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THE EFFECTS OF CONTRACEPTION ON FEMALE POVERTY

by

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I love you, Dad.

Abstract

This paper adds to the growing body of research on the positive effects of access to the birth control pill on women's outcomes. Specifically, I examine the relationship between early legal access to birth control on poverty rates among women. It is well-documented that the prevalence of poverty cases among households headed by single-females is much higher than among the general population. The event that most commonly precedes these spells is the formation of the household itself, often due to childbirth or divorce. Therefore, the birth control pill might prevent some of these transitions into poverty for females. To test this theory, I use exogenous cross-state variation in the timing of when the Pill became legally available to young, single women in order to measure the Pill's effect on female poverty rates. Using census data from 1960-1990, I find that having legal access to the Pill by age 20 does have a significant effect in reducing the likelihood that a woman is in poverty.

I. Introduction

In the U.S., there is significant variation in poverty rates among different subgroups of the population. Poverty rates among single-mother-headed households are higher than for any other type of household. 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). Unlike male-headed households, the primary cause of poverty for female-headed households is the initial transition to female headship (Bane and Ellwood, 1986; Stevens, 1994). 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. This paper tests the hypothesis that having access to birth control decreases the probability that a female is in poverty.

It is reasonable to assume that women with access to birth control are less likely to have an unplanned pregnancy. With more control over their fertility, they may also have greater incentives to invest in their own human capital, through education and work (Goldin & Katz, 2002; Bailey, 2006; Hock, 2008). Both of these effects suggest that earlier access to birth control could reduce poverty rates. If early legal access (ELA) does reduce the chances that women will end up in poverty, it could have important implications for policy-makers. After all, not only might ELA help keep women out of poverty due to unplanned pregnancy, but it would also help prevent more children from being

born into poverty. It is one possible way to break the existing intergenerational poverty trap.

In order to test the relationship between access to birth control and poverty rates, I estimate an OLS regression with a woman's poverty status as the primary outcome of interest. In order to measure birth control access, I use cross-state legal variation in the date at which birth control is first available to young single women. I use census data from 1960, 1970, 1980, and 1990 to examine the effects on women ages 16-44. The estimates suggest that having access to the Pill before age 21 reduced the likelihood that a woman is in poverty by 0.4 percentage points. This is a non-trivial effect, given that the mean poverty rate for non-elderly adult women has been in the range of 10 to 15 percent over this time period (Census Bureau, 2013). This finding adds to a growing literature which has suggested that having access to the Pill at a young age has had numerous positive effects on women's outcomes.

II. Background

The reasons for entry into poverty are very different for male and female-headed households. For male-headed households, the most common cause is a decrease in income. For female-headed households, however, the primary cause of poverty is the initial transition to female headship. 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. These cases account for 11 percent of all entrances into poverty. Among the

cases of female-headed households in poverty, Bane and Ellwood (1986) estimate that 21 percent are caused by unmarried motherhood. Importantly, poverty spells that begin 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 1988-1992 and 1996-1999 periods, with the magnitude of the effect falling over time (McKernan and 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. Clearly, pregnancies play a pivotal role in poverty rates, especially among females.

A logical solution for helping women to gain control over their fertility and prevent unplanned pregnancy is through contraceptives, such as the Pill. Surprisingly, research on the economic effects of birth control has been pretty sparse until quite recently. The use of birth control is difficult to measure, as individuals may be reluctant to reveal this personal information in a standard survey. Misreporting may have been particularly problematic in the early years of pill availability, when the associated social stigma was greater. Even with accurate measures of birth control use, 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). Therefore, the conclusions about the impact that birth control had on women's outcomes were ambiguous. More recent research, however, has addressed issues of selection

and suggested that the Pill has had meaningful effects on wages, college education, fertility, and labor supply choices among women.

A. Access to Birth Control

The first oral contraceptive, otherwise known as "the Pill," was approved by the FDA in 1960. However, it was not immediately widely available to single young females. There were two primary reasons why the Pill was not immediately available to unmarried women: outright bans of birth control and the inability of minors to receive medical care, including birth control, without consent of parents.

The 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 anti-obscenity 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 among married females. While some states removed their ban on the Pill entirely, others modified the law so that the ban only applied to unmarried women. Ohio revised its Comstock law in 1965. Because Ohio had previously implemented a mature minor doctrine, meaning that individuals under age 21 could consent to medical care, it was the first state to effectively change its status of provision of birth control to young women due to *Griswold v. Connecticut.* By 1970, single women under age 21 had access to birth control in 17 states (Bailey, et. al, 2011).

The second source of variation in young women's access to the Pill is the legal age of majority. In 1960, the legal age of majority in most states was 21. Minors (anyone 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 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. The second way the age restriction was removed was the result of 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 decision, many states began changing the age of legal adulthood to 18. These laws were not targeted towards 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 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 1960-1980 period.

Why were some states early adopters of a lower age of majority? Bailey (2006) shows that the timing of states providing expanded access to birth

control did not coincide with other pre-existing state-level characteristics. To test this, Bailey (2006) created a dependent variable for each state that was equal to the difference in the year that the Pill became widely available to young women in a particular state, referred to as the first year of early legal access (ELA), and the year that the Pill was passed by the FDA (1960). The explanatory variables were a variety of state-level characteristics, including demographic characteristics, social characteristics, household technology, and labor market information. For example, she estimated the effects of religious and racial compositions in a state and fertility norms within a state. None of these explanatory variables were statistically significant in predicting the timing of a state's provision of ELA. Bailey (2006) infers that this is strong evidence for the credibility of using the legal changes as a valid quasi experiment. She suggests that the variation in ELA adoption is a result of idiosyncratic regional courts and legislatures, the regional politics of minors' rights, and the war in Vietnam.

B. Effects of Early Access to Birth Control

Fertility

The first order effect of birth control is the impact that access to birth control has on women's fertility rates. To study the effects of birth control on fertility, Bailey (2012) uses the geographic variation in the timing of implementation of federally funded family planning programs between 1964 and 1973. Bailey (2012) finds that these programs led to a decrease in fertility,

especially in younger and poor women. She estimates that the U.S. family planning programs reduced childbearing among poor women by 21 to 29 percent in the first 10 years of implementation.

Bailey (2009) uses the cross-state variation in the timing of when birth control was legally made available to young, single women in order to measure its effects on fertility. While she finds no effect on women's lifetime fertility, she estimates that access to the Pill before 21 resulted in a 1 to 1.2 percentage point reduction in the probability that a woman became a mother before 22. This finding suggests that some of the Pill's impacts on women's outcomes might be a result of its effect on the timing of childbirth, rather than on overall fertility rates.

Ananat and Hungerman (2012) used 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 14-20 in states the year after the Pill was made more widely available. However, they find no effect of the Pill on lifetime fertility rates, which suggests that the Pill helps women to delay pregnancy, rather than avoid childbearing entirely.

Other research has focused specifically on fertility rates among young women. Hock (2007) shows that ELA accounts for 20 percent of the decline in teenage pregnancies between 1960 and 1970. Guldi (2008) shows that ELA did reduce birthrates among white women ages 15-21 by 8.5 percent in the 1970s and 1980s.

Human Capital Investment

Besides basic fertility rates, the next area of interest is birth control's effects on other measures of outcomes for females, such as levels of human capital investment and wages.

Goldin and Katz (2002) find a significant negative effect of adoption of early legal access to birth control on the probability that a college-educated woman was married before age 23. They also look at aggregate effects on 20 cohorts of U.S.-born female college graduates born between 1921 and 1960. They find that access to birth control led to an increase of age of first marriage and an increase in the fraction of women who entered professional school and began professional careers. Bailey, Hershbein, and Miller (2012) use a similar strategy as Goldin and Katz (2002), and find that women who had access to the Pill invested more in education and occupational training and were more likely to work a professional or managerial job.

