Education, Income, and Poverty:
The Long-term Impact of Abortion

Allison Davido
Faculty Advisor: Dr. Martha Bailey
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Abstract

The legalization of abortion is a controversial subject both in and out of the academic literature. Scholars and laypeople alike have argued for each side of the debate citing better or worse outcomes for those impacted by abortion. Economic theory clearly predicts that lowered costs of abortion through legalization should improve the average outcomes of children. Past empirical analyses have supported this assertion, but this analysis shows that while outcomes do improve, this improvement may not be due to the legalization of abortion. This paper also provides alternative explanations for the relative improvement in outcomes in states with lower costs of abortion. On the whole, these findings question the assumption that being "wanted" around the time of conception has an impact of later-life outcomes.

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Introduction

The legalization of abortion has been a controversial issue for years. Not only are the moral and ethical implications heavily contested, but more objective implications for welfare have also been debated. One important question is whether abortion impacts child well-being.

The economic theory used in much of the literature implies that abortion legalization should result in positive selection into a cohort (Gruber, Levine, & Staiger 1999; Ananat et al. 2006). This means that the average child's outcome is expected to be better than that of the children not born. If the children who are expected to have worse outcomes are systematically more likely to be aborted, the average outcomes of those children that are born should improve.

Empirical tests of this framework conclude that the introduction of legal abortion lowered birthrates and changed which women ultimately gave birth. The birthrates for teens, women over 35, and nonwhite women decreased more than for any other group (Levine et al. 1996). Other research has shown that the legalization of abortion improved the living circumstances into which children were born (Gruber, Levine, and Staiger 1999; Donohue and Levitt 2001). Their presumptions are that women with "unwanted" children took greater advantage of the increased availability of abortion and that these "unwanted" children, if they had been born, would have fared worse than their "wanted" peers. But does "wantedness" around the time of conception necessarily alter the later-life outcomes of children? Research by Ananat et al. (2006) provides evidence that abortion legalization improved later-life outcomes such as the likelihood of graduating from college and decreased the probability of receiving welfare.

The timing of the legalization of abortion across states provides a convenient way to test the theory and its implications. Because five states legalized abortion in 1970 prior to *Roe v. Wade*, we can compare outcomes for children born before *Roe v. Wade* in states with different statuses of

abortion legality. Furthermore, we can also compare outcomes for children in different states after the *Roe* decision.

This paper seeks to extend the findings of Ananat et al. using United States census data to investigate the impact of abortion legalization on later-life outcomes. Specifically, I test an alternative model, relaxing some of their restrictions, and provide a more flexible specification for testing the impact of the legalization of abortion. I find little support for the Ananat et al. findings, while the more flexible specification yields still no support for a strong impact of the legalization of abortion on later-life outcomes. The results provide evidence of an impact of birth in a repeal state on later-life outcomes, but the effect does not seem to be due to the legalization of abortion.

Theory

An Economic Model

Fertility decisions, like all other decisions in economics, are made by weighing costs and benefits. There are both benefits and costs associated with abortion and giving birth.[1] The costs of abortion include the monetary cost of obtaining the abortion, in addition to the psychological costs and other repercussions resulting from the abortion decision. Costs to giving birth include the monetary costs of caring for the child as well as the emotional and time investment required. Benefits of abortion include not having to raise a child that was unexpected or not desired, while the benefits to giving birth include the love and companionship a child brings, among other things.

In the most basic scenario, a woman makes a decision about whether to get pregnant (see figure 1). She first evaluates the expected payoff to pregnancy at the time of becoming pregnant.[2] This payoff will be different for each woman. Once pregnant, she decides whether to give birth. In a world of illegal abortion, the cost of abortion is extremely high. Although not necessarily

[1] I ignore here the women for whom there is a negative cost of giving birth (i.e. they want to have a child). For them, the abortion versus birth decision is irrelevant; they will choose to give birth in all cases, regardless of the cost of abortion (which we assume is always positive).

[2] See Levine (2004) for a comprehensive explanation of a model of the woman's choice of the probability of pregnancy.

prohibitory, the high cost of an illegal abortion may deter many women from this choice. The important thing to note, however, is that if abortion is prohibitively expensive, the pregnancy decision is generally dictated by how the woman perceives the payoffs to giving birth *before ever becoming pregnant*.

Because the cost of legal abortion is significantly less than that of illegal abortion, the legalization of abortion makes abortion a more viable option for many women. The option of a lower cost (and therefore, more probable) abortion allows the woman to defer the decision of whether to give birth until further evaluating the costs and benefits of a birth. Once pregnant, the woman continues to receive information about the payoff to giving birth; this information may make her more or less likely to give birth. For example, if, once pregnant, the woman finds her parents are very supportive of her decision, she may be more likely to give birth, since she will have extra support once the baby is born. Conversely, if her partner leaves her after becoming pregnant, the woman may be less willing to give birth, and she may consider abortion. Many other pieces of information about the repercussions of giving birth become apparent after pregnancy, and each of these influences the birth decision.[3]

Assume the payoff to birth is X.[4] This value varies from woman to woman, and its value is unknown when choosing to become pregnant. Further assume this payoff has a normal distribution with mean, μ, and standard deviation, σ. If she does not become pregnant, a woman receives a payoff, normalized to zero.

Once pregnant, the value of X becomes known. At this point, the woman chooses whether to give birth and receive payoff X, or abort and receive payoff –A, where A is the (positive) cost of abortion. This cost encompasses any and all costs associated with the act of abortion, monetary,

[3] For example, information about the health of the mother or child may greatly impact the birth decision.
[4] The following discussion utilizes much of the same notation as that in Ananat, et al. (2004). I change from framing the discussion in terms of costs to terms of benefits or payoffs.

psychological, physiological, and otherwise. In choosing between birth and abortion, the woman wants to maximize her expected payoff.

In this simple model, we assume two costs of abortion: a cost of abortion in states where abortion is legal, and a separate, and higher, cost in states where abortion is illegal (see figure 2). This translates to a higher "payoff" to abortion in states where abortion is legal, compared to states where it is illegal. Because women choose to give birth only when $X > -A$, only births for which the payoff is above –A will occur. Looking at figure 2, we can see that births to the right of $-A^L$ will occur in states with legal abortion, while births to the right of $-A^I$ will occur in states where it is illegal. Stated differently, all births in region B will be aborted in states where abortion is illegal; in states where abortion is legal, those in regions A *and* B will be aborted. The additional, higher payoff births that are aborted in legal states will tend to raise the average payoff of the births that do occur. Given the distribution of X, we can see that the expected payoff to a birth (the mean of the distribution to the right of the relevant "-A" value) is higher in states where abortion is legal (see figure 2). Assuming that higher payoff (i.e. more "wanted") births have better outcomes, children born in states where abortion is legal will fare better than their peers born in states where abortion is illegal.

Theoretical Discussion

The economic theory used to frame the abortion decision makes specific predictions about changes in child well-being with the introduction of legal abortion. Most models frame the legalization of abortion as an issue of selection. The introduction of abortion differentially affects the fertility and birth decisions of every woman, and this changes the "mix" of children born in a cohort. If the average child born is better off than the child not born, we say selection is "positive"; if the average child is worse off, selection is "negative."

The argument for positive selection is as follows: mothers in adverse circumstances are often thought more likely to abort. This makes the children not born due to the legalization of abortion systematically worse off than the average child. If positive selection is in fact the operating mechanism, we would expect *better* outcomes for those born in the wake of abortion legalization (i.e. the average child will have more education, higher income, a lower probability of living in poverty, etc.).

Although this reasoning is most often used, it is important to note, however, that selection need not be positive. This would occur if the women obtaining abortions were better educated or of higher socioeconomic status; these women might be more motivated to utilize abortion once available, because they have a higher opportunity cost of having a child and also better means of obtaining an abortion. Thus, the children not born due to abortion availability may have had *better* outcomes than the average child because more highly educated and wealthier women generally tend to have better living conditions than less educated and poorer women. That is, negative selection implies that those children born after abortion legalization have worse outcomes than the children not born due to abortion legalization.

We can see that the theoretical framework presented above predicts that the legalization of abortion should *improve* children's outcomes through positive selection. Women for whom the payoff to a birth is greater than the payoff to abortion will give birth. Facing different costs (and therefore payoffs) of abortion, not all women face the same abortion/birth decision. Thus, the option of legal abortion changes the "mix" of children born, theoretically improving cohort outcomes.

This model holds only so far as one critical assumption remains true: that more "wanted" births have better outcomes. Different "payoffs" to abortion lead to different fertility decisions. Women choose to give birth as long as the benefits of giving birth are greater than those of the alternative (here, abortion). In states where abortion is legal, because the cost of abortion is lower,

the "payoff" to abortion is higher. This leads women to have a higher expected payoff to a birth (see figure 2). According to theory, because these higher payoff births are more "wanted," they will fare better in the long run.

Implicit in this argument is that these "wanted" children are born into circumstances conducive to better later-life outcomes. However, many factors may interact to determine how a child develops, beyond the degree that the child is "wanted."

Economic theory assumes that mothers with more "wanted" births will be better able and more willing to provide conditions that help the child throughout life. This leads their children to have improved outcomes later in life. Yet some "unwanted" children end up doing extremely well, while other "wanted" children do not. For example, even if a child is "unwanted," a mother with more resources may find herself in a better position to provide for the child; furthermore, mothers of "unwanted" children may exhibit compensatory behaviors after birth that could make up for the fact that the child was not initially "wanted." Conversely, mothers of "wanted" children may find themselves unable to adequately care for their children.

This argument challenges the basic assumption that "wanted" children (at the time of conception) are always better off than their "unwanted" peers. If this does not hold, then the argument underlying positive selection falls apart. If whether the child is wanted does not matter for later life outcomes, then *which* women obtain abortions should not matter either. Even if certain women are disproportionately more likely to have an abortion, as previous research has shown, the effect on children's outcomes may not be systematically changed. Although abortion may impact the living circumstances into which children are born (through selection of which women utilize abortion once legal), the later-life outcomes of these children may not be greatly impacted due to compensatory actions on behalf of mothers. Because we cannot be sure *a priori* whether mothers will compensate after birth for the "wantedness" of their children, it is then an empirical question.

The empirical tests rest on the link between the outcomes of interest (education, wages, and poverty and welfare status) and child well-being. Another crucial assumption of the model is that the outcome measures are an accurate reflection of well-being. It is common in the economic literature to use economic outcomes as a measure of opportunity, with the belief that greater opportunity leads to improved outcomes. Living in poverty or receiving welfare is indicative of restricted opportunities; higher wages are assumed to improve opportunities, and thus, outcomes. And given the positive association between education and wages, more education also improves opportunities and outcomes, as it leads to higher wages.

Literature Review and Empirical Testing of Theory

Since the national legalization of abortion in 1973, scholars have analyzed its impact on children.[5] Many different aspects have been studied, from legalization's impact on the birth rate to the impact on children's living circumstances to the impact on later-life outcomes. The first two outcomes are the most widely cited and researched, while the last has been the topic of more recent study, including the present analysis.

The theoretical impact of changes in the cost of abortion on the birthrate is ambiguous; thus, the impact remains an empirical question.[6] Levine et al. (1996) found the legalization of abortion lowered birthrates between five and eight percent. This indicates that legalized abortion did impact women's fertility decisions.

Interestingly, the impact on the birthrate varied by both age and location. The birthrate declined significantly more for teens (13 percent), women above age 35 (eight percent), and

[5] See Levine (2004) for a comprehensive overview of the research literature on this subject.

[6] The number of births in a given time period is a function of the number of pregnancies and abortions. The legalization of abortion amounts to a discrete (and large) fall in the cost of abortion; it is reasonable to assume that as the cost of abortion falls, its use would increase. If the pregnancy rate stays the same or falls, births would necessarily decrease. However, if women see abortion as a type of insurance, giving them the option to get pregnant before deciding whether to abort or give birth, births may increase, as women who get pregnant may ultimately decide to have the baby once they get more information about the birth decision. See Levine et al. (1996) and Levine (2004) for a more thorough treatment of the theory behind the impact of changes in abortion cost on the birthrate.

nonwhite women (twelve percent). Furthermore, because women in states where abortion was illegal could travel to states where it was legal, the impact on the birthrate varied by distance to a state where abortion was legal. The birthrate in states where abortion was illegal located closest to legal states experienced a smaller decline than those located farther away, presumably because women nearest to legal states could travel to these states to obtain an abortion. Additionally, the convergence in birthrates, as well as general abortion use, was slower in states farther away, further supporting a definitive impact of legalized abortion on fertility decisions.

Using the Levine et al. results as a basis, others have analyzed the impact of legalized abortion on the living circumstances of those not born due to abortion legalization—what this literature refers to as the "marginal child" (Gruber, Levine, & Staiger 1999). In other words, the "marginal child" is the child a women is just indifferent between having or not. Changes in the cost of abortion change the parents of the "marginal child," as abortion costs alter the trade-off between the birth and abortion options.

Importantly, Gruber and colleagues (1999) found children born after the legalization of abortion were 60 percent less likely to be born into single-parent families, 50 percent less likely to live in households in poverty, 45 percent less likely to be on welfare, and 40 percent less likely to die in infancy. That is, they found evidence that children born after abortion legalization were "positively selected."

Not all research, however, has been supportive of positive selection. Pop-Eleches (2005) provides evidence from Romania of negative selection. Dictator Nicolae Ceauşescu outlawed abortion in 1966. Pop-Eleches found that after the implementation of the ban, the children born had higher educational attainment and better labor market outcomes.[7] He contends this occurred because

[7] Note that this is the reverse of the situation in the United States. In the United States, the abortion ban was lifted; negative selection therefore implies that legalization would cause outcomes to deteriorate.

wealthier and more educated Romanian women were more likely to obtain abortions when they were legal and were, therefore, forced to give birth in the absence of legal abortion.

