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

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Abstract

This paper considers how oral contraception's diffusion to young unmarried women affected the number and parental characteristics of children born to these women. In the short-term, pill access caused declines in fertility and increases in both the share of children born with low birthweight and the share born to poor households. In the long-term, access led to negligible changes in fertility while increasing the share of children with college-educated mothers and decreasing the share with divorced mothers. The short-term effects appear to be driven by upwardly-mobile women opting out of early childbearing while the long-term effects appear to be driven by a retiming of births to later ages. These effects differ from those of abortion legalization, although we find suggestive evidence that pill diffusion lowered abortions. Our results suggest that abortion and the pill are on average used for different purposes by different women, but on the margin some women substitute from abortion towards the pill when both are available. JELNo. I0, J13, N12.

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), Miller (2005), and Hock (2007), 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.[i] 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),

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[i]One exception is Pantano (2007), which examines how women's exposure to oral contraception impacts crime among the children born to these women.

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.[ii] 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 (under age 21) 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. We exploit variation within states and years in the ages at which the pill was accessible to single women, and find that extending access to the pill to younger women in a given year lowers birth rates by about 8 percent for those women in the following year (off of a base of 74 births per 1,000 women); 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 (2008), Hock (2007), 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 that the diffusion of the pill to women under 21 led to short-term increases in the fraction of children born to economically disadvantaged households and in the fraction of low birth weight births. These results imply that reductions in fertility caused by pill diffusion were not uniform 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. For example, we estimate that the child not born to pill diffusion would have been 8 percent less likely than the average child to live in a household receiving welfare and 15 percent less likely to be low birth weight.

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. That is, if women used the pill to postpone motherhood to a more "ideal" time, the long-run effect of women's early pill access on the cohort of children they raised may have been positive. Moreover, to the extent that women used the pill to enable greater investments in human capital and in marriage quality, this post-pill "ideal" time might also represent an improvement over even optimal birth timing in the absence of the pill.

We investigate these eventual births, looking at whether early pill access led to changes in the average 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

[ii]Outcomes considered by this literature include total crime committed (Donohue and Levitt, 2001; Foote and Goetz, 2008), drug use (Charles and Stephens, 2006), and college graduation (Ananat et al., 2009).

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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 the effects of access to the pill to the effects of access to legal abortion. These comparisons show that abortion had relatively weak effects on short-term characteristics, and weak long-term effects as well. 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 women under 21. 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[iii]

The pill became the most common form of contraception for married women under 30 soon after its 1960 approval by the FDA,[iv] 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.[v] 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.[vi] 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

[iii]The discussion here draws on Goldin and Katz (2002), Bailey (2006), Asbell (1995) and Watkins (1998).

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

[v]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.

result that women below the age of majority could obtain the pill more easily.[vii] Bailey (2006) and Goldin and Katz (2002) make the argument that these changes stemmed in part from passage of the 26th 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.[viii]

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. This variation allows us to include state-by-year fixed effects which should capture not only observed but also unobserved heterogeneity between states over time.[ix]

III. Conceptual and Empirical Framework

III.A. 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), Hock (2007), and Guldi (2008), 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 women with above average characteristics, there could be a concomitant decline in the characteristics of the average mother, 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 differ between the short term and the long term. Moreover, there is some evidence that delaying childbearing to an older age *per se* changes child outcomes (Miller, 2008; Ashcraft and Lang, 2006); however, other research concludes that early childbearing has little effect on either children's or women's characteristics

[vi]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).

[vii]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.

[viii]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.

[ix]For this approach 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 legally request contraceptive services on their own (Briggs, 1966; House and Goldsmith, 1972; Hulbert and Settlage, 1974).

(Geronimus and Korenman, 1992; Hotz, McElroy, and Sanders, 2005). 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 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 explore 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. Most previous research (such as Levine et al. 1999) has used a "difference-in-difference"-style specification to measure the effect of fertility control access on short-term fertility. We use the following variation on this approach:

$$Outcome_{asy}=\delta Pill_{a-1,s,y-1}+\theta_s+\phi_y+\varphi_a+T_{sy}+\tau_{as}+M_{asy}+\beta X_{sy}+\varepsilon_{asy} \quad (1)$$

where in the case of fertility, *Outcome* represents the birth rate of women of age *a* in state *s* and year *y*. *Pill* is an indicator for whether these women had legal access to the pill in the prior year (the presumed year of conception); this is based on information provided by Melanie Guldi and used in Guldi (2008). The terms θ_s, ϕ_y and φ_a, are state indicators, year indicators, and women's age indicators.

