CHAPTER III

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

The movement she [Margaret Sanger] 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.[29]
--H.G. Wells, 1931 (*Time* 1998)

The release of *Enovid* in 1960, the first birth control pill, afforded U.S. women unprecedented freedom to plan childbearing and their careers. For college women, Goldin and Katz (2002) find that access to oral contraception led to a later age at first marriage and greater representation in non-traditional, professional occupations. But "the pill" may have had durable and far-reaching effects on women's labor market work within occupations and across levels of attainment.

Relatively little work in economics, either theoretical or empirical, has explicitly examined the impact of oral contraception on women's market work. Indeed, this line of research may seem relatively unimportant given the compendium of historical, cross-country, and scholarly research that suggests that birth control mattered very little.[30] Well before the advent of the pill, the U.S. witnessed steady increases in women's labor-force participation and dramatic swings in fertility. In light of this fact, Becker's conclusion summarizes a view held by many scholars: "the 'contraceptive revolution' … ushered in by the pill has probably not been a major cause of the sharp drop in fertility in recent decades" (1991, p. 143).

But even if one concedes that the pill affected fertility, recent quasi-experimental research suggests changes in the number of children in the household can explain remarkably little of the longer-term changes in women's paid work. Based on their estimates and other research summarized in section II, Angrist and Evans (1998, p. 474) conclude that since 1950, "the increase in female labor-force participation has been so large that declining fertility can explain only a small fraction of the overall change." While this study is not explicitly concerned with birth control, the results inform our understanding of the pill's potential effect: the pill should not account for large changes in women's labor-force participation, simply because changes in fertility cannot.

This chapter provides new evidence that technological improvements in birth control catalyzed changes in fertility and the extent and intensity of women's labor-force participation—my

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[29] Margaret Sanger (1879-1966) is known as the founder of the twentieth century's birth control movement.
[30] Becker (1991) provides a nice summary of this evidence in chapter 4 of his *Treatise on the Family*.

results do *not* challenge the validity of estimates of the relationship between the number of children and women's labor supply. Instead, I focus on the potential impact of the pill on women's labor-force participation through birth timing. The pill appears to have lowered the cost of investing in careers (by reducing the cost of delaying childbearing) and increased the expected returns to women's labor market investments. In this manner, the pill recast women's lifecycle labor-force participation decisions.

Two types of evidence support this hypothesis. First, dramatic cross-cohort changes in the distribution of age at first birth and age-specific labor-force participation rates correspond closely to the diffusion of the pill to younger, unmarried women. As more unmarried, women under age 21 gained access to the pill, fewer had children before age 22 and substantially more worked in the labor market during their twenties.

A second source of evidence is generated using state-by-year of birth variation in early legal access to the pill. This variation from 1960 to 1976 resulted from the extension of legal rights 18 to 20 year-olds in general, which specifically empowered unmarried women in this age range to consent to medical care and oral contraception. The history of these laws and issues of endogeneity are carefully considered. The legal changes appear unrelated to other state-level factors that might have altered the timing of first birth and age-specific labor-force participation (they constitute a valid experiment), but they are soundly related to 18 to 20 year-old women's use of oral contraceptives (they constitute a relevant experiment). These legal changes provide cross-state variation for women born in any given year between 1940 and 1956. I report the average "effect of the pill" as the mean difference in outcomes among women born in the same year but who I infer had different legal access to the pill before their 21st birthdays. Changes in state laws facilitate within year of birth comparisons and, therefore, greater confidence that the estimated effects are not driven by smoothly evolving, inter-temporal changes in norms about women's roles, the diffusion of labor saving household technology, or federal enforcement of anti-discrimination legislation.

Using retrospective fertility histories from the June *Current Population Surveys (CPS)*, I find that access to the pill before age 21 reduced the likelihood of becoming a mother by age 22 by 6 percentage points, or around 16 percent. Using the March *CPS*, early access to the pill increased the labor-force participation of women during their twenties by 5 to 6 percentage points. This accounts for roughly 25 percent of the increase in market work during the ages of highest fertility among women born in 1940 to 1956. This effect appears to operate through greater human capital acquisition, as more women with early access to the pill report that they were in school and fewer report housekeeping as the previous week's main activity. In contrast to a long literature

emphasizing the demand factors responsible for the growth in women's work, this analysis provides new evidence that a specific labor supply factor—the pill—enabled women to capitalize on new market labor market opportunities without sacrificing family.

The Theoretical and Empirical Effect of the Pill on Labor Supply

When considering the age-old history of contraception, one might be skeptical of the pill's true contribution. After all, the withdrawal method, or *coitus interruptus,* had been used well before it was popularized in the U.S. in Robert Dale Owen's 1831 pamphlet, *Moral Physiology*. Moreover, a number of contraceptive methods had been adopted before the pill was available (Brodie 1994).[31] But oral contraception differed from these traditional methods in three important ways.

First, the pill constituted the first 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 or knowledge of men or discomfort to either party during sex. In this sense, the pill transferred control of contraception, which had long resided with men, to the hands of those bearing the high physical and opportunity costs of childrearing.

Second, the decision to take the pill occurred at a time separate from the act of intercourse. Before the pill, the most effective and safest methods of contraception (notably, the "Dutch cap," or diaphragm, and condoms) imposed high *marginal* costs on both men and women during intimacy (Marks 2001). The pill improved upon these methods in that it divorced sex from the decision to prevent conception.

Third, the pill's reliability and effectiveness far exceeded that all other methods available in 1960. From the beginning, *Enovid*'s advocates promoted the pill as 99 percent effective.[32] Whereas most couples had accepted an element of risk as part of intercourse, the pill virtually eliminated concerns about unwanted pregnancy. Thus, the pill increased the effectiveness and reliability of contraception *per se* and increased the degree to which women could autonomously choose to

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[31] Misinformation and disagreement about the use and health effects of withdrawal during the nineteenth century, however, compromised its effectiveness (Brodie 1994). By 1924, the condom was the most popularly prescribed contraceptive (Tone 2001), although most doctors prescribed it for the prevention of venereal disease in men in extramarital and premarital relationships. It is unclear whether they were often used to prevent pregnancy. Margaret Sanger fought for the right to obtain them for contraceptive purposes. For a thorough history of the condom, see Brandt (1985) and Valdiserri (1988).

[32] 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 today at around 15 percent and the failure rates of today's modern diaphragms at around 16 percent. Less effective spermicides and materials imply that failure rates of these methods would have been much higher in 1960, and they are still high relative to the 5 percent failure rates of oral contraceptives.

prevent conception. In addition, contraception became a painless and much lower marginal cost endeavor, quite separate from the act of intercourse.

Previous research linking fertility and women's labor-supply

Despite the large potential advantages of oral contraception, relatively little work in economics has explicitly examined the impact of the pill on women's fertility or market work. This scarcity reflects the difficulty of examining the relationship between women's contraceptive effort and their work behavior in a quasi-experimental framework. Because the demand for children and women's labor-supply are jointly determined, separating the causal impact of children or, more broadly, women's fecundity on her market outcomes poses a difficult empirical problem.[33] Changes in women's work and childbearing outcomes may be the result of changes in supply side factors, such as labor saving technologies within the household or gender roles, or shifting demand factors, such as declining discrimination or the growth in sectors where more women worked.

