization of abortion in 1948 but where the pill was not widely available until 1999– illustrates that "the pill is not necessary for demographic change." But, they continue, "a virtually foolproof, easy-to-use, and female-controlled contraceptive...does appear to have been important in promoting real change in the economic status of women."

This argument for the power of the pill overstates its efficacy while failing to provide an alternative, non-demographic, mechanism via which pill access at a young age might generate economic change. One can imagine possible scenarios. Even if legal access to the pill did not cause young women to delay marriage and motherhood, they may still have made human capital investments based on incorrect beliefs about the efficacy of the pill, unaware of the wide gap between clinical and actual failure rates. Or, perhaps they made these choices based on the *existence* of pill technology, correctly anticipating that regardless of policies governing their access for a short period of time, they would be able to obtain the pill as married adults. In this scenario, pill technology changed long-term expectations about motherhood, but policies governing young women's access did not.

Other factors may also have precipitated the epochal social change for young people in the 1960s. The experiences of their grandmothers, who experienced first-wave feminism, or of their mothers, whose labor force participation had increased dramatically between 1940 and 1960 (Goldin, 1990, 2006), may have contributed to the transmission of new norms and expectations regarding women in the workforce. Federal civil rights legislation, including the 1963 Equal Pay Act and the 1964 Civil Rights Act, may have increased relative wages and the returns to human capital investments, prompting women to delay family formation.

Whatever the catalyst, the liberalization of abortion policy played a key role in the continuation of the reaction into the early 1970s. In states where abortion was legalized and young women could obtain an abortion without involving a parent, the likelihood of becoming a mother before age 19 declined by a third and the likelihood of a shotgun marriage by more than one-half. If it is via these two mechanisms that young women's reproductive control in turn influences other economic outcomes,then social scientists and policymakers may wish to devote greater attention to the "powers" of abortion policy.

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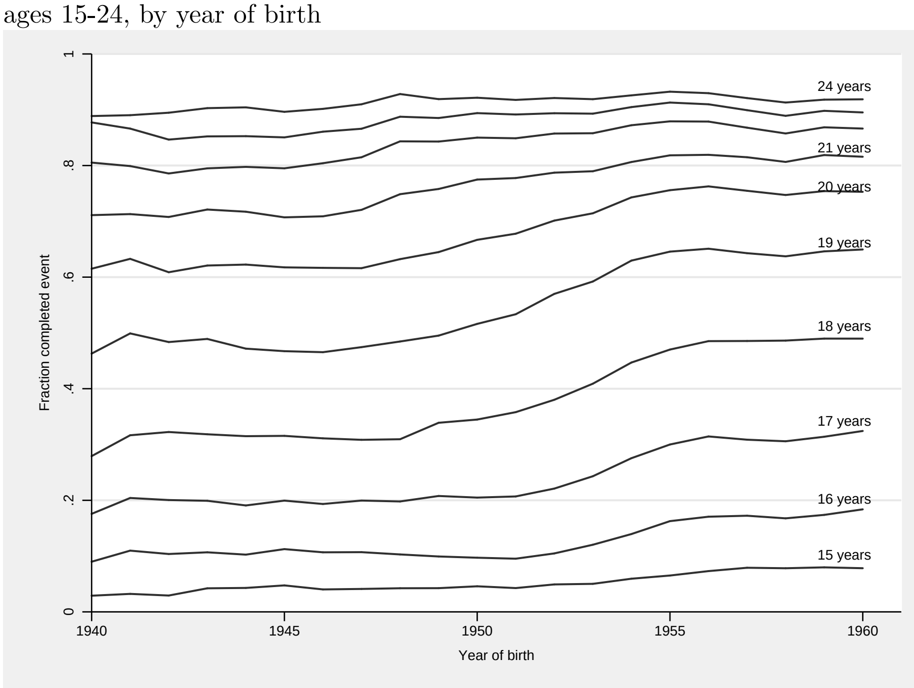
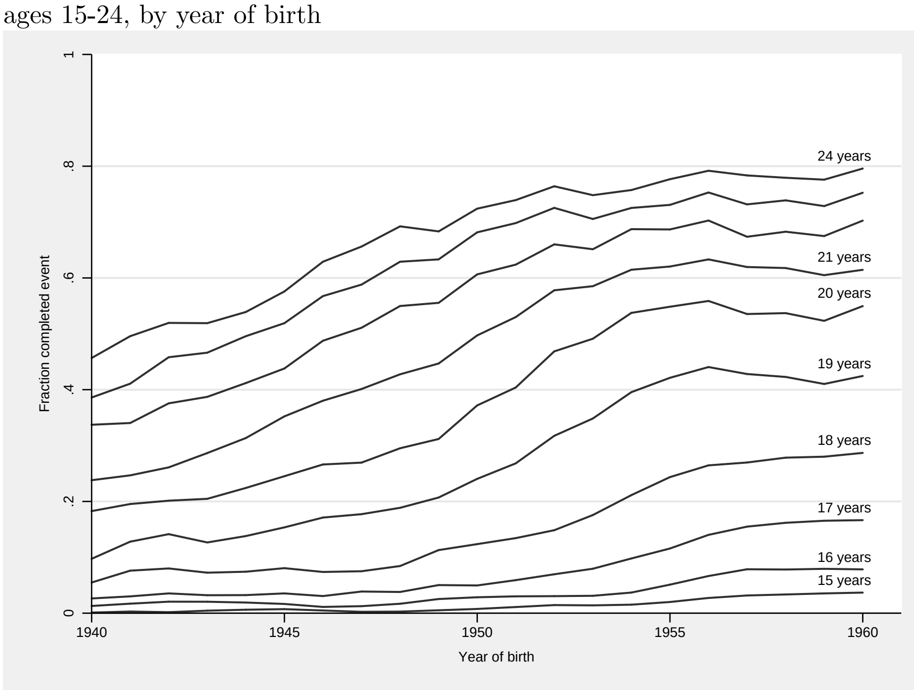
Figure 1: Fraction of women who had engaged in sexual intercourse prior to ages 15-24, by year of birth