Although the results from Gruber and colleagues (1999) provide compelling evidence of positive selection resulting from abortion legalization, they do not, however, speak to outcomes for the children specifically. They demonstrate how the circumstances in which children are raised changed in the wake of abortion legalization.[8]

Donohue and Levitt (2001) also argue for positive selection. They provide evidence that the legalization of abortion (and subsequent increase in its use) reduced crime rates approximately 18 to 20 years later. According to the model of positive selection, the legalization of abortion caused women in adverse conditions to disproportionately abort their children. Donohue and Levitt claim these children would have been more likely to commit crimes in their late adolescent and early adult years.[9] As evidence, Donohue and Levitt show that the states that legalized abortion prior to *Roe v. Wade* experienced significant decreases in crime before the rest of the country. They argue that legalized abortion can explain nearly half of the decrease in crime in the 1990s. However, other researchers have critiqued their findings, citing methodological flaws (Joyce 2004a and 2004b). Donohue and Levitt (2004), in turn, have responded. Although this paper will not expand on the debate, aspects of it centered on whether the abortion rate in a state accurately measures the variation in unwanted births (Ananat et al. 2006).

Ananat et al. (2006) extend the abortion analysis and examine how abortion legalization changed later-life outcomes of the "marginal child." They found significant effects of the legalization of abortion on later-life outcomes such as educational attainment and poverty status. Specifically, some of the estimates show the "marginal birth" 23 to 69 percent more likely to be a

[8] Pop-Eleches (2005) analyzes educational attainment and labor market outcomes, both later-life outcomes of interest; however, as discussed below, the results may not translate to a United States context.

[9] Note also that reduced cohort sizes due to the increased use of abortion could also account for some of the decrease in crime; however, the evidence presented by Donohue and Levitt strongly indicates that lower per capita crime rates among those born after legalization account for much of the decline in crime.

single parent, 73 to 194 percent more likely to receive welfare, and 12 to 31 percent less likely to be a college graduate (Ananat et al. 2006). However, they show that the differences in outcomes existing after early legalization continued to increase after *Roe v. Wade*. This finding indicates that differences in selection may have continued to influence the differences between states that repealed their abortion laws before *Roe v. Wade* and those that legalized as a result of the court decision (Ananat et al. 2006).[10]

Like the Ananat et al. analysis, this paper also looks beyond the living circumstances into which children are born to the actual later-life outcomes of the children impacted by the legalization of abortion. It could be that changes in the cost of abortion have an impact on children's later-life outcomes, but this need not be the case.

The literature supporting selection due to abortion legalization is comprehensive, yet some research may not be applicable to the abortion situation in the United States. For example, the situation in Romania may have been much different from that found in the United States. Contraception has been widely available to many women in the United States since the 1970s. It may have been the case that more highly educated and wealthy women in the United States were already controlling their fertility through contraception. This reduced their overall need for abortion. The most educated and wealthy women would not, therefore, be the ones most likely to obtain abortions after legalization.

This was not the case in Romania. Because abortion was previously the most popular form of birth control, the birthrate doubled in 1967 in Romania after the implementation of the abortion ban (Pop-Eleches 2005). Thus, while there may have been negative selection effects observed in Romania, the same effects might not be observed in the United States. Furthermore, if any negative selection effects existed in the United States, they may have been offset by the use of contraception.

[10] A more detailed discussion of the law changes will be given in the next section.

It does, however, seem reasonable that at least some negative selection existed, and it could have offset some of the effects of positive selection. This makes the impact of legalization on the "marginal child" more difficult to discern.

Hypotheses

This paper follows the empirical strategy used by Levine et al. (1996) to identify the impact of abortion. Levine et al. note abortion was illegal in all states prior to 1967 with several states amending their laws to allow abortion in extreme cases, such as rape or incest, as early as 1967.[11] In 1970, five states legalized abortion. The legislatures in Alaska, Hawaii, New York, and Washington repealed their statutory abortion prohibitions. In California, the California Supreme Court struck down the state's 1967 abortion statute in 1969 in *People v. Belous* [71 Cal. 2d 954; 458 P.2d 194; 80 Cal. Rptr. 354; 1969 Cal. LEXIS 299]. This led to a *de facto* repeal of California's abortion prohibition. Abortion remained illegal in all other states until abortion was legalized nationally in 1973 in *Roe v. Wade* [410 U.S. 959; 93 S. Ct. 1409; 35 L. Ed. 2d 694; 1973 U.S. LEXIS 3282].

For clarity, states are labeled as either "repeal" or "control" states. "Repeal" states are those that repealed their anti-abortion laws prior to *Roe v. Wade*—Alaska, Hawaii, New York, Washington, and California. All other states are labeled "control" states.

Because states legalized abortion in two "waves," the timing of legalization provides a mechanism by which to evaluate its impact. Outcomes for repeal and control states can be analyzed in two different ways.[12] In the first test, education, wage and poverty outcomes of repeal and control

[11] As in Levine et al. (1996), these so-called "reform" states have been grouped with the other states that did not fully legalize abortion prior to *Roe v. Wade*.

[12] Because the timing of the law changes could not, presumably, be anticipated, the timing of the law changes can be considered a random treatment, such that we can deduce a causal relationship between the legalization of abortion and its impact on later-life outcomes. Even though repeal states are not representative of all states, using a differences-in-differences framework allows us to control for the initial difference between repeal and control states.

states are compared for the cohorts born in and around the first "wave" of legalization.[13] Because repeal states changed their existing laws, thus lowering the cost of abortion in these states, we would expect to see a change in outcomes for children born in these states relative to the control group.

In the second experiment, outcomes are compared for the cohorts born directly after *Roe v. Wade* legalized abortion nationally.[14] Because *Roe v. Wade* forced the states that did not legalize abortion prior to the court case to change their legislation, we would expect to see a change in outcomes for children born in these states compared to early legalization states, since abortion would now be a lower-cost option for women in these states.[15]

If legalization did in fact cause mothers in adverse circumstances to abort children they would have otherwise given birth to if abortion was illegal, one would expect to see more favorable outcomes for children born in the early legalization cohort(s) in repeal states, as compared to children born in control states.[16] In other words, we would expect that children born between 1971 and 1973 in Alaska, Hawaii, New York, Washington, and California would have higher educational attainment, higher wage outcomes, higher college graduation rates, and have a smaller proportion of the cohort living in poverty or receiving welfare, on average, than those born at the same time in control states.[17] Additionally, I will examine whether education, wage and poverty outcomes equalized in control and repeal states for the 1974 and 1975 cohorts, after abortion became legal in all states.[18] Given the same legal status of abortion across all states, we would not expect to see any differential impact of abortion legalization on later-life outcomes in later years.

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[13] Because early legalization occurred in 1970, the first birth cohort impacted would be the 1971 cohort. The repeal decisions are assumed to be unanticipated, so fertility decisions would not be affected until after the actual repeal of the laws. The first cohort of children affected would be born roughly nine months later.

[14] Because *Roe v. Wade* was decided in 1973, the first birth cohort impacted by the ruling would be the 1974 cohort. See footnote 13 for further explanation.

[15] If children born in early legalization states had improved outcomes during the years prior to *Roe v. Wade*, we would expect to see children in states that legalized after *Roe* to "catch up" after the court decision.

[16] Again, this hypothesis supports positive selection. Refer to the theoretical discussion above; see also figure 2.

[17] We expect differential outcomes for the 1971 to 1973 cohorts because these are the years in which the status of abortion differed across states.

[18] Including the 1975 cohort allows for a slight delay in the impact of legalization.

Visually, we would expect a graph of the mean values of repeal minus control state outcomes to look like the solid line in figure 3. In the period before early legalization, there is some baseline difference, normalized to zero. After early legalization, we would expect outcomes to improve for individuals born in repeal states compared to those born in control states.[19] This relative improvement should be observed until *Roe* legalized abortion nationally, causing individuals in control states to "catch up" to their peers in repeal states. The baseline difference should resume thereafter.

Notice, however, that the solid line in figure 3 shows an abrupt change in response to abortion legalization; in actuality, the impact may have been more gradual. The Ananat et al. (2006) specification (described below) assumes a sudden change in outcomes in response to legalized abortion that is the same for each cohort group. For example, the model assumes all individuals born between early legalization and the *Roe* decision were impacted the same way. Similarly, the model assumes all individuals born after *Roe v. Wade* exhibited the same response to the national legalization of abortion. It may be the case, however, that different birth cohorts were affected differently. If this is true, then a graph of cohort outcomes may look more like the dashed line in figure 3. This graph shows a differential impact on outcomes for each birth cohort. This gradual response over time to the legalization of abortion leads to a curved shape of the graph of cohort outcomes.

Data and Methodology

The data used are taken from the five percent sample of the Integrated Public Use Microdata Series (IPUMS) of the 2000 U.S. census.[20] These data provide very large sample sizes and allow measures to be analyzed on an individual level. Furthermore, the data include the individual's state of birth, as well as direct measures of each education, wage, and poverty outcome; and only in the

[19] Note that adverse outcomes (such as poverty and welfare status) would be expected to deteriorate.
[20] Refer to http://usa.ipums.org/usa/.

2000 census were the individuals of interest the appropriate age to study these outcomes.[21,22] All data used in the regression analysis have been aggregated at the state- and year-of-birth level. District of Columbia is included in the state analysis; years of birth under consideration are 1960 to 1980. Poverty status was determined by year 2000 data from the U.S. Census Bureau based on income and family size.[23] Welfare status was determined by whether an individual had reported receiving any welfare income in the 2000 census.

My analysis will do two things. It first replicates the model used by Ananat et al. (2006); it then loosens their restrictions and estimates a new model. To replicate the Ananat et al. analysis of the impact of abortion on later-life outcomes, I estimate models of the following form,

$$\ln(Y)_{s,t} = \beta_1 repeal_s * D7173_{s,t} + \beta_2 repeal_s * D7475_{s,t} + \beta_3 repeal_s * D7679_{s,t} + \beta_4 \mathbf{X}_{s,t} + \varepsilon_{s,t}, \qquad (1)$$

where outcomes, Y, are measured as the average value for birth state *s* in year of birth *t*, *repeal* is a dummy variable indicating whether an individual was born in a repeal state, and *D7173, D7475,* and *D7679* are cohort dummy variables indicating if an individual was born in the years 1971 to 1973, 1974 to 1975, or 1976 to 1979, respectively.[24] **X** is a vector of state and cohort dummies, quadratic trends by state of birth, and other economic and demographic controls.

[21] Comparing educational attainment in earlier censuses did not provide instructive analysis, as the distribution of educational attainment was too concentrated to see any differential impact. In 2000, however, the 1971 cohort was around 29 years old. Although all schooling may not be completed by age 29, it is reasonable to assume that high school and most, if not all, of college would be completed. Therefore, analysis of educational attainment in 2000 shows if individuals born in control states in the period of early legalization failed to achieve the same levels of education as those born in repeal states. For the wage analysis, individuals born around the time of abortion legalization would be in the labor force full-time only during the 2000 census.

[22] See table 1 for summary statistics of the variables used in the analysis.

[23] See http://www.census.gov/hhes/www/poverty/threshld/thresh00.html for further details.

[24] These three cohorts appear to be the most relevant for discerning the impact of abortion legalization. The years 1971 to 1973 include individuals born after early legalization but before *Roe v. Wade*, when we would first expect to see differential outcomes for those born in repeal and control states. The years 1974 to 1975 include individuals born directly after *Roe v. Wade*, presumably when the outcomes for individuals born in control states would "catch up" to those in repeal states. 1976 to 1979 is a period long enough after national legalization that the differential impact of early legalization should no longer be observed.

The coefficients of interest in equation (1) are β_1, β_2, and β_3. These coefficients measure the differential percentage change in the outcomes for each cohort group born in repeal states compared to the difference between repeal and control states for individuals born before 1971.

The Ananat et al. specification allows outcomes to vary by *cohort groups*. Individuals are categorized by whether they were born in the years 1971 to 1973, 1974 to 1975, or 1976 to 1979. Categorizing individuals this way assumes that outcomes for each year of birth cohort within the cohort group are the same. To allow for a more flexible specification, this paper also estimates a new model. Rather than including cohort dummy variables for the 1971to 1973, 1974 to 1975, and 1976 to 1979 cohort groups, I estimate a model with dummy variables for each year of birth individually, interacted with a *repeal* dummy variable. This allows the outcomes measured to vary with *each* year of birth, rather than aggregating within a cohort. These specifications yield an estimating equation of the following form,

$$\ln(Y)_{s,t} = \boldsymbol{\gamma}\mathbf{Z}'_{s,t} + \boldsymbol{\alpha}\mathbf{X}'_{s,t} + \boldsymbol{\beta}\text{repeal}_s\mathbf{*X}'_{s,t} + \varepsilon_{s,t}, \quad (2)$$

where outcomes, Y, are measured as the average value for birth state *s* in year of birth *t*, **X** is 21 by 1 column vector of a constant term plus year of birth dummy variables, c = 1, 1961,…,1980, and **Z** is a column vector of all other covariates including race and sex dummy variables. This limited set of covariates is included, as I did not want to include covariates that might have been impacted *by* the change in abortion policy. For example, I did not control for education in the wage and poverty regressions, since education strongly influences wages, and changes in abortion policy may have changed the educational attainment of the affected cohorts. The outcomes measured include educational attainment, whether an individual has at least a high school degree, at least some college, or at least a college degree, earned income, whether an individual lives in poverty, or receives welfare.

To analyze educational attainment, I use the number of years of completed education. I also create outcome measures for whether an individual has received at least a high school diploma (=1, 0 otherwise), at least some college, or at least a college degree. Similar measures were created for poverty and welfare status. All estimates are weighted using census weights.

The coefficients in equation (2) can be interpreted as follows. The elements of $\boldsymbol{\alpha}$ capture the evolution of the outcome for each birth cohort in control states; all comparisons are relative to the omitted category of nonwhite men born in 1960 in control states. For instance, the estimate on the year of birth dummy for 1965 can be interpreted as the average percentage difference in the outcome for individuals born in control states in 1965 relative to 1960. Similarly, the elements of $\boldsymbol{\beta}$ capture the evolution of the difference in outcomes for each year of birth cohort born in repeal states compared to the difference between repeal and control states for individuals born in 1960. For instance, the point estimate on the interaction of *repeal*yob_1965* gives the differential percentage change in outcome for those born in 1965 in repeal states.

These estimates are the primary coefficients of interest. Adding these values to the coefficient on the single *repeal* dummy variable gives the overall impact of being born in a repeal state in a given year.

Lastly, the elements of $\boldsymbol{\gamma}$ capture the constant term, as well as the average percentage difference in the outcomes for whites and females. The constant term is the value of the outcome for the omitted category; whites and females are the two included covariates.