This equation includes a number of controls to address trends in outcomes across place and time. The term T_{sy} is a set of state-specific time trends used to capture phenomena that vary across states over time. The term τ_{as} is a set of interactions of state indicator variables with a linear control for mother's age to address variation in the relationship between mother's age and outcomes across states. Further, the term M_{asy} is an age- and region-specific "moving average" of the dependent variable based on the years before and following the year in question. (Region refers to the four census regions.) Thus for all 19-year-olds in a given state and census region in 1975, M_{asy} is the average of the dependent variable for 19-year-olds across all other states in that region in 1974 and 1976.[x] The results are thus identified by state-specific deviations from broader trends among mothers of a certain age in nearby states.[xi] Lastly, X_{sy} is a set of controls that vary across states and time.

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 δ, 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 for unobserved state-specific variation between years, rather than relying on state linear trends and observable between-year changes in state characteristics. This specification is:

$$Outcome_{asy}=\delta Pill_{a-1,s,y-1}+\theta_{sy}+\varphi_a+\tau_{as}+M_{asy}+\varepsilon_{asy} \quad (2)$$

where θ_{sy} are state-by-year indicators. Equation (2) is identified from joint variation in fertility outcomes along three dimensions: (a) in states changing pill access relative to other states (b) after access changed relative to before and (c) for age groups of young women affected by the access change relative to other age groups of young women. The state-by-year indicators difference-out many of the other right-hand-side controls used in equation (1), including θ_s, ϕ_y, φ_a, T_{sy} and X_{sy}; the remaining controls still included in equation (2) are the age indicators and regional age-trend controls. 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 the

[x]We have also redone the results without the age-based trends and moving average terms; results without these strong age controls are typically qualitatively similar to the results below. This suggests that the short-run effects identified here are driven by changes in outcomes within age groups of women, and not minor shifts in births from younger women to slightly older women.

[xi]We also experimented with including age-by-state indicators and age-by-year indicators. The coefficients estimated from these specifications were often similar to those shown below, although the standard errors sometimes became very large or displayed sensitivity to changes in the specification in this alternative model.

impact of contraception access on young women's fertility: Bailey (2006) and Guldi (2008) provide some evidence that access to the pill lowers the fertility of young women; Arcidiacono, Kwaja, and Ouyang (2005) use post-diffusion data to argue that increased access to contraception may lead 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 1943 and 1965 and observed in the 1980 Census 5-percent individual public use microdata (IPUMS). These women passed through the ages of 14 to 20 during the period 1964 to 1979, the time period when states lowered the legal age of access for the pill.[xii]

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 using child- and maternal-age and child's state of birth. We estimate the population of women of each age in each state and year based on a woman's state of birth.[xiii] This allows us to avoid any potential endogeneity created by selective migration of women over time.[xiv] 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.[xv] We begin by considering the fertility rates of women ages 14 to 20 (and thus potentially affected by diffusion); later we consider outcomes for women over age 20 as a falsification check.

The results from estimating equation (1) on the 1980 Census microdata sample are shown in columns 1 and 2 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.[xvi]

The results indicate that improved access to the pill had the immediate result of decreasing births the next year among the affected cohorts. The results are similar for both logged birthrate and logged number of children born. The mean birthrate in the sample is 74 births per 1,000 women, suggesting that pill access leads on average to about 2 fewer births for each 1,000 women.

Columns 3 and 4 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. Each result is more negative when we include these state-by-year effects.[xvii] The results thus indicate that the pill caused a short-term 7- to 10-percent decrease in the fertility of young women, or

[xii]Given our moving-average controls, the regressions will report births from 1964 to 1978.
[xiii]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.
[xiv]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 their state of birth, to estimate the population of women in each state each year. These estimates will capture the effect of permanent or temporary migration to the fullest extent possible. Our results are very similar 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.
[xv]We have also estimated access using mother's state of birth; results from this approach are similar to those below although the standard errors in the Table 2 estimates are sometimes larger.
[xvi]Weighting by the number of children born in a cohort produces similar estimates.

about 6 fewer births for each 1,000 women. Goldin and Katz (2002) estimate that pill diffusion increases pill use by about 4 percentage points. Taken together, their results and the results here suggest that for every 1,000 women gaining access to the pill, there are about 40 women who subsequently begin to use the pill and there are between 2 and 6 fewer annual births for these women in the years immediately after diffusion. This number may not directly reflect the pill's efficacy as a contraceptive, because (for instance) the pill may have crowded out other contraception (although as discussed in Section II such crowd-out was likely moderate). But our results certainly seem reasonable when compared to estimates of how diffusion affected pill usage.

