The Effects of Contraception on Female Poverty

Stephanie P. Browne
Sara LaLumia

Abstract

Poverty rates are particularly high among households headed by single women, and childbirth is often the event preceding these households' poverty spells. This paper examines the relationship between legal access to the birth control pill and female poverty. We rely on exogenous cross-state variation in the year in which oral contraception became legally available to young, single women. Using census data from 1960 to 1990, we find that having legal access to the birth control pill by age 20 significantly reduces the probability that a woman is subsequently in poverty. We estimate that early legal access to oral contraception reduces female poverty by 0.5 percentage points, even when controlling for completed education, employment status, and household composition.

INTRODUCTION

In the United States there is significant variation in poverty rates across households with different structures. Poverty rates are approximately twice as high for single-mother-headed households as for any other group. In 2010, 31.6 percent of households headed by single women were in poverty, compared to the national poverty rate of 15.1 percent (DeNavas-Walt, Proctor, & Smith, 2011). The substantially higher incidence of poverty among female-headed households has been a persistent pattern over several decades. Furthermore, many of the poverty spells experienced by female-headed households begin with the initial transition to female headship.[1]

If unplanned pregnancies play a pivotal role in explaining poverty rates among females, there is reason to think that providing access to birth control might reduce a woman's likelihood of entering poverty. A growing body of evidence indicates that,

[1] Cellini, McKernan, and Ratcliffe (2008) provide a thorough review of work explaining transitions into and out of poverty. Using data from 1970 to 1982, Bane and Ellwood (1986) find that transition to female headship accounts for 59 percent of entrances into poverty among female-headed households, and that poverty spells beginning with birth are the longest of all spells. Stevens (1994) finds similar patterns when extending the Bane and Ellwood analysis through 1987. Shifts into female-headed households continue to be an important predictor of poverty entrances in the 1988 to 1992 and 1996 to 1999 periods, with the magnitude of the effect falling over time (McKernan & Ratcliffe, 2005). As in the earlier time period, the presence of children in the household is associated with a greater likelihood of transitioning into poverty.

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by granting women more control over their fertility, expanded access to contraception has given women greater incentive and ability to invest in their own human capital (Bailey, 2006; Bailey, Hershbein, & Miller, 2012; Goldin & Katz, 2002; Hock, 2007). Much of this literature has focused on outcomes of greatest relevance to women of high socioeconomic status, including college completion and participation in highly paid professional occupations. Increased educational attainment and the option of greater labor-force attachment are also of importance to the long-run outcomes of less-advantaged women. This paper tests the hypothesis that having access to birth control at a young age decreases the probability that a woman is subsequently in poverty.

The first oral contraceptive, known as *the Pill*, was approved by the Food and Drug Administration (FDA) in 1960. At first, it was not legally available to unmarried minors. Changes in state laws throughout the 1960s and early 1970s steadily increased minors' legal access to birth control, although that was rarely the explicit intent of legislators. Instead, women under age 20 often gained early legal access (ELA) as states lowered the age of legal adulthood in response to Vietnam War-era political sentiment. A number of authors (Ananat & Hungerman, 2012; Bailey, 2006; Goldin & Katz, 2002) have argued that the staggered spread of ELA constitutes a valid quasi-experiment that can be used to identify causal effects of birth control access. We adopt that empirical strategy in this paper.

In order to estimate the relationship between access to birth control and poverty rates, we estimate ordinary least squares (OLS) regressions with a woman's poverty status as the primary outcome of interest. We rely on cross-state legal variation in the date at which birth control is first available to young single women. A woman is considered to have had ELA to birth control if her state's laws made oral contraception legally available to unmarried women at the time she was age 20. We use census data from 1960, 1970, 1980, and 1990 to examine the effects on women ages 16 to 44. Controlling for many of the channels through which birth control access might be predicted to affect poverty, such as educational attainment, marital status, the presence and number of children, and employment status, we find that ELA is associated with a 0.5 percentage point reduction in the probability that a woman is in poverty. When we drop control variables that are themselves potentially influenced by a woman's early access to birth control, we estimate that ELA reduces the probability that a woman is in poverty by 1 percentage point. These are nontrivial effects, given that the mean poverty rate for nonelderly adult women has been in the range of 10 to 15 percent over the time period of our analysis (Census Bureau, 2013). This finding adds to a growing literature that suggests having access to the Pill at a young age has had numerous positive effects on women's outcomes.

The U.S. federal and state governments administer a number of programs designed to cut poverty rates. Taken together, these programs are both effective and costly. Ben-Shalom, Moffitt, and Scholz (2012) report 2007 spending levels of approximately $12 billion on welfare payments delivered through Temporary Aid to Needy Families (TANF), $49 billion of aid to low-income workers paid through the Earned Income Tax Credit (EITC), and $30 billion worth of food stamps disbursed through the Supplemental Nutrition Assistance Program (SNAP). These transfer programs, along with several other components of the social safety net, have reduced the poverty rate to 13.5 percent from the 29 percent estimated to prevail in the absence of antipoverty programs.[2] The expansion of ELA to birth control was not explicitly an antipoverty policy, and the corresponding reduction in poverty

[2] This calculation is for 2004. The full set of antipoverty policies is estimated to have similar effects in 1984 and 1993 (Ben-Shalom, Moffitt, & Scholz, 2012).

that we establish in this paper is not the policy's primary result. However, simply changing the legality of early access to oral contraception was not an expensive policy. This paper suggests that widespread access to birth control plays a small but cost-effective role in ongoing policy efforts to improve the material well-being of disadvantaged individuals.

BACKGROUND ON ACCESS TO BIRTH CONTROL

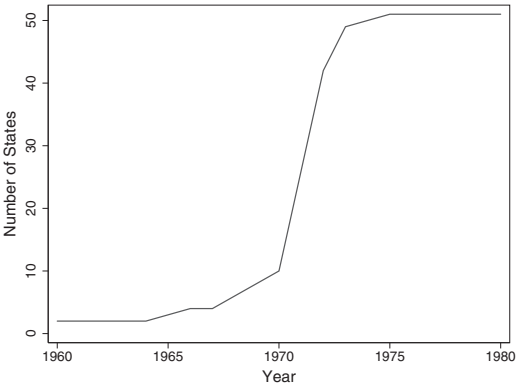
The prescription drug Enovid was approved by the FDA for contraceptive use in 1960. However, it was not immediately widely available to young unmarried women. There were two primary reasons why the Pill was not available to these women: outright state-level bans of birth control and the legal prevention of minors from receiving medical care, including birth control, without the consent of their parents. These two types of restrictions were relaxed at different times in different states.

Bans on birth control were the result of state Comstock laws, named for the Comstock Act of 1873, which explicitly prohibited the sale of contraceptives, among other "obscenities." Many states removed their antiobscenity law's references to contraceptives after the U.S. Supreme Court's decision in *Griswold v. Connecticut* in 1965. The decision overturned Connecticut's ban on the use of contraceptives by married females. While some states subsequently removed their ban on the Pill entirely, others modified the law so that the ban applied only to unmarried women.