Comparing the two models, we can see that the form used by Ananat et al. (2006) is a subset of the new specification. The model estimated by Ananat et al. assumes that $\beta_{1971} = \beta_{1972} = \beta_{1973}$, $\beta_{1974} = \beta_{1975}$, and $\beta_{1976} = \beta_{1977} = \beta_{1978} = \beta_{1979}$. This means that any differences in the impact of abortion legalization on the 1971, 1972, and 1973 cohorts would be grouped together and averaged. The outcomes for the other cohort groups are similarly aggregated. Their restricted specification models

an impact of abortion legalization more similar to the solid line in figure 3. In relaxing these restrictions, the new model allows outcomes to vary by each birth cohort; it assumes nothing about the equality of the βs. It thus makes possible a graph of outcomes like the dashed line in figure 3.

Results

Descriptive Analysis

To begin the investigation of the link between abortion legalization and later-life outcomes, I first chart several raw correlations, not controlling for any additional factors. Doing so provides a rough idea of the effect that being born in a repeal or control state might have had. Figure 4 plots the difference in the mean years of education of men and women born from 1960 to 1980 in repeal and control states in the 2000 census.

If legalized abortion had its anticipated effect, we would expect to see some baseline difference in years of educational attainment between repeal and control states, with an increase in the difference between the years 1971 and 1974. After 1974, we should see a return to the baseline difference (see figure 3).

The initial gap between repeal and control states before abortion legalization is a difference of approximately 0.35 years in 1960. This gap remains positive for the years of birth under consideration. The general trend does not, however, support an impact of the legalization of abortion. The initial gap between repeal and control states decreases between 1960 and 1980, with a small uptick in 1971. Though small, it is a break from the overall decreasing pattern; however, the gap is rather stagnant from 1968 to 1972, and we do not see a clear shift in the pattern between the years of 1971 and 1974. In fact, it appears that a much larger change occurred between 1967 and 1968, when there was significant drop in the difference in educational attainment between repeal and control states. The small increase in 1971 does not signify any great shift from the overall trend. This implies that abortion legalization may not have impacted the mean educational attainment of the

effected cohorts. Furthermore, if the education effects were due solely to differential access to abortion, the effects should be observed until control states legalized in 1974; this is not the case. The general downward trend resumes immediately after 1971. Note also that because education is strongly correlated with age and the most current data are from the 2000 census, it could be that the younger cohorts have not fully completed their educations. Because educational attainment tends to be more similar at younger ages, the difference in educational attainment may naturally be smaller for earlier cohorts.

The effects of abortion legalization may not be readily apparent when comparing mean years of education; it may be the case that abortion legalization caused a discrete change in levels of education, shifting the distribution of educational attainment. Figure 5 plots the difference in the proportion of individuals obtaining three different levels of education: at least a high school degree, at least some college, and at least a college degree.[25] There is a trend towards convergence in the difference between repeal and control state values for all three levels of education, although individuals in repeal states, for the years of birth under consideration, tend to have higher levels of education in general. It is noteworthy that all three graph lines converge to zero, but it is also true that education tends to be more similar at younger ages, so one might expect the lines to converge for more recent cohorts.

As with years of educational attainment, if early legalization improved outcomes for those born in repeal states, we would expect a pattern similar to figure 3. If legalized abortion shifted the distribution of education, it may be true that we would only see an impact for certain levels of education. For example, if legalization increased the number attending college, we may see the expected pattern of impact in the proportion obtaining some college or at least a college degree. In figure 5 we see a small increase in 1971 in the difference between repeal and control states for those

[25] The differences charted represent repeal state values minus control state values.

obtaining at least a college degree, as we expect. In fact, there is a slight increase in the plotted differences between the years 1968 and 1972 for graphs of at least some college and at least a college degree. This indicates that the distribution of education may have shifted upward. Interestingly, looking at these graphs, after the period of increasing differences between 1968 and 1972, we see a subsequent decreasing trend, in line with the expectation that *Roe v. Wade* would cause a "catch up" in educational outcomes for those born in control states following the court decision. This timing, however, does not quite coincide with what is expected from the legalization of abortion. The increase in the difference is not expected to occur until at least 1971, while the subsequent "catch up" should not begin until 1974.

The magnitude of the changes in proportion of individuals obtaining each level of education occurring in 1971 is not as prominent as other changes in figure 5. Notice the large decreases in the difference in the proportions of individuals with each level of education occurring between 1967 and 1968; these changes appear much more prominent than any changes occurring after, when we would expect the early legalization of abortion to have its impact according to the Levine et al. (1996) coding.[26]

Figure 6 plots the difference in mean earned income in repeal and control states for individuals born between 1960 and 1980. Again, we observe an overall downward trend and the increase in 1971 is not anomalous; it is also much smaller than changes from 1964 to 1966.

As with figures 4 and 5 for educational attainment, the effect (if there is one) does not seem to be persistent. There is a slight increase in the difference in earned income of individuals born in repeal and control states in 1971, but the downward trend resumes immediately thereafter.

Note that there may also be confounding age effects that could explain the decreasing trend. The downward trend is consistent with a wage profile that "fans out" over time (see figure 7).

[26] Further coding issues will be discussed in the Alternative Hypotheses section.

Individuals tend to make more as they age. If wages increase with age at a faster rate in repeal states than in control states we would see the wage gap widen with age. This means the wage gap is largest for the oldest cohorts and smallest for the youngest cohorts, yielding a downward trend over time, as we see in figure 6.

In addition to analyzing the impact of birth in a repeal state on the mean level of earned income, it may also be instructive to examine the relationship between state of birth and poverty and welfare receipt status. Similar to using high school and college graduation status to measure changes in the distribution of educational attainment, poverty and welfare status are measures more sensitive to changes in the distribution (as opposed to the mean level) of income. Figures 8a and 9a chart the cumulative distribution of income for individuals born in repeal and control states in both 1971 and 1974, while figure 10 graphs the difference in the proportion of individuals born between 1960 and 1980 in repeal and control states in poverty. Figure 11 charts the analogous results for welfare receipt.

In figures 8a and 9a, the repeal and control state distributions appear to be almost identical below approximately $15,000 for both cohorts. Beyond this point, the graph for repeal states lies entirely below that for control states. This indicates that a higher proportion of individuals born in control states have lower incomes. Interestingly, in both 1971 and 1974, the same proportion of individuals born in repeal and control states live at or below the poverty line.[27] Thus, it does not appear in either year that birth in a repeal state changes the likelihood of living in poverty.[28]

However, closer inspection of these figures reveals different results. Figures 8b and 9b plot the same cumulative distributions of income as figures 8a and 9a, but only for income levels below $15,000. If abortion legalization had the anticipated impact we should see fewer individuals born in

[27] In 2000, the poverty line for a family of four was roughly $20,000 [U.S. Census Bureau]. I have assumed this to average to a poverty level of $5000 per person. Thus, $5000 is only a rough approximation of the poverty threshold in the United States and is used here for descriptive purposes.

[28] Since we anticipate a "catch up" in 1974, the 1974 results are in line with expectation.

repeal states in 1971 living in poverty than their peers in control states; this difference should decrease or disappear in 1974 as control state outcomes "catch up" to outcomes in repeal states. The figures do not show this pattern. Contrary to the hypothesis that the legalization of abortion should have improved outcomes for those living in repeal states, we see a *higher* proportion of individuals born in repeal states in both 1971 and 1974 living in poverty. The difference is small, but noteworthy. Not until incomes reach approximately $10,000 does the line for repeal states fall below that for control states. Beyond this point, the figures show that a higher proportion of individuals born in control states have lower incomes.

Figure 10 graphs the difference in the proportion of individuals born in repeal and control states living in poverty in the 2000 census. In the years before 1970, fewer individuals born in repeal states lived in poverty than those born in control states. However, beginning in about 1970, the difference in the proportion living in poverty began to increase. This continued until around 1977, when the difference began to decrease. The increase after 1970 is contrary to what we would expect if abortion *improved* outcomes. If this were the case we would see a *decline* in the difference in the number of individuals living in poverty in repeal states compared to control states after early legalization. The shape of this graph does not support the claim that the legalization of abortion reduced poverty outcomes.

Figure 11 plots a similar series for welfare receipt. As with the other descriptive graphs, we see a decreasing trend after approximately 1964. In line with expectations, we see a large decrease in the difference in the proportion receiving welfare between 1970 and 1972, with an increase between 1972 and 1973 before the downward trend resumes.[29] This is consistent with an impact of early legalization on welfare outcomes (with slightly altered timing), although we would not expect the downward trend to continue once control states "caught up" after national legalization.

[29] If legalization improved cohort outcomes, we should observe fewer individuals receiving welfare in repeal states compared to control states after early legalization, and hence, a decrease in the difference.

In summary the descriptive results suggest substantial differences in educational attainment, wages, poverty and welfare status between repeal and control states that decrease over time.[30] However, there are almost no discernible changes in the downward trend during the years in which abortion legalization was expected to have had the most impact. In fact, the poverty results are the *opposite* of what we expect.

While suggestive, these time series changes may be driven by changes in the composition of individuals in certain birth states across birth cohorts. In the regression analysis that follows, I include covariates to controls for these differences.[31]

Regression Analysis

The analysis proceeds as follows. I first report results for the Ananat et al. (2006) specification in equation 1. I then relax the restrictions of their model and report regression results for the specification in equation 2.

The results in table 2 estimate the model used in Ananat et al. for educational outcomes. If the legalization of abortion had the hypothesized impact, the coefficients on the different cohort interaction terms in table 2 should have the following pattern: $\beta_{D7173*repeal} > 0$ and $\beta_{D7475*repeal}$ and $\beta_{D7679*repeal}$ very close to zero. In words, outcomes for individuals born in the early legalization cohort in repeal states should improve relative to those born in control states. However, individuals born in control states between 1974 and 1975 (after national legalization) should catch up because of the uniform legal status of abortion, and we should see no further discernible difference after 1976.[32]

[30] This is not the case for poverty status. In general, the difference in the proportion of those born in repeal states versus control states living in poverty increases over time.

[31] Differential shocks to states' economies during adulthood could also impact results. These factors are often uncontrollable and are therefore not accounted for in the regression analysis in this paper; this presents a limitation of the analysis.

[32] Notice that the pattern would be reversed for adverse outcomes (such as poverty status and welfare receipt). In this case, we would expect $\beta_{D7173*repeal} < 0$ (legalization is predicted to result in better overall outcomes, and thus, fewer adverse outcomes) and $\beta_{D7475*repeal}$ and $\beta_{D7679*repeal}$ still very close to zero.

Table 2 includes results for estimates of equation 1 with the following dependent variables: educational attainment (in years) and whether the individual has received at least a high school degree, at least some college education, or at least a college degree. The variables and controls are as described above.[33] The omitted category includes nonwhite males born in control states between 1960 and 1970 and also in 1980.

First note that the point estimates on the interaction terms do not, in general, exhibit the anticipated decreasing pattern across cohorts.[34] For mean educational attainment, all point estimates are positive. This indicates that the difference between the educational attainment of those born in repeal and control states is increasing. The point estimates also increase in magnitude across cohort groups. However, if changes in the cost of abortion do impact later-life outcomes, we would expect to see control states "catch up" after *Roe v. Wade* nationally legalized abortion. In the specification with high school graduation as the dependent variable, the point estimate increases and then decreases across cohorts, while the point estimates increase over time for the some college specification. The college specification is different still—the point estimate decreases (as expected) from the 1971 to 1973 to the 1974 to 1975 cohort, but then increases for the 1976 to 1979 cohort.

In addition to failing to see the expected pattern in point estimates, few of the estimates are statistically significant; furthermore, the statistically significant point estimates are not the ones we would expect to be significant. The 1971 to 1973 cohort interaction term is significant in none of the regressions. Although we expect this interaction term to be positive and significant in all education regressions, this is not the case.[35] It appears that being born in the early legalization cohort in repeal

[33] The variables for birth cohort are 1971 to 1973 (the "early legalization" cohort); 1974 to 1975; 1976 to 1979.
[34] Note that the Ananat et al. (2006) results did not either. Their analysis provided evidence of improved outcomes for the early legalization cohort (1971 to 1973) in repeal states; however, the point estimates in their model generally *increased* in magnitude over time. This increase indicates a stronger impact of birth in a repeal state over time, with no subsequent "catch up" by control states.
[35] Notice, however, that all coefficients are positive, as we would expect if positive selection due to abortion legalization improved educational outcomes. It is the case, though, that most of these positive coefficients are not statistically discernable from zero.

states may have had a small, positive effect on educational attainment, but the impact does not appear significant.[36] However, we do see that the 1974 to 1975 interaction term is significant in only the "some college" and general educational attainment specifications; if control states "caught up" during this period, we would expect these interaction terms to be close to zero, as they generally are. If there was no statistically significant impact in the early legalization period, though, continuing to have a statistically insignificant impact is not noteworthy. It does not appear that legalization had a discernable impact. We do continue to see statistically significant interaction terms for the 1976 to 1979 interactions in those same specifications, which is not what we would expect if outcomes in control and repeal states equalized over time.

Furthermore, F-tests of the equality of the different cohort interaction terms do not suggest strong evidence of a differential impact of abortion legalization over time, as initially expected. If birth in a repeal state during the early legalization period led to improved cohort outcomes, we would expect to see a statistical difference between the 1971 to 1973 interaction and both the 1974 to 1975 and 1976 to 1979 interactions. A statistically significant difference between the 1971 to 1973 and 1974 to 1975 cohorts occurs only in the "some college" specification; a significant difference between the 1971 to 1973 and 1976 to 1979 cohorts also occurs only in this specification. Note, however, that the F-test results for the "some college" specification do not apply because the point estimates are increasing across cohorts and not decreasing as we would expect if the legalization of abortion had the theorized impact. The results indicate that the point estimates on the 1974 to 1975 and 1976 to 1979 interaction terms are significantly greater than that on the 1971 to 1973 interaction.

Looking at the results in table 2 as a whole, however, there is some evidence of a shifting educational distribution. Notice that the magnitude of the point estimate on the *D7173*repeal*

[36] In particular, birth in a repeal state in the 1971 to 1973 cohort is associated with about a one percent higher probability of obtaining some college education and about a two percent higher probability of obtaining a college degree. The latter number agrees with Ananat et al. (2006), who find that birth in a repeal state in the early legalization cohort lowers the probability of not graduating from college by between two and three percent.

interaction increases as the level of education used as the dependent variable in the specification increases. The point estimates show that birth in a repeal state in the early legalization cohort is associated with only a 0.14 percent higher probability of graduating from high school but over a two percent higher probability of obtaining at least a college degree. This indicates that abortion legalization may have had an impact at the upper end of the educational distribution. It could be the case that more "wanted" children receive the necessary support and encouragement to pursue higher education. However, because the point estimates do not exhibit the expected pattern (or significance) across cohorts, the shift in the distribution may not be attributable to the legalization of abortion.