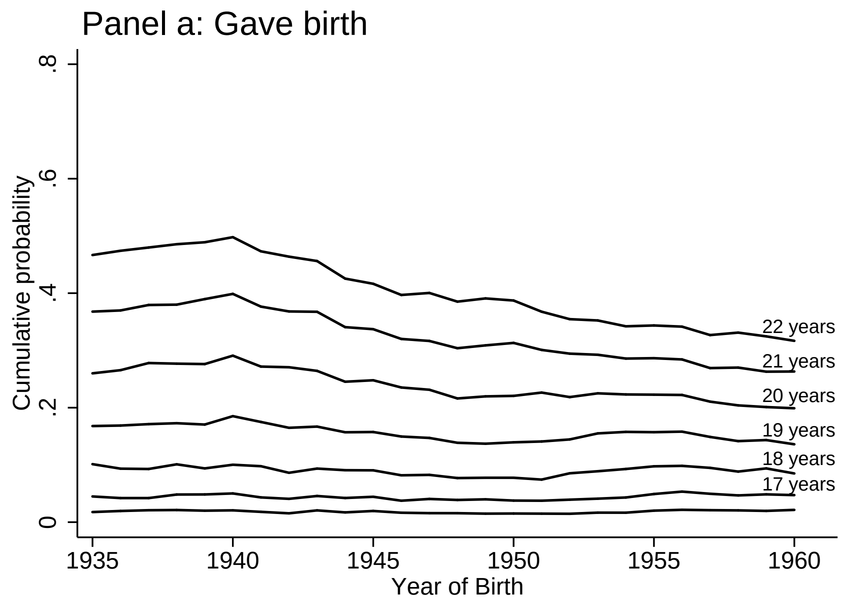
The cumulative probability of having engaged in sexual intercourse is calculated using the National Survey of Family Growth, Cycle 3 (1982), Cycle 4 (1988), and Cycle 5 (1995). Sample: Women born between 1940 and 1960, who were aged 24+ at the time of observation (n=14,959). Because of the small samples sizes, the reported fractions are smoothed using three-year moving averages. Sampling weights are applied.

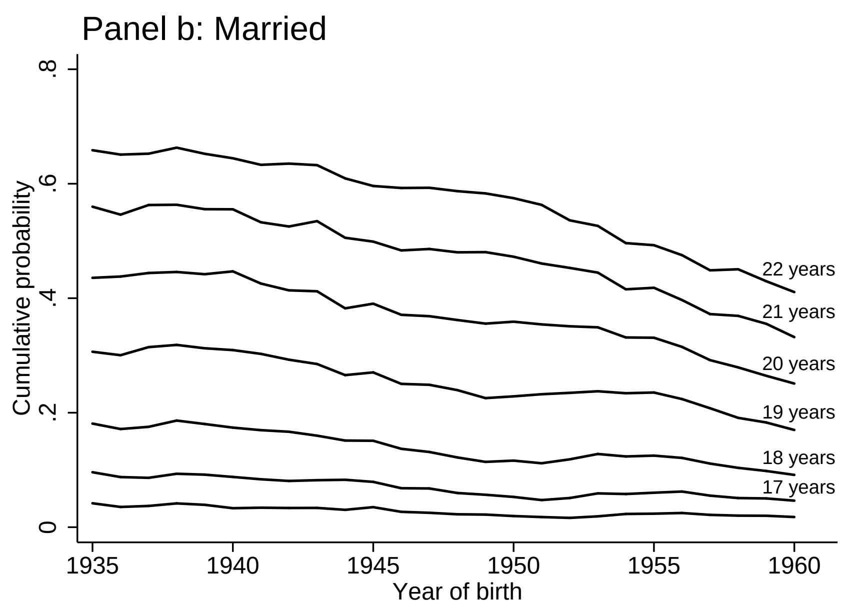
Figure 2: Fraction of women who had received family planning services prior to ages 15-24, by year of birth



Three-year moving averages of the the fraction of women who recall having received family planning services prior to the indicated age. Source: Author's calculations using National Survey of Family Growth, Cycle 3 (1982) and Cycle 4 (1988). This question was not asked in Cycle 5. Sample: Women born between 1939 and 1961 who were aged 24 and over at time of observation (n=10,195).

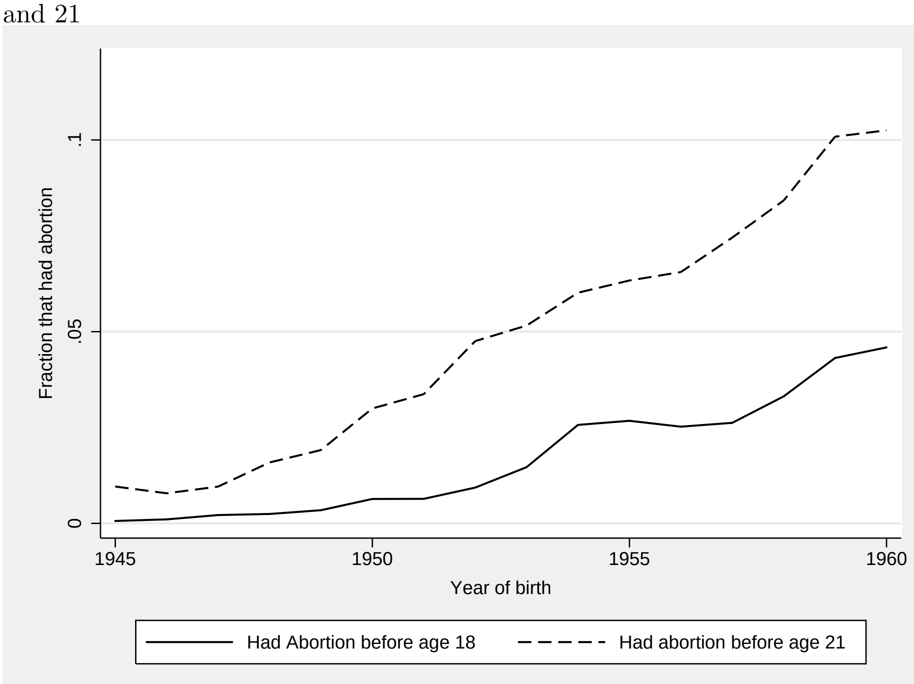
Figure 3: Fraction of women who had married and given birth prior to ages 15-22, by year of birth





