ABORTION LEGALIZATION AND ADULT OUTCOMES:

THE "MARGINAL CHILD" AT AGE 30

by

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I. INTRODUCTION

The legalization of abortion in the United States in the early 1970s represents one of the most important changes in American social policy in the 20th century. Prior to the mid 1960s, abortion was illegal nationwide except in rare instances in which the mother's life was in danger. A decade later, abortion was legal upon request in all states. At that time, over 1 million abortions were performed annually, compared to about 3 million births.

This policy change has obvious implications for the likelihood of giving birth in the case of an unintended pregnancy, but the social significance of this change goes well beyond that. For instance, abortion legalization may have caused major shifts in the characteristics of birth cohorts. Children's outcomes may have been better on average because they are more likely to be born into a household in which they are wanted, or because additional resources may be devoted to the fewer children who are born.

In earlier work, Gruber, Levine and Staiger (1999) found that the legalization of abortion did lead to significant improvements in the circumstances of children born into cohorts where abortion was legal. Such cohorts of children featured lower rates of single motherhood, welfare receipt, poverty, and infant mortality than nearby cohorts of children. A natural question raised by this research is whether the improvement in life circumstances for these cohorts of children persisted later into life.

This paper investigates the later life consequences of abortion access for those early 1970s birth cohorts. We use the 2000 Census, when the cohorts exposed to abortion legalization are in their late twenties, to investigate the impact of abortion legalization on income, education, marital status, and other adult outcomes. To do so, we draw on the identification strategy developed by Levine et al. (1999), and employed in our earlier paper. In 1970, abortion was legalized in a small set of states, most notably New York and California. Abortion was then legalized in the rest of the nation under the Roe vs. Wade decision of 1973. These changes provide two opportunities for "difference-in-difference" estimates of the impact of abortion legalization on outcomes: the change in outcomes for the "early legalizers" in the 1971-1973 period, versus other states where abortion remained illegal, and the change in outcomes for the remainder of the nation after 1973, versus the early legalizers. By studying both of these changes, we are able to control for underlying state-specific trends in cohort outcomes.

We have two findings of importance. First, based on this approach we are unable to find causal effects of abortion legalization on most later-life outcomes. The pattern of the differences across cohorts that we observe between those born in states legalizing abortion early and those born in other states is inconsistent with the effect of abortion legalization on cohort size. Differences in adult outcomes that emerge during 1971-1973 in the "early legalizer" states continue to grow after abortion is legalized in other states after 1973. Thus, these differences appear to be due to some other factor that has changed among cohorts born in the "early legalizer" states, rather than due to selective abortion.

Second, we also estimated comparable models using the identification strategy introduced by Donahue and Levitt (2001) in their analysis of abortion and crime, linking today's criminal activity to the abortion rate in the criminal's time and place of birth. When we adopt that approach, we find that cohorts born in locations with a higher abortion rate do have some improved later life outcomes. We discuss the discrepancy in the results and the differences in the identification strategies that lead to them. Although we are unable to definitively rule out the Donahue and Levitt approach, we conclude that the abortion rate seems a less likely candidate for useful identification of abortion access effects than abortion legalization does.

Our paper proceeds as follows. Section II provides background on abortion legalization and reviews previous studies of its effects. Section III then discusses our data and methodology for the current study. Section IV presents the results. Section V provides a detailed discussion of alternative means of identifying the effect of abortion legalization. Section VI concludes.

II. BACKGROUND

A. Abortion Legalization

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 is dramatic, as Figure 1 shows. 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 (also see Figure 1). 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. The legislative history enabled them to categorize states by abortion legality in different years and to employ a quasi-experimental design. First, the effect of changes in state abortion laws prior to Roe could be identified by comparing fertility rates in these states before and after 1970s to fertility rates in states where the legal status of abortion was unaltered prior to 1973. Second, in 1973 the treatment reversed. The effect of Roe v. Wade can be identified by comparing fertility rates before and after 1973 in states that had not previously legalized abortion to those states that had legalized earlier.

The results obtained based upon these comparisons indicate that the legalization of abortion in the United States in the early 1970s reduced the fertility rate by about 5 percentage points. This effect is shown clearly in Figure 2, from Levine et al. (1999). There is a very clear "hat" pattern to fertility differences across the early repeal states versus other states, with fertility first falling in the early repeal states relative to the others, then rising again as abortion was legalized nationwide.

B. Abortion Legalization and Child Outcomes

Gruber et al. (1999) employed the identification strategy from Levine et al. (1999) to examine the effect of abortion legalization on the outcomes of cohorts of youths born in the early 1970s. They found strong evidence that abortion legalization improved outcomes for those born in the early repeal states in the 1971-1973 period, relative to other states, and that this relative improvement had faded by 1976 (consistent with the evidence of delayed reaction to legalization in the Roe v. Wade states). They go on to estimate the characteristics of those children who would have been born had abortion not been legalized (the "marginal" child) and find their outcomes would have been inferior to the average characteristics of those children who were born. In particular, they find that the marginal child would have been 60 percent more likely to live in a single parent household, 50 percent more likely to live in poverty, 45 percent more likely to be in a household collecting welfare, and 40 percent more likely to die during the first year of life.

Donahue and Levitt (2001) represent a second attempt to identify changes in children's outcomes as a result of abortion legalization. They ask whether the legalization of abortion in the early 1970s contributed to a decline in crime that began nearly two decades later. If fewer unwanted children are born, then crime may be reduced when those children would have reached adulthood. They employ a variety of methods to investigate this claim, but none of them ever replicate the quasi-experimental approach used by Gruber, et al. The strongest of their identification strategies uses data on arrest rates by individuals' state/year of birth. They regress the arrest rate in each cell against the abortion rate in the state/year in which the individual was born. The results of this, and their other analyses, indicate that abortion is strongly related to crime suggesting that abortion legalization in the early 1970s can explain as much as half of the decline in crime observed in the 1990s.

The Donahue and Levitt paper generated a great deal of controversy upon its release, and Joyce (2003) has provided a formal critique, to which Donahue and Levitt (2003) have responded. Although it is beyond the scope of this paper to fully elaborate upon all the points made by Joyce along with Donahue and Levitt's responses to them, we do want to focus on Joyce's criticism of their identification strategy.[1] Joyce argues that including the abortion rate on the right hand side of the regression does not accurately gauge variation in unwanted births. Since abortion legalization may have increased the number of pregnancies, abortions may vary independently of the number of unwanted births, suggesting that the relationship Donahue and Levitt have estimated between crime and abortions may be spurious. Joyce re-estimated the Donahue and Levitt model using the double quasi-experiment implemented by Levine, et al. (1999) and Gruber, et al. (1999) and obtained results that he argued were inconsistent with a causal interpretation. Donahue and Levitt disputed this reading of Joyce's findings, however. We will return to a more complete discussion of this issue later in the paper.