As with educational attainment, I estimate the impact of abortion legalization on income using the model found in Ananat et al. (2006). Table 3 includes results for estimates of equation 1 with log(income) as the dependent variable.

Table 3 shows results similar to table 2. Here the omitted category includes nonwhite males born in control states between 1960 and 1970 and in 1980. Although the cohort interaction terms are all positive, we expect a decreasing pattern in the interaction coefficients across cohorts; the point estimates in the income specification are increasing over time. This indicates that the difference between the mean incomes in repeal and control states is growing over time. The latter two interaction terms are significant, even though we would expect only the first (1971 to 1973) interaction term to be statistically different from zero. Even though the 1971 to 1973 and 1976 to 1979 interaction terms are statistically different from each other (as expected), the point estimate on the latter interaction is larger and the pattern is not consistent with an impact of legalized abortion. In general the results in table 3 are not supportive of an impact of abortion legalization on income, although birth in a repeal state is associated with higher wage outcomes in all cohort groups.[37]

[37] Birth in the early legalization cohort in repeal states is associated with over one percent higher wages, while birth in a repeal state between 1974 and 1975 is associated with approximately 2.8 percent higher wages; birth in a repeal state between 1976 and 1979 is associated with approximately 3.6 percent higher wages.

Table 3 also includes results for estimates of equation 1 with poverty and welfare status as dependent variables. Again, the results are not supportive of a definitive impact of the legalization of abortion. None of the interaction terms is significant in either specification. Note that the welfare results agree with those in Ananat et al (2006). They found that birth in the early legalization cohort was associated with improved welfare outcomes.[38] In the welfare specification, the point estimate increases in magnitude from the 1971 to 1973 to the 1974 to 1975 cohort and decreases for the 1976 to 1979 cohort.[39]

More importantly, notice that the sign on the interaction coefficients in the poverty specification is opposite of expected; the results show those born in repeal states to be *more* likely to live in poverty. This does not agree with the Ananat et al. findings that the average child is *less* likely to live in poverty than the marginal child.

The signs on the interaction coefficients in the welfare specification are as expected, although the pattern is not. The point estimate becomes more negative from the 1971 to 1973 to the 1974 to 1975 cohort, although we would anticipate the estimate to move closer to zero. The point estimate for the 1976 to 1979 interaction becomes closer to zero, as expected.

Although we see an (unexpected) impact of birth in a repeal state on outcomes across all three cohort groups, we should keep in mind that social norms are also involved in the adoption of policy, and this could account for a prolonged impact of legalization. It is possible that women in states that legalized abortion later were less willing to use abortion, even if it was a viable option. States that legalized early may have had more liberal climates generally; individuals in these states were probably more likely to use abortion as soon as it became available. Individuals in the states

[38] The Ananat et al. estimates are generally greater than what this analysis finds. They find birth in a repeal state in the early legalization cohort to be associated with a nearly 15 percent lower probability of receiving welfare; the analogous value here indicates around three percent. For birth between 1974 and 1975, their value is about 22 percent; here it is between five and six percent. They also find birth in a repeal state between 1976 and 1979 to be associated with about an 18 percent lower probability of welfare receipt; the analysis here finds about 4 percent.
[39] The point estimate on the 1976 to 1979 interaction is still larger in magnitude than the point estimate on the 1971 to 1973 interaction, however.

that were required to legalize under *Roe v. Wade* may have taken more time to become comfortable with the use of abortion—hence the continued impact on those born in repeal states (Gruber et al. 1999). However, the fact that the coefficients not only remain positive but often increase over time suggests that lagging social norms may not explain the continued impact of birth in a repeal state.

The Ananat et al. (2006) specification imposes many restrictions. Their model aggregates and averages all outcomes for groups of birth cohorts rather than letting the outcomes vary by individual year of birth. Aggregating outcomes could mask any effects that could be seen in individual years. Alternatively, aggregating outcomes could also imply effects that do not exist. Averaging results across cohort groups implicitly assigns the same change in outcome to all years in the cohort group. What may look like an impact on a cohort group may not be apparent when the cohort groups are disaggregated. The new model I estimate tests for the differential change in outcomes for *each* year of birth for individuals born in repeal states.

Figures 12-15 plot the year of birth times repeal interaction terms (**β** in equation (2)), along with a 95 percent confidence interval for each educational outcome. These values give the differential impact of birth in a repeal state on educational attainment: each point estimate gives the percentage change in the difference between repeal and control state outcomes in a given year.

In figure 12, we see a decreasing trend over time, with no particular pattern supporting an impact of legalized abortion.[40] Notice that the difference in the mean years of educational attainment between individuals born in repeal and control states is falling over the entire range of years of birth under consideration. This indicates that educational outcomes are becoming more similar in repeal and control states as time progresses. There is a noticeable drop in the difference in years of educational attainment in repeal and control states after 1967, with a slight increase between 1968 and 1971. However, the decreasing trend resumes thereafter. The shape of this graph does not

[40] Recall that figure 2 plots the pattern we would expect if abortion legalization resulted in positive selection.

indicate the existence of any positive selection resulting from the legalization of abortion. Despite the small upward trend between 1968 and 1972, the break in pattern begins before early legalization and ends before *Roe v. Wade* legalized abortion nationally.

Figures 13-15 graph the impact of birth in a repeal state on different levels of education. We see different trends over the entire range of years of birth in each figure, but there is no pattern in any figure that indicates positive selection resulting from the legalization of abortion. There is a drop in 1968 in figure 14, with subsequent increases until 1972, but the downward trend resumes after 1974. The breaks from the downward trend (where they exist) do not coincide with the timing of legalized abortion. Figure 15 shows little (if any) impact of birth in a repeal state on the likelihood of college graduation until 1975, when the difference in outcomes begins to grow.

Figure 16 plots impact of birth in a repeal state on earned income. We see the gap between wage outcomes in repeal and control states closing over time; the rate of decline is even stronger than that found in the graphs for educational outcomes. There is no pattern supportive of a discrete change in outcomes for those born in the wake of abortion legalization.

Figures 17 and 18 plot the evolution of the difference between repeal and control states for poverty and welfare outcomes. The upward trend between 1969 and 1975 in figure 17 is unexpected; it indicates that the difference in the proportion of those living in poverty in repeal and control states *increased* over these years. As with the graph for earned income, the overwhelming trend for the differential impact on welfare status in figure 18 is decreasing. The pattern does not support a discrete impact of legalized abortion.

In sum, I find support for the Ananat et al. (2006) findings that birth in the early legalization cohort in repeal states resulted in improved outcomes.[41] However, even though birth in a repeal state is associated with improved outcomes, the evolution of the interaction coefficients over time does not

[41] Note, however, that I found the opposite to be true for the poverty results.

exhibit the expected pattern of "catch up" by control states in the years following national legalization. The persistent effects of birth in a repeal state could be due to the lag in social norms that accompanies discrete policy changes. In general, however, there appears to be an impact of birth in a repeal state, although it does not appear to be due to the legalization of abortion.

In an effort to extend the Ananat et al. analysis, I also estimated models that disaggregated cohorts into individual years of birth. After allowing for this more flexible specification, no discernable impact of the legalization of abortion could be seen.

Alternative Hypotheses

The assumption that the nature of legalization provided a random treatment effect is crucial to the causal inferences of this paper. This paper uses a differences-in-differences framework to control for the initial differences between repeal and control states; as time continues, it is assumed that nothing else changes between repeal and control states until legalization. Assuming the timing of the law changes was random, we can conclude that abortion legalization caused the changes in the differences in outcomes in repeal and control states.

However, one could reasonably argue that the law changes were not necessarily exogenous. The early legalization states tended to be more liberal generally. Their liberal climates and populaces, aside from inducing early abortion proceedings, may have also encouraged the immediate use of abortion once legal. States that legalized under *Roe v. Wade* were, on the whole, forced to adopt this legislation; it is unclear whether the citizens of these states would be receptive to the use of abortion, even if legal. Abortion legality does not necessarily imply abortion accessibility or use.

Additionally, the early legalization states tended to be larger and wealthier states; therefore early legalization may not represent a random treatment of all states. Not only could the legalization of abortion have had a different impact from the rest of the country in these states, but children's outcomes in these states could have been different, even in the absence of legalized abortion. For

example, wealthier states tend to have better school and health systems. Better educational systems encourage more education, which improves later-life outcomes; better healthcare similarly helps individuals. Therefore, comparison by repeal status may not be valid to discern the impact of legalized abortion, since the treatment was not distributed randomly.

Beyond the overarching social climate in a state, accessibility to abortion could have been influenced by other factors as well. Differences in abortion use by race or socioeconomic status could also be important.

For example, wealthy women may have been more likely to have access to and actually have abortions once legal, since they have the resources to obtain an abortion or the ability to travel to a state in which abortion was legal. If women of higher socioeconomic status were disproportionately more likely to have an abortion once legal, the characteristics of the marginal birth would be better than that of the average birth. We would therefore expect cohort characteristics to deteriorate relative to before legalization.

However, wealthy women could have possibly been more likely to use abortion *before* legalization (and therefore less likely after legalization), since they have money for expensive black market abortions. Thus, if the legalization of abortion lowered the cost of abortion to the point where it became more accessible to poor women, these women might have had proportionately more abortions after legalization. As a result, the marginal birth would have characteristics that are worse than that of the average birth, and cohort characteristics would improve.

Similar to the argument for the differential use of abortion by socioeconomic status, there may have been racial differences in abortion use. For example, if white women used abortion proportionally more after legalization than nonwhites (presumably because abortions are expensive and white women would have greater access via travel and monetary resources once legal), then we would expect the marginal birth to be better off than the average birth, as white women are thought to

more often have better living conditions, among other advantages; cohort characteristics would thus decline. However, if white women used abortion proportionately less than nonwhites after legalization (presumably because they had access before legalization via travel and the black market), then we would expect the marginal birth to be worse off than the average birth, and cohort characteristics would improve. Disaggregating results by race or socioeconomic status might enable us to discern the exact impact of the legalization of abortion and the women on whom it had the greatest impact.

Given the results from Levine et al. (1996) of the impact of abortion legalization on birthrates, we might also expect that, in addition to racial or socioeconomic differences, there might also be a differential impact of abortion legalization on the later-life outcomes of teens and women over age 35. Because these two groups (along with nonwhite women) experienced a larger change in birthrates in response to abortion legalization, there might be a different impact on other outcomes as well. Further investigation into the later-life outcomes of children to mothers in these various groups might reveal an impact of abortion legalization not apparent in the aggregate data.

The results in this and other papers also depend on the coding used. The preceding analysis uses the coding found in Levine et al. (1996). According to this coding, states are labeled as either "repeal" or "control." The latter group also includes twelve states that "implemented modest reforms making it legal for women to obtain abortions under special circumstances, such as rape or incest" (Levine et al. 1996, p.6).[42] These reforms occurred between 1967 and 1970. Levine and colleagues chose to group these "reform" states with control states because they found no evidence of an impact of modest abortion reforms on the birthrate. This might be expected, since rape and incest occur relatively infrequently. However, even though the birthrate may not have been greatly impacted, it may also be true that modest abortion reforms did have an impact on later-life outcomes. Children

[42] These states are Arkansas, Colorado, Delaware, Florida, Georgia, Kansas, Maryland, New Mexico, North Carolina, Oregon, South Carolina, and Virginia.

conceived out of rape or incest could be considered highly "unwanted." If "wantedness" impacts later-life outcomes, we might expect these more modest abortion reforms to have a significant effect, since women then had the option of not giving birth under these undesirable circumstances.

In the preceding analysis, most of the changes in the difference between repeal and control state outcomes occurred outside the window of time between early legalization and *Roe v. Wade*. In fact, many of the largest changes occurred between 1967 and 1968 when many of these modest reforms occurred. This supports improved outcomes as a result of abortion reform. If modest abortion reforms improved later-life outcomes, then we should see a relative improvement in the outcomes of control states compared to repeal states, or a decrease in the difference between repeal and control state outcomes.[43] The figures show this. Further analyses should disaggregate control states into those that did not legalize abortion until *Roe v. Wade* and those states that implemented modest reforms before early legalization. Although these reforms may not have significantly impacted the birthrate, they could have influenced later-life outcomes of the effected cohorts.

Finally, it may be that repeal and control state outcomes continued to evolve differentially after legalization. Even if the timing of the law changes was random, outcomes could have continued to change beyond the changes resulting from legalization. Controlling for a differential evolution of outcomes in repeal and control states after legalization is often not practical or feasible and would therefore make the impact of legalization more difficult to isolate.

Conclusion

The preceding analysis has examined education, wage, poverty and welfare outcomes for individuals born around the time of abortion legalization. Given the nature of legalization in the United States, two natural comparison groups arise. These comparison groups allow us to test the causal impact of abortion legalization on the relevant cohorts. If, as past research has maintained,

[43] This is because reform states are included with control states, so improved outcomes for reform states simultaneously improves outcomes for all control states.

positive selection occurred and abortion improved children's outcomes, we would expect to see improved results for individuals born in repeal states in the 1971 to 1973 cohort compared to their peers born in these years in control states. The opposite would be true for the 1974 to 1975 cohort; after the remainder of the United States was required to legalize under *Roe v. Wade*, we would expect control states to "catch up" relative to the more progressive states that legalized earlier. No significant changes in the difference between individuals born in repeal and control states should be observed after. It is entirely possible, however, that some element of negative selection existed as well. This would mute the impact of positive selection.

Previous research has supported that children born during the different "phases" of abortion legalization have had different outcomes. Most research, however, has focused on the living circumstances into which these children were born. Specifically, Gruber, Levine & Staiger (1999) found the "marginal child" not born due to the legalization of abortion much more likely to be born into a single-parent household, live in poverty, and receive welfare than the average child. Ananat et al. (2006) extend this analysis to later-life outcomes. They show that individuals born after abortion legalization are more likely to graduate from college and less likely to receive welfare, although the impact of birth in a repeal state appears to persist over time.

