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ABSTRACT

In this paper we ask how the diffusion of oral contraception to young unmarried women affected the number and maternal characteristics of children born to these women. Using census data, we find that early pill access led to an increase in the share of children whose mothers were married, college-educated, and had professional occupations. The pill's effects on the average mother are different from the pill's effects on the average woman, and the effects of the pill on maternal characteristics are in some instances different from the effects of abortion. We investigate the mechanisms by which the pill led to these differential effects and find that access to the pill led to falls in short-term fertility rates for young women and led to decreases in lifetime fertility at the intensive and extensive margins. The impacts of the pill on household characteristics are thus associated with retiming of births, changes in the characteristics of potential mothers, changes in which women become mothers, and by reductions in completed family size. Finally, while the pill affected maternal characteristics differently than abortion, we find suggestive results that availability of the pill lowered abortions among young women.

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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. This contrasts with the large amount of work on the effects of access to abortion on fertility and children's outcomes. These studies show that access to legal abortion reduces fertility in the short term (Levine et al., 1999; Angrist and Evans, 1999) and long term (Ananat, Gruber, and Levine, 2007), decreases the likelihood a child is raised by a single parent (Gruber, Levine, and Staiger, 1999) and alters the cohorts who are born to these women on important margins such as total crime committed (Donohue and Levitt, 2001; Foote and Goetz, 2005), drug use (Charles and Stephens, 2006), and college graduation (Ananat et al., 2006).

These results raise 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 maternal characteristics of children born to these women.

We first examine the effects of pill access on maternal characteristics such as educational attainment, marital status, and type of career. We focus on maternal characteristics since they

are well-known to affect children's outcomes and since (in some instances) they are likely to be descriptive of a household's characteristics not just when the census was taken but also when a child was born or raised. Using data from the 1970, 1980, and 1990 Censuses, we find that early pill access led to an increase in the share of children whose mothers were married, college-educated, and had professional occupations. The effects are often economically significant: access to the pill decreases the likelihood that a child's mother is divorced by 13 percent and increases the likelihood that a child's mother has some college education by 8 percent. Further, the pill's effects on the average mother were sometimes very different from the pill's effects on 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. The effects of the pill on maternal characteristics are also in some instances different from the effects of abortion. These results allow us to test the theories proposed by Akerlof, Yellen, and Katz (1996) about the effects of pill access on single motherhood; we find evidence consistent with their models' prediction that births to never-married women will increase. Nonetheless, we find that the fraction of divorced mothers fell with pill access so that on net single-motherhood fell after the pill's diffusion.

We then examine how these changes in children's circumstances relate to changes in women's short and long term fertility behavior. This is itself an interesting issue, since (as discussed in Section IV) even the basic question of whether gaining access to the pill had an immediate effect on the birthrates of young women remains disputed. We consider this issue by exploiting variation within states and years in the ages for which the pill was accessible, generating new evidence on the fertility effects of pill access that is more conclusive than past studies.

We find that extending access to the pill to younger women in a given year led to lower birth rates for those women in the next year. The effect is robust to a number of specifications and indicates that access to the pill lowers young women's birth rates by about 10 percent. These results are made *stronger* by the inclusion of state-by-year indicators, supporting the arguments of Bailey (2006) and Goldin and Katz (2002) that legal diffusion of the pill can successfully be used for identification.

We further 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. These decreases in completed lifetime fertility are found at both the intensive and extensive margins. The long-term effects are smaller in magnitude than the short term results, which implies that impacts of the pill on household characteristics are driven both by retiming of births and by permanent reductions in motherhood and family size.

Finally, we consider whether increased availability of the pill led to fewer abortions among young women. The substitutability or complementarity of the pill and abortion is essential to understanding the pill's role as a fertility technology, and is also important in its own right; there is a contentious policy debate over the relationship between oral contraception and abortion. We use two different datasets on abortion incidence that employ different methods of information gathering and cover different time periods and legal regimes. While each dataset has shortcomings, we find in each instance a negative relationship between legal access to the pill and the frequency of abortion.