The second source of variation in young women's access to the Pill comes from the age at which individuals have independent legal access to medical care. In 1960, the legal age of majority in most states was 21. Minors (individuals under 21) were not able to consent to medical care, including obtaining the Pill, without a parent's approval. Most states granted married women legal emancipation if they were married before age 21. Still, the age of majority vastly limited access to the Pill for young single women. This age-related barrier to birth control access was lifted in two ways. In some states, there was an expansion of legal rights for minors, such as *mature minor* doctrines, which allowed doctors to provide medical care for a minor without parental consent as long as the minor was deemed mentally capable of making medical decisions. These mature minor doctrines effectively granted access to birth control. Other states saw direct changes in the definition of legal age of majority. As a result of the political pressure associated with the Vietnam War draft, the voting age was reduced from 21 to 18 with the passage of the 26th Amendment in 1971. Following this federal policy change, many states lowered the age of legal adulthood to 18. These laws were not targeted toward expanding access to birth control. However, providing younger women access to birth control was an unintended consequence of these changes.

By 1975, all women had legal access to oral contraceptives at age 18 (Bailey et al., 2011). Figure 1 shows the cumulative number of states that had adopted ELA over the 1960 to 1980 period. An assumption implicit in this paper's identification strategy is that earlier adoption of legal birth control access was not motivated by trends in state poverty rates. Bailey (2006) predicts the length of time elapsed between the FDA's 1960 approval of the Pill and the year of ELA for unmarried women, using a number of state-level demographic and social characteristics, levels of household technology adoption, and labor market conditions. State-level poverty rates are included in this regression. None of the explanatory variables, with the exception of the fraction of a state that is Catholic, is statistically significant in predicting the timing of a state's adoption of ELA.

Bailey argues that, based on this evidence, these legal changes create a valid quasi-experiment in which women of a particular birth cohort living in different states



Note: Early legal access (ELA) indicates that a state allowed unmarried women age 20 and younger to legally purchase birth control. This figure uses the coding of Bailey et al. (2011) to illustrate the number of states with ELA by calendar year.

Figure 1. Number of States with Early Legal Access to Birth Control, by Year.

have differential access to birth control.[3] As described in the following section, many authors have relied on this plausibly exogenous variation for identifying the causal effects of birth control access on a variety of outcomes. A quasi-experimental setting is important for these estimates, as the use of birth control is particularly difficult to measure. First, individuals may be reluctant to reveal this personal information in a standard survey. Misreporting may have been particularly problematic in the early years of oral contraceptive availability, when the associated social stigma was greater. Second and more importantly, comparing women who choose to use or not use birth control leads to serious and obvious selection problems. Women who choose to use birth control and women who do not are likely fundamentally different in many ways (Daniels, Mosher, & Jones, 2013).

In order for differential *access* to birth control to affect economic outcomes such as poverty, it must be the case that access to birth control is correlated with *use* of birth control. While this relationship is plausible, it has been difficult to document (Joyce, 2013). Few surveys from the time of ELA expansion include questions about birth control, and respondents may have underreported its use. Bailey, Hershbein, and Miller (2012) use the 1970 National Fertility Survey, and find that ELA to birth control is associated with a 16 percentage point increase in the likelihood of using the Pill by age 20, which was 40 percent higher than the national mean. This sample was limited to ever-married women, excluding the young single women who might benefit most from the Pill. We speculate that the relationship between birth control access and use was even stronger for unmarried women.

Effects of Early Access to Birth Control

The first-order effect of birth control is on childbearing. Several authors have relied on cross-state variation in the timing of ELA in order to measure the effects of contraception on various measures of fertility. The general consensus of this research is that providing legal birth control access to young single women reduces the probability of giving birth at a young age, but has no significant effect on lifetime fertility.

[3] Other work questions whether this legal variation is in fact exogenous to changing socioeconomic conditions within states. See Joyce (2013) for a counterargument.

Bailey (2006, 2009), using data from the Current Population Survey, estimates that access to the Pill before age 21 resulted in a 1.0 to 1.2 percentage point reduction in the probability that a woman became a mother at ages 18 to 21. In contrast, she finds that ELA has a near-zero effect on the probability of having at least one child by age 36. Guldi (2008) simultaneously considers the effects of minors' legal access to birth control and to abortion. Using census data from 1970 and 1980, she shows that ELA reduced birthrates among white women ages 15 to 21 by 8.5 percent. Ananat and Hungerman (2012) use census data from 1970, 1980, and 1990 to look at the short-term and long-term effects of birth control diffusion on women. They find that ELA resulted in an immediate reduction in births among women ages 14 to 20 in the year after the Pill was made widely available in a given state. They find no effect of the Pill on lifetime fertility rates.

Together, these results suggest that access to the Pill during the late teen years might affect women's long-run outcomes by helping them to delay pregnancy, rather than by leading them to bypass childbearing entirely. Delaying pregnancy might be expected to reduce adult female poverty if some of the delay comes from a smaller number of teen pregnancies. Certainly teen mothers go on to experience high rates of poverty, but nonrandom selection into teen motherhood makes it difficult to identify a causal estimate. Early efforts to address selection, by comparing sister pairs in which one became a teenage mother and the other did not, show modestly sized adverse effects (Geronimus & Korenman, 1992). Subsequent work has used miscarriage as an instrument (Hotz, McElroy, & Sanders, 2005), but there are concerns that miscarriage is in fact not randomly determined (Ashcraft, Fernandez-Val, & Lang 2013; Fletcher & Wolfe, 2009). While the magnitudes of the effects are debated, there do seem to be at least some negative consequences of teenage childbearing. Beyond the teenage years, delayed entry into motherhood can have long-run positive effects if it allows women to develop more human capital and to shift pregnancies into periods that are conducive to career advancement. Miller (2011) estimates that delays in motherhood are associated with significant increases in wage rates, hours worked, and career earnings.

Closely related research has investigated the fertility effects of making birth control available to women of all ages. Bailey (2012) uses county-level variation in the timing of implementation of federally funded family planning programs between 1964 and 1973 to assess broader effects of birth control access on fertility. These programs, part of Lyndon Johnson's War on Poverty, provided subsidized contraception for mostly low-income women. Bailey (2012) finds that these programs led to delayed childbearing and to a decrease in completed fertility, especially for younger and poor women. She estimates that federally funded family planning programs reduced childbearing among poor women by 19 to 30 percent in the first 10 years of implementation. Using cross-state variation in expanded Medicaid-provided family planning services, Kearney and Levine (2009) find that subsidized contraception reduces births by about 2 percent and reduces births to teenagers by about 4 percent.

By granting young women greater control over their fertility, early access to the Pill promotes investment in human capital. Goldin and Katz (2002), looking at 20 cohorts of U.S.-born female college graduates born between 1921 and 1960, find that early access to birth control is associated with an increase in the fraction of women who entered professional school and began professional careers. Hock (2007) examines college enrollment and college completion. He finds a positive relationship between early access to birth control and educational attainment, with most of the increase in completed education operating through a reduction in the college dropout rate. Ananat and Hungerman (2012) also find positive effects of ELA on educational outcomes, estimating that ELA is associated with a 2.3 percent increase in the share of all women ages 30 to 49 who are college graduates and a 4.5 percent increase in the share of mothers who are college graduates.

