The Power of the Pill for the Next Generation

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Abstract

In this paper we ask how the diffusion of oral contraception to young unmarried women affected the number and parental characteristics of children born to these women. Using census data, we document that access to the pill led to falling short-term fertility rates for young women. We further document the success of the pill in reducing unwanted pregnancies by providing evidence that increased availability of the pill led to fewer abortions among young women. We also find significant effects of pill access at a young age on completed lifetime fertility at both the intensive and extensive margins. Finally, we examine how the pill affected average maternal characteristics. Our results indicate that the pill's effects on the average mother were sometimes very different from the pill's effects on the average woman. Further, we find that early pill access led to an increase in the share of children whose mothers were married, were college-educated, had professional occupations, and who were able to "have it all:" marriage, children, and a professional career.

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I. Introduction

A growing literature documents that the diffusion of oral contraception had profound impacts on the outcomes of young women in the 1960s and 1970s. Starting with Goldin and Katz (2002) and continuing with Bailey (2006), Goldin (2006), and Miller (2005), researchers have found that increased access to the pill by young unmarried women in the 1960s and 1970s affected the marital, educational, and labor market outcomes of these women later in life.

Surprisingly, however, researchers have paid little attention to the effect of oral contraception's diffusion on the children born to these women. Even the basic question of whether gaining access to the pill had an immediate effect on the number of children born to young women remains disputed. Bailey (2006) provides some limited evidence that providing young women access to the pill lowered their fertility in the short run. Guldi (2005), however, finds that access to the pill has little or no immediate effect on fertility for young women. Arcidiacono, Kwaja, and Ouyang (2005) use post-diffusion data to argue that increased access to contraception leads to higher pregnancy rates among teenagers.

In contrast, a large amount of work has been done on the effects of access to abortion on fertility and children's outcomes. These studies suggest that increased access to fertility control for women in the form of legal abortion reduces fertility in the short term (Levine et al., 1999; Angrist and Evans, 1999) and long term (Ananat, Gruber, and Levine, forthcoming) and alters the cohorts who are born to these women on important margins such as likelihood of childhood poverty (Gruber, Levine, and Staiger, 1999), total crime committed (Donohue and Levitt, 2001; Foote and Goetz, 2005), drug use (Charles and Stephens, 2006), and college graduation (Ananat et al., 2006). But these results beg the question of whether the introduction of the pill—the other major fertility control innovation in recent history and the most popular form of contraception in the United States—had similar effects on fertility and selection, and whether the pill serves as a substitute or complement to abortion when both are available.

In this paper, we ask how the diffusion of oral contraception to young unmarried women affected the number and parental characteristics of children born to these women, both in the short and long run. To identify the impact of access to oral contraceptives, we exploit a very detailed dataset on state laws

governing young women's access to the pill, provided by Guldi (2005), which allows for a stronger identification strategy than what has typically been used in previous work. Further, we introduce a new regression specification that allows us to control for omitted characteristics that vary within states in a given year, helping us address the critical concern that the passage of these laws is endogenous to other changes in state environments.

Using data from the 1980 Census, we find that, over the period 1960 to 1980, extending access to the pill to younger women in a given year leads to lower birth rates for those women in the next year. The effect is robust and both statistically and economically significant; access to the pill lowers young women's birth rates by about 10 percent.

To further substantiate the role of the pill in preventing unwanted pregnancies, we then ask whether young women consider oral contraception and abortion as substitutable fertility control technologies. Specifically, we investigate the relationship between legal access to the pill and abortion rates. This relationship is important to understanding the pill's role as a fertility technology, but it is also important in its own right; there is a highly contentious policy debate over the relationship between oral contraception and abortion. For abortion data we exploit two different datasets that employ different methods of information gathering and cover different time periods and legal regimes. In both datasets we find a negative relationship between legal access to the pill and abortions. The relationship is economically significant and generally statistically significant.

We then expand our analysis to examine the lifecycle effects of gaining access to the pill at a young age. Using data from the 1970, 1980, and 1990 Census, we find that providing young women access to the pill caused them to experience permanent decreases in lifetime fertility—that is, avoided births were not completely made up later. Moreover, early access to the pill affected women's life paths in other ways, including lowering the chance that they would marry or divorce, and increasing their choice of careers that require large up-front investments (a phenomenon that had previously been identified among college-graduate women in Goldin and Katz, 2002). The combination of fertility and other decisions interacted, so that the characteristics of the representative *mother* changed differently from

the characteristics of the average *woman*—while the average woman was less likely to be married, the average child's mother was *more* likely to be married, and also more likely to complete college. Thus, one effect of expanding access to the pill was that a larger proportion of children were born to mothers who "had it all"—marriage, family, and career.

These results have a number of important implications. They extend past research on the impacts of the pill by verifying the positive impacts of the pill on women and comparing these effects to those found for mothers. Going beyond past research, these results show that this large-scale diffusion of contraception did not increase abortion or birth rates for young women, a matter of policy debate today. In addition, these results contradict past speculation that access to the pill was responsible for the increase in single motherhood observed in the 1970s. We discuss these implications more in the conclusions.

The remainder of the paper is as follows. Section II provides a brief history of the pill and a discussion of its predicted impacts on fertility decisions. Section III describes empirical estimates of the effects of access to the pill on short-term fertility and the relationship between pill access and abortion. Section IV examines the lifecycle effects of early access to the pill on women's lifetime fertility and marital and human capital decisions, and investigates the implications for child living circumstances. Section V concludes.

II. A Brief History of the Pill

This section provides a brief overview of the development and diffusion of oral contraception to young unmarried women in the United States. The discussion here draws on Goldin and Katz (2002) and Bailey (2006); see also Asbell (1995) and Watkins (1998) for more on the history of the pill.

Oral contraception was first approved for use by the Food and Drug Administration in 1960. While it quickly became the most common form of contraception for married women under 30,[1] the pill remained an unusual form of birth control for unmarried young women during the 1960s. By 1976, however, the pill had become the most popular form of contraception among never-married women ages

[1] See Table II-3 in Westoff and Ryder (1977) for data on contraception use by married women in 1965 and 1970.

15 to 19. About 73 percent of ever-contracepting never-married women ages 18 to 19 in 1976 had used the pill; by contrast in 1971 the equivalent figure was 36.3 percent (Zelnik and Kantner, 1977, Table 10).

This surge in the use of the pill by younger, never-married women coincided with legal changes that granted easier access in obtaining the pill. For much of the 1960s, a woman had to be a legal adult (usually age 21 or over), married, pregnant, or already a mother in order to obtain oral contraception without a guardian's consent. Legal constraints dating back to the federal Comstock Act of 1873 made obtaining a prescription for the pill by mail from out of state infeasible. Also, unlike many other forms of contraception, access to the pill required a prescription from a physician and sale by a pharmacist, making laws restricting birth control more likely to be enforced than laws restricting some other forms of contraception (Bailey, 2006).