These results have a number of important implications. They extend past research on the impacts of the pill by verifying the impacts of the pill on women and comparing these effects to those found for mothers, a subset of women of particular policy interest. These results also

provide new tests for theoretical predictions made by Goldin and Katz (2002) and by Akerlof, Yellen, and Katz (1996). Going beyond past research, these results show that this large-scale diffusion of contraception did not increase abortion or birth rates for young women, matters 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 maternal characteristics. Section III examines the impact of the pill on child living circumstances in the context of women's childbearing, marital and human capital decisions. Section IV describes empirical estimates of the effects of access to the pill on short-term and long-term fertility and the relationship between pill access and abortion. 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 15 to 19. About 73 percent of ever-contracepting never-

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

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, in most states 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 portions of 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. The Pill and Maternal Characteristics

This section examines the impact of the pill's diffusion on marital and human capital decisions for women and for mothers. Outcomes of mothers are important to study since in many cases they are likely to be descriptive of a household's characteristics when a child was born or raised—for instance, if a mother has never been married or never completed college at the time she fills out the census, then she was not married or a college graduate when she gave birth to and raised her child. A large body of research has established that the human capital and marital characteristics of mothers can have important impacts on children's health and human capital.[3] Thus, the effect of fertility control specifically on those women who continue to have

[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] 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). Marital status is also widely believed to matter for child outcomes, including test scores

children—as opposed to those who use contraceptive innovations to avoid becoming mothers—is of particular policy interest.

Prior work on the pill has identified a relationship between diffusion and women's occupational and marital status. In the next subsection, however, we discuss why the impact of the pill on the average child's living circumstances remains an open empirical question.

III.A. *Potential Impact of the Pill on Child Circumstances*

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.[4] Publications in the popular press, such as Sylvia Ann Hewlett's (2002) book *Creating a Life: Professional Women and the Quest for Children*, suggest 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.

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 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

(Guidubaldi, Perry, and Cleminshaw, 1984), mental health (Hetherington and Clingempeel, 1989), and delinquency (Achenbach and Edelbrock, 1983).

[4] 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.

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). 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. Their findings are consistent with a model in which women who use the pill to invest in human capital will delay marriage but ultimately experience better marital matches.

However, their results do not have clear implications for how early pill access changes the circumstances of mothers. For example, some women may for various reasons choose not to use contraception; if, as theorized by Akerlof, Yellen, and Katz (1996), pill availability increases social pressure on non-contracepting women to engage in premarital sex without the promise of marriage should pregnancy occur, then marriage rates might decline among mothers. On the other hand, women with strong demand for children may take particular advantage of the pill's potential to improve match quality, leading (according to the theory laid out in Goldin and Katz's paper) to a decrease in the share of children with divorced parents. Since the Goldin and Katz theory applies primarily to women who initially use contraception, while the Akerlof, Yellen and Katz theory applies to women who forego contraception, the two theories are not incompatible:

pill access may ultimately both increase the fraction of children whose mothers are never-married *and* increase the fraction who grow up in two-parent households, while decreasing the fraction of children whose parents are divorced. The following subsection explores these issues by empirically examining the pill's impact on maternal characteristics.

III.B. *Results on Human Capital and Marital Status for Women and 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.

Of course, to the extent that women's current characteristics do not perfectly reflect their characteristics during their children's childhoods, there will be mismeasurement of childhood living circumstances. This is one 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 (beyond "never-married") and occupational status, on the other hand, are current characteristics that may mismeasure the living environments of offspring in childhood.[5]

As in 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. The units of observation are state of birth-by-year of birth cells. We use these cells of women to estimate the following equation:

$$Outcome = \delta Access + \beta X + \theta_s + \theta_y + \phi_{age} + T + \varepsilon \quad (1)$$

where *Outcome* represents the logged fraction of women in a cell who experience a given outcome; *Access* equals unity if a cell of women born in a given state and year had access to the pill before age 21[6]; X is a set of controls including percent of the state population that is black, the percent that is other nonwhite, and a dummy for access to legal abortion by age 18; and the terms θ_s, θ_y, ϕ_{age} and T are (respectively) state dummies, census-year dummies, women's age dummies, and state-specific linear trends.