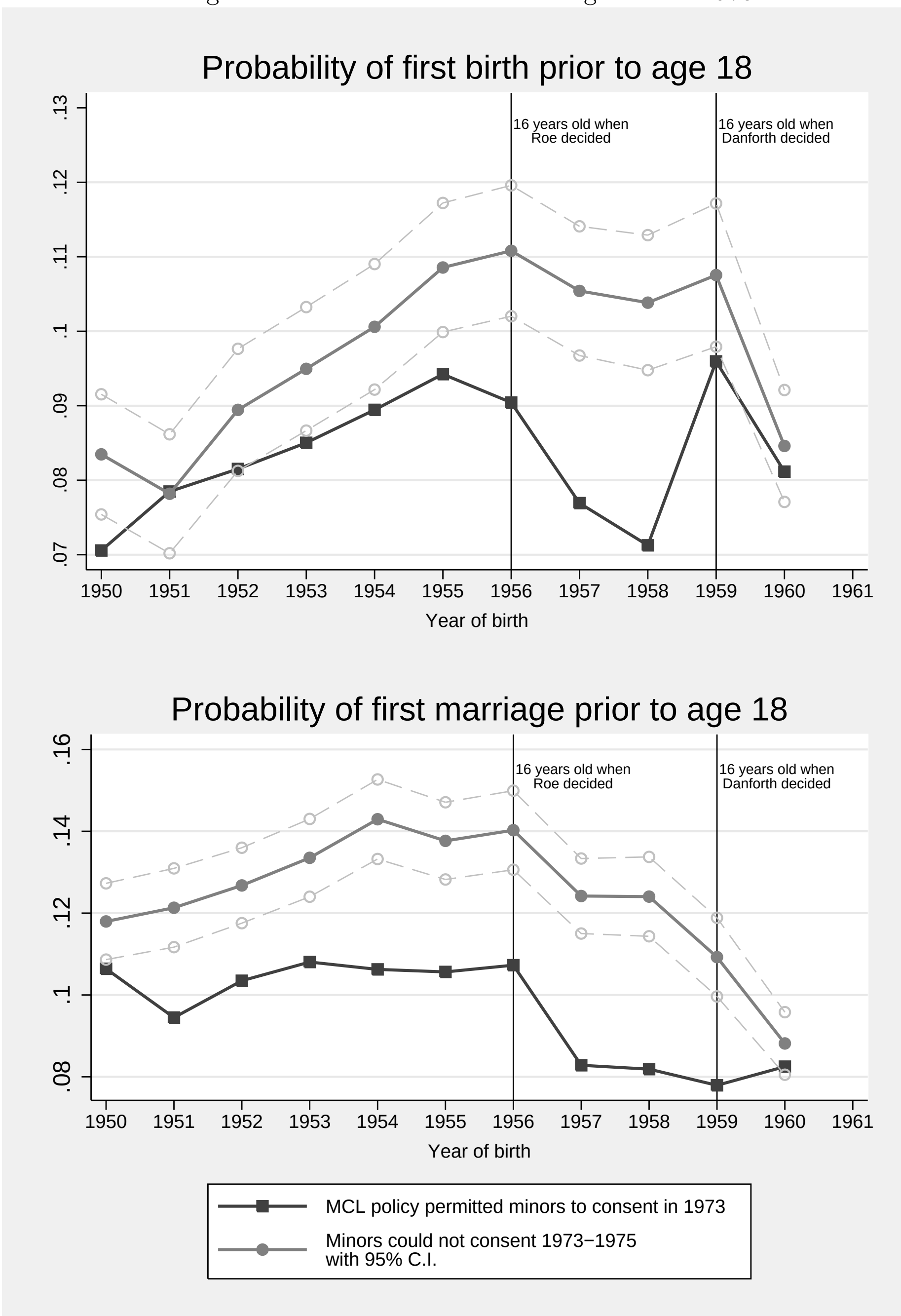
Source: Author's calculations using 1979-1995 CPS Fertility Supplements with information on age at both events. Sample: Women born between 1935 and 1960, who were aged 22+ at the time of observation (n=303, 274). Sampling weights are applied.

Figure 4: Fraction of women who report they had an abortion prior to ages 18 and 21



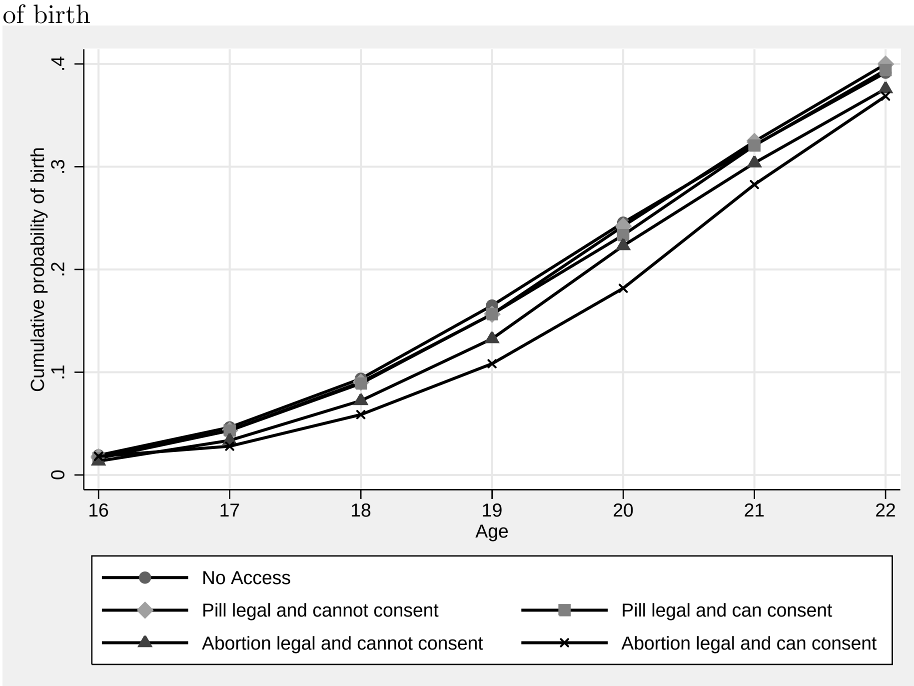
Three-year moving averages of the fraction of women who reported having had an abortion prior to ages 18 and 21. Source: National Survey of Family Growth, Cycle 4 (1988). Sample: Women born between 1939 and 1961, who were aged 24 (n=5,512).

Figure 5: Fraction of women who had married and given birth prior to age 18 for women residing in states where abortion was legalized in 1973



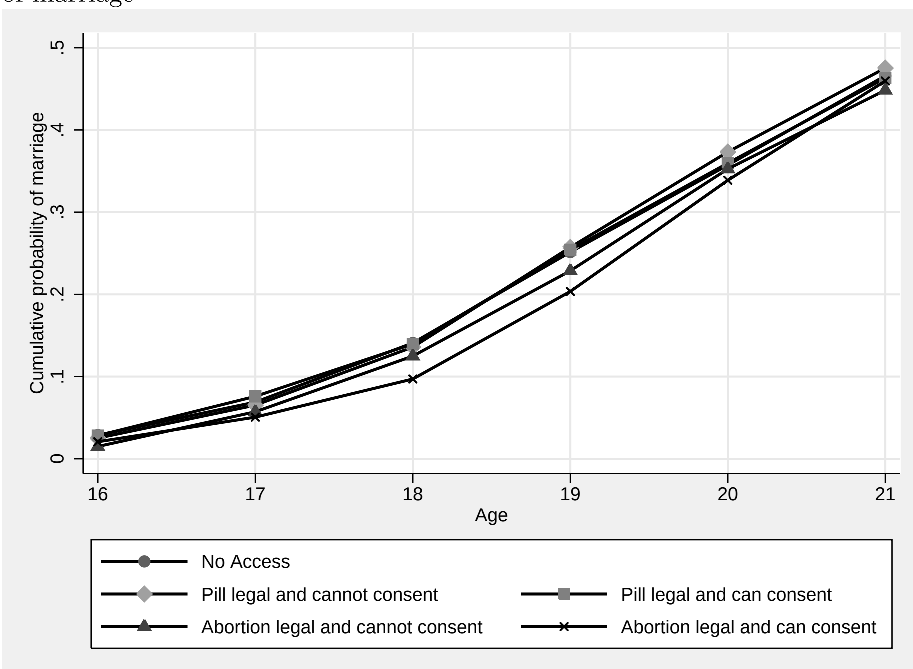
Source: Author's calculations using 1979-1995 CPS Fertility Supplements with information on age at both events. States where MCL policy permitted minors to consent to abortion post-*Roe* are AL, KS, MD, MN, MS, MT, NJ and PA. States where minors could not consent to abortion between 1973 and 1975 are AK, AZ, AR, CA, CT, DE, DC, GA, HI, ID, IL, IO, LA, ME, MA, MI, NV, NM, NY, OK, RI, SD, TN, TX, UT, VT, VA, WA, WV, WI, and WY. Sample: Women born between 1935 and 1960, who were aged 22+ at the time of observation and who lived in the listed states (n=176, 829). Sampling weights are applied.