The diffusion of the pill occurred primarily through two channels. First, some states changed the age of majority, thereby changing the age at which a woman was no longer a minor and could obtain the pill without a guardian's consent. Second, some states expanded the legal rights of minors, so that women who had not yet reached the age of majority could obtain the pill more easily. Guldi (2005), Bailey (2006), and Goldin and Katz (2002) all make the argument that these changes stemmed in part from the passage of the 26th amendment to the U.S. Constitution, which was itself passed in part because of debates related to the legal rights of men being drafted for the Vietnam War.

Passage of these laws does not appear to have been systematically related to changes in social attitudes regarding women's sexuality or other phenomena that may themselves influence childbearing behaviors. Bailey (2006) argues that the laws that changed access to birth control "were enacted at *different* levels of government and targeted *different* policy outcomes. Only indirectly did most of these laws extend access to oral contraception. Precisely this heterogeneity makes it difficult to come up with an alternative omitted variable [that would confound estimated effects of the pill's diffusion]" (p. 308, italics in original). Bailey also presents evidence that variation in timing of state laws is unrelated to

almost all observable characteristics across states.[2] In our empirical analysis, we further address concerns about potential endogeneity of these law changes by exploiting the specific age allowing initial access to the pill for each state and year (from Guldi, 2005), which allows us to introduce controls not only for observed but also for unobserved heterogeneity within states and years.

III. Empirical Evidence on the Pill's Effects on Short-Term Fertility

III.A. *The Pill and Birth Rates*

In this section we examine whether access to the pill affected the likelihood that a young woman gave birth. The data we use for this investigation come from the 1980 Census 5-percent public use microdata.[3] Our sample consists of state-year of birth cohort averages of the number of children born between 1961 and 1980 and observed in the Census with their mothers.[4] This sample restriction allows us to construct the pill access a child's mother had at the time of conception by exploiting the state of birth, year of birth, and age of the mother at birth.

The unit of observation is all women of a certain age, in a given state and year. The sample includes women ages 15 to 20 between the years 1960 and 1979. We assume a child born in a given year and state was conceived in the same state in the year prior to birth. We have redone our estimates using birthplace of the mother, rather than birthplace of the child, as the relevant location for determining birth rates and pill access; those results are extremely close to the results shown here. We estimate the population of women of a given age in a given state and year based on a woman's state of birth, rather

[2] Characteristics she tests include the fraction of the population that is black, the fraction of the population living on a farm, whether a state is located in the South, the fraction of women that are ages 15-21, 22-30, or 31-45, mean education for women in a state, the fraction of the population in poverty, a state's casualty rate in Vietnam, the fraction of households with a radio or with various other appliances, the fraction of men ages 22-30 in the labor force, the fraction of women ages 22-30 in the labor force, and various other controls for economic, household, social, and demographic characteristics.

[3] Some prior work has used data from Vital Statistics, rather than census data. The key advantage of the census is that it allows us to exploit identification of access to the pill for teenage women of different ages in a given state and year, which is crucial to the estimation strategy we use. Furthermore, in Section IV we use the census to study changes in parental characteristics from pill exposure; most such characteristics are not available in the Vital Statistics data.

[4] Guldi's law information is not available for some states in the earliest years. In such situations we used law data from Bailey (2006); discussions with the two authors confirmed the comparability of the law data used in the two papers.

than by current residence in 1980. This allows us to avoid any potential endogeneity created by selective migration of women across time, although re-estimating the results with women's population based on current residence yields results very close to those shown here.

Table 1 presents some basic summary statistics from the 1980 Census. The means are divided between cohorts of women based on their access to the pill. The table shows that women with access to the pill have similar means in terms of the average mother's age, the birth rate, and the socioeconomic characteristics of their children (as measured in the 1980 Census). However, there is some difference in state-level means; women with access to the pill live in states and years with higher unemployment, crime, income, and percent nonwhite. This is partly due to the fact that access to the pill is more common among later cohorts of women and that these state characteristics are trending upward over time.

With these data, we replicate Gruber, Levine, and Staiger's (1999) approach to measuring the effect of abortion access on births. We modify that approach to incorporate age-specific as well as state- and year-specific variation in access to the pill, and estimate the equation

$$Births = \delta Access + \beta X + \theta_s + \theta_y + T + \phi_{age} + \varepsilon$$

where *Births* represents either the birth rate (in logs or levels) or the log of the number of children born to women of a given age in a given state and year; *Access* is an indicator for whether women *of that age* in that state had legal access to the pill in the prior year (the presumed year of conception)[5]; X is a set of year-specific state-level controls including the insured unemployment rate, the crime rate, the percent of the population nonwhite, and per-capita personal income; θ_s is a set of state dummies; θ_y is a set of year dummies; T is a set of state linear and quadratic trends; and ϕ_{age} is a set of mother age dummies.[6] As mentioned in the introduction, there is no consensus in the economics literature on whether δ will be positive, zero, or negative.

[5] This specification depends on a relationship between pill access and fertility, which itself depends on a relationship between pill access and pill use. We do not have information on pill use in the Census. However, Goldin and Katz (2002) show that access to the pill did lead to higher use.

[6] The regressions on log number of births also include the number of women in a cohort, in logs, as a regressor.

The results from estimating this equation are shown in columns 1, 2, and 3 of Table 2. The results include all the regressors described above; residuals are clustered by state and corrected for heteroskedasticity and each observation is weighted by number of women.[7] The results confirm that policies expanding access to the pill had the immediate result of decreasing births among the affected cohorts. The levels estimate suggests that the birth rate declines by about one birth for every hundred women. The results are qualitatively similar using the log number of births or the log of the birth rate, although the former is only marginally significant. Estimates using the log of the birth rate, which are more precise, suggest a highly significant 11.6 percent decline in the birth rate among women who are granted access to the pill.

We further exploit the additional quasi-experimental variation provided by age-specific changes in pill access by including state-year fixed effects as controls. These variables absorb any phenomena in a state in a given year that would have affected all teenagers similarly, in essence allowing the change in childbearing within a state among teenagers whose access to the pill did not change in a given year to serve as a baseline for those teenagers within the state whose access did change that year. This regression specification provides results that are more robust to unobserved time-variant state heterogeneity, and provides more confidence that our results isolate the effect of pill access on teenage childbearing.

We estimate the equation

$$Births = \delta Access + \beta X + \theta_{sy} + \phi_{age} + \varepsilon$$

which is identical to the first equation estimated, except that the state dummies, the year dummies, the state linear and quadratic trends, and the state's annual socioeconomic characteristics have been replaced with a set of state-by-year dummies, θ_{sy}.

The results from estimating this equation, shown in columns 4, 5, and 6 of Table 2, provide further confirmation that policies expanding access to the pill had the immediate result of decreasing births among the affected cohorts. In all cases the result is more negative and more significant than under

[7] Using the number of children born to a cohort as weights produces similar estimates.

the more restrictive Gruber, Levine, and Staiger (1999) specification. Again the results are qualitatively similar using the log number of births or the birth rate (in levels or logs) and again estimates using the log of the birth rate are larger and more precise.