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 GLS (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, we take data on child living circumstances from a sample that includes all US-born children ages 1 to 15 living with their biological mothers in the 1980 five-percent IPUMS. Matching the births in Table 1, these children were born between 1964 and 1978, the period when most diffusion occurred. Our data on child birth-weight come from the Natality Detail Files (also called Vital Statistics data) from 1968 to 1980.[xviii] 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.

With both datasets, we focus on children born to women 20 and under in order to identify immediate changes in offspring characteristics when women 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.[xix] 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 residuals are clustered by state.[xx]

[xvii]We have also repeated these regressions in levels as a robustness test; the pill-access coefficient from equation (1) in this case is −0.0024 (0.0012) and for equation (2) it is −0.0057 (0.0017). The mean of the dependent variable is 0.074; these estimates are thus qualitatively similar to the results in Table 1.

[xviii]We have also examined short-term fertility with Vital Statistics data; estimates from those data are typically similar to those with census data. For our preferred fertility regression, shown in column 3 of Table 1, a regression on Vital Statistics data yields a slightly smaller pill coefficient of −0.041 (se = 0.014); compared to a census estimate (over the same time period) of −0.07 (0.022). A smaller fertility result would imply even larger marginal child effects of the kind discussed in Section IV.C; basing our marginal child effects on the census fertility estimates is in this sense a conservative approach.

[xix]Robustness checks using levels produce estimates that are qualitatively similar to those in logs.

[xx]In the Natality Detail Files, the cohorts are aggregated from individual-level data that were sampled at different rates across states and time. In this case we adjust the number of births in each cohort to match the population number of births for the cohort. We then weight the regressions by this adjusted number of births.

The results of this estimation are shown in the first four columns of Table 2. The first three columns show regressions on the fraction of children living in households on welfare, the fraction living in single-parent households, and the fraction living in impoverished households (all as of 1980). The coefficients on mother's access to the pill at conception are insignificant. Column 4 shows results for fraction low birth weight; the coefficient is positive and marginally significant.[xxi]

The last four columns in Table 2 use the "triple-differences" approach, estimating equation (2). 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. The results under this stronger specification are in every case larger in magnitude and in three cases the results are at least marginally significant. The point estimates for welfare, poverty, and low birth weight are positive, suggesting that the fraction of children experiencing these disadvantages was *higher* if the children were born to young women who had improved legal access to the pill at the time of conception. We provide more interpretation of the results on child outcomes momentarily.

IV.C. Short-term Extensions

Table 3 presents a variety of extensions of the short-term fertility and child living circumstances regressions. The first panel presents estimates where now a control for access to abortion (provided by Melanie Guldi) has been added. As with the access-to-the-pill variable, the abortion control variable equals unity if a cohort of women giving birth in a given year had legal access to abortion in the prior year. The table shows results from estimating equation (2) (which uses state-by-year dummies); as before, these estimates are similar but somewhat more precise than results based on the GLS specification. The first column shows results on logged birth rate; the results are thus comparable to column 3 in Table 1. (Results using logged number of children born, instead of logged birthrate, are given in the notes to Table 3.) The coefficient on access to abortion is negative, suggesting that births to young women fell by about 5 percent when they gained legal abortion access. The coefficient from access to the pill changes little with the addition of the abortion variable. The effects on fertility from pill and abortion access here are similar to the estimates for young white women reported in Guldi (2008), although our abortion estimate is somewhat smaller.

The next four columns in Panel (A) focus on outcomes for children, paralleling the estimates in columns 5 through 8 in Table 2. Again, the pill coefficients change little. The pill coefficients contrast with the abortion coefficients, however. The coefficients on abortion access are in all cases insignificant and in three out of four cases are in the opposite direction of the coefficient on pill access.[xxii] While imprecise, the abortion coefficient point estimates suggest that the effects of abortion on the characteristics of children born to young women may have differed from the effects of the pill.

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 using the information in Panel (A) of Table 3. We divide the change in average characteristics by the change in the birthrate to create a Wald estimate of the marginal child's characteristics. We calculate that the marginal child not born due to pill diffusion would have been 0.0072/0.0895 = 8.0 (standard error = 4.4)

[xxi]We also considered gestational age and average birth weight as outcomes; the coefficients from these regressions were imprecise.
[xxii]Angrist and Evans (1999), without controlling for pill access, similarly find little change in the overall outcomes of teens after legal abortion access.

percent less likely to live in a household receiving welfare, while the marginal child not born due to abortion legalization would have been 49.2 (25.5) percent *more* likely to live in a household receiving welfare.[xxiii] Further, the marginal child not born due to pill diffusion would have been 34.0 (13.6) percent more likely to live in a single-parent household, while the marginal child for not born due to abortion legalization would have been 9.2 (5.2) percent less likely to be in a single-parent household; the marginal child not born due to pill diffusion would have been 15.4 (10.6) percent less likely to be low birth weight while the marginal child not born due to abortion legalization would have been 10.0 (11.4) percent more likely to be low birth weight. These results suggest that children whose births were avoided due to pill access may have had different, and along some dimensions more advantageous, characteristics than children whose births were avoided by abortion access.[xxiv]