Figure 6: Estimated effects of reproductive control on the cumulative probability of birth



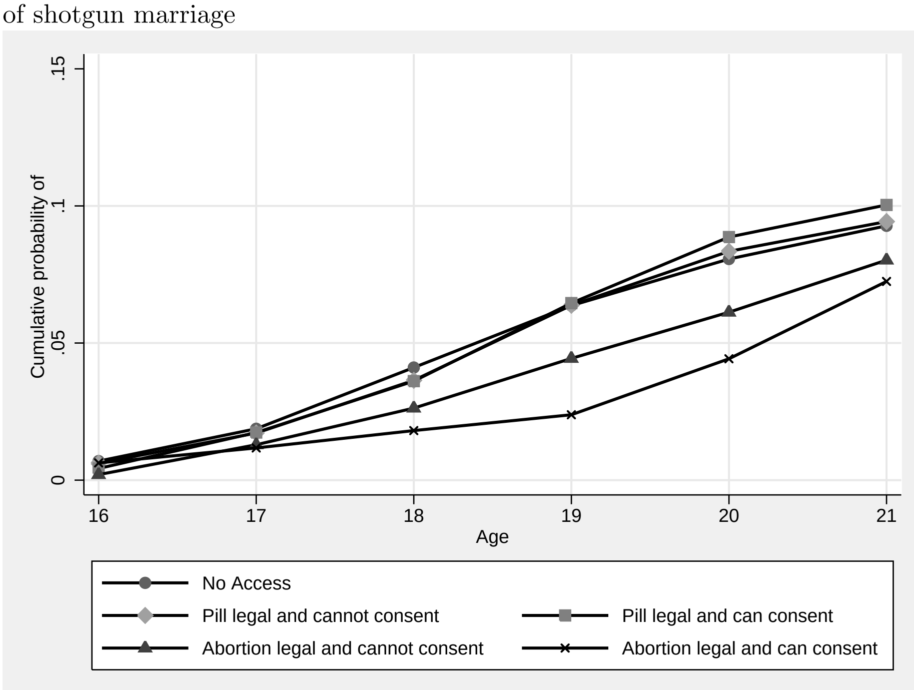
Average adjusted predicted cumulative probabilities of having given birth. Estimates are based on separate linear probability models of having given birth prior to each year of age from 16 through 21. Control variables include race and ethnicity, state fixed effects, year of birth cohort fixed effects, and state*cohort linear time trends. Data: CPS Fertility Supplements, 1979-1995. Sample: Women born between 1935 and 1958 who were aged 22+ at the time of observation (n=279,266). Sampling weights are applied.

Figure 7: Estimated effects of reproductive control on the cumulative probability of marriage



Average adjusted predicted cumulative probabilities of having married. Estimates are based on separate linear probability models of having married prior to each year of age from 16 through 21. Control variables include race and ethnicity, draft deferment policy, minimum age to consent to marriage, state fixed effects, year of birth cohort fixed effects, and state*cohort linear time trends. Data: CPS Fertility Supplements, 1979-1995. Sample: Women born between 1935 and 1958 who were aged 22+ at the time of observation (n=279,266). Sampling weights are applied.

Figure 8: Estimated effects of reproductive control on the cumulative probability of shotgun marriage



Average adjusted predicted cumulative probabilities of having had a "shotgun marriage," which is defined as a first marriage followed in less than eight months by a first birth. Estimates are based on separate linear probability models of having had a shotgun marriage prior to each year of age from 16 through 21. Control variables include race and ethnicity, draft deferment policy, minimum age to consent to marriage, state fixed effects, year of birth cohort fixed effects, and state*cohort linear time trends. Data: CPS Fertility Supplements, 1979-1995. Sample: Women born between 1935 and 1958 who were aged 22+ at the time of observation (n=279,266). Sampling weights are applied.

Table 1: Dates of legal changes affirming the right of young unmarried women to access contraception and abortion, 1960-1979

	Prescription Contraception					Abortion				
	Year of change			Type of change		Year of change			Type of change	
	Ages 21+	Ages 18-20	Ages 15-17	Ages 18-20	Ages 15-17	Ages 21+	Ages 18-20	Ages 15-17	Ages 18-20	Ages 15-17
Alabama	1960	1971	1971	MCL	MCL	1973	1973	1973	MCL	MCL
Alaska	1960	1960[a]	1974	AOM	MCL	1970	1970	1977	PIL	AG+MCL
Arizona	1962	1972	1977	AOM	AG	1973	1973		AOM	
Arkansas	1960	1960	1973	AOM	MCL	1973	1973	1976	AOM	J
California	1963[b]	1972	1976	AOM	MCL	1969	1971	1971	J	J
Colorado	1961	1971	1971	MCL	MCL	1973	1973	1975	MCL PIL	J
Connecticut	1965	1971		MCL		1973	1973		AOM	
Delaware	1965	1971	1972	MCL	MCL	1973	1973	1977	AOM	MCL+AG
District of Columbia	1960	1971	1971	MCL	MCL	1971	1973	1973	J	J
Florida	1960	1972	1972	HH	HH	1973	1973	1975	PIL, AOM	J
Georgia	1960	1971	1972	MCL	MCL	1973	1973		AOM	
Hawaii	1960	1960[c]		AOM		1970	1970		MCL	
Idaho	1960	1960	1974	AOM	LMM	1973	1973		AOM	
Illinois	1961	1961	1969	AOM	HH	1973	1973	1973	AOM	MCL
Indiana	1963	1973		AOM		1973	1973	1975	AOM	J
Iowa	1960	1972[d]		AOM		1973	1973	1976	AOM	AG
Kansas	1963	1970	1970	JMM	JMM	1973	1973	1973	AOM	JMM+AG
Kentucky	1960	1965	1972	AOM	MCL	1973	1973	1974	AOM	J
Louisiana	1960	1972		AOM		1973	1973	1976[e]	AOM	J
Maine	1960	1969[f]	1973	AOM	HH	1973	1973	1979	AOM	J
Maryland	1960	1971	1971	MCL	MCL	1973	1973	1973[g]	MCL	J+MCL
Massachusetts	1972	1974	1977	AOM	JMM	1973	1974	1976	AOM	J
Michigan	1960	1972		AOM		1973	1973	1977	AOM	J
Minnesota	1960	1973	1976	AOM	MCL+J	1973	1973	1973	MCL	MCL
Mississippi	1965	1965[h]	1965[h]	LMM	LMM	1973	1973	1973	MCL	MCL