Charles and Stephens (2002) present the only other contribution to this literature on abortion and children's outcomes of which we are aware. They estimate the impact of abortion legalization on drug use and employ quasi-experimental methods in their estimation in much the same manner as Gruber, et al. (1999). They find that legalized abortion led to a significant reduction in drug use and they argue that selection was the cause. Although these results are consistent with Donahue and Levitt's analysis of crime and the methodological approach employed is strong, their analysis is not without limitations. Perhaps foremost among them is that the magnitude of their estimates may be implausibly large. For instance, in one specification, they report that abortion legalization reduced the use of "any illicit drug except marijuana" by 4.5 percentage points. Although no explicit comparison is made, this impact seems very large considering births only declined by 5 percent as a result.

[1] Joyce's attack on the Donahue and Levitt findings continues in a more recent paper by Joyce (2004) as well, but his arguments are not directly relevant to our analysis so we do not describe them here.

III. METHODOLOGY AND DATA

By the year 2000, the cohorts born in the period around abortion legalization had reached adulthood. The 2000 decennial U.S. Census therefore allows us to examine how the reduction in fertility associated with legalization affected children's outcomes as adults. This section will describe the methods and data employed for our study.

A. Methodology

Our main analytical approach is similar to that used by Gruber, Levine, and Staiger (1999) in their analysis of the immediate effects of abortion availability on child living circumstances. Their analysis included a reduced form component that was intended to identify the causal impact of abortion legalization on several alternative measures of child-well being for those children born. They also estimated structural models designed to estimate the living circumstances of the children who would have been born had abortion law remained unchanged. Once again, we highlight the key advantage of the legislative environment over this period: the fact that abortion was legalized first in several states, and then several years later in the rest of the nation, means that we have two distinct quasi-experiments with which to assess the impact of legalization. Thus, any general trends in birth control or other factors that impact wantedness in one state or another will not confound our estimates, unless those trends also happen to reverse between 1970 and 1973.

The important innovation in the present analysis is our focus on young adult outcomes Specifically, we initially estimate reduced form regression models of the form:

$$OUTCOME_{st} = \beta_1 REPEAL_s{*}D7173 + \beta_2 REPEAL_s{*}D7475 + \beta_3 REPEAL_s{*}D7679 \quad (1)$$
$$+ \beta_4\delta_s + \beta_5\tau_t + \beta_6\delta_s{*}TREND + \beta_7\delta_s{*}TRENDSQ + \beta_8 X_{st} + \varepsilon_{st}$$

where: $OUTCOME_{st}$ is a measure of the adult outcomes of children born in state s in year t; $REPEAL_s$ is a dummy for a cohort born in a repeal state; D7173, D7475, and D7679 are dummies for the eras 1971-1973, 1974-1975, and 1976-1979, respectively; δ_s is a set of state dummies; τ_t is a set of year dummies; TREND and TRENDSQ are linear and squared time trends; 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 regions or take place over time. This specification estimates differences in outcomes between children in repeal and non-repeal states that emerged during the 1971 to 1973 period, in which abortion laws differed across states. It then allows for a transition period in 1974 and 1975 in which any differences that may have emerged earlier are dissipated after the Roe decision legalized abortion nationwide. The 1976-1979 birth cohorts are those for whom any differences in outcomes across cohorts/states should have been eliminated. As such, the impact of abortion legalization in the repeal states is captured by the coefficient β_1 and the impact of abortion legalization in the non-repeal states is captured by $\beta_3 - \beta_1$.

We also plan to implement methods to distinguish what the outcomes would have been for the *marginal* child who was aborted following legalization. Therefore, we will also estimate the model:

$$OUTCOME_{st} = \alpha_1 * \ln(BIRTHRATE_{st}) + \alpha_2\delta_s + \alpha_3\tau_t + \alpha_4\delta_s * TREND + \alpha_5\delta_s * TRENDSQ + \beta_8 X_{st} + \varepsilon_{st}\,, \tag{2}$$

where $BIRTHRATE_{st}$ is the number of births per 1,000 women of childbearing age and the remainder of the notation is identical to that in equation (1). It is straightforward to show that the coefficient α_1 in this model is an estimate of the gap between the marginal outcome and the average outcome in the cohort (see Gruber, et al., 1999, for this derivation). If α_1 is zero, then declines in the birth rate are not associated with changes in adult outcomes, implying that there was no selection (i.e. the outcome for the marginal child that was not born into the cohort would have been the same as the average child). If α_1 is negative, however, then there is positive selection after abortion legalization (the decline in births was associated with an improvement in outcomes, implying the marginal child would have had worse outcomes than average).

OLS estimates of equation (2) will misstate the differences between the average child and the marginal child not born due to abortion access because much of the variation in birth rates is not attributable to changes in abortion access. In order to isolate the effects of abortion legalization, we therefore estimate these equations by two stage least squares, using the variation in abortion legalization across states and years to instrument for the birthrate. Thus, the first stage regression takes the form:

$$\ln(\text{BIRTHRATE})_{st} = \beta_1\text{REPEAL}_s{*}\text{D7173} + \beta_2\text{REPEAL}_s{*}\text{D7475} + \beta_3\text{REPEAL}_s{*}\text{D7679} \quad (3)$$
$$+ \beta_4\delta_s + \beta_5\tau_t + \beta_6\delta_s{*}\text{TREND} + \beta_7\delta_s{*}\text{TRENDSQ} + \beta_8 X_{st} + \varepsilon_{st}$$

The second stage takes the form of equation 2, but uses the predicted value of the log birthrate rather than the observed value. The coefficient α_1 from this second stage represents an estimate of the gap between the marginal outcome and the average outcome in the cohort that is only attributable to the changes in fertility associated with abortion legalization.

Our identification strategy using the 2000 Census is based on a comparison of cohorts born between 1965 and 1979 in repeal and non-repeal states. In 2000, these cohorts were in the 21 to 35 age range. Our identification strategy, which is based on comparisons across groups of birth years (1965-1970, 1971-1973, 1974-1975, and 1976-1979) is equivalent to one in which we base our comparisons by age (30-35, 27-29, 25-26, and 21-24). We include birth year dummies, so we are not simply identifying from differences in outcomes by age, but rather interactions of age with state of birth. That is, in the 2000 Census cross-section, this regression amounts to asking whether there are different patterns in outcomes by age across the early repeal and other states.

A limitation of this identification strategy is that there may be differential age patterns in outcomes across the early repeal and other states. In equation (1), we address this concern by including state-specific linear and quadratic trends in year of birth (which is equivalent to age in the 2000 cross-section). Thus, our regression asks whether there was any deviation from state-specific (linear and quadratic) trends around the time of the abortion legalization.

Another means of confirming that state-specific age patterns in outcomes are not driving our result is to use data from an earlier time period in which abortion legalization did not affect births. That is, if the state-specific age patterns in outcomes are constant over time, then we can isolate them by using data from the 1990 census to measure the age patterns. Data from the 1990 Census offers that possibility since individuals between 21 and 35 in 1990 were born between 1955 and 1969, prior to legalization.

Incorporating both the 1990 and 2000 Census to estimate the impact of abortion legalization amounts to a triple difference identification strategy. In models using this approach, we look for a differential in outcomes for those aged 27 to 29 in 2000 (i.e. born in 1971 to 1973) between repeal and non-repeal states that did not exist at other ages in 2000 or even at those ages in 1990. Results from this approach are reported below as well.

