The Effects of Contraception on Female Poverty*

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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.

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1 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 and 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.\footnote{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 1988-1992 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.}

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, 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 (Goldin and Katz 2002, Bailey 2006, Hock 2007, Bailey, Hershbein and Miller 2012). Much of this literature has focused on outcomes of greatest relevance to women of high socio-economic 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 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 (Goldin and Katz 2002, Bailey 2006, Ananat and Hungerman 2012) 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 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 early legal access 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 16-44. 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 one percentage point. These are non-trivial effects, given that the mean poverty rate for non-elderly 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 which suggests that 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 (Forthcoming) 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% from the 29% estimated to prevail in the absence of anti-poverty programs.\footnote{This calculation is for 2004. The full set of anti-poverty policies is estimated to have similar effects in 1984 and 1993 (Ben-Shalom et al. Forthcoming).} The expansion of early legal access to birth control was not explicitly an anti-poverty policy, and the corresponding reduction in poverty 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.

2 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 anti-obscenity law’s references to contraceptives after the U.S. Supreme Court’s decision in \textit{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 (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-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
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 legal access to oral contraceptives at age 18 (Bailey, Guldi, Davido
and Buzuvis 2011). Figure 1 shows the cumulative number of states that had adopted ELA
over the 1960-1980 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 early legal access 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 have differential access to birth control. 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 and Jones 2013).

2.1 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. Bailey (2006) and Bailey (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 15-21 by 8.5 percent. Ananat and Hungeman (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 14-20 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 non-random 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 and Korenman 1992). Subsequent work has used miscarriage as an instrument (Hotz, McElroy and Sanders 2005), but there are concerns that miscarriage is in fact not randomly determined (Fletcher and Wolfe 2009, Ashcraft, Fernandez-Val and Lang 2013). 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 drop-out 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 30-49 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 early legal access 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. 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, early legal

\footnote{For evidence that particularly early marriages increase later-life poverty, see Dahl (2002).}
access 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 age 26 to 30, and is associated with a 15 percent increase in hours worked among women ages 16 to 30. Bailey et al. (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 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 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 (Forthcoming) 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 towards 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.

3 **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:

\[
\text{Poverty}_{ist} = \beta_1 (ELA20)_{is} + \gamma X_{1ist} + \delta \text{(State Controls)}_{st} + \alpha_t + \alpha_s + \epsilon_{ist} \\
\text{Poverty}_{ist} = \beta_1 (ELA20)_{is} + \gamma X_{1ist} + \gamma X_{2ist} + \delta \text{(State Controls)}_{st} + \alpha_t + \alpha_s + \epsilon_{ist}
\]

In both equations 1 and 2 the dependent variable is a dummy for poverty, equal to one 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 pre-tax 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 which 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 \( \beta_1 \) is predicted 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 is not observable in census data. 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 18-24 than among women ages 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 decline 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.

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. This includes race
dummies for being black or of other non-white 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. 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. 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 $\alpha_t$. Lastly, $\alpha_s$ is a set of state dummy variables, included 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 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

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4 State-specific unemployment rates are not available in 1960, and so every 1960 observation is assigned the national average unemployment rate of that year, 5.5 percent.

5 These data come from Robert Moffitt’s Welfare Benefits Database, 2002.
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 and Ellwood 1986). For these reasons, we include a measure of state-level Vietnam War participation. 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.$^6$

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 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 one if a woman had legal access to abortion in state $s$ when she was age 20.$^7$

Equation 2 adds the vector $X_2$, 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.

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$^6$The data on the number of men drafted per year per state come from Bitler and Schmidt (2011). The age-specific population data comes from the Surveillance, Epidemiology, and End Results (SEER) Program (www.seer.cancer.gov).

$^7$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.
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 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 births. Single is a dummy equal to one if the individual is not married. Employed is a dummy variable equal to one if the individual is employed at the time of observation. A person is considered employed if 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. We include a dummy equal to one 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 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.

4 Data

The data are drawn from the Integrated Public Use Microdata Sample (IPUMS) of the United States decennial census data from the years 1960, 1970, 1980, and 1990. 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. We limit the sample to women between the ages of 16 and 44. We dropped 143,559 observations, equal to 2.4% 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.

