NBER WORKING PAPER SERIES

ABORTION LEGALIZATION AND LIFECYCLE FERTILITY

Elizabeth Oltmans Ananat
Jonathan Gruber
Phillip B. Levine

Working Paper 10705
http://www.nber.org/papers/w10705

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2004

We would like to thank Melissa Kearney and Doug Staiger for helpful conversations and Jean Roth for her assistance acquiring the Vital Statistics data. This research was supported by the National Institute of Child Health and Human Development, grant # RO1 HDO42819. The views expressed herein are those of the author(s) and not necessarily those of the National Bureau of Economic Research.

Monetary Policy and Asset Prices: A Look Back at Past U.S. Stock Market Booms
Elizabeth Oltmans Ananat, Jonathan Gruber, and Phillip B. Levine
NBER Working Paper No. 10705
August 2004
JEL No. I1

ABSTRACT

Previous research has convincingly shown that abortion legalization in the early 1970s led to a significant drop in fertility at that time. But this decline may have either represented a delay in births from a point where they were "unintended" to a point where they were "intended," or they may have represented a permanent reduction in fertility. We combine data from the 1970 U.S. Census and microdata from 1968 to 1999 Vital Statistics records to calculate lifetime fertility of women in the 1930s through 1960s birth cohorts. We examine whether those women who were born in early legalizing states and who passed through the early 1970s in their peak childbearing years had differential lifetime fertility patterns compared to women born in other states and in different birth cohorts. We consider the impact of abortion legalization on both the number of children ever born as well as the distribution of number of children ever born. Our results indicate that much of the reduction in fertility at the time abortion was legalized was permanent in that women did not have more subsequent births as a result. We also find that this result is largely attributable to an increase in the number of women who remained childless throughout their fertile years.

Elizabeth Oltmans Ananat
Department of Economics
MIT
Cambridge, MA 02139
eoltmans@mit.edu

Jonathan Gruber
Department of Economics
MIT
Cambridge, MA 02139
and NBER
gruberj@mit.edu

Phillip B. Levine
Department of Economics
Wellesley College
Wellesley, MA 02481
and NBER
plevine@wellesley.edu

I. INTRODUCTION

The ability to control one's fertility through legal abortion is one of the most profound changes in family planning of the last fifty years. The significant change in the cost of abortion brought about by its legalization enabled women to abort pregnancies that would otherwise have resulted in an unwanted birth. Past research has shown that women responded very strongly to this incentive when abortion was legalized in the early 1970s, reducing their fertility by at least 4 percent (cf. Levine, et al., 1999).

The immediate drop in the birth rate in response to legal abortion does not, however, necessarily indicate that women have fewer children over the rest of their childbearing years. Many of the women who chose to abort rather than have an unwanted birth still had a number of fertile years remaining. If the births that did not occur were "replaced" by additional births subsequently, then women would not experience a net reduction in lifetime fertility. Therefore, the short-run impacts on birth rates estimated in earlier work may not represent long-run reductions in birth rates and completed fertility. For example, the ability to better control one's fertility may have simply led women to delay births to a more preferred later date. In this case, abortion legalization would have had no long run affect on completed fertility. Yet, to our knowledge, no evidence exists regarding the impact of abortion legalization on women's fertility patterns over the remainder of their childbearing years.[1]

Understanding the effect of abortion availability on fertility patterns is important for three reasons. First, it allows for a richer model of the fertility impacts of public policy. If abortion

[1] Perhaps the most closely related study is the one by Angrist and Evans (1999), who examine the influence of reduced teen fertility attributable to abortion legalization on educational outcomes and employment status of women. It is similar to our analysis in that it extends the immediate fertility reduction brought about by legal abortion to women's subsequent outcomes. On the other hand, its focus on education and employment differs from our analysis of fertility patterns of the rest of the women's childbearing years.

availability had only a short-run timing affect on fertility, then it has a very different long-run implication for population dynamics than if there was a permanent effect on the population. There is some literature, reviewed subsequently, on the general sources of variation in birth rates over time in the United States, but none of this work has carefully analyzed the direct impact of fertility control itself in this process. Our work fills this gap by providing clear evidence on the impact of fertility control, through abortion legalization, on both completed fertility and birth timing.

Second, understanding the extent to which legal abortion generates a permanent reduction in women's fertility, compared to a change in the timing of births, is crucial for interpreting the recent literature on abortion access and child outcomes. For instance, in response to the legalization of abortion Gruber, et al. (1999) find improvements in child well-being and Donahue and Levitt (2001) report a reduction in crime when those children mature. One explanation for these findings is that those children who were born are differentially selected among those who could have been born if abortion had not been legal. But if births are just timed differently, then any selection effect would strongly depend upon whether childrens' outcomes are determined by the fixed characteristics of mothers or the age at which those mothers give birth. If it is the mother's fixed characteristics that matter, then the effects reported in Gruber, et al. (1999) and Donahue and Levitt (2001) should diminish over time since the "marginal" children will eventually be born. If it is the age of the mother that matters, then even the delay will lead to lasting improvements in child well-being.

Finally, the observed changes in childbearing in response to abortion legalization may have important implications for women's outcomes as well. A woman's fertility patterns over her childbearing years could easily alter other outcomes she is likely to experience in her life.

For instance, a woman who accidentally becomes pregnant at, say, age 20 may choose to drop out of college if she was unable to abort the pregnancy. If that same birth could have occurred five years later, she may have graduated, but she may have restricted her labor market activity subsequently. If that birth never occurred, she may have also participated in the labor market more fully over the remainder of her worklife.

The purpose of this paper is to estimate the impact of abortion legalization on lifecycle fertility, beginning from the point at which abortion was legalized through the remainder of women's childbearing years. We combine data from the 1970 U.S. Census and microdata from 1968 to 1999 Vital Statistics records to calculate lifetime fertility of women in cohorts born in the 1930s through 1960s. We examine whether those women who were born in early legalizing states and who passed through the early 1970s in their peak abortion and childbearing years had differential lifetime fertility patterns compared to women born in other states and in different birth cohorts. We consider the impact of abortion legalization on both the number of children ever born as well as the distribution of number of children ever born.

We find that much of the reduction in fertility at the time abortion was legalized was permanent; women did not have many more subsequent births as a result. We also find that this result is attributable to an increase in the number of women who remained childless throughout their fertile years.

II. BACKGROUND AND LITERATURE REVIEW

The process by which abortion was legalized in the United States in the early 1970s is an integral component of the identification strategy we employ. A detailed description of the events leading up to the legalization of abortion in the United States is provided in Garrow (1994).

Briefly, prior to the late 1960s, abortion was illegal in every state in America except when necessary to preserve a pregnant woman's life. Between 1967 and 1973, a number of states implemented modest reforms making it legal for some women to obtain abortions under very special circumstances, such as rape, incest or a serious threat to the health of the mother. Abortion became widely available, however, in five states in 1970. In four of these states (New York, Washington, Alaska, and Hawaii), there was a repeal of anti-abortion laws. In the fifth, California, there was a "de facto" legalization, since in late 1969 the California State Supreme Court ruled that the pre-1967 law outlawing abortion was unconstitutional. Following the 1973 Supreme Court decision in Roe vs. Wade, abortion became legal in all states.

These events contributed to a dramatic increase in the frequency with which women chose to end a pregnancy through abortion. Although it is difficult to determine the number of abortions performed prior to legalization, the trend in its immediate aftermath was dramatic. The abortion rate almost doubled in the years following Roe v. Wade. This heightened prevalence of abortion came at the same time as an ongoing steep reduction in fertility rates. Because births had been falling precipitously even before the introduction of legalized abortion, it is not clear to what extent the introduction of legalized abortion contributed to the decline.

To distinguish between these ongoing trends and the causal impact of changes in abortion law, Levine, et al. (1999), used the natural experiment provided by the staggered introduction of legalized abortion across states. Since the methods they used are comparable to those that we will introduce subsequently, we review them in some detail here. The legislative history enabled them to categorize states by abortion legality in different years to jointly exploit two legislative changes. They first compared changes in fertility rates in the "early legalizing" (or "repeal") states from before 1970 to after 1970, relative to other ("non-repeal") states where abortion was still illegal. Then they reversed the treatment in 1973, comparing births in those states where the Roe decision legalized abortion to the repeal states where it had already been legal.

In particular, Levine, et al. estimated regression models of the form:

$$\ln(\text{birth rate})_{st} = \beta_1 * REPEAL_s * D7173 + \beta_2 * REPEAL_s * D7475 \qquad (1)$$

$$+ \beta_3 * REPEAL_s * D7680 + \beta_4 \delta_s + \beta_5 \tau_t + \beta_6 \delta_s * TREND + \beta_8 X_{st} + \varepsilon_{st}$$

where: the birth rate represents births per 1,000 women in the defined age group in state s and time t; $REPEAL_s$ is a repeal state indicator; D7173, D7475, and D7680 are dummies for the eras 1971-1973, 1974-1975, and 1976-1980, respectively; δ_s is a set of state dummies; τ_t is a set of year dummies; TREND is a linear time trend; and X_{st} are state-specific time-varying control variables. This fixed effects specification provides generic controls for the multitude of otherwise unobservable differences that exist across states or take place over time. Within this regression framework, the impact of abortion legalization in the early repeal states is captured by the coefficient β_1. Since the dependent variable is the log of the birth rate, this coefficient is interpreted as the percentage change in the birth rate brought about by legalization. The impact of abortion in the other states, where abortion was legalized by Roe v. Wade, is indicated by $\beta_1 - \beta_3$; if there was a full "rebound" in those states when abortion was legalized, then β_3 should equal zero. The middle coefficient, β_2, tests whether the rebound in the differential was instantaneous or whether it occurred with a lag.

As an extension to this model, Levine, et al. provide estimates separately comparing repeal states to non-repeal states that vary by their distance to repeal states, using the distance categories of within 250 miles (from closest population centroids), 250 to 750 miles, and greater than 750 miles. If we assume that women in closer non-repeal states were more likely to cross state lines to obtain an abortion (e.g. those in New Jersey crossing into New York), then the effects of abortion should be strongest for those living farthest from the early repeal states.

The results obtained from their analysis indicate that the legalization of abortion in the United States in the early 1970s reduced the fertility rate by over 4 percentage points. Teens and women age 35 and over experienced much larger reductions in fertility, as did nonwhite and unmarried women. Levine, et al. also found that this effect was much larger when early repeal states were compared to more distance comparison states, suggesting that travel between states dampened the estimated impact.