Hock (2007) examines college enrollment and college completion, and finds that most of the increases in education from birth control came in the form of preventing college dropouts.

Ananat and Hungerman (2012) examine long-term effects of birth control by considering outcomes of all women 30-49 and of mothers 30-49. They find that ELA resulted in a 2.3 percent increase in the share of all women who are college graduates and a 4.5 percent increase in the share of mothers who are college graduates. ELA also caused 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, suggesting that access to the Pill might have led to better matches in marriage markets. ELA also 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.

Labor Market Effects

Access to birth control could reduce a woman's likelihood of entering poverty for two major reasons. First, access to birth control has been shown to have effects on women's labor supply decisions. Second, having children is associated with reductions in labor income.

Bailey (2006) examines the Pill's effect on labor supply decisions, and estimates that access to the Pill before 21 resulted in an 8 percent increase in labor-force participation among women age 26 to 30. She also finds that early access to the Pill is associated with a 15 percent increase in hours worked among women ages 16 to 30. Bailey suggests that the increases in labor supplied by women could be the result of lowered costs and increasing returns of pursuing a career. The Pill lowered costs of working because women no longer had to choose between being sexually active and pursuing a career. Simultaneously, the returns to a woman's investment in her career increased because a woman faced higher lifetime expected returns to working since she had more control over the timing and number of periods she would be removed from the labor market due to pregnancy and childrearing.

Using the same strategy, 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 cohorts and about 10 percent of the narrowing of the gender wage gap over the 1980s. Importantly, Bailey, Hershbein, and Miller (2012) find that women who have early access to the Pill face a slight wage penalty in their twenties, but a premium in their forties. 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 the Pill allowed for greater education and professional training early on, which resulted in greater lifetime earnings overall.

Another way that the Pill might affect women's labor market outcomes is through motherhood's effects on wages. Lundberg and Rose (2000) find that there is a 23 percent decrease in wages associated with motherhood for women who take a hiatus from the labor force, but not for women who remain continuously tied to the labor force. Korenman and Neumark (1992) also find that having children results in a wage penalty for women. Miller (2005) finds that each year a woman delays motherhood is associated with a 10 percent

increase in career earnings, a 3 percent increase in wage rates, and a 5 percent increase in career hours worked. Potential explanations for this wage penalty have been offered, including a reduction in experience and a lowered attachment to the labor force. Pertaining especially to married women, this wage penalty has been interpreted as a result of specialization between men and women in that women are more likely to focus their time and attention towards homemaking and childrearing tasks. Since having children is associated with lower wages for women, having access to the Pill could help some women prevent this wage penalty either by avoiding pregnancy altogether or postponing it to a time that would be less detrimental to her career. This effect could be especially important if the Pill lets a woman control the timing of her pregnancy in a way that allows her to remain connected to the labor market, which could help her avoid the wage penalty according to Lundberg and Rose (2000).

Teenage Pregnancy

A narrower group of interest in the birth control literature is younger women. Besides its effects on women's incentives to invest in human capital, another channel through which the Pill might work to reduce female poverty rates is by preventing teenage pregnancy.

Researchers have sought to measure the effects of teenage pregnancy. There is some disagreement about the true negative consequences. Geronimus and Korenman (1992) compare the differences in outcomes of sister pairs,

including some where one sister became a teenage mother and one did not. Using this method, they are able to control for unobserved family characteristics. They find modest adverse effects of teenage motherhood. Women who gave birth as a teen had significantly lower levels of family income and a greater chance of being in poverty at ages 28-38 compared to their sisters. Teenage mothers also were significantly less likely to graduate from high school. The size of the effects varied depending on which dataset the authors used. However, overall, this method yielded smaller effect sizes than previous literature.

Ashcraft and Lang (2006) compare the outcomes of women who gave birth as teenagers to women who got pregnant as teenagers but who did not give birth because of miscarriage or abortion. The authors assume that miscarriages are random events. These authors also find modest effects of becoming a teenage mother. Compared to women who got pregnant but miscarried, those who give birth as a teenager complete about 0.15 years less of education, are 5 percent less likely to be employed, and are 3 percent less likely to be married. Women who gave birth as teens also have a lower income to needs ratio, driven partially by the fact that their family is inherently larger after giving birth.

However, Fletcher and Wolfe (2009) suggest that Ashcraft and Lang underestimate the effects of teen birth by assuming that miscarriages are random events. They document that miscarriages are correlated with unobserved community effects. By controlling for these community effects, Fletcher and Wolfe (2009) estimate that becoming a teenage mom decreases a female's probability of receiving a high school diploma by 5 to 10 percent,

reduces annual income as a young adult, and may increase the probability of receiving cash assistance. While there is discrepancy in the literature about the magnitude of the effects of teenage childbearing, there do seem to be at least some negative consequences.

III. Empirical Strategy

I take advantage of geographic variation in the timing of legal birth control availability to estimate the causal effect of birth control access on poverty. Pooling cross-sectional data from four census years, I estimate the following two OLS regressions:

$$P_{ist} = \beta_1(\text{ELA at age 20})_{is} + \Upsilon X_{1\,ist} + \delta(\text{state controls})_{st} + \alpha_t + \alpha_s + \varepsilon_{ist} \quad (1)$$

$$P_{ist} = \beta_1(\text{ELA at age 20})_{is} + \Upsilon X_{1\,ist} + \Upsilon X_{2\,ist} + \delta(\text{state controls})_{st} + \alpha_t + \alpha_s + \varepsilon_{ist} \quad (2)$$

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 outlined by the U.S. Census Bureau, Current Population Reports. Family income is the combined pre-tax cash income of all family members.

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 can be considered the full effect of ELA on poverty, as it captures all of the channels through which birth control might affect poverty. Equation 2 adds additional controls which may themselves be affected by a woman's access to birth control.

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. I expect β_1 to be negative, as having access to birth control should decrease the likelihood that a woman will enter poverty. Early access depends on a woman's state of residence at age 20, which I do not observe in Census data. Instead, I observe state of birth and state of residence at time of observation. I construct two measures of ELA, first assigning ELA status as if women lived in their birth state and second assuming that women lived in the same state at age 20 and at the time of Census observation. Both of these ELA variables contain measurement error, with the extent of errors depending on cross-state migration rates. I prefer specifications in which ELA assignment is on the basis of birthplace. Malloy, Smith, and Wozniak (2011) find that there is a higher interstate migration rate among women 18-24 than women 1-17. 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 travel or attend college in a different state. On the other hand, a woman who did not have ELA

might be more likely to have an unplanned pregnancy, which could reduce her mobility. In practice, the two methods of ELA assignment yield very similar results.

The correlation between the two forms of ELA assignment for all women is 0.94. This correlation is the same for only young women ages 16-35. As expected, the correlation decreases with age, with the largest drop off occurring after age 40. For older women (ages 36-44), 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. Figure 2 shows the annual share of women ages 16 to 44 with ELA equal to 1. In each of the four census years included in my 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. The rapid growth of ELA between 1960 and 1970 reflects the legal changes shown in Figure 1.

Equations 1 and 2 contain a number of additional controls. The vector X_1 includes a set of individual-level demographic controls that are exogenous to ELA. I include race dummies for being black or other, making white the omitted category. I include age and age squared as variables because poverty rates change with age, but not in a linear fashion.

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.