III.B. *The Pill and Abortion*

To further document the role of the pill as a technology for preventing unwanted pregnancies, we next examine whether access to the pill affected the likelihood that a young unmarried woman had an abortion. This is an important question in its own right, as there is a contentious policy debate over the relationship between oral contraception and abortion.[8] While some research outside of economics has considered the relationship between contraception, especially emergency contraception, and abortion, this work is highly inconclusive and often focuses on trends in contraception use, rather than exogenous changes in the availability of contraception.[9] We know of no work in any discipline which exploits birth control's diffusion to examine its relationship with abortion.

The first source of data for this investigation is the 1971 National Survey of Young Women, or NSYW, a nationally representative sample of 4,611 women ages 15 to 19 living in households and college dormitories in the United States. The 1971 NSYW is the only dataset of which we are aware that provides information on the contraception and abortion histories of a national sample of young women, with state-level identifiers, prior to 1973's *Roe v. Wade*. While the NSYW's sample is somewhat small,[10] its early date and the retrospective data it provides are crucial given the timing of pill diffusion. By the time that abortion was becoming legalized and organizations such as the CDC began to collect systematic annual data on abortion, diffusion had already occurred in most states.

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[8] See Shorto (2006) for a non-academic account of the debate regarding contraception and abortion.

[9] For example, Glasier et al. (2004) argue that advanced provision of contraception does not reduce abortion rates, while Marston and Cleland (2003) examine trends in contraceptive use over time and conclude that increased contraception use results in reduced abortion incidence.

[10] The NSYW was also conducted in two other years, but these other surveys do not include information on a respondent's location, making it impossible to know a respondent's legal access to birth control.

The fact that abortion remained illegal for virtually all respondents in the NSYW might lead respondents to give dishonest answers about having had an abortion.[11] The investigators did, however, make significant efforts to elicit honest answers, and in follow-up research (Kantner and Zelnik, 1983) concluded that respondents had been "remarkably candid in their answers." Moreover, so long as misreports of abortion do not vary systematically with access to the pill, it is unlikely that underreporting will lead us to find a spurious effect between pill diffusion and the use of abortion.

We estimate, using the NSYW, the equation

$$Abortion = \delta Access + \beta X + \theta_s + \phi_{age} + \varepsilon$$

where *Abortion* is a dummy that equals unity if an individual has ever had an abortion; *Access* is a measure of a respondent's access to the pill; *X* is a set of individual controls including an indicator for whether the respondent is white, an indicator for whether the respondent is Catholic, an index for church attendance, an index for the importance of church in the respondent's life (the regressions use dummy variables for each value in the two indices), an indicator for a rural location ("rural" means that a respondent does not live in an SMSA), an indicator for a low-income household, dummies for years of education, an indicator for whether the respondent is currently a student, and a set of age-by-census-region interactions; θ_s is a set of state dummies; and ϕ_{age} is a set of age dummies. Table 3 provides selected summary statistics for the NSYW data.

Zelnik and Kantner (1977) report that the median age of first intercourse among sexually experienced never-married women in 1971 is 16.5. Reflecting that, our preferred measure of access to the pill is a lagged indicator for whether a woman had access at the age of 16, based on the woman's current age and state of residence. We prefer lagged access to current access because, since many state laws become effective at 18 or older, access at the time of the survey may not accurately reflect a women's access to the pill at the time she was making the decision to become sexually active. We have also

[11] The only women in the sample who could legally get abortions were those age 19 in Alaska, those living in Hawaii, or those ages 18 or 19 in Washington. None of the abortions reported in the NSYW were by women who could legally have an abortion in their state.

considered other lagged access measures, such as access by age 17 or 15. The effect of access at ages 17 or 15 is not well-defined, however, because almost no states change their laws to allow access at exactly these ages—thus these variables are simply proxies for access by ages 18 or 16. Therefore we define early access as access by 16. In some specifications, we also consider the additional effect of current (i.e., at the time of the survey) access.

Among the sample of sexually-active women ages 16 and older (for whom access at 16 is a relevant control) there are 66 women (nearly 5 percent of the sample) who report ever having an abortion. This is a reasonably high number considering that for most of these women abortion was illegal at the time of the survey. Table 4 shows the distribution of abortion responses by state and by whether or not the woman reporting the abortion had access to the pill at the age of 16.

Table 5 reports linear probability regressions from the NSYW. Residuals are clustered by state and corrected for heteroskedasticity. All regressions include state dummies, women's age dummies and women's age-by-region dummies, but the first column does not include any other controls. The regression shows that among 16 to 19 year-old sexually active women in 1971, having had access to the pill since age 16 significantly lowers the likelihood of ever having had an abortion. The second column adds the other right-and side controls; the main coefficient is very similar and the other covariates are generally insignificant.

The third column adds a dummy for current access to the pill which is wrong-signed and insignificant. It is not surprising that access to the pill over the past few years is more strongly related to reductions in the likelihood of ever having an abortion than is current access, since abortion history is a result of cumulative behavior. The last column restricts the sample to sexually active women over age 16 in the sample. Focusing on older women makes sense because these women are likely to have been sexually active for longer, making their answers to questions more meaningful. The regression results are stronger for this group, which is not surprising. [12]

[12] One concern when interpreting these results is that the sample of women who are sexually active may itself be influenced by access to the pill. If that is the case, then although results in Table 5 are meaningful, they would not

III.C. *Results from the Center for Disease Control*

One may be concerned about the small number of abortions in the NSYW that are driving the results. We attempt to verify our findings from the NSYW using a second and totally different dataset compiled by the Center for Disease Control (CDC) for the years 1974-1979. The CDC only collected data on legal abortions, so we focus on the period for which abortion in all states was legal. The CDC data includes abortion information from all 50 states, New York City, and the District of Columbia during this time period, but only 41 states report information on abortions for those 15 to 19 (these states are listed under Table 6). Some states do not report data every year (in the typical year data are available from about 37 states).

The advantages of the CDC data are that they do not rely on self-reported data from a small survey and that they are available for multiple years. The disadvantages of the CDC data are that they are only available at a time when most states had already diffused oral contraception, and that they are for 15 to 19 year-olds but cannot be broken down within this age group. This final drawback makes the previous specification, which relies on variation in access between teenagers within a state and year, infeasible. We consequently estimate equations of the following form:

$$Abortion = \delta Access + \beta X + \theta_s + \theta_y + \varepsilon$$

where *Abortion* equals either (a) the number of abortions by women ages 15 to 19, (b) the ratio of abortions to live births for women ages 15 to 19, in logs, or (c) the ratio of abortions to women ages 15 to 19 in a given state and year, in logs. The variable *Access* measures young unmarried women's access to the pill, which we describe below. The matrix *X* contains a number of variables controlling for relevant socio-economic factors, including percent nonwhite, the insured unemployment rate, per capita income,

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be useful in constructing counterfactuals on changes in abortion rates in the absence of pill diffusion. We have repeated these results using the full sample of all women, both including and excluding a control on the right-hand side for whether a woman is sexually active. The results in this case are slightly smaller than those reported here (between -0.15 and -0.25) and are less precisely estimated, but are still significant for 17 to 19 year-olds. There is some work suggesting that other episodes of contraception diffusion did not affect women's sexual activity; see for example Chapter 5 of Levine (2004).

and the crime rate (these are the same controls used earlier and suggested by Gruber, Levine, and Staiger, 1999). Finally, we include state and year dummies.