Panel (B) of Table 3 considers 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. Panel (B) shows results from estimating the basic equation (1) difference-in-difference regressions on the sample of women aged 21 to 25 instead of women aged 14 to 20.[xxv] (These results are comparable to those in column 1 in Table 1 and the first 4 columns of Table 2). Indicators for whether 14-year-olds have access to the pill or abortion 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. (Using access to 18-year-olds instead of 14-year-olds produces similar results.)

In Panel (C), 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. This panel estimates a modified version of equation (2)[xxvi] and so can be compared to the results in Panel (A). In addition to including the indicator for the actual law change, we add a placebo indicator for whether teens two years 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 indicator would go from zero to unity for this cohort two years before the law change (and stay at unity thereafter); we add an analogous placebo dummy for abortion access. The placebos yield estimates that are typically small, insignificant, and/or inconsistently signed when predicting fertility or living circumstances. Also, adding these placebos leads to little change in the estimated effects of either pill access or abortion access.

All told, the tests in Panels (B) and (C) produce 20 coefficients with anticipated values of zero. Of these, there are three coefficients significant at the 10 percent level; one of these coefficients (the abortion access placebo in the fertility regression in Panel (C)) has the same sign as the corresponding coefficient in Panel (A). Overall, these results support our evidence that access to the pill, not 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

[xxiii]We calculated the standard errors by estimating the fertility and outcome regressions simultaneously using seemingly unrelated regression. We then used the delta method based on the covariance matrix from this regression.

[xxiv]One exception are the results on single-parent families; it appears that a disproportionate share of births avoided through pill diffusion would have occurred to single women. This may be driven by the nature of the legal change that we use as identification, which was targeted to single women; married women had stable access to the pill across all states starting in 1965.

[xxv]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.

[xxvi]Because the law changes provide variation within state and year by age for the 14-to-20 population, Panel (C) can include the state-by-year dummies necessary to estimate equation (2). Estimating panel (C) with the less-rigorous equation (1) specification yields qualitatively similar results.

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

IV.D. The Pill and Completed Fertility

Most prior work has not considered the long-term fertility effects of the pill.[xxvii] 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 39 and 49. This age range follows previous work on the effects of abortion access on completed fertility (Ananat, Gruber, and Levine, 2007), which argues that, since fertility drops off markedly by age 39, looking at this age range allows the identification of nearly completed fertility.[xxviii] The inclusion of women born as early as 1920 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 of the equation used in Goldin and Katz (2002):

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

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 θ_{yob} to their specification in order to absorb any unobserved changes that might have affected all women born in a particular year similarly,[xxix] and we add an indicator for access to legal abortion prior to age 21. *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 born in the family). Other than the cohort fixed effects and indicator for abortion access, 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[xxx], 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.[xxxi]

[xxvii] 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; she cannot rule out declines as large as 0.23 children or increases as large as 0.11 children at the 5 percent level.

[xxviii] Long-term fertility effects of early pill access for women aged 30 to 49 are reported in Ananat and Hungerman (2007) and are highly similar to those reported here.

[xxix] Without the addition of cohort fixed effects, we found that estimates often failed falsification tests.

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

[xxxi] In addition to the equation in Goldin and Katz, equation (3) and its depiction of abortion access are also constructed to facilitate comparison to the long-run fertility outcomes of abortion access estimated in Ananat, Gruber, and Levine (2007) (although their identification strategy is somewhat different). Below, we compare our estimates to their findings.

The results, which are shown in Table 4, reveal fairly precisely estimated zero effects of early access to the pill on long-term fertility. At the 5% level, we can rule out declines in motherhood greater than 0.6 percent, declines in the size of the average mother's family greater than 1.0 percent (or 0.03 children), and declines in the size of the average child's family greater than 1.3 percent (or 0.05 children).[xxxii] It appears that most if not all of the short-term reductions in fertility represented delay rather than avoidance of childbearing. These results are in contrast to those for abortion access, also shown in Table 4; in a confirmation of the earlier findings of Ananat, Gruber, and Levine (2007), we find that access to abortion significantly reduces motherhood, and does so primarily among women who in the counterfactual would have had small families, so that average family size significantly increases.

These findings of zero effects of early access to the pill on lifetime fertility reduce the set of possible ways that the pill may have altered the long-term average living circumstances of the completed cohort of children born to affected women. Without changes in average 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), Hock (2007) and by Bailey (2006), affected the average circumstances of children as well. We explore the evidence for these changes in the next section.

