The Pill and Reproductive Behavior of Young Women

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Abstract

A growing literature documents how the availability of oral contraception affected the outcomes of young women in the 1960s and 1970s, but the effects of oral contraception on women's fertility remains disputed. In this paper, we examine whether increased access to the pill affected women's fertility and whether the pill substituted for other fertility technologies. Using census data, we document that access to the pill led to falling short-term fertility rates for young women. We then use two different datasets to examine the impact of legal oral contraception on unwanted pregnancies, and in particular on abortions, in order to identify whether these fertility technologies were viewed to some extent as substitutable by young women. In both datasets we find that access to oral contraception lowers the number of abortions for young women.

*Thanks to David Autor, Martha Bailey, Jonathan Gruber, Jacob Vigdor, and seminar participants at Notre Dame, the Midwest Economics Association, and MIT for helpful comments and suggestions. Thanks also to Melanie Guldi for providing the pill and abortion laws data. Email the authors at eoltmans@mit.edu and dhungerm@nd.edu.

I. Introduction

A growing literature documents how the availability of oral contraception affected 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 documented 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 effects of oral contraception's diffusion on the children born to the impacted groups of women. Even the basic question of whether or not 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 provide evidence that increased access to contraception leads to higher pregnancy rates among teenagers.

More research has been done on the effects of access to abortion on fertility and children's outcomes. The results of these studies suggest that increased access to fertility control for women in the form of legal abortion reduces their fertility in the short term (Levine et al. 1999) and long term (Ananat, Gruber, and Levine 2006) and alters the cohorts who are born to these women on important margins such as likelihood of poverty (Gruber, Levine, and Staiger 1999), total crime committed (Donohue and Levitt 2001, Foote and Goetz 2005), and drug use (Charles and Stephens 2006). These results beg the question of whether the introduction of the pill—the other major fertility control innovation in recent history—had similar or different effects on fertility and on 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 children born to these women. We exploit a very detailed dataset on pill access,

provided by Guldi (2005), that 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 1965 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 women's birthrates by about 10 percent.

We then ask whether young women consider oral contraception and abortion as substitutable contraception technologies. Specifically, we investigate the relationship between legal access to the pill and abortion rates. We exploit two different data sources that employ different methods of information-gathering and that cover different time periods and legal regimes. We find a negative relationship between legal access to the pill and abortions. The relationship is economically significant and generally statistically significant.

The remainder of the paper is as follows. Section II provides a brief history of the pill and a discussion on its predicted impacts on fertility decisions. Section III describes empirical estimates of the effects of access to the pill on fertility and the relationship between pill access and abortion. Section IV 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 also become the most popular form of contraception among never-married women ages 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 which 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 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 from the Vietnam War.

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

Passage of these laws do 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 which 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 introduces controls not only for observed but also for unobserved heterogeneity within states and years.

The pill's impact on fertility could either be positive or negative, and further could either be temporary or permanent. Our paper considers the short-term fertility effects but it is important to note that even if we can unambiguously document these effects, the welfare implications of the effects may depend on what happens in the long run.

For example, suppose expanding access to the pill results in smaller, negatively selected cohorts of children in the short run ("negatively selected" means that the reduction in fertility is driven by women with relatively good socioeconomic characteristics). But in the long run, the pill has no fertility effect on those who use it; that is, the pill is used only to change the timing of births. Inferences on immediate changes in the socioeconomic characteristics of children born after pill diffusion could be misleading, since the characteristics of the "completed cohort" of

[2] Characteristics she tests include the fraction of the population that is black, the fraction of a 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.

children born to these women would not have changed. This scenario is, in fact, similar to that conjectured by Angrist and Evans (1999). Here, it is the permanent characteristics of women that matter for their children's outcomes. Such a scenario is not, however, the only one that has been proposed in the economics literature. For example, Bailey (2006) argues that, while the decline in short-term fertility after pill diffusion largely reflects delayed births that are eventually made up, these delays allow women to make greater human capital investments, with permanent effects on their labor market success.[3] Increased human capital of women may improve the circumstances into which children are born.