Although no work of which we are aware has explored the relationship between abortion legalization and lifecycle fertility, others have explored the determinants of lifecycle fertility more generally. Analyses of the distinction between fertility timing and completed fertility have been conducted in the past. For example, much of the variation in birth rates over time in the United States, and particularly the baby boom and subsequent bust, is the result of changes in timing of births rather than changes in completed fertility (c.f. Hotz, et al. 1997). Differences in the timing of fertility across demographic groups over time can account for differences in their fertility patterns (c.f. Chen and Morgan, 1991; Rindfuss, et al. 1996; Morgan, et al. 1999; and Martin, 2000). In addition, a number of recent micro-econometric studies of the fertility process have found a key role of economic factors, and especially a woman's wage, in determining the timing of first birth (Heckman and Walker, 1990; and Hotz and Miller, 1988). Heckman and Walker (1990), in particular, found that the strongest effects of economic variables were through the timing of first birth. But none of this work has carefully analyzed the direct impact of fertility control itself in this process. Our work fills this gap by providing clear evidence on the impact of fertility control, through abortion legalization, on both completed fertility and birth timing.

Finally, as noted in the introduction, our work is indirectly related to the large recent literature on abortion availability and cohort outcomes. This literature started with Gruber et al. (1999), who used the identification strategy of Levine et al. to examine the characteristics of cohorts born before and after abortion became available. They find that outcomes such as living with a single parent, living in poverty, or living beyond one's first birthday all improved significantly once abortion was available, implying that the "marginal child" not born because of abortion would have lived in poor circumstances.

Subsequent work has focused more directly on the implications of abortion for these cohorts as they age. Most notable is Donahue and Levitt (2001), who showed that abortion availability was associated with lower crime rates when these cohorts became teenagers. These authors use a somewhat different identification strategy than that pursued in Levine et al., employing all the variation in abortion rates that took place over this period rather than just indicators of the legal status of abortion. This approach is criticized in Joyce (2004), and defended in Donahue and Levitt (2004). Charles and Stephens (2002) provide an additional example of research in this area, investigating the relationship between abortion legalization and substance abuse as teens. In this body of literature the long-term implications for children's outcomes depend critically on whether the abortion legalization permanently reduced a woman's fertility or just altered the timing of when she gave birth.

III. METHODOLOGY

To examine the impact of abortion legalization on life-cycle fertility, we will again exploit the quasi-experiment provided by the variation across states in the exact timing of abortion legalization in the early 1970s. The main methodological extension in the present analysis is to convert the focus on period effects that Levine, et al. estimated to a focus on cohort effects. The period effect analysis operated by comparing women at different ages at the time of the law changes (either early repeal or Roe vs. Wade), and measuring their current fertility rates. The cohort analysis instead compares women who passed through these different legal states at the time when they were most likely to be affected by abortion availability, and examines measures of completed fertility.

We focus on those cohorts who were most likely to be affected by changes in abortion law in the early 1970s based on their age at that time. Women in these cohorts from repeal states experienced greater exposure to legal abortion and may have had different fertility patterns over the remainder of their lives as a result. Once these "treatment cohorts" are defined, the methodological approach is analogous to that based on period effects. Women in the different sets of states in cohorts before the treatment cohorts and after the treatment cohorts should have been equally affected by changes in abortion policy; abortion would have either been uniformly illegal (for the earlier cohorts) or uniformly legal (for the later cohorts) during the years in which they were most at-risk of unwanted pregnancies.

Our approach for defining the "treatment cohorts" is illustrated in Figures 1 and 2, which apply information on the impact of abortion legalization on births in the 1970s by exact age to determine more precisely which birth cohorts should have been differentially affected. One of the lines in Figure 1 shows the relative impact of abortion legalization on the birth rate by exact age. The points on this line represent the results of regression models analogous to those reported in equation (1) and estimated in Levine, et al. by the exact age of birth using Vital Statistics microdata between 1968 and 1980 (described in the next section). The points plotted represent the percentage reduction in births brought about by abortion legalization at each age.[2] As reported in Levine, et al. (albeit with less age-specificity), legalized abortion had the biggest relative impact on births to teens and to older women. The impact is largest at age 15, declines to zero by age 28, and then rises again.

Figure 1 also displays the level of the birth rate in 1970 by specific age. Birth rates peak at age 23. They are highest between the late teens and late 20s and are low in the early teens and into the 30s and 40s. Therefore, the large relative impact of abortion legalization on births at older ages needs to be tempered by the fact that the base is relatively low. The absolute impact on the number of births is not that large at older ages.

This point is made more clearly in Figure 2. The points plotted in this figure are estimated from the same model as we used to obtain the points in Figure 1, except the dependent variable is the level of the birth rate rather than the natural logarithm. As such, parameter estimates represent the absolute effect on the birth rate rather than the relative effect based on the percentage change. The solid line in the figure represents the average reduction in the birth rate across all ages. In Figure 2 we see that ages 16 through 26 represent the peak ages at which the number of births was affected. At these ages, births fell by roughly 6 per 1,000 women in response to abortion legalization. With the exception of a peculiar spike at exact age 31, the absolute impact was considerably smaller at all other ages. Therefore, we would expect the

[2] More specifically, they represent the coefficient on the interaction between a repeal state indicator and an indicator for births in the 1971-1973 period in equation 1.

largest impact on lifecycle fertility to hit those cohorts who passed through the early 1970s between the ages of 16 and 26.

Table 1 helps facilitate the transition from these age-specific period effects to the definition of what we define to be the treatment cohorts. This table simply translates age at childbearing in specific years into the relevant birth cohorts of women. We know that abortion laws differed between repeal and other states over the 1970 to 1972 period, which, given the nine month delay of pregnancy, largely applies to births in 1971 to 1973. We can combine this information with the fact that the peak ages of abortion legalization's impact was 16 to 26. As Table 1 helps make clear, this means that the birth cohorts of the later 1940s and earlier 1950s whose fertility were the most directly affected by abortion legalization. As an approximation, in our analysis we define the 1946 to 1955 birth cohorts to represent the treatment cohorts.[3] If abortion legalization in the early 1970s reduced fertility at those ages for these women, any permanent impact on lifetime fertility would show up as a gap in children ever born between those who lived in early repeal states and others. Women born after 1955 would have entered childbearing ages following the Roe decision, so they would have faced a lifetime of exposure to legal abortion regardless of their state of residence. Similarly, women born in the 1930s and earlier 1940s were in their later stages of childbearing by the early 1970s. These women passed through their peak childbearing years with no legal abortion access, regardless of where they lived.

These results bring us to an empirical specification of our model of completed fertility. We estimate models of the form:

[3] Our results are not sensitive to small changes in the start and end date of the treatment cohort. We also experimented with alternative specifications within the 1946-1955 cohort window (like breaking it up into two windows – one from 1946-1950 and one from 1951-1955), but found no pattern strong enough to statistically distinguish.

$$CF_{cs} = \beta_1 * REPEAL_s * D4655 + \beta_2 * REPEAL_s * D(post55) \qquad (2)$$

$$+ \beta_3 \delta_s + \beta_4 \tau_c + \beta_5 \delta_s * TREND + \varepsilon_{cs}$$

where CF_{cs} represents alternative measures of completed fertility in cohort c in state s, D4655 is an indicator of the 1946-1955 birth cohorts and D(post55) is an indicator of post-1955 birth cohorts. The omitted term here is the difference before 1946 between repeal and non-repeal states, which is captured by the set of state fixed effects. The coefficient β_1 measures the difference in completed fertility between repeal and non-repeal states for those women born in the 1946-1955 period (the treatment cohorts), relative to those born before 1946 (who were beyond prime childbearing age when abortion was legalized). The coefficient β_2 measures the difference in completed fertility between repeal and non-repeal states for the set of women born after 1955 (for whom there should be no effect since they were all subject to legal abortion throughout their childbearing years), relative to those born before 1946. As with the period tests of Levine et al., this framework provides two tests for the effect of abortion legalization: that β_1 is negative, and that β_2 is zero.

It is important to recognize the additional power that we obtain by including the interaction for the post-1955 cohort. Absent this term, we would be unable to differentiate an early repeal effect from a differential trend by repeal vs. non-repeal states. That is, the early repeal of abortion, followed by Roe v. Wade, allows researchers to go beyond the typical "difference-in-difference" setup, in which it is difficult to distinguish policy-induced changes from underlying trends. By finding *both* that there is an effect on fertility for the treatment cohort, and that there is no effect for later cohorts, we are able to confirm that there is a causal effect of abortion availability and not an underlying trend that differs between these sets of states.

There are a couple of important modifications in making the translation from the period effect model in equation 1 to the cohort effect model in equation 2. First, states no longer represent the state that the birth occurred since states are designed to identify the mother's state of residence in the early 1970s. As described below, however, it is impossible to determine states of residence in the early 1970s from the available data, so we use mothers' states of birth instead. Using state of birth has two advantages for our purposes: it is exogenous to any mobility decisions related to abortion availability; and it is a fixed characteristic that allows us to merge the two data sources we use below. Second, we no longer control for other time varying, state-specific covariates. In the cohort context, individuals are potentially influenced by some complex function of social conditions since they were born and it is unclear how to specifically control for them in a regression framework. Instead, we rely on state of birth fixed effects along with linear trends within states of birth to control for these other factors.[4]

In addition to estimating models of this form, we also follow Levine, et al. and estimate models comparing completed fertility to women in repeal states to women in non-repeal states who differ in their distance to repeal states. If travel between states occurred in response to the early legalization of abortion in repeal states, then the estimated impacts should be greater when comparing repeal states to more distant non-repeal states.

IV. DESCRIPTION OF THE DATA

Based on this discussion, we need data providing measures of women's completed fertility for different birth cohorts of women along with geographical identifiers that can isolate those women exposed to the early legalization of abortion in repeal states. No single dataset

[4] The findings reported later in the paper are similar when we control for state-specific quadratic trends.

provides all of this information.[5] We rely on the Public Use Micro Sample from the 1970 Census and data from the Vital Statistics Natality Detail Files between 1968 and 1999 to construct measures of completed fertility for birth cohorts beginning in the 1930s and going through the 1960s. This combination provides a unique opportunity to examine the life-cycle fertility of the relevant treatment and control cohorts.

The Vital Statistics data represent individual records on every birth that took place in the United States between 1968 and 1999. Microdata prior to 1968 are not publicly available. These data are invaluable because they specify the parity of the birth, the mother's age (from which we can estimate the year she was born), and the mother's state of birth.[6] With these data, we can attribute every birth that takes place in the United States to the mothers' state/year of birth cohort.[7] Moreover, we can use the information available on the parity of each birth to help formulate the distribution of the number of births to each woman over her childbearing years.[8]

For those women born in the mid 1950s and later, these Vital Statistics microdata contain a record of every birth they have had. Since fertility trails off considerably after age 35 and is

[5] Data from the 1970, 1980, and 1990 Censuses provide information on the number of children ever born and each individual's year and state of birth, but there are two drawbacks to using this as the basis of our analysis. The first is that younger women, for whom we expect to see the strongest effects on fertility, have not completed their childbearing by 1990, and the 2000 Census does not include information on children ever born. This means that we can only estimate the effect on childbearing by certain ages (in particular, age 35) rather than lifetime fertility. The second is that the Census provides only a 5 percent (or, in 1970, 4 percent) sample rather than complete count data. We did analyze the 1970-1990 Census data, creating a sample composed of women who were born in the 50 states and the District of Columbia between 1935 and 1955 and using a framework similar to that reported in the text for the Vital Statistics analysis. Our results were consistent with the results we present here, but lacked the precision of the results provided by analysis of the complete-count, 1968-1999 Vital Statistics data.