IV.E. The Pill and Long-term Maternal Characteristics

We next examine whether the children born to cohorts of women who were granted improved access to the pill had, on average, mothers with better or worse maternal characteristics than did the set of children born to other cohorts. Our focus on maternal characteristics is motivated by the large body of research establishing the importance of maternal characteristics for the eventual outcomes of their offspring. By looking at maternal characteristics over time among women who received early access to the pill, we will be able to identify how improved access affected not just children born to young women, but also the family circumstances of children born to the same women later in life. [xxxiii]

We will again estimate equation (3), but will include women aged 30 to 49, as many key long-term outcomes, such as college education, can be measured reliably by the early 30s (Bailey 2006; Goldin and Katz 2002), and as the living circumstances of children are of interest even among those whose mothers have not completed their fertility.[xxxiv] 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.[xxxv]

[xxxii]We cannot estimate the logged number of children born for all woman because of zeros for non-mothers. However, a linear estimate produces an effect of access to the pill on children born per woman of −0.0033 (standard error 0.0113), a statistically and economically insignificant 0.1 percent decline in lifetime fertility.

[xxxiii]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. We therefore focus instead on maternal characteristics that have proven implications for their children's adult outcomes.

[xxxiv]Results are qualitatively similar but less precise when restricted to women 39–49.

[xxxv]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.

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 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.[xxxvi] 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.

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 2.3 percent increase in the share of women who are college graduates. This is similar to Hock's (2007) estimate of a three percent increase from adolescent pill access on BA completion for women over 30. The pill 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 percent among those who received early access to the pill; this is qualitatively similar but smaller in magnitude to Goldin and Katz's (2002) estimate. 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.[xxxvii] 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, suggesting that early access to the pill has increased the ability of women to "have it all" (Goldin 2004, 2006).

Turning to the right-hand column of Table 5, we see that 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. The larger point estimates for the average child's mother versus the average woman suggest that pill was particularly important for changing the investment strategies of women who intended to have multiple children.[xxxviii] Since there are no selection effects into fertility from early access to the pill, the pill's effects on mothers' characteristics presumably occurred because women, conditional on their fertility plans, used the pill to re-time pregnancies in order to facilitate greater marital and educational investment. Intuitively, an improved ability to re-time should have the largest impact on the investment success of women who had several births to re-time. That is, effects are larger

[xxxvi]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.

[xxxvii]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).

[xxxviii]The results for the average woman with at least one child (not shown) are in every case between those for all women and those for the average child's mother.

for the average child's mother because high-fertility women, who derived the most value from retiming, are disproportionately represented among children's mothers.

Table 5 documents the effects of delayed motherhood on maternal characteristics; the characteristics of the average child's mother are of particular interest because of the well-known effects of maternal characteristics on child outcomes. Aggregate changes in maternal characteristics can lead the generation of children born to an affected cohort of women to face different average life prospects than would a generation born to unaffected women. For example, when a cohort of women increases its educational attainment, the children of that cohort are less likely to be born prematurely (Currie and Moretti, 2003) or to repeat a grade (Oreopoulos, Page, and Stevens, 2006). When a cohort of women experiences a higher rate of divorce, the children of that cohort have lower educational attainment and family incomes as adults (Gruber, 2004). Hence the fact that the change in college graduation and divorce in the wake of pill diffusion appears concentrated among women who had multiple children suggests that the pill also may have led to larger changes in the next generation's outcomes than would changes of the same magnitude that were uniformly spread across all women.

IV.F. Long-term Falsification Checks

Table 6 presents a variety of falsification checks on long-term fertility, women's characteristics, 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. The coefficients on the control for abortion access (not shown) remain generally insignificant in the Table 6 regressions, as they were in Table 5.

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; see Hock (2007) for a full exploration of these indirect effects on men.) 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 Tables 4 and 5 can reveal how the long-term impact of the pill relates to the impact of abortion access. The two policies clearly have different impacts: abortion access significantly reduces the probability of motherhood, while the pill has no long-run effect on fertility. By contrast, pill access has much larger effects on the living circumstances of the average child than does abortion access.

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 outcomes in Tables 3 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.[xxxix] While some research outside of economics has considered the relationship between contraception, especially emergency contraception, and abortion, this work is inconclusive and often focuses on trends in contraception use, rather than exogenous changes in the availability of contraception.[xl] We know of no work in any discipline which exploits birth control's historic 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,[xli] its early date and the retrospective data it provides are crucial given the timing of pill diffusion.