As women with early access to birth control delayed childbearing and acquired more human capital, they simultaneously delayed entry into marriage. Goldin and Katz (2002) find a significant negative effect of adoption of ELA to birth control on the probability that a college-educated woman was married before age 23. Delayed entry into marriage can be beneficial if it promotes better eventual matches in marriage markets.[4] Ananat and Hungerman (2012) estimate that ELA is associated with a 1.9 percent decrease in the share of all women who are divorced and a 2.2 percent decrease in the share of mothers who are divorced. They also find that ELA increased by 3.7 percent the share of women who "had it all," defined as attaining a college degree, being married, and having at least one child.

Even after the years in which most women are making schooling decisions, ELA to birth control continues to affect labor market decisions. Bailey (2006) estimates that access to the Pill before age 21 resulted in an 8 percent increase in labor-force participation among women ages 26 to 30, and is associated with a 15 percent increase in hours worked among women ages 16 to 30. Bailey, Hershbein, and Miller (2012) estimate the effects that early access to birth control had on wages, as opposed to hours. They find that early access to the Pill explains about 27 to 37 percent of the annual wage gains and 33 to 46 percent of the hourly wage gains among women born in the late 1940s. The authors estimate that the Pill accounts for one-third of the wage growth between the 1943 and 1961 birth cohorts and about 10 percent of the narrowing of the gender wage gap over the 1980s. Importantly, the authors find that women who have early access to the Pill face a slight wage penalty in their 20s, but a premium in their 40s. This suggests that any effects of ELA on poverty might vary with a woman's age. They also find that ELA is associated with greater occupational training and an increase in the likelihood that a woman is working in a professional or managerial job between ages 25 to 34. When taken together, these findings are consistent with the idea that early access to the Pill allowed for greater education and professional training early on in a woman's life, which resulted in greater lifetime earnings overall.

To our knowledge, no previous research has estimated the relationship between a woman's access to birth control and her own chances of subsequently being in poverty. However, other authors have considered the effect of women's birth control access on the poverty status of their children. Ananat and Hungerman (2012) find different effects in the short run and in the long run. Immediately after birth control becomes legally available to young women, the children born to young mothers (age 20 and under) become a less-advantaged group, more likely to live in poverty. The authors attribute this result to a short-run change in selection into motherhood. The women who use ELA to delay giving birth are a relatively advantaged group. In the long run, because early access to the Pill is associated with better educational and marriage market outcomes for the average woman and for the average child's mother, the positive effects for women are predicted to generate improved outcomes for their children. Bailey (2013) also considers childhood poverty, relating it to whether a state had a widespread ban on contraception sales and to whether a county had implemented a federally funded family planning program. These sources of variation in birth control access differ from the variation exploited in our paper. State sales bans affected all women within a state regardless of age, marital status, or income. The federal family planning programs were directed toward low-income women. Bailey finds that the lifting of a contraception sales ban has little effect on childhood poverty. In contrast, children conceived after family planning programs were made available are 1.1 percentage points less likely to be in poverty.

[4] For evidence that particularly early marriages increase later-life poverty, see Dahl (2002).

EMPIRICAL STRATEGY

The empirical strategy takes advantage of geographic variation in the timing of legal birth control availability to estimate the causal effect of birth control access on a woman's poverty status. Pooling cross-sectional data from four census years, we estimate the following two OLS regressions:

$$Poverty_{ist} = \beta_1(ELA20)_{is} + \gamma X1_{ist} + \delta(\text{State Controls})_{\text{st}} + \alpha_{\text{t}} + \alpha_{\text{s}} + \varepsilon_{\text{ist}} \quad (1)$$

$$Poverty_{ist} = \beta_1(ELA20)_{is} + \gamma X1_{ist} + \gamma X2_{ist} + \delta(\text{State Controls})_{\text{st}} + \alpha_{\text{t}} + \alpha_{\text{s}} + \varepsilon_{\text{ist}} \quad (2)$$

In both equations (1) and (2) the dependent variable is a dummy for poverty, equal to 1 if individual i is below the poverty line when observed at time t currently living in state s. An individual is in poverty if her family income is below the official poverty threshold defined by the U.S. Census Bureau, where family income is measured as the combined pretax cash income of all family members. Standard errors are clustered at the state level.

Equation (1) includes only those controls that are exogenous to the individual and could not plausibly respond to ELA. Equation (1) is useful for measuring what might be considered the full effect of ELA on poverty, as it encompasses all of the channels through which birth control might affect poverty. Equation (2) adds additional controls that may themselves be affected by a woman's access to birth control. These controls include measures of educational attainment, fertility, and household composition.

The explanatory variable of greatest interest in both equations is the ELA variable, which is a dummy equal to 1 if an individual i would have had legal access to birth control at the time she turned 20. The coefficient β_1 is predicted to be negative, as having access to birth control should decrease the likelihood that a woman will enter poverty. We have argued that state laws provide exogenous variation in access to birth control, and hence we interpret the ELA coefficients as causal estimates.

Equations (1) and (2) contain a number of additional controls. The vector $X1$ includes a set of individual-level demographic controls that are exogenous to ELA. This includes race dummies for being black or of other nonwhite race, as well as controls for age and age squared. Both equations (1) and (2) include a set of state-level controls, including the state unemployment rate and controls for state welfare generosity, the percentage of men drafted into the Vietnam War, and abortion access. The state unemployment rate measures economic conditions within a state that would likely affect poverty rates.[5] The generosity of state welfare programs is measured by the real maximum monthly benefit paid to a family of four through the Aid to Families with Dependent Children (AFDC) program.[6] State welfare is very likely to be correlated with the poverty rate. It could plausibly be correlated with other laws in the state, including laws that would affect access to birth control, if such policies reflect the general degree of conservatism within a state. We also include a set of dummy variables for year, represented by α_t. Lastly, α_s is a set of state dummy variables, included in order to control for time-invariant characteristics of states.

[5] For 1970, 1980, and 1990, state-specific unemployment rates are taken from the Bureau of Labor Statistics, but the BLS does not provide state unemployment rates for 1960. For that year, we used the full Integrated Public Use Microdata Sample (IPUMS) of individuals age 16 and older to compute state-level unemployment rates, defined as the number of unemployed individuals divided by the number of individuals in the labor force.

[6] These data come from Robert Moffitt's *Welfare Benefits Database*, 2002.