Continued on next page

Table 1: Dates of legal changes affirming the right of young unmarried women to access contraception and abortion, 1960-1979

	Prescription Contraception					Abortion				
	Year of change			Type of change		Year of change			Type of change	
	Ages 21+	Ages 18-20	Ages 15-17	Ages 18-20	Ages 15-17	Ages 21+	Ages 18-20	Ages 15-17	Ages 18-20	Ages 15-17
Missouri	1965	1977		MCL		1973	1974	1975	PIL	J
Montana	1960	1960[i]		AOM		1973	1973	1973[j]	AOM	MCL
Nebraska	1965	1969[k]		AOM		1973	1973	1975[l]	AOM	J
Nevada	1963	1963	1975	AOM	LMM	1973	1973	1976	AOM	AG
New Hampshire	1960	1971	1971	LMM	LMM	1973	1973	1973	AOM	LMM
New Jersey	1963	1973		AOM		1973	1973	1973	MCL	MCL
New Mexico	1960	1971	1973	AOM	MCL	1973	1973		AOM	
New York	1960	1971	1971[m]	MCL	J	1970	1970[m]	1970[m]		
North Carolina	1960	1971	1977	AOM	MCL	1973	1973	1975[n]	AOM	AG
North Dakota	1960	1960		AOM		1974	1974	1979	AOM	J
Ohio	1965	1965	1965	JMM	JMM	1973	1973	1973[o]	JMM	
Oklahoma	1960	1960		AOM		1973	1973		AOM	
Oregon	1960	1971	1971	MCL	MCL	1973	1973	1973	AOM	J
Pennsylvania	1960	1970		MCL		1973	1973	1973	MCL	MCL+J
Rhode Island	1960	1972		AOM		1973	1973		AOM	
South Carolina	1960	1972	1972[p]	MCL	MCL	1973	1974	1974[p]	PIL	PIL
South Dakota	1960	1972		AOM		1973	1973		AOM	
Tennessee	1960	1971	1971	AOM	MCL	1973	1973	1979	AOM	AG+J
Texas	1960	1973		AOM		1973	1973		AOM	
Utah	1960	1960		AOM		1973	1973		AOM	
Vermont	1960	1971		AOM		1973	1973		AOM	
Virginia	1960	1971	1971	MCL	MCL	1973	1973		AOM	
Washington	1960	1970		MCL		1970	1970	1975	MCL+PIL	J
West Virginia	1960	1972		AOM		1973	1973		AOM	
Wisconsin	1974[q]	1974		AOM		1973	1973		AOM	

Continued on next page

Table 1: Dates of legal changes affirming the right of young unmarried women to access contraception and abortion, 1960-1979

	Prescription Contraception					Abortion				
	Year of change			Type of change		Year of change			Type of change	
	Ages 21+	Ages 18-20	Ages 15-17	Ages 18-20	Ages 15-17	Ages 21+	Ages 18-20	Ages 15-17	Ages 18-20	Ages 15-17
Wyoming	1960	1973[r]		AOM		1973	1973[r]		AOM	

Unmarried women aged 21 and over are coded as gaining access to contraception with the 1960 release of Enovid unless a restrictive Comstock law was in place and as gaining access to abortion in 1973 with the *Roe v. Wade* decision unless the state legalized abortion prior to that. Types of legal change granting access to women under the age of 21 are a change in the age of majority (AOM), enactment of a medical consent law granting unmarried minors capacity to consent (MCL) or permitting physicians to provide the relevant services if they judge that failure to do so would be hazardous a minor's health (HH), judicial or legislative recognition of a mature minor doctrine (JMM or LMM), and an affirmative attorney general opinion (AG). Additional changes affirming young women's access to abortion are a parental involvement law stating a minimum age to consent that is below the age of majority (PIL) and a judicial ruling enjoining enforcement of a restrictive law (J). Source: Myers, 2012.

[a]In Alaska the age of majority was 19 when Enovid was introduced in 1960. Women aged 18 gained access in 1974 with the enactment of a medical consent law.

[b]Enforcement of a Comstock law in California appears to have ceased in 1963.

[c]In Hawaii the age of majority was 20 when Enovid was introduced in 1960. Women aged 18-19 gained access to the pill in 1972 when the age of majority was lowered.

[d]The Iowa legislature lowered the age of majority from 21 to 19 in 1972 and from 19 to 18 in 1973.

[e]A parental involvement law was later enforced in Louisiana from 1978 to 1980.

[f]The Maine legislature lowered the age of majority from 21 to 20 in 1969 and from 20 to 18 in 1972.

[g]The Maryland legislature enacted a parental involvement law in 1977 that appears to have been enforced through 1985.

[h]The Mississippi legislature codified a judicial precedent for a mature minor doctrine for minors seeking medical care in 1966. In 1972 the legislature passed a medical consent law permitting physicians to provide contraception to certain classes of minors that did not include a mature minor provision.

[i]The age of majority was 18 for females and 21 for males in Montana in 1960. In 1971 the legislature set the age of majority at 19 for both males and females; in 1973 the legislature lowered the age of majority to 18 for males and females.

[j]Montana enforced a parental involvement law for abortion from 1974 to 1976.

[k]The Nebraska legislature lowered the age of majority from 21 to 20 in 1969 and from 20 to 19 in 1972.

[l]Nebraska enforced parental involvement laws from 1973 to 1975 and 1977 to 1978.

[m]The New York legislature passed a law in 1971 prohibiting the sale of contraception to people under the age of 16. Enforcement was enjoined in 1975. New York City hospitals performed abortions on minors aged 17 and older without parental consent.

[n]North Carolina enacted a parental consent law in 1977 that lacked a judicial bypass option and was presumably unenforceable.

[o]Ohio enforced a parental consent statute for women under 18 from 1974 to 1976.

[p]Minors under age 16 gained access to contraception in 1976 and to abortion in 1977.

[q]Wisconsin continued to enforce a Comstock law prohibiting the sale of contraceptives to unmarried people until it was enjoined by court order in 1974.

[r]The Wyoming legislature lowered the age of majority from 21 to 19 in 1973; it did not lower it to 18 until 1993.