I include the state unemployment rate as a measure of economic conditions in the state that would likely affect poverty rates. I also control for the generosity of state welfare programs, measured by the real AFDC maximum amount paid per month for a family of four[1]. 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 ELA, if such policies reflect the general degree of conservatism within a state. I also include a set of dummy variables for year, represented by α_t . Lastly, α_s is a set of state dummy variables, in order to control for time-invariant characteristics of states.

Roughly concurrent with the period of ELA expansion, the Vietnam War potentially had important consequences for childbearing and marriage decisions in at least two ways. 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 socio-economic 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

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

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 and Schmidt, 2011). These inferior quality marriages could result in higher divorce rates, which is associated with higher poverty rates among women (Bane & Ellwood, 1986). For these reasons, I include a measure of state-level Vietnam War participation in my regression. Vietnam draft is the percentage of the population of men ages 18-26 from state *s* who were drafted into the Vietnam War during the two years a woman *i* was ages 19 and 20[2].

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 and non-marital 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 birthrate and a 17.2 percent decrease in non-marital

[2] The data on the number of men drafted per year per state come from the unpublished paper,"Marriage Markets and Family Formation: The Role of the Vietnam Draft" by Marianne Bitler and Lucie Schmidt. The population data comes from the Surveillance, Epidemiology, and End Results (SEER) Program (www.seer.cancer.gov)

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 as access to the Pill and could affect poverty by preventing unplanned childbirth, it is important to control for abortion access. Abortion is a variable equal to 1 if a woman had legal access to abortion in state *s* when she was 20[3]. I base the assignment on a woman's state of birth.

In equation 2, I include the same controls as in equation 1 and add the vector $X_{2,}$ which includes a richer set of controls. Some of these controls are what Angrist and Pischke (2008) would call "bad controls," because they are likely to affect poverty rates, but are also themselves likely to be affected by ELA. These controls include 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. When some of the major channels through which ELA affects poverty are controlled for, I measure the "remaining effect" that ELA has on poverty. While tastes vary on whether these "bad controls" should be included or not, I believe it is important to look both at the full effect and the remaining effect.

X_2 is a vector that includes some of these "bad controls." I include three education dummy variables for having completed high school education, some college, and a college degree. I control for number of children, as I would expect

[3] The coding of abortion laws comes from *Power of the Pill or Power of Abortion? Re-Examining the Effects of Young Women's Access to Reproductive Control* (Myers, 2012).

more children to increase the likelihood of being in poverty. I also include a dummy variable for having no children, as I expect that the transition from zero children to one child has a larger effect than any subsequent births. 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 he or she reported working at all in the past week or working at least fifteen hours for a family farm or business. There is no differentiation between part-time or full-time jobs. If an individual is either unemployed or out of the labor force, the dummy equals zero. I 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 post-treatment 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 equation 1 and equation 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 educational attainment, as I 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. Another control that I would have liked to

include is a measure of socioeconomic status, such as parent's income or education. If such information was available, interacting parental background with ELA could be informative about heterogeneous effects of ELA. I hypothesize that ELA would have a larger effect for women at the lower end of the income distribution, who are more likely on the cusp of poverty. For wealthier women, it is unlikely that an unplanned pregnancy would result in poverty, so the effect of ELA would not be as strong.

IV. Data

The data source I use is the Integrated Public Use Microdata Sample (IPUMS) of the United States decennial census data from the years 1960, 1970, 1980, and 1990[4]. For years 1980 and 1990, census data is comprised of 1-in-20 random samples of the national population. For 1960 and 1970, the data is taken from a 1-in-100 national random sample of the population. For my analysis, I limit the sample to women between the ages of 16 and 44. I dropped 143,559 observations, equal to 2.4% of the sample, for which total household income is missing because the person lived in group quarters or institutions.

Table 1 shows summary statistics of the sample for each census year. The average age of women with ELA is younger than women without ELA. This is to be expected, as ELA has expanded over time, so older women would be less likely to have had access to the Pill when they were 20. The share of all women

[4] Steven Ruggles, J. Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B. Schroeder, and Matthew Sobek. ***Integrated Public Use Microdata Series: Version 5.0*** [Machine-readable database]. Minneapolis: University of Minnesota, 2010.

ages 16-44 with ELA expanded from 1960 to 1990 because access to birth control expanded over time. All women born in 1955 or later had ELA.

Figure 2 depicts the expansion of ELA among women ages 16 to 44 between 1960 and 1990. Intuitively, one would expect that expansion of legal access to the Pill is associated with greater use of the Pill. Because of limited data and potential underreporting of Pill use, measuring usage of the Pill is somewhat difficult. Bailey, Hershbein, and Miller (2012) use the 1970 *National Fertility Survey,* and find that early legal access 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. However, the sample was limited to women who were ever-married, which excludes the young, single women who might benefit from the Pill the most. Still, there is relatively strong evidence for the positive relationship between legal access to the Pill and usage of the Pill by young women.

V. Results

A. Effect on Poverty

Baseline

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. The variable of interest, ELA, is associated with a 0.9 percentage

point decrease in the probability that a woman is in poverty when ELA is assigned by birthplace and a 1.0 percentage point decrease when ELA is assigned by current residence. As I have not controlled for many of the channels through which ELA likely affects poverty, this can be interpreted as the full effect that ELA has on female poverty rates.

Other controls behave as expected, as black and non-white women are 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, 2007). A $100 increase in the maximum AFDC payment for a family of 4 within a state is associated with a 0.5 percentage point decline in female poverty rates. A one 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 increase in a woman's chance of being below poverty.

Interestingly and perhaps counter-intuitively, 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 and Lang (2006) 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 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.

Columns 3 and 4 show the results of 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, is significantly associated with lower poverty among women 16-44. Having access to birth control before age 21 is associated with a 0.4 percentage point decrease in poverty among women when ELA is assigned by birthplace and a 0.5 percentage point decrease when ELA is assigned by current residence. This effect is about half as large as the effect of ELA in columns 1 and 2 when the regression is estimated without the "bad controls." This smaller magnitude is

expected since I have controlled for some of the channels through which ELA affects poverty. While the size of the effect is smaller with more controls, the remaining effect of ELA on poverty is non-trivial. This effect is about the same magnitude as reducing the state unemployment rate by 1 percentage point. While the effect of ELA on poverty is not huge, it is large enough to be of interest, especially considering that the Pill is a relatively low cost way to impact poverty rates among women.

As in equation 1, coefficients on most other control variables are consistent with previous literature. More educated women are less likely to be in poverty, as having a high school degree is associated with a 7.8 percentage point decrease in the likelihood of being in poverty and a college degree is associated with a 12.5 percentage point decrease. Black and non-white women are significantly more likely to be in poverty compared to white women. Having more children is significantly associated with higher poverty, as an increase of one child is associated with a 3.5 percentage point increase in poverty. Having no children did not have any significant effect on poverty rates conditional on number of children. The strongest predictor of a female being in poverty is marital status, in that single women have a 25 percentage point greater chance of being in poverty. As expected, women who are employed are less likely to be in poverty. Women not living with either of their own parents are 24.2 percent more likely to be in poverty.

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 the same as the estimate using equation 1. A one 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 0.045 percentage point increase in a woman's chance of being below poverty.

With more controls, the effect of abortion on poverty is slightly smaller in magnitude, as having abortion access is associated with a 0.08 increase in the probability that a female is in poverty.

Heterogeneity across different demographic groups

Table 3 shows the results of variations of the main regression. Each cell of the table represents a unique regression where the sample is limited to a specific subset based on different demographic characteristics. Although only the coefficient for ELA is reported, all controls from equation 2 are also included unless otherwise noted. All controls behave similarly to the results reported in Table 2, columns 3 and 4. Row 1 repeats the results from Table 2 for the sake of comparison.