The findings presented here do not, on the whole, support an impact of abortion legalization. The descriptive analysis shows differences between control and repeal state outcomes, but it does not appear that abortion legalization had anything to do with these differences. Replication of the Ananat et al. (2006) findings provides evidence of their assertion that birth in a repeal state is associated with improved later-life outcomes, with persistent effects over time. However, the increasing pattern exhibited by many of the point estimates on the cohort interaction terms does not support an impact of legalized abortion. The poverty results are also the opposite of what Ananat et al. found. The more flexible regression specifications used allow outcomes to vary by individual

year of birth, and not only by cohort group, yet the results do not show a definitive impact on outcomes that begins in 1971 in repeal states with a "catch up" in control states in the years following national legalization. It is possible, however, that because policy changes are implemented much more quickly than social norms may change, that the impact of birth in a repeal state could be more persistent than expected.

It is also possible that a different identification strategy could yield different results. Modest abortion reform that occurred before early legalization may have also impacted the later-life outcomes of the affected cohorts. Even though the figures presented in this paper did not show the anticipated evolution of outcomes during the years of early legalization and national legalization, the figures do show changes in the difference in outcomes during the years of early abortion law reform. Aggregating outcomes of states that modestly reformed their laws before *Roe v. Wade* with those that did not change their laws may mask important differences in outcomes.

The analysis in this paper takes a broad look at the association between state and year of birth and later-life outcomes, with implications far beyond the individuals at which it looks. It has been shown that the "marginal child" is more likely to be born into a household in poverty, live in a household with a single parent, and receive welfare, but the results here show that this does not necessarily translate to adverse outcomes later in life. The analysis provides evidence that "wantedness" at conception does not necessarily improve later-life outcomes. Contrary to the fundamental assumption and empirical findings on the impact of legalized abortion, being "wanted" at conception does not appear to dictate the course of an individual's life.

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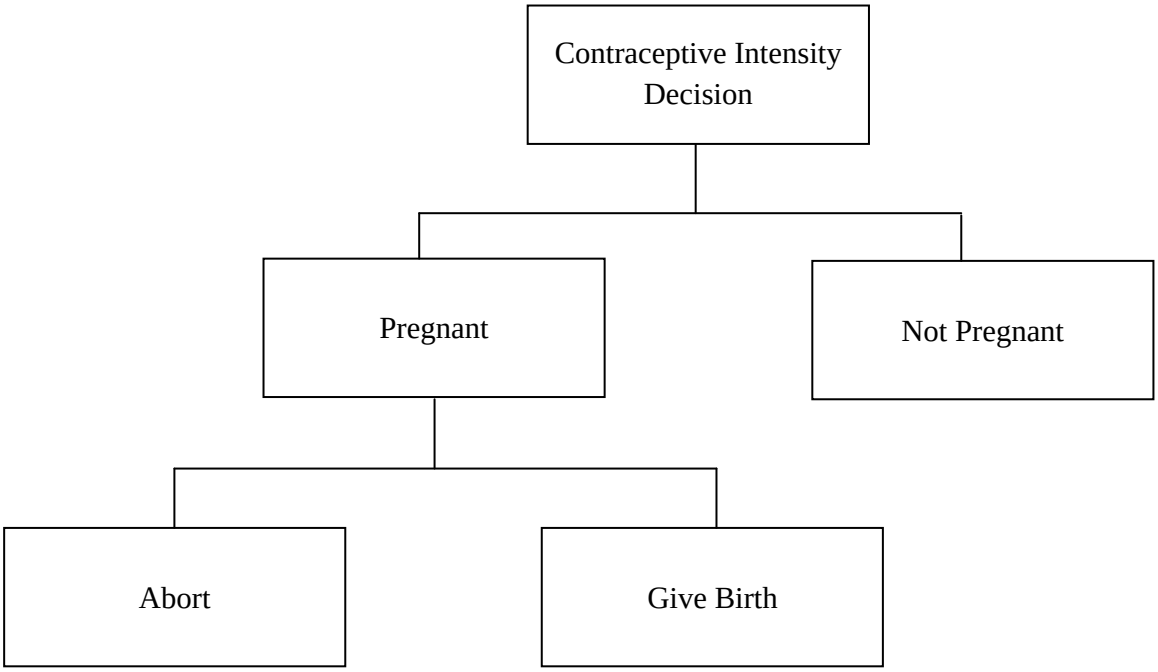
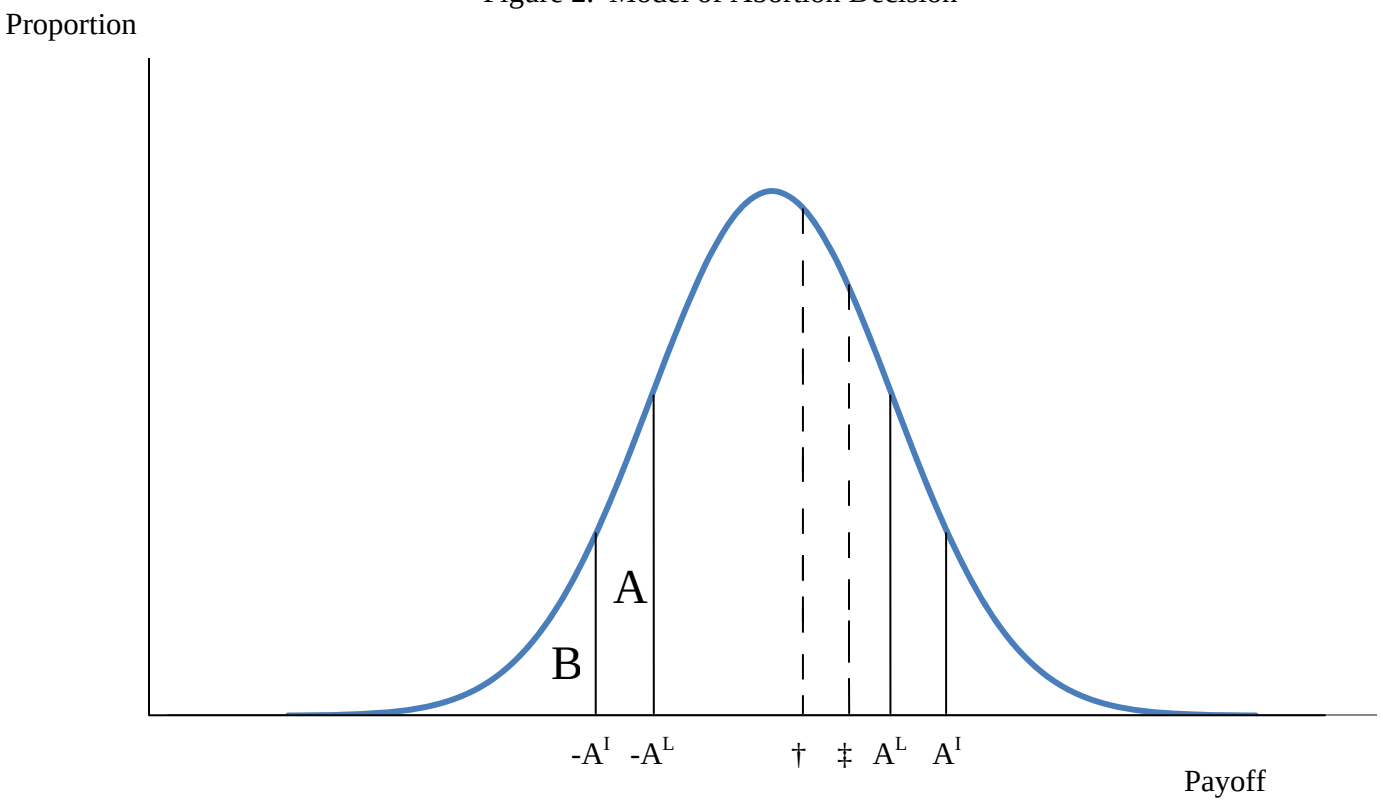
Figure 1. The Contraception/Pregnancy Decision



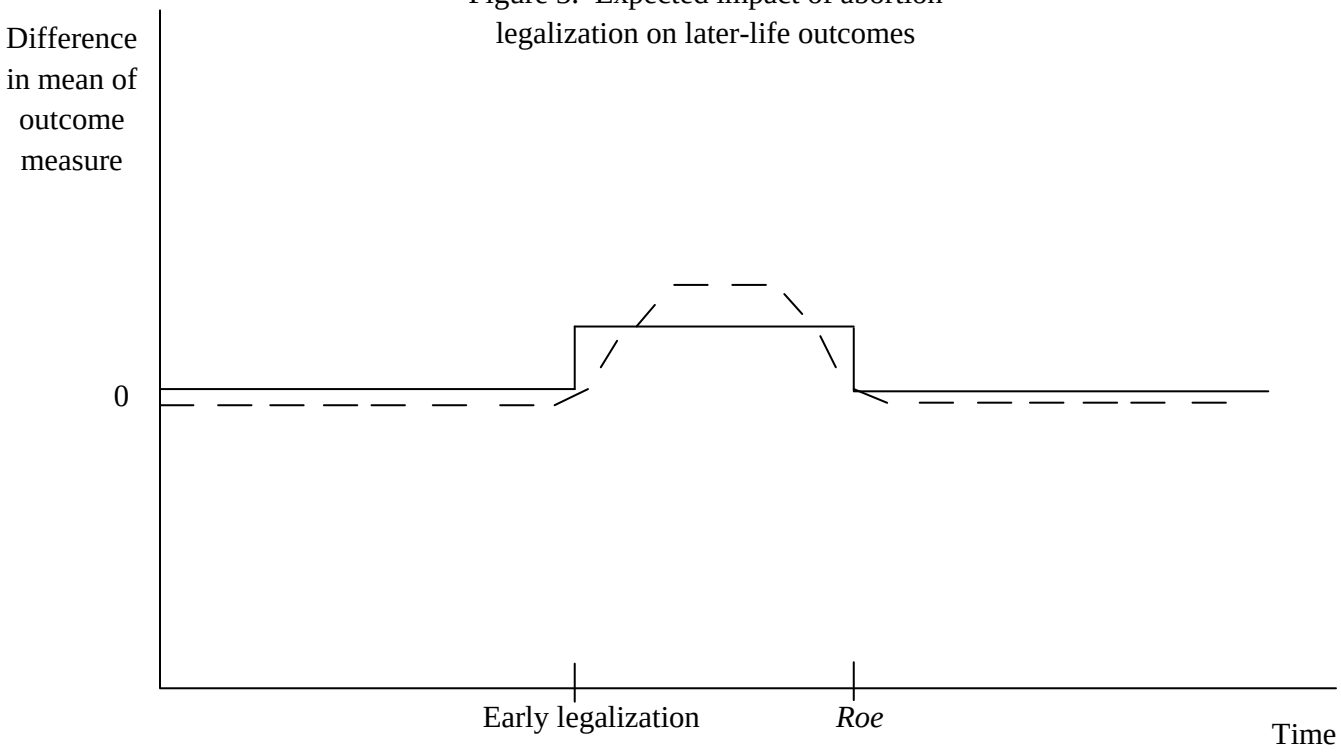
Note: This diagram was taken from Levine (2004).

Figure 2. Model of Abortion Decision



Note: "A" denotes the cost of an abortion, assumed to be positive, while "-A" represents the payoff to an abortion. A woman compares the latter value with the payoff to a birth when deciding whether to give birth. The subscripts I and L denote the cost/payoff in states where abortion is illegal and legal, respectively. † $E_I(X|birth)$; ‡ $E_L(X|birth)$

Figure 3. Expected impact of abortion legalization on later-life outcomes



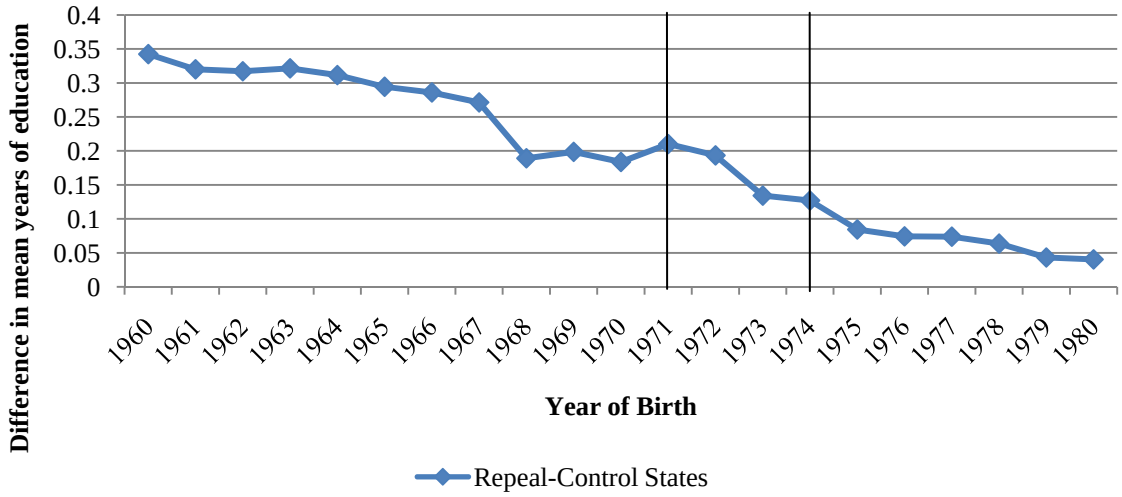
Note: Graph represents repeal-control state values; adapted from Levine (2004); adverse outcomes would be represented by a decrease (as opposed to an increase) in the mean of the outcome measure.