Table 2: Estimated effects of reproductive policies on the probabilities of giving birth and marrying prior to age 19

	Model 1	Model 2	Model 3	Model 4
Panel A: Pr(birth<19)				
Adjusted prediction	0.1554	0.1607	0.1649	0.1650
Pill legal	-0.0027	-0.0029	-0.0085*	-0.0089*
	(0.0053)	(0.0054)	(0.0048)	(0.0048)
Consent pill	-0.0006	-0.0019	-0.0084	-0.0062
	(0.0093)	(0.0090)	(0.0079)	(0.0086)
Abortion legal		-0.0308***	-0.0324***	-0.0309***
		(0.0094)	(0.0083)	(0.0085)
Consent abortion		-0.0520***	-0.0567***	-0.0552***
		(0.0106)	(0.0108)	(0.0106)
Abortion reform			-0.0128	-0.0129
			(0.0105)	(0.0103)
Consent abortion reform			-0.0147	-0.0169
			(0.0390)	(0.0394)
State EPL			-0.0137*	-0.0139*
			(0.0073)	(0.0072)
State racial discrimination FEPA			-0.0085*	-0.0083*
			(0.0046)	(0.0046)
No fault divorce			-0.0031	-0.0039
			(0.0061)	(0.0057)
Consent pill∗Abortion legal				-0.0079
				(0.0182)
Consent pill∗Consent Abortion				-0.0056
				(0.0143)
Controls for race and ethnicity	yes	yes	yes	yes
Additional controls	no	no	yes	yes
State fixed effects	yes	yes	yes	yes
Cohort fixed effects	yes	yes	yes	yes
State linear time trends	yes	yes	yes	yes

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Table 2: Estimated effects of reproductive policies on the probabilities of giving birth and marrying prior to age 19

	Model 1	Model 2	Model 3	Model 4
Panel B: Pr(marriage<19)				
Adjusted prediction	0.2404	0.2462	0.2514	0.2514
Pill legal	0.0131	0.0126	0.0058	0.0053
	(0.0091)	(0.0091)	(0.0077)	(0.0079)
Consent pill	0.0107	0.0109	0.0028	0.0051
	(0.0144)	(0.0140)	(0.0133)	(0.0124)
Abortion legal		-0.0211**	-0.0228**	-0.0202
		(0.0101)	(0.0095)	(0.0138)
Consent abortion		-0.0457***	-0.0479***	-0.0456***
		(0.0116)	(0.0131)	(0.0169)
Abortion reform			-0.0065	-0.0069
			(0.0100)	(0.0100)
Consent abortion reform			-0.0151	-0.0168
			(0.0218)	(0.0216)
State EPL			-0.0247*	-0.0250*
			(0.0143)	(0.0143)
State racial discrimination FEPA			-0.0129	-0.0127
			(0.0119)	(0.0118)
No fault divorce			-0.0050	-0.0062
			(0.0108)	(0.0123)
Consent pill∗Abortion legal				-0.0092
				(0.0294)
Consent pill∗Consent Abortion				-0.0069
				(0.0239)
Controls for race and ethnicity	yes	yes	yes	yes
Additional controls	no	no	yes	yes
State fixed effects	yes	yes	yes	yes
Cohort fixed effects	yes	yes	yes	yes
State linear time trends	yes	yes	yes	yes

Continued on next page

Table 2: Estimated effects of reproductive policies on the probabilities of giving birth and marrying prior to age 19

	Model 1	Model 2	Model 3	Model 4
Panel C: Pr(shotgun marriage<19)				
Adjusted prediction	0.0579	0.0629	0.0637	0.0638
Pill legal	0.0015	0.0011	0.0001	-0.0004
	(0.0050)	(0.0042)	(0.0048)	(0.0048)
Consent pill	0.0014	0.0018	0.0009	0.0028
	(0.0054)	(0.0057)	(0.0066)	(0.0082)
Abortion legal		-0.0191***	-0.0193***	-0.0159*
		(0.0060)	(0.0063)	(0.0081)
Consent abortion		-0.0389***	-0.0398***	-0.0394***
		(0.0059)	(0.0069)	(0.0083)
Abortion reform			-0.0078	-0.0086
			(0.0070)	(0.0068)
Consent abortion reform			-0.0061	-0.0078
			(0.0185)	(0.0185)
State EPL			-0.0061	-0.0063
			(0.0050)	(0.0051)
State racial discrimination FEPA			0.0001	0.0002
			(0.0057)	(0.0057)
No fault divorce			-0.0042	-0.0050
			(0.0057)	(0.0058)
Consent pill∗Abortion legal				-0.0123
				(0.0144)
Consent pill∗Consent Abortion				-0.0027
				(0.0101)
Controls for race and ethnicity	yes	yes	yes	yes
Additional controls	no	no	yes	yes
State fixed effects	yes	yes	yes	yes
Cohort fixed effects	yes	yes	yes	yes
State linear time trends	yes	yes	yes	yes

The table reports coefficients and standard errors from linear probability models in which the dependent variable indicates whether a woman had given birth, married or had a shotgun marriage prior to age 19. *Pill legal* and *Abortion legal* measure the proportion of years from ages 14 to 17 (birth) or 14 to 18 (marriage) in which the pill or abortion were legally available to adults, but young women could not consent. *Consent pill* and *Consent abortion* measure the proportion of prior years in which the pill or abortion were legally available to adults and state law granted young unmarried women capacity to consent to them. "Shotgun marriage" is defined as a first marriage that was followed within eight months by a first birth. Additional control variables are described in the text. All models include state and cohort fixed effects as well as state linear time trends. Standard errors are clustered at the state level. Data: CPS Fertility Supplements, 1979-1995. Sample: Women born from 1935 to 1958 who were aged 22+ at the time of observation (n=279,266). **p<0.10 **p<0.05 ***p<0.01.*

Table 3: Comparison of results using alternative criteria for coding the policy environment

	Preferred policy coding	Remove AG and MM	Add FP policies	Hock's abortion coding	Bailey's pill coding
Pr(birth<19)					
Adjusted prediction	0.1649	0.1644	0.1641	0.1648	0.1624
Pill legal	-0.0085*	-0.0080*	-0.0082*	-0.0083*	-0.0059
	(0.0048)	(0.0045)	(0.0048)	(0.0048)	(0.0046)
Consent pill	-0.0084	-0.0096	-0.0016	-0.0088	0.0028
	(0.0079)	(0.0091)	(0.0086)	(0.0079)	(0.0083)
Abortion legal	-0.0324***	-0.0341***	-0.0333***	-0.0329***	-0.0331***
	(0.0083)	(0.0125)	(0.0088)	(0.0085)	(0.0088)
Consent abortion	-0.0567***	-0.0538***	-0.0572***	-0.0547***	-0.0576***
	(0.0108)	(0.0103)	(0.0113)	(0.0111)	(0.0113)
Pr(marriage<19)					
Adjusted prediction	0.2514	0.2536	0.2514	0.2508	0.2518
Pill legal	0.0058	0.0036	0.0057	0.0061	0.0053
	(0.0077)	(0.0077)	(0.0077)	(0.0076)	(0.0074)
Consent pill	0.0028	0.0005	0.0032	0.003	0.0018
	(0.0133)	(0.0162)	(0.0130)	(0.0133)	(0.0128)
Abortion legal	-0.0228**	-0.0352***	-0.0228**	-0.0244**	-0.0229**
	(0.0095)	(0.0119)	(0.0094)	(0.0099)	(0.0097)
Consent abortion	-0.0479***	-0.0494***	-0.0478***	-0.0430***	-0.0477***
	(0.0131)	(0.0124)	(0.0130)	(0.0132)	(0.0131)
Pr(shotgun marriage<19)					
Adjusted prediction	0.0637	0.0632	0.0636	0.0635	0.0628
Pill legal	0.0001	0.0008	0.0001	0.0003	0.0013
	(0.0048)	(0.0045)	(0.0048)	(0.0048)	(0.0047)
Consent pill	0.0009	0.0067	0.0013	0.0008	0.0018
	(0.0066)	(0.0071)	(0.0067)	(0.0066)	(0.0066)
Abortion legal	-0.0193***	-0.0279***	-0.0194***	-0.0199***	-0.0192***
	(0.0063)	(0.0088)	(0.0061)	(0.0065)	(0.0063)
Consent abortion	-0.0398***	-0.0408***	-0.0399***	-0.0383***	-0.0396***
	(0.0069)	(0.0063)	(0.0068)	(0.0067)	(0.0069)

