

**The Power of the Pill for the Marginal Child:**  
**Oral Contraception's Effects on Fertility, Abortion, and Maternal & Child Characteristics**

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**Abstract**

In this paper we ask how the diffusion of oral contraception to young unmarried women affected the number and characteristics of children born to these women. We find that early pill access led to immediate increases in the fraction of children born to poor households and in the probability of low birthweight, but over the long term it caused an increase in the share of children whose mothers were college-educated and a decrease in the share whose mothers were divorced. We investigate the mechanisms by which the pill led to these differential effects and find that access to the pill led to declines in short-term fertility rates for young women but negligible changes in lifetime fertility. The short-term impacts of the pill appear to be driven by a selection effect—upwardly mobile women opted out of early childbearing—while the long-term effects are driven by the indirect effect of the pill on women's marital and career outcomes. We contrast the effects we find on the short-term and long-term “marginal child” to the very different effects of abortion legalization, and also find suggestive evidence that availability of the pill lowered abortions among young women. While our results suggest that abortion and the pill are on average used for different purposes by different women, it does appear that on the margin women substitute from abortion towards the pill when both are available.

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## I. Introduction

A growing literature shows 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), improves the living circumstances of the average child (Gruber, Levine, and Staiger, 1999) and improves the adult characteristics of the cohorts who are born to these women.<sup>1</sup> 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—to young unmarried women had similar effects on fertility and child circumstances, and whether the pill serves as a substitute or complement to abortion when both are available. The goal of this paper is to address these questions.

We first examine the effects of early pill access on short-term fertility. There is as yet no consensus over the basic question of how improved access to contraception impacts the birthrates of young women: previous research has found negative, zero, and even positive effects, depending on specification and on the source of variation used. We exploit variation within states and years in the ages at which the pill was accessible to single women, generating new evidence on the fertility effects of pill access that is more conclusive than that offered in previous studies. We find that extending access to the pill to younger women in a given year lowers birth rates by about 10 percent for those women in the

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<sup>1</sup> Outcomes considered by this literature include total crime committed (Donohue and Levitt, 2001; Foote and Goetz, 2005), drug use (Charles and Stephens, 2006), and college graduation (Ananat et al., 2006).

following year; this effect is robust to a variety of specifications. These results are made stronger by including state-by-year indicators, lending further support to the arguments of Guldi (2006), Bailey (2006), and Goldin and Katz (2002) that the legal diffusion of the pill can successfully be used for identification of causal impacts of increased access to oral contraception.

We then examine characteristics of children born to young women and find declines in birth weight and increases in the fraction of children born to impoverished households immediately after diffusion of the pill. These results imply that reductions in fertility were not random throughout the population. Rather, the “marginal child,” whose birth was avoided due to the liberalization of access to oral contraception, would have had above-average characteristics.

Providing young women with improved access to the pill does not appear, however, to have caused them to experience significant changes in lifetime fertility, either at the extensive margin of selection into motherhood or at the intensive margin. In other words, the immediate fertility reduction reflects the postponement of above-average-quality births rather than their permanent avoidance. Thus the characteristics of children born *eventually* to women who had early access to the pill need not have worsened. Moreover, to the extent that women used the pill to enable greater investments in human capital and in marriage quality, the marginal forgone early birth may have been replaced by a later birth with better characteristics.

We investigate these eventual births, looking at whether early pill access led to changes in the living circumstances of children born to these women over a longer time horizon. A key challenge in identifying these effects is that in the decades following the pill’s diffusion some of the children born to such women will have become adults; most large datasets contain limited information that can be exploited to determine adults’ childhood circumstances or their mothers’ access to the pill at the time of conception. To surmount this challenge, we use 1970, 1980, and 1990 census data to examine lifetime fertility outcomes for women who were granted improved access to the pill. We then measure whether the children ultimately born to women with early pill access tended to have mothers with above- or below-average maternal characteristics. We find that the pill’s effects on the average child’s

circumstances were as strong as or stronger than its effects on the average woman's circumstances: large gains in college graduation and smaller gains in marriage quality appear to have accrued especially to women who eventually had children, although the findings on marriage quality are somewhat sensitive to robustness tests. The negative selection effect we observe for short-run child characteristics is ultimately mitigated by the long-term treatment effects of the pill on the timing of births and on women's investments in marital and human capital.

Given the large and controversial parallel literature on the effect of abortion access on many of these outcomes, we compare our findings with similar regressions using access to legal abortion. These comparisons show that abortion has stronger, more positive effects on short-term birth characteristics but weak long-term effects. We infer that the typical woman using abortion was more disadvantaged than the typical woman using the pill, and that abortion had less positive effects than the pill on women's long-term investments in marriage and human capital.

Lastly, after identifying these disparate effects, we investigate whether the pill and abortion were complements or substitutes for young women—that is, 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, as there is a contentious policy debate over the relationship between oral contraception and abortion. Using two different datasets, we find in each instance a negative relationship between legal access to the pill and the frequency of abortion. Despite the fact that our previous results suggest that abortion and the pill are on average used for different purposes by different women, it appears that, on the margin, women substitute from abortion towards the pill when both are available.

The remainder of the paper is as follows. Section II provides a brief history of the pill. Section III provides a theoretical framework for thinking about the potential effects of the pill on children, and an empirical framework for estimating these effects. Section IV presents our estimates. Section V concludes.

## II. A Brief History of the Pill<sup>2</sup>

The pill became the most common form of contraception for married women under 30 soon after its 1960 approval by the FDA,<sup>3</sup> but remained an unusual form of birth control for unmarried young women over the 1960s. Between 1971 and 1976, however, the share of ever-contracepting never-married women ages 18 and 19 who had used the pill rose from 36 to 73 percent (Zelnik and Kantner 1977).

This rise coincided with legal changes that granted easier access to obtaining the pill.<sup>4</sup> In most states in the 1960s, to obtain oral contraception without a guardian's consent a woman had to be a legal adult, i.e.: age 21 or over; or under 21 but married, pregnant, or already a mother.<sup>5</sup> Over the next decade, diffusion of the pill to unmarried women under 21 took place in all states, primarily through one of two channels. First, some states lowered the age of majority below 21. Second, some states expanded the legal rights of minors, with the result that women below the age of majority could obtain the pill more easily.<sup>6</sup> Guldi (2005), Bailey (2006), and Goldin and Katz (2002) all make the argument that these changes stemmed in part from passage of the 26<sup>th</sup> amendment to the U.S. Constitution (lowering the voting age to 18), which was itself passed partly due to debate on the legal rights of men drafted for the Vietnam War. Timing of these laws does not appear to be systematically related to changes in social attitudes toward women's sexuality or other phenomena that may themselves influence childbearing behaviors. Bailey (2006) presents evidence that the variation is unrelated to almost all observable state characteristics.<sup>7</sup>

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<sup>2</sup> The discussion here draws on Goldin and Katz (2002), Bailey (2006), Asbell (1995) and Watkins (1998).