The bulk of related empirical research examines the effect of variation in the number of children within a household. They rely on exogenous variation in the number of births due to idiosyncratic biological events: twinning (Bronars and Grogger 1997, Gangadharan and Rosenbloom 1999), miscarriages (Hotz, McElroy and Sanders 1997), or the idiosyncratic determination of the sex of children already born (Angrist and Evans 1998). Taken together, these studies' results suggest that declining family sizes account for very little of the post-war increases in women's labor supply. Angrist and Evans (1998, p. 474) conclude that the decline in childbearing beyond the second child among women ages 21 to 35 can account for roughly two percentage points (of the total 16.8 increase) in employment from 1970 to 1990. Based on these results, even if the pill reduced fertility, it should explain relatively little of the increase in women's market work.

But these results may understate the pill's impact. For instance, the identification strategies used require that a woman become pregnant (or choose not to prevent conception) to enter the "experiment". This may bias the results for two reasons. First, selection into motherhood at a given time may be correlated with the costs or benefits of becoming pregnant in the first place. Thus, the treatment effect of an additional child in select samples may misrepresent the effect in the larger population. Second, by conditioning on becoming pregnant these designs necessarily abstract from

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[33] A long theoretical tradition relates the *number* of children and their "quality" within a household to women's labor-force participation (cf. Becker 1960, Becker 1965, Willis 1973, Becker and Lewis 1973). In these models utility maximizing agents perfectly choose the number of children by weighing the time costs (in terms of a woman's foregone wages) and
resources of rearing children against the satisfaction they bring to the household. See Heckman and Killingsworth (1986) for a summary of the seminal theoretical and empirical research in this area.

the pill's potential impact on the timing of births. Oral contraception allowed couples to shift childbearing to later ages in order to maximize their consumption (Happel, Hill and Low 1984) or minimize the impact of childbearing on their careers (Moffitt 1984).[34] Because couples were fairly accurate at reaching their target fertility before the pill, reliable birth timing—especially the delay of unintended early pregnancies—may be among the pill's most important contributions.[35]

Another reason these studies may understate the role of the pill is that they examine changes in birth outcomes conditional upon being *on* a given career trajectory. If the availability of the pill affected women's expectations about their future wages, it may affect the selection of women into certain careers. Central to this argument, outlined in the next section, is that the pill reduced the price of making career investments and increased the expected returns to these investments.

The theoretical effect of early access to the pill on women's labor supply

The empirical exercise relies on variation in "early access to the pill", defined as legal access between ages 18 and 20. For this reason, this section highlights only the theoretical effect of a change in "early access" on women's lifecycle labor supply, although later access to the pill may have transformed women's labor-force participation as well.[36]

Early access potentially affected labor supply by altering both the costs of and the returns to pursing careers. On the cost side, the oral contraception reduced the price of delaying childbearing to acquire human capital (through formal schooling or on the job training). Before the pill, these costs may have included abstinence, the delay of marriage, or discomfort and effort at the time of intimacy. The pill also reduced the price of combining career and family. On the one hand, delaying marriage in order to pursue a career reduced the likelihood of finding a mate (Goldin and Katz 2002). On the other hand, finding a mate implied the risk of unwanted or ill-timed pregnancies that threatened to disrupt career investments. With the pill, delaying pregnancy to make investments in market skills

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[34] This may be because the opportunity cost of human capital accumulation early in life is relatively higher. On the other hand, it may also arise because older workers earn enough to hire nannies or take advantage of parental leave policies.

[35] The pill revolutionized a couple's ability to time childbearing. Primitive methods of birth control included delay of first marriage, longer breast-feeding to delay the return of menses, withdrawal and the reduced frequency of coitus. Modern alternatives were condoms and diaphragms. Couples anticipated relatively high failure rates of any of these methods and most employed a variety of methods to hit their target fertility. However, none—save abstinence—facilitated the deliberate timing of childbearing.

[36] While this focus may seem narrow, decisions at ages 18 to 20 may strongly influence young women's career paths. During these ages women make important family decisions. Roughly 50 percent of women born in 1940, the oldest cohort considered in my analysis, would have been married and approximately 40 percent would have conceived before age 21 (Source: Author's computations using the June *CPS*. Sample selection is described in subsequent sections.). Moreover, women also make important career choices during these ages. For instance, they decide whether to pursue college degrees, invest in job training and choose their occupations.

did not imply foregoing marriage or sex. As a result, women could invest in family *and* pursue their careers (Goldin 1995, 2004).

The pill also increased the expected lifetime benefits of pursuing careers. Before 1960 the inevitability of pregnancy (for sexually active or married women) and the associated time constraints and market withdrawal reduced for young women the expected returns to pursuing careers. Reliable contraception transformed the timing and number of births from a stochastic into a virtually deterministic process. The ability to determine the number of withdrawals and their timing served to raise the expected lifetime returns to early human capital investments and careers that required greater time investments.[37] Therefore, seemingly small changes in incentives during the ages critical to career and family formation may compound into large differences in women's career trajectories: early access to the pill may have transformed women's market work across the lifecycle.[38]

To be explicit, the argument here is that early access to the pill may have affected women's work behavior even *without* affecting completed fertility. The effects of early access on the total number of children may be weak if detectable at all.[39] After the pill was introduced in 1960, almost all women born later than 1940 obtained access to the pill at age 21 or by legal emancipation by marriage. Assuming the pill does not affect women's target fertility, those with early access should be equally likely to achieve their target number of children (unless it is zero). This is because an ill-timed, early pregnancy could be offset by using contraception (and having one less child) at ages beyond 21.[40]

In summary, early access should affect women's labor-force participation by facilitating the delay childbearing and encouraging early human capital investment (in terms of formal education and early market work) and different career choices. These decisions may have resulted in higher

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[37] Weiss (1986) outlines why expected career interruptions reduce pre-interruption career investments. Mincer and Polachek (1974) cite the expectation of career interruptions as a reason why, even after accounting for past interruptions, earnings differ between men and women later in their careers.

[38] No theoretical model of which that I am aware explicitly considers the effects of early access to the pill on fertility timing or labor-force participation. However, the intuition laid out here is consistent with the theoretical models in which rational agents make choices at the outset of their adult lives, given their preferences and abilities, that effectively determine the sequence of childbearing, labor-force participation, and wage outcomes. Hotz, Klerman and Willis (1997) provide a good summary of this body of research.

[39] Some theoretical models of lifecycle fertility unambiguously predict that wealth constant changes in the prices of preventing childbirth or working in any given period will affect the lifecycle timing of births but not necessarily completed fertility (MaCurdy 1980, Heckman and MaCurdy 1980, Walker 1995). See Hotz, Klerman and Willis (1997, p. 309-317) for an thorough summary of these models.

[40] A deeper and more difficult question is whether early access (not the pill itself) should alter women's target number of children. Becker argues that birth control should not affect the demand for children (1991, p. 143), for which an empirical proxy is completed fertility. Despite a number of theoretical formulations that assume consumption from earnings and a child are substitutes, the answer to this question turns on the degree of substitution and is largely an empirical problem.

rates of labor-force participation across the lifecycle.

Identifying the Effect of the Pill using Variation in State Laws

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 restrictive state laws were enforced for other forms of contraception, legal bans were much more effective in limiting access to oral contraception.[41] Obtaining the pill required a prescription from a licensed physician and sale by a licensed pharmacist. Violations could be punished with heavy fines, jail time, and possibly loss of one's license to practice professionally (Garrow 1994). Thus, state restrictions that required a young woman be herself a legal adult (over the age of 20 in most states), married (most states considered a woman "legally emancipated" if she was married or a mother), or a guardian's legal consent effectively regulated her access to the pill.[42]

The History of Liberalization of Access

During the 1960s and 1970s, the age of legal consent was lowered at different times in different states for reasons largely unrelated to issues surrounding contraception or women's rights. Most of the legal changes, in fact, were due either to the expanding rights of legal minors or to changes in the definition of legal "minority".