The table reports coefficients and standard errors from linear probability models in which the dependent variable indicates whether a woman had given birth, married or had a shotgun marriage prior to age 19. The control variables are as described in the footnote of Table 2 and in the text. All models include controls for race and ethnicity, additional state policy controls, state and cohort fixed effects and state linear time trends. The fourth column uses legal coding for abortion from Hock (2008). The fifth column uses pill coding from Bailey et al. (2011). Data: CPS Fertility Supplements, 1979-1995. Sample: Women born from 1935 to 1958 who were aged 22+ at the time of observation (n=279,266). **p<0.10 **p<0.05 ***p<0.01.*

Table 4: Estimated effects of reproductive policies for different subsets of the population

		Race		Education		Geography	
	Full sample	Whites	Blacks	< college	≥ college	Omits reform states	Limited interstate travel
Pr(birth<19)							
Adjusted prediction	0.1649	0.1393	0.3395	0.1990	0.0301	0.1630	0.1724
Pill legal	-0.0085*	-0.0130***	-0.0069	-0.0094	0.0021	-0.0118**	-0.0089
	(0.0048)	(0.0039)	(0.0308)	(0.0064)	(0.0052)	(0.0050)	(0.0070)
Consent pill	-0.0084	-0.0156**	0.0068	-0.0129	0.0037	-0.0049	-0.0163*
	(0.0079)	(0.0077)	(0.0382)	(0.0102)	(0.0078)	(0.0099)	(0.0088)
Abortion legal	-0.0324***	-0.0227***	-0.0906***	-0.0379***	-0.0013	-0.0433***	-0.0413***
	(0.0083)	(0.0084)	(0.0257)	(0.0087)	(0.0052)	(0.0141)	(0.0099)
Consent abortion	-0.0567***	-0.0461***	-0.1229***	-0.0617***	-0.0201**	-0.0665***	-0.0677***
	(0.0108)	(0.0091)	(0.0349)	(0.0124)	(0.0081)	(0.0124)	(0.0124)
Pr(marriage<19)							
Adjusted prediction	0.2514	0.2615	0.2137	0.3009	0.0584	0.2523	0.2611
Pill legal	0.0058	0.0029	0.0170	0.0072	0.0070	-0.0005	0.0117
	(0.0077)	(0.0073)	(0.0254)	(0.0106)	(0.0075)	(0.0077)	(0.0071)
Consent pill	0.0028	0.0027	0.0178	0.0006	0.0144	0.0047	0.0020
	(0.0133)	(0.0132)	(0.0357)	(0.0163)	(0.0135)	(0.0142)	(0.0163)
Abortion legal	-0.0228**	-0.0331***	-0.0262	-0.0269***	0.0001	-0.0394**	-0.0313**
	(0.0095)	(0.0091)	(0.0315)	(0.0091)	(0.0090)	(0.0159)	(0.0116)
Consent abortion	-0.0479***	-0.0503***	-0.0743*	-0.0530***	-0.0095	-0.0544***	-0.0796***
	(0.0131)	(0.0103)	(0.0397)	(0.0142)	(0.0100)	(0.0152)	(0.0168)

Continued on next page

Table 4: Estimated effects of reproductive policies for different subsets of the population

		Race		Education		Geography	
	Full sample	Whites	Blacks	$<$ college	$\geq$ college	Omits reform states	Limited interstate travel
Pr(shotgun marriage<19)							
Adjusted prediction	0.0637	0.0637	0.0838	0.0789	0.0053	0.0648	0.0629
Pill legal	0.0001	0.0020	-0.0230	-0.0014	0.0065*	0.0024	0.0034
	(0.0048)	(0.0057)	(0.0167)	(0.0062)	(0.0035)	(0.0047)	(0.0063)
Consent pill	0.0009	0.0044	-0.0312*	-0.0017	0.0112**	0.0031	0.0052
	(0.0066)	(0.0080)	(0.0168)	(0.0089)	(0.0055)	(0.0089)	(0.0106)
Abortion legal	-0.0193***	-0.0283***	0.0126	-0.0265***	0.0061*	-0.0283**	-0.0215***
	(0.0063)	(0.0060)	(0.0165)	(0.0084)	(0.0036)	(0.0126)	(0.0078)
Consent abortion	-0.0398***	-0.0470***	-0.0140	-0.0539***	0.0123*	-0.0400***	-0.0450***
	(0.0069)	(0.0104)	(0.0228)	(0.0089)	(0.0069)	(0.0123)	(0.0073)
n	279,266	222,081	29,715	220,558	58,708	196,931	175,116

The table reports coefficients and standard errors from linear probability models in which the dependent variable indicates whether a woman had given birth, married or had a shotgun marriage prior to age 19. The control variables are described in the text and footnote to Table 2. All models include controls for race and ethnicity, additional state policy controls, state and cohort fixed effects and state linear time trends. Standard errors are clustered at the state level. Data: CPS Fertility Supplements, 1979-1995. Sample: Women born from 1935 to 1958 who were aged 22+ at the time of observation. *$p<0.10$ **$p<0.05$ ***$p<0.01$.