B. Data

To implement these methods, we employ data from the 5 percent samples of the 1990 and 2000 decennial Census of the United States These data are taken from the Minnesota Integrated

Public Use Microdata Series (Ruggles and Sobek 2003), which is standardized across years. The available outcomes that we analyze include: high school dropout, high school graduate, have some college, and college graduate (defined as having completed less than twelfth grade, exactly twelfth grade, more than twelfth grade but not four years of college, or completed four or more years of college, respectively); current employment; welfare receipt; the probability of living below the poverty line; current marital status; number of children (a created measure provided in the Minnesota IPUMS data set); and incarceration.[2]

Our sample includes those born in the United States between 1965 and 1979 and observed in the 2000 Census, as well as those born between 1955 and 1969 and observed in the 1990 Census. Based on the methods described earlier, a unit of observation represents mean values for each state/year of birth cohort. The means are weighted by the person weights provided by the Census Bureau, and the cell sizes are used as weights in the regression analysis.

In addition to the census data, we use data on birth rates available by state of birth and year of birth from *Vital Statistics of the United States*. We also use data on the economic and demographic conditions in the state and year of birth to provide some controls for the environment in which the birth decision was made. Per capita income, the crime rate, and the percent of the population that are white are obtained from the Statistical Abstract (various years). The insured unemployment rate is obtained from the United States Department of Labor, Employment Training Administration (1983).

[2] The Census only provides data on institutionalization, not incarceration per se. But past evidence suggests that the vast majority of institutionalized young adults are incarcerated. The 1980 Census is the most recent Census that provides detailed institutionalization data. Based on our calculations, in that year 68 percent of those aged 20 to 35 who were institutionalized were incarcerated. This rate is likely to be much higher today since incarceration rates nearly quadrupled (U.S. Department of Justice, 2003) while the number in mental institutions declined (Grob, 2000) since then.

IV. RESULTS

A. Reduced Form Results from 2000 Census

Table 1 displays the results when we estimate the reduced form models characterized by equation (1). Before describing our results on adult outcomes, we first confirm that the fertility effects associated with abortion legalization reported in Levine, et al. (1999) and Gruber, et al. (1999) are captured in the data roughly 30 years later in the form of reduced cohort size. We construct a measure that we call the "survival rate," which represents the number of individuals in a state/year of birth cohort alive in the 2000 Census per 1,000 women of childbearing age in that state/year those individuals were born. If there were no mortality since birth or, more plausibly, if mortality since birth were small and roughly random, then estimates using this dependent variable should be roughly comparable to the previously estimated birth effect.[3] Therefore, estimates of this model serve as something as a specification check that cohort sizes in the 2000 data reflect the earlier impact of abortion legalization.

Our estimates of this model satisfy that check, indicating that these constructed survival rates are just over 4 percent lower in repeal states relative to non-repeal states in cohorts born between 1971 and 1973 relative to that from earlier cohorts. That gap becomes statistically insignificant in the years following the Roe v. Wade decision. These estimates mimic rather closely those obtained in Levine, et al. (1999) and also Gruber, et al. (1999). For this and the other dependent variables in the table (except for number of children) the coefficients are multiplied by 100 to represent percentage point effects.

[3] In fact, we know that this assumption, taken in its strongest terms, is inaccurate. Gruber, et al. (1999) shows that the infant mortality rate for the marginal child following abortion legalization was 40 percent higher than that for the average child. But the infant mortality rate is so low (1.9 percent during that period) that a somewhat lower rate would be swamped by the magnitude of the impact on fertility itself.

Columns (2) through (10) report the differential patterns in repeal and non-repeal states in children's outcomes as adults, including poverty status, welfare receipt, childbearing, marital status, educational attainment, employment, and the likelihood of being incarcerated.[4] For outcomes that are negative, such as poverty status and welfare receipt, the positive selection story would predict a negative coefficient on repeal*1971-1973, with a zero coefficient on repeal*1976-1979; the opposite is predicted for outcomes that are positive, such as college graduation or employment.

In most cases, the direction of the effect based on the repeal*1971-1973 coefficient is that which would be predicted by the positive selection found in Gruber, et al. (1999). The measured effect of having been born in an early-legalization state between 1971 and 1973 is negative for adverse outcomes such as poverty, welfare receipt, and for being incarcerated, and positive for beneficial outcomes such as being employed. The results on education are perhaps most striking, with negative effects on being a high school dropout or graduate and on having only some college, and a large positive effect on being a college graduate, indicating that abortion legalization shifted the distribution of education upward. There is a negative effect on marriage and a positive effect on number of children, but the welfare consequences of these findings is unclear.

These estimates, however, are not convincing for two reasons. First, they are generally insignificant, with the exception of three of the educational attainment measures and incarceration. Second, there is strong evidence of trends in outcomes that are not related to abortion legalization, in that the coefficient on repeal*1974-1975 and repeal*1976-1979 are

[4] In some cases, we hypothesized that the effect of parental fertility control would be stronger on the outcome of a certain at-risk subpopulation. For example, women are at much higher risk of welfare receipt, while men and African-Americans are at higher risk of incarceration. We therefore conducted the analysis separately by sex and by race, but the results did not differ significantly from what is presented here.

uniformly of the same sign and larger than the coefficient on repeal*1971-1973. Consider, for example, college graduation. The coefficient on repeal*1971-1973 shows that the odds of being a college graduate rose by 2.3 percentage points in the early repeal states over the 1971-1973 period, relative to other states. This is consistent with positive selection due to abortion availability. At the same time, however, the coefficient on repeal*1974-1975 is even larger, at 3.3 percentage points, and the coefficient on repeal*1976-1979 is larger yet, at 5.6 percentage points. This is inconsistent with selection due to abortion availability, since abortion availability was rising (and births falling) in the other states, relative to the early repeal states, over the 1974-1979 period. Moreover, Gruber et al. (1999) found that the child living circumstances of birth cohorts from repeal and non-repeal states re-converged by the end of the 1974-1979 period, in contrast to the apparent divergence seen in adult outcomes during this period.

Of particular interest here are the results in the final column, for incarceration. There is a significant negative coefficient on repeal*1971-1973 indicating that incarceration fell by 0.22 percentage points in the early repeal states in this period, relative to other states. This is consistent with selection effects from abortion, and with the results of Donahue and Levitt. As with the other findings in this table, however, this effect rises over time, so that by the 1976-1979 era when abortion was legal nationwide, the incarceration rate differential for the early repeal states had actually grown to 0.55 percentage points. This is inconsistent with an abortion legalization effect.[5]

B. Reduced Form Results From the 1990 Census

[5] Joyce (2004) reports the results from a similar specification in models of arrest and murder rates (in his Table 2) and finds similar patterns of results.

As noted earlier, one potential weakness of our identification strategy is the assumption that there are no age-specific trends between repeal and other states that are not captured by our linear or quadratic trends. The pattern of results in Table 1 appears inconsistent with that assumption; even including our linear and quadratic trends, there appears to be a steady trend towards improving outcomes in the early repeal states over the entire range of birth cohorts from 1971 to 1979 (i.e. those age 21 to 29 in 2000). Even adding cubic state-specific trends does not change the basic pattern of results.