[6] For a very small number of births, this information is missing. These births are not included in the analysis.

[7] Another minor limitation of these data is that births to women who were born in the United States but gave birth in another country would not be captured in these data. It is our impression that this is a very infrequent event and we ignore it here.

[8] Each birth at a given level of parity contributes to the counts in a cumulative birth distribution, which can then be converted to a probability distribution.

very low after age 40, birth records for the 1968 through 1999 period will capture most, if not all all births to women born as recently as the mid 1960s. Therefore, these data on their own are sufficient to measure completed (or almost completed) fertility for roughly a decade's worth of birth cohorts. But these data are inadequate for capturing completed fertility for women born before the mid 1950s since they will already have given birth to at least some of their children prior to 1968. If they give birth to additional children after 1968, we will see them in the Vital Statistics data from later years, but we would miss any women whose fertility is completed before 1968.

To count births prior to 1968 we use data from the 1970 Census, which provides information on both women's state of birth and their children ever born.[9] Based on these data, we can count children ever born who are more than two years old, and thus were born before 1968, by mothers' birth cohort and state. This becomes a baseline to which we can add information on later births from the Vital Statistics data. For example, consider the cohort of women born in 1940. We use the 1970 census data to measure how many children those women had through 1967. We then use the Vital Statistics data to track the additional births to this cohort that occurred in 1968 or later. Since both sources of data are sorted by state of birth, we can be sure that we are tracking the same women.

We use these combined Census and Vital Statistics microdata to create measures of lifetime fertility for women born in the 1930s through the 1960s. For the earliest of these cohorts, the Census data captures virtually their entire fertile period. For those born after the mid 1950s their fertility history is virtually entirely captured by the Vital Statistics microdata. For the intervening cohorts, these two data sources are combined to create complete fertility histories.

[9] The Census data we use comes from the standardized Minnesota Integrated Public Use Microdata Series (Ruggles and Sobek 2003).

Using the available data, we are able to construct measures of the number of children ever born by age 25, by age 30, by age 35, and then total completed fertility.[10] We convert counts of births by these ages to measures of children ever born per woman by incorporating estimates from the 1970 Census on the number of women in each state/year of birth cohort. In addition, we have constructed distributions of the number of children born to each woman in each state/year of birth cohort and focus, in particular, on the percentage of women who never have children.

While this is, we believe, the best empirical strategy to study the fertility effects of abortion legalization, there are some limitations. First, the Vital Statistics data do not contain information on mother's state of birth in 1968, 1969 and 1972; the only geographic information is the state in which the birth occurred. Assigning births to mother's state of birth based on the state in which the birth occurred introduces two sources of bias: some women move between the time they are born and give birth and some women giving birth in the United States were not born here. To circumvent these problems, we employed data from 1970, 1971, and 1973 to estimate the probabilities of moving between a particular birth state and a state in which a birth occurs and the probability that births in a given state are from a foreign-born mother. We assign those probabilities from the 1970 data to the births in 1968 and 1969 and the average of the 1971 and 1973 probabilities to the births in 1972 to provide estimates of the expected births to women in each state/year of birth cohort.[11]

A second limitation of the Census data is that it pertains to the date the Census was completed by each individual and does not specifically identify births taking place prior to 1968.

[10] Where sufficient data is available, we define total completed fertility to include births up to age 44. In some instances, however, we use births up to age 39 since so little fertility takes place after that.

[11] These probabilities are calculated and assigned separately for each state/year of birth cohort as well as live birth order.

For children still living with their mothers (which would account for the vast majority of those under age 2), we can use data on the ages of the children in the household to remove from our count those children we estimate as being born in 1968 or later.[12] This approach creates an additional problem, however, since the only identifying information is the age of the child and his/her quarter of birth. Without knowing the exact date the Census was completed, it is impossible to know exactly when the child was born. We employ an algorithm that assumes all responses were filed on April 1st to determine the number of children born since the beginning of 1968 and then take the difference between this and children ever born to determine the number born prior to 1968.[13]

Figure 3 provides an indication of the accuracy of these constructed data. It compares aggregated national data on average children ever born for each birth cohort between 1930 and 1960 from the Vital Statistics system (U.S. Centers for Disease Control, National Center for Health Statistics, 2004), from the June Current Population Survey (CPS) Fertility Supplement (Brown, 2002), and from the data we have constructed from the 1970 Census and 1968-1999 Vital Statistics microdata.[14] This comparison indicates that our constructed data come close to those reported from other sources. For the earlier and later birth cohorts, the estimates are extremely close. This may be consistent with the fact that all births from each of these groups of

[12] We use the Minnesota Integrated Public Use Microdata Series identification of mother-child relationships (Ruggles and Sobek 2003).

[13] In the 1970 Census, 78 percent of households filed their forms by April (they are mailed out in March). The remainder filed them sometime subsequent to that after Census enumerators were able to track them down.

[14] For these other sources, we assign the reported statistics on children ever born by age 40 to 44 to the center of the five year birth intervals that the data represent. In our constructed data, we report the average number of children born by age 44 for women born before 1955 and births by age 40 for the remainder. If the Vital Statistics data were reported by the mother's state of birth, we could use those data directly in our analysis. Unfortunately, no such detail is available either in printed reports or through tabulations of publicly available microdata.

birth cohorts comes from a single source, the Census for the earlier cohorts and Vital Statistics for the later cohorts.

But small differences do begin to appear in the middle birth cohorts, in which estimates are spliced together from the two data sources.[15] This analysis suggests that combining these data sources does introduce a small overcount in the number of births in these years in our data. Some double counting of births may be occurring due to any of the problems described earlier in these data sources (and particularly the inability to perfectly define when births occur in the 1970 Census) or from other forms of misreporting. Yet it is important to recognize that the discrepancy is not large and does not appear to perfectly coincide with the 1946 to 1955 birth cohorts that we concentrate on. To introduce bias into our analysis, it would also have to be correlated with the repeal status of women's birth states and, in some specifications, with the distance from a non-repeal state to a repeal state. Moreover, in some of our analyses reported subsequently, we only use births that take place in 1968 or later, which only rely on the Vital Statistics microdata and should not be affected by this problem. Based on this assessment, we believe the data we have constructed are more than sufficient to undertake our intended exercise.

V. RESULTS

Our empirical strategy is based upon converting methods based on period fertility to methods based on cohort fertility, as described earlier. Since previous research has explored the impact of abortion legalization on period fertility, we start from the results of those analyses and

[15] We have also explored an alternative approach, using the Vital Statistics data only from 1970 onwards, allowing us to avoid the problem of assigning birthdates to those under age 2 in the 1970 Census and the problem of missing state of birth identifiers in the 1968 and 1969 Vital Statistics data. This alternative yielded a nearly identical pattern in Figure 3 and similar regression results to those reported below. Since we prefer to use as much of the superior Vital Statistics data as possible, we continue to use those data for births beginning in 1968.

then move toward reporting the results that are based exclusively on the completed fertility of different birth cohorts.

Results for Period Fertility

Table 2 focuses exclusively on period fertility and is intended to show the impact of changes in data and model specification that are required for our cohort analysis. In Column 1, we replicate the results presented in Levine, et al., which shows that births to women in repeal states fell by just over 4 percent during the 1971 to 1973 period relative to births to women in states with no change in their abortion laws. After abortion was legalized nationwide by the Roe decision, the gap in birth rates between states was eliminated (in a statistical sense). These results formed the basis for their conclusion that abortion legalization had a causal impact on births. In Column 2 of this table, the sample period used in the analysis is updated to start in 1968, which is when the microdata available from Vital Statistics for our exercise begin. This change in the sample had very little impact on the results.

The third column of Table 3 examines the impact of dropping all covariates from the regression model other than the repeal/year interactions and the state and year fixed effects along with state-specific trends. The other covariates in the preceding models include controls for states that instituted modest abortion reforms in the late 1960s and early 1970s along with a handful of variables that vary over time within states: the age composition of women of childbearing age, state per capita income, the insured unemployment rate, the percentage of a state's residents that are nonwhite, and the crime rate. We drop reform measures since they have no significant impact and the variables that change over time within states because it is not clear exactly how to incorporate these period-specific control variables into a cohort-specific analysis.

The results in Column 3 indicate that the pattern of results is not influenced by dropping these control variables, although the magnitude of the estimated impact of abortion legalization (i.e. the repeal*1971-1973 coefficient) rises somewhat.[16] The final column of this table replicates this analysis, but categorizes women into repeal states or otherwise based on the mother's state of birth, which we will use in our cohort-based analysis, not where the birth occurred. The results reported here suggest that this change does not alter the results that much.

Cohort-Based Approach – Initial Evidence

Figure 4 continues the transition towards a cohort-based approach. It relies exclusively on age-specific birth rates beginning in 1968 from the Vital Statistics microdata, attributing the births at each age to the relevant birth cohort of the mother (defined as the year of birth minus mother's age). We then plot the percentage difference in age-specific birth rates between women in repeal states and other states for three selected cohorts. For the 1959 birth cohort, the differential timing across states in abortion legalization would have had no impact on the difference in birth rates since these women would not have reached their childbearing years until after Roe v. Wade. Therefore, this cohort can serve as a control group. In this cohort, women in repeal states are more likely to have their births at later ages, as indicated by the general upward slope in this difference.

The treated cohorts are those born in 1954 and 1949, who would have been 17 and 22 years old in 1971, respectively, when abortion was legalized in the early repeal states. The trends in age-specific fertility for these cohorts clearly show the period effects that past research

[16] Among these variables, those that vary with states and years are the main contributors to the change in this magnitude. None of these can be identified as the main culprit. The impact of comparing repeal states to all other states rather than just those with no law change is inconsequential.

has found. Age-specific birth rates for women in repeal states in the 1954 birth cohort show a clear drop exactly at age 17 relative to women in non-repeal states and to other birth cohorts. This drop lasts for the few years in which abortion laws differed across states and then reverts back to the pattern for the control cohorts. Similarly, births to women in repeal states in the 1949 birth cohort fall exactly at age 22, which corresponds to their births in 1971, and remain lower just for the three years in which abortion policies differed across states.[17]

The advantage of this figure is that it enables us to compare births at later ages for these cohorts that were clearly contemporaneously affected by differential abortion access. The impression one would take away from this figure is that the contemporaneous reduction in births did not alter childbearing patterns at later ages. The age-profile in the difference in births for the 1954 cohort is no different after the early 20s from that of the control cohort. If anything, women in the 1949 birth cohorts from repeal states had fewer births over the remainder of main childbearing years compared to women in other states and the control cohort. These results provide the first evidence that the reduction in period fertility attributable to abortion legalization may have been permanent.