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 one, measured in each of the four census years included in the analysis.\footnote{This figure assigns ELA on the basis of birthplace, but the series is virtually identical if ELA is assigned based on state of residence.} 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 et al. (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 a positive relationship between legal access to the Pill and usage of the Pill by young women.
5 Results

5.1 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. The variable of interest, ELA, is associated with a one percentage point decrease in the probability that a woman is in poverty. 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 through increased educational attainment, an increased probability of employment, and changes in household structure.

Other controls in equation 1 behave as expected. Black and other non-white women are substantially more likely to be in poverty compared to white women. Poverty decreases with age. A one 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 one point increase in the state unemployment rate results in a 0.5 percentage point increase in overall poverty rates (Hoynes, Page and 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 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 decrease in a woman’s chance of being below poverty. This result is consistent with Bitler and Schmidt (2011), who find that birth rates decline when more men are drafted. If some women are pulled into poverty by the birth of a child, one would expect lower poverty rates when more men are at war and unavailable to father a child.

Interestingly and perhaps counter-intuitively, having access to abortion before age 21
is significantly associated with a one 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 et al. (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 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 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, is significantly associated with lower poverty among women 16-44. 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. This effect is about half as large as the full effect of ELA in columns 1 and 2. While the size of the

\[9\] 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.
effect is smaller when controlling for some of the channels through which poverty might be affected, the remaining effect of ELA on poverty is non-trivial. This effect is about the same magnitude as reducing the state unemployment rate by one 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. 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 having a college degree is associated with a 12.5 percentage point decrease. Black and other 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. 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 percentage points more likely to be in poverty.

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 one 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 0.08 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 one 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 once the fertility effect documented by Bitler and Schmidt (2011) is controlled for, the remaining effect of being in a cohort with high male Vietnam participation was detrimental to women’s outcomes.

5.2 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 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. Row 1 repeats the baseline results for the sake of comparison.

We first investigate heterogeneity by age at time of census enumeration. Having had early legal access 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 16-19 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 work experience, occupational choice, 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. In the equation 2 specification, early access to the Pill is associated with a 0.7 to 1.0 percentage point reduction in the probability of being in poverty. It makes sense that the effect is strongest for women during their 20s since they are more likely to be living independently and the longer-term effects of birth control access would have time to develop. This is also the age group for which there is the greatest difference between the ELA coefficients estimated with and without the $X^2$ controls. This indicates that the less tangible channels through which ELA affects poverty, possibly including work intensity or educational quality, are particularly important for women in their 20s. Row 4 includes women in their 30s. For this group, the ELA coefficients are similar in magnitude to the results for the full sample. Row 5 shows the results for 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 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 pre-school-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% white. The results for black women follow a different pattern. The equation 1 ELA estimates are larger for blacks than for whites. 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. Because it is well-established that there are racial differences in educational attainment, employment, and household structure for reasons entirely unrelated to birth control availability, we view the equation 2 results as the more meaningful comparison. 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. The pattern of results is suggestive of 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. Black women may have been 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.

5.3 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% 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 equation 1 and equation 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% or below 200% 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 household structure. The remaining effect of ELA on being below 150% or 200% of the poverty line is zero.

5.4 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
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 expected to be 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.

5.5 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 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 be-
tween birth control access and college completion (Ananat and Hungerman 2012, Hock 2007). These papers have not included data from as early as 1960. For the sake of comparison, we drop observations from 1960 and re-estimate 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% 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. This finding underscores that preventing unplanned pregnancies, especially teenage pregnancies, is a primary channel through 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 30-44. 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 divorce, which is consistent with findings from Ananat and Hungerman (2012). 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% of women ages 30-44), 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 one 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.