<sup>3</sup> See Table II-3 in Westoff and Ryder (1977) for data on contraception use by married women in 1965 and 1970.

<sup>4</sup> Goldin and Katz find that, in 1971, sexually-active never-married women ages 17-19 were ten percentage points (or 30 percent) more likely to report using the pill if they lived in a state granting pill access to minors, a sizeable effect. Further, Zelnik and Kantner (1977) show that the rise in pill use in the 1970s among never-married teenagers coincided with large declines in women using no form of contraception during intercourse (cf. Table 12 in their paper), suggesting that this rise in pill use did not simply crowd out other contraception.

<sup>5</sup> Laws restricting the birth control pill appear more likely to be enforced than laws restricting some other forms of contraception. 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 both a prescription from a physician and sale by a pharmacist (Bailey, 2006).

<sup>6</sup> For the sake of brevity we will sometimes refer to the effects of these law changes as "the effects of early access to the pill" or just "the effects of the pill." Both terms are merely shorthand for the legal diffusion described above.

<sup>7</sup> She tests many economic, social, and demographic characteristics, including: the fraction of the population that is black, that is poor, that lives in a household with a radio or with various other appliances, or that is living on a farm; the fraction of men or of women ages 22-30 in the labor force; the fraction of women that are ages 15-21, 22-30, or 31-45; mean education for women; the state's casualty rate in Vietnam; and whether the state is in the South.

In portions of our empirical analysis, we further address concerns about potential legislative endogeneity by exploiting variation in the specific age of initial access to the pill for each state and year.<sup>8</sup> This variation allows us to include state-by-year fixed effects, a methodological innovation that captures not only observed but also unobserved heterogeneity between states over time.<sup>9</sup>

### **III. Conceptual and Empirical Framework**

#### *IIIA. Potential Impact of Pill Access*

A variety of theories, from different parts of economics, can be used to make predictions about the possible effects of these laws on women and children. However, to our knowledge these theories have not been considered jointly. In this section we integrate the predictions of these frameworks, and then outline our empirical strategy for testing these predictions.

One framework, which begins with the empirical work of Goldin and Katz and includes Bailey (2006) and Guldi (2005), identifies positive effects for women from pill diffusion. Many of these effects come through diffusion allowing women to avoid early births. Thus, how effects for women translate into effects on child outcomes depends on whether diffusion caused births to be permanently avoided or merely delayed, and on the selection of women of different types into delay/avoidance. In the extreme case, if permanent reductions in fertility accrue particularly to positively selected women, there could be a concomitant negative selection into motherhood, so that positive effects of pill diffusion on women's outcomes would translate into negative effects on average child outcomes. Even without extreme assumptions, since the benefits to women of pill access accrue over time, child outcomes most likely

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<sup>8</sup> Guldi (2005) provides age variation in the laws; that paper, however, does not exploit state-by-year fixed effects.

<sup>9</sup> For this innovation to work, it needs to be the case that women old enough to have improved legal access to the pill actually did have better access in practice than other young women. While Goldin and Katz (2002) note that a determined young woman was often able obtain contraception regardless of the age of majority or the legal rights of minors, there is, fortunately for our methodology, evidence that health centers and physicians were often aware of the age of majority and were more reluctant to offer contraceptive services to minors who were too young to request contraceptive services on their own (Briggs, 1966; House and Goldsmith 1972; Hulbert and Settlege, 1974).

differ between the short term and the long term.<sup>10</sup> In what follows, we consequently consider both long- and short- term effects of early pill access.

A second framework, the model of a quantity-quality tradeoff in children developed by Becker and others (Becker and Tomes 1976; Becker and Lewis 1973), suggests that the pill could facilitate a substitution away from quantity—and therefore towards quality—of children. This model implies that improving contraceptive technology may improve average child outcomes. However, this model is relevant to the case of pill diffusion only if it is empirically true that diffusion reduced fertility both a) permanently and b) on the intensive margin, neither of which is necessarily true. For example, according to Ananat, Gruber, and Levine (2007), the parallel process of abortion legalization caused permanent fertility to decline only on the extensive margin, so that the average child did not grow up with fewer siblings and quantity-quality substitution could not occur within families. We thus will consider both intensive and extensive long-term fertility responses below.

In a third framework, Akerlof, Yellen, and Katz (1996) argue that improvements in fertility control technology decrease a woman's bargaining power vis-à-vis a man with whom she conceives a child outside of marriage. They argue that the introduction of the pill may have contributed to a decline in “shotgun marriages” and an increase in single parenthood. However, those marriages that continue to occur may have higher match quality, leading to a reduction in divorce. Which effect dominates (reduction in marital uptake or reduction in marital dissolution) will determine how many children grow up in married-parent households, a characteristic many believe to be associated with positive outcomes for children. Below, we will be able to directly investigate whether early pill access increased the fraction of children whose mothers are never-married, the fraction whose mothers are currently married, or both.

In sum, analysis of the relevant economic research implies that the net effects of pill diffusion on children depend on whether: 1) there were short-term declines in fertility; 2) these short-term declines represented delays in fertility or permanent reductions; 3) reductions, if any, occurred on the intensive

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<sup>10</sup> There is also some evidence that age of mother per se affects child outcomes (Miller 2008; cf. Geronimus and Korenman 1992).

margin, the extensive margin, or both; 4) eventual fertility was concentrated among women who differed from the average, either ex-ante or in their investment responses after the introduction of the pill. We next describe the empirical framework we will use to begin our exploration of these issues.

### III.B Empirical Framework

We have two main specifications, both of which exploit the natural experiment of legal access to the pill by age; one we denote the “standard” approach and one is a more demanding “difference-in-difference-in-difference” approach. We discuss each in turn.

#### *The Standard Approach*

Consider the question of whether access to the pill impacts short-term fertility. Previous research (Guldi 2005 on pill diffusion; Levine et al. 1999 on abortion legalization) has used a “difference-in-difference”-style specification to measure the effect of fertility control access on short-term fertility:

$$Outcome_{asy} = \delta Access_{a-1,s,y-1} + \theta_s + \theta_s * y + \theta_y + \beta X_{sy} + \phi_a + \varepsilon \quad (1)$$

where in the case of fertility *Outcome* represents the birth rate of women of age *a* in state *s* and year *y*; *Access* is an indicator for whether these women had legal access to the pill in the prior year (the presumed year of conception); *X* is a set of controls that vary across states and time; and the terms  $\theta_s$ ,  $\theta_s * y$ ,  $\theta_y$ , and  $\phi_{age}$  are (respectively) state indicators, state-specific linear time trends, year indicators, and women’s age indicators. The unit of analysis is all of the women of a given age living in a given state and year. The coefficient of interest is  $\delta$ , which captures whether birthrates change for women who gained access to the pill in the prior year relative to women of the same age in other states and years.

