MORE POWER TO THE PILL:
THE IMPACT OF CONTRACEPTIVE FREEDOM ON WOMEN'S LABOR SUPPLY

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Abstract:

The release of *Enovid* in 1960, the first birth control pill, allowed U.S. women unprecedented freedom to plan childbirth and their labor market careers. With the pill, women could delay pregnancy at will with little cost during the ages so critical to building human capital and learning market skills. Variation in laws, which liberalized access to oral contraception for younger and unmarried women, in conjunction with the June and March Supplements to the *CPS*, allow me to evaluate the importance of the pill in explaining the growth in women's employment from 1960 to the present. The results suggest that the participation of women with early access to contraception was approximately 8 percentage points higher at ages 26 to 30 and, once in the labor market, they worked approximately 142 more hours annually, an increase of more than 25%. These estimates imply that changes in legal access to contraception account for about 14% of the growth in young women's participation from 1970 to 1990 and 35% during the 1980s.

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I. Introduction

The movement she [Margaret Sanger[1]] started will grow to be, a hundred years from now, the most influential of all time. When the history of our civilization is written, it will be a biological history, and Margaret Sanger will be its heroine.

--H.G. Wells, 1931 (*Time*, 1998)

The introduction of highly reliable contraceptive technology was one of the twentieth century's most significant social advances. *Enovid*, the first birth control pill released in the U.S., allowed women unprecedented freedom to time childbirth and plan their labor market careers. Goldin and Katz (2002) document the relationship between access to oral contraception and college graduate women's decisions to marry and pursue professional careers, but the "pill" may have profoundly affected women's work at the extensive and intensive margin regardless of educational attainment. Yet more than forty years after the release of the first oral contraceptive, little is known about the broader, long-term labor supply effects of this important innovation.

The claim that the pill transformed women's labor supply decisions finds little support in related literature. On the contrary, using variation in exposure to abortion (Angrist and Evans, 1999), in childbearing due to twinning (Bronars and Grogger, 1997; Gangadharan and Rosenbloom, 1999) or miscarriage (Hotz, McElroy and Sanders, 1997), or variation in parental preferences for mixed-sex offspring (Angrist and Evans, 1998), the bulk of recent research finds that exogenous changes in the total number of children has had only small, and usually negative, effects on women's participation. Angrist and Evans (1998: 474) sum up these findings saying that, "the increase in female labor-force participation has been so large that declining fertility can explain only a small fraction of the overall change (474)."[2]

But these studies implicitly assume that the time and resource constraints imposed by an additional birth are the only costs imposed by a woman's biology. Studies using twinning or preferences can only identify the impact of an additional child upon sample of women, who had already decided *whether* and *when* to become mothers. Likewise, variation in exposure to abortion identifies the effect of *ex post* fertility control for select sample of women, who either didn't have access to the pill or chose not to use it.[3] Neither of these formulations considers the important costs imposed by the uncertainty of conception, the risk of economic insecurity and

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[1] Margaret Sanger (1879-1966) spearheaded the crusade to end state and federal prohibitions on the distribution of contraceptive supplies and information as well as increase the availability of birth control by establishing nonprofit clinics across the United States. She is known as the founder of the modern birth control movement.

[2] A related literature relates early childbearing to women's outcomes including education, experience, labor force participation and wages. Most find that early childbearing has a negative effect on each of these outcomes, while Klepinger, Lundberg and Plotnick (1999) find that adolescent fertility substantially reduces formal education, teenage work experience, early adult work experience for white women, and wages. This literature is also concerned with the causal effects of early childbearing, not the broader effects of fertility risk on women's outcomes.

[3] The first birth control pills were released in 1960, at least ten years before states began to repeal bans on abortion.

social taboo of single motherhood, or the potential disruption of an education or career due to an unexpected birth. Before the age of the pill, however, either abstinence or uncertainty (including delaying marriage, Goldin and Katz, 2002) rendered delaying pregnancy very costly. In turn, these costs inhibited young women from engaging in job search and labor market investments, especially since the risk of unplanned pregnancy later minimized the long-term benefits of these investments.

Variation in state laws restricting legal access to contraception among unmarried women under age 21 provides an ideal laboratory to evaluate the effect of fertility risk on women's lifecycle labor supply. Liberalization of state restrictions over the course of the 1960s and 1970s, due largely to changes in the legal age of majority and judicial precedent, empowered unmarried women under the age 21 to consent to medical care and allowed physicians to treat them without their parents' permission. As a result, these legal changes secured a legal basis for young women to obtain oral contraception. In contrast to most studies using legal variation, this analysis relies on laws that were revised by various branches and levels of government at different times in different states. Furthermore, since many of these changes extended access to the pill only indirectly, these legal changes provide an especially convenient quasi-experiment for studying the effect of early access to the pill (and lower uncertainty associated with fertility) on the allocation of women's time to household and market work.[4]

This paper provides two types of evidence that earlier access to the pill (before the age of 21 and/or marriage) had an important impact on individual decisions. Using the June and March Supplements to the *CPS* in conjunction with changes in state laws, I first establish that the timing of cohort-specific changes in the age of first birth and participation profiles corresponds closely the liberalization of legal access. As oral contraception diffused among younger, unmarried women, the shape of the distribution of age of first birth changed quickly and young women's labor force participation underwent the most rapid changes of the twentieth century (see Figure 3 and Figure 4).[5]

Second, I exploit cross-state variation in the timing of liberalization to examine changes within synthetic cohorts of women over different ages. Using retrospective fertility and marital information from the 1976-1995 June Supplements to the *Current Population Survey*, I provide evidence that within birth cohort variation in early access is significantly related to a decrease in first births over the same age group. Data from the 1964-2001 March Supplements suggest that the participation of women between the ages 26 and 30 with early access

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[4] These laws correspond largely to those used in Goldin and Katz (2000, 2002). In most states, access was not liberalized due to a direct intervention to extend women's rights to obtain contraception but resulted from a change in the age of legal majority or a judicial decision. In addition, further statistical evidence from the PUMS as well as robustness tests within the analysis are reported to strengthen the case that liberalization was, indeed, exogenous to a wide variety of state-level demographic, social and economic characteristics and unrelated to changes in women's revealed preferences for childbearing.

[5] Beginning with the cohort of 1940, oral contraception diffused quickly among young and unmarried women, and the vast majority of women born in 1955 would have had legal access to the pill by age 18, well before making marital, childbearing and career decisions. The differences cited correspond to the rapid shifts in age of first birth and participation between the cohort of 1940 and 1955.

was approximately 8 percentage points higher than other members of their birth cohort gaining access later. In addition, participants of the same ages with early access worked approximately 142 more hours annually, or 30% more, than their peers. Combining the labor force estimates with compositional shifts in the labor market, counterfactual estimates suggest that access to the pill can account for approximately 14% of the increase in young women's participation between 1970 and 1990 and 35% during the 1980s.

This paper contributes to the larger story of their twentieth century economic progress by focusing on a brief, but remarkable, period in young women's employment growth. In its focus, this analysis departs from a long tradition in labor economics, which has emphasized the importance of demand factors in promoting women's work. Within the tapestry of economic factors, my results suggest that a particular supply-side factor, the pill, had larger and broader economic effects than previously believed. In addition, this paper brings new evidence to bear on the link between women's early fertility and long-term labor force outcomes. While many studies have considered the importance of abortion, this paper contributes to a more complete picture of the impact of fertility control by emphasizing the importance of oral contraception and examining women over a longer age horizon.

II. Identifying the effect of the pill on labor supply using variation in state laws

When considering the long history of contraception, one might be skeptical of *Enovid*'s true contribution. After all, the withdrawal method, or *coitus interruptus,* has been used since Biblical times and was popularized in 1831 in the U.S. in Robert Dale Owen's *Moral Physiology*. Moreover, a number of commercialized, though primitive, contraceptive devices had been adopted well before the pill was available (Brodie, 1994).[6] But oral contraception differed from these traditional methods in three important ways.

First, the pill constituted the first truly female contraceptive. It was painless, discreet and could be used to prevent pregnancy at will. Moreover, a woman could independently decide to take the pill; it did not require the consent of men or discomfort to either party during sex. In this sense, the pill transferred control of contraception, which had long been dependent upon men, to the hands of those bearing the high physical, opportunity and childrearing costs.

Second, the decision to take the pill occurred at a time separate from the act of intercourse. Before the age of the pill, the most effective and safe methods of contraception (notably, the "Dutch cap," or diaphragm, and condoms) involved choices and actions of both men and women during intimacy (Marks, 2001). The pill improved upon these methods in that it divorced sex from the decision to contracept.

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[6] By 1924, the condom was the most popularly prescribed contraceptive by 1924 (Tone, 2001), although it is unclear whether they were often used to prevent pregnancy. Most doctors' prescribed them for the prevention of venereal disease for men in extramarital or premarital relationships. Margaret Sanger fought for the right to obtain them for contraceptive purposes. For a thorough history of the condom, refer to Brandt (1985) and Valdiserri (1988).

Third, the pill's reliability and effectiveness far exceeded all other methods available in 1960. From the beginning, *Enovid*'s advocates promoted the pill as 99 % effective.[7] Whereas most couples had accepted an element of risk as part of intercourse, the pill virtually eliminated concerns about pregnancy. In summary, the pill increased the effectiveness and reliability of contraception *per se* and increased the degree to which women could autonomously choose to prevent conception. In addition, the delay or prevention of pregnancy became a painless and much lower cost endeavor, quite separate from the act of intercourse.

The nature of the birth control pill, a "hazardous" contraceptive, renders variation in legal access a convenient econometric tool for studying its impact. While it is questionable whether laws were enforced for other forms of contraception, legal bans were much more effective in limiting the use of the pill since a prescription was required.[8] Obtaining the pill required the prescription of a licensed physician and sale by a licensed pharmacist. Often, the penalties to these professionals for providing illegal information and contraceptive supplies included heavy fines, jail time, and the loss of the license to practice medicine (Garrow, 1994).[9] For these reasons, laws that required that a young women herself be a legal adult (over the age of 21 in most states), that she be married (most states considered a woman legally emancipated at marriage or if she was a mother), or that she obtain the consent of and be accompanied by her parents were more binding in the case of oral contraception.

Variation in legal access at ages under 21

Over the course of the 1960s and 1970s, the age of legal consent was lowered due in large part to changes (1) in the rights of legal minors and (2) in the legal definition of "minority" or "infancy".

Changes in the rights of legal minors occurred as judicial decisions empowered minors to consent for medical treatment. This series of precedents had begun before the introduction of the pill in 1960. An early Ohio case in 1956 recognized a "mature minor" doctrine, waiving the requirement of parental consent if the minor is "intelligent and mature enough to understand the nature and consequences of the treatment" (Paul et al.,

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[7] Although numbers on the effectiveness of contraception are dubious at best, Planned Parenthood estimates the failure rates associated with *typical use* of the condoms available <u>today</u> at around 15% and the failure rates of today's modern diaphragms at around 16%. Less effective spermicides and materials imply that failure rates of these methods would have been much higher in 1960, and they are still quite high relative to 5% failure rates of oral contraceptives.

[8] Effective regulation of condoms, for instance, required only that distributors (often gas station clerks) check the ages or marital status of those making purchases. There is evidence that illicit distribution of non-hazardous contraceptives was quite common. For instance, in 1961, charges were brought against Thomas Coccomo for his enterprise in selling condoms to gas station clerks in North Haven, Connecticut. He was arrested and approximately $100 (in 1961 dollars) of contraceptive supplies were seized under a 1879 Connecticut statute that forbade the distribution or sale of contraception (Garrow, 1994: 188)

[9] Goldin and Katz (2002) provide corroborating evidence that the use oral contraception was strongly and significantly associated with liberalization. Using the NSYW71, they find that young women residing in states with liberal access laws reported approximately 30% higher pill use than young women in states that restricted access. Among the sample of college graduate women, a liberal state law was associated with an increase in pill usage of 45%. It is reasonable to believe that age of majority, 21 in most states, posed a very real constraint for young and unmarried women.