Changes in the rights of legal minors occurred as judicial decisions held that minors could provide consent for medical care and began 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 was "intelligent and mature enough to understand the nature and consequences of the treatment" (Paul et al. 1976, p. 16). After the pill was introduced, these decisions effectively gave latitude to physicians in prescribing oral contraception to young women without consulting their parents (Paul et al. 1974).

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[41] Effective regulation of condoms, for instance, required only that distributors (often gas station clerks) check the ages or marital status of those making purchases. A substantial amount of evidence suggests that the illicit distribution of non-hazardous contraceptives over the counter or in vending machines was common before being legal (cf. Garrow 1994, p. 188).

[42] One final feature of legislative history makes state legal changes a particularly apt quasi-experiment. The Comstock Act, which was passed by Congress in 1873, declared the inter-state transport or mailing of contraceptives a federal offense. Although the *One Package* U.S. Supreme Court ruling struck down federal bans on the inter-state shipping of contraception to licensed physicians in 1936, federal law continued to prohibit individuals from obtaining oral contraceptives by mail from out of state. Individuals seeking to obtain the pill would have had to drive across state lines regularly to refill prescriptions (and regular visits to a doctor). In contrast, one diaphragm could be used for around to two years. Therefore, state laws tended to be a more relevant constraint for access to the pill than for access to other types of contraceptives.

As judicial precedents extended the legal rights of minors, the war in Vietnam catalyzed changes in the legal definition of adulthood. Under federal law, young men could be drafted for Vietnam at age 18 but were not entitled to vote until age 21. The discrepancy in rights and obligations of young men reached national prominence during the 1968 national presidential election. This debate culminated in the ratification of the Twenty-sixth Amendment to the U.S. Constitution in 1971, which lowered the federal voting age to 18. At the state level, legislatures began extending the privileges and responsibilities of legal adulthood to eighteen-year old men and women as well.[43] These rights included buying and drinking liquor (in some, not all, states); signing contracts; suing and being sued; making wills; inheriting property; holding public office; serving as jurors, policemen and firemen; marrying and divorcing without parental consent; qualifying for welfare benefits; and attending X-rated movies. Although extending the right to obtain contraception to younger women had little, if anything, to do with these legislative changes, a lower age of majority empowered them to consent to medical treatment and, by extension, obtain the pill.[44] (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, p. 17).[45]

During the same period, equally visible and controversial issues had been decided in the nation's courts. Beginning with the *Griswold* decision in 1965, the U.S. Supreme Court had struck down Connecticut's ban on the use and distribution of contraceptives and declared a realm of "procreative privacy" for married individuals.[46] In the 1976 *Planned Parenthood of Central Missouri v. Danforth* decision, the U.S. Supreme Court ruled that states lacked a "compelling interest" in using age to restrict the distribution of contraception. This decision, by no act of popular opinion, rendered the higher age of legal majority inapplicable to the prescription of oral contraception in the remaining states. I have collected the earliest laws—whether statutes or judicial precedent—that would have empowered young, unmarried women below the age of 21 to obtain the pill. To the extent that these legal changes do not capture women's ability to pay for or physician's

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[43] Several 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.

[44] 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.

[45] Missouri lowered the age of majority to 18 in 1975, but this was for the purpose of signing contracts only. The 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).

[46] Procreative privacy was extended to apply to unmarried individuals in *Eisenstadt v. Baird*, in which 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.

willingness to prescribe the pill, they will be poor measures of *de facto* access.

Testing the Validity of Liberalization as a Natural Experiment

By in large, 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.[47] In order to use these laws to make valid inferences about the pill, the timing of liberalization should not reflect differences in other ex ante characteristics. I test the correlation of the time that elapsed until liberalization with a number of 1960 state-level characteristics. While an imperfect test, this sheds light on possible pre-existing characteristics that may have led to a reduction in fertility or an increase in labor supply.

Table 1 reports the cross-state, population-weighted regressions of the timing of liberalization on 1960 state characteristics. The dependent variable is the number of years from the time the pill was released in 1960 to the year when the state law liberalized. State-level characteristics are computed for each of the fifty states and the District of Columbia from the 1960 Public Use Microsample (Ruggles et al. 2004), the Survey of Churches and Church Membership (National Council of Churches of Christ 1956), and the record of Casualties in Southeast Asia (National Archives 1997).[48]

Panel A displays the point-estimates for a variety of 1960 demographic characteristics with the timing of liberalization. Panel B examines the correlation of social factors with the timing of liberalization, including the historic mean age at first marriage and completed fertility of older cohorts, poverty rates, fraction Catholic, and the number Vietnam casualties from 1965-1970 as a fraction of the state population.[49] Panel C and D report the correlation of liberalization and labor market characteristics for men and women, such as mean educational attainment, age-specific participation and employment rates, and wages.

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[47] The handful of states that passed comprehensive family planning laws is the exception. By 1972 four states (Georgia, Florida, Nebraska, Wyoming) and the District of Columbia had explicitly liberalized age and marital restrictions for the purpose of 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 without liability.

[48] 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

[49] 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) are proxies for state fertility norms. Similarly, the age at first marriage is an indicator of marital and fertility norms. The number of casualties in Vietnam is intended to proxy for state-level political pressure to change the age of majority. The date range 1965-1970 is chosen since the 26th Amendment was ratified in 1971. The results are not very sensitive to changes in the dates.

Table 1. 1960 State-Level Predictors of Liberalization

A. Demographic characteristics

	Fraction of population			Fraction of women in age group			
	living on farm	foreign born	Black	15-21	22-30	31-45	South
Point estimate	1.85	10.86	-1.34	-4.33	24.15	-19.41	0.379
S.e.	(5.70)	(6.59)	(3.95)	(12.21)	(18.33)	(21.89)	(1.16)
R^2	0.002	0.030	0.001	0.003	0.053	0.010	0.003

B. Social characteristics

	Women born 1910-19		Women born 1920-29		Fraction of the population		
	Age of first marriage	Children ever born	Age of first marriage	Children ever born	In poverty	Catholic	Casualty in Vietnam
Point estimate	0.124	-20.39	-0.465	-23.374	-0.595	5.24	-2.56
S.e.	(0.277)	(12.38)	(0.705)	(15.340)	(4.153)	(2.11)	(1.38)
R^2	0.001	0.055	0.010	0.046	0.000	0.035	0.022

C. Labor markets for men

		Men ages 22-30			Men ages 31-45		
	Education	In labor force	Unem-ployment	Wages	In labor force	Unemployment	Wages
Point estimate	0.038	-4.149	-10.863	-0.001	1.857	-43.7	0
S.e.	(0.583)	(13.64)	(13.34)	(0.001)	(15.54)	(50.69)	(0.001)
R^2	0.000	0.001	0.008	0.026	0.000	0.035	0.000

D.Labor markets for women

		Women ages 22-30			Women ages 31-45		
	Education	In labor force	Unem-ployment	Wages	In labor force	Unemployment	Wages
Point estimate	-0.111	-1.15	1.81	0.001	6.39	2.70	-0.001
S.e.	(0.750)	(7.37)	(3.98)	(0.001)	(10.13)	(16.53)	(0.002)
R^2	0.000	0.000	0.002	0.011	0.009	0.000	0.007

The dependent variable is the year the state enacted the law – 1960. Regressors are state-level weighted aggregates. The point estimates are obtained by regressing the dependent variable on each characteristic individually. Results from regressions including all the variables in a given panel do not alter the results. All regressions are weighted by state population in 1960, although the unweighted estimates are quite similar. Robust standard errors are in parenthesis. In Panel B, 49 states are included for the regression with fraction Catholic 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). Data on Vietnam casualties obtained from the National Archives (1997).