To test whether we are simply picking up differential age trends across states, we turn to the 1990 census. We estimate similar models in the 1990 census, where our treatment and control groups are not identified by birth cohort, but rather by age. So we estimate models of the form:

$$\begin{aligned} OUTCOME_{st} = {} & \beta_1 REPEAL_s{*}A2729 + \beta_2 REPEAL_s{*}A2526 + \beta_3 REPEAL_s{*}A2124 \\ & + \beta_4\delta_s + \beta_5\tau_t + \beta_6\delta_s{*}TREND + \beta_7\delta_s{*}TRENDSQ + \beta_8 X_{st} + \varepsilon_{st} \end{aligned} \quad (4)$$

where A2729 is an indicator for those age 27-29 in the 1990 census, A2526 is an indicator for those age 25-26, and A2124 is an indicator for those age 21-24. If we are capturing differential state age trends in the 2000 Census, and such trends are time invariant, then we should see them in the 1990 census as well.

The results of estimating equation (4) are presented in Table 2. They do not support the notion of strong negative trends with age in the repeal states. If anything, the trends for many outcomes are positive in these 1990 data. But the coefficients are uniformly small and insignificant. It comes as no surprise, therefore, that estimates from “triple difference” models (not reported here), incorporating both the 1990 and 2000 Census and interact a 2000 Census indicator with the repeal*1971-1973, the pattern of results observed in Table 1 does not change much. Thus, the poor results for the 2000 Census do not appear to be caused by underlying state-specific age trends.

C. Marginal Child Estimates

As discussed earlier, a convenient means of interpreting these legalization effects is in a "marginal child" framework, where the legalization interactions are used to instrument birth rates. Estimates of the characteristics of the marginal child obtained from a two-stage model like that described in equations 2 and 3 are presented in Table 3; each cell represents estimates of α_1 from equation 2 for a different regression. The columns reflect the different outcomes, and rows reflect OLS versus 2SLS estimates where the dependent variable is in logs or levels.

OLS results for these equations are of mixed signs, and generally insignificant. When we instrument in the second set of rows, the sign on the coefficients are generally consistent with a positive selection story: the marginal child was more likely to be on welfare, more likely to be a high school dropout or graduate, and less likely to be a college graduate. But the coefficients are generally insignificant, reflecting the weak reduced form results.

The coefficients on the 2SLS estimates are significant for high school graduate (marginally), college graduate and employment. Yet it is clear from the reduced form that these effects are not due to legalization, but due to underlying repeal-specific trends. This is illustrated by the over-identification test statistics in the bottom of the table: for those outcomes where the 2SLS coefficients are statistically significant, the over-id test fails (only at the 8% for employment). Thus, as our reduced form results indicate, there is no evidence here of differential characteristics for the marginal child.

V. LEGALIZATION VS. ABORTION RATE VARIATION

The finding of no clear impact of abortion legalization on adult outcomes contradicts the strong findings of Donahue and Levitt (2001) for crime. Of course, our identification strategy is quite different than theirs. Donahue and Levitt rely on variation in the abortion rate across states during the entire 1970s, while we focus only on the variation in the birth rates due to legalization. In this section, we compare and contrast the approaches.[6]

A. Results using Abortion Rate Variation

We begin, in the first row of Table 4, by replicating our regressions in the 2000 Census using variation in the abortion rate. That is, we run the same regressions as earlier, but in place of the repeal interactions we include the abortion rate in the individual's state and year of birth. These data are available by state of residence from the Alan Guttmacher Institute, although they contain no information on abortions performed prior to 1973. We follow one approach that Donahue and Levitt (2004) introduced in their reply to Joyce (2004). In one specification, they start at an abortion rate for all states equal to zero in 1969 and linearly interpolate abortion rates between 1969 and the observed values in 1973 to fill in the missing years just for the states that repealed their abortion prohibitions prior to Roe v. Wade.[7]

[6] Donahue and Levitt (2001) label their key right hand side variable as the abortion rate, but its technical label is the abortion ratio (abortions per 1,000 births). The actual "abortion rate" is the number of abortions per 1,000 women of childbearing age (15 to 44). Donahue and Levitt (2004) show that the use of the rate or the ratio does not change the qualitative nature of their results. Since births are endogenous we prefer to use the "abortion rate" in our analyses, although we also find that our results our insensitive to this decision. Their follow-up paper also uses abortion data by state of residence, rather than by state of occurrence as the original paper uses. We follow their more recent approach here as well.

[7] Joyce (2004) criticizes the use of zero values for the abortion rate prior to legalization since at least some abortions were certainly being performed in the years prior to legal abortion. Donahue and Levitt (2004) respond that this form of measurement error is likely to bias downward their estimates of the impact of abortion on crime.

The results suggest that there is a strong positive correlation between the abortion rate in the state and year of birth and better later life outcomes. Those born in states and years with high abortion rates are (marginally) significantly less likely to be on welfare, significantly less likely to be a high school graduate only, (marginally) significantly less likely to only have some college, and significantly more likely to be a college graduate. The effect on incarceration is positive but insignificant.[8] Overall, despite the lack of an effect of abortion legalization on outcomes, there is a strong effect of the abortion rate for some adult outcomes. This finding raises three questions that we address in the following sections.

B. What is the Source of Variation of Abortion Rates?

The first question is: what is the source of variation in the abortion rate that causes it to yield these positive findings for later life outcomes? It is possible, for example, that the abortion rate is simply capturing the trends documented above: that, starting with the 1970 birth cohort, outcomes are getting better and better over time for subsequent cohorts in the repeal states relative to other states. This improvement in the repeal states is above and beyond the linear and quadratic trends we use in this model; it is a sharply increasing improvement in the early repeal states starting in 1970. Is this what drives the abortion rate results?

To address this question, the next panel of Table 4 includes both our legalization interactions and the abortion rate. In fact, we find that the abortion rate by and large continues to be significantly correlated with college graduation, and is now, in fact, marginally significantly related to incarceration. Thus, variation in the abortion rate above and beyond that brought about

[8] This finding does not necessarily contradict that in Donahue and Levitt (2001). Our measure of incarceration is an indication of the number of people who are committing crimes, while most of the Donahue and Levitt analysis is based on the number of crimes committed. As they recognize, a few criminals may be conducting a lot of the crime, so it may be difficult to identify an effect on incarceration while crime may have been significantly reduced.

by the pattern of abortion legalization appears to drive these results suggesting positive selection. This abortion rate finding is also not due to one or two states with particularly strong effects, as the results are robust to dropping states with the strongest effect on the estimate.

Given that the most striking results for both the time*repeal interactions and the abortion rate are for college graduation, we were also concerned that these findings may be driven by some omitted determinant of college going. It could be the case that those states that experienced early abortion legalization or had the highest abortion rates also experienced different trends in, say, college tuition, which would explain the trending coefficients of our repeal dummies.