1976: 16). Often, these doctrines were interpreted to apply to women as young as 14. After the pill was introduced, many of these decisions applied to the prescription of oral contraception as non-emergency medical treatment. Thus, as the lower courts extended the rights of minors, these decisions effectively gave latitude to physicians in prescribing oral contraception to young women without consulting their parents (Paul et al., 1974).

Changes in the definition of legal minority and infancy were enacted in legislatures. As the conflict in Vietnam escalated, the public became increasingly concerned that young men could be drafted for war at age 18 but did not obtain the right to vote in federal elections until age 21. The discrepancy in men's civic responsibilities and civil rights catapulted the issue of "legal adulthood" into the national policy debate of the presidential election in 1968. In less than a month after arriving in office in 1969, Nixon instructed Attorney General J. N. Mitchell to study the possibility of lowering the voting age to 18 (*New York Times*, Feb. 11, 1969, p. 12, col. 1). Mitchell's investigation culminated in the ratification of the 26th Amendment to the U.S. Constitution in 1971, which lowered the federal voting age to 18.

Following the federal lead, state legislatures began extending the privileges and responsibilities of legal adulthood to eighteen-year old men and women as well.[10] Many politicians believed that lowering the voting age allowed them to garner support from a large, politically active group of young adults (*NYT*, August 21, 1972, p. 21, col. 5), but others regarded wide-spread unrest and drug use among 18 to 20 year olds as evidence that they could not be trusted to vote in a responsible and informed manner. While extending the right to obtain contraception to younger women had little, if anything, to do with these legislative changes, the lower age of majority empowered younger women to consent to medical treatment and, by extension, the pill.[11] (In the subsequent analysis, I will refer to these states as "age of majority" states). By 1976, only Mississippi, Pennsylvania and Missouri retained 21 as the age of legal majority for most purposes (Paul et al., 1976: 17).[12]

As these changes were taking place, the women's movement and the legal activism of Planned Parenthood successfully challenged in the courts state bans on contraception as violations of individual rights of "procreative privacy".[13] The last of the landmark cases was decided in the fall of 1976, when the U.S. Supreme

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[10] A handful of states, however, had empowered eighteen-year old women as legal adults much earlier than the 1970s, while retaining twenty-one as the age of majority for men. I take these laws to apply to obtaining contraceptives, because I have found no evidence to the contrary.

[11] A number of changes in the rights of individuals under the age of 21 took place following changes in state ages of majority. For instance, individuals could buy and drink liquor, sign contracts, sue and be sued, incorporate themselves, make wills, inherit property, hold public office, serve as jurors, policemen and firemen, marry and divorce without parental consent, become adoptive parents, qualify for welfare benefits and attend X-rated movies. In many states, court cases challenged specific provisions of the lower age of majority, but none that I am aware of challenged a young woman's right to consent to medical care.

[12] Missouri lowered the age of majority to 18 in 1975, but this was for the purpose of signing contracts only. Missouri Supreme Court ruled that a new law lowering the general age of majority from 21 to 18 was unconstitutional (*NYT*, November 13, 1974, p. 85, col. 3).

[13] As early as *Griswold* in 1965, the U.S. Supreme Court struck down Connecticut's ban of the use and distribution of contraceptives and declared a realm of marital privacy. Procreative privacy was extended in *Eisenstadt v. Baird* to apply to

Court ruled in *Planned Parenthood of Central Missouri v. Danforth* that states lacked a "compelling interest" in using age to restrict the distribution of contraception to patients. This decision, by no act of popular opinion, rendered the higher age of legal majority inapplicable to the prescription of oral contraception in the three remaining states.

For the laws described up to this point, the legal history of the liberalization of access appears to have little connection to state-level characteristics relating to women's fertility and employment choices. A handful of states, which passed comprehensive family planning laws, are an exception. By 1972 four states (Georgia, Florida, Nebraska, Wyoming) and the District of Columbia had explicitly liberalized age and marital restrictions for obtaining contraception. These laws either explicitly allowed for the treatment of "every patient desiring services" or were broad enough that physicians could treat patients of any age or marital status.[14]

In order to use these laws to identify cleanly effect of earlier access to the pill, liberalization should not reflect unobservable state differences (for instance, in social conservatism or political activism) that are also correlated with women's economic and fertility outcomes. If this were the case, the estimation would confound changes due to these other factors with changes in legal access. Though the legal and political histories suggest otherwise, I test the correlation of the time that elapsed until liberalization with a number of 1960 state-level characteristics.

State-level 1960 correlates of time to liberalization

Table 1 reports the cross-state, population weighted regressions of the timing of liberalization on 1960 state characteristics. The dependent variable is defined as the number of years elapsed from the time the pill was released in 1960 to the year when the state law liberalized.[15] State-level characteristics are computed for each of the fifty states and the District of Columbia from the 1960 Public Use Microsample (Ruggles and Sobek, 1997) and the Survey of Churches and Church Membership (National Council of Churches of Christ, 1952).[16]

Panel A examines the point-estimates of a variety of 1960 demographic characteristics with the timing of liberalization: the share of individuals living on farms, the share of African-Americans and Latinos, the share of individuals born abroad, and the share of women in different age ranges (15 to 21, 22 to 30, 31 to 45), and location of the state within the South. Panel B examines the correlation of social factors with the timing of liberalization, including the mean age of first marriage and completed fertility of older cohorts, the share of

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unmarried individuals, when the Supreme Court enjoined a Massachusetts statute banning the distribution of contraception to unmarried individuals. The most famous and controversial of these cases is *Roe v. Wade*, which extended the right to procreative privacy to state bans on abortion.

[14] The legal citations for each of these changes are available from the author upon request.

[15] Using a hazard model does not substantially alter the results. [I may switch OLS analysis out and put in the hazard analysis for SOLE].

[16] Note that the 1952 Survey of Churches and Church Membership only included the 48 contiguous U.S. states and the District of Columbia. When this variable is included, these regressions only include 49 observations

individuals living under the poverty line, as well the fraction of the 1950 population who were members of Catholic churches or other Christian denominations.[17]

Of the all of the demographic and social factors, the fraction of individuals who are members of the Catholic Church or of other Christian denominations appears to be the only variable significantly correlated with liberalization at the 5% level. This relationship of the timing of liberalization with the Catholic membership, however, is determined largely by states liberalizing via family planning laws. Since the Catholic Church exercised considerable power in lobbying against statutes that ***directly*** liberalized access to contraception, the family planning states tended to be those with the smallest Catholic constituencies. After eliminating the states that liberalized via family planning statutes, Catholic membership has a weak and statistically insignificant relationship with the timing of liberalization.[18] (Later in the analysis I will again drop these states to test the robustness of my results.) Thus, although there is a slight relationship of Catholic membership with the timing of liberalization, liberalization via changes in the age of majority or judicial decision appears unrelated to social and demographic characteristics.

Panel C and D report the correlation of labor market indicators for both men and women, such as mean educational attainment, age-specific participation and employment rates and wages. Moreover, separate regressions of women's labor market outcomes on fraction Catholic after accounting for average regional differences in fertility yields little evidence of a statistical relationship. A regression of the participation rate of women ages 21-30 on the share Catholic including region fixed effects yields a coefficient of 0.105 with a standard error of 0.092.[19] Taken together, this analysis reveals no evidence that the timing of liberalization is systematically related to state-labor market conditions in 1960 or the fraction of Catholics residing in the state.

The political history of these laws appears to accurately characterize their enactment in practice. Idiosyncratic differences in the regional judiciary, as well as the regional politics of minors' rights and the war in Vietnam resulted in considerable variation in the timing of adoption. Although a larger Catholic membership tended to delay liberalization, this variable does not appear to predict differences in the age of first marriage or childbearing, or 1960 differences in women's participation. In fact, none of the 28 characteristics is statistically

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[17] Since the 1960 census surveys women about the number of children ever born, proxies for state level fertility norms are computed using the mean number of children ever born to 41 to 50 year olds (women born 1910 to 1919) and 31 to 40 year olds (born 1920 to 1929). The timing of marriage is also an indicator of social expectations about women's role in the family and withdrawal from the labor force (cf. Goldin, 1990), so I include the arithmetic mean of the age of first marriage for the same two groups of women.

[18] The coefficient on Catholic membership decreases to 3.17 with a t-statistic of 1.35. Note, I cannot control for percent Catholic in the analysis since this variable is only available for several of many cross-sections in my analysis.

[19] Similarly, a regression of the participation rate of women ages 31-45 on the share Catholic yields a coefficient of 0.060 with a standard error of 0.118. Regressing the number of children ever born for women of the same age on share Catholic yields a coefficient of –0. 37 with a t-statistic of 1.42. Finally, a regression of the age of first marriage for the same age group of women on share Catholic yields a coefficient of -9.01 with a t-statistic of -1.01. Each of these regressions includes region fixed effects and is weighted by state population.

significant at the 5% level when other variables are included, and only one is significant at the 10% level.[20] In results not reported here, I repeat this exercise using the 1970 Public Use Microsample for the 41 states, which hadn't liberalized access before 1970, and find similar results. Although Goldin and Katz (2002) do not report an explicit test for state-level correlates of these laws, this analysis supports their claim: the changes in laws restricting access to contraception are exogenous with respect to a wide variety state-level factors. Thus, I will use variation in the timing of liberalization to identify the effect of having access to oral contraception at earlier ages on the timing of birth as well as lifecycle labor supply.

III. The mechanism relating the pill to women's participation and time-series evidence

Laws that reduced the age, at which women could legally obtain oral contraception, substantially reduced the costs of delaying pregnancy during ages that were critical in determining the career choices of many women. Goldin and Katz (2002) have emphasized the importance of the pill in reducing the costs of delaying marriage for the sample of college graduate women. This analysis, however, abstracts from the decision to marry and focuses on the effect of early access to the pill on age of first birth. Indeed, the age of first birth and age of first marriage are highly correlated, as marriage marks the beginning of the socially sanctioned period of childbearing and conception often results in and from the decision to marry. However, two reasons suggest that the age of first birth presents a better measure of women's fertility behavior.

First, oral contraception divorced the decision to marry from the decision to have sex and bear children, so early access to the pill should have countervailing effects upon the age of first marriage. While pill allows women to delay marriage without delaying sex, the pill also eliminates the need to delay marriage as more women could delay childbearing within marriage.[21] Second, the rise in illegitimate births since 1960 (see for instance, Carter et al. 2003), a measure of unplanned pregnancy, suggests that changes in age of first birth may better capture changes in women's fertility.

The impact of early access on the age of first birth

Before the advent of the pill, delaying pregnancy came at a high price. For women choosing to have sex, it included the psychic and social costs of violating norms about pre-marital sex or out-of-wedlock childbearing; the costs of a "sub-optimal" shotgun marriage; the psychic, health and resource costs of terminating the pregnancy through abortion (illegal for most women in my sample); and the expected costs of

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[20] A number of other variables including race and age specific poverty rates, race and age specific fertility rates, race and age specific education measures of college and high school graduation by sex, and metropolitan concentration not presented here are statistically insignificant and do not appear to rebut this claim.

[21] The time to first birth within marriage increased from 1.8 to almost 3 years from the birth cohort of 1933-1935 to the birth cohorts of 1956-1960. (Source: Author's computations using the June *CPS*, see Appendix 1). Appendix 2 provides further evidence that the age of first marriage exhibits very little relationship with liberalization after controlling for age,

bearing and raising a child. For women choosing abstinence, delaying sex also imposed costs, most notably those associated with delaying marriage (Goldin and Katz, 2002). Changes in laws governing legal access altered the costs associated with sex and searching for a spouse.