The key variable is the *Access* variable that measures young unmarried women's access to the pill; it is simply a dummy for whether or not the pill was available to all women in the sample. This measure makes sense given the information in Table 6, which shows the youngest age at which a woman could get access to the pill for states which changed their birth control laws after 1973. All the states which change their laws do so by lowering the age at which a woman can obtain the pill to 14, which makes the pill available legally to all women ages 15-19. The table shows that only 7 states changed their access laws after 1974, although there is variation in the location of states that did so.

Table 7 reports regression results from the CDC data. Standard errors are once again corrected for heteroskedasticity and clustered at the state level. The first regression is weighted by cohort size (that is, the number of women ages 15 to 19 in a state and year). The regression reports the effect of pill access on the number of abortions per woman ages 15 to 19 in a state and year (in logs). The coefficient is negative and marginally significant, suggesting that increasing pill access is associated with lower abortion levels. The sample mean of the dependent variable (in levels) is 0.027; the results thus suggest that on average access to the pill lowers the abortion rate from 27 abortions to per every 1,000 women to 22. The other coefficients are generally insignificant, except for percent nonwhite which is negative and significant.

The second column reports abortions per live birth, in logs. The coefficient on pill access is again negative and slightly more significant. The third column repeats this regression, but now the cohorts are weighted by live births to women ages 15 to 19 in a given state and year. The result is very close to before.

The last two columns of the table test the robustness of these results. Column 4 repeats the regression in column 1 but controls for underlying trends in abortion usage by adding state-specific time trends. The result is the same as before (similarly, columns 2 and 3 are robust to the addition of state-specific time trends). The last column considers a more flexible specification: rather than using the log of

the abortion rate as the dependent variable, the regression uses simply the log of the number of abortions as the dependent variable while putting the number of women (in logs) on the right hand side. Column 5, which considers the total number of abortions for women ages 15 to 19, includes the log of the population of 15 to 19 year-old women on the right hand side. The coefficient is very similar to before.

In summary, the results of this section indicate that access to the pill reduced young women's fertility in the short run. Furthermore, data from the NSYW and the CDC both suggest that legal pill access led to fewer abortions among young women. The next section extends these results by examining the long-term impacts of the pill on fertility and on parental characteristics.

IV. The Pill and Long-Term Outcomes

IV.A. *Potential Impact of the Pill on the Characteristics of Mothers*

The fertility effects of the pill documented in the previous section raise an important question: did these legal changes also have long-term effects on the number and distribution of children born to those women who first gained access to the pill as teenagers? Already, we have seen that early pill access reduced teenage childbearing, which is widely believed to be in itself bad for children. In this section we examine the long-term fertility effects of the pill and how early access to the pill impacted the characteristics of the average child's mother.

Prior work has not focused on the long-term fertility effects of the pill.[13] But to understand the effect of pill diffusion on child living circumstances, it is important to identify whether the births that are avoided by young women who get early access to the pill are retimed to a later age, or instead are permanently avoided. If births are retimed, then any effects of the pill on child living circumstances will have to occur through the effects of the pill on women's life choices, such as marriage and education. If births are permanently avoided, then there may be an additional effect of the pill on average child living circumstances through the change in which women are mothers. This effect may act through both the

[13] Bailey (2006) includes a check of the effect of early pill access on the number of children ever born by age 30 in the CPS, but her results are inconclusive; with 95 percent confidence she cannot rule out declines as large as 0.23 children or increases as large as 0.11 children.

extensive margin (some types of women opting out of motherhood) and the intensive margin (some mothers having smaller families).

While prior work on the pill has found a relationship between diffusion and women's occupational and marital status, the effect of fertility control specifically on those women who continue to have children—as opposed to those who use contraceptive innovations to avoid becoming mothers—is of particular policy interest. A large body of research has established that a mother's human capital can have important impacts on her children's health and human capital. To quote Thomas, Strauss, and Henriques (1991), "Many studies have demonstrated that parental education has a significant impact on child health" (p. 183). Currie and Moretti (2003) use an instrumental variables approach to find that mother's education is positively related to child health as measured by birthweight and gestational age. A mother's education may also affect her children's educational outcomes. Oreopoulos, Page and Stevens (2006) and Chevalier (2003) both exploit policy changes in compulsory schooling to demonstrate a strong relationship between mother's and child's educational attainment.

Marital status is also widely believed to matter for child outcomes, including test scores (Guidubaldi, Perry, and Cleminshaw, 1984), mental health (Hetherington and Clingempeel, 1989), and delinquency (Achenbach and Edelbrock, 1983). Much of the literature on the subject suffers from endogeneity problems. But Gruber (2004), using changes in state laws that vary the costs of divorce, finds with a differences-in-differences approach that divorce negatively impacts children's adult outcomes on margins including education, family income, and suicide.

Access to the pill may allow women to avoid unwanted pregnancies and invest in human capital, but this does not mean that the pill will unambiguously lead to higher human capital levels for the average child's mother. For example, either intentionally or unintentionally, those who delay births to invest in human capital might end up realizing fewer births.[14] Publications in the popular press, such as Sylvia Ann Hewlett's (2002) book *Creating a Life: Professional Women and the Quest for Children*, suggest

[14] There is evidence that women of the affected generation inaccurately predicted the number of children they would eventually have: Goldin (2006) finds that, in one longitudinal study of women who entered selective colleges in 1976, 82 percent stated that they expected to have children, but in fact by age 37 only 69 percent actually did.

that for women the costs of pursuing a professional career may include not having any children at all. Moreover, education may change tastes for bearing children. In the US, historically there has been a strong cross-sectional (negative) correlation between women's education and their total childbearing.

If the same women who increased their human capital investments when they received early access to the pill also ended up with lower rates of reproduction than they otherwise would have, then the increase in the human capital of women will be more positive than the change in the human capital of children's mothers (and in fact mothers' average human capital could actually decline). On the other hand, it is possible that the marginal woman whose human capital investment is increased by early pill access is a woman with stronger than average tastes for children. This would be the case if, for example, the pill diminished the sense among women of needing to "choose" between career and family (Goldin, 2004). Women who, if faced with that tradeoff, would have chosen family may have felt that the pill, by allowing them to better coordinate human capital investment and childbearing, made it possible to "have it all"—that is, to have a career without reducing childbearing. If the women who went from low to high human capital in response to the pill had inelastic demand for children, then we may see an increase in the human capital attainment of the average child's mother, and this increase may in fact be *larger* than the increase in human capital observed for the average woman. Thus, on average the cohorts of children born to women who gained access to the pill may or may not see the human capital of their mothers increase.