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. In all the regressions, the results are very similar using either ELA assignment method.

Row 2 includes only women ages 16-35. I would expect this younger subset of women to be more susceptible to the effects of birth control than older

women. A pregnancy is more likely to be economically detrimental to a younger woman. Also, access to ELA is most likely to affect the human capital investment decisions of younger women. Among these younger women, access to the Pill before 21 is associated with a 0.7 percentage point decrease in poverty when ELA is assigned by birth state and a 0.8 percentage point decrease when ELA is assigned by residence at time of observation. The fact that the effect is slightly stronger for women ages 16-35 than for all women in the sample is consistent with the idea that having access to birth control would have the largest effect on younger women.

Row 3 looks at an even younger subset of women, ages 16-20. 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. Many of these women would still be living with their parents, so would report their parents' income and therefore not be indicated as below poverty. Furthermore, some of the potential positive effects of having the Pill, such as work experience, occupational choice, or attaining a higher quality degree would most likely not show up by age 20.

Row 4 looks at women in their 20's. When ELA is assigned by birthplace, access to the Pill reduces a female's probability of being in poverty by 0.7 percentage points. It makes sense that the effect is strongest for women during their 20's since they are more likely to be living independently and the longer-term effects of birth control access would have time to develop. Row 5 includes women in their 30's. For this group, access to birth control when ELA is assigned

by birth place is associated with a 0.5 percentage point decrease in poverty rates. Row 6 shows the results of the oldest group of women in the sample, women ages 40-44. ELA has no significant effect on poverty among these women. It makes sense that the effects of having birth control early in life would not be a significant predictor of poverty by the time women reach their 40s.

The next two rows of the table estimate the effects of birth control separately for black and white women. Using either ELA assignment, ELA does not have a significant effect on poverty among black women. The estimated effects of birth control access on poverty rates of white women when ELA is assigned by birthplace and when ELA is assigned by current residence are 0.4 and 0.5 percentage points, respectively. These effect sizes are identical to the baseline estimates from Row 1. The fact that access to birth control has different effects for different races 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. She theorizes that the differences could be caused by both socioeconomic and racial discriminatory factors of the time. Myers (2012) speculates that black women might be less likely to afford medical care and/or have access to medical care, including a primary physician. Therefore they would be less likely to get the Pill even when it was legally available. However, they might still be affected by the changes in social norms driven by the Pill, which Akerlof, Yellen and Katz (1996) call the "technology shock" effect. It could be the case that black women increased their sexual activity because of new

social norms, regardless of whether they actually obtained the Pill or not. The barriers that black women faced in obtaining medical treatment combined with the "technology shock" effect could explain why there is a difference in the Pill's effect on white women and black women.

Row 9 includes only non-college graduates, limiting the sample to women above age 24 because it is unlikely that younger women would have yet attained a college degree. I use college degree as a proxy for permanent income, in lieu of any control for family background. It is reasonable to assume that college graduates are likely to be at the high end of the income distribution. I focus on non-college graduates to examine the lower socioeconomic status women, whose poverty status may be more sensitive to ELA. Firstly, they are more likely to enter poverty in general, as they are closer to the brink of poverty. Secondly, it is likely that an unplanned pregnancy is more financially devastating to a poorer female compared to a wealthier woman who would have more resources and support at her disposal. An unexpected pregnancy might be more detrimental for a group that had fewer resources to begin with. For this group, ELA is associated with a 0.4 to 0.5 percentage point decrease in poverty. While I am using the restricted sample of non-college graduates in an attempt to examine women with disadvantaged backgrounds, it is important to consider that college graduation is also potentially associated with a woman's access to birth control.

In Row 10 I include only women who are not living with either of their own parents. Logically, a woman who is still living with her own parents is somewhat immune to the effect of ELA on poverty because she has the income

and resources of her parents to rely on. Since I used the "No Parents" variable to restrict the sample, I removed it as a control for this regression. For these women, access to the Pill before age 21 is associated with a 0.6 percentage point decrease in the probability of poverty when ELA is defined by birth and a 0.8 percentage point decrease when ELA is defined by current state of residence.

In Table 4, I repeat the analysis without the potentially endogenous controls, including only the controls from equation 1. When I do not control for many of the channels through which ELA likely affects poverty, most of the point estimates are twice as large (or slightly less than twice as large) as those contained in Table 3. The pattern by age is similar regardless of whether equation 1 or equation 2 is used. The largest effect of ELA on poverty is observed for women in their 20s.

The results for black women are different in Table 3 and Table 4. Table 3, controlling for several channels through which ELA plausibly affects poverty, finds no remaining effect of ELA on black women's poverty rates. The comparison of white and black women in Table 3 is consistent with racial differences in access to medical care, particularly having access to a doctor who would be willing to prescribe the Pill and being able to pay for it. The comparison of white and black women in Table 4 is less consistent with this explanation. Instead, the full effect reported in Table 4 shows that ELA is associated with a 1.2 percentage point reduction in the poverty rate for black women. A comparison of the two tables suggests that the channels of highest degree completed, number of kids, marital status, employment status, and living

with own parents fully explain the effects of ELA on poverty for black women. For white women, there are additional channels through which ELA affects poverty status. This list potentially includes occupational choice, quality of schooling (beyond what is captured in the blunt measure of highest grade completed), on-the-job human capital investments, and husbands' human capital and earning potential.

Robustness to econometric specifications

Table 5 contains the results of different econometric specifications. As in Table 3, ELA is assigned by birthplace in Column 1 and by current state of residence in Column 2. Each cell represents a unique regression. Although only the coefficients on the ELA variables are reported, each regression includes the same set of controls as in equation 2 unless otherwise noted.

Row 1 includes only women who are currently living in their birth state at the time of observation. This is the specification for which there is the highest probability that women are correctly assigned ELA status. If a woman is still living in her birth state at time of observation, then it is highly likely that she was also in the state at age 20, the age at which ELA is assigned. Since a woman's state of residence at time of observation is also her birthplace, ELA assignment for these women is the same for both columns 1 and 2. Among these women, ELA is associated with a 0.6 percentage point decrease in poverty.

In Row 2, I again include all women 16-44, but remove number of children as a control in the regression. When this variable is removed, ELA is associated with a 0.7 percentage point decrease in the female poverty rate.

In Row 3 I include controls for state-year fixed effects instead of separate state and year dummy variables. Because ELA varies at the state level, there is concern that there could be time-varying unobservables that are correlated within a state. To address time-varying unobservables, I estimate a specification with state-year fixed effects. In this specification, the identifying variation comes from women in different birth cohorts living in the same state. When controls for state-year fixed effects are used, access to birth control is associated with a 0.3 percentage point decrease in poverty rates among all women when ELA is assigned by birthplace.

Row 4 contains the results when a probit model is used rather than an OLS regression. I am reporting the marginal effects, evaluated at the mean. Using a probit regression, the marginal effects of ELA are smaller and statistically insignificant.

Although the results are not reported, I also analyzed the effect of birth control on female poverty rates at the cohort level, rather than at the individual level, where cohort is defined as all the women born in a certain state in a certain year. The effect of ELA is measured by looking at the differences in outcomes between the groups of women who had legal access to birth control at age 20 and the groups who did not have ELA. This strategy, which is similar to the one used by Ananat and Hungerman (2011), is useful because it deals with

the concern that there might be errors that are correlated within a state-birth year cohort. When equation 1 is analyzed at the cohort level, ELA is significantly associated with a 0.8 percentage point decrease in poverty rates among women.