Both aggregate and cohort-specific time-series suggest that the fertility behavior of young women changed rapidly as legal changes extended access to the pill to younger, unmarried. As plotted in Figure 1, the fraction of white women giving birth before the age of 22 (the lines in bold) fell rapidly following the release of the pill until around 1976, the year the U.S. Supreme Court struck down state bans restrictions on distributing contraceptives to minors. The timing of the change in age-specific first birth rates corresponds well to the liberalization of laws restricting access among unmarried 18 to 20 year olds and the diffusion of oral contraception to younger women. Some of these changes, however, may be due to the aging of the high fertility cohorts that gave birth to the Baby Boom.

More compelling evidence can be found when examining changes in the timing of cohort-specific age of first birth. Using the 1976-1995 June Supplements to the *CPS,* Figure 2 plots the fraction of ever-married mothers, who had a first birth within a given three-year age interval.[22] For example, the point at age 18 denotes the fraction of ever-married mothers with a birth within the age range of 17 to 19. While many of the women born before 1940 would have been married and had children before the pill was first released, almost all women born after 1955 should have been able to obtain the pill legally by age 18.

The stark differences ***between*** the cohorts without early access to contraception (1933-1940) and those with early access (1955-1960) contrast sharply with the similarity of the distributions ***within*** these groups. In addition, since the right tail of younger cohorts' distributions is relatively large, truncation at age 35 (necessary to make the age composition of these cohorts comparable) tends to understate the observed difference between 1940 and 1955 distributions. Even so, the cohorts born after 1955 had negligibly fewer births around age 18 but significantly fewer had given birth by age 22.

The lack of pre-existing trends between the 1933 and 1940 cohorts or a trend after pervasive legalization was achieved (the cohort of 1956 was the last to have access restricted) again provides suggestive evidence that the large changes in the age of first birth between the cohorts of 1940 and 1955 may be related to liberalization. Moreover, the changes are most evident for women around ages 20-22, but not within the group of younger women who would not have benefited from liberalization.[23] In summary, the cross cohort shifts in age of first

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cohort, state effects for the sample of ever-married women. This differs from the findings of Goldin in Katz (2002), who use a sample that is unrestricted by marital history. The discussion in the appendix seeks to reconcile these findings.

[22] The June *CPS* consist of a sample of ever-married women, who had married for the first time at or before age 34 and had a birth at or before age 34 and were at least 35 years old at the time of the survey. The justification for this sample restriction is provided in footnote 30.

[23] Goldin and Katz (2002) report that a number of states allowed legal access to women as young as 14, but is unclear how many 14 year olds were able to obtain contraception in practice.

birth correspond closely to the diffusion of oral contraception among younger and unmarried women.[24] I test the relationship of age of first birth with liberalization econometrically in the next section.

One final point is worth noting. Completed fertility should be only weakly correlated or even uncorrelated with liberalization, given that most women desire at least one child. Regardless of state legislation, all women obtained access to the pill at age 21 or marriage or upon becoming a mother. Thus, an ill-timed early pregnancy could simply be offset by having one fewer children at older ages when the pill allowed these decisions to be made with certainty. For this reason, those with early access and those without should be equally likely to achieve their target number of children unless it is zero (a very small fraction of the women born from 1940 to 1960). One exception to this would be if women with early access changed their target fertility due to better career prospects resulting from more continuous involvement in the labor market and growing opportunities for women.[25] Another possibility is that women conceive fewer children as the biological ability to conceive and deliver children declines with age. There is, however, little support for this effect in the data. In summary, using variation in access before age 21, this analysis can only identify the effect of the risk and/or reality of early pregnancy on the probability of birth before age 22 and lifecycle employment.

The impact of early access on the labor force participation

Provided that early access to the pill altered the timing of births, early access to the pill may have affected the women's lifecycle labor supply as well. But is it reasonable to believe that a three-year change (from age 21 to 18) in legal access should have an important effect on women's labor market participation? There are two reasons to believe that it should. First, by lowering the cost of delaying in pregnancy, earlier access effectively reduced the *cost* of early job-shopping and career investment. As the *immediate* price of participation (delay of pregnancy) fell, women could substitute their efforts away from searching for a mate and childrearing towards labor market activity or making human capital investments.[26] Early labor market involvement may have long-term effects on women's careers, as individuals and firms better ascertained the quality of the job-match and women could make more informed decisions about their career paths (including withdrawing from the market to become mothers). Early access to the pill should affect market activity both

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[24] The "ever-married" universe of women in the June *CPS* also tends to understate the differences. The fraction of women marrying by age 34 falls quickly over the cohorts of interest. While it is true that approximately 93% of women marry at least once before their thirtieth birthdays, the "ever-married" sampling criterion will render the sample less representative of later cohorts. If one expects never married women to have had fewer or later births than the typical woman who is or has been married, the sampling composition should tend to overestimate the number of children ever born to later cohorts and underestimate the age of first birth. Thus, comparisons in the differentials between women born in 1940 and 1955 appear smaller than they would for women without conditioning upon marital status.

[25] It is quite reasonable to imagine that the increase in wages from more continuous involvement in the market and better job-skill matching provide an indirect disincentive for women to bear children later, but there is no evidence for this effect in the data.

immediately and over a woman's lifetime, and, holding preferences constant, given a strong enough wage effect may affect a woman's likelihood of withdrawing later to have children.

Second, earlier access to the pill increased the long-term *returns* to better job matching and early career investments. By preventing unintended pregnancies, earlier access to oral contraception enabled young women to work and invest in job-specific and general human capital unfettered by the time and resource constraints of motherhood, until choosing to raise a child. The implication of this second effect is that women with early access to the pill acquire more continuous experience in the labor market and incur higher opportunity costs when deciding whether leave it later.

In sum, having the pill at younger ages should affect the *timing* of first birth, and, thus, women's participation at younger ages should be non-decreasing in earlier access to the pill. Liberalization, however, has ambiguous implications for women at older ages. On the one hand, women who delay pregnancy would be more likely to have children at older ages and, therefore, less likely to work. Especially since many women who raised their children during their twenties reentered the labor market later, the participation of women who delayed birth and with younger children might look substantially lower. On the other hand, older women may have a number of opportunities that younger women do not: jobs that allow maternity leave, hiring nannies from savings, and choosing husbands more likely to help in the childrearing and household. These opportunities tend to minimize the time away from work. In addition, higher wages from more continuous involvement in the workforce, a better job match or more human capital increases the opportunity cost of childrearing and provides a disincentive to childbearing at later ages. Thus, it is unclear in theory whether women with earlier access to the pill would tend to work more or less at older ages than their peers.

Time-series changes in the nature and level of women's labor force involvement correspond to the diffusion of the pill to younger women as well. As plotted in Figure 3, 1960 marks a sharp change in the participation trends of younger and older women. While increases in the labor force participation of older women had dominated aggregate trends before 1960, younger women's participation increased most rapidly following the release of the pill. By 1980, faster participation growth among younger women equalized the rates of both groups.[27]

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[26] The marriage model presented in Goldin and Katz (2002) explicitly considers the effect of the pill in changing the costs of delaying marriage, which also relates to the delay in childbirth.

[27] The end of the Baby Boom and the subsequent participation increase, as noted by Goldin, may be endogenous to growing opportunities and rising wages for women during the same period. However, the higher participationrates of participation among younger women from 1960 to 1980 come in spite of generally unfavorable economic and labor market conditions. During this period, economic growth slowed, the economy suffered three recessions and unemployment and inflation increased. Moreover, the earnings of women relative to those of men actually *fell* from the mid-1950s to the mid-1970s (Goldin 2002, Blau and Kahn 2000). While some of this fall in relative wages is certainly due to selection of women into the workforce, the difficult economic conditions and falling market wages of women would correspond well to a story of shifting labor supply.

Important changes in women's work are evident by birth-cohort by arraying participation rates by age and year of birth as well. Figure 4 depicts the progressive increase in lifecycle participation across the twentieth century, but two distinctive periods emerge. During the first forty years of the century, the largest increases in women's work were among older women, who tended to withdraw from the labor force while raising their children and reenter the labor market after their children were grown. Beginning with the cohorts born after 1940 and ending with those born around 1955, the "fertility dip" in labor force participation completely disappeared. For women born after 1950, there is no observable employment decline over the childbearing years (cf. Smith and Ward, 1985: S65). This brief episode of rapid change constitutes the largest inter-cohort shift among *young* women during the 20th century. The participation rates of women born in 1955 were 24 percentage points higher at age 25, and 20 percentage points higher at age 30, than those of women born in 1940.[28]

More interesting for this paper, however, is that the timing and the nature of this shift in young women's participation corresponds closely to the rapid changes in the age of first birth and the diffusion of contraception among younger, unmarried women.

IV. The empirical case for contraception: data and estimation results

In conjunction with state legal changes, I use the June and March Supplements to the *CPS* to examine the effect of the pill. The *CPS* consist of repeated cross-sections of detailed information individual level characteristics including retrospective information on the age of first birth, age of first marriage and completed fertility (June Supplements)[29]; current employment information on earnings and participation including hours worked in the reference week and weeks worked in the previous calendar year (March Supplements) as well as demographic and geographic information (both surveys).

In order to make the June sample comparable across the years 1976 to 1995, I restrict the sample used in the estimation to include ever-married women between the ages of 35 and 44. Because the June *CPS* consistently sampled a universe of ever-married women over the years in the analysis, I additionally require that women in the estimation had married once by age 35.[30] The analysis of age of first birth by necessity further limits this sample to women bearing a child by age 35.[31]

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[28] These trends are born out for married women as well, though the advances of college graduates began earlier (cf. Goldin, 2002: Figure 4, Figure 5). The participation of married women at 30 increased by approximately 27 percentage points between cohorts born in the first forty years of the 20th century. This magnitude of this forty-year increase was exceeded by the rapid increase among married 30 year olds born between 1940 and 1960. Similarly, the increase among college women between cohorts born 1936-40 and 1956-1960 exceeds 20 percentage points.

[29] For the years in which these variables are not directly reported, I compute the age of first birth/marriage by subtracting the mother's birth-year_from the first child's birth year or the year of first marriage.

[30] Without this sample restriction, the analysis would essentially compare a biased sample of the younger cohorts (women who married younger) to a more representative sample from the older cohorts. The sample of younger women would be biased because they are observed at younger ages when fewer are married. Requiring that women be married by age 35

Annual information from the 1964-2001 March Surveys provides a rich portrait of the timing of inter- and intra-cohort participation changes in labor supply. Since the March Supplements consist of annual surveys, I observe synthetic cohorts of women (same year of birth) across their lifetimes. For instance, snapshots of the cohort of women born in 1950 can be observed at ages 14 to age 51, so that participation can be traced across age groups for women residing in different places. The sample is restricted to individuals born between 1930 and 1960 (so that the dataset is tractable), who are between 16 and 45 years old for the outcome in question, and who are not working in agriculture (for descriptive statistics, see Appendix 3).[32]

The regression analysis exploits the repeated cross-sectional sampling of the *CPS* to make age-specific comparisons of women born in the same year who would have obtained legal access to oral contraception before the age of 21 to those who did not. For inferences in this framework to be causal, the timing of legislation must be random with respect to other unobservable factors affecting women's fertility and labor market outcomes. While I provide evidence that the timing of this legislation is unrelated to a number of state level characteristics in 1960, the subsequent analyses document additional evidence to support a causal interpretation of the results.