The effect of pill diffusion on marital status is similarly ambiguous. Goldin and Katz (2002) find that early pill access decreases the percent of college-graduate women who have ever married (consistent with the de-linkage of sex and childbearing) and the percent divorced (consistent with improved match quality). They find no net effect on the percent who are currently married. However, their results do not have clear implications for how early pill access changes the circumstances of mothers. For example, if the pill decreases men's willingness to marry a pregnant woman (Akerlof, Yellen, and Katz, 1996) then marriage rates might decline among mothers as well as among women who use the pill to avoid both marriage and pregnancy. On the other hand, women with strong demand for children may take particular advantage of the pill's potential to improve match quality, leading to a decrease in the share of children

with divorced parents. The net effect of these changes on the percent of children living in intact families is ambiguous.

IV.B. *Lifecycle/Completed Fertility*

We now consider the long-term fertility effects of the pill. To examine the effect of early pill access on the level and distribution of women's lifetime fertility, it would be ideal to have available data from the 2000 decennial Census. In 2000, the youngest cohorts of women affected by pill diffusion were age 35 (those born in 1965 who were just reaching their teen years after complete diffusion of the pill in the late 1970s). Unfortunately, the most recent Census that records children ever born is the 1990 Census. We are therefore unable to look at the completed fertility of the youngest cohorts affected by pill diffusion.

Instead, following Goldin and Katz, we use the 1970 (4-percent pooled sample with state identifiers), 1980 (5-percent sample), and 1990 (5-percent sample) Censuses to look at cohorts of women born after 1920 and before 1961 and observed between ages 30 and 49. In some regressions, we limit the sample to those observed at ages 40 to 49, in order to identify the effects of pill diffusion on those who have completed or virtually completed their fecundity. The units of observation are state-cohort cells, where a cohort is all of the women born in a given year. We estimate the model

$$Fertility = \delta Access + \beta X + \theta_s + \theta_y + \phi_{age} + \varepsilon$$

where *Fertility* is measured either as the logged fraction of women in the cell who have any children or as the logged number of children ever born (among those who have at least one child), *Access* is defined as legal access to the pill before age 21 (as in Goldin and Katz)[15], and the controls in *X* include percent of the state population that is black and percent that is other nonwhite (as in Goldin and Katz). All regressions include state, age, and census-year dummies. Also as in Goldin and Katz, in some regressions we control for access to legal abortion by age 18 and for state-specific linear trends.

[15] Our results are robust to using other age cutoffs.

The results are shown in Table 8. Regressions are weighted by cell population and standard errors are clustered at the state level. The effect of early pill access on the fraction of women with children (shown in Panel A of Table 8) is negative in all specifications. For women observed at ages 30 to 49, the effect is statistically significant in every specification, with declines ranging from 3.9 percent to 4.9 percent in the log share of women who are mothers, from a base fraction of 0.83. The evidence is weaker when restricting the sample to those observed at ages 40 to 49 (columns 5 and 6), but is still significant when linear state trends are included, with an estimate of 2.0 percent (from a base of 0.87). These increases in childlessness are similar to those found in Ananat, Gruber, and Levine (forthcoming) for legal abortion access and are robust to the inclusion of controls for abortion access, suggesting that the two forms of fertility control have parallel effects on the extensive margin of childbearing.

As shown in Panel B of Table 8, early access to the pill leads to decreases in childbearing at the intensive margin as well. The effect is negative and significant in all specifications, and ranges between 3.4 percent and 5.4 percent for mothers ages 30 to 49 (from a base of 2.78) and between 3.3 percent and 7.7 percent for mothers ages 40 to 49 (from a base of 3.03). Our results in Panel B are somewhat larger than those found by Ananat, Gruber, and Levine (forthcoming) for abortion access, suggesting that oral contraceptives may have stronger effects on childbearing at the intensive margin than does legal abortion. While qualitatively compatible with the short-term results on fertility shown in Table 2, the coefficients here are not directly comparable with those short-term results. Table 2 measures the fertility impact of current pill access, while Table 8 measures the long-term impact of having had access before age 21. Further, the dependent variables in the two tables are different. However, taken together the two tables suggest that the pill had economically significant impacts on fertility in both the short and long run, and that these impacts operated on both the intensive and extensive margin. Moreover, these results are consistent with Bailey (2006), who, when looking at CPS data, estimates a (statistically insignificant) decrease of 0.062 children among those aged at least 30 with children.

IV.C. *Human Capital and Marital Status Effects on Women and on Children's Mothers*

Ideally, we would like to be able to explore the effect of early access to the pill for a cohort of women on the life outcomes of the generation of children born to that cohort. In a similar spirit, a variety of papers have examined the effect of women's legal access to abortion on the adult outcomes of their offspring, along such dimensions as crime (Donohue and Levitt, 2001), drug use (Charles and Stephens, 2006), and college graduation (Ananat et al., 2006). Research on abortion, however, has been able to exploit the fact that abortion access changed for all women in a given state in the same year. Therefore, to measure whether someone observed as an adult was born to a mother who had access to abortion, researchers only need to observe the state and year of that person's birth—information that is commonly available in large datasets.

To exploit variation in pill access for the mothers of individuals observed as adults, however, we would need to observe not only the state and year of the individual's birth but also the year of birth of the individual's mother. This information is not available in any large dataset of which we are aware.

Instead we examine women's outcomes, and then weight those outcomes by the number of children they report having ever given birth to—so a woman who has no children is omitted from the analysis, and a woman with three children is counted three times. Using this technique, we can identify how access to the pill changed the characteristics of the average child's mother. We look at education, marital status, and occupational status.[16] Since, as noted above, many researchers have documented the importance of mothers' acquired characteristics on children's life outcomes, these results are themselves suggestive of the implications of pill diffusion for the human capital of the next generation. Of course, to the extent that women's current characteristics do not perfectly reflect their characteristics over their children's childhoods, there will be mismeasurement of childhood living circumstances. This is one

[16] Occupational status is defined based on the most recent job held, regardless of whether a woman is currently working. Since there is controversy over whether having an employed mother is good for children, we could not sign the effect of employment on child outcomes. Moreover, a woman's current employment status is particularly likely to be a poor proxy for her status during her children's childhoods.

benefit to using education, since, once attained, it is a permanent characteristic and since the pill is believed to increase women's educational investment mostly in their early 20s (Bailey, 2006). Marital status and occupational status, on the other hand, are current characteristics that may mismeasure the living environments of offspring in childhood.

In this analysis we again use the 1970, 1980, and 1990 Censuses, and include in our sample women born between 1921 and 1960. We estimate the equation:

$$Outcome = \delta Access + \beta X + \theta_s + \theta_y + \phi_{age} + T + \varepsilon .$$

Our outcomes include both a direct measure of human capital attainment—the fraction of a cell that has completed college—and indirect measures including the fraction reporting a professional occupation (as defined by Goldin and Katz[17]) and the fraction that reports being a doctor or lawyer. We also examine other outcomes including the fraction divorced and the fraction married. Finally, we examine the fraction who "have it all"—that is, who report being married, having at least one child, and having a professional occupation.