6 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.
References


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 2: Share of Women Ages 16-44 with ELA=1

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. ELA assignment is based on state of birth. Data source: IPUMS decennial data from 1960-1990.
Table 1: Sample Means

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>In poverty</td>
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<td>0.12</td>
<td>0.12</td>
<td>0.13</td>
</tr>
<tr>
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<td>28.44</td>
<td>30.34</td>
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<td>0.12</td>
<td>0.13</td>
<td>0.12</td>
</tr>
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<td>0.01</td>
<td>0.02</td>
<td>0.04</td>
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<td>High school diploma</td>
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<td>0.13</td>
<td>0.20</td>
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<td>0.08</td>
<td>0.13</td>
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<td>0.27</td>
<td>0.34</td>
<td>0.44</td>
<td>0.45</td>
</tr>
<tr>
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<td>0.44</td>
<td>0.59</td>
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</tr>
<tr>
<td>Number of children</td>
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<td>1.56</td>
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<td>0.39</td>
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<td>N</td>
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<td>702,302</td>
<td>2,284,178</td>
<td>2,457,958</td>
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</table>

The table reports means for the sample of women ages 16-44 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.
Table 2: The Effect of ELA on Poverty

<table>
<thead>
<tr>
<th></th>
<th>Equation 1</th>
<th>Equation 2</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>ELA by birthplace</td>
<td>ELA by current</td>
</tr>
<tr>
<td></td>
<td></td>
<td>residence</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>ELA</td>
<td>-0.010***</td>
<td>-0.010***</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Age</td>
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<td>-0.004***</td>
</tr>
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<td>(0.001)</td>
</tr>
<tr>
<td>Age squared</td>
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<tr>
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<td>(0.00002)</td>
</tr>
<tr>
<td>Black</td>
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<td></td>
<td>(0.011)</td>
<td>(0.009)</td>
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</tr>
<tr>
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<td>-0.096***</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>College grad</td>
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<td>-0.125***</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Zero children</td>
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<td>0.007</td>
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<tr>
<td></td>
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<td>(0.004)</td>
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<tr>
<td>Number of kids</td>
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<td>0.035***</td>
</tr>
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<td>(0.002)</td>
</tr>
<tr>
<td>Single</td>
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<td>0.250***</td>
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<td>(0.005)</td>
</tr>
<tr>
<td>Employed</td>
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<td>-0.116***</td>
</tr>
<tr>
<td></td>
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<td>(0.002)</td>
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<tr>
<td>Not living with parent</td>
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<td>0.242***</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>State unemployment</td>
<td>0.006***</td>
<td>0.004**</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Max AFDC</td>
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<td>-0.005***</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Men drafted</td>
<td>-0.003***</td>
<td>0.0004**</td>
</tr>
<tr>
<td></td>
<td>(0.0003)</td>
<td>(0.0002)</td>
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<tr>
<td>Abortion access</td>
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<td>0.008***</td>
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<td>N</td>
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<td>5,761,352</td>
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</tbody>
</table>

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 one 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.
### Table 3: Heterogeneity of ELA Effects

<table>
<thead>
<tr>
<th></th>
<th>Equation 1</th>
<th>Equation 2</th>
<th></th>
<th></th>
<th>Number of observations</th>
</tr>
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<tbody>
<tr>
<td></td>
<td>ELA by birthplace</td>
<td>ELA by current residence</td>
<td>ELA by birthplace</td>
<td>ELA by current residence</td>
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</tr>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1. Baseline</td>
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<td>-0.010***</td>
<td>-0.005***</td>
<td>-0.005**</td>
<td>5,761,352</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.002)</td>
<td></td>
</tr>
<tr>
<td>By age category</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2. Women 16-19</td>
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<td>0.002</td>
<td>-0.005</td>
<td>0.002</td>
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</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.012)</td>
<td>(0.009)</td>
<td>(0.011)</td>
<td></td>
</tr>
<tr>
<td>3. Women 20-29</td>
<td>-0.012***</td>
<td>-0.015***</td>
<td>-0.007**</td>
<td>-0.010**</td>
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</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.003)</td>
<td>(0.004)</td>
<td></td>
</tr>
<tr>
<td>4. Women 30-39</td>
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<td>-0.008**</td>
<td>-0.005**</td>
<td>-0.007**</td>
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<tr>
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<td>(0.004)</td>
<td>(0.002)</td>
<td>(0.003)</td>
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</tr>
<tr>
<td>5. Women 40-44</td>
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<td>-0.0008</td>
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<td>832,059</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.007)</td>
<td>(0.003)</td>
<td>(0.006)</td>
<td></td>
</tr>
<tr>
<td>By race</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>6. White</td>
<td>-0.009***</td>
<td>-0.009***</td>
<td>-0.004***</td>
<td>-0.005**</td>
<td>4,915,107</td>
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<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.002)</td>
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<tr>
<td>7. Black</td>
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<td>-0.013**</td>
<td>-0.0009</td>
<td>-0.0003</td>
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</tr>
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<td>(0.005)</td>
<td>(0.007)</td>
<td>(0.003)</td>
<td>(0.005)</td>
<td></td>
</tr>
</tbody>
</table>