In the regression analysis, two features of the data will tend to minimize the estimated effect of liberalization. First, I cannot determine the degree to which state laws were enforced, but unenforced legislation should bias my results toward finding no effect. In addition, my findings will be biased toward zero if age 21 was a nonbinding constraint.[33] Another feature of the data that will tend to attenuate the results is that there is no information regarding a woman's region of residence on or around her 21st birthday.[34] It is, therefore, necessary to assume that changes in access to contraception in the current region of residence applied to a woman's decisions at age 20. In the June *CPS*, individuals are observed between the ages of 35 and 44 and so the estimated effect will be more uniformly attenuated for all cohorts in the sample. In the March *CPS*, however, a synthetic cohort of women is followed over many ages and measurement error should grow with age as the probability of residing in the same place as at age 21 falls. The long-term effect of early access, the point-estimates for older women, is based upon the sample of non-movers. If non-movers are women who do not take

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biases both samples but makes them more comparable. In this way, a woman who married at age 30 and had a first birth at age 32 would be in my sample of women born in 1935 and 1960, whereas a woman who first married at age 40 would be excluded from both samples. Age 35 is chosen because the youngest cohort in this analysis was born in 1960 and the last year I have information on age of first birth is in 1995. Thus, I only observe women from this young cohort who had married at or before age 35.

[31] The June Supplements are not collected annually and surveys before 1976 are omitted since limited geographic information is provided.

[32] The upper age limit of 45 is chosen so that I can average the age effects over all of the birth categories for all cohorts with access. Since the cohort of 1956 is the last in my sample with variation in early access would be age 45 in 2001, the average treatment effect can only be obtained up to age 45.

[33] In either case, a bias toward zero occurs because I average zeros (no effect) with the actual effects.

advantage of labor market opportunities in other states (due to individual preferences, financial constraint or marital status), the long-term effects of early access will also be underestimated.

Despite the fact that these biases tend to work to minimize the estimated effect, the subsequent analysis provides two types of evidence that the pill was instrumental in changing women's participation profiles. First, I estimate the effect of early access to the pill on a variety of fertility measures using the June Surveys. These results provide evidence that liberalization is related to changes in the timing of births, but not in the probability of becoming a mother or the log number of children. Second, I estimate the impact of liberalization on the labor supply of women born in the same year. These results suggest that early access is strongly related to the evolution of labor supply within a synthetic cohort of women.

The causal impact of early access to the pill on age of first birth

While close correspondence in the timing of shifts in the distribution of age of first birth and the diffusion of oral contraception is suggestive, state-level changes in the age of access allow careful testing of this relationship. Using the June data, I estimate equations using pooled annual cross-sectional data of the following general form:

$$(1) \qquad Y_{icas} = \alpha_0 + Access_{ics}\alpha_1 + f_s + g_c + h_a + \varepsilon_{icas}$$

where c *refers* to the year of birth, a to the age, and s to the state of residence for individual i. f_s, g_c and h_a denote a set of state, individual year of birth and age specific dummies. State effects capture time-invariant geographic factors that affected women's outcomes. State-specific linear cohort trends, included in some specifications, capture the secular evolution of cohort behavior within states. Cohort specific effects absorb time-varying national trends in cohort behavior such as shifts in attitudes or the effect of uniformly applied federal legislation. Age effects account for unobserved changes in the composition of the June sample as women age. *Access* is a binary variable equal to one if a woman currently resides in a state where she would have had access to contraception before age 21. In this difference-in-differences empirical framework, the effect of the pill is estimated using intra-cohort variation in legal access to the pill and averaged over the cohorts born from 1940 to 1956. Thus, α_1 reflects differential changes in fertility that are correlated with changes in access to the pill after accounting for state, cohort, and age fixed effects.

Table 2 reports the marginal effect of *Access* (evaluated at the mean) on age of first birth using a probit specification. For comparison, the last row reports the effect of *Access* evaluated for every observation and averaged over the entire distribution. The dependent variable is a binary variable equal to 1 if a woman gave birth by age 21 (potentially gained from liberalization). Women without reported first births are coded as zeros.

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[34] The *CPS* do not report region or state of birth. Instead, the *CPS* report whether an individual was living in the same house in the previous year in March for most years from 1963-1988. Since regions are larger than states, and interregional

Since never-married women are excluded from the June samples and early access to the pill would tend to allow them to delay or forego marriage, the effect of the pill should also be larger in a more general sample of women.

Column 1 presents the base specification, which includes state, age and cohort fixed effects. The point estimate, evaluated at the mean, implies that ever-married women with early access were significantly less likely to have a child by age 21. Using the mean of the dependent variable, this implies a 15 percent reduction (0.051/0.348) in the likelihood of an early birth. This effect grows larger in column 2 when the definition of access is coded as 1 for women who would have had access by age 18 (cf. Goldin and Katz, 2002). The stronger effect in column 2 is consistent with the greater opportunities available to women if they gained access to the pill at younger ages. The magnitude is stronger for the sample of women born from 1940 to 1956 (column 4) (the fixed effects better capture unobserved state effects associated with the group of interest). The inclusion of state-specific year-of-birth linear trends has no observable effect on the magnitude or significance of the point estimate in column 5.

Not only is the effect of early access quite robust across specifications, the effect is much smaller outside the sample of college women used by Goldin and Katz (2002). Restricting the sample to women who at least 16 as the highest grade attended (approximately 17 percent of the original sample), early access to contraception appears to *increase* slightly the likelihood of first birth before age 21. The estimates from the unrestricted sample, however, are negative and account for a significant decrease in every specification.[35] This comparison suggests that the effect of legal access to the pill was at least as strong (if not stronger) and economically important for the sample of ever-married women with less than 16 years of education.

The most important objection to a causal interpretation of α_1 is that legal change might reflect unobservable changes in social norms or public policies relating to sexual behavior and childbearing. That is, unobservable forces that caused the laws to change, albeit indirectly, might have also caused women's fertility outcomes to change though they had nothing to do with legal access to the pill. For example, the development of the strong movement against Vietnam might influence the law and might also influence the strength of the women's movement, which, in turn, affects women's desire to have children and work.

One direct way to test this hypothesis is to examine the correlation of these laws with proxies for a cohort's attitudes about motherhood and childrearing. Although women in the June *CPS* do not report these attitudes directly, they reveal them in their fertility behavior. Since all women gained access to contraception upon marriage or at age 21, liberalization should only have an effect on the *timing* of birth and not on measures of completed fertility, except indirectly through wages or biological constraints on conception or childbearing after age 30. Therefore, finding a strong effect of early access on completed fertility would suggest that the

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mobility occurs less that interstate mobility, this source of error is less of a concern than if states were used in the analysis

[35] Estimating the effect at each point in the distribution and averaging yields a slightly smaller effect of approximately 4%.

laws affected women through channels other than just the timing of first birth. This hypothesis is decisively rejected by the data.

Table 3 reports estimates of the marginal impact of early access on the probability of becoming a mother by age 35 and the total number of children. The dependent variable for the first set of probits is a binary variable equal to 1 if a woman has given birth to any children by age 35. The effect of early access to contraception on becoming a mother is negative and very small in every specification. In the second set of regressions, the dependent variable is the log of total number of children ever born to mothers older than 34. Again, the effect of early access on the total number of children is slightly negative, though very small and never marginally significant. These results support the claim that access laws are correlated with the timing of births, but _not_ secular changes in the likelihood of becoming a mother or the number of children. Taken together with the strong correlation with age of first birth, these results strengthen the case that delay of first birth is a valid mechanism linking early access and labor supply.

In summary, any explanation threatening the validity of a causal interpretation of the correlation of liberalization with changes in the age of first birth must be (1) uncorrelated with state-level characteristics in 1960; (2) uncorrelated with a linear trend in women's first birth within a given state; (3) occur coincident to liberalization at different times in different states; (4) be unrelated to measures of women's completed fertility; and, as argued in the next section, (5) significantly related to women's labor force participation.

The causal effect of early access on lifecycle labor supply

The employment data provided in the March *CPS* improve upon that in the June Supplements, because they provide annual work information. One important shortcoming of the March data is that not all states are individually identified over the entire period of analysis. From 1968 to 1976, information on smaller states is grouped into regions. Though I have used states in the analysis up to this point, the March analysis uses 21 comparable regions (some are individual states) to capture consistently geographic units over the entire period (see Appendix 4). With this limitation, I define "early access to contraception" in *CPS* region *r* in year *t* for a woman *j* years beyond her 20th birthday as

$$(2) \qquad Access_{j,r,t} = \frac{1}{P_{r,t-j}} \sum_{s \in r} P_{s,t-j} \times I(Law_{s,t-j})$$

where $P_{r,\,t-j}$ denotes the population of region *r* in year *t-j*; $P_{s,t-j}$ denotes the population of state *s* in region *r* in the year *t-j*; and I is an indicator function equal to one if state *s* had a liberal access law in year *t-j*. *Access* sums the populations of the states within a *CPS* region only if the state had a liberal access law before the woman's 21st birthday. Thus, *Access* can be interpreted as the probability that a woman currently residing in region *r* in year *t* *would have had* access to contraception before her 21st birthday, assuming that she lived in the same region at that time. The measure ranges from 0 for the birth cohort of 1939 (the pill would not have been available to these women before age 21 since it was released in 1939) and 1 for the birth cohort of 1957 (all women born in this year would have been younger than 21 when they gained access due to *Danforth*). This probabilistic measure of *Access* again introduces error into the analysis.[36]

I estimate equations using pooled annual cross-sectional data from the 1964-2001 March *CPS* of the following general form:

$$(3) \qquad Y_{icrt} = A'_{icrt}\beta_0 + A'_{icrt} \times Access_{icrt} \times \beta_1 + f_{rt} + g_c + h_t + \varepsilon_{icrt}$$

where *i* refers to the individual, *c* to the year of birth, *r* to the *CPS* region, and *t* to the year of observation for the particular outcome. The fixed-effects f_{rt}, g_c and h_t denote region, year and year of birth (cohort) specific effects. Region-specific linear time trends in some specifications capture the gradual evolution of region-specific trends in women's labor market participation. *A* includes a constant and a set of dummy variables for five separate age categories (21-25, 26-30, 31-35, 36-40 and 41-45; 16-20 is omitted). The effect of *Access* interacted with age categories, β_1, is a vector of point estimates. Each component captures the age-group specific impact of liberalized access on participation.

[36] In results not reported here, I examine aggregation error by limiting the analysis to the years 1977-2001, a period over which the *CPS* individually identifies all the states. Controlling for individual, rather than CPS region, fixed effects does not alters the estimates or their significance.

Table 4 reports the marginal effect of early access on labor force participation evaluated at the sample mean. A woman is coded as being in the labor force if she worked in the week prior to the survey or looked for work in the reference week. Columns 1 and 2 present the estimates for women with access at different ages relative to women at the same ages without access. The estimates exhibit similar patterns, and adding region-specific linear time trends in column 2 increases the magnitude of the point estimates at all ages. The participation of women with early access is 5 percentage points higher at ages 26 to 30 and 3 percentage points higher at ages 31 to 35. These estimates imply that early access to the pill led to an increase of nine percent for women in their late twenties and six percent in their early thirties.[37] Limiting the sample to white and to college women (those reporting at least 16 as the highest grade attended) reveals a similar pattern of behavior.[38] Finally, since the age-specific participation rates of college women were higher than those of women with less education, the marginal effect of early access is greatest among non-college women.

The remaining two specifications examine the robustness of the results to a number of sample restrictions. First, I limit the sample to individually identifiable states in column 5 to examine the impact of aggregation error (from using a probabilistic measure of access). In all cases, the estimated results are consistent with attenuation bias, as the point estimates increase and the standard errors fall despite the fact that the sample size is halved. Second, I limit the sample to states, which liberalized through a change in the age of majority in column 6.[39] The estimates change little and in some cases are strengthened although only a fraction of the observations are used.