Different Multiples of the Poverty Line

While most of this paper focuses on being above or below the official poverty line, I also examined other variations of the poverty line, including being below 50 percent of the poverty line, 150 percent of the poverty line, and 200 percent of the poverty line. Table 6 shows the effect of ELA on these other outcomes. "Deep poverty" is defined as being below 50 percent of the poverty line. ELA is associated with a 0.5 percentage point decrease in the probability that a woman is in deep poverty. Having ELA results in a 0.3 percentage point decline in a woman's chance of being below 150 percent of the poverty line. ELA has no significant effect on the probability that a woman is below 200 percent of the poverty line.

Variations in coding of legal changes that provided access to birth control

Various authors have established different codings of the state laws that made the Pill accessible to young single women, based on their interpretations of the laws. In my main regression, I used the coding from Bailey, et al (2011). Table 7 contains the estimates of early access to the Pill using different authors' codings using equation 2.

Row 1 contains the estimate using the coding from Bailey et. al (2011) and assigns ELA based on birth control access for women at age 20. This specification is the same specification used in the main regression reported in Table 2, column 3.

Because some of the codings are only available based on women's access to birth control at age 19, not age 20, I also estimated the effect when ELA is assigned at age 19 rather than age 20 using the coding of Bailey et. al (2011). These estimates are reported in Row 2. Point estimates are generally smaller with alternative codings, 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 measurement error biases coefficients towards zero, and because I believe the extent of measurement error is greater when state of current residence is used to proxy for state of residence at age 20.

I repeat this analysis using equation 1, which only includes the controls that cannot plausibly be affected by ELA. These results are contained in Table 8. Without the "bad controls," most of the estimates of the effect of ELA are about twice as large as the estimates obtained using a fuller set of controls.

B. Other Outcomes of Interest

I look at the direct effect that ELA has on some of the other outcomes that could also affect poverty rates among women, such as college graduation, becoming a young mom, being divorced, and the number of children a woman

has. Table 9 contains the results of regressions with other dependent variables of interest.

ELA is associated with a 1.2 percentage point increase in the probability that a woman receives a college degree. This finding is consistent with other literature (Ananat and Hungerman, 2012; Hock, 2007). In previous literature, authors have not included census data from 1960. Although it is not reported in the table, I ran the regression without 1960 data as well and found a similar point estimate. The fact that access to the Pill at a young age is consistently positively associated with greater educational attainment for women suggests one possible explanation for why birth control would also reduce poverty rates among women, as it is evidence that the Pill increases women's incentive to invest in their own human capital.

ELA is significantly negatively associated with a woman becoming a young mom, which is defined by giving birth before age 21. Having access to the Pill results in a 2.4 percentage point decrease in the chance that a woman has a child before age 21. Preventing unplanned pregnancies, especially teenage pregnancies, is another channel through which the Pill could work to reduce poverty among women.

ELA is negatively associated with a woman's fertility. Having access to the Pill at age 20 is significantly associated with a 0.11 decrease in the number of children a woman has at the time of observation.

ELA is also negatively associated with divorce rates among women 30-44, which is consistent with findings from Ananat and Hungerman (2012). I find that

having access to the Pill is associated with a 0.7 percentage point decrease in divorce rates among women 30-44. I limit the sample to this older age range in order to maintain consistency with previous literature. Divorce is an outcome that is much more likely to occur in the older subset of women, who have had time to both marry and divorce. 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. Although Table 7 does not report the result, I also replicated this regression excluding women who reported being never married. When these women were dropped (about 10% of women 30-44), the effect of ELA remained of similar magnitude.

Because ELA is associated with a lower probability that a woman is in poverty, 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 and 0 otherwise. There is no significant association between access to birth control and the probability that a woman is receiving welfare payments. This result is not overly surprising, as the typical cash welfare recipient is well below the poverty line.

VI. Conclusion

Using the exogenous state-by-year variation in legal access to birth control as a quasi-experiment, this paper finds that having access to birth control at age 20 is significantly associated with a 0.9 to 1.0 probability of poverty among women when only exogenous controls are included. When a fuller set of controls is included, including potentially endogenous controls, access to birth control at age 20 is associated with a 0.4 to 0.5 decrease in the probability that a woman will be in poverty. There are limitations to both of my regression specifications. For both models, there are omitted variables, as discussed previously. The omitted variable problem is likely more severe in equation 1, which includes a sparse set of controls. In the second specification, equation 2, the omitted variable problem is reduced but many of the controls are considered "bad controls," as they are potentially endogenous to the explanatory variable of interest and therefore controlling for some of the full effect of ELA on poverty.

This study also finds that some of the channels through which ELA is likely to affect poverty rates are also directly affected by ELA. ELA is associated with a 1.2 percentage point increase in a woman's probability of graduating from college, a 2.4 percentage point decrease in the probability of becoming a young mom, a 0.1 decrease in the average number of children women have, and a 0.7 percentage point decrease in the probability that a woman is divorced.

This paper adds to the growing body of research that highlights the positive effects of birth control on women's outcomes. 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.

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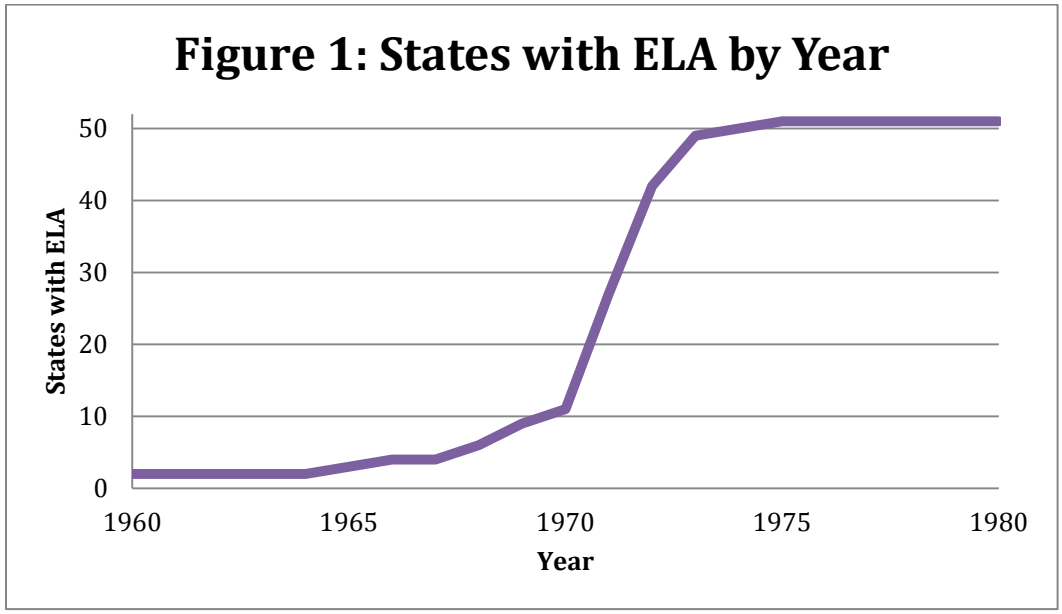
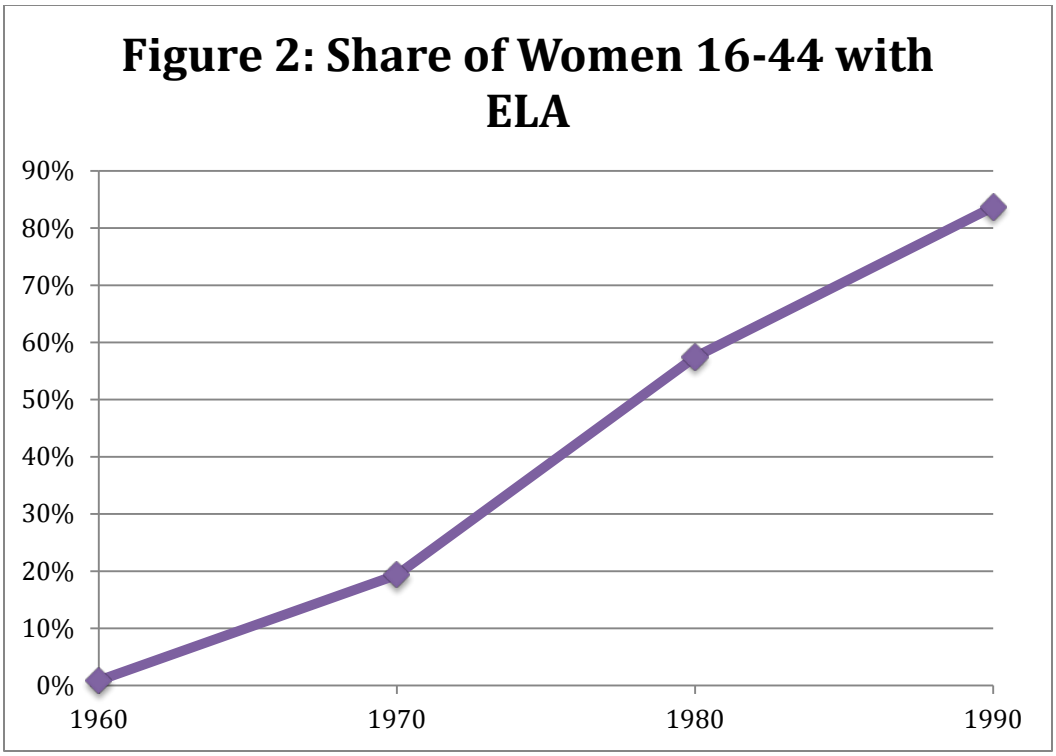