The striking feature of Table 1 is that none of the characteristics is statistically significant, with the exception of the fraction Catholic and the Vietnam casualty rate. The relationship between the number of Vietnam casualties and liberalization confirms empirically the political history: greater casualty rates increased political pressure to change the age of majority. The relationship of the timing of liberalization with 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 underlined directly liberalized access to contraception, liberalization through family planning statutes tended to occur in states with the smallest Catholic constituencies. By omitting family planning states, the point-estimate on Catholic becomes much smaller and is no longer statistically significant.[50] (Later in the analysis I will drop all but the age of majority states to test the robustness of my results.)

Evidence for the Relevance of Liberalization as a Natural Experiment

Taken together there is little evidence that liberalization in the age of majority states is related to pre-existing social and demographic characteristics other than political pressure due to Vietnam. It seems fair to conclude that idiosyncratic differences in the regional judiciary, the regional politics of minors' rights and the war in Vietnam engendered considerable variation in the timing of adoption across states. But because liberalization was not directly related to the pill *per se*, this section summarizes outside evidence that these laws, in fact, mattered for actual pill access.

Because only one dataset in existence, the National Survey of Young Women (1971), appears to contain both state of residence and contraceptive use among unmarried teenage women, the only direct evidence on the relevance of these laws can be found in Goldin and Katz (2002). They find that, in states with liberalized access, pill use was 36 to 40 percent greater among unmarried 17 to 19 year old women. Other qualitative evidence supports this claim as well. The journal of *Family Planning Perspectives* provided regular updates from 1968 to 1978 on changes in the age of majority and mature minor doctrines to inform physicians and family planning organizations of the permissiveness of often ambiguous state law. Moreover, the Department of Health Education and Welfare commissioned a study in 1971 of differences in state laws regarding fertility control (DHEW 1974). Sub-sections of this report on young women's access to contraception regularly begin with references to the legal age of majority in a particular state. It is also clear from these reports that state

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[50] 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.

welfare agencies and public health departments developed rules regarding contraceptive access based upon legal restrictions in a given state. Finally, Goldin and Katz (2002) note that university clinics paid careful attention to changes in state laws that permitted the prescription of oral contraceptives to their female students. Their analysis finds that these laws were strongly related to change in the age of first marriage among college graduate women. In summary, liberalized laws functioned as catalysts for private physicians as well as governmental and nonprofit agencies to expand the provision of contraceptive services to younger, unmarried women.

To bolster further the argument that liberalized access to the pill is, indeed, generating the observed labor market effects through the mechanism of fertility control, I provide additional evidence that early access affected the age at first birth.[51]

Time-Series Evidence

Both aggregate and cohort-specific time-series suggest that the fertility behavior of young women changed rapidly as the pill diffused. As depicted in Figure 1, from 1960, when the pill was introduced, until around 1976, the year when the U.S. Supreme Court struck down the remaining state restrictions on distributing contraceptives to minors in *Danforth*, first birth rates among 18 to 20 year olds fell rapidly. Recall that this group of women would not have had legal access to the pill before liberalization. In contrast, first birth rates among 15 to 17 year olds, individuals who were too young to benefit from most legal changes, underwent almost no discernible changes. Some of these changes, however, may be due to the aging of the high fertility cohorts that gave birth to the Baby Boom.

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[51] Goldin and Katz (2002) emphasize the importance of the pill in reducing the costs of delaying marriage. This paper, however, abstracts from the decision to marry and focuses on the effect of early access to the pill on age at first birth for two reasons. First, early access to oral contraception severed the decision to marry from the decision to have sex or bear children, so early access to the pill may have countervailing effects upon the age at first marriage. While the pill allowed women to delay marriage without delaying sex, it also eliminated the need to delay marriage as more women could defer childbearing in wedlock. While the latter effect may be weak, the age at first birth should be non-decreasing in pill access and should provide a cleaner measure of the effect of early access on fertility risk and labor supply. Second, the rapid increase in illegitimate births since 1960 (see, for instance, Carter et al. 2003), suggests that changes in the age at first birth may better capture changes in young women's fertility in and outside of marriage.
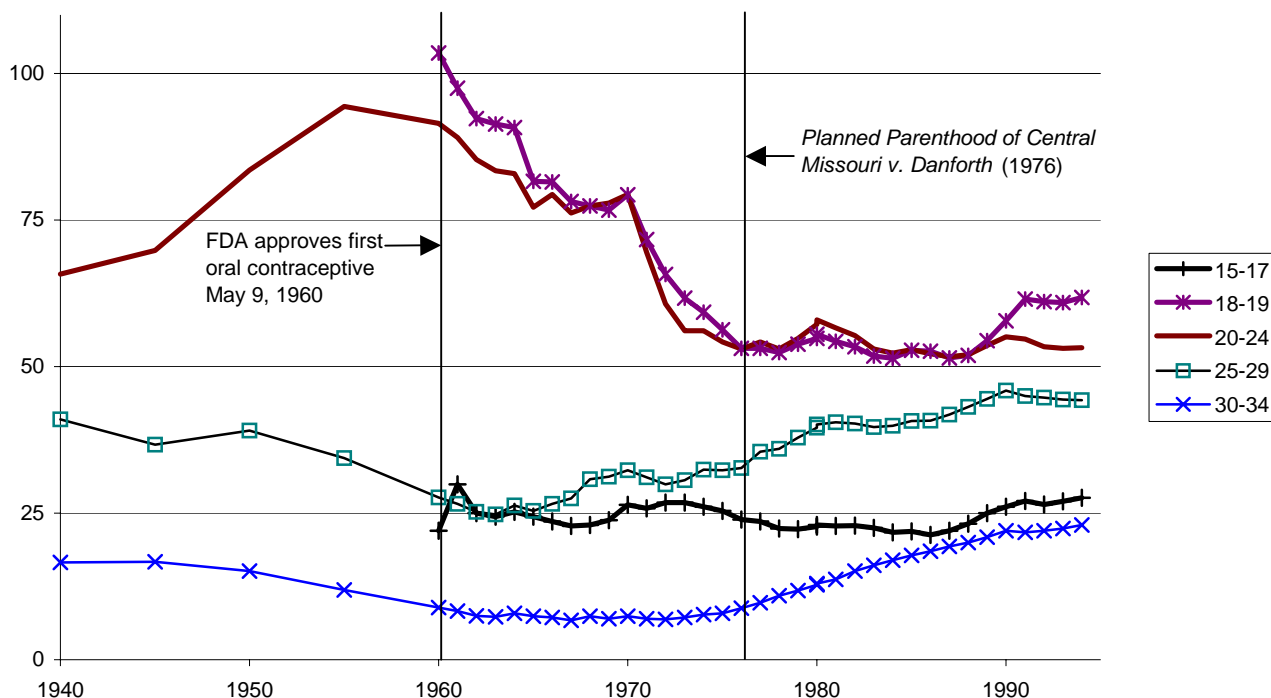
Figure 1. White First Birth Rates by Age of Mother, 1940-1995

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).

A more compelling correspondence in timing can be found when examining changes by cohort in the distribution of age at first birth. Using the 1977-1995 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.[52] 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. Among the women born in 1940, almost 50 percent were married and 42 percent were mothers before the pill was released in 1960. In contrast, women born after 1955 could obtain the pill legally by age 18 in almost every state. As the pill diffuses to younger and unmarried women, the largest changes in age at first birth occurred for women between the ages of 18 and 22 who would have benefited most from liberalization.[53]

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[52] 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 57.
[53] 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 percent

Figure 2. The Distribution of Age at First Birth by Cohort, 1933 to 1960

The figure plots the fraction of white women (vertical axis) with a first birth at a particular age (horizontal axis). Synthetic birth cohorts are 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 35 and had a birth at or before age 34 and were at least 35 years old at the time of the survey. Source: June *CPS* 1977-1995.