The controls in *X* again include percent of the state population that is black and percent that is other nonwhite. We also include state dummies, census-year dummies, age dummies, state-specific linear trends, and a dummy for access to legal abortion by age 18. As in Table 8, *Access* equals unity if a group of women born in a given state and year had access to the pill before age 21.

We estimate the above equation in two ways: we first estimate the equation for the average *woman*, and then we estimate the equation for the average *mother*. The estimates for the average woman follow the form used in the previous subsection, with observations of state-cohort cells of women, weighted by the cell population. Estimates for the average mother separately measure outcomes not only by state and cohort but, further, by parity. These regressions use state-cohort-parity cells and are weighted by total children born to that cell (children ever born * cell population); they therefore measure

[17] Goldin and Katz (p. 761) define "professional occupation" to include professional Census occupations "excluding noncollege teachers and those in health assessment and treating occupations (e.g., nurses, dieticians, therapists, and physicians' assistants)."

the effects of pill access on the average child's mother. These latter regressions consequently exclude women without children from the sample. For both estimates on women and estimates on mothers, outcomes are measured as the log fraction of the cell with a given characteristic, so that the coefficient on pill access can be interpreted as the percent change in the share of the cell with that characteristic due to expanded access.

Results are shown in Table 9. Regressions in column (1) measure the effects of pill access on women and regressions in column (2) measure the effects of pill access on mothers. Residuals are clustered at the state level and corrected for heteroskedasticity. All the regressions include all of the right-hand side controls described above; only the coefficient on access to the pill is reported. The top two reported coefficients from the women's columns are taken directly from columns 4 and 10 of Table 8.

The results of Table 9 show that in several ways the pill appears to have affected the average woman and the average child's mother similarly. Both groups are significantly more likely to report a professional occupation (1.3 percent and 1.6 percent, respectively) and to report being a doctor or lawyer (17.9 percent and 16.6 percent) when they had early access to the pill. Both groups are also less likely to be divorced (12.8 percent and 15.5 percent). In two important ways, however, access to the pill had larger, positive effects on the average child's mother than on women in general.

First, early pill access appears to have led to a significant decline in marriage overall of 0.9 percent, but this result does not generalize to the representative child's mother. Rather, the share of children whose mothers were married rose by a significant 1.1 percent. This is consistent with the literature on abortion, which finds that legalization led to an increase in the proportion of children living with two parents. Combined with the decrease in divorce, this evidence suggests that women with demand for children did leverage the pill to improve marital match quality.

Second, there is a significant increase of 8.8 percent in the share of children's mothers who have completed college, but there is no significant effect of early pill access on overall college graduation

rates.[18] In the context of this finding, the positive effect on mothers' education must result from a differentially smaller reduction in childbearing among college graduate women than among non-college graduate women. This is consistent with previous findings in the abortion legalization literature showing that low-socioeconomic status women reduced childbearing more than did high-SES women when exposed to legal abortion.

Third, pill diffusion has no significant effect on the share of women who "have it all," but does lead to a significant 7.5 percent increase in the share of children whose mothers balance family and professional career. Overall, diffusion does not seem to have allowed more women to "have it all"—it provided them with increased labor market success, in the form of a professional career, but this increase reflects a substitution away from marriage and childbearing, so that the net change in the share of women who are married with children and a professional job is not significantly different from zero.

But when examining the family context of the average child, pill diffusion does cause a significant change. With the diffusion of the pill, fewer women married and had children, and those who became mothers had smaller families. But the children who were born were more likely to be born into intact families, with educated, professional mothers who managed to "have it all."

V. Conclusions

In this paper, we examine the impact of oral contraception's availability on the number and parental characteristics of children born to women who came of age in the 1960s and 1970s. Using a more detailed specification than prior work, we find that access to the pill led to a short-term decline in fertility among these women, consistent with Bailey (2006) but in contrast to some other prior work. Further, we find that this effect was to some extent permanent; women who had access to the pill when young were less likely to become mothers and conditional on becoming mothers had fewer children. In

[18] The lack of an overall college graduation effect is notable in light of the other gains in human capital that occurred with early pill access. To our knowledge no one has directly measured this outcome before: Goldin's work concentrates only on women who are college graduates; other work has looked at employment and other measures of human capital but not at education. Our finding is robust to a variety of specifications, however, and leads us to conclude that college was not a significant source of women's increase in human capital in response to pill diffusion.

addition, we find that the increase in human capital investment (as proxied by occupational status) and the decrease in entry and exit from marriage identified by Goldin and Katz (2002) for college-graduate women hold throughout the population. While we find that family size fell, the pill also seems to have been used to increase career investment and marital quality by women with relatively inelastic demand for motherhood, allowing mothers to "have it all." Interestingly, however, the average woman's likelihood of having it all was unaffected by early pill access.

We also consider whether the pill decreased abortion—that is, whether the pill and abortion were viewed as substitutable fertility-control technologies by young women. We find some evidence that they were. The negative relationship between abortion and pill access among teens is visible in two datasets, is robust to measurement in the period both before and after the legalization of abortion, and appears when relying either on individual survey data or on aggregate figures representing the universe of legal abortions.

While our main interest in this paper is identifying the effect of the pill on women's fertility behavior as it affects the number and characteristics of children born, we note that casting light directly on the substitutability of oral contraception and abortion can be informative for current policy debates on fertility control. For example, the FDA has recently chosen not to allow the sale of emergency contraception without a prescription in part because of concerns regarding its use by young teenagers (Harris, 2005). Opponents of this form of contraception argue that it acts as an abortive agent, while proponents argue that access to this drug will lower abortions by preventing unwanted pregnancies.[19] More generally there is disagreement on whether improved access to contraception lowers women's abortion rates (as argued by Cohen, 1998) or raises them (Smith, 1993). We are aware of no work in economics, and only a limited body of work in other disciplines, which examines whether any form of oral contraception can substitute for abortions. While our work here demonstrates that access to the pill did lower abortion rates among young women in the late 1960s and throughout the 1970s, this finding

[19] See for example Jones, Darroch, and Henshaw (2002), which argues that access to emergency contraception could have prevented over 50,000 abortions in the year 2000. Emergency contraception's properties as an abortive agent are beyond the scope of the present paper.

may not generalize to other forms of contraception (such as emergency contraception) and may not generalize across time. More research is needed in this area.

Our work also suggests that, like abortion, access to birth control may have long-lasting cohort effects. Further work in this area is also needed, although examining the effects of maternal pill access on outcomes such as crime is difficult for the data reasons discussed in Section IV. Furthermore, while we have shown evidence of substitution between abortion and the pill, this does not suggest that the pill's long-term impacts on outcomes of children will mirror the impacts of abortion, because even though some individuals view these two technologies as substitutable the average pill user will not necessarily resemble the average abortion user. Indeed, while Table 9 suggests that abortion and the pill have some similar effects on parental characteristics, the results in Table 8 suggest that the pill and abortion may affect fertility in different ways.