One further test lends credibility to the hypothesis that timing and early fertility link early access to the pill and labor supply. Using a similar variable for participation from the June *CPS*, I relate having a first birth before age 21 to the effects of early access.[40] Column 7 reports the estimates from the same specification as in column 1 (but with state fixed effects) and there appears to be no effect of early access on participation of ever-married younger women. However, after adding a variable to control for an early pregnancy (perhaps the reason for an early marriage for many of the women sampled) and the impact of early access *net* of early childbearing is negligible and statistically zero. Among women who did not have their first child before age 22, the estimated effects follow the same general pattern for the June sub-sample of ever-married women, although the

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[37] These estimates are based on the age-specific participation estimates of .579 for ages 26-30 and .534 for ages 31-35. For white women, the age-specific participation is even lower at .551 for 26-30 and .521 for 31-35 year olds.

[38] The exception is that the marginal effect of access is strongly negative for college-educated women ages 21 to 25. In related work not reported here, I find that early access to contraception increases the unconditional probability of enrolling in another year of school after 12 years of education. Thus, I interpret this negative effect as the likelihood that more women with access are in college rather than in the labor market during their early twenties. The fact that participation rates of women with 16 or more years of education are quite similar to those of all women for other age groups strengthens this interpretation.

[39] The regions that include *only* states liberalizing though a reduction in the age of majority are Connecticut, New Jersey, Indiana, Kentucky-Tennessee, Texas and California.

magnitudes in percentage points and percent are much smaller. Thus, even within the sample of ever-married women early access is associated with an increase in labor force participation, which suggests that delaying births beyond a critical age and gaining greater control over fertility timing allowed women to work more.

These estimates strongly support the claim that earlier access to contraception increased participation. Not only is the pattern of increase consistent with fertility delay, the results are also large and statistically significant in the March *CPS*. Moreover, the range of the estimates is relatively tight and, in most cases, the marginal effect exhibits little statistical difference across specifications. The strength and magnitude of early access to the pill fades over time. This effect could be due to either (1) greater error in measuring access as women age or (2) more women with early access withdrawing from the market around the same time as women without early access re-enter the formal labor market (cf. Hotz, McElroy and Sanders, 1997). Nevertheless, these estimates provide strong evidence that early access to contraception mattered in women's decisions to participate at younger ages, especially during the ages traditionally associated with high fertility.

Two measures of the intensity of labor supply underscore the importance of early access as well. Table 5 and Table 6 suggest that, among participants, work behavior changed as well. Each column of these tables replicates the earlier analysis for the dependent variable log of hours worked in the previous calendar week and log of weeks worked in the previous calendar year, respectively. For hours worked, the results vary little across the different samples and specifications. In addition, the magnitudes are quite similar to those for participation with the exception that women were significantly more likely to have worked more hours at *all* ages over 25. At ages 26 to 35, the increase is largest at around 7 to 8 percent more hours, which translates into slightly more than 2 additional hours per week. Thus, a woman with early access to contraception worked on average 70 more hours annually (2 hours times 35 weeks worked by the average 26-30 year old woman, or slightly less than two full-time weeks per year).

In addition, the estimates in Table 6 are strongly supportive of an effect of early access on weeks worked. As before, the results vary little across all specifications and samples. It is notable that point-estimates even appear larger when the region-specific year trends are included in column 2. The pattern again mirrors the effect on hours and participation: early access to contraception increases weeks worked relative to similarly aged peers by the largest amount at the ages typically associated with high fertility. For women in the workforce, this shift amounts to an increase of approximately 6 percent at ages 26-30, or slightly more than 2 weeks per year; 31 to 35 year olds with early access worked on average 1.4 weeks more per year.[41]

If one believes that weeks worked and hours are substitutes, the results imply that women with early access to the pill worked approximately 2 weeks more per year. On the other hand, if women increased both the

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[40] It should be kept in mind, however, that the ever-married sampling criterion is quite restrictive at younger ages and less so as women age, so the sample universe is changing considerably the age groups.

[41] The average number of weeks worked by women 26-30 years old was 35.67 and by 31-35 year olds 31.56.

hours worked and weeks worked, these estimates imply a much larger shift. Combining the estimates for weeks with those of hours implies that female participants between ages 26 and 30 with early access worked at least 142 more hours (or 3.5 full-time weeks) per year on average.[42]

The magnitude of these results at the intensive margin suggests the pill not only reduced the cost of entering the labor force, but that participants worked substantially more hours and weeks. Since these estimates condition upon being in the workforce, they are more difficult to interpret. First, considering that negative selection into the labor force would tend to bias the estimates for hours and weeks worked downwards for younger women (as the most productive and educated women were already working), the estimates presented thus far should be conservative.

Taking these estimates of weeks and hours worked at face value, one might conclude that women with early access simply worked more. This could be due to two effects. Growing experience and specific-skills due to more continuous involvement in the labor force might have substantially increased the opportunity cost of working fewer hours. Thus, conditional upon being in the labor force, women with early access worked on average more hours and weeks across their lifecycles since the wage gains from doing so were higher. In addition, the prevention of early pregnancies would tend to reduce the constraints on women's time for the duration of the period that would have been spent raising the child in the absence of the pill. Thus, women with early access would work more on the intensive margin as she is unfettered by the time constraints of childrearing.

According to the simple framework in section III, however, early access may also affect selection out of the labor force at older ages to have children. As women who delayed pregnancy slowly withdraw from the labor market to have and raise children, the women who remain in the labor force may be substantially different on unobservable characteristics than those who withdraw. For instance, many of these women may have chosen career over motherhood. Taken to this extreme, the pill set more women on career trajectories allowing them to opt out of motherhood. In this way, the hours and weeks effects may be due largely to the changing composition of the labor force sample.

While the first two explanations would be attributable to the real economic effects of the pill, the third suggests that selection may play a substantial role in determining who remains in the labor force at older ages. To the extent that early access to the pill allowed women select out of motherhood at lower cost than before it's introduction, the changing composition of the older, childless labor force of women is also an economically and socially important consequence of contraceptive freedom. Given the results thus far, each of these factors may plan an important role in determining the strong effect of early access on the intensity of labor supply at older ages.

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[42] This is calculated by multiplying the difference in hours by average weeks among participants and adding it to the

V. The long-term effects of early access to the pill

Up to this point, the discussion has highlighted the importance of early access for cohort decisions to bear children and participate in the labor market. Understanding the broader impact of the pill, however, requires placing these estimates in the context of changing demographic factors within the economy. To this end, I generate counterfactual estimates of participation using the model in equation 3. The counterfactual assumes that every state, from 1960 to the present, prohibited access to contraception among unmarried women under the age of 21.[43]

Table 7 presents the results. Panel A lists the observed participation rates for women ages 16 to 65 from 1940 to 1990, as well as the rates for those ages 16 to 30 and 16 to 45 for the years 1965 to 1990.[44] The counterfactuals in Panel B average the predicted values for the estimation sample for the year and age group indicated. Taking the difference between actual participation rates and the predicted rates in line 2, I compute the fraction of the change over each interval attributable to the pill (see Table notes). This computation indicates that changes in early access accounts for very little of the increase in women's work during the 1960s. During the 1970s and 1980s, however, early access to contraception can account for 12 to 35 percent of the overall changes in participation for women ages 16 to 30, though the estimates are smaller at 7 to 16 percent for the larger sample of women. This is the case because the aggregate effects of the pill grow as the fraction of women with early access increased over time.[45]

The counterfactual estimates suggest that early access to contraception played an important role in transforming women's long-term labor market activity in the 1970s and 1980s. As with any counterfactual computation, these numbers should be viewed cautiously. They are, of course, "partial equilibrium" comparisons and do not, for example, take account of feedback effects of the pill on other variables, such as wages or occupational choice; nor do they incorporate the possibility that greater access to the pill substantially altered social norms governing women's labor market roles. For instance, cohort norms would be averaged into the predicted participation rates since they are capture in the models year of birth and year fixed effects. Finally, this counterfactual does not consider spillovers of the effect of contraception on already married or older women, since the variation in laws only allows for identification of the effect on women under the age of 21 at the time of liberalization. Nevertheless, this simple thought experiment suggests that reductions in uncertainty

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difference in weeks times the average hours worked per week, 2.17*35.67+2.06*31.18≈142.

[43]To generate this counterfactual, I set *Access* equal to zero and predict the participation of each individual in my sample. Note that this counterfactual assumes that the pill had *no* effect on women who were older when the pill was introduced as the model only identifies the effect of *early* access.

[44] The statistics for years prior to 1965 are computed using the 1940-1960 PUMS and are provided for comparison.

[45] Alternately, I compute the full access counterfactual by setting *Access* equal to 1 for all women in the sample who would have been under age 21 when the pill was introduced in 1960. Taking the observed levels as a base, these point suggest that participation would have increased earlier if all women ages 18 to 20 would have gained access to the pill in 1960. This counterfactual suggests that participation would have been 11 in 1965 and 14 percent higher in 1970 among 18-30 year olds.

about fertility outcomes can account for economically significant short and long run shifts in women's participation.

VI. Conclusion

Economists have been hesitant to assign too great an importance to fertility control in shaping women's post-war labor force participation boom. In examining the causes of the rise in women's work (and falling fertility), most studies have focused on shifting demand-side factors. Between 1950 and 1980, studies attribute roughly 50-60 percent of the change in participation to women's real wage growth (Smith and Ward, 1985; 1989; Goldin, 1990), largely due to falling discrimination through the elimination of marriage bars (Goldin, 1988) as well as the rise of the clerical sector (Goldin, 1984; Smith and Ward, 1985). More recently studies consider the importance of growing demand for highly skilled and professional workers (Black and Juhn, 2000; Welch, 2000).

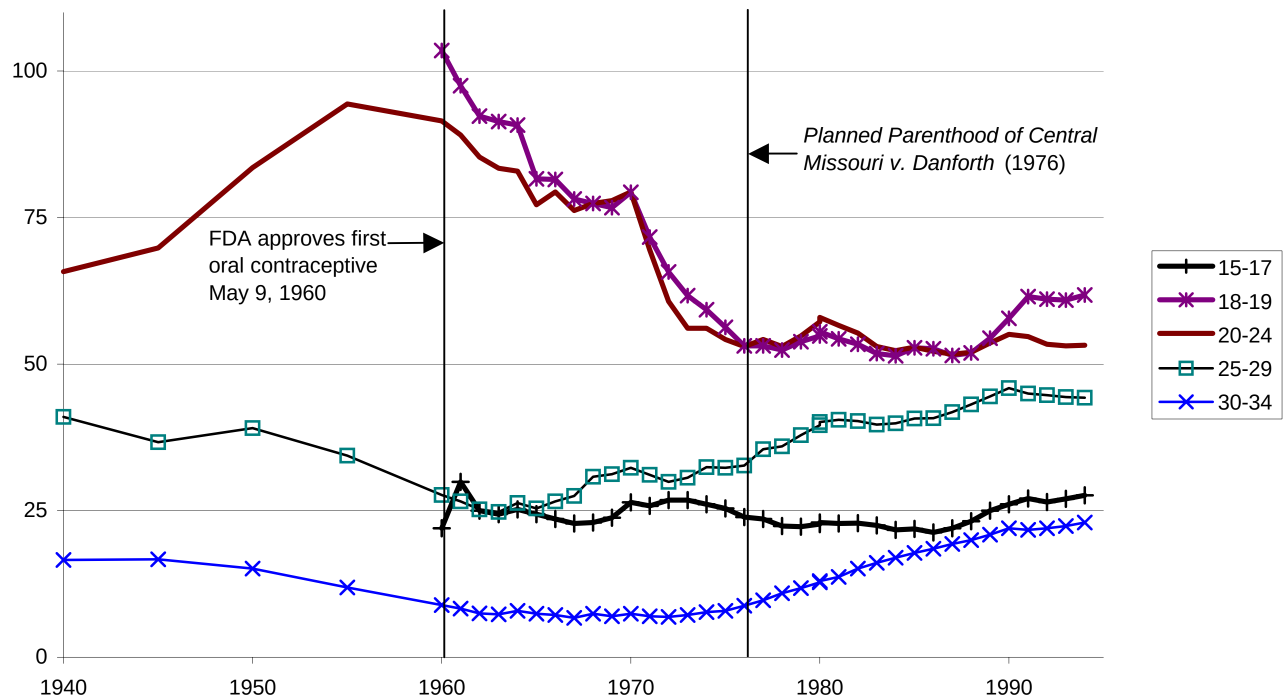
While each of these factors played an important role in increasing women's employment, this paper provides new evidence that access to oral contraception has had large and permanent effects on the timing of young women's fertility and lifecycle labor supply. The fraction of women, who had children by age 21, decreased by around 15 percent among women with early access. In addition, these women participated approximately 8 percent more between the ages 26 and 30 than their peers, though these effects dwindle with age (cf. Hotz, McElroy and Sanders, 1997). Finally, the data provide strong support that the relationship between age of first birth and participation were direct and causally related to earlier access. In summary, relative to the 2% of postwar participation increases attributed to exogenous changes in fertility put forward by Angrist and Evans (1998), the counterfactuals implied by my results claim a much greater role for contraceptive innovation.