The stark differences *between* the cohorts without early access to contraception (1933 to 1940) and those with early access (1955 to 1960) contrast sharply with the similarity of the distributions *within* these groups.[54] 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) supports the case that the large changes in the age at first birth between the cohorts of 1940 and 1956 may be related to liberalization.

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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 born to younger 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.

[54] 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 the 1940 and 1955 distributions.

Time-series changes in the nature and level of women's labor-force involvement mirror the shifts in first births. While little evidence of the pill's effect may be found in aggregate time series, large shifts occur in age-specific labor-force participation profiles in Figure 3. Two distinctive periods in women's lifecycle labor-force participation across the Twentieth Century correspond closely to the pre-pill and post-pill eras. For women born 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. Beginning with the cohorts born after 1940 and ending with those born around 1955, the "fertility dip" in labor-force participation had completely disappeared. 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.[55]
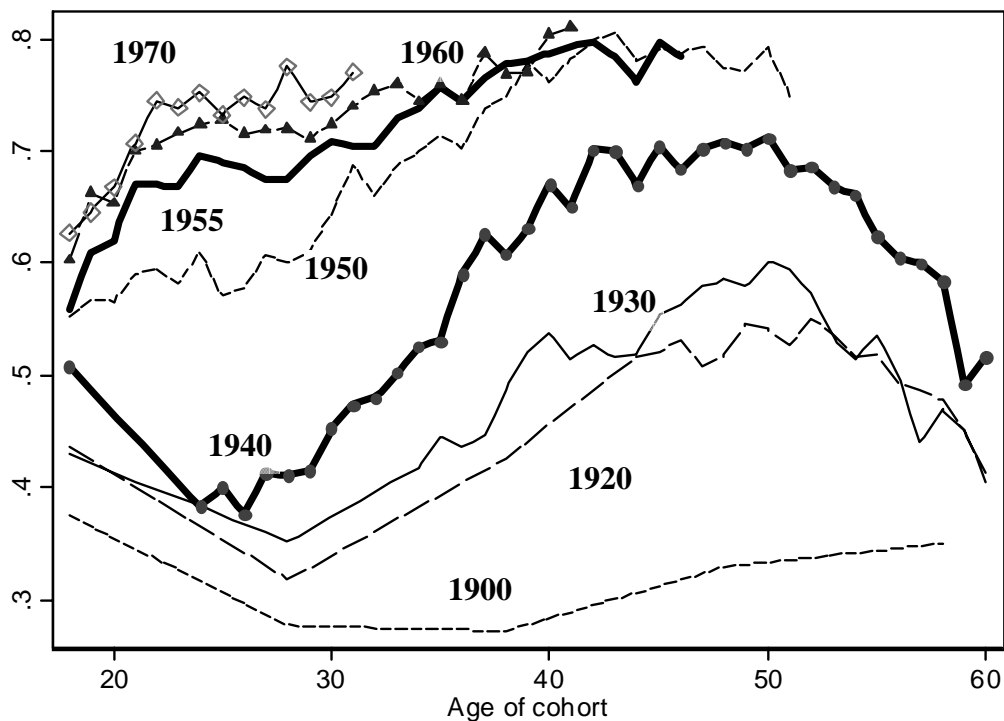

Figure 3. Labor-Force Participation Rates by Synthetic Cohort and Age, 1900 to 1970

Pre-1964 data are averaged over cohorts. For instance, the participation rate for women ages 14 to 19 in 1950 is plotted in this figure as the cohort of 1930 at age 18. Data after 1963 represent participation rates for computed single year of birth cohorts at a reported age. Synthetic birth cohorts are computed by subtracting the reported age from the year of the survey. Bold lines depict the 1940 and 1955 cohorts. The March sample includes all women not in military or inmates ages 16-60. Source: 1964-2001 March *CPS*; for years before 1964, data is taken from Smith and Ward (1985, Table 1).

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[55] Smith and Ward (1985, p. S65) also note that for women born after 1950, there is no observable employment decline over the childbearing years, although they do not explicitly relate this to the pill. Goldin (2002, Figure 4, Figure 5) notes that these trends are borne out for married women as well, though the advances of college graduates appears to have begun earlier.

This brief episode constitutes the most rapid, inter-cohort shift in young women's labor market work during the Twentieth Century. More interesting, however, is that the timing and the nature of this inter-cohort shift corresponds closely to the rapid changes in the age at first birth and the diffusion of contraception among younger, unmarried women.

The Empirical Case for Contraception: Data and Estimation Results

Annual legal changes provide intra-cohort variation for testing the hypothesized relationship between oral contraception, the delay in first births, and the growth in women's labor supply. In conjunction with state legal changes, I use the June and March Supplements to the *CPS* to examine the effect of earlier legal access to oral contraception using within-cohort variation in legal access. The *CPS* consist of repeated cross-sections of detailed information on individual characteristics including retrospective information on the age at first birth and completed fertility (June Supplements);[56] current employment information on earnings and participation including hours worked in the reference week, and weeks worked in the previous calendar year (March Supplements); and geographic information (both surveys).

In order to make the June sample comparable across the years 1977 to 1995, I restrict the sample used in the estimation to include ever-married women ages 35 or older, who were born between 1935 and 1960. I additionally require that women in the estimation had married once by age 35.[57] The March sample is restricted to individuals born between 1935 and 1960 (so that the dataset is tractable), who are between 16 and 44 years old for the outcome in question, and who are not working in agriculture (for sample statistics, see appendix D).[58]

Although these data provide advantages over the census information because they facilitate comparisons by year of birth (and age in the March surveys), several features of the data tend to

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

[57] 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 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 the analysis for both cohorts. Age 35 is chosen because the youngest cohort in this analysis was born in 1960 and the last year for which I have information on age of first birth is 1995. Thus, I only observe women from this young cohort who had married at or before age 35.

[58] 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 and these women would have been 45 years old in 2001, the average treatment effect can only be obtained up to age 45.

minimize the estimated effects. First, unenforced legislation, cross-state travel to obtain the pill, and the inability to afford contraception despite its legal availability should bias my results toward zero. Second, the *CPS* provides neither information on a woman's state of birth nor the location of her residence around her twenty-first birthday. It is, therefore, necessary to assume that changes in legal access in the observed place of residence were relevant to the individual's decisions before age 21.[59] As time passes women are less likely to be observed in the same state as they resided at age 21 and, thus, measurement error in early access should be larger for older women.[60] If the coding error is random, this is a case of classic measurement error and the estimates should be attenuated. However, systematic migration would induce upward bias in the estimates.

To examine this possibility, I use the birth state and state of residence information in the PUMS data (Ruggles et al. 2004). For native U.S. women born from 1940 to 1956 for women ages 25 to 30, I construct a dependent variable that is equal to 1 if a woman moved from her state of birth (0 otherwise). For women without early access in their birth states, I regress the binary mobility measure on whether or not the women would have had early access to the pill in her new state of residence. I am interested in whether states, where a woman would have had early access, were more desirable for unobservable reasons than states where that early access wasn't the case? I find no evidence to support this conclusion. Women without early access are equally likely to move to states where they wouldn't have had early access as to states where they had early access. I further ask the question, of the migrants without early access, were women who moved to early access states more educated than those who didn't? Or did they work more? Again, I find no evidence for this conclusion. Women who moved to states where they would have had legal access were *not* more likely to have finished college or work in the labor force at ages 25 to 30. Therefore, it appears that systematic migration cannot account for the central results.