#### *The Difference-in-Difference-in-Difference Approach*

While we will estimate equation (1), our preferred specification exploits a third difference: the fact that within states that had policy changes, some states increased access only for *some* young women, while other young women in the same state and year did not see their access change. Since the treatment varied by age within state and year, we can control nonparametrically for unobserved state-specific variation within year, rather than relying on state linear trends ( $\theta_s * y$ ) and observable state

characteristics (X). This specification is:

$$Outcome_{asy} = \delta Access_{a-1,s,y-1} + \theta_{sy} + \varphi_a + \varepsilon \quad (2)$$

where  $\theta_{sy}$  are flexible state-by-year indicators. Equation (2) amounts to a “difference-in-difference-in-difference” strategy that compares fertility outcomes in (a) states changing pill access to other states (b) before and after access changed and (c) between age groups of young women affected by the change in access relative to other age groups of young women. The state-by-year indicators difference-out most of the other right-hand-side controls used in equation (1), so equation (2) contains additional indicators only for women’s age and for pill access in the previous year. Where sufficient data are available, we estimate equation (2) as well as equation (1).

The above discussion focuses on short-term fertility as an example of the outcome of interest, but these specifications will be used to look at other outcomes as well. For some other outcomes we change the right-hand-side controls or the unit of analysis; such changes are discussed below. However, equations (1) and (2) capture the basic empirical framework for what follows.

## **IV. Results**

### *IV.A. The Pill and short-term fertility*

The most obvious potential effect of increased access to the pill is an immediate decline in births among young women. Previous research, however, has been inconclusive as to whether or not such a decline took place: Bailey (2006) provides some limited evidence that access to the pill lowers 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; finally, Arcidiacono, Kwaja, and Ouyang (2005) use post-diffusion data to argue that increased access to contraception leads to higher pregnancy rates among teenagers.

To examine whether access to the pill affected the likelihood that a young woman gave birth, we use a sample consisting of women born in the US between 1940 and 1965 and observed in the 1980

Census 5-percent individual public use microdata (IPUMS).<sup>11</sup> These women passed through the ages of 14 to 20 during the period 1960 to 1979, the time period when 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; an estimate of the number of women of that age living in each state each year; and 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 to women of a given age in each state and year is taken from the 1980 Census. We estimate the population of women of each age in each state and year based on a woman's state of birth.<sup>12</sup> This allows us to avoid any potential endogeneity created by selective migration of women over time.<sup>13</sup> Finally, to estimate pill access at the time of conception, we assume a child born in a given state and year was conceived in that state in the previous year. We begin by considering the fertility rates of women ages 14 to 20 (and thus potentially affected by diffusion); we consider outcomes for women over age 20 later on as a robustness check.

The results from estimating equation (1) on the 1980 Census microdata sample are shown in columns 1, 2, and 3 of Table 1. The results include all the regressors described above; following the work of Gruber, Levine, and Staiger (1999) (hereafter GLS), in these regressions the state-level vector of controls  $X$  includes statewide per capita income, the insured-unemployment rate, the crime rate, and the share of the state's population that is nonwhite. Residuals are clustered by state and each observation is weighted by number of women.<sup>14</sup> The results indicate that improved access to the pill had the immediate result of decreasing births the next year among the affected cohorts. The levels estimate, in column 1,

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<sup>11</sup> We have also examined short-term fertility with Vital Statistics data; estimates with Vital Statistics data are close to the estimates from Census data shown here. We focus on the census because Vital Statistics data are available for fewer years and because they cannot be used for our analysis of long-term fertility.

<sup>12</sup> Thus, if 1,000 women were born in Alabama in 1950, we would estimate that there are 1,000 16-year-old women in Alabama in 1966.

<sup>13</sup> 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, or state of residence at the time a woman gave birth, rather than her state of birth, to estimate the population of women in each state each year. These latter estimates will capture the effect of permanent or temporary 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.

<sup>14</sup> Weighting by the number of children born in a cohort produces similar estimates.

suggests that the birth rate declined by 8.8 births for every thousand women (the sample mean is 71 births per thousand). The results are qualitatively similar using the log of the birth rate (column 2) or the log number of births (column 3), although the latter is only marginally significant. Estimates using the log of the birth rate, which are most precise, suggest a significant 11.6 percent decline in the birth rate.

Columns 4, 5, and 6 of Table 1 estimate equation (2). These estimates, which represent the “triple-differences” strategy, provide further confirmation that policies expanding access to the pill had the immediate result of decreasing births among the affected cohorts. In all cases the estimated coefficients roughly double; each result is more negative and more significant when we include these nonparametric state-by-year effects. 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 largest and most precise. The results thus show that the pill caused a clear short-term 10- to 20-percent decrease in the fertility of young women. Moreover, 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. With 95 percent confidence, even when using our more conservative estimates we can rule out declines in the birthrate of less than 5 percent.

#### *IV.B. The marginal child in the short-term*

We next explore whether the short-term decreases in young women’s birthrates immediately after diffusion of the pill translated into immediate changes in the health and economic circumstances of the children being born. A similar question has been extensively considered by economists studying abortion. Starting with work by Gruber, Levine, and Staiger (1999), research has shown that legalizing access to abortion decreases short-term fertility, and that the averted births represent children who would have been poorer and of lower-birth weight than average. However, until now there has been no consideration of whether or not the pill had similar immediate effects.

For these regressions, the unit of analysis shifts from cohorts of women (as in Table 1) to cohorts of children. Following GLS (1999), we take data on child living circumstances from a sample that

includes all US-born children ages 1 to 16 living with their biological mothers in the 1980 five-percent IPUMS. These children were born between 1964 and 1980, the period when most diffusion occurred. Our data on child birth weight come from the Natality Detail Files from 1968 to 1980. These data are obtained from certificates filed for births occurring in each state. During 1968-1971, the data represent a 50-percent sample of certificates. From 1972 on, some states provide a 100-percent sample instead.<sup>15</sup>

With both datasets, we focus on children born to women under 21 in order to identify immediate changes in offspring characteristics when women first got access to the pill. We aggregate children into cohort groups, where a cohort is based on year of birth, state of birth, and mother's age at birth. We then estimate equation (1), where the dependent variable *Outcome* represents (for the census) the logged share of children in the cohort that are in single-parent households, in poverty, or on welfare, or (for Natality Detail Files) the logged share of a cohort with a birth weight below 2500 grams.<sup>16</sup> Following GLS, the right-hand-side controls in  $X$  include: statewide per capita income, the insured-unemployment rate, the crime rate, and the share of the state's population that is nonwhite. Regressions are weighted by the size of the cohort and the sampling percentage, and residuals are clustered by state.