[5] 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 more likely than current marital, educational, or occupational status to be a poor proxy for her status during her children's childhoods.

[6] The results are robust to using other cutoffs, such as access by age 18.

The variables used for *Outcomes* include: a direct measure of human capital attainment, the fraction of a cell that has completed college; indirect measures of human capital attainment, including the fraction reporting a professional occupation (as defined by Goldin and Katz[7]) and the fraction that reports being a doctor or lawyer; the fraction never married; the fraction divorced; and the fraction married.[8] Finally, we examine the fraction who, to use Goldin's phrase, "have it all"—those who report being married, having at least one child, and having a professional occupation.

We estimate equation (1) in two ways: we first estimate the equation for the average *woman*, and then we estimate the equation for the average *child's mother*. The estimates for the average woman use observations of state-cohort cells of women, weighted by the cell population. Estimates for the average child's mother separately measure outcomes not only by state and year of birth 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). These latter regressions consequently exclude women without children from the sample.[9] 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 1, along with dependent-variable means. 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

[7] 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)."
[8] The results are qualitatively similar if levels (rather than logs) of the dependent variables are used.
[9] As the children-ever-born variable plays a key role in this method, and this variable is not available in the 2000 Census, we cannot include the 2000 Census in the analysis.

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; only the coefficients on access to the pill and access to abortion are reported.

The first observation is 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 (1.2 percent and 1.5 percent, respectively) and to report being a doctor or lawyer (14 percent for each group) when they had early access to the pill. Both groups are also less likely to be divorced (11 percent and 13 percent). In three important ways, however, access to the pill had different, more positive effects on the average child's mother than on women in general.

A first difference is that early pill access appears to have led to a significant decline in marriage overall of about 0.6 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 0.96 percent. A second difference is that there is a marginally significant increase of 8.3 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.[10] The positive effect on mothers' education thus results from a smaller reduction in childbearing among contracepting college-graduate women than among other women; this is consistent with 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. A third difference is that pill

[10] 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.

diffusion has no significant effect on the fraction of women who "have it all"—children, professional careers, and husbands—but does lead to a significant 3.4 percent increase in the share of children whose mothers balance family and professional career.

The table also shows how these results compare to the impact of abortion access. For both women and the average child's mother, the results suggest that all types of contraception increased the fraction never married and decreased the fraction divorced. However, the pill has quite different effects from abortion in some cases. Unlike abortion, the pill raised the fraction of mothers with college education and professional occupations. We also find that the pill increased the fraction of women currently married, while abortion did not. The Goldin and Katz model suggests that outcomes in the marriage-market may be improved because women who invest in human capital obtain better matches. The results from the pill show that this could be the case for mothers. In contrast, the results for abortion suggest that mothers who avoided divorce did so not by obtaining a better marital match but by not getting married. There is also limited evidence of improvements in mothers' human capital for abortion. The extension of Goldin and Katz's model to mothers may thus work better for the pill than for abortion.

The results also provide mixed evidence for Akerlof, Yellen, and Katz's model. As predicted by their paper, we find that contraception access leads to an increase in mothers who have never been married. However, the pill also decreases the share of divorced mothers; this latter effect (given its larger base) dominates so that overall the pill led to more children living in two-parent households.

To summarize, we find that the pill impacted both the marital outcomes and human capital investments of the average child's mother, and that these effects were generally positive. These effects are quite different from effects on the average woman. The results from access to

the pill are also in some cases quite different from the effects of abortion access. The Goldin and Katz model accurately predicts the effects of potential mothers gaining access to the pill; the evidence for Akerlof, Yellen, and Katz's model is mixed. In the next section we further explore these findings by studying the pill's short- and long-term effects on fertility and whether access to the pill affected women's use of abortions.