This figure illustrates the number of states that had ELA by year based on coding by Bailey, et. al, 2011. A state is considered to have ELA if single women had legal access to the Pill by age 20.



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

Table 1: Summary Statistics

Panel A: 1960

	All females	Females without ELA	Females with ELA
Age	30.15	30.26	17.75***
Black	0.11	0.11	0.05***
Other	0.01	0.01	0.01
Share in poverty	0.19	0.19	0.154***
Share not married	0.27	0.26	0.763***
Share with HS diploma	0.38	0.38	0.285***
Share with some college	0.10	0.10	0.039***
Share with college degree	0.05	0.06	0.00***
Share employed	0.36	0.36	0.335***
Age at birth of first child[1]	22.96	22.97	17.684***
Number of children in household	1.67	1.68	0.183***
Share with abortion access before age 21	0.000	0.000	0.000
Share of young men in same cohort drafted to Vietnam	0.000	0.000	0.000
Share of women with ELA	0.009	0.000	1.000
Number of observations	316,914	314,136	2,778

[1] the number of observations for this variable are the 208,830 women who have at least one child

*** p<0.01, ** p<0.05, * p<0.1 (These asterisks indicate the significance of a difference of means between women without ELA and females with ELA)

Panel B: 1970

	All females	Females without ELA	Females with ELA
Age	28.708	31.176	18.415***
Black	0.117	0.113	0.131***
Other	0.009	0.010	0.009***
Share in poverty	0.117	0.111	0.144***
Share not married	0.340	0.239	0.763***
Share with HS diploma	0.417	0.457	0.250***
Share with some college	0.133	0.148	0.068***
Share with college degree	0.083	0.099	0.016***
Share employed	0.443	0.469	0.333***
Age at birth of first child1	22.169	22.355	22.354***
Number of children in household	1.557	1.864	1.864***
Share with abortion access before age 21	0.116	0.010	0.557***
Share of young men in same cohort drafted to Vietnam	0.005	0.004	0.01***
Share of women with ELA	0.191	0.000	1.000
Number of observations	702,302	566,449	135,853

[1] the number of observations for this variable are the 22,742 women who have at least one child

*** p<0.01, ** p<0.05, * p<0.1 (These asterisks indicate the significance of a difference of means between women without ELA and females with ELA)

Panel C: 1980			
	All females	Females without ELA	Females with ELA
Age	28.441	36.041	22.814***
Black	0.130	0.118	0.139***
Other	0.016	0.015	0.016***
Share in poverty	0.125	0.099	0.144***
Share not married	0.437	0.250	0.575***
Share with HS diploma	0.414	0.447	0.389***
Share with some college	0.196	0.192	0.198***
Share with college degree	0.135	0.173	0.107***
Share employed	0.587	0.616	0.566***
Age at birth of first child1	22.182	23.190	20.539***
Number of children in household	1.155	1.859	0.633***
Share with abortion access before age 21	0.505	0.019	0.864***
Share of young men in same cohort drafted to Vietnam	0.004	0.006	0.003***
Share of women with ELA	0.574	0.000	1.000
Number of observations	2,284,178	971,727	1,312,451

[1] the number of observations for this variable are the 1,247,489 women who have at least one child

*** p<0.01, ** p<0.05, * p<0.1 (These asterisks indicate the significance of a difference of means between women without ELA and females with ELA)

Panel D: 1990			
	All females	Females without ELA	Females with ELA
Age	30.341	41.468	28.172***
Black	0.118	0.096	0.123***
Other	0.039	0.032	0.041***
Share in poverty	0.134	0.081	0.145***
Share not married	0.455	0.284	0.488***
Share with HS diploma	0.342	0.352	0.341***
Share with some college	0.315	0.313	0.316***
Share with college degree	0.182	0.239	0.171***
Share employed	0.678	0.753	0.664***
Age at birth of first child[1]	23.330	25.499	22.734***
Number of children in household	1.090	1.446	1.02***
Share with abortion access before age 21	0.780	0.044	0.924***
Share of young men in same cohort drafted to Vietnam	0.004	0.015	0.002***
Share of women with ELA	0.837	0.000	1.000
Number of observations	2,457,958	400,942	2,057,016

[1] the number of observations for this variable are the 1,363,085 women who have at least one child

*** p<0.01, ** p<0.05, * p<0.1 (These asterisks indicate the significance of a difference of means between women without ELA and females with ELA)

Table 2: The Effect of ELA on Poverty

	ELA by birthplace (1)	ELA by current residence (2)	ELA by birthplace (3)	ELA by current residence (4)
ELA	-.009***	-.010***	-.004***	-.005**
	(.002)	(.002)	(.001)	(.002)
HS grad	--	--	-.078***	-.078***
	--	--	(.004)	(.004)
Some college	--	--	-.096***	-.096***
	--	--	(0.006)	(.005)
College grad	--	--	-.125***	-.125***
	--	--	(.006)	(.006)
Black	.210***	.210***	.138***	.138***
	(.011)	(.011)	(.009)	(.009)
Other	.123***	.123***	.085***	.085***
	(.022)	(.022)	(.015)	(.015)
Age	-.004***	-.004***	-.002**	-.002**
	(.0009)	(.0009)	(.001)	(.001)
Age^2	.00001	.00001	-.00003*	-.00003*
	(.00001)	(.00001)	(.00002)	(.00002)
Number of children	--	--	.035***	.035***
	--	--	(.002)	(.002)
No chidren	--	--	.007	.007
	--	--	(.004)	(.004)
Single	--	--	.250***	.250***
	--	--	(.005)	(.005)
Employed	--	--	-.116***	-.116***
	--	--	(.002)	(.002)
Not living with parent	--	--	.242***	.242***
	--	--	(.006)	(.006)
State unemployment	.006***	.006***	.004**	.004**
	(.002)	(.002)	(.002)	(.002)
State welfare	-.005**	-.005**	-.005***	-.005***
	(.002)	(.002)	(.002)	(.002)
Men drafted	-.003***	-.003***	.0004**	.0004**
	(.0003)	(.0003)	(.0002)	(.0002)
Abortion access	.010***	.011***	.008***	.008***
	(.002)	(.002)	(.001)	(.002)
Number of observations	5761352	5761352	5,761,352	5,761,352