To test those possibilities, we collected data on tuition levels (Washington State Higher Education Coordinating Board 2004), college openings (Currie and Moretti, 2003), and the unemployment rate (available online from the Local Area Unemployment Statistics division of the Bureau of Labor Statistics). We re-estimated equation (1) with either state average undergraduate tuition and fees at age 18, the number of four-year college openings at age 18, the unemployment rate at age 18, or a combination. None significantly changed the coefficients on the repeal dummies.

We also went back and repeated the exercise relating variation in abortion rates to children's outcomes using data from the 1980 Census and the same outcomes that Gruber, et al. (1999) use. The results of this exercise are reported in Table 5. Here we also find some evidence of an impact of abortion rates on cohort quality. For instance, an additional 10 abortions per 1,000 women is predicted to reduce the rate of children living in single-parent households by 0.44 percentage points. With a mean value of 18.7 percent, this amounts to about a 2 percent reduction. Adding the abortion legalization variables does not change these results much.

C. What is the Mechanism?

Why would abortion variation above and beyond legalization matter for outcomes? One possibility is that the mechanism could be the same (abortions leading to a lower number of unwanted births), but the abortion rate approach might have more power. Certainly there is additional variation in the use of abortion between states (following legalization) and over time within states beyond that solely associated with legalization. It is possible that this additional variation could capture more abortion-induced variation in unwanted births across states and over time than is captured by abortion legalization alone. If this were true, abortion rates would be a more powerful instrument than the legalization variables, yielding more efficient estimates of the impact of abortion availability on cohort outcomes.

But there are at least three arguments against using abortion rates as a more powerful instrument. First, the abortion rate is a behavioral outcome resulting from abortion and pregnancy decisions that may be influenced by social factors that differ across states and time. These social factors may also influence the environment in which children grow up and, hence, their adult outcomes. The variation in abortion access generated by the pattern of abortion legalization across states is more likely to be unrelated to these behavioral issues, as described in more detail in Levine (2004). Although its eventual legalization in different states may have been related to longer standing differences in social attitudes, the precise timing of court decisions and fiercely disputed state legislation suggests that legalization is at least more likely to be exogenous.

Second, if variability in the abortion rate provides a sufficient means for testing the causal impact on adult outcomes, then using the time variation over states in abortion legalization should provide similar, but less precise, results. But the results obtained using indicators of abortion legalization are more suggestive of a spurious correlation than a causal effect. This appears in the fact that the outcomes across states appear to be trending differentially; this does not match the time series pattern predicted by the difference in the legal status of abortion or the results in children's living circumstances estimated using the 1980 Census and reported in Gruber, et al. (1999). Moreover, the over-id tests often reject the use of abortion legalization as an instrument and those variables are more plausibly exogenous than the abortion rate, as we just discussed.

Finally, the abortion rate does not appear to be associated with lower birth rates. The first row of the final column of Table 4 is identical to the first column of Table 1, where the dependent variable is the log of the "survival rate" (surviving cohort size in 2000 per 1,000 women of childbearing age in year of birth), except the legalization variables are replaced with the abortion rate on the right hand side. In Table 4, we find that the relationship between abortion rate and cohort size is statistically insignificant, *despite* the fact that the abortion variable by itself includes legalization effects. In the second panel of Table 4, we include the abortion rate along with the legalization variables. After holding constant the effects of abortion legalization, the estimated impact of the abortion rate on cohort size is now positive, albeit statistically insignificant. Thus, the mechanism for the abortion rate does not appear to be through a direct reduction in births.[9]

[9] This last finding is consistent with Levine (2004), who studied patterns in pregnancies, abortions and births in the early repeal states compared to other states through the 1970s. He found that, in the immediate aftermath of legalization, abortions rose and births fell, with no effect on pregnancies. Over time, however, pregnancies rose in the early repeal states, offsetting the rise in abortions, so that there was no impact on births. A theoretical

In fact, Joyce (2004) raises this criticism and Donahue and Levitt (2004) respond to it, stating: "the ability to improve the timing and circumstances of births means that abortion improves birth and life outcomes even when it does not lower the total number of births." Indeed, if better timed births are higher quality births, either in terms of the child's underlying health or the home environment in which it is raised, then this could explain the results. Yet, once again, this is inconsistent with the results on cohort size: even a delay in timing of births should show up in the cohort size, with higher abortion rates associated with smaller cohorts at a point in time, and larger cohorts later. Moreover, we have little evidence regarding the linkage between the use of abortion and birth timing. Since the additional abortions that occurred in the late 1970s may be attributable to pregnancies that would not have otherwise occurred, we have no way of knowing whether they altered birth timing. In fact, Ananat, et al. (2004) find that the reductions in births associated with abortion legalization in the early 1970s were permanent and were not "made up" in the form of births later in life. Therefore, we are skeptical that changes in birth timing circumvent the problem that variation in the abortion rate beyond legalization does not appear to be associated with variations in fertility.

This leaves only one possible mechanism that we can construct that would link abortion variation beyond legalization to birth selection; it may alter the *mix* of births that do occur. This argument would be constructed on the basis of a matching model, which we do not formally develop here. In this hypothetical model, one could imagine that when abortion access is low, match qualities will be low and children will suffer, leading to poor later life outcomes.[10] When

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explanation for this finding is presented in Kane and Staiger (1996) and Levine (2004). They argue that, for large changes in the cost of abortion (such as through legalization), abortions will be used to terminate unwanted pregnancies. For smaller changes in the cost of abortion, however (such as through changes in abortion access once legal), abortions will be used as *insurance* against pregnancy, with women more likely to become pregnant since this insurance is available.

[10] This mechanism, while plausible, is exactly the *opposite* of that suggested by Akerlof, Yellen and Katz (1996). In their model, the absence of abortion availability gives women power over potential mates: if women become

abortion access increases, however, women are able to shop around more for a prospective husband, using abortion as a birth control device for any unwanted pregnancies. In that case, abortion would improve the quality of the match and this would lead to better outcomes for her children. This matching story would not alter total cohort size and improve cohort quality, but note that even this model is constrained by the fact that it cannot take any longer for the matches to occur because that would alter birth timing.

Another alternative along these lines is suggested by the results of Kane and Staiger (1996). They argue that better access to abortion may lead women to risk getting pregnant with a desired mate to see if they will marry them, and then to abort if that does not work out. As a result, the greater availability of abortion may lead some women to have "wanted" births that otherwise may not occur. These additional births could also counter a reduction in other unwanted births; both forces would work towards positive selection and improved cohort quality.

D. What is the Right Approach?

In summary, we believe it is possible to generate arguments supporting the use of the abortion rate (aside from that associated with abortion legalization itself) as an appropriate method to generate a causal estimate of the impact of abortion on cohort quality. On the other hand, those arguments have significant obstacles to overcome. Perhaps most importantly, it is not clear that the abortion rate provides exogenous variation. In addition, complicated models are required that would lead variation in the abortion rate to generate a changing mix of children born without altering the number of children born or, potentially, the timing of their births. The

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pregnant, they can demand a marriage. Once abortion is available, women lose that power, as their potential mates can simply insist they get an abortion instead. In this case, abortion does not give women the right to select a mate, but rather limits that right by limiting her bargaining power. These explanations have potentially testable implications for the impact of abortion availability on marital satisfaction/stability.

lack of empirical evidence that would support these models further adds to the difficulties of making those arguments. Therefore, we are much more comfortable basing our conclusions on the variability in abortion generated by the double quasi-experiment associated with abortion legalization. Beyond the fact that it provides more plausibly exogenous variation, the mechanism for legalization to affect outcomes is very clear since we are able to point to evidence that it changes the size of cohorts.