The results of this estimation are shown in the first four columns of Table 2. The first two columns show regressions on the fraction of children living in households on welfare and the fraction living in single-parent households as of 1980; the coefficients on mother's access to the pill at conception are small and insignificant. Column 3, however, shows that the fraction of children living in impoverished households (as of 1980) is 2.6 percent *higher* if the children were born to young women who had improved legal access to the pill at the time of conception than if they were born to otherwise similar women who did not have early access. Column 4 shows that the fraction of low-birth-weight children increases with pill access by about 1.4 percent.<sup>17</sup>

The last four columns in Table 2 use the "triple-differences" approach, estimating equation (2).

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<sup>15</sup> Our weights for the Vital Statistics data are adjusted to account for whether a state provided a 50- or 100-percent sample of births in a given year.

<sup>16</sup> Estimates in levels are qualitatively similar to those in logs, although estimates in levels for the census data are somewhat less precise than estimates in logs.

<sup>17</sup> Average birth weight in the sample is 3220 grams, and the average cohort has 9.1 percent low-birth-weight births.

The state-by-year indicators absorb any phenomena in a given state and year that may have affected all children similarly, leaving only variation between children born to mothers with new access and those with stable access within each state and year. Once again, the results under this very strong test are in all cases larger in magnitude. The first two columns are again not precisely estimated (although the coefficient for the single-parent regression is marginally significant), while the estimates on poverty and low birth weight are again positive and statistically significant.<sup>18</sup>

If changes in the composition of births immediately after diffusion result from the differential avoidance of some types of births, then we can impute the characteristics of those avoided, or “marginal,” children by combining the information in Table 1 and in Table 2. We divide the change in average characteristics by the change in the birthrate to create a Wald estimate of the marginal child’s characteristics. Using a reduction in birth rates of 11.6 percent to scale the panel A estimates and a reduction of 21.2 percent to scale the panel B estimates, we calculate that the marginal child not born due to pill diffusion would have been 23 to 38 percent less likely to live in poverty and 11 to 12 percent less likely to be low-birth weight than the average child. By contrast, the marginal child not born due to abortion legalization, according to GLS, would have been 48 percent *more* likely to live in poverty, and 14 percent *more* likely to be low-birth weight. This comparison suggests that the typical woman who took advantage of the pill was more affluent than the typical woman who did not, and was even more advantaged relative to the typical woman who took advantage of abortion legalization.

#### IV.C. *Short-term falsification checks*

Table 3 presents a variety of falsification checks on short-term fertility and child living circumstances. The first two panels consider the question: is adoption of pill-diffusion legislation associated with trends in fertility and family circumstances among women who are outside of the age range addressed by the legislation? The results indicate that it is not. Panels (A) and (B) show results from estimating the basic equation (1) difference-in-difference regressions on the sample of women aged 21 to

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<sup>18</sup> The difference between estimates of welfare and of poverty may be driven by differential behaviors between groups of low income women; specifically, women receiving welfare may have had better access to the pill through Medicaid, while poor women ineligible for Medicaid couldn’t afford the pill even after it became legally available.

25 instead of women aged 14 to 20.<sup>19</sup> (These results are comparable to those in column 2 in Table 1 and the first 4 columns of Table 2). Indicators for whether teens (either 14-year-olds, in Panel (A), or 18-year-olds, in Panel (B)) have access to the pill in a given state and year have no significant relationships with the fertility of women aged 21 to 25, or with their children's living circumstances in 1980.

In Panels (C) and (D), we consider the question: do declines in teen fertility and selection of upwardly-mobile young women out of early childbearing *precede* legislation that diffuses the pill? The answer to this question is also no. Both panels estimate modified versions of equation (2)<sup>20</sup> and so can be compared with column 5 in Table 1 and the last 4 columns of Table 2. In Panel (C), in addition to including the indicator for the actual law change, we add a placebo-dummy for whether teens two cohorts in the future will receive pill access. For instance, if a state improved access to the pill for 17-year-olds in 1972, our placebo dummy would go from zero to unity for 17-year-olds in that state starting in 1970. This placebo yields small, insignificant, and inconsistently signed coefficients when predicting either the fertility of current cohorts of teens or the living circumstances of their children in 1980. At the same time, adding this indicator leads to little change in either magnitude or significance of the estimate of the main effect of early pill access. Similarly, panel (D) replaces the actual law change indicator with a dummy that goes from zero to unity beginning 5 years before a cohort will actually gain improved access; replacing the indicator for actual access with an indicator that pre-dates actual access by five years results in small, nonsignificant estimates.

All told, these falsification tests produce 20 coefficients with anticipated values of zero. Of these, none are significant at the conventional (5%) level, and only two are significant at the 10% level. These results support our evidence that access to the pill, rather than other underlying trends in states that allowed diffusion of the pill, caused changes in the birthrate and affected the average characteristics of the cohorts who were born. Whether these effects were permanent, however, depends on whether women

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<sup>19</sup> Because there is no within-year within-state variation in policies for 21- to 25-year-olds, we cannot estimate equation (2) for this population.

<sup>20</sup> Because the law changes provide variation within state and year by age for the 14-to-21 population, Panels (C) and (D) can include the state-by-year dummies necessary to estimate equation (2). Estimating panels (C) and (D) with the less-rigorous equation (1) specification yields qualitatively similar estimates.

avoided these births permanently or merely delayed them, the question to which we turn next.

#### IV.D. *The Pill and long-term fertility*

Most prior work has not considered the long-term fertility effects of the pill.<sup>21</sup> As discussed earlier, however, it is important to identify whether the births that are avoided by young women who get early access to the pill are replaced at a later age, or instead are permanently avoided. It is also important to understand whether forgone births represent changes on the extensive margin of selection into motherhood and/or changes in the intensive margin of family size.

To determine the effects of pill diffusion on permanent fertility, we examine reports of “children ever born” in the Census. 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 inclusion of women born as early as 1920 follows Goldin and Katz (2002) and allows us a control group of women who completed their fertile years prior to the diffusion of the pill. The units of observation are state-cohort cells, where a cohort is all of the women born in a given year. Because in this case we have lifetime measures of fertility, there is no year-to-year variation within states between cohorts to exploit. Therefore we cannot include state-by-year indicators as in equation (2); instead, we estimate a modified version the equation used in Goldin and Katz (2002):

$$Outcome_{s,yob} = \delta Access_{s,yob} + \theta_s + \theta_y + \theta_{yob} + \beta X_{s,yob} + \phi_{y-yob} + \varepsilon \quad (3)$$

where  $yob$  denotes year of birth and  $y$  now denotes census year. This is equivalent to the estimating equation on p. 759 of Goldin and Katz, except that we add cohort fixed effects  $\theta_{yob}$  to their specification in order to absorb any unobserved changes that might have affected all women born in a particular year similarly.<sup>22</sup> *Outcome* is measured either as: the logged fraction of women in the cell who have any children, the logged number of children ever born among those who have at least one child, or the logged family size of the average child (derived by weighting the regression by number of children in the

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<sup>21</sup> 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.