Alternatively, pill diffusion may affect not only the timing of births but also total births. For example, it is plausible that the effects of pill access are similar to the effects of abortion access, which include not only re-timing of births but also lower lifetime fertility (Ananat, Gruber, and Levine 2006). However, even documenting that the pill and abortion are viewed as substitutes would not necessarily support this hypothesis, because the characteristics of the average pill user may significantly differ from the characteristics of a woman on the margin between using the two contraception technologies.

Other scenarios incorporate the possibility that increased pill access will not lead even to short-term declines in fertility, consistent with Guldi's (2005) estimates. In an extreme example, pill diffusion might cause an immediate increase in birthrates among women who don't successfully adopt new contraceptive technology.[4] Alternately, access to the pill may encourage young women to become sexually active, and these women may voluntarily decide after a period to forgo contraception. This may lead to higher birth rates from pill access, a story supported by Arcidiacono, Kwaja, and Ouyang (2005). While we cannot necessarily make strong welfare implications from documenting a short-term change in fertility from pill diffusion, we *can*

[3] Birdsall and Chester (1987) argue that investments may increase in the face of pill diffusion even without short-term decreases in fertility, if it is the option value of the pill rather than re-timing that most facilitates investments.

[4] This scenario would parallel one proposed in Akerlof, Yellen, and Katz (1996) as part of an explanation for the increase in out-of-wedlock births following increased access to fertility control.

document whether such a long-term effect exists or whether these other competing fertility scenarios are observed. Evidence of substitutability between the pill and abortion would provide further evidence that at least some contracepting women successfully used the pill to avoid unwanted pregnancies.

III. Empirical evidence on the Pill's effects on short-term fertility

The Pill and the Birthrate

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% public use microdata.[5] Our sample consists of state-year of birth-mother cohort averages of the number of children born between 1965 and 1980 and observed in the Census with their mothers. This sample restriction allows us to construct the pill access a child's mother had during her teenage years by using her year of birth and her state of birth (we have also constructed access as a function of current state of residence or child's state of birth; the results are similar).

Table 1 presents some basic summary statistics from the 1980 Census. 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 1965 and 1979. The means are divided between cohorts of women based on their access to the pill. Women with access to the pill have similar means in terms of the average mother's age, the birthrate, 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

[5] 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. However we have also obtained similar results using Vital Statistics microdata.

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 log of the birthrate 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 (that is, the presumed year of conception)[6]; *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.[7]

This results from estimating this equation are shown in columns 1 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 births.[8] The results confirm that policies expanding access to the pill had the immediate result of decreasing births among the affected cohorts. The results are qualitatively similar using the log number of births or the log of the birthrate; estimates using the log of the birthrate, which are more precise, suggest a highly significant 7.1% decline in the birthrate among women who are granted access to the pill.

[6] 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, in the next subsection we show results from a different dataset that confirm the findings of Goldin and Katz (2002) that access to the pill did lead to higher use.

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

[8] Using alternate weights, such as the number of women in a sample, produces similar estimates.

Second, 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 equation estimated above, 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 2 and 4 of Table 2, provide further confirmation that policies expanding access to the pill had the immediate result of decreasing births among the affected cohorts. Again the results are qualitatively similar using the log number of births or the log of the birthrate and again estimates using the log of the birthrate are larger and more precise. The results using this less restrictive specification suggest even larger reductions in births following expanded access to the pill: we estimate a highly significant 12.4% decline in the birthrate in the year subsequent to expanded access.

The Pill and Abortion

In this section we examine whether access to the pill affected the likelihood that a young unmarried woman had an abortion. The first source of data for this investigation is the 1971 National Survey of Young Women, or NSYW (Kantner and Zelnik, 1972), a nationally

representative sample of 4,611 women ages 15 to 19 living in households and college dorms 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,[9] 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.

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. (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.) 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 summarized in Table 3, including an indicator for whether the respondent is white, an index for church attendance (the regressions use dummy variables for each value in the index), an indicator for a rural location ("rural" here means that a respondent does not live in an SMSA), and an indicator for welfare use by the household; θ_s is a set of state dummies; and ϕ_{age} is a set of age dummies.