Roughly concurrent with the period of ELA expansion, the Vietnam War potentially had important consequences for childbearing and marriage decisions. First, as discussed above, the Vietnam War influenced access to birth control by affecting the age of majority in states. Second, the draft led to a large temporary removal of young men from the marriage market. Drafted men were on average less educated and from lower socioeconomic groups, as being enrolled in college was reason for deferment. The removal of young men from the marriage market could disrupt both marriage and fertility behavior among the young women around the same age. Bitler and Schmidt (2011) find that higher Vietnam induction rates in a state are associated with lower birthrates among young women living in that state. Furthermore, if going to Vietnam is a substitute for going to college, a larger fraction of men drafted into the Vietnam War might result in a less-educated population of men in the state even after the war was over and could have longer term effects on a woman's likelihood of entering poverty. Having a higher ratio of women to men in the marriage market is likely to lead to less compatible matches (Bitler & Schmidt, 2011). These inferior quality marriages could result in higher divorce rates, which is associated with higher poverty rates among women (Bane & Ellwood, 1986). Finally, draft deferments available to married men with children appear to have increased first births (Kutinova, 2009). If decisions to marry and have children were made hastily, motivated largely by a desire to avoid the draft, subsequent rates of divorce and female poverty may have been elevated. For all of these reasons, we include a measure of state-level Vietnam War participation. Vietnam draft is the percentage of the population of men ages 18 to 26 from state s who were drafted into the Vietnam War during the two years a woman i was ages 19 and 20.[7]

Another important change that occurred during the time of ELA expansion is modification of abortion laws. Abortion provides an alternative method of preventing childbirth. Abortion became legally available to most women in 1973 as a result of the landmark case *Roe v. Wade*. For some outcomes, such as overall birthrate, nonmarital births (Guldi, 2008), and births among young women (Myers 2012), access to abortion has had stronger effects than the Pill. Guldi (2008) finds that access to abortion is associated with a 10-percent decline in the birthrate and a 17.2 percent decrease in nonmarital births among white women. Myers (2012) estimates that legal access to abortion caused a 5.5 percent decrease in the fraction of women who gave birth before age 19. Because abortion laws changed during the same time that access to the Pill was expanding, and because abortion could also affect poverty by preventing unplanned childbirth, it is important to control for abortion access. We include an abortion dummy equal to 1 if a woman had legal access to abortion in state s when she was age 20.[8]

Equation (2) adds the vector $X2$, a set of controls that could be classified as "bad controls" in the terminology of Angrist and Pischke (2009). These controls are likely to affect poverty rates, but are themselves also likely to be affected by ELA. These controls include measures of educational attainment, fertility, employment, marital status, and living with a parent. Including these controls does absorb some of the effect that ELA has on poverty. With some of the major channels through which ELA affects poverty being controlled for, equation (2) measures the remaining effect that ELA has on poverty. While tastes vary on whether these bad controls should

[7] The data on the number of men drafted per year per state come from Bitler and Schmidt (2011). The age-specific population data come from the Surveillance, Epidemiology, and End Results (SEER) Program (http://www.seer.cancer.gov).

[8] The coding of abortion laws comes from Myers (2012). In assigning access to abortion, state of birth is used to proxy for state of residence at age 20.

be included or not, we believe it is important to look both at the full effect and the remaining effect.

Educational attainment is measured with three dummy variables for having completed high school education, some college, or a college degree. The omitted category is having completed less than high school. The vector $X2$ includes the number of children, with the expectation that having more children increases the likelihood of being in poverty. It also includes a dummy variable for having no children, as the transition from zero children to one child may have a larger effect on a woman's poverty status than any subsequent birth. Single is a dummy equal to 1 if the individual is not married. Employed is a dummy variable equal to 1 if the individual is employed at the time of observation. A person is considered employed if she reported working for pay at all in the past week or working at least 15 hours for a family farm or business. We do not distinguish between part-time and full-time jobs. If an individual is either unemployed or out of the labor force, the dummy equals 0. We include a dummy equal to 1 if a woman is not living with either of her own parents.

As discussed previously, there are limitations to equation (2) in that some of the controls used might suffer from posttreatment bias, as they are correlated with poverty but would also likely be affected by a woman's ELA. For example, a woman with ELA might be more likely to graduate high school and less likely to become a young mom. Another limitation of both equations (1) and (2) is that there could be omitted variables that affect both ELA and poverty rates. Some possible omitted variables are work experience and occupational choice. There are also likely vast differences in human capital investment even among women with the same measured educational attainment, as we have no measure of quality of degree, type of degree, or amount of effort invested in the degree. Another omitted variable is the earnings potential of a woman's husband, as it could be the case that ELA enables women to find better matches in the marriage market, which in turn could affect poverty.

DATA

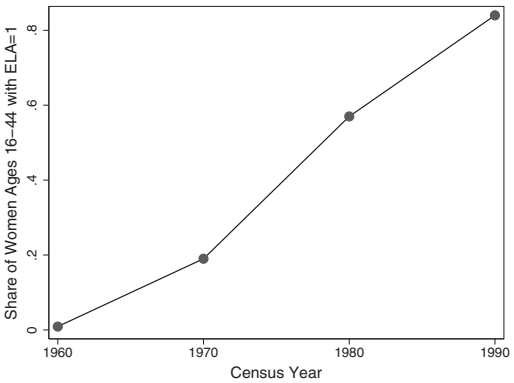
The data are drawn from the IPUMS of the U.S. decennial census data from the years 1960, 1970, 1980, and 1990 (Ruggles et al., 2010). For years 1980 and 1990, census data comprised 1-in-20 random samples of the national population. For 1960 and 1970, the data are taken from a 1-in-100 national random sample of the population. We limit the sample to women between the ages of 16 and 44. We dropped 143,559 observations, equal to 2.4 percent of the sample, for which total household income is missing because the person lived in an institution or group quarters. Table 1 shows summary statistics of the sample for each census year.

We would ideally like to measure whether a woman, when she was age 20, was living in a state in which 20-year-old women had legal access to birth control. However, census data do not report state of residence at age 20. Instead, we observe state of birth and state of residence at time of census enumeration. We construct two measures of ELA, first assigning ELA status as if women lived in their birth state at age 20 and second assuming that women lived in the same state at age 20 and at the time of census enumeration. Both of these ELA variables contain measurement error, with the extent of error depending on cross-state migration rates. Molloy, Smith, and Wozniak (2011) find that there is a higher interstate migration rate among women ages 18 to 24 than among women ages 1 to 17. Because it is less likely that a woman moved in the earliest years of her life, assigning ELA based on birth state should be a more accurate way to assign ELA status than using current residence. Furthermore, a woman's propensity to move after age 20 could be correlated with ELA in that a woman who had access to birth control might be more likely to move from her home state to attend college in a different state. On the other hand, a woman who did not have ELA might be more likely to have

Table 1. Sample means.

	1960	1970	1980	1990
In poverty	0.19	0.12	0.12	0.13
ELA	0.009	0.19	0.57	0.84
Age	30.15	28.71	28.44	30.34
Black	0.11	0.12	0.13	0.12
Other race, nonwhite	0.01	0.01	0.02	0.04
High school diploma	0.38	0.42	0.41	0.34
Some college	0.10	0.13	0.20	0.32
College grad	0.05	0.08	0.13	0.18
Single	0.27	0.34	0.44	0.45
Employed	0.36	0.44	0.59	0.68
Number of children	1.67	1.56	1.15	1.09
No children	0.34	0.39	0.45	0.45
N	316,914	702,302	2,284,178	2,457,958

Note: The table reports means for the sample of women ages 16 to 44 not living in institutions or group quarters. ELA is assigned using a woman's state of birth to proxy for her state of residence at age 20.