<sup>22</sup> Without the addition of cohort fixed effects, we found that estimates often failed falsification tests.

family). Other than the cohort fixed effects, the right-hand variables are defined entirely as in Goldin and Katz: *Access* is an indicator for legal access to the pill before age 21<sup>23</sup>, the controls in  $X$  include percent of the state population that is black and percent that is other nonwhite, and all regressions include state  $s$ , census-year  $y$ , and cohort's current age ( $y-yob$ ) fixed effects. Regressions are weighted by cell population and standard errors are clustered at the state level.

The results, which are shown in Table 4, reveal fairly precisely estimated zero effects of early access to the pill on long-term fertility. With 95% confidence, we can rule out declines in motherhood greater than 0.4 percent, declines in the size of the average mother's family greater than 1.2 percent (or .032 children), and declines in the size of the average child's family greater than 0.7 percent (or .025 children). It appears that most if not all of the short-term reductions in fertility represented delay rather than avoidance of childbearing.

These findings of zero effects on fertility reduce the set of possible ways that the pill may have altered long-term child living circumstances. Without changes in the average child's family size, quantity-quality tradeoffs cannot have occurred. In addition, without changes in the overall probability of motherhood, large changes in selection into motherhood are unlikely. However, unchanged selection into motherhood makes it more likely that effects of the pill on the average woman's investments in human and marital capital, as identified by Goldin and Katz (2002) and by Bailey (2006), affected the average child as well. We explore the evidence for these changes in the next section.

#### IV.E. *The marginal child in the long-term*

We next examine whether the children ultimately born to women granted improved access to the pill had mothers with above or below average maternal characteristics.<sup>24</sup> Our focus on maternal characteristics is motivated by the large body of research establishing the importance of maternal

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<sup>23</sup> Our results are robust to using other age cutoffs.

<sup>24</sup> It would be interesting to explore how one's adult outcomes were affected by maternal exposure to the pill, but such a study would require a large dataset with information on adult outcomes, state of birth, and mother's age; we know of no such dataset. Our focus on maternal characteristics will nevertheless allow us to contrast the short-term effects described earlier with results on family characteristics derived over the decades following pill diffusion.

characteristics on children’s wellbeing.<sup>25</sup> If women who used early pill access to delay birth also used early access to realize improved educational or marital outcomes, then the pill may have had significant effects on child circumstances that are not evident in the period immediately after diffusion. By looking at maternal characteristics over time among women who received early access to the pill, we will be able to examine how improved access affected not just children born to young women, but also the family circumstance of children born to the same women later in life.

Our empirical investigation will again estimate equation (3). In this analysis, the variables used for *Outcomes* include: the fraction of a cell of women, or of mothers, who have completed college; the fractions never married, divorced and currently married; and the fraction who, to use Goldin’s phrase, “have it all”—those who report being married, having at least one child, and having a college education.<sup>26</sup>

We estimate equation (3) in two ways: we first estimate the equation for the average woman, and then 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 measure outcomes not only by state and year of birth but, further, by parity (i.e. number of children born). These are a re-weighted version of the regressions on the average woman that use state-cohort-parity cells as the unit of observation and are weighted by total children born to that cell (parity\*cell population) rather than cell population.<sup>27</sup> By comparing the effects on the average woman to the effects on the average child’s mother, we can impute how much of the pill’s treatment effects on women’s investments accrued among women who had children, and hence how the pill affected the living circumstances of the average child.

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<sup>25</sup> For example, work has shown a relationship between mother’s education and child outcomes (Currie and Moretti 2003; Oreopoulos, Page and Stevens, 2006; and Chevalier, 2003); marital status is also widely believed to matter for child outcomes, including test scores (Guidubaldi, Perry, and Cleminshaw, 1984), mental health (Hetherington and Clingempeel, 1989), and delinquency (Achenbach and Edelbrock, 1983).

<sup>26</sup> If women’s current characteristics do not perfectly reflect their characteristics during their children’s childhoods, there could 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”) however, is a current characteristic that may mismeasure the environments of offspring in childhood.

<sup>27</sup> 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.

Results are shown in Table 5, along with dependent-variable means. 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. Residuals are clustered at the state level and corrected for heteroskedasticity. All regressions include the controls shown in equation (3); only the coefficients on access to the pill and to abortion are reported.

Looking at the first column, we find that early access to the pill caused a significant 2.2 percent increase in the share of women who are college graduates. It also appears to have led women who married into higher-quality matches: the share of women who are divorced falls by a significant 1.9% among those who received early access to the pill. While the coefficients on share of women married and never married are positive, they are not precisely estimated, preventing us from saying whether the fall in divorce is primarily driven by women not entering marriage or (upon entry) not exiting.<sup>28</sup> Finally, early access to the pill increased the share of women who invested in both human capital and family by a significant 3.7 percent.

Did the average child benefit fully from these changes in women's characteristics, or were these changes concentrated among women who had unusually few children? Turning to the right-hand column of Table 5, we see that the average child benefited fully (or perhaps even disproportionately; the point estimates for the average child's mother are in every case as large or larger than the point estimates for the average woman, although the coefficients are never statistically distinguishable). The average child became 4.5 percent more likely to have a college-educated mother, 2.2 percent less likely to have a divorced mother, and 5.3 percent more likely to have a mother who was both married and a college graduate.

#### IV.F. *Long-term falsification checks*

Table 6 presents a variety of falsification checks on long-term fertility, women's characteristics,

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<sup>28</sup> These regressions do not replicate the results in Table 5 of Goldin and Katz because we include cohort fixed effects. We are able to replicate those results by dropping the cohort fixed effects from our specification. However, such results were more sensitive to specification tests than the stronger specification used here (results available upon request).

and children's living circumstances; all checks are modified estimates of equation (3). The left-hand panels addresses the question: do changes in human and marital capital occur within states in the cohorts that *precede* those directly affected by legislation that diffuses the pill? It does not appear so. As shown in panels (A) and (B), adding an indicator to equation (3) for whether cohorts two years in the future will receive pill access leads to little change in the magnitude of the main effects for pill access on either women or mothers; however, the effects for both groups on divorce rates become insignificant. The early indicator has no predictive power for any outcome; estimates are generally small and imprecise for both the average woman and the average child's mother. As shown in panels (C) and (D), replacing the indicator for actual early access with an indicator that pre-dates actual access by five years also results in small, nonsignificant estimates for both the average woman and the average child's mother.