[9] 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.

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 considered other lagged access measures, such as access by age 17, 15 or 14. 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—therefore these variables are simply proxies for access by ages 18 or 16. Many states eventually lowered the age at first access to 14, but among the women in the preferred regressions sample (those ages 18 and 19 and sexually active in 1971) less than 7 percent had access when they were 14. 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 virtually all of these women abortion was illegal at the time of the survey. No respondent reports having more than one abortion. We remove one cohort of women, 19-year-old single women living in Maryland, because out of 29 such women 6 report having an abortion, which we find implausible. However, the basic result is robust to including this cohort, as we show below. Table 4 shows the distribution of abortion responses by state and whether or not access was available at the age of 16. There is considerable variation in the number of abortions within states and years.

Table 5 reports regressions from the NSYW. The first column is a linear-probability regression on the likelihood of ever having had an abortion among sexually-active women ages 18 and 19 based on access to the pill at age 16. Focusing on women ages 18 to 19 makes sense

because these women are likely to have been sexually active for longer, making their answer to the question more meaningful. (We will consider results on all women ages 16 and older momentarily.) Residuals are clustered by state and corrected for heteroskedasticity. All regressions include state dummies and women's age dummies. The regression shows that among 18- and 19-year-old sexually active women in 1971, having had access to the pill for the past 2 to 3 years significantly lowers the likelihood of ever having had an abortion. The second column adds the cohort of 19-year-old unmarried women from Maryland to the sample, the result is somewhat smaller and less precise but still clearly statistically significant.

The third column adds in various right-hand side controls; the result is still negative and significant, although the standard error increases. The fourth 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 fifth column adds in sexually active women ages 16 and 17 to the sample. Adding in these women lowers the estimate, although it is still marginally significant. Again, it is not surprising that the result is weaker for younger women, as it is likely that these women have had less sexual experience than slightly older women.

The NSYW also includes information on whether women have ever used the birth control pill; the last column of Table 5 reports results from a regression on whether a woman has ever used the pill before. Our results mirror those of Goldin and Katz (2002); we find that legal access to the pill increases the likelihood of sexually active 18 and 19 year old women reporting having used the pill. The 9 percent effect of legal access is economically large, the mean of this variable in the sample is 0.43.

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 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 -0.25 and -0.30) but are consistently significant. The results are also significant if restricted to only unmarried women in the sample.

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 and we focus on the period for which abortion in all states was legal. The CDC data collected abortion information from all 50 states, New York City, and the District of Columbia during this time period, but only about 35 states report information on abortions for those 15 to 19.

The advantages of the CDC dataset are that it does not rely on self-reported survey data and that it contains information on the universe of legal abortions. The disadvantages of the CDC data are that they are only available at a time when most states had already diffused oral contraception, that they only cover 35 states, and that they are for 15-19 years 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:

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

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*, described further below, measures young unmarried women's access to the pill. The matrix *X* contains a number of

variables controlling for relevant socio-economic factors, specifically percent nonwhite, the insured unemployment rate, per capita income, 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 aged 15-19 in a state and year. 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 1974. All the states which change their laws after 1974 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. As seen in Table 8, only 8 states changed their access laws after 1974; however, there is significant geographic variation in the states that make changes over this period.[10]

Table 7 reports regression results from the CDC data. Standard errors are once again corrected for heteroskedasticity and clustered at the state level. The regressions are weighted by cohort size (that is, the number of women ages 15 to 19 in a state and year). The first 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 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 three columns of the table test the robustness of these results. Column 4 controls for underlying trends in abortion usage by repeating the regression on abortions-per-teenage

[10] We have also run regressions omitting the year that the laws changed in a given state. Doing so changes none of the results below and in most cases leads to larger estimates whose t-statistics are very close to the coefficients shown below.