Another advantage of the approach taken here is that it only uses micro Vital Statistics data and does not integrate that with data from the 1970 Census. This provides evidence that the small discrepancy in reports of children ever born between other sources and the constructed data we will employ in the remainder of this analysis are not driving the results.

Cohort-Based Approach – Main Results

[17] Note that births for the 1949 cohort are only reported beginning at age 19, which corresponds to their fertility in 1968, the first year in which Vital Statistics microdata are available.

Figure 5 and the top panel of Table 3 present the main results of our cohort-based analysis examining the impact of abortion legalization on children ever born. Figure 5 focuses on children ever born throughout the childbearing years and Table 3 separately considers children ever born by age 25, 30, 35, and completed fertility (by age 44, or age 39 for cohorts who have not yet reached 44 in 1999).[18] As we described earlier, we are looking for a gap to emerge in the difference in children ever born between women in repeal and other states beginning around the 1946 birth cohorts that would then disappear for the post-1955 birth cohorts.

Figure 5 displays the difference in children ever born between women born in repeal states and other states over time. The results indicate a clear upward trend in this difference, as illustrated by the trend line added to the figure. Apparently, it used to be the case that women in non-repeal states had more children than women in repeal states, but this difference has been slowly going away. This highlights the need to consider the deviation from state-specific trend. Such a deviation from trend is clearly apparent for the relevant birth cohorts, those born between 1946 and 1955. For that set of cohorts, there is a sharp reduction in the differential of children ever born between repeal and non-repeal states.

This conclusion is confirmed in the top panel of Table 3. By the end of childbearing (the final column), we see that abortion legalization reduced children ever born by 0.054. Moreover, the difference in children ever born that emerged for the 1946 to 1955 birth cohorts in repeal

[18] Since all fertility information before 1968 comes from the information on children ever born information available in the 1970 Census, it is impossible to date when births occurred prior to that year. This explains why data on births by age 25 and age 30 are only available as far back as the 1943 and 1938 birth cohorts, respectively. To maintain at least some balance to our sample, we do not use birth cohorts prior to 1933 since we would not be able to date births by age 35 prior to that year. At the other extreme, births by age 35 are only available through the 1964 birth cohort since our micro Vital Statistics data ends in 1999. In addition, children ever born represents births to age 44, except for the 1956 through 1960 birth cohorts in which births to age 39 are used since these birth cohorts had not yet reached age 44 by 1999.

states was statistically eliminated in the post-1955 cohorts. The coefficient for that cohort is statistically and substantively insignificant. This further confirms the causal interpretation of our finding. The effects we estimate here are present only for the 1946-1955 cohorts, relative to both cohorts born before 1946 (the omitted group), and cohorts born after 1955; these are not just trends in differential state fertility behavior.

Evidence in the top panel of Table 3 also suggests that the similar reductions in fertility are observed by ages 25, 30, and 35. By each of those ages, we see that children ever born to women in repeal states had fallen by .07 relative to women in non-repeal states in the 1946 to 1955 birth cohorts. Importantly, this gap is statistically and substantively insignificant in the post 1955 cohorts (except, perhaps, by age 25). Although these point estimates for the 1946 to 1955 birth cohorts by age 35 are slightly larger than that by the end of childbearing, they are not statistically different. These results suggest that the reduction in births associated with abortion legalization in the early 1970s was essentially permanent. It does not appear that much, if any, of it was made up in the form of additional births at later ages.

As a rough assessment of the validity of these findings, we applied the impact of abortion legalization on period-specific birth rates estimated in Column 4 of Table 2 to the 1970 age-specific birth rates to determine the steady state impact on children ever born. We include the immediate reduction in fertility that occurred in the 1971 to 1973 period as well as the slow bounce back that is estimated for 1974 and 1975 in simulating the reduction in births. The results suggest that children ever born would have fallen by 0.043, which is comparable to (and not significantly different from) our estimate of 0.054 by the end of childbearing in Table 3.

Cohort-Based Approach – Distance Estimates

A means of confirming this striking finding is to take advantage of the fact that the non-repeal states are better "controls" the farther they were from the repeal states (due to the possibility of travel to the repeal states from nearby states). Following Levine, et al., we can divide the non-repeal states into those within 250 miles, between 250 and 750 miles, and more than 750 miles from repeal states. By doing so, we can assess whether our results are stronger for the farther away set of control states, as we would expect.

Figure 6 and the remainder of Table 3 report the results of our analysis, separately for control states of different distances to the repeal states. Figure 6 displays the difference in children ever born by the end of childbearing to women in repeal states relative to women in each of these distance categories by mother's birth cohort. Again, this figure shows that women in repeal states historically had fewer births, but it also shows that the difference is greater when they are compared to further away states. In each case the general trend is toward a narrowing of the gap. But for far away non-repeal states, another clear pattern is the break from trend that occurs in the difference in children ever born among cohorts born in the late 1940s and early 1950s. No such trend is apparent when comparing children ever born between repeal states and the nearest group of comparison states.

These assessments are confirmed in the bottom three panels of Table 3. By age 25, children ever born to women in repeal states born between 1946 and 1955 fell by an insignificant amount relative to women in the closest group of comparison states, by 0.085 children relative to the middle distance group, and by 0.099 children relative to the most distant group of comparison states. Moreover, the estimates are roughly stable throughout the remainder of the childbearing years. And, for all distances, the results are largely insignificant for the cohorts born after 1955. These results provide further evidence of a causal and largely permanent impact of abortion legalization on children ever born.

Distribution of Children Ever Born

We turn our attention now to the impact of abortion legalization on the distribution of children ever born rather than just the count. We pay particular attention to the percentage of childless women, which is of primary importance since it is the only point in this distribution for which we have an unambiguous prediction; more women should remain childless. Since the remainder of the distribution should be shifting to the left, it is difficult to tell whether the flow out of a particular level should be greater than the flow in from a larger value (e.g. the share of women who only had one child will be falling as those who otherwise would have one child move to zero children, but it will also be rising as those who otherwise would have two children move to one child).

The main results of this analysis are reported in Figure 7 and in the top panel of Table 4. Figure 7 displays the difference in the percentage of childless women between repeal and non-repeal states by birth cohort. For all birth cohorts before the mid 1940s, this difference is reasonably stable. But by the mid 1940s we begin to see a sizeable jump in the number of childless women in repeal states relative to non-repeal states. The gap widens through the early 1950s and then recedes, just as we would predict if the cause of the gap was differential abortion access. By 1956, the difference is at the same level as before the “treatment” cohort began in 1946.

Table 4 presents a regression-based version of these results that also includes the percentage of women with different numbers of children. The results presented here confirm our observations from Figure 7 in that abortion legalization is estimated to have increased the number of childless women in the treatment cohort by 3.57 percent. Once again, there is no significant impact on the post-1955 cohort, consistent with the causal interpretation of this finding.

The evidence regarding the impact at other points in this distribution is not that strong. Although we see a reduction in the number of women with one child and two children in the 1946 to 1955 repeal cohorts, a similar reduction is also observed for the later birth cohorts in those states. Therefore, it is difficult to attribute this change to abortion legalization itself as compared to some other ongoing trend that differed across states in the later birth cohorts.

Figure 8 and the remainder of Table 4 conduct the analogous exercises differentiating comparison states by their distance to repeal states. In Figure 8 we see that the difference in the percentage of childless women between repeal states and the closest set of non-repeal states was stable across birth cohorts. But as the distance increases across comparison states, we see the gap in the percentage of childless women for the late 1940s and early 1960s birth cohorts grow.

The bottom three panels of Table 4 confirm these impressions. Relative to the closest group of comparison states, there was no statistically significant increase in the percentage of childless women among women born in repeal states between 1946 and 1955. Relative to the middle distance and farthest distance groups, however, these women are estimated to be 3.89 percent and 5.45 percent more likely to be childless. The pattern by repeal status, birth cohort, and distance to repeal states provides strong evidence that this finding is causally related to abortion legalization.

Discussion and Implications of Lifecycle Fertility Results

The results from this analysis tell a very consistent story: the early availability of abortion in the repeal states led to a decline not only in contemporaneous but in permanent fertility, and this appears to arise to a large extent from women using abortion to avoid ever having children. This finding is clear both graphically and statistically. And it is confirmed both by the absence of any differential effect for cohorts born after 1955, and by the fact that the effect is much stronger the farther away the control states were from the early repeal states.

These results are also substantively large. The 1945 birth cohort, which had already passed through its peak childbearing years by the time abortion was legalized, averaged about 2.4 births per women; about 11.3 percent had no children at all.[19] By the 1960 birth cohort, which was fully exposed to legalized abortion in all states at the time they hit childbearing age, each woman had 1.9 children on average; 18.2 percent had no children. Our overall estimates imply that abortion availability lowered the number of children ever born by 0.054, and the odds of being childless by 3.6 percent. These figures account for 11 and 52 percent of the time series changes in these variables.

Moreover, this calculation likely provides a lower bound of the effect of legalized abortion because it does not account for the fact that some women in non-repeal states traveled to repeal states prior to the Roe decision. Using our estimates based on the control states farthest from the early repeal states, we would be able to explain 17 and 79 percent of the time series change in these variables.

Other Outcomes for Women

[19] This estimate is based on the data employed in this project. Estimates based on data from Current Population Fertility Supplements are more like 14 percent.

The fact that abortion availability had such a fundamental impact on the lifecycle fertility patterns of women raises the issue of whether legalization had impacts on other aspects of these women's lives as well. Women who reduced the total number of children they had or chose never to have any at all may have invested in more education and worked more, which would have led to higher wages. Their marital decisions may have been altered as well. Changes in both earnings and family composition effects also may have affected their poverty rates and welfare participation.

We attempted to investigate this by looking at measures of these outcomes available in the decennial Census. In order to focus on the full effects of reduced fertility, we looked at outcomes for women aged 35 to 44, for whom most childbearing is complete.[20] We observed the outcomes of the various cohorts of interest at these ages by pooling multiple Censuses. Observations for women born between 1930 and 1935 come from the 1970 Census, so that the outcomes of those born in 1930, for example, are measured at age 40, while the outcomes of the 1935 cohort are measured at age 35. For the 1936 to 1945 cohorts, observations come from the 1980 Census; for the 1946 to 1955 cohorts, from the 1990 Census; and for the 1956 to 1960 cohorts, from the 2000 Census.

We estimated models comparable in format to those reported earlier regarding children ever born to assess the impact of abortion legalization on women's outcomes, categorizing women according to state and year of birth and then examining the patterns in outcomes over time between women born in repeal and non-repeal states. The outcomes examined include: the probability of having completed high school or having completed college; current labor force participation; poverty status, and household income as a percent of the poverty line. Using these

[20] Ideally, we would focus on a somewhat older age range, but the youngest of treated women, who were 15 in 1970, had only reached age 45 by 2000.

samples, we were unable to identify effects of increased fertility control on any of these outcomes, but our results were not sufficiently precise to rule out such effects. Apparently, the availability of virtually universal Vital Statistics data on fertility provided enough statistical power in our earlier analysis that we were unable to attain relying exclusively on the 5 percent samples (or, in the 1970 Census, a 4 percent sample) available in the Census data.