IV. The Pill's Effects on Fertility and Abortion

IV.A. *The Pill and Short-Term Fertility*

One potential explanation for the results of Section III is that women gaining access to the pill delay childbearing from their youth until some future point, enabling them to secure a better socioeconomic situation. If this occurred, we would expect to see an immediate decline in births among young women after the diffusion of the pill. Previous research has been inconclusive as to whether or not such a decline took place. Bailey (2006) provides some limited evidence that access to the pill lowered the fertility of young women 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. We begin this section by examining 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. (We consider results from additional census years below.)[11] The data and specifications used here differ from those used in the prior section, because here we are not

[11] Use of the census is sensible given that it provided the estimates of Section III and will be used for the long term fertility results to come, but we have also examined short-term fertility with Vital Statistics data. Estimates with Vital Statistics data are close to estimates from Census data.

interested in women who have completed their fertility but instead are interested in the short-term effects of pill access on young women subject to diffusion. Our sample consists of women born between 1940 and 1965 and observed between the ages of 14 and 20 during the period 1960 to 1979, the time period when most states lowered the legal age of access for the pill.

To construct age-specific fertility rates, we need an estimate of the number of children born to women of a given age in each state and each year, and we need an estimate of the number of women of that age living in each state each year. We also need an estimate of whether these women had the ability to obtain a prescription for the pill at the time of conception. The number of children born in each state and year can be taken from the Census. We estimate the population of women of a given age in a given state and year based on a woman's state of birth. This allows us to avoid any potential endogeneity created by selective migration of women across time.[12] Finally, to estimate access to the pill at the time of conception, we assume a child born in a given year and state was conceived in the same state in the year prior to birth.[13]

Our baseline specification is similar to Gruber, Levine, and Staiger's (1999) approach to measuring the effect of abortion access on births. We estimate

$$Births = \delta Access + \beta X + \theta_s + \theta_y + T_s + \phi_{age} + \varepsilon \qquad (2)$$

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); X is a set of year-specific state-level controls including the

[12] To check for sensitivity of our results to this approach, we have also repeated these estimates using women's current state of residence in 1980, rather than state of birth, to estimate the population of women in each state each year. These latter estimates will capture the effect of migration to the fullest extent possible. Our results are essentially identical and fairly precise regardless of which method we use to estimate the population of women, suggesting that our approach is robust to any interstate migration that occurs among cohorts of women.

[13] 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.

insured unemployment rate, the crime rate, the percent of the population that is nonwhite, and per-capita personal income; state, year, and mother's age dummies and state trends are all included. The unit of analysis is all of the women of a given age living in a given state and year. The sample includes women ages 14 to 20 between the years 1960 and 1979. The regressions that use logged number of births as the dependent variable also include the number of women in a cohort, in logs, as a regressor. The coefficient of interest is δ, which measures the effect of access to the pill on fertility.

The results from equation (2) 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.[14] The results indicate that access to the pill had the immediate result of decreasing births among the affected cohorts. The levels estimate suggests that the birth rate declined 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.

The last columns in Table 2 introduce state-by-year fixed effects. These regressions thus exploit the additional variation in age-specific changes in pill access by including state-year fixed effects as controls. These state-year dummies will absorb any phenomena in a state in a given year that would have affected all teenagers similarly, 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.

The results from this approach, 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

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

births among the affected cohorts. In all cases the result is more negative and more significant than under 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. The fact that controlling for unobserved phenomena varying across states and time does not weaken the results strengthens the case made by Bailey (2006) and Goldin and Katz (2002) that the legal diffusion of the pill can be used successfully for identification purposes.

The results thus show that the pill caused a clear short-term decrease in the fertility of women under 21. This finding itself provides important evidence on the effect of the pill on child well-being, as teenage childbearing is widely believed to be bad for children. The result also contradicts speculation by past researchers that the pill's diffusion increased teenage childbearing in the 1970s. However, these results cannot clarify whether the effects of the pill were driven by selection into motherhood or by changes in women's timing of births. The next subsection addresses this uncertainty by examining long-term impacts of the pill on fertility.