Panel (E) of Table 6 addresses the question: were changes in pill access for a given state and cohort related to other, unobserved changes for that state and cohort? The answer again is no. If that were the case, it is likely that the outcomes of men in that state and cohort would also be affected. (Of course, men's characteristics might eventually be affected by the pill indirectly because the pill affects their wives' and coworkers' characteristics, but such effects should be smaller as men will be affected by the characteristics of women in nearby cohorts as well as their own.) Panel (E) reports the results of estimating equation (3) on men instead of women; the effects are smaller and never significant. This result gives us confidence that the effects we identify for women come through access to the pill rather than through other unobserved state-cohort changes.

All told, these falsification tests produce 24 coefficients with anticipated values of zero. Of these, none are marginally significant at the conventional (10%) level. However, the falsification tests for divorce, in isolation, are not completely persuasive: as shown in panels (A) and (B), the coefficients on a 2-year-leading indicator for both women and children's mothers are larger than the main effects, and cause the main effects to become insignificant; in addition, the estimate on a 5-year-leading indicator, shown in panel (C), is nearly as large for women as is the true indicator, although the same does not hold for children's mothers. Overall, our results support our claim that access to the pill, rather than other

underlying trends in states that allowed diffusion of the pill, affected the average living circumstances of the cohorts who were born. A single exception is that the evidence that pill access affected children through their parents' divorce rates is suggestive at best. The evidence that pill access caused an increase in the net probability that a child had a married, college-educated mother, on the other hand, is quite robust.

#### IV.G. *The Pill and abortion*

A comparison of the coefficients in Table 5 can reveal how the long-term impact of the pill relates to the impact of abortion access. While the pill significantly raises the fraction of children with college-educated mothers (by 4.5 percent) and decreases the fraction whose mothers are divorced (by 2.2 percent), abortion does not. Rather, the only significant long-term effect of abortion appears to be to decrease the share of women and of children's mothers who are married. This comparison, in which pill access has significant positive effects on the average child's circumstances and abortion access has weak or negative effects, stands in striking contrast to the short-term results on child living circumstances, where abortion appeared to lead to positive selection into childbearing relative to the pill.

We can further explore the relationship between the pill and abortion by examining whether access to the pill affected the likelihood that a young unmarried woman had an abortion. We consider this question for three reasons. First, understanding the relationship between abortion and pill use will facilitate a better understanding of differences in the effects of abortion and the pill on the marginal child in the short- and long-term in Tables 2 and 5. Second, any evidence of substitutability between the pill and abortion will provide further evidence that women used the pill to avoid unwanted pregnancies, bolstering our fertility results in Tables 1 and 4. Third, this is an important question in its own right, as there is a contentious policy debate over the relationship between oral contraception and abortion.<sup>29</sup> 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

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<sup>29</sup> See Shorto (2006) for a non-academic account of the debate regarding contraception and abortion.

contraception use, rather than exogenous changes in the availability of contraception.<sup>30</sup> We know of no work in any discipline which exploits birth control's diffusion to examine its relationship with abortion rates.

We use two data sets for this investigation. The first is the 1971 National Survey of Young Women (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,<sup>31</sup> its early date and the retrospective data it provides are crucial given the timing of pill diffusion.

Using the NSYW, we estimate the equation

$$Abortion_i = \delta Access16_i + \beta X_i + \varepsilon \quad (4)$$

where *Abortion* is an indicator equal to one if individual *i* has ever had an abortion<sup>32</sup> and *Access16* is a measure of a respondent's access to the pill by age 16. *X* is a set of individual controls including indicators for: whether the respondent is white; is rural (does not live in an SMSA); lives in a low-income household; her religious affiliation; church attendance; the importance of church in her life; years of education; current student status; state of residence; years of age; and a set of age-by-census-region interactions. Standard errors are corrected for heteroskedasticity and clustered at the state level.

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 to current access because, since many state

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<sup>30</sup> 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.

<sup>31</sup> 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.

<sup>32</sup> 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. However, as long as misreports of abortion do not vary systematically with access to the pill, errors will merely bias estimates of the relationship between pill diffusion and the use of abortion towards zero.

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 over the years when she was at risk for a teen abortion.<sup>33</sup> Among the sample of sexually-active women ages 16 and older (for whom access at 16 is a relevant control) about 5 percent of the sample report ever having an abortion.<sup>34</sup>

Panel A of Table 7 reports linear probability regressions from the NSYW. The regressions in columns 1 and 2 show 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 third column adds an indicator for current access to the pill, whose coefficient is wrong-signed and insignificant. This estimate is consistent with the fact that abortion history is a function of cumulative behavior rather than current circumstances. The last column restricts the sample to sexually active women age 17 to 19, who are likely to have been at risk for abortion longer, making their histories more meaningful. This sample limitation strengthens our result.<sup>35</sup>

To further explore the relationship between pill diffusion and abortion, we also use data compiled by the CDC on legal abortions, which is available for the years 1974-1979. These data include information for at least one year from 41 states (listed in Table A2) on abortions for those aged 15 to 19; in the typical year data are available from about 37 states.<sup>36</sup> The CDC data provide the advantage of availability in multiple years, and do not rely on self-reported data from a small survey. However, they are only available after abortion legalization—a period when most states had already allowed pill to be

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<sup>33</sup> 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 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. We thus define early access as access by 16. While this definition is reasonable, it does not facilitate creation of the “access in 2 years” dummies used in Tables 3 and 6. However, we will be able to consider the effect of current (i.e., at the time of the survey) access.

<sup>34</sup> Appendix Table A1 shows the distribution of abortion responses by state and by pill access at the age of 16.

<sup>35</sup> One concern when interpreting these results is potential endogeneity of sexual activity to pill access. There is some work suggesting that other episodes of contraception diffusion did not affect women's sexual activity; cf. Chapter 5 of Levine (2004). Nonetheless, to test for sensitivity to this threat we have repeated these results using the full sample of women, both including and excluding a control on the right-hand side for whether a woman is sexually active. These results 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.

<sup>36</sup> Table A2 shows that states changing their laws during this period did so mostly in 1974 and 1975. This prohibits us from using the “access in 2 years” dummies used in Tables 3 and 6; for most observations we lack data two years before access to the pill was changed.

diffused—and they cannot be broken down by single year of age. This final drawback makes the previous specification, which relies on variation in access between teenagers within a state and year, infeasible. We thus estimate equations of the following form:

$$Abortion_{sy} = \delta Access15_{sy} + \beta X_{sy} + \theta_s + \theta_y + \varepsilon \quad (5)$$

where *Abortion* equals, for a given state and year, either: the number of abortions by women ages 15 to 19; the ratio of abortions to live births for women ages 15 to 19, in logs; or the ratio of abortions to women ages 15 to 19, in logs. The variable *Access15* measures young unmarried women’s access to the pill by age 15; we use this age cutoff because all states that changed their laws after 1973 did so by lowering the age at which a woman can obtain the pill from above to below 15. Moreover, this measure reflects complete access for a cell of 15- to 19-year-olds. The matrix *X* contains controls for: percent nonwhite, the insured unemployment rate, per capita income, and the crime rate (the controls GLS use in predicting the effects of abortion access).  $\theta_s$  and  $\theta_y$  represent state and year fixed effects. Standard errors are corrected for heteroskedasticity and clustered at the state level.