Using the NSYW, we estimate the equation

$$Abortion_i=\delta Access16_i+\beta X_i+\varepsilon_i \tag{4}$$

where *Access*16 is a measure of a respondent's access to the pill by age 16, and *Abortion* is an indicator equal to one if individual i has had an abortion since age 16.[xlii] X is a set of individual controls including indicators for: whether the respondent is white; is rural (does

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

[xl] 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. Using variation in pharmacy access to emergency contraception in Washington state, Durrance (2009) finds access leads to lower abortion rates.

[xli] 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. We explored individual fixed-effect regressions with the NSYW but its small size and time span made this approach infeasible.

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; and a set of age-by-census-division 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 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.[xliii] Among the sample of sexually-active women ages 16 and older about 5 percent of the sample report ever having an abortion.[xliv]

Panel (A) of Table 7 reports marginal effects from probit regressions from the NSYW. The regressions in columns 1 and 2 show that among 16- to 19-year-olds in 1971, having had access to the pill since age 16 lowers the likelihood of ever having had an abortion since age 16. The third column adds an indicator for current access to the pill, whose coefficient is small 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 ages 17 to 19, who are likely to have been at risk for abortion longer, making their histories more meaningful. This sample limitation slightly strengthens the result. [xlv]

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 the notes to Table 7) on abortions for those aged 15 to 19; in the typical year data are available from about 37 states. The CDC data provide the advantage of availability in multiple years, and do not rely on self-reported data from a small survey. [xlvi] However, they are only available after abortion legalization—a period when most states had already allowed the pill to be diffused —and they cannot be broken down by single year of age.[xlvii] 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:

[xlii]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, this misreporting should not bias the estimates.

[xliii]We will also consider the effect of current (i.e., at the time of the survey) access to the pill. There are nine states in the sample where access by age 16 varies among respondents: Alabama, Illinois, Kansas, New Hampshire, New York, Oregon, Pennsylvania, Tennessee, and Virginia. Our ability to use other age cutoffs is hampered by a lack of variation in state laws in the sample; for instance only two states (Georgia and Illinois) report variation in access by age 15 in our sample.

[xliv]Appendix Table A1 in Ananat and Hungerman (2007) shows the distribution of abortion responses by state and by pill access at the age of 16.

[xlv]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; e.g. Chapter 5 of Levine (2004). Nonetheless, to test for sensitivity to this threat we have repeated these results using only women who are sexually active. These results are slightly stronger than those reported here (between −0.059 and −0.07) and are still precisely estimated.

[xlvi]The Alan Guttmacher Institute is another well-known source of abortion information, although it does not provide abortion counts by age, as discussed in Levine (2003) and Henshaw (1997). Since focusing on abortions to young women is crucial in light of the nature of the pill's diffusion, we use CDC data here.

[xlvii]There are seven states in the CDC data whose laws change during the available period: Arizona, California, Minnesota, North Carolina, Nevada, New York, and Utah. 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. See Table A2 in Ananat and Hungerman (2007) for details on the timing of diffusion in the CDC data.

$$Abortion_{sy}=\delta Access15_{sy}+\beta X_{sy}+\theta_s+\theta_y+\varepsilon_{sy} \tag{5}$$

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 *Access*15 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). θ_s and θ_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 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 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 also appear that on the margin women substitute from abortion towards the pill when both are available.

Understanding the relationship between pill access and abortion utilization among women under 21 helps us reconcile our findings on the short- and long-term effects on maternal characteristics from the pill with the parallel literature on abortion and childbearing among teens. Ashcraft and Lang (2006), Hotz, McElroy, and Sanders (2005), and Geronimus and Korenman (1992) find only modest (negative or even positive) effects of teen childbearing conditional on pregnancy on women's later outcomes, when compared to the outcomes of teens who abort or miscarry. Our estimates may differ because those papers estimate the effects of access to "crisis management" of pregnancies. If many women are ex-ante unsure of their willingness to abort an unintended pregnancy, then access to "crisis management" may not have a large effect on human and marital capital investment strategies. By contrast, access to the pill allowed planning; the knowledge that pregnancy can be reliably avoided plausibly has a much larger impact on investment strategies (Goldin and Katz 2002).