IV.B. *Lifecycle/Completed Fertility*

Most prior work has not considered the long-term fertility effects of the pill.[15] 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 effects of the pill on child living circumstances will 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 both the extensive margin (some

[15] 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.

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

As in Section III, we will consider outcomes of women nearing or at the end of their fecundity. We again will 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. The units of observation are again state-cohort cells, where a cohort is all of the women born in a given year. We estimate the model

$$Fertility = \delta Access + \beta X + \theta_s + \theta_y + \phi_{age} + \varepsilon \quad (3)$$

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)[16], 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. Regressions are weighted by cell population and standard errors are clustered at the state level.

The results are shown in Table 3. The effect of early pill access on the fraction of women with children (shown in Panel A of Table 3) is negative and significant in all specifications, 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. As shown in Panel B of Table 3, 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 (from a base of 2.78). Our results in Panel B are somewhat larger than those found by Ananat, Gruber, and Levine

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

(forthcoming) for abortion access, suggesting that oral contraceptives may have stronger effects on childbearing at the intensive margin than does legal abortion.

Taken together, Tables 2 and 3 suggest that the pill had significant impacts on fertility in both the short and long run, and that these impacts operated on both the intensive and extensive margins. 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. The increases in childlessness are also similar to those found in Ananat, Gruber, and Levine (2007) 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. However, the estimates in Section III suggested that abortion and the pill had very different effects on some maternal characteristics. But while the average woman who uses the pill looks different from the average woman using abortion, some women could view the two technologies as substitutable. The next subsection explores the relationship between pill access and abortion.

IV.C. *The Pill and Abortion*

In this subsection we 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.[17] 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

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

the availability of contraception.[18] We know of no work in any discipline which exploits birth control's diffusion to examine its relationship with abortion.

We use two data sets for this investigation. 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,[19] 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 Centers for Disease Control began to collect systematic annual data on abortion, diffusion had already occurred in most states.

Using the NSYW, we estimate the equation

$$Abortion = \delta Access + \beta X + \theta_s + \phi_{age} + \varepsilon \tag{4}$$

where *Abortion* is a dummy that equals unity if an individual has ever had an abortion;[20] *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

[18] 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.

[19] 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.

[20] 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 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.

(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.

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 in equation (4) 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 woman's access to the pill at the time she was making the decision to become sexually active.[21]

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

[21] We have also 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.

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 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. 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. [22]

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 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 include 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).

[22] 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 -0.15 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).

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:

$$Abortion = \delta Access + \beta X + \theta_s + \theta_y + \varepsilon \quad (5)$$

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, 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 (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 lowers 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 second column reports abortions per live birth, in logs. The third column also uses logged abortions per birth but now the cohorts are weighted by live births. The results are very close to before. 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: the dependent variable is the log of the number of abortions while the number of women (in logs) is added to the right hand side. The coefficient is very similar to before.

In summary, the results of this subsection indicate that access to the pill reduced young women's use of abortion in the short run. The datasets used for this investigation have deficiencies, making a definitive conclusion difficult. However, it is suggestive that the negative relationship between abortion and pill access among teens is visible in two datasets, is robust to measurement both before and after the legalization of abortion, and appears when relying either

on individual survey data or on aggregate figures of legal abortions.

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. We find that access to the pill increased the likelihood that a child's mother was well-educated, pursued a professional career, and was married. These effects differ from the pill's effect on the average woman and from the effects of abortion.

Using a more detailed specification than prior work, we then 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. 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.

While our main interest in this paper is identifying and understanding the effect of the pill on the number and parental 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. There is disagreement on whether improved access to contraception lowers women's abortion rates (as argued by Cohen, 1998) or raises them (Smith, 1993). While our work suggests that access to the pill did lower abortion rates among young women in the late 1960s and throughout the 1970s, this finding 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 subsection III.B. 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 the results in Table 2 suggest the effects of these two technologies on outcomes are in some cases very different.