Panel B of Table 7 reports regression results from the CDC data. 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 logged number of abortions per woman ages 15 to 19 in a state and year. 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 suggest that on average access to the pill lowers the abortion rate from 27 abortions to per every 1,000 women to 22.

Column 2 reports abortions per live birth, in logs; column 3 repeats the regression in column 1 but controls for underlying trends in abortion usage by adding state-specific time trends. The results are persistent (column 2 is also robust to the addition of state-specific time trends). The last column considers a more flexible specification: the dependent variable is the number of abortions (in logs) while the number of women (in logs) is added as a control. The coefficient remains stable.

The deficiencies in the datasets used for this investigation that are outlined above make a

definitive conclusion difficult. Nevertheless, 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 whether relying on individual survey data or on aggregate rates of legal abortions. These results provide some evidence that the introduction of access to the pill reduced young women's contemporaneous use of abortion.

While our results on women's and children's characteristics suggest that abortion and the pill are on average used for different purposes by different women, it does appear that on the margin women substitute from abortion towards the pill when both are available. Taken together, our results are consistent with the hypotheses that 1) compliers to abortion access are, on average, negatively selected women who do not avoid pregnancy but do avoid unplanned births when abortion is available; 2) compliers to pill access are, on average, positively selected women who do not abort unplanned pregnancies but do avoid pregnancy when the pill is available; 3) always-takers of fertility control technology abort unplanned pregnancies when only abortion is available but prefer to avoid unplanned pregnancies through the pill when both abortion and the pill are available.

## **V. Conclusions**

In this paper we ask how the diffusion of oral contraception to young unmarried women affected fertility, family characteristics, and the prevalence of abortion; we consider both short- and long-term effects. Using a more flexible specification than prior work, we find that access to the pill led to a short-term decline in fertility among these women, consistent with Bailey (2006) but in contrast to some other prior work. We find evidence on reductions in abortion rates that suggest the pill reduced pregnancies even more than it reduced births. These pregnancy declines were temporary; over the long-term the pill had no effect on total childbearing.

The short-term decline in fertility led to immediate declines in the average birth weight and economic circumstances of children born to young women, but in the long-term early access to the pill increased the likelihood that a child had a college-educated, married mother. Together, these results—

effects of the pill on child characteristics that are negative in the short-run but positive in the long run, along with delayed childbearing—are compatible with a story in which “upwardly mobile” young women are especially likely to use the pill to postpone births, and in the meantime pursue better marital and educational outcomes. In such a scenario a selection effect will lead the short-run impact of the pill on cohort characteristics to be negative, as these women forgo having children, while the long-term treatment effects are more positive, as these women realize improved human capital and marital outcomes and then enter motherhood.

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Table 1  
*The Pill and Short-Term Fertility*

	Panel A: GLS Specification			Panel B: 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 (0.0026)	-0.1160 (0.0333)	-0.0396 (0.0249)	-0.0166 (0.0036)	-0.2116 (0.0426)	-0.0839 (0.0400)
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 each year 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 2  
*The Pill and Average Child Characteristics*

	Panel A: GLS Specification				Panel B: State/Year Dummies			
	Welfare Receipt (1)	Single parent (2)	Living in Poverty (3)	Low Birthweight (4)	Welfare Receipt (5)	Single parent (6)	Living in Poverty (7)	Low Birthweight (8)
Access to the Pill	0.0007 (0.0189)	-0.0101 (0.0158)	0.0265 (0.0136)	0.0140 (0.0059)	0.0055 (0.0205)	-0.0281 (0.0191)	0.0797 (0.0182)	0.0225 (0.009)
State-by-Year Controls?	Yes	Yes	Yes	Yes	No	No	No	No
State Dummies?	Yes	Yes	Yes	Yes	No	No	No	No
State Trends?	Yes	Yes	Yes	Yes	No	No	No	No
Quadratic State Trends?	Yes	Yes	Yes	Yes	No	No	No	No
Year Dummies?	Yes	Yes	Yes	Yes	No	No	No	No
Mother's Age Dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year Dummies?	No	No	No	No	Yes	Yes	Yes	Yes
Observations	6034	6034	6034	4201	6034	6034	6034	4201
R-squared	0.78	0.76	0.81	0.86	0.83	0.80	0.85	0.88

*Notes:* Standard errors in parentheses. Regressions are weighted by the number of births; residuals are clustered by state and corrected for heteroskedasticity. The regressions on welfare and poverty cover births for women ages 14 to 20 each year from 1965 through 1979; the regressions on birthweight cover the period 1968-1979. Redoing the welfare and poverty estimates from 1968-1979 produces similar (although somewhat less precise) results. The unit of observation in each regression are all children born in a given state and year to women of a given age. The “Access to the Pill” variable equals unity if the children were born to women who had legal access to birth control in the prior year. The proportions are in logs; estimates in levels produce qualitatively similar results. 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. The average cohort has 9.1% low-birth-weight births (low birth weight is defined as a birth weight under 2500 grams), 16.2% of children were in welfare-receiving households as of 1980, 26.9% of children were in single parent households as of 1980, and 21.6% children were living in impoverished households as of 1980.

Table 3  
*Falsification Tests for Short-Term Outcomes*

	Birth Rate (logged) (1)	Welfare Receipt (logged) (2)	Single Parent (logged) (3)	Living in Poverty (logged) (4)	Low Birthweight (logged) (5)
<i>Among women 21 to 25</i>					
(A) Access given to 14-year-olds	0.0036 (0.0149)	-0.0246 (0.0215)	-0.0128 (0.0169)	0.0094 (0.0238)	0.0067 (0.0068)
(B) Access given to 18-year-olds	-0.0083 (0.0119)	-0.0266 (0.0170)	-0.0136 (0.0105)	-0.0303 (0.0180)	-0.0127 (0.0069)
<i>Among women 14 to 20</i>					
(C) Main effect of pill access	-0.170 (0.0253)	0.0074 (0.0235)	-0.0217 (0.0302)	0.0536 (0.0236)	0.0222 (0.0081)
+ indicator for pill access in 2 years	-0.056 (0.0488)	0.0144 (0.0232)	0.0009 (0.0305)	-0.0035 (0.0283)	0.0004 (0.0101)
(D) Pill will be accessible at this age in 5 years	-0.0329 (0.0381)	0.0215 (0.0176)	0.0034 (0.0145)	0.0263 (0.0166)	0.0029 (0.0086)

*Notes:* Standard errors in parentheses. Regressions are weighted by the number of births; residuals are clustered by state and corrected for heteroskedasticity. The regressions on welfare and poverty cover the period 1965 through 1979; the regressions on birthweight cover the period 1968-1979. The bottom two panels include women age indicators and a full set of state-by-year indicators. In the top two panels, because there is no variation in access for women aged 21 to 25, we cannot include state-by-year indicators; instead we include state-by-year controls following Gruber, Levine, and Staiger (GLS) (the insured unemployment rate, the crime rate, the percent of the population nonwhite, and per-capita personal income).