Note: This table reports the results of an OLS regression. The dependent variable is a dummy variable equal to 1 if the respondent is in poverty at time of observation and equal to 0 otherwise. The regression includes the full set of controls listed and dummy variables for state and year of observation. In columns 1 and 3, ELA is assigned based on a woman's birthplace. In columns 2 and 4, ELA is defined based on a woman's state of residence at time of observation. The coefficients associated with each independent variable are reported. The robust clustered standard errors for each control are reported in parentheses. Standard errors are clustered by state.

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Heterogeneity of Birth Control Effects with "Bad Controls"

		(1)	(2)	
		ELA by Birthplace	**ELA by Residence**	**Number of observations**
	Baseline			
(1)	Women 16-44	-.004***	-.005***	5761352
		(.001)	(.002)	
	Age subsets			
(2)	Women 16-35	-.007***	-.008***	4191851
		(.002)	(.003)	
(3)	Women 16-20	-.002	.002	1039908
		(.008)	(.009)	
(4)	Women 20-29	-.007**	-.010**	2111234
		(.003)	(.004)	
(5)	Women 30-39	-.005**	-.007**	1979132
		(.002)	(.003)	
(6)	Women 40-44	-.0007	.002	832059
	Race subsets	(.003)	(.006)	
(7)	Black women	-.0009	-.0003	705178
		(.003)	(.005)	
(8)	White women	-.004***	-.005**	4915107
		(.001)	(.002)	
	Other specifications			
(9)	Non-college graduates[1]	-.004**	-.005*	3158869
		(.002)	(.002)	
(10)	Women not living with parents	-.006***	-.008***	4463177
		(.001)	(.002)	

Note: This table shows the results from OLS regressions. Each cell represents a unique regression. The coefficients for the ELA indicator are reported along with robust clustered standard errors in parentheses. Standard errors are clustered at the state level. ELA is a dummy equal to 1 if a single woman had access to the Pill when she was age 20. In column 1, the ELA indicator is assigned based on a woman's state of birth. In column 2, ELA is assigned based on a woman's current state at time of observation. Although only the ELA coefficients are reported, each regression includes the controls for high school graduation, some college, college graduation, black, other, age and its square, number of children, having no children, single, employment, state unemployment rate, state welfare generosity, not living with parents, fraction of men from state in age cohort drafted into Vietnam, and abortion access at age 20. These controls include dummy variables for state and year indicators. Each row represents a subset of the sample as indicated by the row titles.

*** p<0.01, ** p<0.05, * p<0.1

[1] for this regression, the sample excludes women under age 25

Table 4: Heterogeneity of Birth Control Effects without "Bad Controls"

		(1) ELA by Birthplace	(2) ELA by Residence	Number of observations
	Baseline			
(1)	Women 16-44	-.009***	-.010***	5761352
		(.002)	(.002)	
	Age subsets			
(2)	Women 16-35	-.012***	-.015***	4191851
		(.002)	(.003)	
(3)	Women 16-20	-.001	.003	1039908
		(.010)	(.011)	
(4)	Women 20-29	-.012***	-.015***	2111234
		(.004)	(.004)	
(5)	Women 30-39	-.006**	-.008**	1979132
		(.002)	(.004)	
(6)	Women 40-44	-.001	.0007	832059
		(.004)	(.007)	
	Race subsets			
(7)	Black women	-.012**	-.013**	705178
		(.005)	(.006)	
(8)	White women	-.009***	-.009***	4915107
		(.001)	(.002)	
	Other specifications			
(9)	Non-college graduates[1]	-.008***	-.008***	3158869
		(.002)	(.002)	
(10)	Women not living with parents	.005	.001	4463177
		(.004)	(.001)	

Note: This table shows the results from OLS regressions. Each cell represents a unique regression. The coefficients for the ELA indicator are reported along with clustered standard errors in parentheses. Standard errors are clustered at the state level. ELA is a dummy equal to 1 if a single woman had access to the Pill when she was age 20. In column 1, the ELA indicator is assigned based on a woman's state of birth. In column 2, ELA is assigned based on a woman's current state at time of observation. Although only the ELA coefficients are reported, each regression includes the controls for black, other, age and its square, state unemployment rate, state welfare generosity, not living with parents, fraction of men from state in age cohort drafted into Vietnam, and abortion access at age 20. These controls include dummy variables for state and year indicators. Each row represents a subset of the sample as indicated by the row titles.

*** p<0.01, ** p<0.05, * p<0.1

[1] for this regression, the sample excludes women under age 25

Table 5: Alternative Econometric Specifications

		(1)	(2)	
		ELA by Birthplace	**ELA by Residence**	**Number of observations**
(1)	Women living in birth state	-.006**	-.006**	3869050
		(.002)	(.002)	
(2)	Without number of children variable	-.007***	-.007***	5761352
		(.001)	(.002)	
(3)	With state-year fixed effects	-.003**	-.004	5761352
		(.001)	(.002)	
(4)	Probit results	-.002	-.002	5761352
		(.001)	(.002)	

Note: This table shows the results from OLS regressions. Each cell represents a unique regression. The coefficients for the ELA indicator are reported along with clustered standard errors in parentheses. Standard errors are clustered at the state level. ELA is a dummy equal to 1 if a single woman had access to the Pill when she was age 20. In column 1, the ELA indicator is assigned based on a woman's state of birth. In column 2, ELA is assigned based on a woman's current state at time of observation. Although only the ELA coefficients are reported, each regression includes the controls for high school graduation, some college, college graduation, black, other, age and its square, number of children, having no children, single, employment, state unemployment rate, state welfare generosity, not living with parents, fraction of men from state in age cohort drafted into Vietnam, and abortion access at age 20. These controls include dummy variables for state and year indicators. Each row represents a subset of the sample as indicated by the row titles. Row 1 includes only women who are living in their state of birth, which was determined by the Census questions that ask about current state of residence and birthplace. Row 2 removes the control for number of children. Row 3 includes state*year fixed effects instead of the separate indicators for state and year of observation. Row 4 shows the coefficients of ELA using probit regressions instead of OLS.

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Different Multiples of the Poverty Line

	% of Poverty Line	ELA by birthplace
(1)	50	-.004***
		(.0009)
(2)	100	-.004***
		(.001)
(3)	150	-.003*
		(.002)
(4)	200	-.001
		(.002)
Number of observations		5761352

Note: This table reports the results of an OLS regression. Each row represents the result of a unique regression. In Row 1, the dependent variable is a dummy variable equal to 1 if the respondent is below 50 percent of the poverty line at time of observation and equal to 0 otherwise. In Row 2, the dependent variable is a dummy variable equal to 1 if the respondent is below 100 percent of the poverty line at time of observation and equal to 0 otherwise. In Row 3, the dependent variable is a dummy variable equal to 1 if the respondent is below 150 percent of the poverty line at time of observation and equal to 0 otherwise. In Row 4, the dependent variable is a dummy variable equal to 1 if the respondent is below 200 percent of the poverty line at time of observation and equal to 0 otherwise. Although only the coefficient of the ELA variable is reported, the regressions include the full set of controls listed in equation 2 and dummy variables for state and year of observation.ELA is assigned based on a woman's state of birth. The clustered standard errors for each control are reported in parentheses. Standard errors are clustered by state.