These results also have implications for the well-known increase in the birth rates of young single women in the 1970s. As noted in the U.S. Department of Health and Human Services in 1980, "teenagers [were] 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.

As discussed in Section III, the results here show the pill attenuated the observed trend in single-parent households instead of causing it. It may be possible to reconcile these results with some of the above claims if out-of-wedlock births do not translate over time into more single-parent households. But Akerlof, Yellen and Katz (1996) themselves argue that many children

born out of wedlock remained in single-parent households years later, and became *more* likely to stay in single-parent households after the pill's diffusion (cf. pages 289-290). More research is clearly needed to assess historic trends in out-of-wedlock birth rates and single parenthood among young women during this important period.

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Table 1

Effect of Access to Fertility Control on Women's and Mothers' Characteristics

		Women	Kid's Moms
Fraction with Professional Occupation (logged)	Pill	0.0115	0.0151
		(0.0047)	(0.0058)
	Abortion	0.0244	-0.0016
		(0.0076)	(0.0078)
	Pop. Mean	*30.1%*	*32.1%*
Fraction Doctors or Lawyers by Occupation (logged)	Pill	0.1362	0.1409
		(0.0654)	(0.0859)
	Abortion	0.0958	0.3527
		(0.0673)	(0.0763)
	Pop. Mean	*0.4%*	*0.2%*
Fraction Never Married (logged)	Pill	0.1366	0.2696
		(0.0152)	(0.0484)
	Abortion	0.1733	0.3923
		(0.0192)	(0.0555)
	Pop. Mean	*9.3%*	*2.2%*
Fraction Currently Divorced (logged)	Pill	-0.1100	-0.1290
		(0.0466)	(0.0517)
	Abortion	-0.1701	-0.1383
		(0.0521)	(0.0510)
	Pop. Mean	*11.9%*	*10.5%*
Fraction Currently Married (logged)	Pill	-0.0066	0.0096
		(0.0022)	(0.0040)
	Abortion	-0.0186	-0.0014
		(0.0032)	(0.0044)
	Pop. Mean	*73.2%*	*80.2%*
Fraction College Graduates (logged)	Pill	-0.0364	0.0833
		(0.0420)	(0.0399)
	Abortion	-0.2309	-0.1529
		(0.0417)	(0.0320)
	Pop. Mean	*18.6%*	*11.7%*
Fraction of Women with Children, Professional Careers, and Spouses	Pill	-0.0115	0.0343
		(0.0056)	(0.0063)
	Abortion	-0.0302	-0.0001
		(0.0064)	(0.0083)
	Pop. Mean	*20.6%*	*27.2%*

Notes: Standard errors in parentheses. Each coefficient is for access to the pill or to abortion before age 21; each pair of pill/abortion coefficients are 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, controls for the proportion of the cohort that is African American and that is other nonwhite, and linear state trends. The first column represents the effect of access before age 21 for the average woman. The second column represents the effect for the average child's mother (see text).

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

The Pill and Lifecycle Fertility

Panel A

	Fraction of Women with Children (logged)			
	(1)	(2)	(3)	(4)
Access to Pill before Age 21	-0.0454	-0.0409	-0.0494	-0.0385
	(0.0039)	(0.0061)	(0.0037)	(0.0041)
Controls for abortion Access?	No	Yes	No	Yes
State trends?	No	No	Yes	Yes

Panel B

	Number of Children among Women with Children (logged)			
	(7)	(8)	(9)	(10)
Access to Pill before Age 21	-0.0337	-0.0486	-0.0371	-0.0542
	(0.0127)	(0.0138)	(0.0136)	(0.0140)
Controls for abortion Access?	No	Yes	No	Yes
State trends?	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 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: Sexually-active women ages 16 and older in the 1971 National Survey of Young Women (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)	
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) (1)	Abortions per Birth (logged) (2)	Abortions per Birth Alternate Weighting (3)	Abortions per Woman (logged) (4)	Abortions (logged) (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)
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 does 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.