One outcome that we have examined using more powerful data is marriage. Clearly, changes in fertility patterns brought about by abortion legalization had the potential to alter marriage outcomes, particularly for younger women who became pregnant, for whom "shotgun marriages" were prevalent around 1970 (c.f. Akerlof, et al., 1996). Again, we estimated models comparable in format to those reported earlier regarding children ever born, taking advantage of publicly available Vital Statistics microdata on marriages to women in the United States that occurred between 1970 and 1990.[21] We combined these data with additional data from the 1970 Census in much the same way that we described earlier to create counts of women who had ever been married by various ages. Specifically, we categorized women according to their state and year of birth and then obtained counts and rates of first marriages to examine patterns over time between repeal and non-repeal states. Using this approach, we examined the likelihood of ever marrying by age 18, 20, 22, 25, and 28.[22]

---

[21] These data exist for 1968 through 1995 (at which point they stopped being released), but the 1968 and 1991-1995 data do not contain information on the bride's state of birth. We also do not use data for 1969 because in the 1970 Census we are only able to identify women who have ever been married by the survey date and cannot date when the marriage occurred. Including 1969 marriages from Vital Statistics data would double count marriages in that year.

[22] Although the Vital Statistics data provide the best available source to study this issue, these data are not without their limitations. First, they do not represent the universe of marriages like the natality data provide. In many states, and particularly the large ones like New York and California, the data included only represent a five percent sample of all marriages that took place in that state. The smaller is the state, the higher is the sampling rate. We apply the sampling weights available to estimate the counts of marriages. Second, they do not report marriages in all 51 states (including the District of Columbia), but just in 42 of them. These states are excluded from this analysis.

Based on this approach, we were not able to identify a causal impact of abortion legalization on marriage. Although there were noticeable breaks from trend in the difference in marriage rates between repeal and non-repeal states beginning just after the 1950 birth cohort, the rebound back to steady state differentials that we would expect for late 1950s birth cohorts (who were all exposed to legalized abortion throughout their fertile years) is not apparent. Moreover, we do not observe any difference in this pattern by distance to repeal states. We are unable to determine whether the weakness in these results indicates that there really was no causal effect on marriage or whether it is attributable to the weaknesses of the underlying data.

VI. CONCLUSIONS

It is clear from past research that the legalization of abortion in the U.S. induced a major change in fertility. Estimates from past research, replicated here, suggest that total fertility fell by roughly 5 percent when abortion was legalized. Previous research on fertility has emphasized the importance of decomposing fertility changes into temporary fertility (timing) effects and permanent (completed fertility) effects. Yet this has not been done for this critical change in the nature of birth planning.

In this paper, we directly investigate the impact of abortion legalization on the lifecycle pattern of fertility of cohorts affected by legalization. The power of our analysis derives from the observation that abortion legalization had its primary effects on those aged 16 to 26; for younger and older ages, there was either a sufficiently small effect of abortion access, or sufficiently low baseline fertility. Those ages 16 to 26 in the early 1970s, when abortion laws differed across states, were mainly in the cohorts born between 1946 and 1955. Thus, by comparing differences in children ever born between women in early repeal states to women in other states during the 1946 to 1955 window relative to the analogous difference for women born before and after those years, we can provide a causal estimate of the impact of abortion legalization on lifetime fertility.

Our results are striking. It appears that the response of fertility to legalization was largely composed of reductions in completed fertility, not changes in timing. Moreover, abortion legalization is found to have led to a very large rise in childless women in these treatment cohorts. The causal nature of our findings is confirmed by the comparison to other cohorts surrounding the treatment cohort, and by the fact that the relationship is strongest relative to the most distant states whose residents could not benefit from early legalization.

There are several important implications of these findings. First, they suggest that improved fertility control does not simply lead to changes in fertility timing, as suggested by some other papers. Indeed, the majority of the effect of abortion legalization was permanent, and not timing effects. Second, they suggest that the availability of abortion can explain a sizeable share of the time series trend in reduced completed fertility and in childless women over these birth cohorts. Finally, there are potentially important implications for the long-term well-being of these cohorts of mothers from the improved fertility control. We were unable to draw strong conclusions about these implications from the available data, but this is a worthy goal for future research.

REFERENCES

Angrist, Joshua and William N. Evans. "Schooling and Labor-Market Consequences of the 1970 State Abortion Reforms," in Solomon Polachek and John Robst (eds.), *Research in Labor Economics*. Vol. 18 (1999). pp. 75-114.

Akerlof, George A., Janet L. Yellen, and Michael L. Katz. "An Analysis of Out-of-Wedlock Childbearing in the United States." *Quarterly Journal of Economics*. Vol. 111, No. 2 (May 1996). pp. 277-317.

Charles, Kerwin and Melvin Stephens, Jr. "Abortion Legalization and Adolescent Substance Abuse." National Bureau of Economic Research, working paper 9193, September 2002.

Chen, Renbao, and S. Philip Morgan. " Recent Trends in the Timing of First Births in the United States." *Demography*, Vol. 28, No. 4. (Nov., 1991), pp. 513-533.

Donahue III, John J., and Steven D. Levitt. "The Impact of Legalized Abortion on Crime." *Quarterly Journal of Economics*. Vol. 116, No. 2 (May 2001). pp. 379-420.

Donohue III, John J., and Steven D. Levitt. "Further Evidence that Legalized Abortion Lowered Crime: A Reply to Joyce." *Journal of Human Resources.* Vol. 39, No. 1 (Winter 2004). pp 29-49.

Downs, Barbara. "Fertility of American Women: June 2002." *Current Population Reports (P20-548)*. Washington, DC: U.S. Census Bureau. October 2003.

Garrow, David J., *Liberty and Sexuality: The Right to Privacy and the Making of Roe v. Wade*. New York, NY: Macmillan Publishing Company. 1994.

Gruber, Jonathan, Phillip B. Levine, and Douglas Staiger. "Abortion Legalization and Child Living Circumstances: Who is the "Marginal Child?" *Quarterly Journal of Economics*. February 1999. pp. 263-292.

Heckman, James and James R. Walker. " The Relationship Between Wages and Income and the Timing and Spacing of Births: Evidence from Swedish Longitudinal Data." *Econometrica*, Vol. 58, No. 6. (Nov., 1990), pp. 1411-1441.

Hotz, V. Joseph, and Robert A. Miller. "An Empirical Analysis of Life Cycle Fertility and Female Labor Supply." *Econometrica*, Vol. 56, No. 1. (Jan., 1988), pp. 91-118.

Hotz, V. Joseph, Jacob Alex Klerman, and Robert J. Willis. "The Economics of Fertility in Developed Countries." Chapter 7 in Mark Rosenszweig and Oded Stark (eds.) *Handbook of Population and Family Economics.* Amsterdam: Elsevier Science. 1997.

Joyce, Theodore. "Did Legalized Abortion Lower Crime?" *Journal of Human Resources.* Vol. 39, No. 11 (Winter 2004). pp. 1-28.

Levine, Phillip B., Douglas Staiger, Thomas J. Kane, and David J. Zimmerman. "Roe v. Wade and American Fertility." *American Journal of Public Health.* February 1999. pp. 199-203.

Martin, Steven P. "Diverging Fertility among U.S. Women Who Delay Childbearing Past Age 30." *Demography*. Vol. 37, No. 4 (November 2000). pp. 523-533.

Moffitt, Robert. "Optimal Life-Cycle Profiles of Fertility and Labor Supply," in T. Paul Shultz and Kenneth Wolpin (eds.), *Research in Population Economics.* Greenwich, CT: JAI Press. 1984.

Morgan, S. Philip, Nikolai Botev, and Renbao Chen. "White and Nonwhite Trends in First Birth Timing: Comparisons Using Vital Registration and Current Population Surveys." *Population Research and Policy Review*. Vol. 18, No. 4 (August 1999).

Rindfuss, Ronald R., S. Philip Morgan, and Kate Offutt "Education and the Changing Age Pattern of American Fertility: 1963-1989." *Demography*, Vol. 33, No. 3. (Aug., 1996), pp. 277-290.

Ruggles, Steven and Matthew Sobek, et al. *Integrated Public Use Microdata Series, Version 3.0*. Minneapolis: Historical Census Projects, University of Minnesota, 2003. http://www.ipums.umn.edu/

U.S. Centers for Disease Control, National Center for Health Statistics. *Vital Statistics of the United States, 1999. Volume I, Natality.* Accessed February 23, 2004 at: http://www.cdc.gov/nchs/datawh/statab/unpubd/natality/natab99.htm

Table 1: Mother's Birth Cohort versus Child's Year of Birth

	Year of Child's Birth																	
Year of Mother's Birth	1968	1969	1970	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984	1985
1927	41	42	43	44														
1928	40	41	42	43	44													
1929	39	40	41	42	43	44												
1930	38	39	40	41	42	43	44											
1931	37	38	39	40	41	42	43	44										
1932	36	37	38	39	40	41	42	43	44									
1933	35	36	37	38	39	40	41	42	43	44								
1934	34	35	36	37	38	39	40	41	42	43	44							
1935	33	34	35	36	37	38	39	40	41	42	43	44						
1936	32	33	34	35	36	37	38	39	40	41	42	43	44					
1937	31	32	33	34	35	36	37	38	39	40	41	42	43	44				
1938	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44			
1939	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44		
1940	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	
1941	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44
1942	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43
1943	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42
1944	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41
1945	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40
1946	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39
1947	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38
1948	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37
1949	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36
1950	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35
1951	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34
1952	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33
1953	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32
1954		15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31
1955			15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
1956				15	16	17	18	19	20	21	22	23	24	25	26	27	28	29
1957					15	16	17	18	19	20	21	22	23	24	25	26	27	28
1958						15	16	17	18	19	20	21	22	23	24	25	26	27
1959							15	16	17	18	19	20	21	22	23	24	25	26
1960								15	16	17	18	19	20	21	22	23	24	25
1961									15	16	17	18	19	20	21	22	23	24
1962										15	16	17	18	19	20	21	22	23
1963											15	16	17	18	19	20	21	22
1964												15	16	17	18	19	20	21
1965													15	16	17	18	19	20

Table 2: Sensitivity of Estimated Impact of Abortion Legalization on Fertility
(Coefficients multiplied by 100, standard errors in parentheses)

	(1)	(2)	(3)	(4)
Repeal*1971-1973	-4.13	-4.38	-6.15	-6.71
	(0.81)	(0.76)	(0.74)	(1.11)
Repeal*1974-1975	-1.45	-0.01	-1.78	-3.90
	(1.16)	(1.14)	(1.09)	(1.62)
Repeal*1976-1980	2.30	2.55	0.75	-1.76
	(1.60)	(1.67)	(1.57)	(2.34)
Vital Statistics Data by State of Occurrence, 1965-1980	X			
Vital Statistics Data by State of Occurrence, 1968-1980		X	X	
Vital Statistics Data by Mother's State of Birth, 1968-1980				X
Original Model Specification	X	X		
Fixed Effects and State-Specific Trends Only			X	X

Notes: The dependent variable in each specification is the natural logarithm of the birth rate, so that each coefficient can be interpreted as a percentage change. Information on mother's state of birth is not available on the 1968, 1969, and 1971 Vital Statistics microdata and is imputed using the methods described in the text. The original model specification also includes per capita income, the insured unemployment rate, and the crime rate in each state/year.