VI. SUMMARY AND CONCLUSIONS

This paper set out to determine whether the improvements in cohort quality observed in childhood as a result of abortion legalization persisted into adulthood. We initially adopted the estimation strategy that Gruber, et al. (1999) employed in their initial work on children's well-being and applied it directly to adult outcomes using data from the 2000 Census. The results based on that approach did not provide much evidence of a causal impact of abortion legalization. Although there are some indications that outcomes improved for children born in repeal states relative to those in other states in the early 1970s, we would have expected that gap to diminish after abortion was legalized in the rest of the country to support a causal effect. No such pattern is observed. In fact, the gap between birth cohorts typically diverges for the various outcomes. We were unable to identify other factors that may have led to this pattern, including differences in underlying patterns by age and state as well as a few other state-specific factors that may have altered children's environment as they developed. Based on all these findings, we are unable to find any evidence of a causal relationship.

We then proceeded to adopt the identification strategy implemented by Donahue and Levitt (2001) in their work on abortion and crime. Their work is based on the variation across states/years in the abortion rate, which incorporates both the variation associated with abortion legalization along with additional state-to-state variation and within state over time variation in the use of abortion. The results of that analysis do provide some evidence of positive selection, particularly in the form of greater educational attainment among children born in cohorts in which the abortion rate was higher. The remainder of the paper described the advantages and disadvantages of the two different approaches. We discussed the limitations of their approach that we believe hinders ones ability to draw causal inferences based on variation in the abortion rate. Although we are unable to definitively rule out that approach, we concluded that those limitations are substantial enough that we would rather draw our conclusions on the abortion legalization experiments.

Based on our analysis, we are unable to conclude that the change in abortion access brought about by abortion legalization had a causal effect on the adult outcomes of children. Part of the problem is that the effect we are looking for is likely to be relatively small and may be difficult to detect. For instance, Gruber, et al. (1999) found that the marginal child was 48 percent more likely to live in poverty than the average child born during that period. That estimate was statistically significant; the standard error was 21.2 percent. But past research on the intergenerational correlation in income suggests that income in childhood is only correlated with income in adulthood at a rate of about 0.4 (Solon, 1992; and Zimmerman, 1992). Therefore, the marginal child is probably much less likely to live in poverty as an adult than the estimate obtained from childhood. Identifying an effect of that size may be difficult even in a dataset as large as the Census.

Moreover, the pattern of outcomes across repeal and non-repeal states over time is troubling. It suggests that the adult outcomes of those born in repeal states are improving monotonically relative those born in non-repeal states over the course of the 1970s. This timing pattern is inconsistent with abortion legalization, which is the basis of our conclusions. But this pattern also suggests that some other underlying factors must have been either on-going at the time of birth or affected these cohorts differentially in the years since birth. Understanding the cause of these strong trends in outcomes would be a fruitful topic for future work.

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Table 1: Impact of Abortion Legalization on Adult Outcomes, 2000 Census

	(1) ln("Survival" Rate)	(2) % in Poverty	(3) % On Welfare	(4) % Single Parent	(5) % HS Dropout	(6) % HS Graduate	(7) % Some College	(8) % College Grad	(9) % Employed	(10) % Incarcerated
repeal*1971-1973	-4.263	-0.319	-0.274	-0.082	-0.370	-1.036	-0.885	2.291	0.029	-0.223
	(1.024)	(0.307)	(0.219)	(0.289)	(0.281)	(0.419)	(0.492)	(0.928)	(0.403)	(0.110)
repeal*1974-1975	-0.437	-0.390	-0.314	-0.046	-0.669	-1.774	-0.885	3.327	1.191	-0.584
	(2.006)	(0.638)	(0.451)	(0.438)	(0.445)	(0.505)	(1.982)	(2.436)	(0.710)	(0.164)
repeal*1976-1979	2.642	-1.045	-0.276	0.125	-0.515	-1.736	-3.305	5.556	2.189	-0.548
	(3.763)	(0.754)	(0.670)	(0.706)	(0.476)	(0.977)	(2.986)	(3.695)	(1.043)	(0.221)
Mean of Dependent Variable		17.0%	2.6%	9.3%	12.6%	28.1%	37.5%	22.6%	74.6%	2.1%
P-value for test of equality of coefficients on repeal*1971-1973 and repeal*1976-1979	0.02	0.14	1.00	0.67	0.72	0.27	0.35	0.25	0.00	0.08
P-value for test of joint significance of reported coefficients	0.00	0.27	0.92	0.91	0.45	0.01	0.00	0.02	0.01	0.00

Notes: Reported coefficients multiplied by 100 to reflect changes in percents for all outcomes. They represent the coefficients on the repeal interactions from regression specifications such as equation (1), including the following other regressors: the insured unemployment rate, per capita income, crime rate, percent of the population that is nonwhite, a full set of state and year dummies, and state-specific quadratic trends. In all specifications, 750 observations are available representing the 50 states and 15 years of birth cohorts (1965-1979). All standard errors are clustered at the state level.

Table 2: Impact of Age/State Patterns on Adult Outcomes, 1990 Census

	(1) ln("Survival" Rate)	(2) % in Poverty	(3) % On Welfare	(4) % Single Parent	(5) % HS Dropout	(6) % HS Graduate	(7) % Some College	(8) % College Grad	(9) % Employed	(10) % Incarcerated
repeal*age27-29	1.922	0.918	0.007	-0.072	0.164	-0.335	0.337	-0.166	-0.448	-0.142
	(1.060)	(0.629)	(0.333)	(0.193)	(0.213)	(0.445)	(0.736)	(0.817)	(0.930)	(0.114)
repeal*age25-26	2.221	1.654	0.133	-0.325	0.137	-1.379	0.575	0.667	-0.774	-0.106
	(2.471)	(1.940)	(0.466)	(0.232)	(0.396)	(0.646)	(2.932)	(2.771)	(2.073)	(0.162)
repeal*age21-24	1.368	1.484	-0.353	-0.356	-0.070	-0.604	-0.010	0.685	0.344	0.065
	(2.145)	(2.943)	(0.648)	(0.411)	(0.385)	(0.764)	(5.449)	(5.152)	(2.791)	(0.247)
Mean of Dependent Variable		14.8%	4.4%	8.3%	10.3%	35.5%	33.9%	20.2%	76.7%	1.3%
P-value for test of equality of coefficients on repeal*1971-1973 and repeal*1976-1979	0.71	0.81	0.29	0.30	0.47	0.65	0.94	0.85	0.68	0.22
P-value for test of joint significance of reported coefficients	0.77	0.07	0.00	0.16	0.51	0.00	0.22	0.24	0.00	0.36

Notes: Reported coefficients multiplied by 100 to reflect changes in percents for all outcomes. They represent the coefficients on the repeal interactions from regression specifications such as equation (4), including the following other regressors: the insured unemployment rate, per capita income, crime rate, percent of the population that is nonwhite, a full set of state and year dummies, and state-specific quadratic trends. In all specifications, 750 observations are available representing the 50 states and 15 years of birth cohorts (1955-1969). All standard errors are clustered at the state level.