Taken together, our results and the results in previous literature are consistent with a strong set of hypotheses. First, those who respond to the introduction of abortion access are, on average, negatively selected women who do not avoid pregnancy but do avoid unplanned births when abortion is available; averting these "crisis pregnancies" may reduce the number of highly disadvantaged children who are born but has relatively little impact on women's measurable long-run outcomes. Second, those who respond to the introduction of pill access are, on average, positively selected women who do not necessarily abort unplanned pregnancies but do increase human and marital capital investment in response to the assurance that they can avoid early pregnancy. Third, there are women who, in the absence of the pill, would conceive and then abort an unplanned pregnancy but opt to instead 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 unmarried women under 21 affected fertility, family characteristics, and the prevalence of abortion; we consider both short- and long-term effects. 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: Difference-in-Difference Specification		Panel B: Triple Difference Specification	
	Birth Rate (logged) (1)	**Children (logged) (2)**	**Birth Rate (logged) (3)**	**Children (logged) (4)**
Access to the Pill	−0.0339 (0.0169)	−0.0203 (0.0130)	−0.0974 (0.0220)	−0.0698 (0.0185)
State-by-Year Controls?	Yes	Yes	No	No
State Dummies?	Yes	Yes	No	No
State Trends?	Yes	Yes	No	No
Year Dummies?	Yes	Yes	No	No
Age Controls?	Yes	Yes	Yes	Yes
State-by-Year Dummies?	No	No	Yes	Yes
Observations	4344	4344	4344	4344
R-squared	0.977	0.98	0.98	0.983

Notes: Standard errors in parentheses. parentheses Regressions are weighted by the number of women; residuals are clustered by state and corrected for heteroskedasticity. Weighting by number of children born in a cohort produces similar estimates. The regressions cover births for women ages 14 to 20 each year from 1964 through 1978. The mean of the dependent variable in the regression sample (in levels) is 0.074. The unit of observation in each regression is 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 born include the number of women in a cohort, in logs, as a regressor. Age controls include a set of mother’s age dummies, a set of moather’s-age-times-state trends, and a region-and-age-specific moving average of the dependent variable; the averages are based on the year prior and the year following the year in question (see text). The state-by-year controls in the difference-in-difference specification include 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 Birth Weight (4)**	**Welfare Receipt (5)**	**Single Parent (6)**	**Living in Poverty (7)**	**Low Birth Weight (8)**
Access to the Pill	0.0018 (0.0171)	−0.0107 (0.0148)	0.007 (0.0104)	0.0089 (0.0057)	0.0037 (0.0204)	−0.0298 (0.0188)	0.0334 (0.0143)	0.0132 (0.0084)
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
Year Dummies?	Yes	Yes	Yes	Yes	No	No	No	No
Age Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year Dummies?	No	No	No	No	Yes	Yes	Yes	Yes
Observations	4344	4344	4344	3492	4344	4344	4344	3492
R-squared	0.807	0.781	0.801	0.865	0.844	0.82	0.836	0.885

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 1964 through 1978; the regressions on birth weight cover the period 1969–1978. Redoing the welfare and poverty estimates from 1969–1978 produces similar (although somewhat less precise) results. The unit of observation in each regression is 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 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. Age controls include a set of mother’s age dummies, a set of mother’s-age-times-state trends, and a region-and-age-specific moving average of the dependent variable; the averages are based on the year prior and the year following the year in question (see text). The average cohort has 9.6% low-birth-weight births (low birth weight is defined as a birth weight under 2500 grams), 19.7% of children were in welfare-receiving households as of 1980, 30.4% of children were in single parent households as of 1980, and 26.0% children were living in impoverished households as of 1980.

Table 3

Extensions for Short-Term Outcomes

Panel A: ***Among women 14–20***

	Logged Birth Rate	Welfare Receipt	Single Parent	Living in Poverty	Low Birth Weight
Access to the Pill	−0.0895 (0.0220)	0.0072 (0.0197)	−0.0304 (0.0193)	0.0306 (0.0129)	0.0138 (0.0084)
Access to Abortion	−0.0476 (0.0135)	−0.0234 (0.0212)	0.0044 (0.0117)	0.0194 (0.0204)	−0.0048 (0.0048)

Panel B: ***Among women 21–25***

	Logged Birth Rate	Welfare Receipt	Single Parent	Living in Poverty	Low Birth Weight
Pill Access given to 14-year-olds	0.0027 (0.0126)	−0.0098 (0.0184)	−0.0085 (0.0168)	0.0132 (0.0208)	−0.006 (0.0072)
Abortion Access given to 14-year-olds	−0.0125 (0.0094)	−0.0171 (0.0156)	−0.015 (0.0113)	−0.0223 (0.0144)	−0.0104 (0.0070)

Panel C: ***Among women 14 to 20***

	Logged Birth Rate	Welfare Receipt	Single Parent	Living in Poverty	Low Birth Weightt
Pill Access	−0.088 (0.0229)	0.0052 [0.0188)	−0.0189 (0.0211)	0.0247 (0.0124)	0.0126 (0.0088)
Abortion Access	−0.043 (0.0157)	−0.0218 (0.0192)	0.0046 (0.0120)	0.0175 (0.0206)	−0.0046 (0.0048)
Pill Access Placebo	0.0504 (0.0201)	−0.0036 (0.0173)	0.0348 (0.0130)	−0.0191 (0.0164)	−0.0083 (0.0078)
Abortion Access Placebo	−0.0425 (0.0215)	−0.0141 (0.0277)	0.0123 (0.0119)	0.0066 (0.0247)	0.0122 (0.0093)