It is clear that the importance of contraceptive freedom extends well beyond its social and legal significance. The innovation of the pill allowed women to delay childbirth in a deterministic manner and early access vastly reduced the cost of controlling fertility during the ages critical to human capital formation, acquiring work experience, and learning about the market value of one's abilities. This control allowed women to gain more labor market experience and, in doing so, make better decisions about whether to work in the market or at home.

By altering the constraints associated with market work, the pill has catalyzed many important changes in the economic role of women. While these effects are not addressed in this paper, these changes have had, and will continue to have, long-term effects on the dynamics of the labor market, household production and childrearing. Further study may shed light on the importance of oral contraception in redefining the economic opportunities available to women and our understanding of how the women to work revolution—the “second demographic transition”—has shaped recent history.[46]

[46] Sara McLanahan described the “second demographic transition” in her presidential address to the Population of America Association Meetings, March 2004, as including the delay of marriage and childbirth and the increasing number of women in the workforce.

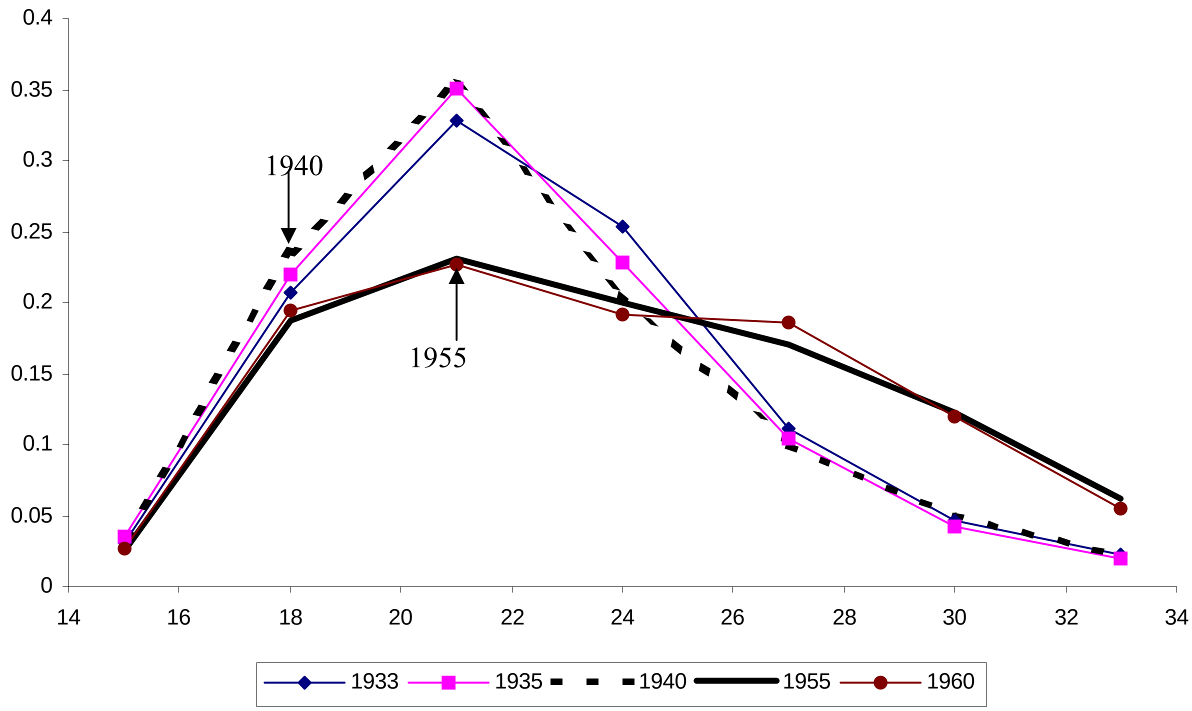
Figure 1. White first birth rates by age of mother, 1940-1995



Notes: First birth rates are computed as the number of live first births per 1000 women in the appropriate age group. "White" refers to the race of the mother through 1980. From 1980-1998, "white" refers to the race of the child.

Source: Division of Vital Statistics, National Center for Health Statistics, Statistical Tables on Births, Table 1-2 First birth rates by Age of Mother, According to Race and Hispanic Origin: United States, Specified Years 1940-1955 and Each Year 1960-1994 (2003).

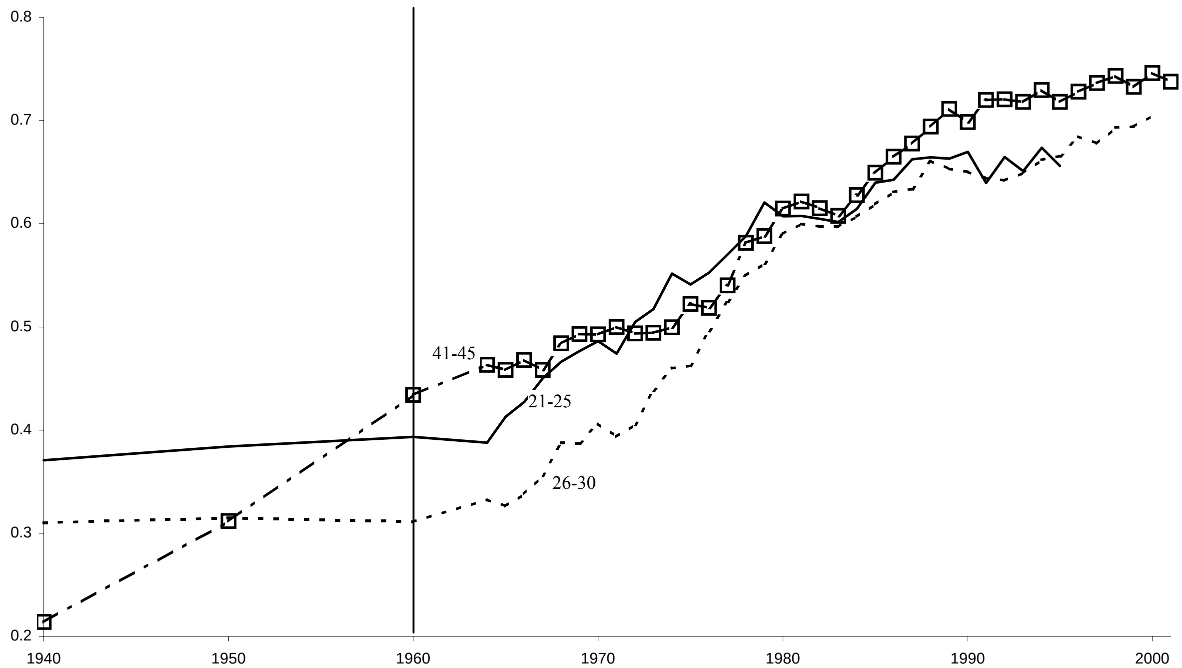
Figure 2. The fraction of women by age of first birth, by cohort



Notes: The figure plots the fraction of white women (vertical axis) with a first birth at a particular age (horizontal axis). Synthetic birth cohorts generated by computing the year of birth (reported age from the year of the survey). Sample includes ever-married women who had married for the first time at or before age 34 and had a birth at or before age 34 and were at least 35 years old at the time of the survey.

Source: June Supplements to the *Current Population Survey* 1976-1995.

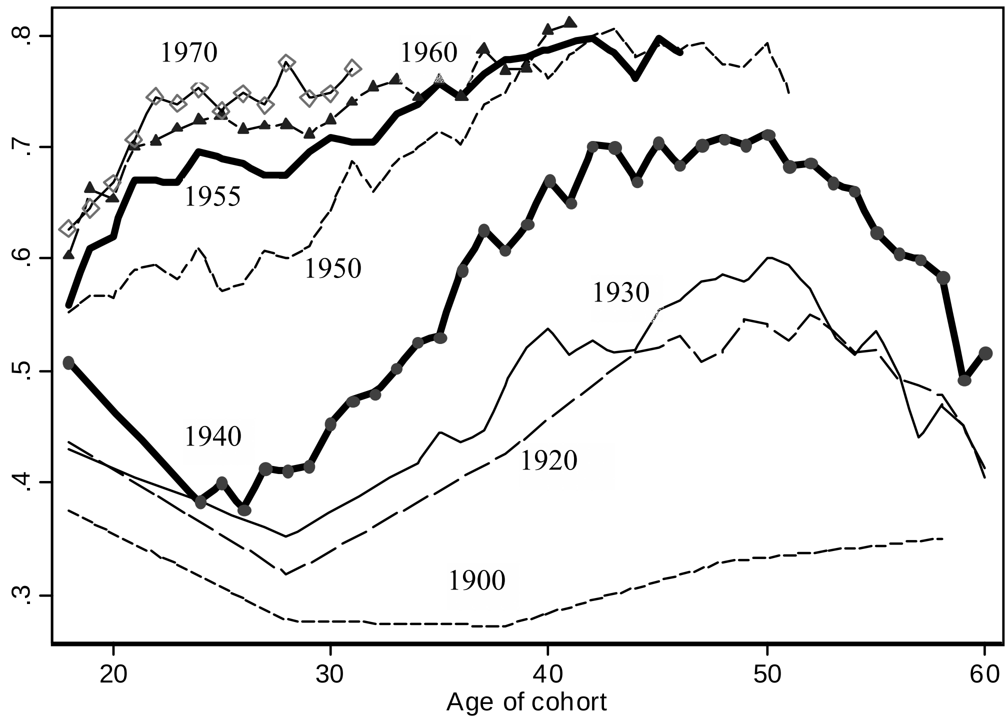
Figure 3. Labor force participation by year and age group, 1940-2000



Notes: Figure plots the fraction of all women ages 16-65 for 1940-2001 employed or seeking employment in week prior to the survey week. Sample includes all women not in military or inmates.

Source: PUMS 1940-1960 (Ruggles and Sobek, 1997) and 1964-2001 March Supplements to the *CPS*.

Figure 4. Age-specific labor force participation rates, by cohort and age 1900-1970



Notes: Synthetic birth cohorts by subtracting the reported age from the year of the survey. Bold lines depict the 1940 and 1955 cohorts. Sample includes all women not in military or inmates ages 16-60 not working on a farm.

Source: 1964-2001 March Supplements to the *CPS* supplemented with information on cohorts prior to 1940 taken from Smith and Ward (1985).