Table 3: Estimates of the Adult Outcomes of the Marginal Child, 2000 Census

	(1) % in Poverty	(2) % on Welfare	(3) % Single Parents	(4) % HS Dropout	(5) % HS Graduate	(6) % Some College	(7) % College Grad	(8) % Employed	(9) % Incarcerated
OLS									
Dep. Var. in Levels	2.296	0.568	0.979	0.282	3.479	8.069	-11.831	0.707	-1.672
	(3.447)	(1.060)	(1.947)	(2.538)	(2.714)	(6.310)	(5.981)	(3.713)	(0.974)
Dep. Var. in Logs	8.480	15.182	7.952	-3.477	12.052	18.492	-48.764	2.639	-57.976
	(15.727)	(43.191)	(21.909)	(22.903)	(9.112)	(13.494)	(40.549)	(5.625)	(46.196)
2SLS									
Dep. Var. in Levels	0.567	2.840	1.781	2.489	6.230	0.719	-9.439	11.567	0.017
	(2.329)	(1.840)	(2.415)	(3.548)	(3.766)	(11.448)	(7.266)	(3.625)	(1.395)
Dep. Var. in Logs	-7.372	93.258	22.059	7.801	23.147	4.059	-139.306	18.308	-3.799
	(20.451)	(67.526)	(26.566)	(35.099)	(14.870)	(23.206)	(55.248)	(6.404)	(72.526)
P-value for over-identification test	0.2972	0.7718	0.9191	0.3871	0.0003	0.0001	0.0242	0.0868	0.0011

Notes: Reported coefficients are those from estimation of equation 2 (multiplied by 100 to reflect changes in percents), where we have allowed the dependent variable to be measured separately in logs or in levels. In each model, other regressors include: the insured unemployment rate, per capita income, crime rate, percent of the population that is nonwhite, a full set of state and year dummies, and state-specific quadratic trends. In all specifications, 750 observations are available representing the 50 states and 15 years of birth cohorts (1965-1979). All standard errors are clustered at the state level.

Table 4. Impact of Variation in the Abortion Rate on Adult Outcomes, 2000 Census

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	% in Poverty	% on Welfare	% Single Parent	% HS Dropout	% HS Graduate	% Some College	% College Grad	% Employed	% Incarcerated	Ln("Survival" Rate)
Rate	-0.013	-0.010	-0.016	-0.011	-0.042	-0.060	0.113	0.017	0.008	-0.030
	(0.027)	(0.006)	(0.008)	(0.014)	(0.022)	(0.036)	(0.035)	(0.027)	(0.008)	(0.047)
Repeal*1971-1973	-0.192	-0.212	0.063	-0.301	-0.719	-0.300	1.287	-0.229	-0.331	-2.207
	(0.425)	(0.273)	(0.241)	(0.268)	(0.483)	(0.500)	(0.928)	(0.524)	(0.101)	(0.947)
Repeal*1974-1975	-0.287	-0.264	0.072	-0.613	-1.518	-0.400	2.518	0.983	-0.671	0.777
	(0.669)	(0.480)	(0.409)	(0.423)	(0.524)	(2.000)	(2.283)	(0.770)	(0.171)	(1.819)
Repeal*1976-1979	-0.978	-0.243	0.202	-0.478	-1.568	-3.000	5.023	2.051	-0.606	2.434
	(0.780)	(0.699)	(0.656)	(0.492)	(0.890)	(2.900)	(3.433)	(1.041)	(0.219)	(2.893)
Rate	-0.014	-0.007	-0.016	-0.008	-0.035	-0.068	0.111	0.029	0.012	0.016
	(0.030)	(0.006)	(0.008)	(0.013)	(0.026)	(0.035)	(0.041)	(0.029)	(0.007)	(0.039)

Notes: Reported coefficients multiplied by 100 to reflect changes in percents for all outcomes. They represent the coefficients on the indicated variables from regression models similar to equation (1), including the following other regressors: the insured unemployment rate, per capita income, crime rate, percent of the population that is nonwhite, a full set of state and year dummies, and state-specific quadratic trends. In all specifications, 750 observations are available representing the 50 states and 15 years of birth cohorts (1965-1979). All standard errors are clustered at the state level.

Table 5. Impact of Variation in the Abortion Rate on Child Outcomes, 1980 Census

	(1) % in Poverty	(2) % on Welfare	(3) % Single Parent	(4) Infant Mortality	(5) Low Birth Weight
Rate	-0.010	-0.017	-0.044	-0.002	-0.004
	(0.010)	(0.008)	(0.011)	(0.001)	(0.003)
Repeal*1971-1973	-0.284	-0.308	-0.429	-0.027	-0.035
	(0.311)	(0.245)	(0.318)	(0.029)	(0.074)
Repeal*1974-1975	0.239	-0.495	-0.279	-0.018	-0.093
	(0.452)	(0.356)	(0.461)	(0.042)	(0.108)
Repeal*1976-1979	0.808	-0.108	0.620	0.040	0.147
	(0.662)	(0.522)	(0.676)	(0.062)	(0.160)
Rate	-0.002	-0.012	-0.034	-0.002	-0.003
	(0.011)	(0.009)	(0.011)	(0.001)	(0.003)

Notes: Reported coefficients multiplied by 100 to reflect changes in percents for all outcomes. They represent the coefficients on the indicated variables from regression models similar to equation (1), including the following other regressors: the insured unemployment rate, per capita income, crime rate, percent of the population that is nonwhite, a full set of state and year dummies, and state-specific quadratic trends. In all specifications, 750 observations are available representing the 50 states and 15 years of birth cohorts (1965-1979). All standard errors are clustered at the state level.