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 1964 through 1978; the regressions on birthweight cover the period 1969–1978. Panels A and C are based on the specification using the full set of state-by-year indicators. In Panel B, 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). Redoing the Panel A and C results with the GLS specification produces similar estimates. The Panel A coefficients on pill and abortion access when using logged number of children are −0.064 (0.02) & −0.034 (0.02), for Panel B they are −0.002 (0.01) & −0.012 (0.009) and for Panel C −0.065 (0.002) & −0.029 (0.016); the placebo pill and abortion dummies are 0.030 (0.02) & −0.041 (0.02). Redoing Panel B using access for 18-year-olds produces similar results.

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)
Population mean	*0.86*	*2.53*	*3.99*
Access to Pill before Age 21	0.0038 (0.0047)	0.0055 (0.0079)	0.0266 (0.0200)
Access to Abortion before Age 21	−0.0176 (0.0040)	0.0335 (0.0079)	0.3357 (0.0266)

Notes: Standard errors in parentheses. Observations include women born in a given state and year between 1921 and 1960 and observed at ages 39 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, and linear controls for the proportion of the cohort that is African-American and that is other nonwhite. The first column represents the effects of access to the pill or abortion before age 21 on the share of women who have at least one child. The second column represents the effects on the number of children born among those who have at least one child. The third column represents the effects on the number of children born 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)
Human capital			
Fraction College Graduates (logged)	Pill	0.0226 (0.0115)	0.0453 (0.0199)
	Abortion	−0.0008 (0.0175)	0.0037 (0.0383)
	Pop. Mean	*18.6%*	*11.7%*
Marital capital			
Fraction Never Married (logged)	Pill	0.0065 (0.0124)	0.0216 (0.0372)
	Abortion	0.0337 (0.0232)	0.0442 (0.1345)
	Pop. Mean	*9.3%*	*2.2%*
Fraction Currently Divorced (logged)	Pill	−0.0191 (0.0092)	−0.0222 (0.0128)
	Abortion	0.0847 (0.0668)	0.0928 (0.0802)
	Pop. Mean	*11.9%*	*10.5%*
Fraction Currently Married (logged)	Pill	0.0017 (0.0029)	0.0017 (0.0027)
	Abortion	−0.0147 (0.0058)	−0.0152 (0.0067)
	Pop. Mean	*73.2%*	*80.2%*
Human capital + marital capital + children			
Fraction with College Degrees, Spouses, and Children (logged)	Pill	0.0367 (0.0142)	0.0527 (0.0217)
	Abortion	−0.0091 (0.0211)	−0.0203 (0.0349)
	Pop. Mean	*10.8%*	*10.2%*

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 21; 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 women born and that in a given state and year between 1921 and 1960 and observed at age 30 to 49 in the 1970, 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 (A)		**Average Child's Mother (B)**		**Average Woman (C)**	**Average Child's Mother (D)**	**Average Man (E)**
	Early Access +	*Access in 2years*	*Early Access +*	*Access in 2years*	*Access in 5 years*	*Access in 5 years*	*Early Access*
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Human capital							
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)
Marital capital							
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)
Human capital + marital capital + children							
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 pill 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 ages 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 an indicator for abortion access before age 21, 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

	Marginal Effects from Probit Regression on Having an Abortion			
	(1)	**(2)**	**(3)**	**(4)**
Access to the Pill by Age 16	−0.0267 (0.0152)	−0.0289 (0.0144)	−0.0289 (0.0145)	−0.032 (0.0183)
Access to Pill Now	-	-	−0.0045 (0.0168)	-
All RHS Controls?	No	Yes	Yes	Yes
Age Controls	Yes	Yes	Yes	Yes
State Dummies?	Yes	Yes	Yes	Yes
Observations	3507	3501	3501	2509

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

	Abortions per Woman (logged) (5)	**Abortions per Birth (logged) (6)**	**Abortions per Woman (logged) (7)**	**Abortions (logged) (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 dependent variable equals unity if a respondent reports having an abortion from age 16 on, and equals zero otherwise. See text for a list of right-hand-side controls. The age controls include a set of age-by-census-division dummies. Sample includes women ages 16 to 19. The last column restricts the sample to women ages 17 to 19.

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. 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. See Table A2 in Ananat and Hungerman (2007) for more details.