Table 1. 1960 State-Level Predictors of Time Until Liberalization of Access to Contraception

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Demographic characteristics								
Share living on farm	11.613	1.846						
	[10.343]	[5.702]						
Percent Black	-2.05		-1.339					
	[7.426]		[3.945]					
Percent foreign born	20.585			10.857				
	[23.410]			[6.592]				
Fraction women ages 15-21	-0.648				-4.325			
	[9.802]				[12.208]			
Fraction women ages 22-30	12.2					24.148		
	[15.094]					[18.327]		
Fraction women ages 31-45	-19.99						-19.41	
	[26.934]						[21.888]	
South	1.535							0.379
	[2.052]							[1.156]
R-squared	0.136	0.002	0.001	0.03	0.003	0.053	0.01	0.003
B. Social characteristics								
Mean age 1st marriage (1910-1919)	0.123	0.124						
	[0.589]	[0.277]						
Mean age 1st marriage (1920-1929)	-0.445		-0.465					
	[0.951]		[0.705]					
Number of children ever born (1910-1919)	-12.214			-20.388				
	[13.090]			[12.378]				
Number of children ever born (1920-1929)	0.463				-23.374			
	[25.428]				[15.340]			
Fraction in poverty	5.833					-0.595		
	[9.392]					[4.153]		
Share Catholic parish membership	9.312						5.236	
	[6.646]						[2.109]**	
Share church members	-1.28							5.769
	[5.333]							[3.074]*
R-squared	0.093	0.001	0.010	0.055	0.046	0.000	0.035	0.031

Notes: The dependent variable is the year the state enacted the law – 1960. The regressors are state-level aggregates and are computed as described in the text. All regressions are weighted by state population in 1960. *denotes significant at 10%; ** at 5%; *** significant at 1%. In column 7 and 8, there are only 49 observations because the 1952 Survey of Churches and Church Membership only included the 48 contiguous U.S. states and the District of Columbia.

Source: 1960 PUMS (Ruggles and Sobek, 1997). Data on church membership obtained from the National Council of the Churches of Christ in the U.S. A. (1956).

Table 1 (cont'd). 1960 State-Level Predictors of Time Until Liberalization of Access to Contraception

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
C. Labor market conditions for men								
Mean highest grade of schooling	0.633	0.038						
	[1.213]	[0.583]						
Fraction in labor force of men ages 22-30	-1.908		-4.149					
	[15.672]		[13.643]					
Fraction in labor force of men ages 31-45	13.268			1.857				
	[19.664]			[15.537]				
Fraction unemployed of men ages 22-30	-7.364				-10.863			
	[16.410]				[13.342]			
Fraction unemployed of men ages 31-45	-45.597					-43.7		
	[48.390]					[50.691]		
Mean wages of male participants ages 22-30	-0.003						-0.001	
	[0.003]						[0.001]	
Mean wages of male participants ages 31-45	0.002							0.000
	[0.001]*							[0.001]
R-squared	0.136	0.000	0.001	0.000	0.008	0.035	0.026	0.000
D. Labor market conditions for women								
Mean highest grade of schooling	-0.186	-0.111						
	[0.703]	[0.750]						
Fraction in labor force of women ages 22-30	-3.367		-1.149					
	[10.076]		[7.367]					
Fraction in labor force of women ages 31-45	8.67			6.388				
	[10.811]			[10.131]				
Fraction unemployed of women ages 22-30	5.784				1.806			
	[7.711]				[3.980]			
Fraction unemployed of women ages 31-45	5.333					2.703		
	[13.392]					[16.526]		
Mean wages of female participants ages 22-30	0.002						0.001	
	[0.002]						[0.001]	
Mean wages of female participants ages 31-45	-0.002							-0.001
	[0.003]							[0.002]
R-squared	0.073	0.000	0.000	0.009	0.002	0.000	0.011	0.007

Notes: The dependent variable is the year the state enacted the law – 1960. The regressors are state-level aggregates and are computed as described in the text. All regressions are weighted by state population in 1960. *denotes significant at 10%; ** at 5%; *** significant at 1%.

Source: 1960 PUMS (Ruggles and Sobek, 1997). Data on church membership obtained from the National Council of the Churches of Christ in the U.S. A. (1956).

Table 2. The marginal effect of early access to contraception on age of first birth

Dependent variable	1= First birth by age 21						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	*All*	*White*	*Born 1940-1956*	*All*	*College*[b]	*College*[b]
Mean dept. variable	*0.348*		*0.339*	*0.346*	*0.348*	*0.131*	
Access before 21	-0.051		-0.054	-0.055	-0.051	0.026	
	[0.029]*		[0.031]*	[0.028]*	[0.029]*	[0.015]*	
Access by 18		-0.057					0.008
		[0.030]*					[0.015]
State effects	X	X	X	X	X	X	X
Year of birth effects	X	X	X	X	X	X	X
Age effects	X	X	X	X	X	X	X
State x year of birth trends[a]					X		
Observations	115765	115765	100773	93939	115765	19856	19856
Log likelihood	-72019.0	-72006.8	-61984.4	-57939.9	-71814.1	-7576.3	-7578.1
Average effect	-0.040	-0.035	-0.041	-0.043	-0.038	0.020	0.005

Notes: Synthetic birth cohorts by subtracting the reported age from the year of the survey. *denotes significant at 10%; ** at 5%; *** significant at 1%. Robust standard errors corrected for clustering on year of birth-state cells reported in brackets. The estimates are evaluated at the mean. Averaging the effect over the entire distribution yields estimates in the last line of the table. [a] This is a set of dummy variables for state interacted with a linear trend in year of birth. [b] This sample is restricted to women who report at least grade 16 as the highest grade attended.

Sample: Ever-married women who married for the first time at or before age 35.
Source: June Supplements to the *Current Population Survey* 1976-1995.

Table 3. The marginal effect of early access to contraception on selection into motherhood and number of children ever born

Dependent variable	1=At least one child ever born by age 35				Log number of children ever born to women over 34			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	All	*White*	*Born 1940-1956*	*All*	*All*	*White*	*Born 1940-1956*	*All*
Access before 21	-0.01	-0.012	-0.004	-0.003	-0.027	-0.028	-0.022	-0.03
	[0.013]	[0.013]	[0.014]	[0.014]	[0.020]	[0.022]	[0.021]	[0.019]
State effects	X	X	X	X	X	X	X	X
Year of birth effects	X	X	X	X	X	X	X	X
Age effects	X	X	X	X	X	X	X	X
State x cohort trends[a]				X				X
Observations	110445	95910	87711	110445	100743	87299	79561	100743
Log likelihood/R^2	-38403.6	-34235.7	-30081.7	-38292.7	0.024	0.027	0.019	0.026
Average effect	-0.008	-0.010	-0.003	-0.002				

Notes: Synthetic birth cohorts by subtracting the reported age from the year of the survey. *denotes significant at 10%; ** at 5%; *** significant at 1%. Robust standard errors corrected for clustering on year of birth-state cells reported in brackets. The estimates for the binary dependent variable are evaluated at the mean. Averaging the effect over the entire distribution yields estimates in the last line of the table for the probit estimates. [a] This is a set of dummy variables for state interacted with a linear trend in year of birth.

Sample: Ever-married women older than 34 who married for the first time at or before age 35.
Source: June Supplements to the *Current Population Survey* 1976-1995.

Table 4. The marginal effect of early access to contraception on labor force participation

Dependent variable	1= Worked last week or looked for job							
	March *CPS*						June *CPS*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	*All*	*White*	*College*[b]	*Individually identifiable states*	*Age of majority states*	*Ever-married*	*Ever-married*
Mean dependent variable	0.640		0.638	0.794	0.621	0.641	0.729	0.777
Access before 21* ages 21-25	0.008	0.012	0.013	-0.051	0.015	0.007	-0.015	0.006
	[0.006]	[0.006]**	[0.006]**	[0.013]***	[0.007]**	[0.011]	[0.024]	[0.023]
Access before 21* ages 26-30	0.052	0.056	0.056	0.051	0.056	0.050	0.000	0.037
	[0.006]***	[0.006]***	[0.006]***	[0.010]***	[0.007]***	[0.011]***	[0.015]	[0.017]**
Access before 21* ages 21-35	0.027	0.031	0.025	0.018	0.019	0.033	-0.016	0.036
	[0.006]***	[0.006]***	[0.006]***	[0.009]**	[0.007]***	[0.011]***	[0.016]	[0.017]**
Access before 21* ages 36-40	0.005	0.009	0.005	-0.023	-0.017	0.017	-0.039	0.021
	[0.006]	[0.006]	[0.007]	[0.009]***	[0.007]**	[0.011]	[0.018]**	[0.022]
Access before 21* ages 41-45	-0.003	0.000	0.005	-0.037	-0.029	0.014	-0.008	0.052
	[0.007]	[0.008]	[0.008]	[0.010]***	[0.010]***	[0.015]	[0.017]	[0.022]**
First birth by age 21								0.010
								[0.012]
Access before 21* first birth by 21								-0.058
								[0.018]***
Region & year effects	X	X	X	X	State	X	State	State
Year of birth effects	X	X	X	X	X	X	X	X
Age categories	X	X	X	X	X	X	X	X
Region x year trends[a]		X	X	X	State trends	X		
Observations	904132	904132	777473	146595	517829	221729	301529	283217
Log likelihood	-558808	-558422	-479957	-72087.9	-326311	-138012	-172834.6	-142119.3

Notes: Synthetic birth cohorts by subtracting the reported age from the year of the survey. The marginal effect presented in the table is the effect evaluated at the mean. *denotes significant at 10%; ** at 5%; *** significant at 1%. Robust standard errors reported in brackets. The estimates are evaluated at the mean. [a] This is a set of dummy variables for *CPS* region interacted with a linear time trend. [b] This sample is restricted to women who report at least grade 16 as the highest grade attended. [c] State indicates that state rather than CPS region fixed effects are included. March sample: Women ages 16-45 not in the military or inmates born 1935 to 1965. June sample: Ever-married women born before 1965. Source: March *Current Population Survey* 1964-2001, June *CPS* 1976-1992.

Table 5. The marginal effect of early access to contraception on hours worked

Dependent variable	Log hours worked last week					
	(1)	(2)	(3)	(4)	(5)	(6)
	All	*All*	*White*	*College*[b]	*Individually identifiable states*	*Age of majority states*
Access before 21* ages 21-25	-0.004	-0.006	-0.007	0.013	-0.022	-0.018
	[0.010]	[0.010]	[0.011]	[0.013]	[0.021]	[0.017]
Access before 21* ages 26-30	0.070	0.070	0.078	0.075	0.060	0.084
	[0.009]***	[0.009]***	[0.010]***	[0.012]***	[0.021]***	[0.016]***
Access before 21* ages 31-35	0.076	0.077	0.082	0.072	0.080	0.054
	[0.009]***	[0.009]***	[0.010]***	[0.011]***	[0.019]***	[0.015]***
Access before 21* ages 36-40	0.068	0.069	0.070	0.048	0.068	0.016
	[0.010]***	[0.010]***	[0.010]***	[0.011]***	[0.020]***	[0.015]
Access before 21* ages 41-45	0.066	0.068	0.074	0.045	0.079	0.015
	[0.010]***	[0.010]***	[0.011]***	[0.012]***	[0.020]***	[0.016]
Region & year effects	X	X	X	X	State & yr effects	X
Year of birth effects	X	X	X	X	X	X
Age categories	X	X	X	X	X	X
Region x year trends[a]		X	X	X	State -year trends	X
Observations	514612	514612	446182	281308	122582	108092
Adjusted R-squared	0.067	0.068	0.068	0.069	0.065	0.01

Notes: Synthetic birth cohorts by subtracting the reported age from the year of the survey. *denotes significant at 10%; ** at 5%; *** significant at 1%. Robust standard errors corrected for clustering on year of birth-state cells reported in brackets. The estimates are evaluated at the mean [a] This is a set of dummy variables for *CPS* region interacted with a linear time trend.

Sample: Women ages 16-45 not in the military or inmates born 1930-1970.
Source: March *Current Population Survey* 1964-2001.