The Impact of Early Access on Age at First Birth

Cross-state heterogeneity in legal access by year of birth facilitates estimation of the pill's within-cohort effect on the fertility of young women. Because early legal access, ELA, varies by year of birth, y, and state of residence, s, for women born from 1940 to 1956, the unit of observation is at

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[59] For example, a 35-year-old woman observed in Massachusetts in 1985 would have been 24 when the Massachusetts liberalized access. Therefore, I code the individual as *without* early access.

[60] Specifically, the estimated long-term effects of early access are based upon the sample of non-movers. If non-movers are women who do not take advantage of labor market opportunities in other states (due to individual preferences, financial constraints or marital status), the long-term effects of early access will also be underestimated. The inability to take account of women's migration compounds the downward bias introduced by inaccurately characterizing access at older ages.

the year of birth-state level (for sample statistics, see appendix E). Using the fertility information in the June CPS, I estimate equations of the following general form

$$Y_{cs} = \alpha_0 + \alpha_1 ELA_{cs} + f_s + g_c + \varepsilon_{cs} \tag{1}$$

where f_s and g_c denote a set of state and individual year of birth dummy variables. In some specifications, linear state-specific time trends are included. These should capture the evolution of unobserved factors within states such as gradual changes in the fraction Catholic or growth in the women's movement. ELA is the fraction of women born in year c that would have had access to oral contraception before age 21 (potentially benefited from liberalization) in their current state of residence, s. The point estimate of interest, α_1, captures within-cohort deviations in the age at first birth from state trends that are correlated with changes in early access to the pill.

Table 2 reports the effects of early access on various measures of fertility timing. In column 1 and 2, the dependent variable is fraction of women that gave birth before age 22 (or conceived by age 21). Women with first births at or after age 22 or those reporting no live births are coded as zeros. The estimates are remarkably robust across specifications and imply a 16 percent reduction in the likelihood of an early birth (-0.065/0.406). In results not reported in Table 2, I find that the effect of ELA is just as strong among women with less than a college education. This suggests that the pill potentially affects the labor-force outcomes of non-college graduates as well, a group not examined by Goldin and Katz (2002).

But are these effects causal? Potential threats to this interpretation are unobservable forces that caused the laws to change that were related to changes in women's fertility outcomes, though they had nothing to do with the pill. One plausible alternate hypothesis is that legal access to abortion that expanded rapidly from 1970 to 1973 may be confounded with the impact of the pill. Controlling for legal access to abortion in column 3, however, does not change the magnitude or statistical significance of the point estimates.

One way to assess concerns about causation more generally is to examine the correlation of earlier access with fertility behavior that should not be affected. Since the laws considered in the analysis generally lowered the age of access to 18, first births by age 19 (conceptions by age 18) should not be affected by liberalization. Column 4 provides evidence that the fraction of first births by age 19 was not affected by early legal access.

Table 2. The Effect of Early Access to Contraception on Fertility

	Fraction with a first birth before 22			Before 19[b]	Before 35[c]	Mean children ever born
Access to pill before 21	-0.065 (0.034)	-0.066 (0.033)	-0.065 (0.034)	-0.018 (0.019)	-0.003 (0.011)	-0.012 (0.024)
Access to legal abortion			-0.007 (0.031)			
State effects	X	X	X	X	X	X
Year of birth effects	X	X	X	X	X	X
State x time trends[a]		X	X	X	X	X
Observations	1304	1304	1304	1304	1304	1304
Adjusted R^2	0.33	0.39	0.39	0.24	0.26	0.26

The marginal effects presented in the table are evaluated at the mean. Synthetic birth cohorts are computed by using either the reported year of birth or, when this is missing, subtracting the reported age in years from the year of the survey. Robust standard errors corrected for clustering on state cells are reported in parenthesis. All computations are unweighted. [a] This is a set of dummy variables for state interacted with a linear trend in year of birth. [b] The dependent variable is equal to one for individuals that had a first birth before age 19. [c] The dependent variable is equal to one for individuals that had a first birth before age 35. Sample: Ever-married women older than 34, who married for the first time at or before age 35 and who were born between 1935 and 1960. Source: June CPS 1977-1995 (not including the years 1978, 1984, 1989, 1991, 1993, and 1994 when the June CPS or information on first birth was not collected).

Another important unobservable factor is the strength of the women's movement. This may have affected judicial or legislative decisions which, in turn, affected women's preferences for children and careers. Observing a strong effect on measures of completed childbearing could suggest that the timing of liberalization is correlated with other unobservable factors influencing career and childbearing. On the other hand, early access to the pill might have influenced women's choice over the number of children and type of career. Columns 5 and 6 examine the impact of earlier access on the probability of becoming a mother by age 35 and family size by the same age. The effect of early access to contraception on becoming a mother is very close to zero. Moreover, the effect of early access on the total number of children is slightly negative, though small and never marginally significant.[61] These results support the claim that earlier access is correlated with changes in the behavior of young women around age twenty, but not in the behavior of women too young to benefit. Moreover, these early behavior changes do not appear to reflect underlying changes in women's desire to be mothers or in their target number of children. Therefore, early legal access to the pill

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[61] In results not reported here, the magnitude and statistical significance of these estimates are robust to the inclusion of linear state time trends and within the subgroup of women reporting less than 16 years of education.

appears to have operated by allowing women to time their childbearing better rather than by reducing the number of children born.

The Effect of Early Access on Lifecycle Labor Supply

The employment data in the March CPS provide annual work information from 1964 to 2001 and allow age-specific labor-force participation effects to be separated from the secular growth in employment across cohorts. One important shortcoming of these data is that information on smaller states is grouped into regions from 1968 to 1976. For this reason, the labor-force analysis groups individual states into 21 regions (some are individual states) in order to capture consistent geographic units.[62] With this limitation, I redefine ELA in CPS region r in year t for a woman j years beyond her twentieth birthday as

$$ELA_{r,t,j} = [P_{r,t-j}]^{-1} \sum_{s \in r} P_{s,t-j} \times 1(Law_{s,t-j}) \tag{2}$$

where $P_{r, t-j}$ denotes the population of region r in year t-j and $P_{s,t-j}$ denotes the population of state s in the year t-j, the date of the woman's twentieth birthday; and 1( ) is an indicator function equal to one if state s had a liberal access law in year t-j.[63] Thus, this variable 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 twenty-first birthday, assuming that she had not moved (see appendix E for sample statistics). This probabilistic approach introduces more error in the measurement of ELA.[64]

The empirical specification allows ELA to alter the shape of women's labor-force participation profiles by interacting the variable with two-year, categorical age dummies denoted by vector A. I treat the unit of observation as age-year of birth-region of residence cells and I estimate the following equation using annual cross-sectional data from the 1964 to 2001 March CPS,

$$Y_{car} = \beta_0 + A \beta_1 + ELA_{cs} \times A \beta_2 + f_r + g_c + h_{c+a} + \varepsilon_{car} \tag{3}$$

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[62] The grouped regions are New England (less Connecticut); Michigan and Wisconsin; Minnesota, Iowa, Montana, North and South Dakota, Nebraska and Kansas; Delaware, Virginia, West Virginia and Maryland; North and South Carolina and Georgia; Kentucky and Tennessee; Alabama and Mississippi; Arkansas, Oklahoma and Louisiana, Missouri, Idaho, Wyoming, Utah, Nevada, Colorado, New Mexico and Arizona; and Washington, Oregon, Alaska and Hawaii. All other states are individually identified.

[63] Notice, that *ELA* varies only by birth year and region, since t and j uniquely define a birth cohort. Year of birth is *t-j*-20.