Note: This figure illustrates the share of women ages 16 to 44, at the time of census observation, who had access to the Pill by age 20. ELA assignment is based on state of birth. *Data source*: IPUMS decennial data from 1960 to 1990.

Figure 2. Share of Women Ages 16 to 44 with ELA = 1.

an unplanned pregnancy, which could reduce her mobility. The direction in which measurement error in ELA will affect its estimated coefficient is not clear. Classical measurement error would bias the estimate toward zero, but in this case it seems likely that higher income women (with higher rates of geographic mobility) will have greater measurement error in ELA.

In practice, our two methods of ELA assignment yield very similar results. For the full sample, there is a correlation of 0.94 between ELA assigned using birthplace and ELA assigned using state of residence at the time of census enumeration. This correlation is nearly the same for only young women ages 16 to 35. As expected, the correlation decreases with age, with the largest decline occurring after age 40. For older women (ages 36 to 44), the correlation is 0.85. In all the regressions, the results are very similar using either ELA assignment method.

As discussed above, the number of states in which young unmarried women had legal access to the Pill expanded between 1965 and 1975. Figure 2 illustrates the diffusion of ELA on an individual level. It shows the share of women in our sample,

ages 16 to 44, who have ELA equal to 1, measured in each of the four census years included in the analysis. This figure assigns ELA on the basis of birthplace, but the series is virtually identical if ELA is assigned based on state of residence.

RESULTS

Baseline Effects on Poverty

Table 2 shows the results of the main regressions discussed above. Columns 1 and 2 show the result of equation (1), which includes a sparser set of controls. In column 1, ELA status is assigned based on a woman's state of birth. In column 2, ELA is defined by a woman's state of residence at the time of observation. We find that ELA reduces the probability that a woman is in poverty by 1 percentage point.[9] As equation (1) does not control for many of the channels through which ELA likely affects poverty, this estimate can be interpreted as the full effect that ELA has on female poverty rates. This includes any effects that operate though increased educational attainment, an increased probability of employment, and changes in household structure. Table 2 shows the results of unweighted regressions. Using household sampling weights produces very similar estimates. In a weighted regression equivalent to column 1 of Table 2, the ELA coefficient is −0.009 with a standard error of 0.002.

Other controls in equation (1) behave as expected. Black and other nonwhite women are substantially more likely to be in poverty compared to white women. Poverty decreases with age. A 1 percentage point increase in the state unemployment rate is associated with a 0.6 percentage point increase in poverty rates among women. This estimate is consistent with previous literature, which has found that a 1 point increase in the state unemployment rate results in a 0.5 percentage point increase in overall poverty rates (Hoynes, Page, & Stevens, 2006). A $100 increase in a state's maximum AFDC payment for a family of four is associated with a 0.5 percentage point decline in female poverty rates. A 1 point increase in the percentage of young men from a woman's marriage cohort who were drafted to the Vietnam War is significantly associated with a 0.3 percentage point decrease in a woman's chance of being below the poverty line. This result could be explained by higher local draft rates delaying marriage and reducing birth rates (Bitler & Schmidt, 2011) in the short run, in turn increasing women's acquisition of early-career work experience and human capital with positive long-run effects on female income.

Interestingly and perhaps counterintuitively, having access to abortion before age 21 is significantly associated with a 1 percentage point increase in the poverty rate among women. While abortion and the Pill are both methods of preventing childbirth, they work in very different ways. The Pill involves a certain degree of planning and allows a woman greater predictability over her future fertility. Abortion can only be carried out after a woman becomes pregnant. Ananat and Hungerman (2012) speculate that because a woman cannot be sure of her willingness to abort if she becomes pregnant, abortion does not promote women's investment in their own human capital in the same way as the Pill. Furthermore, Ashcraft, Fernandez-Val, and Lang (2013) find that teenage women who choose abortion come from different family backgrounds from those who do not choose abortion. Young women who had an abortion are more likely to be white, have more educated parents, and are less likely to have grown up without a mother or father. Because abortion is more

[9] Estimating a probit model yields a similar marginal effect, indicating that ELA reduces the probability that a woman is in poverty by 0.8 percentage points.

Table 2. The effect of ELA on poverty.

	Equation 1		Equation 2	
	ELA by birthplace (1)	ELA by current residence (2)	ELA by birthplace (3)	ELA by current residence (4)
ELA	−0.010***	−0.010***	−0.005***	−0.005**
	(0.002)	(0.002)	(0.001)	(0.002)
Age	−0.004***	−0.004***	−0.002**	−0.002**
	(0.001)	(0.0009)	(0.001)	(0.001)
Age squared	0.00001	0.00001	−0.00003*	−0.00003*
	(0.00001)	(0.00001)	(0.00002)	(0.00002)
Black	0.210***	0.210***	0.138***	0.138***
	(0.011)	(0.011)	(0.009)	(0.009)
Other race	0.123***	0.123***	0.085***	0.085***
	(0.022)	(0.022)	(0.015)	(0.015)
HS grad		−0.078***	−0.078***	
		(0.004)	(0.004)	
Some college		−0.096***	−0.096***	
		(0.005)	(0.005)	
College grad		−0.125***	−0.125***	
		(0.006)	(0.006)	
Zero children		0.007	0.007	
		(0.004)	(0.004)	
Number of kids		0.035***	0.035***	
		(0.002)	(0.002)	
Single		0.250***	0.250***	
		(0.005)	(0.005)	
Employed		−0.116***	−0.116***	
		(0.002)	(0.002)	
Not living with parent		0.242***	0.242***	
		(0.006)	(0.006)	
State unemployment	0.006***	0.006***	0.004**	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)
Max AFDC	−0.004**	−0.005**	−0.005***	−0.005***
	(0.002)	(0.002)	(0.002)	(0.002)
Men drafted	−0.003***	−0.003***	0.0004**	0.0004**
	(0.0003)	(0.0003)	(0.0002)	(0.0002)
Abortion access	0.010***	0.011***	0.008***	0.008***
	(0.001)	(0.002)	(0.001)	(0.002)
N	5,761,352	5,761,352	5,761,352	5,761,352

Note: The table reports OLS coefficients with robust standard errors, clustered at the state level, shown in parentheses. The dependent variable is a dummy equal to 1 if the respondent is in poverty at the time of observation. In addition to the variables listed, each regression includes a set of state and year fixed effects.
*Significant at 10 percent level; **significant at 5 percent level; ***significant at 1 percent level.

common among more advantaged women, it is probably less likely to prevent poverty compared to the Pill since the women utilizing this option are less likely to be on the brink of poverty to begin with. Due to the financial cost of abortion, it is plausible that it is not a viable option for many disadvantaged women for whom an unplanned pregnancy could be most financially detrimental. While these differences in access to the Pill and to abortion could explain an estimated zero effect of abortion access on poverty, they do not satisfactorily explain the observed positive

relationship. It may be the case that state-level abortion access is spuriously correlated with other time-varying determinants of poverty. In fact, Gruber, Levine, and Staiger (1999) find some evidence that living conditions deteriorated between 1965 and 1980 in states that had liberalized access to abortion.