Table 1. Summary Statistics of Variables Used

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Proportion white	1071	0.7956	0.1146	0.2065	0.9944
Prop. female	1071	0.5057	0.0113	0.4433	0.5672
Prop. born in repeal states	1071	0.1995	0.3998	0	1
Prop. born between 1971 and 1973 in control states	1071	0.1282	0.3345	0	1
Prop. born between 1974 and 1975 in control states	1071	0.0843	0.2780	0	1
Prop. born between 1976 and 1979 in control states	1071	0.1844	0.3880	0	1
Prop. born between 1971 and 1973 in repeal states	1071	0.0249	0.1559	0	1
Prop. born between 1974 and 1975 in repeal states	1071	0.0166	0.1279	0	1
Prop. born between 1976 and 1979 in repeal states	1071	0.0369	0.1886	0	1
Years of educational attainment	1071	13.5222	0.4429	12.2247	14.5382
Prop. with at least a high school degree	1071	0.8689	0.0429	0.6861	0.9752
Prop. with at least some college education	1071	0.5666	0.0694	0.3305	0.7619
Prop. with at least a college degree	1071	0.2094	0.1002	0	0.3945
Earned income	1071	23046.95	8859.78	4588.28	40585.15
Hours worked last week	1071	39.7376	3.2704	28.7812	43.7890
Earned income, excluding negative or zero values	1071	26727.45	10163.05	5811.33	47335.24
Prop. receiving any welfare	1071	0.0249	0.0073	0.0039	0.0772
Prop. living in poverty	1071	0.4265	0.1790	0.2162	0.9241

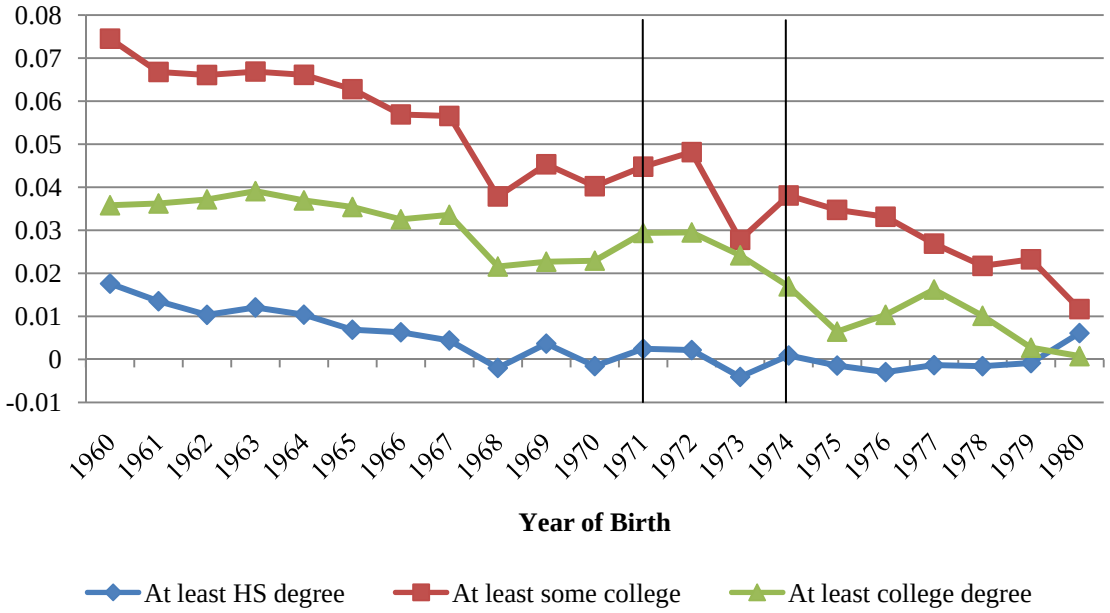
Note: The data used are taken from the five percent sample of the Integrated Public Use Microdata Series (IPUMS) of the 2000 U.S. census. All data are aggregated at the state- and year-of-birth level. District of Columbia is included in the state calculations; years of birth include 1960 to 1980.

Figure 4. Difference in mean years of education by year of birth, 2000 United States Census



Note: Educational attainment was determined by the number of years of education reported in the 2000 census.

Figure 5. Difference in proportion of those born between 1960 and 1980 with at least certain levels of education in 2000 United States Census (Repeal-Control values)



Note: Degree status was determined by whether an individual had reported receiving a high school degree or higher, any college education or beyond, or a bachelor's degree or higher in the 2000 census.

Figure 6. Difference in mean earned income by year of birth, 2000 United States Census

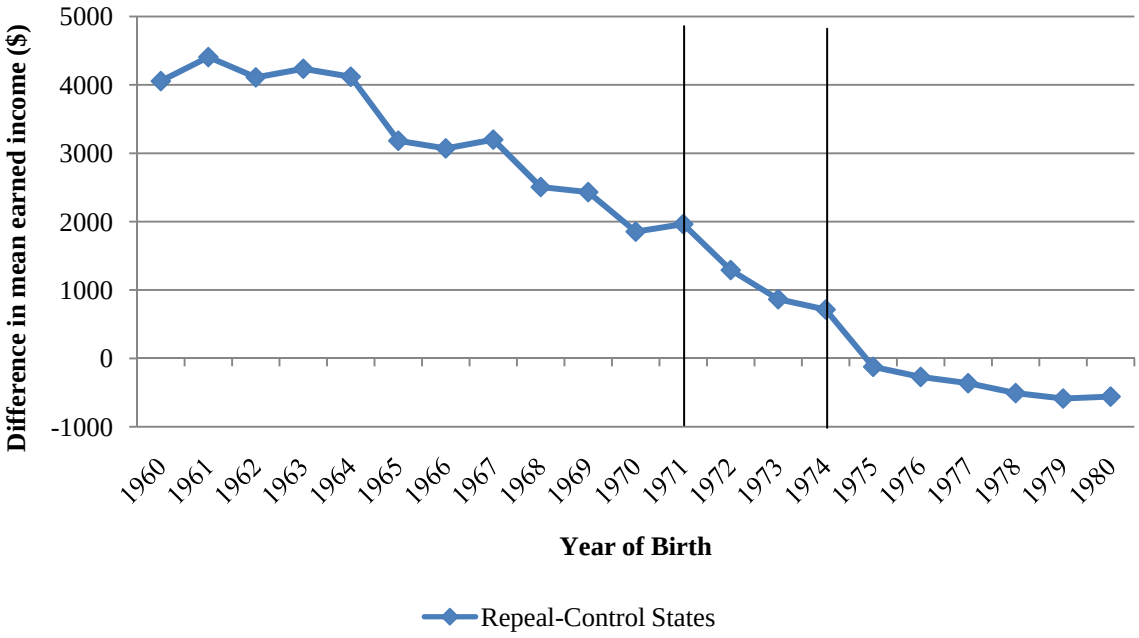


Figure 7. Theoretical Wage Profile

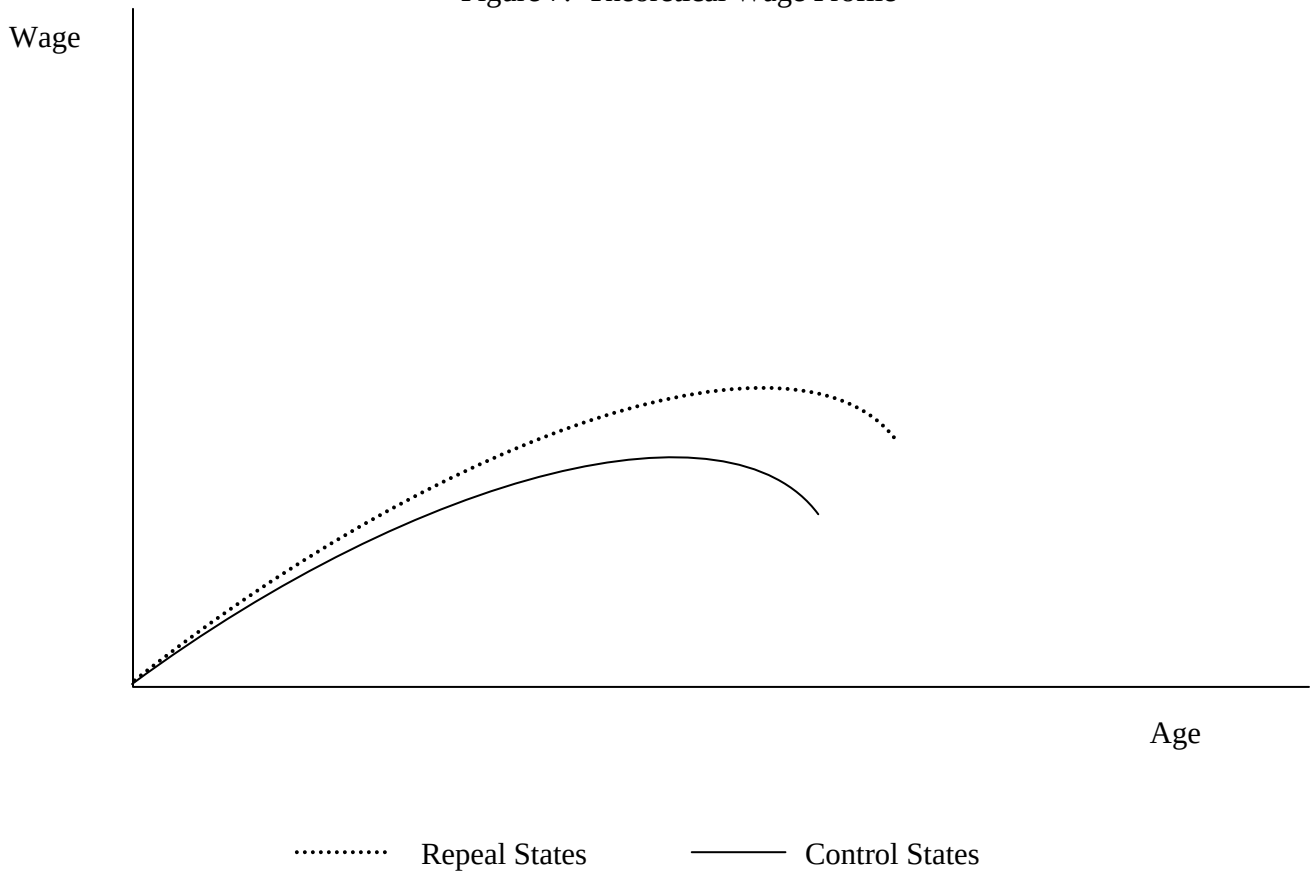


Figure 8a. Cumulative distribution of income in repeal and control states for those born in 1971, United States Census 2000

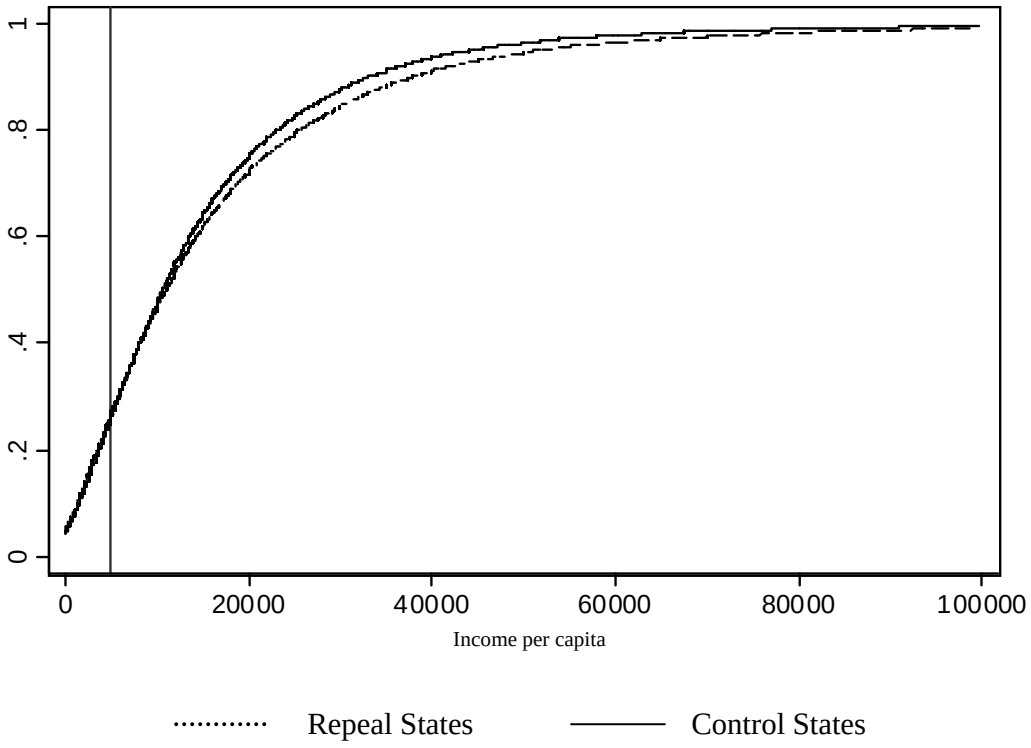


Figure 8b. Cumulative distribution of income below $15,000 in repeal and control states for those born in 1971, United States

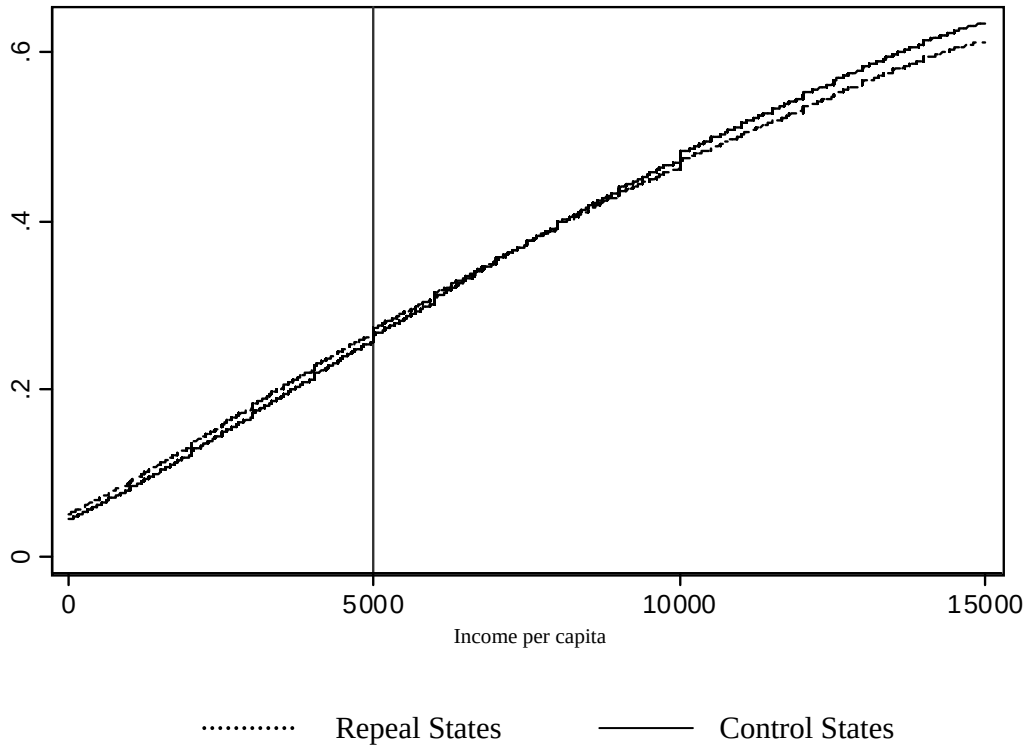


Figure 9a. Cumulative distribution of income in repeal and control states for those born in 1974, United States Census 2000