Table 5: Estimated effects of reproductive control on the probabilities of marrying prior to age 23 for college-educated women using different samples and codings of the legal environment

	Goldin and Katz's data		Myers' data	
	Goldin and Katz's legal coding	Myers' legal coding	Goldin and Katz's legal coding	Myers' legal coding
I(Nonrestrictive law at 18)	-0.0196*	-0.0001	-0.0059	0.0050
	(0.0109)	(0.0114)	(0.0117)	(0.0101)
Pill legal		-0.0030		0.0176
		(0.0136)		(0.0226)
Consent pill		-0.0017		0.0286
		(0.0224)		(0.0277)
Abortion legal		-0.0920***		-0.0868***
		(0.0264)		(0.0192)
Consent abortion		-0.0845***		-0.0295
		(0.0203)		(0.0266)
Data	1980 U.S. Census, IPUMS, 1% sample		CPS Fertility Supplements, 1979-1995	
Sample	College-educated women aged 23+ born 1935-1957		College-educated women aged 23+ born 1935-1957	
n	60,714		55,630	

The table reports coefficients and standard errors from linear probability models in which the dependent variable indicates whether a woman had married prior to age 23. The legal coding for the ability to consent to the pill is either that used by Goldin and Katz (2002) or reported in Table 1 of this paper. Access to reproductive control is determined by state of birth in the U.S. Census and by current state of residence in CPS Fertility Supplements because state of birth is not reported. I(Nonrestrictive law at 18) indicates that the woman was born in/lives in a state where, by the time she was 18, minors under age 18 had been granted capacity to consent to the pill. *Pilllegal*, *Abortionlegal*, *Consentpill*, and *Consentabortion* are the fraction of years prior to age 21 in which different legal environments were in place and are described in the text of the paper and the footnote to Table 2. All models control for race and include state and cohort fixed effects. Standard errors are clustered at the state level. $*p<0.10$ $**p<0.05$ $***p<0.01$.

Table 6: Estimated effects of reproductive control on the probabilities of giving birth prior to age 19 using different samples and codings of the legal environment

	Bailey's sample		Myers' sample	
	Bailey's legal coding	Myers' legal coding	Bailey's legal coding	Myers' legal coding
I(consent to pill prior to age 21)	-0.0147**	-0.0053	-0.0049	-0.0065
	(0.0064)	(0.0054)	(0.0053)	(0.0053)
Pill legal		-0.0100		-0.0049
		(0.0089)		(0.0055)
Consent pill		-0.0103		-0.0087
		(0.0126)		(0.0102)
Abortion legal		-0.0324***		-0.0142*
		(0.0120)		(0.0074)
Consent abortion		-0.0145		-0.0240***
		(0.0096)		(0.0061)
Data	CPS Fertility Supplements, 1979-1995		CPS Fertility Supplements, 1979-1995	
Sample	Women aged 36+ born 1935-1959 who had married and given birth. Allocated values of age at first birth dropped.		Women aged 22+ born 1935-1959.	
n	101,951		291,986	

The table reports coefficients and standard errors from linear probability models in which the dependent variable indicates whether a woman had given birth prior to age 19. The legal coding for the ability to consent to the pill is either that reported in Bailey (2006) or in Table 1 of this paper. The binary variable I(consent to pill prior to age 21) indicates that the woman lives in a state where she could have consented to the pill prior to age 21. *Pill legal*, *Abortion legal*, *Consent pill*, and *Consent abortion* are the fraction of years prior to age 18 in which different legal environments were in place and are described in the text of the paper and the footnote to Table 2. As in Bailey (2009), all models include state and cohort fixed effects but do not include state linear time trends or control for race. I corrected minor errors that Bailey made when preparing the data for analysis; this is described in detail in Appendix E. Standard errors are clustered at the state level. **p<0.10 **p<0.05 ***p<0.01.*

Table 7: Estimated effects of reproductive control on the probability of giving birth prior to age 19, 1935-1949 birth cohorts

	All	Whites	Blacks
Panel A: All States			
Adjusted prediction	0.1592	0.1400	0.3040
Pill legal	-0.0029	-0.0107	0.0209
	(0.0085)	(0.0086)	(0.0513)
Consent pill	-0.0297**	-0.0109	-0.1605**
	(0.0129)	(0.0148)	(0.0632)
n	140800	113504	14131
Panel B: MS excluded			
Adjusted prediction	0.1575	0.1395	0.2905
Pill legal	-0.0012	-0.0111	0.0467
	(0.0085)	(0.0087)	(0.0462)
Consent pill	-0.0233*	-0.0176	-0.0923
	(0.0127)	(0.0154)	(0.0557)
n	138956	112231	13579
Panel C: OH excluded			
Adjusted prediction	0.1592	0.1401	0.3063
Pill legal	-0.0030	-0.0113	0.0143
	(0.0088)	(0.0088)	(0.0563)
Consent pill	-0.0543***	0.0313**	-0.2429***
	(0.0094)	(0.0118)	(0.0525)
n	135135	108468	13601
Panel D: AL placebo policy, MS and OH excluded			
Adjusted prediction	0.1564	0.1386	0.2877
Pill legal	0.0013	-0.0095	0.0536
	(0.0088)	(0.0091)	(0.0483)
Consent pill	-0.0049	0.0394***	-0.1426***
	(0.0090)	(0.0085)	(0.0299)
n	133291	107195	13049
Controls for race and ethnicity	yes	yes	yes
Additional controls	yes	yes	yes
State fixed effects	yes	yes	yes
Cohort fixed effects	yes	yes	yes
State linear time trends	yes	yes	yes

This table estimates the specification corresponding to Column 3 of Table 2, limiting the sample to the 1935-1949 birth cohorts.
**p<0.10 **p<0.05 ***p<0.01.*

Table 8: Estimated effects of reproductive control on the probabilities of giving birth prior to ages 20-22, 1935-1949 birth cohorts

	All	Whites	Blacks	All No MS, OH
Panel A: Pr(birth<20)				
Adjusted prediction	0.2523	0.2376	0.3729	0.2507
Pill legal	-0.0064	-0.0236	0.0981*	-0.0074
	(0.0159)	(0.0173)	(0.0509)	(0.0161)
Consent pill	0.0114	0.0044	-0.0178	0.0792
	(0.0257)	(0.0301)	(0.0485)	(0.0689)
n	140800	113504	14131	133291
Panel B: Pr(birth<21)				
Adjusted prediction	0.3459	0.3324	0.4767	0.3427
Pill legal	-0.0031	-0.0170	0.0680	-0.0024
	(0.0180)	(0.0209)	(0.0666)	(0.0172)
Consent pill	0.0347	0.0240	0.0302	0.0939*
	(0.0320)	(0.0358)	(0.0925)	(0.0555)
n	140800	113504	14131	133291
Panel C: Pr(birth<22)				
Adjusted prediction	0.4427	0.4298	0.6045	0.4461
Pill legal	-0.0168	-0.0261	-0.0292	-0.0284
	(0.0238)	(0.0260)	(0.0977)	(0.0216)
Consent pill	-0.0141	-0.0238	-0.0329	-0.0038
	(0.0185)	(0.0234)	(0.0871)	(0.0251)
n	140800	113504	14131	133291
Controls for race and ethnicity	yes	yes	yes	yes
Additional controls	yes	yes	yes	yes
State fixed effects	yes	yes	yes	yes
Cohort fixed effects	yes	yes	yes	yes
State linear time trends	yes	yes	yes	yes

This table estimates the probabilities of giving birth prior to ages 20, 21 and 22, limiting the sample to the 1935-1949 birth cohorts. **p<0.10 **p<0.05 ***p<0.01.*