These results also have implications for the well-known increase in the birth rate of young single women in the 1970s. As noted in the U.S. Department of Health and Human Services (1980), "teenagers have been the one group not to follow the general downward trend in illegitimacy in recent years" (p. 7); the report suggests contraception as a potential explanatory factor. Cutright (1971) argues that the increasing popularity of the pill may help explain rising out-of-wedlock birth rates in the United States. More recently, Akerlof, Yellen, and Katz (1996) argue that rising out-of-wedlock birth rates have been affected by the decline of shotgun marriages, which themselves were affected by the rise of female contraception. However, our results here suggest that access to the pill attenuated the observed trend in illegitimacy, instead of causing it. More research is clearly needed to assess historic trends in out-of-wedlock birth rates among young women during this important period.

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Table 1

Summary Statistics from 1980 Census

	No Access to the Pill		Access to the Pill	
	Mean	Std. Dev.	Mean	Std. Dev.
	(1)	(2)	(3)	(4)
Mother's Age	16.74	1.93	17.55	1.95
Birth Rate	0.06	0.05	0.07	0.04
Share of children in one-parent HHs	0.33	0.14	0.35	0.15
Share of children in HHs below poverty	0.26	0.13	0.30	0.11
State insured unemployment rate	3.30	1.60	3.44	1.65
State per-capita income	3.58	1.52	6.07	1.81
State crime rate	2.33	1.57	4.36	1.66
State percent nonwhite	13.23	8.12	16.71	8.21
Observations	4,056		1,978	

Notes: Sample includes all observations used in Table 2 and covers women ages 14 to 20 from 1960 through 1979. The unit of observation in each regression are all women of a given age, in a given state and year. The birth rate is calculated as the number of births reported by all women in a cohort over the number of women. These means are weighted by number of women in a cohort; the total weight for observations without access to the pill is 7,389,457; for women with access to the pill the total weight of the cohort is 3,933,767.

Table 2

The Pill and Short-Term Fertility

	GLS Specification			State/Year Dummies		
	Birth Rate (levels)	Birth Rate (logged)	Children (logged)	Birth Rate (Levels)	Birth Rate (logged)	Children (logged)
	(1)	(2)	(3)	(4)	(5)	(6)
Access to the Pill	-0.0088	-0.1160	-0.0396	-0.0166	-0.2116	-0.0839
	(0.0026)	(0.0333)	(0.0249)	(0.0036)	(0.0426)	(0.04)
State-by-Year Controls?	Yes	Yes	Yes	No	No	No
State Dummies?	Yes	Yes	Yes	No	No	No
State Trends?	Yes	Yes	Yes	No	No	No
Quadratic State Trends?	Yes	Yes	Yes	No	No	No
Year Dummies?	Yes	Yes	Yes	No	No	No
Mother's Age Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year Dummies?	No	No	No	Yes	Yes	Yes
Observations	6034	6034	6034	6034	6034	6034
R-squared	0.88	0.95	0.97	0.89	0.96	0.97

Notes: Standard errors in parentheses. Regressions are weighted by the number of women; residuals are clustered by state and corrected for heteroskedasticity. Weighting by number of children produces similar estimates. The regression covers births for women ages 14 to 20 from 1960 through 1979. The unit of observation in each regression are all women of a given age, in a given state and year. The "Access to the Pill" variable equals unity if a cohort of women had legal access to birth control in the prior year. The regressions on number of children include the number of women in a cohort, in logs, as a regressor. The state-by-year controls in the Gruber, Levine, and Staiger (GLS) specification includes the insured unemployment rate, the crime rate, the percent of the population nonwhite, and per-capita personal income.

Table 3

Summary Statistics from the NSYW

	Mean	Std. Dev.
Age	17.68	1.08
Catholic Dummy	0.16	0.37
In School Dummy	0.58	0.50
White Dummy	0.55	0.50
Rural Dummy	0.12	0.32
Has Access to Pill Now	0.48	0.50
Had Access to Pill at Age 16	0.26	0.44

Notes: Total observations: 1,446. Sample includes all sexually active women ages 16 to 19 in the NSYW. The rural dummy equals unity if the respondent does not live in an SMSA. Means are unweighted.

Table 4

State of Residence and Availability of the Pill for Women Reporting Abortions

State	Respondent did not have access at age 16	Respondent had access at age 16
Alabama	1	0
Arkansas	1	0
Arizona	3	0
California	11	0
Connecticut	3	0
Florida	8	0
Georgia	0	3
Illinois	1	1
Louisiana	2	0
Maryland	0	9
Michigan	2	0
Missouri	1	0
North Carolina	3	0
New Jersey	2	0
New Mexico	2	0
New York	1	0
Ohio	0	3
Tennessee	1	0
Texas	1	0
Virginia	4	0
Washington	2	0
Wisconsin	1	0
Washington	2	0
Wisconsin	1	0
Total	50	16

Source : NSYW.

Table 5

Pill Diffusion and Abortion: Evidence from the NSYW

	Linear Probability Model on Likelihood of Ever Having an Abortion			
	(1)	(2)	(3)	(4)
Access to the Pill before Age 17	-0.0417	-0.048	-0.0499	-0.0622
	(0.0238)	(0.0255)	(0.0264)	(0.0303)
Access to Pill Now	-	-	0.0182	-
			(0.0381)	
Access to Abortion	-	-0.006	-0.0243	-0.0211
		(0.0187)	(0.0440)	(0.0171)
Low Income Dummy	-	0.0215	0.0211	0.0173
		(0.0164)	(0.0165)	(0.0187)
Rural Dummy	-	-0.0047	-0.0047	-0.0182
		(0.0176)	(0.0177)	(0.0198)
White Dummy	-	-0.0129	-0.0123	-0.0072
		(0.0145)	(0.0149)	(0.0175)
Catholic Dummy	-	-0.0129	-0.0131	-0.0112
		(0.0179)	(0.0180)	(0.0234)
Currently in School Dummy	-	-0.0079	-0.0079	0.0044
		(0.0175)	(0.0175)	(0.0216)
State Dummies?	Yes	Yes	Yes	Yes
Educational Attainment Dummies?	No	Yes	Yes	Yes
Church Importance Dummies?	No	Yes	Yes	Yes
Church Attendance Dummies?	No	Yes	Yes	Yes
Age Dummies?	Yes	Yes	Yes	Yes
Age*Region Dummies?	Yes	Yes	Yes	Yes
Observations	1446	1446	1446	1183
R-squared	0.05	0.06	0.06	0.06

Notes: Standard errors in parentheses. Residuals are clustered by state and corrected for heteroskedasticity. The low income dummy equals unity for individuals reporting income levels in the bottom decile of the sample. Church attendance is measured on a 1-5 scale (from "never" to "seven or more times a month"). Church importance is measured on a 1-4 scale (from "very important" to "not at all important"). The dependent variable equals unity if a respondent reports ever having an abortion, and equals zero otherwise. Sample includes sexually active women ages 16 to 19. The last column restricts the sample to women ages 17 to 19. Redoing the regressions with all women (not just sexually active women) produces slightly smaller results which are less precise (but still significant for women ages 17 to 19). Only a small fraction of the women in the sample had legal access to abortion (see text).