[64] 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 state fixed effects rather than *CPS* region fixed effects does not substantially alter the estimates or their significance.

where c refers to the year of birth, r to the CPS region, and a to the age of the cohort of observed. The fixed-effects f_r, g_c and h_{c+a} denote dummies for CPS region, year of birth (cohort), and year of observation respectively. Again, region-specific linear time trends capture gradually evolving unobservable factors may have affected labor-force outcomes. In this specification, each element of β_1 captures the average age-group specific, within-cohort, within-state impact of early legal access to the pill on labor market outcomes.[65]

Table 3 reports the effects of early access on labor-force participation at the extensive margin. A woman is coded as being in the labor-force if she worked or looked for work for most of the week prior to the survey. The estimates in columns 1 and 2 (the base specification with and without region-specific linear time trends, respectively) are quite similar. With early legal access, the participation rates of women ages 25 to 30 were slightly more than four percentage points higher (an increase of roughly eight percent) and almost three percentage points higher at ages 31 to 32.[66] At ages beyond 39, early legal access implies a significant reduction in labor-force participation. The transformation of lifecycle participation rates is consistent with the notion that women with early legal access re-sequence childbearing and career: as women without early legal access reenter the labor market after their children have grown, women who delayed childbearing are less likely to be working. Despite the fact that measurement error in ELA grows as women age, there is still a significant negative effect well into the forties.

In columns 3 I limit the analysis to age of majority states only, the states with legal changes most easily argued exogenous. Despite the great reduction in the sample size, both the magnitudes and the significance of the results changes little. Taken together, the estimates suggest that early access to contraception had a strong impact on women's decisions to work during their late twenties—ages usually associated with high fertility. [67]

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[65] Alternatively, equation (3) was estimated with a quadratic in age interacted with *Access* but the results are quite similar.

[66] In results not reported here, the estimates for whites and women reporting at least 16 as the highest grade attended exhibit a similar pattern.

[67] In results not reported here, I investigate the effect of Vietnam on women's outcomes independent of changes in the age of majority. It is plausible that coming of age during Vietnam rather than pill access gave women reason to defer marriage, childbearing or to invest more in formal market work. After allowing for an age-specific effect of the number of casualties in Vietnam as a fraction of the region's population in a given year (available from 1965 to 1976), the effect of early access for 26 to 35 year olds falls slightly, but remains statistically significant.

Table 3. The Effect of Early Access to Contraception on Labor-Force Participation

Early legal access to pill x dummy variable for ages	Fraction in the labor force			Controls for education	Controls for # kids
19-20	-0.074	-0.070	-0.074	-0.057	-0.053
	(0.010)	(0.010)	(0.025)	(0.007)	(0.005)
21-22	-0.056	-0.053	-0.079	-0.047	-0.040
	(0.010)	(0.009)	(0.021)	(0.008)	(0.007)
23-24	-0.006	-0.002	0.008	-0.019	-0.011
	(0.011)	(0.010)	(0.015)	(0.009)	(0.008)
25-26	0.04	0.043	0.059	0.008	0.014
	(0.013)	(0.012)	(0.023)	(0.008)	(0.007)
27-28	0.045	0.048	0.05	0.017	0.020
	(0.012)	(0.011)	(0.018)	(0.009)	(0.008)
29-30	0.044	0.046	0.041	0.008	0.010
	(0.010)	(0.010)	(0.020)	(0.006)	(0.006)
31-32	0.026	0.028	0.021	0.001	0.000
	(0.009)	(0.008)	(0.013)	(0.007)	(0.007)
33-34	0.01	0.012	0.004	-0.004	-0.006
	(0.008)	(0.008)	(0.019)	(0.007)	(0.008)
35-36	-0.002	-0.001	-0.016	-0.011	-0.015
	(0.008)	(0.008)	(0.011)	(0.007)	(0.006)
37-38	-0.003	-0.003	-0.001	-0.009	-0.018
	(0.006)	(0.005)	(0.007)	(0.007)	(0.008)
39-40	-0.025	-0.025	-0.023	-0.015	-0.030
	(0.007)	(0.007)	(0.014)	(0.008)*	(0.009)
41-42	-0.019	-0.019	-0.02	-0.013	-0.023
	(0.009)*	(0.009)	(0.014)	(0.017)	(0.011)
43-44	-0.032	-0.033	-0.005	-0.002	-0.033
	(0.011)	(0.011)	(0.025)	(0.016)	(0.012)
Constant	0.492	0.49	0.585	0.128	0.644
	(0.016)	(0.016)	(0.020)	(0.023)	(0.018)
CPS region f.e.	X	X	X	X	X
CPS region x trend [a]		X	X	X	X
Age of majority only			X	X	X
Additional controls				X	X
Observations	16468	16468	4709	616206	700739
Adjusted R^2	0.656	0.661	0.600	0.086	0.098

Synthetic birth cohorts are computed by subtracting the reported age from the year of the survey. The dependent variable is the fraction of women working any hours or looked for a job in the previous calendar week. All computations are unweighted. Robust standard errors corrected for clustering on state cells are reported in parenthesis. [a] This is a set of dummy variables for CPS region interacted with a linear time trend. [b] Controls for education include eight dummy variables. [c] Controls for the number of kids include dummy variables for 1 to 10. Sample: Women ages 16-45 not in the military or inmates born 1935 to 1960. Source: 1964-2001 March CPS .

Table 4. The Effect of Early Access to Contraception Education and Home Production

Early legal access to pill x dummy variable for ages	Fraction in school	Fraction keeping house	Fraction currently married
19-20	0.041	0.030	0.004
	(0.008)	(0.010)	(0.011)
21-22	0.063	-0.006	-0.074
	(0.007)	(0.011)	(0.009)
23-24	0.049	-0.042	-0.103
	(0.007)	(0.012)	(0.013)
25-26	0.039	-0.066	-0.072
	(0.007)	(0.016)	(0.011)
27-28	0.029	-0.059	-0.051
	(0.007)	(0.015)	(0.010)
29-30	0.022	-0.047	-0.024
	(0.007)	(0.016)	(0.011)
31-32	0.012	-0.019	-0.010
	(0.007)	(0.012)	(0.008)
33-34	0.004	0.006	0.000
	(0.005)	(0.011)	(0.008)
35-36			0.009
			(0.010)
37-38			0.009
			(0.012)
39-40			0.002
			(0.011)
41-42			0.001
			(0.013)
43-44			0.000
			(0.008)
Constant	0.031	0.402	0.417
	(0.012)	(0.019)	(0.016)
Year of birth & year fixed effects	X	X	X
CPS region f.e.	X	X	X
Observations	12373	12373	16468
Adjusted R^2	0.869	0.695	0.838

Synthetic birth cohorts are computed by subtracting the reported age from the year of the survey. Dependent variables, "Fraction in school" and "Fraction keeping house", are computed from the CPS question "What was…doing for most of last week?" for the years 1964 to 1988. The dependent variable "Fraction currently married" is computed from the respondent's current marital status. Robust standard errors corrected for clustering on state cells are reported in parenthesis. All computations are unweighted. Sample: Women ages 16-45 not in the military or inmates born 1935-1960. Source: 1964-2001 March CPS.

The absence of an effect at ages under 23 is consistent with Goldin and Katz's argument about greater human capital investment. This argument is bolstered by the fact that conditioning on reported educational attainment (column 5) or the number of children currently in the household (column 6), substantially reduces or eliminates the effect of early legal access in the late twenties. Thus, it appears that much of observed effect of early legal access to the pill operates through women's early education and childbearing choices—not net of them.