Figure 1: Trends in Birth and Abortion Rates

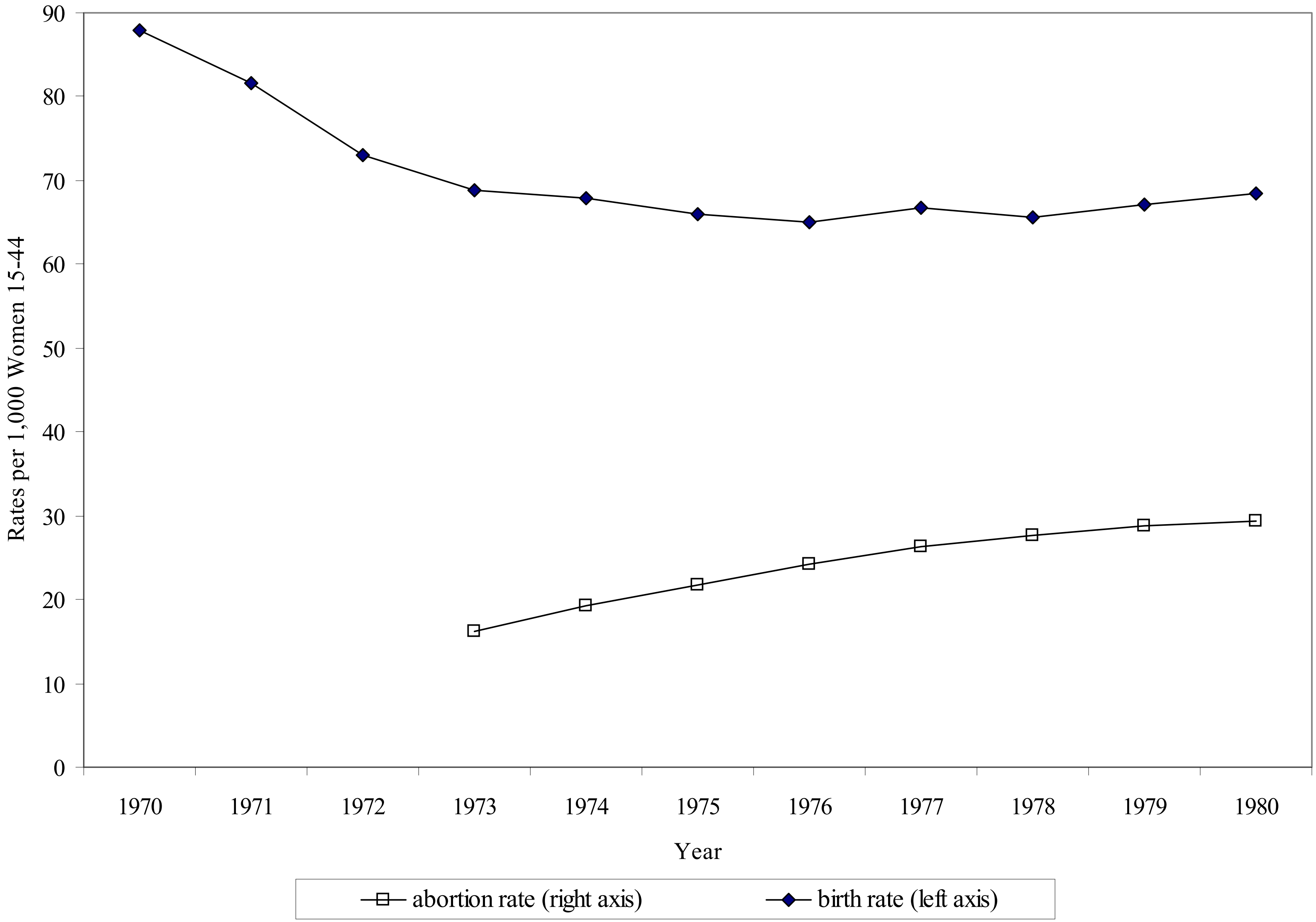
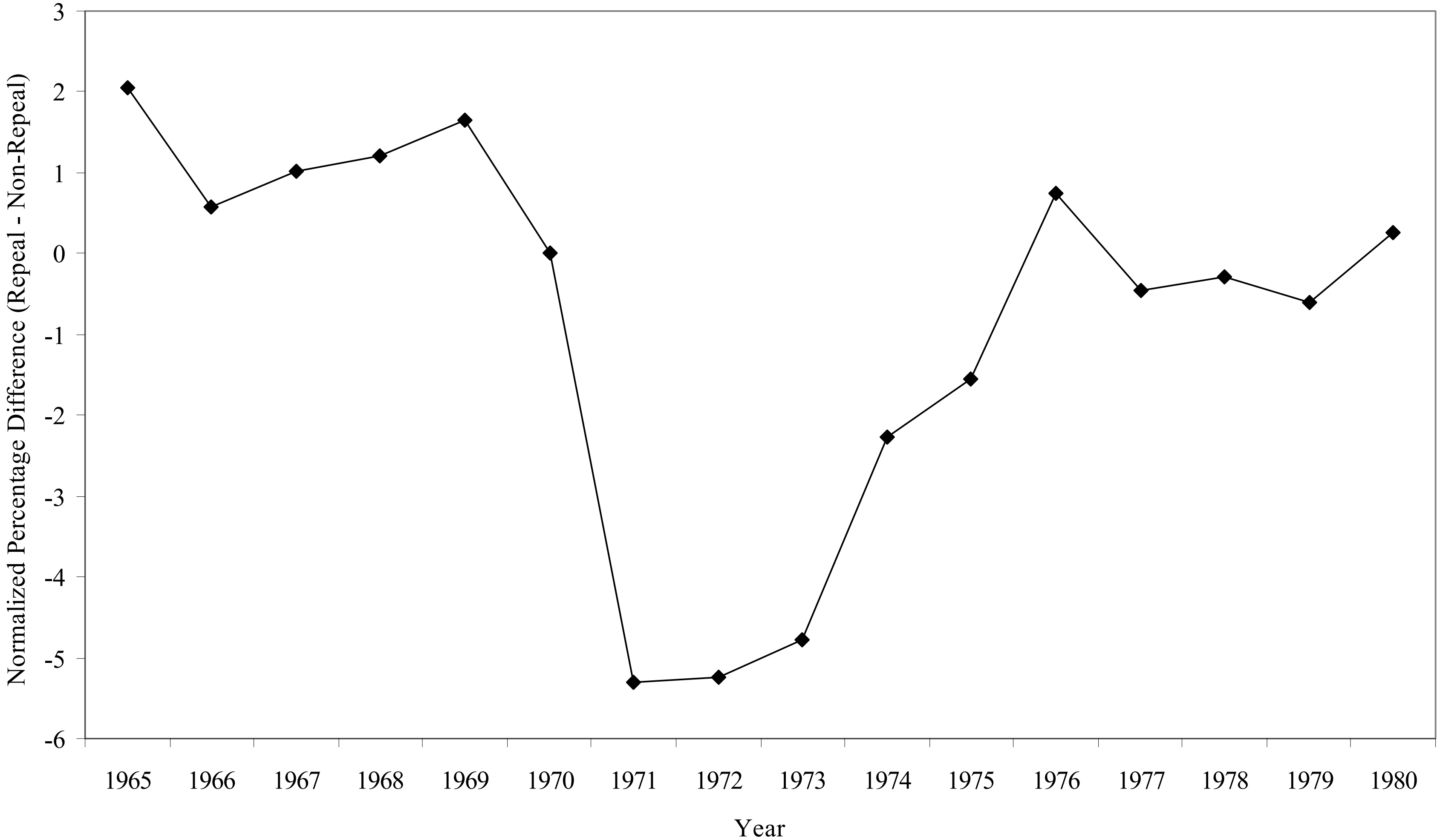


Figure 2: Impact of Abortion Legalization on Birth Rates



Note: The percentage differences have been normalized so that its value equals zerio in 1970.
Source: Levine, et al. (1999).

Figure 3: Abortion Rates by Repeal Status