Table 6

Age when Minor Could Obtain Pill for States which Diffused the Pill After 1973

State	1974	1975	1976	1977	1978	1979
Arizona	18	18	18	14	14	14
California	18	14	14	14	14	14
Minnesota	18	18	14	14	14	14
North Carolina	18	18	18	14	14	14
Nevada	18	14	14	14	14	14
New York	16	14	14	14	14	14
Utah	18	14	14	14	14	14

Notes: Table shows the age when a minor had the ability to consent for the pill without her parents' involvement, from Guldi (2005). The states with CDC data on 15 to 19 year olds available include Alaska, Arkansas, Arizona, California, Colorado, Connecticut, the District of Columbia, Georgia, Hawaii, Iowa, Idaho, Illinois, Indiana, Kansas, Kentucky, Louisiana, Maryland, Massachusetts, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, Ohio, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Utah, Virginia, Vermont, Washington, and Wyoming. While some states do not report their data each year, data from the seven states in the table are available for each year between 1974 and 1979. States whose laws changed after 1973 but whose data are not available from the CDC are excluded from the above table.

Table 7

Pill Diffusion and Abortions by Women Ages 15 to 19: Evidence from the CDC

	Abortions per Woman (logged)	Abortions per Birth (logged)	Abortions per Birth Alternate Weighting	Abortions per Woman (logged)	Abortions (logged)
	(1)	(2)	(3)	(4)	(5)
Pill available to whole sample	-0.1964	-0.2036	-0.2022	-0.1932	-0.1755
	(0.1205)	(0.1180)	(0.1206)	(0.1198)	(0.1131)
Percent Nonwhite	-0.2232	-0.2293	-0.2463	-0.2282	-0.2033
	(0.0793)	(0.0756)	(0.0774)	(0.0807)	(0.0712)
Crime	0.0544	0.0715	0.0803	0.0533	0.0141
	(0.1659)	(0.1595)	(0.1582)	(0.1648)	(0.1460)
Unemployment	-0.0135	-0.0079	-0.004	-0.0137	0.0108
	(0.0411)	(0.0469)	(0.0467)	(0.0410)	(0.0400)
Per-capita income	-0.0302	-0.0257	-0.0298	-0.0309	-0.0141
	(0.0321)	(0.0317)	(0.0315)	(0.0322)	(0.0289)
State Trends?	No	No	No	Yes	Yes
Year Dummies?	Yes	Yes	Yes	Yes	Yes
State Dummies?	Yes	Yes	Yes	Yes	Yes
Observations	209	209	209	209	209
R-squared	0.90	0.94	0.94	0.90	0.98

Notes: Standard errors in parentheses. Residuals are clustered by state and corrected for heteroskedasticity. The variable "Pill available to whole sample" equals unity if the age a woman could consent for the pill is 15 or lower; this variable equals unity for 112 observations in the sample. Columns 1, 2, 4, and 5 are weighted by the population of women ages 15 to 19 in a given state and year. Column 3 is weighted by the number of births to women ages 15 to 19 in a given state and year. Adding trends to columns 2 and 3 do not change their results, nor does removing trends from the last two columns. The mean of abortions per woman ages 15 to 19 (in levels) in the sample is 0.027.

Table 8

The Pill and Lifecycle Fertility

Panel A

	Fraction of Women with Children (logged)					
	(1)	(2)	(3)	(4)	(5)	(6)
Access to Pill before Age 21	-0.0454	-0.0409	-0.0494	-0.0385	-0.0020	-0.0202
	(0.0039)	(0.0061)	(0.0037)	(0.0041)	(0.0066)	(0.0047)
Controls for abortion Access?	No	Yes	No	Yes	N/A	N/A
State trends?	No	No	Yes	Yes	No	Yes
Includes only Women aged 40+?	No	No	No	No	Yes	Yes

Panel B

	Number of Children among Women with Children (logged)					
	(7)	(8)	(9)	(10)	(11)	(12)
Access to Pill before Age 21	-0.0337	-0.0486	-0.0371	-0.0542	-0.0329	-0.0774
	(0.0127)	(0.0138)	(0.0136)	(0.0140)	(0.0139)	(0.0160)
Controls for abortion Access?	No	Yes	No	Yes	N/A	N/A
State trends?	No	No	Yes	Yes	No	Yes
Includes only Women aged 40+?	No	No	No	No	Yes	Yes

Notes: Standard errors in parentheses. Each coefficient is for access to the pill before age 21; each coefficient is taken from a separate regression. Observations include women born in a given state and year between 1921 and 1960 and observed at age 30 to 49 in the 1970, 1980, or 1990 Census; regressions are population-weighted. Residuals are clustered at the state level and corrected for heteroskedasticity. All regressions include state, census year, and age fixed effects and linear controls for the proportion of the cohort that is African-American and that is other nonwhite. The first panel represents the effect of access to the pill before age 21 on the share of women who have at least one child; the mean of the dependent variable is 0.826. The second panel represents the effect on the number of children among those who have at least one child; the mean of the dependent variable is 2.78.

Table 9

Effect of Early Pill Access on Women's and Mothers' Characteristics

	Women	Kids' moms
Fraction of Women with Children (logged)	-0.0385	
	(0.0041)	-
	mean=82.6%	
Number of Children among Women with Children (logged)	-0.0542	-0.0473
	(0.0140)	(0.0120)
	mean=2.78	mean=3.66
Fraction with Professional Occupation (logged)	0.0129	0.0156
	(0.0046)	(0.0051)
	mean=30.1%	mean=32.1%
Fraction Doctors or Lawyers by Occupation (logged)	0.1786	0.1661
	(0.0591)	(0.0600)
	mean=0.4%	mean=0.2%
Fraction Currently Divorced (logged)	-0.1279	-0.1545
	(0.0431)	(0.0436)
	mean=11.9%	mean=10.5%
Fraction Currently Married (logged)	-0.0085	0.0117
	(0.0020)	(0.0032)
	mean=73.2%	mean=80.2%
Fraction College Graduates (logged)	-0.0442	0.0875
	(0.0351)	(0.0395)
	mean=18.6%	mean=11.7%
Fraction Who "Have it All" (logged)	-0.0114	0.0754
	(0.0070)	(0.0160)
	mean=5.2%	mean=4.8%

Notes: Standard errors in parentheses. Each coefficient is for access to the pill before age 21; each coefficient is taken from a separate regression. Observations include women born in a given state and year between 1921 and 1960 and observed at age 30 to 49 in the 1970, 1980, or 1990 Census; regressions are population-weighted. Residuals are clustered at the state level and corrected for heteroskedasticity. All regressions include state, census year, and age fixed effects, linear controls for the proportion of the cohort that is African-American and that is other nonwhite, linear state trends, and an indicator for whether the cohort had access to legal abortion by age 18. The first two coefficients in the left column are taken from Table 8. The first column represents the effect of access to the pill before age 21 for the average woman. The second column represents the effect for the average child's mother (see text).