Table 3: Estimated Impact of Abortion Legalization on Children Ever Born, by Age

	Children Ever Born by: Age 25	Age 30	Age 35	End of Childbearing
Repeal States Relative to All Non-Repeal States				
Repeal*1946-1955 birth cohorts	-0.070	-0.070	-0.070	-0.054
	(0.013)	(0.018)	(0.023)	(0.012)
Repeal*post-1955 birth cohorts	-0.033	0.006	0.018	-0.002
	(0.019)	(0.030)	(0.030)	(0.018)
Repeal States Relative to Non-Repeal States within 250 Miles of a Repeal State				
Repeal*1946-1955 birth cohorts	-0.031	-0.010	-0.007	-0.007
	(0.020)	(0.023)	(0.030)	(0.023)
Repeal*post-1955 birth cohorts	-0.016	0.017	0.006	-0.009
	(0.024)	(0.032)	(0.038)	(0.026)
Repeal States Relative to Non-Repeal States between 250 and 750 Miles from a Repeal State				
Repeal*1946-1955 birth cohorts	-0.085	-0.098	-0.095	-0.073
	(0.017)	(0.021)	(0.026)	(0.013)
Repeal*post-1955 birth cohorts	-0.042	-0.005	-0.006	-0.008
	(0.023)	(0.033)	(0.035)	(0.028)
Repeal States Relative to Non-Repeal States greater than 750 Miles from a Repeal State				
Repeal*1945-1955 birth cohorts	-0.099	-0.108	-0.114	-0.087
	(0.023)	(0.030)	(0.026)	(0.019)
Repeal*post-1955 birth cohorts	-0.040	0.009	0.016	0.028
	(0.033)	(0.048)	(0.039)	(0.033)

Notes: The "end of childbearing" is defined to be the oldest age for which births occur. For younger cohorts additional births are likely to result after the period for which data is available here. To address this, I only include the 1960 and earlier birth cohorts whose fertility at least through age 39 is recorded. Since so few births occur after that, I define lifetime births to equal the births that have occurred through the last age observed. All specifications include state and year fixed effects along with state-specific trends. Standard errors are corrected for heteroskedasticity and an arbitrary covariance structure within birth states.

Table 4: Estimated Impact of Abortion Legalization on the Distribution of Children Ever Born
(standard errors in parentheses)

	Number of Children Ever Born:				
	No Children	1 Child	2 Children	3 Children	4+ Children
Repeal States Relative to All Non-Repeal States					
Repeal*1946-1955 birth cohorts	3.57	-1.73	-1.85	-0.71	0.72
	(1.32)	(1.03)	(0.46)	(0.49)	(0.37)
Repeal*post-1955 birth cohorts	-0.19	-1.12	-1.46	1.43	1.34
	(0.58)	(0.33)	(0.39)	(0.50)	(0.80)
Repeal States Relative to Non-Repeal States within 250 Miles of a Repeal State					
Repeal*1946-1955 birth cohorts	1.55	-1.21	-1.65	0.34	0.97
	(1.54)	(1.11)	(0.49)	(0.58)	(0.45)
Repeal*post-1955 birth cohorts	-0.01	-1.09	-1.35	1.89	0.56
	(0.79)	(0.45)	(0.56)	(0.56)	(0.91)
Repeal States Relative to Non-Repeal States between 250 and 750 Miles from a Repeal State					
Repeal*1946-1955 birth cohorts	3.89	-1.75	-1.64	-1.00	0.50
	(1.35)	(1.08)	(0.56)	(0.51)	(0.46)
Repeal*post-1955 birth cohorts	-0.45	-1.03	-1.15	1.43	1.20
	(0.76)	(0.57)	(0.59)	(0.57)	(1.08)
Repeal States Relative to Non-Repeal States greater than 750 Miles from a Repeal State					
Repeal*1946-1955 birth cohorts	5.45	-2.23	-2.44	-1.57	0.79
	(1.45)	(1.08)	(0.59)	(0.53)	(0.44)
Repeal*post-1955 birth cohorts	-0.53	-1.22	-2.14	1.05	2.83
	(1.01)	(0.55)	(0.59)	(0.59)	(0.94)

Notes: Data represents 1930 to 1960 birth cohorts. All specifications include state and year fixed effects along with state-specific trends. Standard errors are corrected for heteroskedasticity and an arbitrary covariance structure within birth states.