Other evidence for this claim makes use of the CPS question about what the respondent was doing "most of last week" for individuals not in the labor force (in the 1963-1988 surveys only). Two possible answers were "in school" or "keeping house". Table 4 (column 1) suggests that women with early legal access were much more likely to be "in school" during their twenties, especially at ages 19 to 24. Moreover, they were significantly less likely to report "keeping house" at ages 23 to 30 (column 2) as a reason for not working for pay. Finally, significantly fewer women in the early access group report being "currently married" (column 3) at ages 21 to 30, although there is no observable difference in current marital status beyond age 30.

Changes in the intensity of labor supply underscore the possibility of changing career investment in the form of market work as well. Table 5 presents the estimates from the base specification for the dependent variables mean hours worked in the reference week, mean weeks worked in the previous calendar year and the product of the two that provides a proxy for annual hours worked.[68] The pattern of results is quite similar to those for labor-force participation with the exception that women were significantly more likely to have worked more intensively until age 34. The impact on the three measures is largest at ages 27 to 28, when women with early legal access appear to be working approximately 134 more annual hours (or more than 3 full-time weeks per year). This suggests that cohorts with early legal access acquired roughly 300 additional hours of actual experience by age 34, even though more of them were in the labor force. In addition, early legal access is associated significant reductions in intensity at ages under 22 and at ages beyond 39.

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[68] Before 1976, the March *CPS* only reports intervals for weeks worked. I use the values suggested by Unicon in the estimation before 1976.

Table 5. The Effect of Early Access to Contraception on Intensity of Market Work

Early legal access to pill x dummy variable for ages	Hours worked	Weeks worked	Annual hours
19-20	-3.206	-2.775	-128.284
	(0.513)	(0.447)	(21.90)
21-22	-2.122	-1.853	-79.761
	(0.334)	(0.402)	(13.93)
23-24	0.516	0.583	35.946
	(0.499)	(0.506)	(22.54)
25-26	2.384	2.454	127.204
	(0.461)	(0.610)	(22.15)
27-28	2.704	2.991	134.67
	(0.405)	(0.630)	(21.43)
29-30	2.481	2.906	119.921
	(0.386)	(0.628)	(21.42)
31-32	1.825	2.044	87.71
	(0.384)	(0.490)	(19.46)
33-34	0.888	1.057	44.963
	(0.356)	(0.463)	(18.88)
35-36	0.089	0.143	2.017
	(0.371)	(0.504)	(18.88)
37-38	-0.145	-0.341	-13.231
	(0.311)	(0.389)	(18.43)
39-40	-0.701	-1.451	-38.279
	(0.334)	(0.375)	(17.61)
41-42	-1.221	-1.648	-71.3
	(0.448)	(0.491)	(22.79)
43-44	-1.056	-1.745	-46.16
	(0.371)	(0.469)	(18.89)
Constant	12.035	14.596	253.05
	(0.768)	(0.794)	(35.77)
Year of birth & year fixed effects	X	X	X
CPS region f.e.	X	X	X
Additional controls			
Observations	16468	16468	16468
Adjusted R^2	0.724	0.721	0.769

Since the March CPS reports only intervals for weeks worked for years before 1976, the Unicon suggested values are used in place of actual values. Robust standard errors corrected for clustering on state cells are reported in parenthesis. [b] Controls for education include eight dummy variables. [c] Controls for the number of kids include dummy variables for 1 to 10. Sample: Women ages 16-45 not in the military or inmates born 1935-1960. Source: 1964-2001 March CPS.

The Effect of Early Access on Aggregate Labor-Force Participation

Because these estimates are conditional upon being in the workforce, selection makes them difficult to interpret. On the one hand, negative selection into the labor-force at the extensive margin 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). On the other hand, early access may have affected the selection in a less straight forward manner, as women reorganized the traditional sequence of childbearing and work to suit the pursuit of family and career. While the CPS data do not allow me to weigh the relative importance of each of these explanations, more research on the changing age structure of women's employment and childbearing and its relationship to contraception is certainly warranted.

The deeper question at stake in this analysis is how to gauge the pill's long-term effects on the U.S. labor market. Even though the analysis makes use of variation in access below age 21, important life decisions are shaped during these early years. Changes in women's decisions at younger ages, that influence their willingness to work for pay and their labor-force attachment, may affect aggregate labor-force participation rates years later. While I cannot make inferences about the effects of the pill at ages over 21, they provide information on importance of the pill for younger women's participation growth. Using the estimates for the baseline model in equation (3), I generate counterfactual estimates of labor-force participation by assuming that no state had a law that permitted legal access for unmarried women under 21.[69] While changes in early access accounted for very little of the increase in women's work during the 1960s (because few women had it), the impact of early access to contraception grew as more women entered and remained in the workforce. Moreover, small percentage point changes were compounded by the size of the Baby Boom cohorts (recall, early legal access benefited women born from 1940 to 1956). For women ages 16 to 30, the impact of early legal access can account for roughly 12 percent of the increase during the 1970s and 35 percent during the 1980s. For women ages 16 to 44, early access to the pill accounted for seven and 16 percent of the increases in labor-force participation during the 1970s and 1980s, respectively.

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[69] In practice, I simply replace *ELA* with zero and predict the baseline model for labor-force participation at the extensive margin. This allows me to capture how changing cohort size or year specific effects (like *Roe v. Wade* or the U.S. business cycle), state trends and the secular increase in cohort participation would have affected women's participation in the absence of liberalization. I then attribute the difference in the simulated and the observed participation rates to early access. Alternately, I compute the full access counterfactual by setting *Access* equal to one 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 estimates 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 percent higher in 1965 and 14 percent higher in 1970 for women ages 18 to 30.

As with any counterfactual computation, these numbers should be viewed cautiously. They are, of course, "partial equilibrium" comparisons. By using variation in early access to the pill, this analysis does not take into account of the effects of the pill on other variables. For instance, they do not account for the possibility that greater access to the pill substantially altered national norms governing women's labor market roles—these effects, for instance, would be captured in the year of birth fixed effects in the model. Moreover, these estimates will not capture changes in women's behavior who gained access to the pill after age 21, because everyone in my sample was "treated" at that age. Nevertheless, the evidence suggests that even small changes in the age of access to the pill can account for economically significant short and long run shifts in women's labor-force participation.

Conclusion: More Power to the Pill

Economists have been hesitant to credit fertility control with shaping women's post-war labor-force participation. Instead, studies have emphasized real wage growth for women (Smith and Ward 1985, 1989; Goldin 1990), falling discrimination including the elimination of marriage bars (Goldin 1988, 1990), rising demand for labor in the clerical sector (Goldin 1984, Smith and Ward 1985) and, more recently, the growing demand for highly-skilled workers and professionals (Black and Juhn 2000, Welch 2000).

In contrast, this analysis suggests that one important labor supply factor—access to oral contraception—had large and permanent effects on young women's fertility and labor market patterns. Using exogenous changes in laws restricting the age at which young women could consent to medical care, I find that cohorts with earlier legal access to the pill had fewer births before age 21 and worked more for pay during their twenties and early thirties. The estimates are surprisingly strong and quite robust, despite the fact that data limitations—including the inability to observe directly whether laws were enforced, whether women obtained the pill across state lines or could not afford it, or whether they moved between states after age 21—tend to attenuate the estimates. To the extent that the pill allowed women to prevent births at later ages, these estimates may additionally understate the role of oral contraception in the longer-term evolution of the labor market. Still, the conservative counterfactuals implied by my results, which only account for the pill's effect through the ability to time childbearing, suggest an economically significant role for the pill in the post-1960 period.

In light of these findings, further study of the pill may shed light on its importance 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.