Table 6. Marginal effect of access to contraception on weeks worked

Dependent variable	Log weeks worked last year					
	(1)	(2)	(3)	(4)	(5)	(6)
	All	*All*	*White*	*College*[b]	*Individually identifiable states*	*Age of majority states*
Access before 21* age 21-25	0.005	0.008	0.004	-0.056	0.018	0.013
	[0.008]	[0.008]	[0.009]	[0.015]***	[0.010]*	[0.016]
Access before 21* age 26-30	0.058	0.060	0.057	0.051	0.058	0.040
	[0.008]***	[0.008]***	[0.008]***	[0.014]***	[0.008]***	[0.014]***
Access before 21* age 31-35	0.044	0.045	0.042	0.019	0.033	0.043
	[0.008]***	[0.008]***	[0.008]***	[0.012]	[0.009]***	[0.014]***
Access before 21* age 36-40	0.030	0.030	0.025	0.012	0.012	0.031
	[0.008]***	[0.008]***	[0.008]***	[0.013]	[0.010]	[0.016]*
Access before 21* age 41-45	0.033	0.031	0.030	0.009	0.011	0.042
	[0.009]***	[0.009]***	[0.009]***	[0.013]	[0.010]	[0.017]**
Region & year effects	X	X	X	X	State & yr effects	X
Year of birth effects	X	X	X	X	X	X
Age categories	X	X	X	X	X	X
Region x year trends[a]		X	X	X	State -year trends	X
Observations	646702	646702	559612	126799	353476	154458
Adjusted R-squared	0.083	0.084	0.079	0.028	0.078	0.08

Notes: Synthetic birth cohorts by subtracting the reported age from the year of the survey. *denotes significant at 10%; ** at 5%; *** significant at 1%. Robust standard errors corrected for clustering on year of birth-state cells reported in brackets. [a] This is a set of dummy variables for *CPS* region interacted with a linear time trend.

Sample: Women ages 16-45 not in the military or inmates born between 1930 and 1970.
Source: March Supplements to the *Current Population Survey* 1964-2001.

Table 7. The aggregate and long-term effects of early access to the pill, counterfactual estimates 1960-1990

			1940	1950	1960	1965	1970	1980	1990
A. Observed participation rates									
	Participation of 16-65		0.242	0.306	0.362	0.435	0.489	0.590	0.676
	Participation of 16-30 (PT1)				0.356	0.392	0.465	0.617	0.717
	Participation of 16-45 (PT2)				0.356	0.414	0.473	0.625	0.734
B. No access counterfactual									
(1)	No access ages 16-30 (NA1)					0.392	0.463	0.599	0.682
	No access ages 16-45 (NA2)					0.414	0.472	0.613	0.716
(2)	Percentage points attributed to access								
	Women ages 16-30	(PT1-NA1)				0.000	0.002	0.018	0.035
	Women ages 16-45	(PT2-NA2)				0.000	0.001	0.011	0.018
(3)	Percent increase from t-1 to t attributed to access**								
	Women ages 16-30					0.007	0.022	0.121	0.350
	Women ages 16-45					0.003	0.016	0.074	0.162

Notes: The NA counterfactual in Panel B simulates the state of the world if no woman, from 1960 to the present, had gained legal access to the pill before her 21st birthday. Using the estimates obtained for the model in equation (3), I predict individual participation rates and average over the particular year and age group to obtain the estimates in line 1. **These values are calculated by dividing (PT1-NA1) by (NA1(t)-NA1(t-L)) where t is the year and L denotes either a 5 or 10 year date difference.

Source: Estimated effects based on 1964-2001 March Supplements to the *CPS*; observed participation rates based on 1940-1960 PUMS (Ruggles and Sobek, 1997).

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Appendix 1. Changes of age of first birth within marriage, age of first marriage, and age of first birth, by cohort and education

	Women married once before age 35					Women married once before age 35 with 16+ years of schooling				
	Mean	25th	50th	75th	Obs.	Mean	25th	50th	75th	Obs.
A. Age of first marriage- age of first birth										
1933-1935	1.80	1	1	2	7,399	2.45	1	2	3	812
1939-1940	1.56	1	1	2	10,115	2.41	1	2	3	1,421
1944-1945	1.85	1	1	3	11,459	2.82	1	2	4	2,212
1949-1950	2.47	0	2	3	9,200	3.50	1	3	5	1,699
1954-1955	2.79	0	2	4	2,788	3.27	2	3	5	89
1956-1960	2.94	0	2	5	1,150	--	--	--	--	--
B. Age of first marriage										
1933-1935	20.64	18	20	22	20,006	22.84	21	22	25	2,146
1939-1940	20.78	18	20	23	11,855	22.89	21	22	25	1,713
1944-1945	21.21	19	21	23	12,566	22.98	21	22	24	2,603
1949-1950	21.33	19	20	23	11,182	23.50	21	23	25	2,029
1954-1955	21.34	18	20	23	5,251	24.48	22	24	27	116
1956-1960	21.59	18	21	24	4,317	--	--	--	--	--
C. Age of first birth										
1933-1935	22.40	20	22	24	7,399	25.17	23	25	27	812
1939-1940	22.15	19	21	24	10,115	25.09	22	25	27	1,421
1944-1945	22.80	20	22	25	11,459	25.53	23	25	28	2,212
1949-1950	22.98	20	21	25	9,200	26.56	24	27	30	1,699
1954-1955	22.50	19	21	25	2,788	27.03	24	27	30	89
1956-1960	22.22	19	21	25	1,150	--	--	--	--	--

Notes: Age of first birth- age of first marriage is computed for each woman who reports both variables. Since fewer women over the age of 35 are observed for younger cohorts, this number will be biased downward as more women bear children after age 35. 16 or more years of schooling is computed using the highest grade of school attended.

Sample: All ever-married women who married once before the age of 35 and had a child by age 36 between the ages of 35 and 44. Source: June CPS 1975-1995.

Appendix 2. Estimated effect of early access on the age of first marriage

This table reports the marginal effect of early access (evaluated at the mean) on the age of first marriage using a probit specification of equation (1). In the left panel, the dependent variable is equal to 1 if a woman marries before age 22. Though the effect is negative in every specification, it is much smaller (both absolutely and relative to the mean) and is not statistically significant. The final two columns report a specification similar to that in Goldin and Katz (2002) and dependent variation (equal to 1 if a woman marries before age 23) for the June sample. The point-estimate in column 9 is quite comparable to Goldin and Katz (2002), though the standard errors are much larger.[47] Though age of first birth and age of first marriage are highly correlated, the effect on the age of first birth is much stronger for women with less than 16 years of education. I interpret the strong effect of the pill on age of first birth and the weaker effect of the pill on age of first marriage to imply that early access to oral contraception worked largely through the prevention of out-of-wedlock births, which rose quickly as a fraction of all births from the 1960s, as well as through the delay of first birth within marriage.

The marginal effect of early access to contraception on age of first marriage

Dependent variable	**1= First marriage by age 21**							**1=First marriage by age 22[c]**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	*All*	*White*	*Born 1940-1956*	*All*	*College[b]*	*College[b]*	*All*	*College[b]*
Mean dependent variable	*0.606*		*0.621*	*0.625*	*0.606*	*0.333*		*0.689*	*0.494*
Access before 21	-0.012		-0.022	-0.011	-0.015	-0.011			
	[0.022]		[0.022]	[0.022]	[0.021]	[0.020]			
Access by 18		0.013					0.003	-0.001	-0.035
		[0.019]					[0.022]	[0.016]	[0.024]
State effects	X	X	X	X	X	X	X	X	X
Year of birth effects	X	X	X	X	X	X	X	X	X
Age effects	X	X	X	X	X	X	X	X	X
State x year of birth trends[a]					X				
Observations	124563	124563	108215	100099	124563	19878	19878	124563	19878
Log likelihood	-81023.9	-81023.3	-69438.4	-64443.6	-80909.3	-12414.4	-12414.6	-74923.1	-13550.9
Average effect	-0.009	0.008	0.002	0.002	-0.011	-0.009	0.002	-0.001	-0.022

[47] These estimates can be reconciled with those in Goldin and Katz by considering the difference in samples. Goldin and Katz find delay in first marriage to be a significant factor among college graduates. For the sake of consistency, however, my sample restricts on the basis of marriage by age 34. This restriction may exclude many of their "zeros", the women delaying marriage and childbirth until much later in life. Moreover, although my sample size is much larger, my measure of educational attainment is imprecise, as I do not observe who actually graduated from college.

Notes: Synthetic birth cohorts by subtracting the reported age from the year of the survey. *denotes significant at 10%; ** at 5%; *** significant at 1%. Robust standard errors corrected for clustering on year of birth-state cells reported in brackets. The estimates presented in the table are the marginal effects evaluated at the mean. Averaging the effect over the entire distribution yields estimates in the last line of the table. [a] This is a set of dummy variables for state interacted with a linear trend in year of birth. [b] This sample is restricted to women who report at least grade 16 as the highest grade attended. [c] This definition is comparable to the definition used in Goldin and Katz (2002).

Sample: Ever-married women who married for the first time at or before age 35.
Source: June Supplements to the *Current Population Survey* 1976-1995.

Appendix 3. March *CPS* Descriptive Statistics

A. Sample averages by year, for all women over 16 to 65

	IPUMS			March Supplements to the *CPS*						
	1940	**1950**	**1960**	**1965**	**1970**	**1975**	**1980**	**1985**	**1990**	**2000**
In the labor force	0.242	0.306	0.362	0.435	0.489	0.527	0.590	0.632	0.676	0.710
Hours worked	41.36	38.39	35.87	35.84	34.69	34.16	34.68	35.37	36.92	37.55
Weeks worked	39.2	37.1	36.0	38. 3	39.2	40.3	40.6	42.1	44.5	47.0
Currently married	0.658	0.710	0.715	0.720	0.700	0.665	0.626	0.600	0.638	0.668
Never married	0.252	0.167	0.167	0.155	0.170	0.184	0.210	0.223	0.168	0.109
16 or more years of schooling	0.036	0.052	0.053	0.071	0.081	0.103	0.129	0.154	0.199	0.262
Age	36.3	37.6	38.5	38.2	37.7	37.3	37.0	37.1	39.4	45.1

B. Sample averages by year, for all women over 16 to 45

	IPUMS			March Supplements to the *CPS*						
	1940	**1950**	**1960**	**1965**	**1970**	**1975**	**1980**	**1985**	**1990**	**2000**
In the labor force	0.237	0.301	0.356	0.435	0.500	0.562	0.646	0.694	0.743	0.775
Hours worked	41.3	38.3	35.7	35.1	34.0	33.7	34.6	35.2	37.2	37.6
Weeks worked	39.2	36.9	35.9	36.2	37.1	38.7	39.5	41.2	44.3	46.9
Currently married	0.642	0.697	0.699	0.727	0.697	0.646	0.593	0.563	0.618	0.673
Never married	0.270	0.182	0.185	0.198	0.226	0.250	0.283	0.299	0.225	0.146
16 or more years of schooling	0.035	0.051	0.052	0.071	0.084	0.116	0.145	0.172	0.224	0.282
Age	35.8	37.2	37.9	30.0	29.2	28.7	28.7	29.4	32.0	37.6

Sample: Women not in the military or inmates.
Source: March Supplements to the *Current Population Survey* 1964-2001 in conjunction with 1940-1960 PUMS (Ruggles and Sobek, 1997)

Appendix 4. CPS Regions

1	ME NH VT MA RI
2	CT
3	NY
4	NJ
5	PA
6	OH
7	IN
8	IL
9	MI WI
10	MN IA MO ND SD NE KA
11	DE VA MD WV
12	DC
13	NC SC GA
14	FL
15	KY TN
16	AL MS
17	AR OK LA
18	TX
19	MT ID WY UT NV CO NM AZ
20	WA AK HI OR
21	CA