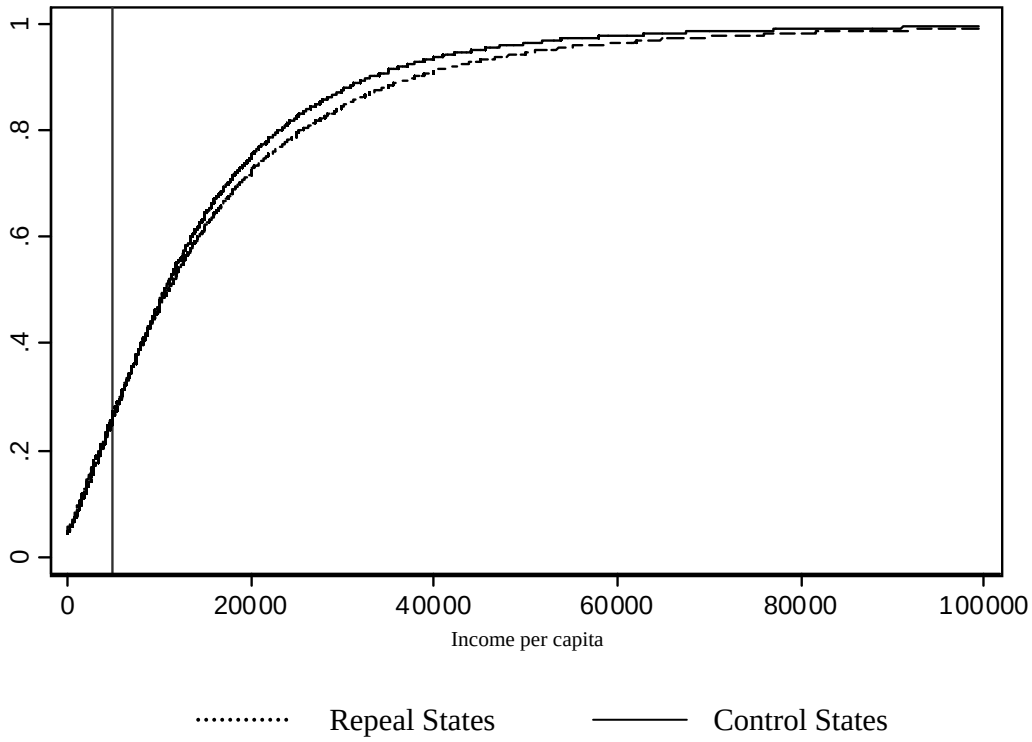


Figure 9b. Cumulative distribution of income below $15,000 in repeal and control states for those born in 1974, United States

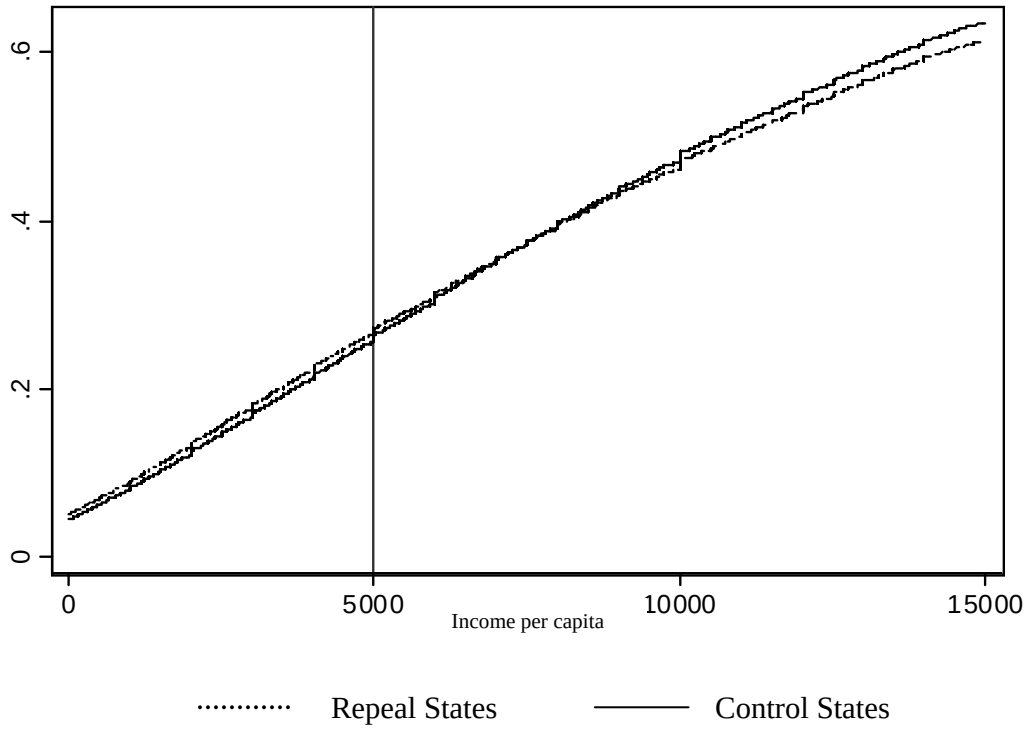


Figure 10. Difference in proportion in poverty by year of birth, 2000 United States Census

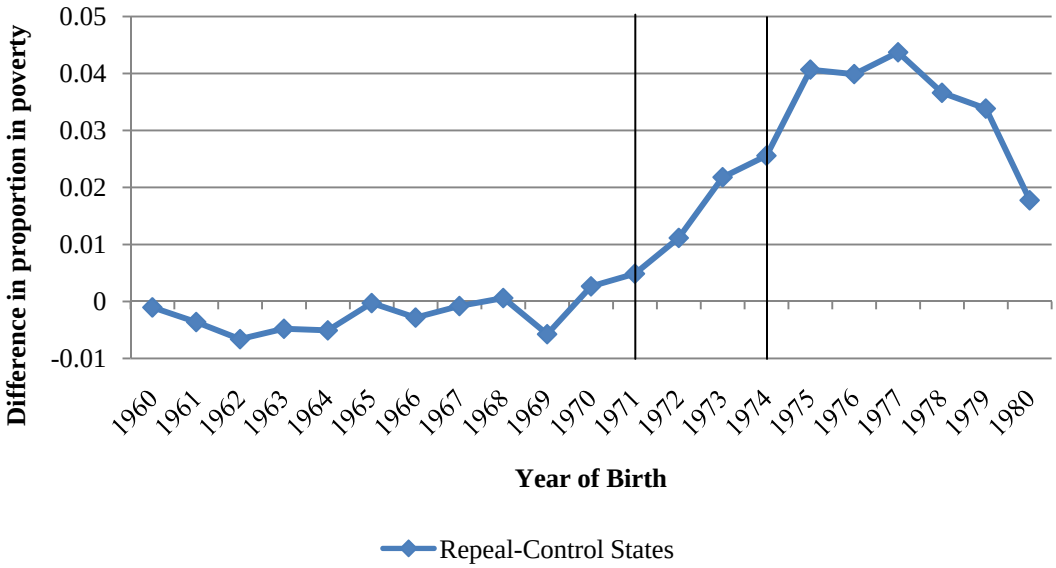


Figure 11. Difference in proportion receiving any welfare income by year of birth, 2000 United States Census

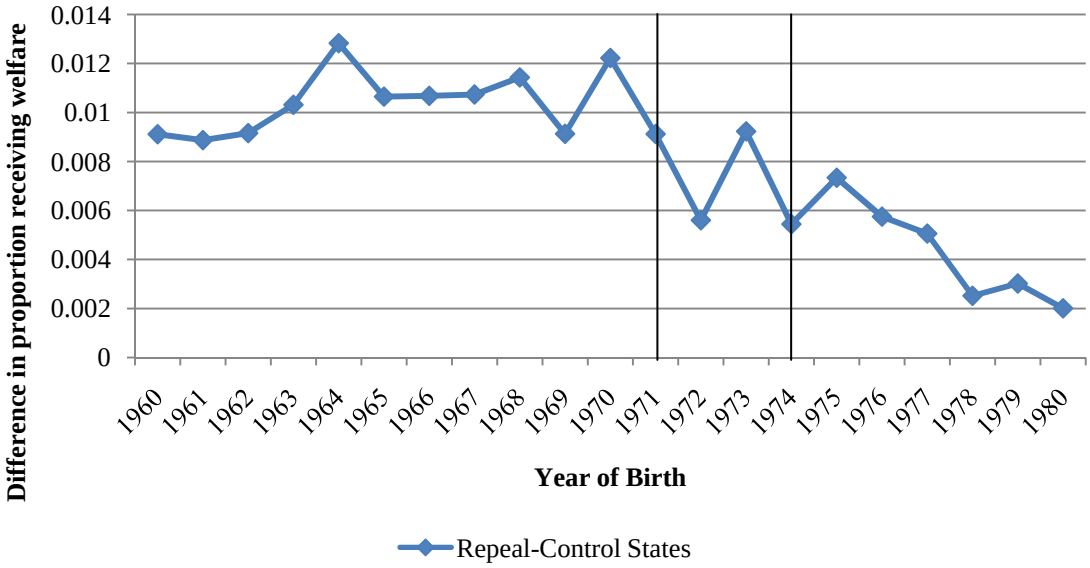


Table 2. Effect of early abortion legalization on educational outcomes for those born between 1960 and 1980, United States Census 2000

Variable	Educational Attainment (years)	At least high school degree	At least some college	At least college degree
Born 71-73	0.0096	0.0159	0.0555	0.856
(D7173)	[0.0008]	[0.0013]	[0.0034]	[0.058]
D7173*****repeal	0.0023	0.00143	0.0103	0.0232
	[0.0017]	[0.003]	[0.0075]	[0.13]
Born 74-75	0.0120	0.0251	0.0777	1.723
(D7475)	[0.0009]	[0.0016]	[0.0039]	[0.0670]
D7475*****repeal	0.0040	0.00451	0.0349	0.0113
	[0.0020]	[0.0036]	[0.0090]	[0.15]
Born 76-79	0.0056	0.0352	0.0925	1.548
(D7679)	[0.0008]	[0.0014]	[0.0037]	[0.0630]
D7679*****repeal	0.0045	0.00321	0.0356	0.0934
	[0.0018]	[0.0032]	[0.0082]	[0.14]
p-value for f-test of equality of coefficients on D7173*****repeal and D7475*****repeal	0.4381	0.4097	0.0098	0.9409
p-value for f-test of equality of coefficients on D7475*****repeal and D7679*****repeal	0.7852	0.7201	0.9405	0.6012
p-value for f-test of equality of coefficients on D7173*****repeal and D7679*****repeal	0.2735	0.6166	0.0052	0.6476
Number of Observations	1071	1071	1071	1065
R-squared	0.9709	0.9616	0.9612	0.9064

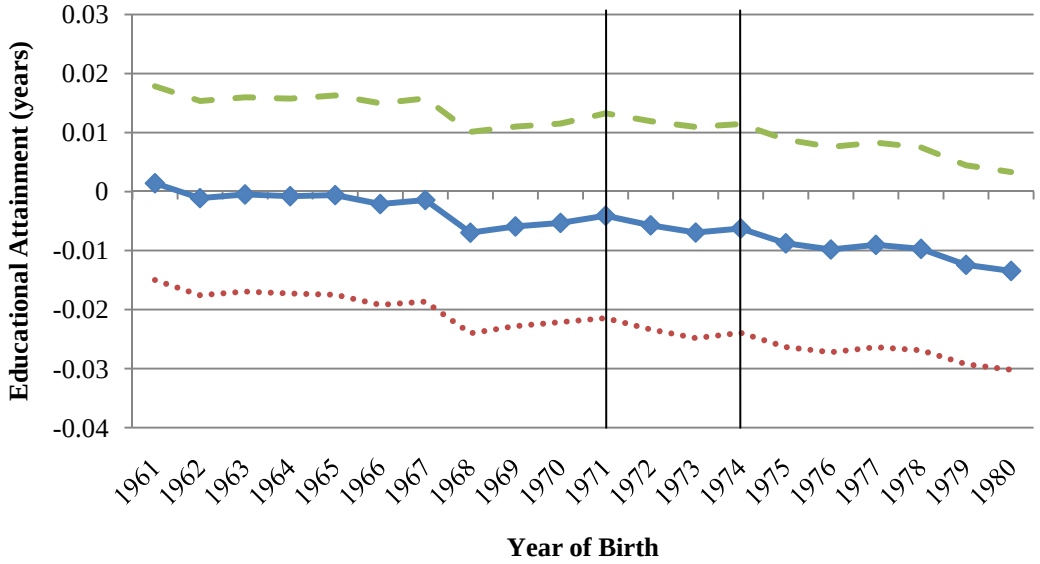
Note: Standard errors in brackets. High school graduation, some college, and college graduation are coded as whether an individual has achieved at least this level of education. Outcomes were aggregated on the state- and year-of-birth level and then measured as log(outcome).