Columns 3 and 4 show the results of estimating equation (2), which includes a richer set of potentially endogenous demographic controls. In column 3, ELA status is assigned based on a woman's state of birth. In column 4, ELA is defined by a woman's state of residence at the time of observation. The variable of interest, ELA, significantly reduces the probability that a woman is poor. Having access to birth control before age 21 is associated with a 0.5 percentage point decrease in poverty among women regardless of whether ELA is assigned on the basis of birthplace or on the basis of current residence.[10] This effect is about half as large as the full effect of ELA in columns 1 and 2. While the size of the effect is smaller when controlling for some of the channels through which poverty might be affected, the remaining effect of ELA on poverty is nontrivial. This effect is about the same magnitude as reducing the state unemployment rate by 1 percentage point. This effect is large enough to be of policy interest, especially considering that the Pill is a relatively low-cost way to impact poverty rates among women. Potential channels through which ELA might be affecting poverty include occupational choice, quality of schooling (beyond what is captured in the blunt measure of highest grade completed), differences in hours worked or other measures of labor supply on the intensive margin, on-the-job human capital investments, and husbands' human capital and earning potential.

As in equation (1), coefficients on most other control variables in equation (2) are consistent with previous literature. Minority women are more likely to be poor, education reduces poverty, and having more children is associated with a higher probability of poverty. Household composition and employment status are strongly correlated with poverty status.

The effect of state characteristics on poverty is generally similar once the richer set of control variables is included. Women in states with higher unemployment rates are more likely to be in poverty, as a 1-point increase in the unemployment rate is associated with a 0.4 percentage point increase in poverty rates among women. The effect of welfare generosity is virtually the same as the estimates using equation (1). With more controls, the effect of abortion on poverty is slightly smaller in magnitude, as having abortion access is associated with a 0.8 percentage point increase in the probability that a female is in poverty. The only state-level control that has different results in equations (1) and (2) is the Vietnam draft variable. A 1-point increase in the percentage of young men from a woman's marriage cohort who were drafted to the Vietnam War is now associated with an *increase* in a woman's chance of being below the poverty line. Our interpretation of this result is that after controlling for the higher levels of education received by women in cohorts with high draft rates, the remaining effects of high exposure to the war, particularly on marriage quality, were detrimental to women's outcomes.

Heterogeneity Across Demographic Groups

Table 3 shows the results of estimating the main regressions for a number of subgroups. Each cell of the table represents a unique regression where the sample is limited to a specific subset based on different demographic characteristics. Columns

[10] An alternative way to address measurement error in the assignment of ELA at age 20 is to restrict the sample to women who are living in their birth state at the time of census enumeration. If a woman is still living in her birth state at time of census enumeration, then it is highly likely that she was also living in the same state at age 20. Among this sample of approximately 3.9 million women, ELA is associated with a statistically significant 0.6 percentage point decrease in poverty.

Table 3. Heterogeneity of ELA effects.

	Equation 1		Equation 2			
	ELA by birthplace (1)	ELA by current residence (2)	ELA by birthplace (3)	ELA by current residence (4)	Number of observations	Poverty rate
1. Baseline	−0.010***	−0.010***	−0.005***	−0.005**	5,761,352	13.2
	(0.002)	(0.002)	(0.001)	(0.002)		
By age category						
2. Women 16 to 19	−0.004	0.002	−0.005	0.002	838,927	16.9
	(0.012)	(0.012)	(0.009)	(0.011)		
3. Women 20 to 29	−0.012***	−0.015***	−0.007**	−0.010**	2,111,234	15.1
	(0.004)	(0.004)	(0.003)	(0.004)		
4. Women 30 to 39	−0.006**	−0.008**	−0.005**	−0.007**	1,979,132	11.3
	(0.002)	(0.004)	(0.002)	(0.003)		
5. Women 40 to 44	−0.001	0.0006	−0.0008	0.002	832,059	9.0
	(0.004)	(0.007)	(0.003)	(0.006)		
By race						
6. White	−0.009***	−0.009***	−0.004***	−0.005**	4,915,107	10.2
	(0.001)	(0.002)	(0.001)	(0.002)		
7. Black	−0.012**	−0.013**	−0.0009	−0.0003	705,178	31.5
	(0.005)	(0.007)	(0.003)	(0.005)		

Note: Each cell reports the results of a different OLS regression. Robust standard errors, clustered at the state level, are shown in parentheses. The dependent variable is a dummy equal to 1 if the respondent is in poverty at the time of observation. The regressions include the full set of controls in equations (1) and (2), including state and year fixed effects.
** Significant at 5 percent level; *** significant at 1 percent level.

1 and 2 show the full effect of ELA, estimated from equation (1)), while columns 3 and 4 show the remaining effect after including the richer set of equation (2) controls. Although only the coefficient for ELA is reported, all controls from equations (1) and (2) are also included, and omitted coefficients behave similarly to the results reported in Table 2. The last column of the table shows the poverty rate for each subgroup. Row 1 repeats the baseline results for the sake of comparison.

We first investigate heterogeneity by age at time of census enumeration. Having had ELA to birth control might have different effects for women observed in their 20s, just at the time in which many human capital investment decisions are being made, and for women observed later in life. Row 2 looks at the youngest subset of women, those ages 16 to 19 at the time of observation. There is no significant effect of ELA on this group. It is plausible that the effect that the Pill has on a female's probability of ending up in poverty would not have time to develop this early in life. Many of these women would still be living with their parents, so would report their parents' income. Furthermore, some of the potential positive effects of having the Pill, such as increased work experience, different occupational choices, or attaining a higher quality degree are most likely not realized before age 20.

Row 3 looks at women in their 20s. The effect of ELA on poverty is largest for women in this age category. The full effect of ELA on the poverty rate for this group is a reduction of 1.2 to 1.5 percentage points. With the larger set of controls in the equation (2) specification, the remaining effect of early access to the Pill is a 0.7 to 1.0 percentage point reduction in the probability of being in poverty. It makes sense that the effect is stronger for women in their 20s than for younger women, as those in their 20s are more likely to be living independently of parents and the longer term effects of birth control access would have time to develop. For a different

reason, it is also plausible that ELA's effect is larger for women in their 20s than for older women. On average, income is lower when women are in their 20s than when they are older. (The group-specific poverty rates in Table 3 are consistent with this pattern.) When there are more women with incomes close to the poverty line, the potential for a policy intervention to reduce poverty is high. It is also for women in their 20s that there is the greatest difference between ELA coefficients estimated with and without the potentially endogenous education, employment, and household structure controls. This is a plausible point in the life cycle at which differences in educational attainment, labor market participation, and marital status are particularly strong predictors of a woman's poverty status.