Figure 1: 1970 Birth Rate and Relative Impact of Abortion Legalization, by Age

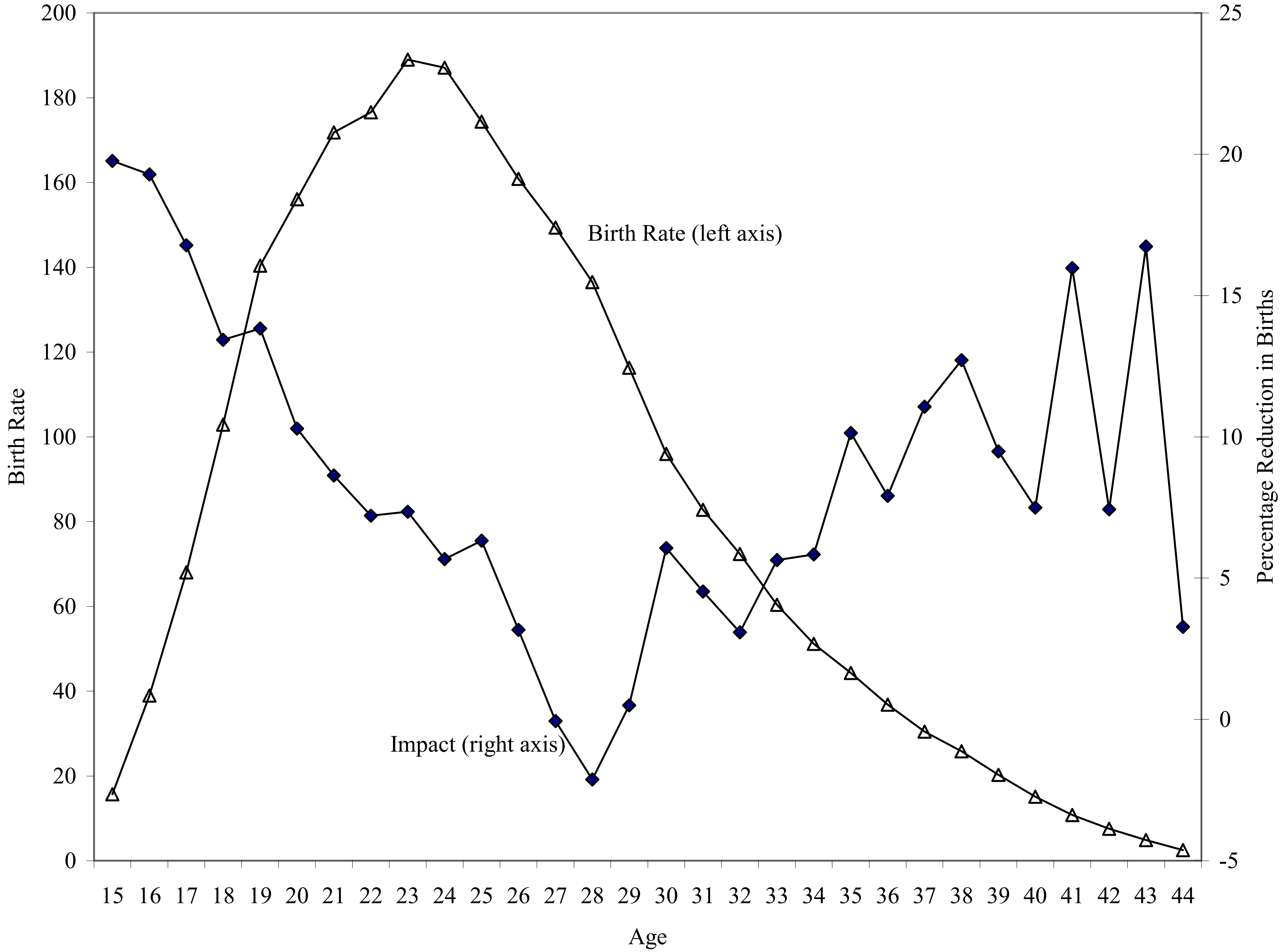

Figure 2: Absolute Impact of Abortion Legalization on Birth Rate, by Age

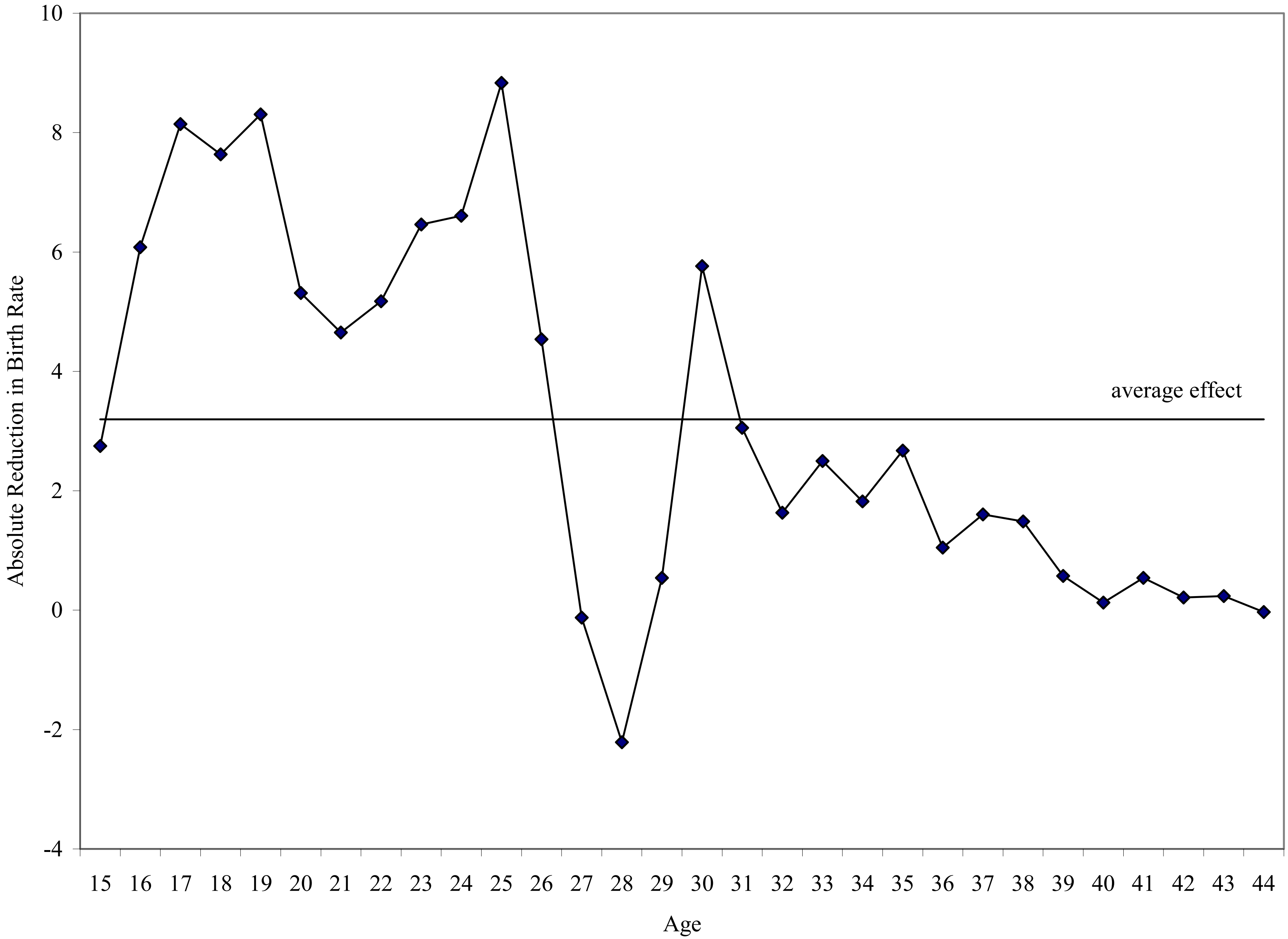

Figure 3: Average Children Ever Born, by Birth Cohort

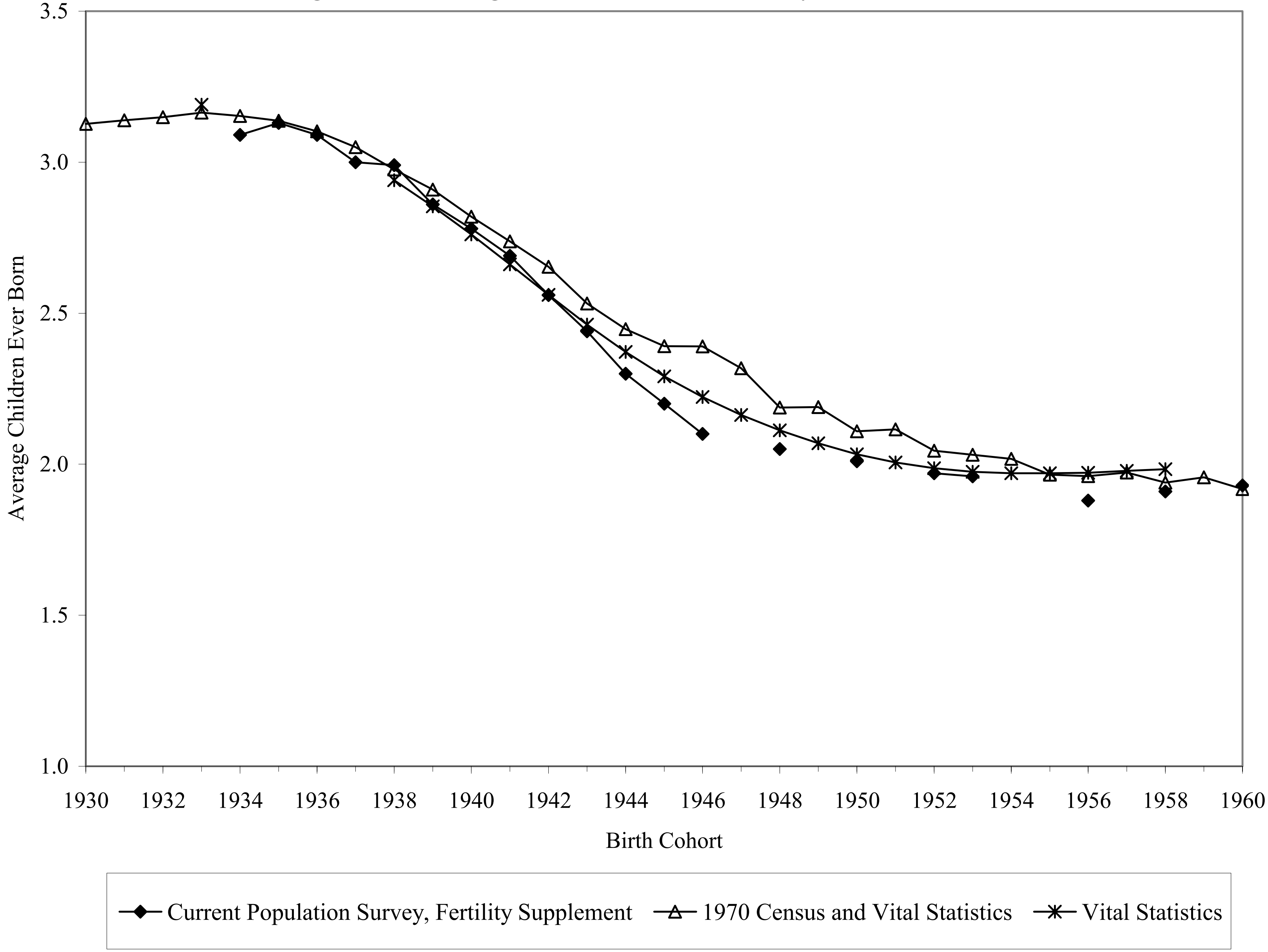

Figure 4: Percentage Difference in Birth Rates between Repeal and Non-Repeal States