woman, with state-specific time trends added. The result is the same as before (similarly, all other regressions in the table are robust to the addition of state-specific time trends). The last two columns consider a more flexible specification: rather than using the log of the abortion rate as the dependent variable, the regressions use simply the log of the number of abortions, 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. Again, the results aresimilar Finally, column 6 tests for concerns related to omitted characteristics by repeating the regressions from column 5, but now the dependent variable is *all* abortions in a given state and year, not just abortions for 15 to19 year-olds (on average, abortions to 15 to 19 year olds make up 34 percent of all abortions). Adding abortions for women unaffected by diffusion of the pill greatly weakens the result; it is no longer statistically significant. This supports a story where changes in abortion rates reflect changes in pill laws, rather than changes in underlying factors that drive the overall demand for abortions in a population.

IV. Conclusions

In this paper, we examine the impact of oral contraception's availability on the fertility of young women in the 1960s and 1970s. Using a more detailed specification than prior work, we find that access to the pill led to a decline in fertility among these women, consistent with Bailey (2006) but in contrast to some other prior work.

We also consider whether the pill decreased abortion—that is, whether the pill and abortion were fertility-control technologies viewed as substitutes 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 especially informative for current policy debates on recent innovations in fertility control, such as emergency contraception. In particular, 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.[11] 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.

This paper suggests that considerable future work is needed on the relationship between birth control and children's outcomes. A growing body of work, such as Ananat, Gruber, Levine, and Staiger (2006), has examined the long-term impacts of abortion access on the cohorts of potential children exposed to it, including both those effects that are driven by decreases in fertility and those that are driven by changes in the mix of women giving birth. Complementary work on the pill should examine: its long-term impacts on child cohort characteristics; whether the "marginal child" not born due to pill access is positively or negatively selected; and whether the ability provided by the pill to better control the timing of births affects child outcomes independent of long-term changes in fertility.

[11] 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.

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Table 1:
Summary Statistics from 1980 Census

	No Access to the Pill		Access to the Pill	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Mother's Age	18.09	1.60	18.51	1.41
Birthrate	0.10	0.05	0.09	0.03
Share of children in one-parent HHs	0.27	0.10	0.28	0.10
Share of children in HHs below poverty	0.22	0.09	0.25	0.08
State insured unemployment rate	2.71	1.42	3.26	1.64
State per-capita income	3.79	1.21	5.79	1.85
State crime rate	2.63	1.41	4.25	1.77
State percent nonwhite	13.50	7.58	16.77	8.64
Observations	2,697		2,012	

Sample includes all observations used in the following regressions and covers women ages 14 to 20 from 1965 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 children born; the total weight for observations without access to the pill is 297,167; for women with access to the pill the total weight of the cohort is 274,026.

Table 2: The Pill and Short-Term Fertility

	On Number of Births, in Logs		On Log of Birthrate	
	GLS Specification	State/Year Dummies	GLS Specification	State/Year Dummies
Access to the Pill	-0.0359 [0.0210]	-0.0594 [0.0336]	-0.0714 [0.0264]	-0.1238 [0.0388]
State-by-year controls?	Yes	No	Yes	No
State Dummies?	Yes	No	Yes	No
State Trends?	Yes	No	Yes	No
Quadratic State Trends?	Yes	No	Yes	No
Year Dummies?	Yes	No	Yes	No
Mother's Age Dummies?	Yes	Yes	Yes	Yes
State-by-Year Dummies?	No	Yes	No	Yes
Observations	4209	4209	4209	4209
R-squared	0.98	0.98	0.92	0.93

Standard errors in parentheses. Regressions are weighted by the number of births; residuals are clustered by state and corrected for heteroskedasticity. The regression covers births for women ages 15 to 20 from 1965 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 births 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 include 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	17.65	1.07
Church Attendance	2.16	1.19
White Dummy	0.56	0.50
Rural Dummy	0.12	0.33
Someone in HH on Welfare	0.16	0.36
Has Access to Pill Now	0.46	0.50
Had Access to Pill at age 16	0.24	0.43

Total observations: 1,420. Sample includes all sexually active women ages 16 to 19 in the NSYW. Church attendance is measured on a 1-5 scale, where 1 is "never in the past month" and 5 is "7 or more times" in the past month. The rural dummy equals unity if the respondent does not live in an SMSA.