Row 4 includes women in their 30s. For this group, the ELA coefficients are similar in magnitude to the results for the full sample. A comparison across columns shows that, for women in their 30s, a smaller portion of the full effect of ELA is operating through the channels of educational attainment, employment status, and marital status. Instead, results from Bailey et al. (2012) suggest that cumulative work experience and occupational choice, not included in the equation (2) specification, have likely become more important determinants of income by the time a woman is in her 30s. Row 5 shows the results for the oldest group of women in the sample, women ages 40 to 44. ELA has no significant effect on poverty among these women. It makes sense that the effects of having access to birth control early in life would eventually taper off, and that this happens at an age when relatively few women are caring for preschool-aged children.

The next two rows of the table estimate the effects of birth control separately for white and black women. The results for white women are very similar to the results for the full sample, which is to be expected when the sample is approximately 85 percent white. The results for black women follow a different pattern. The equation (1) ELA estimates are larger for blacks than for whites. In contrast, the equation (2) estimates, which control for educational attainment, employment, and household structure, show no remaining effect of ELA on black women's poverty status. For black women, the effect of ELA on poverty is operating entirely through channels such as education and marital status. The fact that access to birth control affects the poverty rates of black and white women through different channels is consistent with prior research. Myers (2012) finds that the point estimate of ELA's effect on birthrates for white women is negative, while the point estimate for black women is positive, although neither is statistically significant. Even with no effect on realized fertility, black women could have chosen to acquire more human capital once they had access to the Pill because of changing expectations about their control over future fertility.

Different Multiples of the Poverty Line

While most of this paper focuses on being above or below the official poverty line, we also examine other multiples of the poverty line. Table 4 shows the effect of ELA on these other outcomes. Being in *deep poverty*, defined as below 50 percent of the poverty line, is less likely for women who had early access to birth control. Here there is only a small difference between the equations (1) and (2) estimates of ELA's effect. This suggests that the channels of educational attainment, employment status, and household structure are less important in explaining ELA's long-run impact on women's material well-being at the very lowest part of the income distribution. Instead, ELA might be affecting deep poverty by delaying first births or by improving the match quality of marriages and partnerships. ELA continues to have a significant negative effect on the probability of being below 150 percent or below 200 percent of the poverty line when the full effect is estimated by equation (1). This effect is fully explained by adding controls for educational attainment, employment, and

Table 4. The effect of ELA on being below multiples of the poverty line.

	Equation 1		Equation 2	
	ELA by birthplace (1)	ELA by current residence (2)	ELA by birthplace (3)	ELA by current residence (4)
Dependent variable				
Below 50% of poverty line	−0.006***	−0.008***	−0.004***	−0.005***
	(0.001)	(0.001)	(0.0009)	(0.001)
Below 150% of poverty line	−0.012***	−0.012***	−0.003*	−0.004
	(0.002)	(0.003)	(0.002)	(0.003)
Below 200% of poverty line	−0.013***	−0.013***	−0.001	−0.001
	(0.002)	(0.003)	(0.002)	(0.003)

Note: Each cell reports the results of a different OLS regression. Robust standard errors, clustered at the state level, are shown in parentheses. The regressions include the full set of controls in equations (1) and (2), including state and year fixed effects.
*Significant at 10 percent level; ***significant at 1 percent level.

household structure. The remaining effect of ELA on being below 150 or 200 percent of the poverty line is zero.

Alternative Measures of the Legal Environment

Various authors have relied on somewhat different sets of dates at which the Pill became accessible to young single women, based on their interpretations of the state laws. The extent to which the Pill's estimated effects vary with alternative legal coding has been a matter of debate (Bailey, Guldi, & Hershbein, 2013; Joyce, 2013). The baseline analysis in this paper uses the legal coding of Bailey et al. (2011). Table 5 considers the robustness of results to alternatives used by other authors to measure early access to the Pill. The coding for each alternative is documented in Myers (2012).

For comparison, the top row repeats the baseline results. Some alternative legal codings are only available based on women's access to birth control at age 19, not age 20. Therefore the second row shows results when ELA is assigned at age 19 rather than age 20, again relying on the legal classification of Bailey et al. (2011). Subsequent rows in the table use other authors' classifications of state legal environments. Point estimates are generally smaller with alternative classifications, although the difference in magnitude is small. Across the various codings, results are generally more significant when state of birth rather than state of current residence is used in the assignment process. This is expected, because the extent of measurement error is likely greater when state of current residence is used to proxy for state of residence at age 20. In line with earlier results, the full effect of ELA on poverty (estimated from equation (1)) is about twice as large as when controls for education, employment status, and household structure are included (estimated from equation (2)). This pattern is consistent across alternative legal codings.

Other Outcomes of Interest

Finally, we look at the direct effect that ELA has on several other outcomes. Table 6 contains these results, with ELA assignment based on state of birth. Each column corresponds to a different dependent variable. These outcomes have received some

Table 5. Robustness to alternative coding of legal changes granting early access to the pill.

	Equation 1		Equation 2	
	ELA by birthplace (1)	ELA by current residence (2)	ELA by birthplace (3)	ELA by current residence (4)
ELA at age 20	−0.010***	−0.010***	−0.005***	−0.005**
	(0.002)	(0.002)	(0.001)	(0.002)
ELA at age 19	−0.006***	−0.006*	−0.003**	−0.003
	(0.002)	(0.003)	(0.001)	(0.002)
Myers (2012)	−0.006***	−0.005*	−0.003*	−0.002
	(0.002)	(0.003)	(0.002)	(0.002)
Goldin and Katz (2002)	0.001	0.002	0.002	0.005
	(0.002)	(0.004)	(0.002)	(0.003)
Bailey (2006)	−0.005***	−0.004	−0.003*	−0.003*
	(0.002)	(0.002)	(0.001)	(0.001)
Guldi (2008)	−0.004**	−0.003	−0.003*	−0.001
	(0.002)	(0.002)	(0.001)	(0.002)
Hock (2007)	−0.001	−0.001	−0.00002	0.0002
	(0.002)	(0.003)	(0.002)	(0.003)

Note: Each cell reports the results of a different OLS regression. Robust standard errors, clustered at the state level, are shown in parentheses. The dependent variable is a dummy equal to 1 if the respondent is in poverty at the time of observation. The regressions include the full set of controls in equations (1) and (2), including state and year fixed effects. The source for alternative legal codings is Myers (2012).
*Significant at 10 percent level; **significant at 5 percent level; ***significant at 1 percent level.

previous attention in the literature investigating the effects of making birth control available to young women, and our results are generally similar to existing estimates.

The first column considers the effect of ELA on being a college graduate. The sample is restricted to women ages 25 and older, as most younger women would not have yet had the time needed to complete college. The ELA point estimate is positive, but not significant at conventional levels (P-value = 0.125). Previous research has found a positive relationship between birth control access and college completion (Ananat & Hungerman, 2012; Hock, 2007). These papers have not included data from as early as 1960.[11] For the sake of comparison, we drop observations from 1960 and reestimate the college completion regression. In this case we find evidence that weakly corroborates the earlier work. We find that ELA is associated with a 1.1 percentage point increase in the probability of having completed college, and this result is statistically significant at the 10-percent level.