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Estimates Using Different Codings of Legal Changes That Granted Access to the Pill with "Bad Controls"

		ELA by Birthplace	ELA by Current Residence
		(1)	(2)
(1)	ELA at age 20	-.004***	-.005***
		(.001)	(.002)
(2)	ELA at age 19	-.003*	-.003
		(.002)	(.002)
(3)	Myers (2012)	-.003*	-.002
		(.002)	(.002)
(4)	Goldin and Katz (2002)	.002	.005
		(.002)	(.003)
(5)	Bailey (2006)	-.003*	-.003*
		(.001)	(.001)
(6)	Guldi (2008)	-.003*	-.001
		(.001)	(.002)
(7)	Hock (2008)	-.00002	.0002
		(.002)	(.003)
(8)	Bailey et. al (2011)	-.003*	-.003
		(.001)	(.001)

Note: This table shows the results from OLS regressions. Each cell represents a unique regression. The coefficients for the ELA indicator are reported along with clustered standard errors in parenthesis. Standard errors are clustered on state indicators. In column 1, the ELA indicator is assigned based on a woman's state of birth. In column 2, ELA is assigned based on a woman's current state at time of observation. Each row shows the results of the OLS regression using different authors' coding of the laws that provided access to the Pill. Although only the ELA coefficients are reported, each regression includes the controls for high school graduation, some college, college graduation, black, other, age and its square, number of children, having no children, single, employment, state unemployment rate, state welfare generosity, not living with parents, fraction of men from state in age cohort drafted into Vietnam, and abortion access at age 20. These controls include dummy variables for state and year indicators. In row 1, ELA is a dummy equal to 1 if a single woman had access to the Pill when she was age 20 based on the coding from Bailey, et. al (2011). In row 2, ELA is equal to 1 if a woman had access to the Pill at age 19 using the coding from Bailey, et. al (2011). In rows 2-8 ELA is equal to 1 if a woman had access to the Pill at 19 and 0 otherwise using the coding from the author's name(s) indicated in the row titles.

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Estimates Using Different Codings of Legal Changes That Granted Access to the Pill without "Bad Controls"

		ELA by Birthplace	ELA by Current Residence
		(1)	(2)
(1)	ELA at age 20	-.009***	-.005**
		(.002)	(.002)
(2)	ELA at age 19	-.006***	-.006*
		(.002)	(.003)
(3)	Myers (2012)	-.006***	-.005*
		(.002)	(.003)
(4)	Goldin and Katz (2002)	.001	.002
		(.002)	(.004)
(5)	Bailey (2006)	-.005***	-.004
		(.002)	(.002)
(6)	Guldi (2008)	-.004**	-.003
		(.002)	(.002)
(7)	Hock (2008)	-.001	-.001
		(.002)	(.003)
(8)	Bailey et. al (2011)	-.006***	-.006*
		(.002)	(.003)

Note: This table shows the results from OLS regressions. Each cell represents a unique regression. The coefficients for the ELA indicator are reported along with clustered standard errors in parenthesis. Standard errors are clustered on state indicators. In column 1, the ELA indicator is assigned based on a woman's state of birth. In column 2, ELA is assigned based on a woman's current state at time of observation. Each row shows the results of the OLS regression using different authors' coding of the laws that provided access to the Pill. Although only the ELA coefficients are reported, each regression includes the controls for black, other, age and its square,state unemployment rate, state welfare generosity, fraction of men from state in age cohort drafted into Vietnam, and abortion access at age 20. These controls include dummy variables for state and year indicators. In row 1, ELA is a dummy equal to 1 if a single woman had access to the Pill when she was age 20 based on the coding from Bailey, et. al (2011). In row 2, ELA is equal to 1 if a woman had access to the Pill at age 19 using the coding from Bailey, et. al (2011). In rows 2-8 ELA is equal to 1 if a woman had access to the Pill at 19 and 0 otherwise using the coding from the author's name(s) indicated in the row titles.

*** p<0.01, ** p<0.05, * p<0.1

Table 9: Other Outcomes of Interest

	(1)	(2)	(3)	(4)	(5)
	College graduation[1]	Becoming a young mom	Number of children	Divorced[2]	Receiving welfare
ELA by birth place	.012**	-.024***	-0.111***	-.007***	.0006
	(.006)	(.004)	(.009)	(.002)	(.0007)
Black	-.050***	.167***	0.410***	.049***	.073***
	(.005)	(.004)	(.024)	(.005)	(.004)
Other	-.050***	.081***	.252***	.016*	.029***
	(.009)	(.008)	(.025)	(.010)	(.004)
Age	.056***	.045***	.241***	.030***	.021***
	(.002)	(.002)	(.010)	(.002)	(.0009)
Age^2	-.0008***	-.0009***	-.003***	-.0004***	-.0003***
	(.00002)	(.00004)	(.0001)	(.00003)	(.00001)
Number of children	-.022***	--	--	-.018***	.005***
	(.004)	--	--	(.0009)	(.0006)
No children	.098***	--	--	.011***	-.057***
	(.004)	--	--	(.002)	(.004)
Single	-.021***	-.080***	-.663***	--	.137***
	(.002)	(.004)	(.014)	--	(.006)
No parent	.046***	.123***	.147***	-.059***	.056***
	(.003)	(.004)	(.016)	(.006)	(.003)
Employed	.050***	-.065***	-.517***	.058***	-.066***
	(.002)	(.003)	(.010)	(.003)	(.003)
State unemployment	.0001	-.001	.004*	.002***	.003***
	(.002)	(.0009)	(.002)	(.0007)	(.0006)
State welfare	-.002	-.004***	-.007***	.001**	-.0002
	(.001)	(.0009)	(.002)	(.0005)	(.0006)
Fraction of young men from cohort drafted in Vietnam War	.005***	-.006***	-.055***	.0003	.095***
	(.0006)	(.0009)	(.002)	(.0003)	(.003)
Access to abortion at age 20	-.019**	-.037***	-.064***	-.024***	.007***
	(.005)	(.004)	(.012)	(-.002)	-0.0008
Number of observations	5761352	5761352	5761352	2811191	5761352

Note: This table shows the results of OLS regressions. Each column shows the result of a unique regression with a different dependent variable. In column 1, the dependent variable is a dichotomous indicator equal to 1 if a woman has attained at least a college degree and 0 otherwise. In column 2, the dependent variable is a dichotomous indicator for young mom, which is equal to 1 if a woman gives birth to a child before age 21 and 0 otherwise. In column 3, the outcome of interest is the number of children a woman has at time of observation. In column 4, the dependent variable is a dichotomous indicator equal to 1 if a woman is divorced at time of observation and 0 otherwise. In all columns, the variable of interest, ELA, is assigned based on a woman's state of birth. In column 5, the dependent variable is a dummy equal to 1 if a woman reported recieving any welfare income and 0 otherwise. Along with the set of controls listed in the table, each regression includes dummy variables for state and year indicators.

*** p<0.01, ** p<0.05, * p<0.1

1 When this regression was repeated without 1960 data, the coefficient of ELAbirth is .015**

2 includes women ages 30-44