Table 4  
*The Pill and Lifecycle Fertility*

	Fraction of Women with Children (logged) (1)	Number of Children among Women with Children (logged) (2)	Family Size of the Average Child (logged) (3)
<i>Population mean</i>	<i>0.84</i>	<i>2.78</i>	<i>3.66</i>
Access to Pill before Age 21	0.0011 (0.0027)	0.0018 (0.0066)	0.0082 (0.0076)

*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, age, and cohort fixed effects, linear state time trends, an indicator for early abortion access, and linear controls for the proportion of the cohort that is African-American and that is other nonwhite. The first column represents the effect of access to the pill before age 21 on the share of women who have at least one child. The second column represents the effect on the number of children among those who have at least one child. The third column represents the effect on the number of children in the family of the average child.

Table 5  
*Effect of Access to Fertility Control on Women's and Mothers' Characteristics*

		Average Woman (1)	Average Child's Mother (2)
<i>Human capital</i>			
Fraction College Graduates (logged)	Pill	0.0226 (0.0115)	0.0453 (0.0199)
	Abortion	-0.0008 (0.0175)	0.0037 (0.0383)
	<i>Pop. Mean</i>	<i>17.4%</i>	<i>11.7%</i>
<i>Marital capital</i>			
Fraction Never Married (logged)	Pill	0.0065 (0.0124)	0.0216 (0.0372)
	Abortion	0.0337 (0.0232)	0.0442 (0.1345)
	<i>Pop. Mean</i>	<i>8.5%</i>	<i>2.2%</i>
Fraction Currently Divorced (logged)	Pill	-0.0191 (0.0092)	-0.0222 (0.0128)
	Abortion	0.0847 (0.0668)	0.0928 (0.0510)
	<i>Pop. Mean</i>	<i>11.5%</i>	<i>10.5%</i>
Fraction Currently Married (logged)	Pill	0.0017 (0.0029)	0.0017 (0.0027)
	Abortion	-0.0147 (0.0058)	-0.0152 (0.0067)
	<i>Pop. Mean</i>	<i>80.6%</i>	<i>80.2%</i>
<i>Human capital + marital capital + children</i>			
Fraction with College Degrees, Spouses, and Children (logged)	Pill	0.0367 (0.0142)	0.0527 (0.0063)
	Abortion	-0.0091 (0.0211)	-0.0203 (0.0349)
	<i>Pop. Mean</i>	<i>10.8%</i>	<i>10.2%</i>

*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.

Table 6  
*Falsification Tests for Long-Term Outcomes*

	Effect of early access + indicator that women born 2 years later got early access				Indicator that women born 5 years later got early access		Effect of early access for men
	Average Woman		Average Child's Mother		Average Woman	Average Child's Mother	Average Man
	(A)	(B)	(C)	(D)	(E)		
	<i>Early Access</i>	+ <i>Access in 2</i> <i>years</i>	<i>Early Access</i>	+ <i>Access in 2</i> <i>years</i>	<i>Access in 5 years</i>	<i>Access in 5 years</i>	<i>Early Access</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Human capital</i>							
Fraction College Graduates (logged)	0.0201 (0.0102)	0.0097 (0.0086)	0.0415 (0.0182)	0.0136 (0.0162)	-0.0098 (0.0121)	-0.0127 (0.0151)	0.0105 (0.0075)
<i>Marital capital</i>							
Fraction Never Married (logged)	0.0051 (0.0119)	0.0056 (0.0113)	0.0349 (0.0433)	-0.0512 (0.0431)	0.0150 (0.0137)	0.0096 (0.0680)	-0.0092 (0.0115)
Fraction Currently Divorced (logged)	-0.0131 (0.0091)	-0.0238 (0.0156)	-0.0154 (0.0135)	-0.0241 (0.0147)	-0.0185 (0.0175)	-0.0135 (0.0175)	-0.0053 (0.0190)
Fraction Currently Married (logged)	0.0016 (0.0030)	0.0002 (0.0021)	0.0008 (0.0030)	0.0029 (0.0023)	-0.0011 (0.0023)	0.0010 (0.0026)	0.0007 (0.0033)
<i>Human capital + marital capital + children</i>							
Fraction with College Degrees, Spouses, and Children	0.0355 (0.0128)	0.0046 (0.0116)	0.0470 (0.0203)	0.0203 (0.0158)	-0.0098 (0.0148)	0.0076 (0.0183)	n/a

*Notes:* Standard errors in parentheses. Each coefficient is for access before age 21. In columns (1) and (2) and in columns (3) and (4), each pair of coefficients in a row is taken from a single regression; in columns (5), (6), and (7) 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, cohort, and age fixed effects, controls for the proportion of the cohort that is African-American and that is other nonwhite, and linear state trends.

Table 7

*Panel A: 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 (0.0238)	-0.048 (0.0255)	-0.0499 (0.0264)	-0.0622 (0.0303)
Access to Pill Now	-	-	0.0182 (0.0381)	-
All RHS Controls?	No	Yes	Yes	Yes
State 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 dependent variable equals unity if a respondent reports ever having an abortion, and equals zero otherwise. See text for a list of right-hand-side controls. 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).

*Panel B: Pill Diffusion and Abortion: Evidence from the CDC*

	Abortions per Woman (logged)	Abortions per Birth (logged)	Abortions per Woman (logged)	Abortions (logged)
	(5)	(6)	(7)	(8)
Pill available to whole sample	-0.1964 (0.1205)	-0.2036 (0.1180)	-0.1932 (0.1198)	-0.1755 (0.1131)
State Trends?	No	No	Yes	Yes
Year Dummies?	Yes	Yes	Yes	Yes
State Dummies?	Yes	Yes	Yes	Yes
Observations	209	209	209	209
R-squared	0.90	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. Regressions are weighted by the population of women ages 15 to 19 in a given state and year; weighting by the number of births to women ages 15 to 19 in a given state and year produces similar estimates. Adding trends to column 2 does not change the results, nor does removing trends from the last two columns. The mean of abortions per woman ages 15 to 19 (in levels) in the sample is 0.027.

Table A1  
*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 A2

*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.