Columns 2 and 3 show the relationship between ELA and two measures of childbearing. Column 2 shows that ELA is significantly negatively associated with a woman becoming a young mom, which is defined as giving birth before age 21. Having access to the Pill results in a 0.4 percentage point decrease in the chance that a woman becomes a young mom.[12] This finding underscores that preventing unplanned pregnancies, especially teenage pregnancies, is a primary channel through

[11] Ananat and Hungerman (2012) use census data from 1970, 1980, and 1990. Hock (2007) uses CPS data beginning with 1968.
[12] The census does not directly ask about age at first birth. We calculate this variable as a woman's own age minus the age of her eldest within-household child. Measurement error in this constructed variable could explain why we find a smaller point estimate of ELA's effect on young motherhood than Bailey (2009), who uses self-reported age at first birth from the June CPS fertility supplements.

Table 6. The effect of ELA on other outcomes of interest.

	College graduate (1)	Young motherhood (2)	Number of children (3)	Divorced (4)	Receiving welfare (5)
ELA	0.010	−0.004***	−0.104***	−0.007**	0.0006
	(0.007)	(0.001)	(0.007)	(0.002)	(0.0007)
Age	0.030***	0.017***	0.306***	0.032**	0.021***
	(0.001)	(0.001)	(0.010)	(0.002)	(0.001)
Age squared	−0.0004***	−0.0003	−0.004***	−0.0004***	−0.0003***
	(0.00002)	(0.00001)	(0.0002)	(0.00003)	(0.00001)
Black	−0.071***	0.080***	0.349***	0.043***	0.073***
	(0.006)	(0.003)	(0.024)	(0.004)	(0.004)
Other race	−0.072***	0.024***	0.185***	0.011	0.029***
	(0.012)	(0.002)	(0.017)	(0.009)	(0.005)
HS grad		−0.085***	−0.311***	−0.018***	−0.043***
		(0.003)	(0.012)	(0.002)	(0.002)
Some college		−0.104***	−0.475***	−0.009***	−0.066***
		(0.004)	(0.015)	(0.003)	(0.003)
College grad		−0.120***	−0.781***	−0.060***	−0.075***
		(0.004)	(0.015)	(0.004)	(0.004)
Zero children	0.108***			0.013***	−0.057***
	(0.005)			(0.002)	(0.004)
Number of kids	−0.018***			−0.019***	0.005***
	(0.001)			(0.001)	(0.001)
Single	−0.036***	−0.020***	−0.657***		0.137***
	(0.002)	(0.002)	(0.013)		(0.006)
Employed	0.070***	−0.003***	−0.423***	0.062***	−0.066***
	(0.002)	(0.001)	(0.011)	(0.003)	(0.003)
Not living with parent	0.073***	0.051***	0.179***		0.056***
	(0.004)	(0.002)	(0.013)		(0.003)
State unemployment	0.0001	0.0004*	0.006***	0.002***	0.002***
	(0.001)	(0.0002)	(0.002)	(0.0006)	(0.0006)
Max AFDC	−0.002	−0.001**	−0.007***	0.001***	−0.0002
	(0.002)	(0.0003)	(0.002)	(0.0005)	(0.0006)
Men drafted	0.006***	0.0001	−0.048***	0.0005*	0.0009***
	(0.0007)	(0.0002)	(0.002)	(0.0003)	(0.0002)
Abortion access	−0.026***	−0.001	−0.075***	−0.026***	0.007***
	(0.006)	(0.001)	(0.010)	(0.002)	(0.0008)
N	3,671,741	5,761,352	5,761,352	2,811,191	5,761,352

Note: Each column reports the results of a different OLS regression. Robust standard errors, clustered at the state level, are shown in parentheses. The regressions include state and year fixed effects. The sample is restricted to women age 25 and older in column 1 and women age 30 and older in column 4. ELA is assigned on the basis of state of birth.
*Significant at 10 percent level; **significant at 5 percent level; ***significant at 1 percent level.

which the Pill could work to reduce poverty among women. In column 3, the dependent variable is the number of own children living with a woman at the time of census observation. This should not be interpreted as lifetime fertility, because our sample includes many women with years of potential fertility ahead of them. We find that having access to the Pill at age 20 is significantly associated with a 0.104 decrease in the number of children a woman has at the time of observation. The

results of columns 2 and 3 are consistent with previous evidence that ELA delays entry into motherhood.

Column 4 estimates the effect of ELA on divorce for women ages 30 to 44. We limit the sample to this older age range both to reflect the stylized fact that divorce is an outcome occurring somewhat later in life (women need time to marry before they can divorce), and for the sake of comparison to the literature. Ananat and Hungerman (2012) suggest that the Pill could help individuals find better matches in the marriage market because it reduces the likelihood of shotgun weddings in the aftermath of an unplanned pregnancy. It also allows for the separation of marriage and sexual activity, so women might be more likely to postpone marriage in order to find a better match. Both of these possibilities suggest that there may be a negative relationship between ELA and divorce. We find that having access to the Pill is associated with a 0.7 percentage point decrease in the probability of being divorced, which is consistent with findings from Ananat and Hungerman (2012). Because we observe only current marital status, we do not know if this primarily represents a reduction in having ever divorced or an increase in rates of remarriage after divorce. Although not reported in the table, we have estimated this regression excluding women who reported being never married. When these women are dropped (about 10 percent of women ages 30 to 44), the effect of ELA on divorce remains of similar magnitude.

Because ELA is associated with a variety of positive outcomes for women, having access to birth control might lower the probability that a woman receives welfare. In column 5, the dependent variable is a dummy variable equal to 1 if a woman reports receiving welfare income. There is no significant association between access to birth control and the probability that a woman receives welfare. This result is not inconsistent with ELA's estimated effects on poverty, as the typical cash welfare recipient is well below the poverty line.

CONCLUSION

This paper adds to the growing body of research that documents the effects of birth control access on women's outcomes. Using exogenous state-by-year variation in unmarried young women's legal access to birth control, we estimate the effect of having legal access to birth control at age 20 on the probability that a woman is in poverty. Even when controlling for many of the channels through which birth control access has already been shown to positively affect women, such as educational attainment, we estimate that birth control access reduces the probability that a woman is in poverty by 0.5 percentage points. Although this effect may seem small relative to the persistently high poverty rates experienced by single-female-headed households, it is the result of a very low-cost intervention. Going forward, when policymakers are weighing the costs and benefits of increasing or decreasing the accessibility of birth control, they should take into account its effects on female poverty rates.

STEPHANIE P. BROWNE is an Analyst at J.P. Morgan, 383 Madison Avenue, 3rd Floor, New York, NY 10179 (e-mail: spbrowne21@gmail.com).

SARA LALUMIA is an Associate Professor of Economics, 24 Hopkins Hall Drive, Williamstown, MA 01267 (e-mail: Sara.LaLumia@williams.edu).

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