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 one 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.
Table 4: The Effect of ELA on Being Below Multiples of the Poverty Line

<table>
<thead>
<tr>
<th></th>
<th>Equation 1</th>
<th>Equation 2</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>ELA by birthplace</td>
<td>ELA by current residence</td>
</tr>
<tr>
<td>Dependent Variable</td>
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<td>(2)</td>
</tr>
<tr>
<td>Below 50% of Poverty Line</td>
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<td>-0.008***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Below 150% of Poverty Line</td>
<td>-0.012***</td>
<td>-0.012***</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Below 200% of Poverty Line</td>
<td>-0.013***</td>
<td>-0.013***</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
<tr>
<td></td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>ELA by birthplace</td>
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<td>-0.005***</td>
</tr>
<tr>
<td></td>
<td>(0.0009)</td>
<td>(0.001)</td>
</tr>
<tr>
<td></td>
<td>-0.003*</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
<tr>
<td></td>
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<tr>
<td></td>
<td>(0.002)</td>
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</tbody>
</table>

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.
Table 5: Robustness to Alternative Coding of Legal Changes Granting Early Access to the Pill

<table>
<thead>
<tr>
<th></th>
<th>Equation 1</th>
<th></th>
<th>Equation 2</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>ELA by birthplace</td>
<td>ELA by current</td>
<td>ELA by birthplace</td>
<td>ELA by current</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>residence</td>
<td>(3)</td>
<td>residence</td>
</tr>
<tr>
<td>ELA at age 20</td>
<td>-0.010***</td>
<td>-0.010***</td>
<td>-0.005***</td>
<td>-0.005**</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>ELA at age 19</td>
<td>-0.006***</td>
<td>-0.006*</td>
<td>-0.003**</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
<td>(0.001)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Myers (2012)</td>
<td>-0.006***</td>
<td>-0.005*</td>
<td>-0.003*</td>
<td>-0.002</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Goldin and Katz (2002)</td>
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<td>0.002</td>
<td>0.002</td>
<td>0.005</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.004)</td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Bailey (2006)</td>
<td>-0.005***</td>
<td>-0.004</td>
<td>-0.003*</td>
<td>-0.003*</td>
</tr>
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<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.001)</td>
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<tr>
<td>Guldi (2008)</td>
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<td>-0.003</td>
<td>-0.003*</td>
<td>-0.001</td>
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<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Hock (2008)</td>
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<td>-0.001</td>
<td>-0.00002</td>
<td>0.0002</td>
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<tr>
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<td>(0.002)</td>
<td>(0.003)</td>
<td>(0.002)</td>
<td>(0.003)</td>
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</tbody>
</table>