Table 3. Effect of early abortion legalization on income, poverty status, and welfare status for those born between 1960 and 1980, United States Census 2000

Variable	Income	Poverty	Welfare
Born 71-73	-0.0331	-0.143	0.103
(D7173)	[0.0053]	[0.0084]	[0.0160]
D7173*****repeal	0.0126	0.0216	-0.031
	[0.01]	[0.0190]	[0.0350]
Born 74-75	-0.0564	-0.136	0.213
(D7475)	[0.0067]	[0.0098]	[0.0190]
D7475*****repeal	0.0283	0.036	-0.0551
	[0.012]	[0.022]	[0.043]
Born 76-79	-0.0338	0.0314	0.281
(D7679)	[0.0054]	[0.0091]	[0.0170]
D7679*****repeal	0.0351	0.00642	-0.0377
	[0.011]	[0.02]	[0.039]
p-value for f-test of equality of coefficients on D7173*****repeal and D7475*****repeal	0.2143	0.5416	0.5909
p-value for f-test of equality of coefficients on D7475*****repeal and D7679*****repeal	0.5823	0.1988	0.6901
p-value for f-test of equality of coefficients on D7173*****repeal and D7679*****repeal	0.0616	0.4999	0.8751
Number of Observations	1071	1071	1071
R-squared	0.9952	0.9699	0.8593

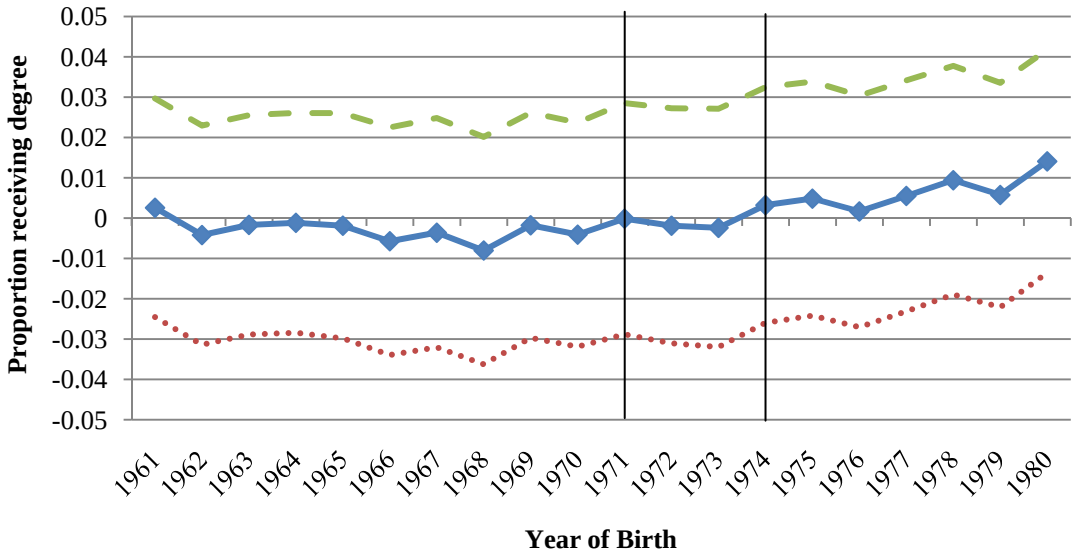
Note: Standard errors in brackets. Poverty status was determined by year 2000 data from the U.S. Census Bureau based on income and family size. Welfare status was determined by whether an individual had reported receiving any welfare income in the 2000 census. Outcomes were aggregated on the state- and year-of-birth level and then measured as log(outcome).

Figure 12. Differences-in-differences estimates of educational attainment in repeal states



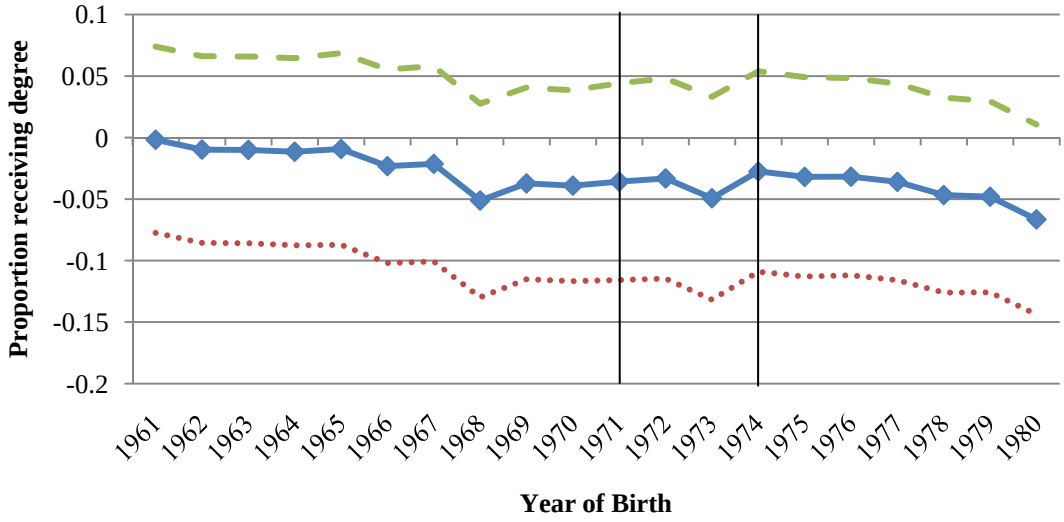
Note: The coefficients graphed are those on a year of birth dummy variable (for years of birth between 1961 and 1980) times a repeal dummy variable (e.g. yob61*repeal). Educational attainment was determined by the highest number of years of education attained as reported in the 2000 census. Outcomes were aggregated on the state- and year-of-birth level and then measured as log(outcome). Confidence interval plotted by dashed lines.

Figure 13. Differences-in-differences estimates of high school degree status in repeal states



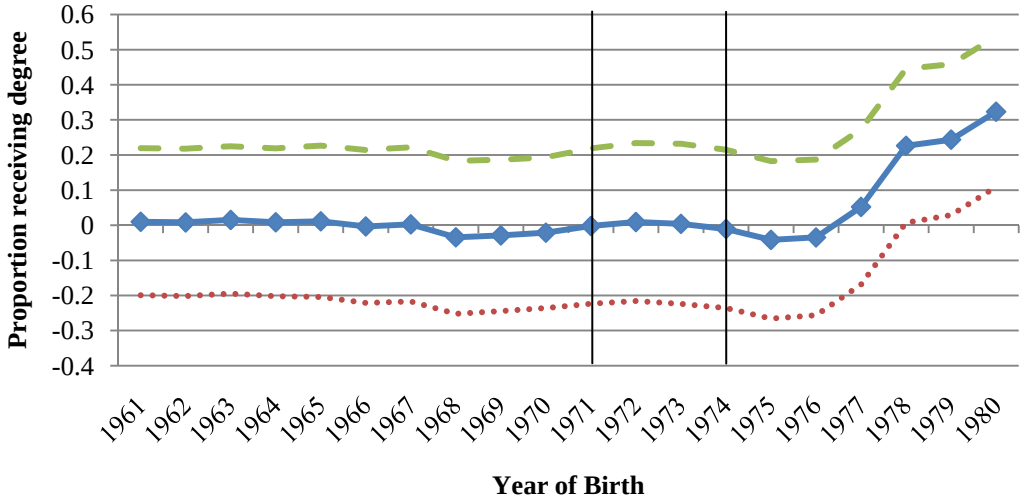
Note: The coefficients graphed are those on a year of birth dummy variable (for years of birth between 1961 and 1980) times a repeal dummy variable (e.g. yob61*repeal). Degree status was determined by whether an individual had reported receiving a high school degree or higher in the 2000 census. Outcomes were aggregated on the state- and year-of-birth level and then measured as log(outcome). Confidence interval plotted by dashed lines.

Figure 14. Differences-in-differences estimates of college education in repeal states



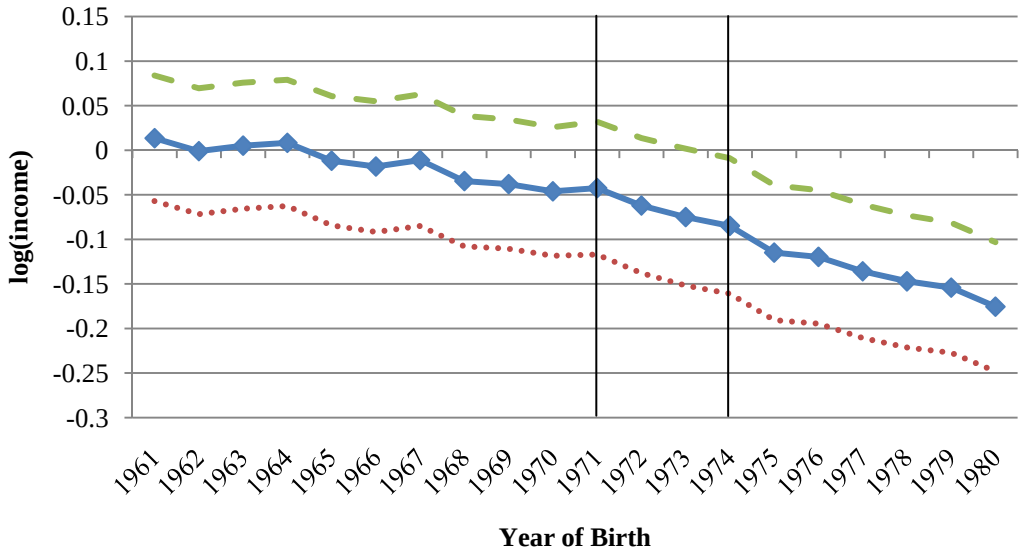
Note: The coefficients graphed are those on a year of birth dummy variable (for years of birth between 1961 and 1980) times a repeal dummy variable (e.g. yob61*repeal). Degree status was determined by whether an individual had reported any college education in the 2000 census. Outcomes were aggregated on the state- and year-of-birth level and then measured as log(outcome). Confidence interval plotted by dashed lines.

Figure 15. Differences-in-differences estimates of college degree status in repeal states



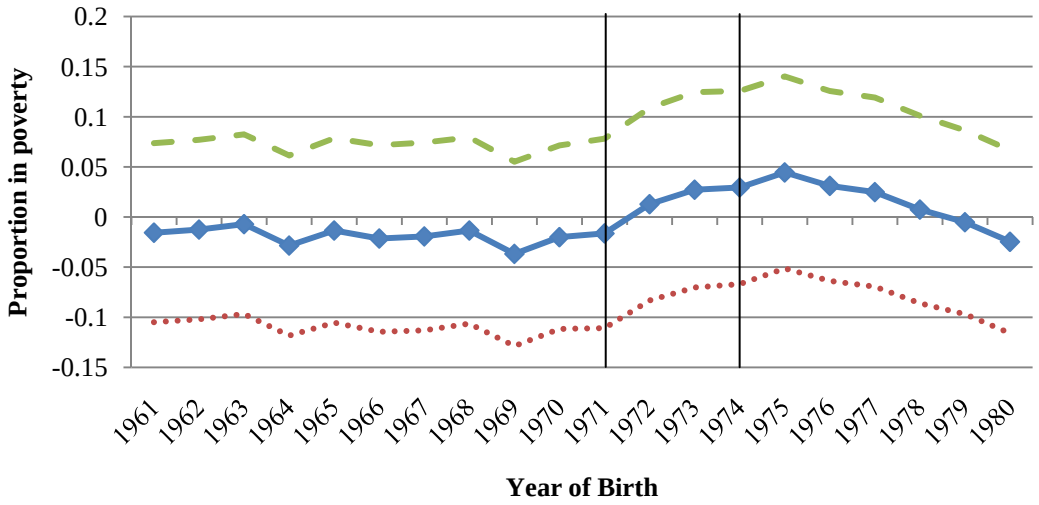
Note: The coefficients graphed are those on a year of birth dummy variable (for years of birth between 1961 and 1980) times a repeal dummy variable (e.g. yob61*repeal). Degree status was determined by whether an individual had reported receiving a bachelor's degree or higher in the 2000 census. Outcomes were aggregated on the state- and year-of-birth level and then measured as log(outcome). Confidence interval plotted by dashed lines.

Figure 16. Differences-in-differences estimates of earned income in repeal states



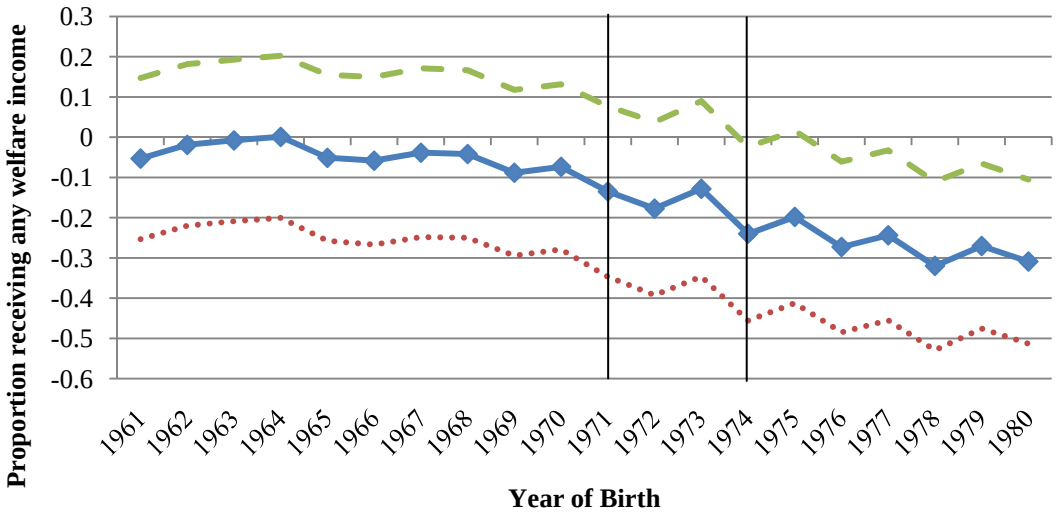
Note: The coefficients graphed are those on a year of birth dummy variable (for years of birth between 1961 and 1980) times a repeal dummy variable (e.g. yob61*repeal). Outcomes were aggregated on the state- and year-of-birth level and then measured as log(outcome). Confidence interval plotted by dashed lines.

Figure 17. Differences-in-differences estimates of poverty status in repeal states



Note: The coefficients graphed are those on a year of birth dummy variable (for years of birth between 1961 and 1980) times a repeal dummy variable (e.g. yob61*repeal). Poverty status was determined by year 2000 data from the U.S. Census Bureau based on income and family size. Outcomes were aggregated on the state- and year-of-birth level and then measured as log(outcome). Confidence interval plotted by dashed lines.

Figure 18. Differences-in-differences estimates of welfare status in repeal states



Note: The coefficients graphed are those on a year of birth dummy variable (for years of birth between 1961 and 1980) times a repeal dummy variable (e.g. yob61*repeal). Welfare status was determined by whether an individual had reported receiving any welfare income in the 2000 census. Outcomes were aggregated on the state- and year-of-birth level and then measured as log(outcome). Confidence interval plotted by dashed lines.