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	3
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	10

Table 5:
Pill Diffusion and Abortion: Evidence from the NSYW

Linear Probability Model on Likelihood of:	Ever Having an Abortion	Ever Having an Abortion	Ever Having an Abortion	Ever Having an Abortion	Ever Having an Abortion	Ever Using the Pill
Access to the Pill before Age 17	-0.0718 [0.0166]	-0.056 [0.0220]	-0.0637 [0.0240]	-0.0638 [0.0242]	-0.0312 [0.0171]	0.0878 [0.0460]
Access to Pill Now	-	-	-	0.0044 [0.0206]	-	-
Dummy for HH receiving welfare	-	-	0.0699 [0.0323]	0.0699 [0.0323]	0.0436 [0.0196]	0.0419 [0.0676]
Rural Dummy	-	-	-0.0407 [0.0243]	-0.0407 [0.0243]	-0.0098 [0.0175]	-0.0954 [0.0559]
White Dummy	-	-	0.0213 [0.0201]	0.0213 [0.0201]	-0.0079 [0.0153]	0.1139 [0.0388]
Church Attendance Index?	No	No	Yes	Yes	Yes	Yes
State Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Age Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	803	832	803	803	1420	803
R-squared	0.06	0.06	0.08	0.08	0.05	0.12

Standard errors in parentheses. Residuals are clustered by state and corrected for heteroskedasticity. Church attendance is measured on a 1-5 scale, with 1 equaling no attendance in the past month. The dependent variable equals unity if a respondent reports ever having an abortion, and equals zero otherwise. Sample includes sexually active women ages 18 and 19, omitting 19-year olds in Maryland. Column 2 includes all 19-year old unmarried women in Maryland, who are excluded from most regressions to do an implausibly high number of abortions being reported. Column 5 includes all sexually active women ages 16 to 19. Repeating the regression in column 3 including both sexually active women and other women, and including a dummy for whether a woman is sexually active on the right-hand side, does not substantially change the results.

Table 6:
Age when Minor could obtain Pill for States which Diffused the Pill after 1974

State	1974	1975	1976	1977	1978	1979
Arizona	18	18	18	14	14	14
California	18	14	14	14	14	14
Delaware	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

Table shows the age when a minor had the ability to consent for the pill without her parents' involvement, from Guldi (2005).

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: 15-19 Year-Olds, Logged	All Abortions
Pill available to whole sample	-0.1964 [0.1205]	-0.2036 [0.1180]	-0.2022 [0.1206]	-0.1932 [0.1198]	-0.1755 [0.1131]	-0.1172 [0.0909]
Percent nonwhite	-0.2232 [0.0793]	-0.2293 [0.0756]	-0.2463 [0.0774]	-0.2282 [0.0807]	-0.2033 [0.0712]	-0.1470 [0.0641]
Crime	0.0544 [0.1659]	0.0715 [0.1595]	0.0803 [0.1582]	0.0533 [0.1648]	0.0141 [0.1460]	0.1149 [0.1401]
Unemployment	-0.0135 [0.0411]	-0.0079 [0.0469]	-0.004 [0.0467]	-0.0137 [0.0410]	0.0108 [0.0400]	0.0188 [0.0336]
Per-capita income	-0.0302 [0.0321]	-0.0257 [0.0317]	-0.0298 [0.0315]	-0.0309 [0.0322]	-0.0141 [0.0289]	-0.0028 [0.0266]
State Trends?	No	No	No	Yes	Yes	Yes
Year Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	209	209	209	209	209	209
R-squared	0.90	0.94	0.94	0.90	0.98	0.98

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 women could consent for the pill is 15 or lower; this variable equals unity for 112 observations in the sample. In each regression in the first four columns, the dependent variable is for women ages 15 to 19; for example, abortions per woman is calculated as all abortions to women ages 15 to 19 over the population of women 15-19. 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. Column 5 is weighted by the number of women of child-bearing age (women 15 to 44). 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.