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 one 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).
<table>
<thead>
<tr>
<th></th>
<th>College Graduate</th>
<th>Young Motherhood</th>
<th>Number of Children</th>
<th>Divorced</th>
<th>Receiving Welfare</th>
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<tr>
<td>ELA</td>
<td>0.010</td>
<td>-0.004***</td>
<td>-0.104***</td>
<td>-0.007**</td>
<td>0.0006</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.001)</td>
<td>(0.007)</td>
<td>(0.002)</td>
<td>(0.0007)</td>
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<tr>
<td>Age</td>
<td>0.030***</td>
<td>0.017***</td>
<td>0.306***</td>
<td>0.032**</td>
<td>0.021***</td>
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<tr>
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<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.010)</td>
<td>(0.002)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Age squared</td>
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<td>-0.0003</td>
<td>-0.004***</td>
<td>-0.0004***</td>
<td>-0.0003***</td>
</tr>
<tr>
<td></td>
<td>(0.00002)</td>
<td>(0.00001)</td>
<td>(0.0002)</td>
<td>(0.00003)</td>
<td>(0.00001)</td>
</tr>
<tr>
<td>Black</td>
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<td>0.080***</td>
<td>0.349***</td>
<td>0.046***</td>
<td>0.073***</td>
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<tr>
<td></td>
<td>(0.006)</td>
<td>(0.003)</td>
<td>(0.024)</td>
<td>(0.005)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Other race</td>
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<td>0.024***</td>
<td>0.185***</td>
<td>0.013</td>
<td>0.029***</td>
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<tr>
<td></td>
<td>(0.012)</td>
<td>(0.002)</td>
<td>(0.017)</td>
<td>(0.009)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>HS grad</td>
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<td>-0.311***</td>
<td>-0.018***</td>
<td>-0.043***</td>
<td>-0.043***</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.012)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
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<tr>
<td>Some college</td>
<td>-0.104***</td>
<td>-0.475***</td>
<td>-0.009***</td>
<td>-0.066***</td>
<td>-0.066***</td>
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<tr>
<td></td>
<td>(0.004)</td>
<td>(0.015)</td>
<td>(0.003)</td>
<td>(0.003)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>College grad</td>
<td>-0.120***</td>
<td>-0.781***</td>
<td>-0.062***</td>
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<td>-0.075***</td>
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<td>(0.015)</td>
<td>(0.004)</td>
<td>(0.004)</td>
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<tr>
<td>Zero children</td>
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<td>-0.018***</td>
<td>0.018***</td>
<td>-0.057***</td>
<td>-0.057***</td>
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<td>(0.002)</td>
<td>(0.004)</td>
<td>(0.004)</td>
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<tr>
<td>Number of kids</td>
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<td>0.019***</td>
<td>0.005***</td>
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<tr>
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<td>(0.001)</td>
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<tr>
<td>Single</td>
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<td>-0.020***</td>
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<td>(0.002)</td>
<td>(0.013)</td>
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<tr>
<td>Employed</td>
<td>0.070***</td>
<td>-0.003***</td>
<td>-0.423***</td>
<td>0.062***</td>
<td>-0.066***</td>
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<tr>
<td></td>
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<td>(0.011)</td>
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<tr>
<td>Not living with parent</td>
<td>0.073***</td>
<td>0.051***</td>
<td>0.179***</td>
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<td>0.056***</td>
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<tr>
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<td>(0.002)</td>
<td>(0.013)</td>
<td>(0.003)</td>
<td>(0.003)</td>
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<tr>
<td>State unemployment</td>
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<td>0.0004*</td>
<td>0.006***</td>
<td>0.002***</td>
<td>0.003***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.0002)</td>
<td>(0.002)</td>
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<td>(0.006)</td>
</tr>
<tr>
<td>Max AFDC</td>
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<td>-0.001**</td>
<td>-0.007***</td>
<td>0.001***</td>
<td>-0.0002</td>
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<tr>
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<td>(0.0003)</td>
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<tr>
<td>Men drafted</td>
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<td>-0.0001</td>
<td>-0.048***</td>
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<td>(0.0002)</td>
<td>(0.002)</td>
<td>(0.0003)</td>
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<tr>
<td>Abortion access</td>
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<td>-0.075***</td>
<td>-0.026***</td>
<td>0.007***</td>
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<tr>
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<td>(0.001)</td>
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<tr>
<td>N</td>
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<td>5,761,352</td>
<td>5,761,352</td>
<td>2,811,191</td>
<td>5,761,352</td>
</tr>
</tbody>
</table>

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 restricted to women age 30 and older in column 4. ELA is assigned on the basis of state of birth.