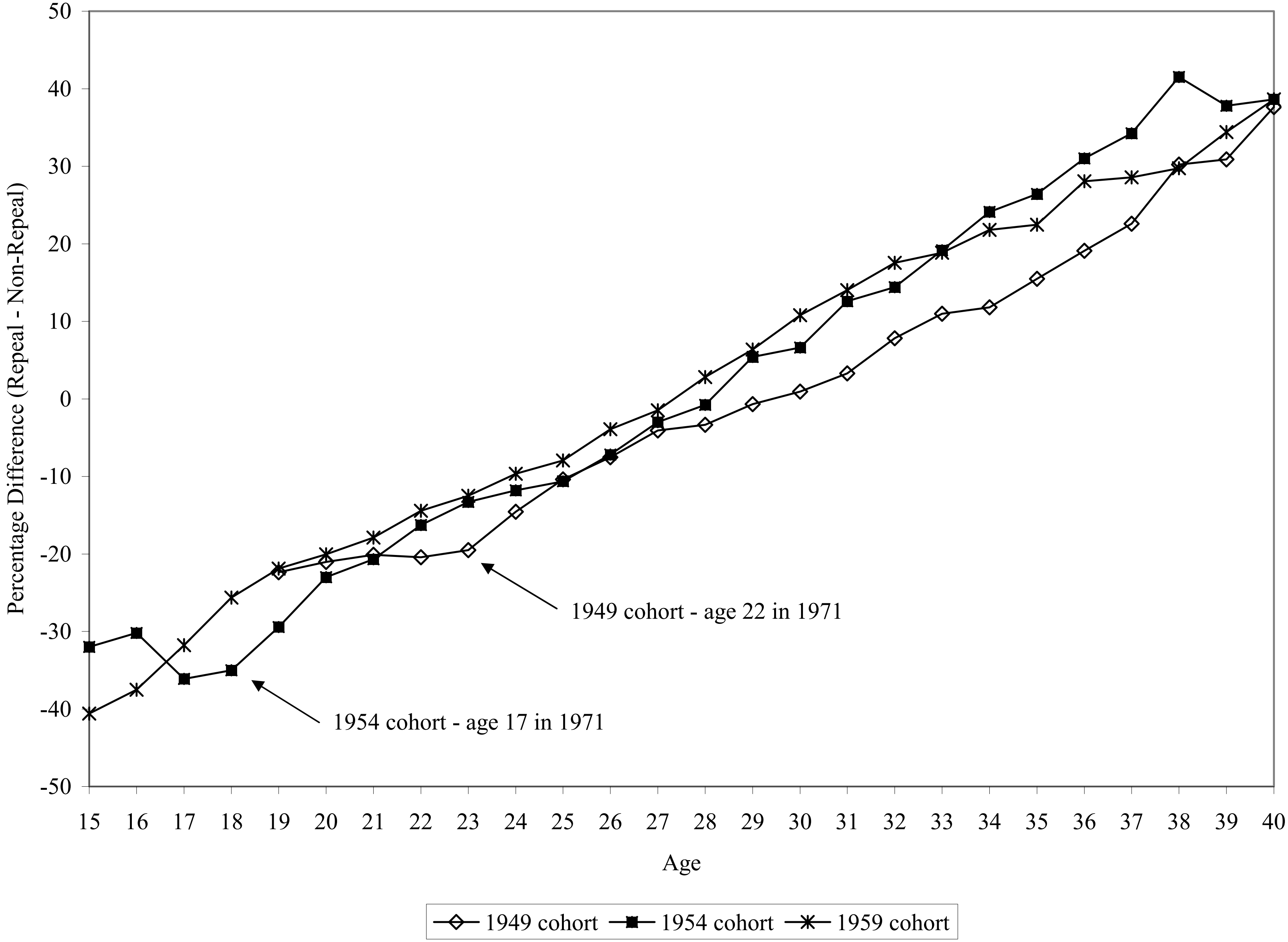

Figure 5: Effect of Abortion Legalization on Children Ever Born

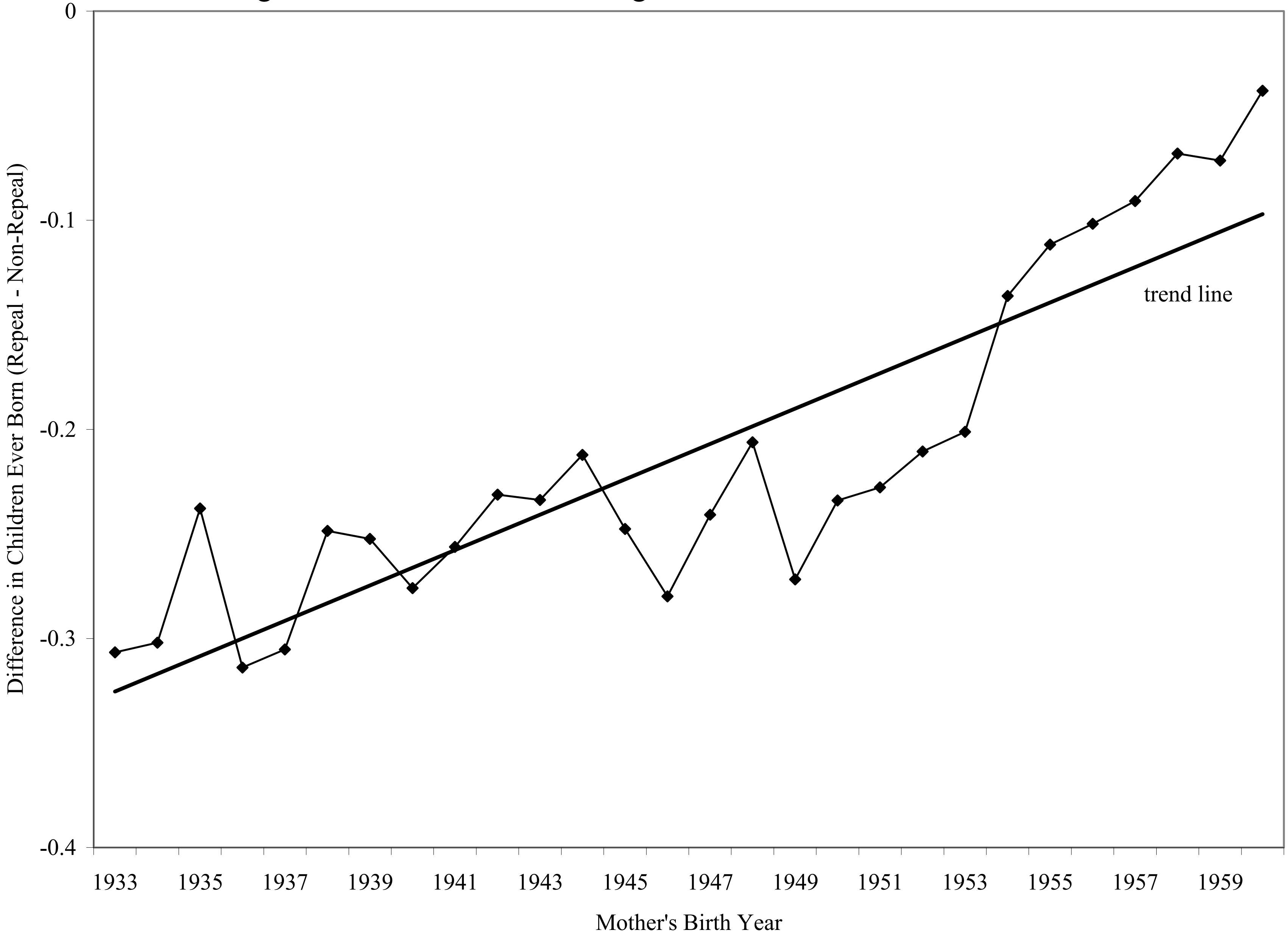

Figure 6: Impact of Abortion Legalization on Children Ever Born, by Distance from Repeal State

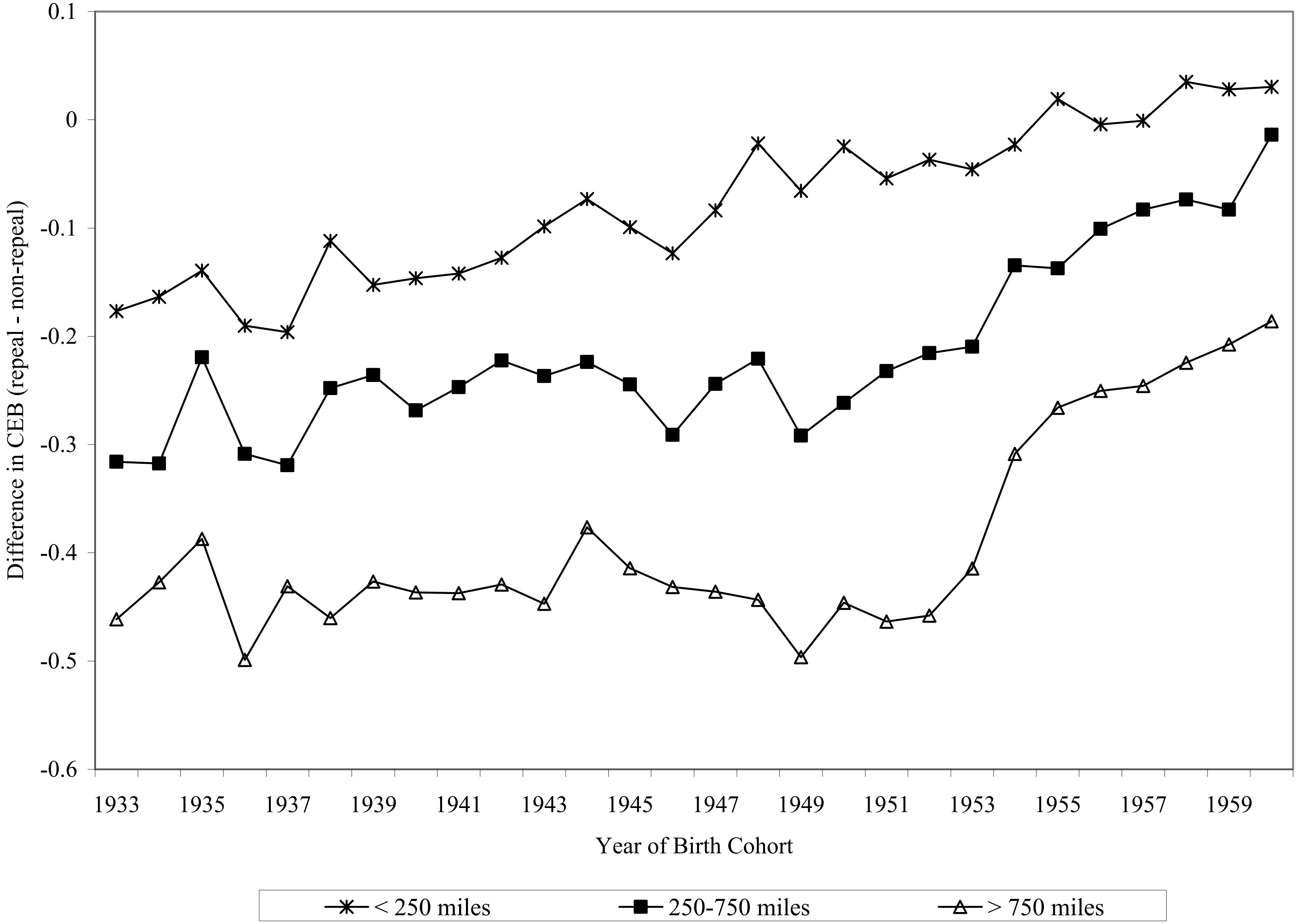

Figure 7: Difference in Percentage of Childless Women, by Birth Cohort

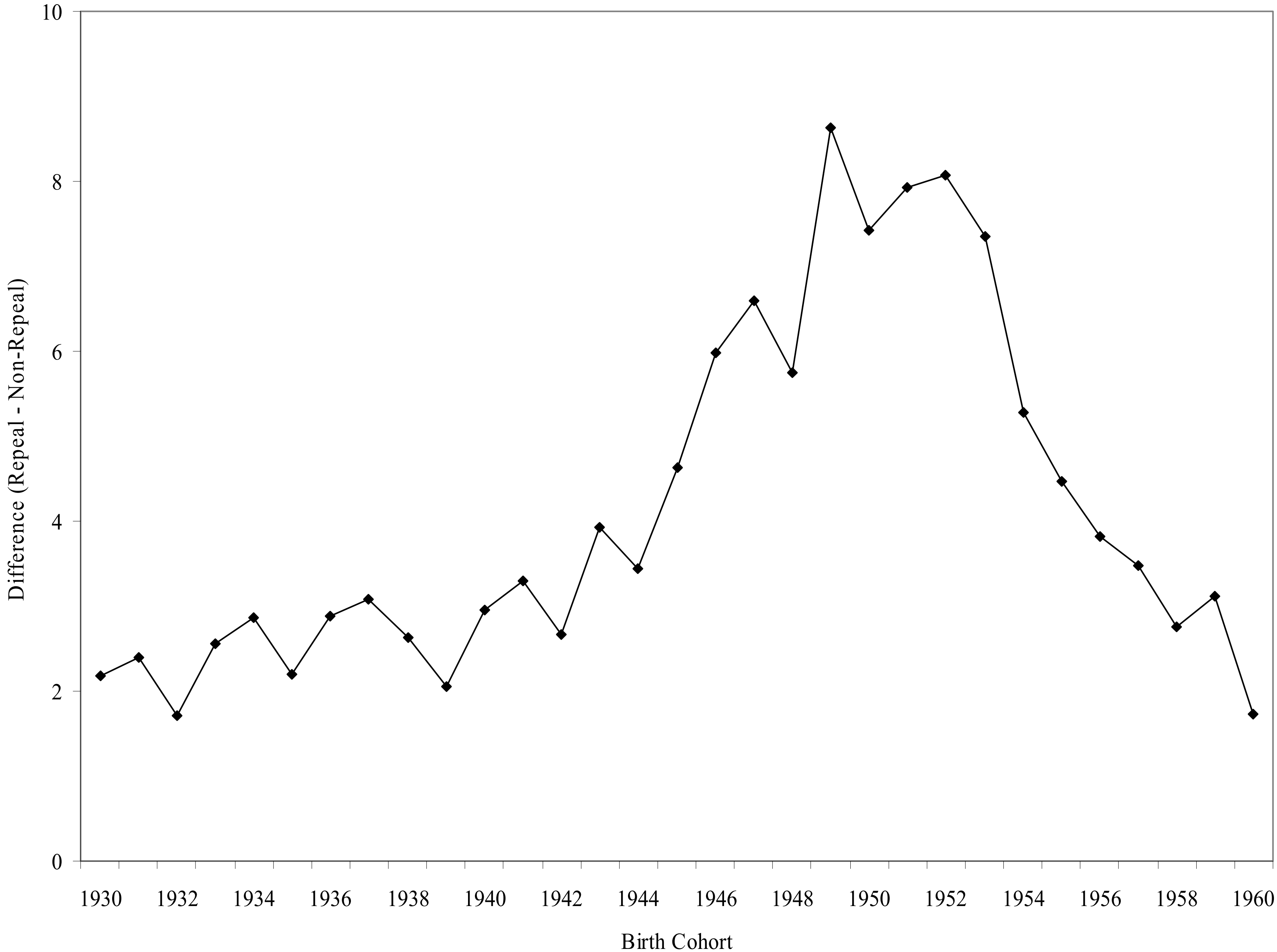

Figure 8: Impact of Abortion Legalization on the Percentage of Women with No Children, by Distance from Repeal State